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ABSTRACT

We provide new evidence about the fiscal and mobility consequences of the introduction of the income tax, a major investment in modern state capacity. Drawing on archival data, we introduce a novel panel database that spans all 50 U.S. states between 1900 and 2008. Our research design exploits the staggered introduction of the income tax across states, while accounting for the potentially selective timing of adoption. We find that tax broadening significantly increased total revenue and expenditure in the short run but not the long run, while revenue and expenditure per capita significantly increased on a permanent basis. To explain these results, we show that the introduction of the income tax led to significant outmigration to non-income-tax states, particularly by high earners. Our findings demonstrate that the introduction of the income tax allowed U.S. states to significantly increase their revenue-raising capacity on a per capita basis. Nonetheless, population mobility provided a partial check on the absolute size of state governments.

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1 Introduction

The state's capacity to extract revenue matters for economic development (Besley and Persson, 2011). Well-funded states can provide the administrative infrastructure that supports a well-functioning market economy: secure property rights, market regulations, and quick and fair legal resolution (North, 1990). History suggests that broadening the tax base is key to expanding the government's extractive capacity. The elimination of traditional tax privileges following the French Revolution, for example, was associated with 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 crucial component of tax broadening (Wallis, 2000, Lindert, 2004, Aidt and Jensen, 2009, Besley and Persson, 2013).

Despite its historical importance, there is a dearth of systematic evidence about how the introduction of the income tax affects the size of government. Broadening the tax base mechanically increases government revenue, holding other tax rates and bases fixed. However, introducing a new tax may crowd out other revenue sources or cause taxpayers to move to lower-tax jurisdictions, leaving the absolute size of government unchanged. If fleeing taxpayers are disproportionately high-earning, tax broadening may even reduce revenue per capita. Therefore, the extent to which introducing a new tax instrument actually expands the government's extractive capacity remains an important open question.

To make progress on this front, this paper analyzes the consequences of the introduction of the income tax – a major investment in modern state capacity – on revenue, expenditure, population, and interstate migration across U.S. states over the entire twentieth century and the start of the twenty-first century. To perform this analysis, we introduce a new panel database, drawn in part from archival data. Crucially, individual states adopted the income tax in a staggered fashion over a span of 65 years, allowing us to control for unobserved state characteristics that influence the levels and long-run trends of our outcomes. Using a semi-parametric difference-in-differences design, we find that tax broadening initially increased total revenue and expenditure by 4-8 percent, but had no long-term impact on the absolute size of state governments. By contrast, revenue and expenditure per capita permanently increased by 7-8 percent. Tax-base erosion due to outmigration helps explain these results: state population fell by 4 percent in the long run, as taxpayers fled to non-income-tax states. High-income households exhibited the strongest outmigration responses.

The results show that adopting the income tax allowed U.S. states to significantly increase their revenue-raising capacity on a per capita basis. The income tax thus emerges as a key tool in expanding the extractive role of the state (Lindert, 2004, Besley and Persson, 2013). Nonetheless, population mobility provided a partial check on the size of government, at least in absolute terms. Our results thus indicate that the return on fiscal-capacity investments depends crucially on the elasticity of the tax base.

Our empirical setting offers many advantages. First, our long panel – spanning 1900 to 2008 – allows us to trace out the long-term effects of tax broadening. Doing so is important, because short-run and long-run effects can differ substantially. Citizens incur adjustment costs when moving from one state to another. Accordingly, population responses to tax policy unfold over many years. Because state population determines, in large part, the tax base, fiscal outcomes also adjust over long periods of time. A short panel would miss the rich dynamics we observe.

Second, the shared language and culture of U.S. states support a geographically mobile workforce. The location decisions of U.S. workers are sensitive to local economic shocks, and overall mobility is high (Bartik, 1991). The United States therefore provides an ideal setting to test whether and how tax broadening influences location decisions.

Finally, our setting allows us to eliminate the influence of confounding variables that would hinder a cross-country analysis. Expansions in fiscal capacity at the national level often occurred during wartime (Tilly, 1992, Scheve and Stasavage, 2012, Gennaioli and Voth, 2015), when the value of common-interest public goods was high (Besley and Persson, 2009). Given a national system of military defense, the external threat environment is held constant across U.S. states. Furthermore, differences in state-level institutions and cultures are small compared to such differences across countries.

A standard difference-in-differences design that exploits the staggered adoption of the state-level income tax thus eliminates many potential sources of bias. Such a design relies on the assumption that outcomes in adopting states and non-adopting states would have followed parallel trends, on average, in the absence of tax broadening. Clearly, this assumption would be violated if the timing of adoption were correlated with other factors that influence the outcomes. Penniman (1980, ch. 1) suggests that the exact timing of the introduction of the state income tax was often a function of idiosyncratic political factors, and uncorrelated with other policy changes. 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 enacted the tax at that time; the Ohio legislature failed to adopt the income tax until 1971.

While idiosyncratic political factors often influenced the timing of adoption, some states may have adopted the income tax in the face of adverse demographic trends or budgetary stress, potentially violating the assumption of parallel trends. To guard against this possibility, we use a semiparametric difference-in-differences design that conditions on several lags of population and fiscal outcomes. Our method imposes a weaker version of the parallel trends assumption: adopting and non-adopting states with similar recent population and fiscal dynamics would have followed parallel trends in the absence of tax broadening. Even after accounting for selection into tax broadening based on recent shocks, adopting states may have had a greater latent demand for spending, at the time of adoption, than non-adopting states. Thus, the potential impact of tax broadening may be greater for adopting states than non-adopting states. Our semiparametric estimator is robust to treatment-effect heterogeneity and recovers the average effect of tax broadening for adopting states.

After reweighting by each state's propensity to adopt the income tax in a given year, adopting and non-adopting states exhibited similar demographic and fiscal trends prior to adoption, lending credence to our approach. In addition, we find that recent negative changes in population, and not long-run shifts, predict selection into tax broadening, further supporting the validity of our identification strategy. We also show that the introduction of the sales tax does not confound our estimates, and that our results are not driven by economic shocks or regional shocks to mobility. Finally, our results are not solely driven by the introduction of tax withholding by individual states starting in 1948, though we show that this instrument plays a complementary role in tax broadening.

A rapidly growing literature examines both the determinants (Besley and Persson, 2013, Casaburi and Troiano, 2015, Kleven et al., 2016, Gillitzer, 2017, Jensen, 2019) and economic and policy consequences of state capacity (Gordon and Li, 2009, Dincecco and Prado, 2012, Acemoglu et al., 2015). Historical accounts indicate that the development of the state's extractive capacity was a hard-fought process (Dincecco, 2011, O'Brien, 2011, Hoffman, 2015). Our study sheds new light on a key mechanism – the introduction of the income tax – through which governments increase their capacity to extract revenue. To the best of our knowledge, ours is the first paper to empirically 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 do population mobility and crowd-out limit the effects of fiscal-capacity investments?

In addition, our paper contributes to the literature on mobility responses to tax policy, summarized by Kleven et al. (2019). Recent studies, mostly focused on top earners, show how taxpayers migrate across countries (Kleven et al., 2013, 2014, Akcigit et al., 2016) as well as within countries (Bakija and Slemrod, 2004, Liebig et al., 2007, Moretti and Wilson, 2017, Schmidheiny and Slotwinski, 2018, Agrawal and Foremny, 2019) in response to a change in tax rates, an intensive-margin reform. However, no such study examines the historical introduction of the income tax, an extensive-margin reform.[1] This distinction is crucial, because reducing the net-of-tax rate by 1 percent starting from a zero tax rate may have a larger effect on outmigration than an equivalent reduction in the net-of-tax rate starting from a positive tax rate. The reason is that introducing a new tax not only reduces after-tax income – it also creates a new administrative burden, and may thus be more salient to taxpayers than an increase in the rate of an existing tax. Our results provide evidence for this channel, showing a greater mobility response to tax changes starting from a rate of zero.[2]

The paper proceeds as follows. Section 2 analyzes the fiscal consequences of tax broadening. Section 3 then evaluates whether interstate mobility is an important mechanism behind the fiscal results. Section 4 provides concluding remarks.

2 Fiscal Consequences of Tax Broadening

This section examines the fiscal consequences of the introduction of the state-level income tax, both in the short run and the long run. We first motivate the empirical analysis with a simple model of the government's budget. We next estimate the dynamic effects of tax broadening on fiscal outcomes.

[1] Moretti and Wilson (2019) analyze the mobility responses of billionaires to an extensive-margin change in estate tax liabilities at the U.S. state level caused by the elimination of a federal credit.

[2] A theoretical literature shows how the threat of migration alters optimal tax formulas (e.g., Wilson, 1980, Mirrlees, 1982, Lehmann et al., 2014). This literature assumes the existence of an income tax.

2.1 Simple Model of the Government's Budget

Suppose the government has access to two taxes with exogenous rates τ_1 and τ_2 and bases B_1 and B_2. For example, the first tax could be a sales or property tax, and the second tax could be a flat income tax. We write $B_k = B_k(\tau_1, \tau_2)$ for $k \in \{1, 2\}$ to express the fact that each tax potentially affects both bases. Assume that each tax base is weakly decreasing in both tax rates. Total revenue is $R(\tau_1, \tau_2) = \tau_1 B_1(\tau_1, \tau_2) + \tau_2 B_2(\tau_1, \tau_2)$.

Suppose the government initially collects revenue using only the first tax. The fiscal impact of introducing the second tax is

$$R(\tau_1, \tau_2) - R(\tau_1, 0) = \tau_1 [B_1(\tau_1, \tau_2) - B_1(\tau_1, 0)] + \tau_2 B_2(\tau_1, \tau_2). \tag{1}$$

The first term on the right-hand side of Equation (1) quantifies the "crowd-out" effect of the new tax due to a reduction in the base of the first tax.[3] For example, introducing an income tax may reduce sales tax revenue through its effect on consumption. Crowd-out would also occur if taxpayers leave the state in response to the income tax. The second term on the right-hand side of Equation (1) represents the revenue from the new tax. For given tax rates, the fiscal impact of the new tax depends on the degree of crowd-out and the size of the new tax base, B_2.

The fiscal impact of the new tax may vary over time, because tax bases may respond gradually due to taxpayer learning and adjustment costs. For example, it may take several years for taxpayers to learn how to effectively evade or avoid the new tax. In addition, labor supply and location decisions may not respond immediately due to hours constraints and moving costs. If taxpayers are sufficiently slow to respond, the short-run fiscal impact will approximately equal the revenue from the new tax, $\tau_2 B_2(\tau_1, \tau_2)$, because $B_1(\tau_1, \tau_2) \approx B_1(\tau_1, 0)$ in the short run. Over time, $B_1(\tau_1, \tau_2)$ and $B_2(\tau_1, \tau_2)$ may fall as taxpayers adjust their behavior, resulting in a long-run effect that is smaller than the short-run effect and may even be negative. On the other hand, if tax enforcement is stricter than anticipated, then taxpayer learning could result in the tax bases falling *less* in the long run than in the short run.

[3] A second type of crowd-out effect occurs if the government reduces τ_1 in response to the new tax. We assume tax rates are fixed in order to keep the exposition simple, but we test for this type of crowd-out ahead.

In per capita terms, the fiscal impact of the new tax is

$$\frac{R(\tau_1,\tau_2)}{P(\tau_1,\tau_2)} - \frac{R(\tau_1,0)}{P(\tau_1,0)} = \tau_1 \left[\frac{B_1(\tau_1,\tau_2)}{P(\tau_1,\tau_2)} - \frac{B_1(\tau_1,0)}{P(\tau_1,0)} \right] + \tau_2 \frac{B_2(\tau_1,\tau_2)}{P(\tau_1,\tau_2)}, \tag{2}$$

where $P(\tau_1, \tau_2)$ is state population as a function of tax rates. If all taxpayers are identical and the only margin of adjustment is location choice, then the tax bases will be proportional to population for any tax policy: $B_k(\tau_1, \tau_2) = \alpha_k P(\tau_1, \tau_2)$ for $k \in \{1, 2\}$. In this case, the per capita fiscal impact will equal the per capita revenue from the new tax, regardless of the degree of outmigration.

On the other hand, if taxpayers are heterogeneous or respond along dimensions other than location choice, then the per capita fiscal impact can be smaller than the per capita revenue from the new tax. For example, if taxpayers vary by income, and high-income individuals are more likely to leave the state in response to the new tax, then tax broadening will cause the tax bases to fall in per capita terms. If B_1/P and B_2/P fall enough in response to the new tax, per capita revenue can even decline. Similarly, if taxpayers engage in increased levels of tax evasion and avoidance in response to the new tax, then the tax bases will fall in per capita terms and the per capita fiscal impact can be negative.

This simple framework underscores how tax broadening can have different effects over different time horizons, and how the responses of revenue and revenue per capita can differ. It also provides two specific predictions that we can test given data availability: (1) if tax broadening causes net outmigration, revenue will increase by more in the short run than in the long run; and (2) if the outmigration response is sufficiently dominated by high-earners, revenue per capita will not increase in the long run. In the remainder of this section, we describe our fiscal data and estimate the fiscal impacts of the introduction of the income tax. The following section then evaluates how the income tax influences the location choices of taxpayers in order to see whether outmigration can help explain the observed fiscal responses.

2.2 Fiscal Data

2.2.1 Income Tax Introduction

Following Wallis (2000), we define the income tax to include individual or corporate income taxes. For more than 60 percent of adopting states, this distinction is immaterial, because individual and corporate income taxes were introduced in the same year. And, for 75 percent of adopting states, the individual and corporate income taxes were introduced within three years of each other. We define the baseline treatment using the year that the individual income tax was introduced. However, our results are similar when we instead use the year the corporate tax was introduced. (Results available upon request.)

In principle, the introduction of the corporate income tax could indirectly affect population mobility through its effect on firm mobility. Given that the income tax and corporate tax are simultaneously introduced in most cases, we cannot cleanly test this channel. We do, however, check the robustness of our results to alternative definitions of tax broadening. As noted above, defining treatment as the introduction of the corporate income tax, or as the introduction of either the individual or corporate income tax, produces results that are very similar to the baseline results.[4]

Appendix Table A.1 describes the introduction of the income tax by states over time according to Penniman (1980). The first state to introduce the individual income tax was Wisconsin in 1911. Seven more states introduced individual income tax laws over the 1910s, followed by five states over the 1920s, and eighteen states over the 1930s. No states introduced the individual income tax over the 1940s, while two states introduced it over the 1950s, eight states over the 1960s, and four states over the 1970s. Six states never introduced an individual income tax.

Figure 1 displays U.S. states shaded according to the year the state adopted the individual income tax, with darker shades indicating later years. The early adopting states, which introduced the tax in the 1910s or 1920s, are scattered throughout the Northeast, South, and Midwest. States that adopted the tax in the 1930s are found in every region of the United States, though they are more prevalent west of the Mississippi River. Many late adopters are located in the Rust Belt, though several late adoptions also occurred in the Northeast,

[4]Penniman (1980) defines two types of corporate income tax: the net income tax and the excise or franchise tax. We always date the corporate income tax that is introduced first, regardless of type.

Alaska, and Hawaii.

Once states introduce the individual income tax, they typically retain it. There are only two cases where states have repealed or fundamentally changed the income tax. Alaska had an income tax when it became a state in 1959, but repealed it in 1980. Connecticut introduced a progressive income tax in 1991; from 1969 to 1990, the state only taxed capital gains and dividends. New Hampshire and Tennessee have individual income taxes that only tax interest and dividends. We define these taxes as individual income taxes. However, coding these two states, and Connecticut from 1969 to 1990, as not having individual income taxes does not change the main results. Once introduced, the corporate income tax is also generally stable. Only two states have repealed or fundamentally changed this tax. Michigan introduced a corporate income tax in 1967, but repealed it in 1975; this tax was re-introduced in 2011. Similarly, Ohio introduced a corporate income tax in 1971, but repealed it in 2005.

In robustness checks, we also use data on the presence of a state sales tax from Gillitzer (2017) and Fox (2004), and the presence of state-level tax withholding from Penniman (1980, pp. 154-5) and the Advisory Commission on Intergovernmental Relations (1977, pp. 206-7).

2.2.2 Income Tax Rates and Bases

Data on state income tax rates and bases come from the University of Michigan's World Tax Database (Office of Tax Policy Research, 2003). This database provides the marginal tax rate for the top and bottom income tax brackets in the years 1941-2003. We extend these data to 2010 using tax rate information published by the Tax Policy Center (Tax Policy Center, 2019). We impute pre-1941 rates using the rate in 1941. Tax rates are always set to zero in years prior to the state's introduction of the income tax. State income tax rates tend to be persistent over time, potentially lessening the extent to which imputation introduces measurement error. Classical measurement error would lead to underestimates of tax elasticities, though non-classical measurement error would introduce a bias of unknown sign. In the Appendix, we show that the results are robust to alternative imputation schemes, including no imputation. Consistent with the classical measurement error story, the elasticity estimate under no imputation is larger than the baseline estimate. We discuss these results ahead.

We use the personal exemption, an amount deducted from gross income in computing taxable income, as a proxy for the breadth of the tax base. The World Tax Database provides the personal exemption for married couples and single filers for the years 1941-2002. We extend these data to 2010 using data from the Tax Policy Center. 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. Four states (Arizona, Iowa, Minnesota, and Wisconsin) introduced the income tax with a personal exemption in the form of a tax credit, which is an amount deducted from the tax liability (Office of Tax Policy Research, 2003). We define the personal exemption for these four states as the tax credit divided by the bottom marginal income tax rate, which is the exemption threshold that would produce the same tax savings as the tax credit for an individual in the bottom tax bracket. We obtain similar results when we redefine the personal exemption for these states using the top marginal tax rate and the average of the top and bottom marginal tax rates. (Results available upon request). For states that introduced the income tax prior to 1941, we impute the initial personal exemption using the nominal personal exemption in 1941. We then convert this value into constant 2008 USD using the value of the price index in the year of adoption.

