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Testing Models of Distributive Politics using Exit Polls to Measure Voters' Preferences and Partisanship*

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Abstract

This paper tests several hypotheses about distributive politics by studying the distribution of federal spending across U.S. states over the period 1978-2002. We improve on previous work by using survey data to measure the share of voters in each state that are Democrats, Republicans, and independents, or liberals, conservatives and moderates. We find no evidence that the allocation of federal spending to the states is distorted by strategic manipulation to win electoral support. States with many swing voters are not advantaged compared to states with more loyal voters, and "battleground states" are not advantaged compared to other states. Spending appears to have little or no effect on voters' choices, while partisanship and ideology have large effects.

Keywords: ideological attitudes, partisanship, distributive politics, federal budget

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1 Introduction

Distributive politics is a core issue in political economy, and scholars have developed a variety of models about how it works. In this paper we test three key hypotheses derived from these models, using data that have not previously been applied to this problem.

The first is the "swing voter" hypothesis, which predicts that politicians will allocate larger shares of distributive goods to interest groups or geographic areas that contain larger percentages of indifferent voters (who are indifferent between the political parties on ideological grounds). The second is the "electoral battleground" hypothesis, according to which distributive goods should be disproportionately allocated to electoral constituencies where the top two major parties have approximately equal numbers of supporters. This hypothesis is especially relevant in systems where two major parties compete in first-past-the-post elections with geographically defined constituencies. The third is the "partisan supporters" hypothesis, which conjectures that politicians will favor areas that contain a large percentage of their core supporters. They might do this in order to send clear signals to voters, induce higher turnout, or exploit informational advantages on policy preferences. For all three hypotheses, one underlying assumption is that politicians are mainly interested in winning elections and, for this purpose, they target government transfers or projects toward voters with *given* ideological attitudes or partisan leaning in order to attract their vote.

Testing these hypotheses is difficult. It requires measures of government spending across groups or geographic units of some sort (the dependent variable), as well as measures of the underlying partisan leanings or ideological attitudes of voters in each group or geographic unit (the key independent variables). The dependent variable is not too much of a problem, at least if one adopts the geographic approach. This is what all previous empirical studies do, using the distribution of spending across units such as districts, states, or provinces. Measuring the key independent variables, however, poses a severe challenge. Researchers do not have good measures of the underlying partisan leanings or ideological attitudes of voters within each geographic unit. As a result, all but one of the previous studies use proxy variables constructed from voting data or election outcomes. This is clearly problematic, however, since within models of distributive politics voting decisions are – by assumption – endogenous to the distribution of government funds.[1]

One important consequence of the endogeneity is that regression estimates of the effect of swing voters or electoral closeness on spending will often be biased toward zero. Overall, the pattern of estimates from existing studies is in fact quite mixed – some studies find statistically significant effects but many do not. However, we do not know whether the large number of insignificant coefficients reflects the fact that there is truly no relationship, or whether it is simply the result of the endogeneity bias. Another consequence of endogeneity is that estimates of the effect of core supporters for the governing party will often be biased upward. We demonstrate these biases more clearly in a simulation exercise discussed below.

In this paper we use *survey data* from exit polls, rather than voting data, to measure the party identification and ideological position of voters across constituencies. We then test the three hypotheses outlined above. The variables based on survey data (as we discuss in detail below) are likely to be more exogenous than variables based on votes. A second advantage of using survey data it that we can construct a *direct* measure of the fraction of "swing voters" in each geographic unit, since we have the fraction who call themselves "independents" (not attached to either major party) and "moderates" (not liberal or conservative). Previous studies have had to rely on proxy measures based on the variability of vote shares. The data are for U.S. states, and the period we study is 1978-2002. The dependent variables measure the distribution of federal spending across states.[2]

Our findings are easily summarized: We find little support for *any* of the three hypotheses listed above. We find no statistically significant support for either the swing voter hypothesis

[1] Most previous studies acknowledge this problem and tend to use lagged values of the vote to mitigate the problem somewhat, but this is at best a partial solution as we will discuss later.

[2] To our knowledge, only one previous study uses survey data for a similar purpose. Dahlberg and Johansson (2002) use the Swedish Election Study to construct a measure of the percentage of swing voters in Swedish regions. They then analyze a specific spending program of ecological grants.

or the electoral battleground hypothesis. We find mixed support for the partisan supporters hypothesis. In any event, the magnitude of estimated coefficients is tiny, implying that, even when an effect might be present, it would be small. Thus, the allocation of federal spending to the states does not appear to be distorted in a major way by strategic manipulation to win electoral support.

The use of survey data also allows us go further than previous studies, by testing the hypothesis that government spending affects voting behavior. We can estimate the impact that government spending in a geographic area has on the vote, using the survey-based measures of party identification and ideology as controls – in these analyses the dependent variable is vote choice and the independent variable of interest is the geographic distribution of federal spending. We find that spending has little or no effect on voters' choices, while (not surprisingly) partisanship and ideology have large effects.

Before proceeding we must discuss two important caveats regarding our measures. First, while it is likely that party identification and ideology are less affected by short-term forces than vote choices, party identification and ideology do change over time, both at the individual and aggregate levels, and may therefore be endogenous to federal spending. Second, since we rely on survey data – and our findings are largely null results – we must be especially attentive to measurement error.

Regarding the first concern, dozens of political science studies over more than fifty years argue that party identification is very stable over time, and much less affected by particular short-term electoral circumstances than vote choice. This idea goes back at least to *The American Voter* (1960). There, party identification is defined as a sense of personal, affective attachment to a political party based on feelings of closeness to social groups associated with the party. Green et al. (2002, p. 78) show that party affiliation is as stable as religious affiliation, and argue that "identification with the political party is analogous to identification with religious, class, or ethnic group." In other words, according to these scholars, party identification is more of an *identity* than an opinion. Similarly, Goren (2005) shows that partisan identity is even more stable than core political values such as principle of equal opportunity, limited government, traditional family values and moral tolerance. Moreover, he shows that past party identification has a significant impact on current political values while the reverse is not true. Ansolabehere et al. (2008) find that ideology is also quite stable, after correcting for measurement error.

Many scholars, however, are critical of the notion that party identification is affective. Party identification changes over time, and many studies find evidence that it changes in response to changes in identifiable factors. These factors range from changes in individual personal circumstances such as marriage, a new job, or change in neighborhood (e.g. Campbell et al., 1960), to more general forces such as the mobilization of new or previously disenfranchised voters (Campbell 1985; Carmines and Stimson 1989), the performance of the economy under a party (Fiorina, 1981; MacKuen et al., 1989), or major changes in the parties' issue positions. For example, Abramowitz and Saunders (1998) argue that the increased ideological polarization of the Democratic and Republican parties during the 1980s and 1990s generated a realignment of party loyalties along ideological lines, in part because the increased polarization made it easier for citizens to identify which party was the better match. As another example, the change in the parties' positions on racial issues was a major determinant of the realignment in the South (e.g. Valentino and Sears, 2005).

While the factors identified above are numerous, they are not likely to be problematic for our study for at least two reasons. First, these factors generally imply that aggregated party identification evolves slowly over time, while in most of our analyses we focus on short-term changes. Second, none of the factors are clearly related to the dependent variable of interest – geographically targeted federal spending.[3] Of course, we cannot rule out the possibility that some of the factors influencing the evolution of party identification might be indirectly related to the distribution of federal funds to the states, even in the short term. For example,

[3] This is even true for the most "economic" factor noted above – party identification as a running tally of past economic performance – since the tally is thought to be mainly about national economic performance rather than voters' individual circumstances.

although the empirical literature does not document this, it is possible that some voters respond to an increase in federal spending in their states by voting for the incumbent party, and that voting decisions influence voters' party identification (at least as measured in exit polls or other surveys, possibly in order to avoid cognitive dissonance), and therefore that party identification is influenced by the distribution of spending. What we argue, however, is that party identification is noticeably "more exogenous" than voting decisions. Thus, while the survey approach does not offer a definitive solution to the endogeneity problem, it is likely a step in the right direction.

Another potential concern with using survey or polling data is the underlying assumption that politicians use similar types of data – or at least highly correlated data – when making their decisions. Models of tactical spending are not explicit about the actual process through which political actors learn about voter preferences. Previous voting results, as well as polls and surveys, all provide important information. It is likely that rational politicians combine these various sources of information, rather than relying on a single measure. Polling data evidently looms large, however, as revealed by the large amounts of campaign spending devoted to collecting it. For example, in the 2008 U.S. Presidential election, the candidates spent $44.94 million on polling and survey research – this accounted for 20% of their overall campaign expenditure, making it the second largest item of campaign spending after campaign events (which accounted for 32.7%).[4]

A second concern, which is especially salient when using survey data, is measurement error. Survey research experts argue that measurement error varies considerably across items. Party identification appears to be relatively well measured, at least with respect to criteria such as reliability (inter-temporal stability in panels).[5] Other items, such as ideology, appear less reliable. However, while this may be a large problem for studies at the individual level, it is less of a problem for us since our focus is on state-level aggregates. We average over

[4] Source: *The Center for Responsive Politics*, http:www.opensecret.org.

[5] See, e.g., Converse (1964) and Green et a. (2002).

hundreds or thousands of individuals, so even if the measurement error at the individual level is large, the measurement error in the aggregated measures should be small.[6] We revisit this issue in more detail below.

2 Previous Literature

One of the dominant theories in political economy is the so-called "swing voter" hypothesis. This posits that the allocation of distributive goods will largely go in favor of groups or regions that contain a conspicuous share of voters that are ideologically indifferent between the political parties. While voters with a clear partisan leaning rarely switch their vote to a different party, indifferent voters often do. If voters trade off their ideological stances in exchange for public funds and projects, then it is cheaper for politicians to "buy" the votes of these indifferent, or swing, voters, and competition for these voters will lead politicians to allocate disproportionate amounts of federal spending to regions or groups with many indifferent voters. Lindbeck and Weibull (1987), Dixit and Londregan (1995, 1996), and Stromberg (2004) analyze models that capture this logic.

The logic of distributive politics is also affected by electoral rules. In particular, winner-take-all systems create incentives to target constituencies that are likely to be pivotal (Lizzeri and Persico, 2001; Persson and Tabellini, 2004). In other words, battleground districts may be favored both in public policy and campaign resources allocation (Snyder, 1989; Stromberg, 2008). The competitiveness of elections is particularly important in the U.S. context, where the electoral college system may induce the channeling of resources toward states that are pivotal in the presidential electoral race.

A competing theory of distributive politics is that parties target spending toward loyal voters (Kramer, 1964; Cox and McCubbins, 1986; Dixit and Londregan, 1996; Sim, 2002; Arulampalam, et al., 2009). This can be a rational strategy in the context of low-turnout elections such as those in the U.S. If spending primarily mobilizes voters – either directly as a

[6] See Page and Shapiro (1992) and Stimson (1998).

form of advertising or retrospective voting, or indirectly by buying the support of local elites or groups who engage in get-out-the vote efforts – then the marginal benefit to spending an additional dollar will be highest in areas with the highest density of a party's own voters. Credit-claiming issues may also provide incentives to target core areas. Who will attend the ribbon-cutting ceremonies for new bridges, schools, hospitals, and libraries? In a heavily Democratic area the politicians will almost all be Democrats, and they will leave no doubt about which party is responsible for the locality's good fortune. In electorally marginal areas, however, roughly half of the politicians will be Democrats and half will be Republicans, and the impression is not likely to be so partisan or clear. Neither party may benefit much in terms of net votes (although individual politicians, running as incumbents, may benefit).

It is also possible that spending targeted towards loyal voters could simply reflect the fact that politicians are, at least to some extent, policy oriented.[7] Democratic politicians may prefer spending on policies that tend to benefit Democratic voters, and likewise for Republicans. These alternate models are not necessarily incompatible with the swing voter hypothesis. For example, loyalists of the out-party may receive disproportionately small shares of the public dollar, while swing areas and loyal areas do equally well.

Finally, other theorists emphasize the importance of factors such as proposal power (Baron and Ferejohn, 1989), legislative seniority (McKelvey and Riezman, 1992), over- and under-representation (Ansolabehere et al., 2003; Knight, 2005), committee structure, presidential leadership, and universalism (Weingast et al., 1981; McCarty, 2000). If factors such as these are the main drivers of distributive spending, then there may be little relationship between spending and partisanship or ideology.

There are many empirical studies of distributive politics, and the findings are mixed. In terms of the swing-voter hypothesis, studies of the allocation of New Deal spending have found some evidence that states with a more volatile presidential vote received more

[7]See, for example, the citizen-candidates models of Osborne and Slivinski (1996) and Besley and Coate (1997).

federal support (Wright, 1974; Wallis, 1987, 1996; Fleck, 1999; Fishback, et al., 2003). However, Stromberg (2004) shows that these findings are not robust to the use of panel data methods with state fixed effects. Similarly, in a more recent study on federal budget allocation by contemporary presidents, Larcinese, et al. (2006) find that states with more frequent presidential vote swings do not receive more funds. All of these studies use lagged presidential vote returns to measure the fraction of swing voters.

So far, there is little support for the battleground hypothesis, at least with respect to public spending. Wright (1974) finds that U.S. states with close presidential races do not receive disproportionately more New Deal spending. Similarly, Larcinese, et al. (2006) find no evidence that states with close presidential races receive more federal monies. On the other hand, several studies find that battleground states receive a disproportionate share of the advertising in presidential campaigns (Colantoni, et al., 1975; Nagler and Leighley, 1990; Stromberg, 2008). All of these studies use lagged presidential vote returns to measure the two-party balance in each state.

Several studies find evidence that loyal voters are rewarded. Some studies find a positive relationship between the share of federal spending going to an area and the Democratic vote in the area.[8] Since Democrats were the majority party in Congress during the years studied, this supports the idea that federal spoils go to the victors. However, the results might also reflect the behavior of the Democratic party or characteristics of areas that tend to vote Democratic.[9] Some studies of U.S. states find a positive relationship between federal spending and past vote for the incumbent president's party (Garrett and Sobel, 2003; Larcinese, et al., 2006).[10]

[8] See, e.g., Browning (1973); Ritt (1976), Owens and Wade (1984), and Levitt and Snyder (1995).

[9] Levitt and Snyder (1995) compare programs passed during years of unified Democratic control with programs passed during years of divided government. They find that programs passed during unified Democratic control exhibit a pro-Democratic geographic bias, while those passed during divided government do not. Levitt and Poterba (1999) also find indirect evidence that the majority party favors its core areas: areas represented by more senior Democrats tend to get more.

[10] Ansolabehere and Snyder (2006), analyzing the distribution intergovernmental transfers, find that counties that traditionally give the highest vote share to the State governing party receive larger shares of transfers. Studies of the distribution of patronage by urban machines also find that the organizations in

The empirical literature finds more support for "swing voter" behavior outside the U.S. Arulampalam et al. (2009) find that Indian states that are "swing" but also aligned with the governing parties receive larger shares of public grants. Dahlberg and Johansen (2002) find evidence that the more pivotal regions (of 20) in Sweden were more successful in winning environmental grants from the central government. Crampton (2004) finds a positive correlation between competitiveness of the race and spending in Canadian provinces that are not ruled by the liberal party. Milligan and Smart (2005) also study Canada, and find that closeness of the electoral race has a positive effect on spending, at least for seats held by the opposition party. Ward and John (1999) find evidence that central government aid to local governments in the U.K. goes disproportionately to marginal districts. Case (2001) finds that during the Berisha administration in Albania block grants tended to be targeted at swing communes. Denemark (2000) also finds evidence that marginal seats in Australia receive a disproportionate amount of local community sports grants.[11]

3 Problems with measures of attitudes and partisanship based on voting data: a simulation

As noted above, almost all of the existing empirical literature uses voting data to measure the percentage of swing voters, partisan balance, or the partisan disposition of each state.

One powerful critique of these measures is that voting behavior is endogenous. Most papers tend to use lagged values of the vote to mitigate the problem, but this is at best a partial solution for several reasons. The first has to do with the relationship between voting and policies. Voters can reward or punish politicians on the basis of their past allocations of the budget (retrospective voting) or on the basis of their promises about future allocations (prospective voting). Prospective voting is rational in an environment

control of their cities tend to reward their core supporters with patronage (Holden, 1973; Rakove, 1975; Erie, 1978; Johnston, 1979).

[11] Other studies find evidence more consistent with the loyal voters hypothesis. See, e.g., Sole′-Olle′ and Sorribas-Navarro (2008).

where politicians keep their pledges. However, in this setting, lagged votes are a function of past promises, which by assumption should be equal to – or at least highly correlated with – current spending. If this occurs, then measures based on past voting are not a satisfactory solution to the endogeneity problem. Even if voters are retrospective – so that past votes should not be automatically correlated with current spending – using lagged votes is potentially problematic. Budgetary allocations are quite persistent over time, because budgetary processes are sluggish, and spending in any given year depends to a large extent on decisions made in previous years. As a result, even in the case of retrospective voting behaviour, lagged votes and current spending are related because of the strong correlation between past and current budgetary allocations. Finally, there is a third reason to suspect that lagged vote measures are not exogenous: omitted variables that are correlated both with voting and budgetary decisions. For example, some groups might be favored in distributive policies because they are associated with "good values" that citizens wish to preserve (e.g., farmers), and these groups might vote in particular ways (e.g., they might favor conservative parties). The introduction of state fixed effects in panel regressions can deal with this problem when omitted factors are constant over time. Many potential omitted factors, however, are not time-invariant. For example, changes in economic conditions, occupational structure, health outcomes, the cost of supplying various public goods, or the flow of immigrants, can simultaneously affect both political preferences and spending. In some cases we can simply measure these variables, but often measures are unavailable or noisy.[12]

Since the measures used by the current literature to test concurrent theories of distributive politics are clearly endogenous under a variety of assumptions, regression estimates that use them are typically biased. The sign and magnitude of the bias, however, are more difficult to determine. In the simplest cases we can compute the expected bias analytically, but most regressions in the literature are fairly complicated, and typically include two or more

[12] For example, officially measured unemployment figures, do not count discouraged workers who are outside the working force; official immigration figures do not include undocumented aliens.

vote-based measures in the same model. In such cases it is often quite difficult to calculate the signs and relative magnitudes of the biases analytically. We therefore ran a series of simulated regressions. These allow us to gauge the biases in a set of models that are similar to many of the standard models in the literature.[13]

The simulations show that the endogeneity of voting data can lead to severely biased estimates. More specifically, using the standard deviation of observed votes rather than the true number of independents can lead either to overestimation or underestimation of the impact of the number of independents on the allocation of federal spending, depending on the specification and the set of variables included in the regression. The effect of an electoral competition is often underestimated but sometimes also overestimated. Finally, using the observed votes to measure the partisanship of a region leads to systematic overestimation of the impact of the number of partisan voters on spending.

