The Political Economy of Public Spending: Evidence from the US States*

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Abstract

This paper tests various hypotheses about distributive politics by studying the distribution of federal spending across U.S. states. We improve on previous work by using survey data to measure the share of voters in each state that are Democrats, Republicans, and independents. 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, nor do "battleground states" attract more federal funds.

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

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1 The question

Contemporary governments allocate a substantial amount of resources through public spending. In most OECD countries public spending amounts to a share of GDP ranging from one third to 50%, and sometimes more. Public spending often implies a substantial amount of redistribution between social groups and between different geographic areas. It is therefore not surprising that distributive politics has been for a long time a central theme in political economy, attracting the attention of both economists and political scientists. These theoretical efforts have delivered a number of testable hypotheses and have been matched by an equally abundant empirical research, particularly focussed on the US federal spending .

The dominant theory of public spending allocation is the so-called "swing voter" hypothesis. This posits that governments allocate a disproportionate share of distributive goods to groups or regions that contain a large share of voters that are indifferent between the political parties. 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. Office-seeking politicians competiting for these voters will therefore allocate disproportionate amounts of federal spending to regions or groups with many indifferent voters. Lindbeck and Weibull (1987) and Dixit and Londregan (1995, 1996), among others, develop analytical models in which parties target disproportionate resources to "pivotal" groups or regions. Colantoni, et al. (1975), Snyder (1989) and Stromberg (2008), among others, develop similar models in the context of allocating campaign resources. 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, federal grants, and public employment have found some evidence that states with a more volatile presidential voting received somewhat more federal moneys.[1] However, Stromberg (2004) shows that these findings are extremely sensitive to specification choices. Similarly, Larcinese, et al. (2006), considering the period 1982-2000, find that states with more frequent presidential vote swings did not receive more funds. All of these studies use lagged presidential vote returns to measure the fraction of

[1] See Wright (1974), Wallis (1987, 1996), Fleck (1999) and Fishback, et al. (2003).

swing voters.

An alternative hypothesis, sometimes confused with the swing voter theory, is that governments allocate prevalently to areas with a very close race. This hypothesis, that we will call "electoral battleground" hypothesis, is especially relevant in systems where two major parties compete in first-past-the-post elections with geographically defined constituencies. 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). Battleground districts should therefore be favored in the allocation of both public spending and campaign resources (Snyder, 1989; Stromberg, 2008). Election competitiveness 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. Existing empirical studies do not find a clear relationship between resource allocation and competitiveness of presidential election at the state level. According to Wright (1974), U.S. states with close presidential races did 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.[2]. 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, 1992; Stromberg, 2008). All of these studies use lagged presidential vote returns to measure the two-party balance in each state.

A third view of the political economy of public spending is represented by the "partisan supporters" hypothesis, which conjectures that politicians tend to favor areas that contain a large percentage of their core supporters (Kramer, 1964; Cox and McCubbins, 1986; Dixit and Londregan, 1996; Sim, 2002; Dasgupta, et al., 2008). 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

[2]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 (2004) finds a positive correlation between competitiveness of the race and spending only in Canadian provinces which are not ruled by the liberal party

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. Finally, targeting public spending towards loyal voters could also simply reflect the fact that politicians are, at least to some extent, policy oriented[3]. Democratic politicians may prefer spending on policies that tend to benefit Democratic voters, and likewise for Republicans.[4] 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. 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]

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, presi-

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

[4]Besley and Case (1995) find that term limits increase the differences between the policies implemented by Republican and Democratic governors. Sole'-Olle' (2006), using Spanish data for the period 1992-99, finds that an increase in the margin of victory leads left-wing (right-wing) local governments to increase (decrease) spending, taxes and deficits.

[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).

dential 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.

Testing these hypotheses 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, the prevalent approach is to use voting data to construct proxies of the percentage of swing voters, partisan balance, or the partisan disposition of each state.[7] This is clearly problematic, however, since voting decisions are – by assumption – endogenous to the distribution of government funds, according precisely to these models of distributive politics. One important consequence of the endogeneity is that estimates of the effect of swing voters or electoral closeness on spending will often be biased.[8] 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.

