# Politics Can Limit Policy Opportunism in Fiscal Institutions: Evidence from Official General Fund Revenue Forecasts in the American States

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***Abstract***

*Governments make policy decisions in the same areas in quite different institutions. Some assign policymaking responsibility to institutions designed to be insulated from myopic partisan and electoral pressures and others do not. In this study, we claim that differences in political context and institutional design constrain the policy choices governments make. Testable propositions based on an analysis of varying electoral incentives and time horizons created by these different contexts are empirically tested using panel data on official general fund revenue forecasts in the American states, 1987 to 2008. The empirical evidence reveals that executive branch agencies and independent commissions produce more conservative forecasts than legislatures with one important exception. Executive branch revenue forecasts in states with gubernatorial term limits are indistinguishable from legislative branch forecasts. Further, we find that legislative branch forecasts are more conservative in the presence of divided partisan legislatures than unified party government. In turn, this implies that entrusting policymaking authority to either the executive branch or an independent commission may only be consequential when the political system itself fails to check legislative excesses or executive myopia.* © *2012 by the Association for Public Policy Analysis and Management.*

Which institution should be assigned with policymaking authority? On a normative level, the answer to this question is of foundational importance to both scholars and practitioners alike interested in democratic governance. Policymakers seek to make effective policy, subject to the political exigencies that they experience in a democratic system. For instance, legislatures often choose to delegate policymaking authority to the executive branch or independent commissions to escape the pernicious effects of myopic electoral pressures and collective action problems that their members encounter. Independent central banks adopted throughout the developed world are intended to prevent politicians from adversely affecting the conduct of monetary policy (e.g., Cukierman, Webb, & Neyapti, 1992; Waller, 2000). In many policy areas, ranging from pension funding to base closings, legislators face a similar choice, knowing that the short-term or myopic incentives of political actors will lead to worse policymaking in the aggregate. In delegation settings, legislatures ascribe policymaking authority to public agencies so as to make it much costlier for elected politicians to alter policy in the future (Lewis, 2003; Moe, 1989). Such policy hand tying can restrict politicians' strong incentives for engaging in strategic policy

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manipulation (e.g., Falaschetti & Miller, 2001; Patashnik, 2000; Spulber & Besanko, 1992).

The aim of this study is to show how alternative political contexts may produce different preferred institutional venues when it comes to limiting opportunistic policymaking behavior. This research puzzle is addressed by analyzing the relative conservatism of official general fund revenue forecasts in the American states. This study constitutes a novel contribution to both the policymaking and governance literatures in three ways. First, in contrast to the central bank independence (CBI) literature, this study provides a comparative policymaking assessment of the legislature vis-à-vis executive and independent commissions as policymaking institutions. Therefore, this study has implications for determining which institutions, and under what conditions, best limit opportunistic policymaking behavior. Also, the empirical analysis conducted in this study is unique from the cross-national CBI literature because it focuses on governmental units that possess the same type of democratic institutions (i.e., separation of powers) at the subnational level. Policymaking comparisons are thus not fraught with the dilemma of adequately accounting for vast differences in constitutional democracy across governments. Finally, analyzing the relative conservatism of official general fund revenue forecasts in the American states is a critical policy matter given that American state governments place a premium on reliable and valid revenue predictions as the basis for effective fiscal policymaking (e.g., Mikesell, 2007, p. 514; see also Cassidy, Kamlet, & Nagin, 1989; Rodgers & Joyce, 1996). General revenue fund forecasts represent the most critical element of government revenues in the American states because they constitute its largest source (National Association of State Budget Officers [NASBO], 2004, p. 94).[1]

The statistical evidence indicates that assigning policymaking authority to nonlegislative institutions often, but not always, leads to more conservative revenue forecasts in the American states. Specifically, executive branch agencies and independent commissions produce more conservative forecasts with one important exception. Executive branch revenue forecasts are indistinguishable from legislative branch revenue forecasts in those states whose governors are subject to term limits. Governors in states without term limits, however, produce the most conservative revenue forecasts among all policymaking institutions. That is, governors who can be reelected on a continuous basis generate more cautious revenue forecasts because they are much more likely to deal with the political fallout resulting from overly optimistic revenue projections. The evidence also reveals that legislative branch revenue forecasts are the least conservative during periods of unified party control. Legislative branch forecasts become more conservative in times of divided government. Presence of a viable opposition party within the legislature, and to a lesser extent from the executive branch, offers an effective check on the legislature's propensity to make policy choices for myopic electoral or partisan reasons. Executive branch or independent commission institutions are best at limiting opportunistic policymaking behavior, albeit only when a single party controls the legislative branch or when governors face an electoral constraint.

[1] General sales, personal income, and corporate income taxes constitute 76 percent of all state general fund revenues (NASBO, 2004, p. 94). The use of aggregate general fund revenue forecasts is also motivated by data availability constraints. We do not analyze total state revenues because earmarked funds are both forecasted by individual line agencies (Franklin & Douglas, 2003), and further depoliticized due to the nondiscretionary nature of these funds (Patashnik, 2000).

## POLICYMAKING VENUE, POLITICAL CONTEXT, AND REVENUE FORECASTING IN THE AMERICAN STATES

Outside the scope of the CBI literature, little research has examined the efficacy of alternative policymaking institutions for limiting political influence over policymaking. Past research has examined the linkage between agency durability, and the extent to which policy administered by these agencies is insulated from political control by structural design (e.g., Lewis, 2003). Although insulated agencies are more durable than other agencies, conflicting empirical evidence exists regarding whether this insulation actually affects the content of policy outputs directly. Politically independent central banks do a better job of adopting policies that provide stable economic growth by keeping inflation lower than less politically independent central banks (e.g., Cukierman, Webb, & Neyapti, 1992; Waller, 2000). Conversely, Besley and Coate (2003) find that U.S. states with public utility commissions (PUCs) comprised of elected regulators will have lower electricity prices, and are also less inclined to pass along cost increases to the public, compared to state PUCs whose members are appointed regulators. Because elected regulators are both narrowly and directly judged by voters only for specific policy actions relating to electricity rates and rate increases, they can be held easily accountable for their policy actions compared to appointed regulators. Research has yet to directly evaluate how these institutional differences shape the ability of political actors to influence policymaking motivated by short-term partisan or electoral considerations.

Research on government revenue forecasts in the American states has focused on the existence of systematic biases in official revenue forecasts (Bretschneider, Gorr, & Klay, 1989; Feenberg et al., 1989; Rodgers & Joyce, 1996), or whether these forecasts are upwardly biased in election years (Boylan, 2008). Some studies analyze how political insulation and greater representation of interests on consensus group independent commissions can enhance the quality of revenue forecasts (Deschamps, 2004; Klay, 1985; Smith, 2007; Voorhees, 2004), while others restrict their focus to analyzing executive branch revenue forecasts to understand how political appointees and civil service staffs within executive budget agencies shape the quality of policymaking (Krause, Lewis, & Douglas, 2006). Extant studies neither focus on revenue forecast differences across multiple institutions, nor how it is affected by variable political constraints.[2]

### Policy Background

Official revenue forecasts by U.S. state governments constitute a useful empirical setting for examining policymaking variations across different institutions and political contexts. Adoption of an official revenue forecast has both tangible policy and political consequences. As John Mikesell (2007, p. 514) explains, "reliable and trusted revenue predictions provide the foundation for fiscal discipline and for the adoption of an executable budget." Public budgeting is a policymaking process that affects the lives of millions of people, as it determines who benefits from and who pays for government services. Revenue forecasts are the lynchpin that holds this process together because all planning for the upcoming fiscal periods regarding government expenditures and the tax code are based upon these forecasts

[2] In addition, once updating the sample period by six time series observations per state, Krause, Lewis, and Douglas' (2006) analysis of executive branch revenue forecasts comprise only slightly more than 20 percent of the total observations analyzed in the present study. This is due to the fact that the official revenue forecasting authority is controlled by either legislatures or independent commissions (i.e., consensus groups [CGs]) in the vast majority of American states.

(Bretschneider & Gorr, 1992; Feenberg et al., 1989, p. 300; Rodgers & Joyce 1996, p. 457).

U.S. state governments are particularly concerned with ensuring that revenues meet their spending plans because of institutional constraints on deficit spending (Poterba, 1994; Primo, 2007; Rose, 2006).[3] Elected officials use revenue forecasts to estimate the extent to which their taxing and spending plans can be expected to result in fiscal deficits or surpluses. Yet, because actual revenues are uncertain when budgets are passed, revenue forecasts can be manipulated by opportunistic politicians seeking to better satisfy constituents' budgetary demands for lower taxes and increased spending. Conversely, the social welfare costs associated with optimistic revenue forecasts are nontrivial because state governments will often make mid-year cuts or tax increases if actual revenue collections are not meeting the earlier projections (Gold, 1995; Poterba, 1994; Rodgers & Joyce, 1996, p. 49; Shkurti, 1990, p. 80). Policymakers are thus faced with a difficult dual policy-political choice between risky (less conservative) forecasts, which offer short-term political payoffs to constituents, or prudent (more conservative) forecasts that credibly mitigate painful mid-year fiscal adjustments. If policymakers are too conservative they will choose to forego providing electorally valuable tax cuts or spending increases to constituents. If they are too optimistic, they risk painful electoral and (possibly) credit market sanctions. For example, 38 states overpredicted revenues in FY 2009 and were thus forced to make over $31 billion worth of mid-year fiscal adjustments (NASBO, 2009). Alternative strategies such as drawing down surpluses or raising deficit spending can lower credit ratings and increase debt payments and borrowing costs for state governments (Rodgers & Joyce, 1996).[4] Revenue shortfalls can also impose substantial negative political costs. A prominent example occurred in Ohio in the 1980s. In 1974, James Rhodes (OH-R) was elected governor of Ohio on a platform of no tax increases. This political promise, coupled with the economic recession of 1982 to 1983, resulted in accusations that he presented rosy revenue forecasts so that taxes would not have to be increased to balance the budget. Within the first four months of fiscal year 1982, the governor's office was forced to recognize an expected deficit of $1 billion for the biennium. Mid-year spending cuts and temporary tax increases had to be passed by the legislature.[5] Upset by the fiscal crisis, voters swept Democrat Richard Celeste into office in 1982 (Shkurti and Winefordner, 1989).

On a conceptual level, revenue forecasting decisions are representative of a broad class of well-known policy decisions made by government officials. Pharmaceutical drug approval (Carpenter, 2002), licensing of hydroelectric permits (Spence, 1999), and cost estimation of government construction projects (Flyvbjerg, 1998), like revenue forecasting, are examples of government policy decisions where electoral incentives can produce optimistic Type I decision errors. For example, legislators are generally thought to be prone to such Type I policy errors because they possess stronger electoral incentives to offer policies which are popular with their constituents (*high benefits*), while also being able to effectively diffuse responsibility for any blame that occurs when their policy decisions result in adverse outcomes (*low costs*).

[3] More specifically, 49 states have some form of legal requirement to balance their budgets, and all states share the concern that deficit spending can harm their bond ratings.

[4] Revenue shortfalls during the fiscal year raise the eyebrows of the bond rating agencies and often lead to negative sanctions for state governments. During the last three quarters of fiscal year 2002, when many states experienced mid-year deficits as they struggled to come out of the decade's early recession, Moody's downgraded the bond outlooks for 16 states and the bond ratings for two states, while Standard and Poor's downgraded the bond outlooks for 15 states and the bond ratings for three states (Gewehr, 2007).

[5] For details, see Ohio Legislative Budget Office (1982).

Finally, on a measurement level, analyzing general fund revenue forecasts in the American states allows us to assess systematic differences in directly comparable policymaking outputs across institutional venue and political conditions. Researchers can analyze the impact of varying institutional venues and political contexts on forecast conservatism, subject to controlling for other factors reflecting differences in state forecasting difficulty and capacity that also explain forecast performance. Analyzing revenue forecast conservatism thus provides a useful measure of the extent to which partisan or electoral myopic pressures affect policymaking across different institutions and political conditions.[6]

## UNDERSTANDING HOW TO LIMIT POLICY OPPORTUNISM BY POLICYMAKING INSTITUTIONS

Granting policymaking authority to nonlegislative institutions is thought to improve policy decisions by limiting deleterious political influence (Falaschetti & Miller, 2001; Patashnik, 2000; Spulber & Besanko, 1992).[7] Policy opportunism is defined here as *making policy decisions on the basis of short-term political expediency at the expense of long-run sound policy judgment*. Elected officials often have strong short-term incentives to deliver distributive benefits at the expense of lower overall social welfare in the long run.[8] In the realm of fiscal policy, elected officials possess short-run incentives for advocating both subsidies for particular interests and an across-the-board sales tax rate reduction at the expense of creating a fiscal deficit. These myopic incentives exist for both left-of-center (e.g., Democratic) and right-of-center (e.g., Republican) political parties. Specifically, left-of-center politicians generally prefer higher levels of government spending, whereas, right-of-center politicians generally prefer lower levels of taxation (Alt & Lowry, 1994). Incentives for engaging in opportunistic policymaking are strong irrespective of partisan or ideological preferences because all politicians are thought to experience time-inconsistent preferences, which favor short-term politically expedient policies at the expense of long-term policy stability (Persson & Svensson, 1989).

In the realm of fiscal institutions, legislators not only possess stronger incentives to manipulate revenue forecasts to accomplish political goals such as delivering particularistic benefits to their constituencies in the form of direct benefits or tax cuts (Mayhew, 1974), but also are less likely to be held individually accountable by voters for sanguine revenue forecasts because they belong to large collective institutions. Any legislative accountability is made further diffuse because legislative branch official revenue forecasts are typically made by a special committee comprised of members from both legislative chambers.[9] Although governors possess keen electoral incentives to manipulate forecasts just like legislators, governors are more likely to incur steeper political costs for sanguine revenue forecasts, especially when economic conditions are poor and result in painful mid-year fiscal

[6] For purposes of this study, attention is restricted to forecast conservatism and optimism rather than accuracy because our substantive focus is on opportunistic policymaking behavior.

[7] Of course, institutional design choices may instead reflect political struggles among competing factions (Moe, 1989). Nonetheless, our aim is to empirically assess whether institutional design choices explain variations in policymaking behavior.

[8] Distinguishing between particularism versus universalism in policy benefits is beyond the scope of the present study because the aim here is to explain how institutional context affects the degree of aggregate policy opportunism by governmental institutions.

[9] For example, the 16-member (eight House members, eight Senate members) Arizona Joint Legislative Budget Committee is charged with responsibility for making official revenue forecasts (see http://www.azleg.gov/jlbc/jlbcback.htm).

adjustments, as the example of Governor Rhodes above suggests.[10] This is particularly true if governors remain in office when revenue shortfalls transpire because they are unitary institutional actors that face short-term policy pressures when revenue shortfalls arise from rosy fiscal projections. Therefore, governors will incur greater electoral costs vis-à-vis individual legislators from poor fiscal and economic conditions (Lowry, Alt, & Ferree, 1998; Niemi, Stanley, & Vogel, 1995).[11] The greater net benefits associated with legislators engaged in opportunistic policymaking behavior has led legislatures to routinely delegate responsibility for a variety of fiscal policymaking functions, including revenue forecasting, to the executive branch for the dual purposes of enhanced fiscal accountability and efficiency (e.g., Clynch & Lauth, 2006; Schick, 1971, pp. 177–180).[12] Because the legislative branch is less electorally accountable vis-à-vis the executive branch, we posit that legislative branch revenue forecasts should be less conservative than those produced by executive branch officials.

Similarly, executive branch agencies will be more susceptible to myopic political pressures than independent commissions because the former are directed by elected officials and the latter are comprised of unelected policymakers (Lewis, 2003; Moe, 1989). In many states, official revenue forecasts are made by consensus group independent commissions. Typically, consensus group members derive independent forecasts prior to meeting as a collective body, and then arrive at a final adopted forecast that reflects a strong norm of unanimity.[13] Consensus groups are comprised of partisan appointees selected by the political branches, and in some instances, nonpartisan appointees (usually university and private sector economists).[14] The partisan appointees represent some combination of persons selected by the

[10] During his presidential bid, Governor Dukakis (D-MA) experienced an unexpected revenue shortfall that helped to produce an expected deficit of $672 million in fiscal year 1989. Governor Dukakis instituted spending cuts, payment deferrals, and more stringent tax enforcement to fill the gap, plus proposed spending cuts and an income tax increase for fiscal year 1990. These fiscal problems helped the Republicans win the governor's office during the 1990 election (Wallin, 1995).

