The Decision Window: How Budget Timing Shapes Fiscal Policy

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Abstract

Whether economic news affects fiscal policy depends on when it arrives. Using hand-collected budget documents for U.S. states over 1995–2024, we show that enacted state budgets lock in 69 percent of within-state spending variation, and we identify when those budgets incorporate information. Exploiting staggered proposal dates, whereby the same national shock falls inside one state's budget-writing months and outside another state's, we find that news arriving in the five months before a state's proposal date—the "decision window"—is incorporated into revenue forecasts and spending, while revenue-relevant news arriving outside the window is not incorporated and instead predicts forecast errors. Budget timing gates the transmission of economic news to the real economy: decision-window news moves private employment by roughly four to five times as much as off-window news, with larger effects for bad news. Monetary policy affects state spending more when it arrives during the state's decision window, suggesting an institutional source of "long and variable lags."

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Economic news arrives continuously, but government policy is decided at discrete dates, based on budget calendars. This mismatch could make the effect of a shock depend on when it arrives. We document this calendar dependence in U.S. state budgeting, where states form spending budgets based on their revenue expectations. Revenue-relevant news enters the enacted budget primarily during a five-month window before the initial budget proposal date—the "decision window"—while news arriving before or after that window is left out of the current budget. Because revenue expectations directly influence government spending and affects private employment, the budget calendar determines not only when fiscal policy responds, but also how economic shocks propagate to the real economy via state governments.

To trace how news affects government expectations, spending plans, actual spending, and then real outcomes, we need data on states' own forecasts and spending plans. We hand-collect the budget documents of 46 U.S. states plus Washington, D.C. from 1995 to 2024, observing for each state-year the government's enacted spending budget, corresponding revenue forecast, and realized revenues and spending. We show that policy is tightly anchored to the budget plan. The enacted spending budget accounts for 69 percent of within-state variation in realized spending growth, and the budget itself is set against the revenue forecast, a link that balanced-budget rules make binding (Poterba, 1994). The forecast is therefore the gate between news and policy: what it misses, fiscal policy misses. Which news becomes fiscal policy thus includes a question of timing: when does information enter the forecast?

Consider New Jersey and Virginia, which both begin their fiscal year on July 1 but propose budgets at different times: Virginia in December, New Jersey in February. In January 2016, oil prices fell sharply and renewed depreciation of the renminbi pushed the S&P 500 down 5 percent in a month.[1] The shock arrived inside New Jersey's decision window but after Virginia's had closed. New Jersey revised its FY2017 revenue forecast down nearly 9 percent and cut planned spending by about 16 percent; Virginia, whose forecast was set before the shock, planned for revenue growth of 6 percent and accordingly increased its budgeted spending by 6 percent.

We generalize this example across all states for our identification. Each state budgets on a fixed annual calendar anchored by three dates: the governor's budget proposal, the legislature's enactment, and the start of the fiscal year. Proposal dates range across states from November to March, and are sticky: they are set by constitutions or long-standing

[1]See Bloomberg News, "Equity Slump Set to Continue in Asia as Crude Lingers Around $34," January 6, 2016, https://www.bloomberg.com/news/articles/2016-01-06/equity-slump-set-to-continue-in-asia-as-crude-lingers-around-34.

procedure. Thus, a national shock falls at different points in each state's budget cycle. We combine this cross-state identification with a state-specific monthly revenue shock, which we construct by weighting national innovations to five revenue bases by each state's lagged revenue composition. Fiscal-year-by-calendar-month fixed effects absorb whatever is common to a month, so identification comes from where news falls relative to each state's proposal date.

This design delivers three sets of results. First, we show that the decision window exists: state budgets incorporate revenue news only when it arrives inside the window, even though off-window news, both before and after it, also predicts revenues. Second, the decision window matters for the real economy: the same news moves employment persistently in government-exposed industries when it arrives on-window and barely at all off-window, a contrast that lets us quantify the real effects of routine budget adjustment. Third, the decision window effect applies to other shocks: monetary policy surprises affect state spending significantly more when they arrive in-window, providing an institutional source of the "long and variable lags" of monetary policy.

We establish the first result with a forecast-incorporation test. Although the enacted revenue forecast is not finalized until enactment, typically months after the governor's proposal, only news arriving in the five months before the proposal enters it. Off-window news on either side instead predicts forecast errors and realized revenue in future fiscal years. Each side of this exclusion is surprising. Pre-window news is months old and freely available by the time the forecast is written, yet it systematically stays out. This differs from standard sticky information, under which earlier news is caught up at the next update (Mankiw and Reis, 2002). We interpret this left boundary as limited processing and imperfect retention of earlier information (Gabaix, 2014; Azeredo da Silveira et al., 2024). Post-window news arriving between the proposal and the start of the fiscal year is the newest information available, yet it is also not incorporated into forecasts or spending decisions. This is the opposite of overreaction to recent news or fading memory, under which the newest signals carry the greatest weight (Bordalo et al., 2018; Nagel and Xu, 2022; Afrouzi et al., 2023). We interpret this right boundary as organizational commitment after the proposal: revising the forecast after the budget is proposed requires reopening the spending plan built around it (National Association of State Budget Officers, 2021). Together, the two frictions produce a discrete period of incorporation defined by the budget calendar.

The forecast, in turn, translates into planned and, ultimately, realized state spending. When we instrument the revenue forecast with decision-window news, a 1 percent news-

induced increase in the forecast raises planned spending by 0.36 percent and realized spending by 0.52 percent; off-window news, instead, does not move the revenue forecast.

Next, we show that the decision window matters for the real economy. Firms in industries that sell more to state and local governments are more exposed to the state budget through procurement. Comparing more- and less-exposed industries within the same state and year, decision-window news that raises the state's revenue forecast by 1 percent raises private employment in exposed industries by 0.10 percentage points and wages by 0.14 percentage points, while the same news arriving off-window has little effect. The differential response appears significantly once the fiscal year begins and persists for several years. We further find that state-level employment responds roughly four to five times more strongly to decision-window news than to off-window news.

The employment response is asymmetric. Negative decision-window news produces large and persistent contractions in exposed industries, while positive news generates smaller and more transient expansions. In contrast, government spending responds more strongly to positive news, indicating that the asymmetric private employment response is not mechanically following the budget. We interpret it instead as a firm-level adjustment friction. When bad news arrives inside the window, states convert it into same-year spending cuts that exposed firms have little time to absorb (Barrot and Nanda, 2020). In contrast, bad news arriving off-window enters a later budget cycle, so the eventual cuts come with over a year of notice, allowing firms with fiscal foresight to adjust gradually (Leeper et al., 2013).

Beyond these industry-level responses, the on/off window design also lets us estimate directly something that standard fiscal-multiplier designs cannot: the real effects of the systematic response of budgets to economic conditions. If we write spending as a rule plus a shock, $G = G(X) + \varepsilon$, the credible designs in the literature identify the effects of ε via military procurement, federal stimulus, or innovations relative to expectations, precisely because these are plausibly unrelated to local conditions. We instead identify the real effects of a key margin of the rule's routine response to news. Dividing the aggregate on- versus off-window employment premium by the corresponding premium in realized spending, we estimate that a 1 percent increase in state spending generated by routine budget adjustment raises private employment by 0.30 percent and private wages by 0.42 percent. This implies between \$41,000 and \$79,000 of induced spending per private job-year, in the range of the cross-region multiplier literature (Chodorow-Reich, 2019). Similar to much of the literature, these are relative, cross-state estimates; they do not capture aggregate general-equilibrium

effects.[2]

Finally, we document that the decision window matters for monetary policy transmission. We use the same month-fixed-effects specification to show that a contractionary monetary policy surprise lowers a state's budgeted and realized spending when it arrives during that state's decision window, relative to the same surprise arriving outside it. We employ similar high-frequency shocks commonly used in the literature, constructed in Bauer and Swanson (2023); given our empirical design, calendar time fixed effects absorb any potential correlation the shocks have with other macroeconomic variables (Bauer and Swanson, 2023; de Fraisse et al., 2026). The aggregate time series also displays a pattern consistent with our tightly identified result. Many states' decision windows overlap the fourth calendar quarter, and we find that indeed, Q4 tightening surprises are followed by larger and more persistent declines in national state-and-local spending that build over two to three years. Meanwhile, surprises in the first three quarters have negligible effects, even though Q4 and non-Q4 surprises are statistically indistinguishable in size and distribution. The Q4 state-and-local spending response is about one-fifth to one-quarter of the corresponding total output response after three to four years. These results provide novel evidence of an institutional narrative of the "long and variable" lags of monetary policy effects (Friedman, 1961): how long policy takes to reach state spending depends on where in the budget cycle it arrives.

One caveat to our results is that budgets can still change during the fiscal year, as states can pass supplemental appropriations and executives can withhold spending in the case of emergencies. Importantly for our exercise, the original enactment accounts for 69% of within-state variation in realized spending (Section 1.3). In the case of very significant crises, like the 2008 financial crisis, the hardest-hit states may attract a larger share of federal stimulus, which offsets these states' own spending cuts and biases our estimated effects toward zero.[3] We conduct a series of robustness checks to show our baseline results hold (and in many cases strengthen) when excluding the Global Financial Crisis (GFC).

This paper contributes to four literatures. We contribute first to the empirical literature on the real effects of fiscal policy. Because government spending is inherently political and

[2]The mapping from cross-state estimates to aggregate multipliers is not one-for-one. Cross-state comparisons difference out the monetary and federal-tax responses that dampen aggregate effects in normal times (Nakamura and Steinsson, 2014), while trade across states and positive spillovers could make them a rough lower bound on the aggregate no-monetary-response, deficit-financed multiplier (Chodorow-Reich, 2019; Auerbach et al., 2020).

[3]This behavior is consistent with models of state-dependent updating, where only sufficiently large shocks move the state outside an inaction band (Barro, 1972; Caballero and Engel, 1999).

can be endogenous to fundamentals like productivity or growth expectations, credible estimates of fiscal effects typically rely on rare and unusual spending variation precisely because it is plausibly exogenous (Ramey, 2011; Nakamura and Steinsson, 2014; Chodorow-Reich et al., 2012; Wilson, 2012; Suárez Serrato and Wingender, 2016); see Chodorow-Reich (2019) for a survey.[4] Our estimates resemble that literature in mapping employment responses to government spending, but differ by identifying the systematic, revenue-expectations-driven component of budget policy rather than deviations from it.[5] Ramey (2011) shows that the timing of fiscal news is central to identifying the effects of spending shocks; we show that timing also determines whether news enters the budget at all. Within state public finance, Poterba (1994) show that budget institutions affect the speed at which states adjust their budgets, and Clemens and Miran (2012) show that balanced budget requirements promote procyclical policy. We differ by holding the rules fixed and exploiting variation in budget calendars, thereby establishing that the timing of news, not just the stringency of the institutions, determines whether procyclical adjustment occurs. The distinction matters for stabilization policy: aid or forecasts that arrive after budgets are committed may prevent later cuts, but they are unlikely to change the same-year spending plans that govern procurement and employment. The relevant policy horizon is the decision window, not the fiscal year.

Second, we contribute to work on expectations formation, information incorporation, and their real effects (Manski, 2004; Coibion and Gorodnichenko, 2012, 2015; Gennaioli et al., 2016; Bordalo et al., 2020). Our test builds on Coibion and Gorodnichenko (2012) by using forecast errors regressed on economic shocks to understand expectations formation, with a few important differences: our forecasts are not an average across many professionals, but rather governments forecasting their own revenues over which they have some control.[6] Our decision window result is a form of time-dependent updating, which can be micro-founded by costly information acquisition (Mankiw and Reis, 2002; Reis, 2006): once the forecast anchors the proposed budget, revising it requires reopening the spending plan. This organi-

[4] Fiscal policy can also move asset prices, as shown in Wiegand (2025).

[5] A related literature shows that fiscal multipliers vary over the business cycle (Auerbach and Gorodnichenko, 2012; Ramey and Zubairy, 2018); our results add that they vary over the budget cycle. Because our spending variation is generated by the budget rule rather than by windfall transfers, it embeds the state's own financing constraint; Clemens and Miran (2012) show that such rule-induced, deficit-financed variation yields smaller multipliers than windfall-financed variation, consistent with Ricardian effects. Appendix G formalizes the object our design identifies.

[6] A related literature studies the bias and rationality of revenue forecasts themselves: state revenue forecasts tend to be biased downward (Feenberg et al., 1989), while national governments' official forecasts tend toward over-optimism (Frankel, 2011; Frankel and Schreger, 2013); Mikesell and Ross (2014) study how consensus forecasting builds political acceptance of the forecast. Our focus is on what information these forecasts incorporate, and the real consequences.

zational friction also resembles contract rigidities, as in Taylor (1980). Incomplete incorporation of older information could reflect experience-based learning, diagnostic expectations, and models in which costly processing or imperfect memory generates declining weights on older information (Malmendier and Nagel, 2016; Bordalo et al., 2018; Afrouzi et al., 2023; Azeredo da Silveira et al., 2024). Our contribution is to show that these forces operate jointly through a directly observed, state-specific institutional calendar: the weight placed on aggregate news is shaped by whether it arrives during the government's decision window.[7]

Third, our real-effects evidence speaks to work on government demand and firm outcomes. State budgets reach the private economy largely through procurement, and our industry-level results locate the response among government-exposed vendors, consistent with evidence that government demand shapes firm employment and dynamics (Barrot and Nanda, 2020; Cox et al., 2024; Ferraz et al., 2015). Our industry-exposure design is similar in spirit to Nekarda and Ramey (2011), who map government-purchasing shares to industry outcomes; however our identification focus is on cross-state variation in budget timing. The asymmetry we document, where negative news contracts exposed employment by more than positive news expands it, is consistent with credit-constrained government vendors (Barrot and Nanda, 2020). A closely related paper is Liebman and Mahoney (2017), who show that expiring federal appropriations distort the timing of procurement outlays, as agencies rush lower-quality spending into the final weeks of the fiscal year. We differ in three respects. First, we study the decision margin rather than the execution margin; second, we observe the full chain from revenue forecast to enacted commitment to realized spending, rather than outlays alone; third, we document the real effects via private sector employment outcomes. More broadly, our setting makes observable a friction emphasized in organizational economics: institutions receive information continuously but revise commitments only at discrete, calendar-fixed decision points (Dessein and Santos, 2006; Dessein et al., 2016; Grenadier et al., 2016; Malenko, 2024). The same friction should operate in any organization that commits to plans on a fixed cycle, from corporate capital budgets to central-bank projections.

Finally, we contribute to the literature on monetary transmission and its lags. A large body of work identifies monetary shocks from narrative records and high-frequency asset-price responses (Romer and Romer, 2004; Gertler and Karadi, 2015; Nakamura and Steinsson, 2018; Bauer and Swanson, 2023) and traces their propagation to real activity (Christiano

[7] Puri (2025) distinguishes uncertainty about payoff timing from uncertainty about outcomes. We study a related timing dimension in a different context: how the arrival of news relative to an institutional deadline shapes its incorporation into official forecasts and subsequent spending decisions.

et al., 1999, 2005; Miranda-Agrippino and Ricco, 2021), finding that transmission strength varies with economic conditions (Tenreyro and Thwaites, 2016). A closely related paper is Bouscasse and Hong (2026), who show how federal fiscal policy responds to monetary policy. We differ from this body of work in two ways: we focus on state spending as an outcome, and add an institutional margin: the same monetary surprise moves a state's spending significantly more when it arrives inside that state's decision window than outside it. Olivei and Tenreyro (2007) document that policy surprises have a stronger effect in Q1-Q2 due to wage rigidities; our evidence isolates a distinct, institutional channel that leads to larger spending effects for Q4 shocks; moreover our effect is also identified in the cross section of states based on budget institutions. Our mechanism parallels Selgrad and Siani (2025), who show that monetary policy transmits through firms' long-horizon investment plans; we document the government analogue, operating through revenue-expectations incorporation and planned spending.

The rest of the paper is organized as follows. Section 1 describes the state budget process, data, and discusses the importance of the original enacted budget for actual spending. Section 2 develops the forecast-error test used to identify when the budget process incorporates revenue-relevant information. Section 3 constructs the revenue-news measure and identifies the budget decision window. Section 4 estimates the real effects of the decision window and shows the implications for monetary policy transmission. Section 5 concludes.

1 The State Budget Process and Data

1.1 The State Budget Process

State operating budgets follow a common sequence. Governments first enter a planning stage in which the executive prepares a revenue forecast and budget proposal, then a deliberation stage in which the legislature revises and enacts the budget, and finally an execution stage in which spending is implemented during the fiscal year. Figure 1 illustrates that budgeting process for Massachusetts fiscal year (FY) 2026, which ends June 30, 2026. Budget preparation begins in the second half of 2024, then the governor submits a budget "proposal" in January 2025, and then the legislature deliberates before reconciling a final enacted budget around June 2025. The budget incorporates revenue forecasts from meetings with economists and legislators, as well as spending plans that incorporate the political priorities of the gov-

ernor and legislative bodies.[8] Across states, procedural details differ, but the core timing fact is shared: major same-year spending is determined several months, and in some cases years, before the fiscal year begins.

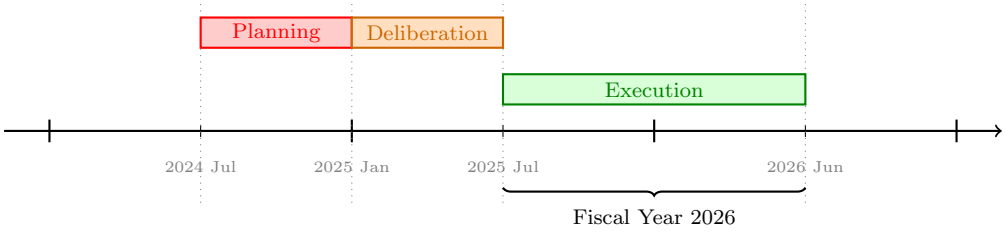


Figure 1: **Timing of the Budget Process for Massachusetts (FY 2026).** This figure illustrates the timing of planning (pre-proposal), deliberation (proposal to enactment), and execution (fiscal-year implementation) for Massachusetts.

This creates an institutional friction: revenue news arrives continuously, but same-year spending commitments are revised only at discrete points in the calendar. The proposal date is the first common observable point at which a state presents a statewide revenue forecast together with a spending plan. Proposal dates differ meaningfully across states, ranging from November to March, even though most fiscal years begin on July 1. Each state's budget timing is typically determined by constitutions, statutes, and long-standing procedures and is therefore plausibly exogenous to contemporaneous fundamentals. Enactment dates, by contrast, can respond to economic conditions (Andersen et al., 2012).

1.2 Data and Sample

We assemble a new state-level fiscal panel covering 1995–2024 by hand-collecting records of the Annual Comprehensive Financial Report (ACFR) from state websites, direct correspondence with state officials, and formal records requests.[9] We digitize these reports using Python and manually verify the extracted data.

We begin ACFR-based analyses in FY2002, when two changes substantially improve comparability. First, GASB Statement No. 34 required certain larger states to report

[8]For revenue forecasts, 28 states develop a consensus forecast, often with participation by economic advisors; in 12 states, both executive and legislative branches draft independent forecasts, while in 10 states, the executive branch develops the forecast on its own (National Association of State Budget Officers, 2021).

[9]The availability of ACFR information varies across states. We obtained 84 additional state-year reports by directly emailing state officials, 11 additional reports from phone calls, and 35 by formal record request; the remainder were available on state websites.

original-budget amounts for periods beginning after June 15, 2001.[10] Second, coverage rises sharply: the number of jurisdictions with complete expenditure data increases from 29 in FY2001 to 40 in FY2002, and the number with complete expenditure and revenue data rises from 25 to 34. We retain earlier observations to document coverage (Appendix Figure A.1) but exclude them from ACFR-outcome regressions to avoid relying on a small, changing pre-standard sample.

We rely on ACFR for budget data rather than the commonly used National Association of State Budget Officers (NASBO) Fiscal Survey of States for two reasons. First, while NASBO is constructed from a semiannual survey collected simultaneously for all states, the ACFR reports the budget dated according to each state's own budget cycle, which is central to our analysis. Second, the two sources measure different fiscal aggregates, and the ACFR is more standard across states. Appendix A.3 describes the differences between the two data sources in detail.

We measure state budget calendars from archival publications of the NASBO, which report proposal dates, enactment dates, fiscal-year start dates, and budget-cycle rules. We construct a piecewise-constant calendar from the NASBO waves, assigning each fiscal year the most recent prior NASBO snapshot.[11] Proposal dates are highly persistent across waves. Figure 2 shows the cross-state variation in proposal timing using the 2021 NASBO snapshot. Proposal months vary substantially across states and do not follow a simple geographic pattern.[12]

Each state's ACFR reports a budgetary comparison statement with three fiscal-year objects: the "original budget," the "final budget," and actual outcomes (see Appendix Figure A.2). The original budget is the first complete appropriated budget adopted for the fiscal year, including legally authorized changes made before the fiscal year begins.[13] Thus, the ACFR original budget is enacted prior to the fiscal year start, and is distinct from the governor's initial proposal. This distinction is important for interpretation. The proposal date is the first common observable point at which the state presents a statewide revenue forecast alongside a spending plan, so we use it to organize the budget calendar. However,

[10] This was known as Phase 1, which comprised governments with at least $100 million in annual revenues.

[11] The NASBO budget-process snapshots used in the paper are 1987, 1989, 1992, 1997, 1999, 2002, 2008, 2015, and 2021. For each fiscal year, we use the most recent snapshot observed at or before that year and carry it forward until a newer snapshot is available. This avoids using future calendar information.

[12] Appendix Table A.1 reports each state's full budget-cycle timing in the 2021 snapshot.

[13] This structure is due to GASB 34. In paragraph 131, the original budget is defined as "the first complete appropriated budget," adjusted for reserves, transfers, and other legally authorized changes made before the fiscal year begins.

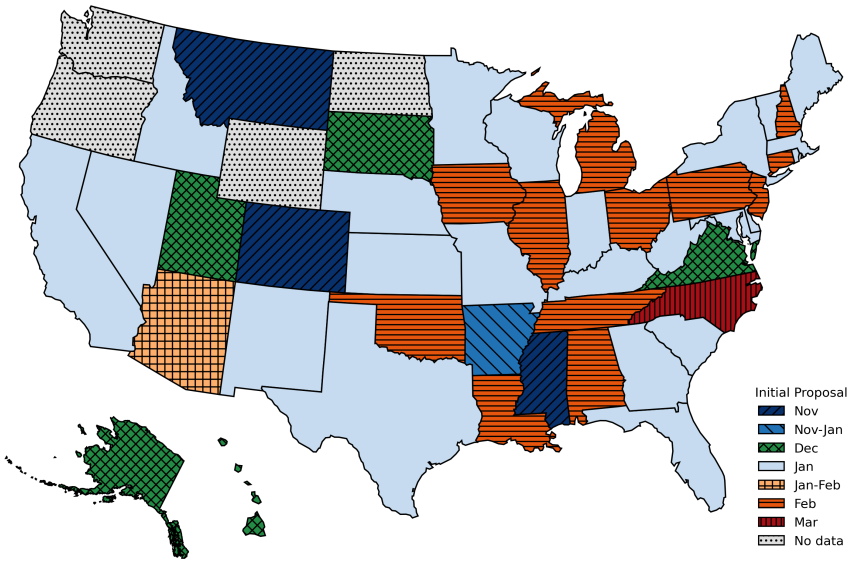


Figure 2: **Cross-State Variation in Original Budget Proposal Month.** The map shades each state by the typical month in which its governor submits the original budget proposal to the legislature, taken directly from Table A.1 in Appendix A. Colors range from November (dark blue) through January (light blue), February (orange), and March (dark red); December is shown in green, and states with two-month proposal windows (Nov–Jan, Jan–Feb) use intermediate colors. Hatch patterns are included so categories remain distinguishable in black-and-white printing. States shown in light grey (North Dakota, Oregon, Washington, Wyoming) are not in our analysis sample.

in our data, we directly observe the enacted original budget, which is finalized later. We then test empirically how much the enacted budget's revenue forecast $\text{Budget}^R_{s,t}$ and planned spending $\text{Budget}^E_{s,t}$ incorporates information before and after the proposal date. We do not use the "final budget" in our analysis, because it incorporates within-year legally authorized changes whose timing is unclear.

Balanced-budget requirements are widespread at the budget stage. Forty-five states require the governor to submit a balanced budget, 44 require the legislature to pass a balanced budget, and 41 require the budget signed by the governor to be balanced. Ex-post requirements are less uniform: 35 states require the executed budget at year end to be balanced (National Association of State Budget Officers, 2021). Consistent with this requirement, state expenditures broadly track revenues over the budget cycle (see Appendix B.3 for details).[14]

[14]States can manage prospective deficits through spending cuts, drawing down on rainy day funds, deferred payments, inter-fund transfers, short-term borrowing, and in rarer cases, revenue increases or accounting changes. See Poterba (1996) for a detailed discussion.

1.3 Original Budget Explains Most Spending Variation

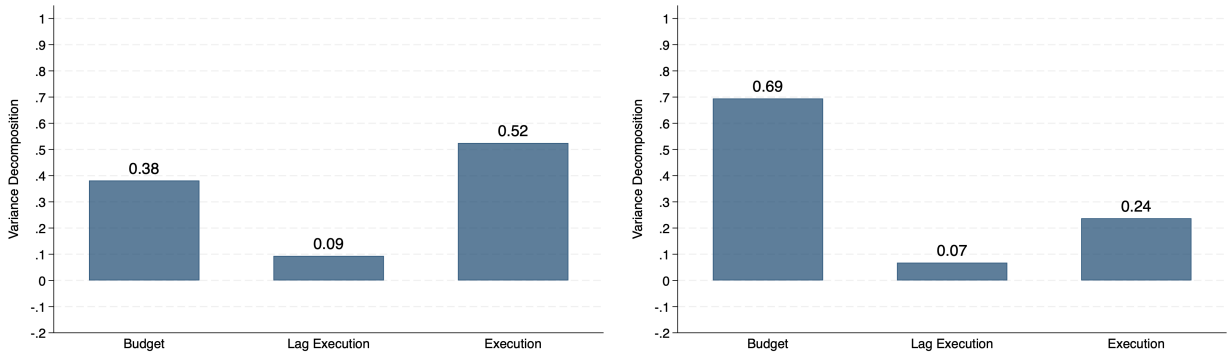
We first show that the original budget is the key commitment margin for state fiscal policy. To quantify its importance, we decompose annual changes in realized outcomes into three components: growth in the original budget, the current-year execution deviation from that budget, and the lagged execution deviation. For fiscal year t, we benchmark original-budget growth to the prior year's original budget, $\text{Budget}_{s,t-1}$, and write:[15]

$$\Delta \log(\text{Act}_{s,t}) = \underbrace{\log\left(\frac{\text{Budget}_{s,t}}{\text{Budget}_{s,t-1}}\right)}_{\text{Budget}} + \underbrace{\log\left(\frac{\text{Act}_{s,t}}{\text{Budget}_{s,t}}\right)}_{\text{Execution}} - \underbrace{\log\left(\frac{\text{Act}_{s,t-1}}{\text{Budget}_{s,t-1}}\right)}_{\text{Lag execution}}. \quad (1)$$

Summary statistics for these components are reported in Table 1; Appendix Figure B.1 plots their distributions.

Figure 3: **Decomposing the Sources of Variation in Actual Growth**

This figure plots the variance-decomposition coefficients implied by Equation (1), for total real revenues (Panel a) and total real expenditures (Panel b). Components are: Budget $\log(\text{Budget}_{s,t}/\text{Budget}_{s,t-1})$, Execution $\log(\text{Act}_{s,t}/\text{Budget}_{s,t})$, and Lag Execution $\log(\text{Act}_{s,t-1}/\text{Budget}_{s,t-1})$.



(a) Total Revenues

(b) Total Expenditures

Figure 3 reports the variance decomposition implied by Equation (1).[16] The planning

[15] This benchmark treats the prior year's original budget as the reference point from which the FY-t budget is drafted. Appendix Table B.1 supports this choice: when the log original budget is regressed on the two most recent available benchmarks, the prior original budget dominates, while two-year-lagged actual outcomes add little conditional on it.

[16] We take the covariance of both sides of Equation (1) with respect to log changes in actual outcomes, then divide by the variance of log changes in actual outcomes. Then we compute each term using an OLS regression with state fixed effects. See Appendix B.2 for more details on how the variance decomposition is

component alone accounts for 38% of the within-state variation in revenue growth and 69% of the within-state variation in expenditure growth. The contrast between the two is notable: while revenues often deviate from the forecast, spending deviates little. Thus, the original budget is important: once those commitments are set, execution deviations in spending contribute relatively little to the annual variation in realized spending within state.

The dominance of the original budget also shows up in line-item adjustments. Appendix B.3 shows that, at the budget stage, positive and negative revenue-forecast updates pass through to spending plans in broadly symmetric ways across categories. The asymmetry appears later, during execution: when realized revenue falls short of the budget, states cut mainly education spending; when revenue exceeds the budget, windfalls flow primarily to infrastructure and general government. Appendix B.3–B.4 reports additional evidence on revenue–expenditure comovement, execution-stage asymmetry, and rainy-day-fund adjustment.

In summary, we find that the original budget is important for actual fiscal policy, and revenue forecasts affect spending plans across a wide range of line items. We next examine how these revenue forecasts are formed and translated into spending plans.

2 Framework

We start by studying how the original budget is formed: which information enters the budget, and when does that information arrive? To do so, we develop a forecast-incorporation test, to identify when in the budget cycle revenue news is embedded into the original budget. We call this period the "budget decision window". Because state spending plans are built on revenue forecasts, identifying when forecasts update also identifies when revenue news can influence fiscal policy.

2.1 The Budget Forecast as the Fiscal State Variable

The state chooses original-budget spending $G_{s,t}^{\text{budget}}$ by trading off expected fiscal balance against a baseline $\bar{G}_{s,t}$, which summarizes service needs, statutory obligations, and political commitments. Most states face balanced budget constraints at initial and final budgeting, so

estimated. Appendix Figure B.2 shows that this conclusion is robust to an alternative benchmark that uses two-year-lagged actual outcomes in the planning stage.

expected revenues based on the information set at the time of the enacted budget $R_{s,t}^{\text{budget}} \equiv \mathbb{E}\left[R_{s,t}^{\text{actual}} \mid \mathcal{A}_{s,t}^{\text{budget}}\right]$ enters into the state's problem as well.

$$G_{s,t}^{\text{budget}} = \arg\min_{G} \left\{ \tfrac{\psi}{2}\left(G - R_{s,t}^{\text{budget}}\right)^2 + \tfrac{\phi}{2}\left(G - \bar{G}_{s,t}\right)^2 \right\} \qquad \text{s.t.} \qquad G \leq R_{s,t}^{\text{budget}}. \tag{2}$$

The first term reflects the cost of fiscal imbalance, where spending less than expected revenues can be politically costly, and the second term is the cost of moving spending away from baseline commitments. The constraint is the balanced-budget requirement at enactment: in most states, the budget cannot commit spending beyond its revenue forecast (Poterba, 1994).

Solving the problem gives

$$G_{s,t}^{\text{budget}} = \min\left\{ \lambda\, R_{s,t}^{\text{budget}} + (1-\lambda)\,\bar{G}_{s,t},\ R_{s,t}^{\text{budget}} \right\}, \qquad \lambda \equiv \frac{\psi}{\psi + \phi} \in (0, 1]. \tag{3}$$

When the balanced-budget constraint is slack, spending is a convex combination of the revenue forecast and baseline commitments; when it binds, spending is capped by the forecast and forecast updates pass through one-for-one. Either way, news that moves expected revenues $R_{s,t}^{\text{budget}}$ can affect the spending budget.

2.2 Testing Forecast Incorporation

We establish a framework to test whether institutions incorporate new information into their expectations continuously or discretely based on their institutional calendars. Suppose we observe news $z_{s,t,m}$ arriving in month m that could positively affect state s's revenue in fiscal year t. We first test if the news affects actual revenues $R_{s,t}^{\text{actual}}$, and then test if the news is incorporated into the revenue forecast $R_{s,t}^{\text{budget}}$. Equivalently, we can test if the news predicts forecast errors, where $R_{s,t}^{\text{fe}} \equiv R_{s,t}^{\text{actual}} - R_{s,t}^{\text{budget}}$. All tests take the form:

$$R_{s,t}^{j} = \sum_{m} \beta_m^j z_{s,t,m} + u_{s,t}^{j}. \tag{4}$$

where $j \in \{\text{actual}, \text{budget}, \text{fe}\}$. Notice that for any month, any news that is informative but not fully incorporated into the forecast will predict the forecast error: $\beta_m^{\text{fe}} = \beta_m^{\text{actual}} - \beta_m^{\text{budget}}$.

The full-information benchmark. The benchmark is full incorporation of all information available before the original budget is finalized. In this case, the revenue forecast $R_{s,t}^{\text{budget}}$ reported alongside the original enacted budget would incorporate any informative news before enactment. Forecast errors should therefore be unpredictable with respect to such information (Coibion and Gorodnichenko, 2012, 2015):

$$\beta_m^{\text{budget}} = \beta_m^{\text{actual}}, \qquad \beta_m^{\text{fe}} = 0, \qquad \text{for all } m \text{ before finalization.}$$

Decision-window hypothesis. We hypothesize that the forecast incorporates news primarily while it is actively being constructed. Two frictions bound this period, one on each side.

