

## Separation of Powers and Turnout\*

Rebecca B. Morton  
Department of Politics  
New York University  
[rbm5@nyu.edu](mailto:rbm5@nyu.edu)

Charles Shipan  
Department of Political Science  
University of Michigan  
[cshipan@umich.edu](mailto:cshipan@umich.edu)

Melanie Springer  
Department of Political Science  
Washington University at St. Louis  
[mspringer@wustl.edu](mailto:mspringer@wustl.edu)

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## **Introduction**

The benefits of separation of powers system are well known. In particular, the foundational Madisonian justification for separating powers across institutions is that such separation prevents tyranny, as any group will find it difficult to capture each branch of government. At the same time, separations of powers systems are not immune from criticism. They can, for example, make policy change difficult. And some studies have suggested that separation of powers systems might depress turnout, providing a potential (and partial) explanation for low turnout in the U.S., relative to most other countries.

Franklin and Hirczy de Mino (1998) and Franklin (2004) offer an argument that explains why separation of powers systems might depress turnout. They argue that the use of separate elections for the executive and legislative branches in the U.S., coupled with shared power between these two branches in making governmental decisions, reduces participation. Their contention is that the separation of powers system makes it difficult for voters to determine responsibility for particular policies and use elections as a way to influence policy choices. When the powers of government are divided, voters are argued to have less information about the choices before them and thus are less willing to participate in the electoral process due to the difficulty of assigning responsibility. This argument is also supported by related research, which shows that voters condition their choices based on perceived accountability (Rudolph and Grant 2002, Rudolph 2003) and that uninformed voters are less likely to participate (see Battaglini, Morton, and Palfrey 2006 for a review).

However, because of the distinctiveness of the U.S. electoral process, identifying the effects of the U.S. separation of powers system on turnout is difficult. That is, as

Franklin (2004) points out, additional differences between the U.S. and other countries, such as the frequency of elections and voluntary registration, make it impossible to determine the effects of separation of powers independently.

Although there is little variation in the separation of powers institutional system over time in the U.S., the transparency of policy authority *does* vary over time (Franklin and Hirczy de Mino 1998). Specifically, in periods when the two branches are controlled by different political parties (i.e., divided government) the separation of powers system is more likely to have negative consequences on turnout than in periods when the two branches are controlled by the same political party (i.e., unified government). Under divided government, then, voters have less information about the candidates' policy choices than they do when government is unified. Thus, because divided government increases the separation between institutions, voters will be less willing to participate under divided government than under unified government. Using this logic Franklin and Hirczy de Mino (1998) estimate the effects of divided government on aggregate turnout in presidential elections from 1840 to 1992. They find that divided government reduces turnout by 1.96 percent on average and conclude that this evidence supports the argument that separation of powers systems produce lower voter participation rates.

In order to make the argument that the empirical results imply that divided government – and by inference separation of powers – causes a reduction in turnout, we must assume that there are no confounding variables that cause both divided government and turnout and that are not included in the empirical estimation. Divided government is intuitively more likely to occur when both political parties are viable and able to mobilize enough voters to elect their candidates. Thus, as political competition increases, divided

government is more likely to occur. But increases in political competition intuitively should also lead to higher turnout as the parties mobilize more voters. Empirical studies that do not control for factors that affect competition may under- or overestimate the effect of divided government on turnout, and by inference separation of powers. Franklin and Hirczy de Mino (1988) recognize this effect and include in their estimation the margin of victory in the presidential election to attempt to control for these effects. But margin of victory is an endogenous variable, since party mobilization efforts, and as a consequence turnout, also affects that margin. Hence, including the margin of victory as an independent variable and treating it as an exogenous variable can lead to misleading inferences.<sup>1</sup>

In this paper we build on Franklin's insight but take a different approach. We exploit the fact that there is significant variation in the institutional structure of separation of powers across states, variation that does not exist over time in the federal government. This institutional variation allows us to avoid the endogeneity problem. And this institutional variation also allows us to determine which systems operate more like classic separation of powers systems and which operate like systems in which power is not separated, thereby giving us a window on the effect of separation of powers on turnout.

We identify three sources of institutional variation across separation of powers systems in the U.S. state. First, states vary in the balance of power between governors and legislatures in the way in which appointments are made. At the U.S. level, of course,

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<sup>1</sup> When an endogenous variable is included as an explanatory variable it is as if the error term included an omitted variable that was correlated with the margin of victory. Hence, the covariance between margin of victory and the error term in the regression is not equal to zero, the estimated parameters are inconsistent, and the causal inference is incorrect (Wooldridge 2002).

officials such as the Attorney General and the Secretary of State generally are appointed in similar ways; but in the states there is tremendous variation in how officials are appointed to deal with issues such as corrections, elementary and secondary education, health services, transportation, public utilities, and welfare. Second, governors also vary in their power over the budget. In some states they have significant power to determine the budget and can ignore the legislature, in others they must share power of the budget with the legislature, and in still others the legislature has more control than the governor over the budget. Third, governors vary in their ability to veto state legislative acts. In some states governors have line item veto authority and state legislatures are constrained in their ability to override these vetoes, while in others governors' vetoes are easily overcome by simple majorities.