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

[7] The one exception is Dahlberg and Johansson (2002), who use survey data to construct a measure of the percentage of swing voters in each Swedish region. Unlike us, who analyze large spending aggregates, they focus on very specific "ecological grant" program.

[8] For a Montecarlo exercise that estimates the size and direction of the biases, see Larcinese, et al. (2008).

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).

In this paper we use direct measures of underlying partisan leaning and voters ideological attitudes to estimate whether federal budget allocations to the states are affected by these voters' characteristics – as posited by models of distributive politics. For this purpose, we need measures that are exogenous with respect to short term policies such as the annual federal budget allocation. Dozens of political science studies over more than fifty years argue that party identification is very stable over time, and less affected by particular short-term electoral circumstances, relative to vote choice. This idea goes back at least to the celebrated book *The American Voter* (Campbell, et al., 1960). 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 (Campbell, et al. 1960; Green at al. 2002). As Green et at. (2002) point out "identification with the political party is analogous to identification with religious, class, or ethnic group" (p. 78). In other words, party identification is more of an identity than an opinion. Similarly, Goren (2005) shows that partisan identity is remarkably stable and 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. Even scholars who are critical of the notion that party identification is affective, such as Fiorina (1981), argue that it is a kind of long-term moving average of past assessments of party performance. Ideology is similarly stable (Ansolabehere, et al., 2008). Hence, the evidence provided by the large body of studies on party identification suggests that measures based on party identity (rather than voting decisions) can provide a valuable tool to test

theories of distributive politics, because these measures are arguably much more exogenous with respect to short term policy outcomes.

To construct direct measures of the key independent variables used to test the alternative hypotheses derived from models of distributive politics, we use survey data. These allow us to measure directly the partisan leaning of voters rather than indirectly through voting choices. We find little support for any of the hypotheses listed above. These results are consistent with those found in a companion paper (Larcinese, et al., 2008) where we use data for the period 1978-2002. Here we focus on a single year, therefore losing the panel dimension and increasing the risk of omitted variables bias. On the other side here we can exploit more detailed information on voters, which unfortunately is only available for a much more limited period. The fact that the results are overall rather similar, and that there is no support for any of the theories in both papers, makes us to claim even more confidently that a renewed effort to understand the political economy of public spending is needed today.

2 Methodology and data

We analyze the allocation of the U.S. federal budget to the states estimating equations of the following type:

$$\begin{aligned} Y_s &= \alpha + \beta X_s + \boldsymbol{\gamma}\mathbf{Z}_s + \epsilon_s, \\ s &= 1, ...48; \end{aligned} \tag{1}$$

where Y_s is spending in state s, X_s is the value of the main explanatory variable (share of swing, share of partisans or closeness in state s), $\mathbf{Z}_s$ is a vector of control variables and ϵ_s is an error term. 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.[9] 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. Moreover, grants contain a large 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. In all cases, our dependent variables are outlays.

It is important to consider the 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 match 2000 survey data with 2001 spending data.

2.1 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 electoral closeness, of the share of independents and of loyal voters that, differently from previous work, are not based on actual voting data but on survey data.

We use various sources. A first source is represented by exit polls conducted by various news organizations – CBS News, CBS News/New York Times, ABC News, ABC News/Washington Post, and Voter News Service.[10] 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).[11] Importantly, these questions are

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

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

[11] 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

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]?"