Left boundary: limited processing of pre-decision information. Behavioral models of sparse processing and imperfect memory can lead decision makers to only recall recent information when forming expectations (Gabaix, 2014; Azeredo da Silveira et al., 2024). In our setting, officials may not fully reprocess older information when they sit down to build the forecast associated with the budget.

Right boundary: organizational commitment after the proposal. Organizations may only sporadically process new information and update decisions, potentially due to costly information acquisition (Mankiw and Reis, 2002; Reis, 2006), contract rigidities (Taylor, 1980), coordination and communication costs (Dessein and Santos, 2006), or nonconvex adjustment costs (Abel and Eberly, 1994; Caballero and Engel, 1999). In our setting, there is a dated point (the "proposal date") at which the proposed budget moves to the legislature for deliberation and bargaining.[17] Informative news arriving after this point may not be incorporated into revenue forecasts and spending budgets, even when it arrives before enactment.

Together, these frictions generate a discrete period of active updating, the decision window $\mathcal{W}_{s,t}^{\text{Decision}}$, and separate tests at its two boundaries. Pre-decision news tests whether previously available information is incorporated once forecast construction begins. Post-proposal news tests whether the forecast continues to update during legislative deliberation.

[17] The governor's proposal is the starting point for legislative deliberation in most states. Legislatures can change appropriations and, in some states, formally revise the forecast (National Association of State Budget Officers, 2021; Mikesell and Ross, 2014).

The decision-window hypothesis implies that, for informative news ($\beta_m^{\text{actual}} > 0$),

$$\begin{aligned} \beta_m^{\text{budget}} &= \beta_m^{\text{actual}}, \qquad \beta_m^{\text{fe}} = 0, \quad m \in \mathcal{W}_{s,t}^{\text{Decision}}, \\ \beta_m^{\text{budget}} &< \beta_m^{\text{actual}}, \qquad \beta_m^{\text{fe}} > 0, \quad m \text{ outside } \mathcal{W}_{s,t}^{\text{Decision}}. \end{aligned} \tag{5}$$

Alternative models. The decision-window hypothesis combines two frictions. Canonical single-friction models can account for one boundary, but generally not both. Sticky information (Mankiw and Reis, 2002) can match the right boundary if the proposal represents an update date. Because sticky information models imply updating is complete when it occurs, however, pre-decision news should be caught up: they predict $\beta_m^{\text{fe}} \simeq 0$ for months m before the decision window.

Models of imperfect memory or overreaction to recent news can generate attenuation of older information (Bordalo et al., 2018; Afrouzi et al., 2023; Azeredo da Silveira et al., 2024). Absent a separate commitment friction, they predict the strongest incorporation of the newest deliberation news; diagnostic overreaction can even imply $\beta_m^{\text{fe}} < 0$ for months m closest to enactment. Canonical rational-inattention models with time-invariant information costs (Sims, 2003; Maćkowiak and Wiederholt, 2009) tie incorporation to a signal's value and processing cost rather than its calendar position, and therefore do not by themselves generate a proposal-date discontinuity. Appendix Table C.1 summarizes these contrasts.

2.3 From Incorporation to Spending

State revenue forecasts matter because they transmit directly to fiscal policy via the optimal budget rule (3). Specifically, original-budget spending responds to month-m news only through the incorporated component,

$$\frac{\partial G_{s,t}^{\text{budget}}}{\partial z_{s,t,m}} = \underbrace{\frac{\partial G_{s,t}^{\text{budget}}}{\partial R_{s,t}^{\text{budget}}}}_{\text{passthrough}} \beta_m^{\text{budget}}, \tag{6}$$

where the passthrough equals λ when the balanced-budget constraint is slack, and equals one when it binds. State fiscal spending therefore should inherit the incorporation pattern of revenue forecasts. If comparable news has the same direct effect on the economy whenever it arrives, the on- versus off-window premium in any state-level outcome identifies the component that is mediated by state spending.

3 Identifying the Budget Decision Window

The framework delivers two testable predictions that jointly locate the decision window empirically: forecast incorporation should be concentrated in the budget decision window, and incorporated forecast updates should pass through to spending. To test these predictions, we exploit cross-state variation in proposal dates to compare similar news at different proposal-relative times.

3.1 Research Design

States propose their budgets at different times, so a common national shock lands at different points in different states' budget calendars. Consider New Jersey and Virginia: both begin their fiscal year in July, but Virginia proposes its budget in December, while New Jersey proposes in February. In January 2016, oil prices fell sharply and renewed depreciation of the renminbi pushed the S&P 500 down 5 percent in a month.[18] New Jersey was still drafting its budget, and revised its FY2017 revenue forecast down nearly 9 percent and cut planned spending by about 16 percent; Virginia, whose forecast was set before the shock, planned for revenue growth of 6 percent and raised planned spending by 6 percent.

Our core identification generalizes this example, exploiting variation in the arrival of state-specific revenue news relative to each state's budget calendar. Because proposal dates vary across states, news arriving in a given month falls inside the decision window for some states and outside it for others. We can then use this plausibly exogenous variation in the cross section of budget calendars to identify the differential effect of news arriving in versus out of the decision window. The key assumption is that this timing is as good as randomly assigned, conditional on fixed effects and controls. The main threat is endogenous proposal timing: if states adjust their budget calendars in response to shocks. We address this concern in Section 3.4.3 and find that proposal dates tend to be sticky.

[18] See Bloomberg News, "Equity Slump Set to Continue in Asia as Crude Lingers Around $34," January 6, 2016, https://www.bloomberg.com/news/articles/2016-01-06/equity-slump-set-to-continue-in-asia-as-crude-lingers-around-34.

3.2 Measuring State-Specific Revenue News

Implementing the empirical tests requires a monthly, state-specific shock that predicts revenues, which is not readily available for our full sample. Thus, we adapt a shift-share strategy built from state-level revenue shares and national shocks to construct state-month level shocks.

To build the shift-share shock, we begin with five key revenue sources k: personal income, general sales, charges and miscellaneous revenue, corporate income, and severance taxes. Together, they account for 62% of general revenue for states.[19] For each base, we identify a national shock $z_{k,m}$ in month m that moves revenue source k, compute a predetermined state exposure weight $\omega_{s,k,t}$, and estimate a national scaling $\hat{\eta}_k$:

$$\text{News}_{s,t,m} = \sum_{k=1}^{K} \hat{\eta}_k \, \omega_{s,k,t} \, z_{k,m}. \tag{7}$$

The shock $z_{k,m}$ is standardized and then residualized in an AR(4) of monthly log changes of the national series to remove the predictable component. Note that all cross-state variation in $\text{News}_{s,t,m}$ comes from the state-specific exposure weights. The weight is state s's revenue share in base k, averaged over five years ending two years before fiscal year t,

$$\omega_{s,k,t} = \frac{1}{5} \sum_{\tau=t-6}^{t-2} \frac{\text{Revenue}_{s,k,\tau}}{\text{General Revenue}_{s,\tau}}, \tag{8}$$

so it is predetermined relative to the FY-t budget. For each revenue category k, we then estimate a scaling variable $\hat{\eta}_k$ that represents the sensitivity of national revenue base k on the corresponding shock:

$$\Delta \log \text{Rev}^{US}_{k,\tau} = \eta_k \, \text{Shock}^{US}_{k,\tau} + u_{k,\tau}. \tag{9}$$

The $\text{News}_{s,t,m}$ then represents the shock in predicted-revenue units. Construction details are in Appendix D.

Two properties make the measure suitable for our design. First, it is predetermined: we

[19] The baseline national source series are national personal income for personal income taxes, personal consumption expenditures for sales taxes, PCE services for charges and miscellaneous revenues, industrial production for corporate income taxes, and crude petroleum prices for severance taxes; Appendix Table D.1 lists the exact series mnemonics. Intergovernmental transfers (about 32% of general revenue) are excluded because they are stable and driven largely by federal policy rather than state tax-base exposure.

construct it as a five-year trailing revenue share ending in $t-2$. Also, neither decision-window nor pre-decision news predicts changes in state top marginal income-tax rates (Appendix Table E.5), which mitigates the concern that the forecast response is driven by contemporaneous changes in personal income-tax rates. Second, it is relevant: we show in Appendix Table E.1 that fiscal-year news predicts realized revenue growth, though this relationship weakens when adding fiscal-year fixed effects, since this absorbs the variation coming from the common national component.

Note that although we construct our shock as a shift-share object, we do not rely strictly on the typical shift-share identifying assumptions. Our main identification comes from the variation in budget calendar. The identifying assumption is then that any non-budget relationship between the shock measure and the outcome does not change discretely at the decision-window boundary (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022).

3.3 Locating the Decision Window

We first identify when news is incorporated into the original revenue forecast. To do this, we build a state–fiscal-year–calendar-month panel: for each state–fiscal-year, we stack the monthly news observations over a 30-month event window, from July of calendar year $t-3$ to December of $t-1$, repeating the annual outcome across those months. We anchor event time at the budget proposal date, because this is the key source of cross-state variation. We define $\ell_{s,t,m}$ as the number of months between calendar month m and state s's FY-t proposal, so $\ell < 0$ precedes the proposal and $\ell \geq 0$ follows it. Following the framework above, we regress original-budget revenue forecast growth, $y_{s,t} = \log(\text{Orig}^R_{s,t}/\text{Orig}^R_{s,t-1})$, on news arriving in the months relative to the budget proposal:

$$y_{s,t} = \sum_h \rho^y_h \left(\text{News}_{s,t,m} \times \mathbf{1}\{\ell_{s,t,m} = h\}\right) + \mathbf{X}'_{s,t}\Pi^y + \alpha_s + \delta_{t,m} + \varepsilon^y_{s,t,m}. \tag{10}$$

The coefficients of interest ρ^y_h are the loadings on news arriving h months from the proposal. State fixed effects α_s absorb persistent cross-state differences in forecasted revenue growth; fiscal-year-by-calendar-month-year fixed effects $\delta_{t,m}$ absorb the common national component of each innovation within a budget round. Our identification then comes from cross-state differences in exposure to the shock and in where that month falls relative to each state's proposal. The controls $\mathbf{X}_{s,t}$ include four lagged quarterly changes of macroeconomic variables (CFNAI, payroll employment, the S&P 500, the five-year Treasury yield, and CPI),

the most recent of which ending four quarters before the proposal, the Borusyak et al. (2022) aggregate-share control, and lagged state rainy-day-fund balances and GDP growth. Standard errors are two-way clustered by state and calendar month-year.

A positive estimate ρ_h^y indicates that news arriving h months from the proposal is at least somewhat incorporated into the original-budget revenue forecast. Figure 4 plots the month-specific coefficients from Equation (10). Visually, the timing profile identifies $\ell \in [-5, -1]$ as the candidate budget decision window. Expected revenue growth is affected by news primarily in the five months before the proposal date; coefficients outside this interval are mostly statistically indistinguishable from zero.

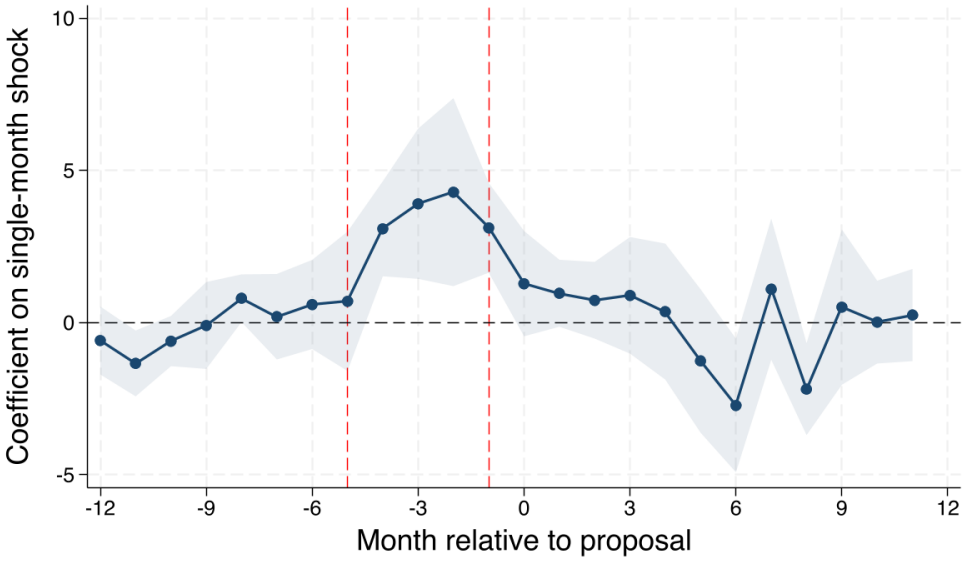


Figure 4: **Monthly Profile of Revenue Forecast Updating Around the Proposal Date.** The figure plots coefficients on the state-specific shift-share news measure interacted with single-month proposal-relative indicators. The dependent variable is one-year original-budget revenue forecast growth. The specification includes state fixed effects, fiscal-year-by-calendar-month fixed effects, predetermined controls, and the aggregate-share control. Shaded areas denote 95 percent confidence intervals. The dashed lines mark the budget decision window used in the window-level specifications.

As a robustness, we repeat the exercise anchoring at the enactment date instead of the proposal date (Appendix Figure E.1). We observe a similar pattern in the data: there is a distinct 5 month window that ends several months prior to budget enactment in which news is more so incorporated. Outside of this window, the loading of each month's news on the revenue forecast is indistinguishable from zero.

For the rest of our analysis, we accordingly partition each budget round's pre-fiscal-

year months into three windows: "pre-decision" ($\ell \in [-11, -6]$), the "decision window" ($\ell \in [-5, -1]$), and "deliberation" (the months after the proposal, before the fiscal year begins). Table 1, Panel B, reports summary statistics for the monthly news measure within each window.[20]

3.4 Forecast Incorporation and the Decision Window

Having posited a candidate decision window, we next test the two predictions in Section 2. Does off-window news affect revenue forecasts, or does it predict forecast errors? We restrict the sample to the three proposal-relative windows defined in Section 3.3. We then run the following regression, including interactions to allow for differential responses across windows:

$$y_{s,t} = \sum_{W \in \{\text{Pre,Decision,Delib}\}} \beta_W^y \left(\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^W\}\right) + \mathbf{X}_{s,t}'\Pi^y + \alpha_s + \delta_{t,m} + \varepsilon_{s,t,m}^y, \quad (11)$$

where $\mathcal{W}_{s,t}^{\text{Pre}}$, $\mathcal{W}_{s,t}^{\text{Decision}}$, and $\mathcal{W}_{s,t}^{\text{Delib}}$ denote the pre-decision, decision, and deliberation windows. We estimate (11) separately for realized revenue, the original-budget revenue forecast, and the forecast error, so the coefficients map directly into $\beta_{\mathcal{W}}^{\text{actual}}$, $\beta_{\mathcal{W}}^{\text{budget}}$, and $\beta_{\mathcal{W}}^{\text{fe}}$ in Equation (5) of the framework.

We next directly test if the incorporation of decision window news is significantly different from off-decision window news. To do this, we run a similar specification but interact the news with one dummy variable for on-window news. The comparison is thus between in- versus off- decision window news, pooling together the pre- and post- decision window news:

$$y_{s,t} = \gamma^y \text{News}_{s,t,m} + \theta^y \left(\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}\right) + \mathbf{X}_{s,t}'\Pi^y + \alpha_s + \delta_{t,m} + \varepsilon_{s,t,m}^y. \quad (12)$$

Because we restrict the estimation sample to end before the fiscal year begins, γ^y is the pooled off-window response and θ^y is what we call the decision window "premium.": the effect of news arriving on-window versus off-window. When the outcome variable is the revenue forecast, Equation (5) of the framework implies $\theta^{\text{budget}} > 0$: news is incorporated into forecasts more strongly when it arrives during the decision window than off. When

[20] Decision-window news has a smaller standard deviation than the other windows in Panel B. The gap reflects COVID, which fell primarily during pre-decision and deliberation windows. Appendix Table D.2 reproduces the panel excluding calendar years 2020–2021; the standard deviations are then similar across windows (0.23–0.26, ×100 scale).

the outcome variable is the forecast error, $\theta^{\text{fe}} = -\beta^{\text{fe}}_{\text{off}} < 0$: revenue-relevant news predicts forecast errors only when it arrives off-window.

The formal counterpart of the timing assumption in Section 3.1 is the moment condition

$$\mathbb{E}\left[\left(\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}^{\text{Decision}}_{s,t}\}\right) \varepsilon^{y}_{s,t,m} \mid \mathbf{X}_{s,t}, \alpha_s, \delta_{t,m}\right] = 0.$$

3.4.1 Main Results

Table 2 reports the results. Panel A reports the separate-window loadings for the original-budget revenue forecast, the forecast error, and realized revenue. All columns share the same sample, so the realized-revenue loadings in Column (3) equal the sum of the forecast and forecast-error loadings, mapping to the framework equation $\beta^{\text{fe}}_m = \beta^{\text{actual}}_m - \beta^{\text{budget}}_m$. Column (1) shows that decision-window news moves the original-budget revenue forecast strongly, with $\beta^{\text{budget}}_{\text{Decision}} = 2.74$, while pre-decision and deliberation news do not: their coefficients are -0.10 and 0.39, respectively, and statistically insignificant.

Column (2) shows the results for forecast errors, which mirror the forecast incorporation tests. News arriving in the pre-decision and deliberation windows predict forecast errors, while decision-window news does not. Note that the deliberation window is after the proposal, so news that arrives then could in principle affect the original enacted budget. Its forecast-error loading shows that deliberation news is not fully incorporated. Pre-decision news is already available before the proposal date, yet it also remains outside the revenue forecast and reappears in forecast errors. These two off-window results confirm the framework's prediction that off-window news is not incorporated into forecasts and predicts forecast errors.

Finally, Column (3) shows how news in each window influences actual realized revenue. News that arrives in all three windows significantly predicts realized revenue, but the decision-window coefficient is largest (though the difference is statistically insignificant). The larger decision-window coefficient is consistent with a potential multiplier effect of spending plans: news written into the budget can also move the tax base through the resulting fiscal choices.[21]

[21] Appendix C formalizes this point. If realized revenue exhibits a feedback component, realized-revenue loadings combine fundamental revenue news with the additional revenue from the endogenous spending response induced by the budget. The forecast-error decomposition is still informative in this case, because (under certain conditions) the feedback term cancels out in $R^{\text{actual}} - R^{\text{budget}}$.

Panel B reports the direct contrast between the in- and out-of-window effects, what we call the decision window "premium." The forecast premium is large (2.53) and significantly positive, consistent with the framework prediction that decision window news is incorporated into forecasts by far more than off-decision window news. The forecast error premium is negative (-1.22), and has a one-sided p-value of 0.066, indicating statistical significance at the 10% level: the interpretation is that decision window news predicts forecast errors significantly less than off-window news.

Taken together, the evidence identifies the five months before the proposal as the budget decision window: the period in which revenue news is written into the original budget. Deliberation window news, which arrives after the proposal date, is relevant for revenues but is not incorporated into the revenue forecasts of enacted budgets.

3.4.2 Alternative Stories

The pattern is difficult to reconcile with a single friction of expectations formation. Under sticky-information updating, it is costly to update a forecast, so forecasts are updated sporadically; however, updates would fully incorporate past information. Yet we show that pre-decision news does not enter the forecast and instead predicts forecast errors. Under recency or overreaction, the most recent information, what we call "deliberation news", should receive the greatest weight; instead, it is not fully incorporated and also predicts forecast errors. We also test whether there is evidence of gradual updating within the decision window, and do not find strong evidence for it: splitting the decision window into early months $\ell \in [-5, -3]$ and late months $\ell \in [-2, -1]$ yields forecast loadings of 2.22 and 3.13, which are statistically indistinguishable (Appendix Table E.3).

Finally, a salience-based attention model would predict that larger shocks are more likely to enter the forecast and thus less likely to predict forecast errors. We first check whether news arriving during decision windows is larger on average. Excluding the pandemic innovations of 2020–21, dispersion is comparable across windows, with standard deviations of 0.23, 0.26, and 0.25 for the pre-decision, decision, and deliberation windows (Appendix Table D.2), so differences in shock size are unlikely to explain the average decision-window premium. Salience models would further predict that large off-window shocks should be more likely to enter the forecast. They are not: interacting pre-decision news with its absolute magnitude yields a small and insignificant coefficient (Appendix Table E.4). However, the forecast-error regression shows that larger decision-window news may be incorporated more

fully. Thus, while the size of off-window shocks does not seem to affect incorporation, salience does seem to be at play within the decision window.

3.4.3 Assessing the Identifying Assumptions

Proposal dates do not change In our empirical strategy, we exploit cross-state variation in proposal dates, which we argue do not correlate with unobserved state fundamentals. One helpful feature of the budget calendar is that proposal dates are generally set by constitutions and statutes that change infrequently. [22] Our key identifying assumption is that states do not adjust their proposal date in response to any of our national shocks, such as oil-price, income, or sales shocks.

We run three tests to show that our identification using proposal dates is robust. First, proposal months are highly persistent: forty jurisdictions never change their reported proposal month across the NASBO waves covering our sample period. Moreover, state fixed effects absorb 93 percent of panel variation in proposal months (Appendix Table A.2). Second, we show that our results hold if we use every state's calendar as of the 2021 NASBO survey (Appendix Table E.2). Third, we run 500 placebo tests where in every test, we relocate each state's decision window to random positions away from the proposal. Using these placebo windows, we estimate a premium centered around zero (Appendix Figure E.2), whereas the estimate on the actual data is significantly positive.

Identification using biennial budgets. Twenty states enact a single two-year budget, but most of these have an annual legislative cycle and thus revise the budget annually through supplemental appropriations. For example, Connecticut and Hawaii consider their budget cycles effectively annual in practice, despite officially having biennial budgeting (National Association of State Budget Officers, 2021). In fact, only four states—Montana, Nevada, North Dakota, and Texas—have both a biennial budget and a legislature that convenes only every other year, so that no official annual budget decision occurs in the second year of the biennium.

We test how shocks during the months that would constitute a decision window but land in the second year of a biennium affect forecasts and budgets. To do this, we define a dummy that equals 1 if that state-fiscal-year is in the second year of a biennium, indicating there is a

[22] Andersen et al. (2012) show that budget enactment can be delayed by economic conditions; to avoid this potential endogeneity, our design focuses on proposal dates.

less formal process for revising the budget. Table E.6 reports the results, comparing them to the baseline reported again in columns (1)-(2). For state-fiscal-year cohorts with an actual decision in the assigned decision window, the forecast loading is 2.83. The second-year interaction is −2.11 and significant at the 5 percent level, implying a loading on second-year news that is close to zero. The forecast-error specification has the opposite directional pattern: second-year decision-window news has an implied loading of 2.80 in the forecast error. One way to interpret this result is that in the usual decision window during the second year of the biennium, when there is less formal (or no) budgeting process, informative news about actual revenues does not enter the forecast.

We further test how our results change when we drop second-year biennium cohorts directly in columns (5)-(6): our results are qualitatively the same. Finally, if we relabel second year biennium cohorts as post-decision window (deliberation) news; the results are again similar, in columns (7)-(8). We interpret these results as consistent with our overall thesis of the importance of decision-window news.

Shift-share composition We report the Rotemberg decomposition in Appendix Table E.7, which shows that two revenue bases drive much of the variation: severance news accounts for 0.63 of the weight and personal-income-tax (PIT) news accounts for 0.25. We run multiple tests on the reliance of our results on these two revenue sources in the Appendix. In summary, we show that we get qualitatively similar results if we remove the severance component of the shock and the most influential year (FY2010, the financial crisis), or jointly use both severance and personal income tax, use personal income tax only, excluding the severance-dependent states and the FY2010 (Appendix Table E.9). We also run our tests using general macroeconomic indicators like consumer sentiment and CFNAI, and produce directionally similar (albeit less precise) results, reported in (Appendix Table E.10).

3.4.4 Robustness

Inference and specification. We report robustness on our baseline specification in Appendix E.6. We re-estimate our baseline results in Table E.11, replacing our two-way clustering with a wild cluster bootstrap by calendar month-year and an exposure-vector permutation. Our decision-window forecast results remain statistically significant at the 1% level, while the decision window forecast-error coefficient is statistically indistinguishable from zero. Our results also hold with less stringent fixed effects and controls, as shown in

Table E.12. Leave-one-out analyses in Figures E.3 and E.4 show that the key results are not driven by any single state or year.

Historical coverage and fund scope. Our main sample begins in 2002 and focuses on General Fund revenues, because they are the most standard reporting across states. However, in Appendix Table E.13, we show our results are not sensitive to the time horizon or the scope of revenues. If we begin each state's time series to the earliest available General Fund disclosure since 1995, we estimate a decision-window forecast coefficient of 2.65 (standard error 0.66). If we add persistent eligible education, health, and transportation operating funds over FY2002–202, our results again are similar: 2.72 (standard error 0.61).

Seasonality and window boundaries. One potential concern is that our estimate reflects seasonality in the news. To rule this out, we run our baseline test and add interacted share by calendar-month controls, both using time-varying shares or fixed 1995 initial value shares. We report the results in Appendix Table E.14, and show the decision-window effect of 2.83–2.84 remains. Our results also hold if we alter the decision-window definition by one month on either end, with estimates ranging 2.02 to 3.18 (Appendix Table E.15).

3.5 Updated Revenue Forecasts Affect Spending Plans

Next, we test whether forecast updates induced by decision-window news translate into spending decisions. Equation (6) implies a pass-through from the original-budget revenue forecast to spending. We estimate this pass-through by instrumenting the revenue forecast update with news from each proposal-relative window. Table 3 reports the results.

Pre- and post-decision window news do not move revenue forecasts, so there is no valid first stage in columns (1) and (3). The decision-window news is a valid instrument for revenue expectations, with a Kleibergen–Paap F-statistic of 20.7. Using the decision-window instrument, a 1 percent news-induced increase in the original-budget revenue forecast raises the original spending plan by 0.36 percent and realized spending by 0.52 percent. Table 3 also reports weak-instrument-robust Anderson–Rubin confidence sets, both of which exclude zero for the decision-window instrument.

Through the lens of our framework, the planned-spending coefficient represents the pass-through from revenue forecasts to the spending budget. Our estimate is well below

one, indicating that forecast updates are only partially translated into planned spending. This potentially reflects reserves or spending rigidities that allow some flexibility for planned spending to not move one-for-one with revenues. The realized spending coefficient is larger, 0.52, showing that forecast updates also predict actual spending. These results indicate that the news that is incorporated into revenue expectations is consequential for spending.

4 Real Effects: Budget Calendar Gates Transmission

The decision window governs which revenue news enters the budget; this section asks whether it also governs which news reaches the real economy. We find that it does. Using the same on- versus off-window comparison of similar shocks, together with state-industry private employment data, we show that revenue news arriving inside the decision window moves employment at firms in government-exposed industries, and does so persistently, while revenue-relevant off-window news has at most small, transient effects. A given shock that arrives during the state's decision window affects private sector employment by roughly four to five times as much as the same shock arriving outside the decision window. We then show that the budget calendar shapes how national monetary policy transmits to state fiscal policy.

4.1 Employment Effects of the Decision Window

We test the real effects of decision windows in three steps: first, is there a differential response to revenue news in industries exposed to state and local government demand? Second, does this differential response depend on the budget cycle? Finally, does the same pattern appear in aggregate state employment and wages?

4.1.1 Effects Concentrated in Government-Exposed Industries

We first test differential private sector responses to revenue news using QCEW state–industry employment and wage data and measure exposure to government demand using BEA input–output accounts. The intuition is that firms in industries that sell relatively more to state and local government demand are more likely to be affected by changes in the state's operating budget via procurement.

The design is a triple difference: we compare government-exposed and less-exposed industries within the same state-year, and ask whether that exposure difference is larger for revenue news arriving on- versus off-window. To measure industry-level exposure to state and local government demand, we compute the state-and-local government purchase share from national input–output data for each two-digit NAICS industry and fiscal year. To keep the exposure variation predetermined with respect to the budget cycle, the measure is a five-year trailing average lagged two years. For robustness, we test two alternative exposure measures: a high-exposure indicator based on the median average state–industry exposure, and a standardized continuous exposure measure.

Let $Y_{s,j,t}$ denote private employment or wages in state s, industry j, and fiscal year t, facing revenue news in each month m leading up to the fiscal year t. The full specification is

$$\begin{aligned}\Delta \log Y_{s,j,t} = \kappa \left(\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\} \times \text{GovShare}_{j,t}\right) \\ + \alpha_{s,t} + \alpha_{j,t} + \alpha_{s,j} + \delta_{t,m} + \mathbf{Z}'_{s,j,t,m}\Lambda + \varepsilon_{s,j,t,m},\end{aligned} \tag{13}$$

where $\mathbf{Z}_{s,j,t,m}$ includes the relevant lower-order interactions. State-by-fiscal-year fixed effects, $\alpha_{s,t}$, absorb all state-year shocks, including the level effect of cumulated revenue news, contemporaneous state policies, and the state-level controls used in specification (12). Industry-by-fiscal-year fixed effects, $\alpha_{j,t}$, absorb national industry cycles, and state-by-industry fixed effects, $\alpha_{s,j}$, absorb persistent local industry composition. Fiscal-year-by-calendar-month fixed effects, $\delta_{t,m}$, absorb common monthly shocks within each fiscal-year cohort as in our previous analysis of state-level revenue news. Identification therefore comes from whether government-exposed industries respond differentially to decision-window news within the same state-year.

We interpret κ as representing real effects mediated via state spending. To do so, we assume that comparable news has the same direct effect on the economy whenever it arrives: any non-fiscal response of exposed industries to revenue news is then common to on- and off-window arrivals and is differenced out, so the premium isolates the component transmitted through the enacted budget. The dynamic estimates below support this condition directly: exposed industries show no differential response when the news itself arrives, only once the budget it entered takes effect (Figure 5).

Table 4 supports the procurement interpretation across exposure definitions and fixed-effect specifications. The news measure has no natural units of its own, so we express the magnitudes in budget units using the paper's fiscal first stage. The news variable raises the

enacted revenue forecast with coefficient 2.74 (Table 2), so dividing the real-effect coefficients by this pass-through estimate converts them into effects per one-percent news-induced revenue forecast revision. In the preferred full fixed-effect specification of Panel A, Columns (2) and (4), decision-window news that raises the state's revenue forecast by 1 percent raises private employment in high-government-exposure industries by 0.10 percentage points (0.27/2.74) and private wages by 0.14 percentage points relative to less-exposed industries in the same state-year. Using the continuous exposure measure in Panel B, the same news raises employment in industries with a one-standard-deviation higher government-purchase share by 0.06 percentage points and wages by 0.06 percentage points.

The response does not appear to operate through public payroll. We detect no significant effect of decision-window news on public-sector employment or wages (Appendix Table F.1). One potential explanation is that public sector employment is a harder margin to adjust and is thus more insulated from spending shocks (Munnell and Cannon Fraenkel, 2013). Combined with the concentration of private employment effects in government-exposed industries, this evidence is consistent with transmission through procurement and other non-payroll spending margins.

The stringent fixed-effects specification rules out many potential alternative explanations. For example, state-year fixed effects imply that the pattern cannot be explained by government-exposed industries being located in states with better economic conditions. The result is also not driven by the 2008–2009 financial crisis: Appendix Table F.2 reports similar magnitudes excluding FY2010, whose planning period for many states overlaps or closely follows the September 2008 fall of Lehman Brothers. A leave-one-state-out exercise shows that no single state drives the estimate (Appendix Figure F.1). Finally, randomization inference that permutes government exposure across two-digit industries places the observed employment coefficient near the upper tail of the placebo distribution ($p = 0.014$; Appendix Table F.3).

4.1.2 Persistent Employment Effects

Next, we test when the differential private employment response materializes, and how long it persists. We estimate quarter-by-quarter versions of specification (13), replacing the dependent variable with cumulative employment growth from the pre-fiscal-year base to quarterly horizon h, $\Delta_h \log Y_{s,j,t} = \log Y_{s,j,t+h} - \log Y_{s,j,t-1}$, using the high-government-exposure indicator and the same fixed effects. Figure 5 plots the differential employment response of high-

relative to low-exposure industries, separately for off-window news and for decision-window news; the vertical distance between the two series is the decision-window premium.

The dynamics show how the budget calendar translates revenue news shocks to the real economy. Prior to the fiscal year, there is no differential response in government-exposed industries: we cannot reject parallel pre-trends. Once the fiscal year begins, and the spending from the enacted budget begins, the employment in government-exposed industries responds positively to on-window news, while the response to revenue-relevant off-window news is small throughout. Because decision-window news has already arrived before the fiscal year starts, a purely informational or anticipatory response would appear when the news arrives. Instead, the response tracks budget execution, consistent with the procurement channel rather than a contemporaneous reaction to the news. Appendix Figure F.2 shows similar dynamics using the continuous exposure measure.

