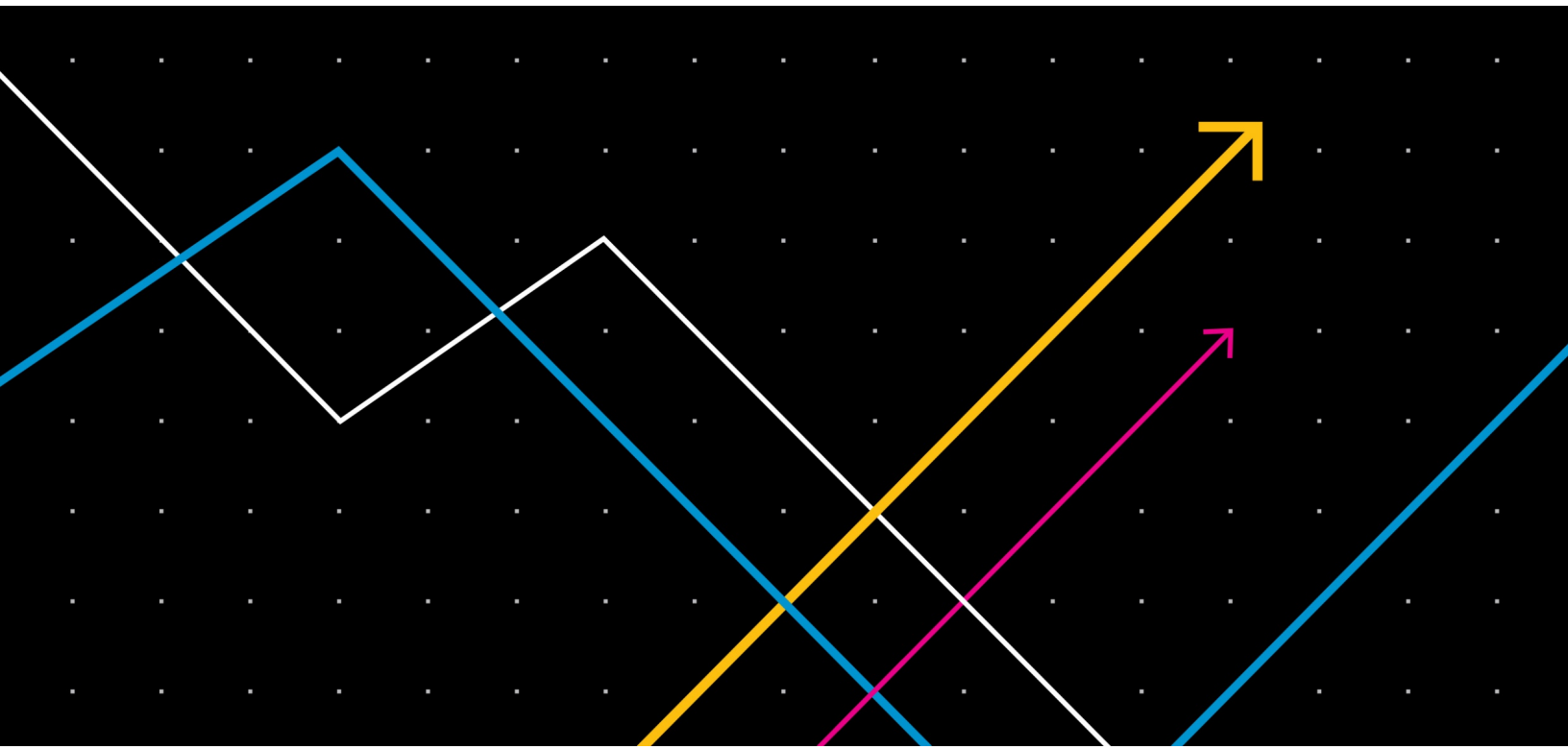

RESEARCH REPORT

Budget Processes and the Great Recession

How State Fiscal Institutions Shape Tax and Spending Decisions

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Executive Summary

In this study, we explore how strict balanced budget requirements (BBRs) and tax or expenditure limits (TELs) influence states' decisions to either cut spending or raise revenues in response to surprise deficits and whether this relationship changed following the onset of the Great Recession. We also examine whether Democratic or Republican control of a state legislature and governorship affected state responses. We find that budget processes and party control influenced both the size and the composition of state responses to deficits, with responses tempering in the period during and following the Great Recession.

An unexpected deficit occurs when, during the fiscal year, a state's spending exceeds or its revenue falls short of what was anticipated. Projected revenue and spending typically balance, partly because of balanced budget rules, so a fiscal gap emerges when actual spending or collections differ from projections. Revenue can fall short of projections because of lower tax receipts that result from lower economic activity, fluctuations in the business cycle, or overly optimistic forecasting methods. Surprise deficits are larger and more likely during recessions, partly because spending increases to meet higher demand for public services, but mostly because revenues are more likely to fall below forecasted levels.

States can respond to surprise deficits by cutting their budgets, raising revenues, dipping into reserve funds, or choosing not to act, instead pushing deficits into the following fiscal year. However, most states are at least nominally required to balance projected revenues with expenditures by way of a BBR and may also restrict the growth of government revenues or spending via a tax or expenditure limit. Our main objective in this study is to explore whether states typically bridge unexpected deficits through spending cuts, revenue increases, or a combination of both, and whether common fiscal controls like BBRs and TELs encourage larger responses or a greater reliance on either spending or revenue actions. Moreover, we want to understand whether these dynamics were different in the periods preceding, and following the onset of, the Great Recession (1990–2007 and 2008–15, respectively), and under unified party control. Such questions are critical for fiscal planning as states prepare to weather future economic downturns. Consistently delaying responses to deficit shocks could have negative consequences for states' future fiscal solvency.

In the period during and following the Great Recession, states bridged less of their deficit gaps than in the prior period. Between 1990 and 2007, states closed about 60 percent of an unexpected deficit

through an even combination of tax increases and budget cuts. However, during and in the aftermath of the Great Recession, states closed only 40 percent of an unexpected deficit and relied more heavily on budget cuts. This discrepancy may, in part, owe to states building robust rainy day fund balances before the start of the Great Recession and receiving increased federal funding in the aftermath. It may also be attributable to the rise of unified Republican control among state governments, which we find is associated with less responsiveness to deficit shocks and a greater reliance on budget cuts. Regardless of the underlying reason, states' widening budget gaps reflect decisions to push necessary cuts or revenue increases into the future, suggesting a possible "new normal" for state fiscal conditions.

When incorporating the effect of budget processes in the period during and following the Great Recession, we find that states with strong BBRs, binding revenue limits, or a combination of strict revenue and expenditure limits bridged the largest share of their deficit gaps (compared with states that had weaker rules). Both before and following the onset of the Great Recession, strong-BBR states made up most of their gaps via budget cuts. Strict revenue-limited states also relied more on budget cuts during both periods, while expenditure-limited states relied more on revenue increases (in the pre–Great Recession period), illustrating how limiting one fiscal option can encourage the use of alternatives. States with binding revenue limits face constraints on tax levels and may require either voter approval or legislative supermajorities to pass new taxes. States with spending limits may have limited their expansion of state programs during economic booms, leaving less room for cuts during downturns.

Between 1990 and 2015, we find, Republican-controlled governments closed less of their deficit gaps than Democratic-controlled or divided governments, relying more on budget cuts than on revenue increases to do so. Unified Republican control became more common during and following the Great Recession, and we find that differences in states' responses pre– and post– Great Recession largely track differences among divided, Democratic-, and Republican-controlled governments. Given the increasing incidence of unified Republican control over our study period, it appears that early-period results may have largely been driven by the higher incidence of Democratic-controlled and divided governments, while later-year results partly reflect the behavior of Republican-controlled governments in the presence or absence of institutions. The effects and efficacy of fiscal institutions can thus vary with party control, suggesting the need to tailor policy to the unique needs of each state.

Our findings, while demonstrating the ability of budget processes to encourage greater fiscal responsiveness, also illustrate how budget processes demand states cut budgets and raise revenues when the economy is already contracting—that is, at the very time when the economy and residents

would likely benefit from states spending more and not raising taxes. Future research should explore how investing in rainy day funds, pairing complementary fiscal controls with one another, or expanding the role of the federal government and stimulus funding can help mitigate some possible negative side effects of stricter budget processes during recessions.

Introduction and Literature

As states enter the 2019 fiscal year, most have passed budgets on time and are finally logging tax revenues at pre–Great Recession levels, arguably exhibiting their greatest fiscal health since before the downturn.[1] Despite improved fiscal health in the aggregate, 10 states struggled to pass a budget on time in 2017.[2] In October 2017, for example, Connecticut Governor Dan Malloy approved the state's two-year, $40.2 billion budget more than 100 days after the fiscal year began.[3] A strong bipartisan effort, the package included spending cuts, new taxes and fees, and new fiscal controls meant to stabilize the state's financial future. These controls included strengthening revenue and spending limits and specifying how money should be deposited into a rainy day fund, with the hope and assumption that additional fiscal controls will help the state move toward a more sustainable fiscal path.

Do we have evidence, however, that fiscal institutions, such as those adopted by Connecticut, help states respond to unexpected fiscal or economic pressures? Previous literature has explored this question and generally concluded that fiscal institutions do matter. However, updated data and the magnitude of states' fiscal challenges during the Great Recession make this an important time to reexamine the role of budget rules in determining state fiscal health and in helping states weather fiscal uncertainty. In this study, we explore (1) how fiscal institutions influence states' decisions to either cut spending or raise taxes in response to unexpected deficits and (2) how this dynamic played out during and after the Great Recession, compared with pre-Recession. Our objective is to expand collective understanding of how budget processes, and the combination of institutional and political factors, influence states' fiscal outcomes, especially in times of unexpected fiscal pressure.

A wide body of literature has examined the effect of fiscal institutions on state spending, debt, and other fiscal outcomes. However, conflicting findings are common in state and local public finance research because of different research methodologies and state idiosyncrasies. Moreover, research is sparse on the interaction between fiscal institutions. States have a variety of fiscal tools available to them, such as rainy day funds, debt limits, and revenue-forecasting practices. In a previous study, Randall and Rueben (2017) reviewed over 30 years of research on a wide range of fiscal institutions, including budget cycles, balanced budget requirements (BBRs), supermajority voting requirements, debt limits, tax and expenditure limits (TELs), and tax-expenditure accounting. Their recommendations included considering state fiscal institutions as toolkits rather than as stand-alone practices, tailoring institutional reforms to each state depending on its revenue volatility and political constraints, and considering social and economic distributional outcomes in addition to traditional measures of fiscal stability.

In this study, we examine the effects of specific budget institutions using a quantitative, econometric approach to supplement Randall and Rueben's (2017) findings and provide a timely update on previous research. Our analysis focused on BBRs and TELs because they directly govern states' responses to fiscal shocks (i.e., the unexpected surpluses or deficits that states sometimes face midyear). However, states may use several tools in combination to address fiscal uncertainty. Although important, these other institutions are outside the scope of this study. In the following sections, we discuss select findings from previous literature on BBRs and TELs, as discussed in Randall and Rueben (2017).

Balanced Budget Requirements

BBRs require states to balance projected revenues with expenditures, although states have varying flexibility when implementing these provisions. In general, research has concluded that stricter BBRs, which prohibit states from carrying deficits into the following fiscal year, are associated with tighter fiscal outcomes, such as reduced spending, smaller deficits, larger surpluses, and more rapid spending adjustments during recessions.

The federal Advisory Commission on Intergovernmental Relations (ACIR 1987), Alt and Lowry (1994), and Campbell and Sances (2013) have found reductions in deficit spending in states with strict BBRs. The ACIR (1987), Crain (2003), Crain and Miller (1990), and Primo (2007) have found that BBRs were associated with less spending,[4] while Alesina and Bayoumi (1996) and Bohn and Inman (1996) have found higher surpluses.

In 2003, Crain controlled for several demographic and institutional variables and found that states with strict BBRs spent on average $88 per capita less than other states. Alesina and Bayoumi (1996) was an early study on BBRs and, although widely cited across the literature, did not address reverse causality or omitted variables, and it neglected to include interactions with other fiscal institutions or factors that might cause fluctuations in the business cycle (Gordon 2012; Knight and Levinson 2000). Going beyond annual budgeting variables, Von Hagen (1991) found that states with stricter BBRs had less general obligation debt while Lowry and Alt (2001) and Poterba and Rueben (2001) found that strict BBRs reduced states' borrowing costs.

Some studies have also concluded that strict BBRs increase fiscal and economic volatility, because they force spending cuts or revenue increases when a state's economy is already contracting (Bayoumi and Eichengreen 1995; Levinson 1998, 2007).[5] Approaches to classifying BBR stringency have been

subject to debate (Hou and Smith 2006; Krol and Svorny 2007), and findings about how BBRs affect fiscal and economic volatility have varied depending on researchers' classification choices.

Poterba (1994) undertook arguably the most holistic examination of how state fiscal institutions and political circumstances interacted to affect tax and spending dynamics in response to fiscal crises. Findings included that states with strict BBRs, which prohibited them from running deficits into the following year, were better able to adjust to deficit shocks, especially if one political party controlled both the governorship and the state house of representatives. The study found, for example, that states with weak antideficit provisions reduced spending by $17 for every $100 deficit overrun, compared with $44 in strong antideficit states.

Tax and Expenditure Limits

TELs restrict the growth of government revenues or spending by capping them at a fixed-dollar amount or by limiting their growth to match increases in population, inflation, personal income, or some combination of those factors.[6] Evidence on whether TELs limit state and local spending is mixed. While some earlier studies suggested that TELs have no effect (e.g., Bails 1990), a larger and more methodologically robust body of literature suggests that TELs result in lower taxes, revenues, and spending (McGuire and Rueben 2006; Poterba 1994; Rogers and Rogers 2000; Rueben 1996). These results, however, depend on how binding the limits are, with smaller effects in states where legislators were able to override the limit with a simple majority.[7]

Poterba (1994) examined the effect of TELs, concluding that states with tax limits raised taxes less during fiscal crises than states without such limits. However, states with TELs did not necessarily cut spending more than those without TELs. Over the following years, many influential papers have cited Poterba's (1994) research while studying these and other facets of public finance (e.g., Alesina and Bayoumi 1996; Alt and Lowry 1994; Battaglini and Coate 2008; Bayoumi and Eichengreen 1995; Besley and Case 1995, 2003; Bohn and Inman 1996; Chodorow-Reich et al. 2012; Groseclose and McCarty 2001; Henisz 2004).[8]

Rueben (1996) found that TELs requiring a legislative supermajority, or a popular vote to modify spending, led to a 2 percent reduction in state general fund expenditures. However, these savings were often partly offset by higher local spending. Rogers and Rogers (2000) reported that revenue limits reduced the size of government in both revenues and expenditures, while expenditure limits only reduced revenues, and to a lesser degree than revenue limitations. Rogers and Rogers actually found a positive relationship between expenditure limits and overall expenditures but pointed out that high-

spending states may be more likely to adopt expenditure limits to rein in spending. Poterba and Rueben (2001) also found that spending and revenue limits had different effects on borrowing costs, with spending limits reducing the interest required on new debt and revenue limits increasing costs. McGuire and Rueben (2006) reviewed recent literature and concluded that some of the strictest TELs were successful in lowering taxes and spending. Researchers have also examined the role of supermajority requirements for raising tax rates or introducing new taxes, with Knight (2000) finding that supermajority requirements have significantly reduced taxes.

Lower spending and taxes, however, may not produce desirable long-term fiscal or economic outcomes. TELs have been tied to structural deficits and higher borrowing costs, while some studies have found no discernable effect on economic growth (Bae, Moon, and Jung 2012; Campbell and Sances 2013; McGuire and Rueben 2006; Poterba and Rueben 2001). McGuire and Rueben (2006), for example, found that Colorado's Taxpayer Bill of Rights did not boost the state's economic growth, despite its effect on revenues and spending. Moreover, they found that the literature was inconclusive as to whether lower taxes produced higher economic growth. Bae, Moon, and Jung (2012) found that TELs actually have a negative effect on employment and no effect on personal income. In a more recent study, Gale, Krupkin, and Rueben (2015) found that state tax cuts did not necessarily lead to economic growth.

Some of the literature that followed Poterba (1994) explored policies that would produce optimal fiscal or economic outcomes. Battaglini and Coate (2008), for example, explored a theoretical model showcasing when public welfare is best advanced by financing deficit shocks through raising additional revenues or taxes. And Chodorow-Reich et al. (2012) evaluated the effect of fiscal transfers on state employment. Overall, the research tends to conclude that fiscal institutions do matter, although it is important to consider design, implementation, and the political environment.

State Politics and Budget Processes

A related body of research that followed Poterba's (1994) work focused on how the political environment influences state fiscal policy choices. Henisz (2004) examined the relationship between veto points, checks and balances, and policy volatility. While Groseclose and McCarty (2001) examined political bargaining and its effect on policy change, Baqir (2002) studied the effects of political districting on municipal spending.

Still others have focused on how fiscal institutions interact with the political environment and election cycle. Besley and Case (1995), for example, found that when Democratic governors were under binding term limits, government spending and taxes increased during the lame-duck term. Later, Besley

and Case (2003) studied the effects of a variety of budgetary institutions, including the line-item veto, TELs, supermajority budget requirements, and divided government on state taxes and spending. They found that states with binding revenue limitations were more likely to have higher taxes, possibly indicating that high taxation is the causal factor leading states (or their electorates) to adopt stricter revenue limits and illustrating the endogeneity challenge that we discuss at further length below.

Past Approaches

The strengths of Poterba's original (1994) approach were (1) its precise measure of fiscal shock, (2) its incorporation of state political factors, and (3) its use of panel data (as opposed to cross-sectional data, which was used in many earlier approaches, such as by the ACIR [1987]).[9] Poterba's measure of fiscal shock, as well as his panel data approach, in particular, helped guard against a common challenge in state econometric research: endogeneity, or the possibility that causality might be in the other direction. Thus, we modeled our approach on Poterba's (1994) research. One of Poterba's primary limitations was its limited sample size, which included only annual-budgeting states over the five-year period from 1988 to 1992. Our research replicated Poterba's strengths, especially in measuring deficit shocks, with a larger and updated sample.

Limiting Endogeneity

Endogeneity, in econometrics, happens when the outcome of interest—in our case, budget cuts and tax changes—influences the supposed explanatory variable. In state policy research, this can happen because resident and voter preferences—which are difficult to measure—influence and are thus baked into fiscal and political *outcomes* (i.e., the dependent variables) as well as policy *choices* (i.e., the independent variables). In the case of budget institutions, for example, it is possible that residents who favor lower taxes are *also* more likely, because of this preference, to favor enacting a revenue limit. It would not be the revenue limit, in this case, but voter preference that is responsible for the observed result.

