

**Congress, in Theory: Subjecting Canonical Models of Distributive Politics to  
Basic (but Long Overdue) Empirical Tests**

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## **Abstract**

More than any other institution, political theorists have set their sights on Congress. Canonical models of universalism, majority party control, and the median voter all generate clear and competing predictions about the relative influence that members of Congress wield in the legislative process. To date, though, empirical scholars have not examined the ability of these theories to explain one of the most fundamental functions of Congress—namely, how to allot programs (and their attendant monies) across districts. Using the most comprehensive dataset ever assembled on the geographic distribution of federal outlays, this paper subjects these three theories to a series of critical tests. We find very little support for any them, suggesting that our conventional understandings of legislative bargaining are, at best, incomplete.

Some of the oldest and most respected models in political science attempt to account for—in Harold Lasswell’s (1935) classic formulation—“who gets what, when, and how.” The workhorse models of distributive politics—which emphasize universalism, the median voter, and party control—all generate clear predictions about the relative influence that different individuals wield in a legislative process. So doing, they provide cogent accounts of the ability of these individuals to direct federal benefits to their home districts.

Curiously, few empirical tests of these models actually address Lasswell’s edict. For the most part, tests are based upon roll call votes, bill sponsorships, restrictive rules, and committee assignments, all elements of the legislative process. Few scholars have examined whether these theories adequately explain basic patterns in legislative outputs. Though they have thoroughly examined the ability of these theories to explain the internal workings of Congress, scholars have yet to systematically consider whether these theories account for the production of different legislative outcomes.

Though theories of distributive politics generate reasonably clear predictions about members’ influence over a wide range of legislative outcomes—co-sponsorships, symbolic resolutions, foreign policy enactments, and regionally and nationally focused legislation—many of these outcomes are not easily linked with specific districts; and those that are tend not to permit comparisons across districts. This paper, therefore, focuses on the dollars that flow from the federal government to congressional districts around the country. In addition to capturing elements of these other outcomes, federal spending offers the added benefits of being readily quantifiable, geographically tracked, and highly salient to the electoral fortunes of individual members.

Our investigations draw upon the most complete dataset of federal outlays ever assembled, tracking all non-defense federal outlays to every congressional district over a 20 year period. These outlays aggregate spending across literally thousands of different federal programs, yielding a comprehensive measure of every elected legislator’s ability to direct federal benefits to his or her home district. The basic patterns of spending, however, bear little relationship with the canonical theories of distributive politics. The vast majority of tests yield null results. The few positive findings that are observed, meanwhile, cannot be easily explained by the theories, as conventionally understood.

The remainder of the paper unfolds as follows. First, we briefly review the three canonical theories of distributive politics and then summarize the existing empirical tests of them. We then describe our data sources and the nature of the analyses to be conducted. Subsequently, we provide the results of those analyses, and we then conclude.

## Canonical Theories

Who, within Congress, is best positioned to secure a disproportionate share of federal outlays? More specifically, how does a member’s ideological orientation and/or partisan affiliation affect her chances of directing federal benefits to her home district? From the vantage point of theory, political scientists would appear well equipped to answer these foundational questions. This section briefly summarizes three of the most prominent theories of distributive politics, which emphasize the importance of the median voter, majority party control, and universalism. We then relate each theory to the empirical tests that follow.

*The Median Voter Theorem.* This widely known result holds that in a unidimensional policy space with single peaked preferences, the winning proposal locates at the median voter’s

ideal point (Black 1958). This essential logic informs, among others, Krehbiel's (1991) informational model of legislative organization and policy making. In each of these analyses, policy outcomes reflect the preferences of the median legislator. Indeed, the median legislator's support is a prerequisite for success, for any proposal that does not adequately satisfy her demands can be defeated by another proposal that does.

In a related literature, models of legislative bargaining often predict that "minimum winning coalitions" will be the primary vehicle for passing legislation (Baron 1991; Baron and Ferejohn 1989; Buchanan and Tullock 1962; Riker 1962). Extensions of this work assume that coalitions must be ideologically contiguous (Axelrod 1970). Because they lower bargaining costs, these "minimal connected winning coalitions" are more likely than simple minimal coalitions. Importantly, in these models the winning coalition always includes the median voter, and frequently legislators near the median. A simple augmentation of the median voter theory thus predicts that members of the winning (connected) coalition will receive more benefits than those outside of the coalition and hence the median voter is still advantaged.

In combination, the median voter theorem and work on minimum winning coalitions generate clear predictions about the distribution of federal outlays. Assuming that constituents typically prefer more spending in their districts to less, and that the median voter must be included in every successful legislative bargain, programs should award more benefits to the median legislator's district and fewer benefits as one moves outward ideologically from the median.

*Universalism.* Other research is skeptical about whether minimum winning coalitions actually emerge from legislative bargaining, since coalitions are often larger than would seem necessary. Groseclose and Snyder's (1996) competing vote buyer model, for example, predicts the regular occurrence of supermajorities of 60 to 80 percent (see also Baron 1989). Indeed, Riker's original theory of coalitions noted that uncertainty about other legislators' preferences could produce oversized majorities. Evans (2004) similarly argues that leaders buy more than a majority of legislators' votes because of this uncertainty. If the uncertainty is great enough, the coalition size could easily be expanded from oversized majorities to a universal coalition of the whole.

