Distributive Politics and Legislator Ideology

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This article examines the relationship between legislative centrism (or conversely, extremism) and the distribution of federal outlays. A substantial body of theoretical work suggests that legislators closer to the chamber median are more attractive and willing candidates to engage in vote buying and hence should receive a disproportionate share of distributive benefits. We investigate this prediction empirically with panel data covering 27 years of federal outlays, using a research design that exploits elections in other districts to identify changes in the relative ideological position of individual legislators. We find a 7% decrease in outlays associated with a one standard-deviation increase in a member's ideological distance from the median voter. We find the effect of exogenous increases in legislative extremism on outlays to be robust across a wide variety of specifications, and we take special care to distinguish this effect from those induced by potentially confounding covariates, most notably majority party status.

As scholars going back at least to Ferejohn (1974) have recognized, distributive politics amounts to more than just a contest among legislators for scarce federal resources. It also involves the deliberate use of these monies to, as Evans (2004) puts it, "grease the wheels" of the legislative process. Whether to buttress preexisting support for a bill against the lobbying of an opposing party or faction, or to purchase the support of a member who, absent the side payment, would vote against a particular bill, policy entrepreneurs routinely make use of outlays to cultivate support for their legislative initiatives (Cann and Sidman 2011; Evans 1994, 2004; Wiseman 2004).

Who within Congress is most likely to benefit from vote buying, an activity that, as Richard Neustadt once quipped, is "as traditional as apple pie"?[1] Who, that is, stands to reap a greater share of federal outlays from successive efforts to build legislative coalitions through side payments? From Snyder (1991) to Dekel, Jackson, and Wolinsky (2008, 2009), a substantial body of theoretical work sheds light on the matter, suggesting that it is ideological moderates who represent the most likely candidates to be engaged by vote buyers. Because they are more likely to be ideologically indifferent (or close to indifferent) between policy alternatives, moderate members should be more frequent targets of congressional vote-buying activities. It is this theoretical prediction that we test in this article.

In order to study the effect of legislative centrism on the geographic distribution of federal outlays, we use a member-by-county fixed-effects research design to analyze distributive outlays over a 27-year period. This research design uses only movements in the ideological position of an individual legislator that are generated by elections of new members from other districts. That is, we hold fixed the ideology of each given legislator and ask how her proximity to the median voter changes after elections that alter the composition of the chamber. We find a statistically and economically significant positive effect of increased legislative centrism on county-level outlays. Specifically, a one standard-deviation increase in a representative's ideological proximity to the House median leads to a 7% increase in outlays received by her constituents.

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1. As cited in Evans (2004).

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We show that these findings are robust to a wide variety of alternative specifications and do not appear to be an artifact of majority party status.

The article proceeds as follows. We first review the relevant theoretical literature on vote buying as well as existing empirical studies of distributive politics. After discussing our identification strategy, model specification, and data, we then present our main results. We subsequently scrutinize the role of majority party status in distributive politics and subject our core analyses to a variety of robustness checks and placebo tests. We conclude by placing our findings in the context of related, ongoing questions in the study of US legislative and electoral politics.

THEORETICAL MOTIVATION

Vote-buying models typically posit a legislative environment in which one or more lobbyists compete over two possible legislative outcomes.[2] Such lobbyists might be conceived of as either traditional interest groups and thus unable to cast votes themselves, nonvoting elected officials such as the president, or as actual voting members or blocs in a collective decision-making body. In any case, these "lobbyists" offer side payments to legislators in exchange for their votes, with payments usually being conditional on support. What constitutes the payments in these models is generally left unspecified, though Baron (2006, 607) writes that "lobbying consists of providing politically valuable resources to legislators," a criterion that budgetary outlays certainly satisfy. The payments compensate legislators for voting against their or their constituents' beliefs, thereby justifying the ideological or electoral compromise.

Snyder (1991) initiates the modern literature on vote buying with a model of a single, price-discriminating lobbyist. The model predicts that a lobbyist will make payments to those initially opposed to her favored position until majority support is procured (see his proposition 1). As Snyder puts it, "the lobbyist pays the highest bribes to legislators whose ideal points are closest to the median of the legislature, but on the side of the median closer to the lobbyist's proposal. That is, a lobbyist does not bribe his close supporters . . . but rather his marginal opponents" (98). Snyder goes on to comment that most empirical work on money in politics neglects this result. Having ignored the ability of vote buyers to price discriminate among legislators, Snyder speculates that previous researchers had mischaracterized the distributive consequences of vote buying.

While much congressional vote buying may occur in a legislative environment populated by just one lobbyist, as Snyder (1991) recognizes and Wiseman (2004) reiterates, multiple lobbyists may compete over opposing legislative outcomes. Recognizing this possibility, a subsequent stream of political economists develop extensive-form vote-buying models with competing lobbyists, the first being Groseclose and Snyder (1996). This two-stage bargaining model, further explored in settings with finite numbers of legislators by Banks (2000) and Groseclose and Snyder (2000), characterizes equilibria in which supermajorities are assembled in order to block threats from an opposing interest. While the Groseclose and Snyder model has had a profound impact on political scientists' thinking about vote-buying activities, its power lies in demonstrating a strong second-mover advantage in a bargaining setting with an exogenously finite time horizon. Its utility in assessing the distributive consequences of vote buying, however, is more limited. Depending on which equilibrium case is under consideration,[3] Groseclose and Snyder's model generates different predictions about the distribution of payments. Without a clear equilibrium selection mechanism, it is virtually impossible to distill clear, testable predictions.

More recently, Dekel, Jackson, and Wolinsky take up the tradition of competitive vote-buying models with a pair of companion papers that make important theoretical advances and generate clearer predictions. Dekel et al. (2008) examine vote buying in general elections, while Dekel et al. (2009), most relevant for our purposes, model vote buying within a legislature. In Dekel et al. (2009), the use of a per-round bidding cost allows the games to be endogenously finite, capturing the dynamic nature of legislative negotiation without undue impositions on the number of bidding rounds. Additionally, careful use of a smallest unit of payment and a reasonable assumption about the irreversibility of bidding (a rule against undercutting one's previous offer to a given legislator) enable the authors to avoid the issues with ties and shifting strategies encountered in Blotto-like allocation games (see Roberson 2006).

The central prediction of the Dekel et al. (2009) model on legislative vote buying is essentially identical to that of Snyder (1991). When payments are made in equilibrium, they are directed to what the authors refer to as "near-median legislators," and payments are again decreasing in amount with distance from the median (see their proposition 3). Such

2. This stands in contrast to alternative formulations of legislative bargaining games, such as Baron and Ferejohn (1989) and Baron (1991), which do not include a role for parties or other organizations within or outside of Congress, instead focusing on individual legislators' proposal power.

3. Where equilibrium cases in the presence of competing vote buyers are determined by the relative and absolute valuations of the two lobbyists.

legislators after all are the "cheapest" that could be bought to secure majority support, and minimum payments are made to secure the requisite support needed for a bill's passage. To further illustrate the logic underlying this result, we present an adaptation of this model in section A of the appendix, available online.

Due to its theoretical robustness across single- and multiple-vote buyer settings, the prediction that legislators receive more "payments" from interested parties the nearer to the median they are ideologically serves as our main hypothesis. Yet these theories leave open a number of practical questions. Who does the vote buying? How is it done? At what levels of aggregation should we expect to observe such an effect? These questions, it turns out, are both closely related and highly pertinent when moving from theory to empirics.

MOVING FROM THEORETICAL TO EMPIRICAL PREDICTIONS

In seeking to better understand the distribution of federal outlays using vote-buying theory, we must clarify and defend the real-world interpretation we apply to these models. As federal outlays constitute the "payments," we take the "lobbyists" to be the party organizations, broadly defined, as it is these groups that have the ability and incentive to manipulate the distribution of federal funds. This could include the president, outside interest groups, or individual legislators—any actor who might work through or with party leadership to deploy distributive outlays in order to garner support for a legislative endeavor.

The claim that party organizations within Congress, and those working on their behalf, can control the more manipulable distributive funds appears well founded observationally.[4] It certainly is borne out by the following story from a House Appropriations Committee staffer as taken from Shepsle et al. (2009): "One House [of Representatives] Appropriations [Committee] staff member, for example, described a budget account that was explicitly divided into four with each partisan delegation in each chamber having authority over its share. Other interviews suggested this was the implicit norm for many of the most heavily earmarked accounts, although it was typically not explicitly codified."

