The introduction of the income tax, fiscal capacity, and migration: evidence from U.S. States

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The Introduction of the Income Tax, Fiscal Capacity, and Migration: Evidence from U.S. States*

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Abstract

We evaluate how fiscal capacity and migration respond to the introduction of the individual income tax, drawing on new panel data on U.S. states from 1900 to 2010. We find that states that introduced the income tax experienced a 12 percent increase in total revenue per capita in the near term, a 15 percent increase in the medium term, and a 17 percent increase in the long term. However, the introduction of the income tax did not significantly change the absolute level of revenue over the long term, at least for post-World War II adopters. To explain this difference in the per capita and absolute results, we show that the introduction of the income tax induced significant outmigration to non-income tax states by middle- and high-earning households.

Keywords: State Capacity, Institutional Change, Fiscal Reform, Taxation, Migration, USA

JEL codes: H11, H41, H71, N42, D78

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1 Introduction

The state's capacity to extract revenue matters for economic development. Well-funded states can provide the administrative infrastructure that supports a market economy (Besley and Persson, 2013). History suggests that broadening the tax base is key to expanding the government's fiscal capacity. The elimination of traditional tax privileges in the aftermath the French Revolution, for example, led to a large increase in the state's ability to tax (Dincecco, 2011). Over the twentieth century, the establishment of the income tax has been a major component of tax broadening (Wallis, 2000, Lindert, 2004, Aidt and Jensen, 2009).

Despite its historical importance, there is a dearth of evidence on how the introduction of the income tax impacts the government's capacity to extract revenue. All else constant, broadening the tax base should "mechanically" increase government funds. The introduction of a major new tax, however, may crowd out other revenue sources and/or induce taxpayers to move to lower-tax jurisdictions, leaving the total amount of revenue unchanged. If fleeing taxpayers are high-earning, then tax broadening may even reduce revenue per capita. The extent to which introducing the income tax actually expands the state's fiscal capacity thus remains open to debate.

To make progress on this front, this paper analyzes the consequences of the introduction of the individual income tax on revenue, expenditure, population, and interstate migration across U.S. states. We construct a new panel database, drawn in part from archival data, that spans the entire twentieth century and the start of the twenty-first century. Our empirical design exploits the fact that individual states introduced the income tax in a staggered fashion over a span of 65 years.

We find that the introduction of the income tax increased total revenue per capita in adopting states by 12 percent in the near term (years 0-3 after introduction), by 15 percent in the medium term (years 10-19), and by 17 percent in the long term (years 20-30). Total expenditure per capita displayed similar trends.

We show, however, that the fiscal responses were heterogeneous over time. There are important differences in income tax introductions before and after World War II. Lower personal exemptions and tax withholding characterized post-World War II introductions of the income tax. Furthermore, improvements in transportation infrastructure reduced moving costs in the post-World War II era. For these reasons, we estimate separate effects for "early adopters" (i.e. prior to the United States' entrance into World War II) and "late adopters" (i.e. following World War II). We find that states that introduced the income tax prior to World War II experienced significant increases in total revenue in both per capita and absolute levels over the near, medium, and long terms, while those that introduced it after World War experienced significant increases in revenue per capita over the same terms (i.e. near, medium, and long), but did not experience any medium or long-term increase in revenue in absolute levels.

We show that the introduction of the income tax in the post-World War II era induced significant outmigration to states that did not have the income tax. This helps explain the difference in the per capita and absolute fiscal responses that we find for late adopters. We find that middle- and high-earning households were the most likely to leave after the income tax was introduced. By contrast, we show that introductions of the income tax prior to World War II did not stimulate outmigration. We argue that lower personal exemptions, tax withholding, and improvements in transportation infrastructure help account for the difference in population responses to the introduction of the income tax in the pre- and post-World War II eras.

Our results indicate that introducing the income tax allowed U.S. states to significantly increase their fiscal capacity on a per capita basis. The income tax thus appears to be a key tool in expanding the extractive role of modern governments (Besley and Persson, 2013). Nonetheless, population mobility provided a partial check on the state's fiscal capacity, at least in absolute terms. Our findings thus suggest that the return on fiscal capacity investments is contingent on the elasticity of the tax base.

To be clear, our results concern state-level tax policy only. They do not speak to what the long-run revenue response to a large cut in or the elimination of the federal income tax might be, a scenario in which the costs of "exit" via migration to a lower-tax jurisdiction would likely be much greater. In a similar vein, they are unlikely to directly apply to potential migration responses to major changes in income tax policy in supra-national federations such as the European Union, a context in which languages, cultures, and institutional structures vary significantly across jurisdictions.

Our empirical context offers two main advantages. First, our panel dataset – spanning 1900 to 2010 – allows us to document the long-term impacts of the introduction of the income tax. This is important as short-run and long-run impacts can differ due to taxpayer learning or adjustment costs. Second, our within-country context enables us to eliminate the influence of a host of potential confounders that would hinder a cross-national analysis. Political, cultural, and economic differences across U.S. states are quite small relative to such differences across countries.

A standard difference-in-differences design thus eliminates several potential sources of bias. This design assumes that outcomes in adopting states and non-adopting states would have followed parallel trends on average had the income tax not been introduced. This assumption would be violated if the timing of adoption were correlated with other factors that influenced the outcomes. Penniman (1980, ch. 1) suggests that the timing of the introduction of the income tax by U.S. states was often a function of idiosyncratic political factors. For example, voters in both Wisconsin and Ohio approved referenda allowing for an income tax around the same time in the early 1900s, yet only Wisconsin introduced this tax at that time. The Ohio legislature failed to introduce the income tax until 1971.

While idiosyncratic political factors often influenced the timing of introduction, states may have adopted the income tax in the face of adverse demographic or fiscal trends, potentially violating the parallel trends assumption. To guard against this possibility, we condition on lagged changes in population, revenue, and expenditure. Our method thus imposes a weaker version of the parallel trends assumption in which adopting and non-adopting states with similar recent population and fiscal dynamics would have followed parallel trends in the absence of the introduction of the income tax.

Traditional estimators in staggered difference-in-differences designs may not recover a reasonably weighted average treatment effect in the presence of treatment effect heterogeneity (de Chaisemartin and D'Haultfœuille, 2020, Goodman-Bacon, 2021, Sun and Abraham, 2021, Borusyak, Jaravel and Spiess, 2022). To address this challenge, our estimator compares adopting states with "clean" controls (i.e. states that never adopted over the analysis window). In addition, we show that our results are robust to controlling for the introduction of the sales tax, state-level economic shocks, and region-by-year effects. Furthermore, our results are robust to allowing for cohort-specific treatment effects and relaxing functional form assumptions via inverse probability weighting.

A growing literature examines both the determinants (Besley and Persson, 2013, Kleven, Kreiner and Saez, 2016, Gillitzer, 2017, Jensen, 2022) and the economic and political consequences of state capacity (Gordon and Li, 2009, Dincecco and Prado, 2012, Acemoglu, García-Jimeno and Robinson, 2015, Casaburi and Troiano, 2016, Dell, Lane and Querubin, 2018). Historical accounts indicate that the development of the state's fiscal capacity was a hard-fought process (Dincecco, 2011, Gennaioli and Voth, 2015, Hoffman, 2015). Our study sheds new light on a key mechanism – the introduction of the income tax – through which governments have increased their capacity to extract revenue. To our knowledge, this is the first paper to systematically analyze the introduction of the state-level income tax across the United States. Furthermore, we address a novel question in this literature: To what extent does outmigration limit the impact of fiscal capacity investments?

In addition, our paper contributes to the literature on mobility responses to taxes (Kleven, Landais, Muñoz and Stantcheva, 2020). Recent work, generally concerned with top earners, shows how taxpayers migrate across or within countries in response to changes in income tax rates.[1] To our knowledge, our paper is the first to estimate migration responses to the *introduction* of a new income tax.

Our study proceeds as follows. Section 2 provides the historical context. Section 3 describes the construction of our dataset. Section 4 discusses our empirical strategy, and Section 5 presents the fiscal results. Section 6 examines how interstate migration responds to the introduction of the income tax. Section 7 provides concluding remarks.

2 Introduction of the State-Level Individual Income Tax

Wisconsin introduced the first modern state-level income tax in 1911. In turn, the state established a tax commission in charge of the assessment of the income of individuals and corporations. Furthermore, the state bolstered enforcement by requiring corporations to report the name, address, and wages of any employee whose wages the firm wished to deduct from gross income (Comstock, 1921, pp. 39-41). Over time, the twin pillars of centralized administration and information reporting became mainstays of modern state income taxes.

Our analysis uses the year that the *individual* income tax was (permanently) introduced. For more than 60 percent of adopting states, the distinction between the individual or corporate income tax is immaterial, because both were introduced in the same year. For 75 percent of adopting states, the individual income tax and the corporate income tax were introduced within three years of each other.[2]

Figure 1 shows a map of continental U.S. states shaded according to the decade of introduction

[1] Cross-country analyses include Kleven, Landais and Saez (2013), Kleven, Landais, Saez and Schultz (2014), and Akcigit, Baslandze and Stantcheva (2016), while within-country analyses include Bakija and Slemrod (2004), Liebig, Puhani and Sousa-Poza (2007), Young, Varner, Lurie and Prisinzano (2016), Moretti and Wilson (2017), Schmidheiny and Slotwinski (2018), Agrawal and Foremny (2019), and Akcigit, Grigsby, Nicholas and Stantcheva (2022).

[2] This suggests that we cannot cleanly disentangle the impacts of the two types of income taxes. It may thus make sense to view our estimates as reflecting the joint effect of introducing both taxes.

of the individual income tax. Appendix Table A.1 lists the specific years of introduction.[3] Fourteen states scattered across the Midwest, Northeast, and South introduced income tax laws in the 1910s and 1920s. Eighteen additional states introduced the income tax in the 1930s. These states were scattered across every region, with the majority located west of the Mississippi River. No states introduced the individual income tax over the 1940s or 1950s. Seven more states introduced the individual income tax over the 1960s, and four more states over the 1970s. Many "late adopters" (i.e. post-World War II) were located in the Rust Belt. Six states never introduced a permanent individual income tax.[4]

Initially, the individual income tax accounted for a modest share of state budgets, but it grew in importance over time. State income taxes were on average 6 percent of total revenue and 10 percent of total taxes between 1922 and the year of the United States' entrance into World War II in 1941. These amounts rose in the post-World War II era, averaging 12 percent and 23 percent, respectively, between 1946 and 1980.[5] Our analysis in Section 5 will test for crowd out of pre-existing revenue sources by the income tax.

Income tax introductions were historically contingent events, at least to an extent. Take, for example, the early efforts of two states, Wisconsin and Ohio, as described by Penniman (1980, ch. 1). Voters in both states approved constitutional amendments allowing for a state income tax, in Wisconsin in 1908 and in Ohio in 1912. Each referendum passed with a large majority. Wisconsin legislators established an income tax in 1911. This survived a legal challenge when the Wisconsin Supreme Court declined to overturn the law (Mehrotra, 2013, ch. 4). In Ohio, by contrast, opponents of the income tax blocked its passage in the legislature following the referendum. It was not until 1971 that Ohio, facing budgetary problems, introduced an income tax with bipartisan support (Penniman, 1980, ch. 1).

The timing of the introduction of the income tax was also historically contingent in other states.

[3]Hawaii introduced an income tax in 1901, while Alaska introduced one in 1949 (this tax was repealed in 1979). However, neither became a state until 1959, limiting data availability. We thus exclude both Alaska and Hawaii from our analysis.

[4]Once a state introduced the individual income tax, it generally retained it. There are only a few cases where states have repealed or fundamentally changed the income tax. Temporary income taxes were passed in South Dakota (1935-1942) and West Virginia (1935-1941). Since our analysis focuses on permanent adoptions, we omit these cases. New Hampshire (since 1923) and Tennessee (since 1931) both have individual income taxes that only tax interest and dividends, while from 1969 to 1990 Connecticut only taxed the capital gains and dividends of individuals. (Connecticut introduced a progressive income tax in 1991.) We treat all three states (i.e. Connecticut, New Hampshire, and Tennessee) as having individual income taxes in our baseline analysis. As a robustness check, however, we code New Hampshire and Tennessee (along with Connecticut from 1969 to 1990) as not having individual income taxes. The main results continue to hold (not reported).

[5]Calculations based on aggregate statistics reported in the Census of Governments (Appendix Table A.2).

In Oregon, the legislature passed an income tax bill in 1923, only to have it overturned by a referendum the next year (National Industrial Conference Board, 1930). Undeterred, the legislature passed income tax bills in 1925, 1927, and 1928, all of which were rejected by voters. It took until 1930 for Oregon to successfully implement an income tax (Warren, 1937). In Nebraska, legislation was narrowly defeated in 1957, and another income tax bill passed in 1965, only to be repealed by voters shortly thereafter (Columbus Daily Telegram, 1967). Nebraska eventually introduced the income tax in 1967.

Early laws establishing an income tax were ruled unconstitutional in Alabama (1920), Arkansas (1925), Illinois (1932), Washington (1932, 1935), and Pennsylvania (1935), while efforts to amend the state constitution to allow for an income tax did not always succeed. Failed attempts occurred in Pennsylvania (1913, 1919, 1920, 1936, 1937, 1939, 1941, 1959), Minnesota (1920), Colorado (1922), Michigan (1924), Indiana (1926, 1930), and Washington (1934, 1936, 1938, 1942, 1970, 1973). In New Mexico, the legislature repealed its first income tax law in 1920. In Iowa, the state assembly passed an income tax bill in 1932 that was subsequently defeated in the state senate. In Colorado, the governor vetoed an income tax bill passed by the legislature in 1935.[6] With the exception of Washington, however, all of these states would eventually introduce an income tax.

This evidence suggests that income tax introductions were often historically contingent. Nevertheless, our analysis ahead will account for selection into the introduction of the income tax based on recent demographic and fiscal shocks. Furthermore, we will show that our results are robust to controlling for other policy changes and economic shocks.

Finally, note that the threat of capital flight could prevent the introduction of the income tax. Policymakers in Rhode Island, for example, feared losing its wealthiest residents. According to Myers (2021, ch. 2), “concerns about alienating the summer denizens of Newport played a major role in keeping the income tax off Rhode Island’s books until the early 1970s.”

[6]The sources for the material in this paragraph include Bigham (1929), Bailey (1930), Groves (1932), Manning (1935, 1936, 1938, 1939, 1941), Blakey and Johnson (1941), McKenna (1960), and Spitzer (1993).

3 Data

3.1 Revenue and Expenditure

For years prior to 1942, we hand coded state-level fiscal data from archival census reports. The earliest fiscal data are available for 1902 from the U.S. Department of Commerce's "Wealth, Debt, and Taxation: 1902." Fiscal data are also available for 1903 and 1913 from the U.S. Department of Commerce's "Wealth, Debt, and Taxation: 1913." We cover the rest of the pre-1942 period using the Statistical Abstract of the United States, which contains fiscal data for the years 1915, 1922-1932, 1937, 1938, and 1940. To our knowledge, we include all available state fiscal data prior to 1942. State fiscal data are available every two years from 1942 to 1948 and annually from 1950 to 2010 from the Census of Governments.

We focus on total revenue, total expenditure, and state property tax revenue, as these outcomes are consistently available.[7] We measure all fiscal variables in constant 2010 USD. The resulting panel spans all continental 48 states plus Washington D.C. between 1902 and 2010.[8] Appendix Table A.2 lists our fiscal data sources.

Annual state population data from 1900 to 2010 come from the census as well as intercensal estimates from the Census Bureau.[9]

3.2 Tax Rates

For the years 1911 to 1940, we hand coded state-level income tax rates, brackets, and exemptions. For years prior to 1930, the National Industrial Conference Board (1930) provides a history of tax rates, brackets, and exemptions in each state that had an individual or corporate income tax by 1929, while the U.S. Department of Commerce's "Digest of State Laws Relating to Taxation and Revenue: 1922" offers information on state tax codes in 1922. For the years 1930 to 1940, the National Tax Association provides annual reports that detail changes to state tax codes, while the U.S. Department of Commerce's "Digest of State Laws Relating to Net Income Taxes: 1938" offers information on state income tax codes in 1938. We draw on state-specific historical summaries and

[7] Data on total tax revenue, income tax revenue, and expenditure broken down by function (e.g. education, health) are generally unavailable prior to 1931.

[8] The 1922-1932 fiscal data include a few extreme outliers which imply implausible deviations from budget balance. We thus exclude fiscal observations in the pre-World War II era that are in the top 5 percent or bottom 5 percent in terms of the expenditure-to-revenue ratio. In addition, we linearly interpolate fiscal variables (in logs) from 1933 to 1936 for states whose income tax status did not change between 1932 and 1937. This mitigates the problem of the sample size falling for our medium-run estimates, and does not change our short- or long-run estimates.

[9] Available at `https://fred.stlouisfed.org/release?rid=118`.

contemporary newspaper accounts to cover any remaining details over this period. For 1941-2003, we take data on state income tax rates, brackets, and exemptions from the World Tax Database.[10] We extend these data to 2010 using information from the Tax Policy Center.[11]

The resulting panel includes the top and bottom marginal state income tax rates for every year in our sample, as well as the top and bottom state income tax brackets and personal exemption for single filers for most years.[12] We add data from Akcigit et al. (2022) on the effective marginal and average tax rates at 90th percentile income between 1934 and 2007.[13]

To proxy for the breadth of the tax base, we use the personal exemption, an amount deducted from gross income in computing taxable income. The annual tax savings due to a personal exemption of X is approximately τX, where $\tau \in [0,1]$ is the individual's marginal tax rate.[14]

Appendix Table A.3 lists our data sources for state income tax rates, brackets, and exemptions.

3.3 Sales Tax

We take data on the years of adoption of the state-level sales tax from Gillitzer (2017) and Fox (2004). Gillitzer (2017) provides data on state sales tax rates for 1934 and 1938, the World Tax Database provides them for 1946 to 2002, and Suárez Serrato and Zidar (2018) provide them for 1975 to 2010.[15]

3.4 Tax Withholding

We take data on the presence of state tax withholding from Penniman (1980, pp. 154-5) and the Advisory Commission on Intergovernmental Relations (1977, pp. 206-7). The federal government implemented tax withholding in 1943. Thereafter, states adopted withholding in a staggered fashion from 1948 to 1987.

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[10] Available at `https://www.bus.umich.edu/otpr/otpr/default.asp`.

[11] Available at `https://www.taxpolicycenter.org/statistics/state`.

[12] Nebraska, Rhode Island, and Vermont defined the individual income tax liability to be a percentage of the federal individual income tax liability in some years. For these cases, we define the top and bottom marginal tax rates as the product of the aforementioned percentage and the corresponding federal marginal tax rate, and define the personal exemption as the federal personal exemption.

[13] Akcigit et al. (2022) derive these data from the tax calculator program of Bakija (2019).

[14] Eight states (Arkansas, Arizona, Iowa, Kentucky, Minnesota, North Dakota, South Dakota, and Wisconsin) have in some years implemented a personal exemption in the form of a tax credit (i.e. an amount deducted from the tax liability). For these cases, we define the personal exemption as the tax credit divided by the bottom marginal income tax rate. This is the exemption threshold which would produce the same tax savings as the tax credit for an individual in the bottom tax bracket.

[15] The first adoption of the sales tax occurred in 1932. We impute the rates in 1932-1933 using the rate in 1934. We then impute the rates during 1935-1937 as the average of the 1934 and 1938 rates, and impute the rates during 1939-1945 as the average of the 1938 and 1946 rates.

Table 1 displays the summary statistics for the fiscal variables in our analysis, while Appendix Figure B.1 depicts the trends over time.

4 Empirical Strategy

A standard difference-in-differences (DiD) design faces three basic challenges in our context.

The historical material in Section 2 suggests that income tax introductions were often contingent events. Nevertheless, states may have selected into the introduction of the income tax based on past demographic or fiscal shocks. For example, states may have introduced the income tax in the face of budgetary stress. Since past fiscal trends may be correlated with future trends, the parallel trends assumption may be violated. We address this challenge by controlling for lagged changes in population, expenditure, and revenue. This enables us to rely on a weaker, *conditional* parallel trends assumption: adopting and non-adopting states with the same recent demographic and fiscal trends would have experienced parallel trends moving forward in the absence of the introduction of the income tax.

Second, the timing of the introduction of the income tax may have been correlated with regional shocks, state-level economic shocks, or major policy changes which also impacted demographic and fiscal outcomes. To address this challenge, we control for these shocks in robustness checks.

Third, the timing of the introduction of the income tax varied across states. In turn, a two-way fixed effects model that contains a single "post income tax" dummy may not recover a reasonably weighted average treatment effect in the presence of treatment effect heterogeneity (de Chaisemartin and D'Haultfœuille, 2020, Goodman-Bacon, 2021). A similar problem may occur in a standard event-study specification (Sun and Abraham, 2021, Borusyak et al., 2022). The overriding concern here is that estimators based on such models implicitly use already treated units as controls in certain years.[16]

To address this third challenge, we estimate a "stacked" DiD model (Gormley and Matsa, 2011, Deshpande and Li, 2019, Cengiz, Dube, Lindner and Zipperer, 2019, Baker, Larcker and Wang, 2022). We construct separate, 50-year panel datasets for each adoption cohort c. Each dataset in-

[16]Another concern is that interstate migration may lead to a violation of the stable unit treatment value assumption (SUTVA), as the introduction of the income tax in one state could affect the population of non-introducing states. Since taxpayers have many states to choose to move to, however, bias due to spillover effects is likely to be small. As we show in Section 6, migration responses to the introduction of the income tax were not concentrated among neighboring states.

cludes "treated" states that adopted the income tax in year c and "clean control" states that never had an income tax during the 50-year window around year c. We combine the datasets and estimate the model

$$
\begin{aligned}
Y_{c,i,t+h} - Y_{c,i,t-1} = \beta^{h,e} \cdot (D_{c,i,t} - D_{c,i,t-1}) \cdot 1_{c \le 1945} + \beta^{h,\ell} \cdot (D_{c,i,t} - D_{c,i,t-1}) \cdot 1_{c>1945} \\
+ \gamma^{h,e\prime} \cdot \boldsymbol{X}_{c,i,t} \cdot 1_{c \le 1945} + \gamma^{h,\ell\prime} \cdot \boldsymbol{X}_{c,i,t} \cdot 1_{c>1945} + \phi^h_{c,t} + \varepsilon^h_{c,i,t} \qquad (1)
\end{aligned}
$$

for different time horizons h. The variable $Y_{c,i,t}$ is a fiscal outcome (measured in logs) in dataset c, state i, and year t. The binary variable $D_{c,i,t}$ equals 1 if state i has an income tax in year t and zero if the state does not have an income tax in this year. This variable is set to missing in the years following the adoption year to ensure that treated states are never used as controls. The model allows income tax introductions and covariates to have different effects for "early adopters" e prior to the United States' entrance into World War II in 1941 and "late adopters" ℓ following World War II for the reasons described in Section 1, including differences in personal exemptions and tax withholding, as well as improvements in transportation infrastructure, between the pre- and post-World War II eras.[17]

We control for cohort-by-state effects via differencing. We also control for covariates $\boldsymbol{X}_{c,i,t}$ and cohort-by-year effects $\phi^h_{c,t}$. The covariate vector $\boldsymbol{X}_{c,i,t}$ includes the lagged 3-year and 5-year changes in log revenue and log expenditure, and the lagged 5-year, 10-year, and 15-year changes in log population. We are able to control for medium-run changes in demographics, but not in fiscal outcomes, due to differences in data availability. We define the lagged and forward differences of our variables in terms of two-year periods due to gaps in the early fiscal data.[18]

The OLS estimator for $\beta^{h,e}$ in Equation (1) identifies an average of cohort-specific treatment effects for early adopting states, weighted by the size of the cohort-specific dataset and the vari-

[17]In Equation 1, we use the subscript 1945 to denote the partitioning between the pre- and post-World War II eras. Since no state introduced the income tax during the time in which the United States participated in World War II (i.e. 1941-45), this subscript is consistent with our use of the term "pre-World War II" in the text.