The notion of universalism flows from a more overtly distributive view of legislative politics. Under this approach, scholars assume that members of Congress represent their constituents' demands for substantial shares of government largesse. The trouble, of course, is that individual members require different programs to advance their shared interest in re-election. Hence, there emerges a strong committee system that helps to institutionalize logrolls that benefits all districts (Weingast and Marshall 1988). In this arrangement, minimum winning coalitions are not dominant because legislators cannot predict whether they will be in a coalition or not (Weingast 1989). The traditional arguments in favor of universalism (Shepsle and Weingast 1981; Weingast 1979) are based on the assumption that each minimal coalition is equally likely to form. If all minimum coalitions are equally possible, legislators prefer universal coalitions in which every district receives benefits. Other prominent models using somewhat different logics also lead to universalism (Niou and Ordeshook 1991).

Theories of universalism suggest that members of Congress will secure roughly equal benefits for their districts. To say "roughly equal," of course, is to appeal more to sense than science. But no advocate of universalism would reasonably predict that every district receives exactly the same share of federal spending. So without identifying a critical threshold in the spatial variance of federal spending, let us state the core proposition as fairly as the existing

theories permit: regardless of their party affiliations or ideological locations, giant logrolls enforced by the committee system and the reelection imperative should distribute comparable numbers of program dollars to every district.

*Majority Party Control.* Whereas the median voter theorem fixates on a member's ideological location in a policy continuum, other theoretical traditions emphasize a member's partisan affiliation. A substantial body of this work scrutinizes the ways in which the majority party dominates congressional proceedings (ADD CITES). Majority party leaders, it is postulated, favor their own members to help them win reelection—both directly and indirectly through the party image—in exchange for support of the party's legislative program. The prominent “cartel” model (Cox and McCubbins 2007) posits that the majority retains primary control over the benefits of office, and therefore allocates more institutional resources to its own members. So doing, the majority control acts as a procedural coalition to control the agenda (Cox and McCubbins 2005).

According to these party-based models, members of the majority party should profit handsomely from their privileged positions within Congress and their opportunities to manipulate the legislative agenda. In particular, these models predict that districts reliably secure significantly higher levels of federal outlays when their representatives move from the minority to the majority party.

*Other Possibilities.* These three models of distributive politics obviously do not exhaust the possible explanations for how legislators divide government benefits among themselves. Congressional leaders may allocate benefits disproportionately to legislators who are electorally vulnerable (Alvarez and Saving 1997; Stein and Bickers 1995),<sup>1</sup> who occupy key committee or other leadership positions (Carsey and Rundquist 1999; Evans 2004; Heischusen 2001), or whose districts appear to be in greater need of government support (Arnold 1979; Stein and Bickers 1995). However, these otherwise quite plausible explanations tend to focus on non-ideological and non-partisan factors that are outside the standard models of distributive politics. Nonetheless, in our empirical analyses we account for these competing theories, in some instances by modeling their predictions explicitly, and in others by including either representative or district fixed-effects.

## Existing Empirical Tests

The canonical models of distributive politics rest on solid theoretical foundations. They also generate clear predictions about the geographic distribution of federal benefits. To date, however, the theoretical insights generated by these literatures vastly outnumber quality empirical tests.

The Median Voter Theorem has been appropriated by Krehbiel (1991), among others, to explain the organization and general policy making stance of Congress. Tests of this “informational model,” however, focus almost exclusively on such things as committee assignments and legislative procedures rather than the distribution of benefits to districts. In fact, we know of no efforts to determine whether federal spending by district is affected by the proximity of representatives to the median legislator. In his own summary of a quarter century of research on distributive politics, Krehbiel does not mention a single study that examines the

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<sup>1</sup> Some authors argue the other way around, that spending in a district improves incumbent vote shares, without considering whether spending is also targeted based on electoral security (Bickers and Stein 1996; Levitt and Snyder 1997).

median voter theorem's capacity to explain legislative outputs (2004). Scholars, for the most part, have simply assumed that it does, without positing even the most basic of empirical analyses to support the contention.

The empirical literatures on majority party control, by contrast, do provide explicit accounts of policy outcomes. Most of these studies, however, are hampered by a variety of data limitations. Levitt and Snyder (1995), for example, focus on how parties influence the distribution of federal spending. Examining a six-year period in the late 1980s, they find that more spending from high variation programs goes to districts where the Democratic share of the presidential vote is higher and where the incumbent legislator is a Democrat. Unfortunately, because the Democrats controlled the House during this period, it is difficult to be certain that a change in party control would actually alter spending patterns.

Bickers and Stein (2000) use the Republican takeover of Congress in 1994 as a natural experiment to determine how party control affects distributive spending. Although Republicans favored contingent liability programs more than did their Democratic predecessors, there were no real effects of partisan or other political variables on spending. Balla et al. (2002) offer a "blame avoidance" model of distributive politics to explain earmarks for higher education. They show the majority party members are in fact more likely to get such earmarks and that they are for larger amounts. We do not know whether this is true beyond the eight year period of Democratic control they examine or in other policy domains. Martin (2003) similarly finds that areas voting more Republican receive less federal money, but because Democrats controlled the House during the period of his study, one again cannot determine if it is party differences per se or the effect of majority party status that causes this difference.

Carsey and Rundquist (1999) estimate a simultaneous equation model of defense spending. They find that partisanship itself has no effect on state defense outlays, although some combinations of variables do. This null finding raises the question of whether such factors are also irrelevant in other policy domains. Additionally, because the analysis was conducted at the state level, it is difficult to know whether a study of House districts would yield the same conclusions. Lowry and Potoski (2004) also find scant evidence across seven different policy domains that the majority party gives more to districts represented by its members, but these conclusions are limited to a seven-year period in the Senate. Evans (1994) sophisticated analysis of pork barrel politics is also limited to a few pieces of legislation, but she also fails to see much effect of majority party control of district project awards.