The models that generate our main hypothesis (Dekel et al. 2009; Snyder 1991) do not elucidate which party organizations might be more or less prominent vote buyers. While they agree on the prediction that payments to legislators will rise up until the median legislator, for any given bill-specific iteration of the game these payments will be made on one side of the median. Which side of the median will this be? Because they do not put any constraints on the identity of the bill proposer, the models are largely agnostic about the matter. The models show that payments will only be made by the side that lacks the ex ante support of a majority of members and to members that fall on that side of the median. If this more often characterizes the minority party, then payments may load on its side of the median member. However, the majority party likely wields greater control over the agenda, and given that some of its bills may lack the ex ante support of a majority of members, payments could load on that side of the median as well.

The "procedural cartel model" first forwarded in Cox and McCubbins (1993) and developed further in Cox and McCubbins (2005) takes a strong stand on this latter possibility. The authors argue for a theory of negative agenda control in which the majority party colludes to prevent unwanted bills from reaching the floor. Those bills that do see the light of day tend to move policy toward majority-party centrists. While negative agenda control should mostly ensure passage of such bills, Cox and McCubbins (2005, chap. 10, 45–47, 159) stipulate that distributive benefits may be needed on the margins to compensate majority members suffering a "policy loss."[5] The implication is that, in effect, the majority party must from time to time partake in vote buying and that majority party members would be the exclusive beneficiaries.

A couple of previous empirical analyses, discussed below, have already provided support for this implication of the procedural cartel model of party government. In our primary analysis, we look for payments to decrease (i.e., benefit moderates) on both sides of the median. In extensions, we investigate whether vote-buying benefits moderate members of both the majority and minority parties, whether it is applied to one group differentially, and whether vote buying with outlays benefits moderate members of the majority party in the same way on either side of the median.

4. Furthermore, this view is entirely in line with the vote-buying models we take as theoretical motivation. Rather than imposing a balanced-budget requirement or considering lobbyist-specific budgets that are likely to bind on each iteration of the game, the valuation-based version of the Dekel et al. (2009) model conceives of the groups seeking to influence legislators as having significant discretionary funds from which they may allocate as much as they wish to a given legislative effort, distributing such funds as they see fit. Dekel et al. also investigate a version of the model that assigns constraints on each vote buyer's coffers, but the distribution of payments in the equilibrium of a single round of the budget-constrained game is identical to that of the unconstrained, valuation-based version, under the conditions relevant to the budget-constrained version in which payments are made.

5. Centrists, if the model assumes an open rule, but potentially members on either extreme of the majority party if the model assumes a closed rule.

Finally, we must confront the level of aggregation at which our analysis takes place. As a practical matter, we are unable to observe the side-payments associated with any single bill. Rather, our outcome of interest is the aggregate outlays distributed to a specific geographic unit in a given year. While the predictions of the single- or competing-vote buyer models reflect payments made in a single iteration of the game, it is straightforward to see why we might expect the sum of payments to monotonically decrease as one moves further away from the median even when aggregating across bills, as we do in the empirical analysis. Because payments, when made, always decrease in magnitude with distance from the median, the prediction of a single iteration of vote buying scales up to considering multiple independent iterations.[6]

From an empirical standpoint, the validity of aggregating payments requires more thought. For instance, vote-buying models predict minimal winning coalitions, yet we know that "divisions on legislative roll calls are seldom near 50-50" (Groseclose and Snyder 1996, 303). As previously discussed, the models also predict that payments on any single bill will occur on one side of the median. Unfortunately, though, there is not a straightforward way to account for either of these facts. Data do not currently exist that would allow us to tie specific federal outlays to voting behavior across a significant number of bills.[7] While such data might provide a more direct test of some of the claims of vote-buying theories, we see the individual-bill and aggregating approaches as complementary. An aggregate analysis enables us to ascertain whether, as theory suggests, legislators closer to the median receive a disproportionate share of federal outlays over the course of a budgetary cycle in Congress, and it allows us to do so without imposing additional and potentially ad hoc assumptions on a bill-by-bill basis.

6. This feature of aggregation stands in contrast to some of the other vote-buying models, notably Groseclose and Snyder (1996). When considering multiple independent iterations of that game, the aggregate distribution of payments relies heavily on the assumed distribution of proposals and its implications for the size of ex ante legislator valuations and thus ex ante majority support. One could derive the result that payments decrease monotonically with distance from the median given certain assumptions and yet derive differing results given other assumptions. Such sensitivity to varying assumptions provides yet another reason to focus on the Dekel et al. model, which requires no further assumptions besides independence of iterations to derive predictions testable with aggregate data. Whether this prediction is as accurate as it is straightforward remains an empirical matter.

7. Data being collected by C. Lawrence Evans on whip counts holds great promise for providing concrete evidence of vote buying or other legislative maneuvering on specific bills or votes. Along the same lines as the evidence presented by Diana Evans regarding earmarks (discussed in the next section), scholars might use whip count data to identify vote-specific instances of coalition formation and legislative deal-making.

PREVIOUS EMPIRICAL WORK ON DISTRIBUTIVE POLITICS AND LEGISLATOR IDEOLOGY

Over the last couple of decades empirical studies on distributive politics have proliferated, but only a small portion of this work specifically examines vote buying.[8] Joining distributive politics and vote-buying theories, Evans (1994, 2004) offers the most sustained empirical examination of the use of distributive side payments to achieve legislative objectives.[9] As support for her argument that federal monies (earmarks in her case, rather than the categorical grants that are our dependent variable) are used to purchase votes, Evans presents interview data as well as in-depth case studies on legislation for the Federal Highway Program and the North American Free Trade Agreement. In these empirical investigations, Evans shows that legislators' promises of votes in exchange for particularized benefits are in fact binding, as demonstrated by bill- and even vote-specific changes in voting behavior.

To our knowledge, only Carroll and Kim (2010), Herron and Theodos (2004), and Jenkins and Monroe (2012) investigate the implications of vote buying (and in the latter two cases, the particular predictions of Cox's and McCubbins's [2005] theory) with explicit regard to ideological moderation and extremism. Herron and Theodos (2004) find evidence from a discretionary grant program in Illinois that extremists received less money than predicted given district need and other political variables. By not looking to test vote-buying theory, however, the finding is characterized as potentially contradictory to theories in which ma-

8. Instead, previous studies have scrutinized the importance of committee membership (Alvarez and Saving 1997a), majority party status (Balla et al. 2002; Cox and McCubbins 1993; Levitt and Snyder 1995), electoral competition (Alvarez and Saving 1997b; Stein and Bickers 1994), state size (Knight 2008; Lee 2000), majoritarian rules and universalism (Bickers and Stein 1997; Groseclose 1996; Shepsle and Weingast 1981), party alignment with various members of the executive branch or the president (Berry et al. 2010; Bertelli and Grose 2009; Gordon 2011; McCarty 2000), partisan contributions (Cann and Sidman 2011), and the roles of local governments (Bickers and Stein 2004; Rich 1989).

9. A burgeoning body of empirical work also investigates features of vote-buying models that do not (directly) concern matters of distributive politics. Wiseman (2004), for instance, classifies bills as likely to have been attractive to multiple vote buyers rather than a single interested group and then looks for voting patterns that conform to the predictions of single and competitive vote-buying models. Herron and Wiseman (2008) consider the implications for redistricting consistent with conditional party government, cartel, and vote-buying theories and uncover evidence that appears at least consistent with vote-buying theories. Taylor (2014) builds on Wiseman (2004) to examine evidence of vote buying in the amount of time it takes bills to pass. In order to bridge theory and data, it bears recognizing that all of these studies adopt auxiliary assumptions about various actors' preferences and their ability to manipulate outcomes.

jority parties reward their most loyal members, rather than as possible evidence in support of vote-buying theories.

The next two articles focus on Congress. Carroll and Kim (2010) find that members of the majority party with higher individual roll rates—votes against bills that ultimately passed—tend to receive greater shares of outlays (both in the number of projects and dollar amounts). They explicitly do not examine the distributive consequences of ideology (as distinct from roll rates) for members of the minority party.