Using this information we can construct state-level variables reporting the percentage of voters that declare themselves Democratic, Republican or Independent. Of course, the use of survey data raises yet another potential methodological problem – measurement error. Survey 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).[12] Other items, such as ideology, appear much less reliable. 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 hundreds or even thousands of individuals, so even if there is a large amount of measurement error at the individual level, the measurement error in the aggregated measures should be small.[13]

One concern with the exit poll data is that we have a 3-category scale of partisan identification – Democrat, Independent, and Republican. Most surveys employ a more detailed, 7-category scale – Strong Democrat, Weak Democrat, Independent Leaning Towards Democrats, Independent, Independent Leaning Towards Republicans, Weak Republican, and Strong Republican. The main potential problem is with the classification of "leaning" independents. The exit poll measure includes these voters with the set of independents. However, many survey analysts argue that "leaning independents" vote more like weak partisans than "pure" independents. We checked whether this matters by using the National Annenberg Election Survey (NAES) of 2000.[14] This survey has a huge sample of 81,208 respondents, with representative samples for each state. And, it has more detailed information on party identification, allowing us to classify the "leaning" independents various ways.

issues.

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

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

[14] See Romer, et al. (2006) for more details about the study.

To test the loyal voters hypothesis we use the share of Democratic vote in the 1996 presidential election, since the 2001 outlays have been appropriated during the year 2000, hence under the Clinton presidency. 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. The NAES survey provides a more detailed partition distinguishing between strong and weak Democrats (Republican) and Independents leaning Democrats (Republican) or pure. We can then construct more refined measures of the share of Independents, distinguishing between Total Independents and Pure Independents; of the share of Democrats, distinguishing between strong Democrats and total Democrats; and of election closeness as follows:

$$\begin{aligned} Total\ Dem &= Strong\ Dem + Weak\ Dem \\ Total\ Rep &= Strong\ Rep + Weak\ Rep \\ Closeness\ 1 &= (1 - |Total\ Dem - Total\ Rep|) \\ Closeness\ 2 &= (1 - |Strong\ Dem - Strong\ Rep|) \end{aligned}$$

We then compare the results obtained with these survey-based measures with those obtained by using the standard voting-based political variables. The share of loyal voters and election closeness are then measured simply by using voting outcomes for each party. To measure the share of swing (independent) voters we use, following a standard procedure, the standard deviation of Democratic vote in the previous three presidential elections.

Finally, 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.[15]

[15] 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," population growth is likely to have a negative affect on 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. For a full analysis of these issues see Larcinese, et al. (2009 and 2010).

3 Results

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 and surveys are used as explanatory variable with the results obtained when observed votes are used (i.e. the standard deviation of Democratic vote). 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 and surveys 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 and survey partisan measures are used instead.

To check the robustness of our results we also 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 suffer from omitted variable bias. All our estimates report standard errors that are robust to heteroskedasticity of unspecified form.

Before moving to our main results, it is worth pointing out that, for what concerns the standard control variables, we do not find any significant surprises or noticeable differences across the various specifications. The percentage of aged is often insignificant but sometimes displays a positive and significant coefficient in the total federal outlays equations and a negative significant coefficient in the targetable spending equations (i.e. when entitlements are removed). The percentage of school-age children displays sometimes a negative significant impact on total federal outlays. The coefficient of population (in logarithm) is negative and significant in most specifications, with an effect which is particularly significant in the case of grants. The coefficient of income per capita is never significant.

3.1 The swing voters hypothesis

The key test of the swing voter hypothesis consists in verifying whether the relationship between the share of independents and spending is positive. Table 2 presents the main results. Consistently with the panel regressions of Larcinese, et al. (2008) we find no evidence that states with a larger share of independent voters receive more funds when we use exit polls measures. This result, however, changes when we use the NAES data, although the change goes in a direction which is opposite to that predicted by the theory. Now states with larger shares of independent voters receive *less* total federal spending. This effect is particularly strong for the share of pure independents but it remains significant at 10% level also for total independents. In the case of pure independents the effect remains strongly statistically significant also in the case of targetable spending and for grants. Voting-based measures are instead never significant at an acceptable level, although, in the case of grants, the standard deviation of Democratic votes reaches a respectable (given the sample size) t-ratio of 1.58.

