

What did the Earmark Ban Do? Evidence from Intergovernmental Grants

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Abstract

Critics of the 2011 congressional earmark ban argue that legislators have simply replaced earmarking with other means of directing federal funds to their home districts. I estimate the impact of the ban using data on federal competitive grants to state and local governments. Because several earmark reforms predated the 2011 ban, I test for a break in the relative trends between regions receiving more earmarks versus those receiving fewer earmarks. My results indicate that for grants to local governments, the 2011 ban had no effect. For grants to state governments, I find that the earmark ban of 2011 may have had an impact; I find statistically and economically significant negative effects, suggesting that the earmark ban may have altered the distribution of intergovernmental grants to state governments. I also show that a large bias in earmarks existed between high and low income districts, suggesting that the earmark ban potentially improved equality.

1 Introduction

“I will do everything I can to protect my district.”

–Peter T. King, (R-NY)

“I’ll be making more phone calls, writing more letters, arranging more meetings and doing whatever I possibly can. That’s the way it’s going to be done...now that they have eliminated these earmarks.”

–Maurice Hinchey, (D-NY)¹

Earmarking is the process by which members of Congress redirect funds already allocated to federal agencies. Most frequently inserted into the report language accompanying appropriations bills, earmarks alter the way projects are funded by agency grant programs and, until recently, have seen rapid growth. Totalling less than \$3 billion in 1991, by 2006, earmarks had increased to a historic high of nearly \$30 billion before falling to zero with the advent of the 2011 congressional earmark ban (Citizens Against Government Waste, 2016).

This paper tests whether legislators were able to find means other than earmarking to re-allocate federal grant dollars and thus circumvent the 2011 earmark ban. Utilizing the wide geographic variation in earmarked federal intergovernmental grants across U.S. states and congressional districts prior to the ban, the empirical strategy employed in this paper identifies the impact of the ban by comparing changes in the differential trends in total grants between more and less heavily earmarked regions for the years before and after 2011. Because legislators and federal grant program administrators often have conflicting preferences over the distribution of funds, if the earmark ban was effective, then it is likely to have left a lasting legacy on the financing of local projects in the public sector.

There are several reasons, however, why the earmark ban might not have had an impact on the distribution of federal funds. Legislators who used earmarking before the ban may have found alternative ways to allocate

¹See Hernandez (2011).

funding to their home districts after the ban. Congressional members may request that agencies fund specific projects by contacting them directly, a practice known as “lettermarking.”² Additionally, the earmark ban may have had a heterogeneous impact across legislators. Members of the House and Senate Appropriations Committees who control the appropriations (and thus the earmarking) process may have been able to use their influence to get around the ban. It is also possible that legislators on other committees might have altered authorizations bills to disproportionately fund programs that benefit their home districts at the expense of others.

Statements made by legislators shortly after the 2011 ban implied a sense of skepticism regarding its impact.³ Moreover, critics in the media and elsewhere have since argued that the 2011 earmark ban has merely exacerbated the lack of budgetary transparency in Washington by allowing members of Congress to direct federal funds to their home districts in ways that are impossible to track (Lipton and Nixon, 2010; Nixon, 2012; Cuellar, 2012; Gold, 2015; Dawson and Kleiner, 2015; Strand and Butcaru, 2016).

While a recent survey shows that public opinion continues to support the ban (Egger, 2016), a proposal to exclude Army Corps of Engineers projects from the earmark ban enjoyed bi-partisan support in a September 2016 hearing by the House Rules Subcommittee (C-SPAN, 2016).⁴ If the 2011 earmark ban prevented legislators from directing spending to their home districts, then the outcome of the current debate may have a non-trivial impact on the overall distribution of federal funds.

To date, there has been no work documenting whether the earmark ban of 2011 actually restricted the ability of legislators to allocate funds to their home districts. In an analysis of earmarks in intergovernmental grant programs, I find no evidence that the 2011 earmark ban impacted the distribution of federal competitive grants to local governments. My results suggest that for local governments, federal grants began falling in more heavily earmarked congressional districts relative to less heavily earmarked congressional districts before the ban of 2011 took effect.⁵ A separate analysis of federal grants to state governments, however, suggests that the 2011 ban was effective and resulted in a decline of between 1.9 and 3.9 percent in grants for each percentage point in grants earmarked. These results are robust to using alternative measures of the percent of grants earmarked.

As a stylized fact, earmarks have frequently distributed large sums to individual recipients. I document that the average earmark awarded to a local government in 2009 was \$517,000. This is a significant amount considering that own-source revenues of the typical local government were \$17.2 million around the same time.⁶

Given that earmarks represent geographically concentrated benefits, an additional question is: Does earmarking create inequalities across demographic groups? While research on earmarks documents strong correlations between legislator attributes and earmarks (Knight, 2008; Boyle and Matheson, 2009; Crespin et al., 2009; Engstrom and Vanberg, 2010; Clemens et al., 2015), no existing literature addresses the question of whether earmarking benefits certain demographic groups at the expense of others. I find that in 2009, the top 5th percentile of congressional districts in terms of personal income per capita received 6.1 percentage points more in earmarks as a percent of total federal grants relative to the bottom 5th percentile of congressional districts, and that this difference is statistically significant at the 99 percent level. Interestingly, this difference is far larger than the difference between districts represented by more powerful legislators

²Lettermarking operates in much the same spirit as earmarking; it is a threat to agencies to fund particular projects by a legislator. Since earmarks were generally not written into law but in the accompanying appropriations conference reports, they were technically not legally binding. However, agencies who would deny them risked having their budgets cut the following fiscal year. See: Dawson and Kleiner (2015).

³For example: “*The appropriators are going to be okay...[W]e know people in agencies.*” -Jim Moran (D-VA) See: Strauss (2011); Hernandez (2011).

⁴The proposal was put forth by Tom Rooney (R-FL) who stated: “...we can’t do anything for our own constituents that pay federal tax dollars and expect us to get things done for them.” (C-SPAN, 2016)

⁵As explained in more detail below, I focus on competitive grants as opposed to formula grants because these programs are most commonly earmarked. I analyze intergovernmental grants because governments can be pinpointed to a fairly precise geographic area, while firms and non-profits often have operations in locations remote from their headquarters. I also include federal contract awards.

⁶Data on earmarks come from the Office of Management and Budget (OMB) earmark database. As discussed below, these numbers exclude earmarks that were part of the American Recovery and Reinvestment Act (ARRA). Data on local government revenues come from the 2007 Census of Governments. For comparison, average own-source revenues were just under \$18 million as of the 2012 Census of Governments.

relative to those represented by less powerful legislators. Because wealthier districts have larger tax bases with which to fund public infrastructure projects, this evidence suggests that the earmark ban helped to increase equality in the distribution of federal funds.

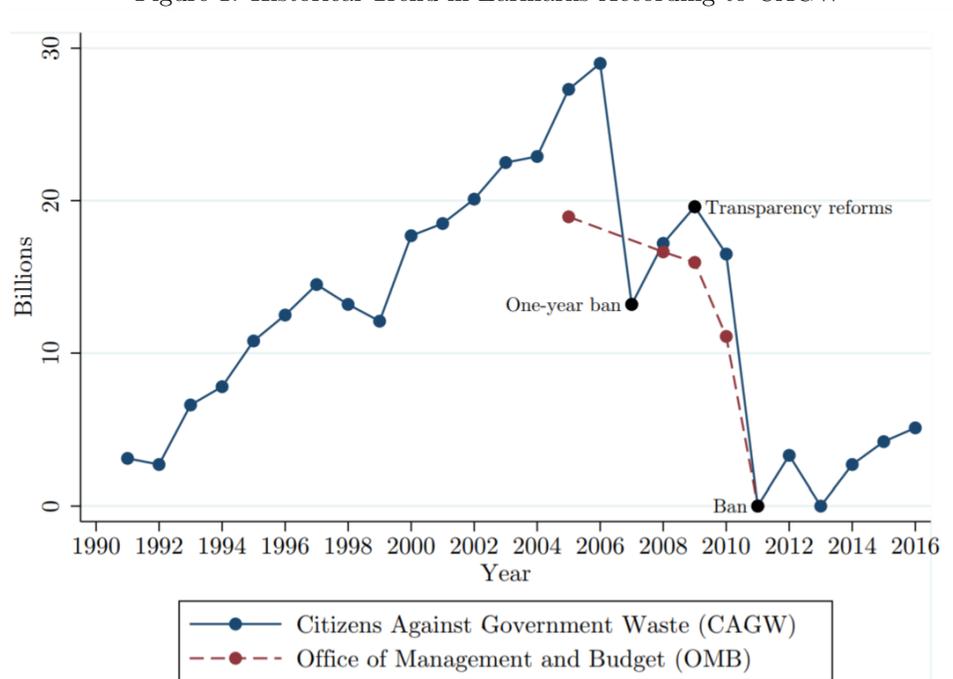
The remainder of the paper is structured as follows: Section 2 describes the background on the earmark reforms and ban, and the data, Section 3 presents the empirical model, including results, and Section 4 concludes.

