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The Introduction of the Income Tax, Fiscal Capacity, and Migration: Evidence from US States[†]

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We evaluate how fiscal capacity and migration respond to the introduction of the individual income tax, drawing on new panel data on US states from 1900 to 2010. We find that the introduction of the income tax increased revenue per capita by 12 percent in the short term, 15 percent in the medium term, and 17 percent in the long term. The absolute level of revenue, however, did not significantly change over the long term for post–World War II adopters. To explain this, we show that the introduction of the income tax induced significant outmigration to non-income-tax states by middle- and high-earning households. (*JEL* H71, H73, N32, N42, N92, R23)

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

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

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broadening may even reduce revenue per capita. The extent to which introducing the income tax actually expands the state's fiscal capacity thus remains open to debate.

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

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

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

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

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

To be clear, our results concern state-level tax policy only. They do not speak to what the long-run revenue response to a large cut in or the elimination of the federal income tax might be, a scenario in which the costs of "exit" via migration to a lower-tax jurisdiction would likely be much greater. In a similar vein, they

are unlikely to directly apply to potential migration responses to major changes in income tax policy in supranational federations such as the European Union, a context in which languages, cultures, and institutional structures vary significantly across jurisdictions.

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

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

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

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

A growing literature examines both the determinants (Besley and Persson 2013; Kleven, Kreiner, and Saez 2016; Gillitzer 2017; Jensen 2022) and the economic and political consequences of state capacity (Gordon and Li 2009; Dincecco and Prado 2012; Acemoglu, García-Jimeno, and Robinson 2015; Casaburi and Troiano 2016; Dell, Lane, and Querubin 2018). Historical accounts indicate that the development of the state's fiscal capacity was a hard-fought process (Dincecco 2011; Gennaioli and Voth 2015; Hoffman 2015). Our study sheds new light on a key mechanism—the

introduction of the income tax—through which governments have increased their capacity to extract revenue. To our knowledge, this is the first paper to systematically analyze the introduction of the state-level income tax across the United States. Furthermore, we address a novel question in this literature: to what extent does out-migration limit the impact of fiscal capacity investments?

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

Our study proceeds as follows. Section I provides the historical context. Section II describes the construction of our dataset. Section III discusses our empirical strategy, and Section IV presents the fiscal results. Section V examines how interstate migration responds to the introduction of the income tax. Section VI provides concluding remarks.

I. Introduction of the State-Level Individual Income Tax

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

Our analysis uses the year that the *individual* income tax was (permanently) introduced. For more than 60 percent of adopting states, the distinction between the individual and corporate income tax is immaterial, because both were introduced in the same year. For 75 percent of adopting states, the individual income tax and the corporate income tax were introduced within 3 years of each other.[2]

Figure 1 shows a map of continental US states shaded according to the decade of introduction of the individual income tax. Online Appendix Table A.1 lists the specific years of introduction.[3] Fourteen states scattered across the Midwest, Northeast, and South introduced income tax laws in the 1910s and 1920s. Eighteen additional states introduced the income tax in the 1930s. These states were scattered across every region, with the majority located west of the Mississippi River. No states introduced the individual income tax over the 1940s or 1950s. Seven more states introduced the individual income tax over the 1960s, and four more states over the

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

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

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

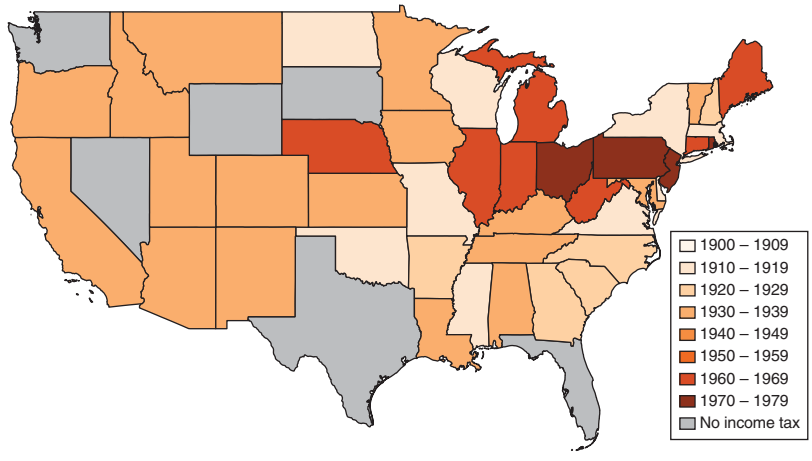

FIGURE 1. DECADE OF INTRODUCTION OF STATE-LEVEL INDIVIDUAL INCOME TAX

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

1970s. Many "late adopters" (i.e. post–World War II) were located in the Rust Belt. Six states never introduced a permanent individual income tax.[4]

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

Income tax introductions were historically contingent events, at least to an extent. Take, for example, the early efforts of two states, Wisconsin and Ohio, as described by Penniman (1980, chap. 1). Voters in both states approved constitutional amendments allowing for a state income tax—Wisconsin in 1908 and Ohio in 1912. Each referendum passed with a large majority. Wisconsin legislators established an income tax in 1911. This survived a legal challenge when the Wisconsin Supreme Court declined to overturn the law (Mehrotra 2013, chap. 4). In Ohio, by contrast, opponents of the

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

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

income tax blocked its passage in the legislature following the referendum. It was not until 1971 that Ohio, facing budgetary problems, introduced an income tax with bipartisan support (Penniman 1980, chap. 1).

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

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

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

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

II. Data

A. *Revenue and Expenditure*

For years prior to 1942, we hand coded state-level fiscal data from archival census reports. The earliest fiscal data are available for 1902 from the US Department of Commerce's "Wealth, Debt, and Taxation: 1902." Fiscal data are also available for 1903 and 1913 from the US Department of Commerce's "Wealth, Debt, and Taxation: 1913." State tax revenues in 1922 are provided by the US Department of

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

Commerce's "Wealth, Debt, and Taxation: 1922. Taxes Collected." We cover the rest of the pre-1942 period using the Statistical Abstract of the United States, which contains fiscal data for the years 1915, 1922–1932, 1937, 1938, and 1940. To our knowledge, we include all available state fiscal data prior to 1942. State fiscal data are available every two years from 1942 to 1948 and annually from 1950 to 2010 from the Census of Governments.

