How Electoral Institutions Shape Citizen Participation and Legislative Behavior

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Abstract

The electoral system is often treated as fixed, but throughout U.S. history significant changes in electoral institutions, or in political conditions dictated by electoral institutions, make it possible to identify more precisely the role that the electoral system plays in the democratic process. This dissertation examines three related questions, each focusing on an aspect of the influence of electoral rules on political behavior. How has the ability to directly elect representatives influenced other forms of citizen engagement with government? How has competitiveness influenced voter turnout? Finally, when separate elections lead to differences in partisan control over the branches of government, what is the effect on policymaking in Congress?

The first chapter shows that petitioning campaigns have historically substituted for the communication and accountability obtained through direct elections. I estimate that rates of petitioning to the Senate declined by 30% when the passage of the 17th Amendment ended the practice of indirect election by state legislatures and replaced it with direct elections. The implication is that electoral reforms meant to improve representation may weaken other ties between citizens and lawmakers.

The second chapter examines the relationship between electoral competition and turnout. Past research has found that citizens vote at higher rates in response to closer elections, either through instrumental voting at the individual level or through voter mobilization by elites. In contrast, this chapter demonstrates that citizens living in competitive congressional districts differ markedly from those in uncompetitive districts along a range of dimensions

other than turnout. Using an individual panel based on voter files from all 50 states and exploiting variation in competitiveness induced by the 2012 redistricting cycle yields a precisely estimated null effect of competitiveness on turnout.

The third chapter re-examines whether divided government reduces legislative productivity. After developing the most comprehensive database to date of significant acts of Congress—from 1789-2010—this chapter shows that unified control corresponds with one additional significant act passed per Congress in the 19th Century and four additional such acts in the 20th Century. However, party control of government cannot explain the broad historical trends in the rate at which Congress passes significant legislation.

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Introduction

Understanding how institutional arrangements influence the political process is crucial for interpreting past political events and for developing theory to explain and to anticipate future change. As an example, consider the case of decennial redistricting. The redistricting process re-shuffles citizens into new congressional districts, changing the underlying level of competitiveness in the district, and (potentially) the links between voters and incumbent politicians. How does the new set of political conditions imposed by redistricting influence political processes that occur downstream? Changes in competition might influence citizens' choices to turn out. Changes in turnout might lead to a different set of elected officials in Congress, and, in turn, a new set of policy outcomes. Tracing out these possibilities need not be left to guesswork; their veracity can be empirically tested.

This dissertation takes advantage of such changes in political conditions to answer several questions central to the study of representation, political participation, and policymaking. Elections for Congress and the presidency are conducted according to a set of rules that influence the behavior of citizens participating in politics and the behavior of officials elected to hold office. The chapters in this dissertation are based around the insight that, while the electoral system in the U.S. is often treated as fixed, in fact throughout U.S. history significant changes in electoral institutions, or in political conditions dictated by electoral institutions, have occurred. These changes make it possible to answer causal questions about the effects of electoral conditions on citizen and legislator behavior, and, more broadly, the place of electoral institutions in the democratic process. This dissertation examines three related questions, each focusing on a different aspect of this relationship.

First, what is the relationship between representation through the vote and representation through other forms of participation, such as sending petitions to Congress? In the first chapter, I show that petitioning campaigns—a form of participation based on direct contact with representatives—have historically substituted for the communication and accountability obtained through direct elections.[1] I estimate that rates of petitioning to the Senate declined by 30% due to the shift to direct elections that occurred with the 17th Amendment. The implication is that electoral reforms meant to improve representation may weaken other ties between citizens and lawmakers. Two institutional details in particular help in identifying the effects of direct election. First, members of the House of Representatives have always been directly elected, so petitions sent to the House serve as a natural control group for petitions sent to the Senate. Second, the implementation of direct election through the 17th Amendment occurred in a staggered manner based on each senator's class—an ordering assigned randomly when each state joined the Union. To implement this study, I draw on a new data set of all petitions submitted to Congress from 1881–1949. This data comprises a vast repository of close to half a million petitions in all and sheds light on a form of political activity that was previously not possible to track systematically.

The second chapter takes advantage of changing political conditions to resolve theoretical questions about the relationship between electoral competition and political participation in the form of turnout.[2] Previously, a wide range of cross-sectional studies have found that electoral competition increases turnout, either through instrumental voting at the individual level or through voter mobilization by elites. In the second chapter, I demonstrate that citizens living in competitive congressional districts differ markedly from those in uncompetitive districts, which calls into question the general consensus on the relationship between competitiveness and turnout for congressional elections. Using an individual panel based on voter files from all 50 states, I exploit within person variation in competitiveness induced

[1] Based on the paper "Representation Replaced: How Congressional Petitions Substitute for Direct Elections."

[2] Based on the paper "Does Electoral Competitiveness Increase Turnout? Evidence from a National Sample of 2 Million Voters" with Daniel Moskowitz.

by the 2012 redistricting cycle to provide credible estimates of the effect of competitiveness on turnout. I find a precisely estimated null effect, suggesting that neither instrumental voting nor elite mobilization theories operate as previously held in this context. Secondary evidence tracking voter perceptions of competitiveness as well as the behavior of campaigns supports this finding. Voters have scant awareness of electoral competitiveness, and, while campaign spending is strongly related to competitiveness, it is directed into avenues that do not appreciably alter turnout.

The third chapter re-examines whether divided partisan control of the presidency and Congress—due to contrasting election outcomes for House, Senate, and President—plays a meaningful role in determining legislative productivity.[3] This chapter presents the most comprehensive database to date of significant acts of Congress—from 1789-2010. A common database on significant legislation, for which the present effort is a starting point, leads to a better understanding of why Congress does what it does and when it does it. This database is used to test, for the entire history of Congress, whether divided party control of government significantly affects the number of important acts that Congress passes, as is widely conjectured in the literature on Congress and divided government. Previous research has focused only on the period since 1946. This chapter finds that unified control corresponds with 1 additional significant act passed per Congress in the 19th Century and 4 additional such acts in the 20th Century. However, party control of government cannot explain the broad historical trends in the rate at which Congress passes significant legislation. Nixon in 1969 was far more successful with a Democratic Congress than was McKinley in 1897 with a Republican one.[4]

[3]Based on the paper "Divided Government and Significant Legislation, A History of Congress from 1789–2010" with Stephen Ansolabehere and Maxwell Palmer.

[4]Replication data will be available on the Harvard Dataverse after an embargo period has elapsed.

1 Representation Replaced: How Congressional Petitions Substitute for Direct Elections

1.1 Introduction

In a representative democracy, the public delegates decision-making authority to lawmakers because it expects faithful representation, including "continued responsiveness of the government to the preferences of its citizens" (Dahl 1971). Direct election, from the perspective of modern political science, is the mechanism through which representation works. Citizens vote for lawmakers who do what citizens want; lawmakers who do not fulfill the wishes of their constituents lose elections and are removed from office (Canes-Wrone, Brady, and Cogan 2002). This, according to Mayhew (1974), is the electoral connection. While there are modes of representation other than elections, such as direct contact between citizens and lawmakers, these other democratic "channels" are thought to be instrumental only to the extent that they influence elections.

This chapter studies an important form of direct contact that is guaranteed in the First Amendment of the Constitution—the right to petition the government. I examine the relationship between representation through the vote and representation through direct contact by studying the effects of the 17th Amendment, which introduced direct election of senators, on the use of petitions.

Researchers have not reached a consensus on precisely how multiple democratic channels, such as elections and petitioning, operate in tandem. Do democratizing reforms, such as instituting direct elections where there previously were none, lead to increases in other forms of engagement?

There are three possibilities. First, such reforms could in fact contribute to increases in other forms of engagement. For example, Fenno (1978) argues that competitive elections compel incumbent lawmakers to engage in additional casework and to encourage constituents to contact them with specific requests; in this scenario, elections and casework complement each other, an example of one outlet for participation in democracy producing greater engagement in other areas as well. Second, a democratizing reform of one type could have no meaningful impact on other forms of engagement. In a pluralist account where political resources are widely dispersed and groups have varying points of access to the government (Dahl 1961), different groups engage with lawmakers in different ways; availability of an additional democratic channel might have no effect on channels already in place. Third, restrictions on one democratic institution could lead to increased engagement in other areas and vice versa—reform opening up one channel could diminish or substitute for engagement in other areas. Verba, Schlozman, and Brady (1995) observes lower voter turnout in the U.S. than in other democracies, but equal or higher levels of other forms of political engagement. The authors do not pursue this point, but one interpretation is that different forms of political engagement substitute for each other. If this is so, then there are several unexplored implications for political representation.

This chapter demonstrates that democratic reforms fostering one type of political expression can reduce other forms of engagement and, in turn, diminish political representation for some groups. I posit that the practice of congressional petitioning—whereby citizens circulated, signed and delivered formal requests to lawmakers in Congress—partially *substituted* for communication facilitated by direct elections. I examine petitions sent to the Senate before and after the 17th Amendment implemented direct elections, and I find that Senate petitioning declined by more than 30% due to this reform to electoral institutions. I evaluate several explanations for this decline. I argue the empirical evidence is most consistent with the theory that increased discretion gained under direct elections gave senators greater license to ignore issue-specific petitioning requests. This historical case represents an instance where electoral reforms meant to improve one aspect of representation weakened other ties between constituents and lawmakers. If alternative mechanisms such as petitioning contribute to a flourishing political life or allow minority perspectives a voice (Carpenter and Moore 2014), then reforms that diminish these alternatives involve trade-offs that have been overlooked.

To implement this study, I assemble a historical data set tracking petitions submitted to Congress from 1881 to 1949 (47th to 80th Congress). Petitions sent to Congress served as a mechanism by which citizens collectively expressed their preferences. This historical data provides a window into political communication and collective political behavior for a time in which, to my knowledge, no other comprehensive sources have been assembled.

Two unique institutional details help in identifying the effects of direct election on petitioning. First, members of the House of Representatives have always been directly elected, so petitions sent to the House serve as a natural control group for petitions sent to the Senate. Second, the implementation of direct elections through the 17th Amendment occurred in a staggered manner based on each senator's class—an ordering assigned randomly when each state joined the Union. These sources of variation yield estimates showing how petitioning behavior shifted after direct election: To the extent that the 17th Amendment strengthened the electoral connection between constituents and representatives, it also led to a reduced reliance on petitioning. This decline occurred across several topics of petitions; it also appears to have been more pronounced among membership groups and associations—suggesting that key policy demanders shifted their focus away from direct contact after the 17th Amendment.

1.1.1 Petitioning In Context

Using alternatives to elections and public opinion polls to communicate with lawmakers has a long tradition. Town meetings in colonial New England regularly sent resolutions to instruct their state representatives. State legislatures initially used the doctrine of instructions to control the senators they had sent to the Capitol.[1] In fact, the practice of petitioning long predates the founding of the Republic: English Parliaments that met from the 13th to the 15th centuries received more than 16,000 petitions. During the English Revolution, petitioners first began making appeals to public opinion in their formal requests (Zaret 1996). In this sense, notions of democratic representation that incorporated public opinion originated from traditions of petitioning.

In the U.S., petitioning has been an important, if understudied, political tool since the founding. The First Amendment of the Constitution grants citizens the right "to petition the government for a redress of grievances," and throughout history Congress has received numerous petitions including memorials for the abolition of slavery, requests for Civil War pension benefits, and pleas for changes in financial policy and economic relief (Huret 2014). At times petitions have comprised a primary source in the stream of information that lawmakers gathered and processed. For example, when controversy erupted over whether the World's Fair, opening in 1893 in Chicago, would remain closed on Sundays, petitions to Congress proved influential. Congress received thousands of petitions urging passage of a resolution requiring the fair's organizers to close the event on Sundays. The petitioning campaign, part of a grassroots effort by sabbatarians, achieved its goal: when Congress appropriated \$5 million for the fair, the terms of the funding required Sunday closures (Miller 2008b). In a disapproving opinion piece, the *New York Times* would remark that "[p]etitions coming from certain classes of citizens in favor of ill-advised action have had more weight than any consideration of general public welfare."[2]

A petition itself consists of two key parts. The "prayer" contains the requests, instructions, or grievances expressed by the petitioners. The "signatory list" contains the signatures of the individuals who endorse the prayer. For organizers, the petition represents an unstructured means of expression—those who create the petition may address any subject.

[1] Without the doctrine of instructions, John Tyler noted, "the power of electing would be... incomplete, and the Senator, instead of being a servant, would be the uncontrollable sovereign" (Riker 1955).

[2] See "Conservatism in the Senate," *New York Times*, July 17, 1892.

However, for those who sign a petition the choice is binary: to sign or not to sign (Herbst 1993). These distinctions suggest that measuring the aggregate number of petitions captures the level of political activity undertaken by organizers. In contrast to "opinion as measured through polls," petitioning provides a measure of "opinion that influences political action" (Lee 2002). Tracking petitioning activity measures a form of active, public political participation while survey measures of opinion capture privately expressed views, which may never be brought to bear on policy.

In the modern context, petitioning has experienced a resurgence online. Advocacy groups and non-governmental organizations use petitions as a means of expression and as an organizational tool—specifically, to identify supporters and to build lists of valuable contact information. Even the White House website includes a portal that allows submission of online petitions. Scholars have just begun to examine this form of online political activity (Margetts et al. 2013; Hale, Margetts, and Yasseri 2013; Hersh and Schaffner n.d.). While the reduction in costs associated with online petitioning fundamentally changes some aspects (i.e., canvassing and organizational costs were substantial in the 19th and early 20th century but have been reduced considerably in the 21st century for online petitions), the fact that petitioning is a mechanism still in use today suggests that it continues to play a role as an organizational tool and as a means of expression.

1.1.2 Representation and Institutional Change

This chapter examines whether petitions, which provide a record of the frequency and character of communication between constituents and representatives, are influenced by reforms to electoral institutions. Canvassers, who can be thought of as policy demanders for groups, use petitions to express their requests to Congress; their efforts are meant, at least in part, to influence lawmakers. The notion that communication influences governance is not without precedent. Geer (1996) develops a theory describing how public opinion polls affect the behavior of elected officials. He suggests that the introduction of public opinion polls amounted to a technological change that transformed the informational environment, reducing uncertainty about citizen opinion and allowing politicians to shape opinion more effectively. Shifts in communication between citizens and representatives had consequences for elected officials and for the people whose opinions they tried to shape.

The approach taken here is more group-centric; it coincides with a growing body of research that has begun to recognize the role of "policy demanders" in shaping government. For example, recent work on the theory of political parties views party formation as driven by coalitions of policy demanders, who aim to advance their goals through attaining political power (Bawn et al. 2012). Petitioning activity from 1881–1949 can be seen as an attempt at influence by policy demanders—one component of a larger back and forth between organized groups and election-minded politicians. My theory is that the 17th Amendment marked a crucial change in this relationship; by strengthening the electoral connection, it displaced policy demands previously expressed through petitions to the Senate.

Before the 17th Amendment, state legislatures rather than citizens elected senators. This afforded senators considerable political freedom and insulated them from their constituents: "It was easy and dignified to run for the Senate—mainly because one did not have to run at all... almost all nineteenth-century Senatorial campaigns did not even begin until after state legislators were elected, and then the campaigns consisted almost entirely of soliciting the votes of one or two hundred state legislators" (Riker 1986, p. 12). When Congress enacted the 17th Amendment in 1913, senators became more explicitly accountable to citizens.[3] On its surface, such a change represents a substantial shock to the electoral institutions governing the relationship between citizens and representatives. While there is now a growing body of research examining this shift, it went largely unstudied by political scientists until recently. The reason for this omission (also noted in Gailmard and Jenkins (2009)) is surely the account put forth in Riker (1955), which casts the 17th Amendment as

[3]Interestingly, the amendment would have passed years before if not for some clever maneuvering by senators who opposed it. At the turn of the century, an amendment proposed by Senator Chauncey DePew (R-NY) to be attached to the legislation on direct elections opened up a second policy dimension that killed prospects of passage (in 1911 when one version of the legislation actually came to the floor Senator George Sutherland (R-UT) attempted a similar maneuver). Specifically, these killer amendments were interpreted by southerners as giving the federal government the authority to enforce voting rights in the South (Riker 1986; Poole and Rosenthal 1997).

an inconsequential change. Riker situates the 17th Amendment within a series of changes that made senators accountable to citizens rather than state legislatures: the authors of the Constitution had conceived of the Senate as a "peripheralizing" institution (meant to represent state interests); but, by the early 20th century, the Senate no longer fulfilled that role. Instead, state legislatures had failed to ensure that senators followed their instructions; shifts in electoral institutions (including the canvass and direct primaries before the 17th Amendment) combined to gut the peripheralizing features of the Senate.[4] In this view, the 17th Amendment represented the final blow in a series of reforms that had already made senators more accountable to citizens than to state legislatures. Other innovations, such as the canvass—whereby candidates for state legislature would be elected on the basis of their choice for senator—had already changed the nature of representation.

Recent research has upended parts of this interpretation, demonstrating that the 17th Amendment led to discernible changes in the Senate.[5] Crook and Hibbing (1997) argues that the composition and responsiveness of the Senate changed after the 17th Amendment. Lapinski (2000) suggests that the amendment changed committee tenure rates in the Senate. Poole and Rosenthal (1997) finds that rates of abstention were 3.4% greater in directly elected Senates, even after controlling for the level of abstention in the House. Schiller, Stewart, and Xiong (2013) examines the election of senators in state legislatures before the 17th Amendment and provides further evidence complicating Riker's interpretation. For example, state legislatures experienced considerable discord over whom to elect: between 1871 and 1913, 31% of all indirect elections required "joint ballots," in which the two chambers of the state legislature could not agree on a winner and went into joint assembly; in fact, there were nineteen absolute deadlocks that left a vacant Senate seat. States often had more candidates vying for a Senate seat than there were parties (demonstrating that

[4] I examine the effect of direct primaries as a robustness check in the main set of regressions and in Supplementary Appendix A.7.

[5] Why would senators give up their right to indirect elections in the first place? The answer appears to be a mix of popular pressure stemming from corruption in senatorial elections occurring in state legislatures along with the fact that some states had already made the switch to direct primaries (which acclimated some senators to electoral pressures).

intra-party conflict also often played a role), and election ultimately came after extensive negotiations and often included bribery (Schiller, Stewart, and Xiong 2013). Newspaper accounts also suggest that voters had little idea who would be their state's next senator, even after elections for state legislature (Schiller and Stewart 2011). In short, up until the 17th Amendment, the selection of senators was a far cry from direct, popular elections.[6]

As a result, the enactment of the 17th Amendment provides an opportunity to test whether theoretical predictions about representation square with empirical data. Gailmard and Jenkins (2009) provides an elegant test of the theory that the 17th Amendment changed the nature of representation in the Senate. The authors suggest that the change to direct elections should have increased the responsiveness of elected officials to the preferences of constituents while also allowing senators more discretion (since the newly empowered voters were likely less well-informed than state legislatures). The empirical evidence supports this theory: directly elected senators voted more in alignment with the average preferences of their constituents and less in alignment with their state representatives' preferences.[7] Looking at senator voting patterns within states, direct election led to more distance between the ideal points of senators from the same state—a result consistent with increased discretion.

The existing literature on the 17th Amendment examines how the change to direct elections altered behavior in the Senate, and it answers this question using available data on what occurred in state legislatures and in the U.S. Senate (i.e., state legislator voting patterns, senator DW-NOMINATE scores, abstention rates, etc.). But the literature has not investigated the full extent of the change in constituent/legislator relations because it has not evaluated shifts in the behavior of the constituents and organizers who lawmakers were

[6]To be clear, the shift from election by state legislatures to election by the public did not amount to a shift from no "responsiveness" to perfect "responsiveness," as Gailmard and Jenkins (2009) point out. Rather, an intermediary (i.e., state legislatures) was removed from the relationship between citizens and legislators. After the 17th Amendment many factors still impinged on this relationship, but by examining this change I focus on one particularly well-identified shift in electoral institutions where the timing and substance of the change is relatively clear.

[7]Gailmard and Jenkins (2009) uses the state-wide vote share for Republican presidential candidates as the measure of citizen policy preferences and the Republican seat share in state legislatures to measure the policy preferences of the state legislature.

meant to represent. Did behaviors such as communication with the Senate also adjust to institutional changes? A comprehensive evaluation of representation should also consider the importance of participation and engagement. The innovation of this chapter is to develop data tracking communication with Congress as well as a framework to test whether behavior changed along this dimension.

1.2 Empirical Approach

Although shifts in electoral institutions may influence a range of legislator and constituent behaviors, several challenges to inference hamper identifying these effects. First, changes in electoral institutions often have theoretical importance but marginal measurable impact. For instance, a state might revise a rule governing primary elections or tighten voter identification rules, but drawing conclusions about the influence of such a change is challenging when it occurs only in one state or affects a small subset of the voting population. Second, changes in such institutions often come in response to factors correlated with the very behaviors I hope to study.

The research design in this chapter takes advantage of a fundamental change in the conduct of general elections: specifically, the use of direct elections to select representatives where direct elections had not previously taken place. While the extent to which this shift in the conduct of elections was exogenous is arguable, there are several methods for dealing with this issue. First, when studying the effect of the change only within the Senate, I can take advantage of the fact that the implementation varies across states. Even though the 17th Amendment passed at a single point in time, not all senators immediately faced direct elections. One-third of states held direct general elections in the year after institution of the Amendment, but not until several years later had all members of Congress faced direct elections. Furthermore, the staggered rollout of direct elections did not occur in a strategic manner but rather was a direct consequence of the rules enacted in the Constitution.[8] When

[8]"Immediately after [Senators] shall be assembled in Consequence of the first Election, they shall be divided as equally as may be into three Classes. The Seats of the Senators of the first Class shall be vacated

the Senate first assembled, members of the Senate were divided into three classes, and, in order to determine who was in the first, second and third class, a number was drawn at random from a box. Senators elected in states that subsequently joined the Union were also assigned to classes randomly.[9] Thus, while the grouping of senators in the initial thirteen states was not necessarily random, the ordering of the classes (and therefore the timing of re-election) was random. For states that joined the Union after the initial thirteen, new senators had classes assigned randomly conditional on keeping the three classes balanced. This randomization in the timing of elections provides variation in election timing within the Senate that is plausibly orthogonal to observed and unobserved covariates that might influence petitioning.[10]

One strategy for estimating the effect of the change to direct elections on the intensity of citizen petitioning to the Senate relies on this variation. I estimate an equation of the form:

$$log(Petitions_{st}) = \alpha_s + \gamma_t + \alpha_s \times t + \beta Direct\ Election_{st} + \varepsilon_{st} \tag{1.1}$$

where $log(Petitions_{st})$ is the natural log of the total number of petitions sent to the Senate from a state s during Congress t, α_s captures state fixed-effects, γ_t captures time fixed effects

at the Expiration of the second Year, of the second Class at the Expiration of the fourth Year, and of the third Class at the Expiration of the sixth Year, so that one third may be chosen every second Year." See U.S. Const. art. I, §3.

[9]For example, when Arizona and New Mexico became states in 1912 the resolution (S. 274, 62nd Cong. (1912)) determining their terms provided for random assignment. It read: "*Resolved*, That the Secretary put two papers of equal size in each of two separate ballot boxes, and in each instance one of such papers shall be numbered 1 and the other shall be a blank. The Senators from the State of Arizona shall proceed to draw the papers from one of such ballot boxes, and the Senators from the State of New Mexico shall proceed to draw the papers from the other ballot box, proceeding to draw in the alphabetical order of their names."

[10]Several states implemented rules meant to give people the opportunity to vote for their Senators even before the 17th Amendment. Direct primaries allowed citizens to select a preferred candidate to stand for election before state legislatures. A handful of states used preference votes in which they selected a Senator with the idea that the state legislatures would then rubber-stamp their chosen candidate. In 1908, a Republican Oregon legislature did in fact elect a Democratic candidate chosen by Oregon voters. Still, at least in contemporary newspaper accounts, party control of the state legislature appears to have been viewed as the chief determinant for selecting a Senator. I explore the effects of direct primaries and preference votes in Supplementary Appendix A.7.

and $\alpha_s \times t$ captures state-specific time trends.[11] Under this specification, the identifying assumption is that the timing of the institution of direct elections is exogenous conditional on the other covariates. Differences across states that are time-invariant, differences across time and state-level differences that change linearly over time are all controlled for in this framework. I code $\textit{Direct Election}_{st}$ as a dummy variable indicating whether any of a state's senators were elected by direct election. As a robustness check, I also allow the "intensity" of the treatment to vary; I code $\textit{Direct Election}_{st}$ as the proportion of directly elected senators in a given state. Under both of these approaches, the coefficient β measures the shift in petitioning activity that occurred in line with the shift to direct elections.

The primary approach I employ in this chapter relies on variation in election timing while also utilizing the existence of a natural control group: the House of Representatives. Because the shift to direct elections occurred only in the Senate, I use petitioning directed at the House as an additional comparison group. The model estimated takes the form

$$\textit{log}(\textit{Petitions}_{ist}) = \alpha_s + \gamma_t + \alpha_s \times t + \beta \textit{Direct Election}_{ist} + \delta \textit{Office}_i \times t + \varepsilon_{ist} \tag{1.2}$$

where the conventions are the same as in Equation 1.1, except that i indexes the office of Congress and $\textit{Office}_i \times t$ controls for different levels of petitioning across the House and Senate as well as office-specific time trends. Here, the estimate relies upon the idea that petitioning to the House was subject to the same forces—including changes in policy climate, public opinion, political engagement and demographic trends—as petitioning to the Senate. Thus, after controlling for any difference in levels of petitioning between the two bodies of Congress, the comparison between petitioning to the Senate and petitioning to the House before and after the change to direct election is a valid one. This approach allows me to formulate $\textit{Direct Election}_{st}$ as a "sharp" treatment variable indicating the moment when the 17th Amendment was enacted.

[11] To be clear, $\alpha_s \times t$ represents a distinct linear time trend for each state. A small share of state-year combinations were zeroes; therefore, in practice I add one to the outcome variable before taking the natural log.

The specifications described above serve as a baseline, but I also explore alternative specifications omitting time trends, using regional time trends, relaxing the use of Congress fixed effects, and including a range of state level covariates such as personal income per capita, share of the state population that is female, non-white, or urban and farms per capita. Furthermore, I examine the robustness of the effect to the inclusion of controls for other Progressive Era reforms.

1.2.1 Data

This chapter relies on an original data set tracking all petitions sent to Congress since 1881. The full set of petitions I gathered, which spans until the present day, approaches half a million petitions. For the analysis at hand, I restrict the time range to 1881–1949. Within this timeframe, the data includes slightly over 370 thousand petitions. Figure A.1 in the Supplementary Appendix displays an example petition sent to the Senate in 1917 by the Anti-Suffrage Party of New York, protesting Women's Suffrage. The prayer of the petition, printed at the top of the page, lists the group's requests. The signatory list, at the bottom of the page, lists the names of the petitioners along with their addresses and service group memberships (i.e., Red Cross, National League for Women's Service, etc.).

The assembly of the data itself amounts to one of the key innovations in this project. I exploit the fact that petitions submitted to Congress are presented to the clerk by a member of Congress and referred to a committee.[12] Since the first Congress, these actions have been recorded in the official record of proceedings and debates. The *Congressional Record* records the member of Congress presenting the petition, descriptions of the petitioners, the topic of the petition, the geographic location of the petitioners and the committee to which the petition is referred (in rare cases, petitions are not referred to a committee and are instead "laid on the table"). For this chapter, I use the text of the *Congressional Record* since 1881 as the primary source material. This covers records for thousands of days of meetings of Congress. Sorting through so much text by hand was not feasible; instead, I wrote a program

[12] See Supplementary Appendix A.3 for a discussion of the rules for processing petitions.

that parses the text of the *Record* and systematically tracks petitions sent to Congress (including who presented the petition, the subject of the petition, the petitioners and their geographic location and the committee to which the petition was referred). Section A.4 in the Supplementary Appendix provides more technical details on the data gathering process and displays examples from the *Congressional Record*.

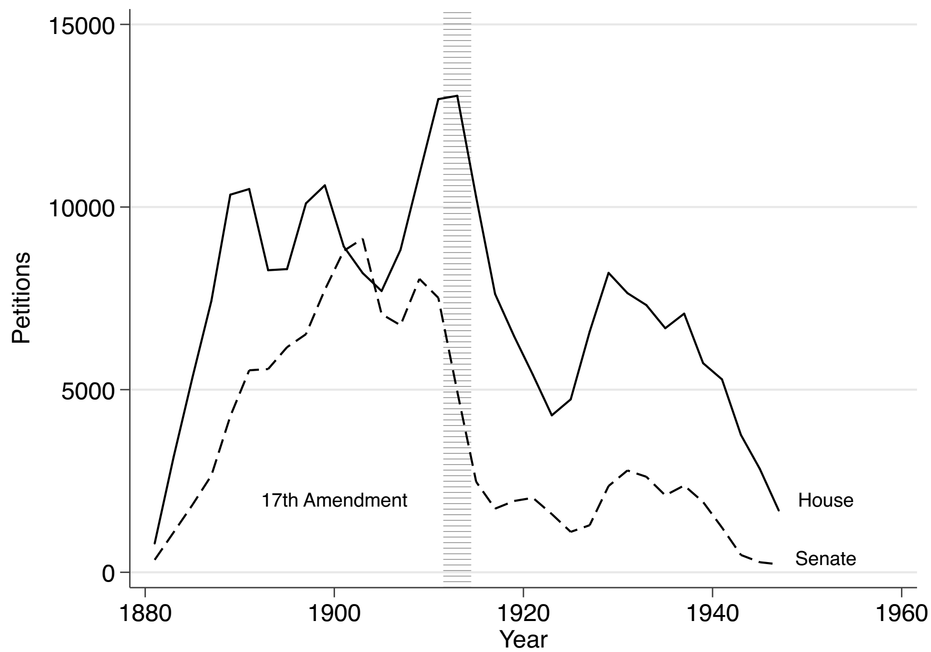

Figure 1.1: Petitioning the House and Senate Over Time. From 1881–1949, there has been considerable variation in the total number of petitions sent to Congress. From peaks at the turn of the century, petitioning steadily declined towards the end of the time period.

The petitioning data gathered provides the first definitive look at how petitioning activity has evolved over time (See Figure 1.1, which tracks petitions sent to Congress from 1881–1949). First, petitioning activity has declined in general since the early 20th century. Peaking at a height of many thousands of petitions per Congress, activity declined to a tiny fraction of this number by 1949. Several theories exist for why petitioning overall has declined in the 20th century, including the rise of formal lobbying groups and the increasing use of public opinion polls. Formalized lobbying gave groups of citizens and organizations another way to communicate their preferences and to shape policy in Congress. For instance, as early as the 1920s, the Farm Bureau gathered proprietary information to pressure members of Congress when important roll call votes approached: "If a congressman seemed to be wavering..., [a Farm Bureau lobbyist] did not hesitate to show him the poll results," which were "district-by-district tabulations of responses to public sentiment polls taken by county farm bureaus" (Hansen, 1991, p. 30; Howard, 1983, p. 131). By the mid 1930s, George Gallup began releasing results of public opinion polls. While lawmakers did not immediately embrace polling, over the next fifty years they began to utilize polls as a new way of ascertaining citizen preferences (Converse 1987). These factors, along with the increasing complexity of governing, all likely help explain the decline in petitioning. Importantly, this chapter purposefully avoids trying to explain the full decline in petitioning. Rather, the focus here will be identifying state-level breaks in petitioning activity that may be attributed to changes in electoral institutions. In this sense, changes in electoral institutions that affected petitioning activity serve as an additional reason for the decline of petitioning in the Senate. But given the overall decline it must only explain a part of the full decline.

A second stylized fact made clear from this newly gathered data is that the House consistently receives more petitions than the Senate. On a relative basis, Senate petitions comprised their greatest proportion of total petitions in the period from roughly 1890 to 1915. By this point, Senate petitioning begins a sharp decline followed by a slightly more gradual decline in House petitioning.[13] Several reasons appear to account for the consistently higher level of House petitions. First, parliamentary tradition originally emphasized petitioning the lower house of a bicameral legislature (Zaret 2000; Carpenter and Moore 2014), and petitioners in the United States picked up on this tradition. Second, the fact that most states have more representatives in the House than the Senate to receive and present petitions also likely explains part of the difference. For example, petitioning efforts directed at specific members of Congress, or originating at the the level of the congressional district, will be more numerous for the House than the Senate since the House has more rep-

[13] A key point: this graph does not allow for straightforward inferences about the effects of direct elections, which I estimate at the state level.

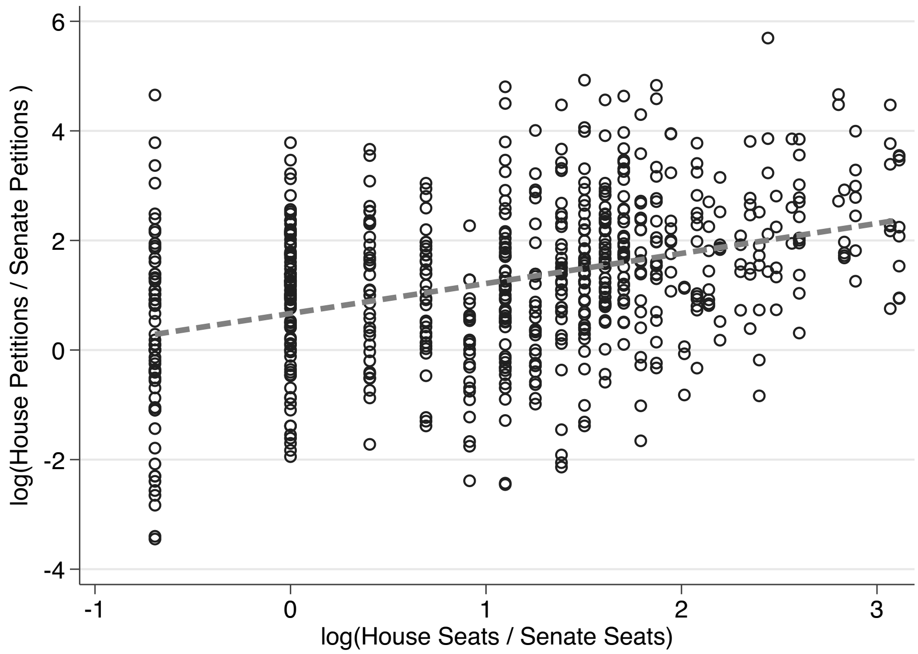

Figure 1.2: Ratio of House vs. Senate Petitions in a State to House vs. Senate Seats in a State, Post 17th Amendment. This figure illustrates that the natural rate of petitions sent to the House is higher than to the Senate, and that this difference is increasing in the ratio of House to Senate seats in Congress.

resentatives per state (with the notable exception of low population states with one House member). Figure 1.2 tests this proposition by comparing the ratio of House versus Senate petitions sent from a state in a given Congress to the ratio of the state's House versus Senate seats. The figure presents data only for the years after the ratification and implementation of the 17th Amendment to ensure that electoral institutions are the same across chambers. The figure illustrates a distinct upward slope—as the ratio of House seats to Senate seats increases, so too does the ratio of petitions sent to the House compared to the Senate.

Based on the information available, we should not expect equal rates of petitioning to the House versus the Senate, even when both have direct elections. Accordingly, to make inferences about the effects of changes in electoral institutions I compare relative changes in petitioning trends over time.

In addition to the petitioning data, I have gathered census and income data for each state. Using the decennial censuses (1880–1950), the data includes female population share, non-white population share, farms per capita and urban population share (where "urban" is a city with more than 25,000 people). Personal income per capita is gathered from the the Bureau of Economic Analysis (BEA) and *Population Redistribution and Economic Growth, United States 1870–1949* (Kuznets and Thomas 1964). Starting in 1929, the BEA gathered income data on a yearly basis for each state. Before this point, income data and all other state characteristics were generated using standard linear interpolation between census years.[14]

1.3 Results

1.3.1 Was Expression through Petitions a Substitute to Direct Elections?

What is the effect of a switch to direct elections on petitioning activity?[15] This section tests the prediction that, in the absence of direct elections, petitioning served as an alternative mechanism for communication.

As a first step in analyzing the effect, I compare levels of petitioning in the years immediately preceding this policy change to petitioning in the years immediately following the policy change.[16] I treat the enactment of the 17th Amendment as a sharp policy change. While this approach is more blunt than using the fully specified models and variation in Senate classes described in Section 1.2, it lends itself to a graphical representation of the immediate effects due to direct elections. For the House and Senate, I separately regress the logged level of state petitioning in each Congress on a set of state dummy variables and state-specific time trends. I then calculate the residuals for each observation in the data—in

[14]See `http://www.bea.gov` for further details on the income data.

[15]To be clear, I can rule out the notion that effects in either direction are due to changes in petitions specifically about direct election of senators. A search in the database for petitions that included the words "senator," "amendment" and "elect" returned under 150 petitions on the subject. This comprises less than a tenth of one percent of all petitions in the database.

[16]The 17th Amendment was formally adopted in May 1913.

this case, the residual is the remaining variation unexplained by location and time trends. Because the policy change directly affected Senate elections but not House elections, I use House petitioning as a baseline and calculate the relative trend in Senate petitioning. Figure 1.3 displays the average difference in residuals for House and Senate petitioning for the six Congresses before and after the policy change. The relative decline coinciding with the policy change should appear clear. All residuals before the policy change are positive; the residuals following the 17th Amendment are almost all negative.

To focus more closely on Senate petitioning, I take advantage of within Senate variation arising from the random assignment to Senate classes. Again, I observe a sharp decline in petitioning coinciding with when senators faced direct elections. There also appears to be a decline in the period immediately preceding the policy shift. This suggests that petitioners and senators might have begun to anticipate the effects of direct election with the passage of the 17th Amendment rather than upon experiencing direct Senate elections first hand—though if this were the case then the effects should be biased towards zero. If anticipation of direct election does account for the pattern in the data, then I can also observe the extent of the problem by comparing results from the sharp treatment to estimates using variation in election timing. In the next section, I try to exploit this empirical pattern to help refine my explanation of the observed effects.

Another concern is the existence of a downward pre-trend in Senate petitioning activity. I attempt to control for this possibility using petitions to the House. Given the importance of using House petitioning as a control, I compare whether there exist different pre-trends in petitioning across the House and the Senate. If trends in petitioning to the House and Senate differed markedly before the 17th Amendment, then a key assumption for using the House as a control would not be met. Figure 1.4 illustrates the pre-trends in petitioning. The plot shows residual log(Petitions) leading up to the enactment of the 17th Amendment in 1913. In this case, petitioning to the House and to the Senate moved largely in parallel. In sum, while there is some evidence of anticipation when looking only at the Senate, the House appears to serve as a valid control in the years before enactment of the 17th Amendment.

