Raise Fast, Cut Slow: Asymmetric Reversal of Recession-Driven State Tax Increases

Jan Philip Steitz*

October 17, 2026

Abstract

Under balanced-budget rules a recession should force states to adjust their tax rates, yet the most comprehensive study of state tax changes to date finds no effect of recessions on rate levels. I document a clear short-run response followed by faster and more frequent reversal, which narrows that puzzle without dissolving it. Using a panel of 50 U.S. states from 1960 to 2022, I show that recessions raise the probability of any major state tax increase by 7.7 percentage points while recoveries produce no offsetting cuts within three years. A formal test rejects equality of the recession increase and decrease responses at the 1% level, and rejects at the 5% level the equality of the recession increase and recovery decrease coefficients. Those increases are then cut sooner and more often, facing more than twice the hazard of a subsequent cut, and 28 percent survive a decade uncut against 62 percent of increases enacted in normal times. Completeness differs at five years but not detectably at ten, and the median increase of either kind still stands in full after a decade, so substantial rate persistence remains. The asymmetry is clearest in the timing and incidence of cuts rather than in how far they go. Separately, cumulative recession exposure is not associated with higher rate levels. Together these weigh against a permanent upward ratchet, though neither establishes that the subsequent cuts account for the absence of accumulation. The contemporaneous differential is a within-state gap between recession and other years, at 0.67 percentage points on the top income rate, rather than an accumulated stock.

Keywords: State Taxation, Business Cycles, Tax Asymmetry, Asymmetric Fiscal Adjustment, Fiscal Capacity

JEL Codes: H71, H72, E32, E62

*University of Wisconsin-Milwaukee. jsteitz@uwm.edu.

First version: February 2026. This paper circulated first under the title "The Fiscal Ratchet: Asymmetric State Tax Adjustment Over the Business Cycle" and subsequently as "Temporary Shocks, Permanent Taxes: Asymmetric Persistence in U.S. State Tax Policy". I am grateful to Sarah Robinson and Alisa Tazhitdinova for making their comprehensive tax dataset publicly available, to Alisa Tazhitdinova for helpful discussion and for sharing the replication package, to Scott Drewianka for helpful comments and guidance, and to seminar participants at the University of Wisconsin–Milwaukee.

1 Introduction

Do state tax rates respond to recessions? The fiscal-pressure logic of balanced-budget rules says they should, because a contraction in the tax base must be resolved within the budget year. Yet Robinson and Tazhitdinova (2025), in the most comprehensive study of state tax changes to date, find no effect of recessions on tax rate levels. This paper documents a short-run response and subsequent reversal patterns that narrow this puzzle without fully resolving it. Recessions raise the probability that a state raises a tax, and those increases are cut sooner and more often than increases enacted in normal times. Using a panel of all 50 U.S. states from 1960 to 2022,[1] I show that recessions raise the probability of any major tax increase by 7.7 percentage points, a 62 percent increase relative to the baseline rate, while recoveries produce no compensating tax cuts within three years. A formal test rejects equality of the recession increase and decrease responses at the 1 percent level, and rejects at 5 percent the equality of the recession increase coefficient and the recovery decrease coefficient. Both are comparisons of annual probabilities rather than tests of full fiscal offset, which would require the cumulated decrease response over the recovery to match the increase, so the cumulative evidence below carries that part of the argument. Those increases are then cut sooner and more often than increases enacted in normal times. Tracking each event forward, 28 percent of top marginal income tax increases enacted during a recession or its immediate recovery survive a decade uncut, against 62 percent of increases enacted in normal times, a gap of 34.6 percentage points significant at the 1 percent level (Table 4), and a Cox model allowing for events observed for less than a decade puts the hazard of a subsequent cut 2.4 times higher. How completely they are reversed is a separate question. The depth of reversal differs sharply at five years and is not statistically distinguishable at ten, where the median increase of either kind is still fully in place (Table 5), so substantial rate persistence remains after a decade and the asymmetry established here is in the timing and incidence of cuts rather than in their eventual depth. Cumulative recession exposure is accordingly not associated with higher rate levels (Table A.4). A design that tests for a lasting change in the rate level and one that tests for the contemporaneous response therefore target different estimands.

A central measurement choice distinguishes this paper from Robinson and Tazhitdinova (2025), who apply the Bry–Boschan turning-point algorithm to state real personal income. I instead measure recessions at the state level as years of negative real personal income growth, which captures the contemporaneous revenue shortfall that balanced-budget rules force a legislature to resolve inside the budget year rather than the peak-to-trough phase of the cycle. The two diverge sharply in practice. Because income contracts in different states at different times, the definition also generates cross-state variation within each year, which is what permits

[1] The panel is constructed from the state tax dataset of Robinson and Tazhitdinova (2025).

both state and year fixed effects. Section 5.3 sets out the choice and what turns on it. The empirical strategy is motivated by a simple model in which a crisis premium raises desired public spending during recessions while loss aversion over spending cuts prevents full reversal during recovery. Results are robust to alternative recession chronologies, including Bry–Boschan turning points applied to state income, NBER federal recession dates, and unemployment-based definitions, reported in Section 6.7, and the asymmetry survives all three. The ratchet operates primarily through income and sales taxes.[2] Pooled TWFE estimates indicate that the average top marginal income tax rate is 0.67 percentage points higher in recession state-years and 0.36 percentage points higher in recovery state-years than in baseline state-years. Partial reversal at the cross-sectional level emerges only after a full electoral cycle and is concentrated in states experiencing gubernatorial party turnover. Even then, the cross-sectional rate differential during recovery remains roughly half of the recession-period differential. The asymmetry extends to state spending, which also ratchets upward during recessions but reverts 74 percent over three years compared to only 46 percent for tax rates. This persistence gap is consistent with a legislative-action mechanism. Spending adjusts through administrative channels that do not require a vote, while reversing a tax increase requires a bill, a floor vote, and a governor's signature. The asymmetric response is concentrated in mild recessions, where federal intergovernmental transfers do not substitute for state fiscal effort, and in multi-year recession episodes, where sustained fiscal pressure overcomes legislative inertia. Electoral constraints suppress offsetting tax decreases.

This paper tests the ratchet hypothesis (Peacock and Wiseman 1961; Bird 1971, 1972; Higgs 1987) on legislated tax rates, the margin on which it should hold most strongly because reversal requires affirmative legislative action. The evidence does not support the permanent upward displacement the hypothesis predicts. Recession-driven increases are enacted quickly and cut slowly, in that no offsetting cuts appear within three years of the downturn, but they are cut, and cut more reliably than increases enacted in normal times, and recession exposure does not cumulate into higher rate levels. The positive finding is this delay followed by reversal. The methodological contribution is to decompose state tax adjustment into separate increase and decrease margins and to formally test for asymmetric persistence using a seemingly unrelated regression framework. The paper also documents a persistence gap between state spending and state taxation, concentrated on the margin where reversal requires legislative action. My analysis departs from Robinson and Tazhitdinova (2025) in two complementary directions. First, the *probability* of a tax change responds sharply and asymmetrically to recessions. States are 7.7 percentage points more likely to raise any tax during recession state-years and 3.5 percentage points less likely to cut income taxes in the first year of recovery, a

[2] Where I use "ratchet" it refers to the short-run asymmetry between the downturn and the first recovery years, in which increases are concentrated in recessions and are not reversed. It does not refer to the permanent expansion claimed by Peacock and Wiseman (1961) and Higgs (1987). Section 6.2 provides evidence against that expansion for legislated state tax rates.

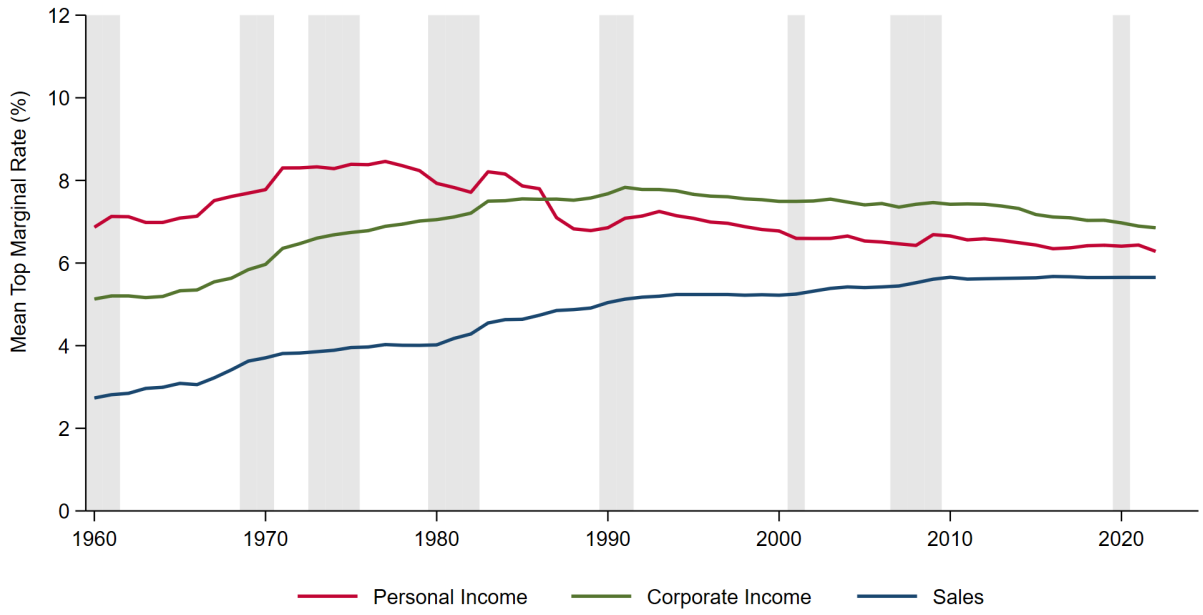


Figure 1: Mean Top Marginal Tax Rates Across States, 1960–2022

Notes: The figure plots the mean top marginal tax rate across all 50 states for personal income, corporate income, and general sales taxes. Gray shaded bars indicate federal recession periods. *Source:* Robinson and Tazhitdinova (2025).

margin their rate-level analysis does not address. Second, pooled estimates show a contemporaneous rate-level differential between recession and non-recession state-years. Their design and mine target different estimands and their recession indicator is a different object from mine, and Section 5.3 separates the two layers and reports what each contributes. The probability ratchet survives the full 1929–2022 sample even as the rate-level effect is concentrated in the post-1960 era. In an era when tax competition (Poterba 1994, 1996), taxpayer revolts, and institutional constraints all push rates downward, recession-driven increases are one of the few systematic forces operating in the opposite direction, even though their imprint on the rate level is temporary.

The paper proceeds as follows. Section 2 reviews the relevant literature. Section 3 describes the data. Section 4 presents the theoretical model. Section 5 outlines the empirical strategy. Section 6 reports the results. Section 7 discusses the findings, and Section 8 concludes.

2 Literature Review

Hercowitz and Strawczynski (2004) provide the closest empirical predecessor to this paper, documenting cyclical ratcheting in government spending across OECD countries, finding that expenditures rise during downturns and do not fully revert during recoveries, producing an upward drift in the spending-to-GDP ratio over successive business cycles. However, their cross-country design cannot control for country-specific macroeconomic shocks, and they examine spending rather than legislated tax rates, where reversal requires affirmative political action. Jaén-García (2021) tests both hypotheses for Spain over a long horizon, finding support for displacement but not for the ratchet in aggregate expenditure. At the subnational level, Sorensen and Yosha (2001) examine whether U.S. state fiscal policy is asymmetric over the business cycle using aggregate revenue and expenditure responses to changes in gross state product. They find significant asymmetries in revenue and expenditure individually but roughly symmetric adjustment in the budget surplus, concluding that state fiscal policy mutes booms and slowdowns to similar degrees. They attribute this pattern to passive rate dynamics, with tax rates held roughly fixed during downturns and slowly reduced during upturns. The present paper reinterprets their aggregate finding. The persistent revenue decline they document in downturns and the subsequent reversion in upturns is consistent with an active, one-directional ratchet in which states raise tax rates during recessions but do not reverse those increases during recoveries, a mechanism that is only visible when examining legislated tax instruments rather than aggregate revenue flows. Their analysis focuses on aggregate fiscal flows rather than legislated tax rates, and their sample ends in 1998, before the post-2000 era in which the ratchet is most pronounced. McGuire and Merriman (2006) similarly examine state social assistance spending from 1979 to 2003, finding that public welfare expenditures became more counter-cyclical after the 1996 welfare reform, a pattern they attribute in part to federal Medicaid match increases during fiscal downturns. These studies focus exclusively on the spending side or on aggregate fiscal flows. Vegh and Vuletin (2015) shift attention to tax rates, building a cross-country dataset of corporate, personal income, and value-added tax rates for 62 countries and finding that tax policy is acyclical in industrial countries but procyclical in developing countries. Their analysis examines the average cyclical response, however, and does not test for the asymmetry between recessions and recoveries that defines the ratchet. Whether tax rates, the most visible margin of state fiscal adjustment and one that faces countervailing downward pressure from interstate competition (Wilson 1999; Agrawal et al. 2022), ratchet asymmetrically over the business cycle has not been formally tested.

Poterba (1994, 1995) show that states adjust taxes and spending in response to deficit shocks and that balanced budget rules accelerate this adjustment, while Alt and Lowry (1994) document that divided government slows it. Clemens and Miran (2012) show that balanced-budget requirements generate pro-cyclical

spending and use the resulting variation to estimate fiscal multipliers, and Baker et al. (2025) document that tax changes are highly correlated across tax types, with personal income, corporate, and sales tax increases frequently occurring simultaneously, meaning estimated effects of any single tax may partly reflect a broader fiscal package. This body of work establishes that states respond to downturns, but does not test whether those responses are reversed during subsequent recoveries, the defining feature of the ratchet.

Robinson and Tazhitdinova (2025) provide a comprehensive descriptive analysis of the same dataset, documenting the evolution of state tax rates and revenues since 1910 and the simultaneity of tax changes across tax types. As part of a broader assessment of whether economic conditions drive tax changes, they find no significant effect of state recessions on tax rate levels under their Bry–Boschan recession definition. The two approaches are complementary, and the reconciliation is developed in Section 5.3 with additional evidence on the federal intergovernmental transfer mechanism in Section 6.5. In a companion paper, Robinson and Tazhitdinova (2022) apply Shapley decomposition and machine learning methods to the same dataset and find that commonly cited economic, political, and institutional factors explain less than 20 percent of the variation in the timing and magnitude of state tax changes. Notably, they find that tax increases are systematically easier to predict than tax decreases, with economic conditions playing a larger role for increases. They suggest that increases may be driven by fiscal need while decreases are more likely ideologically motivated. This independently documented asymmetry is precisely the pattern that the ratchet hypothesis predicts.

Several mechanisms from the political economy literature predict the asymmetry documented here. Status quo bias arising from individual-specific uncertainty about reform consequences (Fernandez and Rodrik 1991), strategic delay in fiscal adjustment (Alesina and Drazen 1991), and constraints on politically feasible rate changes (Bierbrauer et al. 2021) all predict that coalitions supporting crisis-driven increases need not support symmetric reversal. On the supply side, fiscal illusion and budget-maximizing bureaucrats may exploit crises to lock in higher spending (Buchanan and Wagner 1977; Niskanen 1971). Procedural constraints on tax increases may paradoxically *concentrate* increases into recessions, the one context in which political resistance is lowest (Mullins and Wallin 2004), a prediction this paper tests directly.

3 Data

I use the dataset compiled by Robinson and Tazhitdinova (2025), which covers state tax rates, revenue, expenditure, and institutional variables for all 50 U.S. states from 1910 to 2022.[3] The tax rate variables

[3]Data available at https://alisatns.weebly.com/.

include personal income (top and minimum), corporate income, sales, cigarette, gasoline, and alcohol spirit tax rates. Personal income and sales taxes together account for roughly two-thirds of state tax revenue by 2022 (Figure A.1), motivating the focus on these instruments. The dataset also includes state personal income, state expenditure by category, and time-varying institutional indicators for supermajority requirements, rainy day fund provisions, and balanced-budget rules. The divided government indicator is drawn from Robinson and Tazhitdinova (2025) and is available through 2022. Gubernatorial election years, gubernatorial party turnover, and partisan composition of state legislatures are drawn from the State Partisan Balance dataset (Klarner 2013), which provides annual measures of partisan control for all states from 1937 to 2011. The political economy specifications in Section 6.9 are accordingly restricted to the 1960–2011 subsample.

The primary outcome variables are binary indicators equal to one if state i changed a major tax rate (sales, personal income, or corporate income) in year t, constructed by comparing each rate to its lagged value. I construct separate indicators for increases and decreases, as well as a composite indicator equal to one if the state raised (or cut) any of the three major taxes. Tax changes are often implemented simultaneously: Robinson and Tazhitdinova (2025) document that 34 percent of reforms involve two or more rates, and Baker et al. (2025) reach similar conclusions. The composite indicator is therefore the preferred outcome, capturing the broad fiscal packages states typically enact under pressure rather than isolating individual tax types. I also examine tax rate levels directly. The sample covers 50 states from 1960 to 2022, the period in which most states had adopted their major tax instruments (Figure A.2), yielding 3,150 state-year observations containing 345 state-specific recession episodes. The sample includes state-years in which a given tax rate is zero. From the perspective of fiscal burden, a state that introduces a new tax at a given rate during a recession imposes the same marginal increase on taxpayers as a state that raises an existing rate by the same amount. Both are recession-driven tax increases, and both enter the survival analysis of Section 6.2.[4]

I define a state-specific recession as a year of negative real personal income growth. This definition generates variation across states within each year. In any given year, some states experience income contractions while others do not, permitting the inclusion of year fixed effects that absorb all common macroeconomic shocks.[5] Recovery is defined as the first three years following the end of a recession episode. In the phase

[4]Results using the full sample from 1929 onward are reported as a robustness check and are qualitatively similar, though attenuated by structural zeros in the pre-adoption period.

[5]This contrasts with both the Bry–Boschan method applied to state-level income by Robinson and Tazhitdinova (2025), which identifies turning points rather than revenue shortfalls, and NBER federal recession dates, which vary only at the national level. The negative-growth definition is simpler, captures mild downturns that turning-point algorithms may miss (Brown 2017), and generates substantially more identifying variation.

decomposition, I also test each recovery year separately (recovery years 1, 2, and 3) to assess whether the probability of tax decreases rises at any point during the recovery window.

Table 1: Descriptive Statistics by Recession Phase

	(1) Normal		(2) Recession		(3) Recovery	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Any major tax increased	0.12	0.32	0.17	0.38	0.12	0.33
Any major tax decreased	0.12	0.32	0.11	0.32	0.12	0.33
Sales tax rate increased	0.06	0.24	0.08	0.27	0.06	0.24
Sales tax rate decreased	0.01	0.11	0.01	0.10	0.01	0.11
Top income tax rate increased	0.04	0.19	0.09	0.28	0.05	0.22
Top income tax rate decreased	0.08	0.26	0.09	0.28	0.07	0.26
Top corporate tax rate increased	0.06	0.23	0.07	0.25	0.05	0.21
Top corporate tax rate decreased	0.05	0.21	0.05	0.22	0.06	0.25
Sales tax rate	4.07	1.97	4.27	1.95	4.29	1.98
Top marginal personal income tax rate	5.38	3.73	6.17	4.18	5.95	4.03
Top marginal corporate income tax rate	5.82	2.99	6.62	2.98	6.44	3.05
Log(income per capita)	9.48	1.07	9.84	0.92	9.77	0.97
Log(population)	14.97	1.00	14.88	1.11	14.92	1.08
Observations	1849		463		838	

Notes: The sample covers 1960 to 2022. A recession is defined as a period in which state real income growth is negative. Recovery refers to the one-to-three-year period following the end of a recession. *Source:* Robinson and Tazhitdinova (2025).

Table 1 reports summary statistics by business cycle phase. During normal years, the probability that a state raises any major tax (sales, top personal income, or top corporate income) is 12%. This rises to 17% during state-level recessions, a 42% increase, before returning to 12% in the recovery period. The probability of a tax decrease, by *contrast*, is essentially flat across all three phases (12%, 11%, and 12%, respectively). The raw means thus already suggest asymmetry. Recessions trigger tax increases but recoveries do not trigger offsetting decreases.