Conversely, it is possible that, in *high-tax* states, electorates are more likely to introduce revenue limits as a *reaction* to high tax rates. In this case, once again, the fiscal institution is not a causal variable, but a response to an unobserved third factor that is also associated (whether positively or negatively) with our outcome of interest. However, it is difficult to observe and measure these hypothetical relationships and unobserved variables.

While endogeneity is a common challenge in state policy evaluation, researchers can take some steps to mitigate its effects. Besley and Case (2000), for example, discussed this challenge at length and reviewed different strategies to approach state econometric research. As Poterba (1994) explained, his careful measure of deficit shock mitigated endogeneity by focusing on tax and spending *change* rather than *levels*, as previous research had done. Using change as a measure mitigated the risk that general voter preference for low taxes or spending was the influential factor in fiscal outcomes. Low-tax and high-tax (or low-spending and high-spending) states can impose changes in *both* spending and revenues in the face of fiscal pressures and may do so differently depending on their institutions. Focusing simply on levels, by comparison, can amplify possible endogeneity.[10]

State fiscal institutions are also relatively stable over time. In many cases, for example, BBRs were adopted in the original state constitution (often dating back to the late 19th or early 20th century) and remain unchanged (see appendix A for years of BBR and TEL adoption). Many TELs were adopted in the 1970s and 1980s, before the start of our study period. In some cases, states have adopted institutions during our study period or enacted changes at various points in time. However, the long-standing nature of these institutions can help guard against endogeneity. An institution adopted 20 or more years ago is more exogenous to current fiscal conditions, or other unobserved factors like voter preference, than explanatory variables that change each year. The threat of endogeneity can never be fully eliminated, because econometric approaches are not pure experiments. However, in examining 49 states across 26 years, we can treat fiscal institutions as more stable, exogenous factors that influence, rather than are influenced *by*, fiscal outcomes like budget cuts and tax changes.

Examining Great Recession Effects

In addition to using a larger panel of data from 1990–2015, we examine whether the relationships found in earlier research still held in more recent years, specifically during and following the Great Recession, when states experienced some of the largest drops in revenue and increases in spending pressure since the Great Depression. We examined results from the entire sample and then separately for the 1990–2007 and 2008–15 time periods (and tested whether our results were statistically significantly different across these time periods). Where results were not significantly different across time periods, we report on effects over the course of the full study period, but regression results for all three time periods can be found in appendix B.

Data and Approach

To examine the relationship between budget processes and fiscal crisis, we followed Poterba's (1994) approach. We measured unanticipated fiscal shocks and estimated a series of linear regression models that account for the interactions among fiscal institutions, deficit shocks, and political party control. This approach disentangled anticipated spending and tax changes from economic and political activity. We went beyond prior research by

- **expanding the study period** to 26 years, giving us more observations to draw inferences from, as well as insights into contemporary state fiscal dynamics;
- **examining results over different time periods** to understand whether the relationship between institutions and outcomes has been stable over time, or if there have been shifts since the onset of the Great Recession or as fiscal institutions have matured;
- **including both biennial- and annual-budgeting states** in our analysis (as opposed to just annual states), giving us more observations and increasing our results' relevance for a wider pool of states; and
- **updating and refining definitions and data** on BBRs and TELs, examining how these institutions have changed over time and if they truly are binding on states.

Data

We constructed a panel dataset featuring fiscal, political, and institutional variables on 49 states from 1990 to 2015. We excluded Alaska from our analysis, consistent with prior literature, because of its fiscal volatility and unique reliance on oil severance taxes. While several states in our data depend on natural resources as a source of revenues, none do quite so much as Alaska.

We obtained data from a variety of sources, with our main variables coming from the National Association of State Budget Officers' (NASBO's) *Fiscal Survey of States* (for fiscal data) and *Budget Processes in the States* (for fiscal institution data). NASBO surveys state budget officers twice a year, asking about spending and revenues among other fiscal variables, for the prior, current, and the upcoming fiscal years. The surveys ask questions about: (1) the amount of revenues, expenditures, and rainy day fund balances; and (2) any action state legislatures or the executive branch took to increase or lower taxes or cut budgets during the current fiscal year.

We converted all fiscal variables from nominal millions of dollars to 2015 inflation-adjusted dollars per capita using the consumer price index from the Bureau of Labor Statistics and population estimates from the US Census Bureau.[11] All fiscal data reflect each state's own fiscal year, which typically runs from July 1 to June 30, with some exceptions.[12]

NASBO publishes its survey of budget processes in the states periodically, with the most recent report issued in 2015. We augmented NASBO budget process data with original data collection on state fiscal institutions and supplemental information from other authoritative sources (discussed in more detail in the following sections of this report).

We obtained our political variables on state legislative and gubernatorial control from Carl Klarner.[13] For analyses of unified government across legislative houses, we excluded Nebraska because its legislative elections are nonpartisan.

Measuring Fiscal Outcomes

Our outcomes of interest represent different fiscal choices that states have made when faced with an unexpected surplus or deficit during the year. Most states are required to balance their budgets each year, with varying levels of stringency. But when faced with insufficient revenues to meet spending needs midyear, how do states react? States can use existing surplus funds or proactively choose to (1) cut budgets midyear, (2) raise taxes or other fees and revenues during the year, or (3) raise taxes and other fees and revenues for the following year, to either retroactively plug gaps in revenue (if permitted) or ensure larger revenue streams for future years. States could also do nothing and hope that changing economic circumstances will restore revenues or that federal funds will become available to help.

We examined the proactive actions that states took to either *cut* spending on specific state programs during the year or to *raise* tax rates or other own-source revenue instruments, like fees. Our estimates do not reflect changes in revenues or spending caused by economic conditions independent of legislative or executive action. For example, if an increase in economic activity drives revenue up under current tax rates, that would not be included in our measures of tax changes. Similarly, if midyear spending on a social services program is less than anticipated because of decreased demand but not policy action, this would not be included in our measure of budget cuts.

Spending Cuts

Midyear budget cut (budcut) is the net amount by which a state reduced its spending during the year.[14] States often cut spending when they experience an unexpected deficit during the year, and may be more likely to do so if stringent fiscal institutions are in place (as evidenced by prior research). Positive values for budcut indicate a spending reduction, while a value of zero suggests no change from projected spending. Budcut does not relay information on any legislative action to *increase* spending above projected levels during the year (i.e., budcut has no negative values).

Table 1 provides summary statistics on midyear budget cuts and other fiscal outcomes of interest for a sample of years in our 26-year study period, with state per capita general fund spending provided as a point of reference. In 2015, 14 states enacted midyear budget cuts, at an average of $20 per capita for those that made cuts. Mean general fund spending that year, as a point of comparison, was $2,419 per capita. Budget cuts varied over time and across states, often reflecting shortfalls caused by changing economic conditions. For example, in 2010 during the Great Recession, 39 states cut budgets by an average of $106 per capita (i.e., 5 percent of spending), with Minnesota cutting its budget by almost $300 per capita (i.e., roughly 10 percent of spending). In contrast, in 2000 at the height of the dot-com boom, Kansas was the only state to cut its budget midyear (by $34 per capita).

TABLE 1

Fiscal Outcomes: Spending and Revenue Changes

Per capita 2015 dollars

Variable	1990	1995	2000	2005	2010	2015
Mean state general fund spending ($)	1,860	2,039	2,251	2,282	2,222	2,419
Midyear budget cut						
Mean (all observations) ($)	21	6	1	2	84	6
Mean (if > 0) ($)	55	38	34	22	106	20
Standard deviation ($)	46	18	5	9	81	13
Minimum ($)	0	0	0	0	0	0
Maximum ($)	247	84	34	60	299	55
# of states > 0	19	8	1	5	39	14
# of states = 0	30	41	48	44	10	35
Midyear revenue change						
Mean (all observations) ($)	10	-1	-1	1	2	-3
Mean (if > 0) ($)	32	5	37	6	15	6
Mean (if < 0) ($)	-18	-9	-12	-1	-7	-20
Standard deviation ($)	28	7	17	5	12	14
Minimum ($)	-54	-36	-49	-7	-37	-69
Maximum ($)	110	16	97	26	70	30
# of states > 0	17	5	3	10	10	11
# of states < 0	3	10	15	7	6	10
# of states = 0	29	34	31	32	33	28
Next-year revenue change						
Mean ($)	56	-16	-17	14	28	17

Variable	1990	1995	2000	2005	2010	2015
Mean (if > 0) ($)	110	34	40	36	53	52
Mean (if < 0) ($)	-19	-38	-43	-27	-23	-30
Standard deviation ($)	107	51	53	45	50	64
Minimum ($)	-105	-241	-214	-152	-104	-105
Maximum ($)	515	112	138	120	159	252
# of states > 0	26	10	8	29	29	26
# of states < 0	7	29	27	14	7	17
# of states = 0	16	10	14	6	13	6

Source: Authors' calculations using data from the National Association of State Budget Officers, *Fiscal Survey of States*, 1989–2016, https://www.nasbo.org/mainsite/reports-data/fiscal-survey-of-states/fiscal-survey-archives; "Consumer Price Index for All Urban Consumers: All Items (CPIAUCSL)," Bureau of Labor Statistics via the St. Louis Federal Reserve Board, accessed September 26, 2018, https://fred.stlouisfed.org/series/CPIAUCSL; and "Annual Estimates of the Population for the US and States, and for Puerto Rico," Bureau of Labor Statistics via the St. Louis Federal Reserve Board, accessed September 26, 2018, https://fred.stlouisfed.org/series/CAPOP.
Notes: Five-year intervals excerpted from full 1990–2015 panel. Excludes Alaska.

Revenue Changes

Next-year revenue change (revnxt) reflects the amount by which a state increased or decreased its own-source revenue for the following fiscal year by changing its tax rates or other revenue instruments, while midyear revenue change (revch) reflects changes that went into effect in the current fiscal year.[15] Positive values indicate an increase in revenue from an increase in the tax rate or another revenue source, while negative values indicate reduced revenue from a tax cut or other change and zero indicates no change. States may decide to raise their taxes in the following year to retroactively plug holes from an unexpected deficit shock or to ensure better revenue availability next year. In fact, states are more likely to adopt a large tax increase that goes into effect at the beginning of the calendar year or during the next budget year than they are to implement a midyear increase. Between 1990 and 2015, mean next-year revenue changes were consistently larger than midyear revenue changes. Some states increased tax rates, while others lowered some rates. Often states would do both, raising some rates while cutting others.

We summed all revenue actions in a state, as reported by state budget analysts, to arrive at the net change once all legislative changes were accounted for. In 2015, 26 states increased taxes or charges for the following year and 17 lowered them, while 6 reported no changes (table 1). In contrast, over half of states (28) reported no midyear changes in their revenue systems. On average, states increased per capita revenues for the following year by $17, with an average increase of $52 among only those states that increased revenues and a decrease of $30 for those that cut revenues. Midyear revenue changes were smaller, with 11 states increasing taxes by an average of $6 per capita and 10 states cutting revenues by $20 per capita.

States are more likely to implement a large tax change in the following year, rather than midyear, for several reasons. First, given that most taxpayers use a calendar year to calculate income taxes, it is difficult for taxpayers and tax administrators to adjust to a split annual income tax rate. However, states can increase excise taxes (such as motor fuel taxes), or even sales taxes, midyear. Second, some state legislatures are not in session year-round, so it may not be possible to legislate fiscal change midyear.

As with budget cuts, next-year revenue increases varied over time with the business cycle. In 2010, mean revenue changes for the following year were $28 per capita, although this combines an average per capita increase of $53 for the 29 states that raised revenues and a $23 cut for the seven states that cut taxes (i.e., Connecticut, Indiana, Missouri, North Carolina, North Dakota, Ohio, and West Virginia). Next-year revenue changes in 2010 ranged from -$104 per capita to $159 per capita.

The average size of tax increases has also fallen over time in our sample. In 1990, while about half of all states raised taxes, they raised revenues more than in later years—averaging $110 per capita, compared with $52 in 2015. This decline in legislative action could reflect legislators' or constituents' changing preferences, differences in the size of unexpected deficits, or other state actions to address deficits. One notable difference between the recent recession and earlier downturns was that the federal government disbursed targeted funds more quickly (Gordon 2018).

Knowledge relayed by state budget practitioners suggests that, when it comes to responding midyear to an unexpected deficit, states tend to rely more on budget cuts than on tax increases. This is borne out in our data. Midyear revenue changes were small compared with budget cuts and next-year revenue increases, and they varied less over time. Additionally, budget cuts are politically and administratively easier than tax increases to implement midyear. Ben Watkins, director of the Division of Bond Finance in Florida, for example, characterized Florida's informal budget-balancing process in the most recent recession as (1) pursuing budget cuts, (2) dipping into reserve funds, (3) using the influx of federal funds, and then (4) pursuing tax increases.[16]

Measuring State Fiscal Shock

We examined how the states responded to fiscal crises by either cutting budgets or raising taxes, as described above. But how did we identify and measure a fiscal crisis? In 2015, 37 states required the governor to submit a balanced budget to the legislature, and 37 required the legislature to pass a balanced budget.[17] Unexpected surpluses and deficits can still arise, however, when during the fiscal year either revenues or spending differ from initial projections. When spending is greater than

anticipated or when revenues are less than anticipated, states may be faced with a surprise midyear deficit. Similarly, if spending is less than, or revenues exceed, what was originally projected, states will have a surprise midyear surplus. These unexpected midyear differences are called "fiscal shocks" and are how we measure surprise fiscal conditions that pressure states to respond.

Revenue Shock

A state's overall fiscal shock is a product of its annual revenue and expenditure shock. A positive revenue shock occurs when the state collects more revenue than originally projected (after adjusting for any midyear revenue changes). A negative revenue shock happens when the state collects less revenue than anticipated. Revenue shocks can happen when a state is facing an unexpected drop in revenues during a recession, for example, or if revenue-forecasting methods produce inaccurate projections.

Per Poterba's (1994) definition, a revenue shock (revshock) consists of a state's actual general fund revenue collected in a fiscal year (actrev), minus the value of midyear revenue changes (revch), minus the value of originally projected general fund revenues (prorev):

$$\text{revshock} = \text{actrev}^{18} - \text{revch} - \text{prorev}^{19}$$

As Poterba (1994) noted, it is important to subtract midyear revenue changes from actual revenues to arrive at the revenue that would have been collected *but for* the midyear changes. Unless we subtract midyear revenue changes, actual and projected revenues would misleadingly sum to zero if policymakers responded to unexpected deficits by raising taxes during the year. Adjusting for midyear revenue changes reveals the extent of the revenue shock to which policymakers were responding.

Table 2 provides summary statistics on revenue and expenditure shock for a sample of years in our 26-year study period. In 2015, states experienced a mean revenue shock of $32 per capita (i.e., collected more revenue than anticipated), but the shock varied widely across states, ranging from -$438 per capita in Wyoming (17 percent of revenue) to $264 in Hawaii (6 percent of revenue). Sixteen states experienced a negative revenue shock that averaged $57 per capita while 33 had actual revenues higher than projected. Revenue shocks varied with the business cycle. In 2010, for example, 42 states experienced a negative revenue shock, at a mean of −$107 per capita for those with a negative shock, or about 5 percent of actual revenues.

TABLE 2

Revenue and Expenditure Shock in the States

Per capita 2015 dollars

Variable	1990	1995	2000	2005	2010	2015
Mean state general fund spending ($)	1,860	2,039	2,251	2,282	2,222	2,419
Revenue shock						
Mean ($)	12	77	51	135	-81	32
Mean (if > 0) ($)	81	126	113	142	79	76
Mean (if < 0) ($)	-97	-44	-267	-35	-107	-57
Standard deviation ($)	135	154	290	103	87	100
Minimum ($)	-359	-198	-1,820	-39	-261	-438
Maximum ($)	512	744	429	603	149	264
# of states > 0	30	35	41	47	7	33
# of states < 0	19	14	8	2	42	16
# of states = 0	0	0	0	0	0	0
Expenditure shock						
Mean ($)	21	53	-30	49	23	13
Mean (if > 0) ($)	105	103	48	83	108	51
Mean (if < 0) ($)	-84	-59	-157	-30	-73	-44
Standard deviation ($)	208	159	298	111	124	99
Minimum ($)	-367	-164	-2,013	-108	-194	-272
Maximum ($)	1,224	701	350	655	440	575
# of states > 0	27	33	29	34	26	29
# of states < 0	22	14	18	13	23	19
# of states = 0	0	2	2	2	0	1
Fiscal shock						
Mean ($)	-8	24	80	86	-104	20
Mean (if ≥ 0) (surplus shock) ($)	66	73	97	107	64	82
Mean (if < 0) (deficit shock) ($)	-92	-52	-65	-24	-142	-88
Standard deviation ($)	151	104	108	77	127	172
Minimum ($)	-712	-348	-129	-64	-349	-1,012
Maximum ($)	596	461	461	270	248	389
# of states ≥ 0	26	30	44	41	9	31
# of states < 0	23	19	5	8	40	18

Sources: Authors' calculations using data from the National Association of State Budget Officers, *Fiscal Survey of States*, 1989–2016, https://www.nasbo.org/mainsite/reports-data/fiscal-survey-of-states/fiscal-survey-archives; "Consumer Price Index for All Urban Consumers: All Items (CPIAUCSL)," Bureau of Labor Statistics via the St. Louis Federal Reserve Board, accessed September 26, 2018,https://fred.stlouisfed.org/series/CPIAUCSL; and "Annual Estimates of the Population for the US and States, and for Puerto Rico," Bureau of Labor Statistics via the St. Louis Federal Reserve Board, accessed September 26, 2018, https://fred.stlouisfed.org/series/CAPOP

Notes: Five-year intervals excerpted from full 1990–2015 panel. Excludes Alaska. Fiscal shock is revenue shock minus expenditure shock. A positive expenditure shock occurs when actual spending is more than projected, while a negative revenue shock occurs when actual revenues are less than projected.

