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

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Abstract

This paper tests various 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 for the “swing voter" hypothesis { that is, no significant association between the amount of federal funds a state receives and the fraction of independents or moderates in the state. We also find no evidence for the “battleground state" hypothesis - no significant association between the amount of federal funds and the degree of partisan balance in a state. Modest support is found for the \partisan supporters" hypothesis, which conjectures that politicians will favour areas that contain a large percentage of their core supporters.

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Keywords: Electoral competition, swing voter, partisanship, election closeness, US Federal Spending.

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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 has 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 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 districts, states, or provinces where the share of supporters of each major party is closer to 50%. 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 favour 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 avoid excessive deadweight costs.[1] In all three cases, one underlying assumption is that politicians are mainly interested in winning elections, and offer government transfers or projects in order to appeal to voters.

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 virtually 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.[2] This is clearly problematic, however, since, *by assumption, voting decisions are endogenous to the distribution of government funds.* Most previous studies acknowledge this problem, and attempt to deal with it in some way, but the solutions proposed are not convincing.

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[1] Other theorists emphasize factors such as proposal power (Baron and Ferejohn, 1989), legislative seniority (McKelvey and Riezman, 1992), over- and under-representation (Ansolabehere, et al., 2003; Knight, 2006), committee structure, presidential leadership, and universalism (Weingast et al., 1981; McCarty, 2000).

In this paper we use survey data on party identification and ideological positions, rather than voting data, to construct measures of the key independent variables. The variables based on survey data are almost certainly much more exogenous than variables based on votes. In addition, 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). The data are for U.S. states, and the period we study is 1978-2002.

Our results are easily summarized. We find little support for *any* of the three hypotheses listed above. We find no support for either the swing voter hypothesis or the electoral battleground hypothesis. We find mixed support for the partisan supporters hypothesis.

In addition, the use of survey data allows us go further than previous studies. We can also estimate the impact that government spending in a geographic area has on the vote – voting decisions now as the dependent variable and the geographic distribution of funds as an independent variable – using the survey based measures of party identification and ideology as controls. We find that spending has little or no effect on voters' choices.

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.

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[2] The one exception is Dahlberg and Johansson, (2002). This paper has other problems, however, which we discuss below.

Several studies find evidence supporting the swing voter models in some contexts, but mixed or no evidence in other contexts. Studies of the allocation of New Deal spending have found some evidence that states with a more volatile presidential vote received more 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.[3]

The logic of distributive politics is also affected by electoral rules. In particular, winner-takes-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, 2005). The competitiveness of elections is particularly important in the U.S. context, where the electoral college system may induce the channelling of resources toward states that are pivotal in the presidential electoral race.

Existing empirical studies do not find a clear relationship between resource allocation and competitiveness of presidential election at state level. According to Wright (1974), 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.[4]. 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, 2005). All of these studies use lagged presidential vote returns to measure the two-party balance in each state.

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[3] One exception is Dahlberg and Johansson (2002), who use survey data to construct a measure of the percentate of swing voters in each Swedish region. They find that the distribution of environmental grants in Sweden is concentrated most heavily in electorally pivotal regions of the country. Unlike us, who focus on large spending aggregates, they analyse a tiny ”ecological grant” program. The distribution of voters' preferences by electoral constituency is estimated by using the Swedish Election Study of 1994: one problem is therefore that such survey is not guaranteed to be statistically representative by constituency. Most importantly, they use spending on 115 municipalities as dependent variable but have only 29 observations for the distribution of voters' preferences (independent variable). This implies that the standard errors reported in their tables are too small: the authors do not appear to have clustered by constituency.

A competing theory of distributive politics is that parties target spending toward loyal voters (Kramer, 1964; Cox and McCubbins, 1986; Dixit and Londregan, 1996; Dasgupta, et al., 2001; Sim, 2002). 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 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 orientated. 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. It may be the case, for example, that the loyalists of the out-party receive disproportionately small shares of the public dollar, while swing areas and loyal areas do equally well.