We consider the following basic structure. Let $j = 1, ..., J$ index states, and let $t = 1, ..., T$ index years. Assume all states have the same population. Let D_j be the fraction of voters in state j who are loyal to party **D**, let R_j be the fraction who are loyal to party **R**, and let I_j be the fraction who are independents (swing voters). Also, let $\tilde{D}_j = D_j/(D_j+R_j)$ be the fraction of all loyalists who are loyal to party **D**, and let $\tilde{R}_j = R_j/(D_j+R_j) = 1-\tilde{D}_j$. Let $\tilde{C}_j = 1 - |\tilde{D}_j - \tilde{R}_j|$ be the two-party "competitiveness," or partisan balance, of state j. Let X^D_{jt} be the per-capita transfers that party D offers to state j and year t, and let X^R_{jt} be the offer made by party R. Let S^D_{jt} be the "electoral support" party D receives in state j in year t, and let S^R_{jt} be the support received by party R. Finally, let $\tilde{V}^D_{jt}$ be the fraction of votes

[13] Researchers, including ourselves, are often less than fully satisfied with the results from simulation exercises, when these results do not provide a clear intuition. This is not a weakness of simulations per se, but a "weakness" of complicated models.

party D receives in state j in year t, and let $\tilde{V}^R_{jt} = 1 - \tilde{V}^D_{jt}$. We assume:

$$X^D_{jt} = \alpha_I I_j + \alpha_C \tilde{C}_j + \alpha_P \tilde{D}_j + \mu^D_{jt} \quad (1)$$

$$X^R_{jt} = \alpha_I I_j + \alpha_C \tilde{C}_j + \alpha_P \tilde{R}_j + \mu^R_{jt} \quad (2)$$

$$S^D_{jt} = \beta_I X^D_{jt} I_j + (1 + \beta_P X^D_{jt}) D_j + \epsilon^D_{jt} \quad (3)$$

$$S^R_{jt} = \beta_I X^R_{jt} I_j + (1 + \beta_P X^R_{jt}) R_j + \epsilon^R_{jt} \quad (4)$$

$$\tilde{V}^D_{jt} = S^D_{jt} / (S^D_{jt} + S^R_{jt}) \quad (5)$$

If $\alpha_I > 0$, $\beta_I > 0$ and $\alpha_C = \alpha_P = \beta_P = 0$, then we have a linearized approximation of the "swing voter" model of Lindbeck and Weibull (1987) and Dixit and Londregan (1995, 1996). If $\alpha_P > 0$, $\beta_P > 0$, $\alpha_I \geq 0$, $\beta_I \geq 0$, and $\alpha_C = 0$, then we have a version of the "machine politics" model of Dixit and Londregran (1996) or the model of Cox and McCubbins (1986), or what Fishbeck, et al. (2003) call the mandate model. Finally, if $\alpha_C > 0$, $\alpha_I \geq 0$, $\beta_I \geq 0$, $\beta_P \geq 0$, and $\alpha_P = 0$, then we have something approximating the model of Milligan and Smart (2005), or the electoral college model of Colantoni, et al., (1975), Stromberg (2008) and others.[14]

If researchers had direct measures of I_j, D_j and R_j, then they could construct $\tilde{C}_j$, $\tilde{D}_j$ and $\tilde{R}_j$, and then directly estimate equations (1) and (2). In almost all cases, however, they do not. Instead, they use measures based on the actual vote shares, $\tilde{V}^D$. Beginning with Wright (1974), researchers have often used the standard deviation of $\tilde{V}^D$ over a set of elections within each state j as a proxy for I_j. Intuitively, if I_j is large then $\tilde{V}^D$ will vary widely across elections in state j, and the standard deviation of $\tilde{V}^D$ in state j will be large.[15] Researchers also tend to use some historical average of $\tilde{V}^D$ as a proxy for $\tilde{D}_j$, and an analogous average as a proxy for $\tilde{R}_j$. Finally, researchers usually use some historical average of $-|\tilde{V}^D - \tilde{V}^R|$ as a proxy for $\tilde{C}_j$.

[14] This formulation does not do justice to some of these models, such as Stromberg (2008), which takes into account the total probability a state is "pivotal" in the electoral college.

[15] Trending partisanship could also produce a large standard deviation of $\tilde{V}^D$, which is a potential problem.

As noted above, there are many reasons why even historical voting measures are not exogenous: (i) rational prospective voting , (ii) sluggish budgetary process and (iii) omitted variables that are correlated both with voting patterns and budgetary decisions. Although these three mechanisms are different, they have the *same* implication: patterns of current votes and current spending are interdependent. In the first case, the relationship between contemporaneous vote and spending is driven by the link between past promises and current allocations. In the other two, it is due to the correlation of current spending either with past spending (because of inertia) or with an omitted variable correlated with the vote. Rather than constructing complicated historical averages and autocorrelation structures that attempt to incorporate these features more precisely, we simply analyze the effect of the interdependence between vote and spending using contemporaneous voting data freely in our simulations. Let $\bar{V}_j^D = (1/T)\sum_{t=1}^{T} \tilde{V}_{jt}^D$ be the mean of V^D in state j over a sample of T years, and let $\hat{I}_j = [(1/T)\sum_{t=1}^{T}(\tilde{V}_{jt}^D - \bar{V}_j^D)^2]^{1/2}$ be the sample standard deviation. Also, let $\hat{C}_{jt} = 1 - |\tilde{V}_{jt}^D - \tilde{V}_{jt}^R|$ be the closeness of the election in state j in year t.

We consider the following specifications:

$$\text{Model 1a} \quad : \quad X_{jt}^D = a_I \hat{I}_j + \mu_{jt}$$

$$\text{Model 1b} \quad : \quad X_{jt}^D = a_C \hat{C}_{jt} + \mu_{jt}$$

$$\text{Model 1c} \quad : \quad X_{jt}^D = a_P \tilde{V}_{jt}^D + \mu_{jt}$$

$$\text{Model 2a} \quad : \quad X_{jt}^D = a_I \hat{I}_j + a_C \hat{C}_{jt} + \mu_{jt}$$

$$\text{Model 2b} \quad : \quad X_{jt}^D = a_I \hat{I}_j + a_P \tilde{V}_{jt}^D + \mu_{jt}$$

$$\text{Model 2c} \quad : \quad X_{jt}^D = a_C \hat{C}_{jt} + a_P \tilde{V}_{jt}^D + \mu_{jt}$$

$$\text{Model 3} \quad : \quad X_{jt}^D = a_I \hat{I}_j + a_C \hat{C}_{jt} + a_P \tilde{V}_{jt}^D + \mu_{jt}$$

$$\text{Model 4} \quad : \quad X_{jt}^D = a_I I_j + a_C \hat{C}_{jt} + a_P \tilde{V}_{jt}^D + \mu_{jt}$$

We only analyze party **D**, since analogous specifications for party **R** would simply duplicate the results. Note that in Model 4 we use the *actual* value of I_j rather than the vote-based measure. This approximates the "encompassing models" in Dahlberg and Johansson (2002), which include a survey-based measure of I, but a vote-based measure of V_D.

In each simulation, we set $J=50$ and $T=100$, i.e. 50 states over 100 years. Note that this gives much more data on the time dimension than researchers actually have. We do this to focus attention more on the bias produced by endogeneity than on measurement error bias (which also plagues the literature). In all cases, I, D, and R are drawn from independent uniform distributions on $[0, 1]$. Also, in each simulation, I, D, and R are fixed for all 100 years (i.e., all $t = 1, ..., 100$). Next, we choose values for the parameters α_I, α_C, α_P, β_I, and β_C. Finally, we draw μ^D, μ^R, ϵ^D and ϵ^R from independent uniform distributions. We set the standard deviations of μ^D and μ^R to σ_μ, and the standard deviations of ϵ^D and ϵ^R to σ_ϵ.

We focus on four different cases. In Case 1 and Case 2 there is no partisan targeting, that is, $\alpha_P = 0$. In addition, we assume there is no partisan voter response to transfers, that is, $\beta_P = 0$. The difference between the two cases is the value of σ_μ, the degree to which the distribution of transfers across states is determined by random, idiosyncratic factors. In Case 1, $\sigma_\mu = .2$, so the idiosyncratic factors are relatively important. In Case 2, $\sigma_\mu = .03$, so the idiosyncratic factors are less important. In Case 3 and Case 4 there is partisan targeting, with $\alpha_P = .5$. We also assume there is a partisan voter response, with $\beta_P = .5$. The difference between the two cases is again the value of σ_μ, with $\sigma_\mu = .2$ in Case 3 and $\sigma_\mu = .03$ in Case 4. Inside each case, we vary the parameters α_I and α_C. We fix $\beta_I = 1$ and $\sigma_\epsilon = .09$ throughout the simulations.

For each vector of parameters we run 10,000 simulated regressions. Table 1 presents the results. The Table presents four panels, each corresponding to one of the fours cases above. Within each case, the rows correspond to different values for α_I and α_C. The true values are reported in the first two columns. The remaining columns report the averages of the estimates of the parameters of interest for various models ($\widehat{\alpha}_I$, $\widehat{\alpha}_C$, $\widehat{\alpha}_C$).[16] To give an

[16] Rather than reporting all possible specifications, we focus on $\widehat{\alpha}_I$ and $\widehat{\alpha}_C$ in cases 1 and 2, and on $\widehat{\alpha}_P$ in cases 3 and 4. However, we always report the results for the case where all variables are included. We also ran simulations that incorporate measurement error in the "direct" measure of voters' partisanship, i.e.

example, if we take model 2a, the first row gives the average estimates of, respectively, α_I (.01) and α_C (.09) when the true values of these parameters are both equal to 0.

We observe a number of patterns. First, in most cases the average estimates of a_P are biased upward. That is, there is a strong tendency to find "partisan targeting" predicted by the mandate model or machine politics model, even when it does not exist. The effect is large when idiosyncratic factors have a large impact on transfers. This is a direct result of the assumption that independent voters respond to transfers in their voting behavior. When one party happens to spend more than the other party in a state – whether due to the exogenous factors captured in μ^D and μ^R, or to actual partisan targeting – then many independent voters will vote for that party, producing a spurious additional correlation between transfers and votes.

Second, the average estimates of a_I tend to be biased downward, but are sometimes biased upward. They can even have the wrong sign: this appears to be especially the case when σ_μ is low and a_I is high. The average estimates of a_I are not even monotonic in the true value of α_I, as we can see in the models 2b and 3 of case 3.

Also, the average estimates of a_I are often biased even when the true I_j are used (model 4): this is because the other vote-based measures are endogenous and may be correlated with I_j. In fact, the bias on a_I can be even larger using the true I_j: this is especially the case when the true I_j is low.

Third, the average estimates of a_C are sometimes biased downward and sometimes biased upward. When σ_μ is low the coefficient is generally underestimated, while if σ_μ is high then the coefficient can be biased both upwards and downwards depending on the specification.

The difficulty in recovering the true parameters is well illustrated if we consider model 3, which is similar to many specifications used in the empirical literature. Here when σ_μ is

in the share of independent, I_j. In these simulations the estimated coefficient on the term measured with error, i.e. $\hat{a}_I$, is biased toward zero. This is the usual attenuation bias associated with regressors that are measured with error. The other coefficients are almost unaffected, however. Results of these simulations are in Appendix Table A.1.

high (cases 1 and 3) the estimate of α_P is systematically and substantially upward biased. If instead σ_μ is low (cases 2 and 4), then we obtain a much more precise estimate of α_P. This comes at the cost, however, of a deterioration in the estimates of α_I. In fact, there appears to be a trade-off between the consistency of $\widehat{\alpha}_P$ and the consistency of $\widehat{\alpha}_I$. The intuition is straightforward. As noted above, a large degree of random variation in the allocation of spending induces more support to be directed at partisans simply by voters' reaction to the spending. Many independents therefore act as if they are partisans, generating a spurious positive correlation between observed votes and observed spending. At the same time, however, a more random allocation of funds facilitates the identification of the electoral response to spending. Since independent voters respond to spending, random variation in the allocation of funds will produce large fluctuations in their voting behavior. The standard deviation of the vote is then a relatively good measure of the proportion of independent voters. In fact, this means that we encounter a type of contradiction: the swing voter hypothesis is testable using voting data to measure the number of swing voters only insofar as it is false, i.e. only insofar as funds are randomly allocated rather than targeted to independent voters.

4 The data

We analyze U.S. federal budget allocation to the states during the period from 1978 to 2002. We consider three dependent variables: (1) total federal spending per-capita, (2) total spending other than direct transfers to individuals, per-capita, and (3) federal grants per-capita. The second variable should allow us to isolate the most manipulable items in the budget, since it removes the largest of the "non-discretionary" or "entitlement" programs, such as Social Security, Medicare, pensions for public officials, AFDC (TANF), etc.[17] The third variable is arguably the most targetable; and while it is much smaller than (1) or (2) it still constitutes an important part of state finances. In all cases, our dependent variables

[17]Interest on the debt is not included in any of the dependent variables.

are outlays.

It is important to consider that there is a lag between the appropriation and the spending of federal funds. This is relevant when estimating the effect of particular institutional and political variables, since current federal outlays have normally been appropriated in previous calendar years. For this reason, we will always consider lagged values of the political explanatory variables.

As noted above, one of the main independent variables of interest is the percentage of swing voters in a state. We use poll data to measure the share of "independents" (and also the share of Democrats and Republicans). This data comes from exit polls conducted by various news organizations – CBS News, CBS News/New York Times, ABC News, ABC News/Washington Post, and Voter News Service.[18] Voters are surveyed briefly after leaving the polling booth, and asked how they voted. They are also asked to provide their party identification (Democrat, Republican, other, or independent), and their ideological leaning (liberal, conservative, moderate, or don't know).[19] Importantly, these questions are designed to tap into voters' general self-identification, rather than how the voters have just voted. Two typical forms of the party identification question are: "Regardless of how you voted today, do you normally think of yourself as a [Democrat], [Republican], [Independent], [Something Else]?"; and "Do you normally think of yourself as a [Democrat], [Republican], [Independent]?" Two common forms of the ideology question are: "On most political matters, do you consider yourself [liberal], [moderate], [conservative]?"; and "Regardless of the party you may favor, do you lean more toward the liberal side or the conservative side politically [liberal], [conservative], [somewhere in between]?"[20]

[18] Voter News Service is an association of ABC News, CNN, CBS News, FOX News, NBC News and the Associated Press.

[19] In addition, voters are asked a series of questions about their demographic and socio-economic characteristics, questions about the reasons for their vote choice, and, sometimes, questions about salient policy issues.

[20] One possible alternative, at least for partisanship, is to use party registration data. However, this would sharply reduce the sample of states (probably in a non-random way) since only 29 states have party registration.

Using this information we can construct state-level variables reporting the percentage of voters that declare themselves Democratic, Republican or Independent. Due to the relatively small number of respondents in some states in some years, we aggregate the results over four-year periods (two elections). We also drop any cases with fewer than 100 respondents. This yields a sample size of 1,174 state-years for our analysis of totals spending and grant spending, which is 2.1% smaller than the maximum possible size. In the resulting sample, the average number of respondents per state is about 3,700 and the median is about 3,300. Almost 87% of the cases have more than 1,000 respondents, and only 1% of the cases have fewer than 250. We assess the reliability of these variables with respect to exogeneity and measurement error problems in section 4.1 below.

4.1 Endogeneity and measurement error in survey data

One concern is how well survey data can capture the distribution of partisanship within states. This issue is discussed extensively in Erikson, et al. (1993), who conclude that the partisanship measures derived from the surveys correlate in the expected way with other observable measures, including other polls, election returns, and party registration. We present some of our own checks below, and the results make us confident that these data capture the underlying distribution of partisanship by state relatively well and that they are preferable to using simple voting results.

Figure 1 plots the share of Democratic vote by state (averaged across all years) on the share of Democratic partisans in the survey data. Figure 2 does the same for Republicans. There is a clear positive correlation between votes and partisanship, especially for the Republican party. Although our purpose is to go beyond what can be captured by voting data, the correlation between the exit poll measures and observed votes is reassuring and suggests that our measure can be taken as a reliable indicator of partisanship. Of course, actual votes also include non-partisans and final election results are crucially affected by the leaning, in a particular election, of independent voters. Hence, Figure 3 reports the aggregate Democratic share of votes at presidential elections and the share of Democratic supporters from exit polls: it clearly shows that partisanship is much more stable than what electoral results would suggest and that using voting to measure partisanship can therefore be problematic.[21] In Figure 4 we report the standard deviation (over the period we consider) of presidential Democratic votes by state and compare with the standard deviation of party identification: again, this figure suggests that partisanship is much less volatile than voting. Hence, the exit poll data confirm the stable pattern of party identity variables found by other studies, supporting the notion of party identity as a long term stable personal characteristic as opposed to the variable pattern of voting data.[22]

Although party identification is more stable over time than vote choice, it is not perfectly stable. It is likely that some of the observed instability represents real changes in respondents' partisan loyalties. Some of the instability, however, might indicate unreliability in the measure, which is known to be a significant problem in surveys (e.g., Zaller, 1992). For example, a desire to avoid cognitive dissonance could induce voters to align their party identification response to the party for which they voted most recently.[23] For such voters, party identification is equivalent to voting data and therefore equally endogenous. Table 2 provides some information about the possible magnitude of the problem in the exit poll data. The figures in the table show that, although there is a substantial overlap between party identification and reported voting choices, almost one in four voters declares herself an independent in spite of having voted for one of the two major parties. More than 28% of respondents reporting having voted Democratic do not report a Democratic party identification. The percentage is 36% for those voting Republican. Importantly, an overwhelming majority of self-declared Independents vote for one the two major parties rather than for a third party or an independent candidate. When we aggregate party identification at the state level, we find again an overall positive correlation between voting results and party

[21] This is consistent with Green, et al. (2002).

[22] This is consistent with the findings of Green et al. (2002) and Goren (2005).

[23] See for example Mullainathan and Washington (2009).

identification in the states. This correlation is 0.31 for the Democratic party and 0.53 for the Republican party. Hence, although, the correlations between voting choices and party identification are positive, they are hardly overwhelming. The "slack" indicates that party identification is not simply another measure of vote choice. Of course, we cannot entirely rule out cases of positive dissonance and other survey-related problems. It is indeed reasonable to presume that survey responses are not, for various reasons, entirely accurate. Our claim is more modest: using party identification constitutes a movement in the right direction, and therefore improves on existing studies.