The variation in how separation of powers is institutionalized across states means that we can rank states on how balanced the power is between the governor and state legislature. States in which appointments require involvement from both branches, or where budgets are decided through a check and balance procedure, or where the veto process leads to largely equal relations between the two branches, can be viewed as having a more classic separation of powers system. In contrast, states in which powers over appointment, budgets, and vetoes are largely unequal are closer to nonseparation of powers system. By using measures of these institutional differences we can consider the direct effect of these differences in separation of powers on turnout levels across the states. In the next section we discuss our approach and its relationship to the general literature.

### **Our Approach to Turnout**

The decision whether to vote in an election is made by individuals. As such, one of the enduring paradoxes of voting is why individuals participate at all, given that the probability that their votes are likely to be pivotal in determining the outcome of an election is negligible and therefore the expected benefits from voting is less than the cost of participation in most elections (see Feddersen 2004 for a review of the literature). The alternative perspective that voting is primarily explained by demographic and sociological characteristics which determine individuals' consumption benefits from voting independent of strategic calculations suffers because it cannot explain why factors that affect expected benefits such as the closeness of an election or separation of powers might affect turnout decisions.

A growing body of research reconciles these two perspectives by modeling turnout of voters as the outcome of strategic mobilization choices of group leaders who provide voters with consumption benefits in order to induce them to participate (Herrera, Levine, and Martinelli 2005, Martinelli 2005, Morton 1987, 1991, Schram 1991, Uhlaner 1989). At a group level, the probability of being pivotal is sufficiently large enough to overcome the cost of inducing members to participate. Thus, factors that affect the expected benefits are predicted to affect turnout in the aggregate. A second body of work derives group strategic voting endogenously (Feddersen and Sandroni 2006, Herrera and Martinelli 2004, Coate and Conlin 2004). In this research, individual voters understand the interactive effects of their participation choices and either because they derive social utility from participating as a group or ethically desire to contribute to

achieving group welfare, they participate in elections as if their choices are coordinated by an outside leader.

We take a group perspective as well, although we are agnostic as to the exact process by which groups mobilize voters (i.e., whether it is endogenous due to ethical or social motivations or group leaders use consumption benefits to induce voters). We assume that turnout in the aggregate is a function of the expected benefits and costs of participation at that level and we empirically model turnout in this fashion. The group view of turnout has received support from both experimental and nonexperimental studies of turnout (Coate and Conlin 2004, Filer, Kenny, and Morton 1993, Grosser and Schram 2006, and Shachar and Nalebuff 1999).

Although we are agnostic about the sources of why voters might act as a group, the two approaches may have different implications for the relationship between separation of powers and turnout. If mobilization is in response to group leaders' actions and group leaders are less likely to experience informational difficulties under a separation of powers system, then we may find little effect of separation of powers on turnout. If we assume that group voting decisions are endogenous due to social or ethnic motivations of individuals to act as a group rather than group leader driven, then informational difficulties faced by individual voters under separation of powers is more likely to be expected.

Most political science research on turnout uses individual level survey data. The implicit presumption behind such empirical analysis is that turnout is an individual level decision and that there are no effects across voters on those decisions. After controlling for the hypothesized exogenous aggregate and individual variables, the remaining errors

across voters are assumed to be independent. If we were to use individual survey data to estimate the causal effect of separation of powers on turnout decisions, we would need to make what is usually called the Stable Unit Treatment Value Assumption (SUTVA) in order to generalize from the individual data to the aggregate effects that we theoretically hypothesize. SUTVA is actually a host of assumptions, which are explained more expansively in Morton and Williams (2006). One of these assumptions is that there are no equilibrium effects of divided government across voters so that individual effects can be safely aggregated as if they are independent, something that directly contradicts group theories of voting where we assume that voter choices are either endogenously related at a group level or induced to be so by group leaders. Thus, in order to estimate the aggregate effect of separation of powers systems on turnout, we use aggregate turnout data rather than individual data.

### **Empirical Analysis**

Our empirical analysis focuses on the relationship between two principal variables: turnout and institutional differences in separation of powers. Since we will examine the variation in separation of powers across state governments, the relevant turnout variable is turnout in state elections. We gathered data on turnout in all state gubernatorial elections from 1980 to 2000.<sup>2</sup> We obtained vote totals in these elections, which we divided by the eligible voting population, thereby accounting for differences

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<sup>2</sup> We gathered this data, as well as the presidential turnout data we discuss below, from Scammon, McGillivray, and Cook (2006). We are currently compiling state legislative election data.

across states in the number of noncitizens and disenfranchised felons (McDonald and Popkin 2001; McDonald 2002).