4 A Simple Model of the Tax Ratchet

The ratchet hypothesis, that government taxation expands during crises and fails to contract fully afterward, originates with Peacock and Wiseman (1961) and was refined by Bird (1972) and Higgs (1987). The empirical patterns documented in this paper are consistent with several theoretical mechanisms, including status quo bias (Fernandez and Rodrik 1991), strategic delay (Alesina and Drazen 1991), and bureaucratic budget maximization (Niskanen 1971). The reduced-form pattern documented below, in which recession-driven increases are not reversed by the recovery coalition, is the empirical signature predicted by all of these

mechanisms and does not discriminate between them. I present one simple formalization based on a crisis premium and loss aversion that captures the core asymmetry and generates testable predictions. Results would be qualitatively identical under a Bierbrauer et al. (2021) framework in which the post-recession status quo embeds spending commitments that render symmetric tax reversal politically infeasible. The loss-aversion formulation is chosen for tractability rather than to take a stand on the underlying microfoundation.

4.1 Environment

Consider a representative state over three periods $t \in \{0, 1, 2\}$, corresponding to a pre-recession normal, a recession, and a recovery. State income is

$$y_t = \begin{cases} y & \text{if } t \in \{0, 2\} \\ y - \delta & \text{if } t = 1 \end{cases}$$

where $\delta \in (0, y)$ measures recession severity. The government operates under a balanced budget, so public spending equals tax revenue: $g_t = \tau_t \cdot y_t$, where $\tau_t \in [0, 1]$ is the tax rate.

A representative voter evaluates each period according to

$$V_t = u(g_t) + \varphi_t + (1 - \tau_t)y_t - \lambda \max\{g_{t-1} - g_t,\ 0\} \tag{1}$$

where $u(\cdot)$ is strictly concave with $u'(0) = \infty$. The non-standard terms capture the channels that generate the ratchet:

- $\varphi_t \geq 0$ is the *crisis premium* on public spending. Recessions raise demand for public services and create fiscal pressure under balanced-budget requirements, so $\varphi_1 = \varphi > 0$ and $\varphi_0 = \varphi_2 = 0$.
- $\lambda \geq 0$ is the *loss-aversion parameter* for spending reductions below the reference level g_{t-1}. Once spending programs are established, constituencies form around them and voters treat the prevailing level as the status quo (Kahneman and Tversky 1979). Cutting below g_{t-1} is politically costly. Expanding beyond it carries no additional benefit.

The government maximizes V_t myopically each period, which is appropriate for state governments operating under balanced-budget requirements and annual legislative cycles. A forward-looking legislature that anticipated the political cost of future reversal might set a lower τ_1 to begin with, attenuating but not eliminating the asymmetry, because so long as $\lambda > 0$, the recovery rate remains above its pre-recession level. More

fundamentally, even if individual legislators are forward-looking, collective action problems and veto players generate behavior that resembles myopic optimization in equilibrium (Alesina and Drazen 1991). The myopic case is therefore best understood as a useful benchmark that isolates the core mechanism while remaining consistent with the observed pattern of large recession-driven increases followed by incomplete reversal.

4.2 Equilibrium Tax Rates

Period 0 (normal). The economy begins in steady state with $g_{-1} = g^*$, so the loss-aversion term does not bind. With no crisis premium, the government maximizes $u(\tau \cdot y) + (1 - \tau) \cdot y$. The first-order condition is $u'(g_0) = 1$, yielding the optimal spending level $g^* \equiv u'^{-1}(1)$ and tax rate $\tau^* = g^*/y$.

Period 1 (recession). Income falls to $y - \delta$ and the crisis premium activates. Since $\varphi > 0$ raises the marginal value of public spending, the government increases spending above g^*, so $g_1 > g_0$ and the loss-aversion term does not bind. The first-order condition is

$$u'(g_1) + \varphi = 1 \quad \Longrightarrow \quad g_1 = u'^{-1}(1 - \varphi) > g^*$$

since $\varphi > 0$ and $u'' < 0$. The tax rate rises to

$$\tau_1 = \frac{g_1}{y - \delta} = \frac{u'^{-1}(1 - \varphi)}{y - \delta} > \tau^*$$

for two reinforcing reasons. The tax base shrinks (denominator falls) and desired spending increases (numerator rises).

Period 2 (recovery). Income returns to y and the crisis premium vanishes ($\varphi_2 = 0$). The reference level is now $g_1 > g^*$. Two cases arise depending on the strength of loss aversion relative to the crisis premium.

Case 1: $\lambda < \varphi$ (interior solution). The government finds it optimal to cut spending below g_1, so the loss-aversion penalty binds. Substituting $g_2 = \tau_2 \cdot y$, the first-order condition for $g_2 < g_1$ is

$$u'(g_2) - 1 + \lambda = 0 \quad \Longrightarrow \quad g_2 = u'^{-1}(1 - \lambda).$$

Since $\lambda < \varphi$ and u' is decreasing, $g^* < g_2 < g_1$.

Case 2: $\lambda \geq \varphi$ (corner solution). When loss aversion is at least as large as the crisis premium, the objective has a kink at $g_2 = g_1$ and the government maintains recession-era spending in full.

Proposition 1 (Short-Run Tax Asymmetry)**.** *If $\lambda > 0$, the recovery tax rate exceeds the pre-recession rate: $\tau_2 > \tau^* = \tau_0$.*

(i) ***Partial ratchet*** *($\lambda < \varphi$). Spending partially reverts: $g^* < g_2 < g_1$, with*

$$\tau_2 = \frac{u'^{-1}(1-\lambda)}{y}, \qquad \tau^* < \tau_2 < \tau_1.$$

(ii) ***Complete ratchet*** *($\lambda \geq \varphi$). Spending is fully maintained at the recession level: $g_2 = g_1$, with*

$$\tau_2 = \frac{u'^{-1}(1-\varphi)}{y}, \qquad \tau^* < \tau_2 < \tau_1.$$

In both cases, the recovery tax rate remains strictly below the recession-era rate ($\tau_2 < \tau_1$) because the tax base recovers ($y > y - \delta$) even when spending does not fall. If $\lambda = 0$, the tax rate returns to τ^* and no ratchet occurs.

4.3 Mild versus Severe Recessions

If status quo bias is strongest when fiscal changes are least salient, $\lambda(\delta)$ is decreasing in δ. This generates a mild–severe asymmetry confirmed by the continuous severity estimates in Table A.14.

Corollary 1 (Mild–Severe Asymmetry)**.** *Let $\lambda'(\delta) < 0$. Then the ratchet is stronger for mild recessions:*

$$\frac{\tau_2(\delta_{mild})}{\tau_0} > \frac{\tau_2(\delta_{severe})}{\tau_0} \quad \text{for } \delta_{mild} < \delta_{severe}.$$

In the limit, if $\lambda(\delta) \to 0$ for large δ, severe recessions produce a fully symmetric response ($\tau_2 \to \tau_0$).

4.4 Empirical Predictions

The model describes a single recession and its recovery, so it speaks to the short-run asymmetry between the downturn and the immediate recovery rather than to what happens to an increase over the following decade. It generates the three short-run predictions below. The paper's second result, that recession-era increases are eventually reversed more often than increases enacted in normal times, is established empirically in Section 6.2 rather than derived here, and is listed fourth to make the mapping between theory and evidence explicit:

1. *Recession increases*: P(tax increase) rises during recessions.

2. *Incomplete short-run reversal*: P(tax decrease) does *not* rise during the first recovery years, and recovery-era rates remain above the pre-recession level.

3. *Mild–severe asymmetry*: The ratchet is concentrated among mild recessions. Severe recessions produce attenuated or symmetric effects.

4. *Reversibility of crisis-driven increases* (empirical): The crisis premium that motivates a recession increase is transitory, so increases enacted during recessions are reversed more often at longer horizons than increases enacted in normal times. The model rationalizes this intuition but cannot derive it, because a single-episode model contains no normal-times increase to serve as a comparison.

The model is best read as a short-run mechanism that rationalizes the contemporaneous asymmetry. The empirical strategy that follows tests the first three predictions directly and establishes the fourth as a separate result.

5 Empirical Specification and Identification

The empirical strategy estimates the recession effect on state tax outcomes using a panel of 50 U.S. states from 1960 to 2022. Identification exploits idiosyncratic state-specific income contractions conditional on state and year fixed effects, with the identifying assumption that the timing of these contractions is uncorrelated with unobserved determinants of tax changes after absorbing common-year shocks and time-invariant state characteristics. The remainder of the section presents the main regression specifications, discusses the identifying assumptions and the choice of recession indicator, and develops the estimand framing that reconciles the pooled TWFE estimates with the per-episode null reported by Robinson and Tazhitdinova (2025).

5.1 Empirical Specification

The main specification is a linear probability model with two-way fixed effects:

$$Y_{it} = \alpha_i + \beta \cdot \text{Recession}_{it} + X_{it}'\gamma + \delta_t + \varepsilon_{it} \tag{2}$$

where Y_{it} is an indicator for a tax increase (or decrease) in state i in year t, α_i are state fixed effects, δ_t are year fixed effects, and X_{it} includes log real income per capita, which absorbs cross-state differences in income

levels conditional on the year and is distinct from the growth-based recession indicator, log population, and a divided government indicator. Standard errors are clustered at the state level (Bertrand et al. 2004).[6]

The ratchet hypothesis predicts $\beta > 0$ when Y_{it} measures tax increases and $\beta \leq 0$ when it measures tax decreases. To test this asymmetry directly, I replace the single recession indicator with separate dummies for the recession period and recovery years 1–3, allowing me to assess whether tax decreases rise during recovery (the symmetric prediction) or remain flat (the ratchet prediction).

To trace the dynamic response and validate the identifying assumption of no pre-trends, I estimate an event study around recession onset:

$$Y_{it} = \alpha_i + \sum_{\substack{k=-3 \\ k\neq -1}}^{7} \beta_k \cdot \mathbf{1}\left[t - t_i^{\text{onset}} = k\right] + X_{it}'\gamma + \delta_t + \varepsilon_{it} \tag{3}$$

where t_i^{onset} is the year of recession onset and $k = -1$ is the omitted reference period. The lead coefficients β_{-3} and β_{-2} test for differential pre-trends. The lag coefficients β_0 through β_7 trace the dynamic path of the ratchet over the seven years following onset. Because onset is defined as the first year of negative real income growth, the reference period $t-1$ is a positive-growth year by construction while the lead periods carry no such restriction, so all coefficients are measured relative to a year that differs systematically from the rest of the window. This asymmetry is discussed further in Section 6.3 and in the notes to Table A.2.

I complement these reduced-form estimates with three extensions. First, I estimate the increase and decrease equations jointly via seemingly unrelated regression (Zellner 1962) and test the cross-equation restriction $\beta^{\text{inc}}_{\text{recession}} = \beta^{\text{dec}}_{\text{recovery}}$. Rejection implies the asymmetric response that defines the ratchet. Second, I estimate equation (2) with tax rate levels (in percentage points) as the dependent variable, testing whether rates are elevated during and after recessions. Third, I interact the recession indicator with time-varying institutional features, supermajority requirements, rainy day funds (Sobel and Holcombe 1996), and deficit carry-forward bans (Bohn and Inman 1996), to test whether these institutions moderate the ratchet.

5.2 Identification

The empirical design follows the standard two-way fixed effects approach used widely in the state fiscal policy literature (Poterba 1994; Clemens and Miran 2012), relying on within-year, across-state variation in

[6] I estimate linear probability models throughout. The LPM is preferred because fixed effects logit drops states with no within-state variation in the outcome, substantially reducing the sample. Results are qualitatively robust to logit specifications where feasible (Angrist and Pischke 2009). All specifications are estimated using `reghdfe` in Stata (Correia 2017). Results are robust to two-way clustering by state and year. Standard errors change modestly in both directions but no result changes significance level.

recession exposure for identification. State fixed effects absorb time-invariant state characteristics, including industrial composition and baseline fiscal capacity, ensuring that the recession coefficient is not driven by the selection of structurally recession-prone states into treatment. Year fixed effects absorb all shocks common to a given year. Because the recession indicator is defined at the state level, it varies across states within the same year. In any given year some states cross the zero-growth threshold while others do not, and this cross-state variation conditional on the year is what identifies the coefficient. The identifying assumption is that, conditional on these fixed effects and time-varying controls, the timing of state-specific income contractions is uncorrelated with unobserved determinants of tax changes. The primary threat is reverse causality, which I address in Section 6.6. A related concern is that unobserved political-economy shifts might simultaneously predict both economic downturns and tax increases. As a direct test, I re-estimate the baseline specification using NBER federal recession indicators in place of the state-level fiscal-pressure measure (Table A.11). Federal recession timing is determined by national macroeconomic conditions and is plausibly orthogonal to state-level political dynamics. The estimated effect on the probability of any major tax increase is 4.6 percentage points (significant at the 1 percent level), and the top marginal income tax rate is 0.23 percentage points higher in federal recession state-years (significant at the 1 percent level). Both effects confirm the baseline result with an arguably exogenous source of cyclical variation.

5.3 Recession Definitions and the Cyclical Identification of Fiscal Pressure

Different recession definitions are appropriate for different research questions. A definition aligned with cyclical turning points identifies the macroeconomic chronology of expansion and contraction, whereas a definition aligned with contemporaneous fiscal pressure identifies the years in which state revenue contracts in real terms and balanced-budget rules force legislative adjustment. The choice of indicator therefore selects which part of the cyclical distribution receives weight, and different parts of this distribution produce different estimands. This subsection makes the connection between recession definition, estimator, and target estimand explicit.

I use negative real personal income growth for two reasons. First, it captures the mechanism that triggers legislative action, since under balanced-budget requirements a contraction in the tax base forces same-year budget adjustment, while a turning point identified by Robinson and Tazhitdinova (2025)'s Bry–Boschan chronology may fall in a year of positive growth in which no such pressure exists. Two-thirds of Bry–Boschan recession-years in this sample do exhibit positive real income growth. Second, a contemporaneous contraction must be resolved by the sitting legislature in the current budget cycle, whereas an incumbent who may not be in office when a forecasted downturn arrives has little incentive to act on it, so the negative-growth definition

is aligned with the timing of legislative response and business-cycle dating is aligned with macroeconomic conditions. A severity threshold runs the opposite risk. Restricting attention to episodes below −3% growth, as Robinson and Tazhitdinova (2025) do in an auxiliary specification, excludes the milder downturns that still create fiscal pressure, and the continuous severity estimates in Table A.14 place the ratchet among mild recessions and attenuating at higher severity, so a turning-point chronology and a severe-episode threshold understate the effect from opposite directions. Section 6.7 reports the estimates under Bry–Boschan turning points, NBER federal dates and unemployment-based definitions.

The pooled two-way fixed effects estimator in Table 6 reports the average within-state difference in tax rate levels between recession or recovery state-years and baseline state-years, conditional on state and year fixed effects. This is a contemporaneous contrast and not an accumulated stock. Recession and recovery state-years carry higher rates than baseline state-years because increases are concentrated in the downturn and are not reversed within the first recovery years. Cumulative recession exposure is not associated with higher rate levels (Table A.4), and increases enacted during recessions meet subsequent cuts sooner and more often than increases enacted in normal times (Table 4). These are separate pieces of evidence rather than one result shown twice. The 0.67 percentage point differential therefore measures the elevation that prevails while a state is in or just emerging from a downturn, not a permanent upward shift carried forward across episodes. Clean-control stacked difference-in-differences instead isolates the path of rates around a single recession episode within a narrow event window, netting out pre-existing levels through cohort-specific fixed effects. The two estimators recover different objects, both valid for their respective questions. As supplementary robustness, I apply Wing-Freedman-Hollingsworth (2024)-weighted stacked DiD to the probability outcomes, which are flow rather than stock variables, and the asymmetric probability ratchet survives at the one percent level (Appendix Table A.23).

This framing offers a complementary reading of the null on rate levels reported by Robinson and Tazhitdinova (2025). A third layer sits beneath the definition and the estimator. Their adopter-only sample restriction excludes pre-adoption state-years in which late adopters introduced new income tax systems in response to fiscal pressure, removing one channel through which the ratchet operates.

To isolate the recession-definition layer empirically, I apply Robinson and Tazhitdinova (2025)'s exact stacked DiD specification to the same data, swapping only the recession indicator from Bry–Boschan to negative real income growth (Appendix Table A.24). Significant rate-level effects on the top marginal income tax rate emerge at $t + 3$ and $t + 4$ under conventional significance levels, with similar patterns for sales tax increases and income tax decreases. Within their stacked design, the recession definition is therefore the

source of the divergence on rate levels. The converse also holds. Holding the Bry–Boschan definition fixed and moving from their stacked design to pooled TWFE takes the recession-year rate coefficient from a null to 0.64 percentage points (Table A.12, Panel D), so the definition and the estimator each account for the divergence in one dimension and the two layers are not interchangeable.

6 Results

6.1 Main Results

Table 2 presents two-way fixed effects estimates of the recession effect on the probability of tax change. Being in a recession raises the probability of any tax increase by 7.2 percentage points, which is statistically significant at the 1% level (the phase decomposition in Table 3, which separates recovery from recession, yields a slightly larger estimate of 7.7 percentage points), while simultaneously reducing the probability of *any* tax decrease by 4.0 percentage points, statistically significant at the 10% level. The effect is largest for personal income taxes at 5.9 percentage points, significant at the 1% level,[7] followed by general sales taxes at 3.3 percentage points, significant at the 5% level. Corporate tax increases also occur during recessions, though the estimate is insignificant. However, given the simultaneity of tax changes documented by Robinson and Tazhitdinova (2025) and Baker et al. (2025), the tax-specific coefficients should be interpreted with caution, as each may partly reflect the broader fiscal package rather than the isolated effect of that tax type.

Table 2: The Effect of Recessions on State Tax Changes

	(1) Any Inc	(2) Any Dec	(3) Sales Inc	(4) Sales Dec	(5) Income Inc	(6) Income Dec	(7) Corp Inc	(8) Corp Dec
During Recession	0.0723***	-0.0398*	0.0334**	-0.0035	0.0587***	-0.0171	0.0150	-0.0176
	(0.0220)	(0.0213)	(0.0147)	(0.0087)	(0.0189)	(0.0201)	(0.0183)	(0.0148)
Observations	3150	3150	3150	3150	3150	3150	3150	3150
Adj. R^2	0.107	0.093	0.061	0.015	0.083	0.080	0.065	0.057

Notes: The sample covers 1960 to 2022. All specifications include state and year fixed effects. A recession is defined as a period in which state real income growth is negative. Controls include the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Table 3, I decompose the effect into recession and recovery phases. The coefficient on the probability of any increase during the recession remains large, at 7.7 percentage points, and statistically significant at the 1% level. None of the recovery-year indicators are significant for tax increases, indicating that the elevated probability of a tax hike does not persist beyond the recession itself. The first year of recovery shows a

[7] Eleven income tax increases, 7.1 percent of the total, reflect initial tax adoption rather than rate changes. Excluding these events yields a coefficient of 0.071, significant at the 1% level, statistically indistinguishable from the baseline.

modest but insignificant effect. By the second and third years, the coefficients are essentially zero. For tax decreases, recovery coefficients on the aggregate measure are uniformly small and insignificant. There is no compensating wave of tax cuts during recovery. For income taxes specifically, the ratchet is even starker. States are 3.5 percentage points *less* likely to cut income taxes in the first year of recovery than in normal years, significant at the 5% level. Rather than reversing recession-driven increases, states become less willing to lower income tax rates during recovery, reinforcing the asymmetry at the core of the ratchet hypothesis.