The few existing tests of the universalism hypothesis have tended to focus on the distribution of programs rather than the monies attached to them. Bickers and Stein (1995), for instance, reveals that most programs provide awards for only a small number of districts. Moreover, the number of districts covered by a program does not grow over time.<sup>2</sup> In an attempt to uncover logrolling across programs, Bickers and Stein also analyze groups of programs that fall under the same agency's control. Here again, though, they find little evidence of universalism, as agency portfolios rarely cover a majority of districts.

It is difficult to draw strong conclusions from Bickers and Stein's findings. Their analyses overlook the facts that many programs are complementary and spending is more divisible than award counts. And as Weingast (1994) points out, universalism is surely not the only principle affecting distributive politics. A fuller accounting of universalism, hence, would:

- 1) shift from discrete counts of awards to spending, since dollars are more fungible across

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<sup>2</sup> Similarly, Evans (2004) examines the number of districts that benefit from specific legislative initiatives.

program areas and thus more likely to be the result of distributive logrolls; and 2) integrate ideological and/or partisan measures into the analysis.

Despite their centrality to the study of distributive politics, the three canonical theories that guide our inquiry have been subject to relatively light—and quite uneven—empirical scrutiny. Much of this work focuses on the internal workings of Congress. Studies of legislative outputs, meanwhile, tend to contain short time series, focus on a handful of policy domains, and/or fail to account for the distribution of actual spending to districts around the nation. To date, no one has tested theories of the median voter, universalism, or majority party control using a comprehensive, multi-decade dataset of actual federal outlays.

## Data

Our data come from the Federal Assistance Award Data System (FAADS), a government-wide compendium of federal programs. The FAADS archive represents the transfer of almost anything of value from the federal government to a domestic beneficiary, so it includes essentially all federal activities outside of defense. Whereas earlier work examined limited domains (such as defense procurement (Carsey and Rundquist 1999), higher education (Balla et al 2002), transportation (Evans 2004), or agriculture and education (Heitshusen 2001)) or relied on a quite limited time period, our data incorporate most of the major functions of the federal government over a long sweep of time.

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. 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. We replicated the design of the Bickers and Stein federal spending database for 1998 to 2004 using new 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.<sup>3</sup> With 21 years of data for 435 districts, our total sample includes 9,135 observations. To avoid spurious results, we exclude state capital districts, leaving 7,349 observations.<sup>4</sup> We match outlays in year  $t$  to the legislator who represented the district in year  $t - 1$ .

In addition to total non-defense outlays, we identify three distinct types of federal spending. First we distinguish broad-based entitlement programs from federal programs that represent discretionary spending. To do so, we follow Levitt and Snyder (1995, 1997) and calculate coefficients of variation (the standard deviation divided by the mean) for each program contained in the FAADS data. We then use the coefficients to separate programs into two categories: *low variation* programs have coefficients of variation less than  $3/4$ , and *high*

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<sup>3</sup> The detailed Bickers and Stein codebook can be accessed online at <http://www.polsci.indiana.edu/faad/codebook.txt>.

<sup>4</sup> FAADS reports award transactions and recipient congressional districts according to the initial award recipient. This poses a problem for awards made to states for redistribution throughout the state: FAADS grossly inflates federal outlays to the congressional districts that contain state capitol buildings. We therefore omit districts that include some or all of a state capitol county, following Levitt and Snyder (1997). We note that our results do not change notably if we include state capital districts plus a control variable equal to the fraction of the county of the state capitol contained in each congressional district weighted by the state population, as in Levitt and Snyder (1995).

*variation* programs have coefficients of variation greater than or equal to  $3/4$ .<sup>5</sup> The low variation category includes 26 large programs, most of which are entitlement 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, by contrast, comprises hundreds of smaller programs over which Congress has much more discretion.

We next define a third category of spending that reflects specifically the decisions made by the sitting Congress. Because any given Congress inherits a broad array of programs with existing spending commitments, the distribution of outlays in a given year may not fully or exclusively reflect the priorities of the current members. And given that each of the canonical theories generates predictions about the capacities of specific members of Congress to influence legislative outcomes during their own terms of office, we need to distinguish inherited from current programmatic initiatives. We therefore identify those programs that were created by each Congress and then match the monies directed through these specific programs to districts. We thus define the variable *new program spending* as dollars received by a district in year  $t$  from programs created in year  $t$ . We expect that the distribution of new program spending should be most susceptible to influence by sitting members of Congress.<sup>6</sup>

Table 1 provides a summary of annual spending for each of our four types of outlays: total, low-variation, high-variation, and new program spending. The average district received nearly \$3 billion in federal outlays in 2004, the most recent year in our analysis. Unsurprisingly, the vast majority of that money, 88 percent, came from massive low-variation programs, including the behemoths Social Security and Medicare. The average district received around \$349 million in 2004 from high-variation programs, which tend to provide particularistic rather than universal benefits across districts. Finally, the average district received just under \$1 million in 2004 from programs that were created in the same year, which suggests that Congress enacted programs valued at around \$348 million ( $\$800,000 \times 435$ ) in new spending in that year. For the most part, the breakdown of spending across the four types of outlays is roughly equal throughout our study period. The most variable type is new program spending, which is occasionally profligate, as in 1997 and 2003, but sometimes miserly, as in 1990 and 2004. In the next section, we ask whether the canonical theories of distributive politics can explain the allocation of spending across districts.

## Analysis

We begin by graphically displaying the distribution of federal spending across districts and over time. Figures 1 and 2 show the amounts of spending directed to districts represented by

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<sup>5</sup> The results of our analyses are not sensitive to changes in the coefficient of variation cutoff. We experimented with four coefficient of variation thresholds greater than  $3/4$ , none of which produced notably different results. Details are available on request.