Hence, we do not find support for the basic prediction of the swing voter model.[16] States with more independent voters do not receive more federal funds. On the contrary, if we use the more refined NAES measures, we find that the presence of pure independents is associated with *lower* spending in a state. It may then be the case that the insignificant coefficients that we find for the share of independents in the other cases is due to a measurement error problem, whereby "leaning" voters are counted with the independent. The negative correlation between share of pure independent and money received appears consistently in all the spending aggregates we analyse and it is quite robust to controlling for other political variables, as can be seen from Table 5, columns 3, 7 and 11.

[16] Dahlberg and Johansson (2002), using survey data, find support for the swing voter hypothesis in the allocation of "ecological grants" to Swedish municipalities. Their results suggest that tactical distribution may be working in different institutional setting and can be found if one focusses on some very specific discretionary spending programs.

3.2 The "battleground states" hypothesis

We conduct a similar investigation on the "competitiveness" of the electoral race for presidential elections. The results, reported in Table 3, show no statistically significant coefficients with the exception of the negative ("wrong") coefficients obtained by using the exit polls measure in the grants equation. In this case using the more refined NAES measures does not deliver any particular improvement. In most cases the coefficients tend to be negative and statistically insignificant. The results of Table 5 again show negative and significant coefficients in the grants equation when either the exit polls or the voting data are used. Our conclusion is that, in the rare cases in which it is significant, the coefficient of closeness displays a sign which is opposite to what the "battleground states" hypothesis would predict.[17]

3.3 The "loyal voters" hypothesis

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 can use survey data to measure the share of voters who identify themselves with each party and can therefore use this variable to measure partisanship. We can then consider the share of vote for the incumbent president's party (Democrat) as the relevant measure of state partisanship and use it as an explanatory variable of spending.

Table 4 reports our results. Most coefficients of survey-based variables display a positive ("correct") sign. Significance levels, however, are generally far from acceptable, with only one exception. In the grants equation, the share of total Democrats measured by using the NAES survey is significant at the 5% level. This result, however, vanishes in Table 5, when all the hypotheses are considered simultaneously. Hence, the results display only limited differences with the voting-based indicators.

[17] As shown in Larcinese, et al. (2008), results on the battleground hypothesis depend heavily on the specification adopted and, if using panel data, on whether fixed effects are included or not. The overall pattern, in any event, remains rather confused and certainly not supportive of the basic theoretical predictions.

4 Conclusion

Our findings on the allocation of federal spending across U.S. states are disappointing for political economy theories of the distribution of public spending. We find no support for the notion that parties target areas with high numbers of swing voters. In fact, we find that states with a high share of pure (as opposed to leaning) independents tend to be penalised. This is our only robust finding. We find indeed no support for the notion that parties target battleground states or areas with high numbers of their partisan supporters.

Our findings can clearly suffer from potential omitted variable bias, since we only use spending across the US states in one specific year. This choice is dictated by our willingness to use a survey (NAES) which has only been conducted since the year 2000. Compared to other studies, this survey contains a finer classification of the population in terms of partisanship (7 categories from "strong Democrat" to "strong Republican"). We compare the results obtained from the NAES survey with those obtained by using exit polls data, which only allow us to have three groups (Democrats, Republican and Independent). In the latter case, however, we have data for the period 1978-2002 and the panel estimations can be found in Larcinese, et al. (2008). Here instead we can compare the results obtained by using different surveys and a finer partition of the population at the cost, however, of losing the panel dimension.

The general flavour of our results is, in any event, not different from what found in Larcinese, et al. (2008), with the only addition of a strong negative coefficient for the share of pure independents (that could not be identified by using the exit polls data). Whether this new result is the consequence of an omitted variable bias remains moot but it is worth pointing out that it had not been found in Larcinese, et al. (2008) even in regressions that did not include state fixed effects. It is certainly an intriguing result that deserves further investigation.

Our conclusions here remain substantially in line with those we provide in Larcinese, et al. (2008). On one side 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.

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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Variables: Definition and Sources

- **Survey and poll data.** *The National Annenberg Election Study of 2000*, CD ROM, from the Annenberg Public Policy Center, University of Pennsylvania. See text for description of exit poll data sources.