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[4] Milligan and Smart(2003) finds that closeness of the electoral race has a positive effect on spending in the Atlantic Canadian provinces, but a negative effect in Quebec, while Crampton (2003) finds a positive correlation between competitiveness of the race and spending only in Canadian provinces which are not ruled by the liberal party

Empirically, several studies find evidence that loyal voters are rewarded. Some studies find a positive relationship between the share of U.S. federal spending going to an area and the Democratic vote in the area (e.g., Browning, 1973; Ritt, 1976; Owens and Wade, 1984; Levitt and Snyder, 1995). Since Democrats were the majority party in Congress during the years studied, this provides some support for the idea that federal spoils go to the victors, but the results might also reflect the behavior of the Democratic party or the characteristics of areas that tend to vote Democratic.[5] Some studies of U.S. states find a positive relationship between spending and past share vote for the incumbent president's party (Fleck, 2003; Larcinese, et al., 2006; Garrett and Sobel, 2003).[6]

3. Problems with Using Voting Data to Measure Citizens' Preferences or Partisanship: 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 somewhat, but this is at best a partial solution for at least two reasons: (i) budgetary processes are sluggish, and spending in any given year depends to a large extent on decisions made in previous years, and (ii) we do not know if voters are "retrospective" or "prospective." If voters are somewhat prospective and parties keep their promises – as assumed in many models of distributive electoral politics – then lagged votes are a function of lagged promises which are equal to (or at least highly correlated with) current spending. There is a third reason to suspect that lagged vote measures are not exogenous: (iii) omitted variables that are correlated both with voting and budgetary decisions. For example, some groups might be especially 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).

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[5] 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.

[6] Studies of the distribution of patronage by urban machines also find that the organizations in control of their cities tend to reward their core supporters with patronage (Holden, 1973; Rakove, 1975; Erie, 1978; Johnston, 1979).

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 that appear in the literature are fairly complicated, and typically include two or more vote-based measures in the same model. We therefore ran a series of simulated regressions. These allow us to gauge the sign and size of the bias in a set of models that are similar to many of the standard models in the literature.

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_{jt}^R be the support received by party R. Finally, let $\tilde{V}_{jt}^D$ be the fraction of votes party D receives in state j in year t, and let $\tilde{V}_{jt}^R = 1 - \tilde{V}_{jt}^D$. We assume:

$$\begin{aligned}
X_{jt}^D &= \alpha_I I_j + \alpha_C \tilde{C}_j + \alpha_P \tilde{D}_j + \mu_{jt}^D & (1)\\
X_{jt}^R &= \alpha_I I_j + \alpha_C \tilde{C}_j + \alpha_P \tilde{R}_j + \mu_{jt}^R & (2)\\
S_{jt}^D &= \beta_I X_{jt}^D I_j + (1+\beta_P X_{jt}^D) D_j + \epsilon_{jt}^D & (3)\\
S_{jt}^R &= \beta_I X_{jt}^R I_j + (1+\beta_P X_{jt}^R) R_j + \epsilon_{jt}^R & (4)\\
\tilde{V}_{jt}^D &= S_{jt}^D / (S_{jt}^D + S_{jt}^R) & (5)
\end{aligned}$$

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 something like 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 (2002) and others.[7]

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.[8] 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 $\tilde{C}_j$.

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[7] This formulation does not do justice to some of these models, such as Stromberg (2002), which takes into account the total probability a state is "pivotal" in the electoral college.

