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Party Control of State Government and the Distribution of Public Expenditures*

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Abstract

This paper examines the effects of party control of state governments on the distribution of intergovernmental transfers across counties from 1957 to 1997. We find that the governing parties skew the distribution of funds in favor of areas that provide them with the strongest electoral support. This is borne out in two ways. (i) Counties that traditionally give the highest vote share to the governing party receive larger shares of state transfers to local governments. (ii) When control of the state government changes, the distribution of funds shifts in the direction of the new governing party. We find only weak evidence that parties reward electorally pivotal counties or counties in electorally pivotal legislative districts. Finally, we find that increased spending in a county increases voter turnout in subsequent elections. This suggests that parties have an electoral incentive to skew the distribution of funds to influence future election results, and the mechanism through which this works is "mobilization" rather than "conversion" of voters in a fixed electorate.

Keywords: Public expenditures; distributive politics; swing voter; political parties
JEL classification: D72; H72

I. Introduction

Political parties are often viewed as teams of politicians and supporters seeking to gain control of the government. They are instrumental in choosing who will serve as representatives and in determining who holds positions of power after elections.

What do parties and their supporters get from control of government? Among other things, parties are thought to influence how the public dollar is divided—the *distribution* of public expenditures across regions

and groups. The winning party, it is widely conjectured, rewards its supporters with pork, with a larger share of expenditures from existing programs. Commenting on a press report that the Republican party redirected billions in spending to Republican areas in the wake of the 1994 election, Republican majority leader Richard Armey quipped "to the victors belong the spoils".[1]

This paper examines the relationship between party control and the distribution of public funds in the American states from 1957 to 1997. At the state level, we know of only one study that has estimated effects of party control on the distribution of public funds; see Ansolabehere, Gerber and Snyder (2002). That study examines the effects of malapportionment on the distribution of public expenditures in the years surrounding *Baker* v. *Carr*, and, in passing, notes that the areas that give the highest support to the majority party receives a higher share of state expenditures. The present study tests more fully whether the majority party skews public funds toward its areas of core electoral support.

The relationship between party control, voter preferences and the distribution of public funds is important in its own right as it is one way to assess who gets what from politics. Our analysis also speaks directly to an important question engaging many theorists today. A number of recent papers have developed analytical models in which public expenditures are distributed across regions or groups in order to win votes or elections in the future. These models can be divided into two groups—"loyal voter" models and "swing voter" models, according to which segments of the electorate receive higher shares of funds.

The strategy of targeting swing voters rests on the assumption that expenditures affect which party a voter will choose, rather than whether

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[1] Quoted in David Pace, "1994 Shift Seen to Aid GOP Areas", *The Boston Globe*, August 6, 2002, p. A5. Parties might also reward their supporters by changing the ideological bent of the government, or the general direction of government policy on broad issues, such as the overall size of government, spending priorities, and the general contours of economic and social regulation. An extensive literature examines whether the governing party can increase the *level* of spending on particular programs that are central to that party's ideology, such as the Democrats and welfare. At the state level, there is little evidence that the majority party is able to increase spending on its preferred programs. Dye (1966), Fry and Winters (1970), Jones (1974), Winters (1976), Maquette and Hinckley (1981), Plotnick and Winters (1985, 1990), Lowery (1987), Erikson, Wright and Molver (1993) and others explore policy differences across U.S. states and find small, insignificant or "incorrect" effects of party control on policy outcomes. A few studies— e.g., Erikson (1971a), Garand (1985) and Alt and Lowry (1994)—find evidence that party control matters in the predicted direction. Others, such as Jennings (1979) and Dye (1984), find mixed results. At the national level, there are stronger correlations between a party's share of congressional seats and spending on programs that party favors. However, as Kiewiet and Krehbiel (2002) and others point out, these correlations may not actually reflect the causal effects of party control, since shifts in the share of seats held by a party reflect shifts in voter preferences.

someone will vote. Lindbeck and Weibull (1987), Dixit and Londregan (1995, 1996) and others develop analytical models in which parties will target disproportionate resources to "pivotal" groups or regions.[2] In these models turnout is fixed, so electoral competition is driven by efforts at "conversion" rather than mobilization.

Loyal voter models take three forms. First, parties may simply seek rents, allocating more government spending on the projects and programs that benefit their members and supporters. Areas with relatively high concentrations of the majority party's supporters will then receive more money. Second, spending may *mobilize* people to vote, either directly or through interest groups that benefit from state contracts, rather than convert them. In this case, the optimal strategy is to spend more money in areas where a party has more of its own supporters; see e.g. Kramer (1966), Cox and McCubbins (1986) and Sim (2002). Third, shared responsibility and credit for programs may lead a party's politicians to spend money where there are more loyalists. When a single individual or party represents an area, it is a simple matter to reward (or punish) the incumbent. However, when many politicians of different parties represent an area it is difficult to share credit or send a partisan message. Individual legislators, then, will seek to spend funds in ways that maximize the credit they or their parties receive. The strategy that maximizes the credit received by a party or the incumbents from a party receives will be to skew funds toward areas dominated by that party; see e.g. Dasgupta, Dhillon and Dutta (2001).

These models may not be exclusive. It may be the case, for example, that parties choose to target both loyal and swing areas.