Importantly, the employment response to decision-window news persists for several years, whereas the response to off-window news remains small and transitory (Figure 5). One possible explanation is that state officials permanently disregard information arriving outside the budget decision window, so that it never enters subsequent spending decisions. Appendix Table F.4 does not support this interpretation. Pre-decision and deliberation news continue to predict realized revenue at later horizons. Off-window news, although do not affect spending this fiscal year, also eventually appears in realized spending. Thus, off-window information is not permanently excluded from fiscal policy; rather, it is incorporated into spending with a delay.

Moreover, we show that the private employment response to decision-window news is asymmetric and concentrated in negative news. Running the same specification as in Figure 5, we plot the "decision-window premium"—the differential response of high- versus low-exposure industries to on- versus off-decision-window news—separately for positive and negative news (Figure 6). The persistent response comes almost entirely from negative decision-window news, while positive news generates a smaller and less persistent effect. A natural follow-up question is if state government spending similarly responds asymmetrically. In Appendix Table F.5, we test whether state budgets respond differentially to positive versus negative decision-window news. Our results reject symmetry for planned and realized spending at the 10 percent level, but in the opposite direction: state government spending responds more to positive news. The concentration of employment losses in negative news is therefore not inherited mechanically from budget-stage asymmetry.

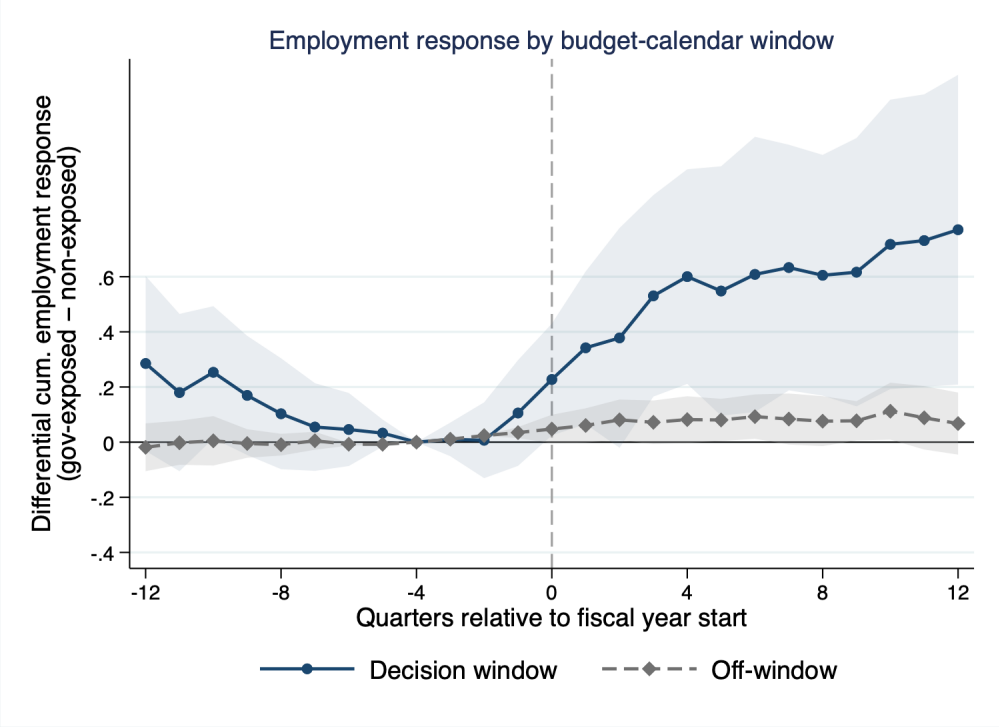


Figure 5: **Dynamic Employment Response by Budget-Calendar Window.** The figure plots horizon-by-horizon local projections where the dependent variable is cumulative private employment growth from the pre-fiscal-year base to horizon h, $\Delta_h \log Y_{s,j,t} = \log Y_{s,j,t+h} - \log Y_{s,j,t-1}$. The off-window series is the coefficient on $\text{News}_{s,t,m} \times \text{GovShare}^{high}$, measuring the differential response of high- versus low-exposure industries to off-window news. The decision-window series adds the triple interaction $\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\} \times \text{GovShare}^{high}$, so the vertical distance between the two series is the decision-window premium. Government exposure is a predetermined high-exposure indicator based on the state-and-local-government purchase share. Regressions include state-by-industry, industry-by-fiscal-year, and state-by-fiscal-year fixed effects. Shaded areas are 90 percent confidence intervals based on clustered standard errors.

We interpret these results as evidence of firm-level adjustment frictions. Decision-window news shifts government demand within the fiscal year, giving exposed firms little notice, whereas off-window news reaches spending through later budgets that firms can anticipate and respond to in advance (Leeper et al., 2013). Because the budget-side asymmetry favors positive news, while the employment asymmetry is concentrated in negative news, the asymmetric employment response supports a firm-level friction, consistent with credit-constrained government vendors who cannot bridge abrupt demand shortfalls, leading to layoffs that outlast the shock itself (Barrot and Nanda, 2020). The budget calendar thus converts similar news into different real outcomes, by changing how much time firms have to adjust.

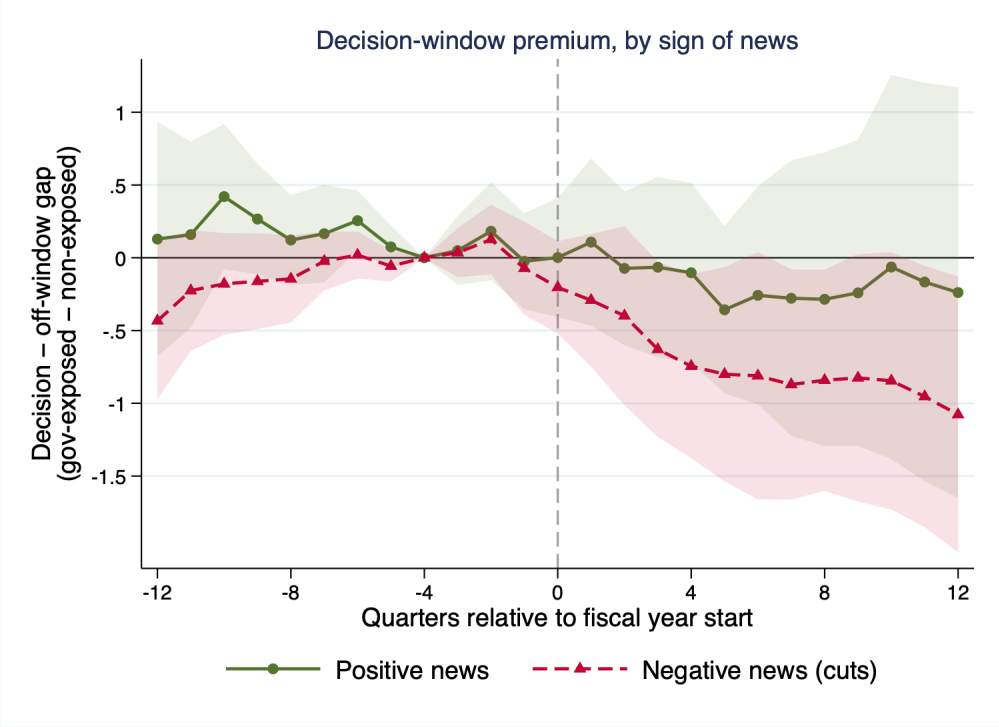


Figure 6: **Asymmetric Dynamic Employment Response to Decision-Window News.** The figure plots horizon-by-horizon local projections where the dependent variable is cumulative private employment growth from the pre-fiscal-year base to horizon h, $\Delta_h \log Y_{s,j,t} = \log Y_{s,j,t+h} - \log Y_{s,j,t-1}$. The plotted coefficients are decision-window premia: the differential response of high- versus low-government-exposure industries to decision-window news relative to off-window news. The positive series uses positive decision-window news; the negative series uses the absolute value of negative decision-window news. Government exposure is a predetermined high-exposure indicator based on the state-and-local-government purchase share. Regressions include state-by-industry, industry-by-fiscal-year, and state-by-fiscal-year fixed effects. Shaded areas are 90 percent confidence intervals based on clustered standard errors.

4.1.3 Aggregate State Responses

Next, we test if the decision-window effect propagates to aggregate state outcomes. We estimate equation (12) with $Y_{s,t}$ equal to state-wide private employment or private wages growth. The specification uses the same news measure, fixed effects, controls, and restricted pre-fiscal-year sample as the budget regressions.

Table 5 shows that the decision-window premium is positive and precisely estimated. The interaction coefficient on news times the decision window indicator is $\theta^Y = 0.40$ for private employment and $\theta^Y = 0.57$ for private wages. The pooled off-window response is much smaller, 0.10 for employment and 0.18 for wages. These estimates show that when the

budget channel is active, state-level private employment and wages also respond by more: roughly four to five times the off-window response. Appendix Table F.6 shows that the aggregate premia are slightly larger when FY2010 is excluded.[23]

In summary, we find that fiscal decision-windows not only determine what shocks enter state spending, but also how shocks transmit to the local real economy. We interpret this differential aggregate response to on- versus off-decision-window news as arising from a combination of state spending responses, the procurement channel effect, and the downstream fiscal multiplier effects. Thus, the decision window can help identify the fiscal multiplier for routine budget adjustment, which we explore next.

Rule-induced fiscal multiplier. We exploit the decision-window effect to construct a rule-induced fiscal elasticity. This differs from the standard fiscal-multiplier exercise. The usual approach exploits spending shocks that are orthogonal to local fundamentals; here, the variation comes from the systematic budget rule that arises from the government's optimization problem discussed in Section 2.

Revenue news moves spending when it is processed through the decision window, and the on- versus off-window contrast isolates the part of the real response generated by that routine fiscal adjustment. The identifying assumption is that comparable news arriving on- and off-window have similar direct effects on the private sector (see details in Appendix G). We estimate the multiplier as a Wald ratio: the reduced-form decision-window premium in the outcome divided by the first-stage decision-window premium in realized spending. The first stage, estimated directly in Table G.1 (Panel B, column 2), is 1.36: a decision-window news realization raises realized spending by 1.36 percent more than the same news off-window.[24] Dividing the aggregate real-effects decision-window premium in Table 5 by this first stage gives

$$\widehat{\mu}^{f}_{\text{Emp}} = \frac{0.40}{1.36} = 0.30, \qquad \widehat{\mu}^{f}_{\text{Wages}} = \frac{0.57}{1.36} = 0.42.$$

Thus a 1 percent increase in realized spending induced by routine budget adjustment is associated with a 0.30 percent increase in private employment and a 0.42 percent increase

[23] The 2008 financial crisis began during the planning period for many states; those states that were more negatively impacted were also more likely to receive federal stimulus, which counters our effects. Thus, excluding FY2010 sharpens our results.

[24] This magnitude is close to the first stage implied by the forecast channel in Equation (6): the on- versus off-window revenue-forecast premium of 2.53 (Table 2), times the pass-through of 0.52 from a news-induced forecast revision to realized spending (Table 3, Column 2), gives $2.53 \times 0.52 = 1.32$.

in private wages.

To put the employment estimate in familiar units, we benchmark to fiscal year 2023, the most recent year with near-complete coverage: realized state operating spending (total spending) averaged roughly $33 billion ($64 billion), and private employment 2.7 million workers per state, implying $41,000–$79,000 of induced state spending per private job-year.[25] The implied cost per job-year is the same order of magnitude as estimates from the cross-region fiscal-multiplier literature (Nakamura and Steinsson, 2014; Auerbach and Gorodnichenko, 2012; Chodorow-Reich, 2019), and comparable to the ARRA-based estimates in Chodorow-Reich et al. (2012). The comparison is only a scaling benchmark: unlike shock-based designs, this estimate uses routine forecast-mediated budget adjustment induced by the decision window.

4.2 Monetary Policy Transmission via Decision Windows

Next, we test if the decision-window effect generalizes to other types of shocks by analyzing the passthrough of monetary policy to state fiscal policy. Monetary policy shocks are common across states and can affect fiscal choices through macro expectations and financing conditions. We find that in both the state-level panel and aggregate time series, tightening surprises have larger and more persistent fiscal spending effects when they arrive during the decision window.

We adapt the monthly event-time design of Section 3, replacing state-specific revenue news with a national monetary policy surprise. We compute monthly sums of shocks from four standard measures of monetary policy surprises, all constructed by Bauer and Swanson (2023): the first principal component of the high-frequency changes in one to four quarter ahead Eurodollar (SOFR in 2023) futures in the 30-minute windows around FOMC announcements, both raw and residualized by lagged macro variables, and the analogous raw high-frequency changes in 2- and 5-year U.S. Treasury yield futures. For all policy surprise measures, the identifying assumption is that asset price movements inside the tight window

[25] Using general-fund operating spending only, the conversion is $33 billion/$(0.30 \times 2.7$ million$) \simeq$ $41,000. Evaluated instead at the means of the FY2002–2024 ACFR estimation sample (roughly $20.5 billion and 2.35 million private workers per state-year), the same conversion gives about $29,000; we prefer the 2023 benchmark because sample means average dollars across more than two decades of price levels. If we assume that the revenue shocks similarly affect all-funds total state expenditure, then the estimates imply an upper bound near $79,000 per private job-year, since $64 billion/$(0.30 \times 2.7$ million$) \simeq$ $79,000. This uses total spending averaging across the states in our sample in 2023 from the Census Annual Survey of State Government Finances.

around FOMC announcements are responding to policy news, and are thus orthogonal to state-level economic fundamentals (Nakamura and Steinsson, 2018; Jarociński and Karadi, 2020; Miranda-Agrippino and Ricco, 2021; Bauer and Swanson, 2023).

We adopt the same identification as in specification (12), with the national monetary-policy surprise MPS_m in place of revenue news. Because the surprise is common to all states in a calendar month, the time fixed effects $\delta_{t,m}$ absorb its level, and identification comes only from its interaction with whether month m lies in state s's decision window for fiscal year t.

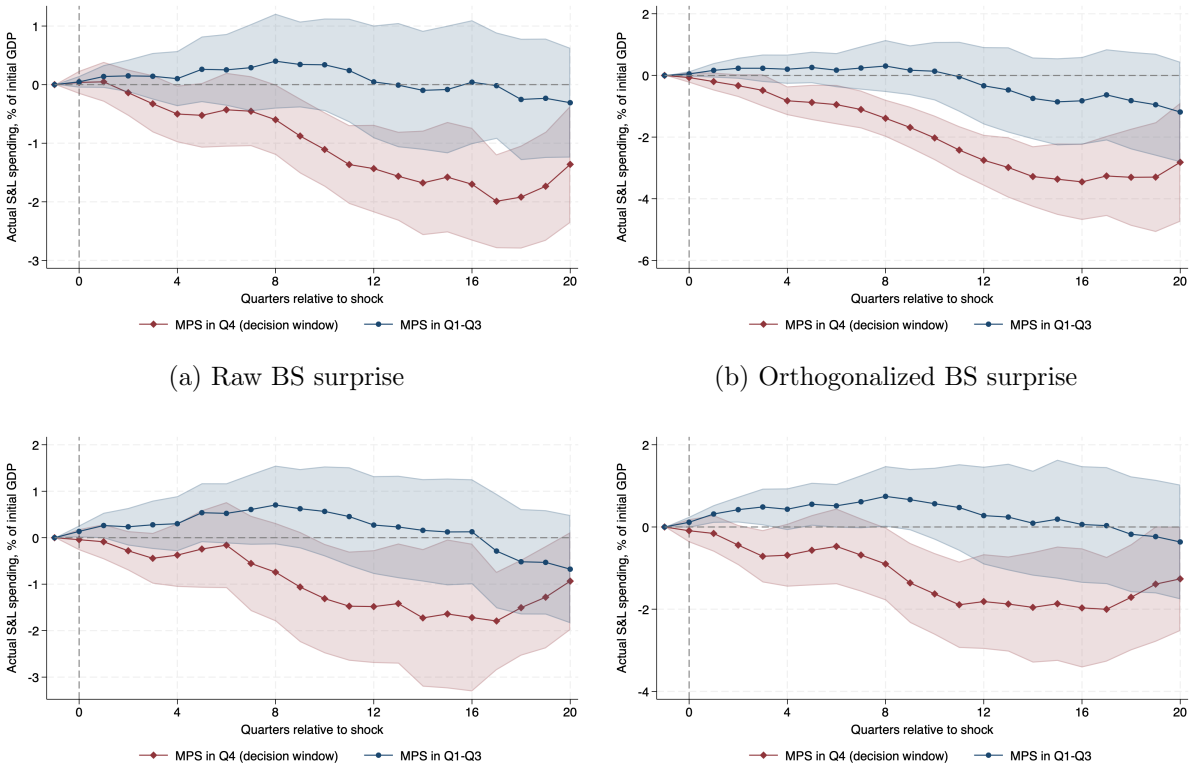
$$y_{s,t} = \theta^{y}_{\text{MPS}} \left(\text{MPS}_m \times \mathbf{1}\{m \in \mathcal{W}^{\text{Decision}}_{s,t}\} \right) + \mathbf{X}'_{s,t}\beta + \alpha_s + \delta_{t,m} + \varepsilon_{s,m,t}, \tag{14}$$

where MPS_m is in percentage-point units. As before, the sample stacks each state-year's pre-decision, decision, and deliberation months, and the outcome $y_{s,t}$ is the spending budget or realized spending relative to the prior-year original budget. The coefficient θ^{y}_{MPS} is the in-window-minus-off-window response to a tightening surprise. While monetary policy surprises may be predictable or correlated with the business cycle (Bauer and Swanson, 2023; de Fraisse et al., 2026), our time fixed effects absorb any aggregate time series confounders.

We find clear evidence that the decision window mediates the pass-through of monetary policy to state fiscal policy. Using our most stringent fixed-effects specification, Table 6 shows that a 5-basis-point surprise tightening, roughly one standard deviation of the high-frequency shock, lowers budgeted spending growth by roughly 0.25–0.50 percentage points when it arrives inside the decision window relative to the same surprise outside the window. The budget coefficient is negative and statistically significant at the 10 percent level or better for all four surprise measures. For realized spending, all four point estimates are negative, but only the orthogonalized Bauer–Swanson estimate is significant in the full sample; all four are significant at 10 percent or better after excluding FY2010, in the bottom panel. As with revenue news, national policy news transmits to state fiscal policy more so when it arrives during the decision window.

Next, we turn to aggregate time-series effects. While the monthly state-panel specification cleanly isolates the decision-window premium using cross-state variation in proposal dates, it absorbs the national time series shock and therefore cannot identify the aggregate fiscal response. Using quarterly NIPA data, we estimate local projections (Jordà, 2005) of state-and-local spending on monetary policy surprises, separating shocks that arrive in the fourth calendar quarter (Q4) from shocks that arrive in the first three calendar quarters (Q1–Q3). While states have different proposal dates, many of the decision windows overlap

with calendar Q4 (see Table A.1 in Appendix A). Figure 7 plots the coefficients over the 20 quarters following each shock. Red curves report the effects of Q4 shocks, and blue curves report the effects of Q1–Q3 shocks. Our aggregate specification controls for four lags of quarterly changes in real GDP, payroll employment, S&P 500 prices, and the CPI, and for eight lags of quarterly changes in the one-year Treasury yield.



(a) Raw BS surprise

(b) Orthogonalized BS surprise

(c) 2-year Treasury surprise

(d) 5-year Treasury surprise

Figure 7: **Monetary Policy and State-and-Local Spending, by Budget Season.** Local projections of cumulative real state-and-local government spending (NIPA), scaled by lagged real GDP, on four quarterly monetary policy surprise measures: the raw and orthogonalized Bauer–Swanson surprises and the 2-year and 5-year Treasury yield surprises from Bauer and Swanson (2023). Monthly surprises are summed within each quarter and kept in their native percentage-point units. Each panel splits shocks by whether they arrive in calendar Q4, when many states draft the upcoming budget, or outside it, in Q1–Q3. The local projections include four lags of state-and-local spending growth; four lags of each season-specific shock (the Q4 and Q1–Q3 components entered separately); quarters $t-2$ through $t-5$ of real GDP growth, payroll-employment growth, S&P 500 returns, and CPI inflation; and quarters $t-2$ through $t-9$ of the change in the one-year Treasury yield. The sample period is 1988–2023, the span of the Bauer–Swanson surprises. The vertical axis is the cumulative spending response in percent of initial GDP. Shaded areas are 90 percent confidence intervals based on horizon-dependent Newey–West standard errors.

We find that Q4 tightenings generate persistent declines in state-and-local spending, while Q1–Q3 tightenings have negligible effects. The response to Q4 shocks is not immediate; it builds over roughly two to three years. This timing is consistent with our channel: a policy-news event affects public spending only after it has been incorporated into budgets that are subsequently enacted and executed. Across measures, the Q4 state-and-local spending response is 0.14–0.35 percent of initial GDP, three to four years after a 10-basis-point surprise. The direct state-and-local spending response is therefore about one-fifth to one-quarter of the corresponding total-output response at these horizons. (Appendix Figure F.4).[26]

This Q4 effect is not driven by the shocks themselves: the Q4 and Q1–Q3 surprise distributions are statistically indistinguishable in mean, variance, and shape (Appendix Figure F.3). Appendix F.4 tests the Q4 difference directly. We also show in this Appendix that the Q4 effect is not driven by the December FOMC meeting alone and survives controlling for federal spending. Federal fiscal spending does not exhibit similar patterns.

Consistent with the private sector employment results, these findings show that news is incorporated into state spending during the decision window. Importantly, the differential decision-window effect appears to be persistent, likely reflecting the multiplier effect of fiscal spending.

Moreover, our monetary policy results offer a complementary institutional perspective on the classic problem that monetary policy operates with long and variable lags (Friedman, 1961; Christiano et al., 1999). Standard accounts emphasize nominal rigidities, information stickiness, and financial frictions (Christiano et al., 2005; Coibion and Gorodnichenko, 2015; Bernanke et al., 1996). The evidence here points to an additional public-sector budget calendar friction: because budgets fix commitments at discrete calendar dates, the same monetary policy shock has a larger and more persistent effect if it arrives when many states are in their decision window.

5 Conclusion

This paper studies how economic shocks that could affect revenue expectations are translated into state fiscal policy and real activity. Using hand-collected budget documents for 46 U.S.

[26]For the orthogonalized Bauer–Swanson measure, the state-and-local spending response is 0.28 percent at three years and 0.35 percent at four years, compared with total-output responses of 1.50 and 1.33 percent of initial GDP, respectively.

states plus D.C., we show that the original enacted budget is the key commitment margin for spending and that revenue news affects this commitment only when it arrives during the budget decision window. Comparable off-window news remains informative about realized revenue, but it is not processed into forecasts, spending plans, or realized spending in the same way. This timing difference carries through to the private economy, especially among industries and firms exposed to state and local government demand.

The main implication is that routine budget adjustment is a quantitatively important propagation mechanism for ordinary economic shocks. The aggregate real-effects estimates imply that news arriving during the decision window affects private employment and wages roughly four to five times as much as similar news arriving outside the decision window. Using this variation, our paper can estimate the real consequences of the endogenous budget response to economic conditions. Our finding that monetary policy transmission is stronger during a state's decision window offers an institutional lens for the long and variable lags of policy transmission to state spending.

The results also change how one should think about fiscal stabilization. If budgets respond only when information arrives inside a narrow budget decision window, then the timing of forecasts, budget revisions, and intergovernmental aid can matter as much as their total size. Aid that arrives after a budget has been committed may prevent later cuts, but it is less likely to change the same-year spending plan that governs immediate procurement and employment. Conversely, negative news that arrives while the budget is being written can be converted quickly into spending cuts and private-sector contractions. The sign-specific results suggest that this propagation is especially important in downturns, when firms and workers may have less capacity to absorb fiscal contractions.

More broadly, the paper shows that fiscal institutions shape shock transmission through timing: budget calendars determine when information becomes policy. This insight extends beyond state public finance. Many large organizations, such as government agencies, corporations, and institutional investors, receive information continuously but commit to actions only periodically. As data on expectations and plans become increasingly available, our simple decision-window framework offers a lens for understanding how information is incorporated into decisions. Applying this framework more widely can illuminate how shocks, such as monetary or federal fiscal policy, transmit to the real economy.

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Table 1: **Summary Statistics**

Panel A summarizes total real expenditures and total real revenues at the state-fiscal-year level over fiscal years 2002–2024 across 46 states and the District of Columbia. "Actual" is total realized real spending or revenue, in billions of dollars. "Budget" is the log change in the original budget across fiscal years, $\log(\text{Budget}^E_{s,t}/\text{Budget}^E_{s,t-1})$ for spending and the analogous object for revenue. "Execution" is the within-fiscal-year gap between the realized outcome and the original budget, $\log(\text{Act}_{s,t}/\text{Budget}_{s,t})$. Panel B summarizes the state-fiscal-year-level shift-share news measure aggregated across proposal-relative windows. Letting ℓ be the calendar-month distance from state s's FY-t proposal date, the windows are pre-decision ($\ell \in [-11, -6]$), decision ($\ell \in [-5, -1]$), deliberation ($\ell \geq 0$ and $m <$ FY start), and execution (FY start $\leq m \leq$ FY end). Panel B statistics are multiplied by 100.

Panel A: Fiscal Changes

	Mean	SD	P5	P25	Median	P75	P95
Spending							
Actual total spending (Bn)	20.470	25.112	1.520	4.879	11.765	26.802	71.371
Budget-$\log(\text{Budget}^E_{s,t}/\text{Budget}^E_{s,t-1})$	0.019	0.083	-0.098	-0.018	0.015	0.049	0.144
Execution-$\log(\text{Act}^E_{s,t}/\text{Budget}^E_{s,t})$	-0.075	0.111	-0.304	-0.100	-0.052	-0.024	0.051
Revenues							
Actual total revenue (Bn)	21.317	26.197	1.733	5.218	11.988	28.791	70.571
Budget-$\log(\text{Budget}^R_{s,t}/\text{Budget}^R_{s,t-1})$	0.019	0.082	-0.101	-0.013	0.018	0.050	0.136
Execution-$\log(\text{Act}^R_{s,t}/\text{Budget}^R_{s,t})$	-0.029	0.123	-0.175	-0.059	-0.018	0.020	0.114

Panel B: Monthly Shift-Share News by Budget Window (×100)

	Mean	SD	P5	P25	Median	P75	P95
News Pre-Decision.	-0.06	0.51	-0.44	-0.14	-0.06	0.02	0.30
News Decision	-0.07	0.27	-0.47	-0.14	-0.06	0.03	0.24
News Deliberation	-0.06	0.53	-0.51	-0.14	-0.05	0.03	0.30
News Execution	-0.06	0.54	-0.54	-0.14	-0.05	0.03	0.29

Table 2: **Timing of News and Original Budget Formation**

Monthly-panel regressions of original-budget forecast revisions, forecast errors, and realized revenue growth on state-specific shift-share news by proposal-relative timing. Panel A reports separate coefficients for pre-decision news ($\text{News}^{\text{Pre}}_{s,t,m}$, $\ell \in [-11, -6]$), decision-window news ($\text{News}^{\text{Decision}}_{s,t,m}$, $\ell \in [-5, -1]$), and deliberation news ($\text{News}^{\text{Delib}}_{s,t,m}$, $\ell \geq 0$ before fiscal-year start). Panel B reports the in/out specification, where the coefficient on $\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}^{\text{Decision}}_{s,t}\}$ is the decision-window premium relative to pooled off-window news. All columns share a common sample (state-fiscal-years with both the forecast revision and the forecast error observed), so realized revenue growth equals the forecast revision plus the forecast error and the coefficients satisfy the adding-up identity column by column. The "one-sided p-value" row in Panel B tests the framework's directional predictions—a positive premium for the forecast and a negative premium for the forecast error; the framework makes no directional prediction for realized revenue, so that entry is left blank. The sample is a stacked monthly panel restricted to these three pre-fiscal-year windows, 2002–2024. All specifications include state and fiscal-year-by-calendar-month fixed effects and predetermined controls: four lagged quarterly windows of CFNAI, payroll employment, the S&P 500, the five-year Treasury yield, and CPI, with the most recent ending one year before budget formation; the Borusyak et al. (2022) aggregate-share control; and two-year-lagged state rainy-day-fund balances and GDP growth. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Window-specific responses

	Forecast $\log(\text{Budget}^R_t/\text{Budget}^R_{t-1})$	Forecast error $\log(\text{Act}^R_t/\text{Budget}^R_t)$	Realized $\log(\text{Act}^R_t/\text{Budget}^R_{t-1})$
	(1)	(2)	(3)
$\text{News}^{\text{Pre}}_{s,t,m}$	-0.10	1.21***	1.11**
	(0.37)	(0.39)	(0.55)
$\text{News}^{\text{Decision}}_{s,t,m}$	2.74***	-0.34	2.40***
	(0.61)	(0.68)	(0.85)
$\text{News}^{\text{Delib}}_{s,t,m}$	0.39	0.69**	1.08**
	(0.62)	(0.30)	(0.45)
State FE	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓
Controls	✓	✓	✓
Observations	15,042	15,042	15,042
Adj. R^2	0.115	0.628	0.538

Panel B: Decision-window premium

	Forecast $\log(\text{Budget}^R_t/\text{Budget}^R_{t-1})$	Forecast error $\log(\text{Act}^R_t/\text{Budget}^R_t)$	Realized $\log(\text{Act}^R_t/\text{Budget}^R_{t-1})$
	(1)	(2)	(3)
$\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}^{\text{Decision}}_{s,t}\}$	2.53***	-1.22	1.31
	(0.49)	(0.80)	(0.83)
One-sided p-value	0.000	0.066	
State FE	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓
Controls	✓	✓	✓
Observations	15,042	15,042	15,042
Adj. R^2	0.115	0.628	0.538

Table 3: **Pass-Through: Revenue Forecast Updates to Spending**

The table reports 2SLS estimates of the pass-through from original-budget revenue forecast updates to spending. Panel A uses $\log(\text{Budget}^E_{s,t}/\text{Budget}^E_{s,t-1})$ as the dependent variable; Panel B uses $\log(\text{Act}^E_{s,t}/\text{Budget}^E_{s,t-1})$. The endogenous regressor is $\log(\text{Budget}^R_{s,t}/\text{Budget}^R_{s,t-1})$, instrumented separately across columns with news arriving in the pre-decision ($\ell \in [-11, -6]$), decision ($\ell \in [-5, -1]$), or deliberation ($\ell \geq 0$ before fiscal-year start) window; the other two window-news terms enter as controls. The sample is the stacked monthly panel restricted to these three pre-fiscal-year windows. All specifications include state and fiscal-year-by-calendar-month fixed effects and the predetermined controls from Table 2. Placebo instruments are reported as dashes because their 2SLS estimates are not interpretable. "Effective F" is the first-stage Kleibergen–Paap cluster-robust Wald statistic. "AR 95% CI" reports weak-instrument-robust Anderson and Rubin (1949) confidence sets; these are bounded for the decision-window instrument and unbounded for the placebo instruments. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Original spending plan

	Spending budget: $\log(\text{Budget}^E_t/\text{Budget}^E_{t-1})$		
	(1)	(2)	(3)
Revenue $\widehat{\text{budget}}$ change	–	0.36**	–
		(0.15)	
Instrument window	Pre-decision	Decision	Deliberation
Effective F	0.1	20.7	0.4
AR 95% CI	Unbounded	[0.03, 0.71]	Unbounded
State FE	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓
Controls	✓	✓	✓
Observations	15,042	15,042	15,042

Panel B: Actual spending growth

	Actual spending growth: $\log(\text{Act}^E_t/\text{Budget}^E_{t-1})$		
	(1)	(2)	(3)
Revenue $\widehat{\text{budget}}$ change	–	0.52***	–
		(0.15)	
Instrument window	Pre-decision	Decision	Deliberation
Effective F	0.1	20.7	0.4
AR 95% CI	Unbounded	[0.11, 0.83]	Unbounded
State FE	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓
Controls	✓	✓	✓
Observations	15,042	15,042	15,042

Table 4: **Real Effects of Decision-Window News**

The table reports triple-difference regressions of one-year log changes in private employment and private wages on the state-specific shift-share news measure, $\text{News}_{s,t,m}$, interacted with an indicator for months in state s's budget decision window for fiscal year t, $\mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$, and a measure of industry exposure to state and local government demand, built from the average five-year-trailing, two-year-lagged BEA government-purchase share of each industry. Panel A uses the binary indicator GovShare^{high}, which equals one for state–industry cells with above-median exposure; Panel B uses the continuous measure GovShare^{sd}, the same share standardized to mean zero and unit standard deviation across cells. The sample is a stacked state-industry-fiscal-year-month panel covering two-digit NAICS industries from FY1991 through FY2024. For each state-fiscal-year, the included months fall before the fiscal-year start. All specifications include the relevant lower-order interactions. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Above-Median Exposure Indicator

