THREE ESSAYS ON DISTRIBUTIVE POLITICS: HOW LEGISLATURE SIZE AND PARTISAN POLITICS IMPACT THE DISTRIBUTION OF GOVERNMENT SPENDING

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Abstract

This dissertation is composed of three essays that investigate how legislature size, political alignment, and political polarization impact the distribution of government expenditures at different levels of government. The first essay focuses on political alignment and polarization while the last two essays focus on legislature size at the cross-country and US state level, respectively.

In the first essay, we find evidence that during times when political polarization in the US Senate is relatively low, states with more senators in the majority receive a larger than average share of federal grant spending per capita. We also find that although states with more senators in the majority receive a larger than average share of federal grant spending per capita when both chambers of the US Congress are aligned, that this amount is smaller than what these states receive when control of Congress is divided. Lastly, we verify that states with the entire Senate delegation in the majority are driving these results.

In the second essay, we find significant evidence that countries with bicameral legislatures experience larger levels of central government expenditures as a percentage of GDP when the upper chamber is larger than average. Conversely, we were not able to show any consistent relationship between unicameral legislature size and central government expenditures as a percentage of GDP or between lower chamber size and central government expenditures as a percentage of GDP.

In the final essay, we examine the role legislature size has in determining the growth in

state-level per capita spending. Overall, we were unable to verify a relationship between lower chamber size, upper chamber size, or the ratio of the lower-to-upper chamber size and the change in total spending per capita. We do find a positive relationship between lower chamber size and the change in per capita welfare spending.

List of Abbreviations Acronyms, and Symbols

$	American Dollar
$=$	Equal to
Δ	First Difference
$\forall$	For All
$>$	Greater than
$\geq$	Greater than or equal to
$\equiv$	Is congruent to
$<$	Less than
$\leq$	Less than or equal to
∂	Partial Derivative
%	Percent
$\surd$	Square Root
$\sum$	Summation
ADF	Augmented Dickey-Fuller Unit Root Test

Alt.	Alternative Hypothesis
ARRA	American Recovery and Reinvestment Act
CFFR	Consolidated Federal Funds Report
cov	Covariance
CPI	Consumer Price Index
GDP	Gross Domestic Product
FE	Fixed Effects
GMM	Generalized Method of Moments
GSP	Gross State Product
ICPSR	Inter University Consortium for Political and Social Research
IFS	International Financial Statistics Database
IMF	International Monetary Fund
LSDV	Least Squares Dummy Variable
Max	Maximum Value
Min	Minimum Value
MWC	Minimum Winning Coalition
OECD	Organization for Economic and Co-operation

PP	Phillips-Peron Unit Root Test
P-value	Percentage chance that an estimated effect exists even if, in reality, the effect does not exist
R^2	R-squared
SD	Standard Deviation
US	United States of America

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Dedication

This dissertation is dedicated to my parents, Cheryl, Bryce, and Bill, and my grandparents, without whose support I would never have made it this far. I would also like to dedicate this to Anna-Leigh Stone, whose love and encouragement pushed me through.

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Introduction

This dissertation is composed of three essays that analyze how the composition and characteristics of political institutions shape fiscal policy, with a primary emphasis on legislatures. In every democratic nation throughout the world and in every state that makes up the United States, the legislature plays a central part in constructing and ultimately approving the government's budget. In the process of determining how and how much the government will spend, politics inevitably plays a role.

The first essay, "Partisan Alignment and Federal Spending at the State Level," is an attempt to understand the political relationships that determine how the U.S. government allocates spending to the states. This work builds off of Hoover and Pecorino (2005) and fits into a body of literature that studies how the political affiliation of a state's House or Senate delegation influences the amount of federal spending a state receives. We find that senators in the majority are able to secure a larger than average share of per capita federal grant spending at relatively low levels of political polarization.

It has also been argued that the structure of the legislature plays a role as well. This is the central theory behind the second and third essays. Weingast, Shepsle, and Johnson (1981) argued that under certain conditions, increasing the number of legislative districts will lead to larger government spending projects. Several authors have tested this idea by studying the relationship between legislature size and government spending. The second essay, "The Relationship Between Legislature Size and Fiscal Policy: A Cross-Country Examination,"

builds upon this literature by examining how legislature size influences government spending in the presence of certain spending shocks that affect several different countries. We find evidence showing a positive relationship between upper chamber size and government spending as a percentage of GDP.

The last essay, "Government Spending, Shocks, and the Role of Legislature Size: Evidence from the American States," extends this analysis to the American states. The individual states are an ideal environment for studying this relationship for several reasons. For one, differences in legislative structure and electoral systems are minimized. This characteristic allowed Chen and Malhotra (2007) to devise an alternative theory regarding how legislature size and government spending are related. This essay will submit their theory to further empirical scrutiny. Secondly, state-level data is more reliable and available for longer periods of time. A data set that is more disaggregated and more balanced will make it possible to examine the relationship between legislature size and different categories of spending. However, we are unable to find a positive relationship between per capita spending and legislature size.

Chapter 1

Partisan Politics and Federal Spending at the State Level

I Introduction

Studies of the distribution of federal expenditure to the states have shown that states and legislative districts that are politically aligned with the president receive more spending per capita. It has also been shown that having more senators or representatives in the majority is associated with more federal expenditure per capita; however, this evidence is less robust. Using state-level federal budget and political data over the period 1983 – 2010 we examine the following questions: 1) is a state with more senators or representatives in the party of the president able to secure a larger share of per capita spending when the president's party also controls both chambers of Congress?; 2) is a state with more senators or representatives in the chamber majority able to procure a larger share of per capita spending if that party controls both chambers of Congress?; 3) does chamber-level political polarization impact the benefits of having more senators or representatives in the chamber majority? Answering these questions will provide further insight into how the national political climate impacts the share of spending a state will receive.

The length of the data set makes it possible to exploit changes in political alignment within Congress and between the Congress and the presidency that have not been possible

in previous studies. Moreover, we are able to take advantage of the recent increase in political polarization that has been observed in both chambers of Congress. Using dynamic fixed-effects estimation we find evidence that the following political determinants are most important in securing federal grant expenditure: states with both senators in the majority are able to secure a larger share of per capita federal grant spending at lower levels of political polarization than at the average level of political polarization and when control of Congress is divided between the two parties. We also find weak evidence that states with a larger percentage of House members in the majority receive a larger share of per capita grant expenditure when House-level political polarization is relatively high and states with a larger percentage of House members in the party of the president receive a larger share of per capita grant spending when the executive and legislative branches are aligned.

Overall, these findings provide evidence that political alignment and political polarization at the federal level matter, though not always in the ways one might expect. These findings also bolster important work by Levitt and Snyder (1995) and Berry, Burden and Howell (2010) by providing more explicit evidence that alignment across branches influences the distribution of spending and by quantifying how much a larger delegation in the party of the president is worth during periods of alignment and divided control.

With longer panel data sets, some data issues arise that are not of concern in shorter panels. Specifically, several spending variables and control variables that are traditional to this literature contain unit roots. Without accounting for unit root issues, researchers run the risk of making inferences from spurious regressions. Also, as Kawaura (2003) showed, changes in per capita federal expenditures to the states reflect changes in both the level of allocation as well as the share of the allocation relative to the federal budget as a whole. Failing to disentangle these changes can result in biased estimates. In studies of distributive politics, it is important to focus on a state's *share* of the federal budget relative to what other states are receiving. In this analysis, we transform certain variables in order to remove

the confounding effects of unit roots and to isolate changes in relative budget shares. Failing to account for these issues can have a nontrivial impact on the model's results. As the data sets used in the study of politics become longer, researchers should carefully consider these issues.

In Section II the literature on the political determinants of federal expenditure to the states is reviewed. Section III provides a discussion of the data and the hypotheses to be tested. Sections IV and V set up the empirical model, review the results, and conduct robustness checks. In Section VI we discuss the results and Section VII concludes.

II Literature Review

The idea that political allies are rewarded with more federal spending compared to those controlled by the opposing party suggests that the politics of distribution is, to quote Baron (1991), "majoritarian and not universalistic (Baron, 1991, p. 57)." That is, members of the party in power will try to maximize the distribution of spending received by fellow party members. The majoritarian hypothesis also relates to the theory of the minimum winning coalition, as put forth by Riker (1962). Conversely, universalism – see Weingast (1979) and Shepsle and Weingast (1981) – is the idea that risk-averse legislators will prefer extending benefits to all states or districts since, *ex ante*, they will not know the composition of the minimum winning coalition.

Levitt and Snyder (1995) compared spending programs that were initiated during periods of partisan alignment between the presidency and Congress with spending programs that began during periods of divided government. They found that congressional districts with a higher percentage of Democratic voters received more federal spending during the 1975 – 1981 period. Spending projects initiated during this period were most likely passed during a period of unified Democratic control of Congress. Programs that began in 1978 were signed

into law by a Democratic president.[1] Conversely, Levitt and Snyder found no relationship between federal expenditures and Democratic voters for programs that were initiated between 1981 and 1990, when the government was divided. Thus, partisan alignment within and between the different branches of government has an effect on how federal expenditures are distributed geographically.

Bickers and Stein (2000) studied the 1994 Republican takeover of Congress. Analyzing the House of Representatives, the authors found a significant and positive increase in the issuance of contingent liabilities, which they claimed was the Republicans' most preferred way of rewarding their constituents.[2] Importantly, Bickers and Stein identified an effect associated with a change in majority control of a chamber of Congress. However, their analysis did not extend to the Senate and the time period was relatively short.

Hoover and Pecorino (2005), using data disaggregated into five spending categories, found weak evidence of majority party effects over the period 1983 – 1999. An advantage of Hoover and Pecorino's study over Levitt and Snyder (1995) is that their sample period included majority party changes in both the Senate and the House of Representatives. The authors expected federal procurement spending and spending on federal grants to be most susceptible to party politics. However, the only robust evidence pointing towards a benefit to belonging to the majority party was a positive and statistically significant relationship between federal grant spending per capita and the percentage of a state's House delegates in the majority party.

Albouy (2013) estimated party effects over the period 1983 – 2004. He was able to take advantage of slightly more changes in majority control of the Senate than Hoover and

[1] While Jimmy Carter's term as president began in 1977 any spending programs he signed into law went into effect in 1978 as the earliest.

[2] "Contingent liabilities include direct loans, guaranteed loans, and federal insurance programs. These programs underwrite risks for individuals and groups by assuring that the federal treasury stands ready to make good on losses. Actual outlays primarily occur when the recipient fails to repay a loan or suffers a loss of insured property (Bickers and Stein, 2000, p. 1073)."

Pecorino (2005) but the number of changes within the House remained the same. Specifically, over this period, majority control of the Senate changed five times. However, majority control of the House of Representatives only changed once. Albouy (2013) found that states represented in the Senate by a delegation belonging to the majority party received approximately $180 million more in grants.[3] Approximately $50 million of this amount was given in the form of transportation grants. Evidence from the House of Representatives was much weaker. However, Albouy did find some evidence that states with a majority delegation received approximately $600 million more in defense spending.

A significant amount of evidence suggests that the president also plays a crucial role in determining how federal spending is allocated to the states. In fact, Berry et al. (2010) go so far as to claim that "the actual proposer inhabits the White House, a basic fact that the distributive politics literatures has overlooked (ibid: p. 785)." Wright (1974) argued that the Roosevelt administration, which had considerable power over the allocation of New Deal spending, targeted expenditures in order to maximize electoral votes. Hoover and Pecorino (2005) controlled for the percentage of a state's house delegation and the number of senators in the same party as the sitting president. The relationships between federal grant spending and both the percentage of house delegates and the number of senators in the same party as the president were positive, highly significant, and robust to a number of different specifications. These variables were not significantly related to any of the other spending categories.

Larcinese, Rizzo and Testa (2006), analyzing a time period similar to that studied by Hoover and Pecorino (2005), found that states where the majority of the house delegation belonged to the same party as the president received more federal expenditures per capita.

Berry et al. (2010) focused almost exclusively on the role the executive branch plays in determining how federal spending is allocated. They claim that the president is not only the

[3] Albouy (2013) calculates this total using 2004 figures.

chief proposer (as has already been referenced) but also retains considerable control over how federal expenditures are distributed even after Congress has approved the budget. Berry et al. tested their theory using congressional district and county-level spending and political data up through 2007.

Berry et al. also attempted to measure within-party differences and determine if a congressman's relative ideological position affected the funding his or her district would receive. In other words, would a president attempt to influence a more moderate member of the opposing party by directing largess towards his or her district? Berry et al. used first-dimension DW-NOMINATE scores to construct a relative measure of polarization for each member of the House of Representatives. First-dimension DW-NOMINATE scores are a widely used measure of where a particular representative or senator falls on the "liberal-conservative" spectrum. DW-NOMINATE scores are available from Voteview.com, a website maintained by Royce Carroll, Jeff Lewis, James Lo, Nolan McCarty, Keith Poole, and Howard Rosenthal.[4] More will be said about this measure in the next section. However, Berry et al. find no evidence that more moderate members of the House, regardless of party, receive more spending. Berry et al. appear to be the first to study how political polarization relates to the distribution of federal expenditures. Polarization measures derived from first-dimension DW-NOMINATE scores will be used in the current work as well. This will be discussed in the next section.

III Data and Hypotheses

The hypotheses put forth here will be tested using state-level data over the period 1983 – 2010. We begin our analysis in 1983 because this is the first year for which the dependent variables of interest are available. This section will first present and discuss the dependent

[4] http://voteview.com/dwnominate.asp

fiscal variables followed by the political variables of interest and hypotheses, and finally the economic and demographic control variables. Summary statistics are provided in Table 1.1 below. Measurement of the House- and Senate-level political variables of interest is affected by mid-year party switches, resignations, and deaths. A discussion of how these issues were dealt with is provided in the appendix.

Table 1.1: Summary Statistics: 1983 – 2010

Variable	Mean	SD	Min	Max
Expenditures				
TOTAL	7716.40	2008.68	4631.43	17704.95
RETIRE	2495.23	415.09	1040.45	4198.86
OTHER	1548.65	657.49	326.53	6620.56
WAGES	987.62	637.02	349.18	5792.75
GRANTS	1512.76	698.19	536.50	5969.25
PROCURE	1192.46	911.87	206.98	7270.43
Normalized Expenditures				
TOTAL	1	0.20	0.67	1.93
RETIRE	1	0.13	0.49	1.42
OTHER	1	0.31	0.37	3.18
WAGES	1	0.63	0.34	4.59
GRANTS	1	0.33	0.53	3.12
PROCURE	1	0.72	0.17	4.80
Economic and Demographic Control Variables				
INCOME	33719.71	6398.26	18762.83	59395.80
POPULATION (in millions)	5.40	5.92	0.45	37.34
LANDAREA	70747.54	85153.20	1044.90	571951.31
ELDERLY	12.49	2.02	1.60	18.60
UNEMPLOY	5.79	2.04	2.20	18.00
Political Variables				
HOUSEP	0.48	0.29	0	1
SENATEP	1.00	0.79	0	2.00
SMAJOR	1.09	0.79	0.00	2.00
HMAJOR	0.56	0.29	0	1
ALIGNC	0.75	0.43	0	1
ALIGNP	0.25	0.43	0	1
HPOLAR	0.79	0.14	0.56	1.00
SPOLAR	0.69	0.08	0.57	0.81
SENATE	0.99	1.01	0.05	4.92
GOVP	0.44	0.50	0	1
MARGIN	0.14	0.10	0	0.52
VOTE	0.71	0.45	0	1
HTENURE	1	0.50	0	3.61
STENURE	1	0.61	0	3.61

Expenditure variables and *INCOME* expressed as per capita figures in 2010 dollars. All political variables are lagged by one year.

III.1 Dependent Variables

The fiscal variables of interest are collected from the US Census Bureau's Consolidated Federal Funds Report (CFFR). Total federal expenditures by state are used along with five subcategories of spending. These are retirement and disability, grants, procurement, salaries and wages, and other direct payments. Spending data for each state is converted to 2010 dollars using the Consumer Price Index (CPI), obtained from the Bureau of Labor Statistics, and divided by state population (US Census, various years) to produce real per capita measures of federal spending. The total expenditure category (*TOTAL*) encompasses all federal payments to the states. This includes expenditures, outlays, and grants to state and local governments as well as nongovernmental entities. Retirement and disability spending (*RETIRE*) includes retirement and disability payments to all federal employees, all types of social security payments, as well as select Veterans Administration programs and other federal programs. The grants category (*GRANTS*) includes federal spending on formula grants and project grants. Formula grants are distributed to states based on a predetermined formula that is a matter of federal law. Funds allocated by formula grants are not limited to specific programs. Conversely, project grants go to specific projects for specific periods of time. These include educational fellowships, scholarships, and research opportunities as well as survey grants and construction grants, among others. The procurement category (*PROCURE*) includes all procurement contracts from all federal departments and agencies. Salaries and wages (WAGES) encompasses the salaries and wages of all federal employees. Lastly, the category other direct spending (*OTHER*) includes payments to individuals apart from retirement and disability payments.

Previous research discussed in Section II informs us about which spending categories should be most susceptible to the type of political relationships that will be tested. Levitt and Snyder (1995) separated federal programs into "low variation" and "high variation" categories (ibid; p. 964). Berry et al. (2010) adopted a similar strategy. High variation pro-

grams were classified as those where funding exemplified greater year-on-year variation and mostly included federal grants and procurement spending. Similarly, Hoover and Pecorino (2005) and Albouy (2013) found that federal grant spending was the category most sensitive to the political variables.

III.2 Primary Political Variables

As discussed in Section II, having legislators in the party of the president has consistently been shown to be an important determinant of federal spending received by a state or congressional district. And while the evidence is weaker, belonging to the Senate or House majority has been shown to be important as well. We will control for the percentage of each state's House delegation that belongs to the president's party (*HOUSEP*) and the number of each state's senators who belong to the president's party (*SENATEP*). The variable *HMAJOR* will measure the percentage of each state's House delegation that belongs to the House majority and the variable *SMAJOR* will measure the number of each state's senators who belong to the Senate majority. In addition, the dummy variables *SENATEP1*, *SENATEP2*, *SMAJOR1*, and *SMAJOR2* will be used in separate regressions to explore whether or not there are important differences between having a split Senate delegation or a unified delegation relative to one with no members in either the party of the president or the chamber majority.

A central contribution of the current work is a test for whether or not belonging to the party of the president is worth more when the president's party also controls both chambers of Congress. Political alignment across the elected branches is controlled for with the variable *ALIGNP*, which will equal 1 if the president's party controls both chambers of Congress and will equal zero otherwise. There are nine years during the sample period during which the executive and legislative branches were controlled by the same party.

Alignment across chambers is also of interest. Specifically, we test for whether or not the

procurement abilities of a senator or congressman who belongs to the chamber majority are enhanced if both chambers are controlled by the same party. An indicator variable *ALIGNC* will equal 1 if the same party controls both chambers of Congress and 0 otherwise. Over the period 1983 – 2010 there were seven years where the House of Representative and the Senate were not controlled by the same party. The final matter of interest is how political polarization affects the relationship between the number of legislators who belong to the chamber majority and the amount of federal spending received by a state. Specifically, we are concerned with political polarization within each chamber of Congress. If the politics of distribution is universalistic, we should not expect political polarization to influence how the federal budget is distributed to the states. That is, relative ideology between the two parties should not lead some states to get a larger share of spending than others. However, if majoritarian politics influences the distribution of spending then we should expect political ideology to play a role. First dimension DW-NOMINATE scores, mentioned in the previous section, will be used to construct a measure of political polarization. Each senator and congressman is assigned a score that falls between the interval [-1,1], with -1 indicating the extreme "liberal" position and +1 indicating the extreme "conservative" position. The scores are determined using the entire available history of roll call votes on all issues and allow for the comparison of how politically polarized the individual chambers have been over time as well as how individual legislators have changed their political positions over time.[5] McCarty et al. (2006) document a period of increased political polarization that began in the 1970s and has yet to abate. The data used in this essay begins during a period when polarization was becoming an entrenched characteristic of American politics. For each chamber, polarization is measured as the absolute value of the difference between the median DW-NOMINATE score for each party (*HPOLAR* and *SPOLAR*, respectively). Thus, larger values of the

[5]See McCarty, Poole and Rosenthal (1997, 2006), Poole and Rosenthal (2000), and Poole (2005) for more detailed explanations of how DW-NOMINATE scores are constructed.

variables *HPOLAR* and *SPOLAR* indicate higher levels of political polarization.

III.3 Political Control Variables

The number of senators per capita (*SENATE*) for each state will be included. Atlas, Gilligan, Hendershott and Zupan (1995) and Hoover and Pecorino (2005) showed that senators per capita is an important explanatory variable in the allocation of federal expenditures.[6] For each state, this variable is measured by simply dividing the number of senators by population.

A variable measuring the tenure of each state's congressional and senatorial delegations (*HTEN* and *STEN*, respectively) will also be included. Crain and Tollison (1977) found the tenure of a state's House delegation (measured as the total number of combined years served by the delegation) to be a significant determinant of the federal expenditures received by that state. They were unable to establish a statistically significant impact of the tenure of a state's Senate delegation. Levitt and Poterba (1999) found a positive association between state-level economic growth and the seniority of a state's House delegation but they did not find such a link related to the seniority of a state's Senate delegation. Levitt and Poterba also found no link between seniority and per capita federal spending received by a state. Mathews, Stevenson and Shughart (2009) measured the impact of *relative* tenure. That is, the average tenure of each state's chamber delegation measured relative to the average tenure for the respective chamber as a whole. In their analysis, Mathews et al. found the relative tenure of a state's House delegation to have a positive effect on the received amount of federal expenditures and similar to Crain and Tollison (1977) found no effect associated with the tenure of a state's Senate delegation. More recently, Young and Sobel (2013) found a positive correlation between the average tenure of a state's House delegation and its

[6]Recently, Larcinese, Rizzo and Testa (2013) have shown that the relationship between Senate malapportionment and federal spending depends heavily upon population dynamics. However, since this is not a relationship of interest we measure *SENATE* in a way consistent with the previous literature. Their work follows from a notable exchange between Wallis (1998, 2001) and Fleck (2001), who debated the interpretation of senators per capita.

receipt of stimulus funds per capita from the 2009 American Recovery and Reinvestment Act (ARRA) but a negative correlation between the average tenure of a state's Senate delegation and ARRA funding per capita. Here, the variables *HTEN* and *STEN* will measure relative tenure in the House and Senate, respectively. Data on congressional tenure from 1983 to 1996 is calculated using data from the Inter University Consortium for Political and Social Research (ICPSR). However, this particular data set only provides data up through 1996. These variables were extended up to 2010 using data from *The Biographical Directory of the United States Congress*.[7]

Certain characteristics of the presidential vote will also be tested. Previous studies have found conflicting evidence regarding the impact of the presidential vote. Whereas Hoover and Pecorino (2005) found that states the sitting president narrowly lost received more spending per capita than states that voted for the sitting president, Larcinese et al. (2006) found that states showing overwhelming support for the sitting president were rewarded more than states the president won by a small margin. Thus, subjecting the presidential vote to further empirical scrutiny is warranted. Similar to Hoover and Pecorino (2005), the present analysis controls for whether or not the sitting president won a particular state in the last election (*VOTE*) and the absolute value of the margin of victory in the most recent presidential election (*MARGIN*). For each state, *VOTE* takes the value of 1 if the sitting president won that particular state in the most recent presidential election and zero otherwise. Also similar to Hoover and Pecorino, an interaction term composed of the variables *VOTE* and *MARGIN* is created so that states the president narrowly won can be distinguished from states the president narrowly lost. Information on each state's presidential election voting history is obtained from *America at the Polls: 1960 – 2000* for the years 1983 to 1999 and from *America Votes* (various years) for the years 2000 to 2010. Lastly, we include a variable that captures whether or not a state's governor belongs to the same party as the president

[7] http://www.senate.gov/pagelayout/history/h_multi_sections_and_teasers/Biographical_Directory.htm

($GOVP$). Both Hoover and Pecorino (2005) and Larcinese et al. (2006) show evidence that states where the governor was aligned with the president received more total spending per capita and Hoover and Pecorino (2005) showed a positive correlation between governor-president alignment and the amount of per capita procurement spending received by a state. Information on each governor's political affiliation is collected from various editions of the Book of the States. All political variables will enter into regression models with a one year lag in order to directly control for the one year lag in the US budget process.[8]

III.4 Control Variables

Several economic and demographic control variables are included as well. We control for the effect income can have on the amount of federal expenditures a state receives by including a measure of real per capita state income ($INCOME$), measured in constant 2010 dollars. The age distribution can also impact the amount of federal expenditures a state receives, particularly concerning programs that depend upon age. The percentage of the population aged 65 and older ($ELDERLY$) is included to control for this effect. The inclusion of state-level unemployment rates ($UNEMPLOY$) is to control for federal spending related to poor economic performance. Lastly, land area per capita ($LANDAREA$) is included to control for economies of scale that may be associated with certain types of spending programs.

III.5 Variable Normalization

Appendix Table A1 shows that we cannot reject the presence of a unit root for many of the spending categories. Furthermore, the goal in this paper is to study how the political process impacts the share of spending each state receives. Thus, in the spirit of Kawaura

[8]For example, the budget passed by the Congress in year t does not take effect until year $t+1$.

(2003), spending variables are normalized in the following way:

$$NSPENDING_{it} = \frac{SPENDING_{it}}{\frac{1}{n}\sum_{j=1}^{n} SPENDING_{jt}}, for t = 1983, 1984, ..., 2010, \tag{1.1}$$

where *SPENDING* refers to the adjusted per capita level of spending for state i in year t. As Kawaura points out, year-on-year per capita spending data reflect both changes in the level of per capita spending and changes in the share of per capita spending relative to the total budget allocation. Normalizing the variables in this way isolates the amount of per capita spending each state receives relative to the national average in a given year. States with spending shares greater than 1 are receiving per capita spending that is greater than the US average and states with spending shares less than 1 are receiving per capita spending that is less than the US average. With the exception of *RETIRE* and *WAGES* the normalized dependent variables are now stationary according to most of the unit root tests. These results are shown in appendix Table A2. Thus, normalization makes the majority of the dependent variables stationary without having to resort to differencing, which can result in lost information. Also, expressing the dependent variables in per capita shares allows us to focus on the distributive nature of federal spending.

In order to make the control variables comparable with the dependent spending variables, the variables *INCOME*, *ELDERLY*, *UNEMPLOY*, *LANDAREA*, and *SENATE* are normalized as well. Thus, *INCOME* is weighted by per capita US income, *ELDERLY* is weighted by the percentage of the total US population aged 65 and above, *LANDAREA* is weighted by US land area per capita, and *SENATE* is weighted by senators per capita for the US as a whole.[9] For redistribution at time t what is important is a state's per capita income relative to other states at time t, not the absolute level of per capita income. A state's relative share of residents over sixty-five years of age and a state's share of unemployed residents

[9] As with the dependent variables, the original variable names refer to the normalized variables.

should matter for distributive purposes as well. That is, it is reasonable to expect states with an above average share of elderly residents to receive an above average share of federal retirement spending. Similarly, we would expect states with an above average share of unemployed residents to receive a larger share of food stamp or unemployment compensation spending, programs that are included in the *OTHER* spending category. However, even after normalization at least two of the four panel unit root tests that are employed indicate that a unit root is still present in the variables *INCOME*, *ELDERLY*, and *SENATE*. These results are provided in Table A3 in the appendix. Since it is not valid to regress a variable integrated to order zero on a variable integrated to order one, the regressors that contain a unit root enter into the regressions in first differences.[10]

IV Empirical Model

We first estimate baseline regressions where each normalized spending variable is regressed on the normalized control variables and all political variables. This model will be discussed first followed by the regressions that control for alignment and polarization. The model specification employs a within transformation to remove state level fixed-effects. An F-test reveals that year fixed-effects are jointly insignificant, thus we do not include them in any of the regressions.[11] The Breusch and Pagan (1979) test reveals the presence of heteroscedasticity and the Wooldridge (2002) test reveals serially correlated errors within panels. Thus, we cluster standard errors by state for each fixed-effects regression. Also included as an explanatory variable in each regression is a lag of the dependent expenditure variable. Including the lagged value of each state's spending share allows for the possibility that a state's share of spending in year t is correlated with its spending share in year $t+1$.

