A Taxing Journey: Tax Adoptions and Interstate Migration in the Early 20$^{\text{th}}$ Century*

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Abstract

This paper examines the effects of sequential income tax adoptions on interstate migration in the US states between 1900 and 1930. Building on a migration gravity model with multilateral resistance, we analyze matched full-count US census data to explore the causal link between state income tax introductions and migration. Our findings indicate that the adoption of personal state income taxes lead to an 8.7% increase and corporate state income taxes lead to an 11.3% increase in internal migration. We conclude that income tax adoptions had substantial implications for interstate migration during this period, lending credible support to foot voting.

Keywords: Tax Policy, Income Tax Adoption, Internal Migration, Fiscal Decentralization

JEL Codes: H24, H25, H26, E65, N32, N92, R23, C23

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1 Introduction

When and how do people avoid taxation? This is a critical question to consider when implementing a new tax. A plausible way of avoiding taxation is to move to a location where such taxation does not apply. If individuals choose to live where their preferences over taxation, and correspondingly public goods provision, are best aligned with the local policies, tax competition between governments will spur mobility. To estimate the relative importance of these effects, we analyze the unharmonized and sequential introduction of state income taxes in the US of the first half of the 20th century. This unique historical setting enables us to causally identify the migration effects of these tax adoptions on the extensive margin, as well as their implications for regional disparities. To our knowledge, we are the first to find evidence of foot-voting in response to the introduction of US state income taxes in the early 20th century.

The theoretical groundwork for peoples' responses to new taxation dates back to Tiebout [1956] and Tullock [1971], who state that individuals can "vote with their feet" when responding to changes in taxation. Especially, in the short run, local policies affect residential choice [Rhode and Strumpf, 2003]. These re-allocative effects of interstate fiscal competition on the population are discussed in works on fiscal federalism (compare Oates et al. [1972], Wooders [1978] and Hochman et al. [1995]. The empirical evaluation of these patterns is met by several challenges: Tax differentials are typically observed across countries, where constraints on international migration of firms and individuals, such as language, cultural barriers or legal difficulties, apply. Furthermore, contemporary tax policies are increasingly complex, and make it challenging to measure tax differentials between different jurisdictions. Finally, income tax adoptions are rare, so the empirical literature typically focuses on tax changes at the intensive margin.[1] The early development of a federal structure in the United States in 1791 provides a high level of revenue decentralization, and allows us to abstract from typical migration barriers, such as language, legal status, cultural differences, or recognition of prior education, and instead focus on the implications of tax adoption. As argued in Prud'Homme [1995], sub-national economies, such as states, are more open than sovereign countries, making it substantially easier to

[1] See Saltz [1998] and Cohen et al. [2014], who both using US data from the 1990s and early 2000s, to show that citizens move across state borders to reduce their income tax burden. Similarly, in Switzerland, young college graduates respond to tax differentials with higher mobility [Liebig et al., 2007].

move between jurisdictions. Building on Borjas [1987], Tiebout's foot voting hypothesis varies across demographic subgroups, revealing significant mobility effects from taxation for scientists, inventors, and high earners regarding taxation [Young et al., 2016; Moretti and Wilson, 2017; Kleven et al., 2020; Akcigit et al., 2022].

Our analysis investigates the causal effects of tax adoptions on migration patterns in the US in the early 20th century, finding support for the foot-voting hypothesis. In contrast to Cassidy et al. [2024], who identify migration responses only after WWII, we add to the empirical literature by focusing on the early adoption period prior to the Great Depression. A key argument for investigating Tiebout competition with changes on the extensive margin of tax policies is that the introduction of a new tax serves as a stronger shock than a change in an existing rate. This argument however depends on the novelty of the shock. Hence, we believe that the estimation of the extensive margin responses are especially relevant for these early adopter states. We rely on the sequential implementation of state income taxes in the early 20th century across the United States as a natural experiment, which together with our empirical strategy allows us to make causal claims. While Cassidy et al. [2024] primarily look at fiscal responses, we focus on the direct changes in mobility caused by the tax adoptions, and provide a structural gravity model to estimate causal links between early tax adoptions and interstate migration using data on bilateral flows. We find significant effects for early adopter states. An added advantage of our approach is that we observe a full cross-section of the population, rather than subgroups, filling a critical gap in the literature [Kleven et al., 2020].

For our investigation, we use linked census data from 1900 to 1930 and focus on the roll-out of personal and corporate income taxes in several U.S. states. During our observation period, sixteen states adopted some type of personal income taxation. In addition, again sixteen states implemented a corporate income tax, with a significant number adopting both personal and corporate income taxes simultaneously.[2] Moreover, the federal government ratified its own income tax in 1913, rendering the state tax as an additional tax burden on personal income. In legal terms, most state income taxes were comparable to the federal policy, including adopting their income tax with a progressive rate scheme. To establish the causal relationship between taxation and migration, it is critical for our identification

[2] Three quarters of the adopter states introduce both income taxes within the same decade, three states only adopt either the personal or corporate income tax.

strategy that tax adoptions were not driven by internal migration. Many contemporary scholars report the mechanism of the introductions [Comstock, 1920; Lutz, 1920; Bigham, 1929]. Extending this literature are empirical studies by Berry and Berry [1992] and Hall et al. [2020]. In our historical setting, the main driver for the tax adoptions is political opportunity, such as a neighboring states' adoption of income tax (regional diffusion). As argued in a report by the National Industrial Conference Board [1930], the failure of state property tax, due to tax avoidance, was a major contributor to the introduction of state income taxes as local municipalities strategically undervalued properties to attract owners. As these new taxes were meant as a substitute for the property tax, state revenues and spending were not affected [Cassidy et al., 2024]. None of this points to a systemic response to preceding migration patterns. Contemporary studies on a state level provide more detailed discussions on adoptions, including rate progressivity and legal definitions of income [Kennan, 1915; Bullock, 1918; Hannan, 1922; Whitehead, 1921; Thoresen, 1928; Briggs, 1936].

Our paper also contributes to a growing literature on historical interstate migration, focusing on different causes of mostly large scale migration amongst states in the 20$^{\text{th}}$ century [Molloy et al., 2011; Boustan et al., 2012; Hornbeck and Naidu, 2014]. Two large scale interstate migration waves, which do not directly interfere with our focus of study, took place in the 20$^{\text{th}}$ century. First, the persistent westward migration as a direct response to the long-lasting westward expansion from the mid 17$^{\text{th}}$ century to the late 19$^{\text{th}}$ century. Zimran [2022] recently discussed the migration to western states, finding a slowdown in in-migration towards the end of the first half of the 20$^{\text{th}}$ century. The second migration wave, called the Great Northward migration, was mainly driven by Black families. Causes and effects have been recently extensively studied by Derenoncourt [2022].

Methodologically, we utilize advances from the gravity literature to study the effects of tax adoptions on interstate migration. Our theoretical framework builds on a combination of classical discrete migration choice by McFadden [1973] and the standard gravity approach by Anderson and Van Wincoop [2003]. Anderson [2011] establishes a link between migration and gravity theory with multilateral resistance. The resulting structural equation can be consistently estimated through a Poisson model, as first introduced in Silva and Tenreyro [2006]. The key advantage of the gravity approach is the ability to extensively control for the entire universe of state-specific factors that could otherwise lead to a substantial omitted variable bias.

As mentioned above, our key findings underscore the significance of tax adoptions for interstate migration in the early 20$^{\text{th}}$ century. Our estimates reveal an 8.7% increase in interstate migration flows. Notably, these effects are more pronounced for young individuals with above-median income. By incorporating the adopted tax rates, we refine our analysis to quantify the effects on an intensive margin. Moreover, we identify a secondary channel through which income taxation influences interstate migration: the adoption of corporate income taxes amplifies migration flows by 11.3%. We conjecture that corporate taxation drives away industry, consequently affecting the movement of workers. Thus, our findings provide evidence of substantial causal migration responses to the early adoption of state income taxation.

2 Historical Context

The Tenth Amendment to the Constitution is the basis of the American Federalism, allowing states to impose their own taxes. In 1911, Wisconsin became the first state to legally impose a modern statute on income taxation.[3] During this period and under the political influence of Robert M. La Follette Sr., Wisconsin had become one of the most progressive states. The adoption of state income taxation was primarily justified by redistributing wealth from the manufacturing to the agricultural sector, as opposed to raising revenues [Stark, 1987]. The law was passed even prior to the federal government's decision to impose federal income taxes (which became effective two years later with the Sixteenth Amendment to the Constitution, 1913). Between 1912 and 1921, five more states adopted state income taxes.[4] Similarly to Wisconsin, tax implementations during this adoption period were often driven by progressive motives such as wealth redistribution, as well as the failure of property taxation in most of the states due to competitive undervaluation. Hence, the adoptions of income taxes can be seen as a mean to adjust revenues rather than driven by the intent to raise additional revenues. Furthermore, expenditures did not change significantly in the states adopting the tax [Cassidy et al., 2024]. The

[3]Technically, Hawaii was the first to adopt an income tax in 1901. However, Hawaii only became a US state in 1959 and is thus not included in this analysis.

[4]Mississippi, Connecticut, Virginia, Massachusetts, Delaware, Missouri, Montana, North Carolina, North Dakota and New York.

introduction of the state income taxes can be seen as a local fiscal centralization, from the local to the state level, which led to an increase in the state share of state-local expenditures from 13.2% in 1913 to 24.1% in 1932 [Wallis and Oates, 1988]. Berry and Berry [1992] show that fiscal health of the state was an insignificant factor in state income tax adoptions between 1916 and 1929. Later adoptions of state taxes can be attributed to 'political opportunity', as these taxes were a direct response to either regional elections, local fiscal crises or to the adoption of certain taxes in neighboring states.[5] Further note that, as shown in Figures A1 and A2 (in appendix A), the adoptions were promoted in both Democratic and Republican states. These comparative findings are relevant for an empirical estimation of average tax effects and the juxtaposition of adopter and non-adopter states.

The discourse surrounding the implementation of state personal income taxation consistently captured public attention. In the context of New York, where the individual income tax was ultimately introduced in 1919, the resistance against state taxation reached a fervent pitch by 1916:

> *Under personal tax and income tax alike many swear to the thing which is not and other flee from the strain on their conscience. The cost to the State is greater in loss of citizens than in loss of resources. Senator MILLS estimated that $400,000.000 of capital was removed from the State because of a tax law. It would be interesting to have this estimate of the number of citizens who would leave New York if his income tax were to be adopted. [New York Times on February 18, 1916]*

This article, published three years before New York's formal adoption of state income taxes in 1919, is a poignant reminder of the contentious issue. The subject of state income tax remained a topic of deliberation even in the wake of its legitimate establishment. A main concern, highlighted by many politicians, such as New York State Comptroller Eugene M. Travis, was an imminent loss of a significant segment of New York's citizens should the tax be enforced:

> *He warned the committee that such a measure would be resented, evaded, and avoided by the people, and declared that its enactment would result in the State's losing many*

[5] By 1933, half of the Contiguous United States, introduced some version of state specific personal income taxation. As of today, there remain nine states without state labor income tax: Alaska, Florida, Nevada, New Hampshire, South Dakota, Tennessee, Texas, Washington and Wyoming.

estimable citizens "willing to pay their just share but intolerant of any unjust scheme." [New York Times on April 9, 1919]

Analogous apprehensions regarding tax evasion through outward migration were echoed in Wisconsin. State Senator Henry W. Bolens articulated this sentiment a year following the implementation of the state tax:

A state income tax necessarily embraces the principle of self-assessment to such a large degree that injustice and inequality become so flagrant and apparent that it breeds a universal desire to escape the tax. [Wausau Pilot on August 20, 1912]

In many states, the adoption of personal income taxes was accompanied by the introduction of corporate taxes. Between 1911 and 1929, 16 states implemented corporate taxes, with 7 of them introducing these concurrently with personal income taxes. In 2 states, the adoption of corporate taxes came within a decade after the introduction of personal taxes. In 7 states, the implementation of corporate taxes preceded personal tax adoption.[6] Empirically, we can treat them separately. In order to disentangle the effects, we use different income groups (as detailed in Section 6.2).[7] As with personal income taxes, we expect a significant exodus of companies from states that are early adopters of corporate taxation. Historical evidence substantiates this perspective through publicly debated claims and explicit threats of companies considering relocation due to state-level corporate taxation. A prime illustration of this phenomenon again is the state of Wisconsin. Starting with its introduction in 1911, the Wisconsin state corporate tax sparked controversy within the corporate sphere. Numerous businesses declared their intention to depart for states without income taxation, illuminating the discourse. In the Wausau Pilot on August 13, 1912, an article on state income taxation mentions that firms like *Frazier* in Manitowoc, *Presto-Lite* in Milwaukee, and *The Brunswick-Balke-Collander* in Jainesville left the state. Moreover, the *Ringling Bros.*, a highly valued circus enterprise headquartered in Baraboo, Wisconsin, had planned to transfer its operations to Illinois.