Research on party control of government and the distribution of public expenditures in the U.S. is surprisingly thin. Several studies of the U.S. federal government find a positive relationship between the share of spending going to an area and the Democratic vote in the area; see e.g. Browning (1973), Ritt (1976), Owens and Wade (1984) and Levitt and Snyder (1995). This finding is consistent with the loyal voter models, but it is limited by the historical cases under investigation. Since Democrats were the majority party in Congress during the years studied the results might also reflect the behavior of the Democratic party or the characteristics of areas that tend to vote Democratic.[3] 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; see Holden (1973), Rakove (1975), Erie (1978) and Johnston (1979). Outside the U.S., Dasgupta *et al.*

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[2] Snyder (1989), Strömberg (2002) and others develop similar models in the context of allocating campaign resources.

[3] Levitt and Snyder (1995) compare programs passed during years of unified Democratic control with programs passed during years of divided government. They find that the former exhibit a pro-Democratic geographic bias, while the latter do not.

(2001) find that in India provinces where the governing parties are stronger receive larger shares of public grants.

Several studies find evidence supporting the swing voter models in some contexts, but mixed or no evidence in other contexts. Wright (1974) finds that the allocation of New Deal spending, federal grants and employment depended on the volatility of the vote in presidential elections, though not whether the state was close to 50–50.[4] Outside the U.S., Dahlberg and Johansson (2002) find that the distribution of environmental grants across the 20 regions of Sweden is concentrated most heavily in electorally pivotal regions of the country.

The states are ideal for measuring the effect of party control on the distribution of public funds. Party control varies considerably across states. All of the studies above have a limited amount of variation in the key independent variables—which party controls the government. In the studies of the U.S. federal government, for example, almost all of the cases have unified Democratic control or a Democratic legislature facing a Republican executive. Unlike the national government, we can contrast a large number of cases of unified Democratic control, unified Republican control and divided control. A further strength of the state data is that party control of states varies considerably over time. This allows us to measure the effects of changes in party control and counties' partisan preferences on changes in the distribution of public expenditures. Within states, partisan preferences of the electorate vary considerably across counties and over time. And, the panel structure of the data allows us to hold constant many factors that are not readily measured.

II. Data and Methods

The Census of Governments provides reliable and comparable data on the distribution of state government expenditures over the last half century. The census collects these data at five-year intervals, yielding a panel with nine waves (1957, 1962, 1967, 1972, 1977, 1982, 1987, 1992 and 1997) and approximately 3,000 counties.

The dependent variable is per capita intergovernmental transfers from the state government to all local governments inside the county, including the county government, municipal governments, school districts and any special districts operating in the county. We study state intergovernmental transfers because this is the most comprehensive measure of the distribution of state funds across locales. Transfers encompass a wide range

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[4] See also Wallis (1987, 1996) and Fleck (1999). Strömberg (2001) studies counties, and finds that the relationship between federal receipts and competitiveness vanishes when state fixed-effects are included.

of programs, including education, highways and roads, hospitals and public health, housing, and welfare. And, transfers account for 35–40% of all state government spending. It is the most comprehensive measure available of the distribution of state government spending across locales, and has been used in numerous previous studies; see e.g. Brady and Edmonds (1967), Fredrickson and Cho (1970) and Ansolabehere *et al.* (2002).

State transfers to local governments account for a large number of programs and a noticeable share of state and local spending, typically one-third of state spending and 40% of local spending. Studies focusing on single programs typically cover 5% or less of state or federal spending; see e.g. Dahlberg and Johansson (2002) and Herron (2004).

Two problems with direct analysis of transfers are that counties vary in populations and states vary in total intergovernmental revenues. To make the measure more readily comparable across counties and across states, we examine per capita transfers for each county, and we measure these quantities relative to the state averages. Let i be a typical county in state j and year t. For each variable X, we define the new variable $\tilde{X}$ as $\tilde{X}_{ijt} = X_{ijt}/\bar{X}_{jt}$, where $\bar{X}_{jt} = (1/n)\sum_{i=1}^{n} X_{ijt}$. In most specifications we take natural logarithms of the variables as well, and use $\log(\tilde{X}_{ijt}) = \log(X_{ijt}/\bar{X}_{jt}) = \log(X_{ijt}) - \log(\bar{X}_{jt})$. We apply this transformation to all of the continuous variables in the analysis. This transformation removes much of the state-level variation in the data that is also captured with fixed effects.

Our dependent variable, then, is per capita state transfers relative to state per capita transfers. Equivalently, this is the county's share of state transfers relative to the county's share of state population. If a county receives funds in proportion to its share of the population, then this measure equals 1. If a county receives a score greater than 1, say 1.2, then that county received 20% more per capita than the state average. Similarly, a score less than 1, say 0.8, means that the county received proportionately less than the state average.

The main independent variables of interest are majority control and pivotality. Theoretical arguments predict an interaction between partisanship of voters and party control of state government. Democratic counties are expected to receive more transfers when the state is under Democratic control than when the state is under Republican control; and Republican counties should receive more transfers when the state is under Republican control than when the state is under Democratic control.[5]

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[5] Because our main hypothesis concerns cross-county comparisons within states, we dropped all states with fewer than 10 counties: Delaware (3 counties), Hawaii (4), Rhode Island (5) and Connecticut (8). Including them does not affect the results. We dropped Nebraska because it had a non-partisan legislature for the entire period under study. We dropped Minnesota prior to 1972 for the same reason. We also dropped Alaska due to data limitations.