[10] Enders (2009) provides a clear discussion of this issue and shows that "regressions using such variables are meaningless (ibid: p. 199)."

[11] The normalization process makes their inclusion unnecessary. In a level regression the F-test unsurprisingly indicated that year fixed-effects were appropriate.

This modeling choice also implies that the impact of the political determinants of a state's spending share might play out over several years, rather than be concentrated entirely within a single year. Moreover, using a similar data set Larcinese et al. (2013) show that including the lagged dependent variable is appropriate when analyzing federal expenditures received by the states.[12] With the lagged dependent variable we can offer both short- and long-run estimates of the impact of the political variables of interest. In particular, if a political variable x has the coefficient β and the lagged dependent variable y_{it-1} has the coefficient γ, then the long-run impact on the equilibrium level of spending, $\bar{y}$, of a permanent increase in the political variable is $\partial\bar{y}/\partial\bar{x} = \beta/(1-\gamma)$.

Nickell (1981) showed that including a lag of the dependent variable in a fixed-effects model can result in a point estimate on the lagged variable that is severely biased unless the time dimension (T) is sufficiently large. Several estimation methods have been developed to control for this bias but most of these methods are designed for data sets with a large number of cross-sectional observations and relatively few observed time periods.[13] However, Judson and Owen (1999) show that as T grows larger the bias associated with the estimated coefficients on the contemporary explanatory variables when using an LSDV regression becomes negligible and the bias associated with the lagged dependent variable becomes smaller, though in some cases it could remain sizable. Moreover, with longer panels such as those used here, implementing GMM and IV estimation procedures can be less efficient and introduce computational difficulties that cannot be overcome. Each panel used here is 28 years long. Thus, we feel comfortable in using an LSDV regression.[14]

[12]Specifically, Larcinese et al. (2013) analyze state level federal expenditures per capita. However, the same intuition should still hold with respect to normalized state level federal expenditures per capita.

[13]These include GMM-type estimation methods such as Arellano and Bond (1991) and instrumental variable methods (IV) such as Anderson and Hsiao (1982).

[14]Judson and Owen (1999) also recommend using a bias-corrected LSDV (LSDVC) estimator of the type first proposed by Kiviet (1995) when using a balanced panel data set. However, implementing this estimator requires either homoscedastic data or correctly weighted data in order to remove the heteroscedasticity. Moreover, Bun and Kiviet (2001) recommend computing standard errors of the LSDVC estimates via a parametric bootstrap procedure (e.g. Bruno (2005)). But it is well known that parametrically bootstrapped

IV.1 Polarization and Alignment

The primary models in this paper assume that the variables (*HOUSEP*, *SENATEP*, *HMAJOR*, and *SMAJOR*) are conditional on several political factors. First, it is assumed that *HMAJOR* and *SMAJOR* are conditional upon political polarization in both the House and the Senate. This assumption is tested by modifying the baseline regression model to include the interaction terms $HMAJOR * HPOLAR$ and $SMAJOR * SPOLAR$. As was discussed in Section III, political polarization in each chamber is measured as the absolute value of the difference between the median DW-NOMINATE score for each political party. However, since *HPOLAR* and *SPOLAR* are continuous, these variables will enter into the regressions as deviations from the sample mean. Thus, the marginal effect of *HMAJOR* when $HPOLAR_{dev}$ equals zero should be interpreted as the marginal effect of *HMAJOR* when *HPOLAR* equals the sample average.[15] It is important to note that since the hypotheses of interest are now conditional in nature, inference cannot be made simply by looking at the coefficients and standard errors associated with *HMAJOR* and *SMAJOR*. For example, the marginal effect of *HMAJOR* is calculated according to the equation

$$\frac{\partial(NSPENDING|HPOLAR_{dev})}{\partial HMAJOR} = \hat{\beta}_1 + \hat{\beta}_2 HPOLAR_{dev}. \tag{1.2}$$

The marginal effect of *SMAJOR* conditional on senate-level political polarization is calculated in a similar way. Equation 2 shows a short-run effect. The long-run marginal effect is calculated as

$$\frac{\partial(NSPENDING|HPOLAR_{dev})}{\partial HMAJOR} = \frac{\hat{\beta}_1 + \hat{\beta}_2 HPOLAR_{dev}}{(1-\hat{\gamma})} \equiv \Phi_1 \tag{1.3}$$

standard errors are invalid if heteroscedasticity is present. Because of these issues we do not implement the LSDVC estimator.

[15] $HPOLAR_{dev}$ refers to the deviation of polarization in year t from the sample mean. *SPOLAR* is similarly modified.

where $\hat{\gamma}$ is the estimated coefficient on the lagged dependent variable. The correct standard error associated with equation 1.2 is

$$\hat{\sigma} = \sqrt{var(\hat{\beta}_1) + HPOLAR^2_{dev} * var(\hat{\beta}_2) + 2HPOLAR_{dev} * cov(\hat{\beta}_1, \hat{\beta}_2)} \tag{1.4}$$

Finally, the correct standard error associated with the long-run marginal effect is computed as

$$\hat{\sigma}_{LR} = \sqrt{\boldsymbol{g_1'}\hat{\sigma}^2\boldsymbol{g_1}} \tag{1.5}$$

where $\boldsymbol{g_1'} \equiv \frac{\partial \Phi_1}{\partial \boldsymbol{\beta'}}$, $\boldsymbol{\beta'}$ is a vector of parameter estimates, and $\hat{\sigma}^2$ is calculated according to equation 1.4. Therefore, it is possible that the marginal effect of a particular variable of interest is actually economically and statistically significant even though the coefficient on the interaction term is not statistically significant in the traditional sense.[16] This can occur if the covariance term in equation 1.4 is negative. In fact, if $cov(\hat{\beta}_1, \hat{\beta}_2)$ is negative and large enough in magnitude it is entirely possible for either one or both of the estimated coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$ to be statistically insignificant but the conditional marginal effect to be significantly different from zero.[17]

In order to capture how *HOUSEP* and *SENATEP* are impacted when the same political party controls both elected branches of government the interaction terms $HOUSEP * ALIGNP$ and $SENATEP * ALIGNP$ are added to the baseline regression specification. The final specification will assume that *HMAJOR* and *SMAJOR* are conditional upon political alignment between the House and the Senate. To compute the conditional marginal effects of these variables separate interaction terms, $HMAJOR * ALIGNC$ and $SMAJOR * ALIGNC$, are created. The short- and long-run marginal effects and standard

[16]For a thorough discussion regarding the proper interpretation of marginal effects when interaction terms are included see Brambor, Clark and Golder (2006).

[17]See Brambor, Clark and Golder (2007) and de Haan, Jong-A-Pin and Mierau (2012) for examples of such findings.

errors for the specifications controlling for *ALIGNP* and *ALIGNC* are calculated according to equations 1.2 – 1.5, with *ALIGNP* and *ALIGNC* replacing $HPOLAR_{dev}$.

In the regressions where the impacts of political alignment and political polarization are examined we have chosen not to include *HPOLAR*, *SPOLAR*, *ALIGNP*, and *ALIGNC* directly into the regression equations. Consider, for example, a case where *HMAJOR* and *SMAJOR* are omitted from the model. It is inconceivable that *SPOLAR* and *HPOLAR* or *ALIGNC*, which, in a given year, are common to all states, would directly increase (or decrease) the spending shares of all states. A similar argument can be made for excluding *ALIGNP*.[18]

IV.2 Base Line Model: Results

Results from the base line fixed-effects estimates are presented in Table 1.2. At this point, we should say something about the *RETIRE*, *WAGES*, and *OTHER* spending categories. Recall that even after normalization, *RETIRE* and *WAGES* still displayed evidence of non-stationarity. Moreover, *a priori*, we did not expect the political variables of interest studied here to have much of an impact on these spending categories. While these spending categories are not immune to politics, annual allocations of this type of spending to the states are not influenced by politics in the same way as the spending categories that display more year-on-year variability. There is certainly a fair amount of politics that decides the creation or alteration of federal retirement programs such as Social Security. Similarly, the creation

[18]Brambor et al. (2006) discuss the special case where a constitutive term that is part of an interaction can be omitted. The conditions they claim should be met are: 1) the expected impact of the omitted variable must be zero when the other constitutive term equals zero; 2) the model should be estimated with all constitutive terms included and it should be confirmed that the variable proposed to be eliminated is, in fact, not statistically different from zero. The second condition is easily verified. All models that include interaction terms were estimated with all constitutive terms included and in each case it was verified that the coefficients on these variables were not statistically different from zero. Furthermore, even with all constitutive terms included in the model the results we reach do not change. In order to meet the first condition, Brambor et al. note that the constitutive terms that remain in the model must have a natural zero. From the variable descriptions it should be obvious that *HOUSEP*, *SENATEP*, *HMAJOR*, and *SMAJOR* each have a natural zero.

Table 1.2: Base Line Regression Results

	(1) *TOTAL*	(2) *PROCURE*	(3) *GRANTS*
LAGGED SPENDING	0.632***	0.506***	0.489***
	(0.0691)	(0.0396)	(0.0694)
$\Delta INCOME$	0.0314	0.502	0.687**
	(0.171)	(0.669)	(0.307)
$\Delta ELDERLY$	0.0213	0.0991	0.0321
	(0.0291)	(0.0751)	(0.0619)
UNEMPLOY	-0.00177	-0.0101	0.0225
	(0.0127)	(0.0577)	(0.0254)
LANDAREA	0.0773***	0.308***	0.0511
	(0.0241)	(0.0996)	(0.0607)
$\Delta SENATE$	0.00529	0.0886	0.00972
	(0.00975)	(0.0760)	(0.0154)
GOVP	0.000752	0.0192*	-0.00772*
	(0.00251)	(0.0112)	(0.00436)
HOUSEP	-0.000191	-0.00588	0.0294**
	(0.00533)	(0.0190)	(0.0114)
SENATEP	0.00332*	0.00322	0.00710*
	(0.00181)	(0.00870)	(0.00376)
HMAJOR	0.00493	0.0215	0.0187
	(0.00656)	(0.0264)	(0.0179)
SMAJOR	0.00287	-0.00137	0.00969**
	(0.00181)	(0.00697)	(0.00392)
MARGIN	-0.0201	-0.217	0.0219
	(0.0407)	(0.245)	(0.0819)
VOTE	-0.00775*	-0.0332	0.00264
	(0.00418)	(0.0198)	(0.00932)
MARGINVOTE	0.0658*	0.270	0.0228
	(0.0389)	(0.175)	(0.0818)
HTENURE	-0.00147	0.00467	-0.00387
	(0.00372)	(0.0138)	(0.00674)
STENURE	0.000691	0.00473	0.00431
	(0.00232)	(0.0157)	(0.00398)
INTERCEPT	0.284***	0.190*	0.391***
	(0.0616)	(0.109)	(0.107)
Observations	1350	1350	1350
Adjusted R^2	0.926	0.866	0.911

Cluster robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$. All regressions include state fixed-effects. The variables *TOTAL*, *PROCURE*, *GRANTS*, *INCOME*, *ELDERLY*, *UNEMPLOY*, *LANDAREA*, and *SENATE* are normalized according to equation 1.1. Δ represents the first difference of a variable. Full regression results available from the authors upon request.

or closing of military bases or other government facilities are political as well. However, once these programs are decided upon, year-on-year payments related to such programs typically remain fairly persistent. Thus, we are unlikely to observe correlations between the contemporaneous political affiliations of a state's federal legislators and its share of per capita retirement and federal wage payments.[19] For these reasons, the *RETIRE* and *WAGES* categories will not be discussed in this paper. In unreported regression results concerning the *OTHER* spending category, we were unable to reject the null hypothesis that the individual regressors were jointly equal to zero. This was also true for subsequent models that will include the interaction terms.[20] Thus, the *OTHER* category will be omitted from the remaining analysis as well.

The base line results clearly show that including a lag of the dependent variable is appropriate for each spending category. In each regression the lagged term is positive and statistically significant at the .01 level. Column 1 shows that the variables *SENATEP*, *VOTE*, and *MARGINVOTE* are all statistically significant at the .10 level, thus providing weak evidence that the distribution of total spending per capita is influenced by presidential politics. The estimated coefficient on *SENATEP* implies that a state with an additional senator in the party of the president can expect its share of total spending per capita to increase by .33 percentage points relative to the average. The estimated coefficient on *VOTE* is negative and, together with the interaction term *MARGINVOTE*, implies that a state the president won by a narrow margin receives a lower share of spending than a state the president won by a larger margin. This finding supports the conclusion reached by Larcinese et al. (2006), which was that states that supported the president by larger margins were rewarded relative to states the president narrowly won.

The only control variable that is statistically significant in the baseline *TOTAL* regression

[19]These assumptions were confirmed in unreported regression results that are available upon request.
[20]These results are available upon request.

is *LANDAREA*, which has a point estimate equal to 0.0773 and is statistically significant at the .01 level. Recall that most of the control variables are differenced in order to be made stationary. This could be a reason that many of the control variables are not statistically significant.

The variable *GOVP* is the only statistically significant political variable in the *PROCURE* regression. These results are shown in Column 2. The estimated coefficient, which is statistically significant at the .10 level, implies that a state where the governor is in the same party as the president can expect its share of procurement spending per capita to increase by 1.92 percentage points relative to the national average. Similar to the *TOTAL* regression *LANDAREA* remains the only statistically significant control variable. The point estimate is equal to 0.308 and is statistically significant at the .01 level.

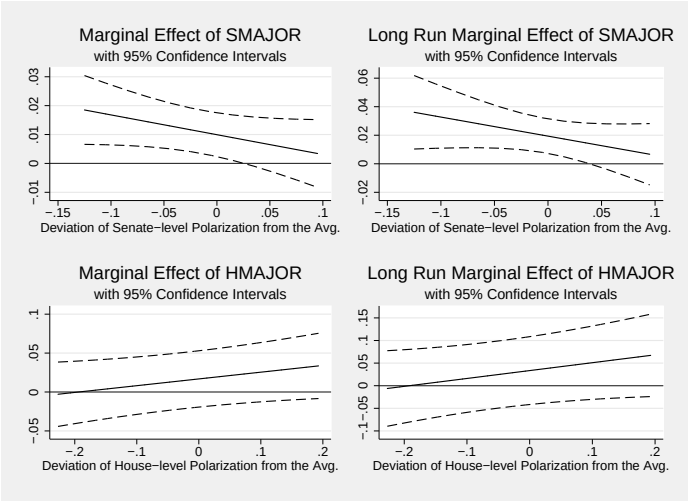
The results in Column 3 show that the distribution of federal grant money is significantly impacted by several of the political relationships. This finding is in line with our initial expectations and much of the previous literature. The coefficient on *HOUSEP* is positive and statistically significant, with a p-value of .013. This estimate suggests that if the percentage of a state's House delegation that belongs to the party of the president increases from 0% to 100%, then per capita grant spending received by the state will increase by a little less than 3 percentage points relative to the national average. The variable *SENATEP* is also positive but is only statistically significant at the .10 level. Thus, each additional senator who belongs to the party of the president is associated with an increase in per capita grant spending of approximately .71 percentage points above the average spending share. The variable *HMAJOR* is not statistically different from zero. However, the point estimate on *SMAJOR* is positive and has a p-value of .017, implying that each senator in the majority is associated with an increase in per capita grant spending of a little less than 1 percentage point above the national average. Interestingly, the variable *GOVP* is negative and statistically significant at the .10 level in this regression. This result is contrary to what we should expect

and there is not a good reason for why this result should hold.

Determinants of Federal Grant Expenditure

Table 1.3 reports regression results from the three specifications that control for the impact of polarization and the different types of alignment on the distribution of federal grant expenditures. Column 1 reports results from the specification that controls for political polarization while columns 2 and 3 report results from those that control for alignment between the president and Congress and between the House and the Senate, respectively. In column 1, the estimated coefficient on *SMAJOR* is statistically significant with a p-value of .011. Thus, at the average level of political polarization ($SPOLAR = .69$) a state with an additional senator in the chamber majority can expect an additional .994 percentage points more in per capita grant spending relative to the US average. Over the long-run, this state could expect an additional 1.94 percentage points more in per capita grant spending relative to the average.

Figure 1.1: Short and Long-Run Effects of *SMAJOR* and *HMAJOR*



The top row of Figure 1.1 shows the short- and long-run marginal effects of *SMAJOR* conditional upon chamber-specific political polarization. These marginal effects are presented

Table 1.3: Partial Regression Results for GRANTS

	(1) *GRANTS*	(2) *GRANTS*	(3) *GRANTS*
LAGGED SPENDING	0.487***	0.487***	0.487***
	(0.0684)	(0.0677)	(0.0694)
GOVP	-0.00796*	-0.00715	-0.00782*
	(0.00425)	(0.00453)	(0.00429)
HOUSEP	0.0245**	0.0273*	0.0295**
	(0.00994)	(0.0138)	(0.0114)
SENATEP	0.00715*	0.0114**	0.00681*
	(0.00368)	(0.00491)	(0.00381)
$HOUSEP * ALIGNP$		0.0128	
		(0.0302)	
$SENATEP * ALIGNP$		-0.0112	
		(0.0119)	
HMAJOR	0.0168	0.0169	0.00830
	(0.0180)	(0.0236)	(0.0242)
SMAJOR	0.00994**	0.0133***	0.0108*
	(0.00377)	(0.00481)	(0.00565)
$HMAJOR * ALIGNC$			0.0149
			(0.0165)
$SMAJOR * ALIGNC$			-0.00254
			(0.00702)
$HMAJOR * HPOLAR_{dev}$	0.0867*		
	(0.0491)		
$SMAJOR * SPOLAR_{dev}$	-0.0685		
	(0.0413)		
MARGIN	0.0129	0.0215	0.0215
	(0.0819)	(0.0829)	(0.0834)
VOTE	0.00340	0.00298	0.00307
	(0.00936)	(0.00904)	(0.00917)
MARGINVOTE	0.0292	0.0183	0.0283
	(0.0807)	(0.0825)	(0.0833)
INTERCEPT	0.399***	0.388***	0.393***
	(0.105)	(0.109)	(0.109)
Observations	1350	1350	1350
Adjusted R^2	0.911	0.911	0.911

Cluster robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$. All regressions include state fixed-effects. The variable *GRANTS* is normalized according to equation 1.1. Full regression results available from the authors upon request.

along with ninety-five percent confidence intervals. For each panel of Figure 1.1, the far left corner of the horizontal axis indicates the lowest level of political polarization while the far right corner indicates the highest and most recent levels of political polarization. This figure reveals an interesting and unexpected finding. The marginal effect of *SMAJOR* is highest at the lowest level of political polarization and decreases as the level of political polarization increases. In fact, for most levels of political polarization above the average the marginal effect of *SMAJOR* is not statistically different from zero. However, at the lowest level of political polarization an additional senator in the majority is worth approximately 1.82 percentage points in additional grant spending per capita relative to the average share. This estimate is statistically significant at the .01 level. Over the long-run an additional senator in the majority is worth an additional 3.54 percentage points in per capita grant expenditures relative to the average when political polarization in the Senate is at its lowest level (p-value = .006).

Column 1 of Table 1.3 also shows that *HMAJOR* is not statistically different from zero. The bottom row of Figure 1.1 plots the short- and long-run marginal effects of *HMAJOR* for all levels of House-level political polarization that are observed over the sample period with ninety-five percent confidence intervals. The marginal effect of *HMAJOR* is never statistically different from zero at the .05 level for any level of political polarization. At the highest level of political polarization in the House ($HPOLAR = 1$) a state that goes from having 0% to 100% of its House delegation in the chamber majority can expect an additional 3.50 percentage points in per capita grant spending relative to the US average, however, the p-value associated with this estimate is only .107. Over the long run this amount increases to 6.83 percentage points in additional grant spending per capita relative to the average but again, this estimate is not statistically different from zero.

Column 2 presents results from the regression specification measuring how political alignment between the executive and legislative branches impacts *HOUSEP* and *SENATEP*. The

marginal effect on *GRANTS* for each of these variables is presented in Table 1.4. This table shows that when the government is divided ($ALIGNP = 0$) a state that goes from 0% to 100% of its House delegation in the party of the president can expect its share of grant expenditures per capita to increase by 2.73 percentage points relative to the average share. The p-value associated with this marginal effect is .055. Over the long-run, a state that goes from 0% to 100% of its House delegation in the party of the president can expect grant expenditures per capita to increase by approximately 5.32 percentage points relative to the average share when the government is divided. When these branches are controlled by the same party the marginal effect of *HOUSEP* is marginally significant at the .10 level with a p-value of .103. Specifically, when the elected branches of government are aligned a state that goes from 0% to 100% of its House delegation in the party of the president can expect its share of per capita grant expenditures to increase by 4.00 percentage points relative to the average share. The long-run marginal effect of going from 0% to 100% of the House delegation in the party of the president during periods of complete alignment is an additional 7.80 percentage points in additional grant spending per capita relative to the average share. This point estimate, however, is not statistically different from zero.

The marginal effect of *SENATEP* is computed in a similar way. When $ALIGNP = 0$, having an additional senator in the president's party is worth approximately 1.14 percentage points in addition grant spending per capita relative to the average spending share. This coefficient is significantly different from zero at the .05 level. Over the long-run an additional senator in the party of the president is worth 2.20 percentage points in additional grant spending relative to the average. However, when $ALIGNP = 1$ both the short- and long-run marginal effects of *SENATEP* are no longer statistically different from zero. The point estimates are also relatively small, with $\frac{\partial(GRANTS_{it}|ALIGNP = 1)}{\partial SENATEP_{it}} = .0001$ for the short-run estimate and .0003 over the long-run.

Table 1.4: Short and Long-Run Marginal Effects of *HOUSEP*, *SENATEP*, *HMAJOR*, and *SMAJOR*

Dependent Variable		Political Variable	*ALIGNP*=0	*ALIGNP*=1		*Political Variable*	*ALIGNC*=0	*ALIGNC*=1
GRANTS	Short-run	*HOUSEP*	0.0273*	0.0400	Short-run	*HMAJOR*	0.0083	0.0232
			(0.014)	(0.024)			(0.024)	(0.017)
	Long-run		0.0532*	0.078	Long-run		0.0162	0.0452
			(0.028)	(0.049)			(0.048)	(0.036)
	Short-run	*SENATEP*	0.011**	0.0001	Short-run	*SMAJOR*	0.0108*	0.0083*
			(0.0057)	(0.0047)			(0.0057)	(0.004)
	Long-run		0.022**	0.0003	Long-run		0.0201*	0.0154**
			(0.009)	(0.018)			(0.0107)	(0.008)
TOTAL	Short-run	*HOUSEP*	-0.00274	0.0054	Short-run	*HMAJOR*	0.00135	0.0062
			(0.0072)	(0.0113)			(0.0116)	(0.0055)
	Long-run		-0.00074	0.0147	Long-run		0.0037	0.0167
			(0.0195)	(0.0298)			(0.0317)	(0.0157)
	Short-run	*SENATEP*	0.00496	0.0004	Short-run	*SMAJOR*	0.0028	0.0026
			(0.0036)	(0.0046)			(0.0035)	(0.0023)
	Long-run		0.0135	0.0069	Long-run		0.0075	0.0069
			(0.0085)	(0.0126)			(0.0095)	(0.006)
PROCURE	Short-run	*HOUSEP*	0.0031	-0.0224	Short-run	*HMAJOR*	0.0316	0.0113
			(0.0305)	(0.0482)			(0.0456)	(0.0259)
	Long-run		0.0063	-0.0452	Long-run		0.0639	0.0229
			(0.0616)	(0.0977)			(0.0927)	(0.0526)
	Short-run	*SENATEP*	-0.001	0.011	Short-run	*SMAJOR*	-0.012	0.0034
			(0.0191)	(0.0229)			(0.0219)	(0.0088)
	Long-run		-0.0019	0.0225	Long-run		-0.0235	0.0068
			(0.0385)	(0.0459)			(0.0440)	(0.0177)

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. Standard errors for long run estimates computed using the delta method. The variables *GRANTS*, *TOTAL*, and *PROCURE* are normalized according to equation 1.1.

As in the baseline specification, the variable *HMAJOR* never appears to be a statistically significant determinant of per capita federal grant expenditures, regardless of whether or not the House and Senate are controlled by the same party. However, the variable *SMAJOR* is statistically significant. As shown in Table 1.4, when control of Congress is divided between the two parties an additional senator in the majority is worth approximately 1.08 percentage points in additional grant spending per capita relative to the average share. Interestingly, when Congress is aligned an additional senator in the majority is worth only .83 percentage points in per capita grant expenditures relative to the average share. In other words, while an additional senator in the majority is still worth more relative to the average, the amount is lower during periods of congressional alignment.

Determinants of Total and Procurement Expenditure

Tables 1.5 and 1.6 show regression results for the *TOTAL* and *PROCURE* categories, respectively. Table 1.5 shows some evidence that an additional senator in the party of the president is worth more total spending per capita relative to the average spending share. In both columns 1 and 3 the point estimate on *SENATEP* is 0.00324 and .00325, respectively, and is statistically significant at the .10 level.

Thus, an additional senator in the party of the president is worth approximately .33 percentage points more in total spending per capita relative to the average share. However, as Table 1.4 shows, when *SENATEP* and *HOUSEP* are conditional upon alignment between the presidency and Congress the point estimate on *SENATEP* is no longer statistically different from zero. Neither *HMAJOR* nor *HOUSEP* are shown to be statistically significant determinants of total spending per capita.

