DEVIATIONS FROM CONSTITUENT INTERESTS: THE ROLE OF LEGISLATIVE STRUCTURE AND POLITICAL PARTIES IN THE STATES

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This paper investigates the determinants of state spending over 1960—1990. Recent empirical studies suggest that state government expenditure is greater than the electorate desires. Our main finding is that expenditure was positively related to the number of seats in a state's legislature. This is consistent with the hypothesis that logrolling leads representatives to spend more than their constituents would like. We also find that political parties do not have a pronounced effect on overall levels of expenditure, but do influence the composition of spending. In particular, Democratic control of state government is associated with higher levels of welfare spending.

I. INTRODUCTION

Government policies often deviate from constituent interests. In part, this can be attributed to information asymmetries between elected representatives and their constituents. One asymmetry arises because many voters rationally choose to remain ignorant about politics (Downs [1957]). Voter ignorance allows representatives to pursue their personal objectives or "ideologies" to some extent without fear of reprisal on election day. If representative and constituent objectives differ, this agency problem may lead to outcomes that are at odds with constituent interests (Kalt and Zupan [1984; 1990]). Perhaps equally important is the fact that representatives have incomplete information about the preferences of their constituents (Matsusaka [1992]). As a consequence, even a well-intentioned legislature may inadvertently implement policies that the electorate does not like. Thus, there are good reasons to expect occasional policy deviations from constituent interests.

What is more difficult to understand is why legislative policies appear to deviate *systematically* from constituent interests. Recent studies by Peltzman [1992] and Matsusaka [1995] covering the last thirty years suggest that the size of government in the United States exceeds that preferred by the electorate. In addition, both studies find some evidence that the electorate is particularly dissatisfied with the amount of redistributional spending. Peltzman's study documents a systematic negative relation between an incumbent's (or, when not standing for re-election, the incumbent's party's) re-election votes and the growth rate of government expenditure. The relation holds for presidential, gubernatorial, and senatorial elections. Growth in welfare spending was especially poisonous at the polls for governors. Matsusaka finds that government spending is significantly lower in states that allow citizens to directly initiate and pass laws

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than in states where laws can only be passed by elected representatives. On the assumption that outcomes in initiative states come closer to constituent interests, one interpretation of the finding is that pure representative states have higher spending levels than the electorate wants. The success of state tax and spending limitation initiatives in the late 1970s and 1980s further suggests that the electorate considers government spending to be excessive.

Asymmetric information between representatives and constituents can explain why policies often deviate from constituent interests, but does not appear to offer an explanation for *systematic* deviations. That is, while it can explain why government is sometimes larger and sometimes smaller than the electorate would like, it cannot rationalize consistently higher spending. The purpose of this paper is to provide evidence on the cause of systematic deviations from constituent interests. We focus on the size of the legislature and the political party that controls it. The theoretical links between spending and these two factors are summarized next.

Size of the Legislature

The main theoretical reason to expect higher spending by legislatures with more seats is logrolling. The gains-from-trade literature associated with Weingast and Marshall [1988] argues that legislatures are organized to facilitate vote trading between representatives. Logrolling in itself does not imply excessive spending—in principle, members can trade tax breaks for their constituents as easily as pork barrel projects. However, while a given spending program typically benefits a particular subset of the population, its cost is usually spread over the entire population because the tax structure of most governments is broad based, especially when it relies on income and sales taxes. Thus, representatives are likely to view the tax base as a "common pool" from which to finance particularistic projects for their constituents, leading to the familiar problem of overexploitation. Dalenberg and Duffy-Deno [1991] argue that this problem could account for their finding that cities with ward election systems spend more than cities with at-large election systems. For additional evidence on this issue, see Del Rossi and Inman [1994] and Inman [1993].

Weingast, Shepsle, and Johnsen [1981] develop a formal model that provides a key testable implication. To briefly summarize their model, let $b_i(x)$ be the benefit of spending x dollars in district i to the constituents of legislator i, and let $c(x)$ be the cost of the spending. The efficient level of spending in the district is the x that solves $b_i'(x) = c'(x)$. If there are n districts and taxes are spread evenly over the districts, then the constituents of legislator i bear only $(1/n)$th of the cost of spending in district i. As a result, the legislator favors spending for his district up to the point where $b_i' = (1/n)c'$. This implies that the optimal level of spending for each legislator is increasing in n. If legislators logroll and defer to each other regarding such expenditure, then total spending increases with n. This implication, called "the Law of $1/n$" by Weingast, Shepsle, and Johnsen, is what we test.

Political Parties

In the two-party American system, if neither the Democratic Party nor the Republican Party adopts the policy position of the median voter then policy outcomes deviate from median voter outcomes. In particular, if the Democrats propose to spend more and the Republicans propose to spend less than what the median voter prefers, but the Democrats are closer to his ideal point than the Republicans (or enjoy some unspecified electoral advantage, for example, favorable gerrymandering), then the Democrats will win office and spending will be high. Thus, policy outcomes may depend on which party is in office.

Party effects of this sort are somewhat problematic in light of Down's [1957] theoretical proposition that vote-maximizing parties tend to converge to the same policy position, the median voter's ideal point. Non-convergence can be justified in the standard theory by noting that parties do not move to the same position when the issue space is multidimensional. Furthermore, Ingberman and Villani [1993] show that convergence does not occur even when there is a unidimensional issue space if parties are risk averse. In short, although there is theoretical reason to doubt that parties matter, sufficient grounds exist to entertain the hypothesis that political parties play a role in the systematic bias of state spending.

Our empirical method is to estimate regressions for state and local expenditure using panel data for 1960–1990. We control for constituent interests by including demographic variables that are known to be important determinants of spending. Then we add variables indicating the size of legislatures and the strength of political parties in a state. Having controlled for constituent interests, if these variables are significant then we can conclude that they are partly responsible for driving spending outcomes away from constituent interests.