[11] Niemi, Stanley, and Vogel (1995, p. 939) note that governors can limit their electoral accountability for tax increases by shifting the burden from highly salient revenue sources (e.g., sales, property, or personal income taxes) toward greater reliance on revenue sources that are less transparent to voters (e.g., user fees, sin taxes, and corporate taxes).

[12] In executive branch states, careerist staff comprised of fiscal analysts and economists are responsible for arriving at revenue forecasts, subject to the approval of both governors and their budget directors. Every state's executive branch is responsible for making revenue forecasts, but not all constitute the official forecast. Official forecasts are those used by legislatures as the basis of fiscal policy deliberations. Unofficial forecasts are those generated by a particular agency or institution, but can only be used to further the policy agenda from the source generating such numbers. These data come from both data reported to the National Association of State Budget officers cited in this study, as well as from personal interviews with state officials. More details on these data can be found in Table A7 in the Supplementary Appendix document or by contacting the authors. All appendices are available at the end of the article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[13] Sixteen of the 26 states that possess a consensus group (CG) institution for at least some of the years covered by our sample follow a consensual (i.e., unanimity or near-unanimity) decision rule with a compulsory vote after agreement on the revenue forecast has been obtained. The remaining 10 states possess CGs that employ some type of a majoritarian voting rule to approve the official revenue forecast, but in practice almost always attain unanimity: Delaware, Hawaii, Iowa, Maryland, Mississippi, Nebraska, Nevada, Oklahoma, South Carolina, and Washington.

[14] This also includes executive agencies that are headed by an independently elected chief. Yet, it is rare for elected officials to serve on CGs. For example, out of the 11 states during our sample period that had at least one nonpartisan member, only Louisiana (House Speaker and Senate president) and Mississippi (State Treasurer) have elected officials formally serving on a CG. Electoral pressures on CGs are strongly muted given both the consensual nature of such boards and the paucity of elected officials serving on them.

legislative and executive branches.[15] Moreover, consensus group commissions also diffuse policymaking authority across several competing interests, and elected officials are frequently restricted in the types of persons that can be nominated or appointed (Krause and Douglas, in press). These nomination restrictions can include factors such as experience, professional expertise, background, partisanship, and institutional affiliation in a way that distances commission members from political influence. Although executive branch agencies often require formal policy approval by the chief executive before decisions are made, this is not true for consensus groups. Therefore, consensus group commissions will be the most likely to err on the side of conservative forecasts because there are fewer pressures pushing commissions to be optimistic for political reasons that run counter to the professional norm of forecast conservativism (Rodgers & Joyce, 1996).

The preceding discussion suggests that structural differences affect policymaking institutions' propensity for engaging in opportunistic behavior. Policymaking institutions differ in both the strength of electoral incentives and accountability, and these differences yield different policymaking decisions. This logic yields two hypotheses regarding the relationship between policymaking institutions and policy opportunism:

**$H_1$:** *Legislatures will exhibit relatively greater policy opportunism compared to nonlegislative institutions.*

**$H_2$:** *The executive branch will exhibit relatively greater policy opportunism compared to independent commissions.*

Applied to revenue forecasting, the testable implication of H1 means that legislatures will produce less conservative revenue forecasts than either the executive branch or consensus groups, all else being equal. H2's testable implication is that the executive branch (governors) will yield relatively less conservative revenue forecasts than those produced by consensus groups, all else being equal.

### Political Context: The Role of Term Limits and Partisan Division in the Legislature

This standard view of incumbent politicians' myopic behavior presumes that the net electoral benefits attributable to opportunistic policymaking are necessarily positive, and hence, it is always preferable for an independent commission to be assigned policymaking authority. Yet, this is not always the case because the incentives and capacity of politicians to engage in opportunistic policymaking behavior may vary across institutional venue, thus dampening politically motivated policy myopia in certain instances. For example, institutional differences and political conditions can make it either harder or easier for voters to assign electoral responsibility for poor revenue forecasts, and thus affect politicians' willingness or capacity to manipulate this class of policymaking decisions. Specifically, a policy actor's incentive to behave opportunistically is influenced by the electoral or political benefits such action provides and the extent to which he or she is worried about being held accountable for the outcome in the future (Krause & Corder, 2007; Persson & Svensson, 1989). Therefore, a policymaker facing a shorter electoral horizon should be more susceptible to engage in opportunistic policymaking behavior than

[15] The subsequent policy-specific information was culled from interviews with state government fiscal policy officials from each of the CG states. A detailed list and corresponding source references can be obtained from the corresponding author.

one possessing long-term incentives for responsible policymaking (Besley & Case, 1995).

Whether politicians are held electorally accountable for making poor fiscal choices crucially depends upon whether they will be in office at a later date to bear the consequences of their risky choices. In the case of the American states, this electoral horizon systematically varies according to the presence or absence of term limit restrictions. Legislators and governors that are eligible for seeking reelection are more likely to offer conservative revenue forecasts because they are acutely concerned about the impending fallout associated with unanticipated budgetary shortfalls or tax increases, and perhaps a negative shock in their state's bond ratings. Conversely, elected officials operating under a binding electoral constraint are less apt to be held accountable for poor fiscal choices, and hence, are more likely to manipulate revenue forecasts for myopic reasons than their chief executive counterparts that are not subject to term limits. This is particularly the case when, for example, governors are lame ducks because they will care little about their reputation with voters (Besley & Case, 1995, p. 773). Moreover, recent research extends this accountability logic in novel theoretical directions by demonstrating not only that first-term incumbents perform better in a two-term limit system vis-à-vis a one-term limit system, but also that second-term incumbents who are not subject to term limit restrictions will outperform those incumbents serving in their second and final term in office (Alt, Bueno De Mesquita, & Rose, 2011). Taken together, this logic implies that the net benefits associated with policy opportunism are decreasing in elected officials' potential time horizon in office. This logic yields the following testable prediction:

**$H_3$:** *Governors and legislators subject to term limit restrictions will exhibit relatively greater policy opportunism compared to those that do not face such electoral restrictions.*

Applied to revenue forecasting, the testable implication of H3 is simply that politicians who are subject to term limits will produce less conservative revenue forecasts than counterparts that can continually seek re-election. Moreover, for term limited governors the incentives for the least conservative revenue forecasts should occur during their final lame duck term in office.

Because legislatures are collective institutions by definition, the optimism of legislative branch revenue forecasts relative to other institutions can also depend upon their capacity to collude regarding opportunistic policymaking behavior. For example, the conflict between opposition majorities in each legislative chamber may limit this institution's capacity to engage in opportunistic policymaking behavior through the use of both their formal powers and their ability to publicize opportunistic actions.[16] Put another way, legislative branch forecasts are susceptible to coordination problems arising from required agreement between two legislative chambers under bicameralism, a separation of powers problem that occurs making policy agreement among legislators more difficult when these chambers possess different policy preferences (Huber & Shipan, 2002; Persson, Roland, & Tabellini, 1997).[17] Intra-legislative partisan division constrains opportunistic behavior by legislators because the coordination costs of engaging in such behavior increases, while

[16] Governments' ability to attain their desired partisan revenue and spending policy targets are much easier when political fragmentation between branches is low under unified government vis-à-vis divided government (Alt & Lowry, 1994).

[17] Other benefits associated with partisan fragmentation are policy moderation (Alesina & Rosenthal, 1995) and lower trade tariffs (Sherman, 2002).

the benefits of such a strategy diminish because economic rents accrued are further divided between political parties.[18] This logic predicts how a partisan fragmented legislature reduces its capacity to engage in policy opportunism:

**$H_4$:** *Divided partisan legislatures will exhibit relatively less policy opportunism compared to unified partisan legislatures under either unified or split branch partisan government.*

Applied to revenue forecasting, the testable implication of H4 simply suggests that legislatures will produce more conservative revenue forecasts under split partisan control of the legislature compared to when this political branch is controlled by a single party either under unified or divided government, ceteris paribus.

## DATA, VARIABLES, AND STATISTICAL METHODS

To test these hypotheses, we use data on the official general fund revenue forecasts of the 50 U.S. states between 1987 and 2008 (observations = 1,097, states = 50; average years = 21.94). Our dependent variable is the *percentage forecast error* (PFE) measured as [(actual state general fund revenues – projected state general fund revenues) / actual state general fund revenues] × 100.[19] The average PFE is 2.66 percent (SD = 7.23) with the highest state (Alaska) averaging an 11.29 percent underprediction of revenues and the lowest state a 1.48 percent overprediction of revenues (Michigan). Positive (negative) values of PFE reveal forecast conservatism (optimism) and are a good measure of the extent to which political pressures influence forecasts. The policymaking institutions responsible for generating these forecasts vary across states, yet do not exhibit any discernible geographic nor state size systematic patterns.

The average PFE by institutional venue and political context is displayed in Table 1. To begin, states with legislative branch forecasts (32.5 percent of sample cases) should produce the most optimistic forecasts—that is, smallest PFE—because legislatures will feel the most pressure to produce optimistic forecasts for political expediency. The average PFE in *Legislative Branch* states translates into the legislature producing an official general fund revenue forecast that underestimates revenues by 2.27 percent (corresponding to an average $ 217 million underprediction). By contrast, the average PFE in *Executive Branch* states (20.7 percent) underestimates revenues by 2.93 percent (corresponding to an average $342 million underprediction). The average PFE in *Consensus Group* states (46.5 percent of sample cases) underestimates revenues by 2.81 percent (corresponding to an average $ 177 million underprediction).[20] Consistent with expectations, *Legislative Branch* states produce the least conservative forecasts suggesting less insulation from myopic electoral and partisan pressures and less direct political accountability for poor forecasts. *Consensus Group* states produce slightly more optimistic forecasts than *Executive Branch* states, which is somewhat surprising, but this finding makes more sense when

[18] Legislators may actually be more inclined to engage in opportunistic policymaking in a fragmented institutional environment compared to a unified one because it is harder for voters to assign responsibility to political actors through electoral sanctions and rewards (Powell & Whitten, 1996). For example, Lowry, Alt, and Ferree (1998) find that politicians are more heavily sanctioned for deviating from partisan fiscal goals under unified government vis-à-vis divided government. Yet, this alternative logic is less germane to legislatures incurring weak electoral sanctions because of collective decisionmaking and plurality of interests (Falaschetti & Miller, 2001).

[19] Our sample consists of three missing cases in which revenue forecasts are not available (Alabama FY 1996, Alaska FY 1987, and Pennsylvania FY 2004).

[20] The dollar amounts are smaller for CGs, on average, because they represent smaller states.

**Table 1.** Percentage forecast error (PFE) by institutional setting and political context.

| Dependent variable | Observations | PFE |
|---|---|---|
| PFE | 1,097 | 2.66 |
| Institutional setting | | |
| Legislative Branch | 358 | 2.27 |
| Legislative Branch: Term limit (0,1) | 54 | 1.75 |
| Legislative Branch: No term limit (0,1) | 304 | 2.36 |
| Executive Branch | 228 | 2.93 |
| Executive Branch: Term limit—lame duck (0,1) | 75 | 1.43 |
| Executive Branch: Term limit—no lame duck (0,1) | 81 | 2.45 |
| Executive Branch: No term limit (0,1) | 72 | 5.03 |
| Consensus Group | 511 | 2.81 |
| Political context | | |
| Legislative Branch: Unified Government (0,1) | 154 | 2.02 |
| Legislative Branch: Split Branch: Unified Legislature (0,1) | 113 | 2.32 |
| Legislative Branch: Split Branch: Divided Legislature (0,1) | 91 | 2.62 |

*Note:* Data are official general fund revenue forecasts of the 50 U.S. states between 1987 and 2008 (observations = 1,097, states = 50; average years = 21.94). Dependent variable is the percentage forecast error (PFE) measured as [(actual state general fund revenues – projected state general fund revenues) / actual state general fund revenues] × 100. Higher values indicate that a state underestimated revenue and, thus, a more conservative forecast.

*Executive Branch* states are disaggregated into states with and without gubernatorial term limits, which we address below.

We disaggregate *Legislative Branch* states by whether or not states have legislative term limits and political context to test H3 (re: term limits) and H4 (re: divided partisan legislatures). The average PFE for *Legislative Branch* states without term limits is 2.36 percent compared to 1.75 percent for *Legislative Branch* with term limits, suggesting initial support for the claim that legislators with shorter electoral horizons are more likely to make optimistic forecasts. The optimism of forecasts in *Legislative Branch* states also varies by political context. The average PFE in *Legislative Branch* states is 2.02 percent, 2.32 percent, and 2.62 percent for unified government, divided government with a unified legislature, and divided government with a split legislature, respectively. The simple differences in average PFE indicate that political context may constrain the political discretion with which legislatures make forecasts. When an opposition party controls either one of the chambers or the governorship, states produce more conservative forecasts on average.

As H1 and H2 suggest, *Executive Branch* forecast states generate larger than average PFEs (i.e., systematically more conservative forecasts) than *Legislative Branch* states because governors are more likely to suffer political sanction for poor forecasts. To account for the variable incentives that governors possess for engaging in policy opportunism via official revenue forecasts captured by H4, we can disaggregate the *Executive Branch* variable by state-year to capture distinctions among (1) governors that are not subject to term limits (6.56 percent of sample cases); (2) governors that are subject to term limits, but are not serving their lame duck term in office according to their state's constitution (8.57 percent of sample cases); and (3) governors serving their lame duck term in office in accordance with their state's constitution (7.66 percent of sample cases). As Table 1 indicates, states with *Executive Branch* forecasts but no term limits have a higher average PFE (i.e., most conservative forecasts) than *Legislative Branch* states and *Consensus Group* states. Governors in these states are most likely to be held accountable for poor forecasts. When states with and without term limits are distinguished, the average PFE is 5.03 percent for nonterm limit states, but is equal to 2.45 percent for term limit states

where the governor is not a lame duck and 1.43 percent for term limit states where the governor is a lame duck. These average PFE differences are notable because term limit states with *Executive Branch* forecasts produce outputs similar to states with legislative branch forecasts, while nonterm limit states produce significantly more conservative forecasts. *Consensus Group* states produce the most conservative forecasts, except for *Executive Branch* states where the governor has a lengthy electoral horizon. In sum, the simple examination of PFE reveals simple patterns along the lines suggested by H1 to H4. Of course, there are other factors that may be correlated with both institutional design and political context that are also correlated with PFE. This necessitates more fully specified econometric models that control for other political, economic, and state institutional factors suggested by the logic and the extant literature.

### Political Control Variables

Because numerous other political factors influence the quality of states' official general fund revenue forecasts, we account for these confounding factors by estimating econometric models that include a series of political control covariates (Table 2). Because governors, unlike legislators, are held accountable for economic conditions (Niemi, Stanley, & Vogel, 1995), they may possess the short-term cyclical incentive to politically distort revenue forecasts for electoral purposes by avoiding tax increases or spending cuts that are unpopular with voters in election years (Boylan, 2008). We include a binary indicator to account for the electoral pressures that arise in gubernatorial election years (The Council of State Governments, 1986–2009). In addition, we control for state government ideology using the Berry et al. (2010) NOMINATE-based version of their original state government ideology scores. Yet, because both Democratic and Republican elected officials have incentives for rosier forecasts for purposes of increasing spending (Democrats) or lowering taxes (Republicans), we also account for how political-based competitive pressures affect these official state revenue forecasts. We account for partisan-ideological competition among the states by including a folded version of Berry et al.'s (2010) NOMINATE-based measure of state government ideology which is simply equal to |50 – *State Government Ideology*|, so that larger deviations from 50 indicate more extreme liberal or conservative state governments. Our expectation is that more extreme ideological states will produce more optimistic forecasts to more fully support ideological goals such as tax cuts or increased spending. We account for any potential partisan differences in official state revenue forecasts attributable to partisan control of political institutions using three separate binary indicators: *Governor's Party* equals +1 under Democratic control, = –1 under Republican control, and = 0 when controlled by a third party or independent (The Council of State Governments, 1986–2009), *House Party* which equals +1 when the Democratic party controls a majority of seats in the lower legislative chamber, = –1 when the Republican party has majority control, and = 0 when neither party has a majority (The Council of State Governments, 1986–2009), and Senate Party equals +1 when the Democratic party controls a majority of seats in the upper legislative chamber, = –1 Republican party has majority control, and = 0 when neither party has a majority (The Council of State Governments, 1986–2009).