Along similar lines, Jenkins and Monroe (2012, 910) present evidence that the majority party "buys its negative agenda control with side payments to its centrist members." They show that the majority party leadership directs a greater share of their campaign contributions to centrist copartisans than to extremists within their party. Consistent with the cartel theory, no such effects are observed within the minority party. They do not explore, however, whether budgetary outlays are deployed in a similar manner.

Lastly, Cann and Sidman (2011) present evidence that parties reward their members with distributive benefits in exchange for raising money for and consistently voting with their party. Members with higher party unity scores and who raise more money for their party tend to receive higher direct outlays, direct awards, and contingent liability rewards. To the extent that party unity varies inversely with ideological centrism, the "exchange theory" on which this study is based produces predictions opposite to that of the vote-buying models discussed above. We investigate whether both mechanisms may be at play in a supplementary analysis below.

Our study builds on and contributes to this literature. It does so first by focusing squarely on the core claims about the distributive consequences of ideological extremism and moderation that come out of a variety of important formal models of vote buying. Moreover, the quasi-experimental research design we offer, which is described in further detail below, supports causal claims where much of the previous literature does not. Finally, in the empirical analyses that follow, we speak to variants of and alternatives to vote-buying theory. Our results, as such, represent the most comprehensive and unified analysis yet of the effects of legislator ideology on distributive politics.

EMPIRICAL STRATEGY AND DATA

Our explanatory variable of interest is a legislator's ideological distance to the median, and our outcome of interest is the total of discretionary outlays received by a given legislator for her constituency. Our primary analysis matches county-level data on federal outlays with corresponding political and demographic variables at the county, district, and state level. The advantage of county-level data is that we can observe the same units over a long period of time, whereas the boundaries of most districts are redrawn decennially. However, we must exclude from our analysis counties that are divided into multiple congressional districts because we cannot cleanly match such counties to a single member of congress.[10] These excluded counties disproportionately represent urban centers around the country and thus encompass a significant proportion of the total US population. Still, after culling out these counties, our main analyses include data on 43% of the total US population from 87% of the nation's counties,[11] varying slightly by year. Several supplementary analyses, including one using district-level data, help to allay concerns that our results are somehow being biased by the exclusion of multimember counties. These are presented under "Robustness Checks and Placebo Tests" below.

A more substantive reason exists for only considering those counties represented by a single legislator. Representation of a single geographic or population unit by a group of legislators is innately a team-production problem. While it is likely that the ideological moderation/extremism of the various members plays a role, the vote-buying theories we draw from provide no insight into how such a collection of ideologies would affect receipt of outlays. The same is true with regard to the role that senators play in the distribution of outlays. Accounting for the Senate, however, does not present the same difficulties for identifying the effect of the ideology of a single member of the House that we face with multimember counties. We conduct an analysis that controls for "Senate-effects" below, discussed among extensions of our main results.

To explain patterns in federal outlays in fiscal year t, which runs from October of year $t - 1$ to September of year t, we use political and demographic characteristics of year $t - 1$. The money spent in year t is the result of the budget passed by congress in year $t - 1$. As such, the distributive effects of vote buying should be observed during the fiscal year after a budget passes Congress.

The data on county-level outlays come from the Consolidated Federal Funds Report (CFFR) over the years in which it was published (fiscal years 1983–2010),[12] excluding

10. County-to-district population correspondence data are from http://mcdc.missouri.edu/websas/geocorr90.html, http://mcdc.missouri.edu/websas/geocorr2k.html, and http://mcdc.missouri.edu/websas/geocorr12.shtml.

11. With the average population by county dropping from 84,271 individuals (across 81,555 total county-year observations) to 41,347 individuals (across 71,199 county-year observations).

12. Although information on federal outlays by county is available through the present from other sources, for the sake of consistency we construct our data set using only outlays as reported in CFFR, of which

FY1983 as we lack a county-to-district correspondence for 1982. A strength of using CFFR is that it enables us to distinguish the set of programs most likely to be subject to political manipulation, nonformula grants, from those that are not, such as entitlements and formula-based grants, which we use for placebo tests. The most relevant summary statistics for this cut of federal funds appear in table 1, and more information on our culling of outlays may be found in subsection B.5 of the appendix. Honing in on nonformula grants makes our analysis more transparent than prior work that relied on a fairly ad-hoc distinction between "low-variation" and "high-variation" programs, which was based on an arbitrary threshold in a program's coefficient of variation across districts to determine a cut-point for exclusion from the analysis (e.g., Berry, Burden, and Howell 2010; Levitt and Snyder 1995).

In estimating the effect of a representative's ideological distance to the median on her receipt of federal outlays, we confront three potential sources of endogeneity. The first includes the many other determinants of outlays that also correlate with a member's ideology. For instance, poorer districts may elect more liberal representatives and also receive more aid from federal programs targeting poverty alleviation, generating a spurious correlation between distance from the chamber median and outlays.[13] Second, voters desiring more federal aid may intentionally choose more centrist representatives. Similarly, extremists may not only possess strong preferences over policies but also value position taking over securing federal monies for their districts. The election of an extremist (or a moderate) may then lead to a decrease (increase) in outlays not because of the ideology of the representative but rather because of the legislator's preferences over position-taking relative to procuring pork barrel spending for her district. Third, individual members seeking to procure more aid for their districts may vote in a more centrist fashion in order to make themselves attractive candidates for vote trading. With regard to the second and third concerns, we would be worried that greater centrism might be associated with other, possibly unobservable, efforts taken by the district or member to obtain more federal spending.

Our research design and measurement strategies offer solutions to each of these concerns. To address the first and second sources of endogeneity, we use a county-by-member fixed-effects model, in which each county-member pair receives its own constant in estimation. The analysis then relies exclusively on within county-by-member changes in distance to the chamber median voter over time. Time-invariant attributes of the county-member pairing are purged by the fixed effects, obviating concerns that, for instance, members representing extremely liberal districts receive more outlays due to the fixed characteristics of their constituents. A range of covariates serve as controls for time-varying characteristics of both members and counties. Further, using county-by-member fixed effects helps us avoid concerns that we are leveraging cross-county variation in the case of members who represent more than one county[14] or that redistricting could introduce an unwanted source of variation if a member's district comprises different counties over time.

Pursuant to the third endogeneity concern, one might still worry that individual members moderate their own voting behavior during years in which they wish to bring home more federal spending—and that they also take other actions to obtain more funding at the same time. To remove this potential source of endogeneity, we rely on a feature of Poole and Rosenthal's Common Space DW-NOMINATE scores (Poole 2005), from which we use the first dimension to de-

Table 1. Key County-Level Descriptive Statistics

Statistic	Absolute Distance from Median	Outlays (in 2010 $1,000s)
Overall mean	.319	9,265
Majority party mean	.190	90,38
Minority party mean	.503	9,590
W/in-group SD	.118	38,900

Note. *Absolute Distance from Median* is the absolute value of median-centered DW-NOMINATE Common Space scores for the calendar years 1983–2009. *Outlays* are county-level grants (excluding formula grants), inflation adjusted to 2010 dollars, for fiscal years 1984–2010. Means are presented for the entire House as well as just the House majority and minority parties. *W/in-group SD* is the within county-by-member standard deviation of each variable. The sample is restricted to only those counties represented by a single congressperson and that are present in our baseline analysis (model 2 of table 2), for a total of 71,199 county-year observations. Of those, 41,872 were represented by a member of the majority and 29,327 by a member of the minority party. Additional summary statistics may be found in table 1 in the appendix.

publication ceased after fiscal year 2010 with the termination of the Federal Financial Statistics program. Data accessed Summer 2013 at http://www.census.gov/govs/cffr/.

13. Indeed if this particular example held true, it would bias us against finding evidence of the vote-buying mechanism we seek to identify, in which more moderate members fare better in terms of distribution.

14. A stark example of this is single-member states, where all counties in the state fall under a single representative, though the phenomenon is hardly limited to such cases.

rive our measure of ideological distance from the median. In the Common Space scores, each legislator's score is fixed over the course of her tenure.[15] Because we use scores that are constant across a member's career in Congress, along with county-by-member fixed effects, we only exploit variation in ideological distance caused by shifts in the location of the median voter. Put another way, changes in a member's distance from the median come only from changes in the composition of the chamber, which are almost surely exogenous with respect to the ideological position of any single representative.