There are a variety of ways to measure party control of the state government. To begin, we define each year in a state as being under Democratic control if (i) Democrats have a majority in both legislative chambers and the governor is a Democrat, or (ii) Democrats have a veto-proof majority in both legislative chambers. Republican control is defined analogously. If neither major party has control then we say the state is under divided control. Because budgets change incrementally, we construct a moving average of past control. Specifically, we define a period of *Democratic Control* in a state as an eight-year period in which the state was under Democratic control for four or more of the years and the state was under Republican control for fewer than four years during the period (so, 0–4 of the years could be years of divided control). Periods of *Republican Control* are defined analogously. Periods that are neither Democratic nor Republican are under *Divided Control*.[6] We also varied the definition of control, changing the length of each period (longer or shorter) and changing the number of years of control required to define a period of control. The results were qualitatively similar. We chose to use the eight-year window as it seems to capture both the notion that budgeting occurs incrementally and that short-term changes in control affect the distribution of funds.[7]

Most states switched party control at least once during the period under study. Thirteen states were controlled by the Democratic party throughout (AL, AR, GA, KY, LA, MD, MS, NC, OK, TN, TX, VA, WV), and one was divided throughout (MI).

To measure the relative partisanship of a county's voters we average the two-party vote received by the Democratic candidates in all races for president, U.S. senator and governor held over the preceding eight years. Thus, for 1962 we using the elections of 1954–1960, for 1967 we using the elections of 1960–1966, and so on. We call this variable the *Average Democratic Vote* in a county. We use the same window for measuring partisanship as for control.

Previous empirical research has generally employed two different measures of the extent to which an area is politically pivotal. First, some

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[6] As an example, to construct the control variables for predicting transfers in 1962 we look at the period 1955–1962.

[7] We are grateful to Robert Inman and Jeff Milyo for their comments on this matter. A lag of at least two years seems necessary because the immediate budget is set by the prior government, not the immediate one. Professor Inman suggested horizons longer than eight years because programs are long-lived and changes may occur at glacial pace. Professor Milyo favored an horizon shorter than eight years, because eight years smooths over many short-run shocks that affect control. One problem with the eight-year window, or any longer window, is that is stretches beyond the five-year interval between Censuses of Governments. We tried six-, five- and four-year windows and found that the results are roughly the same as with eight years, and sometimes even stronger.

studies define pivotal areas as those where the partisan balance is close to 50% Democratic and 50% Republican. Other studies define pivotal areas as those where the vote is highly variable from one election to the next; see Wright (1974).[8] We include both sorts of measures in our analysis. The variable *Closeness to 50–50 of Democratic Vote in County* is the absolute deviation of the Average Democratic Vote from the 0.5. The variable *Standard Deviation of Democratic Vote in County* is the standard deviation of the Democratic share of the two-party vote over the preceding eight years in all races for president, governor and U.S. senate.[9]

If party leaders are concerned about control of the state legislature, then they may care more about *legislative districts* than counties. We therefore contructed analogous measures for the legislative districts containing each county, for all counties where this was possible. For each legislative district, we first aggregated the Democratic and Republican votes across all counties contained inside the district, and using the eight-year window, constructed the Average Democratic Vote in the legislative district. Since there are two legislative chambers in each state (except Nebraska), for each county we then constructed four variables: closeness to 50–50 of the Democratic vote in the lower-house district containing the county, closeness to 50–50 of the Democratic vote in the upper-house district containing the county, standard deviation of the Democratic vote in the lower-house district containing the county, and standard deviation of the Democratic vote in the upper-house district containing the county. We averaged the first two of these to make the variable *Closeness to 50–50 of Democratic Vote in Legislative District*, and we averaged the second two to make the variable *Standard Deviation of Democratic Vote in Legislative District*.[10]

Other political factors also affect the distribution of public spending. One important variable highlighted in prior research is the degree of malapportionment. Ansolabehere *et al.* (2002) show that, before equalization of state legislative district populations, overrepresented counties received substantially greater shares of funds from the state than underrepresented counties.

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[8] An alternative approach is to measure the fraction of Independents or moderates in a state. Dahlberg and Johansson (2002) attempt this using Swedish survey data for regions of that country comparable to states. It is impossible to find adequate survey data for U.S. counties.

[9] The standard deviation is taken around the county mean (not 50%). This is the measure introduced by Wright (1974). We also constructed single measures by interacting these variables, but these composite variables were never statistically significant.

[10] We could not construct these variables for counties that were divided roughly equally across several legislative districts. If a county was divided across districts, but more than 75% of the population lived in one district, we assigned the county to that district and included it in the analysis. For a few state-years we were unable to find district definitions or maps allowing us to match counties and districts.

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We include the variable *Relative Representation Index* to capture this effect.[11]

Beyond political considerations, a variety of demographic factors directly affect state transfers. Because many transfers are for education, poverty, health and welfare programs, we expect that school-aged population, median or per capita income, poverty rates and percent elderly affect levels of transfers. We also include the percentage of the population that is African American. Also, because of "incremental budgeting", county population is likely to negatively affect the levels of expenditures. If there are lags in adjusting the allocation of transfers to population shifts, then as a county's 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 sources for all variables used in our analysis are in Table A1 in the Appendix.

III. The Effects of Party and the Distribution of Funds

A simple analysis is immediately instructive about how party and partisanship shape the distribution of state spending.

Table 1 displays the average share of per capita transfers received by the state across different voting patterns of counties and different conditions of party control. The rows represent the partisan leanings of the county

Table 1. *Party control, county partisanship, and the relative share of intergovernmental transfers from state government, 1957–1997*

	Democratic control	Divided control	Republican control
Democratic County	0.10	0.05	0.00
	(0.33)	(0.31)	(0.31)
	[1,060]	[1,901]	[1,492]
Mixed County	−0.01	−0.01	0.00
	(0.26)	(0.24)	(0.24)
	[1,061]	[1,933]	[1,507]
Republican County	−0.09	−0.04	−0.01
	(0.26)	(0.28)	(0.25)
	[1,092]	[1,979]	[1,554]

Notes: The first entry in each cell gives the mean of Relative Share of Intergovernmental Transfers from State Government. The second entry (in parentheses) gives the standard deviation. The third entry (in brackets) gives the number of observations. Only states in which Control changed at least once are included.