### Economic Control Variables

State economic conditions can also influence revenue forecasts. We control for state-level economic conditions, in the form of personal income growth (i.e., the percentage change in the state's real per capita income from the preceding year; U.S. Department of Commerce, BEA: http://www.bea.gov/regional/statelocal.htm)

**Table 2.** Panel variance descriptive statistics for covariates (FY 1987 to FY 2008).

| Independent variable | Mean | Between standard deviation | Within standard deviation | Between SD / within SD ratio | Modeling choice |
|---|---|---|---|---|---|
| *Institutional venue and political context* | | | | | |
| Legislative Branch | *0.32* | *0.454* | *0.130* | *3.50* | *Time-invariant* |
| *Legislative Branch: Term limit* (0,1) | 0.05 | 0.143 | 0.170 | 0.84 | Time-variant |
| *Legislative Branch: No term limit* (0,1) | 0.27 | 0.396 | 0.212 | 1.87 | Time-variant |
| *Legislative Branch: Unified Government* (0,1) | 0.14 | 0.252 | 0.244 | 1.03 | Time-variant |
| *Legislative Branch: Split Branch: Unified Legislature* (0,1) | 0.10 | 0.278 | 0.271 | 1.03 | Time-variant |
| *Legislative Branch: Split Branch: Divided Legislature* (0,1) | 0.08 | 0.133 | 0.238 | 0.56 | Time-variant |
| Executive Branch | *0.20* | *0.383* | *0.136* | *2.82* | *Time-invariant* |
| *Executive Branch: Term limit—lame duck* (0,1) | 0.07 | 0.178 | 0.174 | 1.03 | Time-variant |
| *Executive Branch: Term limit—no lame duck* (0,1) | 0.07 | 0.203 | 0.168 | 1.21 | Time-variant |
| *Executive Branch: No term limit* (0,1) | *0.06* | *0.240* | *0.058* | *4.13* | *Time-invariant* |
| *Control variables* | | | | | |
| *Gubernatorial Election Year* (0,1) | 0.25 | 0.049 | 0.433 | 0.11 | Time-variant |
| *State Government Ideology* | 50.63 | 9.182 | 8.809 | 1.04 | Time-variant |
| *\|50-State Government Ideology\|* | 10.63 | 3.577 | 5.945 | 0.60 | Time-variant |
| *Change in real personal income*$_{t-1}$ | 3.21 | 0.859 | 2.130 | 0.40 | Time-variant |
| *Economic growth volatility* | 1.69 | 0.918 | 1.182 | 0.78 | Time-variant |
| *Fiscal slack (rainy day + surplus funds)* | 5.97 | 9.948 | 9.628 | 1.03 | Time-variant |
| *Governor's party* (−1, 0, +1) | −0.04 | 0.465 | 0.876 | 0.53 | Time-variant |
| *House partisan control* (−1, 0, + 1) | 0.19 | 0.750 | 0.615 | 1.22 | Time-variant |
| *Senate partisan control* (−1, 0, +1) | 0.12 | 0.767 | 0.604 | 1.27 | Time-variant |
| *Binding forecast* (0,1) | *0.41* | *0.469* | *0.164* | *2.86* | *Time-invariant* |
| *Balanced budget restrictions* (0,1) | *0.52* | *0.505* | *0.000* | *Strictly time-invariant* | *Time-invariant* |
| *Biennial budget* (0,1) | *0.41* | *0.479* | *0.125* | *3.83* | *Time-invariant* |
| *Relative Reliance on sales tax revenue* | *0.23* | *0.111* | *0.019* | *5.85* | *Time-invariant* |
| *Revenue or expenditure limit* (0,1) | *0.43* | *0.454* | *0.209* | *2.17* | *Time-invariant* |

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and economic growth volatility (i.e., the three-year lagged moving standard deviation in real gross state product growth; U.S. Department of Commerce, BEA: http://www.bea.gov/regional/statelocal.htm). Policymakers should make rosier revenue forecasts in response to robust economic growth, while offering more conservative revenue forecasts as the state's economy becomes more volatile if they are risk averse. Accounting for economic conditions ensures that revenue forecast differences observed here across varying institutional and political contexts are independent from the policy conditions confronting those policymakers responsible for making revenue forecasts.

### State Institutional Control Variables

State budget processes vary substantially in ways that can also influence revenue forecasts. For instance, we account for a state's fiscal slack because states with large levels of fiscal slack can afford to produce more optimistic revenue estimates. Our measure is the combined size of the state's rainy day and surplus general funds as a percentage of actual general fund revenues (NASBO, 1986–2009). We also include a measure that accounts for whether the state is legally bound to the official revenue forecast in the construction of their budget (obtained via interviews with state fiscal officers conducted by the authors) because such restrictions could either increase incentives to manipulate the forecast or, conversely, create further pressure to bring revenues in line with expenditures. The existence of a binding forecast requirement provides incentives for policymakers to make more sanguine revenue forecasts because doing otherwise—that is, creating conservative revenue forecasts—can lock-in states to budget and spend less than they truly prefer. In other words, the binding forecast restriction can cause policymakers to strategically raise the budget ceiling via the provision of rosier revenue forecasts. We also control for the extent to which states possess stringent balanced budget restrictions, where it equals 1 if they have a zero deficit carryover restriction in a given fiscal cycle, plus either a statutory or constitutional provision for a balanced budget, 0 otherwise.[21] The logic for this hypothesis is simple. Greater balanced budget restrictions force states to make arbitrary or politically costly fiscal decisions (e.g., across the board cuts) when budgets are shown to be out of balance (Cassidy, Kamlet, & Nagin, 1989). We account for whether a state operates under a biennial budgeting cycle (NASBO, 1987–2010). Our expectation is that states with biennial budget cycles will typically create more pessimistic revenue forecasts because they confront more uncertainty than under an annual budget cycle scenario. The proportion of general fund revenues that come from the sales tax for each state in a given year (The Council of State Governments, 1986–2009; U.S. Census Bureau, n.d.) is accounted for because sales tax is a relatively stable revenue source when it is restricted to general sales taxes.[22] States with a greater dependence upon general sales tax revenues should have an easier time predicting their total general fund revenues.[23] Finally, a binary indicator is included to account for the presence of tax and expenditure limitations (TELs) because this

[21] *Source*: U.S. General Accounting Office (1993). These data were updated using National Conference of State Legislatures (1996 [2004]).

[22] Crain's (2003) evidence of sales tax revenue instability is a byproduct of his decision to combine highly volatile *selective* sales tax revenues (applied to goods and services exhibiting high income elasticity) with stable *general* sales tax revenues.

[23] One feature of state budget processes that may influence the quality of revenue forecasts is the presence of competing revenue forecasts by different actors. However, this issue is not problematic for two reasons. First, all nonexecutive branch institutions possessing formal authority to generate revenue forecasts face a competing forecast from the executive branch by definition. In turn, this means that any observed differences among these nonexecutive branch institutions are attributable to

fiscal rule should lead to less optimistic forecasts because TELs are intended to restrict spending (National Conference of State Legislatures, 1986–2008). TELs place caps on increases in government revenue or spending to a fixed numerical target and have been shown to limit revenue growth in the states (Skidmore, 1999).

### Statistical Methods

Because many of the key predictors of official state revenue forecasting performance rarely change through time, use of standard least-squares dummy variables cross-sectional fixed effects modeling is inappropriate. This is because accounting for unit (fixed) effects will necessarily net out any unique variation from a strictly time-invariant covariate that is fixed for a given panel throughout the entire sample period.[24] We deal with this identification problem by employing a variant of the Hausman–Taylor instrumental variable (IV) regression estimator (Hausman & Taylor, 1981) that is equivalent to the fixed-effects variance decomposition (FEVD) estimator proposed by Plumper and Troeger (2011). The identification assumption for this estimator treats the time-invariant covariates, which change little or none through time as being exogenous (i.e., uncorrelated with unit-specific panel intercepts), while time-varying covariates are assumed endogenous (i.e., correlated with the unit-specific panel intercepts) (Breusch et al., 2011, p. 166). This is a reasonable assumption in settings with aggregated panel units (e.g., nations, states) because time-invariant covariates in aggregated panels are most likely to explain why states may systematically differ from one another with respect to revenue forecasting performance (e.g., see Beck, 2011, pp. 121–122).[25] Technical details of this estimation procedure are reported in the section titled Technical Details of Hausman–Taylor IV Variant of FEVD Estimation in the Appendix.[26]

Distinguishing between what covariates should be treated as being time-variant from time-invariant is a decision made by the researcher based on statistical properties. Plumper and Troeger (2007, p. 137) offer a minimalist rule of thumb that time-invariant covariates must at least contain a between variance that exceeds the within variance (i.e., between–within variance ratio $> 1$). Covariates possessing a between–within variance ratio equal or greater than 2.0 are treated as time-invariant regressors. A listing of the panel descriptive statistics in Table 2 reveals which covariates are treated as time-invariant from those that are handled as time-variant. We are especially confident of our time-invariant treatment of the institutional variables because these covariates possess between–within variance ratios that are rather high (*Legislative Branch*: 3.25; *Executive Branch*: 2.64; *Executive Branch: No Term Limit*: 3.53).[27]

institutional venues, and not the presence of a competing forecast. Because there are no state-years where the executive branch produces the official revenue forecast in the presence of competing government forecasts, any observed differences involving revenue forecast conservativism between term limited and term unrestricted governors cannot be attributed to whether or not a competing forecast exists.

[24] For strictly time-invariant covariates, standard unit fixed-effects models are unidentified because the standard-rank condition assumption is not met (Wooldridge, 2002, p. 269).

[25] The time-invariant covariates capturing institutional design and rule-based differences allow for systematic variation across the American states in unique combinations, as opposed to varying in idiosyncratic ways.

[26] All appendices are available at the end of the article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[27] We also estimate these statistical models using different estimation procedures that are described and reported in the Appendix. All appendices are available at the end of the article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

One possible concern here is that institutional structure is endogenous to the forces determining the quality of revenue forecasts. In this study, we treat institutional structure as being exogenous to policy performance both theoretically and empirically. A careful examination of our data reveal possible evidence of endogenous institutional change in only one instance (Louisiana) out of 13 potential cases where a state altered its institutional venue within our sample period. To address this concern, we also estimate a series of IV models to verify the robustness of the results. We discuss this issue in the section titled Assessing Potential Endogeneity via Within Sample Period Changes of Institutional Venue in the Appendix.[28]

## STATISTICAL FINDINGS

The regression results reported in Table 3 generally confirm the hypotheses above, albeit with some interesting nuances. Although entrusting forecasting authority to nonlegislative institutions can generate more conservative revenue forecasts, the effectiveness of institutional venues can hinge on the incentives and capacity for manipulating revenue forecasts. The first model simply compares *Legislative Branch* states and *Executive Branch* states to the base category, *Consensus Group* states. The second model disaggregates *Executive Branch* states into those with and without term limits and accounts for whether the governor is a lame duck. The third model disaggregates *Legislative Branch* states into those with and without term limits. The final model disaggregates *Legislative Branch* states by political context—unified government, divided government with split branches (but unified legislature), and divided government with a divided legislature. The baseline category in all models is *Consensus Group* states.[29]

### Political Control Variables

The regression results are reported in Table 3. Not surprisingly, gubernatorial election years produce more optimistic forecasts. Forecasters are estimated to produce forecasts about 1.59 percent to 1.66 percent higher in gubernatorial election years (negative coefficients indicate less conservative forecasts), suggesting that electoral pressures predictably influence the content of forecasts. Regular electoral pressures to manipulate forecasts are one reason why elected officials have incentives to delegate forecasting responsibility to actors less influenced by electoral pressures. The ideology or partisanship of states and their elected officials had no discernible influence on forecast conservatism. We could not reject the null that variables measuring state or elected official ideology had no influence on forecast outcomes. This is further evidence that liberals and conservatives, Democrats and Republicans, can each have an incentive to produce rosy forecasts to justify either increased spending or tax cuts.

[28] All appendices are available at the end of the article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[29] Disaggregating these institutional venue and policy contexts is equivalent to interacting institutional venue with term limits and political context. However, numerical comparison for a given institutional venue between the aggregate institutional venue estimates in Model 1 with the disaggregate institutional venue estimates displayed in Models 2 through 4 are not directly comparable because they are based on different model specifications.

**Table 3.** State general fund revenue forecasting error conservatism by policymaking venue (FY 1987 to FY 2008).

| | Model 1 | | Model 2 | | Model 3 | | Model 4 | |
|---|---|---|---|---|---|---|---|---|
| | Coef | SE | Coef | SE | Coef | SE | Coef | SE |
| Institutional venue and political context | | | | | | | | |
| Legislative Branch (0,1) | −3.39** | 1.49 | −3.84** | 1.57 | – | – | – | – |
| Leg. Branch: Term limit (0,1) | – | – | – | – | −6.26* | 3.41 | – | – |
| Leg. Branch: No term limit (0,1) | – | – | – | – | −5.41* | 2.97 | – | – |
| Leg. Branch: Unified Government (0,1) | – | – | – | – | – | – | −7.98** | 3.26 |
| Leg. Branch: Split Branch: Unified Leg. (0,1) | – | – | – | – | – | – | −5.39 | 3.33 |
| Leg. Branch: Split Branch: Divided Leg. (0,1) | – | – | – | – | – | – | −5.60* | 3.00 |
| Executive Branch (0,1) | −0.71 | 1.59 | – | – | – | – | – | – |
| Exec. Branch: Term limit—lame duck (0,1) | – | – | −4.39* | 2.39 | −4.40* | 2.35 | −4.54** | 2.37 |
| Exec. Branch: Term limit—no lame duck (0,1) | – | – | −2.56 | 2.60 | −2.56 | 2.56 | −2.67 | 2.58 |
| Exec. Branch: No term limit (0,1) | – | – | 1.99 | 2.60 | 1.59 | 2.73 | 1.35 | 2.82 |
| Controls and ancillary parameters | | | | | | | | |
| Gubernatorial Election Year (0,1) | −1.59** | 0.41 | −1.62** | 0.41 | −1.61** | 0.41 | −1.66** | 0.41 |
| State Government Ideology | 0.08 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.08 | 0.07 |
| \|50-State Government Ideology\| | 0.05 | 0.04 | 0.05 | 0.04 | 0.05 | 0.04 | 0.10** | 0.05 |
| Governor party (−1,0,1) | −0.52 | 0.67 | −0.44 | 0.67 | −0.43 | 0.67 | −0.54 | 0.67 |
| Lower house party (−1,0,1) | −0.69 | 0.49 | −0.63 | 0.49 | −0.64 | 0.49 | −0.63 | 0.50 |
| Upper house party (−1,0,1) | 0.03 | 0.48 | 0.01 | 0.48 | 0.02 | 0.48 | −0.12 | 0.48 |
| Change in real personal income$_{t-1}$ | 0.49** | 0.10 | 0.49** | 0.10 | 0.49** | 0.10 | 0.50** | 0.10 |
| Economic growth volatility | −0.15 | 0.20 | −0.13 | 0.20 | −0.13 | 0.20 | −0.11 | 0.20 |
| Fiscal slack (rainy day + surplus funds) | −0.18** | 0.03 | −0.18** | 0.03 | −0.18** | 0.03 | −0.18** | 0.03 |
| Binding forecast (0,1) | −2.30* | 1.37 | −2.67* | 1.36 | −3.38* | 1.93 | −3.74* | 2.01 |
| Balanced budget restrictions (0,1) | 1.64 | 1.21 | 1.56 | 1.19 | 1.62 | 1.21 | 1.83 | 1.25 |
| Biennial budget (0,1) | −0.30 | 1.21 | −0.53 | 1.23 | −0.46 | 1.26 | −0.49 | 1.30 |
| Relative Reliance on sales tax revenue | −14.70** | 5.45 | −15.92** | 5.34 | −17.04** | 5.74 | −17.77** | 5.95 |
| Revenue or expenditure limit (0,1) | 2.56** | 1.20 | 2.23* | 1.20 | 2.63* | 1.36 | 2.75* | 1.41 |
| Constant | 2.29 | 3.09 | 3.64 | 3.06 | 4.56 | 3.20 | 4.07 | 3.27 |
| *F*-statistic | 5.30** | | 5.20** | | 4.89** | | 4.88** | |
| Adjusted $R^2$ | 0.16 | | 0.16 | | 0.16 | | 0.17 | |
| $\rho$ | 0.26 | | 0.26 | | 0.26 | | 0.26 | |

*Note:* $N \times T = 1{,}042$. Dependent variable is [(actual state general fund revenues – official projected state general fund revenues) /actual state general fund revenues] × 100. Hausman–Taylor FEVD estimation with AR(1) serial correlation correction. D-W statistics after the transformation are 1.86, 1.87, 1.87, 1.87. Estimates of $\eta$ are 0.93, 0.93, 0.93, and 0.93, respectively.

