The President and the Distribution of Federal Spending

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Abstract

Empirical research on distributive politics emphasizes party and committee leaders in Congress. This paper highlights the president, who most credibly fills the role of the proposer in Baron and Ferejohn's (1989) seminal model, and who has further opportunities to influence the distribution of federal outlays both later in the appropriations process and after a final bill is enacted. We analyze a large database that tracks the geographic spending of nearly every domestic program over a 21-year period. Using a district fixed-effects estimation strategy, we find only sporadic evidence that committee chairs, party leaders, and majority party members receive larger shares of federal outlays. Instead, we find consistent and robust evidence that districts receive systematically more spending when they are represented by legislators in the president's party.

Politics, Harold Lasswell famously argued in 1935, is about "who gets what, when, and how." And ever since, political scientists have placed distributive politics at the very center of studies of legislatures generally, and the U.S. Congress in particular. The received wisdom about how Congress allots benefits among its members, though, continues to lack a firm foundation in data. Surprisingly few empirical tests actually address Lasswell's edict, and those that do have flaws that severely limit their generalizability.

Rather than add another incremental twist to the empirical study of distributive politics, this paper builds from the foundation up. Theoretically, we start with an influential model of legislative bargaining proposed by Baron and Ferejohn 20 years ago. This model focuses our attention on proposal power as the mechanism that privileges some actors over others in the distribution of benefits. Empirically, we assemble a comprehensive dataset of federal spending by congressional district that spans three decades. The expansiveness of the data allows us to isolate critical variables that had previously either been conflated or ignored.

We show that the conventional wisdom about who within Congress holds the power to direct federal resources finds little support in the data. Rather than party leaders or committee chairs, it is the president who wields formal proposal authority over the budget. Moreover, the president can harness the veto, public appeals, and control over agency administrators in pursuit of his (someday her) federal spending priorities. Accordingly, there are strong reasons to believe that the president plays a central role in determining which jurisdictions get "what, when, and how."

Empirically, we demonstrate that members of the president's party are advantaged in the budgetary process. At least since 1984, however, there is only sporadic evidence that members of the majority party have an edge in securing federal dollars, and no evidence that committee

leaders systematically obtain larger shares of federal outlays for their constituencies. Further, spending patterns do not conform to norms of universalism in which all districts benefit roughly equally.

The remainder of the paper unfolds as follows. First, we begin by reviewing the Baron and Ferejohn model, highlighting its emphasis on proposal power. Second, we review the existing empirical literatures on distributive politics, showing that previous studies have yet to establish who benefits from this power. Third, we point out that the president retains proposal rights as well as other formal prerogatives and institutional advantages that strengthen his ability to influence the geographic distribution of federal outlays. Fourth, we describe our data and empirical strategy. Fifth, we present our main statistical results, revealing that members of the president's party receive a disproportionate share of federal outlays, party and committee leaders typically do not, and members of the majority party receive a small, but often statistically insignificant, increase in distributive benefits. Sixth, we conduct several checks on the robustness of the results, incorporating ideological variables, disaggregating the data by programmatic area, and replicating our main findings in other datasets. In the final section we conclude.

The Baron and Ferejohn Model

As it does for so many others who study distributive politics, Baron and Ferejohn (1989) initiates our analysis. Published almost 20 years ago, Baron and Ferejohn's model extends Rubinstein's (1982) classic work to tackle the fundamental question of how a legislature distributes benefits among its members. The model has been called "influential" (Leblanc et al. 2000), "seminal" (Eraslan 2001, 11), "the most common reference point" in the literature on coalition formation (Morelli 1999, 809), and "the most widely used model of legislative

bargaining" (Snyder et al. 2005, 982). Any comprehensive depiction of distributive politics must consider it.

Baron and Ferejohn posit a non-cooperative legislative bargaining model wherein n legislators realize their electoral goals by directing federal benefits to their home districts. Legislators are given the task of dividing a fixed budget (a version of "divide the dollar"), and their utility is defined strictly over the share that they receive. In each stage of the game, a division is proposed, amendments are made (in the case of an open rule), and members vote. If the proposal passes, the game ends; if not, then play continues for another round, and a new division, discounted by δ, is proposed, and play repeats. Under a closed rule, outcomes are typically minimal winning coalition; under an open rule, benefits are distributed more widely. For our purposes, though, equilibrium predictions about how many districts benefit are less interesting than *which* districts benefit. And in this respect, the key feature of the model is the power of recognition: the legislator with proposal power offers a bill that a majority of legislators will support, but one that includes an extra helping for his own constituency.

Most representations of the model assume that legislators are recognized randomly and thus have equal probabilities of being selected as the proposer.[1] Consequentially, bill by bill, outcomes vary widely across districts, with the proposer obtaining more than other members of the coalition, and those members excluded from the coalition receiving nothing at all. Under a closed rule the equilibrium outcome, which occurs in the first stage of the game, yields $1 - [\delta(n - 1)/2n]$ for the proposer, δn for other members of the coalition, and nothing for the remaining $(n - 1)/2$ members outside of the coalition. Because proposers are recognized randomly, however, in expectation districts receive a roughly equal share of federal benefits. In this sense, the core

[1] Strictly speaking, their model does not require that proposers be randomly selected. Indeed, Baron and Ferejohn note that those members with a higher probability of being recognized are less likely to join coalitions for the simple reason that they have a higher continuation value in the game (1989, 1189).

distributive predictions of Baron and Ferejohn have more than a passing resemblance to those of traditional theories of universalism (Shepsle and Weingast 1981; Weingast 1979, 1989; Niou and Ordeshook 1991).

The theoretical literature that builds upon Baron and Ferejohn underscores the fact that institutional structures and norms within Congress systematically privilege some members over others. And much of this work considers how different recognition rules yield different predictions about the distribution of federal benefits (see, for example, Helpman and Persson 2001; McKelvey and Riezman 1992; McCarty 2000a; Knight 2005; Persson 1998; Persson and Tabellini 2002). As Yildirim (2007, 168) summarizes, "A key prediction of this literature is the presence of the 'proposer power' in that the agent who proposes how to allocate the surplus receives a disproportionate share. Thus, understanding how the proposal power is gained and distributed among negotiating parties is crucial in understanding the allocation of surplus, and the parties' payoffs."

Who, within Congress, is well positioned to secure a larger share of federal outlays? More specifically, how do members' partisan affiliations and/or institutional posts affect their chances of serving as the proposer, and hence being capable of exploiting the legislative process for their own district's gain? The empirical literature on the U.S. Congress offers a variety of answers to both of these questions.

Empirical Evidence on Proposal Power

Congressional scholars are keenly aware of the inequalities in agenda power among legislators.[2] Though the literature identifies many different sources of proposal power, we focus on the two most prevalent: committees and parties. As McCarty's (2000a, 509) extension of the

[2] Bertelli and Grose (forthcoming) characterize the literature on distributive spending as "decidedly legislature-centric."

Baron and Ferejohn model summarizes, "rules and norms may provide greater opportunities for leaders of committees and parties to make proposals than other members."[3]

Committees are perhaps the best defined institutional feature of the modern Congress, and legislators' careers are frequently defined by their committee work. It is widely believed that legislators seek committee assignments that allow them to serve their districts' interests, and that logrolls on the floor improve the odds that committee proposals succeed (Adler and Lapinski 1997; Deering and Smith 1997; Mayhew 1974; Shepsle and Weingast 1981, 1987; Weingast and Marshall 1988). Consequentially, in the aggregate we should expect members of key committees or chairs of any committee to receive more benefits. With specific jurisdictions, members of committees also should secure more benefits in the policy domains they oversee.

Empirically, though, the evidence for committee influence over distributive benefits remains surprisingly mixed. Ferejohn's (1974) important book on the Army Corps of Engineers' projects, for instance, demonstrates that members of the Appropriations and Public Works committees directed funds to their districts quite clearly, but that analysis is now over 30 years old. More recently, Alvarez and Saving (1997) show that districts represented by members on Armed Services or Small Business receive more funds devoted to their policy jurisdiction, but those on Appropriations and Public Works do not. Heitshusen (2001) finds that members on the Agriculture Committee secure more agriculture spending, but that members on the Education and Labor Committee fail to direct more for education or labor spending to their home districts. Rich (1989) finds that serving on Appropriations, Banking, and relevant subcommittees had only minimal effect on HUD spending by district. In his study of bargaining over a transportation bill, Lauderdale (2008) finds that being a member of Transportation Committee increases district

[3] Because parties act as cartels to dominate committee actions (Cox and McCubbins 1993), one might view these as distinct proposers or a single set.

earmarks on the initial House bill but not the final legislation. In contrast, Knight's (2005) study of transportation spending reveals large and consistent effects of committee membership on transportation project spending in one's district, although service on Appropriations of the Surface Transportation Subcommittee does not. Finally, Evans (2004) finds that being on Public Works increases the likelihood of a district demonstration project in three of four models, but being on Ways and Means or its Trade Subcommittee had no effect on whether districts received particular benefits from the North American Free Trade Agreement.

Though their findings vary, these studies confront a common set of challenges. Indeed, it is largely because of data limitations that the literature does not speak with one voice about the ability of committee members and leaders to wield proposal power. Most of the analyses examine only one or a few committees, tend to focus on earmarks or other small projects, and seldom track patterns for more than a year or two. Because recent work has challenged the notion that self-selection by legislators causes "high demand" districts to be overrepresented on committees (Frisch and Kelly 2006; Krehbiel 1991, 1994), more scrutiny of the impact of committee assignments on district benefits is in order.

A second and equally substantial body of work scrutinizes the ways in which the majority party dominates congressional proceedings, and thereby secures larger shares of government outlays (Aldrich 1995; Binder 1997; Rohde 1991). Majority party leaders, it is postulated, favor their own members to help them win reelection—both directly and indirectly through the party brand—in exchange for support of the party's legislative program. The prominent "cartel" model (Cox and McCubbins 2005, 2007) further posits that the majority party acts collectively to control the agenda. If true, members of the majority party should profit handsomely from their privileged positions within Congress and proposal powers. As one recent study summarizes,

"majority party legislators should be expected to discriminate against districts represented by the minority party when allocating pork" (Balla et al. 2002).