Examining split-ticket voting for different groups of identifiers helps us assess the degree to which party identification captures the relative degree of "independence" in the vote choices of self-identified independents and partisans. Data from the American National Election Study (ANES) are revealing. Define a ticket-splitter as a respondent who voted for at least one Democrat *and* at least one Republican, in the elections for President, U.S. House, and U.S. Senate held during the year of the survey.[24] For those respondents who voted in at least two races during the year of the survey, 42% of pure independents, 32% of leaning independents, and 22% of self-identified Democrats and Republicans were ticket-splitters. These figures are similar to those in exit polls. In the exit poll data we have respondents' vote choices for President, U.S. House, U.S. Senate, and Governor. For those respondents who voted in at least two races, 33% of independents (including leaners) and 22% of partisans split their tickets in some fashion. Panel data allows us to use the initial party identification for each respondent, and thereby mitigate the possibility of reverse-causation (ticket-splitters tending to self-identify as independents, and those casting straight tickets self-identifying as partisans). We examine the 1992-1994-1996 panel, using party identification in 1992. Define a ticket-splitter as a respondent who voted for at least one Democrat *and* at least one Republican, in the elections for President, U.S. House, and U.S. Senate held during 1992,

[24] Of course, not all respondents have a Senate race in which to vote, and in midterm years no respondents have a presidential race in which to vote.

1994 and 1996.[25] For those respondents who cast votes in at least 4 of the 6 or 7 possible races, the percentages of ticket-splitters are as follows: 75% of pure independents: 52% of leaning independents, and 44% of partisans exhibited at least one instance of a split.

Thus, while party identification is not a perfect measure, it is quite stable over time and captures "independence" in voting to a considerable extent.

4.2 Testing distributive politics hypotheses using survey data

One key prediction of the swing voter hypothesis is that states that have more Independents should receive more federal funds. The alternative theories of distributive politics conjecture that the competitiveness of elections and the share of loyal voters may also affect the distribution of federal funds to the states. Thus, we will test these predictions by using measures of the share of independents, of electoral closeness and of loyal voters that, differently from previous work, are not based on actual voting data but on survey data. Indicating with *Dem*, *Rep*, and *Ind*, respectively the share of Democrats, Republicans and Independents, we use *Ind* to measure the share of independents and $(1 - |Dem - Rep|)$ to measure closeness.

We tried other measures of partisan and independent voters as well. Some voters may be "cross-pressured," in the sense that they identify themselves with a party that is not the closest on the ideological dimension. This is the case for liberal Republicans (not uncommon in the northeast) and conservative Democrats (still somewhat common in the south and west). Such voters are probably more prone to defect in any given election. Thus, we considered an alternative measure of independent voters, in which cross-pressured voters are included with the self-identified independents. In this specification, partisan Democratic voters will therefore only be either liberal or moderate, while Republicans will only be either conservative or moderate. The substantive conclusions do not change when we use these variables, so we do not report the results.[26]

[25] Each respondent could vote in 6 or 7 races – 2 presidential races and 3 House races, and either 1 or 2 Senate races.

[26] Results are available from the authors upon request.

As discussed in the introduction, swing voter models predict that states with a higher share of partisan and/or ideological voters should receive less funds, while the opposite is predicted by models that stress the importance of loyal voters. If legislators reward their supporters, we should observe that incumbents divert money toward states with high shares of voters ideologically leaning toward the incumbent legislator. In the U.S. institutional setting the incumbent is never a unitary actor since federal budget allocation involves both Congress and the President. Therefore, we construct different measures of partisanship by interacting the party affiliation of various actors with the shares of voters that declare to have the same party affiliation of the actor under consideration. To evaluate whether the president favors his supporters we use the variable *Presidential Copartisans*, which is equal to the share of Democratic voters when the incumbent president is a Democrat and the share of Republican voters when the president is Republican.[27]

In addition to political considerations, a variety of demographic factors might directly affect federal spending. Thus, in all regressions we include per-capita income, percent of elderly, percent of population in schooling age, total state population unemployment and dummy variable equal one for state-years in which a natural disaster occurred.[28] [29] Moreover, it is clear that the two states bordering the District of Columbia – Maryland and Virginia – receive more funds simply because of the spill over of federal government activities. A similar case can be made for New Mexico because of the long term investments in military spending. Thus, in the cross section regressions we always include dummy variables for these

[27]We constructed analogous variables using the party affiliation of the majority in the house (House Majority Copartisans) and senate (Senate Majority Copartisans) as well as the political affiliation of state senators (Senator Copartisans). The results are substantively the same as those obtained in the case of president affiliation. We do not report them here but they are available from the authors upon request.

[28]When we use presidential term as the time unit, instead of a dummy for natural disasters we include the share of the term containing years in which a natural disasters occurred: possible values are therefore 0, 0.25, 0.5, 0.75 and 1.

[29]The total population size captures the effects of malapportionment of the U.S. Senate, as small states are extremely over-represented. It may, however, also capture budgetary lags. Because of "incremental budgeting," the growth of the population is likely to negatively affect the levels of expenditure per capita. If there are lags in adjusting the allocation of transfers to population shifts, then, as a state population grows its per-capita transfers will automatically fall. Economies of scale might also lead to a negative effect of population on per-capita transfers.

three states.[30]

The sources for all variables used in our analysis are reported in the appendix.

5 Results

The simulation exercise shows that regressions based on voting data can be substantially biased. By using more exogenous measures based on exit polls, we should be able to obtain less biased estimates. It is therefore important to compare the results in the two cases to verify whether we obtain different estimates. We can then use the simulation exercise as a benchmark to evaluate the potential bias in estimated coefficients.

The key test of the swing voter model is whether the coefficients on the share of independents is positive. We compare, therefore, the results obtained when the share of independents from the exit polls is used as an explanatory variable with the results obtained when observed votes are used. In this case we use the standard deviation of Democratic vote in the previous three presidential elections. The "battleground state" hypothesis stresses the role of the state marginality: thus, we also estimate regressions with closeness as explanatory variable for spending. Results when the competitiveness of electoral races is measured using exit polls can then be compared with regressions when closeness is measured by using voting data. Finally, we test the alternative possibility that loyal voters get more funds. Again, we compare results when the share of votes for the incumbent president is used as explanatory variable with results when exit polls partisan measures are used instead.

To check the robustness of our results we consider several possible variants of these basic models. We first consider specifications in which swing, pivotality and partisan measures are all included in the same regression. Since swing, pivotality, and partisanship are somewhat correlated, and since the various hypotheses regarding these variables are not logically incompatible with each other, specifications that include only one variable at a time might

[30] We do not include variables to measure committee positions or seniority. Previous studies have found little or no evidence that these variables are important determinants of aggregate spending in states or districts (e.g., Owens and Wade (1984), Ritt (1976), Levitt and Snyder (1985)).

suffer from omitted variable bias. We also consider the possibility that the share of swing voters and the closeness could have a positive interaction. There is also the possibility that registration and primary laws induce people to register as independents, which may then lead them to define themselves independents in surveys. In particular, Massachusetts and Rhode Island allow citizens registered as independents to vote in either major party primary (they simply choose on election day), while those registered with a party can only vote in that party's primary. This gives an incentive to register as an independent. Therefore all regressions have been repeated by excluding those two states. We noticed very limited variations in the results (not reported).[31].

The economic data are annual, but voting data are not available for years when there was no election. For these years we use the data from the closest previous election. This can generate autocorrelation in the residuals with the potential problems this generates for standard errors estimates. Hence, in addition to using state-level clustered standard errors, we also run term-based regressions, in which each presidential term is collapsed into one observation and the spending and other control variables are averaged over the period.

Since we consider a large number of specifications, we only report the coefficients of our variables of interest in the main text.[32] These are reported in Table 3. We should point out that for the standard control variables, we do not find any significant surprises or noticeable differences across the various specifications. The percentage of the population who are elderly has a positive and significant effect on total federal outlays, while the percentage who are school-age children has a negative significant impact. The coefficient of population (in logarithm) is negative and significant in most specifications, while the coefficient of income per capita is negative and significant only when fixed effects are introduced.[33]

[31] Results are available from the authors upon request.

[32] Detailed results are available at http://personal.lse.ac.uk/LARCINES/LSTapril2010full.pdf.

[33] Another concern is that federal expediture could be spatially autocorrelated. To deal with this possibility we have included Census division dummies and division-specific trends in the specifications that do not include state fixed effects. When state fixed effects are included we only add division-specific trends. These modifications change only marginally our results and, in the interest of space, we do not include the tables in the paper. Results are available in the online Appendix.

5.1 Share of swing voters

The key test of the swing voter hypothesis consists in verifying whether the relationship between the share of independents and spending is positive. We begin with a simple scatter-plot of the collapsed data, averaged over the period 1978-2002. This is shown in figure 5. In each of the four graphs, the y-axis is average federal spending other than direct transfers. The x- axis measures the share of swing voters, and we do this four different ways. In figure 5(a), we use the average share of voters who identify themselves as moderates; in 5(b) we use the share who identify themselves as independents, in 5(c) we use the share who identify themselves both as moderate and independent, and in 5(d) we use the share who identify themselves both as moderate and independent or who are cross-pressured (voters who are liberal and Republican or conservative and Democratic). Each graph also shows a line of the predicted values from a bivariate regression of spending on the corresponding x-variable. Evidently, the relationships are all pretty weak – none of the estimated slope coefficients are significant even at the 20 percent level. We can do a bit better by dropping the three states which are outliers in terms of average spending – Maryland, New Mexico, and Virginia – or by including a dummy variable for these states. In this case the relationship between federal spending and the share of swing voters becomes statistically significant at the 10 percent level for the measure used in figure 5(c), but not for the other three measures.

Table 3 presents the main results. There we report estimates of the main coefficient of interest from model 1 (with exit poll measures) and model 2 (with voting measures), and model 7 (with other political variables from exit polls also included) and model 8 (with other political variables from voting data also included). We find little evidence that states with a larger share of independent voters receive more funds. This result is robust across various specifications, i.e. whether we use yearly or term data, whether we include or not state fixed effects and whether we use federal expenditure, targetable spending or grants as our dependent variable.

The coefficients in Table 3 are not only statistically insignificant, but they are also substantively small. Consider, for example, model 7 for grants. The point estimate implies that a one percentage point increase in the share of independents in a state increases grants by about \$2.80 per capita. The standard deviation of the share of independents within state is about 4%, so a one-standard deviation increase in the share of independents in a state increases grants by about \$11.20. Since the average amount of grants per capita is about \$500, this represents an increase of only about 2%.

The situation is slightly different when we use the standard deviation of past vote. In this case, the coefficient is insignificant in cross section regressions, but it becomes negative and significant in regressions with total federal spending (and, in one case, with targetable spending) when state fixed effects are included. This is the opposite of what the swing voter model would predict: a higher share of swing voters (measured by the standard deviation of Democratic vote) induces less spending. However, this is also consistent with our simulations, where we found that the coefficient of the share of independent voters tend to be biased downward when voting data are used and can even assume a negative sign while the true parameter is positive. This result is particularly evident when we compare model 7 and 8, i.e. when we also consider closeness and partisan alignment within the same specification. A negative β in model 8 (when voting data are used) is much more common than a negative β in model 7 (when exit poll data are used), and is significant in some cases.

Overall, we find little support for the basic prediction of the swing voter model. States with more independent voters do not receive significantly more federal funds. Also, while based on the regressions with voting data one might be tempted to conclude that states with more independents may actually be penalized, we can in fact conclude, also on the basis of our simulation exercise, that the negative sign is most likely due to endogeneity problems.

5.2 Battleground states

We conduct a similar investigation focusing on the competitiveness of the electoral races for president. In this case the results using poll data (model 3) and voting data (model 4) are quite similar. For total and targetable spending the coefficients on the competitiveness variable are negative – i.e. states with closer races receive fewer funds – which is counter to the predictions of models based on the swing voter logic. However, this only holds in cross section analysis. When we add state fixed effects the coefficients on closeness become insignificant. The situation for grants is the reverse: the negative sign prevails when state fixed effects are included but not in the cross section analysis. The magnitudes are generally larger when we use poll data measures, except for grants. One important difference between the voting and the exit poll regressions is that in the first case the results are not robust to the inclusion of other political variables (model 8), while the results in model 7 (poll data) are quite similar to those of model 3. We also find negative estimates when we remove the cross-pressured voters from the bulk of the partisans (not reported).

The main conclusion is that, when significant, the coefficient displays a sign which is opposite to what the "battleground states" hypothesis would predict. Using voting data delivers a very incoherent set of results, and this again conforms to the variability that we found in the simulation exercise. However, using the poll data does not seem to make any substantial difference in this case, although the results appear more robust to specification variations, at least in term of the significance of the coefficients.

5.3 Partisan supporters

An alternative to the swing voter hypothesis is that politicians reward loyal voters. We consider this possibility from the presidential point of view since this is most common in the literature. Thus, we first consider the share of vote for the incumbent president's party as the relevant measure of state partisanship and use it as an explanatory variable of spending. On the other side, from the exit polls we know the share of voters who identify themselves with each party and can therefore use this variable to measure partisanship. These alternative measures are considered in models 5 and 6. Looking at Table 3, it is clear that this is the only hypothesis that even receives partial support from the data. It is also clear, however, that using voting data to measure partisanship (model 6) leads to a significant overestimation of this effect. This is consistent with the findings of our simulation exercise. In model 6, the partisan share coefficient is always positive and, in some cases, significant at the 5% level. In model 5 the only significant coefficients are again positive; this time, however, some negative coefficients occur and the magnitude of the effect is generally (although not always) smaller. Introducing other political variables (model 7 and 8) induces some changes in magnitudes and significance levels. In this case the polling data measure of partisanship is always positive and, in four cases, significant at the 10% level. Subtracting cross-pressured voters from the count of the partisans does not alter the results significantly. We conclude that this is the only hypothesis for which we find significant coefficients with the correct sign and never a significant coefficient with the wrong sign, the opposite of what we found in the previous cases. The estimated magnitudes remain, however, rather small. Using the estimate of model 7 for total federal spending with fixed effects and term time units (the largest significant $\widehat{\delta}$ in model 7), we have that a 1% increase in the number of partisan supporters in a state corresponds to an increased spending of $4.3 per capita.[34]

5.4 Reliability of exit poll data

One concern is that our "null" findings could be due to measurement error in the key independent variables. While measurement error in surveys is often a serious problem, two factors work to mitigate the problem in our case, as noted in the introduction. First, previous work finds that party identification is one of the most reliably measured items in surveys and polls (e.g., Converse 1964; Green et al., 2002). Second, previous work also finds that

[34] For a within-state standard deviation (with time units given by presidential terms) of approximately 4% we get an increased federal spending of $17.2, which represents only 0.5% of average per capita federal spending ($3,100).

aggregating across individuals sharply reduces measurement error (e.g., Page and Shapiro, 1992; Stimson 1998).

One further concern is that the exit poll data have a 3-category scale of partisan identification (Democrat, Independent, and Republican) rather than the 7-category scale typically found in surveys (Strong Democrat, Weak Democrat, Independent Leaning Towards Democrats, Independent, Independent Leaning Towards Republicans, Weak Republican, and Strong Republican). Given our largely null findings, we are particularly concerned about possible measurement error. The main potential problem here is with the classification of "leaning" independents. Our measure includes these voters with the set of independents. However, many analysts argue that "leaning independents" vote more like weak partisans than "pure" independents. We check whether this matters using the Cooperative Congressional Election Study of 2006, which adopts a 7-point classification, which allows us to distinguish between "pure" and "leaning" independents. First, we find that at the state level the correlation between the average party identification using 3-point scale and the average using the 7-point scale is 0.99. Second, again at the state level, the correlation between the share of "pure" independents and the share of "pure and leaning" independents is 0.67. This is relatively high (although not as high as we would like). Third, we conduct a cross-sectional analysis of predicting the average distribution of federal government spending for the years 2000-2002, and find that the results are quite similar using both measures of independents (results not reported but available on request). In all cases, the coefficient on the variable measuring the share of independents is small, negative, and statistically insignificant.

6 Effects of government expenditures on voting

Our previous results cast some doubt on the idea that voters are responsive to the receipt of federal funds. In fact, one of the premises of the swing voter model is that politicians can buy votes by favoring certain groups in terms of spending allocation: swing voters are then simply cheaper to buy, given their lack of unconditional attachment to a given party. Hence, in this section we turn to the other side of the coin, and ask whether voters do in fact respond to favorable spending by rewarding incumbent politicians.

The relationship between spending and vote depends on how rational voters use their ballot to provide incentives to politicians. If voters are retrospective, they reward politicians for their past performance – i.e. they are more likely to vote for the incumbent, the larger is the amount of federal trasfers they have received when he was in power. On the other hand, if voters are prospective, then campaign promises should be the main driver of voting patterns.[35] The use of individual-level data can advance our understanding of voting behaviour in response to governmental transfers. Some recent studies (Manacorda et al., 2010; Pop-Eleches and Pop-Eleches, 2010) examine individual-level survey data on "expressed political support" and "vote intentions," and find that beneficiaries of targeted transfers declare an increased political support or propensity to vote for the goverment implementing them, thus providing indirect evidence of retrospective voting behaviour. On the other hand, Elinder et al. (2008), uses survey data on individual voting from the Swedish Election Studies, and find that voters respond to promises rather than to implemented policies. This indicates that prospective voting is important.

In our work, we estimate the impact of federal spending on individual voting decisions using voting records from exit polls, which have the desirable feature of collecting the information reported by actual voters when they exit the polling station. Our data also allow us to control for partisanship and ideology. Including such controls means that, to a large extent, we mitigate possible endogeneity problems for the spending variable. On the other hand, since we have information on federal budget *allocations* to the states (outlays) but not on *spending proposals*, we can check whether voters respond to received transfers (i.e.

[35] Assuming that voters are rational, retrospective and prospective voting are not, in fact, mutually exclusive. Rational retrospective voters, while using information about the past, are also forward looking because they reward/punish incumbents on the basis of past performance in order to influence their future behaviour. Similarly, rational prospective voters are to some extent retrospective because they must look at implemented policy to verify that promises are kept (see Besley (2006)).

they behave retrospectively), but not not whether they react to promises (i.e. they are prospective).

We analyze voting decisions in presidential, gubernatorial, senate and house elections. In the first three cases, the swing voter model would posit that incumbents are rewarded for the receipts of federal funds and therefore the dependent variable is a dummy equal to 1 if the voter chooses the incumbent (or a candidate from the incumbent's party). In the case of the House we cannot predict how the funds flowing to a state should affect voting for particular incumbents, since many states have House incumbents from different parties running simultaneously. Moreover, we only know the state of each voter, not her district. Thus, in this case the dependent variable is a dummy equal to 1 if a vote is cast for a Democratic candidate, and the explanatory variable of interest is an interaction term between the amounts received and the share of Democratic representatives from the state.[36]

Table 4 reports our estimations when total federal expenditure in the state is used as explanatory variable. It is clear that the fact that a state receives more federal funds does not induce its citizens to cast more votes in favor of incumbents. The coefficient of total federal expenditure can be even negative and never reaches a 5% significance level, in spite of the very large number of observations. On the contrary, partisanship and ideology have large effects. These results are consistent with Bartels (2000) and others, who find that partisanship has a large impact on voting both at presidential and congressional level.

