INSTITUTIONS AND FISCAL SUSTAINABILITY

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As budgetary commitments outpace current revenues and long-term liabilities balloon, the fiscal sustainability of state and local governments is a matter of mounting concern. Over the years, these governments have experimented with a wide variety of political and fiscal institutions, ranging from direct democracy to balanced budget rules, with the goal of slowing the growth of government and increasing financial responsibility. This article synthesizes the related empirical literature, summarizing what we know (and don’t know) about the effectiveness of various rules and procedures in promoting fiscal sustainability.

Keywords: state and local public finance, institutions, public expenditure, taxation, deficits, debt

JEL Codes: H10, H20, H70

I. INTRODUCTION

Even as U.S. state and local governments struggle to dig themselves out from the Great Recession, it is clear that their fiscal challenges will not miraculously disappear after the economy recovers. Long before the economic crisis, states and localities were already on an unsustainable trajectory, with budgetary commitments growing at a considerably faster pace than current revenues and long-term liabilities ballooning. This trajectory mirrors a trend that is taking place around the world, from Greece to Iceland to Japan.

With a sense of mounting urgency, policymakers and scholars have increasingly focused attention on the issue of fiscal sustainability. Fiscal sustainability can be defined in a number of ways, but perhaps the most common definition is a government’s long-run capability to consistently meet existing spending commitments with available (i.e., economically and politically feasible) resources. The fiscal sustainability of U.S. state and local governments is threatened by a number of pressures, including not only cyclical pressures, such as the recent economic crisis, but also structural pressures, such as the aging of the population.¹

¹ For an overview of issues surrounding the fiscal sustainability of state and local governments, see Chapman (2008) and Ward and Dadayan (2009).
Policymakers and scholars have increasingly asked whether governments can adopt certain institutions to help promote fiscal sustainability. The debate typically centers around two broad categories of institutions: political institutions and fiscal institutions (Besley and Case, 2003). Political institutions determine how policy decisions are made; they include direct democracy, term limits, and the separation of powers between the executive and legislative branches. Fiscal institutions constrain the policies that can be adopted; they include tax and expenditure limits, balanced budget rules, debt limits, and rainy day funds.

As “laboratories of democracy,” state and local governments have experimented with a wide variety of institutions over the years. A large empirical literature exploits this institutional variation within and across state and local governments to estimate the effects of institutions on spending, taxes, deficits, and debt. This article synthesizes the literature, summarizing what we know (and don’t know) about the effectiveness of various institutions in promoting fiscal sustainability.

The findings are often surprising. Institutions that might seem likely to rein in spending, such as tax and expenditure limits, turn out to have little systematic effect. Yet other institutions, such as rainy day funds, seem to be quite effective in increasing government saving. Ultimately, however, the main lesson from the literature is that the devil is in the details — an institution’s effectiveness often depends critically on its specific legal provisions.

A. Institutions: A Theoretical Foundation

Before turning to the empirical question of how institutions shape policy outcomes, it is worth considering why institutions exist in the first place. The purpose of political and fiscal institutions is to bring about different policies than the ones politicians would choose if left to their own devices. But why might representative democracy produce policies that do not reflect the popular will?

According to the “workhorse” model of modern political economy — the median voter theory — a politician who strays from the median voter’s preferences will quickly be driven from office (Downs, 1957; Black, 1958). Similarly, Tiebout (1956) famously argued that citizens determine the size of government by “voting with their feet,” suggesting that a politician whose policy choices alienate voters risks sending them (and their tax dollars) fleeing to other jurisdictions. Both theories suggest that there is no need for institutions to restrain the growth of government — competition among candidates or jurisdictions provides sufficient motivation for politicians to align their policy choices with voters’ preferences.

These models do not seem to conform with reality, however. Empirical evidence suggests that voters tend to be more fiscally conservative than their elected officials (Peltzman, 1992). And the occurrence of taxpayer revolts, such as the passage of Proposition 13 in California, provides further confirmation that competitive forces are insufficient to
“constrain the natural proclivities” of elected officials (Brennan and Buchanan, 1980).² Indeed, the shortcomings of the median voter theory are by now well understood — it ignores important considerations such as imperfect political competition, imperfect information, and interest group pressure, among others. A host of alternative theories have emerged to address these shortcomings. Several of the most prominent theories are outlined briefly below.

The first set of theories relaxes the assumption of perfect political competition. Imperfect political competition might result from imperfect mobility of taxpayers among jurisdictions; alternatively, it might result from incumbency advantage, which creates barriers to challenger entry. Either way, theories of imperfect political competition posit that public officials acquire monopoly power over taxation, which they use to pursue private goals, whether pecuniary (rent-seeking) or non-pecuniary (power and prestige). As a result, government is said to become a budget-maximizing “Leviathan” (Niskanen, 1975; Brennan and Buchanan, 1979).

Second, in political agency or “principal-agent” models, voters have imperfect information about politicians’ actions and/or competence, making it difficult for voters to monitor and discipline their elected officials. In agency models, citizens condition their voting decisions on policy outcomes to give incumbents an incentive to exert costly effort on their behalf, to weed out incompetent politicians, or both (Barro, 1973; Ferejohn, 1986; Rogoff, 1990). But elections are at best an imperfect tool; they may help resolve some of the problems inherent in representative democracy, but they do not eliminate the underlying information asymmetry.

A potentially important consequence of asymmetrical information is “fiscal illusion” — the idea that taxpayers perceive the cost of government to be lower than it actually is, leading politicians to choose higher spending and run larger deficits than if taxpayers had full information (Puviani, 1897; Buchanan and Wagner, 1977). Two widely cited examples of fiscal illusion relate to intergovernmental grants and deficit financing. First, fiscal illusion is said to lead to a “flypaper effect,” whereby governments respond to intergovernmental grants by increasing spending rather than passing on the grant to taxpayers in the form of a tax cut (Gramlich, 1969). Second, voters tend to perceive debt-financed government services as less costly than tax-financed services because they underestimate the future tax liability required to repay the debt (Vickrey, 1961). The temptation to rely on debt financing may be particularly strong immediately before an election, resulting in a “political business cycle” in spending and deficits (Nordhaus, 1975).

² However, it is important to note that voters do not necessarily prefer fiscally sustainable policies either; evidence suggests that at least some voters “want something for nothing” — that is, they prefer a combination of high spending and low taxes (Citrin, 1979). The argument here is simply that institutions have the potential to bring about policies that are more sustainable relative to those that would be chosen under normal electoral processes.
Finally, spending may be higher than taxpayers would like as a result of pressure from special-interest groups. Unlike the median voter model — from which pressure groups and money are conspicuously absent — interest group theories are based on the premise that since winning reelection requires money as well as votes, politicians try to strike a balance between pleasing voters and interest groups. And the latter — by virtue of being large, mobilized, and moneyed — are often more persuasive than the former (Olson, 1965; Stigler, 1971; Peltzman, 1976).

