CORPORATE TAX ADOPTIONS AND FIRM RELOCATIONS*

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Abstract

This paper studies how firm location choices respond to a newly adopted corporate income tax. I exploit the staggered introduction of state corporate taxes in the United States between 1910 and 1930, when 16 states adopted taxation on business income at varying rates and with different sectoral coverage. Using newly linked census data to construct bilateral interstate firm flows, I find that tax adoption increased outflows by 13%. Decomposing this effect, I show that 15–20% reflects a pure adoption response independent of the tax rate level, consistent with fixed compliance costs that do not scale with rates. The remaining effect operates through rate levels. Relocation responses are concentrated among manufacturing, mercantile, and service firms, decline with distance from state borders, and diminish after five years as mobile firms exit. Counterfactual analysis implies early adopters would have retained over 1,600 firms collectively had they not introduced the tax.

Keywords: Business Taxation, Firm Location, State Taxation, Tax Avoidance, U.S. Economic History

JEL Codes: H25, H71, N32, R30

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I. INTRODUCTION

Do firms strategically relocate their operations in response to the introduction of a new tax? Understanding the behavioral responses of firms to corporate taxation is central to evaluating tax policy [Fuest and Neumeier, 2023]. While the existing literature has predominantly focused on the effects of tax rate changes on corporate location and investment decisions [Devereux and Griffith, 1998, 2003; Suárez Serrato and Zidar, 2016], the extensive margin of taxation, the initial adoption of a corporate income tax, remains largely unexplored. This paper provides new evidence on this question by exploiting the staggered introduction of state corporate income taxes in the early 20th century United States.

The empirical investigation of firm relocations in response to corporate taxation faces an array of difficulties. Tax differentials on the extensive margin are mostly observed across countries, where firm relocations are constrained by legal, financial, cultural, and language barriers. International firm relocations are further difficult to track, and profit shifting through separate accounting is prevalent. This paper circumvents these challenges by using the unharmonized introduction of state income taxes in the U.S. between 1910 and 1930 as a natural experiment, when 16 of the then 48 states adopted taxation on corporate income. The variation in adoption timing, tax rates, and sectoral coverage across states provides an ideal setting to estimate the causal effects of tax adoption on firm mobility.

I argue that the transition from zero to positive taxation represents a fundamentally different policy shock than changes in existing tax rates. Beyond the direct financial burden, a newly introduced corporate income tax imposes fixed compliance costs through administrative reorganization, record-keeping requirements, and dedicated personnel for reporting and audits [Slemrod and Blumenthal, 1996; Sandford, 1995]. The extensive margin introduces an additional dimension through a "status quo bias", which makes the initial adoption particularly salient for firm behavior [National Industrial Conference Board, 1930; Einhorn, 2008]. This is supported by historical evidence as contemporary accounts document widespread firm threats to relocate, including prominent enterprises such as Ringling Bros., which announced plans to move from Wisconsin to Illinois following the state's pioneering 1911 tax adoption.

I leverage newly linked census data on employers, which serve as a proxy for firms. The linking algo-

rithm, based on Abramitzky et al. [2021], allows decennial matching of the U.S. population between 1900 and 1930. This is the first comprehensive dataset identifying bilateral interstate firm relocation flows across the entire United States in the early 20th century. With this, I construct a state-pair firm relocation dataset capturing 48×48 pairs across three decades. The methodological framework adapts a structural gravity model to firm relocations, following a discrete choice among the set of all states. This allows modeling bilateral interstate flows over time, incorporating the entire universe of state-specific push and pull factors, and facilitating causal inference of partial equilibrium effects. To analyze effects beyond adopter states, the model further permits estimating changes in total interstate firm flows in a conditional general equilibrium context through counterfactual analysis.

The estimation reveals that corporate tax adoption increased bilateral firm relocations by 13% within a decade. These results withstand a battery of robustness checks, such as accounting for sector-specific agglomeration and the adoption of personal income taxes. Decomposing this effect, I show that 15–20% reflects a pure adoption response independent of the tax rate level, consistent with fixed compliance costs that do not scale with rates. The remaining effect operates through rate levels. Effects are concentrated among manufacturing and mercantile firms, with slightly larger effects for services and public utilities. The financial sector, historically subject to regulatory and legal constraints on mobility, exhibits no significant response.

Several analyses sharpen the interpretation and support the compliance cost mechanism. Relocation effects become statistically insignificant beyond 214 miles from state borders, confirming that geographic costs matter even conditional on deciding to move. I further document that effects peak approximately five years after adoption before diminishing, suggesting a temporary shock rather than a permanent shift in outflows. Decomposing by tax type shows that corporate tax adoptions, not personal income taxes, drive the results, validating that findings reflect firm responses to corporate taxation specifically. Direct evidence for the mechanism comes from showing that corporate tax adoptions increased administrative staffing by 14%. The implied firm stock elasticities are comparable to the literature, though several factors suggest these represent lower bounds. Mediation analysis reveals that roughly 9% of the decline in corporate activity documented by Liu [2014] can be attributed to relocations. Finally, the counterfactual exercise, in which corporate taxes were not introduced, finds large negative effects on total firm outflows in adopter states. New York alone would have retained nearly 500 firms had it not adopted the tax; the total across all adopters exceeds 1,600 firms. Non-adopter states observe

positive counterfactual changes through general equilibrium spillovers, demonstrating that tax policy in adopting states affects the entire spatial equilibrium.

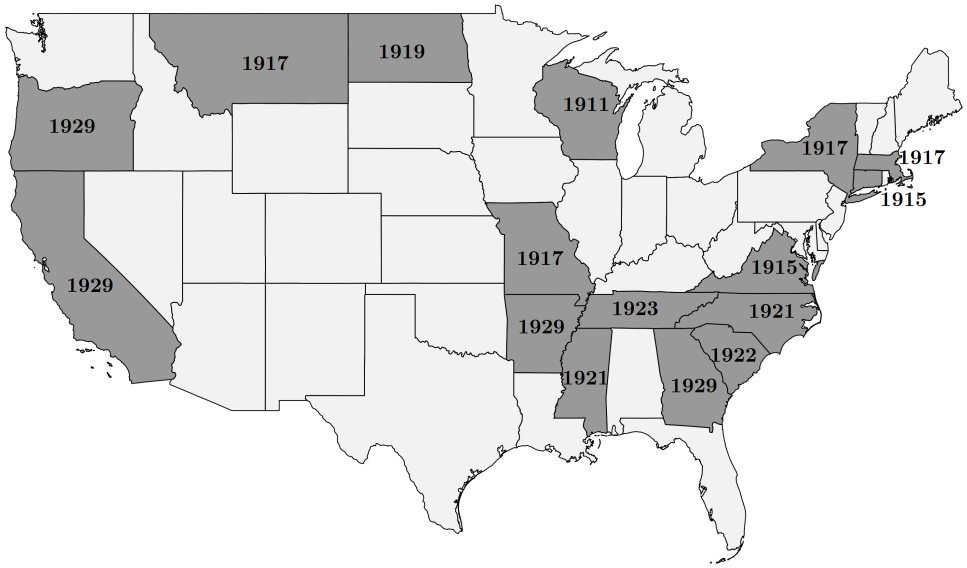
This paper contributes to several strands of the literature. First, it provides the first estimates of firm relocation responses to corporate tax adoptions rather than rate changes. The existing literature on corporate tax base mobility has focused on the intensive margin, finding that regional tax cuts increase taxable corporate income through mobility [Brülhart et al., 2023], with particularly strong effects in less urban areas [Krapf and Staubli, 2020]. Corporate taxes have also been shown to affect workers, with evidence that labor bears roughly half the tax burden through lower wages [Fuest et al., 2018]. Yearly elasticities of corporate location to tax rates have been estimated between 0.4 and 0.8 [Giroud and Rauh, 2019; Suárez Serrato and Zidar, 2016; Fajgelbaum et al., 2019]. This paper extends this literature by examining the extensive margin, capturing fundamentally different firm responses, particularly the fixed compliance costs and status quo bias that make initial adoption salient. Second, I contribute methodologically by utilizing bilateral state-pair data to estimate elasticities on flows rather than stocks, allowing direct measurement of firm relocations instead of inferring them from changes in establishment counts. The gravity framework with high-dimensional fixed effects absorbs time-varying push and pull factors, addressing identification concerns present in cross-sectional comparisons [Devereux and Griffith, 2003]. Third, the paper contributes to the economic history literature on Progressive Era taxation. The implementation of state-level income taxes has been shown to increase out-migration of workers [Smutny and Wandschneider, 2024; Cassidy et al., 2024] and inventors [Akcigit et al., 2022]. This paper demonstrates that firms, like individuals, vote with their feet, but through mechanisms specific to corporate taxation, particularly the compliance cost channel documented here.

The findings have implications for contemporary debates about corporate tax policy. As countries and subnational jurisdictions compete for corporate activity, understanding how firms respond to the introduction of new taxes, and not just changes in rates, informs the design of tax systems [Fuest and Neumeier, 2023]. The evidence that compliance costs, not just tax burdens, drive relocation suggests that tax simplification may complement rate-setting as a tool for retaining corporate activity.

II. WHY WERE THE STATE INCOME TAXES INTRODUCED?

Between 1910 and 1930, sixteen states began taxing domestic firms on the basis of net income [National Industrial Conference Board, 1930]. The federal structure of the U.S., provided by the Tenth Amendment to the Constitution in 1791, enabled states to levy their own taxes, even prior to the adoption of a federal income tax. As depicted in Figure I, the first state to pass a state-specific corporate income tax law was Wisconsin in 1911. In 1913, followed the rollout of a federal corporate income tax with the Sixteenth Amendment to the Constitution. Note that states in different regions are affected by the role out. The South observes many introductions. However, note that the income taxes were rolled out unharmonized and over the course of multiple years. While the federal rate was introduced at 1%, the maximum introduced tax rate was in Wisconsin, with 6%, and the average adopted rate was 2 - 3%, depending on the progressivity of the introduced tax regime.

FIGURE I: Introduction of the Corporate Income Tax



Notes: This figure shows the staggered introduction of state-level corporate income taxes across the U.S. Information on adoptions are taken from the National Industrial Conference Board [1930] and the Advisory Commission and American Council on Intergovernmental Relations [1995].

The National Industrial Conference Board [1930], as well as many contemporaries [Comstock, 1920; Lutz, 1920; Bigham, 1929], argue that the decision to introduce an income tax on businesses was built on challenges and resulting insufficiencies of the property tax. Local governments, which were responsible for the collection of property tax revenues, often decided to strategically undervalue properties to reduce their respective share of state levy. Many contemporary scholars stress that this helped local politicians to grow their political platform and increased chances of re-election. This widespread

form of structural tax evasion, left many states with deficient revenue streams during a time of rapid population growth and hence exponential increase in demand for government spending. The income tax can hence be interpreted as a complement to the existing state-level fiscal landscape [Wallis, 2000]. Different to the property tax, the income tax was introduced to be collected centrally by the state governments. Revenues were to be redistributed to local governments, such as counties or cities. As the objective was primarily to mitigate forgone revenues, states did not observe a large increase in government spendings due to the tax introductions short run.[1]

To further explore driving factors of the tax adoptions, Online Appendix C provides an empirical analysis of potential predictors of the income tax. Neither firm nor migration flows seem to affect the tax adoptions. This helps to argue against potential bias through simultaneity. The political environment, measured by two party alignment dummies (Governor and presidential election), and economic circumstances such as unemployment rates, revenues and expenditures do not play a significant role either. The only significant predictor of the corporate income tax adoption is the share of incorporated manufacturing businesses (plotted in Figure B.III in Online Appendix B). I find that this share is negatively correlated with the adoptions, indicating a certain degree of lobbing and political opportunity, in line with findings by Berry and Berry [1992]. The new tax on income was furthermore introduced into a period of significant political and (socio-) economic changes, including a reorganization of outdated tax systems [Fishback, 2019].

While the theoretical tax base, i.e. net income, was the same in each state, there was subtle variation in the legal nature of these taxes. Seven states adopted the corporate tax as a form of franchise tax on the "right to do business". The main reason was to prevent legal issues concerning the so-called uniform rule, under which property cannot be taxed differently through the property and corporate tax. Many argued that income derived from property can therefore not be taxed separately.[2] States adopting a corporate tax on net income decided to specifically exempt income from property holdings.

Another constitutional challenge was to prevent the violation of the Commerce Clause of the Federal Constitution, under which no tax is allowed to be levied on interstate commerce. States predominantly tax domestic corporations on the part of income derived within the state. For multi-state corporations,

[1] This is empirically supported by findings of Figures B.I and B.II in Online Appendix B using data from Cassidy et al. [2024] to conduct a simple event study of the tax adoption effects on state-level revenues and expenditures based on Callaway and Sant'Anna [2021], where I do not find significant increases for either metrics.

[2] However, most states have ruled that the corporate tax, defined as a tax on net income, is not a tax on property and hence concluded no violation of the uniform rule.

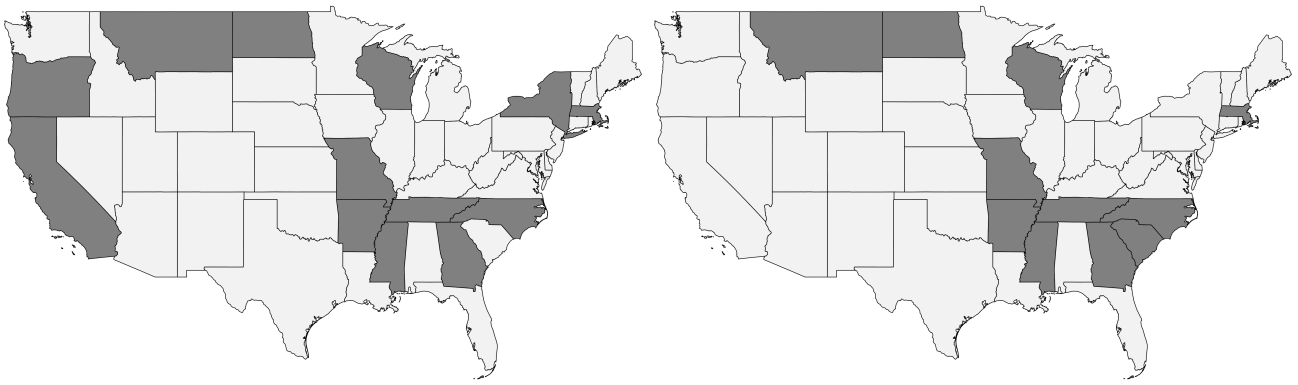
profit shifting became very attractive, especially under separate accounting. Wisconsin, which introduced the "blueprint law" for many other states, therefore adopted the corporate income tax with formulary apportionment [Weiner, 2005], which was aimed to prevent this practice by replacing separate accounting. The Supreme Court's ruling in a case of a Delaware corporation paying taxes in Connecticut established that state corporate income taxation based on reasonable apportionment formulas was in alignment with the Commerce Clause [The Underwood Typewriter Company vs. Frederick S. Chamberlain, 1919].

As captured by the National Industrial Conference Board [1930], the primary focus of state corporate income tax adoptions were the manufacturing and mercantile businesses as well as the service sector, which were taxed in all adopter states. As presented in Figure IIa, thirteen of these states imposed income taxes on financial institutions such as banks and insurance companies. Eleven states, shown in Figure IIb, levied taxes on railroad or other public utilities. The National Industrial Conference Board [1930] uses a rather rudimentary classification of sectors. At this time period, manufacturing and mercantile companies encapsulated a wide range of industries. Hence for better understanding of the types of firms taxed, I quote from the Connecticut Public Acts of 1915, which explicitly defined the companies exempt from the states' new tax.

> *Sec. 19. The term 'company' as used in this part shall include every corporation, joint stock company, or association carrying on business in this state which is required to report to the collector of internal revenue for the district in which such company has its principal place of business and for the purpose of the assessment, collection, and payment of an income tax, except insurance and trust companies, state banks, savings banks organized under the laws of this state, banking institutions organized under the laws of the United States and located in this state, express companies carrying on business on steam or electric railroads or street railways, steam or electric railroad or street railway corporations, companies whose principal business in this state is furnishing, leasing, or operating dining, sleeping, chair, parlor, refrigerator, oil, stock, fruit, or other cars, corporations whose principal business is manufacturing, selling, and distributing illuminating or heating gas, or electricity to be used for heat, light, or motive power, or water for domestic or power purposes, telegraph, cable, and telephone companies.* [*Connecticut Public Acts of 1915, Chapter 292, §19-23 inclusive* (*now General Statutes, §1391-1395*)]

Notes: The maps shows a sector-specific corporate income tax introductions. The figures reveal that financial and utility companies where only taxed by a subset of the adopter states.

FIGURE II: Corporate Income Tax Adoptions by Sectors



(**a**) States taxing financial corporations

(**b**) States taxing railroad and other public utility corporations

II.A. How was the new tax policy received?

The introduction of corporate income taxation was highly contested. Many saw a potential for losing business due to the relocation of firms to states without income taxation. Several reasons can be presented. First, changes in tax policies on the extensive margin introduce additional fixed costs for firms. This can be perceived as an increased bureaucratic burden, particularly for larger enterprises, which may incur higher labor costs due to the need for more legal and administrative staff. The direct impact of new taxes on the net income of businesses can further influence their relocation decisions, especially for firms already considering leaving their origin state. Additionally, a historically documented sentiment against taxation combined with a status quo bias led to firm owners to move their businesses out of state [Einhorn, 2008]. Many contemporary scholars and politicians advocated against the adoption of these taxes, frequently using potential business outflows as a main argument. In Wisconsin, State Senator Henry W. Bolens elaborate that state income taxation

> *The Wisconsin state income tax law is a penalty levied upon the frugal and industrious. It denies to industry its full reward. When industry is not rewarded, industry ceases.* [*Wausau Pilot on August 13, 1912*]

Hence, beyond the financial considerations, historical sources reveal that a sense of injustice may have lead to firms leaving their current state. Senator Bolens further expounds:

> [...] *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*]

There is anecdotal evidence of businesses announcing their plans to relocate to states without income taxation, which further sparked widespread discussion. An article dated August 13, 1912, in the Wausau Pilot highlighted the impact of state income taxation, noting the departure of notable firms such as Frazier in Manitowoc, Presto-Lite in Milwaukee, and The Brunswick-Balke-Collander in Janesville. The owner of the American Thresherman, an influential farming magazine out of Madison, publicly threatened to leave the state unless the income tax was repealed. Additionally, the esteemed Ringling Bros., a circus enterprise based in Baraboo, Wisconsin, warned of moving its operations to Chicago, 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.* [...] *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*]

It is unsurprising that businesses had the ability to, at the very least, pose a credible threat to leave their origin state, given the early adoption during the federal expansion of interstate transportation.[3] In the aftermath of Wisconsin's implementation of state taxes, a growing number of businesses announced their intentions to relocate unless the income tax is rescinded. This controversy over corporate income tax has now garnered national attention.

> *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*]

The narrative evidence from Wisconsin aligns with findings by Liu [2014], suggesting that the adoption of income taxes in the early 20th century led to a decline in corporate activity in the affected states. Nevertheless, it remains to be demonstrated whether this reduction in corporate activity is specifically attributed to firms relocating to states that have not instituted an income tax on net corporate income.[4] This highlights the importance of examining this historical context beyind the opportunity to estimate elasticities for tax policy changes on the extensive margin.

[3] As detailed in Tarr and McMurry [1922], by 1910, every state had been integrated into the interstate railroad system, with a substantial portion also accessible through the interstate water routes system. The culmination of the railroad system occurred in 1916 (see Stover [2008]) followed by subsequent expansions in the interstate highway system, as discussed by Paxson [1946]. This historical context supports the notion that even larger enterprises had ample opportunities to cross state borders.

[4] More on that in Section V and Online Appendix E.

III. DATA

To create data on bilateral firm flows, it is required to obtain establishment level data for each state. As discussed in Vickers and Ziebarth [2017], there exists no usable data for the establishment level census of manufactures between 1880 and 1929 In order to fill this substantial gap in the literature, I use full count historical census data of the early 20th century, provided by IPUMS (see Ruggles et al. [2021]). Individuals coded as "employers" can be used as a proxy for their own firms. The census traces business owners, specifically including owners of corporations, as owners and board members are, by the definition of the Bureau of the Census (e.g. Census of Manufactures [1929]), not employees of their businesses.[5] The sample includes unincorporated firms as well, any results should be interpreted as a lower bound. The sample further does not allow identifying publicly traded firms, which, however, account for less than 10% of all corporations in 1910 [Hannah, 2015]. In Online Appendix D, I further show that the number of employers are strongly correlated with the number of firms. This allows to construct a comprehensive dataset of individual-level employer data for the entire U.S. Linking consecutive census waves allow tracking employers across time which enables observing relocations [Abramitzky et al., 2021; Taft, 1970].

TABLE I: Linked Data Summary

Link	Total (in thous.)	Labor Force	Employers Ratio	Mfg & Merc Ratio	Linking Sucess
1900 - 1910	10,133.9	73.51%	15.51%	93.07%	-
1910 - 1920	12,882.6	72.11%	8.54%	90.86%	26.81%
1920 - 1930	15,925.6	73.47%	6.02%	86.46%	30.48%

Notes: Labor Force defines the share of individuals in the labor force. Employers Ratio computes employers as share of labor force. Mfg & Merc Ratio defines manufacturing and mercantile employers as share of all employers. Linking Success reports the percentage of employers in the onset of a decade successfully matched to the end of the decade, i.e., the onset of the next decade. All values computed at decade end.

Table I reports a descriptive overview of the linked census waves. While the share of individuals in the labor force stays constant across the waves, the amount of employers in the labor force is drastically decreasing over time. The reason behind this is the decline of employers in the agricultural sector. The U.S. was facing a transition from small scale operations to corporate farming [Raup, 1973; Riney-Kehrberg, 2018]. This trend is also observed for the overall share of manufacturing and mercantile

[5]The 1900 census lacks employment status data. I extrapolate this from the 1910 census, classifying 1900 individuals as employers if they meet three criteria: (1) classified as employers in 1910, (2) remained in the same occupation, and (3) worked in the same industry. This approach may underestimate 1900 employers due to transitions between employer and own-account worker status, but prioritizes minimizing false positives (Type I errors). The resulting false negatives (Type II errors) are less consequential given that own-account workers faced no corporate income tax during this period.

businesses.

III.A. Bilateral State Flows

To capture the movement of firms across state borders, I create a dyadic dataset, which quantifies the total number of employers relocating from any given state i to another state j.[6] Hence, a single observation is indexed by a state pair i, j in the respective decade $t = \{t(b), t(e)\} \in \mathcal{T}$, composed of the link between two consecutive census waves. Note that, $t(b) \in \{1900, 1910, 1920\}$ and $t(e) = t(b) + 10$. The dyadic data consists of $3 \times 48^2 = 6,912$ observations. The aggregation can be repeated for several sectors. The full-count census data provides a wide set of sectors (see Table B.I in Online Appendix B). Table II reports dyadic summary statistics for firms, overall, as well as a decomposition by some aggregated industries. A finer decomposition can be found in Table B.II in Online Appendix B.

TABLE II: Average Internal Relocations and Sample Population for Firms of Aggregate Industries

	Pair Outflow		**State Outflow**		**State Sample Population**		
Panel	Mean	Stand. Dev.	Mean	Stand. Dev.	Mean	Stand. Dev.	*Mobility Rate*
All Firms	54.01	129.62	2,551.63	2,390.22	15,739.85	13,783.31	16.21%
Manufacturing and Mercantile	49.25	118.47	2,325.88	2,167.13	14,667.49	12,641.73	15.86%
Services	2.94	9.48	139.51	193.84	662.11	946.44	21.07%
Financial Sector	0.51	1.96	24.19	37.59	121.54	195.88	19.91%
Transport. and Other Pub. Util.	0.78	2.41	36.76	46.96	171.04	225.34	21.49%

Notes: The table presents summary statistics for bilateral interstate firm relocation flows and state-level populations across aggregated sectors. A single observation represents a directed state pair (i, j) in decade t, yielding $3 \times 48^2 = 6,912$ total observations. *Pair Outflow* measures the number of employers relocating from state i to state j during the decade, which serves as the dependent variable in the main gravity specifications. *State Outflow* aggregates total relocations from state i to all other states. *State Sample Population* represents the total number of employers in state i at the decade start. *Mobility Rate* is computed as the ratio of average state outflow to average state sample population, measuring the propensity to relocate. All statistics exclude within-state pairs (i, i). Sector classifications aggregate finer industry categories (detailed in Table B.II) to align with state-specific tax exemptions, particularly for financial institutions and public utilities. The Manufacturing and Mercantile sector drives overall patterns due to its large share of sample firms and universal taxation across adopter states.

