Forward Looking Congress? Evidence from Redistricting Announcements*

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Abstract

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 this paper 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 \$1.15-\$1.80 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.

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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. 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

[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.

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 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.

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, 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].

[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.

[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,

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 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 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 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.

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.

Table 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

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].

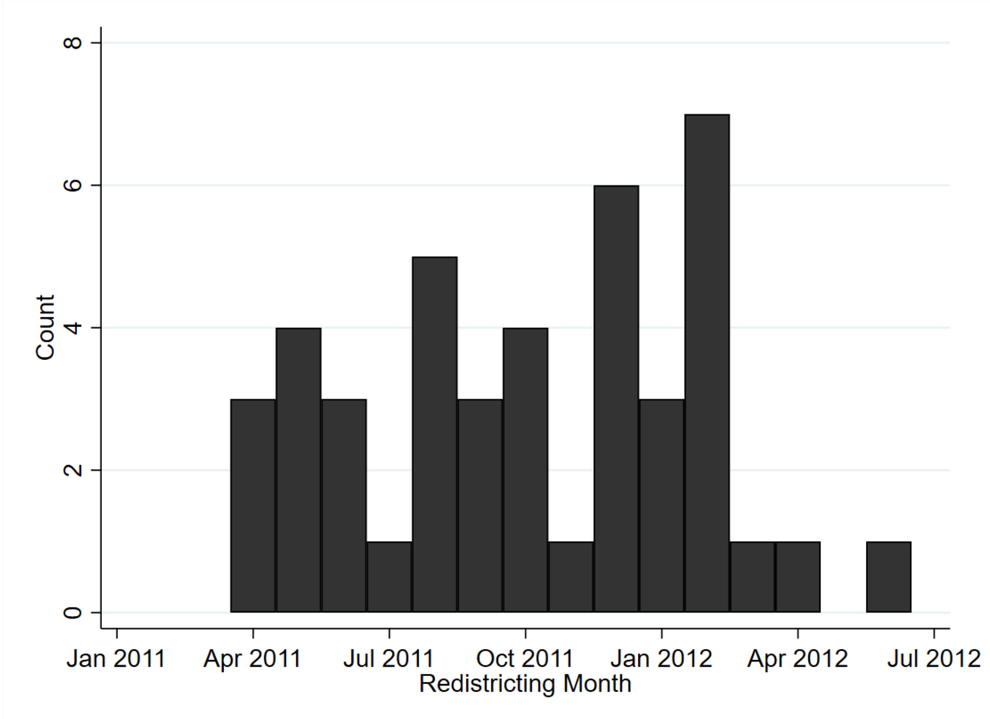
Figure 11, shows the distribution of the timing of the redistricting announcements. These announcements occur between April of 2011 and April of 2012. Most states plans go into effect before the next calendar year.

[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.

Figure 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 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 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.

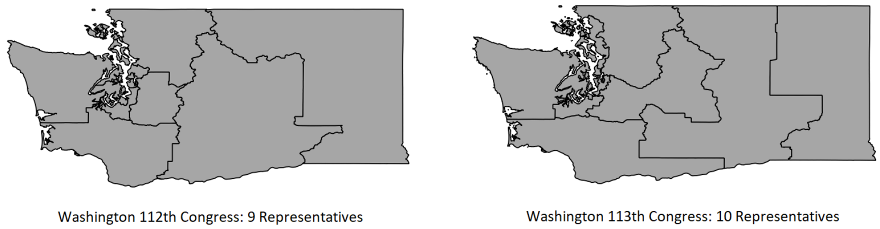
4.1. *Creating Sub Congressional District Geographies*

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, 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 3. I refer to the resulting geographies as district segments. Each district segment has 2 components, a district assignment 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.

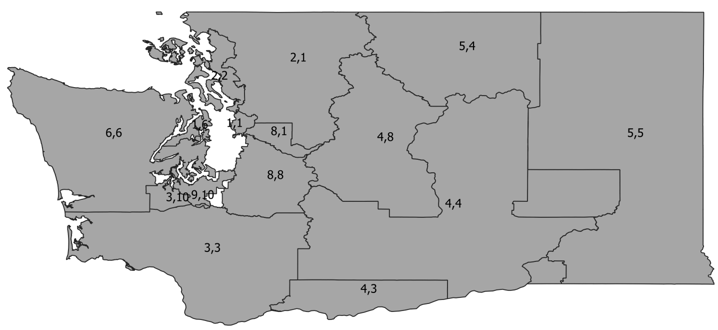
[10] FAADS does not directly produce machine readable data. Instead, a customized piece of software is required to read a sequence of text files. Bickers and Stein (1991, 1995) created an annualized version of these data which collapse the data into congressional districts.