Dislike of the operations of the recently enacted income tax law is back of their decision. They assert the law is unfairly oppressive, and is practically driving them from the state.

[6]In 5 states, the corporate tax preceded the personal tax by less than 10 years.

[7]We can show that a condensed measure of both introductions yield very similar results to our approach.

> *[...] The income tax has been one of the bones of contention in Wisconsin ever since its enaction, and the prospective action of Ringling Bros., now promises to give it new prominence. [Wausau Pilot on August 13, 1912]*

The threat of losing such a significant enterprise was compelling enough for the Legislature to exempt the circus from the tax [Apps, 2005]. However, such exemptions were generally unpopular among the public. As a result, this case remained an exception, with most corporations in Wisconsin remaining subject to the tax. This case exemplifies the dual pressures these new taxes exerted: on businesses struggling to remain profitable, and on policymakers navigating economic and public interests.Furthermore, the proposed Arnold Income Tax Increase in 1919 intensified the situation, leading more corporations to contemplate drastic measures, such as relocating their enterprises to neighboring states devoid of corporate taxation:

> *Manufacturers and merchants have publicly announced that if the bill becomes a law they will move their business out of the State. [New York Times on June 17, 1919]*

While the economic repercussions of a business exodus are sufficiently alarming, this trend could ripple into the labor market, potentially exacerbating the out-migration of the workforce. This is particularly concerning given that over three quarters of the states that introduced individual income taxation between 1910 and 1930 concurrently adopted corporate income taxation.

3 A Gravity Model of Interstate Migration

For the theoretical foundation of our analysis, we adapt a tractable framework based on the gravity trade literature, in particular Anderson and Van Wincoop [2003] and Weidner and Zylkin [2021]. We utilize the gravity model of trade with multilateral resistances in a migration setting by following Anderson [2011] and Beine et al. [2016]. Hereby, we model bilateral flows between all US states, enabled by our linked census data described in Section 5. This allows us to motivate our empirical approach and enables us to formalize theoretical predictions and rationalize our findings. Moreover, the model helps us to claim causality and directional properties (a challenge we discuss in Section 4).

Consider the migration across the contiguous United States with origin state i and destination state

j.[8] We define the utility of living in destination j as x_j, interpreted as a collection of additive financial factors, such as wages.[9] Individual h migrating from i to j faces iceberg costs of migration $d_{i,j} := c_{i,j}/\exp(\alpha\tau_{i,j})$, where $c_{i,j}$ captures standard costs such as geographic distance between i and j. Another cost factor is the introduction of state income taxation $\tau_{i,j}(z_i, \cdot)$, where z_i is defined as an indicator function equal to one if state i enacts an income tax law.[10] Here, $\tau_{i,j}$ is meant to capture the impact of relative tax adoptions on total iceberg costs, where a tax introduction makes the relative costs of living in state i, while making moving to j less expensive. Hence, tax adoptions are both depending on origin state i and destination state j. The net wage at the destination is defined as $x_j/d_{i,j}$.[11] Next, we define the idiosyncratic utility of migrating from i to j as $v_{h,i,j}$, which has an extreme value type I distribution. Hence, h only moves from i to j if $\exists\,(x_j/d_{i,j})v_{h,i,j} \geq x_i$. Expressed as logarithmic utility, we have $\tilde{u}_{i,j} = \ln(x_j) - \ln(d_{i,j}) - \ln(x_i)$.[12] Following Anderson [2011], we impose the log of a CRRA utility structure such that $\exp(u_{i,j}) = (x_j/d_{i,j}x_i)^{1-\theta}/(1-\theta)$, where θ captures the rate of relative risk aversion. Note that on an aggregated level, the probability of moving from i to j is given by the share of migrants i to j. The unharmonized tax introductions require adding a discrete time subscript t. We are primarily focusing on the impact of tax adoptions. Therefore, we let the policy variable capture the short run effects of the introductions, where $\tau_{i,j,t} = 1$ implies $\tau_{i,j,t+1} = 0$.[13] We further define $d_{i,j,t} = c_{i,j}/\exp(\alpha\tau_{i,j,t})$. By denoting $p_{i,t}$ as the population of state i at link t, the migration flow from i to j can then be defined as $y_{i,j,t} = g(u_{i,j,t})p_{i,t}$ with $g(u_{i,j,t}) = \exp(u_{i,j,t})/\sum_k \exp(u_{i,k,t})$, such that this flow reconstructs the CES demand or Ricardian supply shares equivalent to the trade gravity models. With $p_t \equiv \sum_i p_{i,t}$, Anderson [2011] has shown that we can specify the following multilateral

[8]Hence the number of states is $I = J = 48$. Note, that the total number of possible origins or destinations can be arbitrarily high, as long as $I = J$. This imposes a symmetric (or balanced) structure on any possible formulation of this model.

[9]To maintain simplicity of the model, we limit ourselves to these cardinal factors. However, in the empirical method, preferences over destinations depend on no particular set of factors and can include any type of push and pull factor.

[10]Note that the second argument is a placeholder for j-variation. In the identification, this comes from either an interaction with the identifier $\mathbb{1}(i = j)$ or from $z_j = 0$.

[11]While the personal income tax naturally decreases the migration costs by affecting relative wages, the corporate income tax enters the model through business relocation.

[12]The standard migration literature then focuses on personal migration choice. McFadden [1973] shows that if $\ln(v)$ has a type-1 extreme value distribution, the probability of choosing destination j is given by a multinomial logit form.

[13]This aims to disentangle the negative first order effects of the tax on personal income and potential second order effects via an increase in public policy provision, which we assume to have a $t + 1$ lag. Fiscal responses to the income tax adoption during our observation period have shown to be negligible within a ten-year period [Cassidy et al., 2024].

resistance terms

$$\omega_{i,t}^{(1-\theta)} \equiv \sum_j d_{i,j,t}^{(1-\theta)} \frac{p_{j,t}^{\gamma_2}}{\psi_{j,t} p_t} \quad \text{and} \quad \psi_{j,t}^{(1-\theta)} \equiv \sum_i d_{i,j,t}^{(1-\theta)} \frac{p_{i,t}^{\gamma_1}}{\omega_{i,t} p_t}, \tag{1}$$

where we augment the structural equation with migration elasticities γ_1, γ_2. Similar to a trade framework, equation 1 defines the multilateral resistance. These frictions may be interpreted as the average iceberg costs with respect to all possible destinations. This includes the adoption of the state tax of state i with respect to all other $j \in J \setminus \{i\}$. Anderson [2011] compares these terms to "incidences of migration costs". Empirically, these costs include the inverse of push and pull factors, which are imperative to the establishment of migration flows. This allows then to define a structural gravity equation of predicted migration flow y from i to j (also referred to as pair out-migration) as function of individuals' utility of migrating:

$$y_{i,j,t} = \left(\frac{d_{i,j,t}}{\omega_{i,t} \psi_{j,t}} \right)^{1-\theta} \frac{p_{i,t}^{\gamma_1} p_{j,t}^{\gamma_2}}{p_t}, \tag{2}$$

Note that the structural gravity equation 2 captures a partial equilibrium.[14]

The stochastic version of equation 2 can be seen as a bridge to our empirical approach. First, we include a stochastic error factor $\varepsilon_{i,j,t}$. The stochastic form of equation 2 is

$$y_{i,j,t} = d_{i,j,t}^{(1-\theta)} \frac{p_{i,t}^{\gamma_1}}{p_t \left(\omega_{i,t}^{(1-\theta)} \right)} \frac{p_{j,t}^{\gamma_2}}{\psi_{j,t}^{(1-\theta)}} \varepsilon_{i,j,t}, \tag{3}$$

where we have $\varepsilon_{i,j,t} = y_{i,j,t} / \mathbb{E}(y_{i,j,t} | d_{i,j,t}; p_{i,t}, p_{j,t}, \omega_{i,t}, \psi_{j,t})$. The multiplicative gravity equation can be further written as an exponential function such that

$$y_{i,j,t} = \exp \left[(\theta - 1)(\alpha \tau_{i,j,t} - \ln c_{i,j}) + \ln \left(\frac{p_{i,t}^{\gamma_1}}{p_t \left(\omega_{i,t}^{(1-\theta)} \right)} \right) + \ln \left(\frac{p_{j,t}^{\gamma_2}}{\psi_{j,t}^{(1-\theta)}} \right) \right] \varepsilon_{i,j,t}. \tag{4}$$

We define $\beta := (\theta - 1)\alpha$. With $\alpha > 0$, the introduction of taxation in the origin state i are associated with a decrease in migration costs, essentially through a reduction in relative migration costs, which

[14]Inward and outward resistance can be solved for by normalizing with respect to one state, and equation 1 constitute the conditional general equilibrium.

in turn should increase migration flows.[15] There are two potential drivers that support a positive β. The first driver operates through the direct channel, where individuals express a reluctance to bear the burden of new taxes. This reasoning is an interpretation of Tiebout [1956] and relates to personal income taxation. The second driver operates indirectly through corporate income taxation. Corporations, much like individuals, seek to avoid the new taxation. A consequential relocation of businesses across state borders, and the accompanying labor relocations, present another incentive for increasing interstate migration flows.

For notational simplicity, we set $\delta_{i,j} := (1-\theta)\ln c_{i,j}$, $\delta_{i,t} := \ln\left(p_{i,t}^{\gamma_1}/\left[p\left(\omega_{i,t}^{(1-\theta)}\right)\right]\right)$ and $\delta_{j,t} := \ln\left(p_{j,t}^{\gamma_2}/\psi_{j,t}^{(1-\theta)}\right)$. When assuming $\mathbb{E}(\varepsilon_{i,j,t}|\tau_{i,j,t};\cdot) = 1$, we can write equation 4 as a moment condition

$$\mathbb{E}(y_{i,j,t}|\cdot) = \lambda_{i,j,t} := \exp\left[\beta\tau_{i,j,t} + \delta_{i,j} + \delta_{i,t} + \delta_{j,t}\right]. \tag{5}$$

The objective of our empirical approach is to estimate β, which allows a quantification of the migrational impact of income tax adoptions.

4 Empirical Estimation of the Gravity Model

To identify the tax adoption, varying in origin and destination state, we follow Beverelli et al. [2018] by utilizing migration within a state, i.e. non-movers, to formulate a new variable $\tau_{i,j,t} := (z_{i,t} - z_{i,t-1})m_{i,j}$, where $m_{i,j}$ is an indicator function that captures interstate migration.[16] Note that we are interested in the effects of the tax on migration away from i and towards j, where $\tau_{i,j,t}$ captures the effect of tax adoptions on interstate migration relative to non-movers.[17]

We adopt the estimation procedure by Correia et al. [2020]. Silva and Tenreyro [2006] have shown that the estimation of our parameter of interest β requires a generalized linear model. In order to avoid log-linearization, the prevailing model employed in the literature is the three-way fixed effects

[15] The empirical literature on the average US relative risk aversion, such as Gandelman and Hernández-Murillo [2015], has shown that $\theta \approx 2$.

[16] We define $m_{i,j} = 1$ iff $i \neq j$ and 0 otherwise.

[17] Alternatively, we can directly create a non-linear combination of the adoption in origin state i and destination state j. This second approach will be estimated in the robustness checks in Section 6.5.

Poisson Pseudo-Maximum Likelihood (FE-PPML) approach using iteratively reweighted least squares. There are numerous advantages associated with this technique, above all the ability to estimate the parameters of the stochastic equation 5 in its multiplicative form. We do not need to log-linearize the system, including $y_{i,j,t}$. This allows us to disregard potential issues with zero migration observations, as well as biases that stems from heteroskedasticity. This approach delivers consistent estimation of the β. However, in the case of three-way gravity models, this approach introduces an asymptotic bias. To address this issue, we implement an analytical bias correction method proposed by Weidner and Zylkin [2021].[18]

The full set of fixed effects allows us to control for the entire universe of push and pull factors of each state. Specifically, state specific events that may have triggered both an increase in taxation and out-migration (e.g. local economic crises) are captured by the state-time fixed effects, which supports the causal interpretation of our results. These fixed effects further capture the multilateral resistance terms to migration, captured by $\omega_{i,t}^{(1-\theta)}, \psi_{j,t}^{(1-\theta)}$, which capture average migration costs. Empirically, the interpretation of these costs can be broadened to any time-varying state specific characteristic. Bertoli and Moraga [2013] show that these resistances are functions of the relative attractiveness of alternative destinations. To control for resistance varying in i, j and t, Beine and Parsons [2015] and Beine et al. [2016] suggest to use origin-time and destination-time fixed effects, especially for low frequency data. Next, in order to interpret the estimated results as a percentage change in migration flows, we require the following transformation

$$\hat{\hat{\beta}} := \exp([\hat{\beta} - \textit{bias}]) - 1 = \beta \text{ for } N \to \infty. \tag{6}$$

By design, we estimate the effects on migration flows. Theoretically, these flows are directional, namely from origin state i to destination state j. Since a model with multilateral resistances requires us to employ a full set of fixed effects, including origin-time $\delta_{i,t} \equiv \delta_i \delta_t$, destination-time $\delta_{j,t} \equiv \delta_j \delta_t$ and state pair $\delta_{i,j} \equiv \delta_i \delta_j$, we cannot empirically distinguish the direction of the migration flows. In the

[18]It has also been shown that cluster-robust standard error estimates are biased, leading to incorrect confidence intervals. In light of this finding, we introduce alternative estimation methods that are more conservative, such as jackknife and bootstrap confidence interval methods. Pfaffermayr [2021] suggests that these methods provide more accurate standard error estimates and ensure the validity of the confidence intervals.

estimation, results of the main specification are meant to be interpreted as changes in migration flows. The model allows then allows to interpret them as changes in outflow. We will test for and discuss directional effects in Section 6.2.1.

