Taxes Without Borders:

Personal Income Taxation and Bank Credit Supply

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Abstract

This paper documents the role of the banking system in transmitting changes in state personal income taxes to the real economy, both in and beyond the state affected by these changes. Using U.S. state tax reforms and granular banking data, I show that increases in personal income taxes in a state lead to lower deposit supply in that state, consistent with taxpayers drawing down their bank balances to absorb part of the decline in disposable income. Banks exposed to these deposit outflows do not replace the lost deposits with alternative funding. As a result, they respond by cutting small business and mortgage lending, both within and outside the affected state. At the aggregate level, counties more exposed to these banks experience weaker real economic activity. Overall, the effects I document are sizable and asymmetric, as tax cuts generate no comparable expansion in deposits or lending.

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1 Introduction

Personal income taxation is one of the main policy instruments available to governments. Accounting for an average of 8% of GDP across OECD countries, personal income taxes have a large impact on aggregate economic activity (Romer and Romer 2010; Barro and Redlick 2011; Mertens and Ravn 2013; Mertens and Montiel Olea 2018; Zidar 2019).[1] Existing research points to several channels for this impact, including the reallocation of business activity (Giroud and Rauh 2019), individual mobility (Kleven et al. 2020), innovation (Akcigit et al. 2022) and consumption (Johnson et al. 2006).

In this paper I present evidence of a new and economically relevant channel through which personal income taxation is transmitted to the economy: the banking system. I show that banks whose deposit base is exposed to state tax increases reduce their credit supply relative to unexposed banks. Consistent with taxpayers drawing down their bank balances to absorb part of the decline in disposable income, higher state personal income taxes reduce the supply of deposits to banks in the taxing state. Because, as I document, banks do not replace deposits with alternative funding, they cut lending. This channel propagates state tax shocks across state lines: banks that lose deposits in one state, also reduce lending in other states where they operate. Lastly, I find evidence of weaker growth in aggregate small business lending in more exposed counties, accompanied by weaker growth in the number of small establishments and employment in bank-dependent industries. These effects are large and arise only after tax increases, as tax cuts do not raise deposits or lending by a comparable amount.

The United States provides a natural laboratory to study how banks transmit personal income tax changes. Its tax system is highly decentralized, and taxes vary widely across states and over time.[2] Many banks also operate across state borders, raising deposits and extending loans in multiple states. To exploit this tax variation, I construct a state-level panel of personal income tax rates from 1977 to 2019 using the NBER TAXSIM calculator (Feenberg and Coutts 1993) and combine it with a state-level panel of bank deposits covering the same period.[3] To study lending, I use bank-county data on small business loans reported

1. For a review of the empirical evidence on the macroeconomic effects of fiscal policy, see Ramey (2019).
2. State and local taxes account for 33% of total tax revenue in the United States, the fourth highest share among OECD countries (Figure A.2). For details on variation in state personal income taxes across states and over time, see Section 2.1. For a survey of cross-country differences in the share of revenue raised at the subnational level, see Brülhart et al. (2015).
3. The sample begins in 1977, the first year for which TAXSIM covers state tax schedules, and ends in 2019, the

under the Community Reinvestment Act (CRA), available from 1996, and data on mortgage originations reported under the Home Mortgage Disclosure Act (HMDA), available from 1990.

To estimate the effect of state personal income taxes on bank deposits, I implement several and complementary strategies for identification (Akcigit et al. 2016). First, exploiting the full state tax variation, I estimate long-run panel specifications at the state level, controlling for state and year fixed effects, and state-level time-varying controls. Second, I exploit within-state year tax differentials stemming from income variation across counties to control for time-varying state-level policy changes or changes in economic conditions contemporaneous with the tax shocks. Third, I use a border discontinuity design by comparing the deposits of two contiguous counties at state borders (Holmes 1998; Dube et al. 2010). This design further controls for local economic conditions. Finally, I estimate local projections and a stacked event study around sharp and persistent state tax changes to trace the dynamics of the deposit response and test for pre-trends.

All the approaches yield the same result: tax increases lead to a significant reduction in bank deposits. In the state-level panel, a 1% increase in the state net-of-tax rate of the median household, the share of income kept after state taxes, is associated with about one percentage point higher annual deposit growth. For the average household, an elasticity of this magnitude means that each additional dollar of state income tax liability lowers deposits by 44 cents. To put this magnitude in context, the tax-driven bank deposit response is similar in magnitude to the deposit outflow that follows a typical increase in the federal funds rate (Drechsler et al. 2017).

The average deposit response, however, masks a marked asymmetry: whereas tax increases lead to a significant deposit reduction, tax cuts produce no deposit expansion. To explore plausible mechanisms driving this asymmetry, I use the taxable base implied by state sales-tax collections as a proxy of consumption, finding that consumption rises after tax cuts but does not fall after tax increases. My results thus suggest that households pay tax increases out of their deposits and spend part of what tax cuts release. This result is in line with household-level studies which document a similar convex relationship between tax cash flows and spending, and attribute it either to mental accounting (Baugh et al. 2021)

last year before the COVID-19 pandemic and the exceptional surge in U.S. bank deposits that accompanied it (Levine, Lin, Tai, and Xie 2021). Extending the state-year deposit panel through 2021 leaves the estimated elasticity virtually unchanged (Table A.4).

or to rational cash management with income uncertainty (Gelman et al. 2022).[4]

The deposit response to personal income taxes could operate through different channels. On the supply side, households may draw down deposits to absorb the fall in disposable income. On the demand side, weaker lending opportunities may lead banks to seek less deposit funding.[5] To distinguish the two, I compare the same bank's deposits across counties inside and outside the reforming state. This comparison assumes that banks pool funding across markets, so that changes in their overall demand for deposits affect their locations similarly. Consistent with the deposit supply mechanism, I find that in the years after a tax increase, deposits in the bank's reform-state counties fall by about 9% relative to its deposits elsewhere, while tax cuts produce no comparable change.

Arguably, there are two main threats to the causal interpretation of my results. First, one could be concerned about states changing their taxes in response to economic conditions (Giroud and Rauh 2019). However, balance tests provide no evidence of systematic differences in economic performance between reforming and other states before the tax changes. Within-county-pair estimates from the border-discontinuity design are also similar to the baseline panel results. Moreover, both local projections and event studies show that deposits in reforming states follow trends similar to those in other states before a reform and diverge only afterward. A second, important, concern is that personal income tax reforms may coincide with changes in other state-level policies. Yet both the panel and event study estimates remain stable when I control for other taxes and state policies or exclude reforms accompanied by corporate income tax changes. Within-state panel comparisons also yield quantitatively similar results while absorbing common statewide shocks.

Having established that tax increases drain deposits in reforming states, in the second part of the paper I investigate whether banks exposed to these outflows pass the funding shock on to their borrowers. Because banks can use deposits collected in one state to fund

4. This asymmetry is not specific to deposits. Hwang (2026) uses narrative measures of federal personal income tax changes and finds that the contraction in output and employment after tax increases is about twice the expansion after tax cuts, driven by taxpayers in the bottom 90% of the income distribution. On the spending side, Barnichon et al. (2022) find that the multiplier of government spending cuts exceeds one, whereas that of spending increases is well below one. Sign-dependent responses to state reforms also appear in other margins, with the direction of the asymmetry varying by outcome. Heider and Ljungqvist (2015) find that firms raise leverage after state corporate tax increases but do not lower it after cuts, and Agrawal and Tester (2024) find that labor supply responds to tax increases but barely to tax cuts, while Fairlie et al. (2025) find that startup formation and employment respond to tax cuts but not to tax increases.
5. Throughout, I adopt a funding perspective and treat households as suppliers and banks as demanders of deposit funding (Khwaja and Mian 2008; Gilje et al. 2016). An alternative convention, common in the industrial organization literature, treats households as demanding deposit services supplied by banks (Drechsler et al. 2017; Egan et al. 2017). Under that convention, the shock I study is a shift in household deposit demand.

loans in another, a loss of deposits in a state can affect lending elsewhere in their network. I therefore define a bank's exposure to a state tax change as the share of its total deposits raised in the affected state in the year before the reform.[6]

A necessary condition for a credit-supply effect of a tax-induced deposit shock is that exposed banks do not fully replace the lost deposits with other sources of funding. If they could turn to wholesale funding (for example, brokered deposits) or draw down liquid assets, their lending would be insulated from the shock.[7] Bank-level regressions show that they do not. I compare banks that raise their deposits in a single state with banks in the neighboring states, which operate in similar markets but whose depositors are not affected by the reform. After a tax increase, deposits at exposed banks fall by 2.7% relative to their neighbors, and wholesale funding does not rise to replace them. Instead, the balance sheet shrinks, and loans fall by 2.6%, mostly through real estate lending. This result is necessary for the deposit supply shock to affect bank lending supply, but it is not sufficient, since a shrinking loan book may also reflect weaker loan demand.

To separate supply from demand, I turn to a within-market design comparing banks with different funding exposure lending in the same county and year, thus holding local loan demand fixed. I focus primarily on small business lending, a segment well suited to identifying the deposit channel because these loans are information intensive, relationship based, and rarely securitized (Drechsler et al. 2017; Dou 2021). I also exclude bank–county pairs in which the bank operates branches, so that common local shocks and branch-based informational advantages are less likely to drive the results (Agarwal and Hauswald 2010).[8]

I find that, after a tax increase, small business lending growth at a fully exposed bank falls by 13.8 percentage points relative to an unexposed bank lending in the same county, with event study estimates revealing no evidence of pretrends. Importantly, banks cut lending both in counties inside and outside the reforming state, with back-of-the-envelope calculations suggesting that around 26% of the credit supply contraction takes place outside

6. See Chakraborty et al. (2025) and Kundu et al. (2025) for recent papers that measure bank exposure through the geography of deposit funding.
7. Deposit funding is stable and largely insensitive to market conditions (Hanson et al. 2015), allowing banks to extend illiquid long-term credit (Li et al. 2023). Wholesale funding, by contrast, is subject to financial frictions (Choi and Choi 2021) and can become fragile during periods of stress (Pérignon et al. 2018).
8. These results show how much more exposed banks cut lending than less exposed banks serving the same market, thus differencing out any response common to all lenders. The same holds for the comparisons across states and counties elsewhere in the paper. It is beyond the scope of this study to measure the aggregate effect of state tax changes on deposits or credit, the so-called missing intercept (Nakamura and Steinsson 2018; Chodorow-Reich 2020; Guren et al. 2021; Wolf 2023). For a recent paper that estimates the aggregate effect of bank funding shocks, see Kundu et al. (2026).

the reforming state. The effects of a state income tax increase can thus travel through banks, reaching borrowers in states whose taxes did not change.

I verify the consistency of these results in several ways. First, I add controls for bank and local market characteristics that may be correlated with state tax increases and confound the estimated lending response. Second, I examine whether the contraction extends to mortgage lending. Third, as a placebo exercise, I estimate the same specifications around state tax cuts. Finally, I assess the sensitivity of the results to alternative samples and variable definitions. Reassuringly, these exercises support the baseline findings, which remain robust to these specification, sample, and measurement choices.

Finally, I examine the aggregate implications of tax-induced deposit shocks for credit and real economic outcomes. To this end, I first estimate how aggregate lending in a county varies with its exposure to banks that raise deposits in reforming states. Specifically, I find that following a tax increase, an increase of one standard deviation in county exposure is associated with 1.0 percentage point lower small business lending growth. By construction, this comparison cannot separate credit supply from credit demand. However, the estimate is virtually unchanged when accounting for additional county controls, which suggests that unobserved local conditions correlated with exposure are unlikely to drive the result. Next, following Doerr et al. (2026), I examine real economic outcomes by comparing establishment and employment growth across firm sizes and industries within the same county. As in the credit supply analysis, this within-county comparison is designed to absorb local demand shocks that affect all firms in a county equally. I find that, following a tax increase, an increase of one standard deviation in county exposure to these banks is associated with 0.2 percentage points lower growth in the number of establishments operated by small firms relative to larger firms and 0.4 percentage points lower employment growth in bank-dependent industries relative to less dependent industries. The concentration of these effects among small firms and bank-dependent industries is consistent with the contraction in aggregate small business lending.

My results carry implications for fiscal policy and banking regulation, as discussed in more detail in the conclusion. On the one hand, as economies become financially integrated, the economic effects of taxation increasingly spread across jurisdictions through banks' balance sheets. By identifying this banking transmission channel, my analysis offers a new perspective for policymakers interested in fiscal coordination. On the other hand, by showing how income taxation shapes banks' lending, my analysis suggests that the design of

optimal banking regulation should take the effects of income taxation into account.

Related Literature My paper contributes to four strands of literature. First, I build on the long-standing literature on the economic effects of personal income taxation. Prior work shows that changes in these taxes have large effects on aggregate output (Romer and Romer 2010; Mertens and Ravn 2013). The effects operate through several margins, including labor supply (Keane 2011), employment (Zidar 2019), business activity (Giroud and Rauh 2019), innovation (Akcigit et al. 2022) and consumption (Johnson et al. 2006; Cloyne and Surico 2017). I contribute to this literature by identifying bank credit supply as an additional margin through which personal income taxes affect the economy.

Second, I contribute to the literature on spatial tax spillovers (see Agrawal et al. 2022, for a review). While prior research has focused on spillovers driven by the mobility of the tax base, including workers (Kleven et al. 2020), firm activity (Giroud and Rauh 2019) and consumers (Baker et al. 2021), I show that the reallocation of economic activity can also be transmitted through banks' balance sheets, without mobility of the tax base. Close to my paper, Smolyansky (2019) shows that banks operating in several states shift lending away from states that raise taxes on bank profits. My paper differs in both the tax and the channel, as I study a tax that falls on households rather than banks, and a channel that runs through bank funding rather than lending opportunities.

Third, I contribute to the broad literature on the role of banks in transmitting economic policy. Much of this literature has focused on monetary policy transmission (Kashyap and Stein 2000; Jiménez et al. 2012; Drechsler et al. 2017), while research on fiscal policy has examined how banks transmit government spending in settings involving public procurement (Gabriel 2024; Bonfim et al. 2025), defense contracts (Auerbach et al. 2020; Goldman et al. 2022), and unemployment insurance (Arslan et al. 2025). Related work examines how public borrowing crowds out deposit funding, private credit, and investment (Krishnamurthy and Vissing-Jorgensen 2015; Li et al. 2026; Pinardon-Touati 2022; Huang et al. 2020).

More closely related to my paper, research on the banking transmission of tax policy has largely focused on taxes levied on banks. These studies document effects on capital and funding structure (Schepens 2016; Devereux et al. 2019), deposit rates (Buch et al. 2016; Lin and Pennacchi 2018), consumer loan pricing (Granja et al. 2025), securitization (Han et al. 2015), mortgage pricing and risk-taking (Jiménez et al. 2024), and the geographic allocation of lending (Smolyansky 2019). Yet evidence on the banking transmission of taxes levied on other agents is more limited. Bird et al. (2022) show that a tax holiday on repatriated foreign

earnings led banks exposed to multinational borrowers to expand lending to other firms. Carletti et al. (2021) show that an investment income tax reform in Italy induced households to shift funds from bank bonds to deposits, with exposed banks expanding credit lines and long-term lending to low-risk firms. My paper adds to this literature by studying how banks transmit personal income tax changes.

Finally, I contribute to the literature on the role of bank deposits for bank credit (Aguirregabiria et al. 2025; Koont and Walz 2026). Prior work draws on variation in deposit funding associated with monetary policy (Drechsler et al. 2017), local demographics (Becker 2007; Doerr et al. 2024), natural resource booms (Gilje et al. 2016; Gilje 2019; Wang 2021), agricultural productivity (Bustos et al. 2020), lottery winnings (Parra 2022), stock market returns (Lin 2020), natural disasters (Kundu et al. 2025), and bank runs (Khwaja and Mian 2008; Ivashina and Scharfstein 2010; Schiozer and Oliveira 2016). Closest to my paper, Arslan et al. (2025) show that more generous unemployment insurance reduces bank deposits and small business lending, consistent with a decline in precautionary saving. I study a different fiscal instrument, state personal income taxation, and examine how it affects bank funding by changing households' disposable income. Moreover, I estimate the effects of tax increases and tax cuts separately, allowing me to assess whether deposits and lending respond symmetrically to changes in the same tax.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting and the data. Section 3 and Section 4 present the deposit response across all tax changes and large tax reforms, respectively. Section 5 traces the funding shock and presents the credit supply response. Section 6 examines the regional aggregate implications on lending and economic activity, and Section 7 concludes.

2 Data and Measurement

The analysis combines state personal income tax rates with data on bank deposits, balance sheets, loan originations, and local economic activity. I first construct a state-year deposit panel covering 1977–2019. I then use branch-level deposit data to identify where each bank collects deposits. I combine these data with bank balance sheets and bank–county–year loan originations to examine how tax changes in the states where banks raise deposits affect their funding and lending across counties. Finally, I combine the lending data with measures of local business activity and state economic conditions. These data allow me to trace tax-

induced deposit changes through exposed banks to local credit markets and, ultimately, to economic activity. Table A.1 defines all variables, and Tables A.2 and A.3 report summary statistics for the estimation samples of the deposit and credit analyses, respectively.

2.1 State Personal Income Taxes

Personal income taxation is a central component of U.S. public finance and accounts for the largest share of tax revenue at both the state and federal levels, approximately 40% and 55%, respectively (Robinson and Tazhidinova 2025). Individuals face a common federal income tax and, in most states, a separate state personal income tax. Although liabilities are assessed annually, taxes on labor income are collected largely through employer withholding. Changes in tax liabilities can therefore affect households' current disposable income before they file their annual returns. Labor income constitutes the main component of the tax base for most households, although taxable income may also include business and capital income.[9]

State income taxes are collected mainly through employer withholding. When a rate change takes effect, the state revises its withholding tables and employers apply them from the effective date, so take-home pay adjusts in the first paycheck after the change. Retroactive changes enacted during the year are collected through higher withholding over the remaining months or at filing in the following spring, and changes in states that do not index their brackets reach paychecks continuously through nominal withholding formulas. Following Romer and Romer (2010), I date each change by the year in which tax liabilities change. Since deposits are measured on June 30, the first event year captures about half of the annual change in withholding, and the remainder, together with the settlement of the previous year's liability at filing, falls in the following year.

Federal and state income taxes are legally distinct but interact through the definition of taxable income. The federal government applies a common progressive schedule nationwide, whereas most states begin with federal adjusted gross income and then apply their own exclusions, deductions, exemptions, credits, and rate schedules. These provisions vary substantially across states.[10] Most states use graduated rates, some impose a flat rate, and

9. Figure A.3 shows the composition of income across brackets of adjusted gross income in 2019. Wages account for 82% of total income in the bracket that contains the median return, $40,000 to $50,000, and for 17% above $10 million, where capital gains account for 49%.

10. Figure A.4 shows the structure of the tax in each state and year. Between 1977 and 2019, the number of flat-rate states rose from five to nine as Colorado (1987), Utah (2008), North Carolina (2014), and Kentucky (2018) replaced graduated schedules with a single rate.

others levy no tax on wages and salaries.[11] The interaction also runs in the opposite direction because, under the rules prevailing in each year, eligible state income tax payments may be deducted when calculating federal taxable income. Some local jurisdictions also levy personal income taxes, but these taxes are present in fewer than 20% of U.S. counties and account for about 2% of county tax revenue on average (Baker et al. 2025).

Construction of the tax measure. I measure personal income tax burdens using the state average tax rate (ATR) faced by a hypothetical taxpayer at the median of the national income distribution. The state ATR is state income tax liability divided by gross income. It is therefore not a statutory rate, since it incorporates the state tax schedule together with the deductions, exemptions, credits, and interactions with the federal tax system that determine the taxpayer's effective burden. The average rather than the marginal tax rate is the relevant object for this paper because the deposit channel operates through the level of after-tax resources, rather than through the return to an additional dollar of income. Nevertheless, estimates based on state marginal tax rates are similar and are reported as a robustness check.

I construct the tax rates by applying NBER TAXSIM (Feenberg and Coutts 1993) to taxpayers at fixed positions of the national income distribution, following, among others, Moretti and Wilson (2017), Akcigit et al. (2022), and Campello et al. (2024). For each year, I evaluate the same national income points in every state. This keeps cross-state differences in the tax measure from mechanically reflecting differences in local income distributions or the sorting of taxpayers across states. TAXSIM returns federal and state liabilities and marginal tax rates, from which I construct the federal, state, and combined ATRs. Incomes at each percentile are obtained from the World Inequality Database (WID). The county-level measure applies the same calculator to average income per return from the IRS Statistics of Income. Appendix A describes the income data, filing assumptions, and income composition in detail.

The baseline measure is evaluated at the median because deposits are held broadly across the income distribution rather than being concentrated exclusively among top earners (Chakraborty et al. 2025).[12] I also construct rates at the 90th and 99th percentiles. The

11. Throughout the 1977–2019 sample period, Florida, Nevada, New Hampshire, South Dakota, Tennessee, Texas, Washington, and Wyoming did not levy a broad-based individual income tax on wages and salaries. Alaska levied such a tax through 1979 but repealed it in 1980, while Connecticut introduced one in 1991. New Hampshire and Tennessee nevertheless taxed interest and dividend income during the sample period.
12. Chakraborty et al. (2025) show that U.S. households with income below $50,000 hold about one fifth of bank deposits, while households with income above $200,000 hold less than 15%.

90th-percentile measure is used for robustness, while the 99th-percentile measure allows me to distinguish changes affecting the median from tax changes concentrated among top earners, whose response may also operate through geographic mobility.[13] Throughout the paper, τ denotes the state ATR, so that the variation comes from the tax burden imposed by each state. In the state-year panel, tax changes enter as changes in the log net-of-tax rate, $\Delta \log(1 - \tau)$.

Tax variation. State personal income taxes vary substantially across states and over time. Figure A.5 plots the cross-state distribution of the state ATR at the median of the national income distribution. The distribution remains wide throughout the sample. Several states levy no personal income tax and therefore lie near zero, while, among states with an income tax, the gap between the 90th and 25th percentiles of the rate averages 2.15 percentage points over 1977–2019, reflecting persistent differences between low- and high-tax states. This geographic dispersion is also visible in Figure A.6, which maps the state ATR at the median in 1977 and 2019 and shows that high- and low-tax states are not confined to a single region or period.

This cross-sectional dispersion is accompanied by frequent changes in tax policy. Figure A.7 uses the statutory state tax series of Baker et al. (2025) to summarize the frequency and size of changes in the statutory marginal rate faced by the median taxpayer in each state. In the average year, around 15% of states change the applicable statutory rate, and nonzero changes average 0.57 percentage points. The sample contains 116 increases, averaging 0.68 percentage points, and 212 cuts, averaging 0.50 percentage points. Tax changes also differ across the income distribution. The correlation between annual changes in the state ATR at the 10th and 99th percentiles is only 0.04 (Figure A.8). Changes in the state ATR at the median therefore need not coincide with changes affecting top earners. This variation allows the median and top-percentile measures to distinguish broad-based tax changes from reforms concentrated in particular parts of the tax schedule.

2.2 Deposits and Bank Balance Sheets

Deposits. I build the state-year deposit dataset from two sources. For 1977–1986, I use the Call Reports, described below, which report deposits at the bank level. I assign each bank's

13. The literature on the mobility of high-income earners in response to taxation is extensive. See, for example, Kleven et al. (2013), Kleven et al. (2014), Akcigit et al. (2016), Moretti and Wilson (2017), Rauh and Shyu (2024), and Kleven et al. (2020) for a review.

domestic deposits to its headquarters state and aggregate across banks within each state to obtain state-level deposit totals. This assignment is well suited to this period because interstate branching was tightly restricted and interstate expansion, where permitted, generally took place through separately chartered bank subsidiaries rather than branches of a single bank (Morgan et al. 2004). Commercial banks' deposit bases were therefore closely tied to their headquarters states. From 1987 onward, I use the Summary of Deposits (SOD), compiled annually by the Federal Deposit Insurance Corporation (FDIC). The SOD reports branch-level deposits for depository institutions operating in the United States as of June 30 of each year, together with each branch's location and parent institution. I aggregate these branch-level deposits by state to obtain state-level deposit totals. The two sources are comparable at the bank level because SOD reporting instructions require deposits reported across an institution's branches to reconcile with its domestic deposits in the corresponding Call Report. In 1987, the first year in which both measures are available, they yield almost identical state deposit totals, and their annual growth rates remain closely aligned until interstate branching expands following the Riegle-Neal Act, in 1994 (Figure A.9).[14] I restrict both sources to FDIC-insured commercial banks and express deposits in 2010 dollars. The resulting state panel covers the fifty states over 1977–2019. The average state holds $102 billion in deposits in 2010 dollars, and state deposits grow by 2.7% per year on average (Table A.2).

Bank balance sheets. Bank-level information comes from the Call Reports filed quarterly by all FDIC-insured banks. I obtain the filings covering 1987–2019 from the FDIC Research Information System and the historical filings covering 1977–1986 from the Federal Reserve Bank of Chicago archive. Following Gilje et al. (2016), I measure the bank-level variables used in the analysis from the fourth-quarter filing.[15] On the liability side, I construct total deposits, savings deposits, time deposits, wholesale funding, total liabilities, and the deposit rate, measured as interest expense on deposits relative to total deposits. On the asset side, I construct total assets, cash and balances due, securities, total loans, real estate loans, and commercial and industrial loans. I also construct bank size, the equity ratio, return on assets, the securities share, the non-interest income share, and the shares of real estate and commer-

14. Banks came to collect deposits across state lines as geographic banking restrictions were relaxed, culminating in the Riegle-Neal Act of 1994 (Jayaratne and Strahan 1996; Rice and Strahan 2010). For related evidence on the integration of local credit markets through banks, see Becker (2007); Gilje et al. (2016); Cortés and Strahan (2017); Bustos et al. (2020); Parra (2022).
15. I use the June filing only when matching Call Report deposits to the SOD, which reports branch deposits as of June 30.

cial and industrial loans in assets.[16] Ratios and growth rates are winsorized at the 1st and 99th percentiles within each year. In the estimation sample, deposits account for an average of 94% of bank liabilities (Table A.3), underscoring their central role in bank funding.

2.3 Bank Lending

Small business lending. Small business lending is particularly relevant in this setting for two reasons. Small businesses account for nearly half of U.S. economic activity and employment (Kobe and Schwinn 2018), making their access to credit quantitatively important for the real economy. At the same time, small business loans are information intensive and less frequently securitized than other types of lending, such as residential mortgages, tying their provision closely to the originating bank's balance sheet and deposit funding (Drechsler et al. 2017). Accordingly, banks remain the primary source of external credit for small businesses in the United States (Board of Governors of the Federal Reserve System 2025).