In summary, institutions have a potentially important role to play in addressing some of the shortcomings of representative democracy. In the median voter/Tiebout model, voters who want politicians to spend, tax, or borrow less can simply voice their preferences at the polling booth. But under conditions of imperfect competition, imperfect information, and interest group pressure, aligning policy choices with voter preferences may require additional rules and procedures.

B. Where Do Institutions Come From?

The previous section explains why institutions might exist; a closely related question is how they actually come to exist. In the case of U.S. state and local governments, voters impose many institutions — such as tax and expenditure limits and legislative term limits — through the direct democracy process. Other institutions are self-imposed by lawmakers, either as part of the original Constitution — as is often the case with balanced budget rules and debt limits — or through constitutional amendments or statutes — as with rainy day funds.

It might seem curious that lawmakers would voluntarily tie their own hands. A particularly curious example is the line-item veto, which exists in nearly all states because legislative bodies have chosen to adopt it. In doing so, legislators have relinquished some of their power to the executive, compromising their ability to provide legislative “pork” to their constituents. One potential explanation is that legislators view the item veto as a solution to a collective action problem: although each legislator wants pork for her own district, she does not want a budget full of pork for all legislators’ districts (Cox and McCubbins, 1993). In this view, legislators are like Odysseus, tying themselves to the mast to avoid succumbing to the Sirens’ song. Another potential explanation is that conservative majorities adopt the line-item veto to “protect their future interests” in the event that a liberal majority takes control (de Figuerido, 2003). Yet even when politicians have an incentive to tie their own hands ex ante, they may have an incentive to evade rules ex post. Or, as Brennan and Buchanan (1980, p. 3) explain, “A tennis player after hitting a particular shot may reasonably wish that the net was lower, yet prior to the game he may have agreed to a set of rules in which the height of the net was specified.”

Regardless of whether institutions originate with voters or politicians, their adoption is clearly not random. As Riker (1980) famously posited, institutions might be thought of as “congealed preferences,” persisting only as long as voters and lawmakers wish. By contrast, others have argued that institutions — particularly those adopted decades
or centuries ago — are merely the result of historical accident and therefore may exert independent effects apart from legislator and voter preferences (Poterba, 1995). For example, many of the states where direct democracy is present are western states that happened to write their constitutions during the Progressive Era.

From an empirical standpoint, the potential for institutional endogeneity matters because it complicates the task of estimating causal relationships. In particular, endogeneity means that institutions are correlated with an unobservable, omitted right-hand-side variable (preferences), which may lead to biased estimates of institutions’ effectiveness. For example, as Kiewiet and Szakaly (1996, pp. 68–69) explain in their study of debt limitations:

“Even if certain constitutional limitations are associated with less indebtedness, we must . . . be cautious in the inferences we draw . . . perhaps they restrain governments from issuing debt, but they also reflect the degree to which citizens of that state are averse to borrowing. In a similar vein, it is also important to consider just what the absence of constitutional debt limitations might imply. States may not have them because historically there has been little demand, either on the part of the people or their elected representatives, to take on a great deal of debt.”

Thus, researchers who study institutions must be careful to choose appropriate empirical identification strategies. As Besley and Case (2003) note, there is no panacea for institutional endogeneity, but it is often helpful to develop a theoretical account of what motivates institutional change — both to understand the nature of the potential bias and, when possible, to identify an appropriate instrumental variable. A closely related problem is that some institutions have changed very little over time, making it difficult to distinguish the institution’s effect from a state fixed effect. Some researchers have found creative ways around this problem, as discussed below.

With that caveat, I now turn to summarizing what we know (and don’t know) about the estimated effects of institutions on fiscal sustainability. I begin with political institutions, focusing on direct democracy, term limits, and the separation of powers between executive and legislative branches. I then turn to fiscal institutions, focusing on tax and expenditure limits, balanced budget rules, debt limits, and rainy day funds.

II. POLITICAL INSTITUTIONS

A. Direct Democracy

In 1893, California became the first state to allow its citizens to take part in direct democracy at the local level. Today, more than half of U.S. states and cities have adopted direct democracy, and more than two-thirds of the population lives in a state and/or city where direct democracy is available (Matsusaka, 2004). Direct democracy is a disproportionately western phenomenon, although it exists to a lesser extent in other regions of the country as well.
The most prominent form of direct democracy is the ballot initiative, which allows citizens to propose and vote on specific laws. By contrast, a referendum allows citizens to vote on a law that has already been approved by the legislature. The number of signatures needed to qualify an initiative or referendum varies from state to state, as does the vote requirement for final approval (a majority is sufficient in some states while a supermajority is required in others).

Although the ballot initiative and referendum have existed for more than a century, they were not used extensively until the 1970s. During this period of “disillusionment with government and a view that spending was out of tune with what a majority of voters preferred,” direct democracy presented a mechanism for pushing policy closer to the position of the median voter (Besley and Case, 2003, p. 58). During and since the tax revolt of the 1970s, voters have used the initiative and referendum process thousands of times, often with the goal of slowing the growth of taxes and spending. In fact, a common use of direct democracy has been to impose tax and expenditure limits; two famous examples include Proposition 13 in California and Proposition 2 ½ in Massachusetts, both of which capped real estate taxes.\(^3\) However, in other cases the initiative has been used to increase the size of government, for example by directing the legislature to increase spending on K–12 education.

Empirical evidence on the effects of direct democracy is mixed. Several early studies found evidence of a negative relationship between direct democracy and spending. Matsusaka (1995) uses panel data on 49 states between 1960 and 1990 and finds that state and local general expenditures are 4 percent lower in initiative states with a 5 percent signature requirement than in non-initiative states. He finds that initiative states do not have lower revenues than non-initiative states, but do rely less on taxes and more on user charges. He finds no effect of the initiative on debt. However, because the initiative changed in only four states during his sample period, Matsusaka does not control for state fixed effects; thus, the initiative coefficient may be picking up the effects of voter preferences or other state-specific characteristics. In a closely related study, Bails and Tieslau (2000) use panel data from 49 states observed at five-year intervals between 1969–1994, but employ a random effects model to attempt to capture differences among states. They find that the initiative was associated with a $96 reduction in state and local spending per capita.

Other studies find a positive relationship between direct democracy and public spending. Zax (1989) uses cross-section data from 50 states and 1305 municipalities and finds that the initiative is associated with a $265 increase in per capita spending at the state level and a $45 increase at the municipal level. Matsusaka (2000) finds that in the first half of the 20th century, the initiative was associated with an increase of up to 10 percent in per capita spending and revenues. He reconciles this finding with his earlier result as follows:

\(^3\) Tax and expenditure limitations are discussed in more detail later in the paper.
When representatives are more fiscally conservative than the voters, the initiative leads to higher spending, and when representatives are more liberal, the initiative leads to lower spending. According to this view, in the first half of the century representatives were slow to respond to increased voter demands for spending, while in the second half of the century elected officials were slow to respond to voter demands for government downsizing,” (Matsusaka, 2000, p. 641).