[18]For example, the lagged 5-year change is the average value 5-6 years prior to income tax introduction minus the average value 1-2 years prior. The fiscal estimates thus exploit the 15 income tax introductions that occurred from 1929 to 1934 and the 11 income tax introductions that occurred from 1961 to 1976, as these are the adopting states for which the lagged fiscal changes can be measured. Thus, all pre-trend estimates are based on a fixed sample of states, and all adopting states have non-missing data in the reference year (1-2 years pre-introduction). The set of adopting states is fixed in all periods for late adopters. For early adopters, the set of adopting states in the pre-introduction periods (1-8 years pre-introduction) and the periods 8-30 years post-introduction is fixed. Due to a gap in the fiscal data from 1933 to 1936, however, there is no fixed sample of early adopters that have non-missing data across every event period, as these introductions occurred from 1929 to 1934. Taking 2-year averages does not totally resolve the problem, as the gap is four years in length.

ance of treatment status in the dataset (Gardner, 2021). All weights are positive. The OLS estimator for $\beta^{h,\ell}$ identifies a similarly weighted average treatment effect for late adopting states. We report estimates of the weighted average effect, $\omega_e \beta^{h,e} + \omega_\ell \beta^{h,\ell}$, where ω_e is the early-adopting share and ω_ℓ is the late-adopting share, as well as individual estimates of $\beta^{h,e}$ and $\beta^{h,\ell}$. We show in Section 5 that allowing the impact of the introduction of the income tax and the covariates to vary by cohort, and taking the average of the cohort-specific treatment effects weighted by cohort size (as in Callaway and Sant'Anna, 2021, Sun and Abraham, 2021, and Wooldridge, 2021), yields similar results.

Equation (1) is quite flexible. Nevertheless, it still imposes two functional form assumptions, first that potential outcomes have a linear relationship with the covariates, and second that the treatment effects do not vary with the covariates. We will show ahead that the results remain similar when we relax both of these assumptions by using propensity-score reweighting.

We report standard errors that are robust to heteroskedasticity and clustering by state to allow for arbitrary within-state serial correlation. Since our analysis relies on a moderate number of clusters (i.e. 36 states), we also report confidence intervals based on the restricted wild cluster bootstrap (Cameron, Gelbach and Miller, 2008).[19]

5 Main Results

5.1 Selection into the Introduction of the Income Tax

To see how past demographic and fiscal shocks influence selection into tax broadening, Table 2 estimates hazard models of the probability of a state introducing the income tax. Population decline over the past 10 years weakly predicts the introduction of the income tax both before and after World War II. Conditional on lagged population trends, recent growth in revenue and expenditure predicts adoption before World War II, but not after. The coefficients on the revenue and expenditure variables are jointly significant ($p = 0.001$). Overall, these results lend credence to our decision to control for past demographic and fiscal shocks in our main specification.[20]

[19] We use the six-point weight distribution recommended by Webb (2014).

[20] Appendix Table B.1 reports average values of the lagged fiscal variables, lagged population, and income per capita for adopting and non-adopting states in the years that correspond to years of introduction of the income tax. This approximates "baseline" values for "treated" and "control" states at the time of the income tax introduction. We do not find any statistically significant differences between adopting and non-adopting states in the full sample. Dividing the sample into the pre- and post-World War II eras, we only find one statistically significant difference, for income per capita in the pre-World War II era. Our analysis will control for past shocks to log personal income per capita as a robustness check.

5.2 Fiscal and Population Responses

Table 3 reports the average fiscal responses to the introduction of the income tax over different time horizons. In the year of introduction and the following year, total revenue in absolute levels increases by 4.1 percent (S.E. = 2.2), and total expenditure in absolute levels increases by 4.1 percent (S.E. = 2.5) (Panels A and B). In the 2-3 years after adoption, revenue increases by 10.7 percent (S.E. = 3.1), and expenditure increases by 10.5 percent (S.E. = 3.9). The fiscal responses decline by roughly half in years 4-9 after introduction, to 5.4 percent and 5.7 percent, respectively. Revenue strongly rebounds over both the medium run and long run, increasing by 13.0 percent (S.E. = 6.2) in years 10-19 after introduction, and by 15.6 percent (S.E. = 6.9) in years 20-30. Expenditure rises by 12.4 percent (S.E. = 7.1) in the long run.

The fiscal responses are somewhat larger in per capita terms (Panels C and D). In the near run (years 2-3 after the introduction of the income tax), revenue per capita increases by 11.5 percent (S.E. = 3.0) and expenditure per capita increases by 11.3 percent (S.E. = 4.0). These responses decline by roughly half over the next five years, but strongly increase once more in the medium run (years 10-19) by 14.6 percent (S.E. = 5.0) and in the long run (years 20-30) by 16.7 percent (S.E. = 5.5) for revenue per capita, and correspondingly by 9.8 percent (S.E. = 5.7) and 13.5 percent (S.E. = 5.9) for expenditure per capita.

Figure 2 plots the fiscal responses to the introduction of the income tax over time in both absolute and per capita terms. There is no evidence of differential fiscal pre-trends eight years prior to introduction.[21]

Finally, Table 3 shows a slight decline in average state populations in response to the introduction of the income tax, which helps account for the differences in fiscal responses in absolute versus per capita levels (Panel E). There is no evidence of differential pre-trends for population (Appendix Figure B.2).

Overall, these results indicate that the introduction of the income tax significantly increased total revenue in the near run, medium run, and the long run, with a larger increase observed in per capita terms. The revenue and expenditure responses track each other over time. This is consistent with the widespread presence of balanced-budget requirements across states (Berry and Berry,

[21] Since we control for fiscal changes in the six years prior to the introduction of the income tax, we cannot test for differential pre-trends within six years of adoption. Due to missing fiscal data before World War II era, moreover, we cannot test for differential pre-trends beyond eight years prior to the introduction of the income tax without inducing changes in the sample composition. As we will show ahead, however, we are able to estimate differences in pre-trends over a longer period for late adopters.

1992).[22]

5.3 Early vs. Late Adopters

Table 4 indicates that early (i.e. pre-World War II) and late (i.e. post-World War II) introductions of the income tax had different impacts on total revenue in absolute levels (Panel A). Early adopters experienced a significant increase in revenue in the near term (years 2-3), medium term (years 10-19), and long term (years 20-30). In the long term, revenue was 28.5 percent higher (S.E. = 10.2). Late adopters, by contrast, only experienced a significant increase in revenue over the first decade after introduction. The responses for total expenditure in absolute levels track those for revenue (Panel B).

Table 4 shows that early and late adoptions had different impacts on revenue in per capita terms as well (Panels C and D). Adopters of the income tax prior to World War II did not experience any significant increase in per capita revenue until 20-30 years after introduction, when per capita revenue was 17.4 percent higher (S.E. = 8.3). Post-World War II adopters, however, experienced significant increases in per capita revenue in each of the three decades after introduction. Per capita revenue was 15.7 percent higher (S.E. = 5.6) for late adopters in the long run (years 20-30). Expenditure per capita exhibits similar patterns (Panel D).

Figure 3 plots the fiscal responses separately for early adopters and late adopters. There is no evidence of differential pre-trends for either group.[23]

The results for population in Table 4 help explain why we observe different fiscal impacts for early versus late adopters of the income tax (Panel E).[24] In the pre-World War II era, population rose in those states that introduced the income tax. Thus, revenue per capita was lower than revenue in absolute terms for early adopters.

In the post-World War II era, by contrast, population fell significantly in states that introduced the income tax. Our estimates indicate that population fell by 11.6 percent (S.E. = 6.2) in the medium term (10-19 years) and by 16.6 percent (S.E. = 8.1) in the long term (20-30 years). Thus, revenue per capita increased more than revenue in absolute terms for late adopters. In combi-

[22] Given that balanced-budget requirements themselves are endogenous to fiscal policy, there remains debate over whether they actually impact public finance outcomes. For quasi-experimental evidence in favor of the view that balanced-budget requirements matter, see Grembi, Nannicini and Troiano (2016).

[23] For late adopters, enough data are available to estimate pre-trends over a longer time window than for early adopters. The estimates indicate that late adopters and non-adopters were on similar trends in the 16 years leading up to introduction, conditional on covariates.

[24] There is no evidence of differential pre-trends for population for either group of adopters (Appendix Figure B.2).

nation, these results suggest that taxpayers left late-adopting states after the introduction of the income tax, preventing growth in the absolute size of state governments.[25] [26]

The magnitude of the long-run state population decline (i.e. 16.6 percent) that we estimate in response to the introduction of the income tax in the post-World War II era appears large.[27] To provide the proper context in which to interpret this magnitude, we look to historical state population growth. First, between 1960 and 1985, average state population growth across the United States was 28.9 percent. Second, there was significant variation in population growth between states. In Florida, which never introduced the income tax, the population grew by 81.9 percent over this period. In Texas, another non-income-tax state, the population grew by 52.5 percent. Thus, overall, given this historical context, the magnitude of our result for the long-run state population decline in response to the introduction of the income tax following World War II seems quite reasonable in size. We delve further into this point when we discuss our results for migration responses in Section 6.3.[28]

Three factors may help explain why late adopters were able to extract more revenue on a per capita basis than early adopters in the short term and medium term, and why population fell only after late adoptions.

First, most early adopters applied the income tax to a narrow base. Figure 4 shows that personal exemptions were much higher before World War II than after it. The broader tax bases established by late adopters of the income tax may have enabled states to extract greater revenue per capita, while simultaneously inducing more taxpayers to leave.[29]

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[25] Revenue per (potential) tax filer is an alternative approach to gauge fiscal capacity. The introduction of the income tax could impact revenue per capita (i.e. our baseline measure of fiscal capacity) and revenue per tax filer in different ways due to tax exporting, as cross-state commuters generally pay income taxes in the state in which they work, rather than where they reside. We are not able to analyze the role of tax exporting, however, as we are unable to locate historical data on state-level tax filers.

[26] Corporate income tax apportionment can also influence the relationship between state revenue and the location of economic activity. The weight given to sales in apportioning corporate income has been increasing since 1980, shifting tax revenue to the destination of the consumer. For post-World War II adoptions, the sales weight is on average 33 percent when the corporate income tax is introduced, and it rises to 47 percent after 30 years (Appendix Figure B.3).

[27] Technically, the units are log points, not percentages, which differ for large changes.

[28] Recall from Section 2 that the majority of states introduced both the individual income tax and the corporate income tax either in the same year or within three years of each other. This may further explain the large magnitude of our estimate for the long-run state population decline. The introduction of the individual income tax will arguably reduce the supply of workers in a state, while the introduction of the corporate income tax will arguably reduce demand for workers. Thus, both types of income taxes could have independent negative impacts on state population levels.

[29] Differences in state-level income tax rates are unlikely to explain the different effects of early versus late introductions of the income tax. Top marginal state income tax rates were similar before and after World War II, and bottom marginal state income tax rates were only slightly higher after post-World War II (Appendix Figure B.4).

Second, late adopters had better tax administration. After the introduction of federal tax withholding in 1943, states introduced withholding in a staggered fashion from 1948 to 1987. Late adoptions of the income tax frequently overlapped with the introduction of tax withholding. Figure 5 shows a sharp increase in income tax revenue as a share of either total revenue or total taxes in the first few years after the introduction of the income tax among late adopters (i.e. post-World War II), but not among early adopters (i.e. pre-World War II), for whom this increase was relatively gradual. This is consistent with improved state tax administration in the post-World War II era. Furthermore, tax withholding increased revenue collection in states that already had an income tax (Bagchi and Dušek, 2021).[30]

Third, improvements in transportation infrastructure over the second half of the twentieth century lowered the costs of moving. This was coupled with post-World War II increases in educational attainment. In tandem, these shifts promoted greater geographic mobility for individuals aged 50 and younger after 1945 (Rosenbloom and Sundstrom, 2004). Thus, location decisions may have become more sensitive to relative economic opportunities, including local tax environments.

5.4 Crowd Out

Figure 5 indicates that there was a significant increase in income tax revenue as a share of either total revenue or total taxes in response to the introduction of the income tax. To test for crowd out of pre-existing revenue sources, we estimate the effects of introducing the income tax on state property tax revenue and state sales tax revenue. Property taxes declined steadily from an average of 53 percent as a share of total state revenue at the start of the twentieth century to 4 percent in 1945. They remained small thereafter. For this reason, we restrict our test of crowd out of property tax revenue to early adopters prior to World War II. By contrast, state sales tax revenue data are not consistently available prior to the 1940s.[31] We thus focus our test of crowd out of sales tax revenue on late adopters after World War II.

Figure 6 suggests that property tax revenue did not fall in either absolute or per capita terms after early introductions of the income tax (top panel). For late adopters, the absolute level of sales tax revenue fell by approximately 20 percent two to three decades after the introduction of the income tax, though the estimates are imprecise (middle panel, left). However, sales tax revenue

[30] No early adopter introduced withholding until at least 18 years after it introduced the income tax. This may help explain the delayed increase in per capita revenue that we observe for early adopters (see Panel C of Table 4).

[31] We define sales tax revenue as total revenue from any general sales tax plus any specific taxes on items including alcohol, gasoline, and tobacco (i.e. "Sales and Gross Receipts Taxes" from the Census of Governments data).

per capita did not change by much after the introduction of the income tax (middle panel, right). Taken together, this suggests that the decline in sales tax revenue in absolute terms was due to falling population (recall Panel E of Table 4), particularly since states did not significantly alter sales tax rates following the introduction of the income tax (bottom panel).[32]

5.5 Robustness

Introduction of Sales Tax

If the timing of the introduction of the income tax coincided with other major fiscal changes such as the introduction of the sales tax, then this could bias our estimates. Figure 7 shows, however, that there was no correlation between the years of introduction of the income tax and the sales tax. Nevertheless, the fiscal results remain similar to the baseline results when we control for the introduction of the sales tax (Appendix Figure B.6).[33]

Economic Shocks

Recent economic shocks might spur the adoption of the income tax while simultaneously impacting fiscal outcomes, thereby generating bias. We account for the role of economic shocks in three ways. First, we control for changes in log personal income per capita. Second, we control for industry shift-share employment shocks (Bartik, 1991). Finally, we control for changes in state unemployment. In all cases, the results remain quite similar to the baseline results (Appendix Figure B.6).[34]

Fiscal Health

If a state was in poor fiscal health, then it may have been more likely to make tax innovations including the adoption of the income tax, particularly since nearly all states face a legal requirement to show balanced budgets (Berry and Berry, 1992).[35] However, the results remain similar when we control for the lagged fiscal deficit per capita (Appendix Figure B.6).

[32] We also test for crowd out of local government revenue for late adopters using Census of Governments data available from 1953 onward (Appendix Figure B.5). Total local revenue in both absolute and per capita levels temporarily increases due to a transitory increase in state grants to local governments after the introduction of the income tax. In the long term, total revenue, own-source revenue, and tax revenue for local government all fall in absolute levels, though the estimates are imprecise. These patterns are consistent with the decline in state populations for late adopters of the income tax. There are no long-run effects on local revenue per capita.

[33] We include an indicator variable that equals 1 if the sales tax was introduced in the previous 10 years or will be introduced within the next 10 years.

[34] Appendix B.1 describes the data sources and construction methods for the economic shock variables.

[35] Balanced-budget requirements do not imply that current expenditure cannot exceed current revenue each year, but only that borrowing must cover any excess spending. Some states are required to eliminate any incurred deficit in the following fiscal year, while others only need to pass a budget that balances in expectation.

Regional Shocks

Late adopters of the income tax (i.e. post-World War II) were generally located in the Northeast and Midwest (Figure 1). Thus, regional shocks could be another source of bias. To address this concern, we control for arbitrary regional shocks by replacing the cohort-by-year effects with cohort-by-region-by-year effects in Equation (1). Appendix B.2 describes how we construct the regions for this analysis. Given the inherent limitations of this approach (due to the available variation in the data), it should be treated as somewhat speculative.

Appendix Tables B.2 and B.3 report the results of this robustness check. The fiscal and population responses to the introduction of the income in the near term, medium term, and the long term remain quite similar to the baseline results. The exception is the magnitude of the long-run estimate for total revenue in absolute levels, which falls by approximately 5 percentage points relative to the baseline estimate (it remains statistically significant). In terms of heterogeneous effects, controlling for regional shocks reduces the size of the long-run estimates for early adopters of the income tax, while increasing them for late adopters. Late adopters of the income tax continue to display a larger impact on revenue and expenditure in per capita terms than in absolute terms. Finally, the magnitude of the long-run estimate for population decline for late adopters falls to 11.1 percent (relative to the baseline estimate of 16.6 percent) once we include cohort-by-region-by-year effects.

Average Cohort-Specific Treatment Effects

As described in Section 4, OLS estimation of Equation (1) recovers a positively weighted average of cohort-specific treatment effects, where the weights depend on the size of the cohort-specific dataset and the variance of treatment status. For robustness, we estimate separate treatment effects for each cohort and take the average weighting by cohort size (Sun and Abraham, 2021, Callaway and Sant'Anna, 2021, Wooldridge, 2021), rather than letting OLS determine the weighting of the treatment effects.

We use the model

$$Y_{c,i,t+h} - Y_{c,i,t-1} = \sum_{j \in \mathscr{C}} \beta^{h,j} \cdot (D_{c,i,t} - D_{c,i,t-1}) \cdot 1_{c=j} + \sum_{j \in \mathscr{C}} \gamma^{h,j\prime} \cdot \boldsymbol{X}_{c,i,t} \cdot 1_{c=j} + \phi^h_{c,t} + \varepsilon^h_{c,i,t} \qquad (2)$$

where $\mathscr{C}$ is the set of cohorts. This approach allows the impact of the covariates to vary by cohort.

Appendix Table B.4 reports estimates of the average of $\beta^{h,j}$ across all introductions of the income tax, weighted by cohort size, while Appendix Table B.5 reports separate weighted averages for early-adopting cohorts (i.e. pre-World War II) and late-adopting cohorts (i.e. post-World War II). Overall, the results remain very similar to the baseline estimates.

Relaxing Functional Form Assumptions

We relax the two (linear) functional form assumptions of Equation (1) by controlling for covariates via inverse-probability weighting. We adapt the semiparametric approach in Angrist and Kuersteiner (2011) to a panel context, following Acemoglu, Naidu, Restrepo and Robinson (2019) and Suárez Serrato and Wingender (2016). Our approach models the selection of states into the introduction of the income tax without specifying a parametric model for outcomes. Appendix B.3 describes the details of this empirical strategy. The results remain quite similar to the baseline estimates (Appendix Tables B.6 and B.7). Relative to the baseline, the IPW estimates display larger fiscal increases in response to the introduction of the income tax, as well as a larger population decline for late adopters (i.e. post-World War II).

Excluding Individual States

Finally, Appendix Figure B.7 displays estimates of our baseline model after excluding adopting states one by one. The results do not change by much after excluding any specific state. Thus, no single state appears to be pivotal.

6 Migration Responses

The results in the previous section indicate that the introduction of the income tax increased total revenue per capita in the near run, medium run, and the long run for both early (i.e. pre-World War II) and late adopters (i.e. post-World War II), while total revenue in absolute levels only increased in the medium or long runs for early adopters, but not for late adopters. This is because state populations declined significantly in late adopters in response to the introduction of the income tax. In theory, this population decline could be due to changes in fertility, mortality, and/or interstate migration. We do not, however, find evidence for any significant changes in fertility or mortality rates after the income tax was introduced (Appendix Figure B.8).[36] We thus turn our attention to whether interstate migration can help shed light on the declining population levels in states that introduced the income tax in the post-World War II era.

[36] Historical data on births, deaths, and the number of women of reproductive age are from Bailey, Clay, Fishback, Haines, Kantor, Severnini and Wentz (2018).

6.1 Data: Migration

To measure interstate migration flows, we use data from the Integrated Public Use Microdata Series of the U.S. Census (IPUMS) (Ruggles, et al., 2019) for every census year from 1900 to 2010. The data consist of 5-percent random samples of the U.S. population for 1900, 1930, 1960, 1980, 1990, and 2000, and 1-percent random samples for 1910, 1920, 1940, 1950, and 1970. The 2010 data are from the 2010 American Community Survey, a 1-percent random sample. Combining years yields a repeated cross section of households.

For years 1940 and 1960-2000, the census asked individuals to record the state where they lived five years ago. We are thus able to calculate 5-year gross migration rates at the state-pair level for these years using the entire sample of households.

We also use an alternative migration variable that can be constructed for every census year from 1900 to 2010. Following Rosenbloom and Sundstrom (2004), we limit our sample to households with a child aged 4-5. This means that the average birth year of the child will be close to five years before the census date. Using this subsample of households, we can construct 5-year migration rates under the assumption that the child's birth state, which the census always reports, was the household's state of residence five years prior to the census year.

The main advantage of the child-based measure is that it spans the entire analysis period. A potential disadvantage is that households with small children may be less mobile or otherwise less responsive to taxes than other types of households, meaning that our estimates may not be externally valid for all U.S. households. As our analysis will show, however, the child-based measure and the census 5-year migration measure produce similar results for the time periods in which they overlap.

To study migration flows across different income levels, we use household income data, which are available for census years 1940 to 2010.[37] One drawback of the income data is that household income likely changes when a household moves, and income is only measured at the end of each migration period. We thus use an occupational earnings score in our baseline analysis. This corresponds to the median wage earned in the respondent's occupation.[38] The occupational earnings

[37] In 1940 and 1960-2010, the census recorded the wage and salary income of every individual, which we aggregate to the household level. However, the 1950 census only asked one randomly selected ("sample-line") individual within each household to record their wage and salary income. We thus define household income in 1950 as the income of the head of household. For this year, we measure income and calculate income percentiles using only households for which the sample-line individual was the head of household, thereby reducing the sample size.

[38] The IPUMS variable is "ERSCOR50," which is a measure of the median earned income for the respondent's occupa-

score is attractive because it is available for all individuals and all years, and occupation is less likely to change due to a move. A potential disadvantage is that income varies within occupations, which could attenuate the difference in our estimates across occupational earnings groups. However, as we will show, the results based on income display similar patterns to those based on occupational earnings.

6.2 Empirical Strategy: Migration

We estimate a model of migration flows as a function of state differentials in the presence of the income tax. The dyadic regression is

$$\log(P_{odt}/P_{oot}) = \theta(D_{ot} - D_{dt}) + \gamma_{od} + \phi_{r(o,d),t} + u_{odt}, \tag{3}$$

where P_{odt} is the probability that a household living in state o (origin) moves to state d (destination) in year t, and P_{oot} is the probability that a household living in state o stays in state o. The indicator variable D_{dt} equals one if state d has an income tax. The variable D_{ot} is defined analogously for the origin state.[39]

We measure P_{odt}/P_{oot} as the number of households that move from state o to state d over a 5-year period, divided by the number of households that stay in state o. The tax variables and other covariates that are observed annually are measured in the middle of each 5-year period.[40]

The state-pair fixed effects γ_{od} absorb the impacts of origin and destination amenities and state-pair moving costs. The region-pair × year effects $\phi_{r(o,d),t}$ capture common shocks to migration within pairs of regions, which could be due to regional business cycles, region-specific transportation infrastructure (e.g. interstate highway system), or technology that differentially affects the desirability of certain regions (e.g. air conditioning).[41]

We explain how Equation (3) derives from a model of location choice in Appendix C. The key difference relative to standard random utility models is that the payoff to locating in a given state

tion. Income is standardized as a z-score and converted to a percentile, so that ERSCOR50 gives the percentage of persons working in an occupation with lower standardized median earnings than the occupation of the respondent. For households with multiple individuals in the labor force, we assign the maximum occupational earnings score.

[39] Dyadic regression is a common econometric method for studying migration (Young et al., 2016, Moretti and Wilson, 2017), trade (Tinbergen, 1962), war (Russett and Oneal, 2001), risk-sharing (De Weerdt, 2004), and other phenomena. Graham (2020a,b) provides an overview of this method.

[40] The only exception is state personal income, which is measured in 1929 for the period 1925-30 because it is missing prior to 1929.

[41] The results are nearly identical whether we control for year effects or region-by-year effects. For brevity, we only report results controlling for region-by-year effects here.

in period t depends on where the individual was located in period $t-1$. This implies that moving costs vary across state pairs, which generates permanent, non-random migration flows even in the absence of changes to state characteristics such as amenities or wages (Moretti and Wilson, 2017).

We estimate a model of migration flows, rather than population stocks, for two reasons. First, we want to understand whether interstate migration can help explain the fiscal and population responses to the introduction of the income tax in Section 5 (i.e. given no significant changes in fertility or mortality rates as described above). Second, modeling an immediate and constant effect of taxes on migration flows as in Equation (3) implies a gradual and steady reduction in the population stock in response to the introduction of the income tax. This is exactly what the population results indicate (recall Panel E of Table 4).

