Taxed Out? How Early 20th Century Regional Tax Adoptions Shaped Interstate Firm Relocations

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Abstract

Are firms relocating in response to the introduction of a corporate income tax? This paper explores the effects of corporate income tax adoptions on interstate firm relocations in the U.S. during the early 20^{th} century. The historical context allows for an investigation of unharmonized and sequential regional tax adoptions between 1910 and 1930, when 16 states introduced taxation on corporate income. Leveraging newly linked employer-level data, together with a structural gravity model, enables a quantification of firm relocation flows caused by tax adoptions. The partial equilibrium analysis reveals a significant increase in average interstate firm flows by 13.02% attributable to these tax adoptions, where disaggregation by sector demonstrates pronounced effects for manufacturing, mercantile, service, and utility businesses. These effects decrease in the distance to the state border. Counterfactuals show that firm outflow in early adopter states would have been significantly lower without the introduction of the income tax, while non-adopters would have observed slightly larger outflows.

Keywords: Corporate Tax Adoption, Business Relocation, Fiscal Decentralization

JEL Codes: H25, H26, N32, R12

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1 Introduction

Do firms strategically relocate their operations as a response to a new tax burden? The empirical investigation of this question is involved and typically met with an array of difficulties, as corporate tax differentials, especially on the extensive margin, are mostly observed across countries. But cross-country firm relocations are limited by many factors, such as legal and financial constraints or cultural and language barriers. Furthermore, international firm relocations often difficult to track and profit shifting due to separate accounting is prevalent. This paper circumvents these challenges by using the unharmonized introduction of state income taxes in the U.S. of the early 20$^{\text{th}}$ century as a natural experiment and tests whether firms relocate as a reaction to newly imposed taxes on their income.

At the tail-end of the Progressive Era, states started to adopt income taxation as a measure to avoid dependency on the often times failing taxation on property and excise. Between 1910 and 1930, 16 of the then 48 U.S. states adopted a business income tax. The adopted tax rates were heterogeneous across adopter states, and not every state introduced the tax on every industry. The implementation of state-level income taxes in the early 20$^{\text{th}}$ century has been shown to increase the out-migration of workers (Pre-WWII: Smutny and Wandschneider [2024], Post-WWII: Cassidy, Dincecco and Troiano [2024]) and inventors Akcigit et al. [2022]. However, the question remains: Are these historical findings applicable to businesses? Is there evidence for firms relocating across state borders to avoid newly introduced corporate income taxes? Wilson [1999] identifies parallels between individual migration and firm relocations based on public policy preferences, suggesting that businesses, like individuals, may respond to similar incentives for relocation. Entities can choose to relocate when in disagreement with specific policies, especially if the tax burdens outweigh the benefits derived from public goods and infrastructure. Thus, a newly introduced tax is associated with additional costs for firms, which potentially translates to a substantial negative effect on profit margins beyond the financial impact of the taxes themselves. In particular, an introduced corporate income tax rate not only imposes new direct costs through the tax burden, but also administrative costs for reporting and paying the tax. Additionally, firms' status quo bias makes the transition from zero to positive taxation particularly salient for policy analysis. Therefore, an investigation of the introduction rather than a change in tax rate differentials is warranted.

To carry out the empirical investigation of the research question at hand, I use linked census data on employers, which serve as a proxy for firms. The linking algorithm is based on Abramitzky et al. [2021], which enables decennially matching of the U.S. population between 1900 and 1930. Significant results from a correlation exercise between the number of manufacturing firms digitized from the census of manufactures and the collected number of manufacturing employers provide support for the use of this proxy. This is the first comprehensive dataset identifying bilateral interstate firm relocation across the entire U.S. in the early 20$^{\text{th}}$ century. With this, I create a unique state-pair firm relocation dataset, capturing 48×48-pairs across three decades. In order to control extensively for all state-level push and pull factors, the empirical investigation employs a full set of fixed effects. Whether the adoption of local income taxes is influencing relocation decisions is the prime objective of this analysis.

However, many other push and pull factors can be important determinants of business relocation. These factors encompass both the characteristics of the current location influencing the decision to move and those of potential new locations. Push factors may include changes in market orientation, technological advancements, space requirements, location costs, accessibility issues, local policies, and labor market dynamics.[1] Conversely, pull factors, mirroring push factors with positive attributes, encompass location quality, improved market orientation, enhanced accessibility, better labor markets, increased space availability, and favorable local policies. Agglomeration benefits, such as local demand and a skilled workforce, draw firms to densely populated areas.[2]

To investigate firm relocation within this context, I utilize an adapted gravity framework. This allows me to model bilateral interstate flows overtime, which enables the incorporation of the entire universe of state-specific location factors and facilitates a causal inference of partial equilibrium effects of tax policy adjustments and firm location choice. To analyze the effects not only for adopter states but also non-adopters, the model then further allows estimating changes of total interstate firm flows in a conditional general equilibrium context. With this, counterfactual analysis enables me to consider how the absence of the state income tax adoptions affect migration flows among all states simultaneously and to impute every state's total firm outflows that would have been realized if the corporate tax had not been introduced.

The estimation results reveals a strong and significant increase in bilateral business relocations by 13.02% resulting from the introduction of the corporate income tax in the origin state. These results withstand a battery of robustness checks including controlling for sector specific agglomeration or the adoption of personal income taxes. As adopter states differed in which industries they introduced the

[1]See Van Wissen [2000]; Holl [2004]; Kronenberg [2013].

[2]See Bodenmann and Axhausen [2012]; Kronenberg [2013]; Weterings and Knoben [2013]; Rossi and Dej [2020].

tax, effects are further estimated by sectors, revealing that the main results are driven by manufacturing and mercantile businesses. Slightly larger effects are found for services (14.08%) and public utility (16.77%) companies. The financial sector, which has historically been subject to a range of regulatory and legal constraints on mobility, observes no significant effect. Bringing the analysis to the county-level allows accounting for geographic proximity to the state border revealing that the effects positively depend on the distance from the state center and are largest at the border. Utilizing the exact year of introduction further allows estimating the persistence of the adoption shock, showing that the initial years after the adoption drive the main results. The counterfactual exercise, in which the taxes were not introduced, finds large negative effects for total firm outflows in adopter states. Non-adopter states observe positive counterfactual changes. These results lend credible support to the hypothesis that firms also vote with their feet.

My findings align with available suggestive evidence of firm relocations triggered by these tax adoptions. The state level adoption of income taxation in the early 20$^{\text{th}}$ century has been shown to have contributed to a decline in the share of corporate economic activity, indicating a relocation trend toward tax-favorable states [Liu, 2014]. Positive migrational effects of the tax adoptions among individuals earning below typical personal income tax exemption rates, as provided in Smutny and Wandschneider [2024], offer additional suggestive support.

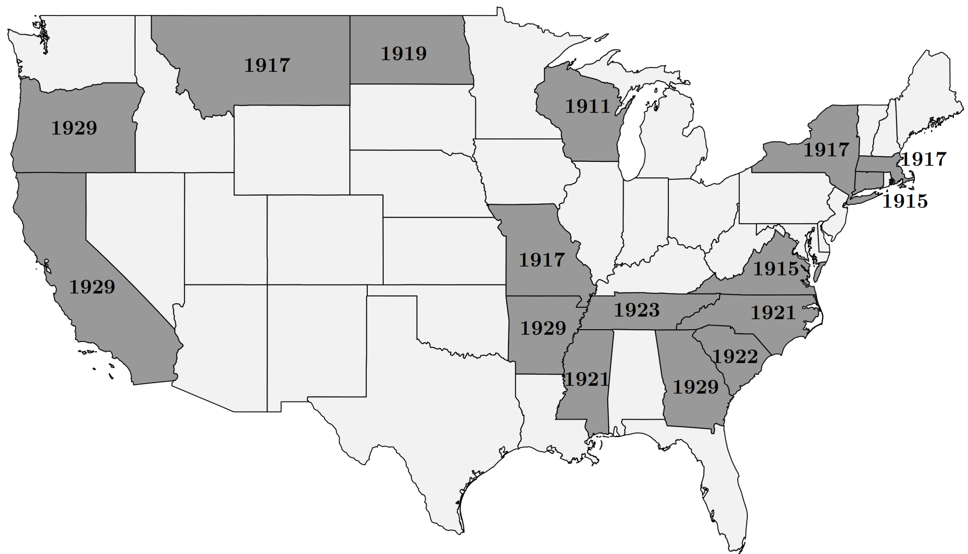
The existing literature on the corporate income tax avoidance through relocations has primarily focused on the impact of changes in tax rates. Hereby, the main objective is to estimate the tax base mobility of unharmonized corporate tax rates. Brülhart et al. [2023] recently asses that regional corporate tax cuts lead to an increase in taxable corporate income through mobility. These effects are especially strong for less urban areas [Krapf and Staubli, 2020]. Holmes [1998] provides evidence for the impact of pro-business state policies (including lower corporate income tax rates) on firms' location choices in the U.S., demonstrating an increase in corporate activity when crossing county borders into states with more business-friendly policies. Suárez Serrato and Zidar [2016] estimate that a 1% decrease in business taxes corresponds to a 4.07% increase in the number of establishments over a decade. Giroud and Rauh [2019] find a 0.40 elasticity of U.S. establishments, while Fajgelbaum et al. [2019] find this elasticity to be 0.81. On an international level, Voget [2011] shows that a 1 percentage point decrease in effective foreign subsidiary tax rates increases the likelihood of headquarters relocation by 0.22 percentage points. This paper extends this literature in several important dimensions. First, it uses a unique historical context and data to investigate relocations effects of tax adoptions, rather than rate changes. By examining the extensive margin of taxation, I capture fundamentally different firm responses than studies of rate changes. Second, I utilize bilateral state-pair data to estimate elasticities on flows rather than stocks, allowing me to directly measure firm relocations instead of inferring them from changes in establishment counts. The underlying model treats tax adoptions as both an increase in location-specific costs and a status-quo bias against any new tax [Holton, 1999; Einhorn, 2008]. The estimated 13.02% increase in relocation flows translates to an elasticity of approximately 1 in total firm stocks, suggesting overall larger magnitudes considering that my estimate should be interpreted as a lower bound. I further add to this literature by digitizing state-level manufacturing data on salaried workers, which allows me to estimate significant effects of the tax introductions on the average number of administrative workers. The direction of my results align with the literature on tax rate elasticities while providing new empirical support of increased firm relocations due to the state corporate tax rollouts of the early 20$^{\text{th}}$ century.

The remainder of this paper is structured as follows. Section 2 discusses the historical context of the corporate state income tax adoptions. Section 3 gives an overview of the data used. Section 4 describes the derivation of a structural gravity equation based on firm relocation choices. Section 5 presents the baseline estimation framework and Section 6 describes the main results, as well as further results extending the baseline model across various dimensions. Section 7 presents counterfactual exercises and estimates state-level changes in state-level firm outflows. Section 8 provides support of the robustness of the results and Section 9 concludes.

2 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 1, the first state to pass a state-specific corporate income tax law was Wisconsin in 1911. Shortly after, 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 1 – Introduction of the corporate income tax



Notes: 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.[3] 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.[4] 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.[5]

[3]Only in the long run, after decades of successfully raising state revenues, did the income tax become more of a substitute [Wallis, 2000].

[4]The tax introductions can hence be seen as a tool of revenue centralization on the state level.

[5]This is empirically supported by findings of Figures A2 and A3 in Appendix A using data from Cassidy, Dincecco and

To further explore driving factors of the tax adoptions, Appendix B 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 A1). 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 out-dated 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.[6] 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, 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

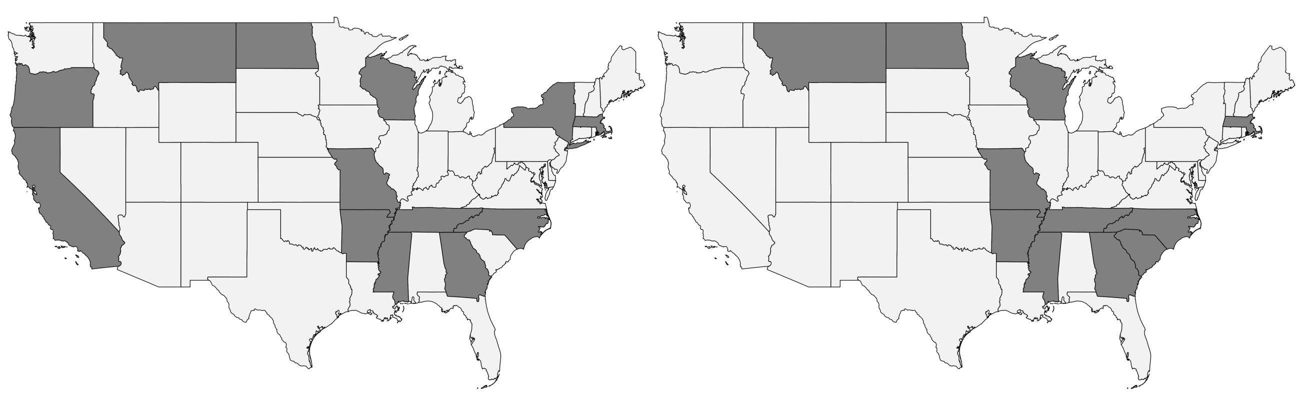
Troiano [2024] to conduct a simple event study of the tax adoption effects on state-level revenues and expenditures based on Callaway and Sant'Anna [2021], where I do not find significant increases for either metrics.

[6] 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.

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 2a, thirteen of these states imposed income taxes on financial institutions such as banks and insurance companies. Eleven states, shown in Figure 2b, 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*)]

Figure 2 – Corporate income tax adoptions by specific sectors



(**a**) States taxing financial corporations

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

2.1 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.[7] 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.[8] This highlights the importance of examining this historical context beyind the opportunity to estimate elasticities for tax policy changes on the extensive margin.

3 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.[9] In order to fill this substantial gap in the literature, I use full count historical census data of the early 20th century, provided by Ruggles et al. [2021]. Individuals coded as "employers" can be used as a proxy for their own firms.[10] Therefore, the census only

[7] 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.

[8] More on that in Section 5 and Appendix C.

[9] The authors trace this back to a mix of neglect and disasters such as fires that destroyed much of these schedules.

[10] The 1920 introduction for census enumerators quote: "*An employer is one who employs helpers, other than domestic servants, in transacting his own business. The term "employer" does not include the superintendent, agent, manager, or other person employed to*

includes business owners [11] and does not allow identifying publicly traded companies. However, publicly funded firms account for less than 10% of all corporations in 1910 [Hannah, 2015]. This allows the constructions of a comprehensive dataset of individual-level employer data for the entire U.S.[12] Multiple consecutive census waves allow tracking individuals across time by following Abramitzky et al. [2021] to link waves based on matching algorithms by Taft [1970]. This procedure is based, inter alia, on phonetic similarities in last names.[13] Note that the 1900 census does not provide information on employment status or class of worker. I hence extrapolate this information from the 1910 census.[14]

Table 1 – Linked data decomposition summary

Link	Total	Labor Force	Employers Ratio	Ag Ratio	Non-Ag Ratio	Ag Switchers
1900 - 1910	10,133,898	73.51%	15.51%	11.21%	4.30%	
1910 - 1920	12,882,629	72.11%	8.54%	5.80%	2.75%	19.59%
1920 - 1930	15,925,624	73.47%	6.02%	3.13%	2.89%	25.10%

Notes: *Labor Force* defines the share of individuals in the labor force. *Employers Ratio* computes the employers share of the labor force. *Ag Ratio* defines the labor force share of agriculture employers, e.g. farmers. *Non-Ag Ratio* then similarily defines the labor force share of employers of all sectors except agriculture. These figures are computed for the end of each decade. *Ag Switchers* denote the percentage of agricultural employers that became employees during the decade.

Table 1 reports a descriptive overview of the linked census waves. The linked sample size is increasing due to population growth, which does not imply any change in linking accuracy. The share of individuals in the labor force stays constant across the waves. I further find that the amount of employers in the labor force is drastically decreasing over time. Here, the main driving force is the agricultural sector. In the decade between 1900 and 1910, the vast majority of employers were working in the agricultural sector. The agricultural employers share of the labor force then almost quarters during the course of my observed time horizon. The period is historically characterized by big structural changes. As presented in Raup [1973] and Riney-Kehrberg [2018], the U.S. was facing a transition from small scale

manage an establishment or business; and it does not include the foreman of a room, the boss of a gang, or the coal miner who hires his helper. All such should be returned as wage or salary workers, for, while any one of these may employ persons, none of them does so in transacting his own business. In short, no person who himself works for wages or a salary is to be returned as an employer."

[11]This includes owners of corporations, as owners and board members are, by definition, not employees of their businesses. However, as the sample includes unincorporated firms as well, the results should be interpreted as a lower bound.

[12]The subsequent Section 3.1 will carefully discuss the validity of this proxy.

[13]Hence, the data only includes male individuals.