Appendix Table A.3 displays the summary statistics for our variables. The average bottom marginal tax rate was 1.08 percent, with a minimum of 0 and a maximum of 6.35 percent. The top marginal tax rate averaged 3.51 percent, ranging from 0 to 19.80 percent. The average personal exemption for a single filer was about 6,900 USD, with a minimum of 0 USD and a maximum of over 41,000 USD.

2.2.3 Population and Fiscal Outcomes

We use census data on state-level population, revenue, and expenditure that span the entire twentieth century and the start of the twenty-first century. State population is available on an annual basis over the period 1900-2008. State-level fiscal data are available online every two years from 1942 to 1948 and annually from 1950 to 2008 from the Census of Governments (US Department of Commerce, 2010). These data employ consistent accounting definitions and revenue and expenditure categories across years. While fiscal data are available from other sources for the years after 2008, these data do not use the same accounting definitions as the Census of Governments data. Thus, adding more years of data introduces accounting complications. In addition, we estimate fiscal impacts up to 30 years after the adoption of the income tax. Because the last adoption occurred in 1976, we do not need data after 2006 to estimate the effects of interest.

For years prior to 1942, we handcoded state-level fiscal data from archival census reports.

The first census fiscal data are for 1902 (US Department of Commerce, 1907). The 1902 census was the first to attempt to collect complete fiscal information (Wallis, 2000). Prior to this census, the Bureau of the Census did not collect data on revenues (as opposed to taxes) or expenditures. Follow-up census data are available for 1903 and 1913 (US Department of Commerce, 1915). 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, 1923, 1926-1932, 1937, 1938, and 1940 (US Department of Commerce, 1924-1942). Appendix Table A.2 provides detailed information on fiscal data sources. To the best of our knowledge, our dataset includes all available state-level fiscal data prior to 1942. The result is an unbalanced panel of fiscal data that covers all 50 states between 1902 and 2008.

Because the Census of Governments and the archival census reports use slightly different accounting definitions, revenue and expenditure figures from the two sources differ in years in which both sources are available, such as 1942. In particular, the revenue and expenditure figures are larger in the Census of Governments data. The change in the data source thus causes a larger-than-expected increase in revenue and expenditure from 1940 to 1942 for all states. We expect this common shock to add noise to our outcomes, but we see no reason why it would cause bias in our estimates. As a robustness check, we replicate our main estimates controlling for a full set of year effects, which nets out additive effects of the change in data source. (Results available upon request.)

We focus on the main public finance outcomes in the census data, which we select according to two criteria. First, to investigate the total effect of our treatment on state finances, we use total revenue and expenditure. Second, we include individual fiscal outcomes that we can match across different censuses, which do not always follow the same accounting procedures. On the revenue side, we focus on revenue from income tax and property taxation, total tax revenue, and total revenue. On the expenditure side, we focus on total expenditure and expenditure on education, health, and public safety. All fiscal variables are measured in constant 2008 USD. We do not examine state debt or deficits, because balanced-budget requirements significantly limited the ability of states to borrow to fund non-capital expenditures.

As shown in Table A.3, the average state population over the sample period is 3.5 million, and total revenue and expenditure averaged 14 million USD and 13 million USD, respectively. State budgets were around 3,000 USD per capita on average.

2.3 Empirical Strategy for Fiscal Outcomes

2.3.1 Case Studies

Two case studies help motivate our empirical strategy. By the beginning of the twentieth century, Americans increasingly saw the property tax as capricious and unfair, falling on those with visible property, such as farmers, while exempting those who held their wealth in intangible personal property. The Progressive Movement, which had grown strong in many states by this time, supported a shift towards taxation based on one's ability to pay – i.e., an income tax (Mehrotra, 2013, ch. 4).

Penniman (1980, ch. 1) describes the early attempts of two states, Wisconsin and Ohio, to realize this Progressive goal. Voters in both states approved constitutional amendments allowing for a state income tax – Wisconsin in 1908 and Ohio in 1912. Both referenda passed with large majorities. However, while Wisconsin legislators established an income tax in 1911, opponents of the income tax in Ohio blocked its passage in the legislatures following the referendum. It was not until 1971 that Ohio, facing budgetary problems, adopted an income tax with bipartisan support (Penniman, 1980, ch. 1).

The case studies illustrate two points. First, idiosyncratic political factors influenced the timing of the adoption of the state income tax. Tax broadening was a contingent outcome. Given the similar voter preferences in Ohio and Wisconsin, a small change in political dynamics may have led Ohio to adopt the income tax in 1915 instead of 1971.[5] Outcomes in Ohio therefore represent a high-quality counterfactual for outcomes in Wisconsin in the early 1900s. Second, budgetary stress can also motivate a state to adopt an income tax. We therefore use econometric methods that exploit idiosyncratic variation in the timing of adoptions, while also allowing for non-random selection into tax broadening.

2.3.2 Semiparametric Difference-in-Differences

In this section we present a semiparametric difference-in-differences approach for estimating the fiscal impacts of tax broadening. A standard difference-in-differences design faces two potential challenges in our setting.

[5]Or vice versa: in Wisconsin, opponents of the income tax attempted to have the law overturned by the Wisconsin Supreme Court, to no avail (Mehrotra, 2013, ch. 4).

One potential challenge is that the introduction of the income tax may be correlated with other changes to state policies that affect fiscal outcomes, biasing our estimates. For instance, the introduction of the sales tax – the other major form of tax broadening over the sample period – may coincide with the introduction of the income tax. However, we will show ahead that the correlation between the adoption years of the two taxes is zero, and our results are unchanged when controlling for the introduction of the sales tax.[6]

The second challenge is that the timing of the adoption of the income tax may be correlated with past fiscal and demographic trends which naturally predict future trends. For example, a state may decide to introduce the income tax in the face of greater budgetary stress, perhaps due to a recent slowdown in population growth. The standard identifying assumption – that adopting and non-adopting states would have followed parallel trends in the absence of adoption – may therefore be implausible. We overcome this problem by using propensity-score weighting in order to relax the strong assumptions required for standard difference-in-differences estimators (Abadie, 2005). We adapt the semiparametric approach of Angrist and Kuersteiner (2011) to a panel context, following Acemoglu et al. (2019) and Suárez Serrato and Wingender (2016). Our approach models the process of selection into "treatment," i.e., tax broadening, but leaves the model of the outcome process unspecified, allowing for a rich analysis of dynamics.

Using the notation of Acemoglu et al. (2019), let $Y^s_{it}(d)$ denote the potential fiscal outcome in period $t+s$ for state i whose tax status in year 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_{it} equal one if state i has an income tax in year t, and zero otherwise. The potential change in the outcome from period $t-1$ to period $t+s$ for a state with no income tax in period $t-1$ ($D_{it-1}=0$) and tax status d in period t ($D_{it}=d$) is $\Delta Y^s_{it}(d) = Y^s_{it}(d) - Y_{it-1}$, where Y_{it} is the actual fiscal outcome in state i and year t. We take the log of all fiscal variables.

The average causal effect of introducing the income tax in period t on the fiscal outcome s periods later for states that introduce the tax is

$$\beta^s = \mathrm{E}(\Delta Y^s_{it}(1) - \Delta Y^s_{it}(0) \mid D_{it}=1, D_{it-1}=0).$$

The causal effect β^s corresponds to an "average treatment effect for the treated," because

[6]The introduction of the income tax also has no impact on sales tax rates. (Results available upon request.)

it applies to states that introduced the income tax at some point. Estimating β^s for several values of s allows us to trace out the impact of tax broadening over time without making parametric assumptions about dynamics. The econometric challenge is that states that introduce the income tax in a particular year may have different potential outcome growth than states that do not introduce the tax at that time. This may be because states select into tax broadening based on demographic or fiscal trends. Let Z_{it} denote the (log) population of state i in year t. We state the key identifying assumption below.

Assumption 1. $\mathrm{E}(\Delta Y^s_{it}(0) \mid D_{it} = 1, D_{it-1} = 0, X_{it}) = \mathrm{E}(\Delta Y^s_{it}(0) \mid D_{it} = 0, D_{it-1} = 0, X_{it})$, *where* $X_{it} = (\Delta Y_{it-1}, \ldots, \Delta Y_{it-J}, Z_{it-1}, \ldots, Z_{it-K}, t)$, *for* $s \geq 0$.

Assumption 1 states that, conditional on recent fiscal shocks, past population dynamics, and year effects, the fiscal outcomes of states that introduced the tax in year t and states that did not introduce the tax at that time would have followed parallel paths on average in the absence of tax broadening. This assumption is weaker than the standard assumption in difference-in-differences designs, because it only imposes parallel trends for states with the same recent fiscal and population dynamics.

The second assumption needed to identify β^s is stated below.

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

This assumption has two parts. First, some fraction of the states eventually adopt the income tax. Second, for every value of the covariates X_{it}, some states do not transition into having an income tax.[7]

Denote the realized changes in the outcome by $\Delta Y^s_{it} = Y_{it+s} - Y_{it-1}$. Following Abadie (2005), Assumptions 1 and 2 imply that β^s can be identified using an inverse probability weighting (IPW) scheme,

$$\beta^s = \mathrm{E}(\omega_{it} \Delta Y^s_{it} \mid D_{it-1} = 0),$$

where the weighting function is

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

[7] Note that Assumption 2 is weaker than the overlap assumption required to identify the average treatment effect, which holds that $0 < \mathrm{P}(D_{it} = 1 \mid D_{it-1} = 0, X_{it}) < 1$ for every X_{it}.

Intuitively, the estimand assigns greater weight to "control" states ($D_{it} = 0$) that did not adopt the income tax in year t but whose recent dynamics were similar to states that did adopt the tax in that year (high $\mathrm{P}(D_{it} = 1 \mid D_{it-1} = 0, X_{it})$).

In order to estimate β^s, we specify a probit model for the propensity score,

$$\mathrm{P}(D_{it} = 1 \mid D_{it-1} = 0, X_{it}) = \Phi\left(\lambda_t + \sum_{j=1}^{J} \delta_j \Delta Y_{it-j} + \sum_{k=1}^{K} \pi_k Z_{it-k}\right), \tag{3}$$

where $\Phi(\cdot)$ is the standard normal distribution function. Following Acemoglu et al. (2019), we rearrange the terms in Equation (3) to express the propensity score in terms of $\{\Delta Z_{it-k}\}_{k=1}^{K-1}$ and Z_{it-K}. While this rearrangement does not change the predicted probabilities, it allows us to separately estimate the effects of temporary and permanent changes in population on the probability of tax broadening. In this formulation of the model, the partial effect of Z_{it-K} is equivalent to the marginal effect of a permanent increase in Z. Assumption 1 allows selection into tax broadening based on temporary changes in population but not permanent shifts. Thus if the assumption holds, Z_{it-K} should have an insignificant effect in the probit model. We test this implication ahead.

Let $\widehat{\mathrm{P}}(X_{it})$ denote the fitted values from estimating Equation (3), and let $\widehat{\mathrm{E}}(\,\cdot \mid D_{it-1} = 0)$ denote the sample average over state-years for which $D_{it-1} = 0$. The semiparametric difference-in-differences estimator is given by

$$\hat{\beta}^s = \widehat{\mathrm{E}}(\widehat{\omega}_{it} \Delta Y_{it}^s \mid D_{it-1} = 0), \tag{4}$$

with the estimated weights

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

Fiscal data are not available on a consistent annual basis in the period prior to 1950, when most adoptions of the income tax occurred. This feature of the data creates two problems. First, it is not possible to construct a consistent set of fiscal lags over much of the sample period. If selection into tax broadening is modeled as a function of yearly fiscal lags, most of the observations prior to 1950 will be dropped. Second, the estimates $\hat{\beta}^s$ given by Equation

(4) will be based on different subsamples of states for different values of s. As a result, variation in $\hat{\beta}^s$ across values of s will reflect not only the treatment-effect dynamics for a given set of states, but also the changing composition of states used for estimation.

We overcome the problems caused by missing data in two ways. First, we redefine the lagged and forward differences of fiscal variables in terms of two-year periods in order to increase the number of observations that are available for our estimator. The base period is one to zero years prior to the introduction of the income tax.[8] Thus, the differenced outcomes take the form, $\overline{Y}_{[s_0,s_1]} - \overline{Y}_{[-1,0]}$, where $\overline{Y}_{[s_0,s_1]}$ is the average outcome s_0 to s_1 periods after the introduction. Missing values are ignored in the calculation of the two-period averages. Second, we only use observations for which the first two lagged first differences of the outcome are non-missing. Doing so greatly reduces the problem of the composition of states changing as we estimate treatment effects over different time horizons.[9]

In practice, we model selection into tax broadening as a function of five lags of population, two lagged differences of the fiscal outcome, and year effects.[10]

2.4 Results for Fiscal Outcomes

We begin by estimating the fiscal consequences of the income tax. Tax broadening need not mechanically increase the size of government, for two reasons. First, the income tax may crowd out other revenue sources, such as property taxes. Second, migration and other taxpayer responses may completely offset the increase in the tax base, leaving revenue unchanged in the long term. Even revenue per capita may not increase, if enough high-earning taxpayers flee the state. In light of these possibilities, we estimate the effect of tax broadening on the size of government in absolute as well as per capita terms. We also test for crowd-out effects on property taxes.

[8]The income tax is implemented starting in the year after the introduction, which is the year the legislation was passed.

[9]The fiscal estimates are thus based on all adoptions that occurred from 1930 onward, with the exception of the 1939 adoption in Washington, D.C. The estimates for population and mobility in Section 3 will make use of all adoptions.

[10]The two lagged differences of the fiscal outcome correspond to the periods of 4-5 years prior to adoption and 2-3 years prior to adoption. Thus we condition on both fiscal and demographic dynamics in the five years leading up to adoption.

2.4.1 Main Results

Figure 2 depicts the total revenue and expenditure responses to tax broadening using the semiparametric difference-in-differences estimator.[11] Revenue increases by 8 percent, and expenditure increases by 4 percent, in absolute terms in the years immediately following tax broadening. This effect is statistically significant at the 1 percent level for revenue and insignificant for expenditure. The effect starts to die out five years after adoption, so that revenue and expenditure return to their initial levels around 8 years after adoption. The absolute size of state governments remain unchanged 30 years after tax broadening.

By contrast, revenue per capita immediately increases by 9 percent, while expenditure per capita increases by 6 percent, and these increases are persistent over time. Tax broadening increases the size of government on a per capita basis by about 7 percent after 30 years. The long-run increase is significant at the 10 percent and 5 percent levels for revenue per capita and expenditure per capita, respectively. The graphs show no evidence of differential fiscal trends prior to the adoption of the income tax.

Table 1 summarizes the effect of tax broadening on total revenue, total taxes, and property taxes over time. The results confirm that total revenue and total taxes significantly increase in the short run, while experiencing no change in the long run. The estimates for property tax revenue are imprecise, but they do not indicate any crowd-out effect.

Table 2 presents the corresponding results for revenue per capita. Revenue per capita and tax revenue per capita increase by 9 percent and 13 percent, respectively, in the short-run, and these responses are significant at the 1 percent level. Tax broadening increases revenue per capita by around 7 percent, and taxes per capita by around 19 percent, after 30 years. Tax broadening does not have a statistically significant effect on property taxes per capita.

Table 3 summarizes the total expenditure responses over time. Total expenditure initially increases by about 4 percent, which is statistically insignificant. Tax broadening has no impact on total expenditure 10, 20, or 30 years after adoption. Education expenditure initially increases by 7 percent before subsequently declining. The results for health and public safety expenditure are too imprecise to be conclusive. Health and safety budgets each are about one-tenth the size of the education budget on average.

[11] The standard difference-in-differences estimator that controls for year effects produces very similar results. (Available upon request.)

Table 4 reports the expenditure responses in per capita terms. Expenditure per capita increases by 6 percent in the first six years and 8 percent 30 years later. Only the long-run effect is significant at the 5 percent level. Education expenditure per capita initially increases by nearly 9 percent, and the effect is significant at the 1 percent level. The increase in education expenditure per capita after 30 years is 4 percent, and this long-run effect is statistically insignificant. The estimated effects on health and safety expenditure per capita are too imprecise to be conclusive.

The fiscal results establish that the introduction of the income tax caused the absolute size of government to increase in the short run but not the long run, while the size of government increased in per capita terms in both the short run and the long run. The revenue and expenditure responses track each other closely over time, which is unsurprising due to the presence of balanced-budget requirements.

2.4.2 Robustness

We now examine several threats to the validity of the baseline fiscal results. Even after controlling for recent fiscal and demographic dynamics, our estimates would be biased if the adoption of the income tax were correlated with other policy changes at the state level, or if economic shocks influenced both the timing of adoption and the fiscal outcomes. We consider both of these threats in turn.

Introduction of Sales Tax

One leading potential confounder is the introduction of the sales tax – the other significant form of state-level tax broadening that occurred during the sample period. Panel (a) of Appendix Figure A.2 plots the introduction year of the sales tax against the introduction year of the income tax for the 40 states that introduced both taxes during the sample period. The correlation between introduction years is essentially zero, suggesting that the sales tax is not a confounder. Nonetheless, we examine whether our results change when we control for whether the sales tax was adopted within the current time window. We add to the propensity score an indicator variable equal to 1 if the sales tax was introduced in the past 10 years or will be introduced within the next 10 years. Appendix Figure A.3 displays the fiscal estimates that control for the adoption of the sales tax, marked with blue circles. The results are extremely similar to the baseline estimates.