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[11] This is the variable used by Ansolabehere *et al.* (2002). Other papers find similar effects of malapportionment in other contexts; see Atlas, Gilligan, Hendershott and Zupan (1995), Lee (1998) and Lee and Oppenheimer (1999).

relative to the rest of the state—more strongly Democratic than the state average, more strongly Republican, and Mixed. The columns represent party control of the government—unified Democratic Control, unified Republican Control and Divided Control. Each entry in the table is the average per capita share of state transfers. This table contains only the cases where party control actually switched from one party to another or from one party to divided control. We omit the states where party control did not change throughout the 40-year period to facilitate interpretation of the table: reading down the columns can be interpreted as changes in the distribution of funds across counties when party control of the state government changes. The patterns are the same for the states that did not vary in their party control.

The data offer clear support for the idea that the majority party targets its core supporters. This holds true when viewed either from the perspective of varying partisanship across counties or changes in control within a state.

First, consider how changes in party control in the state alter the share of state revenues received by a county. To see this, read across each row. The first row in the table represents strongly Democratic counties relative to the rest of the state. These counties receive 9% more money when the Democrats control state government (the first cell in the row) than when Republicans control state government (the last cell), and they receive 5% more money when the Democrats control than when control is divided (the middle cell). Republican counties show the opposite pattern: they receive 8% less money when the Democrats control the state government than they do when the Republicans are in control. The mixed counties receive about the state average regardless of who controls the state government.

Second, consider the distribution of funds across counties, holding constant which party controls government. When Democrats control the state government, strongly Democratic counties receive 10% more from the state than one would expect, and strongly Republican counties 9% less than the state average. When Republicans control state government, the differences across counties are small.

The data offer little support for the swing voter models. A central prediction of such models is that parties will target areas that are closely divided and may swing the election. Counties that split their vote relatively evenly between the parties do not receive more than the average county. Averaging across conditions of party control, the swing counties received a 0.01 share of relative transfers. In other words, they received, on average, about 1% more money than one would expect, given their share of state population. This is not significantly different from 0, which would mean that these counties received almost exactly the share of funds one would expect given their populations—not more. Democratic counties received about 4% more than one would expect and Republican counties received about 4% less than one would expect.

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Table 1 is suggestive, but not definitive. State government expenditures depend on many factors other than partisanship and party control. Formulas for distributing funds within states often include demographic factors like total population, income and school-aged population. These may be correlated with partisanship in ways that might make Table 1 misleading. In particular, it appears that there is a net transfer from Republican counties to Democratic counties, which likely reflects modest income transfers within the states.

Table 2 addresses these concerns. In column 1 we include only the variables measuring each county's partisanship interacted with party control of the state government. In columns 2 and 3 we add the variables measuring each county's pivotalness. In columns 4–6 we add instead the pivotalness of each county's state legislative districts (recall, this is an average of the pivotalness of the county's upper- and lower-house districts). In columns 2 and 5 we include only the explicitly political variables, and in columns 3 and 6 we add other demographic and socioeconomic variables. In column 4 we include only the two main variables associated with the legislative district version of the pivotal voter model.

In all cases, we exploit the panel structure of the data to control for a variety of other potential omitted variables. We include county and year effects.[12] The panel regressions, then, are analogous to regressing differences in the dependent variable on differences in the independent variables. In addition, we include county-level measures of income, poverty, school-aged population and elderly population (in columns 3 and 5). The estimated coefficients and standard errors are shown in Table 2. In all cases, we cluster the standard errors by county to deal with potential serial autocorrelation.[13]

Two sets of independent variables are of immediate interest. First, to measure the direction in which the governing party skews funds we include the interaction between which party controls the state government and the partisanship of the county. Specifically, average Democratic Vote is interacted with Democratic Control, Republican Control and Divided Control. (The sum of these three variables is the average Democratic vote share.) This specification allows for different slopes on Average Democratic Vote for the three cases of party control. In Table 2 these variables are labeled: *Democratic Vote times Democratic Control*, *Democratic Vote times Republican Control* and *Democratic Vote times Divided Control*. Second, to

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[12] In separate estimates, not shown, we included state times year effects, but these did not appreciably improve the fit or affect the estimates.
[13] Our findings reflect changes in party control within states as well as variation in control across states. To test the robustness of our results, we restricted the sample to states where party control changes at least once. The results are substantially the same as those reported in Table 2.