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

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

B. *Tax Rates*

For the years 1911 to 1940, we hand coded state-level income tax rates, brackets, and exemptions. For years prior to 1930, the National Industrial Conference Board (1930) provides a history of tax rates, brackets, and exemptions in each state that had an individual or corporate income tax by 1929, while the US Department of Commerce's "Digest of State Laws Relating to Taxation and Revenue: 1922" offers information on state tax codes in 1922. For the years 1930 to 1940, the National Tax Association provides annual reports that detail changes to state tax codes, while the US Department of Commerce's "Digest of State Laws Relating to Net Income Taxes: 1938" offers information on state income tax codes in 1938. We draw on state-specific historical summaries and contemporary newspaper accounts to cover any remaining details over this period. For 1941–2003, we take data on state income tax rates, brackets, and exemptions from the World Tax Database.[10] We extend these data to 2010 using information from the Tax Policy Center.[11]

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

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

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

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

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

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

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

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

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

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

C. *Sales Tax*

We take data on the years of adoption of the state-level sales tax from Gillitzer (2017) and Fox (2004). Gillitzer (2017) provides data on state sales tax rates for 1934 and 1938, and the World Tax Database provides them for 1946 to 2002. Data on state sales tax rates for 1975 to 2010 come from the *Book of the States* (Council of State Governments 1976–2011), digitized and compiled by Suárez Serrato and Zidar (2018).[15]

D. *Tax Withholding*

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

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

III. Empirical Strategy

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

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

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

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

Table 1—Summary Statistics for State-Level Outcomes

	Mean	SD	Min	Max	Observations
General					
Bottom marginal income tax rate (%)	1.31	1.39	0.00	6.35	4,655
Top marginal income tax rate (%)	4.19	3.95	0.00	19.80	4,655
Personal exemption, single filer	7.17	6.85	0.00	62.51	2,752
Sales tax rate (%)	2.45	2.23	0.00	8.25	4,655
Top total marginal tax rate (%)	63.61	21.71	15.00	95.00	4,655
Outcomes (*absolute*)					
Population (millions)	3.88	4.45	0.08	37.32	4,655
Total revenue	13.36	24.44	0.04	320.56	3,942
Total expenditure	12.64	23.06	0.04	260.61	3,942
Total tax revenue	7.05	11.29	0.02	122.83	3,470
Property tax revenue	0.17	0.42	0.00	5.19	3,890
Sales tax revenue	3.84	5.35	0.02	43.65	3,180
Outcomes (*per capita*)					
Total revenue per capita	2,816.60	2,346.85	54.27	20,856.64	3,942
Total expenditure per capita	2,664.26	2,223.69	50.43	22,924.83	3,942
Total tax revenue per capita	1,460.57	975.86	40.23	9,676.56	3,470
Property tax revenue per capita	66.12	208.34	0.00	3,101.81	3,890
Sales tax revenue per capita	785.65	410.18	95.16	2,478.67	3,180

Note: The personal exemption is measured in thousands of 2010 US dollars, absolute fiscal outcomes are measured in billions of 2010 US dollars, and per capita fiscal outcomes are measured in 2010 US dollars per capita.

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

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

To address this third challenge, we estimate a "stacked" difference-in-differences model (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker, Larcker, and Wang 2022). We construct separate, 50-year panel datasets for each adoption cohort c. Each dataset includes "treated" states that adopted the income tax in year c and "clean control" states that never had an income tax

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

during the 50-year window around year c. We combine the datasets and estimate the model

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

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

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

The OLS estimator for $\beta^{h,e}$ in equation (1) identifies an average of cohort-specific treatment effects for early adopting states, weighted by the size of the cohort-specific dataset and the variance of treatment status in the dataset (Gardner 2021). All weights are positive. The OLS estimator for $\beta^{h,\ell}$ identifies a similarly weighted average treatment effect for late adopting states. We report estimates of the weighted average effect $\omega_e \beta^{h,e} + \omega_\ell \beta^{h,\ell}$ where ω_e is the early-adopting share and ω_ℓ is the late-adopting share, as well as individual estimates of $\beta^{h,e}$ and $\beta^{h,\ell}$. We show in

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

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

Section IV that allowing the impact of the introduction of the income tax and the covariates to vary by cohort and taking the average of the cohort-specific treatment effects weighted by cohort size (as in Callaway and Sant'Anna 2021; Sun and Abraham 2021; and Wooldridge 2021) yields similar results.

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

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

IV. Main Results

A. *Selection into the Introduction of the Income Tax*

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

B. *Fiscal and Population Responses*

Table 3 reports the average fiscal responses to the introduction of the income tax over different time horizons. In the year of introduction and the following year, total revenue in absolute levels increases by 4.1 percent (SE $= 2.2$), and total expenditure in absolute levels increases by 4.1 percent (SE $= 2.5$) (panels A and B). In the two to three years after adoption, revenue increases by 10.7 percent (SE $= 3.1$), and expenditure increases by 10.5 percent (SE $= 3.9$). The fiscal responses decline by roughly half in years 4–9 after introduction to 5.4 percent and 5.7 percent, respectively. Revenue strongly rebounds over both the medium

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

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

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

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

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

run and long run, increasing by 13.0 percent (SE = 6.2) in years 10–19 after introduction and by 15.6 percent (SE = 6.9) in years 20–30. Expenditure rises by 12.4 percent (SE = 7.1) in the long run.

