

Irresponsible Parties, Responsible Voters? Legislative Gridlock and Collective Accountability*

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Abstract
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Legislative gridlock is a failure of one of the key functions of government: to pass legislation. Can voters counter such political dysfunction? This paper examines whether and how voters hold politicians accountable for gridlock. We focus on the passage of the government budget, the central task of any legislature, and define a legislature to experience budgetary gridlock if it fails to pass the budget on time. We document, based on twenty years of budget enactment data, that voters hold state legislators accountable for budget gridlock in US state governments, with gridlocked incumbents losing their seat more often than incumbents passing budgets on time. Based on established theories of party organization in American politics, we develop three competing theoretical hypotheses to guide our understanding of the observed patterns of retrospective voting. We find strong support for collective electoral accountability with voters punishing incumbent members of state legislature majority parties.

Keywords: electoral accountability, collective responsibility, gridlock, responsible parties,
late budgets

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“Good government is [...] more than a forum for competing viewpoints or a sounding board for complaints; it actually gets things done.”

Robert D. Putnam, *Making Democracy Work*, p. 63.

“Why is our legislature so unbelievably incompetent? One of their biggest, and arguably most important, jobs is to pass a budget. They can't even do that. I am so utterly disappointed in my state. It makes me sick to my stomach.”

'Danny' in comments on *The New York Times*' website, May 4, 2010.

I. Introduction

Legislative gridlock, the inability of legislative bodies to pass important legislation (or just legislation at all), is a key concern of democratic politics, particularly in political systems with strong checks and balances. In 2013, the US federal government shut down for sixteen days due to an inability of lawmakers to negotiate a budget or, at least, a continuing resolution. Federal workers were furloughed for a combined total of 6.6 million days during the shutdown, which led to a large number of economic disruptions.¹ The federal government was also shut down in January 2018. In 2009, the state of California had to resort to Registered Warrants (popularly known as IOUs) to cover payments, passing a budget 24 days into the fiscal year; in 2010, the California budget was more than three months late.

Can voters hold lawmakers responsible for such lack of legislative productivity or does gridlock constitute a system failure that electoral accountability cannot solve? Little is known about the electoral consequences of gridlock, both theoretically and empirically, partly due to disagreement over its measurement (Mayhew, 1991, 2006; Binder, 1999, 2003), partly due to a focus on the limiting case of the US Congress (Binder 2003).

In this paper, we focus on electoral accountability for a specific set of gridlock instances across a large number of governments: budget gridlock in US state governments. While not encompassing all types of legislative gridlock, budgetary gridlock, measured by the occurrence of late budgets,

¹ Executive office of the President of the United States (2013).

has several advantages as an empirical operationalization of the broader theoretical phenomenon: Budget delays are salient, easily identifiable and do not suffer from the endogeneity concerns so often a concern in the literature on legislative gridlock and divided government.

We document that voters hold legislators accountable for budget gridlock. Across 20 years of budgets in 31 US states, 83 percent of incumbents with no budget gridlock are reelected, while 80 percent of incumbents overseeing gridlocked budgets are reelected. But this observation does not in itself tell us who voters hold responsible. Based on existing theories of legislative organization and political parties in American politics, we develop three competing hypotheses that would all be consistent with the observed relation between gridlock and reelection rates: Voters punish (1) members of the majority party of the legislature, (2) members of the governor's party or (3) all incumbents regardless of party affiliation. We find unambiguous support for the hypothesis that voters blame majority party members, a key assumption in Cox and McCubbins' (1993, 2005) procedural cartel theory. This means that when majority parties do not take on responsibility for providing good governance, voters impose accountability to hold their members responsible for legislative gridlock, as prescribed by the responsible party government model (Ranney, 1954); our empirical results imply that about one fifth of state legislative elections are sufficiently close that whether the state budget is passed on time or not could be decisive for which party will gain control over the state legislature. At the same time, the literature on state legislative elections has struggled to identify any within-state factors beyond incumbency that actually affects electoral outcomes (Chubb, 1988; Berry et al. 2000; Rogers 2017), partly due to a focus on economic outcomes for which the executive is typically held responsible (Chubb, 1988; Lowry et al., 1998) or individual legislator actions that receive little media coverage (Rogers 2017). Our argument and findings suggest that voters attribute responsibility to the state legislature for *collective* outcomes

that it can in fact affect – such as the timely passage of a budget – rather than for individual legislator actions or collective outcomes over which the legislature has only limited powers.

Budget gridlock is an obvious focus for electoral accountability for three reasons: First, the budget is the key piece of legislation for any legislature, and certainly so for US state legislatures (Rosenthal 1998). Failure to complete a budget on time often has visible consequences for citizens: While state legislatures in some cases pass temporary budgets allowing limited appropriations, other cases result in a shutdown of all non-essential services and interrupted payments to agencies, employees and state contractors. These potentially significant consequences, and the intense coverage they typically receive in local and state-wide media, mean that running a late state budget is salient for voters; this makes our measure of budget delays a natural candidate for exploring the electoral consequences of gridlock, as retrospective voting is argued to take place only over salient and easily communicated issues (Anderson, 2007). Second, studying electoral accountability for budget gridlock is facilitated by a very close link between legislative action and policy outcomes; indeed, in the case of budget passage they are one and the same, making assignment of responsibility to the legislature much clearer than in cases of economic growth or unemployment. Third, budget promptness, argued by Putnam (1993) to be a key indicator of good government, rarely favors one party or political persuasion over the other; if anything, it may be a valence issue (Butler and Powell, 2014; though see Patty, 2016, for a contrasting view, to which we return), enabling voter agreement over government performance, which in turn facilitates retrospective voting.

In addition, our measure of budget gridlock has two methodological advantages. First, as noted by Mayhew (1991), any measure of gridlock should reflect both the supply and demand for legislation: while legislative passage of a limited number of bills could reflect gridlock, it could alternatively reflect a lack of demand for legislative action. Our proposed measure does not suffer

from this problem, as the budget must be passed at regular intervals. Second, budget lateness is a conceptually simple and replicable measure of legislative gridlock, and it is comparable across states as well as time.

In our main empirical analysis, we study the electoral consequences of late budgets at the level of individual state lawmakers. This level of analysis allows us to contrast the effect for majority vs. minority party incumbents, as well as for members of the governor's party vs. members of the opposite party. It also allows us, through multinomial logit analysis, to distinguish non-successful reelection bids by the identity of the successful challenger: Some incumbents may lose to own-party challengers in the primary, while others are defeated by opposition party challengers in the general election. This is crucial for the party composition of the legislature and for our understanding of individual vs. collective responsibility. In supplementary analyses, presented in the appendix, we demonstrate that a key institutional feature of the budgetary process – whether the government shuts down or can continue operations in the case of a late budget – is an important factor for understanding the electoral response to legislative gridlock. Finally, we provide more direct evidence of the party-level effects of late budgets by estimating their impact on the share of lower-house seats won in state legislative elections by the party that had a majority in the lower house before the election. In robustness analyses, we show that our results are qualitatively the same across split vs. unified legislatures, and in the subsample of open seat elections.

These results do not necessarily reflect the causal effects of late budgets per se. If budget lateness correlates with gridlock over other issues, the estimates for the budget gridlock variable will reflect the electoral consequences of legislative gridlock more broadly. Indeed, as explained above, the budget gridlock variable should in fact be thought of as an operationalizable, yet imperfect, measure of the broader phenomenon. For simplicity, we refer below to the estimated

effects as the effects of budget gridlock but we urge the reader to keep this broader interpretation of the variable in mind.

Our paper contributes to several strands of literature: First, it is directly related to the literature on individual accountability for collective performance in American politics. Fenno (1978) and Fiorina (1980) argue that members of the US Congress face little or no accountability for their performance in office. They attribute this to a decline in the role of political parties in American politics: If parties are weak, an incumbent candidate's partisan affiliation is of little value to voters in their efforts to assign individual responsibility for collective performance, and incumbents can escape responsibility by distancing themselves from their own party. However, following the same logic, Jones (2010, 2015) argues that recent increases in party unity and partisan polarization has strengthened accountability, and he finds empirical evidence in favor of this hypothesis in an analysis of electoral races for the House of Representatives. These studies measure accountability as voter reactions to some measure of voters' subjective evaluation of congressional performance, but they do not examine how these evaluations depend on actual legislative output. In a survey-experimental setting, Butler and Powell (2014) focus on the link from legislative output and governance, in the form of late budgets and ethics violations, to voter evaluations. In contrast to this study, our paper examines the direct link from one aspect of observed legislative performance, the ability to pass the budget on time, to actual electoral outcomes.