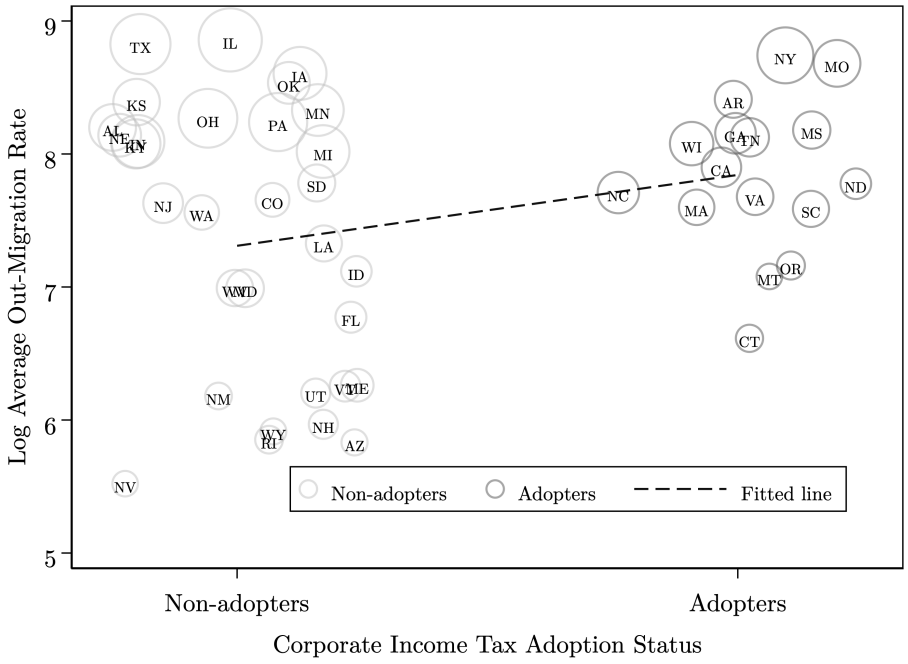
Firms are on average less mobile than the overall population. That is indicative of employers and their firms to be tied to their origin and hence face higher costs of relocating. I observe noticeable variability across sectors, particularly in the context of pair outflow, with the manufacturing and mercantile sector exhibiting the highest average pair outflow. Any other sector observes a drastically lower average pair outflow. Services observe a larger mobility rate, while the financial sector is less mobile n average. The ratio of average state outflow to average state population is mainly driven by an overall low number

[6] The requirement for an individual to be counted as an employer in the dyadic setting is to be coded as an employer at the onset of the respective decade. This approach reflects that the stock of potential movers is determined at the decade start (formalized in Section IV). I do not require employment status at the decade end since I cannot observe the exact relocation timing or whether businesses closed before or after moving (see Online Appendix F).

of firms in my sample rather than a general reluctance of relocation. Findings from Table B.II suggest that the aggregation of sectors into broader categories is imperative as otherwise most sectors lack the necessary variation. As presented in Section II, certain states selectively applied the new corporate tax solely to specific groups of industries. The aggregation on this broader level leads to fewer state dyads with no firm flow.

The final dataset allows to gather further motivational evidence. States that adopt a corporate income tax at some point prior to 1930, observe a larger average rate of firm outflows. Figure III shows the log of average state-level outflows for adopters and non-adopters in my sample. This indicates overall larger observed outflows for adopter states.

FIGURE III: Adopter assignment and total log-outflows



Notes: This figure shows the relationship between corporate income tax adoption status and average log firm outflows at the state level. Each circle represents a state, with filled circles denoting non-adopters and hollow circles denoting adopters of corporate income tax before 1930. The size of each circle is proportional to the average state-level stock of firms. The dashed line represents the fitted linear relationship, which demonstrates that states adopting corporate income taxes experienced systematically higher firm outflows compared to non-adopting states during this period.

IV. A GRAVITY MODEL OF FIRM RELOCATIONS

The underlying structural framework is inspired by McFadden et al. [1973], Anderson [2011] and Beine et al. [2016] to derive a tractable model of firm relocation. First, recall that decade t can be split into

the onset $t(b)$ and the end of the respective decade $t(e)$. I define $f_{i,t(b)}$ as the stock of firms in state i at $t(b)$. The relocation flow $y_{i,j,t(e)}$ from i to $j \in \mathcal{J}$ during decade t, realized at $t(e)$, can be stated as an accounting identity, such that

$$y_{i,j,t(e)} = \phi_{i,j,t(e)} \times f_{i,t(b)} \times g(t), \tag{1}$$

where $\phi_{i,j,t(e)} \in [0, 1]$ defines the share of relocating firms from i to j realized by the end of the decade, i.e. $t(e)$. As this is a dynamic formulation, $g(t)$ weights the accounting identity by a generalized time trend for all i. I further define $t(d)$ as a point in time during decade t such that $t(b) \leqslant t(d) \leqslant t(e)$. To derive the expected value of $\phi_{i,j,t(e)}$, I will use a standard random utility maximization model, where firms optimize over relocation profits such that

$$\Lambda_{h,i,j,t(d)} = \left[\underbrace{\alpha p_{j,t(d)} - \gamma c_{i,j} + \nu_{h,i,j,t(d)}}_{\breve{\Lambda}_{h,i,j,t(d)}} \right] + (\beta^F + \beta^r \text{Rate}_i)\tau_{i,j,t(d)} \tag{2}$$

where $\Lambda_{h,i,j,t(d)}$ (and $\breve{\Lambda}_{h,i,j,t(d)}$) defines the profit maximization of moving from i to j such that $j = \arg\max\{\Lambda_{h,i,j,t(d)}\}_{j \in \mathcal{J}}$. Note that $i \in \mathcal{J}$. Here, $p_{j,t(d)}$ defines a deterministic profit component, including location factors such as real estate prices, average wages or the stock of firms $f_{j,t(d)}$ at the point of potential departure of firm h and is assumed to be constant across origins. The constant relocation costs, e.g. distance between any two states, are defined as $c_{i,j}$. Furthermore, $\nu_{h,i,j,t(d)}$ is an idiosyncratic profit component driven by firm characteristics [Van Dijk and Pellenbarg, 2000].

Additionally, I define the corporate income tax adoption shock $\tau_{i,j,t(d)}$ as an indicator of i relative to j during decade t. The tax adoption affects relocation profits through two distinct channels. First, the introduction of a corporate income tax imposes compliance costs on firms, e.g., hiring accountants, establishing bookkeeping systems, and managing audit obligations, that are largely independent of the tax rate level. Second, the tax rate itself reduces after-tax profits proportionally. I formalize this by decomposing

$$\beta = \beta^F + \beta^r \text{Rate}_i, \tag{3}$$

where $\beta^F \geqslant 0$ captures the rate-independent fixed cost component and $\beta^r \geqslant 0$ weights the adopted tax

rate Rate_i of state i.[7] The composite parameter β can be interpreted as a relative increase in the cost of operating in the origin state for firm h (i.e. $i = j$). Lastly, α and γ serve as respective weights for pull factors and relocation costs. Note that $\beta = \Lambda_{h,i,j,t(d)} - \breve{\Lambda}_{h,i,j,t(d)}$ if $\tau_{i,j,t(d)} = 1$, since the tax adoption defines a shock to the relocation decision process. Hence, $\beta > 0$ holds if the tax adoption increases the relocation profits. I assume that the distribution of $\nu_{h,i,j,t(d)}$ determines that moving to j is optimal for firm h. McFadden et al. [1973] demonstrated that if $\nu_{h,i,j,t(d)}$ follows an iid extreme value type 1 distribution, then the expected value of $\phi_{i,j,t(e)}$ and $y_{i,j,t(e)}$, defined by equation (1), can be expressed as shown in the equation below:

$$\mathbb{E}(y_{i,j,t(e)}) = \left[\underbrace{\frac{\exp(\alpha p_{j,t(d)} + \beta \tau_{i,j,t(d)} - \gamma c_{i,j})}{\sum_{k \in J} \exp(\alpha p_{k,t(d)} + \beta \tau_{i,k,t(d)} - \gamma c_{i,k})}}_{\mathbb{E}(\phi_{i,j,t(e)})} \right] \times f_{i,t(b)} \times g(t). \tag{4}$$

Proposition 1 (Tax Adoption and Interstate Firm Flows) *Let the relocation profit of firm h located in state i from moving to state $j \in \mathcal{J}$ at time $t(d)$ be given by equation (2) with $\beta = \beta^F + \beta^r \text{Rate}_i > 0$ and $\nu_{h,i,j,t(d)}$ iid Type-I extreme value. Define the expected bilateral flow as $\mathbb{E}(y_{i,j,t(e)}) = \mathbb{E}(\phi_{i,j,t(e)}) \times f_{i,t(b)} \times g(t)$. Then:*

(*i*) *The expected flow admits a gravity representation:*

$$\mathbb{E}(y_{i,j,t(e)}) = \exp(\beta \tau_{i,j,t(d)}) \times \frac{\texttt{Push}_{i,t(d)} \times \texttt{Pull}_{j,t(d)}}{\texttt{Costs}_{i,j}} \times f_{i,t(b)} \times g(t), \tag{5}$$

where $\texttt{Pull}_{j,t(d)} = \exp(\alpha p_{j,t(d)})$, $\texttt{Costs}_{i,j} = \exp(\gamma c_{i,j})$, *and*

$$\texttt{Push}_{i,t(d)} = \left[\sum_{k \in \mathcal{J}} \frac{\exp(\beta \tau_{i,k,t(d)}) \times \texttt{Pull}_{k,t(d)}}{\texttt{Costs}_{i,k}} \right]^{-1}.$$

(*ii*) *For any $j \neq i$, the adoption of a corporate income tax in state i (i.e., $z_{i,t} = 1$) strictly increases the expected interstate flow $\mathbb{E}(y_{i,j,t(e)})$ relative to the counterfactual of no adoption ($z_{i,t} = 0$).*

The proof can be found in Online Appendix A. It follows from equation (3):

Corollary 1.1 (Rate Intensity) *Under the conditions of proposition 1 with $\beta^r > 0$, the expected interstate flow $\mathbb{E}(y_{i,j,t(e)})$ for $j \neq i$ is strictly increasing in the adopted tax rate* Rate_i.

[7] The additive separability assumes that compliance costs do not interact with the rate level. This is defensible to a first-order approximation: firms require the same bookkeeping infrastructure whether the rate is 2% or 6%.

The proof follows directly from the monotonicity of the ratio between expected outflows of adopter and non-adopter states established in the proof of proposition 1 (see Online Appendix A). The gravity formulation of equation (5) is based on the assumption that push factors are independent of the destination state j [Beine et al., 2016]. The estimation strategy will include fixed effects to appropriately handle this assumption. I further impose a stochastic error component $\varepsilon_{i,j,t(d)}$ onto expression equation (5). I assume $\mathbb{E}(\varepsilon_{i,j,t(d)}|\cdot) = 1$. Log-linearization of this model entails a problem with the random error assumption as $\mathbb{E}(\ln \varepsilon_{i,j,t(d)}) \leqslant \ln \mathbb{E}(\varepsilon_{i,j,t(d)})$ which implies $\mathbb{E}(\ln \varepsilon_{i,j,t(d)}) \neq 1$ [Silva and Tenreyro, 2006]. Without assuming homoskedasticity, this means that $\ln \varepsilon_{i,j,t(d)}$ is correlated with the other right-hand side variables. I will instead re-write the multiplicative expression such that

$$y_{i,j,t(e)} = \exp\left[\beta \tau_{i,j,t(d)} + \ln(\texttt{Pull}_{j,t(d)}) + \ln\left(\texttt{Push}_{i,t(d)} \times f_{i,t(b)}\right) - \ln(\texttt{Costs}_{i,j}) + \ln\left(g(t)\right)\right]\varepsilon_{i,j,t(d)}. \tag{6}$$

Note that, as states introduced taxes at different rates (and levels of progressivity), β captures the average change in firm flows in a state pair for an average introduced rate. The structural decomposition of equation (3) directly implies a separation of the total adoption effect into extensive and intensive margin components.

Proposition 2 (Separability of Adoption and Rate Effects) *Let $\beta = \beta^F + \beta^r \text{Rate}_i$ as in equation (3), with $\beta^F \geqslant 0$ and $\beta^r \geqslant 0$. Denote by $\mathcal{A}$ the set of adopter states. Then:*

(*i*) *The average adoption effect admits an additive decomposition:*

$$\beta = \underbrace{\beta^F}_{\textit{Pure Adoption Effect}} + \left\{ \underbrace{\beta^r}_{\textit{Intensive Margin}} \times \frac{1}{|\mathcal{A}|} \sum_{i \in \mathcal{A}} \text{Rate}_i \right\}. \tag{7}$$

(*ii*) *Under the assumption that the marginal effect of an additional percentage point of taxation is constant across rate levels, β^r is identified from a specification that interacts the adoption indicator with the tax rate, and the pure adoption effect is recovered as*

$$\beta^F = \beta - \left\{ \beta^r \times \frac{1}{|\mathcal{A}|} \sum_{i \in \mathcal{A}} \text{Rate}_i \right\}. \tag{8}$$

(*iii*) *If the introduction of a corporate income tax imposes strictly positive fixed compliance costs (i.e., $\beta^F > 0$), then the total adoption effect strictly exceeds the rate-proportional component: $\beta > \beta^r \times \frac{1}{|\mathcal{A}|} \sum_{i \in \mathcal{A}} \text{Rate}_i$.*

Part (i) follows directly from averaging equation (3) across adopter states. Part (iii) is immediate from $\beta^F > 0$. The key identifying assumption is in part (ii): the marginal effect of an additional percentage point is constant across rate levels, i.e., the effect of increasing the rate from 2% to 3% is the same as from 5% to 6%. This allows extrapolation to the boundary at zero and thereby isolates the discontinuity between the absence and presence of a tax regime. The jump from $\text{Rate}_i = 0$ to any $\text{Rate}_i > 0$ includes β^F on top of the rate-proportional component $\beta^r \times \text{Rate}_i$ which is the pure adoption effect attributable to fixed compliance costs, administrative burdens, and policy uncertainty. It is recovered by estimating an adapted version of equation (6) with the interaction $\tau_{i,j,t(d)} \times \text{Rate}_i$ as the policy variable.

V. IDENTIFICATION AND ESTIMATION PROCEDURE

The estimation strategy adds state-decade $(\delta_{i,t}, \delta_{j,t})$ fixed effects, which entirely absorb the terms $\ln(\texttt{Pull}_{j,t})$ and $\ln(\texttt{Push}_{i,t} \times f_{i,t})$ [Beine and Parsons, 2015; Bertoli and Moraga, 2015].[8] Hence, the state-decade fixed effects control for the entire universe of time-varying push and pull factors. I further include a decade-interstate fixed effect $\delta_{i\neq j,t}$. This allows to control for U.S.-wide structural changes, i.e. $g(t)$, such as advances in transportation technologies and the overall shift from agriculture to manufacturing operations [Bergstrand et al., 2015; Nagengast and Yotov, 2024]. Finally, I set $\ln(\texttt{Costs}_{i,j})$ equal to state-pair fixed effects $\delta_{i,j}$ [Weidner and Zylkin, 2021]. With that, I re-write equation (6) as

$$y_{i,j,t} = \exp\Big[\beta\tau_{i,j,t} + \delta_{i,j} + \delta_{i\neq j,t} + \delta_{i,t} + \delta_{j,t}\Big]\varepsilon_{i,j,t}. \tag{9}$$

I refer to this baseline specification as a four way-way fixed effects (4WFE) model. Note that this is essentially a state-pair version of a TWFE model, including additional fixed effects to capture push and pull factors. Next, I identify the tax adoption dummy, which my model requires to vary both in origin state i and destination state j. I utilize intrastate flows, i.e. stayers, to create a pair-wise moving policy variable [Beverelli et al., 2018]. I construct the adoption shock $z_{i,t} = 1$ if state i adopts the corporate income tax in decade t, Then, I define $\tau_{i,j,t} := z_{i,t}\mathbb{1}(i \neq j)$. Hence, $\tau_{i,j,t}$ computes the flow-impact of the adoption relative to firms staying in their state of origin i. Hence, the coefficient β captures the percentage increase in interstate flows due to the introduction of a corporate tax in the origin. The timing of the dummy captures the adoption shock by defining $z_{i,t+1} = 0$ if $z_{i,t} = 1$. There are two reasons for this approach. First, I want to capture the short run effects of the tax adoptions, i.e., the reloca-

[8] For notational tractability, the time dimension will be given by decade t.

tion effects upon impact of the ne tax. Second, I aim to analyze the tax consequences independently of any shifts in public goods provision. In the short run, the adopted taxes were used as a substitute for property tax revenues and therefore did not have any measurable effects on government spending (see National Industrial Conference Board [1930]; Cassidy et al. [2024] and Figures B.I and B.II in Online Appendix B). However, in the medium run (i.e. after a decade), state revenues from income taxes increased. Heightened public goods and infrastructure investments resulting from increased tax revenue could have incentivized firms to move to state i. To prevent conflating these effects, the main specification limits the analysis to a single period, avoiding the complexity of simultaneous influences. Alternatively, the traditional approach of constructing the timing of the policy variable is to set $z^{trad}_{i,u\in\mathcal{T}} = 1,\ \forall\ u \geqslant t$. As the short term policy variable only measures the effects of the introduction within the range of ten years, it can be expected that $\widehat{\beta} < \widehat{\beta}^{trad}$.

TABLE III: Examination of Pre-Trends for the 1900 - 1910 Cross-Section

	1910s Adopters			*1920s Adopters*		
Group Comparison	$\mathbb{E}[y^{treat}_{i,j}]$	$\mathbb{E}[y^{cont}_{i,j}]$	*p-value*	$\mathbb{E}[y^{treat}_{i,j}]$	$\mathbb{E}[y^{cont}_{i,j}]$	*p-value*
Panel A: Relocation Pairs $i \neq j$						
Treated vs. Never Treated	24.15	21.67	0.50	28.16	20.25	0.12
Treated vs. (Never + Not Yet Treated)				28.16	21.10	0.16
Panel B: Staying Pairs $i = j$						
Hypothetical: Treated vs. Never Treated	10,508.11	9,038.18	0.61	10,836.71	9,053.78	0.52
Hypothetical: Treated vs. (Never + Not Yet Treated)				10,836.71	8,644.75	0.45

Notes: The table reports pre-treatment comparisons of average interstate firm relocation flows between treated and control states in the 1900s. Treated states are those that adopted a corporate income tax in the subsequent decade, while the control group includes never-treated states and states that adopted in later decades (1910s or 1920s). The analysis assesses whether firm relocations systematically differed prior to tax adoption, providing a pre-trend (parallel trend) test based on the limited pre-treatment period available. $\mathbb{E}[y^{treat}_{i}]$ and $\mathbb{E}[y^{cont}_{i}]$ denote the average of the dependent variable $y_{i,j,t}$ for treated and control groups, respectively, during the pre-treatment decade. P-values are based on two-sample t-tests with unequal variances. Panel B presents a hypothetical comparison in which stayers are assigned to the control group to assess balance robustness.

A central identifying assumption is that the tax adoption had not been influenced by the firm relocations in the pre-treatment period. This is essentially a parallel trend assumption, however only for one pre-treatment period. To test this, I compare the pre-treatment mean dependent variables for the treated and untreated group, which, as the adoptions were not simultaneous, also includes the not yet treated. In the pre-treatment period (1900s), the control group for the states adopting in the 1910s consists of never treated states as well as states who adopt in the 1920s. For the states adopting in the 1920s, the control group are the never treated and the ones adopting in the 1910s. For the traditional

TWFE, the control group of the 1920s' adopters is just the never treated. Results for panel A in Table III find a slightly higher mean dependent variable, however, no statistically significant differences across groups prior to the treatment. Compared to the overall mean in Table II, there is also a noticable upward trend in bilateral flows. This further suggests advancements in relocation technologies, such as railroads, and motivates the employment of a country-wide time fixed effects. Panel B can be interpreted as supporting result for balance among groups. Related to this, it is important to establish that the tax adoptions were not driven by preceding firm relocations. Online Appendix C discusses potential state-level drivers of the income tax adoptions. I show that firm outflows have no significant implication for the tax adoptions, finding no indication for the presence of a simultaneity bias.

For the estimation, I define equation (9) as a Poisson regression model to use iteratively reweighted least squares to recursively estimate the tax adoption effect β [Silva and Tenreyro, 2006; Correia et al., 2020; Weidner and Zylkin, 2021]. This estimation technique is commonly known as Poisson pseudo-maximum likelihood (PPML) estimation, allowing to estimate equation (1) in its multiplicative form. Note that gravity models with higher-dimensional fixed effects structure introduce an asymptotic bias and requires a correction for both the coefficient and the standard errors [Weidner and Zylkin, 2021; Pfaffermayr, 2021]. Another challenge of this approach is the direction of the estimates. The structural model states that β captures the effect of an introduction of corporate income taxes on interstate firm outflow. In the estimation, given the usage of a full set of fixed effects and a balanced and symmetrical dyadic dataset, it can be shown that the resulting coefficients (βs) remain identical irrespective of whether the analysis employs outflows from firm i to j or inflows from j to i as dependent variables.[9] Hence, the gravity estimates have to be interpreted as changes in interstate flows. I argue, that effects can still be interpreted as changes in outflows, as the argument for potential inflows through increase in government spending does neither qualitatively [National Industrial Conference Board, 1930] nor quantitatively [Cassidy et al. [2024], Figures B.I and B.II] hold up to scrutiny.

VI. MAIN RESULTS

In a first step of the estimation procedure, I estimate the effects of the corporate tax adoptions on the entire business landscape of the early 20th century U.S. Note that I estimate three different models. With Model (1), I calculate the benchmark gravity effects of the tax adoption on interstate employer flows,

[9] An extensive proof of this observation can be found in Smutny [2024].

controlling for no migration costs other than distance [Anderson and Van Wincoop, 2003]. Model (2) estimates the standard gravity model without controlling for U.S.-wide time trends (e.g. Weidner and Zylkin [2021]). Model (3a,b) includes a decade-interstate fixed effect [Nagengast and Yotov, 2024]. Model (3b) switches impact measure $z_{i,t}$ with the alternative policy variable $z_{i,t}^{trad}$ to test assumption $\beta < \beta^{trad}$, where $z_{i,t} := z_{i,t}^{trad} - z_{i,t-1}^{trad}$. Table IV reports the results of the baseline estimation. Even without properly controlling for iceberg costs, the standard gravity model seems to fit the variation in firm flows well. Once I control for all static relocation costs, the model estimates a slightly larger and statistically significant positive effect of the tax adoptions on firm relocation flows. The asymptotic bias appears to be minuscule.

TABLE IV: Baseline Regressions for the Full Panel (All Sectors Combined)

	Model (1)	Model (2)	Model (3a)	Model (3b)
	Distance Control	Standard	4WFE	Traditional
Corporate income tax adoption: $\tau_{i,j,t}$	0.378***	0.431***	0.122***	0.295***
	(0.079)	(0.029)	(0.023)	(0.041)
$\log(\text{Dist}_{i,j})$	−0.293			
	(0.002)***			
Bias (*Weidner and Zylkin* [*2021*])		−0.013		
Bias-corrected Standard Errors		(0.047)***		
(*Percentile*) *Bootstrap*		(0.078)***	(0.059)*	(0.132)**
Jackknife Resampling		(0.129)***	(0.070)*	(0.180)
State-Decade FEs	yes	yes	yes	yes
State-Pair FE	no	yes	yes	yes
Decade-Interstate FE (*Bergstrand et al.* [*2015*])	no	no	yes	yes
Mean Dependent Variable	54.01	54.01	54.01	54.01
Observations	6,912	6,765	6,765	6,765
McFadden Pseudo R^2	0.968	0.996	0.997	0.997

Notes: The table presents PPML estimation results for Models (1)–(3), which examine the effects of state corporate income tax adoptions on interstate firm relocation flows. Model (1) reports benchmark gravity estimates controlling only for distance as migration cost, following Anderson and Van Wincoop [2003]. Model (2) adds full bilateral, origin–decade, and destination–decade fixed effects, but excludes aggregate U.S.-wide time trends. Model (3a,b) further introduces decade-interstate fixed effects to account for nationwide technological and structural changes, such as improvements in transportation and the shift toward manufacturing [Bergstrand et al., 2015; Nagengast and Yotov, 2024]. Model (3b) replaces the impact measure $z_{i,t}$ with the traditional policy variable $z_{i,t}^{trad}$ to test the identifying restriction $\beta < \beta^{trad}$, where $z_{i,t} := z^{trad} i, t - z_{i,t-1}^{trad}$. Standard errors, clustered by state pair, appear in parentheses below the coefficients. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. The bias correction of Weidner and Zylkin [2021] is applied where feasible (i.e., for two-way and three-way fixed effects specifications). Statistically separated observations are dropped following Clarkson and Jennrich [1991], and singletons are excluded per Correia [2015]. Distances between state centroids are computed using Vincenty [1975]. The estimation sample includes all sectors combined.

There is reason to believe that a standard gravity formulation is substantially upward biased as the observation period is characterized by pronounced transformations in transportation technology. Once

controlling for nation-wide trends, the observed effect is notably smaller. The estimated baseline effect translates to a 13.02% increase in interstate firm flows. Consistent with the prediction of proposition 1(ii), the adoption of corporate income taxes significantly increases interstate firm flows. In other words, the flows of firms between two states i and j is increasing significantly if the origin state i introduces the corporate income tax. Note that as my sample includes unincorporated firms, these results can be interpreted as a lower bound. The baseline model can be further estimated for a set of detailed sectors separately, results are discussed in Online Appendix G. Lastly, the point estimates indicate that $\widehat{\beta} < \widehat{\beta}^{trad}$. This provides empirical support to the importance of differentiating between short and medium term implications of the tax adoptions, as formalized in proposition 3, in order to mitigate a potential bias due to accompanying policies such as increased infrastructure investments.

VI.A. Sectoral Heterogeneity

So far I assumed each sector to be affected equally by the tax adoptions. However, some states only taxed certain industries. In some states, financial and public utility companies were exempt from the new tax. As these exemptions are defined on a broad definition of sectors, I can use the grouping of Table II to create a set of aggregated sectors S. With this new index, I repeat the estimation of equation (1) (Model (4)). To add sectoral variation, I define an indicator variable $w_{i,S}$ equal to 1 if state i adopts the corporate income tax for S. The gravity specification then becomes

$$y_{i,j,t,S} = \exp\left[\sum_{S} \beta_S(\mathbb{1}_S \times \{\tau_{i,j,t} \times w_{i,S}\}) + \boldsymbol{\mathcal{D}} \times \delta_S\right]\xi_{i,j,t,S}, \tag{10}$$

which I define as Model (5). Note that this specification is equivalent to estimating equation (1) for each sectors, however standard errors are directly comparable. Results of these estimations are collected in Table V. The manufacturing, mercantile and service sectors were subject to the corporate tax in each adoption state, hence estimates remain identical across model specifications. Consistent with proposition 1, sectors directly subject to the new tax exhibit significant relocation responses. These sectors also capture the largest fraction of firms in my sample and hence drive the overall results. For the financial and public utility sectors Model (4) does not find any significant implications of the income tax adoption. Once I adjust the tax indicator for adopter differences, I find that the point estimates for both sector specific tax adoption indicators increase. The effects for financial companies are now positive but still insignificant. Note that financial firms are particularly immobile. Banks and insurance companies are subject to many state laws which makes it disproportionately difficult to move financial

enterprises across state borders. Transportation and other public utility companies observe a much higher and statistically significant effect of the tax adoption. The point estimate translates to a 16.77% increase in interstate flows of transportation and public utility firms, which is of comparable size to the overall estimated effect. This underscores the importance of accounting for legal differences in the adopted tax laws.