5 Data

5.1 Full Count Data

Our analysis relies on full count historical census data for the US from 1900 to 1930. This data has only recently become available as a result of the collaborative work of the Integrated Public Use Microdata Series (IPUMS), "Ancestry.com" and "Family Search", provided by Ruggles et al. [2021] and the University of Minnesota. The data set includes information on gender, race, age, birthplace, current residence, family structure, educational attainment as well as information on occupation and income scores. Note that the census did not provide any status on internal migration prior to 1940. What it does include is a so-called "historical identifier", a unique identification for each individual, which allows us to link agents across census waves, thereby identifying movers through changes in location.

5.1.1 Census Linking

Following Abramitzky et al. [2020], links are generated by multiple matching algorithms of different levels of intensity and enable us to track individuals across different historical census waves. The various linking methods used are carefully described in Abramitzky et al. [2021]. In order to maintain a substantial sample size while minimizing the type I errors, we use the NYSIIS standardization matching.[19] This algorithm was first introduced by Taft [1970] and its modern iterations have been proven to yield accurate matching results.[20] Note, that since the matching is primarily based on last names, our linked samples exclusively feature male individuals.[21] Drawing upon these matching techniques, we

[19] Ó Gráda et al. [2023] show that there is a potential over-representation of movers, especially among immigrants. We argue that this is of no concern to our empirical strategy.

[20] The algorithm is based on phonetic similarities and is broadly used by multiple US government agencies.

[21] Historically, married females assumed their spouses last name.

have created three interconnected data sets that provide information on the same individual[22] during two consecutive census waves. Since the US census has historically been conducted every ten years, these data sets cover a ten-year period each. Table 1 summarizes the scope of each of the linked data sets and offers a brief overview of the demographic characteristics of the sample populations.

Table 1: Linked data decomposition summary

Link	*Total*	*Age* $<$ *18**	*White*†	*Labor Force*	*Avg. Inc. Score***
Linked 1900,1910	10,133,898	24.16%	92.34%	73.51%	21.30
Linked 1910,1920	12,882,629	23.44%	93.13%	72.11%	22.38
Linked 1920,1930	15,925,624	20.63%	93.82%	73.47%	23.13

Notes: * with nonage, we include individuals that did not turn 18 within the sample span. † we note that historical recordings of race are prone to misrepresentation, roughly 3% of our sample have different races at different times. As this issue gets slightly better over time we proceed by using the latter race recording of the respective link. ** Average (Occupational) Income Score ranges $\{0, 80\}$ defines the median income of the respective occupation. Note, that we use a labor-force-only subsample.

It is evident that our linked samples have a higher proportion of white individuals than the overall US population.[23] We also report labor force participation rates and average income scores, which are particularly relevant as our analysis aims to estimate the impact of personal income tax adoptions on migration. Hence, a significant proportion of (self-) employed individuals in our sample is desirable. We observe marginally higher average income scores and consistent labor force participation rates.

5.1.2 Dyadic Data

To conduct our empirical analysis, we utilize dyadic panel data by extracting individual information from the linked data sets to determine the number of in- and out-migrants for every possible state pair, denoted as i, j.[24] Specifically, we count all individuals with $\textit{Residency}_r = i, \textit{Residency}_{r+t} = j$ for each census link $t = \{r, r + 10\}$, where $r \in \{1900, 1910, 1920\}$. Our analysis focuses on the contiguous US, hence the dyadic dataset contains a total of $3 \times 48 \times 48 = 6,912$ observations.[25] Note that our aggregated migration data is perfectly symmetric by design, i.e., the out-migration from state i to j has

[22] Assuming no errors in the linking mechanism.

[23] For example, in 1910, the population was 88.9% white, whereas our data indicates a proportion of 92.3%.

[24] We also include the aggregate of stayers, i.e., the number of individuals who remained in their respective state.

[25] We exclude the 1930s from our analysis, as we expect the Great Depression and its aftermath to overshadow any potential effect of state income tax adoptions on interstate migration. Additionally, after 1929, the predominant reason for tax adoptions became the rapidly worsening economic environment, which was consistent across all states.

to be the in-migration of j from i. Hence, the average net migration has to be equal to zero. In Table 2 we report summary statistics for the full dyadic data. The analysis of linked dyadic data provides us with valuable insights into interstate migration patterns, which are essential for comprehending overall migration trends when interpreting our findings. We generally confirm the migration patterns described in Zimran [2022].

Table 2: Dyadic data summary, main panel

	Mean	*Standard Dev.*	*Min*	*Median*	*Max*
Pair Out-Migration	1,162.44	2,693.50	1	261	42,750
Net Migration	0	1,559.25	$-19,626$	0	19,626
State-wise Out-Migration	55,083.09	43,558.94	2,332	42,326.50	203,563
State-wise Population	269,416.30	245,836.90	6,814	207,045.50	1,246,011
Out-Migration Ratio	20.45%				
Observations	6,768				

Notes: For these statistics, we exclude individuals who do not move. Note, that the empirical analysis will include non-movers. We define the ratio as the average out-migration share of the average sample population. The net in-migration must sum up to 0 as we have a $j \times i$-pair to every $i \times j$-pair. Hence, also the mean of net migration must be 0. Note, that the dependent variable of our main specification is the total pair out-migration.

Figure 1 visually represents the Westward migration pattern, revealing significantly lower levels of out-migration in the westernmost state, California.

Figure 1: State level out-migration share

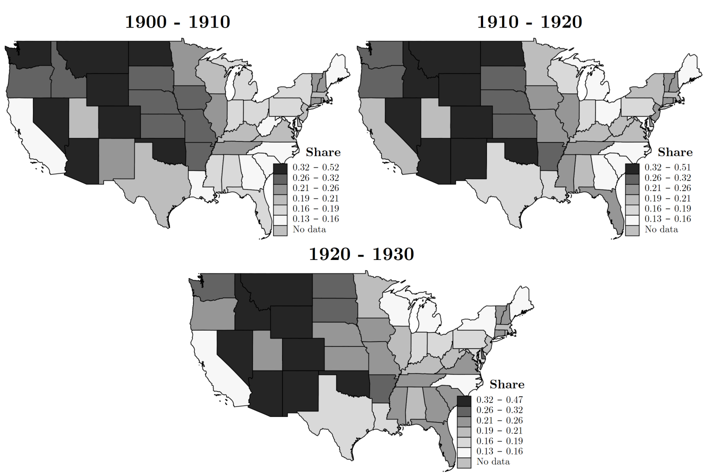
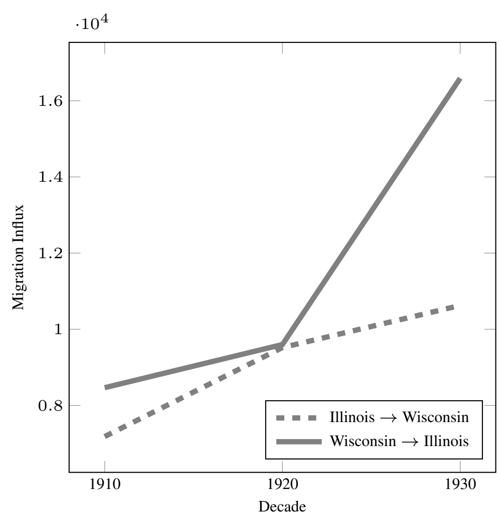


Figure 2: Reciprocal migration influx between Wisconsin and Illinois



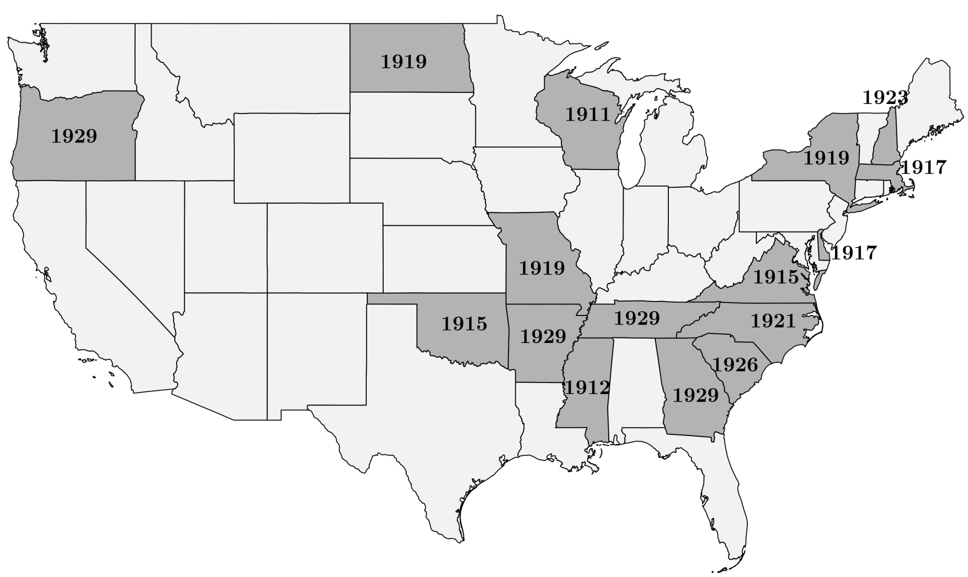
Notes: Figure 1 depicts the total out-migration share of population for each state. Numbers for Figure 2 are computed from the full count linked census data. Figures are not detrended for population growth. Influx exclusively counts realized end-of-decade reciprocal flows of individuals between the two states.

Furthermore, our results offer supportive evidence for Derenoncourt [2022], arguing that the Great Migration to the North did not begin before 1940. To further support our interstate migration hypothesis, Figure 2 presents compelling suggestive evidence focusing on the example of Wisconsin and Illinois. We have chosen these states as examples because Wisconsin became the first state to formally adopt state income taxation in 1911, whereas its neighbor and primary economic partner, Illinois, only introduced such state income taxes in the 1970s. Following the 1910s, our analysis demonstrates a remarkable increase in migration influx from Wisconsin to Illinois. In contrast, the in-migration from Illinois to Wisconsin exhibited a relatively stable trend.

5.2 Income Tax Data

We collect historical state income tax data from National Industrial Conference Board [1930] reports. This document contains a collection of comprehensive reports on state income taxes by the National Industrial Conference Board (NICB)[26] between 1916 and 1930. The reports contain detailed information on the taxation of personal and/or corporate income. Figures 3 and 4 show the respective adoption years of the state income taxes.

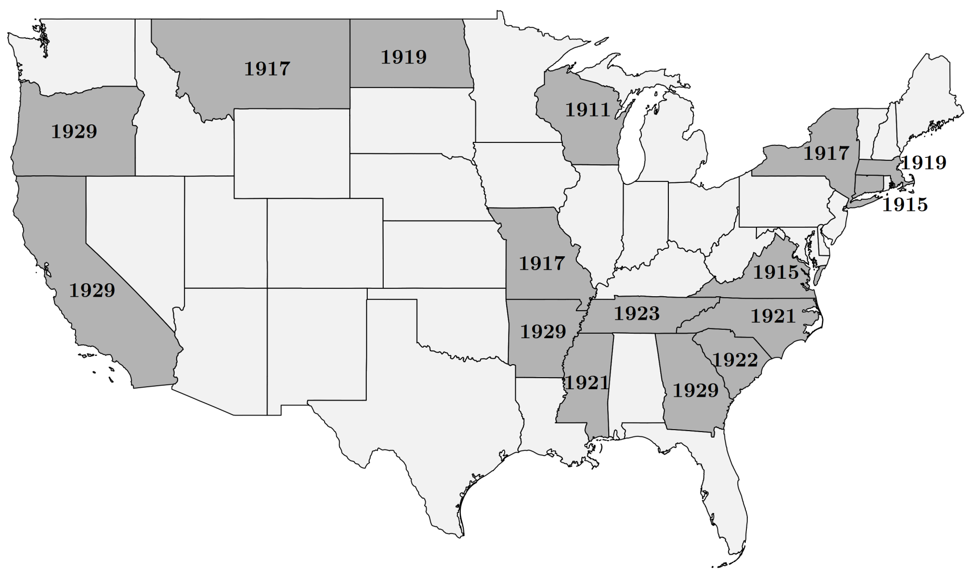
Figure 3: Adoption year of personal income tax



[26]As a response to economic losses and increasing public pressure on industries caused by the First World War, eleven US trade associations formed an organization to foster "harmonious relationships between employer and employees and between both labor and government." The organization provides economic records and analysis of business relevant political decisions, such as the rollout of state income taxes. The following reports are made available by HathiTrust Digital Library.