Figure 2: 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.

Figure 3: Washington State District Segments



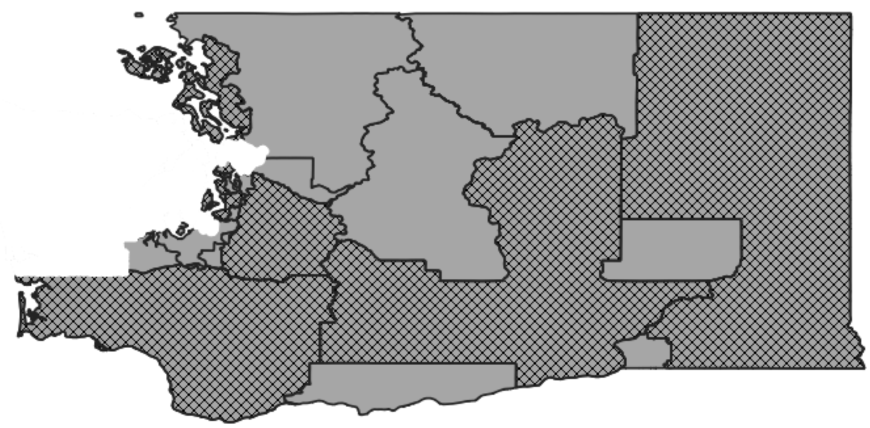
Notes: Figure contains an example of Washington states district segments, the unit of analysis. District segments are constructed by intersecting the congressional districts from the 2010 election and the 2012 election. Each district segment is labeled by its pre-redistricting district number followed 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 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.

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.

Figure 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.

Successful results are returned for upwards of 85% of addresses. The majority of unmatched addresses are the result of incomplete data. In table 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.

Table 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.

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 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).

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.

5.1. *Experimental Details*

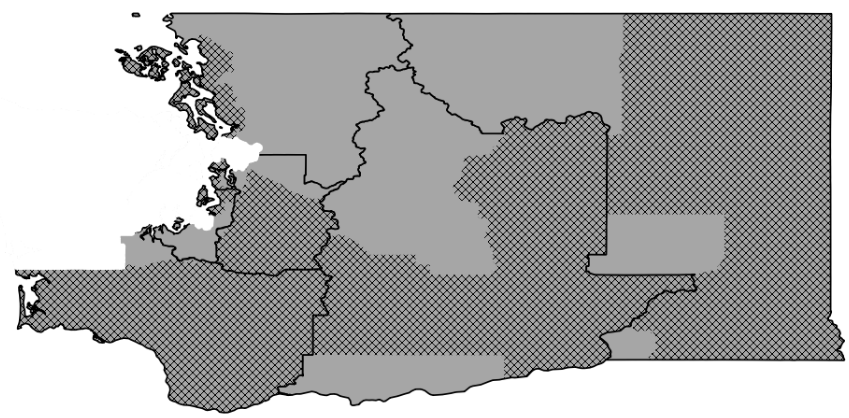
The natural experiment exploits a sharp change in electoral benefits after a redistricting announcement is made. I construct a treatment group, which is the group of district segments where the politician wins office in 2010 and runs for office in 2012. I refer to these regions as "carry-over segments," 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 5 builds on the example from section 4.1. Areas which are hashed are carry-over

[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.

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 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.

