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Abstracts

This thesis consists of three essays that explore the direct and indirect implications of fiscal decentralization for firm relocations, labor mobility, and discretionary fiscal policies.

The first chapter, *"Taxed Out? How Early 20th Century Regional Tax Adoptions Shaped Interstate Firm Relocations"*, estimates the causal effects of decentralized regional tax adoptions on firm relocations within the borders of a country. I rely on the unique historical setting of staggered and uncoordinated introductions of 16 state-level corporate income taxes in the early 20th century U.S. as a natural experiment. I use linked census data on employers and a gravity model to estimate interstate firm relocation responses to these tax adoptions. This framework allows me to account for the entire universe of time-varying location factors beyond taxation. With that, I quantify a 13.02% increase in firm flows between two states when the origin state adopts a corporate income tax of 2.5% on average. Heterogeneity in the introduced rates further allows to estimate the flow elasticity concerning a 1 ppt higher adopted rate to be 4.75%. The effects vary across sectors, finding that the main results are driven by manufacturing, mercantile, and service businesses.

The second chapter, *"A Taxing Journey: Tax Adoptions and Interstate Migration in the Early 20th Century"* asks whether individuals move from the introduction of personal and corporate income taxes. Looking at the same time period, we use linked individual census data to track bilateral migration between states for the entire U.S. for heterogeneous demographics. Employing a gravity estimation approach, we find that the adoption of personal income taxes increases bilateral migration flows by an average of 8.70%. These effects are especially strong for individuals in the labor force and young individuals with above median income. Our results are robust to other interstate migration trends and hold across different subgroups of the population. We conclude that income tax adoptions had substantial implications for interstate migration during this period, potentially operating through two distinct channels. First, the direct taxation of personal income influenced migration decisions. Second, the introduction of corporate income

taxes prompted business relocation, which, in turn, affected migration.

The third chapter of this thesis, "*Discretionary Policy under Fiscal Decentralization*", focuses on the empirical analysis of discretionary policy in environments of varying fiscal decentralization. Here, the objective is to analyze how fiscal decentralization affects the central and subnational discretionary responses of countries to a widening of the output gap (both in magnitude and effectiveness). Specifically, I ask (1), how does fiscal decentralization affect central governments' ability to deploy discretionary fiscal measures, and (2), what role do subnational governments play in macroeconomic stabilization under different federal structures? The main finding is that high levels of fiscal decentralization are associated with a reduction in the central government's response to a negative output shock. While a negative output shock elevates subnational discretionary spending, my findings suggest that the effectiveness in closing the output gap decreases with the level of decentralization. The results provide novel empirical support for potential negative externalities of fiscal decentralization on the macroeconomic stabilization capabilities of federal economies.

Zusammenfassung

Die vorliegende Dissertation besteht aus drei Kapiteln, welche direkte und indirekte Auswirkungen der fiskalischen Dezentralisierung auf Unternehmensverlagerungen, Migration und diskretionäre fiskale Politik.

Das erste Kapitel, *"Taxed Out? How Early 20th Century Regional Tax Adoptions Shaped Interstate Firm Relocations"*, schätzt kausale Effekte dezentralisierter regionaler Steuereinführungen auf Unternehmensverlagerungen innerhalb eines Landes. Hierbei stütze ich mich auf das einzigartige historische Fallbeispiel der gestaffelten und unkoordinierten Einführung von 16 bundesstaatlichen Körperschaftssteuern in den USA des frühen 20. Jahrhunderts als natürliches Experiment. Ich verwende Zensusdaten von Arbeitgebern in einem Gravitationsmodell, um zwischenstaatliche Unternehmensverlagerungen als Reaktion auf diese Steuereinführungen zu schätzen. Dabei ermöglicht dieser Rahmen alle anderen zeitvariierende Standortfaktoren jenseits der Besteuerung zu berücksichtigen. Damit quantifiziere ich einen Anstieg der Unternehmensverlagerungen zwischen zwei Bundesstaaten um 13,02%, wenn der Herkunftsstaat eine Körperschaftsteuer von durchschnittlich 2,5% einführt. Die Heterogenität der eingeführten Steuersätze ermöglicht es zudem, die Elastizität dieser bilateralen Unternehmensansiedlungen bei einem 1 Prozentpunkt höher eingeführten Steuersatzes auf 4,75% zu schätzen. Die Effekte variieren zwischen Sektoren, wobei die zentralen Resultate von Fertigungs-, Handels- und Dienstleistungsunternehmen verursacht werden.

Das zweite Kapitel, *"A Taxing Journey: Tax Adoptions and Interstate Migration in the Early 20th Century"*, eine gemeinsame Arbeit mit Kirsten Wandschneider, ist eng mit dem ersten Kapitel verbunden und untersucht, ob Einzelpersonen durch Abwanderung aus ihrem Bundestaat auf die Einführung von Lohn- und Körperschaftssteuern reagieren. Bei der Betrachtung desselben Zeitraums verwenden wir verknüpfte Mikrozensusdaten, um zwischenstaatlichen Migrationsflüsse für die gesamten USA für heterogene demografische Gruppen zu verfolgen. Mit einer weiteren Gravitationsschätzung stellen wir fest, dass die Einführung von Lohnsteuern die bilateralen Mi-

grationsströme im Durchschnitt um 8,70% erhöht. Diese Effekte sind besonders stark für Personen im erwerbsfähigen Alter und jungen Personen mit überdurchschnittlichem Einkommen. Unsere Ergebnisse sind robust bezüglich anderen zwischenstaatlichen Migrationstrends und gelten für viele verschiedene Bevölkerungsgruppen. Wir schließen daraus, dass Einkommensteuereinführungen erhebliche Auswirkungen auf die zwischenstaatliche Migration in diesem Zeitraum hatten, wobei potentiell zwei verschiedene Kanäle wirksam wurden. Erstens beeinflusste die direkte Besteuerung des persönlichen Einkommens die individuellen Migrationsentscheidungen. Zweitens führte die Einführung von Körperschaftssteuern zu Unternehmensverlagerungen, die wiederum die Migration beeinflussten.

Das dritte Kapitel dieser Dissertation, *"Discretionary Policy under Fiscal Decentralization"*, konzentriert sich auf die empirische Analyse diskretionären Politik unter variierender fiskalischer Dezentralisierung. Das Ziel ist zu analysieren wie fiskalische Dezentralisierung die zentralen und subnationalen diskretionären Reaktionen auf eine Ausweitung der Produktionslücke beeinflusst (sowohl in Bezug auf Größenordnung als auch Wirksamkeit). Konkret frage ich (1), wie fiskalische Dezentralisierung die Fähigkeit der Zentralregierungen beeinflusst, diskretionäre fiskalische Maßnahmen einzusetzen, und (2), welche Rolle subnationale Regierungen bei der makroökonomischen Stabilisierung unter verschiedenen föderalen Strukturen spielen. Das zentrale Ergebnis zeigt, dass ein hohes Maß an fiskalischer Dezentralisierung mit einer Verringerung der Reaktion der Zentralregierung auf einen negativen Outputschock verbunden ist. Während fiskaler Föderalismus die subnationalen diskretionären Ausgaben erhöht, deuten meine Ergebnisse darauf hin, dass die Wirksamkeit bei der Schließung der Produktionslücke mit dem Grad der Dezentralisierung abnimmt. Die Ergebnisse liefern neue empirische Unterstützung für potenzielle negative Externalitäten der fiskalischen Dezentralisierung auf die makroökonomischen Stabilisierungsfähigkeiten föderaler Staaten.

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General Introduction

This dissertation investigates various direct and indirect channels through which fiscal decentralization affects the economy. I use empirical methods in both country-specific natural experiments and a cross-country analysis to test and quantify implications of fiscal federalism in contemporary and historical settings. Fiscal federalism delegates fiscal authority to subnational governments, such as states or other regional and local jurisdictions. Fiscal federalism remains highly debated and contentious in the public and academic spheres. Many researchers focus on the positive implications of fiscal federalism based on [Oates et al. \[1972\]](#), who argued that decentralization improves allocative efficiency by allowing local governments to tailor public services to local preferences. Others have focused on the potential adverse effects of highly decentralized federal structures (such as [Prud'Homme \[1995\]](#)), who warned that decentralization can exacerbate regional inequalities when poorer regions lack adequate fiscal capacity. This thesis contributes to the ongoing debate on fiscal federalism (compare [Martínez-Vázquez, Lago-Peñas and Sacchi \[2017\]](#) or [Fajgelbaum et al. \[2019\]](#)) in two ways. First, it examines how revenue decentralization, specifically state income tax adoptions, affects factor mobility. Second, it analyzes how expenditure decentralization influences the use of discretionary fiscal policy tools.

All three chapters advocate caution when decentralizing fiscal authority as my research identifies three specific risks. First, uncoordinated adoption of state corporate income taxes triggers significant firm relocations across state borders. Second, state-level personal income tax adoption leads to selective labor migration, particularly among young workers, creating demographic imbalances. Third, high expenditure decentralization constrains the effectiveness of national discretionary fiscal policy during economic downturns. However, rather than arguing against decentralization entirely, the results should be interpreted as factors that modulate the efficacy of decentralization across different contexts. These findings offer practical guidance for implementing decentralized fiscal measures. All outcomes suggest governments at various levels could potentially gain by coordinating vertically and horizontally unless subnational compe-

tion is the explicit goal of the decentralization efforts. These insights extend beyond individual countries to supranational structures, such as the European Union, which frequently debates moving toward a fiscal union. Such a union would raise important questions on the allocation of fiscal responsibilities among the different layers of member states and their domestic regional governments. While some form of federal structure is inevitable for such fiscal unification, the insights from this dissertation suggest caution regarding uncoordinated approaches to firm and labor mobility, as well as stabilization instruments. Without sufficient vertical and horizontal coordination among various tiers of government, fiscal decentralization may lead to inefficient competition between member states, potentially undermining the stability and equity goals of the union.

Chapter 1 develops a gravity framework to estimate the effects of the staggered and uncoordinated adoption of state-level corporate income taxes in the early 20th century U.S. on firm relocations among states. Chapter 2, utilizing the same historical context, estimates the effects of state-level personal and corporate income tax adoption on interstate labor force migration. Lastly, Chapter 3 discusses how high levels of fiscal decentralization can lead to availability and efficiency constraints in discretionary policy tools.

1 Decentralized Income Taxation and Firm Relocations

Chapter 1 estimates firms' mobility responses to the adoption of corporate income taxes at the state level. I focus on the early 20th century United States, where states adopted corporate income taxes at different times, creating a natural experiment for studying tax effects within a single country. The estimation relies on a structural gravity model of business relocations, grounded in firm-level decisions to relocate. This allows me to extensively account for all other push and pull factors, related to a firm's (re)location choice, including industry agglomeration and the relocation of partnering firms. A major contribution of Chapter 1 is the historical data on firm relocations. Since no comprehensive firm-level register existed for tracking business relocations between 1900 and 1930, I constructed a dataset by identifying employers in complete count census records. These employers are then linked across census waves according to [Abramitzky et al. \[2021\]](#).

I find a 13.02% increase in firm flows between two states when the origin state adopts corporate taxation. The findings extend the existing literature on corporate tax elasticities by focusing on tax adoptions rather than changes in existing rates. This distinction matters. First, new taxes in-

crease not only variable costs but also fixed costs through higher administrative burdens as firms establish compliance systems. Second, new forms of taxation often face stronger resistance than rate changes of existing taxes, especially when only some states adopt them, creating a sense of competitive disadvantage and perceived injustice.

The findings reveal how uncoordinated decentralization of taxation reshapes economic geography through firm mobility. The historical evidence demonstrates that when subnational jurisdictions independently exercise fiscal authority, inter-jurisdictional competition emerges which may undermine collective fiscal objectives. This analysis informs contemporary discussions on how to decentralize fiscal policy by highlighting the importance of coordination mechanisms when implementing new taxation at regional levels.

2 Decentralized Income Taxation and Labor Mobility

In Chapter 2, analyzing mainly personal income tax adoptions, related questions emerge. Many states not only introduced corporate income taxes but also income taxes on labor and capital income. These uncoordinated state tax introductions are shown to cause interstate labor migration driven by an exodus of workers from adopter states. The gravity model is based on individuals' decisions to move between states. This allows for a causal estimation of changes in interstate migration flows. Drawing from the same linked complete count census data as Chapter 1 enables the estimation of heterogeneous effects across different demographics. This provides a first quantification of how early tax adoption affects labor mobility.

The results confirm that personal income tax adoption increases out-migration, similar to the "foot voting" response to local public expenditures described by Tiebout [1956]. The key findings reveal that this migration response is predominantly driven by young individuals in the labor force. This selective out-migration creates an uneven distribution of labor that may not have been intended by policymakers implementing these taxes. These results contribute to the fiscal federalism literature by providing empirical evidence on the mobility response to uncoordinated revenue decentralization. Migration patterns across demographic groups reveal that fiscal decentralization significantly impacts equity. While traditional Tiebout models focus primarily on efficiency gains from jurisdictional competition, my findings show that decentralized taxation can create uneven demographic distributions. When disproportionately many young, productive workers leave, this can affect local labor markets and the fiscal sustainability of jurisdictions.

By demonstrating how decentralizing tax policy can reshape population distributions along demographic lines, Chapter 2 offers insights into the potential long-term consequences for regional economic development, labor markets, and public finances of adopter versus non-adopter states. Together with the findings from Chapter 1, this provides a comprehensive picture of factor mobility responses to decentralized taxation.

3 Decentralized Fiscal Policy and Discretionary Stabilization

Chapter 3 focuses on the direct implication of fiscal federalism on macroeconomic stability. In highly decentralized economies, especially those where subnational spending depends on intergovernmental transfers, scholars like Prud'Homme [1995] have argued that macroeconomic stabilization tools may be compromised, because central governments lose direct control over a significant portion of public spending, making it difficult to implement coordinated countercyclical fiscal policies. I investigate this concern by focusing on two key channels. First, central governments may lack sufficient fiscal resources to address economic downturns when much of the government budget is controlled by subnational entities. Second, while it can be shown that national macroeconomic shocks are propagated to the regional level (e.g. Blanchard et al. [1992]), subnational governments may not effectively contribute to stabilization. This could occur because local authorities remain inactive and rely on horizontal spillovers from other local governments or vertical spillovers from the reaction of the central government. Additionally, implementing locally rational policies could undermine national stabilization efforts.

To empirically test these relationships, I analyze cross-country panel data with local projections, estimating dynamic effects when output shocks trigger discretionary fiscal policy responses across different levels of fiscal decentralization. I contribute methodologically by developing a new measure of fiscal decentralization. Rather than relying solely on expenditure shares as most studies do, my measure combines both quantitative aspects (share of subnational spending) and qualitative dimensions (degree of budgetary autonomy). This distinction is crucial because true fiscal decentralization requires not just subnational spending but also the autonomous authority to make budgetary decisions, which my measure uniquely captures.

The results confirm that higher fiscal decentralization significantly reduces central governments' fiscal responses to negative economic shocks. While decentralization does increase subnational discretionary spending during downturns, governments close output gaps less effectively as decentralization increases. These findings provide novel empirical support for potential negative

externalities of fiscal decentralization regarding macroeconomic stabilization capabilities. By incorporating a multidimensional measure of decentralization, this chapter offers policymakers more precise guidance than existing single-dimension approaches for designing federal arrangements that preserve effective countercyclical fiscal policy capabilities.

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Chapter 1

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

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 20th 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 the 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.

JEL Classification: H25, H26, N32, R12

Keywords: Corporate Tax Adoption, Business Relocation, Fiscal Decentralization

1.1 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. However, cross-country firm relocations are limited by many factors, such as legal and financial constraints or cultural and language barriers. Furthermore, international firm relocations are 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 20th 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 oftentimes 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 20th 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 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 decennial 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 20th century. With this, I create a unique state-pair firm relocation dataset, capturing 48×48 -pairs across three decades. 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 over time, 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 in total interstate firm flows in a conditional general equilibrium context. With this, counterfactual analysis enables me to consider how the absence of state income tax adoptions affects 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 reveal a strong and significant increase in bilateral business relocations between states 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 tax, effects are further estimated by sectors, revealing that

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

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

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 for 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 20th 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 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. Brühlhart et al. [2023] recently assesses 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 ex-

tensive margin of taxation, I capture fundamentally different firm responses than studies of rate changes as new taxes impose additional fixed costs. 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 20th century.