Following Moretti and Wilson (2017) and Agrawal and Foremny (2019), we calculate standard errors that are robust to heteroskedasticity and three-way clustering by origin-destination pair, origin × year, and destination × year. Clustering by origin-destination pair accounts for serial correlation within the same pair of states. Clustering by origin × year and destination × year accounts for correlation between observations of state pairs sharing an origin state or destination state in the same year.

In Section 5, we showed evidence which suggests that a state that introduces the income tax is in turn able to provide greater public goods per capita (i.e. we find a significant increase in expenditure per capita in the near, medium, and long terms for late adopters after World War II). Migration responses should thus reflect the combined effect of the new income tax plus the additional public goods that it funds. If the benefit of the additional public goods exactly offset the increase in tax liability due to the new income tax, then households would not have the incentive to migrate to another state in response. However, the benefit and cost of the new income tax were not likely to be equal for every (or even most) households. First, state income taxes are typically progressive, meaning that middle- and high-income households bear a greater share of the new tax burden than low-income ones.[42] In addition, middle- and high-income households may value new public goods less than low-income ones. For example, high earners are more likely to send their children to private school.

The key identifying assumption is that migration flows between state pairs for which income tax differentials changed and flows between state pairs for which they did not change would have

[42] However, tax progressivity has fallen over time, due to rising nominal incomes and relatively static nominal tax brackets and exemptions.

followed parallel trends in the absence of the introduction of the income tax. While we cannot test this assumption, it would be more plausible if the two groups of state pairs were on parallel trends prior to the change in the income tax differential. We thus evaluate the timing of migration responses around the change in the income tax differential using the regression

$$\log(P_{odt+h}/P_{oot+h}) - \log(P_{odt}/P_{oot}) = \beta^h E_{odt} + \phi^h_{r(o,d),t} + \nu^h_{odt} \tag{4}$$

for different values of h. The outcome is the change in the outmigration log odds ratio between periods $t+h$ and t, and the tax event E_{odt} is the change in the income tax presence differential between periods t and $t+1$.[43]

Each β^h can be interpreted as a difference-in-differences parameter over a different time horizon. Plotting the estimates of β^h allows us to observe how migration evolves over time in state pairs that experienced a change in their income tax differential relative to state pairs that did not. We estimate the outmigration response using a balanced panel from two decades before the change in the income tax differential to three decades after. The sample includes "treated" state pairs that experienced a change in their income tax differential and "clean control" pairs that did not experience a change during the analysis window.

6.3 Main Results: Migration

Table 5 reports the estimates based on Equation (3). Column 1 shows the results for all migration flows, while columns 2-6 show them for households divided into five occupational earnings groups: below the 25th percentile, 25-50th percentile, 50-75th percentile, 75-90th percentile, and above the 90th percentile.[44]

Panel A reports the results for the child-based measure of migration from 1900 to 2010. We find that the introduction of the income tax increased overall outmigration by 11.3 percent (S.E. = 3.0) (column 1). For low-earning households in the bottom 25 percent of the occupational earnings distribution, however, this relationship was negative (column 2). The sign of this estimate suggests that these households may have been attracted to higher-tax states where they may have received net fiscal benefits. We will return to this issue ahead. For households in the top 75 percent, by con-

[43]That is, $E_{odt} = (D_{ot+1} - D_{dt+1}) - (D_{ot} - D_{dt})$, so E_{odt} equals 1 if the differential increases, -1 if the differential decreases, and 0 if the differential does not change.

[44]The estimates in columns 2-6 are based on a common sample of state-pair-years with at least one migrating household in each of the five earnings groups.

trast, the estimates are always positive and significant (columns 3-6). The largest response, at 14.1 percent (S.E. = 4.4), was for middle-earning households between the 50th and 75th percentiles. For high-earning households in the top 10 percent of occupational earnings, the introduction of the income tax increased outmigration by 13.6 percent (S.E. = 4.0).

Panel B repeats this analysis for the child-based migration measure for the sub-period 1940 to 2010. This is the sample period which overlaps with the available data for the census measure of migration (which we will discuss in the next paragraph). The child-based migration estimates in Panel B display a similar pattern to those in Panel A. However, the magnitudes are systematically larger. This is consistent with the sizable population declines experienced by late adopters of the income tax (Panel E of Table 4). Our estimate indicates that the introduction of the income tax increased outmigration by 16.6 percent (S.E. = 3.2) overall during this sub-period. As in Panel A, low-earning households were attracted to income-tax states where they might benefit from additional public goods. For the 1940-2010 sub-period, our estimate indicates that the introduction of the income tax significantly reduced outmigration by low-earning households by 13.1 percent (S.E. = 4.9).

Panel C reports the results for the census measure of migration, which is only available from 1940 onward. The basic patterns remain similar. Our estimates indicate that the introduction of the income tax continued to induce significant outmigration for middle- and high-earning households. The largest response occurred among high earners, who increased outmigration by 27.6 percent (S.E. = 3.7) in response to the introduction of the income tax. In contrast to the results for the child-based measure, low-earning households did not significantly alter their location choices in response to the income tax according to the census migration measure. This difference could be because low-income households with small children (as captured by the child-based measure) received larger net benefits from income-tax states (e.g. in terms of access to better public schools) than other types of low-income households (as captured by the census measure).

Finally, there is no evidence of differential pre-trends for outmigration (Appendix Figure B.9). The introduction of the income tax has a relatively constant impact on outmigration over time in the post-World War II era.

The migration responses reported in Table 5 appear large in magnitude because they are calculated relative to the small base of migrating households. The percentage change in the population *stock* is smaller. (Note, however, that a constant effect on the migration flow implies a growing effect on the population stock over time.) The census-based estimate in column 1 of Panel C indicates that the introduction of the income tax induced a decline in the population stock by 3.5 percent (S.E. = 0.9) over each ensuing 5-year period until the next change in income tax differentials (if any).[45] This translates into a population decline of 16.4 percent (S.E. = 3.9) after 25 years, which is very similar in magnitude to our long-run population estimate from Section 5.3 (see column 5 of Panel E of Table 4).[46]

To compare our results with recent research on high earners, we compute stock elasticities for households in top 10 percent earning occupations. First, note that our estimates imply a population decline of 24.6 percent (S.E. = 3.0) 25 years after the introduction of the income tax in the post-World War II era for top 10 percent earning households.[47] Second, note that the net-of-average-tax rate at 90th percentile income fell by 5 percent following post-World War II introductions of the income tax (Panel (b) of Figure 8). In addition, the net-of-corporate-tax rate fell by more than 4 percent (Panel (c) of Figure 8). Previous research indicates that the corporate income tax has a similar or larger impact on inventor location decisions compared to the individual income tax (Moretti and Wilson, 2017, Akcigit et al., 2022). If we assume that the individual income tax has the same impact as the corporate income tax, then we obtain a 25-year stock elasticity of 2.7 (i.e. $24.6/(5+4)$). Assuming that the corporate income tax has a larger impact on migration would yield a smaller stock elasticity for the individual income tax.

To our knowledge, there are two works that employ long panels to study individual migration responses to tax regimes across U.S. states: Moretti and Wilson (2017) and Akcigit et al. (2022). Moretti and Wilson (2017) find that a permanent 1-percent decrease in the net-of-average-tax rate reduces the stock of star scientists in a state by 0.4 percent per year, with a slightly larger effect for the corporate income tax. This implies a 25-year stock elasticity of 9.5. In the analysis by Akcigit et al. (2022) on the migration decisions of inventors, the authors report a 20-year elasticity of 2 with respect to the personal marginal tax rate and a 20-year elasticity of 7 with respect to the corporate tax rate.[48]

[45] The percentage change in the stock of households is $(\Delta M/M)\cdot M/P - (\Delta L/L)\cdot L/P$, where M is the number of households moving into the state, L is the number of households leaving the state, and P is the initial stock of households. In the sample using the census measure, the migration-weighted averages of M/P and L/P are 0.107 and 0.092, respectively. Our model implies that $\Delta M/M \approx \exp(-\theta)-1$ and $\Delta L/L \approx \exp(\theta)-1$. For more details, see Appendix C.

[46] The 25-year response is $\left(1+\hat{\delta}\right)^5 - 1$, where $\hat{\delta}$ is the estimated 5-year response (here, -0.035).

[47] We base this on the estimate in Table 5 (Panel C, column 6) and the formulas discussed in the previous two footnotes.

[48] See their Figure 4. A key issue in comparing estimates is that a constant flow elasticity implies a growing stock elasticity over time in response to any permanent tax change. Our paper along with Moretti and Wilson (2017) and Akcigit et al. (2022) show growing stock responses over time. We thus believe that the assumption of a constant flow

With respect to works that study short-run effects, Agrawal and Foremny (2019) estimate a stock elasticity of 0.88 for top earners in Spain using four years of post-tax reform data, while Young et al. (2016) estimate a 1-year stock elasticity of 0.1 for millionaires in the United States using a 13-year panel. This elasticity is deduced from a migration flow elasticity, so it implies a 4-year stock elasticity of 0.4. Our estimate for top 10 percent earning households implies a 4-year population decline of 4.4 percent, yielding a 4-year elasticity of 0.5, assuming the individual and corporate income taxes have the same impact.[49]

Thus, overall, the magnitudes of our estimates of the migration responses for top 10 percent earning households are generally in the same ballpark as those in the current literature. Nevertheless, our magnitudes still appear large in absolute terms. This is striking given that we analyze a relatively “lower income" population (i.e. the top 10 percent versus e.g. the top 1 percent), and we would expect migration elasticities to rise with income.[50] There are several potential explanations for this. First, mobility in the United States has changed over time. While regional mobility increased from 1940 to 1980 (Rosenbloom and Sundstrom, 2004, Molloy, Smith and Wozniak, 2011), it has fallen from 1980 onward (Frey, 2009, Batini, Oya, Dowling, Estevão, Keim, Sommer and Tsounta, 2010, Kaplan and Schulhofer-Wohl, 2017). Our (post-World War II) estimates center primarily on the prior period of high and growing mobility between 1940 and 1980. Beyond changes in mobility, there are likely to be intrinsic limitations in dealing with historical data. Related to this, there can be inherent uncertainty in estimating long-run policy impacts. Both factors could inadvertently inflate the size of our migration estimates. For these reasons, we encourage caution in the interpretation of these magnitudes.

6.4 Robustness: Migration

Controlling for state differentials in the presence of the sales tax or the unemployment rate has virtually no impact on our results for either the child-based or census migration measures (Appendix Figure B.10). When we control for state differentials in log personal income per capita or industry shift-share employment shocks, the estimate falls in magnitude but remains large. Across all the

elasticity is reasonable.

[49] Here we again assume that the individual income tax and the corporate income tax have the same impact. This is for a 5 percent fall in the net-of-tax rate for the individual income tax and a 4 percent fall in the net-of-tax rate for the corporate income tax. Figure 8 shows that in the first four years after the introduction of the income tax, the net-of-average-tax rate and the net-of-corporate-tax rate each fell by about 3 percent (Panels (b) and (c)). Scaling by six instead of nine yields a 4-year elasticity of 0.7.

[50] We obtain similar results when we limit the sample to top 1 percent earning households, but at the expense of inducing greater sample selection for state pairs.

controls, the smallest outmigration response that we estimate for late adopters (i.e. post-World War II) of the income tax is approximately 10 percent, which is both economically meaningful and statistically significant.

While the baseline migration analysis uses data on all continental 48 states plus Washington, D.C., the fiscal analysis in Section 5 omits some states due to a lack of fiscal data around the time of the introduction of the income tax. We thus repeat the analysis using only state pairs for which both states were included in the fiscal analysis. The results remain very similar to the baseline estimates (Appendix Table B.8).

We next rerun the analysis based on income percentiles rather than occupational earnings scores as in the baseline. The results are similar to the baseline estimates (Appendix Table B.9).

Finally, we show that the migration responses to the introduction of the income tax were not concentrated among neighboring states (Appendix Table B.10). This suggests that the fixed costs of moving were more important than the marginal costs of distance.

7 Conclusion

In this paper, we have analyzed the implications of a major investment in modern state capacity – the introduction of the income tax – using a new panel database that covers the entire twentieth century and the start of the twenty-first century across U.S. states. Our empirical strategy exploits the staggered adoption of the income tax over a 65-year period, and accounts for selective timing of adoption based on recent demographic and fiscal trends.

We find that the introduction of the income tax increased total revenue per capita in the near run, medium run, and the long run for both early (i.e. pre-World War II) and late adopters (i.e. post-World War II), while total revenue in absolute levels increased in the medium or long runs for early adopters, but not for late adopters. We explain the fiscal results by showing that the introduction of the income tax by late adopters reduced state populations in the long run, as residents relocated to states that did not have the income tax. We find that both middle- and high-earning households exhibited strong migration responses. Overall, our results indicate that introducing the income tax allowed U.S. states to significantly increase their fiscal capacity on a per capita basis, even if population mobility provided a partial check on the this capacity in absolute terms. The return on fiscal capacity investments thus appears to be contingent on the elasticity of the tax base.

One direction for future research would be to study whether our results for internal migration generalize to other contexts. While the United States has a geographically mobile population, Denmark, Finland, and Great Britain have similar rates of mobility (Molloy et al., 2011). Furthermore, local governments in Denmark and Finland rely heavily on the income tax, making these two countries leading candidates for future research (OECD, 2002). Another research direction would be to examine the consequences of broadening the tax base along other dimensions, such as by introducing a sales tax. Finally, future research should continue to investigate the effects of fiscal capacity investments in low-income countries, where local taxation tends to be low but the returns on public investments are potentially high (Besley and Persson, 2013, Gadenne and Singhal, 2014).

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8 Tables

Table 1: Summary Statistics for State-Level Outcomes

	Mean	Std Dev	Min	Max	Obs.
General					
Bottom Marginal Income Tax Rate (%)	1.31	1.39	0.00	6.35	4,655
Top Marginal Income Tax Rate (%)	4.19	3.95	0.00	19.80	4,655
Personal Exemption, Single Filer	7.17	6.85	0.00	62.51	2,752
Sales Tax Rate (%)	2.45	2.23	0.00	8.25	4,655
Top Total Marginal Tax Rate (%)	63.61	21.71	15.00	95.00	4,655
Outcomes (Absolute)					
Population (millions)	3.88	4.45	0.08	37.32	4,655
Total Revenue	13.36	24.44	0.04	320.56	3,942
Total Expenditure	12.64	23.06	0.04	260.61	3,942
Total Tax Revenue	7.06	11.29	0.02	122.83	3,470
Property Tax Revenue	0.17	0.42	0.00	5.19	3,890
Sales Tax Revenue	3.84	5.35	0.02	43.65	3,180
Outcomes (Per Capita)					
Total Revenue p.c.	2,816.60	2,346.85	54.27	20,856.64	3,942
Total Expenditure p.c.	2,664.26	2,223.69	50.43	22,924.83	3,942
Total Tax Revenue p.c.	1,460.68	975.73	40.23	9,676.56	3,470
Property Tax Revenue p.c.	66.12	208.34	0.00	3,101.81	3,890
Sales Tax Revenue p.c.	785.65	410.18	95.16	2,478.67	3,180

Notes: The personal exemption is measured in 2010 USD thousands, absolute fiscal outcomes are measured in 2010 USD billions, and per capita fiscal outcomes are measured in 2010 USD per capita.

Table 2: Hazard Model of Probability of Introducing the Income Tax

	(1)	(2)	(3)	(4)
Δ Log Population$_{(t-1,t-5)} \times (t \leq 1945)$	−1.37	−1.14	−1.08	−1.16
	(1.56)	(1.46)	(1.36)	(1.38)
Δ Log Population$_{(t-5,t-9)} \times (t \leq 1945)$	−0.30	−0.28	−0.48	−0.66
	(0.85)	(0.76)	(0.73)	(0.74)
Δ Log Population$_{(t-9,t-15)} \times (t \leq 1945)$	0.78*	0.89*	0.75	0.71
	(0.44)	(0.47)	(0.48)	(0.50)
Δ Log Population$_{(t-1,t-5)} \times (t > 1945)$	−0.78	−0.68	−0.96	−0.70
	(1.03)	(0.86)	(0.95)	(0.85)
Δ Log Population$_{(t-5,t-9)} \times (t > 1945)$	−1.32	−1.50	−1.56	−1.53
	(1.07)	(1.18)	(1.11)	(1.10)
Δ Log Population$_{(t-9,t-15)} \times (t > 1945)$	−0.23	−0.17	−0.34	−0.30
	(0.54)	(0.53)	(0.50)	(0.58)
Δ Log Revenue$_{(t-1,t-3)} \times (t \leq 1945)$		−0.04		−0.36
		(0.27)		(0.38)
Δ Log Revenue$_{(t-3,t-5)} \times (t \leq 1945)$		0.37**		−0.42
		(0.16)		(0.33)
Δ Log Revenue$_{(t-1,t-3)} \times (t > 1945)$		−0.58		−0.91
		(0.52)		(0.70)
Δ Log Revenue$_{(t-3,t-5)} \times (t > 1945)$		0.87*		0.36
		(0.50)		(1.01)
Δ Log Expenditure$_{(t-1,t-3)} \times (t \leq 1945)$			0.08	0.22
			(0.19)	(0.26)
Δ Log Expenditure$_{(t-3,t-5)} \times (t \leq 1945)$			0.41***	0.76***
			(0.14)	(0.28)
Δ Log Expenditure$_{(t-1,t-3)} \times (t > 1945)$			0.36	0.51
			(0.51)	(0.79)
Δ Log Expenditure$_{(t-3,t-5)} \times (t > 1945)$			0.32	0.23
			(0.29)	(0.54)
p-value: Population coeffs = 0	0.083	0.077	0.094	0.128
p-value: Revenue coeffs = 0		0.039		0.139
p-value: Expenditure coeffs = 0			0.020	0.077
p-value: Rev. & Expend. coeffs = 0				0.001
Observations	225	225	225	225
States	36	36	36	36

Notes: This table reports average marginal effects from a probit model of the probability of introducing the individual income tax conditional on not having an income tax in the previous year. Years following the year of introduction are dropped. All models include year effects. Standard errors clustered by state are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Average Effects of Introduction of Income Tax

	Average Effect of Income Tax Introduction over Time				
Years Since Introduction:	0 to 1 (1)	2 to 3 (2)	4 to 9 (3)	10 to 19 (4)	20 to 30 (5)
	Panel A: Log Total Revenue				
All Introductions	4.1^{*}	10.7^{***}	5.4	13.0^{**}	15.6^{**}
	(2.2)	(3.1)	(4.6)	(6.2)	(6.9)
	[−0.5, 9.2]	[4.1, 17.5]	[−4.2, 15.1]	[0.0, 25.8]	[0.9, 30.3]
	Panel B: Log Total Expenditure				
All Introductions	4.1	10.5^{**}	5.7	8.2	12.4^{*}
	(2.5)	(3.9)	(4.6)	(6.7)	(7.1)
	[−1.5, 10.0]	[2.0, 19.2]	[−3.9, 15.4]	[−5.9, 22.3]	[−2.9, 27.9]
	Panel C: Log Total Revenue per Capita				
All Introductions	4.6^{**}	11.5^{***}	5.9	14.6^{***}	16.7^{***}
	(2.2)	(3.0)	(4.4)	(5.0)	(5.5)
	[−0.0, 9.6]	[4.9, 18.2]	[−3.4, 15.2]	[4.4, 24.8]	[5.2, 28.0]
	Panel D: Log Total Expenditure per Capita				
All Introductions	4.6^{*}	11.3^{***}	6.2	9.8^{*}	13.5^{**}
	(2.4)	(4.0)	(4.6)	(5.7)	(5.9)
	[−1.0, 10.4]	[2.6, 20.1]	[−3.5, 15.8]	[−2.2, 21.5]	[0.9, 26.2]
	Panel E: Log Population				
All Introductions	−0.1	−0.1	−0.5	−1.5	−1.1
	(0.4)	(0.8)	(1.7)	(3.6)	(5.1)
	[−1.0, 0.9]	[−1.7, 1.6]	[−4.2, 3.2]	[−9.2, 6.2]	[−11.6, 9.4]
Observations	9,122	9,118	9,165	9,118	8,854
States	36	36	36	36	36

Notes: This table reports estimates of $\omega_e \beta^{h,e} + \omega_\ell \beta^{h,\ell}$ ("All Introductions") from Equation (1), where ω_e is the share of early adopters ("Pre-World War II") and ω_ℓ is the share of late adopters ("Post-World War II"). Estimates are averaged over the specified time horizons and multiplied by 100. Standard errors clustered by state are in parentheses. 95-percent confidence intervals based on the restricted wild cluster bootstrap are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Heterogeneous Effects of Introduction of Income Tax by Time Period

	Average Effect of Income Tax Introduction over Time				
Years Since Introduction:	0 to 1	2 to 3	4 to 9	10 to 19	20 to 30
	(1)	(2)	(3)	(4)	(5)
	Panel A: Log Total Revenue				
Pre-World War II	2.3	10.9	1.9	20.3**	28.5***
Introductions	(5.5)	(6.5)	(7.6)	(10.0)	(10.2)
	[−10.8, 16.7]	[−4.8, 25.3]	[−13.8, 17.2]	[−0.1, 40.4]	[7.7, 49.2]
Post-World War II	5.1***	10.5***	10.0***	3.6	−0.9
Introductions	(1.7)	(3.0)	(3.2)	(6.8)	(6.9)
	[1.8, 9.0]	[4.4, 17.6]	[3.3, 16.8]	[−12.3, 18.4]	[−16.0, 14.7]
	Panel B: Log Total Expenditure				
Pre-World War II	0.6	10.0	1.0	11.2	21.3**
Introductions	(6.3)	(8.6)	(7.6)	(10.3)	(9.6)
	[−16.2, 16.5]	[−11.2, 28.6]	[−15.2, 16.4]	[−10.2, 31.9]	[1.7, 40.8]
Post-World War II	6.0**	10.8***	11.6***	4.4	1.2
Introductions	(2.3)	(3.7)	(3.5)	(6.2)	(7.4)
	[1.3, 11.7]	[3.0, 19.9]	[4.9, 19.2]	[−9.3, 17.7]	[−15.5, 18.0]
	Panel C: Log Total Revenue per Capita				
Pre-World War II	1.4	9.4	−1.5	14.1	17.4**
Introductions	(5.5)	(6.6)	(7.5)	(9.0)	(8.3)
	[−12.0, 16.2]	[−6.6, 23.8]	[−17.1, 13.7]	[−4.2, 32.1]	[0.5, 33.8]
Post-World War II	6.4***	13.0***	15.3***	15.2***	15.7***
Introductions	(1.5)	(2.6)	(2.3)	(3.6)	(5.6)
	[3.2, 9.8]	[7.6, 19.0]	[10.6, 20.2]	[6.4, 23.6]	[2.3, 27.8]
	Panel D: Log Total Expenditure per Capita				
Pre-World War II	−0.4	8.6	−2.3	5.0	10.1
Introductions	(6.4)	(8.9)	(7.7)	(9.6)	(8.3)
	[−17.3, 16.1]	[−13.3, 28.1]	[−18.9, 13.2]	[−15.0, 24.2]	[−7.2, 26.8]
Post-World War II	7.3***	13.3***	17.0***	15.9***	17.8**
Introductions	(2.0)	(3.6)	(3.3)	(3.4)	(6.6)
	[3.0, 12.0]	[5.7, 21.9]	[10.2, 24.5]	[8.2, 23.4]	[1.6, 32.9]
	Panel E: Log Population				
Pre-World War II	0.9*	1.9*	3.4	6.4	11.1
Introductions	(0.5)	(1.0)	(2.1)	(4.5)	(7.6)
	[−0.0, 1.8]	[−0.2, 3.9]	[−1.0, 7.8]	[−3.1, 15.7]	[−4.8, 26.7]
Post-World War II	−1.3*	−2.5**	−5.4*	−11.6*	−16.6**
Introductions	(0.7)	(1.2)	(3.1)	(6.2)	(8.1)
	[−2.6, 0.2]	[−5.0, 0.3]	[−12.1, 1.4]	[−25.2, 2.6]	[−33.6, 2.0]
Observations	9,122	9,118	9,165	9,118	8,854
States	36	36	36	36	36

Notes: This table reports estimates of $\beta^{h,e}$ for early adopters ("Pre-World War II") and $\beta^{h,\ell}$ for late adopters ("Post-World War II") from Equation (1). Estimates are averaged over the specified time horizons and multiplied by 100. Standard errors clustered by state are in parentheses. 95-percent confidence intervals based on the restricted wild cluster bootstrap are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