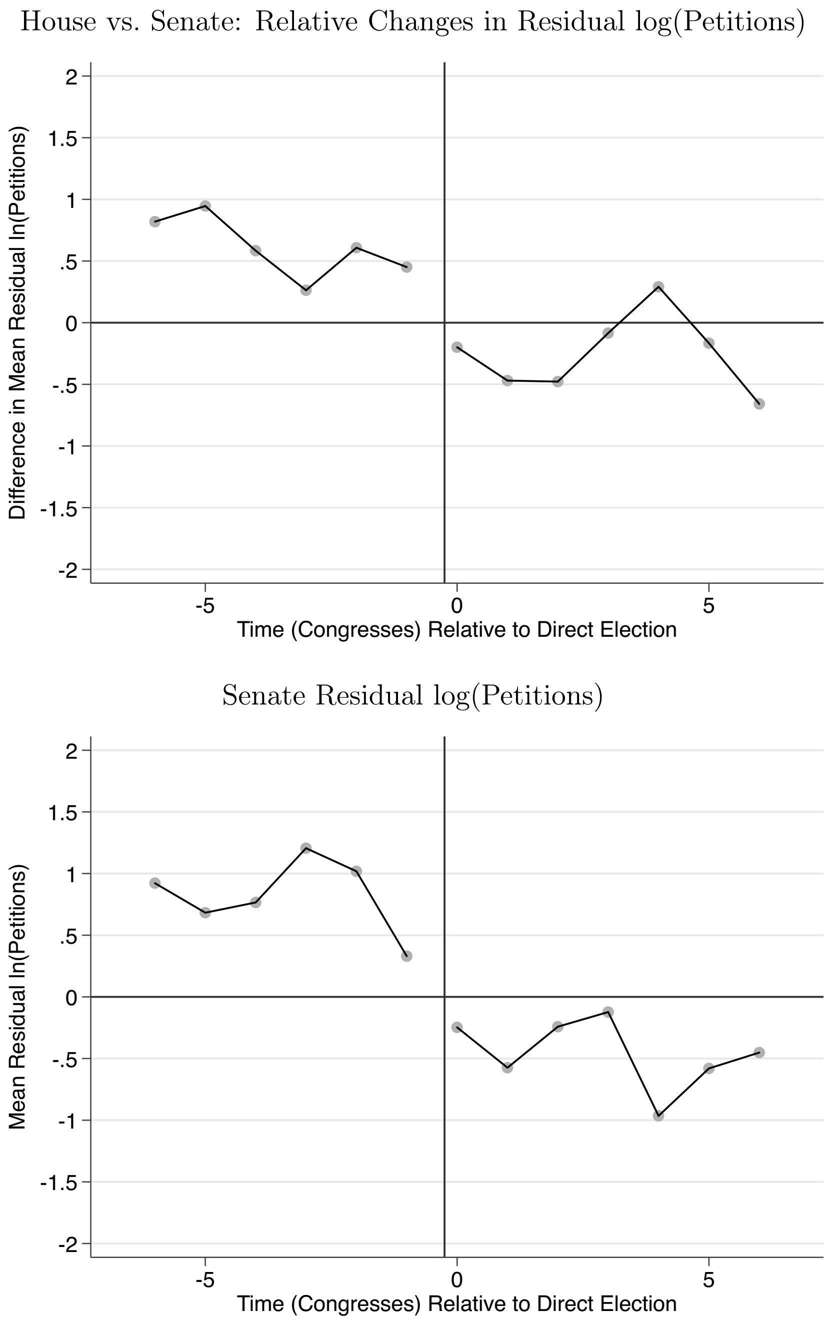

Figure 1.3: Shifts in Congressional Petitioning and Direct Election. This figure illustrates the change in petitioning before and after direct election, when controlling for differences across states and state-specific time trends. The first plot tracks the relative change in petitions sent to the Senate vs. the House before and after the 17th Amendment. The second plot tracks the change in petitions sent to the Senate after a state's senator was directly elected (i.e., using differences in election timing as a source of variation).

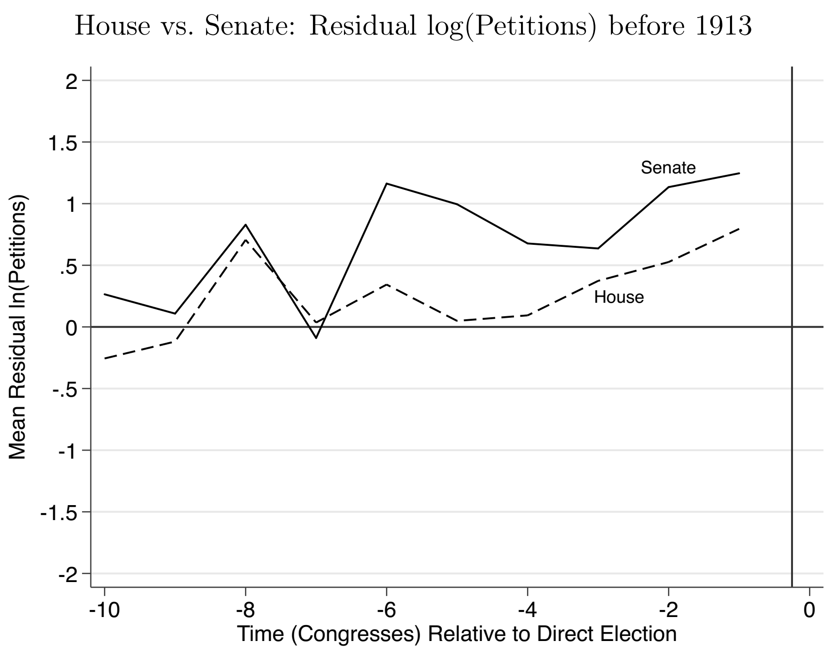

Figure 1.4: Pre-Trends in Residual Petitioning in the House and Senate. This figure plots pre-trends in petitioning activity to the House and the Senate, when controlling for differences across states and state-specific time trends. The plot tracks the trends before 1913 for each state.

The rest of this section examines the results of the models discussed in Section 1.2 and explores a series of different specifications and further robustness checks. Table 1.1 reports the core results. Panel A provides estimates for the full model and looks at shifts in petitioning for both the House and the Senate. Panel B reports estimates looking only within the Senate. Panel C reports the estimates for House petitioning using a "placebo" treatment—testing whether the switch to direct election, which only affected Senate elections, changed the level of petitioning to the House.

Panel A shows that the switch to direct elections in the Senate had a strong negative effect on Senate petitioning; the effect persists whether coded as a sharp or binary treatment. Because of the log dependent variable, the coefficients reported provide a rough estimate of the percentage change in petitioning at the state level after the shift to direct elections. For an estimate of the exact change, transform the coefficient by $exp(\hat{\beta} - \frac{1}{2}\hat{V}(\hat{\beta})) - 1$ (Kennedy 1981).[17] For example, I report an estimate of -0.475 for the direct election binary treatment in Panel A. This coefficient corresponds to a -38.02% decline. Across the different implementations, the results suggest a more than 30% decline in petitioning attributable to the shift to direct election. Given that the mean state sent more than one hundred petitions to the Senate per Congress before direct elections, these effect sizes are substantively meaningful.

Panels B and C also suggest that expression of preferences through petitioning substituted for expression of preferences through direct elections. The switch to direct election coincided with a decrease in petitions sent to the Senate. However, for the House no meaningful decline in petitioning occurred. Clearly, an institutional change in Senate elections should have minimal effects on behavior related to the House. If the placebo estimate for the House was in fact distinguishable from zero, then that might suggest that the empirical strategy did not adequately account for the general decline in petitioning at the end of the

[17] Interpreting the coefficient for a dummy independent variable D in a regression with a log dependent variable is not as straightforward as it might appear. A discrete change from 0 to 1 suggests that the percentage change can be found by $\frac{Y_{D=1} - Y_{D=0}}{Y_{D=0}} = \frac{Y_{D=1}}{Y_{D=0}} - 1 = exp(\hat{\beta}) - 1$. But the non-linear transformation of the estimate $\hat{\beta}$ can lead to bias in calculation of the percentage change. van Garderen and Shah (2002) discusses this in more detail and derives an exact minimum variance unbiased estimator.

Progressive Era.

Table 1.1: The Effect of Direct Elections on Congressional Petitioning

	17th Amendment Sharp Treatment	Direct Election Binary Treatment
Panel A: House and Senate		
Estimate	-0.516***	-0.475***
	(0.088)	(0.082)
DV Untreated Mean	131.695	126.746
Δ in Petitions	-40.54%	-38.02%
Panel B: Senate Only		
Estimate		-0.387*
		(0.205)
DV Untreated Mean		126.746
Δ in Petitions		-33.50%
Panel C: House Only (Placebo)		
Estimate		0.092
		(0.192)
DV Untreated Mean		198.395
Δ in Petitions		7.63%

Standard errors in parentheses

Standard Errors clustered at state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports results for a pooled regression using the House as a control group for the Senate, for just the Senate, and for just the House. The results are reported for the primary specification—state and Congress fixed effects with state-specific time trends—and a variety of other specifications are reported in the Supplementary Appendix. The first column contains estimates when the treatment is the enactment of 17th Amendment (this occurs at the same time for all states and so does not lead to any within chamber variation). The second column reports results for a binary treatment in which states with at least one directly elected senator are considered treated. The table also reports the mean number of petitions sent to the Senate by untreated states, as well as the percentage change in petitioning caused by the switch to direct elections.

Table 1.2 explores the robustness of the results across a range of alternative specifications. The first specification is the sparsest one employed, including only state and Congress fixed effects and regional time trends. In this case, the Congress fixed effects control for idiosyncratic changes influencing petitioning across all states. For example, the outbreak of World War I might result in a shock to petitioning across all states. The inclusion of regional time trends represents an attempt to account for fundamental differences in the political environment in the South. Specification 2 corresponds to the full model described in the previous section. Specification 3 includes an additional set of control variables, including income per capita, non-white population share, female population share, farms per capita and urban population share. Finally, the last specification controls for other landmark Progressive Era reforms (the secret ballot, direct primary and women's suffrage) to ensure that the observed effect is not attributable to other policy changes that occurred in the era.[18]

All told, the effect is consistent across specifications as well as implementations of the treatment. The results suggest a more than 30% decline in petitioning attributable to the shift to direct election—evidence that petitioning operated as a substitute form of political expression in the absence of direct elections. These results are robust to allowing for a sharp treatment, a binary treatment based on first election year and varying treatment intensity (see Table 1.2 rows 1–3).

Another question concerns how to treat territories that became states after 1881. Arizona, Idaho, Montana, New Mexico, North Dakota, Oklahoma, South Dakota, Utah, Washington and Wyoming had not gained statehood by 1881 and did so at various points in the time period studied. Furthermore, the question of which territories became states (or "rotten boroughs") was itself a contentious political decision (Stewart and Weingast 1992). Row 4 in Table 1.2 presents results for a series of regressions that entirely exclude states that had ever been territories after 1881 from the sample. Again, the results remain stable.

The robustness checks so far have pooled petitions sent to the House and the Senate, using the House petitions as a control group. I also examine each chamber of Congress separately under the full set of alternative specifications. Rather than using the House as a control group in the same regression, I rely on variation in the first direct election date

[18] There is considerable variation in the timing of these reforms across states. For instance, Indiana instituted a secret ballot as early as 1889 whereas states such as Tennessee instituted the secret ballot in 1921 (and South Carolina waited until 1949). For women's suffrage, western and plains states implemented the reforms very early (Wyoming in 1869, Utah in 1870, Colorado in 1893 whereas many eastern states waited until the passage of the 19th Amendment in 1920. See Section A.7 in the Supplementary Appendix for a more detailed discussion of the estimated effects of these reforms.

within the Senate. Then, I estimate an identical regression for the House, using the dates of first direct election as a "placebo" treatment.

Row 5 in Table 1.2 presents results for the model estimated only within the Senate. The coefficient for the effect of direct elections is negative and distinguishable from zero in all four specifications. The switch to direct election coincided with a reduction in petitioning to the Senate. Row 6 presents results for the model estimated only within the House—using the "placebo" direct election variable as the treatment. The coefficient estimates in this case do not demonstrate the same effect seen in the Senate. The effect in the House is relatively close to zero.

All told, the findings of a null effect for the House and a negative effect for the Senate square well with theoretical predictions. The 17th Amendment changed the way that citizens engaged with their elected officials in the Senate, while having no impact for the House. The approach taken also helps insulate against the critique that many other, concurrent shifts occurred at a similar time to the passage of the 17th Amendment. Changes in technology that allowed for easier travel back to the district, other Progressive Era reforms like women's suffrage and prohibition—all of these should have affected the representational linkages between constituent and representative in *both* House and Senate. But the effect of direct elections holds only in the Senate, which suggests that spurious correlation does not drive these effects.

Table 1.2: Robustness Checks: The Effect of Direct Elections on Congressional Petitioning

		log(Petitions)			
		(1)	(2)	(3)	(4)
Panel A: House & Senate					
(1)	Sharp Treatment	-0.516***	-0.516***	-0.516***	-0.516***
	N = 3106	(0.087)	(0.088)	(0.088)	(0.088)
(2)	First Election Treatment	-0.475***	-0.475***	-0.475***	-0.476***
	N = 3106	(0.081)	(0.082)	(0.082)	(0.082)
(3)	Varying Treatment Intensity	-0.447***	-0.447***	-0.450***	-0.451***
	N = 3106	(0.083)	(0.084)	(0.084)	(0.084)
(4)	Omit if Ever Territory	-0.482***	-0.482***	-0.481***	-0.482***
	N = 2584	(0.090)	(0.091)	(0.091)	(0.091)
Panel B: Individual Chambers					
(5)	Senate Only	-0.367*	-0.387*	-0.408*	-0.401*
	N = 1553	(0.215)	(0.205)	(0.205)	(0.207)
(6)	House Only	0.050	0.092	0.078	0.058
	N = 1553	(0.176)	(0.192)	(0.188)	(0.190)
	Congress FEs	Yes	Yes	Yes	Yes
	State FEs	Yes	Yes	Yes	Yes
	Regional Time Trends	Yes	No	No	No
	State Time Trends	No	Yes	Yes	Yes
	Demographic Controls	No	No	Yes	Yes
	Other Reform Controls	No	No	No	Yes

Standard errors in parentheses

Standard Errors clustered at state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table presents a series of checks testing the robustness of the effects of direct election on petitioning. Each column corresponds to a different model specification. The first includes time and state fixed effects and regional time trends (South, Midwest, etc.). The second specification uses state-specific time trends rather than regional time trends. The third specification adds a set of control variables: income per capita, non-white population share, female population share, farms per capita and urban population share. The fourth specification adds controls for other Progressive Era reforms: the secret ballot, direct primary and women's suffrage. Each row corresponds to a different implementation or approach. Of particular note is row 4, which omits all states that spent any time as a territory after 1881. Row 5 estimates results using the first election treatment only within the Senate. Row 6 takes the same approach but for the House—i.e., here I estimate a "placebo" effect as no reforms affecting the House actually occurred.

1.3.2 Did Direct Election Lead to Changes in the Content of Petitions or the Characteristics of Petitioners?

The results so far suggest that the 17th Amendment led to decreased petitioning to the Senate. Next, I trace the differential impact of this electoral reform by tracking shifts in the character of petitioning requests. Specifically, *do I observe substantive changes in what was asked for and whose interests were represented through petitioning?* I evaluate this question by tracking changes in the topics expressed by petitioners. Examining petitioning across this dimension in conjunction with the results presented in the previous section allows me to characterize how sensitive different petitioners were to electoral reforms.

Committees

Information on committee referrals provides clues about the nature of a given petition. For example, the Committee on Appropriations received petitions requesting benefits while the Committee on the Judiciary received petitions to change or revise laws. As a result, committee referrals reveal, in broad strokes, categories of petitioning requests.

Figure A.2 in the Supplementary Appendix displays the relative share of petitions referred to eight different Senate committees (petitions referred to other committees or laid on the table are not included in this graphic). The figure reveals considerable variation over time. Petitions to the Committee on the Judiciary comprised a small initial share but ballooned during the Progressive Era; petitions to the Committee on Finance made up a substantial relative share throughout; petitions for pension relief dwindled to nothing as Congress created a pension law making direct requests for relief unnecessary.

To see whether the switch to direct elections coincided with changes in the content of petitions, I test explicitly for shifts in the level of petitions referred to each Senate committee. Figure 1.5 presents estimates of the effect of direct elections for a range of important Senate committees: Agriculture, Appropriations, Commerce, Education and Labor, Finance, Foreign Relations, Judiciary and Pensions/War Claims. I report results for the time period

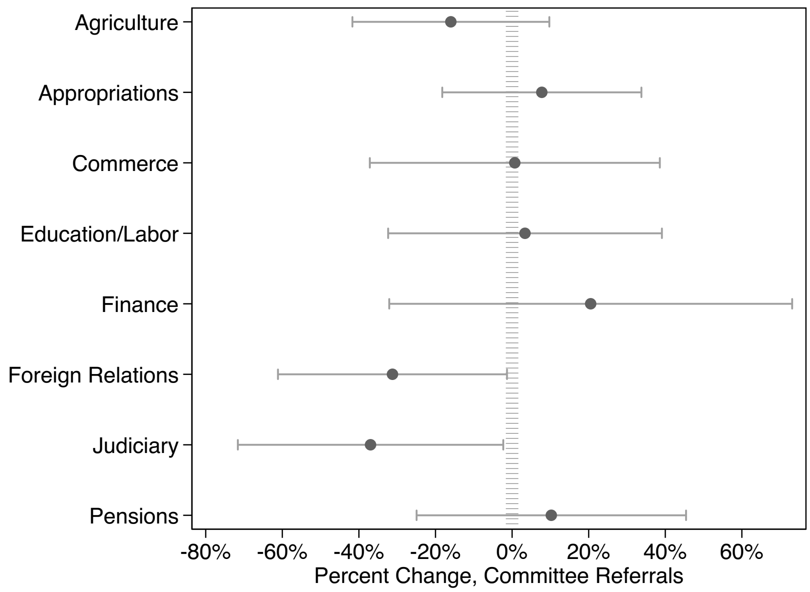

Figure 1.5: The Effect of Direct Elections on Petitions to Senate Committees. This figure displays estimates of the decline in petitioning by Senate committee. Here I restrict the sample to 1900–1930 and include state fixed effects and state specific time trends as controls.

between 1900–1930 to try to make reasonable within committee comparisons across time.[19]

For the subset of committees I examine, the negative effect of direct elections on petitioning appears primarily driven by changes in Judiciary, Foreign Relations, and Agriculture. This finding suggests that the decline in aggregate petitioning after direct election to the Senate is not the product of a change in the level of petitioning of just one type (for example, war claims), but rather occurred across several different types of requests. We might predict a decline across requests for particularistic benefits as well as policy instructions; however, in this admittedly imperfect case study I do not observe a strong negative effect for petitions requesting particularistic benefits. Petitions referred to Appropriations slightly increased after the switch to direct election. I also observe no decline in petitions referred to committees on pensions and other war claims. The null result for these types of particularistic requests is tentative because committee referrals are a noisy proxy for the type of request; however, it does point towards the possibility that the substitution effect I have posited

[19]Table A.2 in the Supplementary Appendix displays the exact point estimates.

holds more strongly for ideological issues than for requests for infrastructure, pensions and other selective benefits.

Committees where petitioners gave policy "instructions" saw the largest declines after the switch to direct elections. Judiciary, which received the bulk of petitions advocating for or against several key Progressive Era reforms (i.e., prohibition, suffrage, etc.), declined considerably. Even if accomplishing several Progressive Era goals explains part of the observed decline in petitions sent to the Committee on the Judiciary, it by no means explains all of the variation. For instance, re-estimating the effect of direct elections on petitioning activity minus all petitions sent to the Committees on the Judiciary in the House and Senate still yields a point estimate of -0.34 with a p-value of 0.001.

There are several explanations for the declines overall and by topic observed here. For instance, as constituents and senators became connected by direct elections, lawmakers became more responsive to mass preferences but also gained discretion in how to deal with issue-specific requests. Both of these shifts, brought about by the 17th Amendment, could generate a decline in petitioning. In the first case, candidates would have became more attuned to ascertaining mass preferences in other ways (such as through campaigning). In the second case, groups would have observed that directly elected senators were less responsive to issue-specific requests and adjusted their behavior by petitioning less. Distinguishing between these explanations is the topic of the next section.

1.3.3 Explaining the Decline in Petitioning After the 17th Amendment

What was the underlying cause of the change in levels of petitioning? I evaluate two competing possibilities. The first possibility is that direct elections provided a substitute set of opportunities to communicate with a senator, in turn reducing the need for more formalized requests from constituents. The second possibility is that direct elections (and the switch from a constituency of informed state legislators to an overall less informed set of general election voters) granted senators more discretion over what issues merited their attention. In turn, petitioners would then believe it less likely that an elected senator would respond to their concerns, unless the petitioners could show that their views corresponded with mass sentiment. This second explanation is similar in spirit to the theory in Gailmard and Jenkins (2009) that the 17th Amendment made senators more responsive to mass preferences while also granting them more discretion; however, it traces the implications beyond roll call votes to the handling of issue-specific requests made in petitions and to the behavior of petitioners.

These different explanations, while not mutually exclusive, do have potentially different implications for representation. If increased discretion (and a lack of responsiveness to issue-specific requests) prompted the decline in petitioning, then the 17th Amendment did indeed diminish a form of representation previously secured through petitioning. On the other hand, if the decline in petitioning resulted predominantly from a switch to alternative channels, then it did not necessarily result in diminished representation for organized groups and minority voices. Rather, it amounted to a shift in the means of communication. I attempt to distinguish between these explanations using several empirical tests that evaluate the relationship between petitioning and changes in discretion due to the 17th Amendment.

Petitioning Individual Senators

I examine the number of petitions sent to senators during the 62nd through 66th Congresses—which includes a brief moment (the 64th and 65th Congress) when directly elected senators served alongside the indirectly elected. I count the number of petitions each senator presented to the floor that were sent from their home state. This data allows for a simple hypothesis test evaluating whether directly elected senators received fewer petitions than their indirectly elected counterparts from the same state. I also test the hypothesis that fewer petitions were presented before direct election versus after.

If a state's citizens immediately stopped petitioning their directly elected senator but continued petitioning their indirectly elected senator, it would suggest that campaigning required candidates to immediately develop a process of ongoing communication and listening that displaced petitioning. On the other hand, if I do not observe within state differences but rather differences across congresses, then petitioning likely declined due to a more gradual change in beliefs among petitioners (as the new incentives for directly elected senators came into focus).

Table 1.3: Average Number of Petitions Presented by Senate Sponsors, 62nd – 66th Congress

	Home State Petitions		Total Petitions	
	Indirect Elect	Direct Elect	Indirect Elect	Direct Elect
64th Congress	24.9	24.0	29.4	27.3
65th Congress	9.2	13.2	11.9	17.5
Within States, 64th–65th	14.3	16.7	17.2	20.42
Across Congresses, 62nd–66th	67.8	21.3	82.7	29.6

This table presents the average number of petitions sent from a senator's home state and overall that were then presented to the floor by a senator. For the 64th and 65th Congress, it compares the number of petitions presented by senators from states with one directly elected and one indirectly elected lawmaker.

Table 1.3 presents the average number of petitions sent to a member of the Senate. I report the number of petitions sent from the home state and overall. I include totals for the 64th and 65th Congress. I also break out the number of petitions sent to directly elected versus indirectly elected senators in cases where a state had one directly and one indirectly elected senator. Finally, I pool across the 62nd through 66th Congress and compare the number of petitions presented by directly elected versus indirectly elected senators overall. The data reveals two key points. Pooling across the 64th and 65th Congress and including only cases where a state had one directly and one indirectly elected senator, the null hypothesis of an equal number of petitions for both classes of senator cannot be rejected.[20] On the other hand, the null hypothesis of no difference across all five congresses can be rejected. These results do not suggest an immediate, candidate-level drop in petitioning in the way one would expect if the process of the campaign immediately provided ongoing communication and listening that replaced petitioning. Instead, this empirical evidence accords with petitioners adjusting behavior over several congresses after the triggering event of having

[20]Note there were some states in the 64th Congress for which neither senator had yet run in a direct election.

a directly elected senator. This result appears more consistent with petitioners observing increased discretion among directly elected senators and reacting by adjusting their level of petitioning.

Membership Associations

If petitioners observed greater discretion on the part of directly elected senators and responded by petitioning less, then the most sophisticated and strategic groups should have shifted their behavior in accordance with this notion. To test this, I examine patterns of petitioning among membership groups and associations before and after the 17th Amendment. Because of the organizational costs of petitioning, membership associations—which provided a ready-built infrastructure and network for gathering signatures—held a considerable advantage in organizing petitioning campaigns. In fact, petitions from membership organizations comprised a substantial share of all petitions submitted to Congress. In order to estimate a lower bound on petitioning by these organizations (as well as how direct elections affected these groups), I used the list compiled in Skocpol, Ganz, and Munson (2000) of organizations that had memberships comprising 1% of the population or greater at some point in their history. These include groups such as the "Ancient and Accepted Free Masons", "Independent Order of Odd Fellows", "Knights of Pythias", "Patrons of Husbandry" and "Woman's Christian Temperance Union"—close to fifty groups in all.

Figure 1.6 displays the share of all petitions submitted by these membership organizations over time. On average, these civic organizations sent roughly 5% to 10% of all petitions submitted. The figure clarifies several points. First, the petitioning activity appears in bursts or spikes, suggesting membership groups engaged in coordinated campaigns or responded to issues all at once. Second, while the share of all petitions submitted to the Senate was greater than the share of petitions submitted to the House before direct elections, the lines essentially converge (with the exception of two spikes in the House) after the switch to direct elections in the Senate. In fact, I estimate that the share of petitions sent by membership groups to the Senate declined by 5% to 6% due to the switch to direct

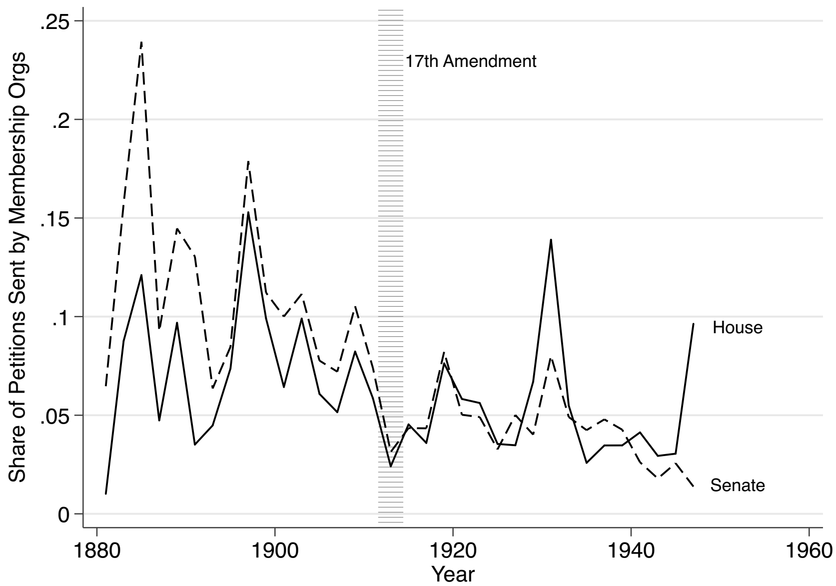

Figure 1.6: Share of Petitions Sent by Membership Associations. This figure displays the share of petitions sent to the House and the Senate by large membership associations. To determine the share of petitions sent by civic organizations, I used the list compiled in Table 1 of Skocpol, Ganz, and Munson (2000), which lists "large" membership associations, in which more than 1% of the population was at some point enrolled. These include groups such as the "Ancient and Accepted Free Masons", "Independent Order of Odd Fellows", "Knights of Pythias", "Patrons of Husbandry", "Woman's Christian Temperance Union", etc. For a full list, see Skocpol, Ganz, and Munson (2000).

elections (see Table A.4 in the Supplementary Appendix). Membership groups—which built civic skills among members (Verba, Schlozman, and Brady 1995) and had above average political resources and sophistication—sent a disproportionate share of petitions to the Senate before the 17th Amendment but not afterwards.

This finding suggests that the 17th Amendment rendered Senate petitioning by membership groups less useful politically. With issue-specific requests at the core of their mission, membership groups would have been most sensitive to perceived changes in the discretion of senators. The fact they exhibited an outsized decline in petitioning the Senate thus appears consistent with petitioners from membership groups and associations observing greater discretion on the part of directly elected senators and responding by petitioning less extensively.[21]

Concentration of Petitioning Across Committees

A final empirical result consistent with the theory that increased discretion among directly elected senators discouraged petitioning among a range of issue groups is that the switch to direct election corresponded with a narrowing in the range of issues that petitioners addressed in their petitions. I construct an index of the concentration of petitions sent to committees (see Appendix A.5 for details) such that an increase in the concentration of petitions within just a few committees corresponds to a higher index value. Table A.5 presents the main result. The switch to direct election coincided with a moderate increase in the concentration of petitions within fewer committees.

In line with previous evidence, this result appears consistent with minority groups anticipating that senators had greater discretion to ignore issue-specific requests. An overall decline in petitioning could occur for multiple reasons; however, the fact that petitioners sent petitions on a narrower range of topics aligns exactly with the theory that increased discretion among senators diminished representation of issue-specific requests.

[21]Membership in such groups also appears to have stayed stable at least through the 1920s and 1930s, so a decline in membership does not explain either the relative decline or the overall decline observed in the data.

The empirical tests put forth are all generally consistent with petitioners reducing political activity after the 17th Amendment because of increased discretion on the part of Senators. The adjustment in petitioning triggered by the 17th Amendment and direct election appears to have taken place over several Congresses. Membership groups, which make issue specific requests, sent fewer petitions to the Senate after direct elections. The scope of topics addressed by petitioners narrowed after the 17th Amendment. In addition, in results I present in Appendix A.6, it appears that high levels of petitioning were associated with less shirking (which I argue was correlated with discretion) before the 17th Amendment but not after. Taken together, a significant share of the reduction in petitioning activity appears to have occurred because canvassers felt that representation through petitioning was diminished under direct election. While surely elections facilitated communication to some degree, the empirical evidence demonstrates that the decline in petitioning was not merely a case of switching to a new means of communication without any consequences for political representation.

1.4 Conclusion

The rise and fall of congressional petitioning represents a hitherto unexplored puzzle for scholars of American politics. This chapter has linked this puzzle to broader questions about the effects of democratizing reforms on engagement and communication. Why did citizens petition at unprecedented rates during the 19th and beginning of the 20th centuries? What accounts for the precipitous decline in petitioning activity by the middle of the 20th century? I have assembled a record of petitions sent to Congress since 1881—a vast repository tracking collective political behavior—in an effort to understand the relationship between electoral reforms and citizen communication through petitioning. My findings suggest that petitioning served as an alternative mechanism to elections for communicating constituent policy preferences to representatives. Petitioning to the Senate declined when electoral institutions shifted in a manner that made senators more responsive to the mass public but also granted them greater discretion. Constituents who had sought representation through direct contact adjusted their political behavior by petitioning less. Institutional changes directly affected the behavior not only of members of Congress but also of constituents. Citizens now related to their representatives differently and this shift had at least one observable implication for their political behavior—a decline in rates of petitioning of more than 30%. The shift to direct elections does not explain the full extent of the decline in petitioning, but for the Senate it played a meaningful role.

The evidence put forth in this chapter may also add nuance to evaluations of democratic reforms. Progressive Era reforms may have led to "more democracy" by removing barriers between constituents and representatives; but these very reforms also diminished the vitality of previously flourishing forms of collective political activity. While it is well known that the extension of the franchise to women in 1920 led to a reduction in the previously thriving activity of women's groups, the possibility that the 17th Amendment had a similar effect has not been explored to my knowledge.[22]

These reforms have repercussions for representation in part because they alter the behavior of policy demanders, such as the canvassers who expended the time and effort to send petitions to Congress. I document not only a decline in the volume of petitions but also a change in their character. Organized groups petitioned the Senate less after the 17th Amendment; policy instructions also appear to have declined. Importantly, the explanation for these declines appears consistent with senators gaining more discretion and, in turn, issue groups reducing their petitioning activity directed at the Senate. To my knowledge, other work on reforms such as the 17th Amendment has focused almost entirely on the responses within Congress, while not considering the extent to which citizen behaviors also adjusted. Given the large shifts in communication I observe, a close consideration of the mechanisms available for transmitting preferences could be relevant in studies of representation that

[22]For example, Skocpol (1995) notes that "Exclusion from the suffrage for most American women [...] stimulated collective consciousness and counter-organization outside of the parties and regular electoral politics." After women were formally included in the political process, there was a "move toward accommodation with standard political routines" (Skocpol 1995, p. 52).

correlate measures of public opinion with Congressional voting records or policy output.

Finally, this project provides the basis for several future inquiries. Petitions comprise a promising source for studying political behavior in the era before survey measures of opinion and political activity existed. For example, future work could look to petitions to gain new insights into the struggles over other Progressive Era reforms such as women's suffrage. Another fruitful line of research would be to link petitioning requests directly to the introduction of new legislation, allowing for direct tests of hypotheses about representation. More broadly, what role have other alternative forms of political expression played in achieving desired policy outcomes? As the technical tools and availability of data tracking actual political behaviors proliferate, there appear to be promising opportunities to re-focus the study of political behavior on new behaviors—to move beyond elections and public opinion.

2 Does Electoral Competitiveness Increase Turnout? Evidence from a National Sample of 2 Million Voters

2.1 Introduction

Participation in elections is considered a primary indicator of democratic performance (Powell 1982). In a given election year, the extent of voter turnout has implications for whose views are represented (Fowler 2013), for which party wins and retains office (Nagel and McNulty 1996), and even for future levels of political participation (Meredith 2009).

Conventional wisdom holds that one of the most reliable ways to raise voter turnout is through increasing electoral competition (Wattenberg 2002). Enos and Fowler (2014) notes that of 70 papers examining turnout published in top political science journals since 1980, 41 mention the importance of competitiveness.[1] In empirical papers that have explicitly documented the relationship between the competitiveness of election outcomes and turnout rates, the implication is that more competitive elections *cause* citizens to vote at higher rates. This interpretation does not fully accord with the canonical rational-choice model of voter behavior (Downs 1957; Riker and Ordeshook 1968). On the one hand, the theory states that the expected benefits of casting a vote are increasing in the odds of casting the pivotal vote; on the other hand, in any election the probability of ever casting a pivotal vote approaches zero, regardless of competitiveness. To explain why citizens exhibit increased turnout rates in close elections, even as their probability of casting a pivotal vote remains near zero,

[1] Throughout the chapter we will use the terms "competitiveness" and "closeness" interchangeably.

scholars have pointed instead to the role of elite mobilization: "[C]loseness stimulates elite effort, elite effort stimulates turnout" (Cox and Munger 1989).

In this chapter, we move to resolve the puzzle over what theory best explains the relationship between elections and turnout by showing that no such relationship exists in the first place. Using data from U.S. House elections between 2008 and 2014, we demonstrate that, in past analyses, competitive districts are not comparable to non-competitive districts across a number of dimensions also related to turnout rates. As a result, past comparisons in turnout between competitive and non-competitive districts have been plagued by bias stemming from different distributions of observable covariates as well as unobserved confounding. By utilizing a unique, individual-level panel of turnout records for over a million voters compiled from state voter files, we provide estimates for the effect of competitiveness on turnout that are the most credible to date.

Our approach offers several key advantages when compared to past efforts at measuring the relationship between electoral competitiveness and turnout. First, we observe the choice to vote at the individual level and over time. The richness of the panel data we use in this study means we do not have to rely on biased self-reports of turnout from surveys. It also means we observe how individual behavior changes over time under different levels of competition, rather than being constrained to a single snapshot of voter turnout in the cross-section. Second, we exploit the 2012 redistricting cycle as a shock to the level of competitiveness for voters' congressional districts, ensuring that some voters experience more competitive districts and some experience less competitive districts relative to pre-redistricting. Third, the large size of our sample yields some of the most precise estimates to date of the effect of competitiveness on turnout. When taking advantage of within person and over time variation in competitiveness, we find the effects are very near to a precise zero, and due to the millions of voter records employed in the study we can discard the possibility that the null effects we estimate are due to noise. The null result remains robust under a variety of measures of competitiveness, under a range of sample restrictions (e.g., restricting to only midterm elections), and when explicitly dealing with observed covariate imbalances (through matching) and time-invariant unobserved confounders (through a difference in differences style approach).

We also offer an explanation for why theories traditionally employed to explain the relationship between electoral competitiveness and turnout (i.e., instrumental/rational voter theories and elite mobilization theories) do not apply in the context of close congressional elections. Drawing on survey results, we note that voters are generally unaware of the closeness of congressional elections in their districts, rendering instrumental theories not relevant for explaining links between competition and turnout. Additionally, using a panel of Cooperative Congressional Election Study (CCES) survey respondents, we find that individuals report only small increases in campaign contact when situated in more competitive congressional districts. This small increase in campaign contact, coupled with past findings on the (relatively small) effects of campaign contact on turnout, leads to minimal increases in turnout due to this form of elite mobilization. Close elections may spur elite efforts at campaign mobilization, but these efforts do not have a meaningful effect on overall turnout in congressional elections.

Our findings illustrate that the link between electoral competition and participation has been overstated. Electoral competition's other salutary benefits may well remain in place—pushing office-seekers to appeal to the median rather than the extremes of the electorate (Downs 1957), maintaining responsiveness of officials in office (Ansolabehere, Brady, and Fiorina 1992), increasing the odds of mixed partisan control of government (Fraga and Hersh n.d.), to name but a few—however, increased turnout should no longer be included in this list.

2.2 Literature Review

The standard model of rational voter participation predicts that citizens only cast a vote when:

$$PB + D > C \tag{2.1}$$

i.e., when the the psychic benefits of voting (D) plus the probability of being decisive (P) multiplied by the benefit accrued from the election of the preferred candidate (B) surpass the costs associated with voting (C), e.g., travel time, waiting in line, etc. (Riker and Ordeshook 1968). This theoretical framework has led to heavy scholarly attention to changes in the P term, i.e., the probability of casting a decisive vote. For example, structural factors such as district size and competitiveness should influence the probability of being decisive and therefore also the individual's decision to vote or not.[2,3]

Most studies examining the relationship between turnout and competitiveness at the national, state, district, or precinct levels report higher turnout during closer elections. Barzel and Silberberg (1973), examining state-level gubernatorial elections for election years from 1962–1968, finds that a one point decrease in competitiveness is associated with a three quarters of a point decline in turnout. Kim, Petrocik, and Enokson (1975) finds that the degree of electoral competition (measured by examining a state's vote share in past presidential elections) explains at least one quarter of the variation in turnout across states. Moving outside of the U.S., Powell (1986) notes that turnout appears higher in countries with nationally competitive districts. In a more recent study focused outside of the U.S., a cross-country examination of competitiveness and turnout finds that going from a dead heat (e.g., 50/50 in a two-party election) to a ten point gap (e.g., 55/45 in a two-party election) between the first and second place parties leads to a 1.5 percentage point decline in turnout (Blais 2000). Nevitte et al. (1999) finds that time-series variation in national level turnout also exhibits a positive relationship between competitiveness and turnout. In another study examining U.S. presidential elections, a one percentage point

[2] Subsequent theoretical work has moved beyond the framework set forth by Riker and Ordeshook (1968), but the literature on closeness and turnout has continued to motivate the relationship between closeness and turnout using their simple model. We follow this norm here, and we note that even in more recent theoretical treatments of participation in elections, the same comparative static holds. For example, in Feddersen and Sandroni (2006), overall turnout is strictly increasing in the population's underlying level of disagreement, and, in turn, the election's anticipated closeness.

[3] Other examinations of voter participation draw on theories of minimax regret (Ferejohn and Fiorina 1974), group benefits (Uhlaner 1989), strategic uncertainty (Palfrey and Rosenthal 1985), and information acquisition (Matsusaka 1995; Feddersen and Pesendorfer 1996).

increase in competitiveness is estimated to lead to a one-third of a percentage point increase in turnout (Shachar and Nalebuff 1999). Furthermore, for statewide and nationwide races, competitiveness at the district level appears to matter in addition to the competitiveness of the race overall (Franklin 2004). These are but a few examples of a host of studies, at various levels of aggregation (both in and outside of the U.S.), that have led analysts to conclude that the positive relationship between competitiveness and turnout is among the "most firmly established" findings in the literature on elections (Blais 2006). In short, the scholarly consensus is that "[c]loseness matters—and not only in horseshoes and dancing" (Geys 2006, p. 647).

To explain why competitiveness matters, scholars have also provided a variety of reasons beyond just instrumental voting at the individual level. Key (1949) theorizes that party elites mobilize more resources when elections are close. Cox and Munger (1989) finds that, in House races, campaign expenditures increase in response to competitiveness. When controlling for campaign expenditures in a cross-sectional regression of turnout on competitiveness, the direct effect of competitiveness on turnout is diminished, though still significantly different from zero. The authors interpret this result as evidence that elite mobilization plays the primary role in explaining the relationship between competitiveness and turnout. In other aggregate-level studies of close elections, scholars have also observed increases in campaign activity (Jackson 1996; Hill and McKee 2005) and media coverage (Clarke and Evans 1983), both of which also correlate with turnout.

Aggregate studies of voter turnout, however, are subject to several critiques. Cox (1988) provides a critique of standard measures of election competitiveness, noting that when *ex post* measures of competitiveness are in percentage terms it leads to spurious correlations (since turnout is in the denominator on the right hand side and the numerator on the left hand side of the equation). Aggregate studies of turnout may also fall victim to aggregation bias. Matsusaka and Palda (1993) finds that, in Canadian national elections, when there is a positive relationship between competitiveness and turnout at the aggregate level, the effect often does not hold at the individual level, based on self-reported voting behavior.

Aggregation problems, the authors theorize, are responsible for the discrepancy. In another critique, Matsusaka (1993) shows that no relationship exists between competitiveness and turnout when examining ballot propositions.[4] However, the competitiveness effect still persists in congressional elections across a range of years (1962, 1970, 1982). Matsusaka concludes, following Cox and Munger (1989), that "closeness drives turnout indirectly... elites in particular national political parties funnel campaign money to congressional candidates in close races," which explains why the relationship exists in congressional elections but not for ballot propositions.