[14]I employ three criteria to identify an individual in the 1900 census to qualify as an employer. Firstly, the person must have been classified as an employer in the 1910 census. Secondly, they must have remained in the same occupation, and finally, the industry in which they are engaged must also have remained consistent. While this approach may lead to some underestimation of the number of employers in 1900, primarily due to transitions between individuals being employers or working on their own account, it's worth noting that the emphasis lies on minimizing Type I errors. These Type II errors, which are more prevalent in such transitions, are of lesser consequence given the absence of corporate income taxation for individuals working on their own account during that period.

operations to corporate farming and other manufacturing industries. Indeed, the summary statistics reveal that the proportion of agricultural employers transitioning into salaried employment is on the rise over successive decades. These country-wide structural changes and the availability of efficient cross-state transportation, highlight the importance of carefully controlling for these trends.[15]

3.1 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. In order to do so, 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

$$\text{\# of Mfg Businesses}_{i,t(b)} = \psi(\text{\# of Mfg Employers}_{i,t(b)}) + \zeta \text{Total Pop}_{i,t(b)} + \mathbf{FE} + \nu_{i,t(b)}, \tag{1}$$

where the fixed effects control for state i-specific effects and time effects for the starting year $t(b)$ of a census wave link t.[16]

Table 2 – Correlation between 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. For the full model with state and decade fixed effects, standard errors are cluster by states.

Note that 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 certain fraction of small businesses. Hence, it is to be expected that $0 < \widehat{\psi} < 1$. Table 2 presents the estimation results for equation (1). After controlling for state-specific total population and state and decade fixed effects, I find

[15] In Section 5, I discuss the strategy to account for these changes, as well as a test for pre-trends.

[16] Note that the numbers of both employers and businesses are stocks either at the onset $t(b)$ or conclusion $t(e)$ of a decade. The baseline specification uses numbers at the onset.

that each manufacturing employer in state i at the onset of decade $t(b)$ is associated with around 0.71 manufacturing businesses. Again, as the Census of Manufactures [1929] excludes small businesses, this coefficient cannot report a one-to-one mapping.[17] The effects further remain positive and significant for the corporate subset of businesses. The results allow deducing that there exists a strong and significant correlation between the number of employers and the number of businesses. I hence argue that the number of employers from the linked census waves is a suitable proxy for the number of firms.[18]

3.2 Bilateral State Flows

To capture the movement of firms across state borders, the next step involves generating a dyadic dataset. This dataset quantifies the total number of employers relocating from any given state i to another state j.[19] Henceforth, a single observation will be 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. Here, $t(b) \in \{1900, 1910, 1920\}$ and $t(e) = t(b) + 10$. As Alaska and Hawaii were not yet states during the observed time period, the dyadic data consists of $3 \times 48^2 = 6,912$ observations.[20] The data is constructed symmetrically by design. That is, the outflows from i to j are equivalent to the inflows of j coming from i.

The aggregation can be repeated for several sectors (or other characteristics) separately. The full-count census data provides a reasonably refined set of sectors. Table 3 reports dyadic summary statistics for firms, overall, as well as a decomposition by industries. A finer decomposition is presented in Table A2. 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. Furthermore, I observe significant variability across sectors, particularly in the context of pair outflow. The agricultural sector exhibits the highest average pair outflow, initially appearing counterintuitive due to the perceived immobility of farms. Several factors contribute to this phenomenon. First, the sheer volume of individuals within the agricultural sector plays a pivotal role in driving higher levels of pair outflow.

[17] I repeat this estimation for the stock values of employers and businesses at the end of the respective decade $t(e)$ and find quantitatively almost identical effects.

[18] For the remainder of the paper, I will use employer and firm interchangeably when addressing the data.

[19] 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. The primary reason for this approach is that the stock of potential firms moving is determined at the beginning of the decade (the model in Section 4 formalizes this). A more technical reason for not choosing a stronger constraint, i.e. an individual being an employer in the beginning and end of a decade, is that I cannot observe the exact timing of the relocation and hence also not if a business was moved and perhaps then closed down or closed prior to the relocation. This is more formally discussed in Appendix E.

[20] Note that the exclusion of Alaska and Hawaii also has practical reasons.

Second, despite its apparent high total outflows, the agricultural sector observes the smallest mobility rate among all sectors. Third, note that the agricultural sector encompasses an array of professions, beyond farmers. Lastly, although relocating a farming business poses inherent challenges, the prospect of acquiring or inheriting a new farming property in a different state offers a viable option to "relocate" one's agricultural operations.

Table 3 – 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: Figures exclude i, i-pairs. I define the *Mobility Rate* as the average outflow share of the average sample population. Note, that this is not the dependent variable of the main specification. The dependent variable is *Pair Outflows*.

Any other sector observes a drastically lower average pair outflow. The ratio of average state outflow to average state population reveals that this is mainly driven by an overall low number of employers in my sample rather than a general reluctance of relocations by the employers. These findings suggest that it is worthwhile to think of more aggregate sector specifications. As presented in Section 2, 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 summary statistics for these aggregated industries have the advantage of presenting larger and hence more meaningful average pair flows.[21] I find that the aggregated sector taxed by all states adopting a state corporate tax, i.e. manufacturing and mercantile businesses, is also the largest in magnitude of interstate firm flows. Furthermore, services observe a larger mobility rate, while the financial sector is less mobile.[22]

The final dataset allows me to gather motivational evidence before delving into the model and subsequent causal estimation. There are two empirical facts worth stressing. First, states that adopt a corporate income tax at some point prior to the Great Depression, observe a larger average rate of firm outflows. Figure 3 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. Second, balancing these two groups with respect to the total log-outflows, as presented by Table 4, reveals that larger outflows are

[21]This will later be important as the empirical estimation loses fewer observations due to statistical separation.
[22]Due to legal differences, regulations and local depositors or policyholders, the financial sector is less mobile by design.

only present after the adoption of the corporate income tax. Inflows, on the other hand, seem to follow the reverse trend, which suggests that inflows into adopter states decreased relative to non-adopter states.

Figure 3 – Adopter assignment and total log-outflows

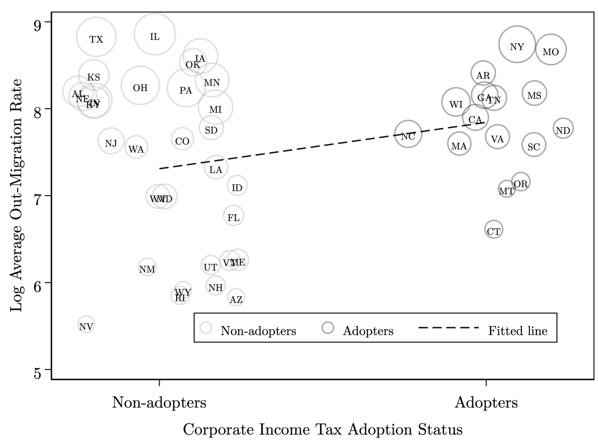


Table 4 – Difference over decades

	Non-adopters	Adopters	*p-value*
Panel A: Outflows			
1900s	6.1	6.6	0.14
1910s	7.7	8.3	0.03
1920s	7.3	7.9	0.03
Panel B: Inflows			
1900s	6.2	6.8	0.09
1910s	7.8	8.3	0.06
1920s	7.3	7.7	0.16
Observations	32	16	

Notes: The size of the respective circle varies with the average state-level stock of firms. For the differences presented in the table, p-values are computed based on a two-sample t-test with unequal variances.

These stylized facts are meant to illustrate the imperative to conduct an empirical analysis to investigate the link between corporate income tax adoptions and interstate firm relocations.

3.3 State-level Data

Manufacturing data on state-level variables such as corporate shares, number of establishments or total number of administrative personnel is drawn from digitizing relevant parts of the Census of Manufactures [1909] and the Census of Manufactures [1929]. Political variables are collected manually from National Archives and the National Governor Association, and fiscal state-level data is taken from Cassidy, Dincecco and Troiano [2024].

The state-level data allows testing a central claim of this paper. Besides anti-tax sentiment, the main channel through which the new tax can have an effect on firms is a change in fixed costs. In theory, it has been established that fixed costs increase with corporate taxation (e.g., Weston [1949]). Moreover, changes in fixed costs can contribute to corporate relocation [Krugman and Venables, 1995]. Fixed costs may rise due to firms' needs to hire additional personnel to help with bookkeeping for tax collection and audits. The digitized data includes a differentiation of managers, administrative staff and workers. Figure A4 depicts the number of administrative employees per corporation at the end of each decade in the sample. Not only does the number of administrative personnel increase over time, but adopter states such as Wisconsin or California observe an especially pronounced increment. To gain more insight into the relationship between tax adoptions and number of salaried personnel, I estimate the following model

$$\text{\# of Salaried Personnel}_{i,t(e)} = \eta z_{i,t} + \textbf{Controls} + \textbf{FE} + \upsilon_{i,t(e)}, \tag{2}$$

where the set of control variables includes the number of corporations, the total population, the total out-migration. Additionally, to control for unobserved state and decade heterogeneity, fixed effect are added. Results of the estimation are presented in Table 5.

Table 5 – %-change in administrative personnel associated with the tax adoption

	$\ln(\text{\# of Salaried Personnel}_{i,t(e)})$			
	Administrative	Managerial	Administrative	Managerial
Corporate income tax adoption: $z_{i,t}$	0.624	0.692	0.086	0.081
	(0.409)	(0.403)*	(0.047)*	(0.046)*
$\ln(\text{Total Corp}_{i,t(b)})$			−0.223	0.370
			(0.142)	(0.138)***
$\ln(\text{Total Pop}_{i,t(b)})$			1.112	0.738
			(0.198)***	(0.192)***
$\ln(\text{Total Out-Migration}_{i,t(e)})$			−0.503	−0.497
			(0.146)***	(0.142)***
State FE, Decade FE	no	no	yes	yes
Observations	144	144	144	144
Adjusted R^2	0.013	0.009	0.992	0.992

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. For the full model with state and decade fixed effects, standard errors are clustered by states.

Note that the effects are decomposed and estimated for managerial and administrative staff separately. Unsurprisingly, the uncontrolled estimations are highly upward biased. Once controlling for key factors, the effects become smaller, yet stay relevant and are statistically significant. I find that the adoption of corporate income tax increases the number of administrative and managerial staff by around 9%. These results show empirical evidence for increased fixed costs associated with the adoption of the corporate income tax.

4 Deriving a Gravity Equation of Firm Relocations

The underlying structural framework is inspired by McFadden et al. [1973], Anderson [2011] and Beine, Bertoli and Fernández-Huertas Moraga [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

$$y_{i,j,t(e)} = \phi_{i,j,t(e)} \times f_{i,t(b)} \times g(t), \tag{3}$$

where $\phi_{i,j,t(e)} \in [0,1]$ defines the share of relocating firms from i to j realized at 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. Applied to the firm level, this becomes a relocation profit maximization exercise such that

$$\Lambda_{h,i,j,t(d)} = \left[\underbrace{\alpha p_{j,t(d)} - \gamma c_{i,j} + \nu_{h,i,j,t(d)}}_{\breve{\Lambda}_{h,i,j,t(d)}} \right] + \beta \tau_{i,j,t(d)} \tag{4}$$

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. This can be interpreted as a relative increase in fixed costs for firm h if staying in the origin state (i.e. $i = j$). Lastly, α, β and γ serve as respective weights for factors affecting relocation profits. 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 (3), can be expressed as shown in the equation below:

$$\mathbb{E}(y_{i,j,t(e)}) = \left[\underbrace{\frac{\exp(\alpha p_{j,t(d)} + \beta \tau_{i,j,t(d)} - \gamma c_{i,j})}{\sum_{k \in J} \exp(\alpha p_{k,t(d)} + \beta \tau_{i,k,t(d)} - \gamma c_{i,k})}}_{\mathbb{E}(\phi_{i,j,t(e)})} \right] \times f_{i,t(b)} \times g(t). \tag{5}$$

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

$$\texttt{Push}_{i,t(d)} = \left[\sum_{k \in J} \frac{\exp(\beta \tau_{i,k,t(d)}) \times \texttt{Pull}_{k,t(d)}}{\texttt{Costs}_{i,k}} \right]^{-1}. \tag{6}$$

With these definitions we can re-write equation (5) such that it has a gravity-like structure

$$\mathbb{E}(y_{i,j,t(e)}) = \exp(\beta \tau_{i,j,t(d)}) \times \frac{\texttt{Push}_{i,t(d)} \times \texttt{Pull}_{j,t(d)}}{\texttt{Costs}_{i,j}} \times f_{i,t(b)} \times g(t). \tag{7}$$

The expected firm relocation flow from i to $j \neq k$ is increasing in the stock of firms $f_{i,t(b)}$, the attractiveness of destination ($\texttt{Pull}_{j,t(d)}$) and $\texttt{Push}_{i,t(d)}$, capturing the inverse attractiveness of all destinations $k \neq j$. On the other hand, expected flows are decreasing in bilateral relocation costs ($\texttt{Costs}_{i,j}$). Additionally, the expected firm flows from i to j are increasing with i adopting a corporate income tax. Hereby, $\tau_{i,j,t(d)}$ serves as an additional push factor for the origin. Note that, as argued in Beine, Bertoli and Fernández-Huertas Moraga [2016], this gravity formulation of equation (7) is based on the assumption that push factors are independent of the destination state j.[23] I can further impose a stochastic error component $\varepsilon_{i,j,t(d)}$ onto expression equation (7). I assume $\mathbb{E}(\varepsilon_{i,j,t(d)} | \tau_{i,j,t(d)}, \texttt{Pull}_{j,t(d)}, \texttt{Push}_{i,t(d)}, \texttt{Costs}_{i,j}, f_{i,t(b)}) = 1$. Any log-linearization of this model entails a problem with the random error assumption. Note that, as shown in Silva and Tenreyro [2006], Jensen's inequality generally implies that $\mathbb{E}(\ln \varepsilon_{i,j,t(d)}) \leqslant \ln \mathbb{E}(\varepsilon_{i,j,t(d)})$. This means that, under no further assumptions, $\mathbb{E}(\ln \varepsilon_{i,j,t(d)}) \neq 1$. In fact, the expected value of the logarithm of any random variable depends on its variance. 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

$$y_{i,j,t(e)} = \exp \left[\beta \tau_{i,j,t(d)} + \ln(\texttt{Pull}_{j,t(d)}) + \ln \left(\texttt{Push}_{i,t(d)} \times f_{i,t(b)} \right) - \ln(\texttt{Costs}_{i,j}) + \ln \left(g(t) \right) \right] \varepsilon_{i,j,t(d)}. \tag{8}$$

[23] The estimation strategy will include fixed effects to appropriately handle this assumption.

Finally, recall that all time periods $t(b)$, $t(d)$ and $t(e)$ are within one decade t and that the policy variable is defined at the $t(e)$.. Introducing a decade fixed effect δ_t controls for decade-specific country-wide events that have been realized by $t(e)$ and hence the time variation in the policy variable that is constant among origin-destination pairs. By design, this means $\delta_t \equiv \ln(g(t))$. For notational tractability, the time dimension will be given by decade t.

5 Identification and Estimation Procedure

The estimation strategy accounts for multilateral resistances by following Beine and Parsons [2015] and Bertoli and Moraga [2015], adding both origin-decade $\delta_{i,t} = \delta_i \delta_t$ and state-decade $(\delta_{i,t}, \delta_{j,t})$ fixed effects, which entirely absorb the terms $\ln(\texttt{Pull}_{j,t})$ and $\ln(\texttt{Push}_{i,t} \times f_{i,t})$. These fixed effects allow controlling for the entire universe of potential push and pull factors. I further include a decade-interstate fixed effect $\delta_{i \neq j,t}$. As proposed in Bergstrand, Larch and Yotov [2015], this allows controlling 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. The fixed effect is defined as a set of dummies equal to one for interstate flows and each decade. Finally, I follow Weidner and Zylkin [2021] and set $\ln(\texttt{Costs}_{i,j})$ equal to a state-pair fixed effect $\delta_{i,j}$. The full set of fixed effects is essential for mitigating potential endogeneity concerns.[24] With that, I re-write equation (8) as

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

I referr to this as a four way-way fixed effects (4WFE) model. Note that this is a multidimensional interpretation of a TWFE model, including additional fixed effects for the resistance terms.

Next, I identify the tax introduction dummy. As my model requires this adoption indicator to vary both in origin state i and destination state j, I follow Beverelli et al. [2018] by utilizing intrastate flows in order to create a pair-wise moving policy variable. I construct the adoption dummy $z_{i,t} = 1$ if state i adopts the corporate income tax in decade t, Then, I can 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 respective state of origin i.[25] The traditional approach of constructing the policy variable is to define $z^{trad}_{i,u\in\mathcal{T}} = 1,\ \forall\, u \geqslant t$. I

[24] As note in Beine, Bertoli and Fernández-Huertas Moraga [2016], an alternative that would control extensively for multilateral resistances is the CCE estimator. However, this approach requires a higher temporal dimension of the panel.

[25] Another way to construct a bilateral policy is to define $\tau^{alt}_{i,jt} = \mathbb{1}(z_{i,t} > z_{j,t})$. Here, I can directly test whether effects are driven by opposing state tax policies.

divert from that by defining $z_{i,t+1} = 0$ if $z_{i,t} = 1$. There are two reasons for this approach. First, I target to capture the short run effects of the tax adoptions. In other words, I am interested in the relocation effects upon impact of the novel tax. Second, I aim to analyze the tax' consequences independently of any shifts in public goods provision, ensuring a clear understanding of its direct influence. 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.[26] However, in the medium run, the National Industrial Conference Board [1930] indicates an increase in state level revenues stemming from the income tax adoptions. Hence, it is conceivable that in the medium run (i.e. after a decade) heightened public goods and infrastructure investments resulting from increased tax revenue could incentivize firms to move to state i. To prevent conflating these effects, the alternative specification limits the analysis to a single period, avoiding the complexity of simultaneous influences. As the short term policy variable only measures the effects of the introduction within the range of ten years, it can be expected that $\widehat{\beta} < \widehat{\beta}^{trad}$.