<sup>6</sup> Bickers and Stein argue that dollars spent is misleading since changing economic conditions automatically trigger changes in spending on entitlements such as unemployment benefits. This criticism can be easily circumvented by removing such programs from the analysis, as Weingast (1994) would suggest and Levitt and Snyder (1997) do in their study. By examining only new and high variation programs over which legislators have the most discretion, there is little chance that spending could be driven by factors beyond elected officials' control. In addition, year fixed effects, which we include in all of our statistical models, will account for secular changes in spending due to changing economic conditions or other national factors. We should also note that we focus coldly on federal spending, without casting judgment on whether such spending is pork or of any other type.

individuals with different ideological orientations, as measured by their DW-NOMINATE scores. Figure 1 pools all district-year observations, and Figure 2 displays Congress-specific outlays to alleviate concerns that change over time is masked in the overall figure. None of the three canonical models finds any support in the raw data displayed in these figures. Contrary to theories of universalism, considerable variation in spending levels is observed across districts. Contrary to theories about the privileged status of majority parties, spending appears evenly balanced across the left and right of the spectrum, even when disaggregating the data by Congress. Contrary to the median voter theory, the observations do not peak around the center of the ideological spectrum, and if anything appear lower near the median. Though these figures display total outlays, we note that we have created similar graphs (not shown) for each of the other three categories of spending and find comparable results.<sup>7</sup>

While the initial look at the data provided by Figures 1 and 2 does not bode well for any of the canonical models, the question demands a more rigorous statistical analysis. We next conduct statistical analyses that are designed to assess the specific core hypotheses of each model.

### Universalism

As previously discussed, it is difficult to formulate a precise statistical test of Universalism, as the theory itself is imprecise. If Universalism is taken to imply *literally* equal spending among legislators, as is implicit in the original Weingast (1979) and Shepsle and Weingast (1981) models, then it is clearly contradicted by the data. However, if Universalism is taken to imply *roughly* equal spending across districts, then the question becomes: how close to equality would spending have to be in order to conclude that it is universalistic? We shall not attempt to specify an arbitrary threshold *ex ante*, but rather we look to the data to reveal how evenly spending is in fact distributed across districts.

To gauge the extent of geographic variation in spending during the time period under consideration, we disaggregate the data by year and calculate some basic descriptive statistics. The results are presented in Table 2. For each year we report the across-district coefficient of variation for each type of spending. For total spending, the coefficient of variation hovers around 0.24 in each year. The results are similar for low-variation spending, which is unsurprising given that low-variation programs account for nearly 90 percent of total outlays. Variation is much greater among high-variation programs—this is by construction and therefore not particularly interesting. More illuminating, the coefficient of variation for new program spending is much higher than for any other category, with the standard deviation being on average about three times larger than the mean. Moreover, the inequality in new program spending varies wildly across year, with the lowest coefficient of variation equal to 0.20 in 1995, and the highest equal to 5.89 in 1984.

In additional analyses not reported, we explored different measures of program spending dispersion, all of which comport with those reported in Table 2. For example, the ratio of spending for the 75<sup>th</sup> percentile district relative to the 25<sup>th</sup> percentile district was 1.35 for total spending, 1.39 for low-variation programs, 2 for high-variation programs, and 3.5 for new programs. Moreover, we note that in 15 of the 21 years in our study period there are some districts that get *zero* spending from new programs, which would appear to contradict the “something for everyone” notion of universalism.

The preceding descriptive statistics, of course, do not account for any variation in spending that is due to district demographics. Perhaps some districts get more spending due to

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<sup>7</sup> To conserve space, we do not report all four sets of figures. Additional figures are available on request.

the attributes of their constituents—for instance, the unemployed, the elderly, military veterans, and so forth—while the remainder is determined by a universalistic budgeting process. To explore this possibility, we examine the variance in the residuals from a regression of spending on a full set of legislator and district characteristics and year indicator variables.<sup>8</sup> Summaries of total variation and residual variation for each program type are reported in Table 4. For all spending categories, variance in the residuals is notably lower than variance in raw spending, suggesting the district attributes and year fixed effects do account for some of the dispersion (more on this below). However, as is immediately apparent, considerable variation persists, especially in new program spending. The inequality in spending across districts simply cannot be attributed to the socio-demographic profile of the constituents represented.

All in all, we find mixed support for universalism. For total spending and for large entitlement programs, spending across districts is not wildly unequal and could be considered consistent with a *rough* norm of universalism. However, such sizable disparities in new program spending—as well as regular instances in which some districts are completely left out—appear inconsistent with universalism. In any given year, there are big winners and losers in the distributive process; but over the long haul, these disparities roughly even out. We interpret these results as problematic for the universalism theory, because the theory specifically pertains to the bargains struck in a given legislative session, and so the theory should explain new program spending if it explains anything at all. At the very least, the theory is substantially incomplete and would need to be extended to explain the existence of universalism in the long-run allocation of benefits but radial inequalities in year-to-year distributive bargains.

### **Median Voter and Majority Party Influence**

We test the median voter and partisan control models simultaneously by estimating a series of spending regressions. In each, we regress one of our measures of spending against two variables capturing the core hypotheses from each model, plus a set of control variables. To test the median voter model, we create a variable representing the distance between the district's representative and the House median legislator, measured in NOMINATE space; that is, we subtract the representative's score from the median legislator's score and take the absolute value of the difference. To test the partisan control model, we simply use a dummy variable equal to one for members of the majority party.