Reports are structured chronologically, include a separate chapter for each state and cover essential features of every adopted state income tax, including the motives of the introduction, the nature, and scope of the tax as stipulated by the original law, the enactment of these laws as well as subsequent modifications of them. We are therefore provided with state-specific information on the tax rates at the time of the adoption, as well as changes to those in the subsequent years until 1930. Moreover, reports include data on financial results of the state income taxes. To increase accuracy, we cross-check information from these reports with the Advisory Commission and American Council on Intergovernmental Relations [1995] fiscal federalism report.

Figure 4: Adoption year of corporate income tax



This information on tax adoptions, together with the linked census data, allows us to obtain insightful descriptives presented in Table 3. First, we identify all movers between 1900 and 1910, who move to states that never adopt an income tax. Note that only one third of all states adopt an income tax in our observation period. We find that, the share of movers moving to non-adoption states increases by around 7% from the 1900s to the 1920s, serving as suggestive evidence supporting the foot voting hypothesis.

Another metric of interest is the labor force share of affected individuals. Note that most states set a personal tax exemption of around $1,000. We see that the vast majority of individuals in our sample are subjected to the personal income tax. Moreover, this share is increasing over time.

Table 3: Descriptive statistics related to the personal tax adoptions

	1900s	*1910s*	*1920s*
Number of Adopters		9	7
Largest Adopter (Tot. Sample Population, in mil.)		New York (1.059)	North Carolina (0.335)
Smallest Adopter (Tot. Sample Population, in mil.)		Delaware (0.036)	New Hampshire (0.083)
Affected Share of the Labor Force		81.12%	86.92%
Share of Movers to Non-Adopters	65.34%	68.17%	69.82%

Notes: Affected share is computed by counting individuals with an occupational income score above $1,000. This cut-off represents the most common exemption from the income tax.

A key component of our identification strategy is that adopter and non-adopter states are as comparable as possible. In other words, that tax adoptions are as exogenous as possible. To check this, our comprehensive dataset allows us to calculate average unemployment rates, occupational income scores and debt per capita for both adoption and non-adoption states, and hence enables us to draw descriptive comparisons.[27] The summarized results, presented in Table 4, reveal no statistically significant differences between these two groups of states. This suggests there is no economic distinction between states that have adopted income taxes and those that have not.

Another related identifying assumption is whether we find parallel trends across adopters and non-adopters. This assumption essentially requires the absence of statistically significant difference in the pre-trend of the dependent variable across both treatment and control groups. We conduct separate tests for both treatment groups to ascertain this. Specifically, we examine the pre-trends for states that adopted the personal income tax in 1910 compared to those that were never treated, as well as those adopting in 1920. For states adopting in 1920, we compare pre-trends in the 1900s to those never treated, as well as to states adopting in the 1910s. Additionally, we assess states adopting in the 1920s exclusively against those never treated[Callaway and Sant'Anna, 2021]. These evaluations are reported in Table 4 and conducted while controlling for state-level population, where we find no statistically significant differences in the pre-trend pair-wise migration flows.

To further complement this exogeneity discussion we estimate two reduced form regressions. The first regresses the tax adoption $z_{i,t}$ on state-level (lagged) out-migration to directly test simultaneity. A second regression analyzes the explanatory power of state-level revenue with respect to out-migration.

[27] We argue that these metrics are representative proxies for the economic condition of the respective state.

Table 4: Statistical comparison of adoption and non-adoption states

	1910s			*1920s*		
	Tax	***No Tax***	p-value	***Tax***	***No Tax***	p-value
Panel A: Personal Income Tax						
Unemployment Rate	6.86%	6.66%	0.70	9.40%	9.13%	0.65
Average Income Score	20.35	20.62	0.80	20.56	21.85	0.12
Debt Per Capita	−0.40	−0.09	0.53	−2.71	−1.46	0.49
Panel B: Corporate Income Tax						
Unemployment Rate	6.88%	6.66%	0.58	9.39%	9.14%	0.69
Average Income Score	21.49	20.39	0.30	20.74	21.76	0.22
Debt Per Capita	−0.72	−0.04	0.23	-2.58	−1.52	0.55
Pre-trend Comparison: *with Average of* $y_{i,j,t}/p_{i,j}$						
Difference to never treated	0.0051	0.0049	0.72	0.0053	0.0043	0.14
Difference to never treated + 1910s				0.0052	0.0043	0.16

Notes: The p-values are obtained from the t-test: $\Delta = 0$ which tests for statistically significant differences between the *Tax* and *No Tax* groups. We use a two-sample t test with unequal variances. Unemployment rates are proxied by the share of labor force participants in the working population. Figures are manually computed from the full count census data. State level debt is proxied by revenue net of expenditures. Fiscal data is provided by Cassidy et al. [2024].

Origin i population is added as an additional time-varying control. Results are reported in Table 5. We find that neither lagged out-migration nor revenues have a significant effect on the income tax adoption. This indicates no evidence for potential simultaneity. Furthermore, revenues seem to be an inadequate indicator not only for tax adoptions but also migration. These findings are in line with historical records, such as Stark [1987], noting that the early adoptions were independent of additional revenue creation.

6 Baseline Results

To further motivate the utilization of a gravity model, we initial estimate a standard gravity model of migration with distance (instead of state-pair fixed effects). We find that our model fits the data generating process of bilateral migration flows very well. The remainder of this paper will use state-pair fixed effect, which control for distance and other time-invariant confounders. For the baseline, we estimate the effects of tax adoptions for the full sample. The corresponding equations 5 and 6 define $\hat{\hat{\beta}}$ as the percentage change in migration flows caused by the adoption of state income taxes in state

Table 5: Reduced form regressions to test exogeneity

	$z_{i,t}$	Out-migration: $\ln(\sum_{j=1}^{J\setminus\{i\}} y_{i,j,t})$
Out-migration: $\ln(\sum_{j=1}^{J\setminus\{i\}} y_{i,j,t-1})$	0.1199	
	(0.1475)	
$\ln(\text{Revenue}_{i,t})$	0.1861	−0.0409
	(0.1421)	(0.0476)
Origin Population: $\ln(\sum_{i=1}^{J} y_{i,j,t})$	−0.8850	1.3629
	(0.3491)**	(0.0.0864)***
State FE, Census Year FE	Yes	Yes
Observations	144	192
Adjusted R^2	0.4371	0.9864

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors underneath the estimates are estimated cluster-robust, and are clustered by state. Observations are on state level, hence $4 \times 48 = 192$ and with lag, which excludes the first census year 1900, $3 \times 48 = 144$.

i. Table 6 shows the untransformed results for Panel A: *Personal Income Tax* adoptions and Panel B: *Corporate Income Tax* adoptions. Once we account for the asymptotic bias by using the correction suggested by Weidner and Zylkin [2021] and transform the coefficient, we conclude that interstate migration flows increase by 8.70% due to the introduction of state income taxation.[28]

The magnitude of this finding is comparable in size to Cassidy et al. [2024], who identify an increase of 11.3% of interstate migration following tax adoptions between 1900 and 2010 for a subsample of young families. Similarly, Martínez [2022] finds an inflow elasticity of 7.2% following a significant income tax cut in one Swiss canton in 2006.[29] These results remain statistically significant, even after applying the most conservative estimation for standard errors (i.e. the Jackknife resampling).

We also observe an 11.28% increase in migration flows following the introduction of corporate income tax. We estimate the effects of personal and corporate taxation separately in two distinct specifications. In Section 6.2, this allows us to disentangle the effects of these two taxes. [30]

The scale of these outcomes does not come as a surprise. Previous research by Molloy et al. [2011]

[28]In absolute terms, this amounts to an average increase of pair migration flows by $1,162.44 \times 0.087 \approx 97.01$.

[29]Note that a direct comparison of our estimates to elasticities is to be interpreted with caution, as the baseline focuses merely on adoption effects.

[30]Combining both taxes into one more particularized variable would require accounting for various scenarios, such as cases where personal taxes preceded corporate taxes, vice versa, or instances where only one of them was implemented. Nevertheless, due to overlaps between personal and corporate taxation, it is challenging to perfectly distinguish the effects of personal income tax from those of corporate income tax introductions.

Table 6: Effects of income tax adoptions on interstate migration flows (equation 5)

	Base Gravity	*Panel A: Pers. Income Tax*	*Panel B: Corp. Income Tax*
Tax Introduction $\tau_{i,j,t}$		0.0802	0.1088
		$(0.0149)^{***}$	$(0.0151)^{***}$
$\ln(Distance_{i,j})$	-0.2734		
	$(0.0022)^{***}$		
Bias		-0.0033	0.0019
Bias Corrected Standard Error		$(0.0198)^{***}$	$(0.0215)^{***}$
(Percentile) Bootstrap		$(0.0346)^{**}$	$(0.0438)^{**}$
Jackknife Resampling		$(0.0415)^{*}$	$(0.0495)^{**}$
State-Decade FEs	Yes	Yes	Yes
State-Pair FE	No	Yes	Yes
Mean Dep. Variable (if $z_{i,j} = 1$)	$1,162.44$	$1,162.44$	$1,162.44$
Observations	$6,912$	$6,912$	$6,912$
Pseudo R^2	0.9632	0.9988	0.9988

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Initial standard errors underneath the estimates are estimated cluster-robust, and are clustered by state pairs. Sample contains the full count of linked individuals. Estimation with FE-PPML and a full set of fixed effects (three-way). Bias is estimated following the approach of Weidner and Zylkin [2021].

has firmly established that the United States has historically experienced significant rates of internal migration. On average, between 1980 and 2000, around one thirds of adults have relocated across states within the last five years. Their study also identifies shifts in state-to-state price differentials as a key driving force behind interstate migration. Moreover, we delve into the effects of tax changes on the extensive margin, which typically surpass effects on the intensive margin. It is plausible to assume that if alterations in variations of cross-state tax rates induce internal migration, the introduction of new tax rates should have a similar effect, potentially even stronger. Aligned with this reasoning, Akcigit et al. [2022] ascertain a mobility elasticity of 1.23 for inventors in the post 1940 period. Once again, our findings reveal a significantly greater increase in internal migration, which aligns with the notion that the introduction of new tax rates serves as a more substantial shock.

6.1 Heterogeneity

The advantage of granular data is that it allows us to count movers and stayers of different demographic and geographic groups. As we know from Borjas [1987], selection into migration varies across demographics. We also assume the effectiveness of the tax adoptions on internal migration to vary significantly among those characteristics. Table 7 summarizes the various groups we analyze. Given the substantial level of labor mobility, particularly among young adults, it is reasonable to assume that younger individuals with sufficiently high income would bear a disproportionate impact. A similar presumption can be extended to the overall labor force population. We anticipate that both these groups will not only respond to individual income taxation but also to corporate income taxation through potential firm relocation.

Table 7: Demographic and geographic groups S and their respective mean dependent variable

Name	***Description***	***Pair Out-Migration*** *Mean*	*Stand. Dev.*	***Mig. Rate***
Full Sample		$1,162.44$	$2,693.82$	20.45%
Expected positive effects of tax adoptions:				
Labor Force	Individuals in the labor force (employed, self-employed)	752.02	$1,736.52$	24.35%
Income Score	Occupational income score $\geq \$1,000$	636.42	$1,519.49$	24.17%
Young	Young adults between 15 and 30	383.13	905.24	25.14%
Young/Income Score	Intersection of young adults and income score	239.01	615.22	25.70%
Black	Black individuals	88.40	303.79	23.96%
White	White individuals	$1,070.25$	$2,574.21$	20.18%
Immigrant	Individuals born outside the US	221.92	776.11	38.11%
Literate	Individuals who can read and write	902.29	$2,080.48$	23.10%
No West	Exclude Western states ($\notin S$, robustness check)	$1,315.65$	$2,984.23$	18.80%
No expected effects of tax adoptions:				
Licensed Professions	Occupational income score $> \$5,000$: mostly attorneys and physicians	6.90	16.36	19.63%
Low Income Score	$0 <$ Occupational income score $< \$1,000$	108.14	235.98	23.61%
Farmers	Individuals owning a farm	128.11	289.71	17.86%
Homeowners	Individuals owning their dwelling or have a mortgage	476.98	$1,050.43$	16.84%

Notes: Characteristics are always defined at the beginning of the respective decade. The pair out-migration is also the mean dependent variable of our model. These statistics exclude observations for which $m_{i,j} = 0$. *Migration Rate* measures the average out-migration share of the average sample population.