TABLE V: Stacked Regressions with State-Specific Taxation

	Model (4)	Model (5)		Model (6)
	4WFE	Adopter Variation		Placebo Test
		Relative	Absolute	
Panel A: Full Panel				
$\tau_{i,j,t}$	0.122***		7.03***	
	(0.023)		(1.39)	
Panel B: Sector-specific effects				
Manufacturing and Mercantile	0.121***	0.121***	6.34***	
	(0.023)	(0.023)	(1.28)	
Services	0.138***	0.138***	0.44***	
	(0.042)	(0.042)	(0.14)	
Financial Sector	−0.045	0.047	0.02	−0.433**
	(0.089)	(0.092)	(0.05)	(0.178)
Transport. and Other Pub. Util.	0.041	0.155*	0.13*	−0.062
	(0.068)	(0.092)	(0.08)	(0.091)
Observations	24,949	24,949		24,949
McFadden Pseudo R^2	0.996	0.996		0.996

Notes: The table presents PPML estimation results for Models (4)–(6), which examine sectoral heterogeneity in firm responses to state corporate income tax adoptions. Model (4) estimates the baseline gravity specification from equation (1) separately for each aggregated sector, pooling all states regardless of sector-specific tax treatment. Model (5) introduces the sector-specific tax indicator $w_{i,S}$, which equals 1 if state i adopts the corporate income tax for sector S, thereby accounting for state-level variation in which industries were subject to taxation. This specification follows equation (10) and allows for direct comparison of standard errors across sectors. Model (6) implements a placebo test using $w_{i,S}^{placebo} = 1 - w_{i,S}$ to verify that the estimated effects capture tax policy rather than other confounding factors; note that this indicator includes both exempt sectors in adopter states and all sectors in non-adopter states, though the interaction with $\tau_{i,j,t}$ effectively excludes non-adopters from identification. All specifications include bilateral state-pair, origin state–decade, destination state–decade, and decade-interstate fixed effects. The absolute changes in the final row are computed as $(\exp(\widehat{\beta}_S) - 1) \times \bar{y}_S$, where $\bar{y}_S$ represents the mean flow for sector S. Sector classifications follow the aggregation scheme presented in Table II. Estimation procedures, standard error clustering, and data processing follow Table IV. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Lastly, the sectoral variation in the tax adoption allows me to run a "placebo" regression with $w_{i,S}^{placebo} = 1 - w_{i,S}$. Note that $w_{i,S}^{placebo}$ also includes states that did not adopt the tax at all. This however does not matter, as in Model (6) β_S captures the effect of $\tau_{i,j,t} \times w_{i,S}^{placebo}$, which excludes the non-adopters. This enables me to discern whether my initial estimate is capturing additional factors beyond the tax policy. I find no significant effects for the public utilities sector, lending credible support to my strategy. This

placebo result further corroborates proposition 1: absent tax exposure ($w_{i,S} = 0$), the model predicts no relocation effect. I do observe a noteworthy negative impact resulting from the absence of taxes on the financial sector. Given that other sectors in these states were subjected to taxation, it is plausible that influential financial entities, leveraging their political clout and affiliations, successfully lobbied for exemption from taxation. This scenario could have induced additional incentives for these firms to remain within these states despite the tax burden on other sectors.

VI.B. Persistence of the Adoption Shock

The baseline specification aggregates effects at the decennial level, potentially masking informative within-decade variation. Exploring dynamics within adoption decades offers three advantages. First, relocations take time: firms may delay moving for several years after a tax is adopted. Second, relocation effects may diminish as responsive firms exhaust the pool of potential movers. Third, if effects diminish within a decade, this supports the main specification's use of decadal differences: $z_{i,t} := z^{trad}_{i,t} - z^{trad}_{i,t-1}$. Since tax adoptions occur at different points within decades, cross-sectional variation in adoption timing can be exploited to recover the within-decade dynamics of the relocation response from decennial data. I define $\text{YSA}_{i,t}$ (years since adoption) as the number of years between state i's adoption and the end of decade t, when flows are observed. Formally, $\text{YSA}_{i,t} = t(e) - \text{Adoption Year}_i$, where $t(e)$ is the decade endpoint. To formalize the conjectured dynamics, I extend the baseline model with two additional assumptions.

Assumption 1 (Finite Pool of Movers) *At the moment of adoption $t(d)$, the tax shock renders relocation strictly profitable for a finite mass $M_i \subseteq f_{i,t(d)}$ of firms for whom $\Lambda_{h,i,j,t(d)} > \Lambda_{h,i,i,t(d)}$ for some $j \neq i$ under the new tax regime. The set M_i is determined at the moment of adoption and is not replenished thereafter.*

Assumption 2 (Gradual Adjustment) *Firms in M_i do not relocate instantaneously. In each year following adoption, a constant fraction $\lambda \in (0, 1)$ of the remaining pool relocates, reflecting search frictions, contractual obligations, and logistical adjustment costs.*

Proposition 3 (Concavity of Relocation Flows in Time Since Adoption) *Under assumptions 1–2, let $Y(YSA_{i,t})$ denote the cumulative interstate relocations from state i observed at the decade endpoint $t(e)$, as a function of $YSA_{i,t} \in [0, T]$ where $T \leqslant 9$. Then:*

(*i*) *Cumulative flows are strictly increasing and strictly concave:*

$$Y(YSA_{i,t}) = M_i \left[1 - (1-\lambda)^{YSA_{i,t}}\right], \tag{11}$$

with $Y'(YSA_{i,t}) > 0$ *and* $Y''(YSA_{i,t}) < 0$ *for all* $YSA_{i,t} > 0$.

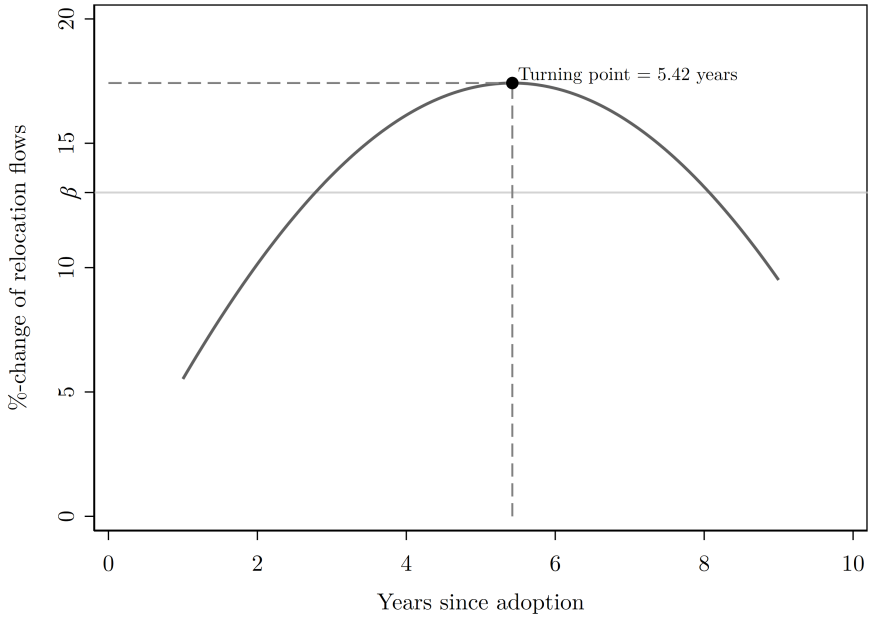
(*ii*) *The marginal flow is strictly decreasing and vanishes asymptotically:* $\lim_{YSA_{i,t}\to\infty} Y'(YSA_{i,t}) = 0$.

The proof of proposition 3 can be found in Online Appendix A. This provides a structural foundation for recovering within-decade dynamics from decennial data: since adoptions occurred at different points within each decade, variation in $\text{YSA}_{i,t}$ traces out the time profile of the relocation response. The concavity prediction implies that the adoption shock is temporary—once all firms for whom relocation is profitable have departed, the marginal flow vanishes. A second-order polynomial $\varphi_1 \text{YSA}_{i,t} + \varphi_2(\text{YSA}_{i,t})^2$ with $\varphi_1 > 0$ and $\varphi_2 < 0$ provides a local approximation to the concave flow function over the empirically relevant range. I interact $\text{YSA}_{i,t}$ with the treatment to estimate

$$\begin{aligned} y_{i,j,t,S} = \exp\Bigg(\sum_S \varphi_{1,S}(\mathbb{1}_S \times \{\tau_{i,j,t} \times w_{i,S}\} \times \text{YSA}_{i,t}) \\ + \varphi_{2,S}(\mathbb{1}_S \times \{\tau_{i,j,t} \times w_{i,S}\} \times \text{YSA}_{i,t})^2 + \boldsymbol{\mathcal{D}}\Bigg) \varrho_{i,j,t,S}, \end{aligned} \tag{12}$$

where the marginal effect $[\exp(\widehat{\varphi}_{1,S}\text{YSA}_{i,t} + \widehat{\varphi}_{2,S}(\text{YSA}_{i,t})^2) - 1] \times 100$ (13) captures how relocation responses vary with time since adoption. Figure IV plots equation (13) for $\text{YSA}_{i,t} \in [0, 9]$ alongside the baseline estimate from equation (9). Consistent with proposition 3, effects follow a concave polynomial function with $\widehat{\varphi}_1 > 0$ and $\widehat{\varphi}_2 < 0$, and coefficients are strongly significant (see Table B.III in Online Appendix B). While effects increase over the beginning of a decade, estimates diminish after 5.42 years. Hence the effects of an additional year after introducing the tax deteriorates as time progresses. This suggests that, at a certain point, all firms with $\Lambda_{h,i,i,t(d)} < \Lambda_{h,i,j,t(d)}$ have already left the state. The findings hence confirm the model's prediction that tax introductions affected relocations through a temporary shock rather than a permanent shift in outflows. This further supports the baseline tax adoption identifier $z_{i,t} := z_{i,t}^{trad} - z_{i,t-1}^{trad}$. Again, manufacturing and mercantile businesses drive overall results. For the rest of aggregated sectors, none of the polynomial terms are significant and the financial sector does not observe any significant effects of the tax adoptions overall.

FIGURE IV: Adoption Effects weighted by Years Since Adoption

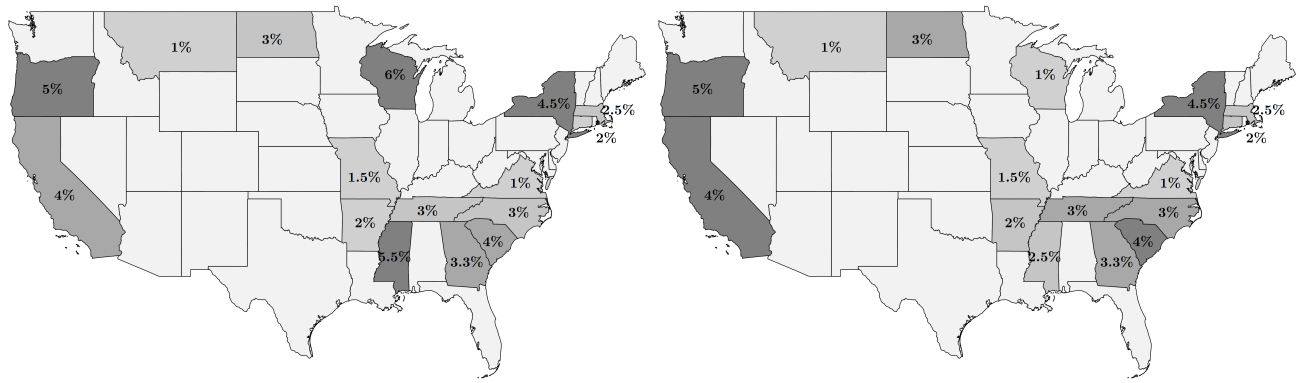


Notes: The figure plots the estimated marginal effect of corporate tax adoptions as a function of years since adoption ($YSA_{i,t} \in [0, 9]$), defined by equation (13). The curve shows the percentage change in relocation flows for each additional year between adoption and decade-end measurement. The horizontal line at 13.02% represents the baseline decadal effect from equation (9) for comparison. Results show a concave pattern: effects initially increase but peak at 5.42 years post-adoption before declining, suggesting the adoption shock is temporary rather than permanent. Full results including sector-level estimates appear in Table B.III in Online Appendix B.

VI.C. Tax Rates and Pure Adoption Effect

The introduced rates differ substantially across adopter states. Some states further adopted a progressive tax regime. Therefore, Figure V reports both maximum and minimum introduced rates.

FIGURE V: Adopted Tax Rates by States



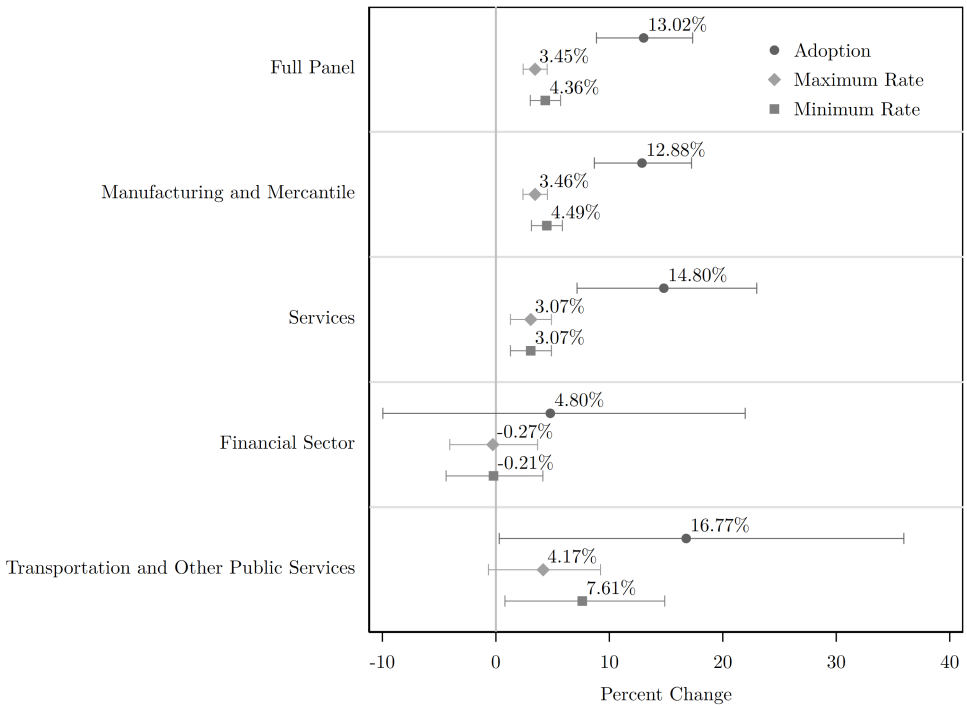
(**a**) Maximum tax rates

(**b**) Minimum tax rates

Notes: The maps display maximum and minimum corporate income tax rates adopted by states during the sample period. Darker shades indicate higher rates. Wisconsin introduced the highest maximum rate at 6% in 1911. Non-adopter states appear in white.

The average maximum rate was 3.2%, the average minimum rate was 2.7%. Wisconsin, as the first adopter state also introduced the largest tax rate at 6%. With the average introduced rates, I estimate the firm flow elasticity of the tax adoption to be between 4.1 and 4.8, depending on the progressivity, aligned with corollary 1.1.[10] To estimate and recover the intensive margin parameter β^r from proposition 2(ii), I define $\tau^r_{i,j,t,S} = (\tau_{i,j,t} \times w_{i,S}) \times \text{Rate}_i$, where $\text{Rate}_i \in \{Max_i, Min_i\}$ defines the adopted tax rates. The policy in equation (10) can be substituted with this new term. Now, β^r_S measures the impact of a one percentage point higher tax rate at the time of the adoption. The interpretation is similar to estimates of tax differentials, i.e., effects on the intensive margin. However, the observed impacts are to be read as the intensity of the extensive margin. Estimation results are reported in Figure VI.

FIGURE VI: Estimated β^r_S by Aggregated Sectors



Notes: The figure displays estimated coefficients β^r_S and 90% confidence intervals from PPML regressions examining the impact of adopted tax rate intensity on interstate firm relocation flows by aggregated sector. The specification replaces the binary tax adoption indicator in equation (10) with $\tau^r_{i,j,t,S} = (\tau_{i,j,t} \times w_{i,S}) \times \text{Rate}_i$, where $\text{Rate}_i \in \{Max_i, Min_i\}$ represents the maximum or minimum adopted tax rate in state i. Coefficients measure the percentage point change in relocation flows associated with a one percentage point increase in the adopted rate, capturing treatment intensity on the extensive margin. All specifications include bilateral state-pair, origin state–decade, destination state–decade, and decade-interstate fixed effects. Standard errors are clustered by state pair. Estimation procedures and data processing follow Table IV. Full regression results including rate flatness measures are reported in Table B.IV in Online Appendix B.

[10] The baseline estimate (13.02%) captures the flow increase at the average adopted rate. The implied elasticity (response to a 1% rate) equals the average rate times the baseline coefficient.

Consistent with the interpretation of the coefficients, the magnitude of the estimates are significantly smaller. Furthermore, I find that the minimum rate is more salient as a larger fraction of firms is affected by the minimum rate. I find that a one percentage point increase in the adopted minimum tax rate increases relocation flows by 4.75% on average. Similar to the baseline, the manufacturing and mercantile industry is driving results and financial businesses observe no significant effects.

To examine how rate progressivity affects relocation flows, I use the ratio $[(\tau_{i,j,t} \times w_{i,S}) \times Min_i]/Max_i \in [0, 1]$ as a regressor. This measure captures rate flatness (the inverse of progressivity): higher values indicate the minimum and maximum rates are closer together. Results appear in Table B.IV in Online Appendix B. For interpretability, I scale the measure by 10, so coefficients represent a 10 percentage point increase in flatness. Consistent with earlier rate effects, flatter rate structures generate larger relocation flows, since all affected firms face at least the minimum rate.

Pure Adoption Effect With estimates of the rate specification, I implement the decomposition of proposition 2 to recover the pure adoption effect. For each sector, I compute

$$\text{Pure Adoption Effect}_S = \widehat{\beta}_S - \widehat{\beta^r_S} \times \overline{\text{Rate}}, \tag{14}$$

depending on $\text{Rate}_i \in \{Max_i, Min_i\}$. This decomposition separates the total relocation response into two components: the mechanical effect of the tax rate level itself (captured by $\widehat{\beta^r_S} \times \overline{\text{Rate}}$) and the pure signaling or informational effect of the adoption event independent of the specific rate chosen. The results for equation (14) are presented in Table VI. I find significant effects of the adoption of the tax itself. Consistent with proposition 2(iii), the total adoption effect strictly exceeds the rate-proportional component, confirming the presence of a positive pure adoption effect $\widehat{\beta}^F > 0$. That is, the introduction per se increases firm flows by about 1.3 - 2 % on average for the full panel. The pure adoption effect is economically meaningful, representing roughly 15-20% of the total baseline effect. Note that the pure adoption effect varies substantially by sector, with services showing considerably larger pure adoption responses than manufacturing and mercantile businesses. This heterogeneity suggests that different industries face varying non-rate-proportional costs or informational frictions associated with new tax regimes.

TABLE VI: Pure Adoption Effects

	Maximum	Minimum
Panel A: Full Panel		
Pure Adoption Effect	1.97%	1.26%
Panel B: Sector-specific effects		
Manufacturing and Mercantile	1.81%	0.76%
Services	4.97%	7.15%
Financial Sector	5.65%	5.37%
Transport. and Other Pub. Util.	3.44%	-3.78%

Notes: The table reports pure adoption effects computed using equation (14): Pure Adoption Effect$_S$ $= \widehat{\beta}_S - \widehat{\beta^r_S} \times \overline{\text{Rate}}$, which decomposes the total relocation response into the rate-proportional component and the effect of adoption independent of the rate level. $\widehat{\beta}_S$ denotes the baseline adoption coefficient from Table IV, $\widehat{\beta^r_S}$ represents the rate intensity coefficient from the specification in Figure VI, and $\overline{\text{Rate}}$ is the average adopted rate (3.2% for maximum, 2.7% for minimum). The pure adoption effect captures non-rate-proportional responses such as fixed compliance costs, administrative burdens, policy uncertainty, and reputational concerns about operating in taxing jurisdictions. Panel A shows effects for the full sample; Panel B presents sector-specific decompositions. All underlying coefficients are estimated using PPML with the full set of fixed effects described in Table IV.

VI.D. Firm Stock Elasticity

Having established the flow effects predicted by proposition 1 and their decomposition following proposition 2, I now translate these findings into stock-level implications. The 13.02% increase in relocation flows represents economically significant redistribution of firm activity across states. To compare with contemporary literature, I compute the stock elasticity—the percentage change in a state's firm stock per percentage point of tax rate:

$$\text{Stock Elasticity} = \frac{\sum_t \sum_j y_{i,j,t} \times (\widehat{\beta}/\overline{\text{Rate}})}{\sum_t f_{i,t}}, \tag{15}$$

where $f_{i,t}$ is the firm stock, $y_{i,j,t}$ represents outflows, and $\widehat{\beta}/\overline{\text{Rate}}$ converts the semi-elasticity to a per-percentage-point effect.[11] This yields stock elasticities of 0.78% (minimum rates) to 0.66% (maximum rates) per percentage point tax rate. These magnitudes are substantial given the historical context and represent economically meaningful responses to tax policy. Auxiliary analysis in Online Appendix F confirms that relocations directly reduced firm stocks, i.e., tax-induced outflows were not offset by closures or entry responses. Moreover, three factors suggest these estimates represent lower bounds

[11] Using $\widehat{\beta} = 0.1302$ and average minimum rate $\overline{\text{Min}} = 0.027$ yields flow elasticity of 4.82 per percentage point. With average maximum rate $\overline{\text{Max}} = 0.032$, the flow elasticity is 4.07.

on true corporate responsiveness. First, the sample necessarily includes unincorporated firms facing minimal tax exposure, mechanically dampening measured elasticities. Second, higher relocation costs and less integrated markets in the early 20th century reduced observed mobility relative to underlying tax sensitivity. Third, decennial aggregation masks immediate responses as firms adjusted gradually to permanent policy changes. Accounting for these factors, the historical corporate sector exhibited tax sensitivity comparable to modern estimates such as Suárez Serrato and Zidar [2016], despite appearing less responsive in aggregate. The mediation analysis in Online Appendix E further reveals that roughly 9% of the decline in corporate shares documented by Liu [2014] can be attributed to these relocations, confirming that firm mobility constituted an economically meaningful margin through which corporate taxation reshaped the geographic distribution of American manufacturing.

VI.E. Mechanism

The baseline results establish that corporate income tax adoptions significantly increased interstate firm relocations. This section investigates why firms respond and which firms are most affected. The structural model attributes the pure adoption effect β^F to fixed compliance costs. I now formalize and test this mechanism by distinguishing between corporate and personal income taxes.

Assumption 3 (Differential Compliance Burden) *Corporate income taxes impose compliance obligations directly on firms including bookkeeping, auditing, and reporting requirements, generating fixed costs independent of the tax rate ($\beta^F > 0$). Personal income taxes are filed by the individual owner and impose no compliance obligations on the firm ($\beta^F = 0$).*[12]

Proposition 4 (Fixed Cost Mechanism) *Under assumption 3 and the conditions of proposition 1:*

(*i*) *Corporate tax adoptions increase firm-level administrative fixed costs. Personal tax adoptions do not.*

(*ii*) *Relocation effects are strictly positive for states adopting corporate income taxes ($\beta > 0$ when $\beta^F > 0$) but zero for states adopting only personal income taxes ($\beta = 0$ when $\beta^F = 0$ and the firm faces no corporate tax liability).*

Part (i) follows directly from assumption 3: corporate taxes require firm-level compliance infrastructure while personal taxes do not. Part (ii) follows from proposition 1(ii), which establishes that interstate flows increase if and only if $\beta > 0$. Under assumption 3, $\beta^F = 0$ for personal-only adoptions.

[12] This distinction is particularly sharp in the early 20th century United States. During the sample period, neither federal nor state governments required employers to withhold personal income taxes from wages. The brief federal attempt (1913 to 1917) had already been repealed, and state-level employer withholding was not introduced until after 1943 (see Section 1). Personal income tax obligations thus fell entirely on the individual.

If additionally the firm bears no corporate tax liability, then $\beta^r \text{Rate}_i = 0$, yielding $\beta = 0$ and hence no relocation response. I test these predictions in the following two subsections: first by examining whether tax adoptions raised administrative staffing (Section 1), and then by decomposing relocation effects by tax type (Section 2).