However, Marschall and Ruhil (2005) find evidence that contradicts this hypothesis. Using panel data from 1960–2000 and a maximum likelihood estimator, they find a positive relationship between the initiative and spending and taxes. Specifically, they find that the initiative is associated with an approximately $160 increase in both per capita spending and per capita taxes. They interpret their finding as evidence that, following the tax revolt, public opinion shifted in favor of more, not less, government spending.

Finally, other studies find that the initiative has no statistically significant, systematic effect on spending and taxes. Using cross-section data from 1989–1990, Camobreco (1998) does not find a reliable statistically significant relationship between the initiative and a state’s revenues or spending. Besley and Case (2003) use state data from 1960–1997 and three different regression specifications — ordinary least squares, random effects, and regression on state means — to examine the effect of direct democracy on per capita spending and taxes. Although the coefficient on the citizens’ initiative is almost always negative, in five out of six cases the standard error is too large to say conclusively that a negative relationship exists.

In summary, direct democracy does not have clear-cut implications for fiscal sustainability. Early findings that the institution is associated with lower spending but not lower revenues seemed to suggest a positive impact, but later studies contradict these results. One potential explanation for the lack of consensus in the literature is that estimating the impact of direct democracy is complicated by not only institutional endogeneity, but also the fact that there has been very little variation in this institution over the past 50 years, and a wide variety of methodological approaches has been used to address these issues. Moreover, voters have used the ballot initiative to implement a variety of other institutions, such as term limits and tax and expenditure limits, which means estimates of direct democracy may be picking up these other effects as well. However, several scholars have taken advantage of this link by using direct democracy as an instrumental variable for other institutions, as described later in the paper.

B. Term Limits

Advocates of term limits have claimed that limiting politicians’ tenure potential slows the growth of spending through a number of different channels. First, some contend that removing the prospect of reelection reduces lawmakers’ incentive to secure pork-barrel projects for their constituents (Will, 1993). Second, term limits are said to discourage self-interested, “careerist” politicians from running for office, facilitating the election
of citizen-legislators who will pursue policies that reflect the public will. Third, term limits are often said to remove “entrenched” lawmakers who over time have succumbed to interest group pressure and the “culture of spending” that permeates government (Payne, 1992). Fourth, term limits may reduce the scope for logrolling, since it takes time for lawmakers to develop reputations and build stable coalitions (Reed et al., 1998). Finally, politicians who serve many years in office are said to acquire “brand-name capital,” protecting them from challengers and thus insulating them from voters’ (relatively fiscally conservative) preferences (López, 2003).

Others argue that term limits may actually increase spending. First, by reducing politicians’ accountability to voters, term limits may reduce incumbents’ incentive to exert costly effort in providing government services efficiently or “holding down the level of spending” (Besley and Case, 1995). A second possibility is that term limits might prevent voters from reelecting incumbents who have proven to be prudent financial managers, or who have developed the expertise needed to provide government services efficiently (Alt, Bueno de Mesquita, and Rose, forthcoming). A third argument is that term limits might simply cause politicians to begin planning their next career move right after entering office, increasing their incentive to cater to special interests that could assist with career advancement (Fiorina, 1996). A final consideration is that term limits may shift the balance of power in a way that affects the level of spending. For example, to the extent that legislative term limits reduce legislative professionalism and expertise, they may increase the relative influence of interest groups, thereby increasing pork-barrel spending; alternatively, they may increase the relative influence of the executive branch in the budget process, potentially reducing pork-barrel spending since executives may, for reasons discussed in the next section, prefer less pork than legislatures (Carey et al., 2006).

In the United States, term limits may apply to the governor, the legislature, or both. In 1776, Delaware became the first state to adopt gubernatorial term limits. Since then, the majority of states have followed suit. At first, these rules typically limited governors to a single term in office; later, most states relaxed their rules to allow governors to serve a second term. Today, Virginia is the only state with a one-term limit.

Despite being such a long-standing institution, gubernatorial term limits have received surprisingly little attention in the literature. In the most widely cited paper on gubernatorial term limits, Besley and Case (1995) find evidence that this institution reduces electoral accountability. Specifically, they find that in the 48 contiguous states between 1950–1986, per capita spending and taxes were modestly higher ($15 and $7, respectively, in 1982 dollars) under term-limited governors than under governors who were eligible for reelection. However, in a later paper the authors find that this effect has disappeared over time (Besley and Case, 2003). Alt, Bueno de Mesquita and Rose (forthcoming) argue that this puzzling change can be explained by the widespread shift from one- to two-term limits. As the states gradually relaxed their laws by allowing

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4 For a more detailed discussion of the arguments for and against term limits, see Karp (1995) and López (2003).
governors to serve a second term, voters were able to weed out incumbents whose first-term policy choices did not match voters’ preferences.

Legislative term limits, by contrast, are a relatively recent phenomenon. In 1990, citizens passed ballot initiatives limiting the number of terms a legislator could serve in three states (California, Colorado, and Oklahoma). Since then, 18 other states have followed suit. However, in several states these measures were subsequently repealed by legislatures or thrown out by state supreme courts. Today, a total of 15 states have legislative term limits. Despite the fact that legislative term limits are both newer and less common than gubernatorial term limits, they are the subject of considerably more inquiry. However, most of this literature is concerned with the effect of term limits on the composition and behavior of legislators rather than on budget outcomes. Nonetheless, some of their findings have potentially interesting implications for fiscal sustainability.

One strain of the literature on legislative term limits examines the conjecture that legislative term limits change the composition of the legislature. A large body of evidence (including studies by Carey, Niemi, and Powell (1998), Carey, Niemi, and Powell (2000), Carey et al. (2006), and Kousser (2005), among others) suggests that term limits have not led to substantial changes in the observable characteristics of state legislators, such as their profession, age, or education level. However, Meinke and Hasecke (2003) find that term limits reduce the share of democrats in the legislature, which may have implications for fiscal policy (although the authors do not investigate whether this is the case).

Another set of studies investigates the hypothesis that limiting lawmakers’ tenure potential shifts their priorities from the self-interested pursuit of reelection to serving the public interest. Carey, Niemi, and Powell (1998) and Carey, Niemi, and Powell (2000) find support for the argument that term limits encourage legislators to spend less time on activities related to their own reelection, such as securing pork-barrel projects. Similarly, Carey et al. (2006, p. 123) find, based on legislators’ self-reports, that term-limited lawmakers “... pay less attention to their constituents — whether one judges attention by constituency service or by pork barreling — and are more inclined to favor their own conscience and the interests of the state over those of the district.” However, Wright’s (2007) study of state legislative roll calls from 1999 and 2000 suggests that term-limited and non-term-limited legislators are equally likely to vote according to their constituents’ ideological preferences.