In order to estimate the effects of institutional differences in separation of powers systems, one approach would be to regress gubernatorial turnout on the specific institutional variables of interest, while also controlling for other factors that are likely to influence the level of turnout. This is the approach followed by, for example, Smith (2001), Horowitz (2005) and Primo and Milyo (2006) in their investigations of the factors that predict state-level turnout. In each of these studies, the researchers identified the variable of interest that differed across states – in the three works just cited, ballot initiatives, divided government, and campaign finance laws, respectively – and then estimated the effects of these variables while controlling for a wide range of demographic variables, such as education, electoral laws, age, and so on. In other words, these studies use these control variables to establish a baseline level of turnout, and then examine whether their variables of interest have an independent effect on turnout.

We take a different empirical approach, one that builds on the recognition that significant variation in turnout in presidential and midterm elections across states is a function of state level differences (Springer 2006). There are undoubtedly many factors (e.g., demographics, electoral laws, etc.) that might influence turnout levels in each state, that vary by state, and that we might control for.<sup>3</sup> The key insight is that, for the most

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<sup>3</sup> Not surprisingly, while there is overlap in the set of variables that others have included, there is also considerable difference both in what they include and in what results they find for these variables. For example, Primo and Milyo (2006) include twelve control variables, three of which (%black, poll tax, off-year election) are significant at  $p < .05$  with a two-tailed test; Horowitz (2005) includes seven control variables, four of which (closing date, closeness of the election, % black, and % unemployed) are significant; and

part, we expect these variables to influence turnout overall and not to have separate effects for different levels of races. Thus, an alternative to including a long list of control variables is to incorporate a baseline measure into our regressions. We do this by constructing our dependent variable, *G-P Turnout*, as the difference between our gubernatorial turnout measure and the turnout in the most recent past or concurrent presidential election. Including presidential turnout in this way allows us to limit our set of independent variables to only those variables that we expect to have differential effects for national and state level elections. In other words, it allows us to examine only those factors that influence turnout for state-level offices above and beyond the factors that influence turnout for national-level offices.

Our central focus concerns whether institutional variation across states influences state-level turnout. As we discussed in the introduction, states in which powers are allocated roughly evenly between the legislature and the executive are examples of systems in which there is both a *de jure* and *de facto* separation of powers. In such systems, voters will have a more difficult time assigning responsibility for policymaking and hence will have less of an incentive to vote. In other states, however, either the governor or the legislature maintains a greater proportion of power, relative to the other branch, and these systems are not “pure” separation of powers systems, but rather exhibit some similarities with systems that do *not* have a separation of powers. In these states, policymaking responsibility is easier to assign, which should increase the likelihood that citizens will vote. In general, then, we expect that the more balanced, or shared, the

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Smith (2001) includes seven controls, two of which (unemployment and purging years) are significant.

power is between the governor and the legislature, the lower the level of turnout (relative to presidential turnout).

To construct our measures of institutional variation in separation of powers, we draw on Thad Beyle's (2005, 2006) coding of governor's powers. In particular we focus on three different institutional measures: the power of the governor to appoint other officials in the executive branch (e.g., Secretary of State); the control the governor has over the state budget; and the ability of the governor to veto state legislative acts. In each of these areas, Beyle examined the power of the governor in each state and each year, and created a scale, with a maximum value of 5, to capture the level of power. For example, in the area of vetoes, a score of 0 is assigned to states in which governors do not have the ability to veto any laws or portions of laws, while a score of 5 is given to states in which governors have not only a regular veto, but also a line item veto, and in which the legislature must muster a supermajority of legislators in order to override a veto.

Since we are interested in measuring institutional variation within separation of powers systems, we wish to create variables that measure the degree to which power is balanced between the governor and the legislature. Thus, we transform the three measures above in order to construct new measures that capture whether power is evenly shared between the two branches. We create three variables – *Appointment Balanced*, *Budget Balanced*, and *Veto Balanced* – that measure the balance between the governor and the legislature in each category. These variables are coded 1 if power over these areas is shared roughly equally by the two branches, and is otherwise coded as 0.<sup>4</sup> More

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<sup>4</sup> Specifically, appointment power is defined as balanced if the governor appoints, but the legislature must approve; budgetary power is defined as balanced if the governor shares responsibility with the legislature over the budget; and veto power is defined as balanced

specifically, if Beyle assigns a score of 3 to a state, then our *Balanced* dummy variable takes on a value of 1; and if Beyle assigns any score other than 3, then our dummy variable takes on a value of 0.