Data on small business loans comes from the Community Reinvestment Act (CRA) disclosure files collected by the Federal Financial Institutions Examination Council (FFIEC). Reporting banks disclose, for every county in which they lend, the annual number and dollar volume of small business loan originations, defined as loans of up to $1 million. I use the raw disclosure files from 1996, the first year available, through 2019 to construct a bank–county–year panel of originations, which I match to the Call Reports and the SOD using banks' regulatory identifiers. Reporting is mandatory only for banks above an asset threshold, so the panel is weighted toward larger institutions (Drechsler et al. 2017).[17] Despite this restriction, the CRA provides the most comprehensive coverage of U.S. small business lending, capturing approximately 86% of all loans below $1 million (Greenstone et al. 2020). Loan amounts are deflated to 2010 dollars. For the credit-supply analysis, the outcome is the symmetric growth rate of small business lending between adjacent years, defined as the change divided by the average of the two levels.[18] This measure is a second-order ap-

16. The Call Report items are deposits in domestic offices (RCON2200), money market and other savings deposits (RCON6810, RCON0352), time deposits (RCON6648, RCON2604), total liabilities (RCFD2948), interest expense on domestic deposits (RIAD4508, RIAD4509, RIAD4511, RIAD4174, RIAD4512), total assets (RCFD2170), cash and balances due (RCFD0081, RCFD0071), securities (RCFD1754, RCFD1773), total loans and leases (RCFD1400), real estate loans (RCFD1410), commercial and industrial loans (RCFD1766), total equity capital (RCFD3210), net income (RIAD4340), total interest income (RIAD4107), and total noninterest income (RIAD4079). Wholesale funding is total liabilities less domestic deposits and subordinated debt (RCFD3200), following Drechsler et al. (2017).
17. The bank asset threshold was $250 million until 2005, when it increased to $1 billion, with annual inflation adjustments beginning in 2007.
18. With Y_{bct} denoting real small business loan originations of bank b in county c in year t, the growth rate is

proximation to the log change and is bounded between -2 and 2. Unlike the log change, it accommodates a bank's entry into and exit from a county, which is why it has become standard in the literature (Huber 2018; Cortés et al. 2020). Following Drechsler et al. (2017), I drop from the sample those bank–county cells below $100,000 in the year before the reform. I also exclude banks from the control group if more than 5% of their deposits are located in other reforming states within the event window.

Mortgage lending. Mortgage lending data come from the Home Mortgage Disclosure Act (HMDA). Under HMDA, lenders report each originated loan together with its amount and purpose, the county of the property, whether the loan was sold within the year and to whom, and borrower characteristics such as reported income, gender, and race or ethnicity. HMDA reporting begins in 1987, but respondents can be matched consistently to bank regulatory identifiers only from 1990 onward using the lender file compiled by Robert Avery. The mortgage lending sample therefore covers 1990–2019. I aggregate originations to the bank–county–year level. Using the purchaser-type codes, I separately construct total originations, loans retained on the bank's balance sheet, and loans sold into the secondary market within the year. This decomposition of the data allows me to examine whether the lending response differs according to whether banks retain the loans they originate or transfer them off their balance sheets. Following Casado and Martinez-Miera (2024), I retain bank–county cells with at least ten loans in the previous year to limit the influence of very small lending cells. The results are robust to removing this threshold.

2.4 Economic Activity and Other Data

County data. County-level data come from several sources. Average income per tax return comes from the county files of the IRS Statistics of Income, available from 1989 onward, and is used to construct the county-level average personal income tax rate. Local economic activity comes from the Census Bureau's County Business Patterns, which report annual establishment counts and employment by county, two-digit NAICS industry, and establishment size class. Industry reliance on bank credit is built following Doerr et al. (2026) and comes from the 2007 Survey of Business Owners (SBO). County population and personal income come from the Bureau of Economic Analysis (BEA).

$\Delta Y_{bct} = 2\,(Y_{bct} - Y_{bc,t-1})/(Y_{bct} + Y_{bc,t-1})$.

State-level and other data. State economic conditions come from the Bureau of Economic Analysis (BEA) and the Bureau of Labor Statistics (BLS). The BEA provides population, personal income per capita, and state GDP, while the BLS provides the unemployment rate. Nominal variables are deflated to 2010 dollars. The top corporate income tax rate, the general sales tax rate and revenue, and the gasoline, cigarette, and spirits excise tax rates come from the state tax panel of Robinson and Tazhidinova (2025). The state minimum wage comes from the State Policy Project of Michigan State University, and the unemployment insurance replacement rate, defined as the average weekly benefit divided by the average weekly wage in covered employment, is built from the Department of Labor Financial Data Handbook 394. Finally, the country-level evidence combines the historical deposit panel of Jamilov et al. (2025) with average personal income tax rates from the OECD Taxing Wages tables and macro controls from Müller et al. (2025), described in Appendix C.

3 Personal Income Taxes and Bank Deposits

This section establishes how personal income tax changes affect bank deposits. I begin with a state-year panel covering 1977–2019, which uses the full distribution of tax changes to estimate the average deposit response. Next I use local projections to trace its dynamics and compare tax increases with tax cuts.

3.1 Baseline Estimates

I begin the panel analysis with the following baseline specification:

$$\Delta \ln D_{st} = \beta \, \Delta \ln(1 - \tau_{st}) + \gamma' \Delta X_{s,t-1} + \delta_t + \delta_s + \varepsilon_{st}, \tag{1}$$

where D_{st} denotes bank deposits in state s and year t, and τ_{st} is the state ATR at the median of the national income distribution. Year fixed effects (δ_t) absorb aggregate shocks to deposit growth and changes in the federal tax schedule that are common across states, so identification comes from cross-state variation in tax burdens induced by state tax policy. State fixed effects (δ_s) in the differenced specification allow states to have different average deposit growth rates, which is equivalent to allowing for state-specific linear trends. The vector X_{st} contains log population, the unemployment rate, and log income per capita, and $\Delta X_{s,t-1}$ is its change between $t-2$ and $t-1$, intended to capture changes in state eco-

nomic conditions. Measuring these changes before the tax change keeps the controls from absorbing any response of local economic conditions to the reform itself. Regressions are weighted by state deposits in 1977. Standard errors are clustered at the state level, allowing for arbitrary serial correlation within states over time.

The coefficient of interest, β, captures the elasticity of deposit growth with respect to the net-of-tax rate. Thus, a positive estimate implies that a higher household retention rate increases deposit growth, or equivalently that a tax increase that lowers $1 - \tau_{st}$ reduces it. Identification requires that, conditional on the fixed effects and controls in equation (1), changes in the state net-of-tax rate at the median are uncorrelated with other state-level shocks that affect deposit growth.

Table 1 reports the estimates. My preferred specification in column (3), which includes state and year fixed effects and state-level controls, yields an elasticity of 0.98. Columns (4) and (5) account for the possibility that personal income tax reforms coincide with other tax and policy changes.[19] Specifically, column (4) adds contemporaneous changes in corporate and sales tax rates, which together with the personal income tax account for about 80% of state tax revenue (Robinson and Tazhidinova 2025). Column (5) further controls for gasoline, cigarette, and alcohol excise taxes and for two policies that directly affect household income, the state minimum wage and the unemployment insurance replacement rate, defined as the average weekly benefit relative to the average weekly wage. The point estimate remains stable across columns.

Column (6) examines whether the deposit response differs across the income distribution by including as control variable the state net-of-tax rate at the 99th percentile.[20] The coefficient on the 99th-percentile rate is small and statistically insignificant, while the coefficient on the median rate remains essentially unchanged. This result suggests that the deposit response primarily reflects tax changes affecting middle-income households rather than top earners. In this respect, the deposit response mirrors the evidence on real effects, where personal income tax changes for the bottom 90% move employment and output while those for the top 10% have little effect (Zidar 2019; Hwang 2026).[21]

The elasticity in column (3) implies that deposits fall by about 44 cents for each addi-

19. Annual changes in the taxes and policies included in the table are weakly correlated in the data, as shown in Figure A.10.
20. Annual changes in these two measures are only moderately correlated, at 0.33 (Figure A.10), leaving substantial independent variation.
21. Table A.5 re-estimates the benchmark specification using the state ATR at the 99th percentile instead of the median. Yet even when included on its own, the estimated coefficient is small and statistically insignificant.

tional dollar of state income tax liability.[22] To put this estimate in perspective, an average marginal propensity to consume of 0.35 (Sokolova 2023) implies that a one-dollar decline in disposable income reduces consumption by 35 cents and saving by 65 cents. Therefore, the estimated deposit decline is roughly 70% as large as this implied reduction in saving. The deposit channel of monetary policy (Drechsler et al. 2017) provides a second benchmark. At the average increase in the federal funds target and the mean level of local deposit-market concentration, the estimated deposit channel implies a 0.52 percentage point decline in annual branch deposit growth.[23] My estimated elasticity implies that a typical large increase in state personal income taxes lowers state bank deposits by a comparable 0.47% in the year of the reform.[24]

3.2 Dynamics and Asymmetry

The panel estimates above capture the average contemporaneous relationship between personal income taxes and deposit growth and assume symmetric responses to tax increases and cuts. I now use local projections (Jordà 2005) to trace the deposit response over time and relax this symmetry assumption. Formally, for each horizon h I define the pooled projection as

$$\ln D_{s,t+h} - \ln D_{s,t-1} = \alpha_s^h + \psi_t^h + \theta^h \, \Delta \ln\left(1 - \tau_{st}\right) + \lambda_h' \Delta X_{s,t-1} + u_{st}^h, \tag{2}$$

where the dependent variable is the cumulative change in deposits from the year before the tax change to horizon h, with the coefficient at $h = -1$ normalized to zero. Tax changes, the controls, and the fixed effects are those of column (3) of Table 1, so the sequence $\{\theta^h\}$ traces the dynamic counterpart of the static elasticity. I estimate equation (2) separately for each horizon h, allowing the outcome windows of successive reforms to overlap. Standard errors are clustered at the state level to account for the resulting serial correlation within states. I also estimate the specification at negative horizons to test for differential movements in

22. I convert the elasticity into dollars using the median ratio of deposits to personal income (0.445) and the mean state average tax rate (2%). The implied decline in deposits per dollar of additional tax liability is $0.975 \times 0.445/(1 - 0.02) \approx 0.44$.
23. Column (4) of Table III in Drechsler et al. (2017) interacts the change in the federal funds target with the deposit-market concentration of the branch's county (Branch-HHI). The coefficient on this interaction implies a decline of $1.827 \times 0.22 \times 1.29 \approx 0.52$ percentage points at the mean Branch-HHI of 0.22 and the average target increase of 1.29 percentage points.
24. Among the large reforms studied in Section 4, the median increase in the state average tax rate is 0.47 percentage points. At the mean state tax rate of 2%, this corresponds to a 0.48% decline in the net-of-tax rate and an implied deposit decline of $0.975 \times 0.48 \approx 0.47\%$.

deposits prior to the tax change, analogous to the leads in an event-study design.[25]

At $h = 0$ the projection coincides with column (3) of Table 1, and the impact response is the same elasticity of 0.98. The response rises to about 1.8 one and two years after the change, although estimates beyond impact are imprecise, and the leads are small and statistically insignificant (Figure A.11).

Thus far, I have imposed a common slope for tax increases and cuts in the local projections. To assess whether the response is asymmetric, I next estimate separate coefficients for increases and cuts in the same regression at each horizon.

$$\ln D_{s,t+h} - \ln D_{s,t-1} = \alpha_s^h + \psi_t^h + \theta_+^h x_{st}^+ + \theta_-^h x_{st}^- + \lambda_h' \Delta X_{s,t-1} + u_{st}^h, \tag{3}$$

where $x_{st}^+ = \max\{\Delta \ln(1-\tau_{st}), 0\}$ captures tax cuts and $x_{st}^- = \min\{\Delta \ln(1-\tau_{st}), 0\}$ captures tax increases, both entering the same regression so that the two responses share a sample, fixed effects, and controls. Panel (a) of Figure 1 plots the response to tax cuts, and panel (b) plots the response to tax increases with the sign reversed so that negative values denote deposit declines. While tax cuts produce a modest deposit response concentrated around the year of the change, tax increases generate a larger decline that grows over the projection horizon.[26] Therefore, the pooled elasticity masks an asymmetric response to tax increases and tax cuts.

What might explain this asymmetry? Intuitively, a change in disposable income has to show up in some margin of the household budget, and deposits are only one of them. If deposits absorb tax increases but not tax cuts, consumption should display the opposite pattern, rising after cuts and changing little after increases. I examine this margin using the taxable base implied by state sales-tax collections as a proxy for consumption.[27] As shown in Figure A.12, consumption rises after tax cuts but does not fall after tax increases. Households thus appear to pay tax increases out of their deposits and to spend part of what tax cuts release. Consistent with these patterns, household-level evidence points to a similar asymmetry in spending around tax payments and refunds. On the one hand, Baugh et al. (2021) find that the same households smooth consumption when making tax payments and increase spending after receiving refunds. They interpret this pattern as evidence of mental

25. See Dube et al. (2025) for the correspondence between local projections and event-study designs.
26. The difference between the two responses is significant at the 10% level at horizon two and at the 5% level at horizon three ($p = 0.06$ and $p = 0.02$). A joint test over horizons zero to three yields $p = 0.18$.
27. The proxy is general sales tax revenue divided by the general sales tax rate, both from Robinson and Tazhidinova (2025), computed for states with a retail sales tax. Revenue is reported by state fiscal year and the rate by the calendar year in which it takes effect. To keep the two aligned, the estimation uses only state-years in which the sales tax rate does not change within three years of the income tax change, which also excludes income tax changes enacted together with sales tax changes.

accounting. On the other hand, Gelman et al. (2022) show that rational cash management under income uncertainty can generate the same asymmetry and provide supporting evidence on spending out of tax refunds. However, given the aggregate nature of my data, distinguishing between these explanations is beyond the scope of this study.

3.3 Robustness

Within-state variation. The state-level estimates could still reflect other statewide policies that coincide with personal income tax changes. I address this concern by exploiting within-state variation in tax exposure across counties. Differences in county income imply that the same change in a state tax schedule can generate different changes in the state ATR across counties. I can therefore compare counties within the same state and year, absorbing statewide economic and policy shocks with state×year fixed effects. To this end, I construct county-level tax rates by applying TAXSIM, under the same filing assumptions as in the state analysis, to each county's average income using the IRS Statistics of Income county files, available since 1989.[28] Table 2 reports the results. Column (1) compares counties within the same state and year. The state×year fixed effects absorb statewide shocks, including other state policies and statewide economic conditions, so the elasticity is identified only from differences in the tax change across counties of the same state. These differences arise because a given change in the state tax schedule shifts the state ATR by different amounts at different income levels. The resulting variation is therefore tied to county income, and counties with different incomes may follow different deposit paths for reasons unrelated to taxes. Columns (2) and (3) address this concern by grouping counties into terciles of income per return, measured two years earlier, within each state and year. Income tercile×year fixed effects in column (2) remove shocks common to counties in the same relative income position across all states in a given year. Income tercile×state×year fixed effects in column (3) compare only counties in the same state, year, and income tercile. The remaining variation comes from income differences within a tercile, which still map into different tax changes because state tax schedules are nonlinear in income. Across all three specifications, the estimated elasticity is remarkably stable and close to the state-level benchmark.

28. Following the taxable-income literature (e.g., Gruber and Saez 2002), I calculate tax exposure using predetermined income, measured two years before the tax change. The specification is $\Delta \ln D_{ct} = \beta \Delta \ln(1 - \tau_{ct}) + \delta_c + \delta_{st} + \varepsilon_{ct}$, where δ_c denote county fixed effects, and δ_{st} state×year fixed effects. Standard errors are clustered at the state level. Figure A.13 maps the state ATR across counties in 2019, while summary statistics at the county level are reported in panel B of Table A.2.

Measurement, sample, and inference. The panel estimates are robust to alternative choices in tax measurement, sample construction, weighting, and inference. The estimated elasticity remains close to the baseline when I compute the state ATR for a single filer ($p = 0.105$), exclude recession years,[29] extend the sample through 2021, estimate an unweighted version of the baseline regression, or winsorize annual deposit growth. The results are also similar when I replace the state ATR with the state marginal tax rate, evaluate the state ATR at the 90th percentile of the national income distribution, or use the combined state and federal net-of-tax rate (Table A.4). The statistical significance of the estimates is also insensitive to the inference procedure. Confidence intervals based on two-way clustering (Cameron et al. 2011), Newey–West (Newey and West 1987), Driscoll–Kraay (Driscoll and Kraay 1998), Conley spatial (Conley 1999), and wild cluster bootstrap (Cameron et al. 2008) standard errors all exclude zero (Figure A.14). The result is likewise not driven by the geographic assignment of deposits. If anything, the elasticity is slightly larger in the pre-Riegle-Neal period, when branching restrictions tied banks' deposit bases more closely to their home states (Table A.6). The estimate is also unchanged when I exclude credit card and internet banks, whose nationally collected deposits are particularly likely to be booked at a single office (Kundu et al. 2025) (Table A.4).

Country-level evidence. A final concern is external validity. U.S. states set their own income taxes but share a federal tax system, a common currency, and an integrated banking market. The state-level estimates may therefore not generalize to countries where personal income taxes are set nationally. Country-level data cannot reproduce the reform-based design, since national reforms are few and deposits are observed only in the aggregate. They nevertheless allow me to test a central implication of the mechanism. If households absorb tax increases partly by drawing down their deposits, aggregate deposits should decline following national tax increases. I examine this prediction using a panel of 21 advanced OECD economies over 2000–2019. I estimate a country-level version of the joint local projection in equation (3), including country and year fixed effects and lagged population, GDP per capita, and unemployment as controls.[30] In line with the state-level evidence, deposits decline in the years following tax increases, while the response to tax cuts is statistically indistinguishable from zero (Figure A.15).

29. Deposits may attract flight-to-safety inflows during downturns (Gatev and Strahan 2006; Cornett et al. 2011).
30. Appendix C describes the data, sample, and specification.

I next turn to large tax reforms, which provide the variation needed to trace the deposit outflows that follow tax increases through bank balance sheets and into local credit supply.

4 Large Tax Reforms and the Deposit Funding Shock

The state-year panel uses the full distribution of tax changes to estimate the average relationship between personal income taxes and deposits. The reform-based analysis serves a different purpose. It isolates discrete state tax changes that can be mapped into bank funding exposure and carried into the subsequent balance-sheet and credit-supply analyses. For this purpose, tax changes must be large enough to generate meaningful cross-bank variation given differences in deposit footprints and sufficiently isolated to provide a clearly defined event date.

4.1 Reform Selection and Stacked Design

Following Akcigit et al. (2022), I first identify candidate reforms using the one-year change in the state ATR at the median. I classify a state-year as a candidate reform when the absolute tax-rate change falls in the top decile of the 1977–2019 distribution.[31] The resulting threshold is 0.23 percentage points. On average, candidate tax increases raise the state ATR at the median by 0.48 percentage points, while candidate tax cuts lower it by 0.61 percentage points. Relative to the mean combined federal and state ATR at the median, 8.3%, these changes raise and lower the total income tax burden of the median household by 5.8% and 7.3%. I use a seven-year event window spanning the three years before and after each reform, chosen to balance the length of the event study against the prevalence of overlapping reforms. I keep only reforms for which the state experiences no other large tax change of either sign within this window. These restrictions leave 57 reforms, comprising 24 tax increases and 33 tax cuts, distributed across states and years rather than concentrated in a small set of either (Figure A.16).

To avoid the problematic comparisons that can arise in conventional two-way fixed-effects DiD designs when treatment timing varies across states (de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021), I study these reforms using a stacked difference-in-differences approach (Gormley and Matsa 2011; Cengiz et al. 2019). Each

31. Other recent applications of stacked designs to state-level tax changes include Butters et al. (2022), Welsch (2023), Jo and Klopack (2025), and Sapollnik and Swonder (2025).

reform defines a cohort. For each cohort, I construct a separate seven-year panel, or stack, containing the reforming state and a set of clean control states. A state is eligible for the control group if it experiences no large tax change of either sign during the same calendar-year window. I then append the cohort-specific stacks into a single data set.

Let t_r denote the reform year of cohort r, let $Treat_{sr}$ indicate that state s is the reforming state in that cohort, and let $\mathbf{1}\{t = t_r + h\}$ indicate that year t is h years from the reform, with negative values of h denoting pre-reform years. I define the post-reform indicator as $\text{Post}_{rt} = \sum_{h=0}^{3} \mathbf{1}\{t = t_r + h\}$. For an outcome Y_{srt}, I estimate the following event-study and difference-in-differences specifications:

$$Y_{s,t_r+h} - Y_{s,t_r-1} = \sum_{\substack{h=-3 \\ h\neq -1}}^{3} \beta_h \, Treat_{sr} \, \mathbf{1}\{t = t_r + h\} + \Gamma' W_{sr}^{\text{pre}} + \delta_{tr} + \varepsilon_{srt}, \tag{4}$$

and

$$Y_{s,t_r+h} - Y_{s,t_r-1} = \beta \, (Treat_{sr} \times \text{Post}_{rt}) + \Gamma' W_{sr}^{\text{pre}} + \delta_{tr} + \varepsilon_{srt}. \tag{5}$$

In both specifications, δ_{tr} denotes year$\times$cohort fixed effects, and outcomes are measured relative to the year preceding the reform. In equation (4), event time $h = -1$ is the omitted period, and β_h traces the differential cumulative change in the reforming state relative to its clean control states at each event time. Both specifications include a vector of pre-reform state controls (W_{sr}^{pre}), state population, the unemployment rate, and income per capita, measured in the year before the reform and interacted with the event-time indicators, so that states with different pre-reform economic conditions may follow different paths through the event window. As in the panel analysis, regressions are weighted by state deposits in 1977. Equation (5) replaces the event-time interactions with a single Treat$\times$Post indicator, so β summarizes the average differential change over the post-reform window. I estimate both specifications separately for tax increases and tax cuts and cluster standard errors at the state level, following the approach suggested by Wing et al. (2024).

4.2 Deposit Response Around Large Reforms

Before turning to deposits, I first establish that the selected events generate large and persistent changes in tax burdens. To this end I estimate equation (4) using the state ATR at the median as the outcome. Figure 2 shows no differential movement in tax rates before

the reforms. In the reform year, the state ATR at the median changes by about 0.5 percentage points in the expected direction, rising after tax increases and falling after tax cuts, and these changes persist throughout the event window. Figure A.17 shows that the same events generate tax-rate changes of similar magnitude from roughly the 30th through the 99th percentiles, with the bottom of the distribution as the main exception. The selected events therefore capture broad changes in state personal income taxation rather than adjustments narrowly targeted at the median household. Taken together, these results show that the reform sample provides large, persistent, and broadly distributed variation in personal income tax burdens.

I next estimate equation (4) using the logarithm of deposits as the outcome. Figure 3 reports the dynamic estimates.[32] Deposits do not move differentially before either type of reform. Because reforms are dated by the year in which tax rates change rather than the year in which they are enacted, the flat pre-reform path also suggests that households do not adjust their deposits in anticipation of tax changes. Following a tax increase, deposits fall by 1.7% in the reform year, and the decline builds to 6.4% three years after the reform.[33] Tax cuts generate no comparable response, either on impact or at subsequent horizons. Table A.7 summarizes these dynamics with the coefficient β of equation (5). Over 1977–2019, deposits are on average 4.5% lower following a tax increase (p-value = 0.04), whereas the estimate following a tax cut is small and statistically insignificant (p-value = 0.60). Columns (2) and (3) repeat the estimation on the reforms that fall within the coverage of the lending data used later in the paper. Tax increases lower deposits by 7.2% over 1990–2019, the period covered by the mortgage data, and by 4.1% over 1996–2019, the period covered by the small business lending data, while the estimates following tax cuts remain close to zero in both windows.

The event-study estimates are larger than what the local projections imply for the same first stage. A typical large tax increase raises the state ATR at the median by about 0.5 percentage points, which lowers the state net-of-tax rate by 0.48%. Applying the local-projection elasticities to a change of this size implies a deposit decline of 0.6% on impact and 2.5% after three years, compared with 1.7% and 6.4% in the event study, so the response to the large reforms is between two and a half and three times as large at both horizons.[34]

32. Figure A.18 plots the raw means behind these estimates, indexed to 100 in the year before the reform.
33. Defining the control group with only not-yet-treated states yields similar results (Figure A.19).
34. The corresponding local-projection elasticities with respect to the net-of-tax rate are 1.16 on impact and 5.13 three years after the reform. The first stage is 0.47 percentage points. At the mean state average tax rate of 2%, this corresponds to a 0.48% decline in the net-of-tax rate. The implied deposit declines are therefore $1.16 \times 0.48 = 0.56\%$ on impact and $5.13 \times 0.48 = 2.46\%$ after three years.

Overall, the reform-based evidence reinforces the asymmetry found in the panel analysis: tax increases generate substantial and persistent deposit losses, whereas tax cuts produce no comparable expansion. However, because deposits are an equilibrium quantity, these estimates do not reveal whether the contraction following tax increases originates in local deposit supply or in banks' demand for funding. I turn to this distinction next.

4.3 Local Deposit Supply: Within-Bank Evidence

The deposit decline following a tax increase may reflect a contraction in local deposit supply, but it may also arise because banks face weaker lending opportunities and demand less funding. I distinguish between these explanations by employing a within-bank estimation, that is, by comparing the deposits of the same bank across counties inside and outside the reforming state. The exercise relies on the assumption that banks allocate funds internally, so that deposits raised in one location can finance lending elsewhere. Under this assumption, a bank's demand for deposits is determined at the bank level rather than separately in each local market. Comparing locations of the same bank can therefore hold its funding needs and lending opportunities fixed while isolating changes in local deposit supply.

I implement this test using data from SOD at the bank–county–year level and restricting the analysis to reform cohorts from 1990 onward, so that the three pre-reform years fall within the SOD coverage, which begins in 1987. I estimate bank–county versions of the stacked DiD and event-study specifications introduced in Section 4.1.[35] Under this within-bank specification, Bank×year×cohort fixed effects absorb changes in the bank's overall funding needs, lending opportunities, and balance-sheet growth, whereas Bank×county×cohort fixed effects absorb persistent differences across the local markets served by each bank.

Figure 4 reports the event study. Deposits in treated and control counties follow similar paths before the reform, and following a tax increase, deposits in a bank's reform-state counties fall steadily relative to its deposits in counties elsewhere. Collapsing the event path into the pooled coefficient, the decline is 9.0% over the post-reform window (Table A.8, column (3)). Following tax cuts, the pooled estimate is close to zero and the event-study coefficients

35. The pooled specification is:

$$D_{bc,t_r+h} - D_{bc,t_r-1} = \beta\,(Treat_{cr} \times \mathrm{Post}_{rt}) + \mu_{btr} + \eta_{bcr} + \varepsilon_{bcrt},$$

where D_{bcrt} is the logarithm of deposits held by bank b in county c, year t, and reform cohort r. Standard errors are two-way clustered by bank and county.

provide no evidence of differential pre-trends or a subsequent deposit response.

One remaining concern is that banks may price deposits differently across local markets. Banks exert more market power over deposit rates in concentrated markets (Drechsler et al. 2017), so if a bank's reform-state counties differ in competition from its other counties, the within-bank decline could reflect local pricing rather than a shift in household deposit supply. I address this concern by interacting *Treat* × *Post* with the deposit-market concentration of the county in the year before the reform, measured by the Herfindahl index (HHI) of deposit shares across banks. A pricing explanation predicts larger outflows where the bank faces less competition. Instead, the interaction is small, positive, and statistically insignificant, and the main coefficient barely changes (Table A.8, columns (4) and (8)).

The within-bank comparison shows that local deposit supply contracts, but it does not by itself rule out a parallel decline in the deposit demand of exposed banks. To gauge this demand component, I follow Khwaja and Mian (2008) and estimate the specification on the same sample without the bank×year×cohort fixed effects. Without these fixed effects, the coefficient also absorbs any change in bank-level deposit demand that is correlated with exposure to the reform, so the gap between the two estimates measures that correlation. If exposed banks reduced their demand for deposits after a tax increase, the estimate without bank fixed effects would be more negative. Instead, the decline shrinks from 9.3% to 5.1% (Table A.8, columns (2) and (1)). Following tax cuts, the gap between the two estimates has the same sign and a similar size (columns (6) and (5)), which points to persistent differences between more and less exposed banks rather than a response of bank funding demand to the tax change. The comparison therefore leaves little room for a contraction in bank deposit demand behind the decline in reform-state deposits.