[8] 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) budgetary lags; (ii) voters could be "prospective" and (iii) there are probably omitted variables that are correlated both with voting patterns and budgetary decisions. Rather than constructing complicated historical averages and autocorrelation structures that attempt to incorporate these features more precisely, we simply use contemporaneous voting data freely in our simulated regressions. 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:

$$\begin{aligned}
\text{Model 1a} &: X_{jt}^D = a_I \hat{I}_j + \mu_{jt} \\
\text{Model 1b} &: X_{jt}^D = a_C \hat{C}_{jt} + \mu_{jt} \\
\text{Model 1c} &: X_{jt}^D = a_P \tilde{V}_{jt}^D + \mu_{jt} \\
\text{Model 2a} &: X_{jt}^D = a_I \hat{I}_j + a_C \hat{C}_{jt} + \mu_{jt} \\
\text{Model 2b} &: X_{jt}^D = a_I \hat{I}_j + a_P \tilde{V}_{jt}^D + \mu_{jt} \\
\text{Model 2c} &: X_{jt}^D = a_C \hat{C}_{jt} + a_P \tilde{V}_{jt}^D + \mu_{jt} \\
\text{Model 3} &: X_{jt}^D = a_I \hat{I}_j + a_C \hat{C}_{jt} + a_P \tilde{V}_{jt}^D + \mu_{jt} \\
\text{Model 4} &: X_{jt}^D = a_I I_j + a_C \hat{C}_{jt} + a_P \tilde{V}_{jt}^D + \mu_{jt}
\end{aligned}$$

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 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 averages of the estimates of the parameters of interest[9]. To give an example, if we take model 2a, the first row gives the average estimates of, respectively, α_I (.01) and α_C (.08) when the true values of these parameters are both set equal to 0: the columns on the left of Table 1 report the true values in each case. 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.

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[9] 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.

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 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 parties 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) 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 to test the various hypothesis about distributive politics. 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.[10] 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 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 variable of interest is the percentage of swing voters in a state. We use survey data to measure the share of "independents" (and also the share of Democratics and Republicans). These data are 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.[11] Voters are interviewed 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).[12] 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]?"

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[10] Interest on the debt is not included in either dependent variable.

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

[12] 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.

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 interviewed in some states in some years, we aggregate the results over four-year periods (two elections).

One concern is how well these data can capture the distribution of partisanship within states. This issue is discussed extensively in Erikson, Wright and McIver (1993), who conclude that the partisanship measures derived from the surveys correlate in the expected way with observable other criteria like other polls, election returns and party registration. A number of checks induce us to think that these data capture the underlying distribution of partisanship by state quite 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 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 of what electoral results would suggest and that using voting to measure partisanship can therefore be problematic[13]. In Fig. 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.

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 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[14].

As discussed in the introduction, swing voter models predict that states with higher partisan and/or ideological balance 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.[15]

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[13] Consistently with Green, Palmquist and Schickler (2002), partisanship also appears rather stable over time or smoothly changing in some states.

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

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 elderly, percent in schooling age and total state population.[16] 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 three states.

The sources for all variables used in our analysis are reported in Table 4.

5. Results

The simulation exercise shows that regressions based on voting data can be substantially biased. By using arguably 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 wether we obtain different estimates. We can then use the simulation exercise as a benchmark to evaluate the potential bias in estimated coefficients.

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[15] 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.

[16] The total population size captures the effects of malapportionment of the U.S. Senate, as small states are extemely 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.

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 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 consider specifications in which swing, pivotality and partisan measures are used in the same regression. This should avoid the possibility that the obvious correlations between those measures could bias the estimate of single coefficients, since, as discussed, the various hypotheses are not necessarily incompatible with each other. We also consider the possibility that the share of swing voters and the closeness could have a positive interaction. Unfortunately the two variables are highly correlated and there appears to be little to learn from such exercise. The results always display insignificant coefficients for the interaction term and are not reported. There is also the possibility that open primaries induce more people to define themselves independent. We indentify Massachusetts and Rhode Island as two states where this is a serious possibility. Therefore all regressions have been repeated by excluding those two states. We noticed very limited variations in the results (not reported)[17].

Using yearly data when voting data are not available for each year (and the closest past election is therefore imputed to subsequent years) 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.

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

Finally, we study three alternative dependent variables. In one specification we use targetable spending, i.e. we remove from total federal expenditure the least manipulable categories such as entitlements. In another specification we use federal grants rather than total spending. Grants contain a larger share of discretionary spending and also often provide the state government with some discretion over the way money is spent. Thus, receiving more grants should be favorably regarded both by the citizens and by the administrators of a given state.