2 The Impact of the 2011 Earmark Ban

2.1 Background

Before the 2011 ban, earmarks were used by legislators in both the House and the Senate to override the geographic allocation of federal funds made by agencies. Figure 1 displays data from CAGW that document a rapid decline of earmarks to zero in 2011, and consistently low levels thereafter.

Figure 1: Historical Trend in Earmarks According to CAGW



Over the period of 2006 to 2010, both the House and the Senate enacted a series of reforms aimed at providing a greater degree of transparency to the earmarking process. Most notable were the 2009 committee-led transparency reforms of the House Appropriations Committee (HAC) and Senate Appropriations Committee (SAC) that required all earmark requests, not just those funded, to be posted on members' websites as opposed to committees' websites. The HAC reforms were more stringent than the SAC reforms because they mandated that all requested earmark information be tabulated by subcommittee bill. The HAC also imposed an additional requirement that agencies be given 20 days to review earmark requests in order to "check that the proposed earmark is eligible for funding and meets goals established in law."⁷ ⁸ Following the GOP victories in the 2010 mid-term elections, Republican-led efforts to ban earmarks ultimately resulted in both chambers adopting moratoriums.⁹

⁷See: U.S. House Committee on Appropriations (2009).

⁸See Doyle (2011) for an encyclopedic description of the earmark reforms over the 2006-2010 period.

⁹See White (2014) for a complete description of the proceedings around the 2011 earmark ban.

While it is clear that some politicians used banning earmarks as a campaign platform, an obvious question is to ask why legislators would give up their earmarking power in the first place, especially in light of the fact that this ability allowed the legislative branch an advantage over the executive branch.¹⁰ ¹¹ Furthermore, legislators in favor of earmarking often argue that they are better equipped to attend to local needs than agency officials, framing earmarks as welfare enhancing rather than an exercise of political power.¹² Regardless, earmarking represents a quantifiable budgetary tension between the legislative and executive branches.

Given that the ban and the transparency reforms leading up to it were self-imposed, the 2011 earmark ban presents a conundrum for the theoretical and empirical literature on distributive politics. One plausible explanation is that the ban was a move to placate public unrest, fomented by increased budget deficits (White, 2014).¹³ ¹⁴ It is also possible that the ban resulted from increased public scrutiny regarding the earmarking process.

In a 2007 report, the U.S. Department of Transportation (DOT) Office of Inspector General found that 9 out of 10 projects earmarked in a Federal Aviation Administration (FAA) grant program for airport facility improvements would not have been funded without being earmarked and that the inclusion of these projects caused higher priority projects to be delayed by 3 years or more (Office of Inspector General, 2007).¹⁵

The DOT report, in addition to political pressure, led to a 2008 Bush Administration executive order that directed the Office of Management and Budget (OMB) to collect information on earmarks including individual amounts, sponsors, and the locations of recipients (Brass et al., 2007). In their examination of appropriations legislation, the OMB defined an earmark as:

“...funds provided by the Congress for projects, programs, or grants where the purported congressional direction (whether in statutory text, report language, or other communication) circumvents otherwise applicable merit-based or competitive allocation processes, or specifies the location or recipient, or otherwise curtails the ability of the executive branch to manage its statutory and constitutional responsibilities pertaining to the funds allocation process” (Office of Management and Budget, 2009).

In addition to the Bush Administration executive order, Congress enacted a series of reforms including earmark disclosure rules added in late 2006, a 1-year moratorium in 2007, and additional transparency requirements in 2009.¹⁶

Despite the reforms, lawmakers continued to request earmarks at significant levels up until 2011, when Congress finally imposed an all encompassing ban on earmarking. Figure 1 displays data from government watchdog group Citizens Against Government Waste (CAGW) that show earmarks falling to zero in 2011.¹⁷ Following 2011, CAGW discovered appropriations that met their earmark classification criteria which resulted in some positive amounts of earmarks after the ban (Citizens Against Government Waste, 2016).

¹⁰For example, Senator McCain. See: Doyle (2011).

¹¹Earmarking has been practiced by both the legislative and executive branches, thus the earmark reform can be seen as a shift in the balance of power towards the executive branch.

¹²Consider Senator Richard Durbin’s (D-Ill.) comment: *“I think that what we need to do is have the Obama administration say, ‘We are looking for local impact, local input on projects and we will give great weight or at least weight to these recommendations.’ And I think that only makes sense. Because, to think that somebody sitting at a desk in Washington, D.C., can appreciate that opportunity down in the Metro East area – I’m not sure they could.”* See: Stein (2014).

¹³In a theoretical model (see online appendix), I show how a shift in “constituent preferences” can lead legislators to optimize by self-imposing an institutional constraint limiting the proposer’s ability to allocate shares of the federal budget across districts.

¹⁴Put another way, legislators can be thought of as behaving in a way to maximize expected utility over their entire career, not just over a single appropriations cycle (Weingast, 1979; Diermeier et al., 2005).

¹⁵Additionally, agencies have sometimes refused to fund earmark requests altogether. Evidence of this can be found in the recently released report on unobligated balances of earmarks for Department of Transportation funds to states. See: American Association of State Highway and Transportation Officials (2012, 2016).

¹⁶See Section II. below for a description of the earmark reforms leading up to the 2011 ban.

¹⁷CAGW uses a different definition in classifying earmarks than the OMB. Also, the CAGW earmark data do not allow for earmarks to be traced to their actual recipients, and thus cannot be included in the analysis below. They do, however, provide a way to observe the general trend in earmarks over a longer time period considering that the OMB data were collected only for 2005, 2008-2010.

2.2 Relevant Literature

Despite the pervasiveness of earmarking from the early '90s up until the time of the ban, little is known about their distribution. The exception is a literature on the distributional implications of legislator bargaining that predicts that legislators with seniority, affiliation with the majority party, or appointment to important committees will enjoy an advantage over their peers in terms of increased budgetary shares (Weingast, 1979; Baron and Ferejohn, 1989; Knight, 2005; Albouy, 2013).

Knight (2005) finds that members of the House transportation authorization committee were able to successfully earmark projects to their home districts to a greater extent than non-members. His work specifically utilizes the practice of earmarking to identify measurable benefits accrued to powerful legislators. Other work has shown correlations in the size and number of earmarks and legislator characteristics such as party affiliation and seniority, electoral vulnerability, and committee appointments (Balla et al., 2002; Knight, 2005; De Figueiredo and Silverman, 2006; Knight, 2008; Boyle and Matheson, 2009; Crespín et al., 2009; Engstrom and Vanberg, 2010; Clemens et al., 2015). Although a large number of members often receive at least one earmark,¹⁸ members of the House Appropriations Committee (HAC) and Senate Appropriations Committee (SAC) have direct control over the process and exercise increased influence (De Figueiredo and Silverman, 2006; Clemens et al., 2015).