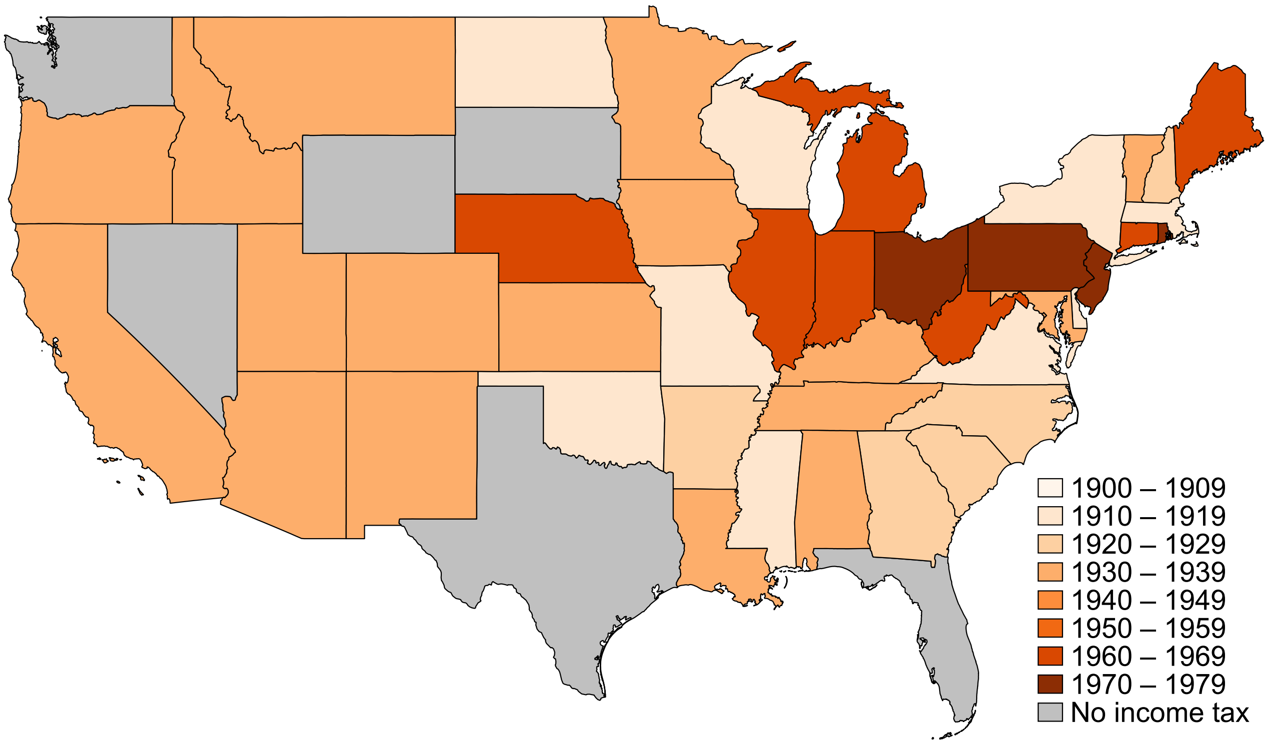
Table 5: Effects of Introduction of Income Tax on Outmigration by Occupational Earnings

	All Flows	Outmigration Flows by Occupational Earnings Percentile				
		< 25	$[25, 50)$	$[50, 75)$	$[75, 90)$	≥ 90
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Child-Based 5-Year Migration Rate, 1900-2010					
Post Income Tax	0.113^{***}	-0.058	0.132^{***}	0.141^{***}	0.109^{**}	0.136^{***}
(Origin − Destination)	(0.030)	(0.044)	(0.039)	(0.044)	(0.042)	(0.040)
Observations	17,780	3,906	3,906	3,906	3,906	3,906
	Panel B: Child-Based 5-Year Migration Rate, 1940-2010					
Post Income Tax	0.166^{***}	-0.131^{***}	0.179^{***}	0.169^{***}	0.156^{***}	0.167^{***}
(Origin − Destination)	(0.032)	(0.049)	(0.045)	(0.056)	(0.050)	(0.046)
Observations	13,619	3,268	3,268	3,268	3,268	3,268
	Panel C: Census 5-Year Migration Rate, 1940-2000					
Post Income Tax	0.177^{***}	0.020	0.206^{***}	0.254^{***}	0.233^{***}	0.276^{***}
(Origin − Destination)	(0.045)	(0.040)	(0.040)	(0.037)	(0.036)	(0.037)
Observations	13,275	8,037	8,037	8,037	8,037	8,037

Notes: This table reports estimates of θ in Equation (3), omitting tax-rate differentials from the regression. The results in Panels A and B are for the child-based 5-year migration rate, and the results in Panel C are for the census 5-year migration rate. Column 1 reports the results for the full sample of interstate migration flows. Columns 2-6 report separate results for different occupational earnings groups, and the sample is restricted to state-pair-years with at least one moving household in each group. The outcome variable is the log odds ratio of the population share that moved from the origin state to the destination state relative to the population share that remained in the origin state. *Post Income Tax (Origin − Destination)* is the difference between the origin's and destination's indicator variable *Post Income Tax*, which equals 1 after the state introduced the individual income tax. All regressions include origin-destination fixed effects and region-pair × year effects. Standard errors, in parentheses, are robust to heteroskedasticity and three-way clustering at the origin-destination pair, origin × year, and destination × year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

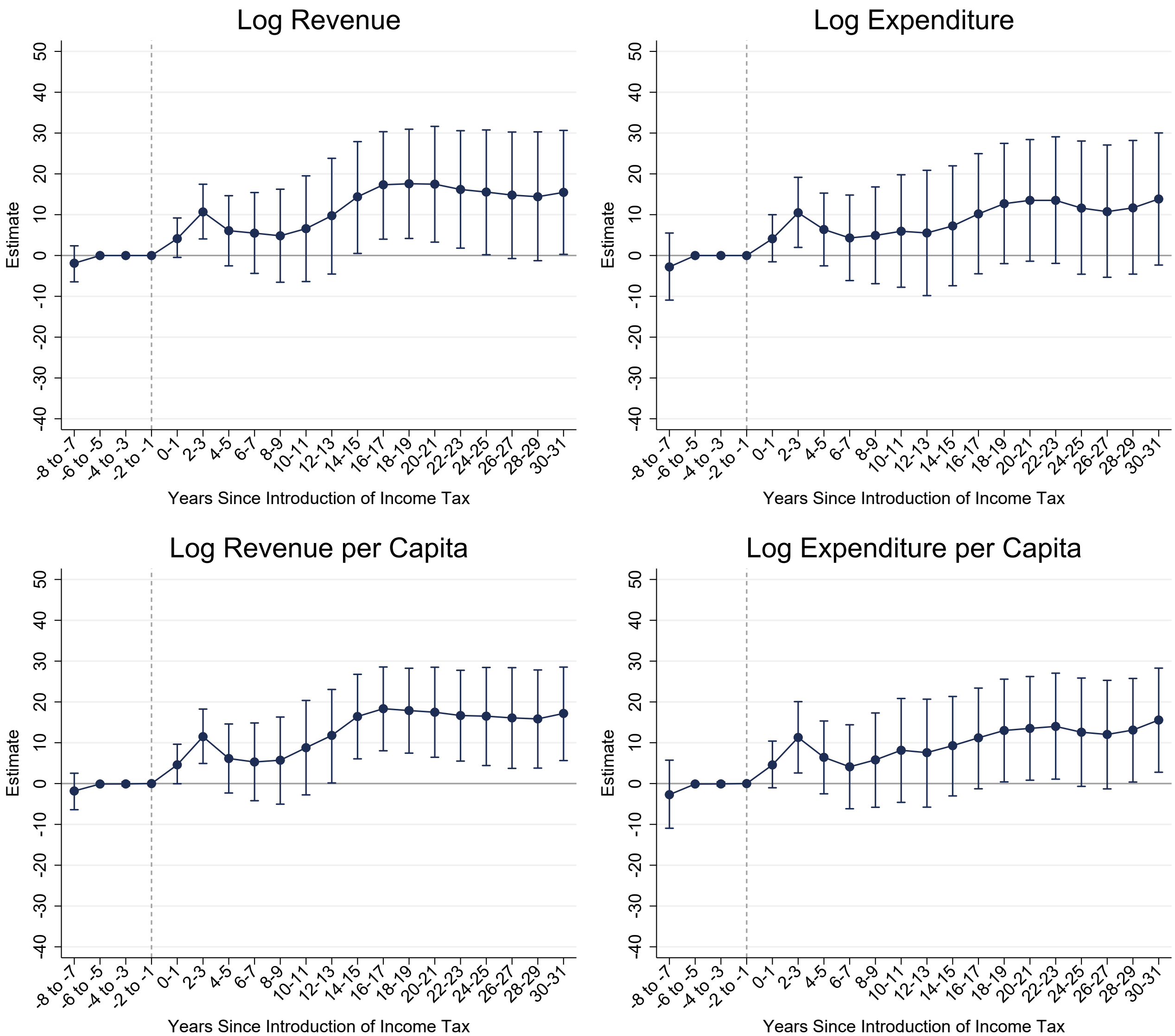
9 Figures

Figure 1: Decades of Introduction of State-Level Individual Income Tax



Notes: This figure displays U.S. states shaded according to the decade in which the state introduced the individual income tax. Darker shades indicate later decades. States in gray never introduced an individual income tax. The source for 1900-1980 is Penniman (1980). We use the University of Michigan's World Tax Database to extend this source for 1980-2010. For further details, see Section 2.

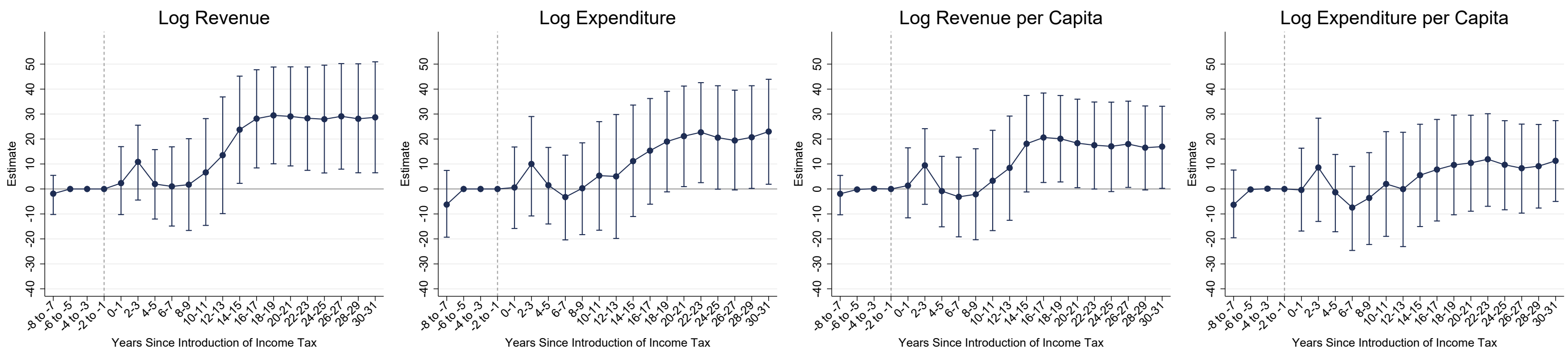
Figure 2: Dynamic Effects of Introduction of Income Tax on Revenue and Expenditure



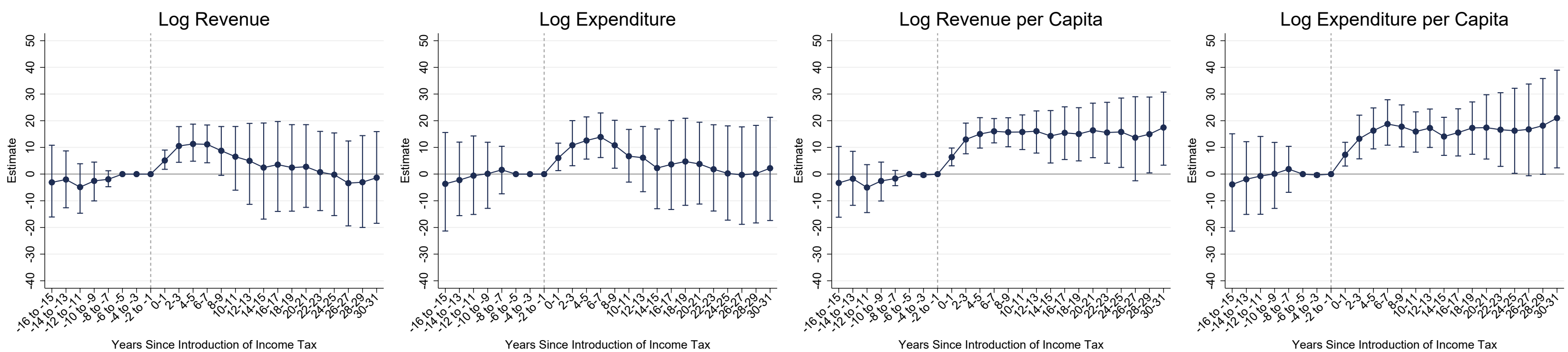
Notes: This figure plots point estimates and 95-percent confidence intervals based on the wild cluster bootstrap for $\omega_e \beta^{h,e} + \omega_\ell \beta^{h,\ell}$ from Equation (1), where ω_e is the share of early adopters ("Pre-World War II") and ω_ℓ is the share of late adopters ("Post-World War II"). All estimates are multiplied by 100.

Figure 3: Heterogeneous Effects of Introduction of Income Tax by Time Period

(a) Early Adopters (Pre-World War II)

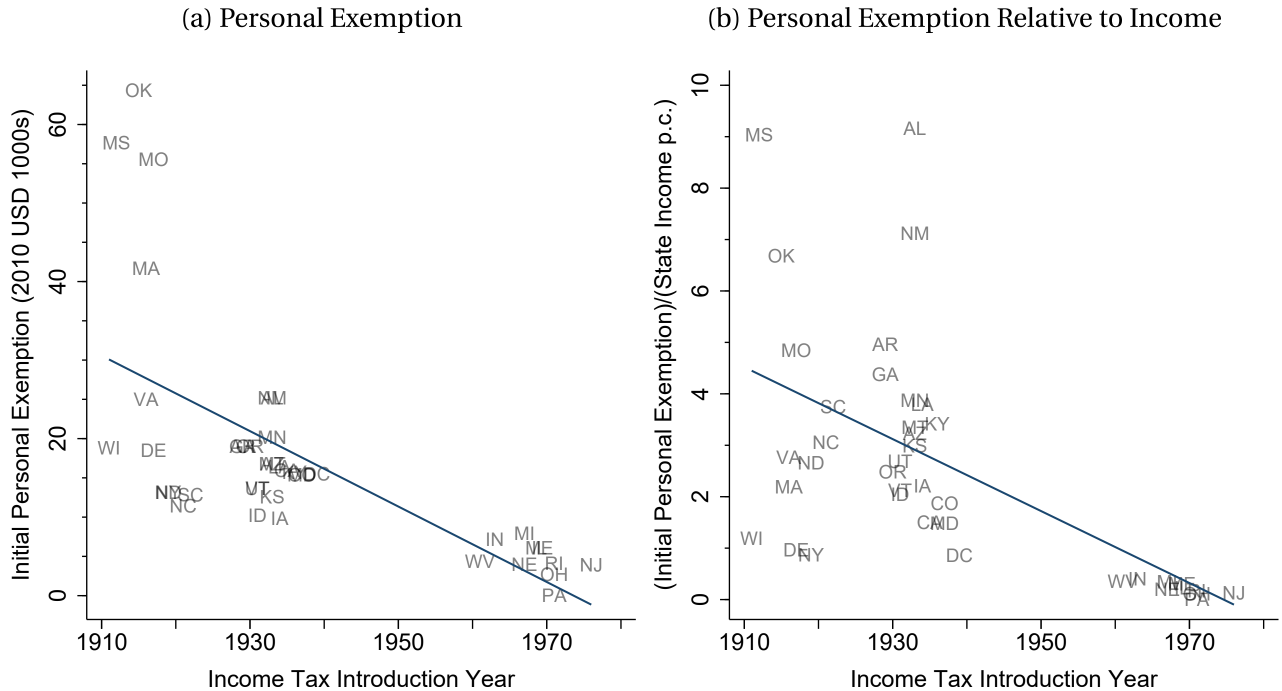


(b) Late Adopters (Post-World War II)



Notes: Panel (a) plots estimates of $\beta^{h,e}$ for early adopters ("Pre-World War II"), and Panel (b) plots estimates of $\beta^{h,\ell}$ for late adopters ("Post-World War II"), both from Equation (1). 95-percent confidence intervals based on the wild cluster bootstrap are reported. All estimates are multiplied by 100.

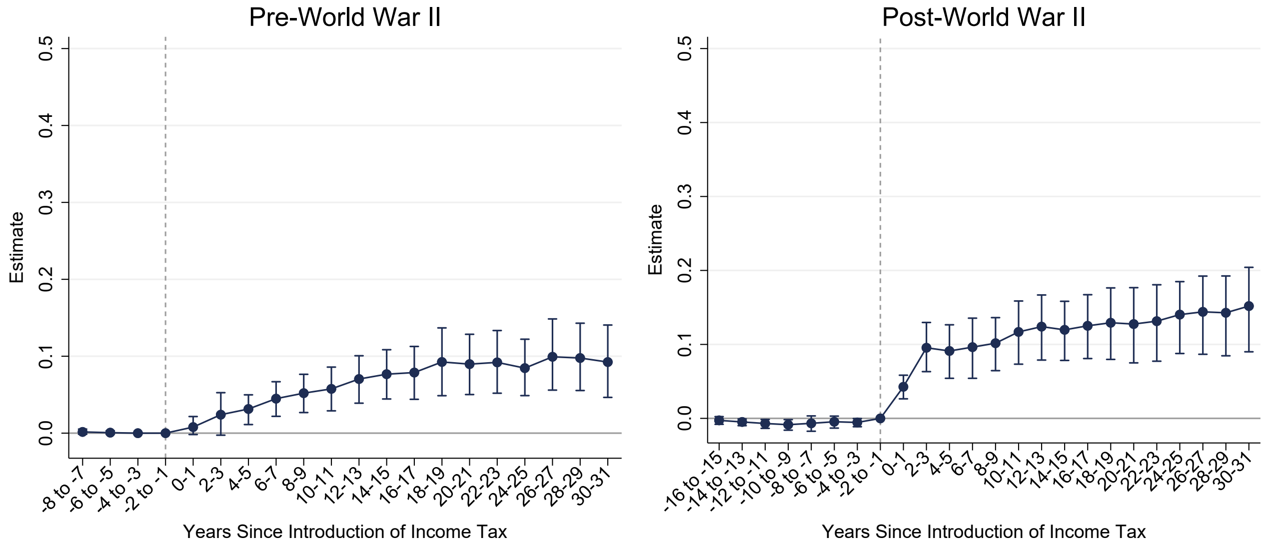
Figure 4: Initial Breadth of Tax Base by Year of Introduction of Income Tax



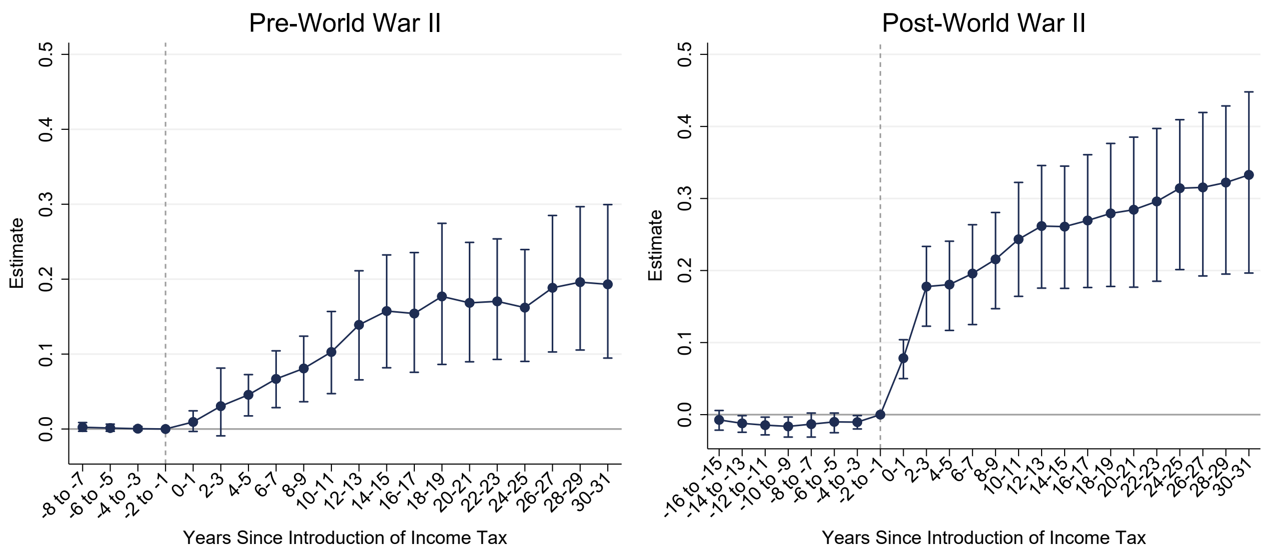
Notes: This figure plots the initial personal exemption (Panel a) and the personal exemption divided by state personal income per capita (Panel b) against the year of introduction of the income tax. The personal exemption applies to single filers. The solid line plots the line of best fit from a univariate regression.

Figure 5: Dynamic Effects of Introduction of Income Tax on Composition of Revenue

(a) Income Taxes as a Share of Total Revenue

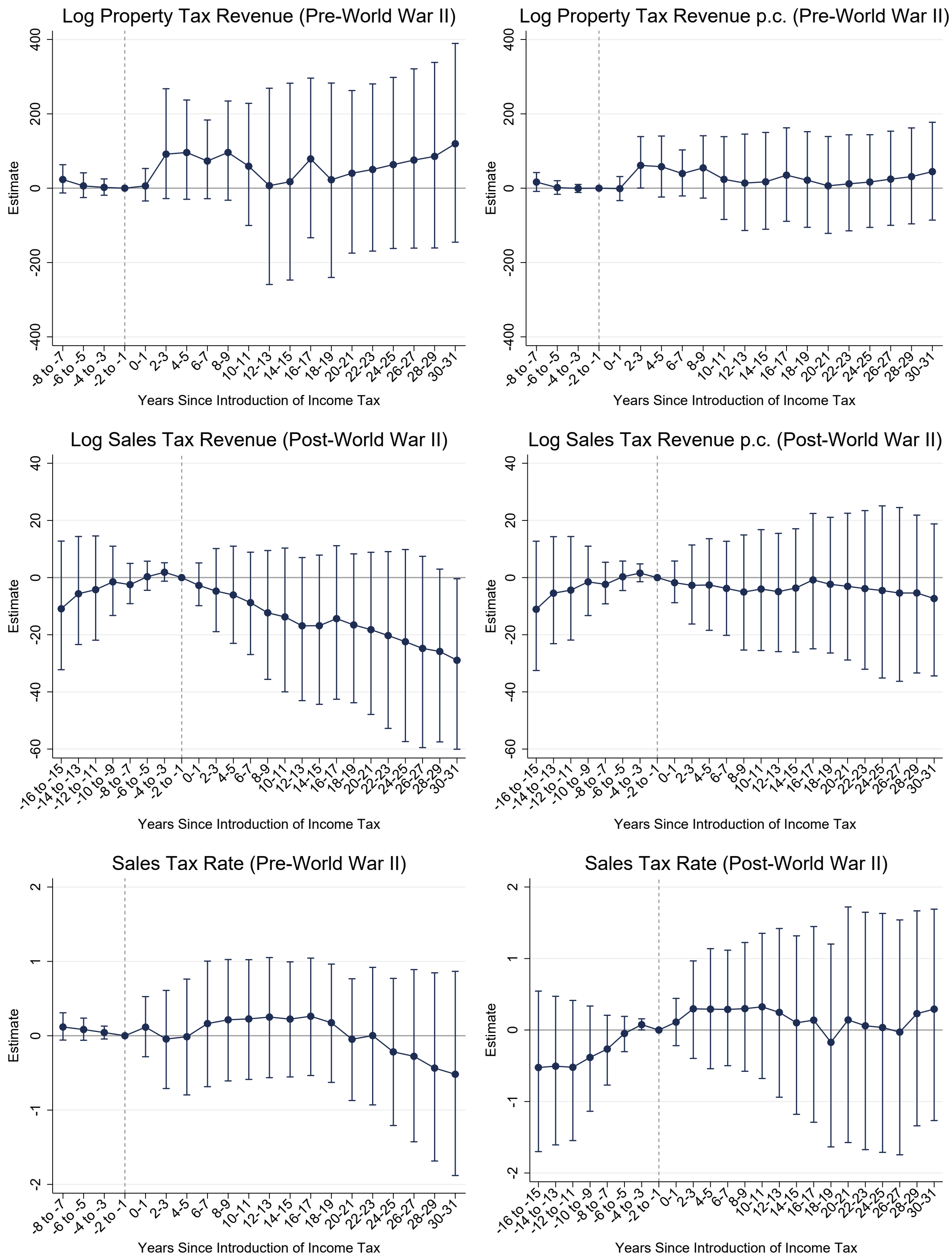


(b) Income Taxes as a Share of Total Taxes



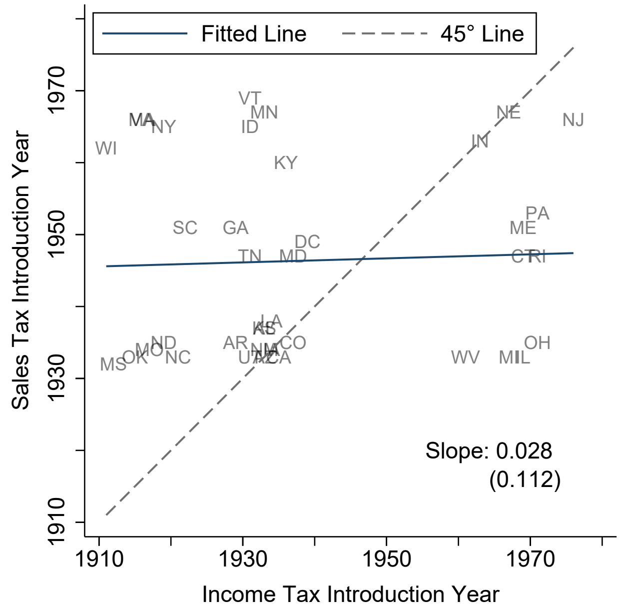
Notes: This figure plots estimates of $\beta^{h,e}$ for early adopters ("Pre-World War II") and $\beta^{h,\ell}$ for late adopters ("Post-World War II"), both from Equation (1). 95-percent confidence intervals based on the wild cluster bootstrap are reported.