Still, some political variables continue to be statistically significant determinants of how total federal spending is distributed to the states. Specifically, the variables *VOTE* and *MARGINVOTE* maintain consistent point estimates across the three specifications shown

Table 1.5: Partial Regression Results for *TOTAL*

	(1) *TOTAL*	(2) *TOTAL*	(3) *TOTAL*
LAGGED SPENDING	0.632***	0.632***	0.632***
	(0.0700)	(0.0692)	(0.0696)
GOVP	0.000555	0.000796	0.000699
	(0.00240)	(0.00247)	(0.00247)
HOUSEP	-0.00188	-0.00274	-0.000235
	(0.00519)	(0.00720)	(0.00531)
SENATEP	0.00324*	0.00496	0.00325*
	(0.00180)	(0.00335)	(0.00183)
$HOUSEP * ALIGNP$		0.00813	
		(0.0148)	
$SENATEP * ALIGNP$		-0.00460	
		(0.00718)	
HMAJOR	0.00393	0.00293	0.00135
	(0.00698)	(0.00925)	(0.0116)
SMAJOR	0.00279	0.00426	0.00277
	(0.00170)	(0.00271)	(0.00354)
$HMAJOR * ALIGNC$			0.00480
			(0.0102)
$SMAJOR * ALIGNC$			-0.000223
			(0.00464)
$HMAJOR * HPOLAR_{dev}$	0.0180		
	(0.0348)		
$SMAJOR * SPOLAR_{dev}$	-0.00418		
	(0.0290)		
MARGIN	-0.0243	-0.0212	-0.0192
	(0.0415)	(0.0413)	(0.0416)
VOTE	-0.00736*	-0.00751*	-0.00765*
	(0.00417)	(0.00417)	(0.00426)
MARGINVOTE	0.0701*	0.0656	0.0676*
	(0.0398)	(0.0395)	(0.0394)
INTERCEPT	0.287***	0.284***	0.285***
	(0.0641)	(0.0629)	(0.0622)
Observations	1350	1350	1350
Adjusted R^2	0.926	0.926	0.926

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. All regressions include state fixed-effects. The variable *TOTAL* is normalized according to equation 1.1. Full regression results available from the authors upon request.

in Table 1.5 with the point estimate on *VOTE* ranging between -0.00765 and -0.00736 and the point estimate on *MARGINVOTE* ranging between 0.0656 and 0.0701. All point estimates are either statistically significant at the .10 level, as in columns 1 and 3, or marginally significant at the .10 level, as in column 2. Thus, we continue to show some evidence that

Table 1.6: Partial Regression Results for *PROCURE*

	(1) *PROCURE*	(2) *PROCURE*	(3) *PROCURE*
LAGGED SPENDING	0.505***	0.505***	0.505***
	(0.0395)	(0.0396)	(0.0398)
GOVP	0.0194*	0.0194*	0.0189*
	(0.0108)	(0.0114)	(0.0110)
HOUSEP	0.00160	0.00313	-0.00732
	(0.0151)	(0.0305)	(0.0184)
SENATEP	0.00309	-0.000959	0.00432
	(0.00887)	(0.0191)	(0.00815)
$HOUSEP * ALIGNP$		-0.0255	
		(0.0688)	
$SENATEP * ALIGNP$		0.0121	
		(0.0388)	
HMAJOR	0.0239	0.0286	0.0316
	(0.0300)	(0.0356)	(0.0456)
SMAJOR	-0.00194	-0.00489	-0.0116
	(0.00738)	(0.0152)	(0.0219)
$HMAJOR * ALIGNC$			-0.0203
			(0.0418)
$SMAJOR * ALIGNC$			0.0150
			(0.0267)
$HMAJOR * HPOLAR_{dev}$	-0.146		
	(0.145)		
$SMAJOR * SPOLAR_{dev}$	0.130		
	(0.147)		
MARGIN	-0.205	-0.212	-0.197
	(0.246)	(0.240)	(0.229)
VOTE	-0.0341*	-0.0340	-0.0345
	(0.0201)	(0.0204)	(0.0206)
MARGINVOTE	0.264	0.269	0.263
	(0.173)	(0.176)	(0.169)
INTERCEPT	0.180	0.190	0.191*
	(0.113)	(0.115)	(0.110)
Observations	1350	1350	1350
Adjusted R^2	0.866	0.866	0.866

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. All regressions include state fixed-effects. The variable *PROCURE* is normalized according to equation 1.1. Full regression results available from the authors upon request.

states the sitting president won by a narrow margin receive less spending per capita relative to states the president won by a relatively larger margin.

The variable *GOVP* remains the only political variable that is a statistically significant across all three regression specifications. Table 1.6 shows that the point estimates range from 0.0189 in column 3 to 0.0194 in columns 1 and 2. All are statistically significant at the .10 level.

V Robustness of Senate-Level Determinants of Federal Grant Expenditure

In the preceding discussion the variables *SENATEP* and *SMAJOR* are count variables that take on values of 0, 1, or 2, depending upon the number of senators a state has that satisfies the respective variable definitions. However, it is possible that going from one to two senators in either the majority or the party of the president has an impact on a state's distribution of federal spending that is different from the impact that going from 0 to 1 senator in either the party of the president or the majority might have. In this section a series of dummy variables are used to separately identify these effects.[21] Replacing the variable *SENATEP* will be the dummy variables *SENATEP1* and *SENATEP2*.[22] *SENATEP1* equals 1 if a state has one senator in the party of the president and equals zero otherwise. The variable *SENATEP2* equals 1 if a state has two senators in the party of the president, otherwise, the variable equals zero. When both of these variables are equal to zero a state has no senators who share a party affiliation with the president. The variable *SMAJOR* will be replaced by the

[21] In this section, analysis is limited to how federal grant expenditures are affected. This is done both because Tables 1.4, 1.5, and 1.6 revealed no statistically significant relationships between the senator variables and total and procurement spending and also to conserve space. In unreported results, the analysis that is conducted in this section was conducted for total and procurement spending and statistically significant relationships between these spending variables and senator-level variables still could not be verified. These results are available upon request.

[22] Obviously, a similar strategy cannot be extended to the number of House members in the party of the president or in the majority since the number of dummy variables that would be required would surpass the available degrees of freedom.

dummy variables *SMAJOR1* and *SMAJOR2*, which have a similar interpretation.

Table 1.7: Robustness of Senate-Level Variables

	(1) *GRANTS*	(2) *GRANTS*	(3) *GRANTS*
LAGGED SPENDING	0.484***	0.486***	0.487***
	(0.0683)	(0.0672)	(0.0690)
SENATEP1		0.0109	
		(0.00940)	
SENATEP2		0.0244**	
		(0.0103)	
$SENATEP1 * ALIGNP$		0.00578	
		(0.0146)	
$SENATEP2 * ALIGNP$		-0.0241	
		(0.0242)	
SMAJOR1	0.0145		0.00641
	(0.0115)		(0.0125)
SMAJOR2	0.0205**		0.0243**
	(0.00785)		(0.0119)
$SMAJOR1 * ALIGNC$			0.00844
			(0.0124)
$SMAJOR2 * ALIGNC$			-0.00759
			(0.0144)
$SMAJOR1 * SPOLAR_{dev}$	0.0490		
	(0.102)		
$SMAJOR2 * SPOLAR_{dev}$	-0.147*		
	(0.0841)		
Observations	1350	1350	1350
Adjusted R^2	0.911	0.911	0.911

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. All regressions include state fixed-effects. The variable *GRANTS* is normalized according to equation 1.1. Full regression results available from the authors upon request.

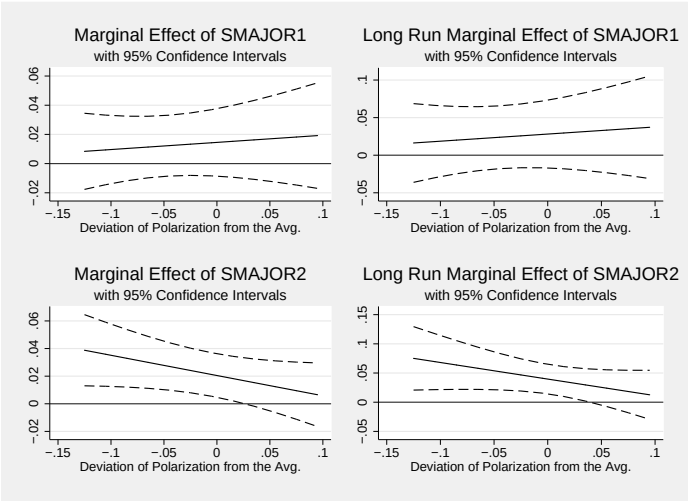
It should be noted that the variables *SENATEP1*, *SENATEP2*, *SMAJOR1*, and *SMAJOR2* can never be included in the same regression because the variables *SENATEP1* and *SMAJOR1* would be perfectly collinear with one another.[23] For this reason whenever *SENATEP1* and *SENATEP2* are included in the regression specification the variable *SMAJOR* will be included to capture the effect of the number of senators in the majority. Similarly, when *SMAJOR1* and *SMAJOR2* are included the number of senators in the party of the president will continue to be captured by the variable *SENATEP*.

[23] If a state has one Republican and one Democratic senator then *SENATEP1* and *SMAJOR1* would both equal 1 regardless of which party controlled the presidency and Congress.

Partial regression results for specifications that include these dummy variables are presented in Table 1.7. It is immediately clear that the biggest impact on spending is associated with going from zero to two senators in either the majority or the party of the president. When controlling for Senate-level political polarization, results of which are shown in column 1, only the point estimate on *SMAJOR2* is statistically significant (p-value = .012) when Senate-level political polarization is at the sample average. Thus, going from zero to two senators in the majority is worth slightly more than 2 percentage points in additional grant spending per capita relative to the US average. The left column of Figure 1.2 plots the marginal effects of *SMAJOR1* and *SMAJOR2*, respectively, conditional on political polarization. While the slope of *SMAJOR1* is actually positive it is never statistically different from zero at a .05 level. However, the marginal effect of *SMAJOR2* remains statistically significant at the .05 level or better for levels of political polarization past the average (*SPOLAR=0.69*). It is also the case that the marginal effect of *SMAJOR2* is decreasing in political polarization similar to what was observed in Figure 1.1. At the lowest level of political polarization going from zero to two senators in the majority is worth an additional 3.81 percentage points in additional grant spending per capita relative to the average spending share. This marginal effect is statistically significant at the .01 level. We cannot uncover a statistically significant difference between *SMAJOR1* and *SMAJOR2* at the average level of political polarization. However at the lowest level of political polarization *SMAJOR1* and *SMAJOR2* are statistically different at the .05 level and the point estimates are .0089 and .0381, respectively. Thus, at the lowest level of political polarization, a state that goes from having one to two senators in the majority can expect an additional 2.92 percentage points more in grant spending relative to the average spending share.

When control of the executive and legislative branches is divided between the two parties, going from zero to one senator in the party of the president cannot be shown to have a statistically significant impact on a state's distribution of federal grant spending. However,

Figure 1.2: Short and Long-Run Effects of *SMAJOR1* and *SMAJOR2*



as column 2 shows, going from zero to two senators in the party of the president is worth 2.44 percentage points in additional grant spending per capita relative to the average during periods of divided government. An F-test shows that when $ALIGNP = 0$ the point estimates associated with *SENATEP1* and *SENATEP2* are statistically different at the .10 level. However, during periods of alignment between the president and Congress the null hypothesis that the marginal effects associated with these variables were equal could not be rejected.

When $ALIGNC = 0$, a state that goes from having zero to two senators in the majority can expect an additional 2.43 percentage points in additional grant spending relative to the average spending share. This point estimate is statistically significant at the .05 level. When both the House and the Senate are controlled by the same party going from zero to two senators in the majority is only worth 1.67 percentage points in additional grant spending relative to the US average. This marginal effect is statistically significant at the .10 level. Lastly, there does not appear to be a statistically significant effect of going from one to two senators in the majority regardless of whether or not Congress is controlled by the same party.

Table 1.8: Marginal Effects of *SENATEP1*, *SENATEP2*,*SMAJOR1*, and *SMAJOR1* on *GRANTS*

Dependent Variable		Political Variable	*ALIGNP=0*	*ALIGNP=1*		Political Variable	*ALIGNC=0*	*ALIGNC=1*
GRANTS	Short-run	*SENATEP1*	0.0109	0.0167	Short-run	*SMAJOR1*	0.0064	0.0148
			(0.0094)	(0.0183)			(0.0125)	(0.0117)
	Long-run		0.0212	0.0325	Long-run		0.0125	0.0289
			(0.0187)	(0.0360)			(0.0244)	(0.0232)
	Short-run	*SENATEP2*	0.0244**	0.0003	Short-run	*SMAJOR2*	0.0243**	0.0167*
			(0.0103)	(0.0180)			(0.0119)	(0.0094)
	Long-run		0.0475**	0.0006	Long-run		0.0473**	0.0325**
			(0.0199)	(0.0350)			(0.0225)	(0.0159)

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. Standard errors for long run estimates computed using the delta method. The variable *GRANTS* is normalized according to equation 1.1.

VI Discussion

The strongest results show that having the entire Senate delegation in the majority is worth a larger share of per capita grant spending when polarization in the Senate is relatively low. When political polarization is the Senate is higher than average, having both senators in the majority does not appear to have an economically or statistically significant impact on a state's share of per capita grant spending. During periods of low political polarization senators in the majority might feel less safe. If both parties are closer ideologically, then challenges from the opposing party might be viewed with more concern. Thus, the majority will award these states with a larger share in order to ensure that their Senate seats remain under the majority party's control. Conversely, during periods of high political polarization, when the parties are farther from one another ideologically, a state with both senators in the majority might be viewed as having relatively safe seats and there is less of a need to increase its share of grant spending.

Having an additional senator in the majority is worth a smaller spending share when the House and Senate are controlled by the same party compared to periods of divided control. Again, robustness checks show that states with both senators in the majority are driving this result.

States with more House or Senate members in the party of the president are able to secure larger than average shares of grant spending per capita when the executive and legislative branches are controlled by different parties. During periods when the presidency and Congress were aligned only the variable *HOUSEP* showed some marginal statistical significance. In this case, the point estimate on *HOUSEP* was larger than periods of divided control between the presidency and Congress. Lastly, there is weak evidence that at the highest level of political polarization in the House going from 0% to 100% of the House delegation in the majority is worth an additional 3.50 percentage points in grant spending per

capita relative to the average share. However, recall that Table 1.2 showed that the unconditional point estimate on *HMAJOR* was not statistically different from zero. Furthermore, the point estimate on *HMAJOR* when conditional upon inter-chamber alignment was never statistically significant either. Therefore, this result may be spurious.

VII Conclusion

Using data on the fifty states from 1983 – 2010 we provide new evidence that political polarization and political alignment influence the distribution of federal grant expenditures to the states. While it is no surprise that federal grant spending is influenced by politics it is interesting that the most robust results point towards *GRANTS* as the only category where our political variables play a role, given that some research has found political variables to influence procurement spending as well.

In this paper we have accounted for several econometric issues that have received little attention in the literature on distributive politics. Correcting these issues has a nontrivial impact on the results. For one, unit root tests show that several variables common to this literature are non-stationary. We transform these variables so that all regressions contain variables integrated of the same order. With longer and richer data sets becoming available, it is important to make the appropriate adjustments to the data to help ensure meaningful estimation results. We also employ dynamic panel estimation to control for the persistent nature of federal expenditures to the states. In doing so we are able to identify both short- and long-run impacts of political determinants of federal spending.

In one respect, the results presented here confirm previous research showing that states with more legislators in the party of the president or the chamber majority receive larger shares of per capita spending. However, we also show that the impacts of these determinants are conditional upon the political environment; having a larger share of the House delegation

aligned with the president is worth more to a state when the same party also controls both chambers of Congress. Also, having more senators in the chamber majority is associated with a larger than average spending share at relatively low levels of political polarization. As political polarization in the Senate increases, another senator in the majority is not worth as much to a state. We also find evidence that having more representatives in the House majority is worth more when political polarization in the House is relatively high. However, this evidence is much weaker and does not appear to persist over the long-run.

The findings associated with the Senate were unexpected. In the *GRANTS* regressions, the point estimate on *SENATEP* decreases when the executive and legislative branches are aligned compared to when they are controlled by different parties. Similarly, the point estimate on *SMAJOR* declines when the composition of Congress changes from divided control to unified control and when political polarization increases. The behavior of these variables is counter to what we observe at the House level. It is difficult to state conclusively why the Senate is different; however, certain institutional differences between the House and the Senate could be relevant factors. Some obvious differences are the relative sizes of the two chambers – 100 members in the Senate, 435 members in the House –, constituencies – statewide versus smaller districts –, the existence of the filibuster in the Senate, and different term lengths and election cycles. Also, we are not the first to notice a difference between the Senate and the House. Crain and Tollison (1977), Levitt and Poterba (1999), Hoover and Pecorino (2005), Larcinese et al. (2006), Albouy (2013), and Young and Sobel (2013) all find that Senate-level variables impacted federal spending per capita – or economic growth in the case of Levitt and Poterba (1999) – differently than House-level variables.

Over the course of the sample period, budgetary matters have generally been immune to the filibuster. However, the use of the filibuster in general debate has steadily increased. In fact, Stanley Bach, a former legislative specialist at the Congressional Research Service, has stated that "at one time, filibusters generally were reserved for matters of obvious national

importance .... Today, by stark contrast, filibusters ... have become almost a routine part of the Senate's floor procedures."[24] When polarization is relatively low, perhaps the Senate's smaller size makes logrolling easier, whereas at relatively high levels of polarization, threats of a filibuster on non-budgetary matters are a concern.

Differences in constituencies could help explain why the results appear to show that senators in the party of the president and in the majority are worth less during periods of political alignment between the president and Congress and within Congress, respectively. Presidents may feel it is in their best interest to target House members who have closer relationships with the voters since they have smaller more geographically concentrated constituencies. In periods where the president's party also controls Congress, this type of targeting may be more easily achieved.

Overall, we show that some important determinants of how spending is distributed are not unconditional. Future research should explore how alignment and polarization impact other aspects of these determinants. For example, does a stronger congressional majority allow members of the House majority to secure even larger shares of federal spending? Is one party better able to exploit political alignment than the other? Is one party better able to procure higher spending shares during periods of relatively high or low political polarization? The findings here provide a foundation for answering these questions.

[24]Statement to the Senate Subcommittee on Rules and Administration Hearing *Examining the Filibuster: History of the Filibuster 1789 – 2008* on April 22, 2010. http://www.rules.senate.gov/public/index.cfm?a=Files.Serve&File_id=25f59865-abbd-4aa9-80aa-c6ce36e08ad7.

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I Treatment of Party Switches, Resignations, and Deaths

Occasional mid-year party switches, resignations, and deaths mean that some legislative districts and Senate positions were held by more than one political party within a given year. In cases such as these, we assign control of the district or Senate position to the party of the representative or senator who was present for the most votes. Roll call data for various years compiled by Keith Poole and Howard Rosenthal were used to tally the number of votes each representative or senator took while in office.[25] In total, there were three party switches in the Senate and twelve in the House. There were also fourteen mid-year resignations or deaths in the House and eleven in the Senate. However, some of these switches or early exits were late enough in the year that the district or Senate position was coded in favor of the party of the initial legislator.

[25] Poole and Rosenthal make this data available at http://www.pooleandrosenthal.com/downloads.asp#PARTYSPLITSDWNL

II Unit Root Tests

Table A1: Panel Unit Root Tests on the Per Capita Dependent Variables

Method	Variable	Statistics	P-Value
Levin et al. (2002) $t*$	*TOTAL*	-0.32	0.37
Null: Unit Root (common process)	*RETIRE*	2.35	0.99
Alt: All Panels Stationary	*WAGES*	8.03	1.00
	GRANTS	-0.28	0.39
	PROCURE	-3.99	0.00
	OTHER	0.86	0.80
Im et al. (2003) W-stat	*TOTAL*	1.04	0.85
Null: Unit Root (individual process)	*RETIRE*	7.18	1.00
Alt: Some Panels Stationary	*WAGES*	2.54	1.00
	GRANTS	0.61	0.73
	PROCURE	-4.36	0.00
	OTHER	0.12	0.55
ADF Fisher-Type Test Chi-Square	*TOTAL*	0.16	0.43
Null: Unit Root (individual process)	*RETIRE*	-3.27	0.99
Alt: At Least One Panel is Stationary	*WAGES*	-3.90	1.00
	GRANTS	2.64	0.00
	PROCURE	8.15	0.00
	OTHER	0.98	0.16
PP Fisher-Type Test Chi-Square	*TOTAL*	4.59	0.00
Null: Unit Root (individual process)	*RETIRE*	-2.93	0.99
Alt: At Least One Panel is Stationary	*WAGES*	-4.43	1.00
	GRANTS	7.98	0.00
	PROCURE	16.71	0.00
	OTHER	8.31	0.00

Dependent variable descriptions provided in Section III. Fisher-type test probabilities computed from an asymptotic Chi-square distribution. Probabilities for other tests assume asymptotic normality. Presentation of unit root test results taken from de Haan et al. (2012).

Table A2: Panel Unit Root Tests on the Normalized Dependent Variables

Method	Variable	Statistics	P-Value
Levin et al. (2002) $t*$	*TOTAL*	-4.76	0.00
Null: Unit Root (common process)	*RETIRE*	-2.24	0.01
Alt: All Panels Stationary	*WAGES*	4.67	0.00
	GRANTS	-5.34	0.00
	PROCURE	-5.95	0.00
	OTHER	-3.84	0.80
Im et al. (2003) W-stat	*TOTAL*	-3.61	0.00
Null: Unit Root (individual process)	*RETIRE*	3.08	0.99
Alt: Some Panels Stationary	*WAGES*	5.74	1.00
	GRANTS	-5.42	0.00
	PROCURE	-5.44	0.00
	OTHER	-4.64	0.00
ADF Fisher-Type Test Chi-Square	*TOTAL*	5.07	0.00
Null: Unit Root (individual process)	*RETIRE*	-1.61	0.95
Alt: At Least One Panel is Stationary	*WAGES*	-3.35	0.99
	GRANTS	6.51	0.00
	PROCURE	-0.15	0.00
	OTHER	7.46	0.00
PP Fisher-Type Test Chi-Square	*TOTAL*	9.55	0.00
Null: Unit Root (individual process)	*RETIRE*	-1.35	0.91
Alt: At Least One Panel is Stationary	*WAGES*	-2.88	0.99
	GRANTS	16.89	0.00
	PROCURE	19.02	0.00
	OTHER	16.58	0.00

Dependent variable descriptions provided in Section III. Fisher-type test probabilities computed from an asymptotic Chi-square distribution. Probabilities for other tests assume asymptotic normality. Presentation of unit root test results taken from de Haan et al. (2012).

Table A3: Panel Unit Root Tests on the Independent Variables

Method	Variable	Statistics	P-Value
Levin et al. (2002) $t*$	*INCOME*	-0.85	0.20
Null: Unit Root (common process)	*ELDERLY*	-1.60	0.05
Alt: All Panels Stationary	*UNEMPLOY*	-5.14	0.00
	LANDAREA	-3.67	0.00
	SENATE	-4.41	0.00
Im et al. (2003) W-stat	*INCOME*	-0.92	0.18
Null: Unit Root (individual process)	*ELDERLY*	2.27	0.98
Alt: Some Panels Stationary	*UNEMPLOY*	-4.48	0.00
	LANDAREA	2.86	0.99
	SENATE	1.84	0.87
ADF Fisher-Type Test Chi-Square	*INCOME*	2.01	0.02
Null: Unit Root (individual process)	*ELDERLY*	0.25	0.40
Alt: At Least One Panel is Stationary	*UNEMPLOY*	4.74	0.00
	LANDAREA	4.08	0.00
	SENATE	0.99	0.16
PP Fisher-Type Test Chi-Square	*INCOME*	2.13	0.00
Null: Unit Root (individual process)	*ELDERLY*	4.00	0.00
Alt: At Least One Panel is Stationary	*UNEMPLOY*	2.96	0.00
	LANDAREA	8.39	0.00
	SENATE	4.96	0.00

Dependent variable descriptions provided in Section III. Fisher-type test probabilities computed from an asymptotic Chi-square distribution. Probabilities for other tests assume asymptotic normality. Presentation of unit root test results taken from de Haan et al. (2012).

Chapter 2

The Relationship Between Legislature Size and Fiscal Policy: A Cross-Country Examination

I Introduction

Bradbury and Crain (2001) study the relationship between legislature size and government spending for a large panel of countries. Their work, though, remains one of the only studies that examine this relationship using cross-country data. Furthermore, they were the first to separately control for chamber size, thus allowing comparisons between unicameral and bicameral legislatures. Bradbury and Crain did not fully exploit the fact that government expenditure data is time-variant and ofter subject to a status quo bias. As Bradbury and Crain and others have noted, legislature chamber size exhibits very little change over time, which means little confidence can be placed in results from fixed-effects estimation. However, methodology developed by Blanchard and Wolfers (2000), which was subsequently applied to studies of political economy by Milesi-Ferretti, Perotti and Rostagno (2002) and Persson and Tabellini (2003), potentially offers a way to circumvent the problem posed by the invariability of legislature size.

Using this methodology will allow us to investigate two questions: 1) does chamber size

affect the response of government spending to unobserved events that are common to all countries and 2) does chamber size affect the response of government spending to country-specific events? Whereas Bradbury and Crain (2001) tested this relationship using total government expenditures as measured by Penn World Tables, we will use central government expenditures as compiled by Brender and Drazen (2005), which includes data from the International Monetary Fund's International Financial Statistics (IMF-IFS). In Section II we review the literature on the relationship between legislature size and government expenditures. We will devote considerable attention to Weingast, Shepsle and Johnsen (1981) - WSJ from this point forward - , the seminal paper in this literature, in order to convey the intuition behind the model. Moreover, since we seek to expand the cross-country investigation of this relationship, particular attention will also be paid to Bradbury and Crain (2001). Section III discusses the dataset. Section IV discusses the empirical strategy and results. Section V concludes.

II Literature Review

Tullock (1959) first modeled fiscal policy as a common pool problem. In this framework, a legislator can finance projects for his constituents from a tax base that includes contributions from all constituent bases, not just his own. The fact that the legislator will be able to pass on the majority of the financing costs can lead to an over-procurement of common pool resources. With many, or perhaps all, legislators acting in this manner, the result can be an economically inefficient level of government spending. However, Tullock also observed that this inefficiency could be reduced through a bicameral legislature, where each chamber represented different constituencies. This last point is central to the analysis by Bradbury and Crain (2001).

WSJ formally show how such inefficient levels of spending could increase as the number of

legislative districts increases. The WSJ hypothesis is formed in the context of a unicameral legislative body deciding upon an omnibus bill of distributive policy where the political state of nature is a world where complete universalism is the norm.[1] Each district is represented by only one legislator. The authors consider a distributive policy as one where the benefits from such a policy are concentrated in a specific geographic district but the costs are diffused across all districts. Thus, a new bridge or a new housing project would be considered a distributive policy but unemployment insurance or spending on Medicare would not.

WSJ first derive the efficient project size, x^E, which is found by maximizing $b(x) - c(x)$. The term $b(x)$ represents economic benefits stemming from the project to consumers within a legislative district and $c(x)$ includes project-related expenditures occurring both inside and outside of the district as well as any project-related externalities imposed upon the district. Thus, the optimal project size x^E solves $b'(x) = c'(x)$.

When decisions regarding project size are made in a legislature with n representatives WSJ assume that each legislator maximizes a political objective function that only accounts for project-related benefits accruing to voters in his or her district, the tax share faced by his or her constituents, and any negative externalities born by members in the district.[2] The legislator's political objective function becomes $b(x) - t * c(x) - e(x)$, where $0 < t < 1$ represents the district's tax rate and $e(x)$ represents negative externalities faced by the district.[3] The politically optimal project size, x^L, is such that $b'(x) = t * c'(x) + e'(x)$. As

[1] According to WSJ, universalism means legislators will attempt to secure votes in excess of the minimum required for a project to pass the legislature. When complete universalism holds, each legislator is assured their desired project size because each legislator wants all other legislators to vote for his or her project.

[2] Political benefits include not only project-related consumption benefits but also project-related expenditures that accrue to project-related expenditures that accrue to project input producers located within a representative's district. In WSJ's exposition, political benefits stemming from in-district expenditures take the form $f[c_1(x)] = c_1(x)$, where $c_1(x)$ represents in-district expenditures. However, as long as $f(\cdot) > 0$ and $f'(\cdot) > 0$ the exact functional form should not matter. In fact, WSJ note that, in reality, expenditure-related benefits will eventually exhibit diminishing returns (i.e. $f"(\cdot) < 0$). Thus, a point will be reached where additional expenditures will not be politically savvy. Regardless, as long as f and its first derivative are positive there is a range where additional expenditures will provide additional political benefits.

[3] $c(x)$ includes in-district expenditures of the type mentioned in note 2 as well as expenditures on inputs produced outside of the district. These out-of-district expenditures will benefit producers of those factors

long as additional benefits from in-district expenditures are growing at a rate faster than the district's tax share, the politically optimal project size will be greater than the economically efficient project size. Furthermore, WSJ show that if t is a decreasing function of district size, e.g. $t = (1/n)$, then the tax share facing each district is decreasing in n. $(x^L - x^E)$ is increasing in n because adding more legislators means the tax share facing each district is decreasing relative to the perceived political benefits. This result is known as the "Law of $1/n$."

The key to deriving the Law of $1/n$ result is the assumption of complete universalism. However, even assuming a universalistic norm, legislators should still account for the costs of the projects proposed by the other legislators. The project size that maximizes legislator $i's$ political net benefits might, at first glance, be found simply be considering the tax burden that is directly related to legislator $i's$ proposed project. But if securing this project means the legislator will voluntarily pass on an additional tax burden related to the other $j \neq i$ projects, it might not be politically feasible after all.