*Significant at the 0.10 level; **significant at the 0.05 level (two-tailed tests).

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### Economic Control Variables

The features of states' economies also had some influence on forecast conservatism. Growth in real state personal income predictably leads policymakers to underestimate revenues relative to forecasts. Higher levels of economic growth make it more likely that forecasters underestimate revenues. More generally, however, economic growth volatility has no discernible influence on forecast conservatism. It is conceivable that states dampen the effects of economic volatility through augmenting their rainy day funds or increase reliance on more stable sources of revenue such as sales taxes.

### State Institutional Control Variables

State budget processes also appear to influence forecast conservatism in interesting ways. Some caution should be taken in the interpretation of these estimates, however, because the rules and restrictions can be more complex than modeled here. In addition, some aspects of state budgetary differences may be correlated with the inherent difficulty of revenue forecasting in the state. We deal with this issue in the Appendix with regard to the key institutional differences, but some caution should be taken in the interpretation of these ancillary covariates.[30] Fiscal slack (in the form of rainy day and surplus general funds) is estimated to result in more sanguine revenue forecasts. One reason this may be the case is that forecasting errors are less costly when the state has more fiscal slack. States possessing rules that bind them to official forecasts are estimated to produce 2.3 percent to 3.7 percent more optimistic revenue forecasts than those that do not face such fiscal restrictions. These results suggest that bright line fiscal rules might create incentives for forecasters to manipulate forecasts to compensate for a loss of flexibility in other parts of the budgetary process. States with strict balanced budget restrictions are estimated to generate more conservative forecasts, suggesting that such rules make forecasters leery of underestimating revenues. Although the estimates are in the expected direction, however, they are also imprecise. This may be due to the fact that there is quite a bit of variation among states in what balanced budget requirements entail (Primo, 2007; Rose, 2006). Specifically, although 49 states possess at least a formal balanced budget restriction (Vermont being the lone exception), only 29 states possess zero deficit carry over laws according to both the Government Accountability Office (1993) and National Conference of State Legislatures (1996 [2004]). Interestingly, forecasters in states with biennial budgets are estimated to produce no more conservative forecasts than states with yearly budget cycles. State budgets that rely more on sales tax revenue sources are estimated to be significantly more optimistic, indicating that more stable revenue sources allow forecasters to be less conservative. Of course, state reliance on different revenue sources may be influenced by the volatility of revenue forecasting itself. To the extent that states where revenues are difficult to forecast rely more on sales tax, these estimates may underestimate the true effect of reliance on sales tax revenues. Finally, states with caps on revenue or spending growth are estimated to produce more conservative forecasts by 2.2 percent to 2.8 percent.

[30] All appendices are available at the end of the article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

## Understanding When Politics Limits Policy Manipulation by Fiscal Institutions

The central findings of this study reveal clear support for H1 that legislative institutions are more likely to respond to myopic electoral or partisan pressures in revenue forecasting. Legislative branch official revenue forecasts are estimated to be 3.39 percent more optimistic relative to those produced by consensus groups (captured in the baseline intercept) and 2.68 percent more optimistic than those generated from executive branch institutions (Model 1).[31] However, the legislative branch's relative optimism vis-à-vis the executive branch is noticeably stronger only in states where governors are not constrained by the existence of term limits (Models 2 to 4). On average, Model 2 indicates that the legislative branch provides a 5.8 percent ($F_{[1, 1024]} = 4.95$, $P = 0.026$) higher revenue forecast than does a governor in a state without term limits. In states with term limits, however, the difference is more modest. In term limit states, governors produce forecasts that are about 1.28 percent more conservative, provided they are not lame ducks. Lame duck governors in term limit states are actually estimated to be slightly more optimistic (0.55 percent) than the legislature. This, in turn, suggests that governors with short-term horizons do not feel compelled to make long-term sustainable policy commitments by making conservative revenue forecasts that will ensure sufficient surplus government resources.[32]

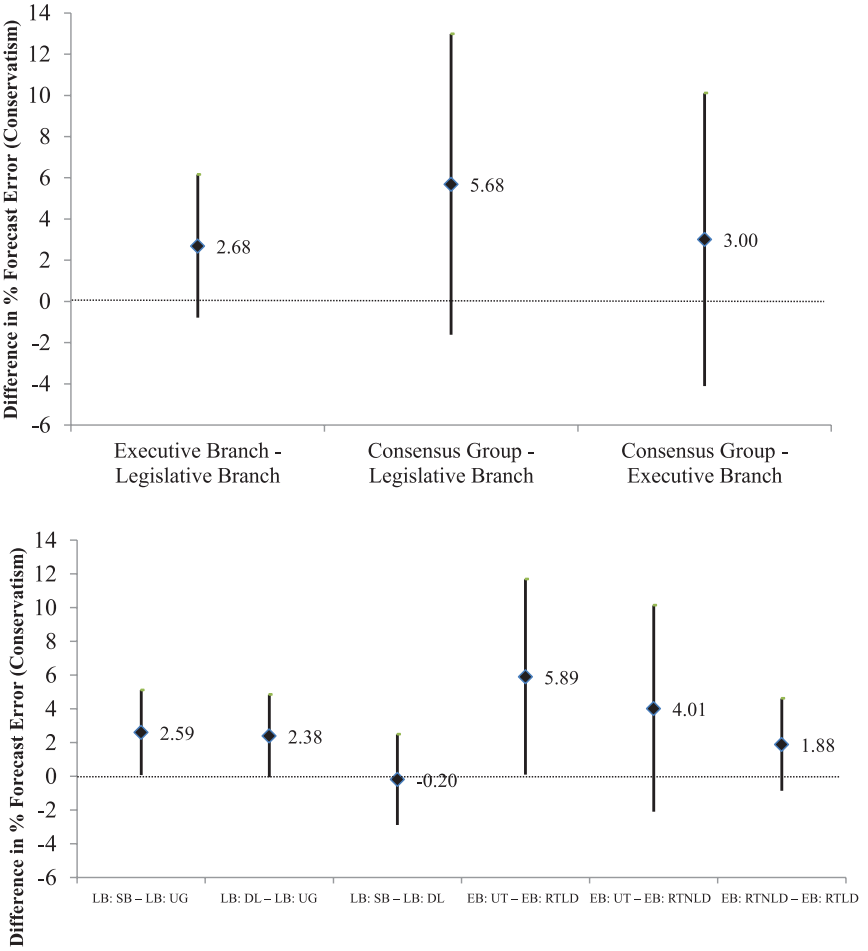
Translating the PFEs into dollars for an average state clarifies the policy implications arising from institutional differences. In a state with average total general fund revenues of about $5 billion during this sample period (e.g., South Carolina), differences in institutional venue are estimated to lead to over and underpredictions in revenue forecasts amounting to hundreds of millions of dollars. Legislative branch forecasts during periods of unified government are estimated to be too optimistic to the tune of about $94 million, while gubernatorial forecasts in states without term limits are estimated to underpredict revenues by over $300 million.

These revenue forecasting differences by institution and political context are summarized in Figure 1. The diamonds are the differences in estimated forecast error between the pairs of institutions (and political contexts) listed on the $x$-axis. The vertical lines are the confidence intervals around Wald coefficient equality restriction tests. The estimates are from Model 1 and Model 4. Larger values reflect larger differences between institutions and political contexts in forecast conservatism. So, for example, starting at the top, the panel compares differences across institutions and shows that executive branch forecasters produce 2.68 percent more conservative revenue forecasts than legislative branch forecasters (first column), but 3.00 percent less conservative revenue forecast than consensus group forecasters (third column).

Figure 1 also helps clarify H2 and evaluate H3 and H4 by testing differences in revenue forecast conservatism by the presence of term limits and different political contexts (unified government and divided government under split and divided legislatures). To begin, consider the executive branch's revenue forecasting, which

[31] When splitting the sample into unified Democratic control and unified Republican control groups, the results indicate the legislative branch produces significantly less conservative revenue forecasts vis-à-vis the executive branch and CGs commissions in the former case, but do not do so in the latter case (where the point estimates indicate that both political institutions exhibit more forecast conservatism than CGs, but the null hypothesis of no difference cannot be rejected at conventional significance levels). These findings can be obtained directly from the data replication materials or by contacting the authors.

[32] Additional statistical analysis fails to uncover any evidence of revenue forecast conservatism for each electoral institution (legislature and governor) significantly differing between election versus nonelection years. These findings can be obtained directly from the data replication materials or by contacting the authors.




*Note:* Estimates and standard errors reflect differences in coefficient values in regressions from Models 1, 4 in Table 3. Vertical bars are 95 percent confidence intervals around Wald coefficient equality restriction tests. LB = *Legislative Branch*, UG = *Unified Government*, SB = *Split Branch* (unified legislature), DL = *Divided Legislature*, EB = *Executive Branch*, UT = *Unrestricted Terms* (no term limits), RT = *Restricted Terms* (Term Limits), LD = *Lame Duck Governor*, NLD = *Not a Lame Duck*.

**Figure 1.** Differences in Estimated Forecast Conservatism by State Forecasting Institution and Political Context.

displays interesting patterns. Limited support is obtained for H2 that the executive branch will produce more optimistic forecasts than independent commissions (consensus groups) for those governors in states with term limits. Models 2 to 4 suggest that this difference is largest when term limits are binding (i.e., governors are lame ducks). In term limit states, governors are estimated to produce forecasts that are about 4.5 percent or 2.7 percent more optimistic than consensus groups, for lame duck and nonlame duck governors, respectively. In states where governors are not subject to term limits, however, executive branch forecasters are estimated to produce 1.35 percent more pessimistic revenue forecasts than those generated

by consensus groups.[33] In an average state, gubernatorial forecasts are estimated to produce forecasts that are $130 million to $230 million more optimistic than what the state's official forecast would be coming from a consensus groups unless the state does not have term limits. In states without term limits, the gubernatorial forecast is estimated to be close to $70 million more conservative than consensus groups. Only the lame duck governor estimates are sufficiently precise to reject the null that executive branch forecasts are comparable to consensus group forecasts. Governors facing term limits, especially lame ducks, are considerably more susceptible to electoral or partisan influence in forecasting than executive branch counterparts facing longer electoral horizons due to an absence of term limit restrictions.[34]

Both legislative and executive branch actors subject to term limits behave differently than those not subject to such restrictions, particularly when they are lame ducks. The results provide clear support for H3. Model 3 estimates suggest that legislatures in term limit states produce forecasts that are 0.85 percent less conservative than legislatures in nonterm limit states, yet this difference is not statistically discernible at conventional significance levels ($F_{[1,\ 1023]} = 0.29$, $P = 0.591$). Similarly, governors in term limit states produce forecasts that are 4.01 percent to 5.89 percent more optimistic than governors in states without term limits, with governors in their lame duck years being the most optimistic. These Wald test results are only statistically distinguishable in the lame duck year ($F_{[1,\ 1023]} = 4.48$, $P = 0.035$), but the estimates clearly suggest that shorter electoral horizons may provide politicians incentives to produce more optimistic forecasts. These differences are graphed in the bottom panel of Figure 1.

Interestingly, although the estimated effects in the base model (Model 1) suggest that legislative branches produce more optimistic revenue forecasts than nonlegislative institutions on average, these findings mask how the political environment can also constrain opportunistic behavior. Estimates from Model 4 indicate that the legislative branch produces more optimistic forecasts during times of unified partisan control. Specifically, legislative branch revenue forecasts under a unified government are significantly higher by 2.59 percent and 2.38 percent than those generated under divided government, either split branches with a unified legislature or split branches due to a divided legislature (Figure 1, bottom panel). If we were to apply these percentages to revenue forecasts in an average state, legislative forecasts are estimated to be $120 to $130 million more optimistic during periods of unified government. Clearly, not all legislative branch revenue forecasts exhibit the same level of policy opportunism. The revenue forecasts generated under unified government are considerably more optimistic. During periods where the governor does not share the partisanship of one or both of the chambers in the legislature, however, revenue

[33] An alternative explanation for the correlation between term limits and forecast optimism is that term-limited governors systematically possess less experience producing revenue forecasts. Ancillary analysis controlling for forecast experience (denoted as the number of consecutive years the governor has served in office) indicates that executive branch revenue forecasts become less conservative the longer governors serve in office. When gubernatorial tenure is interacted with the absence of term limits, however, point estimates suggest that longer tenure leads to marginally more conservative forecasts—albeit the null that gubernatorial tenure had no impact on revenue forecasts in states without term limits could not be rejected by the data.

[34] Governors who are not subject to term limit restrictions may produce less conservative revenue forecasts because their staff are more experienced at this policy task. We tested this alternative logic by estimating the impact of gubernatorial and executive branch experience at revenue forecasting (measured as the number of consecutive years served in office) on revenue forecast conservatism. Although more seasoned governors generate less conservative revenue forecasts, it fails to alter the substantive results pertaining to opportunistic policymaking behavior under alternative term-limit distinctions captured by H3. These findings can be obtained directly from the data replication materials or by contacting the authors.

forecasts are significantly more conservative. These findings suggest that legislative delegation is less necessary to limit electoral and partisan pressures in policymaking when different parties control the executive and at least one legislative chamber.

## DISCUSSION

Democratic governments are entrusted with policymaking authority by their citizens. Stewardship of this authority often means that politicians may need to restrict their own ability to intervene in shaping policy outcomes. This is true in policy areas as varied as macroeconomic policy, public construction projects, and regulatory policy. Unfortunately, existing research has generally not considered policymaking differences between electoral and nonelectoral institutions and the varying incentives that public officials have for behaving opportunistically in different institutional contexts. This is a fundamental issue of interest to both policy scholars and political scientists alike because it is commonly presumed that executive branch or independent agencies are preferred policymaking venues compared to legislatures because the former are thought to be less susceptible to meddle in policymaking for electoral or partisan reasons (e.g., Bendor & Meirowitz, 2004; Falaschetti & Miller, 2001; Patashnik, 2000; Spulber & Besanko, 1992).

Utilizing panel data on official general fund revenue forecasts in the American states from FY 1987 to FY 2008, we obtain compelling empirical evidence that institutional design choices do have direct consequences for policymaking behavior by governments, not only which institutions are entrusted with policymaking authority matters, but also the institutional context in which they are able to exercise such authority. The findings indicate that both executive branch and independent commission institutions yield more conservative revenue forecasts in the American states compared to legislatures, but with a few notable caveats. Specifically, legislative branch forecasters produce the most optimistic revenue projections. Executive branch forecasts are more conservative, except in the case where governors are serving their lame duck term in office due to term limit restrictions. Consensus group commissions are generally the most conservative except when compared to states where governors can serve indefinitely. Political actors with electorally induced short electoral horizons are the most susceptible to the temptation to manipulate forecasts. Counteracting the short-term incentive to produce overly optimistic forecasts by assigning such powers to nonlegislative institutions may not alter forecasts in those states without regular partisan divisions or states with lame duck governors.

