ABSTRACT

Title of Dissertation: ESSAYS ON POLITICAL GEOGRAPHY AND DISTRIBUTIVE POLITICS

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Doctor of Philosophy, 2022

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In this dissertation I study the impacts of a few key features of the United State's majoritarian political system. I investigate the effect of representation, majority political party affiliation in addition to how redistricting shapes the types of politicians that represent a state.

A large literature suggests that U.S. politicians are active participants in geographically targeting funds to their district. However, the motivations for politicians to pursue pork-barrel spending have not been tested empirically. In the initial chapter I use redistricting announcements as a natural experiment to test if politicians respond to changes in electoral incentives. During redistricting, congressional representatives learn mid-term what their new district will look like in the subsequent election, creating differential incentives for targeting within their current district. After learning their new district's boundaries, a representative who uses pork to help win re-election no longer has an incentive to target pork to places within their district that they will no longer seek to represent in the future. I find that following a redistricting announcement is made,

areas that remain in their district receive $0.75-$0.97 in federal project grants per-capita (25% of mean per-capita project grants) more than areas that will be redistricted, quantifying the degree to which members of Congress are forward-looking.

In chapter 3, I estimate the impact of majority political party representation on the distribution of intergovernmental funds using a regression discontinuity design. I use state elections instead of federal elections, which allow for testing heterogenous effects of majority party affiliation with government unification. The results suggest that the distinction between divided and unified governments is a crucial component to local distributive politics. I find effects ranging from increases of 1.2% to 3.7%, with the largest effects occurring when a representative is affiliated with the political party of a unified government. There is no impact found when the government is divided.

In the final chapter, along with co-authors Ken Coriale and Ethan Kaplan, I estimate the impact of a political party's legal ability to unilaterally redistrict Congressional seats upon partisan seat share allocations in the U.S. House of Representatives. Controlling for state-decade and year effects, we find an 8.2 percentage point increase in the Republican House seat share in the three elections following Republican control over redistricting in the past two decades. We find no significant or sizable effect for Democrats. The effects over the past five decades in aggregate are smaller and insignificant for both parties. In the past two decades, these effects are sizable though not pivotal for Congressional control.

ESSAYS ON POLITICAL GEOGRAPHY AND DISTRIBUTIVE POLITICS

by

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Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2022

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Acknowledgments

I am deeply grateful to my advisor, Ethan Kaplan. Ethan provided me with so much guidance it's hard to summarize just how indebted to him I am. I specifically want to thank him for his early insistence on thinking about new ideas and how to translate these ideas into an interesting paper. I also want to thank Ethan for the amount of his time I was allotted. I am also grateful to my other main advisor, Allan Drazen. Working with Allan helped me think about how to see the bigger picture instead of focusing on a narrow issue.

I also would like to acknowledge the political economy workshop that Ethan and Allan run. These seminars directly helped keep me accountable as well as provided early and constant feedback on potential research projects. It was during these seminars that I found the confidence to give useful feedback at seminars.

Numerous other members of the University of Maryland faculty have made contributions to my research. I would like to specifically thank John Wallis, Peter Murrell, Nolan Pope, and Jessica Goldberg for their thoughtful comments during seminars. For their patience with many issues I thank Terry Davis, Vickie Fletcher, and Jessica Gray. And I thank Cindy Clement for the guidance and advice she gave me when I taught my first course. I am also grateful for the generous financial support from the Ann G. Wylie Dissertation Fellowship.

Additionally there were many classmates which made my time at the University of Maryland a memorable one, through friendship and mentorship. I would to specifically like to acknowledge

mentorship from Cody Tuttle, Thomas Hegland, Mateo Uribe-Castro and Heath Witzen.

Finally, the path to becoming a Ph.D economist was filled great mentors. I would like to thank Ron Cheung for introducing me to research economics. Mark Bognanni and Yuliya Demyanyk for their patience during my early research assistance career. My family, for their enthusiastic support during this endeavor and my longtime partner, Jenny, for her reassurance, extensive editing assistance, and willingness to listen to me talk through research ideas.

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Chapter 1: Introduction

The United States' majoritarian political system incentivizes politicians to seek benefits for the areas they represent, at the possible of cost to those they do not. Specifically, geographic representation incentivizes politicians to bring home localized benefits to their constituents that are largely paid for by the tax dollars of non-constituents.

Mayhew (1974) outlines politicians' primary goal of being re-elected. According to Mayhew, to maintain their office, politicians engage in three types of office-motivated activities: advertising, credit-claiming, and position taking. Pork-barrel spending is most often associated with enhancing the politician's ability to claim credit.

When politicians seek votes in a narrowly defined geographic region, the office-motivated politician focuses on this region. In the case of pork-barrel spending, the politician prefers more spending for their constituents. The politician can them claim credit of these benefits, hoping their constituents believe they were responsible for the outcome and vote for them in the next election.

This type of geographic incentive exists not only for congressional representatives, but also for senators who prefer spending to occur within their state. A similar hierarchy also exists within state governments with state politicians representing specific areas within a state. In this dissertation, I study how politicians respond to these geographic incentives and how these effects

may persist.

I begin with two chapters that explicitly study two possible distributive mechanisms. In the initial chapter, I investigate the effect that representation has on the distribution of federal awards exploiting a sharp geographic discontinuity in the electoral benefits of areas within a congressional district. To study this effect, I rely on redistricting creating periods of time within a decade when politicians are reassigned to new districts.

Redistricting produces a moment in the middle of a congressional term when the representative learns the boundaries of the district they will attempt to represent in the future, and how that differs from their current district's boundaries. This results in the country being divided into two types of areas: areas that maintain their politicians and areas that change politicians. This experimental setting provides credible empirical evidence that politicians do respond to the geographic incentives that majoritarian systems provide.

Since 2005, the United States government has spent more than $1 trillion annually on discretionary spending. This type of spending is optional and largely directed through appropriation bills. The important feature of this spending is that members of Congress have the ability to influence it. The appropriations process is part of the budgetary process, which gives more direction as to how exactly funds are to be spent. The president can veto these bills but must reject the entire bill and not specific items within a bill. Outright veto concerns are a major reason these bills are often folded into omnibus spending bills, which contain large packages of funding. This also provides politicians the opportunity to provide targeted spending to their districts.

The initial chapter focuses on where new non-discretionary categorical spending is directed in the United States before and after redistricting announcements are made. This is a significantly

smaller set of funds, at just over $100 million in annual spending. However, this is the set of funds where it is most plausible to observe a change in funding as a response to a change in areas of representation.

In the next chapter, I study how a politician's affiliation with the majority political party impacts the amount of discretionary funds the area receives. Typically, this channel has been studied by looking at congressional representatives and their political affiliation to the majority party in Congress and the president. However, I study this relationship at the state government level using a regression discontinuity design based on state elections. This allows me to estimate how these effects differ depending on the strength of the majority political party. I find that targeted distribution is highest when the district's political party matches both the governor's party and the majority party in the legislature.

The spending studied in this chapter is distributed in a similar method as in the initial chapter, but in this case, the main actors are the state legislators. The state budgetary process closely resembles the federal process with a few key distinctions. In particular, states give varying degrees of line-item veto authority to the governor. This provides governors with more power in the budget process than the president. At the federal level, omnibus spending bills make it more difficult to veto appropriation bills outright, but governors can simply veto line-items within an appropriations bill. However, it is exceedingly rare for governors to utilize this power in relation to spending. The other difference is that states tend to have additional budgetary rules, such as a balanced budget requirements.

Despite these key differences, the general operations are similar. It is a difficult task to study how the strength of the majority party influences distributive tendencies in the federal government due to a lack of variation. However, state level analysis offers 50 state observations for every

single federal observation. This increase in power provides the ability to make distinctions in how the distributive process differs when a single political party holds the majority in both the legislature and office of the governor.

This type of power has important implications not only for studying the distribution of funds within the state, but also for studying the types of politicians a state sends to the national Congress. The final chapter of this dissertation studies how a political party with legal control over the redistricting process in a state can lead to persistent differences in the types of politicians that state sends to Congress.

In the United States, states are responsible for making their own political districts. Every decade, states undergo the redistricting process. In some instances, a single political party has legal control of the process, while in others, it is impossible for this to be case, and in some others, there is no single ruling party. Along with my co-authors Ken Coriale and Ethan Kaplan, we study how federal representation in states changes due to a single-ruling redistricting party.

At first, the connection between redistricting and distributive politics may not be clear, but the two are very closely related. Redistricting impacts both state and federal house members by determining the boundaries for their districts. These boundaries can influence not only which politician wins, but the types and amount of funding the area receives. Finally, at least in the case of state politicians, there is more distributive targeting when there is a single ruling party. This dissertation further expands on these topics.

Chapter 2: Forward Looking Congress? Evidence from Redistricting Announcements

2.1 Introduction

During the 2012 fiscal year, the United States government distributed more than 80 billion dollars of new federal assistance to local governments, firms, and individuals. But who or what determines where these funds eventually land? The vast majority of these funds are allocated according to formulas based upon area characteristics, but a subset of these awards is distributed with discretion. The primary focus of this paper is to provide a causal link between a politician's re-election incentives and the amount of discretionary federal funds an area receives.

In democracies, elections provide incentives for politicians to behave in the interest of those they represent. When a politician supports issues their constituents disagree with, doesn't bring home benefits to the areas they represent, or brings home the wrong benefits, they may face consequences in the coming elections in the form of fewer votes. In majoritarian winner-take-all political systems, such as those in the United States, politicians choose which geographically-defined areas they wish to represent, whether that be a state or congressional district. This geographic representation incentivizes politicians to obtain benefits to the local areas that they represent. This may take the form of directly assisting recipients in obtaining federal awards or passing legislation that their constituents believe in.

A central feature of an election is that it offers voters an opportunity to reward the incumbent

by re-electing them, or punish them by electing a challenger, holding the politician accountable. In turn, the incentives that elections provide should act as a powerful mechanism that pushes politicians towards policies that their voters prefer. However, in the case of distribution, these incentives may lead to allocations that deviate from the socially optimal allocation. In fact, they will encourage the politician to bring home a large amount of benefits since they will be financed at large. This paper offers an opportunity to understand the extent to which politicians respond to these incentives.

Politicians potentially use a combination of backward- and forward-looking behaviors in the dispersal of both funds and policy. I define backward-looking behavior as motivations relating to politicians rewarding their supporters for past support and forward-looking behavior as motivations consistent with politicians attempting to bolster future support. These two incentives are fundamentally different, with backward-looking behavior requiring a politician to commit to voters who previously supported them; on the other hand, forward-looking behavior requires that voters commit to politicians, in which case a politician acts in the voter's interest in hopes of securing re-election.

Perhaps for these reasons the majority of federal funds in the United States are allocated in ways that minimize discretionary control by individual members of Congress. For example, a substantial fraction of federal funds are allocated through the use of formulae. However, there still exist funds through which politicians are able to exercise these behaviors. The literature linking politics and the disbursement of resources, distributive politics, has consistently found that partisan attachments play a role in the distribution of funds (Albouy (2013) and Berry, Burden, and Howell (2010), for example). However, to date, no one has credibly quantified the importance of forward-looking electoral incentives on the distribution of funds.

The empirical challenge is that Congress does not label distributed funds according to their

motives. In this paper, I identify a method for estimating a lower bound of the forward-looking component. To do this, I focus on a period of time when redistricting temporarily eliminates forward-looking incentives for a fraction of a politician's constituents within their congressional districts.

The manner in which federal funds are allocated relies heavily on the characteristics of an area. Formula funds, the largest type of funding, are directly tied to these characteristics. Even non-formula funds are likely influenced by an area's endowments, as a competitive grant will take these characteristics into account. These funds consider characteristics such as population, demographics, and socio-economic variables. Some examples of formula-based funding include federal highway grants and Medicaid. We may then expect that general funding norms are the largest contributor to determining how funds are distributed. Beyond funding norms are ideological norms, which are likely more influential over the types of projects that are funded in an area. Ideological norms help explain why certain areas of the country receive different types of public goods, based on the preferences of their constituents. Finally, political incentives may additionally influence the spatial distribution of funds. Politicians operate with both backward- and forward-looking motivations. The backward-looking component is the set of funds allocated by politicians seeking to pay back their supporters for their votes or contributions in prior elections. The forward-looking component, on the other hand, is the set of funds used by politicians to enhance electoral support for future elections.

The identification challenge of estimating the effect of forward-looking electoral incentives is that it requires holding all other funding determinants fixed while utilizing variation in electoral incentives. The reasons that some districts receive greater or smaller amounts of federal funding may be that the underlying characteristics of these districts or their representatives are sufficiently

different. The naive solution, exploiting the fact that some districts will have incumbents and others do not, does not adequately identify this channel. Under this framework, non-incumbents cannot plausibly influence the distribution of federal awards. However, a politician's decision to run for election may be endogenous to the amount of funds won, or a politician's re-election prospects may influence their position in the bargaining process.

Isolating the impact of electoral incentives requires a framework that holds funding norms, backward-looking incentives, and endogenous determinants fixed, while providing a sharp change in electoral benefits. To properly account for the forward-looking effect of elections on fund distribution, I exploit variation in electoral incentives within rather than across legislators. I exploit a natural experiment from the United States where I can make use of this type of variation. Redistricting in the United States creates a short period of time once in a decade when the political benefits to a politician granting funds changes sharply and immediately without the other determinants of funding changing at the same time.

I employ a research design that takes advantage of this geographic discontinuity in electoral benefits within a congressional district. I create sub-congressional district units of geography based upon the pre- and post-redistricting assignments and geocode federal awards within these areas. This allows me to compare federal funding within a congressional district, where I am able to identify whether an area provides electoral incentives for the incumbent representative or not. I am then able to include a series of both time-varying and time-invariant spatial fixed effects, to account for differences in area characteristics, ideological norms, and the local patterns in funding. I show that my estimates can be interpreted as the difference in electoral incentives between two types of politicians, the carried-over politician and the incoming politician. The conservative interpretation of this estimated effect is hence a lower bound on the effect of electoral

effort on federal awards.

I additionally identify a subsample that is free from the inclusion of incoming politicians. I construct this sample by using experiments when there is no incoming incumbent politician in the redistricted portion of a congressional district. This can occur under any of three scenarios: when a state receives an additional congressional seat, when a representative leaves office, or when two incumbents run for re-election in the same district. Overall, this sample is small, but the point estimates on the estimated electoral incentive effects are similar to the full sample. This is theoretically justified if the primary motivation for electoral incentives is credit-claiming, with the politician claiming credit of localized benefits to win more votes. When a part of the district is redistricted, constituents may assign credit for a localized benefit to either the new or old politician. This uncertainty would cause politicians to be less likely to exert effort for these areas.

Estimates reveal that members of congress are forward-looking, with areas that are not redistricted receiving more funding than areas that are redistricted to new representatives. I find that areas with an incumbent politician who maintains their constituents receive 25-30% more of mean discretionary federal grants. I focus my attention on project grants, discretionary grants, and grants received by non-governmental entities. I prefer these outcomes because they produce highly visible results, which are desirable for electoral benefits.

I also replicate the experiment in periods when no redistricting event occurs. The primary concern is that carried-over regions may differ and may generally be targeted as elections near, especially if politicians prefer awards closer to elections. If this were true, the results should hold when using federal awards from non-redistricting periods. Instead, I show that in the three elections following the 2010 redistricting cycle, there are null results for the pseudo-carried over

regions.

The rest of the paper proceeds as follows. Section 2 presents a review of the theoretical and empirical literature. Section 3 describes the institutional settings of the redistricting process. Section 4 outlines the data used and procedures to create relevant funding variables. Section 5 outlines the primary empirical methods. Section 6 contains results, with section 7 validating identifying assumptions. Finally, section 8 concludes the paper.

2.2 Related Background

This paper attempts to link three strands of literature. First, it takes a common theoretical behavior, an election seeking, pork-barreling politician and tests empirically whether this is an actual phenomenon. Additionally, this work sits between two branches of empirical literature. On one-hand it has been shown that politicians work to bring more funds to their district. It has also been shown that they are electorally rewarded when this happens. However, the question remains are they motivated to bring awards to their district for the re-election benefits and how do politicians respond to changes in electoral benefits?

There is a rich theoretical and empirical literature related to distributive politics. However, there still lacks comprehensive estimates on the significance that electoral benefits play in the distribution of funds. Mayhew (1974), establishes the formal link between the disbursement of federal awards and election seeking behavior. In his model, a single-minded election seeking politician displays election seeking behavior through advertising, credit-claiming and position taking. Federal award disbursement is consistent with all three of these activities. Federal awards can be used to allow the politician to part-take in ribbon-cutting (advertising and credit-claiming)

and showing the types of projects they support (position-taking) with credit-claiming playing a predominant role. A credit-claiming politician attempts to make their constituents believe they are responsible for certain outcomes, particularly localized benefits in hopes of garnering more votes.

In Congress, representatives may not even know whether particularized benefits helps them at the polls; however, Mayhew comments that "the lore is that they count," and that is enough for politicians to use pork-barreling. The role that pork plays in electoral outcomes has been subject of numerous studies. Initially, Feldman and Jondrow (1984) conclude that increased federal spending in a district has no effects on candidate vote margins. Bickers and Stein (1994) suggest that the relationship between federal spending and electoral margins is not that simple, that constituencies may respond heterogeneously to pork-barrel benefits depending on how attentive they are. They conclude that electorally vulnerable politicians are the most likely to seek awards, and are most often rewarded when constituencies are highly attentive.

If vulnerable politicians represent those who work hardest for funds this presents issues with identification. Any estimated effect of the impact of pork on vote-margins will be influenced by electoral vulnerability. Levitt and Snyder (1997) solve this problem by introducing an instrumental variable framework, instrumenting within district federal awards with the out of district, but in state federal awards. The intuition is that how much funding a state receives is a function of many actors, such as senators, house members, and the governor. However, out of district, in state funding is unlikely related to the electoral vulnerability of a given candidate. They find large effects of pork on voting margins, with a 1 standard deviation increase of per-capita federal awards leading to a 2% increase in popular vote.

Models of distributive politics take Mayhew's basis of a single-minded election seeking

politician, and allow for pork-barreling to influence election results. Mayhew's basic model can be shown that politicians prefer more funding all else equal. However, more nuanced models explicitly model who a politician targets; either core-supporters or swing-voters.

The experimental framework in this paper is mostly agnostic as to which constituencies politicians prefer to target. The experiment compares regions with no electoral benefits to regions with electoral benefits. Because of this, it can be argued that outgoing regions are neither core-supporter nor swing-voters for the politician, as they are unable to vote for them in the upcoming election.

In the context of a redistricting experiment, risk-aversion and credit-claiming are closely linked. This relates to how politicians work for funds in their new regions that they do not represent until the following election. Since constituents may attribute the funding to either candidate, a risk-averse candidate would first focus on areas where funds would be fully attributed to the politician.

These models rest on the assumption that voters are aware enough to update their preferences as result of the benefits they receive. The Bickers and Stein (1994) result can be summarized as salience matters. This is further supported by Snyder and Stromberg (2010), where areas with congressional representatives who are covered less by the press receive less federal spending.

Salience plays an important role in the redistricting framework. Voters may not follow when and exactly how their congressional district changes. Under this premise, new politicians have little incentive to attempt to bring funding to the areas they will newly represent prior to representing the area.

More generally, it has been shown that politicians are active participants in geographically targeting funds. For instance, Albouy (2013) and Berry, Burden and Howell (2010) show that

House of Representative members from the same party as the president receive more funding. Clemens and Veuger (2021) show that an additional legislator per million residents leads to significantly more COVID-19 relief funds, additionally Yuan (2020) shows this additional legislator effect more broadly across multiple election-cycles using Japan's mixed-member electoral system. Kolliner (2021) shows that districts with state legislators aligned with the political party that holds a trifecta receive substantially more than districts who are aligned under all other types of regimes. These effects have been extensively studied internationally as well, with Brollo and Nannicini (2012) using a regression discontinuity design to show that mayors from the same party as the Brazilian president receive more funding in pre-election years. Sole-Olle and Navarro (2008) show politically aligned municipalities in Spain receive more government funding and Fiva and Halse (2016) show that local Norwegian politicians from the same party as the regional governors receive more funding for local investments.

Past studies, have used term limits to identify whether politicians respond to changes in electoral incentives have been. Bernhardt, Dubey and Hughson (2004) characterize a model that shows term-limits are useful when senior politicians are able to extract benefits at the expense of junior members. In these models, the use of term-limits forcibly removes a politician's prospects for re-election, and hence removes electoral incentives.

Despite the intuitive appeal of this framework, term-limits do not provide as sharp of change as the models predict. Namely, career politicians still may exhibit strategic forward-looking behavior. The introduction of term-limits does not impede politicians from running for an alternative office. For instance, only 13 states do not have some form of term-limit imposed for governors. Governors in their last term may elect to run for an alternative office, such as the Senate. For governors with career political aspirations, a term-limit does not shutoff

electoral incentives, since voters will evaluate the governor based on their performance in office[1]. Additionally, members of the state legislature face a similar career problem, where their reputation still matters in upcoming elections in different offices[2]. This type of behavior can be further extended to careers outside of holding office, where politicians in their last term may cater to special interest groups to bolster their post-office prospects.

Besley and Case (1997) frame term limits as sources of electoral accountability. When politicians must maintain a reputation for re-election they face different incentives than when they cannot. Using governor term-limits they find that policy choices vary when governors face a term-limit. In particular, they find that a states expenditures grow when a democratic governor in their last term is in office. On the contrary, Aidt and Shvets (2012) formally test the link between term-limits and intergovernmental transfers using state legislatures term limits. They find that when politicians are in their last term, the amount of funds that flow to their district substantially decrease.

It may seem that these two results are at odds with one another, on one hand spending increases with term-limits, and on the other it decreases. However, the finding ignores the pork for policy exchange. If the main way that legislators secure pork is by brokering deals with other politicians in exchange for future benefits, then politicians in their last term would be less valuable trading partners. In the case of term-limits, this may be the driving factor for declining transfers in districts represented by lame-duck representatives.

It's not unreasonable to assume that part of a politicians incumbency advantage is the ability to win federal awards. However, there are alternative reasons as to why politicians

[1]Governor to senator is a common career transition, with the Former Governors Caucus having 9 members in the Senate.

[2]Since 2005, about half of U.S. representatives and senators have had state legislative experience. As of 2021, 49% of Congress are former state legislators.

may seek to win these awards. Politicians may exhibit intrinsic reciprocity, rewarding voters for past behavior. Alternatively, politicians often make campaign promises relating to these expenditures, a politicians reputation may depend on fulfilling these promises which do not vary with geographic electoral benefits.

Political reciprocity has most often been studied using laboratory experiments. Reciprocity and political exchange are closely related. Enemark, Gibson, McCubbins, and Seim (2016) design an experiment that shows reciprocity is learned in office. More directly, Dalmia, Drazen, Ozbay (2020) design a model that studies how reelection concerns affect reciprocity. The laboratory experiment supports that electorally motivated candidates may reduce their intrinsic reciprocity, but it does not completely eliminate it.

This is not the first study to consider how redistricting may interact with pork. Ansolabehere, Gerber and Snyder (2002) exploit a series of court decisions which forced state legislative districts to balance their populations. The results show that redistricting can play a substantial role in altering the geographic pattern of funds, with funds following legislators.

Chen (2010) presents evidence on how electoral geography and pork-barreling are related. He examines a redistricting event of New York State's bicameral legislature and finds that district fragmentation significantly alters the flow of funds to districts. He shows that adding more politicians to a unit of political geography decreases the amount of funds received. He argues this is a result of credit-claiming politicians, and when geographic overlap is high collaboration is easier between politicians. Taken together, these papers provide an intuition that first, the redistricting process can alter the distribution of funds and that when constituencies change, funds also reflect these changes. This paper adds to the literature that uses redistricting as a natural experiment by introducing a new way to exploit what redistricting fundamentally changes,

electoral benefits.

2.3 Institutions And Timing

In the United States, congressional districts form the political geography for the U.S. House of Representatives. All states receive at least one district, with larger states having many districts. All representatives hold office for two-year terms without a term limit and all districts face reelection at the same time in early November of even years. As a result, the two-year term recreates the House every election, so that in any given election every House member is up for re-election.

The process that determines how districts are drawn is a two step procedure. The first step, known as apportionment, occurs nationally with the U.S. Congress dividing the 435 seats of the House among the 50 states based on population counts from the decennial census. This occurs in the January following a decennial census and includes the states resident population as well as overseas federal employees. These counts do not exclude populations that are ineligible to vote, such as unauthorized immigrants or children. The apportionment formula has been unchanged since 1940 and is called the method of Equal Proportions (Huntington-Hill method). The method assigns seats according to a priority value that is determined by multiplying the states population by a multiplier and ranking the outcome[3].

Once this is done, the number of districts allocated to each state is passed onto to the states,

[3]The multiplier is the reciprocal of the geometric mean of adding an additional seat. The multiplier takes the formula of $1/\sqrt{n(n-1)}$ where n is the number of seats for a multiplier. Multipliers are computed for between 2 and a maximum value and then multiplied by each states population. All states receive at least 1 district, so the remaining 385 seats are allocated according to having the highest rank of this formula. For instance, California's population in s 2010 was 37.27 million and was awarded 53 seats. The value for the 53rd seat would be $1/\sqrt{53*52} = .019*37.27 = .709$. On the other hand, Texas was awarded 36 seats. Texas did not receive a 37th seat due to having a proportion value of .69, which was lower than California's 53rd.

who are the primary actors in the second step. In this step, states draw the new congressional boundaries for the following decade. States control how exactly they draw these maps with little federal oversight. However, states must adhere to a few restrictions. In 1964, a Supreme Court ruling, *Wesbury v. Sanders*, established that congressional districts must be redrawn in a "timely manner" following the decennial census and that populations among districts must be roughly equal in population[4]. Prior to this ruling population was not a constraint when it came to drawing districts, resulting in under-representation of fast growing urban areas. Additionally, the Voting Rights Act prohibits plans that intentionally or inadvertently discriminate on the basis of race [5].

The redistricting process dramatically changes the political geography of congressional districts. In 2010, over 33% of the US population was redistricted into a new district, with states being required to part-take in this event due to apportionment and equalizing populations among districts.

States are free to determine their own redistricting procedures. My experimental design only utilizes data from the 2010 redistricting cycle. This redistricting cycle began in January of 2011, with the apportionment step. By April of 2011 states receive detailed population data from the decennial census. Then the redistricting process began in every state that was apportioned to have more than 1 representative[6]. In general, states have varying deadlines as to when the new districts must be agreed upon, with the majority of states using the candidate filing deadline.

Broadly there are two ways in which states redistrict. The majority of states treat redistricting

[4]For congressional districts there little leniency when it comes discrepancies amongst district populations. In 1984, the Supreme Court ruled in Karcher v. Daggett that a population discrepancy of 0.68% in New Jersey's 1982 redistricting plan was unconstitutional, that it did not represent a good faith effort to achieve population equality.

[5]Thornburg v Gingles (1986) led to the Voting Rights Acts being amended to clarify that violations need to have a discriminatory effect. This court case formed the basis of identifying racially gerrymandered districts through the use of the Gingles test. The Gingles test requires compactness of the minority group, political cohesion of that group, and the likelihood that white voters would vote against that groups preferred candidate.

[6]In this period, 7 states had a single representative.

like any other law. In these states, the legislature or the legislature and governor draw the maps. Alternatively, a smaller number of states (12) used a commission to draw the new maps. The composition and nomination of a redistricting committees varies by states, but the goal is often (but not always) to create a non- or bipartisan commission.

In most cases, redistricting is a highly political process within states. The objectives for district map-makers are not explicitly known, however it is often thought that state politicians draw boundaries strategically to benefit their friends. The concern in this experimental design is that the parts of a district a politicians keeps compared to those which they do not systematically differ in such a way that the non-redistricted areas are predisposed to receiving more funds. In section 2.5.4, I find limited evidence of this behavior within congressional districts, which aligns with past work done by Ferejohn (1977), Ansolabehere, Snyder, and Stewart (2000), and Abramowitz, Alexander and Gunning (1991).

Table 2.1 summarizes the relevant redistricting timeline. Redistricting began in January of 2011 with apportionment. Then beginning in April of 2011 states began to create new congressional districts. Iowa was the first state to approve a redistricting plan in April of 2011 and New Hampshire was the last state to approve such a plan in April of 2012. In November of 2012, an election for all members of the House of Representatives was held which used the newly created congressional districts. While there are important intricacies to the redistricting process, this paper relies on a redistricting event producing changes in electoral incentives within regions of congressional districts, which all types of redistricting produce.

Table 2.1: Redistricting Timeline

January 2011	Congress apportions congressional seats
April 2011	States receive detailed population data from the census
April 19, 2011	Iowa is the first state to approve a redistricting plan
April 24, 2012	New Hampshire is the final state to approve a redistricting plan
November 4, 2012	All members of Congress are up for re-election

2.4 Data

I examine the effects that redistricting events have on the distribution of funds within a congressional district. In order to estimate this relationship 3 sources of data are needed. First the date that a redistricting plan is passed is collected for each state. Second, I use election returns to identify each district candidates seek to represent in both 2010 as well in 2012. Third, I geocode federal grants into sub-congressional district geographies assigned by whether the area is redistricted or not. In this section I provide a detailed description of each data source used. In the next section I explain the geocoding procedure.

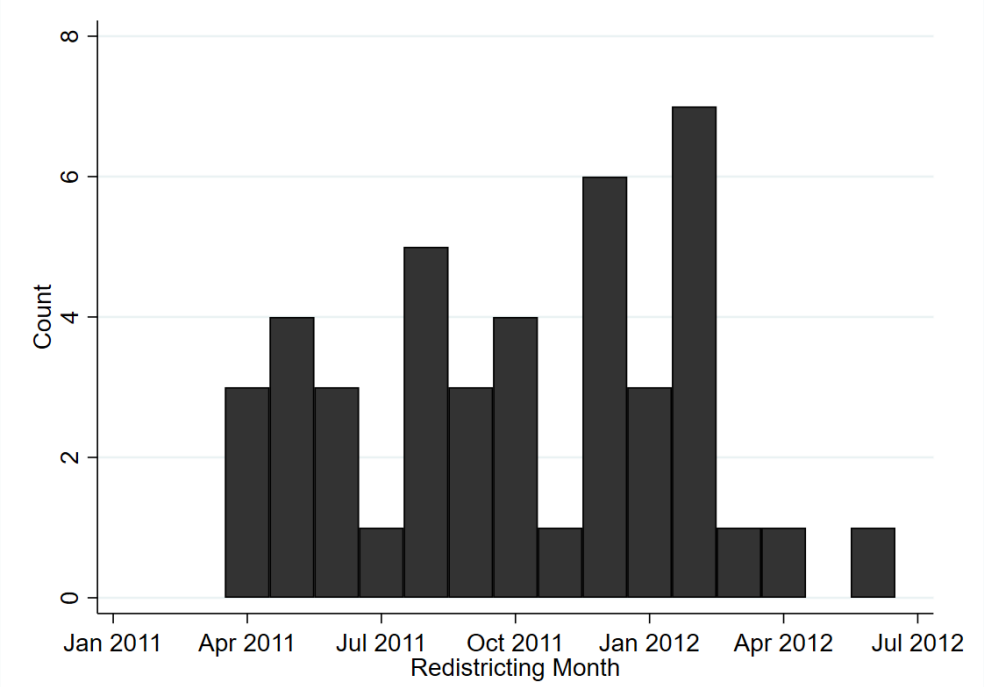
The passage of a redistricting plan are what determine when politicians learn of the change in electoral boundaries. For the 2010 redistricting cycle, I am able to collect the dates of these announcements from a single source, Justin Levitt's All About Redistricting website. The website contains a detailed timeline of each states redistricting process. As an example, I am able to observe that the Pennsylvania legislature passed their congressional plan (SB 1249) on December 20, 2011, which was signed by the governor on December 22, 2011. It's not uncommon for redistricting plans to be challenged in the state court system. On February 7, 2018 the state courts ruled that it contained "impermissible partisan gerrymandering." This resulted in the court drawing a new congressional map on February 19, 2018.

This contains two pieces of important information. First, Pennsylvania's initial congressional

plan went into effect on December 22, 2011. I record the date that a bill is signed into law as the date of redistricting announcement [7]. Pennsylvania also redistricts under a court order in 2018[8]. Importantly, I exclude redistricting events from the analysis where there is a court ordered change after the initial announcement, but during the 2012 election cycle[9].