The main finding is that states with larger legislatures spend more. The effect is strongest for the upper chamber, where each seat is associated at the margin with roughly $10 expenditure per capita. Second, we find only weak evidence that political parties influence the level of spending. We also fail to find convincing evidence that "gridlock"—defined as a situation where branches of the government are controlled by different parties—has an effect, one way or the other, on the level of expenditure. However, our estimates suggest that political parties play a role in determining the *composition* of spending. In particular, Democratic control of both the executive and legislative branches leads to significantly higher welfare expenditure than Republican control, and significant lower highway expenditure. Finally, the size of the legislature appears to have a positive effect on all three categories of spending we consider, welfare, education, and highways, consistent with a vote-trading explanation.

These results tie into two broader literatures, one that asks, "Do institutions matter?" and another that asks, "Do political parties matter?" Both literatures have tended to find mixed answers (for institutions, see Abrams and Dougan [1986]; for parties, see Blais, Blake, and Dion [1993]). Recent institutional studies of state government fiscal outcomes have examined the voter initiative (Matsusaka [1995]), tax and spending limits (Abrams and Dougan [1986]), debt and borrowing limits (Kiewiet and Szakaly [1993]; Poterba [1994]), budgetary rules (Poterba [1995]), and non-proportional representation of states in the U.S. Senate (Atlas, Gilligan, Hendershott, and Zupan [1993]), among others. Only two recent studies, Dye [1984] and Garand [1988], have used state data to look for party effects. Both find weak effects in state-by-state time-series regressions, but neither study exploits the cross-sectional nature of the data. Our paper adds to the institutions literature by identifying another feature of the public decision-making process that affects spending policy. Its contribution to the party literature is more nuanced; our results suggest that parties do matter, not in terms of total spending as is sometimes believed, but in terms of the composition of spending.

Section II describes our research methodology and data. The empirical results are presented in section III. Section IV concludes.

II. EMPIRICAL METHODOLOGY

The paper analyzes the determinants of state expenditure. We use seven cross sec-

tions at five-year intervals beginning in 1960, that is, 1960, 1965, 1970, 1975, 1980, 1985, and 1990. Each cross section contains forty-eight states. Following conventional practice, Alaska is omitted from all estimates as an outlier. Nebraska is also excluded because it has a unicameral legislature.

Basic Model

The basic model assumed to govern behavior is

$$(1) \qquad G_{it} = \alpha X_{it} + \beta S_{it} + \gamma P_{it} + \varepsilon_{it},$$

where i and t index states and years respectively, G_{it} is government expenditure per capita, X_{it} is a vector of demographic variables assumed to be correlated with constituent interests, S_{it} is a two-element vector containing the number of seats in the state legislature's upper and lower chambers, P_{it} is a vector of party variables, ε_{it} is an error term, and α, β, and γ are unknown parameters. All financial variables are expressed in per capita terms.

Because we study panel data, it is appropriate to allow for both state and year fixed effects. This is implemented in the usual way by assuming that the error term in (1) can be written

$$\varepsilon_{it} = u_i + v_t + w_{it},$$

where u_i is a state-specific fixed effect, v_t is a year-specific fixed effect, and w_{it} is a mean zero error. Define G^*_{it} by subtracting the state and year means and adding the full sample mean:

$$(2) \qquad G^*_{it} \equiv G_{it} - \frac{1}{48}\sum_{j=1}^{48} G_{jt} - \frac{1}{7}\sum_{k=1}^{7} G_{ik} + \frac{1}{336}\sum_{j=1}^{48}\sum_{k=1}^{7} G_{jk}.$$

Define X^*_{it}, S^*_{it}, and P^*_{it} analogously. Then we correct for fixed effects by using ordinary least squares on the following model:[1]

$$(3) \qquad G^*_{it} = \alpha X^*_{it} + \beta S^*_{it} + \gamma P^*_{it} + w_{it}.$$

Finally, to correct for possible violations of heteroskedasticity in w_{it}, we use White standard errors for statistical tests on (3).

Controls

Five controls are included in X_{it} to capture constituent interests: (i) state income per capita, (ii) population, (iii) the growth rate of population over the preceding four years, (iv) the percentage of the population living in a metropolitan area, and (v) a measure of the average ideology of the state's U.S. senators. These variables are commonly used to represent constituent interests. Income is a well-known correlant of spending. The main theoretical reason to include income in the regressions is that the demand for a number of government services is believed to be related to income. Population is used as a control for three reasons. First, a large population increases the marginal benefit of spending if population density creates unique public good problems. Second, large populations may present opportunities for economies of scale in the production of government services. Third, the logrolling theory that we test relies on the idea that representatives can target spending to specific subsets of the population. Holding constant the number of districts, this should be more difficult with a small population than a large population. Thus, to test "the Law of $1/n$" it is appropriate to control for the size of the state's population. A

1. The cost of removing fixed effects relative to estimating equation (1) directly is fifty-five degrees of freedom.

high level of population growth is expected to lead to a short-run demand for certain services that are typically provided by the government, particularly capital goods like highways, streets, sewers, and school buildings. Metropolitan electorates can be expected to have different demands for government services than rural electorates.

It turns out that these four demographic variables do a good job explaining the variation in state expenditure. Still, it is unlikely that they completely capture constituent interests. To try to capture residual constituent interests, we also include a variable that represents the ideology of the state's U.S. senators. Our measure of ideology is based on the D-NOMINATE estimates of Poole and Rosenthal [1991]. D-NOMINATE is an estimation procedure that provides a spatial location for each U.S. senator based on the senator's roll call votes. Poole and Rosenthal report that roll call voting behavior can be characterized for the most part by a one-dimensional model. Accordingly, we take the estimated first-dimension position of each senator and average it across a state's senators.[2] With some caveats, this spatial position can be thought of as indicating where a senator lies on the liberal-conservative spectrum. Theoretically, the positions range from –1 for the most liberal to +1 for the most conservative.[3]

We also include two controls to capture the cost of spending, mineral production per capita and federal revenue transfers per capita. Mineral production is included because the presence of large mineral deposits allows a state to finance expenditure through severance taxes which, given that such taxes can be borne by non-residents and are less distortionary than income or sales taxes, can lower the marginal cost of state spending. Per capita federal revenue transfers are included to account for the effects of matching programs and possible wealth effects from federal aid. All financial variables are expressed in 1990 dollars, using the Consumer Price Index.