The area of budgetary politics provides an example of how this works. States that Beyle codes as having a score of 3 are ones in which the governor has full responsibility for submitting a budget, but the legislature has unlimited power to change the budget. This is the category in which power between the legislature and the governor is most evenly shared, and thus in which responsibility for political and policy decisions is most blurred; hence, our *Budget Balanced* measure takes on a value of 1 for states that Beyle codes as a 3, and takes on a value of 0 for all other states (e.g., ones in which either the governor or legislature has more power, relative to the other institution). Similarly, in the areas of appointments and vetoes, states that Beyle scores as a 3 are those in which power is most evenly balanced between the two branches, and we therefore code *Appointment Balanced* and *Veto Balanced* as 1 for those states, and 0 for other states.<sup>5</sup> Descriptive statistics on these variables by state and in total is presented below in Table 1.

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if the governor has a line item veto but only a majority of the legislature is needed to override a veto. We recognize that these definitions are subjective and plan to consider other alternatives to future research.

<sup>5</sup> As we discuss below, the data for appointments is, in our view, more problematic than the data for budgets and vetoes. It is possible that a Beyle score of 4, rather than 3, can be seen as representing the greatest level of balance between the governor and the legislature in the area of appointments. Coding *Appointment Balanced* as 1 for a Beyle score of 4, and 0 otherwise, produces results very similar to those we present. In addition, one might argue that rather than creating a dummy variable that represents whether or not power is balanced, we should take more advantage of Beyle's rankings and create a more continuous measure. We discuss this later in the paper.

**Table 1: Mean Values of Selected Variables by State  
(Standard Deviation of *G-P Turnout* in Parentheses)**

<b>State</b>	<b><i>G-P Turnout</i></b>	<b><i>Appt. Balanced</i></b>	<b><i>Budget Balanced</i></b>	<b><i>Veto Balanced</i></b>	<b>Obs.</b>
Alabama	-9.28 (4.36)	0.8	0.4	0	5
Alaska	-6.30 (9.01)	0.2	0.4	0	5
Arizona	-12.40 (3.58)	0.8	0.4	0	5
Arkansas	-9.14 (9.49)	0.29	0.29	0	7
California	-12.92 (6.03)	0.5	0.25	0	4
Colorado	-16.29 (4.26)	0.75	0	0	4
Connecticut	-17.97 (0.351)	1	0.4	0	5
Delaware	-1.75 (1.13)	0.33	0.33	0	6
Florida	-13.39 (1.91)	0.2	0.4	0	5
Georgia	-13.44 (4.13)	0	0.4	0	5
Hawaii	-1.93 (0.85)	0.5	0.25	0	4
Idaho	-13.65 (5.11)	0.4	0.4	0	5
Illinois	-19.82 (6.29)	0.25	0.25	0	4
Indiana	-1.14 (0.57)	0	0.2	0	5
Iowa	-14.93 (3.73)	0.4	0.4	0	5
Kansas	-13.96 (3.96)	0.25	0.25	0	4
Kentucky	-20.12 (6.64)	0.4	0.2	0	5
Louisiana	-5.33 (8.87)	0.4	0.2	0	5
Maine	-12.81 (7.19)	0	0.25	0	4
Maryland	-15.74 (6.14)	0.4	0	0	5
Massachusetts	-14.41 (7.80)	0	0.25	0.5	4
Michigan	-18.95 (5.23)	0.25	0.25	0	4
Minnesota	-14.19 (8.16)	0.4	0.4	0	5
Mississippi	-11.54 (2.88)	0	0.33	0	6
Missouri	-0.48 (0.38)	0.67	0.33	0	6
Montana	-0.43 (0.44)	0.4	0.2	0.4	5
Nebraska	-10.03 (3.61)	0.4	0	0	5
Nevada	-9.40 (5.35)	0.8	0.4	0	5
New Hampshire	-10.21 (11.54)	0.56	0.22	0	9
New Jersey	-15.96 (4.82)	0.2	0.2	0	5
New Mexico	-8.47 (5.06)	0.4	0.2	0.4	5
New York	-16.37 (6.69)	0	0	0	4
North Carolina	0.50 (0.63)	0.33	0.67	0	6
North Dakota	0.03 (0.73)	0.6	0.2	0	5
Ohio	-16.66 (6.44)	0.5	0.25	0	4

<b>State</b>	<b><i>G-P Turnout</i></b>	<b><i>Appt. Balanced</i></b>	<b><i>Budget Balanced</i></b>	<b><i>Veto Balanced</i></b>	<b>Obs.</b>
Oklahoma	-14.69 (3.41)	0	0.4	0	5
Oregon	-9.24 (4.93)	0.25	0.25	0.5	4
Pennsylvania	-15.10 (3.54)	0	0.25	0	4
Rhode Island	-7.22 (5.76)	0.33	0.22	0	9
South Carolina	-9.39 (3.02)	0	0	0.4	5
South Dakota	-8.17 (2.18)	0.75	0.25	0	4
Tennessee	-17.92 (5.30)	0	0.4	0	5
Texas	-16.63 (1.63)	0	0	0.4	5
Utah	0.57 (0.72)	0.75	0.25	0	4
Vermont	-8.53 (8.82)	0.22	0.22	0	9
Virginia	-14.52 (3.84)	0	0.17	0.5	6
Washington	-0.21 (0.34)	0.33	0.33	0.33	6
West Virginia	-1.49 (3.61)	0.67	0	0	6
Wisconsin	-21.27 (5.21)	0.6	0.4	0.4	5
Wyoming	-5.88 (3.35)	0.2	0.4	0	5
Total	-10.19 (7.85)	0.35	0.26	0.07	257