Figure 6: Testing for Crowd Out Effects of Introduction of Income Tax



Notes: This figure plots estimates of $\beta^{h,e}$ for early adopters ("Pre-World War II") and $\beta^{h,\ell}$ for late adopters ("Post-World War II"), both from Equation (1). 95-percent confidence intervals based on the wild cluster bootstrap are reported. The estimates in the top two panels are multiplied by 100.

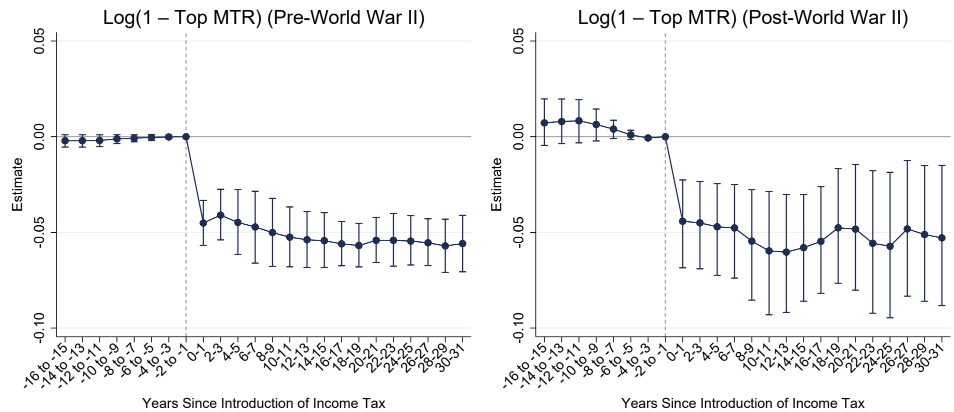
Figure 7: Timing of Introduction of Income Tax versus Sales Tax



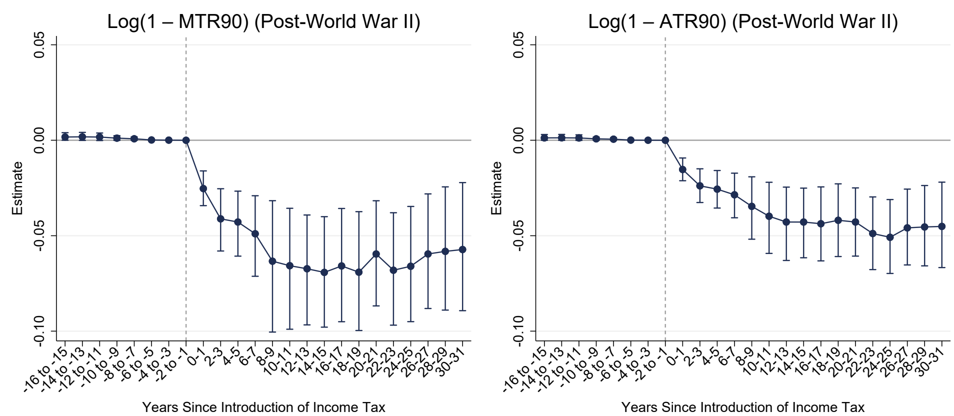
Notes: This figure plots the year of introduction of the income tax against the year of introduction of the sales tax. The solid line plots the fitted values from a univariate regression, and the dashed 45° line plots points corresponding to the simultaneous introduction of the two taxes.

Figure 8: Dynamic Effects of Introduction of Income Tax on Net-of-Tax Rate

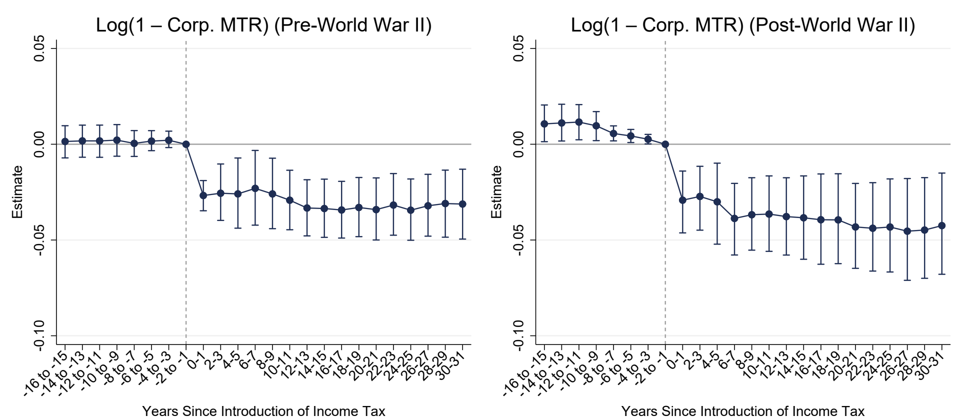
(a) (Net of) Top Total Marginal Tax Rate



(b) (Net of) Marginal Tax Rate and Average Tax Rate at 90th Percentile Income



(c) (Net of) Top Corporate Marginal Tax Rate



Notes: This figure plots estimates of $\beta^{h,e}$ for early adopters ("Pre-World War II") and $\beta^{h,\ell}$ for late adopters ("Post-World War II"), both from Equation (1). The outcome is always $\log(1-\tau)$ for some tax rate τ. In Panel (a) τ is the approximate total marginal tax rate faced by top earners, which accounts for state and federal taxes and the deductibility of state income taxes from the federal tax liability. That is, $1-\tau = \frac{1-\tau^{I,S}-(1-\tau^{I,S})\cdot\tau^{I,F}}{1+\tau^{sales}}$, where $\tau^{I,S}$ and $\tau^{I,F}$ are the top statutory state and federal income tax rates, and τ^{sales} is the state sales tax rate. In Panel (b) τ is the total marginal tax rate at 90th percentile income (left panel) or the total average tax rate at 90th percentile income (right panel). In Panel (c) τ is the top marginal tax rate on corporate income. 95-percent confidence intervals based on the wild cluster bootstrap are reported.

Online Appendices

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Appendix D References **D1**

Appendix A Data

A.1 Years of Introduction of Income Tax

Table A.1: Year of Introduction of State-Level Income Tax

State	Individual	Corporate	State	Individual	Corporate
Wisconsin	1911	1911	Iowa	1934	1934
Mississippi	1912	1921	Louisiana	1934	1934
Oklahoma	1915	1931	California	1935	1929
Massachusetts	1916	1919	Kentucky	1936	1936
Virginia	1916	1915	Colorado	1937	1937
Delaware	1917	1957	Maryland	1937	1937
Missouri	1917	1917	Washington, D.C.	1939	1939
New York	1919	1917	West Virginia	1961	1967
North Dakota	1919	1919	Indiana	1963	1963
North Carolina	1921	1921	Michigan	1967	1967
South Carolina	1922	1922	Nebraska	1967	1967
New Hampshire	1923	1970	Connecticut	1969	1915
Arkansas	1929	1929	Illinois	1969	1969
Georgia	1929	1929	Maine	1969	1969
Oregon	1930	1929	Ohio	1971	1971
Idaho	1931	1931	Pennsylvania	1971	1935
Tennessee	1931	1923	Rhode Island	1971	1947
Utah	1931	1931	New Jersey	1976	1958
Vermont	1931	1931	Florida	None	1971
Alabama	1933	1933	Nevada	None	None
Arizona	1933	1933	South Dakota	None	None
Kansas	1933	1933	Texas	None	None
Minnesota	1933	1933	Washington	None	None
Montana	1933	1917	Wyoming	None	None
New Mexico	1933	1933			

Notes: This table reports the years in which (continental) U.S. states introduced the income tax (individual and/or corporate). The source for 1900-1980 is Penniman (1980, p. 2). We use the University of Michigan's World Tax Database to extend this source for 1980-2010. Note that a typo in Penniman (1980) identifies Virginia's year of adoption of the individual income tax as 1961 instead of 1916. Penniman (1980) defines two types of corporate income tax: the net income tax and the excise or franchise tax. We date the corporate income tax that was introduced first, regardless of type. For further details, see Section 2.

A.2 Data Sources

Table A.2: Sources for Fiscal Data

Data Year	Source	Edition	Tables	Pages
1902	U.S. Department of Commerce (1907)	1907	10	980-95
1903	U.S. Department of Commerce (1915)	1915	7, 9	38-9, 42-3
1913	U.S. Department of Commerce (1915)	1915	6, 8	36-7, 40-1
1915	U.S. Census Statistical Abstract	1929	228	222
1917[a]	U.S. Census Statistical Abstract	1939	224	220
1922	U.S. Census Statistical Abstract	1923	652	741
	U.S. Census Statistical Abstract	1939	224	220
1923	U.S. Census Statistical Abstract	1924	185	199
1924	U.S. Census Statistical Abstract	1925	208	209
1925	U.S. Census Statistical Abstract	1926	214	214
1926	U.S. Census Statistical Abstract	1928	222	216
1927	U.S. Census Statistical Abstract	1930	228	223
	U.S. Census Statistical Abstract	1939	224	220
1928	U.S. Census Statistical Abstract	1931	218	224-5
1929	U.S. Census Statistical Abstract	1931	218	224-5
1930	U.S. Census Statistical Abstract	1932	198	206-7
1931	U.S. Census Statistical Abstract	1933	202	201-2
1932	U.S. Census Statistical Abstract	1934	204	202
	U.S. Census Statistical Abstract	1939	224	220
1937	U.S. Census Statistical Abstract	1939	223	218-9
	U.S. Census Statistical Abstract	1939	224	220
1938	U.S. Census Statistical Abstract	1941	231	240-1
1940	U.S. Census Statistical Abstract	1941	234	243
	U.S. Census Statistical Abstract	1942	234	248-9
1942-2010	Census of Governments	N/A	N/A	N/A

Notes: State-level data from the Census of Governments are available biannually from 1942 to 1948 and annually from 1950 to 2008 at https://www.census.gov/programs-surveys/gov-finances/data/historical-data.html, and from 2009 to 2010 at https://www.census.gov/programs-surveys/gov-finances/data/datasets.html. The Census of Governments also provides nationwide aggregate variables for selected years from 1902 to 1940. The Statistical Abstracts are available at https://www.census.gov/library/publications/time-series/statistical_abstracts.html.

[a] Property taxes only.

Table A.3: Sources for Income Tax Rates, Brackets, and Exemptions

Data Year	Source	Description
1911-1929	National Industrial Conference Board (1930)	History of all rates, brackets, exemptions
1922	U.S. Department of Commerce (1922)	Information on all rates, brackets, exemptions
1923	Witte (1923)	Information on all rates, top brackets, exemptions
1930	Bailey (1930)	Changes to rates, brackets, exemptions
1930-1932	Martin (1932)	Changes to top and bottom rates
1931-1932	Groves (1932)	Changes to rates
1931-2010	State of Idaho (2018)	History of Idaho rates and brackets
1933	Manning (1933)	Changes to rates, brackets, exemptions
1934	Manning (1934)	Changes to rates, brackets, exemptions
1935	Manning (1935)	Changes to rates, brackets, exemptions
1935-2010	Commonwealth of Pennsylvania	History of Pennsylvania rates and brackets
1936	Manning (1936)	Changes to rates, brackets, exemptions
1936	National Tax Association (1936)	Changes to rates, brackets, exemptions
1937	Manning (1937)	Changes to rates, brackets, exemptions
1937-2010	Schrock (2010)	History of Colorado rates and brackets
1938	Manning (1938)	Changes to rates, brackets, exemptions
1938	U.S. Department of Commerce (1938)	Information on all rates, brackets, exemptions
1939	Manning (1939)	Changes to rates, brackets, exemptions
1940	Manning (1940)	Changes to rates, brackets, exemptions
1941-2003	Office of Tax Policy Research (2003)	Top rate and bracket, bottom rate and bracket, exemptions
1970-2010	Government of the District of Columbia (2015)	Changes to Washington, D.C. rates, brackets, exemptions
2003-2010	Tax Policy Center (2019)	Top rate and bracket, bottom rate and bracket, exemptions
1934-2007	Akcigit et al. (2022)	Average and marginal tax rates at 90th percentile income

Notes: For detailed information about these sources, see Appendix D.

Appendix B Robustness Checks

B.1 Data Sources and Construction for Economic Shocks

Income per Capita. State-level personal income data are available from the Bureau of Economic Analysis for 1929-2018. We measure economic shocks for early adopters of the income tax (i.e. pre-World War II) in terms of personal income per capita between 1929 and 1933 for the following reasons: (1) state-level GDP data are not available prior to 1963; (2) state-level income data are not available prior to 1929; and (3) the change in income from 1929 to 1933 approximates the state-level impact of the first phase of the Great Depression. For late adopters (i.e. post-World War II), we measure income shocks in terms of the 1-year, lagged 5-year, and lagged 10-year changes in log personal income per capita, respectively.

Industry Shift-Share Employment. The industry shift-share employment shocks are the state employment growth predicted by national employment growth at the three-digit industry level and the base-year employment shares of each industry at the state level. We estimate national employment growth rates using leave-one-out means to avoid bias due to using own-state information. The employment data come from IPUMS and are missing in 1920. We thus control for the shift-share employment shock from 1910 to 1930 for early adopters of the income tax (i.e. pre-World War II). For late adopters (i.e. post-World War II), we control for the 10-year shift-share employment shocks in the current decade and previous decade.

Changes in State Unemployment. For early adopters of the income tax (i.e. pre-World War II), we control for the change in the state unemployment rate from 1910 to 1930. For late adopters (i.e. post-World War II), we measure unemployment shocks in terms of the 1-year, lagged 5-year, and lagged 10-year changes in log unemployment insurance compensation per capita (excluding state unemployment compensation), respectively. We construct the covariates this way because the employment data are missing in 1920 and the unemployment insurance compensation data are only available for 1948-2017.

B.2 Data Construction for Region-by-Year Effects

To control for arbitrary regional shocks, we replace the cohort-by-year effects with cohort-by-region-by-year effects $\phi^h_{c,r(i),t}$ in Equation (1). We define three regions: North, South, and West. The South and West regions correspond to the U.S. Census Bureau's four-region classification, and the North region combines the Census Bureau's Northeast and Midwest regions. We combine those two regions because the Northeast has no states that never introduced the income tax. As a result, there are no "clean" controls for late adopters of the income tax (i.e. post-World War II) in the Northeast when the four-region classification is used.

A byproduct of this approach is that states that never introduced the income tax in the West (Washington, Nevada, and Wyoming) no longer contribute to the estimates for late adopters, as no Western states introduced the income tax after 1945. As a result, our estimates for late adopters of the income tax exploit variation from only three control states (South Dakota, Texas, and Florida). When the number of control (or treated) clusters is very small, even the wild cluster bootstrap performs poorly, under-rejecting the null hypothesis (MacKinnon and Webb, 2017). We thus report two p-values: one based on the wild cluster bootstrap (in braces in Table B.2), and another based on the wild subcluster bootstrap (in brackets), which uses a bootstrap data-generating process that clusters at the state-year level. The wild subcluster bootstrap performs well in simulations when there are few control clusters (MacKinnon and Webb, 2018).

B.3 Inverse Probability Weighting

Let $Y_{i,t}^h(d)$ denote the fiscal outcome (measured in logs) in period $t+h$ for state i whose income tax status in period t is $d \in \{0,1\}$, where $d=0$ denotes no income tax and $d=1$ denotes an income tax. Let the random variable $D_{i,t}$ equal one if state i has an income tax in period t, and zero otherwise. The average effect of introducing the income tax in period t on the outcome h periods later for adopting states is

$$\beta^h \equiv \mathrm{E}(Y_{i,t}^h(1) - Y_{i,t}^h(0) \mid D_{i,t}=1, D_{i,t-1}=0). \tag{B.1}$$

Following Suárez Serrato and Wingender (2016) and Acemoglu, Naidu, Restrepo, and Robinson (2019), we adapt the semiparametric approach in Angrist and Kuersteiner (2011) to a panel context. Our approach models the selection of states into the introduction of the income tax without specifying a parametric model for outcomes. The key identifying assumption is that adopting and non-adopting states with the same recent fiscal and demographic trends would have experienced parallel fiscal trends going forward in the absence of introduction, denoted by $\Delta Y_{i,t}^h(0) = Y_{i,t}^h(0) - Y_{i,t-1}$. Let $Z_{i,t}$ denote the log population of state i in year t, let $R_{i,t}$ denote log revenue, and let $E_{i,t}$ denote log expenditure. The conditional parallel trends assumption is stated formally as follows.

Assumption 1. $\mathrm{E}(\Delta Y_{i,t}^h(0) \mid D_{i,t}=1, D_{i,t-1}=0, X_{i,t}) = \mathrm{E}(\Delta Y_{i,t}^h(0) \mid D_{i,t}=0, D_{i,t-1}=0, X_{i,t})$, *where*

$X_{i,t} = (\Delta R_{i,t-1}, \ldots, \Delta R_{i,t-J}, \Delta E_{i,t-1}, \ldots, \Delta E_{i,t-J}, \Delta Z_{i,t-1}, \ldots, \Delta Z_{i,t-K}, t)$, *for* $h \geq 0$.

Assumption 1 is weaker than the standard assumption in difference-in-differences designs since it only imposes parallel trends for states with the same recent fiscal and population dynamics. The second identifying assumption is a standard overlap condition.

Assumption 2. $\mathrm{P}(D_{i,t} = 1 \mid D_{i,t-1} = 0) > 0$ *and* $\mathrm{P}(D_{i,t} = 1 \mid D_{i,t-1} = 0, X_{i,t}) < 1$ *for all* $X_{i,t}$ *as defined in Assumption 1.*

Denote changes in the observed outcome by $\Delta Y_{i,t}^{h} = Y_{i,t+h} - Y_{i,t-1}$. Under Assumptions 1 and 2, β^h can be identified via inverse probability weighting (IPW) (Abadie, 2005),

$$\beta^h = \mathrm{E}(\omega_{i,t} \Delta Y_{i,t}^{h} \mid D_{i,t-1} = 0), \tag{B.2}$$

where the weighting function is

$$\omega_{i,t} = \frac{1}{\mathrm{P}(D_{i,t} = 1 \mid D_{i,t-1} = 0)} \cdot \frac{D_{i,t} - \mathrm{P}(D_{i,t} = 1 \mid D_{i,t-1} = 0, X_{i,t})}{1 - \mathrm{P}(D_{i,t} = 1 \mid D_{i,t-1} = 0, X_{i,t})}.$$

Intuitively, non-adopting states ($D_{i,t} = 0$) are given greater weight the more similar their recent dynamics were to states that did introduce the income tax in year t (high $\mathrm{P}(D_{i,t} = 1 \mid D_{i,t-1} = 0, X_{i,t})$). Our IPW estimator replaces the population objects in Equation (B.2) with their corresponding estimates.[B.1] The estimation sample includes, for each year t, only states that either introduced the income tax in year t or never had the income tax between years t and $t + 30$. This ensures that the only "clean" controls are used.

We estimate the propensity score, $\mathrm{P}(D_{i,t} = 1 \mid D_{i,t-1} = 0, X_{i,t})$, via probit. The covariate vector includes year effects, the lagged 3-year and 5-year changes in log revenue and log expenditure, and the lagged 5-year, 10-year, and 15-year changes in log population. In addition, it includes interactions between the lagged changes in the fiscal and population variables and an indicator for post-World War II years. This allows the selection process to differ for early (i.e. pre-World War II) and late adopters (i.e. post-World War II) of the income tax. We report standard errors that are

[B.1] Let $\widehat{\mathrm{P}}(X_{i,t})$ denote the estimated propensity score, and let $\widehat{\mathrm{E}}(\,\cdot \mid D_{i,t-1} = 0)$ denote the sample average over state-years for which $D_{i,t-1} = 0$. The estimator is $\hat{\beta}^h = \widehat{\mathrm{E}}(\widehat{\omega}_{i,t} \Delta Y_{i,t}^{h} \mid D_{i,t-1} = 0)$, where

$$\widehat{\omega}_{i,t} = \frac{1}{\widehat{\mathrm{E}}(D_{i,t} \mid D_{i,t-1} = 0)} \cdot \frac{D_{i,t} - \widehat{\mathrm{P}}(X_{i,t})}{1 - \widehat{\mathrm{P}}(X_{i,t})}.$$

robust to heteroskedasticity and state-level clustering. Appendix Figure B.11 plots the distribution of the propensity score separately for state-years in which the income tax is introduced and not introduced. The two distributions have similar supports, both of which are bounded away from 1 as required by Assumption 2.