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

TABLE 3—AVERAGE EFFECTS OF INTRODUCTION OF INCOME TAX

	Average effect of income tax introduction over time				
Years since introduction:	0 to 1 (1)	2 to 3 (2)	4 to 9 (3)	10 to 19 (4)	20 to 30 (5)
Panel A. log total revenue					
All introductions	4.1	10.7	5.4	13.0	15.6
	(2.2)	(3.1)	(4.6)	(6.2)	(6.9)
	[−0.5, 9.2]	[4.1, 17.5]	[−4.2, 15.1]	[0.0, 25.8]	[0.9, 30.3]
Panel B. log total expenditure					
All introductions	4.1	10.5	5.7	8.2	12.4
	(2.5)	(3.9)	(4.6)	(6.7)	(7.1)
	[−1.5, 10.0]	[2.0, 19.2]	[−3.9, 15.4]	[−5.9, 22.3]	[−2.9, 27.9]
Panel C. log total revenue per capita					
All introductions	4.6	11.5	5.9	14.6	16.7
	(2.2)	(3.0)	(4.4)	(5.0)	(5.5)
	[−0.1, 9.6]	[4.9, 18.3]	[−3.4, 15.2]	[4.4, 24.8]	[5.2, 28.0]
Panel D. log total expenditure per capita					
All introductions	4.6	11.3	6.2	9.8	13.5
	(2.4)	(4.0)	(4.6)	(5.7)	(5.9)
	[−1.0, 10.4]	[2.6, 20.1]	[−3.5, 15.8]	[−2.2, 21.5]	[0.9, 26.2]
Panel E. log population					
All introductions	−0.1	−0.1	−0.5	−1.5	−1.1
	(0.4)	(0.8)	(1.7)	(3.6)	(5.1)
	[−1.0, 0.9]	[−1.7, 1.6]	[−4.2, 3.2]	[−9.2, 6.2]	[−11.6, 9.4]
Observations	9,122	9,118	9,165	9,118	8,854
States	36	36	36	36	36

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

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

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

Overall, these results indicate that the introduction of the income tax significantly increased total revenue in the near run, medium run, and long run, with a larger increase observed in per capita terms. The revenue and expenditure responses

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

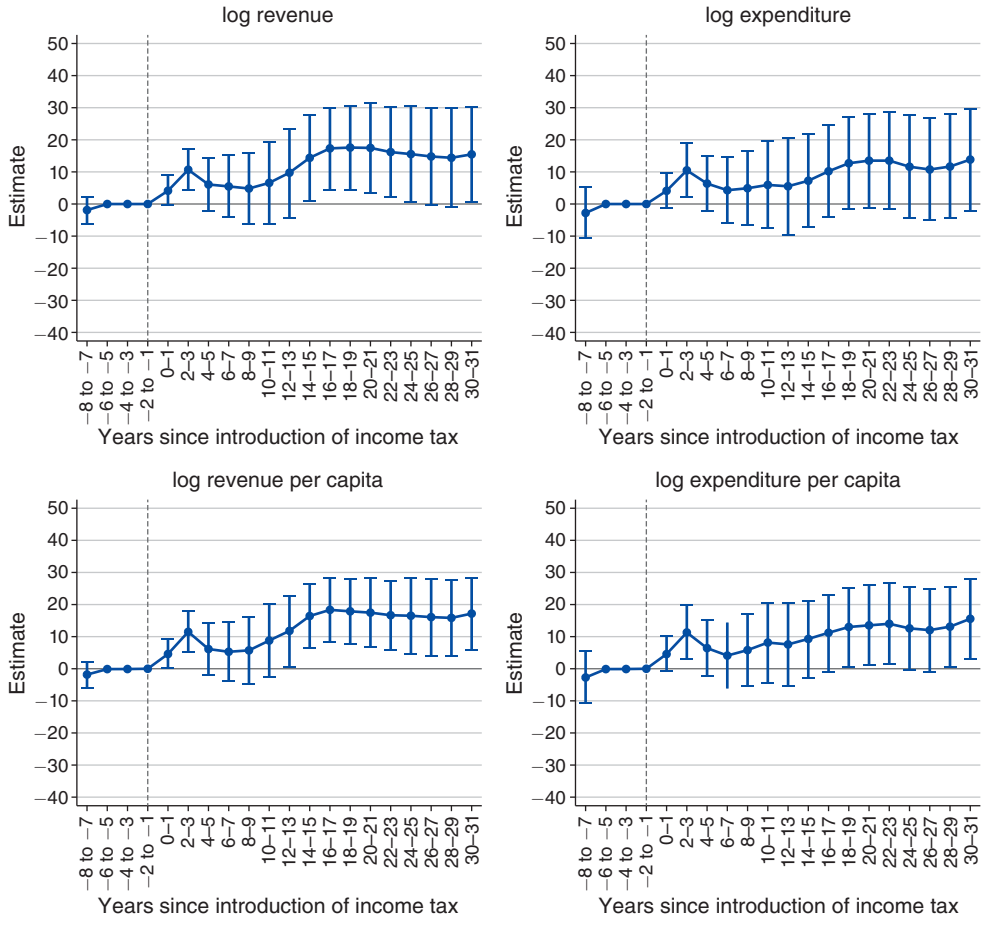

FIGURE 2. DYNAMIC EFFECTS OF INTRODUCTION OF INCOME TAX ON REVENUE AND EXPENDITURE

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

track each other over time. This is consistent with the widespread presence of balanced-budget requirements across states (Berry and Berry 1992).[22]

C. *Early versus Late Adopters*

Table 4 indicates that early (i.e., pre–World War II) and late (i.e., post–World War II) introductions of the income tax had different impacts on total revenue in absolute levels (panel A). Early adopters experienced a significant increase in revenue in the

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

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

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

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

near term (years 2–3), medium term (years 10–19), and long term (years 20–30). In the long term, revenue was 28.5 percent higher (SE = 10.2). Late adopters, by contrast, only experienced a significant increase in revenue over the first decade after introduction. The responses for total expenditure in absolute levels track those for revenue (panel B).