Interestingly, these balance variables are not highly correlated. The correlation coefficient between *Appointment Balanced* and *Budget Balanced* is 0.19, between *Appt. Balanced* and *Veto Balanced* is -0.08, and between *Budget Balanced* and *Veto Balanced* is -0.17. In general, then, none of these measures of executive and legislative power show a high level of correlation with the other measures. Thus, each measure arguably captures a distinct aspect of executive/legislative arrangements in sharing power. By having these three separate measures, we can determine which of these arrangements affects turnout the most (or the least).

Although these three variables are our main focus, we also control initially for three other factors that might affect gubernatorial turnout (and later investigate the effects of other variables). First, turnout for state level elections is likely to be higher when gubernatorial and state legislative races occur concurrently. Although there are many

cases in which state legislative elections occur in the absence of gubernatorial elections, gubernatorial elections without state legislative elections occurred in our dataset only in three instances – Kentucky in 1987, 1991, and 1999. To control for these races, we create a dummy variable, *No Legislative Election*, which takes on a value of 1 for those three observations and 0 for all others.<sup>6</sup> Second, we expect that turnout in gubernatorial elections will be higher relative to most recent presidential elections when the two elections are concurrent. We include a binary independent variable, *Presidential Election*, which we set equal to 1 if the two elections are concurrent and 0 otherwise. Finally, in order to control for any effects of having gubernatorial elections at the same time as congressional elections, we include a binary independent variable, *Midterm Election*, which is equal to 1 if the gubernatorial election occurs concurrently with a midterm congressional election.

### *Estimation and Results*

We estimated a simple ordinary least squares equation with *G-P Turnout* as our dependent variable and our independent variables *Appt Balanced*, *Budget Balanced*, *Veto Balance*, *Pres. Election*, *Midterm Election*, *KY1*, and *KY2*. Because observations within states are not independent, we cluster by state, since failure to do so could result in incorrectly calculated (and inaccurately small) standard errors (Primo, Jacobsmeier, and Milyo 2007). The results from this estimation are presented below in Table 2.

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<sup>6</sup> There are many more cases in which there are state legislative elections but no concurrent gubernatorial election. However, we currently do not have data on legislative elections.

**Table 2: OLS Estimation, Dependent Variable = *G-P Turnout* (1980-2000)**

<b>Independent Variable</b>	<b>Coefficient</b>	<b>Std. Err.</b>	<b>t statistic</b>	<b>P&gt; t </b>	<b>95% Conf. Interval</b>
<i>Appointment Balanced</i>	-0.12	0.77	-0.16	0.87	-1.66 to 1.42
<i>Budget Balanced</i>	-2.35	0.65	-3.64	0.001	-3.65 to -1.05
<i>Veto Balanced</i>	-2.59	1.50	-1.72	0.09	-5.61 to 0.43
<i>Presidential Election</i>	10.65	2.04	5.23	0.00	6.56 to 14.74
<i>Midterm Election</i>	-1.24	2.06	-0.60	0.55	-5.37 to 2.89
<i>No Legislative Election</i>	-10.80	1.92	-5.63	0.00	-14.65 to -6.94
<i>Constant</i>	-11.19	1.95	-5.75	0.00	-15.10 to -7.27
Number of observations = 257					
Adjusted R-Squared = 0.49					

Our results provide significant support for the contention that separation of powers reduces voter turnout. We find that all three of the separation of powers variables have the predicted negative effect on the turnout difference, although the variable measuring balance in appointment power is insignificant. We tested to see if the effect of balanced power in budgets is significantly less than the effect of balanced power in vetoes and we found that we could not reject the null hypothesis of no significant difference. Thus, balanced power in budgets and balanced power in vetoes each reduce turnout in gubernatorial elections relative to presidential ones by over 2%. Given that in some hotly contested races, 2% of the vote can determine the outcome as in the recent gubernatorial contest in Washington, this difference can be substantively meaningful, particularly if those who are choosing not to participate have preferences different from the majority of voters.