B.4 Tables

Table B.1: Baseline State Characteristics

	Adopters	Non-Adopters	Difference
	Panel A: All Introductions		
Lagged Deficit per Capita	−10.39	−23.83	13.44
			(17.77)
Lagged Total Expenditure per Capita	930.80	768.48	162.32
			(177.75)
Lagged Total Revenue per Capita	941.19	792.31	148.88
			(187.92)
Lagged Population (thousands)	3,287.63	3,226.44	61.19
			(691.18)
Personal Income per Capita	12,758.89	11,964.27	794.62
			(1,611.45)
	Panel B: Pre-World War II		
Lagged Deficit per Capita	10.84	1.35	9.49
			(6.40)
Lagged Total Expenditure per Capita	268.93	274.98	−6.05
			(46.62)
Lagged Total Revenue per Capita	258.09	273.63	−15.54
			(45.53)
Lagged Population (thousands)	1,470.11	2,539.55	−1,069.44
			(660.54)
Personal Income per Capita	4,745.11	7,492.07	$-2,746.96^{***}$
			(766.98)
	Panel C: Post-World War II		
Lagged Deficit per Capita	−37.41	−75.69	38.28
			(41.64)
Lagged Total Expenditure per Capita	1,773.18	1,784.95	−11.77
			(234.83)
Lagged Total Revenue per Capita	1,810.59	1,860.64	−50.05
			(255.90)
Lagged Population (thousands)	5,600.85	4,671.99	928.86
			(1,346.04)
Personal Income per Capita	22,958.23	21,375.91	1,582.33
			(1,417.50)
Observations (All Introductions)	25	205	

Notes: This table reports average baseline characteristics for adopting and non-adopting states of the income tax. The sample of years corresponds to the adoption years used in the fiscal analysis. The fiscal variables and personal income are both measured in constant 2010 USD per capita. Lagged variables are defined as the average of their non-missing values in the four previous years (to account for missing data). Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Average Effects of Introduction of Income Tax: Region-by-Year Effects

	Average Effect of Income Tax Introduction over Time				
Years Since Introduction:	0 to 1	2 to 3	4 to 9	10 to 19	20 to 30
	(1)	(2)	(3)	(4)	(5)
	Panel A: Log Total Revenue				
All Introductions	4.8**	10.3**	5.3	12.5	10.8*
	(2.1)	(3.9)	(5.5)	(7.7)	(5.9)
	{0.158}	{0.136}	{0.407}	{0.225}	{0.177}
	[0.077]	[0.053]	[0.344]	[0.136]	[0.101]
	Panel B: Log Total Expenditure				
All Introductions	4.7*	8.4*	3.9	9.2	11.6*
	(2.6)	(4.6)	(5.3)	(7.6)	(6.1)
	{0.229}	{0.290}	{0.504}	{0.316}	{0.169}
	[0.162]	[0.168]	[0.483]	[0.241]	[0.094]
	Panel C: Log Total Revenue per Capita				
All Introductions	5.5**	11.0***	6.4	15.5**	14.7**
	(2.0)	(3.5)	(5.3)	(7.6)	(6.3)
	{0.112}	{0.087}	{0.316}	{0.159}	{0.128}
	[0.045]	[0.026]	[0.247]	[0.085]	[0.057]
	Panel D: Log Total Expenditure per Capita				
All Introductions	5.4**	9.1**	5.0	12.2	15.6**
	(2.5)	(4.5)	(5.2)	(7.5)	(6.7)
	{0.173}	{0.235}	{0.394}	{0.227}	{0.127}
	[0.114]	[0.125]	[0.364]	[0.150]	[0.065]
	Panel E: Log Population				
All Introductions	−0.4	−0.4	−1.0	−2.8	−3.9
	(0.4)	(0.7)	(1.2)	(2.7)	(4.7)
	{0.400}	{0.530}	{0.415}	{0.366}	{0.453}
	[0.432]	[0.589]	[0.501]	[0.356]	[0.429]
Observations	8,919	8,915	8,963	8,924	8,704
States	36	36	36	36	36

Notes: This table reports estimates of $\omega_e \beta^{h,e} + \omega_\ell \beta^{h,\ell}$ ("All Introductions") from Equation (1), where ω_e is the share of early adopters ("Pre-World War II") and ω_ℓ is the share of late adopters ("Post-World War II"), with region-by-year effects Estimates are averaged over the specified time horizons and multiplied by 100. Standard errors clustered by state are in parentheses. p-values based on the restricted wild cluster bootstrap are in braces, and p-values based on the restricted wild subcluster bootstrap (with boostrap draws at the state-year level) are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Heterogeneous Effects of Introduction of Income Tax: Region-by-Year Effects

	Average Effect of Income Tax Introduction over Time				
Years Since Introduction:	0 to 1 (1)	2 to 3 (2)	4 to 9 (3)	10 to 19 (4)	20 to 30 (5)
	Panel A: Log Total Revenue				
Pre-World War II Introductions	2.0	8.3	−1.2	12.1	13.4
	(5.3)	(7.8)	(9.3)	(13.5)	(10.2)
	{0.715}	{0.348}	{0.904}	{0.414}	{0.226}
	[0.720]	[0.346]	[0.898]	[0.372]	[0.188]
Post-World War II Introductions	6.4***	11.7***	13.5***	13.1***	7.5*
	(1.5)	(3.3)	(2.3)	(2.5)	(4.2)
	{0.054}	{0.224}	{0.041}	{0.036}	{0.319}
	[0.014]	[0.060]	[0.008]	[0.005]	[0.216]
	Panel B: Log Total Expenditure				
Pre-World War II Introductions	1.1	7.7	−3.3	6.2	10.7
	(6.7)	(9.9)	(9.2)	(13.5)	(10.4)
	{0.887}	{0.465}	{0.727}	{0.657}	{0.326}
	[0.883]	[0.459]	[0.721]	[0.639]	[0.295]
Post-World War II Introductions	6.7***	8.9**	13.0***	13.1***	12.8***
	(1.6)	(3.9)	(2.0)	(2.7)	(4.4)
	{0.087}	{0.643}	{0.026}	{0.039}	{0.302}
	[0.024]	[0.302]	[0.005]	[0.008]	[0.096]
	Panel C: Log Total Revenue per Capita				
Pre-World War II Introductions	2.0	8.5	−0.6	13.3	11.7
	(5.3)	(7.5)	(9.2)	(13.0)	(10.4)
	{0.723}	{0.317}	{0.948}	{0.348}	{0.296}
	[0.727]	[0.314]	[0.945]	[0.315]	[0.275]
Post-World War II Introductions	7.4***	12.8***	15.4***	18.3***	18.6***
	(1.3)	(2.6)	(2.1)	(3.0)	(4.2)
	{0.039}	{0.064}	{0.017}	{0.042}	{0.205}
	[0.005]	[0.012]	[0.001]	[0.002]	[0.006]
	Panel D: Log Total Expenditure per Capita				
Pre-World War II Introductions	1.1	7.9	−2.8	7.4	9.0
	(6.7)	(9.9)	(9.1)	(13.2)	(11.0)
	{0.882}	{0.452}	{0.771}	{0.593}	{0.436}
	[0.882]	[0.452]	[0.763]	[0.566]	[0.416]
Post-World War II Introductions	7.7***	10.1***	14.9***	18.3***	23.9***
	(1.4)	(3.4)	(1.9)	(3.2)	(5.1)
	{0.025}	{0.441}	{0.010}	{0.021}	{0.202}
	[0.006]	[0.155]	[0.002]	[0.003]	[0.004]
	Panel E: Log Population				
Pre-World War II Introductions	0.1	0.1	−0.2	−0.9	1.7
	(0.5)	(1.1)	(2.0)	(4.5)	(7.9)
	{0.882}	{0.919}	{0.943}	{0.855}	{0.840}
	[0.887]	[0.917]	[0.939]	[0.844]	[0.841]
Post-World War II Introductions	−1.0	−1.1	−2.0	−5.2*	−11.1***
	(0.7)	(0.9)	(1.5)	(2.7)	(3.8)
	{0.263}	{0.339}	{0.398}	{0.351}	{0.207}
	[0.330]	[0.425]	[0.419]	[0.216]	[0.078]
Observations	8,919	8,915	8,963	8,924	8,704
States	36	36	36	36	36

Notes: This table reports estimates of $\beta^{h,e}$ for early adopters ("Pre-World War II") and $\beta^{h,\ell}$ for late adopters ("Post-World War II") from Equation (1) with region-by-year effects. Estimates are averaged over the specified time horizons and multiplied by 100. Standard errors clustered by state are in parentheses. p-values based on the restricted wild cluster bootstrap are in braces, and p-values based on the restricted wild subcluster bootstrap (with boostrap draws at the state-year level) are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Average Effects of Introduction of Income Tax: Cohort-Specific Effects

	Average Effect of Income Tax Introduction over Time				
Years Since Introduction:	0 to 1	2 to 3	4 to 9	10 to 19	20 to 30
	(1)	(2)	(3)	(4)	(5)
	Panel A: Log Total Revenue				
All Introductions	3.9^{*}	10.2^{***}	4.9	10.6^{*}	12.5^{*}
	(2.2)	(2.5)	(4.4)	(6.0)	(6.9)
	[−1.2, 10.0]	[4.4, 16.4]	[−4.4, 14.5]	[−1.9, 23.5]	[−2.6, 27.8]
	Panel B: Log Total Expenditure				
All Introductions	3.9	10.5^{***}	6.2	7.1	10.4
	(2.3)	(3.1)	(4.2)	(6.2)	(7.0)
	[−2.0, 10.0]	[2.2, 19.2]	[−2.8, 15.4]	[−6.1, 20.8]	[−4.9, 26.3]
	Panel C: Log Total Revenue per Capita				
All Introductions	4.1^{*}	10.4^{***}	5.1	12.4^{**}	14.2^{***}
	(2.1)	(2.4)	(4.1)	(4.7)	(5.1)
	[−1.0, 10.2]	[4.8, 16.5]	[−3.5, 14.1]	[2.8, 22.6]	[3.2, 25.5]
	Panel D: Log Total Expenditure per Capita				
All Introductions	4.1^{*}	10.8^{***}	6.4	8.9^{*}	12.1^{**}
	(2.3)	(3.2)	(4.0)	(5.1)	(5.2)
	[−1.9, 10.3]	[2.1, 19.6]	[−2.1, 15.0]	[−1.8, 19.9]	[0.8, 23.7]
	Panel E: Log Population				
All Introductions	0.2	0.4	−0.2	−1.7	−1.8
	(0.3)	(0.6)	(1.2)	(2.7)	(3.8)
	[−0.5, 0.8]	[−0.8, 1.7]	[−2.9, 2.6]	[−7.8, 4.4]	[−10.0, 6.6]
Observations	9,122	9,118	9,165	9,118	8,854
States	36	36	36	36	36

Notes: This table reports estimates of the average of $\beta^{h,j}$ from Equation (2), weighted by cohort size ("All Introductions"). Estimates are averaged over the specified time horizons and multiplied by 100. Standard errors clustered by state are in parentheses. 95-percent confidence intervals based on the restricted wild cluster bootstrap are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Heterogeneous Effects of Introduction of Income Tax: Cohort-Specific Effects

	Average Effect of Income Tax Introduction over Time				
Years Since Introduction:	0 to 1 (1)	2 to 3 (2)	4 to 9 (3)	10 to 19 (4)	20 to 30 (5)
	Panel A: Log Total Revenue				
Pre-World War II Introductions	2.9	11.6^{**}	2.1	18.6^{*}	25.1^{**}
	(5.8)	(5.1)	(7.3)	(10.2)	(10.6)
	[−10.3, 24.5]	[−1.4, 25.6]	[−13.5, 17.3]	[−2.9, 39.7]	[2.8, 47.4]
Post-World War II Introductions	4.4^{***}	9.2^{***}	8.4^{**}	0.5	-3.5
	(1.5)	(2.8)	(3.2)	(6.3)	(5.9)
	[0.6, 8.3]	[2.3, 16.2]	[0.5, 16.4]	[−18.1, 18.9]	[−19.1, 12.4]
	Panel B: Log Total Expenditure				
Pre-World War II Introductions	2.1	11.8	3.0	11.1	19.1^{*}
	(6.3)	(7.2)	(7.3)	(10.5)	(9.8)
	[−15.2, 21.5]	[−7.7, 31.5]	[−12.7, 18.4]	[−11.1, 33.1]	[−1.2, 39.6]
Post-World War II Introductions	4.8^{***}	9.6^{***}	10.3^{***}	2.0	-0.7
	(1.2)	(2.3)	(2.9)	(5.2)	(6.7)
	[2.0, 7.7]	[4.0, 15.4]	[2.7, 17.3]	[−12.3, 17.3]	[−19.2, 16.7]
	Panel C: Log Total Revenue per Capita				
Pre-World War II Introductions	1.8	9.4^{*}	-2.1	11.0	12.9
	(5.7)	(5.1)	(7.1)	(8.9)	(8.5)
	[−11.5, 23.3]	[−3.5, 23.1]	[−17.3, 12.9]	[−7.7, 29.7]	[−4.6, 31.0]
Post-World War II Introductions	5.3^{***}	11.2^{***}	14.2^{***}	14.3^{***}	15.8^{***}
	(1.4)	(2.5)	(3.0)	(4.1)	(5.1)
	[1.8, 8.9]	[5.0, 17.6]	[7.1, 21.2]	[3.9, 24.0]	[2.3, 27.1]
	Panel D: Log Total Expenditure per Capita				
Pre-World War II Introductions	1.0	9.6	-1.2	3.4	7.0
	(6.3)	(7.4)	(7.1)	(9.5)	(8.3)
	[−16.0, 20.0]	[−10.3, 29.8]	[−16.9, 14.0]	[−16.8, 23.6]	[−10.2, 24.5]
Post-World War II Introductions	5.7^{***}	11.7^{***}	16.2^{***}	15.9^{***}	18.6^{***}
	(1.1)	(2.2)	(3.0)	(3.6)	(5.8)
	[3.1, 8.5]	[6.4, 17.0]	[9.1, 23.5]	[6.4, 24.1]	[1.3, 32.6]
	Panel E: Log Population				
Pre-World War II Introductions	1.1^{**}	2.4^{**}	4.3^{**}	7.8^{**}	12.1^{*}
	(0.5)	(1.0)	(1.8)	(3.7)	(6.4)
	[0.1, 2.0]	[0.2, 4.5]	[0.7, 7.9]	[0.3, 15.3]	[−1.0, 25.2]
Post-World War II Introductions	-0.9^{**}	-2.0^{**}	-5.9^{**}	-13.8^{***}	-19.4^{***}
	(0.4)	(0.9)	(2.2)	(4.5)	(5.2)
	[−1.7, −0.1]	[−4.0, 0.1]	[−11.1, −0.4]	[−25.2, −2.6]	[−31.0, −6.7]
Observations	9,122	9,118	9,165	9,118	8,854
States	36	36	36	36	36

Notes: This table reports estimates of the average of $\beta^{h,j}$ from Equation (2), weighted by cohort size, separately for early adopters ("Pre-World War II") and late adopters ("Post-World War II"). Estimates are averaged over the specified time horizons and multiplied by 100. Standard errors clustered by state are in parentheses. 95-percent confidence intervals based on the restricted wild cluster bootstrap are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Average Effects of Introduction of Income Tax: IPW Estimates

	Average Effect of Income Tax Introduction over Time				
Years Since Introduction:	0 to 1 (1)	2 to 3 (2)	4 to 9 (3)	10 to 19 (4)	20 to 30 (5)
	Panel A: Log Total Revenue				
All Introductions	4.3	11.1^{***}	7.6^{*}	11.7^{**}	16.9^{***}
	(2.7)	(3.4)	(4.6)	(4.7)	(5.6)
	Panel B: Log Total Expenditure				
All Introductions	3.8	9.4^{**}	7.4	7.5	15.3^{***}
	(3.0)	(4.7)	(4.8)	(4.8)	(5.3)
	Panel C: Log Total Revenue per Capita				
All Introductions	4.6^{*}	11.8^{***}	8.8^{*}	15.3^{***}	20.6^{***}
	(2.5)	(3.3)	(4.7)	(4.0)	(4.9)
	Panel D: Log Total Expenditure per Capita				
All Introductions	4.1	10.2^{**}	8.6^{*}	11.2^{**}	19.0^{***}
	(2.9)	(4.6)	(5.1)	(4.9)	(5.5)
	Panel E: Log Population				
All Introductions	−0.1	−0.1	−1.2	−3.7	−3.7
	(0.4)	(0.7)	(1.5)	(3.4)	(5.2)

Notes: This table reports IPW estimates of the average effect of the introduction of the individual income tax over different time horizons based on Equation (B.2). We report the average treatment effect on the treated, multiplied by 100. The treatment effect is estimated via inverse probability weighting, where the propensity score is specified as a probit model with year effects, lagged 3-year and 5-year changes in log revenue and log expenditure, and lagged 5-year, 10-year, and 15-year changes in log population. In addition, it includes interactions between the lagged changes in the fiscal and population variables and an indicator for post-World War II years. Standard errors clustered by state are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: Heterogeneous Effects of Introduction of Income Tax: IPW Estimates

	Average Effect of Income Tax Introduction over Time				
Years Since Introduction:	0 to 1 (1)	2 to 3 (2)	4 to 9 (3)	10 to 19 (4)	20 to 30 (5)
	Panel A: Log Total Revenue				
Pre-World War II Introductions	−2.1 (5.5)	6.7 (6.4)	1.6 (7.6)	17.6** (7.9)	26.9*** (7.9)
Post-World War II Introductions	7.3*** (2.1)	13.4*** (3.4)	12.8*** (3.5)	1.8 (5.6)	0.2 (5.3)
	Panel B: Log Total Expenditure				
Pre-World War II Introductions	−1.8 (6.9)	5.4 (9.2)	1.2 (7.3)	9.7 (8.1)	21.7*** (7.9)
Post-World War II Introductions	6.5*** (2.3)	11.6*** (3.9)	13.6*** (4.3)	2.8 (5.3)	2.8 (5.7)
	Panel C: Log Total Revenue per Capita				
Pre-World War II Introductions	−2.3 (5.4)	6.8 (6.4)	−0.9 (7.5)	11.2* (6.7)	16.9** (7.3)
Post-World War II Introductions	7.8*** (1.9)	14.6*** (3.1)	18.2*** (3.2)	17.0*** (3.9)	20.8*** (3.9)
	Panel D: Log Total Expenditure per Capita				
Pre-World War II Introductions	−1.9 (6.7)	5.5 (9.3)	−1.3 (7.5)	3.4 (7.8)	11.6 (8.3)
Post-World War II Introductions	6.9*** (2.2)	12.8*** (3.8)	19.0*** (4.5)	18.1*** (4.6)	23.4*** (4.6)
	Panel E: Log Population				
Pre-World War II Introductions	0.2 (0.3)	0.8 (0.8)	2.4 (1.9)	6.3 (4.0)	10.0 (6.8)
Post-World War II Introductions	−0.5 (0.7)	−1.2 (1.1)	−5.4*** (2.1)	−15.2*** (3.9)	−20.6*** (5.1)

Notes: This table reports IPW estimates of the average effect of the introduction of the individual income tax over different time horizons based on Equation (B.2). We report the average treatment effect on the treated, multiplied by 100, separately for early adopters ("Pre-World War II") and late adopters ("Post-World War II") of the income tax. The treatment effect is estimated via inverse probability weighting, where the propensity score is specified as a probit model with year effects, lagged 3-year and 5-year changes in log revenue and log expenditure, and lagged 5-year, 10-year, and 15-year changes in log population. In addition, it includes interactions between the lagged changes in the fiscal and population variables and an indicator for post-World War II years. Standard errors clustered by state are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.8: Effects of Introduction of Income Tax on Outmigration: Same Sample as Fiscal Analysis

	All Flows	Outmigration Flows by Occupational Earnings Percentile				
		< 25	$[25, 50)$	$[50, 75)$	$[75, 90)$	≥ 90
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Child-Based 5-Year Migration Rate, 1900-2010					
Post Income Tax	0.143***	0.049	0.167***	0.145**	0.164***	0.202***
(Origin − Destination)	(0.037)	(0.054)	(0.049)	(0.061)	(0.052)	(0.045)
Observations	9,820	2,204	2,204	2,204	2,204	2,204
	Panel B: Child-Based 5-Year Migration Rate, 1940-2010					
Post Income Tax	0.186***	−0.038	0.233***	0.191***	0.224***	0.204***
(Origin − Destination)	(0.038)	(0.059)	(0.052)	(0.072)	(0.059)	(0.052)
Observations	7,500	1,851	1,851	1,851	1,851	1,851
	Panel C: Census 5-Year Migration Rate, 1940-2000					
Post Income Tax	0.199***	0.011	0.214***	0.252***	0.245***	0.291***
(Origin − Destination)	(0.036)	(0.043)	(0.046)	(0.042)	(0.042)	(0.041)
Observations	7,137	4,436	4,436	4,436	4,436	4,436

Notes: This table reports estimates of θ in Equation (3), using only state pairs for which both states were included in the fiscal analysis in Section 5. The results in Panels A and B are for the child-based 5-year migration rate between 1900-2010 and 1940-2010, respectively, while the results in Panel C are for the census 5-year migration rate between 1940-2010. Column 1 reports the results for the full sample of interstate migration flows. Columns 2-6 report separate results for different occupational earnings groups, and the sample is restricted to state-pair-years with at least one moving household in each group. The outcome variable is the log odds ratio of the population share that moved from the origin state to the destination state relative to the population share that remained in the origin state. *Post Income Tax (Origin − Destination)* is the difference between the origin's and destination's indicator variable *Post Income Tax*, which equals 1 after the state introduced the individual income tax. All regressions include origin-destination fixed effects and region-pair × year effects. Standard errors, in parentheses, are robust to heteroskedasticity and three-way clustering at the origin-destination pair, origin × year, and destination × year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.9: Effect of Introduction of Income Tax on Outmigration by Income

	Outmigration Flows by Income Percentile				
	< 25	$[25, 50)$	$[50, 75)$	$[75, 90)$	≥ 90
	(1)	(2)	(3)	(4)	(5)
	Panel A: Child-Based 5-Year Migration Rate, 1940-2010				
Post Income Tax	-0.050	0.118^{**}	0.250^{***}	0.215^{***}	0.284^{***}
(Origin − Destination)	(0.058)	(0.047)	(0.060)	(0.058)	(0.058)
Observations	3,064	3,064	3,064	3,064	3,064
	Panel B: Census 5-Year Migration Rate, 1940-2000				
Post Income Tax	-0.001	0.117^{***}	0.301^{***}	0.277^{***}	0.270^{***}
(Origin − Destination)	(0.038)	(0.034)	(0.038)	(0.039)	(0.043)
Observations	7,780	7,780	7,780	7,780	7,780

Notes: This table reports estimates of θ in Equation (3). The results in Panel A are for the child-based 5-year migration rate, and the results in Panel B are for the census 5-year migration rate, both between 1940 and 2010. Separate results are reported for households in different income groups, and the sample is restricted to state-pair-years with at least one moving household in every income group. The outcome variable is the log odds ratio of the population share that moved from the origin state to the destination state relative to the population share that remained in the origin state. *Post Income Tax (Origin − Destination)* is the difference between the origin's and destination's indicator variable *Post Income Tax*, which equals 1 after the state introduced the individual income tax. All regressions include origin-destination fixed effects and region-pair × year effects. Standard errors, in parentheses, are robust to heteroskedasticity and three-way clustering at the origin-destination pair, origin × year, and destination × year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

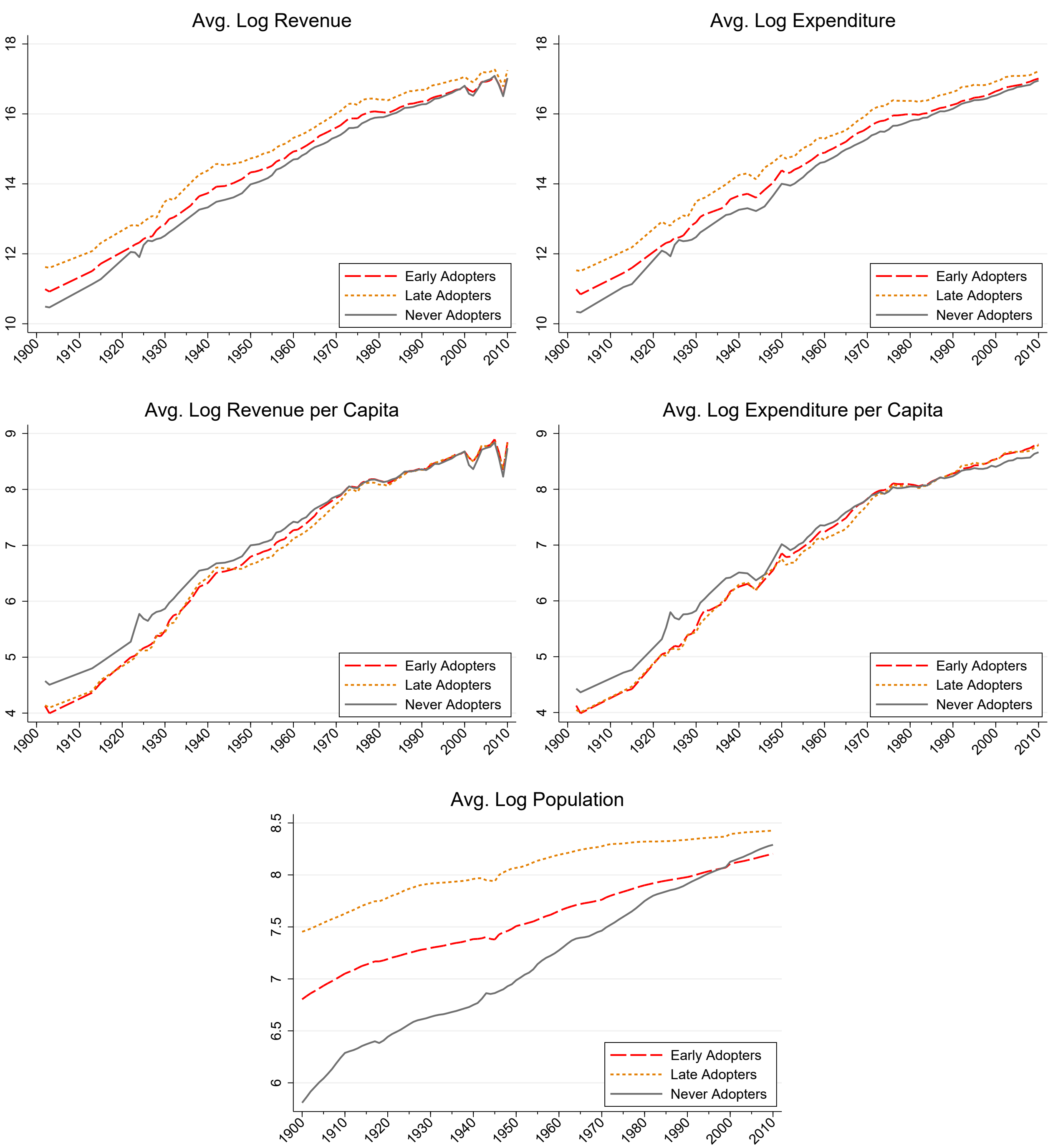
Table B.10: Effect of Income Tax Differentials on Outmigration: Heterogeneity by Proximity