1. *Why Firms Relocate*

The structural model posits that tax adoptions increase relocations by raising fixed costs in the origin state, as formalized in proposition 4(i). These costs include direct compliance burdens, including accountants, legal counsel, administrative staff, and indirect costs from organizational complexity and regulatory uncertainty. A substantial literature documents that tax compliance imposes significant costs on businesses, with estimates suggesting compliance costs amount to roughly 10 cents per dollar of tax collected [Slemrod and Blumenthal, 1996; Sandford, 1995]. Independent historical evidence corroborates the magnitude of these costs: the federal government's first attempt at employer income tax withholding, introduced in 1913, was repealed just four years later in the War Revenue Act of 1917 due to widespread corporate opposition to the administrative burden it imposed [Desai, 2013]. Even the Secretary of the Treasury advocated for its removal, and employer withholding was not reintroduced until the Current Tax Payment Act of 1943. I test this mechanism using data on salaried personnel from the Census of Manufactures (see Figure B.V in Online Appendix B) in

$$\ln \left(\texttt{Admin Staff}\right)_{i,t} = \eta_1 z_{i,t}^{\text{corp}} + \eta_2 z_{i,t}^{\text{pers}} + \texttt{Controls} + \delta_i + \delta_t + \upsilon_{i,t}, \tag{16}$$

where $z_{i,t}^{\text{corp}}$ and $z_{i,t}^{\text{pers}}$ equal one if state i adopted corporate or personal income taxes in decade t, respectively. Controls include the number of corporations, total population, and total out-migration. Results appear in Table VII. Corporate tax adoptions increased administrative personnel by 14.38% and managerial staff by 11.53%. For the average firm in states adopting corporate taxes, this implies approximately 1.73 additional administrative employees and 0.16 additional managers devoted to tax-related activities. Firms hence face a substantial increase in fixed costs, confirming proposition 4(i). Personal income tax adoptions show no significant effect on administrative staffing, consistent with assumption 3 and the prediction that these taxes impose compliance burdens on individuals rather than businesses. If compliance costs are largely fixed rather than proportional to revenue, larger firms face higher absolute cost increases. Combined with the fact that incorporated firms face direct corporate tax liability while unincorporated businesses face only personal income taxes on owner earnings,

this mechanism predicts that larger, incorporated businesses should exhibit the strongest relocation responses.

TABLE VII: Tax Adoptions and Administrative Personnel

	Administrative		Managerial		Total	
$z_{i,t}^{\text{corp}}$	0.798	0.134	0.762	0.109	0.796	0.130
	(0.590)	(0.064)**	(0.582)	(0.063)*	(0.588)	(0.060)**
$z_{i,t}^{\text{pers}}$	−0.241	−0.070	−0.098	−0.041	−0.227	−0.069
	(0.590)	(0.063)	(0.582)	(0.062)	(0.588)	(0.059)
Controls	no	yes	no	yes	no	yes
State FE	no	yes	no	yes	no	yes
Decade FE	no	yes	no	yes	no	yes
Observations	144	144	144	144	144	144

Notes: This table reports the relationship between income tax adoptions and salaried personnel in manufacturing establishments. The dependent variable is the natural logarithm of administrative or managerial personnel count at the state-decade level. Administrative personnel refers to salaried non-managerial staff. The treatment variables $z_{i,t}^{\text{corp}}$ and $z_{i,t}^{\text{pers}}$ equal one if state i adopted corporate or personal income taxes in decade t, respectively. Controls include the number of corporations, total state population, and total out-migration. All specifications include state and decade fixed effects. Observations are recorded at the end of each decade. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors clustered by state.

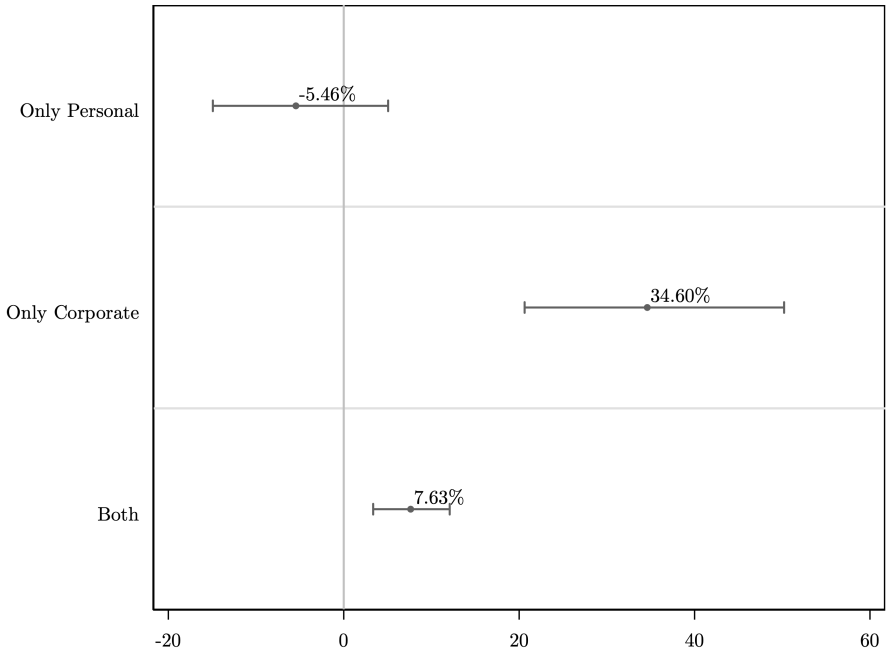
2. *Which Firms Relocate*

I now test proposition 4(ii) directly. The sample includes both incorporated and unincorporated employers. This heterogeneity, combined with state variation in tax adoption timing, allows me to test whether observed relocations reflect corporate tax responses or confounding factors affecting all businesses. While many states adopted corporate and personal income taxes simultaneously or in close succession, there is enough variation to substitute the treatment of equation (9) with (1) *personal-only* states ($\tau_{i,j,t}^{\text{pers}}$), (2) *corporate-only* states ($\tau_{i,j,t}^{\text{corp}}$), and (3) states adopting *both* within the same decade ($\tau_{i,j,t}^{\text{both}}$). Note that $\tau_{i,j,t} = \tau_{i,j,t}^{corp} + \tau_{i,j,t}^{both}$. Results are presented in Figure VII.

First, personal income tax adoptions alone had no significant effect on business relocations. This validates that omitted factors or misspecification do not drive my results. Personal taxes affect owners' after-tax income but impose no direct compliance burden on businesses, as unincorporated firms simply report on the owners' individual returns. Second, corporate-only adoptions generated large effects, exceeding the baseline estimate, suggesting the pooled specification may be conservative. Third, simultaneous adoptions show effects similar to the baseline. These patterns strongly support the fixed cost mechanism. Incorporated businesses facing new compliance requirements show precisely the large responses predicted by theory. Since incorporation rates increase sharply with firm size, these findings

indicate that larger, incorporated firms, i.e., those facing the highest absolute fixed cost increases, drive the observed relocations. While my sample includes all business owners regardless of legal form, treatment intensity varies systematically with firm characteristics: effects concentrate among incorporated firms facing direct tax liability and substantial compliance burdens.

FIGURE VII: Effects by Type of Income Tax Adopted



Notes: This figure presents estimates from specifications that decompose the baseline treatment variable $\tau_{i,j,t}$ by type of income tax adopted. The specifications use PPML estimation with state-pair, origin-decade, destination-decade, and interstate-decade fixed effects. $\tau^{\text{pers}}_{i,j,t}$ indicates states adopting only personal income taxes; $\tau^{\text{corp}}_{i,j,t}$ indicates states adopting only corporate income taxes; $\tau^{\text{both}}_{i,j,t}$ indicates states adopting both taxes within the same decade. Error bars represent 90% confidence intervals. Standard errors are clustered by state-pair. Statistically separated observations are excluded based on fixed effects following Clarkson and Jennrich [1991], and singleton observations are excluded using Correia [2015]. Full regression results, including sectoral breakdowns, are reported in Table B.V.

3. *Where Firms Relocate*

Do firms relocate short or long distances? Table H.I in Online Appendix H decomposes effects by whether state pairs share a border. As the average bilateral flow is about 7 times larger for bordering states, the marginal effect of tax adoption is actually smaller for border pairs (4.23) than non-border pairs (21.53%). This suggests that the tax adoption effects on firm relocations do not increase exponentially with geographic proximity. However, average absolute effects are found to be larger for border pairs. To explore within-state variation, I leverage county-level location data and estimate how relocation responses vary with distance to the nearest state border. Effects decline with distance to the

state border, with the effect zero-crossing at 214 miles. This confirms that geographic relocation costs matter substantially, even conditional on deciding to relocate. To explore the importance of destination characteristics, I classify states as urban (population density > 10 per square mile) or rural and interact this designation with the tax adoption indicator. Table H.IV shows the largest effects for rural-to-urban relocations in manufacturing and mercantile sectors (39.65%), with the reverse pattern for agricultural businesses. These patterns reflect infrastructure-driven self-selection: non-agricultural firms concentrate in urban areas with better transportation and services, while agricultural businesses require rural locations.

4. Robustness

I conduct several robustness checks to validate that baseline estimates reflect tax-induced relocations rather than confounding factors. A primary concern is that observed flows reflect network spillovers or migration-induced labor market changes rather than direct tax responses [Brülhart et al., 2012]. I test this by augmenting the baseline specification with bilateral controls for lagged firm flows and worker migration. If baseline estimates spuriously capture agglomeration spillovers, including these controls should reduce the tax coefficient. However, coefficients show slightly higher coefficients compared to the baseline suggesting baseline estimates are conservative: adopter states experienced lower prior inflows, and conditioning on this negative selection reveals larger effects. Since including lagged controls requires dropping 33% of observations and yields statistically indistinguishable results, I retain the baseline specification. Full details on alternative identification strategies using predicted flows and tax-interaction terms appear in Online Appendix I. The pattern of increased coefficients holds across all specifications. Related to agglomeration concerns, I verify that destination-decade fixed effects adequately absorb sector-specific agglomeration patterns by adding sector-state-decade interactions and sector-adopter controls. Point estimates remain stable across all specifications, confirming the baseline fixed effects structure is appropriate (see Table I.II in Online Appendix I).

Results are not driven by specific adopter states. Leave-one-out analysis shows that dropping any single adopter changes coefficients only within the confidence bands, confirming effects represent systematic responses to tax policy rather than idiosyncratic state characteristics. Washington State presents an ambiguous case, National Industrial Conference Board [1930] lists a 1929 adoption, but Advisory Commission and American Council on Intergovernmental Relations [1995] does not, as political pressure prevented implementation. Including Washington slightly attenuates estimates (see Table B.VI),

consistent with measurement error from misclassified treatment. Tax adoptions could also reduce state-level firm stocks through closures rather than relocations. Using individuals switching from employer to employee status as a proxy for closures, I find no association with tax adoptions (Online Appendix F), confirming observed stock changes are driven by the relocation margin.

I further verify robustness to alternative policy definitions and collect results in Table B.VI. First, using $\tau^{alt}_{i,j,t} = \mathbb{1}(z_{i,t} > z_{j,t})$ to explicitly require origin-destination tax differentials yields nearly identical estimates, validating that effects concentrate in state pairs with opposing adoption timing. Second, excluding late-decade adopters (California, Georgia, Missouri, Oregon) and reassigning North Dakota (1919) to the 1920s cohort preserves all results, confirming effects require sufficient time within the decade for firms to relocate. Third, the traditional ever-treated indicator $z^{trad}_{i,t}$ yields larger coefficients (0.178 vs. 0.130) as expected, since it combines short-run adoption effects with medium-run revenue increases that could generate offsetting inflows through infrastructure investment.[13] Using the Wooldridge [2023] estimator to allow treatment effect heterogeneity following Nagengast and Yotov [2024], I find identically effects to the baseline strategy using $\tau^{trad}_{i,t}$ (see Table B.VII). Event study patterns suggest effects persist beyond a single decade, though isolating long-run inflow versus outflow channels requires additional analysis beyond this paper's scope. Finally, many Southern states adopted taxes in the 1920s, raising concerns about coordinated regional policy. Controlling for South-specific 1920s trends using $\mathbb{1}(t = 1920) \times \mathbb{1}(\text{South}_i > \text{South}_j)$ does not alter estimates, indicating adoptions reflect individual state decisions.[14]

These robustness checks demonstrate that baseline estimates are not driven by agglomeration spillovers, firm closures, specific adopter states, alternative policy definitions, or regional coordination. If anything, the baseline specification provides conservative lower bounds on the true causal effect of corporate tax adoptions on firm relocations.

VII. GENERAL EQUILIBRIUM EFFECTS

The partial equilibrium analysis identifies the average effect of corporate tax adoptions on interstate flows, but two important questions remain. First, by construction, partial equilibrium estimates assume no effects on non-adopting states. The spatial nature of firm location decisions suggests that one state's policy shift should affect flows between all state pairs through changes in multilateral resistance. Sec-

[13] Following Cassidy et al. [2024], tax adoptions did not increase revenues or expenditures within a decade, supporting both the short-run policy variable and the outflow interpretation.

[14] Southern states defined per Census Bureau: AL, AR, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, WV.

ond, the gravity specification with origin-time and destination-time fixed effects in a symmetric dyadic dataset produces coefficients that cannot be interpreted directionally [Smutny, 2024]. Both limitations can be addressed by conducting counterfactual experiments using exact hat algebra (see Dekle et al. [2007]). These experiments exploit the structural parameters of proposition 1 to generate counterfactual outcomes that account for general equilibrium adjustments across all states.

VII.A. Methodology

Unlike difference-in-differences approaches that rely on parallel trends assumptions and never-treated control groups, exact hat algebra leverages the estimated structural parameters to generate counterfactual outcomes for the treated states themselves, accounting for general equilibrium adjustments across all states.

I begin by recovering the matrix of baseline bilateral relocation costs from the estimated gravity model. Using estimates from equation (9) and notation from Yotov et al. [2016], the inverse bilateral costs are $\left(\widehat{\Gamma}_{i,j}\right)^{-1} = \exp\left[\widehat{\delta}_{i,j} - \widehat{\beta}\tau_{i,j}\right]$. Due to multicollinearity and singleton observations [Correia, 2015], some state-pair fixed effects cannot be estimated directly. For these pairs, I predict bilateral costs using a distance-based model with origin and destination fixed effects:

$$\exp\left[\widehat{\delta}_{i,j}\right] = \exp\left[\ln(\text{Dist}_{i,j}) + \delta_i + \delta_j\right] \text{ if } i \neq j. \tag{17}$$

Once the cost matrix is recovered, I impose alternative policy scenarios by re-estimating the gravity model under counterfactual tax adoption patterns. For a given counterfactual policy $\tau^{cf}_{i,j}$, the predicted flows are:

$$y^{cf}_{i,j} = \exp\left[\ln\left(\widehat{\Gamma}^{cf}_{i,j}\right) + \delta^{cf}_{i\neq j} + \delta^{cf}_{i} + \delta^{cf}_{j}\right]\varepsilon^{cf}_{i,j}. \tag{18}$$

where $\widehat{\Gamma}^{cf}_{i,j} = \exp[\widehat{\beta}\tau^{cf}_{i,j} - \widehat{\delta}_{i,j}]$. Note that δ^{cf}_{i}, δ^{cf}_{j}, and $\delta^{cf}_{i\neq j}$ are re-estimated to satisfy the equilibrium conditions under the counterfactual policy, while $\widehat{\delta}_{i,j}$ (structural bilateral costs) remains fixed. I compute the counterfactual percentage change in total state-level outflows as:

$$\Delta\widehat{y}_i = \frac{\sum_{j\neq i}(\widehat{y}^{cf}_{i,j} - \widehat{y}_{i,j})}{\sum_{j\neq i}\widehat{y}_{i,j}} \times 100. \tag{19}$$

Unlike the partial equilibrium coefficient $(\widehat{\beta})$, counterfactual changes are directionally interpretable.

Aggregating bilateral flows by origin state breaks the symmetry that prevents directional interpretation in the partial equilibrium model. A positive $\Delta\widehat{y}_i$ unambiguously indicates that state i would have experienced higher outflows under the counterfactual scenario.

VII.B. No Adoption in the 1910s

The primary counterfactual examines what would have occurred if no state adopted corporate income taxes in the 1910s. I set $\tau_{i,j}^{cf} = 0$ for all state pairs and compute counterfactual flows using equation equation (18). Table VIII reports the results.

TABLE VIII: Adopter Counterfactual: No 1910s Adoptions

State	$\Delta\widehat{y}_i$	$\Delta\widehat{y}_i \times \sum_{j \neq i} y_{i,j}$	$f_{i,t}$
Connecticut	-6.87%	-82.22	$7,675$
Massachusetts	-5.26%	-186.27	$22,896$
Missouri	-3.60%	-306.95	$40,342$
Montana	-5.26%	-76.90	$4,351$
New York	-4.44%	-474.81	$69,840$
North Dakota	-2.74%	-100.67	$11,495$
Virginia	-3.81%	-130.41	$22,158$
Wisconsin	-3.93%	-203.84	$34,884$

Notes: This table reports the change in total state-level firm outflows under the counterfactual scenario that no state adopted corporate income taxes in the 1910s. Counterfactual changes are computed as $\Delta y_i = [(\sum_{j \neq i} \widehat{y}_{i,j}^{cf} - \widehat{y}_{i,j})/(\sum_{j \neq i} \widehat{y}_{i,j})] \times 100$, where $\widehat{y}_{i,j}^{cf}$ represents predicted bilateral flows under the counterfactual policy ($\tau_{i,j}^{cf} = 0$ for all i,j) and $\widehat{y}_{i,j}$ represents observed flows. Negative values indicate states would have experienced lower outflows absent the tax adoptions. Columns report percentage changes, total changes in firm flows, and the stock of firms ($f_{i,t}$) for reference.

All eight adopting states would have experienced lower firm outflows without the tax. Consistent with proposition 1(ii), every adopting state shows reduced outflows in the counterfactual absence of the tax, confirming that the partial equilibrium results extend to a general equilibrium setting. Connecticut shows the largest effect (-6.87%, representing 82 fewer relocating firms over the decade), followed by Massachusetts (-5.26%, 186 firms) and New York (-4.44%, 475 firms). The absolute magnitudes are economically substantial. New York alone would have retained nearly 500 firms, the total across all adopters exceeds 1,600 firms. Given these states' aggregate stock of approximately 200,000 firms, these are large implications. Conversely, non-adopting states show small positive counterfactual changes, indicating modestly higher outflows absent adopters' tax policies. This reflects the multilateral resistance structure derived in proposition 1: the $\texttt{Push}_{i,t(d)}$ term depends on the attractiveness of all destinations, so when adopting states become less attractive origins, the relative cost of staying in non-

adopting states decreases, reducing relocation incentives. These spillovers are modest in percentage terms (typically 0.2to 1.5%) but economically meaningful in levels.

1. Decomposing General Equilibrium Effects

The total effects in Table VIII aggregate distinct channels through which tax adoptions affect the spatial equilibrium. I decompose changes in outflows from each adopting state into three components: (1) direct effects capture changes in flows to non-adopting destinations, (2) indirect effects capture changes in flows to other adopting states, and (3) general equilibrium adjustments reflect the residual from multilateral resistance changes.

TABLE IX: Decomposition of General Equilibrium Effects

	Direct Effect		Indirect Effect		Total Effect	
State	Level	Share	Level	Share	Level	GE Adj.
Connecticut	−26.9	32.7%	−58.6	71.3%	−82.2	−4.0%
Massachusetts	−85.9	46.1%	−95.8	51.4%	−186.3	2.5%
Missouri	−257.8	84.0%	−57.1	18.6%	−306.9	−2.6%
Montana	−45.2	58.8%	−22.3	29.0%	−76.9	12.1%
New York	−296.4	62.4%	−168.6	35.5%	−474.8	2.1%
North Dakota	−43.4	43.1%	−56.9	56.5%	−100.7	0.3%
Virginia	−91.6	70.2%	−32.8	25.2%	−130.4	4.6%
Wisconsin	−123.6	60.6%	−75.3	36.9%	−203.8	2.4%
Mean	−121.3	57.2%	−71.0	40.6%	−195.3	2.2%

Notes: This table decomposes the total counterfactual change in outflows from adopting states into direct and indirect components. Direct effects (columns 1-2) measure changes in flows from adopter i to non-adopting destinations: $\Delta y_i^{\text{direct}} = \sum_{j \notin A}(\widehat{y}_{i,j}^{cf} - \widehat{y}_{i,j})$, where A denotes the set of adopting states. Indirect effects (columns 3-4) measure changes in flows from adopter i to other adopting destinations: $\Delta y_i^{\text{indirect}} = \sum_{j \in A, j \neq i}(\widehat{y}_{i,j}^{cf} - \widehat{y}_{i,j})$. Total effects (columns 5-6) represent the sum: $\Delta y_i^{\text{total}} = \Delta y_i^{\text{direct}} + \Delta y_i^{\text{indirect}} = \sum_{j \neq i}(\widehat{y}_{i,j}^{cf} - \widehat{y}_{i,j})$. Share columns report each component as percentage of total effect. The GE Adj. column shows residual deviation from shares summing to 100%, reflecting numerical precision in the gravity estimation. All level changes represent firm counts over the 1910s decade.

Table IX reports this decomposition. Direct effects account for 33–84% of total changes across states, with substantial heterogeneity. Missouri shows the strongest direct channel (84%), while Connecticut and Massachusetts exhibit more balanced contributions between direct (33–46%) and indirect (51–71%) effects. This heterogeneity reflects differences in trading partner composition: states with many relocations to other adopters (like Connecticut and Massachusetts firms moving to New York) show larger indirect effects. General equilibrium adjustments are typically small (mean 2.2%), confirming that partial equilibrium calculations would closely approximate total effects. This validates the partial equilibrium identification strategy of equation (9): the structural fixed effects adequately absorb

multilateral resistance adjustments. The spillover effects on non-adopters, while individually modest (Table B.VIII), sum to approximately $1,538$ firms across all non-adopting state pairs, demonstrating that tax policy in eight states affects the entire spatial equilibrium through multilateral resistance adjustments.

2. *Individual State Counterfactuals*

To examine heterogeneity across adopting states and isolate spillover patterns, I conduct eight additional counterfactuals where seven states adopt but one does not. For each adopting state $i \in \mathcal{I}$, I define:

$$\tau_{k,j}^{cf(i)} = \tau_{k,j} \times (1 - \mathbb{1}(k = i)), \tag{20}$$

such that state i is the sole non-adopter while all other seven states maintain their historical policies. Figure B.VI displays the results. The state excluded from adoption experiences a substantial counterfactual reduction in outflows (ranging from -4.2% for Missouri to -7.1% for Connecticut), confirming the robustness of the baseline findings in a conditional general equilibrium framework. Neighboring states of the excluded adopter show modest spillover effects, typically 0.2-0.8 percentage points, suggesting that geographic proximity amplifies general equilibrium linkages. These patterns are consistent with Berry and Berry [1992], who documented that state tax policies exhibit spatial correlation due to policy competition and information diffusion among geographically proximate jurisdictions.

VII.C. No Adoption in the 1920s

I extend the analysis to the 1920s adoption wave, examining the counterfactual where the eight states that adopted corporate income taxes in the 1920s (Mississippi, North Carolina, South Carolina, Tennessee, Arkansas, California, Georgia, Oregon) did not do so. This scenario maintains the 1910s adoptions, consistent with the interpretation that adoption effects represent temporary relocation shocks (Section VI.B) that had largely dissipated by the 1920s. Thus, in both observed and counterfactual 1920s scenarios, the 1910s adopters face $\tau_{i,j,1920s} = 0$. This enables me to distinguish where firms from 1920s adopters relocate based on destinations' prior tax experience. Direct effects are decomposed into flows to states that adopted in the 1910s and flows to states that never adopted corporate taxes. Indirect effects cover flows from 1920s adopter i to other 1920s adopters. Table X reports this decomposition. The results reveal substantial heterogeneity across adopting states. Indirect effects range from 11% to

87%, with a mean of 55%. This variation reflects differences in regional integration: Southern states show strong indirect effects, consistent with dense economic ties within the South, while Western states show more balanced distributions. Flows to 1910s adopters account for 8.5% of total changes, while flows to never adopters account for 37%. However, this disparity is entirely mechanical, reflecting differences in group sizes. Per destination, 1920s adopters send virtually identical flows to each group (1910s adopters: $12.2/8 = 1.53$; never adopters: $47.2/32 = 1.48$). This destination equivalence is a joint prediction of proposition 3 and proposition 4.

TABLE X: Decomposition of 1920s Counterfactual

	Indirect Effect		Direct: 1910s		Direct: Never		Total Effect	
State	Level	Share	Level	Share	Level	Share	Level	GE Adj.
Arkansas	−79.3	59.8%	−8.6	6.5%	−54.5	41.2%	−132.5	−7.5%
California	−24.6	10.6%	−50.1	21.6%	−116.4	50.3%	−231.3	17.4%
Georgia	−70.7	61.9%	−3.2	2.8%	−43.2	37.8%	−114.1	−2.5%
Mississippi	−69.2	71.4%	−3.1	3.2%	−35.4	36.5%	−97.0	−11.0%
North Carolina	−54.2	54.0%	−12.6	12.5%	−23.3	23.1%	−100.5	10.4%
Oregon	−36.0	47.9%	−9.0	12.0%	−37.8	50.2%	−75.2	−10.2%
South Carolina	−64.3	86.6%	−0.8	1.0%	−5.9	7.9%	−74.2	4.4%
Tennessee	−63.0	49.0%	−10.3	8.0%	−61.5	47.8%	−128.6	−4.8%
Mean	−57.7	55.1%	−12.2	8.5%	−47.2	36.8%	−119.2	−0.5%

Notes: This table decomposes the total counterfactual change in outflows from 1920s adopting states (Arkansas, California, Georgia, Mississippi, North Carolina, Oregon, South Carolina, Tennessee) into components based on destination characteristics. The counterfactual assumes these states did not adopt corporate income taxes in the 1920s, while maintaining the observed 1910s adoptions (which had dissipated by the 1920s baseline). Indirect effects (columns 1-2) measure changes in flows from 1920s adopter i to other 1920s adopting destinations: $\Delta y_i^{\text{indirect}} = \sum_{j \in A_{1920s}, j \neq i} (\widehat{y}_{i,j}^{cf} - \widehat{y}_{i,j})$, where A_{1920s} denotes the set of 1920s adopters. Direct effects to 1910s adopters (columns 3-4) measure changes in flows to states that adopted in the 1910s: $\Delta y_i^{1910s} = \sum_{j \in A_{1910s}} (\widehat{y}_{i,j}^{cf} - \widehat{y}_{i,j})$. Direct effects to never adopters (columns 5-6) measure changes in flows to states that never adopted: $\Delta y_i^{\text{never}} = \sum_{j \notin (A_{1910s} \cup A_{1920s})} (\widehat{y}_{i,j}^{cf} - \widehat{y}_{i,j})$. Total effects (columns 7-8) equal the sum of all three components: $\Delta y_i^{\text{total}} = \Delta y_i^{\text{indirect}} + \Delta y_i^{1910s} + \Delta y_i^{\text{never}}$. Share columns report each component as percentage of total effect. The GE Adj. column shows residual deviation from shares summing to 100%, reflecting numerical precision in the gravity estimation. All level changes represent firm counts over the 1920s decade.

proposition 3 establishes that the adoption shock is temporary: once the pool of potential movers is depleted, the flow effect vanishes. By the 1920s, the 1910s adoption effects have dissipated, returning these states to their pre-adoption equilibrium. proposition 4 establishes that fixed compliance costs operate at the origin margin. Firms are triggered by the origin state's adoption, not by destination characteristics. Together, these propositions predict that 1920s adopters should be indifferent between 1910s adopters and never-adopters as destinations, which is precisely the pattern observed. This result also reinforces the directional interpretation of the partial equilibrium estimates: the tax adoption operates through the origin (push) channel rather than the destination (pull) channel, consistent with

the structural model in which $\tau_{i,j,t}$ enters through the origin state's adoption decision $z_{i,t}$ and does not appear in the destination attractiveness term $\texttt{Pull}_{j,t(d)}$.