5.2. *Initial Specifications*

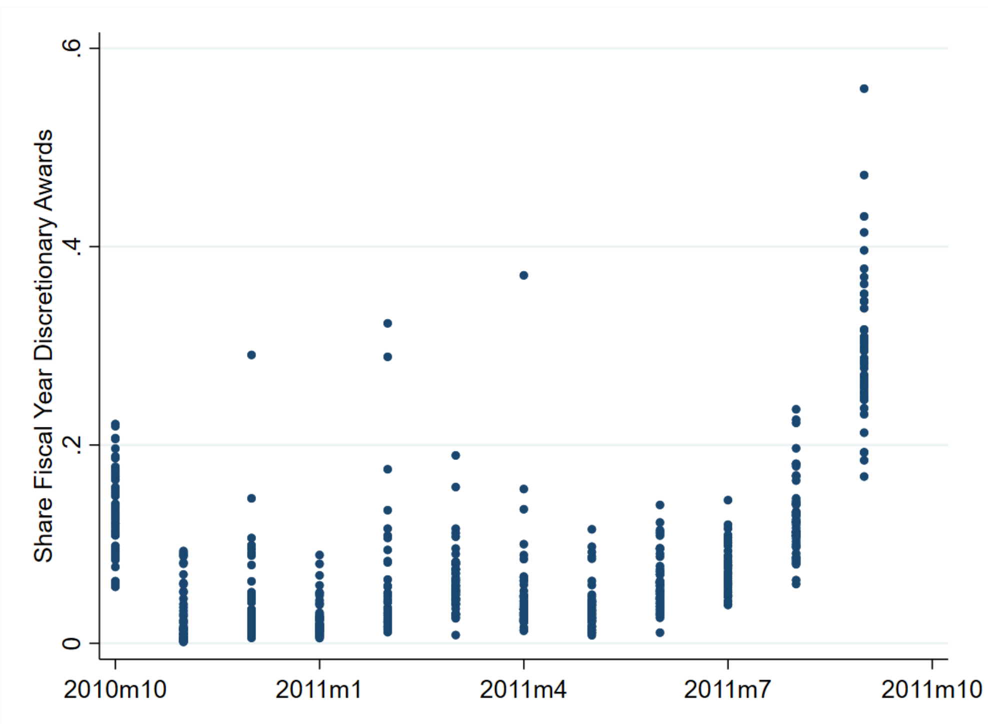
The structure of this design fits well in a difference in difference framework, comparing carry-over to outgoing segments. Using a two-way fixed effects model controls for time-invariant effects across segments and national time-trends.

$$y_{st} = \alpha + \beta C_{st} + \lambda_s + \lambda_t + \epsilon_{st} \tag{1}$$

Equation 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 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 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. 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,

[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.

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.

$$y_{s112t} = \alpha + \beta C_{st} + \lambda_s + \lambda_t + \lambda_{112t} + \epsilon_{s112t} \tag{2}$$

$$y_{s113t} = \alpha + \beta C_{st} + \lambda_s + \lambda_t + \lambda_{113t} + \epsilon_{s113t} \tag{3}$$

Equations 2 an 3 introduce these fixed effects. Equation 2 is indexed by 112, which refers to the congressional district from the 112th congress (elections held in 2010) and equation 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, 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 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.

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 4 and 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.

$$
\begin{aligned}
Y(O) &= Y(O)_{pre} + Y(O)_{post} \\
Y(O)_{pre} &= For^{A}_{pre}(O) + Back^{A}(O) + Endowment \\
Y(O)_{post} &= For^{B}_{pre}(O) + Back^{A}(O) + Endowment \\
Y(O) &= For^{A}_{pre}(O) + For^{B}_{post}(O) + Back^{A}(O) + Endowment
\end{aligned} \tag{4}
$$

$$
\begin{aligned}
Y(C) &= Y(C)_{pre} + Y(C)_{post} \\
Y(C)_{pre} &= For^{A}_{pre}(C) + Back^{A}(C) + Endowment \\
Y(C)_{post} &= For^{A}_{post}(C) + Back^{A}(C) + Endowment \\
Y(C) &= For^{A}_{pre}(C) + For^{A}_{post}(C) + Back^{A}(C) + Endowment
\end{aligned} \tag{5}
$$

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.

$$
\begin{aligned}
Y(C) - Y(O) &= [For^{A}_{pre}(C) + For^{A}_{post}(C) + Back^{A}(C) + Endowment] - \\
&\quad [For^{A}_{pre}(O) + For^{B}_{post}(O) + Back^{A}(O) + Endowment] \qquad (6)\\
Y(C) - Y(O) &= [For^{A}_{post}(C) - For^{B}_{post}(O)]
\end{aligned}
$$

Equation 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 7 outlines the incentive structure in these districts.

$$
\begin{aligned}
Y(I) &= Y(I)_{pre} + Y(I)_{post} \\
Y(I)_{pre} &= For^{B}_{pre}(I) + Back^{B}(I) + Endowment \\
Y(I)_{post} &= For^{A}_{pre}(I) + Back^{B}(I) + Endowment \\
Y(I) &= For^{B}_{pre}(I) + For^{A}_{post}(I) + Back^{B}(I) + Endowment
\end{aligned} \tag{7}
$$

The difference in these segments is that politician B represents the comparison region prior to redistricting, changing the politician exerting backward effort. Equation 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.

$$
\begin{aligned}
Y(C) - Y(I) &= [For^{A}_{pre}(C) + For^{A}_{post}(C) + Back^{A}(C) + Endowment] - \\
&\qquad [For^{B}_{pre}(I) + For^{A}_{post}(I) + Back^{B}(I) + Endowment] \\
Y(C) - Y(I) &= [For^{A}_{post}(C) - For^{A}_{post}(I)]
\end{aligned} \tag{8}
$$

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.