Lab and field experiments seek to address some of the limitations in the aggregate-level research. These approaches often allow for more careful experimental or quasi-experimental manipulation of perceptions of competitiveness. Großer and Schram (2010) relies on a laboratory experiment in which the authors manipulate participants' levels of information about the competitiveness of an upcoming election (analogous to exposure to polls). In the experiment, voters react sharply to the information release. In dead heat elections, turnout increases substantially in response to releases of information; on the other hand, in landslides, releasing information about competitiveness decreases turnout. In a real-world analog, Enos and Fowler (2014) takes advantage of a tied local election in Massachusetts in order to test the effect of pivotality on turnout. By contacting some voters to inform them of the closeness of a tied election result, the authors test whether awareness of potentially casting the pivotal vote affects turnout, and they find very little supporting evidence that competitiveness matters in this respect.

While the lab and field experiments allow for clearer causal inferences, they raise questions about external validity. Simulated elections in a laboratory setting may not operate similarly to elections with real stakes. On the other hand, field experiments exploiting a single election are subject to critiques about the idiosyncrasies of the time and office under study. For example, finding no relationship between competitiveness and turnout may mean

[4]By examining within ballot abstentions, i.e., roll-off, the approach essentially controls for potential variation in costs since the marginal cost of casting a vote once in the booth is zero.

that an increase in the probability of being pivotal has no effect on voter decision-making, or it may mean the stakes of the election studied are too low to influence potential voters.

Naturally occurring experiments that lead to variation in perceptions of competitiveness, but also allow for the study of multiple elections, move past some of the critiques of observational and experimental research. We employ this approach by using the 2012 redistricting cycle as a natural experiment altering the electoral competition experienced by potential voters; we then obtained panel data tracking individual level turnout in congressional elections between 2008 and 2014.

This approach retains external validity while still allowing for credibile causal inferences. Specifically, congressional elections offer stakes high enough that the outcome means something to voters, candidates, and party elites. At the same time, by examining the turnout of citizens over time, we can exploit year to year variation in competitiveness that results from redistricting, allowing us to sidestep aggregation bias and difference out time-invariant confounding variables. Past studies have used redistricting to gain leverage over questions ranging from its effect on roll-off in down-ballot elections (Hayes and McKee 2009) to the personal incumbency advantage (Ansolabehere, Snyder, and Stewart 2000; Sekhon and Titiunik 2012) to the effects of co-ethnic candidates (Fraga 2016b). That said, the change in electoral conditions experienced by citizens due to redistricting cannot be considered identical to an experimental treatment. For instance, some evidence exists that redistricting sorts voters into districts based on race and on propensity to turn out. For example, high participation Hispanic voters appear to be more (or less) likely to be redistricted into districts with co-ethnic candidates depending on the state (Henderson, Sekhon, and Titiunik 2016)—leading to difficulties in inferences about the effect of co-ethnic candidates on turnout. In our study, a related concern is that individuals in competitive districts differ from those in uncompetitive districts in either observed or unobserved ways that are correlated with both competitiveness and turnout. We pay close attention to these concerns, employing a difference in differences style design to account for unobserved confounders and matching to ensure comparability between individuals in competitive versus uncompetitive districts in terms of observed covariates.

2.3 Data & Measures

2.3.1 Measuring Electoral Competitiveness

Using the proper measure of competitiveness stands out as a crucial consideration in any study of closeness and turnout. Geys (2006) classifies measures of competitiveness into ex post and ex ante measures. Ex ante measures capture expectations over an election outcome, while ex post measures use the actual election outcome. Cox (1988) notes the concern that ex post competitiveness depends on turnout in the same election.[5] For instance, a political scandal might boost turnout among supporters of both the incumbent candidate and challenger, causing higher turnout and, as a result, a closer election outcome. Voters and elites, however, would have responded to the scandal rather than to the perceived competitiveness of the election. This endogeneity biases estimates of the effect of competitiveness on turnout. While several studies utilize ex ante measures (e.g., Kunce 2001 uses pre-election polling; De Paola and Scoppa 2014 uses the first-round election in Italian municipal elections as an instrument for the competitiveness of the second-round election; and, Shachar and Nalebuff 1999 uses the predicted vote share based on a model), ex post measures remain the norm in the literature.[6]

Given the endogeneity concerns with ex post measures of competitiveness, we employ an

[5]When measuring the margin of victory (i.e., the typical ex post measure of competitiveness), the numerator is the number of votes cast for the losing candidate subtracted from the number of votes for the winning candidate, and the denominator is the total number of votes cast: $M_i = \frac{W_i - L_i}{W_i + L_i} \times 100\%$. The numerator for the turnout measure is the total number of votes cast, and the denominator is the total number of eligible voters: $T_i = \frac{W_i + L_i}{E_i} \times 100\%$. As is clear, the denominator for the ex post measure of competitiveness is identical to the numerator for the measure of turnout. Cox (1988) notes that there is minimal variation in the number of eligible voters across congressional districts (E_i) post *Baker v. Carr*, so holding constant the numerator of M_i, any variation in the total number of votes cast ($W_i + L_i$) *mechanically* yields a negative correlation between M_i and T_i. In other words, any time that W_i and L_i both increase by a similar amount (for reasons completely independent of the perceived competitiveness of the election), the higher turnout *results* in a lower margin of victory.

[6]Geys (2006) notes that 259 of the 362 (72%) studies in his review of competitiveness and turnout use ex post measures.

ex ante measure of district competitiveness as our primary measure.[7] We derive our measure from the *Cook Political Report's* Partisan Voting Index (PVI).[8] The PVI averages the mean-deviated, Democratic share of the two-party vote in a given congressional district over the past two presidential elections. We use the PVI based on the 2004 and 2008 presidential elections:

$$PVI_i = \frac{(D_{i,2004} - A_{2004}) + (D_{i,2008} - A_{2008})}{2} \tag{2.2}$$

where $D_{i,2004}$ is the Democratic percentage of the two-party vote in the 2004 presidential election for district i, A_{2004} is the average Democratic percentage of the two-party vote in the 2004 presidential election, and $D_{i,2008}$ and A_{2008} are the corresponding percentages for 2008.[9] Intuitively, the PVI indicates the extent to which a given congressional district favors a Democratic candidate or a Republican candidate relative to the average congressional district. A PVI of 0 indicates a 50/50 district, while a PVI of D+10 or R+10 indicates a 60/40 district favorable to a Democratic candidate or Republican candidate, respectively. We define *PVI Competitiveness* as follows:[10]

$$\textit{PVI Competitiveness}_i = -1 \cdot |PVI_i| \tag{2.3}$$

We code a 50/50 district as 0, a 60/40 district (no matter which party is favored) as −10, a 70/30 district (no matter which party is favored) as −20, and so on. Thus, a 10-unit increase

[7]While ex ante measures are preferable to ex post measures, many ex ante measures are still subject to endogeneity concerns. For instance, pre-election polls might suggest a close race precisely because of high anticipated turnout. Even statistical models that predict competitiveness based on, among other things, incumbency status and challenger quality are subject to such concerns given the likelihood of strategic retirement and entry on the part of incumbents and challengers who consider electoral dynamics that affect turnout in their calculus to leave or enter the contest. Expert ratings are similarly subject to such concerns as experts take into account these same dynamics in assigning ratings. PVI, however, measures the underlying partisan composition of the district and, thus, is largely immune to such endogeneity concerns.

[8]For more information, see: http://cookpolitical.com/house/pvi. Fraga (2016b) also uses PVI as an ex ante measure of competitiveness in the technical appendix.

[9]That is, $D_{i,2004}$ is the number of votes for Kerry in district i divided by the total number of votes for Kerry and Bush in district i, multiplied by 100%.

[10]Shachar and Nalebuff (1999) also define their ex ante measure in this way.

in *PVI Competitiveness* corresponds to a shift in competitiveness from 60/40 to 50/50 (or from 70/30 to 60/40, etc.). In other words, positive changes in *PVI Competitiveness* indicate closer elections. In the Appendix to Chapter 2, we demonstrate that *PVI Competitiveness* serves as a valid ex ante measure of electoral competitiveness (see Figure B.1 in the Appendix).

Decennial redistricting occurred between the 2010 and 2012 elections, so we use a PVI measure based on the 2004 and 2008 presidential elections tabulated for both the pre- and post-redistricting boundaries. Thus, our competitiveness measure remains comparable throughout the entire time period under study (2008–2014). For the sake of consistency with past literature, we also report results based on an ex post measure of competitiveness, the two-party win margin. As with *PVI Competitiveness*, the *Ex Post Competitiveness* measure defines a tie (50/50) as having the value 0, a 60/40 election outcome (no matter which party wins) as -10, a 70/30 election outcome (again, no matter which party wins) as -20, and so on. In addition, we report results based on dichotomous versions of both *Ex Post Competitiveness* and *PVI Competitiveness* in which 60/40 through 50/50 elections are coded as competitive (1) and all other elections are coded uncompetitive (= 0).

2.3.2 Aggregate-Level Data

Data tracking congressional district characteristics from 2008–2014 are primarily based on the one-year estimates from the (Census Bureau's) American Community Survey (ACS) congressional district-level summary file.[11] These characteristics include the composition of the district by age, race/ethnicity, education, employment, and income as well as the population density of the district and residential mobility.[12] Definitions for these measures and the ACS summary file table number that contains each of these measures are displayed in Table B.1 in the Appendix to Chapter 2. The district-level turnout rate corresponds

[11] Data were retrieved online from the Census Bureau's American Fact Finder: http://factfinder.census.gov/.

[12] The land area of each congressional district is used to calculate population density. These data are from the Census Bureau (but not the ACS): https://www.census.gov/geo/maps-data/data/cd_national.html.

to the total votes cast in the House election divided by the district's citizen voting-age population (CVAP).[13] The CVAP data depend on one-year estimates from the ACS, and the total votes cast tally comes from the CQ Voting and Elections Collection. Because some states only report vote tallies for contested races, we do not include uncontested races in our sample. We also exclude contests between two Democrats or two Republicans and all Louisiana races due to their unusual rules.[14] We use data from Jacobson and Carson (2016) to determine which races included Democratic and Republican candidates, the two-party margin of victory in each race, and campaign expenditures.

2.3.3 Individual-Level Data

To analyze the relationship between competitiveness and turnout at the individual level, we use data from Catalist, LLC. Catalist is a for-profit data vendor that compiles voter registration lists from all 50 states and the District of Columbia into a "unified national voter file."[15] Catalist extracts information from voter lists regarding individuals' turnout, age, gender, race/ethnicity, and party affiliation, and further supplement the voter file with commercial data.[16] Importantly, the Catalist database tracks individuals' voting records across time even as their registration status and residence may change.[17] Catalist thus allows

[13]Data on the voting-eligible population are not available at the congressional district level, so the "best proxy for congressional or state legislative district voting-eligible population turnout rates is citizen voting-age population turnout rates..." (McDonald 2016).

[14]In Louisiana, the November general election includes all candidates on the ballot (a primary does not restrict ballot access for the general). If no candidate receives a majority of the vote, a top-two runoff election is held (typically in December). California and Washington both have top-two primary elections in which the two candidates with most votes qualify for the ballot in the November general election. The top-two primary occasionally yields two candidates from the same party for the general election. These contests are omitted from our sample.

[15]For more information on the Catalist data, see here: http://www.catalist.us/data/.

[16]Only some states provide information on voters' race/ethnicity and party affiliation in their voter files.

[17]In Catalist, the voting records of a previously registered individual who re-registers are linked together to form a full panel. In the official voter files from states and counties, only the individuals who were currently registered at the particular moment in time the voter list is produced would be listed in the file. As Fraga (2016b) notes, because of the dynamic nature of official voter files, "longitudinal analysis of individual-level registration or turnout is a great challenge to researchers wishing to avoid contracting with a third-party organization, despite the public availability of the voter file."

us to analyze voters' actual turnout histories across time (and importantly for our research design, across redistricting cycles). While Catalist's clients are predominantly progressive organizations engaging in micro-targeting voter outreach efforts (e.g., various PACs and campaign committees affiliated with the Democratic party or its candidates, unions, interest groups), academic researchers have increasingly utilized Catalist data to study voter behavior (e.g., Ansolabehere and Hersh 2012; Fraga 2016b; Hersh and Nall 2014).

Unlike individual-level survey data on voter turnout, the data compiled by Catalist do not suffer from concerns of social desirability bias and faulty memory. Moreover, few panel surveys exist that track voters' behavior across multiple elections. The few panel political surveys that do exist generally have relatively small sample sizes due to the high costs of tracking individuals across time. We analyze a 1-percent sample of the Catalist database, which includes over 2 million individuals.[18]

Catalist provides the post-redistricting (2012 and 2014) congressional district for nearly all individuals based on their registration address. They also provide the pre-redistricting (2008 and 2010) congressional district for a large subset of the sample. We use the congressional districts provided by Catalist when available. When the pre-redistricting congressional district is not available from Catalist, we geocode these individuals into a pre-redistricting congressional district based on the block group of their registration address. Individuals who move during this period of time might be geocoded into the wrong pre-redistricting congressional district, so we exclude all individuals from our analysis sample who Catalist identifies as moving between 2008 and present. We further restrict our analysis sample to individuals who were age 18 or older on election day in 2008 and are not deceased.[19,20] Imposing these restrictions yields a balanced panel of about 1.6 million when examining

[18]The 1-percent sample is a Catalist data product explicitly designed for academic researchers.

[19]In addition to the residential non-mobility, age, and non-deceased sample restrictions, we also exclude voters who reside in districts in Louisiana, districts with an uncontested election, or districts with an election between two Democrats or two Republicans.

[20]As a robustness check, we also restrict the sample to only those individuals who are continuously registered since 2008 (in addition to the residential non-mobility, age, and non-deceased restrictions). Results based on this more restrictive sample are extremely similar to results based on the primary analysis sample.

mid-term elections, and 1.35 million when requiring a balanced panel for all four election years between 2008 and 2014.

Finally we utilize survey data to provide additional insight into existing theories that have sought to rationalize the closeness and turnout relationship in past work. To gauge voter expectations of electoral competitiveness for House races, we analyze data from a Pew October 2006 Survey on Electoral Competition.[21] Additionally, to gauge elite mobilization efforts, we analyze data from the 2010-2014 Cooperative Congressional Election Study (CCES) Panel Study. The 2010-2014 CCES Panel follows 9,500 of the 55,400 individuals from the 2010 CCES through the 2014 election. The CCES provides each respondents' pre- and post-redistricting congressional district and asks respondents' whether they experienced campaign contact (and, if so, which methods of contact) in each election year. The panel design allows us to analyze whether voters report more or less campaign contact under differing conditions of competitiveness.

2.4 Aggregate Level Analysis

In this section, we replicate previous aggregate-level, cross-sectional analyses using data from the 2008-2014 time period. We first analyze the bivariate relationship between district turnout rates and competitiveness, and then we assess the comparability of competitive and uncompetitive districts. Finally, we demonstrate the precariousness of covariate adjustment strategies in this context.

2.4.1 Bivariate Relationship

We first conduct an aggregate-level analysis using data from the same time period as our individual-level analysis. Figure 2.1 displays the relationship between the *PVI Competitiveness*

[21] For more information on the survey and to download data from the survey, see here: http://www.people-press.org/2006/10/27/october-2006-survey-on-electoral-competition/. While the American National Election Studies has asked respondents about the expected competitiveness of presidential elections since 1952, to our knowledge, this Pew survey is one of the few surveys that asks respondents about the expected competitiveness of their U.S. House elections.

measure and the turnout rate for contested House elections between a Democratic and Republican candidate in 2014. Each dot in the figure corresponds to a local mean and is proportional in size to the number of observations within the locale.[22] Figure 2.1 shows that the bivariate relationship between a district's *PVI Competitiveness* and turnout rate is strong and substantively large.[23] The expected difference in the turnout rate between a 50/50 district and a 60/40 district (as measured by *PVI Competitiveness*) is about 2.5 percentage points.[24] While we report estimates based on the sample of House elections in 2014, estimates based on the sample of House elections in any single year between 2008 and 2014, a pooled sample of House elections in 2010 and 2014, or a pooled sample of 2008-2014 House elections with year fixed effects yield similar results to those reported here and are reported in Tables B.2-B.5 in the Appendix to Chapter 2.

2.4.2 Assessing Covariate Balance

While clearly competitiveness is not randomly assigned across congressional districts, the extent to which competitive districts are not comparable to non-competitive districts is perhaps far less obvious. As it turns out, however, competitive districts are very different from non-competitive districts on several observable characteristics that are possible confounders. For ease of explication, we categorize 50/50 through 60/40 districts (based on the *PVI Competitiveness*) as competitive and all other districts as non-competitive.[25]

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[22] See also Figure B.9 in the Appendix to Chapter 2 for a full scatterplot rather than only the local means.

[23] Tables B.6-B.10 in the Appendix to Chapter 2 display estimates based on the ex-post measure of electoral closeness. Estimates based on the ex-post measure are very similar to those based on the *PVI Competitiveness* measure.

[24] More generally, the expected difference in the turnout rate between a district with a *PVI Competitiveness* $= k + 10$ and a *PVI Competitiveness* $= k$ is 2.5 percentage points.

[25] The level of covariate imabalance is not especially sensitive to this particular cutoff. For instance, if we instead categorize only 50/50 through 55/45 districts as competitive, the interpretation of the imbalance plot is essentially unchanged; the eight characteristics with a significant difference in means based on the 60/40 cutoff remain significant based on the 55/45 cutoff, and the two characteristics with a non-significant difference in means remain non-significant.

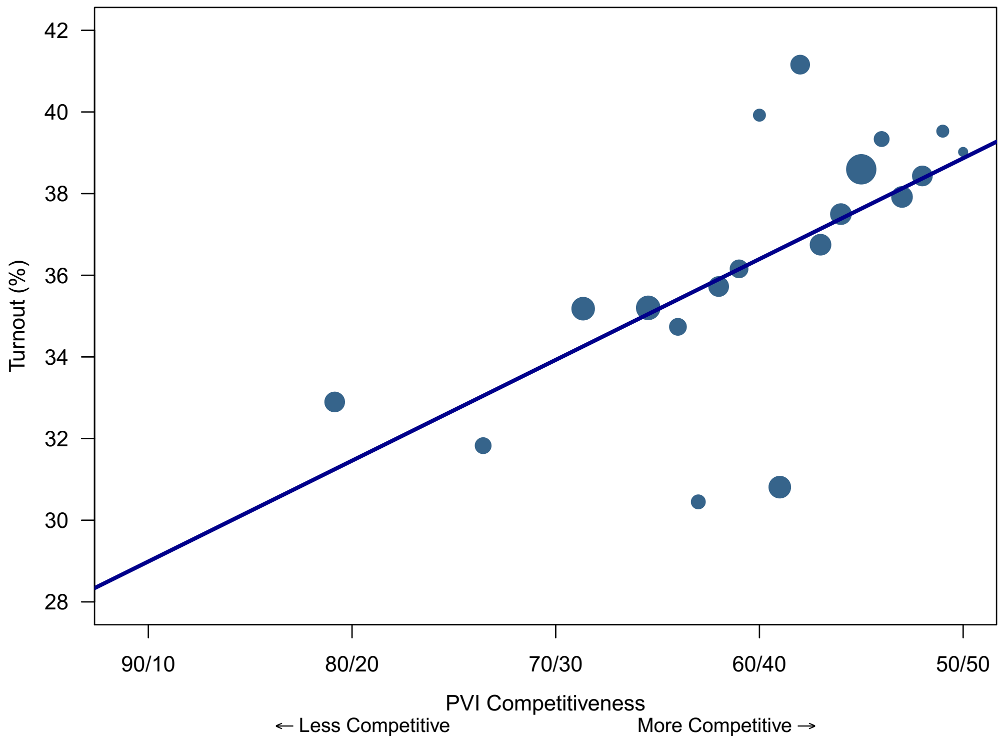


Figure 2.1: Turnout Rate vs. PVI Competitiveness, 2014. This figure illustrates the positive cross-sectional relationship between turnout rate and *PVI Competitiveness* (for the 2014 election year). As the level of competitiveness increases so does the expected turnout in each congressional district.

Figure 2.2 displays a covariate balance plot for 10 district-level characteristics.[26] Of the 10 characteristics, 8 surpass the 0.05 level of significance and the test statistics for most of these characteristics are very far from zero. As the balance plot demonstrates, competitive districts on average have lower shares of black and Asian residents, lower residential mobility, lower population density, higher employment, higher median household income, a higher share of elderly residents, and a higher share of residents who completed high school.[27] These district characteristics are confounding variables to the extent that they are also correlated with district turnout rates (given that the characteristics are correlated with district competitiveness).

[26]See Figure B.12 in the Appendix to Chapter 2 for covariate balance plots based on the ex-post measure of competitiveness.

[27]See Figure B.10 in the Appendix to Chapter 2 for quantile-quantile plots of these covariates.

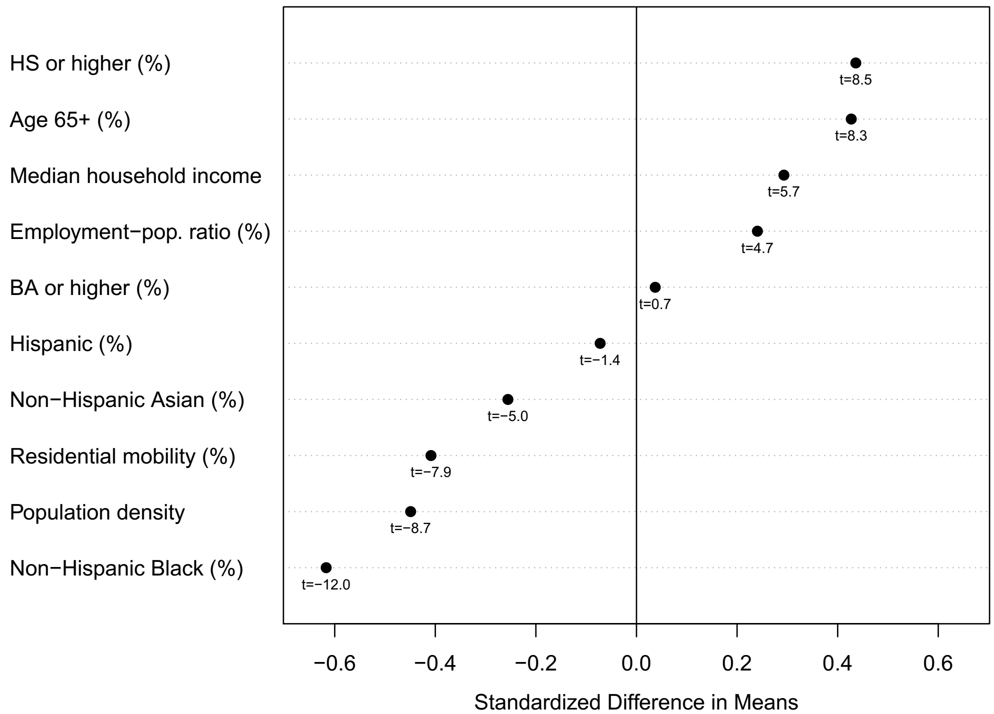


Figure 2.2: Covariate Balance Plot: Competitive (PVI $\leq$ 10) vs. Uncompetitive (PVI > 10) Districts, 2008-2014. This figure illustrates the degree of imbalance in measured covariates across competitive and non-competitive districts. These covariates are all plausibly related to a district's turnout rate. Each point on the plot indicates the standardized difference in means between competitive (i.e., districts with a PVI less than or equal to 10 percentage points or 60/40 through 50/50 districts) and uncompetitive congressional districts (61/39 through 100/0 districts). The t statistic for each standardized difference is displayed below.

2.4.3 Covariate Adjustment

Based on the balance plot, it is evident that the estimate is almost certainly suffering from omitted variable bias. We use a regression framework for covariate adjustment in models (2)-(6) in Table 2.1. In model (2), we include state fixed effects, which account for statewide electoral dynamics (e.g., statewide gubernatorial and senatorial elections, ease of voting based on election laws and administration, etc.), which likely affect statewide turnout in a given election. Model (3) includes six of the district-level covariates (without any fixed effects) that account for the age composition, racial/ethnic composition, and educational attainment of the district, while model (4) includes those same six covariates as well as state fixed effects. Model (5) includes all 10 district-level covariates from the balance plot, and model (6) includes the 10 district-level covariates with state fixed effects. Depending on the specification for the 2014 data, the expected difference in the turnout rate between a 50/50 district and a 60/40 district ranges from about 0.04 to 2.47 percentage points.

Our conclusion from this aggregate analysis is not that any one of these specifications is the "correct" model yielding an unbiased estimate of the effect of competitiveness on turnout. Instead, our main conclusion is that estimating the effect of competitiveness on turnout using aggregate, cross-sectional data is an extremely precarious exercise. Any covariate adjustment strategy requires a selection on observables (conditional independence) assumption in order to identify a causal effect. In other words, conditional on the observable covariates included in the analysis, the level of competitiveness is independent of the potential outcomes.[28] With a regression framework, we must also correctly specify the functional form of all covariates and assume linearly separable confounding.

While the estimated effect of competitiveness on turnout decreases with the inclusion of this particular set of covariates, our estimate remains biased in either direction if any covariate—measured or unmeasured—that is correlated with both competitiveness and turnout is omitted from the model. Given the multitude of possible (un)measured covariates not included in the model and our largely arbitrary decision as to which measured covariates to include in the model, it is nearly impossible to recover an unbiased estimate of the causal effect of competitiveness on turnout. When the selection process and confounding are not well understood, using an aggregate, cross-sectional approach with a selection on observables identification assumption (by necessity) does not yield credibile estimates.

2.5 Individual Level Analysis

The previous section showed that using district-level (aggregate) turnout data from a single year leads to biased estimates of the relationship between competitiveness and turnout. This section illustrates that a similar pattern persists when employing cross-sectional re-

[28]Because electoral competitiveness is measured as continuous variable, following Angrist and Pischke (2009), we express the potential outcomes using district-specific functional notation: $Y_{ci} \equiv f_i(c)$, which denotes the potential turnout rate in district i for electoral competitiveness c.

Table 2.1: Results Based on 2014 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.247***	0.115***	0.119*	0.090***	0.100	0.004
	(0.057)	(0.034)	(0.052)	(0.025)	(0.054)	(0.025)
Age 65+ (%)			0.362**	0.339***	0.553***	0.277***
			(0.126)	(0.073)	(0.140)	(0.074)
Hispanic (%)			-0.067	-0.146***	-0.081*	-0.170***
			(0.039)	(0.027)	(0.038)	(0.025)
Non-Hispanic Black (%)			-0.002	-0.003	0.002	-0.005
			(0.034)	(0.019)	(0.034)	(0.018)
Non-Hispanic Asian (%)			-0.302***	-0.282***	-0.191**	-0.273***
			(0.069)	(0.044)	(0.072)	(0.042)
HS or higher (%)			0.534***	0.043	0.263	-0.147
			(0.140)	(0.078)	(0.154)	(0.077)
BA or higher (%)			0.172**	0.260***	0.275***	0.325***
			(0.056)	(0.030)	(0.078)	(0.037)
Employment-pop. ratio (%)					0.367***	0.076
					(0.100)	(0.054)
Median household income					-0.115*	-0.002
					(0.053)	(0.027)
Population density					-0.341***	-0.341***
					(0.099)	(0.046)
Residential mobility (%)					0.168	-0.222**
					(0.131)	(0.073)
Constant	38.867***	34.163***	-17.102	19.079*	-21.830	56.625***
	(0.738)	(4.479)	(12.104)	(7.485)	(12.383)	(7.388)
Observations	354	354	354	354	354	354
Adjusted R^2	0.048	0.717	0.429	0.893	0.468	0.915
State FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

gressions at the individual level—in essence, replicating the positive relationship between turnout and competitiveness in the previous literature. We outline and then implement a difference-in-differences approach that addresses the biases evident in previous, cross-sectional approaches. We find a precise null effect of competitiveness on turnout. This null effect persists across a variety of specifications and robustness checks, which we detail below.

2.5.1 Cross-Sectional Results

For each election year from 2008–2014, we regress turnout on our measure of electoral competition, *PVI Competitiveness*, as well as a host of individual level voter characteristics including an indicator variable capturing citizens who are female, over age 65, Hispanic, non-Hispanic Black, and non-Hispanic Asian. We also include a variable measuring individual educational attainment—capturing whether someone holds a college degree or higher.[29]

In the Appendix to Chapter 2 we present point estimates for each year across three specifications: the simple correlation between *PVI Competitiveness* and *Turnout*, the estimate when including state indicator variables, and, finally, the estimate when including the control variables along with the state indicators. Each year includes well over one million observations, so the estimates are extremely precise. With few exceptions, we find that a 10-point increase in competitiveness (i.e., going from a 60/40 to a 50/50 election) is associated with between a 1 and 2 percentage point increase in the probability of turning out. For example, when conditioning on state of residence, a 10-point swing in competitiveness is associated with an increase in turnout of 1.66 percentage points in 2008, 1.69 percentage points in 2010, 1.75 percentage points in 2012, and 1.8 percentage points in 2014. Similarly, estimates of the bivariate relationship range between 2 and 2.5 percentage points. These individual level results replicate the accepted finding in the existing literature that a 10-point

[29] This variable takes the form of a percentage giving the probability that an individual holds a college degree or higher. To estimate this probability, the educational attainment model is based on survey data from 25,000 respondents who answered questions about their educational attainment. Using these results, a logistic model of educational attainment infers education levels for the whole population based on other observable characteristics.

swing in competitiveness changes turnout by between 2 and 4 points (Blais 2006).[30,31]

2.5.2 Methods for Individual Turnout Panel

Observing individual behavior across districts and over time grants more flexibility in the estimation strategy. The primary contribution of the chapter, and the departure from the existing literature, is to employ panel data tracking individual voting behavior in order to assess the relationship between competitiveness and turnout. Consider a reduced form empirical model of the turnout decision:

$$E(\text{Turnout}_{ist}) = \alpha + \lambda_{st} + \rho \cdot \text{Closeness}_{ist} + \text{Vote Propensity}_i' \cdot \psi + X_{ist} \cdot \beta \qquad (2.4)$$

where α is a constant term, λ_{st} is a state-year fixed effect, Closeness_{ist} measures a citizen's perception of competitiveness in an upcoming election, Vote Propensity_i is an individual's unobserved underlying tendency to vote (likely a function of their sense of civic duty, political knowledge, etc.), and X_{ist} is a vector of observable characteristics.[32] Under the assumption that unobserved propensity to vote remains constant over time, then we can let $\gamma_i = \alpha +$

[30] Some scholars report the effects of a 10-point swing in *margin of victory*, i.e., moving from a 50/50 election to a 55/45 election, which we multiply by 2 to transform into a 10-point swing in the level of competitiveness. Blais (2006) notes that the accepted finding in the literature is that a swing of 10 points in margin of victory leads to a change in turnout of "one or two points".

[31] The positive relationship we find at the individual level contrasts with some results from past examinations of cross-sectional, individual-level data. Matsusaka and Palda (1993), examining individual-level self-reported turnout for the 1979 and 1980 Canadian national elections, finds no relationship between competitiveness and turnout, and concludes that aggregation bias must be a primary factor for the established result in the literature of a positive relationship between competitiveness and turnout at the aggregate level. Our evidence suggests that U.S. congressional elections do not exhibit the same properties, and they cast additional doubt on any claim that aggregation bias fully explains the cross-sectional results for U.S. congressional elections. Ruling out aggregation bias, two remaining alternatives exist: (1) The effect in the cross-section at the individual level is real and can be explained by the instrumental voting logic of Riker and Ordeshook (1968) or by indirect effects such as elite mobilization, media coverage, campaign activity, etc.; or, (2) problems with estimation, such as unobserved confounding, lead to biased estimates at the individual level in the cross-section.

[32] The inclusion of state-year fixed effects controls for state-specific political conditions for a given election year such as gubernatorial and senatorial elections, changes to election laws, etc.

Vote Propensity$'_i \cdot \psi$ and estimate the model:

$$E(\text{Turnout}_{ist}) = \gamma_i + \lambda_{st} + \rho \cdot \text{Closeness}_{ist} + X_{ist} \cdot \beta \tag{2.5}$$

where, in contrast to the cross-sectional results above, the estimation of ρ relies on *within* person variation in competitiveness. This approach explicitly accounts for the most direct critiques leveled towards observational, cross-sectional regressions of turnout on competitiveness. For example, the level of competitiveness in a district also correlates with individual racial background and ideology; if turnout choices systematically vary with race or with ideology, then estimates of the effect of competitiveness on turnout would be biased if we did not condition on these variables. A similar argument exists for a range of other variables, some of which we cannot possibly measure. The inclusion of individual and state-year fixed effects addresses this type of confounding by differencing out all time-invariant covariates (both measured and unmeasured).

The key variation in competitiveness arises because our sample includes a redistricting cycle. Decennial redistricting leads to a re-shuffling of census blocks into new congressional districts; by changing the composition of districts, expectations over competitiveness also change. Our primary measure of closeness, *PVI Competitiveness*, varies over time only when the composition of a district changes; it can be thought of as capturing the underlying tendency of the district to have a close election. Figure B.2 in the Appendix illustrates the distribution of district-level *PVI Competitiveness* for 2008-2014. The distribution is left-skewed, with the median district having a *PVI Competitiveness* value of -9, i.e., an ex ante expectation of a 59/41 outcome.

While our measure of *PVI Competitiveness* indicates competitiveness at the district level, variation in competitiveness over time occurs in practice at the level of the census-block, which is the smallest level of geography available to those participating in drawing maps for new districts. Figure B.4 in the Appendix to Chapter 2 illustrates the change from pre to post-redistricting in *PVI Competitiveness* for each individual in the sample. Redistricting leads to modest but nonetheless considerable heterogeneity of experience with respect to changes in district competitiveness. First, as expected, the distribution is symmetric about the origin—just as many individuals experience increases in competitiveness as decreases. Second, the majority of citizens do not experience a meaningful change in district competition due to redistricting. For example, the median individual only experiences a shift of two (i.e., going from 50/50 to 52/48.) The bulk of the variation occurs in the top and bottom deciles of the distribution. These individuals all encounter at least a six point shift in district competitiveness (i.e., going from 50/50 to 56/44). Our ex post measure of electoral competitiveness exhibits even larger swings. A full 20 percent of the sample experiences at least a 10-point change in electoral closeness according to the ex post measure.

Past research has considered redistricting as an "as good as random" intervention (Ansolabehere, Snyder, and Stewart 2000; Sekhon and Titiunik 2012). In our case, we do not require an "as good as random" intervention (though this may nonetheless be true), but rather we rely on the assumption that trends in participation across individuals exposed to varying levels of electoral competition would be the same were they in equally competitive districts. This assumption appears plausible given the pre-histories of individual level redistricting across groups receiving different levels of treatment in our sample.

Figure 2.3 illustrates the pre-trends for citizens who experience different changes in competitiveness due to redistricting. We classify individuals in one of four categories: in a close district pre-redistricting only, in a close district post-redistricting only, always in a close district, or never in a close district—where closeness is determined by whether *PVI Competitiveness* ≥ -10 (i.e., districts ranging from 50/50 to 60/40). We then examine their turnout histories since 2002 with the one notable restriction that they were of age to vote in 2002. As the figure illustrates, the trends are largely in parallel, which demonstrates that a key assumption underpinning our empirical approach seems valid. While, as expected, presidential election years see a substantial boost in turnout as compared to midterm election years, the trends still appear the same across different levels of electoral competitiveness. Nonetheless, in addition to pooling all election years from 2008–2014, we

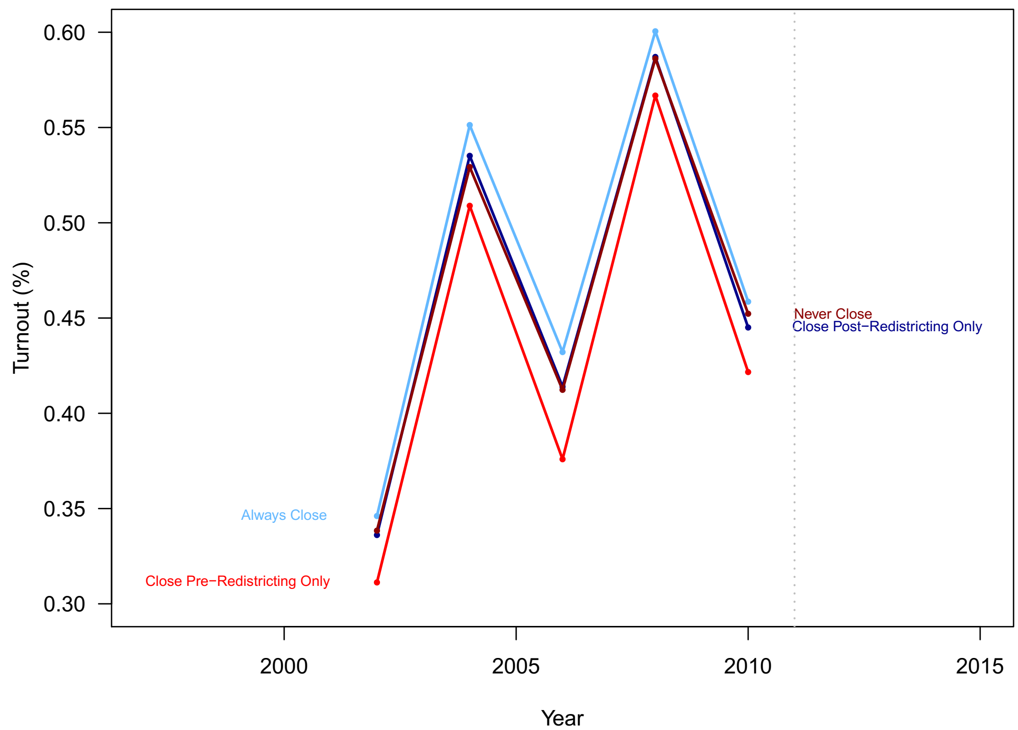


Figure 2.3: Pre-Trends in Turnout by Change in Closeness. This figure displays the parallel trends in turnout for individuals facing different levels of competitiveness. It includes individuals who resided in competitive districts both before and after redistricting, only before redistricting, only after redistricting, and neither before nor after. We classify individuals as in competitive districts based on whether the measure of competitiveness is within ten points of a dead heat (i.e., equal to or more competitive than a 60/40 election outcome).

also include analyses restricting the sample to midterm years only and to presidential years only. In some sense, including the presidential election years might downwardly bias our results since congressional election competitiveness would likely play a considerably less important role in a citizen's decision to vote.[33]

[33]Despite this, existing evidence suggests that down ballot races can play a surprisingly important role in the turnout decision. Hayes and McKee (2009) notes that in their examination of roll-off rates (i.e., the rate at which a voter fails to cast a vote in a down ballot race) a full 20 percent of ballots actually have negative roll-off. In these cases, citizens cast votes for the House but not for the top of the ticket race.

2.5.3 Results for Individual Turnout Panel

Main Specification: Difference in Differences

We apply the generalized difference-in-differences approach outlined above, which accounts for time-invariant confounders, using panel data from all election years between 2008 and 2014. When employing this approach, we estimate a 0.0113 percentage point increase in turnout for a one point increase in competitiveness (Table B.22 in the Appendix displays the full details).[34] Figure 2.4 illustrates the magnitude of the difference-in-differences estimates when compared with estimates from past research, as well as the aggregate, cross-sectional results we estimated in the previous section. We find that a 10-point increase in competitiveness (i.e., going from 60/40 to 50/50) results in a minimal effect on turnout—less than one-fifth of a percentage point, depending on whether we examine all years between 2008 and 2014, just presidential election years, or just midterm years. The upper bound of the 95% confidence interval never reaches a value of one-quarter of a percentage point for a 10-point swing in competitiveness, indicating the minuscule causal effect of competitiveness on turnout. In addition, as the figure illustrates, the large sample size yields extremely precise estimates.

Probably the cleanest test available to us is if we examine only the 2010 and 2014 data. In this case, our preferred measure of competitiveness is based on past behavior (i.e., 2004 and 2008 presidential elections), so it can be rightly considered an ex ante measure of competitiveness. Additionally, in midterm elections, House races are more likely to influence the turnout decision, as compared with presidential election years, where the national race likely plays a more important role in driving turnout.

[34] In the full table in the Appendix, we also report results where we include only state-year fixed effects (which control for state-specific conditions affecting turnout) as well as state-year fixed effects with covariates. In this case, we estimate that going from a 60/40 election to a 50/50 election is associated with an increase in turnout of slightly more than half of a percentage point. The effect is substantively small but precisely estimated, as the 95% confidence intervals do not overlap with zero. Including a set of individual specific controls—capturing age, education, and racial characteristics—does not lead to any meaningful change in the estimate. The individual controls are constant over time other than age. Unfortunately, we do not have access to any time-varying controls at the individual level.