Second, and relatedly, our results can be seen as providing empirical support for some of the fundamental, but empirically untested, assumptions in Cox and McCubbins's influential procedural cartel theory (1993, 2005). The basic premise of this theory is that the reelection prospects of individual majority-party incumbent legislators depend on voters' evaluations of the "brand" of their party, which in turn depend on the party's legislative performance. Existing empirical evidence, e.g. Jones (2010), considers the first part of this basic premise, i.e. the link from party

brand to reelection prospects, whereas our results also extend to the second part, that is, the link to actual legislative performance.

Third, our paper adds to the literature on legislative productivity. Mayhew (1991) rejected the conventional wisdom that divided government hinders the passage of important legislation at the federal level in the US, whereas a number of later studies have found evidence consistent with the conventional view (e.g. Binder 1999, 2003, Coleman 2000, Edwards et al. 1997 and Howell et al. 2000). But while there is no shortage of studies devoted to identifying causes of legislative productivity, there is very little work on how voters react to (the lack of) such legislative productivity. Indeed, to our knowledge the only study to consider electoral accountability for legislative productivity is Binder (2003). She examines the effect of legislative gridlock in Congress on the electoral fortunes of House members, but finds no evidence of a statistically significant relationship in a sample of 22 congresses; however, in our view, the lack of significance could simply reflect low statistical power due to the limited number of observations. Moreover, it is not clear to what extent these results for Congress generalize to legislatures at other levels of government.

2. Gridlock and electoral accountability: Three hypotheses

In this section, we develop a theoretical framework to formulate three competing hypotheses about the electoral consequences of budget gridlock in state legislative elections. Common to all three hypotheses is the assumption that voters have preferences not only over the economic and political outcomes delivered by their elected representatives, but also over the political process leading to these outcomes (Hibbing and Theiss-Morse 2001). In particular, we assume that voters prefer timely budget adoption and dislike delays. Stokes (1963, p. 372) defines a valence issue as an issue over which the preferences of “both parties and voters [are] located at a single point - the

position of virtue in government.” Budget timeliness, we believe, comes close to this characterization; late budgets benefit no one and are so counterproductive that avoiding them should be in the interest of all voters, regardless of ideological preferences.

If voters dislike delays in the budget process, it is natural to hypothesize that such delays affect the reputation of the politicians that are perceived as being responsible for them negatively, worsening their reelection prospects.² This is closely related to the key assumptions underlying *procedural cartel theory* (Cox and McCubbins 1993, 2005) that a party's reputation depends significantly on its record of legislative accomplishment and that this reputation in turn affects the probability of successful election outcomes for party members. In fact, Cox and McCubbins (2005, p. 22) use the decline in popularity of congressional Republicans following the 1995-96 federal budget battle as the main motivating example to support the former of these assumptions. The latter assumption is supported by a number of empirical studies showing that party brand favorability significantly affects outcomes of presidential, congressional and state legislative elections (Woon and Pope 2008, Brewer 2009, Butler and Powell 2014).

The key question separating our three hypotheses is *who* voters perceive as being responsible for budget gridlock. One possibility is that blame is assigned based on party affiliation. In that case, the next question is which party gets the blame? Cox and McCubbins (1993, 2005) argue that legislative outcomes affect the record of legislative accomplishment for the majority party in the legislature only, which implies that majority party members are held accountable for legislative outcomes in state legislative elections, whereas minority party members are not; this can be seen as a variant of the responsible party government model (Ranney, 1954, Arnold, 1990), in which the majority party in the legislature acts as the responsible party and voters should reward or punish

² Contrary to this assumption, Patty (2016) proposes a theoretical model in which the voters reward politicians for obstructing the political process. We discuss how our results relate to this model in section 4.

members of this party for the performance of the government.³ It is also possible, however, that control over the legislative branch is not what really matters: The state budget adoption process is complex and involves not only the legislature, but also the executive branch; the governor is responsible for submitting an executive budget proposal to the legislature and also holds considerable veto power of the final proposal in many states.⁴ As the most prominent figure in state politics, it is therefore possible that voters blame the governor when the budget adoption process is gridlocked. Further, the literature on congressional politics has found that voters' perceptions of the president's performance have a significant impact on their attitudes towards members of the president's party (e.g. Fiorina 1981). If a similar mechanism is present at the state level, we should expect voters to punish members of the governor's party for budget gridlock, irrespective of who controls the state legislature. Finally, a third possibility is that party affiliation is unimportant because voters assign blame based on incumbency alone. Krehbiel (1998) studies the causes of legislative gridlock in Congress and argues that political parties play no major role in this context. If voters in state legislative elections hold similar views, all incumbent legislators may face adverse electoral consequences for budget impasses, and there is no reason to expect differential effects across party lines. We summarize these ideas in the following three hypotheses:

H1: Voters disapprove of budget delays and blame members of the party that holds a majority in the legislature when they occur.

H2: Voters disapprove of budget delays and blame members of the governor's party when they occur.

³ Jones and McDermott (2004) provide empirical evidence for a similar model at the federal level, finding that voters' attitudes toward Congress's job performance affect their support for candidates from the congressional majority party.

⁴ The National Association of State Budget Officers: *Budget Processes in the States*, various years.

H3: Voters disapprove of budget delays and blame incumbent legislators from all parties when they occur.

These hypotheses lead to different predictions about the electoral outcomes for individual legislators: Under H1, majority party incumbents should face a lower probability of reelection if the state has experienced delays in the budget process since the previous election. In contrast, minority party incumbents should experience no such effect. If the damaged reputation of the majority party extends to *all* of its members, including non-incumbent challengers in districts held by the minority party, minority party incumbents could even benefit from budget delays through a higher probability of reelection. H2 predicts a similar dichotomy in the electoral effects of budget delays: incumbents belonging to the same party as the governor should be reelected less frequently when the budget has been delayed, while the effect should be zero or even positive for incumbents belonging to the opposite party, depending on whether the electoral fallout from budgetary gridlock affects *all* members of the governor's party or incumbent members only. In contrast, H3 predicts lower reelection probabilities for all incumbents in the state legislature, irrespective of party affiliation.

The impact of late budgets on the aggregate electoral performance of political parties under each of these hypotheses is not a priori clear, since it depends entirely on whether the ousted incumbents are replaced by someone from their own party (through primary election defeats), or by someone from another party. Obtaining precise predictions therefore requires further assumptions: Under Hypotheses 1 and 2, voters assign responsibility for budgetary gridlock on a strictly partisan basis, so it seems natural to expect defeated incumbents to be replaced by someone from another party. H1 then predicts a negative effect of budget delays on the seat share obtained by the party that held a majority before the election, while H2 predicts a negative impact on the seat share for the governor's party. Things are less clear under H3: This hypothesis

predicts that voters punish all incumbent legislators, but since it leaves no important role for party affiliation, it is not clear who should be expected to replace the defeated incumbents. If these incumbents are mainly replaced by candidates from their own party, budget delays should have little or no impact on the party composition of the state legislature. In contrast, if they are mostly replaced by someone from another party, we should expect the majority party in the legislature to lose seats after a budget delay, since the majority party (by definition) has more incumbent members to start with. Even if incumbents are punished equally hard irrespective of their party affiliation, the total effect of anti-incumbent sentiment on party seat shares will work to the majority party's disadvantage.

Testing the hypotheses: the need for individual-level data

The discussion above illustrates that one cannot hope to learn much about the electoral consequences for individual state legislators by studying outcomes at the party level only, since almost any observation at this level is consistent with at least two mutually exclusive hypotheses about individual-level effects. For example, suppose we observe a negative correlation between budget delays and the seat share obtained by the majority party in subsequent elections: This would be consistent with H1, but also with H3 under the additional assumption that voters punish incumbents by replacing them with someone from the opposite party. Similarly, observing a zero correlation between budget delays and subsequent electoral outcomes at the party level would be consistent with any of the hypotheses presented above if voters replace badly performing incumbents with other candidates from the same party, but it would also be consistent with the hypothesis that delays have no electoral consequences for *any* individual incumbents. These examples illustrate the point already made above: To properly test and distinguish the three hypotheses, we need data on electoral outcomes at the level of the individual legislator.

Further, while a party-level analysis cannot inform us about individual-level effects, the reverse is not true: The fact that we are able to separate incumbent defeats to same-party challengers from defeats to challengers means that we can estimate the impact of budget gridlock on the probability of both of these outcomes. We can then use these estimates to back out an estimated impact on the party composition of the state legislature. However, we also supplement our main analysis for individual state legislators with a party-level analysis. This analysis provides a more direct estimate of the aggregate effect of budget gridlock, thus serving as a convenient robustness check of the results from our main analysis.