For example, consider a member with an ideal point of −0.50 who served in two congressional terms, one in which the median voter's ideal point was −0.25 and one in which the median voter's ideal point was 0.25. We then ask whether the member received more outlays in the first congress, when her absolute distance from the median was 0.25, than in the second, when her absolute distance was 0.75. The relative ideological location of the member changed between the two congresses only because of changes in composition of the chamber arising from elections in other districts across the nation, making the comparison across terms a valid causal estimate of the effect of distance from the median voter on outlays. Any additional effects of national electoral swings will be accounted for with year fixed effects. Furthermore, even if an individual member's roll call voting record did appear artificially moderate due to vote selling, this artificial moderation could not generate fluctuations in ideological distance due to changes in the median voter's location. Artificial moderation, if it existed, would only bias us against finding any results using our research design.[16]

Our source of identifying variation is displayed in figure 1, which shows the location of the House median voter, measured via DW-NOMINATE Common Space scores, from 1983 through 2009. Key descriptive statistics related to this measure of ideology appear in table 1. There are two major swings in the median voter's location, which are associated with changes in majority party control in 1995 and 2007, as well as smaller year-to-year changes throughout our study period. Given the importance of changes in majority control in determining changes in the location of the median voter, it will be important to control for majority status, as well as the interaction of majority status and ideological distance, in our analysis. These issues receive sustained attention below.

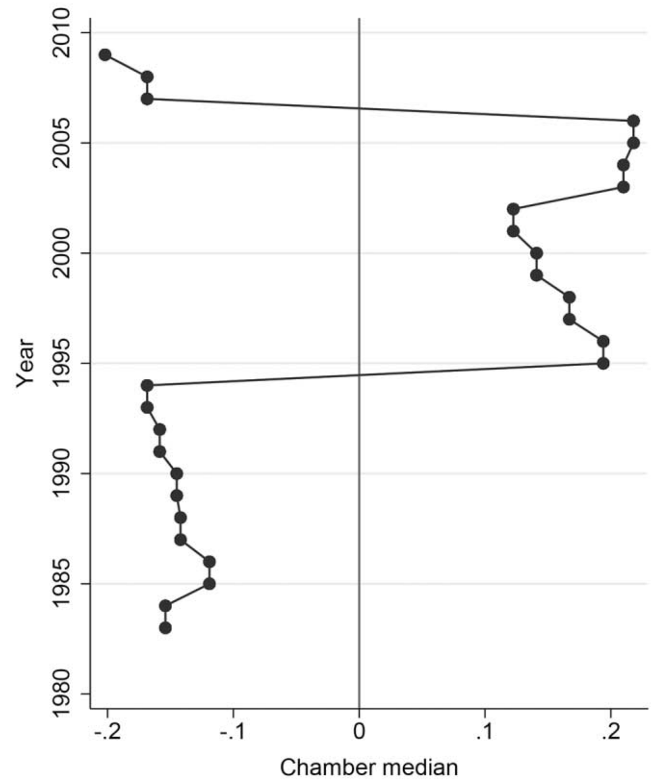

Figure 1. Movement in the House median by year. The horizontal axis represents the location of the median member of the House of Representatives as measured in DW-NOMINATE Common Space scores.

Formally, we estimate the following general model:

$$\ln(outlays_{it}) = \beta_0 + \alpha_i + \delta_{t-1} + \beta_1 |Distance_{i,t-1}| + \mathbf{X}_{i,t-1}\Phi + \varepsilon_{i,t-1}, \qquad (1)$$

where the dependent variable is the log value of county-level outlays in a given fiscal year t.[17] The fixed effects, α_i, are generally county-by-member specific, though we examine the robustness to using just county and just member fixed effects instead. All models also include year fixed effects, δ_{t-1}, for the preceding calendar year and a constant β_0. Our

15. More precisely, when a legislator switches parties, they are assigned a new ICPSR ID number and thus allowed a new fixed ideological estimate in the calculation of Poole and Rosenthal's data. As a result, scores are fixed over the course of a legislator's tenure in a given party affiliation. This affects only a few cases.

16. Moreover, it is likely that the number of votes a given legislator trades for outlays is small relative to the total number of votes she casts, and that the Common Space score is based on a largely ideologically consistent body of decisions, with the few aberrations contributing little to the estimation of the score.

17. We add one to the value of all outlays so that observations in which a county received zero of some type or cut of outlays remain in the analysis and are given a value of zero when log-transformed. Substantively, this amounts to assuming that receiving one dollar in outlays is effectively the same as receiving nothing, and the addition of one has an even less discernible effect on those counties receiving strictly positive amounts. All analyses were run without these observations, and the results change minimally across the board.

variable of interest is the absolute value of the ideological distance to the floor median for the representative of a given county as calculated with DW-NOMINATE Common Space scores. The coefficient for this variable is β_1. A vector of covariates $\mathbf{X}_{i,t-1}$ has corresponding coefficients given by Φ. Political covariates take on member-year specific values, where the member is the representative associated with a given county, and the demographic characteristics are county-year measurements.

When selecting time-varying political covariates, we take our cues from the existing empirical literature on the determinants of federal distribution. We use dummy variables for membership in the party of the president, majority status, party affiliation (if using just county rather than county-member fixed effects) committee membership, and being a party leader, committee chair, or ranking minority committee member (Nelson 2013; Stewart and Woon 2013).[18] A tenure variable tracks the number of terms served by a given representative, and we include an additional indicator for a representative's first term.[19] A measure of party competitiveness, *Close election*, identifies those instances when a member receives less than 5% of the two-party vote share in the last election. The last of the political variables, also electoral in nature, is the absolute value of the state-wide difference in vote shares between the sitting president and the other major party candidate in the previous election.[20] This value decreases in the competitiveness of the previous presidential election in a given area, while the close congressional election dummy identifies more competitive elections.

Among the factors recognized by the existing empirical literature, majority party membership is most likely to confound the effect on the distribution of outlays of our variable of interest, absolute distance to the median. The median voter will almost certainly be a member of the majority party, and other members of the majority will tend to be found closer to the median voter on an ideological continuum than will their peers in the minority. As such, a representative with low ideological distance to the median will more likely be drawn from the majority party than the minority party. One might be concerned then that results indicating the importance of ideological distance reflect majority party influences rather than the vote-buying mechanisms posited in our theory. Moreover, the implication of the majority party cartel theory put forward by Cox and McCubbins (1993) that Jenkins and Monroe (2012) test (using campaign contributions, rather than distributive outlays) represents yet another complication in considering the role of majority party status vis-à-vis ideological distance from the median. Therefore, to disentangle their theoretical claims from those that emerge from the vote-buying literature in the empirical tests that follow, we include majority party status as both a control and later as an interaction with our variable of interest.

With the increase in polarization over our sample period, documented at length in McCarty, Poole, and Rosenthal (2006), it is possible that new members entering the chamber tended to make their senior colleagues relatively more moderate. We might worry about conflating seniority (and the distributive benefits it may confer) with moderation. By controlling for a member's number of terms in office with the *Tenure* variable, we are able to account for the effect of seniority. While this accounts for the most likely way in which increasing polarization would confound our analysis, we more directly explore the interaction of legislator ideology and increasing polarization in a supplementary analysis below.

Given our nearly 30-year sample, the lengthy tenure of many representatives, and the fact that some members in our sample represent multiple counties in our data, it is important to account for intracounty demographic changes over time. We therefore include the log values of county population and income as controls.[21] To account for correlation of the error term ε_{it} both across and within counties over time, we cluster standard errors at the state level. While our primary concern is that the error terms for counties represented by the same legislator (i.e., in the same district) would be correlated, we recognize that counties across a state may also share common effects we have not captured.[22] As such, clustering at the state level, which subsumes clustering at lower levels, represents a rather conservative treatment of the standard errors. Further information about the data and the decisions we made in compiling it may be found in section B of the appendix.

MAIN RESULTS

Table 2 presents our primary estimates of the effect of absolute distance from the chamber median on the distribution of federal outlays. All models include county-by-member

18. Committee data sets accessed summer 2013 at http://web.mit.edu/17.251/www/data_page.html.

19. Information on individual legislators' ideology, political affiliation, and tenure accessed summer 2013 from http://voteview.com/dwnomin_joint_house_and_senate.htm.