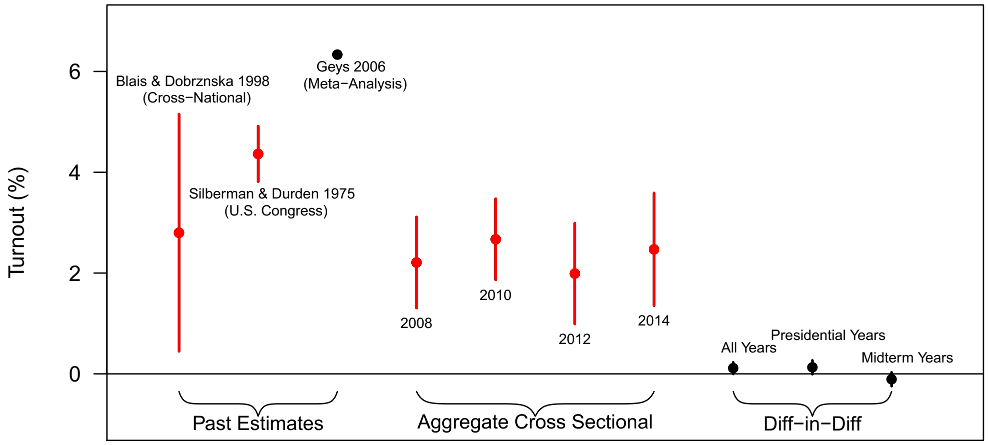


Figure 2.4: Panel Data: Marginal Effect of 10pp Increase in Closeness on Turnout (%). This figure illustrates the point estimates for the preferred difference-in-differences estimates from the individual-level panel data on turnout compared to a range of other estimates. We include estimates from past research, estimates based on the bivariate relationship for aggregate, yearly data, and, finally, our preferred individual fixed effects estimates using the individual panel data. The point estimates for the approach using individual fixed effects are a precise zero effect. Geys (2006) reports effect sizes from a meta-analysis of studies examining competitiveness on turnout; as a result, the effects are reported in standard deviations, which we apply to our aggregate election data to ensure comparability here.

Restricting the analysis to just midterm elections and to just presidential elections serves to confirm our results. We obtain a precise estimate near zero in each case. For the presidential election years, we find that a 10-point swing in competitiveness leads to just over one-tenth of a percentage point increase in turnout—this is essentially zero. The same minimal effects hold up in the midterm years.

We interpret these effect sizes as evidence of a precise null effect of competitiveness on turnout. This finding sharply contrasts with existing research and with our own cross-sectional results, both of which report considerably larger effects of competitiveness on turnout. As Figure 2.4 illustrates, these approaches yield point estimates of at least two percentage points for a ten point swing in competitiveness and, in some cases, suggest even larger effect sizes (4 and 6 percentage points, respectively) for the U.S. Congress (Silberman and Durden 1975) and for a meta-analysis of studies (Geys 2006).[35]

[35] Geys (2006) reports the results in terms of standard deviations (that we apply to our data) and also does not provide confidence bounds for the estimate.

Alternative Measure of Competitiveness

The finding of an extremely precise, zero effect when accounting for time-invariant unobserved confounders holds up across a wide range of specifications and sample restrictions. Figure B.13 in the Appendix shows that in no case does the magnitude of our point estimate approach meaningful levels.[36] We report effects of a difference-in-differences style estimator with individual fixed effects using (1) both ex ante and ex post measures of competitiveness (for the ex post measure of competitiveness, we use the actual closeness of the election outcome) and (2) for different sample restrictions, including all years in the sample (2008–2014), presidential election years (2008 & 2012), election years directly before and after the redistricting cycle (2010 & 2012), midterm election years (2010 & 2014), and, in the case of our ex post measure of competitiveness, 2008 & 2010 (i.e., using variation in competitiveness not from redistricting but that arises endogenously based on candidate entry/exit and other electoral dynamics). The null effect for 2008 and 2010 deserves particular attention, as it helps address concerns that changes in competitiveness due to redistricting comprise part of a bundle of treatments that might alter turnout and that correlate with competitiveness. We discuss this issue in more detail below, but employing changes in electoral conditions other than redistricting as a source of variation shows that the null result persists in a variety of settings. Across this range of approaches, the greatest effect we find is that a 10-point swing in competitiveness increases turnout by one-quarter of one percentage point. If the accepted finding in the literature is roughly a three percentage point increase in turnout for a 10-point increase in competitiveness, then our estimates suggest that past work overstates the effect of competitiveness on turnout by at least 12 times.

Highly Responsive Voters

To test the limits of the null result further, we restrict the sample to subsets of voters most likely to respond to changes in competitiveness. This represents a very stringent test;

[36] Tables B.23 to B.26 display the full results.

thus far, we have followed the norm in the existing literature on competitiveness and turnout of examining the full eligible voting population. We now restrict our study to sub-groups most likely to respond to the treatment. The first test consists of identifying citizens likely to respond to changes in competitiveness based on their past voting behavior. For example, if voters fit into categories such as never-voters, always-voters, and sometimes-voters, then the first two groups would not respond to changes in competitiveness. The third group of voters, who sometimes turn out and sometimes do not, would potentially respond to changes in competitiveness on the margin. We identify these voters by isolating individuals eligible for the 2006 and 2008 elections who only voted in one of those elections. We also restrict the sample to groups traditionally thought of as having more information, political knowledge, or the resources to participate. Specifically, we identify individuals in the top quartile of our measure of education and those who live in census blocks with median household incomes in the top quartile of the full distribution. Finally, we identify partisans by identifying voters registered with one of the two major parties.[37]

Figure B.14 displays the results for those voters most likely to respond to changes in competitiveness.[38] In our view, these estimates represent the ceiling for the plausible magnitudes of the effect of competitiveness on turnout in congressional elections. The effects still appear minimal. When using our preferred measure of closeness, *PVI Competitiveness*, the causal effect of a 10-point swing in competitiveness on turnout remains below one half of one percentage point for "sometimes-voters" (0.485 percentage points), for educated voters (0.232), for high income voters (0.131), and for partisans (0.333). When employing the ex post measure of competitiveness, we find slightly larger magnitudes, though three of four estimates remain at half a percentage point or below. When using ex post competitiveness, sometimes-voters (0.710) have the largest causal effect. To put this in perspective, this is the largest effect size we find—and it still comes in substantially below most past estimates

[37]This last measure is imperfect since some states do not make party registration available. Those states are omitted from the analysis.

[38]Table B.33 includes the full results.

in the literature that rely on the full set of voters rather than those likely to have the largest effects. The subset of voters likely to have the largest causal effects still exhibit minimal effects when compared to past, cross-sectional results; this provides additional support pointing towards a null effect for the full population of eligible voters.

District Level Time-Varying Covariates

Redistricting alters more than just competitiveness. In fact, one might view redistricting as applying a bundle of treatments to an individual who moves to a new district. District competitiveness changes, but so too do a number of other features related to the political environment. As a result, redistricted citizens may find other changes in their district salient beyond just the level of electoral competition. The new racial and economic context for their district could also influence their decisions to vote. For example, voters of a certain race may grow more likely to turn out as they make up a greater overall share of the population in their district (Fraga 2016a). The fact that redistricting changes more than just a district's level of competitiveness poses a problem for inference if these changes are not orthogonal to changes in competitiveness—this would violate a fundamental assumption of difference in differences, that no time-varying, unobserved confounders exist.[39] To address this issue

[39]Keele and Titiunik (2015, 2016) discuss the assumptions required to identify a causal effect based on geographic boundaries. One especially important assumption is what both papers term, "Compound Treatment Irrelevance." Administrative boundaries (e.g., municipal, school district, county) often lie directly on top of one another. Thus, it can be difficult to isolate the effect of one administrative geographic unit from another without assuming the irrelevance of the other geographic units. When comparing units across static boundaries, this is a strong assumption that can be difficult to justify. However, in our case, the congressional district boundaries are dynamic, and we observe the same individuals in different competitive contexts at different points in time. In our empirical setting, the concern of compound treatments remains, but the generalized difference-in-differences framework allows for a weaker assumption. The presence of a compound treatment is only a threat to inference if *changes* in the treatment of interest are correlated with *changes* in a compound treatment. Most administrative boundaries are static, which precludes any correlation. One set of boundaries that does change at the same time as congressional boundaries is state legislative district boundaries. However, it is exceedingly difficult to concoct a scenario in which the changes to state legislative district boundaries present a threat to inference. For one, it is difficult to imagine that state legislative elections drive voters' turnout decisions. Moreover, in order for state legislative boundaries to bias our estimates toward zero, voters would need to be placed in more (less) competitive state legislative districts *and* less (more) competitive congressional districts. Such a strategic redistricting scenario seems entirely implausible. A more worrisome concern is that congressional redistricting is itself a compound treatment: redrawing district boundaries changes the composition of a district in other ways besides competitiveness. For this class of compound treatments to present a threat to inference, changes in the compound treatments still must be correlated with changes in competitiveness. We demonstrate the plausibility of the Compound

explicitly, we present results in which we condition on a set of district-level time-varying covariates that could plausibly influence individual vote choice. Specifically, we include the district's median household income, racial composition (i.e., percent Black, Hispanic, Asian), and whether the district had a Republican or Democratic incumbent candidate. Tables B.28 to B.31 in the Appendix include the full results.[40] In no case does the point estimate appear higher than one-fifth of one percentage point—the null results remain robust and entirely in line with the evidence presented thus far. Furthermore, to the degree that selection on observed time-varying characteristics provides an informative signal about the degree of selection on unobserved time-varying characteristics, the fact that the point estimates remain stable provides reassurance that selection on unobserved time-varying characteristics does not present an important threat to making valid causal inferences.

Strategic Redistricting

If redistricters strategically and systematically place citizens with higher (or lower) levels of turnout into districts with higher (or lower) levels of competitiveness, then those placed in competitive districts might not have a valid comparison group of individuals with similar characteristics in less competitive districts—even in terms of observable variables. Henderson, Sekhon, and Titiunik (2016) describes just such a case of covariate imbalance in California, where redistricters strategically placed Hispanic voters with a higher propensity to turn out into majority-minority districts.

In the case of competitiveness and turnout, a similar issue might arise—for example, if majority-minority districts are systematically also less competitive than other districts. According to this logic, a valid comparison group would not exist for some individuals placed into districts that are not (or are) competitive.

Treatment Irrelevance Assumption by conditioning on various possible compound treatments (in this class of compound treatments) and showing that changes in other compound treatment variables are not correlated with changes in competitiveness.

[40]We also demonstrate in section B.4.1 of the Appendix to Chapter 2 that changes in the competitiveness of a district from pre- to post-redistricting is not correlated with proportion of residents remaining in the district from pre- to post-redistricting.

We guard against this possibility by combining the difference-in-differences style estimation implemented above with matching. This approach accounts for selection on unobservables (through difference in differences), while also ensuring common support and common distributions in terms of observable covariates between competitive (treated) and uncompetitive (control) units. Additionally, matching has the added benefit that, as long as we match exactly on state and pre-redistricting congressional district, identification of the causal effect comes entirely from comparing individuals who started in the same district and ended up in districts with different levels of competitiveness due to redistricting.[41]

To implement this estimation procedure, we first classify individuals as residing in either competitive or uncompetitive districts, based on whether the measure of closeness has an absolute value less than or equal to ten points (i.e., a 55/45 election is considered competitive but a 61/39 election is not). In this framework, "treated" units have a value of one and "control" units take a value of zero. We further restrict the sample to include only individuals who resided in an uncompetitive district pre-redistricting.[42]

We then match all units that ever receive treatment to a weighted set of control units. We employ an entropy balancing matching procedure (Hainmueller 2012), which re-weights control units to achieve covariate balance for their first and second moments. After blocking on state and pre-redistricting congressional district, we match along an individual's turnout choice in 2006 as well as covariates including Black, Hispanic, Asian, female, age, and education. We additionally ensure that age and education are balanced both across treatment and control in the full sample and within racial groups.[43]

Figure B.15 in the Appendix illustrates covariate balance for individuals in competitive (treated) versus uncompetitive (control) districts in midterm elections. The red circle

[41] Crucially, this comes in contrast to the difference-in-differences approach on its own, where comparisons occur across districts.

[42] While less flexible than before, this setup accords with the canonical binary pre/post difference-in-differences approach, where units receive treatment only in the second period.

[43] Similar to Henderson, Sekhon, and Titiunik (2016), we exclude individuals from pre-redistricting congressional districts where there are fewer than twice as many control units as treated units.

denotes the pre-matching standardized mean difference between competitive and uncompetitive districts in terms of a given covariate; the black square denotes the post-matching standardized mean difference.[44] The plot shows that covariate imbalances occur primarily in terms of racial characteristics. Hispanic voters had a higher propensity towards residing in competitive districts; conversely, black citizens tended to reside in less competitive districts on average. These imbalances present problems for inference if there are individuals in competitive districts for whom a reasonable comparison in uncompetitive districts does not exist. For example, consider a Hispanic citizen from a congressional district redistricted into a less competitive district in 2012; the ideal comparison group would consist of Hispanic citizens from the same district who remain in an uncompetitive district. If this comparison group does not exist, then our matching procedure prunes these observations rather than attempt to interpolate (or extrapolate, depending on the case).

Balance improves markedly after implementing the matching procedure, and no meaningful covariate imbalances remain. That said, matching only ensures balance along observable covariates. We thus further employ difference-in-differences estimation to account for time-invariant confounders.

Figure 2.5 reports the results of the estimator using a binary treatment for the matched and unmatched samples for mid-term election years and, separately, for presidential election years (the full results are reported in Table B.32 in the Appendix). In no case do we estimate an effect greater than three-quarters of a percentage point, despite the fact that the binary treatment going from uncompetitive to competitive marks a large swing in district competitiveness. When employing matching, the effect size is never more than three-tenths of one percentage point.

Finally, to deal even more explicitly with the prospect of strategic redistricting based on race, we restrict the sample to include states that do not have any majority-minority districts. This sample restriction means that a redistricter is never confronted with the question of whether to move a minority voter into or out of a majority-minority district.

[44]Figure B.16 displays the same plot but for presidential election years.

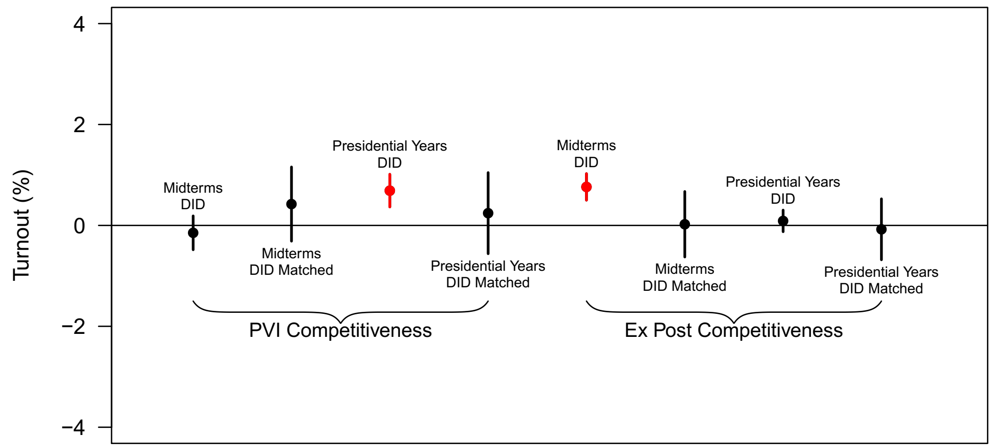


Figure 2.5: Panel Data, Binary Indicator for Competitiveness (DID and Matched DID): Marginal Effect of Closeness on Turnout (%). This figure illustrates the effects when employing a classic difference-in-differences approach. Competitive districts are those that had a competitiveness measure greater than or equal to -10 (i.e., a 60/40 race or better). The sample is restricted only to those individuals who resided in an uncompetitive district pre-redistricting. Second, we also provide estimates after having further pre-processed the data by performing an entropy balancing matching procedure.

Table B.27 in the Appendix to Chapter 2 displays the results of this robustness check for the years 2010 and 2014. Implementing the difference-in-differences approach yields an estimate with a magnitude that is less than one-tenth of a percentage point. Again, this provides evidence of an effect of competitiveness on turnout that approximates a precise zero, even when strategic redistricting based on race is not a factor.

2.6 Proposed Mechanisms

In past literature that has sought to explain the positive relationship between competitiveness and turnout, two causal mechanisms have received the bulk of the attention: the instrumental voting logic (e.g. Riker and Ordeshook 1968) and elite mobilization, such as campaign activity (e.g. Cox and Munger 1989). How do we square our estimates, which suggest a precise zero causal effect of closeness on turnout, with these proposed mechanisms? With regard to instrumental voting, are voters unaware of ex-ante electoral closeness of House elections, or are they aware but simply choose not to change their behavior in response to close elections? Similarly, do elites not actually increase mobilization efforts in response to electoral closeness? Or, are their efforts simply not effective at boosting turnout?

2.6.1 Instrumental Voting

An October 2006 Pew survey asks respondents, "What's your impression – in the race for the U.S. House in your district, is one candidate heavily favored to win or do you think this will be a close contest?"[45] Respondents can answer that "[o]ne candidate is heavily favored," that it "will be a close contest," or that they are unsure. Table 2.2 shows the results for a regression of voters' expected closeness on the two measures of closeness. Models (1) and (3) are bivariate regressions, while models (2) and (4) control for individual characteristics (age, education, income, and race/ethnicity). For the *PVI Competitiveness* measure, a 10-point increase in closeness (e.g., moving from a 60/40 to a 50/50 district) is associated with a 2 percentage point increase in the probability of an individual indicating the election "will be a close contest." For the ex-post closeness measure, a 10-point increase in closeness (e.g., moving from a 60/40 to a 50/50 district) is associated with a 3-4 percentage point increase in the probability of an individual indicating the election "will be a close contest." The estimated coefficient in all four models is not statistically significant from zero. This descriptive analysis suggests that voters remain largely unaware of electoral closeness. To reiterate, the findings suggest *actual* election closeness hardly plays a role in voters' *perceptions* of closeness. This absence of awareness suggests changes in the probability of being pivotal probably do not drive much voter decision-making. For example, the result accords with a story where voters know ex ante that their probability of being pivotal is infinitesimal no matter the closeness of the election and, thus, learning about closeness is not worth the effort. The survey evidence we rely on here says less about what would happen if voters were purposefully exposed to information about the closeness of the upcoming election. But the available evidence on this front also suggests closeness plays no role. For example, some of the most compelling evidence to date on this issue comes from Enos

[45] We found that data on voters' perceptions of the closeness of their upcoming House races are surprisingly scant.

and Fowler (2014), which finds that voters do not increase their turnout in response to a treatment informing voters of the closeness of an upcoming election.

Table 2.2: Voter Perceptions of Closeness for 2006 House Elections

	(1)	(2)	(3)	(4)
PVI Competitiveness	0.002	0.002		
	(0.002)	(0.003)		
Ex Post Competitiveness			0.003	0.004
			(0.002)	(0.002)
Constant	0.587***	0.623***	0.604***	0.641***
	(0.029)	(0.112)	(0.031)	(0.111)
Observations	1374	1168	1374	1168
R^2	0.001	0.032	0.002	0.035
Controls	No	Yes	No	Yes

Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Results are based on Pew survey data collected in October of 2006.
The dependent variable is coded =1 if respondent perceived a close contest.
Control variables include age, education, income, and race/ethnicity.
Respondents from Louisiana are excluded from the sample due to their unusual rules.

2.6.2 Elite Mobilization

The other oft-proposed mechanism explaining a positive relationship between closeness and turnout is elite mobilization. In the cross-section, we find a strong relationship between district competitiveness and campaign spending (see Figure 2.6). A 10-point increase in *PVI Competitiveness* (e.g., moving from a 60/40 to a 50/50 district) is associated with a 48 percent increase in campaign spending, while a 10-point increase in the ex-post closeness measure is associated with 68 percent increase in campaign spending (see Table B.34 in the Appendix to Chapter 2).[46]

The large increase in campaign expenditures in response to closeness without a corresponding boost in turnout presents a puzzle. However, congressional candidates direct much of these expenditures to television advertising. Spenkuch and Toniatti (2015) notes that television advertising accounted for 40-50 percent of campaign budgets in the 2010 midterm

[46]The dependent variable (campaign spending) is log-transformed.

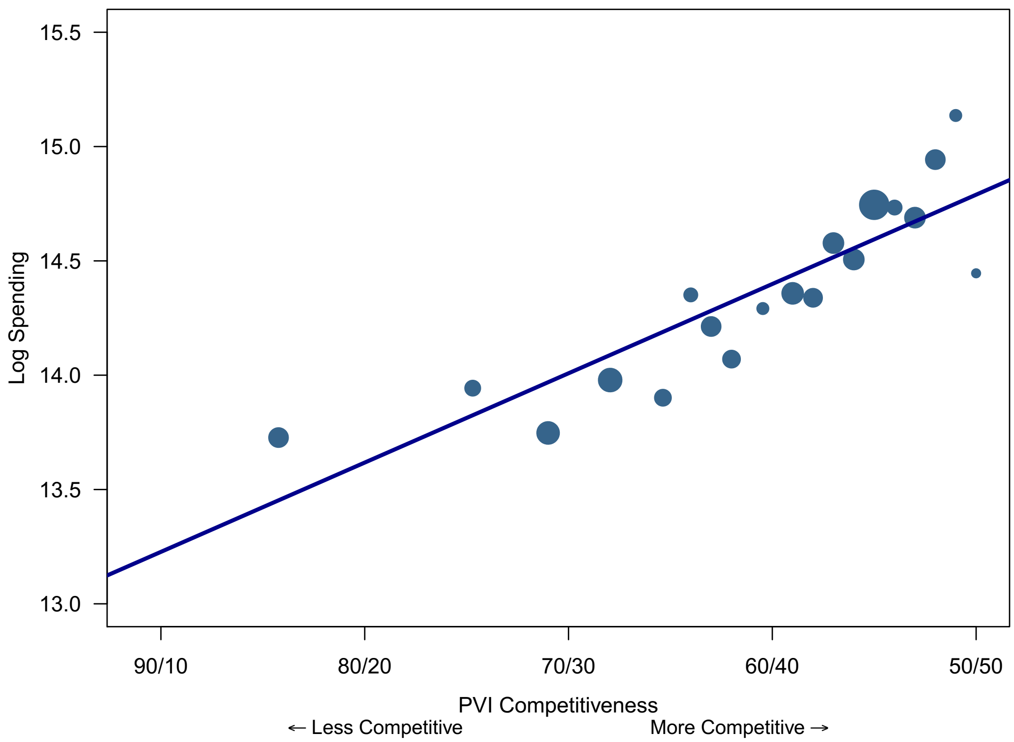


Figure 2.6: Log Campaign Spending vs. PVI Competitiveness. This figure illustrates the positive relationship between competitiveness and spending at the congressional district level. The size of each dot corresponds to the number of districts at that level of competitiveness.

election. While some evidence suggests that advertising increases turnout (e.g., Goldstein and Freedman 2002) and other evidence suggests that (negative) advertising demobilizes voters (e.g., Ansolabehere, Iyengar, and Simon 1999), the consensus in the literature, based on (quasi-)experimental research designs, seems to be that advertising has a very modest effect or no effect on overall turnout (Ashworth and Clinton 2007; Enos and Fowler 2016; Huber and Arceneaux 2007; Krasno and Green 2008; Spenkuch and Toniatti 2015; Vavreck 2007).[47]

In addition to advertising, campaigns also engage in direct contact with voters. In more competitive races, campaigns might deploy more aggressive direct voter outreach efforts. We assess the extent to which voters report more or less campaign contact due to changes in electoral competitiveness using data from the 2010-2014 CCES Panel. Since the CCES

[47]It is important to note that this finding does not mean that spending on advertising is ineffective. For instance, Huber and Arceneaux (2007) find evidence of persuasion effects.

tracks individuals across time, we again employ a panel research design with individual fixed effects and exploit within person variation in competitiveness induced by the redistricting process.[48] Figure 2.7 displays estimates of the marginal effect for a 10-point increase in closeness (e.g., moving from a 60/40 to a 50/50 district) on reported methods of campaign contact.[49] Our estimates suggest that voters situated in competitive districts are more likely to report campaign contact. A 10-point increase in closeness raises the probability of a voter reporting any campaign contact by about 3-4 percentage points.[50] In particular, individuals report additional contact via phone and mail/postcard when situated in more competitive districts. For in-person contact and email/text contact, our estimates for all specifications remain close to zero. Given the costs and logistical difficulties with recruiting volunteers and organizing door-to-door canvassing efforts, it is perhaps not surprising that congressional campaigns do not focus their efforts on in-person contact. Yet, evidence from field experiments suggests that in-person canvassing is one of the most effective forms of contact in terms of increasing turnout. Green, McGrath, and Aronow (2013) pool over 200 published and unpublished get-out-the-vote field experiments, conduct a meta-analysis, and estimate average effects weighted by the precision of the study. They find that in-person canvassing on average raises turnout by 2.536 percentage points; pre-recorded phone calls on average increase turnout by 0.156 percentage points, live calls from commercial phone banks by 0.980 percentage points, and live calls from volunteer phone banks by 1.936 percentage points; finally, "conventional" mailings increase turnout by 0.162 percentage points on average.[51]

We can perform back-of-the-envelope calculations for the effect of competitiveness on

[48]The sample size for the CCES data is considerably smaller than the Catalist data. As a result, our estimates are far less precise.

[49]The estimated marginal effects are based on our preferred specification with state-year fixed effects. See Tables B.35-B.39 in the Appendix to Chapter 2.

[50]For the *PVI Competitiveness* with state-year fixed effects specification, the p-value is 0.06.

[51]The average effect of mailings is an intent-to-treat effect, while the average effects of canvassing and phone calls are complier average causal effects (i.e., the average effect for individuals who open their doors and pick up their phones).

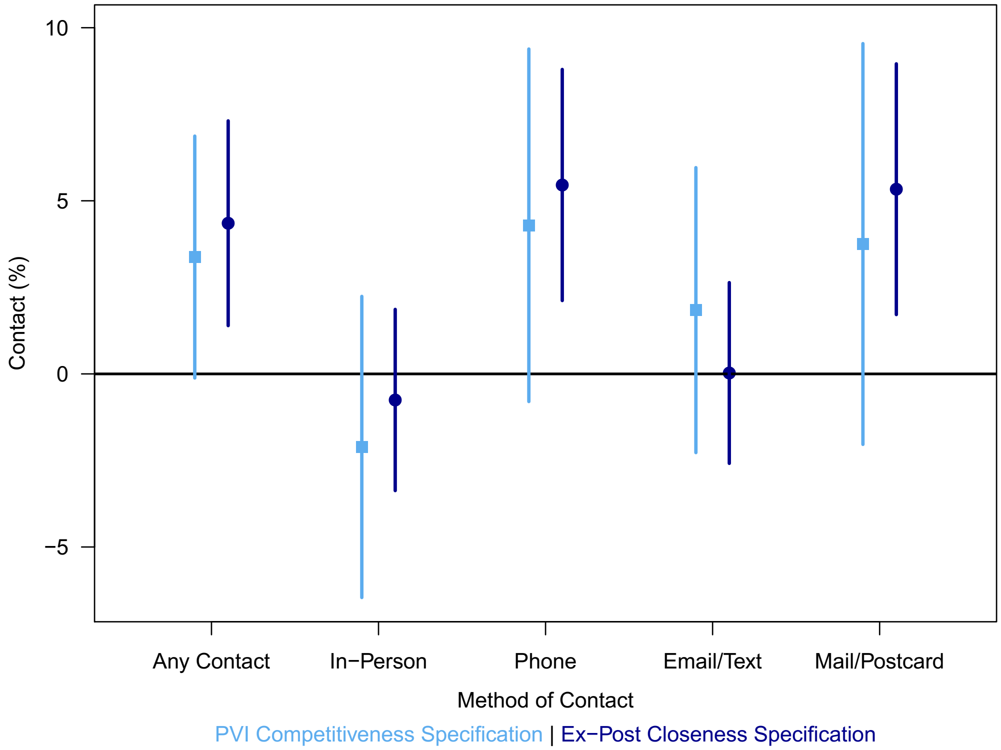


Figure 2.7: Marginal Effect of 10pp Increase in Closeness on Reported Campaign Contact. This figure plots the change in rates of contact reported by individuals in the CCES based on changes in competitiveness. Increases in competitiveness of ten points (i.e., going from a 60/40 to 50/50 election, corresponds with a five percentage point increase in rates of contact overall. Phone contact and mail also appear to increase similarly in response to increased competition.)

turnout through campaign contact. We use our estimates of the effect of competitiveness on reported campaign contact and the average estimates of different methods of campaign contact on turnout from Green, McGrath, and Aronow (2013). If we take the 5 percentage point increase in phone contact (our estimate based on ex-post closeness which is higher than the estimate based on *PVI Competitiveness*) and assume an average effect of 1.936 percentage points on turnout (based on the average effect of volunteer phone calls—the most effective method of phone contact), then increased phone contact from a 10-point increase in closeness would boost turnout by about 0.10 percentage points (i.e., one-tenth of a percentage point).[52] Similarly, if we take the 5 percentage point increase in mail contact

[52] Our calculation is simply: $0.05 \times 0.0193 \times 100\% = 0.0968$ percentage points.

(again, our estimate based on ex-post closeness which is higher than the estimate based on *PVI Competitiveness*) and assume an average (intent-to-treat) effect of 0.156 percentage points on turnout (based on the average effect of "conventional" mailings) with a complier rate of 20 percent (i.e., 20 percent of intended recipients take the treatment and look at the mail), then increased mail contact from a 10-point increase in closeness boosts turnout by about 0.04 percentage points.[53] The combined (back-of-the-envelope) effect of the increased phone and mailing contact is a 0.14 percentage point increase in turnout. This magnitude comports with our point estimates of between a 0 and 0.25 percentage point rise in turnout for a 10-point increase in closeness. That is, the essentially non-existent relationship that we find between closeness and turnout matches up with the minimal effects produced by congressional campaigns' efforts at mobilization in response to increased electoral closeness.

2.7 Conclusion

This chapter calls into question the widely accepted finding of a large, positive relationship between electoral competitiveness and turnout. Previous studies have reported a positive relationship between closeness and turnout across a variety of electoral settings, including the U.S. House of Representatives. Due to data limitations, these studies predominantly rely on cross-sectional research designs at either the individual or aggregate level. We examine data for elections to the U.S. House of Representatives and illustrate that competitive congressional districts are not comparable to uncompetitive districts along a wide range of observable dimensions that are also correlated with turnout rates. Given the degree of imbalance across these observed covariates, the implication is that unobserved confounders are also likely present and have biased estimates of the effect of closeness on turnout in past, cross-sectional studies. By analyzing panel data tracking individuals' turnout decisions across a redistricting cycle, we implement a research design that does not require a selection on observables assumption. As a result of changes to district boundaries from re-

[53]Our back-of-the-envelope arithmetic in this case is: $0.05 \times \frac{0.00156}{0.20} \times 100\% = 0.039$ percentage points.

districting, some voters are situated in more competitive districts, some in less competitive districts, and others in equally competitive districts. Because our panel data tracks the same voters over time, we analyze individuals' turnout decisions under differing levels of competitiveness. Furthermore, the generalized difference-in-differences framework removes all time-invariant confounders. Utilizing our preferred framework, we estimate the effect of closeness on turnout precisely near to zero. The precisely estimated zero effect holds up no matter how we formulate our measure of electoral competitiveness (ex ante or ex post), no matter which years we include in our analysis (midterm years only, presidential years only, or both), and no matter which estimation strategy we use (regression or matching). Furthermore, we run a battery of robustness checks to rule out various threats to inference. The results from our multitude of specifications all support our primary finding: For congressional elections, closeness simply has no effect on turnout.

We next reconcile this finding with existing theories of instrumental voting and elite mobilization. First, available evidence suggests that voters' perceptions of closeness in congressional elections hardly correlate with actual closeness. As many researchers before us have concluded, the instrumental voting logic for increased turnout does not appear to play any role in actual decision-making about whether or not to vote. On the other hand, we do find that elites are aware of and respond to increased electoral competitiveness. Past research has shown, and our results confirm, a sizable positive correlation between closeness and campaign spending. We illustrate, however, that the increased spending does not translate into substantial increases in turnout. Campaigns do increase their efforts to contact and mobilize voters, but for congressional races between 2008 and 2014, the impact of these efforts on turnout is limited.

As a result, our demonstration that closeness and turnout are not related does not serve to invalidate existing theories of elite mobilization; rather, it suggests that the impact of elite mobilization in this electoral context is minimal. It is worth pointing out that our finding does not imply that congressional campaigns are ineffective. The objective of campaigns is not to increase overall turnout. Campaign efforts might persuade voters to support a candidate, encourage supporters to turn out, and dissuade opposing voters from turning out. However, as campaigns continue to target voters more effectively and also bring to bear research on stimulating turnout (as well as persuasion), we may well witness a causal relationship between competitiveness and turnout in future congressional elections.[54] It is an open question (and important area of future research) whether this absence of a relationship between closeness and turnout will persist long into the future.

[54]For instance, Enos and Fowler (2016) find that campaign mobilization efforts in the 2012 presidential election "increased turnout in highly targeted states by 7-8 percentage points, on average, indicating that modern campaigns can significantly alter the size and composition of the voting population."

3 | Divided Government and Significant Legislation, A History of Congress from 1789–2010

3.1 Introduction

Political parties are essential for American democracy. They provide structure to legislative politics and prevent chaos from stalling legislation (Rohde 1991; Aldrich 1995). They simplify the choices voters face, make informed electoral decisions possible (Lupia and McCubbins 1998) and solve collective action problems (Downs 1957). There is the growing concern, however, that the political parties in the American system of government may hamper the ability of government to act, especially when the control of government is divided between two highly polarized parties (Fiorina 1996; McCarty, Poole, and Rosenthal 2006; Mann and Ornstein 2013). Silbey (1996) notes that "divided government stands out in the record for its persistent quality, its importance in [...] political affairs, and its acceptance as a fact of political life." How much do the divisions between the parties and the prospect of divided partisan control of government contribute to the ability of government to pass legislation?

Over the past two decades, a central debate in the study of American politics is whether unified party control is, in fact, more productive than divided government. Mark Peterson's *Legislating Together* 1990 provides one of the first such investigations. He finds that party control of government, without controlling for other factors, does have a substantial effect on the number of laws passed from Truman through Reagan. The signal work in this vein of research, though, is David Mayhew's *Divided We Govern*. In that book, Mayhew develops a classification of significant acts of Congress throughout the post-War era and concludes, somewhat surprisingly, that periods of unified party control of government do not correspond to higher levels of significant legislative accomplishment (Mayhew 1991). Subsequent research has built on and critiqued Mayhew's classification of significant legislation and reexamined this question using alternative measures and methodologies (Kelly 1993; Howell et al. 2000; Clinton 2006; Grant and Kelly 2008; Lapinski 2013).[1] This intriguing approach faces its own complications if the data are not standardized and different databases are on different metrics and scales. Some authors have tried to redefine the problem, arguing that significant legislation is not the right measure of what Congress accomplishes, that different issue areas have different dynamics (Lapinski 2013), that legislation is a response to problems and significance can only be measured against a baseline of what problems face the country (Binder 1999), or that divided control is about blocking not passing legislation and that failure rates are a better measure (Edwards III, Barrett, and Peake 1997). All of these refinements present important observations about the nature and meaning of legislation. Ultimately, though, the literature dissipates into ambiguity, with some authors finding substantial effects of divided control and others no difference between unified and divided control.

The challenge presented by Mayhew's simple observation remains. It appears that divided control is just as productive as unified control, measured as total bills passed or significant bills passed (regardless of domain). We think the problem is not measurement but time. We argue that Mayhew's approach is perfectly valid, but the slice of time is too short and the data too sparse to answer the questions posed. Peterson and Mayhew both focus on the five decades from 1946 to 1991. This era has 22 different Congresses, 9 of which are unified and 13 of which are divided. The limited time frame makes inference difficult, especially since the mid-1960s appear to be unusually productive and perhaps historically unusual. Nearly all other research in this literature focuses on the same era. Clinton (2006) is an exception. Clinton (2006) studies the period 1877-1994 using an item response model

[1] Clinton (2006) and Grant and Kelly (2008) combines multiple summary measures of aggregate legislative productivity to measure productivity.

to assess legislative accomplishment based on a range of other rankings.[2] The contribution of this chapter is to examine whether Mayhew's conclusions—based on data post-1946—are universal throughout U.S. history, using his method to define significant legislation. To this end, we follow the methodology developed by Mayhew, and construct a new dataset of significant legislation for the entire history of the U.S. Congress. We have constructed the database independently from Mayhew's efforts, but we have endeavored to apply the same principles and methodology. As an additional check and for purposes of validation, we compare our dataset to several other databases of significant legislation.

Looking at the entire span of U.S. history, we address two somewhat different questions. First, does divided government have a significant effect on the ability of the national government to pass legislation? This debate has taken on an even larger cast in the discussion among theorists of American governmental institutions over whether parties can capture government (Cox and McCubbins 1993, 2005; Rohde 1991; Aldrich 1995) or whether the median voter remains the pivotal player in the legislative domain (Brady and Volden 1998; Krehbiel 1998). Second, do parties and party control of government offer a substantial explanation of what Congress does and when it does it? Do party-based explanations account for the *historical variation* in legislative productivity?

While this chapter focuses on answering the first of these questions, our newly assembled data also allows for insights into the second question, as well. We estimate the effect of unified and divided control of government on the passage of legislation and of significant legislation throughout the history of the United States Congress—and we assess the stability of this effect in different historical periods.[3]

The long time horizon allows us to see the effects of divided government more clearly. We find that under divided control of government Congress passes fewer pieces of significant

[2] The rankings used include Mayhew's along with an impressive range of other contemporaneous and retrospective sources.

[3] By effect we mean simple effect, that is the difference in the conditional mean between unified and divided control, rather than a causal effect *per se*. The effect we estimate may or may not be causal. Whether it is causal is not our immediate concern, nor is it the question in the literature.

legislation enacted into law — 1 fewer law per Congress in the 19th Century and 4 fewer laws per Congress in the 20th Century. These differences are statistically distinguishable from zero, indicating that unified party control does contribute to legislative accomplishment. However, the incidence of unified and divided control of government throughout the long history of the United States cannot explain the overall historical trends in the passage of historical legislation. Divided party control of government is more prevalent in the second half of the 20th Century than throughout the 19th Century, but much more significant legislation is passed under divided control of government in the 20th Century than was passed under unified control of government in the 19th Century.

To our knowledge, Stathis (2014) represents the only other comprehensive direct attempt to develop a list of significant legislation for the entire history of Congress. Stathis has compiled a catalog of significant legislation, organized by Congress and also indexed by topic. We use Stathis' data as a complement to the database we assembled and as a robustness check. Again, our approach is to test Mayhew's claim, using the same methodology he did, for the entire history of the United States Congress.

Other studies have tweaked the definitions or measurement techniques for "significant" legislation. Howell et al. (2000) divides significant legislation into four different classes: landmark enactments (which correspond to a subset of Mayhew's significant legislation—acts judged significant by contemporaneous sources), major enactments, ordinary enactments, and minor enactments. Based on this criteria and some technical adjustments to the estimation procedure, the authors find that divided government is associated with a 30% reduction in landmark legislation. Similarly, Kelly (1993) argues that the distinction between contemporaneously and retrospectively judged legislation is crucial.Revising the list of significant legislation restores the expected negative effect of divided government.

Another threat to the validity of Mayhew's assertion—and to much of the follow-up work—is that other important explanatory variables have been omitted. Coleman (1999) accounts for several institutional features thought to mediate the influence of unified/divided control such as which party is in control, whether a supermajority exists in the Senate, factionalism within parties, and public mood. Across several different measures of legislative productivity, Coleman finds that on balance unified government does play a role when accounting for important institutional features.[4] Other work in the same vein has examined the role of ideological coalitions cutting across parties (Frymer 1994) and ideological cohesion (Taylor 1998).

Divided government could also influence the character of legislation while not altering the total output of important legislation. Members of Congress might not enact as much "partisan" legislation, essentially deviating from their ideal points in order to facilitate coalition-building so that the legislation will pass (Thorson 1998). Put another way, the legislation passed might shift to less "conflictual" policy areas (Bowling and Ferguson 2001). In addition, a trend towards more omnibus legislation might render simple counts of significant legislation incomparable across eras (Taylor 1998).