Table 3: Recession Phases, During vs Recovery

	(1) Any Inc	(2) Any Dec	(3) Sales Inc	(4) Sales Dec	(5) Income Inc	(6) Income Dec	(7) Corp Inc	(8) Corp Dec
During Recession	0.0766***	-0.0416*	0.0391**	-0.0024	0.0630***	-0.0234	0.0130	-0.0108
	(0.0222)	(0.0229)	(0.0162)	(0.0090)	(0.0192)	(0.0218)	(0.0187)	(0.0155)
Recovery +1yr	0.0239	-0.0182	0.0182	0.0049	0.0153	-0.0354**	0.0046	0.0190
	(0.0216)	(0.0258)	(0.0183)	(0.0063)	(0.0173)	(0.0173)	(0.0173)	(0.0210)
Recovery +2yr	-0.0048	-0.0028	-0.0018	0.0094	0.0048	-0.0166	-0.0153	0.0073
	(0.0238)	(0.0278)	(0.0204)	(0.0117)	(0.0154)	(0.0204)	(0.0146)	(0.0182)
Recovery +3yr	0.0052	0.0144	0.0216	-0.0064	0.0087	0.0146	-0.0088	0.0219
	(0.0266)	(0.0234)	(0.0197)	(0.0081)	(0.0185)	(0.0195)	(0.0175)	(0.0174)
Observations	3150	3150	3150	3150	3150	3150	3150	3150
Adj. R^2	0.107	0.093	0.061	0.015	0.082	0.081	0.064	0.057

Notes: The sample covers 1960 to 2022. The ratchet hypothesis predicts that tax increases are more likely during recessions and that tax decreases do not follow during recoveries. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 2 plots the event-study coefficients around recession onset.[8] The pre-trend coefficients at $t-3$ and $t-2$ are small and statistically insignificant for both P(increase) and P(decrease), supporting the parallel-trends assumption. P(any increase) spikes by 7.6 percentage points at $t+1$, significant at the 1% level, consistent with a one-year legislative lag between the economic shock and enacted tax changes. The effect becomes statistically insignificant by $t+2$. P(any decrease) shows no spike at any horizon near onset. A modest, weakly significant increase of 4 to 5 percentage points emerges only at $t+5$ through $t+7$, far too late and too small to offset the recession-driven increases. The timing of this delayed response is consistent with the electoral cycle, as tax increases enacted during a recession may face partial reversal only once a new governor or legislature takes office, typically 4 to 6 years later. The event study reveals a delayed partial reversal. Beginning at $t+5$, the probability of a tax increase falls below baseline while the probability of a tax decrease rises, with both effects statistically significant. Within the three-year recovery window the reversal is incomplete. Recovery state-years exhibit average top income tax rates 0.36 percentage points above the

[8]The exact coefficients are reported in Table A.1 in the Appendix.

baseline state-year level, roughly half of the recession-period differential. The rate-level event study traces a hump. The top marginal income rate rises to 0.53 percentage points above the reference period at $t+1$, declines through $t+4$, and the coefficients at $t+6$ and $t+7$ are 0.03 and -0.02 (Figure A.3 and Table A.2). The late coefficients are imprecisely estimated and cannot distinguish full reversion from partial persistence, and the lead coefficients are positive rather than flat, so the event path is best read as descriptive of the shape rather than as the basis for the reversal claim. That claim rests on Table 4, where the comparison is between recession-era and normal-times increases rather than against an event-time reference period. The recovery-window elevation is consistent with the partial ratchet in the model ($\lambda < \varphi$). The longer-horizon unwinding falls outside the single-episode model, which speaks only to the contemporaneous asymmetry, and is documented empirically rather than predicted.

6.2 Reversal of Recession-Era Increases

The probability results establish that recessions produce tax increases and that recoveries do not produce offsetting cuts within three years. They do not establish what becomes of those increases afterward. I test this directly at the level of the individual tax-increase event. For each increase I record whether it was enacted during a recession or its first three recovery years, and I follow the rate forward to determine whether any rate cut follows. Table 4 reports the share of increases on which no cut of any size occurs over a ten-year horizon. Across all four instruments, increases enacted during recessions are cut more often than increases enacted in normal times. For the top income tax rate, 62 percent of normal-times increases survive a decade uncut against 28 percent of recession-linked increases, a gap of 34.6 percentage points that is significant at the 1 percent level and robust to state and year fixed effects. The same gap is present for the sales and corporate rates, and is statistically strongest for the income tax. Recession-era increases are not larger than normal-times increases, and increase size does not predict survival, so the pattern is not driven by the magnitude of the adjustment. The gap is stable across alternative definitions of the recession-linked window, ranging from the recession year alone to five recovery years, and under the Bry-Boschan recession classification, and it rests on 146 income increase events across 36 states (Appendix Table A.3).

That measure counts a cut of any size, so it speaks to whether an increase is touched rather than to how much of it survives. Table 5 separates the two. On the incidence measure the recession-normal gap is -34.6 percentage points. On whether the rate still stands at its post-increase level it is -20.0 points and significant at 5 percent, so recession-era increases are also more eroded. On the two measures of how far the reversal goes the answer depends on the horizon. At five years both differ sharply. The share of increases returned to or below their pre-increase level is 19.8 points higher for recession-era increases and the mean fraction

Table 4: Reversal of Tax Increases by Enactment Phase

	Income	Sales	Corporate
Share of increases surviving uncut after 5 years			
Enacted in normal times	0.758	0.879	0.786
Enacted during a recession	0.458	0.787	0.588
Share of increases surviving uncut after 10 years			
Enacted in normal times	0.621	0.838	0.680
Enacted during a recession	0.275	0.687	0.478
Recession − normal difference (10-year)	-0.346***	-0.151*	-0.202***
	(0.082)	(0.084)	(0.069)
Number of increase events	146	188	170

Notes: Each observation is a tax-increase event over 1960 to 2022. An increase is counted as surviving if no subsequent rate cut of any size occurs within the stated horizon, which measures whether an increase is cut rather than how much of it is undone. Table 5 separates incidence from depth for the income rate. An increase is classified as enacted during a recession if it occurred in a recession year or the first three recovery years, and as normal times otherwise. The difference row reports the recession-minus-normal gap in the ten-year survival share, with the gap testing whether recession-era increases are reversed more often. Income refers to the top marginal personal income tax rate. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

still standing is 0.368 lower, with p-values of 0.001 and 0.005. At ten years neither difference is statistically detectable. The completed-reversal share differs by 9.1 points with a p-value of 0.281 and the mean fraction still standing by 0.213 with a p-value of 0.293, and the median increase of either kind is still fully in place. Those are failures to reject rather than demonstrations of equality, and with 146 events the ten-year tests have little power against differences of the size estimated at five years. Reading across the panels, recession-era increases are cut more often and sooner and are less likely to stand at full height, while the evidence that they are eventually undone more completely weakens as the horizon lengthens.

Two features of the design deserve statement. Eight of the 154 income increases were enacted after 2012 and cannot be followed for a full decade, so the ten-year columns rest on 146 events. Treating those eight as censored rather than dropping them, a Kaplan-Meier estimate of the time to the first cut gives survivor functions of 0.623 and 0.297 at ten years, almost exactly the complete-case figures, and a log-rank test rejects equality with $\chi^2(1) = 14.92$. A Cox model clustered on the state puts the hazard of a cut 2.36 times higher for recession-era increases, with a p-value of 0.0006. That hazard ratio is the censoring-correct version of the 62 against 28 comparison and is the statistic I would have a reader carry away.

This is the finding that narrows the gap between the two literatures, though it does not close it. If the crisis premium that motivates a recession-era increase is transitory, the coalition that sustains the increase has reason to dissolve as the downturn recedes, and increases enacted for other reasons face no such pressure. A design that asks whether recessions leave a lasting imprint on the rate level is then asking about the

Table 5: Depth of Reversal, Not Only Its Incidence

	After 5 years	After 10 years
Panel A. Share with no rate cut of any size		
Enacted in normal times	0.758	0.621
Enacted during a recession	0.458	0.275
Recession − normal	-0.300***	-0.346***
	(0.079)	(0.082)
Panel B. Share still at or above the post-increase rate		
Enacted in normal times	0.833	0.712
Enacted during a recession	0.578	0.512
Recession − normal	-0.255***	-0.200**
	(0.068)	(0.092)
Panel C. Share back at or below the pre-increase rate		
Enacted in normal times	0.091	0.197
Enacted during a recession	0.289	0.287
Recession − normal	0.198***	0.091
	(0.055)	(0.083)
Panel D. Mean fraction of the increase still standing		
Enacted in normal times	1.071	0.925
Enacted during a recession	0.703	0.712
Recession − normal	-0.368***	-0.213
	(0.124)	(0.200)
Memo. Median fraction still standing, normal	1.000	1.000
the same, recession	1.000	1.000
Increase events	149	146

Notes: Each observation is a top marginal personal income tax increase enacted between 1960 and 2022. An increase is classified as enacted during a recession if it occurred in a recession year or the first three recovery years, and as normal times otherwise. The four panels measure different things and the distinction matters. Panel A is the measure used in Table 4 and counts any subsequent rate cut, however small, so an increase from 5.0 to 7.0 followed by a cut to 6.8 is recorded as not surviving. Panel B asks whether the rate is still at or above its post-increase level. Panel C asks whether the rate is back at or below where it stood before the increase, which is a completed reversal. Panel D reports the mean fraction of the original increase still standing, winsorised to $[-1, 2]$ because increases of a few hundredths of a point give the ratio an unbounded denominator; the medians in the memo rows are not winsorised and are the more reliable summary. Recession-era increases are cut more often and sooner, but Panels C and D show no significant difference in how completely they are undone at ten years, and the median increase of either kind is still fully in place. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

residue of a response that is cut sooner and more often. It is worth being precise about what that does and does not establish. The survival evidence shows that recession-linked increases meet subsequent cuts sooner and more frequently. Separately, cumulative recession exposure is not associated with higher rate levels for any instrument, and for the income rate the coefficient is significantly negative (Table A.4). Together these weigh against a permanent upward ratchet. Neither establishes that the subsequent cuts are what account for the absence of accumulation, since the median increase survives intact and the two results are estimated on different units, one on increase events and the other on state-years. That link is an interpretation I find plausible rather than a mechanism this paper identifies. The contemporaneous rate-level differential documented next should be read accordingly, as a within-state gap between recession and non-recession years rather than a permanent accumulation.

6.3 Rate Levels

While Tables 2 and 3 establish that recessions raise the probability of a tax change, they do not speak to the rate-level differential between recession and non-recession state-years. Table 6 addresses this by regressing tax rate levels directly on the recession and recovery indicators.[9] Pooled TWFE estimates indicate that top personal income tax rates in recession state-years are 0.67 percentage points higher than in baseline state-years, significant at the 1% level, with rates 0.36 percentage points higher in recovery state-years, significant at the 5% level. These pooled differentials are within-state contrasts between recession and non-recession state-years rather than an accumulated stock. Corporate income tax rates display a similar contemporaneous differential. The corporate event study shows positive lead coefficients that reject the joint pre-trend test (Table A.2), so the corporate level result is not cleanly identified. The income series shows leads of comparable magnitude that do not reject, though with substantially less precision, so the difference between the two is one of statistical power rather than of pre-trend size. The level story is therefore reported for the income tax and treated as suggestive for the corporate tax. Sales tax rates show no significant cross-sectional differential in either phase, despite the significant increase in the probability of a sales tax change documented in Table 2, indicating that recession-driven sales tax changes are frequent but small in per-change magnitude. Rates in recession and recovery state-years are systematically higher than in baseline years, consistent with the asymmetric short-run response documented above and reversed at the longer horizons of Table 4.

[9] Figure A.3 and Table A.2 in the Appendix report the event-study analog for rate levels. The path is a hump peaking near $t+1$ with coefficients declining through $t+7$. The lead coefficients are positive rather than flat and the late coefficients are imprecise, so the figure is best read as descriptive of the trajectory rather than as a test of reversion.

Table 6: Recession Effect on Tax Rate Levels

	(1) Sales Rate	(2) Income Rate	(3) Corporate Rate
During Recession	0.0809	0.6724***	0.3436**
	(0.0882)	(0.2449)	(0.1446)
Recovery (1-3yr)	0.0954	0.3602**	0.2350*
	(0.0768)	(0.1416)	(0.1236)
Observations	3150	3150	3150
Adj. R^2	0.867	0.731	0.806

Notes: The sample covers 1960 to 2022. The dependent variable is the tax rate level in percentage points; positive coefficients indicate that rates rise during recessions. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.4 The Spending Ratchet and the Legislative-Action Mechanism

Table 7 tests whether spending also ratchets. Real state expenditure per capita rises by 5.4 percent during recessions and remains significantly elevated through the third year of recovery, declining from 4.1 percent in the first year to 1.4 percent in the third. The spending-to-income ratio follows the same pattern. The spending ratchet is partial. The coefficient on log expenditure per capita declines from 5.4 percent during the recession to 1.4 percent by the third year of recovery, a reversion of roughly 74 percent. By contrast, the cross-sectional income tax rate differential narrows only 46 percent over the same window, declining from 0.67 to 0.36 percentage points between recession and recovery state-years. The two reversion percentages are not directly commensurable, so the gap is best read as qualitative evidence of differential persistence rather than a precise quantitative comparison. The direction of the contrast nonetheless supports the legislative-action mechanism. Spending partially reverts because it adjusts through administrative channels, such as hiring freezes, deferred maintenance, and the expiration of temporary federal transfers, none of which require legislative action. Tax rate reversal, by contrast, requires a bill, a floor vote, and a governor's signature. Decomposing by category sharpens the test. Direct expenditure, which states control and which requires legislative action to reverse, remains 3.3 percent above its pre-recession level during recovery, significant at the 1 percent level, mirroring the persistence of tax rates. Public welfare spending, driven largely by federally matched programs with automatic expiration, shows no significant persistence (Table A.9).

Table 8 reports results from seemingly unrelated estimation (`suest`), which stacks the increase and decrease equations and permits cross-equation Wald tests while accounting for correlated errors. The asymmetry test, whether recessions affect P(increase) and P(decrease) differently, yields a chi-squared statistic of 11.93 and rejects at the 0.1% level. The ratchet test, whether the recession effect on increases equals the recovery effect

Table 7: Spending Ratchet: Phase Decomposition

	(1) Log expenditure per capita	(2) Expenditure-to-income ratio
During Recession	0.0540***	0.0056***
	(0.0105)	(0.0019)
Recovery +1yr	0.0405***	0.0040***
	(0.0090)	(0.0014)
Recovery +2yr	0.0236***	0.0021*
	(0.0072)	(0.0012)
Recovery +3yr	0.0143*	0.0013
	(0.0080)	(0.0013)
Observations	3,150	3,150
Adj. R^2	0.971	0.879

Notes: The sample covers 1960 to 2022. The dependent variable in column (1) is the log of real state expenditure per capita (deflated by CPI to 2020 dollars); in column (2), total state expenditure divided by state personal income. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

on decreases, produces a chi-squared of 5.83, significant at the 5% level. If tax policy adjusted symmetrically, recession-driven increases would be matched by recovery-driven decreases of equal magnitude. The data decisively reject this. Tax-specific tests confirm the ratchet for personal income taxes at the 1% level and for sales taxes at the 5% level, but not for corporate income taxes, where the asymmetry is statistically indistinguishable from zero.

Table A.8 interacts the recession indicator with fiscal institutions. Supermajority requirements, rainy day funds, deficit bans, and divided government all fail to significantly moderate the ratchet. The deficit ban interaction is negative at 5.2 percentage points, suggestive of a dampening effect, but imprecisely estimated. The supermajority interaction, if anything, is positive at 6.9 percentage points, though imprecisely estimated. One interpretation, consistent with Mullins and Wallin (2004), is that procedural constraints on tax increases paradoxically concentrate increases into recessions, the one context in which political resistance is lowest and supermajority thresholds are more easily met. The same pattern is illustrated in Figure A.4 in the appendix.

6.5 Federal Transfers and the Attenuation of the Ratchet

The ratchet is driven by mild rather than severe recessions (Table A.10), consistent with Corollary 1. One explanation for this pattern is that deep downturns trigger federal fiscal interventions that relieve state-level budgetary pressure, substituting for the tax increases that mild recessions produce. To test this channel, I interact state income growth with lagged federal intergovernmental transfers per capita (Table 9). States

Table 8: Formal Ratchet Test (Seemingly Unrelated Regression)

	(1) P(Any Increase)	(2) P(Any Decrease)
During Recession	0.0756***	-0.0408*
	(0.0225)	(0.0220)
Recovery (1–3yr)	0.0082	-0.0023
	(0.0170)	(0.0166)
Observations	3150	3150
Adj. R^2	0.107	0.093
Asymmetry: χ^2	11.93	11.93
p-value	0.0006	0.0006
Ratchet: χ^2	5.83	5.83
p-value	0.0158	0.0158
Ratchet p: Sales	0.0342	0.0342
Ratchet p: Income	0.0025	0.0025
Ratchet p: Corporate	0.8621	0.8621

Notes: Standard errors, clustered at the state level, are reported in parentheses. Coefficients are jointly estimated via `suest`. The asymmetry test evaluates whether the recession effect differs between increases and decreases ($\beta_{\text{rec}}^{\text{inc}} = \beta_{\text{rec}}^{\text{dec}}$). The ratchet test evaluates whether recession-driven increases are offset by recovery-period decreases ($\beta_{\text{rec}}^{\text{inc}} = \beta_{\text{recov}}^{\text{dec}}$). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

receiving higher federal intergovernmental (IG) revenue are significantly less likely to raise taxes when income falls. A $1,000 increase in lagged federal transfers per capita attenuates the effect of income growth on the probability of a tax increase by 0.54, significant at the 1% level. The effect holds for personal income taxes and is robust to controlling for the interaction of income growth with state income per capita, which is itself insignificant. The federal transfer channel is therefore distinct from general state fiscal capacity. It is the composition of revenue, not its level, that determines whether a recession triggers a tax increase. This finding supports the interpretation that the ratchet operates through fiscal pressure rather than political opportunity. Federal transfers relieve precisely the budgetary constraint that forces states to raise taxes during downturns, and they do so most strongly during the deep recessions where the ratchet is weakest.

6.6 Robustness

The primary concern is that tax increases reduce economic activity, contributing to the measured recession. The event-study timing provides the most direct evidence against this channel. The probability of a tax increase is flat at $t-3$ and $t-2$, spikes at $t+1$, one year *after* recession onset, and returns to baseline by $t+2$ (Figure 2). If tax increases were causing the measured recession, the increase in P(tax increase) would precede or coincide with the income decline rather than follow it with a one-year lag. A direct test interacting the recession indicator with lagged tax rate levels and the tax-revenue-to-income ratio finds no

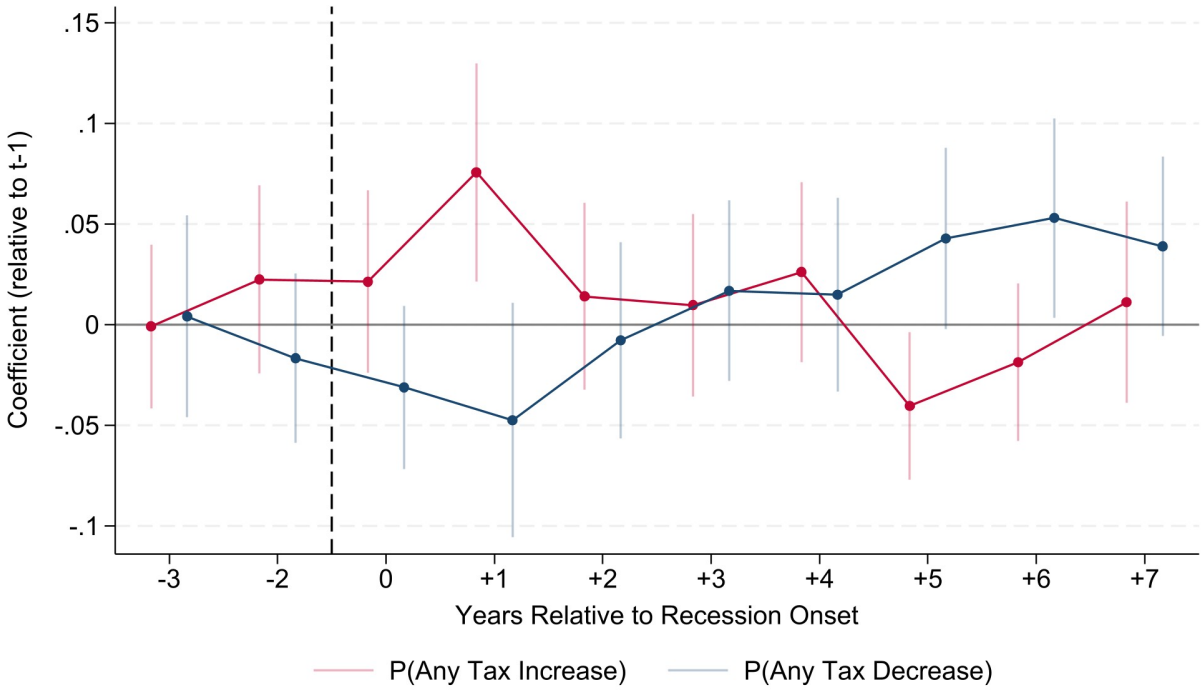


Figure 2: Event Study: The Probability of Tax Increases and Decreases Around Recession Onset

Notes: The figure plots event-study coefficients from equation (3), tracing the probability of any major tax increase (red) and any major tax decrease (blue) over the seven years following recession onset. The omitted reference category is $t-1$, the year prior to onset. The dashed vertical line marks recession onset. Confidence intervals are at the 95% level. Pre-trend coefficients are small and statistically insignificant for both increases ($F = 0.466$, $p = 0.630$) and decreases ($F = 0.631$, $p = 0.536$). The sample covers 1960 to 2022. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors are clustered at the state level. *Source:* Robinson and Tazhitdinova (2025).

significant interaction for the most comprehensive fiscal size measure, indicating the ratchet is not driven by states where tax changes have the largest macroeconomic footprint (Table A.17). Three supplementary features reinforce this conclusion. First, the recession indicator is based on state personal income measured before deduction of state taxes. Second, the implied short-run elasticity required for feedback to generate the recession indicator far exceeds established estimates (Suárez Serrato and Zidar 2016; Zidar 2019). Third, the ratchet is concentrated among mild recessions, where any feedback from tax changes to income is smallest. Replacing state-specific recessions with federal recession dates preserves the result at 3.0 percentage points, though with reduced precision (Table A.10). That column carries no recovery-phase indicator, so recovery years sit in the comparison group. Entering one returns 4.6 percentage points on the recession term and 3.1 on the recovery term (Table A.11), which is the figure quoted earlier. Estimating rate-level ratchets under the same federal definition yields a recession effect of 0.23 percentage points on income tax rates, with rates remaining 0.33 percentage points elevated during recovery, both significant at the 1% level (Table A.11).