When we use targetable spending, our results do not show substantial variations, with the exception of a positive coefficient on the probability of voting for an incumbent governor. Even in this case, however, the significance level (10%) appears rather weak for a sample of this size. For presidential election we encounter again a negative coefficient although only significant at the 10% level. Grants are totally insignificant in the president, governor and senator equations. They appear instead to have a positive impact in the probability of

[36] We checked how well the self-reported individual vote choices aggregate to predict actual state-level electoral results. This is a potential problem for any survey-based analysis of voting decisions. The correlation between the results predicted by the exit poll data and actual electoral results is over 0.79.

voting for a Democrat in Congress when the majority of state representatives in congress are Democrats. This is the only coefficient we encounter which turns out to be significant at the 5% level. Although this could be the consequence of the specification we use (not being able to identify the district of the voters), this is also consistent with related findings by Stein and Bickers (1994) and Levitt and Snyder (1995).[37]

Overall, the evidence that receiving more federal funds induces voters to reward incumbent politicians is rather weak.[38] One possible objection to this conclusion is that, according to swing voter models, in equilibrium, both candidates converge on the same platform: hence, in equilibrium, we should expect no effect, but this does not imply that voters would not react to spending proposals. The idea that electoral competition brings platform convergence appears, in reality, to run against historical evidence. The two major American parties have often proposed very different platforms on spending as well as on other matters.[39] Although identifying causal relationships is not straightforward, there appears to be a clear correlation between the platform proposals and the implemented policies, which is consistent with the "mandate" model (Budge and Hofferbert, 1990; King and Laver 1993; Ginsberg (1976)). In addition, numerous studies of taxation, spending and macroeconomic policies find clear correlations between the partisan composition of Congress and policy outcomes, consistent with a model of policy divergence (see, *e.g.*, Auten *et al.* 1984, Browning 1985, Kiewiet and McCubbins 1985 and 1991, Lowery *et al.* 1985, Hibbs 1987, Alesina *et al.* 1993, and Erikson *et al.* 2002).[40] At the district level the situation does not appear much different: individual

[37]The estimates reported in Table 4 assume that all voters should be affected in the same way by the receipt of federal funds. This is not necessarily the case. Hence, we have considered specifications that introduce interactions between the spending variables and the partisanship and ideological variables. The results suggest that heterogeneous responses are sometimes possible but that, overall, these effects are hardly statistically significant, particularly considering the size of the sample.

[38]Some other studies in the literature also find insignificant effects of state expenditure on voting, e.g. Besley (2006).

[39]See, for example, Sundquist (1983). The different stances on the role of public spending to stimulate the economy taken by the Democrats and the Republicans during the great depression constitute a prime example of policy platform divergence on spending issues, and one that has had long lasting consequences on the subsequent evolution of the two parties.

[40]A few studies find small effects or mixed results — *e.g.*, Kamlet and Mowery (1987) and Kiewiet and

candidates for the House have also been shown to systematically assume divergent positions (Erikson and Wright, 1997; Ansolabehere, et al., 2001). In addition, Poole and Rosenthal (2000), Lee *et al* (2004) and others document stark differences in the roll call voting positions of Democrat and Republican representatives elected from districts with very similar partisan balance.

Another possible explanation for our findings is that although parties (or candidates) do not converge, our estimates nonetheless capture equilibrium behavior that masks structural coefficients. Suppose, for example, that candidates typically manage to meet voters' expectations, or fulfill their campaign promises, regarding spending. Then we may find little correlation in the data because we do not observe "out-of-equilibrium" behavior: rational prospective voters reward politicians based on the expectation that they will be faithful to their election pledges, we would only observe a reaction of voters to past policy if promises are not kept. This is unlikely to happen whenever a large share of campaign pledges are enacted, as it is the case for the U.S. (Mansergh and Thomson, 2007). Thus, while our findings are consistent with the hypothesis that voters rarely respond with their votes to public spending in a clear and systematic way, further research is clearly needed to rule out other possibilities.

7 Conclusion

Our findings regarding the allocation of federal spending across U.S. states are disappointing for theories of distributive politics, but are good news for the working of institutions, designed to provide checks and balances, preventing legislators from abusing their power by tailoring budget allocations to their political goals. We find little robust evidence supporting the notion that parties target areas with high numbers of swing voters. Using polling data, the estimated effect of the share of voters who self identify as independents is statistically insignificant and usually substantively small. Using voting data, the estimated effect of the

Krehbiel (2002).

"volatility" of the partisan vote is often negative, rather than positive as predicted by the swing voter model. We also find no consistent support for the notion that parties target battleground states. We find limited and mixed support for the notion that parties target areas with high numbers of their partisan supporters. Finally, we find no significant effect of distributive spending on voting decisions – thus, it seems most likely that, to the extent that partisan targeting occurs, it is driven more by the policy-motivations of politicians or interest groups than by strategic calculations to win electoral support.

Alternatively, if politicians are informed about the preferences of particular groups of voters for some specific spending items, they might try to gain their support by increasing spending on such items to the expense of others. In this case, the strategic manipulation of the budget would affect its composition but not necessarily the overall amount of funds allocated to a particular geographic unit.[41]

Our findings might reflect features of distributive politics that are particular to the U.S.. Congress is one of the most powerful and decentralized national legislatures in the world. It jealously guards its control over the public purse. Committees are powerful, and jealously guard their own jurisdictions. Strong norms of seniority rule give committee leaders and members a substantial degree of independence from party leaders. Individual senators and representatives frequently pursue their own re-election goals, working to "bring home the bacon" for their state or district. The federal structure of the U.S., with strong and autonomous state governments, further complicates the situation. For example, many federal grants to states are either matching or project grants, and decisions by state governments therefore affect where federal money flows.

As a result of these factors, the president may have relatively little influence over the geographic distribution of federal expenditures. Perhaps, even though he would like to target

[41]Our results do not exclude the possibility that strategic distribution of funds might occur in some particular years (e.g. the pre-election year) when electoral concerns might me stronger. The hypothesis of a 'political cycle' in distributive politics is not considered by the large existing literature that we have revisited in our work, but it represents a very interesting avenue for future theoretical and empirical research on pork-barrel spending.

swing states or swing voters, he cannot. As noted above, studies of other countries have found more support for the swing-voter and battleground hypotheses. Further investigations in other institutional settings are necessary to establish the validity of this conclusion.

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APPENDIX: Variable Definitions and Sources

- **Exit Poll data**. We use questions on reported vote, party identification and ideology. Party identification question are typically of the form: "Regardless of how you voted today, do you normally think of yourself as a [Democrat], [Republican], [Independent], [Something Else]?" ; ideology questions are typically of the form: "regardless of the party you may favor, do you lean more toward the liberal side or the conservative side politically [liberal], [conservative], [somewhere in between]?" . The share of Democratic (Republican, Independent) is then constructed by aggregating individual observations by state. We have proceeded analogously for the ideology data. This information is available every two years but aggregated over four year periods to avoid small samples in some states. Only samples of at least 100 hundred observations have been used. Very few cases have been deleted using this method. All regressions have been repeated not excluding these cases and they deliver the same results. Once obtained the 4-years aggregates, data have been smoothed assuming that variations in ideology and partisanship are gradual (and keeping fixed the years of presidential elections). For example, $D_{1985} = 0.25D_{1984} + 0.75D_{1988}$; $D_{1986} = 0.5D_{1984} + 0.5D_{1988}$; $D_{1987} = 0.25D_{1984}+0.75D_{1988}$. The data obtained with this procedure have been finally lagged by one period. The share of swing voters is measured by the share of independents. Closeness is measured as $1-|D-R|$. Partisanship fo the incumbent president is. D when the president is Democratic and R when the president is Republican. Sources: *CBS News, New York Times, ABC News, Washington Post, Voters News Service.*

- **Spending data.** Federal Expenditure, Targetable Expenditure (defined as Federal Expenditure-Direct Payments to Individuals), Grants are all in real and per capita terms. Targetable spending is total federal expenditure minus direct payments to individuals. *Source: Statistical Abstract of the United States.*

- **Voting Data.** Defining as $\widetilde{D}$ the share of Democratic vote in the last election and $\widetilde{R}$ the share of Republican vote in the last election, we always consider $D = \widetilde{D}/(\widetilde{D} + \widetilde{R})$ and $R = 1 - D$. Swingness is measured as the standard deviation of D in the previous three presidential elections. Election closeness is defined as $1 - |D - R|$. The share of vote for the incumbent president is D when the president is Democratic and R when the president is Republican. *Source: Statistical Abstract of the United States.*

- **Socioeconomic data**. Real income per capita, population (in logarithms), percentage elderly (above 65), percentage in schooling age (5-17) and unemployment rate are taken from the *Statistical Abstract of the United States.* Disaster declarations are taken from the Federal Emergency Management Agency.

Table 1: Simulation Results

Case 1: $\alpha_P = \beta_P = 0$, $\beta_I = 1.0$, $\sigma_\mu = .7$, $\sigma_\epsilon = .3$

		Model 1a	Model 1b	Model 2a		Model 3			Model 4		
α_I	α_C	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$
0.0	0.0	-.00	.09	.01	.09	.01	.09	.28	-.00	.09	.28
0.0	0.5	–	.40	.06	.41	.05	.40	.24	.11	.39	.24
0.0	1.0	–	.55	.09	.58	.07	.57	.20	.26	.56	.20
0.5	0.0	.42	–	.43	.18	.43	.18	.30	.49	.08	.30
0.5	0.5	–	–	.40	.61	.40	.60	.27	.54	.44	.27
0.5	1.0	–	–	.32	.87	.31	.86	.23	.63	.71	.23
1.0	0.0	.49	–	.56	.44	.57	.43	.31	.99	.07	.31
1.0	0.5	–	–	.53	.90	.53	.89	.29	1.00	.48	.28
1.0	1.0	–	–	.41	1.17	.40	1.15	.25	1.05	.81	.24

Case 2: $\alpha_P = \beta_P = 0$, $\beta_I = 1.0$, $\sigma_\mu = .1$, $\sigma_\epsilon = .3$

		Model 1a	Model 1b	Model 2a		Model 3			Model 4		
α_I	α_C	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$
0.0	0.0	.00	.00	.00	.00	.00	.00	.01	.00	.00	.01
0.0	0.5	–	.27	.07	.30	.07	.29	.00	.15	.28	.00
0.0	1.0	–	.24	.11	.27	.09	.28	.00	.32	.28	.00
0.5	0.0	.15	–	.21	.15	.21	.14	.01	.50	.00	.00
0.5	0.5	–	–	.24	.52	.23	.51	.00	.57	.36	.00
0.5	1.0	–	–	.23	.70	.22	.69	.00	.69	.58	.00
1.0	0.0	-.61	–	-.64	-.08	-.64	-.08	.00	1.00	.00	.01
1.0	0.5	–	–	-.53	.25	-.54	.24	.01	1.01	.41	.00
1.0	1.0	–	–	-.34	.61	-.37	.58	.02	1.08	.70	.01

Table 1: Simulation Results (continued)

Case 3: $\alpha_P = \beta_P = .5$, $\beta_I = 1.0$, $\sigma_\mu = .7$, $\sigma_\epsilon = .3$

		Model 2b		Model 2c		Model 3			Model 4		
α_I	α_C	$\hat{a}_I$	$\hat{a}_P$	$\hat{a}_C$	$\hat{a}_P$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$
0.0	0.0	-.00	.84	.05	.84	-.00	.06	.85	.00	.06	.85
0.0	0.5	–	–	.38	.78	.10	.40	.78	.13	.38	.78
0.0	1.0	–	–	.60	.70	.13	.65	.70	.28	.62	.70
0.5	0.0	.39	.89	–	–	.39	.04	.90	.50	.05	.90
0.5	0.5	–	–	–	–	.43	.56	.85	.58	.41	.85
0.5	1.0	–	–	–	–	.36	.89	.78	.69	.71	.78
1.0	0.0	.28	.92	–	–	.27	.12	.93	1.00	.04	.93
1.0	0.5	–	–	–	–	.29	.65	.89	1.05	.45	.89
1.0	1.0	–	–	–	–	.12	.92	.84	1.12	.78	.84

Case 4: $\alpha_P = \beta_P = .5$, $\beta_I = 1.0$, $\sigma_\mu = .1$, $\sigma_\epsilon = .3$

		Model 2b		Model 2c		Model 3			Model 4		
α_I	α_C	$\hat{a}_I$	$\hat{a}_P$	$\hat{a}_C$	$\hat{a}_P$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$
0.0	0.0	-.00	.60	.00	.60	.00	.00	.60	-.00	.00	.60
0.0	0.5	–	–	.30	.54	.07	.32	.54	.17	.31	.54
0.0	1.0	–	–	.43	.45	.02	.44	.45	.38	.47	.45
0.5	0.0	-.14	.66	–	–	-.19	-.08	.66	.50	.00	.66
0.5	0.5	–	–	–	–	-.36	.12	.61	.60	.37	.61
0.5	1.0	–	–	–	–	-.68	.19	.55	.75	.61	.54
1.0	0.0	-.81	.70	–	–	-.89	-.21	.70	1.00	.00	.70
1.0	0.5	–	–	–	–	-1.17	-.20	.66	1.07	.40	.66
1.0	1.0	–	–	–	–	-1.59	-.23	.60	1.17	.71	.61

Table 2: Cross tabulation of party ID and presidential voting decisions (percentage)

		Party ID				*percentage with party-ID different from reported vote*
		Democratic	**Republican**	**Independent**	**Total**	
	Democratic	33.93	3	10.4	47.33	28.31
Voting	**Republican**	6.34	31.97	11.66	49.97	36.02
	Other	0.7	0.65	1.31	2.66	
	Total	40.97	35.62	23.37	100	
percentage of party ID that voted for a different party		17.18	10.25			

TABLE 3: Summary of Spending Regression Results

Model			model 1	model 2	model 3	model 4	model 5	model 6
Coefficients			β	β	γ	γ	δ	δ
Dep. Variable	Time Unit	State F.E.						
fed. exp.	year	no	0.34	0.52	-0.87*	-0.49*	-0.1	0.44*
fed. exp.	year	yes	-0.37	-1.3***	0.19	0.1	0.36*	0.10
fed. exp.	term	no	0.25	0.68	-0.86	-0.71**	0.00	0.81**
fed. exp.	term	yes	0.12	-1.30**	0.2	-0.01	0.38**	0.33*
targetable	year	no	0.41	0.35	-0.75*	-0.29	-0.34	0.14
targetable	year	yes	-0.40	-0.84	0.10	0.06	0.18	-0.04
targetable	term	no	0.29	0.52	-0.71	-0.49*	-0.23	0.50**
targetable	term	yes	-0.33	-0.90	0.19	-0.01	0.15	0.08
grants	year	no	0.11	-0.18	-0.12	-0.04	-0.01	0.05
grants	year	yes	0.09	-0.02	0.05	-0.08*	0.07	0.09**
grants	term	no	0.13	0.04	-0.12	-0.12	-0.00	0.10
grants	term	yes	0.23	0.07	0.02	-0.12**	0.06	0.11**

Model			model 7			model 8		
Coefficients			β	γ	δ	β	γ	δ
Dep Variable	Time Unit	State F.E.						
fed. exp.	year	no	0.43	-0.87*	0.16	0.36	-0.54	-0.15
fed. exp.	year	yes	-0.12	0.18	0.34	-1.46***	0.36	0.56**
fed. exp.	term	no	0.38	-0.86	0.20	0.19	-0.63	0.21
fed. exp.	term	yes	0.35	0.23	0.43**	-1.41**	0.16	0.55**
targetable	year	no	0.33	-0.75*	-0.13	0.35	-0.44	-0.38
targetable	year	yes	-0.31	0.09	0.12	0.87	0.11	0.15
targetable	term	no	0.23	-0.71	-0.11	0.19	-0.45	0.07
targetable	term	yes	-0.26	0.19	0.13	-0.96	-0.01	0.14
grants	year	no	0.17	-0.13	0.09	-0.21	-0.06	0.00
grants	year	yes	0.16	0.05	0.09*	-0.07	-0.07	0.03
grants	term	no	0.20	-0.12	0.11	-0.14	-0.14	0.01
grants	term	yes	0.28*	0.03	0.09*	-0.01	-0.12	0.01

Note. *Each cell corresponds to a regression. In this table we only report the coefficients of interest. Detailed results (and standard errors) can be found in the Statistical Appendix. B is the coefficient of the share of swing voters, γ is the coefficient of election closeness, δ is the coefficient of the share of partisan supporters. Models 1-6 test the three hypotheses separately, models 7-8 jointly. In models 1-3-5-7 our key variables are measured using exit poll data, in models 2-4-6-8 they are measured using voting returns. When state fixed effects are not included, we introduce dummies for Maryland, Virginia and New Mexico.We use Driscoll-Kraay standard errors to correct potential spatial autocorrelation. * indicates significance at 10* level, ** at 5% and *** at 1%.*

TABLE 4: Effects of spending on voting decisions

Dependent Variable: vote for the incumbent in columns 1-3 and vote Democratic in column 4	(1) governor	(2) president	(3) senator	(4) Congress
(1)				
federal expenditure	0.2851	-0.1295	-0.3230	-0.0507
	(0.3099)	(0.0719)*	(0.2692)	(0.0523)
partisan match	2.2109	2.0522	1.9842	
	(0.0952)***	(0.0368)***	(0.0696)***	
ideology match	0.9000	0.7427	0.6730	
	(0.0584)***	(0.0219)***	(0.0613)***	
fed. exp. x democratic share of house representatives				-0.0033
				(0.0660)
share of Democratic representatives in the House				0.0803
				(0.2433)
Observations	121570	129429	181350	190944
Pseudo-R2	0.4523	0.3646	0.3559	0.3407
(2)				
targetable spending	1.2421	-0.1393	-0.3303	0.0168
	(0.6900)*	(0.0726)*	(0.3058)	(0.0672)
partisan match	2.1195	2.2128	1.9677	
	(0.0932)***	(0.0423)***	(0.0700)***	
ideology match	0.8779	0.7303	0.6671	
	(0.0617)***	(0.0199)***	(0.0613)***	
targetable spend. x democratic share of house representatives				-0.0251
				(0.0837)
share of Democratic representatives in the House				0.1372
				(0.2800)
Observations	109711	141451	175323	174387
Pseudo-R2	0.4648	0.3657	0.3514	0.3283
(3)				
grants	0.1538	0.3718	0.9469	0.5154
	(1.7089)	(0.6302)	(1.0639)	(0.2478)**
partisan match	2.2035	2.0505	1.9885	
	(0.0964)***	(0.0367)***	(0.0699)***	
ideology match	0.8998	0.7309	0.6737	
	(0.0610)***	(0.0200)***	(0.0618)***	
grants x democratic share of house representatives				0.1688
				(0.2164)
share of Democratic representatives in the House				-0.0123
				(0.1367)
Observations	121570	141451	181350	190944
Pseudo-R2	0.4518	0.3646	0.3555	0.3408

All regressions include a constant, year dummies, state fixed effects and the following control variables: income per capita, percentage of the population in schooling age, percentage of the population above the age of 65, total population, unemployment rate, dummy equal to 1 for unit-periods in which a natural disaster occurred. The House regressions also include dummies for Democratic partisanship, Republican partisanship, liberal ideology and conservative ideology. Partisan match is a dummy equal to 1 if the voter has the same partisanship of the incumbent politician. Ideology match is a dummy equal to 1 if the voter is liberal and the incumbent politician is a Democrat, or if the voter is conservative and the incumbent is Republican. Robust z-statistics in parentheses (clustered by state). * sign. at 10%; ** sign. at 5%; *** sign. at 1%.