We use a common set of legislator attributes as control variables in all of the models. We control for the member's party via a dummy variable set equal to one for Republicans. We control for the number of terms the member has served in the House, to capture the possibility that more senior members receive a greater share of the spoils. We control for the desirability of the member's committee assignments by using the average "Grosewart" score (Stewart and Groseclose 1999) of the committee(s) on which the member serves. In addition, we control for the victory margin in the representative's preceding election, to allow for the possibility that electorally vulnerable members receive support from their colleagues in the form of increased allotments of discretionary outlays.

We run two separate sets of spending models. In the first, we control for a rich set of district characteristics that are plausibly related to federal outlays. Specifically, we control for

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<sup>8</sup> This is equivalent to the method used in labor economics to estimate "residual wage inequality" (e.g., Katz and Autor 1999). The independent variables in the model are the same as those reported in Table 4 and will be explained below.

basic demographics that may be associated with targeted federal benefits: the percent of the district population that is over 65, percent black, and percent enrolled in school. We also control for the percent foreign-born, to allow for the possibility that non-citizens, and therefore non-voters, are less influential. We control for the percent employed in farming, which we expect to be correlated with receipt of agricultural subsidies. We control for the percent unemployed and for the district's median income, to test the progressivity of federal spending. We include the percent of the population classified as urban because of the large number of programs targeting cities. We control for the percent employed in the military and the percent veterans. Finally, we control for the total population of the district to account for the number of potential beneficiaries of federal outlays.

We emphasize at the outset that we do not give a causal interpretation to the coefficients related to district attributes. For example, we would not infer that having more farmers *causes* the federal government to deliver more spending to a district; the reason why benefits are disproportionately directed toward farmers remains an open question. Rather, we take the models of district attributes to be *descriptive* of the ways in which money is allocated across districts. Our main question is whether controlling for these district attributes alters our conclusions with respect to median voter and majority party influence. To get more directly at causal relationships in the data, we run a second set of models that include redistricting-specific district fixed effects.<sup>9</sup> While causal inference is not unproblematic in this case, the fixed effect models have the benefit of accounting for any unobserved district characteristics that do not change within a redistricting period, and allow for the recovery of estimates based upon within-district changes in the variables of interest. In these models, we are able to ask whether a district receives more spending during the years in which it is represented by a member of the majority party than when it is represented by a member of the minority, or whether it receives more when its representative is closer to the position of the median legislator. Finally, in all of the models reported we cluster standard errors by district to account for heteroskedasticity and serial correlation.

Table 4 presents the results of models controlling for observable district attributes. Note that many variables yield significantly different coefficients for different categories of spending, demonstrating the usefulness of looking separately at different types of outlays as original suggested by Levitt and Snyder (1995). Across all the categories of spending examined, however, the canonical models do not fare well. The distance to the median voter is negative for low-variation programs, as would be predicted, and it is marginally significant statistically. However, the variable is positive and significant for the high-variation and new programs, and in the latter case the relationship is highly significant. For these later two types of programs, then, it would appear that legislators located farther from the median voter secure more spending, directly opposite of expectations. The majority party indicator is significant in two of the four models—for total and low-variation spending—and in both cases the coefficient is negative, implying that members of the majority party get roughly three percent *less* spending than members of the minority party. In summary, not only are the canonical theories not supported in these models, they are often directly contradicted.

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<sup>9</sup> Due to decennial redistricting we can only consider each congressional district to be a unique entity within a single redistricting period. The FAADS data are reported according to constant district boundaries in three time intervals—1984 to 1992, 1993 to 2001, and 2002 to 2004—meaning that we observe a total of 1,305 unique district entities over this 21-year time period. The fixed effects models are based on these 1,305 redistricting-specific district entities.

Among the control variables included in Table 4, there are several results of interest. Republicans appear to secure less outlays from high-variation programs, and members with desirable committee assignments secure more from high-variation and new programs. A member's seniority, measured in congressional terms, is unrelated to spending, all else equal. Members elected by narrow margins tend to earn more in new program spending. We will return to these legislator-specific variables in the context of the fixed effects models reported below.

Most of the included district attributes are related to spending in predictable ways. Districts with larger populations receive more spending, although in models (1) through (3) the elasticity is notably less than one, suggesting that spending does not rise proportionally with population; that is, members from smaller districts extract proportionally more for their constituents. Interestingly, however, for new program spending the elasticity is essentially equal to one, possibly suggesting a norm of proportionality at least at the time of program creation.

Districts with more elderly constituents receive significantly more spending from low-variation programs, which include Social Security and Medicaid, but they receive less from high-variation and new programs. Having won a major stake in entitlement programs, legislators appear less likely to divert additional resources to the elderly. Districts with farmers are in the reverse position, showing a significant edge in high-variation programs, but less in low-variation programs. This is unsurprising, as agricultural subsidies are by their nature high-variation programs. Districts with a large military presence receive less from low-variation programs, which likely reflects that they receive spending from defense-related programs, which are the only programs not included in our data. Districts with higher unemployment and lower median income receive more from every type of program, confirming a basic degree of progressivity in federal spending. Finally, urban districts receive more from high-variation and new programs, which may be another indicator of progressive spending, or simply a focus on urban issues.

As mentioned above, we interpret the results from Table 4 as *correlations*, which provide an interesting description of the way in which federal outlays are distributed across districts. In table 5, we present district fixed effects models, the results of which can more plausibly be interpreted as causal relationships. In these models, we ask how changes in spending within districts over time are related to changes in the attributes of their representatives. In these models, identification comes from when a district elects a new representative, or when the existing representative's position in the legislature changes. Because district demographic data do not vary within a redistricting period—the data are measured only at the Census—they are subsumed within the redistricting-specific fixed effects.<sup>10</sup> We therefore are only able to estimate coefficients for the time-varying legislator-specific attributes in these models, which are the primary variables of interest in any case.