Another 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.

Table 6 – 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: $\mathbb{E}[y^{treat}_i]$ and $\mathbb{E}[y^{cont}_i]$ represent the average of $y_{i,j,t}$ for treated and control groups, respectively, in the 1900s. P-values are computed using a two-sample t-test with unequal variances. Note that panel B is a hypothetical comparison, as stayers are always asigned to the control.

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

[26]For an extensive discussion, see Cassidy, Dincecco and Troiano [2024].

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 6 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 3, 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. Appendix B 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, where $\mathbb{E}(y_{i,j,t}) = \lambda_{i,j,t}$ is defined as an exponential function. Following Silva and Tenreyro [2006] and Correia, Guimarães and Zylkin [2020], I can use iteratively reweighted least squares (IRLS) to recursively estimate the tax adoption effect β. This estimation technique is commonly known as Poisson pseudo-maximum likelihood (PPML) estimation, allowing to estimate equation (10) in its multiplicative form. Therefore, I do not need to assume homoskedasticity for an unbiased estimation of β [Silva and Tenreyro, 2006]. Applying the Frisch-Waugh-Lowell theorem then ensures the consistent estimation of the coefficient of interest [Correia, Guimarães and Zylkin, 2020; Weidner and Zylkin, 2021]. Note that gravity models including multi-way fixed effects are prone to potential asymptotic complications. First, it can be shown that models including a three-way 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.[27] Hence, the gravity estimates have to be interpreted as changes in interstate flows. The effects can nonetheless 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

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

[Cassidy, Dincecco and Troiano [2024], Figures A2 and A3] hold up to scrutiny. This theoretical feature, however, combined with evidence for a rise in more targeted public goods and infrastructure investments after one decade of the adoption supports $\widehat{\beta} < \widehat{\beta}^{trad}$. Consequently, a traditional policy variable would most likely result in an upward bias.

6 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, controlling for no migration costs other than distance, similar to the standard model of Anderson and Van Wincoop [2003].[28] Model (2) is estimating theoretical gravity model without controlling for U.S.-wide time trends. Model (3a,b) includes the decade-interstate fixed effect. As recently discussed in Nagengast and Yotov [2024], the gravity model with three-way fixed effects is controlling for state-specific trends over time, however not for overall time trends affecting all states equally. A separate estimation of Model (2) and (3a,b) investigates this claim. Finally, Model (3b) switches impact measure $z_{i,t}$ with the typical policy variable $z_{i,t}^{trad}$ and can hence be used to test the identifying assumption $\beta < \beta^{trad}$. Recall that I essentially define $z_{i,t} := z_{i,t}^{trad} - z_{i,t-1}^{trad}$. While the focus of the analysis lies on the estimation of short run effects of the corporate tax adoptions, medium run results are a worthwhile addition.[29] Table 7 reports the results of the baseline estimation. A first observation is that even without properly controlling for iceberg costs, the standard gravity model seems to fit the variation in firm flows well. Furthermore, the slightly lower pseudo R^2 indicates that I do not estimate the full gravity model. 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. Further, I find that the asymptotic bias correction is minuscule. However, there is reason to believe that the standard gravity formulation overlooks a crucial control for general time trends and is hence potentially severely upward biased [Bergstrand, Larch and Yotov, 2015; Nagengast and Yotov, 2024]. Typically, these temporal variations are captured within the state-decade fixed effects. However, given the unique historical context of this analysis, a more robust correction is warranted. As discussed in Section 2.1, the observation period was

[28] The distance between geographic centers of respective states is computed with Vincenty [1975].

[29] As previously discussed, the tax adoptions had no effects on government spending or revenues in the short run, i.e., within the initial ten years of the introduction. However, the National Industrial Conference Board [1930] shows that beyond this initial phase, there were documented substantial increases in local revenues. This could then lead to a dichotomous effect of the tax rollouts in the medium run.

characterized by pronounced transformations in transportation technology across the entire U.S. The observed effect is notably smaller in comparison to Model (2). This outcome is unsurprising, given that the new coefficient is disconnected from overarching nationwide time trends, such as advancements in large-scale transportation technologies. The estimated baseline effect translates to a 13.02% increase in interstate firm flows. With an average adopted minimum rate of 2.5%, this corresponds to a flow elasticity of 5.21%. 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. Further note that for any correction or alternative estimation of the standard errors, the results remain statistically significant. 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 in order to mitigate a potential bias due to accompanying policies such as increased infrastructure investments.

Table 7 – 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, Larch and Yotov [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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. Note that the bias correction by Weidner and Zylkin [2021] is only feasible for two-way and three-way fixed effects models. 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].

To further deepen the understanding of these baseline effects, it is worthwhile to analyze the context of firm effects from the tax adoptions beyond the relocation model. Could it be that firms closed as a result of these taxes? Provided the results of this analysis in Appendix E, I find no indication that the adoption of income taxes affected firm closures in the early 20$^{\text{th}}$ century. Therefore, I may approximate a firm stock elasticity by $(15739.85-(15739.85-(2551.63\times 0.0521)))/15739.85 \approx 0.84\%$.[30] Additionally, to further contextualize the baseline insights, I investigate how much of the corporate activity reduction found in the literature (Liu [2014]), can be associated with business outflows. Using mediation à la Baron and Kenny [1986], I find that around $9-10\%$ of the decrease in corporate activity, caused by the introduction of the income tax, can be attributed to state-level firm outflows. I provide a detailed discussion in Appendix C.

6.0.1 Baseline Results by 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 $y_{i,j,t,s}$.[31] This allows me to re-write equation (9) in a sector-specific notation such that

$$y_{i,j,t,s} = \exp\left[\sum_s \beta_s \mathbb{1}_s \tau_{i,j,t} + \boldsymbol{\mathcal{D}} \times \delta_s\right]\xi_{i,j,t,s}, \tag{10}$$

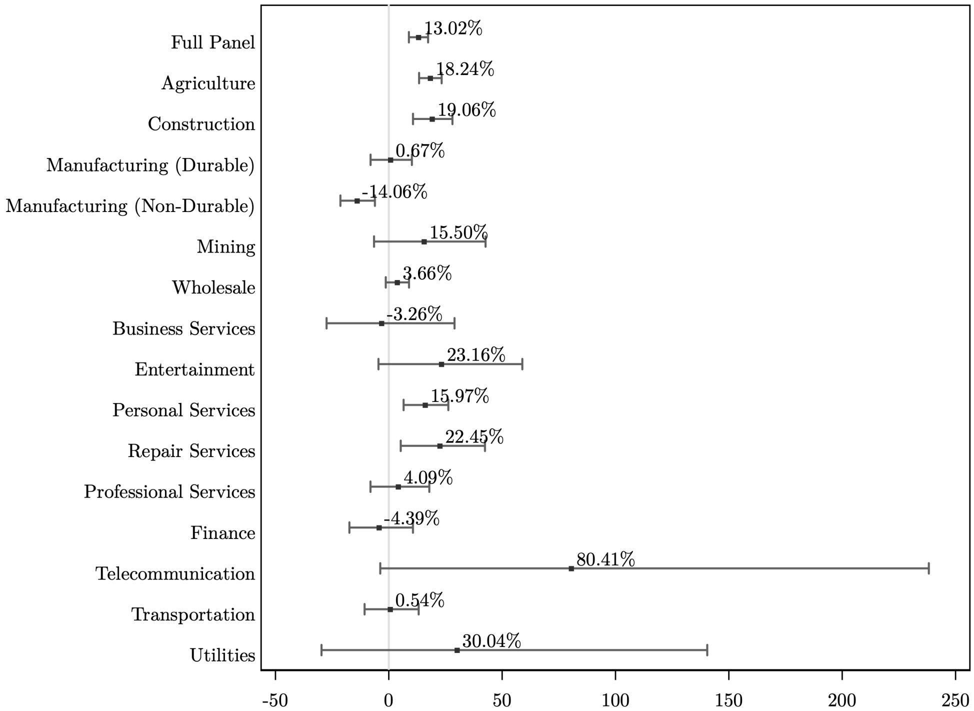
where δ_s denotes a sector fixed effect and $\boldsymbol{\mathcal{D}} = (\delta_{i,j}+\delta_{i\neq j,t}+\delta_{i,t}+\delta_{j,t})$. The sectors are equivalent to the line-up of Table A2. The results of this estimation are presented in Figure 4.[32] It is crutial to note that for this particular specification, 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. 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.

[30] Formally, that computes $(\sum_t f_{i,t} - \sum_t(f_{i,t} - (\sum_j y_{i,j,t} \times (16\times\beta)/\sum_i Min_i)))/\sum_t f_{i,t}$, where Min_i defines the minimum adopted rate in each of the 16 adopter states. Note that this elasticity excludes changes in stock from firm closures, estimates are hence a lower bound.

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

[32] A full tabular listing of the finding are presented in Table A3 in Appendix A.

Figure 4 – Estimated β_s by sectors



Notes: CI $= 90\%$. 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].

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. The subsequent analysis will therefore aggregate sectors into groups defined in Table 3. This is sufficient, as tax policies were targeting businesses assigned to these aggregated sectors.

6.1 Introducing Adopter Variation

A comprehensive tax adoption indicator should vary in sectors, as some states only taxed certain industries. As discussed in Section 2, in a number of states, financial institutions as well as public utility companies were exempt from the new tax. I can use the grouping of Table 3 to create a set of aggregated sectors S. With this new index, I repeat the estimation of equation (10), i.e. no sectoral variation in the tax dummy. The outcome will be denoted under Model (4). Next, I define an indicator variable $w_{i,S}$ equal to 1 if state i adopts the corporate income tax for the aggregated sector S. Hence, equation (10) can be re-written as

$$y_{i,j,t,S} = \exp\left[\sum_{S} \beta_S(\mathbb{1}_S \times \{\tau_{i,j,t} \times w_{i,S}\}) + \boldsymbol{\mathcal{D}} \times \delta_S\right]\tilde{\xi}_{i,j,t,S}, \tag{11}$$

which I define as Model (5). To check the mechanics of the model including the comprehensive tax dummy, I 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. The results of these estimations are collected in Table 8.

Table 8 – Stacked regressions with and without 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

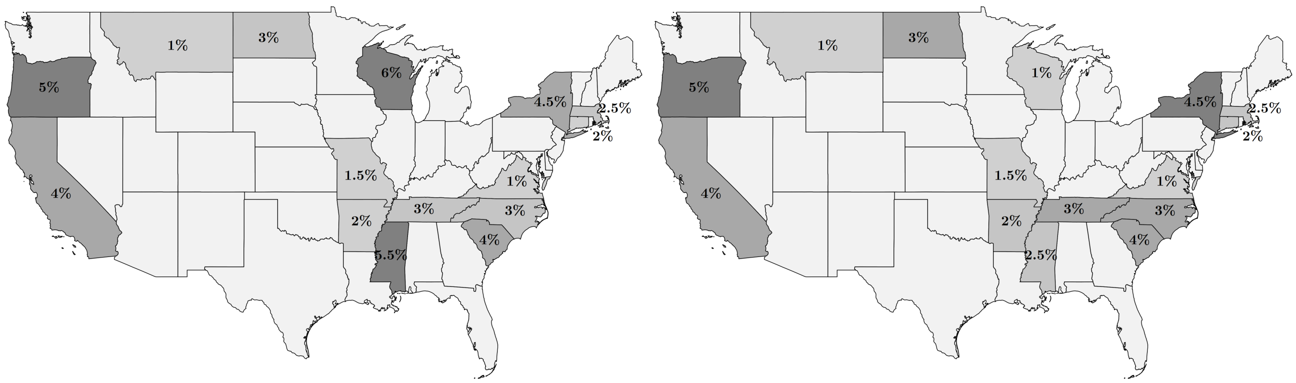
Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. All specifications include state-pair, state-decade and interstate-decade fixed effects. To ensure convergence of the estimates, statistically separated observations are excluded based on fixed effects and Clarkson and Jennrich [1991], and singletons are excluded using Correia [2015]. The absolute changes are computed as $(\exp(\widehat{\beta}_S) - 1) \times \bar{y}_S$.

First, the overall effect, and the effect on manufacturing, mercantile and service businesses do not change across models. As previously discussed, firms of the manufacturing and mercantile sector were subject to the corporate tax in each adoption state. These industries also capture the largest fraction of firms in my sample and hence drive the overall results. For the financial and public utility sectors, similar to Table A3, the baseline strategy from Model (4) does not find any significant implications of the income tax adoption. Here, the model assumes firms to be taxed, even in states that exempt them due to their sectoral classification, which leads to biased estimates. Once I adjust the tax indicator variable 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. This comes from the fact 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. Lastly, I estimate a "placebo" model, wherein I treat firms within sectors of states that implemented the income tax but refrained from imposing it on these specific sectors. 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. I do observe a noteworthy negative impact resulting from the absence of taxes on the financial sector's placebo. Given that other sectors in these states were subjected to taxation, it's 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.

6.2 Adopted Tax Rates

As previously discussed, the adopted tax rates are an important exploitable variation in treatment intensity. The introduced rates differ substantially across adopter states. Some states further adopted a progressive tax regime. Therefore, Figure 5 reports both maximum and minimum introduced rates. Wisconsin, as the first adopter state also introduced the largest tax rate at 6%. The progressivity of the rate was associated with the amount of taxable net income. Given that the individual census data enables the identification of firm owners, the use of firm proxies may lead to an underrepresentation of publicly traded companies. As a result, a significant portion of the sampled firms are assumed to be small to medium-sized enterprises. Consequently, the maximum tax rates can be viewed as indicative of the potential for future tax rate increases, particularly for businesses in this size range. In order to estimate the effects of these adopted rates, 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.

Figure 5 – Adopted tax rates by states

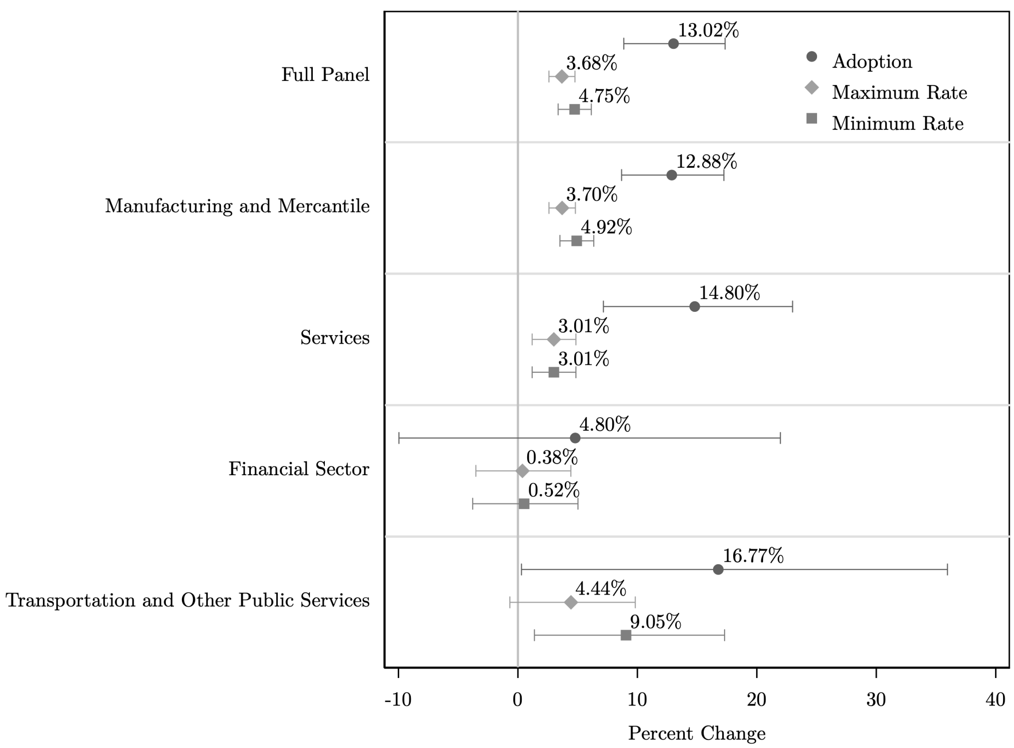


(**a**) Maximum tax rates

(**b**) Minimum tax rates

The policy in equation (11) can be substituted with this new term. Now, β_S^r 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 6.

Figure 6 – Estimated β_S^r by aggregated sectors



Notes: CI = 90%. 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]. Details of the estimation can be found in Table A4.