VIII. CONCLUSION

This paper investigates whether firms relocate in response to the introduction of corporate income taxation. Using the staggered adoption of state corporate income taxes in the United States between 1910 and 1930, I estimate the causal effects of tax adoptions on interstate firm relocations. The unique historical context of unharmonized tax introductions across 16 states with variation in timing, rates, and sectoral coverage provides an ideal setting to study the extensive margin of corporate taxation.

Leveraging newly linked census data to construct bilateral state-pair firm flows, I employ a structural gravity framework with high-dimensional fixed effects to absorb time-varying push and pull factors. The main specification finds that corporate tax adoptions increased interstate firm flows by 13.02%. Decomposing this effect, roughly 15–20% reflects a pure adoption response independent of rate levels, consistent with fixed compliance costs that do not scale with tax rates. Effects are driven by manufacturing, mercantile, and service sectors, while the heavily regulated financial sector shows no significant response. Effects decline with distance from state borders and peak approximately five years after adoption before diminishing, consistent with a temporary shock as responsive firms exhaust the pool of potential movers rather than a permanent shift in outflows.

The mechanism analysis provides direct evidence that corporate tax adoptions increased administrative compliance costs, with adopter states experiencing a 14% rise in salaried personnel per firm. Importantly, personal income tax adoptions show no effect on either administrative staffing or firm relocations, confirming that the findings reflect responses to corporate taxation specifically. The implied stock elasticity of 0.7-0.8 per percentage point is comparable to modern estimates, suggesting that, despite factors biasing estimates downward, early 20th century firms were at least as tax-sensitive as their modern counterparts. Mediation analysis indicates that roughly 9% of the decline in corporate activity documented in prior work can be attributed to these relocations.

Counterfactual analysis reveals substantial general equilibrium effects. Had corporate taxes not been introduced in the 1910s, adopter states would have retained over 1,600 firms collectively. Spillover effects through multilateral resistance adjustments imply that tax policy in adopting states affected firm flows throughout the entire spatial equilibrium.

These findings contribute to the literature on corporate tax incidence and firm location decisions by providing the first estimates focused on the extensive margin of taxation. The evidence that compliance costs, not just tax burdens, drive relocation decisions has implications for contemporary tax policy. As jurisdictions compete for corporate activity, understanding that the introduction of new taxes generates distinct behavioral responses from rate changes can inform both the design and communication of tax policy.

The results are subject to limitations. The use of employers as a proxy for firms, while validated against census of manufactures data, may introduce measurement error. The decennial structure of census data precludes analysis of short-run dynamics. And while the historical context provides clean identification, external validity to modern corporate environments requires caution given differences in regulatory complexity and relocation technologies. Several extensions merit future research: examining heterogeneity by firm size and organizational complexity, investigating long-run effects on state economic development, and applying this framework to contemporary settings where regional tax adoption varies.

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ONLINE APPENDIX

Corporate Tax Adoptions and Firm Relocations

A. PROOFS

Proof of Proposition 1. *Part (i): Gravity Representation.* Firm h in state i chooses destination $j^* = \arg\max_{j \in \mathcal{J}} \Lambda_{h,i,j,t(d)}$. Since $\nu_{h,i,j,t(d)}$ is iid Type-I extreme value, the probability that firm h optimally chooses j is given by the multinomial logit formula [McFadden et al., 1973]:

$$\Pr(j^* = j \mid i) = \frac{\exp(\alpha p_{j,t(d)} + \beta \tau_{i,j,t(d)} - \gamma c_{i,j})}{\sum_{k \in \mathcal{J}} \exp(\alpha p_{k,t(d)} + \beta \tau_{i,k,t(d)} - \gamma c_{i,k})}. \tag{1}$$

By a law of large numbers argument over the population of firms in state i, the expected relocation share equals this choice probability: $\mathbb{E}(\phi_{i,j,t(e)}) = \Pr(j^* = j \mid i)$. Substituting into the accounting identity $\mathbb{E}(y_{i,j,t(e)}) = \mathbb{E}(\phi_{i,j,t(e)}) \times f_{i,t(b)} \times g(t)$ yields:

$$\begin{aligned}\mathbb{E}(y_{i,j,t(e)}) &= \frac{\exp(\beta \tau_{i,j,t(d)}) \times \texttt{Pull}_{j,t(d)}/\texttt{Costs}_{i,j}}{\sum_{k \in \mathcal{J}} \exp(\beta \tau_{i,k,t(d)}) \times \texttt{Pull}_{k,t(d)}/\texttt{Costs}_{i,k}} \times f_{i,t(b)} \times g(t) \\ &= \exp(\beta \tau_{i,j,t(d)}) \times \texttt{Push}_{i,t(d)} \times \frac{\texttt{Pull}_{j,t(d)}}{\texttt{Costs}_{i,j}} \times f_{i,t(b)} \times g(t),\end{aligned}$$

establishing the gravity representation.

Part (ii): Comparative Static. Let $j \neq i$. Denote the regimes under adoption ($z_{i,t} = 1$) and no adoption ($z_{i,t} = 0$) by superscripts A and N, respectively. Recall that $\tau_{i,j,t(d)} = z_{i,t} \mathbb{1}(i \neq j)$, so $\tau_{i,i,t(d)} = 0$ under both regimes. Decompose the denominator of equation (1) into

$$\Omega_i := \frac{\texttt{Pull}_{i,t(d)}}{\texttt{Costs}_{i,i}} \qquad \text{and} \qquad \Omega_{-i} := \sum_{k \neq i} \frac{\texttt{Pull}_{k,t(d)}}{\texttt{Costs}_{i,k}},$$

representing the attractiveness of staying and the aggregate attractiveness of all interstate destinations, respectively. Under no adoption ($\tau_{i,k,t(d)} = 0$ for all k):

$$\mathbb{E}(y^N_{i,j,t(e)}) = \frac{\texttt{Pull}_{j,t(d)}/\texttt{Costs}_{i,j}}{\Omega_i + \Omega_{-i}} \times f_{i,t(b)} \times g(t).$$

Under adoption ($\tau_{i,k,t(d)} = 1$ for all $k \neq i$):

$$\mathbb{E}(y^A_{i,j,t(e)}) = \frac{\exp(\beta) \times \texttt{Pull}_{j,t(d)}/\texttt{Costs}_{i,j}}{\Omega_i + \exp(\beta) \times \Omega_{-i}} \times f_{i,t(b)} \times g(t).$$

Since $f_{i,t(b)}$ and $g(t)$ are common to both regimes, the ratio of expected flows is:

$$\frac{\mathbb{E}(y^A_{i,j,t(e)})}{\mathbb{E}(y^N_{i,j,t(e)})} = \exp(\beta) \times \frac{\Omega_i + \Omega_{-i}}{\Omega_i + \exp(\beta) \times \Omega_{-i}}. \tag{2}$$

Define $\omega := \Omega_i / \Omega_{-i} > 0$ and $B := \exp(\beta)$. Then:

$$R(\beta) := \frac{\mathbb{E}(y^A_{i,j,t(e)})}{\mathbb{E}(y^N_{i,j,t(e)})} = B \frac{\omega + 1}{\omega + B}. \tag{3}$$

Note that $R(0) = 1$. Differentiating with respect to β:

$$R'(\beta) = \frac{B\omega(\omega + 1)}{(\omega + B)^2} > 0, \tag{4}$$

where the inequality follows from $B, \omega > 0$. Since $R(0) = 1$ and $R'(\beta) > 0$ for all $\beta > 0$, it follows that $R(\beta) > 1$ whenever $\beta > 0$, establishing $\mathbb{E}(y^A_{i,j,t(e)}) > \mathbb{E}(y^N_{i,j,t(e)})$. ■

Proof of Corollary 1.1. Since $\beta = \beta^F + \beta^r \text{Rate}_i$ with $\beta^r > 0$, an increase in Rate_i raises β. By equation (3), $R(\beta)$ is strictly increasing in β (as $R'(\beta) > 0$ by equation (4)), establishing the result. ■

Proof of Proposition 3. Under assumption 1, the pool of potential movers at the moment of adoption is M_i. Under assumption 2, in each year following adoption a fraction λ of the remaining pool relocates. After s years, the remaining pool is $(1 - \lambda)^s M_i$, so cumulative relocations are:

$$Y(s) = M_i \left[1 - (1 - \lambda)^s\right].$$

Part (i). Differentiating with respect to s:

$$Y'(s) = M_i (1 - \lambda)^s \ln\left(\frac{1}{1 - \lambda}\right) > 0,$$

$$Y''(s) = -M_i (1 - \lambda)^s \left[\ln\left(\frac{1}{1 - \lambda}\right)\right]^2 < 0,$$

where the signs follow from $M_i > 0$, $\lambda \in (0, 1)$, and hence $\ln(1/(1 - \lambda)) > 0$. This establishes strict monotonicity and strict concavity.

Part (*ii*). From the expression for $Y'(s)$, we have $\lim_{s\to\infty} Y'(s) = M_i \cdot 0 \cdot \ln(1/(1-\lambda)) = 0$, since $\lim_{s\to\infty}(1-\lambda)^s = 0$ for $\lambda \in (0,1)$. ■

B. TABLES & FIGURES

TABLE B.I: Description of the Businesses Included, by Sector

Sector	Types of businesses within industries
Manufacturing and Mercantile	
Agriculture	farming, forestry, fisheries
Construction	
Manufacturing (Dur.)	logging, sawmills, planing mills, and mill work, misc wood products, furniture and fixtures, glass and glass products, cement, concrete, gypsum and plaster products, structural clay products, pottery and related products, misc nonmetallic mineral and stone products, blast furnaces, steel works, and rolling mills, other primary iron and steel industries, primary nonferrous industries, fabricated steel products, fabricated nonferrous metal products, not specified metal industries, agricultural machinery and tractors, office and store machines, misc machinery, electrical machinery, equipment and supplies, motor vehicles and motor vehicle equipment, aircraft and parts, ship and boat building and repairing, railroad and misc transportation equipment, professional equipment, photographic equipment and supplies, watches, clocks, and clockwork
Manufacturing (Non-Dur.)	meat products, dairy products, canning and preserving fruits, vegetables, and seafoods, grain-mill products, bakery products, confectionery and related products, beverage industries, misc food preparations and kindred products, not specified food industries, tobacco manufactures, knitting mills, dyeing and finishing textiles, except knit goods, carpets, rugs, and other floor coverings, yarn, thread, and fabric
Mining	coal and metal mining, crude petroleum and natural gas extraction
Wholesale	motor vehicles and equipment, drugs, chemicals, and allied products, dry goods apparel, food and related products, electrical goods, hardware, and plumbing equipment, machinery, equipment, and supplies, petroleum products, farm products (raw materials), miscellaneous wholesale trade, not specified wholesale trade, food stores, except dairy, dairy products stores and milk retailing, general merchandise, five and ten cent stores, apparel and accessories stores, except shoe, shoe stores, furniture and house furnishings stores, household appliance and radio stores, motor vehicles and accessories retailing, gasoline service stations, drug stores, eating and drinking places, hardware and farm implement stores, lumber and building material retailing, liquor stores, retail florists, jewelry stores, fuel and ice retailing
Services	
Business Services	advertising, accounting, auditing and bookkeeping services
Entertainment	entertainment and recreation services: radio broadcasting and television, theaters and motion pictures, bowling alleys, and billiard and pool parlors
Personal Services	private households, hotels and lodging places, laundering, cleaning, and dyeing, dressmaking shops, shoe repair shops
Repair Services	auto repair services and garages
Professional Services	medical and other health services, except hospitals, hospitals, legal services, educational services, welfare and religious services, nonprofit membership organizations, engineering and architectural services
Financial Sector	
Finance	banking and credit, security and commodity brokerage and investment companies, insurance, real estate, real estate-insurance-law offices
Transportation and Other Public Utilities	
Telecommunication	telephone, telegraph
Transportation	railroads and railway, street railways and bus lines, trucking service, warehousing and storage, taxicab service, water transportation, air transportation, petroleum and gasoline pipelines, services incidental to transportation
Utilities	electric light and power, gas and steam supply systems, electric-gas utilities, water supply, sanitary services

Notes: The table provides detailed industry classifications for the aggregated sectors used throughout the analysis. Sector definitions are based on the 1950 Census Bureau industrial classification system, harmonized backwards to match 1900-1930 occupational codes in the full-count decennial censuses. Manufacturing and Mercantile encompasses all goods-producing industries plus wholesale and retail trade. The distinction between durable and non-durable manufacturing follows standard definitions: durables include products with expected lifespans exceeding three years (machinery, metals, wood products), while non-durables include consumables and soft goods (food, textiles, tobacco). Wholesale represents a sub-sector combining wholesale trade with retail establishments, as early census data did not consistently distinguish these categories. Services covers all non-goods-producing commercial activities, from personal services (hotels, laundries) to professional services (medical, legal, educational). Financial Sector includes all establishments involved in banking, insurance, real estate, and investment activities. Transportation and Other Public Utilities encompasses firms providing transportation services (rail, trucking, water, air) and utility services (electricity, gas, water, telecommunications). The classification scheme reflects historical business organization: many modern business services (consulting, IT) did not exist, while some categories (street railways, telegraph) were major industries. Employers are assigned to sectors based on their reported occupation and industry at the beginning of each decade. Industry classifications follow **?** harmonization.

TABLE B.II: Average Internal Relocations and Sample Population for Firms of Different Industries

	Pair Outflow		State Outflow		State Sample Population		
Panel	Mean	Stand. Dev.	Mean	Stand. Dev.	Mean	Stand. Dev.	*Mobility Rate*
All Firms	54.01	129.62	2,551.63	2,390.22	12,739.85	13,783.31	16.21%
All Individuals	1,162.44	2,693.82	55,083.09	43,558.87	269,416.30	245,836.60	20.45%
Manufacturing and Mercantile							
Agriculture	36.54	93.92	1,724.40	1,695.19	11,635.39	9,825.69	14.82%
Construction	2.63	8.17	124.53	173.17	638.77	907.89	19.50%
Manufacturing (Dur.)	1.61	5.11	75.99	99.62	369.53	547.74	20.56%
Manufacturing (Non-Dur.)	1.72	7.58	81.62	152.40	403.55	808.66	20.23%
Mining	0.35	1.38	16.33	28.11	59.96	107.21	27.24%
Wholesale	6.40	21.04	303.01	430.80	1,560.26	2,245.58	19.42%
Services							
Business Services	0.14	0.69	6.56	13.23	31.81	68.54	20.61%
Entertainment	0.17	0.70	8.26	11.61	27.99	43.77	29.53%
Personal Services	1.59	5.44	75.51	111.90	323.77	486.20	23.32%
Repair Services	0.53	1.64	24.94	29.14	142.57	167.37	17.50%
Professional Services	0.51	1.89	24.23	36.32	135.98	215.37	17.82%
Financial Sector							
Finance	0.51	1.96	24.19	37.59	121.54	195.88	19.91%
Transportation and Other Public Utilities							
Telecommunication	0.03	0.23	1.61	2.75	6.10	9.36	26.39%
Transportation	0.70	2.20	33.28	42.77	157.41	207.85	21.15%
Utilities	0.39	0.23	1.87	2.74	7.53	11.60	24.82%

Notes: The table presents summary statistics for bilateral interstate firm relocation flows and state-level populations across all sectors. A single observation represents a directed state pair (i, j) in decade t, yielding $3 \times 48^2 = 6,912$ total observations. *Pair Outflow* measures the number of employers relocating from state i to state j during the decade, which serves as the dependent variable in the main gravity specifications. *State Outflow* aggregates total relocations from state i to all other states. *State Sample Population* represents the total number of employers in state i at the decade start. *Mobility Rate* is computed as the ratio of average state outflow to average state sample population, measuring the propensity to relocate. All statistics exclude within-state pairs (i, i). The agricultural sector drives overall patterns due to its large share of sample firms and universal taxation across adopter states.

TABLE B.III: PE Effects Within a Decade

	Linear	Polynomial
	(i)	(ii)
Panel A: Full Panel		
$\tau_{i,j,t} \times \text{YSA}_{i,j,t}$	0.018***	0.059***
	(0.005)	(0.016)
$\times \text{YSA}^2_{i,j,t}$		-0.005**
		(0.002)
Panel B: Sector-specific effects		
Manufacturing and Mercantile	0.018***	0.061**
	(0.005)	(0.002)
		-0.006**
		(0.002)
Services	0.030***	0.033
	(0.010)	(0.031)
		-0.000
		(0.004)
Financial Sector	-0.009	-0.006
	(0.026)	(0.070)
		-0.000
		(0.009)
Transportation and Other Public Utilities	0.031*	0.083
	(0.018)	(0.078)
		-0.007
		(0.010)
Observations	24,949	24,949
McFadden Pseudo R^2	0.996	0.996

Notes: The table presents PPML estimation results examining how corporate tax adoption effects evolve with time since adoption. The specifications test whether relocation responses follow a linear or concave pattern within the adoption decade by interacting the tax policy variable with years since adoption (YSA), defined as $\text{YSA}_{i,t} = t(e) - \text{Adoption Year}_i$, measuring the number of years between state i's tax adoption and the decade endpoint $t(e)$ when flows are observed. YSA ranges from 0 to 9 for adopting states within their adoption decade. Column (i) estimates a linear specification with $\tau_{i,j,t} \times \text{YSA}_{i,t}$ as the interaction term. Column (ii) adds a quadratic term $(\tau_{i,j,t} \times \text{YSA}_{i,t})^2$ to test for concavity. All specifications include bilateral state-pair fixed effects $\delta_{i,j}$, origin state–decade fixed effects $\delta_{i,t}$, destination state–decade fixed effects $\delta_{j,t}$, and interstate-decade fixed effects $\delta_{i \neq j,t}$. Standard errors, clustered by state pair, appear in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Estimation procedures and data processing follow Table IV. Marginal effects plotted in Figure IV are computed as $[\exp(\widehat{\varphi}_1 \text{YSA}_{i,t} + \widehat{\varphi}_2 (\text{YSA}_{i,t})^2) - 1] \times 100$ for YSA $\in [0, 9]$.

TABLE B.IV: Regression for Tax Rates at the Time of Adoption

	$r = Max_i$	$r = Min_i$	Tax Flatness
	(i)	(ii)	(iii)
Panel A: Full Panel			
$\tau^r_{i,j,t}$	0.036***	0.046***	
	(0.006)	(0.008)	
$(\tau_{i,j,t} \times Min_i)/Max_i \times 10$			0.014***
			(0.003)
Panel B: Sector-specific effects (collapsed into one row)			
Manufacturing and Mercantile	0.036***	0.048*	0.014***
	(0.006)	(0.008)	(0.003)
Services	0.030***	0.030***	0.013***
	(0.011)	(0.011)	(0.004)
Financial Sector	0.004	0.005	0.009
	(0.024)	(0.027)	(0.010)
Transportation and Other Public Utilities	0.043	0.087*	0.019*
	(0.031)	(0.044)	(0.011)
Observations	24,949	24,949	24,949
McFadden Pseudo R^2	0.996	0.996	0.996

Notes: The table presents PPML estimation results examining the intensive margin effects of adopted tax rates on interstate firm relocation flows. The specifications replace the binary tax adoption indicator with continuous rate measures: $\tau^r_{i,j,t,S} = (\tau_{i,j,t} \times w_{i,S}) \times \text{Rate}_i$, where Rate_i represents either the maximum or minimum adopted tax rate in state i (Figure Va and Figure Vb). Coefficients β^r_S measure the percentage point change in relocation flows associated with a one percentage point increase in the adopted rate. The flatness measure, defined as $[(\tau_{i,j,t} \times w_{i,S}) \times Min_i]/Max_i \in [0, 1]$, captures the inverse of rate progressivity; higher values indicate the minimum and maximum rates are closer together. This measure is multiplied by $100/10$ to interpret coefficients as the effect of a 10 percentage point increase in rate flatness. All specifications include bilateral state-pair, origin state–decade, destination state–decade, and decade-interstate fixed effects. Sector-specific results are reported in one row, where the respective model defines the use of the regressor. Standard errors, clustered by state pair, appear in parentheses below the coefficients. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Estimation procedures and data processing follow Table IV.

TABLE B.V: Decomposition by Type of Adopted Income Tax

	Relative	Absolute
	(i)	(ii)
Panel A: Full Panel		
Only personal tax adoption: $\tau^{pers}_{i,j,t}$	−0.056	−2.95
	(0.064)	(3.27)
Only corporate tax adoption: $\tau^{corp}_{i,j,t}$	0.297***	18.69***
	(0.067)	(4.85)
Both taxes adopted: $\tau^{both}_{i,j,t}$	0.074***	4.12***
	(0.025)	(1.43)
Panel B: Sector-specific effects		
Manufacturing and Mercantile		
$\tau^{pers}_{i,j,t}$	−0.069	−3.31
	(0.066)	(3.03)
$\tau^{corp}_{i,j,t}$	0.285***	16.27***
	(0.069)	(4.51)
$\tau^{both}_{i,j,t}$	0.074***	3.83***
	(0.025)	(1.35)
Services		
$\tau^{pers}_{i,j,t}$	0.068	0.21
	(0.116)	(0.36)
$\tau^{corp}_{i,j,t}$	0.309***	1.07***
	(0.087)	(0.35)
$\tau^{both}_{i,j,t}$	0.079*	0.24*
	(0.045)	(0.14)
Financial Sector		
$\tau^{pers}_{i,j,t}$	0.445*	0.29*
	(0.256)	(0.20)
$\tau^{corp}_{i,j,t}$	0.403**	0.25**
	(0.171)	(0.13)
$\tau^{both}_{i,j,t}$	−0.056	−0.03
	(0.109)	(0.05)
Transportation and Other Public Utilities		
$\tau^{pers}_{i,j,t}$	−0.074	−0.06
	(0.170)	(0.12)
$\tau^{corp}_{i,j,t}$	0.085	0.07
	(0.272)	(0.23)
$\tau^{both}_{i,j,t}$	0.160	0.13
	(0.102)	(0.09)
Observations	24,949	24,949
McFadden Pseudo R^2	0.996	0.996

Notes: The table presents PPML estimation results decomposing corporate tax adoption effects by the type of income tax adopted. The specifications replace the baseline treatment variable $\tau_{i,j,t}$ with three mutually exclusive indicators: $\tau^{pers}_{i,j,t}$ for states adopting only personal income taxes, $\tau^{corp}_{i,j,t}$ for states adopting only corporate income taxes, and $\tau^{both}_{i,j,t}$ for states adopting both personal and corporate income taxes within the same decade. Column (i) reports relative effects, while column (ii) reports absolute effects computed as $(\exp(\widehat{\beta}_S) - 1) \times \bar{y}_S$, where $\bar{y}_S$ is the mean bilateral flow in sector S. All specifications include bilateral state-pair fixed effects $\delta_{i,j}$, origin state–decade fixed effects $\delta_{i,t}$, destination state–decade fixed effects $\delta_{j,t}$, and interstate-decade fixed effects $\delta_{i \neq j,t}$. Standard errors, clustered by state pair, appear in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Estimation procedures and data processing follow Table IV.