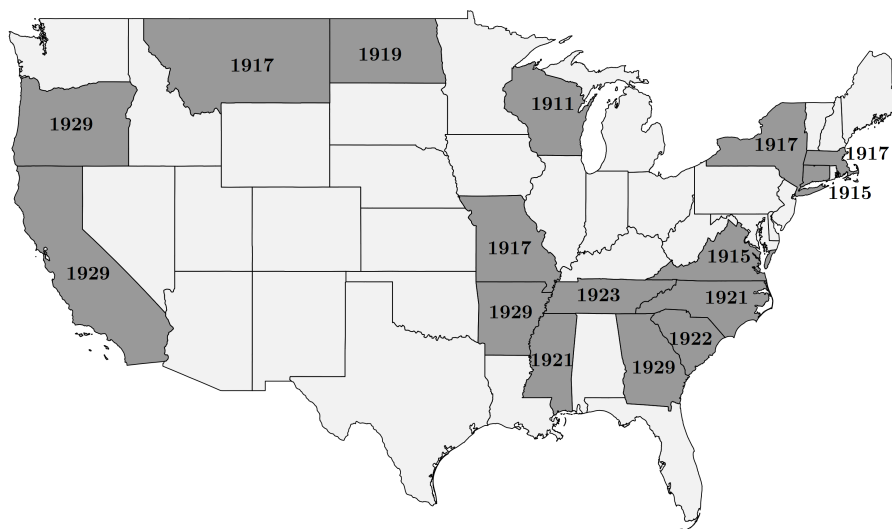
The remainder of this paper is structured as follows. Section 1.2 discusses the historical context of the corporate state income tax adoptions. Section 1.3 gives an overview of the data used. Section 1.4 describes the derivation of a structural gravity equation based on firm relocation choices. Section 1.5 presents the baseline estimation framework and Section 1.6 describes the main results, as well as further results extending the baseline model across various dimensions. Section 1.7 presents counterfactual exercises and estimates state-level changes in state-level firm outflows. Section 1.8 provides support of the robustness of the results and Section 1.9 concludes.

1.2 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 1.1, 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 were affected by the rollout. Especially the South observed 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 1.1 – 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 Inter-governmental 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

To further explore driving factors of the tax adoptions, Appendix A.2 provides an empirical analysis of potential predictors of the income tax. Neither firm nor migration flows seem to affect the

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

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

⁵This is empirically supported by findings of Figures A.1.2 and A.1.3 in Appendix A.2 using data from [Cassidy, Dincecco and 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.

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 A.1.1). I find that this share is negatively correlated with the adoptions, indicating a certain degree of lobbying 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](#)].

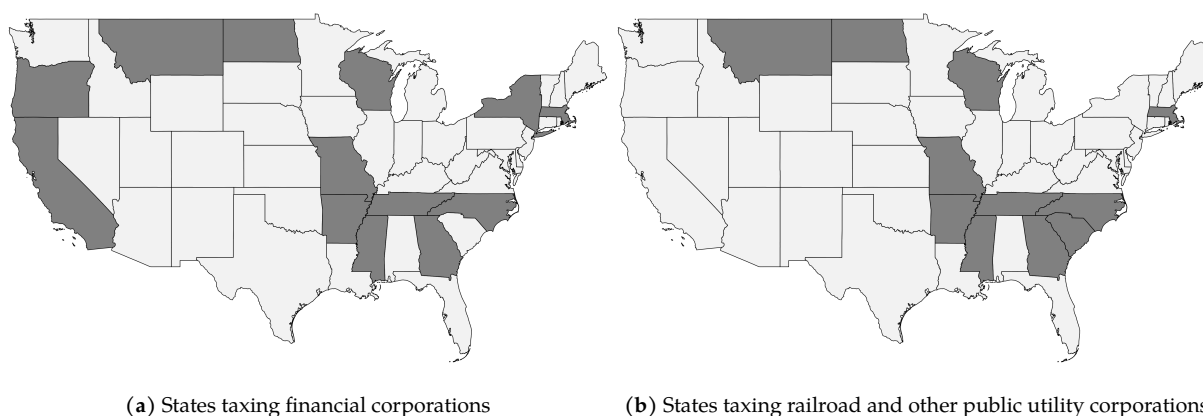
As captured by the [National Industrial Conference Board \[1930\]](#), the primary focus of state corporate income tax adoptions were the manufacturing and mercantile businesses as well as the service sector, which were taxed in all adopter states. As presented in Figure 1.2a, thirteen of these states imposed income taxes on financial institutions such as banks and insurance companies. Eleven states, shown in Figure 1.2b, levied taxes on railroad or other public utilities.

⁶However, 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.

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), see [State of Connecticut \[1915\]](#)]

Figure 1.2 – Corporate income tax adoptions by specific sectors



1.2.1 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 enactment, 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 beyond the opportunity to estimate elasticities for tax policy changes on the extensive margin.

1.3 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 includes business owners¹¹ 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

⁷As 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.

⁸More on that in Section 1.5 and Appendix A.3.

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

¹⁰The 1920 instruction for census enumerators (see IPUMS [1920]) quotes: "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 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."

¹¹This 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.

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 1.1 – 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 similarly 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 1.1 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 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 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

¹²The subsequent Section 1.3.1 will carefully discuss the validity of this proxy.

¹³Hence, the data only includes male individuals.

¹⁴I 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.

¹⁵In Section 1.5, I discuss the strategy to account for these changes, as well as a test for pre-trends.

1.3.1 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. To do so, I use newly digitized parts of the general statistics of the [Census of Manufactures \[1929\]](#) that count the number of manufacturing businesses in each state i between 1900 and 1930. Together with the number of manufacturing employers from the linked census, this allows for the estimation of

$$\# \text{ of Mfg Businesses}_{i,t(b)} = \psi(\# \text{ of Mfg Employers}_{i,t(b)}) + \zeta \text{Total Pop}_{i,t(b)} + \mathbf{FE} + \nu_{i,t(b)}, \quad (1.1)$$

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

Table 1.2 – 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 clustered 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 $0 < \hat{\psi} < 1$. Table 1.2 presents the estimation results for equation 1.1. After controlling for state-specific total population and state and decade fixed effects, I find that each manufacturing employer in state i at the onset of decade $t(b)$ is associated with around 0.71 manufacturing businesses. 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 the existence of a strong and significant correlation between the number

¹⁶Note 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.

¹⁷I 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.

of employers and the number of businesses. Hence, I argue that the number of employers from the linked census waves is a suitable proxy for the number of firms.¹⁸

1.3.2 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 1.3 reports dyadic summary statistics for firms, overall, as well as a decomposition by industries. A finer decomposition is presented in Table A.1.2. Firms are on average less mobile than the overall population. That is indicative of employers and their firms being tied to their origin and hence facing 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. 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.

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

¹⁹The 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 1.4 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 A.5.

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

Table 1.3 – Average internal relocations and sample population for firms of aggregate industries

Panel	Pair Outflow		State Outflow		State Sample Population		Mob.Rate
	Mean	Stand. Dev.	Mean	Stand. Dev.	Mean	Stand. Dev.	
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 *Mob.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*.

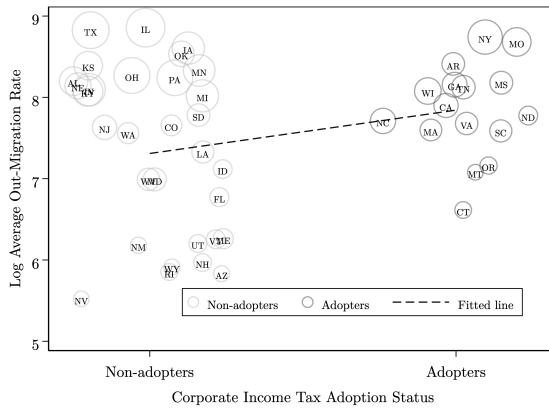
Any other sector observes a drastically lower average pair outflow. The ratio of average state outflow to average state population reveals that this is mainly driven by an overall low number of employers in my sample rather than a general reluctance to relocate by the employers. These findings suggest that it is worthwhile to think of more aggregate sector specifications. As presented in Section 1.2, 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 before the Great Depression, observe a larger average rate of firm outflows. Figure 1.3 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 for the total log-outflows, as presented by Table 1.4, reveals that larger outflows are 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.

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.

²¹This will later be important as the empirical estimation loses fewer observations due to statistical separation.

²²Due to legal differences, regulations, and local depositors or policyholders, the financial sector is less mobile by design.

Figure 1.3 – Adopter assignment and log-outflows**Table 1.4** – Difference over decades

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

1.3.3 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 the 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 for testing a central claim of this paper. Besides anti-tax sentiment, the main channel through which the new tax can affect 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' need for hiring additional personnel to help with book-keeping for tax collection and audits. The digitized data includes a differentiation of managers, administrative staff, and workers. Figure A.1.4 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 the number of salaried personnel, I estimate the following reduced-form model

$$\# \text{ of Salaried Personnel}_{i,t(e)} = \eta z_{i,t} + \mathbf{Controls} + \mathbf{FE} + v_{i,t(e)}, \quad (1.2)$$

where the tax adoption dummy $z_{i,t} = 1$ if state i adopts the corporate income tax in decade t . The set of control variables includes the number of corporations, the total population, and the total out-migration. Additionally, to control for unobserved state and decade heterogeneity,

fixed effects (FE) are added. Results of the estimation are presented in Table 1.5.

Table 1.5 – %-change in administrative personnel associated with the tax adoption

	ln(# of Salaried Personnel _{<i>i,t(e)</i>})			
	Administrative	Managerial	Administrative	Managerial
Corporate income tax adoption: $z_{i,t}$	0.624 (0.409)	0.692 (0.403)*	0.086 (0.047)*	0.081 (0.046)*
ln(Total Corp _{<i>i,t(b)</i>})			−0.223 (0.142)	0.370 (0.138)***
ln(Total Pop _{<i>i,t(b)</i>})			1.112 (0.198)***	0.738 (0.192)***
ln(Total Out-Migration _{<i>i,t(e)</i>})			−0.503 (0.146)***	−0.497 (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 upwardly 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.

1.4 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

$$y_{i,j,t(e)} = \phi_{i,j,t(e)} \times f_{i,t(b)} \times g(t), \quad (1.3)$$

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 $t(b) \leq t(d) \leq t(e)$. 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

$$\Lambda_{h,i,j,t(d)} = \left[\underbrace{\alpha p_{j,t(d)} - \gamma c_{i,j} + \nu_{h,i,j,t(d)}}_{\check{\Lambda}_{h,i,j,t(d)}} \right] + \beta \tau_{i,j,t(d)} \quad (1.4)$$

where $\Lambda_{h,i,j,t(d)}$ (and $\check{\Lambda}_{h,i,j,t(d)}$) 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, and 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 capturing 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 $\beta = \Lambda_{h,i,j,t(d)} - \check{\Lambda}_{h,i,j,t(d)}$ if $\tau_{i,j,t(d)} = 1$, since the tax adoption defines a shock to the relocation decision process. Hence, $\beta > 0$ holds if the tax adoption increases the relocation profits. I assume that the distribution of $\nu_{h,i,j,t(d)}$ determines that moving to j is optimal for firm h . McFadden et al. [1973] demonstrated that if $\nu_{h,i,j,t(d)}$ follows an iid extreme value type 1 distribution, then the expected value of $\phi_{i,j,t(e)}$ and $y_{i,j,t(e)}$, defined by equation 1.3, can be expressed as shown in the equation below:

$$\mathbb{E}(y_{i,j,t(e)}) = \left[\frac{\exp(\alpha p_{j,t(d)} + \beta \tau_{i,j,t(d)} - \gamma c_{i,j})}{\underbrace{\sum_{k \in \mathcal{J}} \exp(\alpha p_{k,t(d)} + \beta \tau_{i,k,t(d)} - \gamma c_{i,k})}_{\mathbb{E}(\phi_{i,j,t(e)})}} \right] \times f_{i,t(b)} \times g(t). \quad (1.5)$$

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

$$\text{Push}_{i,t(d)} = \left[\sum_{k \in \mathcal{J}} \frac{\exp(\beta \tau_{i,k,t(d)}) \times \text{Pull}_{k,t(d)}}{\text{Costs}_{i,k}} \right]^{-1}. \quad (1.6)$$

With these definitions we can re-write equation 1.5 such that it has a gravity-like structure

$$\mathbb{E}(y_{i,j,t(e)}) = \exp(\beta \tau_{i,j,t(d)}) \times \frac{\text{Push}_{i,t(d)} \times \text{Pull}_{j,t(d)}}{\text{Costs}_{i,j}} \times f_{i,t(b)} \times g(t). \quad (1.7)$$

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 $\text{Pull}_{j,t(d)}$ and $\text{Push}_{i,t(d)}$, capturing the inverse attractiveness of all destinations $k \neq j$. On the other hand, expected flows are decreasing in bilateral relocation costs ($\text{Costs}_{i,j}$). 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 1.7 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 1.7. I assume $\mathbb{E}(\varepsilon_{i,j,t(d)} | \tau_{i,j,t(d)}, \text{Pull}_{j,t(d)}, \text{Push}_{i,t(d)}, \text{Costs}_{i,j}, f_{i,t(b)}) = 1$. 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 $\mathbb{E}(\ln \varepsilon_{i,j,t(d)}) \leq \ln \mathbb{E}(\varepsilon_{i,j,t(d)})$. 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

$$y_{i,j,t(e)} = \exp \left[\beta \tau_{i,j,t(d)} + \ln(\text{Pull}_{j,t(d)}) + \ln(\text{Push}_{i,t(d)} \times f_{i,t(b)}) - \ln(\text{Costs}_{i,j}) + \ln(g(t)) \right] \varepsilon_{i,j,t(d)}. \quad (1.8)$$

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 .

1.5 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 $\ln(\text{Pull}_{j,t})$ and $\ln(\text{Push}_{i,t} \times f_{i,t})$. These fixed effects enable to control 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 to control for U.S.-wide structural changes, i.e. $g(t)$, such as advances in trans-

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

portation 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 $\ln(\text{Cost}_{i,j})$ 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 1.8 as

$$y_{i,j,t} = \exp \left[\beta \tau_{i,j,t} + \delta_{i,j} + \delta_{i \neq j,t} + \delta_{i,t} + \delta_{j,t} \right] \varepsilon_{i,j,t}. \quad (1.9)$$

I refer to this as a higher-dimensional two-way fixed effects (HDTWFE) model, as this is a multidimensional interpretation of a TWFE model, including additional fixed effects for the resistance terms in the form of time-varying push and pull factors.

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 to create a pair-wise moving policy variable. Recall that 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 $z_{i,u \in \mathcal{T}}^{\text{trad}} = 1, \forall u \geq t$. I 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

²⁴As noted 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.

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

²⁶For an extensive discussion, see [Cassidy, Dincecco and Troiano \[2024\]](#).

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 $\hat{\beta} < \hat{\beta}^{trad}$.

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 1.6 – Examination of pre-trends for the 1900 - 1910 cross-section

Group Comparison	1910s Adopters			1920s Adopters		
	$E[y_{i,j}^{treat}]$	$E[y_{i,j}^{cont}]$	<i>p-value</i>	$E[y_{i,j}^{treat}]$	$E[y_{i,j}^{cont}]$	<i>p-value</i>
<i>Panel A: Relocation Pairs $i \neq j$</i>						
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
<i>Panel B: Staying Pairs $i = j$</i>						
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: $E[y_i^{treat}]$ and $E[y_i^{cont}]$ 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 assigned 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 as well as states that 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 1.6 find a slightly higher mean dependent variable for adopter states, however, no statistically significant differences across groups prior to the treatment. Compared to the overall mean in Table 1.3, there is also a noticeable upward trend in bilateral flows. This further suggests advancements in relocation technologies, such as railroads, and motivates the employment of country-wide time fixed effects. Panel B can be interpreted as a 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 A.2 discusses potential state-level drivers of income tax adoptions. I show that firm outflows have no significant implication for tax adoptions, finding no indication of the presence of a simultaneity bias.