WSJ do recognize that, more often than not, a legislator's political calculus must account for benefits and costs stemming from projects initiated in other districts. One can thus easily foresee a case where project x^L, the optimal choice for legislator i, is not supported by legislator j because the net costs imposed on $j's$ district are greater than the benefits. Complete universalism led to a vector of policy choices $X^L = (x_1^L, x_2^L, ..., x_n^L)$. WSJ show that legislator j will prefer $X^C = (x_1^C, x_2^C, ..., x_n^C)$ if x^C will lower the district's tax burden by an amount greater than the reduction in potential benefits. Thus, they foresee a scenario where x^C, coupled with compensation stemming from legislative bargaining, is preferred to x^L. If all legislators can agree on x^C then, in relation to project size, the Law of $1/n$ should fail to hold.

Criticism of WSJ has primarily focused on their assumption of universalism.[4] For exam-

but possibly harm consumers of those inputs.

[4]Weingast, Shepsle and Johnsen (1981) is part of a set of papers authored or coauthored by Weingast that

ple, Baron (1991) and Buchanan and Yoon (2002) wonder why legislatures that operate on some variant of majority rule would constrain decision-making activities to a rule such as universalism.

Specifically, Baron (1991) asks why the legislature could not simply adopt rules or procedures that would prevent universalism altogether. Baron considers a model where a legislature decides upon distributive policy, first according to a type of closed rule and then according to a type of open rule.[5] [6] In this non-cooperative, sequential legislative bargaining game, Baron finds that under a closed rule, inefficient distributive policies will be adopted and this inefficiency will increase with the number of districts. Also, Baron shows that the benefits will only be distributed to districts in the minimum winning coalition (MWC). Proposals are characterized by a benefit-cost ratio B/T, where B represents the total benefits that will be allocated to districts in the MWC and T represents the tax cost of providing B. Legislators, though, vote for policies based upon a political benefit-cost ratio, $R(n, \delta)$, where n is legislature size and δ is a discount factor common to all legislators. In Baron's treatment, member i will only vote for a proposal if $i's$ net benefits are at least as large as the discounted value of continuing into the next legislative session. In the model, this is

studies the consequences of universalism. These include Weingast (1979) and Shepsle and Weingast (1981). Criticisms based upon the assumption of universalism has tended to focus on these works collectively.

[5]Baron defines a closed rule as one where a proposal is immediately voted up or down. A vote of up means the issue passes and benefits are distributed to the concerned districts. A down vote, though, means the issue fails and another legislator is allowed to make a proposal. Conversely, under open rule, if a project is proposed by one legislator, another legislator has the ability to offer an amendment, i.e. an alternative proposal, or motion to terminate the amendment process. The motion, if proposed, is voted on. If the motion is upheld, the project proposal goes to an up or down vote. If the project passes, the process is over. If it fails, another member is allowed to make a proposal. If an amendment is proposed, the legislature must vote between the proposal and the amendment. If the amendment wins, it is now the proposal on the agenda and the process repeats itself. If the proposal wins, the benefits are distributed and the process ends.

[6]Testa (2010) incorporates closed and open rules in an empirical analysis of corruption and bicameralism. Under a closed rule regime, the lower chamber has both proposal and veto power while the upper chamber has veto power only. Under an open rule regime, both chambers have proposal and veto power. Typically, legislatures using a closed rule restrict power over financial policy to the lower chamber. Our alternative measure of bicameralism controls for this. Controlling for the type of open and closed rules considered by Baron (1991) would be exceptionally difficult, if not impossible. For example, Baron points out that in the United States the adoption of closed or open rules can vary depending upon the particular bill before Congress.

represented by the equation

$$z_i = b_i - t_i = b_i - \frac{T}{n} \geq \delta V_i,$$

where z represents net benefits accruing to legislator i, b represents benefits from the proposal that are exclusively enjoyed by legislator i, and V represents $i's$ continuation value.[7] Under a closed rule, where the proposal is subject to a strict up or down vote, the proposal only has to attract $(n-1)/2$ votes from among the other legislators. When forming a MWC, the proposing legislator will seek out low-cost members. For example, a legislator k with a continuation value lower than that of the other legislators will be sought out by the proposing legislator. However, k would then attempt to extract more benefits for his or her district. If this attempt to extract more benefits were to raise $k's$ continuation value above that of some other member j, the proposing legislator would then make a proposal that would attract the vote of member j. Thus, in equilibrium, each legislator will have the same continuation value, or, in Baron's notation, $v_i = \bar{V}$, $\forall\ i$. The legislator seeking to form a MWC will offer each coalition member benefits equal to $(1/n)T + \delta\bar{V}$, in other words, his or her district's tax burden plus the discounted continuation value. The remaining members outside the coalition will receive zero benefits and the proposing legislator will receive $\{B-((n-1)/2)(T/n)+\delta\bar{V})\}$.

Each legislator has an $\frac{n-1}{n}$ probability of not being the proposer. Conditional on not being the proposer a legislator has a probability of 1/2 of being in the winning coalition and a probability of 1/2 of not being in the winning coalition. Thus, for legislator i, we have

$$\bar{V} = \frac{1}{n}\left[B - \frac{n-1}{2}(\frac{T}{n} + \delta\bar{V}) - \frac{T}{n}\right] + \frac{n-1}{n}\left[\frac{1}{2}(\frac{T}{n} + \delta\bar{V} - \frac{T}{n}) + \frac{1}{2}(-\frac{T}{n})\right]$$

which reduces to $\bar{V} = \frac{B-T}{n}$.

After solving for the continuation value in terms of (B, T, n), Baron shows that legislator

[7] Moreover, $\sum_{i=1}^{n} b_i \leq B$, i.e. the distribution of benefits cannot exceed the total benefits to be allocated.

i will make a proposal if

$$
\begin{aligned}
z_t &= B - \frac{n-1}{2}(\frac{T}{n} + \delta\bar{V}) - \frac{T}{n} \\
&= B\left[1 - (\frac{n-1}{2})\frac{\delta}{n}\right] - \frac{T}{n}\left[1 + (\frac{n-1}{2})(1-\delta)\right] \geq 0 \\
&\Rightarrow B\left[1 - (\frac{n-1}{2})\frac{\delta}{n}\right] \geq \frac{T}{n}\left[1 + (\frac{n-1}{2})(1-\delta)\right]
\end{aligned}
$$

and eventually,

$$\frac{B}{T} \geq \frac{2 + (n-1)(1-\delta)}{2n - \delta(n-1)}$$

where

$$\frac{B}{T} \geq \frac{2 + (n-1)(1-\delta)}{2n - \delta(n-1)} \equiv R(n, \delta).$$

Thus, legislator i will receive nonnegative benefits from the proposal if $\frac{B}{T} \geq R(n, \delta)$, i.e. if the proposal's benefit-cost ratio is greater than the political benefit-cost ratio. A proposal with a benefit-cost ration at least as large as $R(n, \delta)$ will receive a vote from legislator i. Since $R(n, \delta) < 1$, it is possible that economically inefficient projects will be passed. This inefficiency exists because benefits are limited to the MWC but are paid for by all districts. Moreover, since $\frac{\partial R}{\partial n} < 0$ the more districts across which the tax burden can be spread, the more inefficient policies that will be chosen. This gives us a version of the Law of $1/n$.

Under an open rule, this inefficiency is limited (though never completely eliminated) and, again, benefits are distributed to districts that are members of the MWC. Inefficiency is reduced because now that each member can potentially offer an amendment that is more favorable towards their own district, there is a greater cost in attracting votes. Thus, a legislator making a proposal must allocate more benefits towards other districts than would occur under a closed rule. This limits potential benefits for the proposing legislator. The political benefit-cost ratio is still decreasing in n but the value it takes as $n \to \infty$ is equal

to 1/2, whereas, under a closed rule, this ratio could equal zero. It is important to note that Baron allows for completely rational legislators and voters who know the full benefits and costs of a project. Thus, the model corrects for the assumption made in WSJ that a legislator myopically considers only the costs stemming from the project he or she proposes.

Despite the theoretical objections to the assumption of universalism, empirical studies have upheld its existence as a legislative norm.[8] WSJ mention several of these studies as the basis for why they incorporate universalism into their model. Buchanan and Yoon (2002) also mention that empirical findings consistently uphold universalism – although, they do not directly cite any evidence.

Primo and Snyder (2008) offer several clarifying points regarding the Law of $1/n$. Their main critique does not nullify the original theory but it does make clear why the law actually holds in WSJ's treatment. Using the case of pure public goods they show that the benefits component, $b(x)$, in the original WSJ formulation is misspecified. Essentially, WSJ do not account for the fact that a fixed population means anytime the number of districts changes, there is necessarily a corresponding change in each district's population. When considering pure public goods, a change in the district's population will change the net benefits accruing to the district and thus the net payoff to the district's legislator.[9] Accordingly, this result produces two competing effects: 1) increasing the number of districts reduces each district's tax share, putting upward pressure on the size of the project; 2) as the number of districts increases, the population in each district decreases, which reduces the benefits to increasing project size. As Primo and Snyder (2008) show, these competing effects offset one another, meaning project size is independent of the number of districts. Recall from the discussion of WSJ, though, that the *size* of the project was increasing in n, which would mean the

[8]The exception, though, is Stein and Bickers (1994) who find weak evidence that benefits are distributed based on a universalistic norm.

[9]Primo and Snyder (2008) present a clarifying example involving spillovers. Larger districts will be better able to capture project benefits while smaller districts will see some of the benefits spill over. Thus, the benefits captured by the district depend, in part, on population size.

original model and subsequent interpretation are incorrect. However, as Primo and Snyder (2008) point out, since the *number* of projects is increasing in n, the Law of $1/n$ still holds theoretically in relation to total spending. The assumption of universalism in WSJ, which is also applied to Primo and Snyder (2008), essentially guarantees any interested district a project. This means new districts will demand a project, which will increase total government spending but not the politically efficient project size. It is unclear, though, if we should expect *total* spending to grow in a model that excludes the assumption of universalism. For example, recall that Baron (1991) finds that inefficient distributive policies will still occur but the amount of projects will be limited to the size of the MWC.

The empirical investigation of legislature size and government spending fall into a separate literature that is loosely based on the Law of $1/n$, referred to by Bradbury and Crain (2001) as the "size fragmentation literature (ibid: 310)." This literature supposes that the common pool problem relates to all governmental participants in fiscal and budgetary processes.[10] Von Hagen and Harden (1995) show evidence that European "spending ministers (ibid: p. 773)," i.e. cabinet members, are susceptible to this problem in that one's utility is enhanced by granting benefits to one's constituency. Kontopoulos and Perotti (1999) and Perotti and Kontopoulos (2002) use data from OECD member nations to show evidence that the number of spending ministers and the number of parties in a country's governing coalition are positively related to government expenditures. Stein et al. (1999) use data from a sample of Latin American countries to show that a large district magnitude – i.e. a larger number of legislative seats per district – is positively associated with government expenditures.

Results investigating the particular relationship we are interested in have been mixed but, in general, appear to support the Law of $1/n$. Most research has tested this hypothesis using data from the United States. For example, Gilligan and Matsusaka (1995, 2001) examine this

[10] In fact, Kontopoulos and Perotti (1999) claim that the hypothesis in WSJ can be applied "with a simple relabeling of variables" to generic size fragmentation issues (ibid: p. 82).

relationship over the period 1960 - 1990 and 1902 - 1942. In both time periods they observe a positive relationship between government spending and growth in the upper chamber and no significant relationship between government spending and lower chamber size. Primo (2006) also used data from the United States to investigate this hypothesis. Studying the period 1969 - 2000, Primo affirms the positive relationship between spending and upper chamber size but, conversely, he finds a negative relationship between the lower chamber and spending. Chen and Malhotra (2007) develop a theoretical model that attempts to account for the fact that U.S. House districts are ensconced within Senate districts. That is, each House district occurs geographically within Senate districts, creating an overlap. Thus, they alter the standard $1/n$ model by introducing a House-to-Senate seat ratio, which becomes k/n, where k is the House-to-Senate ratio.[11] Intuitively, smaller values of k are associated with a smaller constituency for each House district, and thus smaller payoffs that can be generated from common pool resources. the implications of their model suggest that spending should be decreasing in the House-to-Senate seat ratio. Using data over the period 1992-2004 and 1964 - 2004 Chen and Malhotra reaffirm the positive relationship between spending and upper chamber size and find a negative relationship between the House-to-Senate ratio and spending.[12] More recently, using U.S. data over the period 1992 – 2010, Crowley (2014) finds evidence of a positive relationship between upper chamber size and per capita state and local government spending and a negative relationship between lower chamber size and per capita spending.

Other studies have focused on the relationship between elected bodies at the local and country level. Using data on American city councils, Baqir (2002) finds a positive relationship between city council size and per-capita expenditures. Bradbury and Stephenson (2003)

[11] This requires single-member House districts.

[12] Chen and Malhotra discuss the difficulties of extending their method to cross-country analysis. For example, they note that not one bicameral legislature is composted of single-member districts in both chambers. Because of this difficulty, we do not incorporate a similar k/n in the empirical analysis.

study county commissions in the state of Georgia and also find a positive relationship between commission size and per-capita spending.

Egger and Koethenbuerger (2010) and Pettersson-Lidbom (2012) represent a unique strand of the literature that exploits discontinuities in local laws governing council size. For the particular municipalities they study, council size is an increasing but discontinuous function of population, thus, when the population crosses a certain threshold, the size of the council increases to a predetermined level. These discontinuities create a natural experiment in which to analyze the relationship between council size and spending. Within this framework, Egger and Koethenbuerger (2010) study this relationship using data from 2,056 municipalities in the German state of Bavaria from 1984 - 2004. They find a positive relationship between council size and government spending. This effect holds for mayor-council systems, where each councilperson represents a distinct geographic district, as well as single-district councils. Pettersson-Lidbom (2012), using a data set comprised of Finnish city councils and Swedish local governments, breaks with the previous literature, finding a negative relationship between council size and spending.[13]

Bradbury and Crain (2001) present cross-country evidence verifying the Law of $1/n$. The authors study a panel of thirty-five countries over the period 1971-1989. the sample includes fourteen unicameral legislatures and twenty-four bicameral legislatures. Bradbury and Crain find that larger unicameral legislatures are associated with larger amounts of government spending, both as a percentage of GDP and in per-capita terms. The authors' model allows them to control for unicameral legislature size and bicameral legislature size.[14] Regressing both measures of government spending on unicameral and bicameral legislature

[13]The methodology used by Egger and Koethenbuerger (2010) and Pettersson-Lidbom (2012) would not be useful in our context because not all countries in the sample have laws similar to those of the municipalities they examine. They criticize the previous research as failing to offer identified causal effects. However, the methodology we will use, which will be discussed in Section IV, reconciles the primary identification issue.

[14]In a recent paper Maldonado (2013) claims that modeling unicameral and bicameral legislature size in the same reduced form regression is inappropriate.

size they confirm the positive association between spending and unicameral legislature size. In the case of bicameral legislatures, they find a positive association between spending and lower chamber size. The positive effect of lower chamber size is lower than that associated with unicameral legislature size, leading Bradbury and Crain to conclude that the Law of $1/n$ is somewhat dampened in bicameral legislatures. The effect of upper chamber size is found to be very small and fluctuates between having both positive and negative effects. The authors attribute this ambiguity to the power asymmetries that render most upper chambers impotent in matters of fiscal policy.

Despite any theoretical concerns or objections to the Law of $1/n$, empirical studies quite often verify a positive relationship between legislature size and government spending. If these criticisms are true, they why do empirical investigations continue to uphold this hypothesis? The answer could be that previous empirical studies are misspecified or under identified, leading us to reach spurious conclusions. We hope the preceding empirical discussion will help to clarify this conundrum.

III Data Set

Since the focus here is on the size of democratically elected legislatures, we only include observations where a certain quality of democracy threshold is achieved. To rate each country's quality of democracy we use the Polity GT index created by Persson and Tabellini (2003).[15] Similar to Persson and Tabellini, we drop from the sample any country observation with a Polity GT score higher than 3.6.[16] Furthermore, we also drop those observations with missing quality of democracy scores. We note that excluding observations in this manner allows certain countries to enter into the sample intermittently and effectively treats these missing observations as random. For example, Ecuador enters the sample in 1968 but is

[15] A full description of the Polity GT index is available in the appendix.
[16] A 3.6 Polity GT score corresponds to a score of zero on the Polity index.

excluded during the period 1972 – 1978, when a military junta took power.

We choose to ignore this potential sample selection issue for several reasons. Firstly, restricting the sample to include only countries that meet the quality of democracy criteria over the entire sample period would reduce the number of cross-sectional observations to the point that fixed-effects estimation would not be meaningful. More importantly, though, it is unclear whether a country dropping out of the sample would lead to inconsistent estimation. If the reason a country drops out of the sample is correlated with the error term then we would have to take this selection issue into account. Thus, the crucial question is whether an unusually high Polity GT score is correlated with the error term. We argue that it is not. Quality of democracy is not the dependent variable of interest, which means dropping these country observations will not lead to attrition bias. Also, the question of interest is whether legislature size influences government spending as a percentage of GDP in countries with democratically elected legislatures. When observations are excluded, it is because the legislature (if it was in session at all) was not elected through free and fair elections. Therefore, exclusion of these observations does not lead to the case where the results are biased towards a specific subset of the sample because these observations are in fact not part of the relevant sample. We also exclude observations where data is unavailable. After accounting for quality of democracy and missing data we are left with an unbalanced panel of forty-eight countries for the period 1960 – 1998.[17] These countries are listed in Table 2.1.

Fiscal data comes from a country panel compiled by Brender and Drazen (2005), which primarily covers the years 1960 - 2000 and uses fiscal data from the IMF-IFS. Unfortunately, revisions to IFS data make this data set and post-2000 IFS data incomparable. Thus, extending the panel is not possible when using IFS data. We only us data up to 1998 because that is the last year for which the Persson and Tabellini (2003) Polity GT scores were available. During the sample period, twenty-eight countries had periods with a bicameral legislature and

[17]Full definitions of the variables along with sources are provided in the appendix.

Table 2.1: Countries in Sample: 1960 – 1998

Country	Years	Country	Years
Argentina	1983-1998	Israel	1960-1998
Australia	1960-1998	Italy	1960-1998
Austria	1960-1998	Japan	1970-1993
Belgium	1960-1998	Luxembourg	1970-1998
Bolivia	1985-1998	Malaysia	1960-1998
Brazil	1985-1998	Mauritius	1968-1993
Canada	1960-1998	Mexico	1988-1998
Chile	1989-1998	Netherlands	1960-1998
Colombia	1971-1998	Nepal	1990-1998
Costa Rica	1970-1998	New Zealand	1960-1998
Cyprus	1975-1998	Nicaragua	1990-1998
Denmark	1960-1998	Norway	1960-1998
Dom. Rep	1978-1998	Paraguay	1989-1998
Ecuador	1938-1971; 1979-1998	Peru	1986-1991; 1996-1998
El Salvador	1964-1971; 1984-1998	Philippines	1960-1971; 1986-1998
Fiji	1970-1986; 1990-1998	Portugal	1975-1998
Finland	1960-1998	Spain	1977-1998
France	1972-1998	Sri Lanka	1960-1998
Greece	1960-1966; 1975-1998	Sweden	1960-1998
Guatemala	1966-1973; 1986-1998	Switzerland	1960-1998
Honduras	1980-1998	Turkey	1968-1970; 1973-1979; 1983-1998
Iceland	1960-1998	United States	1960-1998
India	1960-1998	Uruguay	1985-1998
Ireland	1960-1998	Venezuela	1960-1998

twenty-two had periods with a unicameral legislature.[18] Summary statistics for legislature size in unicameral and bicameral countries are provided in Tables 2.2 and 2.3, respectively. Tables 2.4 and 2.5 provide summary statistics for central government expenditures as a percentage of GDP (*CGEXP*) for unicameral and bicameral countries, respectively. *CGEXP* will be the dependent variable of interest for all regressions. Table 2.6 provides correlation matrices, again, divided into unicameral and bicameral countries.

[18]Sweden's parliament contained two chambers from 1960 - 1970 and was reduced to a single chamber from 1971 onward. Peru's legislature was bicameral from 1986 - 1991 and unicameral from 1993-1998.

Table 2.2: Summary Statistics: Unicameral Legislature Size

Country	Mean	SD	Min	Max
Costa Rica	57	0.00	57	57
Cyprus	47	10.58	35	56
Denmark	178	1.35	175	179
Ecuador	73	4.33	69	82
El Salvador	65	13.61	52	84
Finland	195	12.80	152	201
Greece	300	0.31	299	301
Guatemala	83	23.76	54	116
Honduras	116	23.66	71	134
Iceland	61	2.05	55	63
Israel	120	3.11	109	128
Luxembourg	59	2.51	56	64
Mauritius	70	1.09	66	70
Nepal	191	28.66	140	205
Nicaragua	72	24.16	41	96
Norway	155	6.19	148	165
New Zealand	90	9.63	80	120
Peru	93	30.12	65	120
Portugal	243	11.58	230	262
Sri Lanka	188	40.53	94	225
Sweden	348	3.25	339	350
Turkey	456	41.10	400	550
All	154	105.25	35	550

Table 2.3: Summary Statistics: Bicameral Legislature Size

Country		Mean	SD	Min	Max	Country		Mean	SD	Min	Max
Argentina	Lower	253	11.80	231	274	Italy	Lower	627	10.47	596	631
	Upper	51	10.77	46	72		Upper	315	23.67	246	326
Australia	Lower	132	11.11	124	149	Japan	Lower	498	17.46	456	520
	Upper	67	7.30	59	76		Upper	252	1.13	252	256
Austria	Lower	178	8.21	165	183	Malaysia	Lower	145	32.79	103	192
	Upper	58	4.31	50	63		Upper	61	9.35	38	69
Belgium	Lower	211	19.63	150	236	Mexico	Lower	491	30.15	400	500
	Upper	160	38.28	71	188		Upper	87	32.29	64	128
Bolivia	Lower	129	3.47	117	130	Netherlands	Lower	151	1.84	150	156
	Upper	27	0.00	27	27		Upper	75	0.61	73	75
Brazil	Lower	485	9.49	476	513	Paraguay	Lower	73	8.60	60	80
	Upper	77	5.22	69	81		Upper	45	0.00	45	45
Canada	Lower	276	13.55	258	300	Peru	Lower	176	10.61	154	180
	Upper	103	1.00	102	104		Upper	60	0.00	60	60
Chile	Lower	120	0.00	120	120	Philippines	Lower	145	48.42	95	201
	Upper	57	0.00	57	57		Upper	24	0.29	23	24
Colombia	Lower	191	17.40	161	210	Spain	Lower	345	10.63	320	350
	Upper	111	5.95	102	118		Upper	250	2.96	248	256
Dom. Rep.	Lower	108	14.88	91	121	Sweden	Lower	233	0.52	232	233
	Upper	29	1.52	27	30		Upper	151	0.00	151	151
Fiji	Lower	56	10.44	36	70	Switzerland	Lower	201	3.51	196	209
	Upper	26	5.65	22	34		Upper	45	1.01	44	46
France	Lower	518	46.40	469	585	Uruguay	Lower	99	0.00	99	99
	Upper	307	15.09	283	319		Upper	31	0.00	31	31
India	Lower	543	5.62	524	546	USA	Lower	435	0.00	435	435
	Upper	242	4.63	220	245		Upper	100	0.00	100	100
Ireland	Lower	154	10.58	144	166	Venezuela	Lower	192	22.99	133	214
	Upper	60	0.00	60	60		Upper	49	2.85	46	55

Table 2.4: Central Government Expenditures as a % of GDP for Unicameral Countries

Country	Mean	SD	Min	Max
Costa Rica	16.33	1.76	13.61	20.80
Cyprus	31.40	2.68	26.98	36.89
Denmark	33.86	6.93	18.82	42.76
Ecuador	15.15	1.73	13.12	18.57
El Salvador	12.87	1.69	10.26	15.62
Finland	28.31	6.88	19.78	44.38
Greece	25.96	6.94	16.95	36.56
Guatemala	10.24	1.33	8.58	13.24
Honduras	21.32	2.35	17.80	25.95
Iceland	27.75	4.89	17.35	34.37
Israel	49.49	11.53	26.79	69.09
Luxembourg	40.90	5.74	31.28	51.26
Mauritius	26.35	3.21	21.67	31.96
Nepal	17.87	1.21	15.69	20.18
Nicaragua	29.92	3.20	25.13	36.00
Norway	31.01	8.57	17.94	43.25
New Zealand	33.96	5.82	25.88	44.39
Peru	18.18	0.58	17.32	19.06
Portugal	39.85	3.15	33.56	46.16
Sri Lanka	29.22	3.84	22.43	41.36
Sweden	39.76	6.28	27.40	51.74
Turkey	19.88	4.75	12.20	32.10
All	29.13	11.31	8.58	69.09

Table 2.5: Central Government Expenditures as a % of GDP for Bicameral Countries

Country	Mean	SD	Min	Max
Argentina	12.77	2.34	8.80	16.40
Australia	24.17	3.07	18.74	28.93
Austria	36.74	3.59	31.50	41.90
Belgium	44.74	6.47	36.10	54.80
Bolivia	19.81	6.71	11.76	29.08
Brazil	29.88	4.96	24.35	40.00
Canada	20.37	3.99	13.69	27.11
Chile	20.39	0.79	19.15	21.38
Colombia	13.49	1.62	10.77	16.72
Dom. Rep.	13.62	1.96	9.51	16.34
Fiji	27.15	3.38	20.47	32.80
France	41.09	4.49	31.57	46.35
India	12.42	2.78	7.98	17.10
Ireland	36.98	8.15	26.18	53.70
Japan	16.40	3.13	11.07	23.70
Malaysia	26.60	6.58	13.40	43.32
Mexico	17.13	3.44	14.45	25.18
Netherlands	31.39	5.38	22.33	40.75
Paraguay	13.68	2.95	8.94	17.20
Peru	15.71	2.89	12.82	20.63
Philippines	15.51	3.18	11.28	19.65
Spain	21.86	3.33	13.93	27.01
Sweden	24.68	4.55	19.68	31.30
Switzerland	8.94	1.06	6.72	10.96
Uruguay	27.12	3.18	23.32	32.58
USA	20.61	1.81	16.77	23.48
Venezuela	21.16	3.02	16.86	29.62
All	24.29	10.74	6.72	54.80

Table 2.6: Correlation Matrices

	CGEXP	*LYP*	*TRADE*	*GDPGAP*	*PROP65*	*PROP1564*	*UNISIZE*
CGEXP	1						
LYP	0.547***	1					
TRADE	0.427***	0.366***	1				
GDPGAP	-0.166***	-0.283***	0.142***	1			
PROP65	0.602***	0.743***	0.221***	-0.341***	1		
PROP1564	0.523***	0.649***	0.277***	-0.179***	0.840***	1	
UNISIZE	0.100**	0.0272	-0.440***	-0.184***	0.303***	0.290***	1

* $p < .10$, ** $p < .05$, *** $p < .01$

	CGEXP	*LYP*	*TRADE*	*GDPGAP*	*PROP65*	*PROP1564*	*LOWERSIZE*	*UPPERSIZE*
CGEXP	1							
LYP	0.272***	1						
TRADE	0.472***	0.102***	1					
GDPGAP	-0.142***	-0.374***	0.130***	1				
PROP65	0.512***	0.692***	0.106***	-0.315***	1			
PROP1564	0.302***	0.730***	0.0359	-0.312***	0.806***	1		
LOWERSIZE	-0.0307	0.167***	-0.515***	-0.103***	0.204***	0.302***	1	
UPPERSIZE	0.169***	0.166***	-0.306***	-0.0463	0.343***	0.362***	0.836***	1

* $p < .10$, ** $p < .05$, *** $p < .01$

The simple correlation coefficients calculated in Table 6 reveal some interesting relationships. The correlation coefficients for upper chamber size and *CGEXP* and for unicameral size and *CGEXP* are both positive but the correlation coefficient for lower chamber size and *CGEXP* is negative. *A priori*, a positive correlation between lower chamber size and spending was expected. These statistics, however, reveal very weak linear relationships between chamber size and government spending, which most likely reflects the limited variability of chamber size. Within a sample covering over thirty years of fiscal data, several countries experience large differences in *CGEXP*. Meanwhile, changes in the size of a country's legislative chamber are rare. For example, in the United States *CGEXP* ranges between 16.77% and 23.48% between 1960 and 1998. Yet the size of the legislature never changes. A similar relationship is observed for most of the other countries in the sample as well.