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

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

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

In the post–World War II era, by contrast, population fell significantly in states that introduced the income tax. Our estimates indicate that population fell by 11.6 percent (SE = 6.2) in the medium term (10–19 years) and by 16.6 percent (SE = 8.1) in the long term (20–30 years). Thus, revenue per capita increased more than revenue in absolute terms for late adopters. In combination, these results suggest that taxpayers left late-adopting states after the introduction of the income tax, preventing growth in the absolute size of state governments.[25,26]

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

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

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

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

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

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

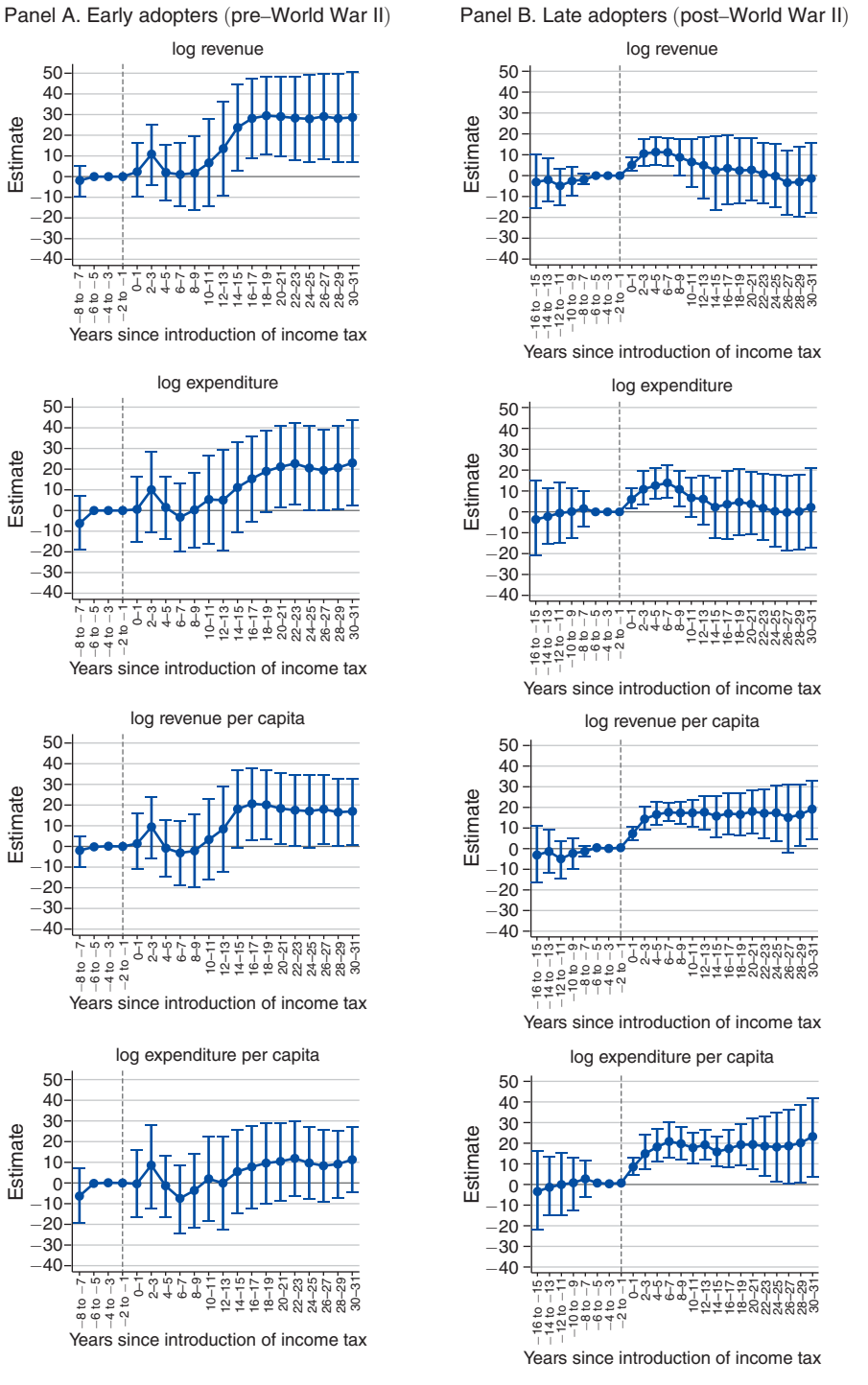

FIGURE 3. HETEROGENEOUS EFFECTS OF INTRODUCTION OF INCOME TAX BY TIME PERIOD

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

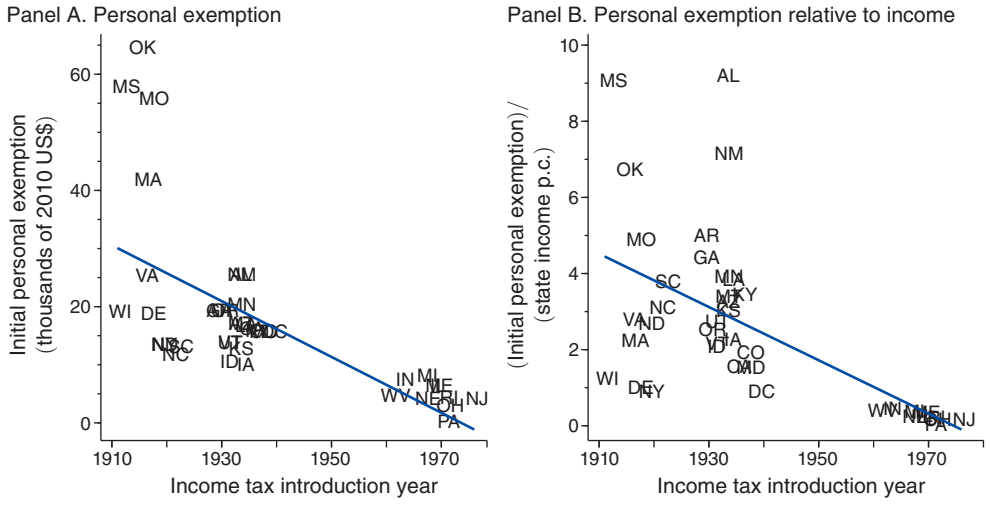

FIGURE 4. INITIAL BREADTH OF TAX BASE BY YEAR OF INTRODUCTION OF INCOME TAX

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

this point when we discuss our results for migration responses in online Appendix Section C.[28]