The difficulty of determining what is significant legislation may also obscure assessments of how partisan control influences what gets done. Divided government appears to result in increases in congressional investigations (Kriner and Schwartz 2008; Parker and Dull 2009), more protectionist trade policies (Lohmann and O'Halloran 1994; Epstein and O'Halloran 1996), and an uptick in placing on the agenda controversial issues that might harm an opposing party President (Rose 2001). According to these studies, even if we cannot observe a large effect of divided government on significant legislation, other aspects related to legislative productivity may be hurt by partisan divisions.

There is also not necessarily a consensus about the implications of reduced legislative productivity, if it does in fact result from divided partisan control. It could be that the electorate actually prefers divided government, and rationally splits votes between candidates of competing parties in order to ensure maintenance of the status quo (Fiorina 1996). In this telling, citizens are less concerned with seeking out positive reforms and rather prefer to limit government action. Or perhaps voters evaluate Presidential candidates differently

[4]However, the robustness of these findings is constrained by the combination of the limited time period under consideration (the chapter's focus on the post World War II era results limits the data to under 25 observations) and the sizable list of covariates.

than members of Congress, and are willing to vote for a Presidential candidate they disagree with on some issues because they know that opposing members of Congress will constrain his or her actions (Jacobson 1990).

Our contribution with this chapter is to introduce a new, independent dataset on all legislation and significant legislation in order to better assess the various claims made in the partisan control literature. The database presented here covers all Congresses, which allows for better estimation of the effects of party control on legislative productivity, both because there are more data points from 1790 to 2013 than from 1947 to 2013 and because the effect may not be constant across historical eras. In constructing this database, we follow Mayhew's original instincts in coding significant legislation, as that is the work we most directly want to engage. Hence, we develop a database, drawing on historical interpretations, that is developed separately from Mayhew's assessment and from subsequent work that started with and amended or emended that database. A different approach, such as one based on cross-referencing of laws, is possible, but left for subsequent work. Most importantly, by taking the broad sweep of history—all 225 years of Congress—we are able to estimate more cleanly the effect of divided party control on the productivity of government. As we will show divided control does have a statistically meaningful effect on legislative productivity, even taking the approach pursued by Mayhew. However, that broad sweep of history reveals that Mayhew's assessment is ultimately right in questioning whether party control can explain what Congress does because long-term trends in party control are at odds with long-term trends in the number and significance of Congressional enactments. Critics of Mayhew's original conclusion that unified party control has statistically insignificant effects on significant legislation may be right on that narrow question, but upon reconsideration of the evidence a larger problem for the party-control theory emerges. Unified and divided party control cannot explain the broad patterns of legislation in American history, especially the gross differences between the 19th Century and the 20th Century, or even between the first half of the 20th Century and the decades since World War II.

3.2 Data

Evaluating the effects of party control for the full history of Congress presented a number of challenges. To guide our efforts, we assembled our dataset using a simple definition of significant legislation based on meeting one of two criteria. First, is the bill important in historical context? When we look back on the legislation from our current perspective, did this bill accomplish something important, such as establish a major governmental agency, introduce a major policy change, declare war, or pass a constitutional amendment? Second, was the bill viewed as an important legislative accomplishment in its own time? This type of bill is harder to identify, and requires histories or the Congressional Record to determine its importance. For example, some slavery related bills that preceded the Civil War did not have long-lasting significance due to the abolition of slavery, but they were major legislative accomplishments addressing the critical issue of their time. In making these assessments, we relied on historical treatments of the Congress and politics of the time period, such as the Ante-Bellum period, the New Deal, and so forth. For our data collection sources, see Appendix C.2.

Our final database includes 1,040 significant bills that Congress enacted into law.[5] We also use counts of total public and private bills passed in each Congress. For the Congresses between 1789 and 1976 we used Appendix F of Galloway and Wise (1976), and for the remaining years we used counts from the Library of Congress.

While our dataset was collected in a similar manner to Mayhew (1991), our dataset does not contain the exact same significant legislation for the overlapping period. There are some bills in our data that Mayhew did not include, and others that Mayhew included that we did not code as significant. For example, Mayhew's dataset excluded the 23rd Amendment, which was passed by the 87th Congress and granted Washington, D.C. votes in the electoral college.

[5]This excludes major legislation that failed and was vetoed and not overridden, judicial nominations, and treaties.

Figure 3.1 plots the number of significant bills by Congress in our dataset and the datasets utilized in Mayhew (1991) and Howell et al. (2000) from the 79th Congress to the 104th Congress. The figure shows that our data roughly corresponds to these other data sets. The Howell et al. data was created by supplementing the Mayhew data with additional legislation. Therefore, the Howell et al. counts of significant legislation are always higher than the Mayhew counts. For a majority of the Congresses, our data falls in-between these two datasets. As an additional validation test for our data collection before 1945, Figure 3.2 compares our data to counts of significant legislation from Stathis (2014).[6]

[6] In almost all Congresses Stathis (2014) codes more legislation as significant than in our dataset. This is partially due to the inclusion of major treaties, which we exclude, as well as to different criteria for determining significance.

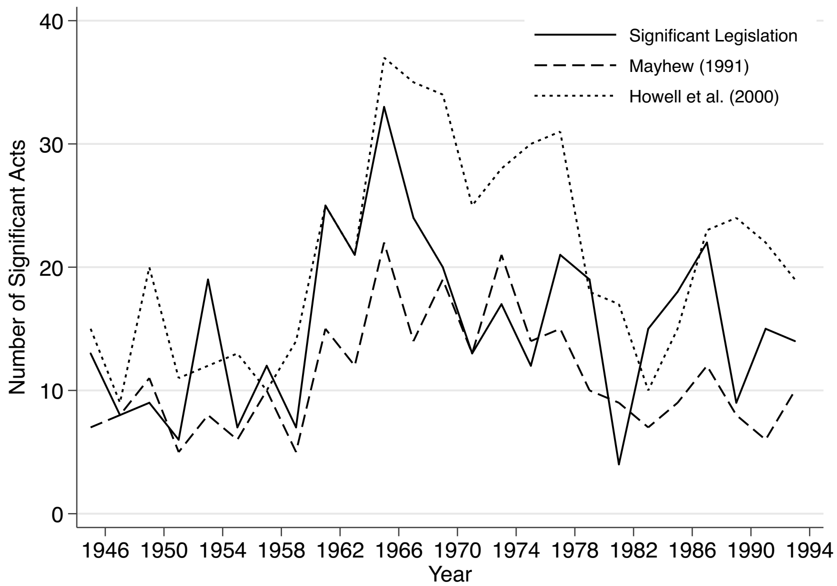


Figure 3.1: Comparison of Significant Legislation to Mayhew. This figure displays our measure of significant legislation in comparison to Mayhew (1991) and Howell et al. (2000). Our measure correlates with Mayhew at 0.6855 and with Howell et al. at 0.6287.

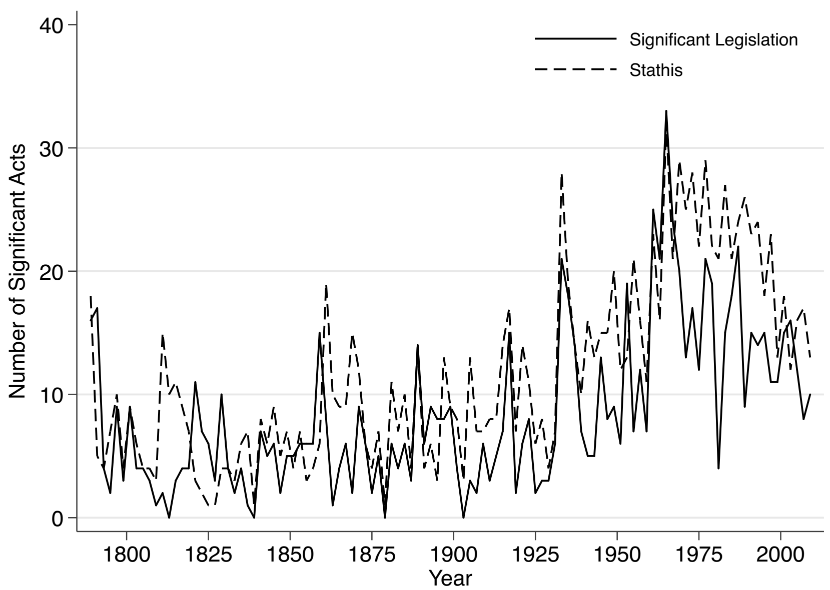


Figure 3.2: Comparison of Significant Legislation to Stathis. This figure displays our measure of significant legislation in comparison to Stathis (2014). Looking back across all past congresses, our measure of significant legislation correlates with Stathis at 0.6825.

3.3 Trends in Legislative Action

In 1789, Congress felt the need to act. The Constitution was a crude architecture, not a complete plan of government. When the members of the First Congress initially convened there were no national laws governing the budget, economy, citizenship, federal crimes, or many other domains that today we take as given. The Constitution had left large portions of the federal government undefined, especially the President's cabinet and the organization of the judiciary. The First Congress could not help but pass significant legislation, as they started on a nearly blank slate. Without federal legislation to enable the functioning of the judiciary and executive, the new constitution would likely have failed. Following the incredible productivity of the First Congress (and the Second Congress, which continued the essential work of implementing the Constitution), what explains when Congress does and does not act? The conjecture in *Divided We Govern* is that the partisan organization of Congress and the Presidency explains a substantial portion of the variation in when Congress acts and when it does not. Before assessing that conjecture, we first examine the overall patterns of legislation and significant legislation over time.

The nature of legislation has evolved substantially from the First Congress. Early bills and acts often were not named when they were introduced. In fact, the first bill introduced into the new Congress was an act to levy fees on the tonnage of ships introduced by Mr. Adams of Massachusetts. The resolution simply lists various types of vessels on which tonnage fees were to be charged, but actual fees are left as blanks to be filled in later. Congress also often proceeded in an ad hoc manner. Appropriations, for example, were made on a need basis; there was no budget process. An act to fund a specific activity or project would ask for a certain amount to be spent on that activity. Internal improvements were not approved in omnibus bills but were taken up one by one — a lighthouse here, a harbor dredged there. Many of these idiosyncratic actions fall out of the scope of "significant legislation" because they do not rise to the level of singularly important actions taken by Congress. Cumulatively, though, they are important.

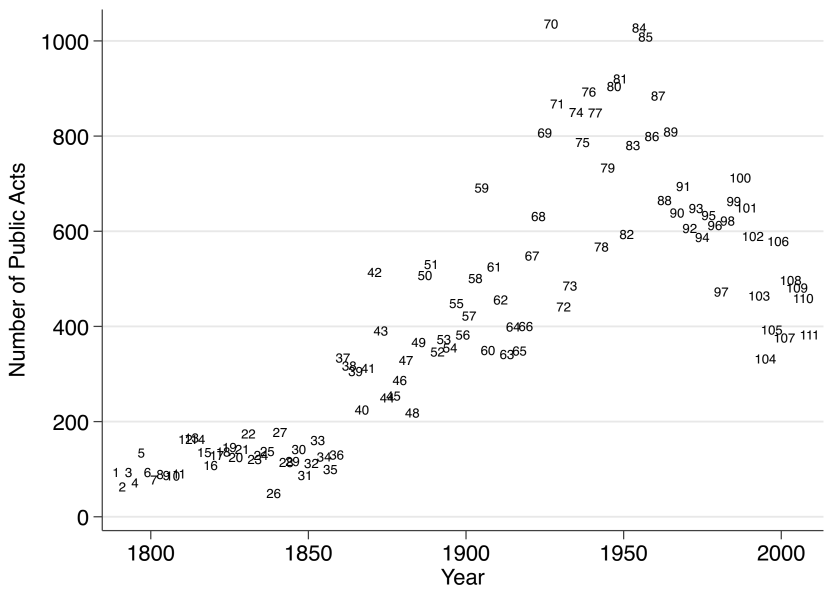


Figure 3.3: Public Acts. This figure displays the total number of public acts plotted over time.

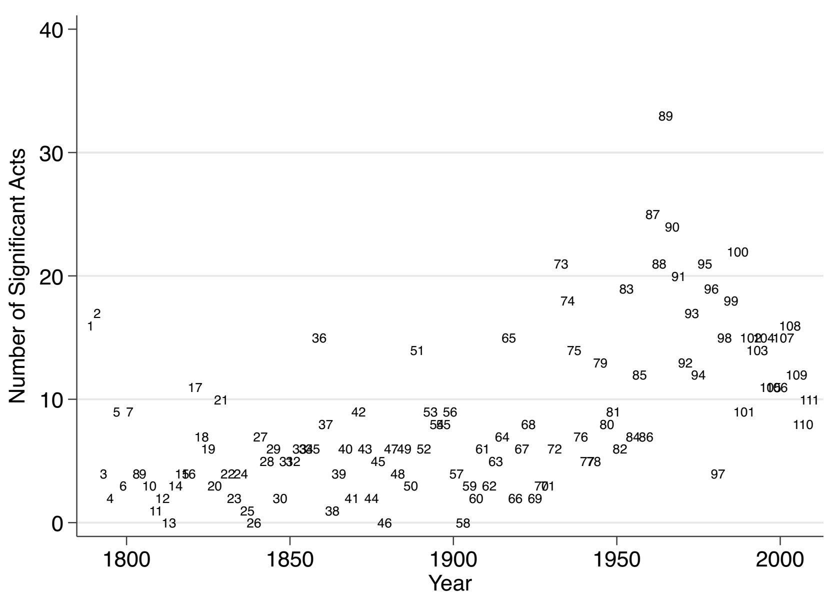


Figure 3.4: Significant Acts. This figure displays significant acts of Congress plotted over time.

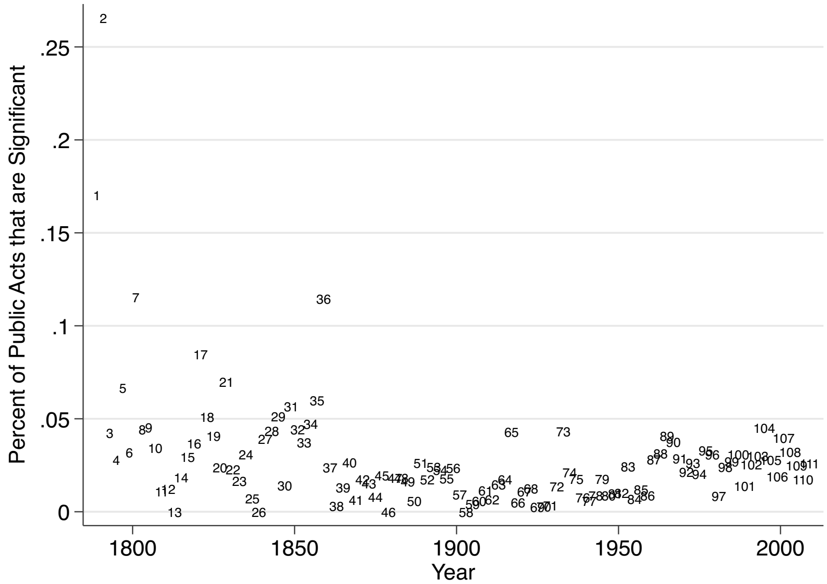


Figure 3.5: Significant Public Acts. This figure displays the percent of all public acts that are significant.

Over the decades, legislation has become more rationalized and bureaucratic. Bills become longer and more specific. Congress eventually came up with a more comprehensive approach to budgeting. Perhaps the clearest example of the rationalization of legislation is the treatment of private bills. Throughout the 19th Century, Congress used private legislation to pay for military pensions, benefits for military widows, compensation for property, and a variety of other particular transactions (Skocpol 1993, 1995). The number of such transactions grew exponentially over the decades following the Civil War, and Congress eventually decided to create a pension law to get the thousands of requests for relief off of the legislature's agenda.

The changing nature of legislation is not as cleanly reflected in our measures of total and significant acts. But, the evolution of the form of legislation and nature of statutory law is an important feature of the history of Congress. It is worth flagging how the changed nature of legislation might affect the picture of various trends. A law that creates a comprehensive approach to such private legislation becomes a significant act, but the many private bills leading up to it are not. The many ad hoc appropriation bills in the first half of the 19th century do not rise to the level of significance, but the budget acts that rationalize the process do.

The growing rationalization of legislation and government are worth keeping in mind when considering the historical trends in legislation. We gauge the amount of legislation and the number of significant laws passed in each Congress. There are also important changes in the content or nature of legislation that are not reflected in these trends. However, each time that Congress moves to rationalize a legislative arena, such as appropriations or pensions or the creation of committee systems, it frees up time for the entire legislature to address other matters. Hence, it may be the case that the growing rationalization of the legislative process itself creates the capacity — but not the need — to create more legislation in the future.

Congress passes two sorts of acts, public acts and private acts. Public acts take the form of statutes, judicial and executive appointments, approval of treaties, and other actions that have the standing of public laws. Private acts are actions taken by the legislature on behalf of individuals, such as a property transaction of the federal government with an individual or a grant of a special privilege, such as a pension, to an individual. Scholars usually refer to public acts when making claims about congressional action. In fact, most theoretical work really pertains just to statutes. Figure 3.3 presents the number of Public Acts passed by each Congress from 1789 to 2012. Each Congress is noted by its number. This is simply the total number of acts passed and does not depend on classifications of significance.

The patterns in Figure 3.3 help us put David Mayhew's original study of divided government in context. Mayhew's study began with the 78th Congress, which passed approximately 600 acts. The succeeding 20 years saw a rapid run up in legislative action cresting with the 84th and 85th Congresses, which produced over 1,000 acts each. Since then, there has been a steady decline in total legislation passed, and the number of public acts passed today is less than half the number passed in the peak years of the late 1950s and early 1960s. Interestingly, the low number of bills passed in the 111th and 112th Congresses appear predictable from the steady trend downward in number of laws passed since the summit in 1959.

The figure also reveals that the post-World War II period differs markedly from what had come before. In terms of total legislative output, there appear to be four periods of congressional history. During the Ante-Bellum period (1789 to 1861), a typical Congress passed only 150 public acts. Despite their obvious importance, the first two Congresses were not that productive. And, the true Do Nothing Congress was the 26th, which managed to pass only a few dozen public acts. From the Civil War through the end of World War I (1862 to 1925) there was a steady rise in the number of public acts from 200 to 500 acts per Congress. This is an era of rapid industrialization in the nation and, interestingly, corresponds almost exactly to the period that Skowronek identifies as the era of the development of the American national executive (Skowronek 1982). Then, in 1927-29 comes a quantum leap in the number of public acts passed by Congress. Congress maintains that very high level of productivity from 1927 through 1966, an era described by some as the Modern Era in Congress, and also the era of modernism in many other aspects of public and private life. This era also coincides with the rise of the conservative coalition, the partisan realignment that leads to the ascendancy of the Democratic party nationally, and the beginning of the incumbency advantage. The post-modern Congress takes hold in 1967. Legislative activity drops substantially from 1965-66 to 1967-68 and has continued to trend downward since. By 1968 a new political alignment has begun to take hold, which Aldrich and Niemi (1996) (among others) characterize as a protracted period of partisan dealignment, rising incumbency advantages and campaign expenditures, and growing public dissatisfaction with Congress. The levels of legislative output in the 112th Congress, which has triggered a new round of criticism of the institution, are back to the levels associated with the period from 1870 through 1920, and the number of public acts in the most recent Congresses continue a trend begun in 1967.

This broad picture of law-making exposes several puzzles. Why the jump in legislative activity in the 1920s? Why the downward trend in legislation since the 1960s? It surprised us that the most productive Congresses are the 70th (1927-29) and 84th (1959-61), not, as we might have guessed, the 73rd (1933-35) or 89th (1965-67). Furthermore, the 97th

Congress (1981-83) had much less legislative action than we expected. We are also struck by the tremendous differences between the 19th and 20th Centuries, made all the more striking by the fact that the First Congress appeared on our first reading to play such an important role in the development of institutions and government.

The incidence of significant legislation tells a subtly different story about Congress. Figure 3.4 presents the history of Significant Acts passed by Congress. Each point in the plot is a Congress. This graph consists of all public acts determined by our project to be significant acts of Congress.

The same general patterns emerge in both total and significant legislation. The 19th Century produced much less significant legislation than the 20th Century. The amount of significant legislation passed by a typical Congress rises from the end of the 19th Century through the middle of the 20th Century, peaks in the 1960s and then steadily declines. Today the number of significant acts passed by a typical Congress is now back to the levels typical of the end of the 19th and beginning of the 20th Centuries (Congresses 55, 56, and 57), but still above the historical average.

The peaks, however, in significant legislation are notably different. The First and Second Congresses stand above the rest of the 19th Century in terms of number of pieces of significant legislation passed. From the Age of Jackson to the New Deal, the 36th (1859-61), 51st (1889-91), and 65th (1917-19) Congresses stand out as passing substantially more significant legislation than other years in the same era. There are tremendous jumps in the numbers of significant acts with the advent of the New Deal (the 73rd, 74th, and 75th Congresses) and the creation of the Great Society programs (the 87th, 88th, and 89th Congresses). In these two eras Congress passed very large numbers of acts that had long-lasting significance to the nation. There are historical explanations as to why these bursts of activity occurred. The political science explanations are much less compelling and powerful.

There is another way to understand the incidence of significant legislation, and that is as a percent of total legislative output. One simple story is that no Congress is particularly special in its ability to produce significant legislation, but that the more legislation a Congress passes the more likely it is to pass significant legislation. This is perhaps the "dartboard theory of Congress." The more darts one throws the more likely one is to hit a bullseye. Figure 3.5 presents this alternative view of the history of Congressional legislative output. We simply took the ratio of Significant Acts to Total Public Acts for each Congress. We view this ratio as a measure of the Effectiveness of Congressional Action.

Viewed from this perspective the Ante Bellum Congresses are exceptional. The First and Second Congresses were, by far, the most effective legislative sessions in our history. One in six acts passed by the First Congress were deemed significant, and one in four acts passed by the Second Congress were determined to be significant. For that reason alone, they deserve special attention. But the rest of the Ante Bellum era also appears to have been unusually effective. The 7th Congress (1801 to 1803), the 17th and 18th (1821-1825), and the 36th (1859 to 1869) had unusually high percentages of significant acts. These Congresses dealt with the expansion of the nation (especially the admission of states), reorganizations of the executive, and the recurring problems of Slavery and Indian relations. The problems pressing on the country, then, meant that they seem bound to pass significant legislation. But this is also an era in which Congress, on occasion, ground to a complete stand still, as with the 13th, 26th, and 38th Congresses. The significance of these early Congresses, though, points to a weakness with prior inquiries. Much happened before 1946 or 1877 that ought to inform how we think about what Congress does and when it does it.

Setting aside the pre-Civil War Congresses, another pattern emerges — really the lack of a pattern. From the 37th to the 112th Congress, the percent of legislation that one might consider significant hovers around 2 to 3 percent of all acts passed. Even the 73rd, 87th, and 88th Congresses show a very low level of effectiveness. In all three cases, less than 5 percent of all legislation was considered significant. There does, however, appear to be a slight difference between the pre-New Deal and the post-New Deal Congresses. The rate at which Congress passes significant legislation is slightly higher since the New Deal.

The patterns in Figures 3.3 to 3.5 provide us some confidence in the coding of significant legislation. Figure!3.5 demonstrates that our database of significant legislation does not suffer from “recency bias”. In percentage terms, bills from the 19th century are no less likely to be considered significant than bills from more recent times.

The patterns displayed also lay out the foundations for the second stage of our inquiry: Explaining why Congress does what it does when it does it. Professor Mayhew laid down an important conjecture—divided government affects the ability of Congress to legislate, and especially the ability of Congress to pass significant legislation. In the next section we estimate how large an effect unified or divided control of government has on the rate at which Congress takes historically and politically significant actions. Nonetheless, it is also important to place the “marginal” effect of unified or divided control within context. Our examination of the overall historical patterns suggests that unified partisan control cannot explain the broad contours of legislative productivity. In Figure 3.4, the 91st and 100th Congresses—both divided—passed as many significant laws as the 73rd. But on the margin there does seem to be a relationship. The First and Second (unified) are more productive than the Third and Fourth (divided), and so forth.

Before turning to the question of divided government, one final comment about the overall patterns here is in order. The rise in productivity in Congress in Figures 1 and 2 corresponds quite closely with the decline in polarization in the House and Senate, and especially with the percent of legislators from each party who are “overlapping” — that is Democrats to the right of at least one Republican and Republicans who are to the left of at least one Democrat. In particular, Poole and Rosenthal 1997 identify the 70th Congress (1927-29) as the beginning of a substantial decline in polarization within the Congress and after the 90th Congress polarization gradually increases. This era from 1927 to 1973 is often looked back on as the standard for how Congress ought to behave by commentators such as Thomas Mann and Norman Ornstein 2013, and it does appear that broad historical fluctuations in polarization correspond with broad ebbs and flows in the tide of significant legislation. The correlation, at least from 1879 to 2012, appears obvious to us, but the causality is less clear as roll call votes and significant legislation are both outputs of the same legislative process.

3.4 Effects of Divided Government

How does divided government influence legislative output? Over the more than 220 years of Congress, the legislature produced an average of 8.24 pieces of significant legislation when the control of government was divided among the parties and 8.58 pieces of significant legislation, roughly one-third of an additional significant act, when there was unified party control of government.[7] While this comparison of means is in line with the idea that unified party control leads to slightly greater legislative productivity, the difference is not large enough to support the conclusion that legislative output depends on party control in a systematic way: the 95% confidence interval on the difference in means includes zero. That difference also does not take into account systematic variation in trends and levels of legislation over time. And, furthermore, divided control of government actually yielded more total legislation (public laws) than unified control did. See Table 3.1.

Table 3.1: Mean Legislative Output per Congress

Party Control	Total Legislation	Significant Legislation	Obs.
Divided	421.02	8.24	42
Unified	407.74	8.58	69

Breaking out legislative output by era corrects for variation in overall legislative product across different periods of the history of Congress. We divide the data into four eras: pre-Civil War (1st-36th Congress), post-Civil War but pre-1900 (37th-55th Congress), the turn of the century to the end of World War II (56th-79th Congress), and post-World War II (80th-111th Congress).[8] Across all four eras, unified government is associated with an uptick in significant legislation; however, the magnitude of the increase varies substantially depending

[7]In most cases, assessing whether Congress operated under a divided or unified government was straightforward. One exception was the 20th Congress when John Quincy Adams held the Presidency as a Democratic-Republican and factions such as the Jacksonians were splitting off from the party. We coded this Congress as unified. That said, coding it the other way makes no material difference in our results.

[8]Our choice of eras is based on beliefs about structural breaks in the history of the United States. For details on a more principled approach to determining structural breaks in politics and history see Wawro and Katznelson (2014).

on time period. In the pre-Civil War era, the difference between unified and divided party is under half a bill — less than a 10% increase. In the second period, the gap between unified and divided control is almost three bills, which represents a more than 60% increase in output. In the third period the gap has narrowed slightly but by the post-World War II period it has widened to a difference of five bills — an over 40% increase in productivity. Thus, we observe that unified government resulted in additional significant legislation both before and after 1900. The data follows a similar pattern when we turn to total legislation, with one key exception. In the post-World War II era, unified governments have actually produced less total legislation when compared with divided control. The other noticeable trend for total legislation is the existence of a general upward trend over time.

Table 3.2: Mean Legislative Output per Congress, by Era

Era of Congress	Party Control	Total Leg.	Significant Leg.	Obs.
1st-36th	Divided	115.00	5.20	10
1st-36th	Unified	120.92	5.54	26
37th-55th	Divided	312.30	4.40	10
37th-55th	Unified	395.22	7.00	9
56th-79th	Divided	412.25	6.50	4
56th-79th	Unified	634.20	7.05	20
80th-111th	Divided	653.39	12.44	18
80th-111th	Unified	624.93	17.43	14

The comparison of means obscures some crucial factors related to legislative output that we must account for when assessing legislative productivity. First, as detailed in the previous section, we observe some sharp differences across time in legislative output driven by factors unrelated to party control; as a result, any comparison of productivity between divided and unified government must account carefully for the time trends in legislative output. We attempt to address this issue with two different approaches: by including indicator variables for the era of Congress and by taking first differences and looking at changes in legislative productivity after changes in party control. A second concern is that comparing across Presidential terms may overlook the fact that historical circumstance, effectiveness of a President's administration, or both play a role in legislative output. For example, Congress' legislative productivity during FDR's first 100 days is perhaps not directly comparable to the first 100 days of Jimmy Carter's administration. If the effectiveness of a President's administration happens to be correlated with party control, then we might wrongly attribute an increase in legislative productivity to unified or divided government. By including President fixed effects, we can estimate the effect of variation in party control on legislative output within a President's term, which rules out differences due to different administrations.

We use OLS to estimate the effect of unified government on legislative output and present the results in Table 3.3. The main result, illustrated in models 5–8, is that unified government is associated with between roughly 2.5 and 3 additional pieces of significant legislation as compared to divided government when we include era dummy variables. This effect is substantively large. Considering that Congress has averaged fewer than 9 significant pieces of legislation during divided control, the observed effect of unified control represents an increase of more than one third. In fact, if we log-transform legislative output and re-estimate the model, unified government is associated with an even larger percentage increase in significant legislation for our preferred specification: Table C.3 in the Appendix suggests that instances of unified control, when compared to divided control in the same presidential administration, coincided with an increase in significant legislation of 38%. Conversely, we do not find consistent evidence that unified government affects total legislation (Models 1-4).

Including the period dummy variables plays an important role in the estimation of unified government's effect on significant legislation, especially with regard to legislative output since the end of World War II. Before the 80th Congress, there were 24 cases of divided government and 55 cases of unified government. After the 80th Congress the numbers were more equal with 18 cases of divided government and 14 cases of unified government. The fact that there have been proportionally more cases of divided government since 1945, combined with Congress' tendency to produce more legislation over time, means that not accounting for the systematic differences in eras would lead us to potentially underestimate the effect of unified government for the full time period.

Examining the models in Table 3.3 more closely, we note that the effect is robust to

Table 3.3: Divided/Unified Government and Legislative Output

	Total Legislation				Significant Legislation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unified Government	-13.28	41.06	20.22	24.98	0.34	2.41	3.06	2.96
	(51.70)	(27.49)	(31.00)	(22.99)	(1.19)	(1.05)	(1.34)	(0.88)
37th-55th Congress		242.51	148.50	453.03		0.79	-2.50	7.11
		(21.61)	(7.85)	(91.84)		(1.05)	(3.23)	(3.43)
56th-79th Congress		473.37	82.50	442.04		1.25	-1.50	6.88
		(41.88)	(7.85)	(89.43)		(1.34)	(3.23)	(3.35)
80th-111th Congress		533.35	161.91	650.20		9.87	-5.81	9.19
		(33.77)	(119.51)	(47.41)		(1.38)	(3.62)	(2.13)
President FEs	No	No	Yes	No	No	No	Yes	No
Decade FEs	No	No	No	Yes	No	No	No	Yes
Observations	111	111	111	111	111	111	111	111
R^2	0.001	0.723	0.912	0.891	0.001	0.417	0.730	0.719

Robust standard errors in parentheses

several different specifications. When we look at the effect of unified government only *within* the same Presidential administration, we still estimate an effect of 3 additional significant acts. Similarly, when we use decade fixed effects—designed to control for any variation in how data on significant legislation was gathered from decade to decade—the effect also remains stable at 3 additional acts under unified government.

We also re-estimate the results using a polynomial time trend rather than era dummy variables. Table C.4 in the Appendix presents these results. Using a time trend rather than era dummy variables does not substantively alter our finding on the effects of unified/divided government. Our estimates using this alternative specification find that unified government is still associated with approximately 2 to 3 additional significant acts depending on the specification.

Studying the effect of a change from unified (divided) control to divided (unified) control provides additional evidence that party control of government influences the output of significant legislation. Taking first differences essentially eliminates time trends from the data.

Changes in legislation from Congress to Congress appear to follow a stationary process with a mean centered at zero and close to constant variance over time.

Table 3.4: Changes in Divided/Unified Government and Legislative Output

	Δ Total Legislation			Δ Significant Legislation		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Congresses						
Change to Unified Government	29.95	26.88	29.64	3.02	2.48	3.04
	(17.67)	(19.08)	(18.20)	(0.92)	(0.90)	(0.92)
Observations	110	109	110	110	109	110
R^2	0.022	0.045	0.029	0.103	0.245	0.105
Panel B: 1st–55th Congress						
Change to Unified Government	44.63	43.79	44.12	1.75	1.33	1.77
	(13.32)	(14.13)	(13.80)	(1.03)	(0.93)	(1.02)
Observations	54	53	54	54	53	54
R^2	0.166	0.170	0.172	0.063	0.248	0.066
Panel C: 56th–111th Congress						
Change to Unified Government	10.39	5.30	10.39	4.72	4.01	4.72
	(36.96)	(40.54)	(37.87)	(1.58)	(1.63)	(1.58)
Observations	56	56	56	56	56	56
R^2	0.001	0.031	0.007	0.157	0.273	0.158
Lagged DV	No	Yes	No	No	Yes	No
Time Period Controls	No	No	Yes	No	No	Yes

Robust standard errors in parentheses

Using OLS to estimate the effect of a change in unified government suggests an increase in significant legislation of more than 3 acts (Panel A in Table 3.4). This finding is robust to including a lagged term for significant legislation as well, under the theory that Congress' momentum from previous years might play some role in legislative output. Again, the estimated effect is substantively quite large—an increase in significant legislation on the order of one-third.[9] To place the result in context, consider that Congress has changed from divided to unified control or vice versa 42 times (21 times from divided to unified

[9]Panel A of Table C.5 in the Appendix shows effect sizes when the dependent variable is log-transformed. Thus, the coefficient for the unified government variable gives the percentage change in significant legislation attributable to a shift.

and 21 times from unified to divided) and that each change is associated with a gain or loss of between 3 and 4 pieces of significant legislation. All told the estimates suggest that Congress' legislative record might be markedly different were there substantially more years of uninterrupted divided or unified control. Also, in contrast to the findings for significant legislation, the results suggest that a change in party control has no meaningful effect on changes in total legislation.

Breaking out the estimated effects by era shows that the effect of a change to unified government has not been constant over time. Panel B in Table 3.4 re-estimates the model using only sessions of Congress from before 1900. Unified control in this era was associated with between 1 and 2 additional pieces of legislation. On the other hand, we observe a larger and more robust effect for 1900 and after. Panel C in Table 3.4 reveals that changes to unified government coincided with an uptick in legislative productivity of 4 bills for Congresses that convened in 1900 and after.

Part of the explanation may be that Congress as a whole has done more after 1900—Congress averaged 200 public acts before 1900 and over 620 public acts since. Indeed, Panels B and C of Table C.5 in the Appendix confirm that on a percentage basis the effect of unified government before 1900 is not significantly different from the effect after 1900. Both estimates hover around 30% depending on the specification. Thus, if we take into account the upward trend in the number of public acts and significant legislation over time, the effect of unified government on its own does not appear to have changed drastically over time.[10]

In sum, examining the results for the entire history of Congress, we find very substantial effects of changes in party control on the passage of significant legislation, but do not find a consistent effect of such changes on the passage of total legislation. These contrasting results underscore the value of studying significant legislation, as opposed to all legislation. Congress passes many symbolic acts, such as naming a post office, declaring a "day" in order

[10] One exception: it appears changes to unified government may indeed have coincided with greater output of total legislation in the era before 1900.

to recognize a particular cause or industry, or a resolution that lacks force of law but expresses the legislature's concern about an issue. Members of Congress have no trouble voting for such inconsequential bills. It is when Congress grapples with a substantial change in the nation's laws that we see the effects of partisan politics in clearer relief. When government has changed from divided to unified partisan control, on the whole there has been a 30 to 40 percent increase in the number of significant laws passed. While the level effect has grown in line with the upward trend in legislation, the effect as a share of significant legislation has remained stable over time.

Given that eras of unified government coincide with increased legislative productivity, a next step is to ask what mechanisms best account for the observed upticks in productivity under unified control. Does having a filibuster-proof majority in the Senate make a difference to legislative productivity? Does the size of the deficit constrain Congress as it seeks to pass significant legislation? Does variation in party control in previous Congresses have any important influence? Or, is the simplest possible explanation—having a majority across both chambers of Congress—sufficient to explain variation in productivity during moments of unified control?

To test these hypotheses, we examined the relationship between legislative productivity and several additional variables: an indicator for a supermajority in the Senate, the size of the budget deficit as a share of federal outlays, time since the President's party last held unified control, time since the party opposing the President last held unified control, and time since the last instance of divided control.

The intuition for the effect of a supermajority is most straightforward. When there is unified control across all branches of government, a filibuster in the Senate might nonetheless lead to gridlock. Having a filibuster proof majority (more than two-thirds since 1917 and more than three-fifths since 1975) would prevent this type of gridlock. To test the degree to which a supermajority makes a difference for legislative productivity, we estimate a model that includes indicator variables for unified government, a supermajority, and, finally, the interaction between the two. A coefficient with positive sign for the interaction term would suggest that Congresses in which there was unified party control and a Senate supermajority coincide with increased legislative productivity. Table C.6 in the Appendix confirms our expectations. We observe a strong positive effect for the interaction term between unified control and supermajorities when significant legislation is the outcome variable. The marginal effect of unified government on its own (without the interaction term) is still positive, but its 95% confidence intervals now include zero. In combination, this suggests that a significant part of the effect of unified government that we have demonstrated is attributable to Congresses in which there was also a Senate supermajority.

Budgetary issues could serve as another way in which institutional constraints shape the effects of unified government on legislative productivity. If the government has run a large deficit in the previous Congress, pressure to balance the budget might reduce possibilities for significant action. Table C.7 demonstrates that this explanation does not carry much weight. In fact, in moments where a higher deficit (in terms of percent of federal outlays) has coincided with unified government, Congress has been more likely to pass significant legislation. This account is consistent with the notion that periods of spending with less regard for short term deficits are associated with higher legislative productivity (i.e., Congresses during the New Deal and Great Society).[11]

Finally, long stretches of opposition party control or of divided government might influence subsequent levels of legislative productivity during periods of unified control. Specifically, a unified Congress and President might have less to do if they held unified control very recently; on the other hand, a long stretch of divided control or of the opposition party holding unified control might elicit a wave of legislation designed to make up for past gridlock (divided control) or to undo previous legislation (unified opposition control). Table C.8 in the Appendix presents the results from this model. With the caveat that focusing on unified government only has reduced our sample size, we find no evidence that who controlled Congress previously makes a difference during periods of unified control.

[11]We estimated the effects since 1901 as these were the years for which yearly budget deficit data was available.

One possible concern about our analysis is that the results are dependent on our particular approach to collecting and identifying significant legislation. To reduce this concern, we replicated our analysis using counts of major legislation from Stathis (2014). Appendix C.1 presents these results. In Table C.1 we replicate the analysis of the effect of unified government on significant legislation (Table 2). When we include data from all congresses, the coefficients are all positive, but the results are only statistically significant in one of the four model specifications. However, when we subset the data to only include our first three time periods from the 1st to 79th Congresses, the results are significant across all specifications. As shown in Figure 2, our data differs more from Stathis' data in the later congresses; Stathis finds more bills to be significant. The difference in results is thus attributable to the coding of recent legislation. In Table C.2, we replicate the analyses in Tables 4, 5, and 6, where we measure the relationship between a change in unified government and a change in significant legislation. We find a statistically significant effect for all congresses and for the 1st–55th Congresses period, but the results for the 56th–111th Congresses alone are not significant. Overall, the models with the Stathis data confirm our findings, and show that when the full legislative output of Congress is studied, there is a positive effect of unified government on productivity. While we see differences in the effect of unified government in legislative productivity for the post-war period, this matches the literature, where the result is dependent upon the coding of significant legislation.

3.5 Conclusion

Today it seems taken as axiomatic that the political parties in the United States have created a dysfunctional legislative process that is incapable of passing legislation, let alone legislation of any significance. This line of thinking, taken as a broad argument about American government, would predict that when government is divided and when parties are polarized ideologically, little can get done. This analysis puts the current Congresses in historical context and shows Congress to be more productive than popular accounts would have the public believe.

Our assessment is that there is definitely an effect of divided partisan control of government on the likelihood that Congress passes significant legislation. Congress passes about 30 percent more significant acts when the House, the Senate, or the Presidency are controlled by the same party than it does when different parties control the branches of government. Interestingly, that is not the case for all bills, only significant legislation. However, this finding does not mean divided government is completely dysfunctional. Instead of passing 12 significant laws, which is the average for unified government in the 20th Century, a divided government typically passes 9 significant laws. Even in a Congress that many observers described as the most dysfunctional ever, the House and Senate managed to hammer out a complete rewriting of the U. S. Patent Law, redefining what is patentable in the country.