TABLE B.VI: Collection of Further Insights and Robustness Exercises

	Baseline	Alternative		Traditional		Include WA		Pushed		South Control	
	(i)	(ii)	(Δ)	(iii)	(Δ)	(iv)	(Δ)	(v)	(Δ)	(vi)	(Δ)
Panel A: Full Panel											
$\tau_{i,j,t}$	0.122***	0.150***	0.027	0.295***	0.172***	0.103***	−0.019	0.094***	−0.029	0.123***	0.001
	(0.023)	(0.029)	(0.037)	(0.041)	(0.047)	(0.022)	(0.032)	(0.027)	(0.036)	(0.023)	(0.033)
Panel B: Sector-specific effects											
Manufacturing and Mercantile	0.121***	0.143***	0.022	0.290***	0.169***	0.104***	−0.017	0.095***	−0.026	0.123***	0.002
	(0.023)	(0.029)	(0.037)	(0.042)	(0.048)	(0.022)	(0.032)	(0.028)	(0.036)	(0.024)	(0.033)
Services	0.138***	0.181***	0.043	0.252***	0.114**	0.092**	−0.046	0.089*	−0.049	0.133***	0.005
	(0.042)	(0.049)	(0.065)	(0.060)	(0.073)	(0.040)	(0.058)	(0.050)	(0.065)	(0.042)	(0.059)
Financial Sector	0.047	0.076	0.029	0.162	0.115	0.021	−0.026	−0.005	−0.052	0.061	0.014
	(0.092)	(0.110)	(0.143)	(0.145)	(0.172)	(0.089)	(0.128)	(0.123)	(0.154)	(0.092)	(0.130)
Transportation and Other Public Utilities	0.155*	0.128	−0.027	0.250*	0.095	0.120	−0.035	0.207*	0.052	0.143	−0.012
	(0.092)	(0.111)	(0.144)	(0.141)	(0.168)	(0.087)	(0.127)	(0.109)	(0.142)	(0.093)	(0.131)
Observations	24,949	24,949		24,949		24,949		24,949		24,949	
McFadden Pseudo R^2	0.996	0.996		0.996		0.996		0.996		0.996	

Notes: The table presents PPML estimation results for various robustness checks on the baseline specification. All specifications include bilateral state-pair fixed effects $\delta_{i,j}$, origin state–decade fixed effects $\delta_{i,t}$, destination state–decade fixed effects $\delta_{j,t}$, and interstate-decade fixed effects $\delta_{i\neq j,t}$. Standard errors, clustered by state pair, appear in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Column (Δ) reports the difference from the baseline coefficient with standard errors computed using the delta method. **Column (i)** reproduces the baseline specification with treatment variable $\tau_{i,j,t} = z_{i,t} - z_{i,t-1}$. **Column (ii) Alternative** uses $\tau^{alt}_{i,j,t} = \mathbb{1}(z_{i,t} > z_{j,t})$ to explicitly require origin-destination tax differentials. **Column (iii) Traditional** uses the ever-treated indicator $z^{trad}_{i,t} = \mathbb{1}(\text{Adoption Year}_i \leqslant t)$. **Column (iv) Include WA** adds Washington State as a 1920s adopter. **Column (v) Pushed** excludes late-decade adopters (California, Georgia, Missouri, Oregon adopted after 1927) and reassigns North Dakota (1919 adoption) from the 1910s to 1920s cohort. **Column (vi) South Control** addresses concerns about coordinated regional policy in the South during the 1920s by adding the interaction term $\mathbb{1}(t = 1920) \times \mathbb{1}(\text{South}_i > \text{South}_j)$, where South is defined per Census Bureau classification. To ensure convergence, statistically separated observations are excluded following Clarkson and Jennrich [1991], and singleton observations are dropped following Correia [2015].

TABLE B.VII: Estimation Results for 4WFE and Staggered Gravity

	Model(3b): 4WFE	Heterogeneous Treatment Effects		
		Simple ATT	Event Study	Group-Specific ATT
Panel A: Traditional Treatment (Baseline)				
$\tau_{i,j,t}^{trad}$	0.295***			
	(0.041)			
Panel B: Staggered Adoption Estimator				
Simple ATT		0.287***		
		(0.088)		
Event Study Coefficients (relative to $t = -1$):				
$t = 0$ (adoption decade)			0.258***	
			(0.064)	
$t = +1$ (post-adoption)			0.344**	
			(0.140)	
Group-Specific ATT:				
Group 2: 1910s Adopters				0.360***
				(0.128)
Group 3: 1920s Adopters				0.140***
				(0.038)
Observations	6,765		6,765	
McFadden Pseudo R^2	0.997		0.997	

Notes: The table presents results from alternative identification strategies addressing staggered treatment adoption and potential treatment effect heterogeneity, using never-treated states as the control group. Panel A reports the coefficient from the four-way fixed effects (4WFE) specification using the traditional ever-treated indicator $\tau_{i,j,t}^{trad} = \mathbb{1}(\text{Adoption Year}_i \leqslant t)$ Panel B implements the Wooldridge [2023] estimator following Nagengast and Yotov [2024] to account for staggered adoption timing. The Simple ATT represents the average treatment effect across all treated units and time periods, properly accounting for differential treatment timing. This estimate is nearly identical to the 4WFE coefficient, validating that the baseline specification does not suffer from bias due to staggered adoption or negative weighting of treatment effects. The Event Study coefficients are reported relative to $t = -1$ (the period immediately before adoption, normalized to zero). The adoption period coefficient $t = 0$ captures the immediate effect in the adoption decade. Group-Specific ATT estimates reveal treatment effect heterogeneity across adoption cohorts. Group 1 comprises eight states adopting in the 1910s and Group 2 comprises eight states adopting in the 1920s. The larger Group 1 effects may reflect stronger treatment intensity (earlier adopters faced greater uncertainty about compliance requirements) or compositional differences in firm types across regions. All specifications use PPML estimation. Standard errors, clustered by state pair, appear in parentheses for the 4WFE estimate. Standard errors for Simple ATT, Event Study, and Group-Specific ATT coefficients are computed using the delta method. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B.VIII: Non-Adopter Counterfactual: No 1910s Adoptions

State	$\Delta\widehat{y}_i$	$\Delta\widehat{y}_i \times \sum_{j\neq i} y_{i,j}$	$f_{i,t}$	State	$\Delta\widehat{y}_i$	$\Delta\widehat{y}_i \times \sum_{j\neq i} y_{i,j}$	$f_{i,t}$
Alabama	1.51%	89.69	29,564	Arizona	1.55%	7.55	1,443
Arkansas	1.24%	85.33	23,784	California	1.15%	49.93	27,342
Colorado	1.07%	33.79	9,377	Delaware	1.60%	6.75	2,811
Florida	1.92%	24.80	6,208	Georgia	1.59%	81.78	31,156
Idaho	1.24%	17.81	4,482	Illinois	0.75%	80.55	56,287
Indiana	1.40%	72.28	37,635	Iowa	0.87%	64.21	34,884
Kansas	0.69%	48.77	27,434	Kentucky	0.94%	48.79	32,546
Louisiana	1.69%	39.46	12,354	Maine	−0.37%	−3.41	8,416
Maryland	0.73%	13.09	14,580	Michigan	0.84%	42.07	37,705
Minnesota	0.23%	13.60	33,722	Mississippi	1.23%	76.02	24,818
Nebraska	0.83%	45.26	21,773	Nevada	0.92%	3.83	1,065
New Hampshire	−0.51%	−3.50	4,563	New Jersey	−0.45%	−15.79	18,721
New Mexico	1.30%	11.97	2,489	North Carolina	0.68%	25.89	32,911
Ohio	1.38%	83.10	49,996	Oklahoma	1.14%	90.64	21,219
Oregon	1.33%	27.28	8,520	Pennsylvania	0.53%	34.44	53,195
Rhode Island	−0.90%	−5.01	3,144	South Carolina	1.23%	39.56	22,889
South Dakota	0.57%	21.13	12,127	Tennessee	1.42%	73.66	26,419
Texas	1.34%	147.99	55,037	Utah	1.52%	8.72	3,567
Vermont	−0.41%	−3.86	6,566	Washington	1.01%	33.41	11,380
West Virginia	0.80%	14.46	14,165	Wyoming	1.18%	6.99	1,476

Notes: Figure reports the change in total state-level outflows under counterfactual scenarios in which all but one adopting state implement corporate income taxes in the 1910s. For each adopting state i, counterfactual flows $\widehat{y}_{i,j}^{cf(i)}$ are generated by setting $\tau_{k,j}^{cf(i)} = \tau_{k,j}(1 - \mathbb{1}(k = i))$ and resolving the general equilibrium gravity system described in Section VII.A. Percentage changes are computed as $\Delta y_i = [(\sum_{j\neq i} \widehat{y}_{i,j}^{cf(i)} - \widehat{y}_{i,j})/(\sum_{j\neq i} \widehat{y}_{i,j})] \times 100$. Labels report Δy_i for each excluded adopter. The figure displays results for the 1910s adoption wave.

FIGURE B.I: Estimates Effects of Tax Adoptions on State-Level Revenues

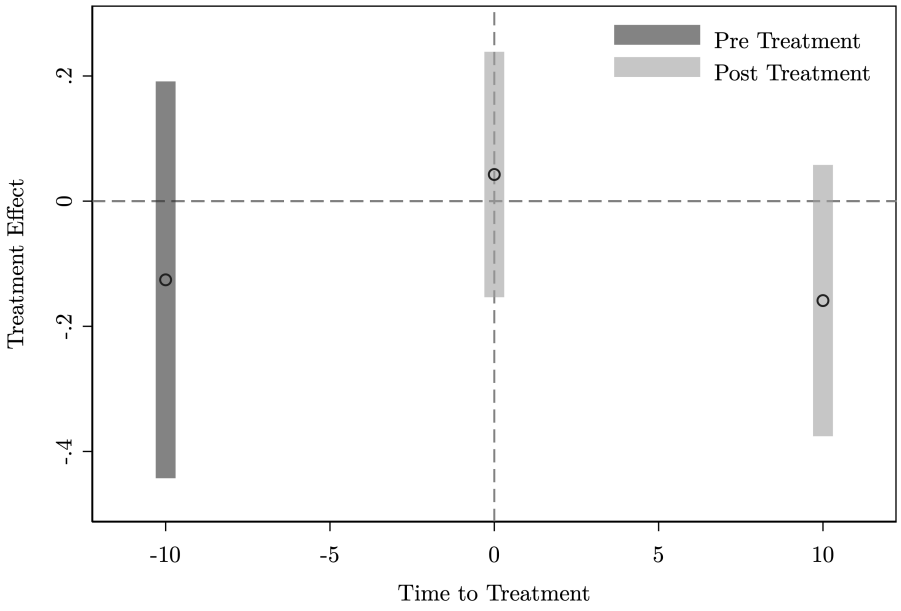
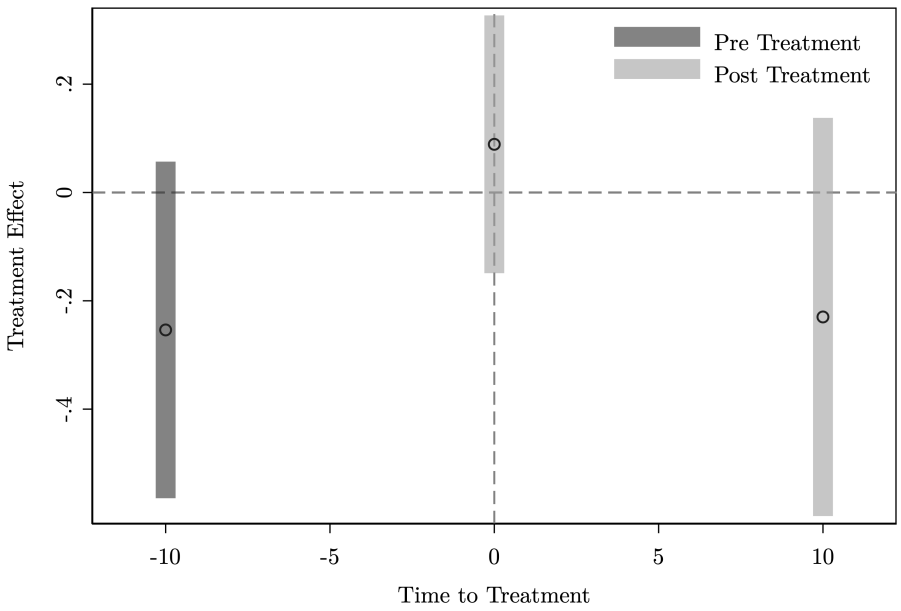
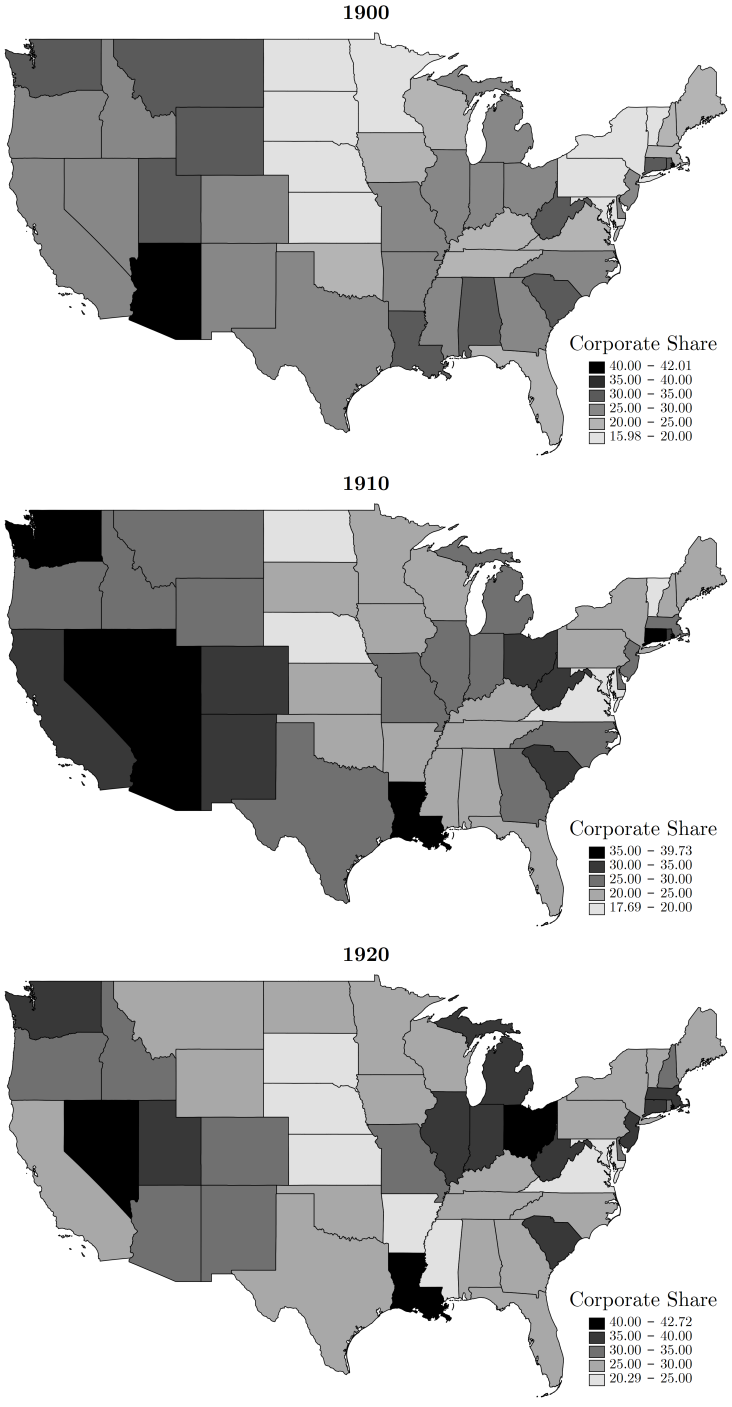


FIGURE B.II: Estimates Effects of Tax Adoptions on State-Level Expenditures



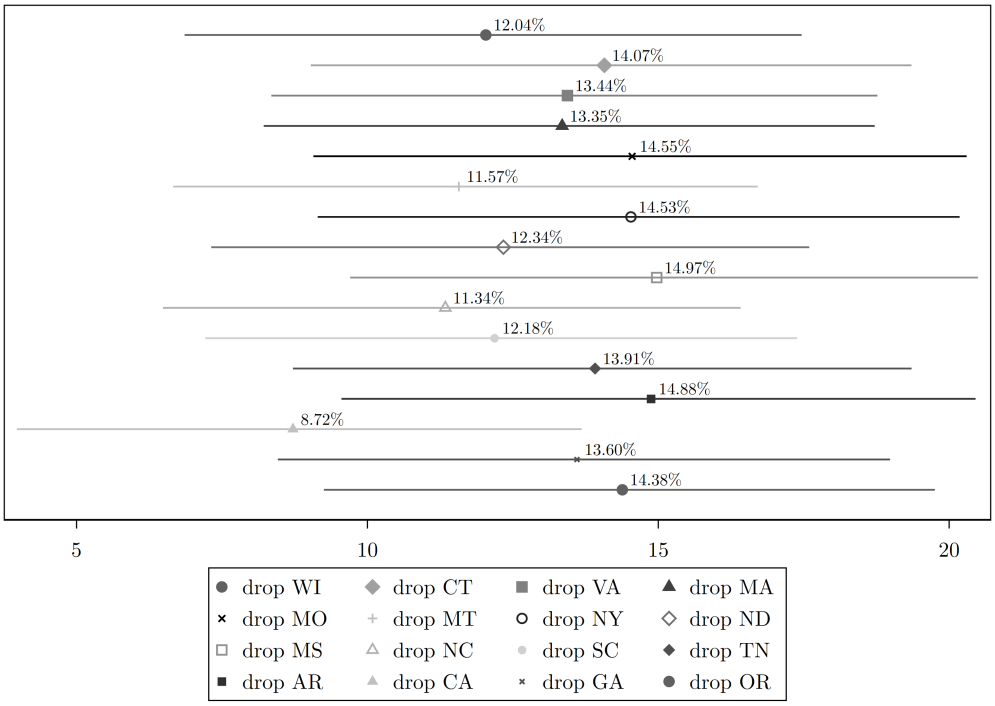
Notes: These figures shows event-study estimates of the effect of state income-tax adoption on log state revenues and expenditures, using the Callaway and Sant'Anna [2021] difference-in-differences framework with not-yet-treated states as the comparison group. Treatment is assumed to be absorbing. All regressions include state and decade fixed effects. Fiscal data come from Cassidy (2024). The balanced panel covers 48 states across three decades ($N = 3 \times 48 = 144$).

FIGURE B.III: Share of Incorporated Manufacturing Firms



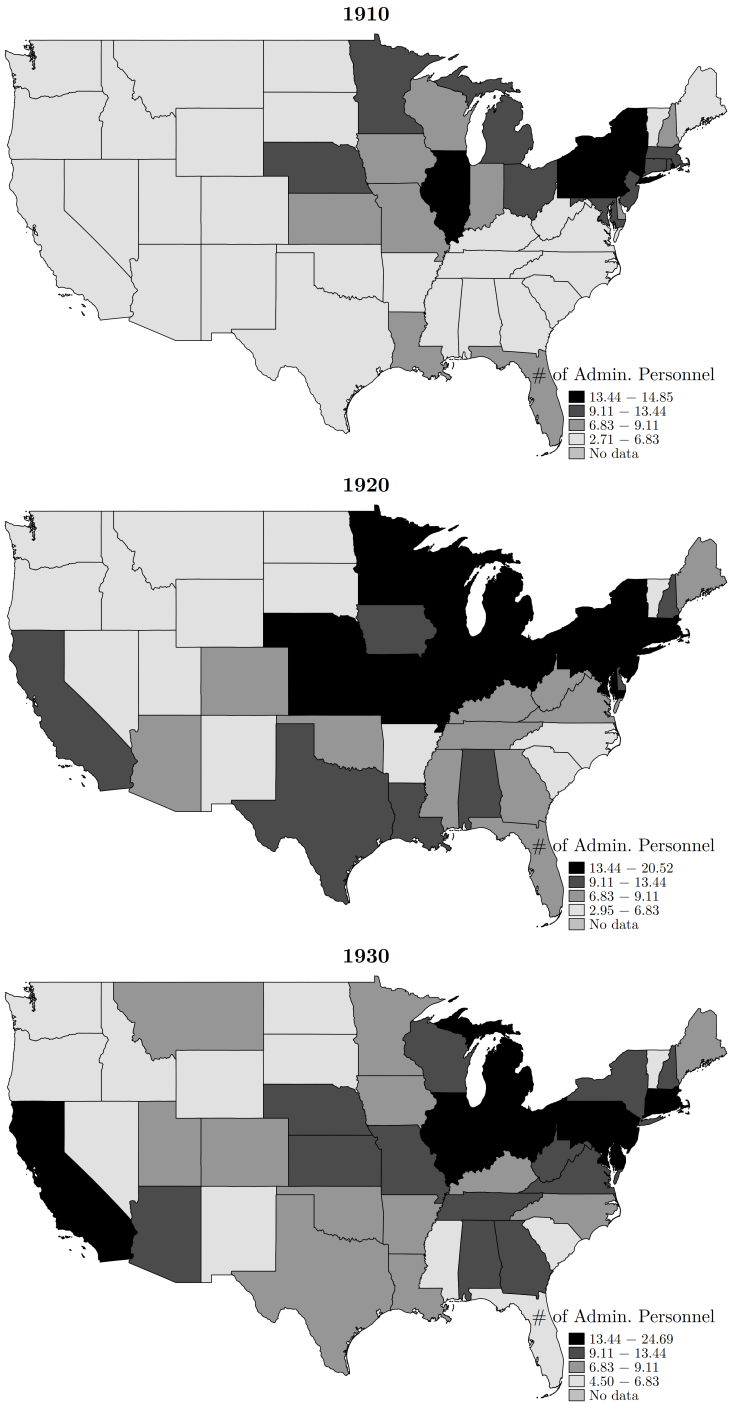
Notes: Maps show the share of incorporated manufacturing establishments across the lower 48 states in 1900, 1910, and 1920. Darker shading indicates a higher share of incorporation. Figures indicate a high level of incorporation in the early 20th century. Data is derived from published Census manufacturing tabulations (Census of Manufactures).

FIGURE B.IV: Estimates Repeated for Subsets, Leaving Out One Adopter State



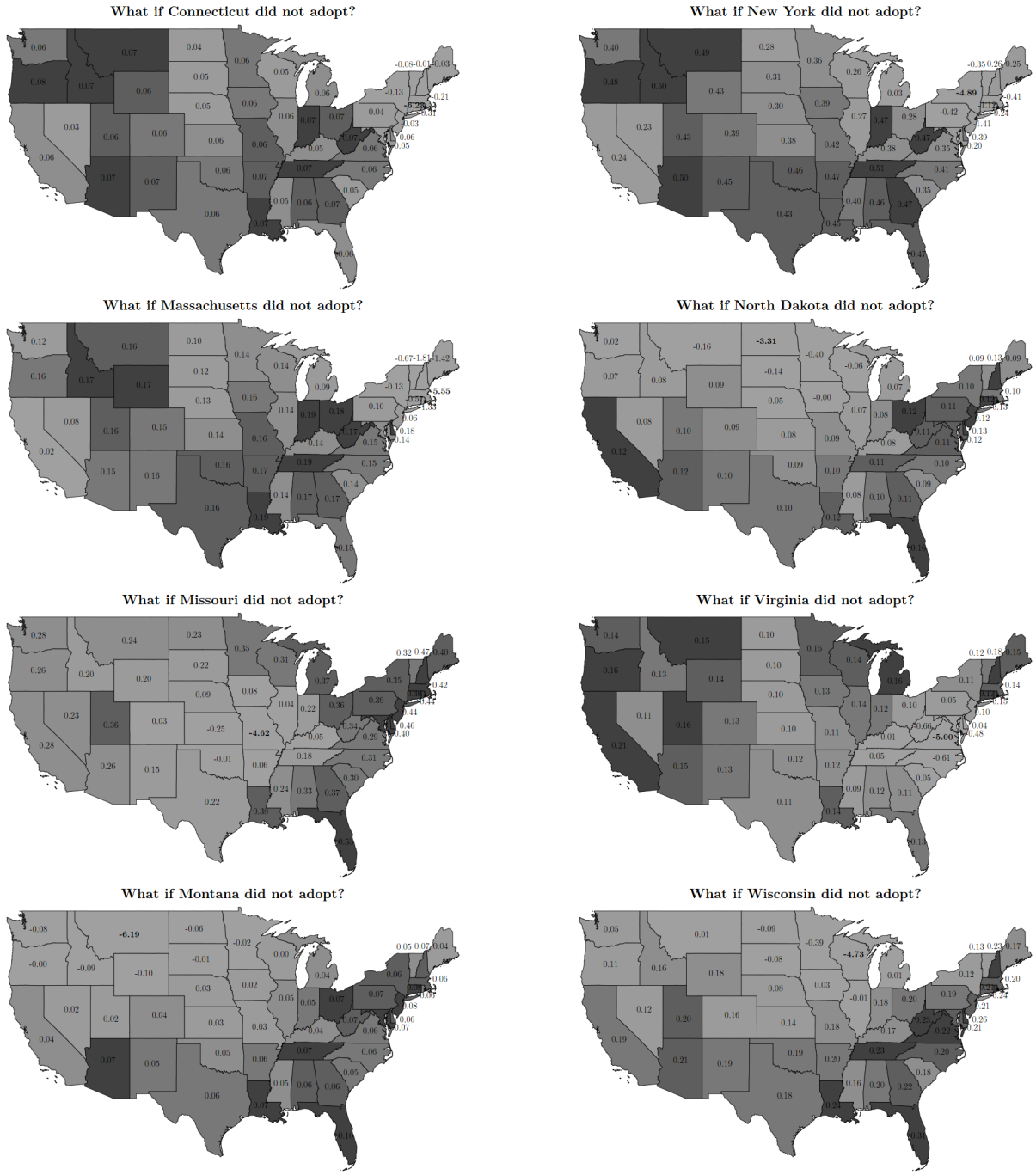
Notes: CI = 95%. Standard errors are clustered by state-pairs. To ensure convergence of the estimates, statistically separated observations are excluded based on fixed effects and Clarkson and Jennrich [1991], and singletons are excluded using Correia [2015]. Confidence intervals for each subset overlap, hence no state systematically drives the baseline results.

FIGURE B.V: Average Number of Administrative Personnel in each Corporation



Notes: Maps show the average number of administrative and salaried personnel per corporation across the lower 48 states in 1910, 1920, and 1930. Darker shading indicates a higher administrative workforce per firm. Data is derived from published Census manufacturing tabulations (Census of Manufactures).

FIGURE B.VI: Counterfactual Change in Relocation Flows, By Adopter State Not Adopting



Notes: Figure reports counterfactual percentage changes in total outflows for each adopting state under scenarios in which all other adopters retain their historical corporate income tax policies but state i does not adopt. For each $i \in \mathcal{I}$, counterfactual flows $\widehat{y}_{i,j}^{cf(i)}$ are computed using the general equilibrium gravity framework described in Section VII.A, holding structural bilateral costs fixed and re-solving for the counterfactual origin, destination, and interstate multilateral resistance terms. Labels report percentage changes in aggregate outflows, calculated as $\left[\sum_{j \neq i}(\widehat{y}_{i,j}^{cf(i)} - \widehat{y}_{i,j}) / \sum_{j \neq i} \widehat{y}_{i,j}\right] \times 100$. The figure displays results for the 1910s adoption wave.