Additional literature finds benefits other than earmarks to regions represented by powerful legislators such as additional vote shares (Levitt and Snyder Jr, 1995; Evans, 2006), increased personal income growth rates (Levitt and Poterba, 1999), larger PAC contributions (Loucks and Bennett, 2011), and increased total federal grants (Ansolabehere and Snyder Jr, 2006; Berry et al., 2010; Albouy, 2013; Alexander et al., 2016), as well as non-budgetary outcomes such as longer operating times before closure for failing banks (Bennett and Loucks, 1996), increased access to swine flu vaccines (Ryan, 2014), and decreased approval times for Medicaid waivers (Helland, 1999).¹⁹

A large literature also documents the role political institutions play in economic policy more generally, including the impact of gubernatorial term limits and party affiliations (Besley and Case, 1995; Fredriksson et al., 2013), voter preferences for government size (Crowley et al., 2016), voter religious preferences and interest group pressure (Halcoussis and Lowenberg, 2015), and the electoral mechanism by which voters choose economic policies (Lee et al., 2004).²⁰

2.3 Data

A roadblock to studying the reaction of legislators to earmark reforms is the difficulty of matching earmarks to geographic areas.²¹ Thus, there has been no empirical test of the earmark ban or its reforms on either the impact of those reforms or their distributional consequences. This paper intends to fill that gap in the literature by analyzing federal grants to state and local governments over the 2007 to 2014 fiscal year period. In order to test the impact of the earmark ban, I use data on federal project (competitive) grants and contracts to state and local governments. I exclude grant types other than project grants, such as formula grants, since these are generally not earmarked (Kirk et al., 2011). The website USAspending.gov is a clearinghouse that records the transfer of grants, contracts, and other assistance between the Federal Government and recipients as categorized by type, including for profit, non profit, and governmental.

I focus on state and local governments because they can be located to a particular geographic area. Including nonprofit and for profit firms would be misleading as they are often headquartered in different areas than where their operations are carried out. I also exclude grants and contracts that were part of the American Recovery and Reinvestment Act of 2009 (ARRA).²² The inclusion of these funds might lead to a mechanical drop in funding after 2010 that would bias the effect of the ban. For state government grants, I also exclude grants to state controlled institutions of higher education. It is unclear whether these grants

¹⁸For example, Knight (2005), in an examination of transportation authorization bills from 1991 and 1998, found that 46 percent of districts received earmarks in 1991, and 337 members of the House voted against stripping the 1998 bill of earmarks. He uses both as measures of the “size of the coalition.”

¹⁹In a similar vein, Washington (2008) finds that legislators with daughters tend to vote more liberally.

²⁰For an overview of this literature, see: (Besley and Case, 2003).

²¹Knight (2005) matches earmarks to congressional districts by the project descriptions in transportation authorization bills.

²²This act funded approximately 13 percent of competitive grants to local governments in 2009.

would be targeted by legislators attempting to benefit the state as a whole, or the specific district where the institution is located.²³

I use data on earmarked grants and contracts from the Office of Management and Budget’s (OMB) earmark database. These data are available for 2005 and 2008–2010, but recipient geographic information is not available for 2010. The OMB data are advantageous over nonprofit groups’ earmark databases since the OMB data rely on agency reports regarding the location of the actual recipients of the funds, as opposed to relying on project descriptions in bill texts.²⁴ As shown in Figure 1, the OMB and CAGW earmark definitions vary.²⁵ Unfortunately, the OMB database was never updated to reflect the recipient information for fiscal year 2010 earmarks. Also, while the OMB earmark data provides the agency, bureau, and account information for each earmark, along with a short description, it does not provide the specific grant program that the earmark came from. Unless the account title happens to be similar enough to the program title, such as the Federal Transit Administration’s (FTA) “Capital Investment Grants” program, then there is no systematic way to determine the earmarked program. However, virtually all earmarked grants are for competitive grant programs, since formula grant programs generally do not deviate from the formulas prescribed in the authorization bill.²⁶ For this reason, I match the earmarked grants with only competitive grants and contracts from the USAspending.gov data.

I match federal grants and earmarks together by the listed recipient’s city, and then match them to congressional districts.²⁷ Where cities were split between more than one district, I weighted the grants and earmarks by population and split them into the different districts.²⁸ As a robustness check (discussed below), I estimated a model where all earmarks and grants split by multiple congressional districts were dropped.

Since the political reality is that earmarks are intended by their sponsors to benefit their own congressional districts in particular, at first glance, an attractive approach would be to simply allocate 100 percent of earmarks to cities split by multiple congressional districts to the district of the earmark’s sponsor. However, in order to calculate the percent of federal grants earmarked for each district, I would then need to arbitrarily allocate 100 percent of some federal grants to single congressional districts. Because I do not observe which individual grant transactions were earmarked, there would be no way to allocate these federal grants in a systematic way.

For the local (state) analysis, I disaggregate the data into district-agency (state-agency) observations. I use the top 16 agencies in terms of funding to local (state) governments in the year 2009 which captures over 90 percent of the grants to local (state) governments. I then match each subcommittee of the HAC (SAC) with the agencies under its jurisdiction and split each district (state) observation into district-agency (state-agency) observations. Finally, I compute the percentage of each district or state-agency funding amount that was earmarked in 2009 in the following way:

$$\%earmarked_{ia,2009} = \frac{earmarks_{ia,t=2009}}{grants_{ia,t=2009}} \times 100\%$$

The letter i indicates either district (d) or state (s) depending on whether the data are for local or state governments, while a denotes the agency.

²³De Figueiredo and Silverman (2006) show strong correlations between earmarks to academic institutions and membership on the HAC.

²⁴See: (Office of Management and Budget, 2007).

²⁵The CAGW definition requires that a request be categorized as an earmark if it is “Requested by only one chamber of Congress; Not specifically authorized; Not competitively awarded; Not requested by the President; Greatly exceeds the President’s budget request or the previous year’s funding; Not the subject of congressional hearings; or Serves only a local or special interest.” Citizens Against Government Waste (2016). The OMB defines an earmark as: “funds provided by the Congress for projects, programs, or grants where the purported congressional direction (whether in statutory text, report language, or other communication) circumvents otherwise applicable merit-based or competitive allocation processes, or specifies the location or recipient, or otherwise curtails the ability of the executive branch to manage its statutory and constitutional responsibilities pertaining to the funds allocation process.” (Office of Management and Budget, 2009). For a thorough description of various datasets on earmarks, see: Doyle (2011).

²⁶For examples of this, see: Kirk et al. (2011).

²⁷See data description in the online appendix for more information on how I do this.

²⁸Since the provided congressional district codes were generated by zip codes, relying on the provided congressional district code alone would be inappropriate given that the recipient’s location may spill over into multiple congressional districts. See the data description in the online appendix for more information.

For the main analysis, this variable—my measure of the impact of the earmark ban across geographic areas and agencies—is measured using 2009 data. As a robustness check, I estimate alternative specifications with $\%earmarked_{ia}$ defined using earmark and grant data from 2005 and 2008.

By measuring earmarks as a percentage of total grants to a given region and for a given agency, I am able to capture the differing impact of the ban to a greater extent than using raw dollar amounts would allow. This is because earmarks as a percentage of total grants measures the relative *marginal* benefit (and thus reduction in benefit following the ban) of additional dollars of earmarks, an effect that is clearly dependent on the size of a government’s total grant awards.

Table 1 displays the wide variation in earmarks for local governments by district-agency observation. A majority of districts received earmarks from the Department of Justice (DOJ), with the average earmark representing 43.8 percent of total DOJ funding to that district.²⁹ Earmarks in Department of Housing and Urban Development (HUD) and Department of Transportation (DOT) programs were also highly spread around, with over 1 in 4 districts receiving an earmark from them, but they represented smaller proportions of the total grants to each district. By contrast, only 4 districts received earmarks in Department of Commerce (DOC) programs. However, all but 61 districts received earmarks from at least 1 agency.^{30 31}

Table 1: Federal Earmarks to Local Governments by Agency, Fiscal Year 2009

	Earmarked districts (total)	Earmarked (%)	Average earmark (\$ thousands)
Defense	1	94.2 (.)	2,625.0 (.)
Commerce	4	25.1 (32.4)	1,056.5 (1,429.0)
Homeland Security	23	49.4 (38.0)	115.8 (120.0)
Energy	22	42.3 (32.3)	347.7 (337.0)
Agriculture	49	22.9 (31.6)	159.9 (276.0)
EPA	36	37.3 (30.9)	366.3 (266.0)
Education	42	2.5 (3.7)	134.9 (240.0)
Health and Human Serv.	50	7.3 (17.4)	104.2 (122.0)
Transportation	116	9.1 (12.4)	860.7 (1,483.0)
Housing and Urban Dev.	115	5.5 (11.1)	327.3 (424.0)
Justice	321	43.8 (28.2)	580.5 (546.0)
Average		26.9	478.7

Note: For observations with positive earmarks. Standard deviations in parentheses. NEH, CNCS, NEA, DOL, and NSF (not shown) all had zero earmarks.