4.4 Robustness

Confounding events. A key concern for the causal interpretation of the state-level estimates in Table A.7 is that large personal income tax reforms may be implemented as part of broader state fiscal packages. This possibility is not ruled out by the low unconditional correlation among annual policy changes shown in Figure A.10, since major reforms may coincide with changes in other taxes. I therefore revisit this concern in Figure A.22. Following Smolyansky (2019), I exclude reforms that occur within one year of a change in the corporate income tax rate. This restriction removes one third of the tax increases, yet the event-study path is

nearly unchanged, and the average post-reform decline in deposits is 4.2%, against 4.5% in the baseline.

Policy endogeneity. A second concern is that states may change taxes in anticipation of changing economic conditions (Giroud and Rauh 2019). If a state raises taxes in anticipation of a downturn, the downturn itself could account for part of the subsequent decline in deposits. The event-study estimates are consistent with parallel pre-reform trends in deposits across reforming and control states. Figure A.20 complements this evidence by comparing both the levels and pre-reform changes of observable economic characteristics. In the year before the reform, states that raise taxes have higher unemployment and collect more income tax revenue per capita than control states. Over that year, I find no statistically significant differences in changes in income per capita, unemployment, deposits relative to income, house prices, or state finances. The only significant difference in pre-reform changes is that population grows more slowly in states that raise taxes. For states that cut taxes, I find no statistically significant differences from their controls in either levels or changes across the characteristics examined.

However, pre-reform trends cannot rule out a downturn that legislators anticipate but that has not yet reached the data. To address this residual concern, I exploit the fact that a state's tax jurisdiction stops at its border while local economic conditions do not. Following Ljungqvist and Smolyansky (2018), I pair each border county of a reforming state with each contiguous county across the state line in states with no large tax change within the $\pm$3-year window, and include pair-by-year fixed effects. These fixed effects absorb any shock common to both sides of the border, including an anticipated regional downturn. In Table A.10, deposits in border counties fall by 3.3% after tax increases relative to their neighbors, and by 5.0% with county fixed effects, against 7.2% at the state level over the same window (Table A.7, column (2)). The estimates for tax cuts are close to zero. The smaller border estimates suggest that local conditions may account for part of the state-level response, but roughly half to two thirds of the decline survives a comparison that holds them fixed.

SUTVA. The stacked design also rests on the assumption that reforms in one state leave deposits in control states unaffected, the so-called stable unit treatment value assumption (Rubin 1980). Tax-induced mobility threatens this assumption. For example, high earners may relocate in response to state tax increases, and if departing households take their deposits to nearby or economically connected states, part of the estimated decline would reflect

relocation rather than a genuine contraction. Such contamination would inflate deposits in connected control states after a tax increase and therefore lead to an upward bias. To gauge this concern, I re-estimate the event study after excluding from the control pool, one set at a time, (i) states sharing a border with the reforming state and (ii) the five largest destinations of the reforming state's outmigration flows.[36] Figure A.21 shows that excluding border states leaves the estimates unchanged, and excluding migration destinations yields a slightly smaller but statistically indistinguishable response to tax increases (-3.7% against -4.5% at the baseline). The stability of the estimates indicates that cross-border relocation of deposits accounts for at most a modest part of the measured decline.

Other. A separate set of checks, displayed in Table A.9, assesses whether the results depend on how large and isolated reforms are defined. Specifically, I use as an alternative top-5% threshold, omit the baseline controls, extend the event window from three to four years, and relax the restriction on other large tax changes occurring within the window. Across these alternative definitions and specifications, the estimated deposit response remains stable. As a final robustness exercise, I replace the baseline stacked specification with the semiparametric difference-in-differences estimator of Abadie (2005), which reweights control states to match each reforming state's pre-reform deposit path and economic conditions. The resulting estimates closely reproduce the baseline pattern (Figure A.23).[37]

I next examine whether this funding shock is transmitted to lending.

5 Deposit Funding and Credit Supply

Having established that tax increases reduce deposits, I proceed to examine whether this funding shock translates into a contraction in bank credit supply. First, I analyze how banks' balance sheet adjustments vary with their exposure to state tax increases to assess whether they offset deposit losses with other sources of funding. Next, I estimate the credit supply response in small business lending by comparing banks with different exposure within the same county and year, thereby controlling for lending opportunities.

36. Border states are defined from the 2010 Census county adjacency file, while migration destinations are the five states with the highest average share of the reforming state's outmigration flows in the ACS state-to-state migration tables for 2005–2018.
37. Appendix B provides details on the procedure.

5.1 Bank Balance Sheets

Banks can respond to a loss of deposit funding by diferent margins. For example, by raising wholesale funding, drawing down liquid assets, or reducing lending. I examine both sides of the balance sheet to assess how banks adjust along these margins. Because banks can reallocate funding across branches, I measure tax exposure at the bank level. Following Drechsler et al. (2017), I use branch-level deposit data to construct $Bank\text{-}Exp_{br}$, the exposure of bank b to reform r. I compute it as a weighted average of state-level exposure to the reform, using the share of the bank's deposits held in each state in the year before the reform as weights. Using this predetermined measure of exposure, I estimate the following stacked specification.

$$y_{bt} - y_{b,t_r-1} = \beta \left(Bank\text{-}Exp_{br} \times \text{Post}_{rt} \right) + \alpha_{br} + \delta_{tr} + \varepsilon_{brt}. \tag{6}$$

y_{bt} is the log of the balance-sheet item of interest for bank b in year t, t_r is the year of reform r, and Post_{rt} indicates the post-reform period. The coefficient β captures how the post-reform change in the outcome varies with bank exposure. The specification includes bank×cohort fixed effects α_{br} and year×cohort fixed effects δ_{tr}. I cluster standard errors by bank. To sharpen the comparison, I restrict the bank-level estimation sample to banks that raise all their deposits in a single state. Each bank therefore has either full or zero exposure to the tax reform. I draw the control group from banks based in states bordering the reforming state so that the two groups are more likely to face similar local economic conditions.

Figure 5 traces the deposits of exposed banks relative to their neighbors around the reform. Before a tax increase, the two groups move in parallel. Deposits at exposed banks start to fall in the year of the reform and are 3.7% lower two years later. Tax cuts leave deposits unchanged throughout the window. Panel A of Table 3 reports the pooled estimates for tax increases. Deposits fall by 2.7%, with declines of 1.9% in savings deposits and 3.2% in time deposits, while the deposit rate does not change. Exposed banks do not replace the lost deposits with wholesale funding, which remains unchanged (column (5)), so total liabilities contract by 2.7%, one for one with deposits.

Panel B shows that the contraction in funding passes through to the asset side. Total assets fall by 2.6%, matching the decline in liabilities. Banks shed securities, which fall by 4.4%, and reduce lending. Total loans fall by 2.6%, driven by real estate loans, which decline by 2.2%, while commercial and industrial loans do not change. Exposed banks therefore shrink rather than offset the funding loss. Tax cuts leave deposits, total liabilities, assets,

and loans unchanged (Table A.11).

5.2 Identifying Credit Supply

The bank-level evidence presented above shows that exposed banks shrink their loan books, but it cannot by itself establish that they cut credit supply. To see why, consider a bank that raises most of its deposits in a state that increases its personal income tax. Suppose this bank also lends to small businesses in a given county. Comparing its lending in that county before and after the reform cannot separate a contraction in credit supply from a change in local lending opportunities. If the county lies in the reform state, the tax increase may reduce local loan demand directly, and even for counties outside the affected state, local economic conditions may change for reasons unrelated to the reform.

The richness of the CRA data allows me to address this identification challenge. In the same county, other banks lend to the same pool of borrowers but raise little or no deposit funding in the reform state. Under the assumption that banks lending in the same county face the same lending opportunities, comparing their lending growth with that of the exposed bank removes the influence of local credit demand. Conceptually, I compare small business lending growth across banks that serve the same county in the same year but differ in their exposure to the funding shock.

There are, however, two reasons why this within-county comparison is still challenging in counties where the exposed bank operates branches. First, a bank's depositors and borrowers belong to the same local economy, so a local shock affects its funding and lending opportunities simultaneously. Second, branches provide banks with information about and relationships with local firms that lenders without a physical presence lack (Agarwal and Hauswald 2010), making the assumption that banks in the same county face the same lending opportunities less plausible. I therefore exclude from the sample bank–county pairs in which the bank operated a branch before the reform. This restriction ensures that the analysis focuses on banks that lend in a given local market but whose funding shocks originate elsewhere. I estimate the following regression:

$$\Delta Y_{bcrt} = \beta \left(\textit{Bank-Exp}_{br} \times \text{Post}_{rt} \right) + \Gamma' X_{br}^{\text{pre}} + \eta_{bcr} + \mu_{ctr} + \varepsilon_{bcrt}, \tag{7}$$

where ΔY_{bcrt} is the growth rate of small business lending by bank b in county c. Post_{rt} is an indicator equal to one after reform r, and $\textit{Bank-Exp}_{br}$ denotes the bank's funding exposure

to the reform, defined above. County×year×cohort fixed effects, μ_{ctr}, absorb local credit demand, as explained above, while bank×county×cohort fixed effects, η_{bcr}, account for persistent characteristics of each bank–county pair (e.g., longstanding ties to local borrowers). To further improve comparability, I include pre-reform bank characteristics, X_{br}^{pre}, interacted with event-time indicators to account for differential lending dynamics associated with these characteristics. These controls include the log of total assets, the equity ratio, return on assets, securities as a share of total assets, the share of non-interest income, real estate and commercial and industrial loans as shares of total assets, the cost of deposits, the share of wholesale funding, and the share of pre-reform small business lending extended outside the bank's branch counties. Standard errors are clustered at the bank level, the level at which treatment exposure varies (Abadie et al. 2023).

5.3 Small Business Lending

Table 4 reports the results, with successive columns adding controls and fixed effects. The estimated coefficient barely changes, from -0.145 in the least saturated specification, to -0.138 in the preferred specification in column (4), even as the R^2 more than doubles. This stability despite the substantial increase in explanatory power suggests limited selection on observables and reduces concerns that omitted factors correlated with the added controls drive the estimated lending response (Altonji et al. 2005; Oster 2019). According to the point estimate obtained in column (4), after a tax increase small business lending growth at a fully exposed bank is 13.8 percentage points lower than at an unexposed bank serving the same county in the same year.[38] Next, to examine the geographic reach of this contraction, column (5) interacts $\textit{Bank-Exp}_{br} \times \text{Post}_{rt}$ with an indicator for counties outside the reforming state. The triple interaction is statistically insignificant ($p = 0.46$), providing no evidence that exposed banks' lending responses differ between counties inside and outside the reforming state. This pattern is consistent with exposed banks transmitting the contraction in credit supply beyond the reforming state. Taken at face value, the point estimates in column (5) imply

38. Scaling this estimate by the deposit response of exposed banks to the same reforms that enter the lending regression (Table A.20) yields a pass-through coefficient of 3.9, so that each log point of reform-induced deposit loss is associated with a 3.9 log-point contraction in small business lending. This coefficient above one follows from comparing a stock with a flow, since deposits fund most of the balance sheet whereas new small business lending is a small share of it. As an example, Arslan et al. (2025) report a similar ratio for unemployment insurance, where an interquartile increase in benefits lowers county deposits by 2.3% and small business lending by banks exposed to those deposits by 8.7%, about 3.8 times the deposit response. Appendix D shows that the pass-through coefficient I obtain is an exact reparameterization of the reduced-form estimate.

that about 26% of the decline in small business lending at exposed banks occurs in counties outside the reforming state.[39]

To allow for the visual exploration of this result, I plot the coefficient from my preferred specification in a year-by-year form.[40] As shown in Figure 6, in the three years preceding a tax increase, the coefficients are close to zero and statistically insignificant, consistent with the conditional parallel trends assumption. Following the increase, small business lending growth declines significantly at exposed banks relative to unexposed banks. The estimate remains negative throughout the three years after the reform.

5.4 Robustness

Alternative explanations. Although the within-market comparison absorbs local credit demand, funding exposure may be correlated with other bank or market characteristics that independently shape lending responses around the reforms. I address this concern by allowing a range of predetermined characteristics to generate their own differential post-reform dynamics. First, banks with more geographically concentrated lending may respond differently from diversified lenders, irrespective of where they raise deposits, so I control for the geographic diversification of each bank's small business lending (Levine, Lin, and Xie 2021). Funding exposure may also capture economic conditions in banks' deposit markets rather than the tax-induced funding shock itself, so I control for deposit-weighted averages of unemployment rates and real GDP growth across the states where each bank collects deposits. I further control for the age and income composition of the bank's depositor base, which may affect funding stability (Doerr et al. 2024), and for the bank's local lending-market share, which may capture borrower relationships or market power (Giannetti and Saidi 2019). Finally, I account for exposure to contemporaneous state corporate-tax changes and for deposit-market concentration, both of which may independently affect bank fund-

39. I multiply the estimated effect for counties inside and outside the reforming state by the pre-reform small business lending of each exposed bank–county pair, weighted by the bank's exposure, and sum over pairs within each group.

40. Specifically, I estimate

$$\Delta Y_{bcrt} = \sum_{\substack{h=-3 \\ h\neq -1}}^{3} \beta_h \left(\textit{Bank-Exp}_{br} \times \mathbf{1}\{t = t_r + h\} \right) + \Gamma' X_{br}^{\text{pre}} + \eta_{bcr} + \mu_{ctr} + \varepsilon_{bcrt},$$

where t_r is the year of reform r, $\mathbf{1}\{t = t_r + h\}$ indicates that year t is h years from the reform, and all other terms are defined as in equation (7). The coefficient for the year before the reform is normalized to zero, so each β_h measures the difference in small business lending growth between a fully exposed and an unexposed bank lending in the same county, h years from the reform relative to the year before.

ing and lending (Smolyansky 2019; Drechsler et al. 2017). Table 5 adds these characteristics, measured in the year before the reform and interacted with the post-reform indicator, to the preferred specification, first one at a time and then jointly. Across all specifications my coefficient of interest remains negative, and when all interactions are included jointly, it is, if anything, larger in magnitude (-0.170) and statistically significant.

Mortgage lending. Mortgages provide a less direct test of the deposit channel because they are more often securitized, but they offer independently reported loan-level data and allow me to assess whether the lending contraction extends beyond small business credit. Table A.15 shows that mortgage origination growth at exposed banks falls by 11.2 percentage points relative to other banks lending in the same county and year.[41] The decline is particularly pronounced for mortgages retained on the bank's balance sheet, the margin most directly tied to deposit funding, with an estimated effect of -0.170.[42] Originations sold in the secondary market also decline, with a similar point estimate of -0.163, although the effect is estimated less precisely. Column (4) shows no statistically significant difference in the mortgage lending response between counties inside and outside the reforming state ($p = 0.34$), thus suggesting that banks transmit the effects of state income tax increases to borrowers in states whose taxes did not change. Finally, and consistent with the pattern in Figure 6, the event study for retained originations in Figure A.26 shows no differential pre-trends and a contraction in credit supply in the year following the tax increase.

Tax cuts. Tax cuts provide a placebo test of the funding mechanism. Since large tax cuts do not generate deposit inflows (Section 4.2), exposed banks should not expand lending relative to other banks serving the same county. Consistent with this prediction, the preferred specification for small business lending yields a coefficient on funding exposure of 0.085 (standard error 0.091), statistically indistinguishable from zero (Table A.14). Similar results hold for mortgage lending (Table A.18).

41. Following Drechsler et al. (2022), I alternatively measure mortgage lending using the annual growth in the number of originations rather than their dollar volume to avoid the influence of changes in house prices. Reassuringly, the result is unchanged (Table A.16).
42. County×year fixed effects absorb local credit demand only if banks lending in the same county face the same change in demand. This would fail if exposed banks lend to borrowers whose demand responds more to the reform, for example middle-income households, whose state ATR is the one that changes. Following Casado and Martinez-Miera (2024), I split the originations of each bank in a county into four groups by borrower income and compare banks lending to borrowers of similar income in the same county and year, using county×year×income fixed effects. The estimates are similar or larger, -0.124 for total, -0.208 for retained and -0.160 for sold originations (Table A.17).

Other. The lending response does not depend on how the outcome is measured or on the baseline sample restrictions. The event study shows the same pattern when the outcome is the log level of loan originations (Figure A.24) and when growth is measured as the log difference of loan originations instead of the symmetric growth rate (Figure A.25). The estimate also remains qualitatively unchanged when I remove the $100,000 minimum lending threshold (Table A.13) or retain bank–county pairs in which the bank operated a branch before the reform (Table A.12).

6 Aggregate Lending and Local Real Effects

It is well established that bank credit supply shocks affect investment and employment, especially among small firms, whose reliance on banks limits their ability to offset a contraction in bank lending with alternative sources of finance (Chodorow-Reich 2014; Amiti and Weinstein 2018; Huber 2018). The previous section shows that tax increases generate deposit outflows that lead exposed banks to reduce small business lending relative to unexposed banks in the same county. This section examines whether other banks offset this credit contraction or whether it translates into lower total lending and weaker local economic activity. To address this question, I proceed in two steps. First, I examine whether total small business lending declines at the county level following tax increases. This allows me to assess whether the contraction is fully offset by other banks, as could occur in a frictionless credit market. Second, I turn to real outcomes and, following Doerr et al. (2026), examine establishment and employment growth. Arguably, a tax increase lowers household income and therefore local demand, so local economic activity may weaken even absent any change in credit supply. I therefore compare firms with different degrees of reliance on bank credit within the same county and year, which absorbs any demand shock that affects these firms similarly in a given county and year.

To study the local economic effects of taxation, the relevant exposure of a county is not its own tax burden, but the funding exposure of the banks that lend there. This is important because banks can transmit funding shocks originating outside the county's own credit market. Therefore, I define county c's exposure to reform r, $\textit{County-Exp}_{cr}$, as the weighted average of banks' reform exposure across all banks lending in the county, using their small business lending shares in the year prior to the reform as weights.[43] I first examine whether

43. Specifically, $\textit{County-Exp}_{cr} = \sum_b \frac{L_{bc}}{L_c} \times \textit{Bank-Exp}_{br}$, where L_{bc} denotes bank b's small business loan origina-

the bank-level lending contraction is also reflected after aggregating across lenders by estimating the following regression:

$$\Delta \ln L_{crt} = \beta_L \left(\textit{County-Exp}_{cr} \times \textit{Post}_{rt} \right) + \Gamma' Z_{cr}^{\text{pre}} + \alpha_{cr} + \delta_{tr} + \varepsilon_{crt}, \tag{8}$$

where L_{crt} is county small business loan originations, and α_{cr} and δ_{tr} denote county×cohort and year×cohort fixed effects. The vector Z_{cr}^{pre} contains the county's pre-reform population and income per capita, together with a set of deposit-weighted bank characteristics (their size, return on assets, equity ratio, deposit ratio, and real estate loan share), each interacted with the event-time indicators. Standard errors are clustered at the county level.

I next examine whether the decline in lending is followed by weaker activity among the firms most reliant on bank finance. I focus on two proxies for bank-dependent firms. First, I compare small firms, defined as those with fewer than twenty employees, with larger firms, as smaller firms are generally more dependent on bank credit. Second, I classify industries as more or less dependent on bank lending.[44] The intuition is that a contraction in bank credit should have a larger impact on firms that depend more on banks. I study the growth in the number of establishments using:

$$\Delta \ln \textit{Est}_{crst} = \beta \textit{County-Exp}_{cr} \times \textit{Post}_{rt} \times \textit{Small}_s + \alpha_{crs} + \mu_{crt} + \nu_{rst} + \varepsilon_{crst}. \tag{9}$$

where s indexes size classes. County×year×cohort fixed effects absorb the level of county exposure together with every contemporaneous county-wide shock, including the direct local effect of the tax reform. The coefficient β is therefore identified by comparing the growth in the number of establishments of small and larger firms in the same county and year. County×size×cohort and size×year×cohort fixed effects further absorb persistent local differences across size groups and time trends in the relative growth of small firms.

Finally, I compare employment growth across industries within the same county and year as follows:

$$\Delta \ln \textit{Emp}_{crit} = \beta_H \textit{County-Exp}_{cr} \times \textit{Post}_{rt} \times \textit{HighDep}_i + \alpha_{cri} + \mu_{crt} + \xi_{rit} + \varepsilon_{crit}, \tag{10}$$

tions in county c and L_c denotes total small business loan originations in the county, both measured in the year before the reform. This weighting follows Drechsler et al. (2017).

44. Following Doerr et al. (2026), I measure an industry's bank dependence with the 2007 Survey of Business Owners, which asks firms about their sources of start-up and expansion capital. Bank dependence is the share of firms in each two-digit industry that report using bank loans to start or expand their business.

where i indexes two-digit industries and $HighDep_i$ distinguishes industries in the top tercile of bank dependence from those in the bottom tercile. The fixed effects compare more and less bank-dependent industries within the same county and year, while absorbing permanent county-industry differences and national industry-specific fluctuations.

Table 6 reports the results. Columns (1) and (2) show that, following a tax increase, annual small business lending growth is 6.7 percentage points lower in a fully exposed county than in an unexposed county. The coefficient is unchanged when the pre-reform controls are introduced. Column (3) shows that, after a tax increase, the number of establishments of small firms grows 1.3 percentage points more slowly than that of larger firms in the same county and year. A remaining concern is that a reallocation of bank lending across states could alter the relative competitiveness of firms within an industry and, with it, patterns of cross-state trade, which matters most in industries that sell in integrated national markets. Following Smolyansky (2019), column (4) excludes agriculture, mining, and manufacturing, and the gap widens slightly to 1.6 percentage points. Columns (5) and (6) re-estimate the specification of column (3) at the county-industry-size-year level, replacing county×size with county×industry×size fixed effects and estimating it separately for industries in the top and bottom terciles of bank dependence. This split shows that the effect is concentrated in bank-dependent industries. In these industries, establishment growth among small firms falls by 1.5 percentage points relative to larger firms and is statistically significant at the 10% level, whereas the corresponding estimate in less dependent industries is small and statistically insignificant. Column (7) shows a similar pattern for employment, with employment in bank-dependent industries growing 2.6 percentage points more slowly than employment in less dependent industries in the same county and year. Excluding tradable industries leaves this estimate essentially unchanged (column (8)). The corresponding estimates for tax cuts are small and statistically indistinguishable from zero (Table A.19). Taken together, the findings in this section support the view that the tax-induced contraction in bank credit weakens local economic activity, especially among small firms and firms in industries that depend more on bank financing.

7 Conclusion

In this paper I have shown how the banking system provides a channel through which personal income taxation propagates to the economy. Using U.S. state tax reforms, I provide

evidence that banks with greater deposit exposure to state tax increases supply less credit to small businesses and mortgage borrowers than less exposed banks. Consistent with taxpayers using their bank balances to absorb part of the decline in disposable income, I find that this contraction in credit supply stems from a reduction in the supply of deposits to banks. The channel operates across state borders through banks' geographic footprints and is asymmetric, arising only in response to tax increases. Further suggestive evidence shows that, following tax increases, counties exposed to banks affected by these reforms experience weaker economic activity.

The main contribution of this paper, therefore, is to propose and test a novel channel through which banks transmit the economic effects of personal income taxation. Previous work has predominantly focused either on the transmission of taxes levied directly on banks or on how banks transmit other fiscal shocks through the asset side of their balance sheets. This paper is the first to show how personal income taxes, although levied on households, are also propagated through banks' balance sheets to the economy.

More broadly, my findings provide important insights into ongoing discussions on tax coordination, banking integration, and regulation. For example, debates over state income taxes on high earners in the United States often center on the risk of taxpayer relocation and its implications for subnational redistribution (Agrawal 2026).[45] By showing how banks' balance sheets transmit income tax shocks across jurisdictions, my results shed light on an overlooked financial source of spillovers that does not require relocation of the underlying tax base.

These spillovers are also relevant to ongoing efforts to deepen banking integration. Although the United States already has an integrated interstate banking system, banking markets in the euro area remain fragmented.[46] European policymakers therefore encourage deeper banking integration to channel savings more effectively into productive investment (European Commission 2025). My findings suggest that the same bank linkages that channel savings across countries can also transmit income tax shocks across them. Tax coordination is itself an EU policy objective, reflected, for example, in efforts to limit tax competition over mobile multinational profits (Council of the European Union 2022). My results suggest that

45. In March 2026, Washington State enacted a tax on income above $1 million (Office of the Governor of Washington 2026). Debate surrounding the reform has focused in part on the potential relocation of high-income taxpayers to other states (Cornfield 2026; Griffin 2026).

46. Intra-euro-area cross-border lending accounts for only around 8% of total bank loans to non-bank borrowers (European Central Bank 2026), while household deposits held with banks in other euro area countries represented just 1.6% of total household deposits in August 2024 (Rumpf 2024).

these two policy objectives are complementary, as deeper banking integration also spreads the effects of national income tax policies across borders.

Regarding banking regulation, existing studies recognize that deposit funding shapes the effects of capital regulation (Begenau 2020; Begenau and Landvoigt 2022), yet their frameworks typically abstract from personal income taxation. My results suggest that models used to evaluate banking regulation should account for personal income tax policy through its effects on bank funding and lending. Ignoring this channel may mischaracterize both banks' responses to regulation and the geographic distribution of the resulting effects on credit.

Finally, this paper leaves several questions for future research. First, my estimates capture relative responses and do not establish whether they translate into aggregate effects on credit. Hence, quantifying the aggregate effects of tax-induced funding shocks would help determine whether and to what extent banks amplify the well-documented output effects of income taxation. Second, the credit spillovers I document raise the possibility of fiscal externalities across jurisdictions. Incorporating bank linkages into a framework of decentralized taxation could help characterize optimal tax policy. Third, the growing use of digital banks may weaken the geographic link between where households pay taxes and where their deposits are held. How this shift in the banking industry affects the transmission channel I document, and in what direction, remains an open question.

