Do Taxes Affect Pre-Tax Income Inequality?

Evidence from 100 Years of U.S. State Policies*

Sarah Robinson Matías Strehl-Pessina Alisa Tazhitdinova

Abstract

We study how U.S. state personal and corporate income taxes have affected pre-tax income inequality (income shares and top incomes) during the last century. The long panel nature of our data, from 1917 to 2018, allows us to study the effect of tax adoptions, tax cancellations, and tax changes, and to assess both immediate and long-term relationships. With event study, synthetic control, and heterogeneity-robust two-way fixed effects designs, we generally find no statistically significant or economically significant relationship between tax measures and inequality.

JEL Classification: D63, H24, H25, H71, N32

Keywords: income inequality, taxation, state tax policy

*Sarah Robinson: Department of Economics, Claremont McKenna College (sarah.robinson@claremontmckenna.edu); Matías Strehl-Pessina: Department of Economics, UCSB (mstrehlpessina@ucsb.edu); Alisa Tazhitdinova: Department of Economics, UCSB and NBER (tazhitda@ucsb.edu). We thank Youssef Benzarti, Max Ghenis, Camila Paleo, Gonzalo Vazquez-Bare, and seminar participants at the 2023 All-California Labor Economics Conference, 2023 NTA meetings, 2025 RIDGE forum, 2025 ECINEQ conference, and Claremont McKenna AERG for insightful suggestions.

Income and wealth inequality have been steadily rising in the U.S. In 1970, just under 10% of pre-tax income accrued to the top 1%, but by 2020, this share had doubled to 20% (updated Piketty and Saez, 2003).[1] Taxation is often proposed as a tool to mitigate inequality: indeed, the *after-tax* income distribution features less inequality than the *pre-tax* income distribution (Cooper et al., 2015). However, this focus on redistribution leaves a critical gap in understanding how taxes influence pre-tax income inequality, even as interest in "predistribution" policies has grown (e.g., Blanchet et al., 2022; Bozio et al., 2024).

Taxes can directly reduce individuals' incentives to work and innovate, and may also have indirect effects such as changing the relative bargaining incentives or power of individuals. As a result, the full impact of taxation on inequality may be even greater than the mechanical effect of redistribution. Despite these theoretical links, empirical evidence on how taxation affects pre-tax inequality remains scarce. The U.S. series of pre-tax top income shares feature a striking negative correlation with federal top personal income and top corporate income tax rates over the last century (shown in Figure A.1), but to what extent this relationship is causal, if at all, is an open empirical question.

The goal of this paper is to provide evidence on the effects of personal and corporate income taxation on pre-tax income inequality, using U.S. states as a laboratory. Our analysis combines data on U.S. state tax policy rules from Robinson and Tazhitdinova (2025) with U.S. state top income shares and other inequality measures from Frank et al. (2015). The resulting dataset allows us to exploit several sources of variation in tax policy – adoptions, cancellations, increases, and decreases – and study their effect on income inequality from 1917 to 2018. Our analysis focuses on the top marginal personal and corporate income tax rates, as these are most directly related to inequality.

Estimating a causal empirical relationship between taxation and inequality is difficult due to the potential for omitted variable bias and reverse causality. We argue in Section 3 that the tax reforms we study were primarily driven by factors orthogonal to inequality

[1] While the precise magnitude of this increase in inequality is still debated, all studies have found at least a small increase in inequality since the 1970s.

considerations, thus allowing for plausibly causal identification. Our setting and approach further present several advantages over the existing work. First, relative to a time series analysis, state-level analysis allows us to construct a plausible control group for each tax event; and relative to a cross-country analysis, we limit the set of other factors that may influence inequality outcomes and hence the likelihood of omitted variable bias. While U.S. states exhibit a wide range of tax policies, they are also collectively affected by many decisions made at the federal level. As a result, many factors that could have a direct effect on inequality (international trade, political environment, etc.) will be held largely constant across states, unlike in a cross-country study.

Second, our long panel data allows us to study a variety of tax events, including tax adoptions, large tax increases, large tax decreases, and tax cancellations. The variety of events that we consider allows us to provide a comprehensive view of the relationship between taxes and inequality, to verify the robustness of our findings, and to explore plausible sources of heterogeneity. We are able to analyze dynamic effects over a long horizon, thus studying both the immediate and long-term effects.

Our analysis proceeds as follows. We start by providing descriptive evidence on the relationship between state tax rates and top income shares. We show that top income shares vary dramatically across U.S. states and over time, and that they exhibit low degrees of persistence within states. This suggests that inequality is susceptible to change and is not an immutable characteristic of each state (e.g., driven by geography or other features). We also show that states with no personal income taxes or no corporate income taxes display higher levels of inequality, providing suggestive evidence of the potential influence of taxes on pre-tax income inequality.

Next, we turn to causal analysis. We start with event studies of income and corporate tax adoptions, which provide the largest tax shocks. Since most personal and corporate income taxes were adopted simultaneously or a few years apart, we interpret our results as measuring the joint effect of both tax adoptions on inequality. States predominantly

adopted these taxes in two waves: an early one in the 1930s, and a later one in the 1960s. We focus on the later wave, as it allows us to consider two distinct control groups. The first control group consists of states that never adopted a personal or corporate income tax. This group satisfies the canonical difference-in-differences (DiD) assumptions but may result in biased results if individuals or firms move across states to avoid taxes. The second control group consists of states that adopted a personal and/or corporate tax prior to 1925. Consequently, this comparison does not suffer from spillover effects but only results in unbiased estimates if the treatment effects on control states have stabilized by mid 1950s (Tazhitdinova and Vazquez-Bare, 2023). We also implement the methodologies of Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfoeuille (2020), in addition to the standard ordinary least squares (OLS) specifications to account for the possibility of heterogeneous treatment effects and the fact that different states adopted taxes in slightly different years. Irrespective of the type of specification employed, we find null results: we do not find evidence of neither statistically significant nor economically significant relationships between personal and corporate income tax adoptions and pre-tax income inequality. We find null effects on top 1% and bottom 90% income shares, and not significant and economically small negative effects on total incomes. Naturally, since our evidence relies on a small set of states, our statistical power is limited. Nonetheless, an examination of our point estimates alone suggests a similar conclusion.

Second, we study the effect of large tax changes on pre-tax income inequality. We estimate four sets of stacked event studies (Wing et al., 2024) around major tax changes: separately for personal and corporate income tax changes, and separately for tax increases and tax decreases. We consider a tax change major if the tax rate change is greater than or equal to 1 percentage point in magnitude, and limit our control group to states that experience no tax changes of more than 0.25 percentage points.[2] For most specifications, we

[2]Even though tax changes of less than 0.25 percentage points are only about 10% of all tax changes, allowing the control group to make small tax changes substantially increases the number of possible sub-experiments, given our stacked event study design.

find statistically insignificant changes in inequality outcomes, both for income shares and for total incomes. The coefficient estimates are also typically small, thus suggesting both an economically and statistically insignificant response. The only statistically significant effects we identify are an increase in the bottom 90% income share in response to personal income tax increases and an increase in top 1% income level in response to corporate income tax decreases.

Third, we analyze a few rare tax cancellations. We study the cancellation of the personal income tax in Alaska in 1980 and in West Virginia in 1942; the cancellation of the corporate income tax in Ohio in 2010; and the cancellation of both personal and corporate income taxes in 1943 in South Dakota. We use a synthetic control approach (Abadie and Gardeazabal, 2003; Abadie et al., 2010) to select an appropriate comparison for each cancellation separately. Our pool of control states is limited to those that maintained the canceled tax throughout the period of study. To ensure the robustness of our results, we consider various matching approaches. For our main results, we simultaneously match on top 1%, top 0.01%, and bottom 90% income shares and total incomes; in the appendix, we consider alternative matches on the Gini coefficient and/or each outcome variable individually. The results provide conflicting evidence. On the one hand, income inequality appears unresponsive to tax cancellations in West Virginia and in South Dakota. On the other hand, we see a large increase in the top 1% income share after the cancellation of the personal income tax in 1980 in Alaska. Unfortunately, the tax cancellation in Alaska coincided with the development of the oil industry in the state, making it difficult to attribute the observed effect to the cancellation of the personal income tax. The results for Ohio are inconclusive and appear to be driven by poor match quality.

Finally, we combine all state variation in tax rates in a single analysis: adoptions, cancellations, and changes of large and small magnitudes. Because estimates from a conventional two-way fixed effects specification may be biased, we use an analogous robust estimator from de Chaisemartin et al. (2025). We find no statistically significant relationship between per-

sonal or corporate tax changes and income shares or total incomes, and the point estimates are near zero. Placebo tests to assess the plausibility of the parallel trends assumption are also close to zero. When analyzing heterogeneity by the size of the tax change, we find more precise null effects and placebo tests for larger tax changes. While smaller tax changes are sometimes associated with changes in income inequality, the corresponding placebo tests show evidence of pre-tends.

To summarize, we generally find statistically insignificant relationships between inequality and tax measures. The coefficient magnitudes and patterns alone suggest that higher income taxes may reduce top income shares and income levels, however, these effects are generally small and not consistent across specifications. Accounting for the fact that specifications with the largest tax variation show the smallest effects, our results suggest that the relationship between state-level inequality and state income and corporate taxes is weak at best. While our null results may be in part driven by the lack of statistical power, the consistency of the results across specifications suggests otherwise.

Our findings indicate that pre-tax income inequality exhibits limited responsiveness to changes of state personal and corporate tax rates. While the changes we observe are large relative to state tax rate levels, even tax adoptions (3-5 percentage points) are small relative to the federal level. This result carries two key policy implications. First, it implies that individual states, acting in isolation, are unlikely to affect pre-tax income inequality as a result of incremental tax reforms. Second, it suggests that modest tax changes are insufficient to alter the economic environment to meaningfully affect inequality. Consequently, more dramatic tax policy changes at the federal level may be required if policymakers wish to change the distribution of pre-tax incomes.

Note that our state-level analysis differs from federal-level analysis in two important ways: ease of migration and the availability of income shifting. Since migration within a country is easier than across countries, we should expect stronger migration responses to state-level tax changes relative to a federal tax reform. Practically, this means that our

estimates, if anything, should be biased upwards (since control states would experience in-migration of top income individuals). Our largely null results render this concern irrelevant. Second, since income shifting opportunities are driven by the sum of federal and state tax rates, it is unlikely that state tax changes alone will affect the relative attractiveness of the personal tax base over the corporate tax base, or vice versa, particularly because differences in federal rates typically dominate state-level differences. Consequently, this channel of response is negligible in a state-level analysis but may be important for federal tax analysis.

This paper primarily relates to a small literature that seeks out a link between taxation and pre-tax income inequality. Some previous work examined such links using national time series data (Piketty et al., 2014; Bargain et al., 2015) or using cross-country data (Duncan and Sabirianova Peter, 2016; Rubolino and Waldenström, 2020). The three studies closest to ours use sub-national variation in tax rates. Marti et al. (2023) study how wealth inequality in Switzerland responds to changes in cantonal *wealth taxes*, while Nallareddy et al. (2022) study *corporate tax* changes that were implemented between 1991 and 2013 and their effect on pre-tax income inequality in the U.S. Our analysis extends these papers' analysis by considering a variety of personal and/or corporate tax reforms, including tax adoptions and tax cancellations, and over a longer time period, thus including the lowest point for the top 1% income share in the 1970s, as well as the previous high point in the 1920s. The closest study, Troiano (2018) explores the effects of personal income tax adoptions, introduction of personal income tax withholding and changes in auditing technology on inequality. Our analysis examines a different set of reforms and applies more recent methodological approaches, leading to different empirical conclusions.

More broadly, our work contributes to the various literatures that study the consequences of state taxation. These include papers that study economic responses to taxation (e.g., Hanlon et al., 2019; Kennedy et al., 2022), the incidence of personal and corporate taxes (e.g., Suárez Serrato and Zidar, 2016), migration in response to personal income tax progressivity changes and adoptions (e.g., Leigh, 2008; Cassidy et al., 2024), and many more.

1 Data Sources and Descriptive Evidence

1.1 Tax Rate Data

We use data from Robinson and Tazhitdinova (2025) that contains federal and state tax rates from 1910 to 2022 for the following: minimum and top personal income, minimum and top corporate income, sales, cigarette per pack, gasoline per gallon, and alcohol spirit per gallon. We focus on the top marginal personal and corporate income tax rates, which approximate the average tax rate for the highest earners and thus are most closely related to inequality.

Figure 1 provides insight into our tax policy variation. Panel A documents the dramatic increase in state personal and corporate income taxes over our study period. Both taxes were collected by fewer than 10 states in 1917 (the first year inequality data is available); yet by 2018, both taxes were used by more than 40 states. Thus, our time horizon includes many extensive-margin tax "events" – primarily tax adoptions, plus a small number of tax cancellations. In addition, we see variation across states in their choice of whether to implement flat or progressive income taxes; most states have opted for progressive personal income taxes, while only a third of states have opted for progressive corporate taxes.

Panel B of Figure 1 shows the new tax adoptions by state. We see an early wave of tax adoptions from 1929 to 1937, and a later wave from 1963 to 1972. The two decades between and four decades after these waves are close to dormant: between 1938 and 1962, zero states adopted a personal income tax for the first time, and only three states adopted a corporate income tax. Similarly, since 1973, only two states have adopted a personal income tax and no states have adopted a corporate tax. There is some geographical clustering in adoption tendencies (e.g., most personal income tax adoptions during the 1960s happened in Midwestern and Northeastern states) but we also see considerable heterogeneity. Furthermore, some states choose initial rates that are much lower than the prevailing rates, while other states adopt higher ones.

Finally, in Panel C of Figure 1, we see that tax rate levels varied dramatically across states and over time. On average, the top personal income and top corporate income tax rates increased steadily through most of the 20th century, before declining starting in the 1980s and 1990s. However, the average trends mask substantial heterogeneity across states.

1.2 Inequality Data

We use the Frank-Sommeiller-Price Series for Top Income Shares (Frank et al., 2015). This data contains the following inequality measures from 1917 to 2018: top 10%, top 5%, top 1%, top 0.5%, top 0.1%, and top 0.01% pre-tax income shares, as well as pre-tax income thresholds and pre-tax total income, for each state in every year. The data are constructed using the IRS SOI Tax Stats on the amount of income and number of taxpayers in different income ranges, and interpolated using a Pareto distribution. Using the same methodology as in the national Piketty and Saez (2003) series, incomes are adjusted to account for changes in the IRS definition of income over time, such that the data in all years represent adjusted gross income including capital gains, and further adjusted to include imputed income from non-filers.[3] Analogous data at the national level comes from Piketty and Saez (2003). We inflation-adjust income thresholds and total income to 2020 dollars, and use the income shares to calculate real income accrued to each top share.

Figure 2 provides an overview of the variation in inequality across states and over time, with Panel A showing the distribution of state top income shares over the last century. We see a pronounced U-shaped trend, similar to inequality at the national level from Piketty and Saez (2003). Thus, despite the heterogeneity in state tax and other policies, and the fact that the top income thresholds differ across states, the dynamics over time for inequality *within* states largely mirrors the overall U.S. Having said that, we see substantial variation within any given year: some states show significantly higher top 1% and top 0.01% shares than

[3]See Piketty and Saez (2003), Sommeiller and Price (2018) Appendix A, and Frank et al. (2015) for further details on the construction of these data.

the overall U.S. series, while others much lower.[4] This suggests that there is some sorting of individuals across states by income. Such sorting may, for example, reflect cost-of-living adjustments where individuals with the same skill sets experience different incomes based on where they reside (Diamond and Moretti, 2021).

Panel B of Figure 2 explores whether inequality levels within each state persist over time by plotting the range of top income shares for each state. To control for the overarching U-shaped trend, we normalize each income share to be a percent of the overall U.S. top share in that year. These figures order states by the average level of inequality over all years, and color each state based on its modal policy during this period. The dots show the level of inequality in the first five years and in the most recent five years. We see that even controlling for the overall U.S. trend, there remains substantial heterogeneity within states, with most states showing considerable variation across years. Furthermore, it is not the case that states with low levels of inequality in the early 20^{th} century continue to have low levels of inequality today. The ordering of states and the degree of heterogeneity varies depending on whether we study the top 1% share or the top 0.01% share.

These figures also provide suggestive evidence that lower income taxes may be associated with pre-tax income inequality: among states with high average levels of inequality over the studied time period, we see a large number of states that have neither personal nor corporate income taxes (marked in red). Having said that, it is also the case that states with progressive personal and corporate income taxes (marked in green) do not appear to be concentrated at the low end of inequality.

[4] Appendix Figure A.2 Panel A shows this explicitly by showing equivalent graphs for the top income shares measured as a percent of corresponding U.S. shares. Across all years, the average state top shares are lower than the overall U.S. top share. The state average and overall U.S. top share can diverge because an income in the top 1% nationally is not necessarily in the top 1% for a given state. In Appendix Figure A.2 Panel B, we show distributions weighted by the number of tax units in each state – the state average is still above but much closer to the overall U.S., meaning that larger states tend to have higher levels of inequality.

2 Potential Mechanisms

We now briefly discuss four key mechanisms through which state taxes may affect pre-tax income inequality. Our first, second, and fourth channels summarize the theoretical models developed by Piketty et al. (2014). We extend to discuss one other channel (out-migration), which is plausibly more relevant to the state taxes we study. Naturally, our summary is not exhaustive, nor does it account for possible interactions of the various channels or general equilibrium effects. Nonetheless, Panel A of Table 1 illustrates how the key channels may affect income levels and income shares.

Our empirical analysis implicitly presumes that personal and corporate tax changes disproportionately affect top incomes. This assumption is consistent with tax changes that are larger (or only) for the top marginal personal rate; flat changes in personal income taxes if high-income individuals are more tax-elastic (as found in, e.g., Gruber and Saez, 2002); and corporate tax changes when high-income individuals bear a greater share of the tax incidence on wages (as in e.g., Kennedy et al., 2022).[5]

First, higher taxes reduce incentives to work or invest. Thus, tax increases may reduce income levels for individuals at the top of the income distribution, and reduce income levels to a lesser extent at the bottom (with potentially no effect for an increase only to a top marginal rate). The top income share falls, the bottom income share rises, and overall inequality decreases. In the case of reforms that lead all individuals to reduce their income by the same percent, income levels for both groups would still decrease, but income shares would remain unchanged due to falling income thresholds for top groups.

Second, higher tax rates increase the incentive to avoid or evade taxes. This can be accomplished in various ways, for example by taking advantage of various loopholes, increasing charitable contributions, hiding earnings abroad, or taking illegal measures. These actions would lead to the same effects on observed income levels and shares as reduced work incen-

[5]Throughout, we assume a non-zero incidence of corporate taxes on workers (e.g., Suárez Serrato and Zidar, 2016; Fuest et al., 2018).

tives, though without any real changes to overall inequality i.e., no real changes in economic activity for any individual. Restructuring income flows from one tax base to another has been found to be a large margin of response to changes in *federal* taxes, as individuals can choose between profits being taxed at the personal level (as an S-corporation), or at the corporate level and subsequently as capital gains (as a C-corporation) (Saez, 2004; Saez et al., 2012). However, because states predominately tax capital gains income at the same rates as ordinary income, and because state tax rates are much smaller in magnitude than federal rates, changes in state income taxes are less likely to affect which tax base is more advantageous, and thus less likely to drive substantial income shifting. In addition, because tax adoptions typically included both personal and corporate income taxes, they represent a source of variation that is particularly unlikely to drive income-shifting.

Third, a particularly straightforward form of tax avoidance is migration (Feldstein and Wrobel, 1998). Since moving across states is easier than across countries, this channel may be particularly strong in our setting. Such migration responses have been documented for high-income individuals (e.g., Cassidy et al., 2024) and business activity (e.g., Giroud and Rauh, 2019) but rarely for low-income individuals (e.g., Yagan, 2019) – we thus consider low-income individuals to be less likely to respond along this margin, even in the case of uniform tax increases. As high-income individuals move out of the state, income levels mechanically fall at the top of the distribution, as do thresholds for top groups and thus income levels at the bottom. As with other forms of avoidance, these changes occur despite no real change to overall inequality. The impact on top and bottom income shares depends on the distribution of incomes overall and for outmigrating individuals – in an extreme case where the entire top 0.01% moves out of the state, and the bottom 99.99% of incomes shares the same distribution as the full population, income shares would remain unchanged.[6]

Fourth, tax policy may affect bargaining positions or incentives to bargain aggressively. For example, if high-income individuals are able to use their wealth to generate a "trickle-

[6]This feature of the income distribution is true for Pareto distributions, which typically match actual income distributions fairly well (Jones, 2015).

up" transfer from bottom to top incomes, then a tax increase that lowered the returns to bargaining could reduce top incomes and shares, and increase bottom incomes and shares. These effects could also arise if another channel (e.g., work incentives) reduced top income, and thus top wealth and bargaining power. Another possibility is that low-income earners are able to use political power (e.g., via unions) to generate a "trickle-down" transfer, and tax increases exacerbate this dynamic. In both the trickle-up and trickle-down cases, the *relative* bargaining power or incentives of top-income earners falls, so the expected results are similar.[7]

As summarized in Table 1 Panel A, all four of these channels predict decreases in top income levels. In addition, they typically predict a decrease in bottom income levels or no change, with the exception of the bargaining mechanism which predicts an increase in bottom income levels. Finally, top income shares are likely to decrease, and bottom income shares are likely to increase, though the effect of out-migration on these outcomes has an ambiguous sign.

3 Identification Challenges

The goal of this paper is to establish a plausibly causal relationship between U.S. state taxes and pre-tax income inequality. Here we discuss the overarching identification and interpretation challenges such analysis involves. We defer the discussions of specific econometric issues (heterogeneous treatment effects, synthetic control group selection, etc.) to respective future sections.

Exogeneity of tax policy. State personal and corporate income taxes typically fulfill two roles. First, they generate revenue, together raising nearly 50% of overall state revenues, or 2.5% of U.S. GDP (Robinson and Tazhitdinova, 2025). Second, they are used for income

[7] As discussed in Piketty et al. (2014), top-income earners being over-paid relative to their productivity would increase the *optimal* tax, while being under-paid would reduce the optimal tax. While distinguishing between these two cases is important from an efficiency perspective, their implications for inequality are identical.

redistribution, with many states featuring progressive tax schedules and means-tested welfare programs. As a result, one potential concern is that income taxes may be adopted or modified as a result of changes in pre-tax income inequality (e.g., Sokoloff and Zolt, 2005; Limberg, 2021). On one hand, rising inequality may prompt policymakers to implement a higher level of taxes and transfers. On the other hand, when inequality is higher, top-income individuals may have greater financial ability to influence policymakers to lower tax rates; and when inequality levels are lower, bottom-income individuals may have greater ability to pressure policymakers into raising top-income tax rates. Another possibility is that tax policy is correlated with other major factors that have a direct effect on inequality. While we cannot rule out such possibilities, we outline a few reasons to believe that this is not a major concern.[8]

With respect to adopting new taxes, the personal and corporate income tax adoptions that we study appear to be driven by revenue pressures – the early wave of tax adoptions coincides with the introduction of the New Deal programs (enacted between 1933 and 1938), and the later wave of adoptions coincides with postwar expenditures as well as the introductions of the Medicaid and SNAP programs (established in 1965 and 1964 respectively). Empirically, we also do not find any evidence that the timing of adoption is systematically correlated with the level of inequality in the state. Panel A of Figure 3 shows the top 1% income shares as a percent of the U.S.-wide share in the year that a given state adopted its personal or corporate income tax. We see neither an increasing nor decreasing pattern between the tax rate and the prevailing level of inequality in the state. Figure 3 does show that states that adopt flat income taxes tend to have higher levels of inequality than states that adopt progressive tax schedules, suggesting that the prevailing level of inequality may restrict the type of tax the state is able to adopt.

Robinson and Tazhitdinova (2025) provide additional evidence showing that the timing,

[8]Given the large number of state tax reforms, collecting information on each reform's motivation *à la* Romer and Romer (2010) is difficult. Furthermore, even if changing pre-tax income inequality was not the official goal, implementing such a change may be more or less feasible in the presence of high/low levels of inequality.

duration, and order of tax adoptions do not appear to predict future tax rates and revenues. Their results suggest that the tax adoption process was likely driven by political constraints (Penniman, 1980; Berry and Berry, 1992; Cassidy et al., 2024), rather than intrinsic state preferences. Indeed, many attempts to adopt income taxes failed or succeeded unexpectedly. For example, income tax legislation in Oregon was passed four times only to be overturned by subsequent referendums.[9] Overall, tax adoptions appear to be rather idiosyncratic.

With respect to changes in tax rates, income taxes are adjusted frequently – on average, 12% of states with a personal income tax change the rate in a given year, while 10% of states change their corporate income tax (see Panel B of Figure 3). These changes are rarely one-directional, such that the same state sometimes increases and decreases its rate within a short period, and the changes are typically small: 50% of changes are smaller than 1 percentage point, and only 10% of tax changes are higher than 3pp (personal) or 2pp (corporate; see Appendix Figure A.6). Since inequality levels do not adjust as rapidly, it seems unlikely that tax changes are motivated by inequality considerations. Nonetheless, one may be worried that tax changes are correlated with other factors that may affect inequality outcomes. Robinson and Tazhitdinova (2022) show that tax changes are hard to predict in general, with economic, political and institutional factors explaining less than 20% of the timing and magnitude of tax changes. Relatedly, Robinson and Tazhitdinova (2025) focus on economic conditions specifically and show that these do not affect the timing of tax changes, nor the magnitude of tax rates. Overall, tax changes appear to be as idiosyncratic as tax adoptions.

Relative to prior work using cross-country data (Duncan and Sabirianova Peter, 2016; Rubolino and Waldenström, 2020), our focus on U.S. states also limits the set of other factors that may influence inequality, and thus the potential scope for omitted variable bias. While U.S. states have discretion over a large range of policies, they are nonetheless bound by many decisions made at the federal level. Thus, such important considerations as international

[9] At the 1929 National Tax Conference, it was noted that "Oregon having passed their fourth law are awaiting the referendum. They never seem to get discouraged in Oregon" (Bailey, 1929).

trade, political environment, etc., which are not held constant in a cross-country study, will be consistent across states. Similarly, our approach allows us to control for observed and unobserved changes in the national environment over time, such as technological progress or rates of return, which is not possible in a time series analysis (e.g., Piketty et al., 2014; Bargain et al., 2015).

Finally, our event study design for tax adoptions and tax changes allows us to test for the presence of pre-trends. If the states that are making changes to their tax policy are trending similarly to the states that are not, this would support the plausible exogeneity of tax changes to other factors that affect inequality. In particular, the order of events will help us to test for reverse causality: we would expect tax changes to precede inequality changes if taxes affect inequality, and vice versa if inequality drives tax policy.

Power. Our use of variation at the state level raises the question of whether there is sufficient power to detect the treatment effects of interest. Increasing the sample size or focusing on the sample with the largest treatment exposure would both increase power, but cannot be done simultaneously in this setting. As a result, our primary approaches focus on large changes in tax policy. If there is truly an effect of tax rates on inequality, then it will be the most detectable in settings with the largest variation in tax rates. We begin by studying the joint effect of personal and corporate income tax adoptions, where both taxes are 3-5 percentage points on average, before turning to large tax changes, which are more frequent and have an average magnitude of 1.5-2 percentage points. We use event study designs, enabling the test for pre-trends, with modifications to avoid using a control group that includes a large number of already-treated units. While this approach reduces our concerns about bias, it also inevitably reduces our sample size. We also analyze the cancellations of tax rates, which are large in magnitude (at least 6 percentage points), but occur very rarely.

We complement this approach with an analysis that maximizes the possible sample by using all variation in tax rates and all 50 states, corresponding to an average change

of 0.5-0.6 percentage points. Our approach is similar in spirit to a two-way fixed effects specification. However, recent work by de Chaisemartin and D'Haultfoeuille (2020), Sun and Abraham (2021), Callaway and Sant'Anna (2021), and Goodman-Bacon (2021) has shown that conventional two-way fixed effects specifications may lead to biased estimates.As a result, we use the robust estimator from de Chaisemartin et al. (2025), which additionally allows us to test for pre-trends and thus assess the suitability of the implied control group.