3. Data and empirical specification

The objective of our empirical analysis is to examine the impact of a state's history of late budgets since the previous election on the subsequent electoral performance of individual incumbents in the lower house of the state legislature. The unit of analysis is an individual incumbent legislator in a given state in a given year. The data source for electoral outcomes is the ICPSR data set on state legislative returns (Klarner et al. 2013). This data set contains information on more than 300,000 candidates who ran for state legislative office since 1967. For each lower-house election between 1989 and 2007, we identify all major-party incumbent legislators in the data and determine whether they ran for reelection, and if so, whether they succeeded. For incumbents who were not reelected, including those who did not run again, we also extract information about the party affiliation of their successor. As explained above, this is crucial for making inference about party-level consequences. However, it also forces us to limit our attention to electoral races in single-member districts, since there is no way of meaningfully identifying a unique successor in a multi-member district. Consequently, we exclude all incumbents running in multi-member districts from our main analysis, as well as those for which we lack information on party affiliation.

Finally, we use the ICPSR data to determine whether the incumbent was ineligible for reelection because of a binding term limit, and whether the incumbent ran for the state senate instead of the lower house. Information about term limits in state legislatures is from the NCSL. We limit our sample to incumbents that were in fact eligible for reelection, i.e. those that were neither term limited nor running for the state senate.

Our measure of budgetary gridlock is based on data for late state budgets. The definition of a delay consists of two things: 1) the criteria for the budget process to be considered completed, and 2) the definition of the appropriate deadline by which this completion is supposed to be achieved. In US state governments the legislature and the governor often face different deadlines. Since we focus on legislatures, we define legislative budget gridlock as a situation in which the budget receives final legislative approval after the state legislature's deadline for passing the budget. Final legislative approval is achieved when the budget is passed in its final form by both chambers of the legislature. The deadline for achieving this varies from state to state: In some states, it coincides with the end of the fiscal year, while other states have earlier deadlines for the state legislature to pass the budget. For example, many state legislatures are required by constitution or statute to end their regular session by a certain date, and such requirements effectively constitute a deadline for all legislative activity, including passage of the budget.

The data for the legislatures' budget enactment dates were collected by Andersen et al. (2012, 2014) from state legislatures' websites, archived newspaper articles and a survey sent to state budget officers. The survey asked state budget officers both to confirm the data collected from other sources and to provide information on budget passage dates not found through such sources; We refer to Andersen et al. (2012, 2014) for further details. Table SI.1 in the online Supplementary Information (SI) provides an overview of the original sources of information on budget adoption dates, as well as the number of occurrences of budget gridlock in each state.

For the years 1988-2007, we observe 190 cases where the budget received final legislative passage after the legislature's state-specific deadline. This amounts to 26 percent of the budgets that we have data on. We focus our analysis on the 33 states that experienced at least one budget delay. These states held a combined total of 283 lower-house elections in the years 1989-2007. We exclude 41 of these from the analysis due to missing data on budget passage dates or election outcomes, or because all incumbents were elected in multi-member districts; for New Jersey and North Dakota, all elections held in this period fall under the latter category, so we exclude these states entirely. This leaves us with an analysis sample of 24,187 incumbent-year observations, distributed across 31 states and 242 lower-house elections in the years 1989 to 2007.

Empirical specification

For each incumbent-year observation, we distinguish between three possible electoral outcomes: (1) Reelection, (2) being replaced by a candidate from the same party, and (3) being replaced by a candidate from another party.⁵ To estimate the probability of each of these outcomes we use a multinomial logit model of the following form:

$$\Pr(\text{outcome}_{jst} = h \mid \mathbf{x}_{jst}) = \frac{e^{\mathbf{x}_{jst}\boldsymbol{\beta}^h}}{\sum_{k=1}^3 e^{\mathbf{x}_{jst}\boldsymbol{\beta}^k}}, \quad h = 1,2,3$$

where j denotes the identity of an individual incumbent legislator, while s and t denote state and year, respectively. \mathbf{x}_{jst} is a vector of explanatory variables, while $\boldsymbol{\beta}_h$ is a vector of parameters specific to outcome h . Since outcome probabilities must sum to one, not all of these parameters

⁵ Outcome (3) includes cases in which the incumbent is replaced by a non-major-party candidate. Cases where an eligible incumbent does not run for reelection (unless running for the state senate) are treated in the same way as incumbent defeats and coded as either (2) or (3), depending on the identity of the successor. Our reason for taking this approach is that an incumbent's decision on whether or not to run may be highly strategic (Jacobson 1989). In the robustness section, we document that our main results are insensitive to limiting the sample to races in which the incumbent did in fact run for reelection.

can vary freely. Hence, we treat outcome 1 (reelection) as the base category, setting $\beta_1 = \mathbf{0}$. The vector of explanatory variables contains individual-specific characteristics as well as state-level variables common to all incumbents running for reelection in state s in year t . The key explanatory variable measuring budget gridlock is the number of budget delays – as defined above – since the previous election in state s , normalized by the total number of budgets enacted in this time period. The normalization ensures comparability between states with two-year vs. four-year electoral cycles, and between states with annual vs. biennial budget enactment.⁶

Motivated by the theoretical analysis in section 2, we allow for heterogeneous effects of the budget gridlock variable by interacting it with dummies indicating i) whether the incumbent belongs to the party that held a majority in the lower house of the state legislature before the election (from now on simply referred to as “the majority party”), and ii) whether the incumbent belongs to the same party as the governor.⁷ The data for partisan control of the different branches of state governments is from Klarner (2013). We analyze the two interactions in separate models. A specification allowing for a full three-way interaction is presented in the online SI.

The control variables include two measures of the incumbent’s previous electoral record: First, we include the vote share that (s)he obtained in the previous election. Second, we include the number of electoral races the incumbent has previously participated in, and its square, to proxy for campaigning experience. Both variables are constructed from the ICPSR data set.

The control variables also capture various other dimensions of the political setting facing the incumbent: Previous studies have found state election outcomes to be affected by concurrent

⁶ For example, when analyzing the outcome for a legislator elected two years earlier in a state with annual budgeting, we use the number of delays in the two most recent years divided by two.

⁷ The hypotheses derived in section 2 abstract from the bicameral structure of all U.S. state legislatures except Nebraska’s. Since our focus is on lower-house electoral outcomes, it is natural to define the majority party as the party that controls the lower chamber. However, it may be that the partisan balance in the state senate also matters for how voters react to budget gridlock. We return to this issue in the robustness section.

elections for higher office (Chubb 1988; Lowry et al. 1998). To address this, we include the major-party vote share obtained by the gubernatorial candidate that belongs to the same party as the incumbent j in same-year gubernatorial races, as well as a dummy for gubernatorial election year. The data source is the Statistical Abstract of the United States (various years). Similarly, we include the state-specific vote share obtained by the majority party's presidential candidate in same-year presidential elections, taken from uselectionatlas.org. Voters in state elections may also be affected by events on the national scene in non-election years. To proxy for national shifts in party support, we construct a job approval rating index for the president at the time of the state election. The index measures the percentage of respondents in nation-wide Gallup polls who approve of the president's performance, minus the percentage that disapproves.⁸ We interact this variable with a dummy variable that takes the value 1 if incumbent j belongs to the same party as the president in year t . This dummy variable is also included directly with no interactions.

A third group of control variables captures the effects of fluctuations in the state economy that, in particular in times of crisis, can make it harder to reach a budget agreement (Andersen et al. 2012). These include the change in the state unemployment rate and the growth in real house prices since the previous year, the change in the ratio of state government expenditures to GDP since the previous election, the state government budget surplus in the year of the election, and tax increases enacted since the previous election, measured in percent of total general fund revenue. To allow for the possibility that these economic variables have different electoral consequences for different legislators, we include interactions between them and the dummy variables indicating membership of the majority party or of the governor's party.⁹

⁸ Polling data were downloaded from the web site of the Roper Center at the University of Connecticut, <http://ropercenter.cornell.edu/>.

⁹ The data sources for these variables are: Bureau of Labor Statistics (state unemployment rate), Federal Housing Agency (house price growth), US Census Bureau (state gov. expenditure and

Finally, state- and time fixed effects in incumbency advantage are allowed for by including full sets of state- and time dummies.¹⁰ The inclusion of state fixed effects is crucial, since it effectively controls for observed and unobserved time-invariant state characteristics that may correlate with both incumbent reelection rates and the frequency of budget delays. However, such characteristics could also have a differential impact on incumbents depending on their party affiliation. For example, one could imagine that majority party incumbents do systematically worse compared to minority party incumbents in states with procedural rules that grant minority party members significant influence on legislative outcomes, such as supermajority requirement rules. If budget delays are also more common in such states, we might falsely interpret the weak performance of majority party incumbents relative to minority party incumbents in these states as a causal effect of late budgets. To account for this, we interact the state fixed effects with the dummy variables indicating the incumbent's partisan affiliation in some specifications. Identification then comes from within-state comparisons between incumbents with the same partisan status (e.g. all majority party members or all members of the same party as the governor), but running in different years with a different record of legislative budget delays.