Table 2. *Intergovernmental transfers and party control, 1957–1997*

Dep. Var. = Relative Share of Transfers from State Government

Average Democratic Vote times Democratic Control	0.16** (0.03)	0.16** (0.03)	0.11** (0.03)	—	0.17** (0.03)	0.13** (0.03)
Average Democratic Vote times Divided Control	0.10** (0.04)	0.11** (0.04)	0.05 (0.03)	—	0.10** (0.04)	0.055 (0.03)
Average Democratic Vote times Republican Control	−0.16** (0.04)	−0.16** (0.05)	−0.13** (0.04)	—	−0.18** (0.05)	−0.13** (0.04)
Closeness to 50–50 of Democratic Vote in County	—	−0.03 (0.04)	0.04 (0.04)	—	—	—
Standard Deviation of Democratic Vote in County	—	0.003 (0.005)	0.003 (0.005)	—	—	—
Closeness to 50–50 of Democratic Vote in Legislative District	—	—	—	0.01 (0.05)	0.07 (0.05)	0.12** (0.04)
Standard Deviation of Democratic Vote in Legislative District	—	—	—	0.006 (0.005)	0.005 (0.005)	0.005 (0.005)
Relative Representation Index	—	—	0.14** (0.01)	—	—	0.14** (0.01)
Population	—	—	−0.16** (0.02)	—	—	−0.16** (0.02)
Per Capita Income	—	—	−0.18** (0.04)	—	—	−0.19** (0.04)
Percent in Poverty	—	—	0.13** (0.02)	—	—	0.13** (0.02)
Children in School Per Capita	—	—	0.66** (0.05)	—	—	0.68** (0.05)
Percent Age 65 and Over	—	—	−0.05 (0.03)	—	—	−0.04 (0.03)
Percent Black	—	—	0.001 (0.003)	—	—	0.001 (0.003)
Number of Observations	25,711	25,664	25,647	24,020	22,969	22,953

Notes: All specifications include county fixed effects, year fixed effects, and dummy variables for Democratic Control and Republican Control (Divided Control is the excluded category). Robust standard errors, clustered by county, are in parentheses. * Significant at the 0.05 level; ** significant at the 0.01 level.

measure the effect of pivotalness on the distribution of funds we included two measures of swing areas—the deviation of the average Democratic vote share from 0.5 and the standard deviation of the vote share. Some scholars use the deviation from 50–50 as an indicator of swing districts, while others argue that standard deviation is a better measure since it captures how much expenditures and other short-term policy decisions are able to move votes. As discussed above, we measure pivotalness at both the county and legislative district level. In the analysis we present results for both variables.

Overall, the estimates in Table 2 provide only weak evidence that parties target pivotal counties or counties in pivotal legislative districts. Instead, the

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estimates support the argument that the majority party rewards areas with high concentrations of loyal voters.

If the pivotal voter model is correct and substantively important, then the Closeness to 50–50 of Democratic Vote and Standard Deviation of Democratic Vote variables should strongly and positively affect the distribution of funds. The coefficients on these variables have the expected sign, but in all but one case they are statistically insignificant and their magnitudes are small. The one significant coefficient is for Closeness to 50–50 of Democratic Vote in Legislative District. It becomes significant once we control for other political variables, including the majoritarian factors, and demographics. This suggests that state legislative leaders consider "swing districts" to some degree, but only in light of other factors.

These findings pose a problem for swing voter models. The basic logic of swing voter models is not conditional, but is supposed to stand on its own. But the results in Table 2 reveal that, at least as applied to the U.S. states, it is at best highly conditional. When we include only the variables that measure pivotal counties (not shown) or counties in pivotal districts (column 4), the estimated coefficients are substantively small and statistically insignificant.

The key tests of the majoritarian argument are whether the slope on Democratic Vote times Democratic Control is positive and the slope on Democratic Vote times Republican Control is negative. The coefficient on Democratic Vote times Divided Control should lie between these two and possibly be indistinguishable from 0. Table 2 shows that in all specifications the estimated coefficient on Democratic Vote times Democratic Control is positive and the estimated coefficient on Democratic Vote times Republican Control is negative, and they are both highly significant. Looking at column 3, for example, which includes all control variables, the coefficient on Democratic Vote is $+0.11$ under Democratic control and -0.13 under Republican Control. The coefficients on Democratic Vote times Divided Control are consistently positive and sometimes statistically significant, although not when all of the control variables are included. In all cases, they are closer to zero as expected.

To gauge the magnitude of these effects, consider a county that is relatively Democratic compared to the rest of the state. Specifically, consider a county that is 65% Democratic in a state that is 50% Democratic (this 15-percentage-point difference is a bit more than one standard deviation). If the state government switches from Republican to Democratic control, then this county can expect to see an increase in per capita intergovernmental transfers of about 6.5%.[14] A county that is only 35% Democratic can expect to lose a similar amount. On average, counties

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[14] The calculation is straightforward: $(65/50)^{(0.11+0.13)} = 1.065$.

received about \$600 per person in state intergovernmental transfers in 1997, so the gain or loss would be roughly \$40 per person. The estimates in column 1 imply an even larger difference—8.8%, or more than \$50 per person in 1997.

Relative Representation affects the distribution of funds on a similar magnitude. This effect is identified via the large discrepancies in state legislative district populations at mid-century, and the interventions of the courts in the mid-1960s requiring equalization of district populations; see David and Eisenberg (1961) and Ansolabehere *et al.* (2002). The coefficient on the Relative Representation Index, which measures the number of legislative seats per capita a county has relative to the state average (in logarithms), is 0.14. A one-standard-deviation change in the Relative Representation Index variable produces a 10% change in per capita transfers, or about \$60 in 1997.[15] This finding is consistent with the findings of previous studies on malapportionment and the distribution of public finances (cited above). The more representation a county has (per capita), the greater the share of funds it receives.

Most of the demographic and socioeconomic controls all have the predicted signs and are quite significant. On average, a county receives relatively more transfers per capita when it becomes smaller and poorer, and when the number of school-age children per capita increases. Percent Age 65 and Over and Percent Black are insignificant.