Spillovers. Tax policy in one state can affect outcomes elsewhere if top-income individuals are mobile and avoid higher tax liabilities by moving to a state with lower tax rates. Evidence of such behavioral responses has been documented by Cassidy et al. (2024), who document how post-World War II income tax adoptions led to significant out-migration to states that did not have the income tax. Naturally, if the state to which high-income individuals move to is included in the control group in a difference-in-difference analysis, then our estimates will be biased away from zero. This is most concerning in our specifications that rely on states without income taxes as the control group, as these states are the most attractive destinations from a tax avoidance perspective. As a result, we conduct a robustness check where we compare states that adopt income taxes to states that introduced income taxes more than three decades ago (and are thus less attractive destinations relative to no-tax states). While this approach violates canonical difference-in-differences assumptions, it is valid as long as the treatment effects among the already-treated group have stabilized (Tazhitdinova and Vazquez-Bare, 2023).

4 How Do Tax Adoptions Affect Inequality?

In this section, we evaluate how pre-tax income inequality across U.S. states responds to adoptions of personal and corporate income taxes. These adoptions create large variation in tax rates, around 3-5 percentage points for each tax rate, which provides the best opportunity to detect any impact on inequality. Because most personal and corporate income taxes

were adopted simultaneously, our estimates correspond to the joint effect of these adoptions on inequality. While we are unable to attribute effects to the taxes individually, this feature also substantially reduces concerns about potential income-shifting: because states predominantly tax capital gains as ordinary income, these adoptions are simultaneous large changes in the ordinary income, capital gains income, and corporate income taxes, and thus cannot be fully avoided by shifting income across tax bases.

4.1 Tax Adoptions: Empirical Approach

We use an event study approach to study the effect of personal and corporate income tax adoptions on inequality. Specifically, we estimate the following equation:

$$\ln(Outcome_{st}) = \sum_{\substack{k=-10 \\ k\neq -1}}^{15} \beta_k \; \mathbb{1}\{t - t^* = k\} \; Treat_s + X'_{st}\gamma + \alpha_s + \lambda_t + \varepsilon_{st} \tag{1}$$

where $Outcome_{st}$ is our inequality outcome, e.g., the share of income that accrues to the top 1%, in state s and year t. The year of tax adoption is denoted t^*, such that the indicator equals one for observations that are k years post-adoption. $Treat_s$ is equal to one if state s has a tax adoption event, and zero otherwise. We control for a small set of time-varying state characteristics X_{st}: tax rates and if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years).[10] We include the latter set of covariates to control for variation stemming from differences in the demographic composition of states and the availability of public goods. State fixed effects α_s ensure that estimates are identified from variation within states, rather than cross-sectional comparisons. Year fixed effects λ_t control

[10] Unfortunately, state-level data for years prior to 1950 is limited. Consequently, we are able to control for a small set of variables only. Furthermore, some controls (state expenditures and unemployment rates/ratio) are only available to be included in the late adoption analysis. Results without any controls are similar, see Appendix Figure B.10.

for idiosyncratic time effects. We cluster standard errors ε_{st} at the state level.

While we are primarily interested in post-reform ($k > -1$) estimates of coefficients β_k, the estimates of pre-reform coefficients allow us to evaluate the likely similarity of treatment and control groups, as well as to provide evidence on the direction of causality. As discussed in Section 3, if inequality levels influence tax policy, we would expect the coefficients for $k < -1$ to be non-zero and statistically significant. In addition to this event study specification, we also use the analogous estimators from de Chaisemartin and D'Haultfoeuille (2020) and Callaway and Sant'Anna (2021), which in this case provide very similar results.

Ideally, we would separately identify responses to the introduction of a personal income tax and to the introduction of a corporate income tax. However, most states introduced these taxes concurrently. For this reason, our analysis will focus on states that experienced a *single* tax adoption event, which includes adopting only one tax as well as adopting both taxes in the same or consecutive years. Larger gaps between adoptions events pose challenges for identification, because it is not clear whether changes in inequality after the "second" event should be attributed to the introduction of the second tax versus to a long-term dynamic effect of the first. In addition, we limit our analysis to states for which we can observe a sufficiently long pre-period (at least ten years), in order to assess the likely validity of the parallel trends assumption.[11]

These criteria together exclude 21 states: 16 of them are excluded because they lack a sufficiently long pre-period, while the other 5 states adopted the personal and corporate taxes at different times. Our treatment group consists of 25 states, of which 23 adopted personal and corporate income taxes simultaneously (i.e., within one year), and 2 states that adopted corporate income taxes only. For the states adopting the taxes one year apart, we consider the first year as the year of adoption. Note that our analysis does not distinguish between flat and progressive tax schedules, because the overwhelming majority of states introduced a progressive personal income tax and a flat corporate income tax (recall Figure 1(a)). The

[11] As our data begins in 1917, this excludes states adopting either tax prior to 1927, as well as Alaska and Hawaii which adopted prior to statehood.

remaining 4 states never adopted either personal or corporate taxes.[12]

We estimate Equation (1) separately for the two waves of tax adoptions i.e., for the 17 early adoptions occurring between 1929-1937, and for the 8 late adoptions occurring between 1963-1972.[13] We estimate our results separately for each adoption wave for several reasons. First, because Cassidy et al. (2024) find differential revenue and migration responses to tax adoptions that occurred before and after the World War II. Second, the separate approaches allow for greater precision when selecting a control group. When analyzing the early wave, our control group consists of states that eventually adopt taxes but not over the time horizon studied, as well as the 4 states that never adopted personal or corporate taxes. When analyzing the later wave, we exclude all early adopters so that our control states only include the never-adopting states. We exclude the early adopters from this analysis out of concern that dynamic treatment effects in a large number of already-treated states would bias our estimates (de Chaisemartin and D'Haultfoeuille, 2020; Sun and Abraham, 2021; Callaway et al., 2021; Goodman-Bacon, 2021). Third, for late adopters, we consider two distinct control groups. In addition to never-adopter group described above, we also consider a control group that consists only of states that adopted personal/corporate income taxes *prior to 1925* and had no adoptions since then. Since these states adopted their taxes at least 39 years ago, the effect of their adoptions is likely to have already stabilized, and therefore they can be used as a control group. Since these states already have a personal and/or corporate income tax, they are less attractive destinations from a tax avoidance perspective compared to the no-tax states. Consequently this comparison allows us to estimate effects that are not affected by cross-state migration responses.

Appendix Figure B.7 shows the magnitude of the tax reforms: on average, the newly introduced income taxes were each between 3 and 5 percentage points (and adopted simul-

[12]States excluded due to lack of pre-period: AK, CT, DE, HI, MA, MO, MS, MT, NC, ND, NY, OK, SC, TN, VA, WI. States excluded due to non-simultaneous adoptions: CA, NJ, PA, RI, WV. Early adopters: AL, AR, AZ, CO, GA, IA, ID, KS, KY, LA, MD, MN, NM, OR, SD, UT, VT. Late adopters: FL (corp. only), IL, IN, ME, MI, NE, NH (corp. only), OH. States that never adopted either tax: NV, TX, WA, WY.

[13]After our exclusion criteria described above, no states adopted taxes between or after these waves.

taneously). In case of early adoptions, the average rates remained roughly constant over time. In contrast, the late adoptions saw a gradual rate increase, starting from just below 4 percentage points for corporate taxes, and reaching 6 percentage points approximately 15 years later. The rise in personal income taxes was much smaller.[14]

4.2 Tax Adoptions: Results

Figures 4 and 5 show how late tax adoptions affected top 1% and bottom 90% income shares and incomes. In Figure 4, the control group consists of never-adopters, while in Figure 5, the control group consists of states that adopted personal/corporate income taxes prior to 1925 and had no adoptions since then. Recall that while the results in Figure 4 may be exxagerrated because of spillover effects, the results in Figure 5 should not suffer from such.

Both Figures 4(a) and 5(a) imply a statistically insignificant and null effect of tax adoptions on top 1% income shares. Both individual year coefficients and pooled DiD results are not statistically or economically significant. The results for top 1% incomes in Figures 4(b) and 5(b) are largely similar, though we observe a decrease in incomes 10 years after adoption in Figure 4(b). Given the delayed nature of this effect, it is unlikely to be attributed to tax adoptions.

Next, we show the corresponding changes in the bottom 90% income share, in Figures 4(c)–5(c), and bottom 90% income level, in 4(d)–5(d). Once again, the results do not provide evidence in favor of a robust relationship between income shares and tax adoptions. Nearly all of the event-study estimates and all but one of the pooled DID estimates for shorter, medium, and longer-term effects are statistically insignificant. The bottom 90% point estimates, before and after tax adoption, are precisely estimated around zero, suggesting a null effect. For bottom 90% income, the results are less precisely estimated, but similarly suggest a null effect.

As the analysis relies on fewer than 30 observations per year, the lack of statistical

[14]Corresponding revenue changes are available in Figure B.8 for late adoptions. Revenue data is not available for early adoptions.

significance is likely to be at least in part driven by the lack of power. Nonetheless, even if we focus on the coefficient pattern, the evidence appears to reinforce the same conclusion. Most point estimates in Figures 4–5 are close to zero, and do not exhibit a notable change in magnitude or pattern around adoption event.

Our baseline approach in Figure 4 does not include states that adopted personal/corporate income taxes prior to 1925 in the control group because in the baseline periods, the control states are already treated while treated states are not. In the presence of dynamic treatment effects, such analysis will be biased (Tazhitdinova and Vazquez-Bare, 2023). Therefore, Figure 5 provides unbiased estimates only if the dynamic effects (if any) have concluded by the beginning of the baseline period in Figure 5. Fortunately, the lack of statistically or economically significant effects in either Figure ensures that this assumption is likely to hold in our setting. Consequently, the analysis in Figure 5 is internally consistent.

Overall, there is no evidence of a relationship between income tax adoptions and pre-tax income inequality. After implementing personal and corporate income taxes, each with average rates of 3-5 percentage points, the short–, medium–, and long–term point estimates suggest no effect on the top 1% or bottom 90% income shares. The results are similar irrespective of the choice of control group, suggesting that, spillovers are not important in our setting.

Early adoption results. The results for early adoptions are shown in Appendix Figure B.9. For bottom 90% shares, the results are qualitatively and quantitavily similar to the results for late adopters. For top 1% income shares and incomes, the pooled estimates are statistically significant at the 5% level but appear to be driven by notably differential pre-trends.

Alternative estimators. In Appendix Figure B.10, we show that our simple OLS event studies are robust to using the estimators from de Chaisemartin and D'Haultfoeuille (2020) and Callaway and Sant'Anna (2021) and to the exclusion of all controls. These estimators yield coefficients and standard errors similar to those shown in Figure 4.

Gini Index. Figure B.11 explores the effect of tax adoptions on the overall level of income inequality, using each state's Gini index as the outcome variable. For the earlier wave of tax adoptions, we see a zero or a net *increase* in inequality, where point estimates are negative during the first 5 years and positive afterwards. For the later wave of adoptions, we see a zero or a small decrease in the Gini index, although the effect is small (less than 3%) and not statistically significant.

5 How Do Large Tax Changes Affect Inequality?

In this section, we study the effect of personal and/or corporate income tax rate changes on pre-tax income inequality. Because tax changes are numerous and often small, we focus on large tax changes, with an average magnitude of 1.5-2 percentage points. While these changes generate less variation in rates than tax adoptions, they allow us to estimate the effects of personal and corporate income tax changes separately, as these events do not always go hand in hand (though frequently they do, see Robinson and Tazhitdinova, 2025).

5.1 Large Tax Changes: Empirical Approach

We employ an event study approach around tax rate changes that are greater than 1 percentage point in magnitude. These represent the largest 50% of tax changes for both personal and corporate income taxes (see Figure A.6). We estimate these event studies separately for tax increases and tax decreases, and separately for top personal and top corporate income tax rates using a stacked difference-in-differences (DID) specification.

Stacked DID approaches (like those used in Callison and Kaestner, 2014; Cengiz et al., 2019; Deshpande and Li, 2019; Butters et al., 2022) avoid including already-treated units as an implicit part of the control group when the treatment times are staggered, while also allowing us to use multiple tax changes per state as identifying variation.[15] We use the

[15]Much of the literature on alternative DID estimators, including the alternative estimators used above in Section 4, has focused on settings where units are treated a maximum of once (e.g., state adoptions of a

specification including sample weights proposed by Wing et al. (2024), who show that the resulting estimator identifies an aggregate average treatment effect on the treated.

The basic idea in a stacked DID approach is to individually identify "sub-experiments," which include units treated in the same year, plus clean control units that are not treated in the years shortly before and after the treatment year. In our setting, each sub-experiment contains one or more treatment states that experience a large tax change (greater than 1pp), plus one or more control states that do not experience any tax changes greater than 0.25pp during the 6 years before and 5 years after the tax change event.[16]

We also ensure that the pre-treatment years are not part of a post-treatment period for an earlier tax change, thus effectively requiring no tax changes in the 10 years preceding the tax change studied.[17] As long as the above conditions are satisfied, states may be included in multiple sub-experiments. To avoid the compositional bias discussed by Wing et al. (2024), we require a balanced panel in event time. Because each sub-experiment requires a balanced control group over 11 years, allowing this group to make small tax changes (up to 0.25pp) substantially increases the number of possible sub-experiments, even though such changes are only about 10% of all tax changes (see Figure A.6).

These sub-experiments are then "stacked" together by event time, such that all treated units are treated in period 0 and all control units are not treated within the event window. We create such stacks separately for personal income tax increases and decreases, and for corporate income tax increases and decreases. For each stack, the treated units only experience a respective tax change (i.e., personal *or* corporate income tax change) while the control units experience no tax changes (neither personal nor corporate) larger than 0.25pp.

policy), whereas states frequently change their tax rates.

[16]We use a pre-period of 6 years so that, after excluding the reference period of $t = -1$, we have 5 periods to test for pre-trends and 5 periods to test for treatment effects.

[17]For example, in our preferred specification with six pre-periods and five post-periods, to be included in the "2000 sub-experiment" treatment group, states must experience a large tax change (greater than 1pp) in 2000, and not have experienced any tax changes (greater than 0.25pp) from 1990-1999. A previous large tax change in 1989 is the latest possible because 1989-1993 would be the post-period for that episode, leaving 1994-1999 as a clean pre-period for the 2000 tax change. The control group includes all states that did not experience any tax changes (greater than 0.25pp) in 1990-2004. For this sub-experiment, the year 2000 (year of treatment for the treated states) will correspond to period 0 for both treated and control states.

Then, DID and event study specifications can be estimated on this stacked dataset, without raising the staggered treatment timing issue referenced above, using the equation:

$$\ln(Outcome_{sta}) = \sum_{\substack{k=-6 \\ k\neq -1}}^{4} \beta_k \; \mathbb{1}\{t = k\} \; Treat_{sa} + X'_{st}\gamma + \theta \; Treat_{sa} + \eta_t + \varepsilon_{sta} \tag{2}$$

where $Outcome_{sta}$ is the inequality outcome in state s, at event time t of sub-experiment a. $Treat_{sa}$ is equal to one if state s experiences a large tax change in sub-experiment a and zero otherwise. As above for tax adoptions, we control for a small set of time-varying state characteristics X_{st}: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). The coefficient θ controls for cross-sectional differences in income inequality between treated and control states, and η_t are event time fixed effects, which control for idiosyncratic time trends. The coefficients of interest, β_k, capture the effect of treatment on income inequality in event time k, relative to excluded period –1. We use sample weights

$$Q_{sa} = \begin{cases} 1 & \text{if } \; Treat_{sa} = 1 \\ \frac{N_a^T/N^T}{N_a^C/N^C} & \text{if } \; Treat_{sa} = 0 \end{cases} \tag{3}$$

where N_a^T is the number of states that are treated in sub-experiment a, N^T is the total number of states that are treated across all sub-experiments, and N_a^C and N^C give similar counts for the control groups. Wing et al. (2024) show that this weighted regression is equivalent to estimating an event study for each sub-experiment separately, then averaging the estimates where each sub-experiment is weighted by its share of the treated sample (N_a^T/N^T).[18] We cluster standard errors at the state level, thus allowing for dependence within states, even across sub-experiments.

[18]Furthermore, they show that estimating Equation (2) without the weights in Equation (3) does not in general identify any convex combination of the sub-experiment effects. The same is true for adding *sub-experiment* × *state* and *sub-experiment* × *event time* fixed effects.

Our main analysis uses an event window from time –6 to 4, where a tax change occurs in period 0. The choice of window involves a simple tradeoff – the longer the window, the "cleaner" are the control and treatment units and the better is our ability to pick up medium-run dynamic effects. At the same time, longer windows restrict the sample of "qualified" treatment and control units, thus narrowing the scope of our analysis and reducing power. For this reason, our main results employ a relatively short 5-year post-event window, and we consider a 10-year post-event window in the appendix.

Appendix Figure C.12 summarizes the first stage by showing how tax rates change during the event window. Our event studies measure inequality responses to 1.9pp personal and corporate income tax increases, and decreases of 2.1pp personal and 1.5pp corporate income taxes on average. Thus the tax changes we study are two to three times smaller than tax adoptions studied in Section 4, in addition to the fact that almost all adoption events include both personal and corporate taxes.

5.2 Large Tax Changes: Results

The main results are presented in Figures 6–7 for tax increases and in Figures 8–9 for tax decreases. Throughout, we find no statistically significant changes in inequality outcomes. Even when combining all periods together into a pooled DID estimate, most estimates are statistically insignificant for income shares and for total incomes.

For some tax changes, the coefficients exhibit patterns that may be consistent with tax increases reducing (or tax cuts exacerbating) inequality. Figure 6(c) shows a 2% increase in the bottom 90% income share after a personal income tax increase, where the pooled DID coefficient is statistically significant at the 5% level. The results are noisier for bottom 90% income level in Figure 6(d). Consistently, the pattern in Figures 6(a) and (b) suggest a decrease in the top 1% income share and level, but the estimates are not statistically significant. Similarly, the results in Figure 9 for corporate income tax decreases, while generally not statistically significant, also exhibit patterns that could be consistent with a

rising top 1% income and income share, as well as a reduction in the bottom 90% income share. However, after restricting our sub-experiments as described above, we are left with only two large corporate tax decreases, and so these estimates draw from an especially small treatment group (the remaining figures rely on 8-10 treated states).

For the remaining tax changes, i.e., corporate income tax increases (Figure 7) and personal income tax decreases (Figure 8), coefficient estimates are small, thus suggesting both an economically and statistically insignificant response.

Changes in the sample criteria. Appendix Figures C.13-C.16 use a less stringent criteria for the control states, allowing tax changes of up to 0.5pp during the event window, which expands the number of treated states studied to as high as 15 at a time. The results are qualitatively similar. Point estimates are small and mostly not statistically significant, and/or accompanied by strong pre-trends, with the exception of Figures C.16(a) and (c), where the pooled DID estimates for top 1% and bottom 90% income shares, respectively, are statistically significant and qualitatively consistent with our findings in Figure 9. Appendix Figures C.17-C.19, on the other hand, are more restrictive by considering a longer post-event time window of 10 years. Again the results are qualitatively similar, with the exception of income shares after personal tax decreases in C.19. However, relative to our main results, requiring this longer window shrinks our number of treated states from 8 to 3.

6 How Do Tax Cancellations Affect Inequality?

Next, we study the effect of personal and/or corporate income tax cancellations on inequality. These events provide large changes in tax incentives, 6 percentage points or higher, but are relatively rare. Personal income taxes have been canceled in Alaska in 1980 (while keeping the corporate income tax) and in West Virginia in 1942 (the tax was re-introduced in 1961); the corporate income tax was canceled by Ohio in 2010 (while keeping the personal income tax); and South Dakota canceled both of its personal and corporate income taxes in 1943.

Since we have only four cancellations and several potential control states, we use a synthetic control approach (Abadie and Gardeazabal, 2003; Abadie et al., 2010) to separately study the effect of each tax cancellation on inequality.[19]

6.1 Tax Cancellations: Empirical Approach

The counterfactual we want to estimate is what would have happened if each state had continued to collect, rather than canceling, that particular tax. Thus, for each cancellation, we restrict the synthetic control donor pool to states that were collecting the canceled tax type throughout the period of study.[20] To the extent possible, we use a 20-year pre-period and a 20-year post-period, though this is not always feasible; in particular, South Dakota and West Virginia adopted their personal income taxes less than a decade prior to the cancellation.

The synthetic control is then chosen using the procedure outlined in Abadie and Gardeazabal (2003); Abadie et al. (2010) by identifying a set of weights for the donor pool that minimizes the discrepancy between the weighted average and the treated state for the matching variables in the pre-cancellation years. Our matching variables are the top 0.01%, top 1%, and bottom 90% incomes and income shares in each year. These outcomes are normalized to the last year of the matching period, so that our synthetic control approach matches on trends rather than levels. By matching on all six inequality measures, we ensure that the results for each of our outcomes are derived using the same composition of donor states (i.e., a consistent synthetic control state within any given cancellation). Furthermore, our approach limits the extent of overfitting, since we match on several measures, not just the outcome studied. Our results are similar when, in addition to inequality measures, we also match on other state-level variables such as minimum personal, minimum corporate, sales,

[19] We exclude from our analysis one additional cancellation, where South Carolina canceled its personal income tax in 1919. Our inequality data begins in 1917, and with only 2 pre-periods we cannot both identify and verify a good synthetic control; furthermore, the state readopted the tax in 1922.

[20] This is similar to including an indicator for collecting that type of tax as a matching variable, except we also are ensuring that the *post*-match synthetic control does not include any cancellations (e.g., ensuring that West Virginia is not part of South Dakota's synthetic control).

and gasoline tax rates. In the appendix, we include results matching only on the Gini Index or only on the outcome of interest.

In order to test how well the synthetic control performs, we exclude the last several pre-treatment years from this matching process: five years for Alaska and Ohio, and three years for South Dakota and West Virginia due to shorter available pre-periods. If the treated state and synthetic control continue to evolve similarly in the years after the matching period but before the cancellation, this would provide supportive evidence that the two would have continued to evolve similarly in the absence of a cancellation, i.e., that the synthetic control represents a valid counterfactual.

6.2 Tax Cancellations: Results

Figure 10 shows the results for the top 1% shares and incomes. Each figure plots the normalized top 1% income share or the total income for the treated state, the synthetic control (weighted average of the donor pool) and the simple average of all potential donors. Similar figures for the bottom 90% are available in Appendix Figures D.20–D.23.

The results for Ohio are inconclusive. We see no effect on the top 1% income share and incomes in the five years after the cancellation, and potentially a small decrease in subsequent years. The results for the top 0.01%, top 10-1%, and bottom 90% income shares, as well as top 0.01% incomes also show a delayed increase in inequality after the cancellation. The top 10-1% income appears to increase and the bottom 90% income appears to decrease after the cancellation, but these results are typically a widening of a gap that emerged after the match process and prior to the cancellation, and thus are plausibly driven by poor fit of the synthetic control.

For West Virginia, we see no effect on top 1% income shares, or on bottom 90% income shares. We see a decrease in top 10%-1% and top 0.01% income shares and a decrease in total incomes for all percentiles. However, these decreases appear prior to income tax cancellation, suggesting that the results again are likely to be driven by poor fit rather than actual effects

of tax cancellation. For South Dakota, we see no effect on the top 1% income shares and total incomes, nor for any other outcomes. Consistently, the cancellation of personal and corporate income taxes in South Dakota appears to result in no change in inequality.

Finally, for Alaska, we see a consistent increase in the top 1%, top 0.01% and top 10%-1% income shares and total incomes. For the bottom 90%, we see an increase in total income but a decrease in the income share. Unfortunately, this tax cancellation coincided with the rapid development of the oil industry in Alaska, which makes it difficult to attribute the observed effect to the cancellation of the personal income tax.[21]

Overall, our analysis of tax cancellations once again does not provide robust evidence in favor of a strong relationship between income inequality and taxation.

Alternative matching criteria. Appendix Figures D.24–D.27 match only on the Gini Index in each year. Since the Gini Index only distantly accounts for the values of the outcome studied, this approach is the least likely to overfit. At the other extreme, Appendix Figures D.28–D.31 match only on the outcome itself in each year (e.g., match on top 1% income shares when estimating results for top 1% income shares). This approach produces the best fit in the pre-cancellation test window, but is most prone to over-fitting. The results are qualitatively similar.

7 Using All Tax Rate Variation

In this section, we explore the relationship between personal/corporate income taxes and inequality using the maximum possible sample of all states in all years. This analysis includes all within-state variation in tax rates: adoptions, cancellations, and changes of all magnitudes, with an average variation in tax rates of 0.5-0.6 percentage points.[22] We implement the robust estimator from de Chaisemartin et al. (2025) that is analogous to the

[21]The sharp increase in oil production started in 1977, at which point production roughly doubled for a few years until it reached its peak in late 1980s.

[22]The calculation of this average excludes years when the tax rate did not change, i.e., is the average amount of variation conditional on some variation occurring.

conventional two-way fixed effects (TWFE) specification:

$$\ln(Outcome_{st}) = \tau'_{st}\ \beta + X'_{st}\gamma + \alpha_s + \lambda_t + \varepsilon_{st}, \tag{4}$$

where $Outcome_{st}$ is the inequality outcome in state s and year t, the vector τ_{st} includes personal, corporate, or both tax rates, and X_{st} are a set of controls.[23]

We do not implement the conventional TWFE specification because it is likely to be biased because using continuous tax rates in τ_{st} means that there are few untreated units. Furthermore, a conventional TWFE design does not allow us to test for the presence of pre-trends, so it is unclear to what extent the implicit control group is a plausible counterfactual.

In contrast, the de Chaisemartin et al. (2025) estimator compares the evolution of outcomes in states that changed their tax rate in year t ("switchers"), to states that did not change their tax rate ("stayers"). In this case, the parallel trends assumption is that switchers and stayers with the same $t-1$ tax rate would have experienced the same evolution in outcomes in the absence of any tax rate change. However, the trends may differ across $t-1$ tax rates. Furthermore, the plausibility of this assumption can be tested by comparing the evolution of outcomes from $t-2$ to $t-1$ for the states that will switch versus stay in year t. If switchers and stayers were on parallel trends prior to the tax change, then the results of this placebo test will be close to zero.

Our estimates reflect the average effect of a 1 percentage point increase in the tax rate, weighted by the magnitude of the tax change. Our analysis uses all consecutive pairs of years from 1917 to 2018 where there is at least one state changing its tax rate and one state not implementing a change. Since states frequently change multiple tax rates within the same year (Robinson and Tazhitdinova, 2025), when estimating the effect of the top personal (corporate) tax, we control for the top corporate (personal) tax as an additional

[23] As with tax adoptions and large tax changes, X_{st} controls for a small set of time-varying state characteristics: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years).

treatment. We also include the set of time-varying controls described above, and cluster standard errors at the state level.

Table 2 shows our results for the effect of the personal tax (Panel A) and the corporate tax (Panel B) on income shares.[24] Overall, we find little evidence that changes in these tax rates affected top or bottom income shares. None of the estimates are statistically significant, and, with the exception of one coefficient, the point estimates are close to zero. Point estimates imply changes in income shares of 0.7% or less for a 1pp increase in personal or corporate rate. In Table 3, we turn to the effect on total incomes. Similarly, with the exception of one coefficient, the results are neither statistically nor economically significant. Again, point estimates imply changes in income of 0.76% or less for a 1pp increase in personal or corporate rate. Again, our placebo tests show no evidence of pre-trends. Thus, TWFE estimates are qualitatively and quantitatively equivalent to our tax adoptions and large tax change results.