For the local government data, 223 observations had earmarked percentages in excess of 100 percent. I attribute this to the earmark being either never or only partially funded. Due to the late passage of the 2009

²⁹The average earmark overall (\$478,700) is slightly lower than the average individual earmark size (\$517,000) noted above due to the fact that observations with earmark percentages over 100 percent had larger average earmark amounts than those with earmark percentages 100 percent and less. The results are robust to including these observations, with the exception of several large outliers.

³⁰Most Department of Homeland Security (DHS) earmarks appeared to be funded by the ARRA, the majority of which were administered by the Federal Emergency Management Agency (FEMA) to local governments for port and transit security, and for fire station construction. Including these earmarks in the analysis will bias the results towards the ban having an effect, as virtually all of the ARRA funding had been obligated by the end of FY2010.

³¹The agency breakdown for the state data (found in the online appendix) shows a similar breakdown of the data. The DOT had the highest average for the percent of grants earmarked at 48.2 percent of grants on average and its earmarks were spread among 31 states. This is not surprising, considering the highly political process of allocating transportation funding (Knight, 2002, 2005, 2008).

appropriations act, some delay might be expected in terms of the timing of 2009 earmarks being funded. I drop observations that had earmark percentages greater than 100 percent because I cannot be sure that they were funded at all.³² For the state government data, 79 observations had earmarked percentages greater than 100 percent. Additionally, some degree of under or unfunded earmarks would be expected, considering that earmarks often override agency preferences.³³ The results for both the local government data and the state government data, detailed below, are not sensitive to including observations with earmarked percentages greater than 100 percent, with the exception of several extremely large values.

Table 2 displays summary statistics for the congressional district observations respectively. Districts with earmarks tended to receive more federal grants, while the average percent of federal grants earmarked for those that received earmarks was 26.9 percent. In terms of the congressional variables, it is not surprising to see that districts that received earmarks also had longer tenured Democrats in both the House and the Senate, and were twice as likely to have membership on the relevant HAC subcommittee.

Table 2: Summary Statistics, FY2009

Variable	No earmarks	Earmarks
Federal Grants (millions)	2.62 (9.86)	6.24 (10.43)
Earmarked (%)	0.00 (.)	26.86 (29.77)
ARRA grants (millions)	0.27 (1.29)	1.99 (3.53)
Population density	2,392 (6,957)	2,820 (7,365)
House Appropriations Subcommittee	0.02 (0.15)	0.04 (0.19)
Senate Appropriations Subcommittee	0.33 (0.47)	0.43 (0.50)
House Tenure, Democrat	3.57 (4.83)	4.08 (5.21)
Senate Tenure, Democrat	13.10 (15.73)	14.50 (15.76)
House Tenure, Republican	2.35 (3.82)	2.37 (4.15)
Senate Tenure, Republican	8.24 (11.33)	8.04 (11.51)
House Appropriations Committee	0.14 (0.34)	0.15 (0.36)
Population (thousands)	708.27 (76.76)	711.06 (83.16)
Unemployment rate (%)	8.00 (2.12)	8.15 (2.38)
Poverty rate (%)	10.28 (4.81)	10.66 (5.06)
Personal income, per capita (thousands)	27.27 (7.38)	27.26 (7.76)
Observations	5,967	779

Note: Sample means. Standard deviations in parentheses. Federal grants are in 2009 dollars. Population, poverty rate, income per capita, and unemployment figures are from the 2010 Decennial Census. Population density in persons per square mile. Sample of 6,746 Congressional District-Agency Observations.

The 2009 Omnibus Appropriations Act, not signed into law until March 11th, 2009, passed the house with a 57 percent majority split largely along party lines. Just over 11 percent of district-agency observations received earmarks, however most districts (374) received at least 1 earmark from some agency. This implies a large coalition, and the possibility of “vote buying,” where members of the HAC give earmarks to non-

³²The 2010 appropriations were enacted on December 16th, so there is less of a concern that those earmarks (unobserved) would spill over into 2011.

³³An example of this is the recently released data on unobligated earmarks for transportation funding to states (American Association of State Highway and Transportation Officials, 2016).

members in exchange for votes supporting appropriations bills (Alexander et al., 2016).³⁴ Districts receiving earmarks also had higher population densities than those not receiving earmarks, reflecting the fact that smaller districts in terms of geographic size were more likely to receive earmarks, and the fact that at-large states were less likely to receive earmarks. Districts that received earmarks also received more project grants from the ARRA than those without.³⁵

As noted above, earmarks were reallocations, not extra amounts of funds. The process of earmarking was controlled entirely by members of the HAC and SAC, who were required to abide by the 302(b) allocations resulting from each year's congressional budget resolution.³⁶ Instead of simply increasing the amount of funding to programs they favored, the appropriators were forced to reallocate funds, effectively reordering agency priorities. To the extent that earmarks actually did conflict with agency priorities, an agency would have been limited in its ability to prioritize funding in line with grant program objectives.

Several important changes took place from the 111th to 112th Congresses that may influence the analysis. The first was the change in the majority party in the House of Representatives. This shifted the balance of power in Congress towards the Republicans, most importantly for my analysis by leading to the replacement of committee chairs with members of the Republican party. Controlling for party affiliation of House Members will thus be important. The second was the major reauthorization bill (MAP-21) which reauthorized grant programs for the DOT. This act distributed more funds through formula programs and fewer funds through competitive grant programs (Kirk et al., 2012). I control for the passage of MAP-21 with DOT-year interacted dummy variables to ensure that I am not capturing mechanical changes from the new authorization legislation.

3 Empirical Analysis

The main challenge in estimating the impact of the 2011 earmark ban is the possibility that preceding (earmark reforms prior to 2011 and the ARRA) or contemporaneous (elections in Congress) events may confound the analysis. To mitigate this concern, I adopt an econometric approach that allows for the impact of the earmark ban to be estimated in a flexible way that reveals whether an event in any year prior to or following 2011 may have impacted relative grant levels between more and less heavily earmarked regions. This methodology is analogous to the approach taken by Finkelstein (2007); Finkelstein and McKnight (2008), who estimate the impact of the introduction of Medicare on various health outcomes using pre-existing geographic variation in elderly insurance rates.

Earmarks were always inserted after the Congressional budget process had begun, taking the total size of the budget as a given parameter. Thus, the nature of the earmarking process lends itself to analysis via a differences-in-differences technique that compares sub-national units rather than macro-level spending. To that end, I disaggregate the data to the greatest extent possible, i.e., the region-agency level.

My identification strategy takes advantage of wide geographic variation in the extent of earmarking across districts, states, and agencies, as measured by the percent of federal grants earmarked in the 2009 Omnibus Appropriations Act. In order to utilize this variation, however, earmarks and grants must be disaggregated to the local and state levels individually. An aggregate analysis where local and state earmarks and grants were lumped together at the state level, for example, would mask extensive variation across agency-district observations, and disaggregating state government grants to the district level is not possible since state level

³⁴Knight (2005) uses a similar interpretation of districts receiving earmarked transportation projects vs. those receiving none.

³⁵For the state-agency data (table included in online appendix), the differences are less distinct due to the higher level of aggregation. The earmark group contained larger states in terms of population, income per capita, and gross state product (GSP), in addition to higher unemployment rates and lower poverty rates. The average state-agency that received earmarks received on average 24.8 percent of grants from earmarks. There are also similar differences for the congressional variables as with the local government data, where the earmark receiving states were more likely to have longer tenured Democrats than Republicans, reflecting the Democrat majority in the Senate for 2007-2014. The SAC subcommittee varies in a similar way between the No earmark and Earmark groups as the HAC subcommittee variable did for the congressional district data. Unlike the local government data, state-agency observations with earmarks received less in total federal grants than those without. However, a t-test for the equivalence of means fails to find statistically significant differences between the No Earmark and Earmark groups.

³⁶302(b) allocations cap the amount that appropriations subcommittees may appropriate in a given fiscal year.

grants benefit states as a whole.³⁷

The strength of the local government analysis is that it allows me to capture the cross sectional variation in earmarking across congressional districts, driven largely by members of the U.S. House of Representatives.³⁸ The state government analysis allows me to capture longitudinal variation that the local government analysis cannot due to redistricting changes that took effect in 2013. This limits the local government analysis to end in 2012, while the state analysis runs to 2014.