Consistent with the interpretation of the coefficients, the magnitude of the estimates are significantly smaller. Furthermore, it is apparent that the minimum rate is more salient. A larger fraction of the sample is affected by the minimum rate leading to consistently larger point estimates across sectors. I find that a one percentage point increase in the adopted minimum tax rate increases relocation flows by 4.75% on average.[33] The manufacturing and mercantile industry as the largest group is driving this result. Corresponding to the adoption results, the introduced rates are of insignificant importance to financial businesses. Interestingly, the public utility sector observes slightly insignificant effect for a marginal change in the introduced maximum tax rate. This indicates that the majority of firms in this sector tend to be small businesses, which are only effected by the minimum rate.[34]

6.3 Potential Bilateral Sources of Endogeneity

Many states adopted both corporate and personal simultaneously. Higher personal income tax can make labor more expensive, depending on the paid wages and whether such taxes are passed on. Therefore, I might pick up some of the effects of the personal tax adoption through my policy variable. In order to disentangle these channels, I again utilize interstate heterogeneity and create $z_{i,t}^{pers} = 1$ if state i adopts the personal income tax in decade t. Together with the corporate tax adoption indicator, I can create a new set of policies. First, a dummy capturing states that only adopted a corporate income tax in decade t denoted as $\tau_{i,j,t}^{corp} = \mathbb{1}(z_{i,t} = 1 \wedge z_{i,t}^{pers} = 0 \wedge i \neq j)$. Similarly, the indicator function for states only adopting personal income taxes in decade t is defined as $\tau_{i,j,t}^{pers} = \mathbb{1}(z_{i,t} = 0 \wedge z_{i,t}^{pers} = 1 \wedge i \neq j)$. Finally, states adopting both income taxes in the same decade t are indicated with $\tau_{i,j,t}^{both} = \mathbb{1}(z_{i,t} = z_{i,t}^{pers} = 1 \wedge i \neq j)$.[35] Note that equation (9) now includes three distinct and statistically comparable policy variables. The findings, presented in Figure 7 reveal a set of fascinating insights. A direct comparison to the effects described in Table 8 presupposes caution, as the original policy is a combination of two policies here, i.e., $\tau_{i,j,t} = \tau_{i,j,t}^{corp} + \tau_{i,j,t}^{both}$. It is however evident that the sole introduction of personal income taxes did not affect the interstate flow of businesses. This first result can be interpreted as a sanity check for the aforementioned battery of findings. A much more pronounced effect can be found

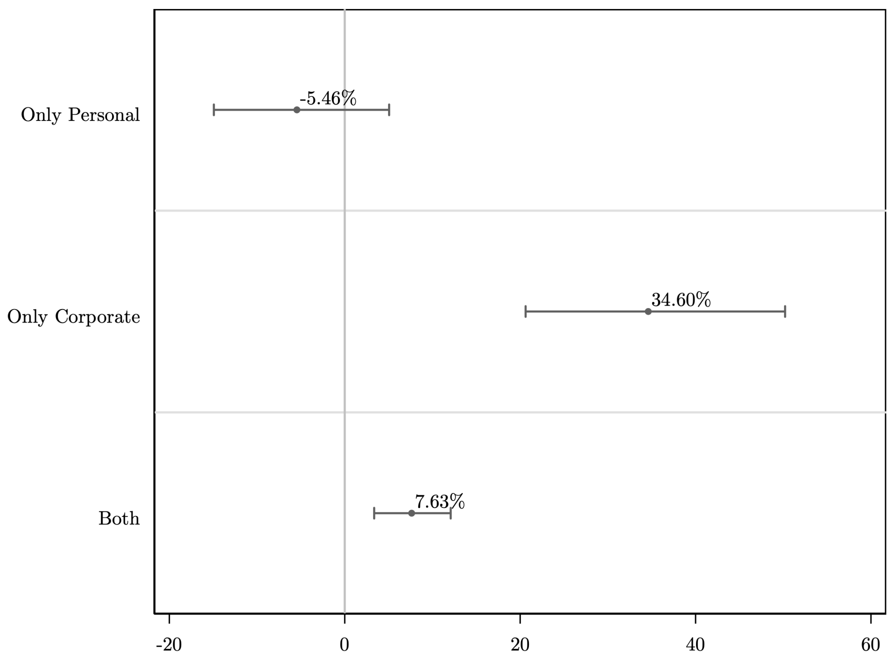
[33] Under a set of assumptions, the pure adoption affect can be approximated by $5.21 - 4.75 = 0.46$. See Appendix F for more details.

[34] Another interesting aspect related to the rates is the level of progressivity of which they were implemented. In order to analyze the effect of rate progressivity on the 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. Note that this newly defined variable captures the inverse of progressivity, i.e. flatness. An increase in the measure is associated with minimum and maximum rate being closer together. Results for the effects of the rate ratio can be found in Table A4. To facilitate interpretability, I take the measure times 10 in order to interpret the coefficient as a 10 ppt. increase in the rate flatness. Consistent with the previous findings of the rate effects, the closer the minimum is to the maximum, the larger the effect on relocation flows. This again stems from the fact that all firms that are affected by the tax are at least affected by the minimum rate.

[35] To avoid multicollinearity with the intercept, the case where neither of the taxes are introduced is omitted.

for the indicator capturing only corporate income taxes. Again, as magnitudes across two different models using different policy variables are difficult to compare, these findings are indicative of no apparent upward biased imposed by higher input prices. If anything, the results raise potential concerns about underestimating the effects of the corporate tax adoptions. This is further supported by the fact that the sample includes employers owning unincorporated companies that are subject to the personal income tax. Hence, the findings in Figure 7 suggest that previous results are driven by corporations relocating and not small enterprise. This is to be expected, as the main channel through which income tax adoptions affect relocation decisions is by increasing fixed costs, which are presumably correlated with firm size.

Figure 7 – Estimates decomposed by type of adopted income tax



Notes: CI = 90%. Standard errors are clustered by state-pairs. Note that the bias correction by Weidner and Zylkin [2021] is only feasible for two-way and three-way fixed effects models. 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]. Details of the estimation (also by sector) can be found in Table A5.

6.3.1 Relocation Complementarities

So far, the analysis has neglected the impact of relocation complementarities across firms. Brülhart, Jametti and Schmidheiny [2012] have shown that agglomeration effects can lead firms' location choices to be less sensitive to tax rate differentials. A similar channel could 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 addition of 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, influence firms' decisions on where to locate. As illustrated in Smutny and Wandschneider [2024], the investigated time frame witnesses 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 A5). 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 (4) such that

$$\widetilde{\Lambda}_{h,i,j,t} = \Lambda_{h,i,j,t} + \kappa_1 f_{j,i(t-1),t} + \kappa_2 \texttt{Workers}_{i,j,t-1}, \tag{12}$$

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 $t-1$.[36] 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 $\texttt{Pull}_{i,j,t} = \texttt{Pull}_{j,t} \times \exp(\kappa_1 f_{j,i(t-1),t} + \kappa_2 \texttt{Workers}_{i,j,t-1})$, which are now origin-specific. The gravity equation follows identical to the baseline specification. The 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 $f_{j,i(t-1),t} \equiv y_{i,j,t-1}$. 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

[36] 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.

captures the effects of 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 (11).

Table 9 – 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. The first decade is dropped for all specifications. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. All specifications include state-pair, state-decade and interstate-decade fixed effects. 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].

All results are presented in Table 9. First, note that regressions including lagged bilateral controls regressions have a smaller sample size due to the exclusion of the 1900s.[37] In order to have a comparable point estimate for the baseline strategy, I repeat the estimation of equation (11) 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

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

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 10 – 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)
$\tau_{i,j,t} \times w_{i,\mathcal{S}}$			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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. The first decade is dropped for all specifications. Standard errors in parentheses underneath the estimated coefficients 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].

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 (11), 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 10 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 7.[38] 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

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

the 95% confidence bounds of the original estimate.

6.4 Geographic Analysis

The geographic relation of states is another important factor for the analysis 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 $\text{Border}_{i,j}$.

A priori, it is difficult to make clear predictions of the results. It is fair to assume that the number of employers moving among bordering states is larger on average. A larger coefficient for bordering states would hence mean an exponentially larger total effect. Furthermore, 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. Therefore, a weaker marginal effect for bordering states can be expected. Table 11 summarizes the estimation results for the border interaction.

Table 11 – 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)
$\tau_{i,j,t} \times \text{Border}_{i,j}$	−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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. Estimations are obtained from a single regression estimation. Specification includes state-pair, state-decade and interstate-decade fixed effects. 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]. Descriptives in part 1 of the table exclude i, i-pairs.

First, the reported average flows are drastically higher for bordering states. Note that, while $\text{Border}_{i,j} = 1$ if $i = j$, these observations are excluded from the figures in the upper part of Table 11. Nonetheless, the overwhelming majority of employers seem to relocate just across the border.[39] The number of neighboring states varies from one state border in Maine to eight in Tennessee. Overall, there are 207 bordering pairs in the lower 48. Of course, the vast majority of state pairs are not bordering. As presumed, the average marginal effects are significantly smaller for bordering states, suggesting 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. This patterned is replicated by the manufacturing and mercantile, as well as the public utility sector (see Table A7 in Appendix A). Similar to the overall effects, the financial sector does not observe any significant relocation effects.

6.4.1 County-level

Are the baseline effects sensitive to the geographic distance to state borders? It is conceivable that firms located closer to the center of state have higher (idiosyncratic) costs of relocating and hence tax adoptions might be found to be weaker. To explore the geographic component within the border of a state, I use information on the county-level location of respective firms. The census data allows to not only follow employers across state lines but also across counties. This additional information helps to further asses the importance of state borders. So far, I have found that, while total effects are larger for bordering states, relative increases in interstate flows are larger for non-bordering states. This could indicate that while the firm, relocations are overall more likely to happen at the border. To test this, I utilize county-level information to estimate the effects with respect to a firm's geographic distance to the closest border of its origin state. I use data provided by Holmes [1998] to gather information of every county $w(i)$'s distance to each neighboring state. Note that each of state i's neighboring state $k \in \mathcal{B}_i$. The measure of distance to the closest state border is then defined as $\text{MD}_{w(i)} = \min_{k \in \mathcal{B}_i}\{\text{Dist}(w(i;k))\}$.[40] Further, the dyadic structure of the data is now on a county level, hence flows $y_{w(i),w(j),t}$ measure county-pair firm flows 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 12, 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

[39] Note that these figures would be even larger when including i,i-pairs. However, note that the table also reports large standard deviations.

[40] An in-depth discussion of the minimum-distance measure can be found in Appendix D.

policy with the minimum of the border distance measure. As this measure is constant across county pairs, I decide to use state-level fixed effects.[41] Figure A6 provides a graphical presentation of county-level mobility rates. These are not depending on the distance to the state border, which is critical for the "quasi-gravity" model, which can be written as

$$y_{w(i),w(j),t} = \exp\left(\vartheta_1\tau_{i,j,t} + \vartheta_2\mathrm{MD}_{w(i)} + \vartheta_3(\tau_{i,j,t} \times \mathrm{MD}_{w(i)}) + \boldsymbol{\mathcal{D}}\right)\varsigma_{w(i),w(j),t}. \tag{13}$$

Summary statistics and estimation results are presented in Table 12. Note that the point estimates for the county-to-county flows are larger compared to the state-level baseline results in Table 7. 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 outcome, illustrated by a lower R^2 and hence worse fit. This may also lead to a slight upward bias due to potentially omitted variables on the county-decade level, which may explain the larger point estimates. Furthermore, the mean dependent variable is drastically lower than in the main analysis and the distribution is heavily right-skewed (see Figure 8b). With that in mind, equation (13) allows the estimation of the fixed point $-\widehat{\vartheta}_1/(\widehat{\vartheta}_2 + \widehat{\vartheta}_3)$, i.e. the point at which the adoption effect on firm relocation becomes zero. This is built on the assumption that the baseline effects are indeed decreasing with the distance to the border. I find a weakly significant decrease in the effects associated with greater distance to the border. Figure 8a plots the estimated coefficients for $\mathrm{MD}_{w_i} \in [0, 400]$. Note that the mean distance to the closest state border is around 50 miles, with 75% of counties lying within 35 miles to the border. Hence, the effects remain positive even for firms located far from the state border, meaning that the vast majority of counties are expected to observe a positive impact. It is unsurprising yet reassuring that firms near a border have a higher probability of relocating due to significantly lower relocation costs, and hence effects are particularly pronounced at proximity to the border. Not that, while it is plausible that firms situated in the state's center may still relocate following the introduction of a tax, provided the associated costs are sufficiently high, it is much less likely. This is captured by the fact that the fixed point is estimated at around 231 miles, graphically depicted as the intersection between the two lines in Figure 8a, which is above the 95th percentile of the distance distribution.

[41] 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.

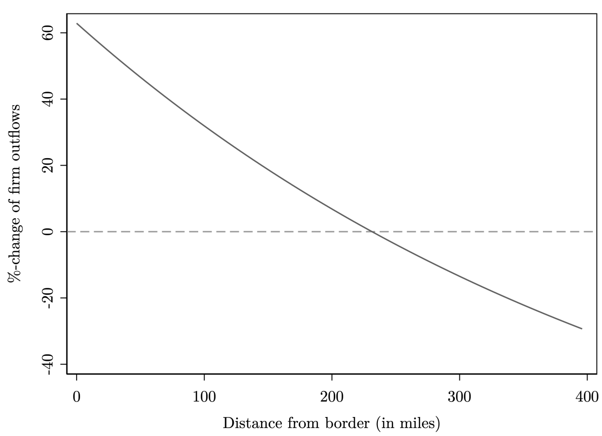
Table 12 – Overall results for the county-level analysis

Descriptive Statistics	Mean	Standard Deviation	Min	P25	P75	Max
Number of counties by state	130.118	70.354	3	82	161	254
MD_{w_i}	49.146	51.762	0	14.1	34.8	396.0
$y_{w(i),w(j),t}$	0.078	5.508	0	0	0	7,737

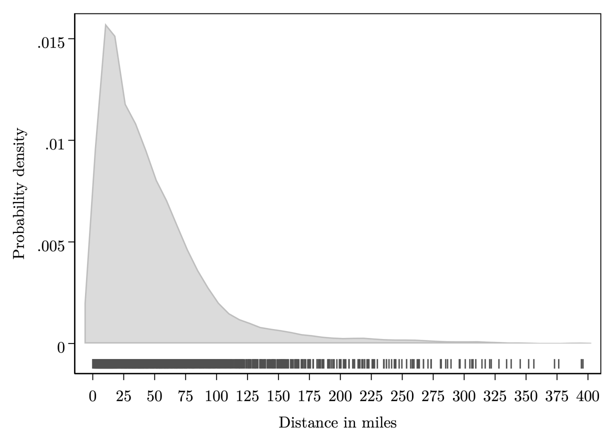
	Baseline Model	Distance Interaction
	(i)	(ii)
$\tau_{i,j,t}$	0.432***	0.488***
	(0.029)	(0.067)
$\text{MD}_{w(i)}$		-0.001*
		(0.000)
$\tau_{i,j,t} \times \text{MD}_{w(i)}$		-0.002**
		(0.001)
Observations	28,899,567	28,266,711
McFadden Pseudo R^2	0.4378	0.4384

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. Specification includes state-pair, state-decade and interstate-decade fixed effects. 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].

Figure 8 – 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: The figure reports the function $(\exp(\widehat{\vartheta}_1 + (\widehat{\vartheta}_2 + \widehat{\vartheta}_3)MD_{w(i)}) - 1) \times 100$. Hence, changes in the outcome variable are interpreted as percentage changes.

6.4.2 Manufacturing and Mercantile Businesses: Where did they move?

Besides geographic proximity, another relevant state-level feature may be the population density as a proxy for urbanization and industrialization [Acemoglu, Johnson and Robinson, 2002]. The distribution of population within the U.S. has always been heterogeneous. Historically, this pattern is of course driven by the fact that the founding states of the U.S. were located in the East. Unsurprisingly, these states are typically associated with a larger population density and degree of urbanization for the observed time period. Whether or not firms are more sensitive to tax changes in less urban states is especially tempting to analysis for the manufacturing and mercantile industry, as it is, by far, the largest sector in the sample and may observe large intrasectoral agglomeration. Furthermore, 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.[42] 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, $i \neq j$.[43] 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 13 show the largest effects for relocations from rural to urban states.

Table 13 – 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. All specifications include state-pair, state-decade and interstate-decade fixed effects. 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].

[42] 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.

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

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 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.

6.5 Persistence of the Adoption Shock

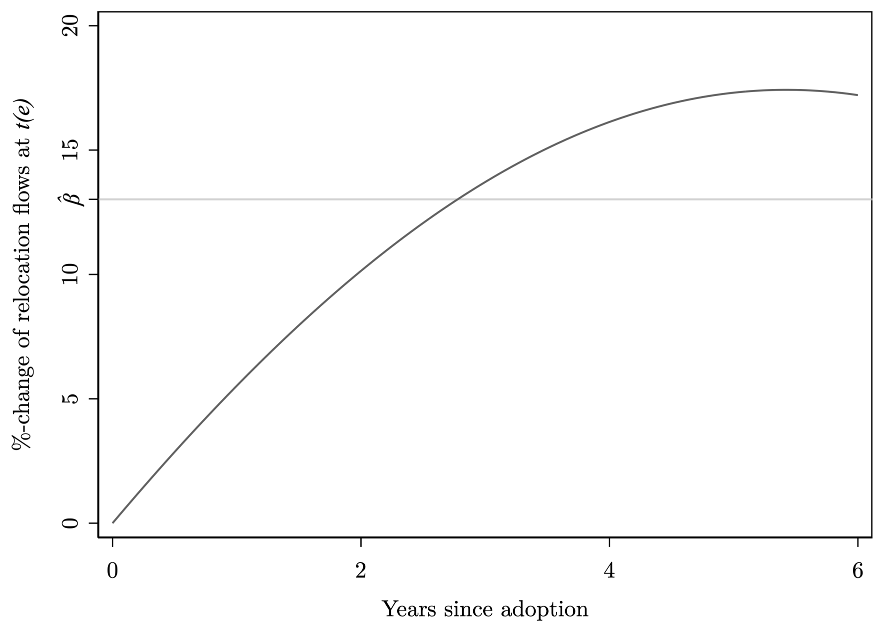
This section focuses on an approach to estimate the relocation effects within the adoption decades. So far, the analysis has been concerned with decennial effects, as permitted by the data structure, leading to the omission of informative variation. Two claims are investigated in this section. First, relocations of businesses take time. Presumably, from the time of the adoption to the relocation decision and then the actual relocation, years may pass. Second, at a certain point in time, firms that wanted to move have already done so. Therefore, there are no new relocations, which could lead to diminishing effects of the adoption over time.