Legislative Structure and Political Parties

As noted above, the S_{it} vector contains the number of seats in the upper and lower chamber. Table I reports the number of seats in each chamber in 1990. Several interesting characteristics of the states' legislative structures can be seen. First, there is a substantial variation in the number of seats across states. The number of lower-house seats range from a low of forty in Alaska to a high of four hundred in the relatively small state of New Hampshire.[4] Upper-house seats range from twenty in Alaska to sixty-seven in Minnesota. Interestingly, Alaska notwithstanding, there is little relation between the size of a given state's upper and lower houses. While it might be expected that states with large upper houses also have large lower houses, a regression of the number of upper house seats on the number of lower house seats and a constant has an R^2 of only 0.012. Nor is there much of a relation between the population of a state and the number of seats. It might be expected that populous states have more seats, but a regression of seats on population and a constant gives an R^2 of only 0.036 for the upper house and 0.005 for the lower house. Although it cannot be seen from the table, there is also a fair amount of time-series variation in the number of seats. Over the 1960–1990 period, thirty-

2. This averaging technique is somewhat inadequate in light of evidence that different senators may serve different constituencies within a state. For a review of this evidence see Jung, Kenny, and Lott [1994].

3. We matched senators to fiscal years as follows. The 86th Congress was used for 1960, the 88th for 1965, the 91st for 1970, the 93rd for 1975, the 96th for 1980, the 98th for 1985, and the 100th (the most recent available at the time of writing) for 1990.

4. We also estimated all regressions without New Hampshire to allow for the possibility that the state is an outlier. The results were not different in any important way.

TABLE I
Number of Seats in Upper and Lower Chambers, 1990

State	Seats		State	Seats	
	Upper	Lower		Upper	Lower
Alabama	35	105	Montana	50	100
Alaska	20	40	Nebraska	49	—
Arizona	30	60	Nevada	21	42
Arkansas	35	100	New Hampshire	24	400
California	40	80	New Jersey	40	80
Colorado	35	65	New Mexico	42	70
Connecticut	36	151	New York	61	150
Delaware	21	41	North Carolina	50	120
Florida	40	120	North Dakota	53	106
Georgia	56	180	Ohio	33	99
Hawaii	25	51	Oklahoma	48	101
Idaho	42	84	Oregon	30	60
Illinois	59	118	Pennsylvania	50	203
Indiana	50	100	Rhode Island	50	100
Iowa	50	100	South Carolina	46	124
Kansas	40	125	South Dakota	35	70
Kentucky	38	100	Tennessee	33	99
Louisiana	39	105	Texas	31	150
Maine	35	151	Utah	29	75
Maryland	47	141	Vermont	30	150
Massachusetts	40	160	Virginia	40	100
Michigan	38	110	Washington	49	98
Minnesota	67	134	West Virginia	34	100
Mississippi	52	122	Wisconsin	33	99
Missouri	34	163	Wyoming	30	64

Note: These data are taken from the 1993 edition of the *World Almanac*. Nebraska has a unicameral legislature, called the senate, with 49 seats.

six states changed the number of seats in one of their chambers at least once, and nineteen states changed at least twice. Among the reasons for changes in chamber size were constitutional links between seats and population, and seats and counties. Without this time-series variation, the state fixed effects would make it difficult to estimate β.

Several different party variables are used in P_{it}. They are discussed in greater detail below. The primary variables indicate which party controlled the governor's office, the upper house, and the lower house. "Democrats" and "Republicans" are representatives so classified by our data sources.[5] Independents, Libertarians, and members of other parties are not classified as Democrats or Republicans.

Data Sources

Fiscal data, income, population, population growth, and federal revenue transfers were collected from various issues of *Governmental Finances* and *State Government Finances*, both publications of the U.S. Department of Commerce, Bureau of the Census (U.S. Government Printing Office, Washington, D.C.). Metropolitan population came from the *Statistical Abstract of the United States*, 1969, 1977, 1987, 1992. Mineral data for the years 1960–1975 were taken from *Minerals Yearbook*, a publication of the U.S. Department of Interior, Bureau of Mines (U.S. Government Printing Office, Washington, D.C.). Thereafter the data were computed from the *Statistical Abstract of the United States*. See Matsusaka [1995] for details.

The number of seats and political party data were drawn from various issues of *The Book of the States*, a publication of the Council of State Governments (Lexington, Kentucky). In order to match expenditure outcomes to the government that produced them, seat and party variables were selected to correspond to the government in place on January 30 of the year before the year of the fiscal data. So, for example, seat and party data for January 1989 were matched to the 1990 expenditure data. This is appropriate because fiscal data for the 1990 fiscal year cover state expenditure from July 1, 1989 to June 30, 1990.[6] In most cases, budgets are approved prior to the start of the fiscal year. Thus, the 1990 fiscal year budgets were produced by the representatives in office in the months prior to July 1989. We approximate this by using the officeholders as of January 30, 1989. Summary statistics for the explanatory variables are reported in Table II.

III. REGRESSIONS

Regressions provide three types of evidence that suggest whether legislative structure and political parties can explain deviations from constituency interests. First, the relevant coefficient estimates can be tested to see if they are significantly different from zero. Second, the magnitudes of the coefficients can be examined to determine if they are quantitatively significant. Third, regression R^2's can be compared to estimate how much power the variables have to organize the data. The regressions are presented so as to facilitate these inferences.

