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Local Government Resilience to Federal Tax Reform: Evidence from the SALT Deduction Cap

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Local Government Resilience to Federal Tax Reform: Evidence from the SALT Deduction Cap

Federico Corredor*

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Abstract

The Tax Cuts and Jobs Act of 2017 capped the federal deduction for state and local taxes (SALT), increasing the tax price of local public services for high-income residents in high-tax jurisdictions. This paper examines the impact of this policy on local government finances, exploiting variation in exposure across counties based on average pre-reform SALT deductions. Counties most exposed to the cap did not reduce public expenditures, experience declines in own-source revenues, or shift toward non-deductible revenue sources. These findings challenge the view that federal deductibility is essential for sustaining local fiscal capacity and underscore both the central role of property taxation in local public finance and the resilience of subnational governments to federal tax policy shocks.

Keywords: local public finance, federal tax deductibility, salt deduction cap, TCJA, tax expenditures, fiscal federalism

JEL Codes: H71, H24, H73, H77

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1 Introduction

U.S. local governments finance essential public services, ranging from education and public safety to infrastructure, primarily through their own-source revenues. Among these, property taxes are particularly important: they are highly visible, politically salient, and central to local fiscal autonomy. Historically, the federal deduction for state and local taxes (SALT) reduced the effective burden of local taxation for high-income households, allowing jurisdictions to raise revenue with lower perceived costs to their constituents. The Tax Cuts and Jobs Act (TCJA) of 2017 fundamentally altered this arrangement by imposing a $10,000 cap on SALT deductions, increasing the tax price of local public services, especially for affluent taxpayers in high-tax jurisdictions. While the reform significantly reduced a major federal tax expenditure, its implications for local public finance were uncertain and potentially far-reaching.[1] This paper examines the extent to which the SALT deduction cap undermined local fiscal capacity and public service provision.

To estimate the dynamic effects of the SALT deduction cap on revenues, expenditures, and their composition, I exploit quasi-experimental variation in exposure across counties, measured using pre-reform SALT deductions relative to the $10,000 policy threshold. I construct multiple treatment measures from Internal Revenue Service Statistics of Income (SOI) data and link them to annual county-level fiscal outcomes from the Government Finance Database (Pierson et al. 2015). Building on recent methodological advances in difference-in-differences designs with continuous treatments (Callaway et al. 2024), the analysis moves beyond binary exposure indicators to capture variation in treatment intensity across jurisdictions.

Contrary to prominent concerns raised during the policy debate, I find no evidence that the SALT deduction cap induced fiscal retrenchment at the local level. Expenditures on core services such as health, education, public welfare, and capital outlays remained stable in more exposed counties. I also find no evidence of substitution toward alternative, non-deductible revenue sources such as user fees, utility charges, or miscellaneous revenues. Property taxes appear to remain central to local fiscal stability, and counties were able to sustain fiscal effort despite the reduction in the

[1] The SALT deduction was among the five largest federal tax expenditures, with an annual cost exceeding 0.5 percent of GDP (Congressional Budget Office 2017).

federal subsidy implicit in deductibility. These results are robust to alternative specifications, placebo tests, and sensitivity analyses.

These findings are consistent with several institutional and economic mechanisms. Property tax revenues are relatively sticky in the short run due to assessment practices and budget cycles. Moreover, the SALT cap primarily affected high-income taxpayers in fiscally strong jurisdictions, where demand for local public goods may be relatively inelastic. Finally, the broader TCJA package, including federal rate reductions, changes in itemization behavior, and state-level policy responses, may have partially offset the effective increase in the local tax price.

This paper contributes to several strands of the literature. First, it advances empirical research in fiscal federalism by documenting how local governments adapt to changes in intergovernmental tax policy. A large body of work emphasizes the interaction between residential mobility, local tax burdens, and public service provision, highlighting the role of revenue capacity in aligning services with community preferences (Tiebout 1956; Glaeser 1996; Brueckner and Saavedra 2001; Oates 2005, 2006; Ahmad and Brosio 2006; Gruber 2013; Tresch 2015; Oates and Fischel 2016). Consistent with political economy models of fiscal structure (Brennan and Buchanan 1980; Hettich and Winer 1984), federal tax deductibility can dampen fiscal competition by lowering households' sensitivity to local tax prices, and in consequence, limiting deductibility mitigates vertical distortions and strengthens horizontal fiscal discipline (Flochel and Madies 2002).

Second, the findings contribute to the literature on the resilience of property taxation. It moves beyond the view that property revenues passively adjust to spending needs, and emphasizes political, institutional, and economic constraints on local tax effort (Ross and Yan 2013; Ihlanfeldt and Willardsen 2014; Ross and Mughan 2018; Brien 2018; Mikesell and Liu 2013; Cromwell and Ihlanfeldt 2015; Horton et al. 2024; Brosy and Ferrero 2025). Despite exposure to housing market shocks, property taxes are often regarded as a stabilizing source of local revenue, and many governments have historically managed to preserve this revenue stream (Alm et al. 2011).

Finally, this paper complements a growing literature documenting household-level responses to the SALT deduction cap, including changes in willingness to pay for public goods (Ambrose and Valentin 2023), housing prices (Tong 2021; Kessler and Bruce 2022; Coleman and Tester 2023; Ambrose and Valentin 2024), charitable giving (Lalumia 2024), and migration decisions (Coen-Pirani and Sieg 2019; Rauh 2022; Drukker 2023). By focusing on local government behavior rather than household responses, this study highlights the institutional dimension of fiscal adjustment, which is essential for understanding the full consequences of federal tax policy.

With the SALT deduction cap originally scheduled to expire in 2025 and recently modified under the One Big Beautiful Bill Act (OBBBA), which temporarily raised the cap from $10,000 to $40,000, this paper provides timely evidence on how limits to deductibility affect local government finance and how subnational governments adjust to federal tax reforms. The results highlight both the resilience and the constraints of subnational fiscal systems and inform ongoing debates on the role of federal tax expenditures in shaping local public finance.

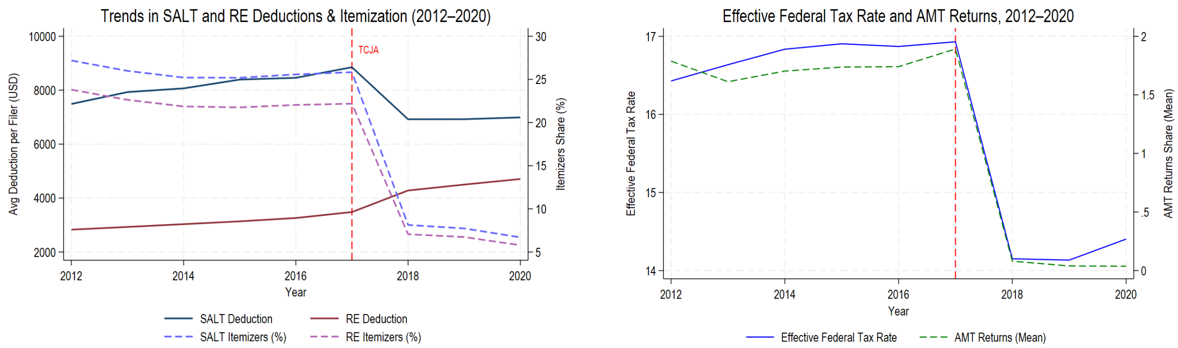
2 Institutional Background and Policy Debate

The deductibility of state and local taxes from federal income taxes has been debated since the inception of the U.S. federal income tax in 1913 (Barker 2019). The original rationale for the deductibility was to prevent the federal government from monopolizing the tax base and to encourage state and local spending by lowering the political cost of raising local taxes (Berg 2020). Because deductibility reduces the effective tax price of local public goods, it could shape fiscal incidence and local fiscal behavior (Buchanan and Pauly 1970; Lindsay 1972). Limiting deductibility could depress local revenues, reduce public spending, raise local tax rates, or induce substitution toward non-deductible revenue sources (Gramlich 1985; Feldstein and Metcalf 1987; Holtz-Eakin and Rosen 1988, 1990; Metcalf 2008; Heim and Abbas 2015; Coyne 2017).

The TCJA substantially restructured federal tax provisions related to state and local tax deductibility. When filing their federal income taxes, taxpayers may choose between claiming a fixed standard deduction or itemizing eligible deductions. Prior to the reform, itemizing taxpayers could fully deduct income or sales taxes, real estate taxes, and personal property taxes from their federal tax base. Beginning in tax year 2018, the TCJA limited total SALT deductions to $10,000 and nearly doubled the standard deduction (from $6,350 to $12,000 for single filers and from $13,000 to $24,000 for joint filers). These changes produced a sharp decline in itemization (Figure 1a): the share of filers claiming SALT deductions fell from 25 percent in 2017 to fewer than 10 percent in 2018, and average SALT deductions per filer declined by roughly $2,000. Nearly 19 million taxpayers stopped itemizing, and total deductible SALT claims fell by approximately $467 billion. At the same time, average real estate tax deductions increased modestly, reflecting a compositional shift toward higher-income taxpayers among the smaller pool of remaining itemizers.

The TCJA also introduced tax relief, especially for high-income households. It lowered marginal income and capital gains tax rates and effectively eliminated the Alternative Minimum Tax (AMT) for most filers (Figure 1b). These provisions suggest that, even as affluent households faced a higher tax price for local services, many still experienced net federal tax cuts. This potentially muted the financial pressure they might otherwise have felt from the SALT cap.

Figure 1. Taxpayer Deductions and Federal Tax Relief and AMT



(a) SALT and RE Deductions *(b) Federal Tax Relief and AMT*

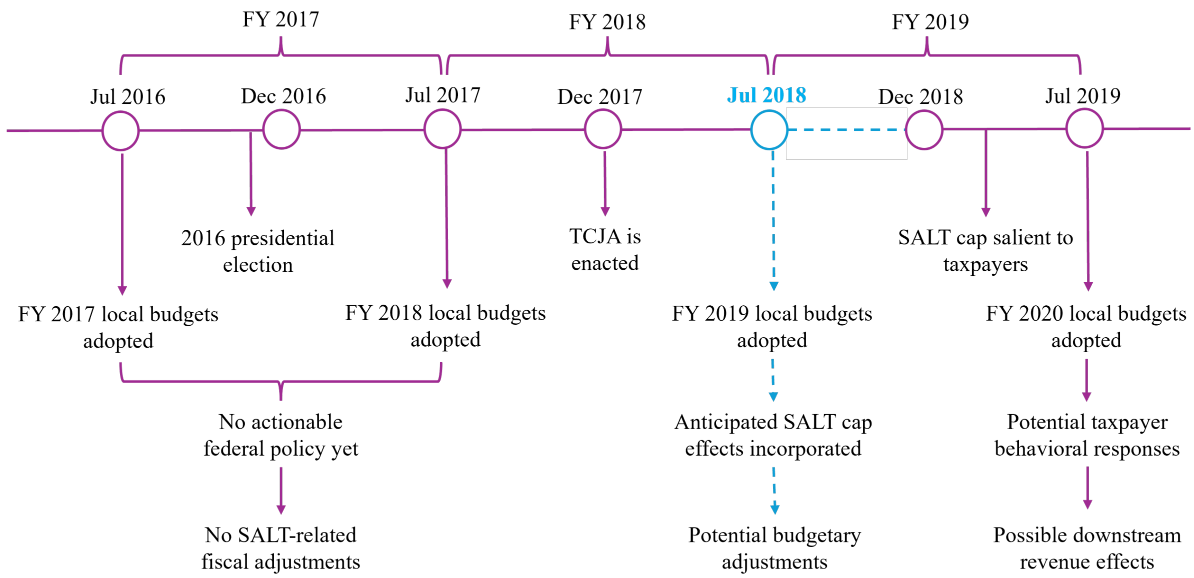
Source: Author's calculations using IRS Statistics of Income (SOI) data
Notes: The left panel displays national trends in the average SALT and real estate (RE) deductions per itemizer and the share of filers claiming each deduction for a balanced panel of 1,135 counties from 2012 to 2020. Following the 2017 TCJA (red vertical line), both the average SALT deduction and the SALT itemization rate drop sharply, by about $2,000 and from 25 percent to under 10 percent, respectively. The average RE deduction increases modestly, reflecting a compositional shift. The right panel shows concurrent national declines in the effective federal income tax rate and the share of returns subject to the AMT, consistent with the TCJA's broader tax relief provisions. Together, the panels illustrate the timing and magnitude of the reform's impact on affluent taxpayers.

Figure 2 presents the policy implementation timeline. The local budgeting process encompasses preparation, drafting, public deliberation, and formal approval that typically unfolds several months prior to the start of the fiscal year.[2] The TCJA was enacted in December 2017, although during fiscal year (FY) 2017 and most of FY 2018 the SALT cap had been discussed as part of the Republican legislative agenda, no enacted federal policy existed for local governments to incorporate into their budgets. Although anecdotal evidence suggests that some high-income

[2] While the federal government operates on a fiscal year beginning in October, local governments follow heterogeneous fiscal calendars, most commonly beginning in January, July, or October. For expositional clarity, the timeline assumes a July–June local fiscal year. The argument remains unchanged under alternative fiscal calendars; the dashed lines denote the fiscal year beginning after June 2018.

taxpayers prepaid 2018 property taxes in December 2017, reflecting early awareness of the cap's implications, the policy became directly salient to households during the 2019 tax filing season.[3] Because budget decisions are generally made well in advance of the fiscal year, FY 2019 represents the first full budget cycle in which local governments could plausibly incorporate anticipated effects of the SALT deduction cap into fiscal planning.

Figure 2. SALT Deduction Cap Timeline



Source: Author's elaboration
Notes: The timeline highlights the fiscal year structure of local governments and the sequencing of the TCJA, assuming a July–June local fiscal year. Although the bill passed in December 2017, most FY 2018 budgets were already finalized. FY 2019 marks the first full year in which counties could respond to the SALT cap, which was a visible component of tax reform debates throughout 2017. The figure also accounts for the possibility of anticipatory effects as early as late 2017 or early 2018.