Another branch of the literature looks at whether term limits affect legislative professionalism and thus shift the balance of power. Moncrief and Thompson’s (2001) survey of lobbyists and Peery and Little’s (2003) survey of legislative leaders reveal that both sets of actors believe term limits increase the influence of interest groups in the legislative process. Similarly, based on case studies and survey data, Kousser (2005) and Kousser and Straayer (2007, p. 162) conclude that, “… term limits have led to a significant erosion of legislative independence in the state budgeting process.”

Only a handful of studies have directly investigated the link between legislative term limits and fiscal policy. Evidence from Bails and Tieslau (2000), based on a random effects model and data from 49 states over the period 1969–1994, suggests that term
limits are associated with a reduction in per capita spending in excess of $100. However, since only a few states adopted term limits at the end of their sample period and the authors do not include year effects or state fixed effects in the model, this finding should be interpreted with caution. In a more recent study of 48 states between 1977–2001, Erler (2007) controls for year and state fixed effects and finds that per capita expenditures are 2.1 percent higher following the adoption of term limits. She interprets this finding as evidence that term limits have “… opened up the legislative process, especially the budget process, to a greater number of participants, including other legislators, interest groups, and agency officials” (Erler, 2007, p. 490).

In summary, the bulk of the empirical evidence suggests that term limits are associated with higher spending. At the gubernatorial level, term limits may induce “shirking,” although there is also evidence that term limits may prevent voters from choosing politicians whose policy choices match their own preferences. Legislative term limits appear to increase the influence of interest groups, which may account for Erler’s finding that they are associated with higher spending. Two lessons about institutional design emerge from this literature. First, the length of term limits has potentially important effects, as spending is higher under one-term limits than under two-term limits. Second, term limits appear to shift the balance of power among different political actors, suggesting that the adoption of both legislative and gubernatorial term limits might have a different effect than the adoption of one or the other. More research is needed on the potential interaction of these two institutions.

C. Separation of Powers

Many of the aforementioned shortcomings of representative democracy — such as accountability and special-interest politics — seem to be exacerbated by the legislative process. Relative to the executive branch, legislatures are often said to be more prone to “pathologies” such as parochialism, pork-barrel politics, and logrolling (Ferejohn, 1974; Mayhew, 1974). By contrast, the executive is said to have “… more incentives to internalize the government budget constraint and is relatively more responsive to the interests of the average taxpayer rather than of any specific pressure group” (Alesina and Perotti, 1996, p. 403).

Thus, one set of institutions designed to limit the growth of spending seeks to shift the balance of power away from the legislature and toward the executive. Among the most important of these are budget proposal and veto authority, which give the executive “… the first word and the last word” in the budget process, respectively (Dearden and Schap, 1994).

In the majority of state and local governments, the executive has the authority to recommend a budget to the legislature. However, in some states and localities the executive can actually submit his or her recommendations to the legislature in the form of an appropriations bill. In such cases, the executive’s budget becomes the reversion point to which legislative amendments are compared, increasing the executive’s agenda-setting power (Romer and Rosenthal, 1978, 1979).
There are surprisingly few empirical studies of the effect of the executive budget proposal on spending, and what little evidence does exist is mixed. Using cross-sectional data from the 50 states over the 1979–1986 period, Crain and Miller (1990) find that when the governor alone has the authority to propose a budget, spending is 1.3 percent lower than when the legislature writes appropriations bills from scratch.\footnote{Similarly, Clarke (1998) finds that in the 50 states from 1985–1994, the percentage change made by the legislature to the governor’s recommendation was smaller when the governor had more institutional authority. However, since this study uses a multidimensional index of gubernatorial powers — including veto power and tenure potential, among others — it is unclear whether this effect is being driven by proposal power.} However, Dearden and Husted (1993) study the 50 states in the late 1980s and find that proposal authority does not have a statistically significant effect on the enacted budget, relative to the governor’s recommendations. Similarly, Endersby and Towle (1997) find no relationship between proposal authority and per capita spending using panel data from 50 states in 1988, 1990, and 1992; however, their regression model does not control for state or year effects.

The empirical literature on veto authority is considerably larger than that on proposal authority for at least two reasons. First, veto authority is widely believed to be a more “potent weapon” than proposal authority; legislatures typically have the authority to make any changes they wish to the governor’s budget proposal, whereas the veto is used after the legislature has made those changes, and typically requires a legislative supermajority to override (Clarke, 1998). Second, the national debate over presidential veto authority has generated interest in the state experience; in particular, President Ronald Reagan’s 1986 request for a line-item veto set off a flurry of state-level studies in the late 1980s.

In all states (and most municipal governments), the executive has the authority to veto appropriations bills in their entirety. This “all-or-nothing” veto is a blunt and often useless instrument; governors rarely wish to reject the budget as a whole. However, the vast majority of governors (43) have the authority to veto individual line items within appropriations bills. In addition, governors in 14 states have the item-reduction veto, allowing them to reduce the amounts of items within appropriations bills. Since the item-reduction veto can, but need not, be used to reduce the amounts of items all the way to zero, it has the potential to provide the governor with more flexibility than the item veto.

Most states adopted the item veto between 1870–1900, although others have adopted the institution more recently. In most cases the legislature rather than voters proposed the item veto, as discussed earlier. However, legislators in some states responded to the item veto by combining “… several otherwise unrelated appropriations into one budget ‘item’ to place it beyond the reach of the governor…” (Baker, 2000, p. 64). Thus, in 1885, Pennsylvania became the first state to adopt an item-reduction veto, and soon many other states followed suit (Benjamin, 1982).

Most empirical studies of the relationship between item veto authority and government spending find negligible effects.\footnote{Carter and Schap (1990) provide a helpful review of the empirical literature on the item veto.} Using cross-section data from 1980, Abrams
and Dougan (1986) find no evidence of a statistically significant relationship between the item veto and per capita state and local spending. In other cross-section studies, the Advisory Commission on Intergovernmental Relations, or ACIR (1987), finds that the item veto does not significantly lower the level or growth rate of state spending, and Rowley, Shughart, and Tollison (1986) find no evidence that the item veto is associated with lower debt. Using cross-sectional data from the 50 states over the 1979–1986 period, Crain and Miller (1990) find that possession of the item-reduction veto — but not the item veto — is associated with slightly slower growth in per capita spending.  

Other studies examine the partisan dimension of the item veto, conjecturing that it is likely to have the largest effect under divided government. Using panel data from 1965–1983, Holtz-Eakin (1988, p. 291) interacts possession of the item veto and item-reduction veto with the party composition of government, and finds that these institutions are associated with modestly lower spending under divided government, but only in the short run, leading him to conclude that “… in general, the line item veto does not appear to significantly alter, on average, the outcomes of the budgetary process.” Similarly, Alm and Evers (1991) use cross-section data from 50 states in 1982 and find that the item veto is more likely to reduce expenditures under divided government, but that this impact is fairly small — around 1 percent.