A number of scholars have investigated the impact of majority party status on congressional outputs. Analyzing state-level federal funding between 1971 and 2004, Albouy (2008) finds that states with two Senators in the majority party instead of the minority garner a roughly 5 percent increase in transportation grants. However, majority effects on spending for the House were insignificant, as were effects for other types of spending in the Senate. Unfortunately, most other studies of majority effects on spending consider short time frames during which majority party control does not change. Levitt and Snyder (1995), for example, examine a six-year period in the late 1980s and find that more spending goes to districts where the Democratic share of the presidential vote is higher and where the incumbent legislator is a Democrat. Because the Democrats controlled the House throughout this period, however, it is impossible to infer whether a change in party control would actually alter spending patterns. Balla et al. (2002) offer a blame avoidance model of distributive politics to explain earmarks for higher education. They present evidence that majority party members, all Democrats, are in fact more likely to secure earmarks, which also tend to be larger in size. They do not show whether this is true beyond the eight year period of Democratic control they examine or in other policy domains. Martin (2003) similarly finds that Republican enclaves receive less federal money, but because Democrats controlled the House during the entire period of his study, we again cannot determine if it is party differences per se or the effect of majority party status that causes this difference.

These limitations characterize other work as well. Lowry and Potoski (2004), for instance, find scant evidence across seven different policy domains that the majority party gives

more to districts represented by its members, but their conclusions are limited to a seven-year period in the Senate. Evans' (1994) analysis of pork barrel politics is also restricted to a few pieces of legislation, and she reveals little evidence of majority party control of district project awards. Bickers and Stein (2000) use the Republican takeover of Congress in 1994 as a natural experiment to determine how party control affects distributive spending. Beyond the fact that Republicans looked more favorably on contingent liability programs than did their Democratic predecessors, Bickers and Stein observe no real effects of partisan or other political variables on spending. Finally, Lauderdale (2008) shows that earmarks in the 2005 transportation bill favored Democratic districts; but again, with only a single year of data Lauderdale cannot distinguish partisan differences from majority party influences.

Being in the majority party clearly comes with benefits. An extensive body of research analyzes the effects of party membership on things such as roll call votes, agenda control, committee assignments, and campaign fundraising (Binder, Lawrence, and Maltzman 1999; Cox and Magar 1999; Cox and McCubbins 2005, 2007; McCarty, Poole, and Rosenthal 2001; Smith 2000). Much less is known, however, about how majority party status affects the distribution of federal funds. Even the few studies that tackle this question directly are hampered by data limitations that prevent them from drawing broad conclusions. Most commonly, these studies focus on a single policy domain over a short period of time, wherein partisanship and party control correlate perfectly. Consequentially, we often cannot neither whether their results apply in other policy domains, or whether they indicate the effects of majority party status, per se, or simply membership in one party or another.

The President Is the Proposer, and More

Committees and parties may strengthen the bargaining position of certain members of Congress on particular pieces of legislation. When crafting the federal budget, however, neither committee nor party leaders fill the role of Baron and Ferejohn's proposer. In point of fact, no one within Congress does. The actual proposer inhabits the White House, a basic fact that much of the distributive politics literature has overlooked. Since the enactment of the Budget and Accounting Act of 1921, the president has been responsible for composing a complete budget, which is supposed to be submitted to Congress in February of each year, and which initiates the actual authorization and appropriations processes.

Producing the president's budget is no trivial undertaking. In multiple volumes and tens of thousands of pages, the president's budget identifies funding levels not just for individual agencies, but also for individual projects and employees within these agencies. The president then supplements specific requests with extensive policy and legislative recommendations, detailed economic forecasts, and exhaustive accounts on the performance and finances of federal agencies and programs. When they ultimately get around to crafting a final budget, members of Congress rely upon the president's budget more than any other document for information about operations within the federal government (Schick 2000, 90, 189-93).

Substantial efforts are made to ensure that the president's budget reflects his policy priorities. Rather than submit requests directly to Congress, agencies seeking federal funding must submit detailed reports to the Office of Management and Budget (OMB). Working at the behest of the president, OMB then clears each of these reports to ensure that they reflect the chief executive's policy priorities.[4] When they reveal discrepancies, officials at OMB either return the

[4] A small number of agencies do not submit budgets directly or only to OMB. Examples include the U.S. Sentencing Commission and the International Trade Commission (Lewis 2004).

reports to the agencies for subsequent amendment, or they simply edit the documents themselves. The end product, then, is a proposed budget that closely adheres to the president's policy agenda.

Additional Sources of PresidentialIinfluence

Upon submission of the president's budget, of course, members of Congress are free to offer any number of changes. So doing, though, they must contend with an actively engaged president. Coinciding with the State of the Union speech, the release of the president's budget is typically a highly public affair, wherein the president makes his case for his most important budget priorities, and agencies follow up with press releases and briefings of their own (Schick 2000, 98). During the actual appropriations process, the president deploys a small army of experts to testify on behalf of his budget priorities. Concurrently, the president himself weighs in with direct solicitations to key members of Congress (Neustadt 1990), public appeals (Canes-Wrone 2006), and ultimately the threat of a veto (Kiewiet and McCubbins 1988; McCarty 2000a; Cameron 2000), all in an effort to control the content of the final budget.

After an appropriation's passage, the president has still more opportunities to influence how federal funds are actually spent. Indeed, a substantial portion of the federal budget supports programs and grants that executive agencies administer. As just one illustrative example, consider the National Science Foundation's (NSF) doctoral dissertation grants. Though Congress decides how much the NSF can spend, bureaucrats within the agency decide where the money goes. And so it is with larger research grants through the National Institute of Health, disaster relief through the Federal Emergency Management Agency, financial assistance through the Small Business Administration, and so on. Bureaucrats within the executive branch, many of whom hold presidential appointments, are ultimately responsible for deciding the geographic distribution of many federal funds.

Presidents, and the department heads who work for them, also have opportunities to redirect those funds that support programs serving specific communities. Presidents can reprogram funds within certain budgetary accounts; and with Congress's approval, they can transfer funds between accounts. Contingency accounts, which are typically established for unforeseen disasters, give presidents further allowance to redirect federal funds towards their preferred projects. As a matter of course, final budgets regularly leave presidents a fair amount of discretion to influence the geographic distribution of federal funds for specific programs. For an artful president intent upon redirecting federal outlays to a preferred constituency, "the opportunity for mischief is substantial" (Fisher 1975, 88). Just as congressional scholars have argued that the *ex post* power of committees enhances their influence in the policy process (Shepsle and Weingast 1987), we contend that the president's *ex post* ability to influence the distribution of funds through executive agencies complements his *ex ante* proposal power.

Fixated on the internal workings of Congress, the preponderance of empirical studies of distributive politics has overlooked the fact that appropriations are introduced, signed, and eventually implemented in the executive branch. Recently, though, a handful of scholars have incorporated the president. A series of unpublished manuscripts have introduced evidence that presidents can target certain forms of federal spending toward specific states (Shor 2006) and counties (Mebane and Wawro 2002). We extend this formative work by tracking non-defense federal spending in every congressional district over a 21-year period, the most comprehensive dataset on the geographic distribution of federal outlays ever compiled. While we are not able to empirically disentangle effects of the president's proposal power from effects of veto threats or other sources of presidential power, we find systematic evidence of presidential influence in the targeting of federal spending.

Who Does the President Target?

We postulate that presidents use their various sources of budgetary influence to benefit members of their own party. They do so for a variety of reasons. Most obviously, perhaps, presidents direct outlays to populations who share their political interests and priorities. More than just generic pork, many federal programs have clear political content that engenders the support of one party and opposition of the other. Democratic presidents, then, tend to support programs that benefit constituents who typically elect Democratic representatives to Congress, just as Republican presidents support programs that benefit constituents who elect Republicans. When the party of the president changes after an election, therefore, we expect to see increases in funding for programs that benefit his co-partisans.

Presidents have additional reasons for directing federal outlays to districts represented by members of their own party within Congress. For starters, presidents may wish to reward co-partisans for their support on other legislative initiatives (Jacobson, Kernell, and Lazarus 2004). Given that the political fates of co-partisans are often linked (Aldrich 1995; Cox and McCubbins 2007), presidents have further electoral incentives to support congressional members of their own party. And finally, presidents, as party leaders, have unique responsibilities to ensure that a preponderance of government outlays remains within the bailiwick of their own party (Galvin 2009). Such a view is consistent with Larcinese et al.'s (2006) finding that a state receives more federal funding when its governor is from the same party as the president.[5]

Our main predictions concern the average difference between allocations for members of the president's party and members of the opposition. But we do not rule out the possibility of important interaction effects. McCarty (2000a), for instance, presents one of the few studies that

[5]In their study of spending in the Departments of Defense and Labor, Bertelli and Grose (forthcoming) find that state spending is higher when cabinet secretaries and senators share party affiliation but not when presidents and senators share party affiliation.

explicitly recognizes the president's ability to influence the geographic distribution of federal spending. Through the veto, McCarty suggests, a president can secure a disproportionate share of the federal budget for his "constituency," which, consistent with our own claims, plausibly consists of districts represented by members of his party. McCarty's model further predicts that the average difference in spending between co-partisans of the president and members of the opposition party will depend upon their respective sizes. In particular, when "the president's party in the legislature is small … spending will be heavily skewed toward the party of the president" (125).

Other presidential strategies may attenuate the observed differences between members of the president's party and the opposition party. Consider, for example, standard vote-buying models of Congress (e.g. Groseclose and Snyder 1999), wherein a proposer builds a supermajority in support of a legislative initiative by paying off at least some individuals who would otherwise oppose it. In these models, the costs associated with purchasing any individual vote typically increase in the distance between a bill's location and a member's ideal point, relative to the reversion policy. As a proposer who also has ex post budgetary influence, the president may use federal outlays to engage in precisely this kind of behavior. Often the president will have to purchase the votes of some members of his own party. To build a majority or possible super-majority, though, he often must secure the additional support of at least some members of the opposition party. And where vote buying is necessary to do so, the president may choose to direct additional federal outlays to the opposition party's more moderate members, whose votes are cheaper to purchase.

Presidents, too, may use federal outlays to influence the electoral fortunes of individual members of Congress. Because they can expect to enact a greater portion of their legislative

agenda when large numbers of their own party reside within Congress (Coleman 1999; Howell et al. 2000), presidents have strong incentives to use their influence over the budgetary process in order to shore up the reelection prospects of co-partisan incumbents and the election prospects of future co-partisan challengers. By targeting congressional districts represented by co-partisans for additional federal outlays, and congressional districts represented by the opposition party for cuts, presidents may be able to influence the partisan composition of the next Congress. More exactly, presidents ought to direct a disproportionate share of federal outlays to electorally vulnerable members of their own party, and a disproportionate share of cuts to electorally vulnerable members of the opposition party. And to the extent that norms or other factors dictate that at least some share of those federal outlays over which the presidents exercises control goes to members of the opposition party, presidents have strong incentives to ensure it goes to members from electorally secure districts, for whom the aid will not have a decisive effect on the results of the next election.