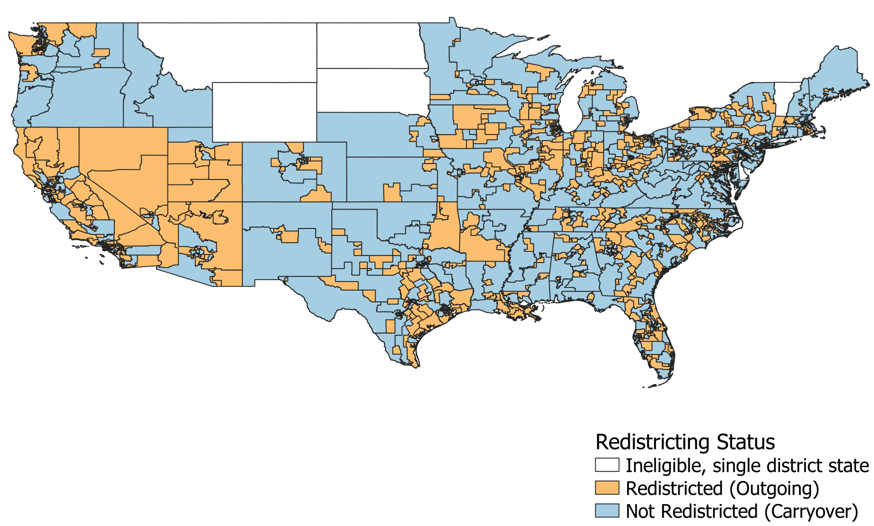
5.4. *Balance and Geographic Restrictions*

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.

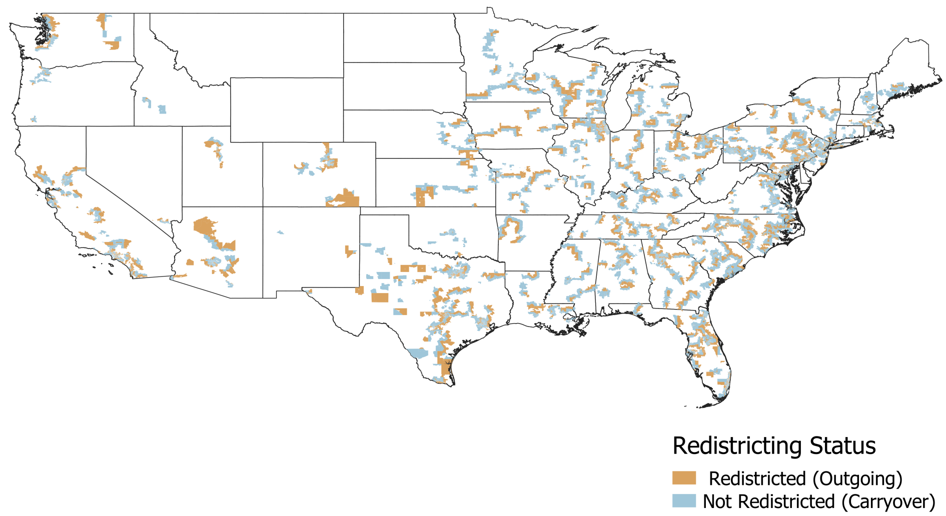
The redistricting experiments creates two classes of segments, carry-over and out-going. In order to further improve observable balance, I impose geographic restrictions that limit the size of areas of interest. This restriction not only adjusts for an asymmetry in the size of segments, but will also 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 7 and 8 display the geographic distribution of redistricting assignment for the entire country and when imposing a 10 mile restriction.

Figure 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 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.

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 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 5.4 summarizes the distribution of federal awards by distance bandwidths.

Tables 4 and 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

[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

Table 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.

the difference between the carry-over and outgoing means never being significant at the 10% level.

In table 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

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.

Table 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 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.

are likely stable [14]. Despite this, I elect to show results for using data from both the full

[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 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