The SALT deduction cap was one of the most controversial components of the TCJA. The Tax Policy Center warned that limiting the deduction could weaken local revenue capacity and reduce investment in public services.[4] Likewise, the National Association of Counties (NACo) argued the cap weakened local fiscal autonomy and disrupted the intergovernmental balance of taxation, while a full restoration of the SALT deduction would improve counties' ability to fund and deliver basic public services.[5] Additionally, it was argued that deductibility helped counterbalance the geographic and distributive imbalances of federal expenditures (Zhang 2020). Critics, however,

[3] The IRS later ruled that only assessments already issued in 2017 were deductible, limiting the effectiveness of these prepayments. See USA Today (2017).

[4] Tax Policy Center, 2017

[5] NACo, 2023; Library of Congress, CRS Report

contend that SALT deductibility disproportionately benefits high-income taxpayers and creates distortions and inequities in the federal tax code by favoring certain goods, regions, and income groups and by subsidizing behavior that would occur even in the absence of such incentives.[6]

Although originally scheduled to expire after 2025, the cap was recently increased to $40,000 for tax years 2025–29 under the OBBBA, and its potential detrimental fiscal effects remain central to an ongoing policy debate.

3 Data

This paper draws on county-level data from the Government Finance Database (Pierson et al. 2015) for fiscal outcomes and from IRS-SOI data to measure exposure to the SALT deduction cap. Property tax revenues and public expenditures are expressed in thousands of nominal dollars per capita and constitute the primary outcomes of interest, reflecting their central role in local public finance and importance for residential sorting.[7] The analysis is conducted at the county level, consistent with prior literature (Coyne 2017) and motivated by the prominent role counties played in advocating for the restoration of SALT deductibility. In addition to property tax revenue, the paper examines four key expenditure categories: health, education, public welfare, and capital outlays investment.

I control for a rich set of county-level covariates: log GDP, log employment, male share, share married, population shares under age 18 and over 65, share of adults with graduate or professional degrees, share earning above $75,000, and share above 150 percent of the federal poverty line. GDP data come from the Bureau of Economic Analysis (BEA) Regional Accounts; labor and income data from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics; and demographic characteristics from the American Community Survey. Some specifications also include political and geographic controls: 2016 presidential vote shares from the MIT Election Lab and rurality indicators from the US Department of Agriculture (USDA) Economic Research Service.

[6] Congressional Budget Office, 2017

[7] On average, roughly 70 percent of county revenues come from own-sources, with taxes comprising the majority of that category and property taxes accounting for roughly three-quarters of local tax revenues (see Appendix B).

To address extreme values and to meet the implicit linearity assumptions of continuous treatment designs, I restrict the sample to counties with average SALT deductions below $25,000 in 2017.[8] The final balanced panel includes 1,135 counties (5-digit FIPS codes) present in all survey years from 2012 to 2020 and preserves nearly 88 percent of the population.[9]

I construct three measures of exposure to the SALT cap. First, I define a continuous treatment variable as the average SALT deduction per filer in county c in 2017, equivalent to the IRS-SOI variable ratio (a01800/n01800):[10]

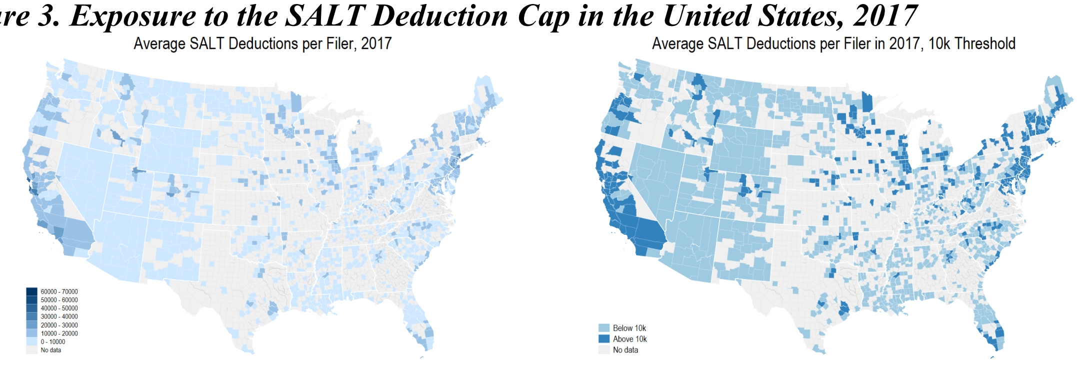
$$\text{SALTcont}_c^{2017} = \frac{\sum_{i=1}^{N_c^{2017}} \text{SALTpaid}_{i,c}^{2017}}{N_c^{2017}} \tag{1}$$

Second, I construct a binary treatment indicator based on the $10,000 threshold, where counties above the threshold are considered treated and those below serve as controls:

$$\text{SALTbi}_c^{2017} = 1\left[\text{SALTcont}_c^{2017} > 10{,}000\right] \tag{2}$$

Figure 3 maps the geographical exposure to the SALT cap, where California and New York alone accounted for over 30 percent of total SALT deductions.

Figure 3. Exposure to the SALT Deduction Cap in the United States, 2017



(a) Continuous Measurement *(b) Binary Measurement*

Source: Author's calculations using IRS Statistics of Income (SOI) data
Notes: Local exposure to the SALT deduction cap in 2017 for a balanced panel of 1,135 counties varied widely

[8] This restriction is motivated by visual diagnostics and methodological recommendations from Callaway et al. (2024), who emphasize the importance of common support in continuous treatment settings. As shown in Appendix A, the relationship between SALT deductions and changes in property tax revenue is approximately linear up to the $25,000 threshold. Beyond that point, the pattern becomes irregular, and the number of observations drops sharply. Trimming the sample improves the credibility of a linear identification strategy while preserving most of the variation in treatment intensity.

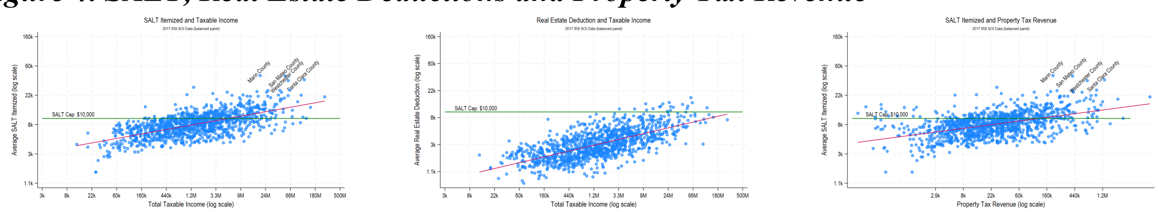
[9] The sample excludes Hawaii.

[10] Results are robust to using pre-TCJA averages or earlier years.

across the United States. Panel (a) displays a continuous measure of exposure based on average SALT deductions per filer, highlighting substantial within-state variation, even in relatively low-tax regions. Panel (b) shows a binary classification of counties according to whether their average deduction exceeded the $10,000 cap. High-exposure counties are heavily concentrated in coastal and high-tax states such as California, New York, and New Jersey. This geographic variation supports the use of both continuous and binary pre-reform SALT deduction levels as credible measures of treatment intensity in the empirical strategy.

Although millions of taxpayers mechanically shifted to the standard deduction after the introduction of the SALT cap, its fiscal incidence was uneven across jurisdictions. Figure 4 shows how counties with higher average SALT deductions tend to have higher taxable income and rely more on property taxes, indicating that only a limited subset of jurisdictions was meaningfully exposed to the cap, particularly when restricting attention to real estate (RE) deductions.[11] Consistent with this pattern, counties above the $10,000 SALT threshold in 2017 exhibited larger local economies (as measured by GDP and employment) and more educated populations. They were also more urban and more likely to have supported the Democratic candidate in the 2016 presidential election.[12]

Figure 4. SALT, Real Estate Deductions and Property Tax Revenue



(a) SALT Deductions and Taxable Income | *(b) RE Deductions and Taxable Income* | *(c) SALT Deductions and Property Tax Revenue*

Source: Author's calculations using IRS Statistics of Income (SOI) and the Government Finance Database (Pierson et al. 2015)

Notes: This figure examines the cross-sectional relationship between the average amount of SALT deductions per itemizer, real estate (RE) deductions, total taxable income, and property tax revenue in 2017. Axes are displayed in logarithmic scale. Panel (a) shows a positive correlation between average SALT deductions and taxable income. The horizontal green line indicates the $10,000 cap imposed by the Tax Cuts and Jobs Act (TCJA), with many affluent counties clustered above this threshold. Panel (b) presents a similar correlation between RE deductions and taxable income, though most counties fall below the cap. Panel (c) shows a positive correlation between average SALT deductions and property tax revenue.

Because these patterns suggest that the cap's fiscal effects were likely concentrated in affluent, high-tax jurisdictions, I construct a measure of treatment intensity. Specifically, I define θ_c as the proportional excess of average SALT deductions above the $10,000 cap in 2017, setting the

[11] According to IRS-SOI data, in 2017 taxpayers with adjusted gross incomes above $100,000 accounted for 48 percent of all SALT itemizers and received nearly 78 percent of total SALT deductions. Among taxpayers earning over $200,000, 93 percent itemized deductions, with SALT averaging 7.78 percent of AGI.

[12] See Appendix C for descriptive statistics for the binary treatment variable and the distributions of covariates.

measure equal to zero for counties below the threshold. The main analysis focuses on this intensity specification, which most directly captures the binding nature of the policy[13]:

$$\theta_c^{2017} = \frac{\text{SALTcont}_c^{2017}}{10{,}000} - 1 \quad \text{for} \quad \text{SALTbi}_c^{2017} = 1 \tag{3}$$

Table 1 reports descriptive statistics for property tax revenue and direct general expenditure per capita before and after the enactment of the TCJA for the continuous treatment variables.[14] In the full sample, average per capita property tax revenue increased from 0.394 to 0.436 (thousands of nominal dollars), a statistically significant rise of 0.042. Direct general expenditure also increased in the post-reform period, from 1.418 to 1.493 per capita, with a statistically significant difference of 0.076. Among the most exposed counties, the increases were larger. Property tax revenue rose from 0.426 to 0.491 per capita, while direct general expenditure increased from 1.445 to 1.531. In both cases, the pre–post differences are statistically significant at conventional levels.

Table 1. Pre- and Post-TCJA Fiscal Outcomes (per capita)

	Mean	**Std. dev.**	**Mean, pre**	**Mean, post**	**Difference**	**t-statistic**
Full Sample						
Property Tax Revenue	0.408	0.448	0.394	0.436	0.042	4.48***
Direct General Expenditure	1.443	1.419	1.418	1.493	0.076	2.53**
Observations			6,829	3,407		
SALT > $10k Sample						
Property Tax Revenue	0.448	0.421	0.426	0.491	0.065	3.91***
Direct General Expenditure	1.472	1.388	1.445	1.531	0.086	2.80**
Observations			1,903	966		

Source: Author's calculations using the Government Finance Database (Pierson et al. 2015)
Notes: This table reports descriptive statistics for property tax revenue and direct general expenditure per capita (in thousands of nominal dollars) before and after the TCJA. The high-exposure sample includes counties with average SALT deductions per filer above $10,000 in 2017 (SALT > $10,000). The difference column reports mean(post) minus mean(pre). Standard t-tests assume equal variances. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

These unconditional means comparisons show no evidence of fiscal retrenchment following the introduction of the SALT deduction cap. If anything, fiscal outcomes improved more in highly exposed jurisdictions, motivating a formal difference-in-differences framework to isolate whether these patterns reflect differential exposure to the cap rather than broader macroeconomic trends.

[13] This reflects only county governments, excluding school districts and other overlapping jurisdictions.
[14] See Appendix C for the descriptive statistics of the binary treatment from equation (2).

4 Empirical Framework

To estimate the impact of the SALT deduction cap on local government fiscal outcomes, I employ a difference-in-differences (DiD) strategy with a continuous treatment, implemented using a two-way fixed effects (TWFE) model and county-level panel data from 2012 to 2020. As illustrated in Figure 2, although the TCJA was enacted in the middle of FY 2018, making FY 2018 the pre-treatment budget year from an institutional perspective, the event-study specifications normalize all coefficients relative to calendar year 2017 (event time -1). This choice follows standard practice and guards against potential anticipation effects. The DiD design exploits cross-sectional variation in pre-treatment exposure to the SALT cap measured at the county level and compares changes in fiscal outcomes before and after the reform across counties with different levels of exposure.

4.1 Continuous Difference-in-Differences

The main analysis is based on the treatment intensity specification (restricted to counties above the cap) as follows:

$$y_{c,t} = \beta(Post_t \times \theta_c^{2017}) + \gamma X_{c,t} + \alpha_{s,t} + \varepsilon_{c,t} \quad (4)$$

Where, $y_{c,t}$ is the outcome of interest (e.g., per capita revenue or expenditure) for county c in year t. The coefficient β captures the differential post-reform change in fiscal outcomes associated with higher SALT exposure and can be interpreted as the average causal response (ACR).[15] All specifications include state-by-year fixed effects ($\alpha_{s,t}$) to account for differential macroeconomic trends across states that could otherwise generate pre-trends between high-income treated states (e.g., California and New York) and lower-income states. Standard errors are clustered at the county level.

The vector of controls, $X_{c,t}$, includes time-varying county-level covariates not absorbed by the fixed effects. These include business cycle indicators (GDP and employment growth) and

[15] As discussed by Callaway et al. (2024), DiD designs with continuous treatments require a more restrictive identification condition, which the authors term the strong parallel trends assumption. Because this assumption cannot be tested directly, they recommend evaluating the functional form imposed on the treatment intensity, as misspecification may distort estimated effects. I present this diagnostic exercise in Appendix A. The results support the appropriateness of the linear specification.

demographic characteristics such as marriage rates, age structure, educational attainment, income distribution, and poverty rates.

4.2 Non-parametric Event Study

To assess the validity of the identifying assumptions, particularly parallel trends, I estimate an event study specification based on treatment intensity:

$$y_{c,t} = \sum_{\tau \neq 2017} \beta_\tau \left(\theta_c^{2017} \times D_{\tau,t}\right) + \gamma X_{c,t} + \alpha_{s,t} + \varepsilon_{c,t} \tag{5}$$

Here, $D_{\tau,t}$ is an indicator for year τ, and 2017 is the omitted (baseline) period. The coefficients β_τ trace the dynamic effect of the SALT cap across counties with varying degrees of exposure. Under the parallel trends assumption, the pre-treatment coefficients ($\tau < 2017$) should not differ statistically from zero, while post-treatment divergence ($\tau > 2017$) would suggest a causal effect of the reform.