This study has two broader implications that advance our general understanding of the politics of institutional policymaking. First, how political actors view the future determines whether they engage in opportunistic policymaking behavior. The statistical evidence reveals that policymakers with a short electoral horizon (i.e., legislators and also governors in states with term limits) are most susceptible to myopic political incentives because they care little about the policy consequences associated with official revenue forecast errors. These results are not only consistent with Besley and Case's (1995) claim that greater fiscal profligacy arises when governors cannot be held accountable, but reveal that policymakers with shorter time horizons may intertemporally discount future reputation costs at a steeper rate than counterparts possessing longer time horizons (Krause & Corder, 2007). Therefore, the political insularity generated by term limits may be less effective at reducing policy opportunism. Perhaps a moderate amount of political pressure may, by some definitions, improve executive branch policy performance by balancing politicization and insularity in a manner that results in superior policy decisions (Krause, Lewis, & Douglas, 2006). In addition, partisan fragmentation can provide a vital corrective to opportunistic policymaking impulses (Persson, Roland, & Tabellini,

1997). Under unified partisan control, legislatures will exhibit their strongest tendency to engage in opportunistic policymaking behavior. This finding is consistent with the view that legislators are often not held accountable by their individual constituencies for collective choices made by the legislature.

Having either the executive branch or independent commissions make forecasts will only noticeably change outcomes when the political system itself fails to serve as a check on legislative excesses or executive myopia. When executive myopia is acute (i.e., governors serving their final term in office due to term limit restrictions) and legislative excesses are muted (i.e., divided partisan control over the legislature), the findings demonstrate that choice of institutional venue has modest policymaking implications. Conversely, choice of institutional venue is most critical for understanding opportunistic policymaking behavior when executives have longer electoral horizons and legislatures have the strongest incentives to manipulate forecasts. Institutional venue has the largest influence on outcomes when the political context (e.g., divided government) itself does not constrain policy choices. Of fundamental interest to policy scholars, this study shows that the institutional venue charged with policymaking authority is of less critical importance for mitigating opportunistic behavior when politics is characterized by separation of powers and vigorous electoral competition.

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## APPENDIX

This Appendix addresses issues that pertain to the JPAM article that could not be included due to document space limitations. These issues pertain to the technical details of our econometric estimation using the Hausman–Taylor instrumental variables (IVs) variant of fixed-effects variance decomposition (FEVD) estimation; addressing the possibility of endogenous forecast performance via within-sample period changes of institutional venue, as well as through IV econometric methods; a series of robustness checks using alternative econometric estimation strategies for Models 1 to 4 reported in Table 3 in the JPAM article, and display of the primary source data (and contacts) on institutional venues charged with policymaking responsibility for official revenue forecasts in each American state that was used to check against secondary source data.

## TECHNICAL DETAILS OF HAUSMAN–TAYLOR IV VARIANT OF FEVD ESTIMATION (DATA, VARIABLES, AND STATISTICAL METHODS SECTION)

Because the institutional venue covariates of central interest to us are often slowly moving variables within each panel (state), care must be given to parceling out statistical relationships. Although weakly time-invariant covariates that rarely change in a given panel can be estimated using standard fixed effects, such estimates will not only be inefficient, but these coefficients will also be highly unreliable (Arellano, 2003, chapter 2; Plumper & Troeger, 2007, p. 127). This creates the dilemma of having to choose between modeling unit effects while forgoing valid estimates of time-invariant covariates that are central to predicting official state revenue forecasting performance, or modeling the time-invariant covariates and improperly handling any remaining unobserved heterogeneity that may exist. In practical applications, this means that one cannot jointly estimate time-varying and time-invariant covariates, alongside unit-specific intercepts, without imposing additional model identification assumptions about which subset of regressors are independent of the unit effects (see Beck, 2011, pp. 121–122).

As noted in the text, the estimation approach best suited to this particular statistical modeling problem is the Hausman and Taylor (1981) variant of the FEVD estimation strategy (Plumper & Troeger, 2011). First, a standard cross-sectional fixed effect (*within-variance estimator*) model is estimated on only a vector containing time-varying covariates ($X_k$) to obtain estimates of the unit-specific panel effects ($\hat{\mu}_{it}$):

$$y_{i\,t} - \bar{y}_i = \beta_k \sum_{k=1}^{K} \left( X_{k,i\,t} - \bar{X}_{k,i} \right) + \varepsilon_{i\,t} - \bar{\varepsilon}_i. \tag{A.1}$$

The unit specific effects obtained from (A.1) are $\hat{\mu}_i = \bar{y}_i - \hat{\beta}_k^{FE} \sum_{k=1}^{K} \bar{X}_{k,i} - \bar{\varepsilon}_i$. Next, the unit-specific panel effects ($\hat{\mu}_{it}$) are regressed on the vector of time-invariant covariates ($Z_m$) via pooled OLS:

$$\hat{\mu}_i = \omega + \gamma_m \sum_{m=1}^{M} Z_m + \eta_i. \tag{A.2}$$

Moreover, the random component of the unit fixed effects ($\hat{\eta}_i$)—that is, $\hat{\eta}_i = \hat{\mu}_i - \omega - \gamma_m \sum_{m=1}^{M} Z_{m,i}$—is assumed to be independent of the time-varying covariates ($Z_m$) by assumption for purposes of model identification noted in the preceding paragraph. Equations (A.1) and (A.2) are equivalent to estimating the following

single-stage panel regression model via pooled OLS that jointly estimates the time-varying covariates ($X_k$), time-invariant covariates ($Z_m$), and the random component of unit effects ($\hat{\eta}_i$) where $\phi = 1$:

$$y_{it} = \alpha + \beta_k \sum_{k=1}^{K} X_{k,\,i\,t} + \gamma_m \sum_{m=1}^{M} Z_{m,\,i} + \phi\hat{\eta}_i + \varepsilon_{it}. \tag{A.3}$$

The inclusion of ($\hat{\eta}_i$) as a covariate ensures that (A.3) will produce valid estimates that account for any omitted variable bias attributable to the fact that the time-invariant covariates are assumed to be orthogonal to the unit-specific panel effects by construction. The variance–covariance matrix estimates produced by (A.3) are generated by using values of both the time-invariant covariates ($Z_{m\,i}$) and unit effects for the time-variant covariates ($\hat{\mu}_i$) as instruments to account for the downward bias that plagues their original variance–covariance matrix estimates (Breusch et al., 2011).[35] Temporal dynamics are modeled via a Prais–Winsten AR(1) serial correlation correction.

In addition, we analyzed these statistical models using alternative estimation approaches. These alternative panel econometric estimation strategies include cross-sectional random effects (CSREs) and time-wise fixed effects (TWFEs), plus also estimating models both with and without first-order serial correlation corrections. Further, we estimate variants of these models that consider institutional venue-political context covariates as endogenous regressors with respect to revenue forecast performance (IV models). These various robustness checks are discussed at considerable length in this Appendix. Although there are some tangible differences among results within models that are to be expected from using sets of 10 different estimation techniques, the core conclusions drawn from the evidence are consistent with those reported in the manuscript.

## ADDRESSING A POTENTIAL ENDOGENEITY CRITIQUE (DATA, VARIABLES, AND STATISTICAL METHODS SECTION)

Institutional structure is treated as being exogenous to policy performance in both theoretical and empirical terms in this study. Although one may question whether policy (forecasting) performance may also affect the choice of policymaking (forecasting) institution, one cannot relax this exogeneity assumption for two reasons. First, there may be coalitional drift so that the original intent of the enacting coalition differs from the intent of the current coalition. If this is the case, then attempts to insulate delegatory institutions from coalitional drift by making it rather difficult to alter them is the solution to this problem (e.g., Horn & Shepsle, 1989; Moe, 1989; Shepsle, 1992). Indeed, it is possible in several states that decisions on proper revenue forecasting venue were made well before our sample period began by coalitions that likely differ substantially from the current dominant coalition. Second, whether a given institutional venue under a particular political context possesses official revenue forecast responsibility may be directly related to constitutional powers embodied in the American states. These issues are addressed in the subsequent pair of subsections analyzing potential endogeneity bias between institutional venue-political context and revenue forecast performance.

[35] Plumper and Troeger's (2011) revised FEVD variance–covariance formula is $V_{\mathrm{FEVD}}\,(\beta,\,\gamma) = (H'W)^{-1}\,H'\Omega H\,(W'H)^{-1}$, where $H = [X^{*},\,Z]$, $W = [X,\,Z]$, and $\Omega = {\sigma_{\varepsilon}}^{2}\,\mathrm{I_{NT}} + {\sigma_{\eta}}^{2}\,\mathrm{I_N} \otimes \iota_{\mathrm{T}}\,\iota'_{\mathrm{T}}$, where $I_{\mathrm{N}}$ is an $N \times N$ identity matrix and $\iota_{\mathrm{T}}$ is a $T \times 1$ vector of ones. We gratefully acknowledge both Thomas Plumper and Vera Troeger for providing us with their updated xtfevd STATA code.

### Assessing Potential Endogeneity via Within Sample Period Changes of Institutional Venue

In the first set of analyses of the potential endogeneity critique, we analyze whether revenue forecasting performance affected changes in the institutional venue responsible for making official revenue forecasts in the American states observed during our sample period. This was implemented through a series of direct tests to determine whether forecast outputs might have systematically influenced a change in forecasting institution by making comparative state-level assessments regarding the revenue forecasting environment prior to the institutional venue change. The first test presupposes that a given state's economic volatility—measured as the three-year lagged standard deviation in state *i*'s real gross state product growth (*Economic Growth Volatility*)—is significantly higher compared to all other states for the years prior to the change in policymaking venue. The second test pertains to *Revenue Shocks* that account for the unanticipated component of revenues that deviate from a long-run trend of general fund revenues collected relative to income (Crain, 2003, pp. 74–75). Specifically, we expect that higher unanticipated negative (positive) revenue shocks for a given state relative to other states will be most (least) apt to cause a change in policymaking venue for the state in question. The third and fourth tests directly inspect a state's revenue forecasting performance relative to those of other states in the year preceding a change in policymaking venue in terms of *Forecast Accuracy* and *Forecast Conservativism*, respectively. Logic suggests states that change institutional venue responsible for making official revenue forecasts will have generated both less accurate and conservative revenue forecast errors relative to all other states during the period preceding this institutional venue change. That is, the endogeneity critique would contain some merit if states with relatively inaccurate or optimistic forecasts compared to other states moved policymaking authority to a more politically insulated institution.

A summary of our statistical findings appear in Table A1. Eleven states changed their policymaking venue within our sample period where we have available data to assess policy environment and performance effects to assess potential endogeneity.[36] Not surprisingly, given the trend toward *Consensus Group*'s independent commissions over the past few decades, most of the changes were from either the legislative branch (LB) or the executive branch (EB) to a consensus group independent commission. Only a single state (Louisiana) shows support for both dimensions of forecasting environment (*Economic Growth Volatility* & *Revenue Shocks*), but does not do so with respect to actual forecasting performance (*Forecast Accuracy* & *Forecast Conservatism*). In all but two of the remaining states that changed institutional venue during our sample period (Maine and Vermont), only one of these four dimensions appear to be consistent with the logic of endogenous institutional change. Most of this modest evidence is derived from *Revenue Shocks*, and in only two states does this emanate from differences arising from *Forecast Conservatism* (Kentucky and New York). Under no circumstance did a state change its institutional venue in response to less accurate revenue forecasts compared to other states in the period preceding the institutional change (*Forecast Accuracy*). There are also other reasons for well-founded skepticism regarding the potential for endogeneity bias relating to the analysis of institutional venue change. In three of four cases where a state moved from an EB forecast to a consensus group, it came in the year

[36] South Carolina changed their *Consensus Group* structure to all partisan members from one containing some nonpartisan members. However, because these structures are combined in our analysis, this institutional change is omitted from the subsequent analysis.

**Table A1.** Summary analyzing potential endogeneity: Changes in forecasting institution related to forecasting performance.

| State | Date of change | Original format | Revised format | Economic growth volatility | Revenue shocks | Forecast accuracy | Forecast conservatism |
|---|---|---|---|---|---|---|---|
| Colorado | 1989 | EB | LB | ND | CS | WS | WS |
| Kentucky | 1994 | EB | CG | ND | ND | WS | CS |
| Louisiana | 1988 | LB | CG | CS | CS | WS | WS |
| Maine | 1993 | EB | CG | ND | WS | ND | ND |
| Michigan | 1990 | LB | CG | ND | CS | WS | ND |
| Mississippi | 1993 | EB | CG | ND | CS | WS | ND |
| Nevada | 1996 | LB | CG | WS | CS | ND | ND |
| New York | 1996 | LB | CG | WS | WS | WS | CS |
| Rhode Island | 1991 | EB | CG | WS | WS | ND | ND |
| Tennessee | 1993 | EB | CG | WS | CS | WS | ND |
| Vermont | 1996 | LB | CG | ND | ND | ND | ND |

*Note:* CS (correct sign) refers to forecasting difficulty being significantly greater in state *i* prior to format change relative to other states. ND (no difference) refers to no significant difference in forecasting difficulty in state *i* prior to format change relative to other states. WS (wrong sign) refers to forecasting difficulty being significantly less in state *i* prior to format change relative to other states. EB refers to unitary executive office. LB refers to legislative branch. CG refers to consensus group. *Economic growth volatility* refers to lagged standard deviation in state *i*'s state growth product from the preceding three years. *Revenue shocks* refers to the unanticipated component of actual general fund revenues (Crain, 2003, pp. 74–75). Forecast accuracy refers to [(|actual state general fund revenues – official projected state general fund revenues|) / actual state general fund revenues] × 100. *Forecast conservatism* refers to [(actual state general fund revenues – official projected state general fund revenues)/actual state general fund revenues] × 100.

after a governor's lame duck year, implying a change possibly was made due to poor forecast performance. Yet, in these three instances, the loss of EB authority over official revenue forecasts transpired during unified party government. In turn, this implies that the change in institutional venue from the governor to the consensus group was probably not driven by a desire to rebuke the governor for manipulating revenue forecasts. Although it is not possible to entirely rule out bias arising from an endogeneity problem in this particular analysis, the bulk of the evidence suggests that changes in revenue forecast institutions were not chosen in response to poor forecast performance in terms of either forecast accuracy or forecast conservatism.

### Assessing Potential Endogeneity via IVs (Statistical Findings Section)

In addition, we consider potential endogeneity bias between institutional venue-political context and revenue forecast performance that accounts for all 50 American states (where 39 states possessed fixed institutional arrangements with respect to official revenue forecast responsibility during the sample period). Addressing potential endogeneity in this manner requires us to rely on constitutional and institutional features as IVs that suitably predict institutional venue-political context, but remain uncorrelated with the residual term in the revenue forecast error structural equations. We offer three candidates for viable exogenous instruments. The first instrument, *Governor Full Budget-Making Powers*, is a binary variable that is coded 1 if the governor exercises unilateral control over a state's budget formulation, 0 if this policymaking responsibility is shared with other governmental institutions

(mean = 0.79, SD = 0.41).[37] The idea underlying this IV is simple. To provide a balance of fiscal policymaking powers, states whose governors exercise full budget formulation authority will be less likely to have responsibility for making official revenue forecasts. Put another way, legislatures and independent commissions (*Consensus Groups*) will be more likely to control official revenue forecasts when the governor exercises unilateral control over constructing the state government's budget. Relatedly, this balancing of power logic means that the likelihood of the EB controlling the official revenue forecast will be considerably lower in those states where the governor is not subject to term-limit restrictions compared to those states where they face this type of electoral constraint. A second IV that assesses EB constitutional powers is *Independent Elected Executive Branch Fiscal Officials* that is measured as a count variable of the number of independently elected EB officials with fiscal policymaking responsibilities (sans the governor). This variable ranges from 0 to 4 (mean = 1.03, SD = 0.66).[38] Higher (lower) values indicate a greater diffusion (concentration) of power residing within the EB. Under such circumstances, diffuse fiscal powers enjoyed by governors should result in a higher probability of the EB being afforded responsibility for official revenue forecasts compared to when power is more heavily concentrated in the hands of the governor's office. The third and final instrument, *Non-Delegation Doctrine*, focuses on the legislature's constitutional capacity to delegate policymaking authority to other institutions (mean = 2.26, SD = 0.69). This variable is coded as an ordinal measure that equals 1 when the legislature experiences considerable latitude for delegating policy tasks to the EB (*weak restrictions*), equals 2 when they possess moderate discretion for delegating policy tasks to the EB (*moderate restrictions*), and equals 3 when they possess very limited ability to delegate policy tasks to the EB (*strong restrictions*).[39] Legislatures operating under increasing delegation restrictions will be more likely to make official revenue forecasts because they are less capable of delegating these tasks to either the EB or independent commissions.[40]

[37] These data come from the table titled "The Governors: Powers—Budget Making Power" in The Council of State Governments (1986–2009).