So far the discussion has focused on econometric studies of the item veto; however, survey data and case studies yield similar findings. In their survey of legislative budget officers, Abney and Lauth (1985) find that the item veto is used more frequently under divided than under unified government, and is used more frequently by minority Republican governors than minority Democratic governors, but is not significantly associated with fiscal restraint. However, in a later study, the authors gather additional survey data and find that about half of respondents said the item veto “significantly promoted fiscal responsibility” in their states, which the authors interpret as “… a moderate endorsement of the item veto as an instrument of fiscal restraint” (Abney and Lauth, 1997, p. 883). In his study of item vetoes exercised by Wisconsin governors between 1975 and 1985, Gosling (1986) finds that the item veto is most frequently used to strike qualifying language rather than dollar amounts, and concludes that governors use the item veto primarily to achieve policy and political goals rather than to restrain spending. Finally, Reese (1997, p. 510) studies more than 4,000 line-item vetoes cast by 63 governors in 10 southern states between 1973–1992 and finds that “… the only factor associated with high usage of the line-item veto is divided party control; however, divided party control is not associated with great amounts of appropriation reduction.”

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7 Another branch of the literature examines the effect of the item veto on the composition — rather than the level or growth rate — of spending. Some find no significant effects (Dye, 1969; Nice, 1988) while others find that executive budget authority — including not only veto authority but also proposal authority — shifts the composition of spending from localized programs to spending with statewide benefits (Barrilleaux and Berkman, 2003).

8 Several studies examine the effect of the item veto on variables other than spending. Baker (2005) finds that the item veto and item-reduction veto are each associated with more centralized government. Carter and Schap (1990) hypothesize that the item veto and item reduction veto increase the value of the governor’s office, but do not find support for this hypothesis.
In summary, the item veto appears to have a negligible effect on spending. There are several potential explanations for this finding. In their amusingly-titled article, “How to Succeed at Increasing Spending without Really Trying,” Gabel and Hager (2000, p. 21) point out that supermajority requirements to override executive vetoes may simply “… force a logroll including more representatives,” resulting in more pork-barrel projects. Second, Schap (1990, p. 240) notes that “… strategy complicates prediction …” — the legislature, anticipating the executive’s use of the item veto, may deliberately enact a larger budget than if the executive did not have item-veto authority. Nonetheless, institutional design again seems to play an important role, with a few studies suggesting that the item-reduction veto is somewhat more effective than the item veto in reducing spending.

III. FISCAL INSTITUTIONS

A. Balanced Budget Rules

Virtually all states and many local governments have some form of balanced budget rule, but there is considerable variation in the institutional design of these rules. In most states, the governor must submit a balanced budget to the legislature, the legislature must pass a balanced budget, and/or the governor must sign a balanced budget. Nearly all states have one or more of these three rules, which are sometimes known as prospective or ex ante balanced budget rules. A stricter form of balanced budget rule is a prohibition on carrying forward a deficit — sometimes called a “no-carry-over” or ex post rule — which exists in approximately half of the states.

In theory, balanced budget rules have the potential to impose fiscal discipline by eliminating “… persistent deficits induced by political distortions or by the politicians’ opportunism and ‘short-termism …’” (Alesina and Perotti, 1996, p. 401). A sizeable empirical literature examines this conjecture. As with the literature on the line-item veto, a driving force behind this literature was the national debate over whether the United States should adopt a balanced budget amendment in the 1990s.

In one of the earliest empirical studies of balanced budget rules, the ACIR (1987) compiled an index of the stringency of balanced budget requirements, with no-carry-over rules receiving the highest score. Using cross-section data from 1984, the ACIR found that more stringent rules are associated with smaller deficits. However, von Hagen (1991) tested this relationship using panel data from 1975–1985 and found no relationship between balanced budget rules and deficits. Instead of an index, Bohn and Inman (1996) use separate dummy variables for each type of balanced budget rule and find that no-carry-over rules were associated with smaller deficits from 1970–1991; however, this effect only holds for constitutional (as opposed to statutory) rules enforced by an elected (as opposed to politically appointed) state supreme court. And in a panel study of 48 states over the period 1965–1992, Alesina and Bayoumi (1996) find that strict balanced budget rules (as measured by the ACIR index) are associated with larger average surpluses as a share of state product, and that moving from no rule to the most stringent rule reduces the cyclical variance of fiscal balance by about 40 percent.
Hou and Smith (2006) have developed an alternative typology of balanced budget rules, based on a close reading of state constitutions and statutes, which distinguishes “political” rules — such as those requiring the governor or legislature to prepare a balanced budget — from “technical” requirements — including no-carry-over rules as well as within-year fiscal controls to avoid a deficit and limits on the amount of debt that can be assumed for deficit reduction, among others. In their 2010 study of 50 states over the period 1950–2004, Hou and Smith find that “technical” rules are more effective than “political” rules in restraining deficits; however, they do not find a statistically significant difference between constitutional and statutory rules.

Scholars have also examined the effect of balanced budget rules on variables other than deficits, such as spending and debt. Using cross-sectional data from the 50 states over the 1979–1986 period, Crain and Miller (1990) find that constitutional balanced budget requirements are associated with 1 percent lower spending growth; however, this result is quite sensitive to the regression specification and the lack of time-series variation or inclusion of fixed effects means these results should be interpreted with caution. Using cross-section data from 1962 and 1982, Nice (1991, p. 77) finds that balanced budget requirements are “essentially unrelated” to levels of and changes in debt. However, von Hagen (1991) finds, in his aforementioned 1975–1985 panel study, that per capita debt is significantly lower in states with stringent balanced budget rules than in states with the weakest form of balanced budget rule.

In addition to reducing deficits and debt, stringent balanced budget rules might also increase the speed with which governments adjust spending and taxes in response to unexpected fiscal shocks. Poterba (1994) finds that, among states with annual budget cycles between 1988–1992, those with strict balanced budget rules (as measured by the ACIR index) cut spending and raised taxes more rapidly following an unexpected shock. Similarly, Alt and Lowry (1994) find that no-carry-over rules are associated with more rapid adjustment to deficits, but only when Republicans control both branches of government.

A related question is whether this rapid adjustment to fiscal shocks translates into better credit ratings, and thus lower borrowing costs. Lowry and Alt (2001) look at the period 1973–1990 and find that deficits depress a state’s credit rating by less when the state has a strict balanced budget rule. Poterba and Rueben (2001) examine the period 1988–1999 and find a similar result: unexpected deficits are correlated with higher borrowing costs, but this effect is smaller for states with strict anti-deficit rules than for states without these rules. As the authors point out, these studies have an important methodological advantage over much of the previous literature on institutions and budget outcomes: “… changes in fiscal conditions are less likely than the level of fiscal variables to be correlated with state voter preferences … avoiding at least some of the endogeneity problems that may have affected earlier studies” (Poterba and Rueben, 2001, p. 538).