Using the robust estimator from de Chaisemartin et al. (2025) additionally allows us to estimate effects separately for tax changes of different magnitudes. In Appendix Figures E.32-E.35, we show effects and corresponding placebo tests for each quintile of tax change in absolute value. The estimated effects for larger tax changes (third, fourth, and fifth quintiles) are close to zero with small confidence intervals. The associated placebo tests are also precise nulls, providing evidence that these tax changes are plausibly exogenous to the inequality outcomes studied. On the other hand, the effects for the first and second quintile of tax changes (under 0.7 percentage points for personal taxes and 0.75 percentage points for corporate) are much less precise. While some of these point estimates suggest sizable effects, they are mostly not statistically distinguishable from zero, and are similar or smaller in magnitude than the placebo estimates. Taken together, these results suggest that to the

[24]The conventional TWFE are shown in Appendix Table E.1 and imply a statistically significant negative effect of the corporate tax on top shares and incomes. If this conventional specification yielded estimates of similar magnitude, then a potential concern would be that our robust estimator results are merely underpowered. However, the conventional specification yields much larger point estimates, and similarly sized standard errors, suggesting that the difference between the two specifications is primarily due to bias.

extent tax changes correspond to changes in income inequality, these associations are driven by small tax changes, made at times when the comparison group is a poor counterfactual. Excluding these in favor of larger tax changes with more plausible comparison groups would generate even more precise nulls than those reported in Tables 2 & 3.

8 Interpretation of the Results and Conclusion

Sections 4–7 provide empirical evidence on how various measures of pre-tax income inequality respond to tax adoptions, tax cancellations, and tax changes. Our empirical results are summarized in Panel B of Table 1.[25] We generally find a statistically insignificant relationship between inequality and tax measures, with point estimates close to zero. Some point estimates do suggest that higher income taxes may reduce top income shares and possibly income levels. Towards the bottom of the distribution, the evidence is mostly consistent with null effects. Considering the fact that the likely presence of spillovers would further bias our estimates away from zero, our results suggest that the relationship between state-level inequality and state taxes is null or very weak at best.

One plausible explanation for our null result is that our analysis is simply underpowered, and we are not able to establish a statistically significant relationship, despite its existence. Finding directionally consistent point estimates across our specifications would have been suggestive evidence of this; however, it is certainly possible for the lack of power to result in point estimates of the wrong sign.

An alternative explanation is that inequality responds to tax changes but only to sufficiently large ones. For example, if the tax-inequality relationship is primarily driven by reduced work efforts, then our conclusions are broadly consistent with the vast literatures that document weak labor supply responses to taxes (Keane, 2011; Saez et al., 2012; McClelland and Mok, 2012; Neisser, 2021). On the other hand, if the tax-inequality relationship is

[25] In Table 1, we label a result as an increase/decrease when it is statistically significant (at least at the 10% level) during the first 5 years, or when it is not statistically significant but its magnitude exceeds 5%.

mainly driven by changes of bargaining power, then the small changes in state income taxes may not be sufficient to change bargaining powers, and hence, affect inequality. While we have focused on the largest changes in state taxes, state income taxes overall are significantly smaller than their federal counterparts. The mean personal and corporate income taxes for states has never exceeded 10 percentage points; meanwhile, the top personal tax rate at the federal level has ranged from 25 to 94pp, and the top corporate tax rate from 6 to 53 pp (Figure A.1). Overall, our empirical evidence suggests that state income taxes may not be large enough to affect income inequality, and that changes at the federal level may be required.

Overall, our empirical findings indicate no systematic relationship between state-level income and corporate taxes and inequality; however, these results should not be interpreted as evidence implying lack of such relationship at the federal level.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller, "Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program," *Journal of the American statistical Association*, 2010, *105* (490), 493–505.

_ **and Javier Gardeazabal**, "The economic costs of conflict: A case study of the Basque Country," *American economic review*, 2003, *93* (1), 113–132.

Bailey, Beulah, "Review of Tax Legislation, 1929," *The Bulletin of the National Tax Association*, 1929, *15* (3), 75–80.

Bargain, Olivier, Mathias Dolls, Herwig Immervoll, Dirk Neumann, Andreas Peichl, Nico Pestel, and Sebastian Siegloch, "Tax policy and income inequality in the United States, 1979–2007," *Economic Inquiry*, 2015, *53* (2), 1061–1085.

Berry, Frances Stokes and William D. Berry, "Tax Innovation in the States: Capitalizing on Political Opportunity," *American Journal of Political Science*, 1992, *36* (3), 715–742. Publisher: [Midwest Political Science Association, Wiley].

Blanchet, Thomas, Lucas Chancel, and Amory Gethin, "Why Is Europe More Equal than the United States?," *American Economic Journal: Applied Economics*, October 2022, *14* (4), 480–518.

Bozio, Antoine, Bertrand Garbinti, Jonathan Goupille-Lebret, Malka Guillot, and Thomas Piketty, "Predistribution versus Redistribution: Evidence from France and the United States," *American Economic Journal: Applied Economics*, April 2024, *16* (2), 31–65.

Butters, R. Andrew, Daniel W. Sacks, and Boyoung Seo, "How Do National Firms Respond to Local Cost Shocks?," *American Economic Review*, May 2022, *112* (5), 1737–1772.

Callaway, Brantly and Pedro H. C. Sant'Anna, "Difference-in-Differences with multiple time periods," *Journal of Econometrics*, December 2021, *225* (2), 200–230.

_ , Andrew Goodman-Bacon, and Pedro H. C. Sant'Anna, "Difference-in-Differences with a Continuous Treatment," arXiv Working Paper 2107.02637 July 2021.

Callison, Kevin and Robert Kaestner, "Do Higher Tobacco Taxes Reduce Adult Smoking? New Evidence Of The Effect Of Recent Cigarette Tax Increases On Adult Smoking," *Economic Inquiry*, 2014, *52* (1), 155–172. Publisher: Western Economic Association International.

Cassidy, Traviss, Mark Dincecco, and Ugo Antonio Troiano, "The introduction of the income tax, fiscal capacity, and migration: evidence from US States," *American Economic Journal: Economic Policy*, 2024, *16* (1), 359–393.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer, "The Effect of Minimum Wages on Low-Wage Jobs," *The Quarterly Journal of Economics*, August 2019, *134* (3), 1405–1454.

Cooper, Daniel H, Byron F Lutz, and Michael G Palumbo, "The role of taxes in mitigating income inequality across the US states," *National Tax Journal*, 2015, *68* (4), 943–974.

de Chaisemartin, Clément and Xavier D'Haultfoeuille, "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects," *American Economic Review*, September 2020, *110* (9), 2964–2996.

_ and _ , "Two-Way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey," SSRN Working Paper 3980758 December 2021.

_ , _ , **Félix Pasquier, Doulo Sow, and Gonzalo Vazquez-Bare**, "Difference-in-Differences for Continuous Treatments and Instruments with Stayers," SSRN Working Paper 4011782 August 2025.

Deshpande, Manasi and Yue Li, "Who Is Screened Out? Application Costs and the Targeting of Disability Programs," *American Economic Journal: Economic Policy*, November 2019, *11* (4), 213–248.

Diamond, Rebecca and Enrico Moretti, "Where is Standard of Living the Highest? Local Prices and the Geography of Consumption," Working Paper 29533, National Bureau of Economic Research December 2021.

Duncan, Denvil and Klara Sabirianova Peter, "Unequal inequalities: Do progressive taxes reduce income inequality?," *International Tax and Public Finance*, 2016, *23*, 762–783.

Feldstein, Martin and Marian Vaillant Wrobel, "Can state taxes redistribute income?," *Journal of Public Economics*, 1998, *68* (3), 369–396.

Frank, Mark, Estelle Sommeiller, Mark Price, and Emmanuel Saez, "Frank-Sommeiller-Price series for top income shares by US states since 1917," WTID Methodological Notes 2015.

Fuest, Clemens, Andreas Peichl, and Sebastian Siegloch, "Do higher corporate taxes reduce wages? Micro evidence from Germany," *American Economic Review*, 2018, *108* (2), 393–418.

Giroud, Xavier and Joshua Rauh, "State taxation and the reallocation of business activity: Evidence from establishment-level data," *Journal of Political Economy*, 2019, *127* (3), 1262–1316.

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, December 2021, *225* (2), 254–277.

Gruber, Jon and Emmanuel Saez, "The elasticity of taxable income: evidence and implications," *Journal of Public Economics*, 2002, *84* (1), 1–32.

Hanlon, Michelle, Jeffrey L Hoopes, and Joel Slemrod, "Tax reform made me do it!," *Tax Policy and the Economy*, 2019, *33* (1), 33–80.

Jones, Charles I., "Pareto and Piketty: The Macroeconomics of Top Income and Wealth Inequality," *Journal of Economic Perspectives*, February 2015, *29* (1), 29–46.

Keane, Michael P, "Labor supply and taxes: A survey," *Journal of Economic Literature*, 2011, *49* (4), 961–1075.

Kennedy, Patrick, Christine Dobridge, Paul Landefeld, and Jake Mortenson, "The efficiency-equity tradeoff of the corporate income tax: Evidence from the Tax Cuts and Jobs Act," *Unpublished manuscript*, 2022.

Leigh, Andrew, "Do redistributive state taxes reduce inequality?," *National Tax Journal*, 2008, *61* (1), 81–104.

Limberg, Julian, "Taxation and inequality," in "Handbook on the Politics of Taxation," Edward Elgar Publishing, 2021, pp. 178–191.

Marti, Samira, Isabel Z Martínez, and Florian Scheuer, "Does a progressive wealth tax reduce top wealth inequality? Evidence from Switzerland," *Oxford Review of Economic Policy*, 08 2023, *39* (3), 513–529.

McClelland, Robert and Shannon Mok, "A review of recent research on labor supply elasticities," 2012.

Nallareddy, Suresh, Ethan Rouen, and Juan Carlos Suárez Serrato, "Do corporate tax cuts increase income inequality?," *Tax Policy and the Economy*, 2022, *36* (1), 35–91.

Neisser, Carina, "The elasticity of taxable income: A meta-regression analysis," *The Economic Journal*, 2021, *131* (640), 3365–3391.

Penniman, Clara, *State Income Taxation*, Baltimore: Johns Hopkins University Press, 1980.

Piketty, Thomas and Emmanuel Saez, "Income Inequality in the United States, 1913–1998," *The Quarterly Journal of Economics*, February 2003, *118* (1), 1–41.

_ , _ , and Stefanie Stantcheva, "Optimal Taxation of Top Labor Incomes: A Tale of Three Elasticities," *American Economic Journal: Economic Policy*, 2014, *6* (1), 230–271.

Robinson, Sarah and Alisa Tazhitdinova, "What Drives Tax Policy? Political, Institutional and Economic Determinants of State Tax Policy in the Past 70 Years," SSRN Working Paper 4035979 February 2022.

_ and _ , "One hundred years of U.S. state taxation," *Journal of Public Economics*, 2025, *241*, 105273.

Romer, Christina D. and David H. Romer, "The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks," *American Economic Review*, June 2010, *100* (3), 763–801.

Rubolino, Enrico and Daniel Waldenström, "Tax progressivity and top incomes evidence from tax reforms," *The Journal of Economic Inequality*, 2020, *18*, 261–289.

Saez, Emmanuel, "Reported Incomes and Marginal Tax Rates, 1960-2000: Evidence and Policy Implications," *Tax Policy and the Economy*, 2004, *18*, 117–173.

_ , Joel Slemrod, and Seth H Giertz, "The elasticity of taxable income with respect to marginal tax rates: A critical review," *Journal of economic literature*, 2012, *50* (1), 3–50.

Serrato, Juan Carlos Suárez and Owen Zidar, "Who benefits from state corporate tax cuts? A local labor markets approach with heterogeneous firms," *American Economic Review*, 2016, *106* (9), 2582–2624.

Sokoloff, Kenneth L and Eric M Zolt, "Inequality and taxation: Evidence from the Americas on how inequality may influence tax institutions," *Tax L. Rev.*, 2005, *59*, 167.

Sommeiller, Estelle and Mark Price, "The new gilded age: Income inequality in the US by state, metropolitan area, and county," 2018. Economic Policy Institute.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, December 2021, *225* (2), 175–199.

Tazhitdinova, Alisa and Gonzalo Vazquez-Bare, "Difference-in-Differences with Unequal Baseline Treatment Status," NBER Working Paper 31063 2023.

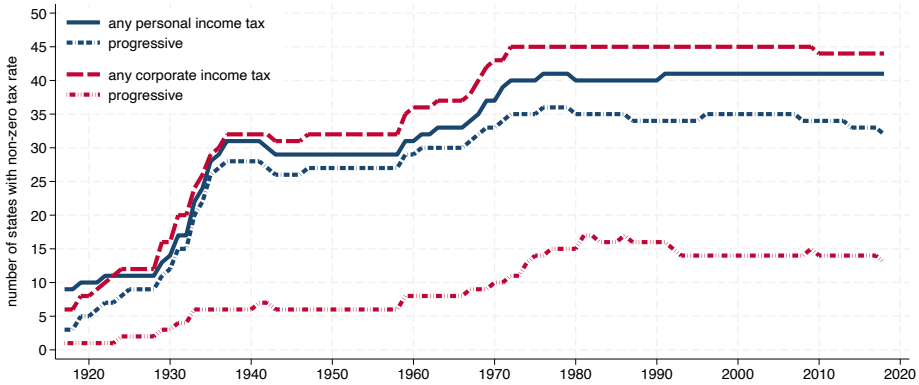
Troiano, Ugo, "Do taxes increase economic inequality? A comparative study based on the state personal income tax," 2018.

Wing, Coady, Seth M. Freedman, and Alex Hollingsworth, "Stacked Difference-in-Differences," NBER Working Paper 32054 2024.

Yagan, Danny, "Employment hysteresis from the great recession," *Journal of Political Economy*, 2019, *127* (5), 2505–2558.

Figure 1: State Tax Policy Variation

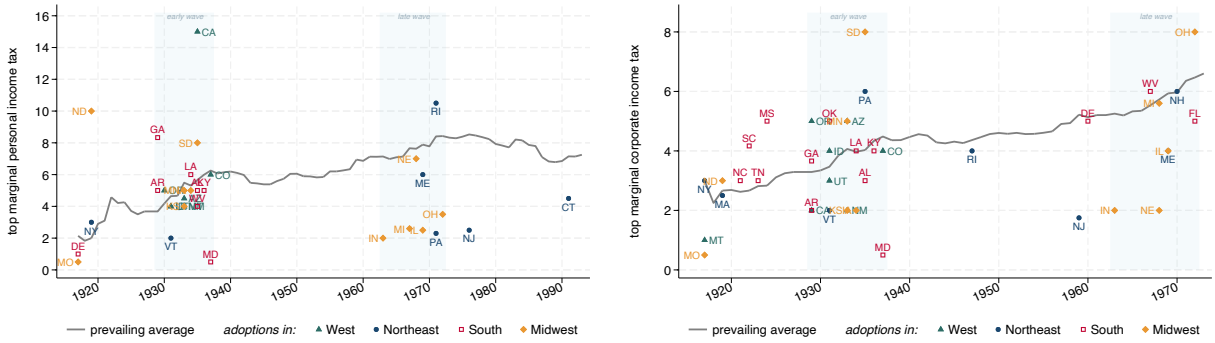
Panel A: States with Personal and Corporate Income Taxes



Panel B: Tax Rates at First Adoption

(a) Top Personal Income Tax

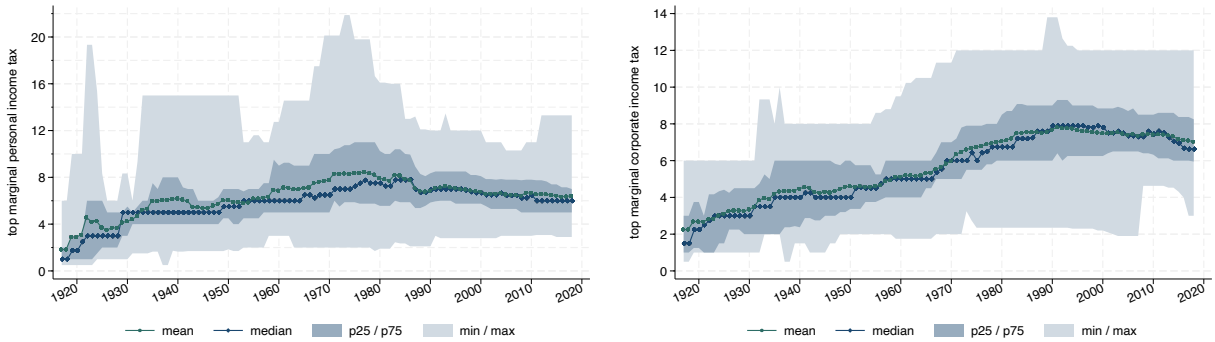
(b) Top Corporate Income Tax



Panel C: Tax Rates Over Time

(c) Top Personal Income Tax

(d) Top Corporate Income Tax



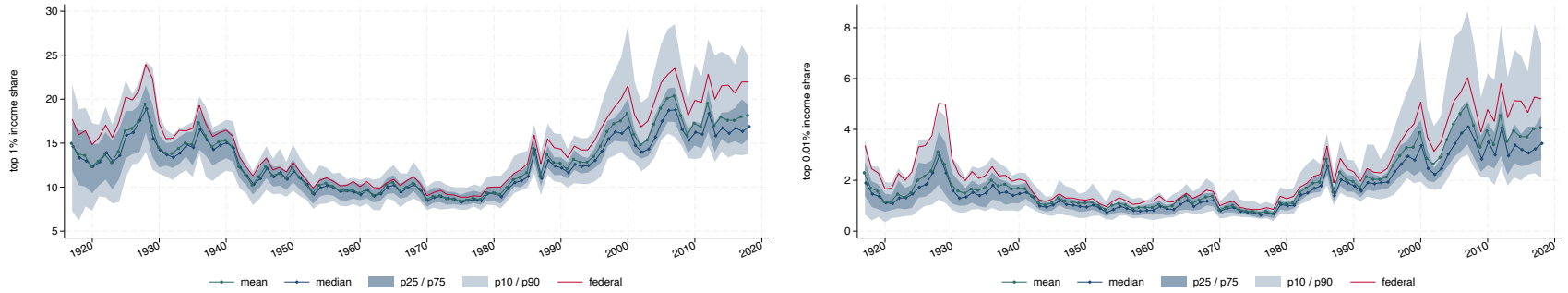
Notes: Panel A shows the number of states with personal and corporate income taxes, as well as the number of states with progressive tax systems for each type. Panel B show the tax rate in the year the tax was first adopted by each state, as well as the prevailing average tax rate at the time (excluding states that first adopted the tax in that year). The shaded years mark the early and late adoption waves. Panel C shows the average, median, 25th and 75th percentiles, minimum, and maximum of state top personal income and top corporate income tax rates. Throughout, only non-zero tax rates are included.

Figure 2: State Inequality Variation

Panel A: Inequality Over Time

(a) Top 1% Income Share

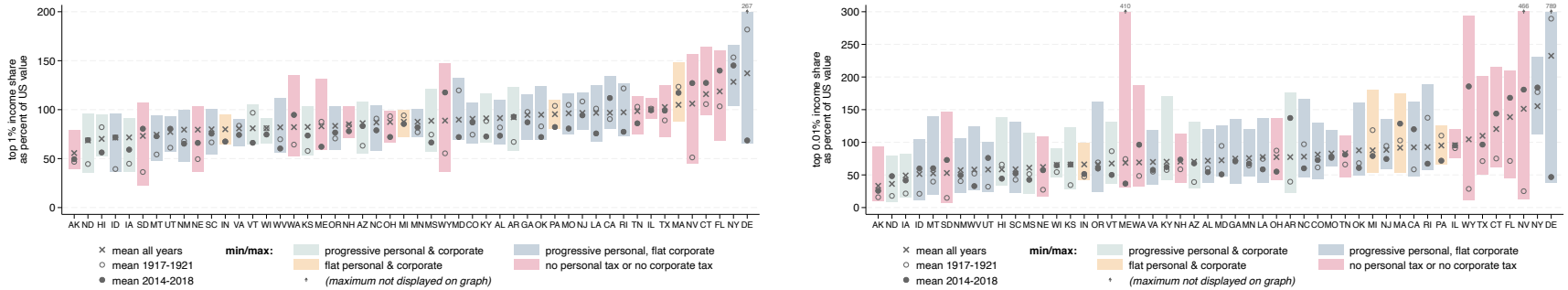
(b) Top 0.01% Income Share



Panel B: Inequality by State

(c) Top 1% Income Share (as Percent of U.S.)

(d) Top 0.01% Income Share (as Percent of U.S.)

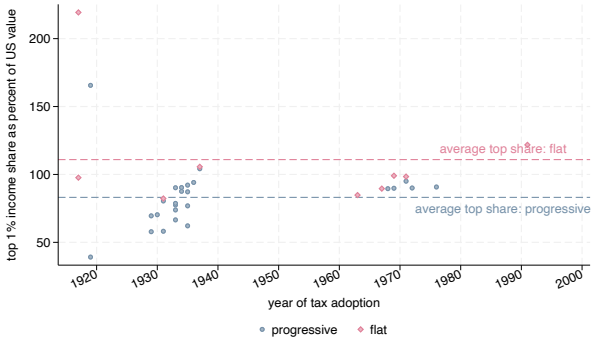


Notes: Panel A shows the average, median, and 10th, 25th, 75th, and 90th percentiles of the top 1% and top 0.01% income shares for all U.S. states over time, as well as the equivalent U.S.-wide shares. Panel B shows, for each state, the minimum and maximum in the top 1% and top 0.01% income shares as a percent of the equivalent U.S.-wide share over time. The mean income shares are also displayed over all years, over the last five years, and over the first five years available (1917-1921, except for AK and HI as 1959-1963). The ranges are colored based on the *modal* tax policy for each state over all years: whether the state collects personal income and corporate taxes, and whether those tax rates are progressive or flat. See the Appendix for income shares as a percent of the U.S.-wide share over time (Figure A.2), the Gini Index over time (A.3), a version of Panel B categorizing states by their 2018 (rather than modal) tax policy (A.4), and all state-specific time series (A.5).

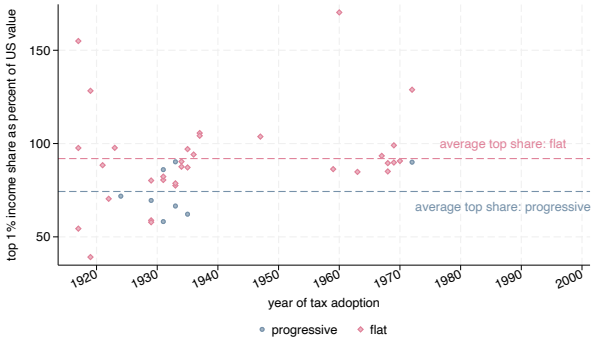
Figure 3: Inequality and Tax Policy

Panel A: Tax Adoptions

(a) Personal, Top 1% share

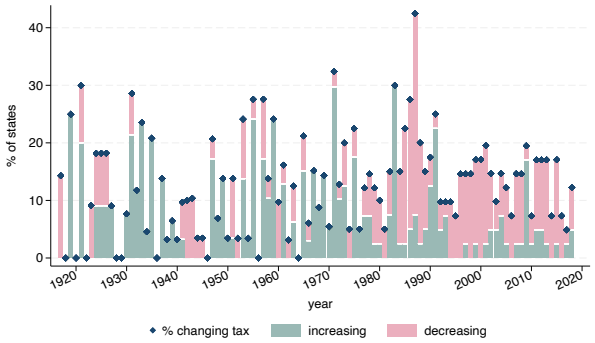


(b) Corporate, Top 1% share

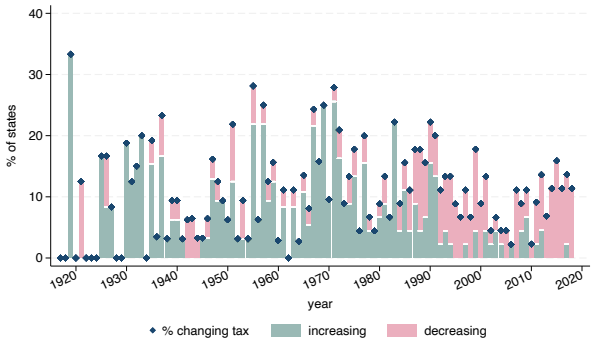


Panel B: Tax Changes

(c) Personal Income Tax Changes



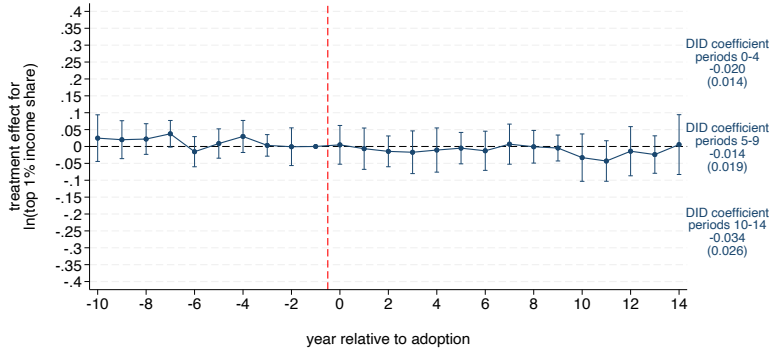
(d) Corporate Income Tax Changes



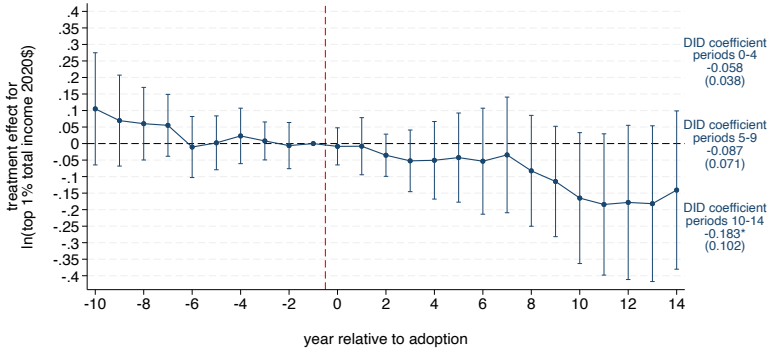
Notes: Figures (a) and (b) show top 1% income shares in the year the state adopts personal or corporate income tax. Each point represents a state. States that adopted a progressive tax scheme are shown in blue and states that adopted flat tax schemes are shown in pink. Finally, the horizontal blue and pink dashed lines represent the average top income shares for states that adopted progressive and flat income tax schemes, respectively, when the tax was initially adopted. Figures (c) and (d) show the percent of states that change a given tax rate in a given year (scatter points), increase it (green bars), or decrease it (pink bars). Only states with non-zero tax rates are included, and only intensive margin tax changes are included (tax adoptions and cancellations are excluded).