The variable *CGEXP* does not include spending by local and regional governments, thus, it is much better suited for cross-country comparisons of government size. Much of the literature on chamber size and spending focuses exclusively on overall central government spending.[19]

The log difference between real GDP and its trend (*GDPGAP*) is used to measure the fiscal response to country-specific shocks. It will be described in more detail in Section IV. Openness to trade (*TRADE*), defined as the sum of imports and exports divided by GDP, and the natural log of per capita GDP (*LYP*) have been shown to be significant determinants of central government expenditures. Rodrik (1998) shows that trade openness is positively correlated with government spending. His findings lend empirical support to the theory that a higher level of government spending counters the risk exposure a country might face from a greater openness to trade. *LYP* is often included in examinations of government spendings to account for Wagner's Law, which is the idea that government spending is a normal good.

[19]Dalle Nogare and Ricciuti (2011) do not that central government expenditures, as collected by Persson and Tabellini (2003) and Brender and Drazen (2005), also has some some drawbacks. For example, they point out that these variables count government debt service as a central government expenditure.

If Wagner's Law is correct, government spending should be increasing as income increases. Consequently, most cross-country comparisons of government size include trade openness and per capita GDP as control variables.

In addition to the aforementioned economic controls, we include variables measuring the proportion of the population above age sixty-five (*PROP65*) and the proportion of the population between the ages of 15 and 65 (*PROP1564*). We can reasonably expect *PROP65* to have a positive relationship with government spending, but theory does not provide us with an *a priori* expectation of the relationship between *CGEXP* and *PROP1564*.

The political variables of interest are *UNISIZE*, *LOWERSIZE*, and *UPPERSIZE*, which measure unicameral size, lower chamber size, and upper chamber size, respectively. These variables were collected from various editions of The Statesman's Year-Book and the Inter-Parliamentary Union's (IPU) PARLINE Database. When determining chamber size, any ceremonial or nonvoting member was not counted. Thus, in some cases, the size of the legislature we use will be slightly smaller than the official number.

Bradbury and Crain do not distinguish between bicameral legislatures where both chambers have power over fiscal legislation and those where only the lower chamber has such power. For example, the legislatures of Austria and Japan are both dual-chambered entities. However, in Austria, the upper chamber is constitutionally prohibited from opposing fiscal legislation while in Japan, the lower chamber is decisive in matters of fiscal policy. As a result, some studies have treated these legislatures as unicameral.[20] Considering legislatures such as Austria or Japan bicameral when it is known *a priori* that upper chambers have negligible power over fiscal policy could potentially bias their results. Here, we will adopt two definitions of bicameralism: the tradition definition that considers any legislature with two chambers to be bicameral and an alternative definition that only considers a legislature to be bicameral if both chambers possess the ability to affect fiscal policy. In dual-chambered legis-

[20]See Heller (1997), for example.

latures where the upper chamber has no power over fiscal policy, the growth of the impotent upper chamber should not affect spending. Thus, these legislatures are *de facto* unicameral when matters of fiscal policy are concerned. This leads to new measures of bicameralism and chamber size that reflect this power discrepancy between chambers. The variables *BICAM1*, *UNISIZE1*, *LOWERSIZE1*, and *UPPERSIZE1* reflect, respectively, the bicameral status, unicameral chamber size, lower chamber size, and upper chamber size, when the definition of bicameralism is modified to account for these differences in power.[21]

Ultimately, we are interested in how legislature size affects government spending over time. however, we can use cross-sectional regressions to further inform what we should expect to find. These results will be discussed but not presented.[22] To remain consistent with the primary results that will be presented below, we divide the sample into unicameral and bicameral legislatures. We also include the same control variables as in the primary regressions. for each type of legislature, we estimate the relationship between chamber size and spending in each year, beginning with 1961, and then for each decade. The estimated coefficient on unicameral size is positive for the majority of the years in the sample. In only six years is the coefficient negative. The estimated coefficient on lower chamber size is negative for the majority of years in the sample. However, the negative coefficients are found primarily in the 1960s and 1970s. The coefficient on upper chamber size is positive for each year in the sample. Due to the limited number of cross-sectional observations in each year none of the estimated coefficients on the chamber size variables are statistically different from zero. Because of this, we also pool observations over each decade. For unicameral chamber

[21] One aspect we have not considered here is how to control for appointed upper chambers. Again, Bradbury and Crain (2001) do not distinguish between elected and appointed upper chambers. As with restrict upper chambers, the incentives for upper chamber members who are appointed are different from those who are elected. It is possible, though, that this issue is taken care of when a legislature's organizational status is determined by its power over fiscal policy. When bicameral status is determined based upon this criterion, Canada remains the only country in the sample with an appointed upper chamber that retains legitimate power over fiscal policy.

[22] These results are available upon request.

size, the estimated coefficient is consistently positive over each decade, with estimates varying between .01 in the 1990s and .04 in the 1980s. The estimated coefficient on lower chamber size is -.036 in the 1960s (t-statistic = -5.28) and -.016 in the 1970s (t-statistic = -1.87) but becomes .018 in the 1980s (t-statistic = 2.50) and is positive but not statistically different from zero in the 1990s. Lastly, the coefficient on upper chamber size is consistently positive, ranging from .021 in the 1990s to .078 in the 1960s. With the exception of the 1990s, this coefficient is statistically different from zero at the 1 percent level.

What these simple cross-sectional results reveal is that the effect of legislature size on government spending changes depending upon the time period in question. Thus, pure cross-sectional analysis will not suffice. An estimation strategy developed by Blanchard and Wolfers (2000) will allow us to account for year-specific and country-specific events while also obtaining reliable estimates of the influence that chamber size has on the growth of government spending.

IV Primary Questions

Using Blanchard and Wolfers approach will allow us to answer the following questions: 1) does chamber size affect the response of government spending to unobserved events that are common to all countries; and 2) does chamber size affect the response of government spending to country-specific events? In other words, certain events, both common and country-specific, affect the behavior of government spending over time. We want to know if and how legislature size influences the relationship between spending and these events.

IV.1 Unobserved Common Shocks

Persson and Tabellini (2003) note that for most of the countries in their sample, central government spending as a percentage of GDP exhibits a similar development over time. They

attribute this to certain common events that affected all countries. Specifically, they mention the increased popularity of left-wing ideologies in the 1970s, the shift to more conservative ideologies in the 1980s, and the slowdown in economic growth and oil price shocks of the 1970s and 1980s. The question they seek to answer is whether broad constitutional rules – i.e. form of government or electoral rules – had an effect on the fiscal policy response to these common events. Since the effect of these constitutional rules is their primary interest, they proxy for the common events with year-specific dummy variables and estimate the interaction between each constitutional rule and the set of year dummy variables.

This method was developed by Blanchard and Wolfers (2000) as a way to estimate the effect of labor market institutions – such as unionization – on European unemployment in the face of unobserved common shocks. The first use of this model in a political economy context was by Milesi-Ferretti, Perotti and Rostagno (2002), who examined how different electoral rules influenced the fiscal policy response to common shocks in OECD countries. A common difficulty faced by Milesi-Ferretti et al. (2002) and Persson and Tabellini (2003) was the invariability of the primary variables of interest. Milesi-Ferretti et al. (2002) were interested in electoral rules that exhibited little or no variability within each country over time. Similarly, Persson and Tabellini (2003) were primarily interested in indicator variables for form of government and electoral rules that exhibited no time variation within each country. Therefore, pure fixed-effects estimation would have been imprudent in either case.

Chamber size, the variable of interest here, tends to exhibit more variability than electoral rules or form of government, but the variation is still extremely limited.[23] Thus, we too will make use of this methodology. The primary regression equations will take the following form:

$$CGEXP_{it} = \boldsymbol{\beta X_{it}} + (1 + \gamma UNISIZE_{it})\boldsymbol{\delta Q_t} + \alpha_i + \epsilon_{it}. \tag{2.1}$$

[23]Bradbury and Crain (2001) report correlation coefficients between the first and last year's observations of .99 for upper chambers and .94 for lower chambers. We find similar correlation coefficients.

$$CGEXP_{it} = \boldsymbol{\beta X_{it}} + (1 + \gamma_1 LOWERSIZE_{it} + \gamma_2 UPPERSIZE_{it})\boldsymbol{\delta Q_t} + \alpha_i + \epsilon_{it}. \quad (2.2)$$

CGEXP is always measured as a percentage of GDP. $\boldsymbol{X}$ is a vector of the relevant economic and demographic control variables discussed in Section III and $\boldsymbol{Q}$ is a vector of period-specific dummy variables that proxy for common shocks and are interacted with each chamber size variable. We also include country-specific dummy variables. The term α_i includes country-specific observed and unobserved time-invariant effects and ϵ_{it} is the error term that varies across countries and time. Each chamber size variable measures how large each chamber is for country i at period t. Chamber size variables enter into the regression as deviations from the cross-country mean. Thus, for a country with the average legislature, changes in spending that occur in the presence of unobserved common shocks are picked up by the vector of time period dummy variables. We can then estimate how the relationship between legislature size and *CGEXP* and compare it with this baseline estimate.

Using this model specification prevents us from including unicameral size, lower chamber size, and upper chamber size within the same regression as Bradbury and Crain (2001) did. Including all three variables in the regression model would imply that δ was measuring the spending response of the country with average values of all three chamber size variables. Since a country can only have a unicameral legislature or a bicameral legislature, such a result would be meaningless. Separating the sample into unicameral and bicameral subsamples means the length of each panel will be longer than the number of cross-sectional observations. Thus, estimating the effect of chamber size on year-to-year changes in fiscal policy would be imprudent. To cope with this difficulty, we follow Blanchard and Wolfers (2000) and Milesi-Ferretti, Perotti and Rostagno (2002) and average all variables over five year intervals, beginning in 1960 or the earliest data for which data is available. In light of this modification, $t = 1$ refers to the period 1960 - 1964, $t = 2$ refers to the period 1965 - 1969, etc. As a robustness check, we also average data over three and ten year intervals. These results will

be discussed below.

This modification to the data set yields subsamples that only contain 129 panel observations in the case of unicameral countries and 159 panel observations in the case of bicameral countries. Working with such a small number of panel observations limits the degrees of freedom and thus constrains the number of variables we can include in each regression. Of particular concern is the variable *TRADE*. This variable usually enters into the right hand side of the regression with a lag. However, including *TRADE* with a lag would reduce the number of panel observations that are available. Thus, we present results where *TRADE* enters into the model contemporaneously. One would also expect form of government or electoral rules to have some effect on central government expenditures. These variables, though, exhibit little (if any) within-country variability and would have to be interacted with the year dummy variables if they were included in the model. This would erode the degrees of freedom even further. Thus, we will assume these country characteristics are picked up by the fixed effects.

Equations 2.1 and 2.2 are estimated by nonlinear least squares. our primary interest is in the coefficients γ, γ_1, and γ_2. We identify these coefficients by interacting $\boldsymbol{Q_t}$ with each chamber size variable as well as allowing it to enter into the regression equation directly. As was already discussed, in a country with the average-sized legislature the chamber size variables equal zero. Thus, the change in government spending that would result from a common shock is equal to

$$\frac{\partial CGEXP_{it}}{\partial Q_t} = \delta.$$

Finishing this explanation using equation 2.1, if we assume that an adverse shock in a country with the average-sized unicameral legislature is associated with an increase in *CGEXP* of one percentage point (i.e. $\frac{\partial CGEXP_{it}}{\partial Q_t} = 1$), then the effect of that same adverse

shock in other unicameral legislatures would be equal to

$$\frac{\partial CGEXP_{it}}{\partial Q_t} = 1 + \gamma UNISIZE_{it}$$

where the spending response would depend upon the size of the legislature.

A coefficient significantly different from zero would indicate that the spending response to an unobserved shock could change depending upon the size of a country's legislature, whereas an insignificant result would indicate that having a chamber size different from the cross-country average yielded no significant effect on fiscal policy. Specifically, a positive coefficient associated with chamber size would imply that the fiscal policy response to a shock common to all countries is larger for countries with larger legislatures. Finding the coefficients on chamber size to be positive and significant would lend additional robustness to the positive relationship between legislature size and spending that has been found to exist. On the other hand, insignificant results might be evidence to the point that previous studies have suffered from an omitted variable bias. Such a finding would be a significant contribution because most of the evidence supporting this hypothesis has not fully accounted for unobserved heterogeneity.

We will also test the alternative classification of bicameralism that relies on each chamber's power over fiscal policy. Recall that Bradbury and Crain (2001) found upper chambers to have an ambiguous effect on total spending. However, as mentioned previously, this result is not surprising given that several of the upper chambers in their sample have little to no power over fiscal policy. Reclassifying legislatures based on authority over fiscal policy could affect the coefficient on upper chamber size and reveal a potential bias inherent in the classification that relies solely upon the number of chambers in the legislature. We consider a legislature to be bicameral with regards to fiscal policy if both chambers possess significant power over fiscal policy. Legislature where the upper chamber is constitutionally prohibited

from voting on matters of fiscal policy and legislatures where the lower chamber is ultimately decisive in these matters are considered unicameral.

Research by Heller (1997) and Heller (2001), Perotti and Kontopoulos (2002), Persson and Tabellini (2003), and Dalle Nogare and Ricciuti (2011) have all identified a status quo bias in relation to government spending. That is, spending in year t is a significant determinant of spending in year $t+1$. Ideally, we would want to include a lag of the dependent variable on the right hand side of the equation. However, since Nickell (1981) it has been well known that including this term as a regressor in a fixed-effects model introduces a bias of order $T-1$. While several methods have been developed to remove this bias (e.g. Anderson and Hsiao (1982) and Arellano and Bond (1991))it would not be feasible to implement one of these methods in the estimation of equations 2.1 and 2.2. The only way to include $CGEXP_{it-1}$ in each equation and be reasonably sure that the bias discussed by Nickell (1981) is negligible is to have a panel with $T \geq 30$.[24] For example, Persson and Tabellini (2003) were able to include a lag of the dependent variable in a similar model because the average country panel in their data set was twenty-six years. Since we will be estimating 2.1 and 2.2 using a panel with $T = 8$, introducing $CGEXP_{it-1}$ would obviously bring with it considerable bias. By not including this term, the model is implying that spending only responds to current common shocks. In reality, though, it is likely that spending is responding to both current and previous shocks. Thus, when viewing the results from the estimation of equations 2.1 and 2.2, we must be mindful of the fact that we are unable to account for the persistent nature of government spending.

[24] Judson and Owen (1999) show that when estimating an unbalanced panel with $T \geq 30$, the least squares dummy variable approach works reasonably well.

IV.2 Unobserved Common Shocks: Results

The primary results from the estimation of equations 2.1 and 2.2 are listed in Table 2.7. In Table 2.7 we define a legislature as bicameral if it is composed of two chambers. In Table 2.9 the definition of bicameralism based on fiscal authority will be used. Due to the limited number of cross-sectional observations in each sample (twenty-two unicameral countries and twenty-eight bicameral countries) we do not use standard errors that are clustered at the country level.[25] This should be kept in mind when viewing the results. For each type of legislature we estimate three models: the first model estimates either equation 2.1 or equation 2.2 using pooled OLS without country fixed-effects or year interactions; the second model interacts the chamber size variable(s) with the vector of year-interval dummy variables; and the third model incorporates both the interactions and country fixed-effects.

Across models (1) - (3) the coefficients on *LYP* and *PROP65* retain the expected sign. However, the coefficient on *LYP* is never statistically different from zero. The coefficient on *PROP65* is significantly different from zero at either the 1 or 5 percent level across all three models. The coefficient on *PROP1564*, which is never statistically different from zero, it negative across all three models. As stated earlier, though, there is not a clear theoretical expectation of the sign this coefficient should take. The coefficient on *TRADE* is positive across all three models and significantly different from zero at the 1 percent level in models (1) and (2) and at the 5 percent level in model (3).

In model (1) the coefficient on *UNISIZE* is positive and significantly different from zero (10 percent level). However, adding year interactions and country fixed-effects lowers the magnitude of the coefficient and it loses statistical significance.

[25] See Nichols and Schaffer (2007) for a discussion of the potential dangers of using clustered standard errors with a limited number of cross-sections.

Table 2.7: Central Government Expenditures and Chamber Size

	(1)	(2)	(3)	(4)	(5)	(6)
	CGEXP	*CGEXP*	*CGEXP*	*CGEXP*	*CGEXP*	*CGEXP*
CONSTANT	2.809	2.711	35.72	37.72***	52.53***	8.258
	(15.40)	(16.77)	(21.83)	(11.94)	(12.86)	(14.26)
TRADE	0.130***	0.119***	0.0920**	0.154***	0.153***	-0.0406
	(0.0273)	(0.0274)	(0.0382)	(0.0213)	(0.0204)	(0.0313)
LYP	1.764	1.007	-3.066	0.634	-1.477	0.927
	(1.429)	(1.733)	(2.638)	(1.149)	(1.273)	(1.458)
PROP65	1.056***	1.090***	2.052***	1.520***	1.832***	0.807**
	(0.345)	(0.378)	(0.408)	(0.256)	(0.277)	(0.364)
PROP1564	-0.164	-0.0663	-0.198	-0.717***	-0.712***	-0.104
	(0.270)	(0.269)	(0.221)	(0.258)	(0.248)	(0.201)
UNISIZE	0.0157*	0.00315	0.000807			
	(0.00848)	(0.00303)	(0.00123)			
LOWERSIZE				0.00351	0.00189	-0.00338**
				(0.00771)	(0.00171)	(0.00151)
UPPERSIZE				0.0207	0.00279	0.00921***
				(0.0135)	(0.00295)	(0.00308)
Observations	129	129	129	161	161	161
Adjusted R^2	0.501	0.490	0.887	0.491	0.510	0.915
Country FE	N	N	Y	N	N	Y
Year Interactions	N	Y	Y	N	N	Y

Standard Errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

Models (4) - (6) analyze the same question with respect to bicameral legislatures. With the exception of *LYP* and *TRADE*, the coefficients on the control variables are consistent across the three different models. The estimated coefficient on *TRADE* is positive and statistically significant at the 1 percent level in models (4) and (5) but becomes negative and statistically insignificant when country fixed-effects are included, as they are in model (6). The estimated coefficient on *LYP* is never statistically significant and alternates sign. The coefficients on the chamber size variables are contrary to what was observed by Bradbury and Crain (2001). Whereas Bradbury and Crain observed a coefficient on upper chamber size that alternated sign and a coefficient on lower chamber size that was consistently positive, we observe the opposite. The positive coefficient on upper chamber size provides the only consistent support for the Law of $1/n$. However, the coefficient is only statistically significant when country fixed-effects are added. While the coefficient on upper chamber size is also small it is larger in magnitude than the coefficient on lower chamber size. This is contrary to what we should expect, given that several upper chambers in the sample possess little to no power over fiscal policy.

Table 2.8: Effect of Chamber Size on *CGEXP* in the Presence of a Common Shock

$UNIMEAN^{+\sigma}$	1.09	$LOWERMEAN^{+\sigma}$	0.44	$UPPERMEAN^{+\sigma}$	1.83
$UNIMEAN^{-\sigma}$	0.91	$LOWERMEAN^{-\sigma}$	1.56	$UPPERMEAN^{-\sigma}$	0.17
$UNISIZE^{75th}$	1.24	$LOWERSIZE^{75th}$	0.41	$UPPERSIZE^{75th}$	1.08
$UNISIZE^{25th}$	0.92	$LOWERSIZE^{25th}$	1.39	$UPPERSIZE^{25th}$	0.42

$UNIMEAN^{+\sigma(-\sigma)}$ refers to a unicameral legislature sized one standard deviation above (below) the average unicameral legislature. $UNISIZE^{25(75)}$refers to a unicameral legislature at the 25th (75th) percentile of the unicameral distribution. $LOWERMEAN^{+\sigma(-\sigma)}$ refers to a bicameral legislature with an upper chamber of average size and a lower chamber that is one standard deviation above (below) the average lower chamber. $LOWERSIZE^{25(75)}$ refers to a bicameral legislature with an upper chamber of average size and a lower chamber that is at the 25th (75th) percentile of the lower chamber distribution. $UPPERMEAN^{+\sigma(-\sigma)}$ refers to a bicameral legislature with a lower chamber of average size and an upper chamber that is one standard deviation above (below) the average upper chamber. $UPPERSIZE^{25(75)}$ refers to a bicameral legislature with a lower chamber of average and an upper chamber that is at the 25th (75th) percentile of the upper chamber distribution.

Table 2.8 shows how we should interpret the results from the estimation of equations 2.1 and 2.2. We will restrict the discussion to bicameral legislatures. If an adverse common shock caused *CGEXP* in a country with the average legislature to increase by one percentage point, what would be the effect in a country with the average upper chamber size but a lower chamber size that was one standard deviation above the cross-country mean? Calculating $(1 + \gamma LOWERMEAN^{+\sigma})$, which corresponds to $LOWERMEAN^{+\sigma}$ in Table 2.8, yields the answer. We could expect the response to be a .44 percentage point increase in *CGEXP* in a legislature corresponding to $LOWERMEAN^{+\sigma}$. In other words, a legislature with this chamber composition would increase *CGEXP* by .56 percentage points less than the average-sized bicameral legislature. Conversely, a legislature corresponding to $LOWERMEAN^{-\sigma}$, that is, a legislature with the average-sized upper chamber but a lower chamber whose size is one standard deviation below the mean, would experience an increase in *CGEXP* of 1.56 percentage points, or, a .56 percentage point increase over the average legislature. Overall, the range between $LOWERMEAN^{+\sigma}$ and $LOWERMEAN^{-\sigma}$ is 1.12 percentage points. We also compare the average bicameral legislature to one with the average-sized upper chamber but with a lower chamber whose size is either at the 25th percentile of the lower chamber distribution ($LOWERSIZE^{25}$) or the 75th percentile of the lower chamber distribution ($LOWERSIZE^{75}$). A shock that caused a one percentage point increase in *CGEXP* in the average-sized bicameral legislature would be associated with a 1.39 percentage point increase in a legislature corresponding to $LOWERSIZE^{25}$ and a .41 percentage point increase in a legislature corresponding to $LOWERSIZE^{75}$.

Regarding upper chambers, a shock that caused *CGEXP* to increase by 1 percentage point in the country with the average bicameral legislature would be associated with an increase of only .17 percentage points in a legislature corresponding to $UPPEPRMEAN^{-\sigma}$, while a legislature with the composition of $UPPEPRMEAN^{+\sigma}$ would be associated with a 1.83 percentage point increase in *CGEXP*. The range between $UPPEPRMEAN^{+\sigma}$ and

$UPPEPRMEAN^{-\sigma}$ in Table 2.8 is by far the widest range for any of the legislative chambers with a 1.66 percentage point difference. We again perform a similar analysis comparing the average bicameral legislature with those containing an average-sized lower chamber but an upper chamber whose size is either at the 25th percentile ($UPPERSIZE^{25}$) or the 75th percentile ($UPPERSIZE^{75}$) of the upper chamber distribution. A legislature with the composition of $UPPERSIZE^{25}$ would experience an increase in *CGEXP* of .42 percentage points while a legislature whose size corresponds to $UPPERSIZE^{75}$ would experience an increase in *CGEXP* of 1.08 percentage points.

Overall, the results presented in Tables 2.7 and 2.8 reveal mixed evidence in support of the Law of $1/n$. We are unable to verify a relationship between unicameral legislature size and *CGEXP*. We find a significant relationship between lower chamber size and *CGEXP* but this relationship is, again, negative and contrary to prior evidence. Furthermore, the negative and statistically significant point estimate only appears when country fixed-effects are added. We find consistent evidence verifying that the relationship between *CGEXP* and upper chamber size is positive. This provides the only clear support for the Law of $1/n$.

Along with averaging the data over five year intervals, we also average the data over three and ten year intervals as well.[26] We experiment with these different intervals in case certain unobserved political cycles are influencing the data. In general, results are fairly robust across the different specifications. However, regressions with data averaged over three year intervals are the most comparable to the results presented in Table 2.7.[27]

The coefficient on *UNISIZE* behaves similarly across the different time specifications. Recall that the coefficient on *UNISIZE* in column 1 of Table 2.7 was .0157 and significant at the 10 level. In the same regression with data averaged over three year intervals the coefficient

[26] These results are available upon request.

[27] Averaging over ten year intervals greatly reduces the sample size to the point where there are only sixty-four panel observations in the unicameral sample and eighty-three panel observations in the bicameral sample.

is .016 and is now significant at the 5 percent level. When year interactions are added the coefficient is no longer significantly different from zero in either case. when country fixed-effects were added in Table 2.7, the coefficient on *UNISIZE* was not statistically different from zero. When data are averaged over three year intervals, the coefficient remains positive (.00097) but is still not statistically significant.

The behavior of the coefficient on *LOWERSIZE* in column 4 of Table 2.7 was positive (.0035) but was not statistically different from zero. When data are averaged over three year intervals the coefficient remains positive (.0025) and is still statistically insignificant. when year interactions are added the coefficient is positive (as in Table 2.7) but not statistically different from zero. Finally, including interactions and country fixed-effects produces a negative coefficient that is statistically different from zero at the 1 percent level. In Table 2.7 the coefficient was -.00338 and with data averaged over three year intervals the coefficient is -.0035.

The coefficient on *UPPERSIZE* is positive across all three specifications regardless of how the data are averaged. In column 4 of Table 2.7 its coefficient was .0207 but was not statistically significant. Its counterpart when data are averaged over three year intervals is .0225, which is significant at the 5 percent level. When year interactions were added in Table 2.7 the coefficient of .0028 was statistically insignificant. This point estimate does not change when data are averaged over three year intervals and it remains statistically insignificant. Lastly, when country fixed-effects are added the coefficients are essentially the same (.009) and both are significant at the 1 percent level.

Table 2.9: Central Government Expenditures, Chamber Size, and Fiscal Power

	(1)	(2)	(3)	(4)	(5)	(6)
	CGEXP	*CGEXP*	*CGEXP*	*CGEXP*	*CGEXP*	*CGEXP*
CONSTANT	25.39**	25.18**	31.29	3.990	24.80	5.453
	(11.39)	(12.28)	(19.58)	(17.20)	(18.60)	(12.93)
TRADE	0.116***	0.106***	0.0399	0.166***	0.171***	0.0698**
	(0.0211)	(0.0205)	(0.0300)	(0.0286)	(0.0268)	(0.0324)
LYP	1.133	0.516	-1.920	-0.756	-3.239*	0.359
	(1.063)	(1.338)	(2.468)	(1.676)	(1.859)	(1.452)
PROP65	1.668***	1.717***	2.064***	0.416	0.701*	0.803*
	(0.241)	(0.264)	(0.344)	(0.375)	(0.399)	(0.422)
PROP1564	-0.492**	-0.436**	-0.230	0.135	0.137	0.0108
	(0.224)	(0.216)	(0.214)	(0.334)	(0.331)	(0.168)
UNISIZE	0.00453	0.000259	0.00000711			
	(0.00497)	(0.00126)	(0.000762)			
LOWERSIZE				0.00654	0.00527	-0.00320
				(0.00918)	(0.00382)	(0.00198)
UPPERSIZE				0.0284*	0.00717	0.0184***
				(0.0162)	(0.00624)	(0.00577)
Observations	173	173	173	115	115	115
Adjusted R^2	0.557	0.564	0.882	0.417	0.439	0.944
Country FE	N	N	Y	N	N	Y
Year Interactions	N	Y	Y	N	N	Y

Standard Errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

These findings lend credence to the idea that across countries, the previously verified positive relationship between legislature size and spending is not robust to differences in the datasets and empirical strategies that have been used to estimate this relationship. The inability to verify this relationship, coupled with the theoretical objections discussed in Section II, gives us reason to question the acceptance of the Law of $1/n$ as an undoubted political fact. However, we observe, once again, a positive relationship between upper chamber size and *CGEXP*. Through the various robustness checks we employ, this positive relationship persists.