The importance of party control comes into sharper focus once we take a long historical perspective, but so too do the limits of party-based arguments about law-making. In particular, party control of government cannot explain most of the variation in what Congress has done throughout U. S. history. Party control cannot explain why there is much more legislation and much more significant legislation in the 20th Century than in the 19th Century. Nor can party control explain the surge in significant legislation and overall legislation from the 1930s to the 1960s and the ebb in significant legislation since. It cannot explain why the 1960s are so much more productive than the 1930s or the 1890s. It cannot explain why unified party control during the 19th Century is less productive than divided party control in the 20th and 21st Centuries. It cannot explain the legislative accomplishments of Congress during the Nixon years, compared to more recent spells of unified government under Bill Clinton, George W. Bush, and Barack Obama. And it cannot explain the steady downward trend in legislative productivity in the US since 1970, a trend that all Congresses — whether during unified or divided government — seem incapable of escaping.

A closer look at the data make even clearer the inadequacy of party control of government as an explanation for the broad trends in legislative activity throughout the history of Congress. First, unified control of government occurred more often during the first 110 years of Congress (65 percent of Congresses were unified) than for the next 115 years of Congress (61 percent). Second, unified party control, without including any other factors in the regressions, explains a tiny percent of the total variation. When we model the data using just party control of government, the R-Squared is less than 1 percent. Third, when the models include simple time trends (four eras of Congressional history), most of the variation in the data is explained. Fourth, indicators of Presidencies account for another 20 to 30 percent of the variability. Who is president, then, may be much more important than whether control of government is unified in the hands of one party or divided between them. We offer no causal or theoretical account for these eras, except to note that they explain 40 to 70 percent of the systematic variation and are far more important than party, a finding consistent with Peterson's *Legislating Together*. Why might that be the case? We leave that question unanswered – a challenge for future theorizing and empirical investigation. The study of Congress and legislation is usually inward looking, focused on the institution itself. The importance of the individual president (rather than the party of the president) in explaining the variation in legislative productivity suggest that forces outside the institution itself are essential to understanding what Congress does and when it does it.

The data marshaled here have helped us to put the party explanations of legislation in a broader historical perspective. They both provide evidence of an effect of party control on law-making and expose the limitations of that line of explanation. Several broad historical patterns cry out for explanation and cannot be accounted for by the usual party-control of government account. In particular, why did the demand for national legislation begin to grow in the second half of the 20th Century? The American Congress since 1945, or perhaps 1932, appears to be a fundamentally different institution in terms of significant legislation passed, than the institution that existed before the middle of the 20th Century. There was a profound change in the 1930s, or perhaps the 1940s, in what people seem to demand from *national* government, and even during the years of New Federalism and deconstruction of the Great Society and New Deal, the amount of significant legislation enacted by Congress has far exceeded what came before the 1920s. Perhaps that puzzle may be waved away by noting that life is just more complicated now, but that only begs the question (or perhaps the explanation). Is it the case, then, that Congress does what it does and when it does it not because of political polarization, party control, or other internal and institutional accounts, but because Congress responds to the needs of an increasingly complex nation? That is, Congress does what it does, as Arthur Maass argued three decades ago and James Madison argued two centuries ago, because it acts in the Common Good.

A | Appendix to Chapter 1

A.1 Additional Tables

Table A.1: State-Level Summary Statistics

	Mean	S.D.	N
Petitions	238.55	378.54	1553
Female Share	0.48	0.03	1553
Non-White Share	0.11	0.15	1553
Personal Income per Capita	465.05	338.52	1553
Farms per Capita	0.07	0.03	1553
Urban Population Share	0.28	0.24	1553

This table presents summary statistics for the data and covariates used in the estimation of the effect of direct elections on petitioning. The means reported are for the observations pooled across chambers and across time.

Table A.2: Effect of Switch to Direct Elections on Petitioning to Senate Committees

	log(Petitions)			
	Agriculture	Appropriations	Commerce	Education and Labor
Direct Election	-0.150	0.118	0.015	0.089
	(0.191)	(0.137)	(0.189)	(0.178)
Observations	705	705	705	705
R^2	0.556	0.438	0.669	0.605

Standard errors in parentheses

Standard Errors clustered at state level.

Includes state and Congress fixed effects and state specific time trends.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Effect of Switch to Direct Elections on Petitioning to Senate Committees (Continued)

	log(Petitions)			
	Finance	Foreign Relations	Judiciary	Pensions/Claims
Direct Election	0.261	-0.322	-0.413	0.162
	(0.212)	(0.198)	(0.299)	(0.176)
Observations	705	705	705	705
R^2	0.615	0.614	0.745	0.620

Standard errors in parentheses

Standard Errors clustered at state level.

Includes state and Congress fixed effects and state specific time trends.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The two tables above break out the effect of direct election on Senate petitions by committee. The committee to which each petition was referred is ascertained from the description of each petition in the Congressional Record.

Table A.4: Effect of Switch to Direct Elections on Share of Petitions Sent by Membership Organizations to the Senate, Binary Treatment, 17th Amendment

	log(Petitions)			
	(1)	(2)	(3)	(4)
Direct Election	-0.052***	-0.052***	-0.052***	-0.052***
	(0.005)	(0.005)	(0.005)	(0.005)
Congress FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Regional Time Trends	Yes	No	No	No
State Time Trends	No	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes
Other Reform Controls	No	No	No	Yes
Observations	3027	3027	3027	3027
R^2	0.236	0.260	0.264	0.265

Standard errors in parentheses

Standard Errors clustered at state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I determine the share of all petitions from large membership organizations using the list compiled in Skocpol, Ganz, and Munson (2000). State-years in which no petitions were sent to the House or Senate are omitted.

Table A.5: Effect of Switch to Direct Elections on Concentration of Petitioning Across Committees

	Senate	
	(1)	(2)
Direct Election	0.068***	0.067**
	(0.025)	(0.031)
Demographic Controls	No	Yes
Observations	1482	1482
R^2	0.259	0.351

Standard errors in parentheses

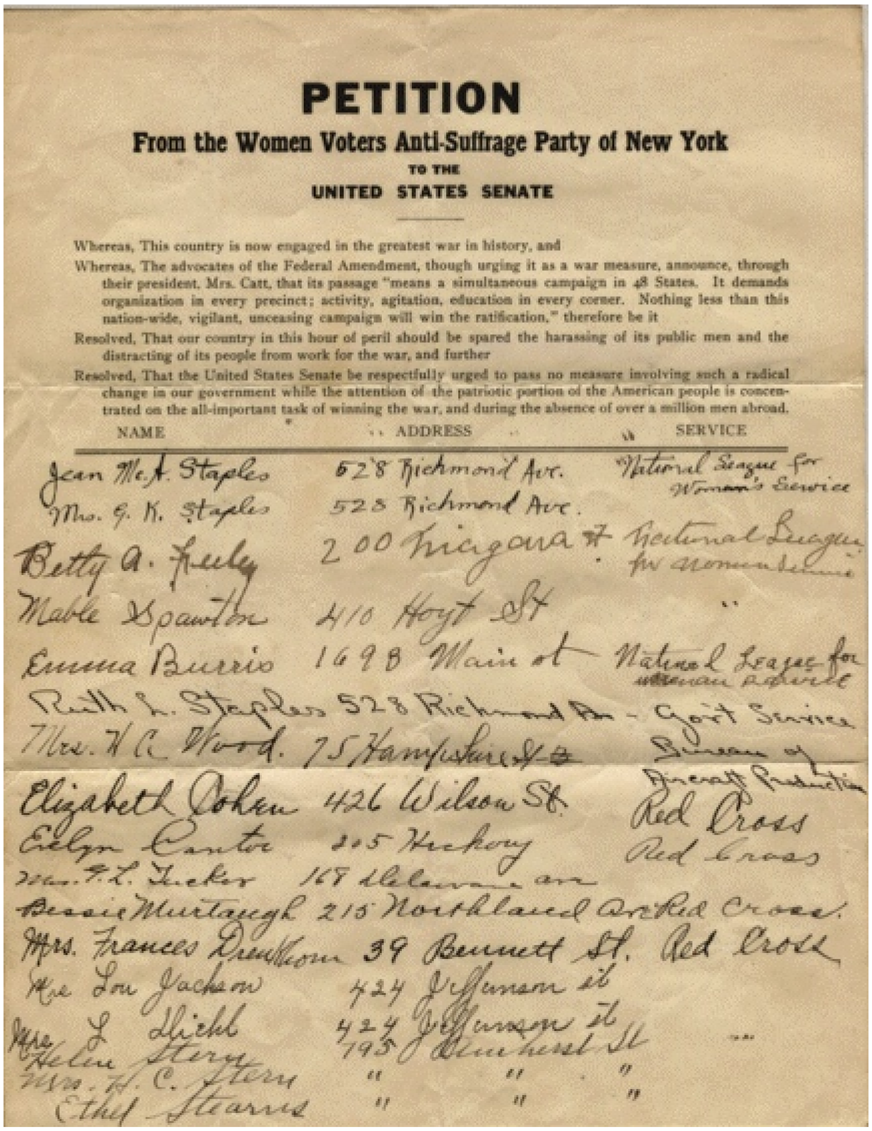
Standard Errors clustered at state level.

Includes state fixed effects and state specific time trends.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The outcome variable measures the concentration of petitions across committees in each Congress. Higher values correspond to greater concentration of petitions referred to a small number of committees. The placebo direct election variable is calculated by assigning the direct election codings used for the Senate to the House instead.

A.2 Additional Figures



PETITION

From the Women Voters Anti-Suffrage Party of New York

TO THE

UNITED STATES SENATE

Whereas, This country is now engaged in the greatest war in history, and

Whereas, The advocates of the Federal Amendment, though urging it as a war measure, announce, through their president, Mrs. Catt, that its passage "means a simultaneous campaign in 48 States. It demands organization in every precinct; activity, agitation, education in every corner. Nothing less than this nation-wide, vigilant, unceasing campaign will win the ratification," therefore be it

Resolved, That our country in this hour of peril should be spared the harassing of its public men and the distracting of its people from work for the war, and further

Resolved, That the United States Senate be respectfully urged to pass no measure involving such a radical change in our government while the attention of the patriotic portion of the American people is concentrated on the all-important task of winning the war, and during the absence of over a million men abroad.

NAME	ADDRESS	SERVICE
Jean McA. Staples	528 Richmond Ave.	National League for Woman's Service
Mrs. G. K. Staples	528 Richmond Ave.	
Betty A. Feeley	200 Niagara St.	National League for Woman's Service
Mable Spawton	410 Hoyt St	"
Emma Burris	1698 Main St	National League for Woman's Service
Ruth L. Staples	528 Richmond Ave.	Gov't Service
Mrs. H. C. Wood	75 Hampshire St.	Bureau of Aircraft Production
Elizabeth Cohen	426 Wilson St.	Red Cross
Evelyn Cantor	205 Hickory	Red Cross
Mrs. F. L. Tucker	168 Delaware ave	
Bessie Murtaugh	215 Northland Ave	Red Cross.
Mrs. Frances Drenkhorn	39 Bennett St.	Red Cross
Mrs. Lou Jackson	424 Jefferson St	
Mrs. L. Wichl	424 Jefferson St	
Helen Stern	193 Amherst St	
Mrs. H. C. Stern	"	"
Ethel Stearns	"	"

Figure A.1: Example Petition. This figure displays an example of the text of a petition against Women's Suffrage sent to the Senate in 1917. *Source: National Archives Record Group 46.*

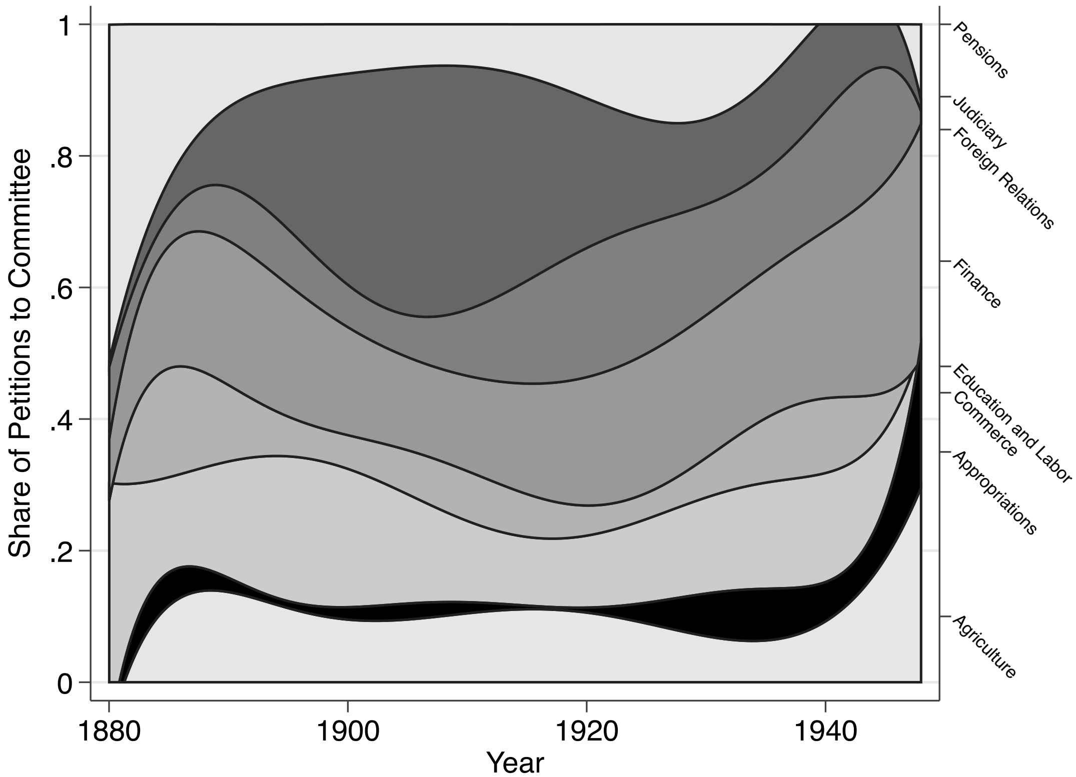


Figure A.2: Topics of Senate Petitions by Committee over Time. This figure illustrates the share of petitions referred to 8 different Senate Committees. Petitions referred to other committees or not referred to a Committee are not included in the graph. The trends over time are smoothed using local polynomial regression.

A.3 Rules for Processing Petitions

Both the House and the Senate draw heavily upon *Jefferson's Manual* for guidance in how to receive and process petitions. The rules for how the House treats petitions have evolved minimally over time. Initially, the rules adopted in 1789 "provided for the presentation of petitions to the House by the Speaker and Members."[1] In 1842 the rules were changed so that petitions were filed with the clerk. For instance, Rule XII, clause 3 of the *Rules of the House of Representatives* for the 59th Congress (1905) states: "Members having petitions or memorials or bills of a private nature to present may deliver them to the Clerk, endorsing their names and the reference or disposition to be made thereof; and said petitions and memorials and bills of a private nature, except such as, in the judgment of the Speaker, are of an obscene or insulting character, shall be entered on the Journal with the names of the members presenting them, and the Clerk shall furnish a transcript of such entry to the official reporters of debates for publication in the Record."

In the Senate, the guidelines for dealing with petitions are outlined in the *Standing Rules of the Senate*. Similarly to the House, few if any substantive changes to the procedure for processing petitions have occurred over time. For instance, the *Rules of the Senate* in 1868 contain a section on "Morning Business, Petitions, Reports, etc." that describes the procedure for handling petitions. The Presiding Officer calls for petitions and memorials; then "every petition or memorial, or other paper, shall be referred, of course, without putting a question for that purpose, unless the reference is objected to by a Senator at the time such petition, memorial, or other paper is presented. And before any petition or memorial, addressed to the Senate, shall be received and read at the table, whether the same shall be introduced by the Presiding Officer, or a Senator, a brief statement of the contents of the petition or memorial shall verbally be made by the introducer".[2] By 1913, the *Standing*

[1]See Jefferson and Sullivan (2011).

[2]See *Rules of the Senate of the United States, and Joint Rules of the Two Houses: Also Rules of Practice and Procedure in the Senate when Sitting for the Trial of Impeachments* (1868).

Rules of the Senate had not changed meaningfully with regard to petitions. The rules noted that "a brief statement of the contents of each petition, memorial, or paper presented to the Senate, shall be entered" into the Record. "Every petition or memorial shall be signed by the petitioner or memorialist and have endorsed thereon a brief statement of its contents, and shall be presented and referred without debate."[3]

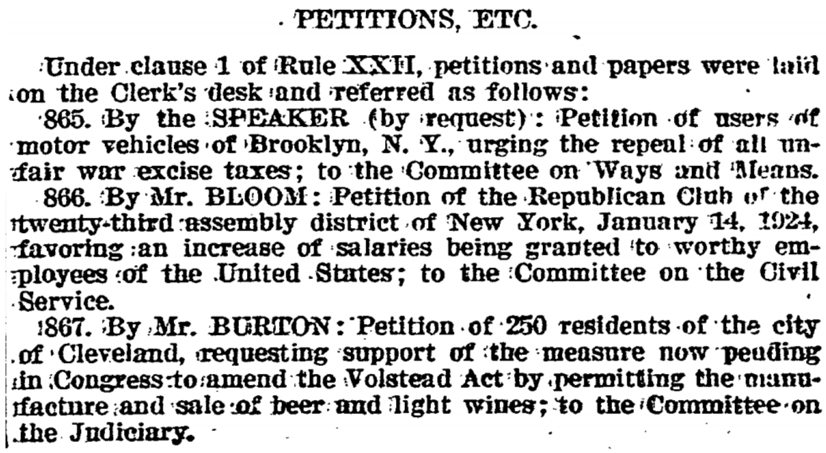
A.4 Assembling the Database of Petitions

This chapter relies on the first comprehensive database, which I have worked to assemble, of petitions sent to Congress since 1881—this source includes hundreds of thousands of petitions. The ability to assemble such data has only become possible recently, with the advent of modern computing power and digitization of textual sources. In this section, I describe the methods used to compile this data.

To create this database, I used a digitized version of the Congressional Record. Because the right to petition Congress is protected in the Constitution, Congress handles the receipt of petitions in a relatively systematic manner. Upon receipt, petitions are brought to the floor by a member of Congress. The Congressional Record records the member who presented the petition, the date it was presented, the petition's subject and purpose, sender and the location of its origin. Most petitions are subsequently referred to a committee, and this information is recorded in the Congressional Record as well. In rare cases, the full text of the petition is printed.

Figure A.3 presents one example of how petitions were recorded for the House of Representatives. The information is recorded in a relatively standardized manner that allows for straightforward harvesting of the relevant data points. Given that this rich source of data on petitioning has been carefully recorded in the Congressional Record, my innovation has been to harvest this data systematically. The sheer number of records makes it impractical to perform this task by hand. Instead, I have written a computer program in Python that

[3]See (*Senate Manual Containing the Standing Rules and Orders of the United States Senate* 1913).



PETITIONS, ETC.

Under clause 1 of Rule XXII, petitions and papers were laid on the Clerk's desk and referred as follows:

865. By the SPEAKER (by request): Petition of users of motor vehicles of Brooklyn, N. Y., urging the repeal of all unfair war excise taxes; to the Committee on Ways and Means.

866. By Mr. BLOOM: Petition of the Republican Club of the twenty-third assembly district of New York, January 14, 1924, favoring an increase of salaries being granted to worthy employees of the United States; to the Committee on the Civil Service.

867. By Mr. BURTON: Petition of 250 residents of the city of Cleveland, requesting support of the measure now pending in Congress to amend the Volstead Act by permitting the manufacture and sale of beer and light wines; to the Committee on the Judiciary.

Figure A.3: Example of House Petitions Recorded in the Congressional Record. This figure displays an example of how petitions were recorded for the House of Representatives in the Congressional Record. This example is from the session on Monday, February 4, 1924.

processes the textual data and converts it into a more standardized database format. This process has involved some trial and error, including a substantial amount of time spent performing manual corrections in cases where the digitized source text was mangled when it was converted from a scanned image to the textual source data that I have used.

Figure A.4 presents an example of how petitions sent to the Senate were recorded in the *Record*. The key difference is that, unlike for House petitions after 1920, the Senate petitions were not numbered. However, because the records appear in a standardized format, I can identify each reference separately. By far the biggest challenge occurs in rare instances where multiple petitions are referenced in the same block of text. This occurs primarily in the 19th century and early 20th century *Congressional Record*. In these instances, the program I wrote counts each distinct reference. For example, in cases where the text refers to multiple or "sundry" petitions and then lists several different towns, I count each town as contributing a distinct petition.

While the strategy I used to extract the relevant information varied depending on the formatting of the records, the general approach involved the following steps for each day recorded in the Record:

Mr. JONES of Washington presented a petition of sundry citizens of Ballard, Wash., praying for the passage of legislation granting adequate compensation to postal employees, which was referred to the Committee on Post Offices and Post Roads.

He also presented a petition of sundry citizens of Seattle, Wash., praying for the passage of House bill 4123, for the reclassification of postal salaries, which was referred to the Committee on Post Offices and Post Roads.

He also presented a petition of sundry citizens of Walla Walla, Wash., praying for the adoption of the so-called Mellon tax-reduction plan, which was referred to the Committee on Finance.

He also presented a petition of sundry citizens of Walla Walla, Wash., praying an amendment to the Constitution regulating child labor, which was referred to the Committee on the Judiciary.

Mr. LADD presented the petition of Zack Shackman and 77 other citizens of Berlin, N. Dak., praying for an increased tariff on wheat and repeal of the drawback and milling-in-bond provision of the so-called Fordney-McCumber Tariff Act, which was referred to the Committee on Finance.

He also presented the petition of Ed. Mack and 75 other citizens of Lewistown, Mont., praying for increased tariff duties on wheat, flour, flax, and linseed oil, which was referred to the Committee on Finance.

He also presented resolutions adopted at a meeting of farmers and manufacturers assembled in Chicago, Ill., at the call of the Illinois Manufacturers' Association, opposing Government fixing of prices of agriculture or other commodities, favoring the cooperative marketing of farm products, utilization of the Muscle Shoals plant, etc., which were referred to the Committee on Agriculture and Forestry.

He also presented a resolution of the Finley Community Club, of Finley, N. Dak., favoring the passage of Senate bill 1597, providing a $50,000,000 revolving loan to the livestock industry, which was referred to the Committee on Agriculture and Forestry.

Figure A.4: Example of Senate Petitions Recorded in the Congressional Record. This figure displays an example of how petitions were recorded for the Senate in the Congressional Record. This example is from the session on Monday, January 28, 1924.

- Find section of *Congressional Record* in which information on petitions is contained
- Identify first petition recorded along with its number
- Cycle through petitions until unable to find the next one in the sequence. This is the last petition recorded for the day.
- For each reference to a petition:
  - Extract and save name of person presenting petition using regular expressions
  - Extract location data from description using regular expressions and natural language processing
  - Extract text description of petition using regular expressions
  - Extract information on Committees using regular expressions

To my knowledge, the approach I have taken to transform the text on petitions in the *Congressional Record* into "data" has not been attempted on this scale before. I have endeavored to gather this data in as transparent and comprehensive a manner as possible. The approach I have taken surely misses some petitions—but I have no reason to think that it results in systematic biases one way or the other.

A.5 Changes in Petitioning Across Committees

I have devised an empirical approach that tests whether the concentration of petitions across committees changed appreciably with the switch to direct elections. I construct a measure H_{st} of the concentration of petitions across committees for a time period t and state s.[4]

$$H_{st} = \sum_{c \in C} \left(\frac{P_{cst}}{N_{st}} \right)^2 \tag{A.1}$$

[4]The approach here is based on a Herfindahl index typically used to calculate the concentration of firms in an industry/market.

P_{cst} denotes the number of petitions sent to a committee c from state s in time period t. N_{st} denotes the total number of petitions sent to the relevant body of Congress from state s in time period t. The index is the sum—across the set of all committees C—of the squared shares of petitions sent to each committee. As a result, a lower value of H_{st} suggests that constituents from a state sent petitions evenly across a wide range of committees, whereas a higher value suggest that constituents sent the bulk of petitions to just a few committees.

A.6 Abstention from Roll Call Votes

Poole and Rosenthal (1997) notes a puzzling increase in abstention rates after direct elections in the Senate compared to the House. While there is a secular trend of declining abstention rates (due, according to Poole and Rosenthal (1997) primarily to advances in transportation), the sharp increase in the Senate relative to the House presents a puzzle. One explanation is that the greater discretion resulting from the passage of the 17th Amendment afforded members of Congress an increased ability to abstain from votes. For example, in their simple model of legislator turnout, Poole and Rosenthal (1997) associate indifference on policy issues with increased levels of abstention among members of Congress. Following this logic, additional discretion—i.e., having more choice over which specific issues to attend to—should then correlate strongly with increased indifference. Similarly, in other work scholars have identified missing roll call votes as a form of shirking and a proxy for legislative effort (Bender and Lott Jr 1996; Rothenberg and Sanders 2000). Building on this framework, I identify a relationship between levels of petitioning and abstention rates.

Table A.6 reveals the results for abstention rates before and after the onset of direct elections. In the time period before direct election to the Senate, higher levels of petitioning corresponded with a decline in abstention rates. This result is consistent with a scenario where some senators used petitioning activity to gauge the importance of issues. A doubling of the number of petitions received from a state coincided with a decline in abstention rates of roughly 2%. However, after the switch to direct elections, the link between petitioning and

Senator behavior changed. The volume of petitioning no longer had any effect on abstention rates. The change in the relationship between petitioning and abstention accords with the idea that petitioning campaigns grew less effective as a signal of issue importance.

Table A.6: Effect of Petitioning on Abstention Rates in the Senate

	Pre Direct Election			Post Direct Election		
	(1)	(2)	(3)	(4)	(5)	(6)
log(Petitions)	-0.015***	-0.011**	-0.019***	0.000	0.005	-0.000
	(0.004)	(0.004)	(0.007)	(0.003)	(0.004)	(0.005)
Herfindahl Petitioning Index		0.058*	0.086***		0.040**	0.019
		(0.030)	(0.031)		(0.019)	(0.016)
Congress FEs	No	No	Yes	No	No	Yes
State FEs	No	No	Yes	No	No	Yes
Observations	1588	1575	1575	1735	1595	1595
R^2	0.011	0.012	0.183	0.000	0.003	0.229

Standard errors in parentheses

Standard Errors clustered at the state-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Abstention rates refer to the share of roll call votes for which a senator did not cast a vote. The Herfindahl Petitioning Index refers to the concentration of petitions across committees for a single Congress. The index ranges from 0 to 1; higher values denote higher concentrations of petitions sent to just a few committees. States that submitted no petitions in a Congress are omitted from the sample in specifications 2, 3, 5 and 6 (explaining the changing observation numbers across specifications).

A.7 Petitioning and Other Progressive Era Electoral Reforms

The theoretical expectations developed in this chapter need not apply only to the implementation of the 17th Amendment; any electoral reform that fundamentally altered the links between constituents and representatives might also change the volume of communication through petitioning. This section briefly considers the effect on petitioning of other landmark Progressive Era electoral reforms: direct primaries, preference votes, the secret ballot and women's suffrage. Previous research has considered the effects of these reforms on subjects as varied as the size of government (Lott 1999), committee assignments (Katz and Sala 1996) and health outcomes (Miller 2008a).

The hypotheses laid out in this chapter do not map equally well onto all of these reforms. The effects of direct primaries and preference votes should be most similar to the effects of the 17th Amendment. These served as attempts by state voters to wrest control of Senate appointments from state legislatures. At the same time, it would be a mistake to consider these reforms as entirely pre-empting the reforms brought about by the 17th Amendment. As Gailmard and Jenkins (2009) points out (referencing an unpublished paper by John Lapinski), direct primary laws were never legally binding because the Constitution held final authority on the relationship between state legislatures and the U.S. Senate. States that had already passed direct primary laws were themselves the most vigorous in lobbying for a direct election amendment to the Constitution. Similarly, preference votes sometimes occurred haphazardly—for example, the terms of the vote were sometimes negotiated between candidates or agreed upon between parties only a month or two before the actual election.[5] When newspapers forecasted the composition of the Senate, they referred to the control of the state legislature even in states with preference elections.[6] Direct primaries and preference votes surely tightened the electoral connection between constituents and representatives, but not perhaps to the extent of the 17th Amendment.

As a robustness check and to determine the effects of direct primaries, I construct a variable that indicates the first direct election reforms for each state—i.e., for the 31 states that implemented direct primaries before the 17th Amendment I use implementation of direct primaries as the date of the first direct elections for Senate. Otherwise, I use the date of first direct election after the enactment of the 17th Amendment. Panel A in Table A.7 reports the results for the effects of direct election when incorporating implementation of direct primaries. The effects of direct election on Senate petitioning still register around -30%; across all specifications the effect is distinguishable from zero. The results suggest that the switch to direct primaries operated similarly to the reforms brought about by the

[5] See "Elect People's Choice. Chairmen of Nevada Parties Pledge Legislators on Senator," *Morning Oregonian*, September 10, 1908.

[6] See "Democrats in Control of National Congress: Severe Rebuke by the People," *The Daily Oklahoman*, November 10, 1910.

17th Amendment. When the electoral connection between constituents and representatives tightened, the need for direct policy instructions diminished. Panel B performs a similar exercise for states with preference votes. The effects also center around -30%.

Secret ballot reforms likely changed the conduct of elections along several dimensions. These reforms marked a change from party ticket voting to office-by-office voting. On the other hand, they also appear to have made literacy essentially a pre-requisite to casting a vote in many states (Katz and Sala 1996; Kousser 1974). In sum, it does not appear clear what effect, if any, secret ballot reforms might have on petitioning. The results in Panel B accord with this story. The specifications yield an effect indistinguishable from zero. With the exception of specification 2, the other estimates are also relatively precise zeroes rather than large but noisy effects.[7]

Finally, women's suffrage reforms enfranchised half of the population at the end of the 19th century and start of the 20th century. In 1920, passage of the 19th Amendment forced all states that had not yet extended the franchise to give women the right to vote. Here, my theoretical expectation would be that extending the franchise to women would lead to a reduction in women's petitioning. But the results of the estimation do not appear to bear this story out. None of the estimates differ from zero significantly, and the point estimates for two of the three effects suggest effects under 10%. This result surely deserves further explanation in future work. For now, I can posit at least one explanation for the null result. Despite the attention garnered by female petitioners, their share of all petitioning to Congress was actually relatively low compared to men. This provides a low ceiling from which to distinguish a decline. It also suggests that the quantity of interest under study—total petitions sent by men or women—may in fact cast too wide a net. Filtering petitions by the gender of the organizers and then restricting the data to only female petitioners might well yield the expected result. Given limitations to the data (i.e., the fact that many records in my database do not list the names of organizers and those that do would require

[7]In Panel C, I estimate the results only for the House. Many of the reforms occurred before direct election and so would have had no bearing on Senate elections anyway.

extensive hand-coding), this null result presents a puzzle to address in future research.

Table A.7: Congressional Petitioning and Other Progressive Era Electoral Reforms

	log(Petitions)		
	(1)	(2)	(3)
Panel A: Direct Primaries			
Estimate	-0.394***	-0.393***	-0.378***
	(0.085)	(0.083)	(0.084)
Δ in Petitions	-32.81%	-32.73%	-31.72%
Observations	3106	3106	3106
R-Squared	0.700	0.721	0.726
Panel B: Preference Votes			
Estimate	-0.357***	-0.377***	-0.385***
	(0.096)	(0.093)	(0.093)
Δ in Petitions	-30.34%	-31.70%	-32.25%
Observations	3106	3106	3106
R-Squared	0.699	0.721	0.726
Panel C: Secret Ballot (House Only)			
Estimate	0.103	0.146	0.021
	(0.138)	(0.140)	(0.118)
Δ in Petitions	9.80%	14.59%	1.41%
Observations	1553	1553	1553
R-Squared	0.749	0.780	0.791
Panel D: Women's Suffrage			
Estimate	0.133	0.152	0.034
	(0.132)	(0.119)	(0.106)
Δ in Petitions	13.23%	15.59%	2.88%
Observations	3106	3106	3106
R-Squared	0.696	0.716	0.724
Congress FEs	Yes	Yes	Yes
State FEs	Yes	Yes	Yes
Regional Time Trends	Yes	No	No
State Time Trends	No	Yes	Yes
Demographic Controls	No	No	Yes

Each estimate comes from a model that is estimated separately.

Standard errors in parentheses

Standard Errors clustered at state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B | Appendix to Chapter 2

B.1 Data & Measures

B.1.1 Tables

Table B.1: Measures for Congressional District Characteristics

Measure	Definition	ACS Table
Age 65+ (%)	Percentage of residents age 65 and over	S0101
Hispanic (%)	Percentage of Hispanic residents	B03002
Non-Hispanic Black (%)	Percentage of non-Hispanic Black residents	B03002
Non-Hispanic Asian (%)	Percentage of non-Hispanic Asian residents	B03002
HS or higher (%)	Percentage of residents (age 25+) with ≥HS/equivalent	S1501
BA or higher (%)	Percentage of residents (age 25+) with ≥BA	S1501
Employment-pop. ratio (%)	Percentage of employed residents (ages 25-64)	S2301
Median household income	Median household income (constant 2013 dollars)	S1901
Population density	Residents per square mile of land area	S0101
Residential mobility (%)	Percentage of residents who moved in the past year	S0701

Data on land area (population density) are from: https://www.census.gov/geo/maps-data/data/cd_national.html
Median household income is adjusted for inflation using the CPI-U-RS.

B.1.2 Figures

To demonstrate that PVI is a reasonable ex ante measure of electoral competitiveness, we regress the win margin on PVI for all House races contested by a Democratic and Republican candidate between 2008 and 2014.[1] PVI explains 82% of the variation in the electoral outcome (win margin), the intercept is relatively close to zero (0.7), and the slope is relatively close to one (1.1). Figure B.1 displays a scatterplot of the win margin vs. PVI. The dark blue line is the least-squares line of best fit and the dotted red line is the 45-degree line. The line of best fit provides a relatively close approximation to the 45-degree line. In sum, PVI serves as a reasonable measure for the expected competitiveness of House election outcomes.

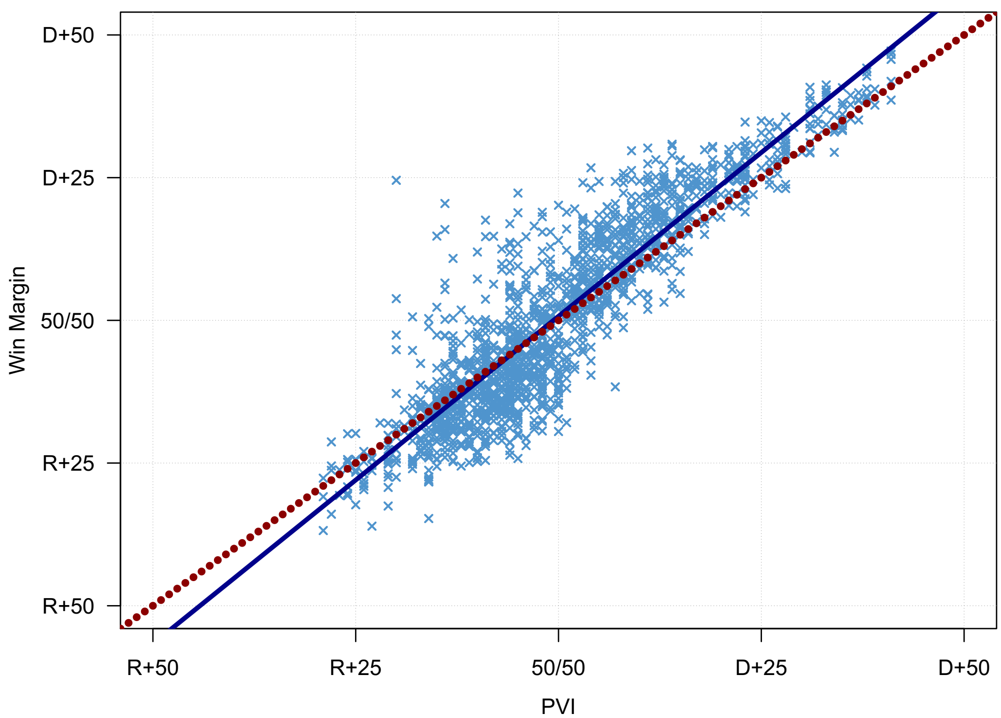


Figure B.1: Win Margin vs. PVI, 2008-2014. This figure plots actual (ex post) win margin against (ex ante) PVI and illustrates the strong correlation between the two measures.

[1] We exclude all House races in Louisiana due to their unusual electoral rules.

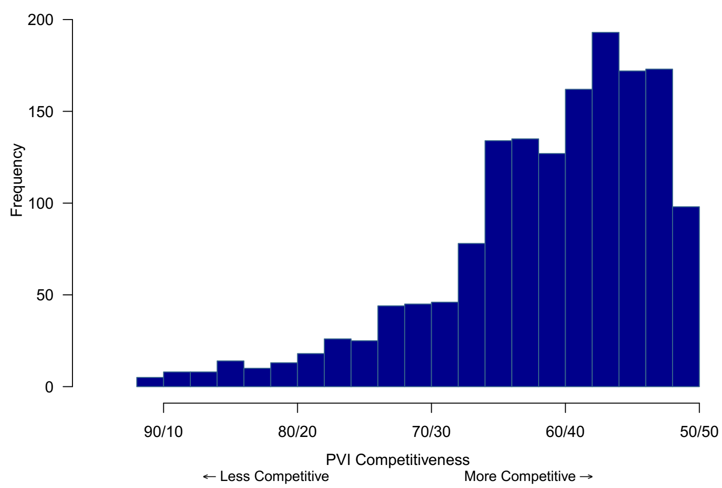


Figure B.2: Distribution of PVI Competitiveness, 2008-2014. This histogram displays the number of districts at different levels of PVI Competitiveness, our ex ante measure, between 2008 and 2014.

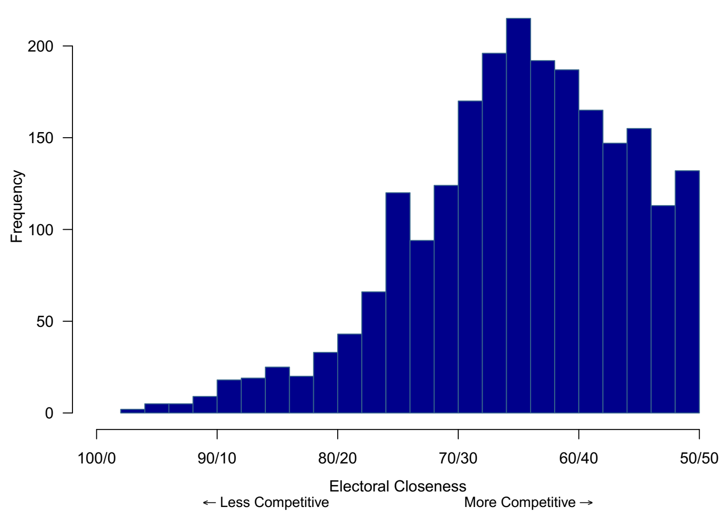


Figure B.3: Distribution of Electoral Competitiveness, 2008-2014. This histogram displays the number of districts at different levels of Electoral Competitiveness, our ex post measure, between 2008 and 2014.

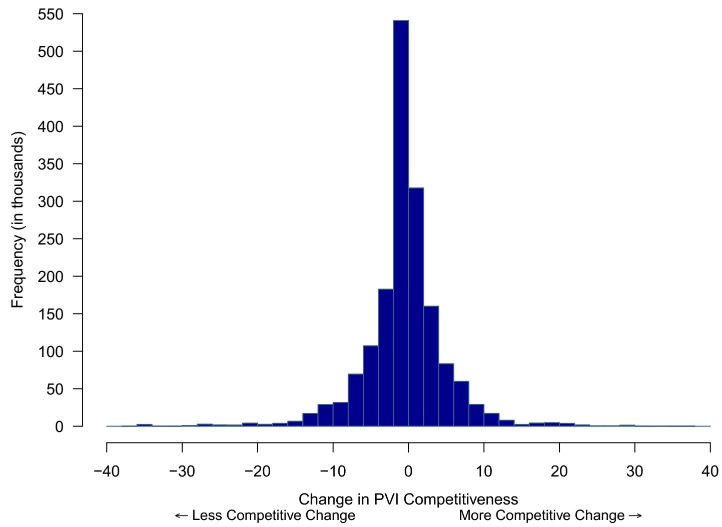


Figure B.4: Distribution of Changes in PVI Competitive from Pre-2012 to Post-2012. This histogram displays the distribution of changes in competitiveness, based on our ex ante PVI measure, for pre versus post redistricting.