4.3 Parametric Event Study

In order to control for potential influence of secular trends in outcomes before the TCJA, I follow (Dobkin et al. 2018) and propose a parametric event study for event-time trends as follows:

$$y_{c,t} = \delta(\theta_c^{2017} \times \tau_t) + \sum_{\tau \geq 0} \beta'_\tau \left(\theta_c^{2017} \times 1\{\tau_t = \tau\}\right) + \gamma' X_{c,t} + \alpha'_{s,t} + \varepsilon'_{c,t} \tag{6}$$

Where $\tau_t = t - 2018$ is the event time (so $\tau = -1$ is 2017, $\tau = 0$ is 2018, $\tau = 1$ is 2019, etc.), and δ captures the linear pre-trend in event time, scaled by exposure. β'_τ captures post-SALT deviations from that linear trend for $\tau = 0,1,2$. The event studies presented in Figure 5, superimpose the estimated parametric event study on the non-parametric event study coefficients following (Freyaldenhoven, C. B. Hansen, et al. 2021; Freyaldenhoven, C. Hansen, et al. 2021).

5 Results

5.1 Property Tax Revenue and Direct General Expenditure

Table 2 reports DiD estimates of the effect of the SALT deduction cap on per capita property tax revenue and direct general expenditure at the county level. The estimates are based on the treatment intensity measure from equation (3), defined as the interaction between a post-2017 indicator and θ_c^{2017}, which measures the proportional excess of average SALT deductions above the $10,000 cap in 2017.[16] Specification (1) only includes state-by-year fixed effects, while specification (2) adds the set of demographic and business cycle controls. To address potential confounders that also affect fiscal outcomes, in specification (3), I estimate extended specifications that control for pre-TCJA housing price growth (2012–17), the 2016 Democratic vote share, pre-TCJA AMT incidence, and urban status.[17]

Table 2. Effects of the SALT Deduction Cap on Local Fiscal Outcomes (per capita)

	(1)	**(2)**	**(3)**
2-4			
Panel A: Property Tax Revenue			
Post × θ, SALT > \$10k	0.0576***	0.0351**	0.0418*
	(0.0127)	(0.0172)	(0.0241)
Observations	2,790	2,790	2,664
R^2	0.820	0.855	0.864
Panel B: Direct General Expenditure			
Post × θ, SALT > \$10k	0.0347	0.00150	0.0347
	(0.0480)	(0.0543)	(0.0572)
Observations	2,790	2,790	2,664
R^2	0.739	0.770	0.795
State × year fixed effects	✓	✓	✓
Time-varying controls		✓	✓
SALT-context controls			✓

Notes: Difference-in-differences estimates of the effect of the SALT deduction cap on county-level per-capita fiscal outcomes. The treatment variable is the interaction of a post-2017 indicator with a continuous measure of exposure, θ, defined as the proportional excess of average SALT deductions above the $10,000 cap in 2017. All specifications include state-by-year fixed effects, with standard errors clustered at the county level. Time-varying controls include economic and demographic characteristics; SALT-context controls include urban status, Democratic vote share (2016), pre-period house price growth, and AMT exposure. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Across specifications, I find no evidence that counties more exposed to the SALT deduction cap experienced economically meaningful or robust declines in public expenditures. Estimates for property tax revenues instead point to modest increases, which attenuate once richer controls are introduced.[18] Next, I assess the identifying assumptions underlying the DiD framework using my

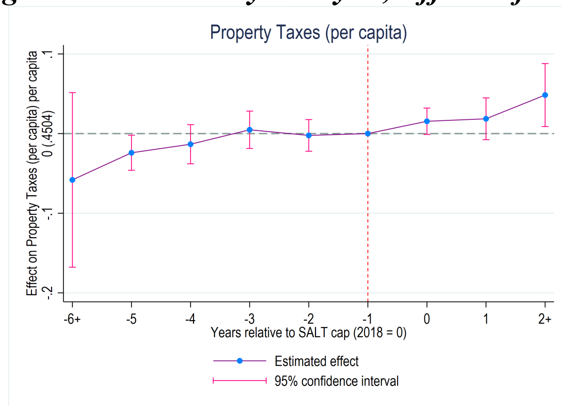
[16] Results for equation (1) and for equation (2) are reported in Appendix E.

[17] To assess whether housing price growth drives the main results, a balance test for pre-treatment housing price trends is reported in Appendix F

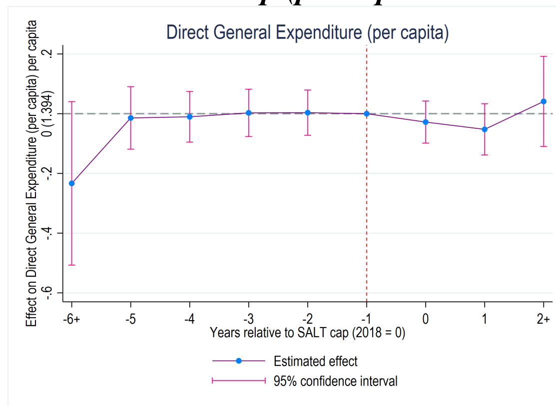
[18] Appendix G reports additional robustness checks using alternative combinations of state, county, and year fixed effects, conditioning on pre-treatment covariates, excluding FY 2020, and allowing for county-specific pre-trends.

preferred specification (column 2), examining pre-trends and dynamic responses to evaluate the plausibility of the parallel trends assumption.

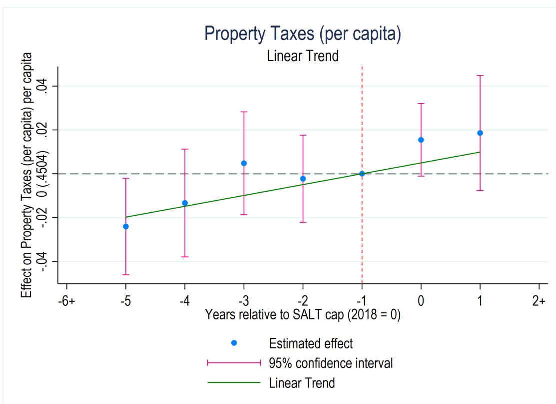
Figure 5. Event Study Analysis, Effects of the SALT Deduction Cap (per capita outcomes)



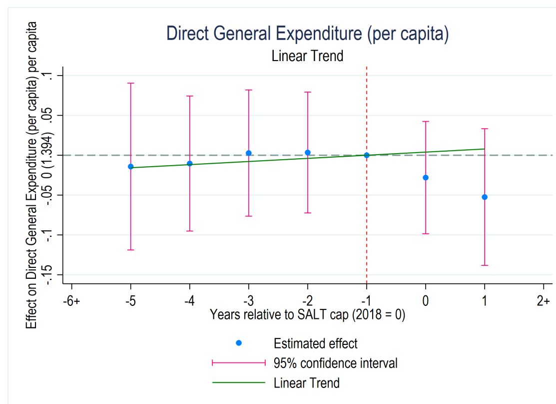
(a) Property Taxes: Base



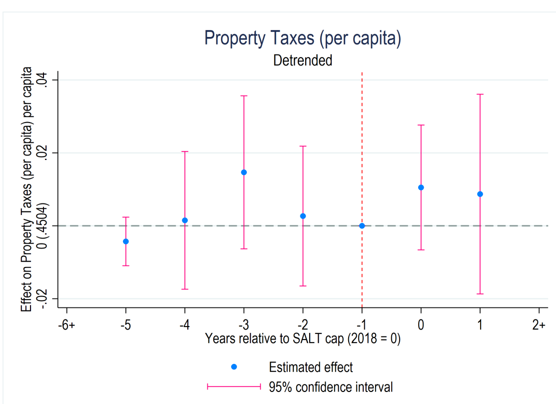
(b) Direct Expenditure: Base



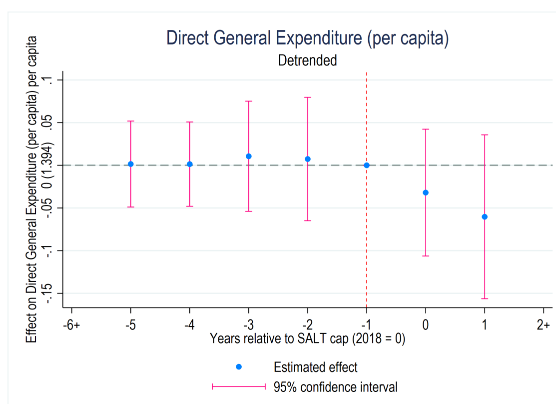
(c) Property Taxes: Linear Trend



(d) Direct Expenditure: Linear Trend



(e) Property Taxes: Detrended



(f) Direct Expenditure: Detrended

Source: Author's calculations using IRS Statistics of Income (SOI); the Government Finance Database (Pierson et al. 2015).
Notes: Each panel presents event study estimates of the effect of the SALT deduction cap on local fiscal outcomes

per capita. The top row shows baseline event studies without trend adjustment. The middle row overlays the estimated linear pre-trend. The bottom row presents detrended estimates where county-specific linear trends estimated from pre-treatment years (2013–17) have been removed. Dashed vertical lines indicate the policy implementation year (2018). Shaded areas represent 95 percent confidence intervals with standard errors clustered at the county level.

Figure 5 presents event-study estimates from equations (7) and (8), which interact treatment intensity with year fixed effects. The dynamics in panels (a) and (b) are consistent with the identifying assumptions of the design. Pre-2017 coefficients are small in magnitude, fluctuate around zero, and are jointly indistinguishable from zero. A formal pre-trends F-test yields $F(5{,}313) = 1.52$ with $p = 0.19$, providing no evidence of differential trends across treatment intensities prior to the reform. A complementary leveling-off test for the 2012–16 period also fails to reject equality ($p = 0.09$), reinforcing the conclusion that more- and less-exposed counties followed similar fiscal trajectories before the TCJA.

While point estimates for property tax revenues become positive in the post-treatment period, particularly in later years, these effects attenuate under more conservative detrending strategies. Motivated by an apparent mild upward pre-trends in panel (a), I conduct an additional robustness exercise in which, excluding the endpoints, I extrapolate the linear trend from equation (8), superimpose it on the baseline coefficients (panels c and d), and subtract it from the post-treatment estimates. As shown in panels (e) and (f) of Figure 5, the positive property tax effect disappears. Importantly, even under this conservative adjustment, there is still no evidence of a negative impact on local revenues or expenditures.

Finally, following Rambachan and Roth (2023), I assess the sensitivity of the estimates to violations of the parallel trends assumption. As shown in Figure 6, confidence intervals widen as increasingly large nonlinear deviations from parallel trends are allowed, as expected. Nevertheless, the estimates remain sufficiently precise to rule out economically meaningful revenue declines. Scaling the continuous treatment by the interquartile range of exposure, the 95 percent confidence interval for an interquartile comparison ranges from roughly 0.1 to 4.7 percent of mean property-tax revenue.[19] The implied minimum detectable effect for this contrast is approximately 2.3 percent of mean revenue.

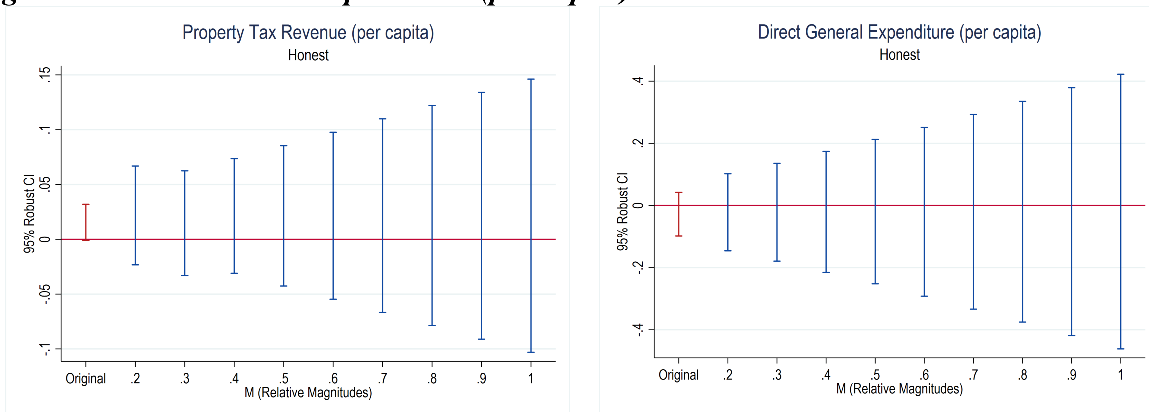
[19] A county at the 75th percentile of SALT exposure ($\theta = 0.41$) differs from a county at the 25th percentile ($\theta = 0.08$) by $\Delta\theta \approx 0.33$.

To benchmark these magnitudes, I compute the distribution of year-to-year changes in property-tax revenue in the pre-reform period. Between 2012 and 2017, the mean absolute annual change in per-capita revenue in the estimation sample is about 5 percent. Under the most conservative robustness scenario ($M = 1$), which allows post-treatment trend deviations as large as the largest pre-treatment imbalance, the data would allow revenue declines of up to about 21 percent of mean revenue. This corresponds to more than four times the typical yearly fluctuation. In other words, only revenue contractions far outside the range of normal historical variation would be observed in the data, and under weak identifying assumptions.

These magnitudes are also large relative to historical experience during major macroeconomic shocks. Evidence from the Great Recession shows that, despite substantial declines in housing values, property-tax revenues in most states continued to grow or declined only modestly, reflecting assessment lags and millage-rate adjustments that stabilized collections (Alm et al. 2011). Revenue contractions of the size permitted only under the most conservative robustness scenario would therefore be atypical even in the presence of a shock far more severe than the SALT reform.

A similar exercise for direct general expenditure yields comparable conclusions. The baseline confidence interval rules out expenditure reductions larger than roughly 6–7 percent of mean per-capita spending. In the pre-reform period, the median year-to-year change in expenditure is about 4.5 percent and the mean absolute change is approximately 10 percent. Under the same conservative robustness scenario, the data allow declines of up to roughly 30 percent of mean spending; however, such contractions are about three times the typical annual fluctuation.