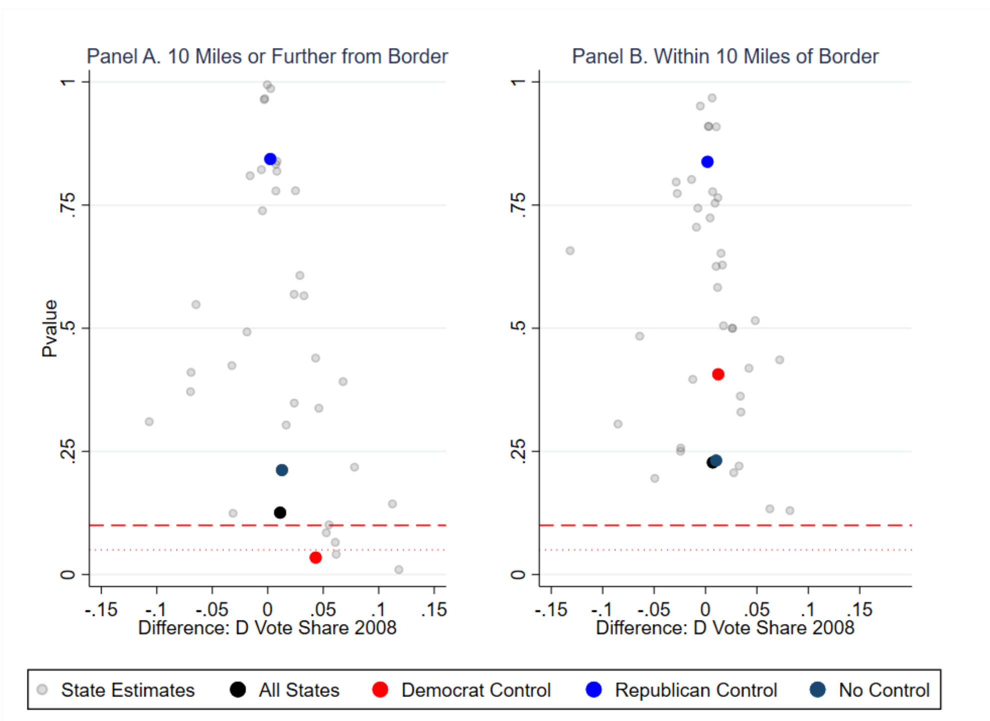
Table displays the results of a joint significance test with treatment status as the outcome of interest. Column 1 shows results with no geographic restriction, column 2 includes a 100 mile geographic restriction, column 3 includes a 30 mile geographic restriction, and column 4 includes a 10 mile geographic 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.

sample, as well as with a 10 mile restriction.

In figure 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 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. 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 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.

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. Second, I remove the smallest 1% of geographies by land-mass. This removes areas that are smaller than 0.25 square miles. In figure 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.

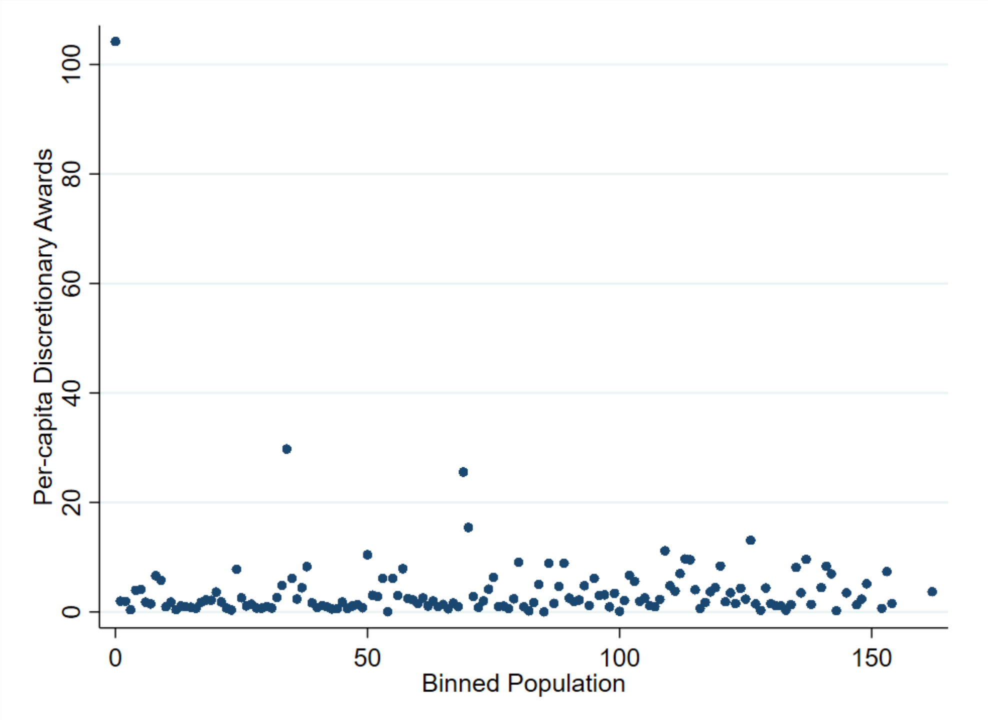
The final sample includes district segments from 369 of the 435 congressional districts from 40 states. I use federal awards in a 22 month window around the 112th congress, which goes from January 2011 until October of 2012. Politicians terms are effective until the beginning of 2013, however I end the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

[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

Figure 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.