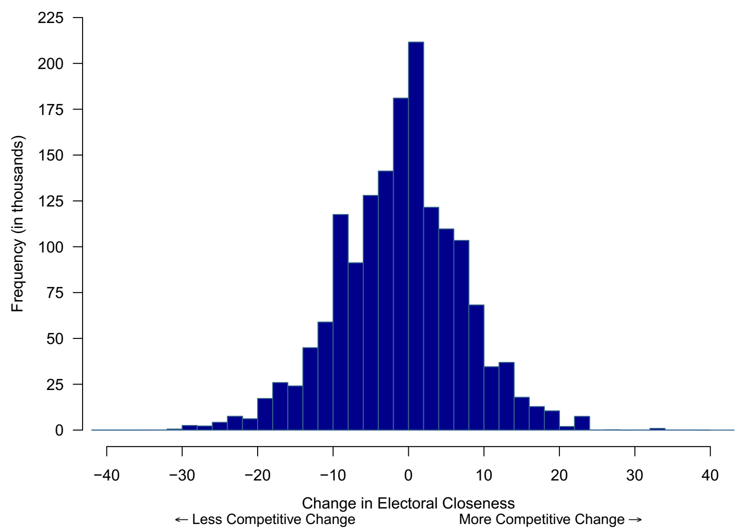


Figure B.5: Distribution of Changes in Electoral Competitiveness from 2010 to 2014. This histogram displays the distribution of changes in competitiveness, based on our ex post competitiveness measure, for the mid-term elections of 2010 and 2014.

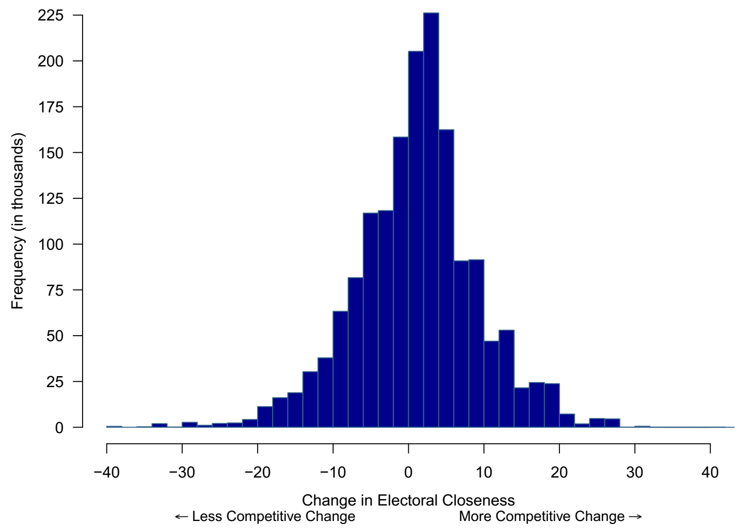


Figure B.6: Distribution of Changes in Electoral Competitiveness from 2008 to 2012. This histogram displays the distribution of changes in competitiveness, based on our ex post competitiveness measure, for the presidential elections of 2008 and 2012.

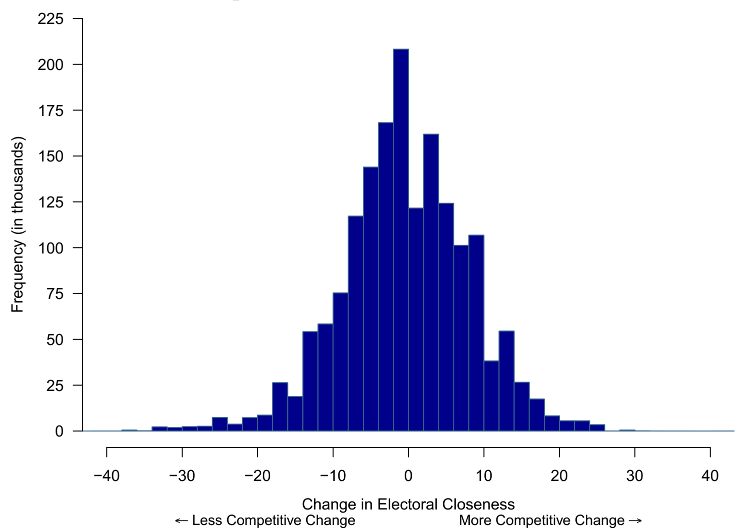


Figure B.7: Distribution of Changes in Electoral Competitiveness from 2010 to 2012. This histogram displays the distribution of changes in competitiveness, based on our ex post competitiveness measure, for the elections immediately before and immediately after the 2012 redistricting cycle.

B.2 Aggregate Results

B.2.1 Tables

Table B.2: Results Based on 2008 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.221***	0.065	0.101*	0.028	0.019	-0.050
	(0.046)	(0.036)	(0.041)	(0.027)	(0.043)	(0.028)
Age 65+ (%)			0.004	0.032	0.099	0.099
			(0.113)	(0.090)	(0.128)	(0.099)
Hispanic (%)			-0.030	-0.058*	-0.030	-0.085**
			(0.032)	(0.028)	(0.031)	(0.027)
Non-Hispanic Black (%)			0.071*	0.068***	0.096***	0.103***
			(0.028)	(0.020)	(0.027)	(0.020)
Non-Hispanic Asian (%)			-0.303***	-0.362***	-0.256***	-0.432***
			(0.060)	(0.053)	(0.061)	(0.050)
HS or higher (%)			0.469***	0.179*	0.278**	-0.030
			(0.099)	(0.076)	(0.105)	(0.075)
BA or higher (%)			0.283***	0.415***	0.356***	0.407***
			(0.050)	(0.037)	(0.062)	(0.043)
Employment-pop. ratio (%)					0.330***	0.223**
					(0.090)	(0.072)
Median household income					-0.051	0.050
					(0.041)	(0.029)
Population density					-0.294***	-0.187***
					(0.051)	(0.035)
Residential mobility (%)					-0.077	-0.243**
					(0.114)	(0.090)
Constant	61.627***	64.076***	13.445	39.433***	5.353	43.370***
	(0.638)	(5.513)	(8.370)	(7.451)	(9.265)	(8.307)
Observations	375	375	375	375	375	375
Adjusted R^2	0.056	0.508	0.480	0.814	0.536	0.846
State FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.3: Results Based on 2010 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.267***	0.165***	0.078*	0.098***	0.013	0.030
	(0.041)	(0.030)	(0.039)	(0.024)	(0.042)	(0.024)
Age 65+ (%)			0.040	0.372***	0.199	0.442***
			(0.113)	(0.079)	(0.126)	(0.083)
Hispanic (%)			-0.073*	-0.108***	-0.083**	-0.126***
			(0.030)	(0.024)	(0.030)	(0.023)
Non-Hispanic Black (%)			-0.068**	0.016	-0.063*	0.045**
			(0.025)	(0.017)	(0.025)	(0.016)
Non-Hispanic Asian (%)			-0.029	-0.248***	-0.031	-0.304***
			(0.054)	(0.041)	(0.057)	(0.039)
HS or higher (%)			0.393***	0.114	0.250*	-0.075
			(0.096)	(0.064)	(0.104)	(0.066)
BA or higher (%)			0.142**	0.297***	0.182**	0.295***
			(0.048)	(0.031)	(0.060)	(0.035)
Employment-pop. ratio (%)					0.030	0.163**
					(0.079)	(0.057)
Median household income					0.033	0.055*
					(0.043)	(0.027)
Population density					-0.211***	-0.157***
					(0.050)	(0.030)
Residential mobility (%)					0.204	-0.183**
					(0.104)	(0.070)
Constant	43.821***	47.530***	5.729	32.344***	7.584	36.690***
	(0.577)	(4.812)	(8.054)	(6.261)	(8.508)	(6.301)
Observations	401	401	401	401	401	401
Adjusted R^2	0.093	0.575	0.446	0.845	0.480	0.872
State FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.4: Results Based on 2012 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.199***	0.057	0.158***	0.078**	0.090*	0.008
	(0.051)	(0.037)	(0.043)	(0.026)	(0.044)	(0.025)
Age 65+ (%)			-0.009	0.084	0.230	0.095
			(0.105)	(0.076)	(0.119)	(0.076)
Hispanic (%)			-0.084**	-0.072**	-0.091**	-0.103***
			(0.030)	(0.024)	(0.029)	(0.022)
Non-Hispanic Black (%)			0.083**	0.095***	0.117***	0.136***
			(0.026)	(0.017)	(0.026)	(0.016)
Non-Hispanic Asian (%)			-0.283***	-0.266***	-0.221***	-0.299***
			(0.053)	(0.039)	(0.054)	(0.035)
HS or higher (%)			0.426***	0.224**	0.177	0.025
			(0.104)	(0.070)	(0.111)	(0.067)
BA or higher (%)			0.296***	0.345***	0.316***	0.333***
			(0.047)	(0.031)	(0.061)	(0.036)
Employment-pop. ratio (%)					0.375***	0.157**
					(0.084)	(0.057)
Median household income					-0.033	0.057*
					(0.044)	(0.027)
Population density					-0.299***	-0.186***
					(0.060)	(0.036)
Residential mobility (%)					0.036	-0.322***
					(0.108)	(0.072)
Constant	58.311***	57.468***	14.247	26.156***	5.769	32.969***
	(0.684)	(5.202)	(8.921)	(6.738)	(9.266)	(6.540)
Observations	386	386	386	386	386	386
Adjusted R^2	0.035	0.583	0.528	0.865	0.571	0.897
State FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.5: Results Based on 2010 and 2014 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.248***	0.144***	0.103**	0.094***	0.063	0.023
	(0.036)	(0.023)	(0.036)	(0.017)	(0.039)	(0.018)
Age 65+ (%)			-0.079	0.363***	0.066	0.363***
			(0.091)	(0.053)	(0.104)	(0.056)
Hispanic (%)			-0.150***	-0.120***	-0.166***	-0.137***
			(0.026)	(0.018)	(0.026)	(0.017)
Non-Hispanic Black (%)			-0.079***	0.009	-0.076**	0.026*
			(0.023)	(0.012)	(0.023)	(0.012)
Non-Hispanic Asian (%)			-0.165***	-0.261***	-0.167**	-0.292***
			(0.049)	(0.030)	(0.052)	(0.029)
HS or higher (%)			0.203*	0.102*	0.054	-0.060
			(0.088)	(0.049)	(0.098)	(0.049)
BA or higher (%)			0.179***	0.274***	0.182***	0.296***
			(0.041)	(0.021)	(0.055)	(0.026)
Employment-pop. ratio (%)					0.027	0.106**
					(0.070)	(0.040)
Median household income					0.054	0.028
					(0.038)	(0.019)
Population density					-0.173***	-0.203***
					(0.052)	(0.025)
Residential mobility (%)					0.275**	-0.208***
					(0.093)	(0.051)
Constant	41.393***	47.386***	22.344**	34.273***	24.110**	42.297***
	(0.481)	(4.658)	(7.407)	(5.108)	(7.829)	(5.080)
Observations	755	755	755	755	755	755
Adjusted R^2	0.060	0.678	0.339	0.881	0.358	0.900
State-Year FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.6: Results Based on 2008 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
Ex Post Competitiveness	0.255***	0.163***	0.087*	0.044	0.032	-0.005
	(0.040)	(0.032)	(0.037)	(0.026)	(0.038)	(0.025)
Age 65+ (%)			0.044	0.051	0.100	0.084
			(0.113)	(0.089)	(0.127)	(0.097)
Hispanic (%)			-0.026	-0.050	-0.030	-0.089***
			(0.031)	(0.027)	(0.030)	(0.026)
Non-Hispanic Black (%)			0.074**	0.078***	0.103***	0.114***
			(0.028)	(0.020)	(0.027)	(0.020)
Non-Hispanic Asian (%)			-0.291***	-0.348***	-0.246***	-0.424***
			(0.061)	(0.052)	(0.062)	(0.050)
HS or higher (%)			0.470***	0.183*	0.272**	-0.029
			(0.098)	(0.074)	(0.104)	(0.074)
BA or higher (%)			0.271***	0.407***	0.361***	0.416***
			(0.049)	(0.035)	(0.062)	(0.041)
Employment-pop. ratio (%)					0.329***	0.223**
					(0.089)	(0.070)
Median household income					-0.061	0.038
					(0.041)	(0.029)
Population density					-0.297***	-0.165***
					(0.049)	(0.033)
Residential mobility (%)					-0.111	-0.266**
					(0.111)	(0.088)
Constant	63.175***	66.098***	13.241	33.472***	7.146	38.369***
	(0.737)	(5.274)	(8.271)	(7.079)	(9.142)	(8.170)
Observations	372	372	372	372	372	372
Adjusted R^2	0.095	0.542	0.482	0.822	0.543	0.853
State FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.7: Results Based on 2010 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
Ex Post Competitiveness	0.231***	0.133***	0.102**	0.074***	0.055	0.047**
	(0.037)	(0.028)	(0.032)	(0.018)	(0.033)	(0.018)
Age 65+ (%)			0.046	0.352***	0.200	0.443***
			(0.112)	(0.079)	(0.126)	(0.082)
Hispanic (%)			-0.067*	-0.102***	-0.082**	-0.128***
			(0.029)	(0.024)	(0.030)	(0.022)
Non-Hispanic Black (%)			-0.066**	0.002	-0.056*	0.047**
			(0.023)	(0.015)	(0.024)	(0.015)
Non-Hispanic Asian (%)			-0.025	-0.251***	-0.026	-0.308***
			(0.053)	(0.041)	(0.057)	(0.039)
HS or higher (%)			0.406***	0.152*	0.249*	-0.077
			(0.093)	(0.062)	(0.103)	(0.065)
BA or higher (%)			0.138**	0.281***	0.176**	0.289***
			(0.047)	(0.030)	(0.060)	(0.035)
Employment-pop. ratio (%)					0.040	0.176**
					(0.079)	(0.057)
Median household income					0.033	0.057*
					(0.042)	(0.027)
Population density					-0.190***	-0.145***
					(0.049)	(0.030)
Residential mobility (%)					0.207*	-0.192**
					(0.103)	(0.070)
Constant	44.112***	53.058***	5.121	22.343***	7.577	36.668***
	(0.634)	(4.847)	(7.812)	(6.122)	(8.418)	(6.199)
Observations	401	401	401	401	401	401
Adjusted R^2	0.085	0.568	0.455	0.845	0.483	0.874
State FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.8: Results Based on 2012 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
Ex Post Competitiveness	0.196***	0.095**	0.128***	0.071**	0.074*	0.023
	(0.043)	(0.031)	(0.037)	(0.022)	(0.037)	(0.020)
Age 65+ (%)			0.010	0.083	0.240*	0.092
			(0.105)	(0.076)	(0.118)	(0.076)
Hispanic (%)			-0.073*	-0.064**	-0.086**	-0.105***
			(0.030)	(0.024)	(0.029)	(0.022)
Non-Hispanic Black (%)			0.085**	0.097***	0.119***	0.140***
			(0.026)	(0.017)	(0.026)	(0.016)
Non-Hispanic Asian (%)			-0.278***	-0.264***	-0.218***	-0.302***
			(0.053)	(0.039)	(0.055)	(0.035)
HS or higher (%)			0.448***	0.224**	0.187	0.007
			(0.104)	(0.069)	(0.111)	(0.066)
BA or higher (%)			0.289***	0.343***	0.310***	0.339***
			(0.047)	(0.030)	(0.061)	(0.035)
Employment-pop. ratio (%)					0.379***	0.169**
					(0.084)	(0.057)
Median household income					-0.031	0.051
					(0.044)	(0.027)
Population density					-0.300***	-0.179***
					(0.059)	(0.035)
Residential mobility (%)					0.022	-0.336***
					(0.108)	(0.072)
Constant	59.012***	62.212***	12.208	26.690***	4.768	37.499***
	(0.738)	(5.075)	(8.877)	(6.718)	(9.217)	(6.744)
Observations	385	385	385	385	385	385
Adjusted R^2	0.048	0.593	0.522	0.865	0.567	0.898
State FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.9: Results Based on 2014 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
Ex Post Competitiveness	0.199***	0.074*	0.125**	0.055**	0.099*	-0.006
	(0.053)	(0.031)	(0.044)	(0.021)	(0.045)	(0.020)
Age 65+ (%)			0.377**	0.330***	0.562***	0.277***
			(0.125)	(0.073)	(0.140)	(0.074)
Hispanic (%)			-0.071	-0.143***	-0.083*	-0.169***
			(0.038)	(0.027)	(0.038)	(0.025)
Non-Hispanic Black (%)			-0.009	-0.020	-0.004	-0.007
			(0.032)	(0.018)	(0.032)	(0.017)
Non-Hispanic Asian (%)			-0.307***	-0.290***	-0.198**	-0.273***
			(0.069)	(0.045)	(0.072)	(0.042)
HS or higher (%)			0.532***	0.073	0.269	-0.144
			(0.139)	(0.078)	(0.154)	(0.077)
BA or higher (%)			0.167**	0.246***	0.266***	0.325***
			(0.056)	(0.030)	(0.077)	(0.037)
Employment-pop. ratio (%)					0.366***	0.075
					(0.099)	(0.054)
Median household income					-0.113*	-0.001
					(0.053)	(0.027)
Population density					-0.332***	-0.348***
					(0.098)	(0.045)
Residential mobility (%)					0.156	-0.221**
					(0.130)	(0.073)
Constant	39.167***	53.326***	-16.300	17.397*	-21.600	56.304***
	(0.890)	(4.521)	(12.017)	(7.531)	(12.347)	(7.353)
Observations	354	354	354	354	354	354
Adjusted R^2	0.036	0.712	0.433	0.891	0.471	0.915
State FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.10: Results Based on 2010 and 2014 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
Ex Post Competitiveness	0.225***	0.109***	0.125***	0.065***	0.093**	0.025
	(0.033)	(0.021)	(0.029)	(0.014)	(0.031)	(0.014)
Age 65+ (%)			-0.063	0.349***	0.077	0.361***
			(0.091)	(0.054)	(0.103)	(0.055)
Hispanic (%)			-0.147***	-0.115***	-0.164***	-0.137***
			(0.026)	(0.018)	(0.026)	(0.017)
Non-Hispanic Black (%)			-0.079***	-0.006	-0.073**	0.025*
			(0.021)	(0.012)	(0.022)	(0.011)
Non-Hispanic Asian (%)			-0.165***	-0.266***	-0.167**	-0.295***
			(0.048)	(0.030)	(0.051)	(0.029)
HS or higher (%)			0.213*	0.136**	0.061	-0.058
			(0.086)	(0.048)	(0.097)	(0.049)
BA or higher (%)			0.174***	0.259***	0.173**	0.293***
			(0.040)	(0.021)	(0.055)	(0.026)
Employment-pop. ratio (%)					0.034	0.109**
					(0.070)	(0.040)
Median household income					0.055	0.029
					(0.037)	(0.019)
Population density					-0.154**	-0.200***
					(0.051)	(0.025)
Residential mobility (%)					0.272**	-0.212***
					(0.092)	(0.051)
Constant	41.927***	53.030***	22.007**	31.809***	23.631**	31.656***
	(0.548)	(4.700)	(7.234)	(5.064)	(7.745)	(5.163)
Observations	755	755	755	755	755	755
Adjusted R^2	0.059	0.671	0.347	0.879	0.363	0.900
State-Year FEs	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

B.2.2 Figures

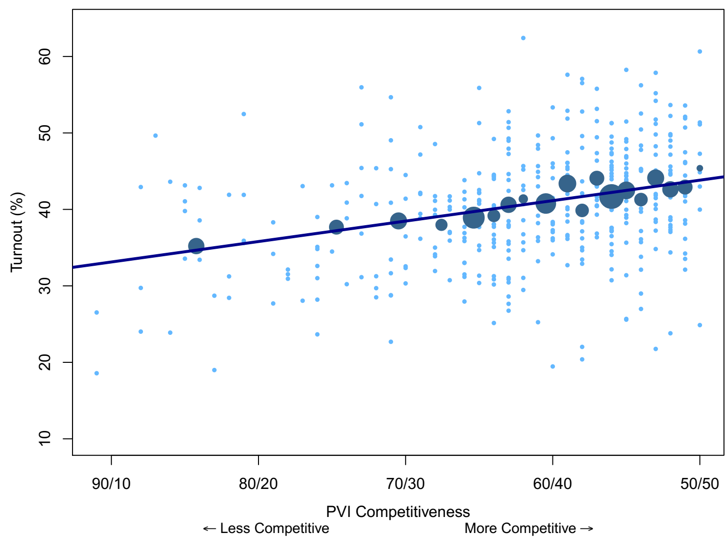


Figure B.8: Turnout Rate vs. PVI Competitiveness, 2010. This figure plots the positive relationship between turnout and PVI Competitiveness for 2010. The size of the larger dots corresponds to the number of observations at that level of competitiveness.

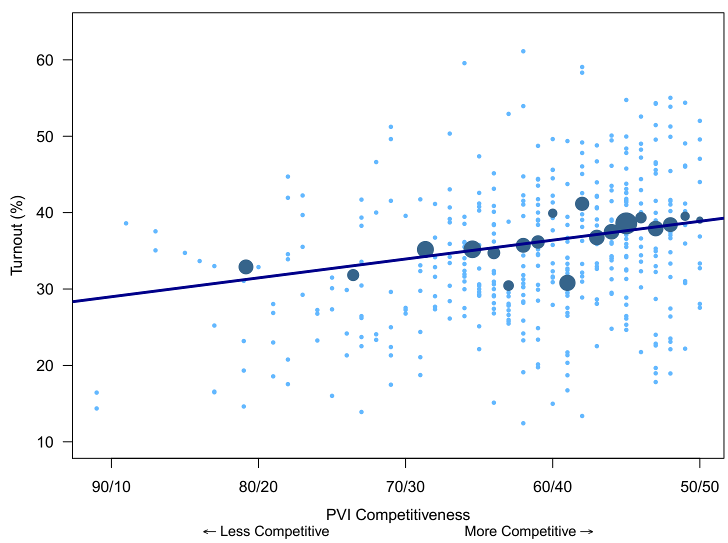


Figure B.9: Turnout Rate vs. PVI Competitiveness, 2014. This figure plots the positive relationship between turnout and PVI Competitiveness for 2014. The size of the larger dots corresponds to the number of observations at that level of competitiveness.

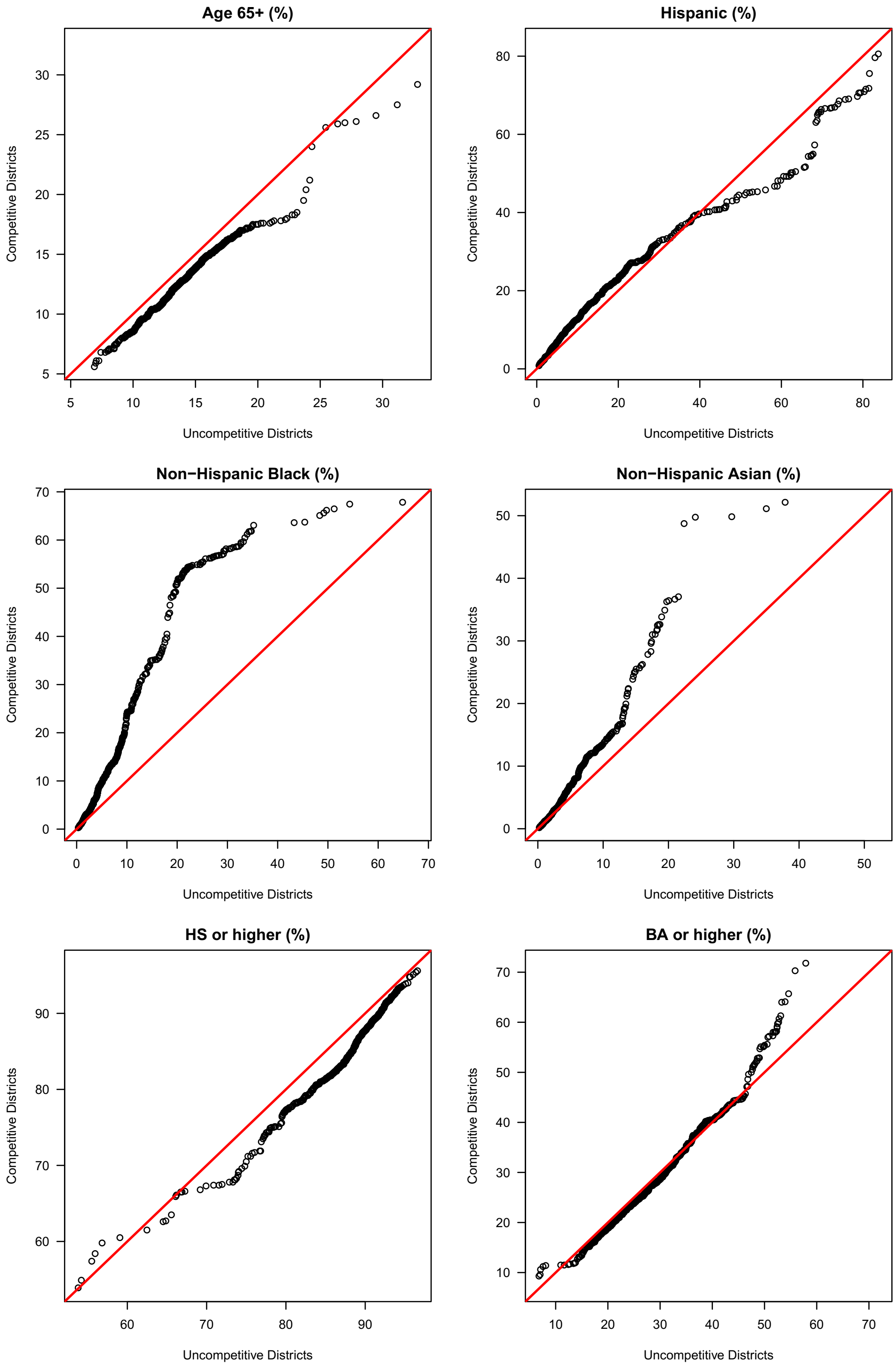


Figure B.10: Covariate Balance Plot. Competitive (Margin of Election Outcome ≤ 10) vs. Non-Competitive (Margin of Election Outcome > 10) Districts, 2008-2014

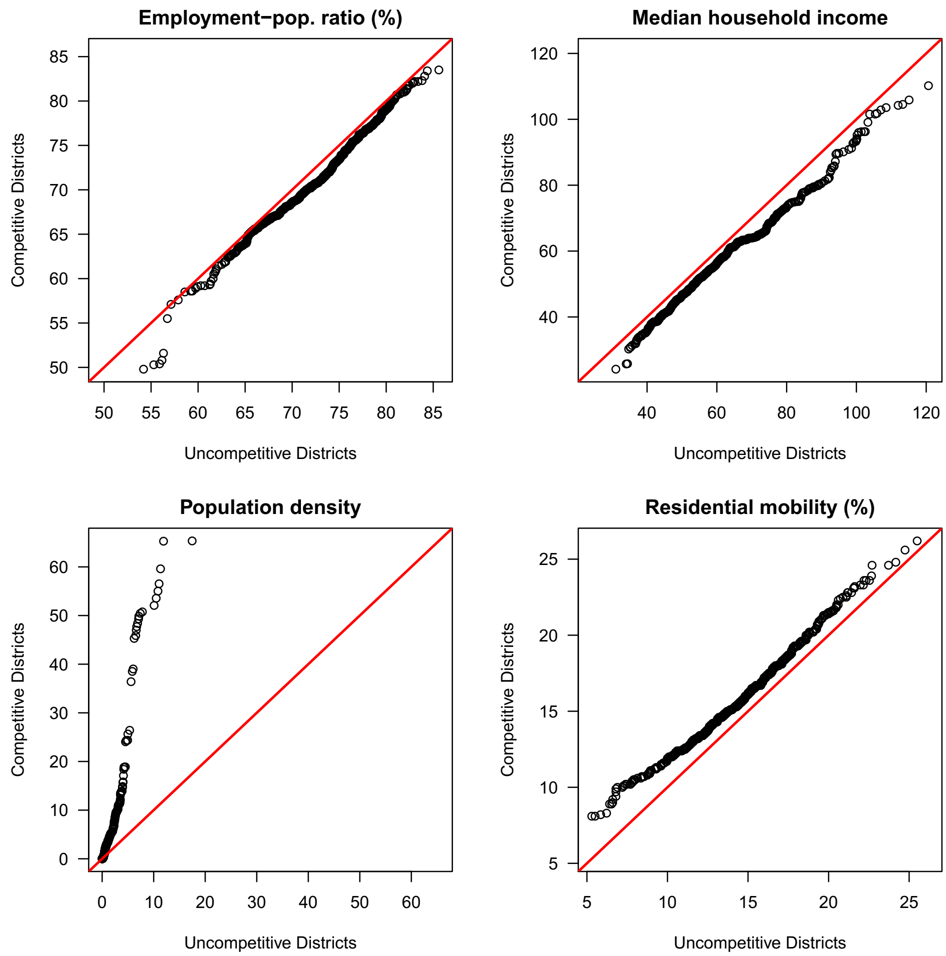


Figure B.11: This figure displays quantile-quantile plots by district competitiveness for each of the 10 measured covariates. Districts between 50/50 and 60/40 (based on PVI) are considered competitive.

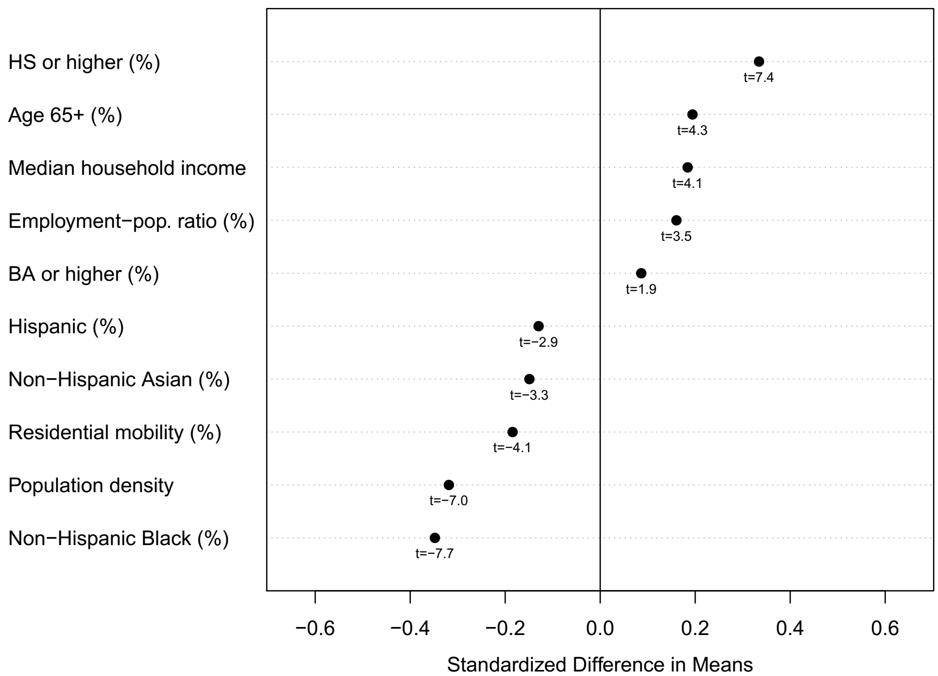


Figure B.12: Covariate Balance Plot: Competitive (Margin of Election Outcome $\leq$ 10) vs. Non-Competitive (Margin of Election Outcome $>$ 10) Districts, 2008-2014. This figure displays covariate balance for competitive versus uncompetitive district-years along a range of covariates. Competitiveness is determined based on whether or not election outcomes were greater than or less than a 60/40 margin.

B.3 Individual Results

B.3.1 Tables

Table B.11: Cross-Section Individual Regressions of Turnout on Closeness: 2008, Moderately Restrictive Sample

	(1)	(2)	(3)
PVI Competitiveness	0.00112***	0.00166***	0.00114***
	(0.0000432)	(0.0000465)	(0.0000469)
Female			0.0390***
			(0.000706)
Age 65+			0.183***
			(0.000977)
BA or Higher (%, Estimated)			0.00442***
			(0.0000200)
Hispanic			-0.0916***
			(0.00132)
Non-Hispanic Black			0.0119***
			(0.00121)
Non-Hispanic Asian			-0.184***
			(0.00214)
Observations	1906140	1906140	1876240
Adjusted R^2	0.000	0.014	0.055
State FEs	No	Yes	Yes

Robust standard errors are in parenthesis; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.12: Cross-Section Individual Regressions of Turnout on Closeness: 2008, Moderately Restrictive Sample

	(1)	(2)	(3)
Ex Post Competitiveness	0.00128***	0.00194***	0.000824***
	(0.0000387)	(0.0000416)	(0.0000421)
Female			0.0389***
			(0.000708)
Age 65+			0.182***
			(0.000980)
BA or Higher (%, Estimated)			0.00440***
			(0.0000201)
Hispanic			-0.0908***
			(0.00132)
Non-Hispanic Black			0.0104***
			(0.00122)
Non-Hispanic Asian			-0.184***
			(0.00214)
Observations	1895865	1895865	1866149
Adjusted R^2	0.001	0.015	0.055
State FEs	No	Yes	Yes

Robust standard errors are in parenthesis; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.13: Cross-Section Individual Regressions of Turnout on Closeness: 2010, Moderately Restrictive Sample

	(1)	(2)	(3)
PVI Competitiveness	0.000944***	0.00165***	0.000609***
	(0.0000421)	(0.0000447)	(0.0000450)
Female			0.00746***
			(0.000678)
Age 65+			0.268***
			(0.000921)
BA or Higher (%, Estimated)			0.00496***
			(0.0000198)
Hispanic			-0.110***
			(0.00121)
Non-Hispanic Black			-0.0232***
			(0.00112)
Non-Hispanic Asian			-0.202***
			(0.00196)
Observations	1954509	1954509	1921934
Adjusted R^2	0.000	0.027	0.095
State FEs	No	Yes	Yes

Robust standard errors are in parenthesis; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.14: Cross-Section Individual Regressions of Turnout on Closeness: 2010, Moderately Restrictive Sample

	(1)	(2)	(3)
Ex Post Competitiveness	0.00126***	0.00127***	0.000570***
	(0.0000384)	(0.0000404)	(0.0000400)
Female			0.00748***
			(0.000678)
Age 65+			0.268***
			(0.000921)
BA or Higher (%, Estimated)			0.00496***
			(0.0000198)
Hispanic			-0.110***
			(0.00121)
Non-Hispanic Black			-0.0243***
			(0.00110)
Non-Hispanic Asian			-0.202***
			(0.00196)
Observations	1954509	1954509	1921934
Adjusted R^2	0.001	0.027	0.095
State FEs	No	Yes	Yes

Robust standard errors are in parenthesis; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
The sample is comprised of contested general elections by a Democratic and Republican candidate.
All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.15: Cross-Section Individual Regressions of Turnout on Closeness: 2012, Moderately Restrictive Sample

	(1)	(2)	(3)
PVI Competitiveness	0.00186***	0.00208***	0.00175***
	(0.0000462)	(0.0000498)	(0.0000503)
Female			0.0332***
			(0.000701)
Age 65+			0.177***
			(0.000870)
BA or Higher (%, Estimated)			0.00535***
			(0.0000198)
Hispanic			-0.0738***
			(0.00134)
Non-Hispanic Black			0.0257***
			(0.00120)
Non-Hispanic Asian			-0.177***
			(0.00216)
Observations	1896635	1896635	1868372
Adjusted R^2	0.001	0.020	0.069
State FEs	No	Yes	Yes

Robust standard errors are in parenthesis; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.16: Cross-Section Individual Regressions of Turnout on Closeness: 2012, Moderately Restrictive Sample

	(1)	(2)	(3)
Ex Post Competitiveness	0.00193***	0.00187***	0.00127***
	(0.0000397)	(0.0000420)	(0.0000425)
Female			0.0330***
			(0.000705)
Age 65+			0.177***
			(0.000874)
BA or Higher (%, Estimated)			0.00534***
			(0.0000199)
Hispanic			-0.0730***
			(0.00135)
Non-Hispanic Black			0.0248***
			(0.00121)
Non-Hispanic Asian			-0.177***
			(0.00217)
Observations	1875217	1875217	1847151
Adjusted R^2	0.001	0.020	0.069
State FEs	No	Yes	Yes

Robust standard errors are in parenthesis; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
The sample is comprised of contested general elections by a Democratic and Republican candidate.
All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.17: Cross-Section Individual Regressions of Turnout on Closeness: 2014, Moderately Restrictive Sample

	(1)	(2)	(3)
PVI Competitiveness	0.00183***	0.00180***	0.00128***
	(0.0000456)	(0.0000484)	(0.0000485)
Female			0.00376***
			(0.000703)
Age 65+			0.240***
			(0.000869)
BA or Higher (%, Estimated)			0.00558***
			(0.0000205)
Hispanic			-0.0975***
			(0.00126)
Non-Hispanic Black			-0.0154***
			(0.00116)
Non-Hispanic Asian			-0.184***
			(0.00204)
Observations	1781944	1781944	1754778
Adjusted R^2	0.001	0.021	0.094
State FEs	No	Yes	Yes

Robust standard errors are in parenthesis; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.18: Cross-Section Individual Regressions of Turnout on Closeness: 2014, Moderately Restrictive Sample

	(1)	(2)	(3)
Ex Post Competitiveness	0.00183***	0.00112***	0.000534***
	(0.0000435)	(0.0000461)	(0.0000454)
Female			0.00357***
			(0.000705)
Age 65+			0.240***
			(0.000872)
BA or Higher (%, Estimated)			0.00555***
			(0.0000206)
Hispanic			-0.0984***
			(0.00126)
Non-Hispanic Black			-0.0207***
			(0.00115)
Non-Hispanic Asian			-0.185***
			(0.00204)
Observations	1770957	1770957	1743848
Adjusted R^2	0.001	0.020	0.094
State FEs	No	Yes	Yes

Robust standard errors are in parenthesis; * p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.19: Panel Data Individual Regressions of Turnout on Closeness: 2008 & 2012, Moderately Restrictive Sample

	(1)	(2)	(3)
PVI Competitiveness	0.00170***	0.00138***	0.000130*
	(0.0000652)	(0.0000638)	(0.0000700)
Observations	3307000	3259460	3307000
R^2	0.017	0.063	0.003
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

Standard Errors, clustered at census block group level, are in parentheses.

* p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Dem. and Rep.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.20: Panel Data Individual Regressions of Turnout on Closeness: 2010 & 2014, Moderately Restrictive Sample

	(1)	(2)	(3)
PVI Competitiveness	0.00121***	0.000692***	-0.000107
	(0.0000690)	(0.0000617)	(0.0000702)
Observations	3196284	3146900	3196284
R^2	0.026	0.097	0.018
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

Standard Errors, clustered at census block group level, are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Dem. and Rep.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.21: Panel Data Individual Regressions of Turnout on Closeness: 2010 & 2012, Moderately Restrictive Sample

	(1)	(2)	(3)
PVI Competitiveness	0.00155***	0.000879***	0.000172**
	(0.0000685)	(0.0000641)	(0.0000688)
Observations	3401326	3349760	3401326
R^2	0.046	0.103	0.114
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

Standard Errors, clustered at census block group level, are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Dem. and Rep.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.22: Panel Data Individual Regressions of Turnout on Closeness: 2008 – 2014, Moderately Restrictive Sample

	(1)	(2)	(3)
PVI Competitiveness	0.000605***	0.000670***	0.000113*
	(0.0000803)	(0.0000761)	(0.0000592)
Observations	5415828	5335952	5415828
R^2	0.048	0.104	0.094
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

Standard Errors, clustered at census block group level, are in parentheses.

* p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Dem. and Rep.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.23: Panel Data Individual Regressions of Turnout on Closeness: 2008 & 2012, Moderately Restrictive Sample

	(1)	(2)	(3)
Ex Post Competitiveness	0.00179***	0.000963***	0.0000566
	(0.0000528)	(0.0000519)	(0.0000478)
Observations	3267210	3220158	3267210
R^2	0.017	0.063	0.003
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

Standard Errors, clustered at census block group level, are in parentheses.