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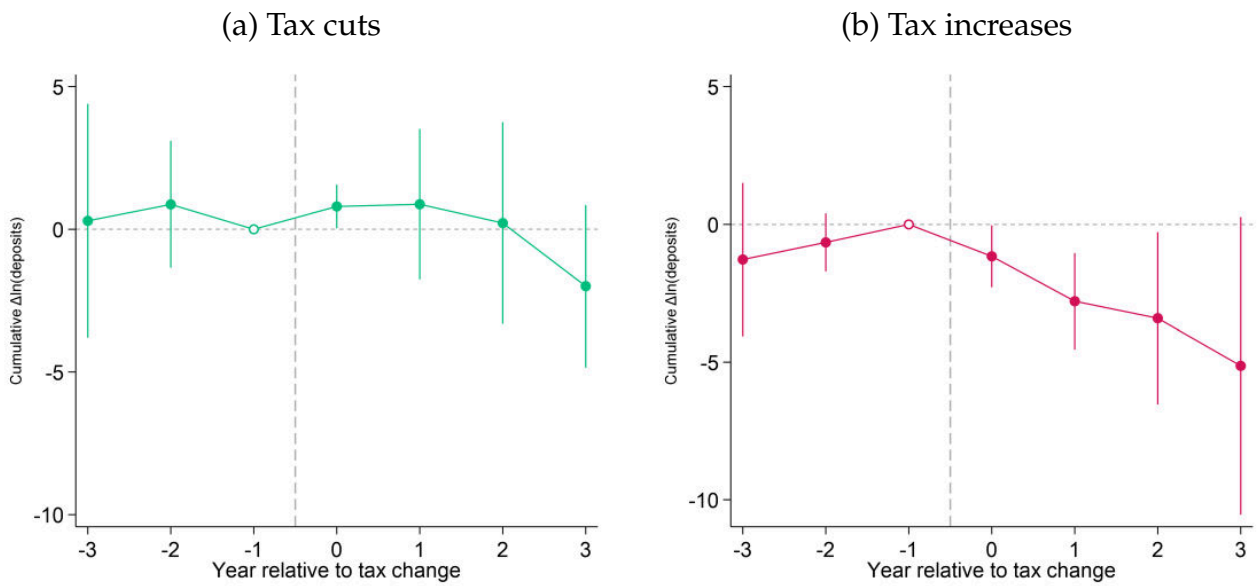
Figures and Tables

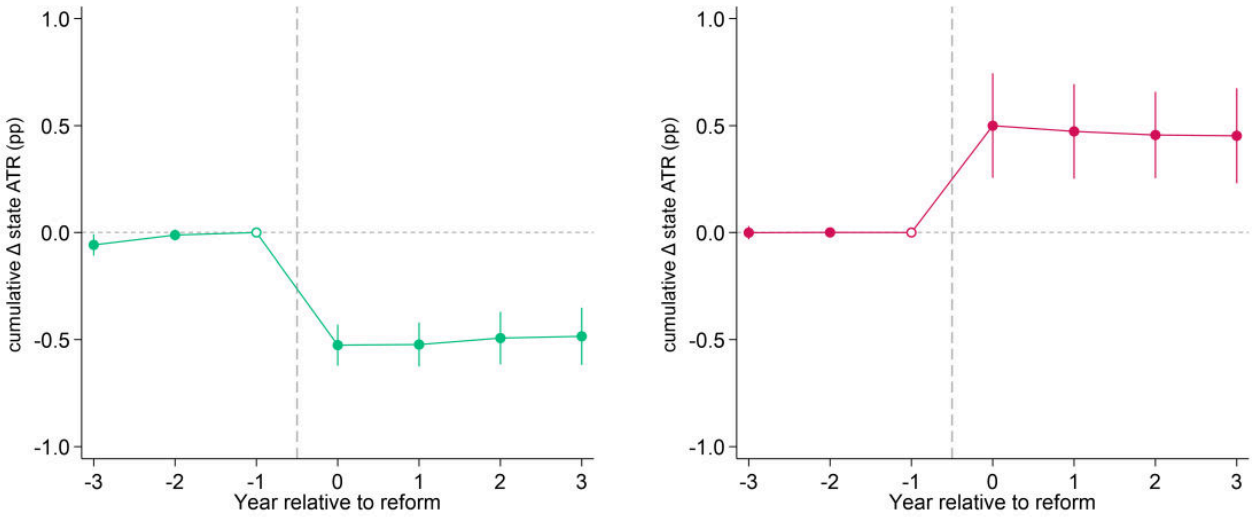
Figure 1. Local projections: dynamic response of deposits to personal income tax changes



Notes. This figure plots coefficient estimates and 90% confidence intervals from equation (3), normalized to zero in the year before the tax change. Panel (a) reports tax cuts and panel (b) tax increases; for tax increases the sign is reversed, so that negative values indicate a decline. The dependent variable is the cumulative change in log deposits from the year before the tax change to each horizon. The regressor is the change in the log state net-of-tax rate at the median, split into its positive and negative parts. Regressions include changes in population, the unemployment rate and income per capita between $t-2$ and $t-1$ and state and year fixed effects, over 1977–2019. Regressions are weighted by state deposits in 1977. Standard errors are clustered at the state level.

Figure 2. First stage: state average tax rate response to the tax reforms

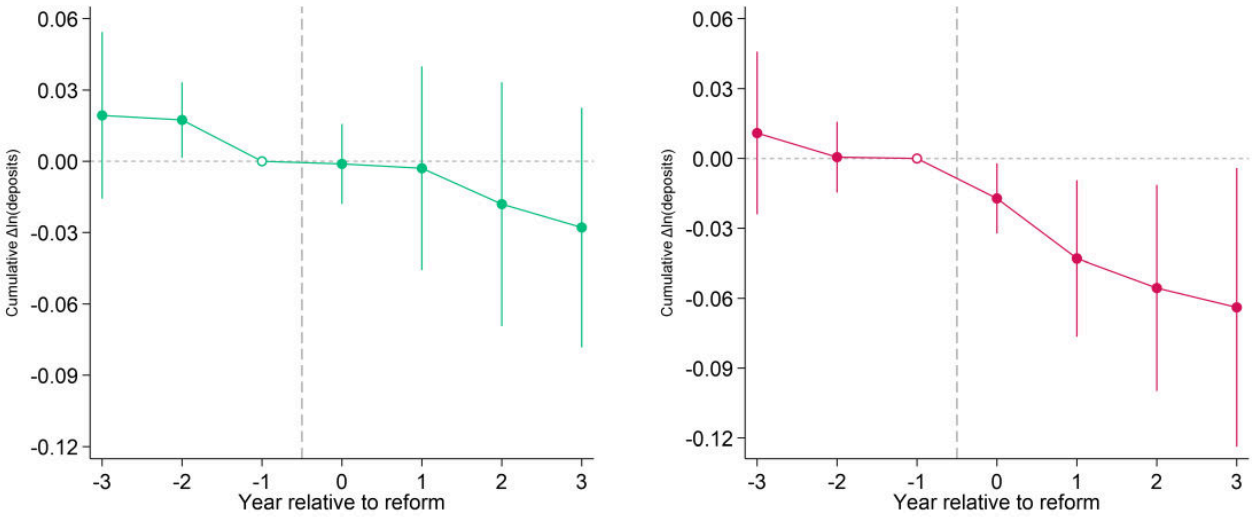
(a) Tax cuts (b) Tax increases



Notes. This figure plots coefficient estimates and 90% confidence intervals from equation (4), normalized to zero in the year before the reform. Panel (a) reports tax cuts and panel (b) tax increases. The dependent variable is the cumulative change in the state average tax rate at the median relative to the year before the reform, in percentage points, and Section 4.1 defines the reforms. Regressions include pre-reform population, unemployment and income per capita interacted with event-time indicators and year fixed effects interacted with reform cohort, over reform cohorts in 1977–2019. Regressions are weighted by state deposits in 1977. Standard errors are clustered at the state level.

Figure 3. State-level deposit response to the tax reforms

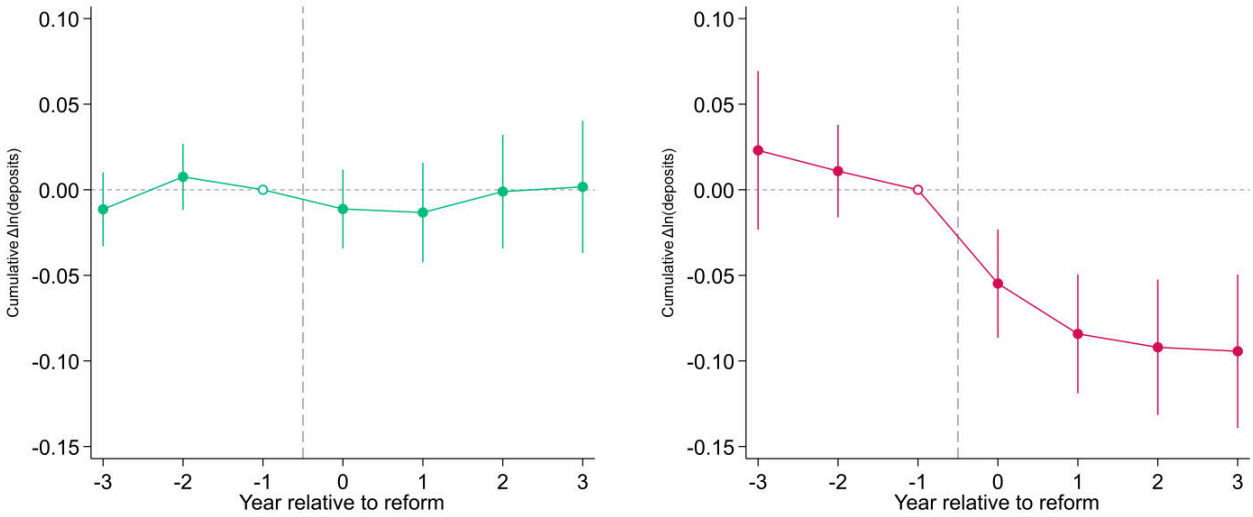
(a) Tax cuts (b) Tax increases



Notes. This figure plots coefficient estimates and 90% confidence intervals from equation (4), normalized to zero in the year before the reform. Panel (a) reports tax cuts and panel (b) tax increases. The dependent variable is the cumulative change in log deposits relative to the year before the reform, and Section 4.1 defines the reforms. Regressions include pre-reform population, unemployment and income per capita interacted with event-time indicators and year fixed effects interacted with reform cohort, over reform cohorts in 1977–2019. Regressions are weighted by state deposits in 1977. Standard errors are clustered at the state level.

Figure 4. Within-bank deposit response to the tax reforms

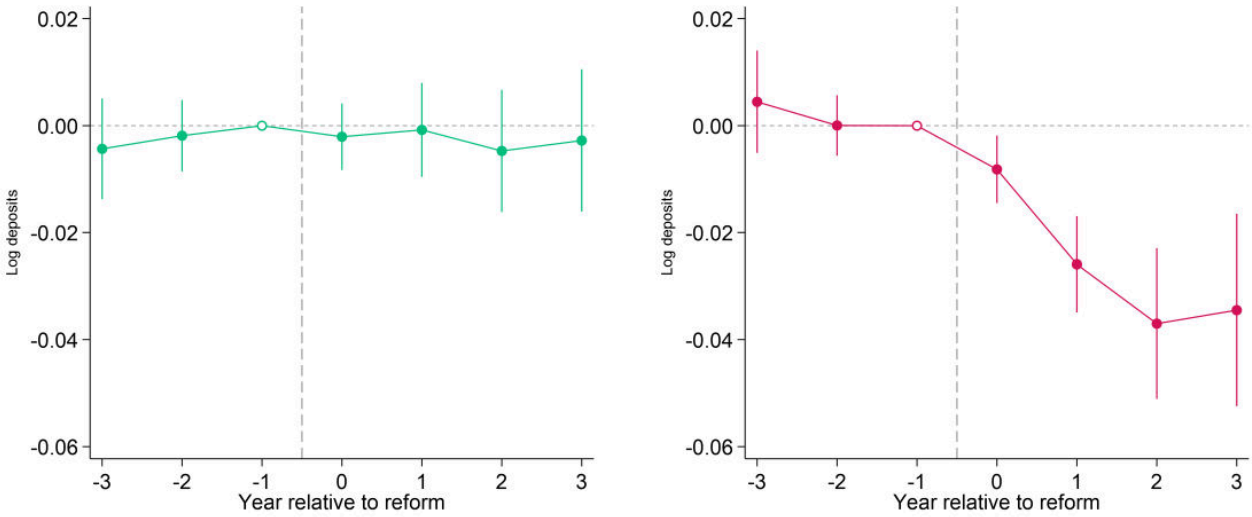
(a) Tax cuts (b) Tax increases



Notes. This figure plots coefficient estimates and 90% confidence intervals from the within-bank event study of Section 4.3, normalized to zero in the year before the reform. Panel (a) reports tax cuts and panel (b) tax increases. The dependent variable is the cumulative change in log deposits of a bank in a county relative to the year before the reform. Treated counties are those of the bank in the reform state, and control counties are those of the same bank in states with no large tax change within the ±3-year window. Regressions include bank×year and bank×county fixed effects interacted with reform cohort, over reform cohorts in 1990–2019. Standard errors are two-way clustered at the bank and county level.

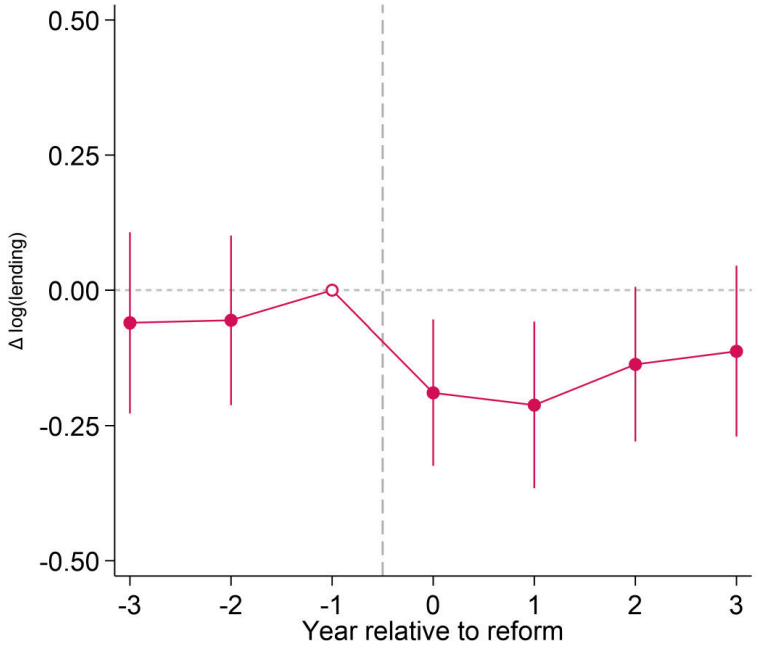
Figure 5. Bank-level deposit response to the tax reforms

(a) Tax cuts (b) Tax increases



Notes. This figure plots coefficient estimates and 90% confidence intervals from the event-study version of equation (6), normalized to zero in the year before the reform. Panel (a) reports tax cuts and panel (b) tax increases. The sample includes banks that hold at least 90% of their deposits in a single state, located in the reform state or in a contiguous state, in the year before the reform. The dependent variable is the cumulative change in log deposits relative to the year before the reform. Regressions include bank and year fixed effects interacted with reform cohort, over reform cohorts in 1990–2019. Standard errors are clustered at the bank level.

Figure 6. Small business lending growth around tax increases



Notes. This figure plots coefficient estimates and 90% confidence intervals from the event-study version of equation (7), as in column (4) of Table 4, normalized to zero in the year before the reform. The dependent variable is the growth rate of small business loan originations of a bank in a county. *Bank-Exp*, interacted with event-time indicators, is the share of the bank's deposits raised in the reform state in the year before the reform. Regressions include ten pre-reform bank characteristics interacted with event-time indicators and bank×county and county×year fixed effects interacted with reform cohort, over tax increase cohorts in 1996–2019. Standard errors are clustered at the bank level.

Table 1. Deposits and personal income tax changes

	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \ln(1-\tau^{p50})_t$	1.622***	1.368***	0.975**	1.029***	0.987***	1.119***
	(0.390)	(0.397)	(0.388)	(0.347)	(0.301)	(0.362)
$\Delta \ln(1-\tau^{corp})$				-0.069	-0.071	-0.048
				(0.225)	(0.223)	(0.234)
$\Delta \ln(1-\tau^{sales})$				-0.484	-0.541	-0.543
				(0.991)	(1.006)	(1.004)
Δ gasoline tax					-0.002	-0.002
					(0.014)	(0.014)
Δ cigarette tax					0.016**	0.016**
					(0.007)	(0.007)
Δ alcohol tax					-0.007	-0.007
					(0.024)	(0.024)
Δ ln MW					0.001	0.001
					(0.016)	(0.016)
Δ ln UI repl. rate					-0.045	-0.046
					(0.067)	(0.068)
$\Delta \ln(1-\tau^{p99})$						-0.253
						(0.329)
Observations	2,100	2,100	2,100	2,100	2,100	2,100
R^2	0.258	0.287	0.312	0.312	0.315	0.315
Year FE	✓	✓	✓	✓	✓	✓
State FE		✓	✓	✓	✓	✓
Demographic controls			✓	✓	✓	✓

Notes. This table reports estimates of equation (1) at the state-year level over 1977–2019. The dependent variable is the annual change in log bank deposits. The main regressor $\Delta \ln(1-\tau^{p50})_t$ is the change in the log net-of-tax rate implied by the state average tax rate at the median of the national income distribution. Demographic controls are the changes in log population, the unemployment rate and log income per capita between $t-2$ and $t-1$. Table A.1 defines all variables. Regressions are weighted by state deposits in 1977. Fixed effects are denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the state level. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.10$.

Table 2. Deposits and personal income tax changes across counties

	(1)	(2)	(3)
$\Delta \ln(1-\tau^{state})$	0.830^{***}	0.894^{***}	0.861^{***}
	(0.221)	(0.242)	(0.210)
Observations	90,007	90,007	89,833
R^2	0.188	0.191	0.226
County FE	✓	✓	✓
State × Year FE	✓	✓	✓
Income × Year FE		✓	
Income × State × Year FE			✓

Notes. This table reports estimates of the county-level analog of equation (1) at the county-year level over 1991–2019. The dependent variable is the annual change in log county deposits. The main regressor $\Delta \ln(1-\tau^{state})$ is the change in the log state net-of-tax rate evaluated at the county's average income per return two years earlier. Table A.1 defines all variables. Fixed effects are denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 3. Bank balance sheet response around tax reforms: tax increases

	Total deposits (1)	Deposit rate (2)	Savings deposits (3)	Time deposits (4)	Wholesale funding (5)	Total liabilities (6)
Panel A: Liabilities						
Bank-Exp × Post	-0.027***	0.007	-0.019**	-0.032***	0.007	-0.027***
	(0.006)	(0.024)	(0.008)	(0.008)	(0.022)	(0.006)
Bank FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	75,687	75,687	68,842	69,095	75,654	75,709
R^2	0.591	0.188	0.609	0.589	0.509	0.575
	Total assets (1)	Cash (2)	Securities (3)	Total loans (4)	Real estate loans (5)	C&I loans (6)
Panel B: Assets						
Bank-Exp × Post	-0.026***	-0.017	-0.044***	-0.026***	-0.022***	-0.008
	(0.006)	(0.012)	(0.013)	(0.008)	(0.008)	(0.014)
Bank FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	75,710	75,671	74,314	75,473	75,136	70,698
R^2	0.605	0.551	0.463	0.577	0.591	0.494

Notes. This table reports estimates of equation (6) for tax increases at the bank-year level over 1990–2019. The sample includes banks that hold at least 90% of their deposits in a single state, located in the reform state or in a state contiguous to it, in the year before the reform. The dependent variable is the cumulative change in the log of the indicated balance-sheet item relative to the year before the reform, and in column (2) of Panel A the change in the deposit rate. *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

Table 4. Small business lending around tax reforms: tax increases

	(1)	(2)	(3)	(4)	(5)
Bank-Exp × Post	-0.145***	-0.154***	-0.134***	-0.138**	-0.165***
	(0.055)	(0.058)	(0.050)	(0.060)	(0.057)
Bank-Exp × Post × Outside					0.058
					(0.078)
Observations	519,092	519,091	519,091	448,589	448,589
R^2	0.204	0.285	0.306	0.452	0.452
p-value, inside = outside					0.46
Bank FE	✓	✓	✓		
Bank × county FE				✓	✓
County FE	✓				
Year FE	✓				
County × year FE		✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓

Notes. This table reports estimates of equation (7) for tax increases at the bank-county-year level over 1996–2019. The dependent variable is the growth rate of small business loan originations of a bank in a county, defined as the change divided by the average of its levels in adjacent years. *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are ten pre-reform bank characteristics, interacted with the post indicator in columns (1)–(2) and with event-time indicators in columns (3)–(5). Column (5) interacts *Bank-Exp* × Post with *Outside*, an indicator for counties outside the reform state, and the *p*-value tests that the two responses are equal. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 5. Alternative explanations: small business lending around tax reforms

	(1) Base	(2) Divers.	(3) Dep. power	(4) Macro	(5) Demogr.	(6) Mkt. share	(7) Bank tax	(8) All
Bank-Exp × Post	-0.138**	-0.140**	-0.151**	-0.193***	-0.092	-0.140**	-0.137**	-0.170**
	(0.060)	(0.060)	(0.062)	(0.071)	(0.063)	(0.058)	(0.059)	(0.069)
Diversification × Post		-0.092						0.072
		(0.087)						(0.082)
Deposit market power × Post			-0.641**					-0.519*
			(0.284)					(0.269)
Home-state unemployment × Post				0.043				0.031
				(0.030)				(0.028)
Home-state GDP growth × Post				-0.004				-0.013
				(0.021)				(0.019)
Depositor share aged 65+ × Post					-2.829***			-2.252***
					(1.003)			(0.801)
Local market share × Post						-3.992***		-3.988***
						(0.423)		(0.400)
Bank corporate-tax exposure × Post							-0.017	0.006
							(0.028)	(0.027)
Observations	448,589	430,881	448,589	433,059	448,581	448,589	448,589	416,853
R^2	0.452	0.449	0.453	0.456	0.452	0.455	0.452	0.459
Bank × county FE	✓	✓	✓	✓	✓	✓	✓	✓
County × year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes. This table reports estimates of equation (7) for tax increases at the bank-county-year level over 1996–2019. The dependent variable is the growth rate of small business loan originations of a bank in a county, defined as the change divided by the average of its levels in adjacent years. *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are ten pre-reform bank characteristics interacted with event-time indicators. Columns (2)–(8) add to the specification of column (4) of Table 4 the indicated bank and market characteristics, measured in the year before the reform and interacted with Post, first one at a time and then jointly. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 6. Local economic activity around tax reforms: tax increases

	Small business lending		Establishments				Employment	
			All	Excl. tradable	High Dep	Low Dep		Excl. tradable
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
County-Exp × Post	-0.067***	-0.067***						
	(0.024)	(0.024)						
County-Exp × Post × Small			-0.013***	-0.016***	-0.015*	-0.008		
			(0.004)	(0.005)	(0.008)	(0.011)		
County-Exp × Post × High Dep							-0.026**	-0.025*
							(0.011)	(0.014)
Observations	70,224	68,849	147,482	146,854	644,144	556,445	764,031	599,328
R^2	0.201	0.206	0.517	0.513	0.183	0.193	0.159	0.180
County FE	✓	✓						
County × size FE			✓	✓				
County × industry FE							✓	✓
County × industry × size FE					✓	✓		
Year FE	✓	✓						
County × year FE			✓	✓	✓	✓	✓	✓
Size × year FE			✓	✓	✓	✓		
Industry × year FE							✓	✓
Controls		✓						

Notes. This table reports estimates of equations (8), (9) and (10) for tax increases at the county level over 1998–2011. The dependent variable is the growth rate of county small business loan originations in columns (1)–(2), of the number of establishments in columns (3)–(6), and of employment in columns (7) and (8). *County-Exp* is the exposure of the banks lending in the county to the reform state, weighted by their pre-reform small business lending shares. Controls in column (2) are the county's pre-reform population and income per capita and the deposit-weighted pre-reform characteristics of its banks, interacted with event-time indicators. *High Dep* and *Low Dep* denote the top and bottom terciles of industries by bank dependence, and columns (4) and (8) re-estimate columns (3) and (7) excluding tradable industries. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

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A Construction of the Tax-Rate Measure

This appendix describes the construction of the state-year personal income tax rates used in the paper. Following the approach in Moretti and Wilson (2017) and Akcigit et al. (2022), I evaluate the federal and state tax codes for taxpayers located at fixed positions of the national income distribution.

For each year from 1977 to 2019, I obtain the income thresholds at the 50th, 90th, and 99th percentiles of the U.S. national income distribution from the World Inequality Database (WID). The underlying income concept is pretax national income among equal-split adults. I convert the thresholds into nominal dollars using the corresponding WID price index. The same national income threshold is used in every state in a given year. Consequently, cross-state differences in the resulting tax rates reflect differences in the tax treatment of the same gross income rather than differences in state income distributions.

I assign gross income between primary wage income and realized long-term capital gains. At the median, I treat gross income as wage income. At the 90th and 99th percentiles, I use the WID long-term capital-gains share for the narrow income groups containing the corresponding threshold: the 90th–95th percentile group for p90 and the 99th–99.5th percentile group for p99. Because the capital-gains series ends in 2015, I hold its 2015 share fixed for 2016–2019. The remaining income is assigned to primary wages.

I pass these income inputs through NBER TAXSIM (Feenberg and Coutts 1993). The representative taxpayer is assumed to be a married couple filing jointly, with both spouses aged 40, no dependents, no childcare expenses, no secondary earnings, and no income other than primary wages and long-term capital gains. Itemized deductions are set to zero apart from state income tax payments calculated by TAXSIM. These assumptions follow the approach of Moretti and Wilson (2017). TAXSIM applies the federal and state tax codes in effect in each state and year, including brackets, exemptions, deductions, credits, the alternative minimum tax, and the interactions between federal and state liabilities.

For percentile p, state s, and year t, let Y_t^p denote gross income, and let $T_{st}^{\text{fed}}(Y_t^p)$ and $T_{st}^{\text{state}}(Y_t^p)$ denote the federal and state liabilities returned by TAXSIM. I define the combined average tax rate as

$$\tau_{st}^p = \frac{T_{st}^{\text{fed}}(Y_t^p) + T_{st}^{\text{state}}(Y_t^p)}{Y_t^p}. \tag{11}$$

The state and federal components are

$$\tau_{st}^{p,\text{state}} = \frac{T_{st}^{\text{state}}(Y_t^p)}{Y_t^p}, \qquad \tau_{st}^{p,\text{fed}} = \frac{T_{st}^{\text{fed}}(Y_t^p)}{Y_t^p}.$$

Although the federal rate schedule is common nationwide, the federal liability returned by TAXSIM can differ across states because state income tax payments may affect federal taxable income. Conversely, federal tax payments may affect state taxable income in states that allow their deduction. The combined rate therefore captures both layers of the tax system and their interaction. The analysis uses the state component, $\tau_{st}^{p,\text{state}}$, which the main text denotes τ_{st} when evaluated at the median. The combined rate enters only as a robustness check.

B Semiparametric Difference-in-Differences

This appendix describes the semiparametric difference-in-differences estimator of Abadie (2005), applied to state taxation by Akcigit et al. (2022) and adapted here to the deposit setting. The exercise revisits the large reforms of the stacked design and asks whether deposits in the reform state evolve differently from a synthetic comparison state constructed from states without contemporaneous large tax changes.

For each reform r, I construct a synthetic comparison state as a weighted average of eligible donor states. The donor pool consists of states that do not experience a large reform of either sign within the event window. The weights minimize the pre-reform prediction error over the four years before the reform, using deposits together with population, the unemployment rate, and income per capita. I then pool the treated states and their synthetic controls across reforms and estimate the following regression:

$$\ln D_{srh} = \alpha_r + \theta_r \, \mathit{Treat}_{sr} + \lambda_h + \sum_{\substack{\ell=-4 \\ \ell \neq -1}}^{4} \gamma_\ell \, \mathit{Treat}_{sr} \, \mathbf{1}\{h = \ell\} + \varepsilon_{srh}, \tag{12}$$

where r indexes reforms, s indexes either the treated state or its synthetic comparison state, and h denotes years relative to the reform. The indicator Treat_{sr} equals one for the treated state and zero for its synthetic comparison. The reform fixed effects α_r absorb average differences across reform episodes, while the reform$\times$treatment fixed effects $\theta_r \, \mathit{Treat}_{sr}$ allow the treated state and its synthetic comparison to differ in levels within each reform. Event-time

fixed effects λ_h absorb average dynamics common to treated and synthetic states at each relative year. The interaction terms $Treat_{sr}\,\mathbf{1}\{h = \ell\}$ trace the treated-synthetic gap at each event time. The year immediately before the reform, $h = -1$, is omitted, so each γ_ℓ measures the treated-state deposit gap relative to its synthetic comparison state in year ℓ, normalized to the pre-reform year. I estimate the equation separately for tax increases and tax cuts. Standard errors are clustered at the reform level.