Figure 2.11, shows the distribution of the timing of the redistricting announcements. These announcements occur between April of 2011 and April of 2012. Most states plans go into effect before the next calendar year.

Figure 2.1: Distribution of Redistricting Months



Notes: Figure displays the distribution of the month when each state passed the redistricting plan that was used in the November 2012 election. Bar heights along the y-axis indicate the number of states which passed their redistricting plan in the corresponding month along the x-axis. Figure includes the passage of 43 redistricting plans, excluding Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont and Wyoming due to there being a single House district.

I next determine which portions of a congressional district are carried-over into the next

[7] There is typically very little delay between the date that legislature passes the plan and when the governor signs off on the plan. My analysis occurs at the monthly level so the decision between these two dates is mostly arbitrary.

[8] A similar event occurs in North Carolina and Virginia.

[9] This excludes Texas and Ohio from the analysis. The issue is that politicians may react to the both sets of boundaries.

election and which are redistricted to a new representative. I collect U.S. election returns from Congressional Quarterly for the 2010 and 2012 House elections. In the 2010 election, I record the name of the winning representative, the state, and district that they represent. I collect the same information for the 2012 election, but additionally include the name of the losing candidate. I then link these two files using legislators' names and the state that they represent. An example would be that Bob Gibbs wins the November 2010 election to represent Ohio's 18th Congressional District. In 2012, Bob Gibbs runs for election in Ohio's 7th Congressional District. Then, the geographic overlap between the 18th and 7th district in 2010 and 2012 respectively, would represent representative Gibb's carried over constituents. It is important to link legislators this way and not through district numbers. District numbers do not need to be consistently ordered between redistricting cycles. For example, reapportionment eliminates a states highest numbered district when a seat is lost.

In section 2.4.1, I explain in detail how I create the sub-congressional units of analysis. This requires having a machine-readable congressional district maps from the 2010 and 2012 elections. Lewis, DeVine, Pritcher and Martis' website provides digital boundary definitions for congressional districts. The site contains shapefiles for each session of Congress which can be read by a variety of mapping software. I collect congressional boundaries for the 2010 (112th Congress) and 2012 (113th Congress). To summarize the geography procedure, I intersect the two boundary files. The resulting units of geography have two components, a district number from the 2010 election and a district number from the 2012 election.

The final source of data, federal grants, are used to generate my main dependent variables. Studies pertaining to the geographic distribution of funds have often used one of two datasets. Either the Federal Assistance Awards Data System (FAADS) or the Consolidated Federal Funds

Reports (CFFR). However, the Federal Funding Accountability and Transparency Act of 2006 (FFATA) required the Office of Management and Budget to create and maintain a "single searchable website, accessible by the public at no cost before January 1, 2008, that contains information on all transactions over $25,000 involving all federal procurement and non-procurement awards within 30 days of the posting of such transactions."

The goal of this site, usaspending.gov, was to eliminate much of difficulties in the former systems, particularly FAADS [10]. The primary difference between FAADS and usaspending are that FAADS reports all federal assistance awards, but on a quarterly basis.

For my study, the primary benefit of the data from usaspending is that it contains the address of the recipient of a federal award. I am able to then use these addresses to allocate federal awards to sub-congressional district geographies. Addresses are observed when there are not multiple recipients for an award. Additionally the data contains the date the award was authorized, the total amount of federal obligations, and also detailed information about the type of federal award.

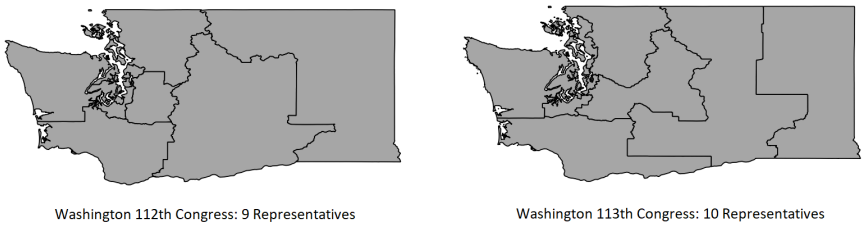
2.4.1 Creating Sub Congressional District Geographies

The process for creating the unit of geography is best explained through the use of an example. In 2010, Washington state had 9 congressional representatives which increased to 10 in 2012. In figure 2.2, I show on the left the congressional districts from 2010 and on the right the congressional districts from 2012.

I use qGIS to intersect these two maps, which I display in figure 2.3. I refer to the resulting geographies as district segments. Each district segment has 2 components, a district assignment

[10] FAADS does not directly produce machine readable data. Instead, a customized piece of software is required to read a sequence of text files. Bickers and Stein (1991, 1995) created an annualized version of these data which collapse the data into congressional districts.

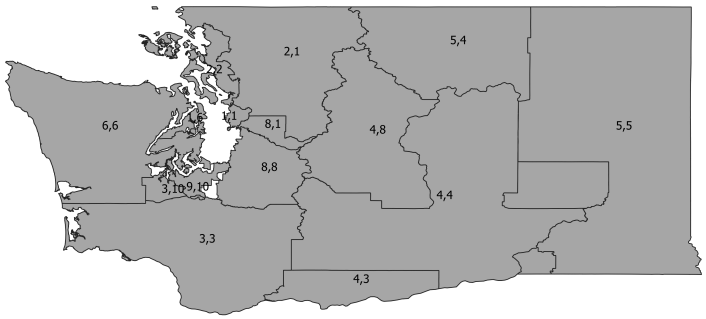
Figure 2.2: Washington State Congressional Boundaries



Notes: Figure contains an example of a change in district boundaries. The map on the left shows the congressional districts used to elect Washington's House of Representative members in the 2010 election. The map on the right shows the congressional districts used to election Washingtonian's House of Representative members in the 2012 election. The redistricting plan was passed on February 7, 2012.

from 2010 and a district assignment from 2012. I am then able to create a dataset where for each unit of geography I can pin-point who the representative was in 2010 and 2012.

Figure 2.3: Washington State District Segments

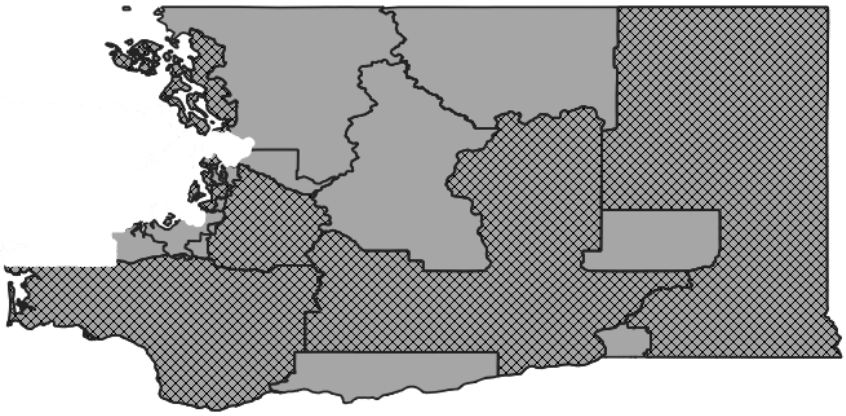


Notes: Figure contains an example of Washington states district segments, the unit of analysis. District segments are constructed by intersecting the congressional districts from the 2010 election and the 2012 election. Each district segment is labeled by its pre-redistricting district number followed by its post-redistricting district number. Geographic units that are seemingly unlabeled are the result of non-contiguous changes in district assignments.

The combined data are shown in figure 2.4. In this figure, hashed segments are where an incumbent runs for re-election and solid color areas are redistricted to a new representative. Even though the representatives in solid colored regions are likely a congressional incumbent, they are not an incumbent to this constituency. The maps are not exactly the same, District 1 and 6 located in northwest Washington are removed. This is due to a congressional retirement and a representative running for the office of governor, hence there is no incumbent politician in these

areas.

Figure 2.4: Carry-over v.s. Out-going Segments



Notes: Outlined shapes denote individual district segments. District segments that are completely gray are district segments which are redistricted to a new representative. District segments that are cross-hashed with black are not redistricted.

2.4.2 Geocoding Awards

To geocode awards, I first convert addresses into latitude and longitude coordinates using the Census Bureau's geocoder. I do this for awards distributed between 2011 and 2016. On average, there are over 200,000 unique addresses in a given year that must be geocoded. Successful results are returned for upwards of 85% of addresses. The majority of unmatched addresses are the result of incomplete data. In table 2.4.2, I show the match quality of this procedure. I am able to exactly geocode 60% of addresses and am able to use incomplete address components to generate non-exact matches for an additional 28-30% of addresses.

With the latitude and longitude coordinates obtained, I combine the cooridnates with the district segments. To increase computational efficiency, I construct a dataset of census blocks within each district and compute the centroid. I geocode addresses to the census block by computing the point distance between the address and census block centroid, allocating the

Table 2.2: Match Quality By Fiscal Year

	(1) Total	(2) Exact	(3) Non-exact	(4) Excluded	(5) Final
2011	326,050	197,800	99,861	11,784	288,357
		(.607)	(.306)	(.036)	(.884)
2012	315,386	189,819	94,774	11,205	275,910
		(.602)	(.301)	(.036)	(.875)
2013	323,555	195,686	91,816	8,927	280,827
		(.605)	(.284)	(.028)	(.868)
2014	325,183	202,681	95,992	9,172	291,130
		(.623)	(.295)	(.028)	(.895)
2015	211,893	129,222	61,734	9,399	183,298
		(.610)	(.291)	(.044)	(.865)
2016	165,056	100,534	46,205	11,686	137,048
		(.609)	(.280)	(.071)	(.830)

Notes: Table displays match rate of federal award addresses by fiscal year. Columns are labeled 1-5. Column 1 contains the total number of unique addresses. Column 2 displays the number of exactly matched addresses. Column 3 displays the number of addresses matched using incomplete addresses. Column 4 shows the total number of addresses excluded due to place of performance and recipient differences. Column 5 contains the number of unique addresses included in the final sample. In columns 2-5 the fraction of total addresses is included in parenthesis.

address to a census block based on which which census block centroid has the minimum point distance to the address. Census blocks are statistical areas bounded by features, they are small in area, for example a single apartment building can be its own census block[11].

The result is that I am able to know how many federal dollars are allocated in abstract geographies. There are some restrictions to this, as I am only able to observe the recipients address. The recipient addresses is not necessarily the place of performance for the award. I restrict my baseline sample to only include awards where the smallest observable unit of geography between the recipient and place of performance are the same (congressional district).

[11]This is a similar procedure to more general spatial joining algorithms, however instead of using an arbitrarily small fishnet of abstract geographies I use census blocks. The primary gain is that I am able to parallelize the algorithm.

2.5 Empirical Design

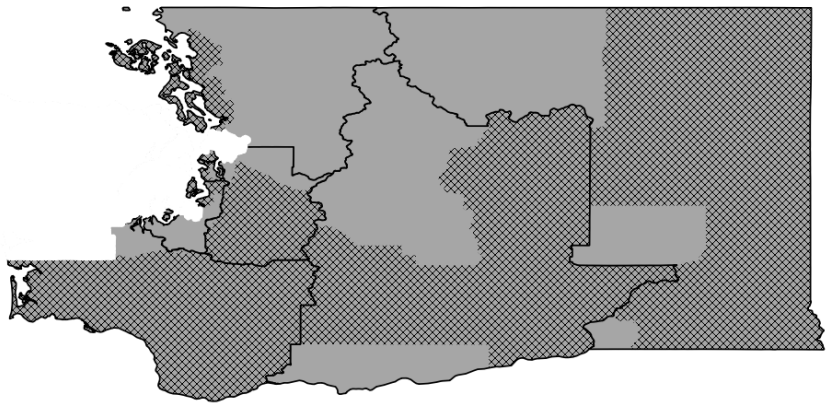
Here I present a detailed overview of the natural experiment as well as empirical methods I use to generate causal estimates of the impact of a change in electoral benefits on federal awards. As a preview, I use two empirical methods. The initial specification is based on a the county-border-regression design from Dube, Lester and Reich (2010), which uses a time-varying local fixed effects to control for time-variant spatial trends in the distribution of federal awards. In the second set of specifications, I limit the areas of interest to various bandwidths along district segment borders, and estimate a geographic discontinuity in electoral benefits.

2.5.1 Experimental Details

The natural experiment exploits a sharp change in electoral benefits after a redistricting announcement is made. I construct a treatment group, which is the group of district segments where the politician wins office in 2010 and runs for office in 2012. I refer to these regions as "carry-over segments," since they carry-over the representative from one period to the next. The comparison areas are district segments where new politicians (to those segments) run for office in 2012. I refer to these segments as "incoming" and "out-going" segments, because these areas are being redistricted to a new representative.

Figure 2.5 builds on the example from section 2.4.1. Areas which are hashed are carry-over segments, while solid colored areas are incoming to a new representative an hence out-going for the incumbent. The borders in this figure represent the congressional districts from 2010.

Figure 2.5: Treatment and Comparison Segments



Notes: Outlined shapes denote the congressional districts prior to redistricting. Areas that are completely gray are areas which are redistricted, the comparison group, and the areas cross-hashed in black are not, the treatment group. The experimental designs identifying variation occurs within the outline congressional districts.

2.5.2 Initial Specifications

The structure of this design fits well in a difference in difference framework, comparing carry-over to outgoing segments. Using a two-way fixed effects model controls for time-invariant effects across segments and national time-trends.

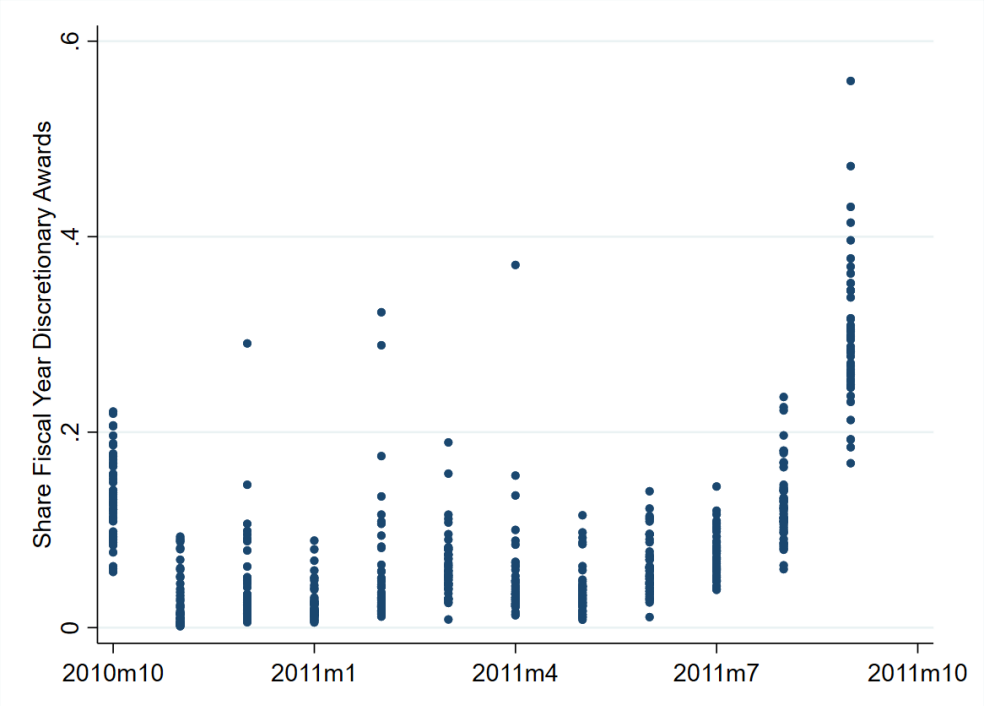
$$y_{st} = \alpha + \beta C_{st} + \lambda_s + \lambda_t + \epsilon_{st} \tag{2.1}$$

Equation 2.1 shows the standard two-way fixed effects specification in this setting. Where y_{st} are per-capita federal awards won by district segment s at date t and C_{st} is the variable of interest, a dummy variable that is 1 for carry-over segments after a redistricting announcement is made. The two sets of fixed effects are, λ_s, a time-invariant district segment fixed effect and λ_t, a spatially-invariant time fixed effect. However, this framework fails to capture spatially varying trends in federal awards. Federal awards won are related to the demographic and industrial

composition of an area, with different endowments using funding in different cycles.

For simplicity, assume that all redistricting events occur simultaneously such that a standard difference-in-difference approach fits. To estimate the treatment effect, differences in the group means could be taken. This approach assumes that any control segment is as good as any other control segment. Figure 2.6 shows that this is unlikely the case. The figure aggregates discretionary funding for the 2011 fiscal year, and plots the share of funding a state received in each month[12]

Figure 2.6: Distribution of Discretionary Awards



Notes: Figure shows the distribution of discretionary awards across states in the 2011 fiscal year. Each point represents the share of discretionary awards a state received of their annual total. The standard deviation by month varies from 0.019 to 0.073 and means fluctuate between 0.025 to 0.30.

Additionally, aggregating controls in this manner produces possibly misleading averages. For example, one concern maybe that Republicans exhibited control of the redistricting process in 21 states in 2010. Republicans may disproportionately assign redistricted areas to urban areas.

[12]For example, one data point Maryland, January is 3.7%, which means that Maryland received 3.7% of their annual discretionary funds in January.

If this is true, the comparison group would be skewed towards urban areas which would not act as a suitable control for their suburban and rural counter parts. Instead, more thoughtful research designs can be employed to account for these trends and improve the quality of controls.

If the data generating process for these trends were known, they could be directly included in the model, for example by including a state-linear time trend. However, given how different funding patterns are across the country a simple trend line is not a sufficient solution. Instead, I provide a more flexible way to account for these trends by restricting the control groups to a more suitable group.

I construct sets of district segments which are completely contained by congressional boundaries, either from 2010 or 2012. This allows for a time-varying fixed effect at the congressional district level. Congressional districts have on average populations of around 700,000. This design accounts for local patterns of funding and restricting controls to only be drawn from nearby segments.

$$y_{s112t} = \alpha + \beta C_{st} + \lambda_s + \lambda_t + \lambda_{112t} + \epsilon_{s112t} \tag{2.2}$$

$$y_{s113t} = \alpha + \beta C_{st} + \lambda_s + \lambda_t + \lambda_{113t} + \epsilon_{s113t} \tag{2.3}$$

Equations 2.2 an 2.3 introduce these fixed effects. Equation 2.2 is indexed by 112, which refers to the congressional district from the 112th congress (elections held in 2010) and equation 2.3 is indexed by 113, congressional districts from the 113th congress (elections held in 2012). Similar to a county-border regression, this creates a sets of treatment and control segments based on a geographic discontinuity. The λ_{112t} and λ_{113t} fixed effects represent time-varying fixed

effects which group district segments according to congressional districts.

These two regressions form two different experiments. Equation 2.2, which I refer to as the outgoing specification, restricts variation to occur within congressional districts from 2010, the 112th congress. In this specification, a politician is elected in 2010 to represent all of the constituents in this district. Once a redistricting announcement is made, there is a sharp change in electoral benefits within this district for the politician. Hence, intuitively I am comparing how parts of a district fared when their legislator will no longer represent them in the future. This method is identical to using a fixed effect on which legislator represents the segment in 2010.

Alternatively equation 2.3, which I refer to as the incoming specification, restricts variation to occur within congressional districts from 2012, the 113th congress. In this specification, the redistricting announcement induces possible electoral benefits in the comparison groups for the politician. In the next section, I outline the identified treatment effect in each specification.

2.5.3 A DID Framework

I now construct a theoretical example which outlines the treatment effects that are identified under both experimental frameworks, outgoing and incoming. In this framework, I simplify the experiment to a standard difference-in-difference between 2 types of segments with a uniform redistricting announcement.

First, consider two types of segments, a carry-over segment which politician A represents before and after redistricting and an outgoing segment which politician A represents before redistricting and politician B will represent after. I claim that the amount of pork that is brought to these segments is a function of the legislators forward efforts, their backward efforts, and the

characteristics of the district, shown in equations 2.4 and 2.5.

The notation introduces 3 different types of district segments, 2 time periods, 3 different effects, and 2 politicians. The different types of segments are outgoing (O), carry-over (C), and incoming (I). Where outgoing segments are segments which are redistricted away from politician A to politician B. Carry-over segments are segments which politician A maintains across elections. And incoming segments are segments which politician A gains as a result of redistricting. The two time periods are pre and $post$, signalling pre-redistricting and post-redistricting timing. The different effects are forward-looking incentives labeled For, backward-looking incentives labeled $Back$, and area characteristics labeled $Endowment$. The outcome of interest is per-capita federal awards, Y.

$$
\begin{aligned}
Y(O) &= Y(O)_{pre} + Y(O)_{post} \\
Y(O)_{pre} &= For^{A}_{pre}(O) + Back^{A}(O) + Endowment \\
Y(O)_{post} &= For^{B}_{pre}(O) + Back^{A}(O) + Endowment \\
Y(O) &= For^{A}_{pre}(O) + For^{B}_{post}(O) + Back^{A}(O) + Endowment
\end{aligned}
\tag{2.4}
$$

$$
\begin{aligned}
Y(C) &= Y(C)_{pre} + Y(C)_{post} \\
Y(C)_{pre} &= For^{A}_{pre}(C) + Back^{A}(C) + Endowment \\
Y(C)_{post} &= For^{A}_{post}(C) + Back^{A}(C) + Endowment \\
Y(C) &= For^{A}_{pre}(C) + For^{A}_{post}(C) + Back^{A}(C) + Endowment
\end{aligned}
\tag{2.5}
$$

Forward effects are consistent with models of competitive elections and represent politicians using their position to enhance their prospects of winning re-election. These effects may take many forms which I do not identify, such as credit-claiming, quid-pro-quo, and type signaling of their ability or policy preferences. In this set of experiments, the backward effect does not change politicians as a result of the redistricting process, an intentional feature of the design. Backward effort can be thought of as making due on past promises or intrinsic reciprocity. Finally, endowments represents the characteristics like the underlying demographics and labor market characteristics in a segment, which impact the types and amounts of grants received.

$$
\begin{aligned}
Y(C) - Y(O) = [For^{A}_{pre}(C) + For^{A}_{post}(C) + Back^{A}(C) + Endowment] - \\
[For^{A}_{pre}(O) + For^{B}_{post}(O) + Back^{A}(O) + Endowment] \\
Y(C) - Y(O) = [For^{A}_{post}(C) - For^{B}_{post}(O)]
\end{aligned}
\tag{2.6}
$$

Equation 2.6 shows what the estimated effect from the difference-in-difference model. To arrive at the resulting equation I assume that $Back^{A}(C) = Back^{A}(O)$, that is the backward efforts politician A exerts between what will become the outgoing and carry-over segments are the same. Second, I assume that $For^{A}_{pre}(C) = For^{A}_{pre}(O)$ which is analogous to the prior assumption, but for forward effort. These assumptions can be summarized as the parallel trends, where politicians don't differentially exert effort to the carry-over segments they represent prior to the redistricting announcement. The endowments of the two segments may not be perfectly equivalent, however to the extent that they are not time-varying within a 22 month window, the models capture their presence.

The remaining term, $For^{A}_{post}(C) - For^{B}_{post}(O)$ represents the difference in forward effort between politician A and B. Previously, I discussed reasons for why the second term is likely smaller than first. Credit-claiming politicians who want to convert pork into votes will avoid focusing efforts where the benefit could be allocated to a different politician. If voters aren't acutely aware of redistricting changes, politician B takes the risk that the benefit will be attributed to politician A. Finally, politicians face possible reputation risks if they reveal that they are electorally motivated. Despite being smaller it may not necessarily be 0 and for this reason I interpret the estimated effects as a lower bound.

The second experiment, the incoming specification, uses a similar framework, but instead of outgoing segments, the comparison group are incoming segments. Incoming segments are segments that a politician does not currently represent, but wishes to represent in the future. Equation 2.7 outlines the incentive structure in these districts.

$$\begin{aligned} Y(I) &= Y(I)_{pre} + Y(I)_{post} \\ Y(I)_{pre} &= For^{B}_{pre}(I) + Back^{B}(I) + Endowment \\ Y(I)_{post} &= For^{A}_{pre}(I) + Back^{B}(I) + Endowment \\ Y(I) &= For^{B}_{pre}(I) + For^{A}_{post}(I) + Back^{B}(I) + Endowment \end{aligned} \tag{2.7}$$

The difference in these segments is that politician B represents the comparison region prior to redistricting, changing the politician exerting backward effort. Equation 2.8 shows the estimated effect of a difference-in-difference using the incoming comparison group. This assumptions to arrive at this effect are slightly different. I assume that $For^{A}_{pre}(C) = For^{B}_{pre}(I)$,

or that prior to redistricting politicians A and B exert the same amount of forward effort. I also assume that $Back^{A}_{pre}(C) = Back^{B}_{pre}(I)$, which is additionally captured by the time-invariant fixed effects. The resulting effect is again an estimated lower bound of a change in electoral benefits.

$$\begin{aligned} Y(C) - Y(I) &= [For^{A}_{pre}(C) + For^{A}_{post}(C) + Back^{A}(C) + Endowment] - \\ &\quad [For^{B}_{pre}(I) + For^{A}_{post}(I) + Back^{B}(I) + Endowment] \\ Y(C) - Y(I) &= [For^{A}_{post}(C) - For^{A}_{post}(I)] \end{aligned} \tag{2.8}$$

The two experiments produce differing lower bounds of the desired estimated effect, $For^{A}_{post}(C)$. The outgoing experiment includes a term that captures compares politician A and B's forward effort. Meanwhile, the incoming experiment includes how politician A responds in both their carried over and redistricted segments. In either case, a politicians desire to credit-claim will weaken the benefits of targeting distributions to the redistricted area. In this sense, I will be estimating a lower bound on the forward-looking incentive effects of elections on the distribution of funds.

2.5.4 Balance and Geographic Restrictions

A primary objective of this research design is generating a suitable comparison group for the carry-over regions. In this section I discuss how similar the groups are and introduce geographic restrictions which improves the balance of the samples.

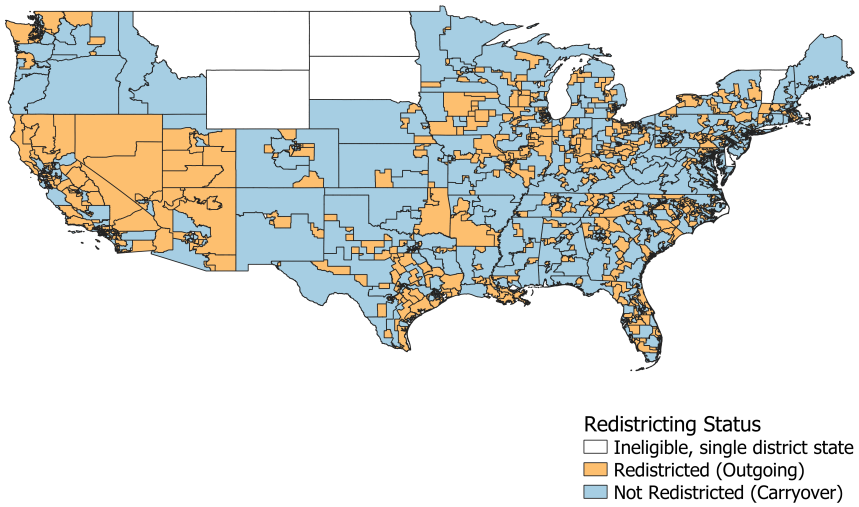
The redistricted segments are in general smaller than their carry-over counterparts. In 2010, around 33% of the population residing in states that redistricted were effectively reallocated

outside of their old district. By construction, this creates more populous carry-over segments. The difference-in-difference design accounts for differing group averages to the extent that they are time-invariant, however if time-varying characteristics are correlated with treatmentment status this can bias the estimated treatment effect. The ideal solution is to test explicitly for this using time-varying covariates as outcomes in the difference-in-difference design. However, monthly data at a sub-congressional district level is not available. Instead, I first show sample averages of the populations of interest and then I jointly test their predictive power for carry-over status.

The redistricting experiments creates two classes of segments, carry-over and out-going. In order to further improve observable balance, I impose geographic restrictions that limit the size of areas of interest. This restriction not only adjusts for an asymmetry in the size of segments, but will also better match treatment and control segments on unobservable characteristics. To do this, I utilize qGIS to create a set of points within each district segment based on the centroid of every census block in the segment. Then, for each individual experiment I calculate the minimum distance from every carry-over and out-going segment. I then create samples based on the minimum distance between treatment and control segements. For example, a 10 mile restriction creates geographies which include all census blocks which are within 10 miles of at least 1 other census block of the opposite treatment status. Figures 2.7 and 2.8 display the geographic distribution of redistricting assignment for the entire country and when imposing a 10 mile restriction.

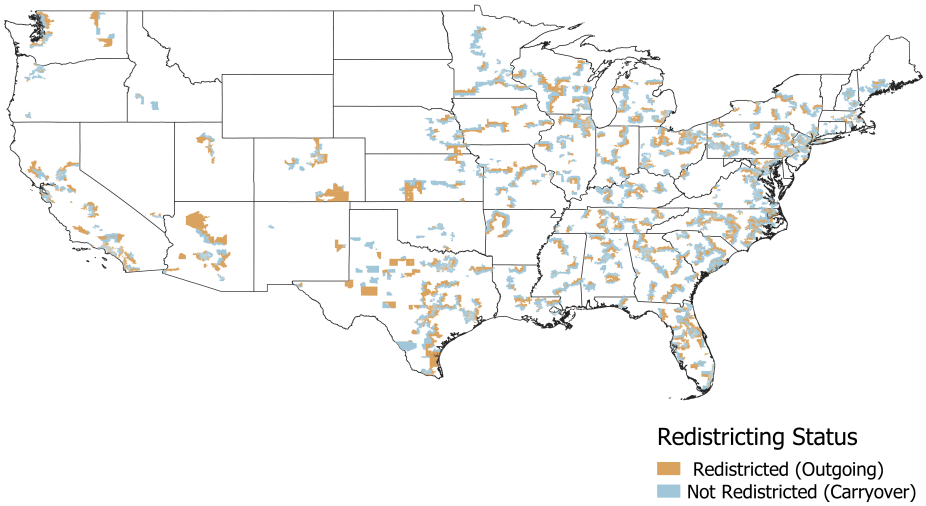
This restriction exploits that there is a geographic discontinuity in electoral benefits that occurs according to a boundary. Typically in a regression discontinuity the decision to use a narrow or wide bandwidth has a trade-off, statistical power for sample heterogeneity. However, these restrictions do not impact the statistical power directly. The restriction preserves redistricting

Figure 2.7: District Segments by Redistricting Status: Full Country



Notes: Map shows areas that are redistricted in gold and areas that not redistricted in blue. States with a single representative are white.

Figure 2.8: District Segments by Redistricting Status: 10 Mile Restriction



Notes: Areas included in the map are based on a 10 mile geographic restriction. Areas included are no more than 10 miles away from the nearest border. Areas in gold are redistricted and areas in blue are not redistricted. White areas are not included in the geographic restriction analysis.

experiments, but instead limits the geographic area within each experiment. The trade-off instead is that using narrower bandwidths reduces the amount awards that are observed In principle, n

should not change, however narrower bandwidths may result in redistricting experiments with no federal awards, excluding them from analysis.. Table 2.5.4 summarizes the distribution of federal awards by distance bandwidths.

Table 2.3: Distribution of Awards by Geographic Restrictions

	(1) Share	(2) Cumulative
100 Miles+	.02	1.00
75-100 Miles	.01	.98
50-75 Miles	.04	.97
40-50 Miles	.02	.93
30-40 Miles	.02	.91
20-30 Miles	.06	.89
10-20 Miles	.11	.83
9-10 Miles	.02	.72
8-9 Miles	.02	.70
7-8 Miles	.02	.68
6-7 Miles	.03	.66
5-6 Miles	.04	.63
4-5 Miles	.06	.59
3-4 Miles	.14	.53
2-3 Miles	.10	.39
1-2 Miles	.10	.29
0-1 Miles	.19	.19

Notes: Table displays the distribution of the amount of discretionary federal awards from January 2011 to November 2012 within various geographic bandwidths. For example, 100+ miles corresponds to federal awards from district segments that are more than 100 miles away from the border. Column (1) displays the share within each bandwidth. Column (2) shows the cumulative amount. This column shows the share of federal dollars that would be used if a bandwidth of 0 to the corresponding value were to be used. For example, the cumulative value of .72 at 9-10 miles denotes that 72% of federal awards are distributed within 0 and 10 miles of the congressional district border.