Expenditure Shock

A positive expenditure shock indicates that actual spending exceeded projected expenditures, while a negative value indicates that actual spending was less than anticipated. Expenditure shocks can arise from unanticipated caseload growth for state programs, such as Medicaid or education, increasing cost

per program recipient (as can often happen in health care), or other unexpected use of state programs. Per Poterba (1994), an expenditure shock (expshock) consists of actual general fund expenditures in a fiscal year (actexp), minus the value of net midyear budget cuts (budcut), minus originally projected general fund expenditures (proexp):

$$expshock = actexp^{20} - budcut - proexp^{21}$$

As with midyear revenue changes for revenue shocks, it is important to adjust here for midyear budget cuts to illustrate the true magnitude of the spending shock that states were responding to during the year. In 2015, 29 states experienced a positive expenditure shock (i.e., spent more than anticipated), spending $51 per capita more than budgeted, and 19 states spent an average of $44 less per capita (table 2). Expenditure shocks varied across states, however, ranging from -$272 per capita in North Dakota (about 6 percent of projected spending) to $575 per capita in Wyoming, or nearly 20 percent of what was projected. Expenditure shock values varied over time with the business cycle, but over half of states consistently experienced spending greater than projected in any given year in our study period. This suggests that states regularly grapple with expenditure shocks, regardless of what is happening with revenues and the economy, while negative revenue shocks happen primarily during economic downturns.

Fiscal Shock

Overall fiscal shock (fiscshock), then, is the value of the revenue shock minus the expenditure shock (i.e., the amount by which unanticipated revenues exceeded unanticipated expenditures):

$$fiscshock = revshock - expshock$$

A positive fiscal shock value represents an unexpected surplus, while a negative value indicates an unexpected deficit. Zero suggests a balanced budget. States experience more pressure to act during periods of unexpected deficit than in times of surplus, as all states can carry over surpluses and make changes in the following fiscal year (except the few states required to remit surplus revenues to voters). Thus, in this study we have highlighted and focused on how many states experience and respond to surprise deficits. This study focused especially on how negative fiscal shocks—what we call "deficit shocks"—influenced states' likelihood of cutting spending or raising taxes, and how fiscal institutions affected this likelihood.

The third panel of table 2 provides summary statistics on fiscal shock and highlights the number and average value for states experiencing a positive (i.e., surplus) shock or negative (i.e., deficit) shock.

Budgets are rarely perfectly balanced after accounting for midyear revenue or budget changes. Virtually every state had some degree of fiscal shock in any given year.[22] We analyzed states with either a surplus or a balanced budget as a single group, in part to highlight how states react when facing unexpected shortfalls compared with more favorable fiscal conditions.

In 2015, 18 states experienced an unexpected deficit that averaged $88 per capita, while 31 had a surplus that averaged $82 per capita. As is the case with most of our fiscal variables, fiscal shocks also varied with the business cycle. At the height of the dot-com boom in 2000, for example, 44 states had a surplus that averaged almost $100 per capita. In contrast, in 2010, 40 states experienced an unexpected deficit that averaged $142 per capita.

State Adjustment to Fiscal Shock

Before examining the effect of institutions on states' behavior, we explored the effect of either a surplus or a deficit shock alone on states' decision to cut spending, raise taxes midyear, or raise taxes the following year.

Estimating Effects

Replicating Poterba (1994), we estimated regression equations of the form (equation 1):

$$\left.\begin{matrix}\textbf{budcut}\\ \textbf{revch}\\ \textbf{revnxt}\end{matrix}\right| = a + b_1{*}\text{surplus} + b_2{*}\text{deficit} + \text{error}$$

Where budcut, revch, and revnxt are dependent variables in three separate equations, *a* represents the constant, b_1 represents the coefficient on surplus shock (i.e., the amount by which states cut their budget, raised their taxes midyear, or raised their taxes next year, respectively, for each per capita dollar of unexpected surplus), and b_2 represents the coefficient on deficit shock (i.e., the amount by which states cut their budget, raised their taxes midyear, or raised their taxes next year, respectively, for each per capita dollar of unexpected deficit), plus an error term. Table B.1 in appendix B reports full results from this first set of regressions, using state random and year fixed effects, for the 1990–2015, 1990–2007, and 2008–15 time periods.[23] Findings are discussed below.

Findings

We find that, despite experiencing larger-than-usual unexpected deficits (-$113 per capita),[24] states' response to deficit shocks tempered during and following the Great Recession. Between 1990 and 2007, states closed 58 percent of their unexpected deficit through either budget cuts or next-year tax increases. This dropped to 38 percent following the onset of the Great Recession. In other words, states closed less of their deficit gap through own-source revenue adjustments or spending cuts in our later period.

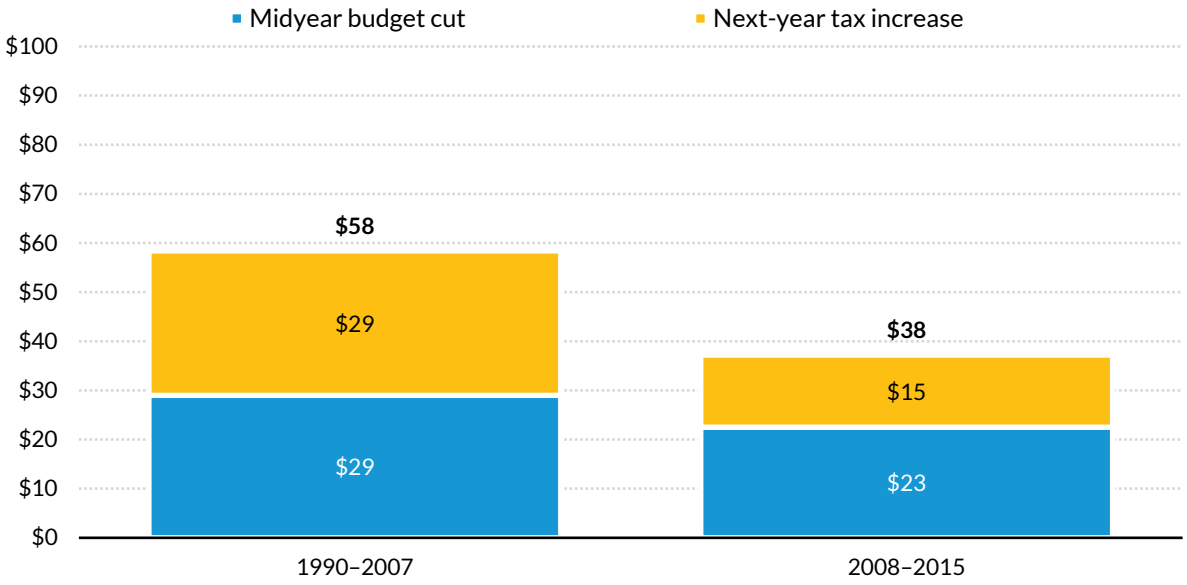
Put differently, we can think about states' response to deficit shock in terms of their "net adjustment." States' net adjustment reflects how much of a deficit gap the state typically closes through spending cuts *and* revenue increases combined. For this analysis, we focused primarily on next-year revenue changes because (as discussed in the previous section) it is easier for states to raise taxes for

the following year than midyear. Thus, we added the value of midyear budget cuts to next-year tax increases to arrive at net adjustment.

FIGURE 1

State Adjustment to Deficit Shock

Per $100 of unexpected deficit per capita (before and after onset of Great Recession)



Source: Authors' analysis. Original regression coefficients reported in table B.1 in appendix B.
Notes: Results reflect 2015 inflation-adjusted, per capita dollars. Analysis excluded Alaska. Midyear budget cuts represent the absolute value of the cut (i.e., a positive value is equivalent to a cut). Positive next-year revenue values reflect revenue increases. Regression coefficients are statistically significant unless otherwise noted, and period results are statistically significantly different from one another.

From 1990 to 2007, states' net adjustment to deficit shock was $58 per $100 of unexpected deficit (figure 1). During this period, states cut budgets midyear by $29 per capita and raised revenues by $29 the following year. By comparison, from 2008–2015, states' net adjustment was only $38 per capita — comprised of $23 in midyear spending cuts and $15 of revenue increases the following year (per $100 of unexpected deficit). Thus, the largest change we observed was a smaller revenue increase during and following the Great Recession.

States also raised taxes midyear less aggressively from 2008 to 2015 than in the prior period. From 1990–2007, states raised taxes by $11 per capita for each $100 of unexpected deficit—compared with only $5 midyear from 2008 to 2015 (see table B.1 results for revch in appendix B). Midyear tax increases, as discussed previously, are typically smaller than next-year increases. Raising taxes during a

fiscal year can be difficult, and legislators and governors often wait for the beginning of the calendar year to raise taxes to be less disruptive.

Thus, even though the Great Recession was more severe and longer lasting than prior downturns, state reactions were less immediate. This may be because rainy day fund balances were robust before the start of the Recession and transmission of federal recovery funds to the states was more effective.

Budget Processes and Fiscal Shock

This study's primary objective was to examine whether more stringent fiscal institutions, designed to constrain taxes or spending, have the anticipated effect on state budgeting. Do strict BBRs, for example, help states achieve balanced budgets? And, if so, do states bridge deficit gaps through spending cuts, tax increases, or both? Do strict state revenue limits reduce states' likelihood of raising revenues to close a gap? Answering these questions required us to

- **classify fiscal institutions as weak or strict**, requiring reexamination of prior systems of BBR and TEL classification; and
- **estimate regression equations using two-way interaction terms** between fiscal and institutional variables, demonstrating how institutions influenced states' behavior during times of either unexpected surplus or deficit.

We focused on BBRs and TELs, as discussed previously, because they directly inform state responses to deficit shocks. BBRs and TELs vary a good deal in stringency across states compared with other budget process and institutions (e.g., the presence of a line-item veto or budget stabilization fund) for which we find little variation.

Previous definitions of both BBR and TEL stringency informed our analysis (e.g., ACIR 1987). To accurately examine institutions' influence over time, however, we augmented and amended previous definitions and classifications by cross-checking multiple authoritative sources and directly examining state laws and constitutions, where necessary, enabling us to further confirm and parse the details of provisions in each state.

While individual fiscal institutions can affect changes in taxes or spending during times of surplus or deficit, we are most interested in how these factors influence a state's actions when it is experiencing unexpected deficits. Thus, our critical variables represent interactions between institutional indicator variables and the size of unexpected deficits and surpluses (i.e., two-way interaction terms).

Balanced Budget Requirements and Fiscal Shock

To answer the question, "Do BBRs influence state responses to unexpected deficits?" we classified state BBRs as weak or strong and modeled their influence on fiscal outcomes. In many previous studies (including Poterba [1994]), researchers relied on an index of BBR stringency constructed by the ACIR

(1987) to classify a state's BBR as strong or weak. The ACIR index relied heavily on whether the legislature or governor was required to pass or sign, respectively, a balanced budget; whether the state was prohibited from carrying over a deficit into the following fiscal year (i.e., "no carryover" provision); and whether such requirements were constitutional or statutory.

The ACIR index was constructed at the discretion of ACIR staff because few prior empirical findings on BBR efficacy were available to inform the staff's classification framework. Recent literature has proposed a more nuanced framework for understanding BBR stringency, incorporating not only the role of political actors, such as the legislature and governor, but technical provisions that direct the state to implement a balanced budget (Hou and Smith 2006). These technical provisions include, but are not limited to, the "no carryover" provisions identified by the ACIR (Hou and Smith 2006; Smith and Hou 2013).

For this study, we adopted Hou and Smith's (2006) framework, which examined BBRs as a complex system made up of nine possible political and technical provisions. We then focused on the combinations most likely to exert a material influence on whether states balance their budget each year. These provisions come into play during different stages of states' budget processes, with later-stage (i.e., implementation-stage) provisions acting as stricter controls on states' budgeting actions. Provisions prohibiting a deficit carryover into the following fiscal year, for example, impose a late-stage technical requirement for the state to implement a balanced budget. However, a combination of other mid- and late-stage requirements can also create a stronger system of balanced budget adherence.

Defining Balanced Budget Requirements

Based on findings from Hou and Smith (2006) and Smith and Hou (2013), as well as prior literature and expert knowledge on this topic, we defined states with strong BBRs as meeting *at least one* of the following three requirements:

1. **The governor must sign a balanced budget**;[25]
2. **The state is prohibited from carrying over a deficit** into the following fiscal year or biennium;[26] or
3. **The legislature must pass a balanced budget**, accompanied by at least one of two late-stage technical requirements:
   - *either* ***controls are in place on supplementary appropriations*** or
   - ***within fiscal year controls*** are in place to avoid deficit.[27]

We classified any state with at least one of the above as having a strong BBR, and the remaining as "weak-BBR" states. Weak-BBR states may have early-stage political requirements for the governor to propose a balanced budget or weak early-stage technical requirements that allow the state to finance deficits through debt, but these do not rise to the standard of a binding requirement for our study.[28] Also included in our weak-BBR classification are the few states that lack any BBR entirely.

Weak BBR (wbbr) is an indicator variable with a value of 1 when the state has either a weak (or no) BBR, as defined above. A value of 0 denotes a strong BBR. Table 3 provides summary statistics on weak BBRs for a sample of years in our 26-year study period. Compared with fiscal and political variables, BBRs vary little over time. In 1990, 33 percent of states had a weak BBR, compared with 24 percent in 2015. Most states had a strong BBR per our classification system, and states have tended to strengthen their BBRs over time.[29]

TABLE 3

Balanced Budget Requirements in the States

Number and percentage of states with weak or strong balanced budget requirements (BBRs)

Institution	1990	1995	2000	2005	2010	2015
Weak BBR (or none)						
# of states	16	14	13	13	13	12
% of states	33	29	27	27	27	24
Strong BBR						
# of states	33	35	36	36	36	37
% of states	67	71	73	73	73	76

Sources: Authors' calculations based on various sources and independent data collection. For years of adoption, see table A.1 in appendix A.

Notes: Five-year intervals excerpted from full 1990–2015 panel. Excludes Alaska.

BBR stringency varied more across states than across time, although several states have adopted stronger requirements during our study period. Four states moved from a weak to a strong-BBR classification during our study period, because they adopted either a legislative requirement to pass a balanced budget or late-stage within-year fiscal controls.[30] Only one state, North Carolina, changed its "no carryover" provision during our study period by repealing it in 2006. However, the state retained its strong-BBR classification because of its existing requirements for the legislature to pass a balanced budget and within-year, late-stage fiscal controls.

Estimating Effects

To examine the effect of weak and strong BBRs on state adjustment to fiscal shock, we estimated (equation 2):

$$\begin{vmatrix} \textbf{budcut} \\ \textbf{revch} \\ \textbf{revnxt} \end{vmatrix} = a + b_1{*}surplus + b_2{*}deficit + b_3{*}wbbr + b_4{*}wbbr{*}surplus + b_5{*}wbbr{*}deficit + error$$

We amended our prior equation (1) to include a level variable (wbbr), illustrating how states responded if they had a weak BBR (coefficient b_3), and interaction terms, which allowed us to observe different responses to unexpected deficits among states with either weak or strong BBRs (illustrated by coefficients b_4 and b_5). Table B.2 in appendix B reports full results from this second set of regressions, using state random and year fixed effects, for our three sample periods.[31] We rejected our null hypothesis that states responded consistently before and following the onset of the Great Recession, so we focused on the results from before and after 2008.[32]

Findings

Beginning in 2008, we find, stronger BBRs were associated with stronger state actions to balance their budgets compared with states with weak BBRs. States with strong balanced budget rules closed 51 percent of their unexpected deficits, while weak-BBR states took little action. In the prior period, by comparison, states with *either* a strong or a weak BBR both closed about 60 percent of their unexpected deficits. Weak-BBR states did this primarily through tax increases the following year, while states with a strong BBR had a more even balance of tax increases and budget cuts.

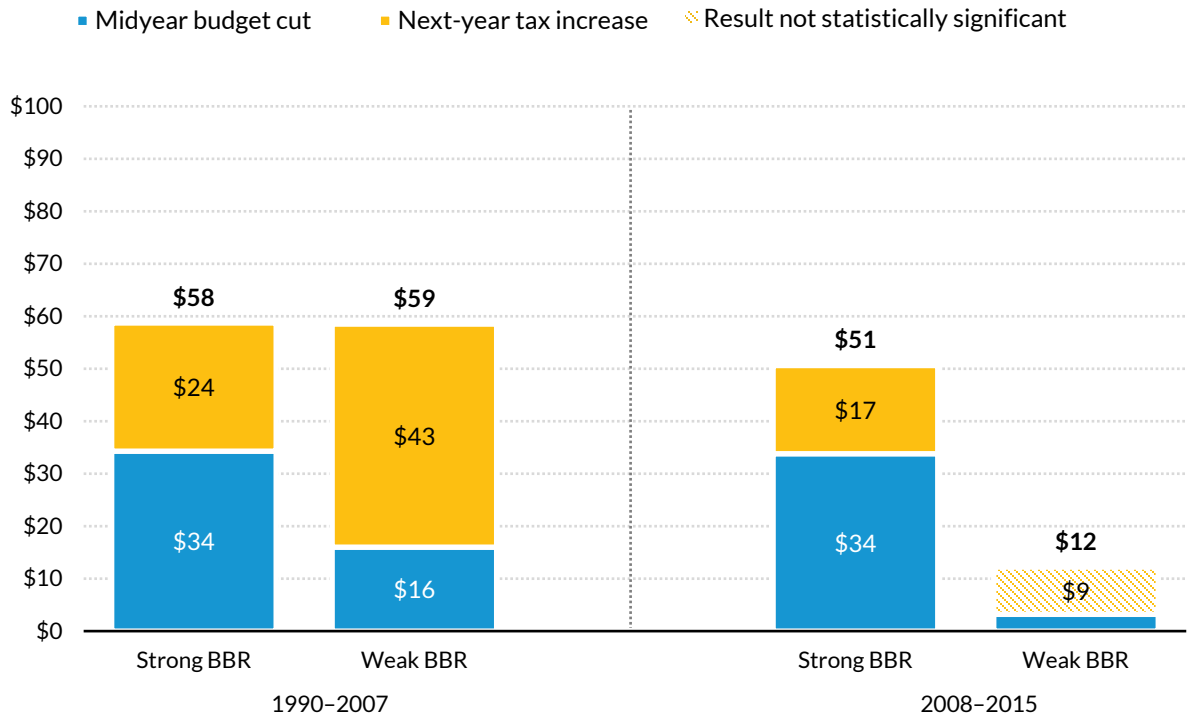
Figure 2 shows that, from 1990 to 2007, states with a strong BBR cut their budgets by $34 per capita for each $100 of unexpected deficit and raised next year's taxes by $24 per capita—for a net fiscal adjustment of $58. Weak-BBR states had a net fiscal adjustment of $59 per capita—composed of $16 in budget cuts and $43 in next-year tax increases. In contrast, from 2008 to 2015, states with strong BBRs continued to cut spending midyear by $34 per capita but raised taxes next year by only $17 per capita—for a net fiscal adjustment of $51 per capita for every $100 of unexpected deficit. Weak-BBR states, however, cut spending by only $3 per capita. They raised next-year taxes by $9 per capita, but this result was not statistically different from the strong-BBR states. Nonetheless, assuming no difference in next-year tax increases from strong-BBR states, the difference in spending cuts alone was enough to illustrate the increased responsiveness of strong-BBR states to deficit shocks.