Table 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.

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

Table 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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 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 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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.

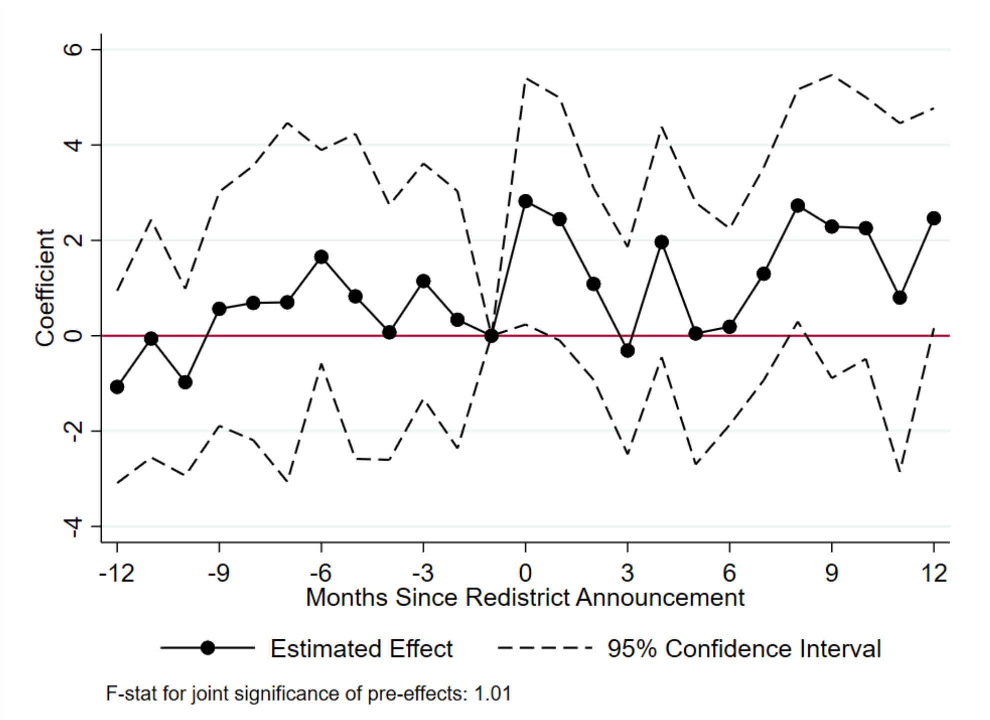
To test for parallel trends, I utilize an event study framework. Equation 9 shows the estimated equation. I regress per-capita awards on a series of leads and lags of the carry-over status of a district segment. The estimation excludes the period just before a redistricting announcement is made, which forms the baseline group. I estimate this relationship for project grants.

$$y_{s112t} = \alpha + \lambda_s + \lambda_{112t} + \sum_{j=2}^{J} \beta_j (Lag_j)_{st} + \sum_{k=0}^{K} \beta_j (Lead_k)_{st} + \epsilon_{s112t} \tag{9}$$

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 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.

Figure 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.

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 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.

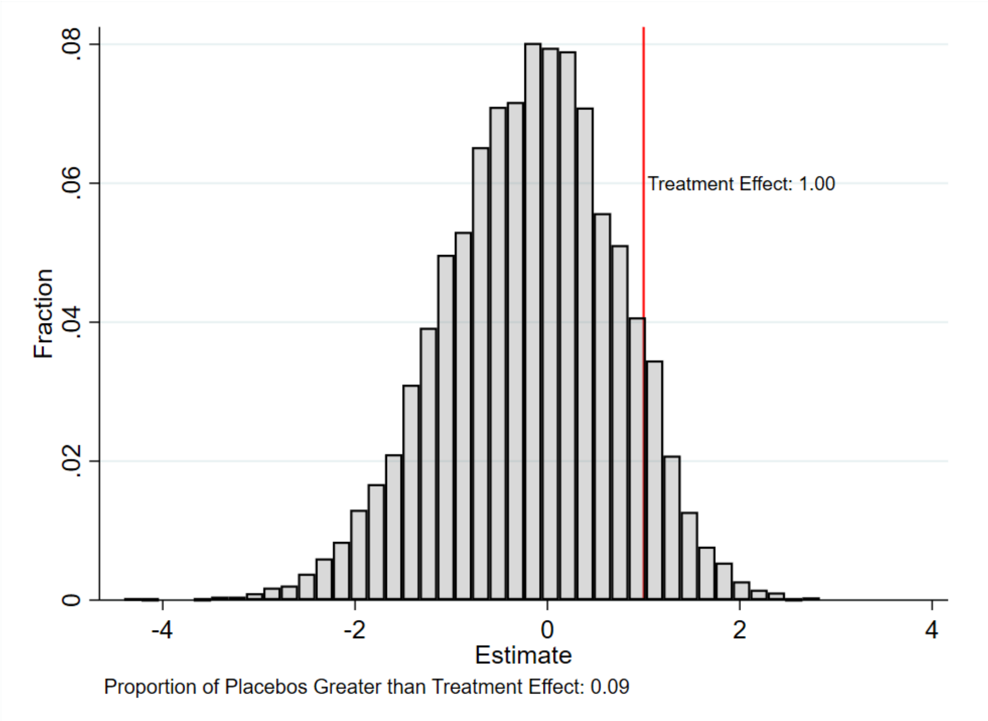
Figure 12 displays the distribution of the coefficients for this test. I find little evidence of anticipatory effects, with at least 91% of the experiments having estimated effects lower than the treatment effect.