Tables 2.4 and 2.5 show the sample characteristics when no restriction is imposed and then when a 10 mile restriction is employed. In practice, the 10 mile restriction utilizes a 10-mile distance threshold around the borders of carry-over regions. I show summary statistics for 13 variables which which broadly contain information about an area's demographics, socioeconomic background, and labor force. I obtain these variables from the American Community Survey's

5-year estimates from 2010-2014 and from the LEHD Origin-Destination Employment Statistics from 2011 [13]. I find similar summary statistics across both samples, with the difference between the carry-over and outgoing means never being significant at the 10% level.

In table 2.6, I show a joint test that better encompasses the experimental design. In this table, I include a fixed effect for each congressional district. This fixed effect restricts variation to only occur within congressional districts. In column 1, I show results using no geographic bandwidth. Columns 2-4 introduce geographic restrictions, ranging from 100 miles to 10 miles. I find no individual coefficients that are significant at the 10%, however the joint F-statistic in column 1 indicates there is joint predictive power. When geographic restrictions are put in place, the F-stats decrease.

The experimental framework controls for these observables, which over a 22 month window are likely stable [14]. Despite this, I elect to show results for using data from both the full sample, as well as with a 10 mile restriction.

In figure 2.9, I provide further justification for the inclusion of a geographic restriction. This figure shows the difference in 2-way vote shares in the 2008 presidential election between carry-over and outgoing segments and includes a fixed effect for the experimental pairing. The figure shows individual state estimates in gray and pooled estimates in various colors. I include pooled estimates by whether a political party has legal control of the redistricting process according to Coriale, Kaplan, and Kolliner (2021). I also include an estimate using all states where no political

[13]The full list of variables is here: Population density, female density, black density, under 18 density, over 70 density, bachelor degree or more holding density, high school or less density, poverty density, renter density, unemployment rate, labor force participation rate, work place area job density, and resident area job density. These figures restrict the sample to exclude arbitrarily small district segments, those that are less than 1 square mile. This excludes 40 redistricted segments.

[14]One concern may be during economic downturns the labor force variables are particularly volatile, however the time period of interest contained no such down turn.

Table 2.4: Sample Characteristics: All Data

	All		Treatment		Control		p-value
	Mean	SD	Mean	SD	Mean	SD	Treat = Cont.
Pop. Dens.	3878.81	(9089.00)	3592.95	(9425.93)	4000.19	(8946.01)	0.52
Fem. Dens.	1992.95	(4770.72)	1859.22	(4952.76)	2049.73	(4693.64)	0.57
Blk. Dens.	637.44	(2303.76)	777.90	(3111.14)	577.80	(1857.36)	0.29
Under 18 Dens.	839.05	(1871.97)	789.61	(2027.71)	860.04	(1802.85)	0.60
Over 70 Dens.	320.53	(777.60)	295.90	(768.43)	330.98	(781.76)	0.51
BA+ Dens.	931.68	(3334.81)	863.58	(3071.39)	960.60	(3442.21)	0.66
HS Dens.	531.99	(1159.46)	483.61	(1164.67)	552.53	(1157.44)	0.38
Poverty Dens.	621.18	(1919.33)	629.13	(2251.81)	617.80	(1761.00)	0.94
Renter Dens.	814.77	(2813.83)	807.32	(2973.24)	817.94	(2745.47)	0.96
Unemp. Rate	0.10	(0.04)	0.10	(0.03)	0.10	(0.04)	0.93
LF. Partic	0.64	(0.07)	0.64	(0.05)	0.64	(0.08)	0.92
WAC Dens.	1936.12	(12438.19)	1839.75	(8878.25)	1977.05	(13677.69)	0.85
RAC Dens.	1561.20	(3676.65)	1447.06	(3732.33)	1609.66	(3654.27)	0.52
N	1030		307		723		

Table 2.5: Sample Characteristics: 10 Mile Data

	All		Treatment		Control		p-value
	Mean	SD	Mean	SD	Mean	SD	Treat = Cont.
Pop. Dens.	4312.17	(9516.56)	3894.72	(9496.10)	4512.84	(9527.50)	0.36
Fem. Dens.	2214.60	(4997.28)	2014.37	(4990.55)	2310.86	(5001.67)	0.40
Blk. Dens.	711.25	(2431.11)	838.60	(3160.72)	650.03	(1987.58)	0.34
Under 18 Dens.	929.73	(1957.02)	860.38	(2042.58)	963.07	(1915.33)	0.47
Over 70 Dens.	355.30	(812.79)	321.24	(774.50)	371.68	(830.68)	0.37
BA+ Dens.	1040.02	(3511.53)	927.63	(3102.24)	1094.05	(3693.34)	0.47
HS Dens.	592.44	(1212.97)	527.56	(1173.15)	623.63	(1231.36)	0.25
Poverty Dens.	686.99	(2016.40)	669.35	(2277.18)	695.47	(1880.13)	0.86
Renter Dens.	907.59	(2961.28)	856.34	(3005.66)	932.23	(2941.83)	0.72
Unemp. Rate	0.09	(0.04)	0.10	(0.04)	0.09	(0.05)	0.87
LF. Partic	0.64	(0.08)	0.64	(0.06)	0.64	(0.09)	0.98
WAC Dens.	2160.83	(13136.01)	1959.47	(8982.83)	2257.63	(14727.07)	0.70
RAC Dens.	1741.29	(3851.09)	1567.03	(3759.06)	1825.05	(3894.77)	0.34
N	921		299		622		

Notes: Table shows summary statistics for demographic and labor market characteristics between the treated and control samples. In the far right column displays the p-value of the treatment and control means being equal.

party exhibits political control and an estimate that pools all states together. The separation by redistricting legal authority is meant to prevent the balancing of means producing a null result.

Table 2.6: Joint Significance

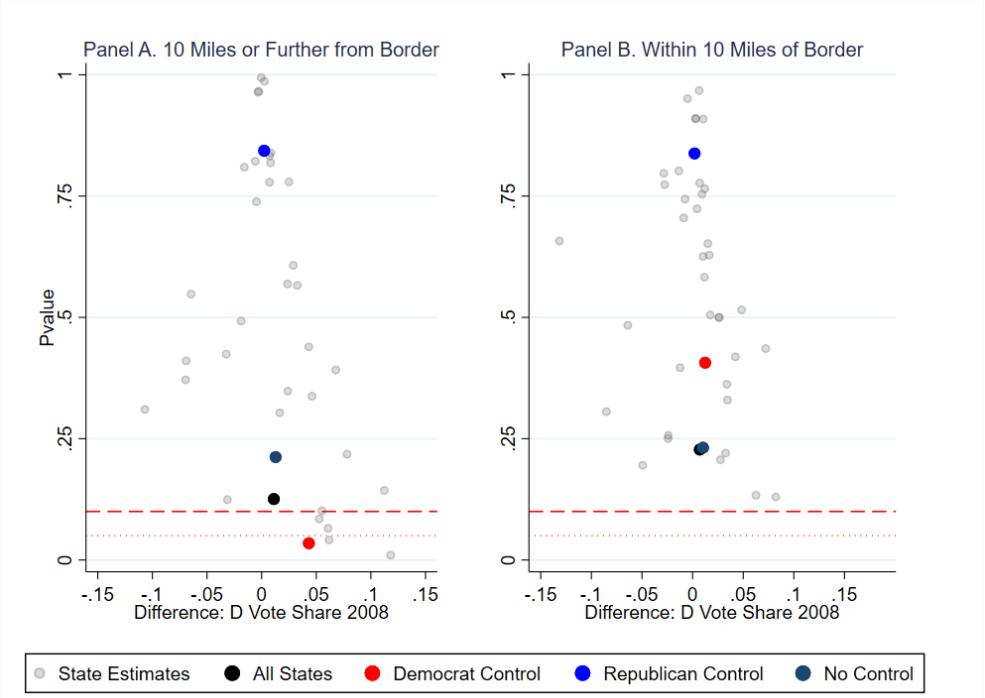
	(1)	(2)	(3)	(4)
Pop. Dens.	-0.552	-0.027	0.007	0.162
	(0.509)	(1.775)	(1.806)	(1.862)
Fem. Dens.	-0.837	-1.041	-0.938	-1.488
	(1.053)	(2.697)	(2.744)	(2.838)
Blk. Dens.	0.275	0.374	0.406	0.560
	(0.170)	(0.359)	(0.368)	(0.377)
Under 18 Dens.	1.569	1.097	0.779	1.245
	(1.150)	(2.018)	(1.927)	(2.113)
Over 70 Dens.	0.810	0.976	0.479	1.049
	(1.429)	(2.359)	(2.364)	(2.476)
BA+ Dens.	0.590	0.349	0.253	0.436
	(0.672)	(1.038)	(1.034)	(1.095)
HS Dens.	0.177	0.008	0.025	0.351
	(1.253)	(2.198)	(2.188)	(2.316)
Poverty Dens.	0.179	-0.560	-0.516	-0.555
	(0.681)	(1.106)	(1.113)	(1.159)
Renter Dens.	0.652	0.530	0.479	0.268
	(0.569)	(0.934)	(0.943)	(0.977)
Unemp. Rate	0.646	0.778	0.423	-0.264
	(0.780)	(0.882)	(0.908)	(0.898)
LF. Partic	0.458	0.623	0.399	0.686
	(0.366)	(0.424)	(0.442)	(0.426)
WAC Dens.	-0.005	0.070	0.070	0.070
	(0.014)	(0.053)	(0.054)	(0.055)
RAC Dens.	0.051	-0.447	-0.316	-0.474
	(0.557)	(1.185)	(1.203)	(1.197)
Bandwidth	None	100 Miles	30 Miles	10 Miles
F Stat	1.42	1.12	0.98	0.98

Table displays the results of a joint significance test with treatment status as the outcome of interest. Column 1 shows results with no geographic restriction, column 2 includes a 100 mile geographic restriction, column 3 includes a 30 mile geographic restriction, and column 4 includes a 10 mile geographic restriction. Geographic restrictions eliminate portions of a district segment that are further than the restriction from the border. F-stats displayed. In all joint tests, a congressional district fixed is included and standard errors are clustered at the congressional district level.

If Republicans redistrict in favor of Republicans and Democrats redistrict in favor of Democrats then the net effect may very well be 0 by construction, despite there being substantial strategic behavior in the redistricting process. In panel A, I limit the sample to only parts of district

segments that are 10 miles or further from the border and in panel B, I limit the sample to parts of district segments within 10 miles of the border. I find that in states where the Democratic party has legal control of the redistricting process the carry-over segments have a larger proportion of the democratic vote share in areas far from the border. However, when imposing the geographic restriction there is little difference in vote shares.

Figure 2.9: Vote Shares Near and Far From Border



Notes: Figure plots regression coefficients for the two-way Democratic vote share in the 2008 presidential election along the x-axis and the associated p-value along the y-axis. Horizontal red lines correspond to a p-values of 0.10 and 0.05. Each point plotted in gray corresponds to a regression for a single state. Points in black pool all states together. Points in red pool all states where the Democratic party had legal control of the redistricting process. Points in blue pool all states where the Republican party had legal control of the redistricting process. Points in navy pool all states where no political party had legal control of the redistricting process. Legal control is defined as legal control during the 2012 redistricting cycle. Units of analysis are district segments. All regressions include a congressional district fixed effects. Standard errors are clustered by congressional district. Panel A (left) only includes district segments that are 10 or more miles apart. Panel B (right) only includes district segments that are within 10 miles of the border.

2.6 Main Results

I now present my main results for the outgoing and incoming experiment. Both experimental designs use district segment variation within congressional districts. The specifications capture

spatially varying trends in the distribution of funds to the extent that they do not vary within a congressional district. One important feature of the outgoing design is that it holds the legislator representing a district segment fixed as of 2010, which excludes the possibility of varying legislator ability biasing the result[15]. As a preview of my results, I show that when a redistricting event occurs and a district segment is carried-over by a politician, they receive between 22% and 29% more funding than average.

I show results using two samples. The first sample does not include any geographic restrictions around congressional district borders and the second utilizes 10 mile restriction around congressional district borders. For district segments to be eligible for inclusion in the experiment there are three criteria. The first criterion is that a state has enough representatives to induce a redistricting event; this excludes 7 states with a single representative from the analysis[16]. Second, in the congressional district grouping, there must be a carry-over representative. This excludes districts from the experiment where the politician retires or runs for another office[17]. Finally, inclusion in the study requires that a redistricting event went into effect, and was not stricken by the courts before the election. This leads to the exclusion of Ohio and Texas, both of which have successful court challenges of their state government approved redistricting maps prior to the 2012 election.

I show results for all project awards and non-government discretionary awards. I trim the sample based on two conditions. First, I remove the smallest 1% of geographies by population. These geographies have less than 100 people and are heavily influenced by a single award.

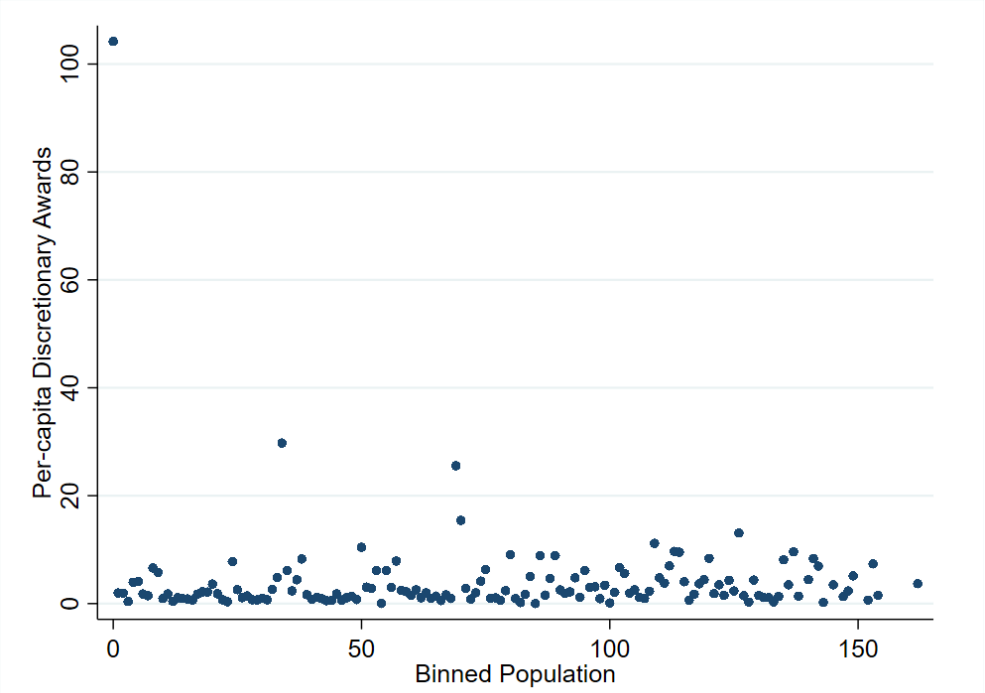
[15] In longer panels, the use of a time-invariant politician fixed effect may be inappropriate if they gain ability over time, however within a 22 month window, a time-invariant control for politician ability is more reasonable

[16] These states are Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont and Wyoming.

[17] For example Jay Inslee was the congressional representative for Washington's 1st congressional district. In June 2011, he announced he would run for governor and not for the House. This results in no carry-over politician for Washington's 1st congressional district, and is hence excluded from the study

Second, I remove the smallest 1% of geographies by land-mass. This removes areas that are smaller than 0.25 square miles. In figure 2.10 I show that per-capita discretionary awards are substantially higher in these low population district segments. Alternatively, these concerns are alleviated with various population based weighting schemes.

Figure 2.10: Distribution of Per-capita Discretionary Awards



Notes: Figure shows per-capita discretionary awards by population from January to April of 2011. The y-axis plots average per-capita discretionary awards for all district segments within a population bin. Each population bin represents district segments within a population. The first bin contains district segments with populations from 1 to 5,000. The second bin contains district segments with populations from 5,001 to 10,000.

The final sample includes district segments from 369 of the 435 congressional districts from 40 states. I use federal awards in a 22 month window around the 112th congress, which goes from January 2011 until October of 2012. Politicians terms are effective until the beginning of 2013, however I end the analysis prior to the November 2012 election.

Table 2.7 contains the main results. Panel 1 contains no geographic restriction, and panel 2 includes a 10 mile restriction. I measure the outcome relative to the dependent variable mean.

Measurement relative to the mean assists with across-sample and outcome comparisons.

Table 2.7: Results: Incoming Congressional District Fixed Effects

	New Funding		Continuation Funding	
	(1)	(2)	(3)	(4)
	No geographic restriction			
Carryover x Announcement	0.247***	0.289**	0.131	0.184
	(0.084)	(0.129)	(0.165)	(0.115)
Dependent Variable Mean	3.94	3.09	1.47	1.39
Number of observations	28,292	28,292	28,292	28,292
	10 mile geographic restriction			
Carryover x Announcement	0.228**	0.247*	0.139	0.173
	(0.106)	(0.139)	(0.215)	(0.126)
Dependent Variable Mean	3.30	2.77	1.41	1.47
All Projects	X		X	
Non-government		X		x
Number of observations	20,563	20,563	20,563	20,563

Notes: Table displays the main results using a set of time-varying fixed effects based on congressional districts prior to redistricting. Columns 1-2 show results for new grants and 3-4 show results using continuation grant fund for various categories. Columns 1 and 3 use project grants, columns 2 and 4 use non-governmental discretionary grants. The first panel shows results using per-capita amounts with no geographic restriction. The second panel shows results using per-capita amounts with a 10-mile geographic restriction. Regressions are trimmed to exclude the smallest 1% of district segments by population. Standard errors show in parenthesis and are clustered at the congressional district level. Dependent variable is measured in means. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In columns 1-2, I consider new grants and in columns 3-4, I show continuation grants. The latter are meant to act as a pre-treatment test. Continuation funding comes from awards when funding decisions were made prior to the redistricting announcement. New grants on the other hand represent no modifications or flows from previous versions of the grant. Meanwhile, continuation grants are precisely these modifications and flows from previous grants. I show results for project grants, and all discretionary grants awarded to non-government entities. Grants distributed with discretion include block grants, project grants, and cooperative agreements.

I find that there is a statistically significant effect of electoral benefits on new project grants. Depending on the geographic restriction used, I find that carry-over district segments receive

between 23% and 25% more new per-capita project grants after the announcement of redistricting relative to average funding, shown in column 1. This amount represents between $.75 and $.97 of the per-capita dollars. Some examples of these project grants are the Rural Energy for America Program, research grants to pharmaceutical companies and grants for improvements to infrastructure. The estimated effect can be interpreted as the difference of forward effort a politician exerts in their carried over district segment compared to another politicians forward politician in the outgoing segments.

The recipients of federal awards are often government agencies, who make decisions as to where these funds eventually are spent. This motivates the use of alternative outcomes which excludes funds going to any government entity. In column 2 and 4, I exclude government grants from the outcome of interest. I find that the general result is robust to the exclusion of these institutions, with the estimated effect again ranging between 25% and 29% of average monthly funding. In general, these funds are estimated much more precisely than when government entities are included. I attribute this difference in precision to measurement error induced by government agencies distributing funds at their discretion in some cases.

In order to alleviate the concern that government agencies maybe distributing funds elsewhere, I impose a restriction that requires the location of an award recipient and where the award is used be within the same congressional district. Congressional districts are the smallest unit of geography for which I consistently observe the performance location of a given grant.

In table 2.8 I show results of electoral incentives on per-capita awards using the incoming congressional district fixed effects. The estimated effect is the difference of the forward effort a politician exerts in their own carried over district segment and the district segments they will adopt in the future. The results are nearly identical to the outgoing fixed effect model. The

similarities in the effect suggest that their little difference in the nuisance term whether it comes from the carry-over or incoming politician.

Table 2.8: Results: Outgoing Congressional District Fixed Effects

	New Funding		Continuation Funding	
	(1)	(2)	(3)	(4)
	Per-capita outcomes. No geographic restriction.			
Carryover x Announcement	0.230***	0.240*	0.114	0.157
	(0.086)	(0.145)	(0.128)	(0.105)
Dependent Variable Mean	3.97	3.12	1.48	1.39
	Per-capita outcomes. No geographic restriction.			
Carryover x Announcement	0.260**	0.249	0.123	0.171
	(0.119)	(0.163)	(0.192)	(0.129)
Dependent Variable Mean	3.32	2.80	1.42	1.48
All Projects	X		X	
Non-government		X		X

Notes: Table displays the main results using a set of time-varying fixed effects based on congressional districts after redistricting. Columns 1-2 show results for new grants and 3-4 show results using continuation grant fund for various categories. Columns 1 and 3 use project grants, columns 2 and 4 use non-governmental discretionary grants. The first panel shows results using per-capita amounts with no geographic restriction. The second panel shows results using per-capita amounts with a 10-mile geographic restriction. Regressions are trimmed to exclude the smallest 1% of district segments by population. Standard errors show in parenthesis and are clustered at the congressional district level. Dependent variable is measured in means. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2.6.1 Identifying Assumptions

The main identifying assumption required for causal interpretation of the estimated effect is that there are parallel trends in funding effort prior to a redistricting announcement. Additionally I also show in this section that the estimated effect is a well estimated lower-bound measure.

To test for parallel trends, I utilize an event study framework. Equation 2.9 shows the estimated equation. I regress per-capita awards on a series of leads and lags of the carry-over status of a district segment. The estimation excludes the period just before a redistricting announcement is made, which forms the baseline group. I estimate this relationship for project

grants.

$$y_{s112t} = \alpha + \lambda_s + \lambda_{112t} + \sum_{j=2}^{J} \beta_j (Lag_j)_{st} + \sum_{k=0}^{K} \beta_j (Lead_k)_{st} + \epsilon_{s112t} \tag{2.9}$$

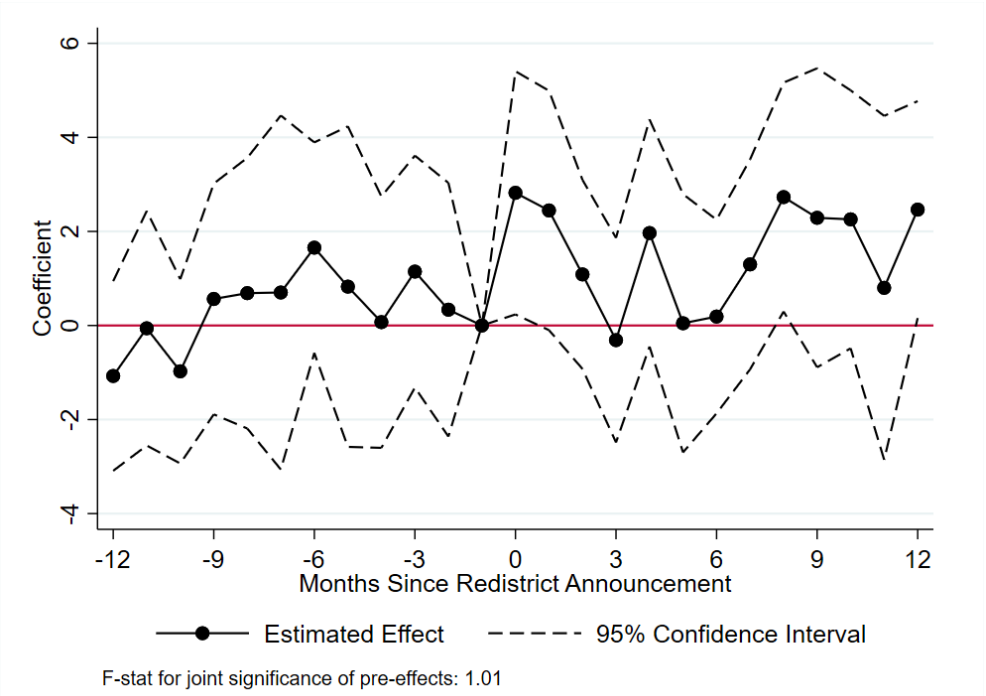
I estimate coefficients at the monthly level. I create 12 pre-period coefficients and 12 post-period coefficients. I include periods that are more than 12 months before or after the redistricting announcement in the right and left most coefficients. The tails of these periods are estimated using a small number of observations. For observations to contribute to identification of the tails, the state must have either redistricted early or late. This results in a thin identifying sample at the tails.

I show the results in figure 2.11. Along the x-axis, I plot the month relative to a redistricting announcement and along the y-axis, I plot the estimated effect. Coefficients are plotted using a solid black circle and the 95% confidence interval is plotted using a dashed line. I show these for both per-capita project and discretionary awards.

The event study approach shows two important findings. First, it shows that there is little concern that there is a violation of the parallel trends assumption. Second, it shows when the treatment effect occurs. There are two waves of treatment, one which occurs immediately after the announcement is made and another towards the end of the election. Not shown, I perform similar tests but exclude states with the earliest redistricting announcements. This allows me to estimate the tails with different groupings of states, and I find that the effect is persistent regardless of which states estimate the effect from the quarter before the election.

One explanation for the immediate increase in funding following an announcement is that I observe the date that funding is approved, not received. There is typically some delay

Figure 2.11: Event Study



Notes: Figure shows results from an event study analysis. The dependent variable is per-capita federal project awards. The event study follows the main empirical design, but includes lags and leads of the first treatment. Each point corresponds to a coefficient from the event study analysis. Along the y-axis the estimated coefficient is plotted, with the 95% confidence interval plotted by a dashed line. The period of comparison is the month immediately prior to the passage of a redistricting plan. Standard errors are clustered at the congressional district level.

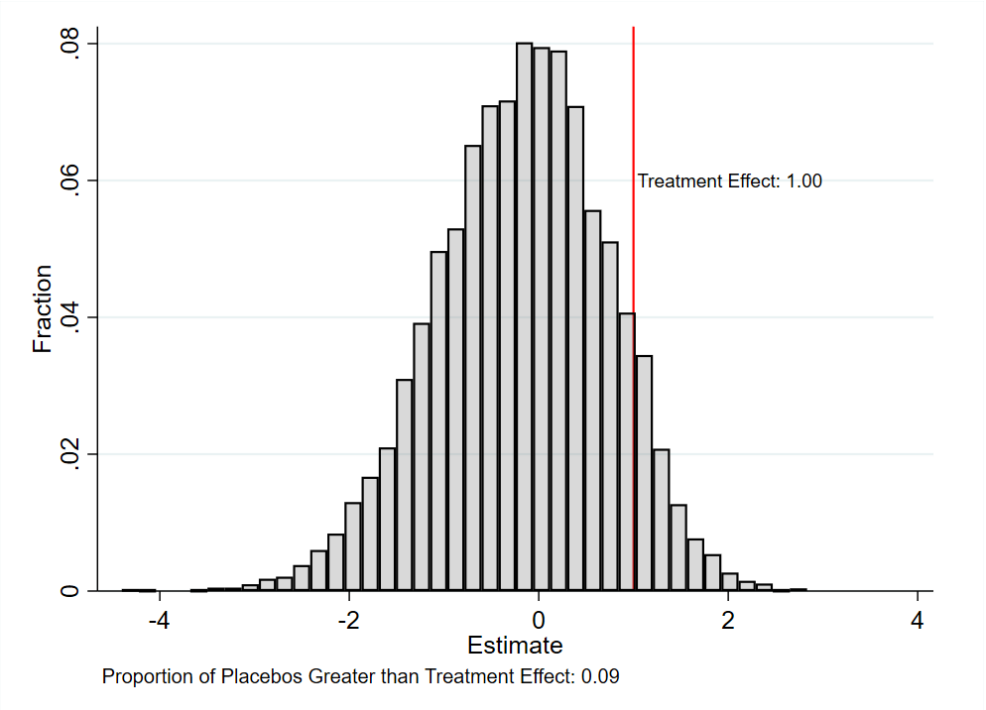
between approval and receiving date. The immediate response could be explained by politicians responding early to the redistricting announcement to possibly claim credit for funding during their campaign.

I also test for anticipation of the treatment effect. To test for the anticipated treatment effects, I restrict my sample to only data prior to when a redistricting announcement is made. I then randomly assign redistricting announcements to states starting in April 2011 and until the month before a states actual announcement. I use these placebo announcements and create new designation of the treatment status of a district segment and perform the same analysis as I do for my main results, repeating this 10,000 times.

Figure 2.12 displays the distribution of the coefficients for this test. I find little evidence of

anticipatory effects, with at least 91% of the experiments having estimated effects lower than the treatment effect.

Figure 2.12: Preperiod Placebos



Notes: Figure plots the distribution of coefficients from 10,000 placebo regressions. Each placebo regression includes periods from January 2011 until the redistricting plan is passed. Each regression uses a randomly generated redistricting passage date from this period and maintains the redistricting assignment for district segments. The mean and standard deviation of the coefficients are shown in the top right, along with actual estimated treatment effect.

The other identifying assumption is that the results are not due to the effort by the incoming politician. In section 2.5.3, I show that the estimated effect is the difference the forward effort of the carry-over politician and the incoming politician. A positive estimated effect can mean that either the carry-over effect is positive and the incoming effect is less than this or the carry-over effect is 0 and the incoming effect is negative. Justifying the latter theoretically is difficult, as it require the politician to work against their future constituents.

To estimate whether this is a lower bound or if the incoming politician does not exert effort in these area, I construct a new sample. In the experimental framework, there are cases

where there is no incoming politician. For instance, when a representative retires and changes the office they run for, some parts of will have no incoming politician. Additionally, when a seat is awarded through apportionment, at least some parts of the state cannot have an incoming incumbent politician.

Using the same framework as before, I use this sample and only includes experiments where the comparison group has no incoming politician in office. Put another way, this sample includes only experiments where the candidates running for office in the comparison district segments were not in office. The sample is small compared to main sample. There are 780 comparison district segments, of which 134 do not have an incoming politician.

In table 2.9, I show results using this subsample. I find mixed results on how the incoming politician may influence the estimated effects. When I use no geographic restriction, the results are quite similar relative to average monthly awards. However, when using a 10-mile restriction, I find smaller effects. In both cases, the estimates have wide standard errors.

2.6.2 Placebos and Robustness

To show that these results are unique to periods where electoral benefits change, I use a series of placebo experiments. The primary concern is that the results may be mechanical. If the experiment produces carry-over segments which politicians already favor the results could simply reflect that there is more spending leading up to an election. To show this is not the case, I repeat the experiment, but use awards data from periods when there is no change in congressional boundaries.

To estimate the placebos, I repeat the same setup I use for the main results. That is, I

Table 2.9: Removing Incoming Politicians

	New Funding		Continuation Funding	
	(1)	(2)	(3)	(4)
	No geographic restriction			
Carryover x Announcement	0.316**	0.315	0.058	0.264*
	(0.125)	(0.229)	(0.115)	(0.149)
Dependent Variable Mean	3.48	2.45	1.16	1.00
Number of Observations	6,842	6,842	6,842	6,842
	10 mile geographic restriction			
Carryover x Announcement	0.269	0.048	0.027	0.315*
	(0.164)	(0.308)	(0.152)	(0.180)
Dependent Variable Mean	2.46	2.13	1.01	0.97
Number of Observations	4,896	4,896	4,896	4,896
All Projects	X		X	
Non-government		X		X

Table displays the main results using a set of time-varying fixed effects based on congressional districts prior to redistricting excluding district segments in the comparison group with an incumbent running for election. Columns 1-2 show results for new grants and 3-4 show results using continuation grant fund for various categories. Columns 1 and 3 use project grants, columns 2 and 4 use non-governmental discretionary grants. The first panel shows results using per-capita amounts with no geographic restriction. The second panel shows results using per-capita amounts with a 10-mile geographic restriction. Regressions are trimmed to exclude the smallest 1% of district segments by population. Standard errors show in parenthesis and are clustered at the congressional district level. Dependent variable is measured in means. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

gather federal awards data from 2013-2016 and geocode their location to district segments. I then apply the same carry-over and out-going status to each district segment as well as redistricting announcement, and repeat the regressions of interest.

In table 3.4, I show results from the placebo analysis. I show results for new per-capita project and non-government grants. Each panel is a stacked regression, which tests for equivalence of the estimated effect from the redistricting and non-redistricting periods. There are 2 sets of placebos, one that uses data from January 2013 to October 2014, and another that uses data from January 2015 to October 2016[18].

I find no estimated treatment effect in the placebo periods. In the 2013-2014 placebo, I find a result estimated tightly around 0. In the 2015-2016 placebo, I find large, negative, non-statistically significant effects in all cases. I test for equality of the coefficients. When using

[18]These placebos exclude state time pairs where a redistricting event took place. This includes North Carolina and Virginia in the 2015-2016 placebos.