This relationship held true for midyear tax increases as well. From 2008 to 2015, states with weak BBRs raised midyear taxes by only $1 per capita for each $100 of unexpected deficit, while states with strong BBRs raised them by $7 (see table B.2 in appendix B). This is in comparison with the prior period (1990–2007) when weak-BBR states actually raised midyear taxes by $21 per capita compared with only $7 per capita in strong-BBR states.

FIGURE 2

Balanced Budget Requirements and State Adjustment to Unexpected Deficit

Per $100 of unexpected deficit per capita (before and after the onset of the Great Recession)



Source: Authors' analysis. Original regression coefficients reported in table B.2 in appendix B.

Notes: Results reflect 2015 inflation-adjusted, per capita dollars. Analysis excluded Alaska. Midyear budget cuts represent the absolute value of the cut (i.e., a positive value is equivalent to a cut). Positive next-year revenue values reflect revenue increases. Regression coefficients are statistically significant unless otherwise noted, and time period results are statistically significantly different from one another.

These findings suggest that strong BBRs do make a difference and influence states' responses to unexpected deficits, and this discipline was greater in later years. States with stronger BBRs cut budgets more aggressively and (in the more recent period) raised taxes more than states with weak BBRs. This dynamic may reinforce concerns about the effect of strong BBRs on fiscal and economic volatility (as discussed in some of the previous research on this topic), as well as on residents who rely on social services during economic downturns. It appears that, in the midst of a budget crisis when the

economy and residents would benefit from states spending more and easing taxes, strong BBRs ask them to take the opposite action more aggressively. While tighter fiscal outcomes and responsiveness to deficit shocks are in many ways beneficial, negative side effects can arise.

Tax and Expenditure Limits and Fiscal Shock

We also examined how the presence of a binding TEL affected a state's likelihood of either cutting spending or raising taxes in response to a deficit. Expenditure limits dictate that spending cannot grow by more than a certain amount each fiscal year, based on changes in either personal income or inflation and demographic growth. Revenue limits, on the other hand, restrict increases in general fund revenues or require a supermajority vote of the legislature (or public voter approval) to increase state tax rates. Limited and conflicting results among previous TEL research prompted us to reexamine TEL characteristics, assess their strictness, and evaluate differences between revenue and expenditure limits.

Defining Tax and Expenditure Limits

Unlike the debate over BBR classification, there has not been significant debate in the literature on what constitutes a binding TEL. Most of the literature has considered (1) whether the provision was a tax or expenditure limit, (2) whether it required a simple majority or a legislative supermajority (or a popular vote) to override, and (3) in some cases, whether it was constitutional or statutory.

We defined a binding tax or expenditure limit as requiring a vote of the people or a supermajority vote of the legislature to override the requirement, consistent with much of the prior literature. For our purposes, this definition includes provisions requiring a supermajority vote of the legislature to raise *new* taxes or revenues. That is, if a limit can be overridden or ignored with the same action or number of legislators required to pass the budget, we considered it to be a nonbinding limit. We did not distinguish between constitutional and statutory provisions, though Rueben (1996) found that statutory limits were more often nonbinding. As with the BBR data, we constructed our TEL dataset by examining state statutes and constitutions and cross-referencing a variety of data sources, including (1) NASBO, *Budget Processes in the States*; (2) Waisanen (2010), "State Tax and Expenditure Limits—2010"; (3) state-specific or other authoritative sources, including Mitchell (2010), Rueben (1996), and Skidmore (1999); (4) direct outreach to state budget staff;[33] and (5) our own review of state statutes and bills, where necessary.

In 2015, 33 states had some sort of tax or expenditure limit in place, whether binding or not (see year of adoption data for TELs in table A.2 in appendix A). However, only 23 of these states had a binding tax or expenditure limit (table 5). The remaining 10 had a nonbinding provision that could be overridden with a simple majority vote. Table 4 provides summary statistics on binding TELs for a sample of years in our 26-year study period.

TABLE 4

Binding Tax and Expenditure Limits in the States

Number and percentage of states with strict tax or expenditure limits (TELs)

Institution	1990	1995	2000	2005	2010	2015
Binding TEL						
# of states	15	18	22	22	22	23
% of states	31	37	45	45	45	47
Binding expenditure limit (only)						
# of states	6	6	6	7	7	7
% of states	12	12	12	14	14	14
Binding revenue limit (only)						
# of states	8	7	11	11	11	12
% of states	16	14	22	22	22	24
Both						
# of states	1	5	5	4	4	4
% of states	2	10	10	8	8	8

Sources: Authors' calculations based on various sources and independent data collection. For years of adoption, see table A.2 in appendix A.

Notes: Five-year intervals excerpted from full 1990–2015 panel. Excludes Alaska. Binding revenue limits include a supermajority to raise new taxes or revenues.

Although TELs, as a general category, include *both* revenue and spending limits and have been treated as interchangeable, we might expect different types of limits to have different effects on budget cuts and tax changes. For example, are states that limit revenues more, or less, likely to implement tax changes as compared with cutting spending? And how does this differ in states with an expenditure limit? Thus, we examined how states with strict revenue or expenditure limits reacted, compared with each other and compared with states without a binding limit.

Binding revenue limit (brevlim) is an indicator variable with a value of 1 when the state has a binding revenue limit. Binding expenditure limit (bexplim) is an indicator variable with a value of 1 when the state has a binding expenditure limit. A value of 0 for either of these variables indicates either no limit or a weak limit that can be overridden with a simple majority vote. In 2015, 12 states had only a binding revenue limit, 7 states had just a binding expenditure limit, and 4 states had both.

State TELs varied more across time and states than did BBRs. Fourteen states had a change in the stringency of either an expenditure or a revenue limit during the study period.[34] States adopted stricter TELs during our study period. In 1990, 31 percent of states had a binding TEL, compared with 47 percent in 2015 (table 4). Notably, other factors that are difficult to measure may influence TELs' stringency. Expenditure or revenue limits, even if constitutional or difficult to override, may be effectively nonbinding if they are set at thresholds the state rarely exceeds. Watkins (2018) reported that Florida's expenditure limit, for example, is not extremely binding in practice because the state rarely hits the upper threshold of spending growth required for the limit to kick in.

Estimating Effects

To examine the effects of strict revenue and expenditure limits, we estimated (equation 3):

$$\left.\begin{matrix}\textbf{budcut}\\ \textbf{revch}\\ \textbf{revnxt}\end{matrix}\right| = a + b_1{*}surplus + b_2{*}deficit + b_3{*}brevlim + b_4{*}bexplim + b_5{*}brevlim{*}surplus + b_6{*}brevlim{*}deficit + b_7{*}bexplim{*}surplus + b_8{*}bexplim{*}deficit + error$$

We amended our prior equation (1) to include a level variable for both a binding revenue (brevlim) and binding expenditure limit (bexplim), illustrating how states responded if they had one of these provisions in place (coefficients b_3 and b_4, respectively). Again, of main interest were the interaction terms, which measured how states with binding tax or expenditure limits responded to unexpected deficits or surpluses compared with states without binding limits (illustrated by coefficients b_5 and b_6 for binding revenue-limited states and coefficients b_7 and b_8 for binding expenditure-limited states). Table B.3 in appendix B reports results from this third set of regressions, using state random and year fixed effects, for the 1990–2007 and 2008–15 time periods.[35] Findings are discussed below.

Findings

We again find that states had statistically significantly different responses in the periods during and following the Great Recession compared with earlier years. In the earlier period, states with binding revenue limits relied solely on budget cuts, while those with spending limits relied more on tax increases. In contrast, in the later period, states with binding revenue limits closed a larger share of their unexpected gaps and, unlike the earlier period, did so through some tax increases, although states with *both* binding tax and spending limits took the largest actions.

To illustrate, from 2008–15, states with no limits closed about one-quarter of an unexpected deficit with either budget cuts or next-year tax increases. States with binding revenue limits but no

expenditure limit cut budgets midyear by $32 per capita for every $100 of unexpected deficit, compared with only $18 per capita for states with no binding TEL. In fact, budget cuts *alone* in solely revenue-limited states exceeded tax increases and budget cuts combined in states without a binding TEL. States with *both* strict revenue and expenditure limits cut spending the most aggressively (by $36 per capita per $100 of unexpected deficit), and raised taxes significantly as well (by $30 per capita). In contrast, our estimates on the effect of having a binding spending limit were less conclusive and not statistically significant.[36]

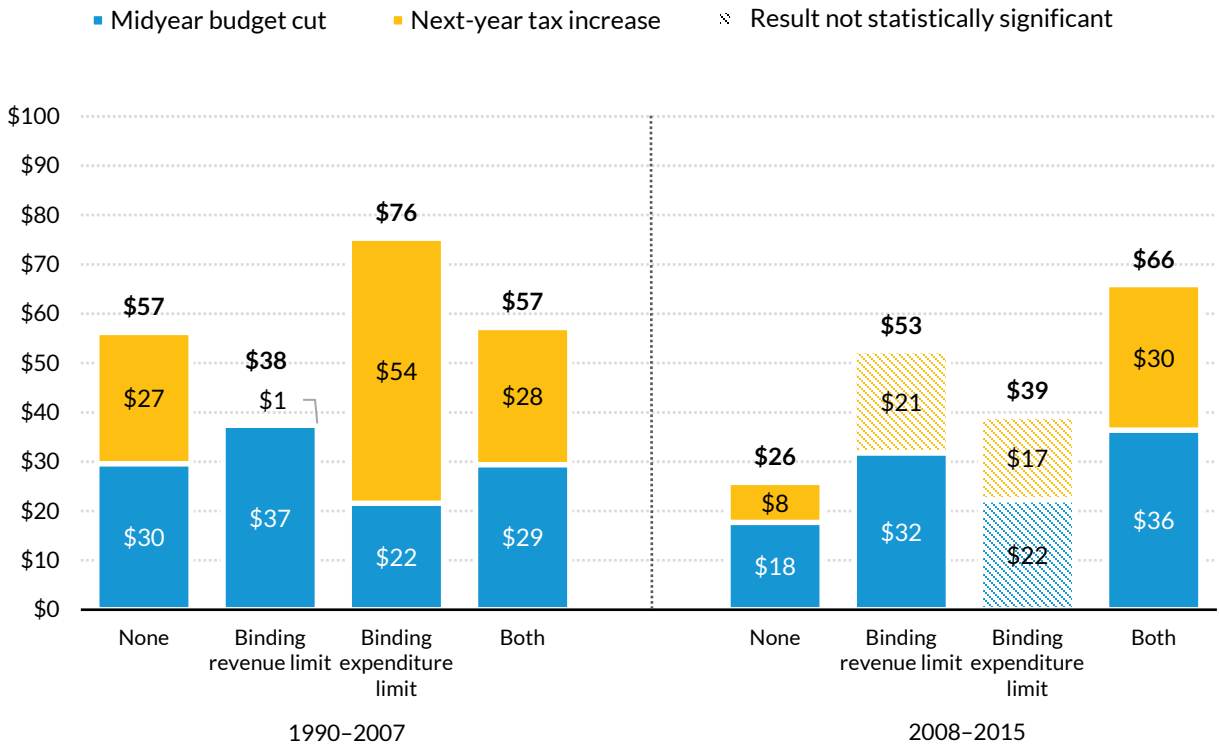
The responses looked different, however, in the period before the Great Recession, for which we have more consistently statistically significant results. Between 1990 and 2007, states with binding expenditure limits both cut spending and raised the following year's taxes. While, overall, states without a binding tax or expenditure limit closed almost 60 percent of an unexpected deficit and did so evenly via budget cuts and tax increases, states with binding TELs responded differently. States with a binding spending limit closed 76 percent of an unexpected deficit largely with tax increases the following year. In contrast, states with a binding revenue limit but no spending limit cut spending by $37 for every $100 of unexpected deficit but did not raise taxes at all. States with both binding revenue and spending limits reacted almost identically to those states with no limit in place at all.

In general, these findings illustrate that binding revenue-limited states relied more on budget cuts than on tax increases in response to deficit shocks, while those with expenditure limits relied more on tax cuts.[37] This highlights how limiting one fiscal option for bridging deficit gaps may encourage the use of alternatives. States with binding revenue limits were more likely to bridge gaps by cutting spending because they may not be permitted to raise additional revenues past the limit or may require voter approval or a supermajority to pass new taxes. In contrast, states with binding expenditure limits might have had less budget growth in the period before the deficit and, because restoring funding may be difficult if spending is cut, may rely more on tax increases.

FIGURE 3

Tax and Expenditure Limits and State Adjustment to Unexpected Deficit

Per $100 of unexpected deficit per capita (before and after the onset of the Great Recession)



Source: Authors' analysis. Original regression coefficients reported in table B.3 in appendix B.
Notes: Results reflect 2015 inflation-adjusted, per capita dollars. Analysis excluded Alaska. Midyear budget cuts represent the absolute value of the cut (i.e., a positive value is equivalent to a cut). Positive next-year revenue values reflect revenue increases. Regression coefficients are statistically significant unless otherwise noted, and time period results are statistically significantly different from one another.

In summation, during and following the Great Recession, states with just binding revenue limits or those with *both* binding revenue and expenditure limits were likely to bridge more of their deficit gaps, suggesting that strict institutions did influence states' responsiveness during this period. However, before the Great Recession, states without binding TELs (as well as states with just expenditure limits) were more likely to bridge a significantly larger portion of their deficit gaps than revenue-limited states. Moreover, solely revenue-limited states tended to rely on budget cuts more than on tax increases, while expenditure-limited states relied more on tax increases. States with both institutions bridged a more even share of their deficit gaps through a combination of spending cuts and revenue increases.

Party Control and Fiscal Shock

The political party in control of the state legislature or governorship can also influence states' decisionmaking during times of fiscal distress. Whether from ideological preferences or responsiveness to voter preference, divided, Democratic-, or Republican-controlled state governments often make different budgeting choices. We included party control and unified government in our analysis to better understand how party preferences drive spending and tax decisions. We sought to understand whether the observable dynamics present for state BBRs and TELs are driven primarily by partisan dynamics, rather than by the institutions themselves. Moreover, while observable party differences could represent different preferences among legislators, they may also highlight voter preferences that are driving both electoral and budgeting decisions.

Examining Unified Party Control and Fiscal Shock

First, we evaluated how either Democratic- or Republican-controlled governments responded to fiscal shocks, compared with each other and with divided-government states (i.e., mixed party control), without accounting for the presence or absence of fiscal institutions.

We excluded Nebraska, in addition to Alaska, from this analysis because its legislative elections are nonpartisan. As with our analyses of BBRs and TELs, we estimated regression equations using two-way interaction terms—this time between fiscal and *political* variables, demonstrating how the political environment influenced states' behavior during times of unexpected surplus or deficit.

Defining Unified Government

We defined unified government as the state senate, house, and governorship being under control of the same party. If any of these bodies were controlled by different parties, then we classified the state as having a divided government.

Unified Republican control (rep) is an indicator variable with a value of 1 when the state house, senate, and governorship are all under Republican control during the legislative year in question. A value of 0 denotes either Democratic or divided control.

Unified Democratic control (dem) is an indicator variable with a value of 1 when the state house, senate, and governorship are all under Democratic control during the legislative year in question. A

value of 0 denotes either Republican or divided control. When a state has a 0 for both unified Republican and Democratic control, it denotes a divided government.

Unified party control, and more specifically unified Republican control, has become more common over time. In 1990, only 8 percent of states were Republican controlled, 33 percent were Democratic controlled, and 58 percent were divided (table 5). By 2015, however, 48 percent of states had a unified Republican government, while only 15 percent had a unified Democratic government and 38 percent were divided. Table 5 provides summary statistics on unified and divided party control for a sample of years in our 26-year study period.

TABLE 5

Unified Party Control in the States

Number and percentage of states with unified political control

Party Control	1990	1995	2000	2005	2010	2015
Divided control						
# of states	28	25	24	29	23	18
% of states	58	52	50	60	48	38
Unified Republican control						
# of states	4	15	15	11	9	23
% of states	8	31	31	23	19	48
Unified Democratic control						
# of states	16	8	9	8	16	7
% of states	33	17	19	17	33	15

Source: Authors' calculations using data from Carl Klarner, "State Partisan Balance Data, 1936–2016," Harvard Dataverse, Harvard University, 2018, https://hdl.handle.net/1902.1/20403.

Notes: Five-year intervals excerpted from full 1990–2015 panel. Analysis excluded Alaska and Nebraska. Unified government indicates where the house, senate, and governorship are all controlled by the same political party. Divided government indicates the absence of unified control by any one party.

Estimating Effects

To examine the effect of unified party control for each party, we first estimated (equation 4a):

$$\left.\begin{matrix}\textbf{budcut}\\ \textbf{revch}\\ \textbf{revnxt}\end{matrix}\right| = a + b_1{*}surplus + b_2{*}deficit + b_3{*}dem + b_4{*}rep + b_5{*}dem{*}surplus + b_6{*}dem{*}deficit + b_7{*}rep{*}surplus + b_8{*}rep{*}deficit + error$$

Where b_3 is the coefficient on unified Democratic control and b_4 is the coefficient on unified Republican control. The interaction-term coefficients (b_5, b_6, b_7, b_8) represent how much more states cut budgets, or either raised or cut taxes, per every dollar of deficit (or surplus), when under either Democratic or Republic control, compared with states with divided government.

As with period results for BBRs and TELs, we performed tests of joint statistical significance to determine whether coefficients for 1990–2007 and 2008–15 were statistically significantly different from one another. The results were not significant at or below the 0.10 level.[38] Thus, state preferences or actions by party do not appear to have changed over the full study period. However, recall that the incidence of Republican control increased significantly over time. Lack of statistical significance between the two periods could indicate either that the relationship had not changed or that we were simply unable to distinguish those changes given the limited number of states with Republican control in the earlier period.

While we tested whether Democratic-controlled, Republican-controlled, and divided governments behaved differently during deficit shocks, we find little difference between Democratic-controlled and divided government, and results were not statistically significant at or below the 0.10 level. We ran additional regressions that distinguished only between Republican-controlled and non-Republican-controlled states, which produced similar results (see equation 4b results reported in table B.5 in appendix B).