Figure 4: Effect of Late (1963-1972) Tax Adoptions on Log(Income Shares) and Log(Income) with Never-Adopters as Control Group

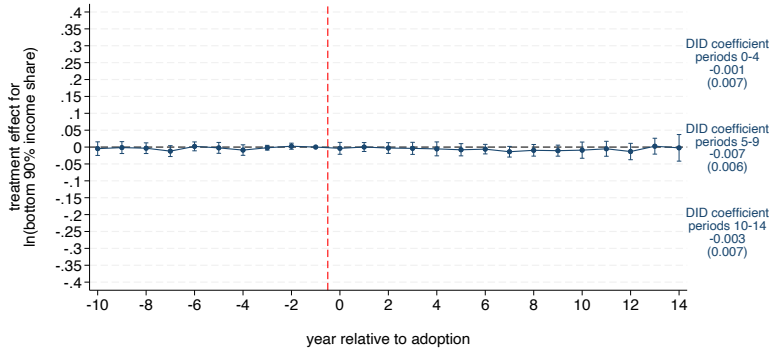
(a) Top 1% Income Share



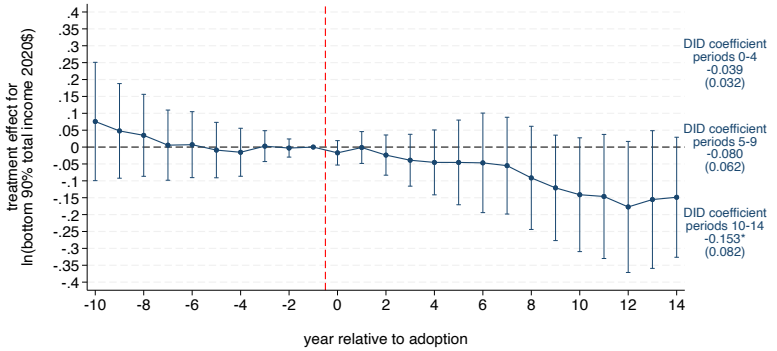
(b) Top 1% Income



(c) Bottom 90% Income Share



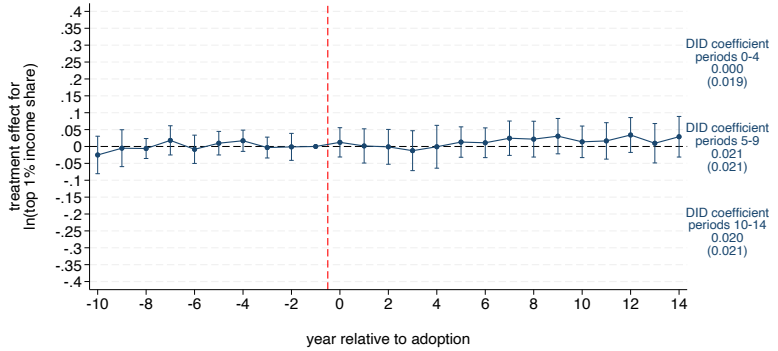
(d) Bottom 90% Income



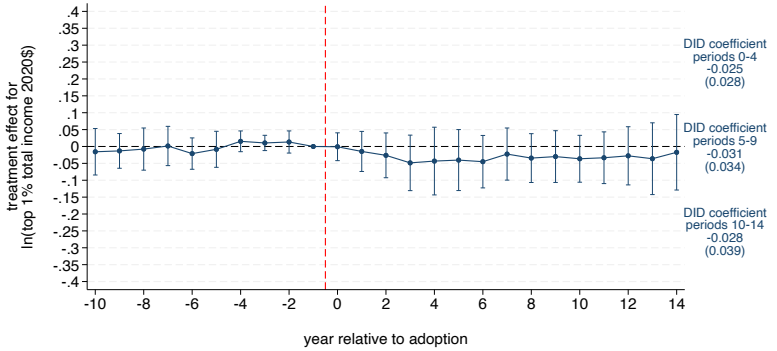
Notes: These figures show the results of estimating Equation (1). The outcome variable is the logarithm of income shares or income levels. Event time 0 corresponds to the year that the state adopted both personal and corporate income taxes, the year of the first adoption if the second adoption occurred the following year, or the year of adoption for the corporate income tax if the state never adopted a personal income tax. Tax rate changes are shown in Appendix Figure B.8. The sample includes states that adopted taxes in 1963-1972 (treated), as well as states that never adopted either tax (controls). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported for periods 0-4, 5-9, and 10-14, with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$. See Figure B.10 for alternative estimators.

Figure 5: Effect of Late (1963-1972) Tax Adoptions on Log(Income Shares) and Log(Income) with Pre-1927 Adopters as Control Group

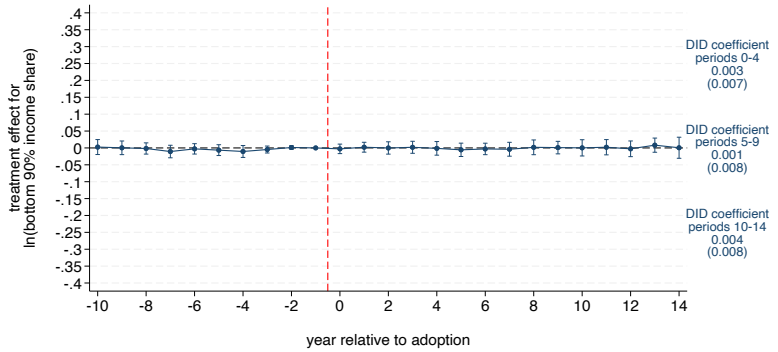
(a) Top 1% Income Share



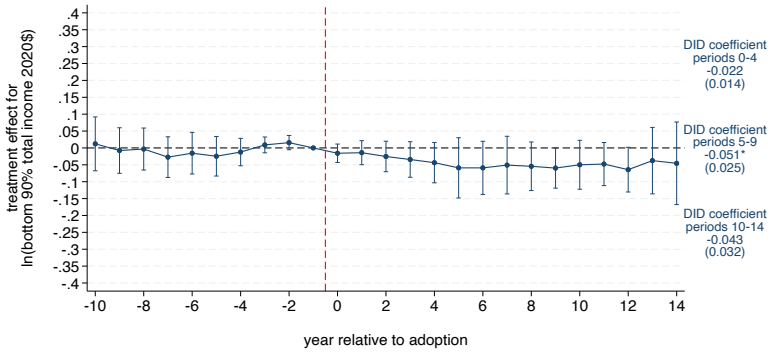
(b) Top 1% Income



(c) Bottom 90% Income Share

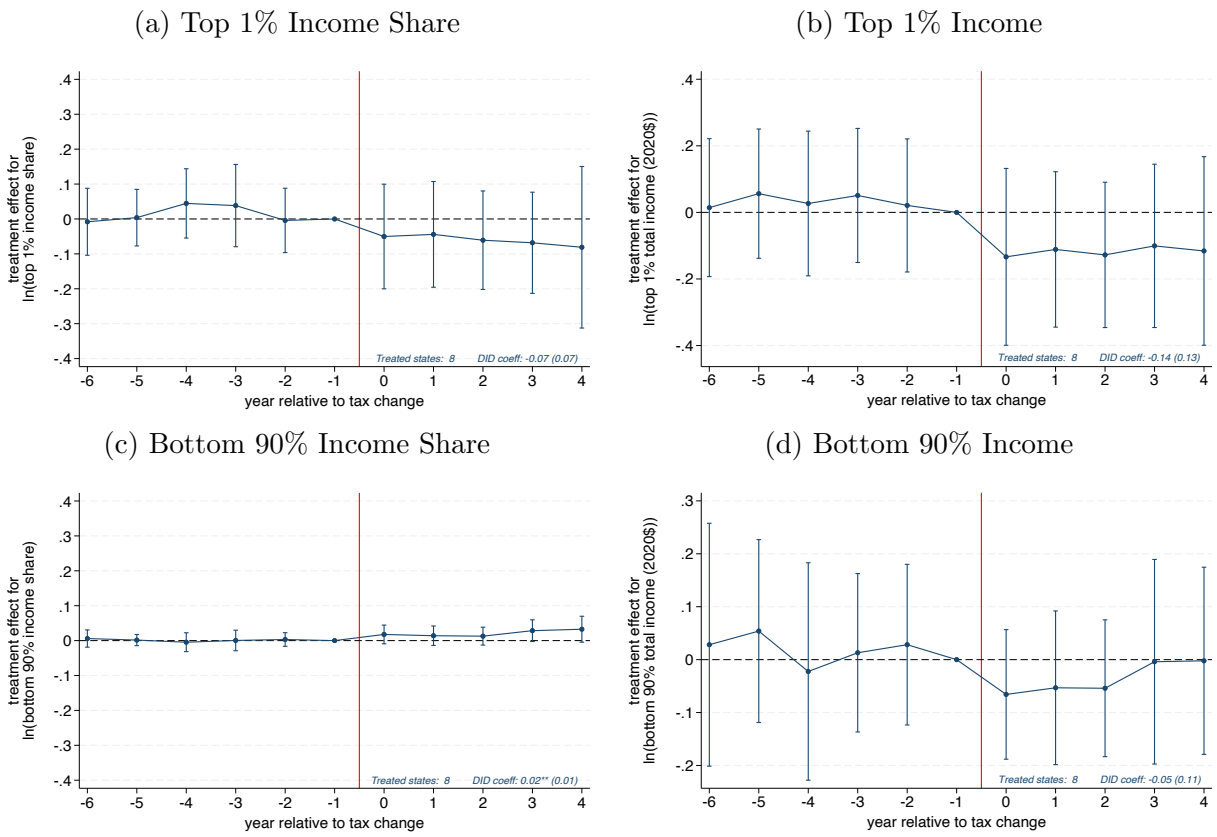


(d) Bottom 90% Income



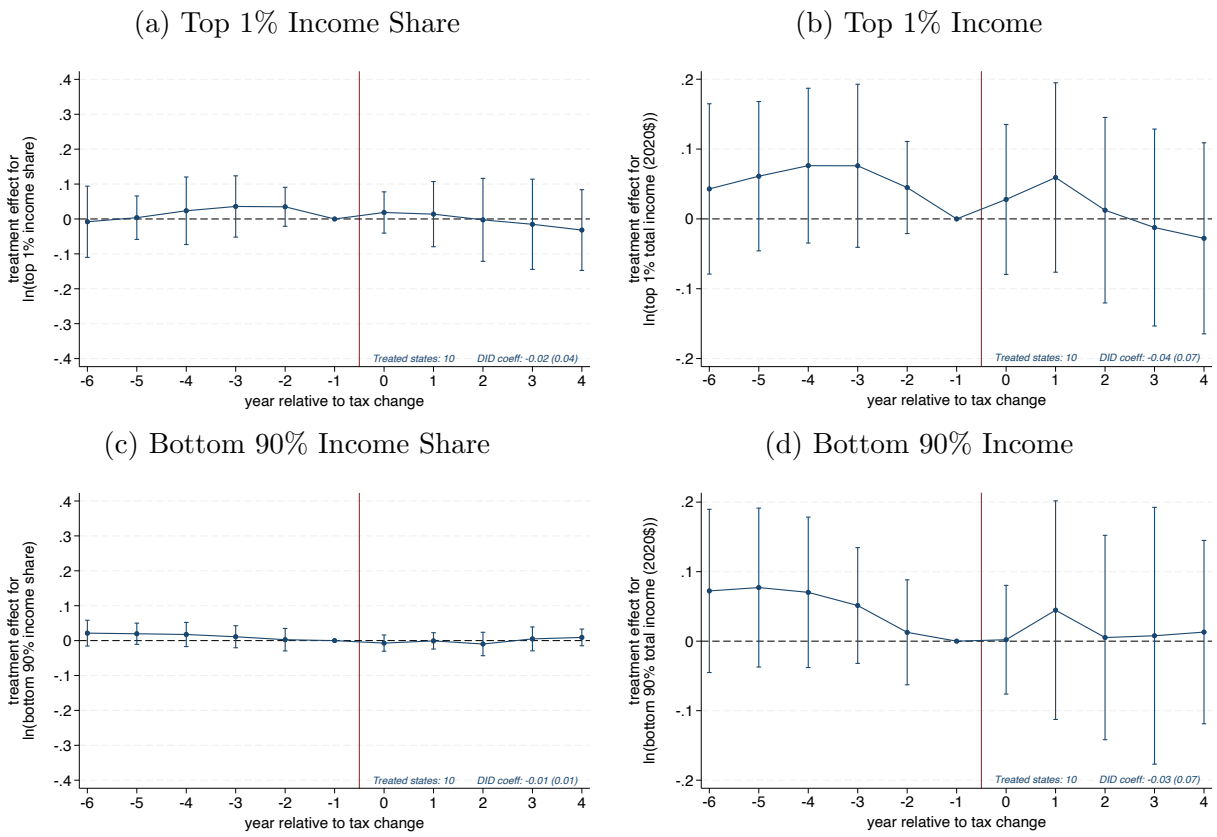
Notes: These figures show the results of estimating Equation (1). The outcome variable is the logarithm of income shares. Event time 0 corresponds to the year that the state adopted both personal and corporate income taxes, the year of the first adoption if the second adoption occurred the following year, or the year of adoption for the corporate income tax if the state never adopted a personal income tax. The sample includes states that adopted income taxes in 1963-1972, and the states that adopted income taxes prior to 1927 and did not adopt any income taxes since then acting as controls. The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported for periods 0-4, 5-9, and 10-14, with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Figure 6: Effect of Personal Income Tax Increases on Log(Income Shares) and Log(Income)



Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states increased the personal income tax. The sample includes states that increased the personal income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.25pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$. See Figure C.12 for the corresponding tax rate changes.

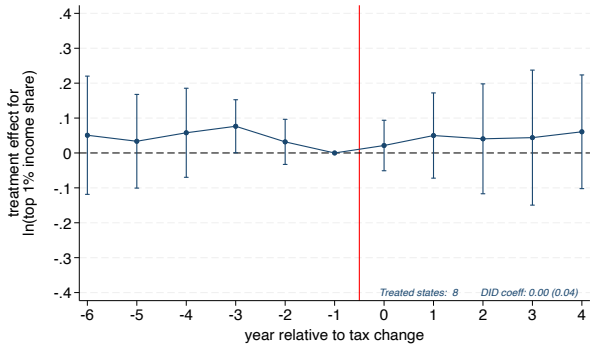
Figure 7: Effect of Corporate Tax Increases on Log(Income Shares) and Log(Income)



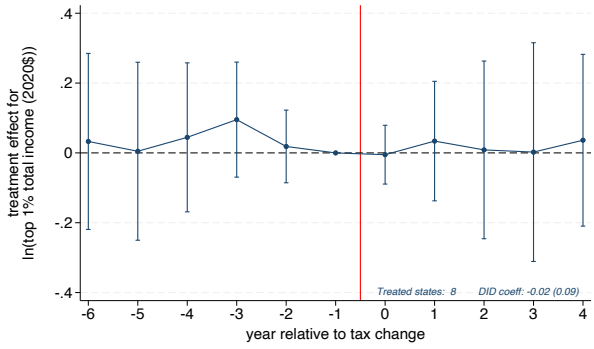
Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states increased the corporate income tax. The sample includes states that increased the corporate income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.25pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$. See Figure C.12 for the corresponding tax rate changes.

Figure 8: Effect of Personal Tax Decreases on Log (Income Shares) and Log(Income)

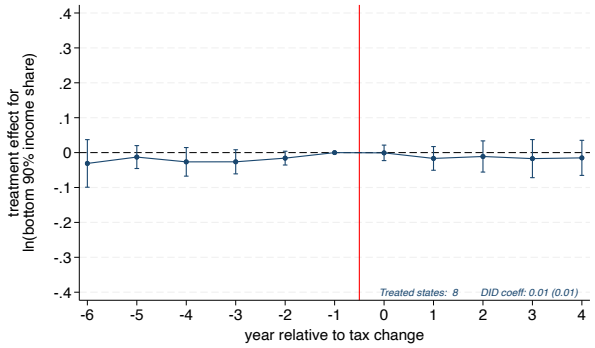
(a) Top 1% Income Share



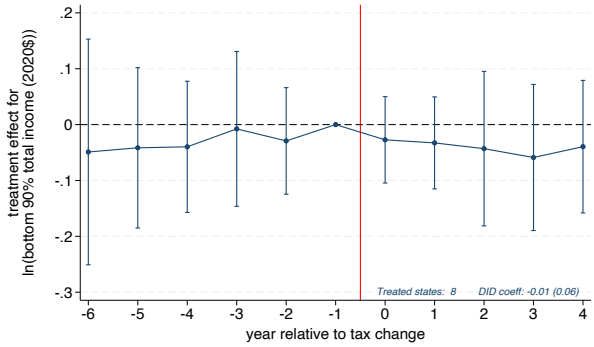
(b) Top 1% Income



(c) Bottom 90% Income Share

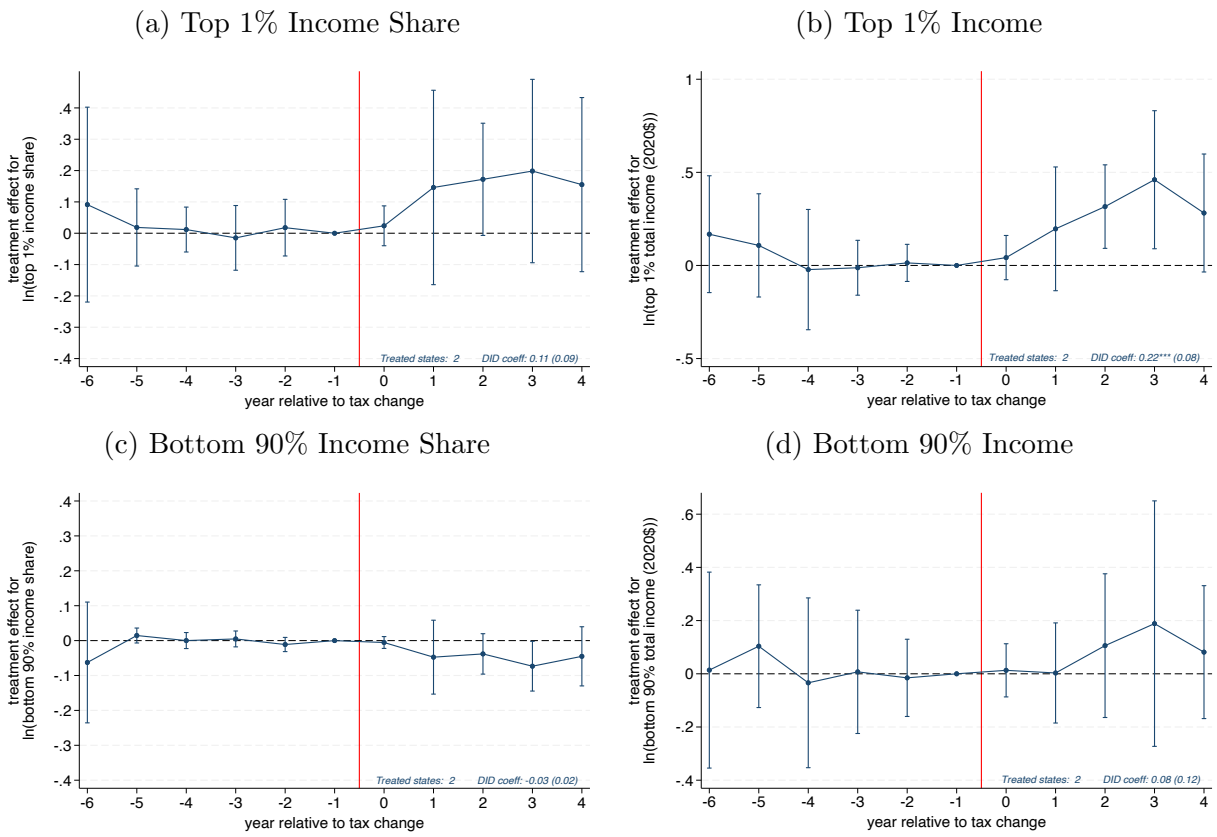


(d) Bottom 90% Income



Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states decreased the personal income tax. The sample includes states that decreased the personal income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.25pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$. See Figure C.12 for the corresponding tax rate changes.

Figure 9: Effect of Corporate Tax Decreases on Log(Income Shares) and Log(Income)

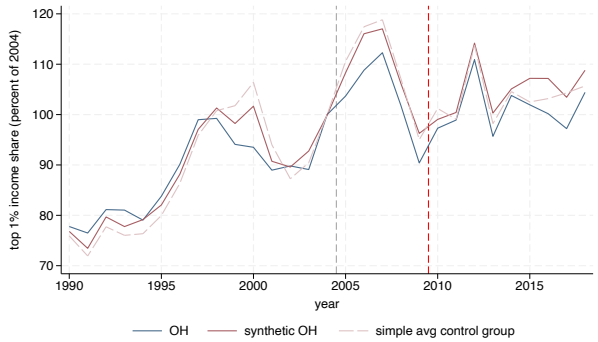


Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states decreased the corporate income tax. The sample includes states that decreased the corporate income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.25pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$. See Figure C.12 for the corresponding tax rate changes.

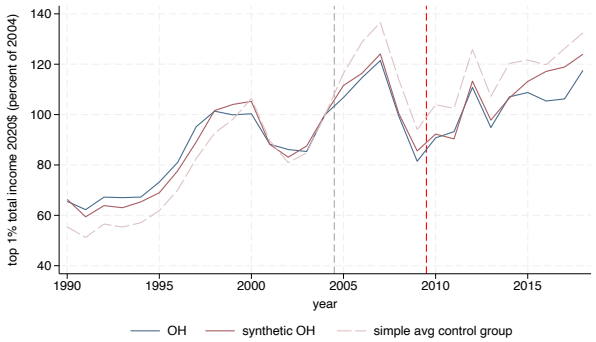
Figure 10: Effect of Tax Cancellations on Top 1% Income Shares and Income (1/2)

Ohio canceled $\tau^{corporate} = 8.5\%$, kept $\tau^{personal} = 6.24\%$ in 2010

(a) Top 1% Income Share

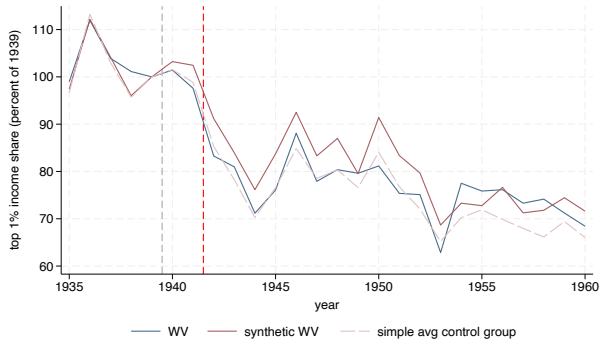


(b) Top 1% Income

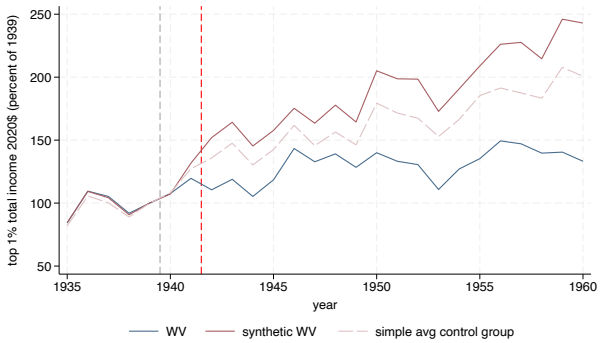


West Virginia canceled $\tau^{personal} = 6\%$, kept $\tau^{corporate} = 0\%$ in 1942

(c) Top 1% Income Share

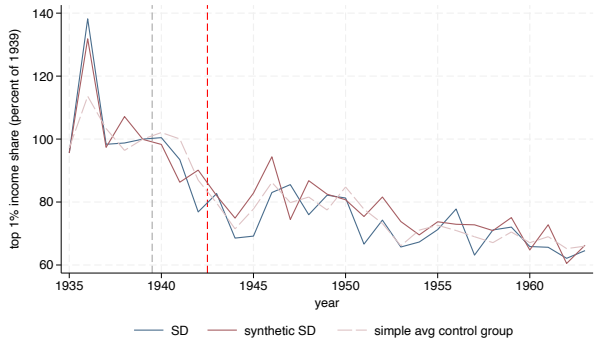


(d) Top 1% Income

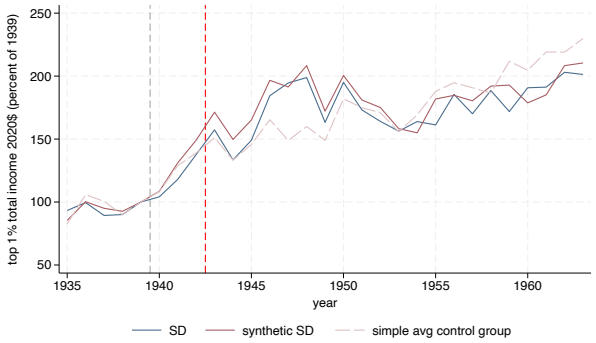


South Dakota: canceled $\tau^{personal} = 6\%$ and $\tau^{corporate} = 8\%$ in 1943

(e) Top 1% Income Share



(f) Top 1% Income

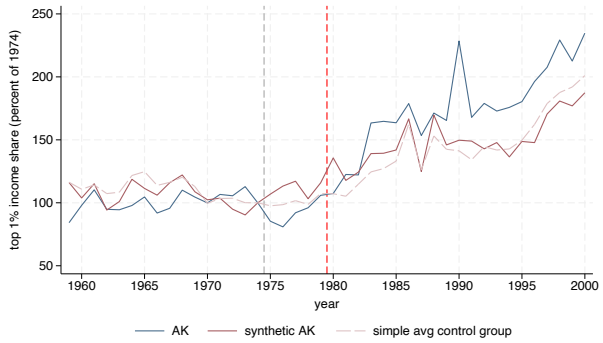


Notes: see next page.

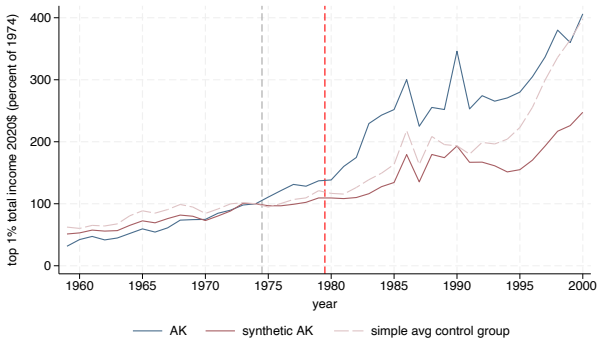
Figure 10: Effect of Tax Cancellations on Top 1% Income Shares and Income (2/2)

Alaska canceled $\tau^{personal} = 14.5\%$, kept $\tau^{corporate} = 9.4\%$ in 1980

(g) Top 1% Income Share



(h) Top 1% Income



Notes: These figures show the results of the synthetic control analysis for cancellations of a personal and/or corporate income tax. Each figure shows changes in the top 1% income and income share, before and after each state's cancellation, normalized to the last year of the matching period. The figures also show both the synthetic control (weighted average of the donor pool) and simple average of all potential donors. The potential donor pool includes states that maintained the canceled tax throughout the period of study. The red line identifies the year of tax cancellation. The synthetic controls are selected using the top 0.01%, top 1%, and bottom 90% incomes and income shares, for each year during the period to the left of the gray dashed line. Thus, the years between the gray dashed line and the red line allow us to assess the performance of the synthetic control. For other income shares and alternative choices of the synthetic control, see Appendix D.

Table 1: Potential Channels and Estimated Effects of Personal/Corporate Income Tax Increases on Inequality

	income		income shares		overall
	top	bottom	top	bottom	inequality
Panel A – Potential Mechanisms:					
1. reduced work/investment incentives	↓	-/↓	↓$^{\alpha}$	↑$^{\alpha}$	↓$^{\alpha}$
2. increased avoidance/evasion	↓	-/↓	↓$^{\alpha}$	↑$^{\alpha}$	-
3. increased out-migration	↓$^{\beta}$	↓$^{\beta}$	↓/↑$^{\gamma}$	↓/↑$^{\gamma}$	-
4. reduced bargaining power/incentives	↓	↑	↓	↑	↓
Panel B – Observed Responses:					
Tax Adoptions (Figures 4 & 5)	-/↓	-	-	-	
Large Tax Increases (Figures 6 & 7)	-/↓	-	-/↓	-/↑	
Large Tax Decreases$^{\delta}$ (Figures 8 & 9)	-/↓	-/↓	-/↓	-	
Tax Cancellations$^{\delta}$ (Figures 10, D.20–D.23)	-	-	-	-	
All Tax Rate Variation (Tables 2 & 3)	-	-	-	-	

Notes: Panel A summarizes the various channels through which personal and corporate income tax increases may affect inequality. We focus this discussion on tax changes that disproportionately affect top incomes, such as changes that are larger (or only) for the top marginal personal rate, flat changes in personal income taxes if high-income individuals are more tax-elastic (e.g., Gruber and Saez, 2002), or corporate tax changes when high-income individuals bear a greater share of the tax incidence on wages (e.g., Kennedy et al., 2022). Panel B summarizes our empirical results from Sections 4–7. We label a result as an increase/decrease when it is statistically significant (at least at the 10% level) during the first 5 years, or when it is not statistically significant but its magnitude exceeds 5%.

$^{\alpha}$ In the case of reforms that led all individuals to reduce their income by the same percent, we would expect no effect for these outcomes.

$^{\beta}$ We consider income tax increases to be less likely to result in migration responses by low-income individuals, even in the case of uniform tax increases.

$^{\gamma}$ Income shares would remain fixed in the simple case where the entire top X% moves out of the state, and incomes follow a Pareto distribution. Otherwise, top income shares may increase or decrease, with bottom income shares moving in the opposite direction.