For the estimation, I define equation 1.9 as a Poisson regression model, where $E(y_{i,j,t}) = \lambda_{i,j,t}$ 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 1.10 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 require 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 (β s) 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 an increase in government spending does neither qualitatively [National Industrial Conference Board, 1930] nor quantitatively [Cassidy, Dincecco and Troiano [2024], Figures A.1.2 and A.1.3] 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 $\hat{\beta} < \hat{\beta}^{trad}$. Consequently, a traditional policy variable would most likely result in an upward bias.

1.6 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 the 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

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

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

model with three-way fixed effects controls 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 corporate tax adoptions, medium-run results are a worthwhile addition.²⁹

Table 1.7 – Baseline regressions for the full panel (all sectors combined)

	Model (1)	Model (2)	Model (3a)	Model (3b)
	Distance Control	Standard	HDTWFE	Traditional
Corporate income tax adoption: $\tau_{i,j,t}$	0.378 (0.079)***	0.431 (0.029)***	0.122 (0.023)***	0.295 (0.041)***
$\log(\text{Dist}_{i,j})$	-0.293 (0.002)***			
Bias (<i>Weidner and Zylkin</i> [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 (<i>Bergstrand, Larch and Yotov</i> [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].

Table 1.7 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

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.

the state-decade fixed effects. However, given the unique historical context of this analysis, a more robust correction is warranted. As discussed in Section 1.2.1, the observation period was 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 $\hat{\beta} < \hat{\beta}^{trad}$. This provides empirical support for 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.

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 A.5, I find no indication that the adoption of income taxes affected firm closures in the early 20th 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 A.3.

Baseline Results by Sectors

Once I establish 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

³⁰Formally, 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.

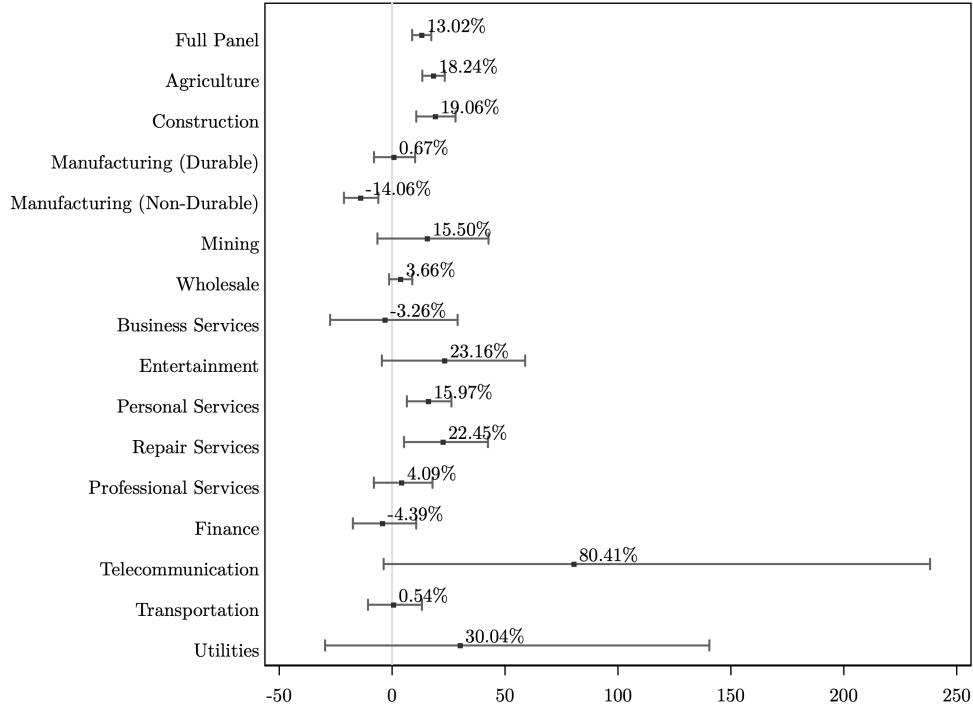
³¹Note that for the sake of comparison, I also include the overall firm flows as a "sector".

allows me to re-write equation 1.9 in a sector-specific notation such that

$$y_{i,j,t,s} = \exp \left[\sum_s \beta_s \mathbb{1}_s \tau_{i,j,t} + \mathcal{D} \times \delta_s \right] \xi_{i,j,t,s}, \quad (1.10)$$

where δ_s denotes a sector fixed effect and $\mathcal{D} = (\delta_{i,j} + \delta_{i \neq j,t} + \delta_{i,t} + \delta_{j,t})$. The sectors are equivalent to the line-up of Table A.1.2. The results of this estimation are presented in Figure 1.4.³²

Figure 1.4 – 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].

It is crucial to note that for this particular specification, I do not account for sectoral adopter variation. In other words, I do not account for the fact that some states adopted the corporate income tax only for a subset of sectors. I find strong and significant effects for some manufacturing and mercantile sectors such as agriculture, construction, or certain service sectors. Interestingly, non-durable manufacturing is estimated to observe a negative effect of the introduction of corporate income taxes, which highlights the fact that some industries with potentially narrower profit margins, may have been more inclined to limit interstate relocation activities in response to the introduction of state income taxation. Conversely, other sectors may have exhibited greater resilience or found opportunities to capitalize on the tax changes by moving across state borders. For most sectors, I find no statistically significant effects. However, many point estimates are esti-

³² A full tabular listing of the finding are presented in Table A.1.3 in Appendix A.2.

mated to be very large, such as for the entertainment industry (e.g. circuses such as the Ringling Bros. in Wisconsin) or telecommunication. The insignificance comes from massive confidence bands due to few observations in these groups. The caveat of analyzing narrowly defined sectors is that many state pairs observe no firm flows for the entire observation period. This leads to singleton observations, i.e. constant flows across time. As these observations need to be excluded (Correia [2015]), many sectors are left with a very small sample size. The subsequent analysis will therefore aggregate sectors into groups defined in Table 1.3. This is sufficient, as tax policies were targeting businesses assigned to these aggregated sectors.

1.6.1 Introducing Adopter Variation

A comprehensive tax adoption indicator should vary in sectors, as some states only tax certain industries. As discussed in Section 1.2, in several states, financial institutions as well as public utility companies were exempt from the new tax. I can use the grouping of Table 1.3 to create a set of aggregated sectors S . With this new index, I repeat the estimation of equation 1.10, 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 1.10 can be re-written as

$$y_{i,j,t,S} = \exp \left[\sum_S \beta_S (\mathbb{1}_S \times \{\tau_{i,j,t} \times w_{i,S}\}) + \mathcal{D} \times \delta_S \right] \tilde{\xi}_{i,j,t,S}, \quad (1.11)$$

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 1.8. First, the overall effect, and the effect on manufacturing, mercantile, and service businesses do not change across models. As previously discussed, firms in 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 A.1.3, 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 indica-

tors increase. The effects on 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 tax adoption. The point estimate translates to a 16.77% increase in interstate flows of transportation and public utility firms, which is of comparable size to the overall estimated effect. This underscores the importance of accounting for legal differences in the adopted tax laws.

Table 1.8 – Stacked regressions with and without state-specific taxation

	Model (4)	Model (5)		Model (6)
	HDTWFE	Adopter Variation		Placebo Test
		Relative	Absolute	
<i>Panel A: Full Panel</i>				
$\tau_{i,j,t}$	0.122*** (0.023)		7.03*** (1.39)	
<i>Panel B: Sector-specific effects</i>				
Manufacturing and Mercantile	0.121*** (0.023)	0.121*** (0.023)	6.34*** (1.28)	
Services	0.138*** (0.042)	0.138*** (0.042)	0.44*** (0.14)	
Financial Sector	−0.045 (0.089)	0.047 (0.092)	0.02 (0.05)	−0.433** (0.178)
Transport. and Other Pub. Util.	0.041 (0.068)	0.155* (0.092)	0.13* (0.08)	−0.062 (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 $(\exp(\hat{\beta}_S) - 1) \times \bar{y}_S$.

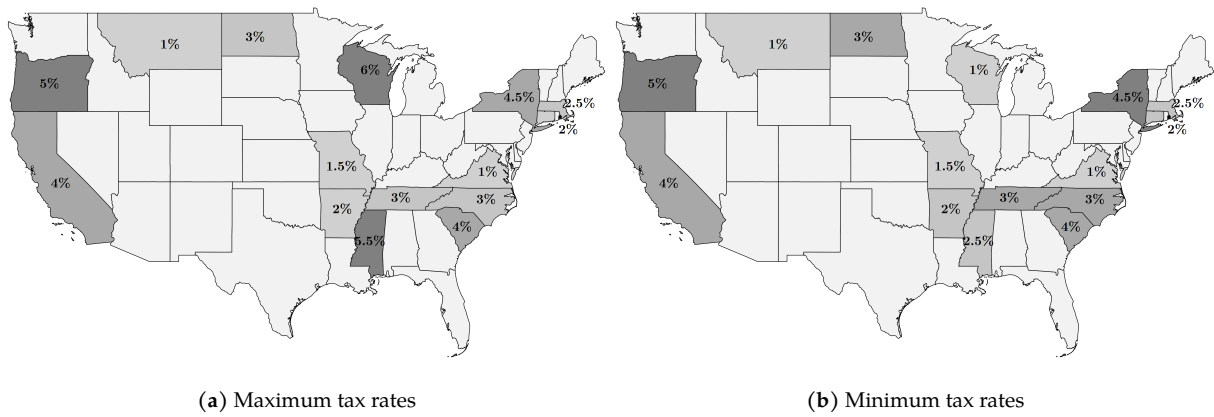
Lastly, I estimate a "placebo" model, wherein I treat firms within sectors of states that implemented the income tax but refrained from imposing it on these specific sectors. This enables me to discern whether my initial estimate is capturing additional factors beyond the tax policy. I find no significant 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.

1.6.2 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 1.5 reports both maximum and minimum introduced rates. Wisconsin, as the first adopter state also introduced the largest tax rate at 6%.

Figure 1.5 – Adopted tax rates by states

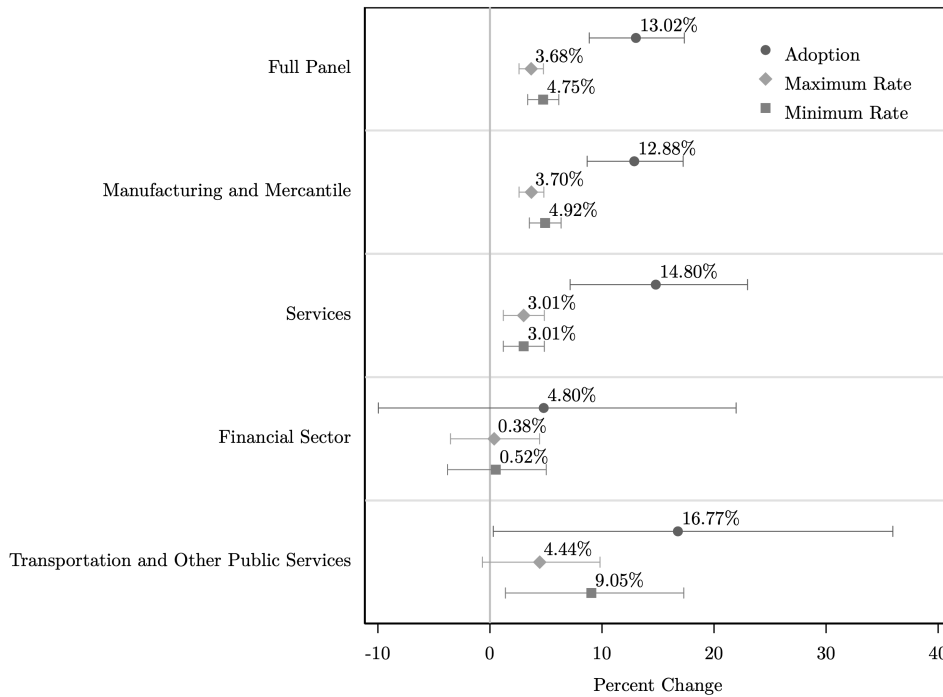


The progressivity of the rate was associated with the amount of taxable net income. Given that the individual census data enables the identification of firm owners, the use of firm proxies may lead to an underrepresentation of publicly traded companies. As a result, a significant portion of the sampled 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. To estimate the effects of these adopted rates, I define $\tau_{i,j,t,S}^r = (\tau_{i,j,t} \times w_{i,S}) \times \text{Rate}_i$, where $\text{Rate}_i \in \{\text{Max}_i, \text{Min}_i\}$ defines the adopted tax rates. The policy in equation 1.11 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 1.6.

Consistent with the interpretation of the coefficients, the magnitude of the estimates is significantly smaller. Furthermore, 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 a 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 affected by the minimum rate.³⁴

Figure 1.6 – 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 A.1.4.

1.6.3 Potential Bilateral Sources of Endogeneity

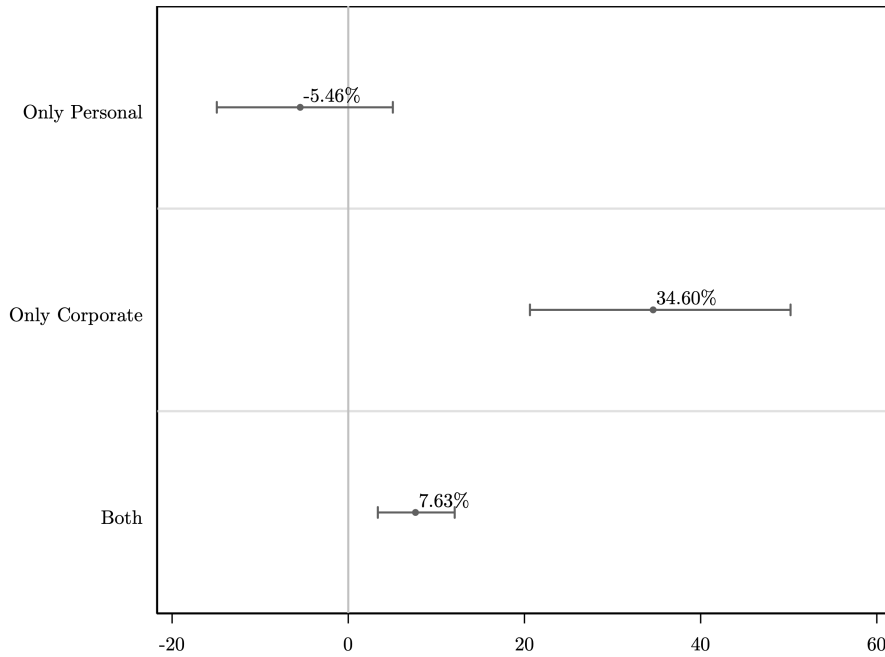
Many states adopted both corporate and personal simultaneously. Higher personal income tax can make labor more expensive, depending on the paid wages and whether such taxes are passed

³³Under a set of assumptions, the pure adoption effect can be approximated by $5.21 - 4.75 = 0.46$. See Appendix A.6 for more details.

³⁴Another interesting aspect related to the rates is the level of progressivity of which they were implemented. To analyze the effect of rate progressivity on the relocation flows, I use the ratio $[(\tau_{i,j,t} \times w_{i,s}) \times \text{Min}_i] / \text{Max}_i \in [0, 1]$ 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 the minimum and maximum rates being closer together. Results for the effects of the rate ratio can be found in Table A.1.4. To facilitate interpretability, I take the measure times 10 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.

on. Therefore, I might pick up some of the effects of the personal tax adoption through my policy variable. 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 1.9 now includes three distinct and statistically comparable policy variables. The findings, presented in Figure 1.7 reveal a set of fascinating insights. A direct comparison to the effects described in Table 1.8 presupposes caution, as the original policy is a combination of two policies here, i.e., $\tau_{i,j,t} = \tau_{i,j,t}^{corp} + \tau_{i,j,t}^{both}$.

Figure 1.7 – 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 A.1.5.

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 for the indicator

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

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 bias imposed by higher input prices. If anything, the results raise potential concerns about underestimating the effects of 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 1.7 suggest that previous results are driven by corporations relocating and not small enterprises. 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.