Figure 6. Direct General Expenditure (per capita)



(a) Property Tax (per capita) *(b) Direct General Expenditure (per capita)*

Source: Author's calculations using IRS Statistics of Income (SOI) and the Government Finance Database (Pierson et al. 2015)
Notes: Sensitivity analysis performed with the command honestdid, following (Rambachan and Roth 2023).

5.2 Specific Service Provision and Non-deductible Taxes

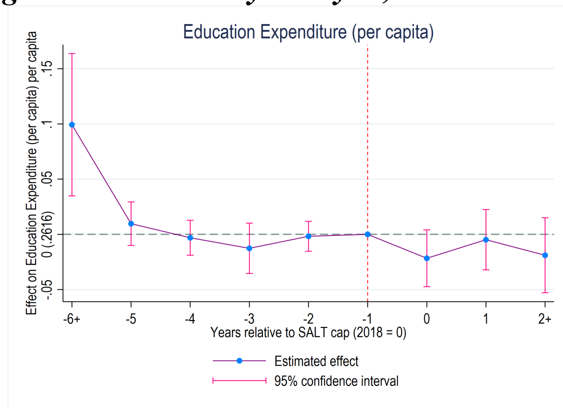
Beyond aggregate revenues and expenditures, I examine whether the SALT deduction cap affected specific components of local government finance. Contrary to concerns raised during the policy debate, there is no evidence of reductions in spending on essential services such as health, education, and public welfare, nor in investment through capital outlays (Figure 7). Another common hypothesis is that local governments might respond to the SALT deduction cap by shifting toward non-deductible revenue sources, such as user fees or charges, that are unaffected by the federal cap (Holtz-Eakin and Rosen 1988; Metcalf 2008). Event-study estimates for charges and miscellaneous revenues, utility revenues, and license taxes show no significant post-reform changes (Figure 8).

Conditional on the identification strategy employed in this paper, counties with greater exposure to the SALT cap tend to be characterized by larger tax bases, higher incomes, and greater fiscal capacity. These characteristics may help explain why such jurisdictions were able to absorb changes in federal tax incentives without experiencing the adverse fiscal outcomes predicted in the public debate. More broadly, these findings are consistent with a willingness among residents to sustain local service levels, at least in the short run, despite a reduction in the federal subsidy. This pattern aligns with relatively inelastic demand for local public goods in high-income jurisdictions, where such goods often exhibit club-like characteristics rather than those of pure public goods (Banzhaf 2013).

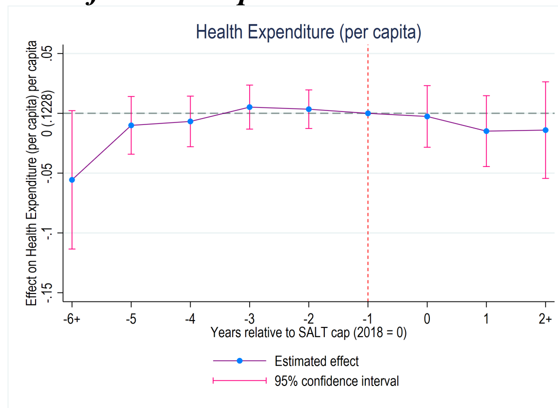
The absence of substitution away from deductible revenue sources, particularly property taxes, reinforces the institutional centrality of this tax instrument in local public finance (Alm et al. 2011). This stability is notable in light of long-standing debates in the fiscal federalism literature regarding the responsiveness of subnational tax structures to federal policy changes. The continued reliance on property taxation suggests that local governments may face political and administrative constraints that limit major shifts in revenue composition, or alternatively, that such adjustments are unnecessary when voter support for public spending remains strong.

Finally, these findings support an interpretation of local governments as capable of maintaining revenue adequacy and public service provision in the face of federal policy shocks. At the same time, they raise questions about the rationale for federal subsidies to local tax burdens through mechanisms such as the SALT deduction, which may function less as incentives for efficient fiscal behavior and more as regressive transfers concentrated in high-income jurisdictions.

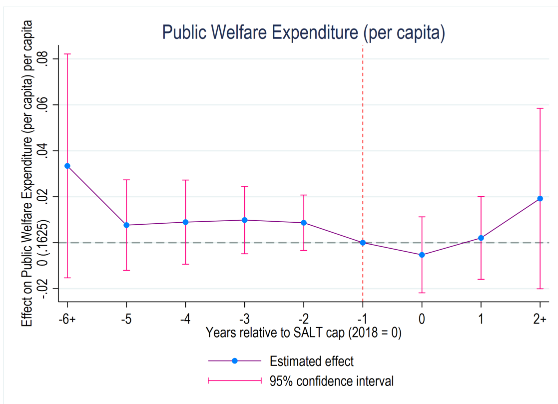
Figure 7. Event Study Analysis, Selected Categories of Public Expenditure



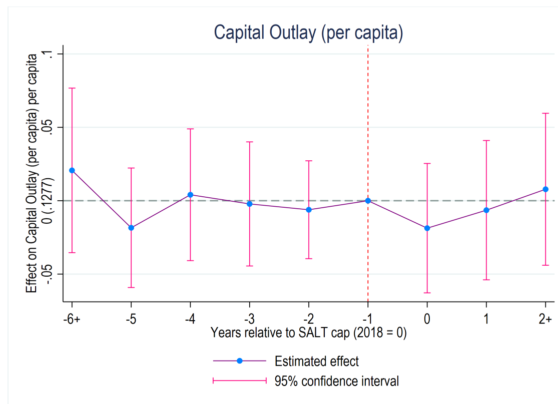
(a) Education



(b) Health



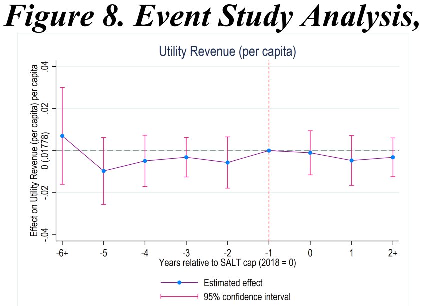
(c) Public Welfare



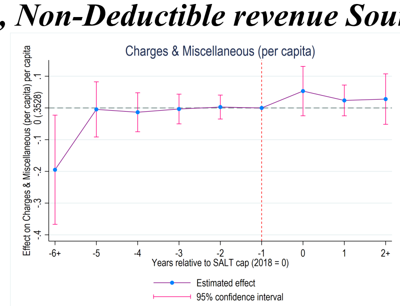
(d) Capital Outlay

Source: Author's calculations using IRS Statistics of Income (SOI); the Government Finance Database (Pierson et al. 2015), BEA Regional Accounts; BLS Local Area Unemployment Statistics; American Community Survey.
Notes: This figure shows event study estimates the impact of the SALT deduction cap on various categories of local government expenditures, using the preferred specification.

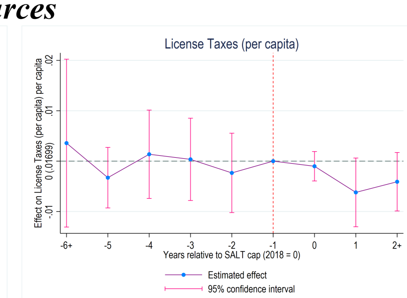
Figure 8. Event Study Analysis, Non-Deductible revenue Sources



(a) Utility Revenue



(b) Charges and Miscellaneous



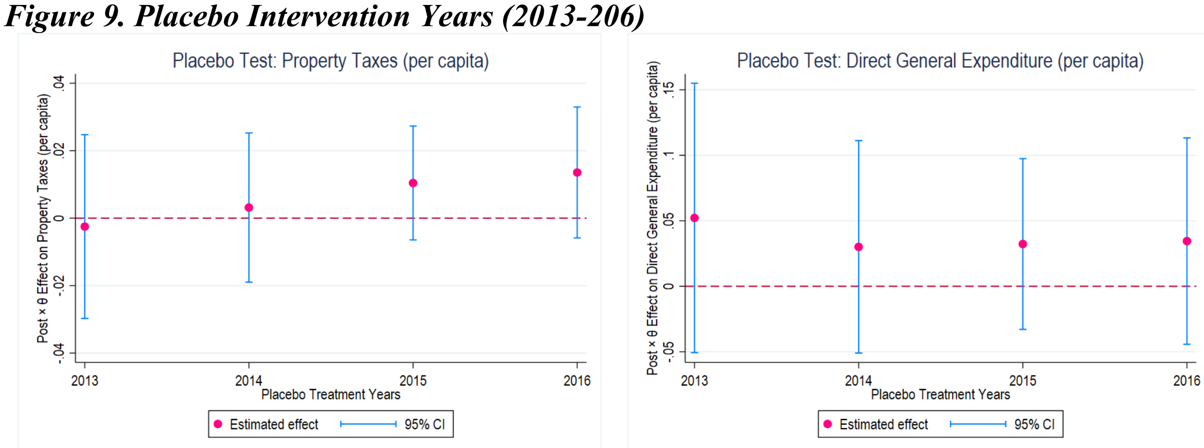
(c) Licenses

Source: Author's calculations using IRS Statistics of Income (SOI); Government Finance Database (Pierson et al. 2015); BEA Regional Accounts; BLS Local Area Unemployment Statistics; American Community Survey.
Notes: This figure reports event-study estimates of the effects of the SALT deduction cap on non-deductible local revenue sources. Vertical bars represent 95 percent confidence intervals.

6 Robustness

To assess the robustness of the main findings, I conduct a series of additional analyses, including placebo tests using alternative treatment dates prior to the TCJA, alternative outcome specifications, and sensitivity checks related to the composition of counties exposed to the SALT deduction cap. Figure 9 reports DiD estimates based on the preferred specification, but using pre-TCJA years as placebo intervention dates. Across all placebo exercises, the estimated effects are small and statistically insignificant, providing no evidence of spurious treatment effects prior to the reform and supporting the validity of the main specification. Figure 10 presents event-study estimates based on the preferred specification using the logarithm of the outcome variables rather than per capita measures. The direction and timing of the results remain unchanged: there is no evidence of detrimental effects on local public finances following the SALT deduction cap.

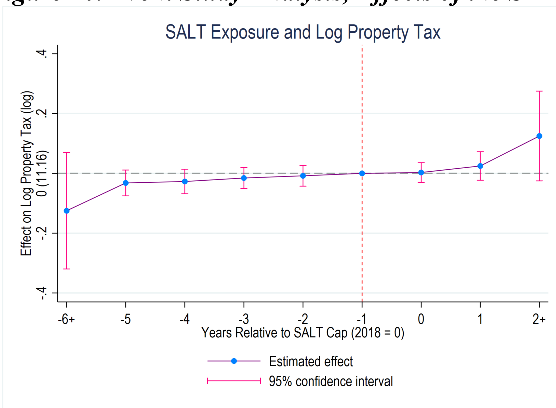
Figure 9. Placebo Intervention Years (2013-206)



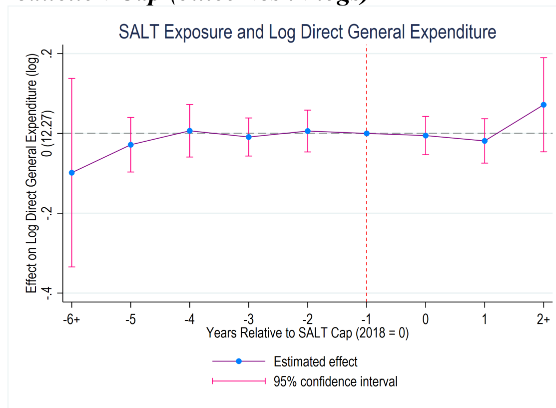
(a) Property Tax (per capita) *(b) Direct Expenditure (per capita)*

Source: Author's calculations using IRS Statistics of Income (SOI) and the Government Finance Database (Pierson et al. 2015)
Notes: This figure plots placebo estimates from DiD regressions using alternative pre-TCJA years as intervention dates. Regressions include year and state fixed effects, as well as controls for log GDP, log employment, and demographics.

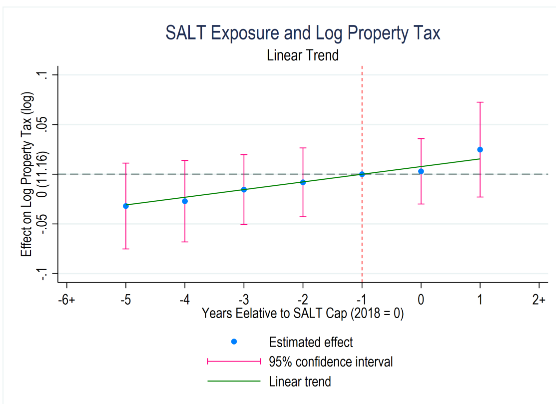
Figure 10. Event Study Analysis, Effects of the SALT Deduction Cap (outcomes in logs)