	$\log(\text{Emp}_{s,t}/\text{Emp}_{s,t-1})$		$\log(\text{Wages}_{s,t}/\text{Wages}_{s,t-1})$	
	(1)	(2)	(3)	(4)
$\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\} \times \text{GovShare}_i^{high}$	0.25**	0.27**	0.34**	0.37**
	(0.12)	(0.13)	(0.16)	(0.18)
Two-way Interactions	✓	✓	✓	✓
State × Year FE	✓	✓	✓	✓
Industry × Year FE	✓	✓	✓	✓
State × Industry FE		✓		✓
FYear × Year-Month FE		✓		✓
Observations	566,079	566,079	566,079	566,079
Adj.R^2	0.55	0.57	0.78	0.78

Panel B: Continuous Exposure Measure

	$\log(\text{Emp}_{s,t}/\text{Emp}_{s,t-1})$		$\log(\text{Wages}_{s,t}/\text{Wages}_{s,t-1})$	
	(1)	(2)	(3)	(4)
$\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\} \times \text{GovShare}_{i,t}^{sd}$	0.15**	0.16**	0.15*	0.17*
	(0.06)	(0.07)	(0.08)	(0.08)
Two-way Interactions	✓	✓	✓	✓
State × Year FE	✓	✓	✓	✓
Industry × Year FE	✓	✓	✓	✓
State × Industry FE		✓		✓
FYear × Year-Month FE		✓		✓
Observations	566,079	566,079	566,079	566,079
Adj.R^2	0.55	0.57	0.78	0.78

Table 5: **Aggregate State-Level Real Effects of Decision-Window News**

The table reports monthly-panel regressions of state-level one-year log changes in private employment and private wages on the state-specific shift-share news measure, $\text{News}_{s,t,m}$. The specification is an in/out parametrization: $\text{News}_{s,t,m}$ enters as a pooled off-window level and is interacted with an indicator for months in state s's budget decision window for fiscal year t, $\mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$. The coefficient on $\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$ is therefore the decision-window premium relative to off-window news, while the coefficient on $\text{News}_{s,t,m}$ is the off-window effect. The off-window category pools the pre-decision and deliberation months before the fiscal year starts. The sample is a stacked monthly panel restricted to one budget round, excluding the earliest before-pre-decision months and execution months. All regressions absorb state and fiscal-year-by-calendar-month fixed effects and include predetermined controls: four lagged quarterly windows of CFNAI, payroll employment, the S&P 500, the five-year Treasury yield, and CPI, with the most recent ending one year before budget formation; the Borusyak et al. (2022) aggregate-share control; and two-year-lagged state rainy-day-fund balances and GDP growth. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	$\log(\text{Emp}_{s,t}/\text{Emp}_{s,t-1})$	$\log(\text{Wages}_{s,t}/\text{Wages}_{s,t-1})$
	(1)	(2)
$\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	0.404^{***}	0.572^{***}
	(0.088)	(0.211)
$\text{News}_{s,t,m}$	0.095^{***}	0.182
	(0.033)	(0.112)
State FE	✓	✓
FYear × Year-Month FE	✓	✓
Macro + State Controls	✓	✓
Observations	20,080	20,080
Adj. R^2	0.84	0.75

Table 6: **Monetary Policy Surprises and the Budget Decision Window**

Monthly-panel regressions in the spirit of Table 2, replacing the state-specific revenue news with a national monetary policy surprise (MPS). The ACFR sample starts in FY2002. It is a stacked state–fiscal-year–calendar-month panel covering the pre-decision, decision, and deliberation windows for each state-year. The key regressor is $\text{MPS}_m \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$, where m indexes calendar months and $\mathcal{W}_{s,t}^{\text{Decision}}$ is the set of calendar months in state s's budget decision window for fiscal year t. Columns vary the surprise measure: Bauer–Swanson raw (BS Raw) and orthogonalized (BS Orth.) surprises and the Bauer–Swanson 2- and 5-year Treasury responses (Bauer and Swanson, 2023). Panel A is the spending budget, Panel B is realized spending, and Panel C repeats Panel B after excluding the FY2010 crisis cohort. All outcomes are measured relative to the budget in $t-1$. All specifications include state and fiscal-year-by-calendar-month fixed effects and predetermined controls: four lagged quarterly windows of CFNAI, payroll employment, the S&P 500, the five-year Treasury yield, and CPI, with the most recent ending one year before budget formation; and two-year-lagged state rainy-day-fund balances and GDP growth. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Spending budget, $\log(Budget_t^E / Budget_{t-1}^E)$

	(1) BS Raw	(2) BS Orth.	(3) UST 2y	(4) UST 5y
$MPS_m \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	-0.06*	-0.07*	-0.10***	-0.08***
	(0.03)	(0.03)	(0.03)	(0.03)
State + FYear × Year-Month FE	✓	✓	✓	✓
Macro + State Controls	✓	✓	✓	✓
Observations	17,034	17,034	17,034	17,034
Adj. R^2	0.145	0.145	0.145	0.145

Panel B: Realized spending, $\log(Act_t^E / Budget_{t-1}^E)$

	(1) BS Raw	(2) BS Orth.	(3) UST 2y	(4) UST 5y
$MPS_m \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	-0.06	-0.11*	-0.09	-0.05
	(0.05)	(0.05)	(0.06)	(0.05)
State + FYear × Year-Month FE	✓	✓	✓	✓
Macro + State Controls	✓	✓	✓	✓
Observations	17,034	17,034	17,034	17,034
Adj. R^2	0.614	0.614	0.614	0.614

Panel C: Realized spending, excluding FY2010, $\log(Act_t^E / Budget_{t-1}^E)$

	(1) BS Raw	(2) BS Orth.	(3) UST 2y	(4) UST 5y
$MPS_m \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	-0.11**	-0.12**	-0.11**	-0.09**
	(0.05)	(0.05)	(0.05)	(0.04)
State + FYear × Year-Month FE	✓	✓	✓	✓
Macro + State Controls	✓	✓	✓	✓
Observations	16,300	16,300	16,300	16,300
Adj. R^2	0.626	0.626	0.626	0.626

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A Institutional Setting and Data

This appendix provides additional detail on the state budget calendar and the primary data source underlying the analysis in Section 1.

A.1 State Budget Calendar

Table A.1 reports the budget calendar for each state in our sample. Columns include the fiscal-year start date and the typical month in which the governor submits the initial budget proposal. These dates define the decision, deliberation, and execution windows used throughout the paper. The key source of cross-state identification is the variation in proposal dates: early-proposing states (e.g., those with October–January proposal months) and late-proposing states (e.g., February–March) face the same national economic conditions but assign them to different budget windows.

The NASBO snapshots are available for 1987, 1989, 1992, 1997, 1999, 2002, 2008, 2015, and 2021. Our baseline uses a piecewise-constant calendar: for each fiscal year, we assign the most recent NASBO calendar observed at or before that year and carry it forward until a newer snapshot is available. This avoids using future calendar information while preserving low-frequency institutional changes in budget timing. One exception is Arkansas: the 1987–2002 NASBO waves report the governor's submission as "Sept./Dec.," whereas the 2008–2021 waves report November for the same institution. We code Arkansas as a November proposer throughout, so its budget calendar does not shift artificially when NASBO rewords the entry.

Beyond timing, the table reports two cycle codings from the same NASBO source, taken as of the 2021 survey wave: the "budget cycle", whether the state enacts a budget every year or a single two-year budget (e.g., Texas and Indiana), and the "legislative cycle", whether the legislature holds a regular session every year or every other year (e.g., Montana and Nevada, whose legislatures convene only in odd-numbered years). Our baseline analysis keeps all state-years, using each state's imputed calendar as constructed; Section 3 and Appendix E.4 show that the decision-window results are essentially unchanged when second-year cohorts are excluded, re-dated, or interacted separately. Note that under GASB 34, amendments authorized before a fiscal year begins are counted in that year's original budget, so even the second year of a biennium can include relevant revisions in its "Original Budget."

Separately, four states—Wyoming, Washington, North Dakota, and Oregon—are out-

side the sample entirely: their ACFR budgetary comparison schedules report the two-year biennium as a whole (Washington's fiscal 2021 report, for example, presents a single "Original Budget 2019–21 Biennium" column), so the annual budget objects our outcomes require do not exist for them.

Table A.1: **State Budget Calendar Timing (2021).** This table summarizes each state's fiscal-year start date, the typical range of months when initial and final budget proposals are submitted, and the legislative and budget cycles (A = annual, B = biennial). All columns reflect the 2021 NASBO snapshot, the most recent available.

State	FY Start	Initial Proposal	Final Proposal	Legislative cycle	Budget cycle
AL	Oct 1	Feb	May	A	A
AK	Jul 1	Dec	Jun	A	A
AZ	Jul 1	Jan-Feb	Jun	A	A
AR	Jul 1	Nov-Jan	Apr-May	A	B
CA	Jul 1	Jan	Jun	A	A
CO	Jul 1	Nov	Apr-Jun	A	A
CT	Jul 1	Feb	Jun	A	B
DC	Oct 1	Mar	Jun	–	–
DE	Jul 1	Jan	Jun	A	A
FL	Jul 1	Jan	Mar-May	A	A
GA	Jul 1	Jan	May	A	A
HI	Jul 1	Dec	May	A	B
ID	Jul 1	Jan	Mar-Apr	A	A
IL	Jul 1	Feb	Jun	A	A
IN	Jul 1	Jan	Apr-May	A	B
IA	Jul 1	Feb	May	A	A
KS	Jul 1	Jan	May	A	A
KY	Jul 1	Jan	May	A	B
LA	Jul 1	Feb	Jun	A	A
ME	Jul 1	Jan	Jun	B	B
MD	Jul 1	Jan	Apr-May	A	A
MA	Jul 1	Jan	Jun	B	A
MI	Oct 1	Feb	Jul	A	A
MN	Jul 1	Jan	May	A	B
MS	Jul 1	Nov	Mar-Apr	A	A
MO	Jul 1	Jan	May	A	A
MT	Jul 1	Nov	May	B	B
NE	Jul 1	Jan	Jun	A	B
NV	Jul 1	Jan	Jun	B	B
NH	Jul 1	Feb	Jun	A	B
NJ	Jul 1	Feb	Jun	A	A
NM	Jul 1	Jan	Mar-Apr	A	A
NY	Apr 1	Jan	Mar	A	A
NC	Jul 1	Mar	Jun	B	B
OH	Jul 1	Feb	Jun	B	B
OK	Jul 1	Feb	May-Jun	A	A
PA	Jul 1	Feb	Jun	A	A
RI	Jul 1	Jan	Jun	A	A
SC	Jul 1	Jan	May	A	A
SD	Jul 1	Dec	Mar-Apr	A	A
TN	Jul 1	Feb	May-Jun	A	A
TX	Sep 1	Jan	May	B	B
UT	Jul 1	Dec	Mar-Apr	A	A
VT	Jul 1	Jan	May-Jun	B	A
VA	Jul 1	Dec	Apr-May	A	B
WV	Jul 1	Jan	Apr	A	A
WI	Jul 1	Jan	Jun	B	B

Reported proposal months are highly persistent within state. Forty of the 52 jurisdictions never change their reported proposal month across the NASBO waves covering the sample period, and Table A.2 documents the twelve jurisdictions with a change. Most changes are a single one-month shift.

Table A.2: **Changes in Reported Proposal Months Across NASBO Waves**

The table lists every state whose reported proposal month changes across the NASBO survey waves assigned to fiscal years 1995–2024 under the baseline piecewise-constant calendar (each fiscal year uses the most recent wave observed at or before that year); the remaining 40 jurisdictions keep the same reported month throughout. Each entry shows the assigned proposal month and the first fiscal year to which it applies.

State	Reported proposal month (first fiscal year assigned)
CO	Jan (FY1995) → Oct (FY1997)
FL	Dec (FY1995) → Jan (FY1997) → Feb (FY2008)
IL	Mar (FY1995) → Feb (FY1997)
MT	Jan (FY1995) → Nov (FY2008)
NC	Feb (FY1995) → Mar (FY2015)
NJ	Jan (FY1995) → Feb (FY2008)
OR	Dec (FY1995) → Jan (FY1997) → Nov (FY2008)
PR	Jan (FY1995) → Feb (FY1997)
RI	Feb (FY1995) → Jan (FY2008)
TN	Jan (FY1995) → Feb (FY1999) → Jan (FY2008)
VA	Jan (FY1995) → Dec (FY1997)
WY	Dec (FY1995) → Nov (FY2015)

A.2 Primary Data Source: Annual Comprehensive Financial Reports

Our primary data source is the Annual Comprehensive Financial Report (ACFR), which each state publishes after the close of the fiscal year. ACFRs contain audited financials and, crucially, comparisons of budgeted and actual figures for both revenues and expenditures. Figure A.2 reproduces a representative budgetary comparison statement. This statement reports the original budget (first enacted appropriation, including pre-fiscal-year amendments), the final revised budget (original budget plus all mid-year amendments through the close of the fiscal year), and actual amounts for each major revenue and expenditure category. GASB Statement No. 34 enforced this structure beginning in fiscal years 2001–2003, with smaller states allowed later adoption of the standard. We hand-collected and digitized analogous tables from 46 states and the District of Columbia over fiscal years 1995–2024. As discussed in Section 1, our analysis focuses on the original budget ("initial budget") and

actual outcomes; the final revised budget is reported but not used as a primary analytical object because it accumulates amendments of heterogeneous institutional origin throughout the fiscal year.

Figure A.1 documents the coverage of the hand-collected panel, which improves over time. Coverage rises from 14 jurisdictions with complete expenditure totals in FY1995 to 40 by FY2002 and 47 at its peak. Revenue totals are available for a slightly smaller subset because some ACFR schedules report appropriations without a corresponding original-budget revenue column.

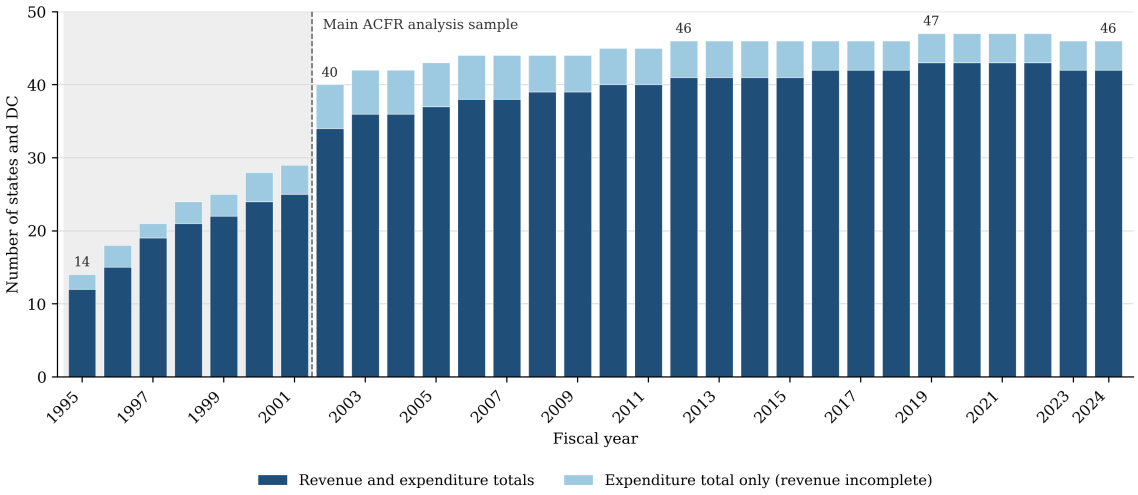


Figure A.1: **Coverage of the Hand-Collected ACFR Panel over Time.** Each bar reports the number of states and the District of Columbia with both an original-budget amount and an actual realization for the general-fund expenditure total. The dark segment identifies jurisdictions that also have both measures for the revenue total; the light segment has complete expenditure data but incomplete revenue data. Puerto Rico is excluded to match the paper's analysis universe. The shaded region denotes FY1995–2001; analyses using ACFR budget outcomes begin in FY2002.

A.3 Comparison with the NASBO Fiscal Survey

Much of the state-fiscal literature measures budgets with the NASBO Fiscal Survey of States, a semiannual survey of state budget offices reporting general-fund revenues and expenditures. The survey is conducted in common calendar windows: roughly February–May for the spring edition and August–October for the fall edition, rather than at each state's proposal, fiscal-year-close date. As a result, the budget and actual figures reported in a given survey wave can correspond to different budget vintages across states, depending on where each state is

Budgeted Funds

Combined Schedule Of Revenues, Expenditures And Changes In Fund Balances - Statutory Basis - Budget and Actual
Fiscal Year Ended June 30, 2002
(Amounts in thousands)

	Original Budget	Final Budget	Actual	Variance over/(under)
REVENUES AND OTHER FINANCING SOURCES				
Revenues:				
Taxes	$ 14,930,000	$ 14,930,100	$ 13,622,710	$ (1,307,390)
Assessments	-	2,700	129,354	126,654
Federal grants and reimbursements	4,126,300	4,126,300	4,334,934	208,634
Departmental	1,672,300	1,422,389	1,205,629	(216,760)
Miscellaneous	-	92,483	265,492	173,009
Total revenues	**20,728,600**	**20,573,972**	**19,558,119**	**(1,015,853)**
Other financing sources:				
Fringe benefit cost recovery	-	-	161,524	161,524
Lottery reimbursements	-	87,341	87,341	-
Lottery distributions	-	857,700	871,136	13,436
Caseload increase mitigation transfer	-	-	4,878	4,878
Tobacco settlement transfer	-	154,728	154,728	-
Operating transfers in	1,062,000	221,911	515,531	293,620
Transfer for transitional escrow	-	-	579,215	579,215
Stabilization transfer	-	-	1,030,000	1,030,000
Capital projects funds year end transfer	-	-	51,369	51,369
Federal reimbursement transfer in	-	-	1,902	1,902
Transfer for tax reduction	-	-	33,605	33,605
Total other financing sources	**1,062,000**	**1,321,680**	**3,491,229**	**2,169,549**
Total revenues and other financing sources	**21,790,600**	**21,895,652**	**23,049,348**	**1,153,696**
EXPENDITURES AND OTHER FINANCING USES				
Expenditures:				
Legislature	88,227	72,850	58,909	13,941
Judiciary	584,123	587,686	580,057	7,629
Inspector General	2,019	1,975	1,973	2
Governor and Lieutenant Governor	5,611	5,655	5,542	113
Secretary of the Commonwealth	35,088	35,028	34,693	335
Treasurer and Receiver-General	187,159	121,024	119,114	1,910
Auditor of the Commonwealth	15,380	15,351	15,347	4
Attorney General	35,687	36,335	35,674	661
Ethics Commission	1,415	1,412	1,411	1
District Attorney	79,566	81,253	81,056	197
Office of Campaign & Political Finance	1,089	1,088	1,440	(352)
Sheriff's Departments	193,220	215,292	211,973	3,319
Disabled Persons Protection Commission	1,616	1,714	1,708	6
Board of Library Commissioners	7,251	7,270	6,984	286
Comptroller	8,352	8,415	8,410	5
Administration and finance	1,429,507	1,386,683	1,344,343	42,340
Environmental affairs	226,182	234,966	221,717	13,249
Communities and development	122,269	123,018	116,790	6,228
Health and human services	4,241,579	4,403,293	4,312,159	91,134
Transportation and construction	129,104	151,990	145,917	6,073
Education	464,899	462,232	412,846	49,386
Higher education	1,014,147	1,039,047	1,029,516	9,531
Public safety	850,519	945,382	917,982	27,400
Economic development	46,362	46,241	34,008	12,233
Elder affairs	276,755	291,729	273,759	17,970
Consumer affairs	46,909	48,552	46,872	1,680
Labor	83,542	89,750	59,319	30,431
Direct local aid	5,191,844	5,193,357	5,189,116	4,241
Medicaid	4,985,458	5,282,868	5,259,275	23,593
Pension	931,029	797,064	795,782	1,282
Debt service:				
Principal retirement	659,802	710,674	692,001	18,673
Interest and fiscal charges	691,889	641,017	612,734	28,283
Total expenditures	**22,637,599**	**23,040,211**	**22,628,427**	**411,784**

Figure A.2: **Example: ACFR for Massachusetts.** This figure illustrates the structure of data from Massachusetts's ACFR for fiscal year 2002. The top panel shows the budgetary comparison statement reporting original budget, final budget, and actual amounts. The bottom panel shows the reconciliation to GAAP-basis financials.

in its budget calendar. We instead build fiscal amounts from ACFR budgetary-comparison schedules. This choice is important for two reasons. First, the ACFR reports the original enacted budget, the final amended budget, and actual outcomes on a common audited budgetary basis, which is the decomposition our design requires. Second, the two sources measure different fiscal aggregates: NASBO reports state-defined general funds, while ACFR budgetary comparisons follow the appropriated budgetary basis used in each state's audited financial statements.

Table A.3 and Figure A.3 compare the two for the totals both report, over the estimation sample. The audited ACFR totals and the NASBO general-fund totals are strongly correlated in logs (0.89 for expenditure and 0.90 for revenue), but they do not match: 44 percent of expenditure state-years and 49 percent of revenue state-years agree within ten percent, and year-over-year growth rates correlate only 0.47 for expenditure and 0.61 for revenue. The gaps are structural, reflecting differences in fund coverage across states: Michigan, for example, routes most spending through the School Aid Fund and other restricted funds, so its NASBO general fund ($9 billion in FY2015) is a fraction of the appropriated budget the ACFR reports ($33 billion), while the ratio runs the other way for states such as Vermont and New York.

We therefore use each source where it is authoritative: NASBO for the budget calendar (proposal and enactment dates and annual-versus-biennial cycles, as in Appendix A.1), and the hand-collected ACFR schedules for the fiscal amounts.

Table A.3: **ACFR versus NASBO: Budget-Data Comparison**

The table compares realized general-fund totals from the audited ACFR budgetary-comparison schedules (this paper) with the NASBO Fiscal Survey of States, for the totals both sources report, over fiscal years 2002–2024. Amounts are placed in common units ($ millions; ACFR is reported in thousands, NASBO in millions), and the sample is state-years with a positive actual value in both sources. "Log-level correlation" is the correlation of log actual amounts; the median ratio and percentiles refer to the ACFR/NASBO ratio of actual amounts; "Within ±10% of parity" is the share of state-years with that ratio in $[0.9, 1.1]$; "Growth-rate correlation" is the correlation of year-over-year log changes, excluding changes exceeding one log point (coverage breaks rather than fiscal news).

	Expenditure	Revenue
Matched state-years	967	888
States	46	44
Log-level correlation	0.89	0.90
Median ratio (ACFR/NASBO)	1.02	1.03
Ratio, 10th–90th pctile	0.87–2.52	0.93–2.33
Within ±10% of parity	44%	49%
Growth-rate correlation	0.47	0.61

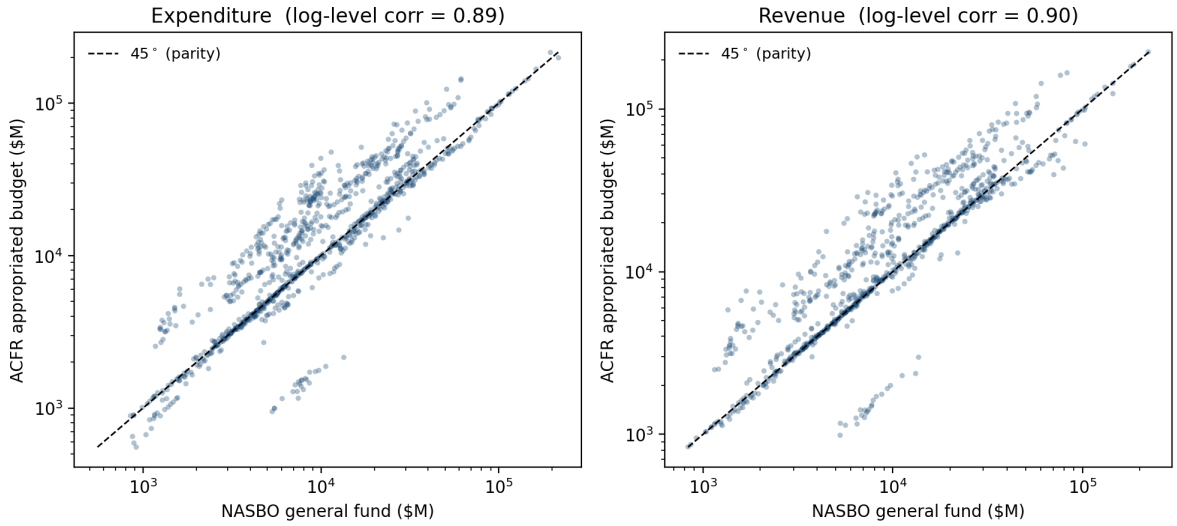


Figure A.3: **ACFR versus NASBO Actual Totals.** Each point is a state-fiscal-year, plotting the realized general-fund total from the NASBO Fiscal Survey (horizontal axis) against the audited ACFR appropriated-budget total (vertical axis), both in $ millions on a log scale, over fiscal years 2002–2024. The dashed line is parity. Left: expenditure; right: revenue. The two are strongly correlated but diverge in level for states whose general fund covers a different share of the appropriated budget.

B Stylized Facts and the Commitment Margin

This appendix follows the order of Section 1.3. It first documents the budget benchmark used in the decomposition, then reports the distribution and variance-decomposition evidence, and finally shows how revenue surprises map into aggregate spending, spending categories, and rainy-day funds.

B.1 Anchoring of the Initial Budget

A key assumption underlying our decomposition is that the prior year's initial budget, $\text{Budget}_{s,t-1}$, is the relevant reference point when the initial budget is drafted. Table B.1 provides direct evidence. We regress the log initial budget on $\log(\text{Budget}_{t-1})$ and $\log(\text{Act}_{t-2})$, the two most recent available benchmarks. This supports the use of Budget_{t-1} as the baseline in the planning component: conditional on the prior initial budget, the two-year-lagged actual adds little explanatory power.

Table B.1: **Anchoring of the Initial Budget**

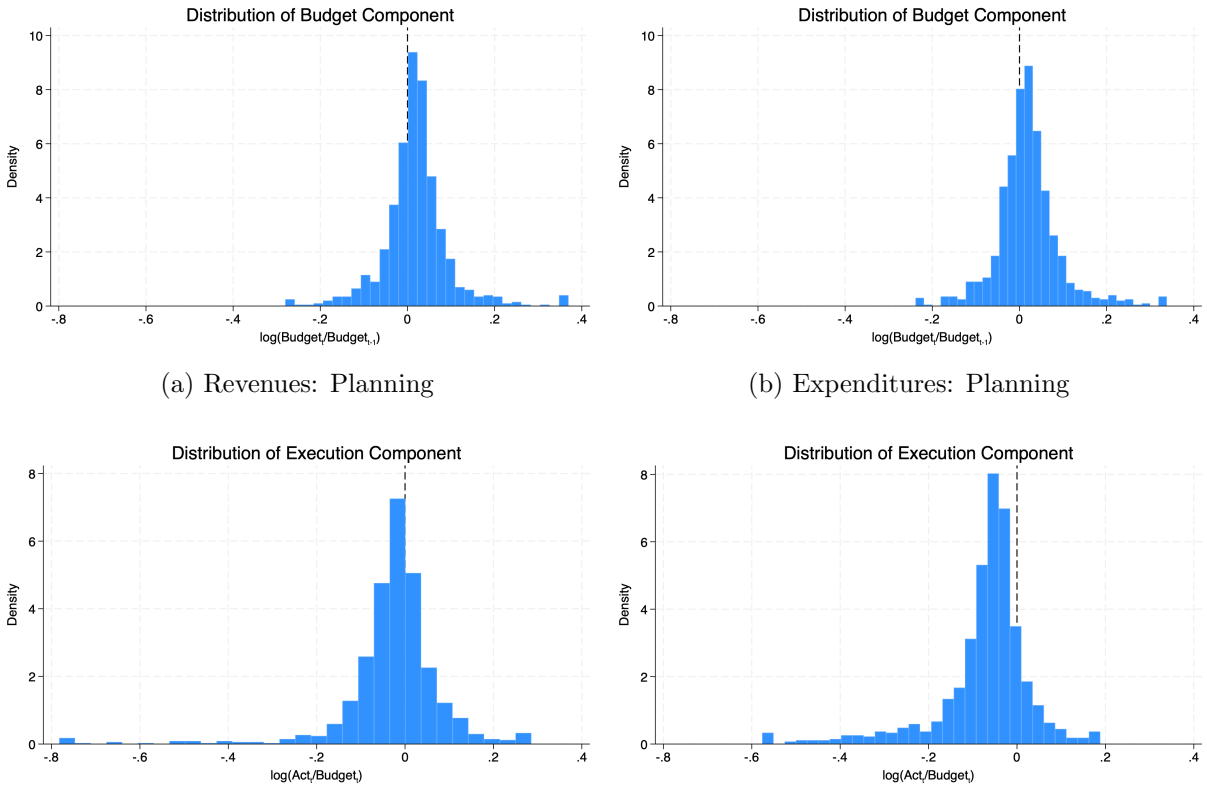
The table reports OLS regressions of the log initial-budget revenue level, $\log(\text{Budget}^R_{s,t})$, on lagged budget benchmarks: the prior year's initial budget, $\log(\text{Budget}^R_{s,t-1})$, and the two-year-lagged actual realization, $\log(\text{Act}^R_{s,t-2})$. The sample is an annual state-fiscal-year panel covering 46 states and the District of Columbia over fiscal years 2002–2024. Standard errors are clustered by state. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Revenue: $\log(\text{Budget}^R_t)$		
	(1)	(2)	(3)
$\log(\text{Budget}_{t-1})$	0.73***		0.67***
	(0.04)		(0.08)
$\log(\text{Act}_{t-2})$		0.55***	0.08
		(0.10)	(0.08)
State + FYear FE	✓	✓	✓
Observations	907	907	907
Adj.R^2	0.995	0.993	0.995

B.2 Decomposing Budget-Cycle Variation

Distribution of budget-cycle adjustments. Figure B.1 displays the empirical distributions of revenue and expenditure changes across the planning and execution phases of the budget cycle defined in Equation (1). Several patterns are noteworthy. First, the planning-phase distributions are centered near zero with moderate dispersion, indicating that initial budgets typically project modest growth relative to the prior year's initial budget. Second, the execution-phase distributions are centered well below zero: initial budgeted expenditures exceed actual outlays in nearly all observations. This systematic overestimation reflects the conservatism inherent in balanced-budget requirements and executive spending controls.

Variance decomposition. To quantify the contribution of each component, we decompose the variance of annual changes in actual outcomes using Equation (1). We first demean each variable within state: $\widetilde{X}_{s,t} \equiv X_{s,t} - \bar{X}_s$, then decompose the variance in actual spending



(a) Revenues: Planning

(b) Expenditures: Planning

(c) Revenues: Execution

(d) Expenditures: Execution

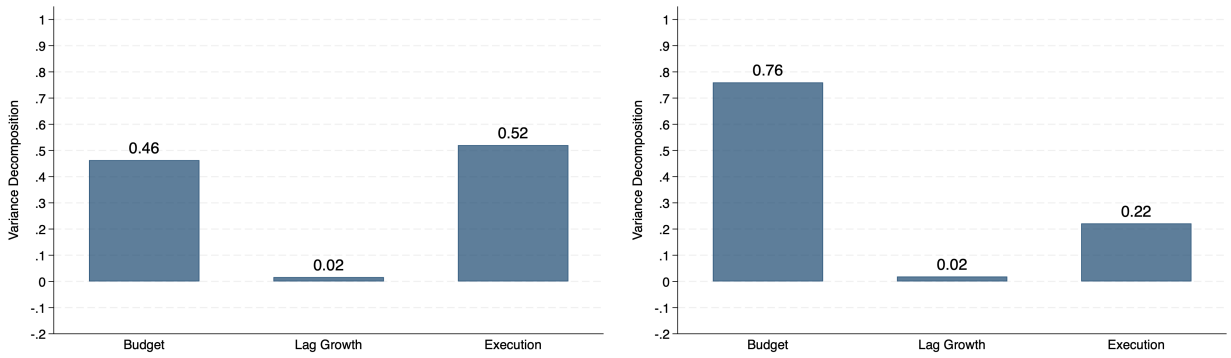
Figure B.1: **Distribution of State Revenue and Expenditure Changes over the Budget Cycle.** The figure shows the distributions of the planning and execution components in Equation (1) for both revenues and expenditures.

growth as follows:

$$1 = \frac{\operatorname{cov}\left(\log(\widetilde{\text{Budget}_{s,t}/\text{Budget}_{s,t-1}}), \Delta\widetilde{\log(\text{Act}_{s,t})}\right)}{\operatorname{var}\left(\Delta\widetilde{\log(\text{Act}_{s,t})}\right)} + \frac{\operatorname{cov}\left(\log(\widetilde{\text{Act}_{s,t}/\text{Budget}_{s,t}}), \Delta\widetilde{\log(\text{Act}_{s,t})}\right)}{\operatorname{var}\left(\Delta\widetilde{\log(\text{Act}_{s,t})}\right)} - \frac{\operatorname{cov}\left(\log(\widetilde{\text{Act}_{s,t-1}/\text{Budget}_{s,t-1}}), \Delta\widetilde{\log(\text{Act}_{s,t})}\right)}{\operatorname{var}\left(\Delta\widetilde{\log(\text{Act}_{s,t})}\right)}. \tag{B.1}$$

Equivalently, each share is the OLS slope from a regression of the corresponding component on $\Delta \log(\text{Act}_{s,t})$ with state fixed effects absorbed.