C Country-Level Evidence

This appendix describes the country-level exercise used to assess whether the deposit response to tax changes extends beyond the U.S. state setting. I combine three data sources. Deposit data come from the historical panel of Jamilov et al. (2025), which reports total bank deposits at the country-year level. I deflate nominal deposits in local currency using the consumer price index from the Global Macro Database of Müller et al. (2025), which also provides the macroeconomic controls used in the local projections. Personal income tax rates come from the OECD *Taxing Wages* tables. I use the average personal income tax rate on gross wage earnings, which excludes social security contributions, for the OECD standard two-earner married couple with no children, in which the principal earner receives 100% of the average wage and the spouse 67%. This is the closest available counterpart to the filing unit behind the state tax rates, a married couple filing jointly with no dependents, since the OECD does not tabulate a one-earner couple without children. I define the net-of-tax rate as $n_{ct} = \log(1 - \tau_{ct})$, where τ_{ct} denotes this average rate in country c and year t. Since the rate is computed from each country's statutory tax schedule at a fixed point of the wage distribution, changes in n_{ct} reflect changes in tax law rather than shifts in income composition, following the same logic as the U.S. state-level measure. The sample comprises 21 advanced OECD economies (Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Korea, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States). I exclude Iceland and Luxembourg, whose banking systems hold large deposits from nonresidents, so that aggregate deposits there are only loosely tied to domestic household income. The *Taxing Wages* tables begin in 2000, so the estimation sample covers 2000–2019. For each horizon

h, I estimate the joint local projection

$$\ln D_{c,t+h} - \ln D_{c,t-1} = \alpha_c^h + \psi_t^h + \beta_+^h \Delta n_{ct}^+ + \beta_-^h \Delta n_{ct}^- + \Gamma_h' X_{c,t-1} + \varepsilon_{ct}^h, \tag{13}$$

where the dependent variable is the cumulative change in the logarithm of deposits from the year before the tax change to horizon h. The vector $X_{c,t-1}$ contains one-year lagged population, GDP per capita, and the unemployment rate, the counterpart of the demographic controls in the state-level benchmark. Each regression includes country and year fixed effects, and standard errors are clustered at the country level.

D The Pass-Through Coefficient as a Reparameterization of the Reduced Form

Section 5 estimates the response of small business lending to a bank's exposure to the deposit shock generated by a tax reform. An alternative way to express the magnitude of this effect is to scale the lending response by the corresponding effect of the reform on bank deposits. This yields a pass-through coefficient measuring the change in lending associated with a one-percent reform-induced change in deposits. This appendix shows that, in this setting, this coefficient is an exact reparameterization of the reduced-form lending estimate rather than an additional source of identifying variation.

Let $Z_i = \textit{Bank-Exp}_{br} \times \text{Post}_{rt}$ denote the treatment variable in the lending regression. Suppressing subscripts for convenience, consider the reduced-form equation $y_i = \beta_{RF} Z_i + \varepsilon_i$. The OLS estimate is $\hat{\beta}_{RF} = \frac{\sum_i Z_i y_i}{\sum_i Z_i^2}$. Then, separately, using the bank-level deposit panel, I estimate $x_i = \beta_{FS} Z_i + u_i$ and obtain the first-stage coefficient $\hat{\beta}_{FS}$. The predicted reform-induced change in deposits assigned to observation i in the lending sample is therefore $\widehat{x}_i = \hat{\beta}_{FS} Z_i$. Consider now the pass-through regression $y_i = \theta \widehat{x}_i + \eta_i$. Its OLS coefficient is $\hat{\theta} = \frac{\sum_i \widehat{x}_i y_i}{\sum_i \widehat{x}_i^2}$. Substituting $\widehat{x}_i = \hat{\beta}_{FS} Z_i$ yields the following expression:

$$\hat{\theta} = \frac{\sum_i (\hat{\beta}_{FS} Z_i) y_i}{\sum_i (\hat{\beta}_{FS} Z_i)^2} = \frac{1}{\hat{\beta}_{FS}} \frac{\sum_i Z_i y_i}{\sum_i Z_i^2} = \frac{\hat{\beta}_{RF}}{\hat{\beta}_{FS}}.$$

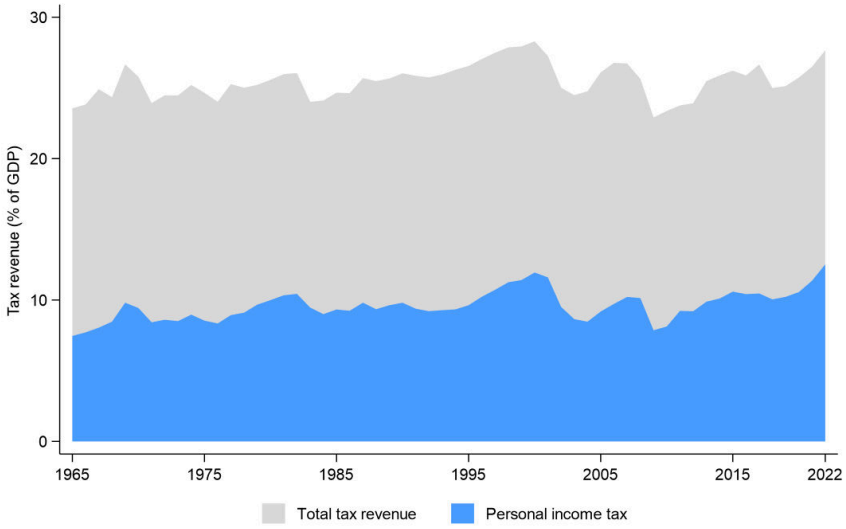
As shown above, the pass-through coefficient is mechanically equal to the ratio of the reduced-form and first-stage estimates. Specifically, $\hat{\theta} \widehat{x}_i = \frac{\hat{\beta}_{RF}}{\hat{\beta}_{FS}} \hat{\beta}_{FS} Z_i = \hat{\beta}_{RF} Z_i$, so the pass-

through regression generates exactly the same fitted values as the reduced-form regression. It simply expresses the same estimated lending response in units of reform-induced deposit changes rather than in units of bank exposure. It follows from the fact that, within the lending sample, predicted deposits are a constant multiple of the original treatment variable. What matters is that the first stage supplies a single coefficient $\hat{\beta}_{FS}$ that scales Z_i uniformly across lending observations. Table A.20 reports the three objects. The first stage, estimated on the same reforms as the lending regression, implies that a fully exposed bank loses 3.6% of its deposits after a tax increase; the reduced form is the lending estimate of Table 4; and the pass-through coefficient is their ratio, 3.9, with a two-sample delta-method standard error that incorporates the first-stage estimation uncertainty.

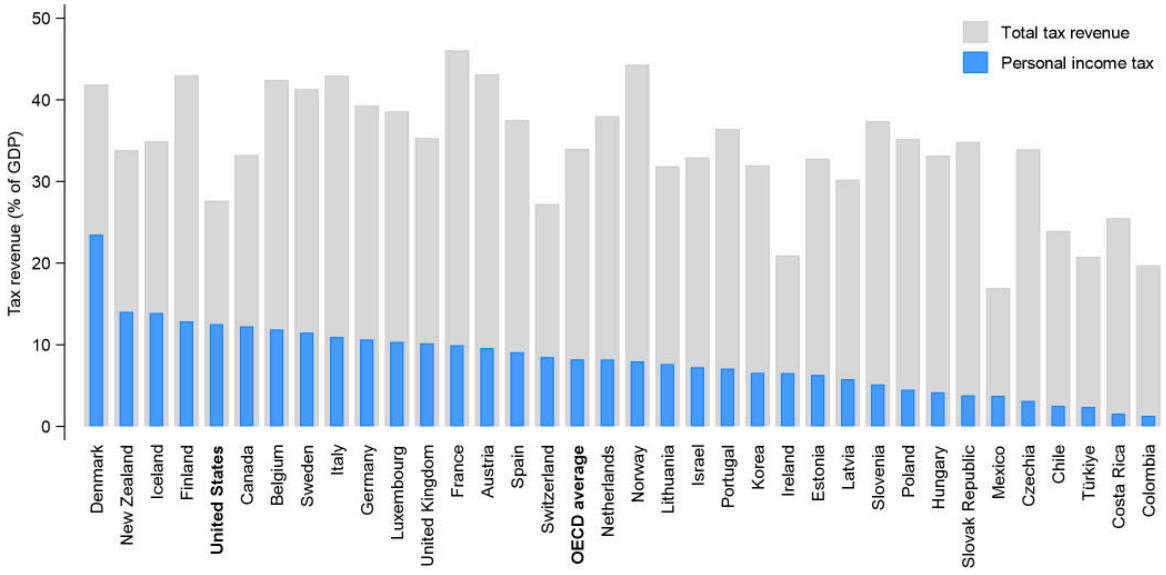
E Additional Figures

Figure A.1. The importance of personal income taxation

(a) Tax revenue as a share of U.S. GDP, 1965–2022

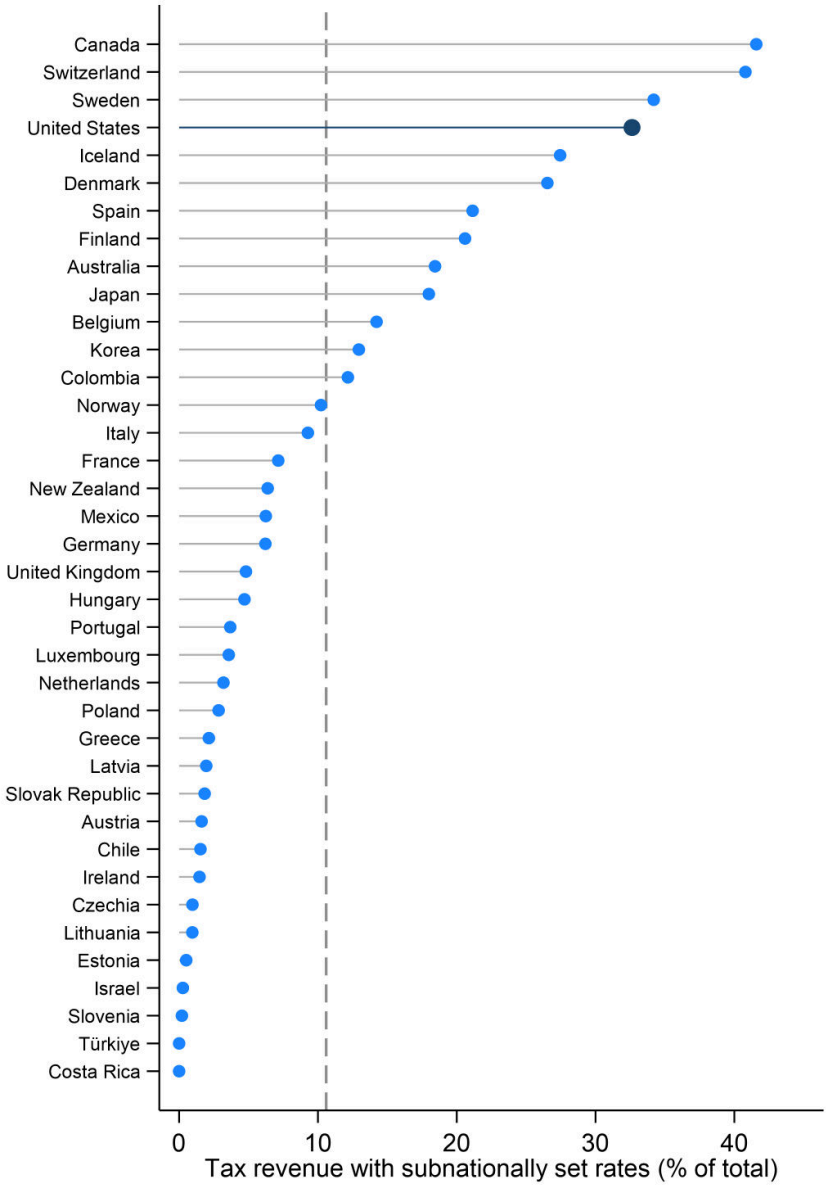


(b) Tax revenue as a share of GDP, OECD countries, 2022



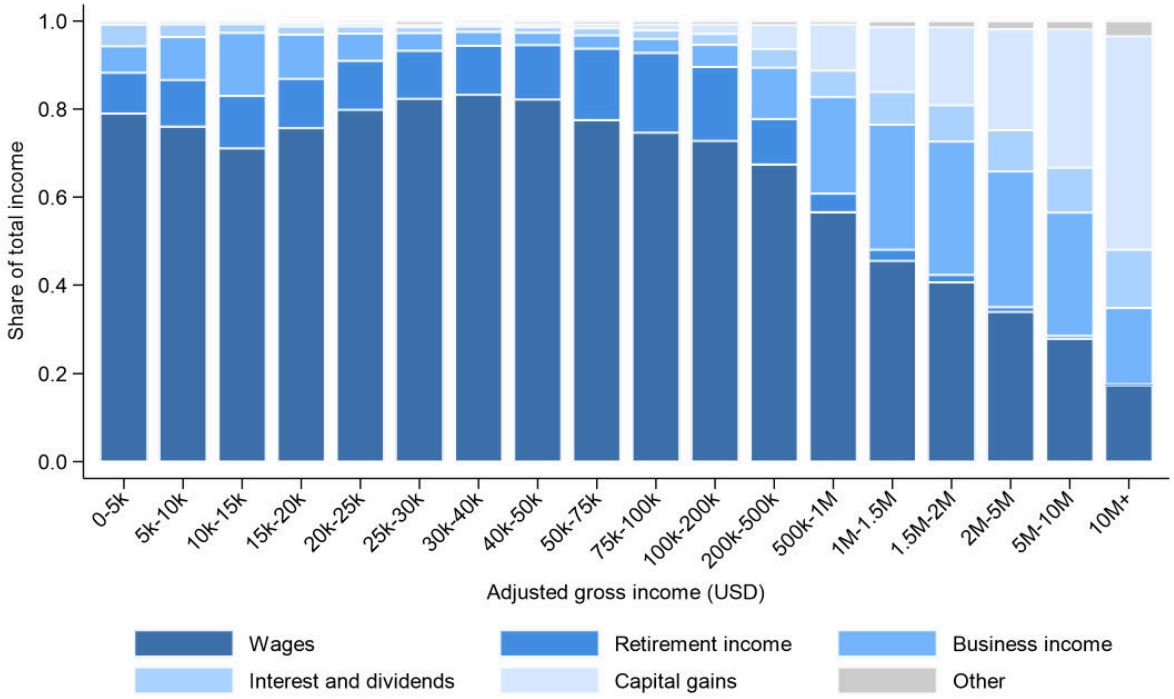
Notes. This figure plots tax revenue as a share of GDP. Panel (a) plots total and personal income tax revenue in the United States over 1965–2022, for all levels of government, and panel (b) plots total and personal income tax revenue across OECD countries in 2022. Source is OECD Revenue Statistics.

Figure A.2. Subnational tax-setting power across OECD countries



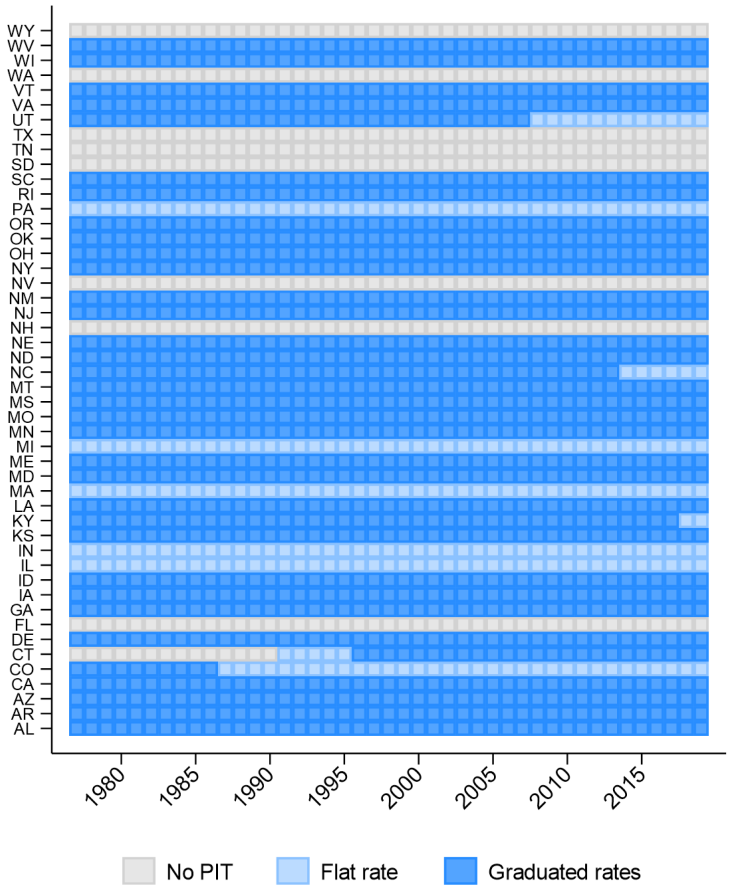
Notes. This figure plots, for each OECD country in 2022, the share of total tax revenue raised by taxes whose rate subnational governments set. The dashed line marks the OECD mean. Source is the OECD Fiscal Decentralisation Database.

Figure A.3. Composition of income by adjusted gross income, 2019



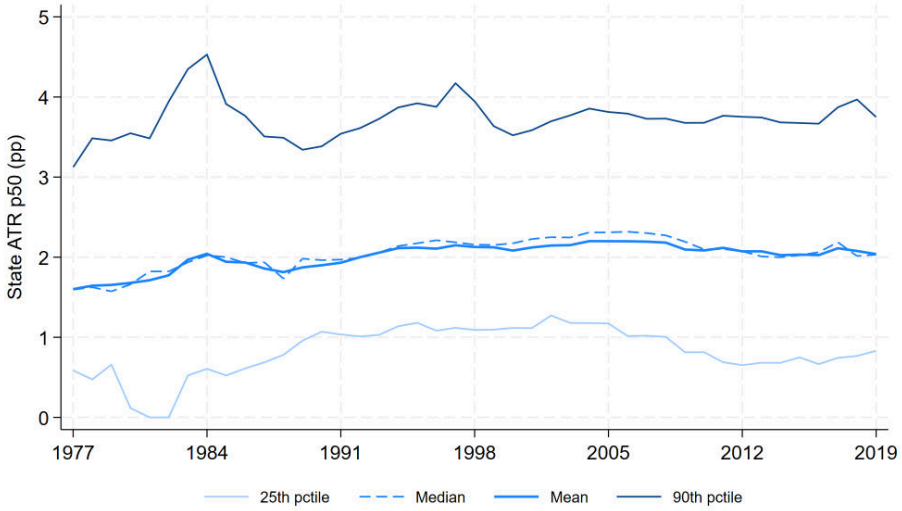
Notes. This figure plots the share of total income by source for each bracket of adjusted gross income in 2019. Source is IRS Statistics of Income.

Figure A.4. Structure of state personal income taxes



Notes. This figure classifies each state-year over 1977–2019 as having no broad-based tax on wages, a flat rate or graduated rates. Statutory rates are from Robinson and Tazhidinova (2025). Alaska and Hawaii are excluded.

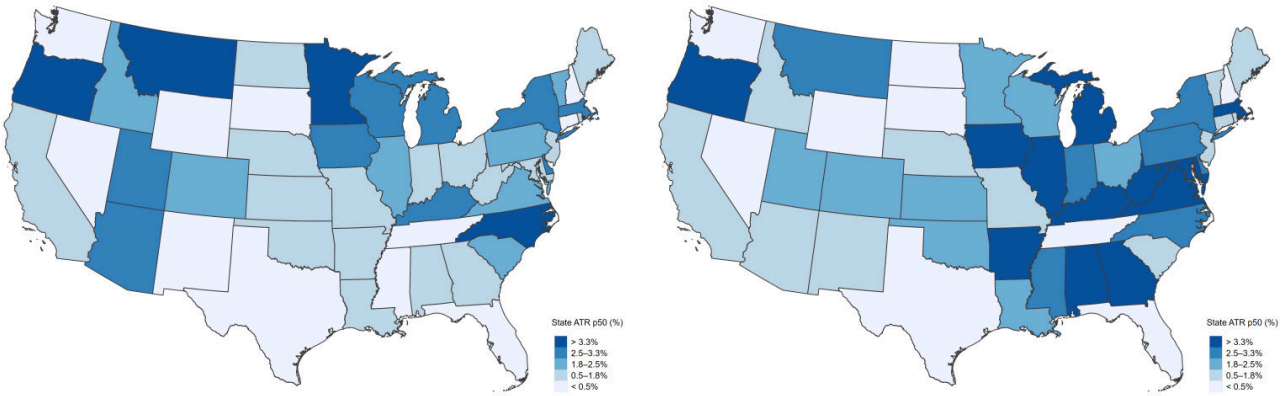
Figure A.5. Distribution of the state average tax rate at the median, 1977–2019



Notes. This figure plots the 25th percentile, median, mean and 90th percentile across the fifty states of the state average tax rate at the median of the national income distribution over 1977–2019. Rates are from NBER TAXSIM.

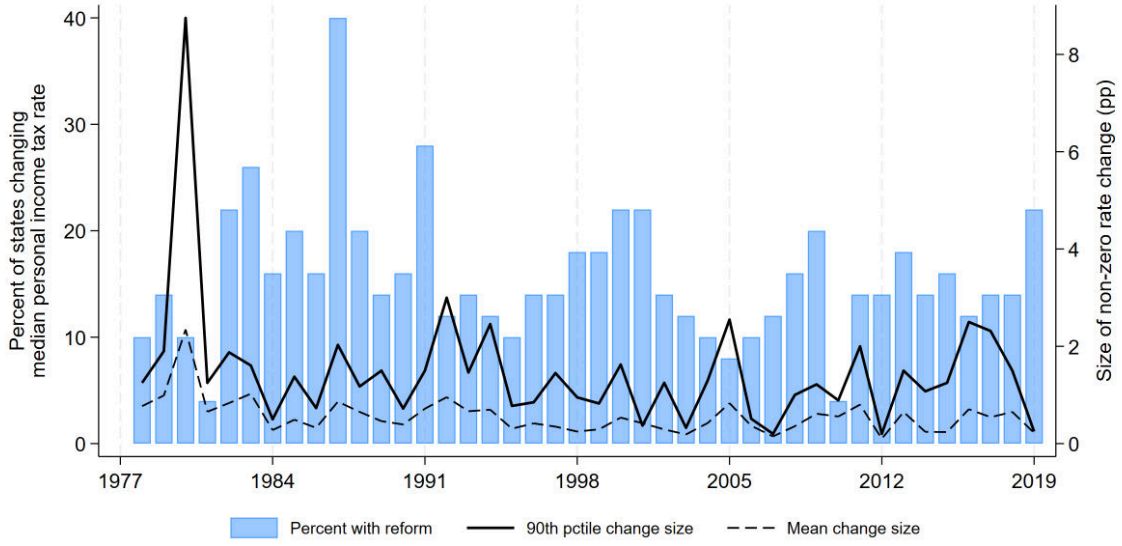
Figure A.6. State average tax rate at the median, 1977 and 2019

(a) 1977 (b) 2019

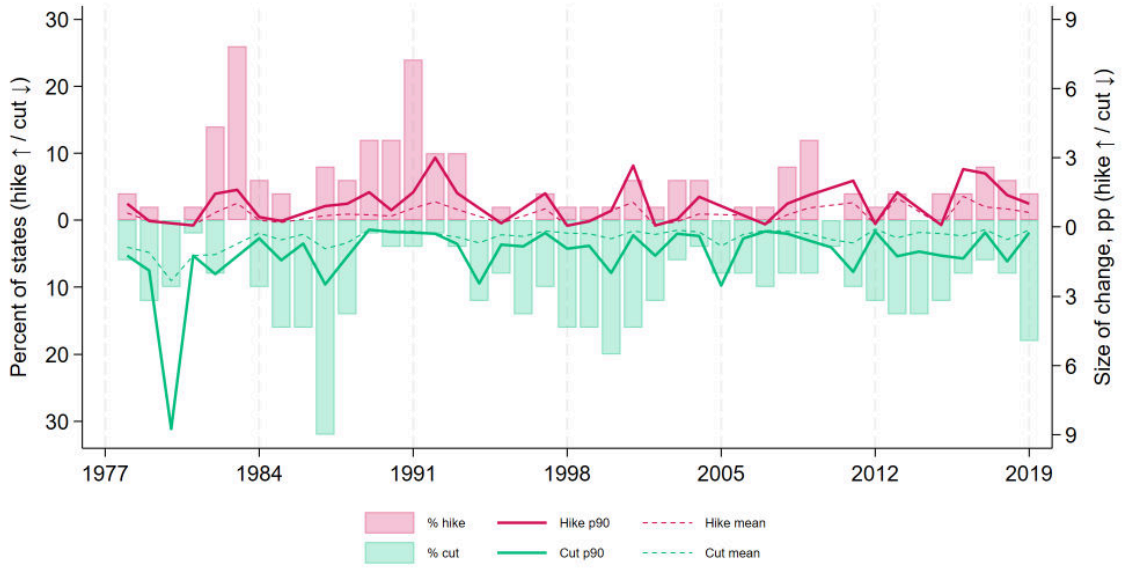


Notes. This figure maps the state average tax rate at the median of the national income distribution. Panel (a) shows 1977 and panel (b) shows 2019. Rates are from NBER TAXSIM.

Figure A.7. Frequency and size of state personal income tax changes, 1977–2019



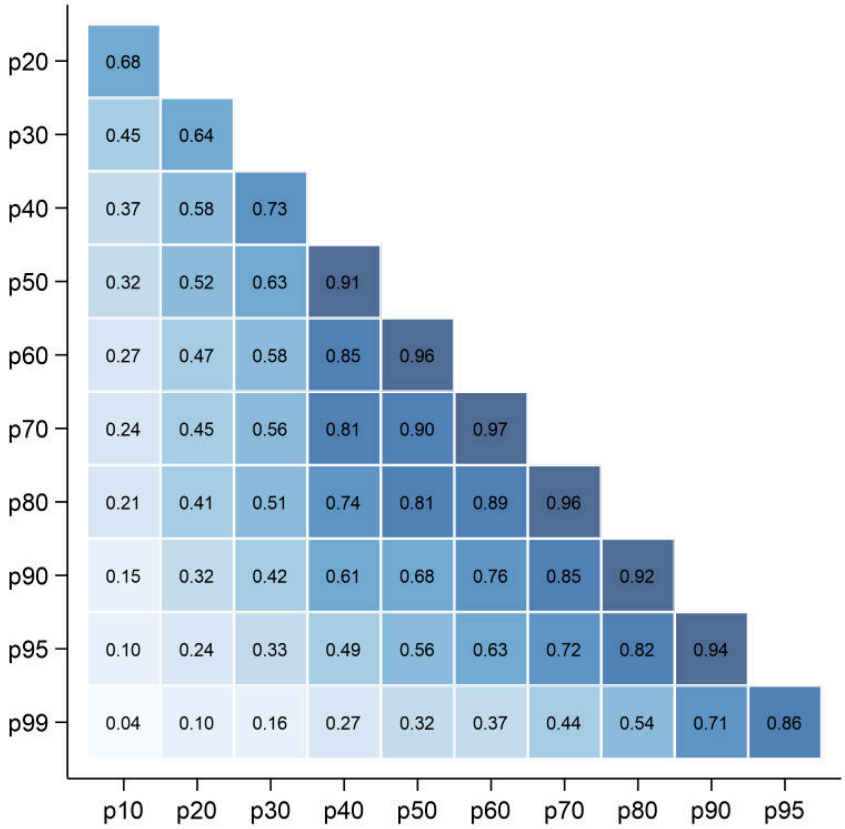
(a) All changes



(b) Tax cuts and tax increases

Notes. This figure plots year-over-year changes in the statutory marginal state income tax rate of the median taxpayer for the fifty states over 1977–2019, from Baker et al. (2025). Panel (a) plots the share of states that change the rate and the mean and 90th percentile of the absolute change among changing states, and panel (b) splits the changes into tax cuts and tax increases.