20. Both electoral variables accessed Summer 2013 at http://library.cqpress.com/elections/.

21. Data accessed accessed summer 2013 at http://bea.gov/iTable/index_regional.cfm.

22. E.g., due to Senate effects, an issue we address in the next section as an extension of our baseline analysis.

Table 2. Absolute Distance and Rank from Median

	(1)	(2)	(3)	(4)	(5)	(6)
Absolute distance from median	−.147*	−.608**	−.591**			
	(.081)	(.300)	(.274)			
Absolute rank from median (/100)				−.026*	−.120**	−.116**
				(.014)	(.045)	(.044)
Majority party		−.162*	−.158*		−.184**	−.178**
		(.091)	(.083)		(.073)	(.072)
President's party		.029	.031		.039	.040
		(.036)	(.033)		(.040)	(.036)
Committee chair		.042	.011		.041	.011
		(.088)	(.087)		(.087)	(.088)
Ranking minority member		−.018	−.034		−.020	−.036
		(.068)	(.062)		(.069)	(.063)
Party leader		.088	.008		.082	.003
		(.089)	(.083)		(.088)	(.082)
First term		.010	−.004		.010	−.004
		(.028)	(.024)		(.028)	(.024)
Tenure (no. of terms)		−.178	−.214		−.179	−.213
		(.124)	(.139)		(.127)	(.140)
Close election		.135***	.136***		.135***	.136***
		(.036)	(.033)		(.036)	(.033)
State presidential margin		.004*	.004*		.004*	.004*
		(.002)	(.002)		(.002)	(.002)
Log income	−.092	−.123	−.125	−.090	−.114	−.118
	(.173)	(.189)	(.178)	(.173)	(.188)	(.178)
Log population	−.064	−.046	−.029	−.065	−.048	−.031
	(.277)	(.279)	(.266)	(.276)	(.279)	(.267)
Constant	14.602***	14.724***	14.586***	14.593***	14.724***	14.602***
	(1.646)	(1.694)	(1.553)	(1.640)	(1.654)	(1.523)
Committee dummies	No	No	Yes	No	No	Yes
Adj. R^2	.218	.220	.221	.218	.220	.221
N	71,199	71,199	71,199	71,199	71,199	71,199

Note. Standard errors are clustered by state. County-by-member and year fixed effects used in all models. The dependent variable is the log value of non-formula grants received by a given county in a given year. The outlays data span fiscal years 1984–2010 and are matched with explanatory variables from the previous calendar year. *Absolute distance from median* is the absolute value of the median-centered first dimension of the DW-NOMINATE Common Space scores. *Absolute rank from median (/100)* is the rank ordering of the *Absolute distance from median* variable divided by 100 for scaling purposes; the higher the rank, the farther a legislator is ideologically from the median.
* $p \leq .10$.
** $p \leq .05$.
*** $p \leq .01$.

fixed effects and as a result only take advantage of exogenous changes in each individual member's distance to the median that are generated by elections in other districts. Models 1–3 regress log outlays at the county-level on our measure of absolute distance from the median in NOMINATE space. As a robustness exercise, models 4–6 regress log outlays on the legislators' rank-ordered absolute distance—that is, the number of other representatives between the member and the median voter—per 100 legislators.

Models 1 and 4 regress the log value of county-level outlays on our main variable of interest, as well as the county-level demographic covariates and log values of population and income. As the dependent variable is in logs, the appropriate interpretation for a one-unit change in the regressor would be, in the case of model 1, an approximately 15% decrease in the amount of outlays a district receives.

In models 2 and 5, we add all of the political covariates except for the dummy variables representing membership on specific committees. In doing so, we see the effect sizes of both *Absolute distance from median* and *Absolute rank from median* (*/100*) increase markedly to −0.608 and −0.120, respectively, and gain statistical significance.

The positive and significant effect of being in a highly contested district, the *Close election* variable, on outlays reflects the importance of electoral objectives in determining the distribution of outlays, but it also helps allay concerns that the significant negative effect of *Absolute distance* on outlays stems from electoral considerations rather than our proposed legislative mechanism. If electorally close districts are more likely to be represented by moderates, then absolute distance would have covaried inversely with our close congressional election measure. As such, it might have been the case that *Absolute distance* reflected safer districts, less in need of targeted federal funds, and we would see a negative coefficient that in fact had nothing to do with our proposed vote-buying mechanism. By including the measure of the electoral competitiveness of a district, we would then expect the effect of *Absolute distance* to fall. In fact, the effect size increases, lending further credence to our core theoretical claims.[23]

Lastly, in models 3 and 6, we add indicator variables for membership on all of the standing committees (variables not shown). Membership on committees, especially those thought to be most influential in distributive politics (Appropriations, Ways and Means), could interfere with the stylistic representation of Congress in vote-buying theory, primarily if such posts provided easier access to procurement processes or project-making opportunities. However, memberships on any of the standing committees appear to make little difference to both the effect sizes and significance levels of our variables of interest. While the committee membership indicators are jointly significant, the *F*-statistic is remarkably small given the number of variables we are testing.[24] We omit the committee dummies in subsequent analyses, although our results are robust to their inclusion.[25]

When interpreting the magnitude of the estimates, it is important to remember that the location of a legislator relative to the median simply never changes by as much as one unit in NOMINATE space. As a result, it makes more sense to consider a standard deviation's worth of change in *Absolute distance*. For model 2, the within-member standard deviation for the sample used in estimation is 0.118.[26] We would then associate a 7.2% decrease in outlays with a one standard deviation increase in distance from the median. Put differently, a one standard-deviation increase in distance from the median leads to an approximately $670,000 decrease in outlays (at the county level, in 2010 dollars), or a loss of a little more than $16 per capita for the average county in our sample. For *Absolute rank*, the within-group standard deviation is 0.681, implying an 8.2% decrease in outlays associated with a one standard-deviation increase in ranked distance from the median in model 5. In subsequent analyses, we continue with only the *Absolute distance* variable, noting that highly similar results are recovered from both measurement strategies. Model 2 in table 2 serves as our baseline model for comparisons in the foregoing analyses.

Numerous auxiliary analyses were performed, all of which may be found in section C of the appendix. Several bear mentioning here. First, we explored different fixed effects strategies, namely, the inclusion of no fixed effects, county fixed effects, and county-member fixed effects.[27] Comparing the different fixed effects strategies, we see that using member fixed effects produces estimates largely identical to the even more stringent county-by-member fixed effects that we use. To preserve our identification strategy, we continue to employ county-by-member fixed effects.[28]

Given that vote-buying theory makes no clear predictions about the specific functional form of equilibrium payments, a variety of other characterizations of the *Absolute distance* variable were tried, including the addition of higher and lower order terms as well as the natural log of *Absolute*

23. While the estimate of the coefficient for the variable *President's party* is not a statistically significant, we refer readers to Berry et al. (2010) for a complete analysis of this effect. The effect we estimate is close in magnitude to theirs, but less precisely estimated. This may be as expected given that we use only single-member counties and impose more fixed effects. Berry et al. (2010) explain that estimating the effect of *President's party* requires only the use of county-level fixed effects, rather than the county-by-member fixed effects used here.

24. Only a couple of these variables display statistical significance, which is an expected result given the number of tests being performed, even if all of the null hypotheses of zero effect held true.

25. Given the lack of significance of most of our control variables, one might worry that their inclusion, while theoretically justified, distorts the true effect of *Absolute distance*. In table 2 of the appendix, we sequentially add the control variables, showing that the jump in significance, both substantive and economic, comes from the inclusion of *Majority party* and, to a lesser extent, *Close election*. These two variables display consistent statistical significance and, more importantly, most plausibly confound the effect of *Absolute distance*. We address the important interaction between *Absolute distance* and *Majority party* in the next section.

26. See table 1.

27. See table 3 in the appendix.

28. It is worth noting that although county-level fixed effects, as in model 2 allow us to employ more variation than models 3 and 4, a good deal more than just a county's representative's distance to the median changes when a county's representative changes. This may lie behind the perhaps surprising finding that the estimated effect of *Absolute distance* is smaller when using only county-level fixed effects.

distance.[29] Only the specification in levels proved to be robust across models and across specifications of the dependent variable. We also carried out an analysis using splines, in which *Absolute distance* was segmented and allowed to take on different slopes on either side of a knot point placed at gradually increasing distances from the median.[30] We found that the effect of absolute distance from the floor median tended to be concentrated around, but not limited to, near-median legislators, as vote-buying theory would suggest. Finally, we estimate a separate effect for members of the majority on the far side of the median relative to most of their copartisans; this analysis is discussed in the next section, following the discussion of majority party status.