$^{\delta}$ For cancellations and tax decreases we record the effect with the opposite sign for ease of comparison (all arrows are in the direction consistent with tax increases).

Table 2: Robust TWFE Estimates of Personal and Corporate Income Taxes on (Log) Income Shares

	Bottom 90%	Top 10-1%	Top 1%	Top 1-0.1%	Top 0.1-0.01%	Top 0.01%
Panel A						
Top Personal Rate	0.0010	-0.0018	0.0005	0.0011	0.0069	-0.0000
	(0.0015)	(0.0023)	(0.0039)	(0.0024)	(0.0077)	(0.0182)
Placebo	-0.0043	0.0114	0.0069	-0.0036	-0.0009	0.0852
	(0.0057)	(0.0080)	(0.0150)	(0.0066)	(0.0159)	(0.0858)
Observations	3,542	3,542	3,542	3,542	3,542	3,542
Panel B						
Top Corporate Rate	-0.0008	0.0016	-0.0050	-0.0071	0.0049	-0.0189
	(0.0026)	(0.0026)	(0.0061)	(0.0047)	(0.0074)	(0.0187)
Placebo	-0.0038	0.0119*	0.0066	0.0026	0.0096	0.0620
	(0.0037)	(0.0069)	(0.0118)	(0.0082)	(0.0110)	(0.0480)
Observations	3,775	3,775	3,775	3,775	3,775	3,775
Avg. Top Personal Rate	4.40	4.40	4.40	4.40	4.40	4.40
Avg. Top Corporate Rate	4.46	4.46	4.46	4.46	4.46	4.46
Avg. Top X% Share	62.4	24.4	13.2	8.1	3.1	2.0

Notes: This table shows results of the analysis using all tax rate variation and implementing the robust estimator from de Chaisemartin et al. (2025). This analysis is analogous to the two-way fixed effects specification in Equation (4), and estimates the effect of a 1 percentage point increase in the top personal (Panel A) or top corporate (Panel B) income tax. The outcome variables are log income shares for each group shown in the first row e.g., ln(top 1% share). The sample includes all states, and all consecutive year pairs from 1917 to 2018 where at least one state implemented a tax change and one state did not. Specifically, we estimate the average change in outcome in states that changed the tax rate in year t, relative to states that did not change their tax rate, weighted by the magnitude of the tax change. The placebo tests assess the plausibility of the parallel trends assumption by comparing outcomes from $t-2$ to $t-1$ for states that will vs. will not change their tax rate in year t. Estimates for the top personal (corporate) tax rate control for the top corporate (personal) rate as an additional treatment, and all estimates also include controls for tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$, standard errors clustered at the state level.

Table 3: Robust TWFE Estimates of Personal and Corporate Income Taxes on (Log) Total Incomes

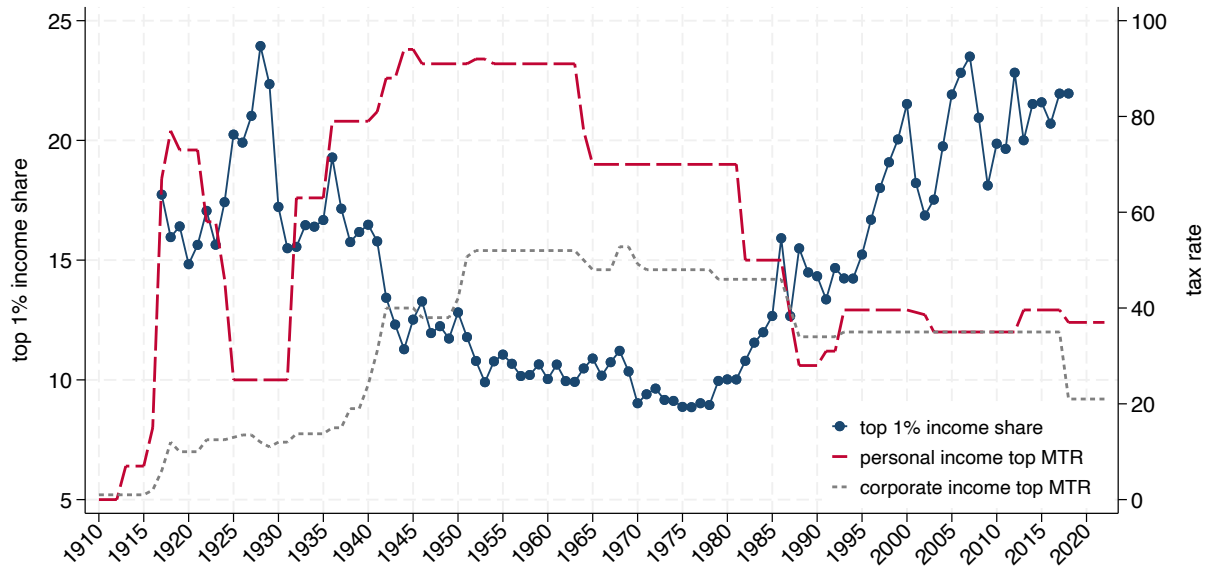
	Bottom 90%	Top 10-1%	Top 1%	Top 1-0.1%	Top 0.1-0.01%	Top 0.01%	Total income
Panel A							
Top Personal Rate	-0.0028	-0.0056**	-0.0033	-0.0028	0.0031	-0.0039	-0.0039*
	(0.0027)	(0.0025)	(0.0046)	(0.0029)	(0.0080)	(0.0191)	(0.0020)
Placebo	-0.0099	0.0057	0.0013	-0.0093	-0.0065	0.0795	-0.0057
	(0.0112)	(0.0064)	(0.0138)	(0.0093)	(0.0196)	(0.0802)	(0.0076)
Observations	3,542	3,542	3,542	3,542	3,542	3,542	3,542
Panel B							
Top Corporate Rate	0.0018	0.0043	-0.0023	-0.0045	0.0076	-0.0162	0.0027
	(0.0038)	(0.0031)	(0.0060)	(0.0045)	(0.0072)	(0.0191)	(0.0025)
Placebo	-0.0105	0.0052	-0.0001	-0.0041	0.0029	0.0553	-0.0067
	(0.0083)	(0.0065)	(0.0112)	(0.0076)	(0.0126)	(0.0456)	(0.0056)
Observations	3,775	3,775	3,775	3,775	3,775	3,775	3,775
Avg. Top Personal Rate	4.40	4.40	4.40	4.40	4.40	4.40	4.40
Avg. Top Corporate Rate	4.46	4.46	4.46	4.46	4.46	4.46	4.46
Avg. Top X% Real Income	55.1	24.1	14.8	8.4	3.6	2.8	94.0

Notes: This table shows results of the analysis using all tax rate variation and implementing the robust estimator from de Chaisemartin et al. (2025). This analysis is analogous to the two-way fixed effects specification in Equation (4), and estimates the effect of a 1 percentage point increase in the top personal (Panel A) or top corporate (Panel B) income tax. The outcome variables are log real income earned by each group shown in the first row. The sample includes all states, and all consecutive year pairs from 1917 to 2018 where at least one state implemented a tax change and one state did not. Specifically, we estimate the average change in outcome in states that changed the tax rate in year t, relative to states that did not change their tax rate, weighted by the magnitude of the tax change. The placebo tests assess the plausibility of the parallel trends assumption by comparing outcomes from $t-2$ to $t-1$ for states that will vs. will not change their tax rate in year t. Estimates for the top personal (corporate) tax rate control for the top corporate (personal) rate as an additional treatment, and all estimates also include controls for tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). The average Top X% real incomes are expressed in 2020 billions of dollars. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$, standard errors clustered at the state level.

APPENDIX FOR ONLINE PUBLICATION

A Additional Descriptive Evidence

Figure A.1: U.S. Top 1% Income Share and
Federal Top Income Tax Rates

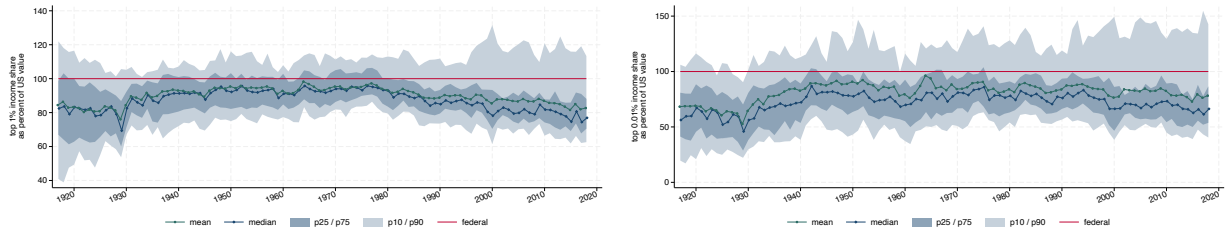


Notes: This figure shows the U.S.-wide top 1% income share and the federal top personal income tax rate, as in Piketty and Saez (2003), as well as the federal top corporate income tax rate.

Figure A.2: State Inequality Over Time as Percent of US

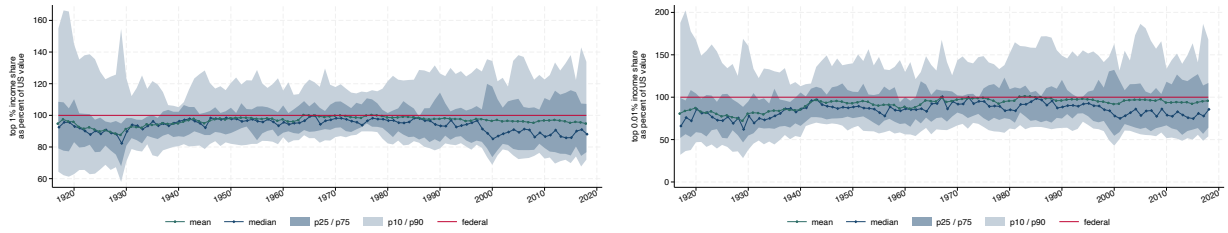
Panel A: Unweighted

(a) Top 1% Income Share (b) Top 0.01% Income Share



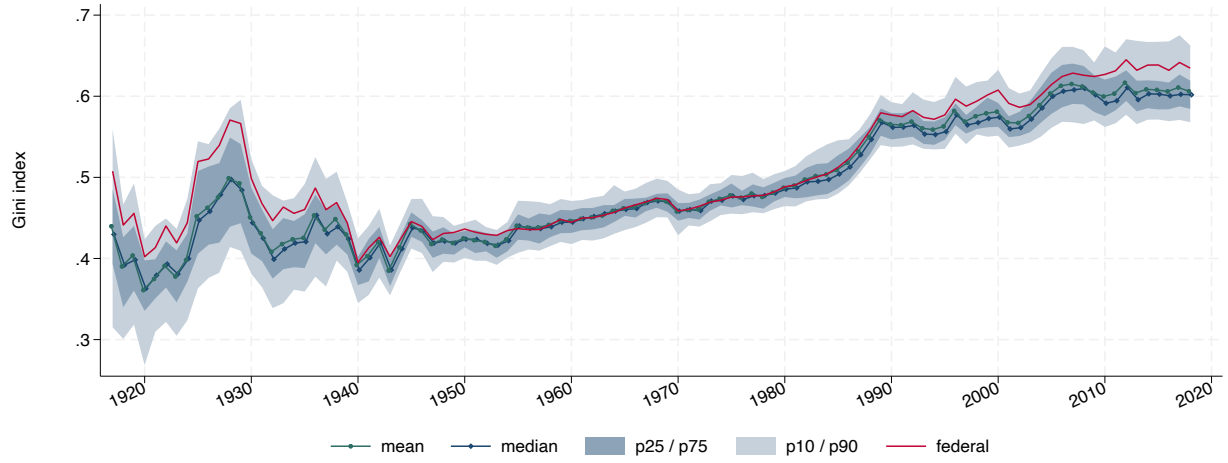
Panel B: Weighted by Number of Tax Units

(c) Top 1% Income Share (d) Top 0.01% Income Share



Notes: These figures show the average, median, and 10$^{\text{th}}$, 25$^{\text{th}}$, 75$^{\text{th}}$, and 90$^{\text{th}}$ percentiles of the top 1% and top 0.01% income shares, as a percent of the equivalent U.S.-wide share, for all U.S. states over time. Panel A is unweighted (as in Figure 2), while in Panel B all statistics are weighted by the number of tax units in each state.

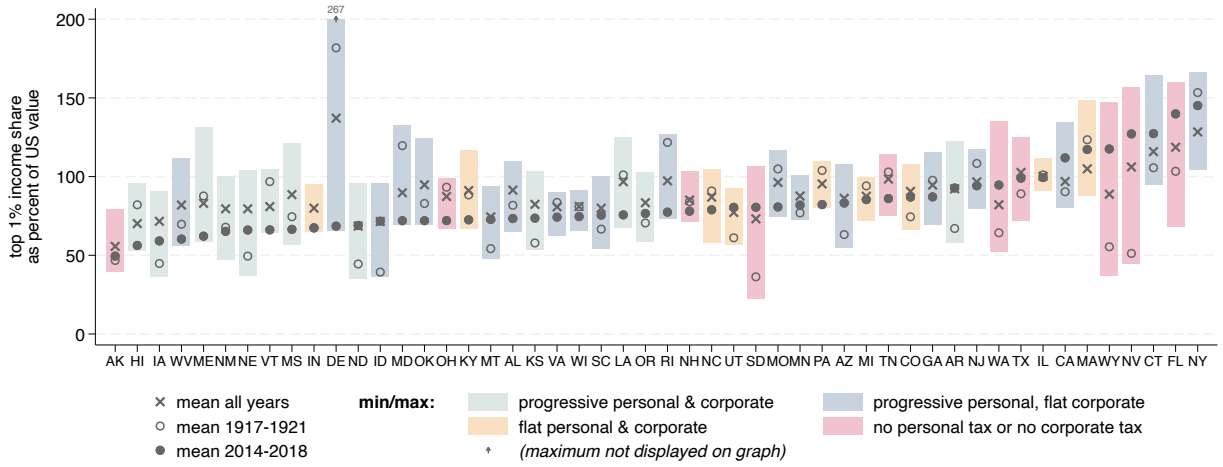
Figure A.3: State Inequality Over Time: Gini Index



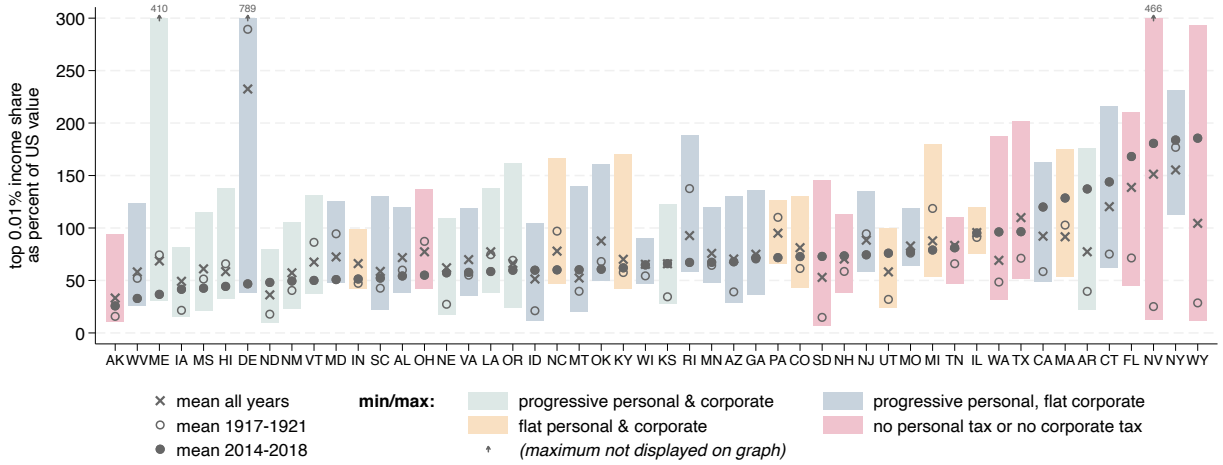
Notes: These figures show the average, median, and 10$^{\text{th}}$, 25$^{\text{th}}$, 75$^{\text{th}}$, and 90$^{\text{th}}$ percentiles of the Gini index, for all U.S. states over time, as well as the U.S. time series.

Figure A.4: Persistence in State Inequality (Categorized by 2018 Tax Policy)

(a) Top 1% Income Share (as Percent of U.S.)

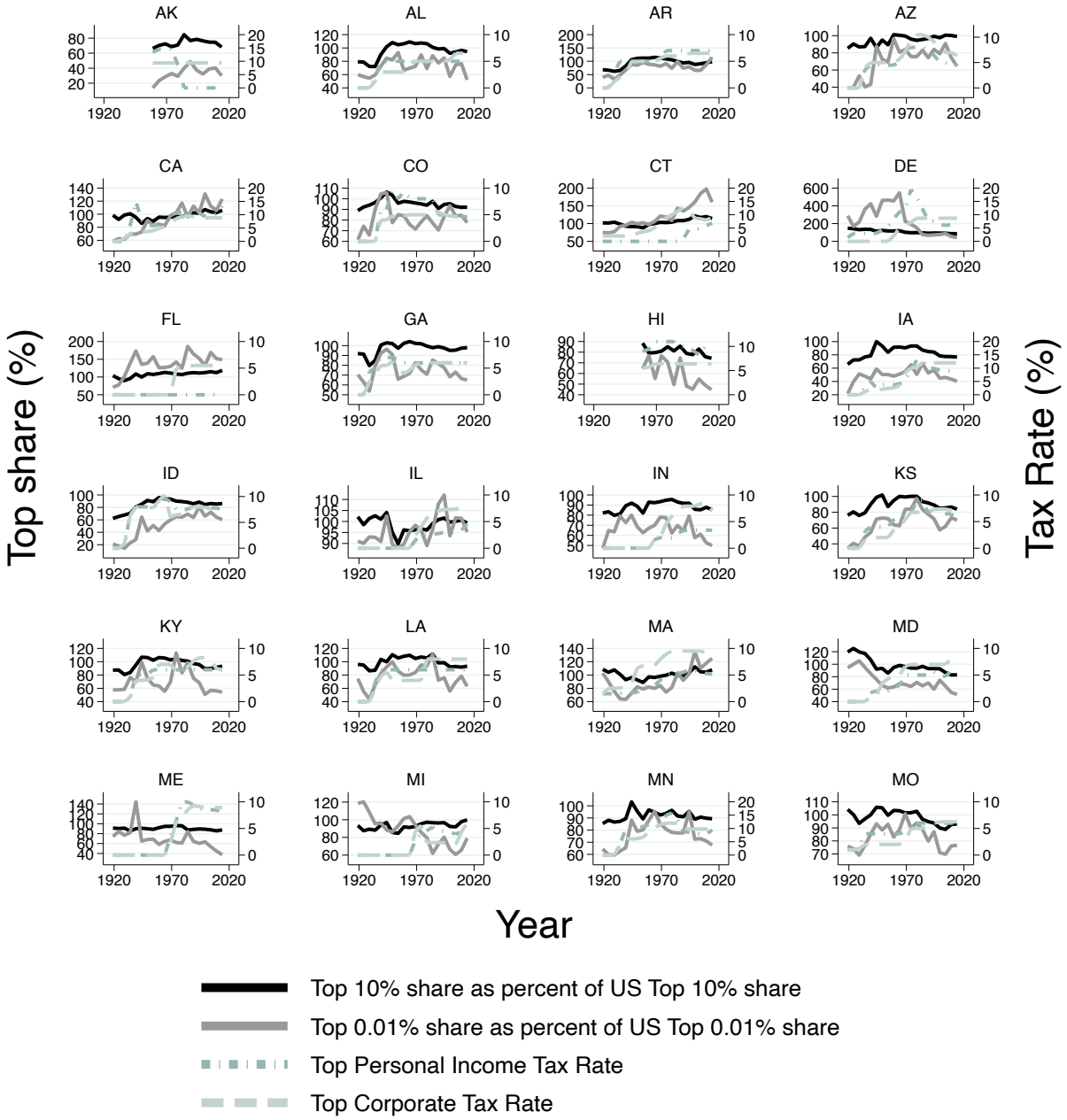


(b) Top 0.01% Income Share (as Percent of U.S.)



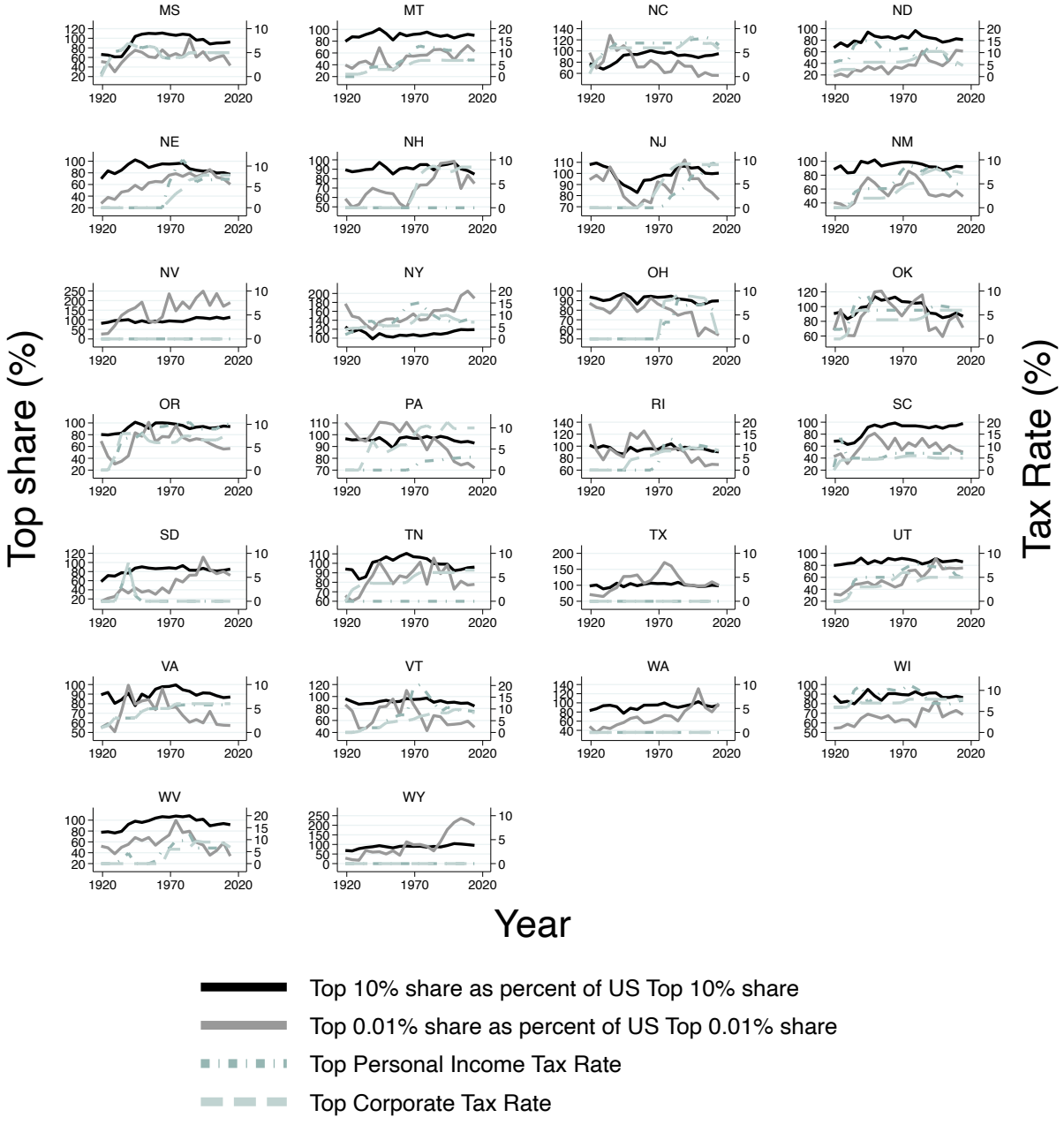
Notes: These figures show, for each state, the minimum and maximum in the top 1% and top 0.01% income shares as a percent of the equivalent U.S.-wide share over time. The mean income shares are also displayed over all years, over the last five years, and over the first five years available (1917-1921, except for AK and HI as 1959-1963). The ranges are colored based on the 2018 tax policy for each state: whether the state collects personal income and corporate taxes, and whether those tax rates are progressive or flat.

Figure A.5: Inequality and Tax Rates Over Time by State (1/2)



Notes: Solid gray lines show the evolution of the 5-year state average Top 10% and Top 0.01% share as a percent of US Top 10% and Top 0.01% share. Dashed green lines show the evolution of 5-year state average Top Personal Income Tax rate and Top Corporate Tax rate over time.

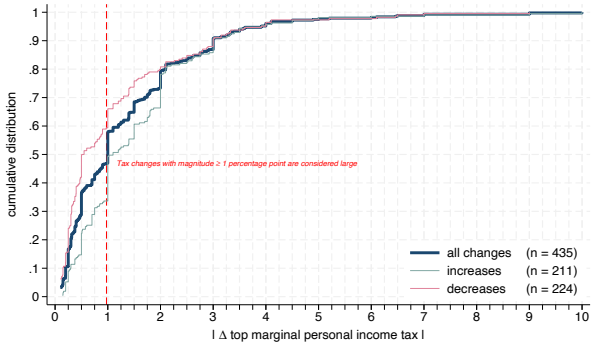
Figure A.5: Inequality and Tax Rates Over Time by State (2/2)



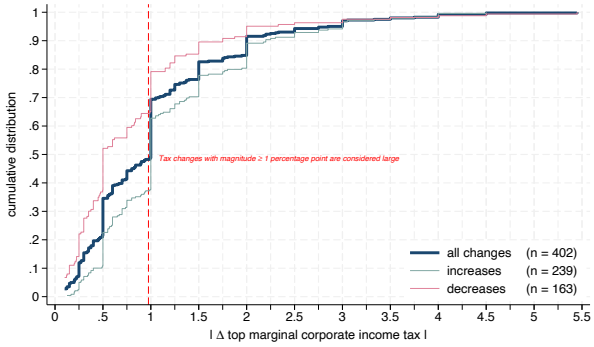
Notes: Solid gray lines show the evolution of the 5-year state average Top 10% and Top 0.01% share as a percent of US Top 10% and Top 0.01% share. Dashed green lines show the evolution of 5-year state average Top Personal Income Tax rate and Top Corporate Tax rate over time.

Figure A.6: Cumulative Distribution of Tax Changes by Magnitude

(a) Top Personal Income Tax Rate



(b) Top Corporate Income Tax Rate



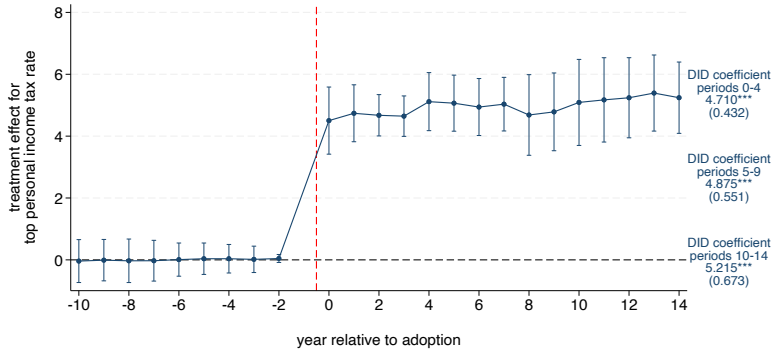
Notes: These figures show the distribution of tax increases, tax decreases, and tax changes overall by magnitude, as well as the minimum size for "large" tax changes by tax type. Only intensive margin tax changes are included; tax adoptions and cancellations are excluded.