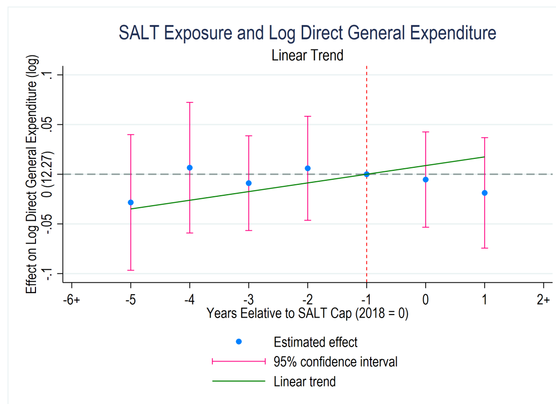
(a) Property Taxes: Base



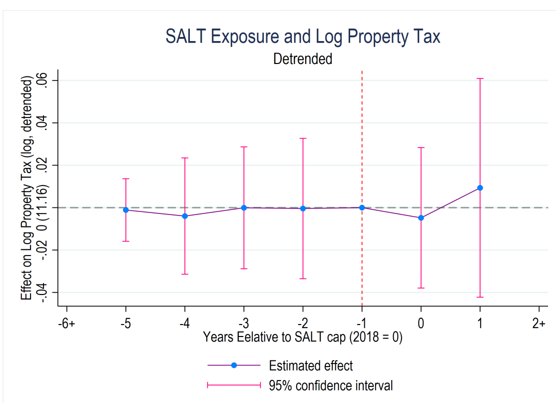
(b) Direct Expenditure: Base



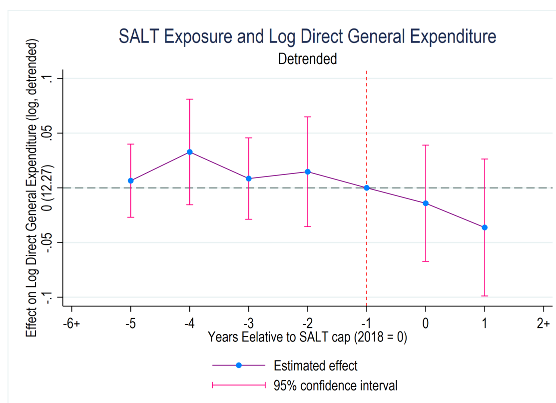
(c) Property Taxes: Linear Trend



(d) Direct Expenditure: Linear Trend



(e) Property Taxes: Detrended



(f) Direct Expenditure: Detrended

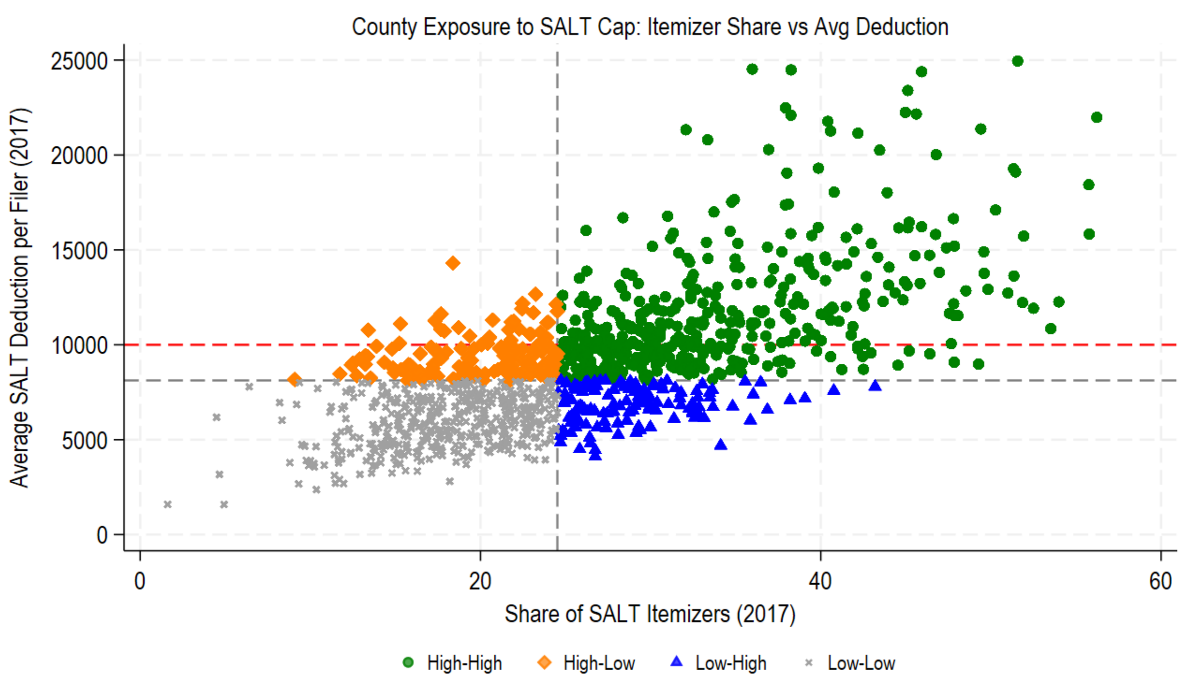
Source: Author's calculations using IRS Statistics of Income (SOI); the Government Finance Database (Pierson et al. 2015).
Notes: Each panel presents event study estimates of the effect of the SALT deduction cap on local fiscal outcomes per capita. The top row shows baseline event studies without trend adjustment. The middle row overlays the estimated linear pre-trend. The bottom row presents detrended estimates where county-specific linear trends estimated from pre-treatment years (2013–17) have been removed. Dashed vertical lines indicate the policy

implementation year (2018). Shaded areas represent 95 percent confidence intervals with standard errors clustered at the county level.

A remaining concern is whether the results could be driven by counties with high average SALT deductions but relatively low shares of affected taxpayers. To address this possibility, I classify counties into four exposure quadrants based on average SALT deductions and itemization rates (Figure 11). I then restrict the analysis to the "High–High" group, consisting of counties with both high average deductions and high itemization rates, which are most clearly exposed to the cap. As shown in Figure 12, even within this more selective sample, the SALT deduction cap is not associated with statistically significant reductions in per capita property tax revenue or public expenditure.

Taken together, these robustness exercises reinforce the central conclusion of the paper. While the SALT deduction cap increased the effective tax price of local revenues, local fiscal responses were not mechanically negative. Instead, the evidence suggests that local governments adjusted within the constraints and opportunities of their intergovernmental environment rather than behaving as passive recipients of changes in taxpayer behavior.

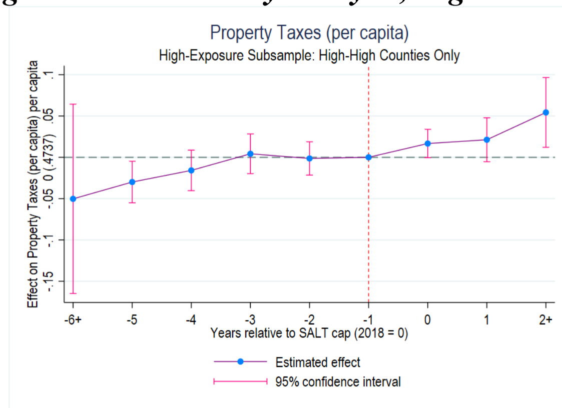
Figure 11. Itemized and Itemizers Quadrants



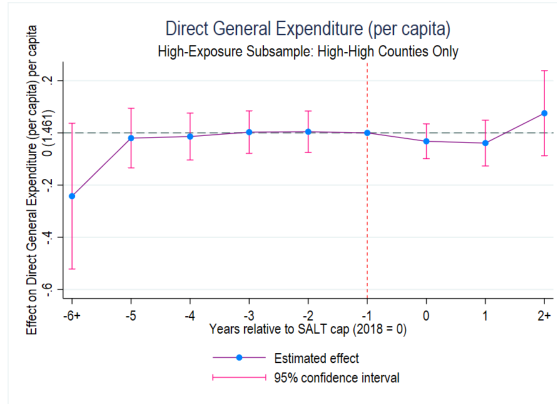
Source: Author's elaboration using IRS Statistics of Income (SOI).
Notes: This figure classifies counties into four groups using 2017 data on average SALT deductions and itemization

rates. High-High counties (green) exceed both the $10,000 threshold and the median itemizer share. These are the jurisdictions most likely to have been affected by the cap.

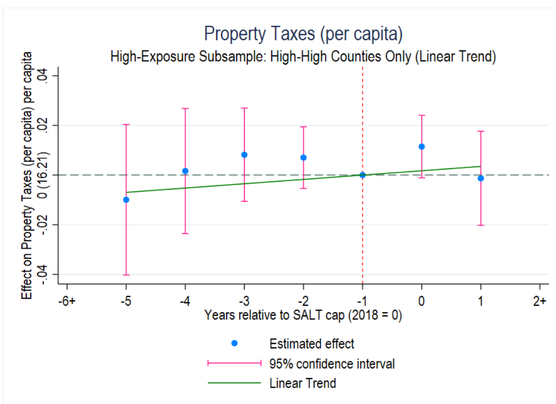
Figure 12. Event Study Analysis, High SALT Deduction and Itemization Rate Counties



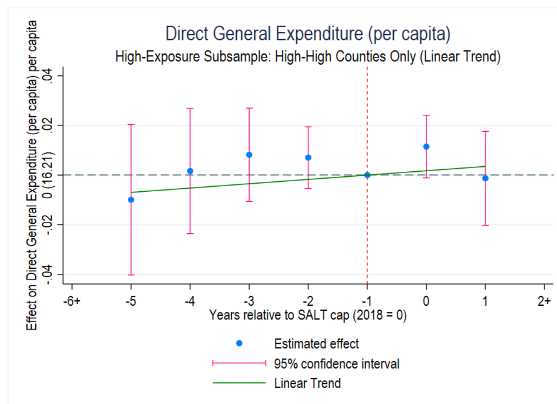
(a) Property Taxes: Base



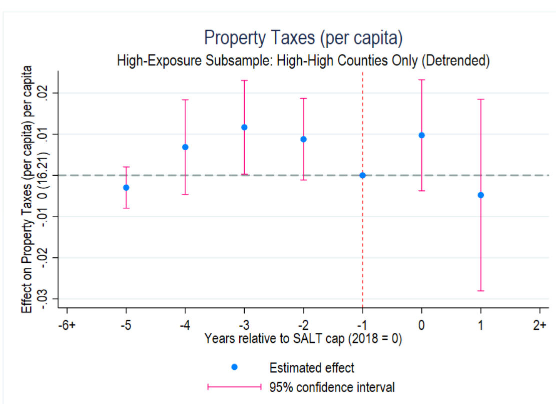
(b) Direct Expenditure: Base



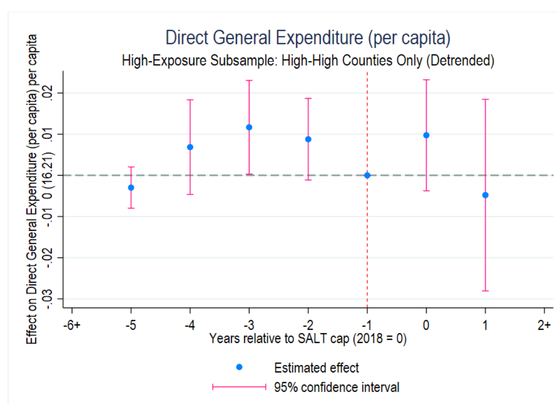
(c) Property Taxes: Linear Trend



(d) Direct Expenditure: Linear Trend



(e) Property Taxes: Detrended



(f) Direct Expenditure: Detrended

Source: Author's calculations using IRS Statistics of Income (SOI); the Government Finance Database (Pierson et al. 2015).
Notes: This figure shows event study results for the High Deduction-High itemizers counties.

7 Mechanism Analysis

A central argument underlying concerns about the potential detrimental effects of the SALT deduction cap on local public finances, often motivated by the fiscal competition literature, is that local governments may face pressure to reduce tax rates in order to offset higher effective tax burdens borne by their residents, thereby eroding their own revenue base.[20]

In principle, changes in property tax revenue may arise through two main channels: adjustments in statutory tax rates or changes in assessments that expand or contract the property tax base. Empirically, however, neither channel is straightforward to observe. Statutory property tax rates are not systematically reported at the county level across the Nation, and assessment data are often fragmented, inconsistent, or unavailable. While several efforts have been made to compile property tax rate data at the national level, most notably the Lincoln Institute of Land Policy platform, these data are not fully harmonized and are not directly suitable for comparative analysis across jurisdictions.[21]

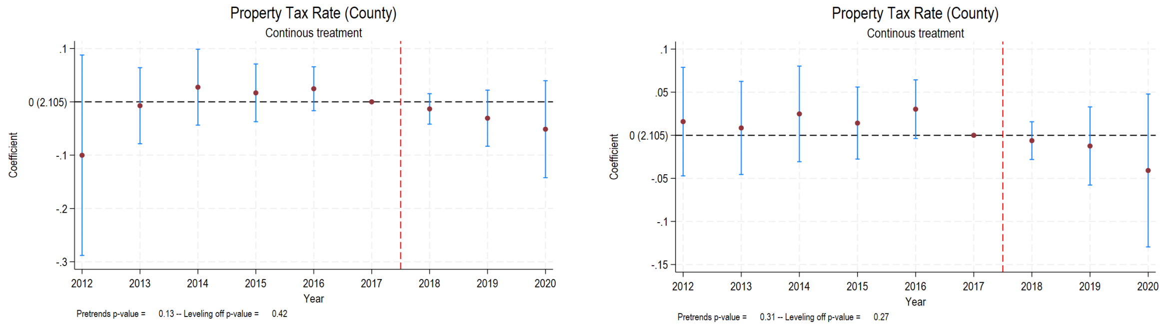
Recent work has made progress in addressing these limitations (Brosy and Ferrero 2025; Baker et al. 2024; Horton et al. 2024), however, coverage remains limited. In Baker's data, only nine states have consistent information on statutory tax rates at the county level spanning the period relevant for this analysis. Event-study estimates based on this restricted sample, reported in Figure 13, show no evidence of changes in property tax rates following 2017.[22]

[20] When jurisdictions set taxes and expenditures independently, fiscal competition may arise as governments compete to attract mobile residents and capital by offering appealing combinations of taxes and public goods (Janeba and Wilson 2011; Barker 2019; Klick and Parisi 2005). Mobility of the tax base is central to this mechanism (Agrawal et al. 2022), and policy-induced changes in one jurisdiction can generate fiscal externalities for others (Keen and Kotsogiannis 2002; Timothy J. Besley and Harvey S. Rosen 1998; Madiès 2008; Agrawal 2016, 2015).

[21] Lincoln Institute of Land Policy.

[22] As a robustness check, I construct an effective property tax rate using ACS data, similar to the Tax Foundation approach (Yushkov 2023), defined as self-reported real estate taxes paid divided by reported home value. While this measure is inherently noisy and does not correspond to statutory millage rates, event-study estimates similarly show no significant post-reform effects (Appendix H).