The House, of course, does not distribute federal outlays alone. The Senate also is involved. For the contributions of senators to confound our main results, however, a decrease (increase) in a House member's distance from the median would need to coincide with an increase (decrease) in the amount of federal outlays driven by a senator to counties represented by that House member. Though unlikely, we cannot dismiss this possibility out of hand. Hence, we add several additional controls to our baseline model that are explicitly intended to account for possible Senate confounders.

In table 6 of the appendix, model 1, we add a variable counting the number of senators from the state of a given county who share the same party as that county's representative, a dummy variable if a county's representative comes from the same party as the senate majority party, and a variable counting the number of senators from the state of a given county who share the same party as the president. All of these variables could positively correlate with the allocation of funds to a given county for reasons that primarily relate to Senate activities. While the estimated coefficients for the first two of these variables display the expected positive sign, none achieve statistical significance at conventional levels. Moreover, including these potential confounders does not notably alter our estimates of the main variable of interest, distance from the house median legislator.

In model 2 of online table 6, we include the natural log of per capita nonformula grants awarded to all other counties in a given county's state. This variable captures the success of the state's entire congressional delegation at procuring pork barrel spending. Without including the county's specific allotment, this variable reflects the aptitude of a state's senators, albeit not exclusively, at obtaining funds for their state. Not surprisingly, this variable is positively signed and highly significant. Importantly, though, the estimate for the effect of *Absolute distance* is similar in magnitude and statistical significance to our baseline model, suggesting that our effect of interest persists even after including the state's level of per capita spending. Model 3 simply includes all variables from models 1 and 2, and results are consistent with those models.

One might be concerned that distance from a member's own party median also influences rewards flowing to the district (see, e.g., Cox and McCubbins 2005), a confounding effect that could inflate and/or deflate our estimates of interest. Running our baseline model with a variable measuring absolute distance from *party* mean, we observe a positive and statistically significant estimate of the coefficient for this new variable.[31] One standard deviation's worth of within-member variation in absolute distance from party mean is so small, however, that we are reluctant to conclude from our analysis that intraparty extremists receive more in distributive outlays.

To examine whether tendencies to vote with or against one's party reflect general extremism or moderation and to explore the possibility that Cann and Sidman's (2011) exchange theory is at work alongside vote buying, we used party unity scores in lieu of absolute distance from the median as well as alongside the distance variable.[32] In the models including both *Absolute distance* and *Party unity*, we too find evidence that (especially majority) members who vote with their party receive greater outlays than peers who vote with the opposition. However, the coefficient on *Party unity* is considerably smaller than the coefficient for *Absolute distance*, which retains its significance and anticipated sign. While party loyalists appear to be rewarded for toeing the line, their support may be taken relatively for granted compared with their more centrist peers.

Lastly, using conservative vote probability (CVP) scores (Fowler and Hall 2012),[33] an alternate roll call-based measure of legislator ideology, yielded similar magnitude of effect size and consistently negative coefficients on the estimates of the effect of absolute distance from the median on log outlays.[34] We take the similarity of the results as encouraging and also point out that CVP scores feature an ease of interpretation that NOMINATE scores lack. The estimates from regressions using CVP scores may be read as the percent increase in outlays associated with a given increase

29. See table 4 in the appendix.
30. See table 5 in the appendix.
31. See table 7 in the appendix.
32. See table 8 in the appendix.
33. CVP scores available at http://www.andrewbenjaminhall.com/papers/.
34. See table 9 in the appendix.

in the probability of voting conservatively relative to the median member of the chamber.

MAJORITY PARTY STATUS

Majority status merits a deeper discussion than most of the other controls and covariates. From a purely empirical standpoint, majority party status plays a key role with regard to *Absolute distance*. Distance from the floor median will be inversely correlated with majority party status for the simple reason that the chamber median lies within the majority party (given that the parties have ceased to overlap in NOMINATE space). More generally, Wiseman and Wright (2008) document that majority members will be on average closer to the chamber median. They emphasize that partisan and floor/median sources of legislative influence are thus often complementary, as the median moves strongly in the direction of the majority party. As a result, empirical tests that seek to find evidence for one influence could simultaneously represent evidence for the other. Accounting for this correlation is essential for identifying the effect of each variable individually.

The relationship between majority status and outlays, however, is theoretically ambiguous. Theories in which the majority party extracts rents for itself would suggest that majority status leads an individual representative to receive more in federal outlays. If the majority party used its control over distributive funds to quiet the opposition, the effect of majority status on outlays could even be negative. Theories of negative agenda control meanwhile predict that the majority moves the agenda to the center of its party's distribution and pays off moderate members of its own party to compensate them for policy losses. If indeed this theory only applies to the majority party, with its dominant ability to set the House agenda, then we might expect to find a discernible relationship between legislator ideology and federal outlays only among members of the majority party.

Given these various sources of ambiguity, we explore the separate and joint contributions of legislative extremism and majority status in table 3. Model 1 includes *Absolute distance* without the variable for majority status but with the remainder of the controls. The coefficient is negative, as expected, but imprecisely estimated. Model 2 includes only majority status along with the controls, leaving out our measure of absolute distance to the median. Although insignificant, the positive coefficient estimated here supports either a rent-seeking theory or, to the extent that majority party covaries inversely with *Absolute distance*, accounts of vote buying. Model 3 includes both absolute ideological distance to the median and majority status, along with the other controls we have employed throughout. The coefficient estimate for *Absolute distance* is statistically significant and approximately four times larger than that in a comparable model that did not include majority party status. Here the coefficient for majority status is now both significant and negative.

Model 4 adds an interaction term between *Majority party* and *Absolute distance*. The effect of *Absolute distance* appears to be stronger within the minority party, but we cannot reject that there is no difference in the effect of absolute distance from the chamber median between the majority and minority parties. Most importantly, however, we see no evidence that our results for the effect of ideological distance to the median are an artifact of majority status. Moreover, the fact that *Absolute distance* remains significant in the presence of an interaction variable suggests that moderate minority members are also beneficiaries of vote buying (whether from their own party or the majority party cobbling together coalitions). Outlays emerge as a broad instrument of coalition building, extending beyond negative agenda control's stricter conception of being used by the majority party on majority party members.

While the interaction term lacks significance in its own right, it does provide traction on the following question: How can the average majority party member obtain more outlays than the average minority member, as implied by model 2, while bringing home less than a minority member located at the same ideological distance from the chamber median, as implied by model 4. The answer is that the average majority party member is much closer to the chamber median than is the average minority party member. Specifically, the average absolute distance from the median for members of the majority party is 0.190, while the average absolute distance from the median for members of the minority party is 0.503.[35] Figure 2 presents a visual representation of the estimates from model 4. The negative relationship between ideological distance and outlays holds within both the majority (solid lines) and minority (dashed lines), which is our main point of interest here. In addition, at any given distance from the median that both minority and majority members might obtain, a minority party member is predicted to garner more outlays than a majority party member at the same location. However, given their generally closer proximity to the median, the "average" member of the majority party (vertical solid line) acquires slightly more overall outlays (horizontal solid line) than the "average" member of the minority party (vertical dashed line) receives in outlays (horizontal dashed line). Still, the graph

35. See table 1.

Table 3. Interacting *Absolute Distance* and *Majority Party*

	(1)	(2)	(3)	(4)
Absolute distance from median	−.168*		−.608**	−.660*
	(.090)		(.300)	(.334)
Majority party		.043*	−.162*	−.221
		(.024)	(.091)	(.140)
Absolute distance × majority				.238
				(.246)
President's party	.039	.041	.029	.028
	(.041)	(.041)	(.036)	(.036)
Committee chair	.037	.043	.042	.040
	(.086)	(.090)	(.088)	(.087)
Ranking minority member	.002	−.001	−.018	−.018
	(.083)	(.083)	(.068)	(.067)
Party leader	.089	.091	.088	.086
	(.086)	(.086)	(.089)	(.088)
First term	.012	.015	.010	.011
	(.027)	(.027)	(.028)	(.027)
Tenure (no. of terms)	−.168	−.167	−.178	−.185
	(.119)	(.119)	(.124)	(.125)
Close election	.136***	.134***	.135***	.135***
	(.037)	(.036)	(.036)	(.035)
State presidential margin	.004*	.004*	.004*	.004*
	(.002)	(.002)	(.002)	(.002)
Log income	−.127	−.128	−.123	−.124
	(.184)	(.183)	(.189)	(.189)
Log population	−.050	−.049	−.046	−.055
	(.276)	(.275)	(.279)	(.283)
Constant	14.588***	14.512***	14.724***	14.839***
	(1.669)	(1.659)	(1.694)	(1.732)
Adj. R^2	.220	.220	.220	.220
N	71,199	71,199	71,199	71,199