	All Outmigration Flows					
	1900-2010			1940-2010		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Child-Based 5-Year Migration Rate, 1940-2010					
Post Income Tax (O − D)	0.124***	0.118***	0.118***	0.185***	0.179***	0.193***
	(0.033)	(0.032)	(0.035)	(0.034)	(0.033)	(0.036)
Post Income Tax (O − D) × Neighbors	−0.071			−0.191***		
	(0.059)			(0.059)		
Post Income Tax (O − D) × Same Region		−0.036			−0.131	
		(0.056)			(0.081)	
Post Income Tax (O − D) × Same Large Region			−0.017			−0.124**
			(0.043)			(0.057)
Observations	17,780	17,780	17,780	13,619	13,619	13,619
	Panel B: Census 5-Year Migration Rate, 1940-2000					
Post Income Tax (O − D)				0.187***	0.187***	0.201***
				(0.047)	(0.044)	(0.043)
Post Income Tax (O − D) × Neighbors				−0.130**		
				(0.054)		
Post Income Tax (O − D) × Same Region					−0.111	
					(0.071)	
Post Income Tax (O − D) × Same Large Region						−0.120**
						(0.059)
Observations				13,275	13,275	13,275

Notes: This table reports estimates of Equation (3) augmented to include interactions with the indicator variables measuring geographic proximity. *Neighbors* equals 1 if the origin and destination states share a border. *Same Region* equals 1 if the origin and destination states are located in the same region according to the U.S. Census Bureau's nine-region categorization. *Same Large Region* equals 1 if the origin and destination states are located in the same region according to the U.S. Census Bureau's four-region categorization. The results in Panel A are for the child-based 5-year migration rate, and the results in Panel B are for the census 5-year migration rate, both between 1940 and 2010. The outcome variable is the log odds ratio of the population share that moved from the origin state to the destination state relative to the population share that remained in the origin state. *Post Income Tax (O − D)* is the difference between the origin's and destination's indicator variable *Post Income Tax*, which equals 1 after the state introduced the income tax. All regressions include origin-destination fixed effects and year effects. Standard errors, in parentheses, are robust to heteroskedasticity and three-way clustering at the origin-destination pair, origin × year, and destination × year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

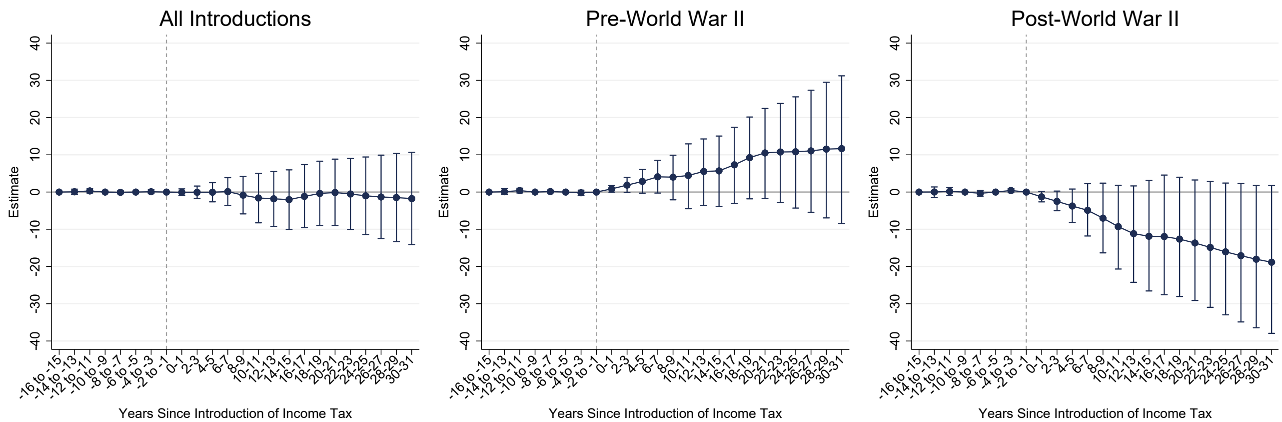
B.5 Figures

Figure B.1: Trends in Outcomes by Treatment Group



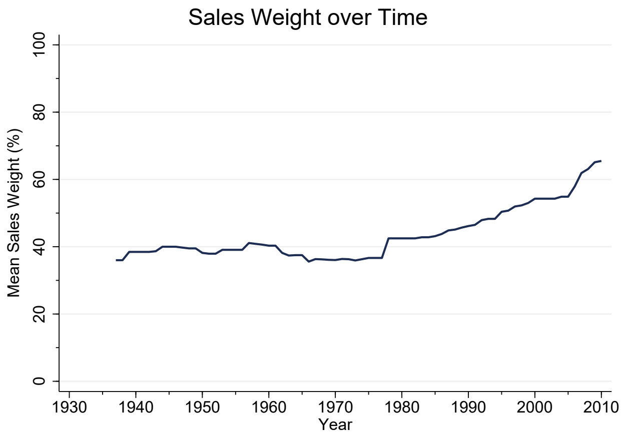
Notes: This figure plots average outcomes over time separately for early adopters ("Pre-World War II"), late adopters ("Post-World War II"), and never-adopters of the income tax.

Figure B.2: Dynamic Effects of the Introduction of the Income Tax on Population



Notes: The outcome is log population. The left panel plots estimates of $\omega_e \beta^{h,e} + \omega_\ell \beta^{h,\ell}$, the middle panel plots estimates of $\beta^{h,e}$, and the right panel plots estimates of $\beta^{h,\ell}$, all from Equation (1), where ω_e is the share of early adopters ("Pre-World War II") and ω_ℓ is the share of late adopters ("Post-World War II"). 95-percent confidence intervals based on the wild cluster bootstrap are reported. All estimates are multiplied by 100.

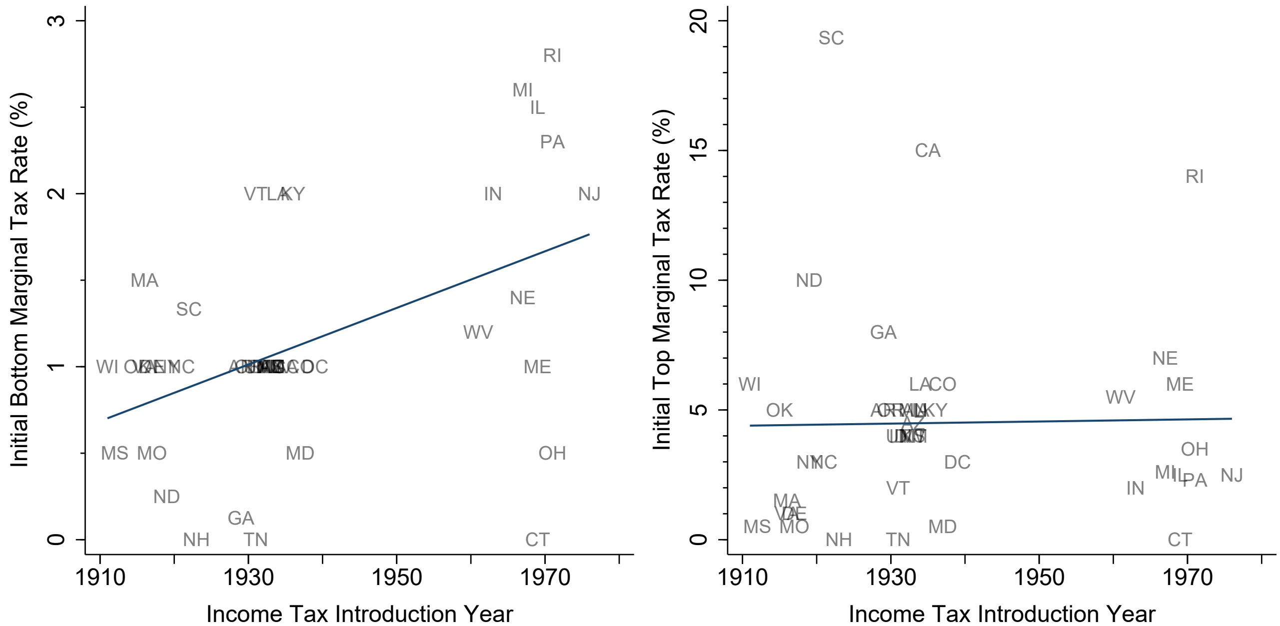
Figure B.3: Evolution of State Corporate Income Tax Apportionment Rules over Time



Notes: The left panel plots the average weight over time given to sales in apportioning corporate income of multi-state firms. The right panel plots the average weight given to sales, and 95-percent confidence intervals clustered by state, as a function of the amount of time since the adoption of the corporate income tax. The data are from Akcigit et al. (2022).

Figure B.4: Initial Tax Rates by Year of Introduction of Income Tax

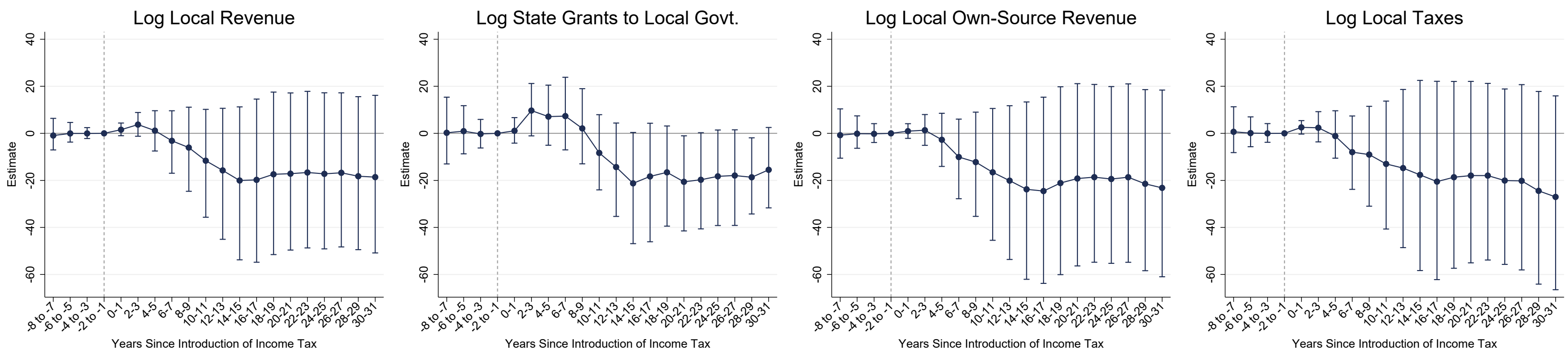
(a) Bottom Marginal Tax Rate (b) Top Marginal Tax Rate



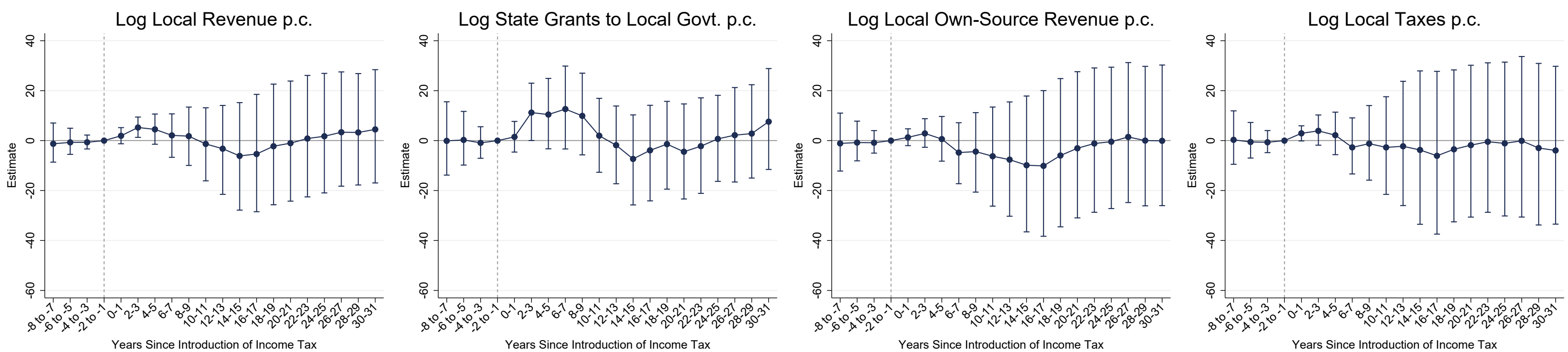
Notes: This figure plots the initial bottom marginal income tax rate (Panel a) and the initial top marginal income tax rate (Panel b) against the year of introduction of the income tax. All tax rates are statutory. The solid line plots the line of best fit from a univariate regression.

Figure B.5: Dynamic Effects of Introduction of Income Tax on Local Government Revenue

(a) Local Government Revenue (Post-World War II)



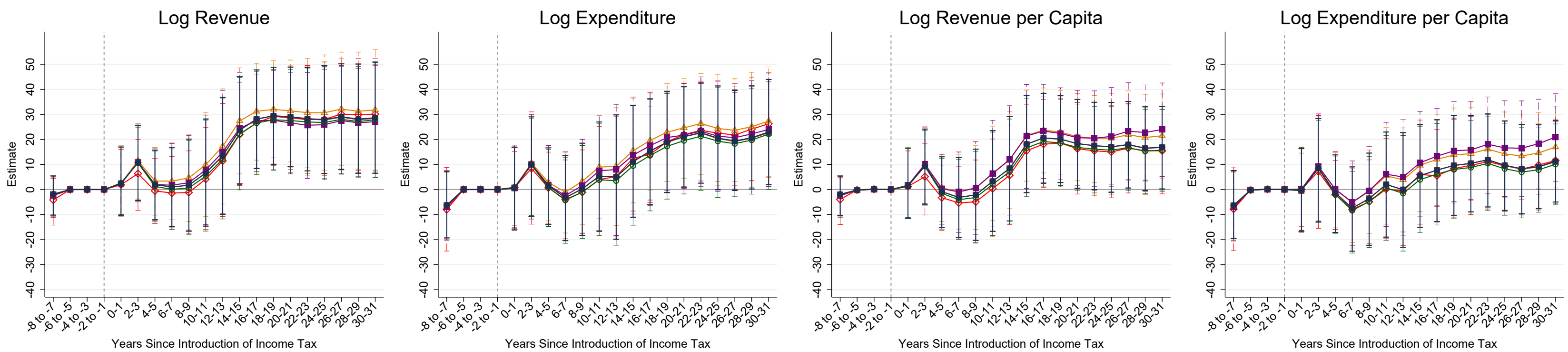
(b) Local Government Revenue Per Capita (Post-World War II)



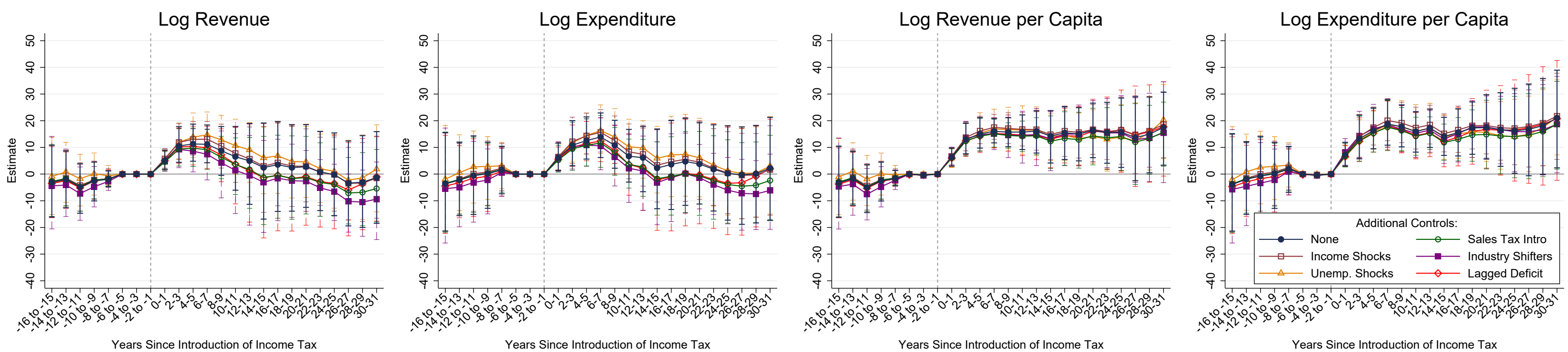
Notes: This figure plots estimates of $\beta^{h,\ell}$ for late adopters ("Post-World War II") from Equation (1). In Panel (a) local government revenue is measured in absolute terms, and in Panel (b) it is measured in per capita terms. Each outcome is aggregated across all local governments in the state. The data come from the Census of Governments and are available for the years 1953, 1957, and 1961-2008. Data on state grants to local government are missing in 1953, so we impute it using total local revenue in 1953 and the share of state grants in total revenue in 1957. To facilitate the estimation of pre-trends, we log-linearly interpolate missing values for years 1954-1956 and 1958-1960 using state-specific annual growth rates from 1953 to 1957. 95-percent confidence intervals based on the wild cluster bootstrap are reported. All estimates are multiplied by 100.

Figure B.6: Dynamic Effects of Introduction of Income Tax on Revenue and Expenditure: Robustness

(a) Early Adopters (Pre-World War II)



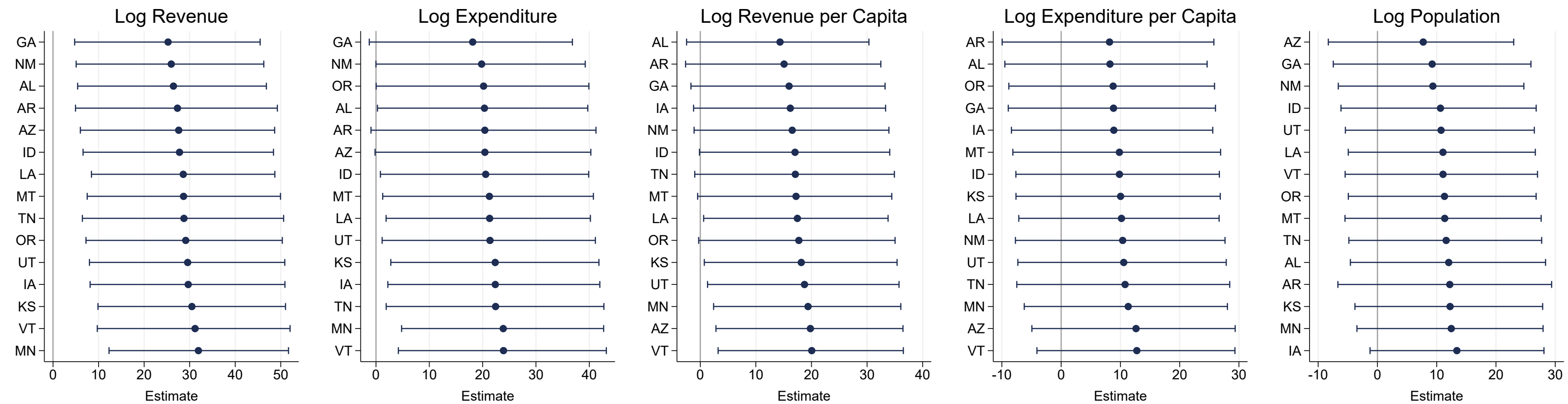
(b) Late Adopters (Post-World War II)



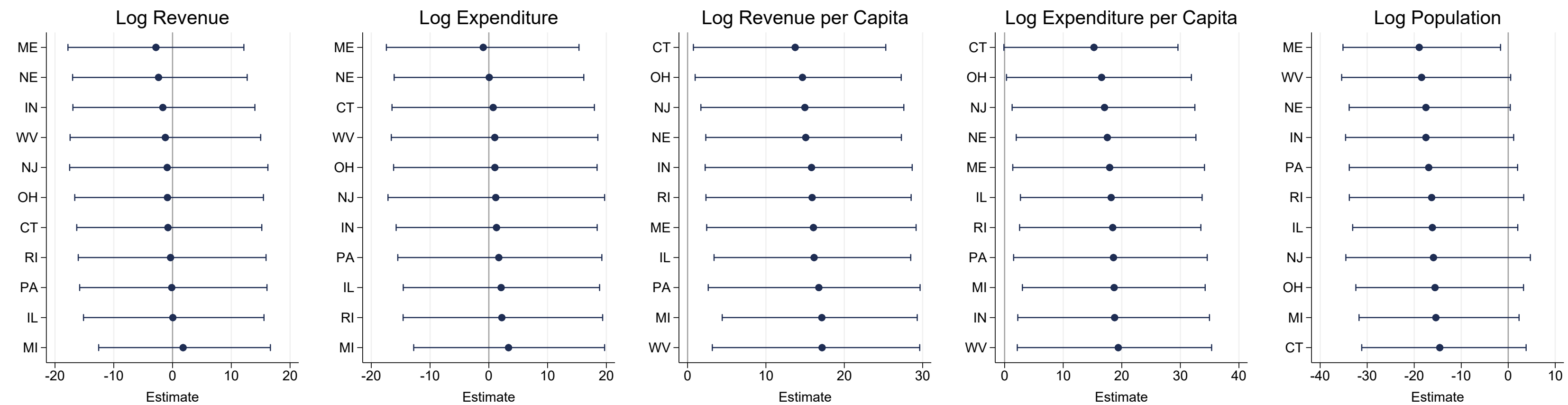
Notes: Panel (a) plots estimates of $\beta^{h,e}$ for early adopters ("Pre-World War II"), and Panel (b) plots estimates of $\beta^{h,\ell}$ for late adopters ("Post-World War II"), both from Equation (1). Additional covariates are added to the model as indicated in the figure's legend. The sales tax control is a dummy variable for the introduction of the sales tax within 10 years (past or future), interacted with pre-1945 and post-1945 dummies. The income shocks are the change in log state personal income per capita from 1929 to 1933, interacted with a pre-1945 dummy; and the 1-year, lagged 5-year, and lagged 10-year changes in log state personal income per capita, all interacted with post-1945 dummies. The industry shifters are the shift-share employment shock from 1910 to 1930, interacted with a pre-1945 dummy; and the 10-year shift-share employment shocks in the current decade and the previous decade, both interacted with post-1945 dummies. The unemployment shocks are the change in the state unemployment rate from 1910 to 1930, interacted with a pre-1945 dummy; and the 1-year, lagged 5-year, and lagged 10-year changes in log unemployment insurance compensation per capita (excluding state unemployment compensation), all interacted with post-1945 dummies. The final covariate is the lagged deficit per capita, interacted with pre-1945 and post-1945 dummies. 95-percent confidence intervals based on the wild cluster bootstrap are reported. All estimates are multiplied by 100.