The other identifying assumption is that the results are not due to the effort by the incoming politician. In section 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

Figure 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.

are 780 comparison district segments, of which 134 do not have an incoming politician.

In table 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.

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 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

Table 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

redistricting announcement, and repeat the regressions of interest.

In table 10, 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 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 re-

[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 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

districting 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 13, 14, and 15, I show the distribution of coefficients from these results. In each figure, I show results for per-capita project grants. In figure 13, I show results using only the 2013-2014 period, figure 14 using 2015-2016, and figure 15 pools the placebo periods together and jointly estimates the 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 13: Redistricting Period Placebos: 2013-2014

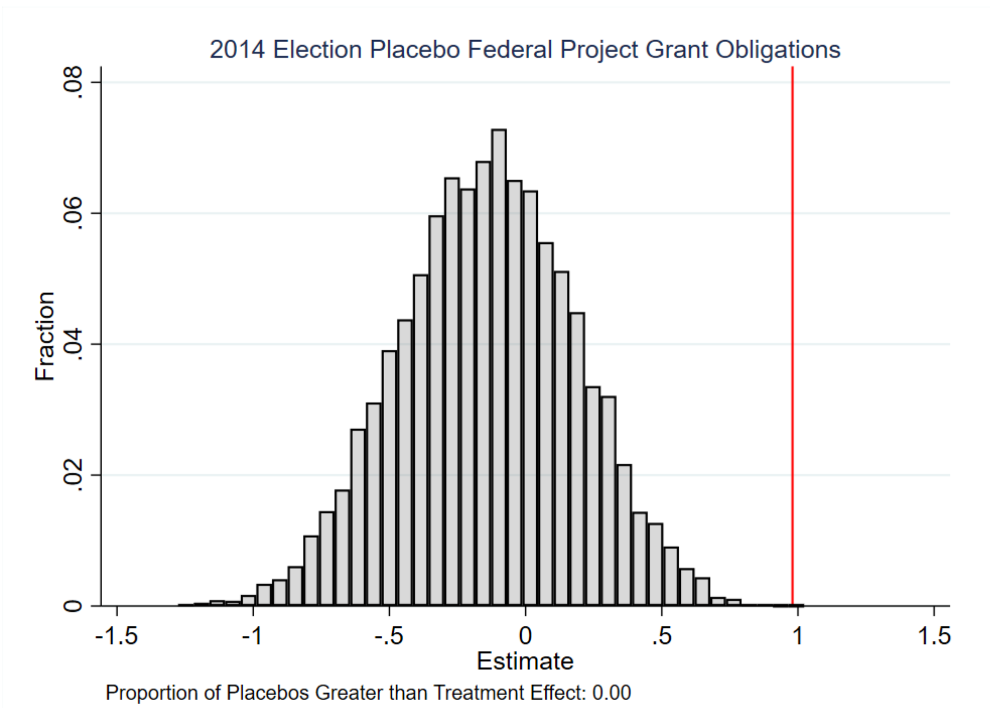


Figure plots the distribution of coefficients from 10,000 placebo regressions. Each placebo regression includes periods from January 2013 until the November 2014. Each regression uses a randomly generated redistricting passage date within April 2013 to June 2015 and and maintains the redistricting assignment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