Table III reports the first set of regressions, which are designed to investigate the determinants of total spending. Each column in the table is a regression. The primary entries are coefficient estimates of equation (3). Beneath each estimate in parentheses is its (White) standard error. The dependent variable is combined state and local direct general expenditure. Over 80 percent of state and local spending is

5. For Minnesota, members of the Democratic-Farmer-Labor Party are classified as Democrats and members of the Independent-Republican Party are classified as Republicans. Until 1972, Minnesota elections were nonpartisan, so Minnesota observations are generally dropped from 1960–1970.

6. Four states begin their fiscal years a few months earlier or later: Alabama, New York, Pennsylvania, and Texas.

TABLE II
Summary Statistics

Variable	Mean	S.D.	Minimum	Maximum
Expenditure				
Total	2,301	765	809	5,061
Non-capital	1,857	703	568	4,309
Capital-only	445	156	212	1,314
Welfare	239	136	38	889
Education	867	271	309	1,912
Highway	294	109	141	934
Controls				
Income per capita	12,436	3,125	5,179	24,319
Population in millions	4.43	4.64	0.29	29.76
Population growth rate	5.30	6.08	−10.45	47.16
Mineral production per capita	959	1,951	5	17,286
Metropolitan population	59.4	24.9	0	100
D-NOMINATE average for U.S. senators	−0.061	0.260	−0.598	0.602
Federal aid per capita	458	191	84	1,175
Legislative structure				
Number of seats in upper chamber	39.3	10.3	17	67
Number of seats in lower chamber	116.3	60.0	35	400
Political parties				
Dummy=1 if governor was a Democrat	0.613	0.488	0	1
Dummy=1 if upper house was controlled by Democrats	0.640	0.481	0	1
Dummy=1 if lower house was controlled by Democrats	0.688	0.464	0	1
Democrats in upper house, percent of seats	61.6	22.3	12.2	100.0
Democrats in lower house, percent of seats	61.8	21.1	17.9	100.0
Dummy=1 if governor and legislature were Democratic	0.411	0.493	0	1
Dummy=1 if governor and legislature were Republican	0.150	0.358	0	1
Competition in upper house	18.68	7.41	0.00	25.00
Competition in lower house	19.18	7.22	0.00	25.00

Note: All observations for Alaska and Nebraska, and the 1960, 1965, and 1970 Minnesota observations are omitted, leaving a total of 333 observations.

classified as direct general expenditure. The biggest omitted expenditure categories are utility, liquor store, and trust fund expenditure. Intergovernment transfers are also excluded, although state and local transfers to the federal government are tiny.

Even though the study investigates state legislatures, we choose to focus on combined state and local expenditure. We believe it is plausible to view combined state and local expenditure as the policy outcome from state legislatures because state governments have the ability to exert decisive influence on local government expenditure. Local government expenditure can be forced up by unfunded mandates. It can be forced down by restrictions on tax collections or by raids on local tax bases.

Column (1) presents a regression including only constituent interest and federal aid variables. Following Peltzman [1984], this regression serves as a "baseline" to compare with the rest of the regressions. The fit of the regression is good ($R^2 = 0.512$), especially considering that fixed effects have been removed. We can be fairly confident that our set of controls captures some important constituent interests. Income, mineral production, and federal aid are positively related to expenditure and significant at better than the 1 percent level. The income coefficient indicates that each one-dollar increase in income translates into 9.3 cents of additional government spending. Each dollar of mineral production is estimated to lead to 7.8 cents of additional spending. A dollar transferred from the federal government increases state and local spending by more than a dollar, \$1.85.[7] Population is also positively correlated with spending, and the coefficient is significant at the 10 percent level. The point estimate indicates that a one-million-person increase in population is associated with \$11.99 additional expenditure per capita.[8] Per capita spending is negatively related to metropolitan population. The coefficient is significant at the 10 percent level and indicates that a 1 percent increase in the fraction of a state's population living in a metropolitan area is associated with \$3.66 lower expenditure per capita.

Column (2) presents a regression in which legislative structure and party variables are added to the set of controls. For legislative structure, we include two variables, the number of seats in the upper chamber and the number of seats in the lower chamber. Three political party variables are introduced, a dummy equal to one if the governor was a Democrat, a dummy equal to one if there were more Democrats than Republicans in the upper chamber, and a dummy equal to one if Democrats outnumbered Republicans in the lower chamber.[9]

One important thing to note is that both seat coefficients are positive and the upper-house coefficient is significant at better than the 1 percent level. The point estimate suggests that each additional seat translates into an additional \$9.87 expenditure per capita. The magnitude of this effect is fairly robust across specifications in Table III, ranging from \$9.87 to \$10.91. Quantitatively this is not a huge effect, but it does not seem trivial. These institutional features affect spending after controlling for important constituent interests. Thus, there is some support for the theoretical

7. This oft-observed relationship is referred to as the "flypaper effect." See Rubinfeld's [1987] survey for other evidence and explanations.

8. It is possible that population is a proxy for the cost of public services, in which case a linear specification may be inappropriate. Accordingly, we ran the regressions allowing for second-order population effects and using the logarithm of population. The fit of such regressions as measured by R^2 was sometimes better than the regressions we report, but always within a few percentage points, and the corresponding seats and party coefficients were consistent with our main conclusions.

9. In the ten cases where the numbers were equal, control was assigned to the party of the governor.

TABLE III
Regressions of State and Local Direct General Expenditure on Constituent Interests, Number of Seats, and Political Party