The median voter model receives absolutely no support from the results shown in Table 5. The representative's distance to the median voter is positively associated with spending, opposite the model's prediction, but the relationship never even approaches statistical significance. Within a district over time, then, federal spending does not change significantly as the district representative's position changes with respect to the median voter in the chamber.

The partisan control hypothesis fares somewhat better. Membership in the majority party is positively associated with spending for every type of program except new programs, and the relationship is highly significant for low-variation and total spending. In other words, when the district elects a member of the majority party, or when the party of its current representative wins the majority, the district receives more spending than it did when its representative was a

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<sup>10</sup> Additional details are provided in the appendix.

member of the minority party. Before crowning the party control model, however, two caveats should be noted. First, the effect is small. Members of the majority party receive roughly two percent more in spending from low-variation programs, hardly the windfall that might be expected from a “Legislative Leviathan.” Not only is the amount small in absolute terms, it is small in comparison with other legislator characteristics that have been shown to affect federal outlays. For example, Anzia and Berry (2008) show that districts represented by women receive roughly 12 percent more in federal spending.

Second, membership in the majority party appears to matter least precisely where theory predicts it should matter most; namely, for new program spending. If the majority party is a procedural cartel that controls the legislative agenda for its own advantage, then the benefits of membership in the cartel should be most evident in the programs passed when it is in control. But the relationship between new program spending and majority status is negative and insignificant. Rather, majority party status matters for low-variation program spending, which reflects large entitlement programs that were on the books long before the current majority came to power. One explanation for the majority edge in low-variation programs could be that the bureaucracy is particularly responsive to the interests of majority party members in its implementation of spending programs. For example, agencies might be especially quick to respond to constituents from majority party districts or to requests from majority members of Congress. Note however, that an *agency-responsiveness* argument represents a completely different mechanism from the *procedural cartel* argument that underlies the canonical model of majority party influence. Thus, we conclude that if there is evidence for a small majority party advantage in distributive politics, current theories do not explain it. At the very least, existing theories would need to be extended to account for the majority party’s edge in spending from already existing programs but not from new programs.

Among the remaining variables, we see almost entirely null results in Table 5, most of them fairly precisely estimated. Of particular note, Republicans do not obtain any more or less from any type of program, suggesting that the relationship estimated in Table 4 reflected a spurious correlation between Republican districts and high-variation spending, likely due to unmeasured attributes of districts that are correlated with both electing Republicans and lower eligibility for federal program spending.

## Conclusion

Observed patterns in federal spending bear little resemblance to the predictions of our discipline’s most prominent theories. Contrary to theories of universalism, we observe dramatic variation in spending across districts, especially when focusing on recently enacted programs. Contrary to theories of party control, we find little evidence that spending is concentrated in the districts of members of the majority party. And contrary to the median voter theorem, moderate members of Congress actually see less spending than extremists. All of these results hold after controlling for a wide range of non-ideological and non-partisan factors that characterize districts and their congressional representatives.

To be sure, there are some signs of universalistic norms. When looking at all programs, variation in geographic outlays appears reasonably small. Theories of universalism, though, focus on the norms that dictate program creation, not accumulation. And when restricting the sample to newly enacted programs, geographic variation increases dramatically. Moreover, we

find that in many years a non-trivial number of district receive absolutely no benefits whatsoever from newly enacted programs.

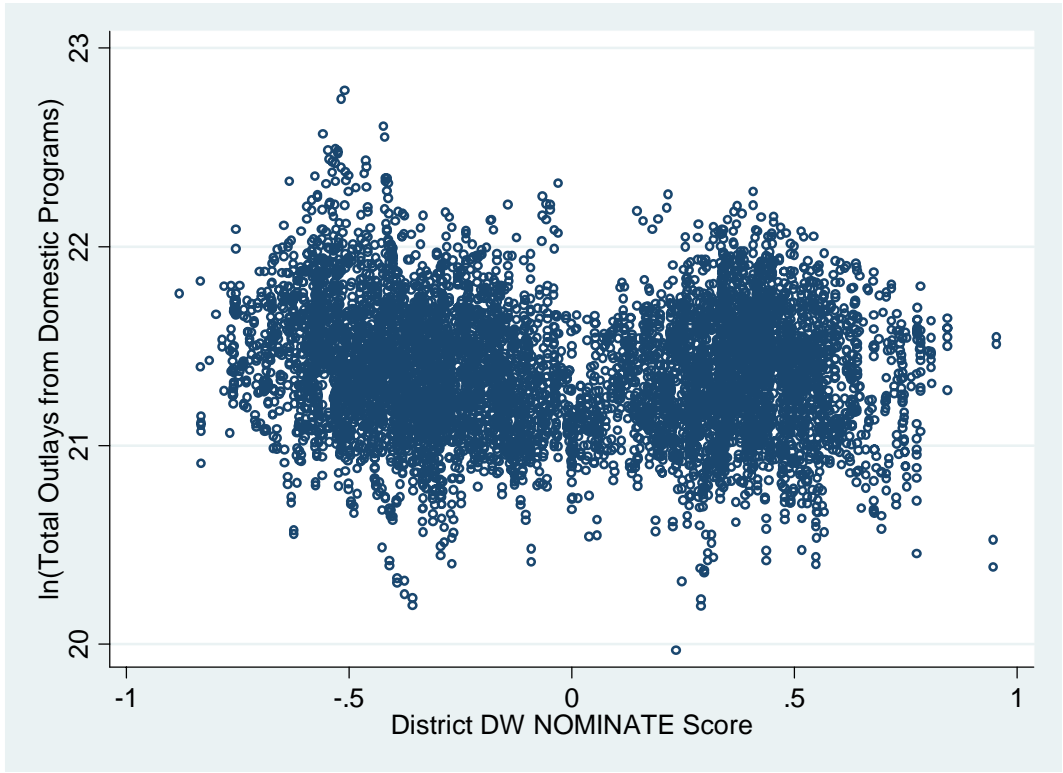
In a few models, we also find some evidence that members of the majority party receive higher outlays. In particular, models that include district fixed effects suggest that members of the majority party receive, on average, larger federal outlays than do members of the minority party. The estimated effect, though, is rather small and appears to be concentrated in low-variation programs, about which current members yield significantly less discretion. Models that focus on either high-variation or new programs, which are subject to the very kinds of legislative manipulation that are the focus of party-based models, consistently yield null effects.