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

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

Second, late adopters had better tax administration. After the introduction of federal tax withholding in 1943, states introduced withholding in a staggered fashion from 1948 to 1987. Late adoptions of the income tax frequently overlapped with the introduction of tax withholding. Figure 5 shows a sharp increase in income tax revenue as a share of either total revenue or total taxes in the first few years after the introduction of the income tax among late adopters (i.e., post–World War II)—but not among early

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

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

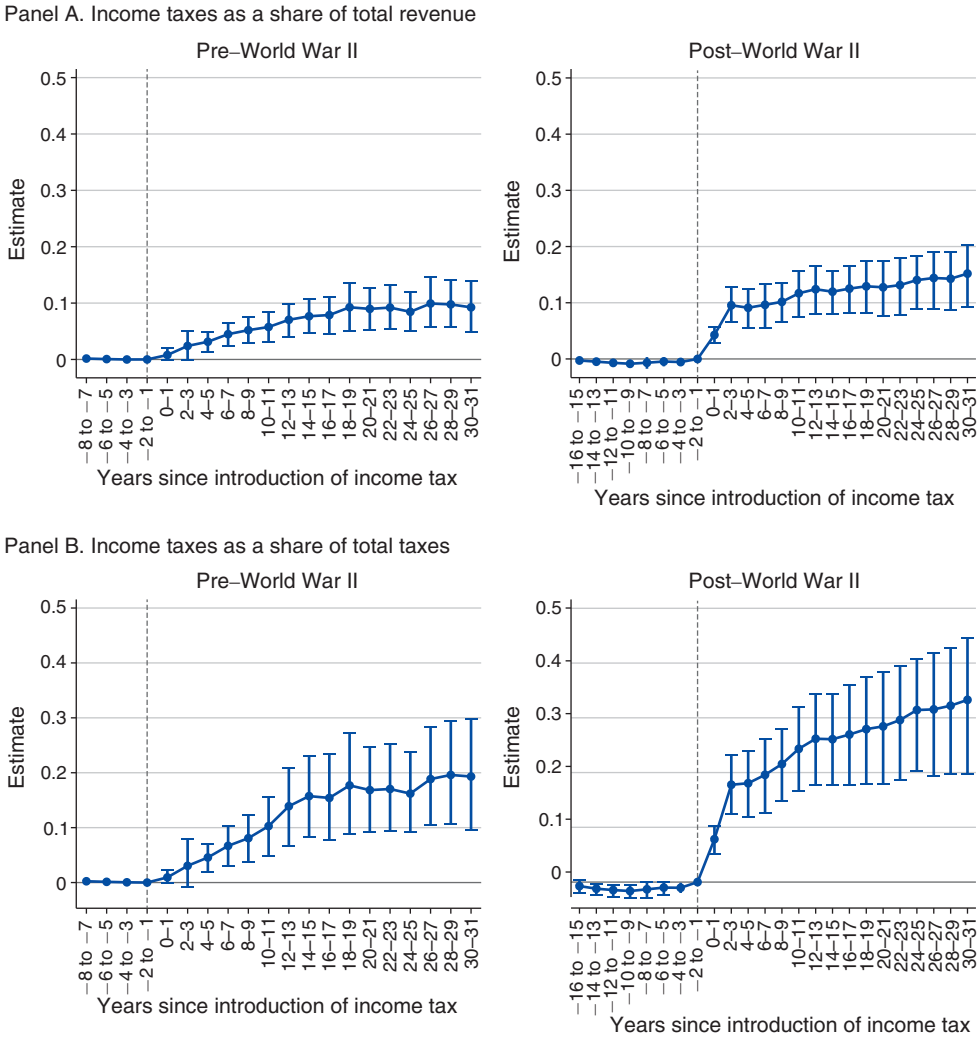

FIGURE 5. DYNAMIC EFFECTS OF INTRODUCTION OF INCOME TAX ON COMPOSITION OF REVENUE

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

adopters (i.e., pre–World War II), for whom this increase was relatively gradual. This is consistent with improved state tax administration in the post–World War II era. Furthermore, tax withholding increased revenue collection in states that already had an income tax (Bagchi and Dušek 2021).[30]

Third, improvements in transportation infrastructure over the second half of the twentieth century lowered the costs of moving. This was coupled with post–World War II increases in educational attainment. In tandem, these shifts promoted greater

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

geographic mobility for individuals aged 50 and younger after 1945 (Rosenbloom and Sundstrom 2004). Thus, location decisions may have become more sensitive to relative economic opportunities, including local tax environments.

D. *Crowd Out*

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

Figure 6 suggests that property tax revenue did not fall in either absolute or per capita terms after early introductions of the income tax (top panel). For late adopters, the absolute level of sales tax revenue fell by approximately 20 percent 2 to 3 decades after the introduction of the income tax, though the estimates are imprecise (middle panel, left). However, sales tax revenue per capita did not change by much after the introduction of the income tax (middle panel, right). Taken together, this suggests that the decline in sales tax revenue in absolute terms was due to falling population (recall panel E of Table 4), particularly since states did not significantly alter sales tax rates following the introduction of the income tax (bottom panel).[32]

E. *Robustness*

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

Economic Shocks.—Recent economic shocks might spur the adoption of the income tax while simultaneously impacting fiscal outcomes, thereby generating