4. Main results

We first present simple descriptive statistics, and then results from regression analysis. Table I shows descriptive statistics for the 242 elections in our sample, splitting them by whether at least one budget was delayed since the previous lower-house election in the state. This splits the

budget surplus), Bureau of Economic Analysis (state GDP), and the National Association of State Budget Officers: *The Fiscal Survey of States* (tax increases).

¹⁰ Three states (Louisiana, Mississippi, Virginia) hold elections in odd years rather than even years. With state- and year fixed effects included, this creates a problem of perfect collinearity; to solve this, we assume that there were no differences in nationwide anti-incumbency sentiment between 1989 and 1990, allowing us to place the 1989 dummy in the reference category along with 1990.

sample in half: In 121 cases, representing 12,153 of the 24,187 observations in our sample, there was at least one instance of budget gridlock. Conditional on at least one delay, the average share of delayed budgets since the previous election is 82 percent. This shows that budget gridlock is typically a recurring phenomenon, rather than an isolated, one-time event.

Table 1 shows that reelection rates were on average lower when there had been a budget delay (80 percent) than when all budgets were adopted on time (83 percent). Conversely, the share of incumbents that were replaced by someone from their own party is slightly higher in the former case (12 percent vs. 11 percent), and so is the share of incumbents replaced by someone from the opposite party (7 percent vs. 6 percent).

Budget gridlock correlates strongly with divided government: 62 percent of the elections characterized by gridlock took place at a time when the governor and the majority in the lower house of the legislature belonged to different parties, against only 45 percent of the delay-free elections. Things are less clear for the economic variables: The average one-year change in the state unemployment rate is a drop of .19 percentage points for elections following budget gridlock, compared to .10 percentage points for delay-free elections. Similarly, the election years following gridlocked cycles are characterized by stronger house price increases than election years following cycles with timely budget adoption. In contrast, the average state government budget balance is weaker when there has been at least one delay, and taxes are raised more in such years, suggesting that gridlock occurs more frequently when the state government is under fiscal pressure.¹¹

¹¹ These observations are consistent with findings in the existing literature that the probability of budgetary gridlock is higher under divided government (Klarner et al. 2012, Andersen et al. 2012) and in times of changing fiscal circumstances, especially fiscal downturns (Andersen et al. 2012).

Table 1: Descriptive statistics

	No budget gridlock since previous election	Budget gridlock in at least one year since previous election	All elections
	----- means -----		
Share of delayed budgets since previous election	0.00	0.82	0.41
Share of incumbents re-elected	0.83	0.80	0.82
Share of incumbents replaced by candidate from same party	0.11	0.12	0.12
Share of incumbents replaced by candidate from other party	0.06	0.07	0.06
Share of incumbents belonging to the majority party in the lower house	0.60	0.61	0.60
Incumbent governor and majority in the lower house belong to different parties	0.45	0.62	0.53
Democratic majority in lower house	0.65	0.72	0.69
One-year change in state unemployment rate (ppts)	-0.10	-0.19	-0.15
One-year growth rate in state house prices (percent)	0.97	1.31	1.14
Change in ratio of state gov. expenditures to GDP since previous election (ppts)	0.26	0.24	0.25
Tax increases enacted since previous election (percent of total general fund)	1.21	1.95	1.58
State government budget surplus in year of election (percent of state GDP)	0.20	0.10	0.15
Number of elections	121	121	242
Number of incumbent-year obs.	12,034	12,153	24,187

Notes: Variable definitions and data sources are described in the text.

Majority party incumbents vs. minority party incumbents

We now turn to the results for the multinomial logit model described above. We begin with a version of the model in which the budget gridlock variable is interacted with a dummy variable indicating whether the incumbent legislator is a member of the majority party in the lower house of the legislature. Table 2 presents estimation results for this model. Since parameter estimates do not have straightforward interpretations in this type of model, we report the average partial effects of budget delays on the predicted probabilities of each of the three electoral outcomes. More precisely, the table reports estimates of the average changes in probabilities when the budget gridlock variable changes from zero to one and all other explanatory variables are evaluated at their actual values. The top part reports average partial effects on the probability of reelection, while the middle and lower parts report the effects on the probabilities of being replaced by a candidate from the same party or another party, respectively. Column 1 reports estimates from a parsimonious version of the model in which only time dummies and the dummy variables indicating majority party membership and membership of the governor's party are included as controls. Columns 2 and 3 sequentially add political and economic controls, while state fixed effects are added in column 4. Finally, column 5 adds state fixed effects interacted with the dummy variable for majority party membership.

Beginning in the top part of the table, the first row reports the average partial effect on the probability of reelection for all incumbents, regardless of their partisan affiliation. The estimated effect is negative across all columns, ranging from -1.2 to -2.0 percentage points, and is statistically significant once state fixed effects are included. However, the next two rows show that these estimates conceal an important difference between majority party members and minority party members: For majority party incumbents, the estimated effect of budget gridlock on the probability of reelection ranges from -2.5 to -4.5 percentage points and it is significant at the

Table 2: Majority vs. minority party members

	(1)	(2)	(3)	(4)	(5)
Outcome 1: Re-election					
All incumbents	-0.016 ↑ (0.010)	-0.013 ↑ (0.011)	-0.012 ↑ (0.011)	-0.020 ↑ (0.008)	-0.020 ↑ (0.008)
Majority party members	-0.025 ↑ (0.013)	-0.027 ↑ (0.014)	-0.027 ↑ (0.014)	-0.038 ↑ (0.011)	-0.045 ↑ (0.011)
Minority party members	-0.001 ↑ (0.014)	0.005 ↑ (0.013)	0.007 ↑ (0.013)	0.006 ↑ (0.012)	0.016 ↑ (0.012)
Difference	-0.024 (0.018)	-0.032 (0.016)	-0.034 (0.016)	-0.044 (0.016)	-0.061 (0.016)
Outcome 2: Replaced by candidate from same party					
All incumbents	0.005 ↑ (0.006)	0.008 ↑ (0.007)	0.007 ↑ (0.007)	0.005 ↑ (0.006)	0.004 ↑ (0.006)
Majority party members	0.004 ↑ (0.007)	0.005 ↑ (0.008)	0.004 ↑ (0.008)	0.004 ↑ (0.007)	0.006 ↑ (0.007)
Minority party members	0.006 ↑ (0.010)	0.012 ↑ (0.011)	0.010 ↑ (0.011)	0.007 ↑ (0.010)	0.002 ↑ (0.010)
Difference	-0.003 (0.011)	-0.007 (0.012)	-0.007 (0.012)	-0.003 (0.011)	0.004 (0.011)
Outcome 3: Replaced by candidate from other party					
All incumbents	0.011 ↑ (0.007)	0.006 ↑ (0.007)	0.006 ↑ (0.007)	0.015 ↑ (0.006)	0.016 ↑ (0.006)
Majority party members	0.021 ↑ (0.010)	0.022 ↑ (0.009)	0.023 ↑ (0.010)	0.035 ↑ (0.008)	0.039 ↑ (0.008)
Minority party members	-0.005 ↑ (0.010)	-0.017 ↑ (0.007)	-0.018 ↑ (0.007)	-0.012 ↑ (0.007)	-0.018 ↑ (0.007)
Difference	0.027 (0.014)	0.039 (0.011)	0.041 (0.010)	0.047 (0.010)	0.057 (0.010)
Party affiliation dummies	Yes	Yes	Yes	Yes	Yes
Electoral record controls	No	Yes	Yes	Yes	Yes
Political controls	No	Yes	Yes	Yes	Yes
Economic controls	No	No	Yes	Yes	Yes
Economic controls x majority party	No	No	Yes	Yes	Yes
State fixed effects	No	No	No	Yes	Yes
State fixed effects x majority party	No	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
No. of states	31	31	31	31	31
No. of elections	242	242	242	242	242
Observations	24,187	24,179	24,179	24,179	24,179

Notes: The table reports average marginal effects of changing the budget gridlock variable from zero to one on the probability of each election outcome for incumbent legislators. The definitions of the three possible outcomes are described in the text. Each row reports the estimated effect on the probability of a particular outcome for a particular type of incumbent. The rows labelled "Difference" report the differences in effects between the two rows immediately above. Standard errors (reported in parentheses) are estimated allowing for clustering at the state-year level.

1 percent level when state fixed effects are included. For minority party members, we find a positive point estimate for the effect on the probability of reelection in four out of five columns, but it is small and never statistically significant. The difference between the two groups is statistically significant at the 5 percent level or less once economic controls are included.