We highlight results from our full study period while reporting results for the 1990–2007 and 2008–15 time periods, as well as for regressions with and without indicators for Democratic control, in tables B.4 and B.5, respectively, in appendix B.[39]

Findings

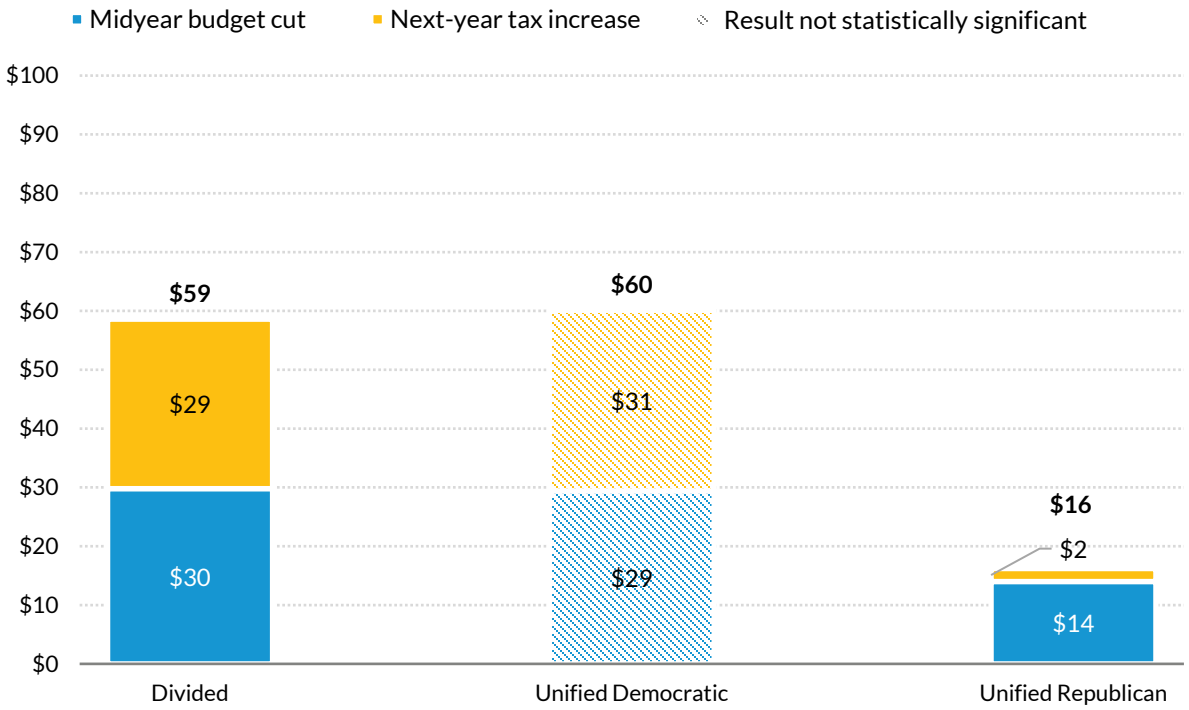
We find that unified Republican governments bridged less of their deficit gaps than divided or Democratic-controlled governments, and did so almost entirely through budget cuts. They also raised taxes less than divided and Democratic-controlled governments in response to unexpected deficits. Moreover, we do find evidence that Republican-controlled governments relied even less on tax increases in the second period, though results were not statistically different across the periods. We do not find statistically significant differences between the actions of Democratic-controlled governments and those of divided governments.

From 1990–2015, divided governments bridged 59 percent of their deficit gaps through an even combination of midyear budget cuts and next-year tax raises (figure 4). Republican-controlled governments, by comparison, bridged only 16 percent of their deficit gaps. They cut budgets by $14 per capita per $100 of unexpected deficit and raised next year's taxes by only $2 per capita.

FIGURE 4

Party Control and State Adjustment to Unexpected Deficit

Per $100 of unexpected deficit per capita (1990–2015)



Source: Authors' analysis. Original regression coefficients reported in table B.4 in appendix B.

Notes: Results reflect 2015 inflation-adjusted, per capita dollars. Analysis excluded Alaska and Nebraska. Midyear budget cuts represent the absolute value of the cut (i.e., a positive value is equivalent to a cut). Positive next-year revenue values reflect revenue increases. Regression coefficients are statistically significant unless otherwise noted.

While we could not rule out the possibility that party responses to deficit shocks were consistent before and following the onset of the Great Recession, period estimates did suggest a possible difference, which future research may decide to explore.[40] In the estimates for 1990–2007, for example, we again observed that Republican-controlled governments closed less of their budget gaps than other states ($33 per $100 of deficit compared with $61 for divided-government states; table B.4 in appendix B). During this period, Republican-controlled states shrank budget gaps evenly through tax increases and budget cuts. This is compared with the 2008–15 period, when they relied exclusively on budget cuts. Comparing Republican- with Democratic-controlled states during the later period is more difficult because Democratic-controlled states' 2008–15 net adjustment was not statistically significantly different from divided-government states', and the two are thus potentially equivalent. However, though not statistically significantly different from one another, estimates for both Democratic-controlled and divided governments from 2008 to 2015 showed a similarly even split between next-year revenue increases and midyear budget cuts.

Examining Unified Party Control and Budget Processes

Our basic finding from prior estimations (i.e., that the effect of fiscal institutions on state budget actions has changed following the onset of the Great Recession) may in fact be attributable to changes in party control over time. Thus, we also explore whether any observable patterns in states' responses to unexpected deficits were attributable to party dynamics and if states' relationships with BBRs and TELs remained constant once we controlled for political party. To evaluate these effects, we incorporated three-way institutional, political, and fiscal interactions into our model. These interaction terms allowed us to examine whether the previously discussed effects of BBRs and TELs still held *within Republican-controlled or non-Republican-controlled states.* This is especially important given that (as demonstrated in table 5) over time, an increasing number of states were under unified Republican control, so Republican governments' response could have driven the changing relationship we saw across fiscal institutions over time.

Estimating Effects

To examine the effect of fiscal institutions (weak BBRs or binding tax and spending limits) and party control, we estimated (equations 5 and 6 for BBRs and TELs as inst, respectively):

$$\left.\begin{matrix}\textbf{budcut}\\ \textbf{revch}\\ \textbf{revnxt}\end{matrix}\right| = b + a_1{*}surplus + a_2{*}deficit + a_3{*}inst + a_4{*}rep + a_5{*}inst{*}surplus + a_6{*}inst{*}deficit + a_7{*}rep{*}surplus + a_8{*}rep{*}deficit + a_9{*}(rep\ {*}inst){*}surplus + a_{10}{*}(rep{*}inst\ ){*}deficit + error$$

In addition to level variables and two-way interaction terms, we incorporated three-way interaction terms among institutions (i.e., wbbr or tax and spending limits), Republican control, and fiscal shock. The above model includes only the Republican interaction terms and combines divided and Democratic-controlled governments into our base for comparison purposes. Building on our earlier findings on TELs, we evaluated the effect of spending and revenue limits separately but focused on whether a state is Republican controlled or not.

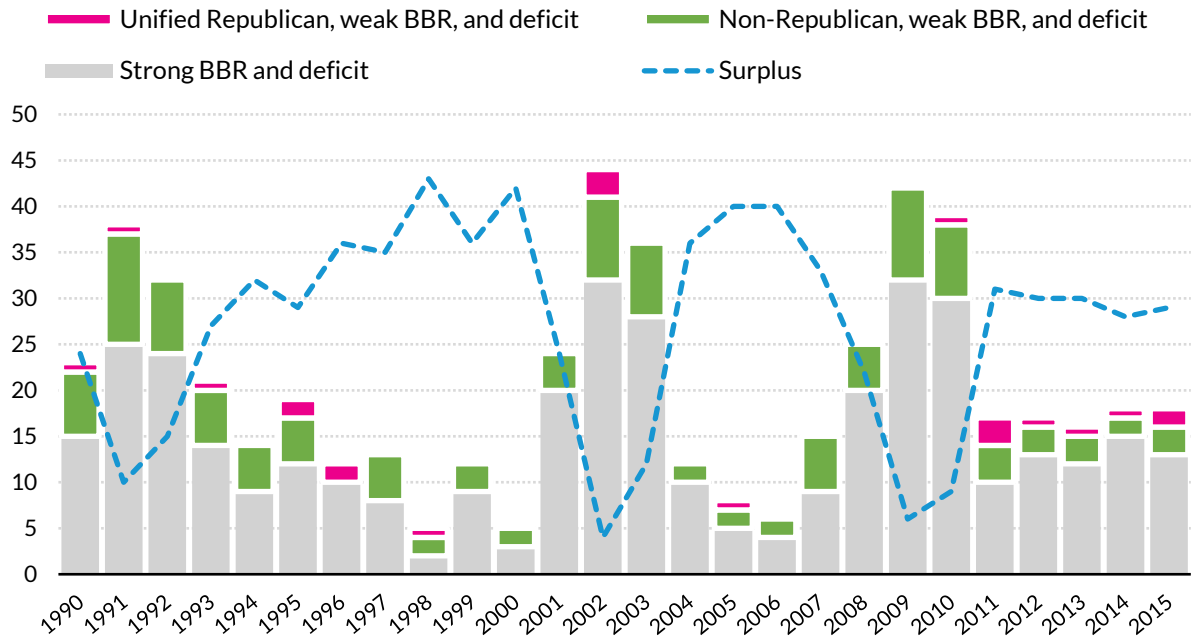
For example, the coefficient a_{10} (associated with term a_{10}*(rep*inst)*deficit) represents the amount by which states cut their budgets or raised taxes when they had a unified Republican-controlled government, a weak BBR (for inst), and a deficit shock, *compared with Republican-controlled governments with a deficit and a strong BBR.* For TELs, we incorporated separate indicators for binding tax and expenditure limits as we did in earlier regressions.

One challenge in working with three-way interaction terms is diminished levels of statistical significance owing to fewer observations falling into each subcategory. As figure 5 demonstrates, each set of interaction terms applies to an increasingly smaller number of observations. Of the states that had a deficit (of primary interest to us), a minority also had a weak BBR. And, of those with a weak BBR, an even smaller subset was Republican controlled. In some years, there were no states with a weak BBR, a deficit, and unified Republican government (i.e., 1992, 1994, 1997, 1999–2001, 2003–04, and 2006–09). In other years, only one state (New Hampshire) met these criteria, and in only five years did two or three states meet them (i.e., 1995, 1996, 2002, 2011, 2015).

FIGURE 5

Republican Party Control, State Deficits, and Balanced Budget Requirements

Number of states with a deficit, weak balanced budget requirement (BBR), and unified Republican government



Source: Authors' analysis using data from the National Association of State Budget Officers, *Fiscal Survey of States*, 1990–2016, https://www.nasbo.org/mainsite/reports-data/fiscal-survey-of-states/fiscal-survey-archives; Carl Klarner, "State Partisan Balance Data, 1936–2016," Harvard Dataverse, Harvard University, 2018, https://hdl.handle.net/1902.1/20403; and compiled miscellaneous sources on state BBRs.

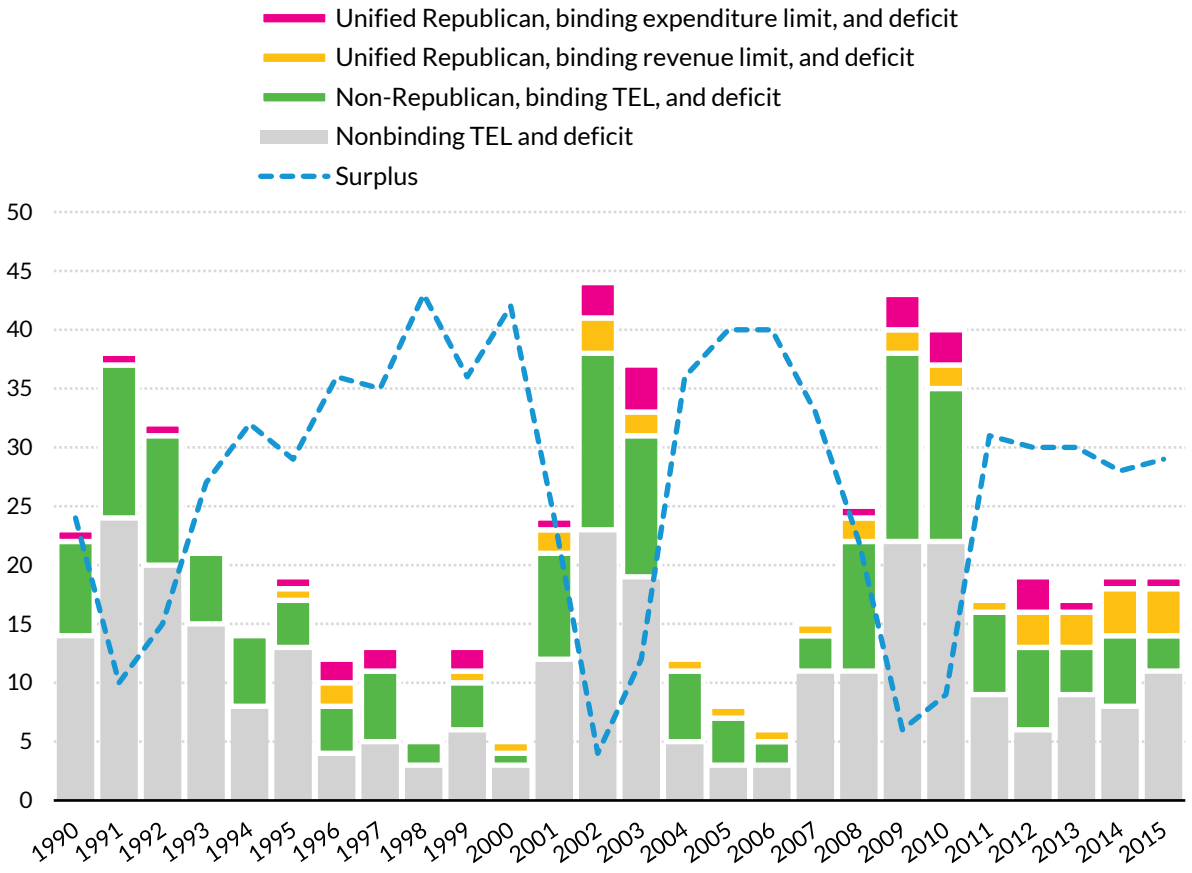
Notes: Excludes Alaska and Nebraska. A surplus shock occurs when unexpected revenues exceed expenditures, while deficit shock occurs when unexpected expenditures exceed revenues. Unified government counts indicate that the house, senate, and governorship are all controlled by the same political party. Divided government indicates the absence of unified control by any one party.

Figure 6 illustrates the small share of states that had a deficit shock, a binding tax or expenditure limit, and unified Republican government. While shares fluctuated over time, the percentage of states that met three-way interaction-term requirements was small compared with our full set of observations.

FIGURE 6

Republican Party Control, State Deficits, and Tax and Expenditure Limits

Number of states with a deficit, binding tax or expenditure limit (TEL), and unified government



Source: Authors' analysis using data from the National Association of State Budget Officers, *Fiscal Survey of States*, 1990–2016, https://www.nasbo.org/mainsite/reports-data/fiscal-survey-of-states/fiscal-survey-archives; Carl Klarner, "State Partisan Balance Data, 1936–2016," Harvard Dataverse, Harvard University, 2018, https://hdl.handle.net/1902.1/20403; and compiled miscellaneous sources on state TELs.

Notes: Excludes Alaska and Nebraska. Unified government counts indicate where the house, senate, and governorship are all controlled by the same political party. Divided government indicates the absence of unified control by any one party.

As with period results for BBRs and TELs, we performed tests of joint statistical significance to determine whether period results for 1990–2007 and 2008–15 were different from one another. Period results on party control, fiscal institutions, and fiscal shock were generally not statistically significantly different from one another.[41] We can't make inferences, therefore, about whether results after the onset of the Great Recession differed from the earlier period. We report and discuss results from our full study period which, nonetheless, provide important insights into the role of party control in state response to deficit shock and strengthen previously reported results. Tables B.6 and B.7 in appendix B report results from this set of regressions, using state random and year fixed effects, for each of our three sample periods.[42]

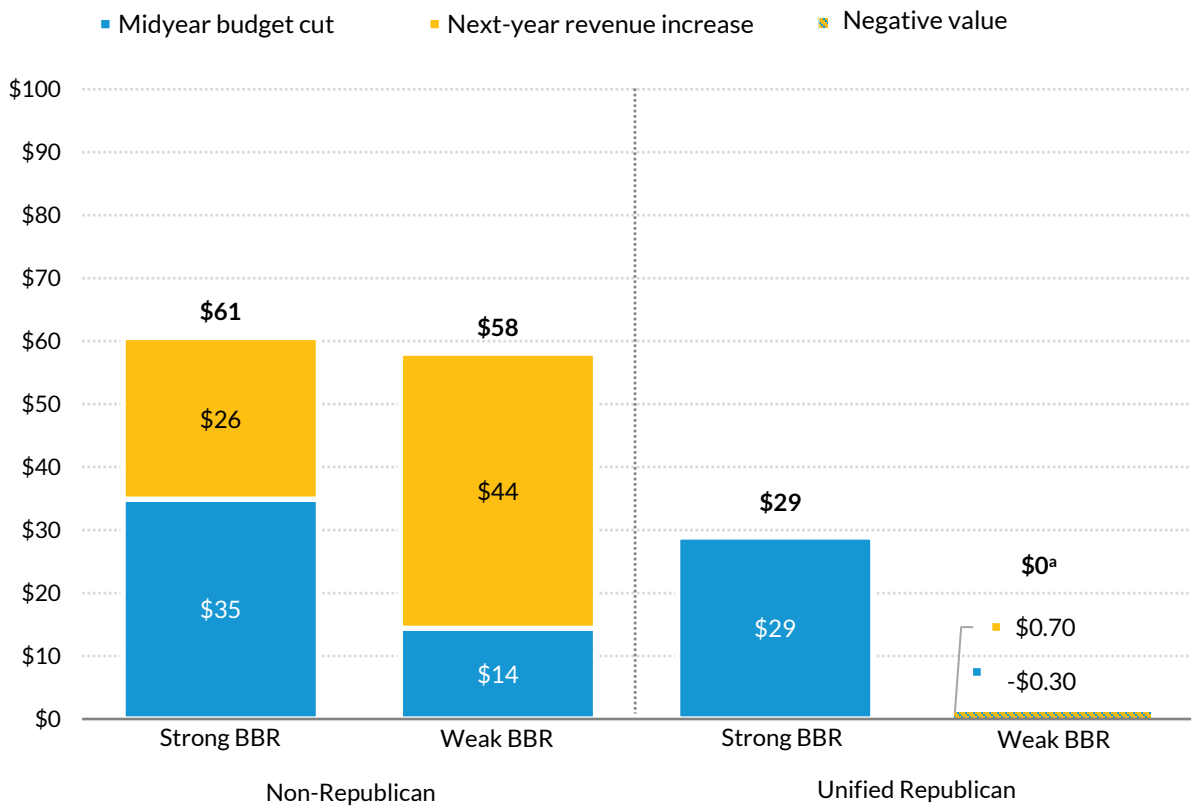
Findings

The results presented in figures 7 and 8 (on BBRs and TELs, respectively) largely reflect findings from previous regression results, with some exceptions. Consistent with findings on unified party control and fiscal shock, Republican governments closed less of their deficit gaps than either unified Democratic-controlled or divided governments, and relied more on budget cuts than on tax increases to do so. Between 1990 and 2015, states with either divided or Democratic-controlled governments closed about 60 percent of an unexpected deficit (whether with a strong or a weak BBR), while Republican-controlled states closed a maximum of 30 percent of their deficit gaps (figure 7).[43] Moreover, states with divided or Democratic-controlled governments raised taxes more aggressively than Republican-controlled states. Republican states closed their deficit gaps exclusively through spending cuts.