Since we consider a large number of specifications, we only report the coefficients of our variables of interest in the main text.[18] These are reported in Table 2. 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 aged has a positive and significant effect on total federal outlays, while the percentage of 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.

5.1 Share of swing voters

The key test of the swing voter hypothesis consists in verifying whether the coefficients on the share of independents in a spending equation is positive. Looking at Table 2, this test is reported in model 1 (with exit poll measures) and model 2 (with voting measures) and then again in model 7 (when other political variables from exit polls are also introduced) and model 8 (other political variables from voting data are introduced). We find no 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.

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[18] Detailed results are shown in the appendix.

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 sign for β in model 8 (when voting data are used) is much more common (and significant, in some cases) than a negative sign for β in model 7 (when exit poll data are used).

Hence, we do not find support for the basic prediction of the swing voter model. States with more independent voters do not receive 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 focussing on the “competitiveness” of the electoral race for presidential elections. This time the results using poll data (model 3) and voting data (model 4) are quite similar. The coefficient for a close race is negative, i.e. states with closer races receive less funds. This runs against the predictions of models based on the swing voter logic. This result, however, only holds in cross section analysis and is not robust to the introduction of state fixed effects in the case of total and targetable spending, although in this last case the coefficients are not significant. The situation is reversed when we consider grants: now the negative sign prevails when state fixed effects are introduced but vanishes in cross section analysis. The magnitude of the negative effect of closeness is larger when we use poll data measures (with the exception of grants regressions). There is one important difference between the voting and the exit poll regressions: in the first case, the results are not robust to the introduction of other political variables (model 8), while the results in model 7 (poll data) are surprisingly similar to those of model 3. A prevalence of the negative sign remains when we remove the cross-pressured voters from the bulk of the partisans (not reported).

The main conclusion that we derive 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 2, 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 significances. In this case the polling data measure of partisanship is always positive and, in four cases, significant at the 10% level. Detracting cross-pressured voters from the count of the partisans do not alter in any significant way these results. 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, just the opposite of what we found in the previous cases.

6. Effects of Government Expenditures on Subsequent 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 favouring 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. By using individual exit poll data we can run regressions of individual voting decisions conditional on funds received and controlling for partisanship and ideological leaning. Including such controls means that, to a large extent, we mitigate possible endogeneity problems for the spending variable.

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 funds (which are measured at the state level) should affect voting for particular incumbents, given that within a state there are simultaneously many House representatives, usually from both parties. 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[19].

Table 2 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 favour 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 massive effects. The results when we use targetable spending 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 voting for a Democrat in Congress when the majority of state representatives in Congress is Democrat. 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 analogous findings by Levitt and Snyder (1995)[20].

Overall, the evidence that receiving more federal funds induces voters to reward incumbent politicians is rather weak. 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.[21] 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). At the district level the situation does not appear much different: individual candidates for the House have also been shown to systematically assume divergent positions (Erikson and Wright, 1997; Ansolabehere, et al., 2001). Hence, we conclude that our findings, rather than the consequence of platform convergence, constitute evidence against the idea that voters respond with their votes to public spending in a clear and systematic way.

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[19] Before moving into the regression analysis we have checked how well self-reported individual data could predict actual state-level electoral results. This is a potential problem for any survey-based analysis of voting decisions. We find a correlation coefficient of 0.792 between the results predicted by the exit poll data and actual electoral results.

[20] The estimates reported in Table 3 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. Detailed results are reported in the appendix.

7. Conclusion

Our findings regarding the allocation of federal spending across U.S. states are disappointing. We find no support for the notion that parties target areas with high numbers of swing voters. We also find no 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. Since we find no significant effect of distributive spending on voting decisions, it seems most likely that to the extent that partisan targeting occurs it is driven more by the policy-motivations of politicians than by strategic calculations to win electoral support.