Turning to the bottom part of the table, we see that for majority party incumbents the estimated effects on the probability of reelection are almost exactly mirrored by opposite-signed effects on the probability of being replaced by a candidate from the other party, which increases by about 2-4 percentage points. The results for minority party incumbents are again quite different: If anything, the results suggest that the probability of being replaced by someone from another party decreases for this group. At 1-2 percentage points, the estimated effect is moderate, but it is numerically larger than the opposite-signed effect on the probability of reelection and also more precisely estimated (p-value below .05 in three out of five columns). The difference between majority party incumbents and minority party incumbents is again sizeable and strongly significant.

In contrast, we do not find any significant effect of budget gridlock on the probability of being replaced by a same-party candidate for either group. For minority party incumbents, this leaves us with some ambiguity about the consequences of gridlock: As mentioned above, we find a (mostly) significant negative impact on the probability that these incumbents are replaced by majority party challengers. By definition, this must result from either an increase in the probability of reelection or a decrease in the probability of being replaced by someone from the same party (or some combination of the two); however, our analysis does not produce sufficiently precise estimates of the magnitudes of these two effects to determine which is more relevant.

Members of governor's party vs. non-members

Hypothesis H2 predicts that rather than majority party membership, what matters for the effect of budget gridlock on an incumbent's reelection prospects is whether (s)he belongs to the governor's party. To test this, we estimate a version of the multinomial logit model in which an interaction term between the gridlock variable and membership of the governor's party is included. The results are shown in Table 3. The table parallels Table 2, except that the central distinction is now between members of the governor's party vs. non-members, rather than majority vs. minority party members.

As in Table 2, we find a negative effect of budget gridlock on the probability of reelection in the order of 1-2 percentage points when we average across all incumbents. However, the results in Table 3 do not reveal any significant difference between incumbents with different party affiliation: The estimated effect of budget gridlock is numerically somewhat larger for members of the governor's party than for non-members, but it is statistically significant in only one out of five columns, and the difference between the two groups is never significant. Similarly, we find no solid evidence of a differential impact of budget gridlock between these two groups of incumbents in the lower parts of the table, which show the estimated effects on the probabilities of being replaced by another candidate from the same party or the opposite party.

To complete the analysis presented here, we have also estimated a version of the model that includes a full three-way interaction between budget gridlock, majority party membership, and membership of the governor's party. The results are in the online SI. In short, allowing for a full three-way interaction does not change the main conclusions: the difference between majority and minority party incumbents is sizeable and statistically significant at the five percent level, regardless of which party controls the executive branch of the state government.

Table 3: Members of governor's party vs non-members

	(1)	(2)	(3)	(4)	(5)
Outcome 1: Re-election					
All incumbents	-0.016 [0.010]	-0.013 [0.011]	-0.012 [0.011]	-0.017 [0.008]	-0.016 [0.008]
Members of governor's party	-0.020 [0.015]	-0.023 [0.015]	-0.022 [0.014]	-0.028 [0.013]	-0.025 [0.014]
Non-members of governor's party	-0.012 [0.013]	-0.004 [0.014]	-0.003 [0.014]	-0.009 [0.011]	-0.010 [0.011]
Difference	-0.009 [0.019]	-0.018 [0.019]	-0.019 [0.018]	-0.020 [0.017]	-0.016 [0.018]
Outcome 2: Replaced by candidate from same party					
All incumbents	0.005 [0.006]	0.008 [0.007]	0.006 [0.007]	0.005 [0.006]	0.004 [0.006]
Members of governor's party	0.011 [0.009]	0.015 [0.010]	0.015 [0.010]	0.014 [0.009]	0.007 [0.009]
Non-members of governor's party	0.000 [0.008]	0.002 [0.009]	-0.001 [0.009]	-0.002 [0.008]	0.002 [0.008]
Difference	0.011 [0.011]	0.014 [0.012]	0.016 [0.012]	0.016 [0.012]	0.005 [0.012]
Outcome 3: Replaced by candidate from other party					
All incumbents	0.011 [0.007]	0.005 [0.007]	0.006 [0.007]	0.012 [0.006]	0.012 [0.006]
Members of governor's party	0.0094 [0.011]	0.0076 [0.009]	0.0071 [0.009]	0.0144 [0.009]	0.0179 [0.011]
Non-members of governor's party	0.0119 [0.010]	0.0027 [0.010]	0.0044 [0.010]	0.0108 [0.007]	0.0077 [0.007]
Difference	-0.002 [0.015]	0.0049 [0.013]	0.0027 [0.013]	0.0037 [0.012]	0.0102 [0.013]
Party affiliation dummies	Yes	Yes	Yes	Yes	Yes
Electoral record controls	No	Yes	Yes	Yes	Yes
Political controls	No	Yes	Yes	Yes	Yes
Economic controls	No	No	Yes	Yes	Yes
Economic controls x governor's party	No	No	Yes	Yes	Yes
State fixed effects	No	No	No	Yes	Yes
State fixed effects x governor's party	No	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
No. of states	31	31	31	31	31
No. of elections	242	242	242	242	242
Observations	24,187	24,179	24,179	24,179	24,179

Notes: The table reports average marginal effects of changing the budget gridlock variable from zero to one on the probability of each election outcome for incumbent legislators. The definitions of the three possible outcomes are described in the text. Each row reports the estimated effect on the probability of a particular outcome for a particular type of incumbent. The rows labelled "Difference" report the differences in effects between the two rows immediately above. Standard errors (reported in parentheses) are estimated allowing for clustering at the state-year level.

Perspectives on voters' views on parties and individual legislators

The evidence presented here supports the view that voters assign responsibility for budget gridlock along party lines, as predicted by H1: Incumbents from the majority party in the lower house of the state legislature face significantly lower reelection probabilities, and voters punish them by replacing them with someone from the opposite party. Minority party incumbents, on the other hand, escape unharmed. Contradicting the predictions of H3, this suggests that incumbency in itself is not sufficient to attract voters' blame for budget gridlock. Moreover, we find no solid evidence that the governor's party affiliation matters for the electoral fallout of budget delays for state legislators, as predicted by H2.

The result that budget gridlock lowers the probability that minority party incumbents lose their seats to candidates from other parties also suggests that the adverse effects for majority party candidates are not confined to incumbent legislators: Even non-incumbent challengers from this party perform worse when state budgets are delayed. This is consistent with findings showing that voters' perception of a party's brand affects the electoral prospects of any candidate running under that party label (e.g., Rahn 1993, Woon and Pope 2008, Butler and Powell 2014). Table 4 provides further evidence in support of this idea: Here we report estimates for the subsample of open-seat races, i.e. elections in which the incumbent did not run for reelection. These races are interesting because they involve only non-incumbent candidates, so any systematic effect of budget gridlock suggests that voters' attitudes towards the party brand are affected. Since reelection is by definition ruled out in open-seat races, we are left with only two possible outcomes. We therefore use a binary variable equal to 1 for outcome 3 (succeeded by someone from another party) and zero otherwise as the dependent variable and estimate the model with logistic regression. If budget gridlock hurts non-incumbent majority party candidates, we should expect a positive sign on the delay variable in races where the outgoing incumbent belongs to the majority

party, and a negative sign when the incumbent belongs to the minority party. As shown in Table 4, this is indeed what we find: In districts with a majority party incumbent not running for reelection, a unit increase in the budget gridlock variable is associated with a 9 percentage points increase in the probability of electing someone from the opposite party. In open-seat races with minority party incumbents the estimated effect is negative, as expected, but numerically much smaller and statistically insignificant. In sum, these results suggest that majority party candidates are hurt by budget gridlock also in open seat races, but only significantly so when the outgoing incumbent is also a member of the majority party. This is consistent with the idea of party labels or brands being important for voter decision-making.

Table 4: Open-seat races

		<u>Outcome 3 : Replaced by candidate from other party</u>
Open-seat races where incumbent belongs to majority party	■	0.091 (0.025)
Open-seat races where incumbent belongs to minority party	■	-0.022 (0.025)
No. of states		31
No. of elections		240
Observations		3,460

Notes: The table reports average marginal effects of changing the budget gridlock variable from zero to one on the probability that the incumbent is replaced by someone from another party. Only lower-house elections in which the incumbent legislator does not run are included. The control variables are the same as in column 4 of Table 2. Standard errors (reported in parentheses) are estimated allowing for clustering at the state-year level.

Patty (2016) proposes a formal model to explain the occurrence of legislative gridlock in which politicians may reap electoral benefits from obstructing the legislative process. Our results show that for majority party incumbents, who have the collective power to obstruct the budget process, legislative obstruction has *adverse* consequences on election day. However, these results reflect the average effect among majority party incumbents, and we cannot rule out that some individual

majority party legislators may actually increase their reelection chances by actively gridlocking the state budget. Nor can we rule out that some among the minority party incumbents may have the capacity to obstruct negotiations, due to for example supermajority requirements or voting across party lines, and that doing so may increase their reelection prospects. Exploring these issues would require accurate and quantifiable information on each legislator's personal role in the budget adoption process and the actions that he or she took to either stall or move it forward. To the best of our knowledge, no such detailed information is currently available.