Variable	(1)	(2)	(3)	(4)	(5)
Income per capita	0.093**	0.090**	0.092**	0.091**	0.092**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Population in millions	11.99^{+}	14.36*	14.19*	13.34^{+}	15.53*
	(6.89)	(7.08)	(7.17)	(7.09)	(7.52)
Population growth rate	−0.86	0.82	0.14	0.29	0.09
	(1.96)	(2.03)	(2.06)	(2.02)	(1.97)
Metropolitan population, percent of total	−3.66^{+}	−3.92*	−3.80^{+}	−3.91*	−3.04
	(1.94)	(1.97)	(2.01)	(1.97)	(2.21)
D-NOMINATE average for U.S. senators	14.30	19.59	26.01	22.13	20.70
	(42.75)	(42.42)	(43.73)	(42.74)	(42.68)
Mineral production per capita	0.078**	0.074**	0.074**	0.075**	0.074**
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Federal aid per capita	1.85**	1.87**	1.82**	1.82**	1.83**
	(0.17)	(0.18)	(0.18)	(0.17)	(0.17)
Number of seats in upper chamber	—	9.87**	10.91**	10.60**	10.20**
		(2.79)	(2.75)	(2.78)	(2.92)
Number of seats in lower chamber	—	0.33	0.50	0.39	0.43
		(0.52)	(0.54)	(0.50)	(0.50)
Dummy=1 if governor was a Democrat	—	16.58	13.32	—	−7.90
		(16.48)	(16.61)		(15.31)
Dummy=1 if upper house was controlled by Democrats	—	35.37	—	—	—
		(24.93)			
Dummy=1 if lower house was controlled by Democrats	—	−52.16*	—	—	—
		(25.34)			
Democrats in upper house, percent of seats	—	—	−0.27	—	—
			(1.10)		
Democrats in lower house, percent of seats	—	—	0.61	—	—
			(1.20)		
Dummy=1 if governor and legislature were Democratic	—	—	—	4.05	—
				(20.58)	
Dummy=1 if governor and legislature were Republican	—	—	—	−19.96	—
				(26.13)	
Competition in upper house	—	—	—	—	−2.05
					(2.88)
Competition in lower house	—	—	—	—	−0.64
					(2.83)
R^2	0.512	0.533	0.528	0.527	0.529
$\bar{R}^2$	0.499	0.512	0.506	0.508	0.507

Note: Each column is a regression; (White) standard errors are reported in parentheses beneath coefficient estimates. The dependent variable in all regressions is state and local direct general expenditure. All observations for Alaska and Nebraska, and the 1960, 1965, and 1970 observations for Minnesota are omitted from all regressions, leaving a total of 333. Statistical significance is indicated as follows: "$^{+}$" is significant at the 10 percent level, "*" is significant at 5 percent, and "**" is significant at 1 percent.

proposition that common pool problems in the tax base lead legislatures to spend more than constituents prefer.

The evidence from the party variables is more difficult to interpret. The governor and upper-house coefficients are positive but the lower-house coefficient is negative. The first two coefficients are measured too imprecisely to achieve statistical significance while the last is different from zero at the 5 percent level. Taken at face value the parameter estimates indicate large economic effects from party control, but they act in inconsistent directions. For example, expenditure is estimated to have been $52.16 per capita lower when Democrats controlled the lower chamber than when it was controlled by Republicans, but $35.37 higher when Democrats controlled the upper chamber. We have no theoretical reason to expect party effects to go in opposite directions. A cautious interpretation of this regression is that it does not allow a reliable conclusion one way or the other about party effects.

The regression in column (3) uses a different specification of the party effects in an effort to achieve a more precise estimate of the party coefficients. The upper-house and lower-house dummies are replaced with continuous variables equal to the number of Democrats divided by the number of Democrats plus Republicans (as a percentage). This specification is not an improvement. The coefficient estimates remain noisy and the $\bar{R}^2$ falls.

The regression in column (4) attempts to detect a party effect by comparing two polar situations, complete Democratic control of the government and complete Republican control. Such a regression might provide a better estimate for two reasons: one, by looking for party effects at the extremes, and two, by evading possible multicollinearity problems between the party variables in regressions (2) and (3). The first party variable is a dummy equal to one if Democrats controlled the governor's office and were not outnumbered by Republicans in either legislative chamber. The second dummy is equal to one if the governor was a Republican and Republicans were not outnumbered by Democrats in either house. These coefficients indicate the effect that complete control of the government by a single party had relative to divided government; the effect of Democratic control relative to Republican control is calculated as the difference between the coefficients on the two dummies.

The estimates indicate that complete Democratic control leads to an additional $4.05 expenditure per capita relative to divided government; Republican control leads to $19.96 expenditure per capita less than divided government. However, neither coefficient comes close to achieving statistical significance at conventional levels. The difference between complete Democratic control and complete Republican control is estimated to be $24.01, but this is also insignificant ($p = 0.440$).

At least since Key [1958], it has been argued that outcomes depend less on which party holds power than on the degree of competition between the two parties. Key argues that party competition is healthy and likely to lead to more representative public decisions. Party competition is also emphasized in current discussions of "gridlock," a condition said to prevail when neither party controls the government. Gridlock is alleged to impede the public decision-making process and lead to unrepresentative outcomes because the lines of responsibility for policy are not clear. Neither the pro-competition "Key" viewpoint nor the anti-competition "gridlock" viewpoint rest on as firm theoretical foundations as could be hoped, but they share a fairly unambiguous (and testable) prediction that government outcomes depend on party competition.

One way to evaluate this hypothesis is suggested by the regression in column (4). The two party dummies represent non-gridlock states of the world. If divided

government affects expenditure behavior, then the dummies jointly should add explanatory power to the regression. This does not seem to be the case. The hypothesis that both coefficients are equal to zero cannot be rejected ($p = 0.715$).

Column (5) reports a regression specified to test the competition hypothesis in a different way. Three party variables are included. One is the usual dummy variable for the party of the governor. The other two variables correspond to the amount of competition in the upper and lower chamber, respectively. If D is defined to be the number of Democrats in a chamber as a fraction of total Democrats and Republicans, then competition is defined as $D(1–D) \times 100$. Competition attains a maximum when the parties are evenly divided in representation, $D = 0.5$. It is at a minimum when either party controls all the seats, $D = 0$ or $D = 1$.

The point estimates of both competition coefficients imply that competition leads to lower spending levels. However, neither coefficient approaches statistical significance. The hypothesis that both coefficients are equal to zero cannot be rejected at conventional levels ($p = 0.510$). The idea that competition matters is not entirely without support, but the regressions do not give strong testimony to its importance.