To further test the robustness of these estimates, we ran the panel analyses for each state separately using the specification in column 6 of Table 2. These regressions exploit changes in party control and district pivotalness within counties in each state. Analysis on the full model exploits some cross-state variation as well. In the state-by-state regressions, one-third of the states produced significant effects of party control that were consistent with the underlying model; only one case was inconsistent.[16] Seven cases showed significant effects of pivotalness with the correct sign, and five showed significant effects with the wrong sign.[17] These patterns suggest that the non-finding for pivotalness reflects the behavior of the data on

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[15] A county with a Relative Representation Index of 1 has exactly the state average amount of representation. The standard deviation of the Relative Representation Index is about 1, so we consider a change from 1 to 2.

[16] Forty-five states have enough counties to estimate the parameters of interest with a reasonable degree of confidence. The states with significant coefficients on party control and partisanship that are statistically significant are AL, AZ, IN, KS, ME, MS, MT, NJ, NV, NY, OH and TX. In one state, NC, there is a significant relationship in the wrong direction.

[17] Statistically significant and positive coefficients on Closeness to 50–50 occur in IA, ID, IL, KS, NV, TX and VA. Statistically significant but negative coefficients occur in FL, KY, NH, NY and SC. Also, statistically significant and positive coefficients on Standard Deviation of the Democratic Vote occur in AL, IN and WI, and statistically significant but negative coefficients occur in MT, OH and TN.

average and within states; more often than not the pivotalness of a county has the wrong sign, although it is rarely significant. Also, the effects of party control are borne out in a large share of the states, but not as many as one would like. This reflects the lack of variation in control in many states, especially most southern states. In terms of our estimates, this robustness check implies that the estimated effect of party control reflects cross-state comparisons as well as within-state changes over time, and that within state analyses often do not offer sufficient variation in party control to completely identify the effects.

This suggests an important methodological lesson. Studies of a single state, or even a single national government, may lack sufficient variation to allow estimation of the effects of party control or pivotalness. The many reasons for non-findings in studies of the distribution of federal funds in the U.S., we conjecture, likely reflects the lack of sufficient variation in party control of the government.

A possible explanation for the insignificant effects of pivotalness is heterogeneity: the effects might vary as the political context differs across states. Specifically, it may be the case that the majority party more readily manipulates funds for electoral purposes when it is electorally vulnerable. To test this, we constructed a measure of party competition at the state level using election returns for president, U.S. Senate and governor. For each state and census year, we calculated the average division of the two-party vote in each state for these three offices over the eight years prior to the census year of the observation. We then calculated the closeness of the partisan division of the state. States divided by 5% or less were considered "marginal" or "competitive".[18] We interacted an indicator variable for competitive state with the measures of pivotalness and the measures of partisanship of the county times party control of the state government. Of note, many states had highly competitive statewide elections but were, nonetheless, controlled by a single party, and many states had relatively less competition statewide but had divided government, because the legislature was of a different party.

We estimated the specifications in Table 2, including the measures of swing counties and the measures of partisanship and party control, and the interactions of these measures with party competition. (The full estimates are available on request.) No significant interactions emerged in the analyses. As in all previous analyses, pivotalness—how close a county's vote is to marginal (i.e., a 50–50 division of the two-party vote) and the volatility of the county's vote (i.e., the standard deviation of the vote)—has no

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[18] So, e.g., for observations in 1977 we use the average presidential, senatorial and gubernatorial vote from 1969 to 1976 within each state. We considered other cutoffs as well, including 3%, 7% and 10%. The results are the same.

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statistical effect on governmental transfers. The effects of these measures are indistinguishable from 0 in highly competitive states and in relatively less competitive states. Partisan counties, again, receive disproportionately more when their party controls government. In the less competitive states (i.e., the non-interacted variables), the coefficients show a strong relationship of a county's share of intergovernmental transfers to county partisanship and party control of state government. But, the overall competitiveness of the state does not magnify the effects. The coefficients on partisanship and party control in the highly competitive states are statistically indistinguishable from the coefficients on these variables in less competitive states.[19] In other words, the tendency to shift funds to loyal voters does not appear to be affected by the strength of the electoral pressures or threat facing the majority party. This pattern suggests that rent-seeking, rather than vote-seeking, may motivate parties.

Overall, our results point to a simple conclusion: transfers to local governments in the American states go disproportionately to areas where the state's majority party has strongest support, and transfers are steered away from areas where the opposing party is strongest. Such findings are broadly consistent with "loyal voter models" and inconsistent with "swing voter models".

IV. Electoral Incentives

Why reward loyal areas? As mentioned in the introduction, three sorts of politics might sustain this pattern: (i) mobilization, (ii) credit-claiming and (iii) rent-seeking. These arguments, of course, are not exclusive. All three might contribute in varying degrees to the empirical pattern observed. Also, credit-claiming may be a special case of mobilization, as the electoral gains to incumbents may come either through mobilization or conversion.[20]

Here, we focus on the assumption that spending mobilizes voters for two reasons. First, that assumption is sufficient to sustain a loyal voter model. A handful of analytical models conjecture that increases in spending spur higher turnout in *subsequent* elections. Government expenditures might produce higher turnout for a variety of reasons. Raising the number of jobs dependent on state grants and contracts might make people in an area feel that they have a greater stake in what government does and therefore that their votes matter personally. Firms and organizations (especially unions)

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[19] The coefficients on the interaction terms were substantively small—none bigger than a plus or minus 0.04—and each had t-statistics smaller than 1.20.

[20] To test the credit-claiming argument requires data that match each elected official to each county. To date there exists no state legislative elections database that predates 1968, and for the existing database it is exceedingly hard to match counties to districts. Further study of the credit-claiming argument is in order but is beyond this study.

that depend heavily on state funds may also be more likely than other firms to mobilize their employees to vote. Second, theoretical models of pivotal voters and swing voters assume that turnout is unrelated either to the distribution of resources or the positions taken by the parties. Any correlations between government transfers and turnout contradict the assumptions of the pivotal voter models.