5. Robustness

The online SI reports results from a series of robustness checks of our analysis. This section summarizes the main points: First, our main result – the difference in electoral consequences of budget gridlock between majority vs. minority party incumbents – is robust to alternative definitions of the budget gridlock variable, including whether the state budget was delayed in *any* year since the previous election, or in the *same* year as the current election. We also show that our main result is unchanged if we use a simple binary logit model for reelection vs. not reelection. Using this simpler model allows us to include observations from multi-member districts in the analysis, since we no longer need to identify a unique successor in the case of incumbent defeats.

The main analysis includes open-seat races in the estimation sample, treating cases in which the incumbent does not run for reelection as incumbent defeats. As explained in the discussion above, the negative effect of budget gridlock on majority party candidates' electoral prospects is strongly present in these races. At the same time, the robustness tests show that our results also hold if we instead exclude open-seat races and focus exclusively on races where the incumbent did in fact run for reelection. This means that our results are not driven by incumbents choosing not to run for reelection, contrasting with Binder's (2003) observation for the US Congress that there is no

effect of (her measure of) gridlock on the probability of reelection conditional on seeking such reelection; she, instead, identifies a small negative effect of gridlock on the probability of seeking reelection, and concludes that gridlock makes politicians leave politics.

Throughout the paper, we have equated partisan control over the state legislature with control over the lower house, ignoring the party composition in state senates. One can argue that split party control over the two chambers of the legislature blurs who is responsible for legislative outcomes, so it is possible that voters respond differently in lower house elections depending on which party controls the state senate. To check whether our results are affected by any such heterogeneity in voter responses, we have estimated our model on the subsample of observations stemming from elections in which the same party controlled both chambers. The results are virtually identical to those in the baseline analysis. If we instead limit the sample to elections following split-legislature cycles, standard errors increase somewhat due to a much smaller sample size, but the point estimates of the marginal effects are very similar to the baseline estimates.¹²

Another potential concern is that our budget gridlock measure captures the state government being under fiscal pressure, despite our efforts to control for state economic and fiscal indicators known to correlate with late budgets (Andersen et al. 2012). To explore this, we have estimated our baseline model on the subsample of elections for which the state unemployment rate fell over the 12 months leading up to the election, indicating a strong state economy. Here, too, we find a strong negative impact of budget gridlock on the reelection prospects for majority party incumbents, whereas there is no significant effect for minority party incumbents. This supports the interpretation that gridlock in itself causes voters to punish majority party incumbents.

¹² In supplementary analysis, we examine the effect of differences across states in what happens if a budget is not passed on time. The major divide is whether governments shut down operations or whether there are institutions in place to prevent this from happening. We find that majority party incumbents suffer electoral consequences in both cases, but the voter response is stronger in states that experience state government shutting down; see Table SI.5.

Finally, the data on budget enactment dates used to construct the gridlock variable is has missing observations in some states, especially in the early years of our data set. As a final robustness check, we have therefore estimated our model on the 20 states in our sample for which we have information about budget enactment dates in all years in the analysis period. The results are again very similar to those in the baseline estimation.

6. Party composition in state legislatures

To estimate the effect of budget gridlock on the aggregate electoral performance of political parties, we can back out the implied effect on the average seat share won by the majority party in lower-house elections. For example, using the point estimates from the specification in column (4) of Table 2, we find that the probability of losing the seat to a candidate from another party increases by an average of 3.5 percentage points for majority party incumbents and drops by 1.2 percentage points for minority party incumbents. For the electoral cycles included in our analysis sample, the average seat share composition is a 60/40 percent split between the majority party and the minority party. A back-of-the-envelope calculation then tells us that the average effect of changing the budget gridlock variable from zero to one on the majority party's seat share is $-.035 \times .60 - .012 \times .40 = -.026$, i.e. a drop of 2.6 percentage points (95% confidence interval goes from -3.6 to -1.6). If we instead use the estimates from column 1 of Table SI.2, we find a corresponding effect of -3.3 percentage points when the governor belongs to the same party as the majority party in the legislature, and -2.2 percentage points when (s)he belongs to a different party.¹³

To obtain direct estimates of the effect of budget gridlock on party-level electoral outcomes, we have also run a series of regressions in which the seat share obtained by the majority party (defined, as usual, as the party that held a majority in the lower house of the state legislature

¹³ The 95% confidence intervals for these effects are [-.048 ; -.017] and [-.037 ; -.008], respectively.

before the election) is regressed on our budget gridlock measure and a set of control variables paralleling those included in the individual-level estimations. The unit of analysis is a lower-house election in a given state in a given year. In the interest of space, we confine the detailed description of these regressions to the SI and only summarize the key results: Using the same sample of 242 elections as in our main analysis, we find that changing the gridlock variable from zero to one reduces the vote share obtained by the majority party by 2.9 percentage points (significant at the 1 percent level). This is very close to the 2.6 percentage points estimate calculated above. Extending the sample to include elections in which incumbents run in multi-member districts does not change the results in any significant way. When introducing an interaction term between the budget gridlock variable and a dummy variable indicating whether the governor belongs to the same party as the majority in the lower house of the state legislature, we find a budget gridlock effect on the majority party's seat share of -3.4 percentage points when the governor also belongs to this party, versus -2.1 percentage points in the opposite case. Again, these point estimates are virtually identical to the ones from our main analysis of electoral outcomes for individual incumbents. In sum, these direct estimates of the party-level effects of budget gridlock are fully consistent with the results from our micro-level analysis of state legislators' electoral fortunes.

Should these reported effects on the majority party's seat share be considered small or large? In the order of 2-3 percentage points, the estimated effects of budget gridlock are not huge, but large enough to be relevant in a fairly large share of lower-house elections: Of the 262 elections included in our analysis, 55 (or 21%) resulted in a seat share within 3 percentage points from the 50% threshold for the party that had a majority before the election. Combined with our estimates of the electoral effects of budget gridlock, this suggests that about one fifth of state legislative elections are sufficiently close that whether the state budget is passed on time or not could be decisive for which party will gain control over the state legislature. Another way to assess the

magnitude of the gridlock effect is to compare it to the estimated effects for the control variables. In general, however, we find limited impact from state economic and fiscal performance on lower-house state legislative elections, consistent with the findings of Chubb (1988) and Lowry et al. (1998). For example, the party-level analysis mentioned above suggests that a one percentage point increase in the one-year change in the state unemployment rate reduces the seat share obtained by the majority party by only 0.3 percentage points on average, and by 1.2 percentage point under unified government, and while both are imprecisely estimated, our estimates of the electoral impact of budget gridlock appear quite sizeable in comparison.¹⁴

7. Concluding remarks

Legislative gridlock is a matter of key concern in democratic politics, since it has the potential to hinder political action on important issues in a timely fashion. In this article, we study voter reactions to a particular set of gridlock instances, budget gridlock in US states, with the aim of understanding whether and how voters contribute to mitigating the problem of gridlock by holding elected lawmakers collectively accountable for legislative impasses.

We find strong evidence showing that voters in state legislative elections do react to occurrences of budget gridlock: On average, reelection rates for incumbent legislators drops by 1-2 percentage points. The effect is entirely driven by lower reelection rates for members of the majority party in the legislature. We find that these incumbents are always punished by voters for budget gridlock, no matter which party controls the other branches of the state government. Non-incumbent members of the majority party also suffer electorally and are less successful in defeating minority party incumbents when the state budget has been delayed. Although the effect

¹⁴ To save space, we generally do not report coefficient estimates for control variables but they are available from the authors upon request.

is smaller than for incumbent majority party members, this is consistent with a key assertion in influential theories of party organization, namely that a poor legislative performance hurts the brand of the majority party in the legislature, to the detriment of *all* candidates running under that brand. For the majority party as a whole, we find that budget gridlock reduces the seat share obtained in the subsequent election by about 2-3 percentage points on average. For one fifth of the state legislative elections in our analysis sample, a change in the party composition of legislative seats of this magnitude would be enough to tip the partisan balance of power in the lower house of the state legislature from one party to the other. Our results therefore indicate that voters hold members of the majority party collectively accountable for budget gridlock to an extent that leaders of the party often cannot afford to ignore. In contrast, we find no evidence for the hypothesis that voters base their judgement of who is responsible for budget gridlock on the party affiliation of the governor and act accordingly when casting their ballot.