Fortunately, as observed in Figure 1, the tax introductions vary heavily within the adoption decades. This allows testing the persistence of the adoption shock by incorporating the information on specific adoption years into the investigation, a variable $\text{YSA}_{i,t} = t(e) - \text{Adoption Year}_i$ is created, where if state i adopts within decade t, the years between the end of the adoption decade, which is used in the baseline dummy, to the specific year of adoption within that adoption decade are measured. Note that flows are observed at the end of a decade, i.e., $t(e)$. This allows defining a weighted treatment variable $\tau_{i,j,t} \times \text{YSA}_{i,t}$, which captures the adoption effects with respect to the temporal distance to the year of adoption. As I presume the introduction effect to be concave within the adoption decade, I estimate

$$y_{i,j,t,S} = \exp\Bigg(\sum_S \varphi_{1,S}(\mathbb{1}_S \times \{\tau_{i,j,t} \times w_{i,S}\} \times \text{YSA}_{i,t}) + \varphi_{2,S}(\mathbb{1}_S \times \{\tau_{i,j,t} \times w_{i,S}\} \times \text{YSA}_{i,t})^2 + \boldsymbol{\mathcal{D}}\Bigg)\, \varrho_{i,j,t,S}, \tag{14}$$

where the exponential polynomial function: $[\exp(\widehat{\varphi}_{1,S}\text{YSA}_{i,t} + \widehat{\varphi}_{2,S}(\text{YSA}_{i,t})^2) - 1] \times 100$ (15) then represents the relocation effects of the corporate tax adoptions with respect to temporal distance between $t(e)$ and the rollout year. Figure 9 plots equation (15) for $\text{YSA}_{i,t} \in [0, 9]$ for the full panel and includes the estimate for the average changes in relocation flows estimated by equation (9).

Figure 9 – Adoption effects weighted by years since adoption



Notes: The figure reports equation (15) and plots the full panel and the estimate from equation (9). The full set of results, including aggregated sectors, can be found in Table A6.

As expected, the relocation effects follow a concave polynomial function with an estimated plateau at 5.42 years. The coefficients are strongly significant, as reported in Table A6. This allows to draw the conclusion that the effect on total flows over the course of a decade is increasing over time up to a certain point. The effect then starts to diminish. As the estimation model of equation (14) can be interpreted as a *Mincer*-like framework, I can draw similar conclusions. While the effects of an additional year of having the tax is increasing the effects on firm relocations, this effect 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. These finds are inline with the interpretation of these effects as "adoption" effects, rather than a permanent shift in outflows. Hence, these adoptions can be seen as temporary shocks to the stock of firms in the origin state. Similar to the results before, manufacturing and mercantile businesses are driving the overall results, as they almost perfectly mirror each other. For the rest of aggregated sectors, it seems that the effects are following a linear function. None of the polynomial terms are significant and, in line with the previous findings, the financial sector does not observe any significant effects of the tax adoptions overall. Lastly, these results on the persistence, in this case temporality of the adoption shock, seem to not be driven by any particular state as shown in Figure A7.

7 Counterfactual Experiments: Exact Hat Algebra

So far, the analysis has been concerned with partial equilibrium effects of the tax adoptions, which by construction assumes no effects on non-adopters. The gravity framework however allows solving for a conditional general equilibrium, incorporating effects to all states. It further enables counterfactual exercises.

Building on the trade literature (e.g. Yotov, Piermartini and Larch [2016]), I now consider how the adoption of corporate income taxes affected all states simultaneously, conditional on the explicit parts of the multilateral resistances (e.g. location costs) to be constant. A key advantage of this counterfactual analysis over the partial equilibrium, beyond the consideration of all changes in firm flows, is the directional interpretability. The effects estimated through this procedure allow making a statement on changes in total outflows for each state. As discussed in Smutny [2024], the partial equilibrium gravity estimation so far solely allowed for flow interpretations. In other words, changes are not directional.

The main counterfactual will be the case that no state adopted the tax in the 1910s. Henceforth, $t = \{1910, 1920\}$ and the subscript t is being dropped. Using estimates from equation (8) together with the notation by Yotov, Piermartini and Larch [2016], allows constructing a matrix of baseline relocation costs such that

$$\left(\widehat{\Gamma}_{i,j}\right)^{-1} = \exp\left[\widehat{\delta}_{i,j} - \widehat{\beta}\tau_{i,j}\right]. \tag{16}$$

Due to multicollinearity, as well as singleton observations (detected using Correia [2015]), missing estimates for relocation costs need to be replaced using a predictive model

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

where the distance between i and j can be used as a predictor of bilateral relocation costs. To increase the level of sophistication, I additionally use origin and destination fixed effects for the prediction. Once the cost vector has been recovered, it can be imposed to constrain the baseline equation (9) such that

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

where $\widehat{\Gamma}^{cf}_{i,j} = \widehat{\beta}\tau^{cf}_{i,j} + \widehat{\delta}_{i,j}$. This essentially endogenizes the relocation costs. The fitted values $\widehat{y}^{cf}_{i,j}$ are defined as the bilateral relocation flows in absence of any income tax adoptions for the 1910s. Note that, $\widehat{\beta}$ is estimated in equation (9). However, since the counterfactual is the absence of a tax introduction it holds that $\widehat{\beta}\tau^{cf}_{i,j} = 0\ \forall i,j$. The counterfactual analysis aims to investigate what would have happened, if states did not adopt the income tax. There are various ways to compare the observed relocation flows from their counterfactual without income tax adoptions. I use counterfactual change in total state-level outflows such that

$$\text{Counterfactual } \Delta y_i = \frac{\sum_{j\neq i}(\widehat{y}^{cf}_{i,j} - \widehat{y}_{i,j})}{\sum_{j\neq i}\widehat{y}_{i,j}}. \tag{19}$$

As mentioned before, the counterfactual changes in outflows further have the advantage that they can be interpreted as true outflows. Hence, they add directional context to the framework, including the partial equilibrium responses. Table 14 report the counterfactual changes for the eight adopter states of the 1910s. Conditional on the effects found in the partial equilibrium, it is to be assumed that the counterfactual scenario would lead to a lower level of firm outflow in the states that adopted the policy. Other states, especially neighbors, may be assumed to observe a slight increase in the case of no adoptions in contiguous states, as this means a relative decrease in relocation costs for local firms. Reassuringly, I find that all adopter states would have observed a lower outflow without imposing the tax. Connecticut observes the largest reduction of firm outflows with 6.87%, followed by both Massachusetts and Montana with 5.26%. Table 14, Figures A8 and A9 report, that almost any other state observes an increase in outflows due to the due to an absence of the tax adoptions. This can be interpreted as evidence for bilateral relocation costs associated with the adoption of the income tax. In other words, firms may have opted for leaving their state for one of the adopter states but decided not to after the change in tax policy. Figure A10 depicts the counterfactual changes in bilateral firm outflows for each adopter state. In line with the effects for total outflows, I find that bilateral relocation flows decrease for each state pair where the origin state i is an adopter state in the 1910s.

Table 14 – Counterfactual change in total relocation outflows if no state adopts in the 1910s

Conditional General Equilibrium					
1910s Adopters		Top Non-Adopters			
Connecticut	-6.87%	Florida	1.92%	Indiana	1.40%
Massachusetts	-5.26%	Louisiana	1.69%	Ohio	1.38%
Missouri	-3.60%	Delaware	1.60%	Texas	1.34%
Montana	-5.26%	Georgia	1.59%	Oregon	1.33%
New York	-4.44%	Arizona	1.55%	New Mexico	1.30%
North Dakota	-2.74%	Utah	1.52%	Arkansas	1.24%
Virginia	-3.81%	Alabama	1.51%	Idaho	1.24%
Wisconsin	-3.93%	Tennessee	1.42%	Mississippi	1.92%

Notes: Figures are denoted in percentage changes of outflow due to the absence of tax, computed by $[(\sum_{j=1}^{J}(\widehat{y}_{i,j}^{cf} - \widehat{y}_{i,j}))/(\sum_{j=1}^{J}\widehat{y}_{i,j})] \times 100$. Adopters are sorted ascending, non-adopters are sorted descending.

Yet another counterfactual exercise is the case where all the eight early adopter states indeed introduce the income tax except for one state. For this analysis, I define

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

where $\mathcal{I}$ defines a set of the eight adopter states. Here, it is reasonable to presume strong differences between the counterfactual and the baseline for state i, whereas all other states should observe minor changes. Results of the exercise are reported in Figure A11. line with earlier findings, the one state that did not adopt the tax in the counterfactual observes a substantial drop in outflows, again indicating the strong effect of the tax adoption, even in a conditional general equilibrium framework. Another interesting finding is, that for most adopter states, there seems to be slight spillovers of the tax effects onto neighboring states. This indicates that geographic proximity and political opportunity (Berry and Berry [1992]) seem to play a role.

8 Further Insights and Robustness

Are tax adoptions driving firm closures? Another important component to the story is whether the tax adoption is related to firm closures. While the data does not directly allow observing closures, I utilize individuals switching from being coded as an employer to an employee with the course of a decade. This allows me to identify the number of closures, which I find no indication for being associated with the tax adoptions. A full discussion can be found in Appendix E.

Alternative Policy Identification The main specification uses an approach by Beverelli et al. [2018], in order to create a bilateral policy variable, which still captures a unilateral policy choice, i.e. state i's decision to adopt the corporate income tax. The model does not allow creating a bilateral variable from a linear combination of two unilateral policies, i.e., the difference. Instead, I can define $\tau^{alt}_{i,j,t} = \mathbb{1}(z_{i,t} > z_{j,t})$. The advantages of using this policy variable go beyond the scope of robustness. While the main specification is meant to capture the push effect of the new tax on firms regardless of the destination state, an implicit assumption of this channel is, that the destination state does not adopt the tax. As the majority of the tax was not imposed simultaneously, this assumption is fairly probable to hold true. The newly created policy variable further allows investigating this assumption, under which $\widehat{\beta} \approx \widehat{\beta^{alt}}$. The results of this exercise can be found in Table A8. All results and insights of the main specification go through, and the point estimates are almost identical. This allows me to support the claim that the estimated effects are drive by state dyads with opposing introduction decades.

Traditional Policy Variable I further repeat *Model (3b)* for the full specification, with policy variation across adopters. As already shown in Table 7, the effects measured with this policy variable are noticeably larger. This observation is repeated for all four aggregated sectors (see Table A8). As previously discussed, this can be expected as the traditional policy variable measures both the effects of the introduction and the involved revenue increases in the medium run. Here, it is conceivable that the effects are not only driven by outflows, but rather are a combination of both in- and outflow increases due to the opposing effects captured by the medium run policy.

Are the results driven by a single state? Additionally, to further exploit the interstate heterogeneity, the model estimation can be repeated while always leaving out one adopter state. This Jackknife procedure helps me to inspect whether the main results are driven by a single state. Results of this robustness exercise are collected in Figure A12. Reassuringly, the exclusion of no state leads to a statistically significant difference in the point estimate.

A related robustness check involves the state of Washington. The case of this state is rather unclear. The National Industrial Conference Board [1930] includes it in the states adopting in 1929, the Advisory Commission and American Council on Intergovernmental Relations [1995] however does not. The reason is that the reports are from 1929, where a tax seemed to be finalized. However, political pressure led to the tax being never introduced. I can include the introduction of the tax in Washington. As the model estimates average effects across all adopter states, the inclusion of Washington could potentially change the point estimates. The inclusion of a state that never really adopted the tax would therefore downward bias the results. Table A8 reports the results of including Washington in $\tau_{s,i,j,t}$. Indeed, the inclusion of this state seems to slightly downward bias the estimates.

Adoptions at the End of a Decade Further, I can investigate whether the introductions had a delayed effect by including North Dakota (1919 introduction) in the adoption group of the 1920s, and exclude California, Georgia, Missouri and Oregon from the policy variable. As reported in Table A8, all results of the main model go through, suggesting that the effects are mostly driven by states adopting the tax during rather than at the end of a decade. This result is consistent with the assumption that most firms need more than a year to relocate.

Staggered Gravity Recent advances in the literature including Nagengast and Yotov [2024] and Wooldridge [2023] suggest the usage of a framework similar to Callaway and Sant'Anna [2021], such that the estimation approach allows for dynamic treatment effects. In contrast, the baseline 4WFE model estimates an average treatment effect assuming the tax adoptions to affect firm relocations similarly in the 1910s and 1920s. The approach further allows then to use both not yet and never treated observations to be part of the control group. Analogously to Nagengast and Yotov [2024], I use a standard DiD framework with the traditional policy variable (*Model 3b*), however now with the approach introduced by Wooldridge [2023] to allow for treatment effect heterogeneity. I find that the adoptions in the 1910s seem to drive the results estimated with a 4WFE model such as the baseline. Additionally, the event study suggests that the long run effects of the tax adoptions may be larger. This discussion is however beyond the scope of this analysis, as it requires a numerical differentiation between in- and outflows.

Southern State Cluster While the corporate taxes in the South were not coordinated to be introduced in the same year, many of them adopted the tax in the 1920s. In order to control for this decennial cluster, I create a control variable that interacts a 1920s identifier, i.e. $\mathbb{1}(t(b) = 1920)$, with an Southern origin-state indicator such that $\mathbb{1}(\text{South}_i > \text{South}_j)$, where South_i indicates whether state i is in the South. I use the definition from the census to include Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia and West Virginia. Controlling for this bilateral indicator does not change the results, as shown in Table A8.

9 Conclusion

This paper aims to shed light on a surprisingly understudied aspect of empirical tax policy evaluations: the effects of corporate tax adoptions on firm relocations. Different to changes in tax rate differentials across locations, the introduction of a new tax can be interpreted as a shock to the profit function, as it potentially drastically increases the costs associated with the firm's current origin. Typically, empirical investigations of such changes of tax policies on the extensive margin are met with several difficulties, such as legal constraints to firm relocations. The paper closes this gap in the literature by investigating the sequential introductions of corporate income taxes in U.S. states between 1900 and 1930. The unique historical context and data allows for a clean identification of the tax shocks in an environment of very little other constraints on corporations. A structural gravity framework allows estimating changes in firm relocations caused by the state income tax adoptions. The main specification finds an increase of interstate firm flows by 13.02%. These changes in relocations are driven by outflows, as government spending did not increase due to these early adoptions. Variations in taxed sectors across adopter states further reveal that the effects are strong for the manufacturing, mercantile, service and public utility sector, however no effects are found for the immobile financial sector. Adopted tax rates matter as an increase of the introduced minimum tax rate by 1 ppt. increases interstate flows by 4.75%, suggesting a high sensitivity to these tax introductions. I find strong support that the findings are not influenced by personal tax adoptions or the migration of interstate workers, indicating that smaller unincorporated businesses did not react as strongly and changes in labor input costs did not play any important role for corporations. The absolute changes in firm relocations between state pairs are more significant for bordering states, suggesting the effects of the tax adoptions on firm relocations to be non-linear in state distance. I find overall greater effects for firms situated nearer to the border within their originating state. Additionally, the impact is more pronounced for relocations from urban to rural states. A decomposition into urban and rural states further reveals that agricultural businesses seem to accelerate their relocations from urban to rural, and non-agricultural manufacturing and mercantile operations from rural to urban states. The counterfactual analysis, which accounts for conditional general equilibrium effects assuming the taxes were not introduced, reveals significant negative effects on total firm outflows in the adopting states, while nearly all other states experience positive changes.

Finally, the results are robust to a battery of robustness checks, including alternative policy definitions and state-specific exercises.

This study provides crucial insights into the mobility of firms in response to corporate tax adoptions, highlighting the importance of considering firm behavior when designing tax policies. Policymakers should be aware of the potential for significant firm outflows following the introduction of corporate taxes, particularly in states, or countries, with substantial manufacturing and mercantile sectors. Future research could further explore the long-term impacts of these tax policies on state economies and investigate the effects in different historical and geographical contexts. The study underscores the need for a nuanced approach to tax policy that carefully weighs the potential benefits of increased tax revenue against the possible economic disruptions caused by firm relocations.

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A Tables

Table A1 – Description of the businesses included in the respective 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: Breakdown of industries is taken from the codebook of the full count census.

Table A2 – 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: Figures exlude i, i-pairs. I define the *Mobility Rate* as the average outflow share of the average sample population. Note, that this is not the dependent variable of the main specification. The dependent variable is *Pair Outflows*.

Table A3 – 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, Larch and Yotov* [*2015*])	no	no	yes
Observations	110,592	54,401	54,397
McFadden Pseudo R^2	0.323	0.994	0.995

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. To ensure convergence of the estimates, statistically seperated observations are excluded based on fixed effects and Clarkson and Jennrich [1991], and singletons are excluded using Correia [2015].

Table A4 – 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. All specifications include state-pair, state-decade and interstate-decade fixed effects. 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]. The flatness measure is multiplied by $100/10$ in order to interpret the coefficient as a change in relocation flows due to a 10 ppt. increase in the level of rate flatness. Sector-specific results are reported in one row, where the respective model defines the use of the regressor.