Economic Shocks

Next we examine whether economic shocks confound our estimates by directly controlling for recent economic shocks, which we measure as lagged first differences of log state personal income per capita.[12] Appendix Figure A.3 displays the fiscal results controlling for one, two, or three lagged economic shocks, marked by diamonds, triangles, and squares, respectively. Because we measure state income starting in 1929, controlling for two or more economic shocks causes the sample size to fall relative to the baseline sample size, resulting in slightly wider confidence intervals. The point estimates are generally similar to the baseline estimates for both short-run and long-run effects. The only exception is that controlling for three lagged economic shocks causes the estimated short-run expenditure responses to double, bringing them more in line with the short-run revenue responses.

The Great Depression

Another way to evaluate whether economic conditions confound our estimates is to directly examine the role of the Great Depression. Twenty states adopted the income tax between 1929 and 1940, raising the possibility that states that were hit the hardest by the Depression were more likely to adopt the income tax. These states would have experienced smaller long-run revenue increases due to adverse demographic and economic shocks compared to states that did not adopt during the Great Depression. To evaluate this hypothesis, we separately estimate the impact of tax broadening before and after 1940. This is equivalent to estimating separate effects for adoptions during the Great Depression and after the Great Depression; states that adopted prior to 1930 are not in the sample because there is not enough fiscal data to construct the lagged fiscal shocks for this period.

Appendix Figure A.4 plots separate fiscal responses for the years 1940 and earlier and the years 1941 and later. Three findings emerge from this figure. First, late adopters experienced sharp increases in absolute revenue and expenditure immediately after adoption, while early adopters experienced a delayed increase in revenue. This result could be due to improved tax administration in later years. Second, late adopters saw persistent decreases in absolute revenue and expenditure starting around 8 years after adoption, whereas early adopters did not. This result is the opposite of what one would expect if the Great De-

[12]Personal income by state is available from the Bureau of Economic Analysis for the years 1929-2018. We use this measure of economic activity rather than GDP, because the state-level GDP series starts in 1963.

pression were a major confounder for our estimates. Finally, early and late adopters saw similar long-run increases in revenue and expenditure per capita, though the estimates are less precise for early adopters. This result, too, suggests that the Great Depression is not an important confounder.

Tax Withholding

To take a closer look at the role of tax administration, we estimate separate treatment effects according to whether the state introduced the income tax concurrently with tax withholding. States introduced tax withholding in a staggered fashion over the period 1948-1987 (Dusek and Bagchi, 2018). States that introduced the income tax after 1940 typically introduced withholding at the same time (see Panel (b) of Appendix Figure A.2.) Unsurprisingly, the results, displayed in Appendix Figure A.5, are very similar to the results broken down by time period. Introductions of the income tax concurrently with state-level withholding are associated with larger short-run increases, but similar long-run increases, in revenue and expenditure per capita compared to introductions that occurred without withholding.

Income Tax Adoption by Neighbors

Motivated by prior research finding fiscal policy interdependence among U.S. states (e.g., Case et al., 1993, Baicker, 2005), we examine whether the adoption of the income tax by neighboring states affects the probability that a state introduces the income tax in a given year. We estimate the propensity score, which is just the conditional probability of adopting the income tax in year t given that the state has not already adopted it, as a function of both past demographic shocks and the adoption of the tax by neighboring states. Appendix Table A.8 reports the marginal effects for four different measures of neighbor adoption: an indicator variable equal to 1 if any neighbor has the income tax, an indicator variable equal to 1 if all neighbors have the tax, the number of neighbors that have the tax, and the fraction of neighbors that have the tax. For all four measures, neighbor adoption is associated with a lower probability of introducing the income tax, though the effect is always statistically insignificant. We therefore do not find evidence that tax broadening is driven by mimicking behavior across states.

3 Mobility Consequences of Tax Broadening

The fact that tax broadening leads to a long-run increase in the size of government in per capita terms, but not absolute terms, suggests that some taxpayers leave the state in response to the introduction of the income tax. Interstate mobility may therefore be a key mechanism that helps drive the fiscal results. To test this hypothesis, we first estimate the effect of tax broadening on state population. We then use detailed data on interstate migration flows to examine the movement of households from states with the income tax to non-income-tax states, as well as the composition of these flows by income.

3.1 Results for State Population

We begin by estimating population responses to the income tax using the semiparametric difference-in-differences approach. The propensity score includes year effects and five lags of population to account for selective adoption of the income tax based on past demographic trends.

3.1.1 Main Results

Appendix Figure A.6 depicts the semiparametric difference-in-differences estimates. The estimates are close to zero and statistically insignificant for $s < 0$, suggesting no differential trends prior to tax broadening. Population gradually adjusts to the introduction of the income tax, declining by 4 percent after 11 years and then flattening out. Appendix Table A.4 summarizes the average effect of tax broadening over several four-year periods, confirming the absence of differential pre-trends as well as a 4-percent population decline 12-19 years later. This effect is significant at the 10 percent level.

Appendix Table A.7 reports the coefficient estimates for the propensity score, confirming that temporary declines in population increase the probability of tax broadening, but permanent declines do not. Appendix Figure A.1 also plots the distribution of the propensity score separately for state-years in which the income tax is adopted and not adopted. The two distributions have similar support. Importantly, the propensity score is bounded away from 1, as required by Assumption 2.

3.1.2 Robustness

The Appendix presents several robustness checks for the population results. Appendix Figure A.7 shows that when we control for the adoption of the sales tax or recent economic shocks, the results are similar to the baseline results. Appendix Figure A.8 shows that adoptions of the income tax prior to 1940 had little impact on state population, but population declined significantly in response to adoptions that occurred after 1940. These results are inconsistent with the Great Depression being a major confounder for our estimates. Instead, state population may have been more responsive to later adoptions due to the increase in geographic mobility from 1940 to 1990 in the United States (Rosenbloom and Sundstrom, 2004, Molloy et al., 2011).[13] The results are very similar when we divide adoptions according to whether the income tax was adopted simultaneously with state-level tax withholding. (Results available upon request.)

3.2 Mobility Data

The previous section established that the introduction of the income tax caused state population to fall in adopting states relative to non-adopting states. The result could be due to changes in fertility, mortality, or interstate migration. We find no evidence that tax broadening affected fertility or mortality rates. (Results available upon request.) We therefore turn our attention to interstate migration.

To measure interstate migration flows, we use data from the Integrated Public Use Microdata Series of the U.S. Census, or IPUMS (Ruggles et al., 2019), for every decade from 1900 to 2010. The data consist of 5 percent random samples of the U.S. population for the years 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 American Community Survey, which is a 1 percent random sample. Combining the data yields a repeated cross section of households. For the years 1940 and 1960-2000 only, the census asked individuals to record the state where they resided five years prior to the census date. We can therefore calculate five-year gross migration rates at the state-pair level for these years using the entire sample of households.[14]

[13] Kaplan and Schulhofer-Wohl (2017) show that interstate mobility declined from 1991 to 2011. Because we consider population responses within 20 years of adoption, excluding observations from after 1991 only causes us to exclude New Jersey's adoption in 1976. The results using this subsample are very similar to the baseline results. (Results available upon request.)

[14] The 2010 American Community Survey did not ask this question. The 10 percent sample from the census is

Because our study period begins in 1900, we focus on an alternative measure that can be constructed for every decade. Following Rosenbloom and Sundstrom (2004), we limit the sample to households with a child aged four or five. The average birth date of the child will be close to five years before the census date. Using this subsample of households, we construct five-year migration rates under the assumption that the child's birth state was the household's state of residence five years prior to the census date.

An advantage of the child-based measure is that it is available for every decade of the study period. A disadvantage is that households with small children may be less mobile than other types of households, so our estimates may not be externally valid for the entire United States. In the empirical analysis ahead, we compare the results using the child-based measure and the report-based measure for census years 1940 and later. The two measures yield estimates that are broadly similar; the estimated impact of taxes is slightly larger for the report-based measure. Furthermore, the two measures yield the same qualitative conclusions. We provide more detail ahead.

In 1940 and following decades, IPUMS reports the wage and salary income of individuals, which we aggregate to the household level. In defining high- and lower-earning households, we face a trade-off between isolating the richest households and maintaining a relatively representative sample. This is because, for example, we observe fewer moves by families above the 99th percentile of the income distribution than families above the 90th percentile. In the baseline results, we define high-earning households as those with income above the 95th percentile of the observed income distribution in the current year. Appendix Table A.6 shows that the results are qualitatively similar when the cutoff is the 90th or 99th percentile.

3.3 Theory and Empirical Strategy for Mobility Outcomes

To guide the empirical analysis, we adapt the model of location choice in Moretti and Wilson (2017) to a context in which the taxpayer's outmigration response may be different starting from a zero tax rate than starting from a positive tax rate. We then use this model to derive our estimating equation.

also missing this question.

3.3.1 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

$$U_{iodt} = -\psi D_{dt} + \alpha \log(1-\tau_{dt}) + \alpha \log w_{dt} + Z_d - C_{od} + e_{idt}, \tag{5}$$

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 (5) is the same as the one used in Moretti and Wilson (2017), except for the additional term $-\psi D_{dt}$. If the introduction of a new tax creates an additional administrative burden for the taxpayer, then ψ is strictly positive and represents the utility cost of tax compliance. The parameter ψ could also, in part, reflect salience effects. The introduction of a new tax may be more salient to taxpayers than changes in the rates of existing taxes. Consequently, tax introductions may influence behavior through channels that are not fully captured by the net-of-tax rate. Both the compliance-costs channel and the salience channel imply that decreasing the net-of-tax rate by one percent starting from a zero tax rate will have a different impact on behavior than a one-percent reduction in the net-of-tax rate starting from a positive tax rate.

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.,

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

Our goal is to estimate how changes in taxes affect migration flows between states. 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),

$$\log(P_{odt}/P_{oot}) = \psi(D_{ot} - D_{dt}) + \alpha[\log(1-\tau_{dt}) - \log(1-\tau_{ot})] + \alpha \log(w_{dt}/w_{ot}) \qquad (6)$$
$$+ (Z_d - Z_o) - C_{od},$$

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 (6) 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 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, say, 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 tax differentials, due to differences in amenities and moving costs.

For many individuals, the migration decision will depend on firm location, which determines labor demand. Assume that every firm hires the same number of workers. Further suppose, for convenience, that the profits of firm j that moves from state o to state d take the form

$$\log \pi_{jodt} = -\rho D'_{dt} + \beta \log(1-\tau'_{dt}) - \log w_{dt} + Z'_d - C'_{od} + \nu_{jdt}$$

where D'_{dt} is an indicator variable equal to one if state d has a corporate income tax, τ'_{dt} is the corporate income tax rate, Z'_d is productivity-enhancing amenities, and C'_{od} is the firm's cost of moving from state o to state d, where $C_{oo} = 0$. The firm's idiosyncratic productivity in state d is ν_{jdt}, which reflect the quality of the match between the firm and the state. The parameter ρ captures the costs of complying with the corporate tax.

A firm relocates from state o to state d if and only if it earns higher profits in state d than in state o or any other state, i.e.,

$$\pi_{jodt} > \max_{d' \neq d}\{\pi_{jod't}\}.$$

If the ν_{jdt} are i.i.d. with an Extreme Value Type I distribution, then the log odds ratio equals the difference in profit levels in the origin and destination states,

$$\log(P'_{odt}/P'_{oot}) = \rho(D'_{ot} - D'_{dt}) + \beta[\log(1-\tau'_{dt}) - \log(1-\tau'_{ot})] - \log(w_{dt}/w_{ot}) + (Z'_d - Z'_o) - C'_{od}, \tag{7}$$

where P'_{odt} is the probability that a firm located in state o moves to state d, and P'_{oot} is the probability that a firm located in state o stays in state o. Because each firm hires the same number of workers, the ratio of moving firms to staying firms equals the ratio of moving individuals to staying individuals. Therefore, Equation (7) characterizes the demand for labor in state d.

3.3.2 Econometric Model

Relative wages are determined in equilibrium according to Equations (6) and (7). Setting labor supply equal to labor demand, $\log(P_{odt}/P_{oot}) = \log(P'_{odt}/P'_{oot})$, and solving for relative wages yields the regression equation

$$\log(P_{odt}/P_{oot}) = \theta(D_{ot} - D_{dt}) + \eta[\log(1-\tau_{dt}) - \log(1-\tau_{ot})] + \theta'(D'_{ot} - D'_{dt}) + \eta'[\log(1-\tau'_{dt}) - \log(1-\tau'_{ot})] + \gamma_{od} + \phi_t + u_{odt}, \tag{8}$$

where $\theta = \psi/(1+\alpha)$ is the effect of the individual income tax introductions, $\eta = \alpha/(1+\alpha)$ is the effect of the individual income tax rates, $\theta' = \rho\alpha/(1+\alpha)$ is the effect of the corporate tax introductions, $\eta' = \beta\alpha/(1+\alpha)$ is the effect of the corporate tax rates, and γ_{od} absorbs origin and destination amenities and state-pair moving costs.[15] We add ϕ_t to capture the common effect of national economic conditions, transportation infrastructure, or technology levels on mobility nationwide.

As already mentioned, our data allow us to calculate five-year migration rates every decade. We measure P_{odt}/P_{oot} as the number of households that move from state o to state d over the five-year period, divided by the number of households that stay in state o.[16] The tax

[15] Note that the fixed effects are specific to the direction of migration, so that $\alpha_{od} \neq \alpha_{do}$ for $o \neq d$.

[16] We calculate sums using household weights provided by IPUMS. Our results are very similar when we use unweighted sums. (Results available upon request.)

variables are measured in the middle of the five-year period. For example, migration over the period 1935-40 is matched to tax policy in 1937. Using the tax values in earlier or later parts of the period does not significantly alter the results. We measure τ_{ot} and τ_{dt} using the top marginal tax rates on personal income in the origin and destination states, respectively.

We focus on estimating θ and η in Equation (8). As noted in Section 2.2.1, the individual and corporate income taxes were usually introduced around the same time. We therefore focus on a parsimonious specification that only includes the individual income tax variables, keeping in mind that θ largely reflects the compound effect of introducing the individual and corporate income taxes. Controlling for the corporate tax variables does not significantly alter our results.[17]

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 roughly $100\theta + \eta$ 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 roughly $100\theta + \eta$ percent. Thus the model assumes that increases and decreases in tax differentials have symmetric effects.

Now suppose state o already has an income tax and decides to raise 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 100θ 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 $\theta = 0$. However, tax introductions may affect outmigration through compliance-costs or salience channels, rather than simply though the net-of-tax rate. If this is the case, then θ will be positive, even when controlling for the net-of-tax rate.

3.3.3 Deriving Mobility Responses

Besides characterizing bilateral migration flows, the parameters θ and η determine total migration responses to tax policy. First consider a small change to one state's individual

[17]Though our simple model assumes that firms only respond to the corporate tax, in reality many firms are not C corporations and thus do not pay the corporate tax. For these firms, profits are taxed at the individual rate. As a result, the individual income tax influences labor demand, not just labor supply.

income tax rate, starting from a positive rate. The inmigration elasticity for state d is

$$\varepsilon_{dt} \equiv \frac{\mathrm{dlog} M_{dt}}{\mathrm{dlog}(1-\tau_{dt})},$$

where M_{dt} is the total number of migrants to state d in period t. Define the overall inmigration elasticity ε to be the weighted average of ε_{dt}, weighting by M_{dt}. In the Appendix, we show that

$$\varepsilon = \eta \cdot (1 - \overline{P}), \tag{9}$$

where $\overline{P}$ is the weighted average of P_{odt}, weighting by the number of migrants from o to d in period t. In our setting, $\overline{P}$ equals 0.016 using the child-based measure of migration and 0.007 using the report-based measure, so the inmigration elasticity is very close to η. The Appendix also shows that the outmigration elasticity is close to $-\varepsilon$.

Next consider the introduction of the income tax at rate τ_1. As shown in the Appendix, the inmigration and outmigration responses, in percentage terms, are approximately

$$\begin{aligned} \Delta^{0,\tau_1} &\equiv \exp(-\theta + \eta \log(1-\tau_1)) - 1, \\ \Omega^{0,\tau_1} &\equiv \exp(\theta - \eta \log(1-\tau)) - 1. \end{aligned} \tag{10}$$

By comparison, the percentage change in inmigration due to a large tax rate increase, from τ_1 to τ_2, is approximately

$$\Delta^{\tau_1,\tau_2} \equiv \exp\left(\eta \log \frac{1-\tau_2}{1-\tau_1}\right) - 1. \tag{11}$$

In the results section ahead, we use the expressions in Equations (10) and (11) to compare tax introductions with large tax reforms.

When a state increases taxes, it is able to provide more public goods on a per capita basis, as the previous section established. The above mobility responses therefore reflect the composite effect of taxes and their associated levels of public goods. If the benefits of additional public goods exactly equal the increase in tax liability, then households would not move in response to a tax increase. However, the benefits and costs of taxation are unlikely to be equal for all, or even most, households, for three reasons. First, public sector inefficiency drives a wedge between tax revenue and non-wasteful spending. Second, most state income taxes are progressive, so high-income households bear a greater share of the tax burden than low-income households. Finally, some households value public goods less than others. For example, high earners are more likely to send their children to private school (Moretti and Wilson, 2017).