In terms of overall explanatory power, the seat and party variables increase R^2 by 0.021, comparing columns (1) and (2). The R^2 increment is 0.013. Both legislative structure and political parties, then, appear to offer only a modest improvement in explaining observed expenditure behavior after controlling for constituent interests. This does not mean that party-only or structure-only regressions cannot fit the data. It only means that we do not need to introduce the idea of structure or parties to explain most of the observed variation. This also does not mean that legislative structure and political parties cannot have large effects on government expenditure. Indeed, the sizeable parameter estimates on upper house seats in columns (2)–(5) belie this. Rather, it implies they are not the primary factors driving expenditure outcome variability in the sample.[10]

Weak party effects are consistent with Peltzman's [1984] conclusion that constituent interests are the fundamental determinants of government outcomes. His study focused on the role of ideology in determining how representatives voted, and he concluded that ideology is largely a "sideshow." The regressions in columns (1)–(5) suggest that political parties may be sideshows as well, at least when it comes to the level of total expenditure.

Given the imprecision of the party coefficients, however, this conclusion may be premature. The next set of regressions in Table IV attempt to uncover more precise estimates of these parameters.[11] The dependent variable in regressions (1)–(3) is *noncapital*, or current, direct general expenditure. That is, capital outlays are stripped from total expenditure.

There are two reasons to hope for more precise estimates after removing capital. First, many capital projects are large enough to require debt financing. As of 1990, twenty-five states required direct voter approval of debt issues, which can have large effects on expenditure behavior. In addition, twenty-seven states had constitutional limits on the amount of debt that could be issued (Kiewiet and Szakaly [1992]). Second, capital spending tends to be lumpy. This creates bubbles in the data

10. This conclusion needs some qualification. The fixed effects may capture institutional or party effects that are not time varying or do not vary cross sectionally. To the extent this happens, the explanatory power of seats and party is underestimated by equation (3).

11. Another possible explanation for the lack of party effects is that party labels have different meanings in different states. In particular, it is commonly believed that Democrats in the South have different policy objectives than Democrats in other parts of the country. To allow for this, we re-estimated all our regressions after deleting Southern states from the sample. This did not result in a substantive improvement in the party coefficients.

TABLE IV

Regressions of Non-Capital and Capital Expenditure on Constituent Interests, Number of Seats, and Political Party

	Non-capital			Capital		
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Income per capita	0.067**	0.063**	0.065**	0.026**	0.027**	0.027**
	(0.011)	(0.011)	(0.011)	(0.006)	(0.007)	(0.006)
Population in millions	2.13	4.49	4.94	9.86	9.87	8.40
	(5.86)	(6.07)	(6.15)	(6.14)	(6.08)	(6.12)
Population growth rate	–8.23**	–6.82**	–7.36**	7.37**	7.64**	7.65**
	(2.09)	(2.17)	(2.17)	(1.32)	(1.36)	(1.34)
Metropolitan population, percent of total	–3.59*	–3.76*	–3.53^{+}	–0.07	–0.16	–0.37
	(1.75)	(1.79)	(1.81)	(1.14)	(1.17)	(1.17)
D-NOMINATE average for U.S. senators	–19.63	–10.01	–10.09	33.93	29.60	32.22
	(40.45)	(40.10)	(40.98)	(24.88)	(25.38)	(25.38)
Mineral production per capita	0.066**	0.064**	0.063**	0.012	0.011	0.012
	(0.018)	(0.018)	(0.018)	(0.011)	(0.010)	(0.011)
Federal aid per capita	1.66**	1.65**	1.64**	0.19*	0.23**	0.19*
	(0.15)	(0.15)	(0.15)	(0.09)	(0.09)	(0.09)
Number of seats in upper chamber	—	6.74**	7.45**	—	3.13^{+}	3.15^{+}
		(2.41)	(2.48)		(1.71)	(1.79)
Number of seats in lower chamber	—	–0.06	0.14	—	0.27	0.25
		(0.52)	(0.50)		(0.21)	(0.22)
Dummy=1 if governor was a Democrat	—	12.88	—	—	3.70	—
		(15.35)			(10.39)	
Dummy=1 if upper house was controlled by Democrats	—	50.32*	—	—	–14.95	—
		(22.06)			(13.44)	
Dummy=1 if lower house was controlled by Democrats	—	–28.61	—	—	–23.55	—
		(26.02)			(15.06)	
Dummy=1 if governor and legislature were Democratic	—	—	22.85	—	—	–18.80^{+}
			(19.92)			(10.84)
Dummy=1 if governor and legislature were Republican	—	—	–18.28	—	—	–1.69
			(23.25)			(16.09)
R^2	0.505	0.521	0.517	0.210	0.226	0.222
$\bar{R}^2$	0.492	0.499	0.497	0.189	0.191	0.190

Note: Each column is a regression; (White) standard errors are reported in parentheses beneath coefficient estimates. The dependent variable in regressions (1)–(3) is non-capital state and local direct general expenditure; in regressions (4)–(6) it is capital-only direct general expenditure. All observations for Alaska and Nebraska, and the 1960, 1965, and 1970 observations for Minnesota are omitted from all regressions, leaving a total of 333. Statistical significance is indicated as follows: "$^{+}$" is significant at the 10 percent level, "*" is significant at 5 percent, and "**" is significant at 1 percent.

that do not accurately reflect the true flow of services provided by the expenditure. Total direct general expenditure, then, is likely to be afflicted with "measurement error" (Peltzman [1992]).

The explanatory variables in regressions (1), (2), and (3) of Table IV are identical to those in columns (1), (2), and (4) in Table III. The regression in column (1) is the baseline regression for noncapital direct general expenditure. Again the fit is good (R^2 = 0.505), but does not improve on column (1) of Table III. The coefficient estimates are roughly the same as well. The notable exception is the significant negative coefficient on growth. Each 1 percent increase in the population growth rate reduces per capita noncapital direct general expenditure by $8.23. Apparently states shift expenditure into capital spending to accommodate growing populations.