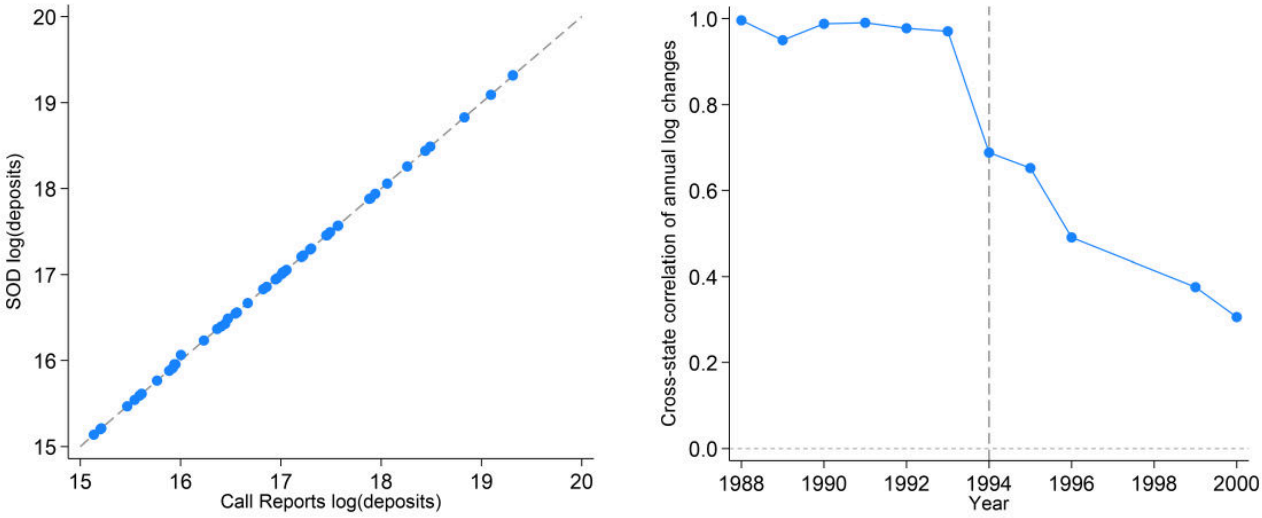
Figure A.8. Correlation of state tax changes across income percentiles



Notes. This figure plots the correlation of annual changes in the state average tax rate between pairs of percentiles of the national income distribution, pooled across states over 1977–2019. Appendix A describes the construction of the rates.

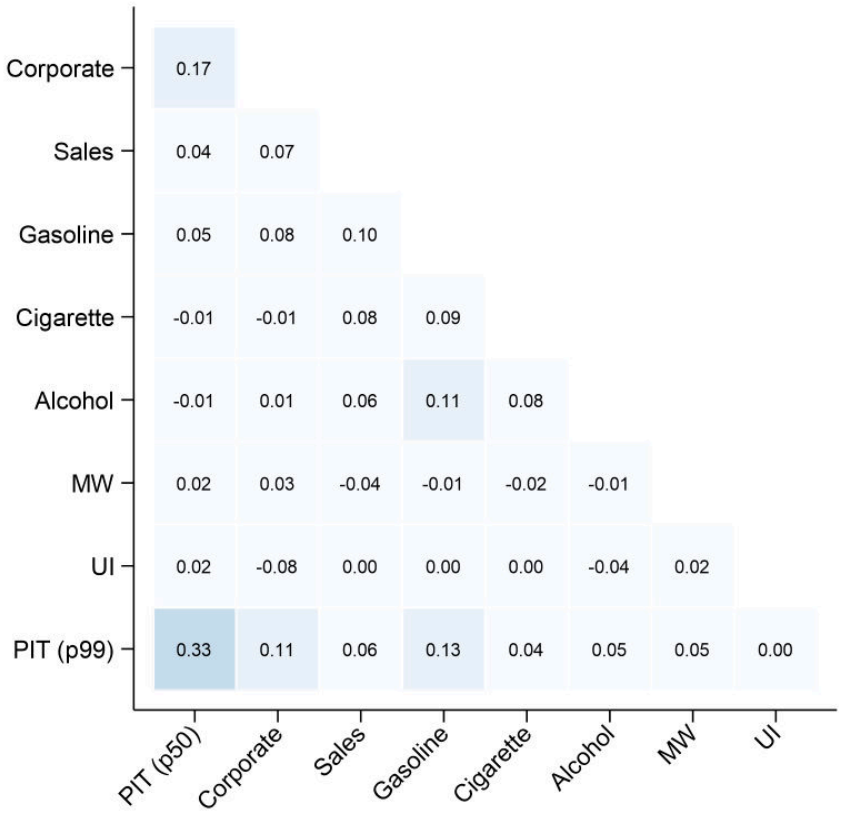
Figure A.9. Validation of the state-year deposit series

(a) State totals in 1987 (b) Growth-rate agreement



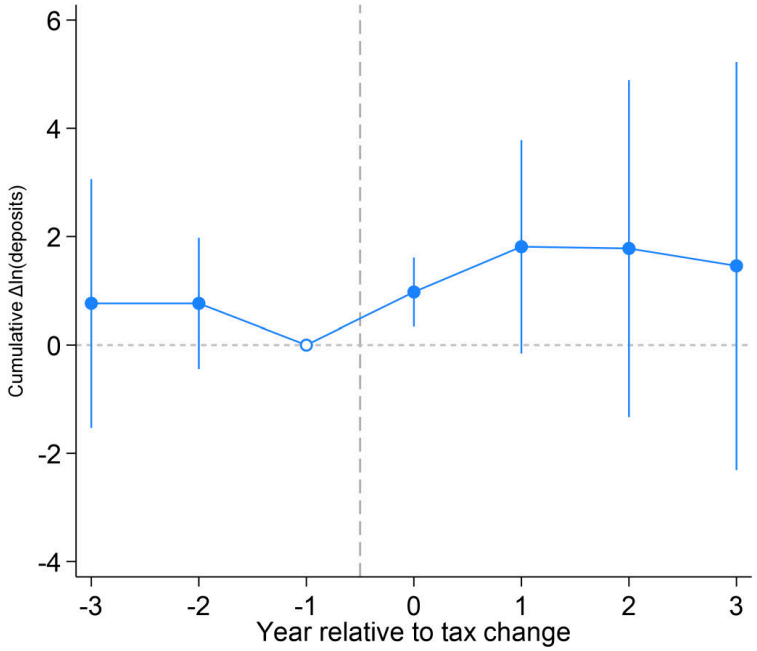
Notes. This figure compares state commercial-bank deposits in the Summary of Deposits and the Call Reports over 1987–2000. Panel (a) plots the two state totals in 1987 against the 45-degree line, and panel (b) plots the cross-state correlation of annual deposit growth in the two sources by year. The dashed vertical line marks the Riegle-Neal Act of 1994. Section 2 describes both sources.

Figure A.10. Correlation of annual changes in state taxes and policies



Notes. This figure plots pairwise correlations of annual within-state changes in the tax and policy variables of Table 1 for the fifty states over 1978–2019. Tax changes are signed so that a positive value is a cut, and the minimum wage and unemployment insurance replacement rate so that a positive value is a more generous policy. Table A.1 defines the variables.

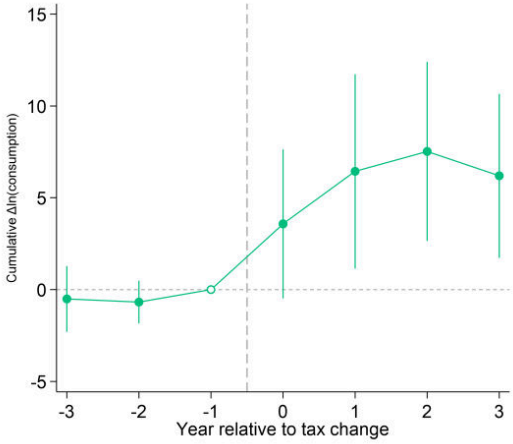
Figure A.11. Local projections: dynamic response of deposits to personal income tax changes, pooled



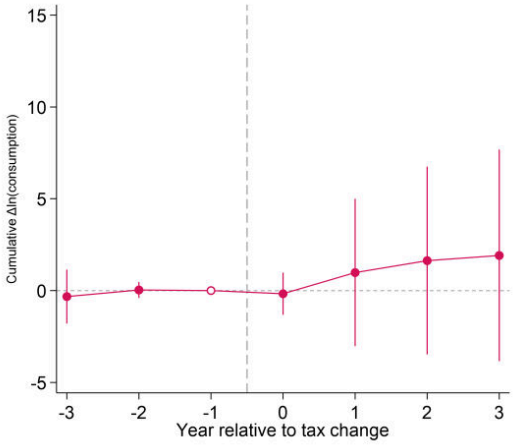
Notes. This figure plots coefficient estimates and 90% confidence intervals from equation (2), normalized to zero in the year before the tax change. The dependent variable is the cumulative change in log deposits from the year before the tax change to each horizon, and the regressor is the change in the log state net-of-tax rate at the median. Regressions include changes in population, the unemployment rate and income per capita between $t-2$ and $t-1$ and state and year fixed effects, over 1977–2019. Regressions are weighted by state deposits in 1977. Standard errors are clustered at the state level.

Figure A.12. Consumption around tax changes: local projections

(a) Tax cuts

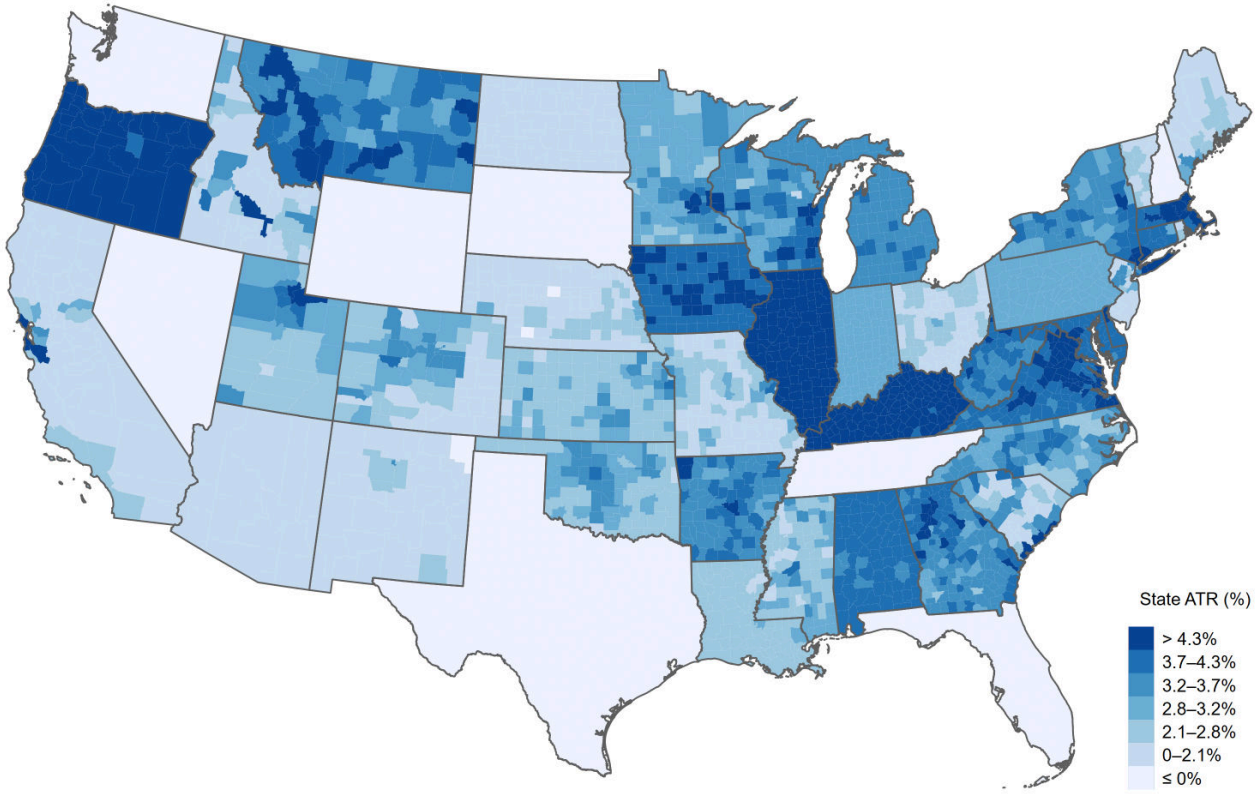


(b) Tax increases



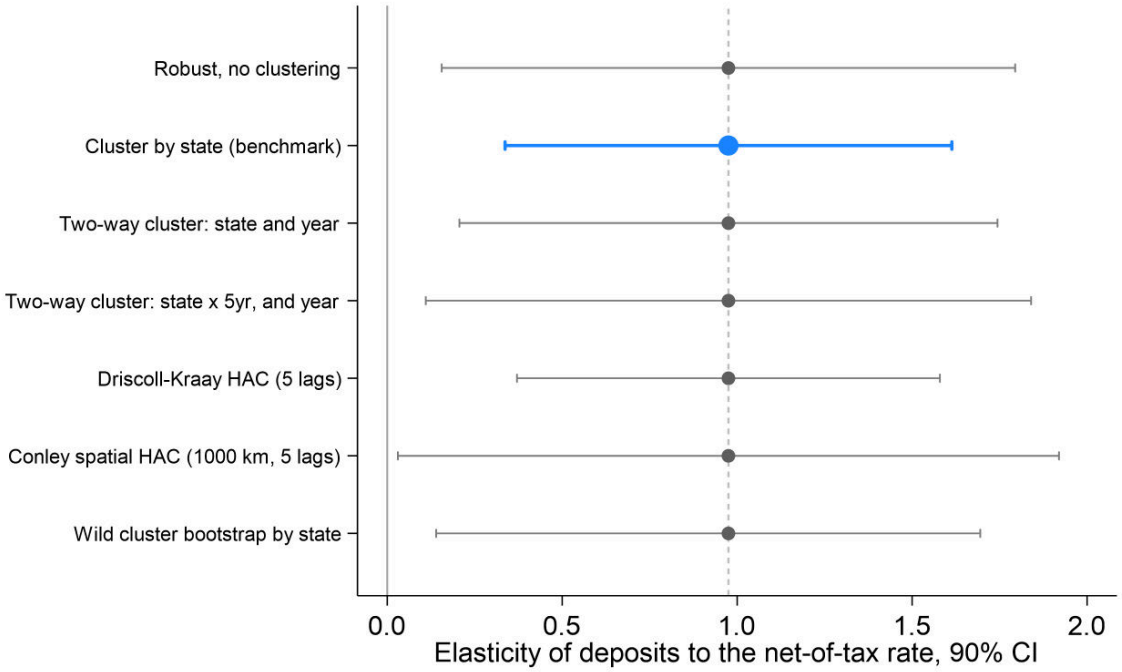
Notes. This figure plots coefficient estimates and 90% confidence intervals from equation (3), normalized to zero in the year before the tax change. Panel (a) reports tax cuts and panel (b) tax increases; for tax increases the sign is reversed, so that negative values indicate a decline. The dependent variable is the cumulative change in log consumption from the year before the tax change to each horizon, where consumption is general sales tax revenue divided by the general sales tax rate. The regressor is the change in the log state net-of-tax rate at the median, split into its positive and negative parts. The sample keeps state-years in which the general sales tax rate does not change within three years of the income tax change. Regressions include changes in population, the unemployment rate and income per capita between $t-2$ and $t-1$ and state and year fixed effects, over 1977–2019. Regressions are weighted by state deposits in 1977. Standard errors are clustered at the state level.

Figure A.13. State average tax rate by county, 2019



Notes. This figure maps the state average tax rate by county in 2019, the tax measure of Table 2. Sources are NBER TAXSIM and IRS Statistics of Income.

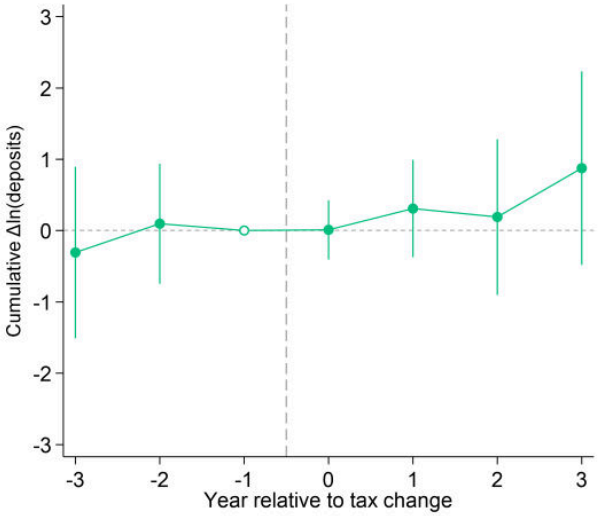
Figure A.14. Confidence intervals under alternative inference procedures



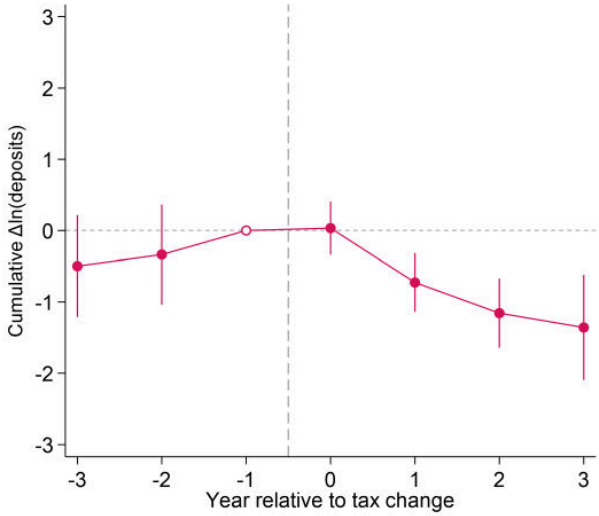
Notes. This figure plots the coefficient estimate and 90% confidence intervals from equation (1), as in column (3) of Table 1, under alternative variance estimators. The specification, sample and point estimate are identical across rows. The dependent variable is the annual change in log deposits. The regressor is the change in the log net-of-tax rate at the median. Regressions include changes in population, the unemployment rate and income per capita between $t-2$ and $t-1$ and state and year fixed effects, over the fifty states in 1977–2019. Regressions are weighted by state deposits in 1977. Standard errors in the benchmark row are clustered at the state level, and the wild cluster bootstrap resamples by state.

Figure A.15. Country-level local projections

(a) Tax cuts

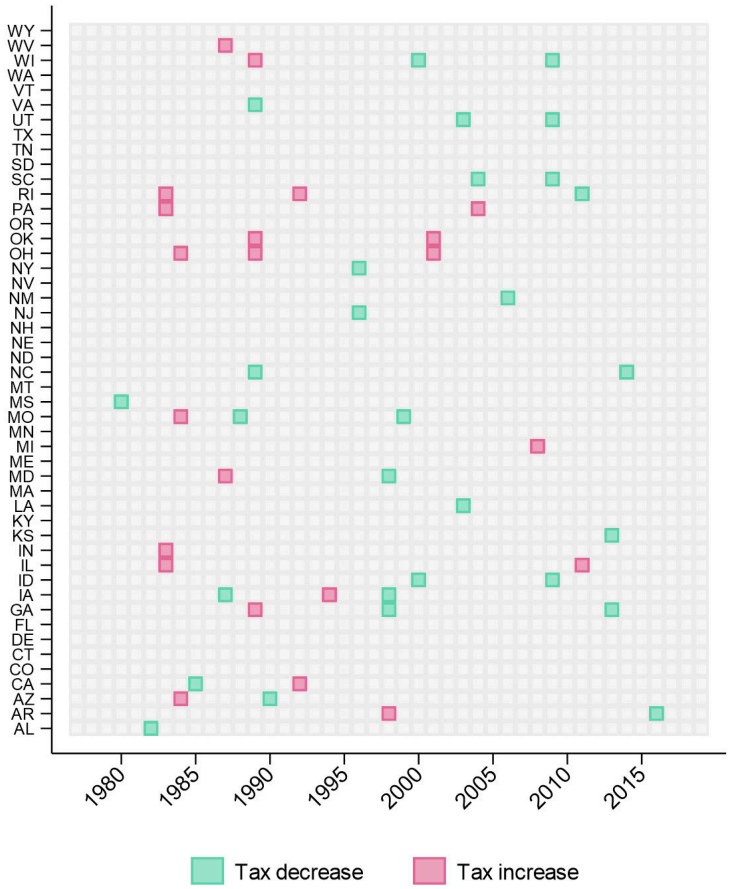


(b) Tax increases



Notes. This figure plots coefficient estimates and 90% confidence intervals from equation (13), normalized to zero in the year before the tax change. Panel (a) reports tax cuts and panel (b) tax increases; for tax increases the sign is reversed, so that negative values indicate a decline. The dependent variable is the cumulative change in log deposits from the year before the tax change to each horizon. The tax rate is the average personal income tax rate on the gross wage earnings of a married couple from the OECD Taxing Wages tables, and Appendix C describes the sample. Regressions include lagged population, GDP per capita and the unemployment rate and country and year fixed effects, over 2000–2019. Standard errors are clustered at the country level.

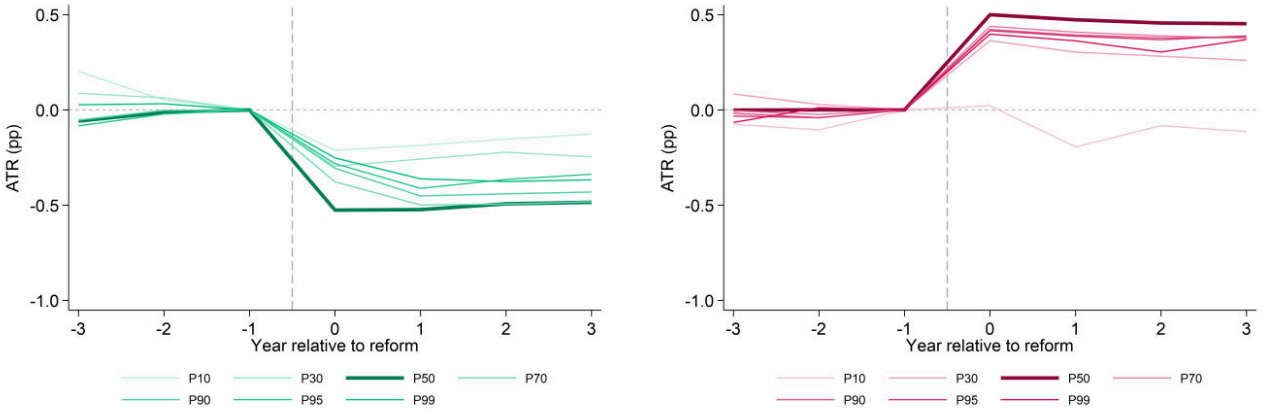
Figure A.16. State tax reforms in the stacked design



Notes. This figure plots the reforms of the stacked difference-in-differences design over 1977–2019. Each cell is a state-year, with tax cuts in green and tax increases in red. Section 4.1 defines the reforms.

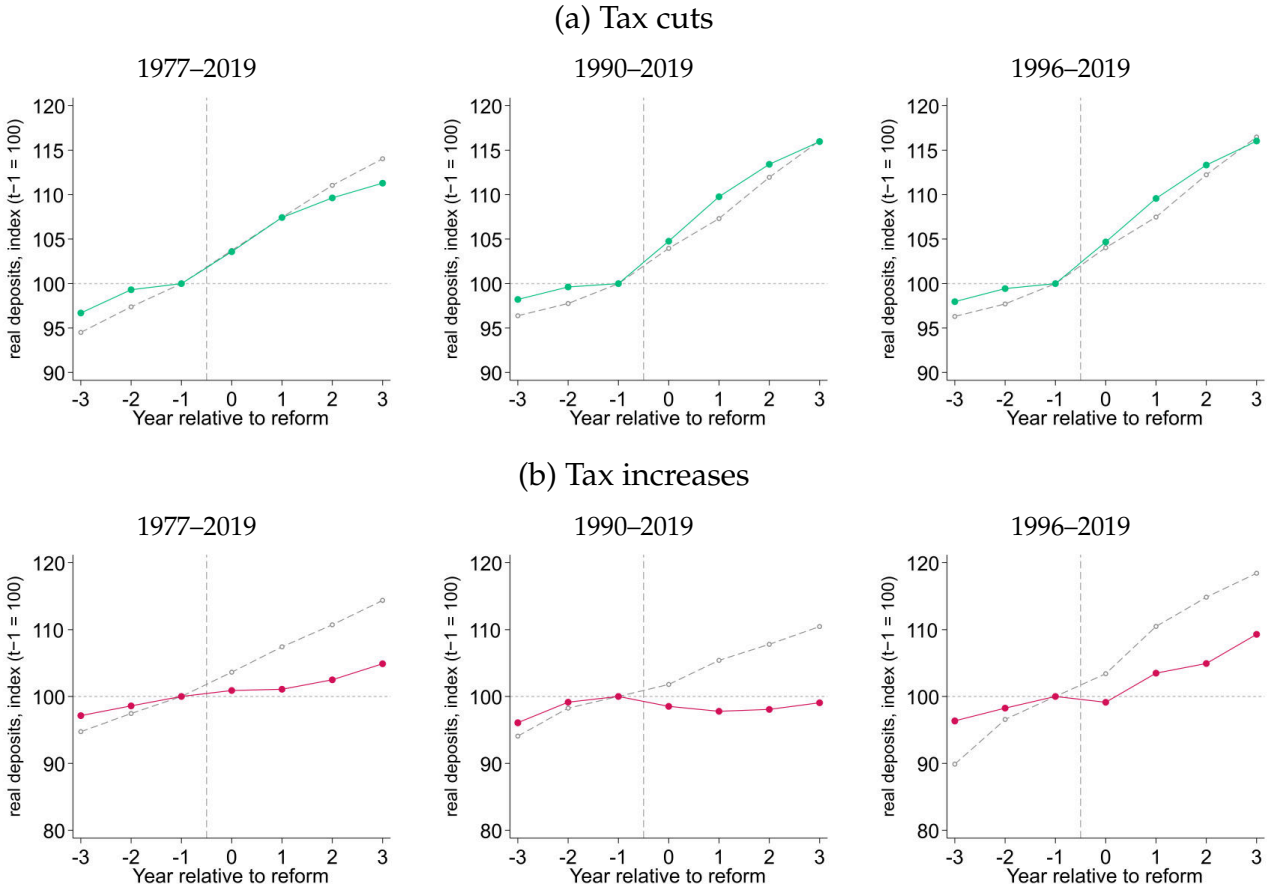
Figure A.17. Pass-through of median tax reforms across the income distribution

(a) Tax cuts (b) Tax increases



Notes. This figure plots coefficient estimates from equation (4), one line per percentile of the national income distribution, normalized to zero in the year before the reform. Panel (a) reports tax cuts and panel (b) tax increases. The dependent variable is the cumulative change in the state average tax rate at the indicated percentile relative to the year before the reform, in percentage points. Section 4.1 defines the reforms on the rate at the median, and the reforms are held fixed across lines. Regressions include pre-reform population, unemployment and income per capita interacted with event-time indicators and year fixed effects interacted with reform cohort, over reform cohorts in 1977–2019. Regressions are weighted by state deposits in 1977.