- **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.*

Table 1. Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
total federal spending (thousand USD per capita)	48	3.545088	0.663956	2.590533	5.590782
targetable spending (thousand USD per capita)	48	1.49154	0.53688	0.942917	3.545424
grants (thousand USD per capita)	48	0.709282	0.196759	0.388149	1.387176
share independents (exit polls)	48	0.265096	0.071719	0.097222	0.429907
closeness (exit polls)	48	0.892309	0.083797	0.645161	1
share of Democrats (exits polls)	48	0.383161	0.066227	0.211696	0.53569
share total independents (NAES)	48	0.33509	0.080567	0.18895	0.537736
closeness 1 (NAES)	48	0.918549	0.07432	0.724227	1
share total Democrats (NAES)	48	0.337484	0.067726	0.195876	0.526087
share pure independent (NAES)	48	0.086327	0.025028	0.041304	0.147059
closeness 2 (NAES)	48	0.948365	0.043622	0.842784	0.998874
share strong Democrats (NAES)	48	0.169337	0.044951	0.079511	0.269529
standard deviation of Democratic vote	48	0.039092	0.013792	0.015614	0.078418
closeness (voting)	48	0.861273	0.098945	0.61402	0.984156
share Democratic vote	48	0.527462	0.049515	0.430142	0.638648
income	48	16.36192	2.545811	12.22647	23.92823
log of population	48	15.12899	1.000238	13.10979	17.35938
aged (65 and above)	48	0.129694	0.040794	0.046137	0.372649
kids (5-17)	48	0.19224	0.066791	0.049207	0.619861

Tab. 2: Testing the swing voter hypothesis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
spending aggregate	total	total	total	total	targetable	targetable	targetable	targetable	grants	grants	grants	grants
independent (exit polls)	-1.5539				-0.9330				-0.0909			
	(0.99)				(0.76)				(0.16)			
pure independent (NAES)		-12.2965				-7.5317				-3.5232		
		(2.86)***				(2.15)**				(2.47)**		
total independent (NAES)			-2.6609				-1.7552				-0.5727	
			(1.92)*				(1.57)				(1.21)	
standard deviation of Democratic vote				-7.5072				-5.2407				4.1193
				(0.90)				(0.71)				(1.58)
income	0.0048	0.0467	0.0268	-0.0140	0.0272	0.0530	0.0427	0.0158	0.0005	0.0166	0.0081	-0.0003
	(0.10)	-1.03	(0.56)	(0.37)	(0.66)	(1.38)	(1.11)	(0.50)	(0.03)	(1.35)	(0.60)	(0.03)
log of population	-0.1939	-0.2978	-0.2521	-0.2027	-0.1425	-0.2066	-0.1831	-0.1522	-0.1022	-0.1401	-0.1203	-0.0757
	(1.91)*	(3.12)***	(2.59)**	(1.92)*	(1.86)*	(3.05)***	(2.32)**	(1.94)*	(2.22)**	(3.80)***	(2.71)***	(2.22)**
aged (65 and above)	7.3584	7.0437	7.3949	6.4637	-5.0994	-5.2805	-5.0185	-5.6303	1.4912	1.6197	1.6494	1.3996
	(1.92)*	(2.07)**	(2.08)**	(1.68)	(1.76)*	(1.95)*	(1.85)*	(1.89)*	(1.23)	(1.49)	(1.39)	(1.07)
kids (5-17)	-4.3004	-4.6367	-4.4653	-2.9941	2.1583	1.9391	1.9848	2.9633	-0.5433	-0.8891	-0.7502	-0.5957
	(1.91)*	(2.39)**	(2.24)**	(1.27)	(1.32)	(1.33)	(1.36)	(1.67)	(0.78)	(1.46)	(1.14)	(0.80)
Constant	6.6832	8.3258	7.7120	6.8720	3.6967	4.7117	4.4190	3.9005	2.1823	2.8213	2.5195	1.6306
	(4.00)***	(5.28)***	(4.78)***	(3.86)***	(2.81)***	(3.99)***	(3.18)***	(2.72)***	(2.93)***	(4.74)***	(3.50)***	(2.72)***
Observations	48	48	48	48	48	48	48	48	48	48	48	48
R-squared	0.1322	0.2416	0.1784	0.1343	0.0851	0.1482	0.1177	0.0899	0.3230	0.4401	0.3554	0.3863