The regression in column (2) includes seat and party variables. The estimates are consistent with column (2) in Table III. A large upper house leads to more spending—the coefficient indicates an additional $6.74 of noncapital direct general expenditure per capita for each upper-chamber seat. The lower-chamber coefficient remains insignificant and quantitatively trivial. The party effects remain mixed, echoing the coefficient in regression (2) of Table III. The main difference is that the coefficient on Democratic control of the upper house is now statistically significant while the coefficient on control of the lower house is insignificant.

Column (3) presents the extreme case regression. The party variables indicate complete control of the government by Democrats or Republicans. The point estimates are sizeable. Democratic control leads to $22.85 more non-capital expenditure per capita while Republican control leads to $18.28 less expenditure per capita relative to divided government. Neither control coefficient achieves statistical significance, however, nor does the difference between the coefficients, $41.70, although the latter comes close (p = 0.153).

The regressions in columns (1)–(3) provide no stronger support for the importance of parties than the regressions in Table III. There is not a complete absence of support, but these variables do not hold out the hope of explaining any sizeable chunk of sample variation. The best-fitting regression (column (2)) increases R^2 by only 0.016 over the baseline demographics-only regression (column (1)). This is better than Table III, but not by much.

Columns (4)–(6) present regressions where capital-only direct general expenditure is the dependent variable. One thing these regressions indicate is whether the regressions in columns (2) and (3) do in fact benefit from reduced measurement error in the dependent variable. If so, then the coefficients in columns (4)–(6) should be estimated less precisely. The regressions also shed light on whether parties have preferences over the composition of spending.

The estimates do not give much support to the measurement error interpretation, but there is a little evidence for party effects. In column (5), none of the party variables are significant. In column (6), when Democrats control both the executive and legislature capital expenditure per capita is $18.80 lower than when control is divided. This effect is significant at the 10 percent level. The Republican control coefficient is negative but trivial and statistically insignificant. The difference between the two dummies fails to achieve statistical significance (p = 0.331). In all, these estimates are not compelling; at most they hint that Democratic officeholders tend to shift expenditure toward current and away from capital programs relative to Republicans.

The coefficients on the number of seats in columns (5) and (6) are positive, and the upper-house coefficients are statistically significant at the 10 percent level. Unlike party, the size of a legislature does not

seem to act in different directions for these categories of spending. This lends cautious support to the logrolling interpretation: if large houses lead to higher spending by making it easier for members to approve expenditure on a quid pro quo basis, then the number of seats should increase spending across a broad array of spending categories. Table IV considers only two categories, of course, so this conclusion is tentative.

The R^2's of the regressions in columns (4)–(6) are less than half as large as in columns (1)–(3). Thus, there is some support for the idea that the capital spending series suffers from greater measurement error. Comparing columns (4) and (5) shows that inclusion of legislative structure and party variables increases R^2 by only 0.016. It remains true for capital expenditure that these variables do not contribute much to explaining observed sample variance.

The evidence to this point suggests that two forces may be at work in the determination of state and local expenditure in addition to constituent interests. First, the number of seats in a state's legislature is positively correlated with overall spending. One explanation for this relation is that common pool problems with the tax base are more severe when there are many legislators than when there are just a few. According to this theory, a large legislature should increase spending more or less across the board as representatives trade votes for their particularistic projects. The finding that the number of seats is positively correlated with both capital and non-capital direct general expenditure is consistent with this hypothesis. Second, political parties have an uncertain effect on total spending, but possibly effects on the composition of expenditure. In particular, Democratic governments may have a tendency to shift spending away from capital projects relative to Republicans.

Table V seeks corroborating evidence on these two interpretations by estimating seat and party effects for functional spending categories. The dependent variable is per capita welfare expenditure in columns (1) and (2), education expenditure in columns (3) and (4), and highway expenditure in columns (5) and (6). These are the three largest categories of spending in the sample period, comprising more than 50 percent of direct general expenditure.[12] We are particularly interested in two things: (i) did more seats lead to more spending in all categories, and (ii) was the effect of party control different across spending categories? Table V is likely to overlap with Table IV to some degree because highways are primarily capital expenditure and welfare is primarily current expenditure, but because the relation is not strict these regressions stand to provide additional information.

Columns (1), (3), and (5) present the usual baseline regressions. They indicate that constituent interests do a good job explaining welfare expenditure (R^2 = 0.485), but are less effective in explaining education and highway expenditure (R^2 = 0.326 and R^2 = 0.174, respectively). Seats and party control variables are introduced in the regressions in columns (2), (4), and (6).

Consider first the seat variables. In all three regressions, the upper-house-seat coefficient is positive and statistically sig-

12. To be precise, welfare expenditure consists of cash assistance paid directly to persons under categorical programs such as Old Age Assistance, Aid to Families with Dependent Children, Aid to the Blind, and Aid to the Disabled, vendor payments made directly to private purveyors for medical care, and spending to operate government welfare institutions. Education expenditure is spending to support elementary, secondary, high school, and other education, including auxiliary services such as school lunches. Highway expenditure includes spending for construction and maintenance of highways, streets, bridges, tunnels, ferries, street lighting, and snow and ice removal.