Table 2.10: Placebo Estimates

	(1)	(2)	(3)	(4)
2011-2012	0.973***	0.425**	0.752***	0.348*
	(0.300)	(0.164)	(0.268)	(0.184)
2013-2014	0.024	0.111	-0.132	0.015
	(0.379)	(0.154)	(0.253)	(0.190)
2015-2016	-0.020	-0.079	-0.633	-0.370
	(0.517)	(0.332)	(0.676)	(0.494)
Sample	All	10m	All	10m
All Projects	X	X		
Non-government			X	X
Pvalue, 2011 = 2013	0.05	0.02	0.16	0.21
Pvalue, 2011 = 2015	0.10	0.06	0.18	0.18

Table displays results of stacking placebo periods with the period where redistricting occurs. Each coefficient corresponds treatment effect from the period of interest. Placebo redistricting announcements are based on the actual date, but are moved 2 and 4 years forward. All dependent variables are new per-capita grants. Columns 1 and 2 show results for project grants, column 3 and 4 shows results for non-governmental discretionary grants. Odd columns include no geographic restriction. Even columns includes a 10-mile geographic restriction. P-values testing for equality are included in each panel. In all instances, equality is rejected at at least the 10% level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

project grants, I reject equality at the 10% level for all samples. However, I am not able to reject equality using non-government grants. Although, this is primarily due to the imprecision in the non-placebo year estimates.

The application of a placebo in this way has an intuitive appeal. It applies the redistricting announcement across district segments in the same way that they actually occurred in 2010 redistricting periods. But, the date at which a state redistricts is not constant across redistricting periods. For this reason, I show a second set of placebos which randomizes the date of the redistricting announcement. For each state, I generate a randomly assigned redistricting date, which is drawn between April of the odd year and June of the even year. I then repeat this exercise 10,000 times and plot the corresponding coefficients.

In figures 2.13, 2.14, and 2.15, I show the distribution of coefficients from these results. In each figure, I show results for per-capita project grants. In figure 2.13, I show results using only

the 2013-2014 period, figure 2.14 using 2015-2016, and figure 2.15 pools the placebo periods together and jointly estimates the effect.

Figure 2.13: Redistricting Period Placebos: 2013-2014

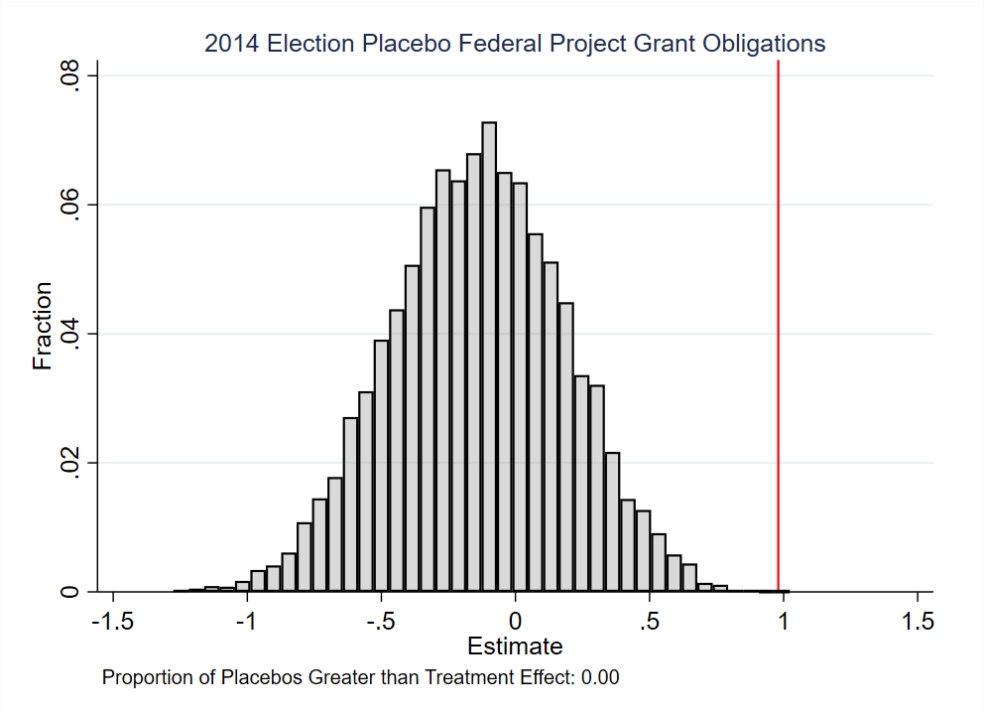


Figure plots the distribution of coefficients from 10,000 placebo regressions. Each placebo regression includes periods from January 2013 until the November 2014. Each regression uses a randomly generated redistricting passage date within April 2013 to June 2015 and and maintains the redistricting assignment for district segments. The mean and standard deviation of the coefficients are shown in the top right, along with actual estimated treatment effect.

The results indicate little evidence that there is any effect of a district segment having carry-over designation during non-redistricting election years. I find the proportion of placebos that are greater than the estimated effect from the redistricting period are less than 5%. I take this as evidence that the research design correctly identifies a change in electoral benefits that leads to a change in how funds are distributed. Additionally, the lack of an estimated effect provides confidence that the research design does not produce a treatment variable that is endogenous to funding when an election nears.

Figure 2.14: Redistricting Period Placebos: 2015-2016

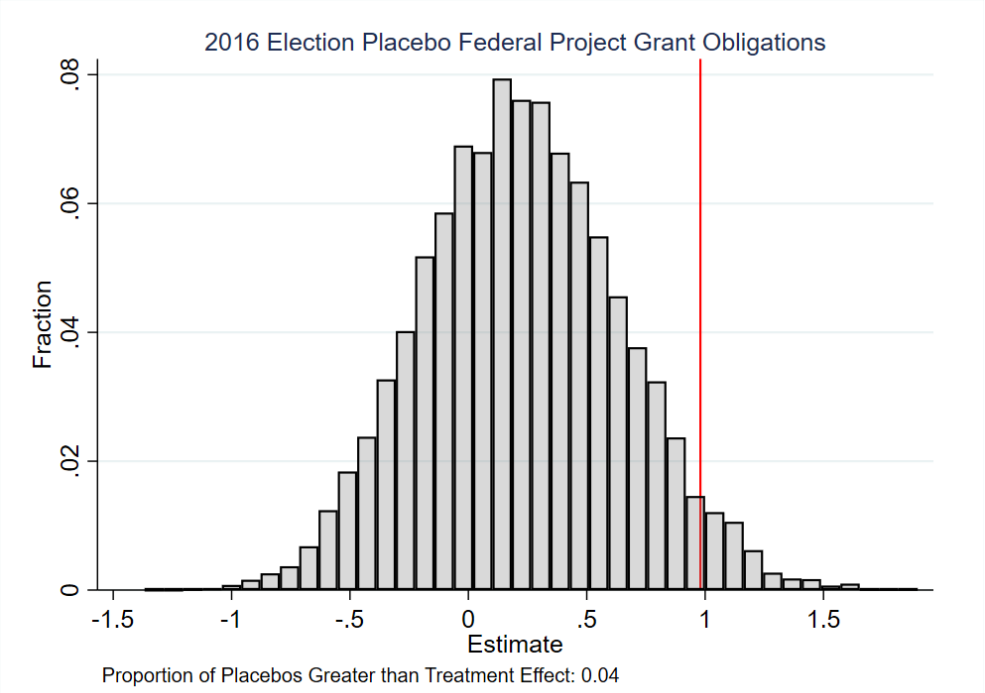


Figure plots the distribution of coefficients from 10,000 placebo regressions. Each placebo regression includes periods from January 2015 until the November 2016. Each regression uses a randomly generated redistricting passage date within April 2015 to June 2016 and and maintains the redistricting assignment for district segments. The mean and standard deviation of the coefficients are shown in the top right, along with actual estimated treatment effect.

Figure 2.15: Redistricting Period Placebos: Pooled 2013-2016

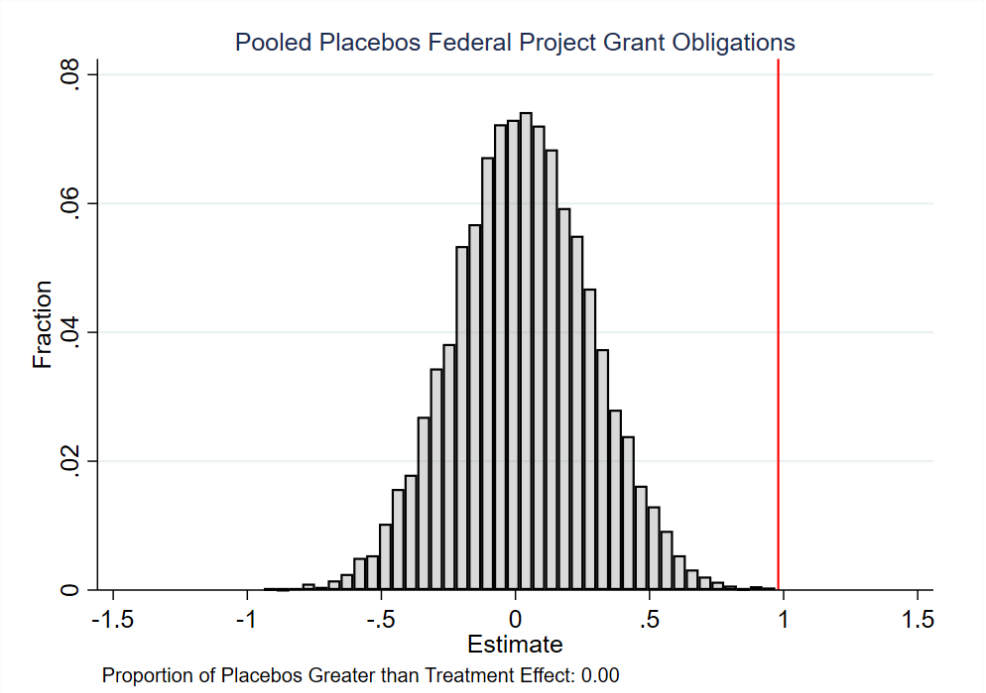


Figure plots the distribution of coefficients from 10,000 placebo regressions. Each placebo regression includes periods from January 2013 until the November 2016. Results from 2013-2014 and 2015-2016 are pooled. Each regression uses a randomly generated redistricting passage date within April 2013 to June 2014 and another from April 2015 to June 2016. The mean and standard deviation of the coefficients are shown in the top right, along with actual estimated treatment effect.

2.7 Conclusion

In this paper, I show that elections incentivize representatives to provide funding to their constituents. I find that areas that carry-over their incumbent politician after redistricting receive more federal awards than areas that are redistricted to a new politician. I use varying geographic restrictions around the border of congressional districts and find these effects to be between $.75 and $.97 per-capita depending on the type of award. In all cases, this effect is large, representing more than 20% of mean per-capita awards.

Additionally, this paper presents a framework for identifying a sharp change in electoral incentives. The strategy produces a lower-bound of forward-effort. However, given enough sample power, it is possible to produce estimates free of any additional terms. For instance, as the 2022 redistricting cycle concludes, it may be possible to estimate a non-lower bound effect due to additional samples.

More generally, this design can be used to answer questions relating to a politician's electoral incentives as well as to redistricting. The two constraints of the design are that it requires the availability of data at a sub-congressional level and that it be at a high enough frequency that pre- and post-redistricting periods are observed. A natural extension that does not have such strict data requirements would be to evaluate the long-term effects of an area being redistricted. Additionally, in 2022, there will be a presidential and congressional election simultaneously – it is possible to think about how the executive branch may influence these decisions. There are examples unrelated to the disbursement of federal awards. For example, we may be interested in voter attentiveness, studying which candidates voters support after redistricting occurs or how institutional donors respond to these changes.

The findings of this paper inform us about a central feature of politics: how elections influence a politician's behavior. My methodology offers a credible lower-bound on how political incentives alter the distribution of funds. By utilizing variation induced by redistricting, I can cleanly observe changes in electoral benefits that representatives face while holding fixed funding norms, backward-looking incentives, and endogenous determinants of funding. The results of this paper indicate that elections ultimately play an important role in the destination of discretionary funds.

Chapter 3: Local Distributive Politics: Party Matching and Government Unification

3.1 Introduction

What role does partisan bias play in the distribution of government funds? We know that partisanship can change the types of expenditures in a government; for instance, Republicans and Democrats are known to have different spending preferences. However, the total amount of revenues states provide to local governments is strikingly similar regardless of the political party in power. Why is it, then, that areas that are represented by the political party in power often receive more funding? A simple regression shows that when a state's government is either fully Republican or Democratic, local areas represented by the party in power on average receive about $100 more of per-capita state intergovernmental revenues than those areas represented by the minority party, an almost 10% difference. Relaxing the definition of the state majority party to only apply to a state's legislature reduces this association to around $70 per-capita. On one hand, political affiliations could be a causal mechanism, with state governments systematically targeting politically aligned districts. Alternatively, this could reflect the different spending priorities of different political parties, as those priorities tend to be related to the underlying characteristics of an area and their constituents' preferences. If these funds represent a vital component of local governments' funding for programs or services, then we should be concerned that political homophily may threaten the fairness of their distribution. In this paper, I isolate

the impact of a district's representative belonging to the majority and show this has a causal impact on the amount of funding received from the state. Additionally, I find that these effects are differential depending on whether or not there is a singular majority political party in the state.

Empirical researchers have worked to show how majority parties impact the distribution of funds. This literature is primarily focused on the federal government's interaction with states' and local governments. Instead, I focus my attention on intergovernmental transfers from state to local governments. Using variation from state governments offers three key improvements over the federal literature.

First, utilizing the state government simplifies the number of distributive channels through which funds travel before reaching their destination. In the United States, many grants awarded at the federal level are first awarded to state governments or agencies. In some instances, these entities have influence over where the funds are awarded at the subgrant level. Considering this, analysis of the distribution of federal funds to local governments is a highly complex problem with many interactions. Studying the distribution of state funds to local governments simplifies some of these interactions.

Second, it increases the statistical power due to the substantial wealth of state legislative districts versus congressional districts in the United States. There are 6,764 legislative districts in the United States compared to 435 congressional districts. I leverage this increase in sample size by using a regression discontinuity design around a narrow bandwidth instead of the global approach used in prior work or relying on two-way fixed effects estimators.

Finally, in a given time frame there are more state government experiments than there are federal ones. Each state government's election cycle can be thought of as a separate experiment.

This produces close to 50 times more experiments per election than those that exist at the federal level. This increase in experiments allows me to heterogeneously estimate treatment effects by unification status of the government. I find the distinction between divided and unified governments is critical and is something that cannot be estimated at the federal level. Under divided governments, majority party affiliation effects are non-existent, but when governments are unified, there is a substantial majority party effect. Understanding how the distributive process differs under divided and unified governments is additionally important due to the ways in which state governments have evolved over the last 30 years, with the number of states with a divided government dropping from 31 states in 1992 to only 12 in 2020.

In addition, state intergovernmental transfers represent a large portion of local government revenues, representing between 27 to 29% of local government revenues from 2010 to 2017. In the United States, local governments' largest expenditures are on their local elementary and secondary education school systems. They also spend substantial revenues on health and hospitals, policing, and highways and roads. Much of this spending is tied to specific formulas, but there are also funds for which politicians can exercise discretion as to how they are distributed. For instance, The Sacramento Bee reported that the 2018 California state budget contains many examples of funding capital projects in specific areas, such as $700,000 to the city of Lakewood, CA, that is to be invested in "community facilities, park, or recreational facilities construction, acquisition, or improvements." Lakewood, CA, is contained by the 63rd State Assembly district, which is represented by the majority speaker of the Assembly[1]. In 2018, California's state government was fully controlled by the Democratic party. Examples such as this bring up two concerns: the level of fairness in the distribution of funds and the possibility of corruption. Being

[1]In California, the State House of Representatives is called the California State Assembly.

represented by the majority party should not systematically lead to more funds within that district. Additionally, a source of targetable funds that are difficult to trace provides the opportunity for politicians to take advantage of their position of power, giving incumbents and majority party candidates a possible advantage in future elections.

Since 1991, the federal government has been under unified control for a total of ten years; however, an insufficient amount of variation makes estimating how the distribution of funds changes as a result of this unification or lack thereof nearly impossible. Instead, results from state governments can be used to learn how distributive politics may differ federally under different types of political unity. There are many similarities when it comes to the distributive process between the federal and state governments – the governing structures are nearly identical, and annual or biannual budgets are a primary mechanism used to distribute funds. The key difference between states and the federal government are the rules for setting the budget. The majority of states have some type of balanced budget requirement. Balanced budget amendments have made a strict interpretation complicated, however what remains important is that they add a possible friction in the state budget process that is not present in the federal government. Given the institutional similarities, the partisan effect within states should operate similarly to how it operates at the federal level. However, the magnitude of the impact of majority representation on pork may be lesser in states due to frictions resulting from balanced budget requirements.

Albouy (2013) estimates how the makeup of a state's delegation in Congress can influence the amount of federal grants that a state receives. He finds strong evidence that states with senators in the majority party receive more federal grants than states without. Additionally, he finds that states with a House delegation of the same party as the president receive more grants. My paper offers two contributions to the existing work on distributive politics.

First, I estimate the effects of local representation. Aggregating funding to the state level still leaves the question of where these funds eventually land. It is possible these funds could be targeted locally to areas that strongly supported the politicians, or they could be used to try and change election outcomes in swing districts. Berry, Burden, and Howell (2010) estimate this relationship locally; however, this introduces the complexities of state governments' interaction with the distribution of pass-through grants. This channel has the possibility to attenuate results at the federal level. As an example, consider that some share of federal grants are awarded to states and then are allocated locally. It is then possible that these pass-through grants could be influenced by the governor. When the governor and president are from different political parties, these funds may disproportionately land in areas in support of the governor's party and not the president's.

Second, due to the substantial increase in the number of government experiments, I am able to heterogeneously estimate the effects of majority party representation by government unification status. The degree of government unification changes the degree of political friction that any bill faces. Intuitively, it should be the case that if majority parties exclude minority parties, it is easier to do so when the majority party is the same political party in both the legislative and executive branches. Government unification also limits the need for pork to be used as a tool to pass legislation. When Lyndon Johnson needed support for the Civil Rights Act, he used pork-barrel spending to gain support, in one example offering his support towards a NASA research grant in exchange for the House Republican leader's vote[2]. It is possible that political frictions as a result of a divided government limit a politician's ability to target their

[2]In this example, Congress was unified; however, the Democratic party was split between Northern and Southern Democrats.

district. Or, it could be the case that when governments are divided, pork-barrel spending is saved to pass legislation, leading to smaller effects of majority representation on transfers. However, when the government is unified, the pork for policy trade is not needed, and therefore, the expected effect of party affiliation is unambiguously positive. Estimating heterogenous treatment effects of majority representation under differing degrees of government representation provides evidence of how the distributive process differs under different types of regimes as well as furthers our understanding of how pork is used.

Ansolabehere and Snyder (2006) estimate the importance of voter alignment in divided versus unified state governments, finding that when state governments are unified, there are differential effects of funding depending on which party is in control. They aggregate funding to the county level and designate counties as either Republican- or Democratic-leaning depending on how their residents voted in several different elections, finding that state governments direct funding towards areas that provide them with the strongest support. The main difference is the distinction between representation and voter alignment. My paper instead offers insight into the effect of a local areas representative belonging to the majority political party.

In my paper, I use a regression discontinuity design akin to Asher and Novosad (2017) to estimate the causal impact of party affiliation on the distribution of funds. Unlike much of the regression discontinuity work in political economy, I use running variables based on the party of the executive office and the majority party of the legislative chamber. I conduct analysis for two treatments: governor party affiliation and legislature party affiliation. This method substantially improves over the two-way fixed effect estimator; however, it requires strong local identification assumptions for external validity. In the most conservative interpretation, I produce estimates of the causal impact of party affiliation on funding in close races. Theoretically, the dispersion

of funds has interesting properties in competitive elections. It is unclear whether politicians would use funds to sway voters in closely lost minority districts or reward their constituents by increasing funding. Regardless, a main concern related to pork-barrel spending is whether it can be used to manipulate elections. Close election districts represent the most plausible set of elections where pork may be used in this way. The question of pork's impact on electoral outcomes is beyond the scope of this paper; however, this paper will identify whether it is a possible lever the state government can pull to try and shift elections.

I construct multiple measures of state legislative district funding within lower and upper districts. Following Aidt's and Shvets' (2012) convention, I break intergovernmental transfers into two categories: discretionary and non-discretionary funding. Non-discretionary transfers are thought to be transfers that are outside of a legislator's ability to control. I additionally look at the effect on total transfers. With a large portion of non-discretionary transfers being awarded by formula using non-discretionary funds as an outcome acts as a robustness test for the regression discontinuity design, providing an additional test of the balanced underlying characteristics assumption.

In my preferred specification, I find that when a state legislative representative belongs to the same party as the governor, they receive 2.1% more transfers from the state and 2.7% more discretionary transfers, and I find no evidence that non-discretionary funds are impacted. I find similar, but quantitatively smaller, impacts when a state legislative representative belongs to the same party as the legislative chamber majority, receiving 1.2% and 2.0% more total and discretionary transfers, respectively. Additionally, I find that the party affiliation impact is largest, a 3.7% increase in discretionary funding, when the state government's legislative chambers and governor are unified under a single party and that there is no significant party affiliation effect for matching either the governor or legislature when the government is divided.

The rest of the paper proceeds as follows. In section 2, I review the state governments, their budget cycle, and its importance for the distribution of funds. Section 3 describes the data used. Section 4 outlines the main empirical strategy. Section 5 contains the baseline results on party affiliation. Section 6 contains various robustness tests. In section 7, I show results for heterogenous effects by government unification. Section 8 concludes the paper.

3.2 State Governments and Budget Cycles

In all states but Nebraska,[3] a state's government consists of an executive branch, the governor, and a bicameral legislative branch. The two bodies that make up the legislature are the State House of Representatives, referred to as the lower house throughout this paper, and the State Senate, referred to as the upper house throughout this paper[4]. Each legislative body consists of members in the majority and minority parties. In the context of United States politics' two-party system, this distinction is simple. The majority party is defined as the political party that has greater than 50% seat share in a given legislature, and the minority party is the political party with less than 50% seat share[5]. Unified governments have a governor, upper house majority and lower house majority all of the same political party. States have divided governments when any one of these bodies belongs to a different political party. With two parties and three bodies of government, this leaves a total of eight possible government arrangements, although these can be more simply thought of as a fully unified government, a government with different party legislatures, and a government with unified legislatures but a different party governor.

State budgets are the primary tool that state governments can utilize to change funding

[3]Nebraska has been excluded from the study.
[4]The formal name of the lower house varies by state but the bodies are functionally similar.
[5]Nebraska is the only state whose legislators are elected with no party affiliation on the ballot.

patterns. The budget determines the amount of funding that specific programs and agencies receive. For example, in the context of education, the state budget determines the amount of funds that a state's department of education receives and can contain language instructing how that money is to be allocated, either categorically or geographically. The budget procedure offers state politicians a way to target funding to their districts, although it is often difficult to track. I next describe how a budget is created and how state legislators and governors can impact it.

Wallin (1999) notes that despite the diverse political approaches of many states, their budget processes are largely similar. In general, budgets start with the governor: they have the budget office send out instructions to agencies to report their projected needs for the next fiscal year. Once these agency requests are received, the governor and their staff prepare a budget proposal with projected revenues and appropriations. Most states (43) have a variation on a balanced budget requirement. Rodriguez-Tejedo and Wallis (2012) document the evolution of balanced budget amendments and how these requirements operate. Generally balanced budgets are meant to keep revenues and expenditures equal, but accounting definitions cloud this generalization. Instead, these requirements should be thought of as a possible friction in the budget process.

Governors then submit this proposal to the state legislature where amendments can be made. Typically, the budget is sent to an appropriations committee in the upper and lower houses. Once these sessions are completed, the budget is taken to each chamber, where the bill can be amended and is eventually voted on. Balancing requirements dictate that the chambers can only alter the bill by cutting spending somewhere to add it elsewhere, which creates additional friction for any amendments.

Once the budget bill is passed by the lower and upper chambers, it is delivered to the governor. The governor has the option to either sign the bill into law, veto the bill outright, or

strike items on the budget without returning the budget to the chambers in an act known as line-item veto authority. Line-item veto authority allows the governor to remove portions of the budget they disapprove of; however, additional items cannot be added. Holtz-Eakin (1988) shows that line-item veto authority has limited long-run impacts on state budgets, but that political incentives can alter spending in the short-run. Gosling (1986) studies how governors of Wisconsin have used this power and finds that most vetoes are not directed towards appropriations. Despite this, the threat of line-item vetoes may alter how legislatures amend the budget.

The budget cycle along with the various veto authorities present opportunities for party affiliation to play a role in the distribution of funds. However, the vast majority of budget items do not specify which localities are to be the recipients of funds. Much of the budget is dedicated to setting state agencies' budgets and grant programs. For instance, a recent Wisconsin state budget created a grant program of $250,000 for the Department of Instruction to distribute to schools for water bottle filling stations. Most of the grant programs do not specify the exact process for the allocation of funds. This leaves room for back-alley deals to be important determinants in the distribution of funds.

Legislators additionally make use of earmarks: specific designations of funds that are attached to specific bills. During the 2017-2018 legislative session the Common Wealth Foundation, a Pennsylvania think tank, estimated that Pennsylvania lawmakers attached $65 million in earmarks to The Fiscal Code, which defines how the budget is implemented. These earmarks are often purposefully written in such a way that without substantial knowledge of that state's sub-state-government classification system, it is unclear where exactly the funds are targeted. For example, the bill proposed "at least $5,000,000 shall be distributed to a hospital in a city of the third class in a home rule county that was formerly a county of the second class A." To explain what this

means, local governments in Pennsylvania belong to one of six types: county, township, borough, town, city and school district. Home rule counties are incorporated under their own charter, which allow them to design their own form of county government and counties and cities are classified according to their population. Taken together, this earmark dictates that funds are to be distributed to a third class city (53), located in a home rule county (7) that was formerly of the second class A (1). There is only a single city which qualifies, Chester city. At the time, Pennsylvania had a divided state government and had gone a year without passing a budget and it's possible earmarks like these were part of the reason for the budget's eventual passing.

Anderson, Lassen, and Nielsen (2012) show that divided state governments are an important underlying factor for some state budgets' tardiness. A late budget brings uncertainty regarding how a state will distribute appropriations to local governments. In some cases, the prior year budget is carried forward, but in other cases a late budget can lead to a government shutdown. This provides evidence that there are in fact meaningful political frictions that change how state budgets are enacted. Given their severity, the possible penalties associated with a late budget may induce more cross-party giving, reducing or even possibly canceling out any majority party affiliation effects.

3.3 Data

I utilize two main types of data: political outcomes and funding decisions. This section describes the underlying data, the process for combining these data, and the process for calculating the size of upper house and lower house district level transfers.

The primary data source for political outcomes is the State Legislative Election Returns

Dataset (SLERs). I use this dataset to procure election returns for both lower and upper house elections from 1992 to 2016. This dataset produces the number of votes by party and incumbency status for each legislative district. I use the State Partisan Balance Data to obtain the seat shares of Republicans and Democrats in the upper and lower chambers as well as the party of the governor from 1992 to 2016. In my final dataset, I stack upper and lower house districts to create a legislative district by year panel which contains vote totals by party, incumbency status of the legislator, majority party of the lower house, majority party of the upper house, and party of the governor [6].

Financial data come from two sources. I use the individual public use files from the Annual Survey of State and Local Government Finances. This dataset contains the amount of intergovernmental revenues that are received by local government units. In years ending in '2' or '7', the entire country is canvassed, and in between, a smaller survey is conducted and data is imputed based off estimates from either a prior year survey or the estimates from the census. The individual public use files contain line items for different types of government financing activities. For this paper, I gather data on all local governments' intergovernmental revenues from the state, which are broken into multiple categories[7]. Intergovernmental revenues from the state are funds that a local government receives directly from the state. These include grants-in-aid, federal aid passed through the state, local share of state-collected taxes, payments in-lieu of taxes on state property, and reimbursement for services performed for the state government.

I supplement the Census of Governments with data on school finances taken from the

[6]Due to previously mentioned reasons, Nebraska has been excluded from this study. Louisiana and Nevada have been excluded due to their exclusion from the SLERs database. Arkansas, California, Florida, Hawaii, Kentucky, Maine, Maryland, Minnesota, Montana, New Hampshire, and Texas were excluded from 1992-2001 due to lack of state legislative boundaries from the Census Bureau's redistricting data program.

[7]The full list of categories is education, other general support, health and hospitals, highways, house and community development, public welfare, sewerage, other, and utilities.

Census Bureau's Annual Survey of School System Finances (ASSSF). The survey is designed to capture all public elementary and secondary school systems in the United States. It is not based on responses of individual school districts, but instead is collected by state governments' departments of education. These agencies submit existing finance information that is either already formatted to the survey's category formats or are converted by the Census Bureau. This survey is designed to be a fully representative picture of public school finances in the United States.

I use two variables from the ASSSF: Total Revenue from State Sources and General Formula Assistance from the State. I define nonformula revenue as total state revenue less general formula revenue. The survey defines general formula revenue as general noncategorical state assistance, which includes "foundation, minimum or basic formula support, apportionment, equalization, flat or block grants, and state public school fund distributions. This category also includes revenue dedicated from major state taxes, such as income and sales taxes[8]." In short, this variable contains the funding a state gives according to its general formula, which is designed to ensure every school has some minimum level of funding per-student. The remainder of the revenues from the state are for categorical aid. These amounts are given with specific purposes such as special education, staff improvement, transportation, or vocational programs. Categorical aid gives the school district less flexibility with how they use funds and offers state politicians more control over how funds are used instead. These revenues represent amounts that can generally be changed in a line-item decision in the budget process and are thus more easily manipulated by state politicians.

Using these two sources, I follow Aidt and Shvets (2012) and their classification method

[8]This definition comes from the ASSF technical documentation.

of funds. They categorize funds as either discretionary or non-discretionary. Non-discretionary transfers are those that are allocated according to some pre-specified formula or rule and include education formula funding, welfare payments, and utility transfers. The second category is discretionary transfers, which are funds that are not allocated according to a fixed rule. These include spending on highways, health, transportation, housing, and local government support.

The primary challenge when it comes to linking state legislative district funding with political outcomes is that intergovernmental revenues are not allocated to a state legislative district, instead they are distributed to a local government. These boundaries do not necessarily need to have any relationship to one another[9]. I perform a matching procedure that allocates funds to state legislative districts based on population-weighted geographic overlap of local governments. To calculate population-weighted overlap, I construct geographic segments between local government boundaries and state legislative districts and overlay census blocks from the decade the redistricting was based on to calculate the population overlap. This procedure requires the additional assumption that the intergovernmental spending in a local government is spread evenly according to its population. This produces a noisy estimate of district level funding; however, similar results can be obtained when I restrict my sample to a subset in which the matching procedure has very high overlap scores between local government boundaries and the state legislative district. The main concern with this method is whether the noise with which I measure government transfers biases the coefficients on majority party affiliation, that is, whether the error that I measure transfers with is correlated with a politician belonging to the majority party and barely winning an election. Given the similar results with a restricted sample as well

[9]Some states have redistricting laws in place that require legislative districts to respect local government boundaries to the best of their ability.

as the strong observed balance of other covariates in my samples, I do not believe this is a valid concern.

3.4 Empirical Strategy

The main goal of this paper is to test whether districts with a representative from the same party as that of the governor or legislative majority receive differential amounts of state funding. I then test for heterogenous effects under a fully unified state government, any type of divided government, and divided governments with a unified legislature using subsample analysis.

The naïve approach of studying the effect of majority representation on funding involves a crucial concern that party status could be correlated with unobserved factors that affect funding decisions such as spending priorities and legislator traits. In the past, researchers have used two-way fixed effects models to obtain valid identification. These models rely on the exogeneity of a local district belonging to the majority party and are identified based off districts entering and exiting the majority party. Recent research has shown that two-way fixed effect models inconsistently estimate cohort-average treatment effects in the presence of treatment effect heterogeneity over time. Given that the number state governments with a single majority party has increased overtime, treatment effect heterogeneity over time is expected.

I instead use a semiparametric regression discontinuity design to account for these unobserved factors. The main identifying assumption is that treatment, i.e., party affiliation, is as good as random near the cutoff, ensuring that barely winning and barely losing districts have similar unobserved and observed characteristics (Lee and Lemieux [2010]). The tradeoff of this research design is that it only uses data near the treatment threshold, so local average treatment effects are

estimated. The concern with this approach is that I produce an estimate that is unique to only a small subset of all elections. In fact, there is reason to suspect that the way politicians distribute pork to competitive and non-competitive districts is different. Given that competitive districts are the most likely districts to be won by the opposing party in future elections, these represent the districts we are most interested in from an electoral fairness perspective. The goal of this paper is not to understand whether targeting influences future election outcomes, but rather to determine that targeting occurs.