It is an empirical question whether presidents end up directing more outlays to the opposition party than to their own; or whether presidents target outlays to specific members of either party in ways that are consistent with models of vote buying or strategic assistance. In the tests that follow, we examine these possibilities.

Data and Empirical Strategy

Our federal spending data come from the Federal Assistance Award Data System (FAADS), a government-wide compendium of federal programs. The FAADS archive documents the transfer of almost anything of value from the federal government to a domestic beneficiary, so it includes essentially all federal programs outside of defense. In total, our

database tracks approximately $20.8 trillion (in 2004 dollars) in federal expenditures from 1984 to 2004.

Extending and refining Bickers and Stein's (1991; 1995) FAADS data, we trace non-defense related federal outlays for each year between 1984 and 2004 to every district in the nation. Bickers and Stein assembled and collapsed quarterly FAADS files from fiscal year 1983 to 1997 into annual data files, and we extended the data through 2004.[6] The complete database tracks the total dollar amount awarded by each non-defense federal program to recipients in each of the 435 congressional districts during each of the fiscal years. To the closest extent possible, we replicated the design of the Bickers and Stein federal spending database for 1998 to 2004 using new FAADS quarterly data files.[7] With 21 years of data for 435 districts, our total sample includes 9,135 observations. To reflect the fact that money spent this year is based on the budget passed last year, we match outlays in year *t* to the legislator who represented the district in year *t* – 1. In the year following redistricting, such matches are not possible, and hence we drop these cases, leaving us with a total of 7,882 observations to analyze.[8]

The FAADS data include a great deal of federal spending by broad-based entitlement programs, such as Social Security and Medicaid, the distributions of which are determined by formula. It hardly seems appropriate to attribute this kind of spending to the efforts of the president or other agenda-setters. To separate broad-based entitlement programs from federal programs that represent discretionary spending, we adopt a tactic used by Levitt and Snyder (1995, 1997). Specifically, we calculate coefficients of variation in district-level spending for each program contained in the FAADS data and use the coefficients to separate programs into

[6] We dropped all observations from 1983 because this was the last year before the 1980s redistricting took effect. Observations from 1983 are in different boundaries from, and therefore not comparable with, observations from any other year.
[7] The detailed Bickers and Stein codebook can be found online at http://www.polsci.indiana.edu/faad/codebook.txt.
[8] Additional details are provided in the data appendix.

two categories: *low variation* programs have coefficients of variation less than 3/4, and *high variation* programs have coefficients of variation greater than or equal to 3/4.[9] The low variation category includes 26 programs, most of which are programs within the Veterans Benefits Administration, the Centers for Medicare & Medicaid Services, and the Social Security Administration, which make up 76 percent of the spending in our data. The high variation category comprises hundreds of smaller programs.

The mean inflation-adjusted value of total outlays per district ranges from $1.56 billion in 1984 to $3.33 billion in 2003. The median value increases from $1.37 billion to $3.03 billion. The mean value of district-level high variation program outlays ranges from $398 million in 1984 to $753 million 2003. The median value increases from $151 million to $361 million. Because these high variation programs should be especially susceptible to political manipulation, we focus on them in the main analyses that follow.

We do not argue that affiliation with the president is the sole determinant of the flow of federal funds to a district. Indeed, an obvious concern with any attempt to isolate the effect of politics on distributive spending is that there are many other attributes of districts—both observable and unobservable to the analyst—that influence the receipt of federal outlays. To control for such district-level factors, we use a differences-in-differences approach based on district and year fixed effects. Moreover, because district boundaries change following redistrictings that occur during our study period, we use redistricting-specific fixed effects, for a total of 1,589 unique district fixed effects.[10]

More formally, we specify the following basic model:

[9] The results presented in later pages are not sensitive to changes in the coefficient of variation cutoff. We experimented with several coefficient-of-variation thresholds greater than 3/4, none of which produced notably different results. Details are provided in the data appendix.
[10] Additional details are provided in the data appendix.

$$\ln(\text{outlays}_{it}) = \beta_0 + \alpha_i + \delta_t + \beta_1 P_{it} + \boldsymbol{\psi}\mathbf{X}_{it} + \varepsilon_{it}$$

where subscript *i* denotes (redistricting-specific) congressional districts and *t* denotes time. The main variable of interest is P_{it}, which is a dummy variable equal to one if the district's representative is of the same party as the president. We include dummies for all but one year, δ_t, to control for secular changes in federal domestic spending over time. The vector $\mathbf{X}_{it}$ denotes other legislator characteristics, explained below. We account for all observable and unobservable, time-invariant district characteristics by including α_i, which are redistricting-specific congressional district fixed effects. β_1 and $\boldsymbol{\psi}$ are regression coefficients, β_0 is a constant, and ε_{it} is an error term.

This model specification allows us to ask whether a district receives more federal spending during the years in which its representative is a member of the president's party. Identification in our models comes from two sources of within-district, within-redistricting period variation. First, holding the identity of the president constant, a district may change its affiliation with the president when it elects a new representative. For instance, a district may replace its Republican representative with a Democrat, which we predict should increase its receipt of federal outlays if the president is also a Democrat. Second, holding the identity of a district's representative constant, the district's alignment with the White House may change with the election of a new president. For example, we would predict that Republican-represented districts will see increases in federal aid when a Democratic president is replaced by a Republican president. Within the data, we find substantial evidence of both kinds of variation. Specifically, we have 979 cases of within-district change in affiliation with the president's party, of which 760 are associated with the election of a new president and 219 are associated with the election of a new representative, holding the president constant.

In addition to partisan considerations, presidents may also focus on electoral ones. Cohen, Krassa, and Hamman (1991) show that presidents are more likely to campaign for midterm Senate candidates in states where the president runs strongly and where the race is competitive. Updating that analysis, Sellers and Denton (2006) document that presidents campaign in states with competitive Senate races, with more electoral votes, and those where the president won a larger share of the vote in the last election. More directly, Larcinese et al. (2006) show that presidents direct more federal spending to states where they won more of the popular vote. Turning to House elections, Jacobson, Kernell, and Lazarus (2004) similarly find that Bill Clinton was most likely to campaign in districts with electorally vulnerable, Democratic incumbents. To allow for the possibility that districts in swing states will be lavished with federal projects, we therefore control for the absolute value of the state-level vote margin in the preceding presidential election.

We include a variety of covariates that are specific to each congressional representative. We first identify actors whom the prior literature suggests should do well in the budgetary process: committee chairs, party leaders, members of the majority party, and members of the prestigious Appropriations and Ways and Means committees. We include a dummy variable for representatives elected in close races (less than 5% victory margin) to control for the possibility that electorally vulnerable members receive priority in discretionary spending (Stein and Bickers 1995). To control for the possibility that inexperience or lack of seniority impede a member's ability to secure program benefits, we include a dummy variable for representatives in their first term. Finally, as previous studies have shown that Democrats bring home more federal spending than Republicans, we control for the member's party affiliation (Alvarez and Saving 1997b).

Notice that our model does not explicitly control for district-level demographics. Because district demographics are only measured once within a redistricting period—in the decennial census—we do not observe variation over time within redistricting periods. Therefore, the redistricting-specific district fixed effects subsume decennial census variables. Given that we are not primarily concerned with estimating relationships between demographics and federal spending, the fixed effects specification appropriately identifies the effects of political variables purged of time-invariant district-level attributes.[11] The fixed effects also capture any time-invariant state-level factors that influence federal spending, such as advantages or disadvantages due to malapportionment in the Senate (Lee 1998).

Finally, even with a broad set of control variables, the unobservable, time-variant predictors of federal spending within a particular district are likely to be correlated across time periods. And the geographic distribution of federal spending may reflect the effects of Senators as well as the quality and effort of House members, suggesting that there may be correlation across districts within a state. We therefore use robust standard errors clustered by state in all of our models.[12]

Main Results

Table 1 presents the results of our fixed effects models of high-variation program spending. In model (1), we include a dummy variable for members of the president's party with no other control variables except for the year and district fixed effects. This simple model

[11] A random effects specification would allow us to estimate the effects of district-level demographics. However, Hausman tests reject random effects in favor of fixed effects in all our models at $p < .001$.

[12] Proper estimation of standard errors in panel data models is a topic that has received substantial attention over the past few years. Wooldridge (2006) provides a useful review of the issue and estimation techniques. Peterson (2007) provides extensive simulation results comparing different techniques, which favors the use of clustered standard errors for panel data. We have tried several different methods for calculating standard errors and found clustering to be the most conservative approach for our data (i.e., producing the largest standard errors). We also recognize that individual legislator characteristics, rather than congressional district characteristics, present another potential source of dependence in the observations (see Primo, Jacobsmeier, and Milyo 2007). Clustering by individual legislators does not change the statistical significance of our results appreciably, nor does clustering by congressional district.

indicates that a district receives about 4 percent more federal spending when its representative is in the same party as the president. In model (2) we add dummy variables indicating other actors who may have influence in the budgetary process: committee chairs, ranking minority members of committees, party leaders, members of the Appropriations and Ways and Means committees, and members of the majority party. The effect of the president's party remains virtually unchanged, while none of the additional variables demonstrates a statistically significant effect on distributive spending. We will have more to say about some of these null results below.

In model (3) we introduce additional legislator attributes that may influence federal spending. We find that representatives who were elected in close races receive about 8 percent more federal spending, consistent with the notion that members of Congress direct resources to their more vulnerable colleagues. Members receive about 3 percent less spending in their first term, suggesting that inexperience or lack of seniority are disadvantages in the budgetary process.[13]

The coefficients for several additional control variables carry the expected signs but fall short of statistical significance. Consistent with Levitt and Snyder (1995), we find that Republicans deliver fewer federal dollars to their districts, a finding, though statistically insignificant, that may reflect conservative distaste for federal programs or an entrenched Democratic advantage in programmatic politics. Note that this result is distinct from whether one is in the majority or minority. Membership in the majority party has the expected positive effect but does not achieve statistical significance. Finally, we observe that districts in swing states—where the presidential vote margin was closer—receive more federal spending, though here again the effect is insignificant statistically.

[13] We explored more complex ways of measuring the relationship between seniority and spending. Aside from the first-year deficit, we did not find that additional terms in office were associated with additional district spending.