3.3.4 Inference

We follow standard practice and calculate standard errors that are robust to heteroskedasticity and three-way clustering by origin-destination pair, origin $\times$ year, and destination $\times$ year (Moretti and Wilson, 2017, Agrawal and Foremny, 2019). Clustering by origin-destination pair accounts for serial correlation in the tax variables and errors within the same pair of states. Clustering by origin $\times$ year accounts for the potential correlation between observations of state pairs sharing an origin state in the same year. This correlation could be generated by the fact that the origin state's tax variables take the same value for these observations. Similar logic justifies clustering by destination $\times$ year.

Intuitively, the standard errors allow for correlation of an unknown form between, say, California-Oregon migration flows in 1900 and California-Oregon migration flows in 2010. They also allow California-Oregon observations to be correlated with California-Texas or Idaho-Oregon observations in the same year. Note that the standard clustering scheme assumes a zero correlation between observations in different years that share either an origin or a destination, but not both. That is, the California-Oregon observation in 1950 is assumed to be uncorrelated with the California-Texas observation in 1960. To probe robustness to violations of this assumption, we also calculated standard errors that allow for two-way clustering by origin and destination. These standard errors rely on strictly weaker assumptions than the standard approach and would allow, for example, a nonzero correlation between California-Oregon in 1950 and California-Texas in 1960. The two-way clustered standard errors are somewhat larger than the baseline standard errors, but the results are still statistically significant – sometimes highly so. (Results available upon request.)

3.3.5 Identifying Assumption and Threats to Validity

The key identifying assumption needed to estimate Equation (8) is that migration flows between state pairs for which tax differentials changed would have, in the absence of a change, experienced similar trends as migrations flows between pairs for which tax differentials did not change. While this assumption is not testable, it would be more plausible if trends were similar in the two groups prior to the change in tax differentials. We test for pre-trends ahead.

One source of potential bias in estimating θ is measurement error in tax rates, which could be correlated with $D_{ot} - D_{dt}$. This measurement error comes from two sources. First, as already mentioned, we impute pre-1941 tax rates using the rate in 1941. In Appendix Table A.5, we show that the magnitude and statistical significance of the estimated θ is very similar across an array of imputation schemes, including no imputation.

The second source of measurement error is due to the fact that rational taxpayers will choose location based on average tax rates (ATR), not marginal tax rates (MTR). Unfortunately, calculating average tax rates is infeasible in our context, as the NBER's TAXSIM calculator does not cover state laws prior to 1977, and no state introduced the income tax after 1977. The top marginal tax rate may be a reasonable proxy for the average tax rate in certain circumstances, since it approximates the average tax rate of top earners well, correlates strongly with the average tax rate of top earners within a state over time, and yields similar mobility elasticities as the average tax rate in prior research (Moretti and Wilson, 2017). However, it is possible that the difference between marginal and average tax rates will bias our estimates of θ away from zero. The top MTR is closer to the ATR for high-income households than for low-income households. Therefore, measurement error is smaller in absolute terms for high earners. If our estimates of θ are biased away from zero due to this source of measurement error, then the bias would be larger for low-income households than for high-income households. In fact, we estimate θ to be twice as large for high-income households than for low-income households, suggesting that our finding of a nonzero θ is not spuriously due to this second source of measurement error.

Finally, the estimates may suffer from selection bias due to the fact that we use migration flows. Because our dataset is based on 1 and 5 percent samples of the U.S. population, we do not observe moves for some origin-destination pairs in some years. These observations are excluded from the analysis, because their log odds ratio is undefined. Out of 30,060 potential observations (51 states $\times$ 50 states $\times$ 12 periods), our child-based analysis uses 18,269 observations, or 61 percent of the potential sample size. Sample selection is less of a concern for the report-based analysis, which uses 13,617 out of 15,300 potential observations (51 states $\times$ 50 states $\times$ 6 periods), or 89 percent. We are more likely to observe positive migration flows between states that are populous or similar to each other geographically or along other dimensions. Nonrandom selection of state pairs may cause either an upward or downward bias in our estimates (Moretti and Wilson, 2017). Compared to previous studies focused on top earners, the amount of missing data in our analysis is relatively small. For example, the sample in Moretti and Wilson (2017) represents 18 percent of potential state-pair-year observations. Another advantage is that we use multiple measures of migration that are subject to selection issues to differing degrees, allowing us to probe the robustness of our estimates by comparing results based on the two measures.

3.4 Results for Mobility Outcomes

3.4.1 Timing of Responses

We first examine the timing of migration responses around the introduction of the income tax. We estimate the local projections (Jordà, 2005, Moretti and Wilson, 2017)

$$\log(P_{odt+h}/P_{oot+h}) - \log(P_{odt}/P_{oot}) = \beta^h E_{odt} + \phi_t^h + \nu_{odt}^h \tag{12}$$

for different values of h. The outcome is the change in the outmigration log odds ratio between periods $t+h$ and t, and E_{odt} indicates a tax event that occurs between periods t and $t+1$. We focus on destination-origin differentials in the presence of an income tax, $D_{ot} - D_{dt}$, and for simplicity assume that increases and decreases in this differential have symmetric effects. Specifically, E_{odt} equals 1 if the differential increases, -1 if the differential decreases, and 0 if the differential does not change, between t and $t+1$.

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 visualize how migration evolves over time in state pairs that experience a tax differential change, relative to state pairs that do not. Finding that $\beta^h \neq 0$ for some $h < 0$ would imply that the two groups were on different migration trends prior to the tax change, casting doubt on the identifying assumption.

Besides testing for differential pre-trends, Equation (12) sheds light on the dynamic response of migration to a change in the tax differential. In particular, it allows us to test another assumption implicit in Equation (8): that migration flows respond immediately to changes in tax differentials.

Figure 3 displays the results. Panel (a) shows the results based on flows of all households with a child aged four or five at the time of the census. Separate results are shown for the entire sample period (1900-2010) and the period 1940-2010. We show both graphs for reference, because the measures disaggregated by income are only available for the period 1940-2010. Over the entire sample period, the outmigration odds ratio increases by about 10 percent following an increase in the tax broadening differential, $D_{ot} - D_{dt}$.[18] The response is immediate and persists for three decades after the tax event. Over the period 1940-2010, the outmigration response is larger at about 20 percent. Once again, the response is immediate and persists for three decades. There is no evidence of differential trends in migration prior to the tax event.

Panel (b) displays the results for migration flows that contain at least one moving household in the top 5 percent of the observed income distribution in the current year.[19] This sample covers the period 1940-2010. The tax event causes the outmigration rate to immediately increase by about 15 percent for households in the bottom 95 percent of the income distribution. However, in the following decades, the response falls to about 5 percent and is statistically insignificant. By contrast, the outmigration rate of households in the top 5 percent of the income distribution immediately increases by about 20 percent following the tax event, and the response grows to about 30 percent three decades later. The graphs suggest that the location choices of top-earning households are more sensitive to the presence of an income tax than households lower in the income distribution. Again, the graphs show no evidence of differential trends in migration prior to the tax event.

3.4.2 Regression Results

Table 5 presents the baseline regression estimates based on Equation (8). The estimates in Panel A are based on the child-based measure of migration. Columns 1 and 2 present results for the entire study period: 1900-2010. In column 1 the coefficient on the income tax presence differential is 0.128, which is statistically significant at the 1 percent level. This means that the introduction of the income tax by the origin state increases the five-year rate of outmigration to the destination state by almost 13 percent, holding fixed the destination state's

[18]Equivalently, the outmigration odds ratio *decreases* by about 10 percent following a *decrease* in the tax broadening differential.

[19]Households must also have a child aged four or five at the time of the census.

tax policy. Controlling for the net-of-tax rate in column 2 has little impact on the estimated effect of introducing the income tax. Somewhat surprisingly, the estimated coefficient on the log net-of-tax rate is close to zero and statistically insignificant.

Columns 3 and 4 present results for the latter part of the sample period: 1940-2010. The estimated effect of tax broadening becomes larger – implying an 18 percent increase in outmigration – and remains significant at the 1 percent level. The larger coefficient may be due to greater average mobility in the United States in the years following the Great Depression. The estimated effect of introducing the income tax is unchanged when we add a control for the net-of-tax rate. Now the estimated coefficient on the log net-of-tax rate is positive, but it is small and statistically insignificant.

The estimates using the report-based measure of migration (Panel B) are somewhat larger, implying that tax broadening increases the rate of outmigration by around 20 percent. The larger effects for the report-based measure compared to the child-based measure may reflect the fact that households with a small child are less mobile – and their location decisions are less responsive to tax policy – than the U.S. population as a whole.

Columns 5-7 present results based on state pairs and years containing at least one moving household in the top 5 percent of the income distribution. These results are based on the period 1940-2010, when we observe household income. Column 5 reports estimates based on all households within the selected sample. The estimated impact of introducing the income tax (0.117) is smaller than in the full sample using the child-based measure, but it remains significant at the 1 percent level. The estimated elasticity of outmigration with respect to the net-of-tax rate is 1.3 and is significant at the 5 percent level. The degree of sample selection in column 5 – due to focusing on flows with high earners – appears to be smaller for the report-based measure, which produces an outmigration response that is very similar to the one estimated in the full sample.

Column 6 presents the outmigration response based on households in the bottom 95 percent of the income distribution, and column 7 presents the outmigration responses of households in the top 5 percent of the income distribution. For both the child-based and report-based measures of migration, the response to the introduction of the income tax is significantly larger for high-earning households. For example, according to the child-based measure, introducing the income tax raises the outmigration rate of lower-income households by 11 percent, yet it raises the outmigration rate of high-income households by 25 percent. The 95 percent confidence interval on the response of high-income households excludes the point estimate for lower-income, so the outmigration responses appear to differ substantially in both economic and statistical terms.

Conditional on the presence of an income tax, top-earning households exhibit larger migration elasticities with respect to the net-of-tax rate compared to lower-earning households. Using Equation (9), the elasticity for top earners is 1.80 and 1.76 for the child-based and report-based measures, respectively. Moretti and Wilson (2017) find a very similar elasticity for star scientists over the period 1977-2010. The similar results are particularly striking given that our analysis uses a more representative subsample of state pairs over a longer period of time, while relying on decennial data rather than yearly data.

How does the mobility response to the introduction of the income tax compare to the response to a large tax increase, starting from a positive rate? Using the expression in Equation (10) and the estimates in Panel A, column 5 of Table 5, introducing the income tax at a rate of 1 percent causes inmigration to fall by 12.2 percent (S.E. = 3.4 percent). Starting from a rate of 1 percent, the state would have to raise the tax rate to 10 percent to achieve the same decline in inmigration.[20] Overall, the results indicate that the introduction of the income tax matters for mobility beyond its effect on the net-of-tax rate.

The mobility responses appear large because they are calculated relative to the small base of households that move over a five-year period. The percentage change in the population *stock* is much smaller. For example, using the estimates in Panel A, column 5 of Table 5, permanently introducing a 1-percent income tax causes the population stock to fall by 4.1 percent (S.E. = 1.2 percent) over each ensuing five-year period until tax differentials change again.[21] This effect is larger than the semiparametric difference-in-differences estimates for population, which showed a 4-percent reduction after 10 years. However, the two results are compatible. The estimate from the location-choice model assumes a permanent change in tax differentials, whereas the DiD estimate compares states that adopted the tax in the

[20] To see this, use Equations (10) and (11) and set $\Delta^{0,\tau_1} = \Delta^{\tau_1,\tau_2}$ and $\tau_1 = 0.01$, and solve for τ_2.

[21] The percentage change in the population stock is approximately $\Delta^{0,\tau_1} \cdot M/P - \Omega^{0,\tau_1} \cdot L/P$, where Δ^{0,τ_1} and Ω^{0,τ_1} are from Equation (10), M is the number of households moving into the state, L is the number of households leaving the state, and P is the initial population. In the child-based sample, $\Delta^{0,\tau_1} = -0.122$ and $\Omega^{0,\tau_1} = 0.139$ for $\tau_1 = 0.01$, and the migration-weighted averages of M/P and L/P are 0.161 and 0.151, respectively.

current year with states that did not adopt this year but that potentially adopted in some future year. The DiD estimates thus largely reflect temporary changes in tax differentials.

3.4.3 Robustness

Introduction of Sales Tax

To check whether the migration results are biased due to confounding state policies, we estimate the baseline specification while controlling for state differentials in the presence of the sales tax. Panel (a) of Appendix Figure A.9 displays the results. Controlling for the presence of the sales tax has virtually no impact on our baseline estimates.

Economic Shocks

Next we check whether economic shocks confound our estimates by controlling for state differentials in log personal income per capita at the state level. Panel (a) of Appendix Figure A.9 shows that when we control for state income, the baseline estimate using the child-based measure falls from to 0.180 to 0.157, and the baseline estimate using the report-based measure falls from 0.203 to 0.151. However, in both cases the estimate remains economically large and highly significant.

The Great Depression

Another potential concern is that the states hit hardest by the Great Depression were more likely to adopt the income tax and would have lost residents to other states even in the absence of adoption. However, as already shown, outmigration was more sensitive to tax broadening after 1940 than before 1940. This result is inconsistent with the Great Depression being a major confounder, and may be due to the increase in geographic mobility from 1940 to 1990 in the United States (Rosenbloom and Sundstrom, 2004, Molloy et al., 2011). Interestingly, interstate mobility actually fell over the period 1991-2011 (Kaplan and Schulhofer-Wohl, 2017). The results are similar when we exclude years after 1990, the main difference being that top earners are somewhat more sensitive to tax rates. (Results available upon request.)

Mobility-Enhancing Technology and Infrastructure

While the year effects in Equation (8) capture common shocks to mobility across all regions, the importance of shocks to, say, technology or infrastructure may differ across regions. For example, the dramatic expansion of residential air conditioning starting around 1960 increased the relative attractiveness of hot states (Biddle, 2008). As a second example, the construction of the interstate highway system starting in the 1950s increased the accessibility of western states. Our estimates may be biased if changes in tax differentials are correlated with region-specific shocks to mobility.

Panel (b) of Appendix Figure A.9 shows how the results change when we control for regional shocks. We first define the indicator variable *Cold-Hot*, which equals one for state pairs that contain one hot state and one non-hot state, where "hot" states are those ranked in the top 10 for average maximum temperature in July from 1901 to 2000.[22] We then control for *Cold-Hot*-by-year effects to allow aggregate shocks to differ for flows into and out of hot regions compared to all other flows. Next we define the indicator variable *East-West*, which equals one for state pairs that contain one western state and one non-western state. Controlling for *East-West*-by-year effects allows aggregate shocks to differ for flows into and out of the west compared to other flows. Finally, we define a set of region-pair indicator variables, using the U.S. Census Bureau's four-region categorization. Controlling for region-pair-by-year effects allows aggregate shocks to differ for each of the 10 pairs of regions. All three approaches for controlling for regional shocks have virtually no effect on the baseline estimates.

Nearby versus Faraway States

We also examine whether migration flows between nearby states are more sensitive to tax differentials than flows between more distant states. To do this, we augment the baseline specification with an interaction between the origin-destination differential in the presence of the income tax, $D_{ot} - D_{dt}$, and an indicator variable that equals one if the pair of states are "close" to each other. We measure proximity in three ways: whether the states share a border (*Neighbors*), whether the states are located in the same region according to the U.S. Census Bureau's nine-region categorization (*Same Region*), and whether the states are located in the same region according to the U.S. Census Bureau's four-region categorization (*Same Large Region*). The first measure captures simple geographic proximity, while the second and third measures could additionally capture cultural and economic proximity. Appendix Table A.9 reports the results. There is no evidence that the migration response to tax broadening is

[22]The states are Texas, Oklahoma, Arizona, Louisiana, Mississippi, Kansas, Arkansas, Alabama, Florida, and Georgia. The results are nearly identical when we use the top 15 hottest states. (Results available upon request.) The temperature data are from the NOAA National Centers for Environmental Information.

greater for pairs of states that are close to each other, regardless of the measure of proximity. One possible interpretation is that the fixed costs of moving are large relative to the marginal costs of distance conditional on moving.

3.5 Relation to Fiscal Results

The two sets of fiscal results in Section 2 can be explained, at least in part, by the long-run decline in state population in response to tax broadening, which represents a significant decline in the tax base. The especially strong outmigration response by high-income households likely reduced the impact of tax broadening on per capita revenue and expenditure, but state governments were nonetheless able to increase the per capita budget in the long run.

Interestingly, the decline in the absolute level of revenue starting three years after adoption appears sharper than the population response. This discrepancy suggests that other factors, such as a reduction in labor supply or an increase in other forms of tax avoidance, may have also contributed to the short-lived nature of the revenue increase.

4 Conclusion

Using a new panel database that covers the entire twentieth century and the start of the twenty-first century, we investigate the consequences of a major investment in state capacity: the introduction of the income tax by U.S. states. Our empirical strategy exploits the staggered adoption of the tax, and accounts for selective timing of adoption based on recent demographic and fiscal trends. We find that tax broadening increased the absolute size of state governments in the short run but not the long run, while per capita budgets permanently increased. We explain the fiscal results by showing that tax broadening reduced state population in the long run, as taxpayers fled to non-income-tax states. This outmigration response was particularly strong for high-income households, but not strong enough to prevent per capita revenue from increasing.

Our results identify the income tax as a key tool for expanding the role of government. Yet they also show that the return on fiscal-capacity investments depends on the elasticity of the new tax base. Population mobility provides a partial check against an ever-expanding state.