Research conducted by Liu [2014] demonstrates that the implementation of state-level corporate income taxes led to a reduction in the corporate share of economic activities within those states, indicating significant business relocation. Consequently, there is a possibility of workers relocating as well. Different assumptions apply to less mobile groups, such as homeowners and farmers, who are likely to be less inclined to move across state borders. Similarly, licensed professionals like lawyers and physicians, whose licenses are often state-specific, are also expected to exhibit limited mobility due to the dependence on local reputation.[31] As a result, we anticipate this group to show a higher degree of reluctance on average towards interstate migration. In line with these conjectures are the summary statistics, presented in Table 7. We find that young individuals and immigrants exhibit a notably high

[31]Note that around 94% of the *Licensed Professions* subsample are attorneys and physicians.

relative rate of out-migration. In contrast, licensed professionals, farmers, and homeowners observe a comparatively lower rate of out-migration.

Estimating the migrational effects on these various groups poses a central challenge, as it requires comparing different sample estimates. To make statistical inference across these groups possible, we need to stack the subsets and introduce group identifiers $\mathbb{1}_s$, where $s \in S$ defines the respective group in Table 7.[32] Further, we introduce group fixed effects δ_s. In order to then estimate the effects for each subgroup in one regression, we define

$$y_{i,j,t,s} = \exp\left[\sum_s \beta_s(\mathbb{1}_s \times \tau_{i,j,t}) + \delta_{i,j,s} + \delta_{i,t,s} + \delta_{j,t,s}\right]\xi_{i,j,t,s}. \tag{7}$$

Note that we include the full sample as one specific group s.[33]

6.2 Results across Groups

To gain further insights into the demographics, we present the results for equation 7 in Figures 5 and 6. A characteristic decomposition of migrational responses to public policies is often motivated by Borjas [1987], arguing a certain level of self selection into migration, especially along the educational and income dimension.

For Panel A (Figure 5), as anticipated, the largest point estimate is registered by the group of adults in the labor force. This group is effectively targeted by the new policy. We find a 22.08% increase in migration flows for this group. Note, that this point estimate is statistically different from the point estimate of the full sample. Our findings further reveal positive impacts on young adults with a yearly income of $\$1,000$ or above, who are expected to experience significant effects from the adoption of an income tax. For this group, we find an increase of 16.28% associated with the introduction of personal income taxes. For our "control groups", including top earners, farmers and homeowners, we do not identify any significant effects on migration flows. These results further confirm the existence of a direct channel of interstate tax avoidance. That is, individuals react to the introduced taxation with

[32] A recent application of this procedure can be found in Cengiz et al. [2019].

[33] Hence we have 13 groups, each with 6,912 observations. The total number of observation therefore increases to 89,856. We do not include the "No West" subgroup in S as this coefficient is simply estimated to assure that our results are not driven by the Westward migration. We abstract from interpreting the specific effect.

migration. These results can be interpreted as historical evidence Tiebout's hypothesis, preceding his work in the 1950s.

Figure 5: ***Panel A: Personal Income Tax***

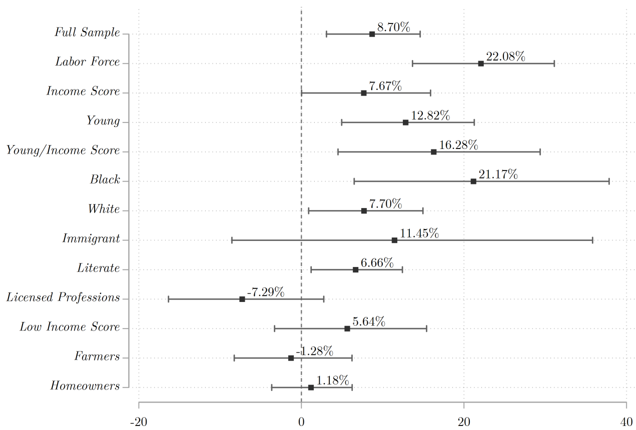
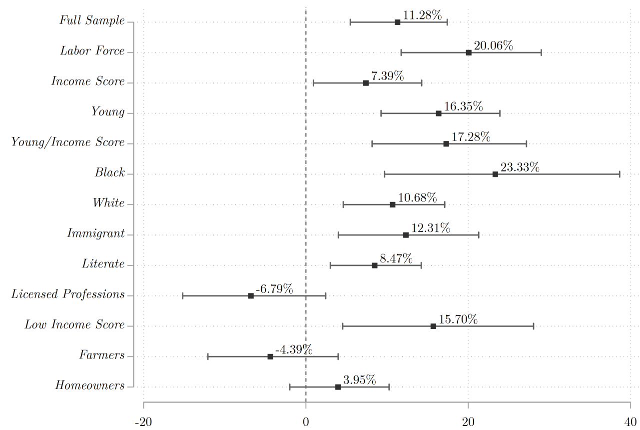


Figure 6: ***Panel B: Corporate Income Tax***



Notes: Confidence intervals and represent the 95$^{\text{th}}$ percentile. The point estimates are transformed using equation 6. Results are bias corrected using Weidner and Zylkin [2021]. Table A1 reports full results.

We aim to identify another influential factor: the personal migration stemming from corporate relocation. Though directly estimating the impact of corporate tax adoptions on firm moves is challenging, we can utilize individual migration as an indirect indicator. Liu [2014] supports our rationale by illustrating reduced corporate activities following corporate income tax adoptions, hinting at businesses moving across borders to avoid taxation. In our previous discussion (Section 2), we highlighted a newspaper excerpt from Wisconsin, one year after the income tax adoption, revealing that numerous firms had shifted operations to Illinois and Iowa. To illustrate this firm relocation, we intend to gauge the effects of corporate tax introduction on individual migration.

In order to disentangle the effects of personal and corporate income tax, we now concentrate on estimating effects within a subgroup of individuals with income below the average exemption of approximately $\$1,000$. This group covers around 8% of the total sample population. While we do not discern notable effects for this subgroup in Panel A regarding personal income tax adoptions, but we do observe substantial and statistically significant effects in Panel B, reflecting corporate tax adoptions. Specifically, in Figure 6, we report a 15.70% surge in internal migration flows among individuals unaffected by personal state income taxation. This robustly suggests the presence of a secondary mechanism through which state income tax adoptions impact interstate migration. Note that every other group is comparable with the estimates of Figure 5.

6.2.1 Is the increase in flows driven by out-migration?

As mentioned previously, our baseline model precludes us from estimating directional effects. Our theoretical framework allows us to infer that flow changes primarily encapsulate a rise in out-migration among individuals unwilling to comply with the recently introduced income taxation. In order to provide further empirical support of this claim, we will utilize the granular data structure of our census links, to estimate the tax policy effects on the discrete choice between moving across the state border or not. This allows us to infer the direction behind the estimated changes in migration flows. The corresponding binary choice logistic model can be stated as

$$\log\left(\frac{\pi_{h,i,j}}{1-\pi_{h,i,j}}\right) := \textit{logit}(\pi_{h,i,j}) = \zeta_1 z_i + \zeta_2 z_j + \phi V_h + \rho_1 W_i + \rho_2 W_j + \nu_{h,i,j}, \tag{8}$$

where $\pi_{h,i,j}$ captures the probability that for individual h we have $i \neq j$, i.e. that the individual moved across the state border. The tax implementation in both the origin and destination states is represented by variables z_i and z_j respectively. If we validate our understanding of the gravity model outcomes, wherein alterations in migration patterns are instigated by out-migration trends, then it logically follows that $\zeta_1 > 0 > \zeta_2$. This scenario implies that the introduction of a tax in the origin state increases the likelihood of out-migration, whereas its introduction in the destination state diminishes said probability. This further aligns with Cassidy et al. [2024], finding no significant impact of the early adoptions on state revenues or spendings, which could otherwise contribute to additional pull factors.

In order to mitigate endogeneity we control for individual characteristics V_h as well as state characteristics W_i, W_j.[34] Similar to Boustan et al. [2012], we estimate a series of logits for the 1910s and 1920s separately. Table 8 presents the results of this analysis for personal and corporate income tax adoptions.

For the entire series of logits we find $\hat{\zeta}_1 > 0 > \hat{\zeta}_2$. Note that, similar to the gravity estimation above,

[34] Individual controls include age, race, marital status, family size, labor force participation, occupational income score, educational attainment and homeownership. All variables are measured in the base year. State controls include the logarithm of population, unemployment rate, average occupational income score as well as the latitude and longitude of the respective origin and destination state.

Table 8: Effects of income tax adoptions on out-migration probability (equation 8)

	1910s				*1920s*			
	Personal Tax		*Corporate Tax*[†]		*Personal Tax*		*Corporate Tax*[†]	
	(i.)	*(ii.)*	*(i.)*	*(ii.)*	*(i.)*	*(ii.)*	*(i.)*	*(ii.)*
Origin z_i	0.3140 $(0.0025)^{***}$	0.2000 $(0.0027)^{***}$	0.1830 $(0.0092)^{***}$	0.3511 $(0.0100)^{***}$	0.9099 $(0.0035)^{***}$	0.0726 $(0.0039)^{***}$	0.4863 $(0.0085)^{***}$	0.0271 $(0.0111)^{**}$
Destination z_j	-0.3590 $(0.0026)^{***}$	-0.0507 $(0.0027)^{***}$	-0.0348 $(0.0093)^{***}$	-0.1700 $(0.0100)^{***}$	-1.0074 $(0.0038)^{***}$	-0.1376 $(0.0041)^{***}$	-0.4890 $(0.0089)^{***}$	-0.1421 $(0.0113)^{***}$
Indiv. Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	12,811,480	12,811,480	1,151,534	1,151,534	15,838,897	15,838,897	972,245	972,245

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Note that in order to transform estimates into odds, we have to take $\exp(logit(\pi_{i,j,h}))$. [†]The model with corporate tax is estimated for *Low Income Score* individuals only, i.e. individuals who are not directly affected by the personal income tax. This serves as a straight forward way to test for the secondary mechanism via corporate relocation.

we disentangle the effects of personal and corporate income tax adoptions by estimating the model for corporate tax introductions for a subset of individuals that are not directly affected by the personal income tax. In Table 7, we denoted this group as *Low Income Score*. The idea is to infer information on the secondary channel through which income tax adoptions affect individual migration, namely firm relocation. Here, again, $\zeta_1 > 0 > \zeta_2$ proposes that any effect on migration flows is driven by a change in out-migration. These results suggest that estimates in Sections 6 and 6.2 are indeed driven out-migration.

6.3 Border States

Another interesting exploration is to look at effects relative to geographic, and hence cultural and political, closeness. Specifically, we are interested in the baseline effects for bordering versus non-bordering states. A priori, it is not clear whether the baseline results obtained so far, are driven by migration into states of close proximity or a broader finding. Additionally, as the average migration flows for bordering states are presumably much larger, we can juxtapose relative and absolute effects. We would expect the effects on migration flows, at least in absolute terms, to be statistically larger for bordering states. For that, let us consider a modified version of equation 5

$$y_{i,j,t} = \exp\left[\beta_1 \tau_{i,j,t} + \beta_2(\tau_{i,j,t} \times b_{i,j}) + \delta_{i,t} + \delta_{j,t} + \delta_{i,j}\right] \varsigma_{i,j,t}, \tag{9}$$

where the indicator function $b_{i,j} = 1$ if origin i and destination j are bordering states.[35] The separate term for $b_{i,t}$ is dropped due to multicollinearity with the pair fixed effect. For the comparison of absolute changes in the migration flows we next need to compute the mean dependent variables $\bar{y}^m_{i,j,t}$ for both values of $b_{i,j}$.[36]

Table 9: Mean dependent variable $\bar{y}^m_{i,j,t}$ by border dummy $b_{i,j}$

	Mean	***Standard Deviation***	***Number of Observations***
for $b_{i,j} = 1$	$5,527.88$	$5,866.48$	621
for $b_{i,j} = 0$	721.42	$1,547.94$	$6,147$

Of course, as for each state i there are more non-bordering states than bordering states, we report that number of observation for $b_{i,j} = 0$ is much larger. However, as to be expected, the mean dependent variable is much larger for $b_{i,j} = 1$. Individuals migrate predominantly to states relatively close to their origin i. With these insights, we can now estimate the model of equation 9.