Including a full set of committee membership dummy variables, as we do in model (4), does not notably alter our estimates for any of the control variables. Moreover, the estimated 5 percent advantage for members of the president's party appears unaffected by the additional committee membership indicators. With districts receiving, on average, $549 million each year in high-variation program spending, the estimated 5 percent reward for the president's co-partisans amounts to about $27 million annually per district, or roughly $50 per capita.

Finally, to confirm that the results we observe in Table 1 reflect a general pattern of presidential influence, rather than the idiosyncratic efforts of a particular president, we reran versions of models (3) and (4) sequentially dropping one president at a time (not reported). The estimated coefficients for the presidential spending advantage were significant in every case and we could not reject the hypothesis that they were equal across the models. Thus, our results are not being driven by any particular president. Rather, they reflect a general pattern across all the administrations in our study period.

Targeting Presidential Benefits?

The evidence presented in Table 1 provides strong support for our central hypothesis: that members of the president's party will be advantaged in the contest for distributive benefits. In our theoretical discussion, we also suggested several ancillary hypotheses related to the more narrow targeting of benefits to *specific* members of either the president's own party or the opposition. Table 2 presents tests of these hypotheses by estimating a series of models in which the presidential dummy variable is interacted with other variables of interest. To conserve space, we only report coefficients for the primary variables of interest, although the full set of control variables used above is included in all of the models in Table 2.

First, in model (1) we test the hypothesis from McCarty (2000a) that the presidential spending advantage will shrink as the size of the president's party increases. To do so, we interact the presidential dummy variable with a variable measuring the size of the member's party.[14] The interaction term is positive and highly significant, which appears to run contrary to McCarty's predictions.[15] Meanwhile, the main effect of party size is negative, suggesting that members of the opposition party obtain fewer benefits per district when their party is larger. We note, though, that this party size effect and its interaction with the president's party indicator are quite sensitive to model specification, and disappear when using different functional forms for party size, such as taking the logarithm.

Second, we hypothesized that the president may engage in vote-buying, with the implication that moderates in the opposition party would receive more federal program spending. To test this hypothesis, we use each member's distance from the House median voter, measured in terms of first-dimension DW-NOMINATE scores, as an indicator of the probability that the member's vote could be pivotal, making her a potential target for vote buying. In model (2) of Table 2, we estimate the interaction between our presidential dummy variable and the member's distance from the median voter. This allows the distance from the median voter to have a different slope with respect to spending for members of the president's party and members of the opposition. Neither the main effect of the distance measure nor its interaction with the presidential dummy is significant, indicating that moderate members do not obtain more spending and that the effect is no different for members of the president's party relative to the opposition.

[14] In this model, we drop the indicator variable for majority party status, given its co-linearity with party size.
[15] Based on McCarty (2000a; 2000b), one might view the positive interaction between the presidential dummy and party size as evidence against the influence of presidential proposal power and in favor of other sources of presidential influence, such as the veto. However, given the fragility of this result in our model, we do not wish to over-interpret the interaction term.

In model (3) we investigate the possibility that the president will differentially target benefits to electorally vulnerable members of his own party, but to electorally secure members of the opposition party. To do so, we interact the presidential dummy variable with the dummy indicating whether the member was elected in a close race. The close race dummy remains highly significant and positive, but the interaction term is nowhere near significant. In other words, electorally vulnerable members of the both the president's party and the opposition receive significantly greater federal program spending. Finally, in model (4) we use the freshman dummy variable as an additional indicator of vulnerability, and again we find no evidence of a differential effect for members of the president's party.

It bears emphasizing that with the introduction of various interactions in Table 2, the coefficient for the presidential main effect hardly changed at all. To facilitate the interpretation of these results, we mean-deviated the two continuous interacting variables—distance from the House median voter and party size—so that the reported coefficient for the presidential dummy variable can be interpreted as the presidential effect for a member with the average value of the interacting value. For the two dummy interaction variables—close elections and freshman—the presidential coefficient reflects the effect of being in the president's party for members not in close elections or their first term, respectively. In all cases, the estimated presidential main effect is roughly 5 percent.

Robustness Checks and Extensions

The results presented in the preceding section demonstrate that a district receives more federal funding when its representative comes from the president's party. Meanwhile, we found little support for the two main competing theories of institutional influence in distributive politics, namely those pertaining to committees and the majority party. In this section, we explore the

sensitivity of both our positive and null findings. To conserve space, we do not report additional tables in this section but all results are available on request.[16]

Ideology versus Party

Our first robustness check contrasts the effects of presidential partisanship with ideological factors. While the literature does not provide much guidance on how the ideological locations of legislators might influence the flow of benefits to their districts, we allow for several possibilities, focusing on members' proximity to prominent actors in the budgetary process. Using first dimension DW-NOMINATE scores, we measure each member's distance from the chamber's median voter, to allow for the possibility that members who are more likely to cast decisive votes will be able to extract programmatic benefits for their districts. Next, we measure the distance of each member from the median member of the majority party, capturing the possibility that the dominant party rewards members whose voting patterns reflect the party's platform. Finally, we assess whether loyalty to one's own party, regardless of majority status, attracts more district funds. We measure party allegiance using two variables: the member's distance from the median own-party NOMINATE score; and the standard party unity score, which measures the percentage of times a member votes with her party when the parties are divided.

None of the four additional measures demonstrated much influence.[17] There is weak evidence that members who tow the party line secure more federal outlays: ideological closeness to the own-party median voter and party unity scores are both positively associated with spending, but neither relationship is statistically significant. Ideological distance from the chamber's median voter was positively, though insignificantly, associated with spending.

[16] For review purposes, we have included all the additional tables in a for-referees-only appendix.
[17] Referees, please see Table R1.

Distance from the majority median voter was negatively associated with spending, though again the effect was small and statistically insignificant. More importantly, even after controlling for these various measures of ideology, our estimates of presidential influence are essentially unchanged at roughly 5 percent.

Policy Specific Effects

Our preceding analysis did not reveal significant effects of committee assignments on *aggregate* district spending. The literature on congressional committees, however, also emphasizes the influence of members over the *specific programs* under the direct control of their committees. While we have no specific hypotheses about which programs the president will seek to influence, we do want to give the competing theories a fair shake. We therefore explore the influence of committee membership on spending from programs under the committee's purview.

In four policy domains, we were able to separate the annual district-level spending sums by originating agency. We then matched the spending patterns of each agency to the primary committees that oversee it. We focus on four agencies: the Department of Health and Human Services (which accounts for 25 percent of all high variation spending during our period of study), the Department of Agriculture (23 percent), the Department of Transportation (14 percent), and the Department of Education (14 percent). Scholars generally agree that "pork barrel" considerations, which are so common in the standard view of distributive politics in Congress, are especially prevalent in these four policy domains. As before, we limit the analysis to high variation programs where legislators are in a position to influence spending flows to their districts.

Even after matching spending from specific agencies to the committees that govern them, we do not find compelling results for committee membership.[18] When we reprise our main specification for each of these three policy areas, including indicators for whether the legislator is a member of the relevant committee of jurisdiction, we do find that committee members overseeing agriculture, public works, and health policy obtain more spending in their particular jurisdictions, but the results are not significant. The null result for public works spending is especially surprising to advocates of the traditional distributive model, since this is the area often thought of as most amenable to pork barrel politics (e.g. Ferejohn 1974). If true, then it would appear unlikely that repeating this analysis for other committees such as Judiciary or Banking would yield any more evidence in favor of committee influence. Meanwhile, members of the president's party receive between 6 and 12 percent more spending in each of the policy domains, and the estimated effects are statistically significant in education and health care.

If we exclude the fixed effects from the estimating procedure, we find significant results for committee membership in three of the four models: members of the Agriculture committee appear to get more spending from agriculture programs, and members of Appropriations and Ways and Means obtain more spending from both the Department of Education and the Department of Health and Human Services. The difference between the fixed effects and the OLS results is telling, as it suggests that members of the relevant committees do secure more (in the cross-section) from the expected programs, but that they also secured more before and after serving on the committee.[19] This fact may help explain why some earlier research that relied

[18] Referees, please see Table R2.

[19] We also estimated random effects (RE) models and found significant effects of membership on the Agriculture committee. As noted earlier, though, RE assumes that the unobservable district-level effects are uncorrelated with the other explanatory variables. When this assumption is satisfied, RE estimates will be consistent and efficient whereas FE estimates will be inefficient (though still consistent). However, when the assumption is violated, as a Hausman test indicates is the case here, RE estimates will be inconsistent. Regardless, it is worth noting that the

heavily on cross-sectional data or short time series found sporadic evidence for committee effects, and we do not. Prior studies could not isolate the direction of causation, that is, whether membership on the Agriculture committee allows a representative to secure more agriculture spending, or whether representatives who demand more agriculture spending sort onto the agriculture committee. Our results support the latter contention.

We do not claim, however, that our findings demonstrate that committee membership confers no benefits whatsoever. The fact that we do not find evidence of committee effects on the distribution of federal outlays does not rule out the possibility that committee members exert disproportionate influence over other aspects of the policy-making process in their jurisdictions—such as the probability that certain kinds of bills receive a vote or the types of bureaucratic oversight that occur. Moreover, we cannot rule out the possibility that key decisions about the distribution of spending occurs primarily at the subcommittee rather than committee level. Finally, we recognize the possibility that there is not enough within-district variation in committee membership to allow its effects to be precisely estimated in our fixed effects model. For example, whereas we have nearly 1,000 cases of within-district change in partisan alignment with the president, we have only 180 cases of within-district change in membership on the Agriculture Committee.

Defense Spending

Though otherwise comprehensive in scope, the FAADS data omits defense spending. To examine whether our main findings are sensitive to its exclusion, we re-estimate our main models on an entirely different dataset that includes defense spending: the Consolidated Federal Funds Report (CFFR), which the federal government releases on an annual basis. The CFFR

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estimated effects of membership in the president's party remain significant even when (inappropriately) using OLS or RE.

contains information on military spending and government procurement, information not available in FAADS. Scholars of distributive politics, however, generally prefer FAADS because it provides greater detail on the recipients of government aid and their geography (see Bickers and Stein 1990). In any case, our basic result stands using either data set. The presidential spending advantage is 4 percent when we use the CFFR data, and remains significant at the 1 percent level.[20] At the same time, the majority party effect becomes significant ($p < 0.05$) when using the CFFR data, and remains comparable in magnitude to the effect obtained using the FAADS data, at roughly 3 percent.