I construct a running variable based on the vote share of the majority party in the same fashion as Asher and Novosad (2017). The running variable consists of the vote share that the candidates from the majority and minority parties received. The legislative majority party is defined as the political party that holds the largest seat share in that legislative chamber, and the governor majority party is defined as the political party of the governor. In the two-party system in the U.S., this means that the majority and minority party vote shares always correspond to either the Democratic or Republican party vote shares. Equation 3.1 presents the formula used to derive this. I define the variable $margin_{dt}$ as the vote share that the majority party candidate received in district d in year t, centered at 0, v^{maj} as total votes received by the majority party candidate, and v^{min} as total votes received by the minority candidate [10]. This results in a positive margin variable when a representative is a member of the majority coalition and a negative margin variable when the minority candidate wins. By construction, this variable is centered at 0, so that barely positive values indicate that the majority party candidate barely won an election and barely negative values indicate that the majority party candidate barely lost.

[10]Constructing the running variable in this way does not allow a third-party candidate to be the minority candidate, even if they received the second highest vote share.

$$margin_{dt} = \frac{v^{maj} - v^{min}}{2(v^{maj} + v^{min})} \tag{3.1}$$

I compute this margin for two different types of party affiliations. I utilize data from both upper and lower house districts and construct a first margin variable based on the district representative's party relative to the governor's party. The second running variable is created using the majority party of the same chamber as the district, so that I use lower house majority affiliation in lower house districts and upper house majority affiliation in upper house districts. Creating these distinct party affiliation variables allows me to differentiate which political margins are most important in the distributive process and uncover possible differential effects.

Under the standard regression discontinuity assumptions, additional controls or fixed effects are unnecessary for identification. However, their inclusion reduces the sampling variability. To ensure that I am not meaningfully changing the regression-discontinuity estimator by including covariates I show results with and without covariates. I estimate models using local linear regressions with a triangular kernel function[11]. Later I test whether the result is sensitive to local polynomial order, finding similar results using up to a fourth-degree polynomial. To select the bandwidth used, I use a data-driven approach to determine the MSE optimal bandwidth; however, I additionally test the sensitivity of the results under various bandwidth specifications.

To formalize the model, let Y_{dt} represent per-capita state intergovernmental funding in district d in year t. Treatment assignment, M_{dt}, is based on whether a district representative is a member of the majority party, defined as when $margin_{dt}$ is positive. Districts are then treated according to equation 3.2.

[11]The choice of kernel has little impact on the interpretation of the results; similar results are found using epanechnikov and rectangular kernels.

$$M_{dt} = 1(margin_{dt} \geq 0) \tag{3.2}$$

Before deciding on modeling restrictions, the baseline estimating equation is then given by equation 3.3.

$$Y_{dt} = f(margin_{dt}) + \beta M_{dt} + \epsilon_{dt} \tag{3.3}$$

In equation 3.3 $f(margin_{dt})$ is an unknown smooth function of majority party vote share and β is the impact of majority representation on per-capita state intergovernmental funding. The major decision is how to model the unknown smooth function, $f(margin_{dt})$.

In my main estimations, I use a local linear regression (Hahn, Todd, and Van der Klaauw [2001]). In these models, treatment effects are estimated in one step using a kernel-weighted regression with kernel weights $w_{dt} = K(\frac{margin_{dt}}{h})$ for any bandwidth h[12].

$$y_{dt} = \alpha + \beta_1 margin_{dt} + \beta_2 M_{dt} + \beta_3 M_{dt} * margin_{dt} + \epsilon_{dt} \tag{3.4}$$

The baseline equation with no controls is described by equation 3.4. The coefficient of interest is β_2, which can be interpreted as the causal impact of a representative's membership in the majority party. Since $margin_{dt}$ is constructed such that its cutoff point is 0, the effect of membership in the majority is only identified through β_2 and not the addition of β_1 and β_3 x $margin_{dt}$. I estimate this model with bandwidths as large as 20 percentage points, i.e., I include elections when the minority candidate lost by 20 percentage points or fewer and elections when

[12]This method has been used in numerous regression discontinuity designs such as Asher and Novosad (2017) and Ludwig and Miller (2007).

the majority candidate won by 20 or fewer percentage points. I similarly include results with bandwidths as small as 5 percentage points.

$$y_{dst} = \alpha + \beta_1 margin_{dst} + \beta_2 M_{dst} + \beta_3 M_{dst} * margin_{dst} + \gamma X_{dst} + \lambda_{st} + \epsilon_{dst} \tag{3.5}$$

Equation 3.5 shows a similar regression but with district controls and state-year fixed effects included. In this model, subscript s denotes the state. The additional terms are λ_{st}, representing state-year fixed effects and X_{dst}, a vector of district level covariates. These controls include demographic and legislature characteristics[13]. I additionally include state-year fixed effects to account for potential time varying regional variation in state funding. To account for correlations within states and their legislative environments, the error, ϵ_{dt}, is clustered at the state chamber-year level.

3.4.1 Balance Tests

The key identifying assumption of the regression discontinuity is that districts where the majority party candidate barely won are similar to districts where the majority candidate barely lost in both unobservable and observable characteristics. It is possible to formally test the observable portion of this hypothesis against a set of observables. I test this hypothesis using all baseline covariates from the main model. These covariates can broadly be broken down into three categories: demographic, political, and school characteristics. Demographic variables

[13]The full set of controls are state-year fixed effects; dummies for multi-member districts, Democrat legislator, upper house, and incumbent; population; average adjusted gross income; average wages; percent of the population over 65, under 14, female, black; total education expenditures; and local education revenues.

included are population; population squared; the percent of the population over 65, under 14, female, and black; average adjusted gross income; average wages; and number of tax exemptions. Political variables used are dummies for a Democratic representative, multi-member district, upper district, and incumbent legislator. School based characteristics used are total education expenditures and local education revenues not from state sources. In table 3.4.1, I show regression discontinuity estimates for each covariate using a fixed bandwidth of 10% and include state x year fixed effects for governor party affiliation and legislature party affiliation[14]. In total, I test for continuity among 15 covariates across 2 treatments, for a total of 30 tests of continuity. In total, I find 1 estimate with a p-value less than 10% and in general the point estimates on each regression discontinuity estimate are relatively small. This indicates that both treatments are balanced across a broad range of observable characteristics.

I then test for possible manipulation of the running variable. The concern is that candidates from the majority party may disproportionately win close elections, which could be taken as evidence of systematic manipulation of treatments. Intuitively, this amounts to testing for a discontinuity in the density of units near the cutoff. If there is a discontinuity in the density, it is possibly a sign of nonrandom sorting of units into treatment. Grimmer, Hersh, Feinstein, and Carpenter (2011) show that structurally advantaged U.S. House candidates systematically win more close elections, and there may be a similar threat in state elections[15]. Since state governments are responsible for drawing their own district boundaries, it is possible that district boundaries are designed in such a way that the majority party disproportionately wins close elections. Friedman and Holden (2008) show that it is optimal for states to engage in concentrating

[14]I additionally perform this test using the MSE optimal bandwidth selection procedure and find similar results.

[15]This result has been shown to be specific to US House elections in Eggers, Fowler, Hainmueller, Hall, and Snyder (2015).

Table 3.1: Covariate Balance

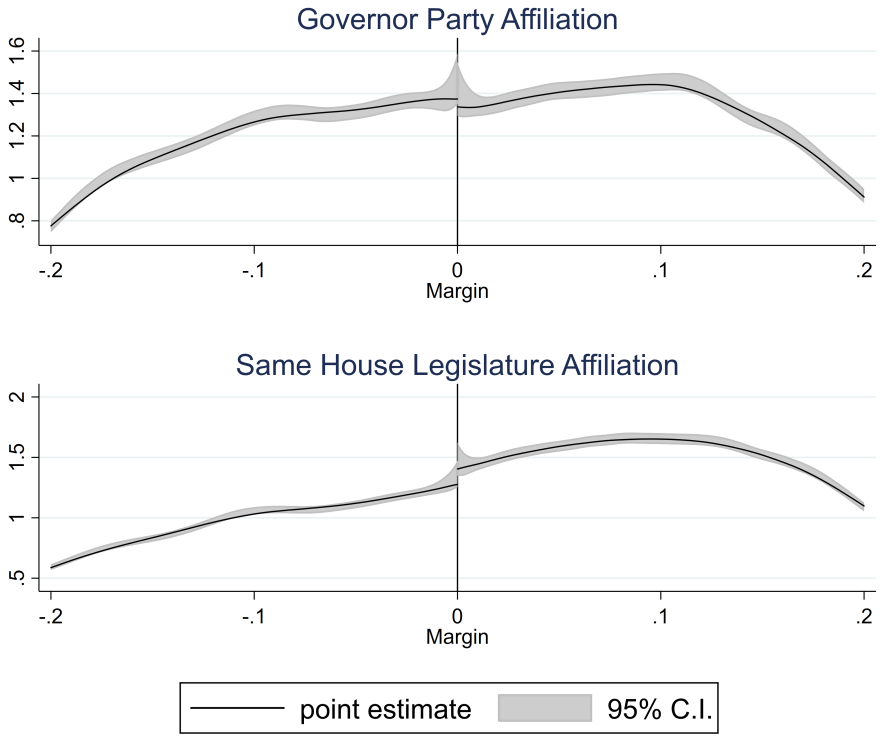
	(1) Governor	(2) Legislature
Demographic Variables		
Population	-287.816	4483.667
	(2697.013)	(2784.467)
Population Squared	-48.267	89.680
	(194.966)	(227.629)
Percent Over 65	0.009	0.037
	(0.108)	(0.113)
Percent Under 14	-0.018	-0.011
	(0.063)	(0.064)
Percent Female	0.005	-0.047
	(0.036)	(0.038)
Percent Black	-0.014	-0.349
	(0.313)	(0.328)
Mean AGI	-0.321	-0.121
	(0.452)	(0.480)
Mean Wage	-0.066	-0.095
	(0.240)	(0.246)
Number Tax Exemptions	-737.564	3518.151
	(2408.583)	(2517.793)
Political Variables		
Democrat Dummy	-0.020	-0.060
	(0.049)	(0.050)
Multimember District	-0.006	-0.012
	(0.008)	(0.008)
Upper District	0.004	0.029*
	(0.017)	(0.017)
Incumbent	-0.008	0.002
	(0.019)	(0.020)
School Characteristic Variables		
Total Education Expenditures	-19.310	-28.646
	(22.800)	(23.219)
Local Education Revenues	-4.897	-5.238
	(15.640)	(16.239)

Notes: Results are based on local linear regression using each covariate as an outcome. All regressions use a bandwidth of 10% and include state-year fixed effects. Column 1 contains results for governor party affiliation. Column 2 contains results for legislature party affiliation. Standard errors shown in parenthesis are clustered at the state-congress level.

the minority party's constituents into a small number of districts, a practice known as packing. The other primary strategy for redistricting, cracking, produces a large number of highly competitive majority party districts. This practice makes it possible and even likely that the majority party candidate may disproportionately win a large number of close elections if state politicians engage in cracking. I test for manipulation of the running variable using the Cattaneo, Jansson, and Ma (2019) local polynomial density estimator. This tests nonparametrically for a discontinuity in the running variable at the cutoff in a similar way that local polynomials are used in the regression

discontinuity design. Figure 3.1 shows the local polynomial manipulation test for both governor and legislature party affiliation. The manipulation test statistic is -0.54 and 1.36 with p-values of .59 and .18 for governor and legislature party affiliation, respectively. This test indicates that there is minimal evidence of systematic manipulation using governor party affiliation and possible concern of manipulation with legislature party affiliation. Nonetheless, due to this possible manipulation, I later use a donut-hole analysis to test the robustness of my results to the exclusion of results closest to the cutoff, those that are most likely to be manipulated.

Figure 3.1: Manipulation Test



Notes: Graph shows the local polynomial density test from Cattaneo, Jansson, and Ma (2017) using a second degree polynomial with a triangular kernel. The bandwidths are selected based on the mean squared error of each density separately. The bandwidths chosen are .051 to the left of the cutoff and .043 to right of the cutoff for the governor margin and .055 and .041 for the legislature margin. The manipulation test statistics are -.542 with a pvalue of .588 for governor party affiliation and 1.36 with a pvalue of .175 for legislature party affiliation.

3.5 Results

I separate results into two categories based on the type of political party affiliation. The data are set up such that lower district and upper district observations are stacked; therefore, when I refer to districts, I am referring to both upper and lower districts. It is possible to separately estimate the effect of party affiliation for upper house and lower house districts, but when I do this the effects are not substantially different. Instead, I choose to stack the data to gain additional statistical power. To account for possible correlations across legislative branches, I cluster standard errors at the state-congress-year level.

The first set of results pertains to district party affiliation with the governor. I report estimates that include state-year fixed effects and series of district controls, which can be broadly defined as demographic, political, and school controls. When comparing these results to versions without controls and fixed effects, the regression discontinuity estimator's magnitude is very similar; however, as expected, the addition of controls increases statistical precision. I perform a regression discontinuity for five different types of state transfers: total, discretionary, non-discretionary, non-formula education, and formula education. The expectation is that non-discretionary and formula education transfers from the state are allocated in such a way that it is difficult for a legislator to manipulate these funds. On the other hand, discretionary and non-formula education transfers from the state reflect items that are more easily changed in a line-item on the budget, a discretionary grant, or in an earmark in a bill.

These results can be seen in table 3.2, where all outcome variables are the natural log of per-capita transfers[16]. The table contains two panels, columns 1-5 contain regression discontinuity

[16]Measuring annual population at the district level is imperfect. There are at least three possible choices: hold

estimates without fixed effects and controls, and columns 6-10 include the covariates and fixed effects. The coefficient of interest is Party Affiliation, which represents the regression discontinuity estimator. In columns 1 and 6, the outcome is all transfers from the state. The coefficient on Party Affiliation is very similar with and without added controls and fixed effects, but it is much more precise when they are included. I find that belonging to the party of the governor increases the transfers a district receives from the state by 2.1% with a p-value of less than .01. Next, in columns 2 and 7, I estimate the effect of governor party affiliation on discretionary transfers from the state. These represent the funds that are most susceptible to manipulation. If there is a party affiliation effect, it is most likely greatest on this set of transfers. I find that discretionary transfers from the state increase by 2.7% when the district is aligned with the governor, with a p-value of less than .05, matching the hypothesis. In columns 3 and 8, I estimate the effect on non-discretionary funds, which represent funds that are allocated in such a way that it would be difficult to target them to a specific district. I find no evidence that there is an effect of a representative belonging to the same party as the governor on these funds. In columns 4 and 9, I take a subset of discretionary funds, nonformula education. Nonformula education transfers are not based on imputations or surveys and additionally compromise the largest portion of discretionary transfers. However, I do not find a statistically significant effect of party affiliation with the governor on these funds. Similarly, in columns 5 and 10, I estimate the effect of party affiliation on formula education funding, which is the largest component of non-discretionary transfers and is measured without imputation. I find a precisely estimated party affiliation effect

population constant across the decade to measure population precisely by decade, use annual county populations and assign populations based on geographic overlap of districts and counties, or utilize that the effect of equal population laws that require districts to have nearly equal populations and assign district population using annual state population data divided by the number of districts. The choice does not substantively change the results, and to keep consistent with the literature, I employ the last strategy, as is done in Aidt and Shvets (2012).

close to 0.

Table 3.2: Governor Party Affiliation on State Transfers

	(1)	(2)	(3)	(4)	(5)
Margin of Victory	-0.292	-0.840*	0.134	-0.798	0.358
	(0.438)	(0.449)	(0.451)	(0.526)	(0.406)
Margin x Affiliation	0.029	1.100*	-0.774	0.774	-1.197**
	(0.539)	(0.603)	(0.607)	(0.699)	(0.558)
Party Affiliation	0.011	0.023	0.001	0.021	-0.002
	(0.022)	(0.028)	(0.024)	(0.035)	(0.024)
State x Year FEs	No	No	No	No	No
District Controls	No	No	No	No	No
	(6)	(7)	(8)	(9)	(10)
Margin of Victory	-0.409***	-0.300*	-0.241*	-0.099	-0.170
	(0.128)	(0.156)	(0.137)	(0.122)	(0.128)
Margin x Affiliation	0.260	0.219	0.268	0.129	0.160
	(0.184)	(0.228)	(0.215)	(0.176)	(0.189)
Party Affiliation	0.021***	0.027**	0.009	0.014	0.007
	(0.007)	(0.012)	(0.008)	(0.009)	(0.008)
State x Year FEs	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes
Outcome	All	Disc.	Non Disc.	Nonformm.	Formula
Y Mean	1245.7	384.4	735.5	259.3	680.6
MSE Optimal Bandwidth	0.08	0.10	0.08	0.11	0.09
Number of Observations	21399	27974	23316	30445	25088

Notes: Table contains two sets of local linear regressions for governor party affiliation. Columns 1-5 show the regression discontinuity effects excluding district controls and state-year fixed effects. Columns 6-10 show the regression discontinuity effect including district controls and state year fixed effects. In columns 1 and 6 the outcome is all transfers; in columns 2 and 7 the outcome is discretionary transfers; in columns 3 and 8 the outcome is non-discretionary transfers; in columns 4 and 9 the outcome is non-formula education transfers; and in columns 5 and 10 the outcome is formula education transfers. The regression discontinuity estimator corresponds to Party Affiliation. Standard errors in parenthesis are clustered at the state-congress level.

The second set of results is for legislative chamber party affiliation, shown in table 3.3. The data allow for me to identify four different types of legislative matching: matching the lower house majority party, matching the upper house majority party, matching chamber served majority party, or matching the non-served chamber's majority party. In practice, both legislative houses tend to be led by the same majority party. In my sample this is true for 80% of the observations. I report estimates for matching the same chamber majority party [17].

The results in general are similar to the effects of party affiliation with the governor, but in all cases the magnitudes of the estimates are smaller. Notably, columns 2 and 7 show the effect of legislative party affiliation on discretionary transfers from the state. I find that transfers to

[17] I use the lower house majority party for lower house districts and the upper house majority party for upper house districts.

Table 3.3: Legislative Chamber Party Affiliation on State Transfers

	(1)	(2)	(3)	(4)	(5)
Margin of Victory	-0.173	-0.770*	-0.035	-0.510	0.188
	(0.297)	(0.430)	(0.479)	(0.384)	(0.482)
Margin x Affiliation	-0.029	1.257**	-0.673	0.892*	-1.005
	(0.370)	(0.572)	(0.648)	(0.508)	(0.663)
Party Affiliation	0.030	0.028	0.032	-0.001	0.029
	(0.020)	(0.028)	(0.026)	(0.032)	(0.026)
State x Year FEs	No	No	No	No	No
District Controls	No	No	No	No	No
	(6)	(7)	(8)	(9)	(10)
Margin of Victory	-0.131	-0.172	-0.099	-0.020	-0.020
	(0.093)	(0.151)	(0.156)	(0.086)	(0.161)
Margin x Affiliation	0.103	0.201	0.282	0.102	0.223
	(0.125)	(0.211)	(0.226)	(0.123)	(0.223)
Party Affiliation	0.013**	0.020*	0.002	0.009	-0.002
	(0.006)	(0.012)	(0.009)	(0.009)	(0.009)
State x Year FEs	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes
Outcome	All	Disc.	Non Disc.	Nonformm.	Formula
Y Mean	1249.4	387.7	736.4	262.4	681.8
MSE Optimal Bandwidth	0.10	0.11	0.08	0.14	0.08
Number of Observations	27826	28977	22238	38528	22381

Notes: Table contains two sets of local linear regressions for legislature party affiliation. Columns 1-5 show the regression discontinuity effects excluding district controls and state-year fixed effects. Columns 6-10 show the regression discontinuity effect including district controls and state year fixed effects. In columns 1 and 6 the outcome is all transfers; in columns 2 and 7 the outcome is discretionary transfers; in columns 3 and 8 the outcome is non-discretionary transfers; in columns 4 and 9 the outcome is non-formula education transfers; and in columns 5 and 10 the outcome is formula education transfers. The regression discontinuity estimator corresponds to Party Affiliation. Standard errors in parenthesis are clustered at the state-congress level.

affiliated districts increase by 2.0% with a p-value less than .1. The fact that there are greater rewards for matching the party of the governor is not surprising considering the amount of power the governor has in the distributive process. The governor's agenda-setting privileges coupled with the fact that the majority of states have balanced budget rules results in an environment in which, even when the governor faces a unified legislature of the opposite party, it may be difficult for the legislature to make substantive changes. Additionally, even if majority power legislators want to make changes, most governors have the ability to strike the items from the budget without the legislature's approval.

For both legislative and governor party affiliation, the differences in treatment effects for discretionary and non-discretionary funding provide evidence that the research design is valid. It would be concerning to find a treatment effect for non-discretionary funds for two reasons.

First, a large portion of non-discretionary funds are awarded by formula; therefore, finding a treatment effect would question the validity of the regression discontinuity. I do not find evidence of covariate imbalance across the threshold, but I cannot capture every possible demographic or political factor. Second, non-discretionary funds should represent a set of funds that are less manipulable and thus have a lower ability to be a source of targeted spending. I consistently find no evidence that there is an impact of party affiliation, either from the governor or legislature, on this set of funds.

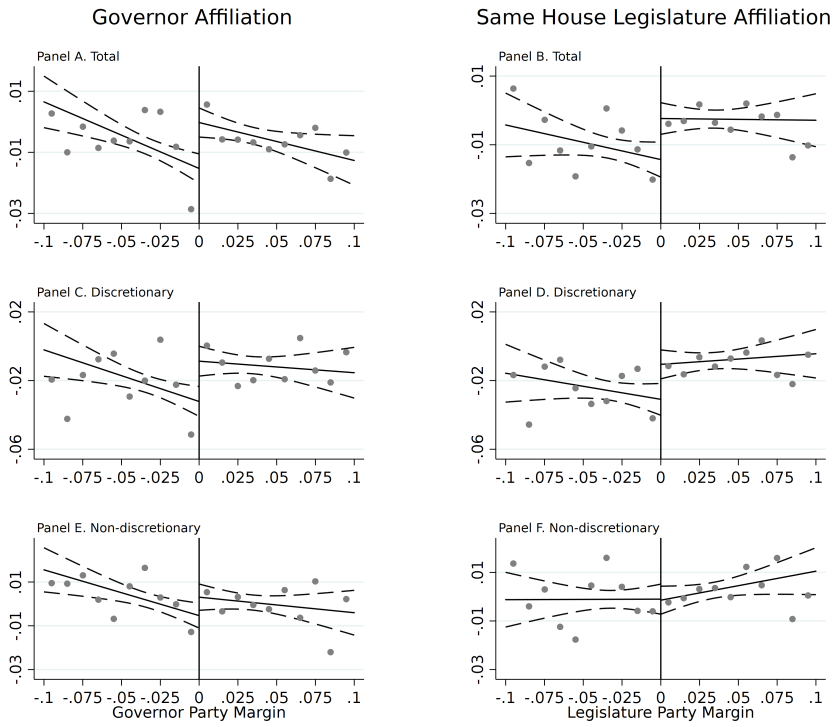
To visualize the results and further understand the party affiliation relationship beyond the discontinuity, I include regression discontinuity plots in figure 3.2. This figure plots residualized total, discretionary, and non-discretionary transfers for both governor and legislature party affiliation, with the log of transfers on the y-axis and the vote margin on the x-axis[18]. To provide a depiction of the raw data, I include binned elections such that there are 10 equally spaced bins on each side of the threshold, with each bin representing a 1% change in the vote margin. When the vote margin barely surpasses 0, there is a discontinuous change in treatment; representatives from districts in these states are aligned at the relevant margin. The regression-discontinuity estimator shown in tables 3.2 and 3.3 are akin to the vertical distance between the lines at 0. The figure provides visual evidence of the party affiliation effect and also provides some intuition for how persistent these effects may be beyond the most competitive elections. A line to the right of the cutoff with a negative slope suggests that the distributive gain from party affiliation may fall off in less competitive elections. However, the slope of the lines for discretionary funds are consistently flat. The slopes of the lines are notably different [19]. Legislature affiliation is associated with a

[18]I residualize the outcome using district level controls and state-year fixed effects.

[19]These correspond to the Margin of Victory and Margin x Affiliation coefficients in tables 3.2 and 3.3.

flatter slope and governor affiliation is associated with a negative slope. This is suggestive that governors are strategic, disproportionately targeting swing districts belonging to their party while legislators enjoy a more uniform effect regardless of how close the election was.

Figure 3.2: Main RD Figures



Notes: The figure plots the local linear regression discontinuity graphs of all, discretionary, and non-discretionary transfers. Results for governor party affiliation are shown on the left and results for same house legislature party affiliation are shown on the right. In all graphs the outcome is residualized on the full set of district level controls and state-year fixed effects. In each panel, points to the left of the vertical line are districts where the representative from the affiliated political party marginally lost and points to the right of the vertical line consist of districts where the representative from the affiliated party marginally won. Estimates are based on local linear regressions using a triangular kernel and a fixed bandwidth of .1. For observation, data are averaged in equally spaced bins, which is each point represents 1% of vote margin, for a total of 20 points in each panel. The 95 percent confidence intervals are plotted by the dashed line.

These results are notably different to estimates based on federal grant funding to states. Albouy (2013) estimates the effect of a state having the majority of its congressional representatives in the majority party of the House on different types of government grants. For total government grants, he finds a positive effect of between 3.6-4.5% when a state's House member majority matches the party of the president, and statistically insignificant results for matching the majority

party of the House or Senate. Berry, Burden, and Howell (2010) find that a congressional representative's party matching the political party of president leads to between a 4.0-4.6% increase in federal funding in that district. They find mixed results for the importance of congressional party affiliation, with effects ranging from 1.1-5.1%. In comparison, I find that state politicians parties matching the party of the governor leads to an increase of 2.7% of state funds, and when the state politician matches the legislative majority, there is a 2.0% increase of state funds. Notably, the results I find are smaller than in the federal literature. This is particularly interesting given the fact that Berry, Burden, and Howell (2010) additionally find that transfers are also larger in close election districts, which are the only elections my study utilizes.

In fact, one argument for studying how state governments transfer funds to local governments is that there is a possibility that state governments could attenuate treatment effects when state politicians do not match the president, for instance. There are at least two reasons for the smaller affiliation effects at the state government level. First, prior studies have relied on the two-way fixed effect estimator. A growing body of econometric literature (Abraham and Sun [2018], de Chaisemartin and D'Haultfoeuille [2019], and Goodman-Bacon [2018]) finds that the standard two-way fixed effects estimators do not properly aggregate an average of cohort-specific treatment effects. If treatment effects grow overtime, then the two-way fixed effects estimator likely produces an upward-biased result. Second, there are some structural differences between how the state and federal government distribute funds. Given that the vast majority of states must adhere to some degree of a balanced budget, it is likely that this provision substantially decreases the amount of funds that can be distributed. In appendix A.1, I provide a brief exploratory exercise, separating out states by various measures of balanced budget provisions. When I restrict analysis to states with no constitutional or statutory citations for the legislature passing a balanced

budget, I find larger party affiliation estimates that are more consistent with the estimates from the federal literature, between 3.4-7.2% [20]. I take these sub-sample regressions as suggestive evidence that the reason that estimates of party affiliation are lower at the state level are due to limitations regarding how states are able to spend funds.

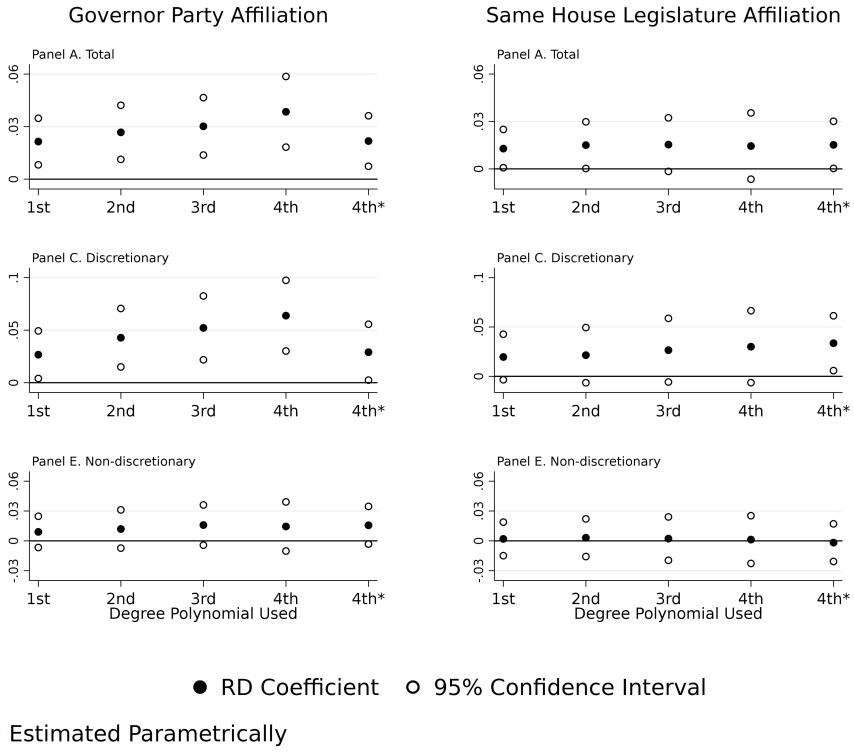
3.6 Robustness

I perform several robustness checks. First, I test how sensitive party affiliation results are to the degree of local polynomial used. In figure 3.3, I plot the regression discontinuity coefficient from the model with state x year fixed effects and legislative district controls included with a solid black dot and plot the 95% confidence interval of this estimator with open circles on the y-axis. Along the x-axis, I plot the degree polynomial is used for the regression discontinuity. I show how the estimator changes nonparametrically using 1st, 2nd, 3rd, and 4th degree polynomials. Finally, the last observation shows results of a parametric model with a 4th degree polynomial. Panels A, C, and E show results for governor party affiliation as the treatment, and panels B, D, and F show results using legislature party affiliation. For governor party affiliation, I find that the treatment effect grows slightly in magnitude with higher order polynomials. However, the results are in general similar to the baseline result using a 1st degree polynomial. For legislature affiliation, the degree of polynomial used has very little influence on the magnitude of the estimate. Finally, when I estimate the model with a global approach using a 4th degree polynomial, I find results that are in line with the baseline results using local linear regressions.

Next, I perform falsification tests using placebo cutoffs. Figure 3.4 plots the regression

[20] This set of states includes Hawaii, Indiana, Missouri, New Hampshire, New York, Pennsylvania, Vermont, Virginia, Washington, and Wyoming.