Table A5 – Regression of effects decomposed 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. To ensure convergence of the estimates, statistically seperated observations are excluded based on fixed effects and Clarkson and Jennrich [1991], and singletons are excluded using Correia [2015]. The absolute changes are computed as $(\exp(\widehat{\beta}_S) - 1) \times \bar{y}_S$.

Table A6 – Regression of effects w.r.t. years since adoption

	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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. All specifications include state-pair, state-decade and interstate-decade fixed effects. 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]. Note that the linear coefficients reported are estimated in a regression with equation (14) excluding the polynomial term $\text{YSA}^2_{i,j,t}$.

Table A7 – 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)
$\tau_{i,j,t}$ × 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)
$\tau_{i,j,t}$ × 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)
$\tau_{i,j,t}$ × 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)
$\tau_{i,j,t}$ × 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. Specification includes state-pair, state-decade and interstate-decade fixed effects. 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].

Table A8 – 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	

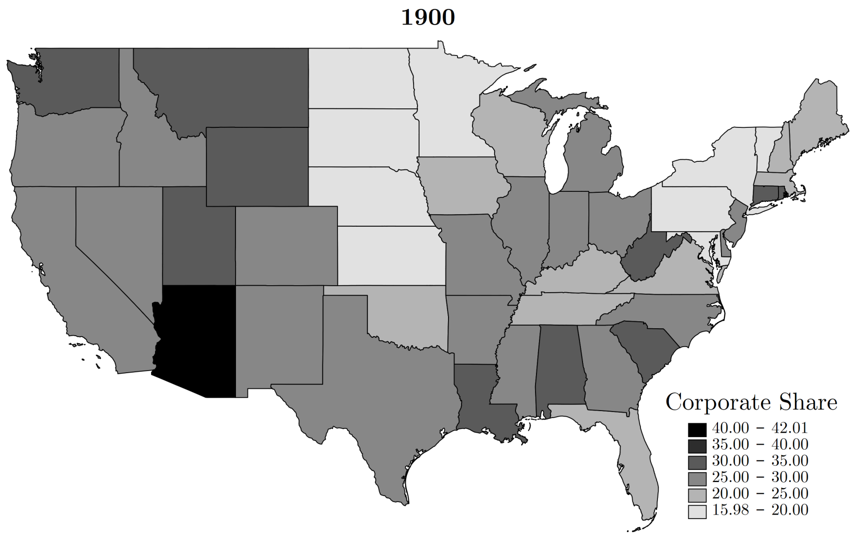
Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. All specifications include state-pair, state-decade and interstate-decade fixed effects. To ensure convergence of the estimates, statistically separated observations are excluded based on fixed effects and Clarkson and Jennrich [1991], and singletons are excluded using Correia [2015].

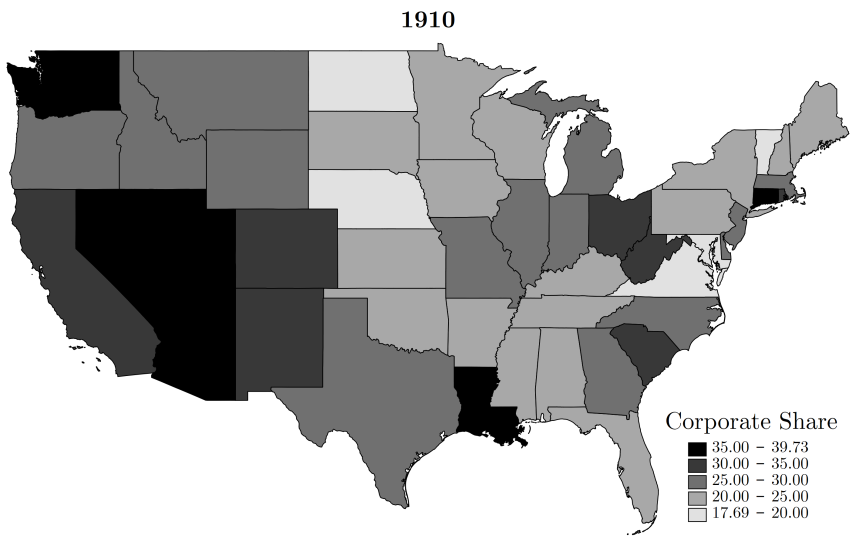
Table A9 – Estimation results for 4WFE and staggered gravity

	Model(3b): 4WFE	Heterogeneous Treatment Effects		
		Group 1: 1910s Adopters		Group 2: 1920s Adopters
Panel A: All				
		1910s	1920s	1920s
$\tau_{i,j,t}^{trad}$	0.295***	0.633***	0.660***	0.162***
	(0.041)	(0.104)	(0.124)	(0.045)
Panel B: By Adoption Group				
ATT		0.646***		0.162***
		(0.113)		(0.045)
Observations	6,765			6,765
McFadden Pseudo R^2	0.997			0.997
Panel C: Dynamic Aggregation				
			at t	at $t + 10$
Event Study			0.398***	0.660***
			(0.057)	(0.124)

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses underneath the estimated coefficients are clustered by state-pairs. Standard errors for the ATT and the event study were estimated using the Delta-Method. The event study coefficients only provide the post-treatment ATT aggregations.

Figure A1 – Share of incorporated manufacturing firms





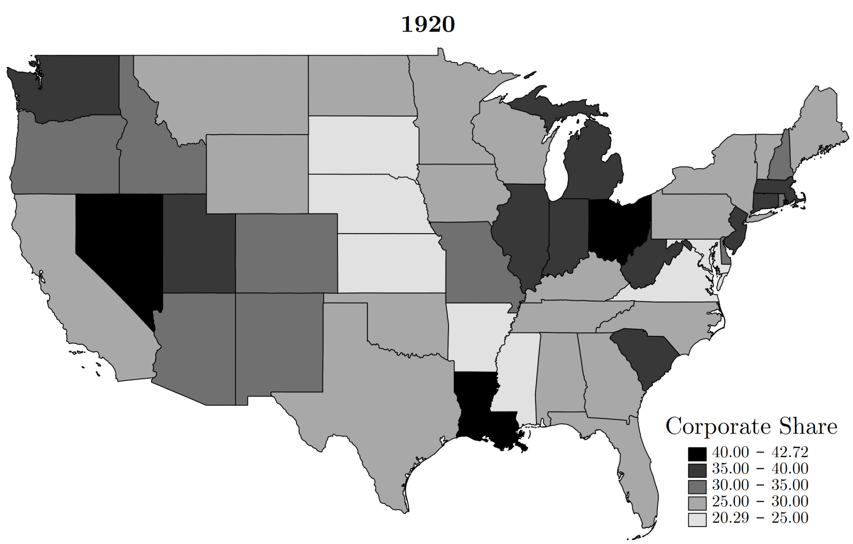


Figure A2 – Estimates effects of tax adoptions on state-level revenues

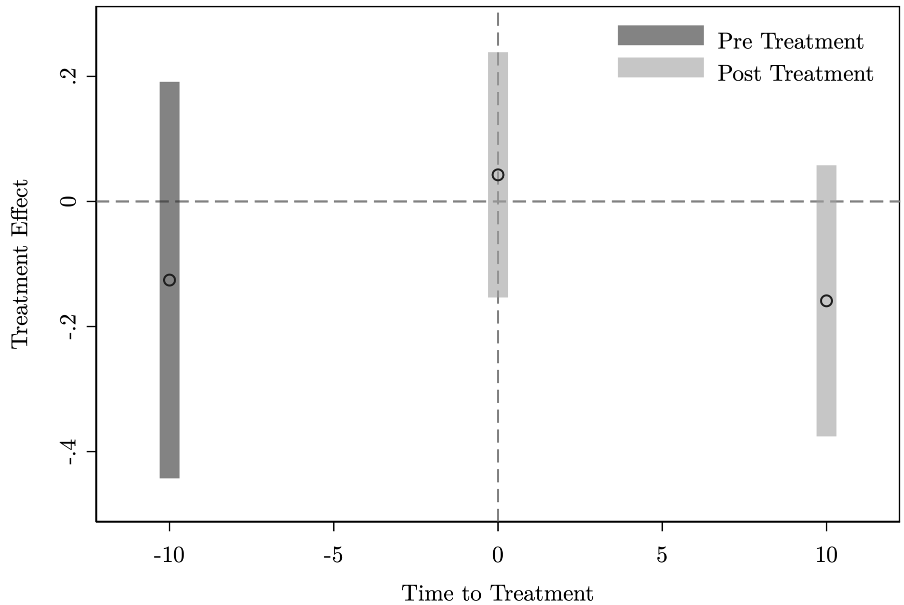
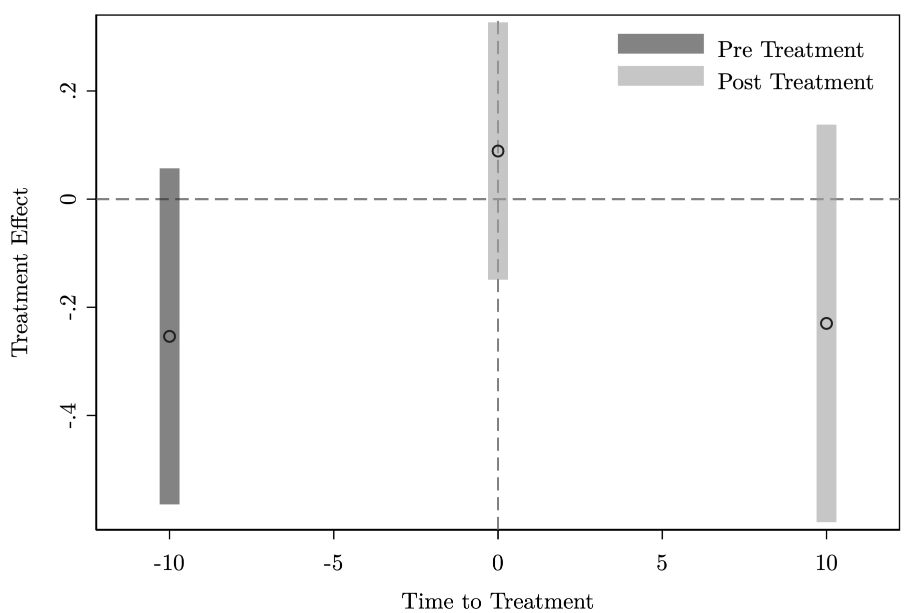


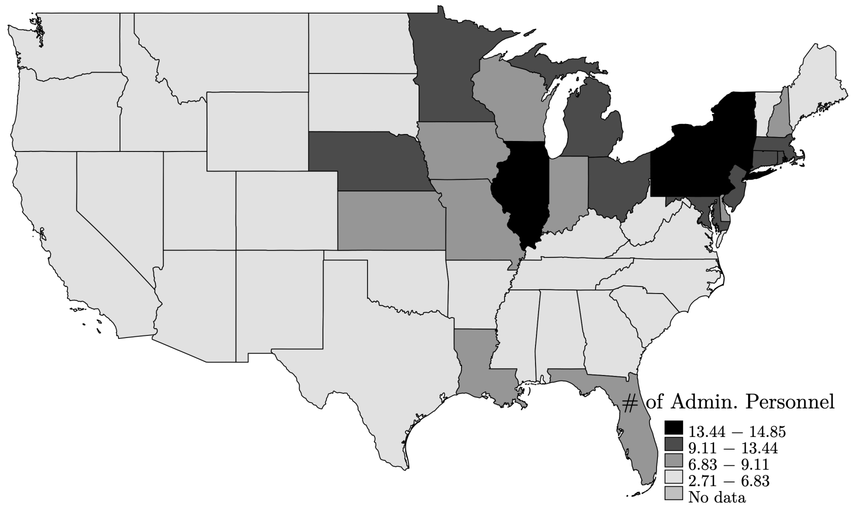
Figure A3 – Estimates effects of tax adoptions on state-level expenditures



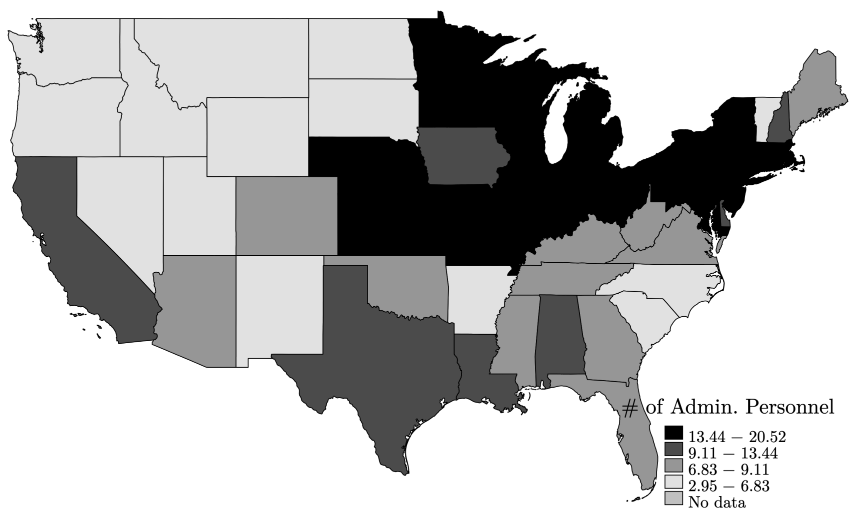
Notes: Effects are estimated regressing the logarithm of state-level revenues and expenditures on the tax adoption. Callaway and Sant'Anna [2021] is used for estimation, and not yet treated states are included in the control group. Note that this approach requires treated states to remain treated, i.e. $z_{i,t}^{trad}$. Specification includes a state and decade fixed effect. Fiscal data is derived from Cassidy, Dincecco and Troiano [2024]. Number of observations is $3 \times 48 = 144$.

Figure A4 – Average number of administrative personnel in each corporation

1910



1910



1920

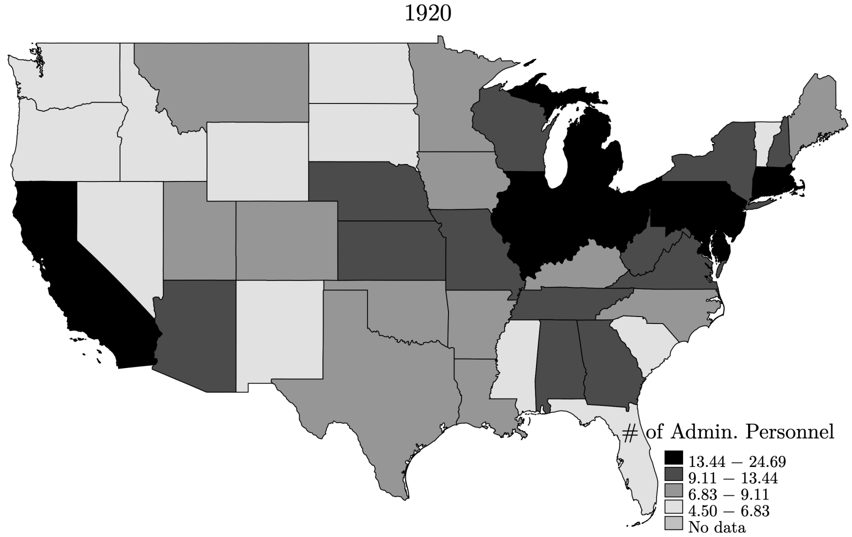
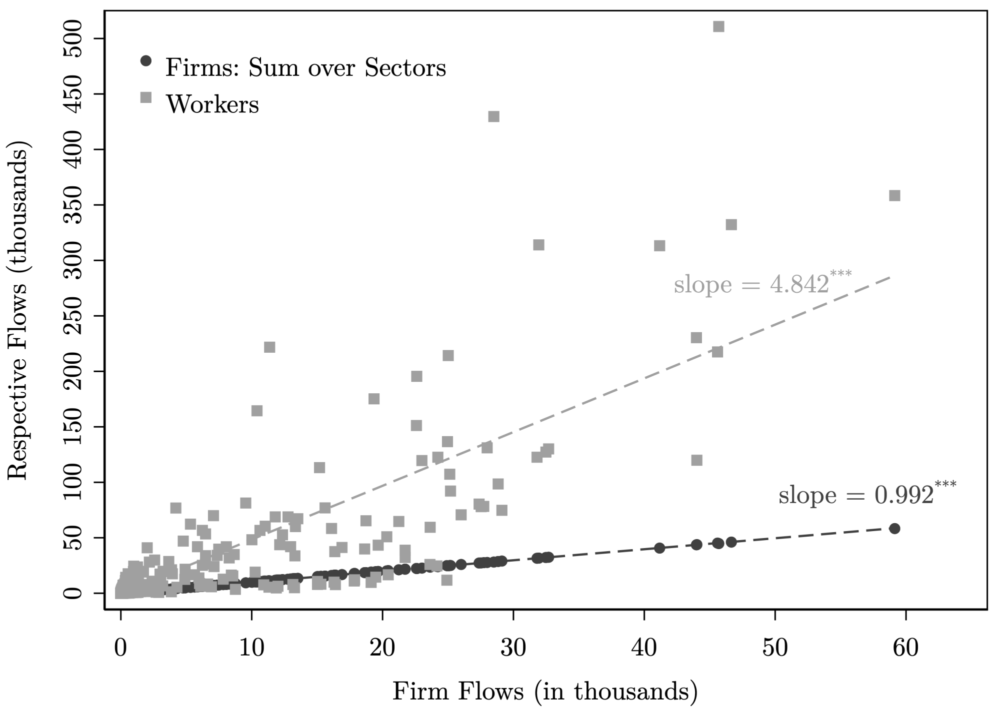


Figure A5 – Scatter and correlation between bilateral firm and worker flows



Notes: Darker dots and line represent the scatter and correlation between the firms counted in the linked data and the sum of all firms of each sector. The slope indicates strong correlation. Lighter dots and line show the number of worker flows associated with the number of firm flows. On average, around 5 workers are moving with each firm relocation. Note that this is a strictly non-causal interpretation and should be merely interpreted as a rough association.