Number of Awards versus Spending

Stein and Bickers (1995) argue that voters reward politicians for the number of projects delivered to their district rather than the aggregate level of funding. While we believe that outlays measured in dollars more closely reflect the theories of distributive politics that we are attempting to test, we have also run our models on the number of program awards as a robustness check. Specifically, using Bickers and Stein's data on the number of total awards and the number of newly enacted awards by district from 1984 to 1997 as the dependent variable, we replicated model (3) from Table 1.[21] The presidential effect is positive in both cases, with results suggesting that members of the president's party receive between 5 and 7 percent more program awards, although the effects fall short of statistical significance, possibly because of the smaller sample size in these models. We also found that members of the majority party, ranking committee members, Republicans, and freshmen all received significantly fewer total awards, though none of these effects are significant in models that focus on newly enacted awards, and none of the other variables in either model demonstrated a significant effect.

[20] Referees, please see model (1) of Table R3.
[21] Referees, please see models (2) and (3) of Table R3.

Universalism

In the face of null findings for several prominent theories of distributive politics, one may wonder whether program spending is, instead, universalistic, with a roughly equal share going to each member. Universalism is a popular explanation for both how Congress operates on its own (Niou and Ordeshook 1985; Shepsle and Weingast 1987; Weingast 1979) and for how presidents try to influence goings on within it (Fitts and Inman 1992; Inman 1993). Universalism thus deserves our consideration as an alternative to our empirical findings.

To gauge the extent of geographic variation in spending during the time period under consideration, we disaggregated the data by year and calculated some basic descriptive statistics.[22] The 75th percentile district receives on average twice as much spending from high-variation programs as the 25th percentile. The standard deviation of spending is nearly equal to the mean in every year. Moreover, we note that in 15 of the 21 years in our period of study there are some districts that receive *zero* spending from new programs, which would appear to contradict the most basic "something for everyone" notion of universalism.

The Missing Majority

Throughout our analyses, we found mixed evidence that members of the majority party have an advantage in securing spending from federal programs. Though the effects are almost always positive, they only attain statistical significance in the model using CFFR data. While not the central question of this paper, the weak findings for majority status are surprising in light of past research that emphasizes the influence of party cartels in Congress on budgetary outcomes (e.g., Kiewiet and McCubbins 1993). As a final robustness exercise, we investigated whether specific members of the majority party experience a spending advantage. More exactly, we estimated a series of models in which the majority party dummy variable was interacted with

[22] Referees, please see Table R4.

distance from the House median voter and electoral vulnerability, measured either by the close election dummy or the freshman dummy.[23] The majority party dummy variable remained positive in every case, but never attained statistical significance. Among the interaction effects, only that with the freshmen dummy yielded a statistically significant effect, though the sign is perverse, as it suggests that freshmen in the majority receive significantly less than do freshmen in the minority, all else equal. However, this relationship is significant only at the 10 percent level. On the whole, then, we uncovered no evidence that moderates or electorally vulnerable members in the majority received more federal benefits.

By all indications there is considerable variation to be explained in the distribution of federal benefits across districts. Models that focus on the majority party simply do not appear to do a good job of it. The influence of majority party status, instead, may lie elsewhere, affecting such things as: which bills are voted on, what policies are subject to congressional hearings, the content of symbolic or moral legislation, how campaign funds are distributed, and how internal legislative resources are allocated (e.g., Binder, Lawrence, and Maltzman 1999; Cox and Magar 1999; Cox and McCubbins 2005, 2007; McCarty, Poole, and Rosenthal 2001; Smith 2000).

Conclusion

To this point, conventional wisdom has dictated that all legislators wish to divert federal spending to their districts, and that certain party and committee leaders are better equipped to do so. If true, then members in the majority party as well as those who hold party and committee leadership positions should secure a disproportionate share of federal benefits. The empirical basis for these claims, though, remains weak. Most studies are plagued by data limitations that make it difficult to generalize across committees or to separate majority party membership from

[23] Referees, please see Table R5.

partisan affiliation; and virtually all ignore the fundamental role of the president in budgetary politics.

Analyzing a comprehensive database of domestic federal spending, we show that members of the president's party receive systematically more federal outlays than do members of the opposition party. In addition, districts where a legislator or states where the president narrowly won their last elections secure more funds. Districts represented by freshmen, meanwhile, tend to receive less federal spending.

Importantly, we find mixed evidence that members of the majority party obtain more federal outlays, and no evidence that committee assignments, party leadership positions, or other institutional positions of power bestow an advantage in the geographic distribution of federal spending. These factors may be important in affecting such things as roll call votes and the lawmaking process more generally. In shaping distributive politics, however, the president appears to predominate.

Our empirical findings, we recognize, do not distinguish among the various sources of presidential influence. We have highlighted the president's ability to propose a budget, rally legislative and public support for it, threaten to veto deviations from it, and then, once enacted, to further manipulate how moneys are actually spent. As McCarty (2000b) notes, it is extraordinarily difficult to isolate the relative importance of different elements of the president's arsenal (in particular, proposer and veto prerogatives) in influencing policy outcomes. Indeed, it is not even clear that the influence of these elements is additive in nature. While veto and proposal prerogatives typically reinforce one another, McCarty shows that there are some conditions under which they may act at cross purposes; and no one has demonstrated how, theoretically, the full panoply of presidential prerogatives interact with one another. Having

established the importance of the president in distributive politics, future empirical and theoretical work would do well to investigate the particular conditions under which different prerogatives either individually or interactively yield varying levels of influence over the final distribution of outlays.

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Table 1. Determinants of Federal Spending by Congressional District

Variables	(1)	(2)	(3)	(4)[1]
President's Party	0.044***	0.048***	0.049***	0.049***
	(0.009)	(0.011)	(0.013)	(0.013)
Majority Party		0.012	0.013	0.016
		(0.029)	(0.028)	(0.027)
Committee Chair		0.005	0.003	-0.005
		(0.041)	(0.041)	(0.046)
Ranking Committee Member		-0.008	-0.014	-0.017
		(0.038)	(0.040)	(0.046)
Party Leader		-0.067	-0.073	-0.066
		(0.110)	(0.108)	(0.103)
Appropriations Committee		0.028	0.024	0.027
		(0.023)	(0.025)	(0.039)
Ways & Means Committee		-0.051	-0.054	-0.052
		(0.045)	(0.042)	(0.056)
Republican			-0.036	-0.036
			(0.028)	(0.028)
First Term			-0.034**	-0.032**
			(0.013)	(0.014)
Close Election			0.082***	0.077***
			(0.022)	(0.021)
President's Vote Margin			-0.121	-0.115
			(0.153)	(0.153)
Constant	19.501***	19.496***	19.502***	19.500***
	(0.038)	(0.043)	(0.051)	(0.067)
Observations	7939	7898	7882	7882
Number of unique districts	1589	1585	1585	1585
R-squared (within)	0.118	0.118	0.123	0.128

The dependent variable is the natural log of district-level funding from high-variation federal programs. Robust standard errors clustered by state are in parentheses. All models include district and year fixed effects. *Close election* is a dummy variable equal to one if the representative's victory margin in the preceding election was less than 5 percent. *First term* is a dummy variable equal to one for representatives in their first term. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[1]Dummies for individual committee positions included but not reported.

Table 2: Presidential Interaction Effects

Variable	(1)	(2)	(3)	(4)
President's Party	0.073***	0.050***	0.050***	0.052***
	(0.015)	(0.015)	(0.015)	(0.016)
Party Size	-0.002**			
	(0.001)			
President's Party x Party Size	0.005***			
	(0.001)			
Distance from House Median Voter		0.044		
		(0.119)		
President's Party x Distance from House Median Voter		0.049		
		(0.128)		
Close Election			0.087***	
			(0.028)	
President's Party x Close Election			-0.010	
			(0.037)	
First Term				-0.027*
				(0.015)
President's Party x First Term				-0.013
				(0.028)
Constant	19.422***	19.487***	19.503***	19.500***
	(0.034)	(0.053)	(0.050)	(0.051)
Observations	7882	7882	7882	7882
Number of unique districts	1585	1585	1585	1585
R-squared (within)	0.124	0.123	0.123	0.123

The dependent variable is the natural log of district-level funding from high-variation federal programs. Robust standard errors clustered by state are in parentheses. All models include district and year fixed effects, as well as all the control variables reported in table 1 (not shown). *Close election* is a dummy variable equal to one if the representative's victory margin in the preceding election was less than 5 percent. *First term* is a dummy variable equal to one for representatives in their first term. *Distance from the house median voter* and *party size* are both centered (i.e., mean-deviated) so the main effect for the president's party in these models can be interpreted as the effect at the average value of these variables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

APPENDIX A

TABLES FOR REVIEWERS ONLY

This Appendix contains the results of additional analyses discussed in the text. To conserve space, we do not envision publishing these tables should the article be accepted. However, we include them here to give reviewers full access to all of the model results discussed in the paper. Should the editor or reviewers prefer that we publish the attached tables, we are of course happy to do so. We would also be open to posting them on-line for interested readers.

Table R1: Controlling for Ideological Considerations

Variables	(1)	(2)	(3)	(4)
President's Party	0.052***	0.048***	0.049***	0.049***
	(0.017)	(0.014)	(0.014)	(0.015)
Distance from House Median Voter	0.069			
	(0.112)			
Distance from Majority Party Median Voter		-0.008		
		(0.084)		
Distance from Own Party Median Voter			-0.089	
			(0.116)	
Party Unity Score				0.089
				(0.089)
Majority Party	0.035	0.009	0.014	0.010
	(0.055)	(0.060)	(0.032)	(0.031)
Committee Chair	0.003	0.003	0.004	0.003
	(0.046)	(0.046)	(0.046)	(0.046)
Ranking Committee Member	-0.013	-0.014	-0.015	-0.013
	(0.045)	(0.044)	(0.045)	(0.045)
Party Leader	-0.075	-0.073	-0.079	-0.075
	(0.121)	(0.122)	(0.123)	(0.118)
Appropriations Committee	0.026	0.024	0.023	0.025
	(0.027)	(0.028)	(0.028)	(0.028)
Ways and Means Committee	-0.054	-0.054	-0.056	-0.055
	(0.047)	(0.047)	(0.048)	(0.047)
Republican	-0.038	-0.037	-0.037	-0.039
	(0.033)	(0.034)	(0.032)	(0.032)
First Term	-0.034**	-0.034**	-0.034**	-0.035**
	(0.015)	(0.015)	(0.015)	(0.015)
Close Election	0.081***	0.082***	0.082***	0.082***
	(0.024)	(0.025)	(0.025)	(0.024)
President's Vote Margin	-0.126	-0.120	-0.119	-0.127
	(0.172)	(0.171)	(0.172)	(0.172)
Constant	19.465***	19.508***	19.512***	19.429***
	(0.082)	(0.090)	(0.060)	(0.093)
Observations	7882	7882	7882	7882
Number of unique districts	1585	1585	1585	1585
R-squared (within)	0.123	0.123	0.123	0.123