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

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

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

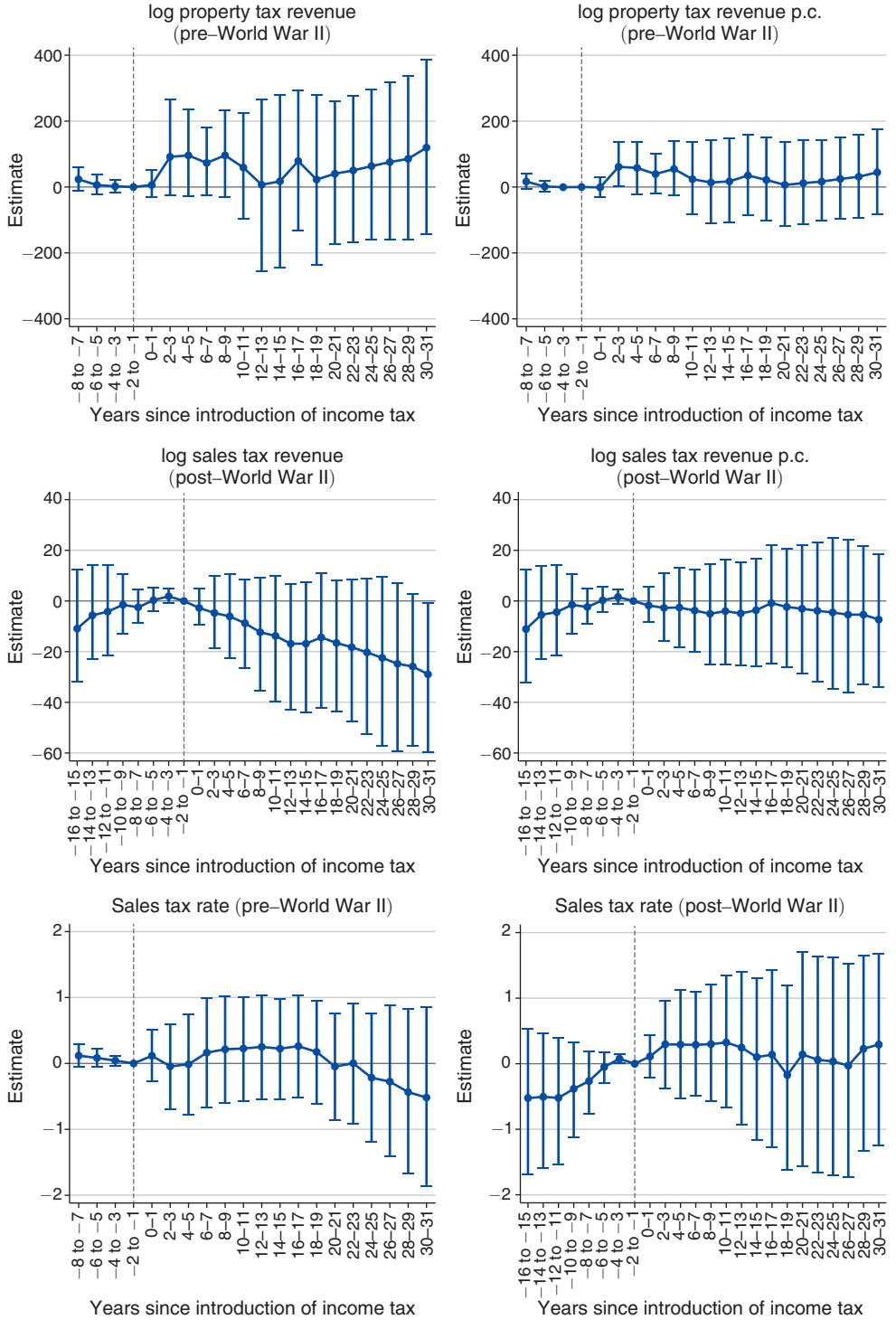

FIGURE 6. TESTING FOR CROWD-OUT EFFECTS OF INTRODUCTION OF INCOME TAX

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

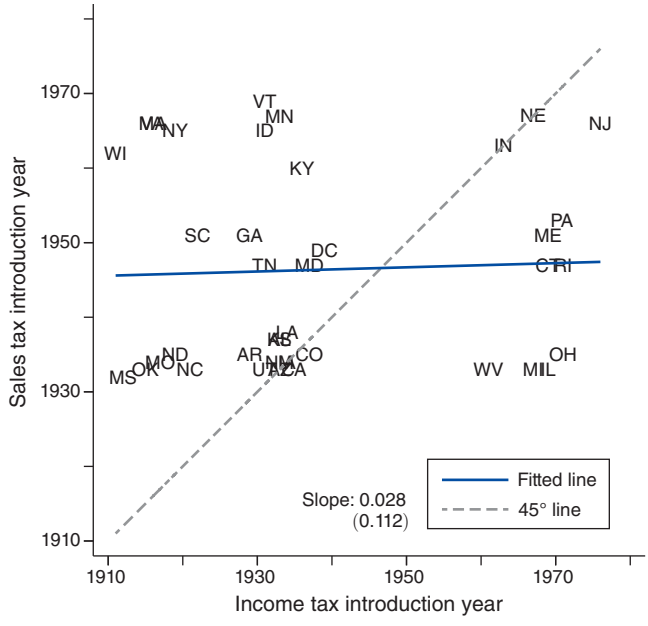

FIGURE 7. TIMING OF INTRODUCTION OF INCOME TAX VERSUS SALES TAX

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

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

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

Regional Shocks.—Late adopters of the income tax (i.e., post–World War II) were generally located in the Northeast and Midwest (Figure 1). Thus, regional shocks could be another source of bias. To address this concern, we control for arbitrary regional shocks by replacing the cohort-by-year effects with cohort-by-region-by-year

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

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

effects in equation (1). Online Appendix B.2 describes how we construct the regions for this analysis. Given the inherent limitations of this approach (due to the available variation in the data), it should be treated as somewhat speculative.

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

Average Cohort-Specific Treatment Effects.—As described in Section III, OLS estimation of equation (3) recovers a positively weighted average of cohort-specific treatment effects, where the weights depend on the size of the cohort-specific dataset and the variance of treatment status. For robustness, we estimate separate treatment effects for each cohort and take the average weighting by cohort size (Sun and Abraham 2021; Callaway and Sant'Anna 2021; Wooldridge 2021) rather than letting OLS determine the weighting of the treatment effects.