B Tax Adoptions: Additional Evidence

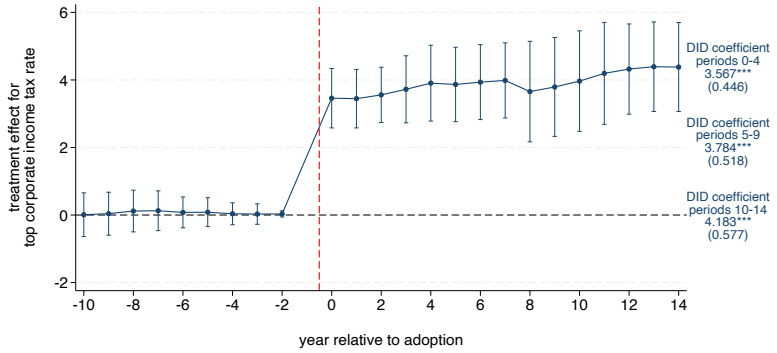
Figure B.7: Effect of Tax Adoptions on Tax Rates

Panel A: Early (1929-1937) Tax Adoptions

(a) Personal Income Tax Rate

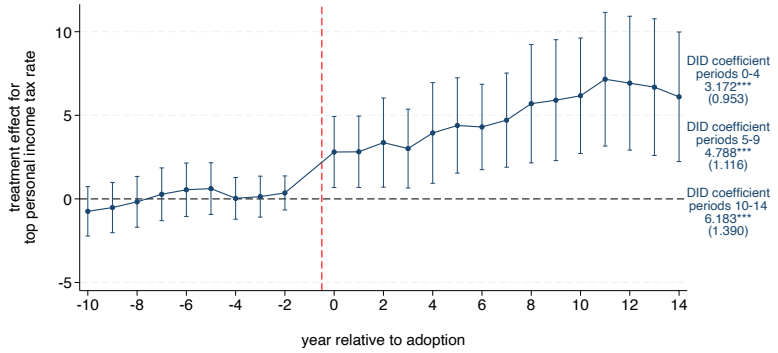


(b) Corporate Income Tax Rate

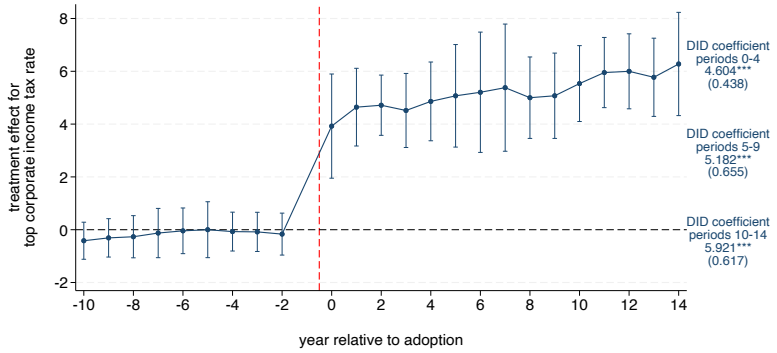


Panel B: Late (1963-1972) Tax Adoptions

(c) Personal Income Tax Rate



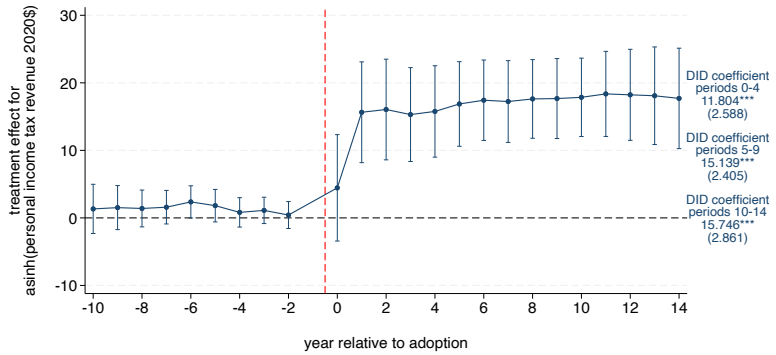
(d) Corporate Income Tax Rate



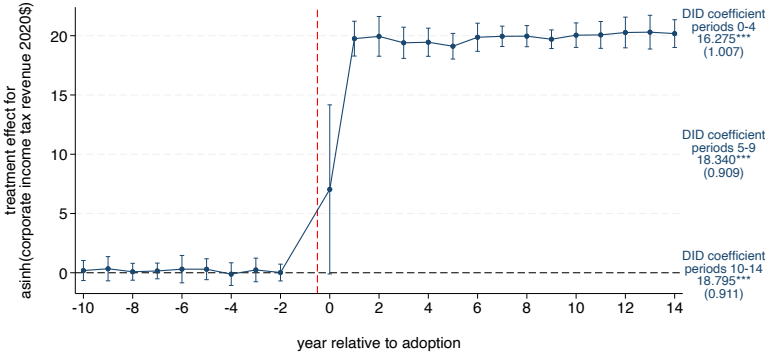
Notes: These figures show the first stage results of estimating Equation (1). The outcome variables are the tax rate levels. Event time 0 corresponds to the year that the state adopted both personal and corporate income taxes, the year of the first adoption if the second adoption occurred the following year, or the year of adoption for the corporate income tax if the state never adopted a personal income tax. In Panel A, the sample includes early adopters (treated), as well as late-adopters and never-adopters (control). In Panel B, the sample includes late-adopters (treated) and never-adopters (control). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, and lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years, Panel B only), and lagged total state expenditures (3 and 5 years, Panel B only). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported for periods 0-4, 5-9, and 10-14, with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Figure B.8: Effect of Late (1963-1972) Tax Adoptions on Tax Revenue

(a) Personal Income Tax Revenue



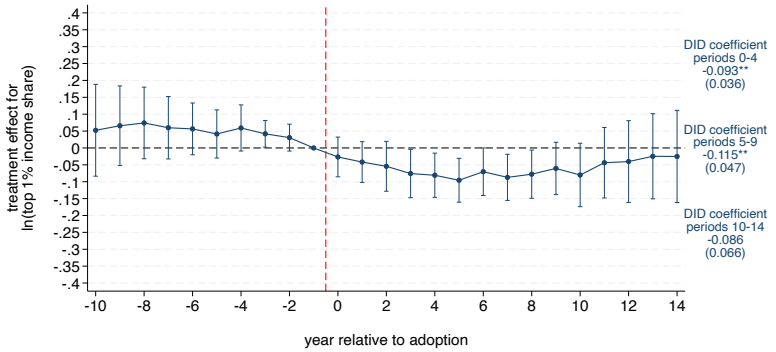
(b) Corporate Income Tax Revenue



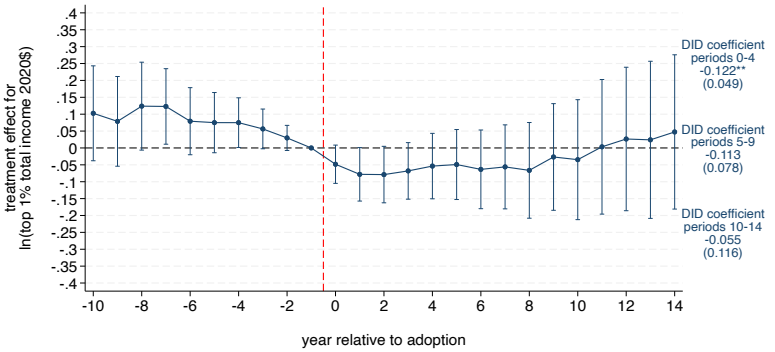
Notes: These figures show the results of estimating Equation (1). The outcome variables are the (inverse hyperbolic sine) tax revenues. Event time 0 corresponds to the year that the state adopted both personal and corporate income taxes, the year of the first adoption if the second adoption occurred the following year, or the year of adoption for the corporate income tax if the state never adopted a personal income tax. The sample includes states that adopted taxes in 1963-1972 (treated), as well as states that never either tax adopted (control). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported for periods 0-4, 5-9, and 10-14, with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Figure B.9: Effect of Early (1929-1937) Tax Adoptions on Log(Income Shares) and Log(Income)

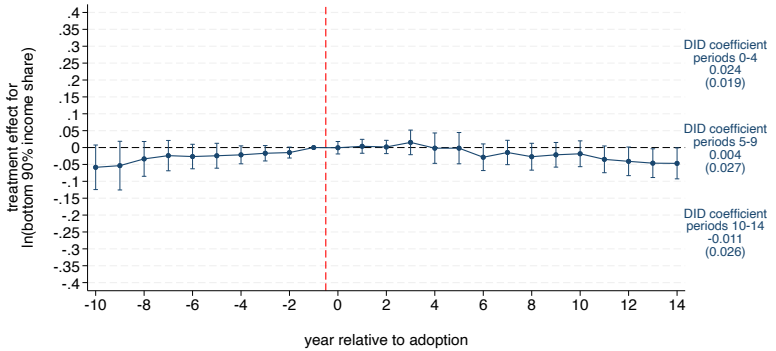
(a) Top 1% Income Share



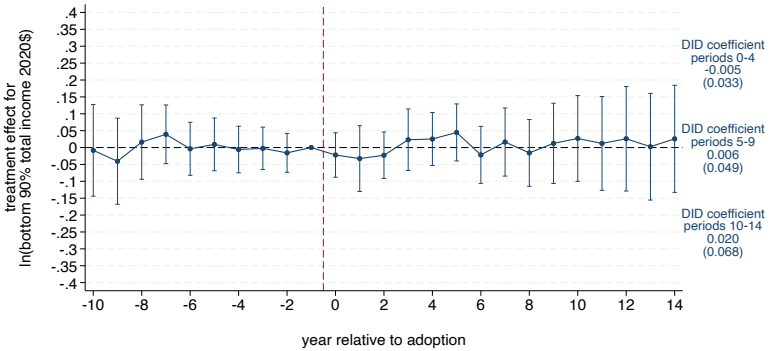
(b) Top 1% Income



(c) Bottom 90% Income Share

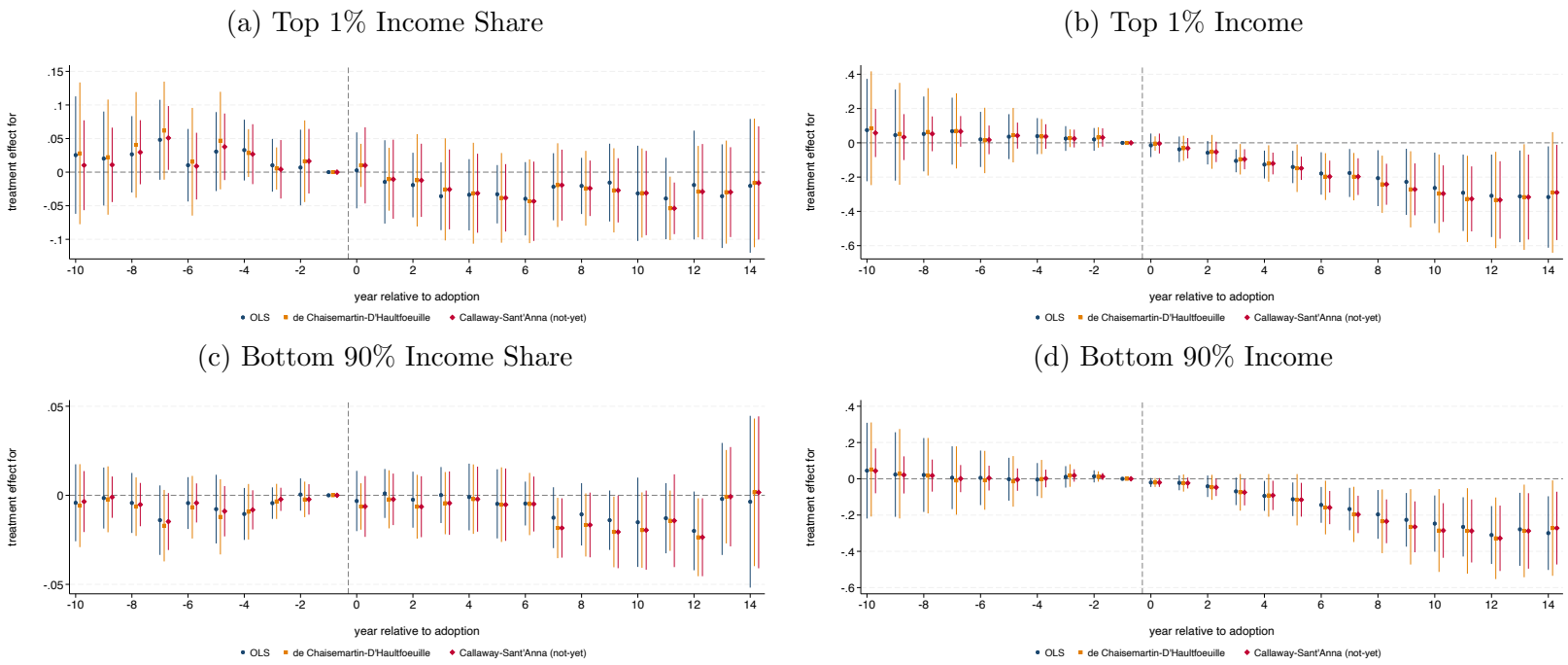


(d) Bottom 90% Income



Notes: These figures show the results of estimating Equation (1). The outcome variable is the logarithm of income shares or income levels. Event time 0 corresponds to the year that the state adopted both personal and corporate income taxes, the year of the first adoption if the second adoption occurred the following year, or the year of adoption for the corporate income tax if the state never adopted a personal income tax. Tax rate changes are shown in Appendix Figure B.7. The sample includes states that adopted taxes in 1929-1937 (treated), as well as states that adopted after 1962 or never adopted (controls). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, and lagged population (5, 10, and 15 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported for periods 0-4, 5-9, and 10-14, with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

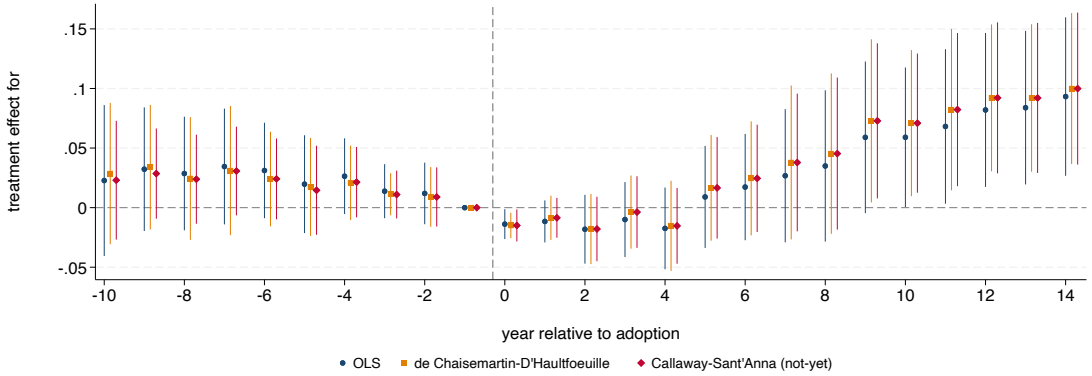
Figure B.10: Effect of Late (1963-1972) Tax Adoptions on (Log) Income Shares and Income



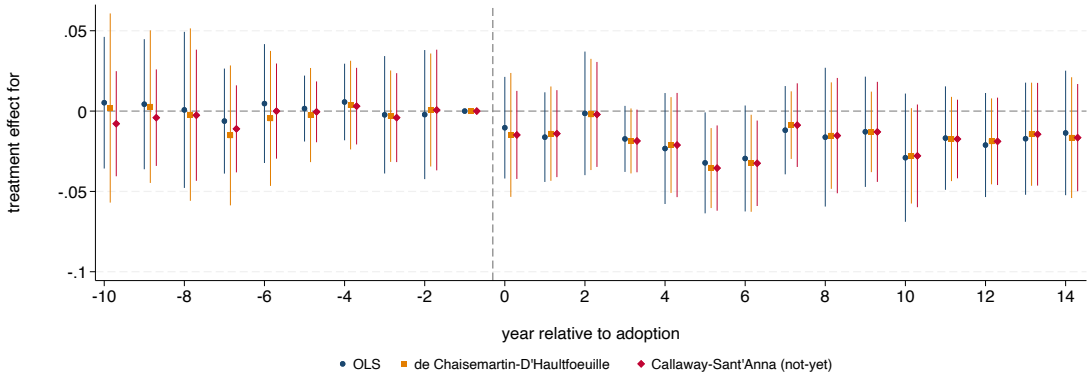
Notes: These figures show results from estimating Equation (1) along with alternative estimators proposed in de Chaisemartin and D'Haultfoeuille (2021) and Callaway and Sant'Anna (2021). Figures (a) and (c) each show a different log income share as the outcome variable. Figures (b) and (d) each show a different log income as the outcome variable. Event time 0 corresponds to the year that the state adopted both personal and corporate income taxes, the year of the first adoption if the second adoption occurred the following year, or the year of adoption for the corporate income tax if the state never adopted a personal income tax. The sample includes states that adopted taxes in 1963-1972 (treated), as well as states that never adopted either tax (controls). The OLS specification includes state fixed effects and year fixed effects, none of the estimates include additional control variables. Standard errors are clustered at the state level and 95% confidence intervals are reported.

Figure B.11: Effect of Tax Adoptions on (Log) Gini Index

(a) Early (1929-1937) Tax Adoptions



(b) Late (1963-1972) Tax Adoptions



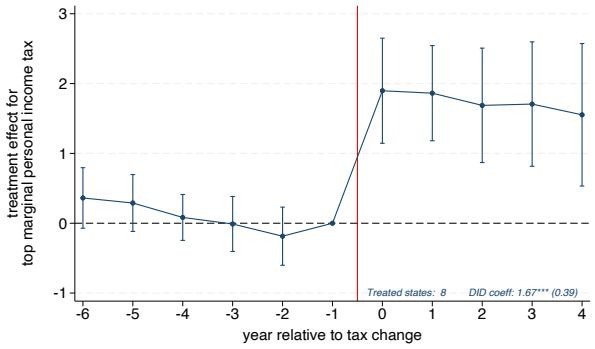
Notes: These figures show results from estimating Equation (1) along with alternative estimators proposed in de Chaisemartin and D'Haultfoeuille (2021) and Callaway and Sant'Anna (2021), using the (log) Gini Index as the outcome variable. Figure (a) includes states that adopted taxes 1929-1937, with states that adopted 1963+ or never adopted either tax acting as controls. Figure (b) includes states that adopted taxes 1963-1972, with states that never adopted either tax acting as controls. Event time 0 corresponds to the year that the state adopted both personal and corporate income taxes, the year of the first adoption if the second adoption occurred the following year, or the year of adoption for the corporate income tax if the state never adopted a personal income tax. The OLS specification includes state fixed effects and year fixed effects, none of the estimates include additional control variables. Standard errors are clustered at the state level and 95% confidence intervals are reported.

C Large Tax Changes: Additional Evidence

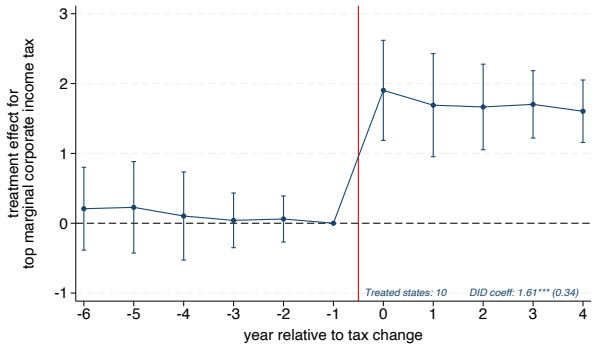
Figure C.12: Effect of Tax Changes on Tax Rates

Panel A: Tax Increases

(a) Personal Income Tax Rate

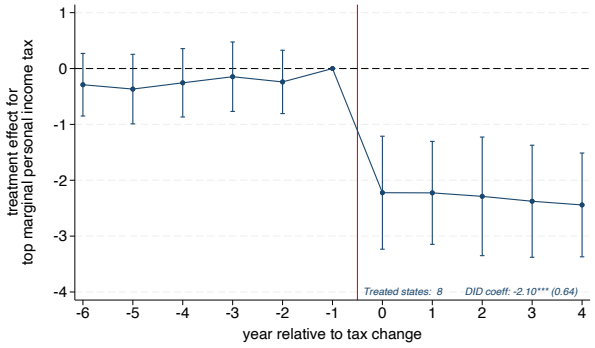


(b) Corporate Income Tax Rate

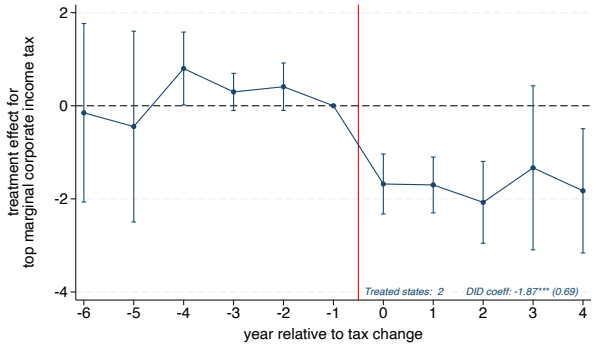


Panel B: Tax Decreases

(c) Personal Income Tax Rate



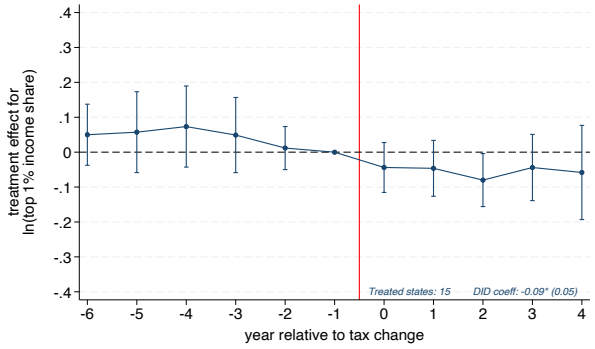
(d) Corporate Income Tax Rate



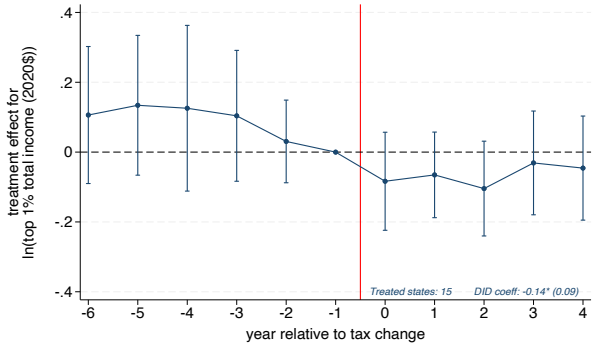
Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variables are the tax rates. Event time 0 corresponds to the year that the state increased or decreased personal or corporate income taxes. The sample includes states that increased/decreased the personal/corporate income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.25pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported.

Figure C.13: Effect of Personal Tax Increases on Log(Income Shares) and Log(Income) with Expanded Control Group

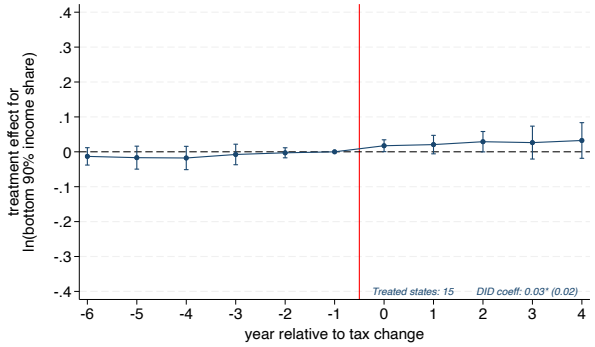
(a) Top 1% Income Share



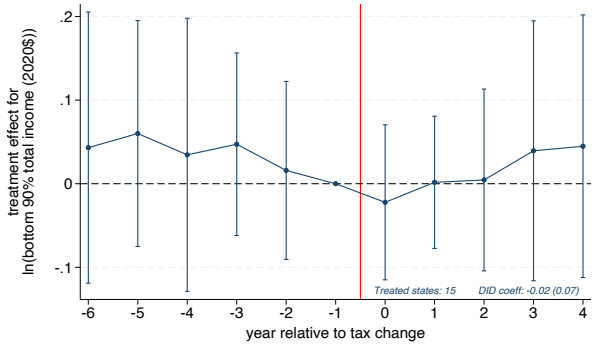
(b) Top 1% Income



(c) Bottom 90% Income Share



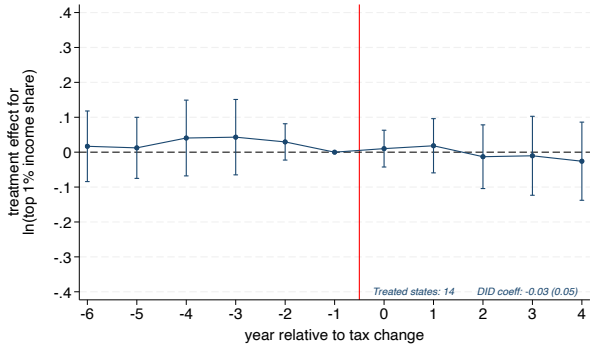
(d) Bottom 90% Income



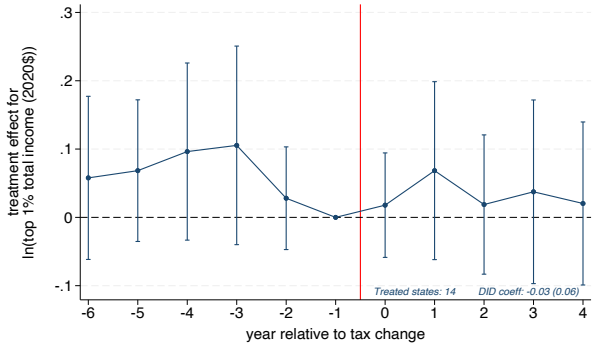
Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states increased the personal income tax. The sample includes states that increased the personal income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.5pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Figure C.14: Effect of Corporate Tax Increases on Log(Income Shares) and Log(Income) with Expanded Control Group

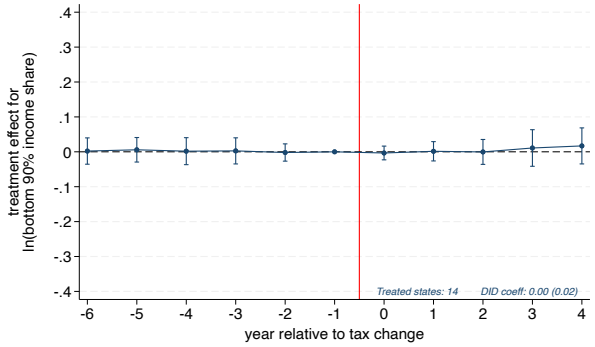
(a) Top 1% Income Share



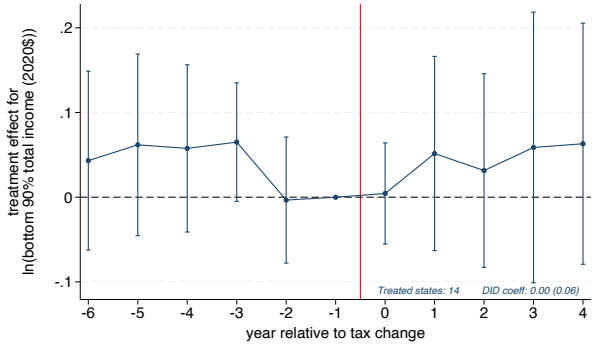
(b) Top 1% Income



(c) Bottom 90% Income Share



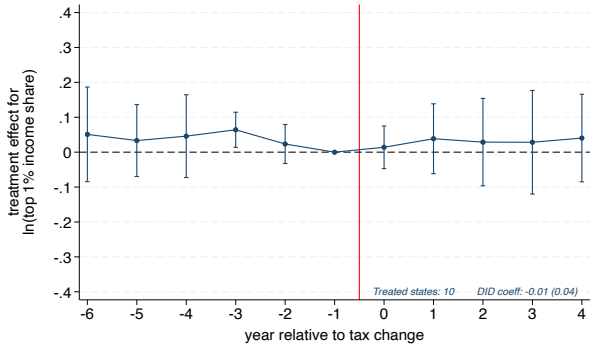
(d) Bottom 90% Income



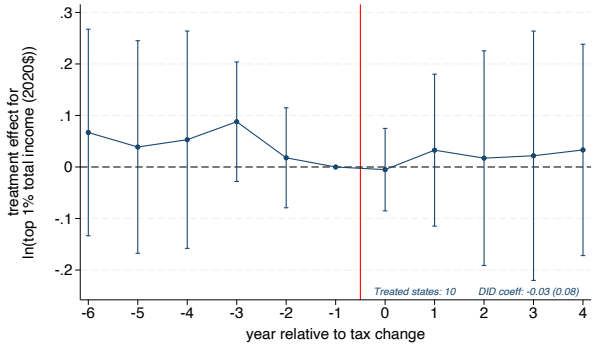
Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states increased the corporate income tax. The sample includes states that increased the corporate income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.5pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Figure C.15: Effect of Personal Tax Decreases on Log (Income Shares) and Log(Income) with Expanded Control Group