Due to the reforms that lead up to the 2011 ban, a change in the difference in grants between earmark and non-earmark districts (or states) may have occurred earlier than 2011. I thus look for a change in the pre-2011 ban trend in the relative grant levels between earmarked districts and non-earmarked districts. I interact the impact of the earmark ban ($\%earmarked_{ia}$) with year dummies, allowing me to observe changes in the differences between the earmark and non-earmark groups over time. The identifying assumption is that without the ban, *trends* in relative grant levels would have continued unchanged. The year-interacted trend approach is preferable to a simple differences-in-differences approach because it allows me to avoid making any ex ante assumptions about when a change might occur. The reforms before the ban might have had more of an impact than the actual ban, masking the fact that the ban may have only been a superficial change. While a simple differences-in-differences analysis would only capture the tendency for earmarked districts to change in grants more than non-earmarked districts, by looking for a change in the relative trends around the 2011 earmark ban, I am looking for a reversal in a pre-existing, time-varying relationship.

Fundamentally, my measure of the impact of the ban ($\%earmarked_{ia}$) is a way to identify a valid treatment and control group. Because a component of this measure (the denominator) is also represented as the key dependent variable (the natural log of total grants), my empirical strategy rests on the assumption that changes in grant level trends were unrelated to the total amount of grants during 2009 (or 2005 and 2008 for the robustness checks). Intuitively, this seems to be a reasonable assumption considering the fact that the analysis examines competitive grant allocations, not block or formula grant programs. Since competitive grants are allocated based on a merit-based process, increases and decreases to competitive grant awards are less likely to be linked to grant amounts as they vary widely year to year on a case-by-case basis.

Table 3: Log federal grants to local governments

Group	<i>Pre</i>		<i>Post</i>		N	Treatment
	2010	2011	2011	2012		
Earmarks	14.36 (0.09)	13.23 (0.13)	12.70 (0.15)		779	
No Earmarks	7.79 (0.09)	7.14 (0.09)	6.76 (0.09)		5,967	
Differences by year	6.57 (0.25)	6.09 (0.25)	5.94 (0.25)			
DnD 1	6.57 (0.25)		6.02 (0.17)			-0.55 (0.30)
DnD 2	-0.47 (0.15)		-0.16 (0.16)			0.32 (0.22)

Note: Standard errors in parentheses. Federal grants in logs. DnD 1 shows the resulting treatment effect assuming that the level of the differences between the earmark and non-earmark groups would have continued from 2011 on (the “post” period) in the absence of the treatment, while DnD 2 displays the treatment effect given the assumption that the trend in the differences would have continued in the post period had their been no treatment.

³⁷As a robustness check, however, I estimate such a specification where local and state level earmarks and grants are aggregated together at the state level. See section on robustness checks below.

³⁸Among earmarks to local governments, 75 percent had at least 1 House member as a sponsor, while just over 60 percent had at least 1 Senate member as a sponsor. Recall from Table 2 that earmark districts were twice as likely to have representation on a relevant HAC subcommittee than non-earmark districts, while they were 50 percent more likely to have representation on the relevant SAC subcommittee.

Table 3 displays a comparison of the means in log federal grants by group and year. For simplicity, the treatment effect here is defined as a binary indicator rather than a continuous variable as it is in the formal regression model. The rows labeled “DnD 1” and “DnD 2” illustrate estimation of the treatment effect under two different counter-factual assumptions. The row labeled “DnD 1” in Table 3 shows the resulting treatment effect to be negative from assuming that the level of the differences between the earmark and non-earmark groups would have continued from 2011 on (the “post” period). However, given a counter-factual assumption that the trend in the differences would have continued in the post period (DnD 2), the resulting treatment becomes positive. This illustrates the importance of choosing an appropriate counter-factual assumption.

As noted above, members of the HAC and SAC controlled the earmarking process by dividing their subcommittees’ 302(b) allocations across appropriations accounts, and by including specific requests in accompanying report language. It is possible that members of the HAC and SAC would have been able to direct funds to their home districts despite the ban due to their close proximity to the appropriations process. I thus test for whether the effect of the earmark reforms and ban was different for members of the HAC and SAC by including interaction terms in secondary specifications.

3.1 Local Government Analysis

I estimate the following equation to test for the impact of the earmark ban on federal grant receipts by local governments:

$$\ln(\text{grants})_{adt} = \alpha_{ad} \times 1(\text{district}_d \times \text{agency}_a) + \delta_t \times 1(\text{year}_t) + \sum_{t=2007}^{2012} \lambda_t \% \text{earmarked}_{ad,2009} \times 1(\text{year}_t) \quad (1) \\ + \sum_{t=2007}^{2012} \rho_{dt} 1(\text{DOJ}) \times 1(\text{year}_t) + X_{adt} \beta + \epsilon_{adt}$$

The dependent variable is the log of total federal grants to local governments by agency a , in district d , and year t . I use the log of federal grants as opposed to the level, since using the level would constrain the effect on grants to be the same within each year. This would not be appropriate considering the large variation in grant amounts across districts. The district-agency effects ($1(\text{district}_d \times \text{agency}_a)$) control for the cross-sectional differences in grants to districts by agencies.³⁹ The year effects ($1(\text{year}_t)$) capture the variation in large, national changes in grants. I am not able to include observations past 2012 due to congressional redistricting that took effect in 2013.

The coefficients of interest are the λ_t ’s which measure the interaction of the year effects with the treatment variable, $\% \text{earmarked}_{ad}$. Their trend shows the time-varying differences in grants between districts where the ban had more of an effect versus districts where it had less of an effect. If the ban had no impact, then the trend in the λ_t ’s should be the same before and after 2011. Due to the reforms of 2008 and 2009, it is highly possible that breaks may occur in earlier years. Estimating the impact of the ban by restricting the pre and post periods would not allow the data to reveal if prior reforms caused breaks instead of the ban.

I also include in the analysis X_{adt} , a vector of district-year variables including the log of grants funded by the ARRA, tenure-party interactions for both the House and the Senate, and whether the district is in a state with a senator on the relevant Senate Appropriations Subcommittee (SAC). The results are unchanged without including these control variables.^{40 41}

³⁹The fixed effects are important in controlling for time-invariant differences such as the fact that virtually all transit grants go to urban areas, and that particular programs are targeted to particular socioeconomic groups.

⁴⁰Berry et al. (2010); Alexander et al. (2016) lag a similar set of congressional variables one period to reflect the difference between calendar and federal government fiscal years, however my results are not sensitive to either specification and given that the 2009 Omnibus Appropriations Act was enacted late (March of FY2009), I include the contemporaneous congressional variables.

⁴¹Albouy (2013) shows that states with Republican representation receive more in defense and transportation spending, while those represented by Democrats receive more in education and urban development. Additionally, Beland and Oloomi (2017) show that Democratic governors allocate more of their state’s spending to health related uses than Republican governors.

I also include year dummy-agency interactions for the Department of Justice ($\sum_{t=2007}^{2012} \rho_{dt} 1(DOJ) \times 1(year_t)$). As Table 1 shows, the DOJ is an outlier in terms of earmark distribution. Estimations of Equation 1 without the DOJ yield similar coefficients as they do with including the DOJ, except that the λ_t for 2007 is negative instead of positive. The reason for this is because despite the 2007 earmark moratorium which effectively banned earmarks for all but the Department of Defense (DOD) appropriations, some earmarks from FY2006 were funded again in FY2007.⁴² An Office of Justice Programs data search shows \$21.8 million in earmarks for FY2007, over \$4 million of which were carry overs from FY2006.^{43 44}

Figure 2 plots the λ_t coefficients estimated by OLS with 95 percent confidence intervals displayed by the bars, while Table 4 displays the same coefficients and confidence intervals as well as results for Equation 2 (discussed below), along with the computed values for the change in trends over different time periods.