To be sure, other theories of distributive politics generate alternative predictions about the geographic disbursements of federal spending. A variety of literatures, for instance, highlight the importance of committee chairs, the president, and veto-override and filibuster pivots. The three theories examined in this paper, however, are unparalleled for their stature and prominence within the discipline. It is striking, then, that when examining basic patterns in the most elemental of legislative outcomes—namely, the federal funds that flow to districts around the nation—these theories have so very little to say.

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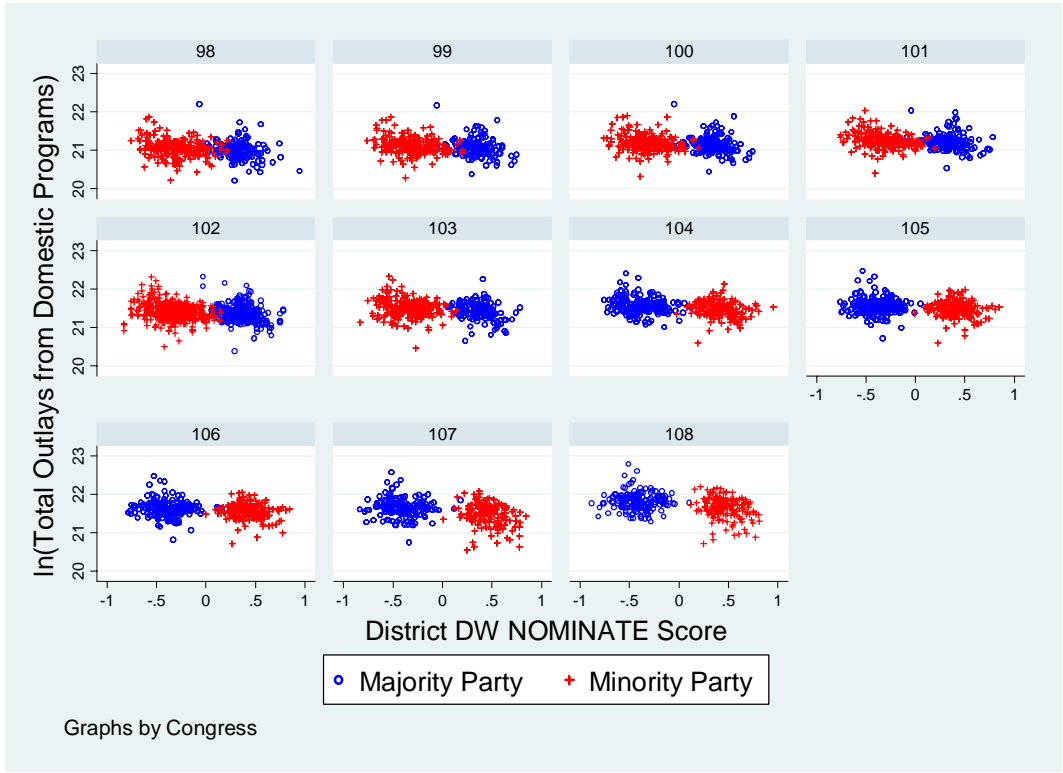
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**Figure 1: Federal Expenditures by NOMINATE Score**

Note: Each dot represents one district-year observation (1984-2004)



**Figure 2: Expenditures by NOMINATE Score by Congress**

Note: Each dot represents one district-by-Congress observation

**Table 1: Average Spending by District by Year (\$ Millions)**

Year	Total Spending	Low-Variation	High-Variation	New Programs
		Programs	Programs	
1984	1,370	1,190	178	0.8
1985	1,450	1,240	210	0.4
1986	1,500	1,280	214	1.9
1987	1,520	1,310	213	1.6
1988	1,590	1,340	251	0.6
1989	1,630	1,370	258	1.1
1990	1,680	1,450	236	0.3
1991	1,770	1,530	240	1.6
1992	1,910	1,670	240	2.1
1993	2,040	1,740	292	1.2
1994	2,130	1,850	291	4.8
1995	2,180	1,900	282	2.7
1996	2,210	1,950	273	1.4
1997	2,290	2,010	294	39.3
1998	2,260	1,970	296	1.0
1999	2,250	1,980	276	1.4
2000	2,390	2,050	335	14.2
2001	2,520	2,180	337	1.2
2002	1,960	1,670	288	3.5
2003	2,950	2,590	370	51.6
2004	2,830	2,490	349	0.8

Note: All dollar amounts are inflation-adjusted 2004 dollars.