Our results suggest several directions for future research. One direction is to examine the consequences of broadening the tax base in other dimensions, such as through the introduction of the sales tax. Another is to see whether our results for within-country population mobility generalize to other countries. While the United States has one of the most geographically mobile populations in the world, Finland, Denmark, and Great Britain have similar rates of mobility (Molloy et al., 2011). Local governments in Finland and Denmark rely heavily on the income tax, making these two countries leading candidates for future research (OECD, 2002). Finally, future research should continue to investigate the effects of state-capacity investments in low-income countries, where local taxation tends to be limited (Gadenne and Singhal, 2014) but the return on government investments is potentially high.

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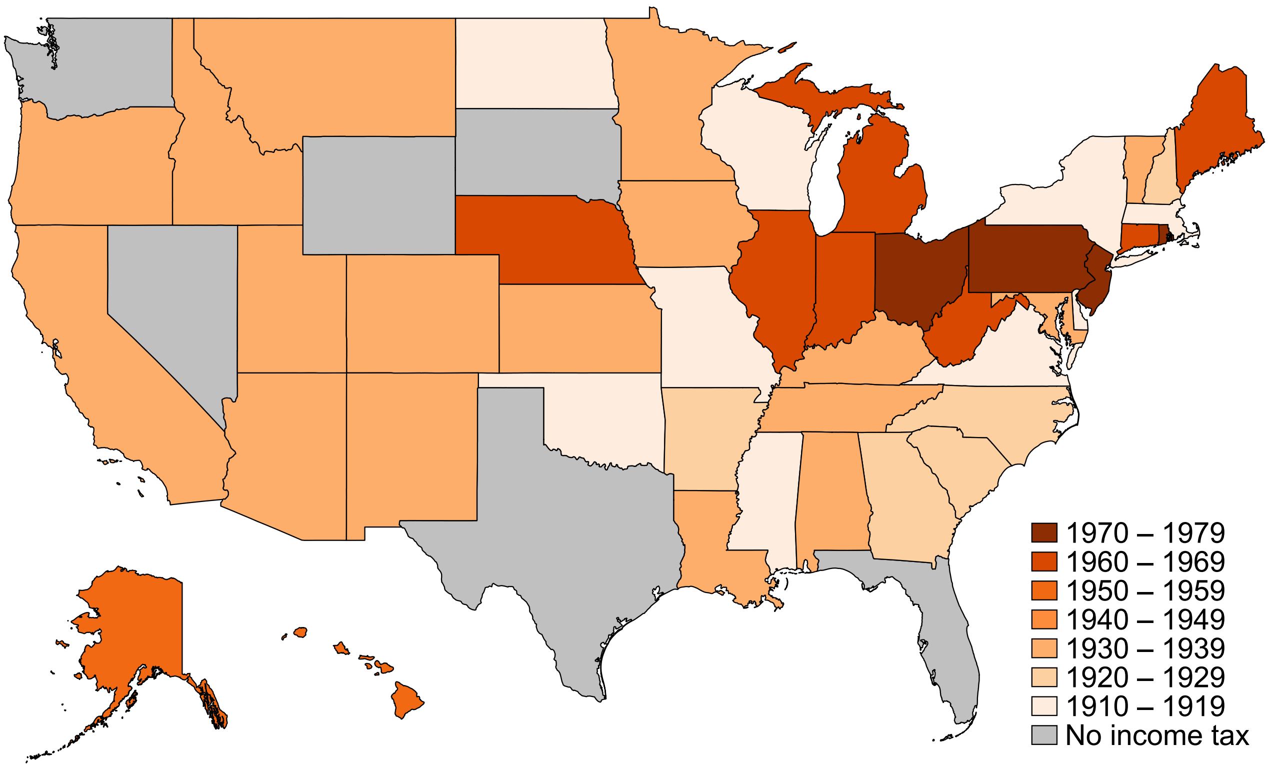
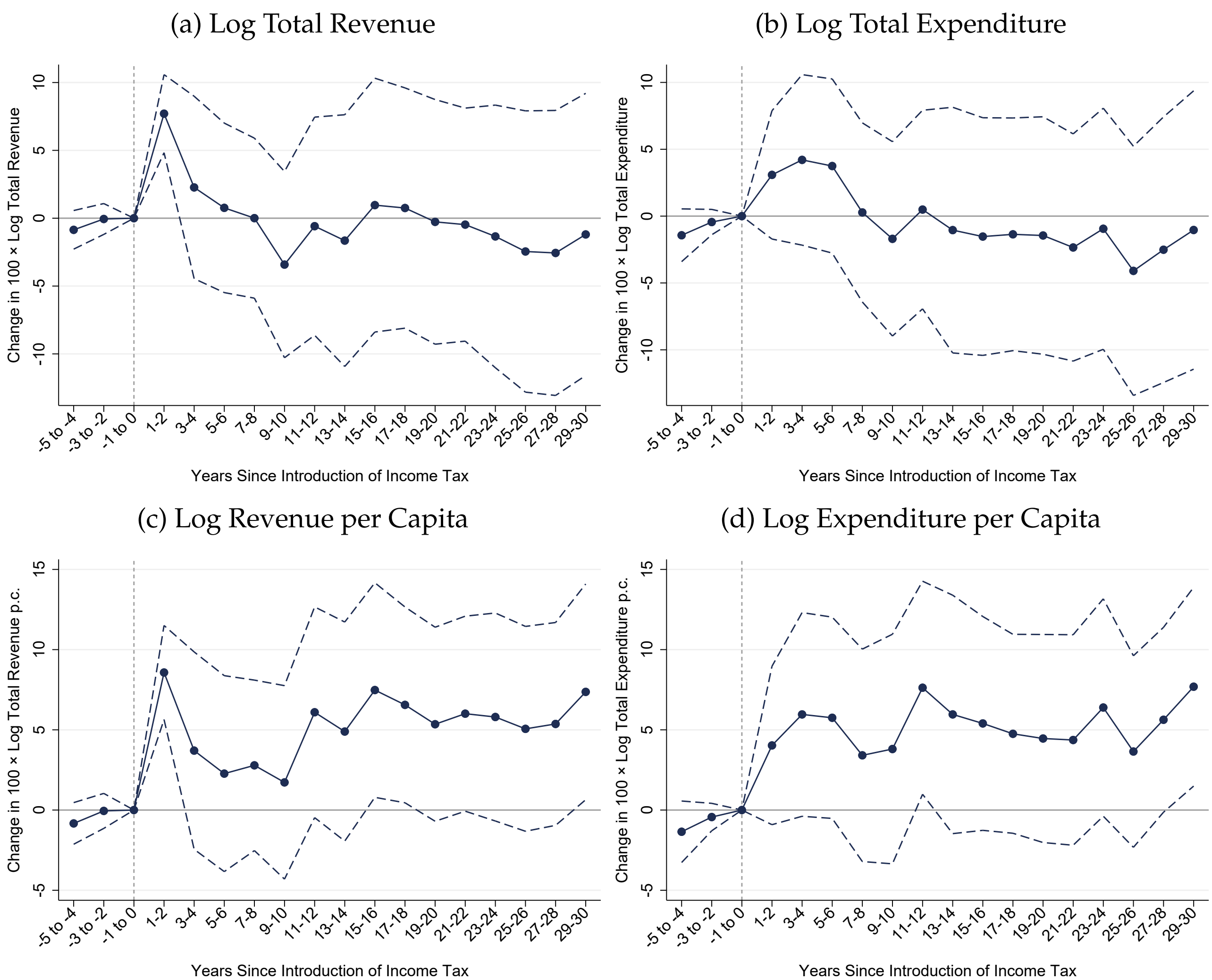
Figure 1: Year of Introduction of State-Level Individual Income Tax



Notes: This figure displays U.S. states shaded according to the decade the state adopted the individual income tax, where darker shades indicate later decades. States colored gray never adopted an individual income tax. The source for 1902-1980 is Penniman (1980). We use administrative data to extend this source for 1980-2008. Appendix Table A.1 lists the years each state adopted the individual income tax and corporate income tax.

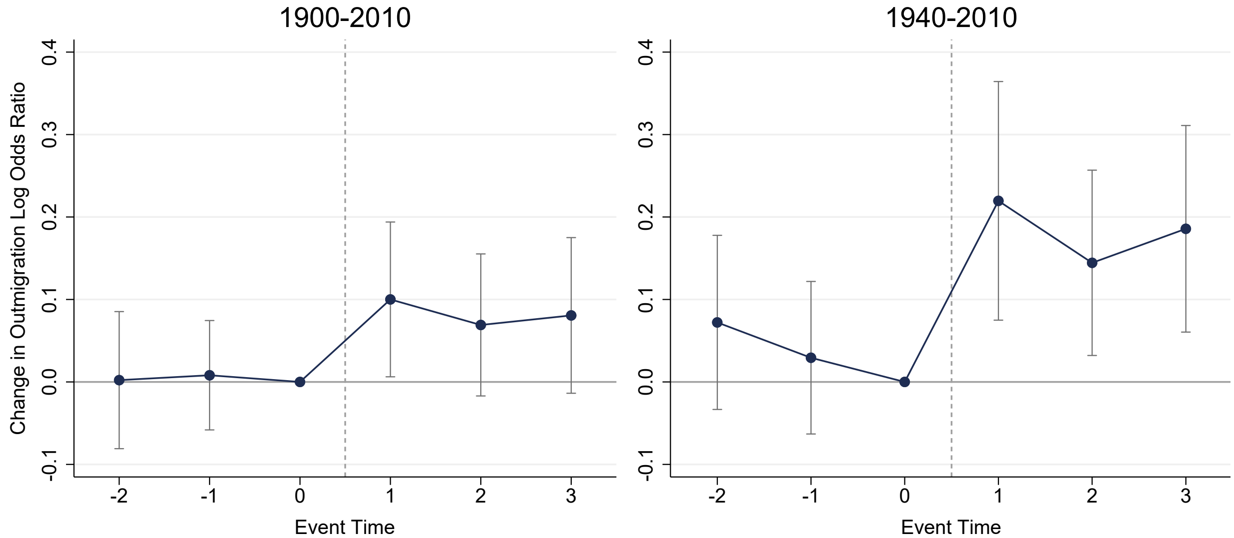
Figure 2: Dynamic Effects of Tax Broadening on Revenue and Expenditure



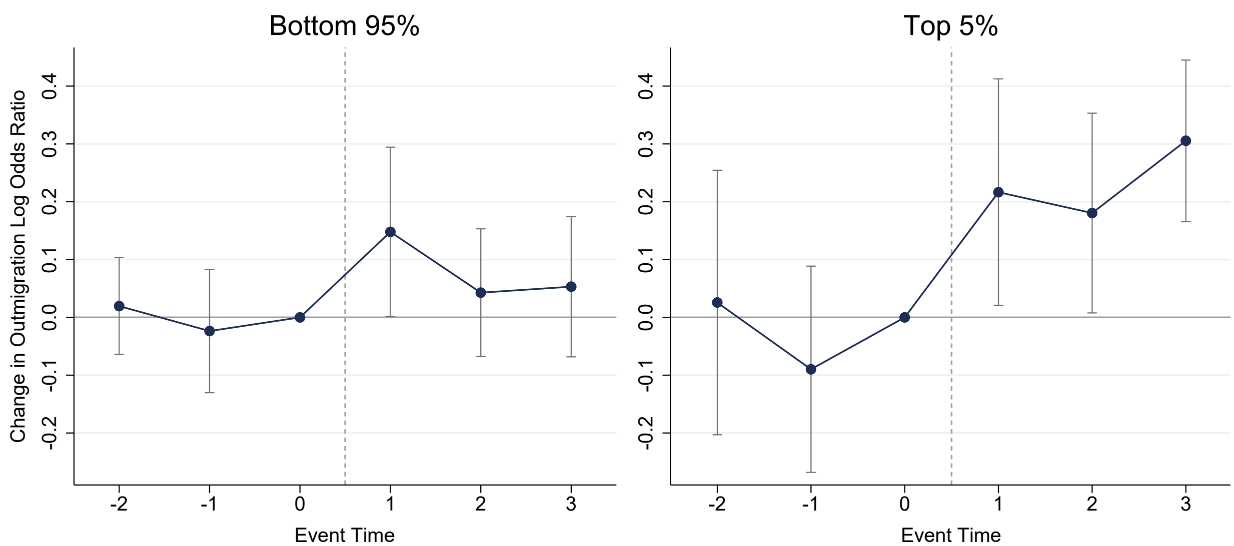
Notes: This figure plots semiparametric difference-in-differences estimates of the effect of the introduction of the income tax on the change in 100 $\times$ log revenue and expenditure, in non-per capita and per capita terms, over different time horizons. Lagged differences and forward differences are defined in terms of two-year periods to reduce the influence of missing data. The solid line plots the average treatment effect on the treated, and the dashed lines plot 90 percent confidence intervals. The x-axis measures time periods relative to the introduction 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, five lags of log population, and the first two lagged first differences of the outcome variable as covariates. Confidence intervals are robust to heteroskedasticity and clustering at the state level.

Figure 3: Dynamic Effects of Tax Broadening Event on Outmigration

(a) All Outmigration Flows



(b) Heterogeneity by Income (Flows with Top Earners, 1940-2010)



Notes: This figure plots point estimates and 90 percent confidence intervals for β^h from Equation (12) for different values of h. Event time is measured in decades relative to the introduction of the income tax, where the introduction occurred between periods 0 and 1. The confidence intervals are robust to heteroskedasticity and three-way clustering at the origin-destination pair, origin $\times$ year, and destination $\times$ year levels.

Table 1: The Effect of Tax Broadening on Revenue

Years since tax broadening:	-5 to -4 (1)	-3 to -2 (2)	1 to 2 (3)	5 to 6 (4)	9 to 10 (5)	19 to 20 (6)	29 to 30 (7)
	Panel A: Log Total Revenue						
Average effect of tax broadening	-0.86	-0.06	7.71^{***}	0.76	-3.42	-0.27	-1.20
	(0.86)	(0.69)	(1.75)	(3.80)	(4.17)	(5.48)	(6.32)
Observations	192	192	163	158	192	192	192
	Panel B: Log Total Tax Revenue						
Average effect of tax broadening	0.95	1.63	12.18^{***}	3.77	-1.61	-2.78	1.03
	(1.49)	(1.56)	(2.26)	(3.67)	(5.88)	(6.54)	(6.04)
Observations	96	96	96	96	96	96	96
	Panel C: Log Total Property Tax Revenue						
Average effect of tax broadening	4.78	6.78	-14.45	-0.89	17.31	11.62	12.34
	(8.33)	(9.46)	(22.51)	(44.96)	(47.31)	(65.87)	(70.64)
Observations	192	192	163	158	192	192	192

Notes: This table presents semiparametric difference-in-differences estimates of the average effect of tax broadening (i.e., the introduction of the income tax) over different time horizons. Changes in the outcome are measured relative to the baseline period of 0 to 1 years before the introduction of the income tax. We report the average treatment effect on the treated. The treatment effect is multiplied by 100 to increase readability. The estimators use inverse probability weighting, where the propensity score is estimated by probit and is a function of year effects, five lags of log population, and the first two lagged first differences of the outcome variable. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: The Effect of Tax Broadening on Revenue per Capita

Years since tax broadening:	−5 to −4 (1)	−3 to −2 (2)	1 to 2 (3)	5 to 6 (4)	9 to 10 (5)	19 to 20 (6)	29 to 30 (7)
			Panel A: Log Revenue per Capita				
Average effect of tax broadening	−0.83	−0.05	8.57***	2.27	1.72	5.35	7.37*
	(0.79)	(0.66)	(1.78)	(3.71)	(3.67)	(3.68)	(4.09)
Observations	192	192	163	158	192	192	192
			Panel B: Log Tax Revenue per Capita				
Average effect of tax broadening	0.65	1.37	13.39***	8.30***	6.13*	9.09**	19.11***
	(1.23)	(1.33)	(2.04)	(2.83)	(3.68)	(4.00)	(3.87)
Observations	96	96	96	96	96	96	96
			Panel C: Log Property Tax Revenue per Capita				
Average effect of tax broadening	−0.16	2.35	−23.22	−17.32	−6.52	−23.64	−13.17
	(4.90)	(6.54)	(15.71)	(25.16)	(27.21)	(35.46)	(38.33)
Observations	192	192	163	158	192	192	192

Notes: This table presents semiparametric difference-in-differences estimates of the average effect of tax broadening (i.e., the introduction of the income tax) over different time horizons. Changes in the outcome are measured relative to the baseline period of 0 to 1 years before the introduction of the income tax. We report the average treatment effect on the treated. The treatment effect is multiplied by 100 to increase readability. The estimators use inverse probability weighting, where the propensity score is estimated by probit and is a function of year effects, five lags of log population, and the first two lagged first differences of the outcome variable. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: The Effect of Tax Broadening on Expenditure

Years since tax broadening:	−5 to −4 (1)	−3 to −2 (2)	1 to 2 (3)	5 to 6 (4)	9 to 10 (5)	19 to 20 (6)	29 to 30 (7)
	Panel A: Log Total Expenditure						
Average effect of tax broadening	−1.43	−0.44	3.09	3.75	−1.70	−1.45	−1.04
	(1.20)	(0.58)	(2.92)	(3.96)	(4.42)	(5.40)	(6.33)
Observations	192	192	163	158	192	192	192
	Panel B: Log Education Expenditure						
Average effect of tax broadening	−0.22	0.53	7.38***	−2.49	−8.78*	−13.11*	−14.41**
	(1.80)	(1.02)	(2.78)	(4.05)	(4.99)	(6.81)	(7.15)
Observations	96	96	96	96	96	96	96
	Panel C: Log Health Expenditure						
Average effect of tax broadening	−2.51	−2.21	−2.39	−1.69	−7.79	−17.39*	−16.63
	(3.70)	(2.43)	(6.82)	(9.53)	(10.55)	(9.79)	(14.21)
Observations	96	96	96	96	96	96	96
	Panel D: Log Safety Expenditure						
Average effect of tax broadening	0.53	1.33	−7.97	−17.33	−29.34	−21.88	−32.67**
	(1.75)	(1.06)	(14.69)	(14.90)	(17.94)	(14.53)	(13.79)
Observations	96	96	96	96	96	96	96