Table 9: Federal Transfer Channel: Intergovernmental Revenue vs. State Income

	(1) Any Tax	(2) Sales	(3) Income	(4) Corporate
Income growth	2.4286	-0.2722	0.3006	1.9428
	(2.0735)	(1.6984)	(2.1932)	(1.3258)
Lagged IG Revenue per capita	-0.0540***	-0.0152*	-0.0287**	-0.0213
	(0.0185)	(0.0090)	(0.0127)	(0.0154)
Growth × Lagged IG Revenue per capita	0.5396***	0.1523	0.2994**	0.2279
	(0.1488)	(0.1125)	(0.1372)	(0.1434)
Growth × Log Income per capita	-0.4274*	-0.0654	-0.1621	-0.2375
	(0.2318)	(0.1860)	(0.2410)	(0.1420)
Recovery (1–3yr)	-0.0071	0.0086	-0.0003	-0.0129
	(0.0160)	(0.0128)	(0.0103)	(0.0105)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	3100	3100	3100	3100
Adj. R^2	0.109	0.063	0.085	0.066

Notes: The sample covers 1960 to 2022. The dependent variable is an indicator for a tax increase. Intergovernmental revenue is lagged real federal transfers per capita, measured in 1,000s of 2020 dollars. A positive interaction indicates that federal transfers attenuate the negative relationship between income growth and tax increases. All specifications include state and year fixed effects and control for log income per capita, log population, and divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The recession effect survives inclusion of state-specific linear trends at 7.2 percentage points, significant at the 1% level, and extension to the full pre-1960 sample at 4.0 percentage points, significant at the 5% level, though attenuated because fewer states had income taxes before 1960 (Table A.10). The income tax rate ratchet operates through two distinct legislative channels, rate increases by states with an existing income tax and tax-system adoption by states that previously had none. Both are deliberate legislative responses to fiscal pressure. Excluding the eight states whose top marginal personal income tax rate is zero throughout the sample (FL, NH, NV, SD, TN, TX, WA, WY) raises the probability estimate from 6.3 to 7.5 percentage points and the rate-level estimate from 0.67 to 0.72 (Table A.18, column 6).[10] These states have constitutionally or politically foreclosed income taxation and contribute no identifying variation, so including them as zero-rate controls only dilutes the rate-level estimate. Restricting further to state-years with an active income tax (Table A.18, column 5) attenuates the estimate because it discards the pre-adoption years in which late adopters introduced income taxes in response to fiscal pressure. This restriction artificially excludes the adoption channel and recovers the null reported by Robinson and Tazhitdinova (2025), who

[10] New Hampshire and Tennessee taxed interest and dividends over part of this period but not wage income, so their top marginal rate is zero throughout and they are excluded by this rule. Alaska is not excluded: it levied a top marginal rate of 14.56 percent until repeal in 1980 and so contributes twenty years of identifying variation.

similarly restrict to non-zero rate observations. The same pattern holds for corporate income taxes, where the unconditional rate-level estimate of 0.34 percentage points, significant at the 5% level, attenuates to an insignificant 0.09 when conditioning on adoption. Sales taxes show no significant rate-level ratchet under either specification. Extending the sample to 1929–2022 eliminates the rate-level ratchet for all three tax types regardless of conditioning, confirming that the rate-level effect is a post-1960 phenomenon (Table A.19). The probability-of-change result, by contrast, survives the full sample. Recessions raise the probability of an income tax increase by 4.5 percentage points over 1929–2022, significant at the 1% level, while recoveries produce no compensating decreases for any tax type (Table A.20). The mechanism that legislatures raise taxes under fiscal pressure has operated throughout the sample. The concentration of the rate-level effect in the post-1960 era reflects the maturation of state tax systems, after which rate adjustment replaced adoption as the primary fiscal margin. The sample also ends in a pandemic, and the 2020 downturn was met by federal transfers on a scale no earlier recession in the panel received, so states did not reach for the tax instrument and the 2021 and 2022 surpluses produced a wave of cuts instead. Ending the sample in 2019 leaves the results where they are. The probability of any major increase during a recession is 7.6 percentage points against 7.7, the top income tax rate differential is 0.64 against 0.67, and states are 4.2 percentage points less likely to cut income taxes in the first recovery year against 3.5 over the full sample. The year effects absorb a shock common to every state, so the recession coefficient, which is identified from variation across states within a year, is unaffected by it. Extending the recovery window to five years does not weaken the results. Income tax rates remain 0.31 percentage points above their pre-recession level, and the formal ratchet test continues to reject symmetric adjustment (Tables A.5, A.7, A.6). The era comparison in Table A.15 reveals that the ratchet is a post-1980 phenomenon. The recession coefficient is essentially zero from 1960 to 1979, rises to 11.0 percentage points from 1980 to 1999, and remains at 8.2 percentage points after 2000. Decomposing by severity, mild recessions drive the ratchet at 8.3 percentage points, significant at the 1% level, while severe recessions produce a smaller, insignificant effect of 2.7 percentage points, consistent with Corollary 1. A continuous severity specification confirms the inverted-U relationship without an arbitrary cutpoint (Table A.14). Results are robust to alternative recession thresholds of -2% and -3% real income growth. The income tax rate effect is larger at stricter thresholds, consistent with the concentration of the ratchet in moderate downturns (Table A.22). As a placebo test, excise taxes on cigarettes, gasoline, and alcohol show no significant ratchet (Table A.16), consistent with these rates being set in nominal terms and adjusted through distinct legislative processes unrelated to cyclical fiscal pressure.[11]

[11] The two-way fixed effects bias identified in the recent literature (Goodman-Bacon 2021; De Chaisemartin and d'Haultfoeuille 2020) arises when treatment is absorbing and early-treated units serve as controls for late-treated units. The recession indicator used here is transitory and switches on and off, so the forbidden-comparison structure does not directly apply. The `twowayfeweights` diagnostic of De Chaisemartin and d'Haultfoeuille (2020) confirms this empirically. Negative weights

6.7 Alternative Recession Chronologies

A potential concern is that the recession definition used here, any year of negative real state income growth, is broader than standard business cycle chronologies. Table A.12 replicates the main results across five recession definitions: the baseline negative-growth definition, the Bry–Boschan turning-point algorithm of Robinson and Tazhitdinova (2025), and three income-decline thresholds (-1%, -2%, -3%). The results reveal a clear gradient. The probability of any tax increase is 7.8 percentage points higher during recessions under the baseline definition, attenuates to 4.6 percentage points under Bry–Boschan, remains marginally significant at the -1% threshold, and becomes insignificant at -2% and -3%. The -1% threshold is informative because it eliminates measurement-error concerns associated with near-zero income growth while preserving the mild recessions that drive the ratchet. Its intermediate position between the baseline and -2% thresholds confirms that the recession definition, not statistical noise, explains the divergence between the present findings and those of Robinson and Tazhitdinova (2025).

Under the Bry–Boschan definition, the first recovery year shows a significant increase of 10.6 percentage points, consistent with a legislative lag. The Bry–Boschan algorithm identifies the income turning point, but the legislative process triggered by fiscal stress has not yet completed. Both the recession and first recovery-year coefficients are positive and significant, confirming that the fiscal response extends beyond the algorithmically identified trough. The event study under the Bry–Boschan definition (Table A.13 and Figure A.5) shows that the tax increase effect peaks at $t+2$, where Robinson and Tazhitdinova (2025) report insignificant event-study coefficients using a stacked difference-in-differences design. Their stacked design measures the within-cohort effect of a single recession episode within a clean-control window. The delayed legislative response observed at $t+2$ here falls near the endpoint of their event window, where cohort-specific estimation has lower power. The two specifications are therefore complementary rather than contradictory. Their design isolates the within-cohort effect of a single recession episode, while the pooled TWFE design captures the average within-state differential between recession or recovery state-years and baseline state-years that is the object of interest here (see Section 5.3). Pre-trend tests confirm the absence of differential trends prior to recession onset for both increases and decreases.

No specification shows evidence of tax reversal in recovery years, confirming the ratchet is not an artifact of the recession definition. The pattern extends to income taxes specifically. Under the baseline definition, the probability of an income tax increase rises during recessions (Panel C), and top marginal income tax rates increase and remain significantly elevated through recovery (Panel D). The rate-level effect is remarkably

account for less than one percent of the TWFE coefficient in all specifications (Appendix Table A.25). Wild cluster bootstrap p-values are virtually identical to conventional clustered p-values for all headline results (Table A.21).

robust. Income tax rates are significantly elevated during recessions under all five definitions, including the strictest -3% threshold, confirming that the ratchet operates on the intensive margin even when the probability effect attenuates. The negative-growth and Bry–Boschan estimates in Panel D agree closely, with recession-year coefficients of 0.691 and 0.636 (within one standard error) and similarly close estimates across all three recovery years. The divergence is concentrated in the discrete-action margin of Panel A, consistent with the Bry–Boschan algorithm dating recessions one year after the income contraction that triggers the legislative response.

The deeper distinction is between inflection points and fiscal stress. The Bry–Boschan algorithm identifies the moment an economy transitions from contraction to expansion, which is the appropriate concept for business cycle dating but not for fiscal policy analysis. State legislatures do not respond to inflection points. They respond to the accumulated revenue shortfall that balanced-budget requirements force them to resolve, typically one to two years after the initial income contraction. Negative income growth is both a recession indicator and a direct measure of this fiscal pressure, which is why the preferred definition aligns more naturally with the legislative response. Of the 510 state-years classified as recessions by the Bry–Boschan algorithm, two-thirds exhibit positive real income growth, with a mean growth rate of 1.4 percent. The algorithm captures decelerations, not the revenue shortfalls that trigger legislative action. Three features of the data reinforce this interpretation. First, 417 of the 463 state-recession state-years occur while aggregate US income is growing, meaning identification derives almost entirely from idiosyncratic state-level downturns, precisely the variation absorbed by year fixed effects. Second, 49.5 percent of state-recession state-years fall outside NBER federal recession dates, confirming that the definition captures fiscal stress episodes that national chronologies miss entirely.

6.8 Onset versus Continuation: The Role of Sustained Fiscal Pressure

Decomposing recession years into onset and continuation years reveals that the ratchet is concentrated in multi-year episodes (Table 10). The sample contains 345 distinct state-level recession episodes comprising 463 state-year observations, of which 253 last exactly one year, 74 last two years, and the remaining 18 span three or more years.[12] The probability of a tax increase is 18.7 percentage points higher in continuation

[12]The 18 recession episodes lasting three or more years cluster in three groups. The first comprises manufacturing-dependent states during the Volcker disinflation of 1979–1982. Indiana, Michigan, and Ohio each record four consecutive years of negative income growth, while Idaho records four years over the same period, and Delaware, Maine, Oregon, Pennsylvania, and Wisconsin record three-year episodes, consistent with the severe and prolonged contraction in durable goods production during this era. The second comprises commodity-dependent states experiencing sector-specific downturns, including Alaska (oil, 1977–1980), North Dakota (agriculture and energy, 1974–1977, 1985–1988, and 2015–2017), Wyoming (energy, 1982–1984 and 1986–1988), and West Virginia (coal and manufacturing, 1980–1983). Hawaii's three-year episode (1994–1996) reflects the collapse of the state's real estate market following the broader Pacific Rim contraction. Connecticut's post-crisis episode (2011–2013) reflects the prolonged recovery of its finance-heavy economy following the 2008 financial crisis. These episodes represent genuine and sustained fiscal stress rather than measurement error in the income series.

years, compared with only 3.9 percentage points at recession onset. An F-test rejects equality of the two coefficients at the 5 percent level. This nearly five-to-one ratio is one of the clearest tests of the underlying mechanism in the paper. If recessions triggered tax increases through automatic fiscal mechanisms (balanced-budget shortfalls translating mechanically into rate increases), the response would be largest at onset when the shock first hits. Instead, the concentration in year two and beyond suggests that legislatures require sustained fiscal pressure (accumulated deficits, depleted rainy-day funds, exhausted spending cuts) before overcoming the political costs of enacting a tax increase. This pattern is consistent with the finding that mild but prolonged contractions produce stronger ratchets than deep but brief downturns, and with the legislative-action mechanism, since tax rate changes require a bill, a vote, and a signature, a process that takes time even under fiscal duress. The biennial budget cycle reinforces this pattern. A revenue shortfall created by a mid-cycle income contraction cannot be resolved until the next appropriations process, so a second consecutive year of negative growth hits the legislature precisely when it is writing the new budget under accumulated fiscal pressure. Restricting the event study to single-year recessions confirms the null, with no significant response at any horizon for either increases or decreases (Figure A.6), reinforcing the conclusion that the ratchet is a product of prolonged fiscal stress rather than an immediate shock response.

Table 10: Multi-Year Recession Decomposition: Onset vs. Continuation

	(1) Any Inc	(2) Any Dec	(3) Sales Inc	(4) Income Inc	(5) Corp Inc
Recession year 1 (onset)	0.0388*	-0.0346	0.0146	0.0407**	0.0016
	(0.0218)	(0.0226)	(0.0169)	(0.0179)	(0.0172)
Recession year 2+	0.1868***	-0.0622*	0.1106***	0.1282***	0.0463
	(0.0537)	(0.0324)	(0.0350)	(0.0368)	(0.0383)
Recovery +1yr	0.0335	-0.0199	0.0244	0.0209	0.0075
	(0.0220)	(0.0261)	(0.0181)	(0.0178)	(0.0173)
Recovery +2yr	-0.0025	-0.0033	-0.0003	0.0062	-0.0146
	(0.0235)	(0.0280)	(0.0203)	(0.0154)	(0.0147)
Recovery +3yr	0.0055	0.0144	0.0218	0.0089	-0.0087
	(0.0267)	(0.0234)	(0.0199)	(0.0187)	(0.0175)
Observations	3150	3150	3150	3150	3150
Adj. R^2	0.111	0.093	0.064	0.086	0.065

Notes: The sample covers 1960 to 2022. Year 1 = recession onset. Year 2+ = continuation years of multi-year recessions. The sample contains 345 recession episodes: 253 single-year (73%), 74 two-year (21%), and 18 episodes of three or more years (5%). An F-test rejects equality of onset and continuation coefficients ($F = 6.61$, $p = 0.013$). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.9 Political Determinants of the Ratchet

I merge the tax panel with the State Partisan Balance dataset (Klarner 2013) and interact the recession and recovery indicators with gubernatorial turnover and election timing. The results reveal an asymmetric role for politics (Tables 11 and 12). Neither gubernatorial turnover nor election timing significantly modifies the probability of tax increases during recessions. The baseline recession effect of 6.62 percentage points remains stable across specifications. The upward leg of the ratchet is driven by fiscal pressure, not political opportunity.

Table 11: Political Economy of the Ratchet, P(Any Tax Increase)

	(1) Baseline	(2) Gov Turnover	(3) Election Year
During Recession	0.0662**	0.0538**	0.0587*
	(0.0260)	(0.0264)	(0.0334)
Rec × Gov Turnover		0.1116	
		(0.0943)	
Rec × Election Year			0.0222
			(0.0622)
Recovery (1–3yr)	0.0065	0.0085	-0.0009
	(0.0198)	(0.0207)	(0.0211)
Recov × Gov Turnover		-0.0164	
		(0.0613)	
Recov × Election Year			0.0306
			(0.0430)
Observations	2600	2600	2600
Adj. R^2	0.100	0.101	0.102

Notes: The sample covers 1960 to 2011 (Klarner partisan data coverage). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Gov turnover = governor's party changed from prior year. Election year = gubernatorial election held that year. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Political conditions matter on the decrease margin. Election-year recessions reduce the probability of a tax decrease by 7.68 percentage points, significant at the 1% level, and gubernatorial party turnover is associated with a 10.56 percentage point reduction in recession-era tax cuts, significant at the 5% level. The partial reversal at $t+5$ is concentrated in states experiencing interim gubernatorial turnover (Appendix Table A.27), suggesting that unwinding recession-era increases requires political replacement. Partisan decompositions are largely insignificant (Appendix Tables A.28–A.29).

Table 12: Political Economy, P(Any Tax Decrease)

	(1) Baseline	(2) Gov Turnover	(3) Election Year
During Recession	-0.0425*	-0.0295	-0.0162
	(0.0241)	(0.0251)	(0.0245)
Rec × Gov Turnover		-0.1056**	
		(0.0459)	
Rec × Election Year			-0.0768***
			(0.0259)
Recovery (1–3yr)	0.0129	0.0095	0.0109
	(0.0195)	(0.0209)	(0.0204)
Recov × Gov Turnover		0.0565	
		(0.0501)	
Recov × Election Year			0.0078
			(0.0300)
Observations	2600	2600	2600
Adj. R^2	0.111	0.112	0.111

Notes: The sample covers 1960 to 2011 (Klarner partisan data coverage). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Gov turnover = governor's party changed from prior year. Election year = gubernatorial election held that year. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7 Discussion

The ratchet is not a high-frequency phenomenon. Recessions occur in roughly 15 percent of state-year observations, contributing little to the overall R^2. Robinson and Tazhitdinova (2022) show that economic, political, and institutional factors together explain less than 20 percent of the variation in tax changes, yet within that limited explanatory power, economic factors matter disproportionately for tax increases relative to tax decreases. The permutation test in Robinson and Tazhitdinova (2025) randomizes recession timing and examines whether tax changes co-occur with recessions more often than expected by chance. The recession ratchet is consistent with this pattern and identifies one of the few systematic regularities in an otherwise largely idiosyncratic policy landscape. The asymmetry shapes the short-run trajectory of tax rates one recession at a time, even though individual increases revert at longer horizons.

U.S. states provide an ideal laboratory for testing the ratchet because balanced-budget requirements force fiscal pressure to be resolved through taxes or spending within the year. Deficit financing is not available as a buffer. This institutional constraint sharpens the mechanism relative to national governments, which can smooth shocks through borrowing, and explains why the ratchet is detectable at the subnational level even

when cross-country tests using aggregate spending have produced mixed results (Hercowitz and Strawczynski 2004; Jaén-García 2021). Whether the mechanism documented here operates in other balanced-budget settings such as European subnational governments or developing-country states is an open question, but the logic applies wherever borrowing constraints force contemporaneous fiscal adjustment.