Figure 13. Property Tax Rates



(a) Property Tax Rate, SFE (9 states)

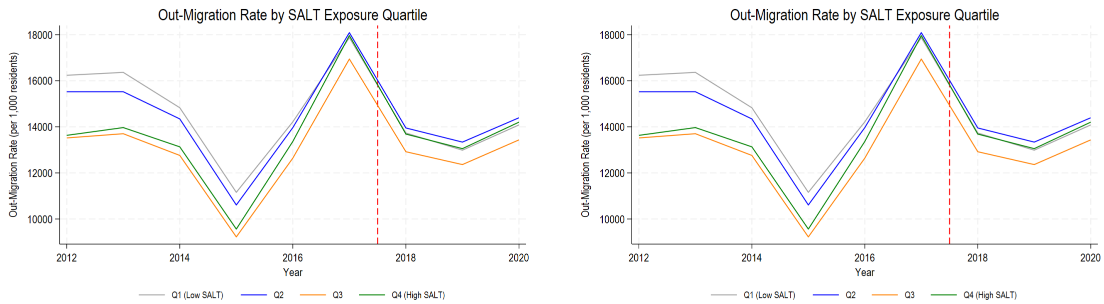
(b) Property Tax Rate, CFE (9 states)

Source: Author's calculations using the panel dataset on tax rates assembled by Baker et al. (2024).
Notes: This figure explores potential mechanisms driving the SALT cap's effects on local government revenues using continuous treatment event study specifications. Panels (a) and (b) show estimates for county-level property tax rates under state and county fixed effects (SFE and CFE). A balanced panel for 2012–20 using these sources yields complete data for only nine states: California, Colorado, Florida, Illinois, Maryland, Nebraska, New York, Ohio, and Utah.

Migration has also been proposed as a potential mechanism through which the SALT deduction cap could negatively affect local public finances, particularly if high-income taxpayers respond to higher effective tax burdens by relocating (Coen-Pirani and Sieg 2019; Rauh 2022; Drukker 2023). I find no evidence of increased out-migration in response to the SALT cap (Figure 14), a result consistent with (Lalumia 2024). That said, the sharp increase in residential mobility following the onset of the COVID-19 pandemic complicates efforts to cleanly isolate SALT-specific migration responses in the post-2020 period.

Finally, it is important to emphasize that the main results in this paper are based on a censored sample for which the continuous treatment approach is plausibly valid. While it is possible that some high-income individuals most responsive to the SALT cap reside in counties with average itemized deductions exceeding $25,000, and therefore lie outside the estimation sample, the policy debate surrounding the SALT deduction cap largely emphasized its potential effects on middle-income households rather than only the very top of the income distribution. In this sense, the absence of revenue losses or fiscal retrenchment in the analyzed sample is directly relevant for assessing the core claims made about the reform's impact on local public finance.

Figure 14. Out-migration



(a) Whole sample *(b) Above $10,000*

Source: Author's calculations using IRS migration data.
Notes: This figure examines migration as a potential mechanism in response to the SALT deduction cap. Panel (a) plots out-migration rates across all counties by SALT exposure quartile, while Panel (b) focuses only on counties where average SALT deductions exceeded $10,000. Across both panels, all groups exhibit a synchronized spike in 2017, likely reflecting broader economic or policy factors unrelated to SALT. Crucially, the highest exposure counties (Q4) do not show a disproportionate or persistent increase in out-migration post-reform.

8 Discussion and Implications

The Joint Committee on Taxation estimates that fully eliminating the SALT deduction would reduce the federal deficit by approximately $1.1 trillion between 2023 and 2032.[23] With the SALT cap modified under the One Big Beautiful Bill Act (OBBBA) and extended through 2029, the findings of this paper are directly relevant to ongoing debates over federal tax policy and its local consequences.

Contrary to concerns raised at the time of the TCJA, the SALT deduction cap did not generate fiscal distress in the jurisdictions most exposed to the reform. Instead, the evidence points to the adaptive capacity of local governments: counties sustained fiscal effort, maintained property taxes as their primary revenue source, and avoided widespread expenditure retrenchment or substitution toward alternative revenue instruments. Adjustment appears to have occurred without evidence of broad-based taxpayer flight or a "race to the bottom" in local tax rates.

Several factors help explain these results and reconcile them with the existing literature. First, this paper documents short-run responses to the SALT cap. Property tax revenues are relatively stable over the business cycle and do not adjust as rapidly as other revenue sources, reflecting a form of fiscal "stickiness." This institutional rigidity is consistent with the long-standing view of the property tax as a resilient and reliable source of local revenue. As a result, even a sudden increase

[23] Congressional Budget Office, 2023

in the perceived tax price induced by the loss of federal deductibility need not translate into immediate declines in collections.

Second, concerns about broad-based negative effects on middle-income homeowners appear overstated. The SALT cap primarily affected a small fraction of taxpayers at the top of the income distribution, concentrated in high-tax jurisdictions. These taxpayers typically enjoy substantial purchasing power and reside in communities with strong local fiscal capacity. In such jurisdictions demand for local public services, many of which exhibit club-good characteristics, is likely relatively inelastic. This combination of high willingness to pay and strong fiscal capacity helps explain why the most exposed counties were able to maintain local fiscal effort despite the reduction in the federal subsidy implicit in deductibility, reinforcing their continued reliance on property tax revenues.

Third, many theoretical predictions and simulation-based analyses consider changes in federal deductibility in isolation. In practice, the SALT cap was introduced as part of a broader reform package under the TCJA, which included several provisions that may have partially offset its effects. Reductions in marginal federal income tax rates generated income effects that could counteract the substitution effect associated with a higher local tax price, potentially leaving high-income taxpayers better off on net. In addition, the Alternative Minimum Tax had already limited the value of deductions for many high-income households prior to the TCJA. Most importantly, the doubling of the standard deduction altered itemization behavior, pushing many taxpayers who previously itemized into the standard deduction regime and muting the direct relevance of the SALT cap for a sizable group. State-level responses, particularly pass-through entity workarounds, may have further mitigated the cap’s impact.

Finally, the timing of local budget processes also matters. There is typically a lag between when local governments prepare and adopt their budgets and when federal tax changes are fully reflected in taxpayers’ liabilities. This lag may have allowed state and local governments to anticipate the reform and adjust proactively, consistent with the rapid emergence of policy workarounds following the TCJA. Taken together, these patterns point to a degree of forward-looking behavior and institutional adaptability in subnational public finance.

From a distributional perspective, the SALT deduction has long been characterized as a regressive federal tax expenditure. The evidence presented here suggests that limiting the deduction improved federal distributional equity without undermining local fiscal capacity.

More broadly, these findings challenge the assumption that federal tax subsidies are necessary to sustain local public finance. They underscore the importance of institutional foundations and local political economy in supporting fiscal resilience, even in the face of major changes in federal tax incentives. Future research should explore these institutional mechanisms in greater detail to better understand how local governments navigate fiscal shocks and policy-induced changes in intergovernmental fiscal arrangements.

9 Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author used ChatGPT-5.2 in order to correct grammatical mistakes and improve overall readability. After using this tool/service, the author reviewed and edited the content as needed and takes full responsibility for the content of the publication.

10 References

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11 Appendix A: Continuous Treatment Supporting Evidence

A key component of the identification strategy is the use of a continuous treatment variable, θ, defined as the proportional excess of average SALT deductions above the $10,000 threshold in 2017. In the main difference-in-differences specification, this variable is interacted with a post-2017 indicator to capture variation in treatment intensity. While informative, this interaction imposes a linear functional form on the relationship between exposure and fiscal response. If the true effect is nonlinear or varies across the distribution of θ, the average treatment effect may mask important heterogeneity.

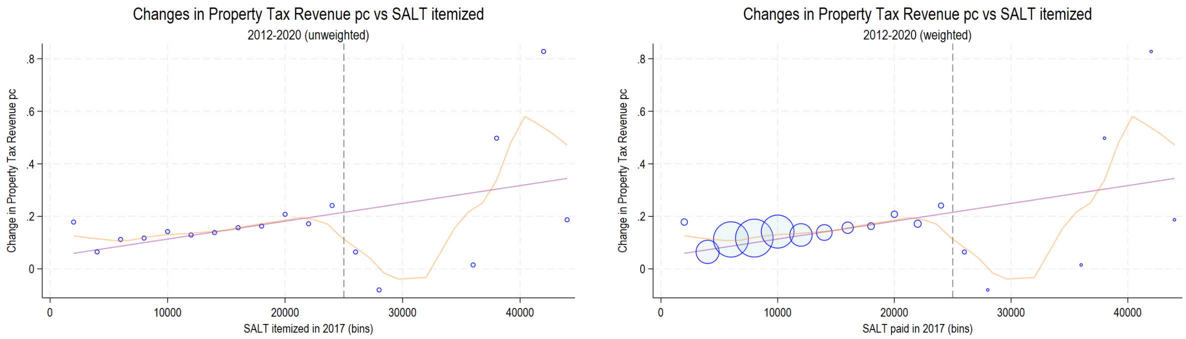
To assess the plausibility of the linearity assumption, I follow the guidance of (Callaway et al. 2024) and examine two complementary forms of evidence. First, Figure A1 shows a plausibly linear relationship between average SALT paid and changes in per capita property tax revenue for counties with average SALT deductions below $25,000. Beyond this threshold, the relationship becomes more erratic, and the number of observations declines sharply, motivating the sample restriction in the main analysis.

Second, I implement a nonparametric analysis using locally weighted scatterplot smoothing (LOWESS). Figure A2 plots the change in the log of per capita property tax revenue, from pre- to post-reform against θ for all counties ($\Delta\ln(\text{Property Tax}_{pc})$). The initial LOWESS fit using the full sample exhibits a relatively flat pattern, potentially driven by outliers in the upper tail of the distribution.

To address this, I trim the top and bottom 5 percent of observations based on both θ and the outcome variable and re-estimate the LOWESS curve. The resulting plot reveals a clear, positive, and monotonic relationship between treatment intensity and changes in property tax revenue per capita. This pattern lends visual support to the linear specification used in the TWFE regressions and offers intuitive evidence of a "dose–response" relationship between SALT exposure and fiscal adjustment.

This nonparametric evidence also reinforces the interpretation of the DiD coefficient as an Average Causal Response (ACR), under the assumption that treatment effects are approximately constant across levels of θ.

Figure A1. Change in Property Tax Revenue vs. SALT Exposure Measurement

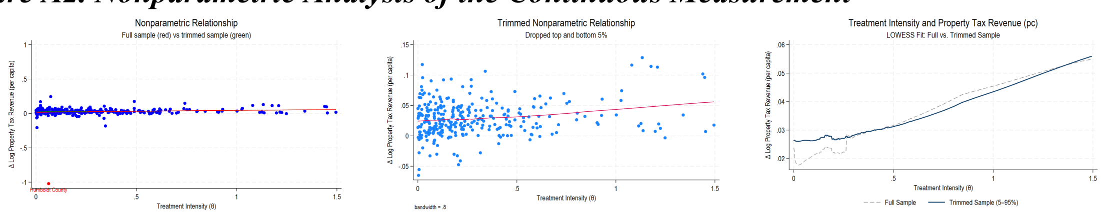


(a) Unweighted *(b) Weighted by population*

Source: Author's calculations using IRS Statistics of Income (SOI); the Government Finance Database (Pierson et al. 2015).

Notes: This figure plots the nonparametric relationship between SALT exposure in 2017 and the change in property tax revenue per capita from 2012 to 2020, using both unweighted (top) and population-weighted (bottom) local averages. The x-axis bins counties by their average SALT itemized deduction in 2017, while the y-axis reflects the change in log property tax revenue. As emphasized in (Callaway et al. 2024), flexible approaches such as binning or local polynomial smoothing are essential when treatment varies continuously, as they reveal nonlinearity and heterogeneity that would be masked in linear DiD specifications. Notably, the relationship appears approximately linear only up to around \$25,000 in SALT itemized deductions per itemizer, beyond which the pattern becomes erratic and driven by a small number of outlier counties. In light of this, I restrict our main estimation sample to counties with average SALT itemized deductions below \$25,000. This restriction ensures that the linear specification of θ as a continuous treatment intensity is valid and interpretable.

Figure A2. Nonparametric Analysis of the Continuous Measurement



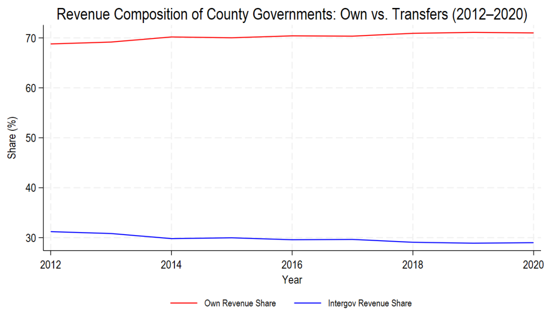
(a) Whole Sample *(b) Trimmed Sample* *(c) Both Samples*

Source: Author's calculations using IRS Statistics of Income (SOI); the Government Finance Database (Pierson et al. 2015).

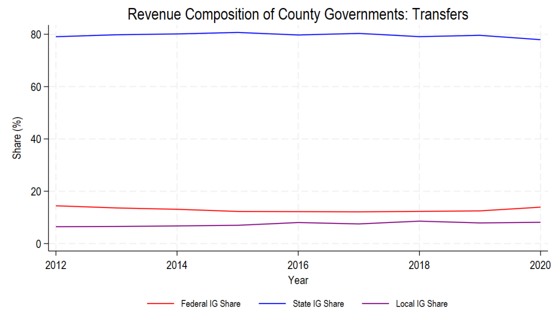
Notes: This figure explores the functional form of the continuous treatment variable θ (the proportional excess of average SALT deductions above the \$10,000 threshold in 2017) using nonparametric methods. Panel (a) displays a local polynomial fit using the full sample, where the fitted relationship appears nearly flat due to influential outliers (e.g., Humboldt County) and extreme leverage at low θ values. The resulting fit in Panel (b) reveals a clearer and more plausible upward-sloping relationship between treatment intensity and property tax revenue growth, suggesting that linear DiD specifications are more appropriate in this central range. Panel (c) compares LOWESS curves for the full and trimmed samples, confirming that the trimmed specification avoids distortions from extreme values while preserving the underlying trend.