Robust t statistics in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Tab. 3: Testing the electoral battleground hypothesis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
spending aggregate	total	total	total	total	targetable	targetable	targetable	targetable	grants	grants	grants	grants
closeness (exit polls)	-0.8820				-0.3388				-0.5131			
	(0.57)				(0.26)				(1.81)*			
closeness 1 (NAES)		-0.4734				0.2579				-0.3775		
		(0.27)				(0.18)				(0.85)		
closeness 2 (NAES)			-1.1661				0.5232				-0.6304	
			(0.36)				(0.20)				(0.78)	
closeness (voting)				-0.7197				-0.0671				-0.6350
				(0.63)				(0.07)				(1.67)
income	-0.0110	-0.0162	-0.0187	-0.0198	0.0172	0.0177	0.0185	0.0156	0.0008	-0.0028	-0.0034	-0.0062
	(0.28)	(0.42)	(0.44)	(0.48)	(0.51)	(0.55)	(0.52)	(0.46)	(0.09)	(0.30)	(0.34)	(0.73)
log of population	-0.1506	-0.1473	-0.1362	-0.1275	-0.1182	-0.1271	-0.1310	-0.1182	-0.0957	-0.0914	-0.0882	-0.0730
	(1.90)*	(1.63)	(1.53)	(1.35)	(2.02)**	(1.90)*	(1.90)*	(1.49)	(2.66)**	(2.25)**	(1.90)*	(2.09)**
aged (65 and above)	7.5793	6.8482	7.1816	7.2251	-5.2220	-5.9200	-6.0264	-5.5984	2.1216	1.7931	1.8581	2.1638
	(1.55)	(1.52)	(1.49)	(1.50)	(1.33)	(1.61)	(1.57)	(1.47)	(1.54)	(1.25)	(1.19)	(1.52)
kids (5-17)	-4.0833	-3.6688	-3.9311	-3.6450	2.4784	3.0671	3.1404	2.7743	-0.9894	-0.8480	-0.8711	-0.8663
	(1.38)	(1.25)	(1.22)	(1.34)	(1.06)	(1.27)	(1.23)	(1.34)	(1.29)	(0.97)	(0.90)	(1.12)
Constant	6.5925	6.2908	6.8407	6.1810	3.5022	3.0653	2.8517	3.2755	2.5158	2.4147	2.6233	2.3472
	(3.63)***	(3.46)***	(2.18)**	(4.31)***	(2.50)**	(2.19)**	(1.17)	(3.17)***	(4.13)***	(4.06)***	(4.04)***	(4.30)***
Observations	48	48	48	48	48	48	48	48	48	48	48	48
R-squared	0.1258	0.1176	0.1193	0.1249	0.0783	0.0769	0.0771	0.0761	0.3614	0.3365	0.3344	0.4041