Alternative benchmark. The main-text variance decomposition in Figure 3 benchmarks the planning component to the prior year's initial budget, $\log(\text{Budget}_{s,t}/\text{Budget}_{s,t-1})$. An alternative is to benchmark to two-year-lagged actual outcomes, $\log(\text{Budget}_{s,t}/\text{Act}_{s,t-2})$, which avoids any mechanical dependence on the prior year's initial budget. Figure B.2 replicates the variance decomposition under this alternative benchmark. The planning component continues to account for the majority of the variation in realized spending, confirming that the concentration of fiscal adjustment in the initial budget is not an artifact of the benchmark choice.



(a) Total Revenues

(b) Total Expenditures

Figure B.2: **Decomposing the Sources of Variation in Actual Growth (Alternative Benchmark).** This figure replicates Figure 3 using two-year-lagged actual outcomes, $\log(\text{Budget}_{s,t}/\text{Act}_{s,t-2})$, as the planning-stage benchmark. Components are: Planning $\log(\text{Budget}_{s,t}/\text{Act}_{s,t-2})$, Execution $\log(\text{Act}_{s,t}/\text{Budget}_{s,t})$, and Lag Growth $\log(\text{Act}_{s,t-1}/\text{Act}_{s,t-2})$.

B.3 Revenue Pass-Through to Spending

Revenue constraints and expenditure responses. Table B.2 quantifies the relationship between revenue and expenditure changes across the budget phases. Revenue changes explain a substantial share of the variation in expenditure adjustments, with particularly strong co-movement during the planning phase. These estimates are consistent with the balanced-budget constraint binding most tightly during budget formation, when policymakers must align planned expenditures with projected revenues.

Table B.2: **Revenue Constraints and Expenditure Responses Across Budget Phases**

The table reports OLS regressions of phase-by-phase expenditure adjustments on the corresponding phase-by-phase revenue adjustment from the decomposition in Equation (1). The dependent variable in columns (1)–(2) is the budget component for total spending, $\log(\text{Budget}^E_{s,t}/\text{Budget}^E_{s,t-1})$; in columns (3)–(4) it is the execution component, $\log(\text{Act}^E_{s,t}/\text{Budget}^E_{s,t})$. The regressor in each column is the corresponding revenue component of Equation (1). The sample is an annual state-fiscal-year panel covering 46 states and the District of Columbia over fiscal years 2002–2024. Standard errors are clustered by state. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Total Spending			
	Budget $\log(\text{Budget}^E_{s,t}/\text{Budget}^E_{s,t-1})$		Execution $\log(\text{Act}^E_{s,t}/\text{Budget}^E_{s,t})$	
	(1)	(2)	(3)	(4)
Rev (Budget)	0.51***	0.47***		
	(0.06)	(0.06)		
Rev (Execution)			0.46***	0.24***
			(0.08)	(0.05)
State + FYear FE		✓		✓
Obs.	925	925	944	944
Adj.R^2	0.33	0.34	0.32	0.70

Asymmetric spending adjustment. Table B.3 documents asymmetric execution-stage spending adjustment to revenue surprises. In columns (1)–(2), revenue shortfalls are associated with substantial spending cuts, while windfalls have little effect on aggregate spending, consistent with balanced-budget enforcement and executive impoundment authority. With fiscal-year fixed effects in column (3), pass-through is moderate and statistically similar across signs: the windfall slope is 0.16, and the shortfall differential is 0.08 and insignificant. Thus, the asymmetry arises in within-state changes across fiscal years and is absorbed by fiscal-year-common components rather than within-year cross-state variation.

Table B.3: **Asymmetric Spending Adjustment to Revenue Shortfalls and Windfalls**

The table reports OLS regressions of the execution-phase expenditure adjustment, $\log(\text{Act}^E_{s,t}/\text{Budget}^E_{s,t})$, on the execution-phase revenue adjustment, $\log(\text{Act}^R_{s,t}/\text{Budget}^R_{s,t})$, an indicator for a within-year revenue shortfall, $\mathbf{1}\{\text{Rev shortfall}\} \equiv \mathbf{1}\{\log(\text{Act}^R_{s,t}/\text{Budget}^R_{s,t}) < 0\}$, and their interaction. Column (1) includes no fixed effects; column (2) adds state fixed effects; column (3) adds state and fiscal-year fixed effects, so its coefficients are identified from within-year cross-state variation only. The sample is an annual state-fiscal-year panel covering 46 states and the District of Columbia over fiscal years 2002–2024. Standard errors are clustered by state. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

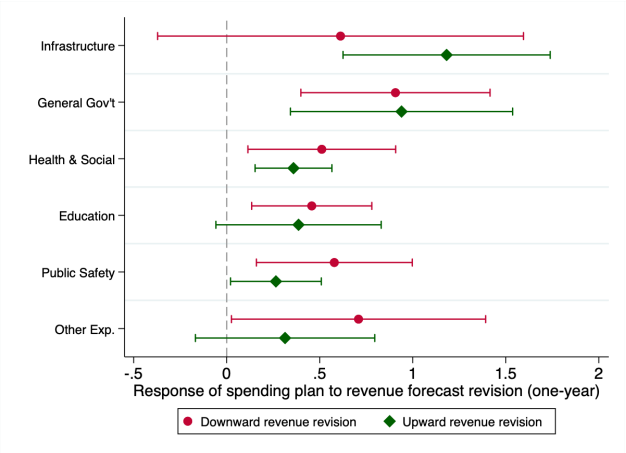
	Execution $\log(\text{Act}^E_{s,t}/\text{Budget}^E_{s,t})$		
	(1)	(2)	(3)
$\log(\text{Act}^R_{s,t}/\text{Budget}^R_{s,t}) \times 1(\text{Rev shortfall})$	0.54**	0.31**	0.08
	(0.21)	(0.14)	(0.14)
$\log(\text{Act}^R_{s,t}/\text{Budget}^R_{s,t})$	0.03	-0.03	0.16
	(0.20)	(0.12)	(0.13)
1(Rev shortfall)	-0.00	-0.01	-0.01
	(0.01)	(0.01)	(0.01)
State FE		✓	✓
FYear FE			✓
Obs.	944	944	944
Adj.R^2	0.36	0.66	0.70

Spending category adjustments. Our hand-collected schedules provide line-item spending detail, allowing us to trace budget and execution adjustments across categories. Panel (a) of Figure B.3 relates each category's original-budget growth, $\log(\text{Budget}^E_{\ell,t}/\text{Budget}^E_{\ell,t-1})$, to the revenue-forecast revision by sign. Forecast increases and decreases pass through similarly across categories; infrastructure is somewhat more responsive, but the difference is insignificant.

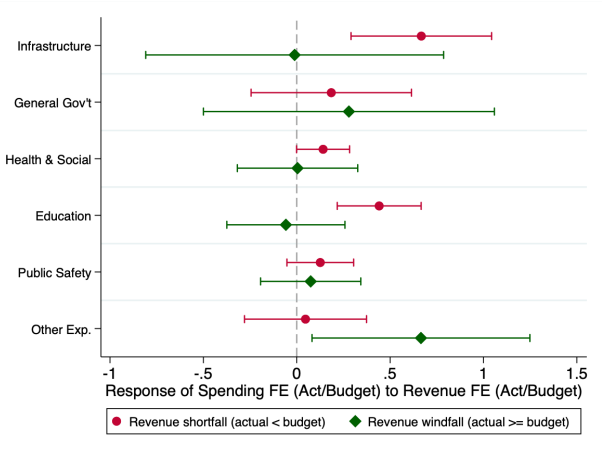
Panel (b) relates each category's execution deviation from budget to the revenue surprise, measured as realized revenue relative to the original forecast. Shortfalls are absorbed primarily through education cuts, whereas windfalls are concentrated in infrastructure and general government. Although the category-level windfall responses are larger in percentage terms, these categories account for only about 6 and 7 percent of total spending, respectively. Education is the largest category, so the smaller percentage shortfall response there has a larger effect on aggregate spending. The large windfall coefficients in Panel (b) therefore do not imply a larger aggregate spending response to windfalls.

Figure B.3: **Revenue Passes Through to Spending Plans Across Categories**

Each marker is the coefficient from a separate regression of one spending category's outcome on the corresponding total-revenue object, with the revenue term split by sign and 90% confidence intervals shown. Panel (a) regresses the one-year revision in a category's original spending plan, $\log(\text{Budget}^E_{\ell,t}/\text{Budget}^E_{\ell,t-1})$, on the one-year revision in the original total-revenue forecast, $\log(\text{Budget}^R_t/\text{Budget}^R_{t-1})$, entered separately when that revision is downward (cranberry) and upward (green diamonds). Panel (b) regresses the within-year execution error in category spending, $\log(\text{Act}^E_{\ell,t}/\text{Budget}^E_{\ell,t})$, on the within-year total-revenue execution error, $\log(\text{Act}^R_t/\text{Budget}^R_t)$, entered separately for revenue shortfalls (cranberry) and windfalls (green). All regressions absorb state and fiscal-year fixed effects and cluster standard errors by state. The six categories are Infrastructure, General Government, Health & Social, Education, Public Safety, and Other. Sample: fiscal years ≥ 2002.



(a) Planning: spending-plan revision vs. revenue-forecast revision



(b) Execution: spending error vs. revenue error

B.4 Rainy Day Funds: Realized Surpluses vs Budget Surprises

Rainy-day funds (RDFs) are intended to smooth resources across the fiscal cycle: states deposit when revenues exceed budgeted needs and draw down when a shortfall materializes. We examine how the funds serve this role. We examine realized surplus, $\text{Act}^R_{s,t} - \text{Act}^E_{s,t}$, which measures the resources available after budget execution, regardless of whether that outcome was anticipated. Results in Table B.4 shows that accumulation is significantly positively associated with favorable fiscal outcomes, consistent with states channeling realized surpluses into the fund. In the case of a shortfall, the point estimate is larger in magnitude, but the coefficient is not precisely estimated. This could due to withdrawals are constrained by the balance available and by statutory access rules.

Table B.4: **Rainy Day Fund Response to Budget Surplus and Forecast Errors**

The table reports OLS regressions of the change in the state's rainy-day-fund balance scaled by lagged realized expenditure, $\Delta\text{RDF}_{s,t}/\text{Act}^{E}_{s,t-1}$, where $\Delta\text{RDF}_{s,t} \equiv \text{RDF}_{s,t} - \text{RDF}_{s,t-1}$. The realized surplus is defined as $\text{Surplus}_{s,t} \equiv (\text{Act}^{R}_{s,t} - \text{Act}^{E}_{s,t})/\text{Act}^{E}_{s,t-1}$. Column (1) imposes a common slope for surpluses and deficits. Columns (2)–(3) allow the slope to differ depending on whether the realized fiscal position is positive or negative, and column (3) additionally controls for state GDP growth. All specifications include state and fiscal-year fixed effects. Unlike a log-change outcome, the scaled change is defined for state-years with zero rainy-day-fund balances, so these observations remain in the sample. The sample covers fiscal years 2002–2023.. Standard errors are clustered by state. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Dependent variable: $\Delta\text{RDF}_{s,t}/\text{Act}^{E}_{s,t-1}$		
	(1)	(2)	(3)
$\text{Surplus}_{s,t}$	0.14		
	(0.08)		
$\text{Surplus}_{s,t} \times 1(\text{Surplus}_{s,t} > 0)$		0.06^{**}	0.06^{**}
		(0.03)	(0.03)
$\text{Surplus}_{s,t} \times 1(\text{Surplus}_{s,t} < 0)$		0.27	0.26
		(0.19)	(0.19)
State GDP growth			0.12
			(0.08)
State + FYear FE	✓	✓	✓
GDP control			✓
Obs.	873	873	873
Adj.R^2	0.126	0.150	0.152

C Framework Predictions

This appendix records the forecast-incorporation predictions used in Section 2. It follows the framework section in the main text and precedes the empirical construction of the shift-share revenue-news measure.

Table C.1: **Forecast-Incorporation Patterns Under Alternative Models**

The table conditions on incrementally informative news, $\beta_w^{\text{actual}} \neq 0$. Budget loadings refer to the response of the original-budget revenue forecast; forecast-error loadings refer to the response of $R^{\text{actual}} - \widehat{R}^{\text{budget}}$. "Off-window" refers to pre-decision and deliberation windows outside the budget decision window. The sticky-information row assumes that the final update occurs at the proposal. Decision-dependent attention and the decision-window hypothesis are observationally equivalent in these loadings.

Model	Budget loading	Forecast-error loading
Full information	$\beta_w^{\text{budget}} \simeq \beta_w^{\text{actual}}$	$\beta_w^{\text{fe}} \simeq 0$
Sticky information	$\beta_{\text{Pre}}^{\text{budget}} \simeq \beta_{\text{Pre}}^{\text{actual}}, \quad \beta_{\text{Delib}}^{\text{budget}} \simeq 0$	$\beta_{\text{Pre}}^{\text{fe}} \simeq 0, \quad \beta_{\text{Delib}}^{\text{fe}} \simeq \beta_{\text{Delib}}^{\text{actual}}$
Imperfect memory / recency	$\beta_{\text{Pre}}^{\text{budget}} < \beta_{\text{Pre}}^{\text{actual}}, \quad \beta_{\text{Delib}}^{\text{budget}} \simeq \beta_{\text{Delib}}^{\text{actual}}$	$\beta_{\text{Pre}}^{\text{fe}} > 0, \quad \beta_{\text{Delib}}^{\text{fe}} \simeq 0$
Rational inattention, time-invariant costs	$0 \leq \beta_w^{\text{budget}} < \beta_w^{\text{actual}}$ for costly signals; no calendar discontinuity	$\beta_w^{\text{fe}} = \beta_w^{\text{actual}} - \beta_w^{\text{budget}} > 0$; no calendar discontinuity
Overreaction / diagnostic expectations	$\beta_w^{\text{budget}} > \beta_w^{\text{actual}}$ for recent or salient news	$\beta_w^{\text{fe}} < 0$ for recent or salient news
Decision window: two frictions	$\beta_{\text{Dec}}^{\text{budget}} \simeq \beta_{\text{Dec}}^{\text{actual}}, \quad \beta_{\text{Pre}}^{\text{budget}}, \beta_{\text{Delib}}^{\text{budget}} \simeq 0$	$\beta_{\text{Dec}}^{\text{fe}} \simeq 0, \quad \beta_{\text{Pre}}^{\text{fe}}, \beta_{\text{Delib}}^{\text{fe}} > 0$

Fiscal feedback to the revenue base. The framework outlined in the main text takes revenue expectations as a function of news, and not the government's actions. A natural concern is that realized revenue may itself respond to fiscal policy: if decision-window news raises spending, and spending raises the tax base, then realized revenue is not a pure measure of the news' fundamental revenue content. This appendix shows why that concern affects the interpretation of realized-revenue loadings but not the core forecast-error test.

Let realized revenue contain an autonomous component and a fiscal-feedback component,

$$R_{s,t}^{\text{actual}} = R_{s,t}^{0} + \kappa G_{s,t}^{\text{actual}}, \qquad \kappa \in [0, 1),$$

where $R_{s,t}^{0}$ is the revenue base absent same-year state-spending feedback and κ is the within-

year own-revenue recovered per dollar of state spending. Let $\widehat{R}^0_{s,t} \equiv \mathbb{E}[R^0_{s,t} \mid \mathcal{A}^{\text{budget}}_{s,t}]$. If the original-budget revenue forecast internalizes the effects of the enacted spending plan, then

$$R^{\text{budget}}_{s,t} = \widehat{R}^0_{s,t} + \kappa G^{\text{budget}}_{s,t}.$$

In the benchmark case in which the enacted spending plan is realized, $G^{\text{actual}}_{s,t} = G^{\text{budget}}_{s,t}$, the feedback term cancels from the forecast error:

$$R^{\text{actual}}_{s,t} - R^{\text{budget}}_{s,t} = R^0_{s,t} - \widehat{R}^0_{s,t}.$$

Thus forecast-error predictability continues to measure information omitted from the budget forecast.

Fiscal feedback does change the interpretation of realized-revenue loadings. For news in window w,

$$\frac{\partial R^{\text{actual}}_{s,t}}{\partial Z_{s,t,w}} = \frac{\partial R^0_{s,t}}{\partial Z_{s,t,w}} + \kappa \frac{\partial G^{\text{actual}}_{s,t}}{\partial Z_{s,t,w}}.$$

The realized-revenue response therefore combines fundamental revenue content with any fiscal response generated by the budget. A larger realized-revenue loading for decision-window news need not mean that decision-window news is intrinsically more informative; it may partly reflect the fact that only decision-window news changes current spending. This is why the empirical test emphasizes the decomposition between the original-budget forecast and the forecast error, rather than equality of realized-revenue coefficients across windows.

If official forecasts do not incorporate the feedback from the spending plan, then the cancellation is no longer exact. In that case, decision-window spending raises realized revenue without being fully embedded in the forecast, causing decision-window news to predict positive forecast errors. This attenuates the decision-window forecast-error contrast toward zero and would work against us.

D Shift-Share Measure: Construction Details

This appendix provides the implementation details needed to reproduce the shift-share news measure: the national source series paired to each revenue category, the construction of monthly innovations, the predetermined state exposure weights, and the scaling that puts category shocks into common revenue-relevance units. We avoid restating the identification argument here; the purpose of this appendix is measurement.

D.1 Revenue Categories and National Sources

Table D.1 reports the exact national source series used in the baseline shift-share measure. The five categories are the own-source revenue bases that both vary substantially across states and have clear national tax-base proxies. Intergovernmental transfers are excluded because they are driven primarily by federal policy rather than by state tax-base exposure; property taxes are excluded because the national house-price proxy has little aggregate predictive content for state property-tax revenues.

Table D.1: **Baseline Revenue Categories and National Source Series**

Revenue category	National source series	FRED / NIPA series
Personal income tax	Personal income	PI
General sales tax	Personal consumption expenditures	PCE
Charges and misc. revenue	PCE services	PCES
Corporate income tax	Industrial production	INDPRO
Severance tax	WTI crude oil price	WTISPLC

The table lists the five own-source revenue categories used to construct the baseline state-specific shift-share news measure $\text{News}_{s,t,m}$ together with the national source series paired to each category. Source series are pulled from FRED / NIPA at monthly frequency where available and converted to monthly log changes, then residualized on a series-specific AR(4) and standardized as described in Section 3.2.

Let $x_{k,m}$ denote that source series in month m. We construct the category-level innovation directly from this paired source: convert the series to monthly frequency where needed, take monthly log changes, residualize those changes on four monthly lags of the same series, and standardize the residuals to unit variance. Denoting the raw monthly change by $\Delta x_{k,m}$,

the monthly innovation for revenue category k is

$$z_{k,m} = \frac{\hat{u}_{k,m} - \overline{\hat{u}}_k}{\mathrm{sd}(\hat{u}_k)}, \qquad \hat{u}_{k,m} \text{ from } \Delta x_{k,m} = \alpha_k + \sum_{r=1}^{4} \rho_{kr} \Delta x_{k,m-r} + \hat{u}_{k,m}, \tag{D.1}$$

Thus the baseline innovation captures news relative to recent national dynamics, not the predictable AR component of the national tax base. We do not partial out calendar-month dummies in the production shock. Instead, because proposal dates cluster in a few calendar months, Appendix Table E.14 directly tests whether exposure-specific calendar seasonality explains the decision-window premium. The filtered series are shown in Figure D.1.

D.2 Exposure Weights and Elasticity Scaling

State exposure to revenue category k is measured using a five-year trailing average ending in fiscal year $t-2$:

$$\omega_{s,k,t} = \frac{1}{5} \sum_{\tau=t-6}^{t-2} \frac{\text{Revenue}_{s,k,\tau}}{\text{General Revenue}_{s,\tau}}. \tag{D.2}$$

Three implementation choices are important. First, the denominator is total general revenue, not the sum of the included categories. Thus the included exposure weights are not renormalized to sum to one; states with large transfer shares have mechanically lower exposure to own-source revenue news. Second, the $t-2$ endpoint makes the weights predetermined with respect to the monthly news entering the fiscal-year-t budget cycle. Third, cross-state heterogeneity in the shift-share measure comes from these exposure shares, while the monthly source innovations are common national shocks. Figure D.3 shows the broad-category revenue composition underlying this variation.

The standardized source innovations have common variance but different fiscal content. To put categories into common predicted-revenue units, we first aggregate the category-level monthly innovations into fiscal-year category shocks,

$$\text{Shock}^{US}_{k,\tau} = \sum_{m \in FY_\tau} z_{k,m}. \tag{D.3}$$

We then estimate a category-specific scaling coefficient from aggregate revenue data over the full sample of fiscal years:

$$\Delta \log \text{Rev}^{US}_{k,\tau} = \eta_k \, \text{Shock}^{US}_{k,\tau} + u_{k,\tau}. \tag{D.4}$$

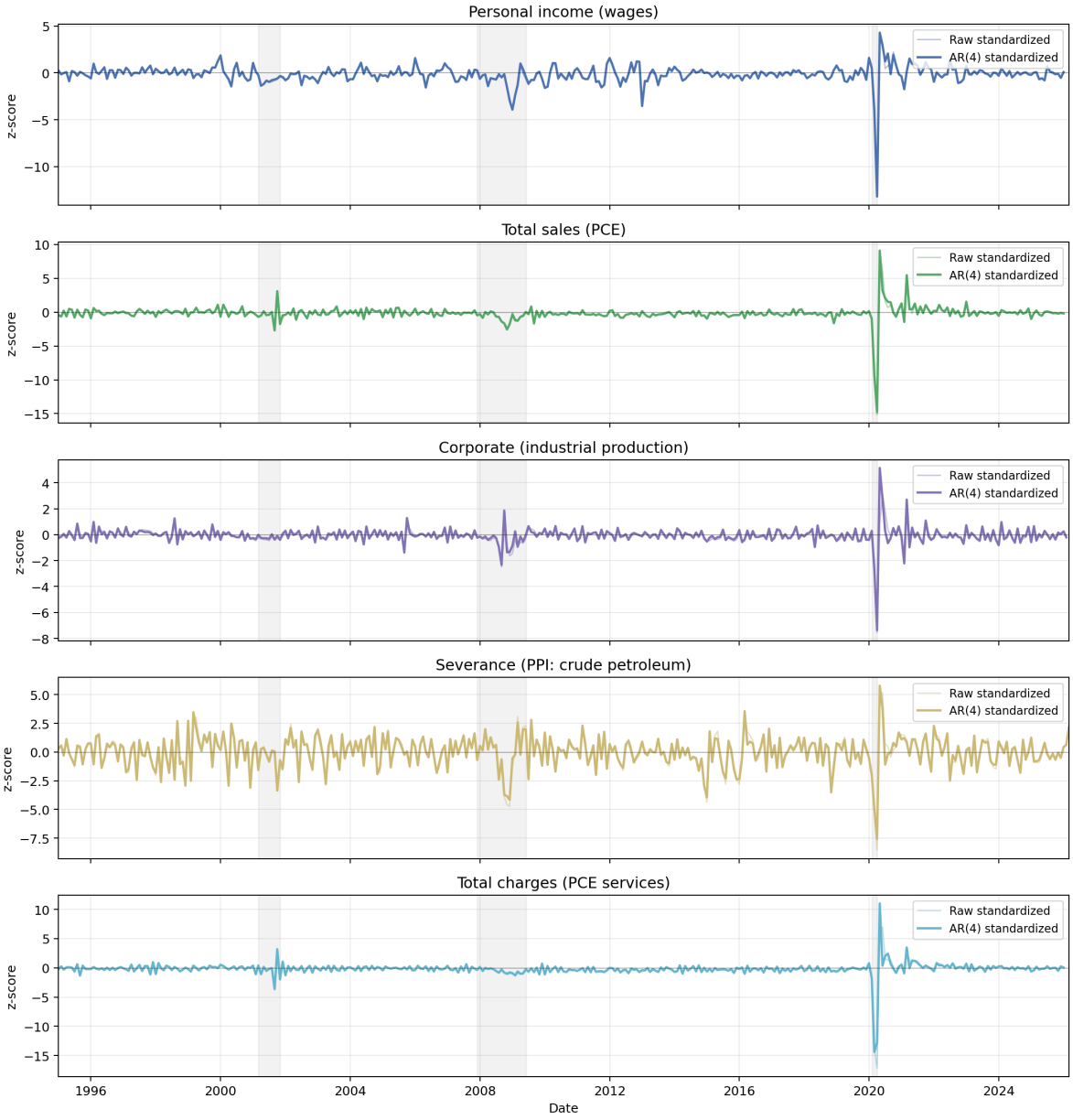


Figure D.1: **Standardized Monthly Source Innovations.** Each panel plots the raw monthly source change and the corresponding standardized innovation residualized on four own lags. The figure illustrates the source-level normalization step that makes innovations comparable in dispersion before category aggregation.

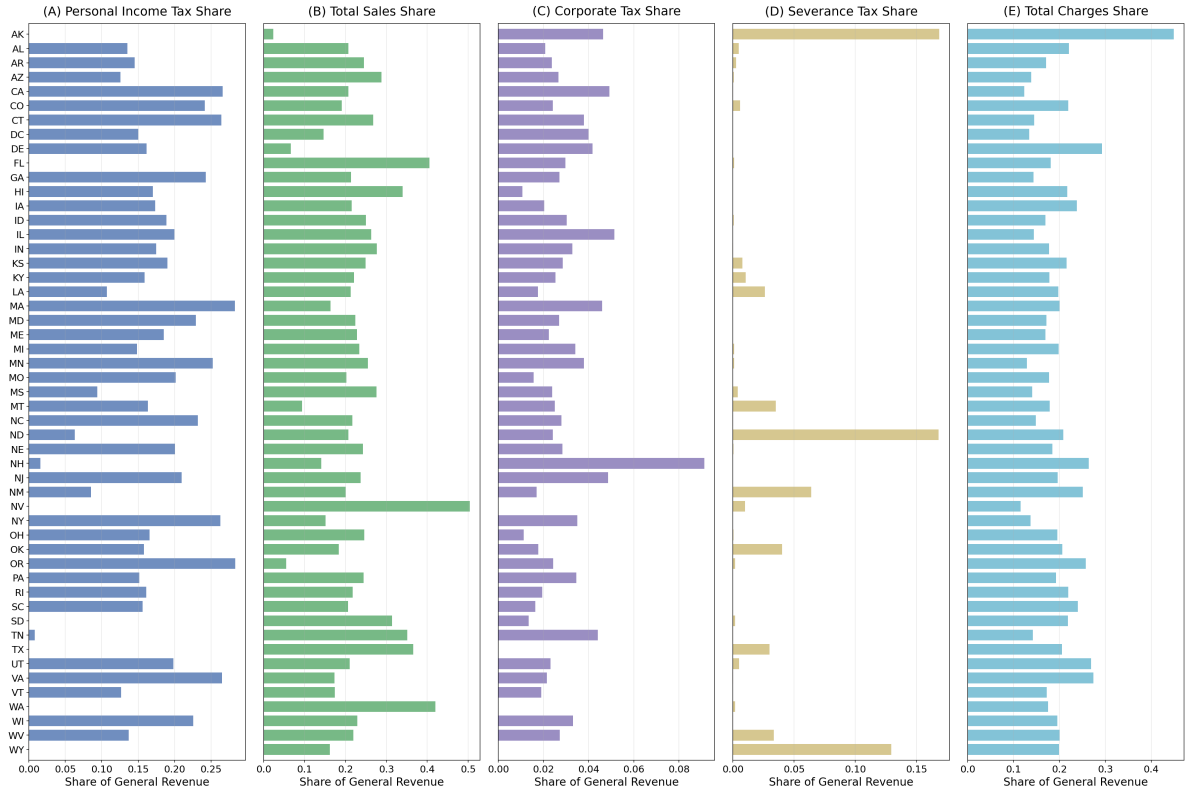


Figure D.2: **Cross-State Variation in Revenue Composition.** Each panel displays the average share of a revenue source in total general revenue for each state. Panel (A): individual income tax; Panel (B): general sales tax; Panel (C): charges and miscellaneous revenue; Panel (D): corporate income tax; Panel (E): severance tax. States are sorted alphabetically in all panels. Data source: Census Bureau Annual Surveys of State and Local Government Finance.

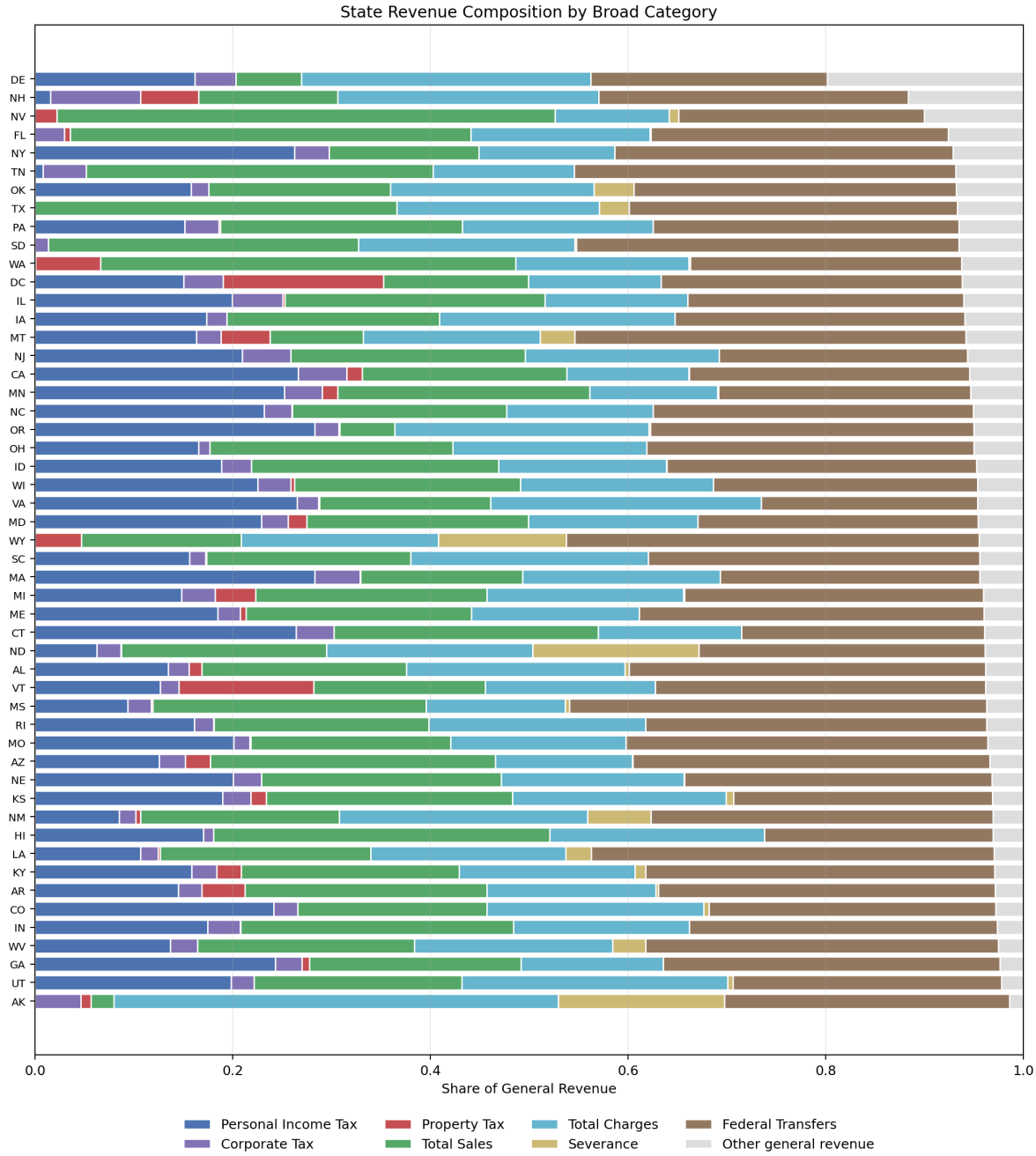


Figure D.3: **State Revenue Composition by Broad Category.** Each row shows one state's average share of general revenue across broad revenue categories over fiscal years 1995–2024. States are sorted by the residual "other general revenue" share. The five own-source categories entering the baseline shift-share (personal income, sales, corporate, severance, and total charges and miscellaneous) together average roughly 62% of general revenue. Property tax is displayed for descriptive completeness but is not used in the Bartik instrument because its aggregate R^2 against the Case-Shiller national HPI is near zero. Data source: Census Bureau Annual Surveys of State and Local Government Finance.

The fitted slope $\hat{\eta}_k$ is a single national, category-level constant, applied uniformly across states and years. The scaling is estimated on aggregate category-specific revenue growth intentionally: state-level heterogeneity is already captured by $\omega_{s,k,t}$, while $\hat{\eta}_k$ only changes the relative units of the national source innovations before they are interacted with state exposure.

Figure D.4 reports the fitted coefficients $\hat{\eta}_k$ and the explanatory power of the category-level scaling regressions. All five coefficients are positive; severance and corporate income carry the largest $\hat{\eta}_k$ (their revenues are the most sensitive per unit shock), while sales, severance, and charges are the most predictable in R^2 terms. Personal income and corporate income have lower R^2s, reflecting the roles of discretionary tax-policy changes and the volatility of profits in those bases.