Our findings might reflect features of distributive politics that are particular to the U.S.. Congress is one of the most powerful and decentalized 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, the president may have relatively little influence over the geographic distribution of federal expenditures. Perhaps, even though he would like to target swing states or swing voters, he cannot.

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[21] 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.

On the other hand, our findings might reflect a broader truth about distributive politics: it is probably messy everywhere. Further investigations in other institutional settings are necessary to establish the validity of this conclusion.

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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

Fig.1: Democratic vote share and partisanship by state

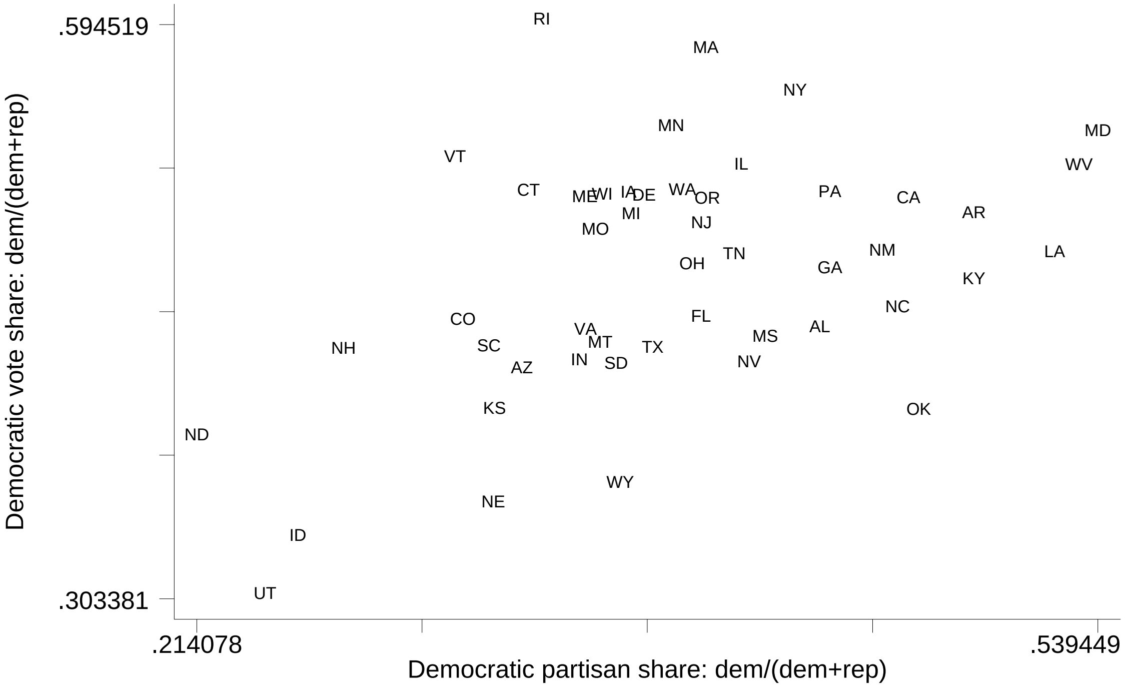
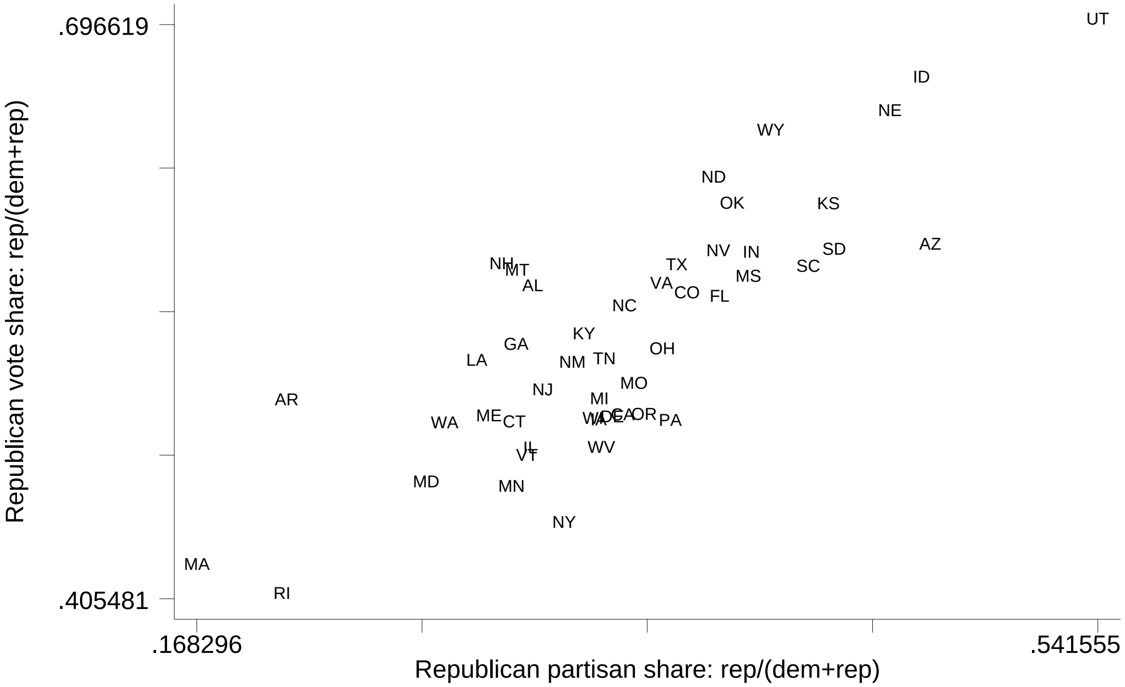