Robust t statistics in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Tab. 4: Testing the partisanship hypothesis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
spending aggregate	total	total	total	total	targetable	targetable	targetable	targetable	grants	grants	grants	grants
Democratic partisans (exit polls)	0.8372				-0.2842				0.1763			
	(0.48)				(0.16)				(0.22)			
total Democratic partisans (NAES)		2.6252				1.9101				0.9286		
		(1.18)				(1.13)				(2.10)**		
strong Democratic partisans (NAES)			3.1967				2.4442				1.0587	
			(0.99)				(1.03)				(1.57)	
share of Democratic vote				0.6026				-0.9336				-0.1152
				(0.16)				(0.28)				(0.09)
income	-0.0013	-0.0075	-0.0100	-0.0066	0.0121	0.0205	0.0188	0.0056	0.0020	0.0015	0.0005	-0.0019
	(0.02)	(0.19)	(0.25)	(0.11)	(0.24)	(0.63)	(0.56)	(0.10)	(0.11)	(0.15)	(0.05)	(0.10)
log of population	-0.1663	-0.2513	-0.2307	-0.1507	-0.1184	-0.1888	-0.1765	-0.1327	-0.1018	-0.1330	-0.1241	-0.1015
	(2.09)**	(1.95)*	(1.91)*	(1.50)	(1.88)*	(1.82)*	(1.91)*	(1.59)	(2.47)**	(2.95)***	(2.74)***	(3.03)***
aged (65 and above)	7.3079	4.4575	5.2668	6.8628	-5.9838	-7.0881	-6.5411	-6.3936	1.6264	0.7483	1.0600	1.3464
	(1.47)	(1.03)	(1.28)	(1.16)	(1.48)	(2.09)**	(2.10)**	(1.39)	(1.11)	(0.63)	(0.90)	(0.89)
kids (5-17)	-3.6971	-3.2525	-3.2758	-3.4406	2.9818	2.7816	2.7621	3.1780	-0.5826	-0.4956	-0.5022	-0.4345
	(1.32)	(1.58)	(1.55)	(1.06)	(1.37)	(1.79)*	(1.75)*	(1.31)	(0.72)	(0.83)	(0.79)	(0.53)
Constant	5.5242	6.6308	6.6042	5.3867	3.3964	3.7524	3.7575	4.1187	2.0505	2.3817	2.3574	2.2459
	(2.94)***	(4.20)***	(4.34)***	(1.35)	(2.10)**	(2.91)***	(3.16)***	(1.12)	(3.57)***	(3.93)***	(3.79)***	(2.20)**
Observations	48	48	48	48	48	48	48	48	48	48	48	48
R-squared	0.1197	0.1585	0.1479	0.1165	0.0767	0.1107	0.1048	0.0791	0.3244	0.3834	0.3625	0.3227

Robust t statistics in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Tab. 5: Testing the three hypotheses jointly

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
spending aggregate	total	total	total	total	targetable	targetable	targetable	targetable	grants	grants	grants	grants
independent (exit polls)	-1.6567				-1.6136				0.0038			
	(0.68)				(0.81)				(0.01)			
closeness (exit polls)	-0.8421				-0.3553				-0.5070			
	(0.55)				(0.27)				(1.87)*			
Democratic partisans (exit polls)	-0.2973				-1.3335				0.1179			
	(0.11)				(0.54)				(0.13)			
total independent (NAES)		-2.0860				-1.3668				-0.1795		
		(2.15)**				(1.47)				(0.32)		
closeness 1 (NAES)		0.0406				0.6212				-0.2683		
		(0.03)				(0.45)				(0.63)		
total Democratic partisans (NAES)		1.3170				1.1345				0.7787		
		(0.64)				(0.69)				(1.48)		
pure independent (NAES)			-11.3784				-6.9980				-3.1184	
			(2.78)***				(2.19)**				(1.83)*	
closeness 2 (NAES)			-0.3581				1.0291				-0.4058	
			(0.12)				(0.41)				(0.52)	
strong Democratic partisans			1.7333				1.5631				0.6530	
			(0.70)				(0.80)				(0.99)	
standard deviation of Democratic vote				-9.4434				-5.3249				3.6473
				(1.17)				(0.78)				(1.65)
closeness (voting)				-0.9525				-0.3302				-0.6446
				(0.84)				(0.35)				(1.97)*
share Democratic vote				0.5308				-0.7830				-1.0219
				(0.14)				(0.23)				(0.90)
Observations	48	48	48	48	48	48	48	48	48	48	48	48
R-squared	0.1415	0.1862	0.2508	0.1521	0.0975	0.1312	0.1637	0.0933	0.3623	0.3940	0.4591	0.4651

Robust t statistics in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include income, log of population, aged, kids and a constant