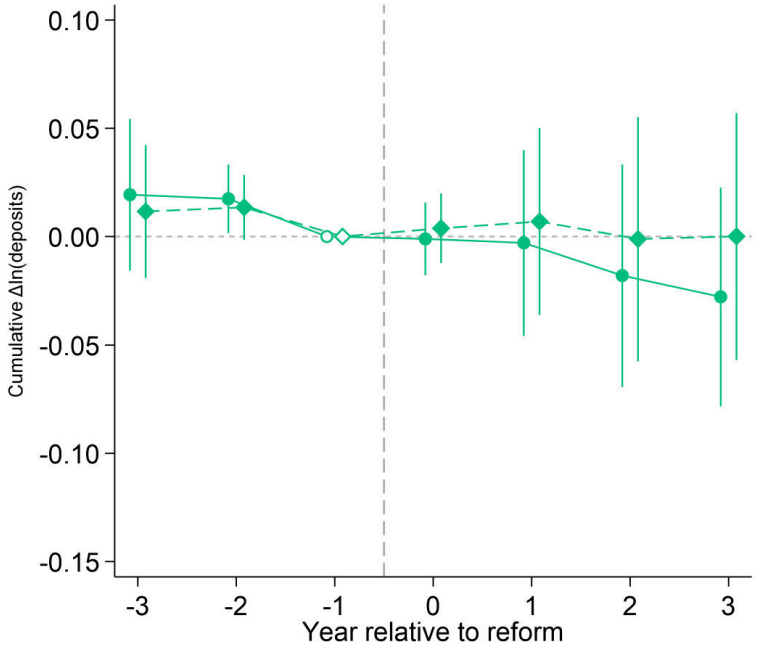
Figure A.18. Deposits in reforming and control states



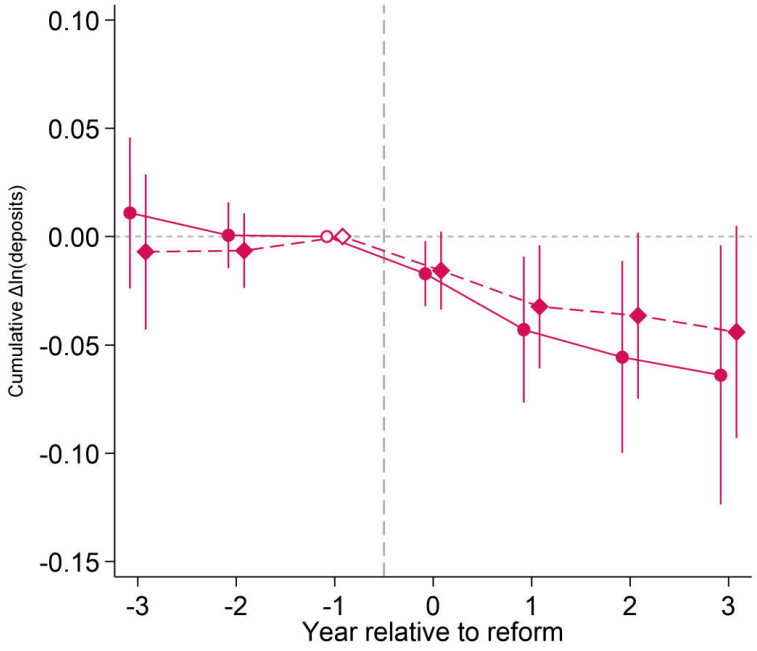
Notes. This figure plots mean deposits of reforming states and their control states in the stacked sample of Figure 3, indexed to 100 in the year before the reform and weighted by state deposits in 1977. Panel (a) reports tax cuts and panel (b) tax increases, for reform cohorts over 1977–2019, 1990–2019 and 1996–2019.

Figure A.19. State-level deposit response to the tax reforms: not-yet-treated control states

(a) Tax cuts



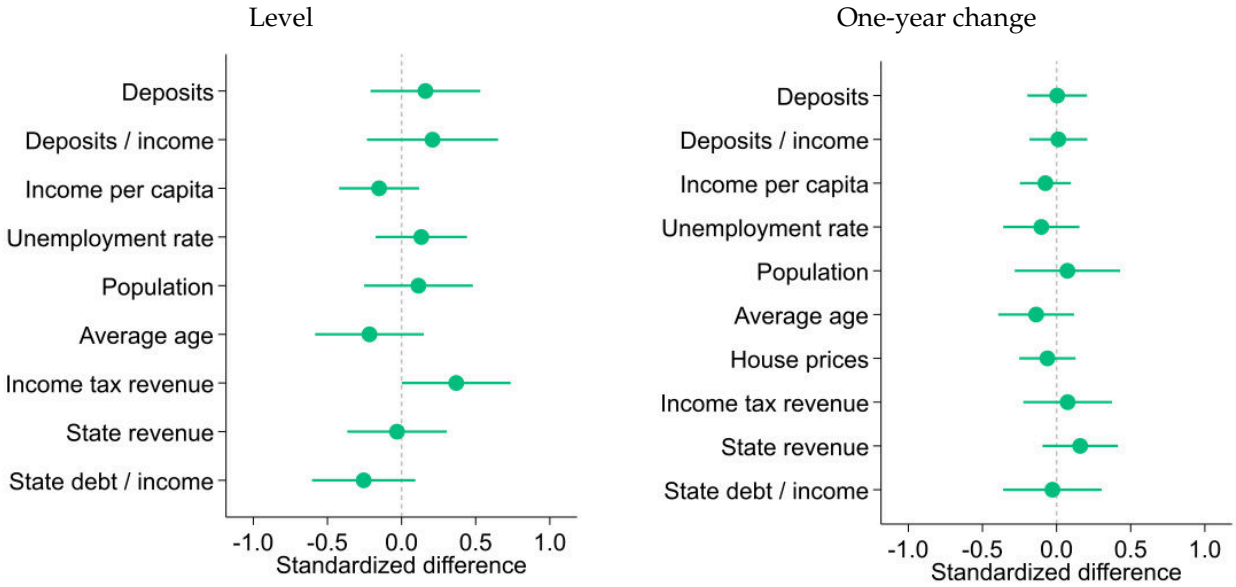
(b) Tax increases



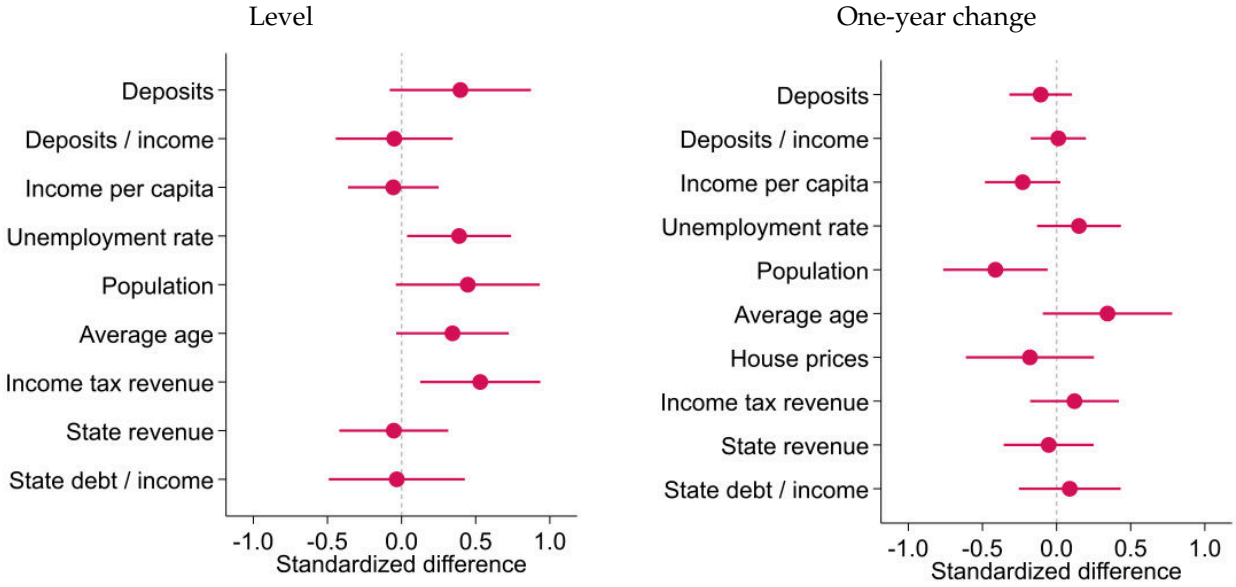
Notes. This figure re-estimates Figure 3, changing only the control pool. Circles and solid lines reproduce the baseline, in which control states have no large tax change of either sign within the ±3-year window, and diamonds and dashed lines further require that a control state has had no large tax change of the same sign as the reform before the end of the window. Panel (a) reports tax cuts and panel (b) tax increases. Whiskers are 90% confidence intervals. Standard errors are clustered at the state level.

Figure A.20. Pre-reform characteristics of reforming states

(a) Tax cuts



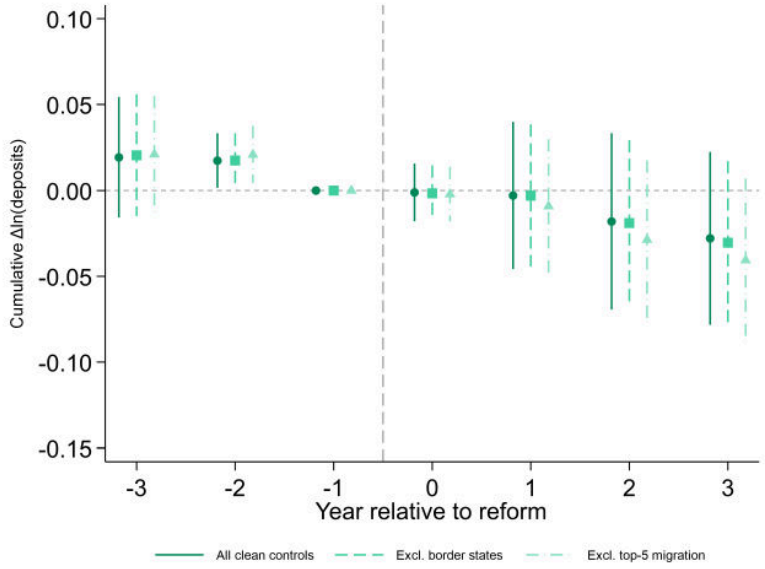
(b) Tax increases



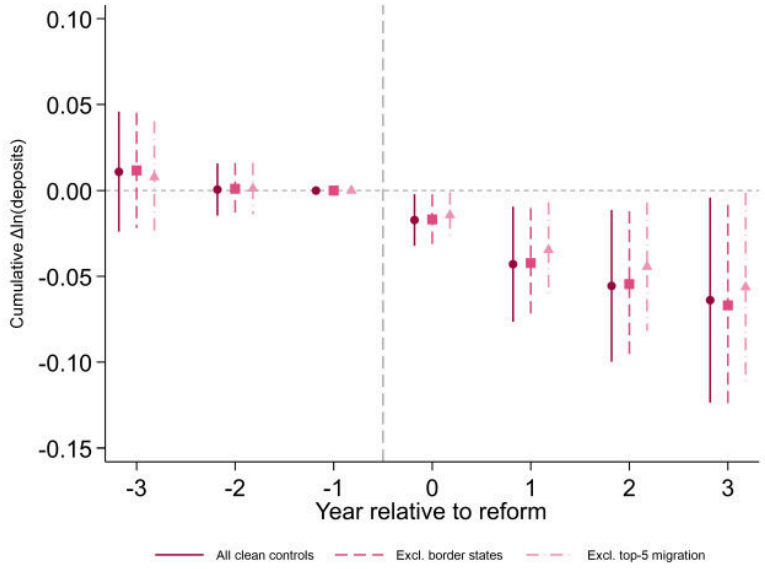
Notes. This figure plots coefficient estimates and 95% confidence intervals from regressions of a state characteristic on an indicator for the reforming state, on the stacked sample of Figure 3 with one observation per state and cohort. Panel (a) reports tax cuts and panel (b) tax increases. The dependent variable is the characteristic in the year before the reform in the left panels and its change between two years and one year before the reform in the right panels, standardized over the stacked sample so that coefficients are in standard deviations. Table A.1 defines the characteristics. House prices enter only as a change, and income tax revenue excludes states without a personal income tax. Regressions include reform-cohort fixed effects, over reform cohorts in 1977–2019. Standard errors are clustered at the state level.

Figure A.21. State-level deposit response to the tax reforms: excluding connected control states

(a) Tax cuts



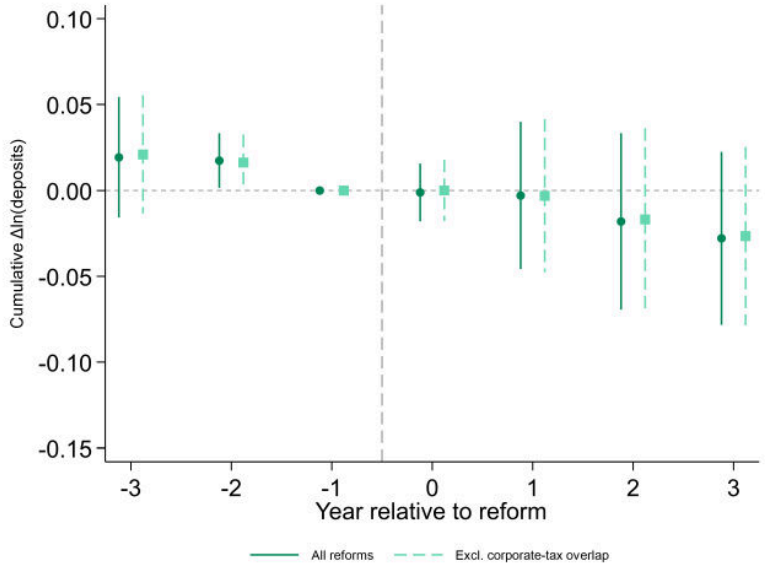
(b) Tax increases



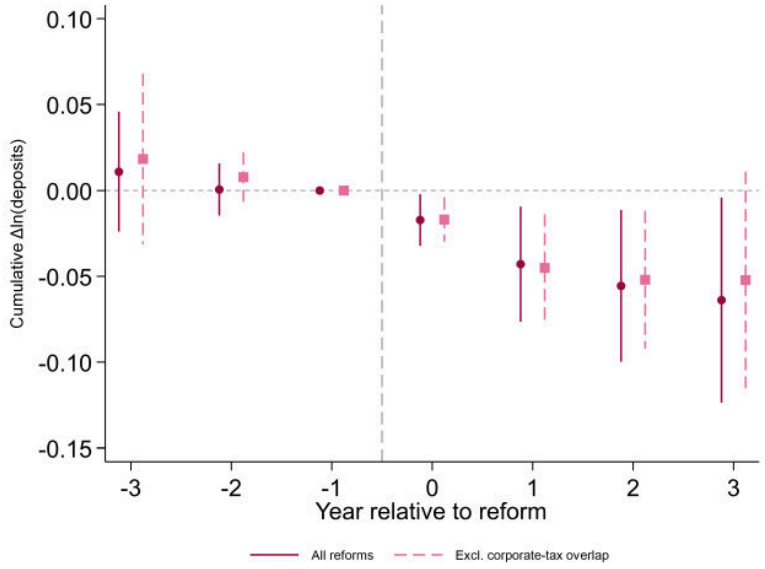
Notes. This figure re-estimates Figure 3, changing only the control pool. Circles reproduce the baseline. Squares exclude, for each reform, the control states that share a border with the reforming state, from the 2010 Census county adjacency file, and triangles exclude the five states receiving the largest share of the reforming state's outmigration flows, from the ACS state-to-state migration tables. The two sets are excluded separately. Panel (a) reports tax cuts and panel (b) tax increases. Whiskers are 90% confidence intervals. Standard errors are clustered at the state level.

Figure A.22. State-level deposit response to the tax reforms: excluding corporate tax changes

(a) Tax cuts



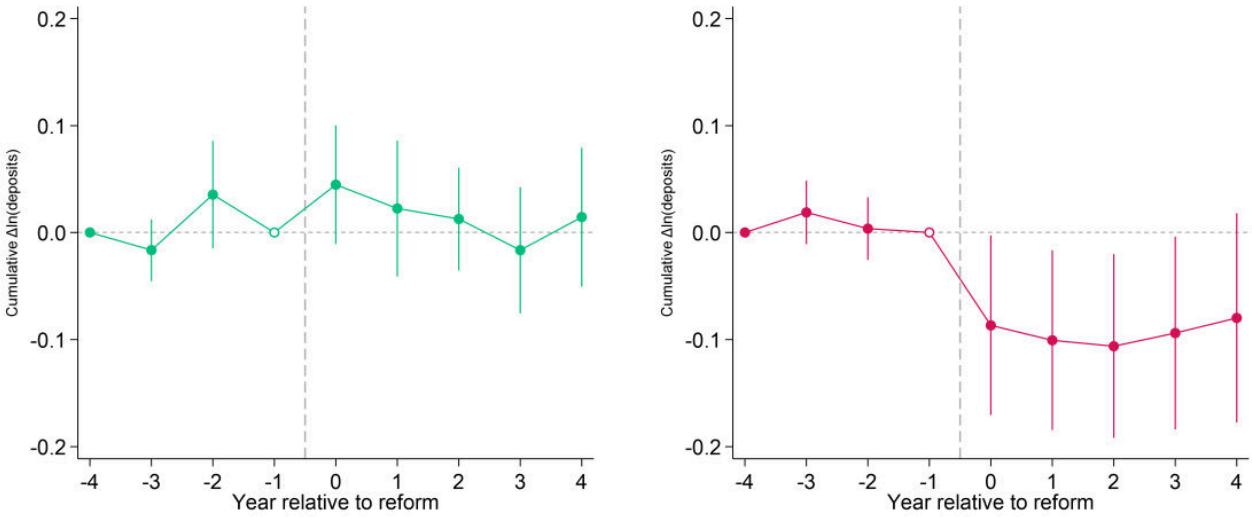
(b) Tax increases



Notes. This figure re-estimates Figure 3, changing only the set of reforms. Circles reproduce the baseline. Squares exclude the reforms that occur within one year of a change of at least 0.25 percentage points in the state corporate income tax rate, which leaves 16 tax increases and 31 tax cuts. The control pool is unchanged. Panel (a) reports tax cuts and panel (b) tax increases. Whiskers are 90% confidence intervals. Standard errors are clustered at the state level.

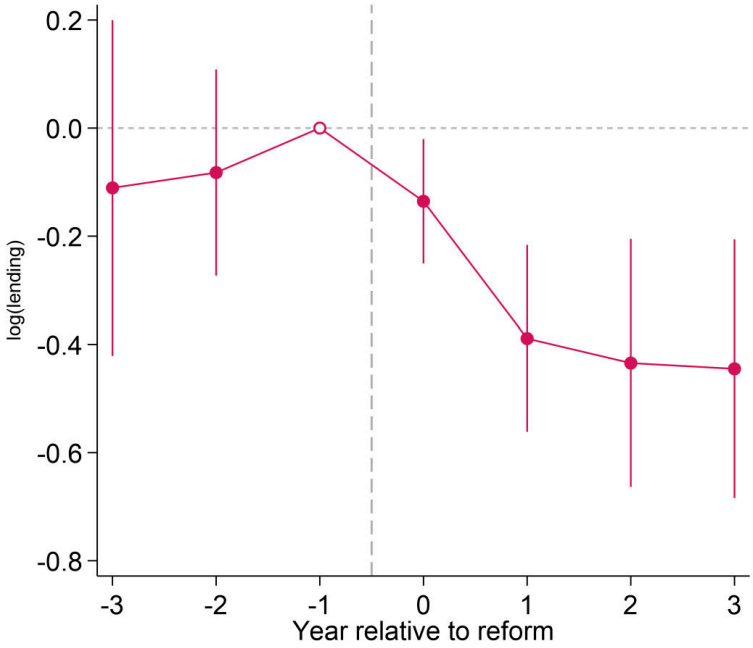
Figure A.23. Semiparametric difference-in-differences around large tax reforms

(a) Tax cuts (b) Tax increases



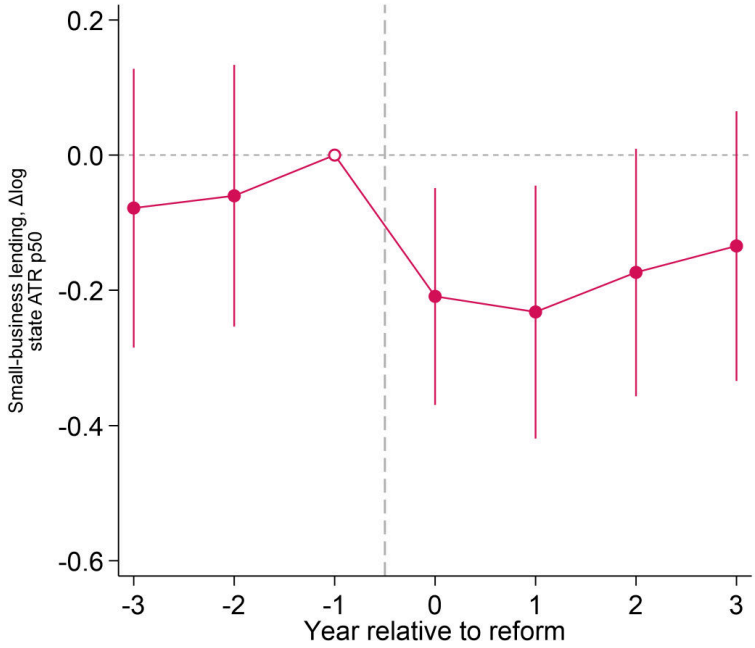
Notes. This figure plots coefficient estimates and 90% confidence intervals from equation (12), normalized to zero in the year before the reform. Panel (a) reports tax cuts and panel (b) tax increases. The dependent variable is log deposits of the reforming state and of its synthetic comparison state, built from states without a large tax change in the event window, and Appendix B describes the construction. Regressions include reform, reform×treatment and event-time fixed effects, over reform cohorts in 1977–2019. Standard errors are clustered at the reform level.

Figure A.24. Small business lending around tax increases: log levels



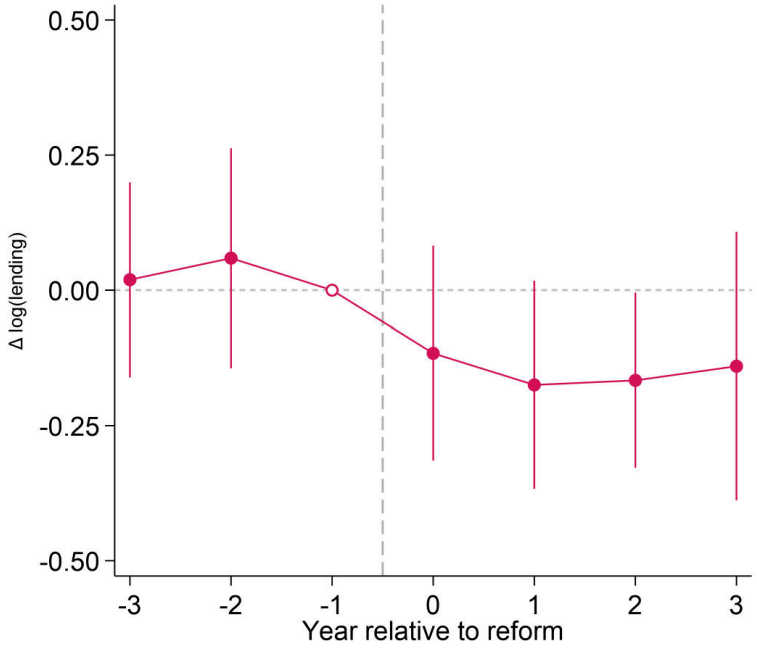
Notes. This figure plots coefficient estimates and 90% confidence intervals from the event-study version of equation (7), normalized to zero in the year before the reform. The dependent variable is the log of small business loan originations of a bank in a county. *Bank-Exp*, interacted with event-time indicators, is the share of the bank's deposits raised in the reform state in the year before the reform. Regressions include ten pre-reform bank characteristics interacted with event-time indicators and bank×county and county×year fixed effects interacted with reform cohort, over tax increase cohorts in 1996–2019. Standard errors are clustered at the bank level.

Figure A.25. Small business lending around tax increases: log differences



Notes. This figure plots coefficient estimates and 90% confidence intervals from the event-study version of equation (7), normalized to zero in the year before the reform. The dependent variable is the log difference of small business loan originations of a bank in a county. *Bank-Exp*, interacted with event-time indicators, is the share of the bank's deposits raised in the reform state in the year before the reform. Regressions include ten pre-reform bank characteristics interacted with event-time indicators and bank×county and county×year fixed effects interacted with reform cohort, over tax increase cohorts in 1996–2019. Standard errors are clustered at the bank level.

Figure A.26. Retained mortgage lending around tax increases



Notes. This figure plots coefficient estimates and 90% confidence intervals from the event-study version of equation (7), as in column (2) of Table A.15, normalized to zero in the year before the reform. The dependent variable is the growth rate of conventional mortgage originations that a bank retains on its balance sheet in a county. *Bank-Exp*, interacted with event-time indicators, is the share of the bank's deposits raised in the reform state in the year before the reform. Regressions include ten pre-reform bank characteristics interacted with event-time indicators and bank×county and county×year fixed effects interacted with reform cohort, over tax increase cohorts in 1990–2019. Standard errors are clustered at the bank level.

F Additional Tables

Table A.1. List of variables

Variable	Definition	Source
State-level		
D_{st}	Real bank deposits in the state, headquarters-state Call Reports through 1986 and branch-level SOD thereafter	FFIEC Call Reports, FDIC SOD
τ_{st}	State component of the ATR at the median of the national income distribution, $\tau_{st}^{50,\text{state}}$, the tax measure used throughout the paper	NBER TAXSIM, WID
$\tau_{st}^{p,\text{state}}$	State component of the ATR at percentile p of the national income distribution, see Appendix A	NBER TAXSIM, WID
τ_{st}^{p}	Combined federal and state ATR at percentile p, used as a robustness check	NBER TAXSIM, WID
$\tau_{st}^{p,\text{MTR}}$	State marginal tax rate at percentile p, used as a robustness check	NBER TAXSIM, WID
$Treat_{sr}$	Reforming state of cohort r, top-decile change in the state ATR	NBER TAXSIM, WID
$IBBEA_t$	Indicator for the years from 1994 on, when the Riegle-Neal Interstate Banking and Branching Efficiency Act was enacted	Riegle-Neal Act
Population_{st}	State resident population	Census Bureau
$\text{Unemployment rate}_{st}$	State unemployment rate	BLS
$\text{Income per capita}_{st}$	Real personal income per capita	BEA
$\text{Deposits / income}_{st}$	Real bank deposits over real personal income	FFIEC Call Reports, FDIC SOD, BEA
Average age_{st}	Mean age of the state population	CPS ASEC (IPUMS)
House prices_{st}	Annual change in the log all-transactions house price index, county indices averaged with county population weights	FHFA, BEA
$\text{Income tax revenue}_{st}$	State individual income tax revenue, real per capita, by fiscal year	Census Annual Survey of State Government Finances

Continued on the next page

Table A.1 (continued)

Variable	Definition	Source
State revenue$_{st}$	State own-source general revenue (taxes, current charges, and miscellaneous revenue), real per capita, by fiscal year	Census Annual Survey of State Government Finances
State debt$_{st}$	State government debt outstanding over personal income, by fiscal year	Census Annual Survey of State Government Finances
Top corporate rate$_{st}$	Combined federal and state top statutory corporate income tax rate, with the state rate net of federal deductibility	Akcigit et al. (2022), Robinson and Tazhidinova (2025)
Sales tax rate$_{st}$	General sales tax rate	Robinson and Tazhidinova (2025)
Consumption$_{st}$	Real general sales tax revenue divided by the general sales tax rate, by fiscal year	Robinson and Tazhidinova (2025)
Excise rates$_{st}$	Gasoline, cigarette, and spirits per-unit excise rates in 2019 dollars	Robinson and Tazhidinova (2025)
Minimum wage$_{st}$	State minimum wage	CSPP
UI replacement rate$_{st}$	Average weekly UI benefit over the average weekly wage in covered employment	DOL Financial Data Handbook 394
County-level		
τ_{ct}	State component of the ATR at the county's average adjusted gross income per return measured two years earlier	NBER TAXSIM, IRS SOI
D_{ct}	County bank deposits, aggregated from branches	FDIC SOD
County-Exp$_{cr}$	Exposure of the banks lending in the county, weighted by their pre-reform small business lending shares, see Section 6	FDIC SOD, FFIEC CRA disclosures
L_{crt}	County small business loan originations	FFIEC CRA disclosures
Est_{crst}	Number of establishments by size class	Census CBP
Emp_{crit}	Employment by two-digit industry	Census CBP
$Small_s$	Establishments with fewer than twenty employees	Census CBP

Continued on the next page

Table A.1 (continued)

Variable	Definition	Source
$HighDep_i$	Top tercile of industries by the share of firms using bank loans to start or expand	Census SBO 2007
Income per return$_{ct}$	Average adjusted gross income per tax return, also used to rank counties into within-state income terciles	IRS SOI
Population, income$_{ct}$	County population and real personal income per capita	BEA
Bank characteristics$_{cr}$	Deposit-weighted pre-reform log assets, return on assets, equity ratio, deposits over assets, and real estate loans over assets of the banks operating in the county	FFIEC Call Reports, FDIC SOD
Bank-level		
$Bank\text{-}Exp_{br}$	Share of the bank's branch deposits raised in the reforming state, see Section 5.1	FDIC SOD
Home state$_{bt}$	State holding most of the bank's branch deposits	FDIC SOD
Single-state bank$_{br}$	Bank holding at least 90% of its branch deposits in one state in the year before the reform	FDIC SOD
Balance-sheet outcomes$_{bt}$	June-filing deposits, savings and time deposits, wholesale funding, liabilities, assets, cash, securities, and loan categories	FFIEC Call Reports
Deposit rate$_{bt}$	Interest expense on domestic deposits over domestic deposits, June filing	FFIEC Call Reports
X_{br}^{pre}	Pre-reform log assets, equity ratio, return on assets, securities share, non-interest income share, loan shares, funding ratios, and the share of lending extended outside the bank's branch counties	FFIEC Call Reports, FFIEC CRA disclosures
Bank-county-level		
ΔY_{bcrt}	Growth rate of the bank's real small business loan originations in the county	FFIEC CRA disclosures
Mortgage originations$_{bct}$	Real conventional mortgage originations, total, retained, and sold within the year	FFIEC HMDA
Deposits$_{bct}$	Branch deposits of the bank in the county	FDIC SOD

Continued on the next page

Table A.1 (continued)

Variable	Definition	Source
Country-level		
D_{ct}	Total banking-system deposits, CPI deflated	Jamilov et al. (2025)
τ_{ct}	Average personal income tax rate on gross wage earnings, excluding social security contributions, of a two-earner married couple without children earning 100% and 67% of the average wage	OECD Taxing Wages
Macro controls$_{ct}$	Population, GDP per capita, and the unemployment rate	Müller et al. (2025)

Notes. This table lists the variables used in the analysis, grouped by the unit at which they vary. Dollar values are in 2010 dollars unless stated otherwise.