However, rapid adjustment to fiscal shocks has a potential downside. According to the “tax smoothing” theory, it is optimal for governments to run budget deficits during downturns to smooth temporary fluctuations in revenues and spending and avoid excess volatility in tax rates (Barro, 1979; Lucas and Stokey, 1983). Evidence suggests
that states operating under relatively stringent balanced budget rules undertook considerably less fiscal stabilization than those with weaker rules, according to Bayoumi and Eichengreen’s (1995) study of the 1971–1990 period. Similarly, Levinson (1998) extends the sample period to 1969–1995 and finds that states with strict balanced budget rules experience greater cyclical variability. In a more recent study, Fatas and Mihov (2006, p. 101) find further evidence that strict rules impair a government’s ability to run counter-cyclical fiscal policy; however, they also find evidence that balanced budget rules are associated with “… less aggressive use of discretion in conducting fiscal policy,” which serves to dampen the business cycle. And Rose (2006) finds that states with no-carry-over rules do not experience political business cycles in spending and deficits, whereas states with weaker rules do.

In summary, balanced budget rules appear to improve fiscal sustainability: they are associated with smaller deficits, lower debt, better credit ratings, more rapid adjustment to fiscal shocks, and less political manipulation of budgets. However, the devil is again in the details: these effects only hold for states with no-carry-over rules, and do not apply to the weaker ex ante balanced budget rules. In some studies, the distinction between constitutional and statutory rules also seems to matter.

B. Debt Limits

Throughout U.S. history, debt has played an important role in financing infrastructure, including roads, bridges, dams, and water-sewer systems. Financing infrastructure with debt accords with the “pay-as-you-use” principle of public finance, whereby equity requires that citizens who benefit from long-lived capital assets help to pay its cost (Clingermayer and Wood, 1995). Nonetheless, fiscal illusion may lead politicians to rely too heavily on debt financing, as discussed above. Indeed, in the nineteenth century, many states adopted debt limits (along with balanced budget rules) amidst allegations of excessive borrowing, scandals, and even a few cases of default (Nice, 1991).

There are four main types of restrictions on public debt in U.S. state and local governments. The most stringent and least common variety is an outright prohibition on guaranteed debt. Less stringent debt limits include debt ceilings and voter referendum requirements. Debt ceilings are either expressed as a percentage of the tax base or in dollars. Some have provisions for exceeding the limit, either for certain purposes (such as roads or sewers) or for certain types of jurisdictions (such as home rule municipalities). Voter referendum requirements may require either a simple majority or a supermajority of voters to approve new bond issues. The fourth type of limit is a legislative super-majority requirement. Most debt limits apply only to “guaranteed” general obligation debt.

Scholars have put forward a variety of theories about which types of limits will be most effective. Moak (1982) argues that since the electorate tends to be more fiscally conservative than politicians, referendum requirements should be most effective in limiting debt. By contrast, Buchanan (1958) argues that because voters like benefits but dislike taxes, direct democracy will be biased toward debt financing, and thus debt
ceilings or legislative supermajority requirements will be more effective in constraining debt. But Kiewiet and Szakaly (1996, p. 66) argue that legislative supermajority requirements may be ineffective because “… to the extent there is a tendency for expenditure logrolls to grow large (Weingast, 1979), borrowing logrolls might do the same.”

Several early studies find evidence of a negative relationship between debt-limit restrictiveness and government borrowing. ACIR (1961) develops an index of restrictiveness and, using cross-section data from 1957, finds that more restrictive limits are associated with lower general obligation debt as a share of personal income. Similarly, Pogue’s (1970) study of 48 states and 173 Metropolitan Statistical Areas (MSAs) in 1957 and 1962 finds a negative relationship between debt-limit restrictiveness and debt, and that this effect is primarily due to lower spending rather than higher taxes. McEachern (1978) studies average per capita debt levels in the 50 states in 1974 and concludes that state-imposed debt ceilings are an “effective curb” on local indebtedness and that supermajority referendum requirements are also associated with lower debt.

Several more recent studies have found no such relationship, however. In a study of 250 cities in 1977, Sharp (1986) finds that there is no statistically significant relationship between general obligation debt constraints and per capita guaranteed or non-guaranteed debt. Similarly, a study of 50 states in 1980 suggests that debt limits have no effect on per capita spending (Abrams and Dougan, 1986). Using panel data from 48 states from 1961–1989, Clingermayer and Wood (1995) find that debt ceilings, public referenda, and legislative supermajority requirements have no impact on the growth of long-term state debt.

One potential explanation for these non-results is that since debt limits typically apply only to guaranteed debt — thereby excluding revenue bonds as well as bonds issued by special districts, authorities, or commissions — these rules can be evaded through heavier reliance on revenue bonds or the creation of separate entities with borrowing power. Indeed, based on data from southern states and MSAs from 1972–1977, MacManus (1981) concludes that the more stringent a state’s debt limit, the greater the proliferation of special districts. Similarly, Bunch’s (1991) cross-sectional analysis reveals that even when a state has debt limits that apply to both general obligation and revenue debt, politicians can circumvent these rules by creating more public authorities and relying more heavily on such authorities.

Indeed, in recent years a strong consensus has emerged in the literature that helps to reconcile the conflicting findings of earlier studies: debt limits may reduce guaranteed debt, but they do so at least partly through substitution of non-guaranteed debt, so the effect on total debt is less than dollar-for-dollar (and, according to some estimates, negligible). Farnham (1985) evaluates the experience of 2,000 large municipalities in 1976–1977 and finds evidence that although the most stringent debt limits have a “depressing effect” on overall debt per capita, they also increase reliance on non-guaranteed debt. Von Hagen’s (1991, p. 199) state-level panel study reveals that median per capita state debt is significantly lower among states with stringent limits, but that these limits also appear to “… induce substitution into nonrestricted debt instruments.” Nice (1991) uses cross-section state data from 1982 and finds that debt limitations are
associated with lower levels of full faith and credit debt but have a negligible effect on overall state debt. Such findings may help explain Bahl and Duncombe’s (1993) finding, based on 1988–1989 data, that states with limits on both general-obligation debt and revenue bonds have much lower state and local government debt burdens than those with general-obligation limits alone. Finally, Kiewiet and Szakaly (1996) study the 50 states over the period 1961–1990 and find that states with debt ceilings and referendum requirements have less guaranteed debt than those with legislative supermajority requirements, but that debt limits are “not insurmountable obstacles” and, among other things, promote the devolution of debt to lower levels of government.

In summary, debt limits have the potential to improve fiscal sustainability, depending on their specific provisions. Debt ceilings and referendum requirements are associated with lower debt, while the evidence on legislative supermajority requirements is more mixed. The potential to evade debt limits by issuing revenue bonds means that the combination of general obligation and revenue debt limits is more effective than limits on general obligation bonds alone. Bunch (1991) notes that revising the legal language so that debt limits clearly apply to debt issued by authorities as well could further increase their effectiveness.