Fig. 1: Democratic vote share and partisanship by state

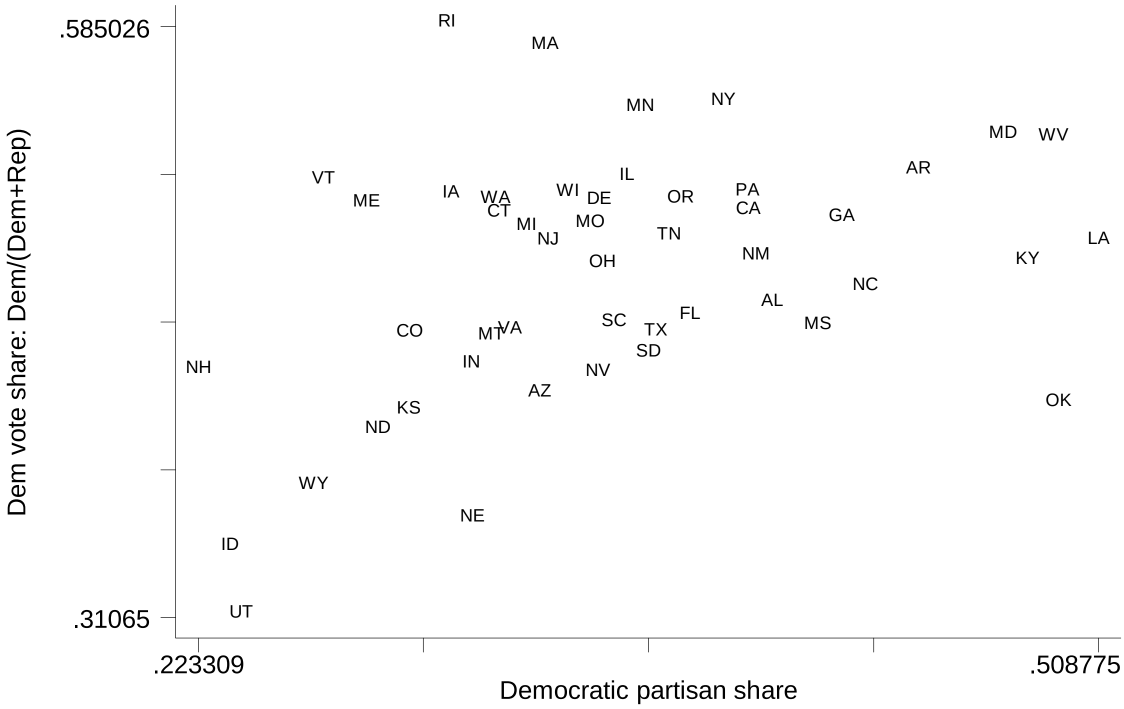
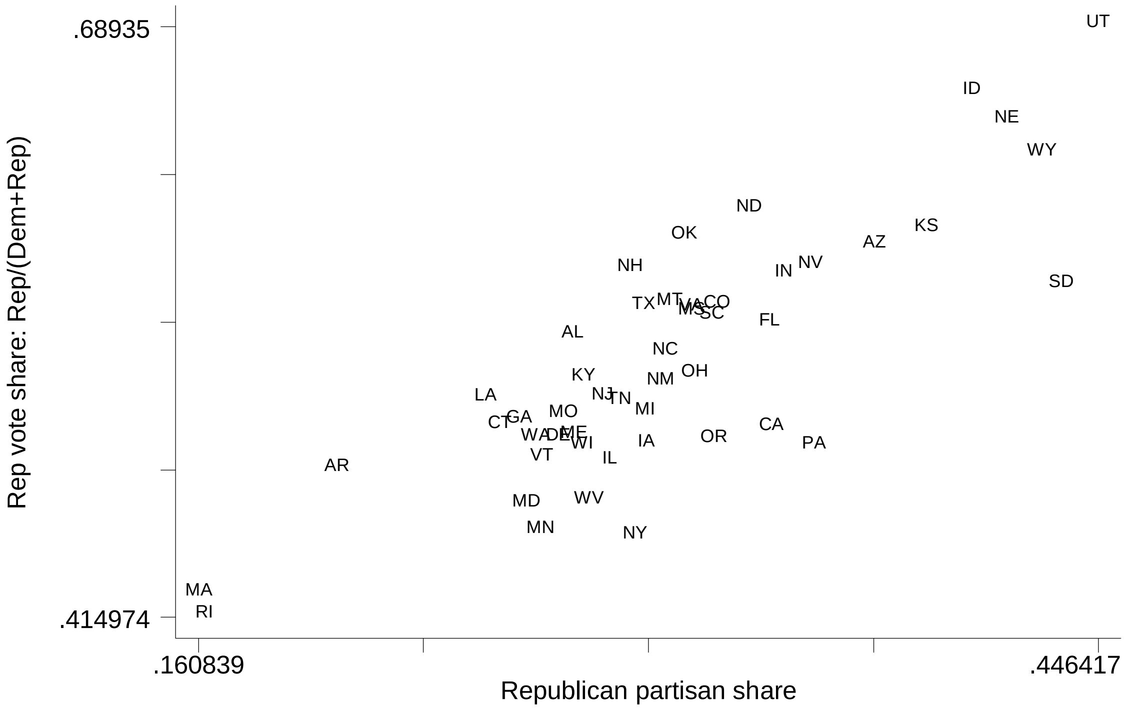


Fig. 2: Republican vote share and partisanship by state



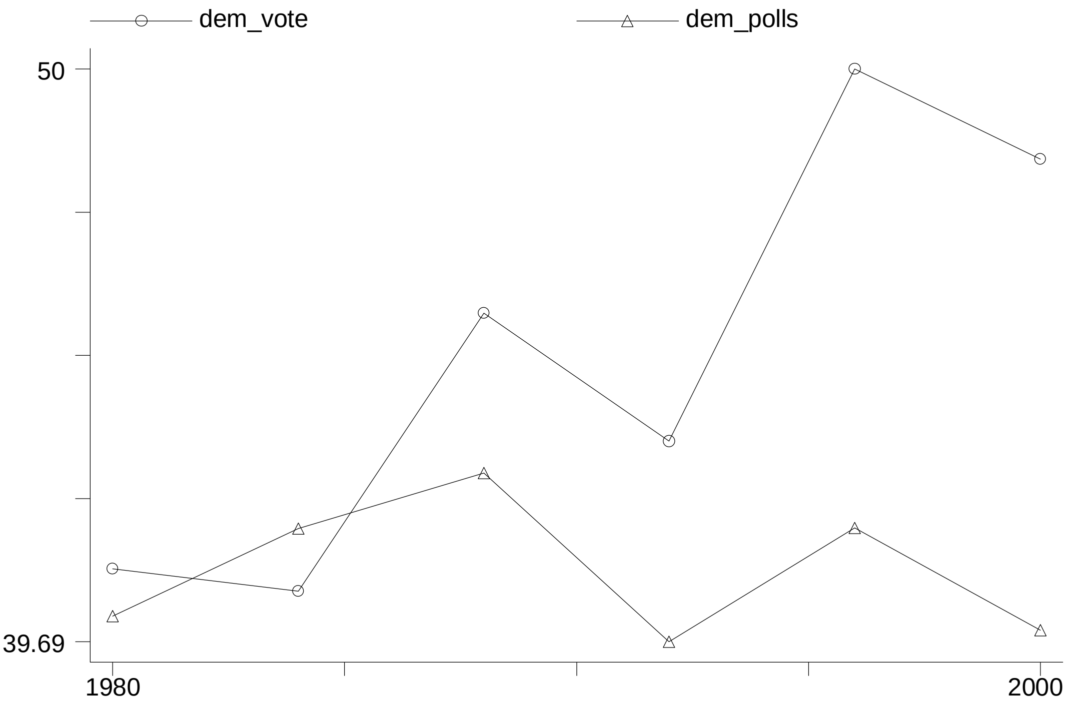


Fig3. Aggregate Democratic Vote and Partisanship over Time

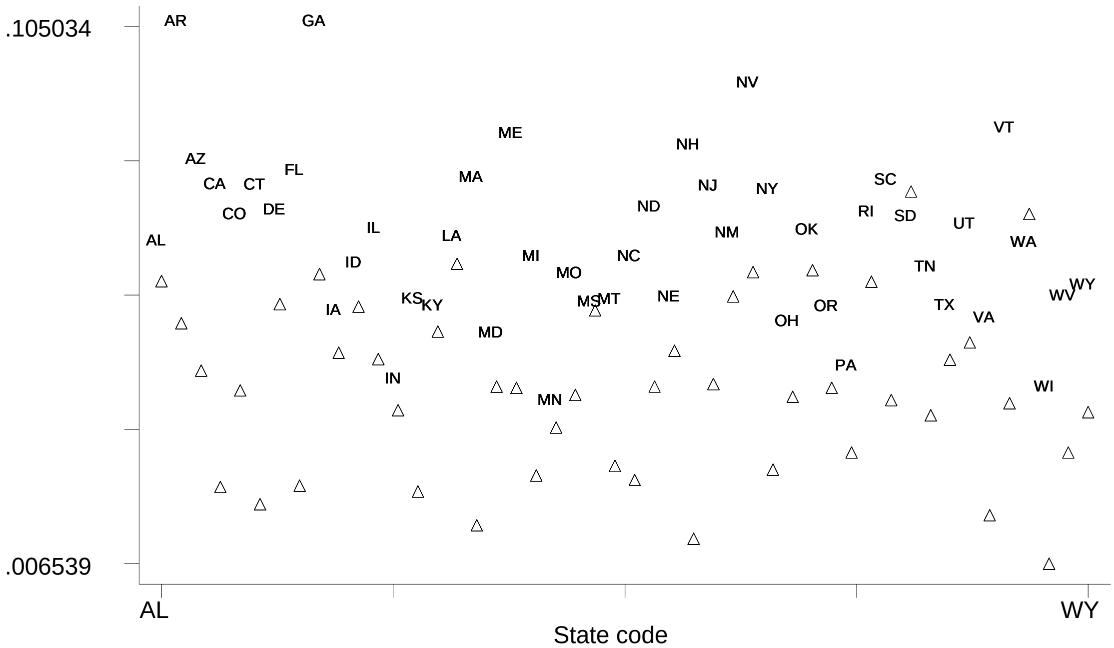
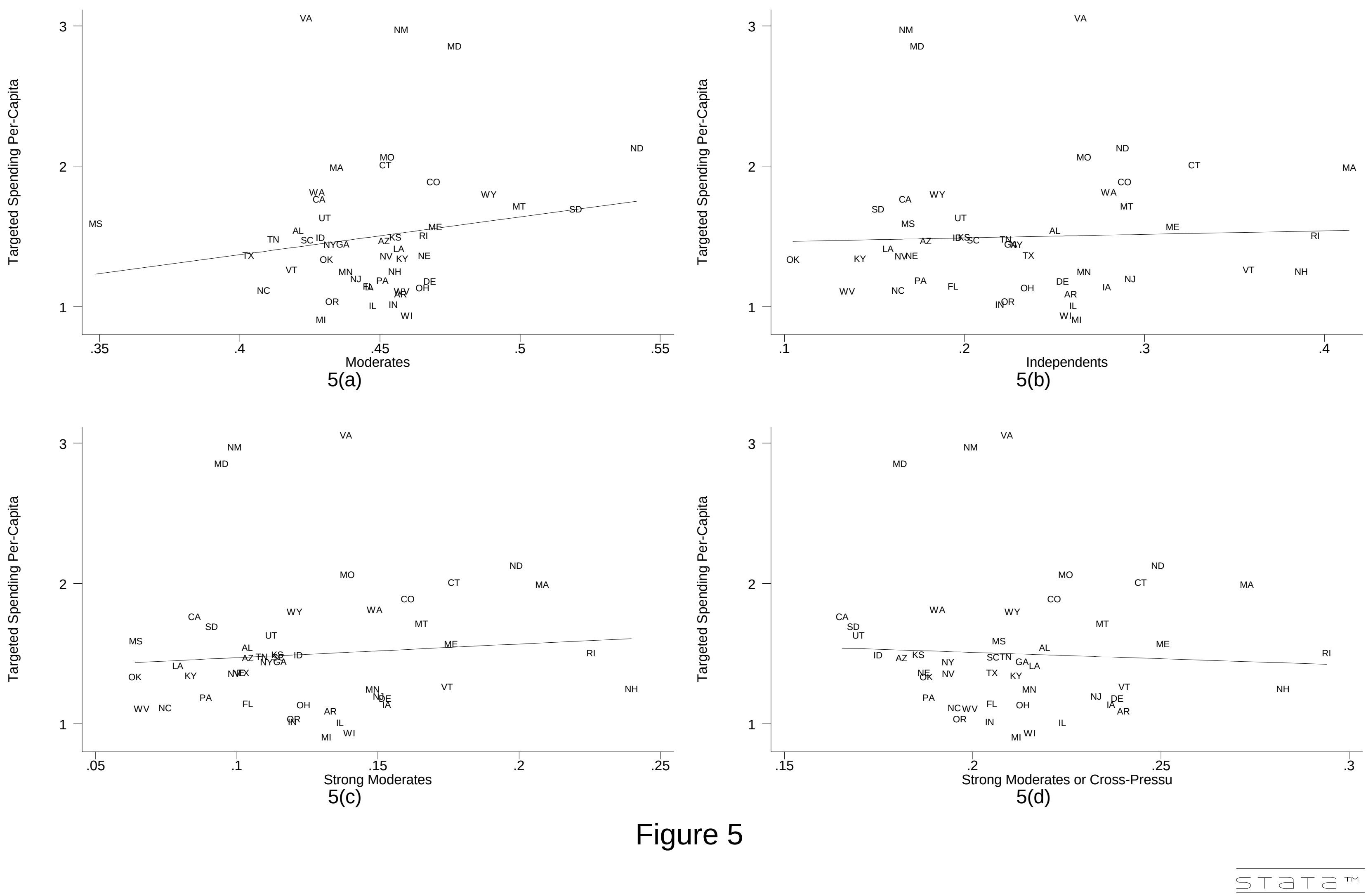


Fig.4: Standard deviation of Democratic vote and partisanship by State

Note: *In this graph the states are listed in alphabetical order. The state code is placed in correspondence of the standard deviation of the Democratic vote in the state. The corresponding triangles represent the standard deviation of Democratic partisanship*



5(a)

5(b)

5(c)

5(d)

Figure 5

TESTING MODELS OF DISTRIBUTIVE POLITICS USING EXIT POLLS TO MEASURE VOTER PREFERENCES AND PARTISANSHIP

by Valentino Larcinese, James Snyder and Cecilia Testa

Appendix

In this Appendix we provide further information and robustness checks relative to the material presented in the paper "Testing Models of Distributive Politics Using Exit Polls to Measure Voters' Preferences and Partisanship".

The Appendix is divided in three Sections.

In Section A we provide the results of a simulation exercise that, compared to Table 1 in the paper, takes into account the possibility of measurement error (Table A1). We also provide summary statistics for all variables (Table A2) and a correlation matrix for our main explanatory variables (Table A3)

In Section B (Tables B1-B12) we report detailed regression tables corresponding to the coefficients presented in the main text in Table 3.

In Section C (Tables C1-C12), we report detailed regression tables when we include Census division dummies to take into account possible spatial correlation in the dependent variables. In specifications without state fixed effects we include division fixed effects and division-specific trends. In specifications with state fixed effects we only include division-specific trends. The Census divides the US contiguous states into nine divisions reported below:

Division 1: CT, MA, ME, NH, RI, VT

Division 2: NJ, NY, PA

Division 3: IL, IN, MI, OH, WI

Division 4: IA, KS, MN, MO, ND, NE, SD

Division 5: DE, FL, GA, MD, NC, SC, VA, WV

Division 6: AL, KY, MS, TN

Division 7: AR, LA, OK, TX

Division 8: AZ, CO, ID, MT, NM, NV, UT, WY

Division 9: CA, OR, WA

Table A1: Simulation Results with and without measurement error							
Case 1: $\alpha_P = \beta_P = 0$, $\beta_I = 1.0$, $\sigma_\mu = .20$, $\sigma_\epsilon = .09$							
		Model 4, meas error			Model 4, no error		
α_I	α_C	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$	$\hat{a}_I$	$\hat{a}_C$	$\hat{a}_P$
0.0	0.0	-.01	.09	.27	-.00	.09	.27
0.0	0.5	.05	.39	.22	.11	.39	.24
0.0	1.0	.13	.54	.19	.26	.56	.19
0.5	0.0	.20	.08	.29	.49	.08	.29
0.5	0.5	.23	.43	.26	.54	.44	.26
0.5	1.0	.28	.69	.23	.63	.71	.22
1.0	0.0	.39	.11	.30	.99	.07	.30
1.0	0.5	.40	.51	.27	1.00	.48	.27
1.0	1.0	.43	.80	.25	1.05	.80	.24

The table above gives the basic type of results from the simulations when there is measurement error in the "direct" measure of voters' partisanship – i.e. in the share of independent, I_j. We focus on the first panel from the original simulations. The figures from the original simulations are shown on the right, in the columns under the heading "Model 4, no error." The new results are shown on the left,in the columns under the heading "Model 5, meas error." Not surprisingly, the estimated coefficient on the term measured with error – i.e., $\hat{a}_I$ – is biased toward zero. This is the usual attenuation bias, and is large because the amount of error added to the underlying variable is quite large. Not much else is going on, however. In particular, the average estimated values for $\hat{\alpha}_c$ and $\hat{\alpha}_p$ are quite similar to the average estimated values when there is no measurement error in I_j.