Previous research on the electoral effects of spending and transfers has focused almost exclusively on the effects of public spending programs on the vote shares of incumbents in U.S. congressional elections. Findings are mixed; earlier papers show little effect, while more recent work suggests a larger effect; see e.g. Stein and Bickers (1994), Alvarez and Saving (1997) and Levitt and Snyder (1997). We know of no research that looks for effects of public spending programs on turnout.

We exploit two features of the panel of states to identify the causal effect of Transfers on Turnout. First, we estimate the effect of spending on turnout within counties over time, essentially regressing differences in turnout on differences in spending. Possible simultaneity between spending and turnout remains. Second, we construct an instrument for spending using reapportionment of the states in the 1960s. We use court-ordered reapportionment as a quasi-experiment to estimate the effect of spending on turnout. The degree of malapportionment varied dramatically across counties within states and across states. Legislative bargaining under malapportionment produced highly unequal distributions of public spending, which the imposition of equal population representation reduced substantially; see Ansolabehere *et al.* (2002). Court-ordered reapportionment, then, offers a strong potential instrument for changes in a county's share of inter-governmental transfers within counties over time.[21] One caveat with this instrument is the amount of campaigning may serve as a possible excluded intervening variable. Areas that had fewer legislative seats than they deserved might have seen a large increase in campaign activities once they were allotted the appropriate number of seats. That is possible but it is not of a sufficiently large magnitude to completely account for the results here.[22]

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[21] Of course, for this change to provide a valid instrumental variable, malapportionment must not affect turnout directly. Past research on the direct electoral effects of reapportionment has found little evidence of a systematic effect of malapportionment on state-level electoral competition; see e.g. Erikson (1971b). In our data the cross-sectional correlation between the degree of malapportionment and the electoral competitiveness of a county is just -0.11; the within-county correlation between change in malapportionmnent and change in competitiveness is just -0.07.

[22] Data on state legislative elections are not available. To gauge the possible effects we examined U.S. House elections and decennial reapportionment. In the small number of states where reapportionment increased or decreased representation by at least 20%, the effect of

The dependent variable is *Total Turnout* as a fraction of population, averaged over the two governor elections subsequent to a specific census. For example, the expenditures recorded for 1957 in the Census of Governments are assumed to affect turnout in the 1958 and 1962 governor elections.[23] To remove state-year effects and to facilitate interstate comparisons, each county's turnout is divided by the state turnout in the relevant years.

The key predictor of turnout is *Share of Intergovernmental Transfers*. In the least squares panel model, this is an exogenous variable. Because we include fixed effects for each county, the coefficient on this variable reflects how changes in transfers translate into changes in future turnout. In the instrumental variables estimates, Share of Intergovenmental Transfers becomes an endogenous included variable. The first-stage regression predicts this variable using the county's share of legislative seats relative to its share of population, i.e., the Relative Representation Index, and other exogenous variables.

As control variables, we included lagged turnout, various demographics and electoral closeness. Lagged turnout and demographics are important because many other factors account for turnout. It should be noted that the relationship between spending and turnout might imply that lagged turnout should not serve as a control variable in the analyses reported in Table 2. Sensitive to this possibility, we performed the analyses with and without lagged turnout included. We also include year effects to capture short-term variations and trends in turnout.[24]

Table 3 presents results from four different specifications: two estimated using ordinary least squares and two using instrumental variables estimators; and two with lagged turnout and included and two without.

Turnout depends positively and significantly on the county's share of state transfers per capita in all specifications. The coefficients differ across specifications, but not widely. The instrumental variables estimates are somewhat larger than the least squares estimates. Including lagged turnout lowered the estimated coefficients only slightly. The estimates imply that a standard deviation rise in transfers boosts turnout modestly. Specifically, the within-county standard deviation in per capita share of transfers is 0.26. The predicted effect of a one-standard-deviation increase in per capita shares of transfers ranges from 1.3 in specification (1) to 3.0 in specification (3).

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a 1% increase in seats was a 0.28% increase in campaign spending, and the coefficient was 0.08 for all states. Across all elections the effect of a 1% increase in campaign spending was a 0.07% increase in turnout. This bias then is in the range of 0.005 to 0.03.

[23] We also considered a shorter lead time of four years and found similar results. We use an eight-year period to remain consistent with the analysis in Section III.

[24] We also ran specifications that include state-times-year fixed effects, to capture the effects of state laws on registration and voting, as well as changes in these laws. The results are essentially the same as those reported below.

Table 3. *Estimated effects of intergovernmental transfers on turnout*

Dep. Var. = County Turnout Relative to State Turnout in Succeeding Elections, 1957–1997

	Least squares		Instrumental variables	
Share of Per Capita	0.04**	0.03**	0.09**	0.09**
Intergovernmental Transfers	(0.01)	(0.01)	(0.03)	(0.02)
Lagged Turnout	—	0.51**	—	0.51**
(average over last four years)		(0.01)		(0.01)
Population	0.05**	0.07**	0.05**	0.07**
	(0.01)	(0.01)	(0.01)	(0.01)
Per Capita Income	0.24**	0.06**	0.25**	0.07**
	(0.02)	(0.01)	(0.02)	(0.01)
Percent in Poverty	0.00	−0.02**	−0.01	−0.03**
	(0.01)	(0.01)	(0.01)	(0.01)
Children in School Per Capita	0.01	0.04*	−0.02	−0.01
	(0.03)	(0.02)	(0.03)	(0.02)
Percent Age 65 and Over	0.13**	0.06**	0.13**	0.06**
	(0.02)	(0.01)	(0.01)	(0.01)
Percent Black	−0.004**	−0.002**	−0.004**	−0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Number of Observations	26,750	26,750	26,750	26,750

Notes: All specifications include county fixed effects and year fixed effects. Robust standard errors, clustered by county, are in parentheses. *Significant at the 0.05 level; ** significant at the 0.01 level.