FIGURE 7

Balanced Budget Requirements, Republican Party Control, and State Adjustments to Fiscal Shock

Per $100 of unexpected deficit per capita (1990–2015)



Source: Authors' analysis. Original regression coefficients reported in table B.6 in appendix B.

Notes: Results reflect 2015 inflation-adjusted, per capita dollars. Analysis excluded Alaska and Nebraska. Midyear budget cuts represent the absolute value of the cut (i.e., a positive value is equivalent to a cut). Positive next-year revenue values reflect revenue increases. Regression coefficients are statistically significant unless otherwise noted.

[a] Value is not statistically significant.

Fiscal institutions influenced states' behavior, after incorporating party control, in a similar manner as previously estimated. Strong-BBR states with either a divided or a Democratic-controlled government had a similar net adjustment to deficit shock to their counterparts with weak BBRs. This is similar to results from 1990 to 2007, suggesting that results from the earlier period may be driven largely by the more frequent incidence of divided or Democratic governments. These strong-BBR states closed their gaps through a more even combination of budget cuts and revenue increases, with a slight preference for budget cuts, while weak-BBR states relied heavily on next-year revenue increases (raising revenues by $44 per capita per every dollar of unexpected deficit, compared with only $14 in spending cuts).

Results for Republican-controlled states are more difficult to interpret, as Republican-controlled states with weak BBRs were not statistically significantly different from Republican-controlled states with strong BBRs. This is likely because of the limited number of Republican governments with weak BBRs in most years, thus a lack of precision in estimating these effects. Our estimates, though not statistically significant, do show Republican-controlled governments with weak BBRs closing none of their deficit shocks, compared with a $30 per capita adjustment among their counterparts with strong BBRs. These estimates are similar to those from the 2008–15 period for BBRs and fiscal shock, suggesting that latter-period results may be driven by a higher incidence of unified Republican government.

In summation, strong BBRs do, as a rule, exert influence over states' responsiveness to deficit shocks, especially regarding the composition of states' responses. For non-Republican governments, states with weak BBRs raised revenues more and cut taxes and spending less than those with strong BBRs.

Interacting the party control and TEL variables produced results similar to our previous findings. Under divided and Democratic-controlled governments, states with neither a binding revenue limit nor an expenditure limit closed $55 per capita of every $100 of unexpected deficit ($30 in budget cuts and $25 in following-year revenue increases; figure 8). Their counterparts with a binding spending limit but no revenue limit closed even more of their unexpected deficits and relied more on tax increases than on spending cuts. To illustrate, non-Republican states with spending limits but no revenue limit closed 73 percent of their deficit gaps, through $22 per capita in budget cuts and $51 per capita in next-year revenue increases per $100 of unexpected deficit. Non-Republican states with binding revenue limits but no spending limit, conversely, closed only 47 percent of their deficit gaps and did so primarily through spending cuts ($40 per capita in spending cuts compared with $7 in revenue increases). Non-

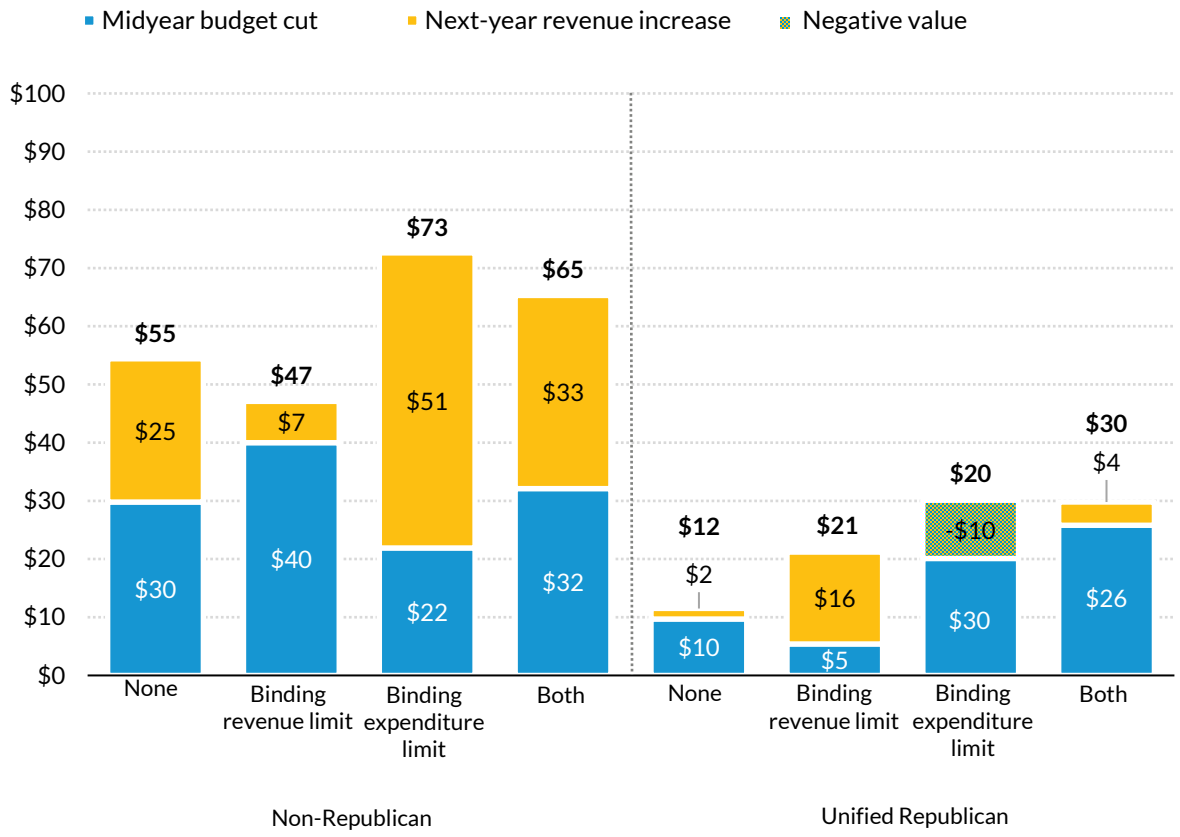
Republican states with both binding revenue and spending limits closed 65 percent of their deficit gaps, relying equally on spending cuts and tax increases.

Consistent with previous findings, Republican-controlled states closed much less of any unexpected deficit (regardless of the presence of a TEL) compared with states with divided or Democratic-controlled governments. Republican-controlled states closed 30 percent of any shortfall at most, with states that had both binding spending and revenue limits making the most gains. Republican-controlled states with neither a binding revenue limit nor a spending limit closed only 12 percent of any unexpected deficits. Republican states with either a binding revenue limit or a spending limit did close more of their deficit gaps than their counterparts with neither limit ($21 per capita for binding revenue-limited states and $20 for binding expenditure-limited states per $100 of unexpected deficit).

FIGURE 8

Tax and Expenditure Limits, Republican Party Control, and State Adjustments to Fiscal Shock

Per $100 of unexpected deficit per capita (1990–2015)



Source: Authors' analysis. Original regression coefficients reported in table B.7 in appendix B.
Note: Results reflect 2015 inflation-adjusted, per capita dollars. Analysis excluded Alaska. Midyear budget cuts represent the absolute value of the cuts (a positive value is equivalent to a cut). Positive next-year revenue values reflect revenue increases. Regression coefficients are statistically significant unless otherwise noted.

The composition of Republican states' responses to deficit shocks differed from prior findings on revenue and expenditure limits. Republican-controlled states with spending limits but no revenue limit largely cut spending to bridge a deficit gap. Moreover, these states actually *cut* taxes when faced with a shortfall, leading to an even smaller net adjustment. In contrast, Republican-controlled states with revenue limits but no expenditure limit primarily raised taxes to close deficits ($16 per capita in next-year tax increases compared with $5 in budget cuts per $100 of unexpected deficit).

In divided or Democratic-controlled states, tax or spending limits appear to encourage the use of the nonlimited instrument to bridge deficit gaps. This may reflect a limited ability to make adjustments due to constrained spending (if the state had prior spending limits) or to raise taxes (if this required a supermajority). Interestingly, Republican-controlled states appear to rely more on the limited instrument. In expenditure-limited states, Republican-controlled governments primarily cut spending. Republican-controlled, expenditure-limited states also seem more likely to experience unexpected deficits even after the economy recovered. For revenue-limited Republican states, the heavier reliance on tax increases may reflect the need for such states to reverse previous tax cuts, which states are more likely to enact when economic conditions begin improving than when faced with a shortfall.

In summation, TELs do exert influence on states' budgeting choices. For divided or Democratic-controlled governments, binding revenue limits led to spending cuts while spending limits led to tax increases, in a pattern similar to what we found before. For Republican governments, binding tax and spending limits encouraged greater responsiveness to deficit shocks. However, the composition of Republican states' responses differed—with binding revenue limits resulting in a greater reliance on revenue increases and spending limits resulting in a greater reliance on budget cuts.

As with the BBR findings, differences between parties' responsiveness in the presence of fiscal institutions largely track period findings from prior sections of the report. Given the increasing incidence of Republican-unified control over our study period, it appears that early-period results may have largely been driven by the higher incidence of Democratic- and divided governments, reflecting how TELs influenced behavior for this subset of states. In later years, results in part reflected the behavior of Republican-controlled governments in the presence or absence of institutions.

Conclusion

The results in this report suggest that budget processes and political factors still matter for short-run deficit dynamics. Our results are similar to those found by prior authors, beginning with Poterba (1994). However, we also find that some relationships changed during and following the Great Recession.

In general, states' response to deficits tempered during and following the Great Recession. This could reflect either federal support that lessened the spending burden on states or padding from state reserve funds. It also may reflect the interaction between budgeting decisions and shifts in party control. By electing more Republican-controlled governments, voters could be signifying a shifting preference for less budget balancing and fewer tax increases to balance state budgets. Regardless of the reason, however, this finding illustrates that states are pushing decisionmaking about necessary cuts or revenue increases into the future. Additionally, states continuing to experience unexpected deficits, even while the economy was improving, suggests that states were perhaps too optimistic in their forecasting or initial budgeting. Consistently delaying responses to deficit shocks could have negative consequences for states' fiscal planning. Going forward, states should consider how they can either avoid unnecessary surprise deficits via more sophisticated forecasting methods or ensure that fiscal padding from rainy day funds is available to respond to deficit shocks promptly.

We also find that budget institutions affected both the size and the composition of states' responses to deficits. This was true both before and following the onset of the Recession, although some relationships were more pronounced during the latter period. States with relatively strong BBRs, for example, cut their budgets more than states with weaker rules. This was true in the period before the Recession but became more pronounced from 2008 to 2015. Additionally, in more recent years, states with strong BBRs bridged less of their gaps via revenue increases than they did in years prior. This, in part, illustrates an increasing reluctance to raise taxes when faced with unexpected shortfalls, forcing states to offset their gaps by cutting spending instead. It may also reflect underlying changes in party control, as the number of unified Republican governments (tending to favor budget cuts over tax increases) increased between 1990 and 2015.

Similarly, we find that, during and following the Great Recession, strict revenue-limited states closed more of their unexpected deficit gaps than states without a binding revenue limit. States with *both* strict expenditure and revenue limits closed the largest share of their deficits during and following the Recession with a fairly even balance of budget cuts and revenue increases. Revenue-limited states

did not have this same edge before the Great Recession, however, when states with expenditure limits (or expenditure and revenue limits combined) actually bridged the largest share of their deficit gaps.

In pre–Great Recession years, states with binding expenditure limits not only closed more of their deficit gaps than other states, but they did so primarily by raising revenues. By comparison, both before and following the onset of the Recession, states with strict revenue caps bridged a larger share of their deficits via midyear budget cuts than via revenue increases. This finding illustrates how states may have more flexibility in the side of the budget that is not restricted. For example, a binding revenue limit requires that the state obtain voter approval or a supermajority vote of the legislature to raise taxes. This can be a higher hurdle to overcome than simply cutting spending, which requires only a simple majority vote of the legislature. Similarly, states with expenditure limits might already have less slack in their budgets from prior limits on spending growth and therefore be less able to cut spending. Spending-limited states may also recognize that, depending on the mechanism for limiting growth, restoring spending to pre–Great Recession levels could be difficult. This encourages these states to make up more of their gaps via revenue increases.

When examining party control, we find that from 1990 to 2015, Republican-controlled governments closed less of their deficit gaps than Democratic-controlled or divided governments, and they relied more on budget cuts than on revenue increases to do so. This is not totally surprising given the political rhetoric on fiscal discipline widely debated between the two parties. It may be easier to act, moreover, if some responsibility for unpopular cuts or tax increases can be shared under divided government.

Findings on the interactions between party control and budget processes from 1990 to 2015 reflect many of the findings above while also illustrating the likely influence of party control on pre– and post–Great Recession results. For both BBRs and TELs, the influence of institutions before and following the onset of the Great Recession largely tracks the influence of institutions among either non-Republican or Republican governments. This suggests that post-2008 results are driven, at least in part, by the behavior of (and influence of institutions on) unified Republican governments. These findings illustrate one possible explanation for pre- and post-Recession effects. Moreover, in order to best tailor policy to the unique needs of each state, it is important to understand how the effects of fiscal institutions differed under divided, Democratic-, or Republican-controlled governments.

Our findings, while demonstrating institutions' ability to encourage greater fiscal responsiveness, also illustrate how institutions demand states cut budgets and raise revenues at the very time when the economy and residents would benefit from states spending more and easing taxes. Future research

should explore how investing in rainy day funds, pairing complementary institutions with one another, or expanding the role of the federal government and stimulus funding can help mitigate some possible negative side effects of stricter institutions during times of recession.

Moreover, knowing that states generally rely more on budget cuts than on revenue increases to plug gaps during the year and that strict fiscal controls prevent them from pushing deficits off into future fiscal years, the federal government should consider the consequences of further devolving spending and administration of social programs to the states. Unlike states, the federal government can spend more during economic downturns and so can more easily help states avoid large cuts or tax increases when their economies are contracting. Both states and the federal government should consider policy options that help states avoid deep cuts to vital state services like education and public infrastructure during recessions, when spending contractions could amplify and lengthen recessions. In future research, we hope to further examine the role of rainy day funds and federal assistance and to investigate recent shifts in budget forecasting practices.

Appendix A. Years of Adoption

Tables A.1 and A.2 present years of adoption for strict BBRs and TELs. For BBRs, we report year of first adoption (whether for a weak or a strict BBR), as well as years of adoption for specific provisions that we used to define strong BBRs. For TELs, we report year of first adoption (whether for a weak or a binding TEL), as well as years of adoption for binding TEL, binding revenue limit, binding expenditure limit, and supermajority to raise taxes or revenues, separately. In our analysis, we classified states requiring a supermajority to raise revenues as strict-revenue-limited states.

For a more detailed description of how we defined BBR and TEL strictness, see the Budget Process and Fiscal Shock section of this report.

TABLE A.1

Year of State Balanced Budget Requirement Adoption

Years of adoption for critical BBR provisions

State	Year of first adoption[a]	Legislature must pass[b]	Governor must sign[c]	Controls on supplementary appropriations[d]	Within-year fiscal controls[e]	Deficit carryover prohibited[f]
AL	1932	1976	—	—	1932	1932
AZ	1911	1911	—	2003	1966	—
AR	1955	1973	—	—	1955	—
CA	1922	1983	2004	—	1983	—
CO	1876	1876	—	—	1941	—
CT	1949	1992	—	1949	1992	—
DE	1897	1931	—	1897	—	—
FL	1968	1968	—	—	1969	—
GA	1877	1962	—	1877	—	—
HI	1970	1978	—	1978	1970	—
ID	1889	1889	—	—	1981	—
IL	1970	1970	—	1987	—	—
IN	1851	—	—	—	—	—
IA	1846	—	—	—	—	—
KS	1859	1859	—	—	1953	—
KY	1891	1891	—	1994	1983	—
LA	1974	1974	—	—	1989	—
ME	1848	—	—	—	—	—
MD	1974	1974	—	1974	—	—
MA[g]	1941	1941	1941	1941	1941	—
MI	1963	1963	—	1963	1963	—
MN	1939	—	—	—	1973	1962
MS	1890	1984	—	—	1966	1966
MO	1875	—	—	—	1945	—
MT	1889	1889	—	1961	1991	1919
NE	1875	—	—	—	1974	—
NV	1864	1864	—	1949	—	—
NH	1942	1998	—	pre-1990[h]	1990	—
NJ	1844	1844	—	1944	1944	—

State	Year of first adoption[a]	Legislature must pass[b]	Governor must sign[c]	Controls on supplementary appropriations[d]	Within-year fiscal controls[e]	Deficit carryover prohibited[f]
NM	1911	—	—	—	—	—
NY	1894	1992[i]	—	—	—	—
NC	1929	1969	—	—	1969	1929–2006
ND	—	—	—	—	—	—
OH	1851	1976	—	—	1991	—
OK[j]	1941	1941	—	1988	1941	—
OR	1859	1859	—	—	pre-1990[h]	—
PA	1968	1968	—	—	—	—
RI	1935	1935	—	1990	1991	—
SC	1932	1985	—	—	1986	1932
SD	1889	1889	—	—	1933	—
TN	1977	—	—	—	1993	—
TX	1942	1942	—	—	1942	1987
UT	1896	1896	—	—	1987	—
VT	1993	—	—	—	—	—
VA	1971	—	—	—	—	1984
WA	1959	2012	—	—	1959	—
WV	1872	1872	—	1872	—	—
WI	1848	1848	—	1971	1848	—
WY	—	—	—	—	—	—

Source: Authors' analysis based on various sources and independent data collection. Key sources include Hou and Smith (2006), upon which this classification framework is based; Smith and Hou (2013); National Association of State Budget Officers, *Budget Process in the States*, 1975–2015, https://www.nasbo.org/reports-data/budget-processes-in-the-states; Snell (2010); and correspondence with budget officers in various states.