Table A2. Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
federal expenditure (real per capita 1,000 USD)	1174	3.0705	0.6095	1.7948	5.6807
targetable spending (real per capita 1,000 USD)	1126	1.4840	0.5199	0.4325	3.5832
grants (real per capita 1,000 USD)	1174	0.5118	0.1673	0.2315	1.3872
share of independents	1174	0.2732	0.0819	0.0934	0.7500
closeness (poll data)	1174	0.8850	0.0880	0.5610	1.0000
partisan alignment	1174	0.3633	0.0761	0.0000	0.6161
standard deviation of democratic vote	1174	0.0579	0.0296	0.0017	0.2218
closeness (voting data)	1174	0.8559	0.1012	0.4407	0.9999
vote share of incumbent president	1174	0.5497	0.0636	0.3435	0.7797
income (real per capita 1,000 USD)	1174	13.9622	2.5302	8.6011	24.0687
population (thousand)	1174	5291	5501	425	35100
share of population above 65	1174	0.1226	0.0214	0.0455	0.3760
share of population in schooling age (5-17)	1174	0.1941	0.0271	0.0233	0.6199
unemployment (percentage)	1174	6.0026	2.1078	2.2000	18
natural disaster	1174	0.5162	0.5000	0.0000	1

Table A3. Correlation matrix of main explanatory variables

	share of Independents	standard deviation of Democratic vote	closeness (exit polls)	closeness (voting)	share of Presidential copartisans (party identification)	share of Presidential vote
share of Independents	1					
standard deviation of Democratic vote	-0.0074	1				
closeness (exit polls)	0.1015	-0.3469	1			
closeness (voting)	0.0298	-0.1168	0.166	1		
share of Presidential copartisans (party identification)	-0.5559	0.0003	-0.3469	-0.2629	1	
share of Presidential vote	-0.1101	0.2165	-0.0477	-0.684	0.3887	1

Table B1. Testing the swing voter hypothesis (federal expenditure)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.
share independents	0.3409		-0.3660		0.2520		0.1167	
	(0.5912)		(0.3539)		(0.6581)		(0.5754)	
standard deviation of Democratic vote		0.5218		-1.3085***		0.6772		-1.3023**
		(1.0483)		(0.4751)		(1.0917)		(0.5213)
income	0.0089	0.0161	-0.0598*	-0.0574*	0.0093	0.0157	-0.0677*	-0.0656*
	(0.0263)	(0.0260)	(0.0333)	(0.0315)	(0.0271)	(0.0267)	(0.0372)	(0.0347)
population (log)	-0.0791	-0.0899*	-1.3526***	-1.3660***	-0.0951	-0.1022*	-1.3004***	-1.3354***
	(0.0520)	(0.0504)	(0.2110)	(0.1958)	(0.0572)	(0.0541)	(0.2293)	(0.2114)
share aged >65	5.3127***	5.3792***	9.5203***	9.6081***	5.6518***	5.7140***	11.1723***	11.0043***
	(1.3666)	(1.4186)	(2.5517)	(2.4358)	(1.4032)	(1.4664)	(2.8633)	(2.7512)
share aged 5-17	-2.2241***	-2.3886***	-4.4705***	-4.3372***	-2.5409**	-2.6765***	-5.3428***	-5.1628***
	(0.8260)	(0.8722)	(1.3216)	(1.2960)	(0.9634)	(0.9916)	(1.4399)	(1.4101)
unemployment rate	0.0001	-0.0009	-0.0042	-0.0015	-0.0017	-0.0027	-0.0079	-0.0048
	(0.0182)	(0.0187)	(0.0137)	(0.0128)	(0.0244)	(0.0246)	(0.0174)	(0.0158)
natural disaster	0.0356	0.0355	0.0226	0.0238	0.1193	0.1131	0.0520	0.0601
	(0.0341)	(0.0351)	(0.0151)	(0.0154)	(0.1076)	(0.1099)	(0.0572)	(0.0587)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.6360	0.6351	0.9178	0.9194	0.6602	0.6604	0.9472	0.9490

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B2. Testing the battleground hypothesis (federal expenditure)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.
closeness (poll data)	-0.8658*		0.1896		-0.8579		0.2039	
	(0.4869)		(0.1949)		(0.5404)		(0.2004)	
closeness (voting data)		-0.4902*		0.1005		-0.7128**		-0.0142
		(0.2824)		(0.1593)		(0.3196)		(0.1753)
income	0.0204	0.0121	-0.0600*	-0.0573	0.0185	0.0113	-0.0676*	-0.0679*
	(0.0264)	(0.0260)	(0.0337)	(0.0343)	(0.0271)	(0.0264)	(0.0371)	(0.0379)
population (log)	-0.0875*	-0.0763	-1.3469***	-1.3742***	-0.0987*	-0.0840	-1.3088***	-1.2982***
	(0.0481)	(0.0508)	(0.2088)	(0.2246)	(0.0523)	(0.0527)	(0.2249)	(0.2364)
share aged >65	5.8026***	5.8547***	9.7411***	9.7230***	6.1834***	6.5063***	11.0145***	11.0896***
	(1.4610)	(1.4542)	(2.5239)	(2.5420)	(1.5319)	(1.5978)	(2.8707)	(2.8647)
share aged 5-17	-2.9687***	-2.7366***	-4.4989***	-4.5581***	-3.1819***	-3.1651***	-5.2243***	-5.3027***
	(0.9908)	(0.8689)	(1.3197)	(1.3277)	(1.1142)	(1.0657)	(1.4597)	(1.4520)
unemployment rate	-0.0062	0.0034	-0.0026	-0.0033	-0.0092	0.0050	-0.0062	-0.0079
	(0.0185)	(0.0187)	(0.0140)	(0.0142)	(0.0244)	(0.0245)	(0.0171)	(0.0174)
natural disaster	0.0311	0.0353	0.0236	0.0232	0.1048	0.1224	0.0573	0.0528
	(0.0340)	(0.0351)	(0.0153)	(0.0150)	(0.1069)	(0.1098)	(0.0572)	(0.0560)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.6470	0.6393	0.9178	0.9176	0.6702	0.6688	0.9474	0.9472

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B3. Testing the partizanship hypothesis (federal expenditure)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.
partisan alignment	-0.0975		0.3610**		-0.0004		0.3832**	
	(0.4230)		(0.1739)		(0.4711)		(0.1903)	
vote share of incumbent president		0.4364*		0.1024		0.8062**		0.3281*
		(0.2445)		(0.1555)		(0.3045)		(0.1719)
income	0.0135	0.0139	-0.0591*	-0.0600*	0.0134	0.0143	-0.0647*	-0.0656*
	(0.0256)	(0.0262)	(0.0332)	(0.0336)	(0.0262)	(0.0269)	(0.0369)	(0.0376)
population (log)	-0.0888*	-0.0868*	-1.3190***	-1.3342***	-0.1031*	-0.0971*	-1.2796***	-1.2846***
	(0.0502)	(0.0504)	(0.2173)	(0.2083)	(0.0544)	(0.0533)	(0.2322)	(0.2219)
share aged >65	5.3332***	5.4806***	9.7144***	9.7966***	5.7173***	5.9860***	11.0792***	10.9830***
	(1.3502)	(1.4037)	(2.5196)	(2.5482)	(1.4048)	(1.4766)	(2.7893)	(2.8095)
share aged 5-17	-2.3465***	-2.4471***	-4.4764***	-4.5833***	-2.6542***	-2.8248***	-5.2234***	-5.2654***
	(0.8217)	(0.8463)	(1.3178)	(1.3320)	(0.9538)	(0.9771)	(1.4081)	(1.4169)
unemployment rate	-0.0008	0.0000	-0.0024	-0.0039	-0.0015	0.0001	-0.0057	-0.0088
	(0.0191)	(0.0186)	(0.0135)	(0.0139)	(0.0256)	(0.0241)	(0.0166)	(0.0171)
natural disaster	0.0370	0.0322	0.0187	0.0214	0.1182	0.1099	0.0462	0.0484
	(0.0350)	(0.0351)	(0.0146)	(0.0150)	(0.1080)	(0.1077)	(0.0567)	(0.0565)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.6347	0.6361	0.9186	0.9176	0.6596	0.6640	0.9481	0.9478

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B4. Testing swing, battleground and partizanship hypotheses in one equation (federal expenditure)

Dependent Variable	(1) federal exp.	(2) federal exp.	(3) federal exp.	(4) federal exp.	(5) federal exp.	(6) federal exp.	(7) federal exp.	(8) federal exp.
share independents)	0.4288		-0.1183		0.3755		0.3484	
	(0.5280)		(0.3745)		(0.5941)		(0.5763)	
closeness (poll data)	-0.8662*		0.1792		-0.8572		0.2317	
	(0.4796)		(0.1951)		(0.5390)		(0.2058)	
partisan alignment	0.1637		0.3390		0.1998		0.4330**	
	(0.2779)		(0.2041)		(0.3159)		(0.2129)	
standard deviation of Democratic vote		0.3597		-1.4605***		0.1858		-1.4103**
		(1.0264)		(0.4945)		(1.0509)		(0.5719)
closeness (voting data)		-0.5389		0.3644		-0.6263		0.1591
		(0.3870)		(0.2553)		(0.3744)		(0.2608)
vote share of incumbent president		-0.1528		0.5583**		0.2129		0.5524**
		(0.3773)		(0.2551)		(0.3443)		(0.2613)
income	0.0149	0.0133	-0.0595*	-0.0510	0.0146	0.0124	-0.0647*	-0.0590
	(0.0272)	(0.0259)	(0.0331)	(0.0336)	(0.0281)	(0.0267)	(0.0373)	(0.0382)
population (log)	-0.0759	-0.0760	-1.3281***	-1.4399***	-0.0898	-0.0845	-1.2774***	-1.3567***
	(0.0493)	(0.0513)	(0.2179)	(0.2164)	(0.0547)	(0.0534)	(0.2322)	(0.2163)
share aged >65	5.7955***	5.8673***	9.5740***	9.2919***	6.2092***	6.4804***	11.2675***	10.7552***
	(1.4128)	(1.4783)	(2.4926)	(2.3691)	(1.4882)	(1.6026)	(2.8156)	(2.6454)
share aged 5-17	-2.8257***	-2.7581***	-4.3689***	-4.2336***	-3.0785***	-3.1542***	-5.2538***	-5.0662***
	(0.9595)	(0.8930)	(1.2975)	(1.2502)	(1.0959)	(1.0652)	(1.4183)	(1.3439)
unemployment rate	-0.0048	0.0032	-0.0017	-0.0013	-0.0077	0.0043	-0.0034	-0.0053
	(0.0178)	(0.0188)	(0.0135)	(0.0131)	(0.0240)	(0.0246)	(0.0168)	(0.0158)
natural disaster	0.0292	0.0360	0.0201	0.0212	0.1043	0.1183	0.0476	0.0540
	(0.0341)	(0.0353)	(0.0152)	(0.0151)	(0.1073)	(0.1115)	(0.0590)	(0.0592)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.6485	0.6396	0.9189	0.9205	0.6712	0.6691	0.9486	0.9502

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B5. Testing the swing voter hypothesis (targetable spending)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.
share independents	0.4078		-0.4018		0.2909		-0.3337	
	-0.5614		-0.3384		(0.6098)		(0.5771)	
standard deviation of Democratic vote		0.3528		-0.8402		0.5181		-0.8957
		-0.9516		-0.5333		(0.9824)		(0.5653)
income	0.0218	0.0297	-0.031	-0.0295	0.0233	0.0298	-0.0330	-0.0320
	-0.0258	-0.0249	-0.0375	-0.0364	(0.0257)	(0.0247)	(0.0423)	(0.0410)
population (log)	-0.0656	-0.0791	-0.5504*	-0.5474*	-0.0730	-0.0815*	-0.4362	-0.4528
	-0.0507	-0.0486	-0.2818	-0.2792	(0.0526)	(0.0476)	(0.3429)	(0.3386)
share aged >65	-2.9507**	-2.8717**	5.4185**	5.5689**	-2.7422**	-2.6691**	6.8382**	7.0395**
	-1.3476	-1.3915	-2.3877	-2.2291	(1.2439)	(1.2997)	(2.9018)	(2.7542)
share aged 5-17	0.9882	0.8085	-2.4858**	-2.3899**	1.2085	1.0606	-2.9684**	-2.9952**
	-0.9199	-0.9426	-1.0084	-0.9767	(0.8945)	(0.9115)	(1.3861)	(1.3395)
unemployment rate	-0.0286	-0.0295	-0.0141	-0.0123	-0.0325	-0.0332	-0.0170	-0.0148
	-0.0178	-0.0185	-0.0135	-0.0128	(0.0229)	(0.0233)	(0.0166)	(0.0159)
natural disaster	0.036	0.0365	0.0237	0.0241	0.0957	0.0905	0.0399	0.0423
	-0.0326	-0.0334	-0.0161	-0.0164	(0.0896)	(0.0913)	(0.0634)	(0.0640)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1126	1126	1126	1126	322	322	322	322
R-squared	0.5726	0.5701	0.8942	0.8948	0.6003	0.5998	0.9231	0.9241

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B6. Testing the battleground hypothesis (targetable spending)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.
closeness (poll data)	-0.7512*		0.0997		-0.7076		0.1849	
	-0.4338		-0.1929		(0.4732)		(0.1984)	
closeness (voting data)		-0.294		0.0612		-0.4850*		-0.0090
		-0.2593		-0.1584		(0.2786)		(0.1721)
income	0.0336	0.0269	-0.0312	-0.0296	0.0323	0.0266	-0.0333	-0.0335
	-0.0254	-0.0255	-0.0382	-0.0391	(0.0252)	(0.0249)	(0.0428)	(0.0434)
population (log)	-0.0771	-0.071	-0.5414*	-0.5584*	-0.0786*	-0.0692	-0.4358	-0.4274
	-0.0466	-0.05	-0.2861	-0.2893	(0.0463)	(0.0474)	(0.3439)	(0.3546)
share aged >65	-2.5686*	-2.6232*	5.6880**	5.6525**	-2.2821	-2.1297	7.0315**	7.0978**
	-1.4371	-1.413	-2.3234	-2.3329	(1.3716)	(1.3712)	(2.8416)	(2.8179)
share aged 5-17	0.2258	0.5542	-2.5545**	-2.5861**	0.6425	0.7300	-3.0208**	-3.0913**
	-1.1282	-0.963	-1.013	-0.9935	(1.0522)	(0.9692)	(1.3752)	(1.3554)
unemployment rate	-0.0340*	-0.0268	-0.0131	-0.0134	-0.0387*	-0.0279	-0.0154	-0.0170
	-0.0184	-0.0185	-0.0137	-0.0138	(0.0230)	(0.0232)	(0.0168)	(0.0169)
natural disaster	0.0316	0.0362	0.0242	0.0241	0.0833	0.0973	0.0413	0.0373
	-0.0326	-0.0334	-0.0162	-0.0159	(0.0889)	(0.0908)	(0.0622)	(0.0615)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1126	1126	1126	1126	322	322	322	322
R-squared	0.5826	0.5721	0.8938	0.8938	0.6096	0.6053	0.9232	0.9229

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B7. Testing the partizanship hypothesis (targetable spending)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.
partisan alignment	-0.3352		0.1755		-0.2313		0.1470	
	(0.4050)		(0.1851)		(0.4342)		(0.1816)	
vote share of incumbent president		0.1410		-0.0376		0.5006**		0.0840
		(0.2615)		(0.1897)		(0.2469)		(0.1704)
income	0.0259	0.0281	-0.0311	-0.0307	0.0256	0.0285	-0.0323	-0.0329
	(0.0248)	(0.0254)	(0.0379)	(0.0381)	(0.0248)	(0.0251)	(0.0428)	(0.0431)
population (log)	-0.0743	-0.0780	-0.5262*	-0.5419*	-0.0788	-0.0785	-0.4214	-0.4256
	(0.0486)	(0.0489)	(0.2924)	(0.2858)	(0.0487)	(0.0474)	(0.3505)	(0.3438)
share aged >65	-3.0025**	-2.8469**	5.6803**	5.7007**	-2.8100**	-2.4997*	7.0922**	7.0680**
	(1.3283)	(1.3822)	(2.3241)	(2.3275)	(1.2239)	(1.2951)	(2.8052)	(2.8163)
share aged 5-17	0.9218	0.7886	-2.5735**	-2.5975**	1.1540	0.9717	-3.0603**	-3.0808**
	(0.8971)	(0.9245)	(0.9907)	(0.9926)	(0.8684)	(0.8944)	(1.3520)	(1.3520)
unemployment rate	-0.0307	-0.0290	-0.0129	-0.0135	-0.0343	-0.0313	-0.0161	-0.0172
	(0.0186)	(0.0185)	(0.0134)	(0.0136)	(0.0239)	(0.0229)	(0.0165)	(0.0167)
natural disaster	0.0387	0.0360	0.0220	0.0240	0.0968	0.0892	0.0348	0.0362
	(0.0331)	(0.0334)	(0.0157)	(0.0161)	(0.0899)	(0.0894)	(0.0617)	(0.0618)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1126	1126	1126	1126	322	322	322	322
R-squared	0.5718	0.5700	0.8941	0.8937	0.5999	0.6016	0.9231	0.9229

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B8. Testing swing, battleground and partizanship hypotheses in one equation (targetable spending)

Dependent Variable	(1) targetable sp.	(2) targetable sp.	(3) targetable sp.	(4) targetable sp.	(5) targetable sp.	(6) targetable sp.	(7) targetable sp.	(8) targetable sp.
share independents	0.3250		-0.3074		0.2309		-0.2589	
	(0.5095)		(0.3760)		(0.5515)		(0.5935)	
closeness (poll data)	-0.7451*		0.0857		-0.7103		0.1902	
	(0.4295)		(0.1905)		(0.4729)		(0.2008)	
partisan alignment	-0.1254		0.1207		-0.1116		0.1305	
	(0.2639)		(0.2138)		(0.2785)		(0.2027)	
standard deviation of Democratic vote		0.3481		-0.8733		0.1908		-0.9600
		(0.9436)		(0.5429)		(0.9556)		(0.6028)
closeness (voting data)		-0.4401		0.1130		-0.4470		-0.0130
		(0.3659)		(0.2487)		(0.3288)		(0.2631)
vote share of incumbent president		-0.3775		0.1504		0.0726		0.1410
		(0.4005)		(0.2945)		(0.2914)		(0.2769)
income	0.0273	0.0284	-0.0313	-0.0279	0.0274	0.0274	-0.0321	-0.0313
	(0.0263)	(0.0250)	(0.0374)	(0.0381)	(0.0265)	(0.0249)	(0.0426)	(0.0440)
population (log)	-0.0646	-0.0699	-0.5415*	-0.5710*	-0.0696	-0.0695	-0.4328	-0.4427
	(0.0485)	(0.0502)	(0.2869)	(0.2855)	(0.0508)	(0.0479)	(0.3445)	(0.3525)
share aged >65	-2.6799*	-2.5694*	5.4412**	5.5044**	-2.4102*	-2.1485	6.8303**	6.9987**
	(1.3941)	(1.4353)	(2.3952)	(2.2237)	(1.3081)	(1.3741)	(2.9320)	(2.7579)
share aged 5-17	0.4053	0.4925	-2.4466**	-2.3801**	0.7815	0.7356	-2.8985**	-2.9756**
	(1.1057)	(0.9960)	(1.0249)	(0.9688)	(1.0379)	(0.9715)	(1.4126)	(1.3408)
unemployment rate	-0.0343*	-0.0262	-0.0131	-0.0122	-0.0398*	-0.0284	-0.0147	-0.0151
	(0.0176)	(0.0188)	(0.0134)	(0.0130)	(0.0226)	(0.0234)	(0.0165)	(0.0159)
natural disaster	0.0313	0.0383	0.0231	0.0237	0.0854	0.0948	0.0411	0.0406
	(0.0325)	(0.0337)	(0.0162)	(0.0163)	(0.0893)	(0.0926)	(0.0647)	(0.0645)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1126	1126	1126	1126	322	322	322	322
R-squared	0.5855	0.5730	0.8944	0.8949	0.6111	0.6054	0.9236	0.9243