Figure A6 – County-level mobility rates for 1900s, 1910s and 1920s

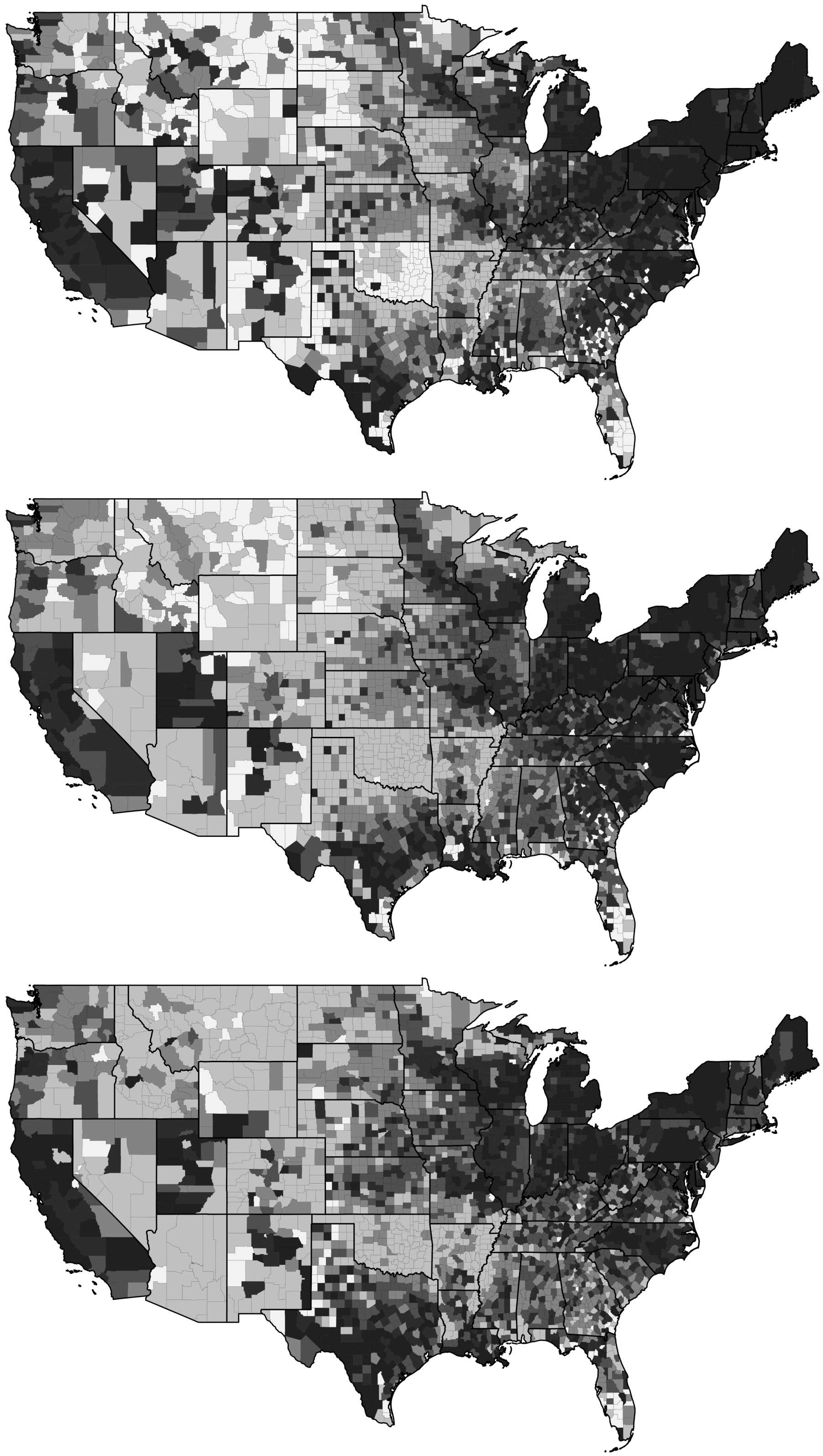
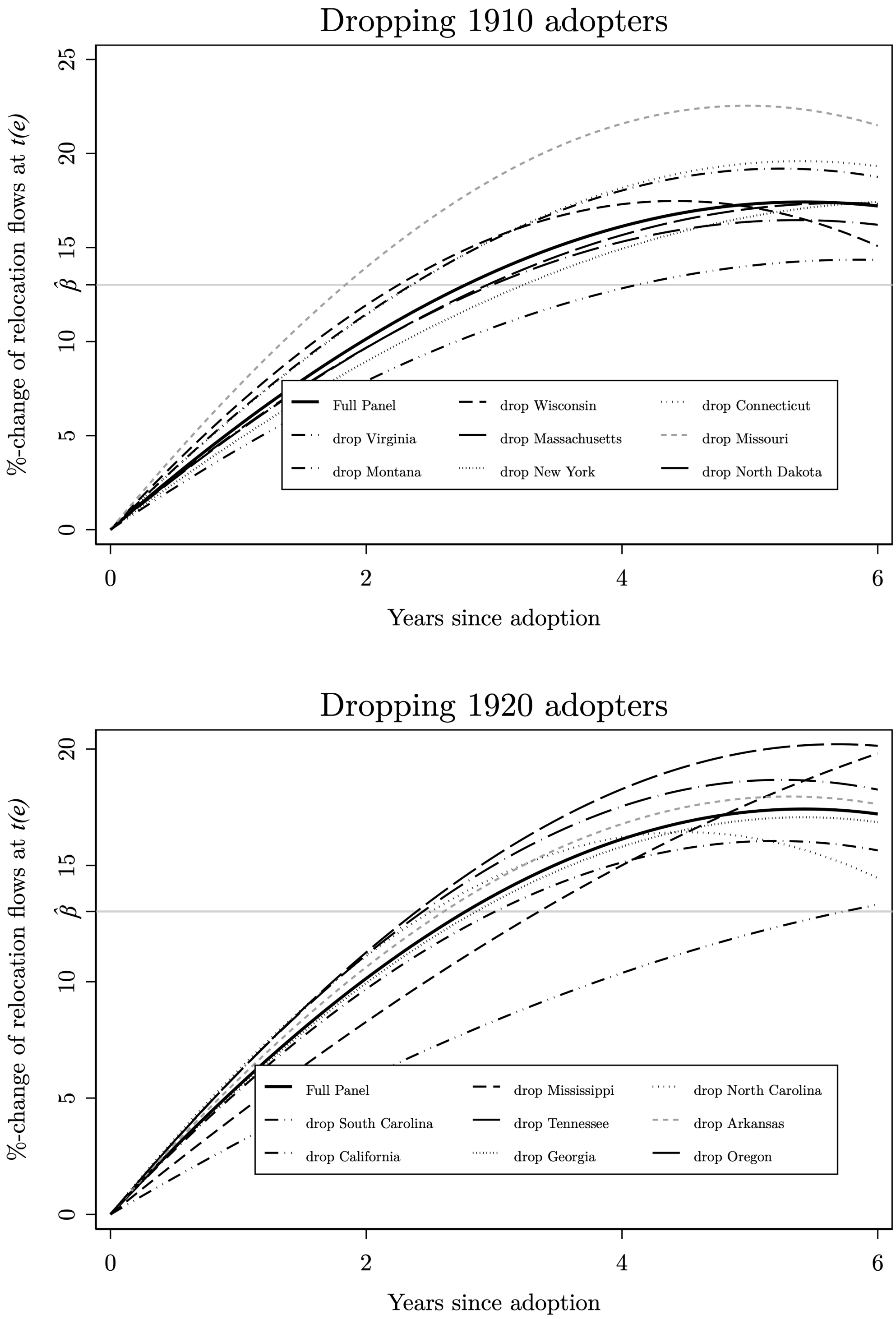
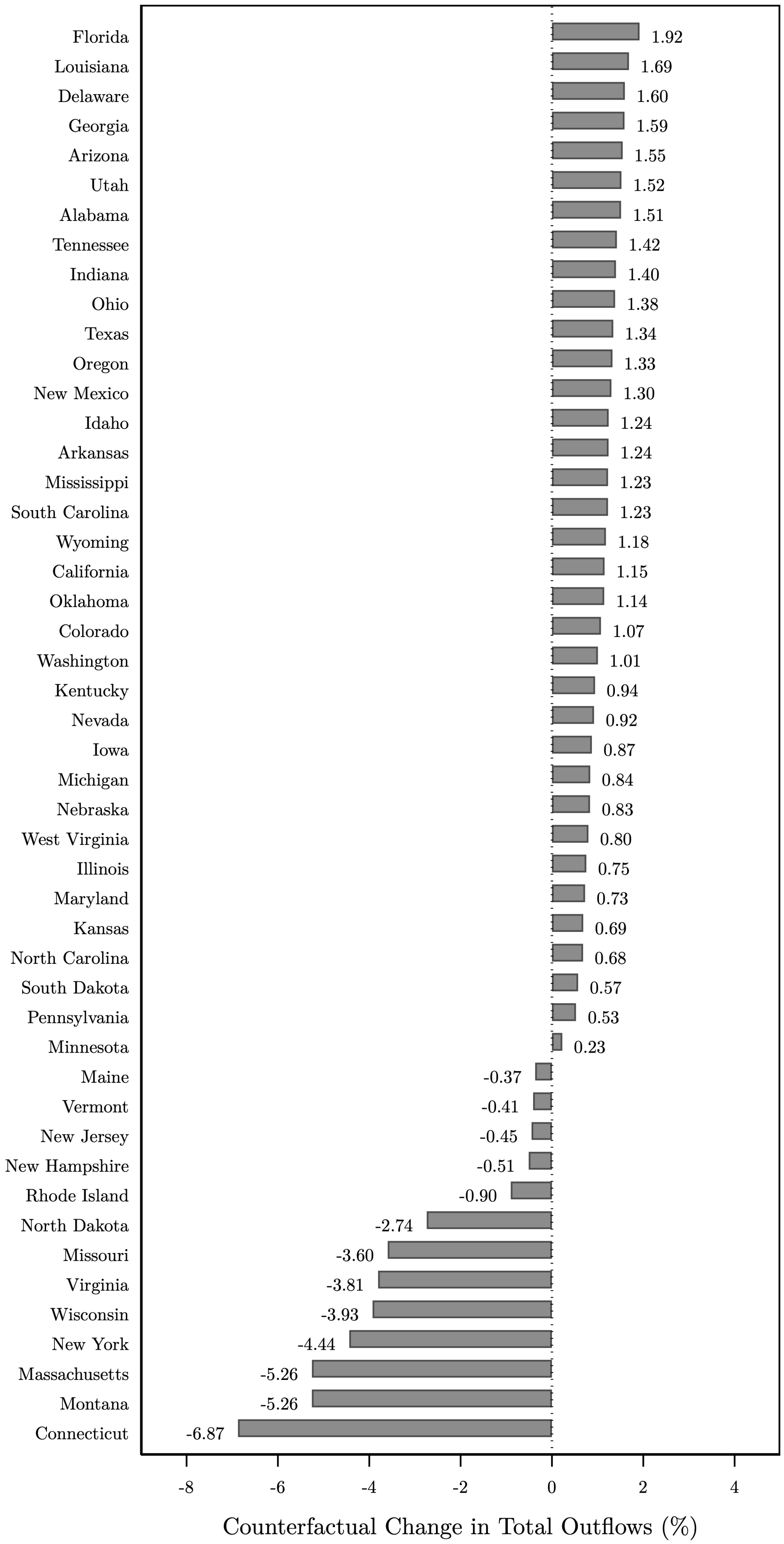


Figure A7 – Robustness of persistency effects of tax adoptions using Jackknife



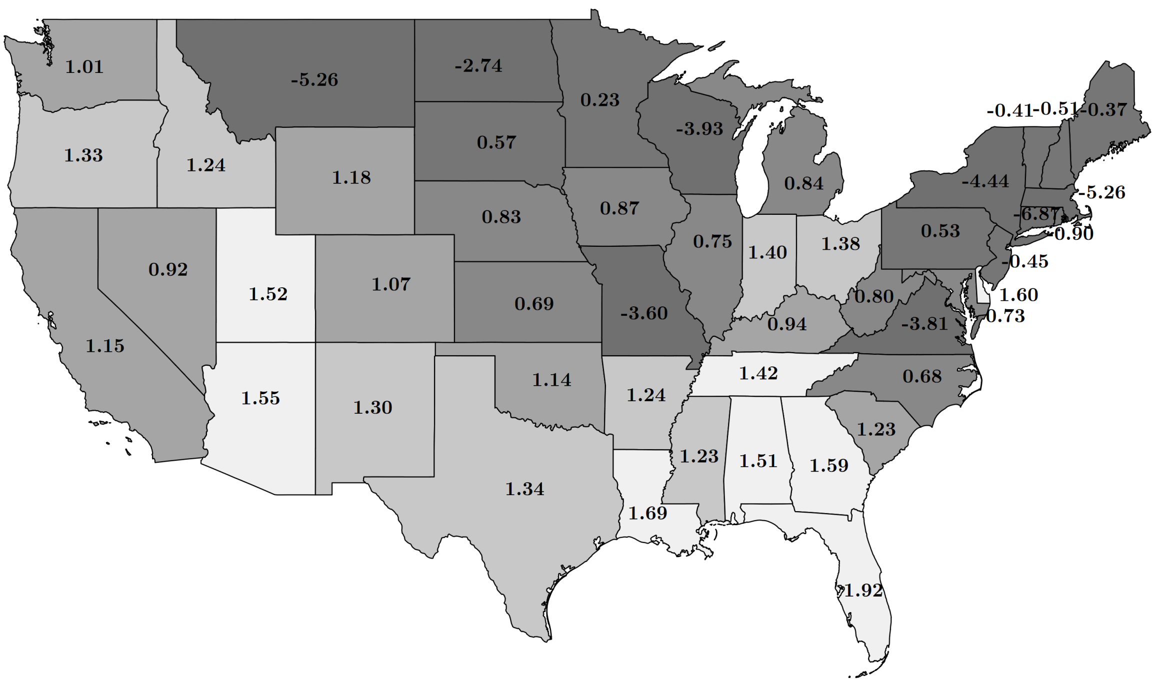
Notes: Jackknife effects are estimated by regression model from equation (14) by dropping each adopter state at a time. The overall effect is plotted as a reference in both figures.

Figure A8 – Counterfactual change in relocation flows if no state adopts in the 1910s



Notes: The labels report percentage changes so that $[(\sum_{j=1}^{J}(\widehat{y}^{cf}_{i,j} - \widehat{y}_{i,j}))/(\sum_{j=1}^{J}\widehat{y}_{i,j})] \times 100$. Figure depicts changes for the 1910s only and bars are sorted by size of the counterfactual change.

Figure A9 – Counterfactual change in relocation flows if no state adopts in the 1910s

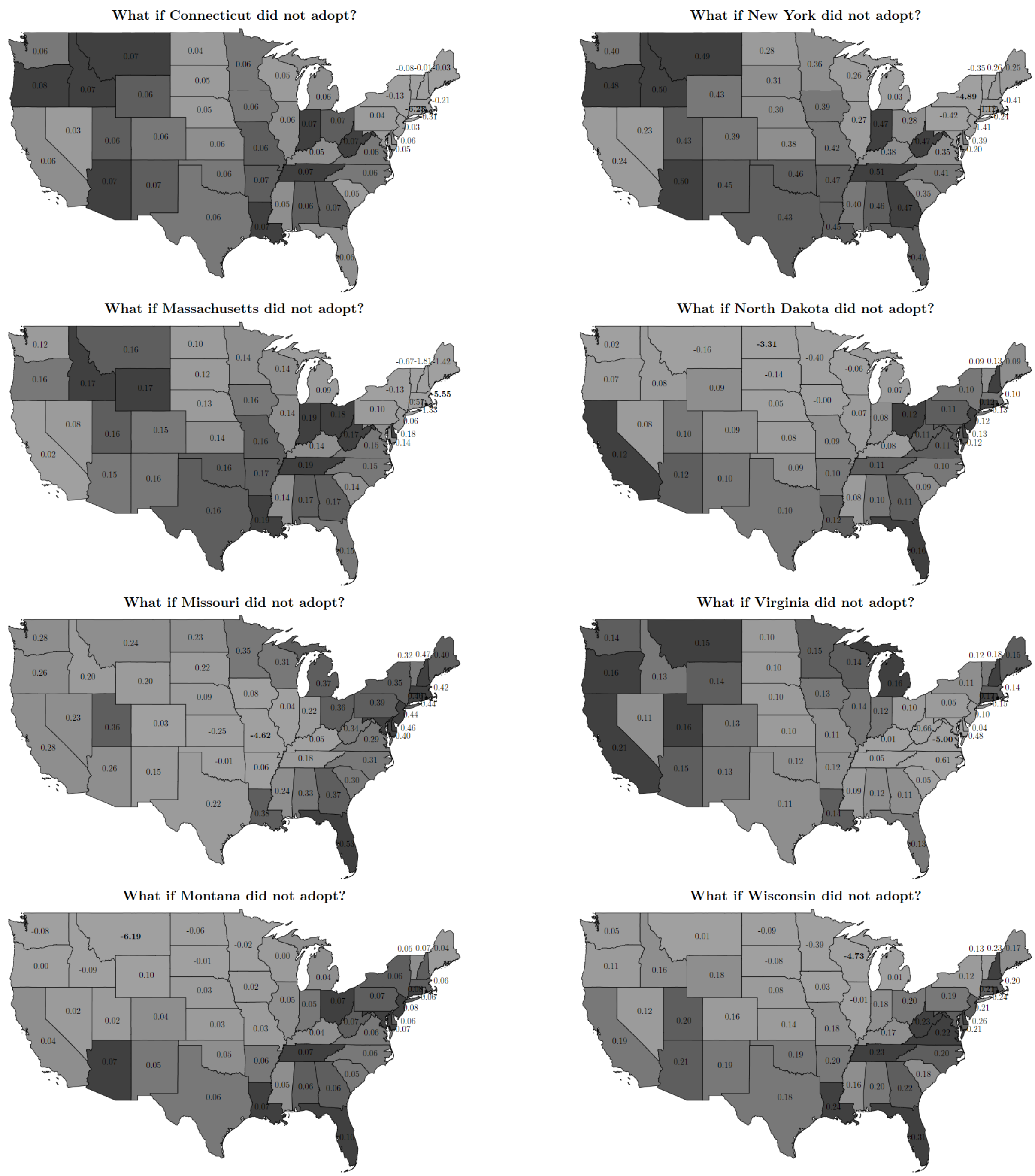


Notes: The labels report percentage changes so that $[(\sum_{j=1}^{J}(\widehat{y}_{i,j}^{cf} - \widehat{y}_{i,j}))/(\sum_{j=1}^{J}\widehat{y}_{i,j})] \times 100$. Figure depicts changes for the 1910s only.