Figure 14: Redistricting Period Placebos: 2015-2016

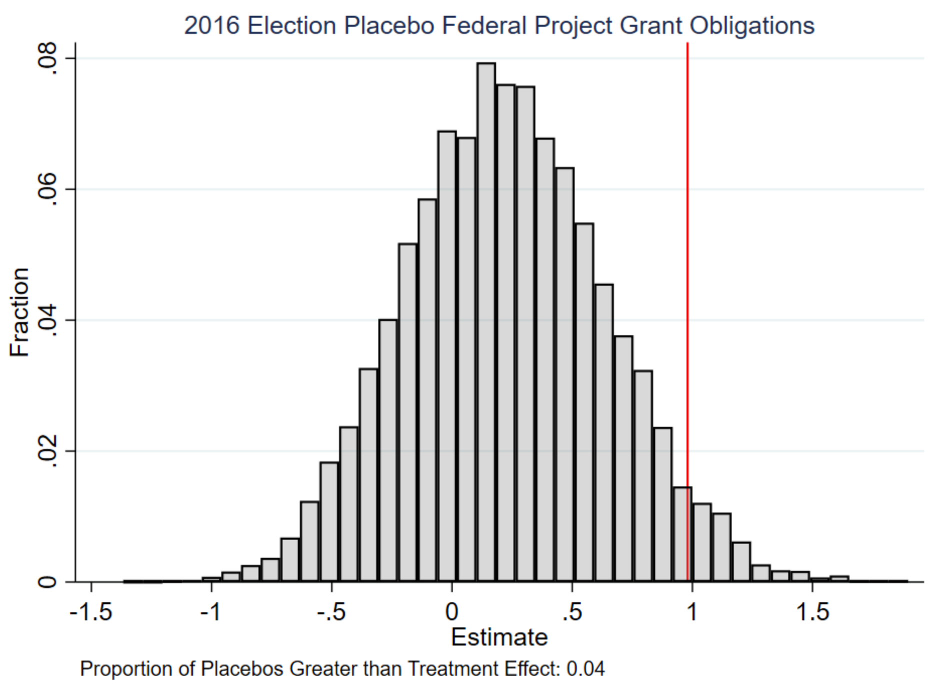


Figure plots the distribution of coefficients from 10,000 placebo regressions. Each placebo regression includes periods from January 2015 until the November 2016. Each regression uses a randomly generated redistricting passage date within April 2015 to June 2016 and and maintains the redistricting assignment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

Figure 15: Redistricting Period Placebos: Pooled 2013-2016

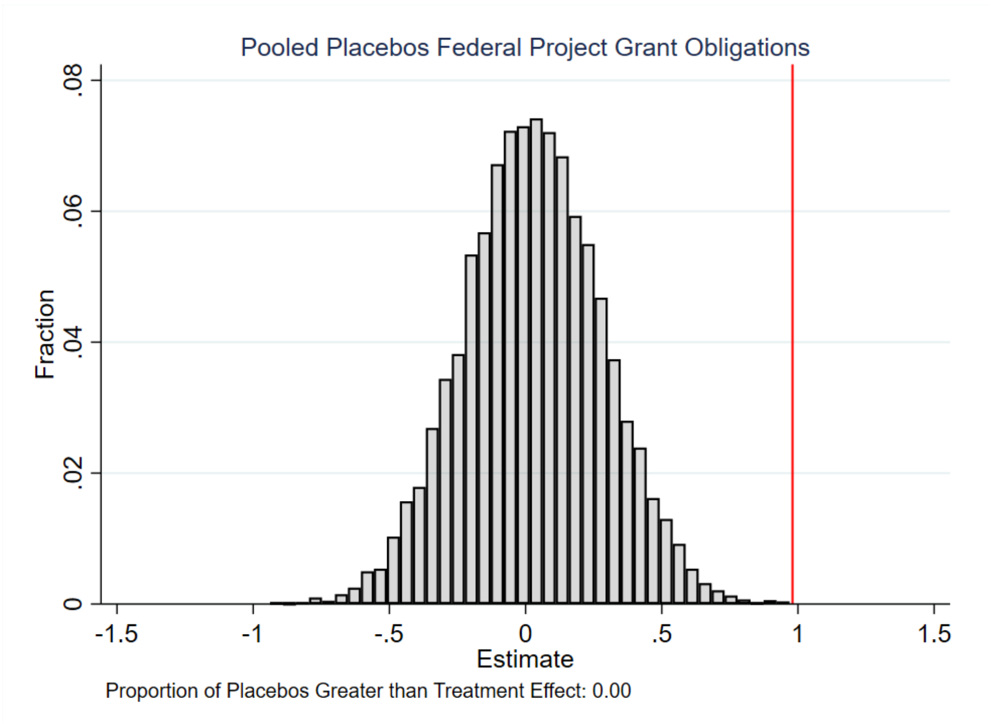


Figure plots the distribution of coefficients from 10,000 placebo regressions. Each placebo regression includes periods from January 2013 until the November 2016. Results from 2013-2014 and 2015-2016 are pooled. Each regression uses a randomly generated redistricting passage date within April 2013 to 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.

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.

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