The dependent variable is the natural log of district-level funding from high-variation federal programs. Robust standard errors clustered by state are in parentheses. All models include district and year fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table R2: Domain-Specific Spending Results

Variables	Agriculture (1)	Transportation (2)	Education (3)	Health & Human Services (4)
President's Party	0.076	0.121	0.090*	0.066**
	(0.062)	(0.145)	(0.054)	(0.027)
Agriculture Committee	0.015			
	(0.082)			
Public Works and Transportation committee		0.010		
		(0.178)		
Education and Labor Committee			0.008	
			(0.040)	
Energy and Commerce Committee				0.059
				(0.086)
Appropriations Committee	-0.033	0.211	-0.001	0.114**
	(0.155)	(0.306)	(0.043)	(0.056)
Ways and Means Committee	-0.028	0.248	-0.077	-0.057
	(0.084)	(0.362)	(0.085)	(0.060)
Majority Party	0.000	-0.246	0.062	-0.006
	(0.099)	(0.160)	(0.039)	(0.046)
Committee Chair	-0.048	-0.379	0.061	-0.014
	(0.117)	(0.287)	(0.079)	(0.068)
Ranking Committee Member	-0.088	-0.063	-0.006	-0.092
	(0.088)	(0.306)	(0.055)	(0.068)
Party Leader	-0.159	0.521	-0.114	0.054
	(0.135)	(0.655)	(0.270)	(0.255)
Republican	0.069	-0.127	-0.103***	-0.143***
	(0.082)	(0.188)	(0.038)	(0.034)
First Term	-0.064	-0.213	-0.023	-0.049
	(0.043)	(0.165)	(0.022)	(0.030)
Close Election	0.173**	0.427*	0.009	0.061
	(0.080)	(0.240)	(0.025)	(0.043)
President's Vote Margin	-1.281	-1.912	-0.103	-0.235
	(1.238)	(1.240)	(0.282)	(0.145)
Constant	16.455***	14.824***	17.149***	18.126***
	(0.137)	(0.280)	(0.053)	(0.055)
Observations	7882	7882	7882	7882
Number of unique districts	1585	1585	1585	1585
R-squared (within)	0.059	0.057	0.123	0.079

The dependent variable in each column is the natural log of district-level funding from high-variation federal programs administered by the named agency. Robust standard errors clustered by state are in parentheses. All models include district and year fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table R3: Alternative Expenditure Variables

Variables	CFFR Spending (1)	Number of Awards (2)	Number of New Awards (3)
President's Party	0.040***	0.045	0.066
	(0.012)	(0.038)	(0.045)
Majority Party	0.028**	-0.037*	-0.023
	(0.011)	(0.021)	(0.027)
Committee Chair	-0.001	-0.045	-0.027
	(0.026)	(0.035)	(0.060)
Ranking Committee Member	-0.061*	-0.060*	-0.005
	(0.032)	(0.032)	(0.052)
Party Leader	-0.006	0.012	0.112
	(0.038)	(0.106)	(0.180)
Appropriations Committee	0.023	0.008	0.058
	(0.027)	(0.047)	(0.057)
Ways and Means Committee	-0.024	0.009	-0.013
	(0.025)	(0.042)	(0.056)
Republican	0.012	-0.077*	-0.067
	(0.015)	(0.042)	(0.047)
First Term	0.001	-0.041*	-0.031
	(0.010)	(0.021)	(0.025)
Close Election	0.028*	0.015	0.039
	(0.014)	(0.023)	(0.028)
President's Vote Margin	-0.074	0.498*	0.585
	(0.188)	(0.286)	(0.405)
Constant	21.001***	7.526***	6.658***
	(0.063)	(0.050)	(0.084)
Observations	7882	5326	5326
Number of unique districts	1585	1077	1077
R-squared (within)	0.384	0.690	0.714
Years	1984 to 2004	1984 to 1997	1984 to 1997

The dependent variable in model (1) is the natural log of district-level funding from high-variation federal programs contained in the CFFR; in model (2) is the number of federal awards contained in FAADS; in model (3) is the number of new awards contained in FAADS. Robust standard errors clustered by state are in parentheses. All models include district and year fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table R4. Coefficient of Variation in District Spending

Year	Total Spending	Low-Variation Programs	High-Variation Programs
1984	0.26	0.23	0.95
1985	0.26	0.23	0.92
1986	0.25	0.22	0.95
1987	0.25	0.22	1.03
1988	0.24	0.21	0.89
1989	0.25	0.22	0.95
1990	0.22	0.22	0.73
1991	0.22	0.21	0.75
1992	0.25	0.22	0.98
1993	0.24	0.22	0.82
1994	0.25	0.22	1.01
1995	0.24	0.22	0.99
1996	0.23	0.21	0.88
1997	0.22	0.21	0.88
1998	0.23	0.21	0.91
1999	0.23	0.21	0.91
2000	0.22	0.21	0.75
2001	0.22	0.20	0.82
2002	0.28	0.25	1.01
2003	0.27	0.27	0.90
2004	0.27	0.25	1.02
Total	0.33	0.32	0.94

Each cell reports the coefficient of variation in district-level spending.

Table R5: Additional Majority Party Analyses

Variable	(1)	(2)	(3)	(4)	(5)
President's Party		0.051***	0.049***	0.051***	0.045***
		(0.015)	(0.013)	(0.013)	(0.012)
Majority Party	0.006	0.035	0.014	0.010	0.027
	(0.027)	(0.049)	(0.028)	(0.029)	(0.033)
Distance from House Median Voter		0.063			
		(0.106)			
Majority Party x Distance from House Median Voter		0.012			
		(0.123)			
Distance from Own Party Median Voter			-0.068		
			(0.118)		
Majority Party x Distance from Own Party Median Voter			-0.032		
			(0.223)		
Close Election				0.066**	
				(0.025)	
Majority Party x Close Election				0.032	
				(0.027)	
First Term					-0.001
					(0.021)
Majority Party x First Term					-0.057*
					(0.032)
Constant	19.53***	19.49***	19.50***	19.51***	19.50***
	(0.046)	(0.055)	(0.051)	(0.051)	(0.053)
Observations	7882	7882	7882	7882	7882
Number of unique districts	1585	1585	1585	1585	1585
R-squared	0.121	0.123	0.123	0.123	0.124

The dependent variable is the natural log of district-level funding from high-variation federal programs. Robust standard errors clustered by state are in parentheses. All models include district and year fixed effects, as well as all the control variables reported in table 1 (not shown). The president's party dummy variable is excluded from model (1). *Close election* is a dummy variable equal to one if the representative's victory margin in the preceding election was less than 5 percent. *First term* is a dummy variable equal to one for representatives in their first term. *Distance from the house median voter* and *distance from own-party median voter* are both centered (i.e., mean-deviated) so the main effect for the majority party in these models can be interpreted as the effect at the average value of these variables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

APPENDIX B:

Data Description

I. Federal Outlays

The federal spending data used for this project come from the Federal Assistance Award Data System (FAADS), which is a database operated by the Bureau of the Census within the U.S. Department of Commerce and overseen by the Office of Management and Budget. FAADS tracks the individual financial assistance award transactions of 33 executive branch departments and agencies, all of which have grant-making authority.[24] At the end of each quarter of the federal fiscal year, federal agencies submit to the Bureau of the Census the details of the financial assistance awards they made during that three month period.[25] The Bureau of the Census assembles these records into a quarterly report of federal assistance award transactions.

For each fiscal year, FAADS reports contain well over one million award transaction records. On average, FAADS contains records for 850 federal programs per year. The major omission is defense: military spending and defense procurement programs are not included. Despite these omissions and the exclusions we describe below, award transactions recorded by FAADS in fiscal year 2004 comprised about 60% of the total federal budget.

While FAADS records date back to 1981, the database did not track the geographic location of award recipients until fiscal year 1983. As of 1983, FAADS records contain detailed information on the nature of each award transaction, the specific program that awarded the funds, and the geographic location of the award recipient, including the recipient's state and congressional district.

Update of Bickers and Stein's Federal Spending Database

Bickers and Stein (1991, 1995) assembled and collapsed quarterly FAADS files from fiscal year 1983 to 1990 into annual data files. They later updated their data to include fiscal years 1991 to 1997. The complete database tracks the total dollar amount awarded by each federal program to recipients in each of the 435 congressional districts during each of the fiscal years.[26] We replicated the design of the Bickers and Stein federal spending database for 1998 to 2004 using the FAADS quarterly data files. The strategy we used to assemble the data matches the strategy used by Bickers and Stein to the closest extent possible.[27]

Following Bickers and Stein, we eliminated all award transactions of the following types: insurance, guaranteed or insured loans, direct loans, and other reimbursable, contingent, intangible, or indirect financial assistance. These types of assistance transactions reported by FAADS do not always represent a flow of federal funds to a recipient. In many cases, FAADS reports the total appraised value of assets insured by the government, representing the total amount of the government's liability if all asset-holders were to file claims at once.

Additionally, a small number of FAADS records reported recipients in congressional districts that do not correspond to actual districts. Some of these nonexistent districts actually did exist at one time but were redistricted away by a decennial redistricting prior to the year of the federal outlays record. Outlays

[24] For a list of these agencies, see Appendix A of the 2006 FAADS User Guide at http://ftp2.census.gov/govs/faads/guide2006.pdf (accessed July 11, 2007).
[25] Award records submitted more than 30 days after the end of the quarter are included in the FAADS report for the following quarter.
[26] Bickers and Stein (1991) include Washington, DC in their database, which we exclude.
[27] The detailed Bickers and Stein codebook can be accessed online at http://www.polsci.indiana.edu/faad/codebook.txt.

to these districts totaled $30.9 billion for 1998 to 2004. Other nonexistent districts that appeared in FAADS never existed at all. For example, FAADS reported outlays to an at-large district in Florida as well as to Florida's 80th congressional district, neither of which ever existed. Outlays to these nonexistent districts totaled just $139 million for 1998 to 2004. We deleted federal outlays observations if the congressional districts of the recipients were erroneously coded in either of these two ways.