Table A.2. Descriptive statistics: deposit samples

	Obs.	Mean	SD
Panel A: State-level			
Deposits (real 2010 USD, millions)	2,100	101,628	147,202
Δ ln deposits	2,100	0.027	0.102
ATR, p50 (state + federal)	2,100	8.338	2.272
State ATR, p50	2,100	2.018	1.411
ATR, p99 (state + federal)	2,100	29.599	3.204
Top corporate rate	2,100	40.412	5.663
Sales tax rate	2,100	4.631	1.889
Gasoline tax (2019 USD)	2,100	0.294	0.077
Cigarette tax (2019 USD)	2,085	0.908	0.851
Spirits tax (2019 USD)	2,092	3.912	3.780
Minimum wage (USD)	1,900	5.095	1.841
UI replacement rate	2,100	0.365	0.059
Population (thousands)	2,100	5,496	6,077
Unemployment rate	2,100	5.811	2.079
Income per capita (USD)	2,100	35,098	7,829
Panel B: County-level			
Δ ln deposits	90,007	0.012	0.072
County ATR (state + federal)	90,007	10.156	2.779
County state ATR	90,007	2.619	1.563
Income per return (USD)	90,007	39,635	14,200
Panel C: Country-level			
Δ ln deposits	396	0.037	0.047
ATR	396	16.096	6.510
Population (millions)	396	45.2	67.5
GDP per capita (USD)	396	43,402	14,135
Unemployment rate	396	6.805	3.482

Notes. This table reports summary statistics for the estimation samples of the deposit analysis. Panel A covers the fifty states over 1977–2019 (Table 1), panel B counties over 1991–2019 (Table 2), and panel C advanced economies over 2000–2019 (Figure A.15). Table A.1 defines the variables.

Table A.3. Descriptive statistics: credit samples

	Obs.	Mean	SD
Panel A: Bank-level			
Total assets (USD millions)	75,687	437	7,300
Deposits/liabilities	75,687	0.943	0.096
Deposit rate (%)	75,687	1.598	0.741
Δ ln deposits (cumulative)	75,687	0.047	0.310
Δ deposit rate (pp)	75,687	-0.018	0.559
Bank-Exp	75,687	0.244	0.429
Panel B: Bank-county-level, small business lending			
Real originations at $T-1$ (USD thousands)	519,092	2,151	9,100
Origination growth (midpoint)	519,092	-0.132	0.797
Bank-Exp	519,092	0.024	0.145
Bank-Exp, exposed banks	26,848	0.473	0.438
ln assets (USD thousands)	519,092	16.054	1.966
Equity ratio	519,092	0.130	0.059
Return on assets	519,092	0.0153	0.0097
Securities share	519,092	0.119	0.116
Non-interest income share	519,092	0.308	0.182
Real estate loan share	519,092	0.270	0.228
C&I loan share	519,092	0.135	0.099
Interest expense over deposits	519,092	0.024	0.014
Wholesale funding share	519,092	0.308	0.215
Out-of-branch origination share	519,092	0.522	0.418
Panel C: Bank-county-level, mortgage lending			
Origination growth, total (midpoint)	554,579	-0.046	0.616
Origination growth, retained	529,677	-0.056	0.841
Origination growth, sold	436,807	-0.038	0.921
Bank-Exp	554,579	0.027	0.154
Bank-Exp, exposed banks	44,662	0.341	0.435
Panel D: County-level, real activity			
County-Exp	70,224	0.029	0.155
Δ ln small business lending	70,224	0.023	0.519
Δ ln establishments	147,482	0.002	0.108
Δ ln employment	764,031	0.004	0.422
ln population (persons)	68,849	10.058	1.401
ln income per capita (USD thousands)	68,849	3.286	0.272
Dep.-weighted ln assets (USD thousands)	68,849	13.399	1.907
Dep.-weighted return on assets	68,849	0.0109	0.0053
Dep.-weighted equity ratio	68,849	0.102	0.019
Dep.-weighted deposits/assets	68,849	0.800	0.064
Dep.-weighted real estate loan share	68,849	0.399	0.125

Notes. This table reports summary statistics for the estimation samples of the credit analysis. Panel A covers the bank-level design of Table 3, panel B the small business lending design of Table 4, panel C the mortgage design of Table A.15, and panel D the county-level designs of Table 6. Table A.1 defines the variables.

Table A.4. Deposits and personal income tax changes: robustness

	(1)	(2)	(3)	(4)	(5)	(6)
Benchmark	1.622***	1.368***	0.975**	1.029***	0.987***	1.119***
	(0.390)	(0.397)	(0.388)	(0.347)	(0.301)	(0.362)
Single filer	1.995***	1.750**	1.250*	1.360*	1.331*	1.909**
	(0.683)	(0.717)	(0.757)	(0.740)	(0.744)	(0.863)
Excluding recessions	1.606***	1.410***	1.273**	1.318***	1.222***	1.400***
	(0.531)	(0.525)	(0.520)	(0.488)	(0.410)	(0.438)
Sample through 2021	1.576***	1.315***	0.992**	1.039***	0.991***	1.188***
	(0.391)	(0.397)	(0.403)	(0.362)	(0.316)	(0.385)
Unweighted	1.456***	1.230***	0.996**	1.018**	1.043**	1.076**
	(0.443)	(0.440)	(0.440)	(0.431)	(0.440)	(0.538)
Dep. var winsorized	1.501***	1.260***	0.908**	0.942***	0.898***	1.094***
	(0.374)	(0.370)	(0.363)	(0.313)	(0.278)	(0.357)
Excl. card and cyber banks	1.539***	1.283***	0.880**	1.059***	1.016***	1.126***
	(0.401)	(0.410)	(0.400)	(0.365)	(0.320)	(0.369)
State MTR	0.824**	0.776**	0.740**	0.755**	0.741***	0.789***
	(0.350)	(0.345)	(0.306)	(0.313)	(0.285)	(0.291)
State ATR (p90)	2.015***	1.832***	1.303**	1.423**	1.352**	2.263***
	(0.600)	(0.641)	(0.647)	(0.674)	(0.601)	(0.837)
Combined ATR	1.436***	1.209***	0.841**	0.886***	0.848***	1.144**
	(0.365)	(0.369)	(0.362)	(0.319)	(0.274)	(0.446)
Year FE	✓	✓	✓	✓	✓	✓
State FE		✓	✓	✓	✓	✓
Demographic controls			✓	✓	✓	✓
Corporate and sales				✓	✓	✓
Excises, MW, UI					✓	✓
99th percentile rate						✓

Notes. This table reports estimates of equation (1) at the state-year level over 1977–2019. Each row changes one feature of the specification of Table 1, as indicated, and reports the coefficient on that row's own net-of-tax regressor. The dependent variable is the annual change in log bank deposits. The main regressor is the change in the log net-of-tax rate implied by the state average tax rate at the median of the national income distribution. Demographic controls are the changes in log population, the unemployment rate and log income per capita between $t-2$ and $t-1$. Table A.1 defines all variables. Regressions are weighted by state deposits in 1977, except in the *Unweighted* row. Fixed effects are denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.5. Deposits and personal income tax changes: 99th-percentile rate

	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \ln(1-\tau^{p99})$	0.498	0.341	0.180	0.171	0.093	-0.253
	(0.316)	(0.301)	(0.293)	(0.285)	(0.316)	(0.329)
$\Delta \ln(1-\tau^{p50})$						1.119***
						(0.362)
Observations	2,100	2,100	2,100	2,100	2,100	2,100
R^2	0.256	0.285	0.311	0.311	0.314	0.315
Year FE	✓	✓	✓	✓	✓	✓
State FE		✓	✓	✓	✓	✓
Demographic controls			✓	✓	✓	✓
Corporate and sales				✓	✓	✓
Excises, MW, UI					✓	✓

Notes. This table reports estimates of equation (1) at the state-year level over 1977–2019. The dependent variable is the annual change in log bank deposits. The main regressor $\Delta \ln(1-\tau^{p99})$ is the change in the log net-of-tax rate implied by the state average tax rate at the 99th percentile of the national income distribution. Demographic controls are the changes in log population, the unemployment rate and log income per capita between $t-2$ and $t-1$. Table A.1 defines all variables. Regressions are weighted by state deposits in 1977. Fixed effects are denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the state level. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.10$.

Table A.6. Deposits and personal income tax changes before and after interstate branching

	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \ln(1-\tau^{p50})$	1.959***	1.706***	1.272**	1.281**	1.213**	1.303**
	(0.452)	(0.492)	(0.522)	(0.509)	(0.546)	(0.525)
$\Delta \ln(1-\tau^{p50}) \times$ IBBEA	-0.887	-0.888	-0.779	-0.720	-0.647	-0.574
	(1.441)	(1.499)	(1.570)	(1.599)	(1.665)	(1.699)
Observations	2,100	2,100	2,100	2,100	2,100	2,100
R^2	0.258	0.287	0.312	0.312	0.315	0.315
Year FE	✓	✓	✓	✓	✓	✓
State FE		✓	✓	✓	✓	✓
Demographic controls			✓	✓	✓	✓
Corporate and sales				✓	✓	✓
Excises, MW, UI					✓	✓
99th percentile rate						✓

Notes. This table reports estimates of equation (1) at the state-year level over 1977–2019. The dependent variable is the annual change in log bank deposits. The main regressors are $\Delta \ln(1-\tau^{p50})$, the change in the log net-of-tax rate implied by the state average tax rate at the median of the national income distribution, and its interaction with IBBEA, an indicator for the years from 1994 on. Demographic controls are the changes in log population, the unemployment rate and log income per capita between $t-2$ and $t-1$. Table A.1 defines all variables. Regressions are weighted by state deposits in 1977. Fixed effects are denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the state level. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.10$.

Table A.7. State-level deposit response to the tax reforms: alternative event windows

	(1) 1977–2019	(2) 1990–2019	(3) 1996–2019
Tax increases	-0.045**	-0.072***	-0.041***
	(0.022)	(0.021)	(0.014)
Observations	4,834	2,765	1,561
R^2	0.537	0.500	0.536
Tax cuts	-0.012	0.007	0.004
	(0.023)	(0.020)	(0.020)
Observations	7,719	6,489	6,342
R^2	0.536	0.541	0.544
Year FE	✓	✓	✓
Controls	✓	✓	✓

Notes. This table reports estimates of equation (5) separately for tax increases and tax cuts at the state-year level over 1977–2019. Each column restricts the reform cohorts to the window indicated in the column header. The dependent variable is the cumulative change in log deposits relative to the year before the reform. The main regressor is the interaction of the treated-state indicator with the post-reform indicator. Controls are pre-reform state population, the unemployment rate and income per capita, interacted with the event-time indicators. Table A.1 defines all variables. Regressions are weighted by state deposits in 1977. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.8. Within-bank deposit response to the tax reforms

	Tax increases				Tax cuts			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	-0.051**	-0.093***	-0.090***	-0.085***	0.031*	-0.005	-0.006	0.000
	(0.025)	(0.028)	(0.024)	(0.023)	(0.017)	(0.019)	(0.018)	(0.019)
Treat × Post × HHI				0.022				0.017
				(0.020)				(0.012)
Observations	102,219	102,219	102,111	102,111	282,220	282,220	282,105	282,105
R^2	0.192	0.319	0.531	0.533	0.194	0.365	0.578	0.580
Year FE	✓				✓			
County FE	✓	✓			✓	✓		
Bank × year FE		✓	✓	✓		✓	✓	✓
Bank × county FE			✓	✓			✓	✓
Post × HHI				✓				✓

Notes. This table reports estimates of the bank-county stacked difference-in-differences specification described in Section 4.3 separately for tax increases and tax cuts at the bank-county-year level over 1990–2019. The dependent variable is the cumulative change in log deposits of a bank in a county relative to the year before the reform. *Treat* × *Post* is the interaction of an indicator for the bank's counties in the reform state with the post-reform indicator, and HHI is the Herfindahl index of deposit shares across banks in the county in the year before the reform, standardized to mean zero and unit standard deviation. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are two-way clustered at the bank and county level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.9. State-level deposit response to the tax reforms: alternative definitions and specification

	(1) Tax increases	(2) Tax cuts
Benchmark	-0.045**	-0.012
	(0.022)	(0.023)
Reforms	24	33
No controls	-0.060***	-0.010
	(0.020)	(0.026)
Reforms	24	33
Top 5% threshold	-0.060***	-0.010
	(0.018)	(0.037)
Reforms	7	22
Isolation window ± 4	-0.046*	-0.013
	(0.026)	(0.024)
Reforms	19	31
Opposite-sign isolation only	-0.034*	-0.004
	(0.017)	(0.021)
Reforms	45	56

Notes. This table reports estimates of equation (5) separately for tax increases and tax cuts at the state-year level over 1977–2019. Each row changes one feature of the specification of column (1) of Table A.7, as indicated. The dependent variable is the cumulative change in log deposits relative to the year before the reform. The main regressor is the interaction of the treated-state indicator with the post-reform indicator. Controls are pre-reform state population, the unemployment rate and income per capita, interacted with the event-time indicators. Table A.1 defines all variables. Regressions are weighted by state deposits in 1977. Regressions include year fixed effects interacted with reform cohort. Standard errors (in parentheses) are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.10. Border-county deposit response to the tax reforms

	Tax increases		Tax cuts	
	(1)	(2)	(3)	(4)
Treat × Post	-0.033**	-0.050**	-0.014	0.002
	(0.015)	(0.019)	(0.032)	(0.037)
Observations	5,362	5,362	12,754	12,754
R^2	0.872	0.914	0.816	0.887
Pair × year FE	✓	✓	✓	✓
County FE		✓		✓

Notes. This table reports estimates of the border-county analogue of equation (5) separately for tax increases and tax cuts at the county-pair-year level over 1990–2019. The dependent variable is the cumulative change in log commercial-bank deposits in the county relative to the year before the reform. *Treat* × *Post* is the interaction of an indicator for the border county of the reform state with the post-reform indicator, and each pair matches that county with a contiguous county across the state line. Table A.1 defines all variables. Regressions are weighted by county commercial-bank deposits in 1987. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.11. Bank balance sheet response around tax reforms: tax cuts

	Total deposits (1)	Deposit rate (2)	Savings deposits (3)	Time deposits (4)	Wholesale funding (5)	Total liabilities (6)
Panel A: Liabilities						
Bank-Exp × Post	-0.000	0.003	-0.003	0.005	0.064***	-0.005
	(0.006)	(0.004)	(0.007)	(0.007)	(0.018)	(0.005)
Bank FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	126,869	126,869	117,171	117,570	126,755	126,883
R^2	0.630	0.180	0.582	0.582	0.519	0.633
	Total assets (1)	Cash (2)	Securities (3)	Total loans (4)	Real estate loans (5)	C&I loans (6)
Panel B: Assets						
Bank-Exp × Post	-0.004	-0.037***	-0.002	-0.005	-0.007	-0.012
	(0.005)	(0.010)	(0.011)	(0.006)	(0.007)	(0.012)
Bank FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	126,884	126,782	124,855	126,400	125,669	119,420
R^2	0.661	0.548	0.452	0.617	0.622	0.511

Notes. This table reports estimates of equation (6) for tax cuts at the bank-year level over 1990–2019. The sample includes banks that hold at least 90% of their deposits in a single state, located in the reform state or in a state contiguous to it, in the year before the reform. The dependent variable is the cumulative change in the log of the indicated balance-sheet item relative to the year before the reform, and in column (2) of Panel A the change in the deposit rate. *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.12. Small business lending around tax increases: branch counties included

	(1)	(2)	(3)	(4)
Bank-Exp × Post	-0.109**	-0.113**	-0.092**	-0.085
	(0.049)	(0.054)	(0.046)	(0.052)
Observations	681,948	681,948	681,948	608,013
R^2	0.175	0.244	0.262	0.402
Bank FE	✓	✓	✓	
Bank × county FE				✓
County FE	✓			
Year FE	✓			
County × year FE		✓	✓	✓
Controls	✓	✓	✓	✓

Notes. This table reports estimates of equation (7) for tax increases at the bank-county-year level over 1996–2019. The dependent variable is the growth rate of small business loan originations of a bank in a county, defined as the change divided by the average of its levels in adjacent years. *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are ten pre-reform bank characteristics, interacted with the post indicator in columns (1)–(2) and with event-time indicators in columns (3)–(4). The specification is that of columns (1)–(4) of Table 4, keeping the bank-county pairs in which the bank operated a branch before the reform. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.13. Small business lending around tax increases: no minimum lending threshold

	(1)	(2)	(3)	(4)
Bank-Exp × Post	-0.128**	-0.126**	-0.112**	-0.129**
	(0.056)	(0.058)	(0.053)	(0.059)
Observations	807,127	805,480	805,480	718,569
R^2	0.139	0.217	0.245	0.364
Bank FE	✓	✓	✓	
Bank × county FE				✓
County FE	✓			
Year FE	✓			
County × year FE		✓	✓	✓
Controls	✓	✓	✓	✓

Notes. This table reports estimates of equation (7) for tax increases at the bank-county-year level over 1996–2019. The dependent variable is the growth rate of small business loan originations of a bank in a county, defined as the change divided by the average of its levels in adjacent years. *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are ten pre-reform bank characteristics, interacted with the post indicator in columns (1)–(2) and with event-time indicators in columns (3)–(4). The specification is that of columns (1)–(4) of Table 4, without the minimum lending threshold on bank-county cells in the year before the reform. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.14. Small business lending around tax reforms: tax cuts

	(1)	(2)	(3)	(4)
Bank-Exp × Post	0.086	0.098	0.078	0.085
	(0.077)	(0.092)	(0.084)	(0.091)
Observations	1,529,416	1,529,412	1,529,412	1,297,030
R^2	0.164	0.246	0.260	0.413
Bank FE	✓	✓	✓	
Bank × county FE				✓
County FE	✓			
Year FE	✓			
County × year FE		✓	✓	✓
Controls	✓	✓	✓	✓

Notes. This table reports estimates of equation (7) for tax cuts at the bank-county-year level over 1996–2019. The dependent variable is the growth rate of small business loan originations of a bank in a county, defined as the change divided by the average of its levels in adjacent years. *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are ten pre-reform bank characteristics, interacted with the post indicator in columns (1)–(2) and with event-time indicators in columns (3)–(4). Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.15. Mortgage lending around tax reforms: tax increases

	(1) Total	(2) Retained	(3) Sold	(4) Total
Bank-Exp × Post	-0.112**	-0.170**	-0.163	-0.074
	(0.050)	(0.068)	(0.101)	(0.053)
Bank-Exp × Post × Outside				-0.116
				(0.122)
Observations	554,579	529,677	436,807	554,579
R^2	0.410	0.276	0.406	0.410
p-value, inside = outside				0.34
Bank × county FE	✓	✓	✓	✓
County × year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes. This table reports estimates of equation (7) for tax increases at the bank-county-year level over 1990–2019. The dependent variable is the growth rate of conventional mortgage originations of a bank in a county, defined as the change divided by the average of its levels in adjacent years, for all originations in columns (1) and (4), originations the bank retains on its balance sheet in column (2), and originations it sells within the year in column (3). *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are ten pre-reform bank characteristics interacted with event-time indicators. Column (4) interacts *Bank-Exp* × Post with *Outside*, an indicator for counties outside the reform state, and the *p*-value tests that the two responses are equal. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.16. Mortgage lending around tax increases: number of loans

	(1) Total	(2) Retained	(3) Sold
Bank-Exp × Post	-0.118*	-0.182**	-0.156
	(0.061)	(0.074)	(0.101)
Observations	554,579	529,677	436,807
R^2	0.434	0.288	0.408
Bank × county FE	✓	✓	✓
County × year FE	✓	✓	✓
Controls	✓	✓	✓

Notes. This table reports estimates of equation (7) for tax increases at the bank-county-year level over 1990–2019. The dependent variable is the growth rate of the number of conventional mortgage originations of a bank in a county, defined as the change divided by the average of its levels in adjacent years, for all originations in column (1), originations the bank retains on its balance sheet in column (2), and originations it sells within the year in column (3). *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are ten pre-reform bank characteristics interacted with event-time indicators. The specification is that of columns (1)–(3) of Table A.15, with loan counts in place of dollar volumes. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.17. Mortgage lending around tax increases: within borrower income groups

	(1) Total	(2) Retained	(3) Sold
Bank-Exp × Post	-0.124**	-0.208***	-0.160**
	(0.052)	(0.071)	(0.076)
Observations	2,155,695	1,869,279	1,561,694
R^2	0.313	0.248	0.357
Bank × county × income FE	✓	✓	✓
County × year × income FE	✓	✓	✓
Controls	✓	✓	✓

Notes. This table reports estimates of equation (7) for tax increases at the bank-county-income-year level over 1990–2019. The dependent variable is the growth rate of conventional mortgage originations of a bank in a county to borrowers in an income group, defined as the change divided by the average of its levels in adjacent years, for all originations in column (1), originations the bank retains on its balance sheet in column (2), and originations it sells within the year in column (3). *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are ten pre-reform bank characteristics interacted with event-time indicators. The specification is that of columns (1)–(3) of Table A.15, with originations split into four groups by applicant income and fixed effects interacted with income group. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.18. Mortgage lending around tax reforms: tax cuts

	(1) Total	(2) Retained	(3) Sold
Bank-Exp × Post	0.020	-0.060	0.016
	(0.035)	(0.058)	(0.089)
Observations	2,060,107	1,970,799	1,625,836
R^2	0.397	0.261	0.390
Bank × county FE	✓	✓	✓
County × year FE	✓	✓	✓
Controls	✓	✓	✓

Notes. This table reports estimates of equation (7) for tax cuts at the bank-county-year level over 1990–2019. The dependent variable is the growth rate of conventional mortgage originations of a bank in a county, defined as the change divided by the average of its levels in adjacent years, for all originations in column (1), originations the bank retains on its balance sheet in column (2), and originations it sells within the year in column (3). *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are ten pre-reform bank characteristics interacted with event-time indicators. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.19. Local economic activity around tax reforms: tax cuts

	Small business lending		Establishments				Employment	
			All	Excl. tradable	High Dep	Low Dep		Excl. tradable
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
County-Exp × Post	0.032	0.029						
	(0.022)	(0.022)						
County-Exp × Post × Small			-0.004	-0.008*	-0.009	-0.001		
			(0.004)	(0.004)	(0.008)	(0.009)		
County-Exp × Post × High Dep							-0.001	-0.013
							(0.012)	(0.012)
Observations	213,437	209,204	452,834	450,912	1,855,131	1,597,931	2,209,575	1,733,855
R^2	0.183	0.184	0.516	0.514	0.187	0.199	0.164	0.186
County FE	✓	✓						
County × size FE			✓	✓				
County × industry FE							✓	✓
County × industry × size FE					✓	✓		
Year FE	✓	✓						
County × year FE			✓	✓	✓	✓	✓	✓
Size × year FE			✓	✓	✓	✓		
Industry × year FE							✓	✓
Controls		✓						

Notes. This table reports estimates of equations (8), (9) and (10) for tax cuts at the county level over 1998–2011. The dependent variable is the growth rate of county small business loan originations in columns (1)–(2), of the number of establishments in columns (3)–(6), and of employment in columns (7) and (8). *County-Exp* is the exposure of the banks lending in the county to the reform state, weighted by their pre-reform small business lending shares. Controls in column (2) are the county's pre-reform population and income per capita and the deposit-weighted pre-reform characteristics of its banks, interacted with event-time indicators. *High Dep* and *Low Dep* denote the top and bottom terciles of industries by bank dependence, and columns (4) and (8) re-estimate columns (3) and (7) excluding tradable industries. Table A.1 defines all variables. Fixed effects are interacted with reform cohort and denoted at the bottom of the table. Standard errors (in parentheses) are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.20. Deposit pass-through to small business lending: tax increases

	(1) First stage	(2) Reduced form	(3) 2SLS
Dependent variable	Δ ln deposits	SBL growth	SBL growth
Bank-Exp $\times$ Post	-0.036***	-0.138**	
	(0.006)	(0.060)	
Predicted Δ ln deposits $\times$ Post			3.878**
			(1.803)
First-stage F	34.3		34.3
Observations	347,128	448,589	448,589

Notes. This table reports two-sample instrumental-variable estimates for tax increases. Column (1) is estimated at the bank-year level and its dependent variable is the change in log deposits relative to the year before the reform. Columns (2) and (3) are estimated at the bank-county-year level and their dependent variable is the growth rate of small business loan originations of a bank in a county. *Bank-Exp* is the share of the bank's deposits raised in the reform state in the year before the reform. Controls are pre-reform deposit growth in column (1) and ten pre-reform bank characteristics interacted with event-time indicators in columns (2) and (3). Column (2) reproduces column (4) of Table 4, and column (3) replaces *Bank-Exp* with the predicted reform-induced change in log deposits, the column (1) coefficient times *Bank-Exp*. Table A.1 defines all variables. Column (1) includes year fixed effects and columns (2) and (3) the fixed effects of column (4) of Table 4, all interacted with reform cohort. Standard errors (in parentheses) are clustered at the bank level, and the standard error in column (3) is the two-sample delta-method standard error. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.