C. Tax and Expenditure Limitations

Tax and expenditure limitations (TELs) were introduced through the direct democracy process during the tax revolt of the 1970s, as discussed above. In 1976, New Jersey became the first state to adopt a tax and expenditure limit; within 10 years, 19 other states had followed suit, and today 30 states have TELs. These rules fall into five broad categories: (1) limits on revenue based on an index of income, inflation, and/or population growth, (2) limits on expenditures based on an index of income, inflation, and/or population growth (the most common form of tax and expenditure limit), (3) limits that restrict appropriations to 95–99 percent of the official revenue forecast, (4) requirements that voters must approve tax increases, and (5) legislative supermajority requirements for tax increases.

The majority of evidence on state TELs suggests that these institutions have not been effective in slowing the growth of the public sector. In one of the earliest empirical studies of TELs, Kenyon and Benker (1984) report that, according to survey data, state budget officers and citizen tax groups in a majority of states with TELs believed that these rules had had no effect and would probably continue to have no effect. The authors’ analysis of expenditure data from 1978–1983 lends support to these observers’ impressions, revealing no discernible difference between spending and revenue growth in TEL states and non-TEL states.

Using cross-section data from 1980, Abrams and Dougan (1986) find that the level of spending in states with TELs is the same as in states without TELs. Bails (1990) uses data from the early 1980s and similarly finds no significant difference in expenditure or revenue growth between TEL states and non-TEL states. Cox and Lowery (1990), Joyce and Mullins (1991), and Shadbegian (1996) use panel data from some
subset of the years 1960–1990, and all three find that state TELs have had little to no effect on spending or revenues. Kousser, McCubbins, and Moule (2008) use a “quasi-experimental” approach in which they examine changes within a given state following the adoption of TELs instead of comparing TEL states to non-TEL states. However, like the previous literature, they find little impact of state TELs.

A few studies do find evidence suggesting that state TELs slow the growth of government, however. Poulson and Kaplan (1994) study 22 states with TELs from 1946–1990 and find that the institution is associated with lower expenditures as a share of personal income in five of those states (Colorado, Hawaii, South Carolina, Tennessee, and Texas) but only in the short run. Rueben (1996) uses direct democracy as an instrument and estimates a two-stage-least-squares model; she finds that the ratio of taxes to personal income is 2 percent lower in states with TELs. However, since she is using a time-invariant institution as an instrumental variable, her model excludes state fixed effects. Bails and Tieslau (2000) study 49 states from 1969–1994 and estimate that real per capita state and local spending is $41 lower in states with TELs than in states without TELs. The reason for the discrepancy between these studies’ findings and the non-findings in the majority of the literature may be due in part to methodological choices: the first study uses an ARIMA regression, the second uses an instrumental variables approach without fixed effects, and the third uses a random-effects model instead of fixed effects.

What might explain the modest to negligible effect of state TELs? Kenyon and Benker, (1984) note that state TELs typically cover only about 60 percent of a state’s revenues, leaving plenty of avenues for evasion. Specifically, state officials might respond to TELs by simply relying more heavily on user charges (Mullins and Joyce, 1996), adopting or expanding state lotteries (Glickman and Painter, 2004), or borrowing more (Bahl and Duncombe, 1993; Clingermayer and Wood, 1995). Another possibility is that state TELs lead state officials to shift responsibility for spending to lower levels of government (Martell and Teske, 2007; Skidmore, 1999).9

Evasion of TELs also seems to occur at the municipal level, although municipal tax and expenditure limits appear to be somewhat more effective than state TELs in slowing the growth of government. Lowery (1983) studies the period 1957–1976 and finds that local TELs have reduced reliance on property tax revenues and increased reliance on state aid and other revenue sources; however, he does not — as Shadbegian (1999) notes — control for important determinants of fiscal policy such as income and population. Shadbegian (1999) looks at a sample of 2,955 counties from 1962–1987 and similarly finds that TELs induce a shift from taxes to user fees. In two separate studies of municipalities in Illinois, Dye and McGuire (1997) and Dye, McGuire, and McMillen (2005) find that TELs are effective in reducing property taxes — and increasingly so over time — as well as lowering school expenditures. Similarly, Blankenau and Skidmore (2004) present evidence that TELs led to lower own-source spending on education by

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9 For a nice discussion of this literature on the unintended consequences of TELs, see Bowler and Donovan (2004).
localities over the period 1971–1993. Shadbegian (2003) examines roughly the same period (1966–1992) and similarly finds a reduction in local education spending, but also finds that TELs shift the funding burden to the state level. Poterba and Rueben (1995) find that tax limits are associated with slower growth in a major object of government expenditures: government employee salaries.

Other studies have looked at the effect of municipal TELs on variables other than the size of government, and find evidence suggesting that these limits have several potential disadvantages. Poterba and Rueben (2001) find unexpected deficits lead to larger increases in a state’s borrowing costs when the state has a tax and expenditure limit, which compromises the state’s capacity to eliminate the deficit by raising taxes. And Figlio and Rueben (2001) find evidence suggesting that tax and expenditure limits reduce the average quality of new public school teachers.

Whereas most of the literature has focused on the most common form of TELs — numerical limits on spending or revenue — a handful of studies investigate the effect of legislative supermajority requirements for tax increases. Using cross-sectional data from the 50 states over the 1979–1986 period, Crain and Miller (1990) find evidence that supermajority requirements reduce the growth rate of spending by 1–3 percent. Following an instrumental variables approach similar to that used by Rueben (1996), Knight (2000) uses direct democracy as well as the legislative vote required to pass a constitutional amendment as instrumental variables, and finds that supermajority requirements decrease the tax rate by 8–23 percent, and reduce tax revenues as a percent of personal income by 1.7–3.6 percentage points.

In summary, tax and expenditure limits appear to be modestly effective in slowing the growth of government, particularly at the municipal level, although the evidence is somewhat mixed. The effectiveness of TELs seems to be diminished by the substitutability of tax and non-tax revenues as well as the substitutability of state and local spending. The possibility that TELs induce shifts in the funding burden between levels of government suggests that adopting both state and municipal TELs might have a different effect than merely adopting one or the other; more research is needed on the interactions among these two institutions. Research also suggests that the type of limit matters, and in particular that supermajority requirements for tax increases may be more effective than other types of TELs.