Results using the alternative definition of bicameralism are shown in Tables 2.9 and 2.10. Recall that in this section, we consider a legislature to be bicameral if both chambers possess the ability to affect fiscal policy. The presentation of the models in Table 2.9 is identical to how they were presented in Table 2.7. Across models (1) - (3), the economic and demographic control variables behave similarly to how they did in Table 2.7. The primary focus is on the behavior of *UNISIZE*. The estimated coefficient remains positive but is now statistically insignificant across all three specifications.

In models (4) - (6) the control variables do not achieve the same level of consistency. The point estimates on *LOWERSIZE* and *UPPERSIZE* also exhibit some differences depending upon how one defines a bicameral legislature. Regarding *LOWERSIZE*, the coefficient exhibits the same behavior as in Table 2.7 but the point estimate is now statistically insignificant (p-value = .11) in model (6). Across models (4) through (6) the magnitude of the coefficients on *LOWERSIZE* is larger in Table 2.9 than in Table 2.7, though the differences are very small. The coefficient on *UPPERSIZE* remains positive and is larger than what was estimated in the corresponding models from Table 2.7. This last result is not too surprising. It simply reflects the fact that all upper chambers in the redefined bicameral sample have constitutional authority to affect fiscal policy. Also, the estimated coefficient on *UPPERSIZE* is now statistically significant in model (4) as well.

Table 2.10: Effect of Chamber Size on *CGEXP* in the Presence of a Common Shock

$UNIMEAN^{+\sigma}$	1.001	$LOWERMEAN^{+\sigma}$	0.52	$UPPERMEAN^{+\sigma}$	2.51
$UNIMEAN^{-\sigma}$	0.9989	$LOWERMEAN^{-\sigma}$	1.48	$UPPERMEAN^{-\sigma}$	-0.51
$UNISIZE^{75th}$	1.002	$LOWERSIZE^{75th}$	-0.27	$UPPERSIZE^{75th}$	1.17
$UNISIZE^{25th}$	0.9989	$LOWERSIZE^{25th}$	1.58	$UPPERSIZE^{25th}$	0.06

$UNIMEAN^{+\sigma(-\sigma)}$ refers to a unicameral legislature sized one standard deviation above (below) the average unicameral legislature. $UNISIZE^{25(75)}$refers to a unicameral legislature at the 25th (75th) percentile of the unicameral distribution. $LOWERMEAN^{+\sigma(-\sigma)}$ refers to a bicameral legislature with an upper chamber of average size and a lower chamber that is one standard deviation above (below) the average lower chamber. $LOWERSIZE^{25(75)}$ refers to a bicameral legislature with an upper chamber of average size and a lower chamber that is at the 25th (75th) percentile of the lower chamber distribution. $UPPERMEAN^{+\sigma(-\sigma)}$ refers to a bicameral legislature with a lower chamber of average size and an upper chamber that is one standard deviation above (below) the average upper chamber. $UPPERSIZE^{25(75)}$ refers to a bicameral legislature with a lower chamber of average and an upper chamber that is at the 25th (75th) percentile of the upper chamber distribution.
For each legislature with a composition as described above, the corresponding value indicates the percentage point change in *CGEXP* that would result from a shock that caused *CGEXP* in a country with the average unicameral or bicameral legislature to increase by one percentage point.

Using results from Table 2.9 we also perform the exercise from Table 2.8 and compare how specific chamber sizes are expected to influence *CGEXP* in the presence of a common shock that caused *CGEXP* to increase by one percentage point in the average-sized legislature. These results are presented in Table 2.10 above. Recall that now we only consider a legislature to be bicameral if both chambers have the ability to influence fiscal policy. Since upper chamber size is now the only chamber size variable that exhibits a statistically significant relationship with *CGEXP* the discussion will be limited to this variable. Once we control for fiscal power, upper chambers corresponding to $UPPERMEAN^{+\sigma}$ are associated with an increase in *CGEXP* of 2.51 percentage points, compared with an increase of 1.83 percentage points when fiscal power was not accounted for. In Table 2.8, a legislature corresponding to $UPPERMEAN^{-\sigma}$ was associated with an increase in *CGEXP* of .17 percentage points. However, in Table 2.10, when we only observe upper chambers that can significantly affect fiscal policy, the change in *CGEXP* for an upper chamber of similar composition is -.51. If we compare $UPPERSIZE^{25}$ across Tables 2.8 and 2.10 we observe an increase in *CGEXP* of .42 percentage points when fiscal policy is not accounted for but only a .06 percentage

point increase when all upper chambers possess the power to affect fiscal policy. Similarly, when comparing $UPPERSIZE^{75}$, the change in $CGEXP$ when fiscal policy is not accounted for is 1.08 percentage points, compared to an increase of 1.17 percentage points when fiscal power is accounted for.

IV.3 Country-Specific Shocks

Does the Law of $1/n$ hold with respect to certain country-specific shocks? To answer this question we will use data on country-specific output gaps. Several authors have studied the cyclical variations of government expenditures. Gavin and Perotti (1997) find that government expenditures tend to be somewhat procyclical during periods of economic growth and significantly countercyclial during economic contractions. Persson and Tabellini (2003) present evidence showing that the fiscal response to output shocks depends upon certain constitutional variables. They show that spending in countries with a parliamentary form of government and proportional representation is significantly countercyclical. Specifically, they find that a five percent decline in real income is associated with a one percent increase in central government spending (as a percentage of GDP). Their results also show spending in countries with a presidential form of government to be acyclical.

By decomposing output deviations into positive and negative gaps, Persson and Tabellini (2003) identify an asymmetry similar to that discovered by Gavin and Perotti (1997). Hercowitz and Strawczynski (2004) show evidence that this asymmetry in spending leads to an upward "cyclical ratcheting" effect (ibid; p. 353). That is, countercyclical spending during contractions, coupled with pro- or acyclical spending during periods of growth, leads to long run increases in government spending. Buchanan and Yoon (2002) explain this effect as a result of interest group politics. With greater tax revenues during periods of economic growth, lobbying from interest groups will make it increasingly difficult to wind down recessionary spending that was intended to stimulate economic output. The result is that countercyclical

spending is never truly reduced.

Recessionary spending is often subject to the desires of legislators. Some will seek to direct spending to their districts or towards avenues that benefit their constituents. If the Law of $1/n$ holds then the response of government expenditures to output shocks should depend upon the size of the legislature. Specifically, larger chambers should be positively related to larger spending increases during periods of countercyclical spending.

As in Section IV.1, we will employ the methodology used by Milesi-Ferretti, Perotti and Rostagno (2002) and Persson and Tabellini (2003). The general form our regressions will take is

$$PCGEXP_{it} = \alpha_i + \phi GDPGAP_{it} + \gamma UNISIZE_{it} * GDPGAP_{it} + \boldsymbol{\beta X_{it}} + \boldsymbol{\delta_t} + \epsilon_{it}. \quad (2.3)$$

$$\begin{aligned} PCGEXP_{it} = \alpha_i + \phi GDPGAP_{it} + \gamma_1 LOWERSIZE_{it} * GDPGAP_{it} \\ + \gamma_2 UPPERSIZE_{it} * GDPGAP_{it} + \boldsymbol{\beta X_{it}} + \boldsymbol{\delta_t} + \epsilon_{it}. \end{aligned} \quad (2.4)$$

With the exception of *PCGEXP*, *GDPGAP* and its interactions, all variables should be interpreted as they were in equations 2.1 and 2.2. The term $\boldsymbol{\delta}$ is a vector of period-specific dummy variables. The variable *GDPGAP* measures the difference between the natural logarithm of GDP and its country-specific trend, also referred to as the output gap. Persson and Tabellini (2003) and Brender and Drazen (2005) use the Hodrick-Prescott filter to generate the country-specific trends. Interacting chamber size with country-specific shocks to GDP allows us to test whether the response of spending to output shocks is conditional upon chamber size. The variable *GDPUNI* measures the interaction between unicameral chamber size and the output gap, *GDPLOWER* measures the interaction between lower chamber size and the output gap, and *GDPUPPER* measures the interaction between upper chamber size and the output gap. If common pool problems and the Law of $1/n$ are relevant, we should expect to see a larger spending response to the output gap in countries with larger

legislatures.

The variable *PCGEXP* is defined as central government expenditures as a percentage of potential GDP. This variable is used in place of *CGEXP* in order to ensure that any inverse relationship between *GDPGAP* and spending is actually due to reduced spending and not simply because of changes in GDP. More specifically, if in a given year GDP was larger than potential GDP but central government expenditures remained unchanged, then an inverse relationship between *GDPGAP* and *CGEXP* would still exists since GDP, the denominator in *CGEXP*, had grown larger.

The variable *PCGEXP* is not directly available from Brender and Drazen (2005). However, we are able to construct this variable using the spending and GDP data they have available and following their method for constructing the variable *GDPGAP*.

All variables are averaged over 5-year intervals and the variables *GDPGAP*, *UNISIZE*, *LOWERSIZE*, and *UPPERSIZE* enter into the regressions as deviations from the cross-country mean. Thus, for a legislature of average size, *GDPGAP* measures the relationship between spending and deviations from trend GDP. Each regression includes country and year fixed-effects and the sample is again divided by legislative structure.

IV.4 Country-Specific Shocks: Results

Results from the estimation of equations 2.3 and 2.4 are reported in Table 2.11.[28] Model (1) is estimated using the unicameral sample and model (2) is estimated using the unicameral sample when accounting for fiscal power. Similarly, model (3) is estimated using the bicameral sample and model (4) is estimated using the bicameral sample, again, accounting for power over fiscal policy.

[28] Classical standard errors are reported. Again, this is primarily done because the number of countries in each subsample is too low to justify clustering. We do test for heteroscedasticity using the Breusch-Pagan LM test. For both the unicameral and bicameral samples, the null hypothesis of homoscedasticity is never rejected. However, serial correlation is probably still a concern.

Table 2.11: Central Government Expenditures, Chamber Size, and Output Gaps

	(1) *PCGEXP*	(2) *PCGEXP*	(3) *PCGEXP*	(4) *PCGEXP*
CONSTANT	-9.806	23.26	35.37	-5.775
	(34.92)	(30.69)	(25.39)	(27.36)
TRADE	0.0992**	0.0136	-0.0411	0.0879*
	(0.0479)	(0.0375)	(0.0348)	(0.0442)
LYP	0.828	-1.182	0.305	1.245
	(3.722)	(3.300)	(2.071)	(2.265)
PROP65	1.586**	1.191**	0.343	1.001
	(0.693)	(0.561)	(0.520)	(0.628)
PROP1564	0.152	-0.0299	-0.329	0.0551
	(0.309)	(0.277)	(0.275)	(0.292)
GDPGAP	-0.285	-0.366	-0.567	-0.0617
	(0.405)	(0.342)	(0.354)	(0.452)
UNIGAP	-0.0000277			
	(0.00339)			
UNI1GAP		-0.00152		
		(0.00204)		
LOWERGAP			0.0123***	
			(0.00438)	
UPPERGAP			-0.0286***	
			(0.00788)	
LOWER1GAP				0.0154***
				(0.00487)
UPPER1GAP				-0.0383***
				(0.00828)
Observations	129	173	161	115
Adjusted R^2	0.852	0.861	0.902	0.913

Standard Errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

Across both unicameral models, the relationship between *PCGEXP* and GDPGAP is negative but is never statistically significant. Similarly, the point estimates on both *UNIGAP* and *UNI1GAP* are negative but not statistically significant. The point estimates associated with these variables support the idea that countries engage in countercyclical spending and those with larger than average unicameral chambers engage in even more countercyclical spending. However, because the standard errors are so large we cannot reject the null

hypotheses that central government expenditures are acyclical and do not depend upon legislature size.

Models (3) and (4) show some evidence that larger than average upper chambers are associated more spending in the presence of a specific shock to GDP. Still, the point estimate on *GDPGAP*, while negative, is not statistically different from zero. Thus, for a country with the average-sized bicameral legislature, we cannot reject the null hypothesis that central government expenditures as a percentage of potential GDP is acyclical. The negative point estimate on *UPPERGAP* supports the conclusion that a legislature with an upper chamber that is larger than average will engage in countercyclical spending that encompasses a larger percentage of potential GDP as the size of the upper chamber grows larger. When the bicameral sample is restricted to include only those legislatures where both chambers possess power over fiscal policy the point estimate on the interaction term *UPPER1GAP* is larger in magnitude and remains statistically significant. Thus, legislatures with upper chambers that are legally able to impact fiscal policy will experience an increase spending as a percentage of potential GDP as the upper chamber grows larger.

The point estimates on *LOWERGAP* and *LOWER1GAP* are positive and smaller in magnitude that the point estimates on *GDPGAP* for both models (3) and (4). Thus, the marginal effect on *PCGEXP* of a shock to *GDPGAP* (i.e. $\frac{\partial(PCGEXP|LOWERSIZE)}{\partial GDPGAP}$) is countercyclical for legislatures with larger than average lower chambers but the countercyclical response is dampened as the lower chamber grows larger. This finding is in line with the results from Tables 2.8 and 2.9, where it was shown that larger than average lower chambers are associated with *less* spending as a percentage of GDP in the presence of common shocks.

V Conclusion

Empirical tests of the Law of $1/n$ have yielded mixed results regarding the relationship between legislature size and government spending. this paper is an attempt to clarify this literature by using a methodology that allows us to control for unobserved heterogeneity despite the limited variability of legislature size. Specifically, we examine whether legislature size influences the growth of government spending. The results indicate that a positive relationship between upper chamber size and government spending exists. Furthermore, the magnitude of this relationship is stronger once impotent upper chambers are removed from the sample. We are unable to consistently verify relationships between unicameral size and spending and between lower chamber size and spending. The magnitude and direction of these relationships tends to change depending upon the model specification and how bicameralism is defined. In most cases, though, the estimated coefficient on unicameral size and lower chamber size is not statistically different from zero.

When power over fiscal policy is controlled for, the effect of upper chamber size on spending is consistently positive and larger than what we observed when fiscal power is not a factor in the definition of bicameralism. While this finding is counter to Bradbury and Crain (2001) findings, it make sense if we consider the political process involved in the passage of spending legislation. In each bicameral legislature where both chambers have power over fiscal policy, spending bills originate in the lower chamber and must also pass the smaller upper chamber. In this bargaining arrangement, the upper chamber will act as the binding constraint. The smaller upper chambers, which represent larger and therefore fewer districts, will prefer less spending compared to their larger counterparts in the lower house. Thus, spending bills that originate in the lower chamber will have to account for this preference for less government spending. It is in this sense that the upper chamber is the binding constraint.

We also examine the relationship between chamber size and government spending in the presence of country-specific deviations from trend GDP. However, unlike the spending variable used in the first analysis of this paper and in the previous literature, central government expenditures are measured as a percentage of *potential* GDP. This is done in order to guard against spurious results that are due to fluctuations in GDP rather than actual changes in government spending. Regarding unicameral legislatures, chamber size does not appear to have a significant effect on the spending response to output gaps and we cannot reject the null hypothesis that spending is acyclical. In bicameral legislatures, there is some evidence that a larger lower chamber size has a dampening effect on countercyclical spending. This finding conforms to the behavior exhibited by lower chambers in the presence of common shocks when country fixed-effects are controlled for. We find a negative point estimate on the interaction between upper chamber size and the output gap, which indicates that growth in the upper chamber is associated with higher levels of countercyclical spending. Once again, this finding is consistent with the behavior of upper chambers in the presence of common shocks.

Above all, we find that the existing empirical evidence supporting a positive relationship between legislature size and government spending does not hold up in the presence of substantial changes to the analysis. Changing the measure of government spending, accounting for the fiscal power of upper chambers, controlling for time invariant observed and unobserved country-specific characteristics, and extending the data set yield results that offer a different account of how legislature size impacts fiscal policy.

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I Description of Polity GT

Polity GT is an interpolation based on a measure of democracy known as Polity and another known as the Gastil Index.[29] Persson and Tabellini (2003) created this index to overcome the exclusion of some countries from the Polity IV index. Their interpolation rescales Polity scores to match the unit of measure used by the Gastil Index. The Polity Index assigns each country an integer score in the range [-10, 10]. Positive and larger scores are associated with better functioning democracies. The Gastil index assigns each country a score within the range [1, 7], where 1 denotes the most free and 7 denotes countries that aren't free. Countries with a score between [1, 2.5] are "Free," those with a score between [3, 5] are "partly free," and, finally, countries with a score between (5.5, 7) are "not free."[30]

II Variables,Definitions, and Data Sources

CGEXP: Central government expenditures as a percentage of GDP. Source: Brender and Drazen (2005).

PCGEXP: Central government expenditures as a percentage of potential GDP. Source: Author's elaboration using data from Brender and Drazen (2005)

LYP: Natural Logarithm of Real Per-Capita GDP. Source: Brender and Drazen (2005).

TRADE: The sum of exports and imports divided by GDP. Source: Brender and Drazen (2005).

GDPGAP: The difference between the natural logarithm of real GDP and its country-specific trend. Source: Brender and Drazen (2005).

[29]Polity is a measure of democracy that is produced by The Polity IV Project. It is derived from a combination of two separate variables that attempt to measure "institutionalized democracy" and "institutionalized autocracy," respectively (Polity IV user manual). The Gastil Index, reported by Freedom House, is an average of scores that measure "political freedom" and "civil rights," respectively.

[30]Source: Freedom in the World 2012, page 35 (`http://www.freedomhouse.org/sites/default/files/inline_images/FIW\%202012\%20Booklet--Final.pdf`)

PROP1564: The proportion of the population aged between 15 and 64. Source: Persson and Tabellini (2003).

PROP65: The proportion of the population aged above 65. Source: Persson and Tabellini (2003).

BICAM : Dummy variable equal to 1 if the legislature contains two chambers and 0 if the legislature is composed of a single chamber. Sources: POLCON Dataset Henisz (2010) and Beck, Clarke, Groff, Keefer and Walsh (2001).

UNISIZE: Size of unicameral legislature. Sources: Inter-Parliamentary Union (IPU) PARLINE Database (`http://www.ipu.org/parline/`) and The Statesman's Yearbook, Various Issues. St. Martin's Press, New York.

LOWERSIZE: Size of the lower chamber in a bicameral legislature. Sources: Inter-Parliamentary Union (IPU) PARLINE Database (`http://www.ipu.org/parline/`) and The Statesman's Yearbook, Various Issues. St. Martin's Press, New York.

UPPERSIZE: Size of the upper chamber in a bicameral legislature. Sources: Inter-Parliamentary Union (IPU) PARLINE Database (`http://www.ipu.org/parline/`) and The Statesman's Yearbook, Various Issues. St. Martin's Press, New York.

Chapter 3

Government Spending, Shocks, and the Role of Legislature Size: Evidence from the American States

I Introduction

Throughout history, voters have supported a number of different ways in which to restrain the power of elected state officials. These restraints mainly include balanced budget amendments, line-item veto provisions, and gubernatorial and legislative term limits. While one can legitimately question the effectiveness or success of these measures, their popularity among voters is of little doubt. Nearly all state governments are restrained by a balanced budget amendment, Vermont being the lone exception, while forty-three states give their governor the power to strike certain lines from proposed bills. Thirty-six states limit the number of terms the governor can serve and fifteen states impose term limits on state legislatures.[1] Historically, voters have attempted to restrain government officials through controlling how or how long they can serve. In response to the most recent financial crisis and ensuing recession legislators in some states have proposed limiting *how many* can serve. In 2011,

[1] Information on legislative term limits and balanced budget amendments is from the National Conference of State Legislatures (http://www.ncsl.org/legislatures-elections/legisdata/chart-of-term-limits-states.aspx). Information on line-item veto and gubernatorial term limits is from various editions of The Book of the States.

The Wall Street Journal reported on five state legislators who had proposed reducing the number of legislators serving in both houses of their state legislature, each citing the need to reduce state-level spending as their primary motivator.[2,3] More recently, a current voter initiative in California proposes to increase the size of California's legislature from the current size of 120 seats to a little less than 12,000.[4]

In the wake of renewed interest in reducing the size of state legislatures, we revisit the scholarly analysis of the fiscal effects of legislature size. Studies that attempt to empirically estimate the effect of legislature size on government spending have largely focused on American state legislatures. While state legislature size is not as time invariant as deep constitutional characteristics such as form of government or electoral rules, changes to the size of a state's legislature are very infrequent. This characteristic imposes upon the researcher a significant estimation difficulty. Legislature size cannot be analyzed in a state level fixed-effects model because most states rarely alter the size of the legislature. However, the little variability that does exist precludes the use of two-stage fixed effects models that allow for consistent estimation of time-invariant variables. These issues have forced economists and political scientists to mostly rely upon inferences from methods that either pool time-series and cross-sectional data or apply fixed-effects methods to data from historical cross-sections at either five- or ten-year intervals. Both estimation strategies have shortcomings. Pooled time-series cross-sectional data ignores potential state-level unobserved heterogeneity while panels covering cross-sections at predetermined intervals prevent researchers from observing year-on-year changes in fiscal policy.

We employ an estimation strategy first proposed by Blanchard and Wolfers (2000) that allows us to circumvent these econometric issues and estimate the relationship between

[2]The states were Connecticut, Kansas, Maine, Minnesota, and Pennsylvania.

[3]Connor Dougherty, "State Lawmakers Aim to Reduce Ranks," *The Wall Street Journal*, March 4, 2011.

[4]Alejandro Lazo, "In California, a bid to Radically Overhaul Government," *The Wall Street Journal*, December 27, 2013.

legislature size and government spending growth using annual data. Specifically, we will estimate the relationship between legislature size and changes in various types of spending in the presence of shocks that are common to all states. This methodology, which, up to now, has been used to estimate various relationships at the cross-country level, is ideally suited for similar examinations at the state level. Chen and Malhotra (2007) highlight the advantages of testing this relationship on state legislatures. While cultural, economic, and ethnic characteristics, as well as political and governmental characteristics can vary widely across countries and national governments, these differences are minimized with respect to the American states. State-level data also has the advantage of being reliable. A final component of this analysis submits Chen and Malhotra's "Law of k/n" to greater empirical scrutiny. The Law of k/n is a recent and innovative theory concerning how a legislature's lower-to-upper chamber size ratio affects state-level government spending.

The results using this methodology show no statistically significant evidence of a positive relationship between legislature size and the first difference of total spending per capita for legislatures that deviate from the sample average. We also find no statistically significant evidence of a positive relationship between legislature size and state level highway spending per capita. In fact, the only statistically significant evidence supporting the "Law of $1/n$ (Weingast, Shepsle and Johnsen (1981))" is a positive relationship between lower chamber size and the first difference of welfare spending per capita. Lastly, we find an inverse relationship between the first difference of total spending per capita and lower-to-upper chamber size, as predicted by Chen and Malhotra (2007) but the estimated coefficient is not statistically significant.

The focus of Section II concerns the literature on the relationship between legislature size and government spending at the state level.[5] This literature has already received an

[5]The first essay of this dissertation contains a more thorough literature review that discusses cross-country and local analysis as well.

extensive review in the first essay of this dissertation. Thus, attention will be devoted primarily to empirical examinations that study American state legislatures. The purpose of this section will be to inform the current analysis of the political, economic, and demographic characteristics that have been of particular interest to previous studies. Section III will discuss the data to be used in this paper. As Section II will make clear, a wide variety of variables are used by different authors. Therefore, Section III will also be used to make the case for the variables that are included in the current analysis, and explain why certain variables will not be included. Section IV will outline the methodology to be used in this essay and address the specific research question. Again, this methodology has been discussed in some detail in the previous essay. Thus, the current focus will be how the methodology relates to the current topic. Section VI includes a presentation of the results followed by a discussion. Section VII will conclude.

II Literature Review

The study of the relationship between legislature size and government spending is rooted in the "Law of $1/n$," the key result from Weingast et al. (1981) seminal article on the political economy of distributive spending. Weingast, et al. model distributive spending policy in a unicameral legislature composed of single-member districts that operate under the principle of universalism. If $b(x)$ denotes district-specific economic benefits from a particular spending policy and $c(x)$ encompasses all costs associated with the policy, then the efficient project size, x^E, solves $b'(x) = c'(x)$. However, in a legislature with n representatives, Weingast, et al. assume that legislators will only account for benefits stemming to his or her district and the tax share faced by the district. If the tax share is decreasing in the number of districts, i.e. $t = (1/n)$, then the politically optimal project size for each district, x^P, is the one that solves $b'(x) = (1/n)c'(x)$. Since each district is only responsible for financing a fraction of

the total cost of its project, $x^P > x^E$. Moreover, when universalism holds, each district will receive x^P and the total amount of government spending will be larger than the economically efficient level.

Primo and Snyder (2008) revisit the original formulation of the Law of $1/n$ and offer several clarifying points regarding its interpretation. Most importantly, they show that Weingast et al. (1981) model does not account for how a legislative district's population changes when the number of districts increases. An increase in the number of districts will give each legislator the incentive to ask for larger spending projects for his or her district. However, as the number of districts increases, the population in each district decreases. This effect will reduce the political benefits from increases in spending project size and, in their model, directly offsets the incentive to increase project size. Primo and Snyder (2008) note, though, that despite no change in the efficient project size, it is still possible for total spending to increase if the number of projects is increasing in the number of districts. The authors also derive the theoretical possibility of a "reverse law of $1/n$ (ibid: 478)." To give an idea of how such a phenomenon could arise, the authors consider a geographic area carved into several smaller districts. Some districts may opt for a project with a lower total cost but a larger in-district benefit, as opposed to a project where the total cost would be larger, but where the in-district benefits would be lower.

Gilligan and Matsusaka (1995) were the first to examine the Law of $1/n$ in the context of American state legislatures. With data covering forty-eight states at seven historical cross-sections covering the period 1960 - 1990, the authors found a positive relationship between upper chamber size and various measures of state-level per-capita government expenditures, which included general government expenditures (including local spending), capital and non-capital expenditures, and welfare, education, and highway spending.[6] With respect

[6]In studies of state-level fiscal policy, Alaska is often omitted as an outlier because of its extremely high level of per-capita expenditures that are possibly due to its abundance of natural resources. Nebraska is also omitted because its state legislature has a unicameral composition.

to general spending, the authors estimated an additional upper chamber seat to be worth between \$9.87 and \$10.91 per capita. The estimated effect of an additional upper chamber seat was \$1.70 per capita for welfare spending, \$2.29 for education spending, and \$1.64 for highway spending. The effect of lower chamber size was ambiguous and never statistically different from zero. These findings were puzzling to the authors, who were unable to offer an explanation for the results. Nonetheless, Gilligan and Matsusaka interpreted their results as support for the Law of $1/n$. The authors also tested how the political composition of a state's legislature affected fiscal policy. Specifically, the authors controlled for the party of a state's governor, which party controlled each chamber of the legislature, the percentage of seats in each chamber held by Democrats, alignment of the governor and the majority party in the legislature, and a measure of legislative competition. In general, none of the political variables were robust across the authors' different regression specifications. We will give careful consideration to which, if any, of these political variables should be included in the current analysis.

Gilligan and Matsusaka (2001) apply the analysis of Gilligan and Matsusaka (1995) to the first half of the twentieth century. Results from the period 1902 – 1942 confirm their previous finding that upper chamber size has a positive and significant effect on state-level fiscal policy. However, they are again unable to verify that partisan characteristics of state legislatures have an independent effect on fiscal policy.