Notes: This table presents semiparametric difference-in-differences estimates of the average effect of tax broadening (i.e., the introduction of the income tax) over different time horizons. Changes in the outcome are measured relative to the baseline period of 0 to 1 years before the introduction of the income tax. We report the average treatment effect on the treated. The treatment effect is multiplied by 100 to increase readability. The estimators use inverse probability weighting, where the propensity score is estimated by probit and is a function of year effects, five lags of log population, and the first two lagged first differences of the outcome variable. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: The Effect of Tax Broadening on Expenditure per Capita

Years since tax broadening:	−5 to −4 (1)	−3 to −2 (2)	1 to 2 (3)	5 to 6 (4)	9 to 10 (5)	19 to 20 (6)	29 to 30 (7)
	Panel A: Log Expenditure per Capita						
Average effect of tax broadening	−1.35	−0.44	4.03	5.75	3.80	4.46	7.69**
	(1.16)	(0.52)	(3.00)	(3.81)	(4.35)	(3.94)	(3.77)
Observations	192	192	163	158	192	192	192
	Panel B: Log Education Expenditure per Capita						
Average effect of tax broadening	−0.67	0.26	8.60***	1.70	−1.11	−1.33	3.50
	(1.97)	(1.04)	(2.66)	(4.69)	(6.23)	(6.77)	(6.27)
Observations	96	96	96	96	96	96	96
	Panel C: Log Health Expenditure per Capita						
Average effect of tax broadening	−3.20	−2.59	−1.02	3.04	0.52	−4.51	1.61
	(3.62)	(2.40)	(6.60)	(8.92)	(9.22)	(8.16)	(11.45)
Observations	96	96	96	96	96	96	96
	Panel D: Log Safety Expenditure per Capita						
Average effect of tax broadening	−0.08	0.94	−6.44	−11.98	−19.95	−8.21	−12.76
	(1.78)	(0.95)	(14.40)	(14.32)	(16.33)	(12.62)	(9.67)
Observations	96	96	96	96	96	96	96

Notes: This table presents semiparametric difference-in-differences estimates of the average effect of tax broadening (i.e., the introduction of the income tax) over different time horizons. Changes in the outcome are measured relative to the baseline period of 0 to 1 years before the introduction of the income tax. We report the average treatment effect on the treated. The treatment effect is multiplied by 100 to increase readability. The estimators use inverse probability weighting, where the propensity score is estimated by probit and is a function of year effects, five lags of log population, and the first two lagged first differences of the outcome variable. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

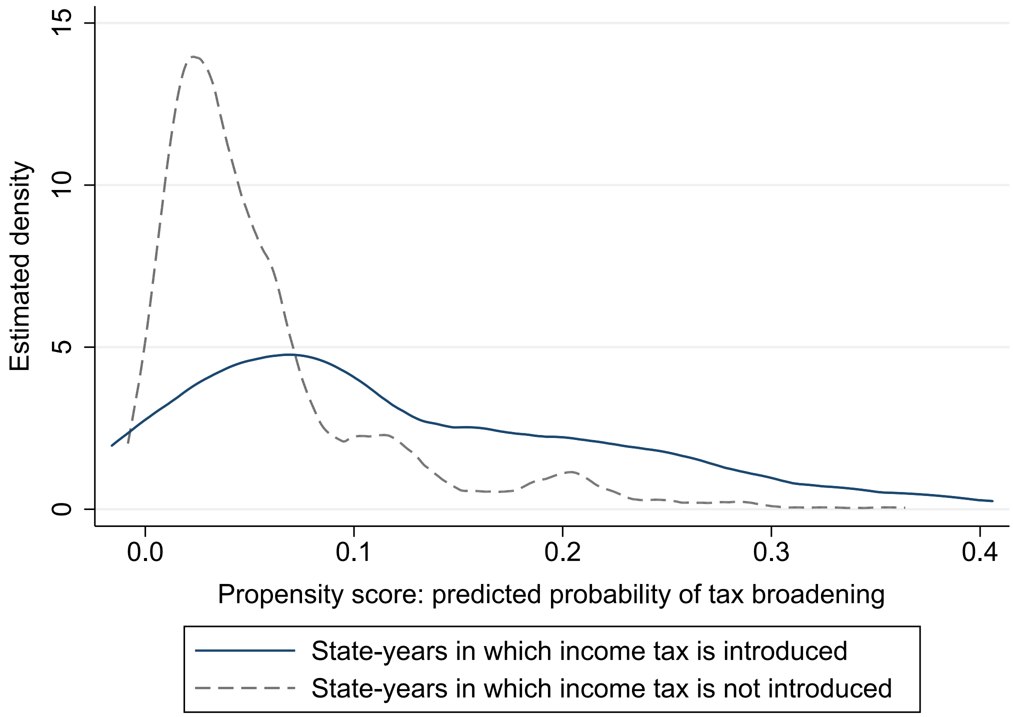
Table 5: The Effect of Tax Differentials on Outmigration

	All Outmigration Flows				Flows with Top Earners, by Income		
	1900-2010		1940-2010		All	$<$ p95	$\geq$ p95
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Child-Based Measure						
Post Income Tax (O $-$ D)	0.128***	0.134***	0.180***	0.178***	0.117***	0.109**	0.252***
	(0.036)	(0.042)	(0.040)	(0.046)	(0.041)	(0.044)	(0.059)
Log(1 $-$ Top MTR) (D $-$ O)		$-$0.141		0.267	1.305**	1.443**	1.826**
		(0.440)		(0.440)	(0.516)	(0.563)	(0.713)
Observations	18,269	17,669	14,065	13,464	3,645	3,645	3,645
	Panel B: Report-Based Measure						
Post Income Tax (O $-$ D)			0.205***	0.196***	0.188***	0.183***	0.312***
			(0.047)	(0.051)	(0.039)	(0.040)	(0.055)
Log(1 $-$ Top MTR) (D $-$ O)				0.552	0.988**	0.991**	1.769**
				(0.488)	(0.465)	(0.480)	(0.763)
Observations			13,617	12,731	7,011	7,011	7,011

Notes: This table reports estimates of θ and η in Equation (8). In Panel A, the sample consists of origin-destination-years with non-zero migration flows of households with a child aged four or five at the time of the census, and five-year moves are identified by comparing the household state of residence to the child's birth state. In Panel B, the sample consists of origin-destination-years with non-zero migration flows of households that reported their state of residence five years before the census, and five-year moves are identified by comparing the household state of residence to the household's previous state of residence. The last three columns are based on flows with at least one moving household in the top 5 percent of the observed income distribution in the current year. These flows are for the years 1940-2010, when we observe household income. 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 one after the state introduces an individual income tax. *Log(1 $-$ Top MTR) (D $-$ O)* is the difference between the destination's and origin's log net-of-tax rate, which is based on the top marginal income tax rate. All regressions include origin-destination fixed effects and year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and three-way clustering at the origin-destination pair, origin $\times$ year, and destination $\times$ year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

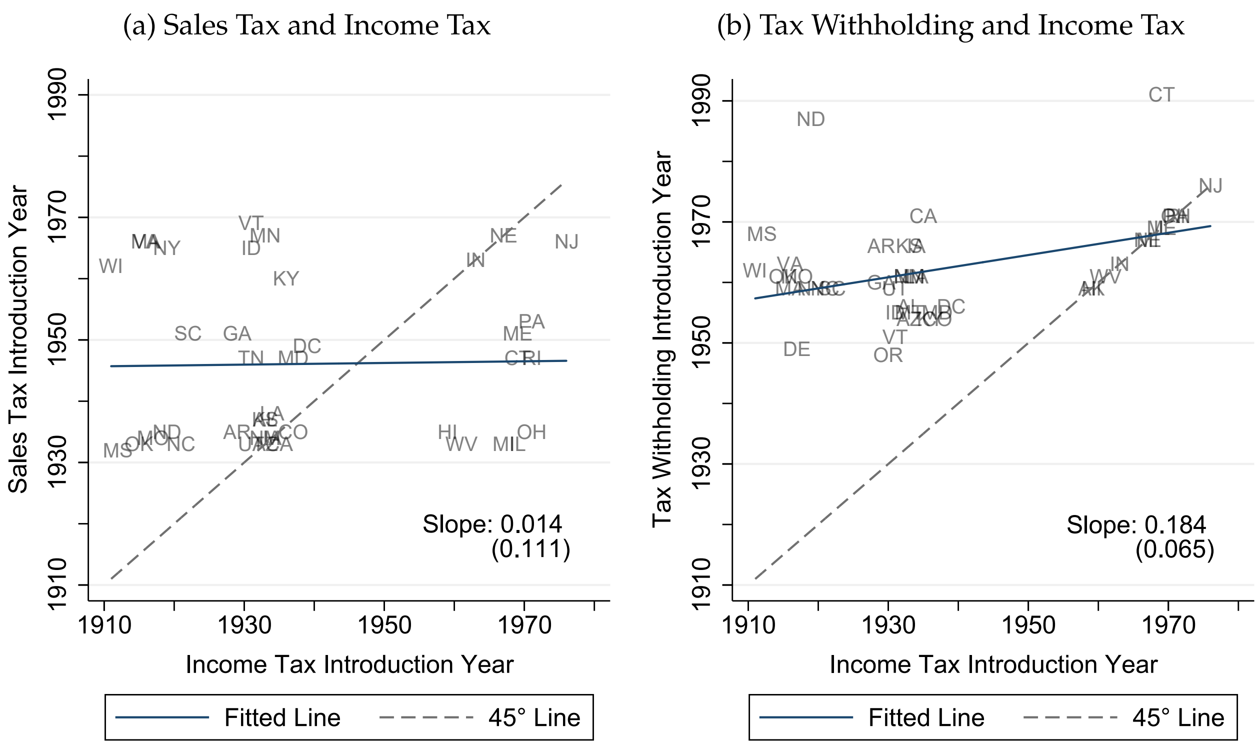
Online Appendix

Figure A.1: Density of Estimated Propensity Scores



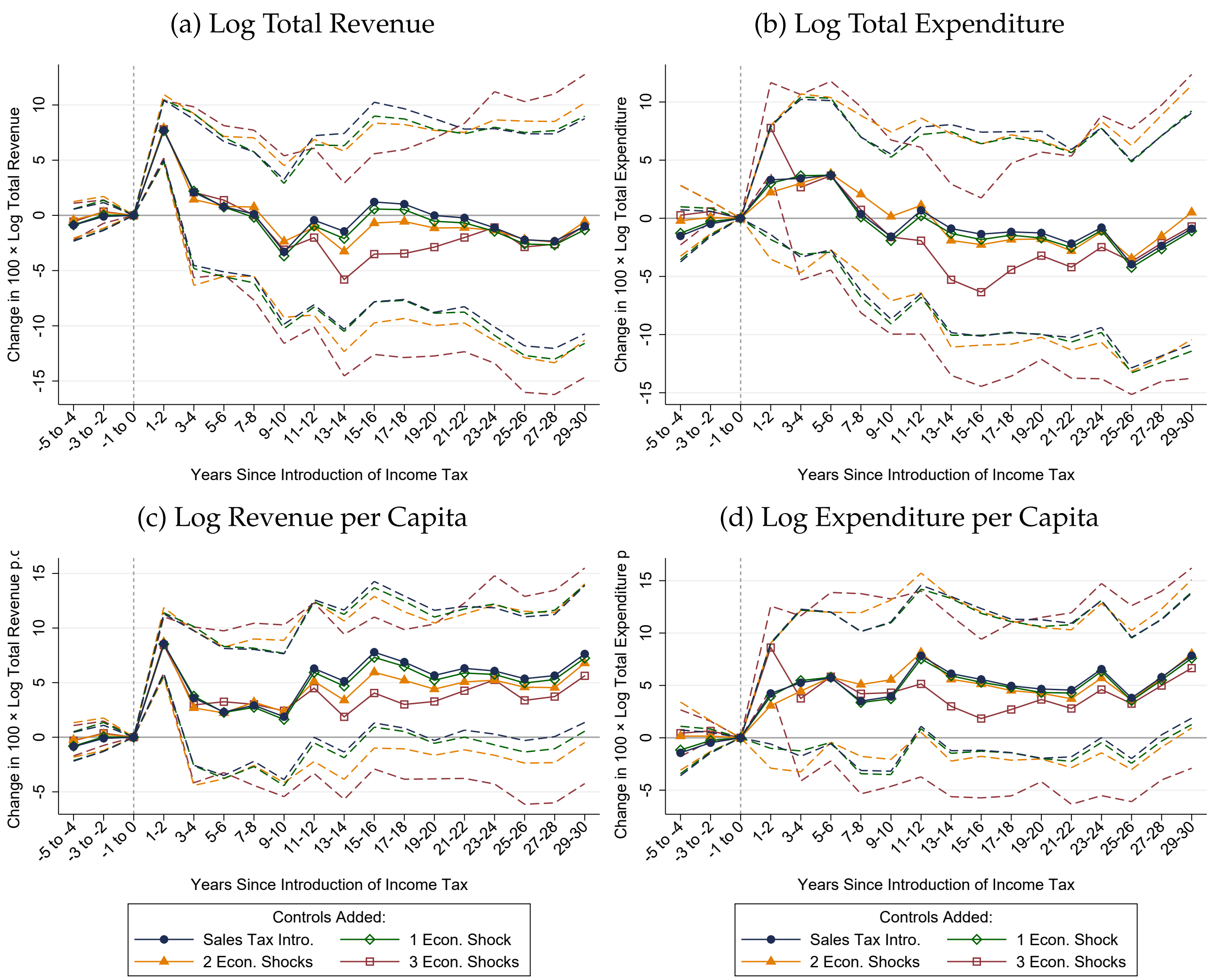
Notes: This figure plots the density of the estimated probability of introducing the income tax in year t conditional on not having the income tax in year $t-1$. The solid line plots the density for state-years in which the income tax was introduced, and the dashed line plots the density for state-years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

Figure A.2: Timing of Sales Tax, Income Tax, and Tax Withholding Introductions



Notes: This figure plots the year of introduction of the sales tax (Panel (a)) and the year of introduction of tax withholding (Panel(b)) against the year of introduction of the income tax. The solid line plots the fitted values from a univariate regression, and the dashed line plots points corresponding to the simultaneous introduction of the two policies.

Figure A.3: Dynamic Effects of Tax Broadening on Revenue and Expenditure: Robustness

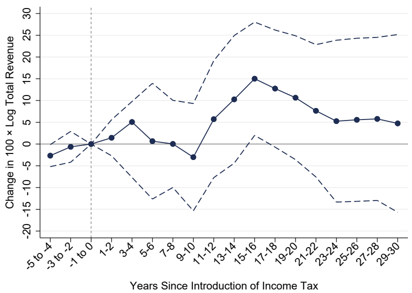


Notes: This figure plots semiparametric difference-in-differences estimates of the effect of the introduction of the income tax on the change in 100 $\times$ log revenue and expenditure, in non-per capita and per capita terms, over different time horizons. Lagged differences and forward differences are defined in terms of two-year periods to reduce the influence of missing data. The solid line plots the average treatment effect on the treated, and the dashed lines plot 90 percent confidence intervals. The x-axis measures time periods relative to the introduction 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, five lags of log population, and the first two lagged first differences of the outcome variable as covariates. Additional covariates are added to the propensity score as indicated in the figure. The sales tax control is a dummy variable for the introduction of the sales tax within 10 years (past or future). The economic shocks are lagged first differences of log state personal income per capita. Confidence intervals are robust to heteroskedasticity and clustering at the state level.