Figure 2: Congressional District-Agency Specification

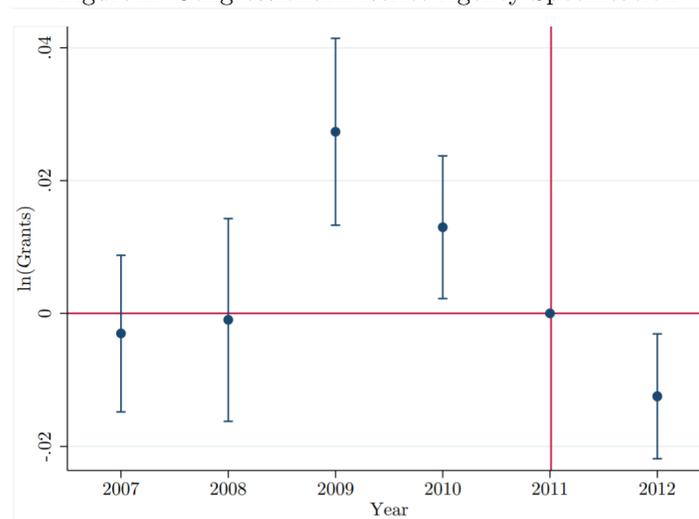


Figure 2 displays the time pattern in the coefficients of log grants on earmarked% (λ_t). The reference point of the graph is set to zero in 2011, the year the ban took effect. The bars show 95 percent confidence intervals for each coefficient. Control variables include the log of grants funded by the ARRA, House and Senate tenure-party interactions and a dummy variable for membership on the relevant SAC subcommittee.

Figure 2 shows that federal grants were already declining in earmarked districts relative to non-earmarked districts before 2011, and that this trend continued unchanged through 2011. Had the earmark ban of 2011 impacted relative grant levels between more and less heavily earmarked districts, the graph would show a break in the trend around 2011. The graph thus implies that the 2011 earmark ban had no impact on the distribution of federal grants. Based on Figure 2, a simple differences-in-differences approach would have found a strong negative impact from the earmark ban on federal grants. This is easy to see in the graph in that by defining the pre-period to be 2009–2010 and the post period to be 2011–2012, the result would have been that the ban had a negative impact equal to the difference between the average of λ_{2009} and λ_{2010} and the average of λ_{2011} and λ_{2012} .

I perform statistical tests to corroborate visual impressions from the graph. Formally, I test whether the n -year change in λ_t after the earmark ban is the same as the n -year change in λ_t from before the earmark ban ($\Delta_{n,2011}$). The impact of the earmark ban in the first year is computed in the following way:

⁴²(Sec. 112) “Declares that any language specifying an earmark in a committee report or statement of managers accompanying an appropriations Act for FY2006 shall have no legal effect with respect to funds appropriated by this Continuing Resolution” (U.S. Congress, 2007). Evidently, this did not prevent all earmarks, even those in FY2007 appropriations bills enacted after the earmark moratorium.

⁴³See: Department of Justice Programs (2007).

⁴⁴I also experimented with including $year \times agency$ sets of dummy variables for each agency individually and the only agency that made a difference was the DOJ.

Table 4: District-Agency level data, 2007-2012
Dependent variable: Log of federal grants

Year interacted	Specification (1)		Specification (2)	
	% earmarked	% earmarked HAC Subcom.	HAC Subcom. × % earmarked	
2007	-0.003 (0.006)	-0.004 (0.006)	-0.723 * (0.412)	0.006 (0.008)
2008	-0.001 (0.008)	-0.001 (0.007)	-0.479 (0.458)	-0.007 (0.021)
2009	0.027 *** (0.007)	0.027 *** (0.007)	0.007 (0.378)	-0.008 (0.011)
2010	0.013 ** (0.005)	0.012 ** (0.005)	0.197 (0.347)	-0.003 (0.012)
2011	0.000 (.)	0.000 (.)	0.663 * (0.382)	-0.020 (0.012)
2012	-0.012 ** (0.005)	-0.013 *** (0.005)	0.442 (0.309)	-0.014 (0.009)
Obs.	40,444	40,444	40,444	40,444
District-Agencies	6,746	6,746	6,746	6,746
Estimated trend changes:				
$\Delta_{1,2011}$	0.001 (0.96)	0.000 (0.98)		

Note: Standard errors in parentheses. Standard errors clustered at the state level. Specification (1) is the OLS estimation of equation 1, while Specification (2) is the OLS estimation of equation 2. Control variables include log of grants funded by the ARRA, House and Senate tenure-party interactions, and a dummy variable for membership on the SAC. $\Delta_{1,2011}$ is the 1 year computed change in trends in the coefficients around the year 2011; P-values are in parentheses.

$$\Delta_{1,2011} = (\lambda_{2012} - \lambda_{2011}) - (\lambda_{2011} - \lambda_{2010})$$

As Table 4 shows, the test fails to reject the null hypothesis that the 1 year change in λ_t before and after the ban is statistically significant from zero.⁴⁵

I also test whether members of the HAC were able to subvert the earmark ban somehow due to their power over appropriations. I interact dummies for district-agency observations having a representative on the agency-relevant HAC subcommittee with year dummies, and I include a similar term interacted with the $\%earmarked_{ad}$ variable. This specification is:

$$\begin{aligned} \ln(grants)_{adt} = & \alpha_{ad} \times 1(district_d \times agency_a) + \delta_t \times 1(year_t) + \sum_{t=2007}^{2012} \lambda_t \%earmarked_{ad,2009} \times 1(year_t) \quad (2) \\ & + \sum_{t=2007}^{2012} \gamma_t HACsubcom \times 1(year_t) + \sum_{t=2007}^{2012} \psi_t \%earmarked_{ad,2009} \times HACsubcom \times 1(year_t) \\ & + \sum_{t=2007}^{2012} \rho_{dt} 1(DOJ) \times 1(year_t) + X_{adt} \beta + \epsilon_{adt} \end{aligned}$$

Table 4 shows that the HAC subcommittee group interaction coefficients (ψ_t) are largely insignificant, indicating that the impact of the ban was no different for members of the HAC than the average.⁴⁶ This is not surprising, considering that the chairs of the HAC and SAC led the reforms of 2009.

⁴⁵Note that, in Table 4, the numbers in the parentheses below the 1 year changes are p-values, not standard errors.

⁴⁶The results are robust to using the lag of HAC membership instead of the contemporaneous measure.

The results are virtually unchanged without the inclusion of the control variables.⁴⁷ The coefficient on the dummy for SAC subcommittee representation is insignificant and negative, most likely the result of the lack of variation over time. The Republican tenure variables for both the House and the Senate show positive coefficients significant at the 90 and 95 percent levels respectively, while the Democrat tenure variables are negative and insignificant. This is most likely due to the fact that during the 2010 midterm elections, Democrats lost a particularly large number of seats in both the House and the Senate, despite retaining control of the Senate. The coefficient for the ARRA log grant variable is positive and highly significant, indicating that districts that received more ARRA funding also received more non-ARRA funding.

3.2 State Government Analysis

Aggregating to the state-agency level allows me to look further beyond the earmark ban for an impact. I estimate a similar specification as before:

$$\ln(\text{grants})_{ast} = \alpha_{as} \times 1(\text{state}_s \times \text{agency}_a) + \delta_t \times 1(\text{year}_t) + \sum_{t=2007}^{2014} \lambda_t \% \text{earmarked}_{as,2009} \times 1(\text{year}_t) \quad (3) \\ + \sum_{t=2007}^{2014} \rho_t 1(\text{DOT}) \times 1(\text{year}_t) + X_{ast} \beta + \epsilon_{ast}$$

The dependent variable is the log of total federal grants to state governments by agency a , in state s , and year t . I include state-agency effects ($1(\text{state}_s \times \text{agency}_a)$) and year effects ($1(\text{year}_t)$), in addition to a vector of control variables (X_{ast}) containing the tenure-party interactions for the senate, tenure-party interactions for the average of all House members within each state, and the percent of a state's representatives in the House on the HAC. I include state level measures of state GSP, population, the unemployment rate, the poverty rate, and personal income per capita. I also include year-agency interaction dummies for the Department of Transportation. This is to control for the impact of the Moving Ahead for Progress in the 21st Century Act (MAP-21), an authorization that shifted funding away from competitive grant programs and towards formula grant programs for the 2013-2014 fiscal years (Kirk et al., 2012). As expected, the coefficients for the 2013 and 2014 DOT interactions (ρ_{2013} and ρ_{2014}) are negative and statistically significant from zero at the 95 percent level.

I estimate Equation 3 by OLS. Results are displayed in Figure 3 and Table 5. These results suggest that the earmark ban did have an impact. From 2008, after the 2007 earmark moratorium, to 2011, funding in earmarked districts was growing at a faster rate than funding in non-earmarked districts, but after 2011 this trend reversed.