C. WHAT DRIVES THE STATE TAX ADOPTIONS?

The historical context of the tax adoptions can be further explored by testing potential state-level drivers. Contemporary sources such as the National Industrial Conference Board [1930] proclaim the failure of the property tax as one reason for the adoptions. The literature further explores whether the adoption of neighboring states increased the likelihood of other adoptions, with mixed evidence [Berry and Berry, 1992; Cohen et al., 2014]. I can add to the quantitative discussion on drivers of state income tax adoption by estimating an exploratory model of predictors. Note that this analysis does not stand in contrast to the parallel trend assumption in Section V, as the dyadic panel structure allows controlling for state-time fixed effects in the gravity estimation. I estimate the following structure consisting of time-varying (TV) and constant predictors

$$z_{i,t(e)} = \beta_0 + \sum_{n=1}^{N} \beta_n \ln(\texttt{TV Predictor}^n_{i,t(b)}) + \sum_{m=1}^{M} \beta_m \ln(\texttt{Const. Predictor}^m_i) + \delta_t + \varepsilon_{i,t}, \quad (1)$$

where $z_{i,t(e)}$ is equal to 1 if state i adopts the tax during decade t and 0 otherwise. Note that $z_{i,t(e)+1} = 0$ if $z_{i,t(e)} = 1$. The adoption dummy is measured at the end of a respective decade $t(e)$, whereas the (TV) explanatory variables are measured at the beginning of the decade $t(b)$. This allows to alleviate concerns of simultaneity. I further add a decade fixed effect δ_t. All continuous predictors are denoted on the logarithmic scale. Hence, I can interpret β_n as percentage point changes in the probability of state i adopting the corporate income tax.

As reported in Table C.I, most potential predictors show no significant relationship with adoption timing. The low adjusted R^2 suggests that adoptions were not systematically driven by observable economic or political conditions, consistent with the idiosyncratic and politically contingent nature of early tax policy documented in historical accounts. To include unemployment, I have to drop the first decade from the sample, as labor market variables are unavailable for the 1900 census. The only significant predictor is the corporate share of manufacturing firms, where a 1 ppt increase in corporate share is associated with a decrease in adoption probability by roughly 0.3 ppt. The negative relationship suggests states with larger corporate manufacturing sectors and thus potentially stronger business lobbying capacity or greater sensitivity to firm exit threats were less likely to adopt corporate income taxes. Importantly, firm outflows themselves do not predict adoption, providing further evidence against reverse causality concerns for the main analysis.

TABLE C.I: Explorative Model of Potential Tax Adoption Drivers

	Tax adoption indicator: $z_{i,t(e)}$	
	(i)	(ii)
Firm-related Variables		
Total Firm Outflow	−0.004	−0.005
	(0.105)	(0.104)
Corporate Share of Manufacturing Firms	−0.280	−0.268
	(0.153)*	(0.152)*
Manufacturing and Mercantile Share of Total Firms	−0.305	−0.965
	(1.265)	(1.326)
Financial Share of Total Firms	0.036	0.026
	(0.072)	(0.072)
Public Utility Share of Total Firms	−0.021	−0.014
	(0.090)	(0.089)
Macroeconomic Variables		
Unemployment Rate		0.310
		(0.580)
Total Revenue	−0.223	−0.254
	(0.233)	(0.233)
Total Expenditure	0.216	0.213
	(0.193)	(0.192)
Political Variables		
Republican Governor Dummy	−0.084	−0.112
	(0.083)	(0.084)
Republican Presidential Dummy	0.026	0.026
	(0.102)	(0.102)
Population Variables		
Total Population	0.127	0.186
	(0.194)	(0.196)
Total Migration Outflow	−0.104	−0.133
	(0.225)	(0.225)
Geographic Variables		
Total State Area	0.003	−0.005
	(0.046)	(0.046)
Southern State Dummy	0.013	0.015
	(0.121)	(0.120)
Western State Dummy	0.088	0.095
	(0.112)	(0.111)
Decade FE	Yes	Yes
Observations	137	96
Adjusted R^2	0.018	0.127

Notes: The table reports estimates of the probability that state i adopts a corporate income tax during decade t. All continuous regressors are in logarithmic terms and measured at the beginning of each decade, while the adoption indicator is measured at the decade's end. Both specifications include decade fixed effects. Column (ii) includes unemployment but drops the first decade due to data availability. Fiscal data on revenues and expenditures are from Cassidy et al. [2024]. Political variables are collected manually from National Archives and the National Governors Association. *Republican Governor Dummy* indicates whether a state has a Republican governor at decade start, while *Republican Presidential Dummy* indicates if a state voted Republican in the previous presidential election. Corporate and firm data are from the Census of Manufactures. Population data are from the decennial US Census. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Beyond this, several coefficients are economically meaningful though statistically insignificant. Higher unemployment and expenditure levels show positive associations with adoption, potentially reflecting

fiscal stress or demand for new revenue sources. Conversely, higher state revenue correlates negatively with adoption, consistent with states having less need for new tax instruments when existing sources were adequate. The manufacturing and mercantile share also shows a sizable negative coefficient, though with large standard errors. These patterns align with fiscal capacity explanations from the historical literature, even if the limited statistical power prevents definitive conclusions. Importantly, firm outflows themselves do not predict adoption, providing evidence against reverse causality concerns for the main analysis.

D. EMPLOYERS AS A PROXY FOR FIRMS

As the objective of this paper is to analyze firm relocations, it is important to assess the effectiveness of employers as a representative proxy for firms. Given the historical context of this analysis, it is highly unlikely that an employer would not live in close proximity of their firm. To formally test this claim, I use newly digitized parts general statistics of the Census of Manufactures [1929] that count the number of manufacturing businesses in each state between 1900 and 1930. Together with the number of manufacturing employers from the linked census, this allows for the estimation of

$$\text{\# of Mfg Businesses}_{i,t(b)} = \psi\left(\text{\# of Mfg Employers}_{i,t(b)}\right) + \zeta \text{Total Pop}_{i,t(b)} + \delta_i + \delta_t + \nu_{i,t(b)}, \qquad (1)$$

where δ_i collects state fixed effects and δ_t time fixed effects for the starting year $t(b)$ of decade t.

TABLE D.I: Correlation of Manufacturing Firms and Employers

	# of Mfg Businesses$_{i,t(b)}$		# of Mfg Corporations$_{i,t(b)}$
	(i)	(ii)	(iii)
# of Mfg Employers$_{i,t(b)}$	4.826***	0.706***	0.240***
	0.236	0.150	0.080
Total Pop$_{i,t(b)}$		0.009***	0.008**
		0.003	0.003
State FE, Decade FE	No	Yes	Yes
Observations	144	144	144
Adjusted R^2	0.745	0.994	0.981

Notes: The table reports estimates of the correlation between the number of manufacturing businesses and manufacturing employers at the state level. Manufacturing employers are identified from linked census records. Total population controls for state size. Standard errors are clustered by state in specifications with fixed effects. The Census of Manufactures includes only establishments with product values exceeding \$500 (1900–1921) or \$5,000 (1921–1930), excluding the smallest businesses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The Census of Manufactures [1929] includes manufacturing enterprises with product values larger than $500 until 1921 and larger than $5,000 afterward, which excludes a fraction of small businesses. Hence, the expected correlation coefficient satisfies $0 < \widehat{\psi} < 1$. Table D.I presents the estimation results for equation (1). After controlling for state-specific total population and state and decade fixed effects, I find that each manufacturing employer in state i at the onset of decade $t(b)$ is associated with around 0.71 manufacturing businesses. Given that the Census of Manufactures [1929] excludes small businesses, this coefficient is consistent with a close relationship between employers and firms, where some employers operate below the census reporting threshold. The coefficient remains positive and significant when restricting to the corporate subset of businesses. These results demonstrate a strong correlation between the number of employers and the number of businesses, validating the use of employer counts from linked census waves as a suitable proxy for firm counts.

E. MEDIATING CORPORATE ACTIVITY

Liu [2014] shows that corporate activity decreased with the introduction of state-level income taxation, driven primarily by effects on organizational form choice and entry decisions. I explore a complementary mechanism through firm relocations. Digitizing parts of the Census of Manufactures' general statistics allows collecting state-level corporate shares of manufacturing enterprises. Matching this with total state-level manufacturing outflows from the linked individual census enables a mediation analysis to quantify the share of the tax effect on corporate activity that operates through firm relocations [Baron and Kenny, 1986].[15] Following the standard mediation framework, I first estimate the total effect of tax adoption on corporate shares

$$\text{Corporate Share}_{i,t} = \beta z_{i,t} + X_{i,t} + \delta_i + \delta_t + \varepsilon_{i,t}, \tag{1}$$

where $z_{i,t}$ is an adoption indicator for state i during decade t. The corporate share is measured at the end of the decade to capture the realized outcome. Controls $X_{i,t}$ include total population (full count) and log state expenditures, both measured at period start. Further, δ_i collects state fixed effects and δ_t time fixed effects for the starting year $t(b)$ of decade t In equation (1), $\widehat{\beta}$ captures the total estimated effect of tax adoption on corporate activity. Next, I estimate the mediator equation linking tax adoption

[15] Since total outflows include both incorporated and non-incorporated firms, this analysis provides a lower bound on the mediation effect through corporate relocations specifically.

to firm outflows

$$\log(\text{Total Outflow}_{i,t}) = \theta z_{i,t} + X_{i,t} + \delta_i + \delta_t + \tilde{\varepsilon}_{i,t}. \tag{2}$$

After computing the effects of the adoptions on manufacturing firm outflows, I estimate the direct effect of the outflows on corporate activity, while controlling for the tax

$$\text{Corporate Share}_{i,t} = \breve{\beta} z_{i,t} + \rho \log(\text{Total Outflow}_{i,t}) + X_{i,t} + \delta_i + \delta_t + \breve{\varepsilon}_{i,t}. \tag{3}$$

Utilizing estimates from equations (1) to (3) allows to compute the mediation share $F = (\theta \times \rho)/\beta = \left(\beta - \breve{\beta}\right)/\beta$. Results of the exercise can be found in Table E.I.

TABLE E.I: Estimates for Corporate Activity and Firm Relocation

	Corporate Share$_{i,t}$	log(Total Outflow$_{i,t}$)
	(i)	(ii)
Equation (1): Total Effect		
$z_{i,t} : \beta$	-0.034^{***}	
	(0.011)	
Adjusted R^2	0.903	
Equation (2): Mediator		
$z_{i,t} : \theta$		0.109^{**}
		(0.054)
Adjusted R^2		0.971
Equation (3): Direct Effect		
$z_{i,t}$	-0.031^{***}	
	(0.010)	
log(Total Outflow$_{i,t}$) : ρ	-0.027^{*}	
	(0.016)	
Adjusted R^2	0.908	
Controls	Yes	Yes
State FE, Decade FE	Yes	Yes
Observations	144	144

Notes: The table presents estimates for a mediation analysis following Baron and Kenny [1986]. Equation (1) estimates the total effect of tax adoption on corporate share (β). Equation (2) estimates the effect of tax adoption on manufacturing outflows (θ). Equation (3) estimates the direct effect of tax adoption on corporate share controlling for outflows ($\breve{\beta}$), and the effect of outflows on corporate share (ρ). The mediation share is $F = (\theta \times \rho)/\beta \approx 0.09$. Corporate share data are from the Census of Manufactures' general statistics, outflow data is from linked individual census records. Controls include total population and log state expenditures at period start. All specifications include state and census link fixed effects. Standard errors clustered by state. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Consistent with Liu [2014], corporate share decreases with tax adoption. Tax adoption also significantly increases outflows by approximately $(\exp(0.109) - 1) \times 100 \approx 11.5\%$, similar in magnitude to the bilateral effects in the main analysis. The direct effect of outflows on corporate share is negative and marginally significant, indicating that states experiencing larger outflows see greater declines in corporate activity. The mediation share is estimated at $\widehat{F} \approx 0.09$, indicating that roughly 9% of the decline in corporate activity can be attributed to firm relocations. While other mechanisms documented by Liu [2014] explain the majority of the aggregate decline, the relocation channel represents a non-trivial and economically meaningful component. Importantly, this 9% captures only the aggregate effect on state-level corporate shares and since total outflows include non-incorporated firms, this estimate provides a lower bound on the true mediation effect operating through corporate relocations. The analysis thus confirms that tax-induced firm relocations constitute an important margin of adjustment that contributed meaningfully to the spatial reorganization of American businesses in the early 20th century.

F. APPROXIMATING FIRM EXITS

The empirical analysis tracks employers whose employment status is established at the onset of each decade, focusing on whether corporate income taxation induces firms to relocate across state borders. A natural complementary question is whether tax adoption instead caused firms to close altogether. While direct data on firm closures is unavailable, I approximate this by examining employers who transition to wage-earner status between census waves. Therefore this approach assumes when an employer becomes a wage-earner, their firm has ceased operations.[16] Using the complete count linked census, I count the number of employers in state i at time $t(b)$ who became wage-earners by $t(e)$, separately for those who relocated across state borders and those who remained in state i. This yields the following specification

$$\text{\# of Switchers}_{i,r,t} = \beta_1 z_{i,t} + \beta_2 \texttt{Relocate}_r + \beta_3 (z_{i,t} \times \texttt{Relocate}_r) + \delta_i + \delta_t + \varepsilon_{i,r,t}, \tag{1}$$

where $\text{Switchers}_{i,r,t}$ counts employers who lived in state i at $t(b)$ and became wage-earners by $t(e)$. The subscript $r \in \{0, 1\}$ indexes whether these switchers remained in state i ($r = 0$) or relocated to a

[16] This assumption likely overstates closures, as some former employers may have become managers or executives in other firms. However, in the early 20th century context with limited managerial hierarchies, most employers operated their own businesses, making this a reasonable first approximation.

different state $j \neq i$ ($r = 1$). The binary indicator $\texttt{Relocate}_r$ equals 1 for observations where $r = 1$. The corporate tax adoption indicator $z_{i,t}$ equals 1 if state i adopts during decade t. The interaction term captures whether tax adoption differentially affects the employer-to-wage-earner transition for movers versus stayers. I further include state fixed effects δ_i and decade fixed effects δ_t. I exclude observations from 1900-1910 because for this census link, employment status at $t(b)$ must be approximated using status at $t(e)$, which would bias the dependent variable. The sample comprises 192 observations across 48 states and two census links (1910-1920, 1920-1930), with each state-period contributing up to two observations (one for stayers, one for movers).

TABLE F.I: Tax Adoption and Employer-to-Wage-Earner Transitions

	# of Switchers$_{i,r,t}$	ln(# of Switchers$_{i,r,t}$)
$z_{i,t}$	-0.76	-0.14
	(493.81)	(0.13)
$\texttt{Relocate}_r$	-1560.40^{***}	-0.53^{***}
	(371.71)	(0.09)
$z_{i,t} \times \texttt{Relocate}_r$	-284.66	0.03
	(705.50)	(0.15)
State FE, Decade FE	Yes	Yes
Observations	192	192
Adjusted R^2	0.7746	0.9214

Notes: The table presents the relationship between corporate tax adoptions and the firm closure proxy. The dependent variable is the number (column 1) or log number (column 2) of individuals who were employers in state i at the beginning of decade t and became wage-earners by the end of the decade. Each state-decade contributes up to two observations: one for individuals who remained in state i (mean switchers = 3,220) and one for those who relocated to a different state (mean switchers = 1,613). $\texttt{Relocate}_r$ is an indicator for the relocated subsample. The sample excludes the 1900-1910 link due to data limitations and includes 192 observations across 48 states and two census links. Standard errors are clustered by state. The specification includes state and decade fixed effects. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The results in Table F.I reveal several key patterns that are consistent across both the level and log specifications. First, the coefficient on $z_{i,t}$ is precisely zero in both specifications, indicating that tax adoption had no detectable effect on the rate at which employers transitioned to wage-earner status. Second, the coefficient on $\texttt{Relocate}_r$ is large, negative, and highly significant. To contextualize this magnitude: on average, states experienced 3,220 employer-to-wage-earner transitions among those who stayed in state, compared to only 1,613 transitions among those who relocated across state borders. The regression coefficient indicates that, after controlling for state and time fixed effects, employers who relocated were substantially less likely to switch occupational status. This strong negative relationship suggests

that interstate migration was associated with maintaining employer status—consistent with employers relocating their ongoing business operations rather than closing them and becoming employees elsewhere. Third, the interaction term, while statistically insignificant, is negative, suggesting that among employers in adopter states, those who relocated were if anything even less likely to become wage-earners. This pattern is consistent with tax-induced relocations representing genuine business moves rather than firm exits. Taken together, these results strongly support the interpretation that observed firm relocations in response to corporate tax adoption represent true relocations of ongoing business operations rather than closures. The substantial difference in transition rates between movers and stayers reinforces that relocation was strongly tied to business continuity. The findings confirm that the mobility responses documented in the main analysis reflect the spatial reallocation of productive economic activity across state borders, not firm exits from the market.

G. BASELINE RESULTS BY DETAILED SECTORS

Once I established the gravity formulation for overall firm relocations, I can define the flow equation for several industries. I define the respective firm level flows in sector s as $y_{i,j,t,s}$.[17] This allows me to re-write equation (9) in a sector-specific notation such that

$$y_{i,j,t,s} = \exp\left[\sum_{s} \beta_s \mathbb{1}_s \tau_{i,j,t} + \boldsymbol{\mathcal{D}} \times \delta_s\right] \xi_{i,j,t,s}, \tag{1}$$

where δ_s denotes a sector fixed effect and $\boldsymbol{\mathcal{D}} = (\delta_{i,j} + \delta_{i\neq j,t} + \delta_{i,t} + \delta_{j,t})$. The sectors are equivalent to the line-up of Table B.II. The results of this estimation are presented in Table G.I.

It is crucial to note that for this particular specification, I do not account for sectoral adopter variation. In other words, I do not account for the fact that some states adopted the corporate income tax only for a subset of sectors. I find strong and significant effects for some manufacturing and mercantile sectors such as agriculture, construction or certain service sectors. Interestingly, non-durable manufacturing is estimated to observe a negative effect of the introduction of corporate income taxes, which highlights the fact that some industries with potentially narrower profit margins, may have been more inclined to limit interstate relocation activities in response to the introduction of state income taxation. Conversely, other sectors may have exhibited greater resilience or found opportunities to capitalize on the tax changes by moving across state borders.

[17] Note that for the sake of comparison, I also include the overall firm flows as a "sector".

TABLE G.I: Baseline stacked regressions by detailed sector panels

	Model (1)	Model (2)	Model (3)
Panel A: Full Panel			
$\tau_{i,j,t,s}$	0.378***	0.431***	0.122***
	(0.091)	(0.029)	(0.023)
Panel B: Sector-specific effects (detailed)			
Agriculture	0.240**	0.380***	0.168***
	(0.099)	(0.032)	(0.026)
Construction	0.518***	0.636***	0.174***
	(0.108)	(0.043)	(0.044)
Manufacturing (Dur.)	0.296***	0.116***	0.007
	(0.153)	(0.053)	(0.055)
Manufacturing (Non-Dur.)	0.330**	0.093*	−0.152***
	(0.149)	(0.049)	(0.054)
Mining	0.306**	0.326**	0.144
	(0.154)	(0.134)	(0.129)
Wholesale	0.509***	0.356***	0.036
	(0.112)	(0.029)	(0.030)
Business Services	0.414**	0.010	−0.033
	(0.182)	(0.158)	(0.175)
Entertainment	0.506***	0.333**	0.208
	(0.166)	(0.0143)	(0.155)
Personal Services	0.644***	0.530***	0.148***
	(0.121)	(0.049)	(0.052)
Repair Services	0.670***	0.594***	0.203**
	(0.111)	(0.091)	(0.092)
Professional Services	0.453***	0.370***	0.040
	(0.140)	(0.072)	(0.076)
Finance	0.426***	0.247***	−0.045
	(0.143)	(0.088)	(0.089)
Telecommunication	0.461	0.551	0.590***
	(0.342)	(0.379)	(0.382)
Transportation	0.381***	0.248***	0.005
	(0.112)	(0.067)	(0.072)
Utilities	0.364	0.515***	0.263
	(0.270)	(0.354)	(0.0374)
State-Decade FEs	yes	yes	yes
State-Pair FE	no	yes	yes
Decade-Interstate FE (*Bergstrand et al.* [*2015*])	no	no	yes
Observations	110,592	54,401	54,397
McFadden Pseudo R^2	0.323	0.994	0.995

Notes: The table presents PPML estimation results by sector, following the specification described in Section VI. Models (1)–(3) correspond to those in Table IV, with Model (3) including the full set of bilateral, origin–decade, destination–decade, and decade-interstate fixed effects. Standard errors, clustered by state pair, appear in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Statistically separated observations and singletons are dropped following Clarkson and Jennrich [1991] and Correia [2015], respectively.

For most sectors, I find no statistically significant effects. However, many point estimate are estimated to be very large, such as for the entertainment industry (e.g. circuses such as the Ringling Bros. in Wisconsin) or telecommunication. The insignificance comes from massive confidence bands due to few observations in these groups. The caveat of analyzing narrowly defined sectors is that many state pairs observe no firm flows for the entire observation period. This leads to singleton observations, i.e.

constant flows across time. As these observations need to be excluded (Correia [2015]), many sectors are left a very small sample size.

H. GEOGRAPHIC ANALYSIS

The geographic relation of states is another interesting dimension of interstate firm relocations. The main results control for regional proximity by employing a state-pair fixed effect. Nonetheless, it is worthwhile to dissect the estimated effects for bordering and non-bordering states. This can be achieved by interacting the policy variable $\tau_{S,i,j,t}$ with the identifier $\texttt{Border}_{i,j}$.

TABLE H.I: Overall Results Decomposed by Bordering Status

	Bordering	Not Bordering
	(i)	(ii)
Part 1: Descriptive Statistics		
Average Flow	241.7	35.0
Standard Deviation	267.2	85.9
Number of Pairs	621	6,147
Part 2: Regression Analysis Results		
$\tau_{i,j,t}$		0.195***
		(0.029)
$\tau_{i,j,t} \times \texttt{Border}_{i,j}$	−0.153***	
	(0.035)	
Average Marginal Effects	4.23%	21.53%
Average Absolute Effects	10.45	7.54
Observations	6,765	
McFadden Pseudo R^2	0.997	

Notes: The table reports PPML estimates from a single regression interacting the policy variable with a border indicator. Specification includes state-pair, state-decade, and interstate-decade fixed effects. Standard errors, clustered by state pair, appear in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Descriptive statistics in the upper panel exclude same-state pairs ($i = j$). Statistically separated observations and singletons are dropped following Clarkson and Jennrich [1991] and Correia [2015], respectively.

Table H.I summarizes the estimation results for the border interaction. The negative sign on the border interaction can be rationalized by two factors. First, Table H.I shows that the number of firms moving among bordering states is significantly larger on average. A larger coefficient for bordering states would hence mean an exponentially larger total effect. Second, the political dimension should be considered. As argued in Berry and Berry [1992], state politicians may have used tax introduction in neighboring states as an opportunity to adopt their own tax. By foreseeing this, corporations could have avoided relocating to bordering states and instead opted for states further away.

TABLE H.II: Sector-Specific Results Decomposed by Bordering Status

	Bordering	Not Bordering
	(i)	(ii)
Panel B.1: Manufacturing and Mercantile Interaction		
$\tau_{i,j,t}$		0.188***
		(0.029)
$\tau_{i,j,t} \times \texttt{Border}_{i,j}$	−0.140***	
	(0.036)	
Average Flow for $i \neq j$	222.5	31.7
Standard Deviation for $i \neq j$	242.9	78.5
Average Marginal Effects	4.93%	20.68%
Average Absolute Effects	10.96	6.56
Panel B.2: Services Interaction		
$\tau_{i,j,t}$		0.244***
		(0.046)
$\tau_{i,j,t} \times \texttt{Border}_{i,j}$	−0.237***	
	(0.049)	
Average Flow for $i \neq j$	11.9	2.0
Standard Deviation for $i \neq j$	22.6	6.2
Average Marginal Effects	0.70%	27.63%
Average Absolute Effects	0.08	0.56
Panel B.3: Financial Interaction		
$\tau_{i,j,t}$		0.112
		(0.108)
$\tau_{i,j,t} \times \texttt{Border}_{i,j}$	−0.171	
	(0.124)	
Average Flow for $i \neq j$	1.8	0.4
Standard Deviation for $i \neq j$	4.6	1.4
Average Marginal Effects	−5.72%	11.87%
Average Absolute Effects	−0.10	0.04
Panel B.4: Transportation and Other Public Utilities Interaction		
$\tau_{i,j,t}$		0.220*
		(0.0116)
$\tau_{i,j,t} \times \texttt{Border}_{i,j}$	−0.147	
	(0.155)	
Average Flow for $i \neq j$	3.3	0.5
Standard Deviation for $i \neq j$	5.7	1.5
Average Marginal Effects	7.48%	24.56%
Average Absolute Effects	0.25	0.13
Number of Pairs for $i \neq j$	621	6,147
Observations	24,949	
McFadden Pseudo R^2	0.996	

Notes: The table reports sector-specific PPML estimates from regressions interacting the policy variable with a border indicator. Specification as in Table H.I. Standard errors, clustered by state pair, appear in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

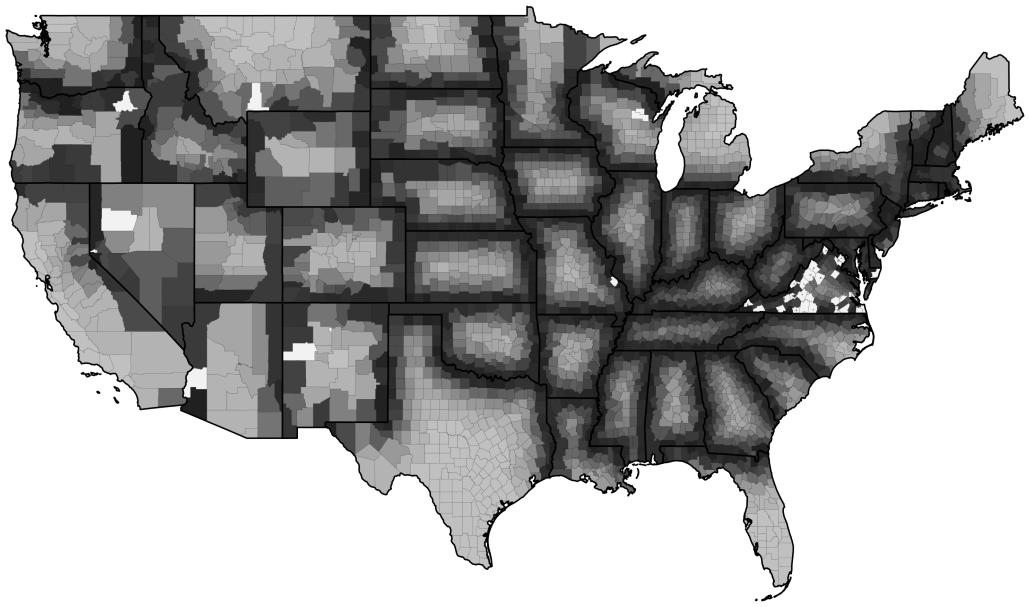
While the average marginal effect is significantly smaller for bordering states, suggesting that the tax adoption effects on firm relocations do not increase exponentially with geographic proximity, average

absolute effects are larger for border pairs. This pattern is replicated by the manufacturing and mercantile, as well as the public utility sector (see Table H.II). Similar to the overall effects, the financial sector does not observe any significant relocation effects.