We use the model

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

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

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

Relaxing Functional Form Assumptions.—We relax the two (linear) functional form assumptions of equation (1) by controlling for covariates via inverse-probability weighting. We adapt the semiparametric approach in Angrist and Kuersteiner (2011) to a panel context, following Acemoglu et al. (2019) and Suárez Serrato and Wingender (2016). Our approach models the selection of states into the introduction of the income tax without specifying a parametric model for outcomes. Online Appendix B.3 describes the details of this empirical strategy. The results

remain quite similar to the baseline estimates (online Appendix Tables B.6 and B.7). Relative to the baseline, the IPW estimates display larger fiscal increases in response to the introduction of the income tax, as well as a larger population decline for late adopters (i.e., post–World War II).

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

V. Migration Responses

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

A. *Data: Migration*

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

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

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

[36] Historical data on births, deaths, and the number of women of reproductive age are from Bailey et al. (2018).

assumption that the child's birth state, which the census always reports, was the household's state of residence five years prior to the census year.

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

To study migration flows across different income levels, we use household income data, which are available for census years 1940 to 2010.[37] One drawback of the income data is that household income likely changes when a household moves, and income is only measured at the end of each migration period. We thus use an occupational earnings score in our baseline analysis. This corresponds to the median wage earned in the respondent's occupation.[38] The occupational earnings score is attractive because it is available for all individuals and all years, and occupation is less likely to change due to a move. A potential disadvantage is that income varies within occupations, which could attenuate the difference in our estimates across occupational earnings groups. However, as we will show, the results based on income display similar patterns to those based on occupational earnings.

B. *Empirical Strategy: Migration*

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

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

where P_{odt} is the probability that a household living in state o (origin) moves to state d (destination) in year t, and P_{oot} is the probability that a household living in state o stays in state o. The indicator variable D_{dt} equals 1 if state d has an income tax. The variable D_{ot} is defined analogously for the origin state.[39]

We measure P_{odt}/P_{oot} as the number of households that move from state o to state d over a five-year period, divided by the number of households that stay in state o.

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

[38] The IPUMS variable is "ERSCOR50," which is a measure of the median earned income for the respondent's occupation. Income is standardized as a z-score and converted to a percentile so that ERSCOR50 gives the percentage of persons working in an occupation with lower standardized median earnings than the occupation of the respondent. For households with multiple individuals in the labor force, we assign the maximum occupational earnings score.

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

The tax variables and other covariates that are observed annually are measured in the middle of each five-year period.[40]

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

We explain how equation (3) derives from a model of location choice in online Appendix C. The key difference relative to standard random utility models is that the payoff to locating in a given state in period t depends on where the individual was located in period $t-1$. This implies that moving costs vary across state pairs, which generates permanent, nonrandom migration flows even in the absence of changes to state characteristics such as amenities or wages (Moretti and Wilson 2017).

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

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

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

[40] The only exception is state personal income, which is measured in 1929 for the period 1925–1930 because it is missing prior to 1929.

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

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

households may value new public goods less than low-income ones. For example, high earners are more likely to send their children to private school.

The key identifying assumption is that migration flows between state pairs for which income tax differentials changed and flows between state pairs for which they did not change would have followed parallel trends in the absence of the introduction of the income tax. While we cannot test this assumption, it would be more plausible if the two groups of state pairs were on parallel trends prior to the change in the income tax differential. We thus evaluate the timing of migration responses around the change in the income tax differential using the regression

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

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

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

C. *Main Results: Migration*

Table 5 reports the estimates based on equation (3). Column 1 shows the results for all migration flows, while columns 2–6 show them for households divided into five occupational earnings groups: below the twenty-fifth percentile, twenty-fifth to fiftieth percentile, fiftieth to seventy-fifth percentile, seventy-fifth to ninetieth percentile, and above the ninetieth percentile.[44]

Panel A reports the results for the child-based measure of migration from 1900 to 2010. We find that the introduction of the income tax increased overall out-migration by 11.3 percent (SE = 3.0) (column 1). For low-earning households in the bottom 25 percent of the occupational earnings distribution, however, this relationship was negative (column 2). The sign of this estimate suggests that these households may have been attracted to higher-tax states where they may have received net fiscal benefits. We will return to this issue ahead. For households in the top 75 percent, by contrast, the estimates are always positive and significant (columns 3–6). The largest response, at 14.1 percent (SE = 4.4), was for middle-earning households between the fiftieth and seventy-fifth percentiles. For high-earning households

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

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

Table 5—Effects of Introduction of Income Tax on Out-Migration by Occupational Earnings

		Out-migration flows by occupational earnings percentile				
	All flows	< 25	$[25,50)$	$[50,75)$	$[75,90)$	≥ 90
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Child-based 5-year migration rate, 1900–2010						
Post–income tax	0.113	−0.058	0.132	0.141	0.109	0.136
(*Origin − Destination*)	(0.030)	(0.044)	(0.039)	(0.044)	(0.042)	(0.040)
Observations	17,780	3,906	3,906	3,906	3,906	3,906
Panel B. Child-based 5-year migration rate, 1940–2010						
Post–income tax	0.166	−0.131	0.179	0.169	0.156	0.167
(*Origin − Destination*)	(0.032)	(0.049)	(0.045)	(0.056)	(0.050)	(0.046)
Observations	13,619	3,268	3,268	3,268	3,268	3,268
Panel C. Census 5-year migration rate, 1940–2000						
Post–income tax	0.177	0.020	0.206	0.254	0.233	0.276
(*Origin − Destination*)	(0.045)	(0.040)	(0.040)	(0.037)	(0.036)	(0.037)
Observations	13,275	8,037	8,037	8,037	8,037	8,037

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

in the top 10 percent of occupational earnings, the introduction of the income tax increased out-migration by 13.6 percent (SE = 4.0).

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

Panel C reports the results for the census measure of migration, which is only available from 1940 onward. The basic patterns remain similar. Our estimates indicate that the introduction of the income tax continued to induce significant out-migration for middle- and high-earning households. The largest response occurred among high earners, who increased out-migration by 27.6 percent (SE = 3.7) in response to the introduction of the income tax. In contrast to the results for the child-based measure, low-earning households did not significantly alter their location choices in

response to the income tax according to the census migration measure. This difference could be because low-income households with small children (as captured by the child-based measure) received larger net benefits from income tax states (e.g., in terms of access to better public schools) than other types of low-income households (as captured by the census measure).