12 Appendix B: County Governments Revenue Composition

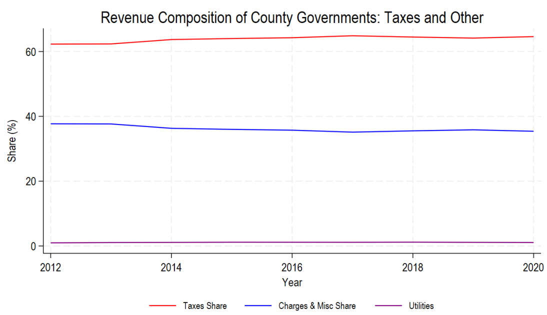
Figure B1. County Governments Revenue Composition



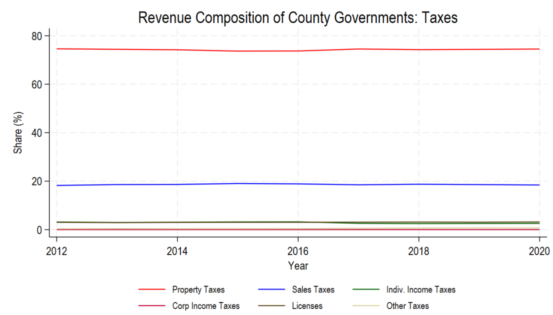
(a) Own vs. Transfers



(b) Transfers



(c) Taxes and Other



(d) Taxes

Source: Author's calculations using the Government Finance Database (Pierson et al. 2015).
Notes: Panel (a) shows that approximately 70 percent of total revenue is derived from own sources. Panel (b) breaks down intergovernmental transfers, with state transfers dominating. Panel (c) shows that taxes account for over 60 percent of own-source revenue. Panel (d) highlights the dominance of property taxes, which constitute about 74 percent of total tax revenue. Together, the panels underscore the centrality of property taxes to local public finance

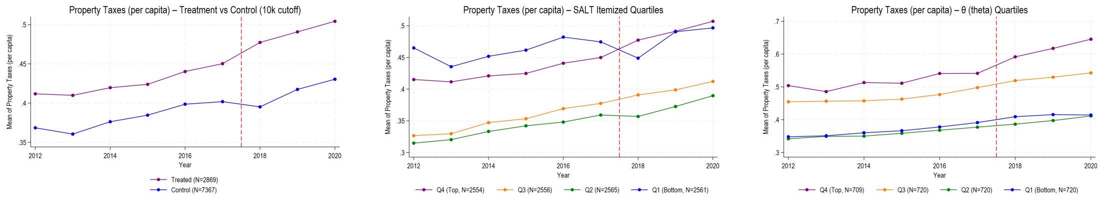
13 Appendix C: Descriptive Statistics

Table C1. Descriptive Statistics

Variable	Full sample, pre	Treatment, pre	Treatment, post	Control, pre	Control, post
Property Taxes (per capita)	0.394	0.426	0.491	0.382	0.414
	(0.454)	(0.398)	(0.462)	(0.473)	(0.420)
Log of GDP	14.95	16.09	16.19	14.51	14.58
	(1.538)	(1.346)	(1.378)	(1.374)	(1.375)
Log of Employment	10.45	11.48	11.54	10.04	10.10
	(1.459)	(1.179)	(1.195)	(1.355)	(1.350)
Share Male	49.64	49.31	49.35	49.77	49.81
	(1.656)	(0.989)	(0.983)	(1.835)	(1.908)
Share Married	51.31	51.30	50.80	51.31	50.50
	(6.378)	(5.688)	(5.531)	(6.626)	(6.557)
Share < 18	23.38	22.92	22.02	23.56	22.88
	(3.305)	(3.099)	(3.004)	(3.365)	(3.298)
Share ≥ 65	15.45	14.49	16.53	15.82	17.67
	(4.232)	(3.958)	(4.227)	(4.276)	(4.570)
Share Graduate or Professional	8.619	12.49	13.92	7.125	7.950
	(4.448)	(4.815)	(5.279)	(3.235)	(3.501)
Share Income > $75k	9.251	13.02	16.54	7.795	10.05
	(4.650)	(5.306)	(6.055)	(3.391)	(3.974)
Share Above 150% Poverty	74.46	79.45	81.59	72.54	74.66
	(8.359)	(6.785)	(6.338)	(8.110)	(7.887)
Urban (Metro = 1)	0.596	0.828	0.829	0.507	0.510
	(0.491)	(0.377)	(0.377)	(0.500)	(0.500)
Share Democrat (2016)	36.17	46.14	46.50	32.32	32.39
	(14.98)	(13.03)	(13.30)	(13.87)	(13.79)
Growth in FHFA HPI (Avg. % Change, 2000–17)	3.505	4.055	4.145	3.267	4.525
	(3.729)	(3.567)	(2.036)	(3.772)	(2.969)
Observations	6,829	1,903	966	4,926	2,441

Source: Author's calculations using IRS Statistics of Income (SOI); the Government Finance Database (Pierson et al. 2015), BEA Regional Accounts; BLS Local Area Unemployment Statistics; American Community Survey; MIT Election Lab; USDA Economic Research Service.
Notes: Counties are classified as treated if their average SALT deduction per itemizer in 2017 exceeded $10,000. The table compares mean values of fiscal, economic, and demographic characteristics across treated and control counties before and after the TCJA. Standard deviations are in parenthesis. Per-capita values are reported in thousands of nominal dollars. Data come from the Government Finance Database, BEA, BLS LAUS, and U.S. Census ACS.

Figure C1. Average Property Tax Revenue Per Capita, 2012-2020

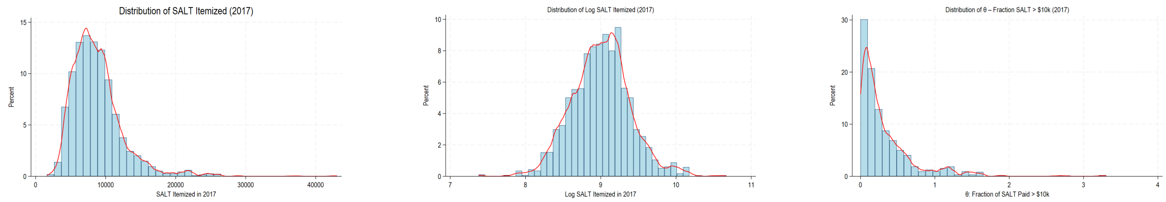


(a) Binary Threshold *(b) SALT Quartiles* *(c) Quartiles of θ*

Source: Author’s calculations using the Government Finance Database (Pierson et al. 2015).
Notes: Each panel plots average property tax revenue per capita for counties from 2012 to 2020. Panel (a) compares counties above and below the $10,000 average SALT deduction threshold in 2017. Panel (b) groups counties by quartiles of SALT itemized deductions. Panel (c) stratifies counties by quartiles of θ, defined as the share of itemized SALT deductions exceeding $10,000 in 2017. Revenue increased across groups in the post-TCJA period, with larger increases among counties with higher pre-reform exposure.

14 Appendix D: Distributions of Variables

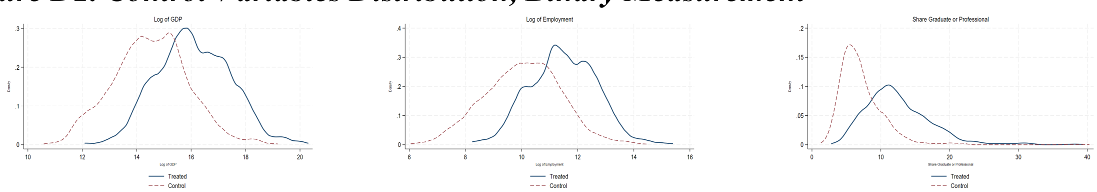
Figure D1. Exposure to the SALT Deduction Cap in the U.S. in 2017



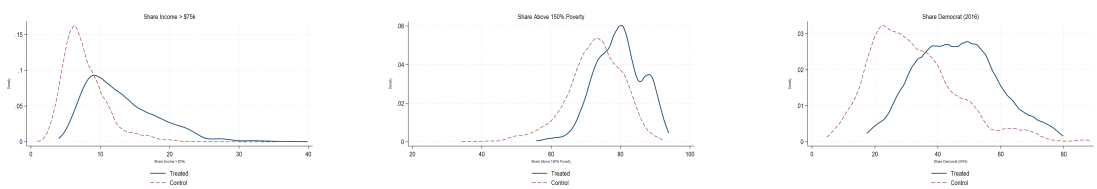
(a) Average SALT Itemized per Filer *(b) Average SALT Itemized per Filer (logs)* *(c) Intensity Measurement (θ)*

Source: Author's calculations using the IRS Statistics of Income (SOI).
Notes: This figure displays key measures of exposure to the SALT deduction cap using 2017 IRS data. Panel (a) shows the distribution of average SALT deductions itemized per filer across counties, highlighting substantial variation above and below the $10,000 cap. Panel (b) presents the logged distribution, more symmetrically centered around typical deduction levels. Panel (c) introduces the primary continuous treatment variable (θ), defined as the the proportional excess of average SALT deductions above the $10,000 threshold in 2017. The distribution of θ is highly skewed, indicating that the policy disproportionately affected a concentrated subset of counties.

Figure D2. Control Variables Distribution, Binary Measurement



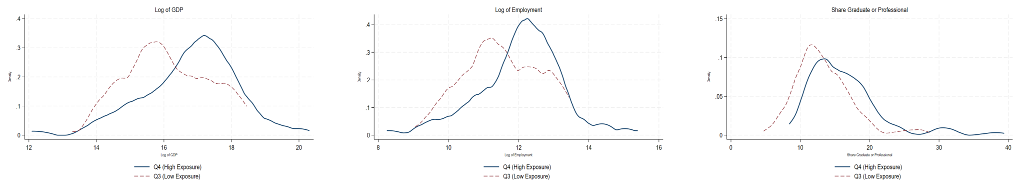
GDP (logs) *Employment (logs)* *Graduate or Professional*



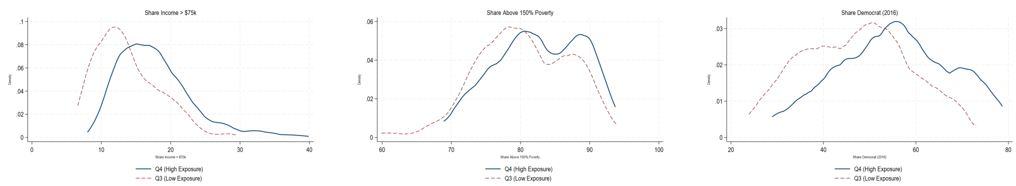
Income above $75,000 *Poverty 150%* *Democrat Votes*

Source: BEA Regional Accounts; BLS Local Area Unemployment Statistics; American Community Survey; MIT Election Lab; USDA Economic Research Service.
Notes: This figure compares the distributions of six baseline control variables between treated and control counties using kernel density estimates. Panel (a) and (b) show that treated counties tend to have higher levels of GDP and employment (both in logs). Panels (c) and (d) indicate that treated counties have higher shares of graduate or professional degree holders and households with income above $75,000. Panel (e) shows that treated counties are more likely to have populations above 150 percent of the poverty line, while panel (f) reveals a greater concentration of Democratic vote share in treated counties. These differences underscore the importance of including demographic and economic controls in the regression framework.

Figure D3. Control Variables Distribution, Quartiles 2 and 3



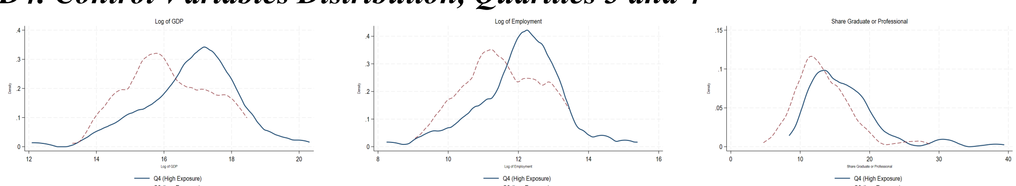
GDP (logs) *Employment (logs)* *Graduate or Professional*



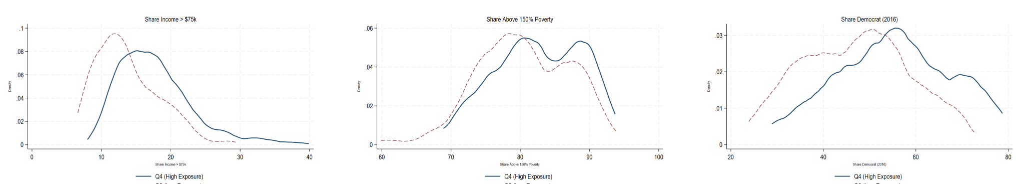
Income above $75,000 *Poverty 150%* *Democrat Votes*

Source: BEA Regional Accounts; BLS Local Area Unemployment Statistics; American Community Survey; MIT Election Lab; USDA Economic Research Service. Notes: This figure compares the distributions of control variables across counties in the third (Q3) and fourth (Q4) quartiles of treatment intensity (θ). The differences are more pronounced than in the previous figure, indicating that Q4 counties, those most exposed to the SALT deduction cap, are more affluent and urban. Panels (a) and (b) show higher GDP and employment for Q4 counties. Panels (c) and (d) confirm greater educational attainment and income levels, while panels (e) and (f) reveal higher shares of population above 150 percent of the poverty line and greater Democratic vote shares. These trends justify the need for flexible controls and robustness checks in interpreting the main treatment effects.

Figure D4. Control Variables Distribution, Quartiles 3 and 4



GDP (logs) *Employment (logs)* *Graduate or Professional*



Income above $75,000 *Poverty 150%* *Democrat Votes*

Source: BEA Regional Accounts; BLS Local Area Unemployment Statistics; American Community Survey; MIT Election Lab; USDA Economic Research Service. Notes: This figure compares the distributions of control variables across counties in the third (Q3) and fourth (Q4) quartiles of treatment intensity (θ). The differences are more pronounced than in the previous figure, indicating that Q4 counties, those most exposed to the SALT deduction cap, are more affluent and urban. Panels (a) and (b) show higher GDP and employment for Q4 counties. Panels (c) and (d) confirm greater educational attainment and income levels, while panels (e) and (f) reveal higher shares of population above 150 percent of the poverty line and greater Democratic vote shares. These trends justify the need for flexible controls and robustness checks in interpreting the main treatment effects.