Note. Standard errors are clustered by state. County-by-member and year fixed effects used in all models. The dependent variable is the log value of nonformula grants received by a given county in a given year. The outlays data span fiscal years 1984–2010 and are matched with explanatory variables from the previous calendar year. *Absolute distance from median* is the absolute value of the median-centered first dimension of the DW-NOMINATE Common Space scores. This variable is interacted with the dummy variable for majority party status in model 4.
* $p \leq .10$.
** $p \leq .05$.
*** $p \leq .01$.

emphasizes one crucial point: minority party members located closest to the median have the highest expected outlays. This pattern is consistent with a model in which marginal members of the opposition are often targets of vote buying.

As additional tests, we interact an indicator variable for periods of unified government (defined as instances when the Senate and House majorities as well as the president are all of the same party) with the *Absolute distance* and *Majority party* variables.[36] If legislation is more difficult to pass during periods of divided government, as some have found (Howell et al. 2000), we may observe a greater reliance on vote buying and/or agenda setting using federal outlays as the instrument. While the *Unified government, Absolute distance*, and *Majority party* variables remain significant as the regressions progress toward full saturation, the interaction terms do not retain significance throughout this process. As such, although model 3 of that analysis would actually suggest vote buying is more active during times of unified government, we hesitate to draw firm conclusions.

We may similarly wonder whether increased polarization makes vote buying a more essential feature of the legislative process. To investigate this, we interact McCarty et al.'s

36. See table 10 in the appendix.

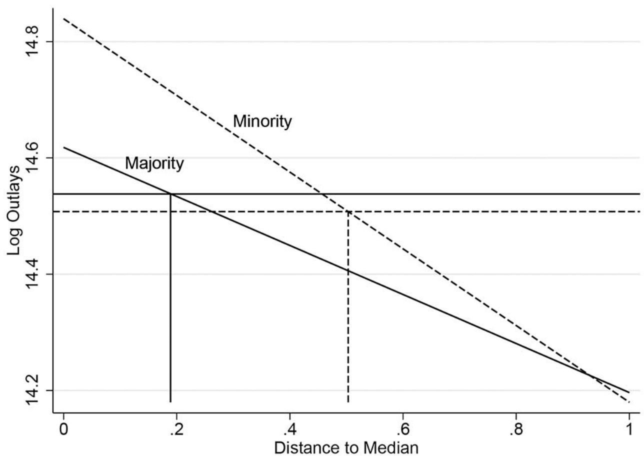

Figure 2. Understanding the interaction between *Distance from Median* and *Majority Party*. The solid lines correspond to the majority party, while the dashed lines correspond to the minority party. The downward-sloping lines are the outlays awarded to members of each party as a function of the members' absolute distance to the median in DW-NOMINATE Common Space scores. These are based on the estimates from model 4 of table 3. The vertical lines represent the average absolute distance from the median for each party, as in table 1. The horizontal lines pass through the intersection of each pair of diagonal and vertical lines. The horizontal line for the majority party (14.538) lies just above the horizontal line for the minority party (14.508), indicating the average member of the majority party receives more than the average member of the minority party, as found in model 2 of table 3.

(2006) polarization index, measured as the difference between the mean Republican and mean Democrat DW-NOMINATE score, with our *Majority* and *Absolute distance* variables.[37] *Absolute distance* remains significant when we reintroduce it to the models, with an almost identical coefficient as in our baseline model, leaving us with confidence that our results are not an artifact of polarization. Beyond this, the analysis provides only weak evidence that vote buying may have become more active over this period of increasing polarization.

In a final analysis related to the majority party, we again relax the implicit assumption of a single linear effect of distance from the median across the entire chamber and focus on majority members on the far side of the floor median from the majority of their party.[38] We find that evidence of vote buying is at least as strong with these moderate majority members as with the moderate majority members closer to their party median. This is further support that vote buying is not limited to negative agenda control, but takes place on both sides of the median, and possibly with both parties doing the buying.

37. See table 11 of the appendix.

38. See table 12 in the appendix.

ROBUSTNESS CHECKS AND PLACEBO TESTS

We perform two sets of checks against our baseline model.

Alternative characterizations of outlays

In table 4, we present the results of additional robustness tests and a "placebo" test for which we expect to find no effects. The first three models consider alternative segmentations of federal grants. The first column is our baseline model, culling out formula-based spending from the grants category of outlays (as in the models in table 2)—the spending that should be most susceptible to political manipulation.[39] The second column uses the entire grants category, and the third column looks at only formula-based grants, where we would expect to see the smallest, if any, effects of vote buying. We see that the significance of the negative coefficient for absolute distance to the floor median is robust to all of these specifications, but as expected, the effect size and significance attenuate as the focus moves to what we would presume would be less manipulable, formula-based grants.

We see the same pattern in the *Close election* variable. Again, it is commonly held that marginal seats are targeted with funds to bolster the incumbent's strength in the coming election. The ideal funds for such purposes would presumably be the more manipulable, nonformula based grants. Indeed we see that as we introduce formula-based spending and then consider only formula-based spending across models 1–3, the positive coefficient on the *Close election* variable steadily attenuates, as expected.

Model 4 uses an altogether different CFFR category, namely direct disability and retirement payments, which are the major categories of entitlements. Because they should not be subject to vote-buying activities, these payments provide a useful placebo test. While the estimated coefficient for *Absolute distance* is statistically significant, it switches sign and is an order of magnitude smaller than our baseline estimate. Not seeing a negative coefficient and losing all economic significance, this last, placebo model serves as encouragement that the results of our baseline model reflect the vote-buying mechanism as predicted by theory.

Were our baseline results driven by the exclusion of multimember counties?

As discussed above, counties with multiple representatives present difficulties for identifying the effect of a single legislator's ideology on the outlays allotted to her constituency.

39. See table 13 in the appendix for models which use all grants as the dependent variable. Patterns of significance are largely similar across models and variables, though as seen in table 4, results attenuate vis-à-vis nonformula-based grants.

Table 4. Robustness and Placebo Tests

	Robustness			Placebo
	No Formula (1)	All Grants (2)	Formula Only (3)	Dis. and Ret. (4)
Absolute distance from median	−.608**	−.321**	−.188	.058*
	(.300)	(.133)	(.115)	(.029)
Majority party	−.162*	−.085*	−.046	.022**
	(.091)	(.044)	(.040)	(.011)
President's party	.029	.004	−.007	.001
	(.036)	(.008)	(.007)	(.003)
Committee chair	.042	.038*	.053**	.007
	(.088)	(.020)	(.024)	(.011)
Ranking minority member	−.018	.003	−.023	−.025*
	(.068)	(.023)	(.028)	(.015)
Party leader	.088	.073*	.068	.022
	(.089)	(.043)	(.042)	(.016)
First term	.010	−.011	−.020	.005
	(.028)	(.012)	(.014)	(.003)
Tenure (no. of terms)	−.178	−.039	−.015	−.015*
	(.124)	(.030)	(.025)	(.008)
Close election	.135***	.042***	.021*	−.003
	(.036)	(.010)	(.012)	(.003)
State presidential margin	.004*	.003***	.003**	−.000*
	(.002)	(.001)	(.001)	(.000)
Log income	−.123	.008	.071	.005
	(.189)	(.059)	(.050)	(.047)
Log population	−.046	.209**	.296***	.719***
	(.279)	(.086)	(.061)	(.051)
Constant	14.724***	12.481***	10.555***	8.822***
	(1.694)	(.649)	(.575)	(.348)
Adj. R^2	.220	.674	.808	.926
N	71,199	71,199	71,199	71,199

Note. Standard errors clustered by state. County-by-member and year fixed effects used in all models. The dependent variable is the log value of outlays received by a given county in a given year. Outlays are defined as all nonformula grants in model 1, all grants in model 2, only formula grants in model 3, and disability and retirement payments to individuals in model 4. The outlays data span fiscal years 1984–2010 and are matched with explanatory variables from the previous calendar year. *Absolute distance from median* is the absolute value of the median-centered first dimension of the DW-NOMINATE Common Space scores. Models 1–3 are marked "Robustness" as we expect to observe some, perhaps attenuated, effect of *Absolute distance* on outlays even across different cuts of the grants category. Model 4 is labeled "Placebo" as we do not expect to observe any effect of *Absolute distance* on outlays for disability and retirement payments to individuals.
* $p \leq .10$.
** $p \leq .05$.
*** $p \leq .01$.