I test for differences in the 1 year, 2 year, and 3 year changes around the 2011 earmark ban. For a 1 percentage point increase in $\% \text{earmarked}_{as}$, Federal grants fell by 1.9 percent in the first year, by 3.5 percent over a 2 year window, and by 3.8 percent over a 3 year window. Because of different reference points, these numbers are not directly comparable. In order to compare them, the calculation must be:

$$\Delta_{2,3} = (\lambda_{2014} - \lambda_{2011}) - (\lambda_{2011} - \lambda_{2009}) \quad (4)$$

Comparing Δ_2 with $\Delta_{2,3}$ suggests that the impact of the ban increased over time: from 3.5 percent to 3.9 percent.

In a second specification (equation not shown), I also include an interacted term for the SAC subcommittee with year dummies, and an additional term for SAC subcommittee interacted with $\% \text{earmarked}_{as}$ and year dummies. The coefficients for the interaction terms are displayed in Table 5. As with the HAC subcommittee interactions, the coefficients are not statistically different from zero. I thus conclude that members of the SAC subcommittees, like their House counterparts, did not evade the earmark ban.

⁴⁷However, an F-test for joint significance indicates that these variables are significantly different from zero.

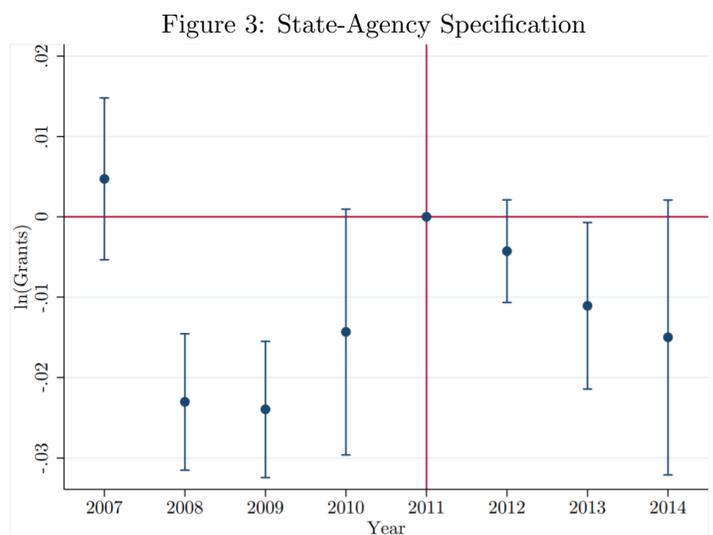


Figure 3 displays the time pattern in the coefficients of log grants on earmarked% (λ_t). The reference point of the graph is set to zero in 2011, the year the ban took effect. The bars show 95 percent confidence intervals for each coefficient. Control variables include the log of grants funded by the ARRA, House and Senate tenure-party interactions, the average of a states House members on the HAC, the poverty rate, the unemployment rate, personal income per capita, population, and state GSP.

Table 5: State-Agency level data, 2007-2014

Year	Specification (1)		Specification (2)	
Interacted	% earmarked	% earmarked	SAC Subcom.	SAC subcom. x % earmarked
2007	0.005 (0.005)	-0.004 (0.006)	-0.205 (0.304)	0.019 * (0.009)
2008	-0.023 *** (0.004)	-0.024 *** (0.006)	0.236 (0.230)	0.000 (0.008)
2009	-0.024 *** (0.004)	-0.027 *** (0.006)	0.106 (0.200)	0.004 (0.008)
2010	-0.014 * (0.008)	-0.019 (0.012)	0.334 ** (0.150)	0.005 (0.010)
2011	0.000 (.)	0.000 (.)	-0.107 (0.328)	0.000 (0.009)
2012	-0.004 (0.003)	-0.002 (0.006)	0.148 (0.245)	-0.007 (0.006)
2013	-0.011 ** (0.005)	-0.009 (0.006)	-0.133 (0.209)	-0.004 (0.012)
2014	-0.015 * (0.009)	-0.019 * (0.011)	-0.074 (0.241)	0.008 (0.012)
Obs.	5,809	5,809	5,809	5,809
State-Agencies	727	727	727	727
Estimated trend changes:				
$\Delta_{1,2011}$	-0.019 (0.04)	-0.021 (0.17)		
$\Delta_{2,2011}$	-0.035 (0.00)	-0.037 (0.00)		
$\Delta_{3,2011}$	-0.038 (0.00)	-0.044 (0.00)		
$\Delta_{2,3,2011}$	-0.039 (0.00)	-0.047 (0.00)		

Note: Standard errors in parentheses. Standard errors clustered at the state level. Specification (1) is the OLS estimation of equation 3, while Specification (2) is the OLS estimation of equation 3 with SAC interactions. Control variables include the log of grants funded by the ARRA, House (average in state) and Senate tenure-party interactions and a dummy variable for average House membership on the HAC. $\Delta_{1,2011}$, $\Delta_{2,2011}$, $\Delta_{3,2011}$, and $\Delta_{2,3,2011}$ are the computed changes in trends in the coefficients around year 2011; P-values are in parentheses. Dependent variable: Log of federal grants.

3.3 Robustness Checks and Discussion

One possibility is that the above results may be driven by an individual federal agency's idiosyncratic behavior. As a robustness check, I thus estimate all of the specifications discussed above while dropping a different agency at a time, and the results were very similar.

I also re-estimated Equation 1 excluding all earmarks and grants to cities that were split by multiple congressional districts. The omission of these earmarks and grants yielded very similar results as well.

Another concern is that the results highlighted above might be a statistical artifact arising from the year in which the $\%earmarked_{ia}$ variable is measured. More specifically, because the $\%earmarked_{ia}$ variable essentially defines the treatment and control "groups," using the 2009 year data to define these groups might be what is driving the results. If the 2011 earmark ban truly impacted the distribution of intergovernmental grants to state and local governments, then such an effect should be detectable regardless of the year of grant data used to define the treatment and control groups. It is also possible that the use of 2009 data introduces a sort of "mean reversion" into the results such that the trend in relative grant levels might change simply due to timing specific to the year 2009.

To address these concerns, I estimate identical specifications with earmark data from 2005 and 2008 appropriations legislation instead of the 2009 Omnibus Appropriations Act, and the results (available in the online appendix) were very similar.⁴⁸ In addition, specifications with interacted HAC and SAC effects using the 2005 and 2008 earmark data (not shown) were similar to the 2009 specifications as well. These results provide additional evidence that the earmark ban did not impact the distribution of grants to local governments, but did impact the distribution of grants to state governments.

Examining the earmark distributions across years, both in terms of dollar amounts and the $\%earmarked_{ia}$ variable, reveals very little persistence in earmarks: the correlation coefficients between earmarks across years are very small for both measures. These low correlations imply that using different years to define the $\%earmarked_{ia}$ variable is effectively defining different treatment and control groups. The robustness of the results to using alternative years in defining $\%earmarked_{ia}$ thus reinforces the original findings and points towards the ban, not the particular group of regions defined as "treated," as the driving factor behind the results.

A final robustness check consists of estimating the state government model with the local government earmarks and grants added to the state government earmarks and grants. Figure 4 shows that the coefficients are similar in magnitude to the state government results, but that the trends before and after 2011 are flat rather than up and down (respectively). Clearly, this would lead to a smaller estimated effect of the ban than the state analysis showed, due to the fact that the local government analysis displayed a negative trend throughout the time period.

As the results show, the earmark ban did not impact the distribution of federal grants to local governments, while it appears to have impacted the distribution of federal grants to state governments. This presents somewhat of a puzzle, as the ban was instituted in both the House and the Senate during the same time period. A plausible explanation lies in the fact that before the ban, restrictions were enacted in the House that were not enacted in the Senate. Although the ban was equally enforced in both chambers of Congress, the events that took place before 2011 varied across the House and the Senate in one particularly important way: the House instituted stricter reforms prior to the 2011 ban than did the Senate. Specifically, in 2009, both chambers' appropriations committees began requiring that members post earmark requests on their websites.⁴⁹ The HAC instituted an additional reform, mandating that agencies have 20 days to review each earmark request and "check that the proposed earmark is eligible for funding and meets goals established in law."⁵⁰ It is possible that it was this additional reform that initiated a change in the House, thus impacting the distribution of federal grants to local governments but not to state governments.