**Table 2: Coefficient of Variation in District Spending**

<b>Year</b>	<b>Total Spending</b>	<b>Low- Variation Programs</b>	<b>High- Variation Programs</b>	<b>New Programs</b>
1984	0.26	0.23	0.95	5.89
1985	0.26	0.23	0.92	2.49
1986	0.25	0.22	0.95	1.60
1987	0.25	0.22	1.03	1.22
1988	0.24	0.21	0.89	1.28
1989	0.25	0.22	0.95	2.04
1990	0.22	0.22	0.73	3.31
1991	0.22	0.21	0.75	0.23
1992	0.25	0.22	0.98	1.35
1993	0.24	0.22	0.82	1.04
1994	0.25	0.22	1.01	1.25
1995	0.24	0.22	0.99	0.20
1996	0.23	0.21	0.88	1.17
1997	0.22	0.21	0.88	1.32
1998	0.23	0.21	0.91	1.78
1999	0.23	0.21	0.91	1.88
2000	0.22	0.21	0.75	0.88
2001	0.22	0.20	0.82	1.78
2002	0.28	0.25	1.01	2.73
2003	0.27	0.27	0.90	1.22
2004	0.27	0.25	1.02	3.49
<b>Total</b>	<b>0.33</b>	<b>0.32</b>	<b>0.94</b>	<b>3.18</b>

Note: The coefficient of variation is equal to the standard deviation of spending across districts divided by the mean.

**Table 3: Variance and Residual Spending Variance**

	<b>Total Spending</b>	<b>Low- Variation Programs</b>	<b>High- Variation Programs</b>	<b>New Programs</b>
Variance	0.098	0.098	0.658	17.079
Residual Variance	0.026	0.023	0.531	6.460

**Table 4: Models Controlling for Observable District & Legislator Characteristics**

	Total Spending	Low-Variation Programs	High-Variation Programs	New Programs
	(1)	(2)	(3)	(4)
Distance to median voter	-0.00889 (0.034)	-0.0520* (0.028)	0.208* (0.12)	0.971** (0.39)
Majority dummy	-0.0260* (0.014)	-0.0307*** (0.011)	0.00268 (0.049)	0.100 (0.15)
Republican dummy	-0.0109 (0.0098)	-0.00293 (0.0093)	-0.120*** (0.034)	0.0950 (0.12)
Terms in office	-0.000421 (0.0012)	0.0000517 (0.0011)	-0.00297 (0.0037)	0.00200 (0.013)
Committee desirability	-0.000217 (0.0026)	-0.00391* (0.0024)	0.0236*** (0.0079)	0.0786*** (0.028)
Electoral margin	0.0185 (0.013)	0.0211* (0.012)	-0.0441 (0.050)	-0.441** (0.18)
ln_population	0.181*** (0.047)	0.151*** (0.048)	0.302** (0.14)	0.988** (0.50)
age65_pc	2.420*** (0.23)	3.180*** (0.22)	-3.335*** (0.68)	-9.728*** (2.43)
black_pc	0.136*** (0.049)	0.0756 (0.048)	0.188 (0.17)	0.816 (0.59)
school_pc	-1.723*** (0.44)	-0.479 (0.31)	-7.987*** (1.42)	-22.23*** (3.66)
farmer_pc	0.780 (0.56)	-3.136*** (0.45)	22.97*** (2.39)	21.43*** (7.22)
foreign_pc	0.0502 (0.098)	0.0408 (0.098)	-0.297 (0.31)	1.755** (0.86)
ln_median_income	-0.152*** (0.039)	-0.141*** (0.037)	-0.267** (0.12)	-0.520 (0.38)
unemployed_pc	5.804*** (0.71)	4.525*** (0.64)	15.39*** (2.25)	37.17*** (8.94)
veterans_pc	-0.00261 (0.0058)	-0.00241 (0.0062)	0.00176 (0.020)	-0.0426 (0.051)
miltpop_pc	-1.231*** (0.38)	-1.075*** (0.41)	-0.559 (0.84)	6.171 (4.02)
urban_pc	-0.0182 (0.034)	-0.0504 (0.031)	0.253** (0.12)	0.803** (0.38)
Constant	19.85*** (0.44)	19.75*** (0.49)	18.56*** (1.24)	1.903 (5.04)
Observations	7322	7322	7322	7322
R-squared	0.72	0.76	0.17	0.47

Robust standard errors clustered by district in parentheses. All dependent variables are inflation-adjusted and expressed in natural logs. All models include year fixed effects (not shown). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 5: District Fixed Effects Models**

	<b>Total Spending</b>	<b>Low-Variation Programs</b>	<b>High- Variation Programs</b>	<b>New Programs</b>
	(1)	(2)	(3)	(4)
Distance to median voter	0.0189 (0.026)	0.0137 (0.023)	0.0850 (0.12)	0.0240 (0.53)
Majority dummy	0.0222** (0.0088)	0.0206*** (0.0079)	0.0325 (0.046)	-0.132 (0.17)
Republican dummy	-0.000693 (0.011)	0.0110 (0.0097)	-0.0347 (0.040)	0.308 (0.19)
Terms in office	-0.00111* (0.00067)	-0.000226 (0.00042)	-0.00261 (0.0028)	-0.0116 (0.016)
Committee desirability	0.000970 (0.0016)	-0.00000379 (0.0011)	0.00286 (0.0052)	-0.0000317 (0.037)
Electoral margin	0.0000677 (0.0059)	0.00361 (0.0044)	-0.00669 (0.047)	-0.0513 (0.20)
Observations	9062	9062	9062	9062
Number of clusters	1298	1298	1298	1298
R-squared	0.70	0.82	0.05	0.41

Robust standard errors clustered by district in parentheses. All dependent variables are inflation-adjusted and expressed in natural logs. All models include year fixed effects (not shown). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1