The focus in this paper is *collective* accountability for budget gridlock: We explore whether different groups of incumbents – as defined by their party affiliation – experience different electoral consequences when they collectively fail to pass a state budget on time. Future research should consider two paths: First, does increasing political polarization increase collective electoral accountability for legislative gridlock? Jones (2010) argues that increasing political polarization has increased electoral accountability for majority party members in Congress, and increasing polarization in state legislatures could have similar implications for the electoral consequences of gridlock. Second, do voters hold incumbent legislators *individually* accountable for their roles in the budget adoption process? For example, one can imagine that legislators who have a direct responsibility for getting the budget passed – through membership of an appropriations committee or the party leadership, for example – are particularly harshly punished when the legislative process reaches a deadlock. On the other hand, one can also imagine, as argued by Patty (2016),

that some legislators can actually benefit personally from obstructing the legislative process, even while their fellow party members suffer as a group, if by doing so they can convince voters of their “toughness.” Exploring this issue empirically would require detailed and quantifiable data on each legislator’s formal responsibilities and actual behavior in the budget adoption process, as well on his or her personal conduct and actions during that process. We do not know of any existing data sources that fulfill these criteria, but we believe this to be an interesting path for future research.

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Supplementary Information (for online publication only)

Budget gridlock in each state: Occurrences and data sources

Table SI.1 shows the number of budget delays recorded in each state in the analysis period, the years for which information about budget completion dates are available, and the original sources of information for this data.

Interacting budget gridlock, majority party status, and membership of governor's party

The analysis in the main text presents results for regressions in which the budget gridlock variable is interacted with *either* a dummy for majority party membership *or* a dummy for membership of the governor's party. To complete the analysis, we have also estimated a version of the multinomial logit model that includes a full three-way interaction term between these three variables. Estimation results from this model are reported in Table SI.2. For brevity, we only report the average partial effects of budget gridlock on the probability of outcome 1, reelection.

Column (1) reports results from a specification paralleling column (4) in Table 2: All political and economic controls are included, but the economic controls are now, like the gridlock variable, included in a full three-way interaction with the dummies for majority party membership and membership of the governor's party. State fixed effects are included, but not interacted with either of the two-party affiliation dummies. Such two-way interactions are included in column (2), while column (3) includes a full three-way interaction between the state fixed effects and the party affiliation dummies.¹⁵

¹⁵ The simultaneous inclusion of interaction terms between state fixed effects and both party affiliation dummies requires estimation of a separate state fixed effect for each combination of majority party membership and membership of the governor's party. For this reason, we drop Delaware from the analysis. The reason is that the governor of Delaware belonged to the same party as the majority in the state legislature in all of the years in our analysis period, implying that majority party membership is inseparable from membership of the governor's party.

Table SI.1. Occurrences of budget gridlock and source of information, by state

	(1) Years with non-missing information about budget gridlock	(2) Number of budget delays
Alabama	1989, 1991-2007	5
Alaska	-	-
Arizona	<i>1988-90, 1991-2007</i>	0
Arkansas	1988-2007	0
California	1988-2007	20
Colorado	1988-89, 1991, 1993-2007	0
Connecticut	1988-2007	9
Delaware	1992-2007	4
Florida	1988-2007	9
Georgia	1988-92, 1993, 1994-2007	0
Hawaii	-	-
Idaho	<i>1988-97, 1998-2007</i>	0
Illinois	1988-2007	8
Indiana	1988-2007	4
Iowa	1992-2007	0
Kansas	1989-91, 1993, 1997-2007	0
Kentucky	1988-2007	3
Louisiana	1988-2007	1
Maine	1991-2007	3
Maryland	1991-2007	1
Massachusetts	1988-2007	10
Michigan	1988-98, 1991, 1995-2007	2
Minnesota	1988-2007	6
Mississippi	1997-2007	1
Missouri	1988-2007	2
Montana	1999-2007	1
Nebraska	1997-2007	0
Nevada	1991-2007	5
New Hampshire	1991-2007	2
New Jersey	1988-2007	5
New Mexico	<i>1988-94, 1995-2007</i>	5
New York	1988-2007	18
North Carolina	1988-2007	17
North Dakota	1988-2007	1
Ohio	1988-2007	2
Oklahoma	1993-2007	1
Oregon	1988-90, 1992-2007	7
Pennsylvania	1988-2007	9
Rhode Island	<i>1988-2000, 2001-2007</i>	2
South Carolina	1988-2007	0
South Dakota	1996-2007	0
Tennessee	1995-2007	2
Texas	1988-2007	2
Utah	1989-2007	0
Vermont	1988-2007	0
Virginia	1988-2007	6
Washington	1988-2007	10
West Virginia	<i>1988-92, 1993-2007</i>	0
Wisconsin	1988-2007	7
Wyoming	1998-2007	0

Notes: Information about timeliness in the budget adoption process is from Andersen et al. (2012, 2014). Column (1) indicates the years for which this information is available for the state in question and what the original source of information is: Normal font indicates that the information is based on data collected from online sources only. Italics indicate that the information is based exclusively on survey responses from state budget offices. Bold indicates that information is available from both sources. Column (2) indicates the number of occurrences of budget gridlock - as defined in section 3 - recorded in the years indicated in column (1).

Table SI.2: Three-way interaction btw. gridlock, majority party, and governor's party

	(1)	(2)	(3)
	Outcome 1: Re-election		
Majority party members, governor from same party	-0.051 (0.018)	-0.060 (0.019)	-0.069 (0.021)
Majority party members, governor from different party	-0.025 (0.014)	-0.039 (0.014)	-0.044 (0.015)
Difference	-0.025 (0.022)	-0.021 (0.023)	-0.025 (0.027)
Minority party members, governor from same party	-0.004 (0.017)	0.012 (0.018)	0.004 (0.019)
Minority party members, governor from different party	0.017 (0.016)	0.011 (0.016)	0.006 (0.018)
Difference	-0.021 (0.022)	0.001 (0.023)	-0.002 (0.027)
Electoral record and political controls	Yes	Yes	Yes
Economic controls	Yes	Yes	Yes
Economic controls x majority party	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
State fixed effects x majority party	No	Yes	Yes
State fixed effects x governor's party	No	Yes	Yes
State fixed effects x majority party x governor's party	No	No	Yes
Year fixed effects	Yes	Yes	Yes
No. of states	31	29	29
No. of elections	242	232	232
Observations	24,179	23,666	23,666

Notes: The table reports estimates of average marginal effects of changing the budget gridlock variable from zero to one on the probability of re-election for incumbent legislators. Each row reports the estimated average marginal effect for a particular type of incumbent. The rows labelled "Difference" report the differences in estimated marginal effects between the two rows immediately above. Standard errors (reported in parentheses) are estimated allowing for clustering at the state-year level.

The top part of the table focuses on the average partial effects of budget gridlock for majority party incumbents: We find a strong effect for this group when the governor belongs to the same party as the majority in the legislature. Ranging from -5 to -7 percentage points across the three columns, the estimates are quite stable and strongly statistically significant. Turning to the second row, the point estimates fall somewhere in the range between -2 and -4 percentage points when we condition on the governor belonging to a different party. While this is somewhat smaller than in the same-party case, the difference is in no case statistically significant.

The lower part of the table shows results for minority party members. Recall that the results in Table 2 showed no significant effect of budget gridlock on the probability of reelection for this group. The results in Table SI.2 show that this conclusion does not change when we also condition on membership or non-membership of the governor's party. Comparing with the results for majority party incumbents, the difference between the two groups (not reported directly in Table SI.2) is around 5-8 percentage points when the governor belongs to the same party and around 4 percentage points when the governor belongs to a different party, and it is always statistically significant at the 5 percent level.

Robustness checks of main analysis

Table SI.3 shows results from a series of robustness tests of our main analysis. We focus on our central result: The contrast in the electoral consequences of budget gridlock between majority vs. minority party incumbents. The baseline is the specification from column 4 of Table 2, which is reproduced in column 1 of Table SI.3 for convenience. We only report marginal effects for outcome 1, reelection.

Columns 2 and 3 report results from specifications in which we have replaced our preferred measure of budget gridlock with i) a dummy variable that equals one if the state budget was

delayed in any year between the previous and the current election for the state legislature (column 2), and ii) a dummy variable that equals one if the state budget was delayed in the same year as the current election (column 3).¹⁶ These alternative definitions of our key explanatory do not change the results much: The estimated marginal effects for majority party incumbents are slightly smaller than in the baseline specification, but they remain significantly negative and the difference compared to the estimated effects for minority party incumbents is still significant at the five percent level.

The baseline specification distinguishes between incumbent defeats to same-party challengers and defeats to challengers from other parties. As explained in section 3 of the main text, this means that we have to drop observations from multi-member districts where there is no unique successor to a defeated incumbent. In column 4 of Table SI.3 we report results from a model where these observations are retained in the sample. To do this, we pool all incumbent defeats in one base category and estimate a binomial logit model using a simple dummy for reelection as the dependent variable. The estimated marginal effects on the probability of reelection from this model are virtually identical to those from the baseline specification.