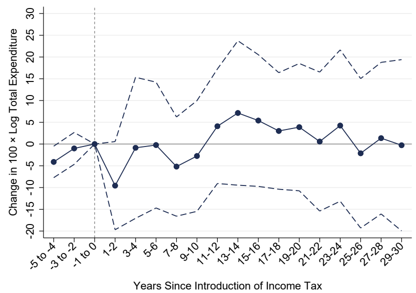
Figure A.4: The Effect of Tax Broadening on Revenue and Expenditure: Heterogeneity by Adoption Year

Panel A: Adoption Years: 1911-1940

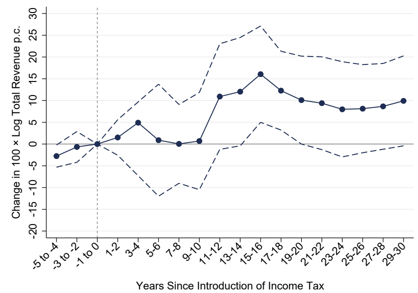
(a) Log Total Revenue



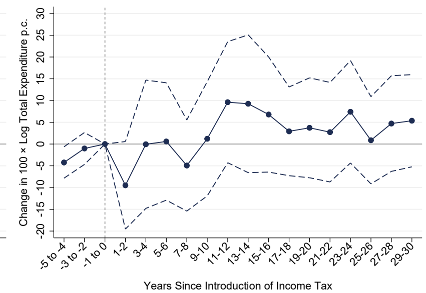
(b) Log Total Expenditure



(c) Log Revenue per capita

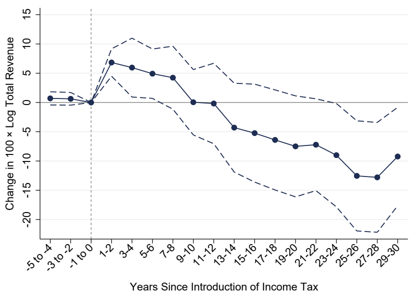


(d) Log Expenditure per capita

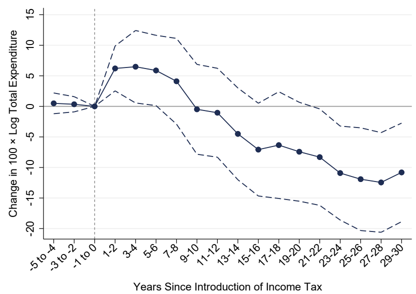


Panel B: Adoption Years: 1941-1976

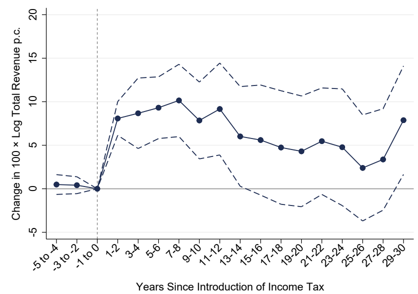
(e) Log Total Revenue



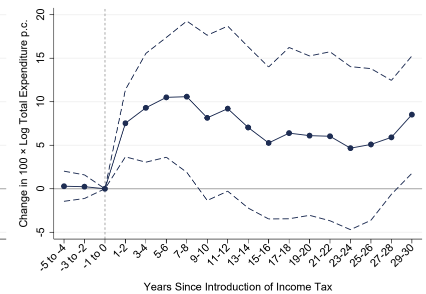
(f) Log Total Expenditure



(g) Log Revenue per capita



(h) Log Expenditure per capita

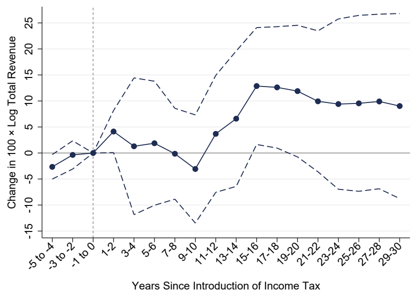


Notes: This figure plots semiparametric difference-in-differences estimates of the effect of the introduction of the income tax on the change in 100 $\times$ log revenue and expenditure, in non-per capita and per capita terms, over different time horizons. Lagged differences and forward differences are defined in terms of two-year periods to reduce the influence of missing data. Panel A plots the results for the years 1940 and earlier, and Panel B plots the results for the years 1941 and later. The solid line plots the average treatment effect on the treated, and the dashed lines plot 90 percent confidence intervals. The x-axis measures time periods relative to the introduction 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, five lags of log population, and the first two lagged first differences of the outcome variable as covariates. Confidence intervals are robust to heteroskedasticity and clustering at the state level.

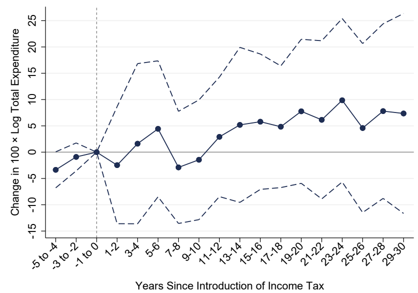
Figure A.5: The Effect of Tax Broadening on Revenue and Expenditure: Heterogeneity by Presence of Tax Withholding

Panel A: Income Tax Adopted before Withholding

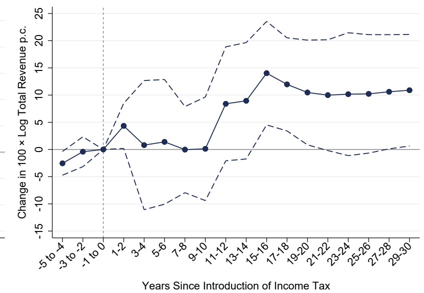
(a) Log Total Revenue



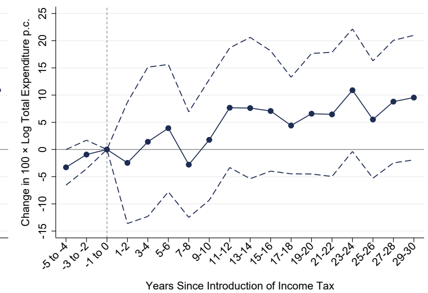
(b) Log Total Expenditure



(c) Log Revenue per capita

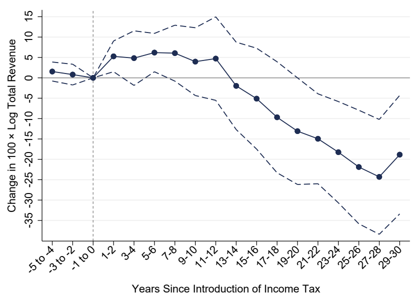


(d) Log Expenditure per capita

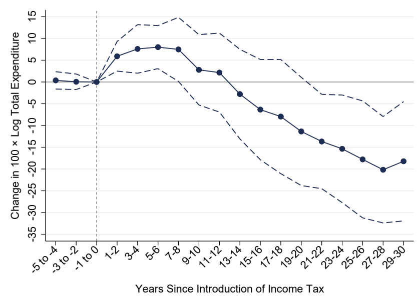


Panel B: Income Tax Adopted Simultaneously with Withholding

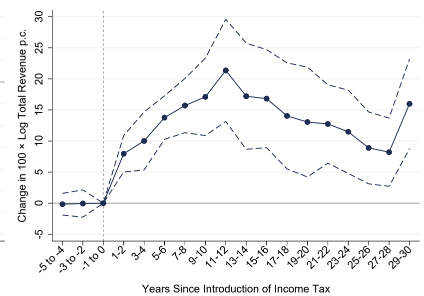
(e) Log Total Revenue



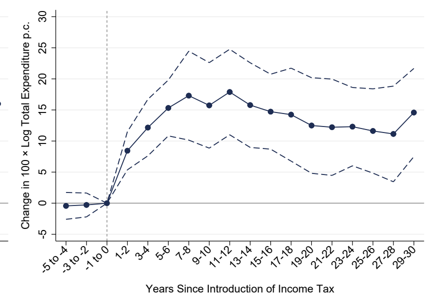
(f) Log Total Expenditure



(g) Log Revenue per capita

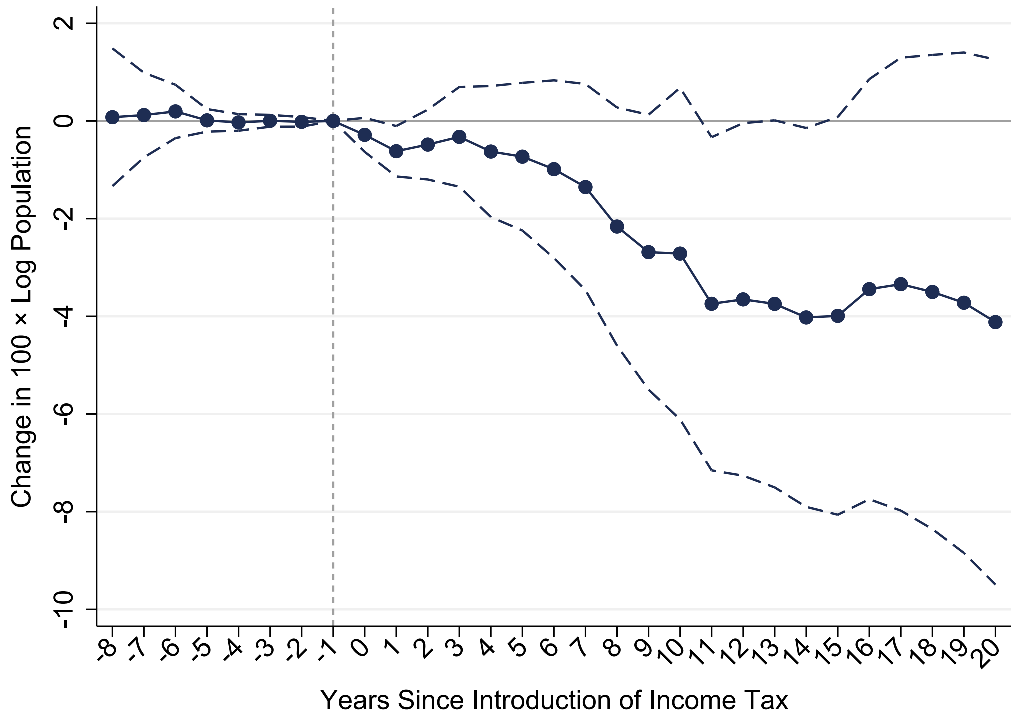


(h) Log Expenditure per capita



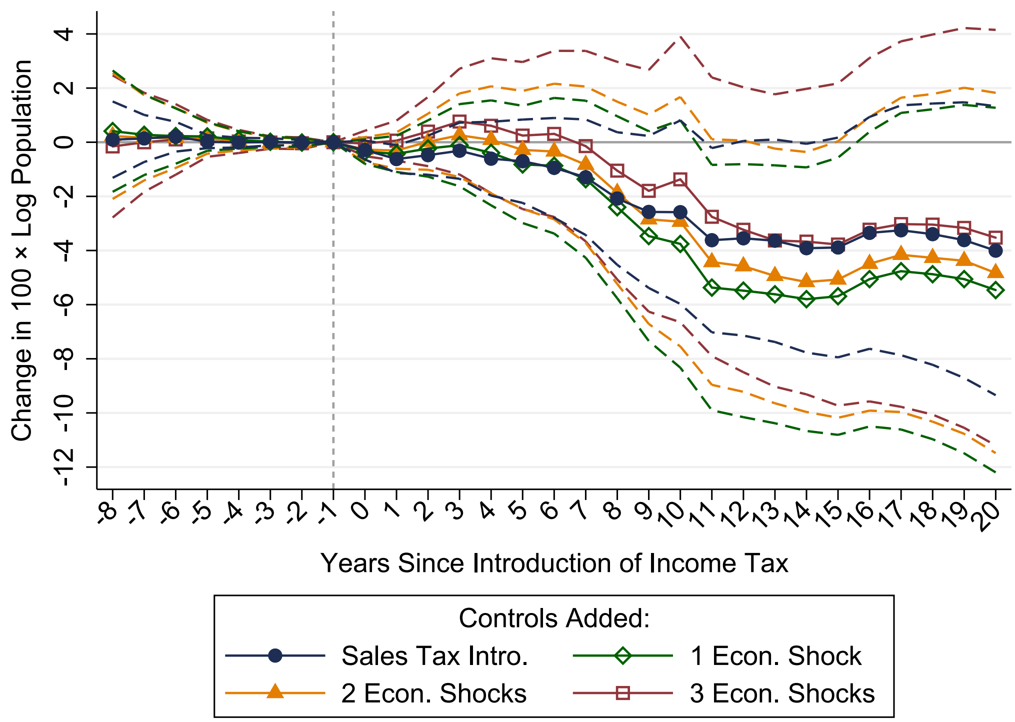
Notes: This figure plots semiparametric difference-in-differences estimates of the effect of the introduction of the income tax on the change in 100 $\times$ log revenue and expenditure, in non-per capita and per capita terms, over different time horizons. Lagged differences and forward differences are defined in terms of two-year periods to reduce the influence of missing data. Panel A plots the results for the years 1940 and earlier, and Panel B plots the results for the years 1941 and later. The solid line plots the average treatment effect on the treated, and the dashed lines plot 90 percent confidence intervals. The x-axis measures time periods relative to the introduction 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, five lags of log population, and the first two lagged first differences of the outcome variable as covariates. Confidence intervals are robust to heteroskedasticity and clustering at the state level.

Figure A.6: Dynamic Effects of Tax Broadening on State Population



Notes: This figure plots semiparametric difference-in-differences estimates of the effect of the introduction of the income tax on the change in 100 $\times$ log population over different time horizons. The solid line plots the average treatment effect on the treated, and the dashed lines plot 90 percent confidence intervals. The x-axis measures years relative to the introduction 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 and five lags of log population as covariates. Confidence intervals are robust to heteroskedasticity and clustering at the state level.

Figure A.7: Dynamic Effects of Tax Broadening on State Population: Robustness

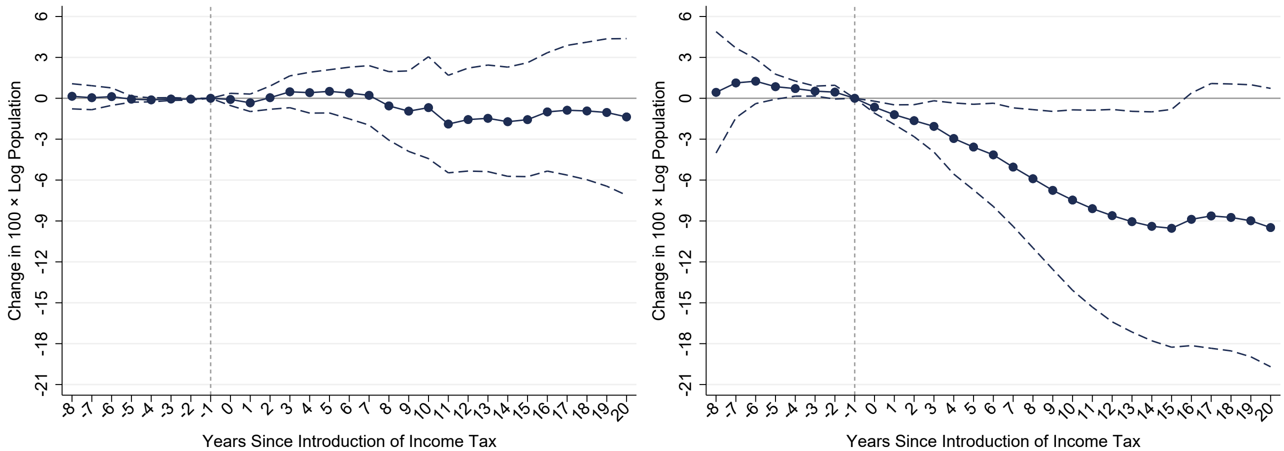


Notes: This figure plots semiparametric difference-in-differences estimates of the effect of the introduction of the income tax on the change in 100 $\times$ log population over different time horizons. The solid line plots the average treatment effect on the treated, and the dashed lines plot 90 percent confidence intervals. The x-axis measures years relative to the introduction 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 and five lags of log population as covariates. Additional covariates are added to the propensity score as indicated in the figure. The sales tax control is a dummy variable for the introduction of the sales tax within 10 years (past or future). The economic shocks are lagged first differences of log state personal income per capita. Confidence intervals are robust to heteroskedasticity and clustering at the state level.

Figure A.8: The Effect of Tax Broadening on Population: Heterogeneity by Adoption Year

(a) Adoption Years: 1911-1940

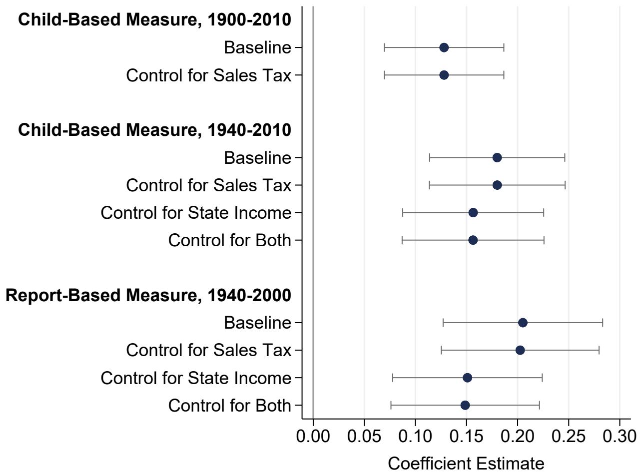
(b) Adoption Years: 1941-1976



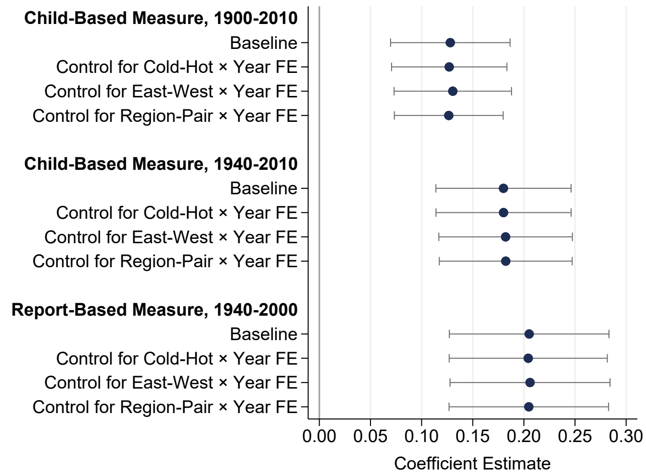
Notes: This figure plots semiparametric difference-in-differences estimates of the effect of the introduction of the income tax on the change in 100 × log population over different time horizons. Panel (a) plots the results for the years 1940 and earlier, and Panel (b) plots the results for the years 1941 and later. The solid line plots the average treatment effect on the treated, and the dashed lines plot 90 percent confidence intervals. The x-axis measures years relative to the introduction 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 and five lags of log population as covariates. Confidence intervals are robust to heteroskedasticity and clustering at the state level.