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 effects 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 A.1.5). 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 1.4 such that

$$\tilde{\Lambda}_{h,i,j,t} = \Lambda_{h,i,j,t} + \kappa_1 f_{j,i(t-1),t} + \kappa_2 \text{Workers}_{i,j,t-1}, \quad (1.12)$$

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 $\text{Workers}_{i,j,t-1}$ 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 $\text{Pull}_{i,j,t} = \text{Pull}_{j,t} \times \exp(\kappa_1 f_{j,i(t-1),t} + \kappa_2 \text{Workers}_{i,j,t-1})$, which are now origin-specific. The gravity equation follows the baseline specification. The stock $f_{j,i(t-1),t}$ can be identified in various ways. A straightforward approach 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. To better capture this aspect, I pursue two alternative strategies. The first interacts lagged firm flows with a lagged policy variable, which captures the effects of relocating firms conditional on the adoption of the tax in the origin state. The second strategy involves the fitted values $\hat{y}_{i,j,t-1}$ from the estimation of equation 1.11. All results are presented in Table 1.9. First, note that regressions including lagged bilateral controls regressions have a smaller sample size due to the exclusion of the 1900s.³⁷ To have a comparable point estimate for the baseline strategy, I repeat the estimation of equation 1.11 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 sectoral results, the 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 proceeds without incorporating these additional controls.

³⁶As the complementarity channel requires prior collaboration between firms, I assume that only recent (last decade) relocations from i to j influence subsequent firm movements.

³⁷For the 1890 census, there is no information on employer status.

Table 1.9 – Regressions including different bilateral controls for relocation complementarities

		Type of bilateral control			
	Model (5)	Lag Workers	Lag Firms	Interaction	Fit. Values
<i>Panel A: Full Panel</i>					
$\tau_{i,j,t}$	0.077*** (0.024)	0.107*** (0.022)	0.129*** (0.021)	0.197*** (0.032)	0.113*** (0.022)
$\ln(y_{i,j,t}^{work})$		0.170*** (0.015)	0.075*** (0.022)	0.078*** (0.022)	0.170*** (0.015)
$\ln(y_{i,j,t-1})$			0.102*** (0.017)	0.096*** (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)	
$\ln(\hat{y}_{i,j,t-1})$					0.111** (0.052)
<i>Panel B: Sector-specific effects ($\tau_{i,j,t}$ only)</i>					
Manufacturing and Mercantile	0.072*** (0.024)	0.110*** (0.022)	0.130*** (0.020)	0.193*** (0.033)	0.112*** (0.022)
Services	0.122*** (0.034)	0.125*** (0.033)	0.046 (0.036)	0.119** (0.055)	0.137*** (0.035)
Financial Sector	−0.055 (0.054)	−0.017 (0.053)	−0.094* (0.054)	0.105 (0.076)	0.040 (0.061)
Transport. and Other Pub. Util.	0.147*** (0.052)	0.092* (0.050)	0.023 (0.048)	0.076 (0.063)	0.082 (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].

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, even for sectoral variation, I introduce $\mathcal{S} = \mathcal{S} \setminus \{Full\ Panel\}$ as an index of all aggregated sectors. This allows to control for sector-specific agglomeration in the baseline strategy. Note that, similar to the specification in equation 1.11, 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 1.10 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 1.7.³⁸ 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

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

adopter variation, as the firm flows are now decomposed by sectors. I find that controlling for adopter variation slightly lowers the point estimate, however within the 95% confidence bounds of the original estimate.

Table 1.10 – Regressions including different sectoral fixed effect interactions

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

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. 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].

1.6.4 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 1.11 summarizes the estimation results for the border interaction. 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 1.11. 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 pattern is replicated by the manufacturing and mercantile, as well as the public utility sector (see Table A.1.7 in Appendix A.2). Similar to the overall effects, the financial sector does not observe any significant relocation effects.

Table 1.11 – Overall results decomposed by bordering status

	Bordering	Not Bordering
	(i)	(ii)
<i>Part 1: Descriptive Statistics</i>		
Average Flow	241.7	35.0
Standard Deviation	267.2	85.9
Number of Pairs	621	6,147
<i>Part 2: Regression Analysis Results</i>		
$\tau_{i,j,t}$		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.

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 the 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.

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

The census data allows to not only follow employers across state lines but also across counties. This additional information helps to further assess 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 on 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 $MD_{w(i)} = \min_{k \in \mathcal{B}_i} \{\text{Dist}(w(i); k)\}$.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 1.12, 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 policy with the minimum of the border distance measure. As this measure is constant across county pairs, I decided to use state-level fixed effects.⁴¹ Figure A.1.6 provides a graphical presentation of county-level mobility rates. These do not depend on the distance to the state border, which is critical for the "quasi-gravity" model, which can be written as

$$y_{w(i),w(j),t} = \exp \left(\vartheta_1 \tau_{i,j,t} + \vartheta_2 MD_{w(i)} + \vartheta_3 (\tau_{i,j,t} \times MD_{w(i)}) + \mathcal{D} \right) \varsigma_{w(i),w(j),t}. \quad (1.13)$$

Summary statistics and estimation results are presented in Table 1.12. Note that the point estimates for the county-to-county flows are larger compared to the state-level baseline results in Table 1.7. 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 explains a smaller fraction of the variation in 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 1.8b). With that in mind, equation 1.13 allows the estimation

⁴⁰ An in-depth discussion of the minimum-distance measure can be found in Appendix A.4.

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.

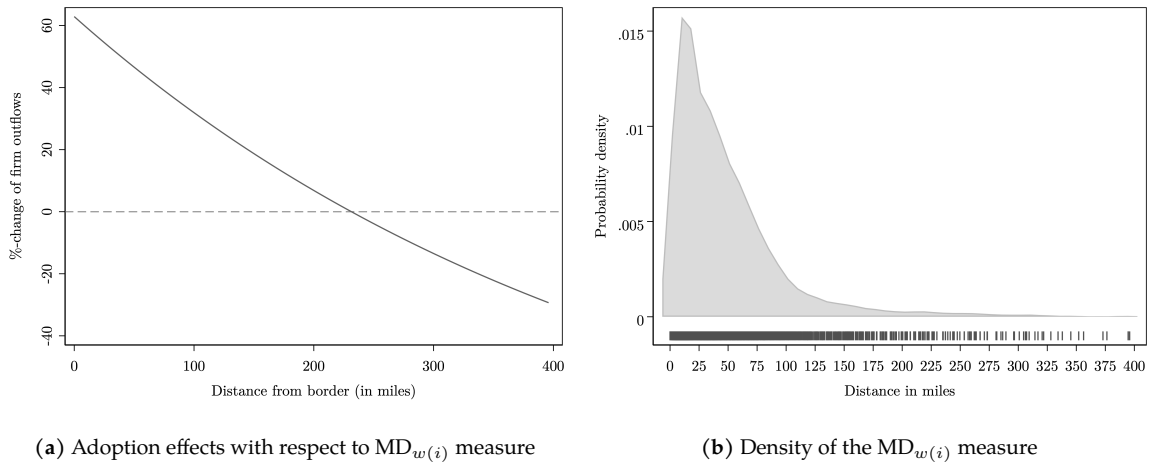
of the fixed point $-\hat{\vartheta}_1/(\hat{\vartheta}_2 + \hat{\vartheta}_3)$, i.e. the point at which the adoption effect on firm relocation becomes zero.

Table 1.12 – Overall results for the county-level analysis

<i>Descriptive Statistics</i>	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*** (0.029)		0.488*** (0.067)			
$MD_{w(i)}$			-0.001* (0.000)			
$\tau_{i,j,t} \times MD_{w(i)}$			-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 1.8 – County-level effects and density by distance to the closest state border



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

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 1.8a plots the estimated coefficients for $MD_{w_i} \in [0, 400]$. Note that the mean distance to the closest state border is around 50 miles, with 75% of counties lying within 35 miles of 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 1.8a, which is above the 95th percentile of the distance distribution.

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 analyze 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 $\tau_{r,r,t} = z_{i,t} \mathbb{1}(i \notin U \wedge j \notin U \wedge i \neq j)$. To estimate the effects directly, rather than as deviations from the main effect, I define $\tau_{i,j,t}^{all} = \tau_{i,j,t} - (\tau_{r,u,t} + \tau_{u,r,t} + \tau_{r,r,t})$. The overall results in column (ii) of Table 1.13 show the largest effects for relocations from rural to urban states. A potential explanation is that firms already contemplating leaving 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 on relocations

⁴²This 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.

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

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 also provides suggested support for 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 be located in urban states, and vice versa for agricultural businesses.

Table 1.13 – 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)				
$\tau_{i,j,t}^{all}$		0.096** (0.040)	0.116*** (0.041)	0.279*** (0.049)	-0.188*** (0.031)
$\tau_{r,u,t}$		0.335*** (0.042)	0.330*** (0.043)	0.362*** (0.048)	0.165*** (0.052)
$\tau_{u,r,t}$		0.308*** (0.046)	0.343*** (0.048)	0.672*** (0.063)	-0.083* (0.49)
$\tau_{r,r,t}$		0.046 (0.029)	0.040 (0.029)	0.077** (0.030)	0.135*** (0.334)
<i>Observations</i>	6,765	6,765	6,738	6,492	6,273
<i>McFadden Pseudo R²</i>	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 \[2015\]](#).

1.6.5 Persistence of the Adoption Shock

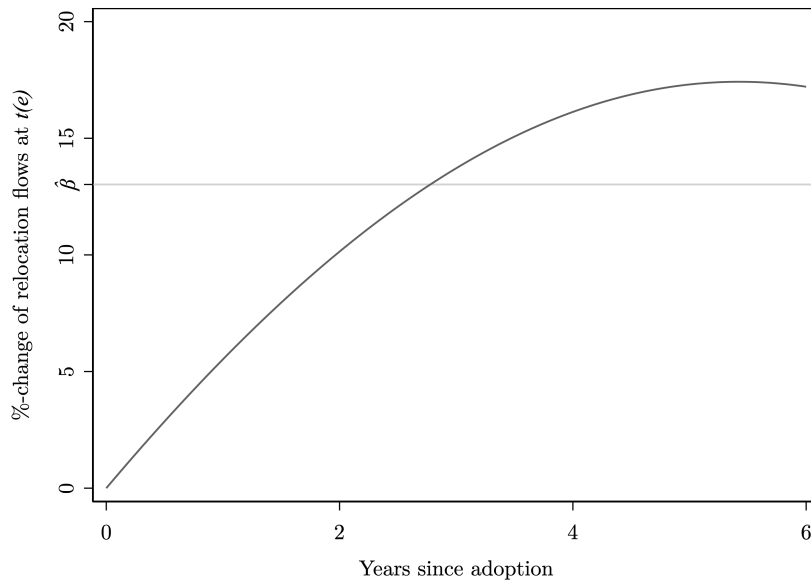
This section focuses on an approach to estimate the relocation effects within the adoption decades. So far, the analysis has been concerned with decennial effects, as permitted by the data structure, leading to the omission of informative variation. Two claims are investigated in this section. First, 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 1.1, 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 $YSA_{i,t} = t(e) - \text{Adoption Year}_i$ 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 $\tau_{i,j,t} \times YSA_{i,t}$, which captures the adoption effects for the temporal distance to the year of adoption. As I presume the introduction effect to be concave within the adoption decade, I estimate

$$y_{i,j,t,S} = \exp \left(\sum_S \varphi_{1,S} (\mathbb{1}_S \times \{\tau_{i,j,t} \times w_{i,S}\} \times YSA_{i,t}) + \varphi_{2,S} (\mathbb{1}_S \times \{\tau_{i,j,t} \times w_{i,S}\} \times YSA_{i,t})^2 + \mathcal{D} \right) \varrho_{i,j,t,S}, \quad (1.14)$$

where the exponential polynomial function: $[\exp(\hat{\varphi}_{1,S} YSA_{i,t} + \hat{\varphi}_{2,S} (YSA_{i,t})^2) - 1] \times 100$ (1.15) then represents the relocation effects of the corporate tax adoptions with respect to temporal distance between $t(e)$ and the rollout year.

Figure 1.9 – Adoption effects weighted by years since adoption



Notes: The figure reports equation 1.15 and plots the full panel and the estimate from equation 1.9. The full set of results, including aggregated sectors, can be found in Table A.1.6.

Figure 1.9 plots equation 1.15 for $YSA_{i,t} \in [0, 9]$ for the full panel and includes the estimate for the average changes in relocation flows estimated by equation 1.9. 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 A.1.6. This allows to conclude 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 1.14 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 findings are in line 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 the aggregated sectors, it seems that the effects follow 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, the temporality of the adoption shock, seem to not be driven by any particular state as shown in Figure A.1.7.

1.7 Counterfactual Experiments

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 1.8 together with the notation by [Yotov, Piermartini and Larch \[2016\]](#), allows constructing a matrix of

baseline relocation costs such that

$$\left(\hat{\Gamma}_{i,j}\right)^{-1} = \exp \left[\hat{\delta}_{i,j} - \hat{\beta}\tau_{i,j} \right]. \quad (1.16)$$

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

$$\exp \left[\hat{\delta}_{i,j} \right] = \exp \left[\ln(\text{Dist}_{i,j}) + \delta_i + \delta_j \right] \text{ if } i \neq j, \quad (1.17)$$

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 1.9 such that

$$y_{i,j}^{cf} = \exp \left[\ln \left(\hat{\Gamma}_{i,j}^{cf} \right) + \delta_{i \neq j}^{cf} + \delta_i^{cf} + \delta_j^{cf} \right] \varepsilon_{i,j}^{cf}. \quad (1.18)$$

where $\hat{\Gamma}_{i,j}^{cf} = \hat{\beta}\tau_{i,j}^{cf} + \hat{\delta}_{i,j}$. This essentially endogenizes the relocation costs. The fitted values $\hat{y}_{i,j}^{cf}$ are defined as the bilateral relocation flows in the absence of any income tax adoptions for the 1910s. Note that, $\hat{\beta}$ is estimated in equation 1.9. However, since the counterfactual is the absence of a tax introduction it holds that $\hat{\beta}\tau_{i,j}^{cf} = 0 \forall i, j$. 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

$$\text{Counterfactual } \Delta y_i = \frac{\sum_{j \neq i} (\hat{y}_{i,j}^{cf} - \hat{y}_{i,j})}{\sum_{j \neq i} \hat{y}_{i,j}}. \quad (1.19)$$

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 1.14 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 1.14, Figures A.1.8 and A.1.9 report, that almost any other state observes an increase in outflows due to the 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 to leave their state for one of the adopter states but decided not to after the change in tax policy. Figure A.1.10 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 1.14 – 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 $[(\sum_{j=1}^J (\hat{y}_{i,j}^{cf} - \hat{y}_{i,j})) / (\sum_{j=1}^J \hat{y}_{i,j})] \times 100$. Adopters are sorted ascending, and 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

$$\tau_{i,j}^{cf(\mathcal{I})} = \tau_{i,j} \times (1 - \mathbb{1}(i \in \mathcal{I})), \quad (1.20)$$

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 A.1.11. In 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.

1.8 Further Insights and Robustness

Are tax adoptions driving firm closures? Another important component of 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 over the course of a decade. This allows me to identify the number of closures, which I find no indication of being associated with the tax adoptions. A full discussion can be found in Appendix A.5.

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 $\tau_{i,j,t}^{alt} = \mathbb{1}(z_{i,t} > z_{j,t})$. 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. The newly created policy variable further allows investigating this assumption, under which $\hat{\beta} \approx \hat{\beta}^{alt}$.

The results of this exercise can be found in Table A.1.8. 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 driven 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 1.7, the effects measured with this policy variable are noticeably larger. This observation is repeated for all four aggregated sectors (see Table A.1.8). 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 A.1.12. 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 adopted in 1929, the [Advisory Commission and American Council on Intergovernmental Relations \[1995\]](#) however does not. The reason is that the reports are from 1929, when 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 A.1.8 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 excluding California, Georgia, Missouri, and Oregon from the policy variable. As reported in Table A.1.8, 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 HDTWFE model estimates an average treatment effect assuming the tax adoptions to affect firm relocations similarly in the 1910s and 1920s. The approach further allows them 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 an HDTWFE 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. To control for this decennial cluster, I create a control variable that interacts a 1920s identifier, i.e. $\mathbb{1}(t(b) = 1920)$, with a 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 A.1.8.