[38] The relevant state-level executive branch fiscal officials accounted for in this measure are as follows: Treasurer, Comptroller, Financial Officer, and Revenue Officer. 20.33 percent of cases are coded 0, 56.06 percent are coded as 1, and 23.61 percent of the cases are coded as 2. These data come from the table titled "Selected State Administrative Officials: Methods of Selection" in The Council of State Governments (1986–2009).

[39] The relevant breakdown of this variable by state-year observations is as follows: *Weak Restrictions*: 14.04 percent, *Moderate Restrictions*: 45.94 percent, and *Strong Restrictions*: 40.02 percent. These data come from Rossi (1999, p. 1201, Table 1). We thank Gbemende Johnson for generously sharing her data with us.

[40] Our initial choice of instruments was based upon a belief that theoretically they make sense. We also evaluate this empirically as suggested in the text above. We have evaluated the quality of the instruments in other ways. First, we verified that the instruments themselves are exogenous. The only instrument that we examined that we felt completely confident was exogenous on substantive grounds was the nondelegation doctrine trichotomous indicator. Second, we evaluated each of the three instruments adopted here by implementing a simple *reverse causation test* (Angrist & Pischke, 2009) and then re-estimated the models described above including only the instruments that this method suggests are truly exogenous based upon these test results. This involves estimating models that regress the institutional venue variables on the instruments and the instruments at $t + 1$. If the coefficient on the instrument at $t + 1$ is significant, then this suggests that the instrument may be endogenous to institutional venue-political context. We re-estimated the Hausman–Taylor model with first-order autocorrelated disturbances [HT-ar(1)] and HT models including only those instruments that this method suggested were exogenous, and the results generally confirm what is described in the text except that in two cases the IV models exogeneity tests rejected the null hypothesis that the *Institutional Venue-Political Context* covariates are exogenous to forecast performance since they approach conventional levels of statistical significance ($P < 0.07$; models corresponding to Table A5, HT and Table A6, HT). The full estimates are available from the data replication materials or upon request from the authors.

In the subsequent IV-reduced form regression analysis, we employ these three exogenous instruments to arrive at the best joint prediction of the institutional venue-political context under consideration based on a Wald $\chi^2$ exclusion test.[41] Individual binary Probit equations are estimated that predict the absence or presence of a particular institutional venue-political context measured as covariates appearing in Models 1 to 4 displayed in Table 3.[42] As the results in Table A2 reveal, the set of instruments chosen vary in each Probit equation, and their predictive content also varies by equation as evinced by the Wald $\chi^2$ exclusion tests. Model A shows that a governor's budget making powers are positively correlated with the probability of the legislature possessing official revenue forecast policymaking authority, although the coefficient is imprecise. Model B reveals that governors' exercising unilateral control over budget formulation are less likely to obtain the power to issue official revenue forecasts consistent with the balance of powers logic noted earlier. The strength of this inverse relationship is much greater when the governor can be elected to an unlimited number of terms (Model C) compared to when they are subject to term limits (Model D). Models G, H, and J suggest that states with governors exercising unilateral control over budget formulation are also more likely to have legislatures conducting official revenue forecasts. Legislatures with greater restrictions on delegation are estimated to be less likely to delegate forecasting responsibility to the EB with the odd exception of states experiencing periods of split branch government.

The consequences of endogeneity bias on the institutional venue-political context estimates are addressed in the next section, which offers a comparative analysis of the robustness of our findings using alternative estimation techniques. In short, although the instruments used to predict institutional venue are often strong predictors as noted in Table A2, accounting for this potential source of endogeneity bias often (though clearly not always) fails to improve model performance based on (1) the Hausman exogeneity test results and (2) the sizable efficiency loss from these two-stage residual inclusion (2SRI) model specifications (see next section for technical description of the 2SRI method). In several instances (Models 3 and 4: CSRE-ar(1), CSRE, and TWFE), this efficiency loss is relatively minor and the corresponding Hausman exogeneity tests rejected the null hypothesis that the *Institutional Venue-Political Context* covariates are exogenous to forecast performance. Yet, in these particular instances, the findings from the 2SRI method reveal no clear patterns in terms of relative coefficient differences produced by the HT-ar(1) estimation method reported in the manuscript (as indicated by Wald coefficient restriction tests appearing on the bottom portion of Table A5 and Table A6). In some instances, these relative coefficient differences reported in the manuscript are more conservative than those generated by the relatively efficient IV models (e.g., those involving various LB and EB term-limit lame duck differences), and in other instances they are less conservative (e.g., those involving various LB covariates and EB covariates pertaining to no-term-limit differences). These issues are discussed in greater detail in the next section comparing the revenue forecast error models across several different estimation methods.

[41] By selecting the covariates that provide the strongest set of instruments, we attempt to ensure that the chosen instruments are best for assessing endogeneity bias subsequently estimated in the structural-outcome equations using the 2SRI method discussed in the next section.

[42] The *Independent Elected Executive Branch Fiscal Officials* is omitted from these IV-Probit model specifications because it is either not exogenous to *Institutional Venue-Political Context* based on the reverse causality test, or results in an inferior model fit.

**Table A2.** Instrumental variable reduced-form probit regression results for institutional venue-political context covariates (only instrumental variable results reported).

| | Model A: LB | Model B: EB | Model C: EB-UT | Model D: EB-RTLD | Model E: EB-RTNLD | Model F: LB:NTL | Model G: LB:TL | Model H: LB:UG | Model I: LB: SB-UL | Model J: LB: DL |
|---|---|---|---|---|---|---|---|---|---|---|
| Governor full budget making powers | 0.43 (0.38) | −0.52* (0.29) | −0.82** (0.37) | −0.49* (0.27) | _____ | _____ | 0.30 (0.30) | 0.26 (0.33) | _____ | 0.58* (0.31) |
| Nondelegation doctrine | _____ | _____ | 0.34 (0.29) | _____ | −0.29 (0.29) | 0.06 (0.25) | _____ | 0.32 (0.25) | −0.41* (0.23) | 0.35* (0.20) |
| Joint $\chi^2$ ~(k) test: Instrument exclusion | 1.26 [0.26] | 3.18* [0.07] | 5.22* [0.07] | 3.35* [0.07] | .81 [0.37] | 0.06 [0.81] | 0.99 [0.32] | 2.85 [0.24] | 3.40* [0.07] | 6.07** [0.05] |
| Pseudo $R^2$ | 0.37 | 0.21 | 0.40 | 0.24 | 0.20 | 0.28 | 0.15 | 0.43 | 0.44 | 0.19 |
| Effective sample size | 1,099 | 1,099 | 1,099 | 1,099 | 1,099 | 1,099 | 1,099 | 1,099 | 1,099 | 1,099 |

*Notes:* Model A: LB (dependent variable = 1 if legislative branch has official revenue forecast responsibility, dependent variable = 0 otherwise); Model B: EB (dependent variable = 1 if *Executive Branch* has official revenue forecast responsibility, dependent variable = 0 otherwise); Model C: EB: UT (dependent variable = 1 if *Executive Branch-No Term Limit Restrictions* has official revenue forecast responsibility, dependent variable = 0 otherwise); Model D: EB: RTLD (dependent variable = 1 if *Executive Branch-Term Limits, Lame Duck* has official revenue forecast responsibility, dependent variable = 0 otherwise); Model E: EB:RTNLD (dependent variable = 1 if *Executive Branch-Term Limits, Non-Lame Duck* has official revenue forecast responsibility, dependent variable = 0 otherwise); Model F: LB: NTL (dependent variable = 1 if *Legislative Branch-No Term Limit Restrictions* has official revenue forecast responsibility, dependent variable = 0 otherwise); Model G: LB:TL (dependent variable = 1 if *Legislative Branch-Term Limit Restrictions* has official revenue forecast responsibility, dependent variable = 0 otherwise); Model H: LB: UG (dependent variable = 1 if *Legislative Branch-Unified Government* has official revenue forecast responsibility, dependent variable = 0 otherwise); Model I: LB: SB-UL (dependent variable = 1 if *Legislative Branch-Split Partisan Branches-Unified Legislature* has official revenue forecast responsibility, dependent variable = 0 otherwise); Model J: LB: DL (dependent variable = 1 if *Legislative Branch-Divided Partisan Legislatures* has official revenue forecast responsibility, dependent variable = 0 otherwise). Standard errors appear inside parentheses. Probability values appear inside brackets.

*$P \leq 0.10$; **$P \leq 0.05$; ***$P \leq 0.01$.

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## ROBUSTNESS CHECKS ACCOUNTING FOR ALTERNATIVE ESTIMATION TECHNIQUES (STATISTICAL FINDINGS SECTION)

To address potential endogeneity bias between institutional venue-political context covariates and revenue forecast performance, we utilize the 2SRI method for handling potential endogeneity bias. The 2SRI method simply involves estimating the structural or outcome equation comprised of endogeneous regressors and exogenous covariates as if endogeneity were to be ignored, plus adding the predicted residual probability for each relevant endogenous regressor in the reduced-form or IV estimated Probit equations appearing in Table A2. In this manner, endogeneity bias is treated as an omitted variable problem that is properly accounted for by inclusion of these residual probability covariates relating to the endogenous regressors. The 2SRI method is desirable in our empirical application for three reasons. First, the 2SRI technique produces consistent estimates when either the endogenous regressor(s) or outcome variable (regressand) are measured as a discrete or limited dependent variable (Terza, Basou, & Rathez 2008).[43] Second, this technique allows one to treat multiple binary endogenous regressors *as is*, as opposed to relying on continuous measures of institutional venue-political context that are inconsistent with the discrete, mutually exclusive concepts analyzed here. We can thus make direct comparisons between IV and non-IV model specifications. Finally, the 2SRI method allows one to test endogeneity bias as a restriction within the confines of the original structural equation of interest (Hausman, 1978).

### Reassessing Model 1 Results Using Alternative Estimation Approaches

Besides dealing with endogeneity corrections, we also utilize alternative estimation techniques that either correct [ar(1)] or do not correct for first-order serial correlation, plus use CSREs and TWFEs methods as an alternative to the Hausman–Taylor/FEVD [HT] based estimates reported in the manuscript. The first set of analyses cover the Model 1 specification in Table 3 that considers the LB and EB control over official revenue forecast responsibility, with consensus group independent commissions (CG) captured in the intercept term as the baseline category. These results appear in Table A3. The HT-ar(1) results reported in the manuscript yield coefficients that are generally (though not always) larger in magnitude, but estimated less precisely, relative to the CG baseline than compared to other non-IV estimation approaches [HT, CSRE-ar(1), CSRE, and TWFE]. Yet, the Wald coefficient differences between LB and EB are more modest in the reported model [HT-ar(1)] than in the other non-IV estimation approaches [HT, CSRE-ar(1), CSRE, and TWFE]. The IV model estimates [HT-ar(1)*, HT*, CSRE-ar(1)*, CSRE*, and TWFE*] are estimated with considerable imprecision, and thus are highly inefficient—a fact further corroborated by the failure to reject the null hypothesis that the residual probabilities from the LB and EB Probit equations are jointly different from zero via the Hausman IV exogeneity test. Therefore, endogeneity bias does not appear to be a problem in the various Model 1 specifications, and accounting for it as omitted variable bias results in highly inefficient estimates that falsely obscure differences between legislative and EB revenue forecasts.

[43] The 2SRI technique has been used in many applications involving various types of limited endogenous or dependent variables (e.g., Blundell & Smith, 1989; Newey, 1987; Rivers & Vuong, 1988; Wooldridge, 2002).

**Table A3.** Model 1 alternative estimation results (only institutional venue results reported).

| | HT-ar(1) | HT | HT-ar(1)* | HT* | CSRE-ar(1) | CSRE | CSRE-ar(1)* | CSRE* | TWFE | TWFE* |
|---|---|---|---|---|---|---|---|---|---|---|
| *Legislative Branch* | −3.39** | −2.62** | −5.81* | −3.10 | −2.41** | −1.80** | −0.86 | −0.46 | −1.64** | 0.45 |
| | (1.49) | (1.00) | (3.20) | (2.79) | (0.80) | (0.64) | (1.92) | (1.63) | (0.56) | (1.53) |
| *Executive Branch* | −0.71 | −0.47 | −3.68 | −3.86 | 0.31 | −0.004 | 1.26 | −0.10 | 0.15 | −1.65 |
| | (1.59) | (1.03) | (5.02) | (3.98) | (0.83) | (0.66) | (2.94) | (2.42) | (0.57) | (2.21) |
| Hausman IV | _____ | _____ | 1.19 | 0.66 | _____ | _____ | 1.45 | 0.77 | _____ | 1.25 |
| exogeneity test | | | [0.30] | [0.52] | | | [0.49] | [0.68] | | [0.29] |
| $H_O$: LB = EB | 2.31 | 3.52* | 0.11 | 0.02 | 5.14** | 5.89** | 0.32 | 0.01 | 7.80** | 0.55 |
| | [0.13] | [0.06] | [0.75] | [0.89] | [0.02] | [0.02] | [0.57] | [0.91] | [0.01] | [0.46] |
| $R^2$ | 0.17 | 0.17 | 0.18 | 0.17 | 0.07 | 0.09 | 0.07 | 0.09 | 0.08 | 0.08 |
| Effective sample size | 1,042 | 1,097 | 1,042 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 |

*Notes:* HT-ar(1): Hausman–Taylor/FEVD estimation with AR(1) serial correlation correction; HT: Hausman–Taylor/FEVD estimation; HT-ar(1)*: Hausman–Taylor/FEVD estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue* covariates; HT*: Hausman–Taylor/FEVD estimation and endogeneity correction for *Institutional Venue* covariates; CSRE-ar(1): cross-sectional random effects estimation with AR(1) serial correlation correction; CSRE: cross-sectional random effects estimation; CSRE-ar(1)*: cross-sectional random effects estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue* covariates; CSRE*: cross-sectional random effects estimation and endogeneity correction for *Institutional Venue* covariates; TWFE: time-wise fixed effects estimation; TWFE*: time-wise fixed effects estimation and endogeneity correction for *Institutional Venue* covariates. Standard errors appear inside parentheses. Probability values appear inside brackets.

*$P \leq 0.10$; **$P \leq 0.05$; ***$P \leq 0.01$.

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### Reassessing Model 2 Results Using Alternative Estimation Approaches

The various estimation approaches for Model 2 appear in Table A4. The institutional venue-political context covariates are estimated with greater precision in the alternative non-IV models, but have coefficients that are considerably smaller in magnitude than compared to the ones reported in Table 3 based on the HT/FEVD-ar(1) technique. Once again, the IV model methods are highly inefficient given their much larger standard errors compared to non-IV models, and also corroborated by the failure to reject the null of exogeneity displayed in the Hausman IV exogeneity test statistics. Moreover, the Wald coefficient difference tests among the various institutional-venue-political context covariates reveals that the reported results based on the HT/FEVD-ar(1) method are more conservative relative to other non-IV models. The failure to reject coefficient differences between the institutional venue-political context covariates in the IV models is a manifestation of the highly inefficient nature of these set of statistical estimates. As a result, the IV model estimates are neither suggestive of endogeneity bias (based on the null evidence offered by the Hausman IV exogeneity test results) nor produce superior estimates.