TABLE V
Regressions of Welfare, Education, and Highway Expenditure on Constituent Interests, Number of Seats, and Political Party

	Welfare		Education		Highway	
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Income per capita	0.014** (0.003)	0.013** (0.003)	0.019** (0.005)	0.019** (0.005)	0.014** (0.004)	0.015** (0.004)
Population in millions	1.03 (2.91)	2.34 (3.15)	−1.33 (4.69)	−1.23 (4.65)	−0.04 (2.20)	−1.59 (2.15)
Population growth rate	−1.10 (0.72)	−0.87 (0.72)	2.03** (0.81)	2.25** (0.85)	2.17* (0.99)	2.27* (0.98)
Metropolitan population, percent of total	−2.93** (0.58)	−2.73** (0.59)	1.60+ (0.86)	1.46 (0.90)	−0.37 (0.66)	−0.70 (0.66)
D-NOMINATE average for U.S. senators	−23.41* (11.94)	−18.52 (11.75)	−18.28 (17.02)	−18.43 (17.19)	7.41 (12.50)	3.53 (12.51)
Mineral production per capita	−0.003 (0.005)	−0.005 (0.005)	0.030** (0.011)	0.030** (0.011)	−0.002 (0.007)	−0.001 (0.007)
Federal aid per capita	0.55** (0.05)	0.53** (0.05)	0.45** (0.06)	0.45** (0.07)	0.22** (0.06)	0.23** (0.06)
Number of seats in upper chamber	—	1.70* (0.81)	—	2.29* (1.16)	—	1.64+ (0.88)
Number of seats in lower chamber	—	−0.10 (0.17)	—	0.29 (0.26)	—	0.35* (0.17)
Dummy=1 if governor and legislature were Democratic	—	10.73 (6.62)	—	2.54 (9.03)	—	−15.08* (6.57)
Dummy=1 if governor and legislature were Republican	—	−20.04** (8.18)	—	1.58 (11.83)	—	7.42 (8.24)
R^2	0.485	0.503	0.326	0.334	0.174	0.206
$\bar{R}^2$	0.471	0.482	0.308	0.307	0.153	0.173

Note: Each column is a regression; (White) standard errors are reported in parentheses beneath coefficient estimates. The dependent variable in each regression is indicated at the head of the column. All observations for Alaska and Nebraska, and the 1960, 1965, and 1970 observations for Minnesota are omitted from all regressions, leaving a total of 333. Statistical significance is indicated as follows: "+" is significant at the 10 percent level, "*" is significant at 5 percent, and "**" is significant at 1 percent.

nificant. The lower-house coefficient remains quantitatively smaller than the upper-house coefficient, but attains significance in regression (6). The only negative coefficient is for lower-house seats in column (2), and it does not approach statistical significance. It appears that an increase in the number of seats results in increased expenditure in all three categories.

The party effects are also pronounced. In column (2), the coefficient on complete Democratic control of the government is positive and the Republican coefficient is negative and statistically significant.

Moreover, the magnitudes are large. Welfare spending per capita is $10.73 higher under a government controlled by the Democratic party and $20.04 lower when Republicans control the government—both these effects are relative to divided government. The difference between the coefficients, $30.77, is statistically significant at the 1 percent level ($p = 0.010$). Significant party effects cannot be detected in column (4). In column (6), it can be seen that Democratic control leads to lower levels of highway expenditure. The Democratic coefficient is significantly negative and the difference between the two coefficients is significant ($p = 0.025$). Unlike the number of seats, then, it appears that Democratic control of government causes a substitution between expenditure categories, not a uniform increase across them.

Even though structure and party effects can be seen within categories of spending, it remains the case that they are of marginal importance. The R^2 statistics reinforce this idea. Adding seats and party variables to the basic controls increases the R^2 in the welfare regressions by 0.018. It increases the education and highway R^2 statistics by 0.006 and 0.032, respectively.

IV. SUMMARY AND CONCLUSIONS

This paper focuses on expenditure by state and local governments over the 1960–1990 period. Our objective is to assess how important legislative structure and political parties are for explaining spending behavior. Our main findings are the following:

1. After controlling for constituent interests, the number of seats in the upper house of a state's legislature is positively associated with per capita state and local direct general expenditure. In addition, a large legislature appears to lead to greater spending for both capital and non-capital programs, and across welfare, education, and highway expenditure categories.

2. After controlling for constituent interests, we were unable to determine whether expenditure varies according to which party is in power. However, party control makes a difference for the composition of expenditure. In particular, Democrats compared to Republicans spend more for welfare, and less for highways.

3. There is no evidence that divided government, or gridlock, has an affect on total spending one way or the other.

The findings concerning the number of seats in the upper house are consistent with the hypothesis that large legislatures suffer from more severe common pool problems concerning the tax base, what Weingast, Shepsle, and Johnsen [1981] call "the Law of $1/n$." Representatives push excessively for spending projects that deliver particularistic benefits to their constituents because the cost of such projects is spread over the state's entire population. However, the inability to detect such effects in the lower house is a little troubling for this interpretation.

The results are also consistent with our recently proposed hypothesis that large houses are more susceptible to bipartisan policy gerrymandering. Even if each representative has the same preferences as the median voter in his district, the preferences of the median legislator are not in general the preferences of the median voter in the population. If policy is set by the median legislator, then policy will deviate from the median voter outcome. In Gilligan and Matsusaka [1993] we suppose that someone wants to allocate voters to districts so as to bias the policy outcome and ask how far it can be pushed from the median voter outcome. We show that the potential bias is increasing in the number of seats. The basic intuition is suggested by the observation that with only one seat, the median legislator has the same preferences as the median voter in the population, while with three seats the gerrymanderer has some flexibility to dilute the strength of voters with opposing views.

We should emphasize that our results on seats and spending do not speak directly to the question of whether legislators spend more than their constituents would like. The existence of "overspending" by legislators is one way (but not the only way) to interpret the evidence in Peltzman [1992] and Matsusaka [1995]. Our paper provides evidence consistent with the well-known logrolling theory of overspending. The findings in our paper and the Peltzman and Matsusaka papers can be arranged into a picture of logrolling legislators who deliver more spending than their constituents would like, but there may be other ways to assemble the pieces that give a more appealing view of legislatures.

The findings concerning the party variables only weakly support the conjecture that political parties matter for outcomes. In addition, to the extent that parties do make a difference, the idea that Democrats spend more than Republicans appears to miss the key distinction between the parties. To the extent that party effects are visible, they can be seen in the composition of spending.

Finally, expenditure behavior appears to have been primarily affected by the number of seats and the political party that controlled a state's *upper* house. We did not anticipate this finding nor is there an obvious explanation for it. Further inquiry into the apparent pivotal nature of upper chambers would seem to be in order.

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