It is worth noting, at this point, a difference between *Appointment Balanced* and the other two institutional variables. For both *Budget Balanced* and *Veto Balanced*, higher scores on Beyle's scale have two implications – first, the higher (or lower) the score, the more powerful (or weaker) the governor; and second, the increase (or decrease) in the governor's power comes at the expense (or benefit) of the legislature. For *Appointment Balanced*, however, only the first of these implications holds. Higher scores do indeed correspond to more powerful governors. A score of 5, for example, indicates that the governor acts alone in making appointments, while a score of 1 indicates that someone else makes the appointment, with the governor playing no role at all. This decrease in power as we move down Beyle's scale, however, does not necessarily go hand in hand with an increase in the legislature's power. For a score of 5, obviously, the legislature plays no role at all, since the governor has complete control over appointments. A score of 1, on the other hand, does not necessarily imply a large amount of legislative power, since a state receives this code if anyone other than the governor appoints people, and the governor plays no role. In other words, the legislature might not play any role at all in states that Beyle codes as a 1. Similarly, for a score of 3, someone other than the governor – although not necessarily the legislature – makes the appointment, which the governor then approves; or else the governor and the legislature share power. Again, however, the legislature might not play a role in this category. Given this ambiguity in the coding, an ambiguity that does not exist to nearly the same extent in the other two areas, it is not altogether surprising that we do not find significant results for *Appointment Balanced*.

The other results in Table 2 generally conform to expectations. We find, not surprisingly, that when presidential and gubernatorial elections are concurrent, gubernatorial turnout relative to presidential turnout is significantly less, by approximately 10%. We also find that gubernatorial elections that are held independently of state legislative elections had significantly lower turnouts, relative to presidential turnout, than other gubernatorial elections. On the other hand, we find no significant difference in turnout if gubernatorial elections are held concurrently with congressional midterm elections or are not.

#### *Additional tests*

Our results so far indicate that systems in which power is shared equally between legislative and executive institutions – that is, systems that most closely approximate pure separation of powers systems – produce a decline in turnout. Voters are more likely to turnout in systems where responsibility can be more accurately assigned to either the governor or the legislative branch.<sup>7</sup> Given the nature of our dependent variable, which compares gubernatorial turnout to presidential turnout, we need to be cautious. In

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<sup>7</sup> A counter argument would be that people are more likely to vote for a powerful governor than a weak governor, irrespective of the legislature's power. Although we do not include the overall level of power in our regressions along with the balance measures, due to collinearity, if we substitute the overall level for the balance measures, we find some support for this argument: both the overall level of the governor's budget and veto powers are positive, as this argument would predict, although only the budget power is significant. The other problem is that state legislative and gubernatorial elections generally take place at the same time. As we reported in the text, there were only three observations in which a state held a gubernatorial election but did *not* elect legislators at the same time. Consequently, gubernatorial turnout is likely to be affected by factors that influence turnout in state legislative elections; and one could argue that with either strong governors *or* strong legislatures we should see an increased level of gubernatorial turnout. We address this, at least partially, below.

particular, our results in Table 2 also show that turnout is markedly higher when gubernatorial and presidential elections are held concurrently, which raises the possibility that the effects of our institutional separation of powers variables may operate differently for observations in which gubernatorial elections are distinct from presidential elections than for observations in which they are concurrent. More specifically, in presidential years, turnout in gubernatorial elections is likely to be artificially high, due to the presidential election (i.e., people who vote for governor might not have turnout to vote in the absence of the presidential election). Consequently, state-level institutional factors might not have much of an effect in presidential years, as turnout is driven by factors related to the national race. In non-presidential years, on the other hand, the construction of our dependent variable allows us to compare gubernatorial turnout to a baseline level of turnout, allowing us to investigate state-level factors.

We investigate this possibility by splitting our dataset into two groups – presidential elections years and non-presidential election years. Initial analysis supports the idea that turnout will be different in these two categories, with gubernatorial turnout only 1.34% lower, on average, than presidential turnout in presidential election years, but 13.31% lower in non-presidential election years. Table 3 then demonstrates that the institutional variables do indeed operate differently depending on whether the gubernatorial election takes place concurrently with a presidential election, and that the differences conform to our expectations. Equation 1, which replicates the analysis from Table 2 for the purpose of comparison, contains all observations; Equation 2 includes only those gubernatorial elections that take place independent of presidential elections;

and Equation 3 includes only those gubernatorial elections that take place concurrently with presidential elections.

**Table 3: OLS Estimation, Dependent Variable = *G-P Turnout* (1980-2000)**

<b>Independent Variable</b>	<b>Equation 1 <i>All Years</i></b>	<b>Equation 2 <i>Non-Presidential Election Years</i></b>	<b>Equation 3 <i>Presidential Election Years</i></b>
<i>Appointment Balanced</i>	-0.12 (0.77) 0.87	-0.77 (1.02) 0.45	0.69 (0.82) 0.41
<i>Budget Balanced</i>	-2.35 (0.65) 0.001	-3.51 (0.74) 0.00	1.10 (0.49) 0.04
<i>Veto Balanced</i>	-2.59 (1.50) 0.09	-2.91 (1.53) 0.07	-2.10 (3.20) 0.52
<i>Presidential Election</i>	10.65 (2.04) 0.00	--	--
<i>Midterm Election</i>	-1.24 (2.06) 0.55	-1.08 (2.09) 0.61	--
<i>No Legislative Election</i>	-10.80 (1.92) 0.00	-10.60 (1.97) 0.00	--
<i>Constant</i>	-11.19 (1.95) 0.00	-10.77 (2.02) 0.00	-1.74 (0.68) 0.02
N	257	190	67
Adjuster R	0.49	0.11	0.09
Notes: Entries in each cell are, from top to bottom: coefficient, standard error, p-value.			