Fig.2: Republican vote share and partisanship by state



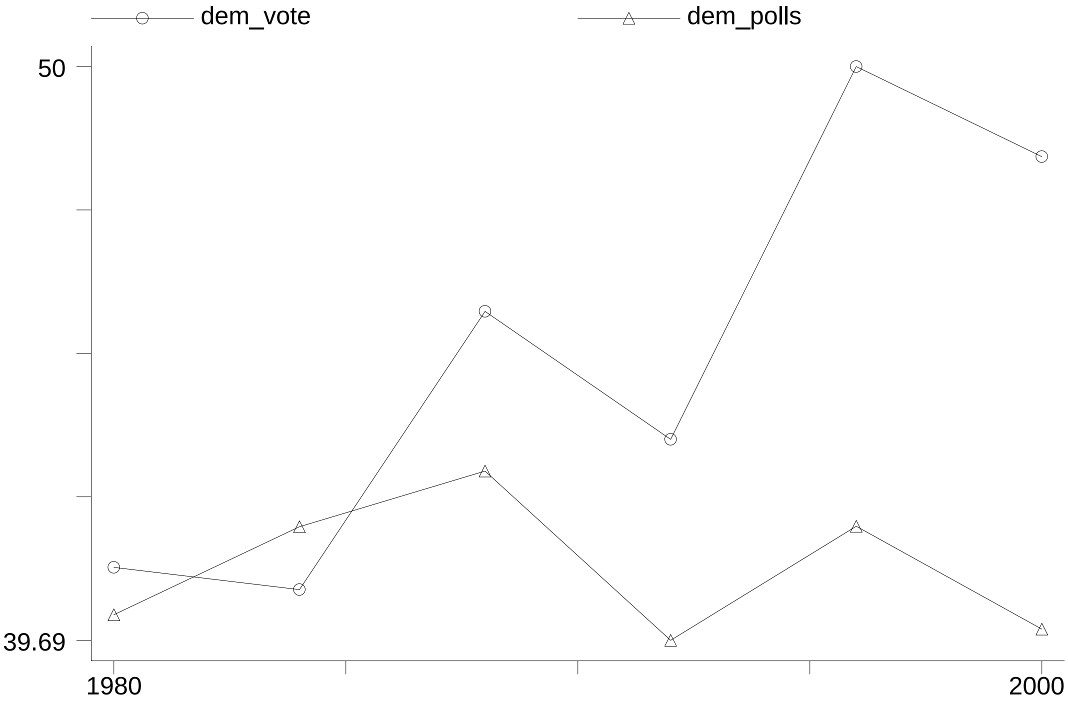


Fig3. Aggregate Democratic Vote and Partisanship over Time

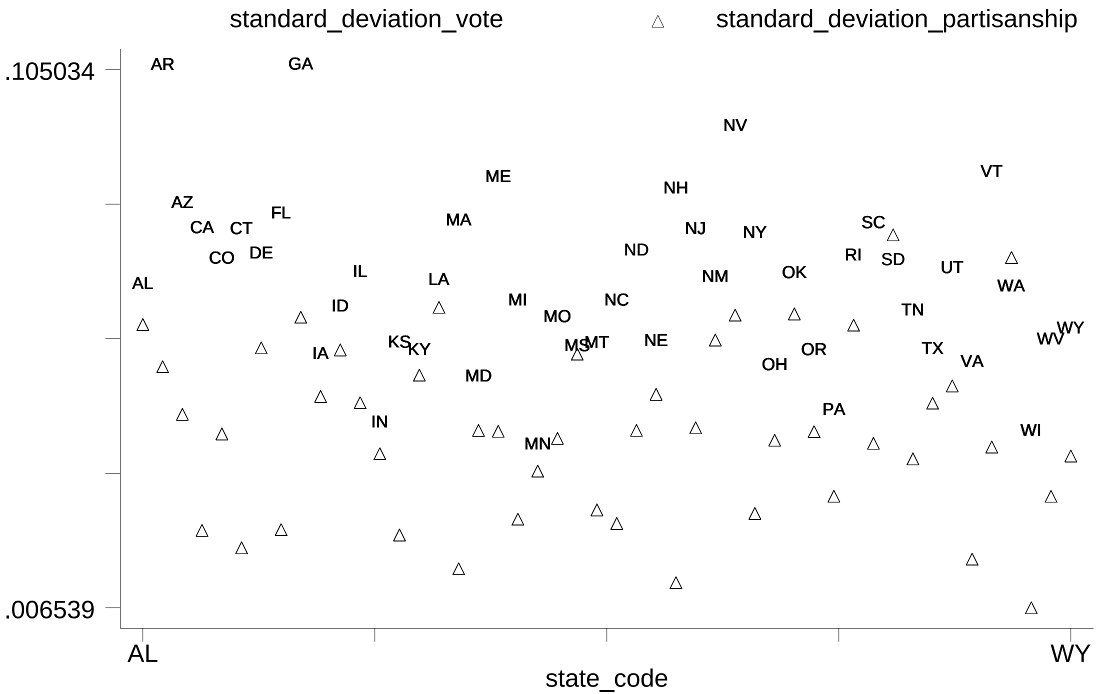


Fig.4: Standard dev. of Democratic vote and partisanship by State

TABLE 2: 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	0.54	-0.86*	-0.49*	-0.09	0.46*
fed. exp.	year	yes	-0.35	-1.3**	0.19	0.1	0.37**	0.11
fed. exp.	term	no	0.24	0.73	-0.85	-0.7**	-0.02	0.83**
fed. exp.	term	yes	0.15	-1.29**	0.2	-0.01	0.41**	0.33*
targetable	year	no	0.42	0.27	-0.68	-0.35	-0.26	0.16
targetable	year	yes	-0.38	-0.88	0.11	0.06	0.2	-0.03
targetable	term	no	0.28	0.44	-0.63	-0.54*	-0.11	0.56**
targetable	term	yes	-0.31	-0.93	0.2	0.00	0.18	0.08
grants	year	no	0.11	-0.11	-0.16	-0.01	-0.04	0.05