The absence of a ratchet before 1980 and its emergence afterward admits two complementary explanations. First, most states were still adopting major tax instruments through the mid-1970s (Figure A.2), driven by fiscal modernization and the rapid growth of state budgets documented by Baicker et al. (2012) rather than cyclical pressure. Once adoption was complete, rate adjustment became the primary channel through which recessions generate tax increases. Second, the tax revolt era and the subsequent adoption of tax-and-expenditure limitations (e.g., California's Proposition 13 in 1978, Massachusetts' Proposition $2\frac{1}{2}$ in 1980, Colorado's TABOR in 1992) may have paradoxically concentrated tax increases into recession episodes, the one context in which political resistance is lowest, since many such limitations include fiscal-emergency provisions or supermajority waivers that lower the procedural cost of raising taxes precisely when revenue shortfalls are acute. This interpretation is consistent with the insignificantly positive supermajority interaction in Table A.8, and with institutional capacity rather than partisan identity being what predicts the response.[13]

Some recession-driven tax increases are enacted with built-in expiration dates, and Table 4 does not distinguish a sunset that expires from a cut that a later legislature enacts. The distinction matters for the interpretation of the reversal result. If recession-era increases are reversed because they sunset, the reversal is pre-committed at enactment rather than the dissolution of a crisis coalition, and it occurs without the affirmative legislative act on which the persistence gap between spending and taxes in Section 6.4 rests. The share of recession-era increases that carry an expiration clause, and whether the survival gap in Table 4 remains among increases that do not, therefore determine how much of the reversal reflects political will and how much reflects statutory design. Appendix A.8 codes expiration clauses for the increases that can be coded and finds that recessions do not favour the temporary instrument in any way these data can detect, but it classifies increases at enactment and does not attribute each subsequent cut to an expiry or to a new vote. The legislative-action reading of the persistence gap remains provisional on that decomposition.

The federal transfer channel documented in Section 6.5 provides the clearest evidence that severe recessions produce weaker ratchets. Federal fiscal stabilizers substitute for state tax increases precisely when downturns

[13] Legislative turnover, defined as a change in majority party control of either chamber, does not significantly predict tax changes during recessions or recovery, suggesting that the political channel operates through executive rather than legislative transitions.

are deepest. This channel also bears on the null reported by Robinson and Tazhitdinova (2025). Their primary definition and their auxiliary -3% threshold both place more weight on deep contractions, where federal transfers are largest and the ratchet is weakest, while the negative-growth definition used here also reaches the mild contractions that generate fiscal pressure without drawing federal aid. Section 5.3 separates that layer from the choice of estimator and Section 6.7 reports the severity gradient. The two papers agree on the underlying data and differ in which part of the severity distribution each definition selects. More broadly, the ratchet is not a universal law but a specific mechanism, concentrated in mild recessions that generate genuine fiscal stress without triggering federal substitution, in multi-year episodes that sustain pressure long enough to overcome legislative inertia, and in a political environment in which reversing tax increases requires affirmative legislative action. Identifying these boundary conditions is as informative as documenting the effect itself. Chodorow-Reich et al. (2012) document that federal fiscal relief under the 2009 American Recovery and Reinvestment Act significantly increased state employment, implying that state policymakers absorbed less of the recession's fiscal pressure when federal aid was forthcoming. Leduc and Wilson (2015) reach a related conclusion using highway grants under the same Recovery Act. The federal transfer mechanism documented in Section 6.5 operates through the same substitution logic. Relatedly, Sobel and Crowley (2014) show that federal grants create a distinct ratchet in which states expand programs with temporary federal funding and then raise own-source taxes when the grants expire, estimating future tax increases of roughly 40 cents per grant dollar. Knight (2002) provides further evidence on the vertical fiscal relationship. Using political power of state congressional delegations to instrument for endogenous federal highway grants, he finds that grants significantly crowd out state government spending, resolving the flypaper effect puzzle. Together, these findings imply that federal transfers interact with the recession ratchet in two opposing ways. In the short run, they substitute for state fiscal effort during deep recessions (attenuating the ratchet, as documented in Table 9), but in the long run, the expiration of temporary grants may generate its own ratchet as states raise taxes to sustain programs they can no longer fund federally. The inability of fiscal institutions to moderate the ratchet should be interpreted with caution, as these institutions are not randomly assigned and may offset unobserved political preferences.

The concentration of the ratchet in multi-year episodes rather than at recession onset (Section 6.8) is hard to reconcile with automatic fiscal mechanisms, under which the largest response would coincide with the largest shock. The biennial budget cycle most states use supplies the reason. A shortfall created by a mid-cycle contraction cannot be resolved immediately and accumulates until the next appropriations process, which is why the increase spike falls at $t + 1$ rather than $t + 0$ and why pressure sustained across several budget cycles produces a stronger response than a single-year contraction that can be absorbed administratively.

Together with the null for single-year recessions, this is evidence against pure balanced-budget mechanics and for reading the ratchet as a politically gated response. The cycle governs the timing of the upward leg without affecting its persistence, since reversing an increase requires an equally deliberate act in a later budget cycle.

Several caveats apply. The analysis captures statutory tax rates but not the tax base, exemptions, or effective rates, which represent additional margins of adjustment. The rate-level event study also has positive rather than flat lead coefficients, so the dynamic path is reported as descriptive and the reversal result rests on the event-level survival evidence in Table 4. It is also worth considering whether the ratchet is necessarily inefficient. If recessions reveal under-provision of public goods, the resulting tax increases may correct a prior shortfall. However, several features weigh against this interpretation. The asymmetry implies a one-directional response regardless of underlying demand, and partial reversals are concentrated in states experiencing gubernatorial party turnover rather than responding to new fiscal information. On balance, the ratchet is more consistent with a political friction than with efficient fiscal adjustment.

8 Conclusion

State tax rates respond to recessions, and the increases that follow are cut sooner and more often than increases enacted in normal times. That narrows the puzzle that motivated the paper, namely why the most comprehensive study of state tax changes finds no recession effect on rate levels even though balanced-budget pressure implies one, without fully resolving it. Recessions raise the probability of a state tax increase by approximately 7 percentage points while recoveries produce no compensating tax cuts within the first three years, and the formal test rejects equality of the increase and decrease responses within recession years. Income tax rates remain elevated through the recovery window, and recession-era increases face more than twice the hazard of a subsequent cut. What the evidence does not establish is that those increases are generally undone. The median increase of either kind still stands in full after ten years, and the difference in how completely they are reversed, clear at five years, is no longer statistically detectable at ten. The asymmetry this paper documents is therefore one of timing and incidence, with long-run persistence left mixed.

The ratchet is a post-1980 phenomenon, is driven by mild rather than severe recessions, and is not moderated by supermajority requirements, rainy day funds, or balanced-budget rules. The concentration of the ratchet among mild recessions, where federal intergovernmental transfers are smallest, offers a complementary interpretation of the null reported by Robinson and Tazhitdinova (2025), whose recession definitions either

identify turning points rather than revenue shortfalls or restrict attention to severe episodes where the effect is weakest. Replicating the analysis under five recession definitions, including Robinson and Tazhitdinova's Bry–Boschan algorithm, confirms a monotonic gradient from significant to null as the recession threshold deepens (Table A.12). Tax increases during recessions operate largely independently of political conditions, while electoral constraints suppress offsetting tax decreases. The partial reversal at $t + 5$ is concentrated in states experiencing gubernatorial party turnover, suggesting that unwinding recession-era increases requires political replacement rather than automatic reversion.

These findings suggest that recessions leave a temporary imprint on state tax rates that fades as the increases are reversed, so business cycle shocks shape the short-run path of state taxation more than its long-run level. The evidence is more consistent with a political friction than with efficient fiscal adjustment. Whether this pattern will continue is an open question, but the post-2020 expansion of state fiscal obligations and the expiration of pandemic-era federal transfers suggest the conditions for future ratcheting are, if anything, strengthening. The initial sunset analysis does not fully distinguish scheduled expirations from newly legislated cuts. A direct decomposition of subsequent reversals remains a direction for future work.

References

Agrawal, D. R., W. H. Hoyt, and J. D. Wilson (2022). Local Policy Choice: Theory and Empirics. *Journal of Economic Literature 60*(4), 1378–1455.

Alesina, A. and A. Drazen (1991). Why Are Stabilizations Delayed? *The American Economic Review 81*(5), 1170–1188.

Alt, J. E. and R. C. Lowry (1994). Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States. *American Political Science Review 88*(4), 811–828.

Angrist, J. D. and J.-S. Pischke (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Baicker, K., J. Clemens, and M. Singhal (2012). The Rise of the States: U.S. Fiscal Decentralization in the Postwar Period. *Journal of Public Economics 96*(11-12), 1079–1091.

Baker, S. R., P. Janas, and L. Kueng (2025). Correlation in State and Local Tax Changes. *Journal of Public Economics 242*, 105275.

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How Much Should We Trust Differences-in-Differences Estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Bierbrauer, F. J., P. C. Boyer, and A. Peichl (2021). Politically Feasible Reforms of Nonlinear Tax Systems. *American Economic Review 111*(1), 153–91.

Bird, R. M. (1971). Wagner's Law of Expanding State Activity. *Public Finance 26*(1), 1–26.

Bird, R. M. (1972). The "Displacement Effect": A Critical Note. *FinanzArchiv / Public Finance Analysis 30*(3), 454–463.

Bohn, H. and R. P. Inman (1996). Balanced-Budget Rules and Public Deficits: Evidence from the US States. In *Carnegie-Rochester Conference Series on Public Policy*, Volume 45, pp. 13–76. Elsevier.

Brown, J. P. (2017). Identifying State-Level Recessions. *Economic Review 102*(1), 85–108.

Buchanan, J. M. and R. E. Wagner (1977). *Democracy in Deficit: The Political Legacy of Lord Keynes*. New York: Academic Press.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008). Bootstrap-Based Improvements for Inference with Clustered Errors. *Review of Economics and Statistics 90*(3), 414–427.

Chodorow-Reich, G., L. Feiveson, Z. Liscow, and W. G. Woolston (2012). Does state fiscal relief during recessions increase employment? evidence from the american recovery and reinvestment act. *American Economic Journal: Economic Policy 4*(3), 118–145.

Clemens, J. and S. Miran (2012). Fiscal Policy Multipliers on Subnational Government Spending. *American Economic Journal: Economic Policy 4*(2), 46–68.

Correia, S. (2017). `reghdfe`: Estimating Linear Models with Multi-Way Fixed Effects. *The Stata Journal 17*(1), 95–112.

De Chaisemartin, C. and X. d'Haultfoeuille (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review 110*(9), 2964–2996.

Fernandez, R. and D. Rodrik (1991). Resistance to Reform: Status Quo Bias in the Presence of Individual-Specific Uncertainty. *The American Economic Review 81*(5), 1146–1155.

Francis, N. and B. D. Moore (2014, November). Temporary taxes: States' response to the great recession. Technical report, Urban-Brookings Tax Policy Center, Washington, DC.

Goodman-Bacon, A. (2021). Difference-in-Differences with Variation in Treatment Timing. *Journal of econometrics 225*(2), 254–277.

Hercowitz, Z. and M. Strawczynski (2004). Cyclical Ratcheting in Government Spending: Evidence from the OECD. *Review of Economics and Statistics 86*(1), 353–361.

Higgs, R. (1987). *Crisis and Leviathan: Critical Episodes in the Growth of American Government*. Oxford University Press.

Jaén-García, M. (2021). Displacement Effect and Ratchet Effect: Testing of Two Alternative Hypotheses. *SAGE Open 11*(1), 21582440211003577.

Kahneman, D. and A. Tversky (1979). Prospect Theory: An Analysis of Decision under Risk. *Econometrica 47*(2), 263–291.

Klarner, C. E. (2013). State Partisan Balance Data, 1937–2011. Harvard Dataverse. Dataset. Retrieved February 15, 2026, from https://dataverse.harvard.edu/dataset.xhtml?persistentId=hdl:1902.1/20403.

Knight, B. (2002). Endogenous Federal Grants and Crowd-out of State Government Spending: Theory and Evidence from the Federal Highway Aid Program. *American Economic Review 92*(1), 71–92.

Leduc, S. and D. Wilson (2015). Are state governments roadblocks to federal stimulus? evidence from highway grants in the 2009 recovery act. Working paper, Federal Reserve Bank of San Francisco.

McGuire, T. J. and D. F. Merriman (2006). State spending on social assistance programs over the business cycle. In R. M. Blank, S. Danziger, and R. F. Schoeni (Eds.), *Working and Poor: How Economic and Policy Changes are Affecting Low-Wage Workers*, pp. 289–311. New York: Russell Sage Foundation.

Mullins, D. R. and B. A. Wallin (2004). Tax and Expenditure Limitations: Introduction and Overview. *Public Budgeting & Finance 24*(4), 2–15.

National Association of State Budget Officers (1979–2023). The fiscal survey of the states. Washington, DC, semiannual.

Niskanen, W. A. (1971). *Bureaucracy and Representative Government*. Chicago: Aldine-Atherton.

Peacock, A. T. and J. Wiseman (1961). *The Growth of Public Expenditure in the United Kingdom*. Princeton University Press.

Poterba, J. M. (1994). State Responses to Fiscal Crises: The Effects of Budgetary Institutions and Politics. *Journal of political Economy 102*(4), 799–821.

Poterba, J. M. (1995). Balanced Budget Rules and Fiscal Policy: Evidence from the States. *National Tax Journal 48*(3), 329–336.

Poterba, J. M. (1996). Budget Institutions and Fiscal Policy in the US States. *The American Economic Review 86*(2), 395–400.

Robinson, S. and A. Tazhitdinova (2022). *What Drives Tax Policy? Political, Institutional, and Economic Determinants of State Tax Policy*. SSRN Working Paper, last revised November 2025. Available at https://ssrn.com/abstract=4035979.

Robinson, S. and A. Tazhitdinova (2025). One Hundred Years of US State Taxation. *Journal of Public Economics 241*, 105273.

Sobel, R. S. and G. R. Crowley (2014). Do Intergovernmental Grants Create Ratchets in State and Local Taxes? *Public Choice 158*(1), 167–187.

Sobel, R. S. and R. G. Holcombe (1996). The Impact of State Rainy Day Funds in Easing State Fiscal Crises During the 1990–1991 Recession. *Public Budgeting & Finance 16*(3), 28–48.

Sorensen, B. E. and O. Yosha (2001). Is State Fiscal Policy Asymmetric Over the Business Cycle? *Economic Review-Federal Reserve Bank of Kansas City 86*(3), 43–64.

Suárez Serrato, J. C. and O. Zidar (2016). Who Benefits from State Corporate Tax Cuts? A Local Labor Markets Approach with Heterogeneous Firms. *American Economic Review 106*(9), 2582–2624.

Vegh, C. A. and G. Vuletin (2015). How is Tax Policy Conducted Over the Business Cycle? *American Economic Journal: Economic Policy 7*(3), 327–370.

Wilson, J. D. (1999). Theories of Tax Competition. *National Tax Journal 52*(2), 269–304.

Zellner, A. (1962). An Efficient Method of Estimating Seemingly Unrelated Regressions and Tests for Aggregation Bias. *Journal of the American Statistical Association 57*(298), 348–368.

Zidar, O. (2019). Tax Cuts for Whom? Heterogeneous Effects of Income Tax Changes on Growth and Employment. *Journal of Political Economy 127*(3), 1437–1472.

A Appendix

A.1 Descriptive Figures

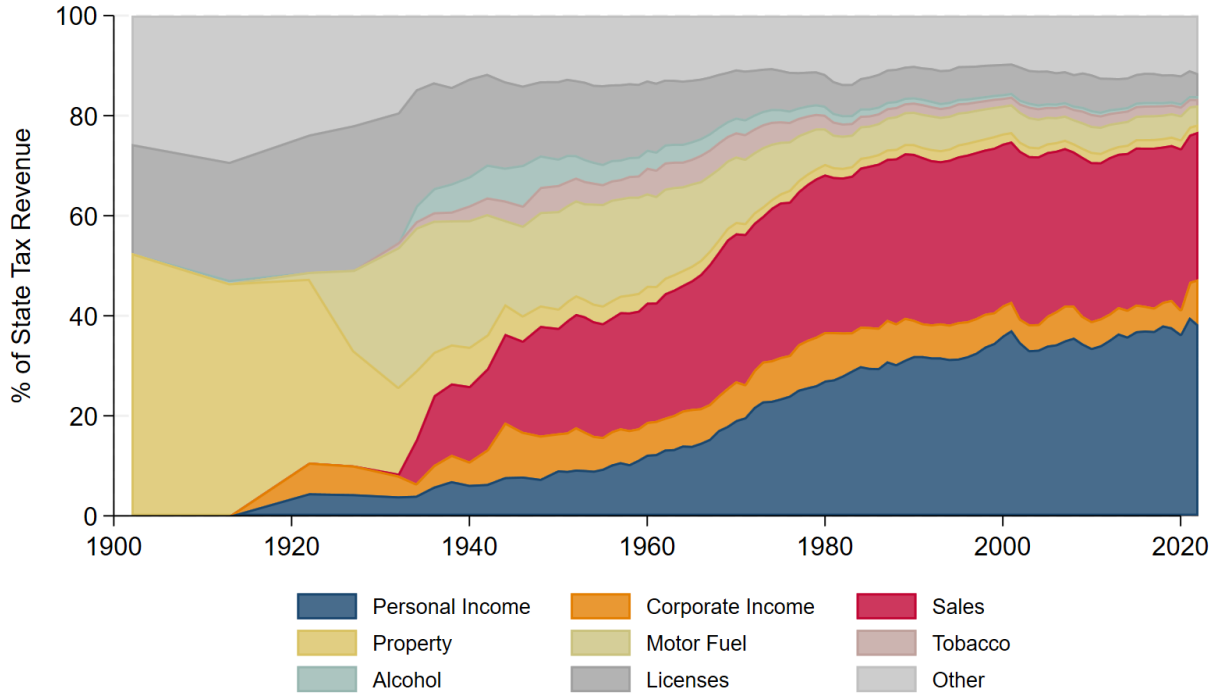


Figure A.1: State Tax Revenue Composition, 1900–2022

Notes: The figure plots the share of total state tax revenue by source from 1900 to 2022. Personal income and sales taxes have grown substantially over the period, together accounting for roughly two-thirds of state tax revenue by 2022. In contrast, property, motor fuel, tobacco, alcohol, and license taxes have declined as shares of the total. Corporate income taxes have remained a modest but relatively stable share. *Source:* Robinson and Tazhitdinova (2025).

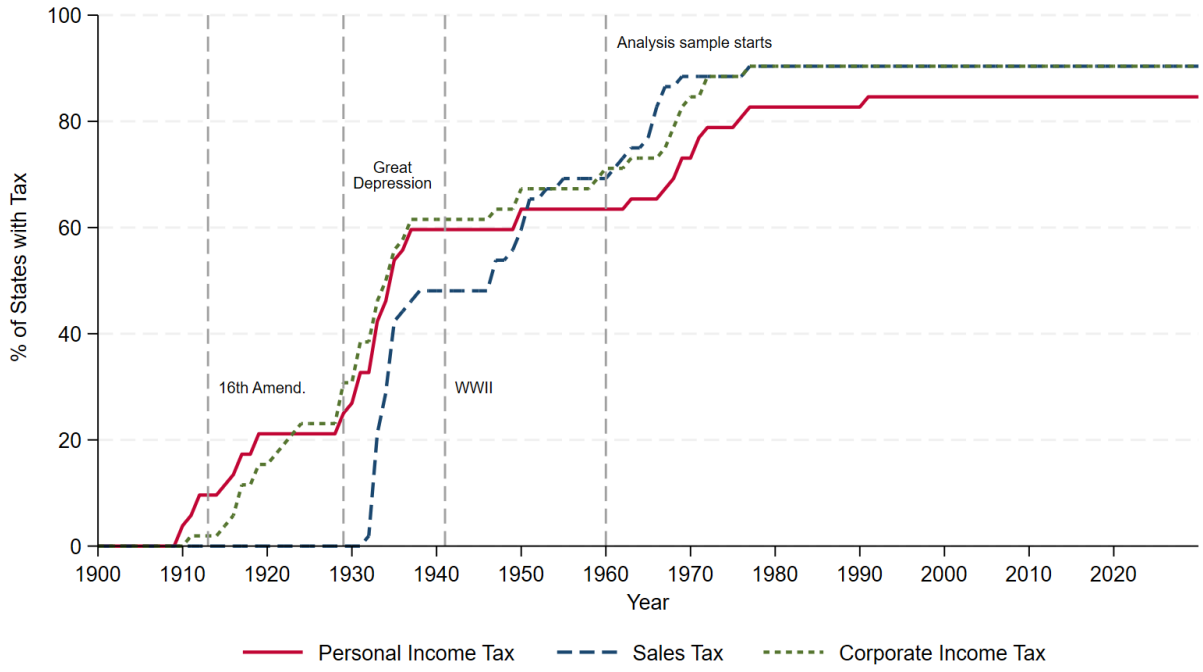


Figure A.2: Cumulative Tax Adoption Across States

Notes: The figure shows the percentage of 50 states that have adopted each major tax type. Dashed vertical lines mark the 16th Amendment (1913), the onset of the Great Depression (1929), U.S. entry into World War II (1941), and the start of the analysis sample (1960). Most states adopted their major tax instruments between 1930 and 1975, motivating the post-1960 sample restriction. *Source:* Robinson and Tazhitdinova (2025).