### Reassessing Model 3 Results Using Alternative Estimation Approaches

The comparison of estimation procedures for Model 3 appears in Table A5. There are some similarities to the patterns apparent in Tables A3 and A4. Specifically, the IV model estimates are often estimated with poor precision. That said, the Hausman IV exogeneity test restrictions are rejected at conventional levels of statistical significance in several of the models. The Wald coefficient differences between various institutional venue-political context variables tend to be more conservative based on the HT-ar(1) estimates relative to the non-IV models based on alternative panel estimation strategies. However, in those instances where endogeneity bias appears to be a tangible problem based on the significant Hausman IV exogeneity test statistic, it is driven by the predicted residual probability corresponding to when the legislature is not subject to term limits (*Legislative Branch: No Term Limit*).[44] The differences between both LB scenarios (term-limit restrictions and no-term-limit restrictions) and a governor not subject to term limits, as well as the distinction between governors not subject to term limits and those that are subject to such term-limit restrictions but are not lame ducks, are no longer significant in the models based on IVs. Interestingly enough, the difference between legislatures not subject to term-limit restrictions and governors that are term limited serving in their lame duck terms in office becomes significant once one accounts for endogeneity bias. Nonetheless, these various results from these alternative robustness checks are consistent with the main findings reported in the manuscript. Specifically, EB revenue forecasts in states with gubernatorial term limits are generally indistinguishable from LB forecasts, and when they differ they indicate that the legislature not subject to term-limit restrictions will produce more conservative revenue forecasts than a lame duck governor completing their tenure in office.

[44] There are three exceptions to this general pattern: *Executive Branch: Term Limit-Lame Duck = Executive Branch: No Term Limit* [EBRTLD = EBUT]; *Legislative Branch: Term Limit = Executive Branch: No Term Limit* [LBTL = EBUT]; *Legislative Branch: No Term Limit = Executive Branch: No Term Limit* [LBNTL = EBUT].

**Table A4.** Model 2 alternative estimation results (only institutional venue results reported).

| | HT-ar(1) | HT | HT-ar(1)* | HT* | CSRE-ar(1) | CSRE | CSRE-ar(1)* | CSRE* | TWFE | TWFE* |
|---|---|---|---|---|---|---|---|---|---|---|
| *Legislative Branch* | −3.84** | −3.29*** | −5.55* | −3.26 | −2.43*** | −1.82*** | −0.68 | −0.20 | −1.69*** | 0.70 |
| | (1.57) | (1.09) | (3.36) | (2.96) | (0.78) | (0.62) | (1.96) | (1.68) | (0.56) | (1.58) |
| *Executive Branch:* | 1.99 | 1.33 | 5.45 | 3.65 | 1.98 | 2.08** | 3.13 | 3.59 | 2.13** | 2.97 |
| *No Term Limit* | (2.60) | (1.75) | (4.97) | (3.92) | (1.28) | (1.02) | (2.84) | (2.40) | (0.89) | (2.17) |
| *Executive Branch:* | −4.39* | −4.70** | −11.98** | −13.96** | −1.69 | −1.39 | −2.89 | −4.31 | −1.09 | −5.76* |
| *TL: Lame Duck* | (2.39) | (1.90) | (5.69) | (5.09) | (1.14) | (0.95) | (4.33) | (3.65) | (0.86) | (3.35) |
| *Executive Branch:* | −2.56 | −2.92 | −4.42 | 8.67 | −0.96 | −0.41 | 3.43 | 3.17 | −0.49 | 3.16 |
| TL: No Lame Duck | (2.60) | (2.04) | (8.08) | (9.30) | (1.12) | (0.91) | (5.50) | (4.62) | (0.82) | (4.17) |
| Hausman IV | ——— | ——— | 1.00 | 0.94 | ——— | ——— | 2.32 | 2.52 | ——— | 1.24 |
| exogeneity test | | | [0.41] | [0.44] | | | [0.68] | [0.64] | | [0.29] |
| $H_O$: LB = EBUT | 5.06** | 6.95*** | 3.72 | 2.01 | 10.51*** | 13.05*** | 1.37 | 1.87 | 16.29*** | 0.80 |
| | [0.03] | [0.01] | [0.05] | [0.16] | [0.00] | [0.00] | [0.24] | [0.17] | [0.00] | [0.37] |
| $H_O$: LB = EBRTLD | 0.30 | 0.71 | 0.77 | 3.05 | 0.39 | 0.19 | 0.20 | 0.97 | 0.47 | 2.83* |
| | [0.59] | [0.40] | [0.38] | [0.08] | [0.53] | [0.66] | [0.66] | [0.33] | [0.49] | [0.10] |
| $H_O$: LB = EBRTNLD | 0.06 | 0.04 | 1.14 | 1.32 | 1.50 | 2.06 | 0.44 | .41 | 1.88 | 0.27 |
| | [0.80] | [0.84] | [0.29] | [0.25] | [0.22] | [0.15] | [0.51] | [0.52] | [0.17] | [0.61] |
| $H_O$: EBUT = EBRTLD | 5.19** | 8.65*** | 5.21** | 6.72*** | 5.31*** | 7.12*** | 1.23 | 2.92* | 7.71*** | 4.30** |
| | [0.02] | [0.00] | [0.02] | [0.01] | [0.02] | [0.01] | [0.27] | [0.10] | [0.01] | [0.04] |
| $H_O$: EBUT = EBRTNLD | 2.37 | 3.90** | 0.01 | 0.28 | 3.52* | 3.89** | 0.00 | 0.01 | 5.47** | 0.00 |
| | [0.12] | [0.05] | [0.91] | [0.60] | [0.06] | [0.05] | [0.96] | [0.93] | [0.02] | [0.97] |
| $H_O$: EBRTLD = EBRTNLD | 1.73 | 1.89 | 2.55 | 3.58* | 0.33 | 0.72 | 0.60 | 1.23 | 0.31 | 2.15 |
| | [0.19] | [0.17] | [0.11] | [0.06] | [0.57] | [0.40] | [0.44] | [0.27] | [0.58] | [0.14] |
| $R^2$ | 0.16 | 0.17 | 0.18 | 0.18 | 0.08 | 0.09 | 0.08 | 0.08 | 0.09 | 0.09 |
| Effective sample size | 1,042 | 1,097 | 1,042 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 |

*Notes:* HT-ar(1): Hausman–Taylor/FEVD estimation with AR(1) serial correlation correction; HT: Hausman–Taylor/FEVD estimation; HT-ar(1)*: Hausman–Taylor/FEVD estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; HT*: Hausman–Taylor/FEVD estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; CSRE-ar(1): cross-sectional random effects estimation with AR(1) serial correlation correction; CSRE: cross-sectional random effects estimation; CSRE-ar(1)*: cross-sectional random effects estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; CSRE*: cross-sectional random effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; TWFE: time-wise fixed effects estimation; TWFE*: time-wise fixed effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates. Standard errors appear inside parentheses. Probability values appear inside brackets.

*$P \leq 0.10$; **$P \leq 0.05$; ***$P \leq 0.01$.

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**Table A5.** Model 3 alternative estimation results (only institutional venue results reported).

| | HT-ar(1) | HT | HT-ar(1)* | HT* | CSRE-ar(1) | CSRE | CSRE-ar(1)* | CSRE* | TWFE | TWFE* |
|---|---|---|---|---|---|---|---|---|---|---|
| *Legislative Branch:* | −5.41* | −7.32*** | 2.77 | 2.70 | −2.28*** | −1.72*** | 2.81 | 3.17** | −1.57*** | 3.08** |
| *No Term Limit* | (2.97) | (2.46) | (3.08) | (2.45) | (0.80) | (0.64) | (1.59) | (1.32) | (0.58) | (1.24) |
| *Legislative Branch:* | −6.26* | −8.05*** | −11.64 | −11.43 | −3.50** | −2.76** | −5.35 | −3.99 | −2.41** | 0.43 |
| *Term Limit* | (3.41) | (2.80) | (15.11) | (12.94) | (1.44) | (1.13) | (9.84) | (8.27) | (1.03) | (7.58) |
| *Executive Branch:* | 1.59 | 0.33 | 5.07 | 2.89 | 1.96 | 2.04** | 2.45 | 2.88 | 2.13** | 2.37 |
| *No Term Limit* | (2.73) | (2.04) | (5.33) | (4.10) | (1.28) | (1.03) | (2.83) | (2.37) | (0.89) | (2.15) |
| *Executive Branch:* | −4.40* | −4.71** | −9.69 | −11.74** | −1.67 | −1.40 | −5.04 | −6.40 | −1.06 | −7.95** |
| *TL: Lame Duck* | (2.35) | (1.93) | (6.38) | (5.42) | (1.14) | (0.96) | (4.60) | (3.86) | (0.86) | (3.54) |
| *Executive Branch:* | −2.56 | −2.92 | 2.47 | 6.14 | −0.94 | −0.41 | 0.44 | 0.33 | −0.47 | 1.36 |
| *TL: No Lame Duck* | (2.56) | (2.06) | (7.67) | (8.88) | (1.12) | (0.92) | (5.02) | (4.17) | (0.82) | (3.78) |
| Hausman IV | ——— | ——— | 1.05 | 2.13* | ——— | ——— | 9.70* | 12.21** | ——— | 2.92** |
| exogeneity test | | | [0.39] | [0.06] | | | [0.08] | [0.03] | | [0.01] |
| $H_O$: LBNTL = LBTL | 0.28 | 0.33 | 1.00 | 1.27 | 0.79 | 0.94 | 0.64 | 0.69 | 0.68 | 0.11 |
| | [0.59] | [0.57] | [0.32] | [0.26] | [0.38] | [0.33] | [0.43] | [0.41] | [0.41] | [0.74] |
| $H_O$: LBNTL = EBUT | 4.70** | 8.83*** | 0.18 | 0.00 | 9.59*** | 11.62*** | 0.01 | 0.01 | 14.90*** | 0.10 |
| | [0.03] | [0.00] | [0.67] | [0.96] | [0.00] | [0.00] | [0.91] | [0.91] | [0.00] | [0.76] |
| $H_O$: LBTL = EBUT | 4.79** | 8.64*** | 1.30 | 1.36 | 9.13*** | 11.26*** | 0.62 | 0.68 | 12.49*** | 0.06 |
| | [0.03] | [0.00] | [0.26] | [0.25] | [0.00] | [0.00] | [0.43] | [0.41] | [0.00] | [0.80] |
| $H_O$: LBNTL = EBRTLD | 0.09 | 0.96 | 2.26 | 4.59** | 0.26 | 0.10 | 2.06 | 4.36** | 0.33 | 4.39** |
| | [0.76] | [0.33] | [0.13] | [0.03] | [0.61] | [0.75] | [0.15] | [0.04] | [0.56] | [0.04] |
| $H_O$: LBTL = EBRTLD | 0.26 | 1.28 | 0.01 | 0.00 | 1.15 | 0.97 | 0.00 | 0.07 | 1.14 | 0.59 |
| | [0.61] | [0.26] | [0.92] | [0.99] | [0.28] | [0.33] | [0.98] | [0.79] | [0.29] | [0.44] |
| $H_O$: LBNTL = EBRTNLD | 0.67 | 2.42 | 0.13 | 0.58 | 1.23 | 1.74 | 0.23 | 0.48 | 1.55 | 0.22 |
| | [0.41] | [0.12] | [0.72] | [0.45] | [0.27] | [0.19] | [0.63] | [0.49] | [0.21] | [0.64] |
| $H_O$: LBTL = EBRTNLD | 0.93 | 2.75* | 1.02 | 1.30 | 2.22 | 2.91* | 0.27 | 0.21 | 2.40 | 0.01 |
| | [0.34] | [0.10] | [0.31] | [0.26] | [0.14] | [0.09] | [0.61] | [0.65] | [0.12] | [0.91] |
| $H_O$: EBUT = EBRTLD | 4.31** | 5.05** | 2.85* | 3.99** | 5.22** | 6.88*** | 1.76 | 3.77* | 7.56*** | 5.59** |
| | [0.04] | [0.03] | [0.09] | [0.05] | [0.02] | [0.01] | [0.19] | [0.05] | [0.01] | [0.02] |

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**Table A5.** Continued.

| | HT-ar(1) | HT | HT-ar(1)* | HT* | CSRE-ar(1) | CSRE | CSRE-ar(1)* | CSRE* | TWFE | TWFE* |
|---|---|---|---|---|---|---|---|---|---|---|
| $H_O$: EBUT = EBRTNLD | 1.86 | 1.90 | 0.08 | 0.11 | 3.44* | 3.67* | 0.13 | 0.29 | 5.37** | 0.06 |
| | [0.17] | [0.17] | [0.78] | [0.75] | [0.06] | [0.06] | [0.72] | [0.59] | [0.02] | [0.81] |
| $H_O$: EBRTLD = EBRTNLD | 1.73 | 1.88 | 2.85* | 2.73* | 0.33 | 0.75 | 0.48 | 1.07 | 0.31 | 2.47 |
| | [0.19] | [0.17] | [0.09] | [0.10] | [0.57] | [0.39] | [0.49] | [0.30] | [0.58] | [0.12] |
| $R^2$ | 0.18 | 0.17 | 0.18 | 0.17 | 0.08 | 0.09 | 0.08 | 0.09 | 0.09 | 0.09 |
| Effective sample size | 1,042 | 1,097 | 1,042 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 |

*Notes:* HT-ar(1): Hausman–Taylor/FEVD estimation with AR(1) serial correlation correction; HT: Hausman–Taylor/FEVD estimation; HT-ar(1)*: Hausman–Taylor/FEVD estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; HT*: Hausman–Taylor/FEVD estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; CSRE-ar(1): cross-sectional random effects estimation with AR(1) serial correlation correction; CSRE: cross-sectional random effects estimation; CSRE-ar(1)*: cross-sectional random effects estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; CSRE*: cross-sectional random effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; TWFE: time-wise fixed effects estimation; TWFE*: time-wise fixed effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates. Standard errors appear inside parentheses. Probability values appear inside brackets.

*$P \leq 0.10$; **$P \leq 0.05$; ***$P \leq 0.01$.

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### Reassessing Model 4 Results Using Alternative Estimation Approaches

The final set of analysis comparing results from Model 4 across different estimation strategies appears in Table A6. In the HT*, CSRE*, and TWFE* models, the Hausman IV exogeneity tests are clearly rejected by the data. For consistency purposes, we present the HT-ar(1) model results in Table 3 of the manuscript. Moreover, in general, the Wald coefficient differences between various institutional venue-political context variables tend to be more conservative based on the HT-ar(1) estimates relative to the IV models, especially those where endogeneity bias appears to be a tangible problem based on the significant Hausman IV exogeneity test statistic. There are a handful of cases where the Wald tests after the HT-ar(1) models identify a statistically distinguishable difference in coefficients but these disappear in the IV models.[45] Nonetheless, across all 10 model estimation methods the statistical results consistently show that EB actors not subject to term-limit restrictions (EBUT) produce more conservative revenue forecasts than when the legislature controls this policymaking responsibility under times of unified party government (LBUG). Moreover, the evidence across these 10 models consistently demonstrates that one cannot distinguish revenue forecasts conservatism between legislatures under either unified party government (LBUG) or split partisan branch-unified legislature (LBSB: UL) controlling revenue forecasts from those instances when governors control the revenue forecast and are serving in their lame duck term in office (EBRTLD).[46] Finally, in keeping with a major finding of this study reported in the manuscript, the IV estimation-based results support the claim that LB forecasts are more conservative in the presence of divided partisan legislatures than unified party government. As a matter of fact, this particular finding becomes stronger or robust when accounting for endogeneity bias. In turn, these pair of key findings corroborates a major point of this study—that understanding the policy consequences of institutional design requires understanding the political context in which institutional actors exercise policymaking responsibility. To unequivocally declare that the best solution for arriving at conservative revenue forecasts is for the legislature to delegate this policymaking responsibility to the EB is erroneous. Rather, determining which policymaking institution is most apt to offer conservative revenue forecasts requires a nuanced understanding of the political incentives and pressures facing each political institution, and how these characteristics predictably vary across the American states. Specifically, the optimal choice of a policymaking venue should explicitly account for the possibility of legislative excesses and executive myopia, respectively.

Additional information regarding the institutional venue charged with official revenue forecast policymaking responsibility, and the contact person involved in obtaining this primary source data (and their formal position) is displayed in Table A7.