* p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Dem. and Rep.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.24: Panel Data Individual Regressions of Turnout on Closeness: 2010 & 2014, Moderately Restrictive Sample

	(1)	(2)	(3)
Ex Post Competitiveness	0.000717***	0.000370***	0.000144***
	(0.0000589)	(0.0000517)	(0.0000499)
Observations	3182996	3133686	3182996
R^2	0.026	0.098	0.018
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

Standard Errors, clustered at census block group level, are in parentheses.

* p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Dem. and Rep.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.25: Panel Data Individual Regressions of Turnout on Closeness: 2010 & 2012, Moderately Restrictive Sample

	(1)	(2)	(3)
Ex Post Competitiveness	0.00134***	0.000716***	0.000246***
	(0.0000536)	(0.0000497)	(0.0000453)
Observations	3367960	3316742	3367960
R^2	0.046	0.104	0.114
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

Standard Errors, clustered at census block group level, are in parentheses.

* p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Dem. and Rep.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.26: Panel Data Individual Regressions of Turnout on Closeness: 2008 – 2014, Moderately Restrictive Sample

	(1)	(2)	(3)
Ex Post Competitiveness	0.000744***	0.000433***	0.000143***
	(0.0000591)	(0.0000557)	(0.0000343)
Observations	5352556	5273584	5352556
R^2	0.048	0.105	0.094
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

Standard Errors, clustered at census block group level, are in parentheses.

* p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Dem. and Rep.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.27: Panel Data Individual Regressions of Turnout on Closeness (Only States with No Majority Minority Districts): 2010 & 2014, Moderately Restrictive Sample

	(1)	(2)	(3)
PVI Competitiveness	0.00164***	0.00164***	-0.0000884
	(0.000208)	(0.000208)	(0.000259)
Observations	706440	706440	706440
R^2	0.031	0.031	0.012
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

Standard Errors, clustered at census block group level, are in parentheses.

* p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Dem. and Rep.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.28: Panel Data Individual Regressions of Turnout on Closeness (Including Time Varying District Characteristics): 2008 & 2012, Moderately Restrictive Sample

	(1)	(2)	(3)	(4)
PVI Competitiveness	0.000119	0.0000995	0.000114	0.000100
	(0.000139)	(0.000154)	(0.000127)	(0.000155)
Income Controls	Yes	No	No	Yes
Race Controls	No	Yes	No	Yes
Party/Incumbency Controls	No	No	Yes	Yes
Observations	3307000	3307000	3307000	3307000
R^2	0.003	0.003	0.003	0.003

Standard Errors, clustered at census block group level, are in parentheses.

* p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.29: Panel Data Individual Regressions of Turnout on Closeness (Including Time Varying District Characteristics): 2010 & 2014, Moderately Restrictive Sample

	(1)	(2)	(3)	(4)
PVI Competitiveness	-0.000125	-0.000127	-0.000107	-0.000233
	(0.000176)	(0.000186)	(0.000161)	(0.000193)
Income Controls	Yes	No	No	Yes
Race Controls	No	Yes	No	Yes
Party/Incumbency Controls	No	No	Yes	Yes
Observations	3196284	3196284	3196284	3196284
R^2	0.018	0.018	0.018	0.018

Standard Errors, clustered at census block group level, are in parentheses.

* p < 0.05, ** p < 0.01, *** p < 0.001.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.30: Panel Data Individual Regressions of Turnout on Closeness (Including Time Varying District Characteristics): 2008 & 2012, Moderately Restrictive Sample

	(1)	(2)	(3)	(4)
Ex Post Competitiveness	0.0000450	0.0000142	0.0000218	-0.00000881
	(0.0000954)	(0.000100)	(0.0000938)	(0.000107)
Income Controls	Yes	No	No	Yes
Race Controls	No	Yes	No	Yes
Party/Incumbency Controls	No	No	Yes	Yes
Observations	3267210	3267210	3267210	3267210
R^2	0.003	0.003	0.003	0.003

Standard Errors, clustered at census block group level, are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.31: Panel Data Individual Regressions of Turnout on Closeness (Including Time Varying District Characteristics): 2010 & 2014, Moderately Restrictive Sample

	(1)	(2)	(3)	(4)
Ex Post Competitiveness	0.000145	0.000157	0.0000337	-0.0000235
	(0.000121)	(0.000122)	(0.000122)	(0.000135)
Income Controls	Yes	No	No	Yes
Race Controls	No	Yes	No	Yes
Party/Incumbency Controls	No	No	Yes	Yes
Observations	3182996	3182996	3182996	3182996
R^2	0.018	0.018	0.018	0.018

Standard errors, clustered at census block group level, are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.32: Canonical Diff-in-Diff Individual Regressions of Turnout on Closeness: Binary Indicators for Competitiveness,

	2010 & 2014 PVI		2008 & 2012 PVI		2010 & 2014 Vote Marg.		2008 & 2012 Vote Marg.	
	DID	DID Matched	DID	DID Matched	DID	DID Matched	DID	DID Matched
PVI Comp.	-0.00147	0.00423	0.00689***	0.00243				
	(0.00171)	(0.00375)	(0.00165)	(0.00410)				
Ex Post Comp.					0.00763***	0.000226	0.000891	-0.000774
					(0.00134)	(0.00331)	(0.00109)	(0.00308)
Observations	1195758	410166	1298752	440964	1855950	519792	2193994	647292
R^2	0.024	0.017	0.003	0.005	0.021	0.016	0.003	0.004
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard Errors, clustered at census block group level, are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table B.33: Individual Regressions of Turnout on Closeness: Voters on the Margin, Mid-Term Elections (2010 & 2014)

	Marginal Voters		Top Quartile Educ.		Top Quartile Med. HH Inc.		Partisans (Reg. D/R)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PVI Comp.	0.000485***		0.000232*		0.000131		0.000333***	
	(0.000162)		(0.000128)		(0.000127)		(0.000115)	
Ex Post Comp.		0.000710***		0.000504***		0.000504***		0.000211***
		(0.000117)		(0.0000977)		(0.0000962)		(0.0000813)
Observations	696482	692688	892462	891250	870610	869876	1315588	1306226
R^2	0.043	0.044	0.020	0.020	0.025	0.025	0.025	0.025

Standard Errors, clustered at census block group level, are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The sample is comprised of contested general elections by a Democratic and Republican candidate.

All elections in Louisiana are excluded from the sample due to their unusual rules.

B.3.2 Figures

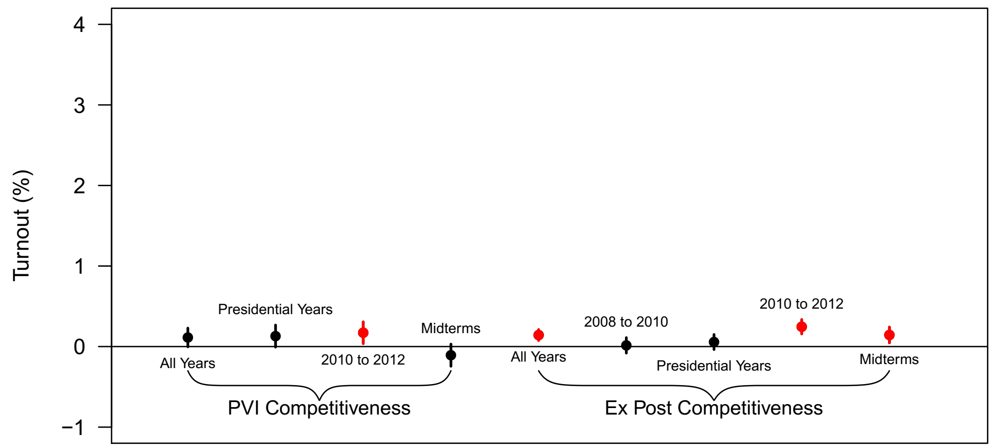


Figure B.13: Panel Data, Individual Fixed Effects: Marginal Effect of 10pp Increase in Closeness on Turnout (%). This figure illustrates the precise null effect when using individual fixed effects for both our ex ante and ex post measures of competitiveness, across all elections between 2008 and 2014, just mid-term elections, and just Presidential elections. The precise null also holds up when looking at the elections immediately before and after redistricting.

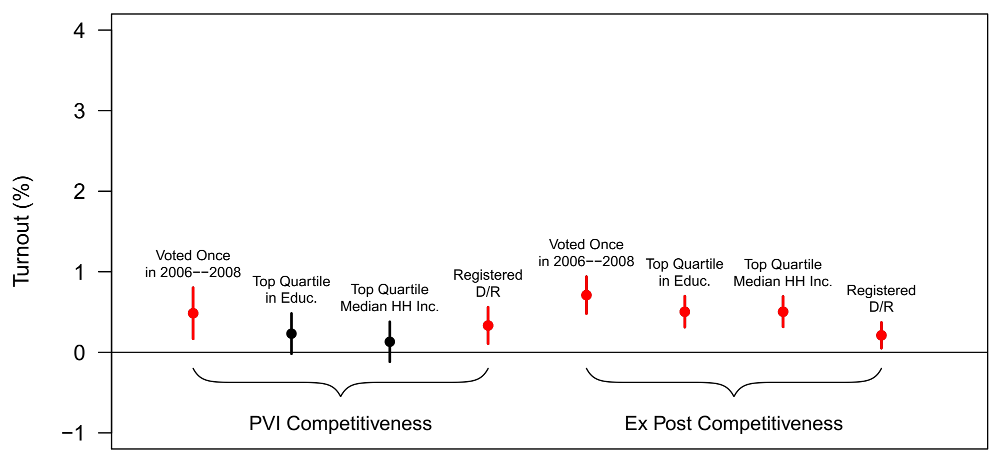


Figure B.14: Panel Data, Individual Fixed Effects: Marginal Effect of 10pp Increase in Closeness on Turnout for Voters Likely to Have High Responsiveness to Variation in Closeness (%). This figure illustrates the effects for subsets of the sample that should be particularly responsive to changes in closeness. For both measures of competitiveness, we estimate the effects for marginal voters (defined as people who were eligible to vote in 2006 and 2008 but only voted once), for highly educated voters, for voters residing in districts with a median household income in the top quartile, and for partisan voters (i.e., registered with one of two major parties). Among these groups, in which one would expect to find the largest effect possible, the effects are still minimal.

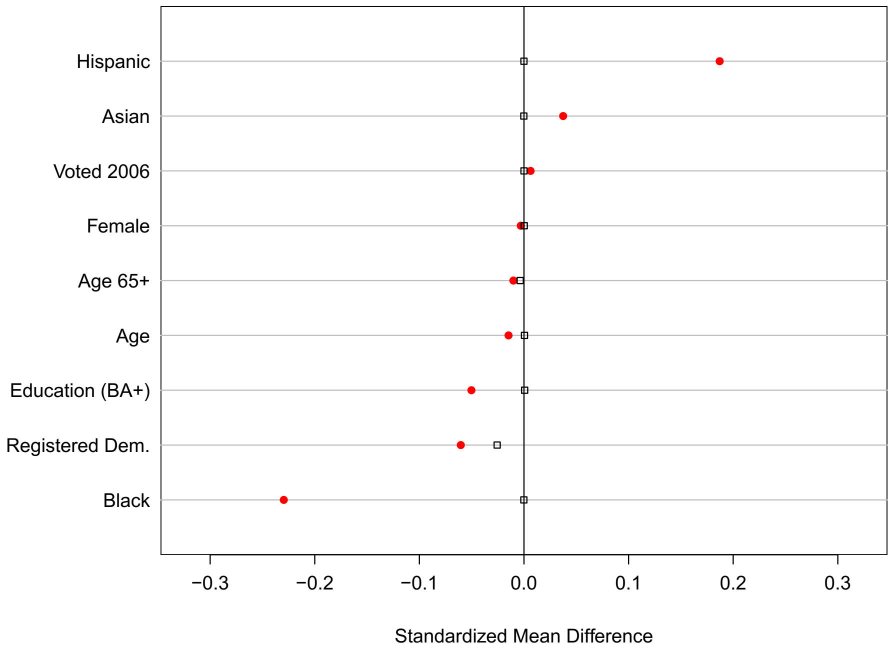


Figure B.15: Individual-Level Covariate Balance Plot: Competitive (PVI ≤ 10) vs. Uncompetitive (PVI > 10) Districts, 2010 & 2014. This figure illustrates the balance plot for a set of observed individual covariates (midterm election years) before and after employing an entropy balancing matching procedure. The red circles illustrate covariate balance for competitive versus uncompetitive districts (defined as having a PVI score with magnitude less than ten) before matching; the squares illustrate covariate balance after matching. The figure illustrates considerable balance particularly for several racial characteristics. We do not actually match on the Registered Dem. variable (due to a high incidence of missing data), but include it to illustrate that considerable improvement in balance occurs even for variables we did not match on.

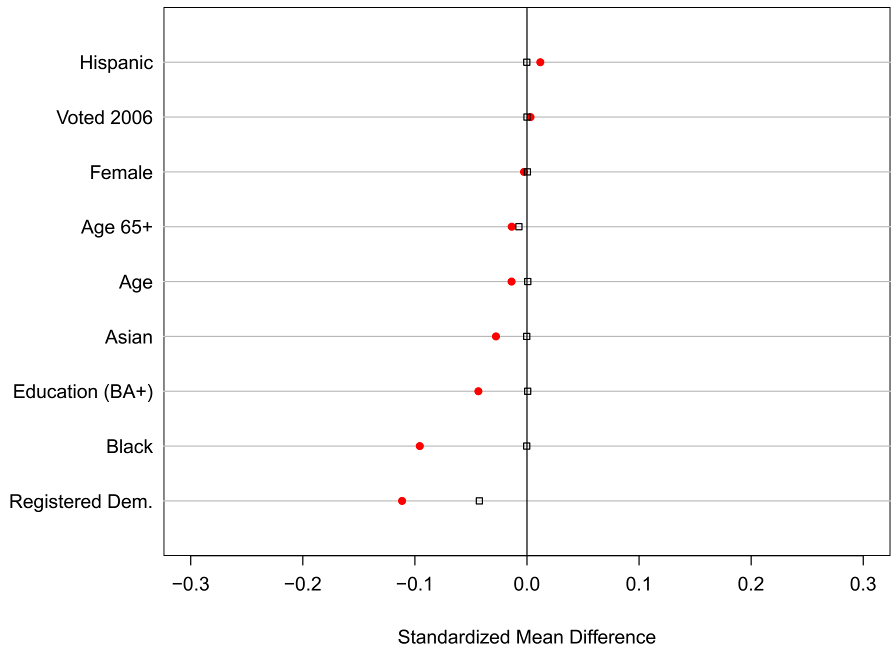


Figure B.16: Individual-Level Covariate Balance Plot: Competitive (PVI ≤ 10) vs. Uncompetitive (PVI > 10) Districts, 2008 & 2012. This figure illustrates the balance plot for a set of observed individual covariates (presidential election years) before and after employing an entropy balancing matching procedure. The red circles illustrate covariate balance for competitive versus uncompetitive districts (defined as having a PVI score with magnitude less than ten) before matching; the squares illustrate covariate balance after matching. The figure illustrates considerable balance particularly for several racial characteristics. We do not actually match on the Registered Dem. variable (due to a high incidence of missing data), but include it to illustrate that considerable improvement in balance occurs even for variables we did not match on.

B.4 Additional Robustness Checks

B.4.1 Incumbents Focus on Voters from Their Old Districts

One potential concern is that incumbents might focus their mobilization efforts on the voters they are most familiar with: voters who resided in their pre-redistricting district. Some incumbents experience dramatic changes to their districts (in terms of the share of residents who remain from the pre-redistricting period), while other incumbents experience minimal changes to their districts. If the degree to which incumbents' districts change (in terms of the share of residents from their old districts in their new districts) is correlated with changes in competitiveness, this would present a serious threat to inference. In this section, we investigate the extent to which the share of residents remaining with their incumbent from the pre-redistricting period is related to changes in competitiveness. For Figure B.17, we link districts across the redistricting period through incumbents. In other words, an observation is an incumbent seeking reelection in 2012. The horizontal axis indicates the proportion of residents in the 2012 district who resided in the incumbent's 2010 district, and the vertical axis indicates the difference in competitiveness between 2012 and 2010 districts for each incumbent. As is clear from the figure, incumbents who experience only modest changes in their districts also tend to experience only modest changes in competitiveness, while incumbents who experience more substantial changes in their district often experience more substantial changes in competitiveness. However, these substantial changes in competitiveness go in both directions: increases and decreases in competition. Overall, the slope is extremely flat, and the R^2 is a mere 0.004. In Figure B.18, we link districts from the pre- to post-redistricting period based on the share of voters. More specifically, a 2012 district i is linked to 2010 district j if at least 50% of the 2012 residents in i resided in j. If we link districts using this method, the results are nearly identical. Again, the slope is extremely flat, and the R^2 is 0.001. Based on these results, incumbent mobilization of voters from their old districts is *not* a threat to inference.

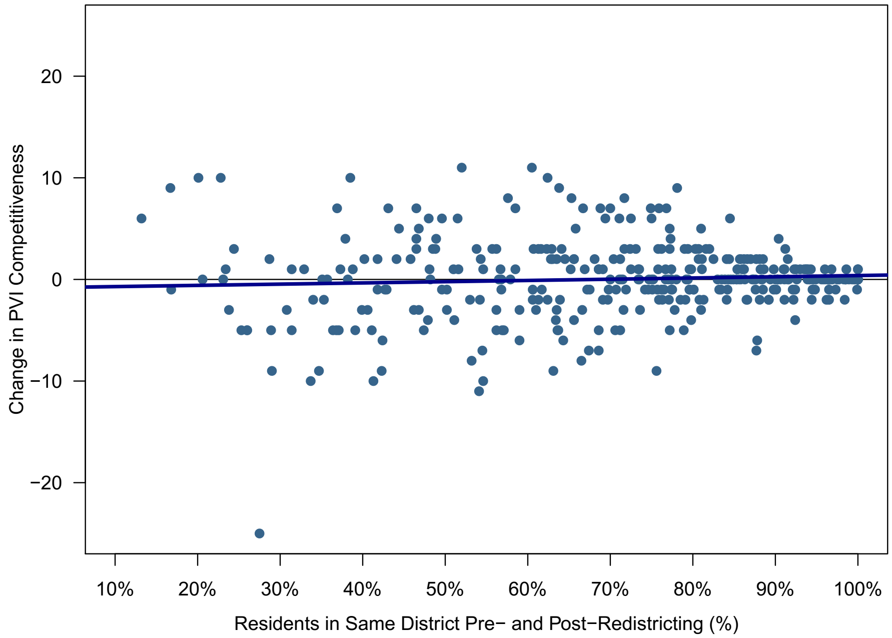


Figure B.17: Changes in Incumbents' Districts vs. Changes in Competitiveness. This figure illustrates the relationship between the degree to which incumbents' new districts are comprised of voters from their old districts and changes in competitiveness.

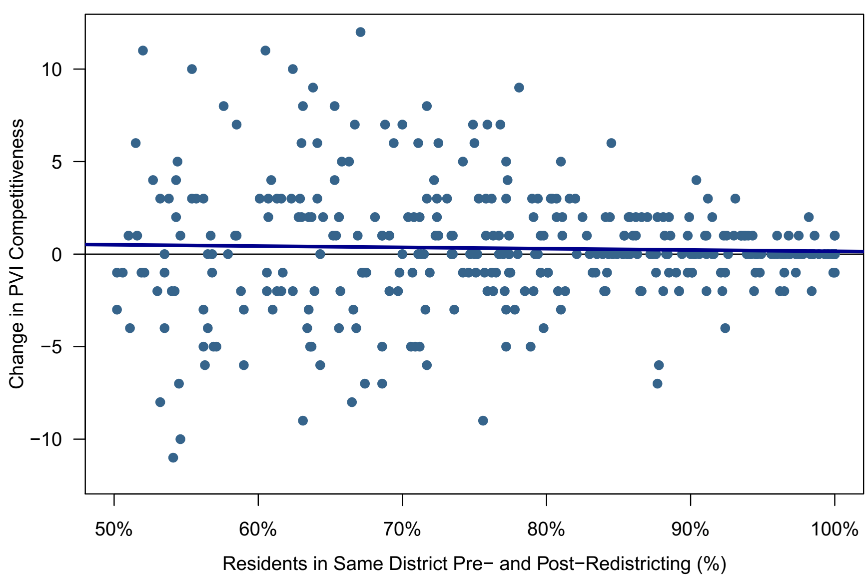


Figure B.18: Changes in District Population vs. Changes in PVI Competitiveness. This figure illustrates the relationship the extent to which new districts are comprised of voters from the same old districts and changes in competitiveness.

B.5 Proposed Mechanisms

B.5.1 Tables

Table B.34: Log Campaign Spending in 2010 House Elections

	(1)	(2)
PVI Competitiveness	0.040***	
	(0.004)	
Ex Post Competitiveness		0.052***
		(0.003)
Constant	14.795***	15.084***
	(0.054)	(0.050)
Observations	401	401
Adjusted R^2	0.206	0.434
State FEs		

Standard errors in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001
The dependent variable is the log of campaign spending by the two major-party candidates. The sample is comprised of contested general elections by a Democratic and Republican candidate. All elections in Louisiana are excluded.

Table B.35: Any Campaign Contact

	(1)	(2)	(3)	(4)
PVI Competitiveness	0.005**	0.003		
	(0.002)	(0.002)		
Ex Post Competitiveness			0.006***	0.004**
			(0.002)	(0.002)
Observations	13074	13074	13065	13065
R^2	0.003	0.055	0.010	0.059
Individual FEs	Yes	Yes	Yes	Yes
State-Year FEs	No	Yes	No	Yes

Standard errors (clustered by individual) in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001
The sample is based on voters in the CCES 2010-2014 panel and is restricted to voters in contested general elections by a Democratic and Republican candidate.
All Louisiana voters are excluded from the sample due to their unusual rules.
The dependent variable is coded =1 if the respondent reported any campaign contact.

Table B.36: In-Person Campaign Contact

	(1)	(2)	(3)	(4)
PVI Competitiveness	-0.001	-0.002		
	(0.002)	(0.002)		
Ex Post Competitiveness			0.001	-0.001
			(0.001)	(0.001)
Observations	13074	13074	13065	13065
R^2	0.000	0.037	0.000	0.036
Individual FEs	Yes	Yes	Yes	Yes
State-Year FEs	No	Yes	No	Yes

Standard errors (clustered by individual) in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
The sample is based on voters in the CCES 2010-2014 panel and is restricted
to voters in contested general elections by a Democratic and Republican candidate.
All Louisiana voters are excluded from the sample due to their unusual rules.
The dependent variable is coded =1 if the respondent reported in-person campaign contact.

Table B.37: Phone Campaign Contact

	(1)	(2)	(3)	(4)
PVI Competitiveness	0.005	0.004		
	(0.003)	(0.003)		
Ex Post Competitiveness			0.007***	0.005**
			(0.002)	(0.002)
Observations	13074	13074	13065	13065
R^2	0.003	0.065	0.015	0.070
Individual FEs	Yes	Yes	Yes	Yes
State-Year FEs	No	Yes	No	Yes

Standard errors (clustered by individual) in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
The sample is based on voters in the CCES 2010-2014 panel and is restricted
to voters in contested general elections by a Democratic and Republican candidate.
All Louisiana voters are excluded from the sample due to their unusual rules.
The dependent variable is coded =1 if the respondent reported phone campaign contact.

Table B.38: Text or Email Campaign Contact

	(1)	(2)	(3)	(4)
PVI Competitiveness	0.001	0.002		
	(0.002)	(0.002)		
Ex Post Competitiveness			0.001	0.000
			(0.001)	(0.001)
Observations	13074	13074	13065	13065
R^2	0.000	0.032	0.000	0.032
Individual FEs	Yes	Yes	Yes	Yes
State-Year FEs	No	Yes	No	Yes

Standard errors (clustered by individual) in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001
The sample is based on voters in the CCES 2010-2014 panel and is restricted
to voters in contested general elections by a Democratic and Republican candidate.
All Louisiana voters are excluded from the sample due to their unusual rules.
The dependent variable is coded =1 if the respondent reported text or email campaign contact.

Table B.39: Postcard or Mail Campaign Contact

	(1)	(2)	(3)	(4)
PVI Competitiveness	0.004	0.004		
	(0.003)	(0.003)		
Ex Post Competitiveness			0.006**	0.005**
			(0.002)	(0.002)
Observations	13074	13074	13065	13065
R^2	0.001	0.032	0.008	0.037
Individual FEs	Yes	Yes	Yes	Yes
State-Year FEs	No	Yes	No	Yes

Standard errors (clustered by individual) in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001
The sample is based on voters in the CCES 2010-2014 panel and is restricted
to voters in contested general elections by a Democratic and Republican candidate.
All Louisiana voters are excluded from the sample due to their unusual rules.
The dependent variable is coded =1 if the respondent reported postcard or mail campaign contact.

B.5.2 Figures

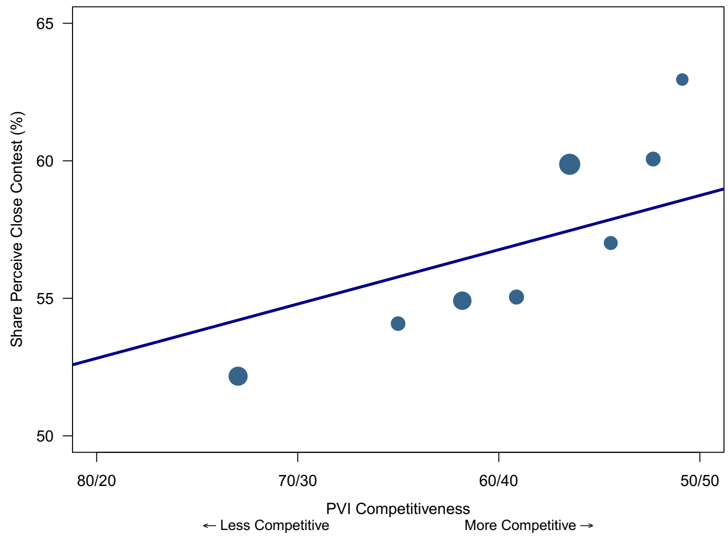


Figure B.19: Perceived Competitiveness vs. PVI Competitiveness. Based on tabulations of a 2006 Pew survey on perceived competitiveness of House elections. This figure illustrates the relationship between perceived competitiveness in the 2006 Pew survey and PVI Competitiveness; the correlation is relatively flat, as a 30 point swing in PVI Competitiveness is associated with only a ten point swing in perceived competitiveness.

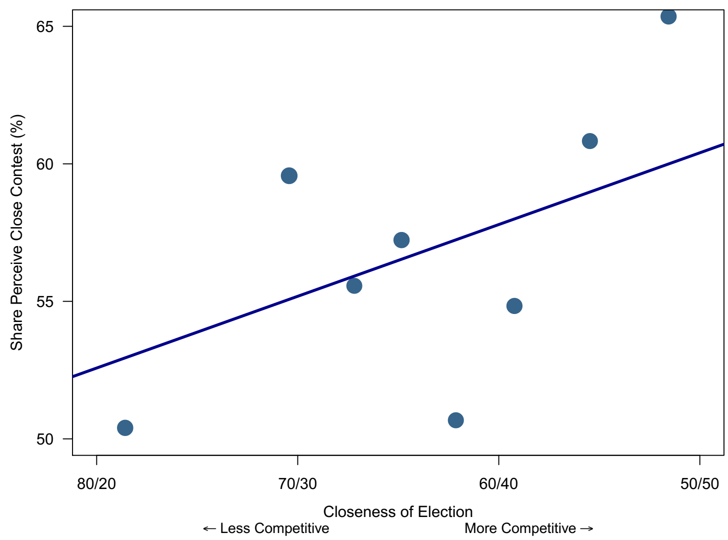


Figure B.20: Perceived Competitiveness vs. Actual Competitiveness. Based on tabulations of a 2006 Pew survey on perceived competitiveness of House elections. This figure illustrates the relationship between perceived competitiveness in the 2006 Pew survey and actual competitiveness in the election.

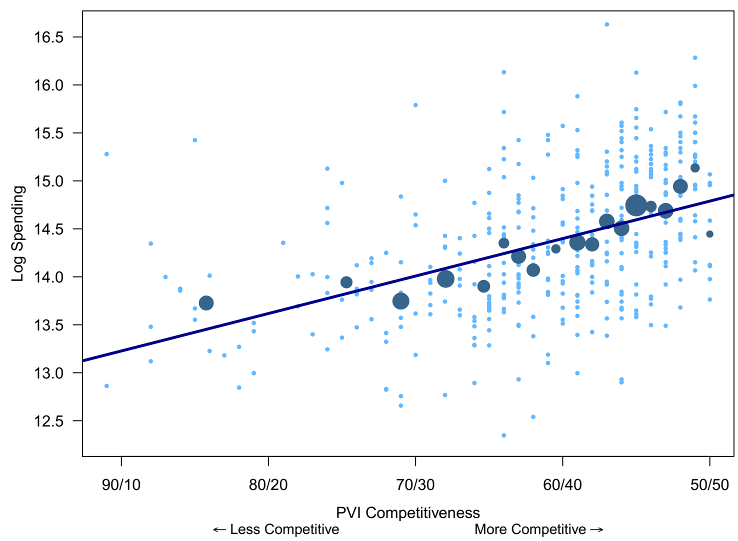


Figure B.21: Log Campaign Spending vs. PVI Competitiveness. This figure displays the relationship between campaign spending and competitiveness. There is a strong positive correlation between the two.

C | Appendix to Chapter 3

C.1 Analysis Using Stathis Data

The tables below replicate our primary analysis using counts of total legislation from Stathis (2014).

Table C.1: Significant Legislation in Divided/Unified Government

	All Congresses				1st-79th Congresses			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unified Government	-1.22	1.47	1.82	2.24**	3.21***	3.13***	2.29	3.58***
	(1.57)	(0.96)	(1.25)	(0.91)	(0.99)	(0.93)	(1.48)	(0.96)
37th-55th Congress		2.97**	6.50	1.31		3.38***	6.50	-4.17**
		(1.15)	(4.16)	(3.88)		(1.08)	(4.22)	(1.68)
56th-79th Congress		4.89***	2.50	2.69		4.71***	2.50	-3.46
		(1.29)	(4.16)	(3.45)		(1.30)	(4.22)	(2.23)
80th-111th Congress		14.74***	3.77	6.65**		0.00	0.00	0.00
		(1.24)	(4.90)	(3.14)		(.)	(.)	(.)
President FEs	No	No	Yes	No	No	No	Yes	No
Decade FEs	No	No	No	Yes	No	No	No	Yes
Observations	111	111	111	111	79	79	79	79
R^2	0.006	0.600	0.806	0.808	0.088	0.268	0.579	0.631

Standard errors in parentheses

Robust Standard Errors

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Δ Significant Legislation

	All Congresses			1st-55th Congresses			56th-111th Congresses		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δ Unified Gov.	3.14***	2.47***	3.13***	3.96***	3.36***	3.94***	2.06	1.44	2.06
	(0.85)	(0.85)	(0.85)	(0.79)	(0.78)	(0.77)	(1.66)	(1.63)	(1.67)
Lagged DV	No	Yes	No	No	Yes	No	No	Yes	No
Time Period Controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	110	109	110	54	53	54	56	56	56
R^2	0.107	0.345	0.108	0.252	0.430	0.254	0.032	0.319	0.032

Standard errors in parentheses
Robust Standard Errors
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.2 Data Collection Procedure and Sources

The data was assembled using a team of student coders. To ensure reliability in assembling the database, each decade was assigned to multiple coders. We anticipated some decades would be especially difficult, such as the 1930s or 1960s, so we assigned additional coders to those decades. Creating a database in this way is a complex task, as coders could approach their decades differently. We worked with the coders to standardize coding methods and databases across decades. We developed a common template, agreed on a common definition of "significant legislation" (which we describe in the body of this chapter), and collected data from the same set of initial sources. The key variables in the database template are bill names, descriptions, categories, outcomes, and roll call votes and dates. We also asked coders to collect information on committees and primary sponsors in each chamber when the data was available.

The use of common sources across time periods simplified the process of determining significance, as the authors of these works had already decided what bills they thought were important based on their own criteria. While these criteria may not match ours perfectly, they at least provided consistency across time periods. For legislation from 1789 through 1945, coders began with the bills listed in Castel and Gibson's 1975 *The Yeas and the Nays: Key Congressional Decisions, 1774-1945*. Castel and Gibson (1975) identified key legislation from each Congress and provided descriptions and vote totals for each. The American Political Science Review between 1910 and 1940 occasionally presented summaries of significant Congressional action during the term. For the 1950s through 2010s, coders began with the *CQ Almanac* for each year, and recorded all of the bills listed in the key votes section of each almanac. The 1940s were a particular challenge, as our key sources either ended in the 1940s or began in the 1950s. As a result, the coders working on the 1940s used a variety of sources, including *The Yeas and the Nays*, Mayhew's database on congressional actions, and Charles Cameron's database on major legislation. The coders supplemented these books with a variety of other sources that the librarians at Harvard University helped us to identify.

Additional sources included histories of Congress, online resources from the Library of Congress, and the Congressional Record (and its antecedents). Galloway and Wise's *History of the House of Representatives* and Josephy's *The American Heritage History of the Congress of the United States* were particularly useful for many coders. Galloway also included many useful figures in appendices, including counts of total public and private legislation in each Congress. Coders collecting data from the 101st Congress through the present used *The Library of Congress: THOMAS* (2015). The Library of Congress' site *A Century of Lawmaking For a New Nation: U.S. Congressional Documents and Debates 1774–1875* was also very helpful for collecting information on the first fifty Congresses. Coders looking for more detail on particular bills used the Congressional Record to collect information and understand the debates surrounding major bills. We spent substantial time working with the Congressional Record (as well as the Annals of Congress, Register of Debates, and Congressional Globe). The websites for the House, Senate, National Archives, and govtrack.us were also useful.

We encouraged all of the coders to make a pass through the Congressional Record for their given decade. They were asked to find the laws identified by *CQ Almanac* or *Yeas and Nays* or other sources as significant legislation in the Congressional Record. They were also asked to identify subjects on which there was much debate or activity in the index of the Record. The next step in assembling the database was to compile the individual databases from each coder into one comprehensive database and review the coders' work for consistency. We reviewed the database to remove duplicate entries (some decades were assigned to more than one coder) and any legislation that did not meet our significance criteria or was missing critical information. We then used keywords in the coders' categories and descriptions to categorize the bills into 46 categories. We also included counts of total public and private bills passed in each Congress. For the Congresses between 1789 and 1976 we used Appendix F of Galloway and Wise (1976), and for the remaining years we used counts from the Library of Congress.

Key Sources:

- *The Yeas and the Nays: Key Congressional Decisions, 1774-1945* by Albert Castel and Scott L. Gibson (1975).
- *The American Political Science Review*: between 1910 and 1940 occasionally presented summaries of significant Congressional action during the term.
- *Congressional Quarterly Almanacs*: 1948–2010.
- *History of the House of Representatives*, by George B. Galloway and Sidney Wise (1976). Includes counts of total public and private legislation in each Congress.
- *The American Heritage History of the Congress of the United States* by Alvin M. Josephy (1975).
- The Library of Congress
  - "A Century of Lawmaking For a New Nation." `http://lcweb2.loc.gov/ammem/amlaw/lawhome.html`.
  - THOMAS. `http://thomas.loc.gov/home/thomas.php`.

C.3 Robustness Checks

Table C.3: Divided/Unified Government and log(Legislative Output)

	log(Total Legislation)				log(Significant Legislation)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unified Government	-0.10	0.10	0.05	0.09	-0.04	0.18	0.38	0.31
	(0.15)	(0.06)	(0.06)	(0.05)	(0.14)	(0.12)	(0.15)	(0.11)
37th-55th Congress		1.11	0.61	1.81		0.11	-0.63	0.14
		(0.07)	(0.02)	(0.14)		(0.18)	(0.69)	(0.29)
56th-79th Congress		1.58	0.45	1.77		0.17	-0.53	0.13
		(0.09)	(0.02)	(0.13)		(0.19)	(0.69)	(0.29)
80th-111th Congress		1.71	0.54	2.07		1.05	-0.89	0.46
		(0.07)	(0.17)	(0.06)		(0.14)	(0.72)	(0.08)
President FEs	No	No	Yes	No	No	No	Yes	No
Decade FEs	No	No	No	Yes	No	No	No	Yes
Observations	111	111	111	111	111	111	111	111
R^2	0.004	0.863	0.954	0.935	0.001	0.342	0.697	0.629

Robust standard errors in parentheses
We transformed the outcome variable for log(Significant Legislation) by adding
one to address the one instance where a Congress produced no significant legislation.

Table C.4: Divided/Unified Government and Legislative Output with Time Trend

	Total Legislation				Significant Legislation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unified Government	-13.28	75.50	24.50	24.58	0.34	1.83	2.30	2.90
	(51.70)	(31.93)	(35.98)	(22.16)	(1.19)	(1.00)	(1.35)	(0.86)
Time Trend (Polynomial)	No	Yes	Yes	Yes	No	Yes	Yes	Yes
President FEs	No	No	Yes	No	No	No	Yes	No
Decade FEs	No	No	No	Yes	No	No	No	Yes
Observations	111	111	111	111	111	111	111	111
R^2	0.001	0.676	0.913	0.893	0.001	0.363	0.744	0.722

Robust standard errors in parentheses

Table C.5: Changes in Divided/Unified Government and log(Legislative Output)

	Δ log(Total Legislation)			Δ log(Significant Legislation)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Congresses						
Change to Unified Government	0.11	0.08	0.10	0.34	0.22	0.34
	(0.05)	(0.05)	(0.05)	(0.10)	(0.10)	(0.10)
Observations	110	109	110	110	109	110
R^2	0.041	0.127	0.048	0.077	0.248	0.078
Panel B: 1st–55th Congress						
Change to Unified Government	0.16	0.13	0.16	0.29	0.16	0.30
	(0.06)	(0.07)	(0.06)	(0.15)	(0.13)	(0.15)
Observations	54	53	54	54	53	54
R^2	0.086	0.190	0.089	0.055	0.271	0.056
Panel C: 56th–111th Congress						
Change in Unified Government	0.03	0.02	0.03	0.40	0.30	0.40
	(0.06)	(0.07)	(0.06)	(0.13)	(0.14)	(0.13)
Observations	56	56	56	56	56	56
R^2	0.004	0.057	0.010	0.117	0.227	0.117
Lagged DV	No	Yes	No	No	Yes	No
Time Period Controls	No	No	Yes	No	No	Yes

Robust standard errors in parentheses

Table C.6: Divided/Unified Government, Senate Supermajorities and Legislative Output, 1917-2010

	Total Legislation		Significant Legislation	
	(1)	(2)	(3)	(4)
Unified Government	39.84	36.73	0.11	2.27
	(65.26)	(69.12)	(2.11)	(1.98)
Senate Supermajority	85.32	87.18	-2.50	-3.80
	(91.73)	(93.29)	(2.24)	(2.25)
Unified Government x Supermajority	50.87	47.23	7.39	9.93
	(108.14)	(110.70)	(4.47)	(3.69)
56th-79th Congress		11.82		-8.21
		(64.64)		(2.12)
Observations	47	47	47	47
R^2	0.098	0.099	0.076	0.350

Robust standard errors in parentheses

Table C.7: Divided/Unified Government, the Budget Deficit and Legislative Output, 1901-2010

	Total Legislation		Significant Legislation	
	(1)	(2)	(3)	(4)
Unified Government	39.11	54.57	-0.80	2.70
	(67.02)	(65.79)	(2.03)	(2.02)
Deficit (Lagged)	-19.76	-15.75	-2.74	-1.83
	(276.86)	(249.94)	(8.41)	(4.13)
Unified Government x Deficit (Lagged)	-49.70	-39.70	10.75	13.01
	(309.10)	(290.42)	(9.53)	(5.12)
80th-111th Congress		42.76		9.68
		(58.91)		(1.80)
Observations	54	54	54	54
R^2	0.013	0.023	0.049	0.427

Robust standard errors in parentheses

Table C.8: Determinants of Productivity Under Unified Government

	Total Legislation		Significant Legislation	
	(1)	(2)	(3)	(4)
log(Time Since Pres. Party Held Unified Control)	-28.27	-36.62	1.79	0.65
	(47.82)	(30.14)	(1.27)	(1.12)
log(Time Since Party Opposing Pres. Held Unified Control)	17.41	15.37	1.04	-0.52
	(54.54)	(31.97)	(1.21)	(0.99)
log(Time Since Divided Government)	-63.79	6.91	-1.35	0.16
	(60.30)	(28.58)	(1.20)	(0.82)
Time Period Controls	No	Yes	No	Yes
Observations	62	62	62	62
R^2	0.022	0.731	0.106	0.526

Robust standard errors in parentheses

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