Figure B.7: Long-Run Effects of Introduction of Income Tax: Exclude Adopting States One by One

(a) Early Adopters (Pre-World War II)



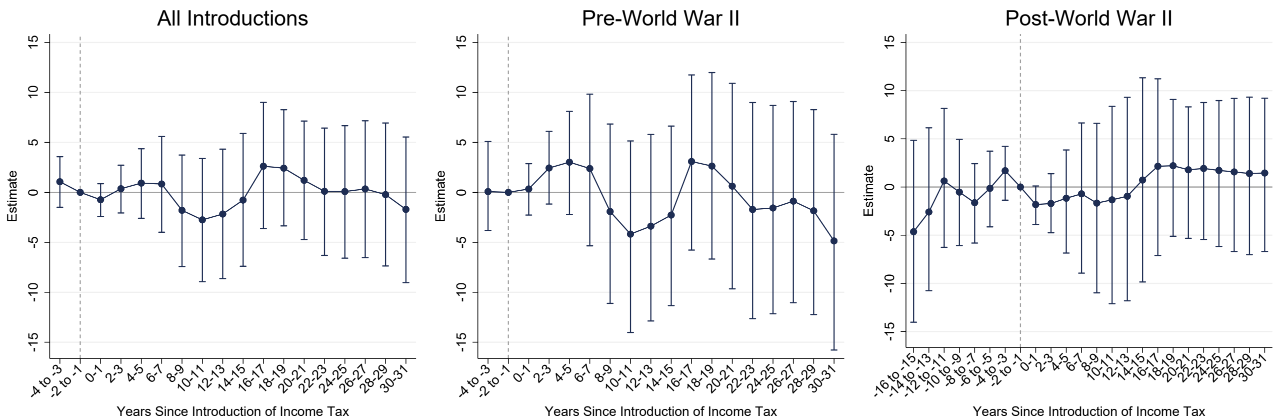
(b) Late Adopters (Post-World War II)



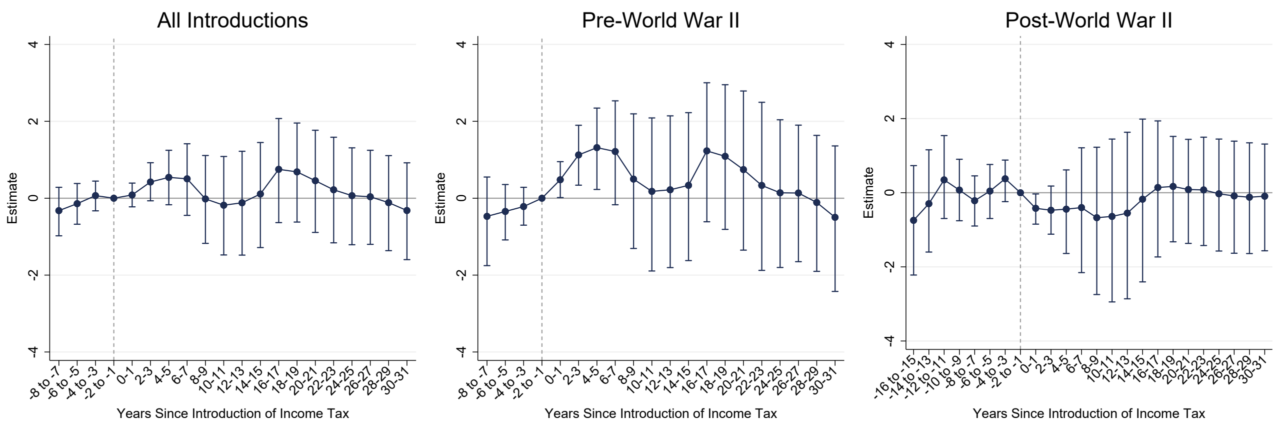
Notes: This figure plots the results when excluding states that introduced the income tax one by one, as indicated in the y-axis. Panel (a) plots estimates of $\beta^{h,e}$ for early adopters ("Pre-World War II"), and Panel (b) plots estimates of $\beta^{h,\ell}$ for late adopters ("Post-World War II"), both from Equation (1) and averaged from $h = 20$ to $h = 30$. 95-percent confidence intervals based on the wild cluster bootstrap are reported. All estimates are multiplied by 100.

Figure B.8: Dynamic Effects of Introduction of Income Tax on Fertility and Mortality

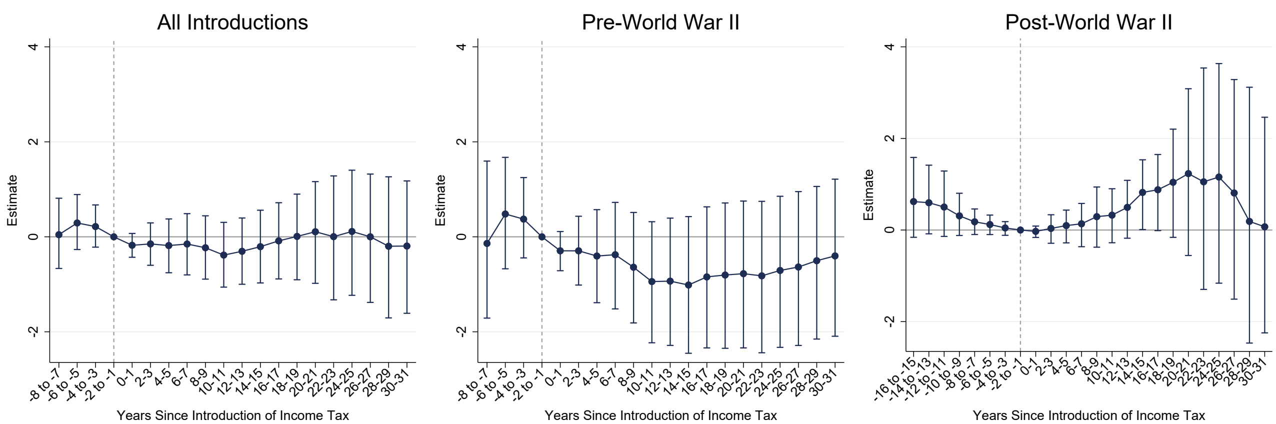
(a) Fertility Rate



(b) Birth Rate

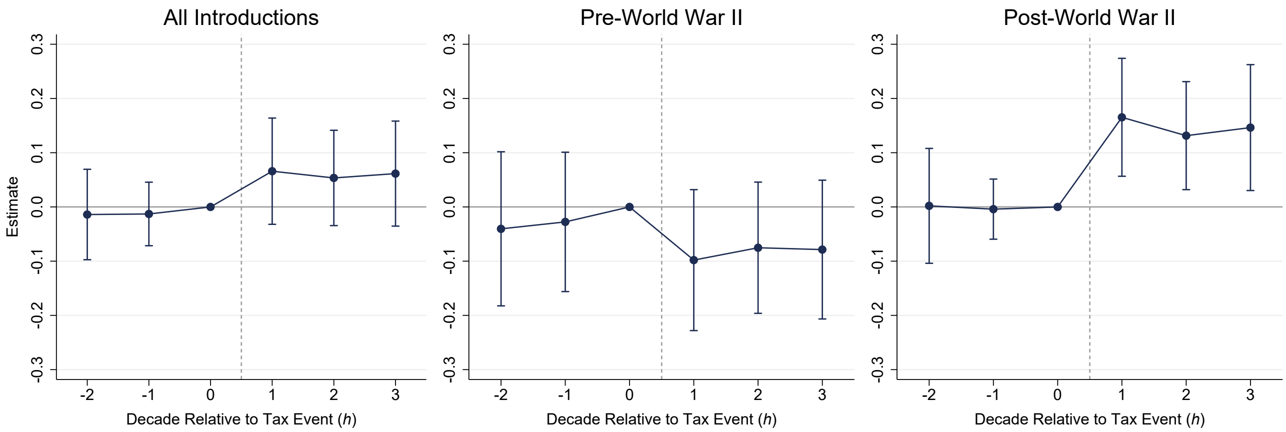


(c) Mortality Rate



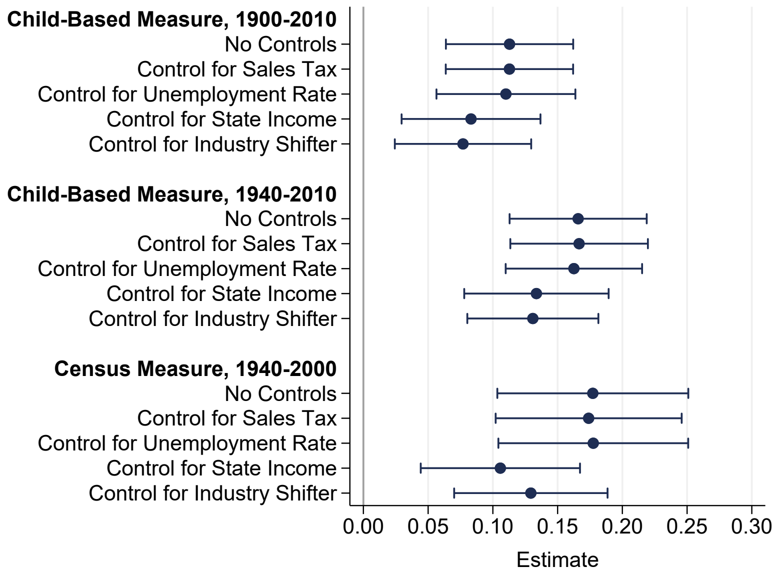
Notes: This figure plots estimates of $\omega_e \beta^{h,e} + \omega_\ell \beta^{h,\ell}$ ("All Introductions"), $\beta^{h,e}$ for early adopters ("Pre-World War II"), and $\beta^{h,\ell}$ for late adopters ("Post-World War II") from Equation (1). The fertility rate is the number of births per 1,000 women aged 15–44. Birth and death rates are both measured per 1,000 people. Fewer periods are available to estimate pre-trends in fertility, because the data on the number of women of reproductive age only starts in 1930. The data on births and deaths begin in 1915. 95-percent confidence intervals based on the wild cluster bootstrap are reported.

Figure B.9: Dynamic Effects of Introduction of Income Tax on Outmigration



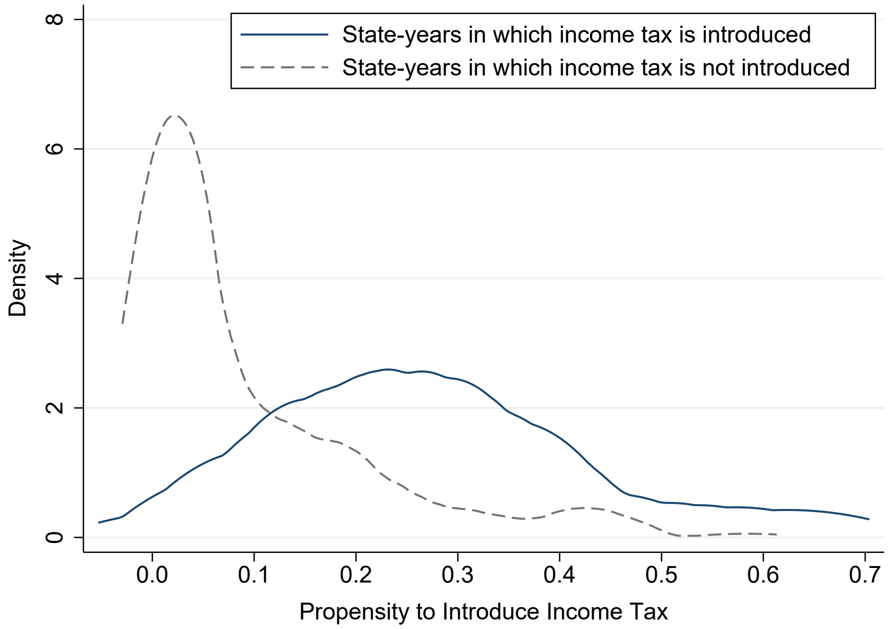
Notes: This figure plots estimates of β^h from Equation (4) using the child-based 5-year migration rate. The estimates are based on a balanced panel from two decades before the event to three decades after. The sample includes "treated" state pairs that experienced the introduction of the income tax and "clean control" pairs that did not during the analysis window. Event time is measured in decades relative to the introduction of the income tax, where the introduction occurred between periods 0 and 1. 95-percent confidence intervals are robust to heteroskedasticity and three-way clustering at the origin-destination pair, origin × year, and destination × year levels.

Figure B.10: Effects of Introduction of Income Tax on Outmigration: Robustness



Notes: This figure plots point estimates and 95-percent confidence intervals for θ in Equation (3) for different measures of outmigration, time periods, and sets of control variables. The sales tax control is $D^S_{ot} - D^S_{dt}$, where D^S_{ot} is an indicator variable equal to one if the origin state had a sales tax in year t. D^S_{dt} is defined similarly for the destination state. The remaining controls are defined as the destination-origin differentials in the state unemployment rate, log state personal income per capita, and the industry shift-share employment shock. The unemployment rate is missing in 1900 and 1920, and state income is missing in 1900, 1910, and 1920. The industry shifter is missing prior to 1940, because it is based on 10-year changes in employment, and employment is missing in 1900 and 1920. Thus, for the estimates using the child-based measure during 1900-2010, we control for the industry shifter interacted with a dummy for years 1940 and later, and the 20-year industry shifter for 1910-1930 interacted with a dummy for year 1930. Years 1900-1920 are omitted when we control for these industry shifters.

Figure B.11: Density of Estimated Propensity Scores



Notes: This figure plots the density of the estimated probability of introducing the income tax in year t conditional on not having the income tax in year $t-1$. The solid line plots the density for state-years in which the income tax was introduced, and the dashed line plots the density for state-years in which the income tax was not introduced. The propensity score is estimated using the probit model from Table 2. Densities are estimated using the Epanechnikov kernel.

Appendix C Deriving Migration Responses to Tax Reforms

C.1 Model of Location Choice

Let the utility of individual i who lived in state o (origin) in year $t-1$ and moves to state d (destination) in year t be

$$U_{iodt} = -\psi D_{dt} + \alpha \log(1-\tau_{dt}) + \alpha \log w_{dt} + Z_d - C_{od} + e_{idt}, \tag{C.1}$$

where D_{dt} is an indicator variable equal to one if the state of residence has an income tax, τ_{dt} is the personal income tax rate in the state of residence, w_{dt} is the before-tax wage in the state of residence, Z_d measures the effect of amenities and cost of living on utility, and C_{od} is the utility cost of moving from state o to state d, where $C_{oo} = 0$. The individual's idiosyncratic preferences for state d in time t are represented by e_{idt}.

The utility function in Equation (C.1) is the same as the one used in Moretti and Wilson (2017), except for the additional term $-\psi D_{dt}$, which allows tax introductions to affect behavior through channels other than the net-of-tax rate.

In every period, individuals choose their location to maximize utility. The desirability of each destination depends on the current state of residence, as indicated in Equation (C.1). An individual currently living in state o moves to state d if and only if she receives higher utility in state d than in state o or any other state, i.e.,

$$U_{iodt} > \max_{d' \neq d}\{U_{iod't}\}.$$

If idiosyncratic preferences, e_{idt}, are i.i.d. with an Extreme Value Type I distribution, then the log odds ratio equals the difference in utility levels in the origin and destination states (McFadden, 1974),

$$\begin{aligned}\log(P_{odt}/P_{oot}) = {} & \psi(D_{ot} - D_{dt}) + \alpha[\log(1-\tau_{dt}) - \log(1-\tau_{ot})] + \alpha \log(w_{dt}/w_{ot}) \\ & + (Z_d - Z_o) - C_{od},\end{aligned} \tag{C.2}$$

where P_{odt} is the probability that a household living in state o moves to state d, and P_{oot} is the probability that a household living in state o stays in state o.

As noted by Moretti and Wilson (2017), Equation (C.2) characterizes the supply of labor to state

d. Individuals with strong preferences for the origin state (high $e_{iot} - e_{idt}$) are unlikely to move in response to a change in income tax differentials between states o and d. However, individuals that are less attached to their home state (low $e_{iot} - e_{idt}$) may be induced to move if state o introduces an income tax and state d does not. Because this is a model of migration flows, and not population stocks, the model allows for long-run differences in migration across state pairs even in the absence of income tax differentials, due to differences in amenities and moving costs. We do not assume that the initial distribution of households across states is random. Rather, households were making optimal location choices prior to the start of our sample.

C.2 Econometric Model

The dyadic regression based on Equation (C.2) is

$$\log(P_{odt}/P_{oot}) = \psi(D_{ot} - D_{dt}) + \alpha[\log(1-\tau_{dt}) - \log(1-\tau_{ot})] + \gamma_{od} + \phi_{r(o,d),t} + u_{odt}, \tag{C.3}$$

where we have omitted the term with wages because it is endogenous. (Though we do control for state differentials in income per capita in a robustness check.) The fixed effect γ_{od} absorbs origin and destination amenities and state-pair moving costs. We add $\phi_{r(o,d),t}$ to capture common shocks to migration within pairs of regions, due to business cycles, transportation infrastructure, or technology.

To interpret the parameters, consider a pair of states, o and d, that initially both lack an income tax. If state o introduces the income tax at a rate of 1 percent, and state d does not introduce the income tax, then the outmigration rate (P_{odt}/P_{oot}) is expected to *increase* by about $100 \cdot \theta + \eta$ percent. If instead state d introduces the income tax at a rate of 1 percent and state o does not introduce the tax, then the outmigration rate is expected to *decrease* by about $100 \cdot \theta + \eta$ percent. Thus, the model assumes that increases and decreases in income tax differentials have symmetric effects. Now say that state o already has an income tax and raises the tax rate, such that the net-of-tax rate falls by 1 percent. Holding the tax policy of state d fixed, the outmigration rate is expected to fall by η percent. Thus, the outmigration response to a tax increase is larger by about $100 \cdot \theta$ when the initial tax rate was zero compared to when the initial tax rate was positive. If only the net-of-tax rate matters for location choices, then $\eta > 0$ and $\theta = 0$.

C.3 Response to Tax Rate Change

According to the model of location choice, the probability that individual i initially living in state o moves to state d in period t is

$$P^i_{odt} = \frac{\exp(-\theta D_{dt} + \eta \log(1-\tau_{dt}) + \gamma_{od})}{\sum_k \exp(-\theta D_{kt} + \eta \log(1-\tau_{kt}) + \gamma_{ok})},$$

where we have ignored the region-pair × year effects to simplify notation. Consider a small change to τ_{dt}, conditional state d already having an income tax. The individual-specific migration elasticity is given by

$$\varepsilon^i_{odt} \equiv \frac{\mathrm{dlog} P^i_{odt}}{\mathrm{dlog}(1-\tau_{dt})} = \eta \cdot (1 - P^i_{odt}).$$

Let I_{ot} denote the set of individuals initially living in state o in period t. Then the number of migrants to state d in period t is $\sum_{o \neq d} \sum_{i \in I_{ot}} P^i_{odt}$, and the inmigration elasticity for state d is

$$\begin{aligned}
\varepsilon_{dt} \equiv \frac{\mathrm{dlog}(\sum_{o\neq d}\sum_{i\in I_{ot}} P^i_{odt})}{\mathrm{dlog}(1-\tau_{dt})} &= \frac{\sum_{o\neq d}\sum_{i\in I_{ot}} \mathrm{d}P^i_{odt} / \mathrm{dlog}(1-\tau_{dt})}{\sum_{o\neq d}\sum_{i\in I_{ot}} P^i_{odt}} \\
&= \frac{\sum_{o\neq d}\sum_{i\in I_{ot}} \eta \cdot (1-P^i_{odt}) P^i_{odt}}{\sum_{o\neq d}\sum_{i\in I_{ot}} P^i_{odt}}
\end{aligned}$$

Let N_{ot} denote the number of individuals initially living in state o in period t. Then because the (ex-ante) migration probability does not depend on individual characteristics, we can write $P^i_{odt} = P_{odt}$ and define the number of migrants from o to d as $M_{odt} = N_{ot} P_{odt}$. The elasticity can therefore be written as $\varepsilon_{dt} = \eta \cdot (1 - \overline{P}_{dt})$, where $\overline{P}_{dt} = (\sum_{o\neq d} M_{odt} P_{odt}) / (\sum_{o\neq d} M_{odt})$ is the weighted average of migration probabilities.

Finally, define the overall inmigration elasticity ε to be the weighted average of ε_{dt}, weighting by the number of migrants $M_{dt} = \sum_{o\neq d} M_{odt}$. Then

$$\varepsilon \equiv \frac{\sum_t \sum_d M_{dt}\varepsilon_{dt}}{\sum_t \sum_d M_{dt}} = \eta \cdot (1 - \overline{P}), \tag{C.4}$$

where $\overline{P} = (\sum_t \sum_d \sum_{o\neq d} M_{odt} P_{odt}) / (\sum_t \sum_d \sum_{o\neq d} M_{odt})$. In our setting, $\overline{P}$ equals 0.016 using the child-based measure of migration and 0.007 using the census 5-year migration measure, so the inmigration elasticity is very close to η.

We can similarly define state o's outmigration elasticity with respect to the net-of-tax rate,

$$\xi_{ot} \equiv \frac{\mathrm{d}\log(\sum_{i \in I_{ot}} (1 - P^i_{oot}))}{\mathrm{d}\log(1 - \tau_{ot})}.$$

Calculations similar to those above yield $\xi_{ot} = -\eta \cdot P_{oot}$. Define the overall outmigration elasticity ξ to be the weighted average of ξ_{ot}, weighting by the number of people leaving state o, $L_{ot} = N_{ot}(1 - P_{oot})$. Then

$$\xi \equiv \frac{\sum_t \sum_o L_{ot} \xi_{ot}}{\sum_t \sum_o L_{ot}} = -\eta \cdot \tilde{P},$$

where $\tilde{P} = (\sum_t \sum_o L_{ot} P_{oot}) / (\sum_t \sum_o L_{ot})$ is the weighted average probability of staying, weighting by the number of people leaving the state. In our setting $\tilde{P}$ equals 0.852 using the child-based measure of migration and 0.907 using the census 5-year migration measure, so the outmigration elasticity is close to $-\eta$.

C.4 Response to Tax Introduction

Next we calculate the percentage change in migration due to the introduction of the income tax at initial rate τ. Define $V_{odt} \equiv -\theta D_{dt} + \eta \log(1 - \tau_{dt}) + \gamma_{od}$ and let $P_{odt}|_{\tau_{dt}=\tau}$ denote the individual migration probability (which does not vary across i) when the destination tax rate is τ. The inmigration response to the introduction of the income tax at rate τ is

$$\Delta^{0,\tau}_{odt} \equiv \frac{P_{odt}|_{\tau_{dt}=\tau} - P_{odt}|_{\tau_{dt}=0}}{P_{odt}|_{\tau_{dt}=0}} = A \cdot \exp(-\theta + \eta \log(1 - \tau)) - 1,$$

where

$$A = \frac{1 + \sum_{k \neq d} \frac{\exp(V_{okt})}{\exp(\gamma_{od})}}{\exp(-\theta + \eta \log(1 - \tau)) + \sum_{k \neq d} \frac{\exp(V_{okt})}{\exp(\gamma_{od})}}.$$

Note that $A > 1$ but $A \approx 1$ because $\sum_{k \neq d} \frac{\exp(V_{okt})}{\exp(\gamma_{od})} = 1/P_{odt}|_{\tau_{dt}=0} - 1$ is large due to the fact that $P_{odt}|_{\tau_{dt}=0}$ is very small.[C.1] We thus use the approximation

$$\Delta^{0,\tau}_{odt} \approx \exp(-\theta + \eta \log(1 - \tau)) - 1 \equiv \Delta^{0,\tau}. \tag{C.5}$$

[C.1] $\sum_{k \neq d} \frac{\exp(V_{okt})}{\exp(\gamma_{od})} = 61.5$ when we plug in the average moving probability based on the child-based measure.

This approximation slightly overstates the negative effect of the introduction of the income tax on inmigration in the same way that η slightly overstates the inmigration response to a small change in the tax rate.

Because the approximation to $\Delta^{0,\tau}_{odt}$ does not vary across origins or destinations, the percentage change in the number of migrants M_{dt}, as well as the weighted average of inmigration responses across destinations, are also approximated by $\Delta^{0,\tau}$.

The outmigration response to the introduction of the income tax at rate τ is

$$\Omega^{0,\tau}_{ot} \equiv \frac{(1 - P_{oot}|_{\tau_{ot}=\tau}) - (1 - P_{oot}|_{\tau_{ot}=0})}{1 - P_{oot}|_{\tau_{ot}=0}} = \frac{1 - \exp(-\theta + \eta \log(1-\tau))}{\exp(-\theta + \eta \log(1-\tau)) + \sum_{d \neq o} \frac{\exp(V_{od})}{\exp(\gamma_{oo})}}.$$

Note that $\sum_{d \neq o} \frac{\exp(V_{od})}{\exp(\gamma_{oo})} = 1/P_{oot}|_{\tau_{ot}=0} - 1$, which is close to zero because $P_{oot}|_{\tau_{ot}=0}$ is close to one. We can therefore use the approximation[C.2]

$$\Omega^{0,\tau}_{ot} \approx \exp(\theta - \eta \log(1-\tau)) - 1 \equiv \Omega^{0,\tau}. \tag{C.6}$$

[C.2] The calculated effects are very similar if, instead of using an approximation, we plug in values between 0.8 and 1 for $P_{oot}|_{\tau_{ot}=0}$.

Appendix D References

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