Figure 3.3: Specification Robustness

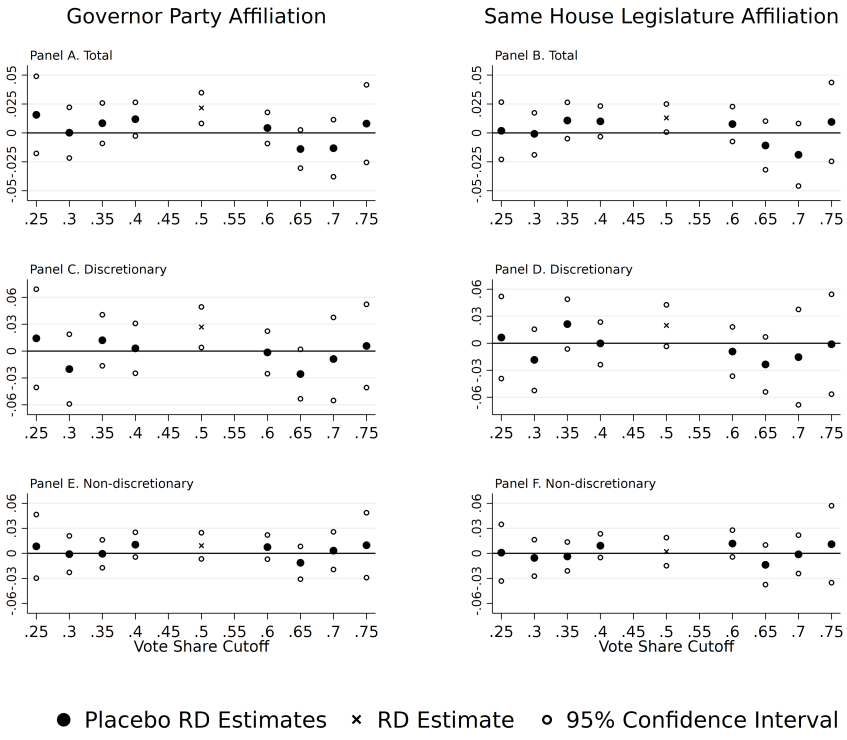


Notes: The figure plots various regression discontinuity estimators using different functional forms. Results for governor party affiliation are shown on the left and results for same house legislature party affiliation are shown on the right. Each panel contains estimates using a 1^{st}, 2^{nd}, 3^{rd}, and 4^{th} degree polynomial. Each specification uses a triangular weighted kernel and the mean square error optimal bandwidth. The furthest right point is estimated globally with a 4^{th} degree polynomial. The filled in circle represents the estimated impact of party affiliation and the open circles show the 95% confidence interval of the estimate. All regression use the full set of district controls and state-year fixed effects. Standard errors are clustered at the state-congress level.

discontinuity estimate with solid black circles and the 95% confidence interval of the estimate with open circles on the y-axis. I plot the various cutoffs used along the x-axis. For ease of interpretation, I rescaled the x-axis so that the regression discontinuity cutoff is 0.5, the point at which a legislator switches from being non-affiliated to affiliated. Points to the left of 0.5 represent a placebo regression discontinuity design, which utilizes elections in non-affiliated districts. Points to the right of 0.5 represent a placebo regression discontinuity design using affiliated districts. There is some evidence that as the placebo cutoff approaches 0.5 from the left, the estimator increases in magnitude. This suggests that it is possible that funds are distributed differently to competitive versus non-competitive minority districts, where, intuitively, barely lost

minority districts could receive more funds than uncompetitive ones, though such a relationship is only speculative and not statistically significant.

Figure 3.4: Cutoff Falsification Tests

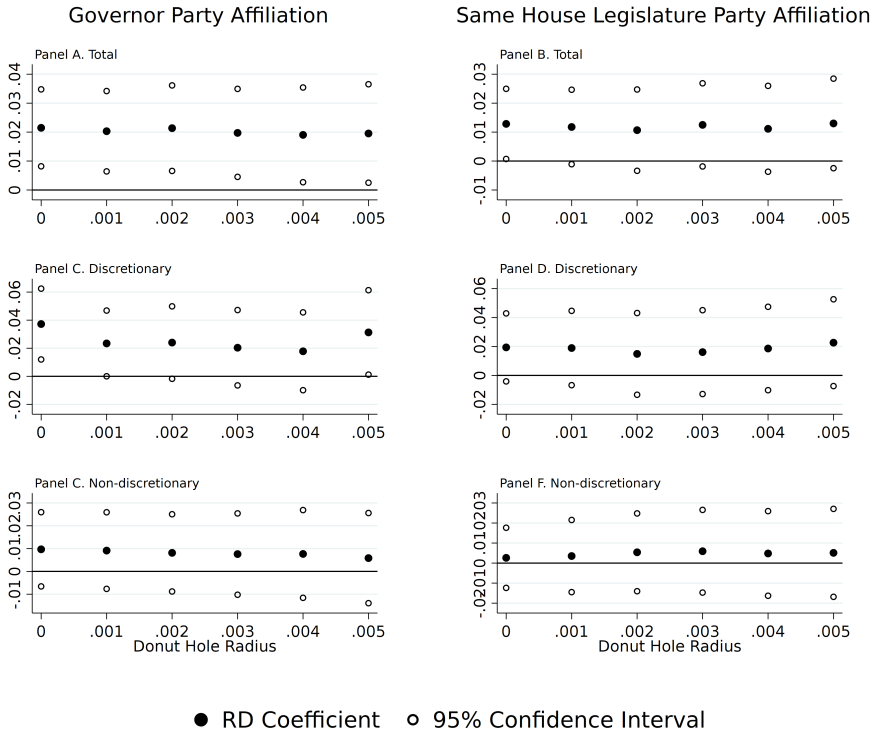


Notes: The figure plots various regression discontinuity estimators using different functional forms. Results for governor party affiliation are shown on the left and results for same house legislature party affiliation are shown on the right. In panels A and B the outcome is total transfers, C and D the outcome is discretionary transfers and E and F the outcome is non-discretionary transfers. Each panel contains estimates using a 1^{st}, 2^{nd}, 3^{rd}, and 4^{th} degree polynomial. Each specification uses a triangular weighted kernel and the mean square error optimal bandwidth. The furthest right point is estimated globally with a 4^{th} degree polynomial. The filled in circle represents the estimated impact of party affiliation and the open circles show the 95% confidence interval of the estimate. All regression use the full set of district controls and state-year fixed effects. Standard errors are clustered at the state-congress level.

The semiparametric regression discontinuity relies on a kernel weight such that observations near the cutoff are weighted more heavily than observations further from the cutoff. In figure 3.5, I test how robust the results are to the inclusion or exclusion of observations that are closest to the threshold using a donut-hole analysis. This procedure is a useful exercise if there is possible manipulation of treatment status, since it is natural to assume that observations nearest the cutoff are the most likely to have been manipulated. In the figure, I plot the regression discontinuity

estimator on the y-axis in black circles with the 95% confidence interval in open circles, and I plot the donut-hole radius used along the x-axis. The donut-hole radius is the size of the observations near the cutoff that I exclude from the model. I show results excluding election that were won or lost by .1% to .5%, which constitute the most competitive elections. Despite the seemingly small size of the radius, these donut-hole radii exclude between 264 and 1415 elections for the smallest and largest radius' chosen, respectively. I find that the model is generally robust to the inclusion or exclusion of these points.

Figure 3.5: Donut Hole Analysis

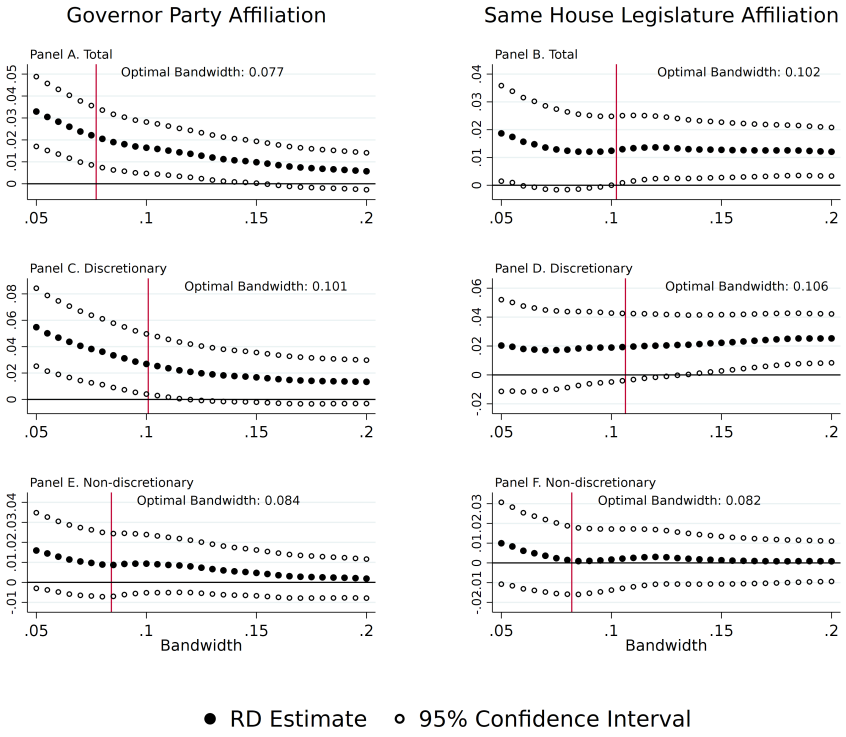


Notes: The figure plots regression discontinuity estimators using excluding points near the cutoff. Results for governor party affiliation are shown on the left and results for same house legislature party affiliation are shown on the right. In panels A and B the outcome is total transfers, C and D the outcome is discretionary transfers and E and F the outcome is non-discretionary transfers. Baseline estimates are shown when the x-axis is 0. Moving to the right increases the size of the radius of excluded observations. At it's maximum value of .005 1,436 observations are excluded, at it's minimum value of .001 266 observations are excluded. Filled in circle represents the estimated impact of party affiliation and the open circles show the 95% confidence interval of the estimate. All regression use the full set of district controls and state-year fixed effects. Standard errors are clustered at the state-congress level.

I then test how the regression discontinuity changes using various bandwidths. In figure

3.6, I plot the regression discontinuity estimator with 95% confidence intervals on the y-axis and the bandwidth used on the x-axis. I estimate regression discontinuities using bandwidths as small as 5% and as large as 20%. Additionally, I plot the location of the bandwidth used in tables 3.2 and 3.3. In panels A, C, and E, I show tests using governor party affiliation. In these panels. I find that there is a consistently positive effect on total and discretionary transfers, and a near 0 result on non-discretionary transfers. I find that as the bandwidth shrinks, the magnitudes of the regression discontinuity estimator increase. This suggests possible differential effects for highly competitive elections that are affiliated with the governor's party. In panels B, D, and F, I show tests using legislature party affiliation. The regression discontinuity estimator does not change substantially for total and discretionary transfers and there is a consistent null result on non-discretionary transfers.

Figure 3.6: Multiple Bandwidth Testing



Notes: The figure plots regression discontinuity estimators using various bandwidths. Results for governor party affiliation are shown on the left and results for same house legislature party affiliation are shown on the right. In panels A and B the outcome is total transfers, C and D the outcome is discretionary transfers and E and F the outcome is non-discretionary transfers. Moving to the right increases the size of the bandwidth used. The smallest bandwidth used is .05 and the largest is .2, bandwidths are iteratively increased by .005. A vertical line depicts the mean square error optimal bandwidth. Filled in circle represents the estimated impact of party affiliation and the open circles show the 95% confidence interval of the estimate. All regression use the full set of district controls and state-year fixed effects. Standard errors are clustered at the state-congress level.

3.7 Unified and Divided Government

These results ignore an important underlying institutional factor: the degree of government unification in the state. There are strong reasons to believe that depending on the environment of the state government, the effects of matching the legislature's or governor's party could be different. First, the incentives for distributing may be different depending on the state government's unification status. Different parties have different policy preferences, and when states are divided, the representing parties must balance their preferences for policy and pork. Put in simple terms, in divided governments, there are two effects driving how pork expenditures: a positive effect

from the majority party working to bring pork home and a negative effect from the majority party trading pork for policy support. When governments are fully unified and if the party is cohesive, there is limited need to buy policy support using pork. In addition to different motivations for the use of pork, the mechanisms are also affected by the state government environment. When states are divided, there is likely more conflict involved when deciding how to distribute resources.

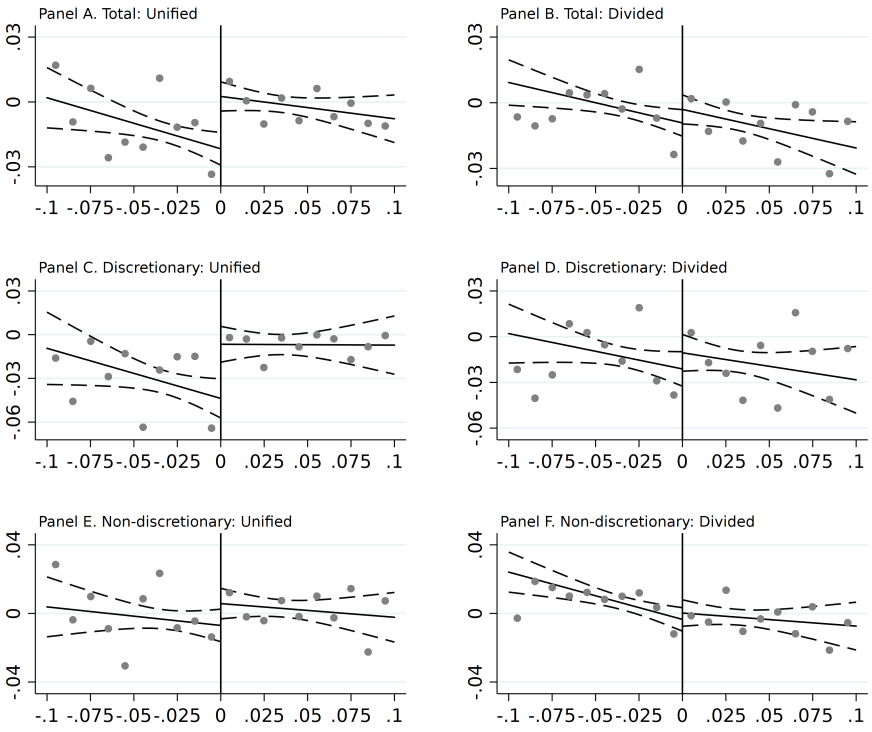
There are primarily two types of environments: those in which the governor is aligned with a unified legislature, and those in which the governor is not[21]. Figure 3.7 shows regression discontinuity graphs for governor party affiliation comparing how the affiliation effect changes in different samples. In panels A, C, and E, I restrict the sample to include only state-year pairs when the state government is fully unified, and in panels B, D, and F, I restrict the sample to include only state-year pairs when the state government was divided. In each panel, points to the left of 0 represent elections in which the district representative from the governor's party barely lost.

Among minority districts, the slope of the discretionary funding line is steeper in unified states, suggesting that competitive minority districts in unified states receive less funding than competitive minority districts in divided states. The right side of each panel contains districts in which the representative from the governor's party barely won. In these districts, the opposite holds true: districts in unified states consistently receive more than districts in divided states. This preliminary exercise is useful because changes in point estimates on majority affiliation could result from either the affiliated district receiving more or the non-affiliated district receiving less funding. The figure provides evidence that both occur. The estimates from the previous sections

[21] In total, there are 8 possible combinations of party affiliation configurations between the governor and the lower and upper house. In my sample of 855 state-year pairs, there are 451 instances of fully unified state governments and 235 instances of unified legislatures with the governor in an opposing party.

disregard the unification status and can be thought of as a weighted average of this data. In the following section, I further explore how different degrees of political unification can lead to differential effects of party matching.

Figure 3.7: Unification Regression Discontinuity Figures



Notes: The figure plots the local linear regression discontinuity graphs. Results for governor party affiliation in divided government state-years are shown on the left and results for governor party affiliation in unified government state-years are shown on the right. In panels A and B the outcome is total transfers, C and D the outcome is discretionary transfers and E and F the outcome is non-discretionary transfers. In all graphs the outcome is residualized on the full set of district level controls and state-year fixed effects. In each panel, points to the left of the vertical line are districts where the representative from the affiliated political party marginally lost and points to the right of the vertical line consist of districts where the representative from the affiliated party marginally won. Estimates are based on local linear regressions using a triangular kernel and a fixed bandwidth of .1. For observation, data are averaged in equally spaced bins, which is each point represents 1% of vote margin, for a total of 20 points in each panel. The 95 percent confidence intervals are plotted by the dashed line.

To explore heterogeneity of party affiliation with government unification, I conduct sub-sample analysis. I break the data into 3 sub-samples: unified governments, defined as when the lower and upper house majority parties are aligned with the governor's; unified legislatures with divided governors, defined as when the lower and upper house majority parties are aligned and the governor is from a different party; and any divided government, defined as any non-unified-

controlled state government. I then perform the regression discontinuity analysis for discretionary and nondiscretionary transfers from the state including covariates and fixed effects. I summarize the results in figure 3.8.

In figure 3.8, each plotted point corresponds to a regression discontinuity estimator from a different sub-sample. The sub-sample type is differentiated by the shape of the marker and the party affiliation margin is differentiated by the color of the marker. I plot the p-value of the estimator on the y-axis, and the party affiliation effect size on the x-axis. Circles correspond to the baseline estimates using all the data, squares indicate that the sub-sample is any type of divided government, triangles indicate a unified legislature with a divided governor (a specific subset of divided government), and X represents a unified government. Symbols in black correspond to governor party affiliation and symbols in gray correspond to legislature party affiliation[22].

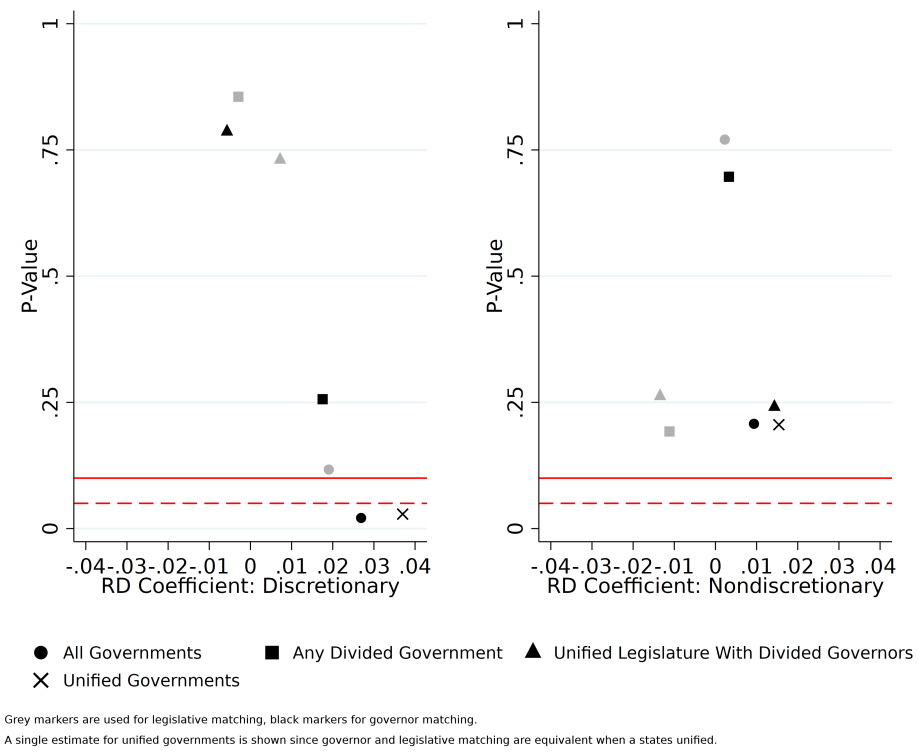
In panel A, I show results on discretionary transfers. For discretionary transfers, I find that the largest party affiliation effects occur when the state is fully unified. Compared to the baseline estimate of governor party affiliation, 2.7%, and legislature party affiliation, 2.0%, I find that the effect of party affiliation in a unified state is 3.7%. For the remaining samples, which consist of different types of divided governments, I find smaller, less precisely estimated coefficients[23]. In all cases, the divided government point estimates are lower than the baseline and are not statistically significant at conventional levels. In panel B, I show results on non-discretionary funding. I consistently find in all sub-samples coefficients between -1.5% and 1.7%, none of which are statistically significant at conventional levels.

These results show that the underlying government environment is potentially an important

[22]There is only 1 observation for a unified government because legislature and governor affiliation are equivalent when the government is unified.

[23]The gray and black triangles in principal should be symmetric about 0 since a district either matches with the legislator or the governor. However, they differ slightly due to small differences in the bandwidth choice.

Figure 3.8: Heterogenous Treatment Effects



Notes: The figure plots regression discontinuity estimators using various subsamples based on the unification status of a state government. Panel A plots the estimates for discretionary funds and panel B plots the estimates for non-discretionary funds. The degree of government unification is indicated by the shape of the symbol. A circle corresponds to the baseline estimate using all state years, a square represents state-years when a state government is divided, a triangle represents state-years when the legislature majority is the same in both chambers and the governor is from a different party, and the X represents state-years when a state government is fully unified state. The color of the shape denotes the party affiliation margin, where black shapes correspond to governor party affiliation and grey shapes correspond to legislature party affiliation. The regression discontinuity estimate is plotted on the x-axis and the pvalue on the y axis. Two horizontal lines are plotted, a solid line when the pvalue corresponds to .1 and a dashed line when the pvalue corresponds to .05. All regression use the full set of district controls and state-year fixed effects, mean square error optimal bandwidth, and triangular kernel. Standard errors are clustered at the state-congress level.

part of the distributive puzzle. I find that regression discontinuity estimates on discretionary funds are largest when the state is fully unified and that when the government is divided the party affiliation effect is near zero. There are at least two possible explanations for these results. First, in a fully unified state, it is less likely that the majority party would seek the support of minority party members to pass policy. This eliminates the need to trade pork for policy and results in majority districts receiving more funds. Second, it is possible that targeting funds is difficult without the support of the lower house, upper house, and governor. Typically, a budget or appropriations bill must be approved by each of these institutions and divided governments

may act as a blockade when targeting funds to specific districts. The above results cannot differentiate between these explanations but suggest that the degree of government unity is an important component to the distribution story.

3.8 Conclusion

This paper identifies that political party affiliation plays an important role in the allocation of funds. I find that the unification status of the state government is a critical factor that differentiates how majority representation impacts the distribution of funds. When the state government is divided, I find that political alignment with either the legislature or governor leads to no statistically different amount of funding. However, when the state government is fully unified under a single political party the political affiliation effect is large and statistically significant.

By utilizing variation from state governments, I am able to provide two key contributions to the larger literature on distributive politics. First, because there are 50 states and new representatives are elected every two years, there are more than 400 legislative sessions in my relatively short period. In comparison, work focusing on federal funding to state or local governments include as few as 11 legislative sessions. More legislative sessions are akin to more experiments. In this case, utilizing more experiments allows me to disentangle heterogeneous effects of divided and unified governments and cluster stand-errors at the legislative level. I find that there is a large difference in the degree of targeting when states are unified compared to when they are divided. Second, the state level is free of attribution problems that arise from examining funding given by the federal government to local governments when the state acts as an intermediary, because there are less intermediaries between the state and local governments

Unlike much of the federal literature, I utilize a regression discontinuity design to obtain causal estimates on party affiliation. The local linear regression discontinuity design relies on data in a small bandwidth around a threshold, resulting in the estimating of the impact of party affiliation on only a subset of competitive elections. It is possible that funding decisions for districts with competitive elections are systematically different than those in non-competitive districts. In the conservative case, my results should be interpreted as the causal impact of party affiliation in close elections, which I argue are the elections we are most interested in when it comes to understanding whether pork is a possible lever that can be used to sway elections.

In the future, more work should be done to understand what effect this has on election outcomes. Are funds distributed strategically such that they significantly alter the winner of a district? If so, is targeted spending a significant motivation for some states to maintain political unification? Additionally, it would be of interest to fully understand how state budget laws impact the magnitude of the party affiliation effect. Do balanced budget requirements limit the amount of targeted spending, or does it simply limit spending overall? Questions such as these begin to answer questions about the ways in which distributive politics impact the fairness of democratic elections.

3.9 Appendix

The relative magnitude of my results compared to the federal literature warrants further exploration. One concern of studying distributive politics at the federal level is that the state governments can act as intermediaries. If federal representatives work to distribute money to their districts, but that district is not aligned with the governor, it is reasonable to expect that this diminishes the effect of federal targeting. If this were the only difference between the federal and state distributive environments, we would then expect that estimates from state governments would be larger. Instead, I find the effects of party affiliation in state governments around to be two-thirds the size of their federal counterparts. To explain this difference, I use a regression discontinuity framework on different samples of data.

The institutional difference I explore is how various budgetary settings may lead to differential effects of party affiliation. Previously discussed, most states have a variation of a balanced budget requirement, which focus on balancing the state operating budget. This includes a state's general fund, which is the primary fund from which appropriations are made within states. The strictness of these provisions can vary by state; some examples are differences in the allowance for carrying over unavoidable deficits, enforcement mechanisms, and the time within the legislative process when the budget must be balanced. In some states, only the governor's proposed budget must be balanced, and in others, the enacted budget must be balanced. In 2008, the National Association of State Budget Officers (NASBO) conducted a survey of state balanced budget requirements. In this survey, they distinguished between states with constitutional or statutory requirements that require the governor to submit a balanced budget, the legislature must pass a balanced budget, and whether the state is permitted to carry over a deficit. The strictest measure is a constitutional

or statutory requirement for the legislature to pass a balanced budget. From the 2008 survey, only 10 states did not have such a requirement. Additionally, in 1984, the Advisory Commission on Intergovernmental Relations (ACIR) rated each state's balanced budget requirement on a scale of 1 to 10. States with a score of 10 had the strictest budgeting requirements which prohibit carrying over a deficit and require the governor to propose a balanced budget and the legislature to pass one. According to this definition, 26 states have the strictest budgeting requirements, and 3 more states were given a score of 9.

In table 3.9, I construct subsamples of states based on these two definitions and show regression discontinuity results on both total and discretionary transfers. I use the 2008 NASBO definitions and include states with a requirement that the legislature pass a balanced budget in column 1 and states without such a requirement in column 2. I find that there is a small statistically significant party affiliation effect in states with a balanced budget requirement of 1.2% on all transfers and 2.6% on discretionary transfers. In column 2, I find that the party affiliation effect is much larger in states where the legislature is not required to pass a balanced budget, 4.8% for all transfers and 7.2% for discretionary transfers. I use the 1984 ACIR index and include states with a budget stringency score of 9 and 10 in column 3 and those with score of 0 to 8 in column 4. For states with the strictest budget requirements, I do not find a party affiliation effect that is statistically different from 0 for either all transfers or discretionary transfers. For states with more relaxed budget requirements, I find that there is a party affiliation effect of 3.4% for all transfers and 5.2% for discretionary transfers. Using either set of definitions, I find that the party affiliation effect is largest when balanced budget provisions are less strict.

Table A.1: Party Affiliation and Balanced Budgets

	(1)	(2)	(3)	(4)
	Outcome: All Funding			
Margin of Victory	-0.417***	-0.394	-0.249*	-0.549***
	(0.137)	(0.245)	(0.143)	(0.195)
Margin x Affiliation	0.488**	-0.251	0.203	0.437
	(0.190)	(0.394)	(0.199)	(0.308)
Party Affiliation	0.012*	0.048***	0.008	0.034***
	(0.007)	(0.017)	(0.008)	(0.012)
	Outcome: Discretionary Funding			
Margin of Victory	-0.618**	-0.770*	-0.429	-0.668**
	(0.245)	(0.414)	(0.279)	(0.312)
Margin x Affiliation	0.763**	-0.053	0.430	0.567
	(0.350)	(0.729)	(0.398)	(0.510)
Party Affiliation	0.026*	0.072***	0.017	0.052***
	(0.015)	(0.026)	(0.017)	(0.019)
Sample	Balanced	No Balanced	Index: 9-10	Index: 0-8
State x Year FEs	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes
Bandwidth	0.08	0.08	0.08	0.08
Number of Observations	17930	4189	13311	8808

Notes: Table contains two panels of local linear regressions for governor party affiliation conditioning on different definitions of balanced budget stringency. In the top panel, the outcome is the natural log of total funding per-capita and in the bottom panel the outcome is the natural log of discretionary funding per-capita. Columns 1 and 2 use the 2008 National Association of State Budget Officers survey and columns 3 and 4 use the 1984 Advisory Commission on Intergovernmental Relations (ACIR) balanced budget index. In column 1, the sample contains only states with a requirement that the legislature passes a balanced budget. In column 2, the sample contains only states that have no requirement that the legislature pass a balanced budget. In column 3, the sample contains states with the strictest balanced budget rules according to the ACIR. In column 4, the sample contains states with more lenient balanced budget requirements. All regressions use state x year fixed effects, district controls, and a fixed bandwidth of .08. The regression discontinuity estimator corresponds to Party Affiliation. Standard errors in parenthesis are clustered at the state-congress level.

Chapter 4: Political Control Over Redistricting and the Partisan Balance in Congress

4.1 Introduction

”Representatives and direct Taxes shall be apportioned among the several States which may be included within this Union, according to their respective Numbers, which shall be determined by adding to the whole Number of free Persons, including those bound to Service for a Term of Years, and excluding Indians not taxed, three fifths of all other Persons. The actual Enumeration shall be made within three Years after the first Meeting of the Congress of the United States, and within every subsequent Term of ten Years, in such Manner as they shall by Law direct.” - Article 1 Section 2, U.S. Constitution

In majoritarian single member-district political systems, representatives are elected in geographical districts. As population imbalances across districts accrue increasingly over time, new district boundaries need to be drawn. In many countries such as Australia, Canada, Mexico and the United Kingdom, maps of political districts are drawn by non-partisan, independent bodies. In the United States, the drawing of district boundaries is delegated to partisan actors: namely state legislators. Article 1 Section 2 of the U.S. constitution requires the federal government to undertake a census of the population and use it as a basis for reapportioning the numbers of

districts across states. State legislators then are responsible for drawing district boundaries within states. This process, unfortunately, allows politicians to redraw political boundaries in order to affect partisan control over both federal and state legislatures. This paper estimates the impact of partisan control over the redistricting process on partisan seat shares.

Partisan interest in redistricting became apparent as early as 1812, when the Massachusetts State Senate redrew electoral boundaries, as mandated by the U.S. constitution. The redrawn districts benefited the Democratic-Republican party over the opposition Federalist party. Governor Elbridge Gerry of Massachusetts, a Democratic-Republican, signed the redistricting bill into law though he personally lamented the highly partisan process. The redistricting resulted in some oddly shaped districts. One Federalist newspaper, the Boston Gazette, noted that one of the state senate districts looked like a salamander. The Gazette coined the term "Gerry-mander" in a political cartoon from March, 1812.

In recent years, there has been increased concern over whether or not redistricting leads to gerrymandering: i.e. whether or not parties redistrict in order to increase their share of legislative seats. A body of theoretical work shows that self-interested political parties will redistrict by *cracking* opposition districts with a narrow majority and *packing* the opposition into lop-sided districts in an attempt to increase own-party seat share, Shotts (2001); Gilligan and Matsusaka (1999). More recent work points out that packing is beneficial but when a party is uncertain of partisan leanings and voter turnout, it is usually not optimal to crack, Friedman and Holden (2008)

There also has been an empirical literature on the impact of redistricting. Much of the best empirical work to date simulates counterfactual maps subject to legal or norms-based constraints such as requirements that the district be connected and that it be compact, Chen, Rodden et

al (2013); Chen and Rodden (2015); Stephanopoulous and McGhee (2015). This literature then computes probabilities of an outcome at least as partisan as the actual outcome. Though this work tends to find that in many states few alternative ways of drawing districts yield greater imbalance in the relationship between voting behavior and representation, it is nonetheless possible that these districts which confer partisan advantage to one party or another reflect natural geographical boundaries or reflect natural political communities, Rodden (2019). A recent book, A recent book, McGann et al (2016), shows that there is greater skewness in the vote shares of Democratic as opposed to Republican districts which they interpret as evidence of partisan redistricting in favor, on net, of the Republicans. Some of this work suggests that partisan advantage due to asymmetries in clustering across districts is due to the clustering of like-minded individuals, Chen, Rodden et al (2013) rather than the intentional design of parties. A large literature has noted increased political sorting over time, Bishop (2009); Hopkins (2017); Kaplan and Sullivan (2018). Nonetheless, a recent paper Jeong and Shenoy (2016) shows that parties engineer narrow state government victories in the election before redistricting.

Instead of looking at maps relative to counterfactual potential maps, we look at whether federal seat shares increase in the direction of a party when that party has legal control over the redistricting process. To do so, in contrast with most of the prior literature, we provide comprehensive evidence on the prevalence of partisan gerrymandering over 50 years of American history, across parties, and heterogeneously by the number of seats in a state. Stephanopoulos and McGhee (2015) introduces a measure of wasted votes called the efficiency gap and, in a follow-up paper Stephanopoulos (2017) estimates the impact of unified control on the efficiency gap using a two-way fixed effects model. We carefully code legal control over redistricting which differs frequently from unified control. Second, we estimate the impact on federal seat shares,

which is what is of interest, as opposed to vote shares. We also use two estimation strategies which are immune to aggregation problems of heterogeneous

We first develop a measure of the amount of redistricting as the fraction of a state which changes districts. We show empirically that almost all redistricting happens once a decade by legislatures in power in years that end in 1. We then estimate the impact of the ability of one party to pass a redistricting bill without votes from the opposition party on that party's fraction of seats in Congress in subsequent elections.