The analysis presented in the main text also includes electoral races in which the incumbent did not run for reelection, despite being eligible for it. As explained in section 3, these races are treated in the same way as elections in which the incumbent ran and lost. Column 5 shows that our conclusions are robust to altering this approach: This column reports estimates from a model that is identical to the baseline in column 1, but estimated on the subsample of electoral races in which the incumbent did in fact run for reelection. The results are again similar to the baseline.

¹⁶ In states with biennial budgets that are passed in non-election years, the dummy is equal to one if the budget was delayed in the year before the election.

We have also estimated our baseline model on the subsample of observations stemming from elections in which the same party controlled both chambers of the legislature, as well as on the subsample of observations for which the opposite was true. The results are reported in columns (6) and (7) of Table SI.3, respectively. Limiting the sample to elections following a cycle of unified party control of the state legislature does not change our results in any way, as the estimated marginal effects reported in column (6) are almost identical to those in the baseline analysis. When we limit the sample to elections following split-legislature cycles, standard errors increase somewhat due to the smaller sample size, but the point estimates of the marginal effects are again very similar to the baseline estimates.

Our data on budget timeliness suffers from a relatively large number of missing observations, especially in the early years of the analysis period (see table SI.1 for details). A potential concern is that the absence of information could be systematically related to whether the budget was in fact delayed or not, which could bias our results. To address this concern, we have estimated our model on the subsample of states for which we have non-missing information about budget timeliness in all years in the analysis period. The results of this exercise, shown in column (8), are very similar to the baseline results, suggesting that sample selection bias due to missing data on budget enactment dates is not an issue of concern.

Finally, we have estimated our model on the subsample of state elections in which the state unemployment rate was lower in the month of the election than it was 12 months prior to the election. We again find that budget gridlock lowers the reelection chances for majority party incumbents but has no significant effect for minority party incumbents. The difference between the two groups is less precisely estimated than in our baseline analysis (p -value of 0.075) but the point estimate is very similar in size.

Analyzing aggregate election outcomes at the party-level

This section presents results from an analysis of election outcomes at the party level. The unit of analysis is an election for the lower house of the state legislature in a given state in a given year. As in our main analysis, we concentrate on elections held in the years between 1989 and 2007 in the 33 states that experienced at least one budget delay between 1988 and 2007. There are 283 such elections in total, but missing data on budget passage dates (predominantly in the early years) brings the number of elections in our analysis sample down to 261.¹⁷

We estimate variants of the following linear model using ordinary least squares:

$$majshare_{it} = \beta_0 + \beta_1 gridlock_{it} + \beta_2 govsame_{it} + \beta_3 gridlock_{it} * govsame_{it} + \mathbf{x}_{it} \boldsymbol{\delta} + \eta_i + \gamma_t + \epsilon_{it}$$

The dependent variable is the seat share won in the lower-house election in state i in year t by the party that held a majority in the house before the election. Party seat shares are based on Carl Klarner's data set on partisan balances in state legislatures (Klarner 2013). The key explanatory variable is $gridlock_{it}$, the normalized number of legislative budget delays since the previous lower-house election described in section 3 of the main text. We include this variable directly, as well as interacted with the dummy variable $govsame_{it}$, which takes the value one if the governor belongs to the same party as the majority in the lower house of the legislature in state i in year t . We also estimate a variant of the model in which $govsame$ is interacted not only with the $gridlock$ variable, but also with a number of economic control variables, as well as with the state fixed effects.

The control variables in \mathbf{x}_{it} parallel those in the analysis of outcomes for individual incumbents: First, we include the seat share won by the current majority party in the previous election. Second, we control for gubernatorial and presidential coattails by including the major

¹⁷ In contrast to the individual-level analysis, we do have data on state-level election outcomes for all elections held in New Jersey and North Dakota in the analysis period. This adds 19 elections to the analysis sample.

party vote share obtained by the candidate belonging to the same party as the majority in the state legislature in same-year gubernatorial/presidential elections, along with a dummy variable for gubernatorial election year. Also included is the presidential approval rating index described in section 3, now interacted with a dummy variable indicating whether the president belongs to the same party as the majority in the state legislature (this dummy is also included with no interaction). Third, we include the same economic controls as in the micro-level analysis: The one-year changes in the state unemployment rate and in real house prices, the change in the ratio of state government expenditures to GDP since the previous election, tax increases enacted since the previous election (in percent of general fund revenue), and the state government budget surplus (percent of state GDP) in the year of the election. State- and year fixed effects are included. Standard errors are computed allowing for clustering at the state level.

Table SI.4 presents regression estimates for the coefficients of main interest, i.e. β_1 - β_3 . Column (1) presents results for the same sample of elections as used in our micro-level analyses of election outcomes for individual incumbents. The coefficient on the budget gridlock variable is -.029 and significant at the 1 percent level. Column (2) extends the sample to include all 261 elections for which we have data, which does not change the results in any important way. The extended sample is also used in column (3), in which we add an interaction term between the budget gridlock variable and $govsame_{it}$. The point estimates in this column suggest a budget gridlock effect on the majority party's seat share of -.034 when the governor also belong to this party, versus -.021 in the opposite case, but the coefficient on the interaction term is not significant. Finally, column (4) adds interaction terms between $govsame_{it}$ and the economic controls as well as state fixed effects. As in the micro-level analysis, adding such interaction terms makes the coefficient on the gridlock numerically larger, but the coefficient on the interaction term between this variable and $govsame_{it}$ remains insignificant.

Table SI.4: Budget gridlock and majority party seat shares, party-level analysis

	Dependent variable: Seat share obtained by majority			
	(1)	(2)	(3)	(4)
Budget gridlock	-0.029 (0.010)	-0.027 (0.010)	-0.021 (0.011)	-0.034 (0.012)
Governor from same party as majority in legislature	-0.004 (0.008)	-0.002 (0.008)	0.003 (0.011)	0.044 (0.026)
Budget gridlock x gov. from same party as majority in legislature			-0.013 (0.019)	-0.007 (0.026)
Political controls	Yes	Yes	Yes	Yes
Economics controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Economic controls x gov. same party	No	No	No	Yes
State fixed effects x gov. same party	No	No	No	Yes
Observations	242	261	261	251
No. of states	31	33	33	31
R-squared	0.82	0.79	0.79	0.84

Notes: The table reports regression results from estimation of the model described in this appendix. Column (1) reports results from an estimation on the same sample of 242 elections used in the micro-level analyses described in the main text. Columns (2) and (3) use the full sample of 261 elections for which we have sufficient information about budget gridlock, including elections in which all incumbents run in multi-member districts. Column (4) also includes elections with only multi-member districts, but the inclusion of interaction terms between *govsame* and state fixed effects implies that elections in Delaware and Rhode Island must be omitted from the sample to avoid perfect collinearity. Standard errors (reported in parentheses) are estimated allowing for clustering at the state level.

Budgetary institutions

State governments differ considerably in what happens if a budget is not passed on time (Andersen et al. 2012). In some states, temporary budgets or continuing resolutions keep the government running at normal or close to normal scale. In other states, government employees are sent home without pay and all non-essential facilities are closed. Do such differences across states affect voter reactions to late state budgets? Table 5 shows that the effects of budget gridlock on reelection probabilities for majority and minority party members are different in states that have no provisions in place that prevent the state government from shutting down following a delayed budget (26 percent of cases) compared to states where such provisions exist (74 percent of

cases). Consistent with our earlier results, minority party members are not assigned responsibility in either case, but in states where government shuts down following gridlock, majority party incumbents' probability of reelection decreases by 6.1 percentage point compared to 3.4 percent in states with no-shutdown provisions in place.

In sum, all majority party legislators overseeing legislative gridlock are at risk of losing electoral support, but the electoral response is stronger when the consequences of gridlock in terms of welfare are more severe, consistent with standard accounts of economic voting (Anderson, 2007).

Table SI.5: Provisions to keep government operations running

	(1)	(2)
	Outcome 1: Reelection	
Majority party members	-0.061 (0.019)	-0.034 (0.013)
Minority party members	0.001 (0.018)	0.014 (0.016)
Difference	-0.063 (0.027)	-0.049 (0.018)
No. of states	9	22
No. of elections	63	179
Observations	7,029	17,150

Notes: The table reports average marginal effects of changing the budget gridlock variable from zero to one on the probability of reelection for incumbent legislators. Column (1) reports results for the subsample of states in which there are no provisions to prevent the government from shutting down in the event of a delayed budget. Column (2) reports results for the subsample of states that do have such provisions. Control variables corresponding to those in column 4 of Table 2 included in all columns. Standard errors (reported in parentheses) are estimated allowing for clustering at the state-year level.