The FAADS database has one significant limitation. It does not provide full individual award transaction history for large volume programs that make thousands or even millions of assistance awards every quarter, such as programs within the Social Security Administration, the Veterans Benefit Administration, and the Department of Health and Human Services. Instead, FAADS reports one aggregate dollar amount for outlays awarded from the program to a particular county in a given quarter. Unlike the individually reported action-by-action records, county-aggregate records do not directly identify the congressional district of each award recipient. Moreover, these county-aggregate records make up approximately 40 percent of the records in quarterly FAADS files and about 60 percent of the total federal spending reported. They pose a problem only for the small fraction of counties that contain more than one congressional district.

We used the same formula as Bickers and Stein to attribute spending reported in county-aggregate records to each congressional district: we multiplied the county-aggregate spending by the fraction of the total county population that lived in a particular congressional district at the time of the preceding decennial census. Therefore, for any congressional district that shares a county with another congressional district, federal spending from county-aggregate records is estimated.[28] For a county that contains two congressional districts, one that is home to ¼ of the county population and the other to the remaining ¾, this method would apportion ¼ of the county-aggregate outlays to the first congressional district and the remainder of the outlays to the second. Fortunately for our purposes, 88% of counties contain only one congressional district. For these counties, county-aggregate records were accurately attributed to a single district.

The annual data files created by Bickers and Stein for 1983 to 1997 also contain information on the number of new and continuing awards provided by each program. Due to some ambiguity as to how those variables were created in the Bickers and Stein annual files, we did not to recreate these variables for 1998 to 2004.

We next summed all the outlays received by each congressional district from various federal programs in a given year. The resulting data set of 9135 observations tracks federal outlays to 435 congressional districts from 1984 to 2004. (We explain our reason for excluding 1983 below.)

II. Legislator Characteristics and District Fixed Effects

Having assembled the federal spending data, we next matched the federal outlays for each congressional district in each fiscal year to an individual member of the House of Representatives. We extracted each legislator's name, party, and number of votes from Poole and Rosenthal's DW-NOMINATE data.[29] In cases where more than one representative held a particular House seat during a given congress, we chose the member who cast the greatest number of votes during that congress. In the one case where two representatives shared a seat and cast an equal number of votes, we selected the member who had held the seat for the greatest number of days during that congress.

[28] We note that much of this problem is also mitigated by our separation of federal programs into low and high variation programs, which we discuss below. County aggregate records generally represent low-variation programs, which are not central to our analysis.

[29] Poole and Rosenthal make their DW-NOMINATE data available online at http://voteview.com/ (accessed July 11, 2007).

The strategy we used to match the legislator information to the FAADS data warrants attention. As we mentioned earlier, FAADS is a collection of award transactions as reported on a quarterly basis by departments and agencies. While these records do not represent exact disbursements of funds organized like an accounting system, they do represent the initiation of an award obligation within the agency that administers the program. Oftentimes awards are recorded in a one-time fashion according to the fully obligated amount rather than as piecemeal expenditures over time. The agency then allows the recipient a certain length of time to exhaust the funds.[30] The budget and appropriations process in Congress, however, is concluded *before* the start of the new fiscal year when financial assistance awards are given out by federal agencies. It is therefore appropriate to attribute the federal outlays records for each fiscal year to the member of Congress who represented the congressional district in the calendar year prior. In other words, rather than attribute the spending received by Illinois' 10th congressional district during fiscal year 1994 to the member of Congress who represented that district in calendar year 1994, we attributed that spending to the member who represented the district in calendar year 1993. The appropriateness of this distinction becomes clear when we consider FAADS reports for the first quarter of every fiscal year. The first quarter of fiscal year 1989 ran from October 1 to December 31, 1988. It would be absurd to attribute program spending in late 1988 to a member of Congress who did not represent the district until 1989. In order to credit the members who administered the budget and appropriations process for the spending in the following fiscal year, we matched each FAADS observation to the member who held the seat in the calendar year before.

This matching strategy, while accurate, did present one problem. In fiscal years immediately following the introduction of new congressional district drawings, the congressional seats from the year prior are set according to old district boundaries. Not being able to match a representative to the district in the preceding year, we drop the first year of data after a district is redrawn.[31]

For purposes of computing congressional district fixed effects, we count each district as a unique entity during each redistricting period. That is, we estimate one fixed effect for each district in each redistricting period. If districts were redrawn only after the decennial censuses, this approach would produce 1,305 (435 *x* 3) unique district fixed effects. However, some districts are also redrawn in years between the decennial redistrictings, as a result of court challenges and other events.[32] Counting these additional episodes of redistricting, we arrive at a total of 1,589 unique district fixed effects. And after dropping the first year of data after a district is redrawn, our initial sample of 9,135 district-by-year observations is reduced to 7,939. The sample is slightly smaller in some models due to missing values for other covariates, as explained below.

For each legislator represented in the data set, we added committee membership and leadership variables using data from Garrison Nelson (2005) and Stewart and Woon (2005). Variables for each standing committee equal 1 if a particular member was on the committee and 0 if she was not. We also created a dummy variable equal to 1 if she was the chair of any committee and another dummy equal to 1 for ranking minority members of committees. The data also include a leadership variable that equals 1 if a member was the Speaker, a majority or minority leader, or a chief whip. Lastly, the committee and leadership data include a seniority variable equal to the number of terms a member served prior to the year of the federal outlays record. We were unable to match the committee, seniority, and leadership

[30] See Bickers and Stein 1991.

[31] Redistricting poses a particular problem for fiscal year 2002, since its first quarter outlays were reported according to 1990s district boundaries and its second through fourth quarter outlays were reported according to 2000s district boundaries by FAADS. To resolve this discrepancy, we excluded all outlays records from the first quarter of fiscal year 2002. Therefore, the annual congressional district file for 2002 only contains approximately 3/4 of the spending data for that fiscal year. We control for this irregularity by using year dummy variables in our analysis, so it will not bias our estimates.

[32] See Carson et al. (2007) for a discussion patterns of congressional redistricting over time. We are grateful to Jeff Clemens for drawing our attention to the importance of between-census redistricting.

variables to 14 unique members in our dataset; as a result, these variables are missing values for 46 observations.

It is also worth noting that we sorted independent members of Congress by caucus. Independent representatives who caucused with the House Democratic Caucus were considered Democrats, and those who caucused with the House Republican Conference were considered Republicans.

In addition to the district demographic control variables, we created a variable equal to the size of each House member's victory margin in his or her last congressional election. This data was drawn from two sources: For the 1982 election through the 1992 election, we used King's (1994) House election data. The congressional election margin variable is equal to the absolute value of the difference between the percentage of the two-party vote received by the Democratic candidate and the percentage of the two-party vote received by the Republican candidate. In cases where the Democratic or Republican candidate was unopposed by another major party candidate, the variable equals 1. In cases where King's election data set contained missing information, we used his Exceptions Data to fill in blanks wherever possible. For all but one district (represented by Bernie Sanders, an independent member), we considered districts with missing election data to be uncontested races.

For congressional elections from 1994 to 2002, we assembled the Election Statistics from the Office of the Clerk of the U.S. House of Representatives (2005b). Again, the margin variable is equal to the difference between the percentages of the two-party vote received by each major party candidate. The variable is missing a value for any election in which the winner was not a major party candidate.

During the time period spanned by our data, several members of the House came into office by special election. (As we mentioned earlier, for cases in which a House seat was held by more than one person in a given congress, we used the legislator data from the single member who cast the greatest number of votes during that congress.) We first identified the 47 individual members within our dataset who came into office by special election during a particular congressional term. Most of those special elections had primaries, which meant that their general elections paired one Democratic candidate against one Republican candidate. For these cases, we were able to calculate a two-party vote share margin. However, six members were brought into office via special elections without primaries, which meant that multiple candidates from each major party (and also candidates from minor parties) were allowed to run. For these open elections, we calculated the total number of votes for the two candidates who received the greatest number of votes. We then differenced the vote percentages received by those two candidates. Those vote percentages were based on the total number of votes received by those two candidates alone.

The congressional election data was matched to each legislator so that the victory margin variable corresponds to the election that brought them into office most recently.

Our data set also includes state-level vote share data for presidential elections. We take the absolute value of the president's vote share in the preceding election to capture the possibility that spending is targeted toward "swing" states (i.e., those where the election was close). We match these presidential margin variables into the federal spending data using a method similar to the one used for the congressional election margin variable. As an example, for federal spending in fiscal year 1997, the corresponding congressional calendar year would be 1996, and the most recent presidential election during the calendar year 1996 was actually in 1992. In other words, the 1980 presidential election corresponds to 1984-1985, the 1984 election corresponds to 1986-1989, the election in 1988 corresponds to 1990-1993, and so on.

III. Defining High- and Low-Variation Programs

In order to separate broad-based entitlement program spending from discretionary spending, we adopted a tactic used by Levitt and Snyder (1995). Specifically, we calculated coefficients of variation for each program contained in the FAADS data. The coefficient of variation for a federal program is equal to the standard deviation of its outlays across districts and years divided by the mean of its outlays across districts and years. A program that distributed an equal amount of funds to all 435 congressional districts in every year would have a coefficient of variation of zero.

We then used the coefficients of variation to split programs into categories. Levitt and Snyder found a "natural break" in their data at 2/3 and used that as a threshold for dividing programs into low and high variation types: programs with coefficients of variation greater than 2/3 were considered high variation programs, and programs with coefficients of variation less than 2/3 were considered low variation programs. Like Levitt and Snyder, when we tabulated the coefficients of variation for programs in our data, we discovered a natural break at about 3/4 (see figures A1 and A2). The difference between the thresholds was not surprising. Levitt and Snyder calculated an average amount of spending distributed to each district by each program for the entire period of 1984 to 1990 before calculating coefficients of variation. We calculated our coefficients of variation across districts *and* time, and we did so using nominal federal outlays figures. Therefore, we expected our coefficients of variation to be slightly greater than theirs.

Based on our threshold, we divided the programs into two categories: low variation programs have coefficients of variation less than 3/4, and high variation programs have coefficients of variation greater than or equal to 3/4. The low variation category includes 26 programs, all of which are housed in the Department of Veterans Affairs (mostly the Veterans Benefits Administration), the Department of Health and Human Services (mostly programs within the Centers for Medicare & Medicaid Services), the Social Security Administration, and the Railroad Retirement Board. Of the $20.8 trillion accounted for in 21 years of FAADS records, the 26 low variation programs make up 76%, or $15.9 trillion.

The coefficients of variation for low variation programs range from 0.25 to 0.73. The median coefficient of variation among low variation programs is 0.56, and the standard deviation is 0.16. Table A2 contains information on an illustrative subset of low variation programs.