As Equation 2 shows, in non-presidential election years we continue to find the expected pattern. All three institutional variables are negative, with *Budget Balanced* achieving a high level of statistical significance and *Veto Balanced* remaining significant at the  $p < .10$  level. Overall, in non-presidential election years, turnout is about 3% lower

in states in which veto power between the president and the legislature is evenly balanced, and 3.5% lower in states in which budgetary power is balanced evenly. In other words, in political systems in which power is not clearly apportioned between the two branches, and thus systems that are closer to the pure idea of separation of powers, balance decreases turnout. But when we look at presidential years, we see a different story. Neither *Veto Balanced* nor *Appointment Balanced* are statistically significant, and although *Budget Balanced* is significant, its sign is the opposite from what we would expect. In general, then, it seems clear that the results shown in Equation 1 are driven by the effects of our institutional variables in non-presidential election years.

Finally, we address two additional questions. First, we examine whether our results hold up if we control for additional factors. Recall that our dependent variable is the difference between gubernatorial turnout and an established baseline of turnout, as measured by turnout in the most recent presidential election. This approach allows us to omit any potential independent variables that might influence gubernatorial turnout but that also influence presidential turnout, and to focus on only those variables that should either affect only gubernatorial turnout or that might have differential effects on presidential and gubernatorial turnout. Our three balance variables, for example, fall into the first category: they can be predicted to influence gubernatorial turnout, but are not likely to have any independent effect on presidential turnout.

In Equation 1 of Table 4, we include several other variables as controls.<sup>8</sup> The first two can be posited to have only state-level effects, and thus can be considered exogenous to presidential turnout. First, building on Smith (2001), who found that the initiative can

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<sup>8</sup> Building on our findings in Table 3, our results in Table 4 use observations from only non-presidential election years.

spur turnout in gubernatorial elections, we include a dummy variable, *Initiative*, that indicates whether or not the state has an initiative.<sup>9</sup> The expectation is that the presence of initiatives should act to increase turnout. Second, following Horowitz (2005), Franklin and Hirczy de Mino (1998), and Franklin (2004), we investigate whether divided government increases or decreases turnout. We distinguish between two types of divided government – *Divided Government (Unified Legislature)* and *Divided Government (Split Legislature)* – to see whether either has an effect on turnout.<sup>10</sup> We also include two other variables that might influence gubernatorial turnout differently from presidential turnout. First, the larger the population in a state, the less connected citizens may feel to their state's government. Hence, we expect *Population* to have a negative influence on turnout. Second, education might have an influence on gubernatorial turnout, above and beyond its influence on presidential turnout, with more educated populations more likely to pay attention to, and vote in, gubernatorial elections than presidential elections. We capture this effect with *High School Education*, which measures the percentage of the state's population that has graduated from high school.

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<sup>9</sup> Smith's (2001) argument involves the salience of initiatives on the ballot, and he finds, counter to previous research, the presence of salient initiatives increases turnout. Given that we do not account for the salience of initiatives, but rather only whether the state allows for initiatives, this likely creates a bias against obtaining significant results. Furthermore, Tolbert, Grummel, and Smith (2001) find that the presence of initiatives alone is associated with an increase in state-level turnout for national offices, which could further decrease the likelihood that we would find significant results, given the construction of our dependent variables. It is worth noting also that other studies have demonstrated that the presence of an initiative (e.g., Boehmke 2005; Gerber 1999) can have indirect effects on political actions and outcomes.

<sup>10</sup> Franklin (2004) and Franklin and Hirczy de Mino (1998) find that divided government decreases turnout. Their analysis, however, focuses on U.S. national elections. More relevant for our analysis, Horowitz (2005) finds no effect for divided government. Our results are unchanged if we combine our two divided government measures into one.