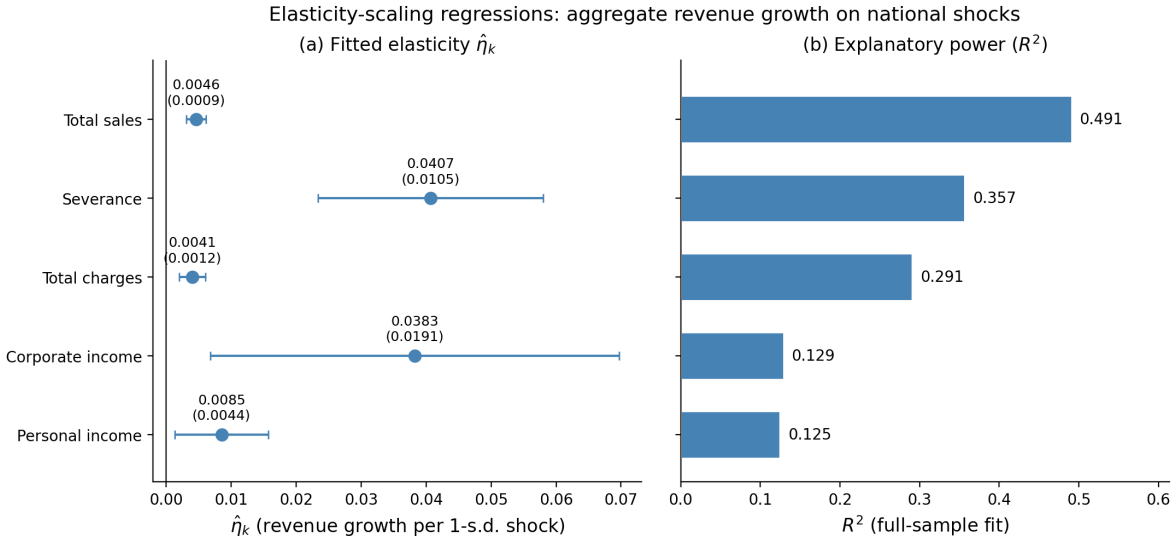


Figure D.4: **Elasticity-Scaling Regressions: Coefficients and Explanatory Power.** Both panels summarize the univariate category-level regressions of aggregate national revenue growth on the corresponding fiscal-year national tax-base shock (equation 9), estimated over the full sample of fiscal years. Panel (a) reports the fitted scaling coefficient $\hat{\eta}_k$ for each revenue category—the predicted category-revenue growth per one-standard-deviation shock—with 90% confidence intervals (horizontal whiskers) and the point estimate and its standard error printed above each marker. All five intervals exclude zero. Panel (b) reports the R^2 of the same regressions. Categories are ordered by R^2 in both panels. Shocks are AR(4)-residualized and z-scored at monthly frequency, then summed to fiscal-year frequency before estimation.

Combining standardized monthly innovations, predetermined state exposure weights, and category-specific scaling coefficients gives the monthly state-specific news measure used

in Section 3.2:

$$\text{News}_{s,t,m} = \sum_{k=1}^{K} \hat{\eta}_k \, \omega_{s,k,t} \, z_{k,m}. \tag{D.5}$$

This is the same object as equation (7) in the main text. The proposal-relative windows used in the regressions do not create new shock objects; they classify this same monthly news series by each month's distance from the state's proposal date.

Table D.2: **Monthly Shift-Share News by Budget Window, Excluding COVID**

The table reproduces Table 1, Panel B, the distribution of the monthly state-specific news $\text{News}_{s,t,m}$ ($\times 100$) by budget window, excluding calendar years 2020–2021. The exclusion is by calendar rather than fiscal year because the stacked monthly panel assigns a single calendar-month shock to several fiscal-year cohorts at different event times, so dropping COVID fiscal years would leave the spring-2020 innovations in adjacent cohorts' pre-decision and deliberation windows. Sample and construction are otherwise identical to Table 1.

	Mean	SD	P5	P25	Median	P75	P95
News Pre-Decision.	-0.05	0.23	-0.31	-0.13	-0.06	0.02	0.21
News Decision	-0.08	0.26	-0.49	-0.14	-0.07	0.02	0.22
News Deliberation	-0.06	0.25	-0.34	-0.13	-0.05	0.02	0.24
News Execution	-0.06	0.26	-0.35	-0.13	-0.05	0.03	0.24

E Shift-Share Results: Diagnostics and Robustness

This appendix collects the results-side material for the shift-share analysis: diagnostics supporting the design and robustness of the decision-window premium. Construction details are in Appendix D.

E.1 Shift-Share Validity

We first validate whether the shift-share measure captures information about contemporaneous realized state revenue growth.

Table E.1: **Validation: Shift-Share Measure Predicts Realized State Revenues**

The table reports OLS regressions of realized fiscal-year revenue growth, $\log(\text{Act}^R_{s,t}/\text{Act}^R_{s,t-1})$, on the fiscal-year-aggregated shift-share news measure $\text{News}^{FY}_{s,t}$, defined as the sum of the monthly state-specific news $\text{News}_{s,t,m}$ over the fiscal year. The sample is an annual state-fiscal-year panel covering 47 annual-budget jurisdictions and fiscal years 2002–2024. $\text{News}_{s,t,m}$ is constructed in Section 3.2. Macro controls are four quarterly lag windows of CFNAI, payroll employment, S&P 500, five-year Treasury, and CPI. The aggregate-share control of Borusyak et al. (2022) is the unit-level sum of exposure shares across the five tax categories. Standard errors are clustered by state. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Realized revenue growth: $\log(\text{Act}^R_{s,t}/\text{Act}^R_{s,t-1})$		
	(1)	(2)	(3)
$\text{News}^{FY}_{s,t}$	1.37***	1.56***	1.35
	(0.32)	(0.36)	(1.25)
State FE		✓	✓
Fiscal-Year FE			✓
Macro Controls		✓	✓
Aggregate-share control (BHJ)		✓	✓
Observations	956	936	936
Adj. R^2	0.066	0.103	0.129

Relevance and variance decomposition. Table E.1 regresses realized fiscal-year revenue growth, $\log(\text{Act}^R_{s,t}/\text{Act}^R_{s,t-1})$, on the fiscal-year news measure $\text{News}^{FY}_{s,t} \equiv \sum_{m \in FY_t} \text{News}_{s,t,m}$. Column (1) is the raw correlation; column (2) adds state fixed effects, lagged macro controls, and the Borusyak et al. (2022) aggregate-share control; column (3) further adds fiscal-year fixed effects. The point estimate is positive, large, and stable across columns: $\hat{\beta} = 1.56$ in column (2) and $\hat{\beta} = 1.35$ in column (3). The slope is similar under the more demanding

identification; the main change is precision. The column (3) standard error is roughly three and a half times larger, reducing the t-statistic from 4.3 to 1.1.

The mechanical reason is that the underlying Bartik shifters are national shocks, so most of the news measure's variation is common across states. A variance decomposition of $\text{News}_{s,t}^{FY}$ on the estimation sample confirms this: state fixed effects explain only 1.2% of its variance, fiscal-year fixed effects explain 81.7% (the common national component), and the remaining 17.4% is the within-state-year residual. The between-state component is absorbed by state fixed effects in both columns (2) and (3); the common-time component is absorbed by fiscal-year fixed effects in column (3) but used to identify column (2). Column (3) is therefore identified from the 17.4% within-state-year residual under the shock-exogeneity assumption of Borusyak et al. (2022). This residual is the same variation that identifies window-specific effects in the monthly panel of Section 3, where calendar-month-by-year fixed effects play the role of fiscal-year fixed effects.

E.2 Validity of the Budget-Calendar Assignment

The empirical design assigns common national innovations to different budget windows using predetermined state proposal calendars. This interpretation requires that proposal timing not respond to the same contemporaneous news. We evaluate these conditions using the persistence of reported proposal months, a fixed-calendar reconstruction, proposal-versus-enactment timing, and a pseudo-window placebo.

Persistence and fixed-calendar assignment. The baseline assigns each fiscal year the most recent NASBO calendar observed at or before that year. Reported proposal months are highly persistent within state: 40 of 52 jurisdictions never change their reported proposal month across the NASBO waves covering the sample, state fixed effects absorb 93 percent of the panel variation in proposal months, and Appendix A.1, Table A.2, documents the twelve jurisdictions whose reported months change. As a measurement check, Table E.2 fixes every jurisdiction's proposal month at its most recent 2021 NASBO value for all sample years and reconstructs event time. This re-dates seven jurisdictions, typically by one month. The resulting forecast decision-window loading is 2.70 (standard error 0.63); the pre-decision and deliberation loadings remain small. The estimated timing pattern is therefore insensitive to the NASBO vintage used to assign proposal months.

Table E.2: Fixed 2021 NASBO Budget Calendar

The table re-estimates the forecast and forecast-error columns of Table 2, Panel A, holding each jurisdiction's proposal month fixed at its most recent (2021) NASBO survey wave for all sample years, instead of the baseline's piecewise-constant assignment of the most recent wave observed at or before each fiscal year. Relative event time and the three windows are re-dated from the fixed proposal month using the same conventions as the baseline construction: proposal months September–December are assigned to calendar year $t-2$, other months to $t-1$; the decision window is $[-5,-1]$, the pre-decision window is $[-11,-6]$, and the deliberation window runs from the proposal month to the fiscal-year start. The fixed calendar re-dates seven jurisdictions (FL, MT, NC, NJ, PR, RI, and TN), typically by one month; the final regression sample has 15,040 observations, two fewer than the 15,042-observation baseline. The news measure is unaffected by the calendar. Outcomes, fixed effects, controls, common-sample restriction, and clustering are identical to Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	(1) Forecast	(2) Forecast error
$\text{News}^{\text{Pre}}_{s,t,m}$	-0.10	1.20***
	(0.37)	(0.39)
$\text{News}^{\text{Decision}}_{s,t,m}$	2.70***	-0.30
	(0.63)	(0.66)
$\text{News}^{\text{Delib}}_{s,t,m}$	0.40	0.69**
	(0.62)	(0.29)
State FE	✓	✓
FYear × Year-Month FE	✓	✓
Controls	✓	✓
Observations	15,040	15,040
Adj. R^2	0.115	0.628

Proposal versus enactment timing. The baseline calendar is indexed to the governor's proposal because this is the first observable date by which the original budget has been assembled. As a complementary timing diagnostic, Figure E.1 re-indexes the monthly profile around legislative enactment. This exercise asks whether forecast updating is concentrated near the initial formulation of the budget rather than mechanically around its later enactment.

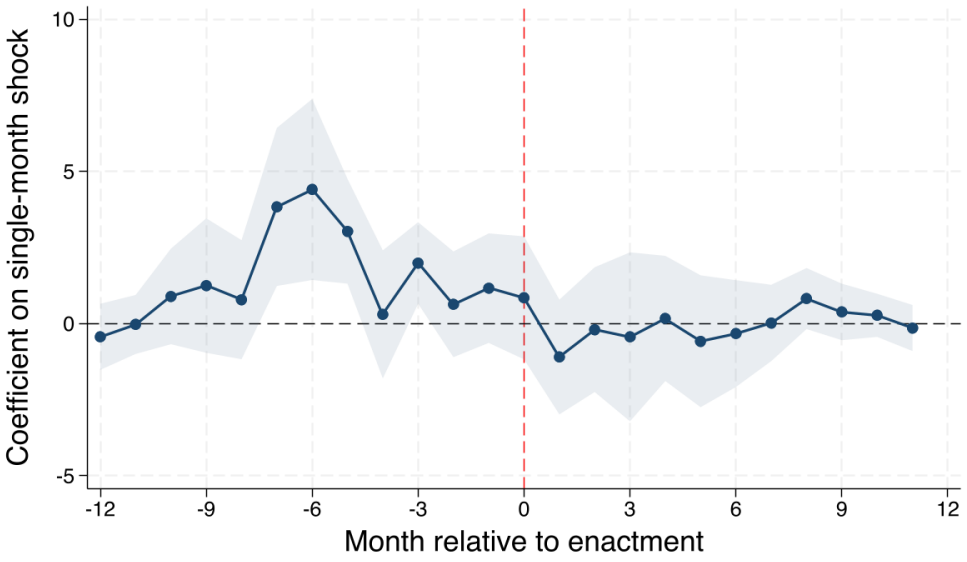


Figure E.1: **Monthly Profile of Revenue Forecast Updating Around the Enactment Date.** The figure plots coefficients from the same stacked monthly specification as Figure 4, but indexes event time relative to the legislative enactment date rather than the proposal date. The dependent variable is one-year original-budget revenue forecast growth. The specification includes state fixed effects, calendar-month-by-year fixed effects, lagged macro controls, and the aggregate-share control. Shaded areas denote 95 percent confidence intervals based on state-clustered standard errors.

Calendar-specificity placebo. The decision-window premium in Table 2 is identified from where a national innovation falls in the state's budget calendar. A potential concern is that the premium is a mechanical feature of the stacked monthly panel, in which case any five-month window would produce a similar result. We rule this out with a pseudo-window placebo. Holding the estimation sample and the news series fixed, we relocate each state–fiscal-year's five-month "decision window" to a random block drawn uniformly from positions that are disjoint from the true window $\ell \in [-5, -1]$: a pre-decision block starting at $\ell = -11$ or -10 ($[-11, -7]$ or $[-10, -6]$), or a deliberation block starting at $\ell = 0, 1, 2$ ($[0, 4]$, $[1, 5]$, or $[2, 6]$). Draws are independent across state–fiscal-years. For each of 500

draws we re-estimate our in/out specification of Table 2—the news level plus its interaction with the relocated window—and record the estimated premium. Because the true decision window now falls inside the pooled news level, a correctly specified design should return a placebo premium near zero.

Figure E.2 plots the distribution of the 500 placebo premiums. They are centered at -0.07 with a standard deviation of 0.26, and none reaches the true premium of 2.53. The slight negative centering reflects that an off-window block looks marginally weaker than the sample average since the true decision window is pooled into the baseline level. The observed premium lies far outside the distribution obtained from placebo five-month blocks, indicating that the premium is specific to the actual decision-window assignment rather than a mechanical feature of the stacked panel. This exercise does not, by itself, identify decision-dependent attention as the unique mechanism.

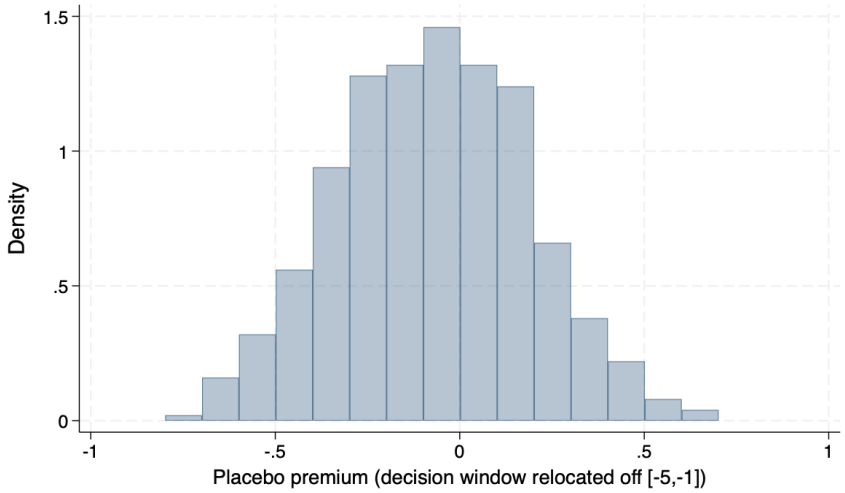


Figure E.2: **Pseudo-Window Placebo: Relocated Decision Windows Carry No Premium.** Distribution of the estimated decision-window premium across 500 placebo draws that relocate each state–fiscal-year's five-month window to a random block disjoint from the true window $\ell \in [-5, -1]$ (a pre-decision or deliberation block), holding the estimation sample and news series fixed and re-estimating the same in/out specification as Table 2. The red line marks the true premium (2.53). Placebo premiums center near zero and never reach the true value (permutation $p < 0.002$), indicating that the premium reflects the true budget calendar rather than a mechanical feature of the stacked panel.

E.3 Discussion of Alternative Stories

This subsection considers whether the concentration of forecast updating in the decision window can instead be explained by standard information frictions, recency, shock salience, or contemporaneous tax-policy responses. Appendix Table C.1 summarizes stylized predictions from sticky information (Mankiw and Reis, 2002), rational inattention with time-invariant information costs (Sims, 2003; Maćkowiak and Wiederholt, 2009), and overreaction or diagnostic expectations (Bordalo et al., 2018). These comparisons are diagnostic benchmarks, not structural tests of the underlying models.

The window-level results in Table 2 reject the simplest single-friction alternatives. Pre-decision news is available before the forecast is updated but is not caught up, contrary to standard sticky information. Deliberation news is the newest pre-fiscal-year information but is not overweighted, contrary to simple recency or diagnostic expectations. These findings are difficult to reconcile with a simple age-only account under which all available pre-proposal information is incorporated at the proposal date, but they do not rule out richer versions of sticky information, rational inattention, or diagnostic expectations.

The exercises below ask whether incorporation varies smoothly with news age within the decision window, whether unusually large shocks receive greater attention, and whether the revenue response instead reflects contemporaneous changes in state top individual-income-tax rates. Together, they distinguish the calendar pattern from these simpler alternatives without claiming to identify decision-dependent attention as the unique mechanism.

Decision window splits. Table E.3 splits the baseline window into early ($\ell \in [-5, -3]$) and late ($\ell \in [-2, -1]$) halves. The two forecast loadings are statistically indistinguishable ($p = 0.442$). We therefore detect no systematic gradient between the early and late portions of the decision window. This evidence is consistent with a window-like response.

Shock magnitude and salience-based attention. A potential alternative explanation for the decision-window pattern is that shocks arriving during this window are unusually large or salient, inducing a stronger forecast response independently of the budget calendar. We test for both parts of this explanation. First, Table 1, Panel B, shows that decision-window news is less dispersed than pre-decision or deliberation news in the full sample. Appendix Table D.2 removes the pandemic innovations and finds similar dispersion across the three windows: the standard deviations are 0.23, 0.26, and 0.25 for the pre-decision,

Table E.3: **Early versus Late Decision-Window News**

The table re-estimates Table 2, Panel A, splitting the decision window into an early half ($\ell \in [-5, -3]$) and a late half ($\ell \in [-2, -1]$), with the pre-decision ($\ell \in [-11, -6]$) and deliberation ($\ell \geq 0$ before fiscal-year start) terms unchanged. The "p: early = late" row reports the p-value of the Wald test that the two half-window loadings are equal. Outcomes, sample, fixed effects, controls, and clustering are identical to Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$	Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$	Realized $\log(\text{Act}_t^R/\text{Budget}_{t-1}^R)$
	(1)	(2)	(3)
$\text{News}_{s,t,m}^{\text{Pre}}$	-0.12	1.20***	1.08*
	(0.37)	(0.39)	(0.55)
$\text{News}_{s,t,m}^{\text{Decision},[-5,-3]}$	2.22***	-0.65	1.57*
	(0.68)	(0.88)	(0.93)
$\text{News}_{s,t,m}^{\text{Decision},[-2,-1]}$	3.13***	-0.11	3.01*
	(0.98)	(0.97)	(1.61)
$\text{News}_{s,t,m}^{\text{Delib}}$	0.43	0.71**	1.14**
	(0.63)	(0.31)	(0.48)
p: early = late	0.442	0.684	0.492
State FE	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓
Controls	✓	✓	✓
Observations	15,042	15,042	15,042
Adj. R^2	0.115	0.628	0.538

decision, and deliberation windows, respectively. Decision-window shocks do not appear to be systematically larger than off-window shocks.

Second, we test if larger shocks cause more significant responses in Table E.4. The decision-window interaction with the absolute magnitude of the shock for the revenue forecast is positive but statistically weak. The corresponding pre-decision and deliberation interactions are also small and statistically insignificant. These results indicate that the forecast response is not generated simply by larger shocks occurring during the decision window. However, in the forecast error regression, the interaction coefficient of decision window news and the size of the shock is negative and statistically significant, suggesting that unusually large decision-window shocks are more fully incorporated and thus lead to forecast errors closer to zero. The interaction coefficients for forecast error regression off-decision window are also negative but not statistically distinguishable from zero. This result suggests that the size of the shock may affect how much officials incorporate the news into their budget forecast.

Tax-policy responses to revenue news. A potential alternative interpretation is that our shocks spur changes in tax policy, so the revenue forecasts we observe would contain not only response to the shocks themselves but also the impact of policy changes. We test this using Tax Foundation data on state individual-income-tax top marginal rates over 2000–2014. For each state-year, we construct indicators for any rate change, an increase, and a decrease, in this tax rate. We regress these dummy variables on pre-decision, decision-window, and deliberation news with state and fiscal-year fixed effects, lagged macro controls, and state-clustered standard errors.

Our results are in Table E.5, which shows no evidence that news arriving in any budget window predicts changes in the top individual-income-tax rate. This finding reduces concern that contemporaneous changes in personal-income-tax rates drive the estimated revenue-forecast response. Note however that the data cover only top marginal income-tax rates from 2000–2014, so does not rule out every form of discretionary revenue policy.

E.4 Institutional Evidence from Biennial Budgeting

Many states follow some form of biennial budgeting, creating useful institutional variation. In the first year of a biennium, there is an actual proposal and budget decision during the

Table E.4: **Incorporation and Shock Size**

The table re-estimates Table 2, Panel A, adding for each window an interaction of the news term with the absolute monthly news magnitude $|\text{News}_{s,t,m}|$, standardized to unit standard deviation and centered at its mean over the estimation sample. Each window's base loading is thus the response at an average-magnitude shock, and the "$\times$ |News| (SD)" row is the change in that loading per one-standard-deviation-larger shock; a coefficient near zero means incorporation does not vary with shock size. Column 1 is the original-budget revenue forecast, column 2 the forecast error. Sample, fixed effects, controls, and clustering are identical to Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	(1) Forecast	(2) Forecast error
$\text{News}^{\text{Pre}}_{s,t,m}$	0.24	2.44*
	(0.70)	(1.22)
$\times$ |News| (SD)	-0.06	-0.17
	(0.08)	(0.16)
$\text{News}^{\text{Decision}}_{s,t,m}$	1.71*	1.97
	(0.95)	(1.65)
$\times$ |News| (SD)	0.37*	-0.84**
	(0.22)	(0.36)
$\text{News}^{\text{Delib}}_{s,t,m}$	-0.27	0.91
	(0.97)	(0.76)
$\times$ |News| (SD)	0.09	-0.02
	(0.17)	(0.10)
State FE	✓	✓
FYear $\times$ Year-Month FE	✓	✓
Controls	✓	✓
Observations	15,042	15,042
Adj. R^2	0.115	0.629

Table E.5: **Revenue News and Changes in State Top Income-Tax Rates**

The dependent variable is an indicator equal to one if the state's individual income tax top marginal rate changed (column 1), increased (column 2), or decreased (column 3) in fiscal year t. The regressors are the fiscal-year aggregates of the monthly news over the decision window, $\text{News}_{s,t}^{\text{Decision}}$, pre-decision window, $\text{News}_{s,t}^{\text{Pre}}$, and deliberation window, $\text{News}_{s,t}^{\text{Delib}}$. Specifications include state and fiscal-year fixed effects and standard macro controls (four quarterly lag windows of CFNAI, payroll employment, the S&P 500, the five-year Treasury yield, and CPI). The sample is restricted to state-years with Tax Foundation top-rate data, 2000–2014. The test covers only top individual-income-tax rates and does not measure other rate, base, credit, fee, or revenue-policy changes. Standard errors are clustered by state. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	(1) Any change	(2) Increase	(3) Decrease
$\text{News}_{s,t}^{\text{Decision}}$	4.41	-0.17	4.58
	(4.08)	(0.87)	(4.11)
$\text{News}_{s,t}^{\text{Pre}}$	-0.33	-0.50	0.17
	(3.00)	(1.26)	(2.65)
$\text{News}_{s,t}^{\text{Delib}}$	5.94	0.24	5.70
	(4.45)	(2.01)	(4.49)
State FE	✓	✓	✓
Fiscal-year FE	✓	✓	✓
Macro controls	✓	✓	✓
Observations	630	630	630
Adj. R^2	0.156	0.064	0.125

assigned decision window; in the second year, there may not be an actively revised budget during the assigned calendar decision window. We test if our forecast loading estimates are more pronounced in state-years where the assigned window contains an actual budget decision. Appendix Table A.1 reports the state budget and legislative cycles, and Appendix A describes their construction.

Actual versus imputed decision windows. Table E.6 first reproduces the baseline and then allows every window-news loading to differ in the second year of a biennium. To do this, we define a dummy that equals 1 if that state-fiscal-year is in the second year of a biennium, indicating there is a less formal process for revising the budget. For state-fiscal-year cohorts with an actual decision in the assigned window, the forecast loading is 2.83. The second-year interaction is -2.11 and significant at 5 percent, implying a second-year loading of 0.72 that is not statistically distinguishable from zero. The forecast-error specification has the opposite directional pattern: second-year decision-window news has an implied loading of 2.80 in the forecast error. This indicates that during what would be the decision window but is in the second year of that state's biennium, news is informative about actual revenues but does not enter the forecast.

In Columns (5)-(6), we test how our results change when we drop second-year biennium cohorts directly: decision window news has larger loadings on the forecast, and forecast errors for off-window news are larger, though these differences are not statistically significant. Finally, columns (7)-(8) test our baseline results if we relabel second year biennium cohorts as post-decision window (deliberation) news; the results are again similar. We interpret these results as consistent with our overall thesis of the importance of decision-window news.

E.5 Sources of Identification and Generality

This subsection examines where the identifying variation comes from and how broadly the timing pattern applies. We first use a Rotemberg decomposition to quantify category leverage, then use leave-one-state and leave-one-fiscal-year exercises to assess concentration in particular observations. We next compare oil and personal-income-tax news and estimate an ex-oil specification excluding the influential FY2010 cohort. Finally, we replace the constructed shift-share measure with general macroeconomic indices.

Table E.6: **Biennial Budgets: Second-Year Cohorts and the Decision Window**

The table re-estimates the forecast and forecast-error columns of Table 2, Panel A, under four treatments of biennial-budget states' second-year cohorts, whose imputed decision window contains no budget decision (the biennium covering the year was proposed and enacted roughly twelve months earlier). Biennial-budget state-years are NASBO budget-cycle code "B": CT, HI, IN, KY, ME, MN, MT, NC, NE, NH, NV, OH, TX, VA, and WI in the estimation sample. The first fiscal year of each biennium is even for odd-year enactors, which is every biennial sample state except Kentucky and Virginia (which enact in even years); the second-year indicator follows this parity. Columns (1)–(2) reproduce the baseline. Columns (3)–(4) add interactions of each window-news term with the second-year indicator, together with the indicator and its interactions with the three window indicators, so the news interactions are not driven by level differences. Columns (5)–(6) drop second-year cohorts from the sample. Columns (7)–(8) relabel all second-year pre-fiscal-year months as deliberation (post-proposal) months and re-estimate the baseline specification on the full sample. Cycle codings are assigned to fiscal years from the most recent NASBO survey wave at or before each year. Outcomes, window definitions, fixed effects, controls, common-sample restriction, and clustering are identical to Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Baseline		Off-window interaction		Excl. yr. 2		Yr. 2 as controls	
	(1) Forecast	(2) Forecast error	(3) Forecast	(4) Forecast error	(5) Forecast	(6) Forecast error	(7) Forecast	(8) Forecast error
$\text{News}^{\text{Pre}}_{s,t,m}$	-0.10	1.21***	-0.11	1.22***	-0.04	1.26***	0.01	1.12***
	(0.37)	(0.39)	(0.37)	(0.42)	(0.39)	(0.45)	(0.27)	(0.34)
× Biennial, yr2			-0.01	0.05				
			(0.47)	(0.61)				
$\text{News}^{\text{Decision}}_{s,t,m}$	2.74***	-0.34	2.83***	-0.47	2.89***	-0.38	2.66***	-0.50
	(0.61)	(0.68)	(0.58)	(0.73)	(0.56)	(0.68)	(0.59)	(0.71)
× Biennial, yr2			-2.11**	3.27**				
			(1.01)	(1.62)				
$\text{News}^{\text{Delib}}_{s,t,m}$	0.39	0.69**	0.40	0.73**	0.43	0.90***	0.33	0.83***
	(0.62)	(0.30)	(0.61)	(0.29)	(0.47)	(0.22)	(0.38)	(0.23)
× Biennial, yr2			-0.06	-0.43				
			(0.22)	(0.65)				
State FE	✓	✓	✓	✓	✓	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	15,042	15,042	15,042	15,042	12,277	12,277	15,042	15,042
Adj. R^2	0.115	0.628	0.115	0.629	0.127	0.642	0.115	0.629

E.5.1 Category Leverage and Revenue-Base Diagnostics

Rotemberg weights and leverage. Table E.7 provides an algebraic decomposition of the decision-window coefficient following Goldsmith-Pinkham et al. (2020): $\hat{\beta} = \sum_k \alpha_k \hat{\beta}_k$, where α_k measures category k's contribution to the covariance of residualized category news with the residualized composite measure, and $\hat{\beta}_k$ is the corresponding just-identified diagnostic coefficient.

Severance accounts for 0.63 of the identifying weight, with $\hat{\beta}_k = 3.92$. Personal income accounts for 0.25 of the weight, with $\hat{\beta}_k = 1.96$. Each remaining category carries about four percent of the identifying weight. On average, in the Census data, the five categories in our shock construction make up 62.2% of general revenues. The pooled coefficient is thus sensitive to severance variation. Our next tests evaluate how much our baseline results rely on severance states or revenues.

Table E.7: **Rotemberg Weights: Which Revenue Categories Drive Identification**

The table decomposes the pooled decision-window coefficient of Table 2, Panel A, into revenue-category components following Goldsmith-Pinkham et al. (2020): $\hat{\beta} = \sum_k \alpha_k \hat{\beta}_k$, where $\alpha_k = \text{cov}(\tilde{c}_k, \tilde{N})/\text{var}(\tilde{N})$ is category k's Rotemberg weight, $\hat{\beta}_k = \text{cov}(\tilde{c}_k, \tilde{y})/\text{cov}(\tilde{c}_k, \tilde{N})$ is the just-identified coefficient using only category k's contribution, $\tilde{c}_k$ is category k's decision-window news contribution ($\hat{\eta}_k \omega_{s,k,t} z_{k,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$), $\tilde{N}$ is the composite decision-window news, and $\tilde{y}$ is the outcome, all residualized on the other window terms, controls, and fixed effects of the baseline specification. Weights sum to one by construction, and the final row verifies that the weighted components reproduce the pooled coefficients. Mean revenue share is the unweighted mean of the predetermined five-year-average category share across the 884 effective estimation state-years; its denominator is Census general revenue. The parenthesized standard errors are from the just-identified IV regression of the residualized outcome on the residualized composite decision-window news, instrumented by category k's contribution. $\hat{\beta}_k$ for low-weight categories rests on little identifying variation and should be read with the corresponding standard errors. Sample and specification are identical to Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Revenue category	Mean revenue share	Rotemberg weight α_k	β_k: Forecast	β_k: Forecast error
Personal income	0.158	0.245	1.96* (1.01)	1.30 (1.31)
General sales	0.228	0.044	-1.49 (3.39)	1.36 (3.78)
Corporate income	0.028	0.043	-0.60 (2.19)	-0.47 (3.15)
Severance	0.010	0.633	3.92*** (0.50)	-0.86 (1.05)
Charges & misc.	0.199	0.034	-3.80** (1.87)	-4.59 (3.84)
$\sum_k \alpha_k \beta_k$ (pooled coefficient)	0.622	1.000	2.74	-0.34

How to interpret the different coefficients $\hat{\beta}_k$. The general sales, charges, and corporate income categories have negative $\hat{\beta}_k$. While this may seem counterintuitive, the estimates

do not indicate that these negatively load on revenue expectations. To see the basic intuition, consider a two-base portfolio with sales and oil revenues whose shares sum to one and whose two bases respond to the same national shock z_m. After common time effects absorb the aggregate shock, the coefficient on $z_m \times w_{s,\text{sales},t}$ identifies approximately $\beta_{\text{sales}} - \beta_{\text{oil}}$. The $\hat{\beta}_k$ estimate can therefore be negative even when both bases respond positively, if the oil revenues are more cyclical.

We report the correlation across the shifters and exposure shares of different categories in Table E.8. The sales and charges shifters have a correlation of 0.907, sales and corporate shifters have a correlation of 0.769, and corporate and charges shifters have a correlation of 0.681. Sales and charges shares correlate −0.517, while PIT and severance shares correlate −0.357. Thus the negative sales, corporate, and charges cells in Table E.7 should be interpreted as covariance-weighted differential-exposure estimates.