H.A. County-level

With the relevance of state borders, it is conceivable that firms located closer to the center of a state have higher (idiosyncratic) costs of relocating. To explore this, I utilize county-level information to estimate the adoption effects interacted with firms' geographic distance to the closest border in the origin state. I use data provided by Holmes [1998] on each county $w(i)$'s distance to every of state i's neighboring states $k \in \mathcal{B}_i$. The measure of distance is given by two coordinates. First, the centroid of the county, which defines the arithmetic mean position as the center. Second, a measure for the point at each of state i's neighboring state k's border that minimizes the distance to the centroid. The length of the line between these two points is defined as $\text{Dist}(w(i;k))$. Each county $w(i)$ therefore has a set of distances equal to the number of states in $\mathcal{B}_i$. The measure of distance to the closest state border is then defined as $\text{MD}_{w(i)} = \min_{k \in \mathcal{B}_i}\{\text{Dist}(w(i;k))\}$. To validate the distance measure, I plot Figure H.I.

FIGURE H.I: Distance measure to the closest state border



Notes: Darker shades indicate shorter distances to the nearest state border. Distance is measured from the county centroid to the closest point on any neighboring state's border. Distances are provided by Holmes [1998].

Reassuringly, the map depicts closer distances in darker shades, with a clear pattern of darker areas along state borders. It's important to note that Western states generally have larger counties, which is

reflected in the map. Consequently, some border counties appear lighter shaded due to their size. This nuance is crucial, as the measure accounts for county size, which is essential for the main objective of the county analysis: estimating tax adoption effects with respect to distance. In larger counties, firms may be further from the border, even when their county is situated at the state's edge.

The dyadic structure captures county-pair firm flows $y_{w(i),w(j),t}$ in decade t, where $i \in \mathcal{J}$ with $w(i) \in \mathcal{W}_i$ defining the set of all counties in state i. Note that, as shown in Table H.III, with an average of 130 counties per state, the number of potential flow pairs is substantially higher. The objective of the exercise is to estimate the policy effects as a function of counties', and therefore firms', distance to the border. To do so, I interact the adoption policy with the minimum of the border distance measure. As this measure is constant across county pairs, I use state-level fixed effects.[18] The "quasi-gravity" model can be written as

$$y_{w(i),w(j),t} = \exp\left(\vartheta_1 \tau_{i,j,t} + \vartheta_2 \text{MD}_{w(i)} + \vartheta_3(\tau_{i,j,t} \times \text{MD}_{w(i)}) + \sum_{w(i),w(j)} \chi_{w(k)} \ln\left(f_{w(k),t}\right) + \boldsymbol{\mathcal{D}}\right) \varsigma_{w(i),w(j),t}, \tag{1}$$

where $f_{w(k),t}$ is the stock of firms in county $w(k)$ and $\boldsymbol{\mathcal{D}} = (\delta_{i,j} + \delta_{i \neq j,t} + \delta_{i,t} + \delta_{j,t})$. Summary statistics and estimation results are presented in Table H.III. Note that the point estimates for the county-to-county flows are larger compared to the state-level baseline results in Table IV.

As this iteration of the gravity model assumes that county-level push and pull factors are, at least partially, controlled for by the state-level fixed effects, the variation in the regressors explain a smaller fraction of the flow variation, illustrated by a lower R^2. The structure may also lead to a slight upward bias due to omitted variables on the county-decade level, which helps explain the larger point estimates. Furthermore, the mean dependent variable is drastically lower than in the main analysis and the distribution is substantially right-skewed (see Figure H.IIb).

The equation (1) implies that the effect of policy adoption on firm relocations varies systematically with distance to the nearest state border. Because tax adoption is a binary treatment, the relevant object is the discrete proportional change in expected county-pair firm flows. Evaluated relative to the

[18] This approach diverges from the standard gravity framework by assuming constant policy effects within a state. In essence, it posits that counties within a state are reasonably comparable, differing primarily in their geographic location.

TABLE H.III: Overall Results for the County-Level Analysis

Panel A: Descriptive Statistics						
	Mean	Std. Dev.	Min	P25	P75	Max
# of counties/state	130.118	70.354	3	82.0	161.0	254
MD_{w_i}	49.146	51.762	0	14.1	34.8	396
$y_{w(i),w(j),t}$	0.078	5.508	0	0.0	0.0	7,737

Panel B: Model Estimates				
	Baseline	Dist. Int.	Baseline + Stocks	Dist. Int. + Stocks
	(i)	(ii)	(iii)	(iv)
$\tau_{i,j,t}$	0.432***	0.488***	0.223***	0.279***
	(0.029)	(0.067)	(0.024)	(0.037)
$\text{MD}_{w(i)}$		−0.001*		0.0001
		(0.000)		(0.000)
$\tau_{i,j,t} \times \text{MD}_{w(i)}$		−0.002**		−0.0014**
		(0.001)		(0.001)
$\ln f_{i,t}$			0.899***	0.899***
			(0.012)	(0.012)
$\ln f_{j,t}$			0.871***	0.871***
			(0.009)	(0.009)
Observations	28,899,567	28,266,711	28,899,567	28,266,711
Pseudo R^2	0.4378	0.4384	0.5686	0.5685

Notes: The table reports PPML estimates of county-pair firm relocation flows based on equation (1). Descriptive statistics are reported in the upper panel. All specifications include state-pair, state-decade, and interstate-decade fixed effects. Columns (3) and (4) additionally control for the logarithm of origin- and destination-county firm stocks. Standard errors, clustered at the state-pair level, are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Statistically separated observations and singletons are dropped following Clarkson and Jennrich [1991] and Correia [2015], respectively.

untreated baseline at zero distance, this effect is given by

$$\begin{aligned} \Delta\text{MD}_{w(i)} &= \frac{\mathbb{E}[y|\tau = 1, \text{MD} > 0] - \mathbb{E}[y|\tau = 0, \text{MD} = 0]}{\mathbb{E}[y|\tau = 0, \text{MD} = 0]} \\ &= \exp\left(\vartheta_1 + (\vartheta_2 + \vartheta_3)\text{MD}_{w(i)}\right) - 1. \end{aligned} \tag{2}$$

The zero-crossing is defined as the point at which tax adoption no longer affects expected firm relocations. Since the exponential function is strictly increasing, the zero-crossing satisfies

$$\vartheta_1 + (\vartheta_2 + \vartheta_3)\text{MD}_{w(i)} = 0, \tag{3}$$

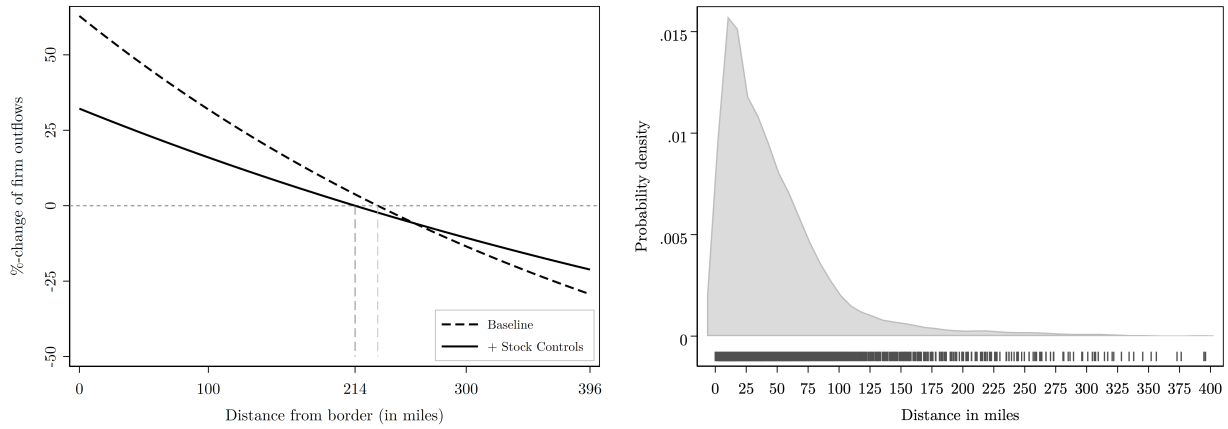
which yields

$$\text{MD}^* = -\frac{\vartheta_1}{\vartheta_2 + \vartheta_3}. \tag{4}$$

This formulation relies on the maintained assumption that $\vartheta_2 + \vartheta_3 < 0$, implying that the adoption effect

attenuates with distance from the state border. Empirically, I find a nuanced decline in the adoption effect as distance increases. Figure H.IIa plots the implied proportional effects for $\text{MD}_{w(i)} \in [0, 400]$. The mean distance to the closest state border is approximately 50 miles, and 75% of counties lie within 35 miles of a border. Consequently, the estimated effects remain positive for the vast majority of counties, including those located far from the border. This pattern is economically intuitive: firms located near state borders face substantially lower relocation costs and therefore respond more strongly to tax adoption. While firms situated in a state's interior may still relocate if policy-induced costs are sufficiently high, such responses are considerably less likely. The estimated zero-crossing lies at approximately 214 miles, well above the 95th percentile of the distance distribution, and is depicted graphically in Figure H.IIa as the intersection of the estimated effect curves.

FIGURE H.II: County-level effects and density by distance to the closest state border



(**a**) Adoption effects with respect to $\text{MD}_{w(i)}$ measure

(**b**) Density of the $\text{MD}_{w(i)}$ measure

Notes: Panel (a) plots the estimated effect $(\exp(\widehat{\vartheta}_1 + (\widehat{\vartheta}_2 + \widehat{\vartheta}_3)\text{MD}_{w(i)}) - 1) \times 100$ as a function of distance to the nearest state border. The dashed line marks zero effect; the intersection indicates the zero-crossing at approximately 231 miles. Panel (b) shows the density of the distance measure across counties.

H.B. Manufacturing and Mercantile Businesses: Where did they move?

So far, this analysis has been mostly agnostic about the characteristics of respective destination states. A relevant state-level feature may be urbanization and industrialization, which can be proxied by population density [Acemoglu et al., 2002]. The distribution of population within the U.S. has always been heterogeneous. Historically, this pattern is driven by the fact that the founding states of the U.S. were located in the East. Unsurprisingly, these states are associated with a larger population density and degree of urbanization, especially in the early 20$^{\text{th}}$ century. The goal of this analysis is to test whether or not firms are more sensitive to tax changes in less urban states. The are two reasons for the focuses

on the manufacturing and mercantile industry. First, it is, by far, the largest sector in the sample and may observe large intrasectoral agglomeration. Second, all adopter states imposed the tax on these businesses and there is an exploitable variation within this aggregated sector: agricultural and non-agricultural.

I define a state i to be *urban* if the average population density is larger than 10 individuals per square mile.[19] This allows me to identify 11 states as urban, all of which are located in the North-East, which lends support for this rudimentary indicator. For the empirical analysis, I follow Beverelli et al. [2018]. First, I denote U as the set of urban states. Then I define a new group of policy variables. The impact of the tax adoption on relocations from rural to urban states is defined as $\tau_{r,u,t} = z_{i,t}\mathbb{1}(i \notin U \wedge j \in U)$. Note that, by construction, $i \neq j$.[20] Additionally, I estimate rural-to-rural effect with $\tau_{r,r,t} = z_{i,t}\mathbb{1}(i \notin U \wedge j \notin U \wedge i \neq j)$. In order to estimate the effects directly, rather than as deviations from the main effect, I define $\tau^{all}_{i,j,t} = \tau_{i,j,t} - (\tau_{r,u,t} + \tau_{u,r,t} + \tau_{r,r,t})$. The overall results in column (ii) of Table H.IV show the largest effects for relocations from rural to urban states.

TABLE H.IV: Estimation results decomposed by urban versus rural relocation flows

	Baseline	Overall	All M&M	Agriculture	Non-Agriculture
	(i)	(ii)	(iii)	(iv)	(v)
$\tau_{i,j,t}$	0.122***				
	(0.023)				
$\tau^{all} i,j,t$		0.096**	0.116***	0.279***	−0.188***
		(0.040)	(0.041)	(0.049)	(0.031)
$\tau_{r,u,t}$		0.335***	0.330***	0.362***	0.165***
		(0.042)	(0.043)	(0.048)	(0.052)
$\tau_{u,r,t}$		0.308***	0.343***	0.672***	−0.083*
		(0.046)	(0.048)	(0.063)	(0.49)
$\tau_{r,r,t}$		0.046	0.040	0.077**	0.135***
		(0.029)	(0.029)	(0.030)	(0.334)
Observations	6.765	6, 765	6, 738	6, 492	6, 273
McFadden Pseudo R^2	0.997	0.997	0.997	0.996	0.993

Notes: The table reports PPML estimates for relocation flows decomposed by origin-destination urbanization. A state is classified as *urban* if average population density exceeds 10 persons per square mile. All specifications include state-pair, state-decade, and interstate-decade fixed effects. Standard errors, clustered by state pair, appear in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Statistically separated observations and singletons are dropped following Clarkson and Jennrich [1991] and Correia [2015], respectively.

A potential explanation is that firms who were already contemplating to leave their rural state for a more urban destination had an additional incentive to do so. Similar effects can be observed for firms

[19] This definition can be modified for robustness and should not be interpreted as a hard cut-off. Furthermore, the measure is very rudimentary and can be easily complicated by adding conditions.

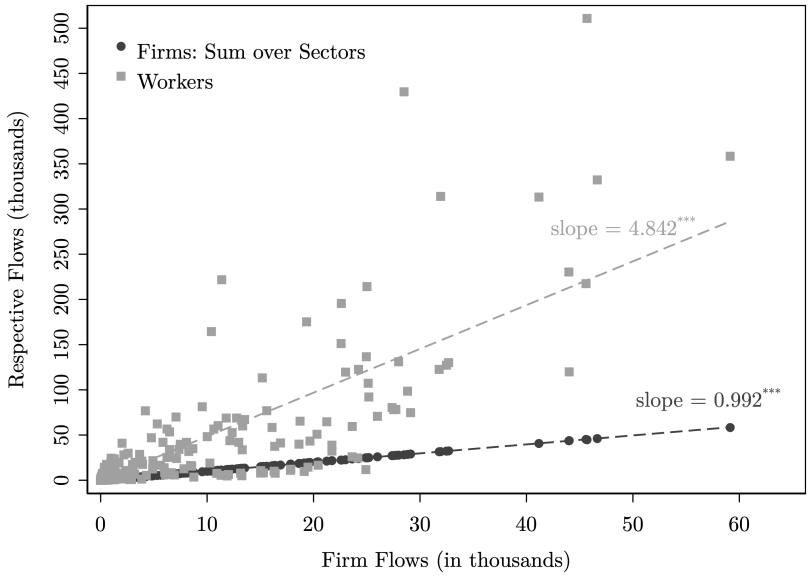
[20] The mirrored version of this policy is for relocations from urban to rural states $\tau_{u,r,t} = z_{i,t}\mathbb{1}(i \in U \wedge j \notin U)$.

moving from urban to rural states. Note that there is no significant effect for relocations among rural states. This further lends support to the hypothesis that the tax introduction is one of many factors in the relocation decision process. It further provides suggest support to some level of agglomeration. To further investigate this I turn to manufacturing and mercantile sectors. Here, I observe opposing effects for agricultural and non-agricultural businesses. While agricultural businesses see strong relocation effects from urban to rural states, non-agricultural manufacturing and mercantile firms observe strong and significant effects for relocations from rural to urban states. Both effects can be rationalized by agglomeration and the characteristics of these businesses, as they will naturally self select into areas with required infrastructure. That is, non-agricultural enterprises tend to locate in urban states and vice versa for agricultural businesses. As illustrated in Smutny and Wandschneider [2024], the early 20^{th} century witnessed a rise in interstate migration following the implementation of personal income taxes. In the spirit of Borjas [1987], it can be shown that younger individuals with higher incomes are more susceptible to experiencing a pronounced effect from the adoption of taxes. Consequently, it can be inferred that these migration patterns, to some extent, correlate with changes in local labor market compositions, potentially influencing firms' relocation decisions. This is further supported by a high correlation between firm and worker flows (see Figure I.I).

I. RELOCATION COMPLEMENTARITIES

So far, the analysis has been agnostic about the impact of relocation complementarities across firms. Brülhart et al. [2012] have shown that agglomeration effects can lead firms' location choices to be less sensitive to tax rate differentials. Similarities may be assumed for tax adoptions. Firms may react to other firms moving to destination j, caused by agglomeration effects. This could happen through network effect of firms that create an additional pull factor. Partially, these effects are being controlled for by the state-pair fixed effects. Unbiasedness however requires the assumption that the deterministic profit component associated with destination j is constant over alternative origins, and hence ignores the existence of potential agglomeration economies. Additionally, changes in the labor market composition, stemming from large-scale migration waves, can influence firms' decisions on where to locate.

FIGURE I.I: Estimated β_s by sectors



Notes: Dark dots and fitted line show the correlation between linked firm counts and total sector employment. Light dots and fitted line show the association between worker and firm flows; on average, approximately five workers relocate with each firm. These correlations are descriptive and should not be interpreted causally.

Furthermore, as previously discussed, a significant number of states implemented both income taxes during the same period. Thus, it is plausible that some of the previously identified effects might be, at least partially, attributable to individual migration responses to the introduction of personal income taxes rather than a direct consequence of corporate tax adoption. To address challenges of both the complementarities and the change in migration flows, I adapt the relocation profit equation (2) such that

$$\widetilde{\Lambda}_{h,i,j,t} = \Lambda_{h,i,j,t} + \kappa_1 f_{j,i(t-1),t} + \kappa_2 \texttt{Workers}_{i,j,t-1}, \tag{1}$$

where $f_{j,i(t-1),t}$ is the number of firms in state j in t that came from state i during the previous decade $t-1$.[21] The variable $\texttt{Workers}_{i,j,t-1}$ counts all working individuals that had moved from i to j in the previous decade. Augmenting the formulation of the pull factors with these variables yields

$$\texttt{Pull}_{i,j,t} = \texttt{Pull}_{j,t} \times \exp(\kappa_1 f_{j,i(t-1),t} + \kappa_2 \texttt{Workers}_{i,j,t-1}), \tag{2}$$

which are now origin-specific. The gravity equation follows identical to the baseline specification. The

[21] As the complementarity channel requires prior collaboration between firms, I assume that only recent (last decade) relocations from i to j influence subsequent firm movements.

stock $f_{j,i(t-1),t}$ can be identified in various ways. The most obvious is to simply use the observed, lagged firm flows since $f_{j,i(t-1),t} \equiv y_{i,j,t-1}$.

TABLE I.I: Regressions including different bilateral controls for relocation complementarities

		Type of bilateral control			
	Model (5)	Lag Workers	Lag Firms	Interaction	Fitted Values
Panel A: Full Panel					
$\tau_{i,j,t}$	0.077***	0.107***	0.129***	0.197***	0.113***
	(0.024)	(0.022)	(0.021)	(0.032)	(0.022)
$\ln(y^{work}_{i,j,t})$		0.170***	0.075***	0.078***	0.170***
		(0.015)	(0.022)	(0.022)	(0.015)
$\ln(y_{i,j,t-1})$			0.102***	0.096***	
			(0.017)	(0.016)	
$\ln(y_{i,j,t-1}) \times \tau_{i,j,t-1}$				0.042***	
				(0.013)	
$\tau_{i,j,t-1}$				−0.095	
				(0.084)	
$\ln(\widehat{y}_{i,j,t-1})$					0.111**
					(0.052)
Panel B: Sector-specific effects ($\tau_{i,j,t}$ only)					
Manufacturing and Mercantile	0.072***	0.110***	0.130***	0.193***	0.112***
	(0.024)	(0.022)	(0.020)	(0.033)	(0.022)
Services	0.122***	0.125***	0.046	0.119**	0.137***
	(0.034)	(0.033)	(0.036)	(0.055)	(0.035)
Financial Sector	−0.055	−0.017	−0.094*	0.105	0.040
	(0.054)	(0.053)	(0.054)	(0.076)	(0.061)
Transport. and Other Pub. Util.	0.147***	0.092*	0.023	0.076	0.082
	(0.052)	(0.050)	(0.048)	(0.063)	(0.060)
Observations	23,040	23,040	23,040	23,040	16,546
McFadden Pseudo R^2	0.997	0.997	0.997	0.997	0.997

Notes: The table reports PPML estimates controlling for relocation complementarities via lagged firm and worker flows. The first decade (1900s) is excluded from all specifications due to data availability. All specifications include state-pair, state-decade, and interstate-decade fixed effects. Standard errors, clustered by state pair, appear in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Statistically separated observations and singletons are dropped following Clarkson and Jennrich [1991] and Correia [2015], respectively.

However, several potential challenges with this identification may arise. First, it might not entirely capture the intended scope of the complementarities. Specifically, whether firms moved due to the tax and then potentially influenced other firms to move too. In order to better capture this aspect, I pursue two alternative strategies. The first interacts the lagged firm flows with a lagged policy variable, which captures the effects on relocating firms conditional on the adoption of the tax in the origin state. The second strategy involves the fitted values $\widehat{y}_{i,j,t-1}$ from the estimation of equation (10).

All results are presented in Table I.I. First, note that regressions including lagged bilateral controls regressions have a smaller sample size due to the exclusion of the 1900s.[22] In order to have a comparable

[22] For the 1890 census, there is no information of employer status.

point estimate for the baseline strategy, I repeat the estimation of equation (10) excluding the first census link. Strikingly, when comparing the point estimates across specifications, I find that the lack of controls for relocation complementarities leads to a noticeable and robust downward bias. This is surprising, as the potential omitted variable bias from the omission of relocation complementarities could be assumed to increase the point estimate. The opposite seems to be the case. Further, looking at the sectoral results, effects for the public utility sector become insignificant. This could mean that for these businesses, firm networks play a crucial role in the relocation decision process. Other sectoral findings are in line with the baseline insights. Since including bilateral complementarity controls requires excluding one-third of the observations, I interpret the results as evidence that Model (5) provides a conservative estimation and proceed without incorporating these additional controls.

TABLE I.II: Regressions including different sectoral fixed effect interactions

	Sectoral specification			
	Baseline	Sector-State FE	Adopter Variation	Full FE Interaction
$\tau_{i,j,t}$	0.122***	0.120***		0.119***
	(0.023)	(0.023)		(0.023)
$\tau_{i,j,t} \times w_{i,S}$			0.111***	
			(0.023)	
Average Marginal Effect	12.93%	12.75%	11.75%	12.68%
State-Decade FEs	yes	no	no	no
State-Pair FEs, Interstate-Decade FEs	yes	yes	yes	no
Sector-State-Decade FEs	no	yes	yes	yes
Sector-State-Pair FEs, Sector-Interstate-Decade FEs	no	no	no	yes
Observations	27,060	27,060	26,750	18,184
McFadden Pseudo R^2	0.996	0.996	0.996	0.996

Notes: The table reports PPML estimates with varying sector-specific fixed effect interactions to control for agglomeration economies. The first decade is excluded. Standard errors, clustered by state pair, appear in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Statistically separated observations and singletons are dropped following Clarkson and Jennrich [1991] and Correia [2015], respectively.

Another aspect of agglomeration economies does not come from the complementarities of origin-specific firms moving to j but rather the complementarities with firms that are already established at j. This should be controlled for by the destination-decade FE. To test if that holds true, even for sectoral variation, I introduce $\mathcal{S} = S \backslash \{\textit{Full Panel}\}$ as an index of all aggregated sectors. This allows to control for sector-specif agglomeration in the baseline strategy. Note that, similar to the specification in equation (10), the index adds an additional dimension to the panel, where $y_{i,j,t,\mathcal{S}}$ measures the total flows of firms in sectors $\mathcal{S}$ from state i to state j in decade t. Table I.II presents the results. Since $y_{i,j,t} \approx \sum_{\mathcal{S}} y_{i,j,t,\mathcal{S}}$, the baseline of this specification is virtually the same as the average effect estimated in

Table IV.[23] Furthermore, the use of sector-state-decade fixed effects as well as adding additional sector fixed effect interactions do not change the coefficient in any quantitatively meaningful way. The sector specification further allows to control for adopter variation, as the firm flows are now decomposed by sectors. I find that controlling for adopter variation slightly lowers the point estimate, however within the 95% confidence bounds of the original estimate.

[23]The reason this is not exactly the same is that some employers lack a sector classification in the census data.

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