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B9. Testing the swing voter hypothesis (grants)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	grants	grants	grants	grants	grants	grants	grants	grants
share independents	0.1149		0.0949		0.1313		0.2281	
	(0.2252)		(0.1038)		(0.2524)		(0.1516)	
standard deviation of Democratic vote		-0.1775		-0.0229		-0.0449		0.0714
		(0.2568)		(0.1419)		(0.2723)		(0.1650)
income	0.0084	0.0096	-0.0063	-0.0063	0.0085	0.0105	-0.0073	-0.0071
	(0.0106)	(0.0089)	(0.0066)	(0.0068)	(0.0109)	(0.0091)	(0.0085)	(0.0087)
population (log)	-0.0647**	-0.0685***	-0.3512***	-0.3543***	-0.0664**	-0.0706***	-0.3498***	-0.3521***
	(0.0260)	(0.0221)	(0.0676)	(0.0672)	(0.0269)	(0.0230)	(0.0757)	(0.0742)
share aged >65	0.5511	0.5721	1.5890**	1.5133**	0.6392	0.6737	2.0903**	1.9201**
	(0.6864)	(0.6990)	(0.6027)	(0.6024)	(0.6827)	(0.7118)	(0.8382)	(0.8245)
share aged 5-17	0.1444	0.1005	-0.6620**	-0.6289**	0.0762	0.0186	-0.8951**	-0.8197*
	(0.3547)	(0.3866)	(0.2979)	(0.2991)	(0.3556)	(0.3900)	(0.4414)	(0.4363)
unemployment rate	0.0168***	0.0169***	0.0081***	0.0081***	0.0184***	0.0186***	0.0095**	0.0093**
	(0.0045)	(0.0047)	(0.0026)	(0.0026)	(0.0054)	(0.0056)	(0.0039)	(0.0039)
natural disaster	0.0102	0.0108	-0.0007	-0.0007	0.0350	0.0348	-0.0065	-0.0051
	(0.0075)	(0.0075)	(0.0033)	(0.0033)	(0.0239)	(0.0239)	(0.0127)	(0.0130)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.5838	0.5825	0.9245	0.9243	0.6189	0.6167	0.9457	0.9448

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B10. Testing the battleground hypothesis (grants)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	grants	grants	grants	grants	grants	grants	grants	grants
closeL1	-0.1245		0.0496		-0.1231		0.0173	
	(0.1144)		(0.0422)		(0.1288)		(0.0591)	
closeness (voting data)		-0.0408		-0.0836*		-0.1239		-0.1230**
		(0.1030)		(0.0424)		(0.1383)		(0.0520)
income	0.0111	0.0099	-0.0064	-0.0076	0.0114	0.0103	-0.0070	-0.0095
	(0.0087)	(0.0083)	(0.0068)	(0.0071)	(0.0087)	(0.0083)	(0.0087)	(0.0084)
population (log)	-0.0680***	-0.0668***	-0.3551***	-0.3212***	-0.0699***	-0.0672***	-0.3544***	-0.3162***
	(0.0221)	(0.0224)	(0.0672)	(0.0663)	(0.0231)	(0.0236)	(0.0740)	(0.0757)
share aged >65	0.6340	0.5797	1.5013**	1.3673**	0.7403	0.8105	1.9100**	1.9733**
	(0.7084)	(0.7205)	(0.5980)	(0.5817)	(0.7209)	(0.7699)	(0.8289)	(0.8436)
share aged 5-17	0.0088	0.0742	-0.6116**	-0.5519*	-0.0586	-0.0716	-0.8057*	-0.8326*
	(0.3855)	(0.3770)	(0.2943)	(0.2846)	(0.3975)	(0.4130)	(0.4364)	(0.4430)
unemployment rate	0.0158***	0.0171***	0.0083***	0.0075***	0.0174***	0.0197***	0.0096**	0.0089**
	(0.0048)	(0.0048)	(0.0026)	(0.0025)	(0.0058)	(0.0058)	(0.0038)	(0.0037)
natural disaster	0.0097	0.0107	-0.0004	0.0000	0.0325	0.0351	-0.0044	-0.0056
	(0.0076)	(0.0074)	(0.0034)	(0.0033)	(0.0246)	(0.0238)	(0.0130)	(0.0131)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1222	1174	1222	322	322	322	322
R-squared	0.5851	0.5777	0.9245	0.9213	0.6194	0.6201	0.9448	0.9465

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B11. Testing the partizanship hypothesis (grants)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	grants	grants	grants	grants	grants	grants	grants	grants
partisan alignment	-0.0065		0.0666		-0.0006		0.0641	
	(0.1432)		(0.0447)		(0.1890)		(0.0518)	
vote share of incumbent president		0.0535		0.0913**		0.1030		0.1053**
		(0.0788)		(0.0427)		(0.1530)		(0.0493)
income	0.0102	0.0100	-0.0062	-0.0060	0.0106	0.0107	-0.0065	-0.0064
	(0.0091)	(0.0084)	(0.0069)	(0.0073)	(0.0096)	(0.0087)	(0.0089)	(0.0088)
population (log)	-0.0683***	-0.0675***	-0.3496***	-0.3416***	-0.0705***	-0.0698***	-0.3501***	-0.3482***
	(0.0231)	(0.0216)	(0.0683)	(0.0639)	(0.0242)	(0.0225)	(0.0743)	(0.0723)
share aged >65	0.5699	0.5518	1.5009**	1.3168**	0.6731	0.7078	1.9151**	1.8837**
	(0.6784)	(0.6925)	(0.6006)	(0.5705)	(0.6720)	(0.7060)	(0.8203)	(0.8198)
share aged 5-17	0.0964	0.0956	-0.6138**	-0.5411*	0.0173	-0.0046	-0.7993*	-0.8009*
	(0.3719)	(0.3776)	(0.2984)	(0.2790)	(0.3676)	(0.3851)	(0.4373)	(0.4316)
unemployment rate	0.0167***	0.0168***	0.0082***	0.0074***	0.0185***	0.0187***	0.0098**	0.0092**
	(0.0051)	(0.0047)	(0.0025)	(0.0025)	(0.0062)	(0.0056)	(0.0037)	(0.0037)
natural disaster	0.0105	0.0106	-0.0014	0.0001	0.0344	0.0333	-0.0059	-0.0062
	(0.0075)	(0.0071)	(0.0033)	(0.0032)	(0.0236)	(0.0235)	(0.0133)	(0.0130)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1222	1174	1222	322	322	322	322
R-squared	0.5817	0.5775	0.9248	0.9210	0.6166	0.6175	0.9451	0.9456

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table B12. Testing swing, battleground and partizanship hypotheses in one equation (grants)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	grants	grants	grants	grants	grants	grants	grants	grants
share independents	0.1661		0.1628		0.1968		0.2778*	
	(0.2137)		(0.1120)		(-0.2258)		(-0.1522)	
closeness (poll data)	-0.1252		0.0539		-0.1227		0.0251	
	(0.1105)		(0.0425)		(-0.1244)		(-0.0583)	
partisan alignment	0.0888		0.0920*		0.1103		0.0948*	
	(0.0692)		(0.0503)		(-0.1125)		(-0.0555)	
standard deviation of Democratic vote		-0.2051		-0.0729		-0.135		-0.014
		(0.2609)		(0.1403)		(-0.2854)		(-0.1602)
closeness (voting data)		-0.0619		-0.0699		-0.1378		-0.1168
		(0.1762)		(0.0763)		(-0.1789)		(-0.081)
vote share of incumbent president		0.0015		0.0331		-0.014		0.0141
		(0.1919)		(0.0796)		(-0.1992)		(-0.0832)
income	0.0092	0.0092	-0.0062	-0.0079	0.0094	0.0098	-0.0066	-0.0093
	(0.0107)	(0.0087)	(0.0067)	(0.0065)	(-0.011)	(-0.0087)	(-0.0087)	(-0.0083)
population (log)	-0.0639**	-0.0667***	-0.3447***	-0.3304***	-0.0653**	-0.0672***	-0.3440***	-0.3177***
	(0.0257)	(0.0233)	(0.0704)	(0.0707)	(-0.0266)	(-0.0238)	(-0.0769)	(-0.0777)
share aged >65	0.6429	0.6326	1.6025***	1.5582**	0.7573	0.8219	2.1190**	1.9652**
	(0.6847)	(0.7435)	(0.5891)	(0.6104)	(-0.6826)	(-0.7739)	(-0.8218)	(-0.8485)
share aged 5-17	0.0573	0.0550	-0.6323**	-0.6365**	-0.0062	-0.0742	-0.8848**	-0.8285*
	(0.3519)	(0.3910)	(0.2906)	(0.3026)	(-0.362)	(-0.4149)	(-0.4357)	(-0.4463)
unemployment rate	0.0165***	0.0174***	0.0089***	0.0078***	0.0182***	0.0200***	0.0103***	0.0089**
	(0.0046)	(0.0048)	(0.0025)	(0.0025)	(-0.0055)	(-0.0058)	(-0.0037)	(-0.0037)
natural disaster	0.0088	0.0107	-0.0014	-0.0015	0.0322	0.0364	-0.008	-0.0056
	(0.0076)	(0.0070)	(0.0034)	(0.0033)	(-0.0248)	(-0.0228)	(-0.0136)	(-0.0135)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.5881	0.5835	0.9257	0.9255	0.6227	0.6205	0.9464	0.9465

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C1. Testing the swing voter hypothesis (federal expenditure)

Dependent Variable	(1) federal exp.	(2) federal exp.	(3) federal exp.	(4) federal exp.	(5) federal exp.	(6) federal exp.	(7) federal exp.	(8) federal exp.
share independents	0.4776		-0.1799		0.3453		0.4262	
	(0.5957)		(0.4168)		(0.3819)		(0.4001)	
standard deviation of Democratic vote		2.7774**		0.6983		3.2506***		0.9364*
		(1.0784)		(0.6794)		(0.9558)		(0.5499)
income	0.0042	0.0107	-0.0622*	-0.0610*	0.0036	0.0097	-0.0796***	-0.0783***
	(0.0240)	(0.0230)	(0.0319)	(0.0328)	(0.0140)	(0.0137)	(0.0237)	(0.0235)
population (log)	0.0485	0.0418	-1.5012***	-1.4835***	0.0358	0.0330	-1.4749***	-1.4413***
	(0.0561)	(0.0555)	(0.1928)	(0.1935)	(0.0318)	(0.0321)	(0.1461)	(0.1413)
share aged >65	6.0432***	5.9765***	6.3129***	6.2792**	6.2267***	6.1888***	7.5139***	7.0977***
	(1.4541)	(1.4498)	(2.3058)	(2.3769)	(0.8795)	(0.8551)	(1.9991)	(1.9679)
share aged 5-17	-2.0802**	-2.3800***	-3.3550***	-3.4080***	-2.3814***	-2.6757***	-4.1678***	-4.0566***
	(0.8093)	(0.8595)	(1.1778)	(1.2228)	(0.6594)	(0.6759)	(1.0079)	(1.0064)
unemployment rate	0.0201	0.0203	-0.0080	-0.0078	0.0185	0.0175	-0.0166	-0.0180
	(0.0127)	(0.0126)	(0.0112)	(0.0115)	(0.0134)	(0.0134)	(0.0118)	(0.0119)
natural disaster	0.0279	0.0212	0.0189	0.0175	0.0873	0.0610	0.0559	0.0530
	(0.0240)	(0.0237)	(0.0132)	(0.0132)	(0.0807)	(0.0801)	(0.0410)	(0.0407)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.7520	0.7573	0.9267	0.9270	0.7741	0.7824	0.9569	0.9572

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C2. Testing the battleground hypothesis (federal expenditure)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.
closeness (poll data)	-0.7230*		-0.0800		-0.7507***		-0.2202	
	(0.3744)		(0.2197)		(0.2686)		(0.1682)	
closeness (voting data)		-0.2753		0.0781		-0.5500**		-0.1000
		(0.2679)		(0.1468)		(0.2547)		(0.1370)
income	0.0116	0.0079	-0.0621*	-0.0622*	0.0100	0.0077	-0.0769***	-0.0789***
	(0.0232)	(0.0224)	(0.0320)	(0.0326)	(0.0132)	(0.0130)	(0.0232)	(0.0235)
population (log)	0.0379	0.0452	-1.4902***	-1.5193***	0.0270	0.0380	-1.4516***	-1.4425***
	(0.0536)	(0.0556)	(0.1996)	(0.2062)	(0.0313)	(0.0312)	(0.1474)	(0.1589)
share aged >65	6.2483***	6.0987***	6.4303***	6.3273**	6.5538***	6.5499***	7.4937***	7.4018***
	(1.4508)	(1.5014)	(2.3652)	(2.3586)	(0.8935)	(0.9377)	(1.9991)	(2.0053)
share aged 5-17	-2.6378***	-2.3044***	-3.4144***	-3.3662***	-2.9042***	-2.6926***	-4.1680***	-4.0912***
	(0.8756)	(0.8386)	(1.1980)	(1.2002)	(0.6988)	(0.7229)	(1.0167)	(1.0185)
unemployment rate	0.0150	0.0218*	-0.0079	-0.0076	0.0120	0.0239*	-0.0176	-0.0170
	(0.0141)	(0.0127)	(0.0114)	(0.0114)	(0.0142)	(0.0135)	(0.0115)	(0.0116)
natural disaster	0.0241	0.0252	0.0184	0.0196	0.0775	0.0820	0.0540	0.0576
	(0.0231)	(0.0241)	(0.0130)	(0.0132)	(0.0799)	(0.0807)	(0.0414)	(0.0410)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.7571	0.7515	0.9267	0.9267	0.7795	0.7780	0.9569	0.9568

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C3. Testing the partizanship hypothesis (federal expenditure)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.
partisan alignment	0.0579		0.3436**		0.2045		0.3865**	
	(0.3008)		(0.1547)		(0.2719)		(0.1614)	
vote share of incumbent president		0.3740*		0.1597		0.6615*		0.3066*
		(0.1929)		(0.1445)		(0.358)		(0.1708)
income	0.0071	0.007	-0.0622*	-0.0619*	0.0067	0.0061	-0.0777***	-0.0775***
	-0.0228	(0.0227)	(0.032)	(0.0323)	(0.0134)	(0.0132)	(0.0234)	(0.0236)
population (log)	0.0421	0.0438	-1.4692***	-1.4901***	0.0314	0.0355	-1.4366***	-1.4526***
	-0.0569	(0.0562)	(0.1962)	(0.193)	(0.0321)	(0.031)	(0.1458)	(0.1449)
share aged >65	5.9319***	5.9335***	6.3734***	6.3825***	6.2119***	6.2199***	7.4088***	7.2969***
	-1.4763	(1.4766)	(2.3286)	(2.3721)	(0.8876)	(0.8818)	(1.9583)	(1.9605)
share aged 5-17	-2.1794**	-2.1956**	-3.3276***	-3.3864***	-2.4706***	-2.4989***	-4.0428***	-4.0697***
	-0.8198	(0.829)	(1.1818)	(1.2027)	(0.6765)	(0.672)	(0.9924)	(0.9976)
unemployment rate	0.0202	0.0202	-0.0069	-0.0079	0.0208	0.0197	-0.0152	-0.0179
	-0.0127	(0.0131)	(0.0109)	(0.0113)	(0.0136)	(0.0135)	(0.0114)	(0.0116)
natural disaster	0.0267	0.0231	0.0151	0.0172	0.0799	0.0723	0.0509	0.0539
	-0.0256	(0.0237)	(0.0128)	(0.013)	(0.0816)	(0.0799)	(0.0408)	(0.0408)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.7503	0.7513	0.9277	0.9269	0.7737	0.7762	0.9577	0.9573

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C4. Testing swing, battleground and partizanship hypotheses in one equation (federal expenditure)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.	federal exp.
share independents	0.6228		0.0393		0.5680		0.6684*	
	(0.6207)		(0.4389)		(0.4404)		(0.3999)	
closeness (poll data)	-0.6997*		-0.0666		-0.7272***		-0.1861	
	(0.3728)		(0.2182)		(0.2658)		(0.1631)	
partisan alignment	0.3129		0.3479**		0.3993		0.4425***	
	(0.2032)		(0.1706)		(0.3079)		(0.1602)	
standard deviation of Democratic vote		2.6906**		0.5538		2.8330***		0.7615
		(1.0874)		(0.6712)		(0.9451)		(0.5657)
closeness (voting data)		-0.1924		0.3645		-0.3683		0.1464
		(0.3772)		(0.2537)		(0.2635)		(0.1792)
vote share of incumbent president		-0.0702		0.4347*		0.0527		0.3624
		(0.3593)		(0.2548)		(0.3601)		(0.2239)
income	0.0095	0.0113	-0.0619*	-0.0581*	0.0090	0.0107	-0.0787***	-0.0764***
	(0.0245)	(0.0228)	(0.0316)	(0.0346)	(0.0137)	(0.0135)	(0.0235)	(0.0242)
population (log)	0.0453	0.0436	-1.4616***	-1.5625***	0.0329	0.0373	-1.4109***	-1.4688***
	(0.0528)	(0.0550)	(0.2013)	(0.2109)	(0.0305)	(0.0316)	(0.1492)	(0.1521)
share aged >65	6.4462***	6.0959***	6.4227***	5.9790**	6.7646***	6.4531***	7.8115***	7.0033***
	(1.4731)	(1.5102)	(2.2628)	(2.3060)	(0.9285)	(0.9107)	(1.9827)	(1.9827)
share aged 5-17	-2.5365***	-2.4611***	-3.3579***	-3.3192***	-2.8445***	-2.8178***	-4.2668***	-4.0293***
	(0.8619)	(0.8847)	(1.1502)	(1.1800)	(0.6935)	(0.7140)	(0.9922)	(1.0037)
unemployment rate	0.0172	0.0215*	-0.0070	-0.0083	0.0151	0.0211	-0.0155	-0.0188
	(0.0135)	(0.0120)	(0.0112)	(0.0115)	(0.0138)	(0.0133)	(0.0116)	(0.0118)
natural disaster	0.0218	0.0207	0.0146	0.0164	0.0738	0.0614	0.0417	0.0500
	(0.0229)	(0.0230)	(0.0129)	(0.0130)	(0.0800)	(0.0805)	(0.0411)	(0.0408)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.7593	0.7577	0.9277	0.9277	0.7812	0.7846	0.9584	0.9576

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C5. Testing the swing voter hypothesis (targetable spending)