Table 10: Estimates of tax adoption effects for the border dummy specification

	Panel A: Personal Income Tax	***Panel B: Corporate Income Tax***
Tax Introduction $\tau_{i,j,t}$	0.0916	0.1390
	$(0.0187)^{***}$	$(0.0196)^{***}$
Interaction $\tau_{i,j,t} \times b_{ij}$	-0.0220	-0.0594
	(0.0222)	$(0.0226)^{***}$
$\hat{\beta}_1 + \hat{\beta}_2$	0.0696	0.0797
	$(0.0185)^{***}$	$(0.0176)^{***}$
Observations	6,912	6,912
Pseudo R^2	0.9988	0.9988

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors underneath the estimates are estimated cluster-robust, and are clustered by state pairs. Sample contains the full count of linked individuals.

We observe a slightly reduced relative effect for state pairs sharing a border, with $\hat{\beta}_1 + \hat{\beta}_2$ being statistical significance. Notably, the marginal effect for $b_{i,j} = 1$ is 7.21%, indicating a magnitude smaller than the overall effect. While this seems contradictory to the idea that migration costs increase in distance,

[35] Note that we define $b_{i,i} = 1$.

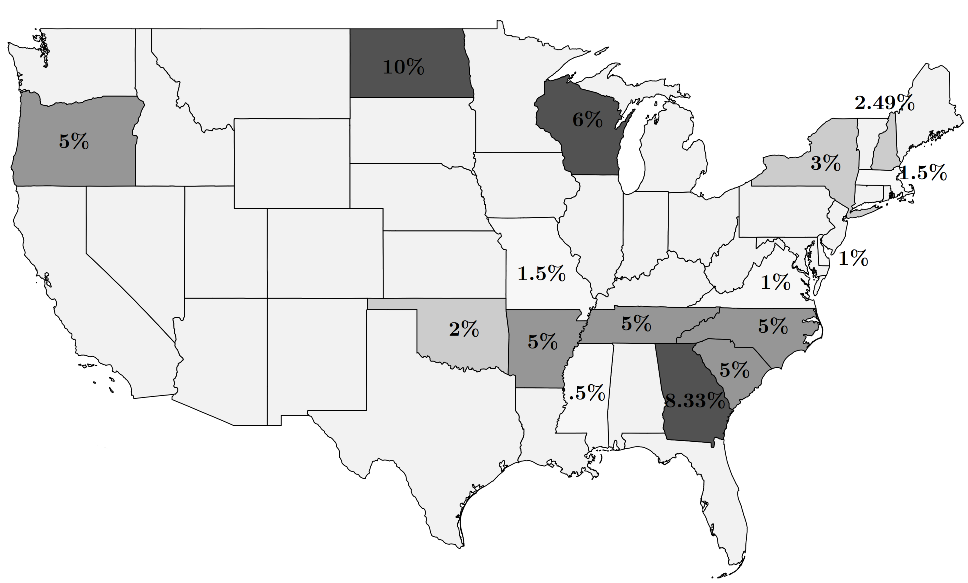
[36] We compute the mean dependent variable for a subset of N where $i \neq j$, i.e. $m_{i,j} =^1$ we define as N^m. There are 48 states for 3 time periods, hence $N^m = 6,912 - 144 = 6,768$. For notational simplicity, can then define $\bar{y}^m_{i,j,t} = 1/N^m \sum_i = y_{i,j,t} \times m_{i,j}$.

it highlights the complexity of migration responses. It is crucial to account for variations in the base migration, i.e. the mean dependent variable in this context. Specifically, the mean absolute change in migration flows for bordering states resulting from the introduction of personal income taxation is $5,527.88 \times 0.0721 \approx 398,29$. Conversely, the mean absolute change in migration flows for non-bordering states stands at $721.42 \times 0.0959 \approx 69.20$. Unsurprisingly, we find a much larger estimated average increase of migration flows among bordering states. A similar conclusion can be drawn for the adoption of corporate taxation. Here, the mean absolute changes in migration flows for bordering states is 458.44 and 107.61 for non-bordering states. We conclude that the effect in absolute terms was significantly larger in bordering states.

6.4 Adopted Tax Rates

Another essential aspect of the tax rollouts were the adopted rates. States differed with respect to the design implemented to tax their constituents. Some states, such as Mississippi introduced a flat income tax rate, others, such as Wisconsin, adopted a progressive tax regime. The maximum income tax rate in the various states is presented in Figure 7, with an average of 3.9%. We can see that there is a large heterogeneity across states. The maximum adopted tax rate differential is 9.5 percentage points.

Figure 7: Maximum personal income tax rates at the time of adoption



Note that, however, an investigation of the effects of these differences in tax rates are not equivalent to effects on the intensive margin per se. We rather analyze the effects of the "intensity" of the introduction. In order to proceed, we first have to restate our model. Recall that the variable of interest $\tau_{i,j,t} = z_{i,t}m_{i,j}$ captures the binary nature of the introduction, without considering the intensity of the treatment. We model the tax adoption as a reduction in migration costs $d_{i,j,t} = c_{i,j}/\exp(\alpha\tau_{i,j,t})$, where our empirically results from Section 6 established that $\alpha > 0$.

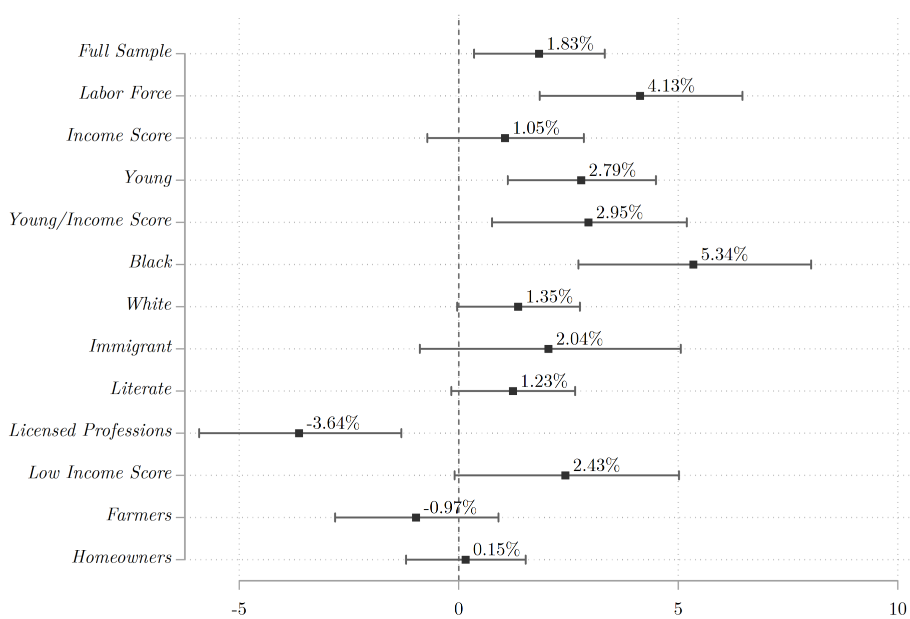
Now, let us define $d^r_{i,j,t} = c_{i,j}/\exp(\alpha^r\tau^r_{i,j,t})$, where $\tau^r_{i,j,t} = (z_{i,t}m_{i,j})r_i$. Here, r_i captures the maximum personal income tax rate at the time of the adoption. The gravity equation is equivalent to equation 7, but now capturing, the effects of the treatment intensity through the adopted maximum tax rates.

$$y_{i,j,t,s} = \exp\left[\sum_s \left(\mathbb{1}_s \times \left\{(\theta-1)(\alpha^r_s\tau^r_{i,j,t} - \ln c_{i,j}) + \ln\left(\frac{p^{\gamma_{1,s}}_{i,t}}{p_t\left(\omega^{(1-\theta)}_{i,t}\right)}\right) + \ln\left(\frac{p^{\gamma_{2,s}}_{j,t}}{\psi^{(1-\theta)}_{j,t}}\right)\right\}\right)\right]\eta_{i,j,t,s}. \tag{10}$$

Here, $\beta^r = (\theta-1)(\alpha^r)$ measures the marginal effect of a one percentage point higher adopted maximum income tax rate on interstate trade flows. Conceptually, we presume $\beta > \beta^r > 0$.

Figure 8 reports the estimation results for equation 10.

Figure 8: Maximum personal income tax rate at the time of the adoption



Notes: Confidence intervals and represent the 95th percentile. The point estimates are transformed using equation 6. Results are bias corrected using Weidner and Zylkin [2021]. Table A2 reports full results.

As expected, we find qualitatively similar effects to Figure 5, but quantitatively on a much smaller scale. The overall effect of a one percentage point larger adopted personal income tax rate increases interstate migration by about 1.83%. These results are similar to findings by Akcigit et al. [2022] who estimate a mobility elasticity of 0.34% for US inventors after the 1940s. As these mobility results are yearly, our estimate is comparably small. Note however, that we estimate the mobility responses for the entire population.

Moreover, we observe a similar overall pattern, including especially strong and significant effects for individuals in the labor force. We again find insignificant effects for farmers and homeowners, whereas licensed professions are now significantly negatively affected.[37]

6.5 Additional Mechanistic Insights and Robustness

To gain further insights on the underlying mechanism as well as strengthen the robustness of our results, we explore various alternative specifications. Robustness checks will be juxtaposed against our primary model outlined in equation 2.

Years since adoption As the effects measured by the baseline are supposed to be interpreted as decennial changes in migration flows, it is worth to explore the development of these effects within the first ten years of the adoption. Recall that migration flows are measured at the end of a respective decade. This allows to use the year of adoption relative to the end of the respective decade of adoption to estimate dynamic effects of the tax adoption. To do so, we define a variable

$$l_{i,t} = \begin{cases} r + 10 - \textit{Year of Introduction}_i & \text{if state } i \text{ adopts in decade } t \\ 0 & \text{otherwise}^{38} \end{cases} \tag{11}$$

Recall that $t = \{r, r+10\}$, with $r \in \{1900, 1910, 1920\}$. Then, we can define $\tau^{ysa}_{i,j,t} = \tau_{i,j,t} l_{i,t}$ as a measure of treatment effects weighted by the years since the adoption. This allows to estimate the effects on a yearly basis, despite our data being decennial. In order to investigate non-linearity of the

[37] We can statistically compare the coefficients $\hat{\beta}$ and $\hat{\beta}^r$. We find a chi-squared test statistic of 16.22 with 1 degree of freedom. The corresponding p-value is below 0.001, indicating that the observed difference between the two policies is statistically significant.

tax adoption effects over time, we add a polynomial term to our specification such that

$$y_{i,j,t} = \exp\left[\vartheta_1 \tau^{ysa}_{i,j,t} + \vartheta_2 (\tau^{ysa})^2_{i,j,t} + \delta_{i,t} + \delta_{j,t} + \delta_{i,j}\right] \varrho_{i,j,t}. \tag{12}$$

This regression equation allows us to capture a potential curvature of the tax adoption effects. Here, $\tilde{\beta}_1$ can be interpreted as the percentage change in bilateral migration flows due to the tax being introduced one year earlier.

We report the estimation results in Table 11. First, we find statistically significant non-linear effects of the tax adoptions. The policy's positive effect on interstate migration is increasing within the initial years of adoption.

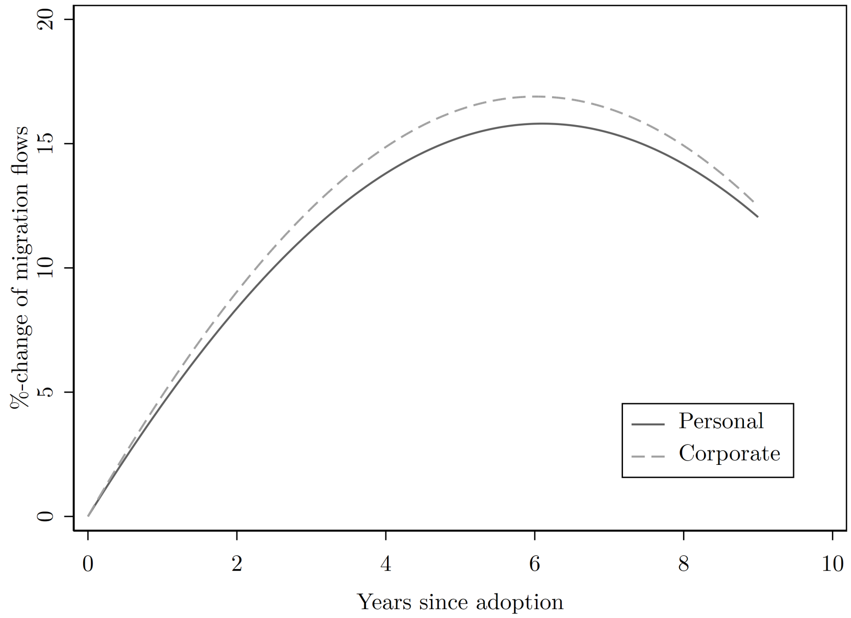
Table 11: Estimates of tax adoption effects with respect to years since adoption

	Panel A: Personal Income Tax	***Panel B: Corporate Income Tax***
$\tau^{ysa}_{i,j,t}$	0.0481	0.0519
	(0.0113)***	(0.0196)***
$(\tau^{ysa}_{i,j,t})^2$	−0.0039	−0.0043
	(0.0015)***	(0.0013)***
Observations	6,912	6,912
Pseudo R^2	0.9988	0.9988

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors underneath the estimates are estimated cluster-robust, and are clustered by state pairs. Sample contains the full count of linked individuals.