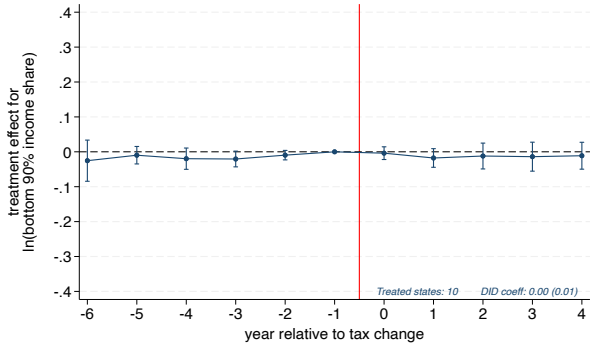
(a) Top 1% Income Share



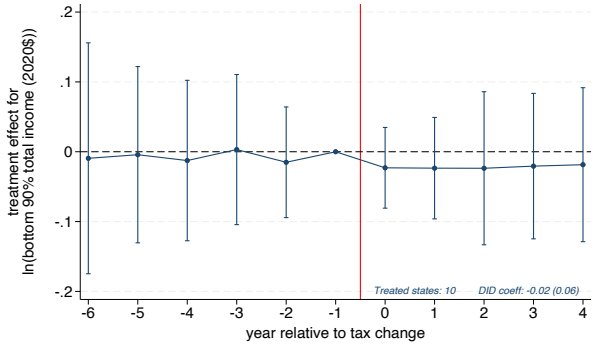
(b) Top 1% Income



(c) Bottom 90% Income Share



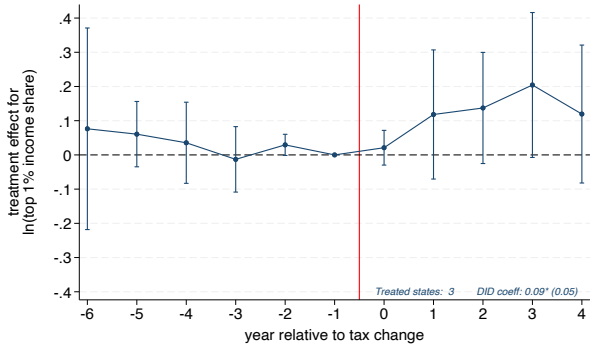
(d) Bottom 90% Income



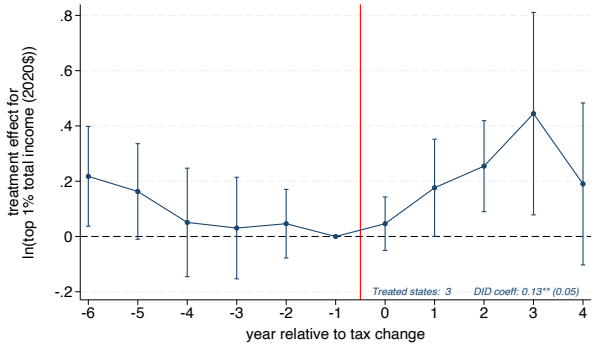
Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states decreased the personal income tax. The sample includes states that decreased the personal income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.5pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Figure C.16: Effect of Corporate Tax Decreases on Log(Income Shares) and Log(Income) with Expanded Control Group

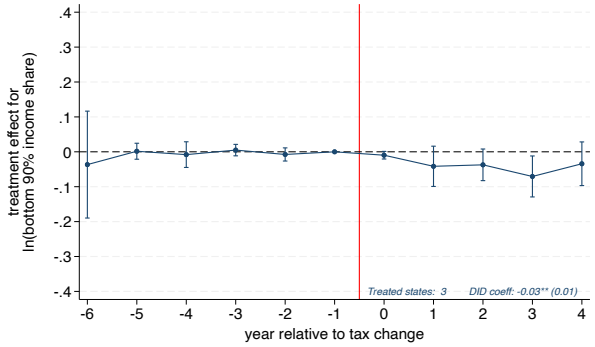
(a) Top 1% Income Share



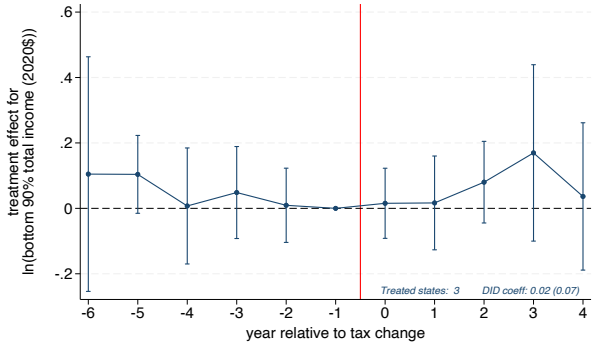
(b) Top 1% Income



(c) Bottom 90% Income Share



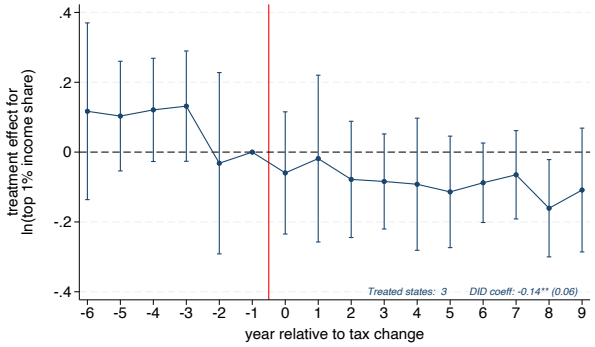
(d) Bottom 90% Income



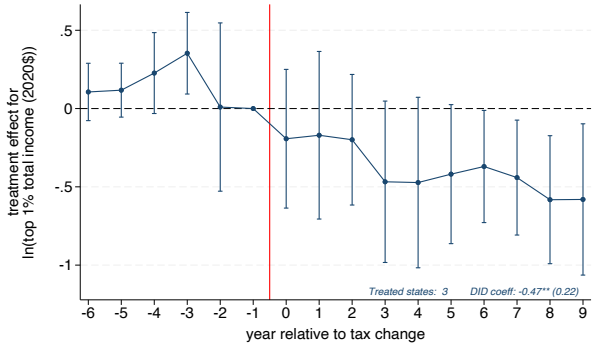
Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states decreased the corporate income tax. The sample includes states that decreased the corporate income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.5pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Figure C.17: Effect of Personal Tax Increases on Log(Income Shares) and Log(Income) with Extended Event Window

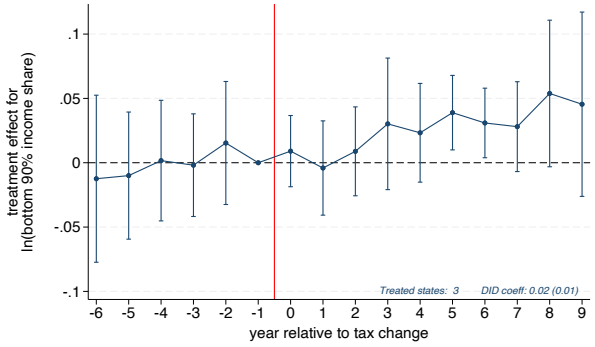
(a) Top 1% Income Share



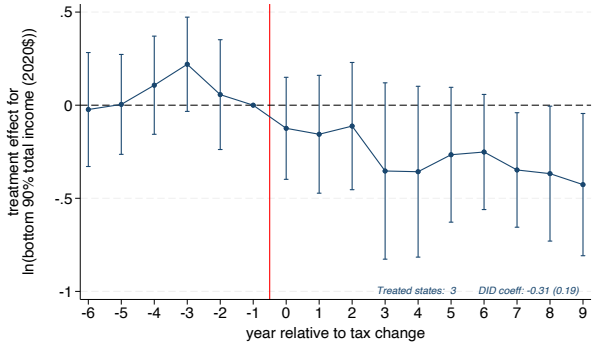
(b) Top 1% Income



(c) Bottom 90% Income Share



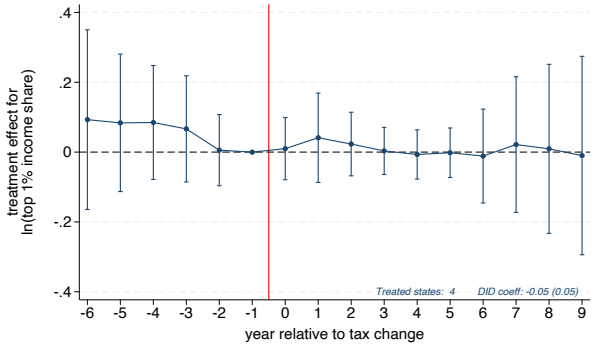
(d) Bottom 90% Income



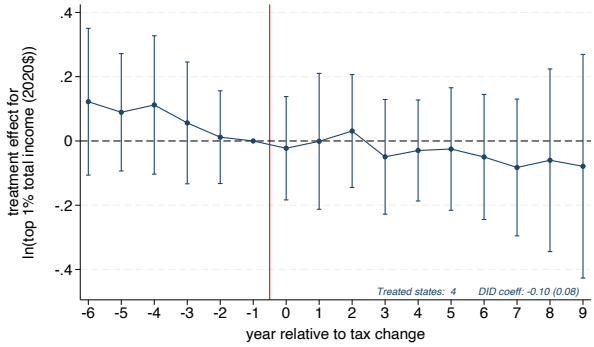
Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states increased the personal income tax. The sample includes states that increased the personal income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.25pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Figure C.18: Effect of Corporate Tax Increases on Log(Income Shares) and Log(Income) with Extended Event Window

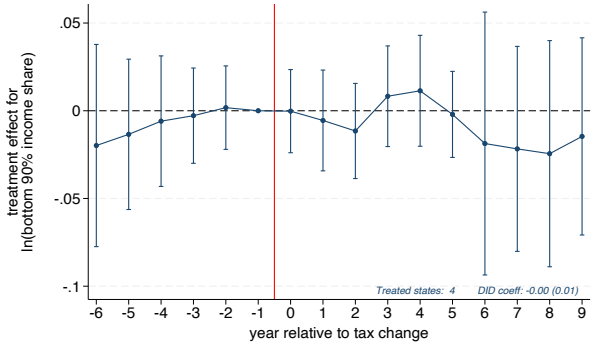
(a) Top 1% Income Share



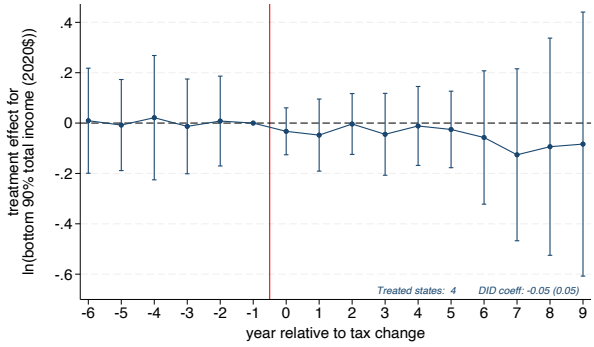
(b) Top 1% Income



(c) Bottom 90% Income Share



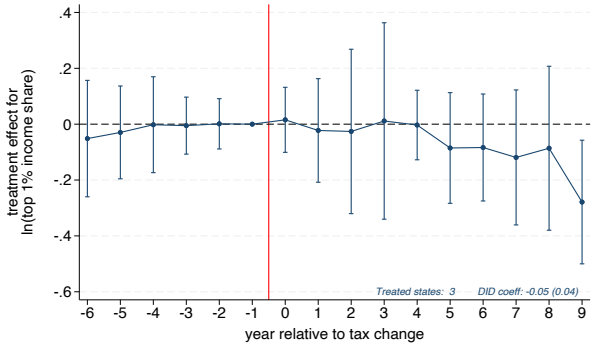
(d) Bottom 90% Income



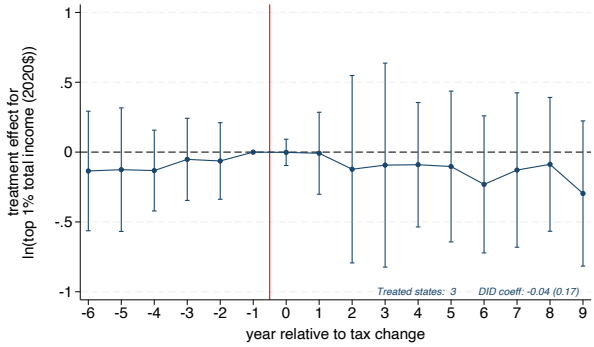
Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states increased the corporate income tax. The sample includes states that increased the corporate income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.25pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Figure C.19: Effect of Personal Tax Decreases on Log (Income Shares) and Log(Income) with Extended Event Window

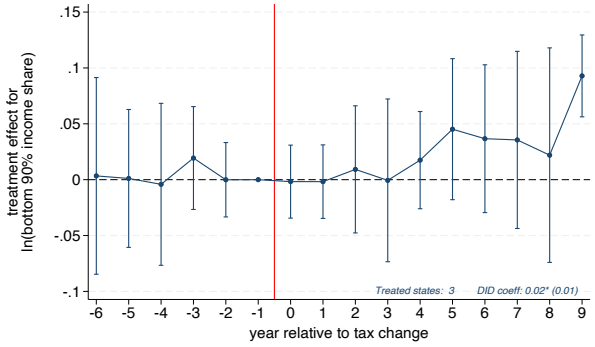
(a) Top 1% Income Share



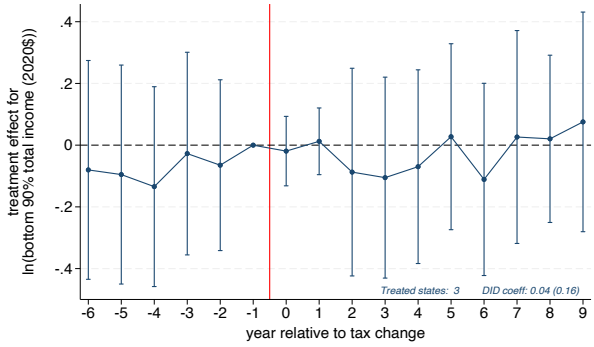
(b) Top 1% Income



(c) Bottom 90% Income Share



(d) Bottom 90% Income

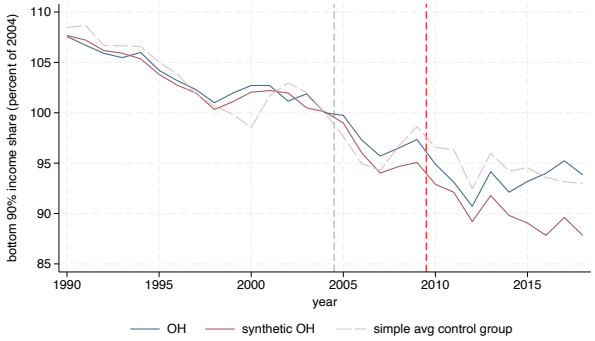


Notes: These figures show the results of estimating the stacked event study regression in Equation (2) using sample weights from Equation (3) following Wing et al. (2024). The outcome variable is the logarithm of top income shares. Event time 0 corresponds to the year that states decreased the personal income tax. The sample includes states that decreased the personal income tax by more than 1pp (treated states) as well as states that did not change either the personal or corporate income tax by more than 0.25pp during the relevant period of analysis (control states). The specification includes an indicator for treated states, event time fixed effects, and controls for: tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). Standard errors are clustered at the state level and 95% confidence intervals are reported. Pooled DID coefficients are also reported with * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

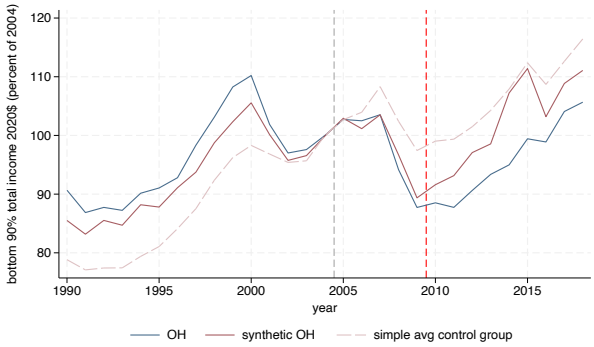
D Tax Cancellations: Additional Evidence

Figure D.20: Ohio canceled $\tau^{corporate} = 8.5\%$, kept $\tau^{personal} = 6.24\%$ in 2010

(a) Bottom 90% Income Share



(b) Bottom 90% Income

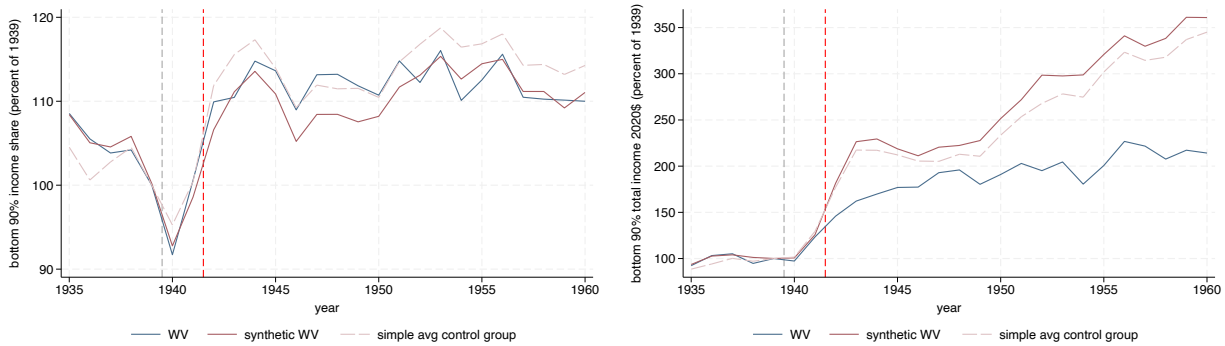


Notes: These figures show results of the synthetic control analysis for Ohio's cancellation of its corporate income tax. Each figure shows changes in income shares or incomes for the top 0.01%, top 10-1%, and bottom 90%, before and after cancellation, normalized to the last year of the matching period. The figures also show both the synthetic control (weighted average of the donor pool) and simple average of all potential donors. The potential donor pool includes states that maintained the corporate income tax throughout the period of study. The red line identifies the year of tax cancellation. The synthetic control weights are always selected using the top 0.01%, top 1%, and bottom 90% incomes and income shares, for each year during the period to the left of the gray dashed line. Thus, the composition of donor states is consistent across outcomes, and the years between the gray dashed line and the red line allow us to assess the performance of the synthetic control.

Figure D.21: West Virginia canceled $\tau^{personal} = 6\%$, kept $\tau^{corporate} = 0\%$ in 1942

(a) Bottom 90% Income Share

(b) Bottom 90% Income



Notes: These figures show results of the synthetic control analysis for West Virginia's cancellation of its personal income tax. Each figure shows changes in income shares or incomes for the top 0.01%, top 10-1%, and bottom 90%, before and after cancellation, normalized to the last year of the matching period. The figures also show both the synthetic control (weighted average of the donor pool) and simple average of all potential donors. The potential donor pool includes states that maintained the personal income tax throughout the period of study. The red line identifies the year of tax cancellation. The synthetic control weights are always selected using the top 0.01%, top 1%, and bottom 90% incomes and income shares, for each year during the period to the left of the gray dashed line. Thus, the composition of donor states is consistent across outcomes, and the years between the gray dashed line and the red line allow us to assess the performance of the synthetic control.

Figure D.22: South Dakota: canceled $\tau^{personal} = 6\%$ and $\tau^{corporate} = 8\%$ in 1943

(a) Bottom 90% Income Share

(b) Bottom 90% Income

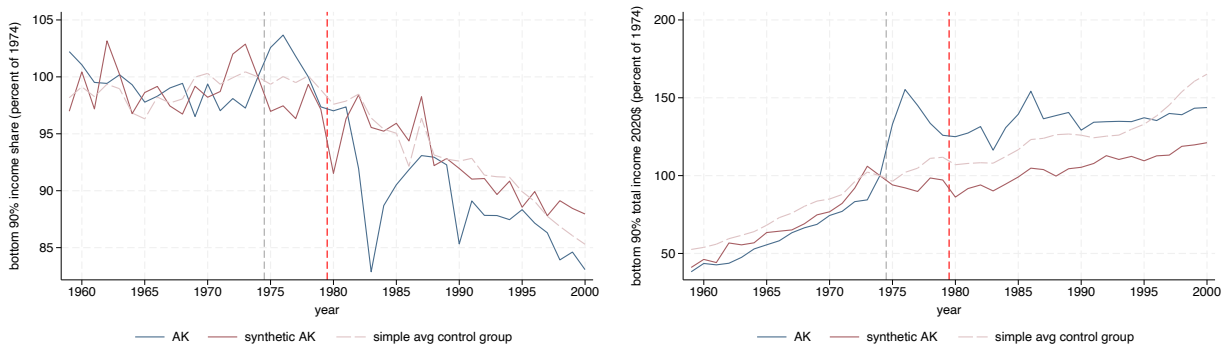


Notes: These figures show results of the synthetic control analysis for South Dakota's cancellation of its personal and corporate income taxes. Each figure shows changes in income shares or incomes for the top 0.01%, top 10-1%, and bottom 90%, before and after cancellation, normalized to the last year of the matching period. The figures also show both the synthetic control (weighted average of the donor pool) and simple average of all potential donors. The potential donor pool includes states that maintained personal and corporate income taxes throughout the period of study. The red line identifies the year of tax cancellation. The synthetic control weights are always selected using the top 0.01%, top 1%, and bottom 90% incomes and income shares, for each year during the period to the left of the gray dashed line. Thus, the composition of donor states is consistent across outcomes, and the years between the gray dashed line and the red line allow us to assess the performance of the synthetic control.

Figure D.23: Alaska canceled $\tau^{personal} = 14.5\%$, kept $\tau^{corporate} = 9.4\%$ in 1980

(a) Bottom 90% Income Share

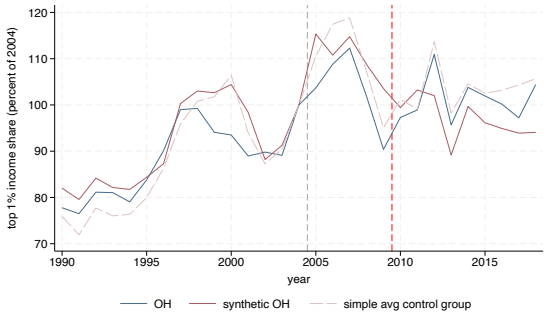
(b) Bottom 90% Income



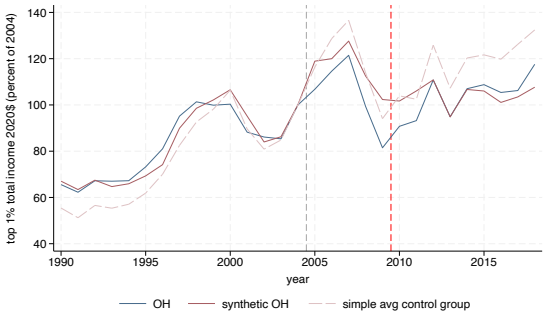
Notes: These figures show results of the synthetic control analysis for Alaska's cancellation of its personal income tax. Each figure shows changes in income shares or incomes for the top 0.01%, top 10-1%, and bottom 90%, before and after cancellation, normalized to the last year of the matching period. The figures also show both the synthetic control (weighted average of the donor pool) and simple average of all potential donors. The potential donor pool includes states that maintained the personal income tax throughout the period of study. The red line identifies the year of tax cancellation. The synthetic control weights are always selected using the top 0.01%, top 1%, and bottom 90% incomes and income shares, for each year during the period to the left of the gray dashed line. Thus, the composition of donor states is consistent across outcomes, and the years between the gray dashed line and the red line allow us to assess the performance of the synthetic control.

Figure D.24: Ohio canceled $\tau^{corporate} = 8.5\%$, kept $\tau^{personal} = 6.24\%$ in 2010
Matching Only on Gini Index

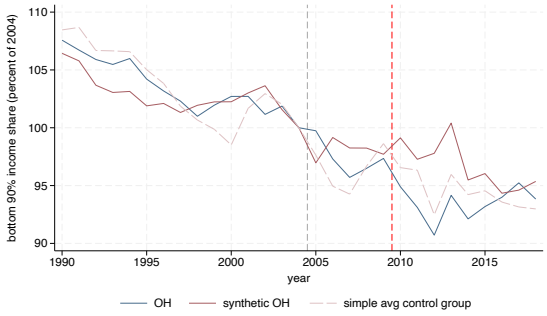
(a) Top 1% Income Share



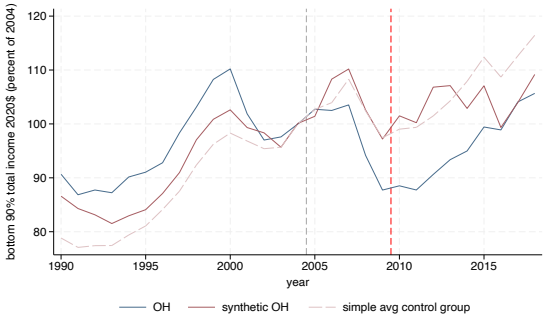
(b) Top 1% Income



(c) Bottom 90% Income Share



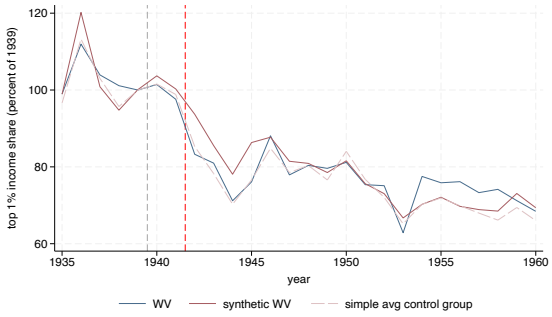
(d) Bottom 90% Income



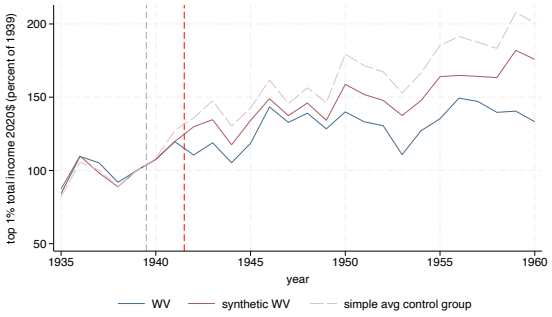
Notes: These figures show results where the synthetic control weights are selected using only the Gini Index (see Figure 10 notes for other details).