Figure A.9: The Effect of Tax Broadening on Outmigration: Robustness

(a) Policy and Economic Shocks



(b) Regional Shocks



Notes: This figure plots point estimates and 95 percent confidence intervals for θ in Equation (8) for different measures of outmigration, time periods, and sets of control variables. In Panel (a) 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 has a sales tax in year t, and D^S_{dt} is defined similarly for the destination state. The state income control is the difference between log personal income per capita in the destination state and log personal income per capita in the origin state. In Panel (b) the Cold-Hot indicator variable equals one for pairs of states containing one of the top 10 hottest states and one state not in the top 10, based on average maximum temperature in July from 1901 to 2000. The East-West indicator variable equals one for pairs of states containing one western state and one non-western state. Regions are defined according to the U.S. Census Bureau's four-region categorization.

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

State	Individual	Corporate	State	Individual	Corporate
Wisconsin	1911	1911	Louisiana	1934	1934
Mississippi	1912	1921	California	1935	1929
Oklahoma	1915	1931	Kentucky	1936	1936
Massachusetts	1916	1919	Colorado	1937	1937
Virginia	1916	1915	Maryland	1937	1937
Delaware	1917	1957	Washington, D.C.	1939	1939
Missouri	1917	1917	Alaska	1959-80	1959
New York	1919	1917	Hawaii	1959	1959
North Dakota	1919	1919	West Virginia	1961	1967
North Carolina	1921	1921	Indiana	1963	1963
South Carolina	1922	1922	Michigan	1967	1967-75
New Hampshire	1923	1970	Nebraska	1967	1967
Arkansas	1929	1929	Connecticut	1969	1915
Georgia	1929	1929	Illinois	1969	1969
Oregon	1930	1929	Maine	1969	1969
Idaho	1931	1931	Ohio	1971	1971-2005
Tennessee	1931	1923	Pennsylvania	1971	1935
Utah	1931	1931	Rhode Island	1971	1947
Vermont	1931	1931	New Jersey	1976	1958
Alabama	1933	1933	Florida	None	1971
Arizona	1933	1933	Nevada	None	None
Kansas	1933	1933	South Dakota	None	None
Minnesota	1933	1933	Texas	None	None
Montana	1933	1917	Washington	None	None
New Mexico	1933	1933	Wyoming	None	None
Iowa	1934	1934			

Notes: The source for 1902-1980 is Penniman (1980). We use administrative data to extend this source for 1980-2008. Note that a typo in Penniman (1980) identifies the Virginia's year of adoption of the individual income tax as 1961 instead of 1916.

Table A.2: Sources for Fiscal Data

Data Year	Source	Edition	Tables	Pages
1902	US Department of Commerce (1907)	1907	10	980-95
1903	US Department of Commerce (1915)	1915	7, 9	38-9, 42-3
1913	US Department of Commerce (1915)	1915	6, 8	36-7, 40-1
1915	US Census Statistical Abstract	1929	228	222
1917[a]	US Census Statistical Abstract	1939	224	220
1922	US Census Statistical Abstract	1929	228	222
	US Census Statistical Abstract	1939	224	220
1923	US Census Statistical Abstract	1924	185	199
1926	US Census Statistical Abstract	1928	222	216
1927	US Census Statistical Abstract	1930	228	223
	US Census Statistical Abstract	1939	224	220
1928	US Census Statistical Abstract	1931	218	224-5
1929	US Census Statistical Abstract	1931	218	224-5
1930	US Census Statistical Abstract	1933	202	201-2
1931	US Census Statistical Abstract	1933	202	201-2
1932	US Census Statistical Abstract	1934	204	202
	US Census Statistical Abstract	1939	224	220
1937	US Census Statistical Abstract	1939	223	218-9
	US Census Statistical Abstract	1939	224	220
1938	US Census Statistical Abstract	1941	231	240-1
1940	US Census Statistical Abstract	1941	234	243
	US Census Statistical Abstract	1942	234	248-9
1942-2008	Census of Governments	N/A	N/A	N/A

Notes: Census of Governments data are available biannually from 1942 to 1950 and annually after 1950 at https://www.census.gov/programs-surveys/gov-finances/data/historical-data.html and the Statistical Abstracts are available at https://www.census.gov/library/publications/time-series/statistical_abstracts.html

[a] Property taxes only.

Table A.3: Summary Statistics

	Mean	Std. Dev.	Min.	Max.	Obs.
General					
Post Income Tax	0.57	0.49	0.00	1.00	5661
Bottom Marginal Income Tax Rate (%)	1.08	1.35	0.00	6.35	5523
Top Marginal Income Tax Rate (%)	3.51	3.92	0.00	19.80	5523
Personal Exemption, Single Filer	6.88	6.40	0.00	40.59	2648
Outcomes (Non Per Capita)					
Population (millions)	3.51	4.21	0.04	37.32	5561
Total Revenue	12.31	22.84	0.01	312.27	3958
Total Taxes	6.27	10.42	0.00	119.65	3622
Total Income Taxes	2.28	5.26	0.00	67.60	3523
Property Tax Revenue	0.15	0.40	0.00	5.06	3910
Total Expenditure	11.58	21.11	0.01	246.68	3908
Education Expenditure	4.14	6.70	0.00	74.74	3428
Public Safety Expenditure	0.44	0.83	0.00	10.47	3427
Health Expenditure	0.39	0.90	0.00	11.99	3414
2-Year Colleges	8.04	13.51	0.00	105.00	5559
4-Year Colleges	6.71	5.25	0.00	36.00	5559
Outcomes (Per Capita)					
Total Revenue p.c.	2822.00	2675.50	17.30	29797.37	3958
Total Taxes p.c.	1392.25	1060.53	14.45	13447.59	3622
Total Income Taxes p.c.	444.88	487.97	0.00	5235.59	3523
Property Tax Revenue p.c.	63.08	200.02	0.00	3668.79	3910
Total Expenditure p.c.	2645.20	2321.57	16.31	18615.11	3908
Education Expenditure p.c.	925.72	651.52	0.53	4300.75	3428
Public Safety Expenditure p.c.	105.76	168.62	0.00	2112.83	3427
Health Expenditure p.c.	81.53	94.45	0.00	925.38	3414
2-Year Colleges p.c. (per 1 million)	2.42	2.71	0.00	21.28	5445
4-Year Colleges p.c. (per 1 million)	3.25	2.64	0.00	24.19	5445

Notes: The personal exemption is measured in 2008 USD thousands, non per capita fiscal outcomes are measured in 2008 USD billions, and per capita fiscal outcomes are measured in 2008 USD per capita.

Table A.4: The Effect of Tax Broadening on State Population

Years since tax broadening:	-8 to -5 (1)	-4 to -1 (2)	0 to 3 (3)	4 to 7 (4)	8 to 11 (5)	12 to 15 (6)	16 to 19 (7)
	Panel A: Inverse probability weighting using 3 lags of log population						
Average effect of tax broadening	0.03	0.04	-0.46	-0.93	-2.81	-3.79	-3.39
	(0.75)	(0.08)	(0.40)	(1.13)	(1.93)	(2.51)	(3.11)
Observations	731	731	731	731	731	731	731
	Panel B: Inverse probability weighting using 4 lags of log population						
Average effect of tax broadening	-0.23	-0.01	-0.38	-0.75	-2.53	-3.41	-2.91
	(0.65)	(0.05)	(0.39)	(1.10)	(1.90)	(2.44)	(3.02)
Observations	731	731	731	731	731	731	731
	Panel C: Inverse probability weighting using 5 lags of log population						
Average effect of tax broadening	0.10	-0.01	-0.43	-0.92	-2.83	-3.85^{*}	-3.50
	(0.43)	(0.05)	(0.36)	(1.01)	(1.79)	(2.32)	(2.87)
Observations	731	731	731	731	731	731	731

Notes: This table presents semiparametric difference-in-differences estimates of the average effect of tax broadening (i.e., the introduction of the income tax) on log state population over different time horizons. We report the average treatment effect on the treated. The treatment effect is multiplied by 100 to increase readability. The estimators use inverse probability weighting, where the propensity score is estimated by probit and is a function of year effects and three, four, or five lags of log population, as indicated. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Effect of Tax Differentials on Outmigration (Alternative Imputations)

	All Outmigration Flows			
Years with imputed tax rates:	None (1)	1930-1940 (2)	1920-1940 (3)	1911-1940 (4)
	Panel A: Child-Based Measure			
Post Income Tax (O − D)	0.173***	0.168***	0.142***	0.134***
	(0.048)	(0.046)	(0.043)	(0.042)
Log(1 − Top MTR) (D − O)	0.245	0.148	−0.124	−0.141
	(0.451)	(0.444)	(0.444)	(0.440)
Observations	16,199	16,813	17,417	17,669
	Panel A: Report-Based Measure			
Post Income Tax (O − D)	0.165***	0.196***	0.196***	0.196***
	(0.053)	(0.051)	(0.051)	(0.051)
Log(1 − Top MTR) (D − O)	0.553	0.552	0.552	0.552
	(0.484)	(0.488)	(0.488)	(0.488)
Observations	11,246	12,731	12,731	12,731

Notes: This table reports estimates of θ and η in Equation (8) using differing degrees of imputation for the income tax rate. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: The Effect of Tax Differentials on Outmigration (Alternative Income Cutoffs)

	Flows with 90th Percentile Earners			Flows with 99th Percentile Earners		
	All	< p90	≥ p90	All	< p99	≥ p99
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Child-Based Measure					
Post Income Tax (O − D)	0.144***	0.141***	0.261***	0.097	0.096	0.281**
	(0.040)	(0.044)	(0.053)	(0.062)	(0.066)	(0.111)
Log(1 − Top MTR) (D − O)	1.317***	1.481***	1.690**	0.782	0.950	1.154
	(0.449)	(0.500)	(0.697)	(0.887)	(0.954)	(1.334)
Observations	5,397	5,397	5,397	1,057	1,057	1,057
	Panel B: Report-Based Measure					
Post Income Tax (O − D)	0.174***	0.166***	0.288***	0.158***	0.156***	0.340***
	(0.042)	(0.043)	(0.056)	(0.041)	(0.042)	(0.070)
Log(1 − Top MTR) (D − O)	0.932**	0.950**	1.525**	1.267**	1.245**	2.263**
	(0.456)	(0.469)	(0.732)	(0.571)	(0.579)	(1.059)
Observations	8,793	8,793	8,793	2,998	2,998	2,998

Notes: This table reports estimates of θ and η in Equation (8). In Panel A, the sample consists of origin-destination-years with non-zero migration flows of households with a child aged four or five at the time of the census, and five-year moves are identified by comparing the household state of residence to the child's birth state. In Panel B, the sample consists of origin-destination-years with non-zero migration flows of households that reported their state of residence five years before the census, and five-year moves are identified by comparing the household state of residence to the household's previous state of residence. The last three columns are based on flows with at least one moving household in the top 5 percent of the observed income distribution in the current year. These flows are for the years 1940-2010, when we observe household income. 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 one after the state introduces an individual income tax. *Log(1 − Top MTR) (D − O)* is the difference between the destination's and origin's log net-of-tax rate, which is based on the top marginal income tax rate. All regressions include origin-destination fixed effects and year effects. Standard errors, reported 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 A.7: Marginal Effects of Population Lags on Probability of Tax Broadening

	Probability of Introducing Income Tax				
	(1)	(2)	(3)	(4)	(5)
Δ Log Population, $t-1$		-1.412^{***}	-1.408^{***}	-1.346^{***}	-1.463^{***}
		(0.519)	(0.523)	(0.507)	(0.510)
Δ Log Population, $t-2$			-0.232	-0.239	-0.322
			(0.353)	(0.368)	(0.357)
Δ Log Population, $t-3$				-0.270	-0.241
				(0.326)	(0.315)
Δ Log Population, $t-4$					0.364
					(0.444)
Log Population level effect	0.001	0.003	0.003	0.003	0.004
	(0.010)	(0.009)	(0.009)	(0.010)	(0.010)
Observations	731	731	731	731	731

Notes: This table presents estimated marginal effects from a probit model of the probability of introducing the income tax conditional on not having an income tax in the previous year. This probability is modeled as a function of year effects, lags 1 through $K-1$ of the change in local population, and lag K of the level of log population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Marginal Effects of Neighbor Adoption and Population Lags on Probability of Tax Broadening

	Measure of Neighbor Adoption of Tax			
	(1) Any Neigh.	(2) All Neigh.	(3) # Neigh.	(4) Frac. Neigh.
Neighbor Adoption of Tax	-0.034	-0.022	-0.005	-0.061
	(0.028)	(0.033)	(0.007)	(0.040)
Δ Log Population, $t-1$	-1.448^{***}	-1.404^{***}	-1.466^{***}	-1.387^{***}
	(0.495)	(0.502)	(0.503)	(0.493)
Δ Log Population, $t-2$	-0.362	-0.293	-0.355	-0.341
	(0.346)	(0.363)	(0.343)	(0.349)
Δ Log Population, $t-3$	-0.261	-0.207	-0.255	-0.225
	(0.316)	(0.310)	(0.311)	(0.316)
Δ Log Population, $t-4$	0.288	0.398	0.341	0.363
	(0.452)	(0.435)	(0.443)	(0.451)
Log Population level effect	0.005	0.003	0.004	0.003
	(0.010)	(0.010)	(0.009)	(0.010)
Observations	731	731	731	731

Notes: This table presents estimated marginal effects from a probit model of the probability of introducing the income tax conditional on not having an income tax in the previous year. This probability is modeled as a function of year effects, lags 1 through $K-1$ of the change in local population, and lag K of the level of log population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: The Effect of Tax Differentials on Outmigration: Heterogeneity by Proximity

	All Outmigration Flows					
	1900-2010			1940-2010		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Child-Based Measure					
Post Income Tax (O − D)	0.131***	0.130***	0.128***	0.183***	0.186***	0.197***
	(0.039)	(0.038)	(0.040)	(0.042)	(0.041)	(0.043)
Post Income Tax (O − D) × Neighbors	−0.024			−0.037		
	(0.071)			(0.085)		
Post Income Tax (O − D) × Same Region		−0.015			−0.059	
		(0.063)			(0.085)	
Post Income Tax (O − D) × Same Large Region			0.001			−0.079
			(0.049)			(0.058)
Observations	18,269	18,269	18,269	14,065	14,065	14,065
	Panel B: Report-Based Measure					
Post Income Tax (O − D)				0.208***	0.210***	0.218***
				(0.049)	(0.048)	(0.049)
Post Income Tax (O − D) × Neighbors				−0.050		
				(0.078)		
Post Income Tax (O − D) × Same Region					−0.066	
					(0.077)	
Post Income Tax (O − D) × Same Large Region						−0.063
						(0.062)
Observations				13,617	13,617	13,617

Notes: This table reports estimates of Equation (8) 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. In Panel A, the sample consists of origin-destination-years with non-zero migration flows of households with a child aged four or five at the time of the census, and five-year moves are identified by comparing the household state of residence to the child's birth state. In Panel B, the sample consists of origin-destination-years with non-zero migration flows of households that reported their state of residence five years before the census, and five-year moves are identified by comparing the household state of residence to the household's previous state of residence. The last three columns are based on flows with at least one moving household in the top 5 percent of the observed income distribution in the current year. These flows are for the years 1940-2010, when we observe household income. 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 one after the state introduces an individual income tax. *Log(1 − Top MTR) (D − O)* is the difference between the destination's and origin's log net-of-tax rate, which is based on the top marginal income tax rate. All regressions include origin-destination fixed effects and year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and three-way clustering at the origin-destination pair, origin × year, and destination × year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Deriving Mobility Responses to Tax Reforms

This section derives expressions for migration responses to intensive-margin and extensive-margin tax reforms. In the first subsection we calculate the elasticity of migration with respect to the net-of-tax rate, conditional on the presence of the income tax.[1] In the second subsection, we derive the migration response to the introduction of the income tax at some initial tax rate. In the final subsection, we examine how the response to the introduction of the income tax compares to the response to a large increase in the tax rate starting from a positive rate.

Intensive-Margin Reform

According to the model in Section 3.3.1, 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 effect of corporate taxes to simplify notation.[2] 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

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

[1] Some of the calculations in the first subsection are similar to those in Kleven et al. (2013).

[2] Recall that γ_{od} captures the value of amenities, cost of living, and moving costs. The year effects, ϕ_t, from Equation (8) factor out of the above expression.

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

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

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 report-based 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,

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

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 0.849 using the child-based measure of migration and 0.904 using the report-based measure, so the outmigration elasticity is close to $-\eta$.

Extensive-Margin Reform

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

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

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.[3] We therefore use the approximation

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

This approximation slightly overstates the negative effect of tax broadening 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 $\Delta_{odt}^{0,\tau}$ 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

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

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[4]

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

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

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

Tax Introduction vs. Large Rate Increase

Finally, we compare the effect of the introduction of the income tax to the effect of a large increase in the tax rate starting from a positive rate. Consider two tax rates, τ_1 and τ_2, where $\tau_1 < \tau_2$ and the difference between the two is "large." Using calculations similar to those in the previous subsection, it is straightforward to show that the percentage change in inmigration due to increasing the tax rate from τ_1 to τ_2 is approximately $\Delta^{\tau_1,\tau_2} \equiv \exp\left(\eta \log \frac{1-\tau_2}{1-\tau_1}\right) - 1$.

A natural question is how large τ_2 would have to be in order for the tax rate increase to have the same effect on inmigration as the introduction of the tax at rate τ_1. Setting $\Delta^{0,\tau_1} = \Delta^{\tau_1,\tau_2}$ yields $\tau_2 = 1 - \exp(-\theta/\eta + 2\log(1-\tau_1))$. Introducing the income tax at a rate of 1 percent causes inmigration to fall by 12 percent, according to the estimates of θ and η from the specification using the child-based measure and all flows containing top earners. Starting from a rate of 1 percent, the state would have to raise the tax rate to 10 percent to achieve the same decline in inmigration.