grants	year	yes	0.08	0.01	0.03	-0.09*	0.06	0.1**
grants	term	no	0.13	0.05	-0.17	-0.09	-0.05	0.09
grants	term	yes	0.22	0.1	-0.00	-0.13**	0.05	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	0.45	-0.86*	0.19	0.38	-0.52	-0.11
fed. exp.	year	yes	-0.10	0.18	0.35*	-1.46**	0.37	0.58**
fed. exp.	term	no	0.40	-0.85	0.25	0.28	-0.59	0.26
fed. exp.	term	yes	0.39	0.22	0.46**	-1.40**	0.17	0.57**
targetable	year	no	0.42	-0.68	0.01	0.27	-0.63	-0.44
targetable	year	yes	-0.26	0.1	0.16	0.92	0.21	0.17
targetable	term	no	0.33	-0.63	0.08	0.1	-0.51	0.08
targetable	term	yes	-0.21	0.21	0.17	-0.99	0.00	0.15
grants	year	no	0.13	-0.16	0.03	-0.14	-0.00	0.05
grants	year	yes	0.14	0.04	0.08	-0.04	-0.07	0.03
grants	term	no	0.15	-0.17	0.03	-0.01	-0.08	0.02
grants	term	yes	0.26	0.00	0.07	0.01	-0.12	0.01