1.9 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. Unlike 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 in 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 allow for a clean identification of the tax shocks in an environment of very few other constraints on corporations. A structural gravity framework allows estimating changes in firm relocations caused by 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 sectors, 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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CHAPTER 1:
APPENDIX

A.1 Tables and Figures

Table A.1.1 – 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 (see [Ruggles et al. \[2021\]](#)).

Table A.1.2 – Average internal relocations and sample population for firms of different industries

Panel	Pair Outflow		State Outflow		State Sample Population		Mob.Rate
	Mean	Stand. Dev.	Mean	Stand. Dev.	Mean	Stand. Dev.	
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 exclude i, i -pairs. I define the *Mob.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 A.1.3 – Baseline stacked regressions by detailed sector panels

	Model (1)	Model (2)	Model (3)
<i>Panel A: Full Panel</i>			
$\tau_{i,j,t,s}$	0.378*** (0.091)	0.431*** (0.029)	0.122*** (0.023)
<i>Panel B: Sector-specific effects (detailed)</i>			
Agriculture	0.240** (0.099)	0.380*** (0.032)	0.168*** (0.026)
Construction	0.518*** (0.108)	0.636*** (0.043)	0.174*** (0.044)
Manufacturing (Dur.)	0.296*** (0.153)	0.116*** (0.053)	0.007 (0.055)
Manufacturing (Non-Dur.)	0.330** (0.149)	0.093* (0.049)	−0.152*** (0.054)
Mining	0.306** (0.154)	0.326** (0.134)	0.144 (0.129)
Wholesale	0.509*** (0.112)	0.356*** (0.029)	0.036 (0.030)
Business Services	0.414** (0.182)	0.010 (0.158)	−0.033 (0.175)
Entertainment	0.506*** (0.166)	0.333** (0.0143)	0.208 (0.155)
Personal Services	0.644*** (0.121)	0.530*** (0.049)	0.148*** (0.052)
Repair Services	0.670*** (0.111)	0.594*** (0.091)	0.203** (0.092)
Professional Services	0.453*** (0.140)	0.370*** (0.072)	0.040 (0.076)
Finance	0.426*** (0.143)	0.247*** (0.088)	−0.045 (0.089)
Telecommunication	0.461 (0.342)	0.551 (0.379)	0.590*** (0.382)
Transportation	0.381*** (0.112)	0.248*** (0.067)	0.005 (0.072)
Utilities	0.364 (0.270)	0.515*** (0.354)	0.263 (0.0374)

State-Decade FEs	yes	yes	yes
State-Pair FE	no	yes	yes
Decade-Interstate FE (<i>Bergstrand, Larch and Yotov [2015]</i>)	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 separated observations are excluded based on fixed effects and [Clarkson and Jenrich \[1991\]](#), and singletons are excluded using [Correia \[2015\]](#).

Table A.1.4 – Regression for tax rates at the time of adoption

	$r = \text{Max}_i$	$r = \text{Min}_i$	Tax Flatness
	(i)	(ii)	(iii)
<i>Panel A: Full Panel</i>			
$\tau_{i,j,t}^r$	0.036*** (0.006)	0.046*** (0.008)	
$(\tau_{i,j,t} \times \text{Min}_i) / \text{Max}_i \times 10$			0.014*** (0.003)
<i>Panel B: Sector-specific effects (collapsed into one row)</i>			
Manufacturing and Mercantile	0.036*** (0.006)	0.048* (0.008)	0.014*** (0.003)
Services	0.030*** (0.011)	0.030*** (0.011)	0.013*** (0.004)
Financial Sector	0.004 (0.024)	0.005 (0.027)	0.009 (0.010)
Transportation and Other Public Utilities	0.043 (0.031)	0.087* (0.044)	0.019* (0.011)

<i>Observations</i>	24,949	24,949	24,949
<i>McFadden Pseudo R²</i>	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 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 A.1.5 – Regression of effects decomposed by type of adopted income tax

	Relative	Absolute
	(i)	(ii)
<i>Panel A: Full Panel</i>		
Only personal tax adoption: $\tau_{i,j,t}^{pers}$	−0.056 (0.064)	−2.95 (3.27)
Only corporate tax adoption: $\tau_{i,j,t}^{corp}$	0.297*** (0.067)	18.69*** (4.85)
Both taxes adopted: $\tau_{i,j,t}^{both}$	0.074*** (0.025)	4.12*** (1.43)
<i>Panel B: Sector-specific effects</i>		
Manufacturing and Mercantile		
$\tau_{i,j,t}^{pers}$	−0.069 (0.066)	−3.31 (3.03)
$\tau_{i,j,t}^{corp}$	0.285*** (0.069)	16.27*** (4.51)
$\tau_{i,j,t}^{both}$	0.074*** (0.025)	3.83*** (1.35)
Services		
$\tau_{i,j,t}^{pers}$	0.068 (0.116)	0.21 (0.36)
$\tau_{i,j,t}^{corp}$	0.309*** (0.087)	1.07*** (0.35)
$\tau_{i,j,t}^{both}$	0.079* (0.045)	0.24* (0.14)
Financial Sector		
$\tau_{i,j,t}^{pers}$	0.445* (0.256)	0.29* (0.20)
$\tau_{i,j,t}^{corp}$	0.403** (0.171)	0.25** (0.13)
$\tau_{i,j,t}^{both}$	−0.056 (0.109)	−0.03 (0.05)
Transportation and Other Public Utilities		
$\tau_{i,j,t}^{pers}$	−0.074 (0.170)	−0.06 (0.12)
$\tau_{i,j,t}^{corp}$	0.085 (0.272)	0.07 (0.23)
$\tau_{i,j,t}^{both}$	0.160 (0.102)	0.13 (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 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 $(\exp(\hat{\beta}_S) - 1) \times \bar{y}_S$.

Table A.1.6 – Regression of effects w.r.t. years since adoption

	Linear	Polynomial
	(i)	(ii)
<i>Panel A: Full Panel</i>		
$\tau_{i,j,t} \times \text{YSA}_{i,j,t}$	0.018*** (0.005)	0.059*** (0.016)
$\times \text{YSA}_{i,j,t}^2$		-0.005** (0.002)
<i>Panel B: Sector-specific effects</i>		
Manufacturing and Mercantile	0.018*** (0.005)	0.061** (0.002) -0.006** (0.002)
Services	0.030*** (0.010)	0.033 (0.031) -0.000 (0.004)
Financial Sector	-0.009 (0.026)	-0.006 (0.070) -0.000 (0.009)
Transportation and Other Public Utilities	0.031* (0.018)	0.083 (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 1.14 excluding the polynomial term $\text{YSA}_{i,j,t}^2$.

Table A.1.7 – Sector specific results decomposed by bordering status

	Bordering	Not Bordering
	(i)	(ii)
<i>Panel B.1: Manufacturing and Mercantile Interaction</i>		
$\tau_{i,j,t}$		0.188*** (0.029)
$\tau_{i,j,t} \times \text{Border}_{i,j}$	-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
<i>Panel B.2: Services Interaction</i>		
$\tau_{i,j,t}$		0.244*** (0.046)
$\tau_{i,j,t} \times \text{Border}_{i,j}$	-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
<i>Panel B.3: Financial Interaction</i>		
$\tau_{i,j,t}$		0.112 (0.108)
$\tau_{i,j,t} \times \text{Border}_{i,j}$	-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
<i>Panel B.4: Transportation and Other Public Utilities Interaction</i>		
$\tau_{i,j,t}$		0.220* (0.0116)
$\tau_{i,j,t} \times \text{Border}_{i,j}$	-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 A.1.8 – Collection of further insights and robustness exercises

	Baseline	Alternative	Traditional	Include WA	Pushed	South Control
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
<i>Panel A: Full Panel</i>						
$\tau_{i,j,t}$	0.122*** (0.023)	0.150*** (0.029)	0.027 (0.037)	0.172*** (0.041)	0.094*** (0.027)	0.123*** (0.023)
				0.103*** (0.022)	−0.029 (0.036)	0.001 (0.033)
<i>Panel B: Sector-specific effects</i>						
Manufacturing and Mercantile	0.121*** (0.023)	0.143*** (0.029)	0.022 (0.037)	0.169*** (0.042)	0.095*** (0.028)	0.123*** (0.024)
Services	0.138*** (0.042)	0.181*** (0.049)	0.043 (0.065)	0.114** (0.073)	0.089* (0.050)	0.133*** (0.042)
Financial Sector	0.047 (0.092)	0.076 (0.110)	0.029 (0.143)	0.115 (0.172)	−0.005 (0.123)	0.061 (0.092)
Transportation and Other Public Utilities	0.155* (0.092)	0.128 (0.111)	−0.027 (0.144)	0.095 (0.168)	0.207* (0.109)	0.143 (0.093)
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 A.1.9 – Estimation results for 4WFE and staggered gravity

	Model(3b): 4WFE	Heterogeneous Treatment Effects		
		Group 1: 1910s Adopters	Group 2: 1920s Adopters	
<i>Panel A: All</i>				
		1910s	1920s	1920s
$\tau_{i,j,t}^{trad}$	0.295*** (0.041)	0.633*** (0.104)	0.660*** (0.124)	0.162*** (0.045)
<i>Panel B: By Adoption Group</i>				
ATT		0.646*** (0.113)	0.162*** (0.045)	
Observations	6,765		6,765	
McFadden Pseudo R^2	0.997		0.997	
<i>Panel C: Dynamic Aggregation</i>				
		at t	at $t + 10$	
Event Study		0.398*** (0.057)	0.660*** (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 A.1.1 – Share of incorporated manufacturing firms

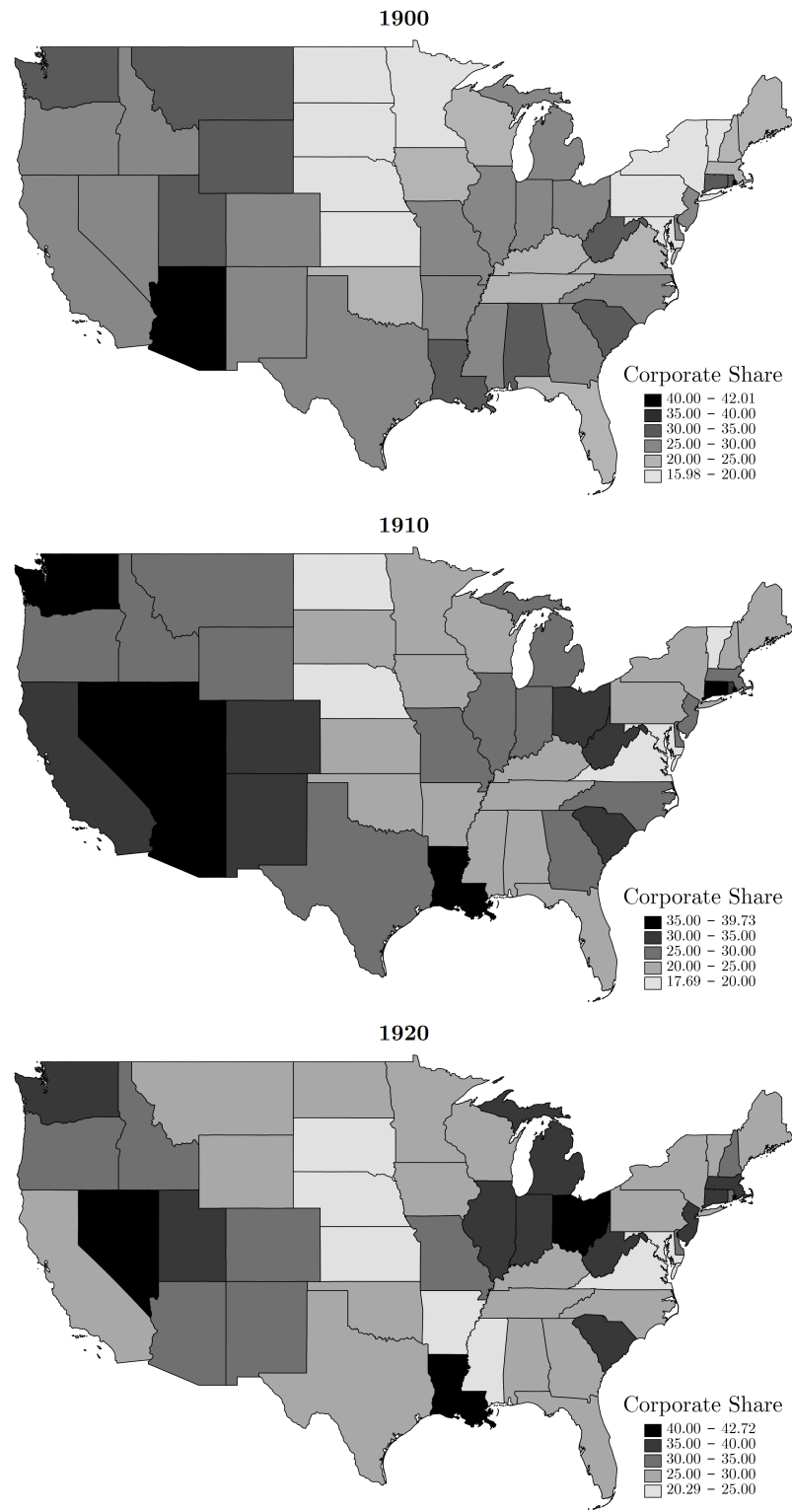
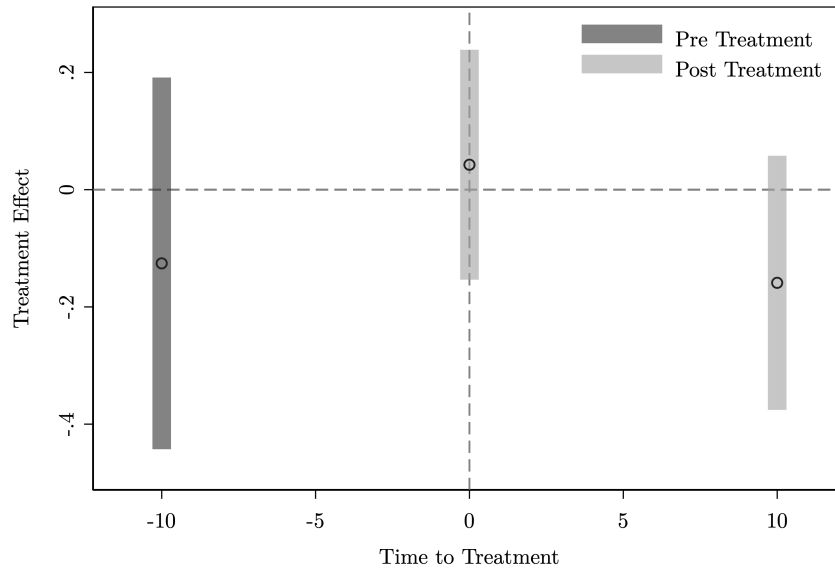
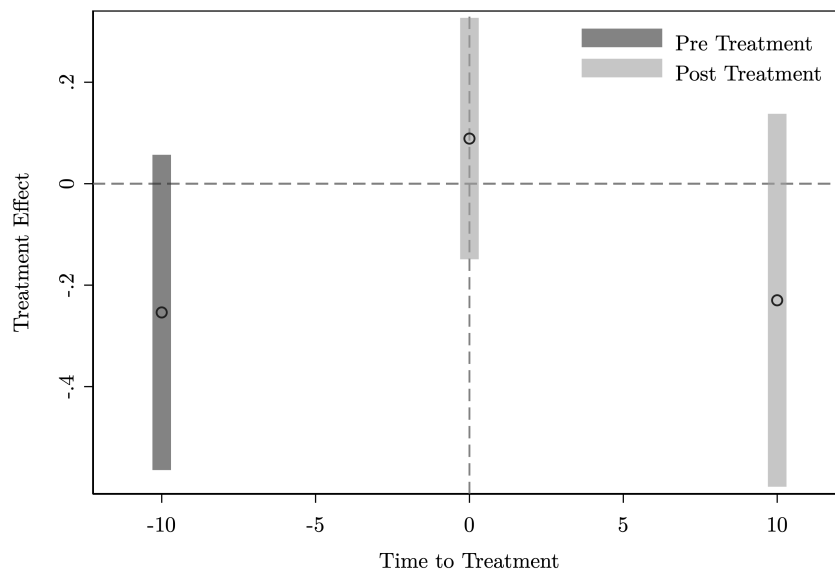