To graphically illustrate the concavity of the adoption effects, we plot the effects with respect to years since the introduction in Figure 9. To plot these lines, we use estimates from equation 12. We find that the increasing positive effects on migration flows last for around 6 years before decreasing and converging back to zero. Note that the time dimension does not exceed 9 years, as there is no state in our sample that adopts the tax at the turn of a decade. This exercise provides evidence that the adoption effects estimated by the baseline strategy are averages of a non-linear process over time. In line with the interpretation of the main results, the initial years after the adoption seems to be the driving force behind these average effects.

Figure 9: Non-linear adoption effects with respect to years since adoption



Notes: The lines represent the polynomial function $\left[\exp(\hat{\vartheta}_1 \tau^{ysa}_{i,j,t} + \hat{\vartheta}_2 (\tau^{ysa})^2_{i,j,t}) - 1\right] \times 100$ with $\tau^{ysa}_{i,j,t} \in [0, 9]$ to represent percentage changes in bilateral migration flows.

County-level analysis Another advantage of the census data is that we know not only the state of residence of every individual but also the county. This allows to augment the analysis with additional heterogeneity as we can now include within-state variation in location. Here, the most important information is the distance to the border. Section 6.3 has established that the absolute increase in migration flows due to the tax adoptions is larger between border states. Now we can further analysis if proximity to the border plays a role. To investigate this, we define $w_1(i)$ the origin county in state i and $w_2(j)$ the destination county in state j. Note that if $w_1 = w_2$, we have $i = j$. The county-level analysis allows us to differentiate between effects right at the border versus towards the middle of a respective adopter state. To do so, we rely on a generalized distance-to-border measure provided by Holmes [1995]. This allows us to find the shortest distance from every county center to the closest state border and denote is as $D_{w_1(i)}$.[39] We use the gravity framework to estimate the effects of the tax adoption interacted with the distance measure to assess whether the baseline results, as well as the border state results from Section 6.3, are driven by migration at the border. The estimation equation

[39]Note that the distance is measured in miles.

can be stated as

$$y_{w_1(i),w_2(j),t} = \exp\left[\varphi_1\tau_{i,j,t} + \varphi_2 D_{w_1(i)} + \varphi_3(\tau_{i,j,t} \times D_{w_1(i)}) + \delta_{i,t} + \delta_{j,t} + \delta_{i,j}\right] \daleth_{i,j,t}. \tag{13}$$

Assuming that the tax effect on out-migration is linearly decreasing in the county's distance to the border, we can estimate the fixed point $-\hat{\varphi}_1/(\hat{\varphi}_2+\hat{\varphi}_3)$ at which the introduction has no effect anymore. This pivotal point can then be further used as a cutoff for an indicator $\mathbb{1}(D_{w_1(i)} > -\hat{\varphi}_1/(\hat{\varphi}_2 + \hat{\varphi}_3))$ instead of the continuous distance measure itself. This allows to estimate the direct impact of proximity to the border, or lack thereof, instead of a continuous interaction. Table 12 presents the results for both the continuous measure and the dummy approach.

Table 12: County-level estimates of tax adoption effects with respect distance to border

	Panel A: Personal Income Tax		***Panel B: Corporate Income Tax***	
	(Continuous)	*(Indicator)*	*(Continuous)*	*(Indicator)*
$\tau_{i,j,t}$	0.3464 (0.0901)***	0.1548 (0.0864)*	0.1534 (0.0918)*	0.1772 (0.0864)**
$D_{w(i)}$	−0.0019 (0.0113)		−0.0020 (0.0005)***	
$\tau_{i,j,t} \times D_{w(i)}$	−0.0083 (0.0001)***		−0.0011 (0.0009)	
$\mathbb{1}(D_{w_1(i)} > -\hat{\varphi}_1/(\hat{\varphi}_2 + \hat{\varphi}_3))$		−0.1877 (0.0361)***		−0.1886 (0.0361)***
$\tau_{i,j,t} \times \mathbb{1}(D_{w_1(i)} > -\hat{\varphi}_1/(\hat{\varphi}_2 + \hat{\varphi}_3))$		−0.2005 (0.0428)***		−0.1672 (0.0468)***
Observations	28,344,940	28,978,992	28,344,960	28,978,992
Pseudo R^2	0.4865	0.4859	0.4865	0.4859

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors underneath the estimates are estimated cluster-robust, and are clustered by state pairs. Sample contains the full count of linked individuals.

First, we note that, as presumed, the bilateral migration effect of the tax adoption is clearly and significantly decreasing with the distance to the border. The fixed point is estimated to be at around 34.03 miles away from the border. Figure A5 plots the negative distance measure, where we observe that county borders are coded to have a distance measure below the estimated fixed point. This lends support to the hypothesis that the main results are driven by individuals who live closer to the border. The effects seem to be even stronger geographically concentrated for the adoption of the corporate income tax. This insight is further facilitated by the findings of the indicator model. Here, the assignment to the group of counties that lie outside the fixed point distance, has a negative effect on migration flows. To summarize the results of this exercise, it seems that the baseline results are driven by out-migration of individuals living close to a state border.

Destination state tax This exercise involves estimating the migratory effects of a tax adoption indicator that takes into consideration whether the destination state has implemented the income tax. We assume that individuals aiming to evade income taxation within their own state would be less likely to relocate to another state that also enforces income taxation. Hence, we define a new $\tau^*_{i,j,t} = 1$ iff $z_{i,t} > z_{j,t}$.[40] The purpose of this alternative investigation is to ascertain whether the outcomes presented in Table 6 primarily stem from individuals relocating to states that do not impose state income taxes. This serves as a supplementary measure to validate our hypothesis concerning tax avoidance. Correspondingly, the findings detailed in Table A3 (column (i) and (v)) closely mirror our principal results. As a consequence, we deduce that the observed estimations are indeed influenced by significant migrations into states without state income taxation. With this newly constructed indicator, we can, similar to Moretti and Wilson [2017], further estimate the effects excluding the observations where $m_{i,j} = 0$, i.e. the non-movers. Ideally our results should not be solely driven by the inclusion of these observations as they primarily serve as a support for the identification of $\tau_{i,j,t}$. However, note that the exclusion of these observations will inevitably lead to different point estimates as we drop important information from the control group. And indeed, Table A3 reports smaller point estimates in columns (ii) and (vi). Qualitatively, our findings are robust to the exclusion of these observations.

Influence of westward expansion To account for the effects of the overall Westward migration, we repeat the regression analysis reported in Table 6 without the most Western states.[41] If the Westward migration, which was still prevalent in the early 20$^{\text{th}}$ century, had an effect on our results, we would

[40] Note that including the case where $z_{i,t} < z_{j,t}$ does not change the estimation outcome, as again the in- and out-migration across our sample is perfectly symmetric.

[41] We define the most Western states to be Washington, Oregon, California, Idaho, Nevada, Utah and Arizona.

expect for this subset to find lower overall effects of the tax introductions.[42] We present our findings in columns (iii) and (vii) of Table A3 and we observe convincing similarities between these results and our baseline estimates. It is crucial to emphasize, however, that since this pertains to a different sample, a quantitative juxtaposition of the coefficients with those in Table 6 is inappropriate. Hence, we find no evidence of the aforementioned bias. Hence, we conclude that the Westward migration did not alter our estimation results.

Do single states drive results? Another robustness exercise seeks to discern if the outcomes of our analysis in 6 are driven by specific individual states. To that end, we will replicate our main analysis, each time excluding a distinct state that implemented an income tax within the period of 1910 to 1930. We report the results in Figure A3 and Figure A4. We note that, qualitatively, dropping single states does not change our main results in any way.

End-of-decade adopters Some states implement their income taxes at the very end of a respective decade, allowing us to investigate potential anticipation effects. If anticipation significantly influences our estimates, reclassifying end-of-decade adopters into the next decade should decrease the point estimates relative to the baseline.[43] The results, presented in columns (iv) and (viii) of Table A3, show that the point estimates are slightly larger compared to the baseline, suggesting that there are no significant anticipation effects regarding the tax adoptions.

Condensed tax measure Given the significant overlap in states adopting personal and corporate income taxes (see Figures 3 and 4), Cassidy et al. [2024] chose to condense these policies into a single introduction dummy. Unlike this condensed measure, our main specification partially disentangles the two introductions. Nonetheless, it is a good robustness exercise to estimate the effects of the condensed policy variable. We find an 11.66% increase due to the early adopted income taxes. These results are inline with our baseline estimation as well as figures for post-WWII introductions, estimated by Cassidy et al. [2024].

[42]Note that the reason for not including a bilateral Western state indicator as a control is that such geographic variables are constant over time and hence should be absorbed by the fixed effects.

[43]Besides testing for anticipation effects, this alternative definition of introduction also allows us to further check robustness.

7 Conclusion

Tax competition is an important side effect of decentralized fiscal policy. As we have shown, this phenomenon dates back to the introduction of personal and corporate state income taxes in the US in the early 20$^{\text{th}}$ century. Using a migration gravity model with multilateral resistance and applying it to linked US census waves between 1900-1930, we causally identify the impact of state tax introductions on migration flows. We find a substantial 8.7% increase in interstate migration flows due to the introduction of personal income taxation. Additionally, using a logit model we show that our estimates are driven by out-migration, and we find larger effects among young individuals with above-median income. Our refined analysis, incorporating adopted tax rates, provides insights into the intensive margin of these effects, showing that a one percentage point increase in the introduced maximum tax rate corresponds to a 1.83% rise in interstate migration. These results extend to a secondary channel through which income taxation influences interstate migration — specifically, the adoption of corporate income taxes raises migration flows by 11.3%. This suggests that corporate taxation's impact extends beyond financial considerations, potentially driving away industries and thereby affecting the movement of workers. Bordering states experienced a particularly pronounced impact, especially in absolute terms. Robustness checks show that our results are not driven by individual states or the broader Westward migration trend, which was notable during that period. We further show that our estimation technique is robust to the usage of alternative specifications. Finally, we confirm that estimated migration effects are indeed driven by migration into states that do not adopt an income tax. All together, we find significant effects of foot voting in the early 20$^{\text{th}}$ century United States, lending credible support to the original Tiebout Hypothesis [Tiebout, 1956]. While the transfer of these historical results to the current setting should proceed with caution, our results hold lessons for contemporary policymakers. For example, the debate over differential state income tax policies in the USA continues.[44] Likewise, modern day Europe reveals many similarities to our historical setting, including the free movement of people. Our analysis thus helps to advance the understanding of how a fiscally federal structure affects the European Union. Contemporary policymakers should consider that the relocation effects of fiscal policy differentials might be sizeable.

[44] From 2017 to 2018, the US Census Bureau registered a doubling in net migration flows from California to Texas (more than 50,000), which coincides with a substantial state tax differential.

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A Graphics & Tables

Table A1: Estimates of tax adoption effects across groups (stacked regressions)

	Panel A: Personal Income Tax	***Panel B: Corporate Income Tax***
Full Sample: $\tau_{i,j,t}$	0.0834	0.1069
	(0.0270)***	(0.0274)***
Labor Force: $\tau_{i,j,t}$	0.1995	0.1828
	(0.0364)***	(0.0367)***
Income Score: $\tau_{i,j,t}$	0.0739	0.0713
	(0.0376)**	(0.0317)***
Young: $\tau_{i,j,t}$	0.1206	0.1514
	(0.0368)***	(0.0321)***
Young/Income Score: $\tau_{i,j,t}$	0.1508	0.1594
	(0.0545)***	(0.0413)***
Black: $\tau_{i,j,t}$	0.1920	0.2097
	(0.0659)***	(0.0598)***
White: $\tau_{i,j,t}$	0.741	0.1014
	(0.0333)**	(0.0288)***
Immigrants: $\tau_{i,j,t}$	0.1084	0.1161
	(0.01009)	(0.0392)***
Literate: $\tau_{i,j,t}$	0.0645	0.0813
	(0.0269)**	(0.0263)***
Licensed Professions: $\tau_{i,j,t}$	-0.0757	-0.0703
	(0.0525)	(0.0449)
Low Income Score: $\tau_{i,j,t}$	0.0548	0.1458
	(0.0451)	(0.0518)***
Farmers: $\tau_{i,j,t}$	-0.0129	-0.0449
	(0.0374)	(0.0428)
Homeowners: $\tau_{i,j,t}$	0.0117	0.0387
	(0.0249)	(0.0300)
Observations	85,968	85,968
Pseudo R^2	0.9986	0.9987

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors underneath the estimates are estimated cluster-robust, and are clustered by state pairs. Sample contains the full count of linked individuals.