Dependent Variable	(1) targetable sp.	(2) targetable sp.	(3) targetable sp.	(4) targetable sp.	(5) targetable sp.	(6) targetable sp.	(7) targetable sp.	(8) targetable sp.
share independents	0.5119		-0.1135		0.2752		0.1452	
	(0.5862)		(0.3482)		(0.3458)		(0.3881)	
standard deviation of Democratic vote		2.0781**		0.4931		2.4684***		0.7083
		(1.0299)		(0.6943)		(0.8456)		(0.5137)
income	0.0188	0.0247	-0.0425	-0.0411	0.0181	0.0229*	-0.0524**	-0.0520**
	(0.0227)	(0.0212)	(0.0379)	(0.0390)	(0.0126)	(0.0121)	(0.0261)	(0.0260)
population (log)	0.0668	0.0585	-0.7998***	-0.7925***	0.0545*	0.0523*	-0.8204***	-0.7949***
	(0.0500)	(0.0503)	(0.2382)	(0.2366)	(0.0282)	(0.0279)	(0.1803)	(0.1752)
share aged >65	-2.5088*	-2.5926**	0.6388	0.6266	-2.3524***	-2.3838***	1.0351	0.7909
	(1.2651)	(1.2825)	(1.8944)	(1.8769)	(0.6794)	(0.6730)	(1.9334)	(1.9143)
share aged 5-17	1.2989*	1.0293	-0.7136	-0.7646	1.3019**	1.0761**	-0.8414	-0.7966
	(0.7326)	(0.7898)	(0.7738)	(0.7876)	(0.5060)	(0.5151)	(0.9456)	(0.9455)
unemployment rate	-0.0027	-0.0027	-0.0156	-0.0155	-0.0081	-0.0089	-0.0210*	-0.0220*
	(0.0119)	(0.0120)	(0.0114)	(0.0115)	(0.0123)	(0.0122)	(0.0114)	(0.0115)
natural disaster	0.0310	0.0267	0.0221	0.0214	0.0941	0.0740	0.0644	0.0611
	(0.0237)	(0.0236)	(0.0139)	(0.0139)	(0.0680)	(0.0685)	(0.0392)	(0.0390)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1126	1126	1126	1126	322	322	322	322
R-squared	0.7232	0.7258	0.9049	0.9051	0.7515	0.7584	0.9380	0.9383

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C6. Testing the battleground hypothesis (targetable spending)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.
closeness (poll data)	-0.5500*		-0.0860		-0.5023**		-0.0909	
	(0.3225)		(0.1991)		(0.2247)		(0.1615)	
closeness (voting data)		-0.0820		0.1153		-0.3609*		-0.0027
		(0.2547)		(0.1717)		(0.2156)		(0.1312)
income	0.0250	0.0220	-0.0423	-0.0426	0.0227*	0.0212*	-0.0514**	-0.0519**
	(0.0215)	(0.0209)	(0.0379)	(0.0387)	(0.0119)	(0.0118)	(0.0259)	(0.0262)
population (log)	0.0565	0.0605	-0.7879***	-0.8248***	0.0481*	0.0554**	-0.8107***	-0.8196***
	(0.0490)	(0.0506)	(0.2418)	(0.2450)	(0.0275)	(0.0273)	(0.1788)	(0.1871)
share aged >65	-2.4420*	-2.6063**	0.7208	0.5606	-2.1425***	-2.1502***	1.0390	0.9787
	(1.2809)	(1.2928)	(1.9180)	(1.9397)	(0.7064)	(0.7189)	(1.9262)	(1.9293)
share aged 5-17	0.8036	1.1423	-0.7606	-0.6887	0.9442*	1.0890**	-0.8483	-0.8096
	(0.8274)	(0.7533)	(0.7999)	(0.7915)	(0.5377)	(0.5474)	(0.9504)	(0.9484)
unemployment rate	-0.0065	-0.0023	-0.0156	-0.0154	-0.0124	-0.0045	-0.0214*	-0.0211*
	(0.0134)	(0.0120)	(0.0115)	(0.0115)	(0.0128)	(0.0124)	(0.0114)	(0.0114)
natural disaster	0.0274	0.0298	0.0215	0.0229	0.0872	0.0902	0.0635	0.0653*
	(0.0230)	(0.0239)	(0.0138)	(0.0138)	(0.0676)	(0.0682)	(0.0395)	(0.0392)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1126	1126	1126	1126	322	322	322	322
R-squared	0.7259	0.7206	0.9049	0.9050	0.7549	0.7538	0.9380	0.9379

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C7. Testing the partizanship hypothesis (targetable spending)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.
partisan alignment	-0.1824		0.0935		0.0095		0.1173	
	(0.3223)		(0.1816)		(0.2434)		(0.1456)	
vote share of incumbent president		0.0635		-0.0805		0.3672		0.0626
		(0.1981)		(0.1913)		(0.2803)		(0.1599)
income	0.0210	0.0216	-0.0424	-0.0432	0.0198	0.0200*	-0.0518**	-0.0518**
	(0.0213)	(0.0210)	(0.0380)	(0.0382)	(0.0122)	(0.0118)	(0.0261)	(0.0261)
population (log)	0.0603	0.0600	-0.7899***	-0.8009***	0.0514*	0.0535*	-0.8088***	-0.8159***
	(0.0509)	(0.0511)	(0.2480)	(0.2411)	(0.0280)	(0.0274)	(0.1802)	(0.1787)
share aged >65	-2.6677**	-2.6491**	0.6881	0.6404	-2.4057***	-2.3732***	0.9968	0.9676
	(1.2945)	(1.2868)	(1.9112)	(1.9107)	(0.7011)	(0.6844)	(1.9211)	(1.9187)
share aged 5-17	1.2313	1.1853	-0.7289	-0.7062	1.2519**	1.2219**	-0.7999	-0.8084
	(0.7391)	(0.7524)	(0.7767)	(0.7839)	(0.5128)	(0.5099)	(0.9435)	(0.9451)
unemployment rate	-0.0040	-0.0029	-0.0152	-0.0154	-0.0077	-0.0073	-0.0206*	-0.0213*
	(0.0121)	(0.0126)	(0.0112)	(0.0114)	(0.0124)	(0.0125)	(0.0114)	(0.0114)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
natural disaster	0.0319	0.0297	0.0212	0.0228	0.0916	0.0851	0.0630	0.0644
	(0.0250)	(0.0238)	(0.0138)	(0.0139)	(0.0689)	(0.0682)	(0.0393)	(0.0392)
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1126	1126	1126	1126	322	322	322	322
R-squared	0.7209	0.7205	0.9050	0.9049	0.7509	0.7522	0.9381	0.9380

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C8. Testing swing, battleground and partizanship hypotheses in one equation (targetable spending)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.	targetable sp.
share independents	0.4957		-0.0645		0.3371		0.2202	
	(0.5896)		(0.3708)		(0.3818)		(0.3982)	
closeness (poll data)	-0.5305		-0.0839		-0.4931**		-0.0808	
	(0.3216)		(0.1957)		(0.2246)		(0.1619)	
partisan alignment	0.0320		0.0803		0.1220		0.1343	
	(0.2071)		(0.1890)		(0.2616)		(0.1469)	
standard deviation of Democratic vote		2.1811**		0.5705		2.2643***		0.7258
		(1.0307)		(0.6935)		(0.8436)		(0.5319)
closeness (voting data)		-0.0998		0.1429		-0.2693		0.0788
		(0.3510)		(0.2535)		(0.2270)		(0.1761)
vote share of incumbent president		-0.2402		0.0121		-0.0967		0.0671
		(0.3337)		(0.2815)		(0.2814)		(0.2112)
income	0.0222	0.0253	-0.0419	-0.0406	0.0214*	0.0236*	-0.0520**	-0.0513*
	(0.0233)	(0.0210)	(0.0375)	(0.0400)	(0.0126)	(0.0121)	(0.0260)	(0.0266)
population (log)	0.0633	0.0586	-0.7828***	-0.8249***	0.0518*	0.0547**	-0.7983***	-0.8150***
	(0.0477)	(0.0500)	(0.2480)	(0.2477)	(0.0275)	(0.0276)	(0.1816)	(0.1844)
share aged >65	-2.3095*	-2.5404*	0.7026	0.4756	-2.0460***	-2.2025***	1.1425	0.7304
	(1.2904)	(1.3019)	(1.9309)	(1.9144)	(0.7294)	(0.7098)	(1.9567)	(1.9363)
share aged 5-17	0.9135	0.9876	-0.7506	-0.7233	0.9926*	0.9764*	-0.8822	-0.7813
	(0.8100)	(0.7934)	(0.7942)	(0.7893)	(0.5396)	(0.5485)	(0.9558)	(0.9520)
unemployment rate	-0.0060	-0.0022	-0.0155	-0.0155	-0.0117	-0.0065	-0.0208*	-0.0221*
	(0.0126)	(0.0112)	(0.0114)	(0.0116)	(0.0126)	(0.0121)	(0.0115)	(0.0115)
natural disaster	0.0279	0.0281	0.0208	0.0221	0.0875	0.0761	0.0596	0.0608
	(0.0230)	(0.0235)	(0.0140)	(0.0139)	(0.0679)	(0.0691)	(0.0399)	(0.0393)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1126	1126	1126	1126	322	322	322	322
R-squared	0.7283	0.7261	0.9050	0.9053	0.7556	0.7596	0.9382	0.9384

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C9. Testing the swing voter hypothesis (grants)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	grants	grants	grants	grants	grants	grants	grants	grants
share independents	0.0347		0.0770		0.0224		0.1814*	
	(0.2018)		(0.1015)		(0.1189)		(0.0943)	
standard deviation of Democratic vote		-0.0066		-0.1148		0.2303		-0.0280
		(0.3314)		(0.1737)		(0.3459)		(0.1559)
income	-0.0039	-0.0037	-0.0093	-0.0094	-0.0046	-0.0042	-0.0100	-0.0094
	(0.0103)	(0.0100)	(0.0059)	(0.0062)	(0.0053)	(0.0051)	(0.0065)	(0.0065)
population (log)	-0.0711**	-0.0716***	-0.3881***	-0.3918***	-0.0702***	-0.0704***	-0.4075***	-0.4086***
	(0.0268)	(0.0256)	(0.0726)	(0.0729)	(0.0137)	(0.0130)	(0.0676)	(0.0663)
share aged >65	0.0679	0.0590	1.5672**	1.5520**	0.1306	0.1284	1.9415***	1.8765***
	(0.5418)	(0.5230)	(0.6658)	(0.6781)	(0.3038)	(0.2982)	(0.6390)	(0.6420)
share aged 5-17	0.3814	0.3751	-0.6566*	-0.6400*	0.3799*	0.3594*	-0.8223**	-0.7826**
	(0.3466)	(0.3524)	(0.3324)	(0.3301)	(0.2024)	(0.2092)	(0.3190)	(0.3145)
unemployment rate	0.0108***	0.0108***	0.0047**	0.0046**	0.0099**	0.0098**	0.0053*	0.0053*
	(0.0034)	(0.0035)	(0.0019)	(0.0019)	(0.0043)	(0.0043)	(0.0031)	(0.0031)
natural disaster	0.0109	0.0109*	0.0005	0.0007	0.0326	0.0307	-0.0023	-0.0010
	(0.0067)	(0.0064)	(0.0030)	(0.0030)	(0.0280)	(0.0279)	(0.0108)	(0.0112)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.6538	0.6537	0.9323	0.9322	0.6904	0.6909	0.9551	0.9546

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C10. Testing the battleground hypothesis (grants)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	grants	grants	grants	grants	grants	grants	grants	grants
closeness (poll data)	-0.1662		0.0510		-0.1638*		0.0225	
	(0.1270)		(0.0502)		(0.0949)		(0.0556)	
closeness (voting data)		-0.0117		-0.0768**		-0.0947		-0.1050***
		(0.0918)		(0.0381)		(0.1085)		(0.0383)
income	-0.0026	-0.0041	-0.0094	-0.0087	-0.0035	-0.0041	-0.0096	-0.0101
	(0.0097)	(0.0097)	(0.0061)	(0.0067)	(0.0050)	(0.0050)	(0.0065)	(0.0064)
population (log)	-0.0726***	-0.0706***	-0.3945***	-0.3629***	-0.0715***	-0.0694***	-0.4100***	-0.3733***
	(0.0249)	(0.0250)	(0.0742)	(0.0738)	(0.0130)	(0.0125)	(0.0671)	(0.0704)
share aged >65	0.1340	0.0645	1.5088**	1.4729**	0.2128	0.1938	1.8539***	1.9301***
	(0.5326)	(0.4941)	(0.6619)	(0.6813)	(0.3129)	(0.3194)	(0.6317)	(0.6362)
share aged 5-17	0.2678	0.3601	-0.6250*	-0.5970*	0.2751	0.3329	-0.7724**	-0.8009**
	(0.3524)	(0.3318)	(0.3285)	(0.3330)	(0.2168)	(0.2190)	(0.3129)	(0.3148)
unemployment rate	0.0096***	0.0105***	0.0047**	0.0049**	0.0084*	0.0108**	0.0053*	0.0050*
	(0.0035)	(0.0034)	(0.0019)	(0.0019)	(0.0045)	(0.0044)	(0.0031)	(0.0030)
natural disaster	0.0101	0.0101	0.0008	0.0006	0.0309	0.0320	-0.0007	-0.0022
	(0.0069)	(0.0065)	(0.0031)	(0.0030)	(0.0281)	(0.0279)	(0.0110)	(0.0108)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1222	1174	1222	322	322	322	322
R-squared	0.6585	0.6501	0.9323	0.9294	0.6940	0.6921	0.9546	0.9558

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C11. Testing the partizanship hypothesis (grants)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	grants	grants	grants	grants	grants	grants	grants	grants
partisan alignment	0.0303		0.0679*		0.0627		0.0649	
	(0.1041)		(0.0382)		(0.1157)		(0.0496)	
vote share of incumbent president		0.0584		0.0939**		0.1363		0.1030**
		(0.0717)		(0.0440)		(0.1579)		(0.0504)
income	-0.0036	-0.0041	-0.0091	-0.0082	-0.0041	-0.0044	-0.0093	-0.0092
	(0.0100)	(0.0096)	(0.0062)	(0.0066)	(0.0053)	(0.0050)	(0.0065)	(0.0064)
population (log)	-0.0716***	-0.0704***	-0.3837***	-0.3792***	-0.0706***	-0.0697***	-0.4011***	-0.4000***
	(0.0257)	(0.0249)	(0.0738)	(0.0703)	(0.0131)	(0.0126)	(0.0657)	(0.0641)
share aged >65	0.0641	0.0606	1.5301**	1.4533**	0.1430	0.1391	1.8801***	1.8534***
	(0.5229)	(0.5185)	(0.6770)	(0.6662)	(0.3031)	(0.3032)	(0.6253)	(0.6235)
share aged 5-17	0.3713	0.3611	-0.6324*	-0.6018*	0.3673*	0.3643*	-0.7769**	-0.7809**
	(0.3568)	(0.3547)	(0.3375)	(0.3253)	(0.2077)	(0.2078)	(0.3121)	(0.3094)
unemployment rate	0.0110***	0.0104***	0.0047**	0.0046**	0.0105**	0.0101**	0.0055*	0.0049
	(0.0036)	(0.0034)	(0.0018)	(0.0019)	(0.0044)	(0.0043)	(0.0030)	(0.0030)
natural disaster	0.0105	0.0098	-0.0002	0.0007	0.0310	0.0299	-0.0025	-0.0027
	(0.0066)	(0.0061)	(0.0029)	(0.0029)	(0.0279)	(0.0278)	(0.0112)	(0.0110)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1222	1174	1222	322	322	322	322
R-squared	0.6538	0.6504	0.9326	0.9294	0.6908	0.6919	0.9549	0.9554

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.

Table C12. Testing swing, battleground and partizanship hypotheses in one equation (grants)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	grants	grants	grants	grants	grants	grants	grants	grants
share independents	0.0566		0.1380		0.0682		0.2270**	
	(0.1946)		(0.1053)		(0.1251)		(0.0947)	
closeness (poll data)	-0.1641		0.0566		-0.1596*		0.0280	
	(0.1277)		(0.0504)		(0.0935)		(0.0534)	
partisan alignment	0.0532		0.0910**		0.0825		0.0907*	
	(0.0605)		(0.0442)		(0.1266)		(0.0494)	
standard deviation of Democratic vote		-0.0523		-0.2113		0.1153		-0.1530
		(0.3350)		(0.1812)		(0.2987)		(0.1519)
closeness (voting data)		0.0004		-0.0491		-0.0612		-0.0910*
		(0.1480)		(0.0642)		(0.1179)		(0.0540)
vote share of incumbent president		0.0568		0.0655		0.0698		0.0425
		(0.1494)		(0.0739)		(0.1696)		(0.0692)
income	-0.0027	-0.0037	-0.0096	-0.0095	-0.0034	-0.0040	-0.0102	-0.0099
	(0.0104)	(0.0100)	(0.0061)	(0.0065)	(0.0055)	(0.0051)	(0.0066)	(0.0064)
population (log)	-0.0720***	-0.0713***	-0.3851***	-0.3772***	-0.0709***	-0.0693***	-0.4015***	-0.3803***
	(0.0259)	(0.0256)	(0.0798)	(0.0771)	(0.0133)	(0.0125)	(0.0687)	(0.0722)
share aged >65	0.1560	0.0595	1.5615**	1.6042**	0.2467	0.1777	1.9560***	1.9557***
	(0.5482)	(0.5048)	(0.6629)	(0.6827)	(0.3269)	(0.3184)	(0.6330)	(0.6493)
share aged 5-17	0.2743	0.3753	-0.6307*	-0.6492*	0.2784	0.3339	-0.8132**	-0.8006**
	(0.3391)	(0.3298)	(0.3282)	(0.3313)	(0.2149)	(0.2204)	(0.3163)	(0.3151)
unemployment rate	0.0100***	0.0108***	0.0051***	0.0045**	0.0091*	0.0105**	0.0058*	0.0051*
	(0.0036)	(0.0035)	(0.0018)	(0.0019)	(0.0046)	(0.0044)	(0.0030)	(0.0030)
natural disaster	0.0096	0.0103*	-0.0002	-0.0002	0.0297	0.0300	-0.0038	-0.0018
	(0.0066)	(0.0060)	(0.0029)	(0.0029)	(0.0281)	(0.0282)	(0.0113)	(0.0112)
Division fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Division-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Period unit	Year	Year	Year	Year	Presid. Term	Presid. Term	Presid. Term	Presid. Term
Observations	1174	1174	1174	1174	322	322	322	322
R-squared	0.6589	0.6540	0.9334	0.9334	0.6946	0.6925	0.9558	0.9560

Robust t statistics in parentheses, clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

All regressions contain a constant, period dummies and, if state fixed effects are not included, dummies for Maryland, New Mexico, and Virginia.