**Table 4: OLS Estimation, Dependent Variable = *G-P Turnout* (1980-2000)**

<b>Independent Variable</b>	<b>Equation 1</b>	<b>Equation 2</b>
<i>Appointment Balanced</i>	-1.08 (1.00) 0.28	--
<i>Budget Balanced</i>	-3.85 (0.75) 0.00	--
<i>Veto Balanced</i>	-2.48 (1.32) 0.07	--
<i>Appointment, Degree of Balance</i>	--	0.57 (0.66) 0.40
<i>Budget, Degree of Balance</i>	--	1.73 (0.36) 0.00
<i>Veto, Degree of Balance</i>	--	1.89 (0.75) 0.02
<i>Midterm Election</i>	-0.99 (1.96) 0.62	-0.80 (2.01) 0.70
<i>No Legislative Election</i>	-10.82 (2.21) 0.00	-10.60 (1.97) 0.00
<i>Initiative</i>	2.28 (1.24) 0.07	2.00 (1.27) 0.12
<i>Divided Government (Unified Legislature)</i>	0.13 (1.22) 0.92	0.14 (1.16) 0.90
<i>Divided Government (Split Legislature)</i>	0.63 (1.69) 0.71	0.71 (1.69) 0.68
<i>Population (millions)</i>	-3.10 (1.31) 0.02	-3.64 (1.47) 0.02
<i>High School Education</i>	-0.09 (0.13) 0.51	-0.15 (0.13) 0.24
<i>Constant</i>	-3.36 (9.78) 0.73	-5.84 (9.63) 0.55

N	190	190
Adjusted R-squared	0.20	0.20
Notes: Entries in each cell are, from top to bottom: coefficient, standard error, p-value. Observations include all gubernatorial elections in non-presidential election years.		

The results demonstrate that the presence of initiatives can increase gubernatorial turnout, relative to presidential turnout, and the larger state populations produce a decrease in turnout. The level of education does not have any effect, independent of its effect on presidential turnout. And divided government has no influence on turnout, although arguably this is because divided government is endogenous, in that some of the factors that increase presidential turnout can also produce divided government.<sup>11</sup> More importantly, we continue to find support for the idea that institutional factors affect the level of turnout, and thus that a sharing of powers across institutions leads to a decrease in turnout.

Finally, we construct a different measure of balance. The measure we have been using so far is simply a dummy variable that takes on a value of 1 when the powers of the governor and the legislature are balanced (i.e., when they are coded as a “3” in Beyle’s ratings) and 0 otherwise. Instead of conceiving of balance as a dichotomous proposition, it is also possible to think of it as being a more continuous variable. In other words a state that receives a 3 from Beyle in one of these categories is balanced; a state that receives a 4 is less balanced; and a state that receives a 5 is even less balanced. Hence, we create three variables that correspond to our earlier measures of appointment,

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<sup>11</sup> Divided government might also be hypothesized to have a conditioning effect on our institutional variables – that is, our measures of balance might matter differently under divided government than under unified government. Although this seems plausible, we found no such effects when we tested for them.

budgetary, and veto power, but that focus instead on the *degree of balance*. These scores are simply calculated as the absolute value of the difference between the state's score and 3, which is the point on Beyle's scale that indicates the most balance between the two branches. Higher values of this score, then, indicate *less* balance, clearer lines of responsibility regarding which branch has political power, and a less "pure" separation of powers system. We therefore expect to find a positive coefficient for these variables.

Equation 2 of Table 4 demonstrates that we do indeed find such an effect. All three variables that measure the degree of balance produce positive effects, and two of these three variables (*Budget, Degree of Balance* and *Veto, Degree of Balance*) are strongly significant, indicating that less balanced systems produce an increase in turnout. Once again, the results show that more classic separation of powers systems, where power is shared roughly equally between the two branches, show evidence of lower turnout.

### **Conclusions**

A puzzling issue in studies of voter turnout is the lower participation rates in U.S. elections as compared to other countries. One of the many potential causes that have been suggested by researchers is the difference between the U.S. and most other countries in how governmental power is shared. However, because of the multiplicity of other potential causes of differences in voter turnout compared to available observations, cross-national studies cannot determine accurately whether a causal relationship exists between separation of powers systems and lower voter turnout. In this paper we take advantage of variation in separation of powers across states in the United States and are better able to

measure the effects of this variation on turnout in state elections. We find that indeed when power over budgets and through vetoes is more balanced between governors and state legislatures, turnout is significantly lower. The effect is substantial enough to affect the outcomes of close races for governor. Our results strongly support the hypothesis that one of the causes of lower voter turnout in the U.S. is the separation of powers system.

As noted above, in our analysis we take the approach that voters participate in response to benefits and costs at a group level but are agnostic as to how these groups of voters are induced to participate – whether participation is induced through group leaders’ actions to mobilize voters with selective incentives or individual voters endogenously act as a group due to ethical or social motivations. However, our results showing the effects of separation of powers on participation provide more support for the view that individual voters act endogenously as groups in response to ethical and social motivations since the informational problems caused by separation of powers are less likely to affect group leaders’ actions and are more likely to affect the ability of individual voters to perceive the benefits for their groups. Similarly, these results suggest that voters’ participation decisions are not purely expressive as well, but reflective of strategic concerns, albeit influenced by ethical and social motivations, since how power is shared should not affect voters’ willingness to simply express preferences for a particular party or candidate.

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