Each cell corresponds to a regression, of which only the relevant coefficient is reported. Detailed results can be found in the Statistical Appendix. Indicating with I the share of independents, with σ the standard deviation of Democratic vote, with CP and CV the closeness between the two main parties measured, respectively, using poll and voting data and with PP and PV the share of partisan supporters for the incumbent president measured, respectively, with poll and voting data, the more general models are specified as follows:

model 7: $X_{st} = \alpha+\eta_s + \lambda_t + \beta I_{st} + \gamma CP_{st} + \delta PP_{st}+ \theta Z_{st} +e_{st}$

model 8: $X_{st} = \alpha+\eta_s + \lambda_t + \beta\sigma_{st} + \gamma CV_{st} + \delta PV_{st}+ \theta Z_{st} +e_{st}$

where X is expenditure (federal expenditure, targetable spending or grants), Z are control variables (real income per capita, share of population aged 5-17, share of elderly, total population), s stands for state and t for the time unit (year or presidential term). Model 1 and model 2 set $\gamma=\delta=0$ and use, respectively, I and σ. Models 3 and 4 set $\beta=\delta=0$ and use, respectively, CP and CV. Models 5 and 6 set $\beta=\gamma=0$ and use, respectively, PP and PV. All regressions are repeated both with and without ηs (as indicated). When fixed effects are not used we introduce control dummies for Maryland, Virginia and New Mexico. Robust z-statistics in parentheses (clustered by state). * indicates significance at 10% level, ** at 5% and *** at 1%.

TAB 3: 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
	(0.92)	(1.80)*	(1.20)	(0.97)
partisan match	2.2109	2.0522	1.9842	
	(23.23)***	(55.71)***	(28.51)***	
ideology match	0.9000	0.7427	0.6730	
	(15.41)***	(33.98)***	(10.97)***	
fed. exp. x democratic share of house representatives				-0.0033
				(0.05)
share of Democratic representatives in the House				0.0803
				(0.33)
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
	(1.80)*	(1.92)*	(1.08)	(0.25)
partisan match	2.1195	2.2128	1.9677	
	(22.73)***	(52.27)***	(28.11)***	
ideology match	0.8779	0.7303	0.6671	
	(14.24)***	(36.78)***	(10.89)***	
targetable spend. x democratic share of house representatives				-0.0251
				(0.30)
share of Democratic representatives in the House				0.1372
				(0.49)
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
	(0.09)	(0.59)	(0.89)	(2.08)**
partisan match	2.2035	2.0505	1.9885	
	(22.85)***	(55.81)***	(28.46)***	
ideology match	0.8998	0.7309	0.6737	
	(14.75)***	(36.53)***	(10.91)***	
grants x democratic share of house representatives				0.1688
				(0.78)
share of Democratic representatives in the House				-0.0123
				(0.09)
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. 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%.

Table 4: Variable Definition 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), and Percentage in Schooling Age (5-17), are taken from the *Statistical Abstract of the United States.*