A.2 Extended Main Results

Table A.1: Event Study Around Recession Onset

	(1) P(Any Increase)	(2) P(Any Decrease)
-3	-0.0008	0.0041
	(0.0202)	(0.0250)
-2	0.0225	-0.0167
	(0.0233)	(0.0210)
0 (Onset)	0.0213	-0.0312
	(0.0225)	(0.0201)
+1	0.0757***	-0.0475
	(0.0270)	(0.0290)
+2	0.0141	-0.0078
	(0.0231)	(0.0242)
+3	0.0097	0.0168
	(0.0225)	(0.0223)
+4	0.0262	0.0149
	(0.0223)	(0.0240)
+5	-0.0404**	0.0428*
	(0.0183)	(0.0224)
+6	-0.0186	0.0530**
	(0.0195)	(0.0247)
+7	0.0112	0.0389*
	(0.0249)	(0.0222)
Observations	3150	3150
Adj. R^2	0.107	0.095

Notes: The sample covers 1960 to 2022. The reference category is the year prior to recession onset. Pre-trend tests fail to reject the null of no differential trends for both increases ($F = 0.466$, $p = 0.630$) and decreases ($F = 0.631$, $p = 0.536$). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

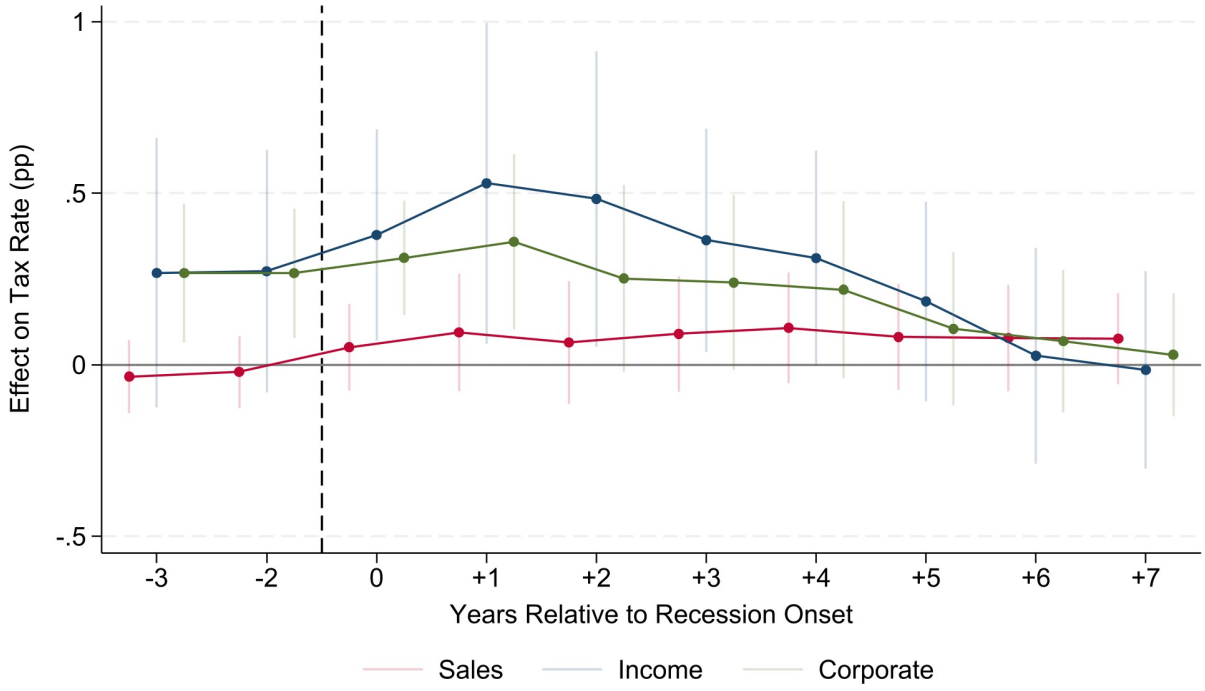


Figure A.3: Recession Effect on Tax Rate Levels by Tax Type

Notes: The figure plots event-study coefficients for the effect of recession onset on tax rate levels (in percentage points) for sales, personal income, and corporate income taxes. The omitted reference category is $t-1$. The dashed vertical line marks recession onset. Confidence intervals are at the 95% level. The top income tax rate rises after recession onset, peaks near $t+1$, and declines thereafter, with the coefficients at $t+6$ and $t+7$ close to zero but imprecisely estimated. The lead coefficients for the income and corporate rates are positive, at roughly 0.27 percentage points, and the joint pre-trend test rejects for corporate but not for income, a difference driven by the precision of the corporate series rather than by the size of the leads. The sales rate shows no pre-trend and a smaller, less persistent response. The sample covers 1960 to 2022. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors are clustered at the state level. *Source:* Robinson and Tazhitdinova (2025).

Table A.2: Event Study of Tax Rate Levels Around Recession Onset

	Income	Sales	Corporate
$t-3$	0.268	-0.034	0.268**
	(0.195)	(0.053)	(0.101)
$t-2$	0.273	-0.021	0.267***
	(0.176)	(0.052)	(0.094)
t (onset)	0.379**	0.051	0.312***
	(0.153)	(0.063)	(0.083)
$t+1$	0.529**	0.095	0.359***
	(0.233)	(0.086)	(0.127)
$t+2$	0.483**	0.065	0.252*
	(0.214)	(0.089)	(0.135)
$t+3$	0.363**	0.090	0.240*
	(0.162)	(0.084)	(0.127)
$t+4$	0.311*	0.108	0.219*
	(0.156)	(0.080)	(0.128)
$t+5$	0.185	0.081	0.105
	(0.145)	(0.077)	(0.112)
$t+6$	0.027	0.078	0.069
	(0.156)	(0.077)	(0.103)
$t+7$	-0.015	0.076	0.030
	(0.143)	(0.066)	(0.089)
N	3150	3150	3150
Pre-trend F (t-3, t-2)	1.67	0.81	4.06
Pre-trend p-value	0.198	0.449	0.023

Notes: Event-study coefficients from equation (3) with the tax rate level in percentage points as the dependent variable. Income refers to the top marginal personal income tax rate. The omitted reference category is the year prior to onset, $t-1$. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. The lead coefficients are positive for the income and corporate rates, at roughly 0.27 percentage points at both $t-3$ and $t-2$, and near zero for the sales rate. The joint pre-trend test fails to reject for income and sales and rejects at the 5 percent level for corporate, but the income and corporate point estimates are nearly identical, and the difference in test outcomes reflects the greater precision of the corporate series rather than a larger pre-trend. The tests on the income and sales series have limited power against pre-trends of the magnitude observed here. Note also that $t-1$ is a year of positive real income growth by construction, since onset is defined as the first year of negative growth, while the earlier lead periods carry no such restriction. Coefficients are therefore measured relative to a reference year that differs systematically from the rest of the event window. Standard errors, clustered at the state level, are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.3: Robustness of the Income-Tax Reversal Gap to the Recession-Linked Window

Recovery window counted	Normal uncut	Recession uncut	Difference (rec. − normal)	95% CI
0 years (recession year only)	0.505	0.231	-0.274**	[−0.48, −0.07]
1 year	0.533	0.259	-0.273***	[−0.45, −0.09]
2 years	0.580	0.246	-0.334***	[−0.50, −0.17]
3 years (baseline)	0.621	0.275	-0.346***	[−0.51, −0.19]
5 years	0.636	0.308	-0.329***	[−0.51, −0.15]

Notes: Each row reports the ten-year survival share for top marginal income tax increases, split by whether the increase was enacted in normal times or in a window classified as recession-linked, and varies the recovery window used in that classification from the recession year alone (0 years) to five recovery years. The estimate rests on 146 income increase events across 36 states. The difference is the recession-minus-normal gap, with standard errors clustered by state, and the final column reports its 95% confidence interval. Reclassifying recession-linked increases using the Bry-Boschan recession dating yields a difference of −0.203, with a 95% confidence interval of [−0.39, −0.02]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Cumulative Recession Exposure and Tax Rate Levels

	Income	Sales	Corporate
Cumulative recessions	-0.481**	0.007	-0.220*
	(0.222)	(0.080)	(0.120)
During recession	0.884***	0.078	0.440***
	(0.293)	(0.109)	(0.152)
Recovery (1–3 years)	0.491***	0.093	0.295**
	(0.173)	(0.088)	(0.132)
N	3150	3150	3150

Notes: The dependent variable is the tax rate level in percentage points. Cumulative recessions is the count of recession episodes a state has experienced up to year t, and the during-recession and recovery indicators capture the contemporaneous phase. Income is the top marginal personal income tax rate. The contemporaneous phase coefficients are positive, reflecting the transitory elevation documented in the main text, while the coefficient on cumulative recessions is negative or statistically indistinguishable from zero for every instrument, so the contemporaneous differential does not accumulate into a permanently higher rate. Adding state-specific linear trends leaves the cumulative coefficient negative or insignificant for every instrument (income −0.35, not significant; sales −0.09, not significant; corporate −0.51, $p < 0.01$), so in no specification is cumulative exposure positively associated with rate levels. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Recession Phases, During vs Recovery (5-Year Horizon)

	(1) Any Inc	(2) Any Dec	(3) Sales Inc	(4) Sales Dec	(5) Income Inc	(6) Income Dec	(7) Corp Inc	(8) Corp Dec
During Recession	0.0712***	-0.0310	0.0367**	-0.0022	0.0660***	-0.0133	0.0081	-0.0043
	(0.0234)	(0.0233)	(0.0165)	(0.0087)	(0.0200)	(0.0208)	(0.0202)	(0.0152)
Recovery +1yr	0.0175	-0.0064	0.0154	0.0049	0.0182	-0.0240	-0.0004	0.0253
	(0.0211)	(0.0270)	(0.0193)	(0.0061)	(0.0165)	(0.0165)	(0.0181)	(0.0228)
Recovery +2yr	-0.0100	0.0067	-0.0041	0.0093	0.0072	-0.0073	-0.0194	0.0125
	(0.0236)	(0.0297)	(0.0206)	(0.0113)	(0.0163)	(0.0208)	(0.0149)	(0.0204)
Recovery +3yr	0.0008	0.0232	0.0196	-0.0062	0.0112	0.0230	-0.0128	0.0272
	(0.0269)	(0.0247)	(0.0201)	(0.0082)	(0.0188)	(0.0198)	(0.0187)	(0.0179)
Recovery +4yr	-0.0043	0.0211	-0.0023	0.0041	0.0128	0.0192	-0.0155	0.0270
	(0.0240)	(0.0300)	(0.0171)	(0.0102)	(0.0157)	(0.0222)	(0.0144)	(0.0224)
Recovery +5yr	-0.0360*	0.0520	-0.0154	-0.0046	0.0051	0.0516*	-0.0155	0.0118
	(0.0210)	(0.0352)	(0.0145)	(0.0053)	(0.0168)	(0.0301)	(0.0187)	(0.0221)
Observations	3150	3150	3150	3150	3150	3150	3150	3150
Adj. R^2	0.107	0.093	0.060	0.014	0.082	0.082	0.064	0.057

Notes: The sample covers 1960 to 2022. The ratchet hypothesis predicts that tax increases are more likely during recessions and that tax decreases do not follow during recoveries. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Formal Ratchet Test (Seemingly Unrelated Estimation, 5-Year Recovery)

	(1) P(Any Increase)	(2) P(Any Decrease)
During Recession	0.0685***	-0.0284
	(0.0235)	(0.0230)
Recovery (1–5yr)	-0.0061	0.0189
	(0.0127)	(0.0200)
Observations	3,150	3,150
Adj. R^2	0.107	0.094
Asymmetry: χ^2	8.65	8.65
p-value	0.0033	0.0033
Ratchet: χ^2	2.77	2.77
p-value	0.0958	0.0958
Ratchet p: Sales	0.0422	0.0422
Ratchet p: Income	0.0213	0.0213
Ratchet p: Corporate	0.5269	0.5269

Notes: Standard errors, clustered at the state level, are reported in parentheses. Coefficients are jointly estimated via `suest`. The asymmetry test evaluates whether the recession effect differs between increases and decreases ($\beta^{\text{inc}}_{\text{rec}} = \beta^{\text{dec}}_{\text{rec}}$). The ratchet test evaluates whether recession-driven increases are offset by recovery-period decreases ($\beta^{\text{inc}}_{\text{rec}} = \beta^{\text{dec}}_{\text{recov}}$). Recovery is defined as 1–5 years after recession ends. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Recession Effect on Tax Rate Levels, 5-Year Recovery

	(1) Sales Rate	(2) Income Rate	(3) Corporate Rate
During Recession	0.0998	0.7135**	0.3584**
	(0.1064)	(0.2751)	(0.1686)
Recovery (1-5yr)	0.0961	0.3131*	0.1843
	(0.0896)	(0.1656)	(0.1360)
Observations	3150	3150	3150
Adj. R^2	0.867	0.731	0.806

Notes: The sample covers 1960 to 2022. The dependent variable is the tax rate level in percentage points; the recovery window is extended to 5 years. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.3 Mechanisms

Table A.8: Institutional Heterogeneity, P(Any Tax Increase)

	(1) Baseline	(2) Supermaj.	(3) Rainy Day	(4) Deficit Ban	(5) Divided	(6) Term Limit
During Recession	0.0756***	0.0644**	0.0596*	0.1141**	0.0770**	0.0686*
	(0.0225)	(0.0254)	(0.0338)	(0.0496)	(0.0292)	(0.0356)
Rec × Supermajority		0.0688				
		(0.0449)				
Rec × Rainy Day Fund			0.0275			
			(0.0471)			
Rec × Deficit Ban				-0.0524		
				(0.0492)		
Rec × Divided Govt					-0.0025	
					(0.0304)	
Rec × Term Limit						0.0126
						(0.0367)
Recovery (1-3yr)	0.0082	0.0027	-0.0167	0.0042	0.0082	0.0112
	(0.0157)	(0.0176)	(0.0260)	(0.0270)	(0.0158)	(0.0269)
Recov × Supermajority		0.0315				
		(0.0301)				
Recov × Rainy Day Fund			0.0437			
			(0.0281)			
Recov × Deficit Ban				0.0056		
				(0.0293)		
Recov × Term Limit						-0.0057
						(0.0292)
Observations	3150	3150	3150	3150	3150	3150
Adj. R^2	0.107	0.107	0.107	0.107	0.107	0.106

Notes: The sample covers 1960 to 2022. A negative interaction coefficient indicates that the institution attenuates recession-driven tax increases. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

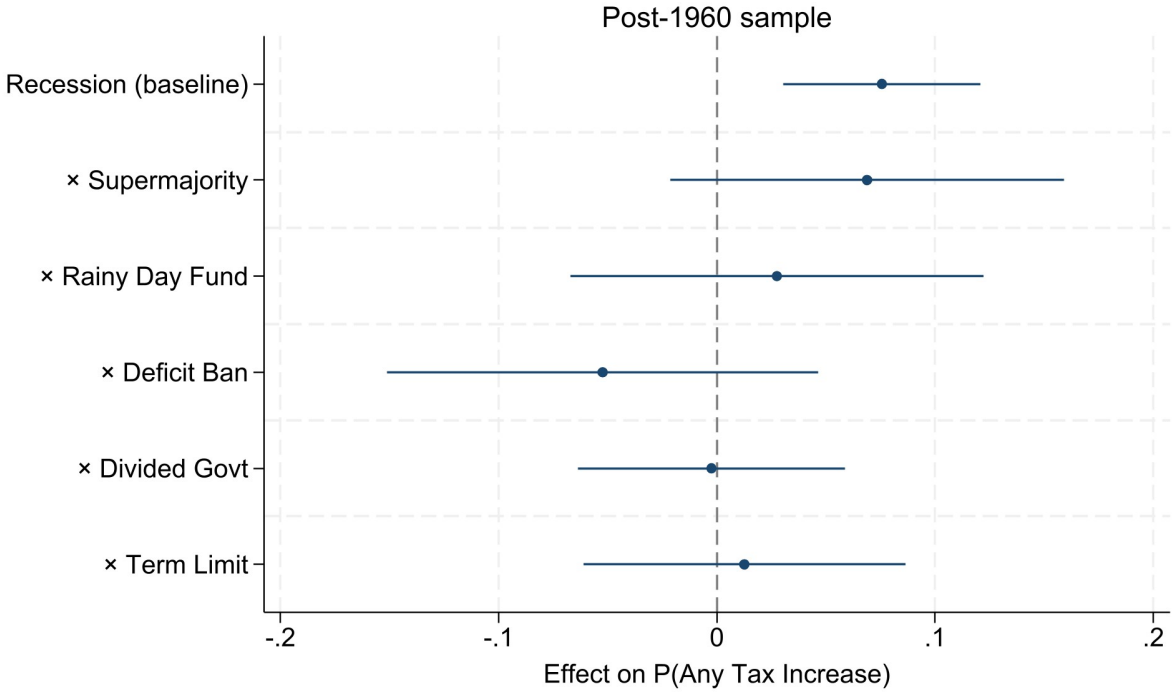


Figure A.4: How Institutions Moderate Recession-Driven Tax Increases

Notes: The figure plots the coefficient on the recession indicator and its interactions with three fiscal institutions: supermajority requirements for tax increases, rainy day fund provisions, and balanced-budget deficit bans. Point estimates are shown with 95% confidence intervals. The baseline effect (top row) represents the recession-driven increase in the probability of any major tax change for states without the relevant institution. Interaction terms below zero would indicate that the institution attenuates the ratchet effect. The sample covers 1960 to 2022. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors are clustered at the state level.