Primo (2006) studies how exogenous budgeting rules affect state government spending. His empirical model relies upon data from forty-seven of the fifty states covering the period 1969 - 2000. Thus, his data set displays considerable overlap with Gilligan and Matsusaka (1995). Even though the variables of interest in Primo's (2006) model do not include chamber size, he follows Gilligan and Matsusaka (1995, 2001) and includes lower and upper chamber size as explanatory variables. While Primo (2006) verifies a positive and statistically significant relationship between real state and local expenditures per-capita and upper chamber

size, he finds evidence of a negative and statistically significant relationship between per-capita expenditures and lower chamber size. This last finding was contrary to results found by Gilligan and Matsusaka (1995, 2001). Similar to Gilligan and Matsusaka, Primo (2006) found partisan characteristics of the state government to have little overall effect on the total amount of per-capita spending. Whereas Gilligan and Matsusaka (1995, 2001) controlled for state fixed-effects, Primo (2006) is unable to do so because the primary budgeting rules he is interested in estimating are perfectly collinear with state-fixed effects over the entire sample period.

Chen and Malhotra (2007) extend the Law of $1/n$ to account for bicameral bargaining. Their model accounts for the fact that within the American states, lower chamber districts are geographically ensconced within upper chamber districts. The authors assume that spending projects are divisible at the upper chamber level but not the lower chamber level. Thus, the smaller lower chamber districts receive positive spill-overs from spending bills generated at the upper chamber level.[7] However, as the number of lower chamber districts increases relative to the number of upper chamber districts, lower chamber representatives will receive a smaller share of the benefits and thus less incentive to pursue pork barrel spending. Spending is, therefore, decreasing as the number of lower chamber districts increases relative to the number of upper chamber districts. Chen and Malhotra dub this theoretical finding the "Law of k/n," where k is the aforementioned ratio between lower and upper chamber size. The authors test their hypothesis on state-level per-capita spending over the periods 1992 – 2004 and 1964 – 2004. In both samples, per-capita total expenditures (excluding local data) are regressed on upper chamber size, the ratio of lower-to-upper chamber size, and several economic, demographic, and political controls. A positive relationship between upper chamber size and spending was confirmed and the relationship between

[7]This assumption was informed by anecdotal evidence collected from various state representative offices in Missouri and Iowa. See Chen and Malhotra (2007: 659).

k and spending was found to be negative, as hypothesized. Similar to the other studies discussed in this section, political variables such as divided control of the state government and democratic control were not significantly different from zero over either sample period. Chen and Malhotra's model is developed as a one period game with no time discounting in the spirit of the "divide-the-dollar" game from Baron and Ferejohn (1989). Furthermore, their primary empirical results measure the change in per-capita spending given an increase in lower-to-upper chamber size ratio (or an increase in upper chamber size) holding all else constant. An interesting empirical exercise would be to examine how this ratio affects the relationship between per-capita spending and certain common shocks.

Several other authors have studied the relationship between spending and legislature size in different contexts. See Bradbury and Crain (2001) for a cross-country analysis. See Baqir (2002), Bradbury and Stephenson (2003), Egger and Koethenbuerger (2010), and Pettersson-Lidbom (2012) for evidence at the local level. These studies have been discussed at length in the first essay of this dissertation.

III Data

We examine the relationship between legislature size and the fiscal response to common and state-specific shocks using yearly data on forty-seven of the fifty states over the period 1978-2008.[8] Summary statistics for all variables over the entire sample is provided in Table 1. As is customary in studies concerning state-level fiscal policy, Alaska and Hawaii are excluded as outliers. Nebraska is excluded because its legislature is unicameral.

[8]The last year of available data is 2008. Members of Minnesota's upper chamber were elected on non-partisan ballots until 1976. Thus, 1978 is chosen to ensure that the panel is balanced.

Table 3.1: Summary Statistics

Variable	Mean	SD	Min	Max
Expenditures (Per Capita, 2008 Dollars)				
Total	4299.12	1249.84	1922.56	9533.93
Direct Education	611.03	195.66	208.78	1407.63
Direct Welfare	765.49	374.92	113.01	2138.67
Direct Highway	313.88	137.59	94.36	1093.93
Economic and Demographic Control Variables				
Revenue from Federal Government (2008 Dollars)	1073.20	444.38	358.47	4075.25
Per Capita GSP (2008 Dollars, in thousands)	37.49	8.23	21.48	72.89
Percent of Population Aged 65 and Older	12.40	1.82	1.60	18.60
Population (in millions)	5.48	5.77	0.45	36.58
Political Variables				
Lower Chamber Size	113.95	54.87	40.00	400.00
Upper Chamber Size	40.20	10.14	20.00	67
Lower-to-Upper Chamber Size Ratio	3.00	2.19	1.67	16.67
Democratic Control	0.26	0.44	0	1
Divided Control	0.57	0.50	0	1

The fiscal variables of interest include per capita total state expenditures ($TOTAL$) as well as per capita direct state-level spending on welfare ($WELFARE$), education (EDU), and highway (HWY) spending. The $TOTAL$ category encompasses all expenditures budgeted by the state government and includes expenditures to local and municipal governments, and expenditures spent through state-operated liquor stores and retirement and insurance trusts. Fiscal data is collected from the State and Local Government Finances and Employment section of the Statistical Abstract of the United States Bureau (Various years). Per capita spending variables are expressed in constant 2008 dollars using the Consumer Price Index (CPI), which is obtained from the Bureau of Labor Statistics, and population data from the U.S. Census Bureau.

The primary variables of interest, lower chamber size ($LOWER$) and upper chamber size ($UPPER$), are collected from The Book of States (of State Governments, ed, Various Years). Table 2 provides summary statistics on legislature size for the individual states. Clearly, legislature size is time-invariant for the majority of states over this sample period. Between 1978 and 2008 only four states altered the composition of both legislative chambers and

only two of these states altered the composition more than once.[9] Four more states altered the composition of at least one chamber with two changing the size of a chamber more than once.[10] The limited variability of legislature size bolsters the choice of the estimation strategy that will be discussed in the next section.

Control variables include real gross state product per capita (GSP), which is to control for the possibility that wealthier states will demand more government services. Gross state product data is available from the Bureau of Economic Analysis. Real per-capita revenue from the federal government ($REVFED$) is also included. Intuitively, federal aid could generate an income effect that results in a greater demand for state-level expenditures. Also, as Gilligan and Matsusaka (1995) point out, some federal spending programs require states to match funds that are provided to the states. Thus, in the case of matched spending, higher levels of federal aid could lead to higher levels of state-level expenditure.

A positive relationship between federal aid and state-level expenditures is strongly related to the "flypaper effect" – the idea that state or local government spending will be more responsive to increases in federal aid or revenue than to increases in the private incomes of the citizens of a given locality or state.[11] Knight (2002), though, shows evidence that federal grants can in fact crowd out state government spending. Regardless of the direction of the effect, revenue from the federal government has been recognized as an important determinant of state-level spending, and is thus controlled for here.

Most studies examining the fiscal impact of state legislature size include a measure of population as a demographic control variable. The primary reason for the inclusion of this variable is to account for potential economies of scale in spending. If economies of scale

[9] Idaho (1985 and 1992), Nevada (1981), North Dakota (1982, 1992, and 2002), and Rhode Island (2002).

[10] Illinois (lower chamber, 1982), Maine (upper chamber, 1985), New York (upper chamber, 1982 and 2002), and Wyoming (lower chamber, 1982 and 1992).

[11] For a discussion of the "flypaper effect" see Oates (1999).

Table 3.2: Legislature Size by State

State		Mean	SD	Min	Max	State		Mean	SD	Min	Max
Alabama	Lower	105	0.00	105	105	Nevada	Lower	42	0.61	40	42
	Upper	35	0.00	35	35		Upper	21	0.31	20	21
Arizona	Lower	60	0.00	60	60	New Hampshire	Lower	400	0.00	400	400
	Upper	30	0.00	30	30		Upper	24	0.00	24	24
Arkansas	Lower	100	0.00	100	100	New Jersey	Lower	80	0.00	80	80
	Upper	35	0.00	35	35		Upper	40	0.00	40	40
California	Lower	80	0.00	80	80	New Mexico	Lower	70	0.00	70	70
	Upper	40	0.00	40	40		Upper	42	0.00	42	42
Colorado	Lower	65	0.00	65	65	New York	Lower	150	0.00	150	150
	Upper	35	0.00	35	35		Upper	61	0.57	60	62
Connecticut	Lower	151	0.00	151	151	North Carolina	Lower	120	0.00	120	120
	Upper	36	0.00	36	36		Upper	50	0.00	50	50
Delaware	Lower	41	0.00	41	41	North Dakota	Lower	100	4.74	94	106
	Upper	21	0.00	21	21		Upper	50	2.37	47	53
Florida	Lower	120	0.00	120	120	Ohio	Lower	99	0.00	99	99
	Upper	40	0.00	40	40		Upper	33	0.00	33	33
Georgia	Lower	180	0.00	180	180	Oklahoma	Lower	101	0.00	101	101
	Upper	56	0.00	56	56		Upper	48	0.00	48	48
Idaho	Lower	73	6.02	70	84	Oregon	Lower	60	0.00	60	60
	Upper	37	3.01	35	42		Upper	30	0.00	30	30
Illinois	Lower	124	18.00	118	177	Pennsylvania	Lower	203	0.00	203	203
	Upper	59	0.00	59	59		Upper	50	0.00	50	50
Indiana	Lower	100	0.00	100	100	Rhode Island	Lower	94	10.75	75	100
	Upper	50	0.00	50	50		Upper	47	5.16	38	50
Iowa	Lower	100	0.00	100	100	South Carolina	Lower	124	0.00	124	124
	Upper	50	0.00	50	50		Upper	46	0.00	46	46
Kansas	Lower	125	0.00	125	125	South Dakota	Lower	70	0.00	70	70
	Upper	40	0.00	40	40		Upper	35	0.00	35	35
Kentucky	Lower	100	0.00	100	100	Tennessee	Lower	99	0.00	99	99
	Upper	38	0.00	38	38		Upper	33	0.00	33	33
Louisiana	Lower	105	0.00	105	105	Texas	Lower	150	0.00	150	150
	Upper	39	0.00	39	39		Upper	31	0.00	31	31
Maine	Lower	151	0.00	151	151	Utah	Lower	75	0.00	75	75
	Upper	35	0.81	33	35		Upper	29	0.00	29	29
Maryland	Lower	141	0.00	141	141	Vermont	Lower	150	0.00	150	150
	Upper	47	0.00	47	47		Upper	30	0.00	30	30
Massachusetts	Lower	160	0.00	160	160	Virginia	Lower	100	0.00	100	100
	Upper	40	0.00	40	40		Upper	40	0.00	40	40
Michigan	Lower	110	0.00	110	110	Washington	Lower	98	0.00	98	98
	Upper	38	0.00	38	38		Upper	49	0.00	49	49
Minnesota	Lower	134	0.00	134	134	West Virginia	Lower	100	0.00	100	100
	Upper	67	0.00	67	67		Upper	34	0.00	34	34
Mississippi	Lower	122	0.00	122	122	Wisconsin	Lower	99	0.00	99	99
	Upper	52	0.00	52	52		Upper	33	0.00	33	33
Missouri	Lower	163	0.00	163	163	Wyoming	Lower	62	1.87	60	64
	Upper	34	0.00	34	34		Upper	30	0.00	30	30

exist, then per-capita spending and population should be inversely related. However, previous authors have found mixed results. Gilligan and Matsusaka (1995) found a positive relationship between per-capita general expenditures and population that was significantly different from zero at either the 5- or 10-percent level. However, when investigating education, highway, and welfare spending, the coefficient on population alternated sign and was never significantly different from zero. In their analysis of the first half of the twentieth century, Gilligan and Matsusaka (2001) could not determine whether the coefficient on population was significantly different from zero in any of their regressions. Conversely, Chen and Malhotra (2007) found a negative and statistically significant relationship between the natural log of population and per-capita expenditure that was robust to several specifications. Given the findings of these authors, a measure of population (POP) is included here as well. Lastly, we include a variable measuring the percentage of the population aged 65 and older ($ELDERLY$), which is available from The Statistical Abstract of the United States.

To account for partisan impacts on spending we include indicator variables measuring Democratic control (DEM) and divided governments (DIV), respectively. The variable measuring Democratic control will take a value of 1 if the governorship and both chambers of the state legislature are controlled by Democrats and zero otherwise. The divided government variable will assume a value of 1 if no party controls the governor's office and both chambers of the legislature. Thus, for a state-year observation where the state Republican Party controls the government, Democratic control and divided government will both take values of zero. This strategy is taken from Chen and Malhotra (2007). However, one difference here is that we account for the one year lag in state budgeting cycles by allowing DEM and DIV enter into all regression equations with a one year lag.[12] Data on the political affiliation of governors and partisan control of legislatures is collected from various editions of The Book of the States.

[12]Expenditure data from the US Census is provided in fiscal years while political data is in calendar years.

Several panel unit root tests show that all dependent variables (with the exception of highway spending) as well as *GSP*, *REVFED*, *POP*, and *ELDERLY* are non-stationary. Since *LOWER* and *UPPER* are stationary we take the first difference of all non-stationary variables in order to avoid meaningless regression results. Panel unit root tests confirm that the first differences of these variables are stationary.[13] However, since the previous literature has primarily used historical cross-sections and not annual data, non-stationarity has not been addressed. For purposes of comparison, the following models are also tested without first differencing. These results will be discussed in Section V but to conserve space will not be presented.[14]

IV Model

The data discussed in the preceding section is used to examine how legislature size impacts government spending. The limited variability of legislature size puts this variable in a virtual identification "no man's land." On the one hand, the variable exhibits so little variation that results from traditional fixed-effects models are unreliable. On the other hand, there is just enough variability within some of the states that results from two-staged fixed-effects estimation methods would be equally unreliable.[15] One way this issue has been dealt with in the past is that researchers haven chosen not to control for unobserved heterogeneity at the state-level. By not including state fixed-effects researchers are unable to control for time invariant state level characteristics that could be correlated with per capita spending. Another method used by researchers to correct for the invariability of legislature size is to use historical cross-sections separated by decade rather than yearly data. However, while this

[13]Results from panel unit root tests conducted on the dependent and control variables are presented in the appendix.

[14]These results are available from the author upon request.

[15]These models, which stem from Hausman and Taylor (1981) require the variable in question to be completely time invariant.

can increase the variation slightly it may still not be enough to include state fixed effects. For example, the correlation between lower chamber size in 1960 and 2008 and between upper chamber size in 1960 and 2008 is .9388 and .9293, respectively.

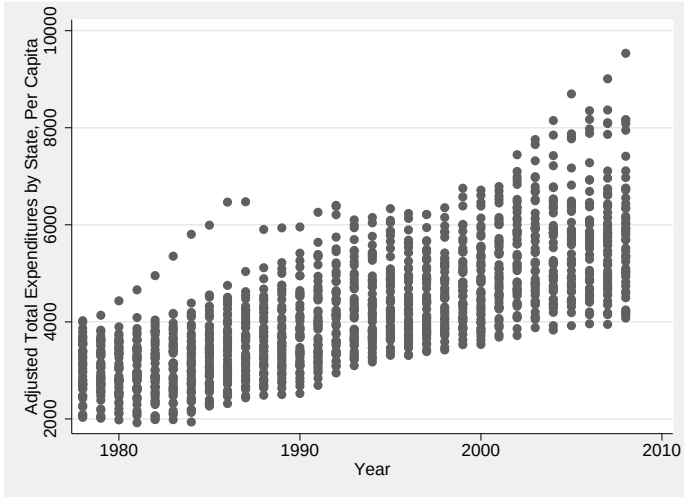
To circumvent this problem we use a methodology developed by Blanchard and Wolfers (2000) that will allow us to estimate the fiscal impact of legislature size while controlling for state fixed-effects. Blanchard and Wolfers developed this estimation method in order to study how labor market institutions affected unemployment rates in European countries.[16] Milesi-Ferretti, Perotti and Rostagno (2002) adopted this methodology to study the fiscal effects of electoral rules and were thus the first to apply this method to studies of government size. Persson and Tabellini (2003) extended the analysis of by addressing how form of government, as well as electoral rules, affected the growth of government. The variables of interest to Milesi-Ferretti et al. (2002) and Persson and Tabellini (2003) exhibited little, if any, variability within each country in the dataset. In both cases, using pure fixed-effects estimation to control for unobserved heterogeneity would have led to unreliable coefficients associated with the variables of interest. However, Blanchard and Wolfers' (2000) methodology made it possible to estimate the impact of the variable of interest while also including country fixed-effects. Exploiting the data with this technique will allow us to address the following hypothesis:

H1: Does a positive relationship between legislature size and total state spending per capita exist in the presence of shocks that are common to all states?

Figure 3.1 shows a scatterplot of per capita general expenditures for each state. It is evident from the figure that each state's per capita expenditures evolve similarly over the sample period. Thus, it is reasonable to consider that this behavior at least partially reflects

[16]Specifically, Blanchard and Wolfers (2000) studied how these institutions affected unemployment in the presence of common *and* country-specific events. It is much more difficult to argue that an economic or political event is solely state-specific. Thus, we only adopt the common shocks component of their analysis.

Figure 3.1: Adjusted Total Expenditures by State, Per Capita



certain economic, political, and social events that were common to all states over the sample period. The primary regression equation that will address *H1* is show below:

$$\Delta y_{it} = \boldsymbol{\beta_1 \Delta X_{it}} + \boldsymbol{\beta_2 P_{it}} + (1 + \gamma_1 LOWER + \gamma_2 UPPER)\boldsymbol{\delta Q_t} + \alpha_i + \epsilon_{it} \tag{3.1}$$

In equation 3.1 Δy_{it} denotes the first difference of either *TOTAL*, *EDU*, or *WELFARE*. The variable HWY is stationary and will not be differenced. The vector $\boldsymbol{Q_t}$ contains year-specific dummy variables that proxy for unobserved shocks that are common to all states in a given year. The year-specific dummy variables are interacted with the lower and upper chamber variables, which are expressed as deviations from the average lower and upper chamber, respectively. The coefficients on *LOWER* and *UPPER* are identified by interacting $\boldsymbol{Q_t}$ with each chamber size variable and by allowing $\boldsymbol{Q_t}$ to enter directly into equation 3.1. The following example will show how these coefficients should be interpreted. A state with lower and upper chambers of average size will have values for *LOWER* and *UPPER* equal to zero. Therefore, in a given year a shock to the change in per capita spending is captured

by the coefficient δ. In the analysis that follows, δ is restricted to equal a one standard deviation increase in Δy_{it}, denoted by $\delta^{+\sigma}$. Thus, the equation $(1+\gamma_2 UPPER^{\pm\sigma})\delta^{+\sigma}$ shows how the change in per capita spending responds to the same shock in a state with a lower chamber size at the average but an upper chamber size that deviates from the average by one standard deviation. A similar exercise is conducted for legislatures that are larger than the average by one lower chamber seat. On its own, the interpretation of an increase in Δy would not be easily interpreted. However, here it is used as a baseline measurement. Thus, if states with larger legislatures spend more per capita relative to states with an average size legislature then that should still be reflected by the coefficient on the chamber size variables.

The vector $\boldsymbol{\Delta X_{it}}$ contains the economic and demographic control variables discussed is Section III, all in first differences. The vector $\boldsymbol{P_{it}}$ contains the dummy variables that capture the partisan characteristics of government. State fixed-effects are absorbed by α_i and ϵ_{it} describes an error term that varies across both states and time. All regressions are estimated using nonlinear least squares and standard errors are clustered at the state level to account for heteroskedasticity and autocorrelation.

Equation 3.1 will also be used to submit the "Law of k/n" to further empirical scrutiny. The regression equation will now take the form

$$\Delta y_{it} = \boldsymbol{\beta_1 \Delta X_{it}} + \boldsymbol{\beta_2 P_{it}} + (1 + \gamma_1 LURATIO + \gamma_2 UPPER)\boldsymbol{\delta Q_t} + \alpha_i + \epsilon_{it} \qquad (3.2)$$

where $LURATIO_{it}$ denotes the ratio of lower chamber size to upper chamber size and enters into equation 3.2 as the deviation from the average. Recall from Section II that Chen and Malhotra found an inverse relationship between $LURATIO$ and per capita spending and a positive relationship between upper chamber size and spending. Thus, equation 3.2 will be used to test the following hypothesis:

H2: Does an inverse relationship between *LURATIO* and the change in per capita spending exist in the presence of shocks that are common to all states?

V Results

In this section we will present the estimation results from testing the hypotheses presented in Section IV. However, as a first step we will replicate the methods of Chen and Malhotra (2007) with the data gathered for this paper to compare our findings. We will also use equations 3.1 and 3.2 to data from cross-sections at five year intervals and over each decade from the 1980s to the 2000s. The results from these replications are provided in an appendix. Finally, we will present the primary results of this paper.

V.1 Preliminary Results

Since the expenditure data is the same as that analyzed by Chen and Malhotra we only attempt to replicate their work and not the earlier studies (i.e. Gilligan and Matsusaka (1995, 2001)). Along with the spending category *TOTAL* we extend their analysis to the other spending categories as well. Year and state fixed-effects are controlled for and standard errors are clustered at the state level. There are some differences between the replication and the original regressions from Chen and Malhotra (2007). They used historical cross-sections from 1964, 1974, 1984, 1994, and 2004. We were unable to recover some data from 1964, thus the replication uses data from 1960, 1970, 1980, 1990, 2000, and 2008.[17] Contrary to Chen and Malhotra, the number of days the legislature is in session is not controlled for. The method by which state legislatures record the number of days in session varies by state, with some using calendar days and others legislative days. Chen and Malhotra convert seven calendar days into five legislative days. However, this method is questionable. The length of

[17]Results excluding 2008 are similar to those presented in the paper. These results are available upon request.

a legislative day is the time between when a chamber meets after an adjournment and the next adjournment. Thus, a legislative day can last for multiple days or even weeks.

Replication results for the *TOTAL* category closely match those found by Chen and Malhotra (2007). The coefficient on *LOWER*, which is statistically insignificant, is -1.88 and is close to the coefficient of -2.04 that was estimated by Chen and Malhotra. The estimated coefficient on *UPPER* is 22.96 and is statistically significant at the .05 level. Chen and Malhotra estimated a coefficient of 26.09 when state and year fixed effects were included. The point estimates on the remaining variables closely match the estimates from Chen and Malhotra in direction, magnitude, and statistical significance. The one exception is the point estimate associated with Democratic control of government. Chen and Malhotra estimate a negative but statistically insignificant coefficient when both state and year fixed-effects are included. Conversely, a positive but statistically insignificant coefficient is estimated in the replication.

Neither lower nor upper chamber size is significantly related to per capita education spending or welfare spending. The point estimate on *LOWER* alternates sign between the different spending categories. The point estimate on UPPER remains positive but is much smaller in magnitude compared with the estimated coefficient estimated using total spending per capita. Finally, there is a positive correlation between upper chamber size and per capita highway spending that is statistically significant at the .10 level. The point estimate, though, is much smaller than that associated with total spending per capita and implies that an additional member in the upper chamber is worth approximately $3.02 in additional highway spending per capita.

Contrary to Chen and Malhotra, the relationship between lower-to-upper chamber size ratio and per capita total spending is not statistically significant. However, the point estimate is negative and similar in magnitude to their original estimates. It is also the case that upper chamber size is no longer significantly related to per capita total spending. Ex-

tending this analysis to the other spending categories reveals that the hypothesized negative relationship between *LURATIO* and per capita spending is only true for the welfare spending category. Surprisingly, we find a positive and statistically significant (p-value = .10) relationship between *LURATIO* and per capita highway spending. This would imply that highway spending is divisible at the lower chamber district level and is increasing as the size of the lower chamber grows relative to the upper chamber.

Equation 3.1 from the previous section is first estimated for all spending categories using historical cross-sections from every five years beginning in 1978.[18] The estimated coefficients on *LOWER* and *UPPER* are both positive, whereas when replicating the model from Chen and Malhotra (2007) the point estimate on *LOWER* was negative. However, the starkest difference is that the coefficient on *UPPER* is now statistically insignificant. In regressions using the other spending categories, the point estimates on *LOWER* and *UPPER* alternate sign and statistical significance depending on the time period analyzed. Regressions using per capita education spending show that upper chamber size is only statistically significant during the 2000s. However, the point estimate is negative. Thus, this would imply that a shock which resulted in a one standard deviation increase in per capita education spending (δ = \$190.04) in a state with the size of the legislature equal to the average would be associated with an increase in spending that was approximately \$32.31 lower in a state with a legislature that was larger than the average by a one standard deviation increase in the upper chamber. Per capita welfare expenditures are positively related to lower chamber size in regressions using data from the 1990s. The coefficient is relatively large (0.011) and is statistically significant at the .01 level. Relative to a shock that caused per capita education spending to increase by one standard deviation (δ\$237.43) in a state with a legislature of average size, a state with a legislature that was larger than the average by a one standard

[18]Given that the data set spans not quite three decades five-year historical cross-sections are chosen in order to have more data.

deviation increase in the lower chamber (55 seats) would experience an increase in per capita welfare spending that was $144.95 larger. Finally, neither *LOWER* nor *UPPER* were significantly related to per capita highway spending. Equation 2 is also estimated using 5 year historical cross-sections. The point estimates on *LURATIO* and *UPPER* are never statistically significant for any spending category. However, the point estimates on the control variables display the same behavior as in the traditional fixed-effects regressions.

Next, equation 3.1 is tested using annual data for the 1980s, 1990s, and 2000s. Results from the 1980s reveal that the point estimates on both *LOWER* and *UPPER* are negative and statistically insignificant. Another difference between the 1980s and the five-year historical cross-sections is a positive and statistically significant coefficient on $\ln POP$. When equation 3.1 is estimated over the 1990s the coefficient on UPPER is again negative and statistically insignificant. Conversely, the point estimate on *LOWER* is now positive, though it remains statistically insignificant. Finally, when estimating the period 2000 – 2008 the point estimates on *LOWER* and *UPPER* remain negative and statistically insignificant. Across the 1980s, 1990s, and 2000s the point estimate on *LURATIO* is only statistically significant in the case of per capita welfare spending during the 1990s. The point estimate is .299, which is rather large, and is statistically significant at the .01 level. The point estimate on *UPPER* is negative and statistically significant in the *EDU* regression during the 2000s. This result is similar to the point estimate on *UPPER* in the *EDU* regression over the 2000s when both lower and upper chamber size was controlled for.

These preliminary results reveal a weak and inconsistent relationship between legislature size and per capita spending. The relationship between per capita spending and chamber size appears to be dependent upon the time period that is analyzed and the regression model that is used.

V.2 Results: Common Shocks

Table 3.3 reports results from the estimation of equations 3.1 and 3.2 using $\Delta TOTAL$, ΔEDU, $\Delta WELFARE$, and HWY as dependent variables and all available data over the sample period 1978 – 2008. For each spending category, the left column shows the results when using equation 3.1 and the right column shows the results when using equation 3.2. While the point estimate on $UPPER$ in the $\Delta TOTAL$ regression remains positive it is also still statistically insignificant. Still, to give an idea of what the point estimate means, consider a common shock that results in a one standard deviation increase in $\Delta TOTAL$ ($\delta = \$183.15$) in a state with a lower and upper chamber of average size. In a state where the legislature was larger than the average by a one standard deviation increase in the upper chamber the common shock would be associated with an increase in $\Delta TOTAL$ that is \$16.51 larger than the average response. This amounts to roughly a 9% increase over the response by the average legislature. However, this result is still statistically insignificant. Also, the point estimate on $LOWER$ continues to be small and statistically insignificant as well.

There are some noticeable differences regarding the control variables compared to the previously estimated models. The point estimates on DEM and DIV are now both statistically significant at the .05 level. Thus, if a state government switches from Republican control to Democratic control it can expect $\Delta TOTAL$ to increase by approximately \$48. If control of the state government switches from Republican control to divided control the expected change in $\Delta TOTAL$ is \$32. The point estimate associated with ΔGSP is positive but statistically insignificant. In other words, we cannot say that an increase in ΔGSP is associated with a positive increase in $\Delta TOTAL$. Lastly, the coefficient on $\Delta ELDERLY$ is now negative and statistically significant at the .05 level, which would imply that a one standard deviation increase in $\Delta ELDERLY$ (.51) is associated with a decrease in $\Delta TOTAL$ of almost \$7.15.[19]

[19] $ELDERLY$ is measured as a percentage rate. Thus $\sigma = .51$ is approximately one half of one percent.