Figure D.25: West Virginia canceled $\tau^{personal} = 6\%$, kept $\tau^{corporate} = 0\%$ in 1942
Matching Only on Gini Index

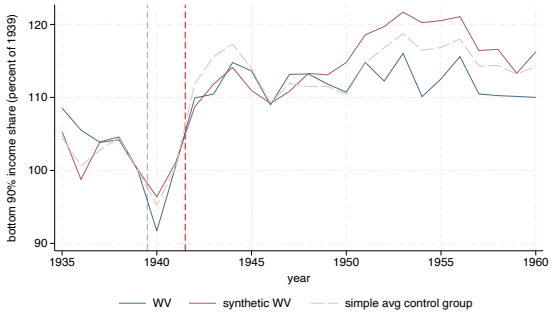
(a) Top 1% Income Share



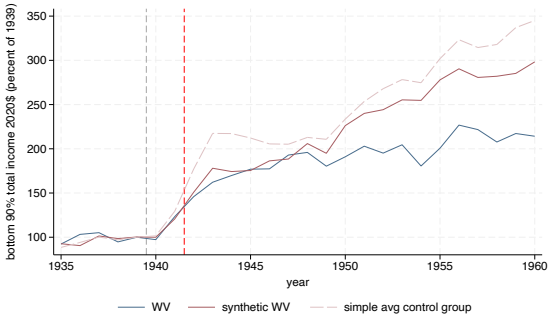
(b) Top 1% Income



(c) Bottom 90% Income Share



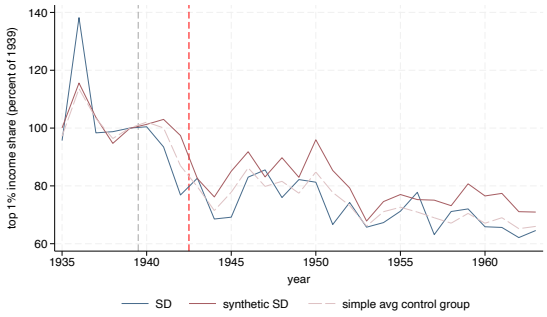
(d) Bottom 90% Income



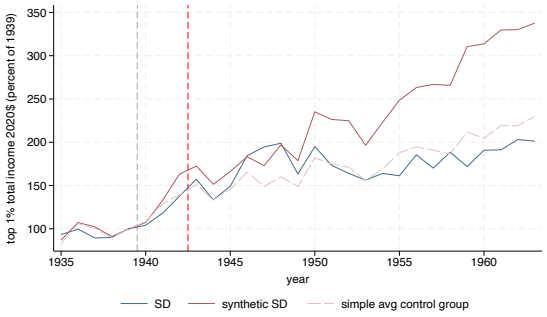
Notes: These figures show results where the synthetic control weights are selected using only the Gini Index (see Figure 10 notes for other details).

Figure D.26: South Dakota: canceled $\tau^{personal} = 6\%$ and $\tau^{corporate} = 8\%$ in 1943
Matching Only on Gini Index

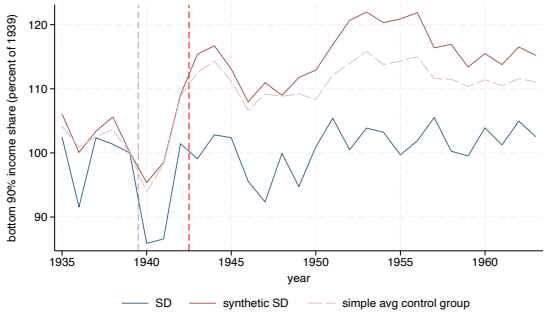
(a) Top 1% Income Share



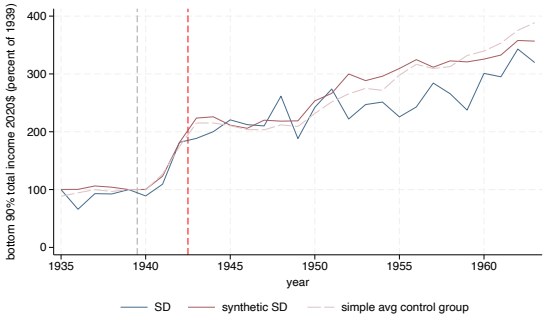
(b) Top 1% Income



(c) Bottom 90% Income Share



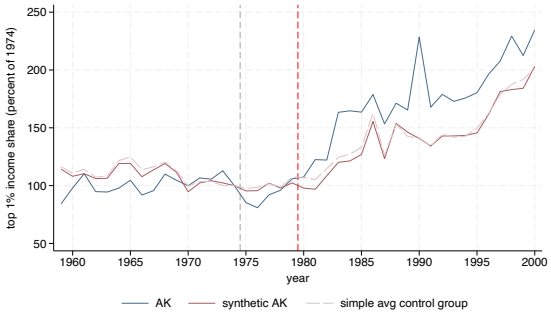
(d) Bottom 90% Income



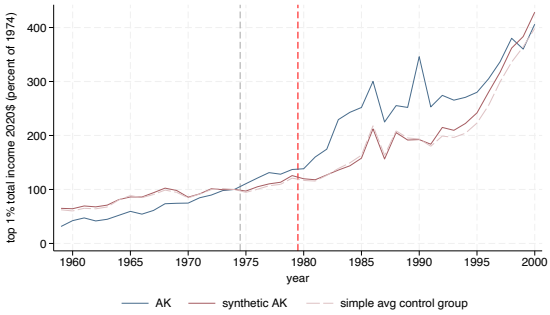
Notes: These figures show results where the synthetic control weights are selected using only the Gini Index (see Figure 10 notes for other details).

Figure D.27: Alaska canceled $\tau^{personal} = 14.5\%$, kept $\tau^{corporate} = 9.4\%$ in 1980
Matching Only on Gini Index

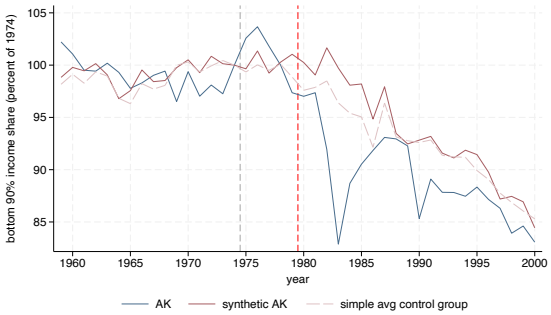
(a) Top 1% Income Share



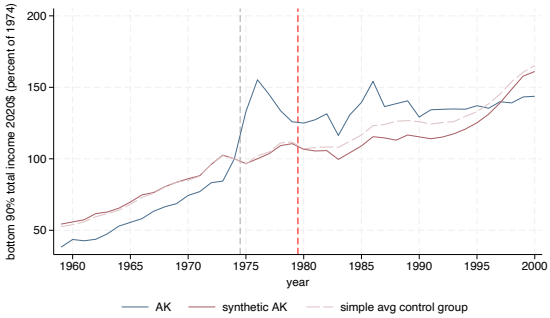
(b) Top 1% Income



(c) Bottom 90% Income Share



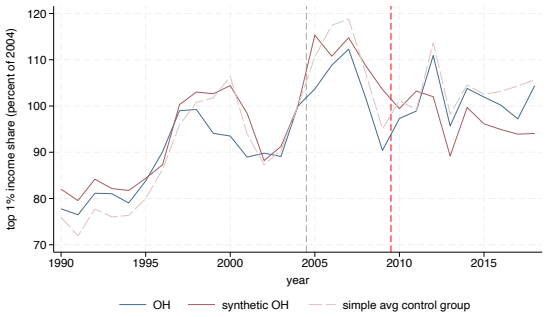
(d) Bottom 90% Income



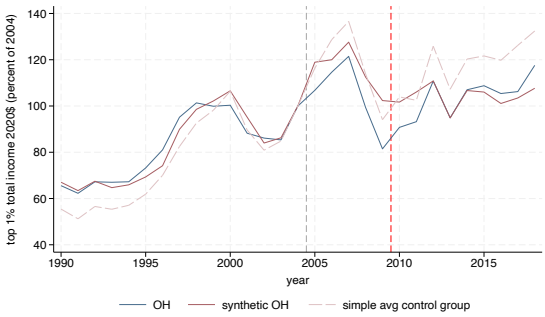
Notes: These figures show results where the synthetic control weights are selected using only the Gini Index (see Figure 10 notes for other details).

Figure D.28: Ohio canceled $\tau^{corporate} = 8.5\%$, kept $\tau^{personal} = 6.24\%$ in 2010
Matching Only on Outcome

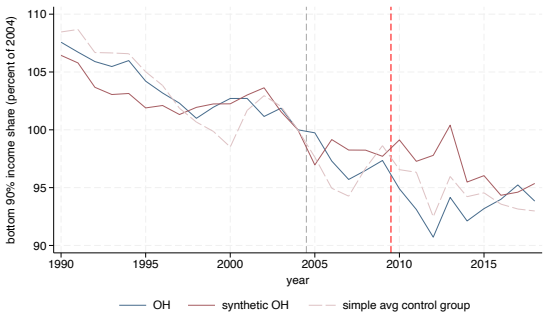
(a) Top 1% Income Share



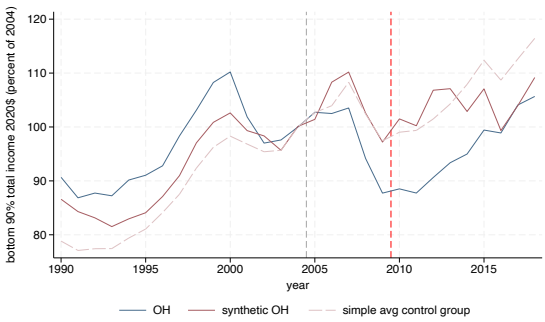
(b) Top 1% Income



(c) Bottom 90% Income Share



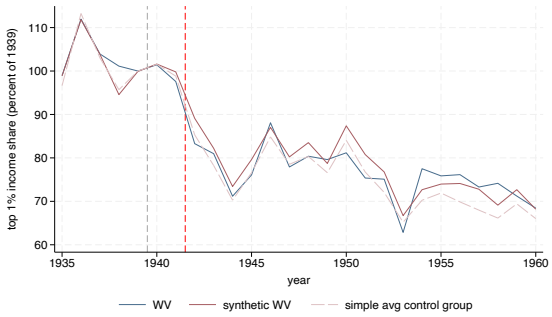
(d) Bottom 90% Income



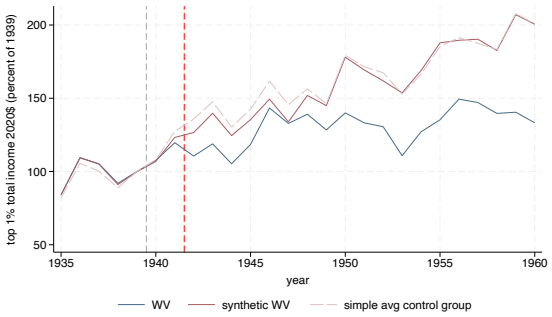
Notes: These figures show results where the synthetic control weights are selected using only the outcome variable (see Figure 10 notes for other details). As a result, the composition of donor states varies across (a)-(h).

Figure D.29: West Virginia canceled $\tau^{personal} = 6\%$, kept $\tau^{corporate} = 0\%$ in 1942
Matching Only on Outcome

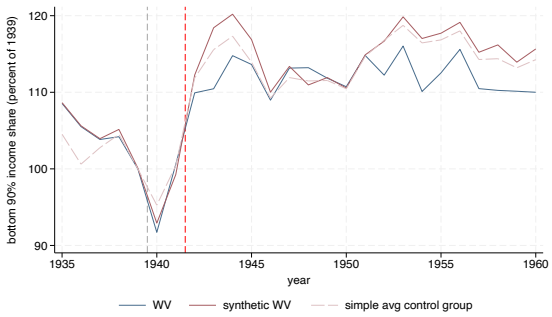
(a) Top 1% Income Share



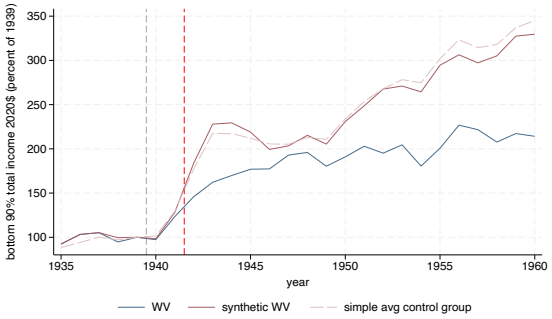
(b) Top 1% Income



(c) Bottom 90% Income Share



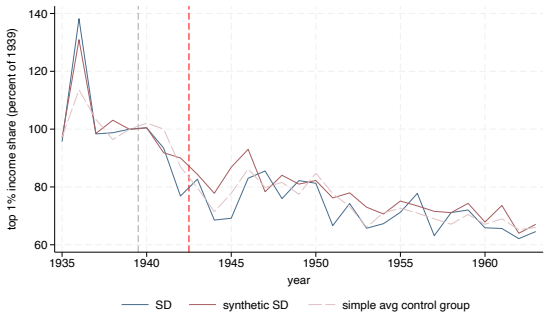
(d) Bottom 90% Income



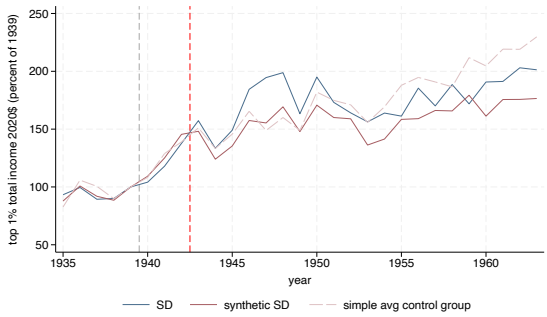
Notes: These figures show results where the synthetic control weights are selected using only the outcome variable (see Figure 10 notes for other details). As a result, the composition of donor states varies across (a)-(h).

Figure D.30: South Dakota: canceled $\tau^{personal} = 6\%$ and $\tau^{corporate} = 8\%$ in 1943
Matching Only on Outcome

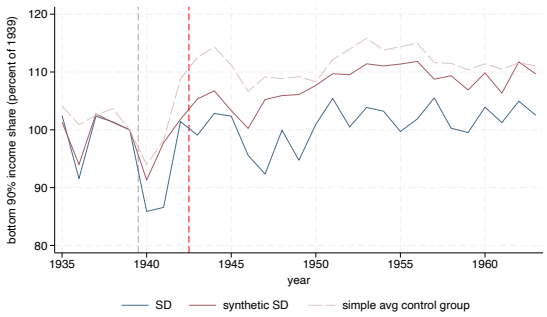
(a) Top 1% Income Share



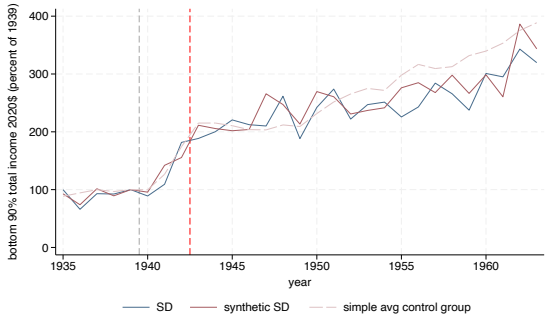
(b) Top 1% Income



(c) Bottom 90% Income Share



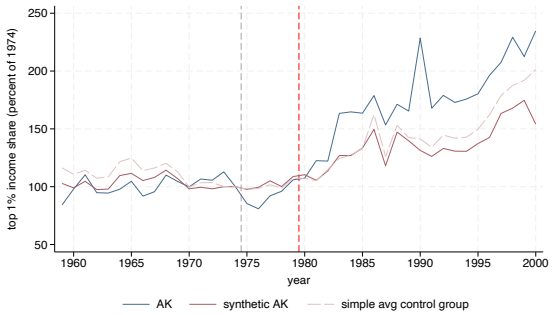
(d) Bottom 90% Income



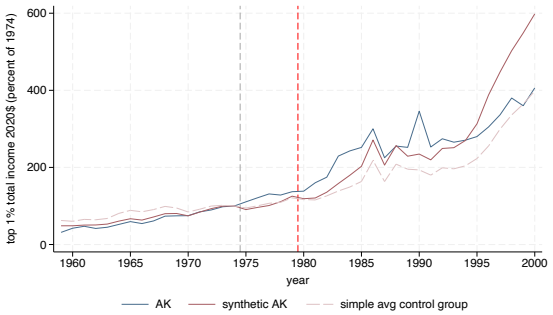
Notes: These figures show results where the synthetic control weights are selected using only the outcome variable (see Figure 10 notes for other details). As a result, the composition of donor states varies across (a)-(h).

Figure D.31: Alaska canceled $\tau^{personal} = 14.5\%$, kept $\tau^{corporate} = 9.4\%$ in 1980
Matching Only on Outcome

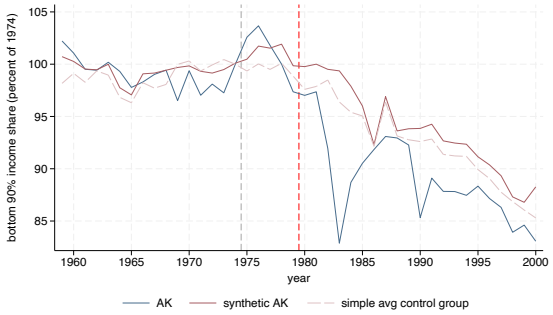
(a) Top 1% Income Share



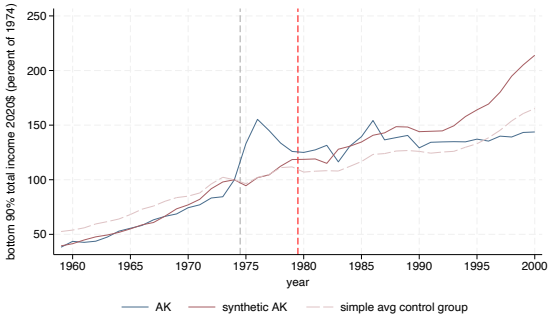
(b) Top 1% Income



(c) Bottom 90% Income Share



(d) Bottom 90% Income



Notes: These figures show results where the synthetic control weights are selected using only the outcome variable (see Figure 10 notes for other details). As a result, the composition of donor states varies across (a)-(h).

E Using All Tax Rate Variation: Additional Evidence

Table E.1: Conventional TWFE Estimates of Personal and Corporate Income Taxes on (Log) Income Shares

	Bottom 90%	Top 10-1%	Top 1%	Top 1-0.1%	Top 0.1-0.01%	Top 0.01%
Panel A						
Top Personal Rate	0.001	-0.000	-0.006*	-0.005*	-0.007*	-0.013*
	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)
Panel B						
Top Corporate Rate	0.004*	0.002	-0.017**	-0.009**	-0.022**	-0.036**
	(0.002)	(0.003)	(0.007)	(0.004)	(0.009)	(0.014)
Panel C						
Top Personal Rate	-0.000	-0.001	0.000	-0.002	0.001	0.001
	(0.001)	(0.002)	(0.003)	(0.002)	(0.004)	(0.007)
Top Corporate Rate	0.004*	0.003	-0.017**	-0.008*	-0.023**	-0.037**
	(0.002)	(0.003)	(0.008)	(0.004)	(0.010)	(0.017)
Observations	5,016	5,016	5,016	5,016	5,016	5,016
Avg. Top Personal Rate	4.40	4.40	4.40	4.40	4.40	4.40
Avg. Top Corporate Rate	4.46	4.46	4.46	4.46	4.46	4.46
Avg. Top X% Share	62.4	24.4	13.2	8.1	3.1	2.0

Notes: This table presents results from estimating the conventional two-way fixed effects specification in Equation (4) using all states from 1917 to 2018. The outcome variables are log income shares for each group shown in the first row e.g., ln(top 1% share). The independent variables are state top personal and/or corporate income tax rates, state and year fixed effects, and controls for tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$, standard errors clustered at the state level.

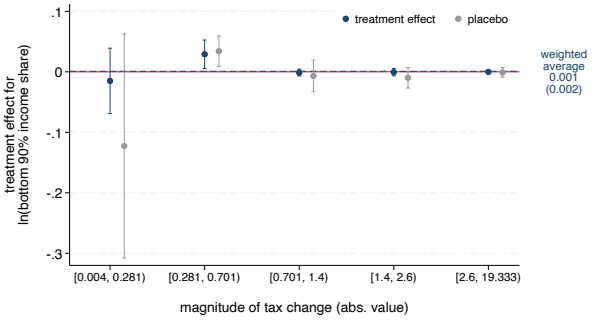
Table E.2: Conventional TWFE Estimates of Personal and Corporate Income Taxes on (Log) Real Incomes

	Bottom 90%	Top 10-1%	Top 1%	Top 1-0.1%	Top 0.1-0.01%	Top 0.01%	Total income
Panel A							
Top Personal Rate	-0.008	-0.010	-0.016**	-0.014**	-0.017**	-0.022**	-0.010
	(0.006)	(0.006)	(0.008)	(0.007)	(0.008)	(0.010)	(0.006)
Panel B							
Top Corporate Rate	-0.017	-0.018	-0.037**	-0.030*	-0.043**	-0.057***	-0.021
	(0.012)	(0.013)	(0.016)	(0.015)	(0.017)	(0.021)	(0.013)
Panel C							
Top Personal Rate	-0.003	-0.004	-0.003	-0.004	-0.001	-0.001	-0.003
	(0.005)	(0.005)	(0.006)	(0.005)	(0.007)	(0.010)	(0.005)
Top Corporate Rate	-0.015	-0.016	-0.036**	-0.027*	-0.042**	-0.056**	-0.019
	(0.012)	(0.013)	(0.017)	(0.015)	(0.018)	(0.023)	(0.013)
Observations	5,016	5,016	5,016	5,016	5,016	5,016	5,016
Avg. Top Personal Rate	4.40	4.40	4.40	4.40	4.40	4.40	4.40
Avg. Top Corporate Rate	4.46	4.46	4.46	4.46	4.46	4.46	4.46
Avg. Top X% Real Income	55.1	24.1	14.8	8.4	3.6	2.8	94.0

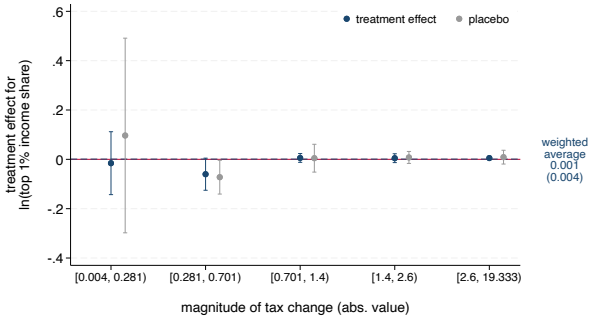
Notes: This table presents results from estimating the conventional two-way fixed effects specification in Equation (4) using all states from 1917 to 2018. The outcome variables are log real income earned by each group shown in the first row. The independent variables are state top personal and/or corporate income tax rates, state and year fixed effects, and controls for tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). The average Top X% real incomes are expressed in 2020 billions of dollars. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$, standard errors clustered at the state level.

Figure E.32: Effect of Personal Income Tax on (Log) Income Shares by Size of Tax Change

(a) Bottom 90% Share



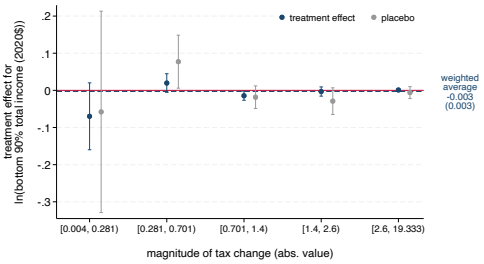
(b) Top 1% Share



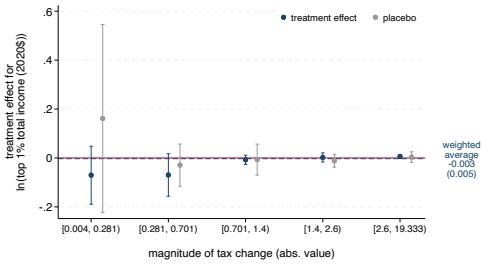
Notes: These figures show the effect of top personal income tax changes on (log) income shares, separately by quintile of tax change (in absolute value), using the methodology from de Chaisemartin et al. (2025). Results for all quintiles are normalized to be per 1 percentage point change in tax. Estimates control for the top corporate tax rate as an additional treatment, and also include controls for tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$, standard errors clustered at the state level.

Figure E.33: Effect of Personal Income Tax on (Log) Total Incomes by Size of Tax Change

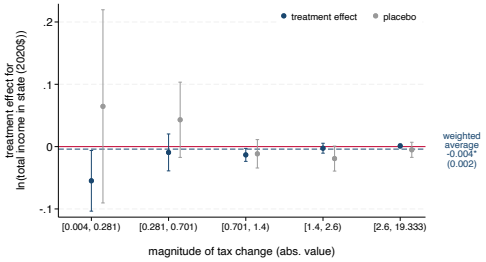
(a) Bottom 90% Income



(b) Top 1% Income



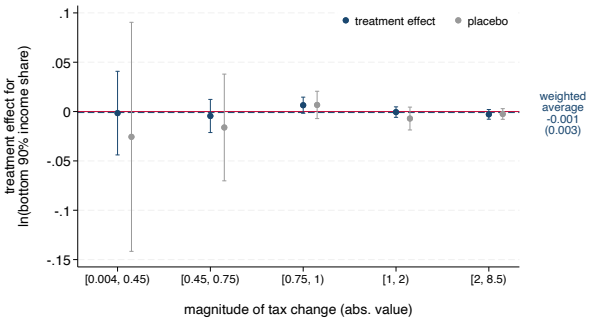
(c) Total Income



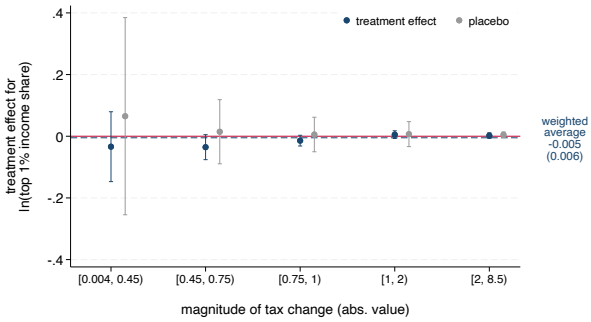
Notes: These figures show the effect of top personal income tax changes on (log) total incomes, separately by quintile of tax change (in absolute value), using the methodology from de Chaisemartin et al. (2025). Results for all quintiles are normalized to be per 1 percentage point change in tax. Estimates control for the top corporate tax rate as an additional treatment, and also include controls for tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$, standard errors clustered at the state level.

Figure E.34: Effect of Corporate Income Tax on (Log) Income Shares by Size of Tax Change

(a) Bottom 90% Share



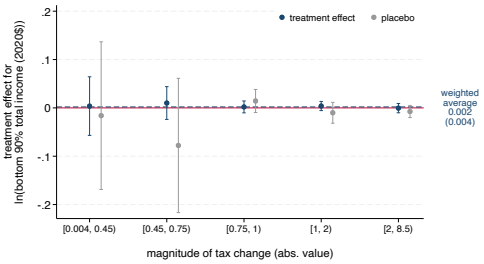
(b) Top 1% Share



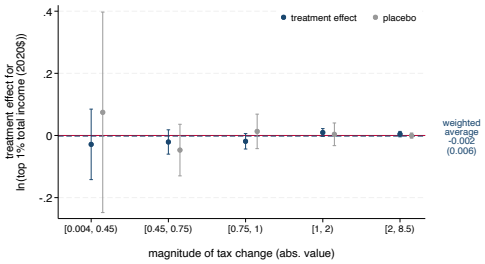
Notes: These figures show the effect of top corporate income tax changes on (log) income shares, separately by quintile of tax change (in absolute value), using the methodology from de Chaisemartin et al. (2025). Results for all quintiles are normalized to be per 1 percentage point change in tax. Estimates control for the top personal tax rate as an additional treatment, and also include controls for tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$, standard errors clustered at the state level.

Figure E.35: Effect of Corporate Income Tax on (Log) Total Incomes by Size of Tax Change

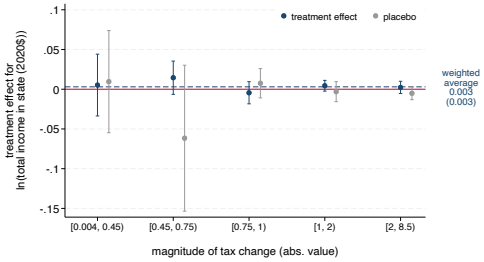
(a) Bottom 90% Income



(b) Top 1% Income



(c) Total Income



Notes: These figures show the effect of top corporate income tax changes on (log) total incomes, separately by quintile of tax change (in absolute value), using the methodology from de Chaisemartin et al. (2025). Results for all quintiles are normalized to be per 1 percentage point change in tax. Estimates control for the top personal tax rate as an additional treatment, and also include controls for tax rates & if the tax is non-zero for sales, gasoline, alcohol, and cigarette taxes, the percent of population that is Black, population, lagged population (5, 10, and 15 years), lagged unemployment and employment ratios (3 and 5 years), and lagged total state expenditures (3 and 5 years). * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$, standard errors clustered at the state level.