[45] Specifically, *Legislative Branch: Unified Government = Legislative Branch: Split Branch-Unified Legislature* [LBUG = LBSB-UL] (HT-ar(1), HT*, CSRE-ar(1)*, and CSRE*), *Legislative Branch: Divided Legislature = Executive Branch: No Term Limit* [LBDL = EBUT] (HT-ar(1)*, HT*), and *Executive Branch: No Term Limit = Executive Branch: Term Limit-No Lame Duck* [EBUT = EB-RTNLD] (TWFE*).

[46] The lone exception is LBUT = EBRTLD being rejected at $P < 0.08$ in the HT model.

**Table A6.** Model 4 alternative estimation results (only institutional venue results reported).

| | HT-ar(1) | HT | HT-ar(1)* | HT* | CSRE-ar(1) | CSRE | CSRE-ar(1)* | CSRE* | TWFE | TWFE* |
|---|---|---|---|---|---|---|---|---|---|---|
| *Legislative Branch:* | −7.98** | −9.94*** | −9.09** | −9.55** | −3.61*** | −2.85*** | −4.34 | −2.94 | −2.61*** | −2.72 |
| *Unified Government* | (3.26) | (2.81) | (4.59) | (3.99) | (0.99) | (0.83) | (2.59) | (2.19) | (0.79) | (2.02) |
| *Legislative Branch:* | −5.39 | −7.02** | −6.80 | −7.51* | −1.69 | −1.05 | −2.45 | −1.10 | −1.02 | −1.47 |
| *Split Branch-Unified Legislature* | (3.33) | (2.86) | (4.96) | (4.34) | (1.04) | (0.87) | (2.17) | (1.86) | (0.82) | (1.70) |
| *Legislative Branch:* | −5.60* | −7.18*** | 6.07 | 4.23 | −1.68 | −1.44 | 9.92** | 9.50** | −1.34 | 10.46*** |
| *Divided Legislature* | (3.00) | (2.51) | (6.88) | (6.16) | (1.07) | (0.89) | (4.40) | (3.79) | (0.86) | (3.49) |
| *Executive Branch:* | 1.35 | 0.12 | 6.41 | 4.43 | 1.98 | 2.05** | 3.60 | 4.10* | 2.15** | 3.44 |
| *No Term Limit* | (2.82) | (2.17) | (5.50) | (4.90) | (1.28) | (1.03) | (2.83) | (2.40) | (0.95) | (2.17) |
| *Executive Branch:* | −4.54* | −4.83*** | −13.20** | −14.94*** | −1.64 | −1.36 | −4.35 | −5.72 | −1.17 | −7.02** |
| *TL: Lame Duck* | (2.37) | (1.96) | (5.64) | (5.36) | (1.14) | (0.96) | (4.36) | (3.69) | (0.91) | (3.37) |
| *Executive Branch:* | −2.67 | −3.01 | 5.42 | 7.39 | −0.95 | −0.43 | 5.68 | 4.71 | −0.40 | 6.25 |
| *TL: No Lame Duck* | (2.58) | (2.09) | (8.42) | (10.21) | (1.12) | (0.92) | (5.57) | (4.75) | (0.87) | (4.33) |
| Hausman IV | | | 1.21 | 1.86* | | | 9.47 | 11.79* | | 2.58** |
| exogeneity test | | | [0.30] | [0.08] | | | [0.15] | [0.07] | | [0.02] |
| $H_O$: LBUG = LBSB-UL | 4.05** | 6.19** | 0.42 | 0.43 | 2.67* | 3.04* | 0.36 | 0.47 | 2.45 | 0.25 |
| | [0.04] | [0.01] | [0.52] | [0.51] | [0.10] | [0.08] | [0.55] | [0.49] | [0.12] | [0.62] |
| $H_O$: LBUG = LBDL | 3.66* | 5.85** | 4.15** | 4.17** | 2.92* | 2.04 | 5.97** | 6.15*** | 1.69 | 8.16*** |
| | [0.06] | [0.02] | [0.04] | [0.04] | [0.09] | [0.15] | [0.02] | [0.01] | [0.19] | [0.00] |
| $H_O$: LBSB-UL = LBDL | 0.02 | 0.02 | 3.00* | 3.06* | 0.00 | 0.15 | 6.28** | 6.34*** | 0.10 | 9.56*** |
| | [0.88] | [0.90] | [0.08] | [0.08] | [0.99] | [0.70] | [0.01] | [0.01] | [0.75] | [0.00] |
| $H_O$: LBUG = EBUT | 7.05*** | 11.95*** | 4.43** | 4.33** | 14.07*** | 16.28*** | 4.36** | 4.79** | 17.58*** | 4.41** |
| | [0.01] | [0.00] | [0.04] | [0.04] | [0.00] | [0.00] | [0.04] | [0.03] | [0.00] | [0.04] |
| $H_O$: LBSB-UL = EBUT | 3.68* | 5.98** | 3.11* | 3.00* | 5.79** | 6.14** | 3.02* | 3.06* | 7.30*** | 3.31* |
| | [0.06] | [0.02] | [0.08] | [0.08] | [0.02] | [0.01] | [0.08] | [0.08] | [0.01] | [0.07] |
| $H_O$: LBDL = EBUT | 4.39** | 7.40*** | 0.00 | 0.00 | 5.71** | 7.73*** | 1.61 | 1.60 | 8.69*** | 3.21* |
| | [0.04] | [0.01] | [0.97] | [0.98] | [0.02] | [0.01] | [0.21] | [0.21] | [0.00] | [0.07] |
| $H_O$: LBUG = EBRTLD | 0.94 | 3.03* | 0.32 | 0.75 | 2.12 | 1.70 | 0.00 | 0.43 | 1.73 | 1.23 |
| | [0.33] | [0.08] | [0.57] | [0.39] | [0.15] | [0.19] | [1.00] | [0.51] | [0.19] | [0.27] |
| $H_O$: LBSB-UL = EBRTLD | 0.06 | 0.54 | 0.72 | 1.33 | 0.00 | 0.08 | 0.15 | 1.24 | 0.02 | 2.13 |
| | [0.81] | [0.46] | [0.40] | [0.25] | [0.98] | [0.78] | [0.70] | [0.27] | [0.89] | [0.15] |

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**Table A6.** Continued.

| | HT-ar(1) | HT | HT-ar(1)* | HT* | CSRE-ar(1) | CSRE | CSRE-ar(1)* | CSRE* | TWFE | TWFE* |
|---|---|---|---|---|---|---|---|---|---|---|
| $H_O$: LBDL = EBRTLD | 0.10 | 0.76 | 4.12** | 5.37** | 0.00 | 0.00 | 4.48** | 7.07*** | 0.02 | 11.12*** |
| | [0.75] | [0.39] | [0.04] | [0.02] | [0.98] | [0.95] | [0.03] | [0.01] | [0.88] | [0.00] |
| $H_O$: LBUG = EBRTNLD | 2.04 | 5.02** | 1.55 | 1.94 | 3.90** | 4.63** | 2.4831 | 1.97 | 4.25** | 3.26* |
| | [0.15] | [0.03] | [0.21] | [0.16] | [0.05] | [0.03] | [0.12] | [0.16] | [0.04] | [0.07] |
| $H_O$: LBSB-UL = EBRTNLD | 0.53 | 1.64 | 1.06 | 1.49 | 0.28 | 0.28 | 1.59 | 1.10 | 0.31 | 2.30 |
| | [0.47] | [0.20] | [0.30] | [0.22] | [0.60] | [0.60] | [0.21] | [0.30] | [0.58] | [0.13] |
| $H_O$: LBDL = EBRTNLD | 0.69 | 2.12 | 0.02 | 0.06 | 0.28 | 0.76 | 0.41 | 0.71 | 0.72 | 0.68 |
| | [0.41] | [0.15] | [0.90] | [0.81] | [0.60] | [0.38] | [0.52] | [0.40] | [0.40] | [0.41] |
| $H_O$: EBUT = EBRTLD | 3.97** | 4.47** | 5.98** | 6.07** | 5.21** | 6.79*** | 2.11 | 4.41** | 7.26*** | 6.05** |
| | [0.05] | [0.04] | [0.02] | [0.01] | [0.02] | [0.01] | [0.15] | [0.04] | [0.00] | [0.01] |
| $H_O$: EBUT = EBRTNLD | 1.65 | 1.62 | 0.04 | 0.08 | 3.50* | 3.78* | 0.12 | 0.01 | 4.58** | 0.37 |
| | [0.20] | [0.20] | [0.84] | [0.78] | [0.06] | [0.05] | [0.73] | [0.90] | [0.03] | [0.54] |
| $H_O$: EBRTLD = EBRTNLD | 1.81 | 1.94 | 2.83* | 2.94* | 0.30 | 0.66 | 1.46 | 2.24 | 0.47 | 4.42** |
| | [0.18] | [0.16] | [0.09] | [0.09] | [0.58] | [0.42] | [0.23] | [0.13] | [0.50] | [0.04] |
| $R^2$ | 0.17 | 0.18 | 0.19 | 0.19 | 0.08 | 0.09 | 0.09 | 0.10 | 0.09 | 0.10 |
| Effective sample size | 1,042 | 1,097 | 1,042 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 | 1,097 |

*Notes:* HT-ar(1): Hausman–Taylor/FEVD estimation with AR(1) serial correlation correction; HT: Hausman–Taylor/FEVD estimation; HT-ar(1)*: Hausman–Taylor/FEVD estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; HT*: Hausman–Taylor/FEVD estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; CSRE-ar(1): cross-sectional random effects estimation with AR(1) serial correlation correction; CSRE: cross-sectional random effects estimation; CSRE-ar(1)*: cross-sectional random effects estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; CSRE*: cross-sectional random effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; TWFE: time-wise fixed effects estimation; TWFE*: time-wise fixed effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates. Standard errors appear inside parentheses. Probability values appear inside brackets.

*$P \leq 0.10$; **$P \leq 0.05$; ***$P \leq 0.01$.

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**Table A7.** Institutional venue responsibility for official revenues forecasts.

| State name | Who is responsible for the official forecast? | Fiscal years | Source |
|---|---|---|---|
| Alabama | L | 1987–2008 | Jennie Smith<br>Executive Budget Office |
| Alaska | CG | 1987–2008 | Rob Carpenter<br>Legislative Finance Division; and<br>Dennis Hawes<br>Department of Revenue |
| Arizona | L | 1987–2008 | Bret Kloninger<br>Office of Strategic Planning and Budget |
| Arkansas | E | 1987–2008 | Mike Storms<br>Department of Finance and Administration |
| California | L | 1987–2008 | Cedrik Zemitis<br>Department of Finance |
| Colorado | E<br>L | 1987–1988<br>1989–2008 | Tod Harried<br>Legislative Council |
| Connecticut | L | 1987–2008 | Tom Fiore<br>Budget & Financial Management Division |
| Delaware | CG | 1987–2008 | Dave Gregor<br>Economic & Financial Advisory Council; and<br>Burt Scalene<br>State Budget Office |
| Florida | CG | 1987–2003 | Amy Baker<br>Legislative Office of Economic & Demographic Research; and<br>Don Langston<br>House Economist |
| Georgia | E | 1987–2008 | Thomas Lauth<br>University of Georgia |
| Hawaii | CG | 1987–2008 | John Mapes and Dean Hirata<br>Department of Budget and Finance; and<br>Pearl Iboshi<br>Council on Revenues |
| Idaho | L | 1987–2008 | Michael Ferguson<br>Division of Financial Management |
| Illinois | L | 1987–2008 | Jim Muschinske<br>Commission on Government Forecasting & Accountability |
| Indiana | CG | 1987–2008 | Bob Lain<br>State Budget Agency; and<br>Jim Landers<br>Legislative Services Agency |
| Iowa | CG | 1987–2008 | Joel Lunde<br>Department of Management |
| Kansas | CG | 1987–2008 | Anne Durkes and Sean Tomb<br>Division of the Budget |
| Kentucky | E<br>CG | 1987–1993<br>1994–2008 | Robert Cox<br>Governor's Office for Economic Analysis; and<br>Frank O'Connor<br>Eastern Kentucky University |

**Table A7.** Continued.

| State name | Who is responsible for the official forecast? | Fiscal years | Source |
|---|---|---|---|
| Louisiana | L | 1987 | Greg Albrecht |
| | CG | 1988–2008 | Legislative Fiscal Office |
| Maine | E | 1987–1992 | George Bernstein |
| | CG | 1993–2008 | Bureau of the Budget; and<br>Marc Cyr<br>Office of Fiscal and Program Review |
| Maryland | CG | 1987–2008 | David Roose<br>Bureau of Revenue Estimates |
| Massachusetts | CG | 1987–2008 | Kazim Ozyurt<br>Department of Revenue |
| Michigan | L | 1987–1989 | Rebecca Ross |
| | CG | 1990–2008 | House Fiscal Agency |
| Minnesota | E | 1987–2003 | Tom Stinson<br>Department of Finance |
| Mississippi | E | 1987–1992 | Deborah Biggers |
| | CG | 1993–2008 | Office of Fund and Budget Management |
| Missouri | CG | 1987–2008 | Kevin Highfill and Kevin Suress<br>Division of Budget and Planning |
| Montana | L | 1987–2008 | Judy Paynter<br>Office of Budget and Program Planning |
| Nebraska | CG | 1987–2008 | Michael Colvert<br>Legislative Fiscal Office |
| Nevada | L | 1987–1995 | Russell Guindon |
| | CG | 1996–2008 | Fiscal Analyst Division, Nevada Legislature |
| New Hampshire | L | 1987–2008 | Tom Martin<br>Office of the Comptroller |
| New Jersey | L | 1987–2008 | Dick Caluzzni<br>Department of the Treasury |
| New Mexico | CG | 1987–2008 | Melissa Vigil and Sam Flaim<br>State Board of Finance |
| New York | L | 1987–1995 | Robert Megna and Lynn Holland |
| | CG | 1996–2008 | Division of the Budget |
| North Carolina | L | 1987–2008 | David Crotts<br>Legislative Economist |
| North Dakota | E | 1987–2008 | Joe Morrissette<br>Office of Management and Budget |
| Ohio | L | 1987–2008 | Tim Keen<br>Office of Management and Budget |
| Oklahoma | CG | 1987–2008 | James Wilbanks<br>State Treasurer's Office |
| Oregon | E | 1987–2008 | Michael Kennedy<br>Budget and Management Division |
| Pennsylvania | E | 1987–2008 | Stacy Knavel<br>Department of Revenue |
| Rhode Island | E | 1987–1990 | Paul Dion |
| | CG | 1991–2008 | Office of Revenue Analysis; and<br>Thomas Mullaney<br>State Budget Office |

**Table A7.** Continued.

| State name | Who is responsible for the official forecast? | Fiscal years | Source |
|---|---|---|---|
| South Carolina | CG | 1987–2008 | William Gillespie<br>Board of Economic Advisors; and<br>Gordon Shuford<br>Budget and Control Board |
| South Dakota | L | 1987–2008 | Angella Van Scharrel<br>Bureau of Finance and Management |
| Tennessee | E | 1987–1992 | James White |
| | CG | 1993–2008 | Fiscal Review Committee; and<br>Zhijie Qi<br>Department of Revenue |
| Texas | E | 1987–2008 | James LeBass<br>Comptroller of Public Accounts Office |
| Utah | L | 1987–2008 | Peter Donnor and Lance Rovick<br>Governor's Office of Planning and Budget |
| Vermont | L | 1987–1995 | Steve Kline |
| | CG | 1996–2008 | Joint Fiscal Office |
| Virginia | E | 1987–2008 | John Layman<br>Department of Taxation |
| Washington | CG | 1987–2008 | Eric Swenson and Desiree Carson<br>Economic and Revenue Forecast Council |
| West Virginia | E | 1987–2008 | Mark Mushow<br>Department of Revenue |
| Wisconsin | L | 1987–2008 | Rob Reinhardt<br>Legislative Fiscal Bureau |
| Wyoming | CG | 1987–2008 | Buck McVeigh<br>Consensus Revenue Estimating Group |

E: executive branch; L: legislative branch; CG: consensus group.

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