Table A.9: Expenditure Ratchet by Category

	(1) Welfare	(2) Education	(3) Direct
During Recession	0.0283	0.0509***	0.0908**
	(0.0208)	(0.0137)	(0.0405)
Recovery (1–3yr)	0.0073	0.0147	0.0825**
	(0.0182)	(0.0115)	(0.0332)
Observations	3150	3150	3119
Adj. R^2	0.934	0.940	0.768

Notes: The sample covers 1960 to 2022. The dependent variable is log real per-capita expenditure (2020 dollars). Public welfare is expected to revert (no ratchet) because enhanced federal matching rates (FMAP) expire automatically. Education and direct expenditure should persist (ratchet) because reversing requires firing teachers or cutting programs. Direct expenditure is defined as total direct expenditure minus public welfare and education expenditure. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.4 Robustness and Alternative Specifications

Table A.10: Robustness, P(Any Tax Increase)

	(1) Baseline	(2) Full Sample	(3) Federal Rec	(4) Severe/Mild	(5) State Trends
During Recession	0.0756***	0.0397**			0.0719***
	(0.0225)	(0.0169)			(0.0237)
Recovery	0.0082	0.0085		0.0078	0.0076
	(0.0157)	(0.0131)		(0.0156)	(0.0176)
Federal Recession			0.0298*		
			(0.0154)		
Severe Recession				0.0274	
				(0.0372)	
Mild Recession				0.0828***	
				(0.0244)	
Observations	3150	4638	3150	3150	3150
Adj. R^2	0.107	0.097	0.048	0.107	0.107

Notes: Column (1) reports the baseline specification using the post-1960 sample with state-specific recessions defined as negative real income growth and both state and year fixed effects. Column (2) extends the sample to include all years prior to 1960. Column (3) replaces state-specific recessions with Bry-Boschan recession dates and includes only state fixed effects, as year fixed effects are collinear with a common recession indicator. Column (4) decomposes the recession indicator by severity, where severe recessions are defined as real income growth below the sample median among recession episodes. All specifications control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Recession Definition Comparison: State-Specific vs. Federal

	State-Specific Recession		Federal Recession	
	(1) P(Any Increase)	(2) P(Any Increase)	(3) Income Rate (pp)	(4) Income Rate (pp)
During Recession	0.0756***		0.6724***	
	(0.0225)		(0.2449)	
Recovery (1–3yr)	0.0082		0.3602**	
	(0.0157)		(0.1416)	
During Recession		0.0455***		0.2308***
		(0.0163)		(0.0772)
Recovery (1–3yr)		0.0313**		0.3299***
		(0.0118)		(0.0751)
Recession definition	State-specific	Federal (NBER)	State-specific	Federal (NBER)
Fixed effects	State + Year	State only	State + Year	State only
Observations	3150	3150	3150	3150
Adj. R^2	0.107	0.050	0.731	0.709

Notes: The sample covers 1960 to 2022. Columns 1–2: the dependent variable is an indicator for any major tax increase. Columns 3–4: the dependent variable is the top marginal income tax rate (in percentage points). State-specific columns use negative real income growth with state and year fixed effects. Federal columns use NBER recession dates with state fixed effects only (year fixed effects are absorbed by the common shock). All specifications control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Recession Ratchet Across Five Recession Definitions

	(1) $< 0\%$	(2) Bry-Boschan	(3) $< -1\%$	(4) $< -2\%$	(5) $< -3\%$
Panel A: P(Any Tax Increase)					
Recession	0.0778***	0.0459**	0.0542*	0.0525	-0.0112
	(0.0219)	(0.0206)	(0.0287)	(0.0378)	(0.0379)
Recovery $t+1$	0.0263	0.1063***	0.0066	0.0866**	0.0577
	(0.0215)	(0.0232)	(0.0266)	(0.0412)	(0.0568)
Recovery $t+2$	0.0047	0.0257	0.0066	0.0407	0.0738
	(0.0250)	(0.0242)	(0.0305)	(0.0339)	(0.0513)
Recovery $t+3$	0.0046	0.0113	-0.0080	0.0295	0.0792
	(0.0275)	(0.0272)	(0.0320)	(0.0435)	(0.0531)
Panel B: P(Any Tax Decrease)					
Recession	-0.0404*	-0.0224	-0.0316	-0.0519*	-0.0698
	(0.0226)	(0.0256)	(0.0278)	(0.0287)	(0.0540)
Recovery $t+1$	-0.0166	-0.0340	0.0021	0.0386	-0.0212
	(0.0260)	(0.0234)	(0.0277)	(0.0318)	(0.0372)
Recovery $t+2$	-0.0043	-0.0113	0.0205	-0.0083	-0.0696*
	(0.0299)	(0.0260)	(0.0374)	(0.0463)	(0.0368)
Recovery $t+3$	0.0248	-0.0047	0.0453	0.0548	0.0242
	(0.0267)	(0.0297)	(0.0360)	(0.0507)	(0.0511)
Panel C: P(Income Tax Increase)					
Recession	0.0643***	0.0296**	0.0386	0.0583	-0.0160
	(0.0193)	(0.0144)	(0.0279)	(0.0364)	(0.0252)
Recovery $t+1$	0.0165	0.0760***	0.0033	0.0476	0.0401
	(0.0174)	(0.0229)	(0.0202)	(0.0339)	(0.0288)
Recovery $t+2$	0.0073	0.0134	0.0268	0.0564*	0.1001**
	(0.0167)	(0.0164)	(0.0219)	(0.0299)	(0.0472)
Recovery $t+3$	0.0171	0.0104	0.0063	0.0528	0.0764*
	(0.0200)	(0.0179)	(0.0233)	(0.0340)	(0.0420)
Panel D: Income Tax Rate Level					
Recession	0.6914***	0.6357***	0.9901***	1.2802***	0.8259**
	(0.2494)	(0.2318)	(0.3199)	(0.3842)	(0.3765)
Recovery $t+1$	0.5449***	0.5327***	0.7651***	0.9571***	0.4753
	(0.1786)	(0.1920)	(0.2196)	(0.2753)	(0.3386)
Recovery $t+2$	0.3944***	0.3907**	0.6363***	1.0325***	0.5220*
	(0.1467)	(0.1471)	(0.1774)	(0.2477)	(0.2937)
Recovery $t+3$	0.2690	0.2925*	0.5358***	1.0754***	0.6557**
	(0.1638)	(0.1569)	(0.1918)	(0.2394)	(0.3107)
Observations	3150	3150	3150	3150	3150

Notes: Each column uses a different recession definition. Column 1 defines recession as any year of negative real state personal income growth (baseline). Column 2 uses the Bry–Boschan turning-point algorithm applied to state real personal income, the definition used by Robinson and Tazhitdinova (2025). Columns 3–5 restrict to income declines exceeding 1%, 2%, and 3%, respectively. Panel A reports the probability of any major tax increase (sales, personal income, or corporate). Panel B reports the probability of any major tax decrease. Panel C reports the probability of a personal income tax increase. Panel D reports the top marginal personal income tax rate level. All specifications include state and year fixed effects and control for log income per capita, log population, and divided government. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Event Study Around Recession Onset: Bry–Boschan Definition

	(1) P(Any Increase)	(2) P(Any Decrease)
−3	-0.0117	0.0189
	(0.0274)	(0.0241)
−2	-0.0084	0.0101
	(0.0219)	(0.0252)
0 (Onset)	0.0297	0.0004
	(0.0234)	(0.0283)
+1	0.0293	-0.0342*
	(0.0234)	(0.0195)
+2	0.0828***	-0.0545*
	(0.0275)	(0.0281)
+3	0.0275	-0.0131
	(0.0245)	(0.0252)
+4	0.0154	0.0184
	(0.0250)	(0.0251)
+5	0.0239	0.0091
	(0.0228)	(0.0259)
+6	-0.0379**	0.0417*
	(0.0182)	(0.0244)
+7	-0.0176	0.0460*
	(0.0203)	(0.0268)
Observations	3150	3150
Adj. R^2	0.107	0.094

Notes: Event study around recession onset using the Bry–Boschan turning-point algorithm of Robinson and Tazhitdinova (2025). The omitted reference category is $t-1$. The dashed vertical line marks recession onset. Pre-trend tests fail to reject the null of no differential trends for both increases ($F = 0.125$, $p = 0.883$) and decreases ($F = 0.308$, $p = 0.736$). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

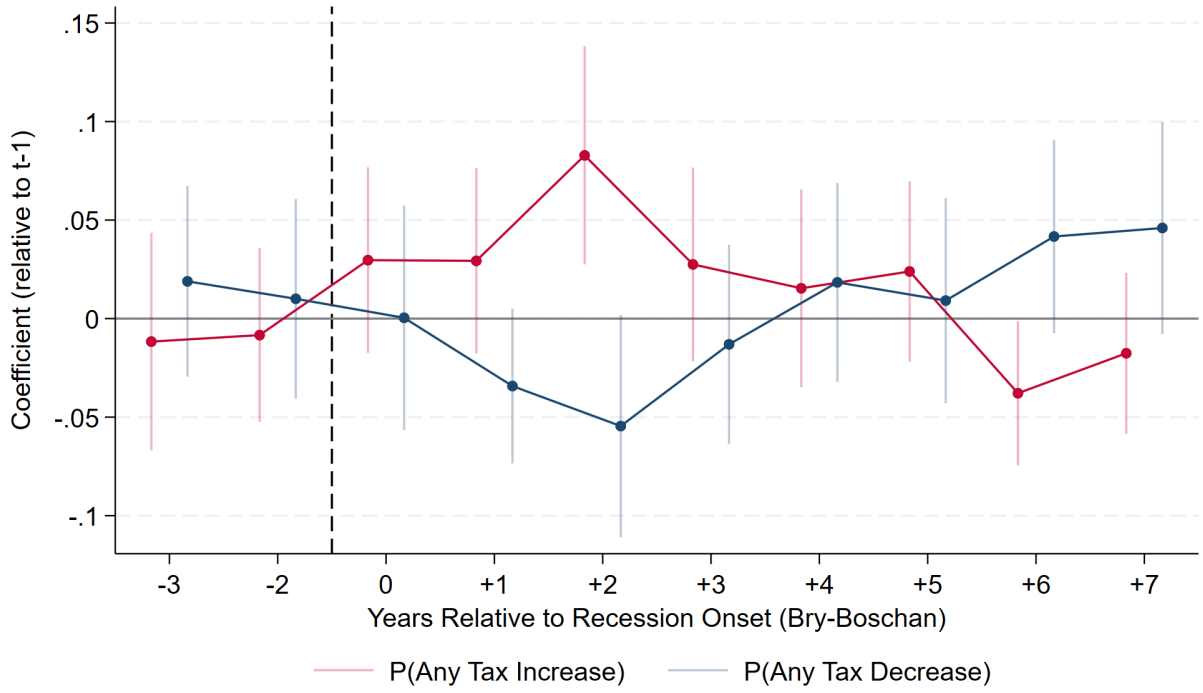


Figure A.5: Event Study Around Recession Onset: Bry–Boschan Definition

Notes: The figure plots event-study coefficients for the probability of any major tax increase (red) and any major tax decrease (blue), using the Bry–Boschan recession definition of Robinson and Tazhitdinova (2025). The omitted reference category is $t-1$. The dashed vertical line marks recession onset. Confidence intervals are at the 95% level. The tax increase effect peaks at $t+2$ (8.3 pp, $p<0.01$), consistent with a legislative lag relative to the algorithmically identified trough. The sample covers 1960 to 2022. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors are clustered at the state level.

Table A.14: Recession Severity and P(Tax Increase)

	(1) P(Any Increase)
Recession severity (pp)	0.0310**
	(0.0141)
Severity squared	-0.0036**
	(0.0015)
Recovery (1-3yr)	-0.0035
	(0.0148)
Observations	3150
Adj. R^2	0.105

Notes: The sample covers 1960 to 2022. Recession severity is the absolute value of real state income growth (in percentage points) in recession years and zero otherwise. A negative quadratic term indicates that the effect peaks at moderate severity and diminishes for deep recessions. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.15: Era Comparison, P(Any Tax Increase)

	(1) 1960-1979	(2) 1980-1999	(3) 2000+
During Recession	-0.0001	0.1104**	0.0824**
	(0.0422)	(0.0498)	(0.0389)
Recovery (1-3yr)	-0.0362	0.0180	0.0323*
	(0.0396)	(0.0308)	(0.0193)
Observations	1000	1000	1150
Adj. R^2	0.095	0.106	0.058

Notes: Each column estimates the baseline specification on a subsample of the post-1960 period. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16: Sin Tax Ratchet, P(Tax Increase)

	(1) Cigarette	(2) Gasoline	(3) Alcohol
During Recession	0.0442	0.0336	0.0120
	(0.0272)	(0.0244)	(0.0157)
Recovery (1-3yr)	0.0257	-0.0059	-0.0077
	(0.0160)	(0.0174)	(0.0103)
Observations	3099	3150	3123
Adj. R^2	0.100	0.109	0.047

Notes: The sample covers 1960 to 2022. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Reverse Causality Test: Tax Size Interaction

	(1) Income rate	(2) Corporate rate	(3) Sales rate	(4) Tax-to-income ratio
Recession	0.026	0.032	0.039	0.048
	(0.036)	(0.036)	(0.046)	(0.065)
Recession × Lagged income rate	0.008*			
	(0.004)			
Lagged income rate	-0.005			
	(0.004)			
Recession × Lagged corporate rate		0.007		
		(0.006)		
Lagged corporate rate		-0.014**		
		(0.005)		
Recession × Lagged sales rate			0.009	
			(0.010)	
Lagged sales rate			-0.047***	
			(0.008)	
Recession × Lagged tax-to-income				0.004
				(0.009)
Lagged tax-to-income ratio				-0.019**
				(0.008)
Observations	3100	3100	3100	3100
Adj. R^2	0.109	0.111	0.118	0.112

Notes: The sample covers 1960 to 2022. The dependent variable is an indicator for any major tax increase. The reverse causality hypothesis predicts that the ratchet should be stronger where the tax system is larger relative to the economy ($\beta_2 > 0$), so that rate increases produce meaningful macroeconomic feedback. Column (4) tests this directly using the lagged ratio of state tax revenue to personal income and finds no significant interaction. Columns (1)–(3) show modest positive interactions with individual lagged rates, consistent with a mechanical relationship between rate levels and adjustment probability rather than macroeconomic feedback. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.18: Excluding No-Tax States

	Baseline (all states)			Excluding no-tax states		
	(1) P(Any Inc)	(2) P(Inc Inc)	(3) Income Rate	(4) P(Inc Inc)	(5) Income Rate	(6) Income Rate
During recession	0.0766***	0.0630***	0.6724***	0.0683***	0.2315	0.7189**
	(0.0222)	(0.0192)	(0.2449)	(0.0224)	(0.1874)	(0.2780)
Recovery +1yr	0.0239	0.0153		0.0110		
	(0.0216)	(0.0173)		(0.0219)		
Recovery +2yr	-0.0048	0.0048		0.0008		
	(0.0238)	(0.0154)		(0.0189)		
Recovery +3yr	0.0052	0.0087		0.0098		
	(0.0266)	(0.0185)		(0.0227)		
Recovery (1-3yr)			0.3602**		0.1138	0.3586**
			(0.1416)		(0.1271)	(0.1638)
Observations	3150	3150	3150	2485	2485	2646
Adj. R^2	0.107	0.082	0.731	0.103	0.660	0.573

Notes: The sample covers 1960 to 2022. P(Any Inc) is unchanged by construction, as the composite includes all states regardless. Column (4) sets income tax indicators to missing for state-years without the tax. Column (5) restricts to state-years with an active income tax. Column (6) drops 7 never-adopter states (AK, FL, NV, SD, TX, WA, WY) but keeps pre-adoption zero-rate years for late adopters. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.19: Rate-Level Ratchet by Tax Type and Sample Period

	Income Tax		Sales Tax		Corporate Tax	
	(1)	(2)	(3)	(4)	(5)	(6)
During Recession	0.6724***	0.0348	0.0809	-0.0586	0.3436**	0.0932
	(0.2449)	(0.1595)	(0.0882)	(0.0878)	(0.1446)	(0.1328)
Recovery (1-3yr)	0.3602**	-0.0389	0.0954	0.0462	0.2350*	0.1333
	(0.1416)	(0.1304)	(0.0768)	(0.0782)	(0.1236)	(0.1133)
Sample period	Post-1960	1929–2022	Post-1960	1929–2022	Post-1960	1929–2022
Observations	3150	4640	3150	4640	3150	4640
Adj. R^2	0.731	0.690	0.867	0.813	0.806	0.739

Notes: All specifications are unconditional (zero-rate state-years included). Columns (1), (3), and (5) restrict to 1960–2022; columns (2), (4), and (6) use the full 1929–2022 sample. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

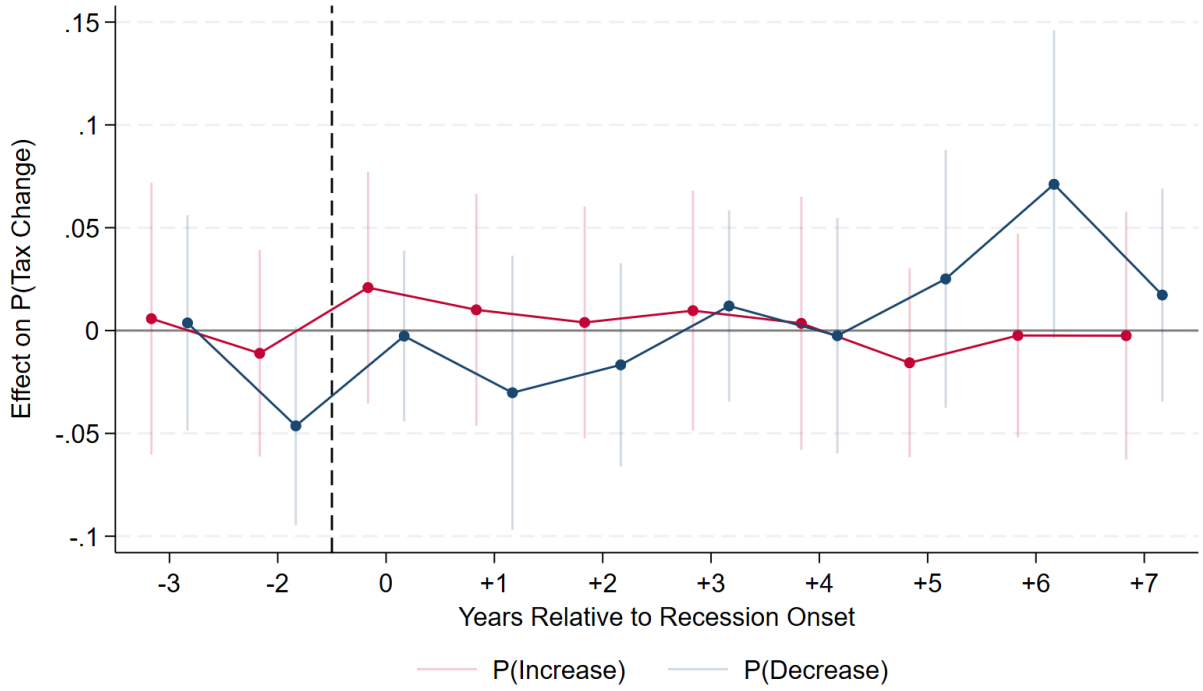


Figure A.6: Event Study: Single-Year Recessions Only

Notes: The figure plots event-study coefficients for the probability of any major tax increase (red) and any major tax decrease (blue), restricted to single-year recession episodes. The omitted reference category is $t-1$. The dashed vertical line marks recession onset. Confidence intervals are at the 95% level. No significant response emerges at any horizon for either increases or decreases, consistent with the concentration of the ratchet in multi-year recession episodes (Table 10). The sample covers 1960 to 2022. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors are clustered at the state level.

Table A.20: Recession Phases, Full Sample (1929–2022)

	Any Major Tax		Sales Tax		Income Tax		Corporate Tax	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
During Recession	0.0417**	-0.0216	0.0157	-0.0013	0.0448***	-0.0157	0.0041	-0.0000
	(0.0166)	(0.0170)	(0.0120)	(0.0068)	(0.0133)	(0.0156)	(0.0121)	(0.0105)
Recovery +1yr	0.0235	-0.0091	0.0250	0.0042	0.0036	-0.0192	0.0010	0.0152
	(0.0175)	(0.0184)	(0.0151)	(0.0046)	(0.0137)	(0.0135)	(0.0164)	(0.0144)
Recovery +2yr	0.0172	-0.0022	0.0093	0.0132	0.0070	-0.0137	0.0066	0.0081
	(0.0198)	(0.0195)	(0.0169)	(0.0084)	(0.0137)	(0.0151)	(0.0139)	(0.0125)
Recovery +3yr	-0.0170	0.0024	-0.0037	-0.0060	0.0031	0.0068	-0.0132	0.0103
	(0.0214)	(0.0181)	(0.0143)	(0.0071)	(0.0160)	(0.0140)	(0.0166)	(0.0123)
Observations	4638	4638	4638	4638	4638	4638	4638	4638
Adj. R^2	0.097	0.085	0.064	0.012	0.070	0.074	0.051	0.041

Notes: The sample covers 1929 to 2022. This table replicates Table 3 using the full sample period. Odd-numbered columns report the probability of a tax increase; even-numbered columns report the probability of a tax decrease. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.21: Wild Cluster Bootstrap Inference

Panel A: Table 2, Recession Effect on P(Tax Change)

Outcome	Coefficient	Clustered p	Bootstrap p
Any Increase	0.0723	0.002	0.002
Any Decrease	−0.0398	0.069	0.068
Sales Increase	0.0334	0.029	0.030
Sales Decrease	−0.0035	0.687	0.679
Income Increase	0.0587	0.003	0.002
Income Decrease	−0.0171	0.403	0.398
Corp Increase	0.0150	0.419	0.425
Corp Decrease	−0.0176	0.244	0.246

Panel B: Table 3, Phase Decomposition (Key Coefficients)

Outcome	Variable	Coefficient	Clustered p	Bootstrap p
Any Increase	During Recession	0.0766	0.001	0.001
Any Increase	Recovery +1yr	0.0239	0.277	0.276
Any Decrease	During Recession	−0.0416	0.077	0.072
Any Decrease	Recovery +1yr	−0.0182	0.488	0.488
Income Increase	During Recession	0.0630	0.002	0.002
Income Increase	Recovery +1yr	0.0153	0.384	0.391
Income Decrease	During Recession	−0.0234	0.291	0.285
Income Decrease	Recovery +1yr	−0.0354	0.048	0.046
Corp Increase	During Recession	0.0130	0.495	0.502
Corp Increase	Recovery +1yr	0.0046	0.794	0.789
Corp Decrease	During Recession	−0.0108	0.495	0.496
Corp Decrease	Recovery +1yr	0.0190	0.373	0.377

Notes: The table reports wild cluster bootstrap p-values alongside conventional cluster-robust p-values for the main results in Tables 2 and 3. Outcomes include any major tax, sales tax, personal income tax, and corporate income tax increases and decreases. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Bootstrap p-values are computed using Rademacher weights with 9,999 replications (Cameron et al. 2008).