We do not view this analysis as definitive. Questions remain about the validity of the instrumentation, even though the bias is unlikely to be large, and further inquiry should attempt to identify the precise mechanisms at work. This may work through interest groups aligned with a party—such as firms that contract with the government and labor unions, which mobilize their workers to vote. It may also work directly through the voters, who may be dependent on the government or who may reward and punish the majority party for its willingness to spend money on important programs.

What is clear from our analysis of electoral incentives is the strong connection between turnout and government spending. The distribution of public expenditures directly affects turnout. Parties can gain, therefore, by targeting their core areas.

V. Conclusions

We have tested the validity of two important theoretical models of the electoral strategies of parties, the swing voter model and the party loyalist model. The swing voter model is by far the most common sort of model employed in political economy today. In the American states, a setting

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ideally suited to the assumptions of this theoretical framework, we find little or no empirical support for the notion that parties target areas with high numbers of swing voters. In this respect, our findings contradict recent theoretical analyses by Lindbeck and Weibull, Dixit and Londregan, and others, which predict that pivotal areas will get more. At least in the American states, they do not. Our analysis further identifies a potential flaw in this line of theory. The Lindbeck–Weibull and Dixit–Londregan models assume that government spending does *not* affect turnout. The results in Table 3 provide substantial evidence that turnout is indeed tied to government spending.

Party loyalist models are less commonly used to explain electoral outcomes and the political economy of government policy. We show very clear evidence for such models. We have documented that state transfers to local governments in the U.S. show a distinctly partisan bias. From 1957 to 1997, areas where the majority party within states have higher levels of electoral support received, on average, larger shares of state transfers.

We have further found at least one electoral mechanism through which public finances affect politics—turnout. The logic behind such an effect is simple enough. Suppose government spending and the credit-claiming it engenders produce more voters but change few minds. How should parties best appeal to the electorate? Spending resources where there are a high fraction of supporters only mobilizes supporters. Spending funds in areas with large numbers of uncommitted voters or areas that are evenly divided might not help a legislator or party's electoral prospects. Such a strategy would mobilize some supporters, but also some opponents. Spending resources in an area where there are many opposition voters would clearly be counter-productive. The best strategy, then, is to devote disproportionately more resources to areas where there are high concentrations of a party's supporters. Kramer (1966), Cox and McCubbins (1986) and Sim (2002) offer simple decision-theoretic and game-theoretic models that capture this logic.

The link between government spending and turnout also allows us to draw clear comparisons of the American states with other electoral circumstances. As discussed in the introduction, the loyal voter and swing voter models have been examined in at least three other contexts: U.S. federal government expenditures, as in Levitt and Snyder (1995), Indian national government expenditures, as in Dasgupta *et al.* (2001), and environmental grants in Sweden, as in Dahlberg and Johansson (2002). In Sweden, there is some evidence that resources go disproportionately to pivotal areas with high fractions of undecided or independent voters. Like the American states, intergovernmental transfers and expenditures by the U.S. federal government provide evidence that parties shift resources to areas with more loyal voters. Expenditures by the Indian national government exhibit a mixed pattern.

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Variation across electoral contexts may be consistent with the importance of turnout in distributive politics. In Sweden, turnout is very high. The marginal returns to mobilizing new voters may be slight, so politicians and parties must, instead, convert people who might back one party to switch allegiances. Marginal increases in expenditures, then, are likely to have only slight effects on future turnout. In the U.S. and India, turnout is comparatively low. In this context, marginal increases in turnout may be easily made through higher government spending.

Whatever the precise motivation, it is clear from our analysis that in the American states the distribution of state transfers to local governments, accounting for 40% of local government expenditures, shows evidence of a substantial partisan bias and clear evidence against the swing voter model.

Appendix

Table A1. *Variable definitions and sources*

Relative Share of Intergovernmental Transfers from State Government: U.S. Census Bureau, *Census of Governments* (1957, 1962, 1967, 1972, 1977, 1982, 1987, 1992, 1997).

Voting data for governor, senator and president used to make Average Democratic Vote, Turnout, Closeness to 50–50, and Std. Dev. of Democratic Vote: ICPSR (1995, ICPSR Study No. 00013); *America Votes* (1992, 1994, 1996, 1998, 2000).

Legislative seats held by each party, and party of governor, used to make Democratic Control, Republican Control and Divided Control: Burnham (198?, ICPSR Study No. 00016); Council of State Governments, *Book of the States* (various years).

Matching counties and state legislative districts required considerable effort and a large number of sources. ICPSR Study No. 8907 provides complete coverage for the period 1968–1989. For the period 1950–1967 the matching had to be done state by state. The sources for each state are mainly those listed in Ansolabehere and Snyder (2002).

Relative Representation Index: David and Eisenberg (1961).

Population, Per Capita Income, Percent in Poverty, Children in School Per Capita, Percent Age 65 and Over, and Percent Black: U.S. Census Bureau, *County and City Data Book* (1952, 1957, 1962, 1967, 1972, 1977, 1983, 1988, 1994); U.S. Census Bureau, *U.S.A. Counties*; U.S. Census Bureau, *Census 2000*.

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