We estimate our effects in two different ways and get very similar estimates. First, controlling for year effects as well as state X decade effects, we find a statistically significant positive impact of 4.7 percentage points of Republican legal control on the Republican seat share in Congress in the subsequent election. The effect is 9.1 percentage points restricted to the past two decades. The average effect over the next three federal elections is positive but not statistically significant for the full five decade sample but increases in size to 8.2 percentage points and becomes statistically significant at conventional levels when we restrict our data to the past two decades. In contrast, we do not find statistically significant effects or sizes as large with Democratic control except for large state delegations restricted to the past two decades. Our estimates for the impact of Republican control over redistricting are relatively stable regardless of state delegation size. However, we do find statistically significant increases in the Democratic seat share following Democratic control over redistricting only when we restrict to the past two decades and only in states with more than five seats. Second, we compare state-decades with unified government versus those with a unified legislature and a governor of a different party due to small differences in the gubernatorial vote share. When using this source of variation, we restrict our sample to the large majority of states where both chambers of the state legislature and the signature of the

governor are required for a redistricting bill, We find remarkably similar results.

In a second component of the paper, we investigate why

Overall, simple back of the envelope calculations suggest that partisan redistricting can on net account for less than 5% of the gap between Republicans and Democrats in the House during each of the 1970s, 1980s and 1990s. However, the same calculations show that it can account for 57% of the gap in the 2000s and 51% in the 2010s.

In the next section, we discuss important institutional features of the U.S. redistricting process. In section 3, we discuss our empirical methods. In section 4, we give an overview of the data we use for our estimation. In section 5, we present our main results. In section 6, we perform an exercise in which we compute aggregate impacts of the rights to redistrict upon the partisan balance in Congress. Finally, in section 7, we conclude.

4.2 Institutional Background

The use of district boundary creation to influence elections goes back to the period before the Constitution when the Articles of Confederation was law. Patrick Henry, along with other anti-Federalists, purportedly altered Virginia's 5^{th} Congressional District in an attempt to prevent the strong Federalist, James Madison, from returning to Congress, Labunsk(2006). In 1789 (the following year), the states ratified decadal redistricting. Since the adoption of the Constitution, redistricting has happened within the first three years of the decade in almost all states in every decade with the exception of the 1920s.[1]

The process of redrawing districts happens in two phases. In the first phase, reapportionment,

[1]In the 1920s, reapportionment would have led to a shift of 11 seats away from rural areas towards urban areas which had grown in size dramatically due to migration. A coalition of representatives from rural areas made sure that the reapportionment was blocked until 1929, Anderson (2015).

the U.S. Congress uses the Census data and by January 25^{th} of the year following the completion of the Census, assigns numbers of seats in the House of Representatives to each state. Though there are multiple possible methods to apportion seats, Congress uses the Hamilton-Hill method which minimizes deviations in number of representatives per person across states. Though the first Congress had 105 members and an average of approximately 33,000 individuals per representative, the size of Congress grew over time until it was capped in 1911 at 435 [2]. This cap was reauthorized in 1929 and has been in place continuously since then except for a temporary increase in 1959 when Alaska and Hawaii joined the United States and the number of representatives rose temporarily to 437.

After reapportionment, the states are notified by Congress of the number of representatives that they are apportioned. The federal government historically has given individual states wide latitude to redistrict as they see fit. Though reapportionment results in relative balance across states in representation in the House of Representatives, individual states had created districts with a high degree of population imbalance. For example, one state house district in Tennessee represented 2,340 people and another in the same state represented 42,298 people. The worst example of representational imbalance was in the Nevada state legislature where one district contained 568 voters and another approximately 127,000. In the early 1960s, the Warren court handed down three rulings. First, in 1962, *Baker v. Carr* established that redistricting was subject to judicial review. Then, in 1964, *Wesberry v. Sanders* mandated equal population in federal Congressional districts. *Reynolds v. Sims* also in 1964, then extended equal representation to state legislative districts. In subsequent decisions (*Karcher v. Daggett*, 1983; *Vieth v. Jubelirer*, 2003), the Supreme Court clarified that Congressional Districts should be exactly equal in size to

[2]This followed the addition of the states of Arizona and New Mexico to the United Statesin 1912.

the degree possible whereas for state legislative districts deviations of up to 10% across districts would be allowed (*Brown v. Thomson*, 1983), Ansolabehere and Snyder (2008).

In addition, the Supreme Court has also decided that as long as districts are sufficiently compact, redistricting in order to create majority minority Congressional districts is legal but other racially-based reasons are not (Thornburg v. Gingles, 1986; Shawn v. Reno, 1993; Miller v. Johnson, 1995). However, the courts have been more reluctant to disallow redistricting for partisan gain (Davis v. Bandemer, 1986; Vieth v. Jubelirer, 2004). In Vieth v. Jubelirer (2004), Supreme Court Justice Anthony Kennedy made the argument that in principle the Supreme Court could intervene to prevent partisan gerrymandering but that there was no empirical metric with which the Supreme Court could intervene. He stated, "That no such standard has emerged in this case should not be taken to prove that none will emerge in the future." In 2019, the Supreme Court decided in Rucho v. Common Cause that the Supreme Court did not have the authority to intervene in order to limit partisan redistricting. However, court battles are ongoing at the state level.

States differ in their redistricting laws and processes. Seven states did not redistrict federal Congressional boundaries through most of the late 1900s and early 2000s because they only have one federal representative: Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont and Wyoming. We drop these states from our main analysis as they do not participate in redistricting. Moreover, since these districts have 100% partisan seat shares unless they elect an independent, including these high variance observations decreases our precision.

We also drop Nebraska because since 1934, Nebraska has not allowed political parties to operate at the state level. Thus, it is difficult to tell whether or not Democrats or Republicans have control over legislative bodies and thus whether one party has legal control over the redistricting

process[3].

Over the fifty years which our data set encompasses, 12 states have used a commission to draw maps and implement redistricting in at least some decades. The composition of the redistricting commissions varies by state but many attempt to appoint a bipartisan commission by balancing the number of Democratic appointees and Republican appointees to the commission (Arizona, California, Hawaii, Idaho, Maine, Montana, New Jersey and Washington). [4] Three other states (New York, Ohio, and Rhode Island) use commissions that do not have partisan balance. In Iowa, non-partisan staff draw maps every decade and the maps are then sent for approval to the legislature and the governor. Commissions vary from state to state in the extent of their redistricting powers and restrictions. We treat all state-decades with a commission as being of a singular "commission" type. We include these states in our benchmark specification but also show robustness to dropping them as well as recoding them.

In the rest of the states, the legislatures or legislature and governor draw the maps. When one party has a trifecta, they have the capacity to draw the maps and pass a redistricting bill without help from the other party. In Connecticut and North Carolina, control over both chambers of the legislature is sufficient because the governor cannot veto the redistricting bill.

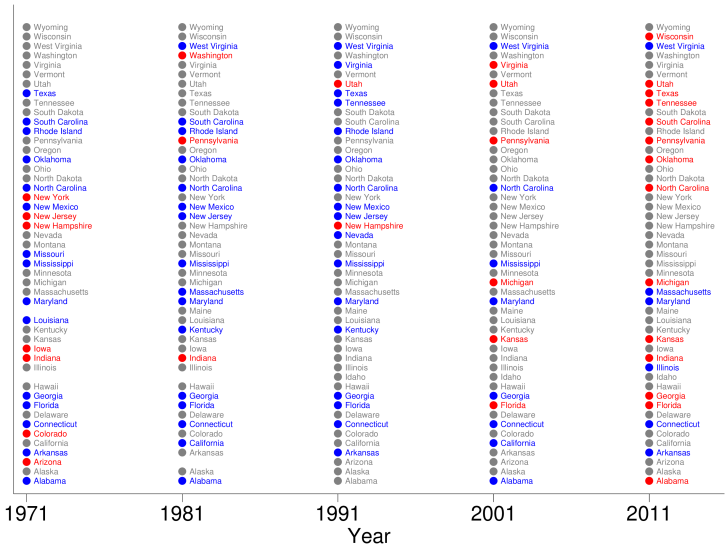
We estimate the impact of the legal ability of a party to redistrict upon subsequent federal partisan seat shares. This depends upon the laws in the state, the number of seats, the distribution of legislative seats across both chambers and usually the control of the governorship. We use variation across decades and across states to estimate the impact of partisan control over redistricting on partisan seat shares. The details of the laws and how they vary by states over time are explained

[3]In addition, Nebraska also became the only state to have a unicameral state legislature with the passage of the same 1934 law

[4]Montana uses the commission in years in our data set where they have more than one Representative.

in greater detail in the Data Appendix. In Figure 4.1, we color code states by decade with blue for Democratic control, red for Republican control, and gray if neither party had control We code neither party as having control if the government was divided and required approval by all chambers plus the governor, if the legislature was divided and only the legislature was required to pass a redistricting bill, or if redistricting was delegated to a commission. In the 1970s through 1990s, the Democrats had a much higher share of states with legal control. However, in the 2000s, control was largely balanced across parties and in the 2010s, the Republicans maintained partisan control in a substantially higher fraction of states.

Figure 4.1: States with Legal Control



Notes: Blue dots indicate Democrat legal control over redistricting in a state-decade; red dots indicate Republican legal control; grey dots indicate no legal control.

4.3 Empirical Methods

In this section, we present the empirical methods that we will use to estimate our main effects. We present two main estimation methods. The first uses variation across state-decades in legal control over redistricting, controlling for year effects. The second uses a regression

discontinuity estimator in the vote share of the governor given unified legislative control in redistricting years.

In our first main specification, we regress an electoral outcome variable, $O_{s,d,y}$, on a measure of partisan legal control conditional upon stateXdecade and year effects. Our main outcome variable is the the Republican House of Representative seat share; however, we do use other outcome variables in the paper. The seat share outcomes are indexed to the year in which the Congress is elected. Thus, it is indexed to years ending in 0, 2, 4, 6, or 8, s denotes the state and d reflects the decade. $DemControl$ and $RepControl$ are dummy variables which take on a value of 1 if either the Democrats or the Republicans respectively have the legal ability to pass a redistricting bill solely on votes from their own party in a year ending in 1 [5]; the dummy variable takes on a value of 0 if neither party controls the redistricting process unilaterally. This can happen if government if different parties control the two chambers of the state legislature, if the governor is a different party from a unified legislature and redistricting bills require a gubernatorial signature, or if maps are drawn by a commission with the legal authority to force use of the maps they draw. $\gamma_{s,d}$ and δ_y are stateXdecade and year fixed effects respectively.

We regress the outcome on our main treatment variables, $DemControl$ and $RepControl$, as well as their interactions with year dummies for years ending in 8, 2, 4, and 6 respectively. These jointly saturate the decade and allows us to trace the dynamic path of our effect over five elections where the baseline year is the last Congress elected before redistricting. All coefficients, then, are computed relative to the state delegation's partisan composition in that base year. We

[5]Redistricting is supposed to happen in years ending in 1. In a small number of cases, redistricting is done later in the decade. Due to endogeneity concerns as well as concerns about aggregation of cohort effects over time, we base our definition of legal control off of the composition of state legislatures and governorships in years ending in 1. However, we show that our results are nearly identical if we instead define our treatment status variable based upon the year when redistricting actually occurs.

also use a non-traditional definition of decade. We define decade as beginning in years ending in 8 and running through years ending in 6. Thus, in addition to the two coefficients which are normalized to zero, we estimate one coefficient for each of the two treatment variables from before redistricting (i.e. 'leads') and three from after redistricting (i.e. 'lags'). Our specification is given by:

$$O_{s,d,y} = \alpha + \beta_y^D I(year = y) DemControl_{s,d} + \beta_y^R I(year = y) RepControl_{s,d} + \gamma_{s,d} + \delta_y + \epsilon \tag{4.1}$$

We are further concerned that there are long run trends in partisanship, even within decades, due to realignment of the party system particularly after the passage of the 1964 Civil Rights Act (Abramowitz and Saunders (1998); Gentzkow et al (2016); Jensen et al (2012); Kuziemko and Washington (2018); Schickler (2016)). We therefore use, as a robustness check, an additional very taxing specification; we add in state specific linear time trends in the outcome variable.

We additionally show as a robustness check a two-way fixed effects model with state and year effects. Our main specification with stateXdecade rather than state effects is more restrictive in order to improve upon identification of the average effect of partisan control upon partisan seat shares. A recent econometric literature, Abraham and Sun (2018); de Chaisemartin and D'Haultfoeuille (2020); Goodman-Bacon (2018) notes that two way fixed estimators do not properly aggregate an average of cohort-specific treatment effects in the presence of heterogeneous treatment effects by cohort. Abraham and Sun (2018), for example, show that in simple models where each cross-sectional unit is treated once and permanently, the two way fixed effects estimator can be represented as a weighted sum of cohort-specific treatment effects. Since early adopters

are largely used as controls for late adopters, their treatment effects are usually weighted negatively. In fact, Abraham and Sun (2018) show that, for this reason, two way fixed effects estimates can lie outside of the convex-hull of cohort-specific treatment effects. In our context, we show in the results section that estimates increase over time and are thus likely to be biased upwards, affirming our choice of specification with state-decade and year fixed effects rather than state and year effects.

We also assume that all treatment of partisan control happens based upon the state legislatures and governors in power in a year ending in one. In a very small fraction of cases, redistricting is done by legislators not in power in years ending in one. To the degree this is the case, our ITT estimates are likely attenuated ATE estimates by introducing measurement error in treatment. We show, however, that almost all redistricting is done by state legislatures in power in years ending in one. In return, however, all treated units are treated at the same time within a decade and thus the issues of concern for Abraham and Sun (2018); de Chaisemartin and D'Haultfoeuille (2020); Goodman-Bacon (2018) are not of concern for us.

In a second specification, we estimate a regression discontinuity model in the gubernatorial vote share. We limit our sample to states with a unified legislature requiring redistricting bill passage with a gubernatorial signature. A narrow electoral victory for a governor whose party aligned is with the unified legislature than confers legal control. Our regression discontinuity estimator compares outcomes in state-decades where a party has narrowly achieved legal control to others where legal control was not obtained due to a small difference in the vote share for governor. The running variable for the regression discontinuity is thus the top-two party vote share for the party with unified control in both chambers of the state legislature in the election which determines the governor for the year ending in 1. Since the randomization happens at the

level of the state-decade, five observations are randomized by each state-decade in the sample. As a result, we also include decade-year fixed effects. Thus, in our sample of state-decades where the governor in a year ending in 1 won a narrow victory, our identifying assumption is that assignment of legal control is uncorrelated with state seats-shares within a Congress. We estimate separately for unified Republican legislature (and thus Republican legal control) and unified Democratic legislature (and thus unified Democrat control). We use a local linear regression with a triangular kernel. Our estimation equation is given by:

$$O_{s,d,y} = \alpha + \beta_y I(year = y) Control_{s,d} + \gamma_{d,y} + \delta_y +$$

$$g_{Lose}(GovVS_{d,s}) + win_{d,s} X g_{Win}(GovVS_{d,s}) + \epsilon_s, d, y$$

Note that we estimate coefficients for each year. Since we do not put in state-decade fixed effects, we estimate effects for Congresses elected in years ending in 0, 2, 4, 6, and 8. In other words, we report coefficients for $\beta_2, \beta_4, \beta_6$ and β_8 by subtracting off the coefficient β_0. This makes our coefficient directly comparable to our panel estimates using state-decade and year fixed effects.

For both of our main specifications, we report individual year coefficients. However, we also report the average coefficient for the three elections after redistricting (i.e. for Congresses elected in years ending in 2, 4, or 6). We thus report:

$$\frac{\beta_2^D + \beta_4^D + \beta_6^D}{3} - \beta_0^D = 0$$
$$\frac{\beta_2^R + \beta_4^R + \beta_6^R}{3} - \beta_0^R = 0 \tag{4.2}$$

Finally, we cluster all of our results by state-decade. We do this for two reasons. First, our data is heteroskedastic. Variances in state delegation shares are substantially higher in smaller states for mechanical reasons. However, since delegation sizes do not change much over time, errors are only heteroskedastic within a state. second, clustering accounts for serial correlation within states over a decade. However, because realignment led to large reversals in partisanship, we do not cluster on decade; doing so decreases our standard errors though not by much [6]. Clustering at the state-decade level accounts both for the patterns of serial correlation and the patterns of heteroskedasticity present in the data.

4.4 Data

4.4.1 Vote Shares and Seat Shares

Our main independent variable is legal control by a party over redistricting. We compute this using state partisan control data from Klarner et al (2013). We obtain this back to 1969. This data is available through 2011. From 2012 onward we collect state partisan control data from the National Conference of State Legislatures' legislative partisan composition tables. We thus have a balanced panel of states from 1969 through 2018.

[6] Estimates with clustering by state as opposed to state-decade are available from the authors upon request

4.4.2 Legal Control and Unified Control

In order to determine whether a party controls the redistricting process in each state-decade, we collect data on how redistricting is conducted. For each state, we collect the state's statutory and constitutional rules for the redistricting process, including any changes to the rules over time. We code each state-decade as one of (1.) Single district state, (2.) Legislature + Governor state, (3.) Legislature only state, or (4.) Commission. If the state has a commission, we furthermore classify it as an advisory commission if it merely provides a recommendation or a statutory commission if it has legal authority to pass a redistricting plan. We classify each commission as partisan or non-partisan depending upon whether a majority of commission members can be appointed in a partisan manner. In our main specification, we treat all commission types as selected in a non-partisan manner. We use further sub-classifications of commission types in Table 4.5.4.

From 2000 onward, data on rules comes from Justin Levitt's website. For the pre-2000 period, we employed a team of undergraduates to collect documents from individual state legislatures and from the National Conference of State Legislatures. We present and document our main treatment variable in the Data Appendix.

As a robustness check, we also use the data on legal control from Friedman and Holden (2009). This data goes from 1969 through 2004. It was assembled by Friedman and Holden based upon prior work by Cox and Katz (ICPSR 6311) and subsequent work by Gary Jacobson [7].

We also estimate the impact of legal control over redistricting on political polarization of

[7] Richard Holden graciously provided us with the data

the state delegation to the House of Representatives. We measure polarization as the absolute value of the first dimension of the DW-Nominate score, which we obtained from 1968 through 2018 from the voteview website. We also estimate the impact of legal control on the share minority of the state's delegation. We computed from lists of all current and historical minority legislators maintained by the House of Representatives on its website. Finally, we also compute the efficiency gap measure of the gap in differential wasted for the Republican party. In cases of uncontested elections, we follow Stephanopoulos and McGhee (2015) and fill in with the presidential vote share in the district. We additionally compute differential wasted presidential votes by House district, which we obtained from Jim Snyder.

4.5 Main Results

In this section, we present our main results. We begin by documenting that legal control of a party over redistricting leads to a greater land area redistricted and that almost all redistricting happens between Congresses. We then show our main estimates of effects of legal control on partisanship, starting with our state-decade and year fixed effect model followed by the regression discontinuity model.

4.5.1 Measuring the Extent of Redistricting

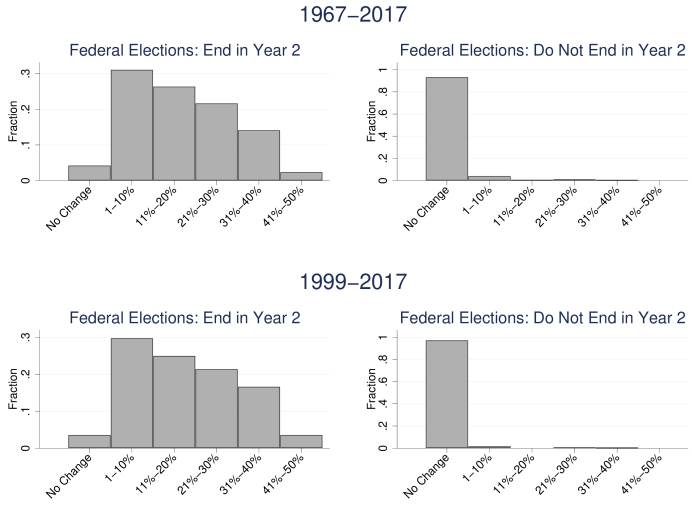
In our main specification, we assume that redistricting is in effect for Congresses elected in years ending in 2. To document that this is in fact the case, we create a quantitative measure of the extent of redistricting. We use ARC-GIS to geocode every map from every state for every Congress between 1969 and 2018. When there is a change between two Congresses, we compute

the geographical overlap between each pre-existing Congressional district and each new district. For each pre-existing district, we assign to it a unique new district with which it has maximum geographical overlap. We then sum over all pre-existing districts and compute the fraction of overlap as a share of all land. Thus, we compute the change in land area in every district[8]. Our results are presented in Figure 4.2. We show our measure separately for Congresses elected in years ending in 2 and for those elected in years not ending in 2. Moreover, we do this for both the full sample as well as the more recent sample incorporating only the past two decades. Both over the full sample and in the recent sample, almost all state-decades in our sample do redistrict. Approximately 30% shift between 1% and 10% of their land; approximately 25% shift between 10% and 20% of their land; well over 90% of state-decades shift less than 40% of their land and all shift less than 50% of their land. By contrast, in other years, almost 100% of states have no change in district boundaries. This is even true in the more recent period of time where there have been more legal challenges to redistricting.

We follow our main regression specification (4.1) and regress our measure of the amount of redistricting on our two partisan control variables, stateXdecade fixed effects and year fixed effects. We show our results in Table 4.5.1. Our preferred specifications, in columns 2 and 5, use the full sample and the 2000+ sample respectively. The full five-decade sample show that 6.3% more land is redistricted when Republicans have control relative to no party having legal control; these results are significant with a standard error of 0.021. The coefficient for Democrats, by contrast, is 1.4% and very far from statistically significant at conventional levels. In the past two decades, the coefficient for Republicans is larger. It is 9.8% with a standard error of 0.04. The

[8]We computed this measure based upon land area rather than population since census tracts, which are population-based, were only introduced across the entire United States for the 2000 census.

Figure 4.2: Timing of Redistricting



Notes:Graphs show the distribution of percentages of land whose district changed in a state since the prior federal election. The top panel shows changes over the entire sample period. The bottom panel restricts the sample to the 1999-2017 time period. Graphs on the left show the distribution of changes for federal elections ending in 2; graphs on the right show the distribution of changes for federal elections ending in all other years.

coefficient for Democrats is also larger; it is 4.5% but not statistically significant at conventional levels.

Of course, it is possible that Republicans have legal control and are more dominant in more rural areas where larger shifts in land do not substantively translate into larger shifts in population. Therefore, we also show the same results with a population-based rather than land-based measure of district change. These estimates show the percentage of people rather than land who switch districts as a result of redistricting. We show these population-based results in Column 6. Due to data constraints, we only show results for the past two decades. Overall, the estimates are of similar magnitude though slightly less precise.

Table 4.1: District Characteristic Changes from the Prior Election Period

	(1)	(2)	(3)	(4)	(5)	(6)
Republican Control: Effect on District Change						
Control x Election Ending in 2	0.070**	0.063*	0.097**	0.084*	0.098**	0.084*
	(0.029)	(0.031)	(0.040)	(0.043)	(0.040)	(0.043)
Control x Election Ending in 4		-0.012			-0.005	-0.006
		(0.009)			(0.005)	(0.006)
Control x Election Ending in 6		-0.009			0.001	0.000
		(0.008)			(0.002)	(0.002)
Control x Election Ending in 8		-0.005			0.006	0.005
		(0.013)			(0.004)	(0.004)
Democrat Control: Effect on District Change						
Control x Election Ending in 2	0.009	0.014	0.040	0.017	0.045	0.021
	(0.022)	(0.024)	(0.040)	(0.044)	(0.042)	(0.046)
Control x Election Ending in 4		0.002			-0.010	-0.014
		(0.008)			(0.008)	(0.010)
Control x Election Ending in 6		0.005			0.015	0.016
		(0.010)			(0.020)	(0.023)
Control x Election Ending in 8		0.013			0.016	0.016
		(0.010)			(0.016)	(0.017)
Sample	All	All	2000+	2000+	2000+	2000+
Outcome Basis	Land	Land	Land	Pop	Land	Pop
Number of Observations	1060	1060	420	420	420	420
R2	0.641	0.642	0.667	0.681	0.668	0.683’

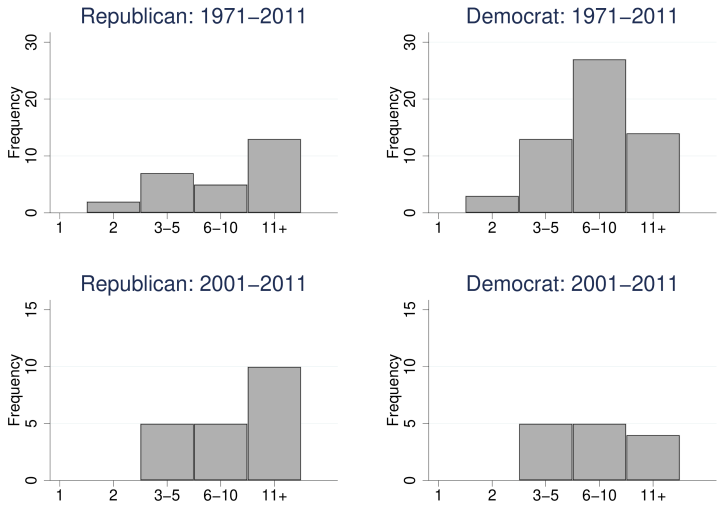
Notes: Each column display coefficients from a single regression. The dependent variable is the fraction of land within a state changing districts since the prior decade in Columns 1 and 3, the fraction of land within a state changing districts since the prior election in Columns 2 and 5, the fraction of population within a state changing districts since the prior election in Column 4 and the fraction of the population within a state changing districts since the prior election in Column 6. The treatment variable is legal control in the years ending 1. State-year level regressions are conditional upon state-decade and year fixed effects. State-decade level regressions are conditional upon decade and state fixed effects. Columns 1-2 estimate over the full sample. Columns 3-6 estimate over the 2000+ sample.

4.5.2 Effects on Seat Shares: Fixed Effect Estimates

We now present our main results with stateXdecade and year fixed effects. Our variation comes from comparing seat shares across state-decades where a party has legal control to other states where it does not. We additionally control for electoral waves with year effects. It is crucial for our results that we have enough instances of legal control. In Figure 4.3, we show four histograms which display the number of instances of partisan control separately for each party

by size of state delegation. We show histograms for both Democrats and Republicans for the full sample as well as separately restricted to the past two decades. In fact, Republicans only had 12 instances of partisan control in the first three decades of our sample. Seven of these 12 instances are from the 1970s.

Figure 4.3: Distribution of State Size with Legal Control



Notes:Graphs show distributions of delegation sizes under partisan control of redistricting. The top panel shows distributions over the entire sample period. The bottom panel restricts to the 1999-2017 time period. Graphs on the left show the size distribution under Republican control. Graphs on the right shows the size distribution under Democratic control.

Overall, we include 212 state-decades in our sample. Out of these, we find 62 instances of Democratic control over redistricting. In contrast, we find 32 instances of Republican control. This is due to the dominance of Democrats in the earlier portion of our sample. In fact, when we restrict to the past two decades, we see 20 instances of Republican partisan control but only 14 of Democratic control. In part, this more recent Republican dominance is due to historic losses of control by the Democratic party in the 2010 elections.

In general, the size distribution of states skews slightly larger for Republican control than it does for Democratic control. This may seem somewhat surprising. However, many of the larger

Democrat-dominated states either have had Republican governors (California, New York) during redistricting or have used redistricting commissions (California, New Jersey, Washington).[9]

Our main estimates are presented in Table 4.5.2. The results are split into two panels: a top panel for the effect of Republican control and a bottom panel for the effect of Democratic control. The coefficients are jointly estimated in a single regression for a given column across panels. Different columns represent different regressions, estimated with Equation(4.1). The first four columns show estimates from the full sample and the second four columns show estimates restricted to the past two decades. For each sample, we show results for states with more than one representative, more than two representatives, more than five representatives and more than 10 representatives respectively.[10] We do not ever include single representative states because they do not redistrict.

In the first election after redistricting, we find a 4.7 percentage point increase in a state delegation's Republican seat share following Republican control over redistricting. The effect sizes rise to 8.0 percentage points as we raise state size restriction to states with more than 10 representatives. These results are all statistically significant with at least a 95% level; effect sizes for recent decades are larger. In columns 5-8, we present estimates restricted to the past two decades, these estimates are larger in magnitude. In the maximal sample of states with more than one representative, Republican control leads to an 9.1 percentage point increase in a state delegation's Republican share following Republican redistricting control. Size restrictions lower coefficients but all estimates lie at 6.5 percentage points or above and are all statistically significant with a 0.06 p-value or less.[11]

[9] California moves to a commission system during our study period.

[10] We compute the number of representatives in a state-decade as given by the number of representatives elected in years ending in 2 - i.e. just after federal reapportionment.

[11] Note that the t-statistic for the 10 or more size restriction is above 2 but because of the substantial degrees of

Table 4.2: Main Specification

	All Years				2000 Onward			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Republican Control: Effect on Republican Representative Seat Share								
Control x Election Ending in 2	0.047**	0.048**	0.063**	0.080***	0.091**	0.065***	0.076**	0.078*
	(0.023)	(0.023)	(0.027)	(0.024)	(0.035)	(0.024)	(0.028)	(0.038)
Control x Election Ending in 4	0.002	0.024	0.022	0.040	0.059*	0.054*	0.071**	0.075
	(0.043)	(0.039)	(0.040)	(0.050)	(0.034)	(0.029)	(0.029)	(0.048)
Control x Election Ending in 6	0.034	0.058	0.044	0.064	0.094	0.071*	0.084**	0.078
	(0.048)	(0.046)	(0.038)	(0.047)	(0.058)	(0.039)	(0.039)	(0.059)
Average Effect	0.028	0.043	0.043	0.061	0.082**	0.063**	0.077**	0.077
	(0.035)	(0.032)	(0.034)	(0.039)	(0.040)	(0.028)	(0.031)	(0.046)
	[0.462]	[0.196]	[0.232]	[0.113]	[0.044]	[0.050]	[0.033]	[0.116]
Control x Election Ending in 8	-0.000	-0.008	-0.048**	-0.014	0.022	0.002	-0.047*	-0.010
	(0.041)	(0.040)	(0.020)	(0.021)	(0.045)	(0.039)	(0.026)	(0.033)
Democrat Control: Effect on Republican Representative Seat Share								
Control x Election Ending in 2	-0.018	-0.009	-0.014	-0.027	-0.003	-0.022	-0.096**	-0.127**
	(0.023)	(0.021)	(0.017)	(0.023)	(0.045)	(0.041)	(0.040)	(0.042)
Control x Election Ending in 4	-0.010	0.020	0.030	0.024	-0.015	-0.019	-0.105**	-0.131**
	(0.031)	(0.026)	(0.024)	(0.029)	(0.050)	(0.049)	(0.039)	(0.053)
Control x Election Ending in 6	0.016	0.050	0.049	0.028	0.027	0.004	-0.066	-0.119**
	(0.033)	(0.031)	(0.029)	(0.039)	(0.066)	(0.052)	(0.041)	(0.051)
Average Effect	-0.004	0.021	0.022	0.008	0.003	-0.013	-0.089**	-0.126**
	(0.025)	(0.023)	(0.019)	(0.028)	(0.049)	(0.043)	(0.038)	(0.044)
	[0.846]	[0.370]	[0.270]	[0.783]	[0.942]	[0.747]	[0.069]	[0.035]
Control x Election Ending in 8	-0.055**	-0.041	-0.015	-0.016	-0.035	-0.050	0.024	0.008
	(0.026)	(0.026)	(0.018)	(0.016)	(0.051)	(0.050)	(0.025)	(0.031)
Sample	All	All	All	All	2000+	2000+	2000+	2000+
Size Restriction >	1	2	5	10	1	2	5	10
Republican Treatments	32	30	22	14	20	20	15	10
Democrat Treatments	62	57	42	18	14	14	8	4
Number of Observations	1060	900	675	295	420	370	260	120
R2	0.778	0.840	0.851	0.846	0.869	0.877	0.922	0.856

Notes: Each column presents coefficients from a single regression. Each observation is a state-year. Treatment is unilateral legal control of a political party over redistricting. The dependent variable is the state's Republican share of seats in federal House of Representatives. All specifications include state-decade and year fixed effects. Columns 1-4 use data from 1967-2017. Columns 5-8 use data from 1999-2017. Size restrictions along columns restrict the sample to states with more than the listed number of representatives. Rows show estimates of the effect of legal control. Average effect reports the average of the effects for elections for federal elections ending in 2, 4, and 6 respectively. Average effects represent the mean of the coefficients on elections year ending in 2, 4, and 6. Control X Election Ending in 8 represents the coefficient on control for elections in the year 8 from the previous decade. Standard errors clustered by state are in parentheses. P-values from the Wild Cluster bootstrap are presented in square brackets for average effects.