Table A2: Examples of Low Variation Programs

Program Number	Program Name	Coefficient of Variation
96.001	Social Security Disability	0.55
96.002	Social Security Retirement	0.35
96.004	Social Security Survivors Insurance	0.34
96.006	Supplemental Security Income	0.60
93.773	Medicare Hospital Insurance (Medicare Part A)	0.39
93.774	Medicare Supplementary Medical Insurance (Medicare Part B)	0.50
93.778	Medical Assistance Program (Medicaid; Title XIX)	0.73

We consider the programs in the high variation group to be those for which fund allocation is more in the hands of national legislators (Levitt and Snyder 1995). The median coefficient of variation for high variation programs is 5.99. Table A3 presents a sample listing of high variation programs.

Table A3: Examples of High Variation Programs

Program Number	Program Name	Coefficient of Variation
11.429	Marine Sanctuary Program	14.37
15.022	Tribal Self-Governance	9.23
16.726	Juvenile Mentoring Program (JUMP)	5.16
66.467	Wastewater Operator Training Grant Program (Technical Assistance)	11.73
81.121	Nuclear Energy Research, Development and Demonstration	7.00
84.042	TRIO_Student Support Services	1.19
93.118	Acquired Immunodeficiency Syndrome (AIDS) Activity	4.10

Figure A1 shows the distribution of federal spending by coefficient of variation. The following figure A2 plots the frequency of coefficients of variation across federal programs for coefficients less than 3. Figure A3 shows the frequency of coefficients of variation for a greater range of coefficient values. Clearly, while there are very few programs with coefficients of variation between 0 and 1, those programs are by far the biggest spenders. There are many more programs with larger coefficients of variation, but all of those programs distribute significantly smaller amounts of federal funds.

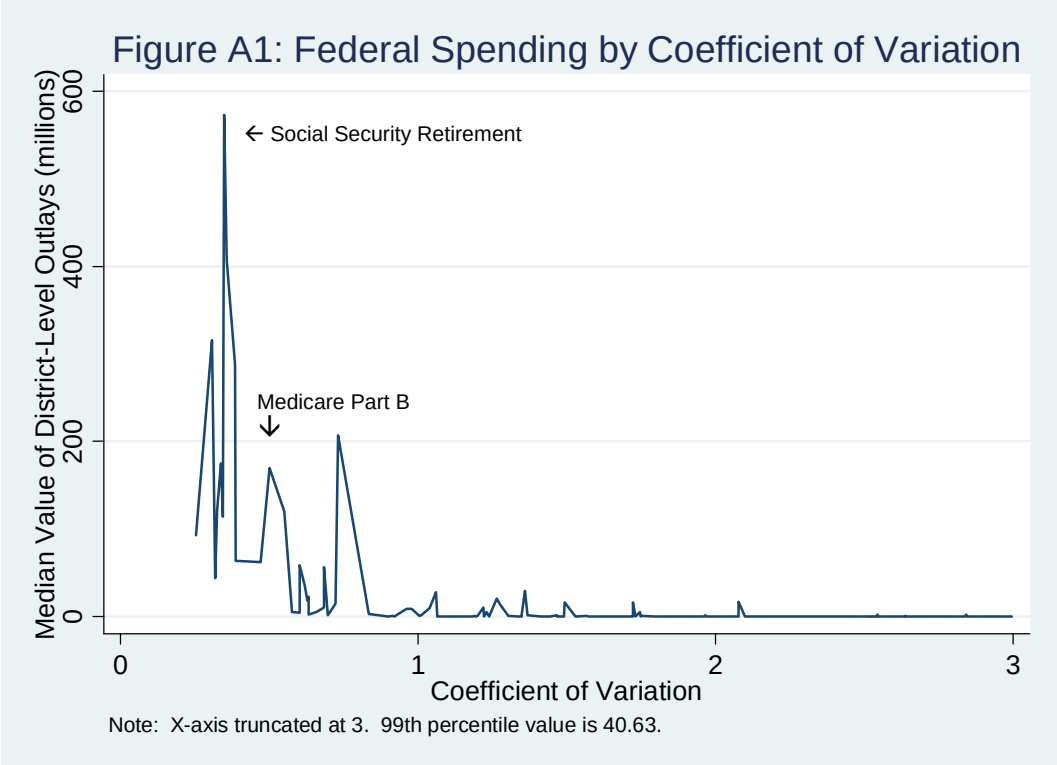
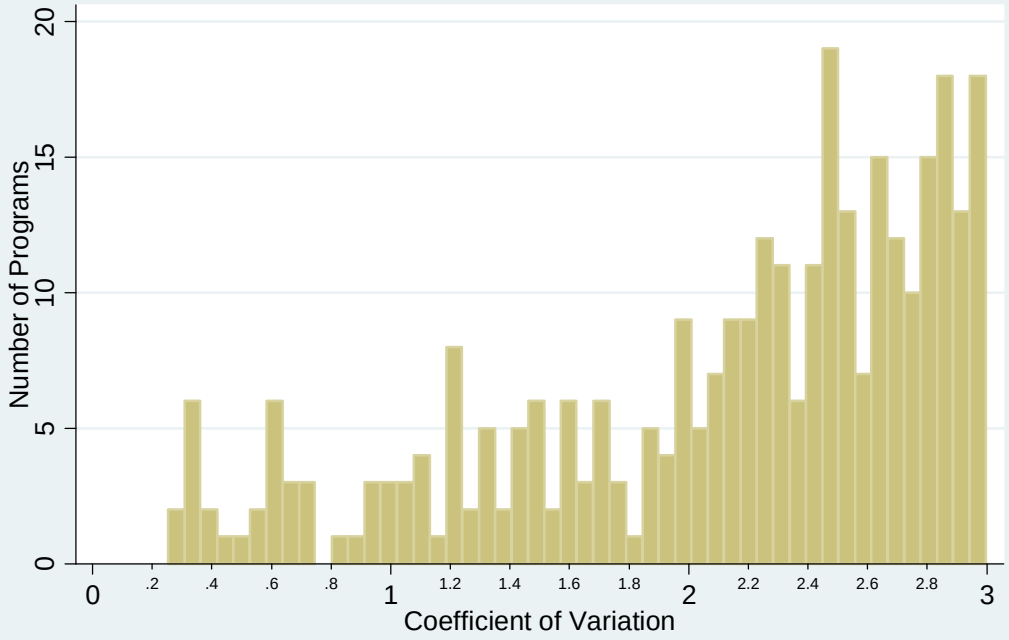
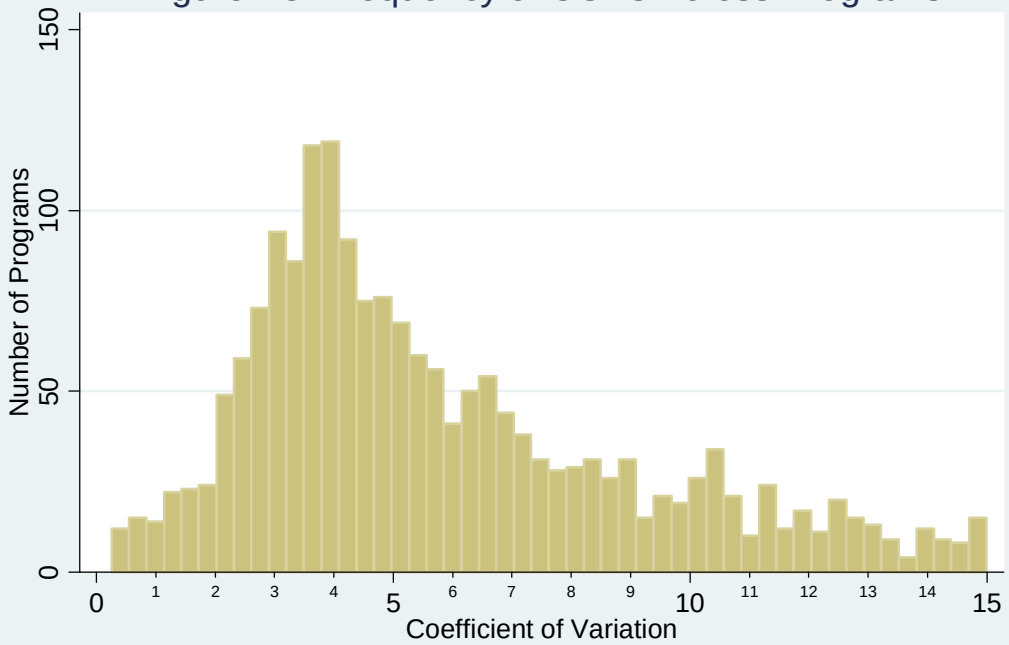


Figure A2: Frequency of COVs Across Programs - Detail



Note: X-axis truncated at 3. 99th percentile value is 40.63.

Figure A3: Frequency of COVs Across Programs



Note: X-axis truncated at 15. 99th percentile value is 40.63.

Sensitivity Analysis

Our choice of a 3/4 coefficient of variation threshold for dividing programs into high and low variation categories was necessarily arbitrary. While we discovered a break in the distribution of coefficients of variation at 3/4, we admit that we could easily have defended a handful of other threshold choices. For this reason, we ran each of the models from this paper using three additional thresholds to divide program spending into high variation and low variation categories. We found that variation in threshold choice did not have any substantial effect on the relationship between presidential party affiliation and high variation program spending.

We first selected three alternative coefficient of variation thresholds to divide the programs into categories. By closely examining the density of coefficients of variation across programs, we selected three coefficient values that corresponded to possible "breaks" in the distribution. The original threshold and the three additional thresholds are listed in table A5.

Alternative Thresholds for Defining High-Variation Program Spending

Threshold	*Coefficient of Variation*	*Mean High Variation Outlays (millions)*	*Median High Variation Outlays (millions)*	*Estimated Presidential Effect (standard error)*
1st	0.75	$549	$241	0.049 (0.013)
2nd	1.63	$443	$133	0.054 (0.016)
3rd	2.39	$406	$97	0.065 (0.018)
4th	3.30	$365	$64	0.061 (0.018)

Note: Presidential effects are estimated by running versions of model (3) from Table 1 using alternative definitions of the dependent variable.

The presidential spending result from the various models presented in the paper was resilient to increases in the coefficient of variation threshold. The first threshold is the one used in the paper. The effects were statistically significant ($p < 0.01$) for all versions of the dependent variable. Based on these estimates, our results do not appear sensitive to the criterion used to distinguish low- from high-variation program spending.