⁴⁸These appropriations acts are the Consolidated Appropriations Act of 2005 and the Consolidated Appropriations Act of 2008.

⁴⁹This reform was not uniformly adhered to, with some legislators posting information and others not (Doyle, 2011).

⁵⁰See: Congress, House, Committee on Appropriations, "Press Release: Pelosi, Hoyer, and Obey Announce Further Earmark Reforms", March 11, 2009.

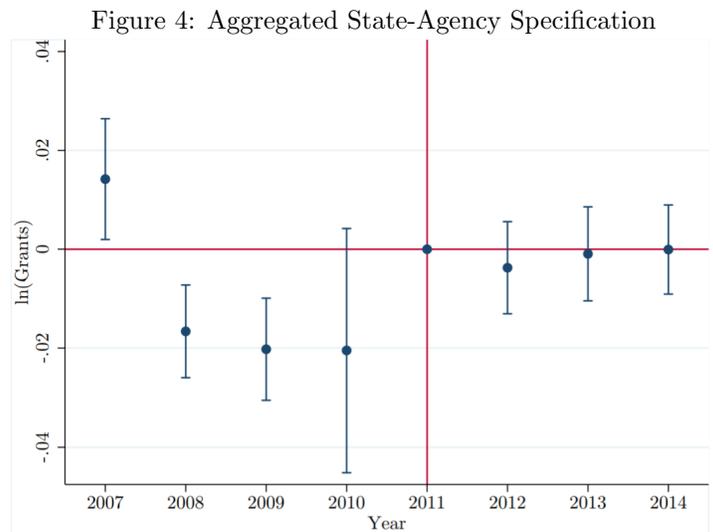


Figure 4 displays the time pattern in the coefficients of log grants on $\%earmarked$ (λ_t), which includes local government grants and earmarks added to the state government grants and earmarks. The reference point of the graph is set to zero in 2011, the year the ban took effect. The bars show 95 percent confidence intervals for each coefficient. Control variables include the log of grants funded by the ARRA, House and Senate tenure-party interactions, the average of a states House members on the HAC, the poverty rate, the unemployment rate, personal income per capita, population, and state GSP.

3.4 Distributional Effects of the Earmark Reforms

I aggregated local government earmarks and grants to the district level and performed t-tests of the equivalence of means in the average $\%earmarked_d$ across demographic groups as defined by data from the 2010 Decennial Census. I used earmarks and grants from the 2009 Omnibus Appropriations Act, since this was the closest year to the 2010 Decennial Census.

Table 6 displays the means across demographic groups split into the 95th and 5th percentiles for each group. T-tests for the equivalence of means show statistically significant differences in the mean level of $\%earmarked_d$ only for the income per capita groups.

The top 5 percent of districts in terms of income per capita received 6.1 percentage points more in earmarks as a share of total grants than the bottom 5 percent. This difference is statistically significant at the 99 percent level and represents an average amount of additional funding of \$3.31 million in federal grants to local governments going to rich congressional districts over poor districts that would not have been awarded in 2009 without earmarks. In contrast, I find only a small difference in the means of the percent earmarked between districts with members on the HAC and those without. On average, HAC districts received only 0.39 percentage points more in earmarks as a percent of grants than non-HAC districts, and this difference is not statistically significant from zero. Earmarking thus impacted equality between rich and poor districts by substantially more than it did between HAC and non-HAC districts. In light of the fact that the existing distributive politics literature on earmarks documents the relative differences in earmarks between different geographic areas defined exclusively by legislator characteristics such as committee membership, this finding represents an important contribution to the distributive politics literature. Lastly, I find no evidence of differences across other demographic groups.

Taken together, these findings imply that earmarking, at least in 2009 just before the ban, benefited the rich relative to the poor, which makes intuitive sense considering that rich districts are more likely to have wealthy donors who may be willing to trade campaign contributions for specific projects. Alternatively, local governments in rich districts may be more likely to be able to afford to hire lobbyists. This implies that the earmarking system distorted the Federal Government's distributive goals of increasing an equal distribution of federal funds through grants to local governments. To the extent that future earmark reforms, such as those being discussed as of September 2016, are effective in loosening the ban on earmarking, the

Table 6: Equivalence of Means T-tests by Demographic Group, Fiscal Year 2009

	<i>Top 5%</i>				<i>Bottom 5%</i>			
	Earmarks	Grants	Earmarked (%)	N	Earmarks	Grants	Earmarked (%)	N
Government workers	4.19 (8.67)	114.16 *** (99.89)	6.91 (15.16)	23	1.43 (2.54)	46.79 *** (47.42)	4.93 (7.05)	22
Senior citizens	1.01 (0.96)	40.41 * (27.42)	3.38 (3.79)	22	2.57 (6.04)	76.46 * (93.83)	4.62 (5.77)	26
Female	1.45 (1.73)	123.39 (68.95)	1.68 (2.02)	22	3.99 (7.70)	123.02 (107.33)	2.62 (2.65)	23
Black	2.93 (4.37)	106.65 (67.32)	3.65 (3.79)	22	2.42 (2.85)	98.85 (75.94)	3.90 (5.41)	25
Income	2.76 (4.67)	79.19 (92.18)	8.78 *** (10.22)	22	1.72 (1.50)	97.06 (71.31)	2.68 *** (2.78)	22
Unemployment rate	2.19 (1.97)	103.35 * (60.64)	3.33 (3.82)	23	2.91 (6.29)	67.64 * (64.60)	6.83 (13.69)	25
	<i>= 1</i>				<i>= 0</i>			
HAC	3.00 (3.27)	86.55 ** (78.70)	5.51 (6.99)	61	2.18 (4.40)	64.72 ** (64.47)	5.12 (9.18)	374

Note: *** denotes 99 percent confidence level, ** denotes 95 percent confidence level, * denotes 90 percent confidence level in rejecting the null that the difference in the means for each group is zero against the alternative hypothesis that the Top 5% group is different than the Bottom 5% group. Sample means. Standard deviations in parentheses. Demographic groups are defined by using the 2010 Decennial Census data for congressional districts. Earmarks and Grants in 2009 dollars per capita. Government workers is the share of workers employed by the government. Sample of 435 Congressional Districts.

distribution of federal funds between income groups may also be impacted.

4 Conclusion

My results suggest that the earmark ban had a differential impact on the distribution of federal competitive grants and contracts. For grants to local governments, the 2011 earmark ban was not responsible for the relative decline in grants between districts with earmarks versus those without; that trend had already begun prior to the ban. For state government grants, the ban of 2011 may have helped to reverse the trend of increasing grant levels in more heavily earmarked states relative to less heavily earmarked states. These results are robust to controlling for legislator attributes, the impact of the ARRA, the set of agencies in the data, and the year in which the treatment variable is measured. A possible explanation for the differential impact of the ban on local government grants versus state government grants lies in the fact that local government grants were largely influenced by members of the House of Representatives, who adopted stricter reforms in 2009 than did the Senate. I also find that the earmarking system decreased the level of equality in the distribution of federal grants. The top 5 percent wealthiest districts garnered more earmarks relative to the poorest districts. Considering that rich districts are more easily able to fund public works projects due to having larger tax bases, from the perspective of equality, the earmark reforms and ban appear to have been an improvement.

Of course, there are perhaps benefits to social welfare from earmarking that must be considered in making any sort of value judgment regarding the utility of the practice. Critics of the earmark ban have argued that important legislation is now more apt to be stymied by the political impasses that have become a feature of the law making process in recent years (Evans, 2004; LaTourette, 2014; Gold, 2015). Without a measure of these benefits, it would be difficult to draw conclusions regarding earmarking's overall efficiency. However, much of the current debate within Congress over earmarks centers around the idea that only a small minority of earmarks are bad or wasteful, while the majority are good.

This rhetoric misconstrues the real question, which is one of information and incentives. An earmark that one group might consider to be wasteful, might be entirely appropriate from the perspective of another group. The point of the debate, then, should not be to label potential projects as good or bad, but instead to ask: Who is better incentivized and better informed to determine the allocation of federal grants; legislators or agencies? Future work that attempts to answer the question of whether the earmark ban should be relaxed will need to shed light on this fundamental question in a systematic way.

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