These counties include, at a minimum, the most populous regions, as well as any smaller counties through which district lines happen to be drawn. While the results from our culled sample of single-member counties have proven robust to a wide variety of analyses, there remains the possibility that these results are not representative of the entire population and are being somehow biased by the exclusion of the most populous counties. We therefore perform four additional analyses that, taken together, suggest that our findings are not an artifact of our specific sample.

First, we adopt several strategies to expand our sample. We begin by simply including each county-representative pair as an observation, matching this pair to the overall spending of the county. This constitutes model 1 of table 14 (available in the appendix). This approach is of course imperfect, as it attributes all nonformula grants of a county to each of the representatives sharing the county and, perhaps more troubling, uses this same level of spending as the outcome variable over multiple observations. That said, our results on *Absolute distance* prove robust to the inclusion of these additional observations. The effect size attenuates only slightly and retains statistical significance.

In models 2–4 of table 14, we choose a single representative of the multi-member county, resulting in a single observation for each county. In selecting a representative, we choose the member with the smallest absolute distance from the median in model 2 and the member with the largest absolute distance from the median in model 3. The estimated effect size in model 2 is almost identical to that in the culled sample of our baseline model, and with a similarly high level of statistical significance. Only in model 3 when choosing the member farthest from the median to represent multi-member counties do we see the estimated effect size (slightly) attenuate and fall (just) below statistical significance, as might be expected. Model 4 chooses the member whose district contains the largest share of a county's population as the representative for that county in the sample. The results in this model are again nearly identical to our baseline results. These models, then, not only mollify concerns that our results were biased by the exclusion of urban counties but also yield additional evidence in line with the predictions of vote-buying theory.

Second, we divide the culled sample into four quartiles based on population. We do so in order to investigate whether there exist differential effects by population among those counties for which we have a clean county-to-representative match, suggesting that it would be wrong to extrapolate the results from our culled sample to the excluded counties with larger populations. The results of this analysis are presented in table 15 of the appendix. The estimated effects of *Absolute distance* across the four quartiles of population are remarkably similar to the findings for the sample as a whole. All retain statistical significance, none are statistically distinguishable from the others, and no evidence of a monotonic trend emerges. As such, there is no reason to expect that the effect would be different for the more dense counties which are excluded from our main analysis.

To further assess the robustness of our core findings, we employ an altogether different data set: the Federal Assistance Award Data System (FAADS) data on district-level outlays.[40] The FAADS maps federal outlays into congressional districts, and thereby avoids the challenges associated with ensuring a clear correspondence between a county and a single congressperson. This feature of the data allows us to use all districts, without excluding the most urban areas as we had to do in the county-level analysis. As with the CFFR data, it is reasonably straightforward to identify and cull out formula-based spending. When using FAADS, however, new complications arise. We must remove redistricting years, which introduce a mismatch between districts in which outlays are distributed and the political and demographic variables associated with their authorization. Additionally, when using district-member fixed effects, we must employ redistricting-specific member fixed effects for each period in which different district boundaries apply in order to account for the fact that members may represent substantively different geographies following decennial redistricting. As such, we significantly reduce the temporal variation present in this model vis-à-vis the county-level analysis that spanned nearly thirty years within units.

Using only nonformula grants as the dependent variable and district-member fixed effects, the coefficient of the absolute distance variable does not appear significantly different from zero and displays a sign contrary to our expectation. Models employing all grants as the dependent variable or CVP scores as the basis for *Absolute distance* more consistently retain the expected negative sign.[41] In other words, the district-level results are uneven and inconclusive when using district-member fixed effects. Notable, however, is that in none of these analyses do the results become stronger when restricting the sample to only those districts represented in the county-level analysis. This suggests that our county-level results were not driven by the exclusion of the most populous counties (i.e., those containing more than one district).

The district-level results could fail to hold for two main reasons: the inclusion of the urban areas we had to exclude in the county-level analyses, or the fact that district boundaries change, forcing us to reset the district-member fixed effects every 10 years and thus limiting the variation available for estimation. The two sets of analyses that include the most populous counties and split up the sample by population quartiles weigh in favor of the second explanation. In the fourth and final analysis related to the representativeness of the culled sample, we impose artificial redistricting on our county-level sample by resetting the county-by-member fixed effects in 1993 and 2003.[42] If the results are similarly inconclusive, it suggests that the mixed results of

40. Our data use and extend district-level outlays data as documented in Bickers and Stein (1991). See Berry et al. (2010) for a previous use and extension of the FAADS data.

41. We omit results here and refer the reader to tables 16–19 in the appendix.

42. In fact, not all redistrictings occurred in exactly the years preceding these races, and the district-level analysis reflects this fact.

the district-level analysis stemmed not from including the urban centers excluded from the county-level analysis, but rather from the need to observe our units of observation over smaller, disjointed periods of time.

In table 20 in the appendix, we find that indeed our estimated effect sizes attenuate relative to our baseline estimates, and statistical significance vanishes in a couple of cases. In conjunction with the three preceding sets of analyses, this indicates that the inconclusiveness of the district-level analysis stems from our accounting for redistricting rather than the systematic exclusion of the most populous areas.

CONCLUSION

This article presents the most comprehensive and compelling evidence to date that ideological moderates receive more distributive outlays than do ideological extremists within Congress. The estimated 7% decrease in outlays associated with an exogenously derived one standard-deviation increase in ideological distance from the median remains significant both statistically and substantively, resistant to false positives in placebo tests, and robust to various specifications and measures of both absolute distance to the median and outlays.

The vote-buying models with which we motivate our study offer both theoretical justification for the study as well as an explanation of the results. While a representative may forgo some distributive benefits for her district, if she or the voters she represents value taking an ideological stance enough, the benefits of position-taking outweigh any distributive costs. Conversely, if a representative or her voters are more moderate, then the opportunity to tender one's vote in order to bring outlays back to the district may carry the day.

Due to the inherent unobservability of side payments, we cannot be completely sure that we have captured the unique effects of vote buying on the distribution of outlays. Our research design rules out objections related to the taste of legislators for pork barrel relative to legislative activities and other similarly first-order threats to our causal claim. Past and future work that ties vote-buying efforts on specific bills directly to earmarks and outlays is mutually complementary with our analysis in providing evidence that vote buying takes place and does so in line with theoretical predictions. Future data collection efforts that link payments and specific bills may better illuminate the particular conditions under which vote buying actually occurs. The existing theoretical literature scrutinizes who will be paid for voting for a single bill. Empirically, however, payments may be offered on the basis of continued support across multiple bills. Further, the propensity of any payments to be made may depend, in turn, on substantive features of the policies for which existing theory does not account. To move beyond the average effects of ideological extremism on overall federal outlays, which we have documented here, new and substantial investments in data collection are required.

The findings in this article also suggest that the effect of majority party status is more difficult to track empirically than to predict theoretically, where it is widely held to be an important determinant in legislative bargaining and distributive politics. The analysis performed herein suggests that this may be due to the covariation and interaction of majority status with ideological distance to the median, as well as a result of the possible concurrence of negative and positive agenda control. A deeper understanding of these forces may help subsequent research better account for majority party influence in distributive politics.

Finally, the team production issues that prevented a straightforward application of vote-buying theory to the ideology of senators and to counties served by multiple representatives deserve attention in their own right. How might theories of vote buying and agenda setting be modified or adapted to consider such cases? While the findings in this article provide compelling evidence for the role of ideology in outlays received by a single representative for a given area, its applicability to cases of overlapping political jurisdictions remains unanswered.

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