Figure A.1.2 – Estimates effects of tax adoptions on state-level revenues**Figure A.1.3** – Estimates effects of tax adoptions on state-level expenditures

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 A.1.4 – Average number of administrative personnel in each corporation

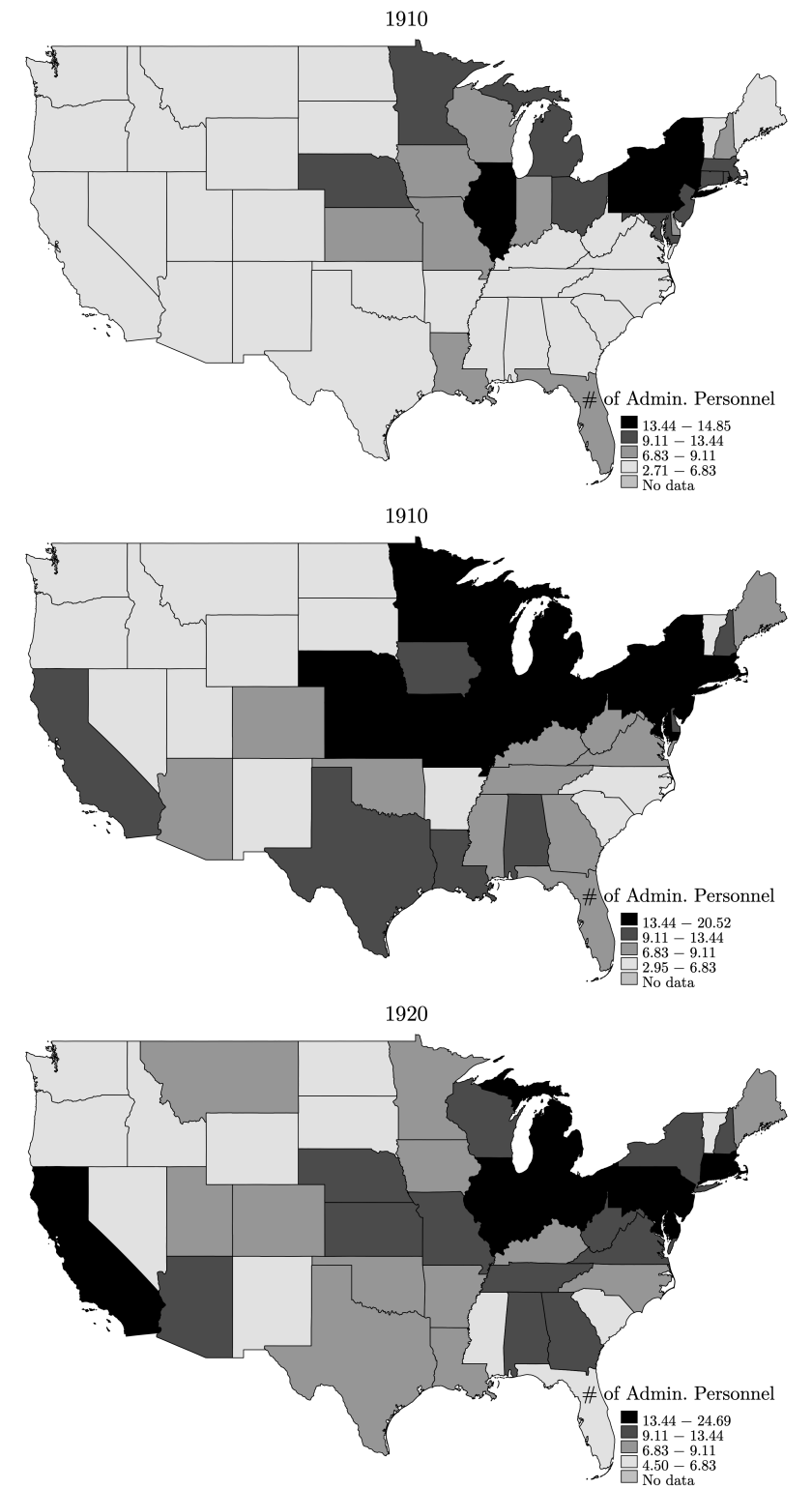
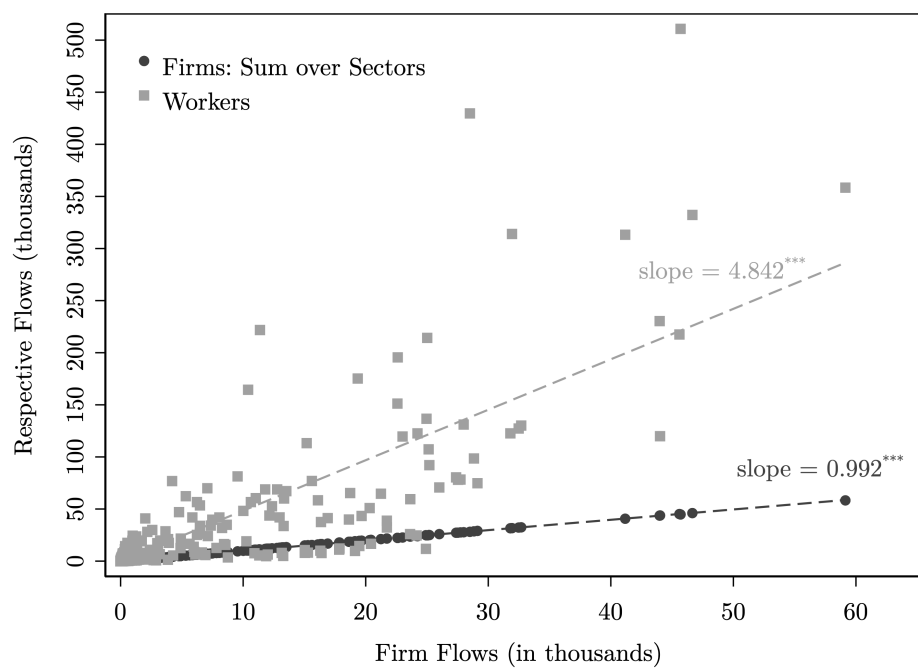
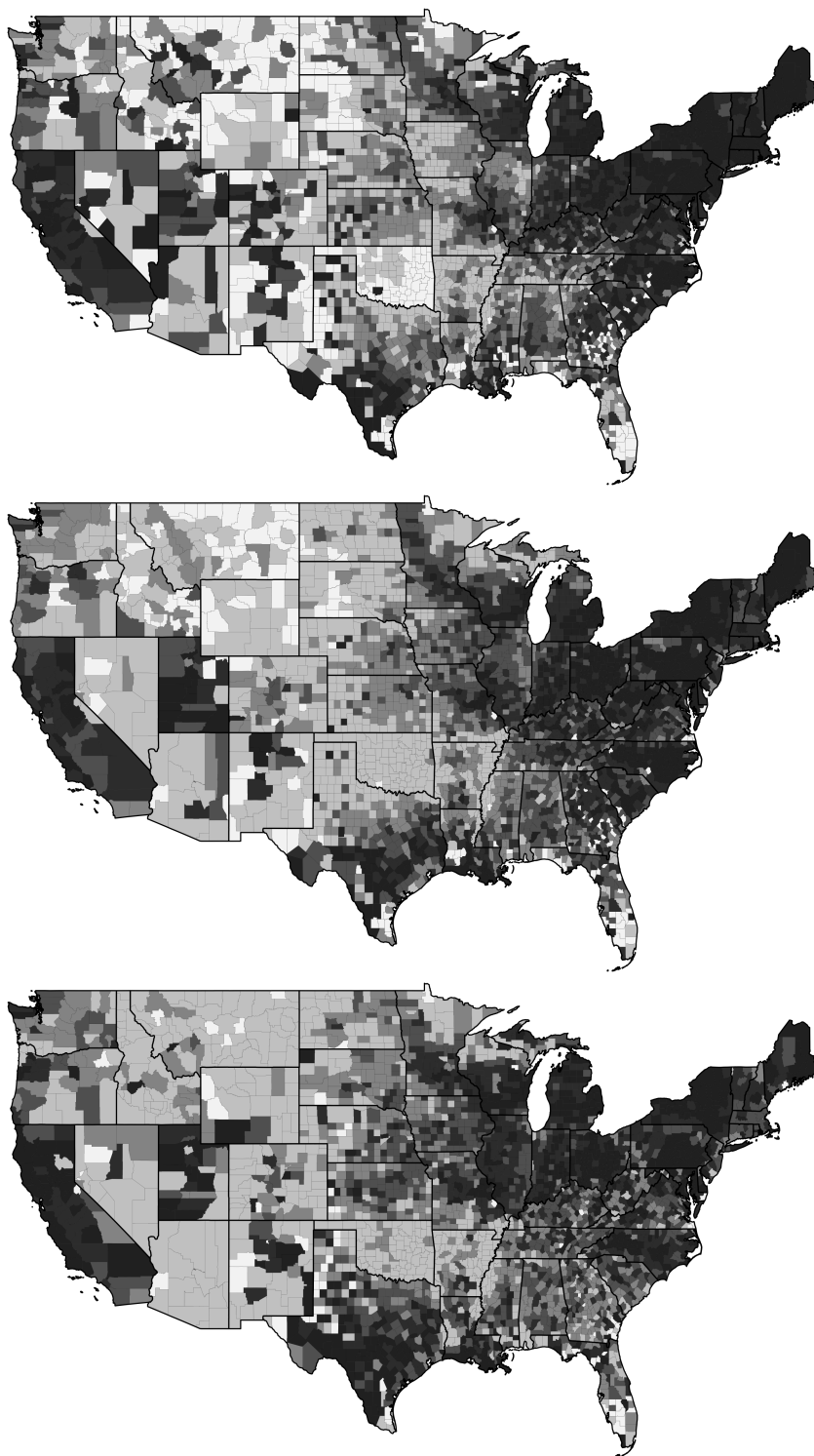


Figure A.1.5 – Scatter and correlation between bilateral firm and worker flows



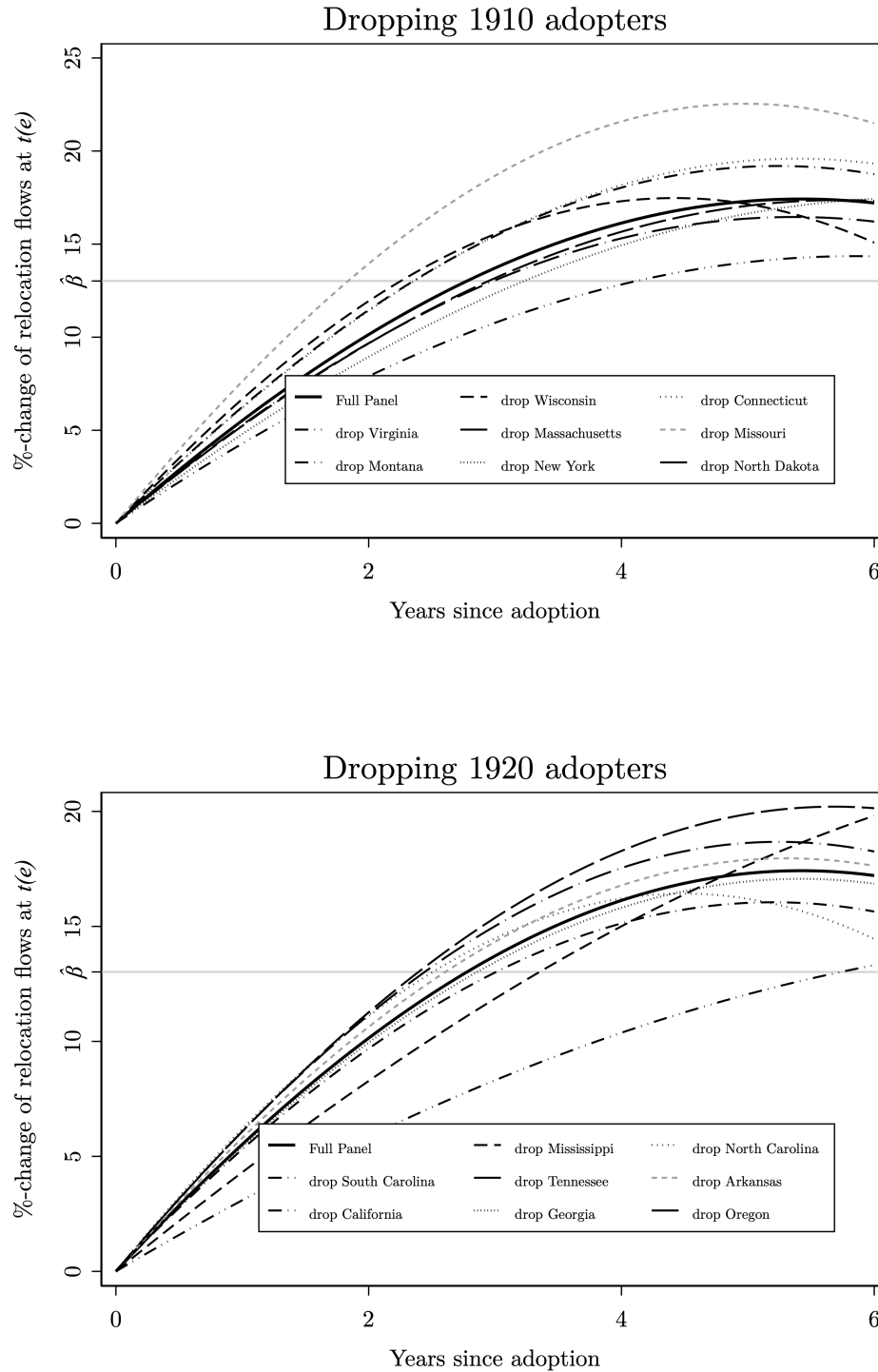
Notes: Darker dots and line represent the scatter and correlation between the firms counted in the linked data and the sum of all firms of each sector. The slope indicates a strong correlation. Lighter dots and line show the number of worker flows associated with the number of firm flows. On average, 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 A.1.6 – County-level mobility rates for 1900s, 1910s and 1920s

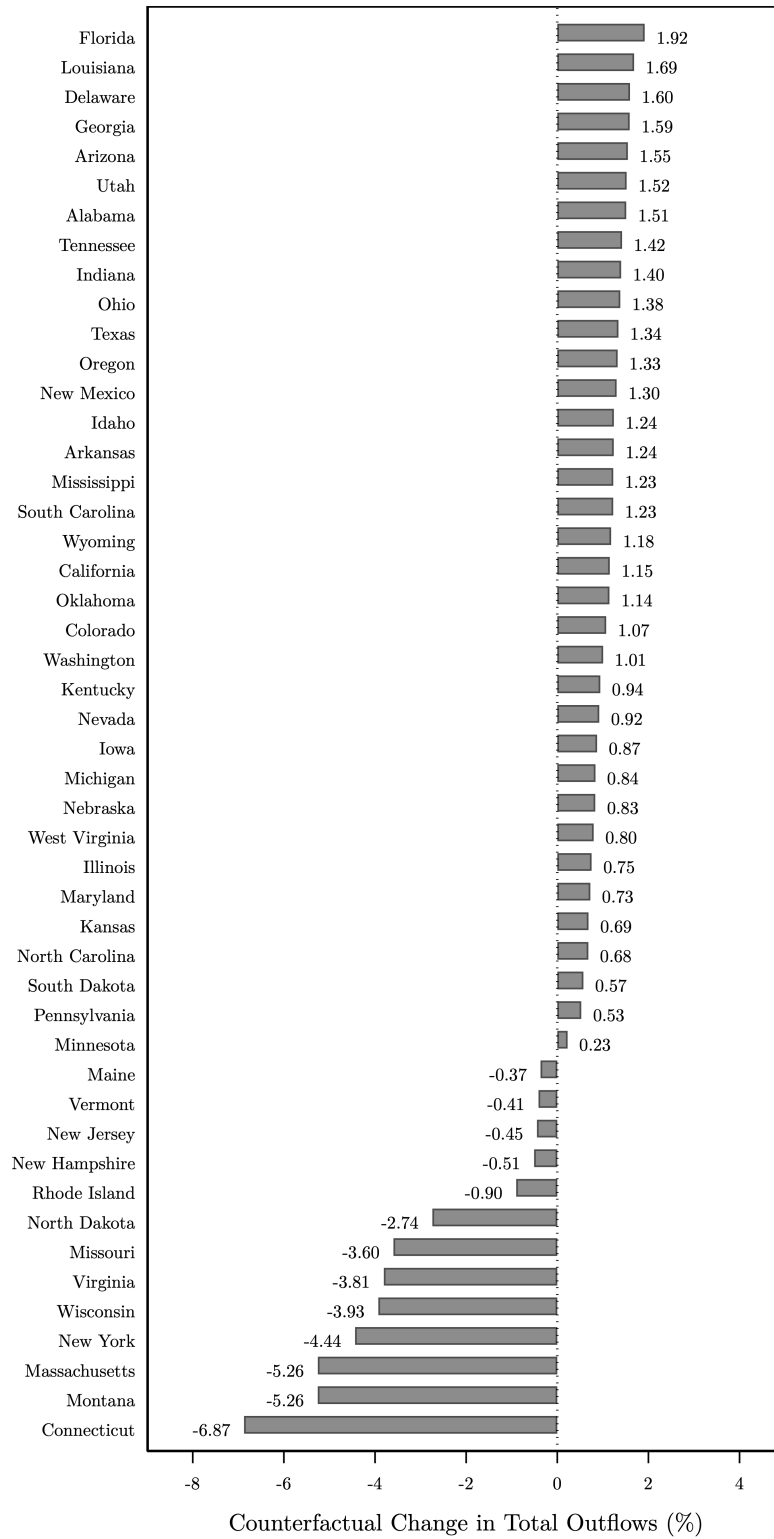


Notes: Darker shaded areas refer to higher mobility rates in a respective county, defined as the average outflow share of the average sample population.