Note: Excludes Alaska. — = not applicable.

[a] Year of first adoption may refer to either a weak or a strong BBR, whichever the state may have adopted first.

[b] Political rule 4, Hou and Smith (2006).

[c] Political rule 6, Hou and Smith (2006).

[d] Technical rule 7, Hou and Smith (2006).

[e] Technical rule 8, Hou and Smith (2006).

[f] Technical rule 9, Hou and Smith (2006).

[g] Smith and Hou (2013) report Massachusetts' first date of statutory adoption as 1941. We deferred to this date as we were unable to confirm year-of-adoption for relevant statutes from other primary or secondary sources.

[h] Original year of adoption not confirmed, but before beginning of study period (1990–2015).

[i] Smith and Hou (2013) report New York's' first date of statutory adoption as 1992. We deferred to this date as we were unable to confirm year-of-adoption for relevant statutes from other primary or secondary sources.

[j] Oklahoma dates of adoption reflect confirmed date of last amendment. Original adoption date may predate amendments, and certainly our study period, but could not be confirmed.

TABLE A.2

Year of State Tax and Expenditure Limit Adoption

Years of adoption for critical TEL provisions

State	Year of first adoption[a]	Binding TEL[b]	Binding expenditure limit[b]	Binding revenue limit[b]	Supermajority to raise revenues
AL	—	—	—	—	—
AZ	1978	1978	1978	—	1992
AR	—	—	—	—	1934
CA	1979	1979	1979	—	1979

State	Year of first adoption[a]	Binding TEL[b]	Binding expenditure limit[b]	Binding revenue limit[b]	Supermajority to raise revenues
CO	1977	1991	1991	1992	—
CT	1991	1991	1991	—	—
DE	1978	—	—	—	1980
FL	1994	1994	—	1994	1971
GA	—	—	—	—	—
HI	1978	1978	1978	—	—
ID	1980	1980	1980	—	—
IL	2011	—	—	—	—
IN	2002	—	—	—	—
IA	1992	—	—	—	—
KS	—	—	—	—	—
KY	—	—	—	—	2000
LA	1979	1993	1993	—	1966
ME	2005	—	—	—	—
MD	—	—	—	—	—
MA	1986	—	—	—	—
MI	1978	1978	—	1978	1994
MN	—	—	—	—	—
MS	1992	—	—	—	1970
MO	1980	1980	—	1980	1996
MT	1981–2005	1981–2005	1981–2005	—	—
NE	—	—	—	—	—
NV	1979	—	—	—	1996
NH	—	—	—	—	—
NJ	1990	1990	1990	—	—
NM	—	—	—	—	—
NY	—	—	—	—	—
NC	1991	—	—	—	—
ND	—	—	—	—	—
OH	2006	2006	2006	—	—
OK	1985	—	—	—	1992
OR	1979	—	—	—	1996
PA	—	—	—	—	—
RI	1992	—	—	—	—
SC	1980	—	—	—	—
SD	—	—	—	—	1996
TN	1978	—	—	—	—
TX	1978	—	—	—	—
UT	1989	1989	1989	—	—
VT	—	—	—	—	—
VA	—	—	—	—	—
WA	1979	1979	1993	1979–1992	1993–2013
WV	—	—	—	—	—
WI	1981	—	—	—	2011
WY	—	—	—	—	—

Source: Authors' analysis based on various sources and independent data collection. Key sources included National Association of State Budget Officers, *Budget Processes in the States*, 1975–2015, https://www.nasbo.org/reports-data/budget-processes-in-the-states; Waisanen (2010); state-specific or other authoritative sources, including Mitchell (2010), Rueben (1996), and Skidmore (1999); and direct outreach to state budget staff.

Note: Excludes Alaska. — = not applicable.

[a] Year of first adoption may refer to either a weak or a binding TEL, whichever the state may have adopted first.

[b] Binding is defined as requiring a vote of the people or a supermajority vote of the legislature to override.

Appendix B. Regression Results

The following tables present full results from our regression analyses. All models report state random and year fixed effects. Standard errors are reported in parentheses. For tests of statistical significance, a plus sign (+) indicates that $p < 0.10$, one asterisk (*) that $p < 0.05$, two (**) that $p < 0.01$, and three (***) that $p < 0.001$. Because we care about both within- and between-unit variation (although within-unit variation is limited), we report the overall R-squared, which is a weighted combination of the within- and between-R-squared estimates in random effects models.

Deficit values are negative (with a maximum of zero), surplus values are positive (with a minimum of zero), budcut values are positive (representing the absolute value of midyear budget cuts, with a minimum of zero), positive values for revch and revnxt reflect revenue increases, and negative values reflect revenue cuts. Our definition of surplus included states with balanced budgets. All analyses excluded Alaska. Tables B.4 through B.7 exclude Nebraska as well.

Please note, we also performed a Wald chi-square test for period results, and we report results that are jointly statistically significant at the 0.10 level or below. Where results were not statistically significantly different between the two time periods, we present results from the full-period regression (1990–2015).

TABLE B.1

State Adjustment to Fiscal Shock

Equation 1, state random effects with year fixed effects

	1990–2015			1990–2007			2008–2015		
	budcut	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**
surplus	-0.016	-0.041***	-0.118***	-0.004	-0.018*	-0.131***	-0.018	-0.075***	-0.069
	(0.013)	(0.008)	(0.026)	(0.014)	(0.009)	(0.033)	(0.026)	(0.013)	(0.043)
deficit	-0.259***	-0.078***	-0.220***	-0.290***	-0.106***	-0.294***	-0.225***	-0.046***	-0.147***
	(0.013)	(0.008)	(0.025)	(0.015)	(0.01)	(0.033)	(0.024)	(0.012)	(0.037)
_cons	10.647*	8.204**	50.278***	8.887*	6.187*	47.530***	56.937***	2.814	3.893
	(4.915)	(2.921)	(9.647)	(4.224)	(2.821)	(9.562)	(7.61)	(3.152)	(10.151)
*r*2 (overall)	0.488	0.174	0.213	0.471	0.201	0.246	0.493	0.171	0.148
N	1274	1274	1274	882	882	882	392	392	392

Source: Authors' analysis.

Note: Prob > chi2 = 0.063 (budcut), 0.000 (revch), 0.006 (revnxt) (Wald chi-square test for 1990–2007 and 2008–2015 comparison).

TABLE B.2

Balanced Budget Requirements and State Adjustment to Fiscal Shock

Equation 2, state random effects with year fixed effects

	1990–2015			1990–2007			2008–2015		
	budcut	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**
surplus	-0.005	-0.036***	-0.170***	0.001	-0.033***	-0.166***	-0.01	-0.040*	-0.144*
	(0.016)	(0.009)	(0.032)	(0.015)	(0.01)	(0.037)	(0.039)	(0.02)	(0.066)
deficit	-0.337***	-0.068***	-0.204***	-0.343***	-0.065***	-0.244***	-0.338***	-0.073***	-0.168***
	(0.014)	(0.009)	(0.029)	(0.017)	(0.011)	(0.038)	(0.028)	(0.015)	(0.047)
weak bbr (wbbr)	2.573	-1.138	-12.021	0.851	-7.350***	-15.843+	6.171	2.057	-12.701
	(3.303)	(1.735)	(7.763)	(3.267)	(1.956)	(8.635)	(6.64)	(3.135)	(12.423)
wbbr*surplus	-0.02	-0.016	0.134**	-0.028	0.070***	0.141+	-0.005	-0.055*	0.140
	(0.025)	(0.015)	(0.052)	(0.032)	(0.021)	(0.074)	(0.05)	(0.026)	(0.087)
wbbr*deficit	0.242***	-0.031*	-0.041	0.183***	-0.147***	-0.181**	0.306***	0.067**	0.078
	(0.024)	(0.015)	(0.048)	(0.029)	(0.019)	(0.066)	(0.043)	(0.023)	(0.071)
_cons	9.702*	8.481**	55.022***	9.044*	8.176**	52.304***	43.455***	-2.504	9.524
	(4.833)	(2.979)	(10.004)	(4.244)	(2.801)	(9.938)	(7.836)	(4.089)	(10.902)
*r*2 (overall)	0.535	0.180	0.214	0.500	0.253	0.252	0.561	0.200	0.155
N	1274	1274	1274	882	882	882	392	392	392

Source: Authors' analysis.

Note: Prob > chi2 = 0.051 (budcut), 0.000 (revch), 0.002 (revnxt) (Wald chi-square test for 1990–2007 and 2008–2015 comparison).

TABLE B.3

Tax and Expenditure Limits and State Adjustment to Fiscal Shock

Equation 3, state random effects with year fixed effects

	1990–2015			1990–2007			2008–2015		
	budcut	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**
surplus	-0.025 (0.016)	-0.053*** (0.009)	-0.114*** (0.031)	-0.013 (0.019)	-0.022+ (0.012)	-0.146*** (0.043)	-0.024 (0.029)	-0.083*** (0.014)	-0.050 (0.048)
deficit	-0.236*** (0.016)	-0.047*** (0.009)	-0.173*** (0.030)	-0.296*** (0.019)	-0.083*** (0.012)	-0.266*** (0.042)	-0.177*** (0.028)	-0.012 (0.014)	-0.081+ (0.044)
binding revlim (brevlim)	-5.918+ (3.469)	0.113 (1.606)	-0.393 (6.949)	-5.214 (3.309)	2.788 (1.851)	1.083 (8.013)	-7.559 (6.974)	-2.192 (3.111)	-9.866 (11.916)
brevlim*surplus	0.043 (0.03)	0.052*** (0.016)	0.022 (0.058)	0.041 (0.029)	0.030+ (0.017)	0.056 (0.066)	0.013 (0.085)	0.043 (0.041)	0.050 (0.137)
brevlim*deficit	-0.118*** (0.026)	0.043** (0.015)	0.089+ (0.050)	-0.078* (0.031)	0.104*** (0.019)	0.259*** (0.068)	-0.141** (0.05)	-0.024 (0.024)	-0.126 (0.078)
binding explim (bexplim)	3.651 (3.984)	-3.304+ (1.870)	-1.857 (7.989)	6.290+ (3.737)	-3.699+ (2.095)	-4.988 (9.126)	-0.986 (8.306)	-0.493 (3.762)	12.686 (14.115)
bexplim*surplus	-0.006 (0.032)	-0.023 (0.018)	-0.065 (0.063)	-0.024 (0.032)	-0.048* (0.020)	-0.026 (0.071)	0.026 (0.085)	0.019 (0.041)	-0.249+ (0.135)
bexplim*deficit	0.015 (0.024)	-0.124*** (0.014)	-0.209*** (0.047)	0.080** (0.029)	-0.145*** (0.018)	-0.271*** (0.063)	-0.046 (0.046)	-0.081*** (0.023)	-0.088 (0.072)
_cons	12.070* (4.975)	9.739*** (2.831)	52.262*** (9.684)	9.320* (4.321)	7.202** (2.694)	48.969*** (9.657)	57.194*** (7.84)	3.02 (3.305)	42.372*** (12.564)
r2 (overall)	0.497	0.248	0.235	0.48	0.311	0.285	0.509	0.219	0.174
N	1274	1274	1274	882	882	882	392	392	392

Source: Authors' analysis.

Note: Prob > chi2 = 0.040 (budcut), 0.000 (revch), 0.000 (revnxt) (Wald chi-square test for 1990–2007 and 2008–2015 comparison).

TABLE B.4

Party Control and State Adjustment to Fiscal Shock

Equation 4a, state random effects with year fixed effects

	1990–2015			1990– 2007			2008–2015		
	budcut	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**
surplus	-0.003	0.003	-0.114**	0.005	0.002	-0.125**	-0.004	-0.008	-0.017
	(0.020)	(0.011)	(0.039)	(0.018)	(0.011)	(0.042)	(0.079)	(0.039)	(0.124)
deficit	-0.298***	-0.108***	-0.288***	-0.292***	-0.120***	-0.316***	-0.313***	-0.088***	-0.277***
	(0.017)	(0.01)	(0.033)	(0.018)	(0.012)	(0.040)	(0.039)	(0.019)	(0.061)
democratic (dem)	2.481	2.806	10.265	6.259+	1.171	5.374	-4.292	7.081+	17.255
	(3.356)	(1.966)	(6.524)	(3.501)	(2.293)	(7.921)	(8.01)	(3.887)	(12.558)
dem*surplus	-0.026	-0.041*	-0.007	-0.034	-0.047*	-0.008	-0.019	-0.025	-0.099
	(0.033)	(0.019)	(0.064)	(0.035)	(0.023)	(0.079)	(0.096)	(0.047)	(0.15)
dem*deficit	0.004	0.006	-0.019	-0.001	0.027	0.044	0.005	-0.013	-0.082
	(0.028)	(0.017)	(0.055)	(0.032)	(0.022)	(0.072)	(0.057)	(0.029)	(0.089)
republican (rep)	3.761	4.963**	-2.519	2.366	2.396	-9.161	2.59	7.379*	3.105
	(3.285)	(1.863)	(6.457)	(3.931)	(2.513)	(9.096)	(7.118)	(3.405)	(11.262)
rep*surplus	-0.014	-0.090***	0.012	-0.012	-0.063**	-0.02	-0.01	-0.091*	-0.034
	(0.028)	(0.016)	(0.054)	(0.034)	(0.022)	(0.078)	(0.085)	(0.042)	(0.134)
rep*deficit	0.158***	0.100***	0.266***	0.121*	0.090*	0.162	0.179***	0.085***	0.283***
	(0.027)	(0.016)	(0.053)	(0.053)	(0.035)	(0.117)	(0.048)	(0.024)	(0.075)
_cons	8.518+	4.516	40.664***	7.283+	5.406+	43.615***	51.178***	-7.851+	-2.761
	(5.046)	(2.969)	(9.855)	(4.411)	(2.923)	(9.998)	(8.749)	(4.297)	(12.455)
*r*2 (overall)	0.505	0.218	0.242	0.472	0.219	0.250	0.523	0.240	0.234
N	1248	1248	1248	864	864	864	384	384	384

Source: Authors' analysis.

Note: Prob > chi2 = 0.841 (budcut), 0.112 (revch), 0.387 (revnxt) (Wald chi-square test for 1990–2007 and 2008–2015 comparison).

TABLE B.5

Republican Party Control and State Adjustment to Fiscal Shock

Equation 4b, state random effects with year fixed effects

	1990– 2015			1990– 2007			2008– 2015		
	budcut	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**
surplus	-0.013	-0.011	-0.119***	-0.005	-0.008	-0.125***	-0.021	-0.020	-0.077
	(0.017)	(0.009)	(0.032)	(0.016)	(0.01)	(0.037)	(0.044)	(0.022)	(0.069)
deficit	-0.296***	-0.107***	-0.294***	-0.293***	-0.113***	-0.304***	-0.310***	-0.094***	-0.309***
	(0.015)	(0.009)	(0.028)	(0.015)	(0.01)	(0.034)	(0.032)	(0.016)	(0.050)
republican (rep)	3.023	4.051*	-5.113	0.861	2.082	-10.058	3.975	4.345	-3.699
	(3.143)	(1.763)	(6.202)	(3.858)	(2.447)	(8.881)	(6.216)	(2.941)	(9.956)
rep*surplus	-0.005	-0.076***	0.016	-0.003	-0.052*	-0.02	0.008	-0.080**	0.022
	(0.026)	(0.015)	(0.050)	(0.033)	(0.022)	(0.075)	(0.054)	(0.026)	(0.086)
rep*deficit	0.157***	0.099***	0.274***	0.120*	0.084*	0.153	0.176***	0.091***	0.315***
	(0.026)	(0.015)	(0.050)	(0.052)	(0.035)	(0.116)	(0.042)	(0.021)	(0.066)
_cons	9.151+	5.087+	44.583***	9.160*	4.820+	44.592***	49.539***	-0.312	5.487
	(4.957)	(2.923)	(9.693)	(4.316)	(2.863)	(9.736)	(8.14)	(3.446)	(11.039)
*r*2 (overall)	0.504	0.215	0.240	0.469	0.214	0.250	0.522	0.225	0.220
N	1248	1248	1248	864	864	864	384	384	384

Source: Authors' analysis.

Note: Prob > chi2 = 0.949 (budcut), 0.577 (revch), 0.568 (revnxt) (Wald chi-square test for 1990–2007 and 2008–2015 comparison).

TABLE B.6

Balanced Budget Requirements, Republican Party Control, and State Adjustment to Fiscal Shock

Equation 5, state random effects with year fixed effects

	1990– 2015			1990– 2007			2008– 2015		
	budcut	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**
surplus	-0.009	-0.023*	-0.160***	0.002	-0.023*	-0.175***	-0.033	-0.022	-0.086
	(0.017)	(0.010)	(0.035)	(0.017)	(0.01)	(0.040)	(0.045)	(0.023)	(0.074)
deficit	-0.349***	-0.083***	-0.257***	-0.346***	-0.066***	-0.243***	-0.359***	-0.103***	-0.304***
	(0.015)	(0.009)	(0.031)	(0.017)	(0.011)	(0.039)	(0.032)	(0.017)	(0.053)
weak bbr (wbbr)	1.402	-7.111***	-20.071*	2.443	-9.274***	-25.845**	1.807	2.498	-5.978
	(4.01)	(2.128)	(8.939)	(3.796)	(2.21)	(10.014)	(9.338)	(4.584)	(16.034)
wbbr*surplus	-0.036	0.083**	0.213*	-0.058	0.097***	0.251*	0.019	0.006	-0.006
	(0.046)	(0.027)	(0.092)	(0.044)	(0.029)	(0.102)	(0.130)	(0.068)	(0.21)
wbbr*deficit	0.205***	-0.113***	-0.180**	0.197***	-0.179***	-0.244***	0.229**	0.069+	-0.020
	(0.030)	(0.018)	(0.061)	(0.031)	(0.020)	(0.071)	(0.076)	(0.040)	(0.124)
republican (rep)	-0.808	3.676+	-0.352	2.341	2.211	-18.371+	-6.058	5.502	14.879
	(3.811)	(2.212)	(7.826)	(4.449)	(2.767)	(10.602)	(7.498)	(3.707)	(12.699)
rep*surplus	0.014	-0.082***	-0.088	-0.018	-0.082**	-0.010	0.084	-0.069	-0.243+
	(0.037)	(0.022)	(0.075)	(0.040)	(0.026)	(0.093)	(0.087)	(0.044)	(0.146)
rep*deficit	0.060+	0.073***	0.257***	0.082	0.064	0.089	0.052	0.097***	0.364***
	(0.034)	(0.021)	(0.068)	(0.061)	(0.040)	(0.139)	(0.052)	(0.027)	(0.085)
rep*wbbr	0.539	4.371	4.047	-6.438	4.011	34.431+	8.201	-9.621	-39.598+
	(6.779)	(3.979)	(13.838)	(8.233)	(5.199)	(19.368)	(14.185)	(7.154)	(23.631)
rep*wbbr*surplus	0.019	-0.056	0.007	0.081	0.014	-0.161	-0.083	-0.007	0.371
	(0.061)	(0.037)	(0.123)	(0.073)	(0.047)	(0.166)	(0.153)	(0.079)	(0.251)
rep*wbbr*deficit	0.087	0.120***	0.173	0.073	0.132+	0.296	0.084	-0.067	-0.044
	(0.054)	(0.033)	(0.107)	(0.109)	(0.071)	(0.247)	(0.100)	(0.052)	(0.162)
_cons	9.782*	6.598*	49.663***	9.219*	7.093*	51.993***	42.730***	-4.679	19.484+
	(4.918)	(2.951)	(10.052)	(4.348)	(2.815)	(10.201)	(8.422)	(4.348)	(11.839)
*r*2 (overall)	0.545	0.241	0.248	0.504	0.288	0.261	0.570	0.238	0.232
N	1248	1248	1248	864	864	864	384	384	384

Source: Authors' analysis.