Figure A10 – Counterfactual change in bilateral relocation flows by adopter states

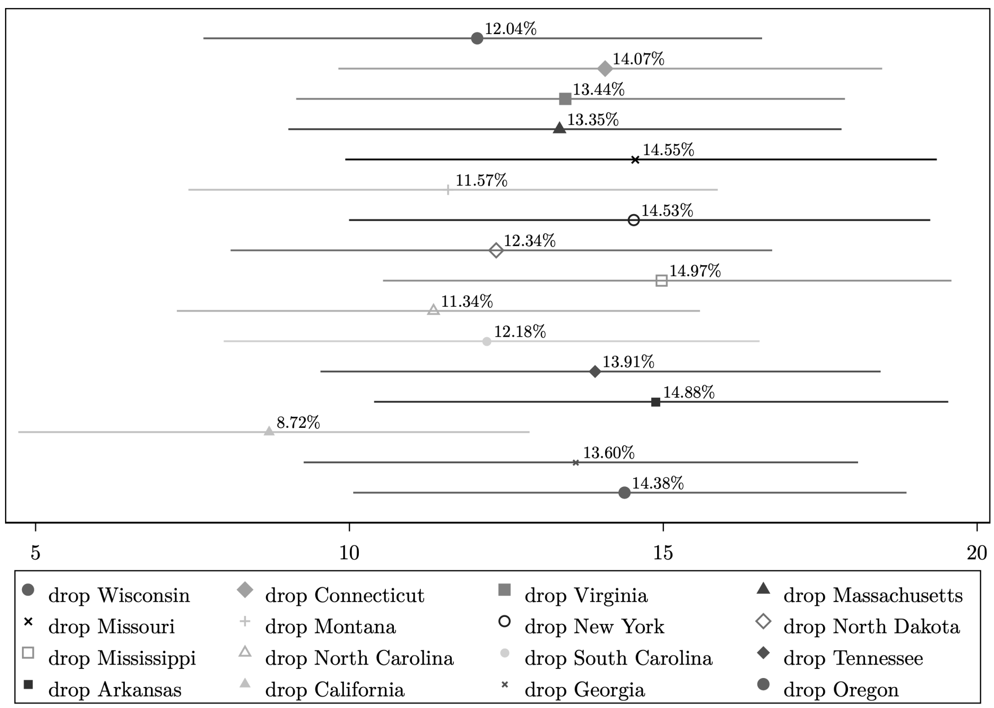
Notes: The labels report percentage changes so that $[(\widehat{y}^{cf}_{i,j} - \widehat{y}_{i,j})/(\widehat{y}_{i,j})] \times 100$ if $i \neq j$. Figure depicts changes for the 1910s only.

Figure A11 – Counterfactual change in relocation flows by adopter state not adopting



Notes: The labels report percentage changes so that $[(\sum_{j=1}^{J}(\widehat{y}_{i,j}^{cf(\mathcal{I})} - \widehat{y}_{i,j}))/(\sum_{j=1}^{J}\widehat{y}_{i,j})] \times 100$. Figure depicts changes for the 1910s only.

Figure A12 – Estimates repeated for subsets leaving out one adopter state



Notes: CI = 90%. 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]. Estimates stem from repeated computations and are hence not (directly) statistically comparable.

B What drives the adoptions on a state level?

While contemporary sources such as the National Industrial Conference Board [1930] proclaim the failure of the property tax as the main reason for the adoptions, it is essential for the understanding of the historical context of the natural experiment to further explore potential state-level drivers of the adoption. Note that this analysis does not stand in contrast to the parallel trend assumption in Section 5, as the dyadic panel structure allows controlling for state-time fixed effects in the gravity estimation. In order to identify potential driving factors of the adoption, I estimate an explorative model of the following structure

$$z_{i,t(e)} = \beta_0 + \sum_{n=1}^{N} \beta_n \ln(\text{TV Predictor}_{i,t(b)}^{n}) + \sum_{m=1}^{M} \beta_m \ln(\text{Const. Predictor}_{i}^{m}) + \textbf{Decade FE} + \varepsilon_{i,t}, \qquad (1)$$

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 time varying (TV) explanatory variables are measured at the beginning of the decade $t(b)$. As all continuous predictors are denoted on the logarithmic scale, I can interpret β_n as percentage point changes in the probability of state i adopting the corporate income tax. As reported in Table B1, the selected variables seem to not indicate any state-specific patterns unique to adopter states. This observation is in line with the general assumption of randomness of the tax policy. The table is categorized into different groups of variables. To include unemployment, I have to drop the first decade from the sample, as labor market variables are unavailable for the 1900 census. The most important group being firm-related variables includes firm outflows and shares by type of firms. Firm outflows do not significantly impact tax introductions. This is important as it helps to presuppose the absence of reverse causality. The only significant regressor of the model is the corporate share of manufacturing firms. I find that a 1 ppt increase in the corporate share decreases the probability of adopting the corporate income tax by roughly 0.3 ppt. This suggests a level of influence of corporations through a larger and hence potentially larger political lobby.

Table B1 – Explorative model of potential drivers of the tax adoption

	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 —"—	0.036	0.026
	(0.072)	(0.072)
Public Utility —"—	-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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Note that all regressors are in logarithmic terms. Fiscal data on revenues and expenditures are provided by Cassidy, Dincecco and Troiano [2024]. Data on political variables are collected manually from National Archives and the National Governor Association. *Republican Governor Dummy* indicates whether a state has a Republican governor while *Republican Presidential Dummy* indicates if a state voted Republican in the previous presidential election. Corporate data is provided by the census of manufactures.

C Corporate Activity

Based on results from Liu [2014], it can be shown that corporate activity decreased with the introduction of state level income taxation. Digitizing parts of the census of manufacturer's general statistics allows collecting the state level corporate share of manufacturing enterprises for 1904, 1909, 1919 and 1929. I match this information with the total state-level manufacturing outflows from the individual census.[44] This allows me to explore the share of the effect of the tax introductions on the share of corporations that is mediated by firm relocations, denoted as F. [Baron and Kenny, 1986] In other words, how much of the decrease in corporate activities can be associated to the outflow responses captured by the main model. I start by repeating a similar analysis as Liu [2014], where I first estimate

$$\text{Corporate Share}_{i,t} = \beta z_{i,t} + \textbf{Controls} + \textbf{FE} + \varepsilon_{i,t}, \tag{1}$$

where $z_{i,t}$ is an adoption dummy for state i during decade t. The respective corporate share is always taken from the end of the decade, as it captures the *realized* share of t. Controls include the full count total population and the log of state level total expenditures, both captured at the start of the period. In equation (1), $\widehat{\beta}$ captures total estimated effect of the tax adoption on corporate activity. Next, I estimate a mediator equation

$$\log(\text{Total Outflow}_{i,t}) = \theta z_{i,t} + \textbf{Controls} + \textbf{FE} + \tilde{\varepsilon}_{i,t}. \tag{2}$$

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

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

Utilizing estimates from equations (1) to (3) allows to compute $F = (\theta \times \rho)/\beta = \left(\beta - \breve{\beta}\right)/\beta$. Results of the exercise can be found in Table C1.

[44]Note that these figures include non-incorporated firms, hence I interpret the following estimation as lower bound.

Table C1 – Estimates for corporate activity and firm relocation

	Corporate Share$_{i,t}$	$\log(\text{Total Outflow}_{i,t})$
	(i)	(ii)
Equation (1): Total Effect		
$z_{i,t}$: β	-0.034 $(0.011)^{***}$	
Adjusted R^2	0.9032	
Equation (2): Mediator		
$z_{i,t}$: θ		0.1090 $(0.054)^{**}$
Adjusted R^2		0.9708
Equation (3): Direct Effect		
$z_{i,t}$	-0.031 $(0.010)^{***}$	
$\log(\text{Total Outflow}_{i,t})$: ρ	-0.027 $(0.016)^{*}$	
Adjusted R^2	0.9075	
Controls	Yes	Yes
Fixed Effects	Yes	Yes
Observations	144	144

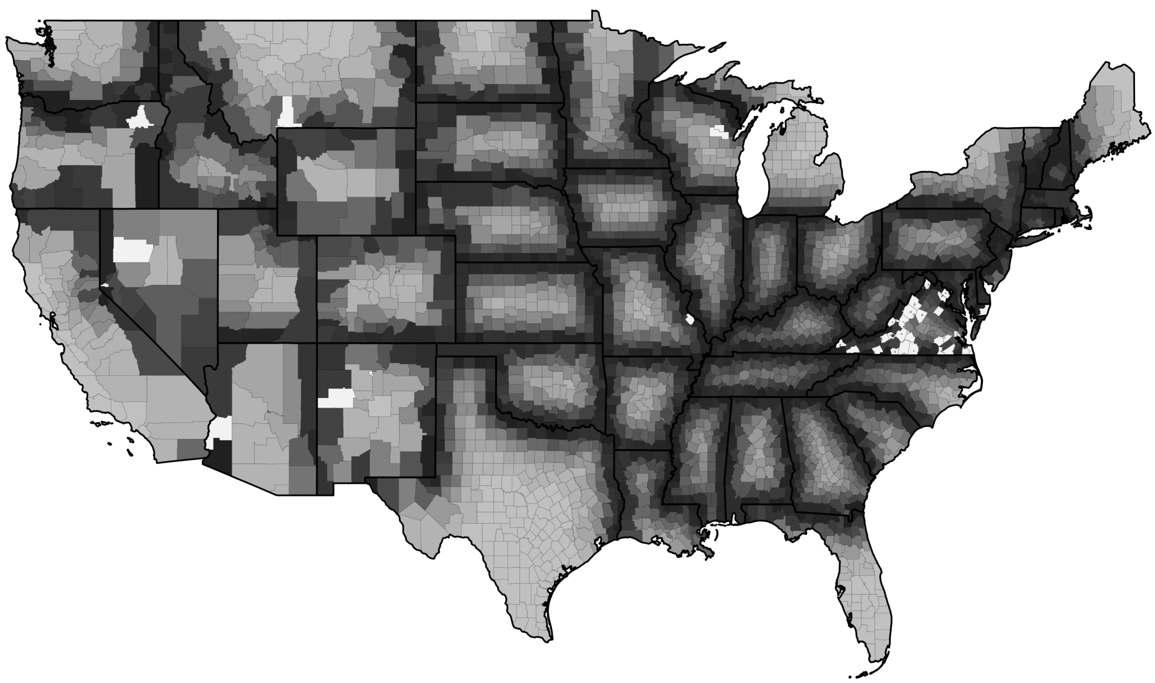
Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors clustered by states. Specification includes a state fixed effect ρ_i and a census link fixed effect ρ_t.

In line with Liu [2014], the corporate share is decreasing with the adoption of the corporate income tax. Following Baron and Kenny [1986], this allows for a mediation analysis, where the next step is to identify the effects of the tax adoption on changes in state-level outflows. Similar in magnitude to the bilateral effects in the main part of the paper, state-level outflows increase by about $(\exp(\widehat{\theta}) - 1) \times 100 \approx 11.52\%$. Finally, the direct effect of the tax adoption on corporate shares, mediated by outflows, is estimated to be slightly smaller than the effects in equation (1). This suggests partial mediation, quantified by $\widehat{F} \approx 0.09$. In words, this means that around one tenth of the decrease in corporate activity can be explained by firm outflows.

D County-level: Distance to the Closest Border

To code each county's distance to all state borders, I use data provided by Holmes [1998]. This allows to merge in information of every county $w(i)$'s distance to each neighboring state $k \in \mathcal{B}_i$ of state i. To measure the distance, two coordinates are needed. First, the centroid of the respective county, which defines the arithmetic mean position within the county. 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 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 newly created variable, I plot a U.S. county map (Figure D1).

Figure D1 – Distance measure to the closest state border



Notes: Darker shades reflect a lower level of $\text{MD}_{w(i)}$, capturing less distance to the border.

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.

E Approximating Firm Closures

The empirical model utilizes data from employers whose employment classification is established at the onset of a decade. This approach is chosen to align with the central aim of this investigation: examining whether the implementation of corporate income taxation prompts firms to relocate out of the state. However, it is crucial to evaluate whether employers relocated or transitioned into wage-earners, which implies that their firms ceased operations altogether. Consequently, an important inquiry revolves around whether the adoption of corporate income taxation resulted in firm closures. Due to data availability, I need to make a strong assumption: when an employer transitions into wage-earner status, his firm ceases to exist. With the complete count linked census, I can count the number of employers becoming employees, and whether they moved across state borders. The model can then be states as

$$\text{\# of Switchers}_{i,t} = \beta_1 \text{Tax Adoption}_{i,t} + \beta_2 \text{Migrant}_{i,t} + \beta_3 \text{Interaction} + \mathbf{FE} + \varepsilon_{i,t}, \tag{1}$$

where Switcher$_{i,t}$ define employers who lived in state i at $t(b)$ who became wage-earners by $t(e)$. The indicator Migrant$_{i,t}$ is equal to 1 for the number of individuals who reside in $i \neq j$ by $w + 10$. I also define a corporate tax adoption dummy Tax Adoption$_{i,t}$ indicating whether state i adopts a tax adoption during decade t. I additionally include an interaction term of the main variables, as well as a decade and state fixed effect. Note that I exclude the observations between 1900 and 1910, as for this census link I have to approximate the employment status at the onset of the decade with the employment status at the end of the decade. Therefore, each state has four observations, two for each census link and Migrant$_{i,t}$ equal to one or zero. With 48 states, this yields 192 observations. I report the estimation results in Table E1. First, it appears that the adoption of the tax policy did not significantly influence the transition of individuals from employers to wage-earners. As such, I find no evidence to suggest that the introduction of corporate taxes had an immediate discernible impact on firm closures, assuming that the number of individuals transitioning roles serves as a reasonable proxy for such closures. Hence, I infer that firm closures are not related to the introduction of corporate income taxes. Additionally, I estimate the effects of migration on the transition from employers to wage-earners. Here, I find a strong and statistically significant negative effect, indicating that employers who move across state borders are more likely to stay employers in their respective destination state. This and the negative coefficient of the interaction term insinuates that employers deciding to move due to the tax adoptions will move with their operation.

Table E1 – Correlation between firm closures and corporate tax adoptions

	# of Switchers$_{i,t}$
Tax Adoption$_{i,t}$	-0.76
	(493.81)
Migrant$_i$	-1560.40***
	(371.71)
Tax Adoption$_{i,t}$ $\times$ Migrant$_i$	-284.66
	(705.50)
Observations	192
Adjusted R^2	0.7746

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors clustered by states. Specification includes a state fixed effect and a census link fixed effect.

F Introduction Elasticity Premium

The average introduction elasticity at the minimum rate is computed by $\hat{\beta}/2.5 = 5.21\%$ and Section 6.2 reports marginal adoption rates at 4.75%. Under a set of assumptions, the introduction premium to the flow elasticity, i.e. the additional effect coming from the adoption itself, can be approximated by the difference of these margins. This essentially translates to a kink in the tax elasticity at the time of the adoption shifting the elasticity upward. Figure F1 plots a stylized visualization of this premium shift. The estimate is a lower bound for two reasons. First, the estimated effects from the baseline model is a lower bound due to data limitations leading to the inclusion of non-incorporated firms in the sample. Second, the pure rate effect is approximated by the intensive margin estimated in Section 6.2, which itself is contaminated by the adoption. This exercise further assumes linearity in the marginal effect of the rate changes and separability the adoption effect. For simplicity, I further assume no change in the slope at the adoption.

Figure F1 – Stylized visualization of tax elasticity premium for on the introduction