Figure A.1.7 – Robustness of persistency effects of tax adoptions using Jackknife



Notes: Jackknife effects are estimated by regression model from equation 1.14 by dropping each adopter state at a time. The overall effect is plotted as a reference in both figures.

Figure A.1.8 – Counterfactual change in relocation flows if no state adopts in the 1910s

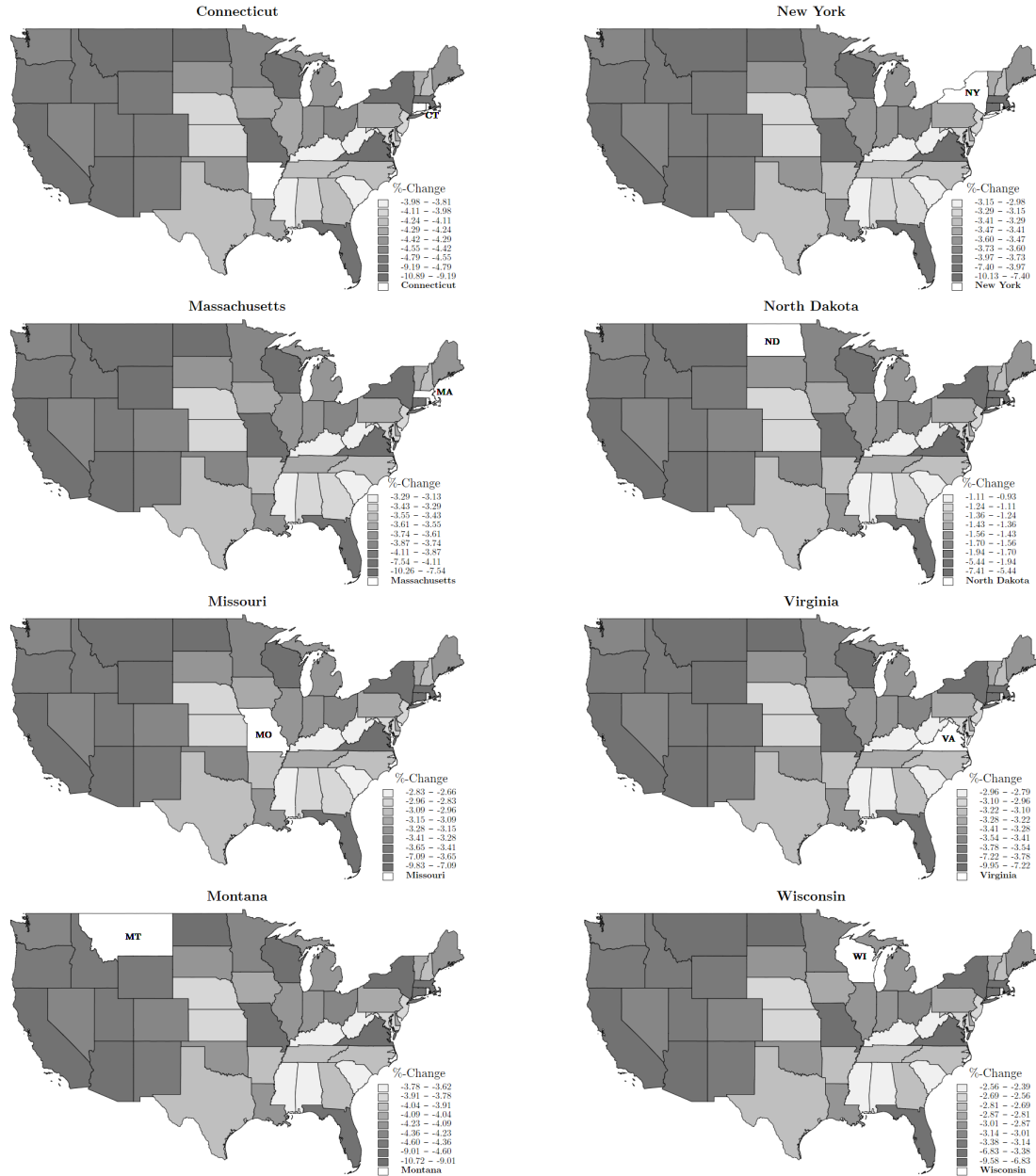
Notes: The labels report percentage changes so that $[(\sum_{j=1}^J (\hat{y}_{i,j}^f - \hat{y}_{i,j})) / (\sum_{j=1}^J \hat{y}_{i,j})] \times 100$. Figure depicts changes for the 1910s only and bars are sorted by size of the counterfactual change.

The map displays the percentage change in the population aged 65 and older for each state. The values are as follows:

State	Percentage Change
Alaska	1.01
Arizona	1.33
California	1.24
Colorado	1.18
Connecticut	0.92
Delaware	1.15
District of Columbia	1.52
Florida	1.55
Georgia	1.30
Hawaii	1.34
Idaho	1.07
Illinois	1.52
Indiana	1.30
Iowa	1.07
Kansas	1.34
Kentucky	1.14
Louisiana	1.24
Maine	1.23
Maryland	1.51
Massachusetts	1.59
Michigan	1.92
Minnesota	1.42
Mississippi	1.23
Missouri	1.51
Montana	1.59
Nebraska	1.92
Nevada	1.42
New Hampshire	1.23
New Jersey	1.51
New Mexico	1.59
New York	1.92
North Carolina	1.42
North Dakota	1.23
Ohio	1.51
Oklahoma	1.59
Oregon	1.92
Pennsylvania	1.42
Rhode Island	1.23
South Carolina	1.51
South Dakota	1.59
Tennessee	1.92
Texas	1.42
Vermont	1.23
Virginia	1.51
Washington	1.59
West Virginia	1.92
Wisconsin	1.42
Wyoming	1.23

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

Figure A.1.10 – Counterfactual change in bilateral relocation flows by adopter states

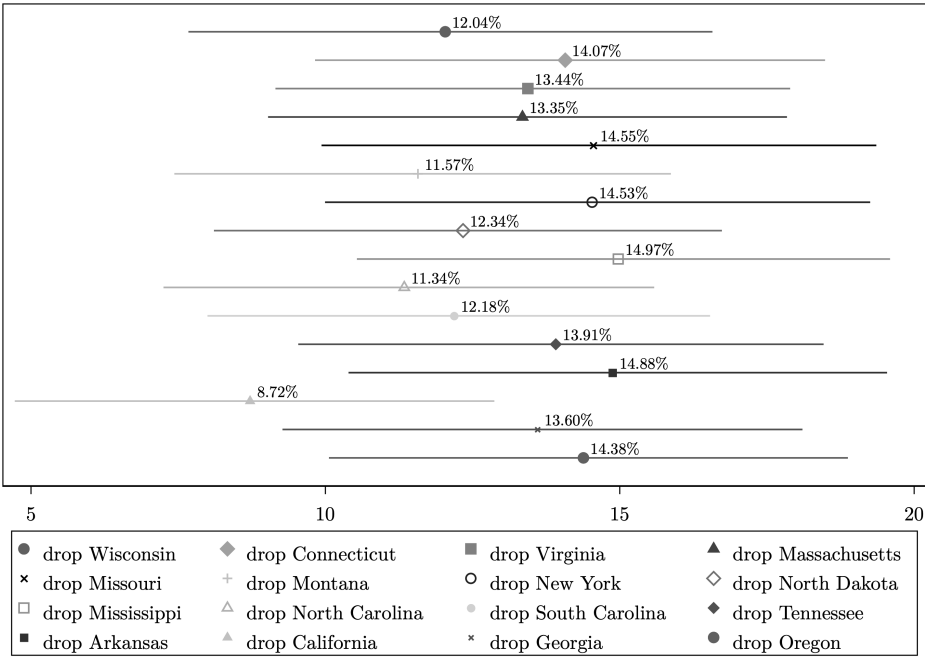


Notes: The labels report percentage changes so that $[(\hat{y}_{i,j}^{cf} - \hat{y}_{i,j}) / (\hat{y}_{i,j})] \times 100$ if $i \neq j$. Figure depicts changes for the 1910s only.

Figure A.1.11 – Counterfactual change in relocation flows by adopter state not adopting

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

Figure A.1.12 – 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.

A.2 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 1.5, as the dyadic panel structure allows controlling for state-time fixed effects in the gravity estimation. To identify potential driving factors of the adoption, I estimate an explorative model of the following structure

$$z_{i,t(e)} = \beta_0 + \sum_{n=1}^N \beta_n \ln(\text{TV Predictor}_{i,t(b)}^n) + \sum_{m=1}^M \beta_m \ln(\text{Const. Predictor}_i^m) + \text{Decade FE} + \varepsilon_{i,t}, \quad (\text{A.2.1})$$

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 B.1, 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, firm-related variables, includes firm outflows and shares by type of firm. 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 B.1 – Explorative model of potential drivers of the tax adoption

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

<i>Decade FE</i>	Yes	Yes
<i>Observations</i>	137	96
<i>Adjusted R²</i>	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 the 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.

A.3 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 for 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 are 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 with the outflow responses captured by the main model? I start by repeating a similar analysis as Liu [2014], where I first estimate

$$\text{Corporate Share}_{i,t} = \beta z_{i,t} + \mathbf{Controls} + \mathbf{FE} + \varepsilon_{i,t}, \quad (\text{A.3.1})$$

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 A.3.1, $\hat{\beta}$ captures the total estimated effect of the tax adoption on corporate activity. Next, I estimate a mediator equation

$$\log(\text{Total Outflow}_{i,t}) = \theta z_{i,t} + \mathbf{Controls} + \mathbf{FE} + \tilde{\varepsilon}_{i,t}. \quad (\text{A.3.2})$$

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

$$\text{Corporate Share}_{i,t} = \check{\beta} z_{i,t} + \rho \log(\text{Total Outflow}_{i,t}) + \mathbf{Controls} + \mathbf{FE} + \check{\varepsilon}_{i,t}. \quad (\text{A.3.3})$$

Utilizing estimates from equations A.3.1 to A.3.3 allows to compute $F = (\theta \times \rho) / \beta = (\beta - \check{\beta}) / \beta$. Results of the exercise can be found in Table C.1.

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 $(\exp(\hat{\theta}) - 1) \times 100 \approx 11.52\%$. 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 A.3.1. This suggests partial mediation, quantified by $\hat{F} \approx 0.09$. In other words, this means that around one-tenth of the decrease in corporate activity can be explained by firm outflows.

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

Table C.1 – Estimates for corporate activity and firm relocation

	Corporate Share _{<i>i,t</i>}	log(Total Outflow _{<i>i,t</i>})
	(i)	(ii)
Equation (1): Total Effect		
$z_{i,t} : \beta$	−0.034 (0.011)***	

<i>Adjusted R</i> ²	0.9032	
Equation (2): Mediator		
$z_{i,t} : \theta$		0.1090 (0.054)**

<i>Adjusted R</i> ²		0.9708
Equation (3): Direct Effect		
$z_{i,t}$	−0.031 (0.010)***	
log(Total Outflow _{<i>i,t</i>}) : ρ	−0.027 (0.016)*	

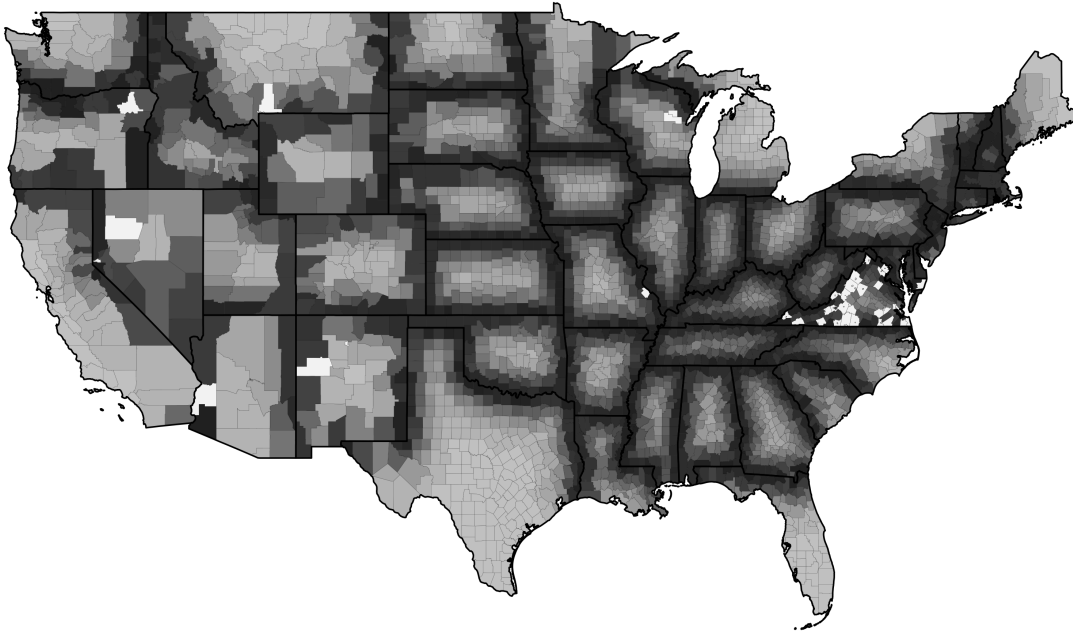
<i>Adjusted R</i> ²	0.9075	
<i>Controls</i>	Yes	Yes
<i>Fixed Effects</i>	Yes	Yes
<i>Observations</i>	144	144

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 .

A.4 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 the merging of information of every county $w(i)$'s distance to each neighboring state $k \in \mathcal{B}_i$ of state i . Two coordinates are needed to measure the distance. 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 $\text{Dist}(w(i); k)$. Each county $w(i)$ therefore has a set of distances equal to the number of states in $\mathcal{B}_i$. The measure of distance to the closest state border is then defined as $\text{MD}_{w(i)} = \min_{k \in \mathcal{B}_i} \{\text{Dist}(w(i); k)\}$. To validate the newly created variable, I plot a U.S. county map (Figure D.1).

Figure D.1 – 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 concerning distance. In larger counties, firms may be further from the border, even when their county is situated at the state's edge.