Table E.8: **Correlation Structure of Category Shifters and Exposure Shares**

Panel A reports correlations among the five national monthly category innovations over the 283 calendar months represented in the FY2002+ baseline estimation sample. Panel B reports correlations among the predetermined five-year-average category shares over the 884 state-years in that sample. Shares use Census general revenue as their denominator. The sample is defined by the revenue-forecast regression in Table 2, Panel A.

	PIT	Sales	Corporate	Severance	Charges
Panel A: National category shifters					
Personal income	1.000	-0.044	-0.039	-0.058	-0.049
General sales	-0.044	1.000	0.769	0.559	0.907
Corporate income	-0.039	0.769	1.000	0.413	0.681
Severance	-0.058	0.559	0.413	1.000	0.500
Charges & misc.	-0.049	0.907	0.681	0.500	1.000
Calendar months			283		
Panel B: Predetermined exposure shares					
Personal income	1.000	-0.284	0.023	-0.357	-0.271
General sales	-0.284	1.000	-0.349	-0.341	-0.517
Corporate income	0.023	-0.349	1.000	0.108	0.148
Severance	-0.357	-0.341	0.108	1.000	0.469
Charges & misc.	-0.271	-0.517	0.148	0.469	1.000
State-years			884		

E.5.2 Oil Dependence and Alternative Revenue Bases

Our category decomposition above shows that severance news makes up a significant component of the shock measure's identifying variation. Here, we explore whether our results survive a non-oil sample.

Influential oil-producing states and non-oil revenue news. We assess how much the baseline results depend on severance tax revenue. Panel A of Table E.9 reproduces the results from Panel B of Table 2 after removing the severance-tax component of the shock and excluding FY2010. The decision-window premium falls to 1.57 but remains statistically significant at the 5% level. The forecast-error premium remains negative.

Panel B focuses on the two largest contributors to aggregate revenue news: severance-tax and personal income tax (PIT) news. We include both shocks jointly to assess their own effect. In the full sample, the decision-window effects of both severance-tax news (3.46) and PIT news (2.15) are positive and statistically significant. Excluding FY2010 reduces the severance-tax estimate to 1.91, significant at the 5% level, but increases the PIT estimate to 2.98. This suggests that the severance-tax result is partly driven by the financial crisis, whereas the PIT result is not. Columns (3) and (4) exclude both severance-tax news and FY2010. PIT news alone continues to exhibit a decision-window effect when we exclude high-severance states (2.38) or all states with severance-tax revenue (8.14). Because the latter estimate is based on only 14 states, its magnitude should be interpreted cautiously. The forecast-error premiums are generally not statistically different from zero.

E.5.3 Share-Free Macroeconomic Timing Diagnostics

Next, we test if our baseline results hold using general macroeconomic indices, which are not sensitive to oil exposure or state revenue shares. Table E.10 reports our key coefficients using the aggregate monthly time series the Chicago Fed National Activity Index (CFNAI) and the Michigan consumer sentiment, as well as the Philadelphia Fed State Leading Index which varies by state and month. While the State Leading Index has the benefit of having state-month variation, its series was discontinued in 2020, so the sample is shorter. Because the two national series only vary in the time series, we estimate an interaction regression where each monthly shock is interacted with a decision-window indicator, so the interpretation is the effect of decision-window news relative to news arriving in the pooled pre-decision and

Table E.9: **Oil Dependence and Alternative Revenue Bases**

Panel A re-estimates the baseline five-category composite specification in columns (1) and (3) and the same composite with severance ("oil") news removed—"Ex-oil" in the shock row—in columns (2) and (4). Panel B splits the news into its oil and personal-income-tax ("PIT") components: both enter the same regression in columns (1)–(2) and (5)–(6), while columns (3)–(4) and (7)–(8) retain PIT news only, and blank cells indicate a regressor that is not in the specification. In the sample rows, "All" is the full FY2002–2024 window and all states; "Ex-2010" drops the FY2010 crisis cohort; "Ex-high-sev." also drops the six represented high-severance states (AK, NM, MT, WV, TX, and LA), fixed from predetermined mean severance shares in the effective estimation sample; and "Ex-sev." keeps only the states with no severance news. The simple Alaska and FY2010 omissions are already reported in the comprehensive leave-one-out figures and are not repeated here. Every column uses Table 2, Panel B's in/out reparameterization: the news level carries a common off-window loading across the pre-decision and deliberation months, and the reported decision interaction is the premium over it. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Composite-news leverage

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$		Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$	
	(1)	(2)	(3)	(4)
$\text{News}_{s,t,m}\times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	2.53***	1.57**	-1.22	-0.53
	(0.49)	(0.76)	(0.80)	(0.95)
Shock	Composite	Ex-oil	Composite	Ex-oil
Sample: fiscal years	All	Ex-2010	All	Ex-2010
Sample: states	All	All	All	All
State FE	✓	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	15,042	14,397	15,042	14,397
States	43	43	43	43
Adj. R^2	0.115	0.121	0.628	0.639

Panel B: Oil and personal-income-tax news

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$				Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Oil $\text{News}_{s,t,m}\times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	3.46***	1.91**			-2.36	0.47		
	(0.55)	(0.89)			(1.60)	(1.50)		
PIT $\text{News}_{s,t,m}\times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	2.15*	2.98***	2.38**	8.14***	0.47	-0.14	0.13	-1.78
	(1.07)	(0.96)	(0.93)	(2.21)	(1.45)	(1.19)	(1.16)	(2.71)
Shock(s)	Oil + PIT	Oil + PIT	PIT only	PIT only	Oil + PIT	Oil + PIT	PIT only	PIT only
Sample: fiscal years	All	Ex-2010	Ex-2010	Ex-2010	All	Ex-2010	Ex-2010	Ex-2010
Sample: states	All	All	Ex-high-sev.	Ex-sev.	All	All	Ex-high-sev.	Ex-sev.
State FE	✓	✓	✓	✓	✓	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	15,042	14,397	12,331	4,251	15,042	14,397	12,331	4,251
States	43	43	37	14	43	43	37	14
Adj. R^2	0.116	0.122	0.129	0.193	0.629	0.640	0.457	0.671

deliberation months.

Using the Michigan consumer sentiment index, the forecast premium is positive in both samples: 0.40 in the full sample and 0.43 excluding FY2010, significant at the 10 and 5 percent levels, respectively. The CFNAI forecast premium is also positive in both samples—0.93 in the full sample and 0.89 excluding FY2010—and significant at the 5 and 10 percent levels. The State Leading Index forecast premium is imprecise in the full sample but rises to 0.18 and is significant at the 10 percent level after FY2010 is excluded. Forecast-error estimates are generally imprecise, although the Michigan estimate is negative at the 10 percent level without FY2010. Thus the share-free forecast estimates reproduce the baseline timing direction, with greater precision outside FY2010, while the forecast-error evidence is weaker.

E.6 Inference and Robustness

This subsection first addresses inference when the shift-share measure is built from a small set of common national shifters and then builds up the baseline specification. It next evaluates sensitivity to historical coverage and fund scope, exposure-specific calendar seasonality, and the exact decision-window boundaries.

Inference with Common National Shifters. Our news measure combines five national innovations, so conventional two-way standard error clustering may not fully capture common-shifter dependence (Adão et al., 2019; Borusyak et al., 2022). In Table E.11, we run the baseline specification with a wild cluster bootstrap by calendar month-year and 500 permutations that reassign exposure vectors across states while holding national innovations and budget calendars fixed. The bootstrap tests for common-shock dependence, whereas the permutation tests whether the observed exposure–calendar alignment is unusual.

We report associated p-values in the bottom of the table. For the revenue forecast, the observed decision-window coefficient of 2.74 is statistically significant under both reference distributions: the wild-bootstrap and exposure-permutation p-values are each 0.004, and the permutation distribution has mean 0.13 and a 5th–95th percentile range of $[-1.29, 1.57]$. The forecast-error coefficient of -0.34 lies near the center of both reference distributions ($p = 0.774$ and 0.565).

Table E.10: **Share-Free Macroeconomic Timing Tests**

This table reports the analysis using alternative macroeconomic indices: the Michigan Index of Consumer Sentiment, the Chicago Fed National Activity Index (CFNAI), and the State Leading Index. The key regressor is $\text{Shock}_{s,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$. The national shocks are identical across states within a calendar month; the state subscript is binding only for the State Leading Index. Fiscal-year-by-calendar-month fixed effects absorb national shock levels, so the omitted group pools the pre-decision and deliberation months and each reported coefficient is the decision-minus-off-window premium. Innovations are standardized before treatment, and coefficients are in percentage points per standard deviation of the monthly innovation. The official State Leading Index ends in February 2020 and therefore has a shorter effective sample. Outcomes, controls, fixed effects, and two-way clustering follow Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Full FY2002–2024 sample

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$			Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Shock}_{s,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	0.40*	0.93**	0.12	-0.31	-0.19	-0.03
	(0.20)	(0.45)	(0.10)	(0.23)	(0.53)	(0.08)
Shock	Michigan	CFNAI	State leading	Michigan	CFNAI	State leading
State FE	✓	✓	✓	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	15,042	15,042	12,775	15,042	15,042	12,775
Adj. R^2	0.114	0.114	0.112	0.628	0.628	0.661

Panel B: Excluding FY2010

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$			Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Shock}_{s,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	0.43**	0.89*	0.18*	-0.39*	-0.15	-0.12
	(0.20)	(0.46)	(0.09)	(0.23)	(0.51)	(0.07)
Shock	Michigan	CFNAI	State leading	Michigan	CFNAI	State leading
State FE	✓	✓	✓	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	14,397	14,397	12,130	14,397	14,397	12,130
Adj. R^2	0.121	0.121	0.127	0.639	0.639	0.676

Table E.11: **Shift-Share Inference: Wild Bootstrap and Exposure Permutation**

The table reports inference diagnostics for the decision-window coefficient from the baseline three-window specification of Table 2, Panel A, for the forecast and forecast-error outcomes. The first rows report the window-specific coefficients and their conventional standard errors. Standard errors are two-way clustered by state and calendar month-year. "p: wild cluster bootstrap" is the wild cluster bootstrap p-value (999 replications) for the decision-window coefficient, clustered by calendar month-year and computed from the algebraically equivalent regression with explicit state and fiscal-year-by-calendar-month indicators. The permutation rows reassign entire predetermined exposure vectors one-to-one across the sample states, rebuild the news measure from the permuted exposures and observed national innovations, and re-estimate the specification 500 times; $p = (1 + \#\{|\hat{\beta}^{perm}| \geq |\hat{\beta}|\})/(1 + 500)$. The remaining rows summarize the permutation distribution. Sample and specification are identical to Table 2. The wild bootstrap is clustered only by calendar month-year, whereas the reported baseline standard error is two-way clustered by state and calendar month-year. The exposure permutation is interpreted as a diagnostic reference distribution under exchangeability of the reassigned exposure vectors. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$	Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$
	(1)	(2)
$\text{News}_{s,t,m}^{\text{Pre}}$	-0.10	1.21***
	(0.37)	(0.39)
$\text{News}_{s,t,m}^{\text{Decision}}$	2.74***	-0.34
	(0.61)	(0.68)
$\text{News}_{s,t,m}^{\text{Delib}}$	0.39	0.69**
	(0.62)	(0.30)
State FE	✓	✓
FYear × Year-Month FE	✓	✓
Controls	✓	✓
Observations	15,042	15,042
Adj. R^2	0.115	0.628
Decision-window inference diagnostics		
p: wild cluster bootstrap (month-year)	0.004	0.774
p: exposure permutation	0.004	0.565
Permutation mean	0.13	0.12
Permutation 5th–95th percentile	[-1.29, 1.57]	[-0.84, 1.15]
Permutation replications	500	500

Specification build-up. We show how our results adjust without controls and the stringent time fixed effects in Table E.12. We compute our revenue forecast and forecast-error regressions with increasing fixed effects and controls, holding the estimation sample fixed at the common sample of Table 2, Panel A. Columns (1) and (4) have state fixed effects only; in columns (2) and (5) we add fiscal-year-by-calendar-month fixed effects, and finally in columns (3) and (6) we add the lagged controls, mirroring our baseline specification. The decision-window forecast loading falls from 3.37 to 3.16 once the common national innovation is absorbed, and is further attenuated slightly by the controls (2.74); the off-window loadings are near zero throughout. Columns (4)-(6) report the forecast error loadings. The main forecast error result, where only off-decision window news predicts forecast errors while decision-window news does not, appears once the common shock is absorbed with time fixed effects.

Table E.12: **Specification Build-Up for the Window-Specific Loadings**

The table reports the window-specific loadings of Table 2, Panel A, across three specifications. Columns (1)–(3) use the original-budget revenue forecast revision, $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$, as the dependent variable; columns (4)–(6) use the within-year forecast error, $\log(\text{Act}_t^R/\text{Budget}_t^R)$. Columns (1) and (4) include state fixed effects only; columns (2) and (5) add fiscal-year-by-calendar-month fixed effects; columns (3) and (6) add the predetermined controls of Table 2 and reproduce its Panel A columns. All columns are restricted to the common estimation sample of Table 2, Panel A (the full-specification sample), so observations are identical across columns and coefficient changes reflect specification rather than sample composition. The sample is the stacked monthly panel restricted to the pre-decision, decision, and deliberation windows, 2002–2024. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$			Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{News}_{s,t,m}^{\text{Pre}}$	-0.05	-0.09	-0.10	0.09	1.27***	1.21***
	(0.59)	(0.31)	(0.37)	(0.42)	(0.35)	(0.39)
$\text{News}_{s,t,m}^{\text{Decision}}$	3.37***	3.16***	2.74***	1.54**	-0.30	-0.34
	(0.80)	(1.12)	(0.61)	(0.76)	(0.87)	(0.68)
$\text{News}_{s,t,m}^{\text{Delib}}$	0.48*	0.74	0.39	0.29	0.84*	0.69**
	(0.28)	(0.64)	(0.62)	(0.83)	(0.45)	(0.30)
State FE	✓	✓	✓	✓	✓	✓
FYear × Year-Month FE		✓	✓		✓	✓
Controls			✓			✓
Observations	15,042	15,042	15,042	15,042	15,042	15,042
Adj. R^2	0.027	0.075	0.115	0.551	0.599	0.628

Leave-one-out tests. Figures E.3 and E.4 re-estimate the model after excluding one fiscal-year cohort or one state at a time, respectively. Dropping any single fiscal year leaves the premium between 2.00 (dropping FY2010) and 2.83 (dropping FY2014), with a minimum t-statistic of 3.4. Dropping any single state leaves the decision premium between 1.61 and 2.66; the smallest value comes from dropping Alaska. Once the FY2010 crisis cohort is excluded, the leave-one-state-out premia range from 1.73 (dropping Texas) to 2.18 (dropping Alaska), with a minimum t-statistic of 2.7. Thus individual states and cohorts affect the magnitude, but no single state or year defines the key results.

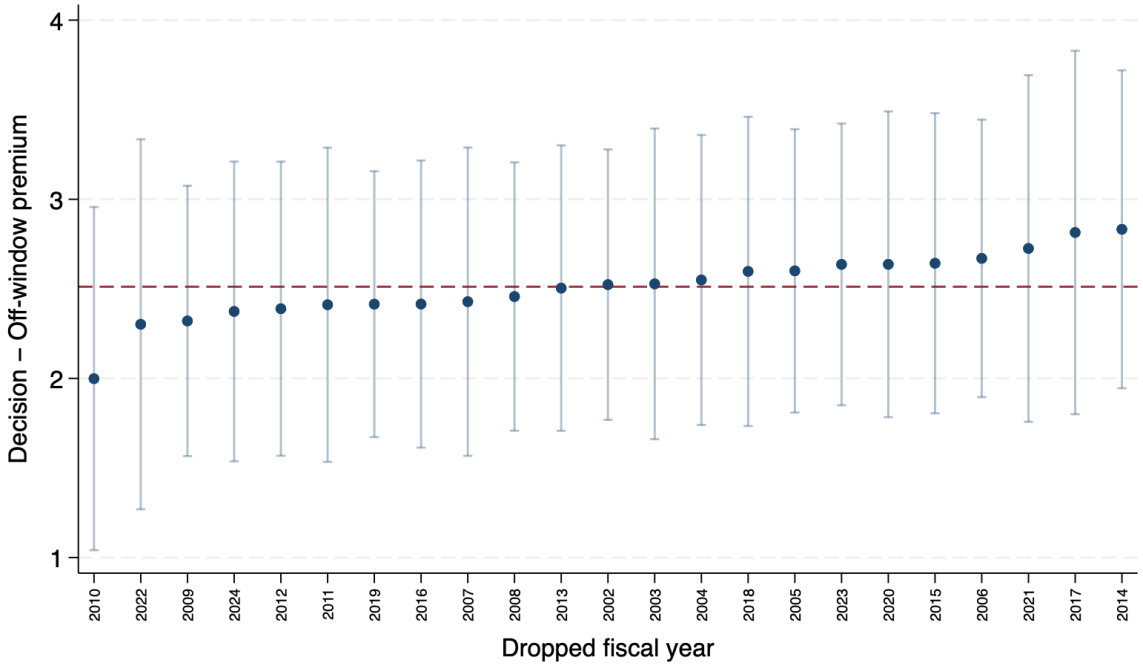
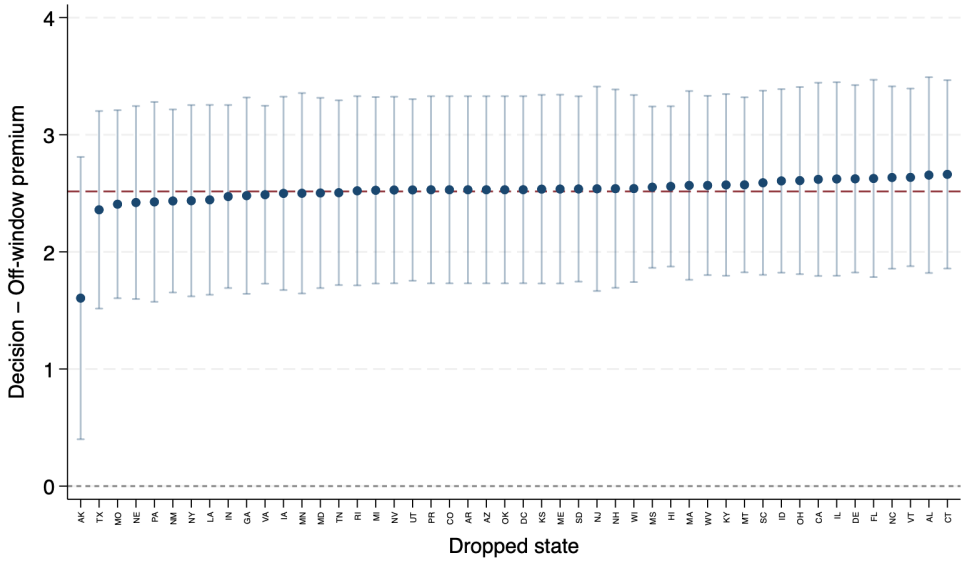
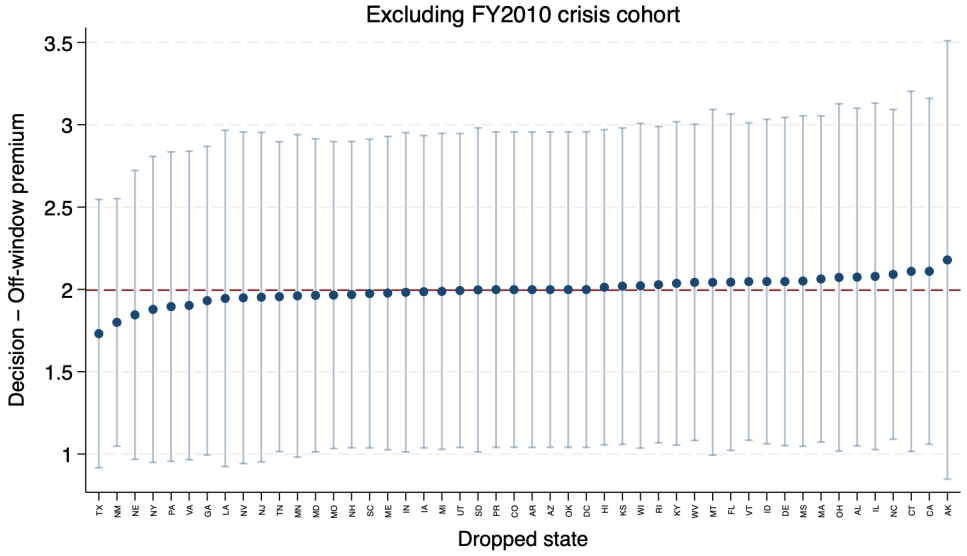


Figure E.3: **Leave-One-Fiscal-Year-Out Estimates of the Decision-Window Premium.** Each marker re-estimates the in/out decision-window premium of Table 2, Panel B, column (1) after dropping the indicated fiscal-year cohort. Cohorts are ordered by the estimated premium. Vertical bars are 90 percent confidence intervals based on standard errors two-way clustered by state and calendar month-year; the dashed horizontal line marks the mean across leave-one-out estimates.

Historical sample and fund scope. The preferred ACFR specification begins in FY2002 because reporting and fund definitions are more comparable in the later period. Table E.13 reports two revenue-only robustness exercises without reproducing the preferred FY2002+ General Fund estimates. Panel A extends the General Fund sample using every observation in each state's final continuous valid segment. Because budget updates require a lag, the first effective observation is FY1996. Across 997 state-years and 43 states, the decision-window



(a) Baseline sample



(b) Excluding the FY2010 crisis cohort

Figure E.4: **Leave-One-State-Out Estimates of the Decision-Window Premium.** Each marker re-estimates the in/out decision-window premium of Table 2, Panel B, column (1)—the coefficient on $\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$ for the original-budget revenue forecast—after dropping the indicated state. Panel (b) additionally excludes the FY2010 cohort from every regression. States are ordered by the estimated premium. Vertical bars are 90 percent confidence intervals based on standard errors two-way clustered by state and calendar month-year; the dashed horizontal line marks the mean across leave-one-out estimates.

coefficient on the revenue-budget update is 2.65 (standard error 0.66); the pre-decision and deliberation coefficients are −0.06 and 0.42. The forecast-error pattern is reversed: pre-decision and deliberation news have coefficients of 1.30 and 0.70, whereas the decision-window coefficient is -0.39.

Panel B broadens the revenue concept to the General Fund plus persistent eligible education, health, or transportation operating funds. The expanded measure adds a fund in 350 state-years across 16 states; states without an eligible addition retain their General Fund values. The revenue-budget coefficients are −0.16, 2.72, and 0.44 for the pre-decision, decision, and deliberation windows, and the corresponding forecast-error coefficients are 1.29, -0.32, and 0.64. Ten FY2002 observations are unavailable because a newly added fund lacks a comparable FY2001 lag. We do not report a combined FY1995+ expanded-scope specification because no operating fund is actually added before FY2002; such an exercise would not be a genuinely historical expanded- scope comparison.

Exposure-specific calendar seasonality. Proposal months cluster in the late fall and winter: one concern is that the window is capturing seasonality. The fixed effects absorb common calendar-month levels, but the potential remaining concern is that states with different revenue compositions could also have different seasonal gradients in forecasts or outcomes. Table E.14 addresses this with period-interacted share controls (Goldsmith-Pinkham et al., 2020): calendar-month-specific slopes on the five predetermined exposure shares, using each cohort's own weights and, more conservatively, weights fixed at their 1995 values. The decision-window loading is essentially unchanged (2.74 at baseline, 2.83 and 2.84 with the controls), and the forecast-error coefficient remains indistinguishable from zero, suggesting that spurious correlation between shares and calendar seasonality is not driving our effects.

Alternative decision-window boundaries. Next, we test if our results are fragile to the exact definition of the decision window. Table E.15 runs our baseline specification from Table 2, Panel A, under four alternative decision-window definitions: $[-4,-1]$, $[-6,-1]$, $[-5,0]$ (adding the proposal month itself), and $[-5,-2]$ (dropping the final pre-proposal month). In each case, the pre-decision window begins at $\ell = -11$ and ends one month before the decision window, and deliberation begins one month after it, so the three windows always span the sample months. The forecast loading is significant under every alternative specification and largest for $[-4,-1]$; dropping the final pre-proposal month ($[-5,-2]$) barely moves it, so the result is not driven by last-minute news. Adding the proposal month ($[-5,0]$)

Table E.13: **Historical Sample and Revenue-Fund Scope Robustness**

Following Table 2, Panel A, the rows report pre-decision, decision, and deliberation news loadings. Panel A uses all observations in each state's final continuous valid General Fund segment beginning in FY1995 or later. Panel B uses General Fund plus eligible operating funds over FY2002–2024. Eligible additions are persistent state education, health, or transportation operating funds selected by the upstream continuity and stable-definition rules. States without an eligible addition retain their General Fund values; federal funds, one-off funds, temporary special-purpose funds, and debt proceeds are excluded. Ten FY2002 observations with a newly starting expanded scope lack a comparable FY2001 lag. Both specifications use the baseline windows, fixed effects, and controls. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Historical General Fund sample

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$	Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$	Realized $\log(\text{Act}_t^R/\text{Budget}_{t-1}^R)$
	(1)	(2)	(3)
$\text{News}_{s,t,m}^{\text{Pre}}$	-0.06	1.30***	1.25**
	(0.35)	(0.43)	(0.58)
$\text{News}_{s,t,m}^{\text{Decision}}$	2.65***	-0.39	2.26***
	(0.66)	(0.68)	(0.82)
$\text{News}_{s,t,m}^{\text{Delib}}$	0.42	0.70**	1.12**
	(0.62)	(0.31)	(0.45)
State FE	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓
Controls	✓	✓	✓
Observations	16,947	16,947	16,947
Adj. R^2	0.113	0.620	0.532

Panel B: Eligible operating-fund scope

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$	Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$	Realized $\log(\text{Act}_t^R/\text{Budget}_{t-1}^R)$
	(1)	(2)	(3)
$\text{News}_{s,t,m}^{\text{Pre}}$	-0.16	1.29**	1.12*
	(0.46)	(0.48)	(0.58)
$\text{News}_{s,t,m}^{\text{Decision}}$	2.72***	-0.32	2.40***
	(0.61)	(0.69)	(0.85)
$\text{News}_{s,t,m}^{\text{Delib}}$	0.44	0.64*	1.08**
	(0.65)	(0.33)	(0.51)
State FE	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓
Controls	✓	✓	✓
Observations	14,869	14,869	14,869
Adj. R^2	0.128	0.634	0.552

Table E.14: **Exposure-Specific Calendar Seasonality**

The table re-estimates Table 2, Panel A, adding calendar-month-specific slopes on the five predetermined revenue-exposure shares, the standard period-interacted share controls for shift-share designs. For each outcome, the *Base* column reproduces the baseline; *Shares* × *month* interacts calendar-month indicators with each cohort's own predetermined exposure shares $\omega_{s,t}$ (the five-year trailing averages lagged two years that also enter the news measure); *Shares* × *month (1995)* instead interacts the month indicators with the shares fixed at their initial 1995 values, $\omega_{s,1995}$, a conservative variant immune to any drift in the weights. The share-by-month terms are level controls, so they absorb exposure-specific seasonal gradients in the outcomes while leaving the window-timing variation intact. Outcomes, window definitions, fixed effects, controls, common-sample restriction, and clustering are identical to Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Forecast			Forecast error		
	(1) Base	(2) Shares × month	(3) Shares × month (1995)	(4) Base	(5) Shares × month	(6) Shares × month (1995)
$\text{News}^{\text{Pre}}_{s,t,m}$	-0.10	-0.08	-0.10	1.21***	1.15**	1.24**
	(0.37)	(0.44)	(0.45)	(0.39)	(0.55)	(0.47)
$\text{News}^{\text{Decision}}_{s,t,m}$	2.74***	2.83***	2.84***	-0.34	-0.46	-0.34
	(0.61)	(0.73)	(0.71)	(0.68)	(0.74)	(0.83)
$\text{News}^{\text{Delib}}_{s,t,m}$	0.39	0.39	0.41	0.69**	0.70	0.71*
	(0.62)	(0.66)	(0.65)	(0.30)	(0.45)	(0.42)
$\omega_{s,t}$× Cal. month		✓			✓	
$\omega_{s,1995}$× Cal. month			✓			✓
State FE	✓	✓	✓	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	15,042	15,042	15,042	15,042	15,042	15,042
Adj. R^2	0.115	0.115	0.112	0.628	0.637	0.627

attenuates the result, consistent with the budget being largely locked by the submission month. The forecast-error columns (5)–(8) show the mirroring results in every alternative specification: the decision-window error loading is not distinguishable from zero, while off-window news systematically predicts forecast errors.

Table E.15: **Alternative Decision-Window Boundaries**

The table re-estimates the window-specific loadings of Table 2, Panel A, with the decision window redefined as $\ell \in [-4,-1]$, $[-6,-1]$, $[-5,0]$, or $[-5,-2]$ (columns as labeled; the baseline $[-5,-1]$ is Table 2). Columns (1)–(4) use the original-budget revenue forecast revision, $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$, as the dependent variable; columns (5)–(8) use the within-year forecast error, $\log(\text{Act}_t^R/\text{Budget}_t^R)$. In each variant the pre-decision window is $[-11, \ell_{\text{lo}}-1]$ and deliberation runs from $\ell_{\text{hi}}+1$ to the fiscal-year start, so the three windows partition the sample months. The sample is the common estimation sample of Table 2 (stacked monthly panel, 2002–2024). All specifications include state and fiscal-year-by-calendar-month fixed effects and the predetermined controls of Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Forecast $\log(\text{Budget}_t^R/\text{Budget}_{t-1}^R)$				Forecast error $\log(\text{Act}_t^R/\text{Budget}_t^R)$			
	(1) $[-4,-1]$	(2) $[-6,-1]$	(3) $[-5,0]$	(4) $[-5,-2]$	(5) $[-4,-1]$	(6) $[-6,-1]$	(7) $[-5,0]$	(8) $[-5,-2]$
$\text{News}_{s,t,m}^{\text{Pre}}$	-0.08	-0.04	-0.13	-0.10	0.96***	1.08*	1.22***	1.22***
	(0.30)	(0.43)	(0.38)	(0.38)	(0.31)	(0.57)	(0.42)	(0.39)
$\text{News}_{s,t,m}^{\text{Decision}}$	3.18***	2.33***	2.02**	2.61***	0.03	0.05	-0.28	-0.19
	(0.91)	(0.71)	(0.84)	(0.84)	(0.59)	(0.75)	(0.44)	(0.86)
$\text{News}_{s,t,m}^{\text{Delib}}$	0.44	0.35	0.33	0.63	0.72**	0.73**	0.88*	0.56*
	(0.62)	(0.65)	(0.74)	(0.59)	(0.29)	(0.28)	(0.44)	(0.33)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	15,042	15,042	15,042	15,042	15,042	15,042	15,042	15,042
Adj. R^2	0.116	0.115	0.115	0.115	0.628	0.628	0.629	0.628

F Real Effects: Additional Results and Robustness

This appendix presents robustness for the results in Section 4. It first presents supporting evidence for the industry-level employment results, then examines dynamic responses and delayed fiscal adjustment, budget-side sign asymmetry, and aggregate state-level robustness. It concludes with diagnostics for the monetary-policy exercise.

F.1 Government-Exposed Industry Effects

Public-payroll diagnostic. Table F.1 asks whether decision-window news directly moves public-sector payroll. We construct public employment and wages from the annual QCEW panel as total employment and wages minus private employment and wages, aggregate to the state-year level, and estimate the same decision-window news specification. The coefficients are negative and statistically insignificant for both public employment and public wages. Thus, the private-sector response in Table 4 is not simply a direct public-payroll response. It is more consistent with adjustment through procurement, grants, contracts, or other non-payroll spending margins.

Table F.1: **Decision-Window News and Public-Sector Payroll**

The table estimates state-level regressions of public-sector employment and wages on decision-window news. Public employment is $\text{Emp}^{\text{total}} - \text{Emp}^{\text{private}}$, and public wages are $\text{Wages}^{\text{total}} - \text{Wages}^{\text{private}}$, both constructed from the annual QCEW state-industry panel and aggregated to the state-year level. The dependent variables are one-year log changes. All specifications include state and fiscal-year-by-calendar-month fixed effects. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Public employment	Public wages
	(1)	(2)
$\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	-1.414	-0.128
	(1.009)	(0.344)
State FE	✓	✓
FYear × Year-Month FE	✓	✓
Observations	25,444	25,444
Adj. R^2	0.044	0.083

Excluding the financial-crisis budget round. The financial crisis is a useful stress test because FY2010 contains the September 2008 shock for many state budget cycles. Table F.2

repeats the main industry specification excluding FY2010. The coefficients are similar to the baseline estimates in Table 4, indicating that the industry response is not driven by the crisis budget round alone.

Leave-one-state-out robustness. Figure F.1 re-estimates the main industry specification after omitting one state at a time. The plotted coefficients are the decision-window triple interaction for private employment and private wages. The dashed horizontal line is the full-sample estimate. The exercise checks whether the main industry result is driven by a single large state or a single unusual budget calendar.