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

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

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

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

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

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

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

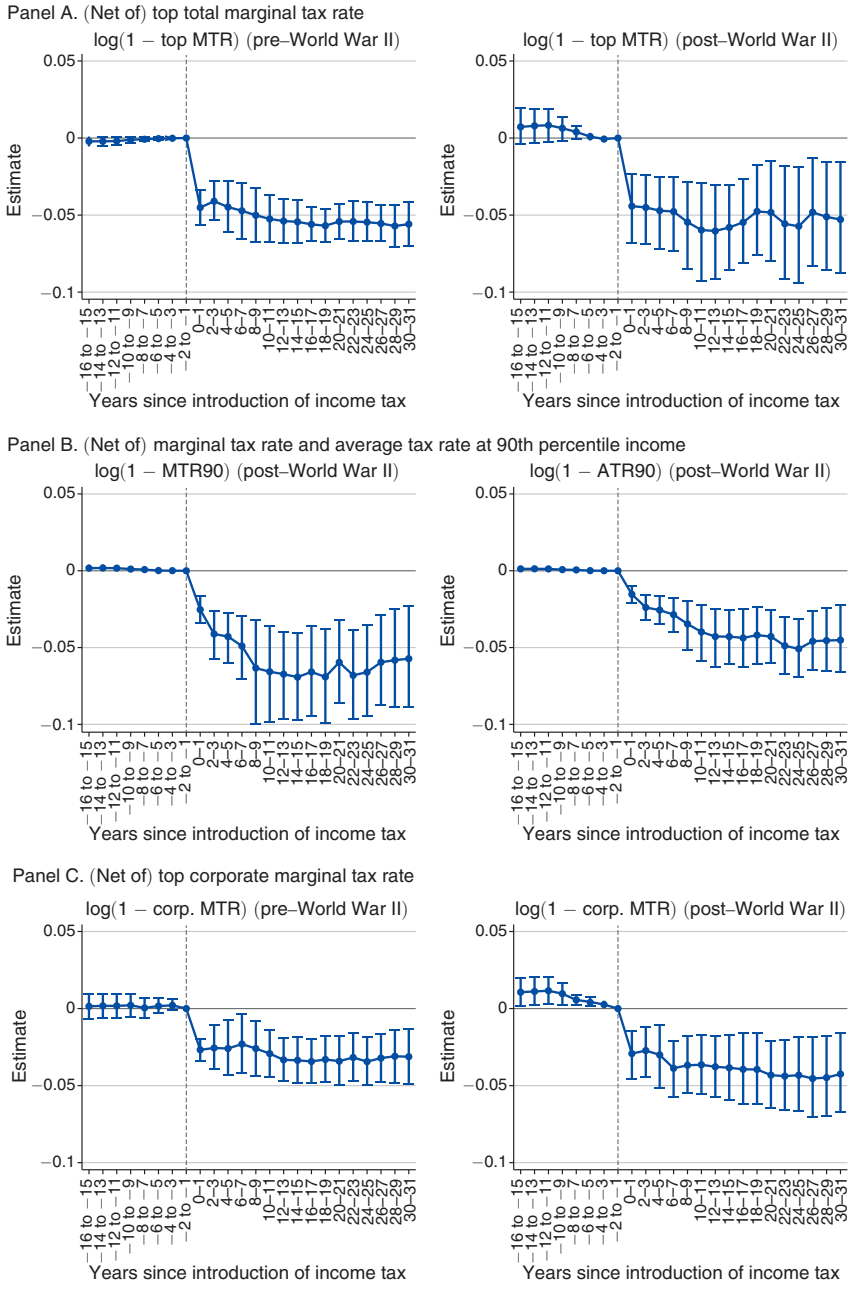

FIGURE 8. DYNAMIC EFFECTS OF INTRODUCTION OF INCOME TAX ON NET-OF-TAX RATE

Notes: This figure plots estimates of $\beta^{h,e}$ for early adopters ("pre–World War II") and $\beta^{h,\ell}$ for late adopters ("post–World War II"), both from equation (1). The outcome is always $\log(1-\tau)$ for some tax rate τ. In panel A, τ is the approximate total marginal tax rate faced by top earners, which accounts for state and federal taxes and the deductibility of state income taxes from the federal tax liability. That is, $1-\tau = \left[1-\tau^{I,S}-\left(1-\tau^{I,S}\right)\cdot\tau^{I,F}\right]/\left(1+\tau^{sales}\right)$, where $\tau^{I,S}$ and $\tau^{I,F}$ are the top statutory state and federal income tax rates and τ^{sales} is the state sales tax rate. In panel B, τ is the total marginal tax rate at ninetieth percentile income (left panel) or the total average tax rate at ninetieth percentile income (right panel). In panel C, τ is the top marginal tax rate on corporate income. Ninety-five percent confidence intervals based on the wild cluster bootstrap are reported.

2 with respect to the personal marginal tax rate and a 20-year elasticity of 7 with respect to the corporate tax rate.[48]

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

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

D. *Robustness: Migration*

Controlling for state differentials in the presence of the sales tax or the unemployment rate has virtually no impact on our results for either the child-based or census migration measures (online Appendix Figure B.10). When we control for state differentials in log personal income per capita or industry shift-share employment shocks, the estimate falls in magnitude but remains large. Across all the controls, the smallest out-migration response that we estimate for late adopters (i.e., post–World War II) of the income tax is approximately 10 percent, which is both economically meaningful and statistically significant.

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

[49] Here, we again assume that the individual income tax and the corporate income tax have the same impact. This is for a 5 percent fall in the net-of-tax rate for the individual income tax and a 4 percent fall in the net-of-tax rate for the corporate income tax. Figure 8 shows that in the first four years after the introduction of the income tax, the net-of-average-tax rate and the net-of-corporate-tax rate each fell by about 3 percent (panels B and C). Scaling by 6 instead of 9 yields a 4-year elasticity of 0.7.

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

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

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

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

VI. Conclusion

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

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

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

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