Table 3.3: Chamber Size, Lower-to-Upper Chamber Size Ratio, and State Level Spending

	(1) $\Delta TOTAL$		(2) ΔEDU		(3) $\Delta WELFARE$		(4) HWY	
$CONSTANT$	-72.36***	-73.05***	-11.15*	-11.13*	-10.54	-9.46	232.20***	231.60***
	(25.04)	(25.08)	(6.42)	(6.41)	(11.10)	(11.11)	(14.99)	(15.30)
ΔGSP	4.63	4.65	0.67	0.67	-0.95	-0.92	-2.28	-2.43
	(4.06)	(4.07)	(0.85)	(0.85)	(1.27)	(1.28)	(1.79)	(1.79)
$\Delta REVFED$	0.61***	0.61***	0.035**	0.035***	0.26**	0.23**	0.067***	0.066***
	(0.19)	(0.19)	(0.013)	(0.013)	(0.088)	(0.089)	(0.014)	(0.014)
$\Delta ELDERLY$	-14.02**	-14.07**	-0.68	-0.66	-3.21	-3.33	-0.14	-0.11
	(6.76)	(6.76)	(1.07)	(1.07)	(2.08)	(2.10)	(1.45)	(1.44)
ΔPOP (in millions)	38.60	42.29	27.95**	27.81**	-46.30*	-44.12**	-59.29*	-58.11*
	(59.43)	(58.27)	(13.57)	(13.56)	(26.44)	(25.27)	(32.11)	(32.18)
DEM	48.02**	48.51**	1.84	1.84	17.68***	17.58***	-14.48	-14.22
	(21.07)	(21.05)	(3.47)	(3.48)	(6.48)	(6.57)	(10.30)	(10.51)
DIV	32.00**	32.47**	4.88*	4.88**	10.99**	11.30**	0.80	1.19
	(14.60)	(14.52)	(2.76)	(2.75)	(4.86)	(4.89)	(8.15)	(8.35)
$LOWER$	0.0002		-0.0009		0.0087***		0.0012	
	(0.001)		(0.0007)		(0.0016)		(0.002)	
$UPPER$	0.009	0.0089	0.0006	-0.0011	-0.018***	0.0038	0.0084	0.0096
	(0.0071)	(0.0076)	(0.0063)	(0.0066)	(0.0063)	(0.0065)	(0.011)	(0.013)
$LURATIO$		-0.0037		-0.019		0.24***		0.0098
		(0.018)		(0.014)		(0.033)		(0.04)
Observations	1410	1410	1410	1410	1410	1410	1410	1410
Adjusted R^2	0.306	0.306	0.122	0.122	0.306	0.306	0.844	0.844

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. Δ represents the first difference of a variable. All regressions include state fixed-effects. The variables $LOWER$, $LURATIO$ and $UPPER$ are centered on the sample average.

Legislature size does not have a statistically significant impact on the change in per capita education spending. Thus, we are unable to show that a state with a legislature size larger than average would respond to a common shock any differently than a state with a legislature size equal to the national average. The estimated coefficient associated with DIV is positive and statistically significant at the .10 level, thus showing some evidence that a divided state government is associated with a faster increase in per capita education spending compared to a Republican-controlled government. Per capita revenue from the federal government and population are both positively correlated with per capita education spending. A one standard deviation increase in $\Delta REVFED$ is associated with a \$3.58 increase in ΔEDU, which is statistically significant at the .05 level. A one standard deviation increase in ΔPOP (.11) is associated with an increase in ΔEDU of \$2.93.

The relationship between per capita spending on welfare and legislature size exhibits stark differences compared to those observed with respect to general spending and education spending. The estimated coefficient on lower chamber size is positive and statistically significant at the .01 level. In response to a shock that results in $\Delta WELFARE$ increasing by one standard deviation in a state with a lower and upper chamber of average size ($\delta = \$76.38$) a state with a legislature that is larger than the average by a one standard deviation increase in the lower chamber size (approximately 55 seats) will experience an increase in welfare spending that is \$37.56 higher than the baseline legislature. The point estimate associated with upper chamber size is also statistically significant at the .01 level but is negative and slightly larger in magnitude than the estimated coefficient on *LOWER*. Thus, holding lower chamber size at the average, a legislature that deviates from the baseline legislature by a one standard deviation increase in the upper chamber experiences an increase in spending that is \$13.29 lower than the baseline legislature. The impact on the change in welfare spending associated with larger legislatures does not appear to be very large. However, given the conflicting coefficients on *LOWER* and *UPPER* it is difficult to say unambiguously how a

larger legislature would impact per capita welfare spending.

The political composition of government is strongly associated with welfare spending as well. Switching from a Republican to a Democrat-controlled government is associated with a \$17.68 increase in the change in per capita welfare spending. The estimated coefficient associated with DIV suggests that going from a Republican-controlled government to a divided government is associated with an increase in $\Delta WELFARE$ of approximately \$11. The point estimate on DEM is statistically significant at the .01 level while the point estimate on DIV is significant at the .05 level. The estimated coefficient associated with $\Delta REVFED$, which is also statistically significant at the .05 level, suggests that a one standard deviation increase in $\Delta REVFED$ is associated with a \$30.07 increase in $\Delta WELFARE$. Lastly, the estimated coefficient on ΔPOP is negative and statistically significant at the .10 level. The magnitude of the coefficient is quite large relative to the other spending categories. This suggests that states experiencing population growth are also enjoying economic growth, and thus have fewer individuals receiving welfare payments. According to the estimate, a one standard deviation increase in ΔPOP is associated with a decrease in $\Delta WELFARE$ of \$4.87.

Per capita highway spending, the final category, exhibits no statistically significant relationship with either $LOWER$ or $UPPER$. Recall that HWY is not in first difference form since panel unit root tests showed the variable to be stationary. The only other variables that are related to per capita highway spending are $\Delta REVFED$ and ΔPOP. The estimated coefficient on $\Delta REVFED$ implies that a one standard deviation increase in the change in per capita revenue from the federal government is associated with an increase in per capita highway spending of \$6.97. The negative point estimate associated with ΔPOP shows evidence of economies of scale associated with highway spending. For example, if the change in population increases by one standard deviation (approximately 105,000) per capita highway

spending should decrease by \$6.26.[20]

Recall, the key variables of interest in equation 3.2 are *LURATIO*, which measures the lower-to-upper chamber size ratio, and *UPPER*. Since the estimated coefficients associated with the control variables do not change in a substantive way, the discussion here will focus on *LURATIO* and *UPPER*. Regarding total spending, the point estimates maintain the same sign as what the Law of k/n predicts, however, the standard errors are large enough that we cannot show that the relationship between either *LURATIO* or *UPPER* and total spending per capita is statistically different from zero. In this section, the baseline legislature will be one where the upper chamber size and the ratio of the lower-to-upper chamber size is equal to the national average (the average lower-to-upper chamber size ratio equals 3).[21] The point estimate implies that a legislature that deviates from the baseline by a one unit increase in *LURATIO* will experience a change in per capita spending that is \$6.37 less than the baseline legislature.

There is also no evidence of a statistically significant relationship between *LURATIO* and ΔEDU or *LURATIO* and *HWY*. Thus, we cannot show that a legislature with a larger than average lower-to-upper chamber ratio experiences a change in these spending categories that are different from a legislature with the average ratio of lower-to-upper chamber seats.

The relationship between *LURATIO* and $\Delta WELFARE$ is positive and the estimated coefficient is statistically significant at the .01 level. The magnitude of the coefficient is also the largest of the four spending categories. This estimate implies that a one standard deviation shock to the change in per capita welfare spending in the baseline legislature is associated with an increase in the change in per capita welfare spending equal to approximately \$93.70, which is \$17 greater than the baseline legislature.

[20]Since population is measured in millions $\sigma = .105$

[21]Holding *LURATIO* and *UPPER* at the average implies that the value of *LOWER* is equivalent to 120 seats.

VI Discussion

The results presented in Section V show that the relationship between legislature size and total spending per capita is not as strong as those found in previous studies. In the presence of a shock that is common to all states, a state with a larger lower or upper chamber does not experience an increase in $\Delta TOTAL$ that is different than that experienced by a state with the average lower and upper chamber. The conclusion that should be drawn from this result is that the positive relationship between upper chamber size and total spending per capita is dependent upon both the model and the how one uses observations over time. When the relationship is estimated using a model that allows for both fixed effects and yearly data the relationship does not appear to exist. Remember, though, that this is not the first instance of the robustness of the Law of 1/n being called into question. Primo and Snyder (2008) noted several instances where the Law of $1/n$ may not hold.

The only instance where a positive relationship between spending and legislature size is shown is between lower chamber size and welfare spending per capita. This magnitude of a one standard deviation increase in lower chamber size was associated with a response in welfare spending that was almost 50% larger than the response would have been had the legislature been of average size. Conversely, the relationship between upper chamber size and welfare spending per capita was negative and statistically significant, though, the magnitude of the point estimate was not as large at that associated with lower chamber size. This finding was unexpected, given that most evidence shows a positive relationship between upper chamber size and total spending. However, it is not clear why welfare spending would be expected to grow with legislature size, unlike highway spending, which can be geographically concentrated and resembles the type of spending projects Weingast et al. (1981) had in mind. However, when using annual highway expenditures, we cannot show a statistically significant and positive relationship between highway spending per capita and

upper chamber size.

Lastly, the only statistically significant relationship between the lower-to-upper chamber size ratio and spending per capita occurs with welfare spending. However, the point estimate on *LURATIO* is positive, which goes against the prediction of the Law of k/n. The positive point estimates on *LOWER* and *LURATIO* would indicate that welfare spending is targeted at the lower chamber district level, though there is no evidence to support such a claim. The estimated coefficient on *LURATIO* is negative in the case of both total spending and education spending per capita but is never statistically different from zero.

VII Conclusion

Since the financial crisis of 2008, several state legislatures have proposed reducing the number of legislators as a way to shrink state budgets. This idea has some empirical support in that several researchers have shown a positive relationship between state legislature size and per capita spending. However, showing a meaningful correlation between legislature size and per capita spending is made difficult by the fact that state governments rarely make changes to the number of legislators. Thus, the nature of legislature size forces researchers to make estimation choices that have important drawbacks.

We re-examine the relationship between legislature size and spending using a methodology first proposed by Blanchard and Wolfers (2000). This estimation technique allows for the use of annual data while also controlling for state level unobserved heterogeneity despite the invariability of legislature size. Using data at 5-year intervals and for each decade between the 1980s and 2000s we are unable to find a statistically significant and positive relationship between either lower or upper chamber size and total spending per capita. In fact, the only positive relationship between legislature size and spending lower chamber size and welfare spending per capita during the 1990s. Similarly, we are unable to find the negative

relationship between the lower-to-upper chamber size ratio and total spending per capita as proposed by Chen and Malhotra (2007).

Over the period 1978 – 2008 we found no evidence that states with larger-than-average lower or upper chambers experienced an increase in total spending per capita that was larger than a state with an average-sized legislature. We also found no differences in total spending per capita between states with an average lower-to-upper chamber size ratio and those with a larger than average chamber size ratio.

These findings are important in at least two respects. First, the way in which researchers test for the existence of this relationship has important implications on inference. The existence of a positive relationship between legislature size and spending depends upon whether one is using annual data or selecting certain years from successive decades and whether or not one is controlling for state level unobserved heterogeneity. Second, the lack of a robust positive relationship between legislature size and spending means that reducing the number of legislators might not have much of an effect. In other words, the size of the legislature is unlikely to impact state-level spending.

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I Tests for Non-stationarity and Preliminary and Replication Results

Table A1: Panel Unit Root Tests on the Dependent Variables

Method	Variable	Statistics	P-Value
Levin et al. (2002) t^*	*TOTAL*	0.03	0.51
Null: Unit Root (common process)	*EDU*	0.84	0.80
Alt: All Panels Stationary	*WELFARE*	0.92	0.82
	HWY	-5.79	0.00
Im et al. (2003) W-stat	*TOTAL*	1.03	0.85
Null: Unit Root (individual process)	*EDU*	0.83	0.80
Alt: Some Panels Stationary	*WELFARE*	2.54	0.99
	HWY	-6.28	0.00
ADF Fisher-Type Test Chi-Square	*TOTAL*	83.29	0.78
Null: Unit Root (individual process)	*EDU*	99.89	0.32
Alt: At Least One Panel is Stationary	*WELFARE*	67.20	0.98
	HWY	214.87	0.00
PP Fisher-Type Test Chi-Square	*TOTAL*	98.56	0.35
Null: Unit Root (individual process)	*EDU*	146.65	0.00
Alt: At Least One Panel is Stationary	*WELFARE*	90.33	0.59
	HWY	243.19	0.00

Dependent variable descriptions provided in Section III. Fisher-type test probabilities computed from an asymptotic Chi-square distribution. Probabilities for other tests assume asymptotic normality. Presentation of unit root test results taken from de Haan, Jong-A-Pin and Mierau (2012).

Table A2: Panel Unit Root Tests on the Independent Control Variables

Method	Variable	Statistics	P-Value
Levin et al. (2002) t^*	*GSP*	-3.46	0.00
Null: Unit Root (common process)	*ELDERLY*	-2.36	0.01
Alt: All Panels Stationary	*POP*	2.37	0.99
	REVFED	0.76	0.78
Im et al. (2003) W-stat	*GSP*	-1.05	0.15
Null: Unit Root (individual process)	*ELDERLY*	0.63	0.74
Alt: Some Panels Stationary	*POP*	10.12	1.00
	REVFED	2.57	0.99
ADF Fisher-Type Test Chi-Square	*GSP*	115.89	0.06
Null: Unit Root (individual process)	*ELDERLY*	129.29	0.01
Alt: At Least One Panel is Stationary	*POP*	29.62	1.00
	REVFED	65.74	0.99
PP Fisher-Type Test Chi-Square	*GSP*	81.98	0.81
Null: Unit Root (individual process)	*ELDERLY*	192.11	0.00
Alt: At Least One Panel is Stationary	*POP*	39.45	1.00
	REVFED	105.6	0.19

Dependent variable descriptions provided in Section III. Fisher-type test probabilities computed from an asymptotic Chi-square distribution. Probabilities for other tests assume asymptotic normality. Presentation of unit root test results taken from de Haan et al. (2012).

Table A3: Replication of Chen and Malhotra (2007) Table 4 and Extension to Education, Welfare, and Highway Spending

	(1) *TOTAL*		(2) *EDU*		(3) *WELFARE*		(4) *HWY*	
CONSTANT	4827.90*	5231.20*	2463.40***	2496.60***	1833.10	2041.20*	1255.30**	1163.90**
	(2471.20)	(2500.10)	(841.30)	(833.80)	(1171.70)	(1208.20)	(557.00)	(558.40)
GSP	53.87***	52.96***	0.236	-0.148	10.88*	10.38*	4.67	4.80
	(16.40)	(16.14)	(3.58)	(3.61)	(5.82)	(5.83)	(3.02)	(3.02)
REVFED	1.32***	1.31***	0.0407	0.0402	0.30**	0.298**	0.0493*	0.0505*
	(0.16)	(0.16)	(0.05)	(0.045)	(0.13)	(0.126)	(0.03)	(0.026)
$\ln POP$	-372.60**	-381.10**	-165.90***	-165.30***	-137.40*	-141.70*	-80.09**	-77.89**
	(161.70)	(161.00)	(57.57)	(56.85)	(74.88)	(75.06)	(38.60)	(38.19)
DEM	160.90	157.60	-22.40	-23.84	103.40***	101.50***	-38.51*	-38.03*
	(100.30)	(99.54)	(26.78)	(26.70)	(37.64)	(37.27)	(19.17)	(19.17)
DIV	96.59	96.78	-5.72	-5.94	74.58***	74.65***	-22.52	-22.62
	(67.97)	(67.37)	(19.78)	(19.68)	(24.82)	(24.50)	(18.12)	(18.12)
LOWER	-1.88		0.0155		-0.951		0.465	
	(1.83)		(0.73)		(0.75)		(0.31)	
UPPER	22.96**	17.81	3.239	3.08	2.201	-0.42	3.022*	4.25***
	(11.11)	(10.73)	(2.38)	(2.53)	(4.92)	(4.72)	(1.80)	(1.58)
LURATIO		-87.44		-7.84		-45.21*		19.66
		(53.69)		(29.65)		(25.10)		(11.16)
Observations	278	278	278	278	278	278	278	278
Adjusted R^2	0.969	0.969	0.914	0.914	0.917	0.917	0.784	0.784

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. Regressions include year and state fixed effects. The variables *LOWER*, *LURATIO* and *UPPER* are centered on the sample average.

Table A4: Chamber Size and *TOTAL* State Level Spending Across Decades

	(1)		(2)		(3)		(4)	
CONSTANT	12692.70*	12744.30*	-8533.90	-8463.00	14745.80	15241.30	2423.40**	24171.60**
	(6490.40)	(6462.90)	(5839.50)	(5940.10)	(9833.40)	(9760.10)	(11847.10)	(11867.70)
GSP	37.63***	37.75***	13.67	13.74	20.30*	23.18*	30.58	30.70***
	(11.25)	(11.28)	(15.04)	(14.97)	(11.90)	(11.79)	(10.41)	(10.25)
REVFED	0.838***	0.838***	1.498***	1.943***	1.133***	1.135***	0.867***	0.866***
	(0.20)	(0.20)	(0.39)	(0.39)	(0.19)	(0.19)	(0.15)	(0.15)
ELDERLY	-54.90	-55.72	-31.08	-32.23	-305.60***	-304.10***	113.90	116.70
	(70.42)	(70.04)	(19.20)	(19.69)	(80.58)	(80.51)	(127.80)	(128.50)
$\ln POP$	-735.50*	-740.50*	662.90*	659.50	-607.90	-642.00	-1547.20*	-1545.30*
	(405.70)	(404.30)	(394.10)	(400.30)	(641.10)	(636.00)	(773.10)	(774.70)
DEM	-27.82	-28.30	-33.67	-40.17	91.68	96.82	121.10	122.70
	(94.11)	(94.24)	(56.70)	(54.76)	(61.80)	(61.82)	(74.40)	(74.93)
DIV	23.71	23.77	37.50	31.04	81.94*	85.42*	99.79**	100.10**
	(70.43)	(70.62)	(43.27)	(41.03)	(46.87)	(46.76)	(43.28)	(43.38)
LOWER	0.000208		-0.00109		0.00003		-0.00103	
	(0.0006)		(0.0013)		(0.0012)		(0.0011)	
UPPER	0.00164	0.00204	-0.00577	-0.00873	-0.00129	-0.00212	-0.00362	-0.00608
	(0.0047)	(0.0052)	(0.0098)	(0.0099)	(0.0077)	(0.0088)	(0.0097)	(0.0109)
LURATIO		0.00204		-0.0368		-0.0010		-0.029
		(0.0139)		(0.0251)		(0.0226)		(0.0251)
Time Period	5 Year		1980s		1990s		2000s	
Observations	282	282	470	470	470	470	423	423
Adjusted R^2	0.956	0.956	0.957	0.957	0.967	0.967	0.971	0.971

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. Regressions include year and state fixed effects. The variables *LOWER*, *LURATIO* and *UPPER* are centered on the sample average.

Table A5: Chamber Size and State Level Education Spending Per Capita Across Decades

	(1)		(2)		(3)		(4)	
CONSTANT	3871.60***	3942.10***	2304.60**	2314.60**	698.40	761.60	9138.20**	9000.50**
	(1269.60)	(1262.30)	(1028.40)	(1038.20)	(2370.10)	(2347.30)	(3671.70)	(3659.30)
GSP	3.63***	3.67***	2.89**	2.94**	3.24	3.27	1.18	1.30
	(1.25)	(1.25)	(1.25)	(1.26)	(2.16)	(2.14)	(2.20)	(2.19)
REVFED	-0.051	-0.052	0.032	0.032	0.035	0.035	0.03	0.03
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
ELDERLY	17.50*	16.96*	-0.97	-1.07	-25.41**	-25.24**	40.31	41.35
	(9.50)	(9.50)	(2.20)	(2.24)	(11.26)	(11.32)	(28.19)	(28.26)
$\ln POP$	-230.90***	-235.20***	-117.90*	-118.50*	12.03	7.66	-581.50**	-573.70**
	(81.41)	(81.05)	(67.92)	(68.58)	(156.70)	(155.30)	(243.70)	(243.30)
DEM	14.18	14.24	24.04	23.56	8.30	8.75	-0.11	-0.026
	(16.21)	(16.15)	(17.93)	(18.00)	(15.84)	(16.08)	(18.47)	(18.61)
DIV	6.30	6.66	23.69	23.16	6.72	7.03	-0.03	-0.03
	(14.37)	(14.43)	(16.31)	(16.38)	(14.37)	(14.59)	(9.31)	(9.42)
LOWER	0.0005		0.00005		0.0006		-0.001	
	(0.0007)		(0.0014)		(0.0016)		(0.001)	
UPPER	0.0039	0.0047	-0.0059	-0.0057	0.019	0.02	-0.017*	-0.019**
	(0.006)	(0.007)	(0.009)	(0.01)	(0.018)	(0.02)	(0.007)	(0.008)
LURATIO		0.0093		-0.0062		0.0094		-0.02
		(0.016)		(0.033)		(0.032)		(0.02)
Time Period	5 Year		1980s		1990s		2000s	
Observations	282	282	470	470	470	470	423	423
Adjusted R^2	0.934	0.933	0.952	0.952	0.96	0.96	0.946	0.946

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. Regressions include year and state fixed effects. The variables *LOWER*, *LURATIO* and *UPPER* are centered on the sample average.

Table A6: Chamber Size and State Level Welfare Spending Per Capita Across Decades

	(1)		(2)		(3)		(4)	
CONSTANT	-2680.40	-2602.50	1244.20	1258.50	-2892.20	2924.20	12749.90*	12878.70*
	(2973.80)	(2961.40)	(2918.70)	(2923.50)	(3253.60)	(3271.90)	(6736.10)	(6800.60)
GSP	4.50	4.68	2.02	2.05	2.53	2.27	-2.75	-2.80
	(3.16)	(3.14)	(2.01)	(2.01)	(3.64)	(3.62)	(5.13)	(5.20)
REVFED	0.48**	0.48**	0.273***	0.27***	0.68***	0.67***	0.25***	0.25***
	(0.20)	(0.20)	(0.09)	(0.09)	(0.06)	(0.06)	(0.09)	(0.09)
ELDERLY	-0.55	-0.66	-3.49	-3.55	-23.69	-22.77	-76.73	-75.78
	(30.30)	(29.94)	(4.86)	(4.90)	(24.66)	(24.59)	(56.08)	(56.62)
$\ln POP$	164.80	159.60	-80.78	-81.72	200.70	203.00	-732.20	-741.30
	(181.40)	(180.70)	(191.30)	(191.70)	(219.90)	(221.10)	(443.30)	(447.00)
DEM	31.82	29.40	31.92	31.70	4.95	3.83	97.89**	97.65**
	(34.87)	(35.29)	(21.28)	(21.12)	(20.19)	(20.18)	(40.62)	(40.54)
DIV	20.50	19.68	25.49	25.25	12.97	13.34	39.03	38.46
	(28.92)	(29.15)	(17.11)	(16.88)	(13.59)	(13.66)	(28.40)	(28.49)
LOWER	0.0026		0.0002		0.011***		-0.0005	
	(0.0017)		(0.0017)		(0.0034)		(0.0014)	
UPPER	0.011	0.017	0.011	0.011	-0.007	0.02	-0.007	-0.008
	(0.014)	(0.017)	(0.017)	(0.018)	(0.015)	(0.015)	(0.01)	(0.01)
LURATIO		0.07		0.002		0.30***		-0.02
		(0.04)		(0.04)		(0.08)		(0.03)
Time Period	5 Year		1980s		1990s		2000s	
Observations	282	282	470	470	470	470	423	423
Adjusted R^2	0.907	0.907	0.934	0.934	0.937	0.937	0.921	0.921

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. Regressions include year and state fixed effects. The variables *LOWER*, *LURATIO* and *UPPER* are centered on the sample average.

Table A7: Chamber Size and State Level Highway Spending Per Capita Across Decades

	(1)		(2)		(3)		(4)	
CONSTANT	1514.10	1474.60	-6002.70**	-6032.90**	3347.00	3351.00	-538.70	-392.50
	(992.80)	(1027.70)	(2849.60)	(2848.50)	(2920.40)	(2889.10)	(2579.70)	(2660.40)
GSP	6.65***	6.69***	3.50	3.64	2.13	2.17	3.45	3.31
	(1.35)	(1.34)	(2.71)	(2.74)	(3.40)	(3.42)	(2.07)	(2.11)
REVFED	0.10***	0.10**	0.38***	0.38***	0.085**	0.085**	0.047	0.047
	(0.03)	(0.03)	(0.08)	(0.08)	(0.04)	(0.04)	(0.03)	(0.03)
ELDERLY	-2.30	-2.37	0.08	0.11	-44.41**	-44.40**	19.19	18.94
	(10.04)	(10.06)	(3.42)	(3.45)	(20.73)	(20.78)	(28.95)	(29.32)
ln *POP*	-98.77	-96.26	387.40**	389.00***	-178.70	-179.00	25.44	16.37
	(62.63)	(64.71)	(187.30)	(187.20)	(197.60)	(195.90)	(161.50)	(165.90)
DEM	-23.80	-23.42	17.20	17.24	29.37	29.12	-28.11*	-28.84*
	(14.66)	(14.58)	(18.82)	(18.85)	(20.29)	(29.12)	(15.96)	(16.20)
DIV	-14.48	-14.34	32.24**	32.17**	20.44*	20.18*	-6.05	-6.44
	(12.02)	(12.21)	(13.33)	(13.45)	(10.57)	(10.65)	(12.79)	(12.99)
LOWER	0.0036		-0.0036		0.0027		-0.0038	
	(0.0038)		(0.0034)		(0.0052)		(0.0048)	
UPPER	-0.0021	0.0063	-0.0006	-0.0043	-0.0507	-0.0447	-0.0428	-0.0499
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)
LURATIO		0.10		-0.05		0.08		-0.08
		(0.09)		(0.07)		(0.12)		(0.09)
Time Period	5 Year		1980s		1990s		2000s	
Observations	282	282	470	470	470	470	423	423
Adjusted R^2	0.873	0.873	0.912	0.912	0.883	0.883	0.907	0.907

* $p < .10$, ** $p < .05$, *** $p < .01$. Cluster robust standard errors in parentheses. Regressions include year and state fixed effects. The variables *LOWER*, *LURATIO* and *UPPER* are centered on the sample average.

Conclusion

The conclusions reached in this dissertation have important implications, both for the literature on distributive politics and policy. The first essay shows that states with the entire Senate delegation in the majority receive a larger share of grant spending per capita when Senate-level polarization is relatively low, and a smaller share when the Senate majority party also controls the House. These results provide evidence that politically "safe" states may received spending shares that are lower than those received by more competitive states.

The second and third essays show that finding a positive relationship between legislature size and government spending depends heavily upon the model specification, the time period in question, and the type of fiscal data that is employed. Specifically, in the third essay the only evidence of a positive relationship between legislature size and government spending is found in a sample of countries with bicameral legislatures. Even then, only upper chamber size is positively related to increases in government spending. The third essay of this dissertation analyzes the legislature size question across the US states. Here, we are unable to find a robust positive relationship between legislature size and spending growth for a majority of the fiscal categories analyzed. Overall, the conclusion from the second and third essays is that legislature size is a relatively unimportant determinant of government spending.