In the second-to-bottom row of the panel, we average effects over the three elections following redistricting. The full sample estimates show a statistically insignificant positive coefficient of 2.8 percentage points for Republican control. Independent of size restrictions, we do not see statistically significant increases in the Republican share of a state's delegation following Republican-controlled redistricting. However, in the past two decades, we do see large and

freedom correction, the p-value is 0.06.

statistically significant average increases in the Republican seat share of a delegation following Republican redistricting. Our benchmark estimate yields an 8.2 percentage point increase on average over the following three elections. These estimates are statistically significant at conventional levels. Due to concerns over cluster size, we also present Wild-Cluster bootstrapped p-values in brackets for average effects[12]; results are similar. Estimates are 6.3 percentage points in states with more than two seats and 7.7 percentage points restricted to both states with more than five as well as more than ten seats respectively. The averages do not include effects for estimates beyond three elections because the other two elections in a decade are the baseline election (years ending in zero) and the first lead (years ending in eight).

In contrast to the effects we find of Republican control, we do not find statistically significant or large decreases in Republican seat share after Democratic-controlled redistricting. This is true whether we estimate over the full fifty year sample or whether we restrict to recent years. The reduction in the Republican seat share even in the initial election after Democratic control over redistricting is 1.8 for the full sample and 0.3 in the recent period. We do see large, statistically significant effects only in the recent period when looking at large Democratic states. Restricting to the eight instances in the past two decades with Democratic control in states with more than five seats, we find an average decrease in the Republican seat share of 8.9 percentage points; restricting to states with more than ten seats, we find a decrease of 12.6 percentage points. These estimates are statistically significant with a 95% level of confidence using conventional standard errors. Using the wild cluster bootstrap, both have a p-value less than 0.1 and the more than five restriction yields a p-value less than 0.05. Though these large estimates are estimated off of a

[12]We do not show wild cluster bootstrap p-values for individual year coefficients in order to limit the size of the table. However, wild cluster bootstrap p-values are available from the authors upon request.

small number of clusters: 8 for state-decades with more than 5 seats and 4 for state-decades with more than 10 seats.

In the last row of each panel, entitled "Control X Election Ending in 8", we show the coefficients on partisan legal control for the election prior to the one in which the government with legal control was elected. These 'leads' are small and statistically insignificant with the exception of our estimates restricted to states with more than five seats for our Republican control estimates and our main sample for Democratic control. The small number of statistically significant leads are consistent [13] with what one would expect by random chance.

4.5.3 Effects on Seat Shares: Regression Discontinuity Estimates

We now present an alternative estimation strategy: a discontinuity estimate in the vote share for the gubernatorial election. In particular, we estimate off of a sample where small differences in the vote share for the governor led to a differences across-state decades in legal control by a party over redistricting. Of course, in order for gubernatorial races to be pivotal in determining legal control over redistricting, two things must be true. First, states must require both a gubernatorial signature and legislative approval in order to pass a redistricting bill. This is a sizable majority of states in all decades. However, it does exclude commission states on the one hand because parties do not control redistricting process when that authority is delegated to a commission. It also excludes the states of Connecticut and North Carolina because they do not require a gubernatorial signature. Second, one party must have control over both chambers of the state legislature. That way, a gubernatorial win for the party aligned with the unified state legislature will translate into legal control. We estimate separately by party for both the full sample and for the recent

[13] Three of sixteen estimates are statistically significant at conventional levels.

period. We use a bandwidth of 0.1 across all samples though we show robustness to bandwidth in Appendix Figur4.1 1. Our sample size are substantially smaller: 46 state-decades over the full sample for Republicans, 75 over the full sample for Democrats, 23 over the recent period for Republicans, and 22 over the recent sample for Democrats.

Overall, estimates are broadly similar to those using state-decade and year effects. We find large, statistically significant effects of Republican control in the past two decades but otherwise we find relatively small and statistically insignificant effects. Legal control by Republicans in the past two decades increases the Republican seat share in Congress by an average of 12.3 percentage points over the subsequent three elections. This is almost 50% larger than the same estimate using the state-decade and year fixed effects. However, the two estimates are far from statistically distinguishable at even a 90% confidence level. The similarity in the results are surprising for three reasons. First, the sample size for the regression discontinuity estimates are small and it is surprising that there is sufficient precision. Second, the samples are quite different and therefore it is not clear the estimates would be the same. Even more important, the identification is mainly from the marginal gubernatorial elections[14].

In Appendix Figure 1, we show four panels. Each panel shows a graph of RD estimates by bandwidth for each of our four samples. The overlaid blue line shows the cluster size (i.e. the number of state-decades) in the sample for the bandwidth. The estimates are quite robust, particularly for the effect of Republican control over the most recent two decades. The one exception is estimates are substantially larger and even statistically significant at conventional levels for very low bandwidth estimates of Republican control over the full sample. These

[14]Note that since the vote share fully determines unified control, the local average treatment effect is the average treatment effect at the discontinuity.

estimates are only statistically significant for a cluster size below ten where the standard errors are not asymptotically valid. Moreover, the estimates are large because the full sample only reflects the last two decades for small bandwidths.

Overall, we find robust evidence of an 8 percentage point to 12 percentage point increase in Republican representation in Congress following Republican legal control over redistricting in the 2000+ time period but we otherwise find little effect of legal control.

4.5.4 Robustness

In Table 4.3, we show the robustness of our results to alternative specifications and data. Our estimates are largely robust. In column 1, we repeat our baseline estimates. In column 2, we replace our legal control variable with unified control as our main treatment variable. [15] In column 3, we use the treatment variable from the Friedman-Holden data Friedman and Holden (2009). The Friedman-Holden data end in 2004 and thus we extend their cutoff year past 2004 using their method of classifying partisan redistricting.[16] The estimates for the impact of Republican control over the full sample are slightly more than double those in the main sample but remain statistically indistinguishable from zero at conventional levels. In column 4, we control linearly for the statewide vote share for House of Representatives races to account for time-varying political preferences of the electorate. In other words, we control linearly for the seat-share/vote-share map. We do this because we are concerned that legal control may be endogenous to partisan preference shocks at the state level. Our estimates decline slightly to 6.9 percentage points and statistical significance falls slightly to just below the 95% level of confidence. We do not see any

[15] Unified control is usually used in the political science literature to look at the impact of control over redistricting because of the costs of collecting the legal control variable.

[16] We do not report estimates using the Friedman-Holden data for the 2000+ time period since their actual data only covers 20% of the time period.

sizable or statistically significant estimates for Democrats.

Table 4.3: Robustness and Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All Years								
Rep Average Effect	0.028	0.032	0.062	0.032	0.032	0.113***	0.036	0.096***
	(0.035)	(0.030)	(0.038)	(0.032)	(0.038)	(0.031)	(0.041)	(0.033)
Dem Average Effect	-0.004	0.000	0.007	-0.014	-0.006	-0.038	-0.007	-0.019
	(0.025)	(0.025)	(0.023)	(0.021)	(0.030)	(0.037)	(0.030)	(0.047)
Number of Observations	1060	1060	1060	1060	885	1060	1060	1060
R2	0.778	0.778	0.778	0.838	0.750	0.495	0.794	0.778
2000 and Onward								
Rep Average Effect	0.082**	0.072*		0.069*	0.108*	0.121***	0.096*	0.086**
	(0.040)	(0.042)		(0.036)	(0.056)	(0.036)	(0.051)	(0.038)
Dem Average Effect	0.003	0.002		-0.007	0.022	0.049	-0.005	0.025
	(0.049)	(0.049)		(0.040)	(0.059)	(0.073)	(0.066)	(0.068)
Number of Observations	420	420	420	420	315	420	420	420
R2	0.869	0.871	0.867	0.885	0.828	0.753	0.903	0.869
Legal Control	X							
Unified Control		X						
Holden Data			X					
Vote Share Control				X				
Exclude Commissions					X			
2-Way FE						X		
State Linear Trends							X	
Change in Control								X

Notes: Each column within a panel presents the average of coefficients from a single regression. The top panel presents coefficients over the full sample from 1969-2017. The bottom panel presents coefficients from a restricted panel spanning 1999-2017. All specifications include state-decade and year fixed effects. Treatment is unilateral legal control of a political party over redistricting. The dependent variable is the state's Republican share of seats in the federal House of Representatives. Rows show estimates of the average effect of legal control by a party on seat-shares in federal elections ending in 2, 4, and 6. Column 2 replaces legal control with unified control. Column 3 replaces our legal control variable with a similar measure due to Holden-Friedman. Column 4 adds in statewide Republican vote share in elections for the House of Representatives as a control. Column 5 excludes states with electoral commissions from the sample. Column 6 replaces state-decade fixed effects with state fixed effects. Column 7 adds state-specific linear time trends to the baseline model in Column 1. Column 8 drops decades with legal control where the same party had legal control in the prior decade. Standard errors clustered by state are in parentheses.

In column 5, we present estimates in which we drop commission state-decades from our sample. The coefficient on Republican control for the recent sample rises by 3.6 percentage points when we drop states with an electoral commission for all specifications; in particular, the Republican effect in the recent sample becomes statistically significant at the 10% level. This potentially suggests that commission states redistrict in a slightly more partisan manner than states with divided government. However, the differences are not large. However, the other

estimates including the estimates restriction to the prior two decades are within one percentage point of the benchmark estimates.

In column 6, we show our two way fixed effects estimates. The estimates are substantially larger though still statistically insignificant at conventional levels for Democrats. The effect size increases by 50% for Republican control in the recent period and by 300% in the full sample. Since effect sizes for Republicans are increasing over time, this is exactly what we would expect given the recent work in the new panel effects estimation literature. The differences between the two-way fixed effects design and the baseline state-decade fixed effects and year effects design precisely validate the need for our baseline design.

We then show results for our very taxing specification state-specific linear trends baseline model. We estimate this model out of concerns that state specific trends such as the realignment of the parties may induce a correlation both between legal control and increases in the dominant party's seat share. These could even happen within decades. Our estimates become less precise, likely due to over-fitting given the limited degrees of freedom. However, the estimates remain remarkably similar given the large number of covariates added. All estimates are within 1.4 percentage points of our baseline estimates.

Finally, we reclassify partisan legal control as non-partisan where the same party also had legal control in the prior decade. Since parties with ongoing legal control may not gerrymander much if their redistricting goals have already been achieved, we expect our coefficients to increase in magnitude, potentially by a sizable amount. In most cases of legal control over redistricting in our five decade sample, that same party did not have legal control in the prior decade. In column 8, we estimate on this sample of new legal control and we precisely find that all coefficients are larger in magnitude as well as of the same sign. The increase in the effect for Republicans is

particularly large. Thus, our results are consistent with a time-invariant effect of new Republican control; the difference between the earlier and later time periods then is due to the greater prevalence of new Republican control in the recent period.

We also show robustness to our definition of legal control and present the results in Table **??**. We consider two different types of robustness. In columns 2-3, we reclassify types of commissions and allow different ways of defining commissions as instances of partisan legal control. In the last three columns, we reclassify instances of a unified legislature with a governor of the opposite party as instances of partisan legal control. We do this because legislatures can often over-ride gubernatorial vetos with a strong enough majority. Moreover, legislatures can threaten governors to over-ride vetos on unrelated legislation. Since veto thresholds can vary by type of bill, we consider different levels of minimal legislative majority in columns 4-6 as instances of partisan legal control by the legislature in the cases of divided government with a unified legislature.

Returning to commissions, we define commission types and then discuss our different codings for the purposes of our robustness checks. A partisan commission is a commission which can be appointed with a net partisan balance. These commissions are often appointed by the Governor or the majority leaders of the state legislatures. Some commissions are appointed in a non-partisan or a bi-partisan (i.e. balanced in partisanship) manner.[17] An advisory commission draws maps and submits them to the legislature and governor for legislative and gubernatorial approval. However, advisory commissions have no legal authority to redistrict. A non-advisory commission draws maps and the maps are automatically accepted as law. Non-advisory commissions

[17]For example, in some states, commissions are composed of five members, one member appointed by each of the majority and minority leaders in each of the two chambers. The fifth member is then appointed by a majority of the four directly appointed members.

Table 4.4: Varying Definitions of Legal Control

	(1)	(2)	(3)	(4)	(5)	(6)
All Years						
Rep Average Effect	0.028	0.034	0.051*	0.031	0.026	0.023
	(0.035)	(0.034)	(0.029)	(0.035)	(0.032)	(0.035)
Dem Average Effect	-0.004	0.009	0.013	0.008	-0.018	-0.022
	(0.025)	(0.025)	(0.025)	(0.026)	(0.025)	(0.024)
Number of Observations	1060	1060	1060	1060	1060	1060
R2	0.778	0.779	0.779	0.779	0.777	0.778
2000 and Onward						
All Years						
Rep Average Effect	0.082**	0.083**	0.104**	0.081**	0.082**	0.076**
	(0.040)	(0.039))	(0.039)	(0.039)	(0.037)	(0.036)
Dem Average Effect	0.003	0.006	0.013	0.002	0.001	-0.040
	(0.049)	(0.025)	(0.049)	(0.055)	(0.025)	(0.035)
Number of Observations	420	420	420	420	420	420
R2	0.869	0.870	0.871	0.870	0.869	0.870
Baseline Legal Control	X					
Partisan Appointed Commissions Included in Treatment Pool		X				
Advisory Commissions Included in Treatment Pool			X			
Super Majority ME, CT				X		
Super Majority 60%					X	
Super Majority 66%						X

Notes: Each column within a panel presents the average of coefficients from a single regression. The top panel presents coeffcieints over the full sample from 1969-2017. The bottom panel presents coefficients from a restricted panel spanning 1999-2017. All specifications include state-decade and year fixed effects. Treatment is unilateral legal control of a political party over redistricting. The dependent variable is the state's Republican share of seats in federal House of Representatives. Rows show estimates of the average effect of legal control by a party on seat-shares in federal elections ending in 2, 4, 6. In column 2, partisan legal control includes states with commmisions with net partisan appointment. In column 3, partisan legal control treatment includes states with advisory commisions with unified legislative and gubernatorial control. In column 4, partisan legal is modified to reflect the 60% thresholds used for redistricting bills in Connecticut and Maine. In column 5, partisan legal control treatment includes states with a unified legislature and divided governor where both legislative chambers are above a 60% threshold. In column 6, partisan legal control treatment includes states with a unified legislature and divided governor where both legislative chambers are above a 66% threshold.

do not need gubernatorial or legislative approval. We consider redefining commissions as being party-controlled depending upon how they are selected and whether they have the legal ability to directly implement the maps that they draw.

In the first column, we present our baseline results. Then, We consider redefining advisory commissions as instances of partisan legal control when they are appointed in a partisan manner (column 2). We also estimate a model (column 3) where we redefine partisan legal control to include commissions when they are merely advisory (i.e. they do not have the ability to

directly implement the maps they draw).[18] Column 2 shows estimates of partisan legal control with net partisan-appointed commissions redefined as instances of partisan legal control. The estimates barely change from our baseline estimates. In column 3, we show estimates with advisory commissions reassigned as partisan in the case of unified control.[19] All estimates show an increased positive impact on the Republican seat share, mostly by approximately two percentage points. These results suggest that advisory commissions do not necessarily reduce partisan redistricting though the differences in the estimates are due to a small number of state-decades and results are only sizable for the case of Republican unified control.

In the last three columns of Table 4.5.4 we also redefine legal control as partisan when the governor is a different party from the legislature but the legislature is unified and has over 60% (column 5) and over 66.7% (column 6) majority in both houses respectively. We do this because this may give the legislature the ability to pass a redistricting bill over a Gubernatorial veto. For Connecticut and Maine, which have legislative redistricting thresholds of 60% to pass a bill, regardless of gubernatorial approval, we redefine partisan control to those levels (column 4). None of these changes make a substantial difference in our estimates. Across all definitions, the variation in estimates are similarly small. We thus conclude from Table 4.5.4 that our estimates do not depend upon our particular definition of legal control.

One issue with our estimates is that observations are naturally heteroskedastic. A state-

[18] We do not show estimates based upon samples where we redefine combinations of advisory/non-advisory and partisan/non-partisan appointment as partisan appointment because all non-advisory commissions are also appointed in a bi-partisan or non-partisan manner. Thus, redefining advisory commissions as partisan legal control when government is unified is the same as redefining only advisory commissions with partisan appointment under the same circumstances. Also, redefining partisan appointment state-decades as instances of partisan legal control is akin to redefining partisan appointment with advisory commissions. Since, in all of these cases, results are identical, we limit redefinitions based solely upon changing advisory commissions as well as partisan-appointed commissions individually.

[19] All the advisory commission states require passage of redistricting by a majority of each legislative chambers and the signature of the Governor.

decade with two seats (i.e. Maine over the full sample) inherently changes the seat share by 0.5 when one seat changes party. However, a state-decade with 53 seats (i.e. California in the 2000s and 2010s) changes the seat share by less than 0.02 when one seat changes party. We thus re-estimate Tables 3 and 4, weighting them by delegation size. These estimates appear in Tables A1 and A2 in the Online Appendix respectively. We notice that the estimates are largely similar; however the standard errors are approximately 20% lower on average. The reduction in the standard errors is consistent with a reduction in heteroskedasticity.

Estimates of Democratic control do increase in Table A1 relative to Table 4.3 since larger states are more heavily weighted and effect sizes are larger for Democrats in larger states during recent period.[20] We note that since we include our baseline estimates in Tables 3 and 4, these estimates also appear in the Online Appendix Tables. Table A2, showing the weighted version of the legal control definition robustness table yields relatively similar estimates to Table 4.5.4. Also, estimates are similar across the different alternative definitions of legal control.

We additionally perform four quasi-placebos for the full sample as well as for the most recent two decades. For each sample period, these placebos lead to four placebo coefficients each for Republicans and for Democrats. In Table 4.5.4, we re-estimate effects as if redistricting were done by the state governments in power in years ending in 3, 5, 7, and 9 respectively. These are not actual placebos. If legal control in the treatment year persists, we may be picking up actual lags and leads of treated effects. Thus, it is all the more striking that we find only one statistically significant coefficient for Democrats or Republicans out of the eight quasi-placebo coefficients in the full sample and none in the 2000+ sample. Moreover, the true effect estimated

[20] Additionally, unified control estimates in the recent period become substantially smaller and statistically indistinguishable from zero. Since one of the contributions of our paper is to code and use legal control, we see the robustness of our legal control measure as opposed to the traditional unified control measure as further validation of the benefits of using legal control.

for Republicans in the 2000+ sample is the largest in magnitude of the 20 coefficients. That would happen by random chance if all the estimates were independent (as noted above, they are not) with a probability of 5%. Also 15 out of the 20 estimates are less than half the size of our estimate of Republican control in the past two decades.

Table 4.5: Year of Control Placebos

	(1)	(2)	(3)	(4)	(5)
All Years					
Republican Control Average Effect	0.028	-0.031	-0.006	-0.032	-0.057*
	(0.035)	(0.038)	(0.023)	(0.033)	(0.034)
Democrat Control Average Effect	-0.004	-0.002	0.040	0.011	0.042
	(0.025)	(0.024)	(0.033)	(0.027)	(0.025)
Number of Observations	1060	1016	1060	1060	1016
R2	0.778	0.788	0.827	0.844	0.809
2000 and Onward					
Republican Average Effect	0.082**	-0.065	0.048	0.043	-0.041
	(0.040)	(0.076)	(0.035)	(0.031)	(0.043)
Democrat Average Effect	0.003	-0.030	-0.042	-0.012	0.021
	(0.049)	(0.048)	(0.094)	(0.070)	(0.045)
Number of Observations	420	378	420	420	378
R2	0.869	0.863	0.890	0.880	0.877 ’
Year of Control	1	3	5	7	9

Notes: Each column within a panel presents averages of coefficients from a single regression. Each observations is a state-year. Treatment is unilateral legal control of a political party over redistricting in the year ending in the Year of Control listed at the bottom of the table. Column 1 shows baseline results and columns 2-5 show placebos. The dependent variable is the Republican share of seats from the state. All specifications include state-decade and year fixed effects. The top panel presents estimates over the full sample from 1969-2017. The bottom panel presents estimates from the restricted sample from 1999-2017. Standard errors clustered by state are in parentheses.

Overall, our robustness and heterogeneity tests find substantial support for partisan gerrymandering by the Republican party in recent decades and also for the Democratic party in recent decades only for large states.

We additionally show that following Republican legal control, Republican wasted votes in House elections went down in comparison with wasted votes for the Democrats. We compute the efficiency gap for House elections in presidential years, following Stephanopoulos (2017), and use it for our dependent variable. The wasted votes for a party in a given election is the

sum across districts in the number of wasted votes. If a party loses in the district, the number of wasted votes is the number of votes for the party. If a party wins in the district, the number of wasted votes is the number of votes above what they would have needed to win the district. If a party wins districts narrowly and loses by a large margin, then there will be a small number of wasted votes. However, if a party wins districts by a large margin and loses by a small one, then wasted votes will be very high. A party which gerrymanders should reduce wasted votes for its party and increase wasted votes for the opposing party. We compute wasted votes at the state level by summing wasted votes across districts within states for each party. If the district is sufficiently lop-sided, a race be uncontested in which case there will be no wasted votes for the losing party. In these cases, we follow Stephanopoulos (2017) and compute wasted votes for the district using the vote share for president. For this reason, we restrict our sample to presidential election years when presidential vote shares are available. Our results are presented in

4.6 Aggregate Effects

We have so far estimated the impact of party legal control over redistricting on subsequent seat shares. What has the been the impact of this upon the aggregate balance in the House of Representatives? We now translate our estimates of average seat share impacts by party into aggregate partisan effects and compare them in size to partisan seat margins.

We do this by year and party. In particular, we use decade and party specific estimates for each year for both Democrats and Republicans and compute implied seat share changes, rounding to the nearest seat. We then multiply by the number of treated states and the average number of seats in each treated state. We also note when the changes would have resulted in a shift in the

balance of the House of Representatives. Analytically, we compute:

$$\Delta DemSeats_{y,d} = 2XInt\left[\beta_y^D N_{d,y}^D I\left(Control_d^D\right)\right] - Int\left[\beta_y^R N_{d,y}^R I\left(Control_d^R\right)\right] \tag{4.3}$$

where $\beta_{y,d}^P$ is the effect of party P control on a state's seat share fraction for party P in year$y$ and decade d, $N_{d,y}^P$ is the number of seats in that state's delegation in decade and year, and $I\left(Control_d^P\right)$ is a dummy which takes on a value of 1 if party P has legal control over redistricting in decade d.

We show the results of these computations in Table 4.6. Overall, we find little evidence of a sizable shift in partisan balance in the House of Representatives until the 2000s. Before the 2000s, net effects are no more than five seats. In the 2000s, we compute that seats shifted by 12 seats towards the Republicans and in the 2010s, we see a shift of 27 seats towards the Republican party. The reason for the small net effects through most of the past 50 years but much larger recent effects is due to a combination of two factors. First, the effect of partisan control upon seat shares has increased over time. We can see this in the difference between the effects for the 2000+ period and in the full period. We can also this by looking at the differences in partisan control in the 1970s and in 2000s. Second, state legislatures have shifted from overall Democratic dominance to overall Republican dominance. This is partly due to realignment and the shift of the South of the United States to the Republican party as well as to the poor performance of the Democratic party in the 2010 election which were critical for redistricting. This has been consequential because this switch in dominance has been from a party with a low impact of legal control on seat shares to one with a high impact of legal control on seat shares. For example,

in the 2000s, the Democrats had legal control in two more states than the Republican party. Moreover, the average delegation size in both Republican and in Democratic legal control states was 13. However, because of the greater impact of Republican legal control, our estimates imply a net shift of 12 seats to the Republican party.

Table 4.6: Aggregate Partisan Effects by Decade

	1970s	1980s	1990s	2000s	2010s
States with Dem Control	15	16	17	8	6
Average Seats, Dem Control	8	10	9	13	8
States with Rep Control	7	3	2	6	14
Average Seats, Rep Control	12	14	3	13	12
Seat Share Effect: Dems	1	1	1	1	0
Seat Share Effect: Reps	5	2	0	13	27
Net Effect	4	1	1	12	27
Average Margin	95	86	62	21	53
Net Effect as % of Avg Margin	4%	1%	2%	57%	51%

Each column presents numbers for a particular decade. States with Dem Control and States with Rep Control show the number of states with Democratic and Republican legal control in the decade respectively. Average Seats is the average number of seats after redistricting in states with Democratic and Republican legal control respectively. Seat Share Effect presents a back-of-the-envelope computation of the gross number of seats gained from legal control over redistricting, broken down by party. Net effect is the absolute value of the net change in seats as a result of redistricting. Average margin is the average of the absolute value of the difference between Republican seats and Democratic seats in the Congresses elected in the years ending with 2, 4 and 6 in the decade.

Interestingly, we do not find that greater imbalance in legal control over redistricting plays an important role. In fact, imbalances were much larger earlier in the sample. In the 1970s, Democrats held control in 8 more seats than the Republicans; in the 1980s, this increased to 13 and in the 1990s, it increased to 15. In the past two decades, precisely when legal control has become more consequential for net partisan balance, these gaps have fallen to 2 in the 2000s and 8 in favor of the Republicans in the 2010s.

Overall, we find an increased role for legal control over redistricting in recent decades. This increased impact is driven by two main factors: (1.) the rise in the effect of legal control and (2.)

the switch from the lower impact Democratic party to the higher impact Republican party.

4.7 Conclusion

In this paper, we have shown that parties sometimes act in their own political interest by reshaping districts to increase their party's representation in Congress when they have the power to do so. The estimated size of the effects are large. In the past two decades, Republican control over redistricting has led to an increase of 8.2 percentage points in the average of a state delegation's Republican seat share in the subsequent three elections. We do not, however, find a similar effect of Democratic control except for a small number of large, Democratic states.

It may not be surprising that parties manipulate vote aggregation to benefit themselves. However, there are reasons why they might not. First, there may be a moral sense of fairness in political competition which may restrain parties from engaging in manipulative behavior. Second, parties in non-competitive environments may not feel the need to gerrymander. Third, parties in competitive states may worry about future retribution. Fourth, parties may limit themselves for fear of incurring court involvement in redistricting.[21] Unfortunately, we cannot distinguish between these different motives to the degree they exist. However, they provide unanswered questions for future research. Finally, though currently there is not enough sample size to look at the impact of independent commissions using our methodology, given the increasing numbers of parties who have switched to independent or bipartisan commissions, future research on their efficacy would be of great interest.

[21] The reasons behind why partisan legal control sometimes leads to partisan redistricting and sometimes does not are both interesting and amenable to empirical analysis but beyond the scope of the current paper.

4.8 Data Appendix

We compile a novel data set on the legal rules that states use to create Congressional district lines from 1968 to 2012. We coded types of legal systems for redistricting across states over 5 decades. We grouped state-decades into one of six categories: (1.) Single district states not eligible for redistricting, (2.) States where redistricting bills are passed by state legislatures and are not subject to a Gubernatorial veto, (3.) States where redistricting bills are passed by state legislatures but where the Governor has veto rights, (4.) States where potentially-partisan advisory commissions (i.e. commissions that are not appointed in a bi-partisan or non-partisan manner) draw the maps but the legislature needs to pass a redistricting bill in order for it to become law, (5.) States where advisory commissions, appointed in a non-partisan or balanced partisan manner, draw the maps but the legislature needs to pass a redistricting bill in order for it to become law, and (6.) States with an independent commission which is appointed in a non-partisan or bi-partisan manner and which has the legal authority to implement a redistricting plan without legislative or gubernatorial approval.

In the 2000+ time period, we rely upon descriptions from Justin Levitt's website: `https://redistricting.lls.edu/2010districts.php`. In the pre-2000 period, we rely upon a combination of sources. First, the National Conference of State Legislatures has documented all historical commissions. Second, we rely upon state legislative documents for each non-single-district state. Third, we rely on law.justia.com. Finally, we also make use of academic articles in some cases. Our sources are documented in greater detail in: `https://docs.google.com/spreadsheets/d/1nZuugxJe09PfCHVIsLyXjGx5cnlKTVnvtDYivDNFdiM/edit?usp=sharing`.

In this document, we point out general patterns, a few anomalies and coding decisions. Most states are of the legislative + gubernatorial veto type. Only Connecticut and North Carolina do not allow for a Gubernatorial veto. In addition, two states, Connecticut and Maine, set a 2/3 majority threshold for passage of a redistricting bill. Five states are one-district states throughout the five-period decade spanning our data. Two others, Montana and South Dakota, start as 2-district states and change to a 1-district state during our time span, while Nevada starts as a 1-district state and eventually reaches 4-districts in our time span. Some states transition to commission states during the time period spanned by our data. However, no states revert from a commission back to legislative redistricting. Montana does transition from a commission state to a 1-district state. For our main specification, we code any state with a commission of any type (4, 5 or 6) as not having legal control by either party. We show robustness to redefining commissions of type 4 as under partisan legal control depending upon the composition of the state legislature in Appendix Table 1.

For all states, we estimate an intention to treat estimate. Thus, we code based upon the law for the decade that was in place in years ending in 1 when redistricting normally happens. Hawaii, in 1968, passed a constitutional amendment which called for redistricting in 1969, 1973 and then every ten years starting in 1981. It also called for a commission system as of 1973. We thus code Hawaii in the 1970s as a commission state.

Table D.A1: State-Level Congressional Redistricting Laws By Decade

State	1970s	1980s	1990s	2000s	2010s
Alabama	3	3	3	3	3
Alaska	1	1	1	1	1
Arizona	3	3	3	6	6
Arkansas	3	3	3	3	3
California	3	3	3	3	6
Colorado	3	3	3	3	3
Connecticut	2	2	2	2	2
Delaware	1	1	1	1	1
Florida	3	3	3	3	3
Georgia	3	3	3	3	3
Hawaii	6	6	6	6	6
Idaho	3	3	3	6	6
Illinois	3	3	3	3	3
Indiana	3	3	3	3	3
Iowa	3	5	5	5	5
Kansas	3	3	3	3	3
Kentucky	3	3	3	3	3
Louisiana	3	3	3	3	3
Maine	3	5	5	5	5
Maryland	3	3	3	3	3
Massachusetts	3	3	3	3	3
Michigan	3	3	3	3	3
Minnesota	3	3	3	3	3
Mississippi	3	3	3	3	3
Missouri	3	3	3	3	3
Montana	6	6	1	1	1
Nebraska	3	3	3	3	3
Nevada	1	3	3	3	3
New Hampshire	3	3	3	3	3
New Jersey	3	3	3	6	6
New Mexico	3	3	3	3	3
New York	3	4	4	4	4
North Carolina	2	2	2	2	2
North Dakota	1	1	1	1	1
Ohio	3	3	4	4	4
Oklahoma	3	3	3	3	3
Oregon	3	3	3	3	3
Pennsylvania	3	3	3	3	3
Rhode Island	3	3	3	3	4
South Carolina	3	3	3	3	3
South Dakota	3	1	1	1	1
Tennessee	3	3	3	3	3
Texas	3	3	3	3	3
Utah	3	3	3	3	3
Vermont	1	1	1	1	1
Virginia	3	3	3	3	3
Washington	3	3	6	6	6
West Virginia	3	3	3	3	3
Wisconsin	3	3	3	3	3
Wyoming	1	1	1	1	1

Note: Numbers represent different legal systems for redistricting: 1: Single District - The state was apportioned a single congressional district and thus there was no need for districting. 2: Legislature Only - The State Legislature has full control over the redistricting process with no possibility of a Gubernatorial veto. 3: Legislature and Governor: The State Legislature is in charge of developing a Congressional Redistricting plan but the Governor has veto rights. 4: Advisory Commission: An advisory commission draws redistricting maps and presents them to the legislature for passage. Advisory commissions of this type are appointed in a manner that lacks partisan balance. 5: Non-Partisan Advisory Commission: An advisory commission which is appointed in a non-partisan manner or on a bi-partisan basis so as to maintain partisan balance on the commissions. 6: Independent Commission - Independent commissions are appointed on a non-partisan basis and have the legal authority to draw and implement a redistricting plan without gubernatorial or legislative approval. For the 2000s and 2010s redistricting cycles data was collected from a website by Justin Levitt. For the 1980s and 1990s cycles the majority of the data came from court cases whose summaries were aggregated by the National Conference of State Legislatures website. The full documentation of the cases were then examined, often via law.justia.com. For the 1970s redistricting cycle, a variety of sources were used. The primary ones were state specific sites either documenting the history of redistricting in the state or documenting historical state constitutional amendments as well as a paper on the 1970s redistricting cycle in which the processes were characterize

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