Table A2: Estimates of adoption maximum tax rate effects across groups (stacked regressions)

	Panel A: Personal Income Tax
Full Sample: $\tau_{i,j,t}$	0.01811
	$(0.0075)^{***}$
Labor Force: $\tau_{i,j,t}$	0.0404
	$(0.0113)^{***}$
Income Score: $\tau_{i,j,t}$	0.0105
	(0.0090)
Young: $\tau_{i,j,t}$	0.0275
	$(0.0084)^{***}$
Young/Income Score: $\tau_{i,j,t}$	0.0291
	$(0.0110)^{***}$
Black: $\tau_{i,j,t}$	0.0521
	$(0.0128)^{***}$
White: $\tau_{i,j,t}$	0.0134
	$(0.0070)^{*}$
Immigrants: $\tau_{i,j,t}$	0.0202
	(0.0149)
Literate: $\tau_{i,j,t}$	0.0122
	$(0.00071)^{*}$
Licensed Professions: $\tau_{i,j,t}$	-0.0370
	$(0.0122)^{***}$
Low Income Score: $\tau_{i,j,t}$	0.0240
	$(0.0127)^{*}$
Farmers: $\tau_{i,j,t}$	-0.0098
	(0.0096)
Homeowners: $\tau_{i,j,t}$	0.0015
	(0.0069)
Observations	85,968
Pseudo R^2	0.9987

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors underneath the estimates are estimated cluster-robust, and are clustered by state pairs. Sample contains the full count of linked individuals.

Table A3: Robustness: Effects of income tax adoptions on interstate migration flows

	Panel A: Personal Income Tax				*Panel B: Corporate Income Tax*			
	(i)	*(ii)*	*(iii)*	*(iv)*	*(v)*	*(vi)*	*(vii)*	*(viii)*
Tax Introduction $\tau_{i,j,t}$			0.1045 $(0.0152)^{***}$	0.1039 $(00173)^{***}$			0.1084 $(0.0152)^{***}$	0.1231 $(0.0170)^{***}$
Tax Introduction $\tau^*_{i,j,t}$	0.0849 $(0.0178)^{***}$	0.0534 $(0.0288)^{*}$			0.1312 $(0.0186)^{***}$	0.0683 $(0.0288)^{**}$		
Bias	−0.0039	0.0066	−0.0059	−0.0088	−0.0008	0.0061	−0.0036	−0.0083
Bias Corr. St. Error (Percent.) Bootstrap Jackknife Resampling	$(0.0237)^{***}$ $(0.0337)^{**}$ $(0.0481)^{*}$	(0.0319)	$(0.0187)^{***}$	$(0.0222)^{***}$	$(0.0255)^{***}$ $(0.0407)^{***}$ $(0.0528)^{**}$	$(0.0316)^{**}$	$(0.0195)^{***}$	$(0.0217)^{***}$
Observations	6,912	6,768	5,043	6,912	6,912	6,768	5,043	6,912
Pseudo R^2	0.9988	0.9898	0.9990	0.9988	0.9988	0.9898	0.9990	0.9988

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Initial standard errors underneath the estimates are estimated cluster-robust, and are clustered by state pairs. Sample contains the full count of linked individuals. Estimation with FE-PPML and a full set of fixed effects (three-way). Bias is estimated following the approach of Weidner and Zylkin [2021].

Figure A1: Government partisan affiliation in charge of personal income tax

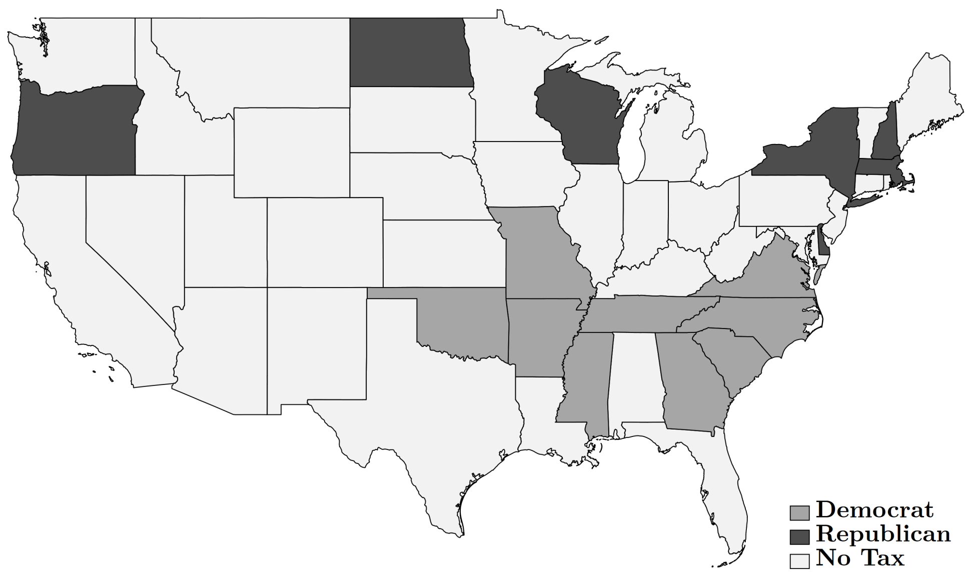


Figure A2: Government partisan affiliation in charge of corporate income tax

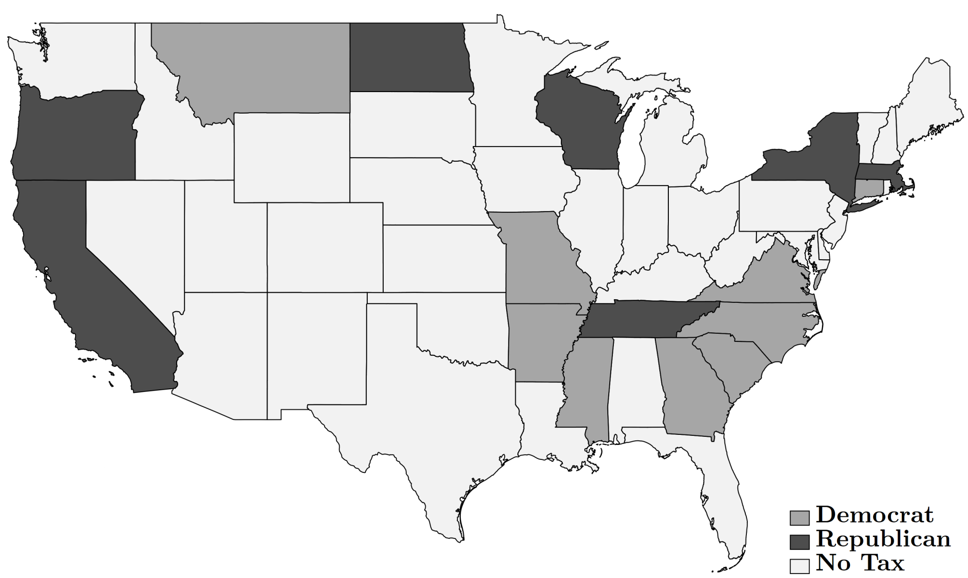


Figure A3: ***Panel A: Personal Income Tax***

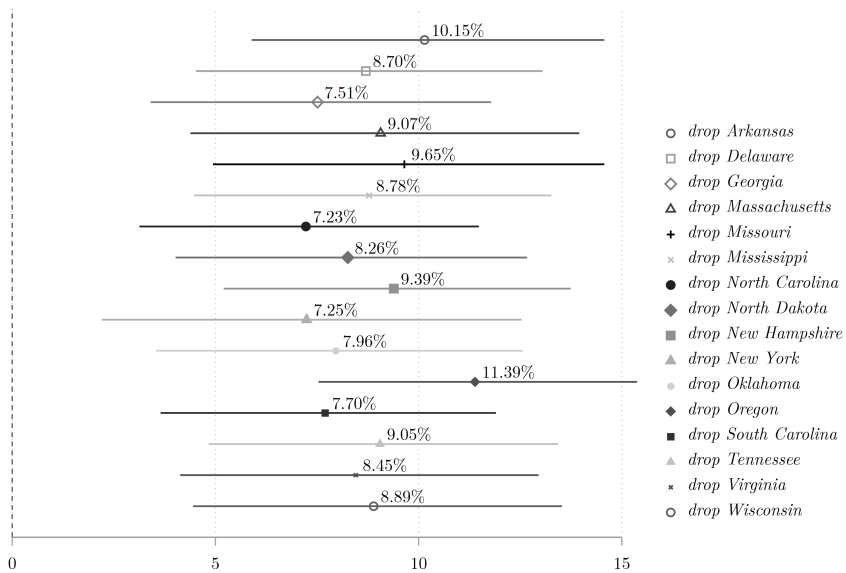
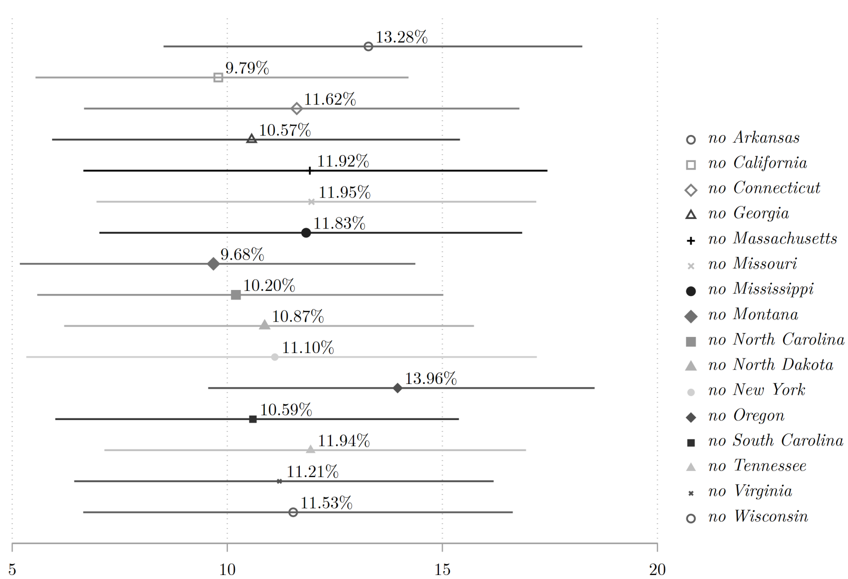
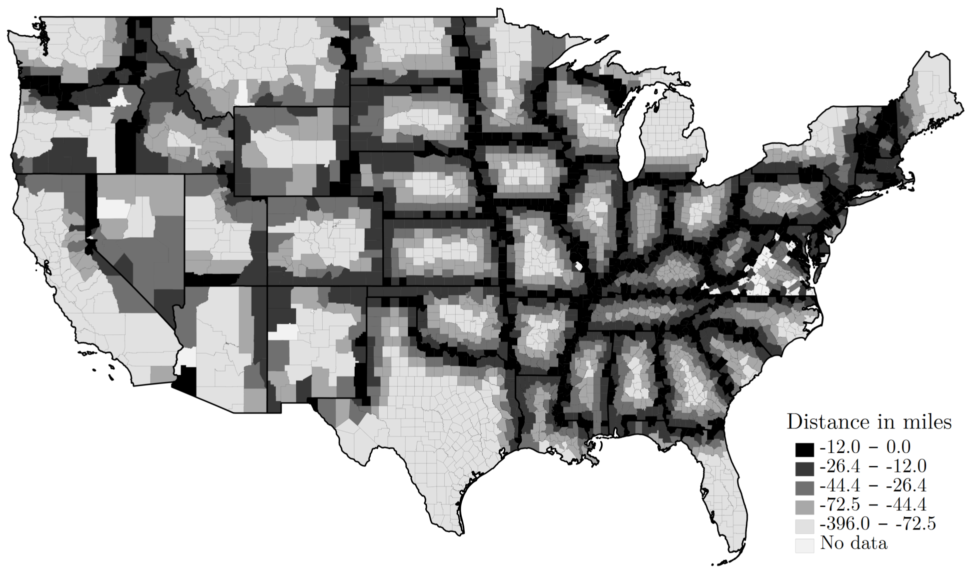


Figure A4: ***Panel B: Corporate Income Tax***



Notes: Confidence intervals are bias corrected and represent the 95th percentile. The point estimates are transformed using equation 6. Number of observation for each iteration is 6,627.

Figure A5: Distance of county center to the closest state border



Notes: The distance is measured away from the center being at the border, hence $-D_{w(i)}$.