D. Rainy Day Funds

In 1980, nine states had rainy day funds (RDFs); today, all but four states have adopted this institution. What motivates a state to adopt a rainy day fund? Since most states adopted their rainy day funds after an economic downturn, the conventional wisdom is that the motivation is to be better prepared for the next recession (Douglas and Gaddie, 2002). However, Wagner and Sobel (2006) provide an alternative hypothesis: for some state lawmakers, the desire for a rainy day fund may be motivated by citizen-imposed tax and expenditure limits that make it difficult to accumulate general fund surpluses. Either way, however, the goal seems to be to increase state saving.
Rainy day funds in the states are governed by a wide variety of provisions specifying the conditions under which deposits are required and withdrawals are permitted. In some states, deposits are made by legislative appropriation, giving lawmakers considerable discretion over the timing and amount of deposits. In more than half of the states, all or some percentage of general-fund year-end surpluses must be deposited in the fund. And in a few states, a formula linked to the state of the economy triggers automatic deposits. Similarly, withdrawals are made by appropriation in some states, can only be used to cover a general fund revenue shortfall in most states, and are triggered by a formula linked to the economy in a handful of states.

Several studies have found evidence suggesting that rainy day funds are quite effective in increasing state saving. Using panel data from 1984–1997, a period over which 27 states adopted rainy day funds, Knight and Levinson (1999) find that the adoption of a fund increases total state saving dollar-for-dollar. However, Wagner (2003), studying the period from 1973–1997 and using time-series methods to account for non-stationarity in the data, finds that amounts saved in the rainy day fund partly reflect the displacement of general fund surpluses. As such, he arrives at a smaller estimate than Knight and Levinson, but nonetheless a substantial one: each dollar deposited in the rainy day fund contributes nearly 50 cents to total state saving. Hou and Duncombe (2008) estimate the effect of rainy day fund adoption on state saving in percentage terms, and find that adopting a RDF increases saving by between 2–3 percentage points.

Not all rainy day funds are created equal, and it appears that the provisions governing deposits and withdrawals are of critical importance to determining their effectiveness. States with deposit and withdrawal formulas appear to save considerably more in their rainy day funds than do states that allow lawmakers greater discretion over the timing and size of withdrawals, according to both Knight and Levinson (1999) and Hou (2004).

Because these studies use the timing of rainy day fund adoption as their source of empirical identification, they may be vulnerable to institutional endogeneity since “…states that choose to adopt the funds may do so during the years in which they plan to save,” — such as following a recession, as noted above (Knight and Levinson 1999, pp. 460–461). An additional endogeneity concern arises from the fact that states that are especially serious about increasing saving for stabilization purposes might adopt more stringent restrictions on politicians’ discretion. Indeed, Wagner and Sobel (2006) find that, compared to states that adopt RDFs following a recession, those that adopt funds after the imposition of tax and expenditure limits tend to adopt weaker RDF rules. As such, these estimated effects of rainy day funds on state saving should be interpreted with caution.

A related question is whether rainy day funds help states weather economic downturns. Two studies of the 1990–1991 recession find evidence suggesting that the answer is yes. General expenditures fell by less in states with funds than in states without funds, particularly when the fund had a deposit formula provision (Sobel and Holcombe, 1996; Douglas and Gaddie, 2002). Because these studies use state responses to an exogenous shock as their source of empirical identification, they are arguably somewhat less vulnerable to institutional endogeneity problems than the aforementioned papers on the level of saving.
Institutions and Fiscal Sustainability

To the extent that rainy day funds improve a state’s ability to weather economic downturns, it seems plausible that this improved outlook would be reflected in the state’s credit ratings. Wagner (2004) examines this conjecture using data from 36 states from 1973–1999. He finds that when a state adopts a rainy day fund with deposit and withdrawal formulas, its borrowing costs decline by 33 basis points; states that adopt funds with weaker provisions experience smaller reductions in borrowing costs.

Despite the apparent effectiveness of rainy day funds in promoting fiscal sustainability in the face of cyclical shocks, there is also some evidence that these funds are subject to political manipulation. In a panel study of 32 states with rainy day funds over the period 1989–2005, Rose (2008) finds that lawmakers withdraw nearly three times as many funds in response to a deficit shock of a given size if it occurs in an election year than if it occurs in a non-election year. The presence of a withdrawal formula does not appear to deter this effect, although requiring the governor to declare an emergency before the funds can be accessed does appear to help.

In summary, rainy day funds seem to improve fiscal sustainability. Evidence suggests that each dollar deposited in a rainy day fund generates between 50 cents and one dollar of additional state saving, and that states with rainy day funds experience less distress during economic downturns. However, these effects depend on the fund’s provisions: deposit and withdrawal formulas appear to be crucial to a rainy day fund’s effectiveness.

IV. CONCLUSIONS

Improving the fiscal sustainability of state and local governments is a daunting task, but an enormous body of empirical evidence on fiscal and political institutions can provide some helpful guidance. State and local experimentation with a wide variety of rules and procedures over the years has shed light on the relative effectiveness of various mechanisms for slowing the growth of government and reducing deficits and debt, with important implications for optimal institutional design.

What do we know about the relationship between fiscal institutions and fiscal sustainability? Evidence suggests that rainy day funds with strict deposit and withdrawal provisions are quite effective in promoting saving and that strict “no-carry-over” balanced budget rules succeed in reducing deficits. The evidence on debt limits and tax and expenditure limits is more mixed; the former seems to increase reliance on non-restricted forms of debt while the latter seems to increase reliance on non-tax revenue sources. Nonetheless, many studies find that these institutions have modest effects on indebtedness and the growth of government, respectively, depending on their specific provisions.

What do we know about the relationship between political institutions and fiscal sustainability? Direct democracy does not appear to have a systematic effect on spending or taxes, although the ballot initiative has led to the adoption of several other institutions — most notably tax and expenditure limits — that have the potential to reduce the growth of government, as noted above. Term limits appear to increase spending by inducing shirking, preventing voters from reelecting fiscally conservative incumbents, and/or increasing the influence of special interest groups. Finally, shifting the balance
of power from the legislature to the executive — for example by giving the executive exclusive proposal authority or the line-item veto — appears to have at most a modest effect on the growth of spending.

What don’t we know about institutions and fiscal sustainability? A lot. The jury is still out on the fiscal effects of relatively new institutions such as legislative term limits — yet older institutions such as executive proposal authority have received surprisingly little attention in the literature as well. Since the devil is in the details, more research is needed on the specific legal provisions underlying political and fiscal institutions, along the lines of Hou and Smith’s (2006) study of balanced budget rules. We also need a better understanding of the interactions among institutions, such as state-level and municipal-level TELs and gubernatorial and legislative term limits. Other institutions not discussed in this paper — including executive rescission authority and budget transparency, among others — also present interesting directions for future research. A better understanding of the origins of institutions is important in its own right and could also help researchers come up with appropriate methodological solutions to problems of institutional endogeneity. Finally, most of the existing literature on institutions focuses on the state level; more municipal-level research is needed. Recent contributions to the literature — such as Gordon’s (2009) study of the causes and consequences of local voter initiatives and Brooks and Phillips’s (2010) survey of the origins of locally-imposed tax and expenditure limits — suggest that several of these gaps in the literature may be short-lived.

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