Table A.22: Recession Threshold Robustness

	(1) $< 0\%$	(2) $< -2\%$	(3) $< -3\%$
Panel A: P(Any Tax Increase)			
During Recession	0.0756***	0.0518	-0.0110
	(0.0225)	(0.0379)	(0.0381)
Recovery (1–3yr)	0.0082	0.0529**	0.0700*
	(0.0157)	(0.0235)	(0.0386)
Adj. R^2	0.107	0.106	0.106
Panel B: P(Any Tax Decrease)			
During Recession	-0.0408*	-0.0518*	-0.0695
	(0.0228)	(0.0286)	(0.0539)
Recovery (1–3yr)	-0.0023	0.0283	-0.0222
	(0.0205)	(0.0302)	(0.0277)
Adj. R^2	0.093	0.093	0.093
Panel C: Income Tax Rate Level			
During Recession	0.6724***	1.2815***	0.8275**
	(0.2449)	(0.3839)	(0.3760)
Recovery (1–3yr)	0.3602**	1.0203***	0.5498*
	(0.1416)	(0.2444)	(0.3020)
Adj. R^2	0.731	0.735	0.730
Observations	3150	3150	3150

Notes: The sample covers 1960 to 2022. Column (1) defines recession as any year of negative real state personal income growth (the baseline definition). Column (2) uses a -2% threshold, and column (3) uses a -3% threshold. Each panel reports a separate regression. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.5 Stacked DiD Robustness

The tables in this subsection report supplementary stacked-DiD estimates that complement the pooled TWFE specifications in the main text. Table A.23 reports Wing-Freedman-Hollingsworth (2024)-weighted stacked DiD estimates for the probability outcomes, where the empirical object (the per-period rate of legislative action) is a flow rather than a stock. The asymmetric probability ratchet documented in Tables 2–3 survives this clean-control specification at the one percent level. Table A.24 applies Robinson and Tazhitdinova (2025)'s exact stacked DiD specification (clean-control window, WFH weights, adopter-only sample, no additional controls) with only the recession indicator swapped from Bry–Boschan to negative real income growth. The top marginal income tax rate effect is significant at $t+3$ (0.38 pp) and $t+4$ (0.36 pp). The probability of a sales tax increase is significant at $t+3$ (10.4 pp). The probability of an income tax decrease is suppressed at $t+2$. Because these specifications net out pre-existing levels through cohort-specific fixed effects within a clean-control window, they are not subject to the positive lead coefficients documented in Table A.2. Table A.25 reports the De Chaisemartin and d'Haultfoeuille (2020) `twowayfeweights` decomposition, which confirms that negative weights account for less than one percent of the TWFE coefficient in all specifications. The Goodman-Bacon (2021) forbidden-comparison bias is therefore empirically negligible in this setting.

Table A.23: Wing-Freedman-Hollingsworth Stacked DiD: Probability of Tax Change

	(1) Any Inc	(2) Any Dec	(3) Income Inc	(4) Income Dec
During Recession	0.0693**	-0.0035	0.0687***	-0.0014
	(0.0260)	(0.0395)	(0.0218)	(0.0334)
Recovery +1yr	0.0229	-0.0736***	0.0174	-0.0872***
	(0.0324)	(0.0270)	(0.0227)	(0.0267)
Recovery +2yr	0.0455	-0.0382	0.0273	-0.0199
	(0.0370)	(0.0249)	(0.0287)	(0.0231)
Recovery +3yr	0.0289	0.0328	0.0293	0.0213
	(0.0371)	(0.0281)	(0.0252)	(0.0285)
Observations	8287	8287	8287	8287

Notes: The sample covers 1960 to 2022. The table reports Wing-Freedman-Hollingsworth (2024)-weighted stacked DiD estimates of the recession effect on the probability of tax change. Treated states enter recession at year c; clean controls have no recession in $[c-3, c+5]$. All specifications include cohort×state and cohort×year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.24: Robinson and Tazhitdinova (2025) Stacked DiD with Negative-Growth Recession Indicator

	(1) Top PIT Rate (nz)	(2) Sales Increase	(3) Income Decrease
$t-3$	0.0648	0.0104	-0.0359
	(0.1010)	(0.0487)	(0.0567)
$t-2$	0.0786	0.0189	-0.0398
	(0.0640)	(0.0442)	(0.0521)
$t-1$	0.0000	0.0000	0.0000
	(ref)	(ref)	(ref)
$t+0$	0.0186	0.0219	0.0075
	(0.0524)	(0.0511)	(0.0389)
$t+1$	0.1256	0.0834*	-0.0403
	(0.1074)	(0.0503)	(0.0386)
$t+2$	0.2672*	0.0350	-0.0636*
	(0.1436)	(0.0449)	(0.0377)
$t+3$	0.3801**	0.1038**	-0.0191
	(0.1515)	(0.0504)	(0.0357)
$t+4$	0.3613**	0.0054	0.0290
	(0.1777)	(0.0468)	(0.0532)
$t+5$	0.1995	-0.0007	0.0578
	(0.2016)	(0.0399)	(0.0580)
Observations	2754	2754	2754
Sub-experiments	20	20	20

Notes: The sample covers 1960 to 2022. The table reports clean-control stacked DiD estimates with WFH weights following Robinson and Tazhitdinova (2025), swapping their Bry–Boschan recession indicator for negative real state personal income growth. The event window is $[-3, +5]$ around recession onset; $t-1$ is the reference period. Column 1 reports the top marginal personal income tax rate for the adopter-only sample. Column 2 reports the probability of a sales tax rate increase for the sales-tax-adopter sample. Column 3 reports the probability of a personal income tax rate decrease for the adopter-only sample. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.25: Goodman-Bacon Decomposition of TWFE Weights: Forbidden-Comparison Diagnostic

		Positive Weights		Negative Weights	
Outcome	β_{TWFE}	N	Σ weight	N	Σ weight
P(any tax increase)	0.074	442	1.006	21	−0.006
P(any tax decrease)	−0.036	442	1.006	21	−0.006
Top PIT rate (all states)	0.456	442	1.006	21	−0.006
Top CIT rate (all states)	0.244	442	1.006	21	−0.006
Sales tax rate (all states)	0.034	442	1.006	21	−0.006
Top PIT rate (adopters only)	0.256	367	1.007	17	−0.007
Top CIT rate (adopters only)	0.072	401	1.006	16	−0.006
Sales tax rate (adopters only)	−0.028	383	1.010	28	−0.010

Notes: The table reports the De Chaisemartin and d'Haultfoeuille (2020) `twowayfeweights` decomposition of the TWFE coefficient into a weighted sum of group-time average treatment effects. The "Positive weights" and "Negative weights" columns report the number of weights of each sign and the sum of those weights. Across all specifications, negative weights account for less than one percent of the total weight ($|\Sigma_{neg}|/\Sigma_{pos} < 0.01$), confirming that the Goodman-Bacon (2021) forbidden-comparison bias is empirically negligible. The diagnostic is run on the unbalanced panel of 50 states from 1960 to 2022 with the state-recession indicator (negative real income growth) as the binary, switching treatment.

A.6 Bracket Decomposition: Top vs Minimum Tax Rates

Table A.26 decomposes the recession-driven rate-level response by bracket. The top marginal PIT rate rises by 0.65 percentage points during recession state-years (column 1, significant at the 1 percent level), while the minimum (entry) PIT bracket rises by 0.20 percentage points (column 2, significant at the 5 percent level), a ratio of approximately 3.3 to 1. The corresponding bracket spread (top minus minimum) widens by 0.47 percentage points in the full sample (column 3, significant at the 5 percent level), with a similar proportional pattern persisting through recovery. The corporate income tax shows a smaller relative gap, reflecting the fact that most states have flat corporate rate schedules in which top and minimum brackets coincide by construction.

The full-sample spread effect partly reflects recession-era tax-system adoptions: when a state moves from zero income tax to a new bracketed system, the top and minimum rates are simultaneously established with a non-trivial spread, and this adoption event loads onto the spread coefficient. Restricting to states with existing income tax systems, the bracket-spread effect attenuates to 0.14 pp (statistically insignificant), indicating that within established systems, the top and minimum brackets respond proportionally rather than reflecting a within-system progressivity shift. The overall pattern (larger response on the top rate, partial widening of the bracket spread in the full sample, no spread shift among established systems) is consistent with the political-economy of legislative-action politics. Top-rate increases concentrate the burden on a narrower constituency and face less per-dollar political resistance, but legislatures supplement these increases with smaller broad-based adjustments to generate sufficient revenue.

Table A.26: Recession Effect on Top, Minimum, and Spread Tax Rate Levels

	(1) PIT Top	(2) PIT Min	(3) PIT Spread	(4) CIT Top	(5) CIT Min	(6) CIT Spread
During Recession	0.6724***	0.1998**	0.4726**	0.3436**	0.3252**	0.0184
	(0.2449)	(0.0772)	(0.2167)	(0.1446)	(0.1246)	(0.1112)
Recovery (1-3yr)	0.3602**	0.1233*	0.2369*	0.2350*	0.1609*	0.0740
	(0.1416)	(0.0618)	(0.1353)	(0.1236)	(0.0865)	(0.0694)
Observations	3150	3150	3150	3150	3150	3150
Adj. R^2	0.731	0.776	0.675	0.806	0.806	0.739

Notes: The sample covers 1960 to 2022 and comprises 50 states over 63 years. Columns 1, 2, 4, and 5 report tax rate levels in percentage points; columns 3 and 6 report the spread (top minus minimum) in percentage points. A larger coefficient on the top rate than the minimum rate indicates the ratchet is concentrated on high-income filers; comparable coefficients indicate base broadening. Columns 1 and 4 reproduce columns 2 and 3 of Table 6 exactly; the spread coefficients in columns 3 and 6 are the arithmetic differences between the adjacent top and minimum columns. All columns include zero-rate state-years (full sample). When restricted to states with the relevant tax in place (adopters only), the spread coefficients attenuate to 0.14 pp (n.s.) for PIT and −0.07 pp (n.s.) for CIT, indicating that the full-sample spread effect partly reflects adoption events that mechanically widen the spread. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.7 Political Economy

Table A.27: Mechanism: Political Turnover and the Partial Reversal

	(1) P(Inc)	(2) P(Inc)	(3) P(Dec)	(4) P(Dec)
During Recession	0.0602**	0.0606**	-0.0292	-0.0285
	(0.0272)	(0.0272)	(0.0247)	(0.0245)
Recovery (1–3yr)	0.0009	0.0021	0.0253	0.0264
	(0.0199)	(0.0194)	(0.0213)	(0.0215)
Late Recovery (4–5yr)	-0.0225	0.0161	0.0494*	0.0280
	(0.0209)	(0.0273)	(0.0281)	(0.0299)
Late Recov × Interim Turnover		-0.0840**		0.0479
		(0.0359)		(0.0413)
Observations	2600	2600	2600	2600
Adj. R^2	0.100	0.101	0.112	0.113

Notes: The sample covers 1960 to 2011 (Klarner partisan data coverage). Interim turnover = governor's party changed at any point in preceding 5 years. If late reversal is concentrated in turnover states, the ratchet unwinds through political replacement rather than automatic policy reversion. All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.28: P(Any Tax Increase), Partisan Dimensions

	(1) Baseline	(2) Unified	(3) Party	(4) Veto-Proof
During Recession	0.0662**	0.0578*	0.0524*	0.0677**
	(0.0260)	(0.0294)	(0.0290)	(0.0291)
Rec × Unified Govt		0.0204		
		(0.0410)		
Rec × Dem Unified			0.0807*	
			(0.0469)	
Rec × Rep Unified			-0.0656	
			(0.0517)	
Rec × Veto-Proof				-0.0087
				(0.0421)
Recovery (1–3yr)	0.0065	0.0119	0.0044	0.0195
	(0.0198)	(0.0223)	(0.0218)	(0.0208)
Recov × Unified Govt		-0.0115		
		(0.0346)		
Recov × Dem Unified			-0.0075	
			(0.0385)	
Recov × Rep Unified			-0.0014	
			(0.0495)	
Recov × Veto-Proof				-0.0445
				(0.0304)
Observations	2600	2600	2548	2548
Adj. R^2	0.100	0.099	0.093	0.093

Notes: The sample covers 1960 to 2011 (Klarner partisan data coverage). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.29: P(Any Tax Decrease), Partisan Dimensions

	(1) Baseline	(2) Unified	(3) Party	(4) Veto-Proof
During Recession	-0.0425*	-0.0324	-0.0338	-0.0484
	(0.0241)	(0.0314)	(0.0318)	(0.0296)
Rec × Unified Govt		-0.0232		
		(0.0369)		
Rec × Dem Unified			-0.0611*	
			(0.0310)	
Rec × Rep Unified			0.0485	
			(0.0561)	
Rec × Veto-Proof				0.0093
				(0.0371)
Recovery (1–3yr)	0.0129	0.0341	0.0359	0.0145
	(0.0195)	(0.0257)	(0.0260)	(0.0240)
Recov × Unified Govt		-0.0461		
		(0.0284)		
Recov × Dem Unified			-0.0591*	
			(0.0297)	
Recov × Rep Unified			-0.0329	
			(0.0402)	
Recov × Veto-Proof				-0.0079
				(0.0262)
Observations	2600	2600	2548	2548
Adj. R^2	0.111	0.111	0.117	0.113

Notes: The sample covers 1960 to 2011 (Klarner partisan data coverage). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.30: P(Any Tax Increase), Electoral Cycle, Post-1980

	(1) Baseline	(2) Gov Turnover	(3) Election Year
During Recession	0.0829**	0.0573	0.0744*
	(0.0342)	(0.0351)	(0.0399)
Rec × Gov Turnover		0.2434**	
		(0.1028)	
Rec × Election Year			0.0254
			(0.0792)
Recovery (1–3yr)	0.0210	0.0157	0.0229
	(0.0198)	(0.0200)	(0.0218)
Recov × Gov Turnover		0.0474	
		(0.0560)	
Recov × Election Year			-0.0090
			(0.0442)
Observations	1600	1600	1600
Adj. R^2	0.093	0.097	0.092

Notes: The sample covers 1980 to 2011 (Klarner partisan data coverage). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.31: P(Any Tax Decrease), Electoral Cycle, Post-1980

	(1) Baseline	(2) Gov Turnover	(3) Election Year
During Recession	-0.0647**	-0.0535*	-0.0434
	(0.0291)	(0.0295)	(0.0286)
Rec × Gov Turnover		-0.0983	
		(0.0675)	
Rec × Election Year			-0.0709**
			(0.0338)
Recovery (1–3yr)	0.0185	0.0150	0.0201
	(0.0256)	(0.0264)	(0.0263)
Recov × Gov Turnover		0.0607	
		(0.0764)	
Recov × Election Year			-0.0050
			(0.0417)
Observations	1600	1600	1600
Adj. R^2	0.127	0.129	0.127

Notes: The sample covers 1980 to 2011 (Klarner partisan data coverage). All specifications include state and year fixed effects and control for the log of income per capita, the log of population, and an indicator for divided government. Standard errors, clustered at the state level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.8 Sunset Provisions

The survival measure in Table 4 counts any subsequent rate cut, which makes a scheduled expiration and a legislated cut the same event. That distinction matters for the interpretation of the gap. Many states attach an expiration date to a tax increase in order to pass it, and where they do, the rate falls at the appointed time without a further vote, so inertia produces the reversal rather than preventing it. If recession-era increases are disproportionately the ones written with an end date, the survival gap is a feature of how the increases were drafted rather than of what happened afterward.

I coded the increases directly. The sources are the state-by-state revenue tables in NASBO's *Fiscal Survey of the States*, which I read for every edition from 1979 to 2023 (National Association of State Budget Officers 2023), the Tax Policy Center's inventory of temporary measures enacted between 2008 and 2011 (Francis and Moore 2014), and state statutes where those are silent or disagree. An increase counts as carrying a sunset when the enacting measure set a statutory expiration date, and as permanent when the measure describes the rate change and sets none. The record is explicit in both directions. Montana enacted "a three-year 10% surcharge, 1/87–12/89" and North Dakota a one-year surcharge dated 1/87–12/87; Vermont's 1991 provision "expires December 31, 1993" and duly "sunsets from 28 percent to 25 percent" in January 1994; Maine records the "sunset of the personal income tax surcharge" in July 1993 and Rhode Island "allow[s] surtax on upper income taxpayers to sunset" that December. Against those, Illinois converted its temporary rate into a "permanent extension" in 1993, Nevada moved to "eliminate [the] sunset on [its] 1989 increase," and Connecticut's 1991 income tax was written at "1.5 percent for calendar 1991 and 4.5 percent thereafter," with no end date at all.

Table A.32 reports what the coding supports and what it does not. Temporary increases are common: 19 of the 37 increases from 1979 that could be coded carried an expiration, and the instrument is not new, since NASBO counted 24 states using temporary revenue increases in fiscal 1983 against 27 using permanent ones. But recessions do not favour the temporary instrument in any way these data can detect. On the state-year panel a recession raises the probability of a temporary increase by 2.8 percentage points and of a permanent one by 2.0, and the difference between them is 1.3 points against a standard error of 3.0 (Panel B). The two components sum to roughly the 5.7-point effect on the probability of any increase, so the extensive-margin result of Table 2 decomposes cleanly without either component dominating.

Panel C puts the question to the survival gap itself. Conditioning on whether an increase carried a sunset leaves the recession coefficient essentially where it was, moving it from -0.405 to -0.389, so the sunset absorbs about four percent of the gap. The sunset indicator carries -0.150 with a standard error of 0.138:

increases with an expiration date do survive less often, 16 percent against 33, but not significantly so and not by enough to account for the recession differential. The reversal of recession-era increases is therefore not reducible to the way they were drafted.

Two limits bound these conclusions. The coded sample is small, 37 increases with seven in the normal-times cell, so Panel B can detect only a large difference and Panel C bounds the sunset channel rather than eliminating it. And coverage is incomplete by construction: NASBO begins in 1979 and carries no state-level income tax detail before 1986, so the events from the 1960s and 1970s, and the 1983 cohort, cannot be coded from it at all. The unknown share is also unbalanced, 44 percent of recession-linked increases against 63 percent of normal-times ones, because NASBO describes a state's actions in more detail in the years it is raising taxes. That imbalance would inflate a positive finding in Panel B; since Panel B reports a null, it works in the direction of credibility rather than against it.

Table A.32: Sunset provisions and the reversal of recession-era increases

Panel A. Coding coverage, top-rate increases from 1979

	Events	Coded	Unknown	Share with a sunset
Recession-linked	54	30	24	53%
Normal times	19	7	12	43%

Panel B. Does a recession favour the temporary instrument?

	In recession	In recovery
Any increase	0.057 (0.031)	0.002 (0.014)
Temporary increase	0.028 (0.019)	−0.010 (0.009)
Permanent increase	0.020 (0.023)	0.018 (0.010)
Difference, temporary − permanent	0.013 (0.030)	−0.024 (0.014)

Panel C. Does the sunset explain the survival gap?

	Recession-linked	Enacted with a sunset
All 146 events	−0.346 (0.078)	—
The 37 coded events	−0.405 (0.205)	—
Adding the sunset	−0.389 (0.216)	−0.150 (0.138)

Notes: An increase is coded as carrying a sunset when the enacting measure set a statutory expiration date, and as permanent when the measure describes the rate change and sets no end date. Sources are the state-by-state revenue tables in NASBO's *Fiscal Survey of the States* for every edition from 1979 to 2023, the Tax Policy Center's inventory of temporary measures enacted from 2008 to 2011 (Francis and Moore 2014), and state statutes where those disagree or are silent. Panel A covers the increases from 1979, the first year of the *Fiscal Survey*; NASBO carries no state-level income tax detail before 1986, which together with the events predating 1979 is why half the coded cells are empty. Panel B is a linear probability model on 1,350 state-years from 1986 to 2012 with state and year fixed effects and the controls of Table 2, standard errors clustered on the state; the three outcomes are indicators for a state-year in which the state enacted a temporary increase, a permanent one, or an increase whose status could not be coded, and the difference row comes from stacking the first two outcomes and interacting. Panel C regresses the ten-year survival indicator of Table 4 on recession linkage, then on recession linkage and the sunset indicator together, on the events that could be coded. Standard errors in parentheses.