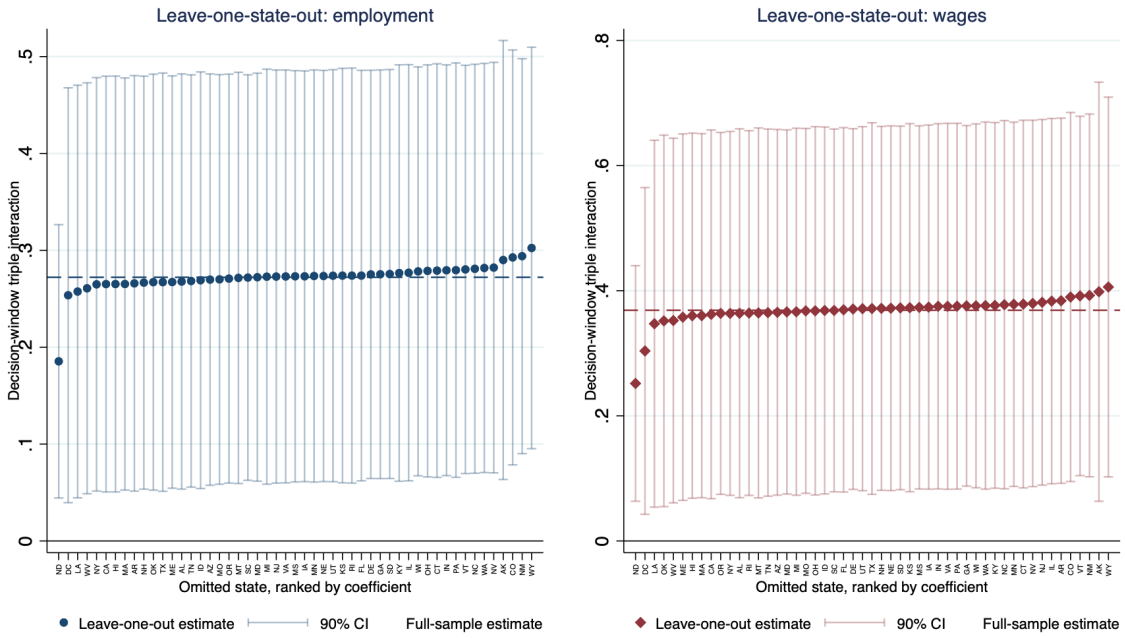


Figure F.1: **Leave-One-State-Out Robustness for Government-Exposed Industries.** The figure re-estimates the main industry-level specification after omitting one state at a time. Each point is the coefficient on $\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\} \times \text{GovShare}^{high}$, with 90 percent confidence intervals. The dashed line is the full-sample estimate. The x-axis labels indicate the omitted state, ranked by the leave-one-out coefficient.

Industry-exposure randomization inference. Because government exposure is measured at the two-digit industry level, Table F.3 reports a randomization check that permutes the high-exposure indicator across industries and re-estimates the baseline specification. The observed employment coefficient, 0.416, lies near the upper tail of the placebo distribution: the permutation mean is -0.017, the 5th–95th percentile range is $[-0.297, 0.271]$, and the

Table F.2: **Real Effects of Decision-Window News, Excluding FY2010**

The table repeats the industry-level triple-difference regressions from Table 4, excluding fiscal year 2010. The dependent variables are one-year log changes in private employment and private wages. The key regressor is the state-specific shift-share news measure, $\text{News}_{s,t,m}$, interacted with an indicator for months in state s's budget decision window for fiscal year t, $\mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$, and a measure of industry exposure to state and local government demand. Panel A uses GovShare^{high}, an indicator for above-median exposure. Panel B uses GovShare^{sd}, the continuous exposure share standardized to mean zero and unit variance. The sample is a stacked state–industry–fiscal-year–month panel covering two-digit NAICS industries; for each state-fiscal-year, included months fall before the fiscal-year start. All specifications include the relevant lower-order interactions. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Above-Median Exposure Indicator

	$\log(\text{Emp}_{s,t}/\text{Emp}_{s,t-1})$		$\log(\text{Wages}_{s,t}/\text{Wages}_{s,t-1})$	
	(1)	(2)	(3)	(4)
$\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\} \times \text{GovShare}_i^{high}$	0.31*	0.32*	0.45**	0.46**
	(0.16)	(0.17)	(0.21)	(0.23)
Two-way Interactions	✓	✓	✓	✓
State × Year FE	✓	✓	✓	✓
Industry × Year FE	✓	✓	✓	✓
State × Industry FE		✓		✓
FYear × Year-Month FE		✓		✓
Observations	549,395	549,395	549,395	549,395
Adj.R^2	0.55	0.57	0.78	0.78

Panel B: Continuous Exposure Measure

	$\log(\text{Emp}_{s,t}/\text{Emp}_{s,t-1})$		$\log(\text{Wages}_{s,t}/\text{Wages}_{s,t-1})$	
	(1)	(2)	(3)	(4)
$\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\} \times \text{GovShare}_{i,t}^{sd}$	0.19**	0.20**	0.23**	0.24**
	(0.08)	(0.09)	(0.10)	(0.10)
Two-way Interactions	✓	✓	✓	✓
State × Year FE	✓	✓	✓	✓
Industry × Year FE	✓	✓	✓	✓
State × Industry FE		✓		✓
FYear × Year-Month FE		✓		✓
Observations	549,395	549,395	549,395	549,395
Adj.R^2	0.55	0.57	0.78	0.78

two-sided permutation p-value is 0.014. The response is therefore concentrated in the industries actually exposed to government demand, rather than reflecting a generic industry-level pattern.

Table F.3: **Government-Exposure Permutation Inference**

The table reports randomization inference for the employment coefficient in the main industry-level specification. The observed coefficient is the coefficient on $\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\} \times \text{GovShare}^{high}$. The placebo distribution permutes the above-median government-exposure indicator across two-digit industries, re-estimates the baseline specification, and compares the observed coefficient with the resulting placebo coefficients. The specification, fixed effects, sample, and clustering match Table 4. The observed standard error is two-way clustered by state and calendar month-year.

Statistic	Value
Observed coefficient	0.416
Observed standard error	0.129
Permutation p-value	0.014
Permutation mean	-0.017
Permutation 5th–95th pctile	[-0.297, 0.271]
Permutations	500
Observations	499,837

Dynamic response with continuous exposure. The main text reports the dynamic response using the high-exposure indicator. As an additional check, we build estimates the same local projection using a continuous government-exposure measure; see Figure F.2. This exercise confirms that the delayed and persistent response in Figure 5 is not tied to the binary exposure definition.

F.2 Dynamics and Asymmetry of Fiscal Adjustment

Delayed fiscal adjustment to off-window news. Table F.4 examines whether off-window information is permanently excluded from future fiscal outcomes. News from all three windows predicts realized revenue at one or more subsequent horizons, consistent with persistence in the underlying revenue shocks. Although off-window news does not significantly predict contemporaneous spending, it predicts realized spending at later horizons. Off-window information therefore reaches state spending with a delay, and the weak persistent employment response cannot be explained simply by its never affecting future fiscal outcomes.

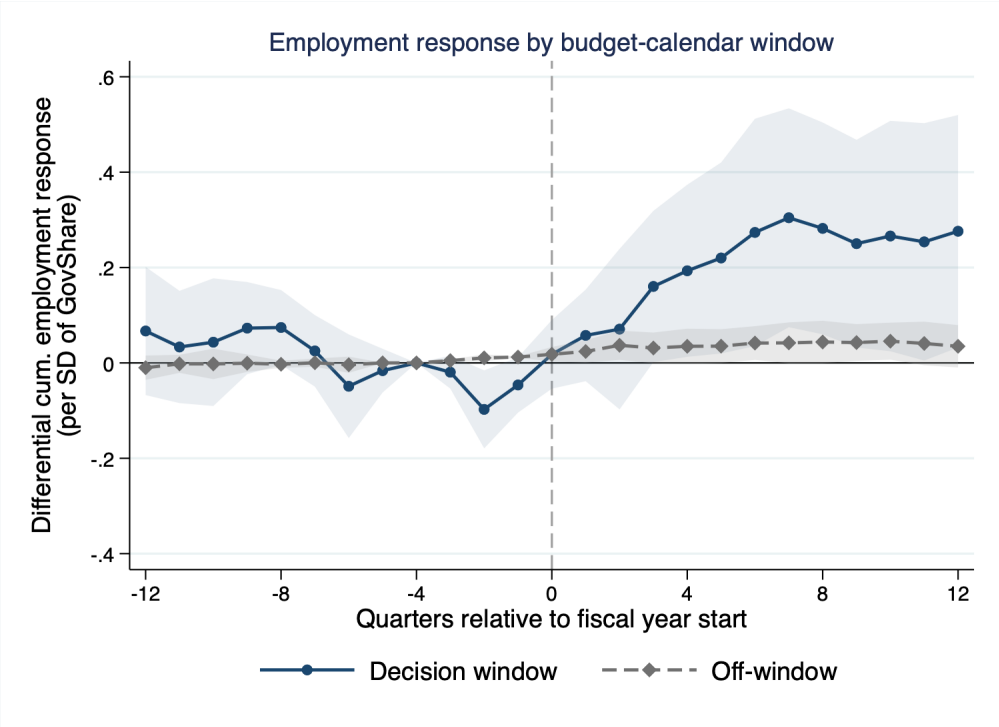


Figure F.2: **Dynamic Employment Response: Continuous Exposure.** The figure repeats the dynamic local projections in Figure 5 using the standardized continuous government-exposure measure. The dependent variable is cumulative private employment growth from the pre-fiscal-year base to horizon h. Regressions include state-by-industry, industry-by-fiscal-year, and state-by-fiscal-year fixed effects. Shaded areas are confidence intervals based on clustered standard errors.

Table F.4: **Realized Revenue and Spending Paths by Window of News Arrival**

The table reports monthly-panel regressions tracing realized revenue and spending following news arriving in each proposal-relative window. All outcomes use the corresponding fiscal-year-$t-1$ budget as a common pre-news benchmark. Columns (1)–(4) report $\log(\text{Act}^R_{t+k}/\text{Budget}^R_{t-1})$, and columns (5)–(8) report $\log(\text{Act}^E_{t+k}/\text{Budget}^E_{t-1})$, for $k = 0, 1, 2, 3$. Within each horizon, the revenue and spending regressions use the same state-fiscal-year-month observations. As in Panel A of Table 2, the regressors are the window-specific news measures: pre-decision news ($\text{News}^{\text{Pre}}_{s,t,m}$, $\ell \in [-11, -6]$), decision-window news ($\text{News}^{\text{Decision}}_{s,t,m}$, $\ell \in [-5, -1]$), and deliberation news ($\text{News}^{\text{Delib}}_{s,t,m}$, $\ell \geq 0$ before fiscal-year start). The sample is the stacked monthly panel restricted to these three pre-fiscal-year windows, 2002–2024; the number of observations declines with the availability of each future outcome horizon. All specifications include state and fiscal-year-by-calendar-month fixed effects and the predetermined controls described in Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Realized revenue $\log(\text{Act}^R_{t+k}/\text{Budget}^R_{t-1})$				Realized spending $\log(\text{Act}^E_{t+k}/\text{Budget}^E_{t-1})$			
	t	t+1	t+2	t+3	t	t+1	t+2	t+3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{News}^{\text{Pre}}_{s,t,m}$	1.11**	1.27	2.81**	2.54	0.50	0.09	0.93*	1.68**
	(0.55)	(1.22)	(1.13)	(1.54)	(0.51)	(0.41)	(0.47)	(0.71)
$\text{News}^{\text{Decision}}_{s,t,m}$	2.40***	1.20	-0.96	-2.72***	1.42**	0.96*	1.43*	0.37
	(0.85)	(0.78)	(1.05)	(0.78)	(0.62)	(0.52)	(0.76)	(0.58)
$\text{News}^{\text{Delib}}_{s,t,m}$	1.08**	0.87	0.35	1.06***	0.43	0.44	0.36	0.87**
	(0.45)	(1.05)	(0.42)	(0.25)	(0.40)	(0.31)	(0.30)	(0.36)
Observations	15,042	14,308	13,592	12,876	15,042	14,308	13,592	12,876
Adj. R^2	0.538	0.500	0.471	0.458	0.548	0.503	0.504	0.525

Budget-Side Sign Asymmetry The large employment response to negative news could arise if negative news generated larger spending cuts. Table F.5 tests this mechanism. We split decision-window news into a positive component and the absolute value of its negative component and estimate the responses of the revenue forecast, the spending plan, and realized spending.

The results do not support a budget-stage explanation for the negative employment asymmetry. Revenue forecasts respond approximately symmetrically to positive and negative news (3.60 versus −2.22). Spending responses, if anything, are larger for positive news: 1.99 versus −0.70 for planned spending and 2.50 versus −1.47 for realized spending. The concentration of employment losses following negative news therefore cannot be attributed to larger budget cuts.

F.3 Aggregate State-Level Robustness

Table F.6 repeats the aggregate state-level analysis after excluding FY2010. The results are robust, with magnitudes similar to those reported in Table 5.

F.4 Monetary-Policy Diagnostics

Season balance of the surprises. The aggregate exercise in Section 4.2 compares shocks arriving in Q4, when almost all states are writing budgets, to shocks arriving in Q1–Q3. A concern is that the monetary surprises themselves differ by quarters. Figure F.3 compares the distributions of the four surprise measures used in Figure 7. The balance diagnostics show that Q4 surprises are not systematically larger or differently distributed than surprises in the rest of the year.

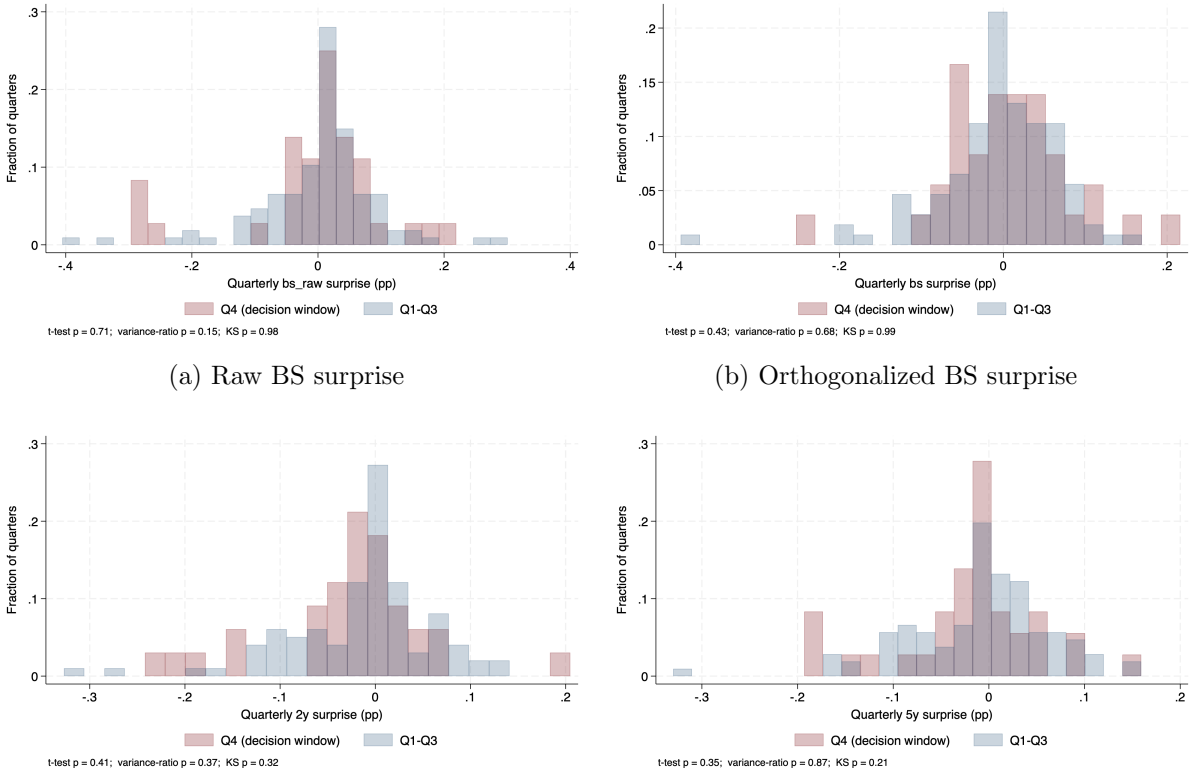
Magnitude benchmark. Figure F.4 reports the total response of real GDP to the orthogonalized Bauer–Swanson surprise from the same local-projection design, in the same units as the spending responses (percent of initial GDP), so the two are directly comparable.

Distinguishing Q4 timing from Q4 confounds. We report the difference tests between Q4 and Q1–Q3 responses and a set of checks for the confounds specific to Q4.

Table F.5: **Asymmetric Pass-Through of Decision-Window News**

The table reports monthly-panel regressions of revenue-budget growth, $\log(\text{Budget}^R_{s,t}/\text{Budget}^R_{s,t-1})$ (column 1), spending-budget growth, $\log(\text{Budget}^E_{s,t}/\text{Budget}^E_{s,t-1})$ (column 2), and realized expenditure growth benchmarked to the lagged spending plan, $\log(\text{Act}^E_{s,t}/\text{Budget}^E_{s,t-1})$ (column 3), on sign-split budget-decision-window news. Budget-decision-window news, $\text{News}^{\text{Decision}}_{s,t,m}$, is split into a positive component $\text{News}^{\text{Decision},+}_{s,t,m}$ and an absolute-value-of-negative component $|\text{News}^{\text{Decision},-}_{s,t,m}|$, where the decision window is $\ell \in [-5,-1]$ and ℓ is the calendar-month distance from state s's FY-t proposal date. News in the other windows enters as signed single coefficients: pre-decision ($\ell \in [-11,-6]$) and deliberation ($\ell \geq 0$ and $m <$ FY start). The estimation sample is restricted to the pre-decision, decision, and deliberation windows; these three windows partition the included pre-fiscal-year observations. Because the negative-news regressor is expressed in absolute value, sign symmetry implies $\beta_+ + \beta_- = 0$. A positive sum indicates a larger response in absolute value to favorable news. The table reports this sum and the p-value of the symmetry test. All specifications include predetermined controls: four lagged quarterly windows of CFNAI, payroll employment, the S&P 500, the five-year Treasury yield, and CPI, with the most recent ending one year before budget formation; the Borusyak et al. (2022) aggregate-share control; and two-year-lagged state rainy-day-fund balances and GDP growth. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	Revenue budget $\log(\text{Budget}^R_t/\text{Budget}^R_{t-1})$	Spending budget $\log(\text{Budget}^E_t/\text{Budget}^E_{t-1})$	Spending growth $\log(\text{Act}^E_t/\text{Budget}^E_{t-1})$
	(1)	(2)	(3)
$\text{News}^{\text{Pre}}_{s,t,m}$	-0.11	-0.25	0.62
	(0.38)	(0.39)	(0.49)
$\text{News}^{\text{Decision},+}_{s,t,m}$	3.60***	1.99***	2.50***
	(0.87)	(0.48)	(0.65)
$\lvert\text{News}^{\text{Decision},-}_{s,t,m}\rvert$	-2.22**	-0.70	-1.47**
	(0.84)	(0.65)	(0.59)
$\text{News}^{\text{Delib}}_{s,t,m}$	0.41	0.26	0.45
	(0.61)	(0.52)	(0.35)
$\beta_+ + \beta_-$ (asymmetry)	1.39 (1.08)	1.28* (0.71)	1.02* (0.54)
p(symmetry)	0.204	0.078	0.066
State FE	✓	✓	✓
FYear × Year-Month FE	✓	✓	✓
Controls	✓	✓	✓
Observations	15,042	17,034	17,034
Adj. R^2	0.115	0.145	0.614



(a) Raw BS surprise

(b) Orthogonalized BS surprise

(c) 2-year Treasury surprise

(d) 5-year Treasury surprise

Figure F.3: **Are Monetary Policy Surprises Different by Budget Season?** The figure plots the distribution of each quarterly monetary policy surprise, split by whether it arrives in Q4, when many states draft the upcoming budget, or outside that season. Histograms use common bins and are normalized by the number of quarters within each season. Each panel reports p-values from a two-sample t-test of equal means, a variance-ratio test of equal dispersion, and a two-sample Kolmogorov–Smirnov test of equal distributions.

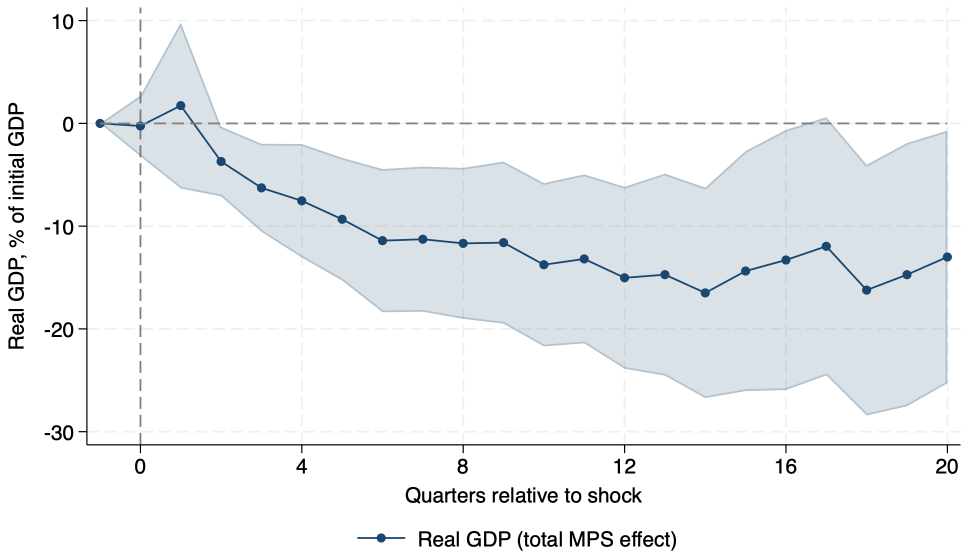


Figure F.4: **Magnitude Benchmark: Total Response of Real GDP to the Monetary Policy Surprise.** The figure plots local-projection estimates of the cumulative change in real GDP from quarter $t-1$ to $t+h$, scaled by real GDP in quarter $t-1$ and multiplied by 100 (percent of initial GDP—the same units as the spending responses in Figure 7), on the quarterly sum of orthogonalized Bauer–Swanson monetary policy surprises (Bauer and Swanson, 2023). The regression uses the total shock, not the Q4/Q1–Q3 split, since monetary transmission to aggregate output is not seasonal. Controls follow the spending local projections: four lags of the shock and four lags of state-and-local spending growth. The additional predetermined controls are quarters $t-2$ through $t-5$ of log changes in real GDP, payroll employment, the S&P 500, and the CPI, plus quarters $t-2$ through $t-9$ of the change in the one-year Treasury yield. The contemporaneous quarter is excluded because asset prices and yields respond to the shock within the quarter. Quarterly NIPA data, 1988–2023. Shaded areas are 90 percent confidence intervals based on Newey–West standard errors with lag length $\max(h+1, 4)$.

Table F.6: **Aggregate State-Level Real Effects, Excluding FY2010**

The table repeats the aggregate state-level regressions from Table 5, excluding fiscal year 2010. The dependent variables are one-year log changes in private employment and private wages. The specification is the same in/out parametrization as in the main table: $\text{News}_{s,t,m}$ is the pooled off-window response and $\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$ is the decision-window premium. The sample is a stacked monthly panel restricted to one budget round, excluding the earliest before-pre-decision months and execution months. All regressions absorb state and fiscal-year-by-calendar-month fixed effects and include the predetermined controls used in Table 5. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	$\log(\text{Emp}_{s,t}/\text{Emp}_{s,t-1})$	$\log(\text{Wages}_{s,t}/\text{Wages}_{s,t-1})$
	(1)	(2)
$\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}_{s,t}^{\text{Decision}}\}$	0.500^{***}	0.718^{***}
	(0.102)	(0.251)
$\text{News}_{s,t,m}$	0.103^{**}	0.199
	(0.040)	(0.125)
State FE	✓	✓
FYear × Year-Month FE	✓	✓
Macro + State Controls	✓	✓
Exclude FY 2010	✓	✓
Observations	19,327	19,327
Adj. R^2	0.84	0.72

Table F.7 directly tests the differences across horizons. All four measures yield negative Q4-minus-Q1–Q3 estimates over the two- to four-year range.

Table F.8 addresses potential confounds specific to calendar Q4. First, because Q4 marks the beginning of the federal fiscal year, we add lags of federal spending as controls; the results remain unchanged. Moreover, federal current expenditures exhibit no comparable Q4 premium in response to monetary policy shocks (Figure F.5). Second, to rule out effects associated with the year-end FOMC meeting, we reconstruct the Q4 surprise using only October and November monetary policy surprises. The results remain robust.

Table F.7: **Q4-versus-Q1–Q3 Difference in the Aggregate Spending Response**

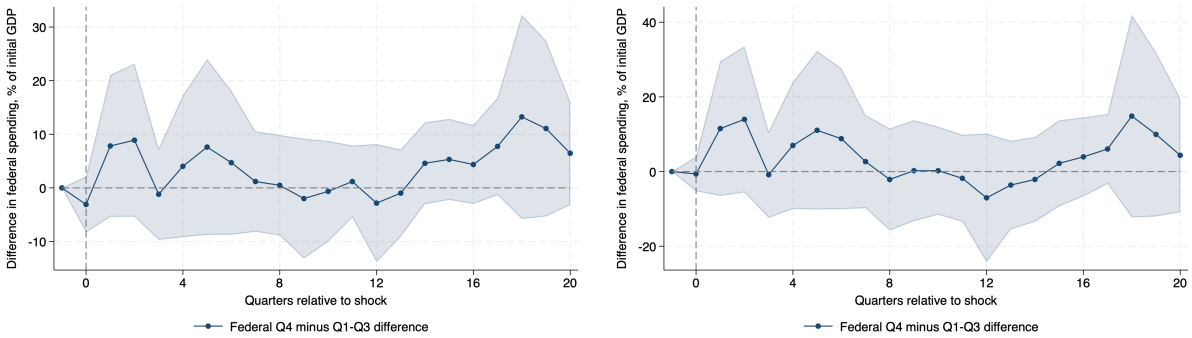
The table reports the decision-window difference in the aggregate state-and-local spending response, $\Delta(h) =$ (Q4 response) − (Q1–Q3 response), from the local projection of Figure 7 reparametrized so that the coefficient on the Q4 interaction is the difference at horizon h. Columns report Δ at horizons $h = 8, 12, 16$ quarters. The orthogonalized Bauer–Swanson surprise is the primary measure; the four surprises are correlated, so they are not four independent tests. The specification, controls, and sample match Figure 7. Standard errors use the horizon-dependent Newey–West estimator. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Surprise	Δ at h=8	Δ at h=12	Δ at h=16
BS raw	-0.843	-1.281	-1.739**
	(0.602)	(0.829)	(0.856)
BS orthogonalized	-1.609***	-2.339***	-2.695***
	(0.616)	(0.810)	(0.930)
2-year Treasury	-1.435*	-1.721*	-1.850
	(0.780)	(1.015)	(1.172)
5-year Treasury	-1.591**	-2.017*	-2.094*
	(0.709)	(1.094)	(1.191)

Table F.8: **Q4 Confound Checks**

The table reports the Q4-minus-Q1–Q3 difference in the cumulative state-and-local spending response at the three-year horizon ($h = 12$) under each confound check, for the two Bauer–Swanson surprises (orthogonalized and raw). "Federal placebo" replaces the outcome with federal current expenditures (FRED FGEXPND), deflated and scaled identically to the state-and-local series; "S&L, control federal spending" adds four predetermined quarterly lags of federal-spending growth to the state-and-local regression; and "No-December" rebuilds the Q4 surprise from October and November only, dropping December's monthly contribution before quarterly aggregation. The specification, controls, and sample otherwise match Figure 7. Standard errors use the horizon-dependent Newey–West estimator. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Confound check (h=12)	BS raw	BS orthogonalized
Federal placebo (federal spending)	1.184 (4.038)	-1.791 (6.896)
S&L, control federal spending	-1.726*** (0.629)	-2.020*** (0.677)
No-December (Oct+Nov Q4)	-2.222*** (0.634)	-2.515*** (0.737)



(a) Raw BS surprise

(b) Orthogonalized BS surprise

Figure F.5: **Federal-Spending Placebo.** Q4-minus-Q1–Q3 difference in the cumulative federal spending response (FRED federal current expenditures), from the same local-projection design as the state-and-local difference in Figure **??**, for the two Bauer–Swanson surprises. A band covering zero indicates no federal-fiscal-year artifact. Shaded areas are 90 percent confidence intervals from horizon-dependent Newey–West standard errors.

G Rule-Induced Multipliers

This appendix formalizes a rule-induced fiscal elasticity: the response of aggregate outcomes to realized spending variation generated by routine forecast-mediated budget adjustment. This differs from a conventional fiscal multiplier identified from residual spending shocks.

Let $Z_{s,t,w}$ denote revenue news arriving in window w, with D denoting the budget decision window and O the pooled off-window comparison. Define the reduced-form responses of the outcome and of realized spending to window-w news,

$$\gamma_w^Y \equiv \frac{\partial \log Y_{s,t}}{\partial Z_{s,t,w}}, \qquad \Gamma_w \equiv \frac{\partial \log G_{s,t}^{\text{act}}}{\partial Z_{s,t,w}},$$

together with the fiscal elasticity $\mu_Y^f \equiv \partial \log Y_{s,t} / \partial \log G_{s,t}^{\text{act}}$. Revenue news can affect Y through a private channel and a fiscal channel:

$$\gamma_w^Y = \underbrace{\eta_w^Y}_{\text{private channel}} + \underbrace{\mu_Y^f \, \Gamma_w}_{\text{fiscal channel}},$$

where η_w^Y is the non-fiscal response to the news. The fiscal channel is the multiplier times Γ_w, and news enters this channel only through the budget it funds.

The identifying condition is that decision-window and off-window news have the same private-channel effect:

$$\eta_D^Y = \eta_O^Y.$$

This assumption is plausible because on- and off-window news reflect the same underlying innovations: the statutory calendar determines whether news enters the current budget, not how firms and households respond to its economic content. Differencing the decision-window and off-window reduced forms therefore removes the private channel:

$$\gamma_D^Y - \gamma_O^Y = \mu_Y^f \, (\Gamma_D - \Gamma_O).$$

The differential spending response $\Gamma_D - \Gamma_O$ is the decision-window premium in realized spending, which we estimate directly (Table G.1, Panel B, column 2). Then, the estimated multiplier is

$$\widehat{\mu}_Y^f = \frac{\gamma_D^Y - \gamma_O^Y}{\Gamma_D - \Gamma_O}.$$

Table G.1: **Spending Response to Decision-Window Revenue News**

Monthly-panel regressions of the enacted spending budget and realized spending on state-specific shift-share news by proposal-relative timing. Column (1) is enacted spending-budget growth, $\log(\text{Budget}^E_{s,t}/\text{Budget}^E_{s,t-1})$; column (2) is realized spending benchmarked to the prior enacted budget, $\log(\text{Act}^E_{s,t}/\text{Budget}^E_{s,t-1})$. Panel A reports separate coefficients for pre-decision news ($\text{News}^{\text{Pre}}_{s,t,m}$, $\ell \in [-11,-6]$), decision-window news ($\text{News}^{\text{Decision}}_{s,t,m}$, $\ell \in [-5,-1]$), and deliberation news ($\text{News}^{\text{Delib}}_{s,t,m}$, $\ell \geq 0$ before fiscal-year start). Panel B reports the in/out specification, where the coefficient on $\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}^{\text{Decision}}_{s,t}\}$ is the decision-window premium relative to pooled off-window news. The sample is a stacked monthly panel restricted to these three pre-fiscal-year windows, 2002–2024. All specifications include state and fiscal-year-by-calendar-month fixed effects and the predetermined controls from Table 2. Standard errors are two-way clustered by state and calendar month-year. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Window-specific responses

	Spending budget: $\log(\text{Budget}^E_t/\text{Budget}^E_{t-1})$	Spending growth: $\log(\text{Act}^E_t/\text{Budget}^E_{t-1})$
	(1)	(2)
$\text{News}^{\text{Pre}}_{s,t,m}$	-0.25	0.61
	(0.38)	(0.49)
$\text{News}^{\text{Decision}}_{s,t,m}$	1.20**	1.86***
	(0.46)	(0.55)
$\text{News}^{\text{Delib}}_{s,t,m}$	0.25	0.44
	(0.54)	(0.37)
State FE	✓	✓
FYear × Year-Month FE	✓	✓
Controls	✓	✓
Observations	17,034	17,034
Adj. R^2	0.145	0.614

Panel B: Decision-window premium

	Spending budget: $\log(\text{Budget}^E_t/\text{Budget}^E_{t-1})$	Spending growth: $\log(\text{Act}^E_t/\text{Budget}^E_{t-1})$
	(1)	(2)
$\text{News}_{s,t,m} \times \mathbf{1}\{m \in \mathcal{W}^{\text{Decision}}_{s,t}\}$	1.15**	1.36***
	(0.49)	(0.50)
State FE	✓	✓
FYear × Year-Month FE	✓	✓
Controls	✓	✓
Observations	17,034	17,034
Adj. R^2	0.145	0.614