Note: Prob > chi2 = 0.967 (budcut), 0.000 (revch), 0.489 (revnxt) (Wald chi-square test for 1990–2007 and 2008–2015 comparison).

TABLE B.7

Tax and Expenditure Limits, Republican Party Control, and State Adjustment to Fiscal Shock

Equation 6, state random effects with year fixed effects

	1990–2015			1990–2007			2008–2015		
	budcut	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**	**budcut**	**revch**	**revnxt**
surplus	-0.026	-0.031*	-0.154***	-0.019	-0.026*	-0.157**	-0.039	-0.045	-0.153+
	(0.022)	(0.012)	(0.043)	(0.022)	(0.013)	(0.050)	(0.057)	(0.028)	(0.088)
deficit	-0.298***	-0.076***	-0.246***	-0.305***	-0.087***	-0.266***	-0.302***	-0.047*	-0.224**
	(0.019)	(0.011)	(0.037)	(0.020)	(0.012)	(0.044)	(0.045)	(0.022)	(0.069)
binding revlim (brevlim)	-4.729	2.500	-1.585	-5.709	3.486+	0.109	-3.515	-0.093	-18.525
	(3.916)	(1.845)	(7.612)	(3.763)	(2.041)	(8.745)	(8.495)	(3.984)	(14.426)
brevlim*surplus	0.044	0.034+	0.064	0.051	0.032+	0.068	0.010	0.025	0.252
	(0.034)	(0.018)	(0.065)	(0.032)	(0.018)	(0.072)	(0.109)	(0.052)	(0.172)
brevlim*deficit	-0.102***	0.080***	0.175**	-0.089**	0.120***	0.291***	-0.099+	0.015	-0.031
	(0.028)	(0.016)	(0.054)	(0.032)	(0.020)	(0.070)	(0.058)	(0.028)	(0.089)
binding explim (bexplim)	5.117	-5.978**	-5.053	6.361	-5.419*	-0.388	1.031	-6.884	-6.355
	(4.625)	(2.180)	(8.993)	(4.402)	(2.409)	(10.241)	(9.879)	(4.637)	(16.878)
bexplim*surplus	-0.005	0.010	0.013	-0.022	-0.006	-0.015	0.060	0.060	-0.009
	(0.04)	(0.022)	(0.077)	(0.039)	(0.024)	(0.087)	(0.104)	(0.050)	(0.161)
bexplim*deficit	0.079**	-0.144***	-0.261***	0.103***	-0.163***	-0.312***	0.048	-0.116***	-0.159+
	(0.027)	(0.015)	(0.052)	(0.030)	(0.019)	(0.066)	(0.058)	(0.028)	(0.088)
republican (rep)	3.966	2.191	-10.602	0.424	-0.387	-11.071	6.906	-0.049	-19.531
	(4.009)	(2.183)	(7.760)	(5.092)	(3.039)	(11.330)	(8.191)	(3.876)	(13.459)
rep*surplus	0.009	-0.037*	0.106+	0.013	0.027	0.061	0.018	-0.046	0.173+
	(0.031)	(0.017)	(0.060)	(0.044)	(0.027)	(0.096)	(0.066)	(0.032)	(0.104)
rep*deficit	0.201***	0.076***	0.228***	0.205**	0.041	0.027	0.213***	0.048+	0.228**
	(0.031)	(0.018)	(0.061)	(0.072)	(0.045)	(0.158)	(0.056)	(0.027)	(0.085)
rep*brevlim	0.734	-4.596	5.008	5.380	-4.080	-5.169	-6.666	-1.806	31.488
	(7.206)	(3.852)	(13.954)	(8.833)	(5.203)	(19.738)	(13.850)	(6.579)	(22.554)
rep*brevlim*surplus	-0.040	-0.024	-0.200	-0.093	-0.054	-0.110	0.027	-0.002	-0.535+
	(0.085)	(0.047)	(0.165)	(0.101)	(0.062)	(0.223)	(0.181)	(0.087)	(0.290)
rep*brevlim*deficit	0.145+	-0.128**	-0.315+	0.075	-0.101	-0.223	0.170	-0.064	-0.071
	(0.085)	(0.048)	(0.164)	(0.126)	(0.077)	(0.277)	(0.148)	(0.072)	(0.225)
rep*bexplim	-5.006	14.290***	21.203	-4.666	13.102*	4.683	-0.952	27.215***	67.592*
	(7.645)	(4.081)	(14.806)	(8.635)	(5.109)	(19.290)	(17.101)	(8.163)	(27.742)
rep*bexplim*surplus	-0.002	-0.144***	-0.283*	0.021	-0.168***	-0.126	-0.117	-0.295**	-0.855**
	(0.068)	(0.038)	(0.131)	(0.073)	(0.045)	(0.161)	(0.200)	(0.097)	(0.309)
rep*bexplim*deficit	-0.283***	0.163***	0.379**	-0.344**	0.250***	0.656**	-0.247*	0.161**	0.348*
	(0.060)	(0.034)	(0.116)	(0.113)	(0.070)	(0.248)	(0.101)	(0.050)	(0.154)
_cons	9.659+	6.362*	47.223***	9.813*	5.794*	45.983***	47.117***	2.618	18.321
	(5.072)	(2.844)	(9.819)	(4.452)	(2.730)	(9.886)	(8.852)	(3.764)	(12.584)
r2 (overall)	0.520	0.300	0.274	0.485	0.342	0.299	0.540	0.286	0.258
N	1248	1248	1248	864	864	864	384	384	384

Source: Authors' analysis.

Note: Prob > chi2 = 0.996 (budcut), 0.001 (revch), 0.052 (revnxt) (Wald chi-square test for 1990–2007 and 2008–2015 comparison).

Notes

[1] The Pew Charitable Trusts reported in May 2018 that tax revenues exceeded their pre–Great Recession levels in 34 states at the end of 2017. Tax revenues recovered nationally from states' Recession-driven losses in 2013. See "Fiscal 50: State Trends and Analysis," May 2, 2018, http://www.pewtrusts.org/en/research-and-analysis/data-visualizations/2014/fiscal-50#ind0.

[2] For trends in annual state revenue growth from 1979 to 2018, see the National Association of State Budget Officers, *Fiscal Survey of States*, Spring 2018, https://www.nasbo.org/reports-data/fiscal-survey-of-states. Ten states passed FY18 budgets late according to the National Conference of State Legislatures, "FY 2018 Budget Status," October 31, 2017, http://www.ncsl.org/research/fiscal-policy/fy-2018-budget-status.aspx.

[3] See Rick Rojas, "Connecticut Adopts a Budget after Months of Debate and Delays," *New York Times*, October 31, 2017, https://www.nytimes.com/2017/10/31/nyregion/connecticut-budget.html.

[4] Many early studies on state budget institutions were not as methodologically rigorous as later studies. For example, Gordon (2012) explained that Crain and Miller (1990), while cross-cited in many early literature reviews (e.g., Poterba 1995, 1996a), did not control for endogeneity. Gordon (2012) also explained that the ACIR study (1987) suffered from challenges with endogeneity and featured only cross-sectional data. Knight and Levinson (2000) used the ACIR's BBR index and commented that, while the study only included cross-sectional data, its results have been borne out by later studies.

[5] Bayoumi and Eichengreen (1995) was an early study and did not distinguish between income and employment effects as effectively as Bohn and Inman's study from the same period (1996). See Gordon (2012) for a more in-depth critique of early budget process studies.

[6] See "What Are Tax and Expenditure Limits?" *Briefing Book*, Urban-Brookings Tax Policy Center, 2016, http://www.taxpolicycenter.org/briefing-book/what-are-tax-and-expenditure-limits.

[7] In her literature review on state fiscal institutions, Gordon (2012) discussed Bails (1990) as an example of early TEL research. While widely cited, more recent studies (e.g., McGuire and Rueben 2006; Poterba and Rueben 2001) that employed more robust methods have reached different conclusions. Gordon also cited Kenyon and Benker (1984) as an early study on TELs and the size of government but did not comment on its quality. Shadbegian (1996) is cited in Gordon (2012) and Poterba (1996b) as an early study that found TELs are not binding in states with income growth.

[8] Other frequently cited papers that reference Poterba (1994) include Afonso and Furceri (2010); Coleman (1999); Dosi et al. (2015); Fatás and Mihov (2006); Milesi-Ferretti (2004); Tsebelis (2002); and Wildasin (1999).

[9] Bohn and Inman (1996) discussed Poterba's (1994) strengths as compared with ACIR (1987), Alt and Lowry (1994), and Von Hagen (1991). Gordon (2012) praised Poterba (1994) for its precise fiscal shock measure but pointed out that it only covers a limited number of fiscal years.

[10] For more information on endogeneity in state policy evaluation, see Besley and Case (2000), which also cites Poterba (1994).

[11] See "Consumer Price Index for All Urban Consumers: All Items (CPIAUCSL)" from the Bureau of Labor Statistics, accessed via the St. Louis Federal Reserve Board, https://fred.stlouisfed.org/series/CPIAUCSL; and "Annual Estimates of the Population for the US and States, and for Puerto Rico" from the US Census Bureau, accessed via the St. Louis Federal Reserve Board, https://fred.stlouisfed.org/series/CAPOP.

[12] Alabama, Michigan, New York, and Texas operate on a different fiscal year than most states. We adjusted population and income numbers to reflect each state's fiscal year rather than the calendar year. We converted calendar-year variables to fiscal years per states' respective fiscal-year calendars as follows: (1) A 50 percent split between current and prior calendar-year data for the 45 states in our panel with fiscal years beginning July

1; (2) A 33 to 67 percent split for Texas, whose fiscal year begins September 1; (3) a 75 to 25 percent split for New York, whose fiscal year begins April 1; and (4) a 25 to 75 percent split for Alabama and Michigan, whose fiscal years begin October 1.

[13] Klarner provides datasets in his Harvard-hosted Dataverse (2013, https://hdl.handle.net/1902.1/20403), but we reached out to Klarner separately for his most recent updates. His data are also available through the Michigan State University Institute for Public Policy and Social Research, "Correlates of State Policy Project," 2017, http://ippsr.msu.edu/public-policy/correlates-state-policy.

[14] NASBO reports net midyear budget cuts for FY[xx] in its fall *Fiscal Survey of States* published that year (i.e., the fall FY[xx] survey).

[15] Data reflect total revenue changes, including impacts to general and other state funds that come from personal and corporate income, sales, fuel, tobacco, and alcohol taxes or other fees and sources of revenue. NASBO reports tax changes that apply to the following fiscal year (FY[xx+1]) in the fall survey for FY[xx], and we apply the full value of their estimate to measure next-year tax changes. Some of those changes that apply to the following fiscal year were adopted mid-year and thus also apply to the current fiscal year (FY[xx]). We calculated the portion of FY[xx+1] revenue changes that applied to FY[xx] by prorating their value based on the date of enactment. NASBO reports mid-year revenue changes for FY[xx] in its spring *Fiscal Survey of the States* published the same year (i.e., the spring FY[xx] survey). We added spring and pro-rated fall FY[xx] revenue changes together to arrive at a final mid-year revch value for FY[xx].

[16] See Ben Watkins' discussant response to a preliminary conference version of this study (Watkins 2018).

[17] Authors' analysis based on multiple sources. For more information, please see the Budget Processes and Fiscal Shock section of this report.

[18] NASBO reports final actual revenues for FY[xx] in the fall *Fiscal Survey of States* published the following fiscal year (i.e., the fall FY[xx+1] survey).

[19] NASBO reports final projected revenues for FY[xx] in its fall *Fiscal Survey of States* published the previous fiscal year (i.e., the fall FY[xx-1] survey).

[20] NASBO reports final actual expenditures for FY[xx] in its fall *Fiscal Survey of States* published the following fiscal year (i.e., final FY[xx] revenue data reported in fall FY[xx + 1] survey).

[21] NASBO reports final projected spending for FY[xx] in its fall *Fiscal Survey of States* published the previous fiscal year (i.e., FY[xx] projected spending reported in fall FY[xx − 1] survey).

[22] Arkansas, South Dakota, and Wyoming were the only states that had a fully balanced budget (i.e., fiscal shock = $0) at various years during our study period (but not for all years).

[23] In our analysis, we focused on results from our state random effects models because we have little within-state variation in our key explanatory variables (state fiscal institutions) over time. We also performed state fixed effects regressions but do not report the results of those regressions. We included year fixed effects in all models. In response to a reviewer question, we also estimated regressions of our dependent variables on lagged values of deficits and found that the results for budget cuts were about half as large, and none of the results on tax variables were statistically significantly different from zero. This result is unsurprising given the temporary nature of the deficit shocks.

[24] Among states experiencing a deficit shock between 2008 and 2015, the average deficit shock was $113 per capita compared with $95 per capita among states with deficits over the full study period (1990–2015).

[25] This corresponds to Hou and Smith's (2006, 28) political rule 6 (i.e., "governor must sign a balanced budget"). Only two states have this requirement that we can confirm (California and Massachusetts). Even without

meeting this requirement, however, both states otherwise meet the requirements for a strict BBR classification per our criteria.

[26] This corresponds to Hou and Smith's (2006, 28) technical rule 9 (i.e., "no deficit may be carried over to the next fiscal year [or biennium]").

[27] In Hou and Smith's (2006, 28) framework, these correspond to political rule 4 (i.e., "the legislature must pass a balanced budget"), technical rule 7 (i.e., "controls are in place on supplementary appropriations"), and technical rule 8 (i.e., within fiscal year controls are in place to avoid deficit").

[28] These would correspond to Hou and Smith's (2006, 28) early-stage political rule 1 (i.e., "the governor must submit a balanced budget"), early-stage technical provisions two and three (i.e., "own-source revenue must match [meet or exceed] expenditures" and "own-source revenue and general obligation [or unspecified] debt [or debt in anticipation of revenue] must match [meet or exceed] expenditures," respectively), and technical rule 5, which is considered weak because, though imposing a limit, it implicitly allows deficits to be financed through debt (i.e., "a limit is in place on the amount of debt that may be assumed for the purpose of deficit reduction").

[29] Please note, our classification system defined strong BBRs more narrowly than Poterba (1994) and other prior studies, primarily because of our more sophisticated and narrower definition of the "no carryover" provision. Therefore, fewer states in our study met the standard for strong classification criteria, and we classified more states as weak compared with prior classification frameworks.

[30] Three of these states adopted a legislative requirement to pass a balanced budget during our study period: Connecticut, New Hampshire, and Washington. Two of them (Connecticut and Ohio) passed late-stage within-year fiscal controls. Other states may also have adopted one or more of these provisions during our study period, but such changes did not change their classification as either weak or strong for the purposes of this study. See table A.1 in appendix A for years of adoption.

[31] In our analysis, we focused on results from our state random effects models because we have little within-state variation in our key explanatory variables (state fiscal institutions) over time. We also performed state fixed effects regressions but do not report the results of those regressions. We included year fixed effects in all models.

[32] As discussed in appendix B, we performed a Wald chi-square test to assess the statistical significance of period effects. These results are reported for each set of regressions in appendix B.

[33] Where data on TELs and their year of adoption were conflicting or unavailable, we reached out directly to state budget offices for more clarification.

[34] The following states adopted a binding revenue limit during our study period: Arizona, Colorado, Kentucky, Nevada, Oklahoma, Oregon, South Dakota, and Wisconsin. The following states adopted binding expenditure limits: Colorado, Connecticut, Louisiana, New Jersey, Ohio, and Washington. Montana repealed a binding expenditure limit during the study period, while Washington repealed its binding revenue limit (following a court order).

[35] In our analysis, we focused on results from our state random effects models because we have little within-state variation in our key explanatory variables (state fiscal institutions) over time. We also performed state fixed effects regressions but do not report the results of those regressions. We included year fixed effects in all models.

[36] If we maximize the possible net adjustment of expenditure-limited states, it would be $39 per capita, while if we minimize the possible net adjustment of revenue-limited states, it would be $40 per capita ($32 in statistically significant budget cuts plus $8 in tax raises, assuming the same level as non-TEL states). Thus, these institutions are possibly within $1 of one another, in terms of net adjustment to fiscal shock, making it difficult to draw any definitive conclusions.

[37] We cannot, however, make statistically significant inferences regarding revenue increases for the 2008–2015 period.

[38] We find some of our estimates for within-year revenue changes marginally statistically different across the two time periods, and readers can see the separate period estimates in appendix B.

[39] In our analysis, we focused on results from our state random effects models because we have little within-state variation in our key explanatory variables (state fiscal institutions) over time. We also performed state fixed effects regressions but do not report the results of those regressions. We included year fixed effects in all models.

[40] See Wald chi-square results in appendix B, tables B.4 and B.5.

[41] Period results for equations 5 and 6 were not statistically significantly different from one another except for midyear revch results, which were significant at or below the 0.10 level. While we focused on budget cuts and following-year tax changes, readers can see these results in tables B.6 and B.7 in appendix B.

[42] In our analysis, we focused on results from our state random effects models because we have little within-state variation in our key explanatory variables (state fiscal institutions) over time. We also performed state fixed effects regressions but do not report the results of those regressions. We included year fixed effects in all models.

[43] Results for Republican-controlled states with weak BBRs were not statistically significantly different from Republican-controlled states with strong BBRs.

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