15 Appendix E: Alternative Treatment Definitions

Table E1. Robustness: Continuous Treatment (Log SALT Itemized in 2017) and Whole Sample

	(1)	(2)	(3)
Panel A: Property Taxes			
Post × SALT itemized 2017 (logs)	0.0183	-0.0421*	0.0118
	(0.0178)	(0.0219)	(0.0199)
Observations	10,215	10,214	8,207
R^2	0.382	0.527	0.696
Panel B: Direct General Expenditure			
Post × SALT itemized 2017 (logs)	0.0156	-0.109	0.00436
	(0.0672)	(0.0774)	(0.0863)
Observations	10,215	10,214	8,207
R^2	0.337	0.485	0.537
State × year fixed effects	✓	✓	✓
Time-varying controls		✓	✓
SALT-context controls			✓

Notes: Time-varying controls include economic and demographic covariates; SALT-context controls include urban status, pre-TCJA housing price growth, Democratic vote share in 2016, and AMT exposure. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

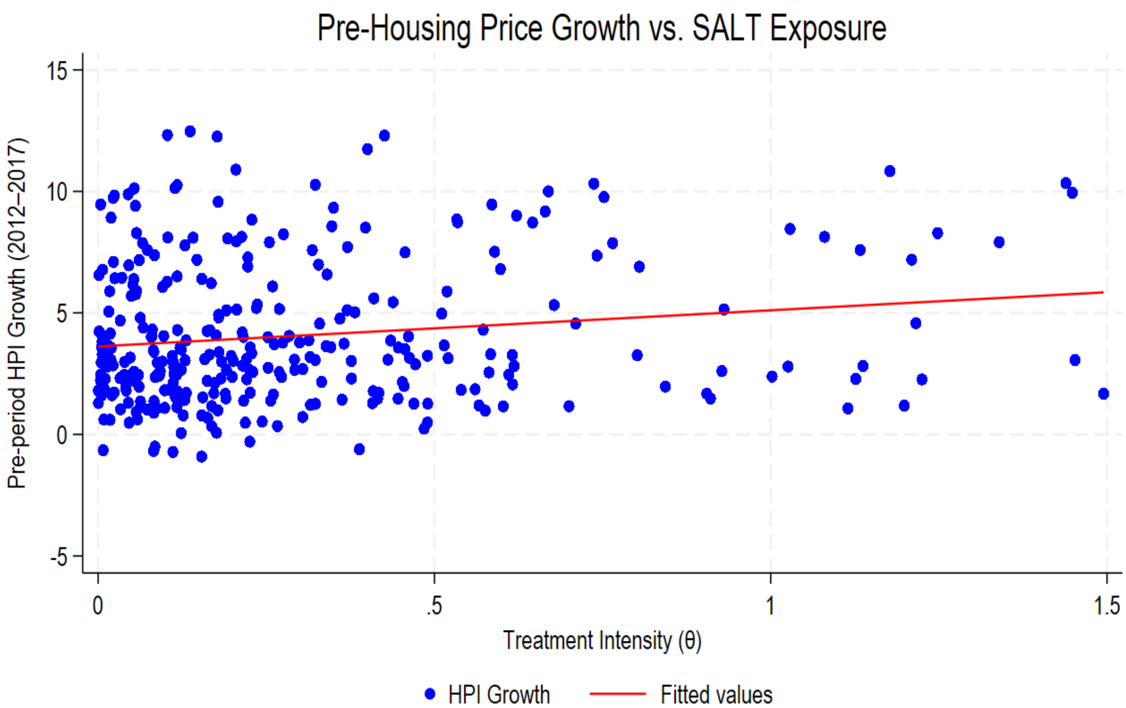
Table E2. Robustness: Binary Treatment (SALT > $10k) (per capita outcomes)

	(1)	(2)	(3)
Panel A: Property Taxes			
Post × SALT > $10k	0.0193***	-0.00840	0.00315
	(0.00628)	(0.00828)	(0.00790)
Observations	10,215	10,214	8,207
R^2	0.383	0.527	0.696
Panel B: Direct General Expenditure			
Post × SALT > $10k	0.00597	-0.0583*	0.00386
	(0.0317)	(0.0348)	(0.0339)
Observations	10,215	10,214	8,207
R^2	0.337	0.486	0.538
State × year fixed effects	✓	✓	✓
Time-varying controls		✓	✓
SALT-context controls			✓

Notes: Time-varying controls include economic and demographic covariates; SALT-context controls include urban status, pre-TCJA housing price growth, Democratic vote share in 2016, and AMT exposure. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

16 Appendix F: Housing Market Balance Check

Figure F1. Pre-TCJA Housing Price Growth vs. Treatment Intensity



Source: Author's calculations using the Federal Housing Finance Agency (FHFA) Hosing Price index data.
Notes: This figure examines the relationship between treatment intensity and pre-TCJA house price appreciation (2012–17). Each dot represents a county. The red line is the OLS fit. The absence of a strong relationship supports the claim that the treatment variable is not confounded by differential housing price growth across counties

17 Appendix G: Additional Robustness Test

This appendix reports additional robustness checks assessing the sensitivity of the main results to alternative specifications, samples, fixed effects structures, and treatment definitions. Throughout, the focus remains on the core treatment intensity measure used in the main analysis, defined as the interaction between a post-2017 indicator and county-level exposure to the SALT deduction cap, while alternative treatment definitions are presented as sensitivity checks.

Table G1 evaluates robustness to the inclusion of pre-treatment covariates and alternative detrending approaches. Starting from the baseline specification with state-by-year fixed effects, the table sequentially adds pre-treatment controls, SALT-context controls, and county-specific linear and quadratic pre-trends.[24] Estimated effects on property tax revenues remain positive and of similar magnitude, while no economically meaningful or statistically significant effects are detected for direct general expenditures.

Table G2 re-estimates the main specifications excluding FY 2020, a period in which the average post-treatment effect is particularly sensitive. When 2020 is excluded, the estimated increase in property tax revenues attenuates substantially. This pattern is consistent with a timing interpretation: fiscal adjustments may have materialized with a lag and become most visible in 2020, rather than reflecting an immediate and sustained shift following the 2017 reform. Results are therefore reported both including and excluding 2020.

Tables G3 and G4 assess sensitivity to alternative fixed effects structures. Table G3 replaces state-by-year fixed effects with county and year fixed effects, while Table G4 includes state and year fixed effects separately. Across both tables, the estimated responses of property tax revenues and expenditures are similar to those in the main analysis, indicating that the results are not driven by a particular fixed effects choice.

Taken together, the appendix tables reinforce the central conclusion of the paper: counties most exposed to the SALT deduction cap did not experience systematic declines in public expenditures following the reform, and any observed changes in property tax revenues are modest in magnitude and robust across a wide range of specifications.

[24] These detrending approaches differ from the event-study specification in equation (8), which models dynamic treatment effects relative to treatment timing. In contrast, the specifications in Table E1 allow each county to follow its own smooth trend in calendar time within the difference-in-differences framework.

Table G1. Robustness: Pre-treatment Controls and Alternative Detrending Methods (per capita outcomes)

	(1)	(2)	(3)	(4)	(5)
Panel A:					
Property Taxes					
Post × SALT > \$10k × θ	0.0576***	0.0576***	0.0608***	0.0247**	0.0485**
	(0.0127)	(0.0127)	(0.0133)	(0.0118)	(0.0217)
Observations	2,790	2,790	2,664	2,664	2,664
R^2	0.820	0.855	0.864	0.811	0.646
Panel B:					
Direct General Expenditure					
Post × SALT > \$10k × θ	0.0347	0.0347	0.0512	0.00235	-0.000802
	(0.0480)	(0.0481)	(0.0502)	(0.0497)	(0.0657)
Observations	2,790	2,790	2,664	2,664	2,664
R^2	0.739	0.770	0.795	0.769	0.753
State × year fixed effects	✓	✓	✓	✓	✓
Pre-treatment controls		✓	✓	✓	✓
SALT-context controls			✓	✓	✓
Linear detrending				✓	
Quadratic detrending					✓

Notes: The table reports difference-in-differences estimates of the effect of the SALT deduction cap on county-level fiscal outcomes per capita. The treatment variable is the interaction between a post-2017 indicator and a continuous measure of exposure to the SALT cap, θ, defined as the share of SALT itemizers in a county whose deductions exceeded \$10,000 in 2017, among counties with average SALT deductions above \$10,000. All specifications include state-by-year fixed effects, with standard errors clustered at the county level. Pre-treatment controls consist of 2017 county-level values of economic and demographic covariates. SALT-context controls include urban status, Democratic vote share in the 2016 election, pre-period house price growth, and AMT exposure. Columns (4) and (5) use outcomes detrended by removing county-specific linear and quadratic pre-trends estimated over the pre-treatment period (2013–17).

$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G2. Robustness: Exclusion of Fiscal Year 2020 (per capita outcomes)

	(1)	(2)	(3)
Panel A: Property Tax Revenue			
Post × SALT > \$10k × θ	0.0434***	0.0240	0.0277
	(0.0107)		
Observations	2,480	2,480	2,368
R^2	0.821	0.857	0.866
Panel B: Direct General Expenditure			
Post × SALT > \$10k × θ	0.000706	-0.0265	0.00278
	(0.0450)	(0.0487)	(0.0520)
Observations	2,480	2,480	2,368
R^2	0.741	0.773	0.798
State × year fixed effects	✓	✓	✓
Time-varying controls		✓	✓
SALT-context controls			✓

Notes: Difference-in-differences estimates of the SALT cap on county-level per capita fiscal outcomes (excluding 2020). Treatment is Post × θ, where θ is the share of SALT itemizers with deductions above \$10,000 in 2017 among counties above the cap. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G3. Robustness: County and Year Fixed Effects (per capita outcomes)

	(1)	(2)	(3)
Panel A: Property Taxes			
Post × SALT > \$10k × θ	0.0632***	0.0595***	0.0610***
	(0.0175)	(0.0173)	(0.0185)
Observations	2,826	2,826	2,700
R^2	0.986	0.986	0.986
Panel B: Direct General Expenditure			
Post × SALT > \$10k × θ	0.0189	0.0151	0.0223
	(0.0538)	(0.0554)	(0.0578)
Observations	2,826	2,826	2,700
R^2	0.972	0.973	0.976
County fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Time-varying controls		✓	✓
SALT-context controls			✓

Notes: The table reports difference-in-differences estimates using county and year fixed effects. The treatment variable is the interaction between a post-2017 indicator and θ, a measure of county-level exposure to the SALT deduction cap. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G4. Robustness: State and Year Fixed Effects (per capita outcomes)

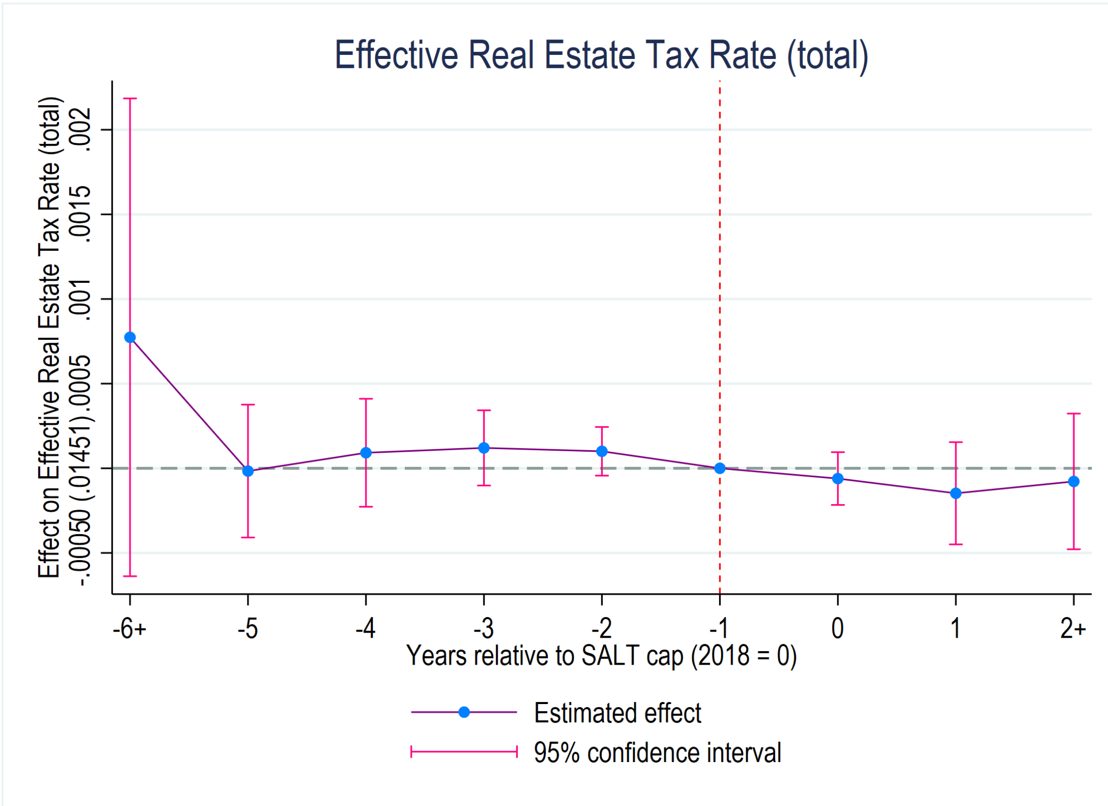
	(1)	(2)	(3)
Panel A:			
Property Taxes			

	(1)	(2)	(3)
Post × SALT > \$10k × θ	0.0632***	0.0443**	0.0466**
	(0.0176)	(0.0189)	(0.0231)
Observations	2,826	2,826	2,700
R^2	0.813	0.847	0.857
Panel B:			
Direct General Expenditure			
Post × SALT > \$10k × θ	0.0189	-0.0117	0.00374
	(0.0542)	(0.0596)	(0.0628)
Observations	2,826	2,826	2,700
R^2	0.732	0.763	0.788
State fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Time-varying controls		✓	✓
SALT-context controls			✓

Notes: The table reports difference-in-differences estimates using state and year fixed effects. The treatment variable is the interaction between a post-2017 indicator and θ, a measure of county-level exposure to the SALT deduction cap. All outcomes are expressed in per capita terms. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

18 Appendix H: Effects of the SALT Cap on Effective Property Tax Rates (ACS-based Measure)

Figure H1. Event-Study Estimates: Effective Property Tax Rate



Source: Author's calculations using the American Community Survey (ACS).
Notes: The effective property tax rate is computed as median real estate taxes paid divided by median owner-occupied home value using ACS five-year estimates. The figure reports event-study coefficients from the preferred specification. Vertical bars represent 95 percent confidence intervals.