A.5 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 stated as

$$\# \text{ of Switchers}_{i,t} = \beta_1 \text{Tax Adoption}_{i,t} + \beta_2 \text{Migrant}_{i,t} + \beta_3 \text{Interaction} + \text{FE} + \varepsilon_{i,t}, \quad (\text{A.5.1})$$

where $\text{Switcher}_{i,t}$ define employers who lived in state i at $t(b)$ who became wage-earners by $t(e)$. The indicator $\text{Migrant}_{i,t}$ 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 $\text{Tax Adoption}_{i,t}$ 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 $\text{Migrant}_{i,t}$ is equal to one or zero. With 48 states, this yields 192 observations.

Table E.1 – Correlation between firm closures and corporate tax adoptions

	# of Switchers _{<i>i,t</i>}
Tax Adoption _{<i>i,t</i>}	-0.76 (493.81)
Migrant _{<i>i</i>}	-1560.40*** (371.71)
Tax Adoption _{<i>i,t</i>} × Migrant _{<i>i</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.

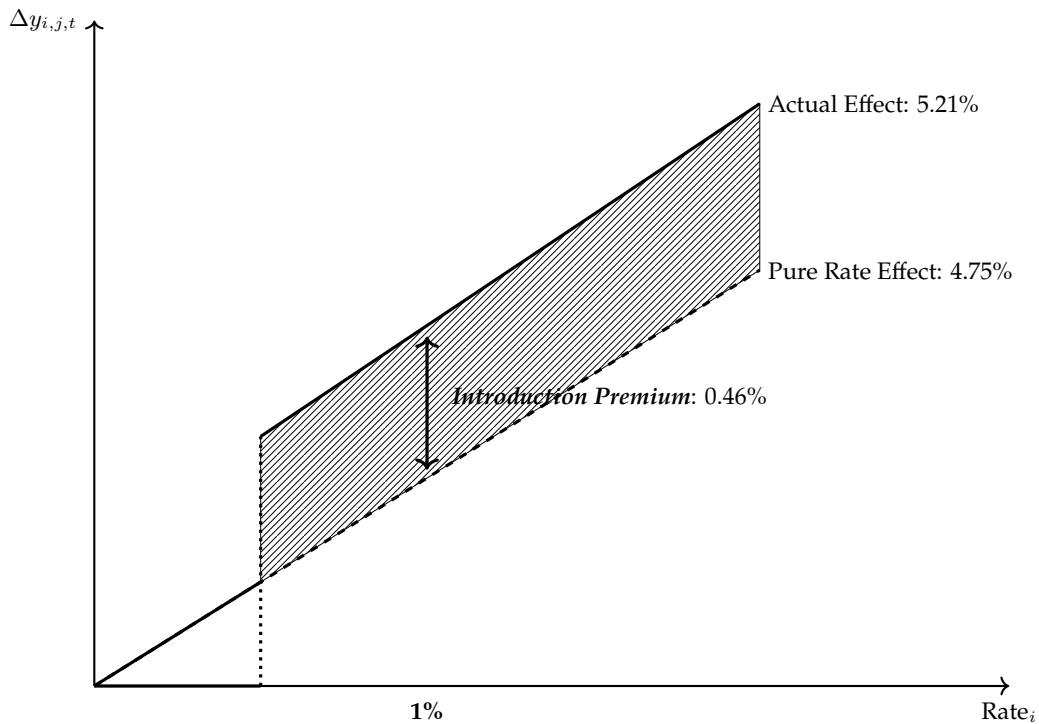
I report the estimation results in Table E.1. First, it appears that the adoption of the tax policy did not significantly influence the transition of individuals from employers to wage earners. As such, I find no evidence to suggest that the introduction of corporate taxes had an immediate discernible 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 insinuate that employers deciding to move due to the tax adoptions will move with their operation.

A.6 Introduction Elasticity Premium

The average introduction elasticity at the minimum rate is computed by $\hat{\beta}/2.5 = 5.21\%$ and Section 1.6.2 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 F.1 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 are 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 1.6.2, which itself is contaminated by the adoption. This exercise further assumes linearity in the marginal effect of the rate changes and separability of the adoption effect. For simplicity, I further assume no change in the slope at the adoption.

Figure F.1 – Stylized visualization of tax elasticity premium for on the introduction



Chapter 2

A Taxing Journey: Tax Adoptions and Interstate Migration in the Early 20th Century

with Kirsten Wandschneider

Abstract

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

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

Keywords: Income Tax Adoption, Internal Migration, Fiscal Decentralization

2.1 Introduction

When and how do people avoid taxation? This is a critical question to consider when implementing a new tax. A plausible way of avoiding taxation is to move. If individuals choose to live where their preferences over taxation and public goods provision are best aligned with the local policies, tax competition between governments will spur mobility. This idea goes back to Tiebout [1956] and Tullock [1971], who state that individuals can "vote with their feet" when responding to changes in taxation.¹ To test for the extent of tax avoidance through migration, we exploit a unique natural experiment, namely the unharmonized and sequential introduction of state income taxes in the U.S. of the first third of the 20th century. This historical setting enables us to causally identify the effects that personal and corporate state income tax adoptions had on migration patterns, in the absence of typical barriers, such as language, legal status, cultural differences, or recognition of prior education.²

During our observation period, nineteen states adopted either personal or corporate income taxation, seven of these adopted both simultaneously.³ We combine the information on state-wise tax adoptions with linked census data from 1900-1930 to identify how many individuals moved between states. The complete count census allows to aggregate dyadic flows of individuals, both overall and for a selection of demographic characteristics. The resulting dataset allows us to analyze bilateral migration patterns for a representative cross-section of the U.S. population in the early 20th century. To establish the causal relationship between taxation and migration, it is critical for our identification strategy that tax adoptions were not driven by internal migration. In the historical setting, the main driver for the tax adoptions was political opportunity, such as a neighboring state's adoption of income tax (regional diffusion). As argued in a report by the National Industrial Conference Board [1930], the avoidance of state property taxes was a key contributor to the introduction of state income taxes, as local municipalities strategically undervalued properties to attract owners. None of this points to the adoption of income taxes as a systemic response to preceding migration patterns. Historical studies on a state level provide more detailed discussions on adoptions, including rate progressivity and legal definitions of income [Kennan, 1915; Bullock, 1918; Hannan, 1922; Whitehead, 1921; Thoresen, 1928; Briggs,

¹The re-allocative effects of interstate fiscal competition on the population are discussed in works on fiscal federalism, e.g. Oates et al. [1972], Wooders [1978] and Hochman, Pines and Thisse [1995].

²As argued in Prud'Homme [1995], sub-national economies, such as states, are more open than sovereign countries, making it substantially easier to move between jurisdictions.

³Three quarters of the adopter states introduce both income taxes within the same decade, three states only adopt either the personal or corporate income tax.

1936].⁴

Methodologically, we utilize advances from the gravity literature to study the effects of tax adoptions on interstate migration. Our theoretical framework builds on a combination of classical discrete migration choice using [McFadden \[1973\]](#) and the standard gravity approach by [Anderson and Van Wincoop \[2003\]](#). [Anderson \[2011\]](#) establishes a link between migration and gravity theory with multilateral resistance. The resulting structural equation can be consistently estimated through a Poisson model, as first introduced in [Silva and Tenreyro \[2006\]](#). In contrast to recent seminal papers in the interstate migration literature, such as [Moretti and Wilson \[2017\]](#), the key advantage of the gravity approach is the ability to extensively control for the entire universe of time-varying state-specific push and pull factors that could otherwise lead to a substantial omitted variable bias. To our knowledge, we are the first to rely on a gravity model for the analysis of historical migration data.

Our work extends the literature on contemporary effects of taxation on relocation, which has identified significant mobility effects for scientists, inventors, and high earners regarding taxation [[Young et al., 2016](#); [Moretti and Wilson, 2017](#); [Kleven et al., 2020](#); [Akcigit et al., 2022](#)]. Similarly, in Switzerland, young college graduates respond to tax differentials with higher mobility [[Liebig, Puhani and Sousa-Poza, 2007](#)], and [Saltz \[1998\]](#) and [Cohen, Lai and Steindel \[2014\]](#), both using U.S. data from the 1990s and early 2000s, to show that citizens move across state borders to reduce their income tax burden. Using decentralized tax changes in Italy, [Rubolino and Giommoni \[2023\]](#) show that taxation greatly shapes location choices. As [[Kleven et al., 2020](#)] have emphasized, the literature on recent income tax changes and introductions especially lacks estimates on the broader cross-section of the population, something that our approach can provide. An additional advantage of our historical setting is that it allows us to cleanly estimate Tiebout competition on the introduction of new taxes, i.e. changes at the extensive margin. In the contemporary context, income tax adoptions are rare, so the empirical literature typically focuses on tax changes at the intensive margin. Furthermore, contemporary tax policies are increasingly complex, making it challenging to measure tax differentials between different jurisdictions. The introduction of a new tax, however, possibly serves as a stronger shock than a change in an existing rate. Citizens may respond more strongly to the introduction of a previously non-existent tax than to an incremental change in an existing tax rate, as the former represents a more salient fiscal policy shift and introduces new administrative burdens. The estimation of the extensive

⁴Contemporary scholars also report the mechanism of the introductions [[Comstock, 1920](#); [Lutz, 1920](#); [Bigham, 1929](#); [Berry and Berry, 1992](#); [Hall, Lacombe and Tackett, 2020](#)]

margin responses is thus especially relevant for these early adopter states.

Our paper also relates to the work of [Cassidy, Dincecco and Troiano \[2024\]](#). In an independent line of research focusing predominantly on the revenue effects of the state income introductions, they identify migration responses to the introduction of state income taxes only for young families for the period after WWII. Our work significantly extends their results on migration, by focusing on the full cross-section of the population in the pre-WWII period, the time of the very first state tax adoptions. By relying on a structural gravity model of bilateral state-to-state migration flows we also select a methodology, which can better control for possible time-varying confounders. In contrast to their findings, we estimate causal and statistically significant increases in migration flow elasticities for early adoptions pre-WWII. Hence our findings address a significant gap in the literature by confirming migrational responses across all adoption periods. Our paper moreover contributes to a growing literature on historical interstate migration, focusing on different causes of mostly large-scale migration among states in the 20th century [[Molloy, Smith and Wozniak, 2011](#); [Boustan, Kahn and Rhode, 2012](#); [Hornbeck and Naidu, 2014](#)]. We show that people moved in response to the introduction of state taxes, in addition to the two large-scale interstate migration waves which took place in the 20th century: First, the persistent westward migration [[Zimran, 2022](#)], and second, the Great Northward migration, mainly driven by Black families [[Derenoncourt, 2022](#)].

Our baseline estimates reveal an 8.7% increase in interstate migration flows in response to the introduction of state-level personal income taxation. In line with [Borjas \[1987\]](#), these effects are more pronounced for young individuals with above-median income. By incorporating the adopted tax rates, we refine our analysis to quantify the effects on an intensive margin – a 1% larger tax rate at introduction, increased inter-state migration flows by 1.83%. Moreover, we identify a secondary channel through which income taxation influences interstate migration: the adoption of corporate income taxes raises migration flows by 11.3%.⁵ We conjecture that corporate taxation drives away industry, consequently affecting the movement of workers. Our findings thus provide causal evidence of substantial migration responses to the early adoption of state income taxation, supporting the foot-voting hypothesis.

Our paper proceeds as follows: Section 2.2 provides the historical context for the personal and corporate state taxa adoptions, section 2.3 outlines our gravity model of interstate migration. Section 2.4 presents the empirical estimation of the model while section 2.5 presents the data,

⁵[Smutny \[2024\]](#) studies the relocations of firms in response to the introduction of the corporate state income taxes.

including summary statistics. Section 2.6 gives our results and robustness checks and section 2.7 concludes.

2.2 Historical Context

The Tenth Amendment to the Constitution is the basis of the American Federalism, allowing states to impose their own taxes. In 1911, Wisconsin became the first state to legally impose a modern statute on income taxation.⁶ During this period and under the political influence of Robert M. La Follette Sr., Wisconsin had become one of the most progressive states. The adoption of state income taxation was primarily justified by redistributing wealth from the manufacturing to the agricultural sector, as opposed to raising revenues [Stark, 1987]. The law was passed even prior to the federal government's decision to impose federal income taxes (which became effective two years later with the Sixteenth Amendment to the Constitution, 1913). Between 1912 and 1921, five more states adopted state income taxes.⁷ Similarly to Wisconsin, tax implementations during this adoption period were often driven by progressive motives such as wealth redistribution, as well as the failure of property taxation in most of the states due to competitive undervaluation. Hence, the adoption of income taxes can be seen as a means to adjust revenues rather than driven by the intent to raise additional revenues. Furthermore, expenditures did not change significantly in the states adopting the tax [Cassidy, Dincecco and Troiano, 2024]. The introduction of the state income taxes can be seen as a local fiscal centralization, from the local to the state level, which led to an increase in the state share of state-local expenditures from 13.2% in 1913 to 24.1% in 1932 [Wallis and Oates, 1988]. Berry and Berry [1992] show that fiscal health of the state was an insignificant factor in state income tax adoptions between 1916 and 1929. Later adoptions of state taxes can be attributed to 'political opportunity', as these taxes were a direct response to either regional elections, local fiscal crises or to the adoption of certain taxes in neighboring states.⁸ Further note that, as shown in Figures A.2.1 and A.2.2 (in ??), the adoptions were promoted in both Democratic and Republican states. These comparative findings are relevant for an empirical estimation of average tax effects and the juxtaposition of adopter and non-adopter states.

⁶Technically, Hawaii was the first to adopt an income tax in 1901. However, Hawaii only became a U.S. state in 1959 and is thus not included in this analysis.

⁷Mississippi, Connecticut, Virginia, Massachusetts, Delaware, Missouri, Montana, North Carolina, North Dakota, and New York.

⁸By 1933, half of the Contiguous United States, introduced some version of state-specific personal income taxation. As of today, there remain nine states without state labor income tax: Alaska, Florida, Nevada, New Hampshire, South Dakota, Tennessee, Texas, Washington and Wyoming.

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

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

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

He warned the committee that such a measure would be resented, evaded, and avoided by the people, and declared that its enactment would result in the State's losing many estimable citizens "willing to pay their just share but intolerant of any unjust scheme." [New York Times on April 9, 1919]

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

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

In many states, the adoption of personal income taxes was accompanied by the introduction of corporate taxes. Between 1911 and 1929, 16 states implemented corporate taxes, with 7 of them introducing these concurrently with personal income taxes. In 2 states, the adoption of corporate taxes came within a decade after the introduction of personal taxes. In 7 states, the implemen-

tation of corporate taxes preceded personal tax adoption.⁹ [Smutny \[2024\]](#) provides an in-depth analysis on the functionalities of these early corporate income taxes.

Empirically, we can treat them separately. In order to partially disentangle the effects, we use different income groups (as detailed in Section 2.6.2).¹⁰ As with personal income taxes, we expect a significant exodus of companies from states that are early adopters of corporate taxation. Historical evidence substantiates this perspective through publicly debated claims and explicit threats of companies considering relocation due to state-level corporate taxation. A prime illustration of this phenomenon again is the state of Wisconsin. Starting with its introduction in 1911, the Wisconsin state corporate tax sparked controversy within the corporate sphere. Numerous businesses declared their intention to depart for states without income taxation, illuminating the discourse. In the *Wausau Pilot* on August 13, 1912, an article on state income taxation mentions that firms like *Frazier* in Manitowoc, *Presto-Lite* in Milwaukee, and *The Brunswick-Balke-Collander* in Jainesville left the state. Moreover, the *Ringling Bros.*, a highly valued circus enterprise headquartered in Baraboo, Wisconsin, had planned to transfer its operations to Illinois.

Dislike of the operations of the recently enacted income tax law is back of their decision. They assert the law is unfairly oppressive, and is practically driving them from the state. [...] The income tax has been one of the bones of contention in Wisconsin ever since its enactment, and the prospective action of Ringling Bros., now promises to give it new prominence. [Wausau Pilot on August 13, 1912]

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

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

⁹In 5 states, the corporate tax preceded the personal tax by less than 10 years.

¹⁰We can show that a condensed measure of both introductions yield very similar results to our approach.

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

2.3 A Gravity Model of Interstate Migration

For the theoretical foundation of our analysis, we adapt a tractable framework based on the gravity trade literature, in particular, [Anderson and Van Wincoop \[2003\]](#) and [Weidner and Zylkin \[2021\]](#). We utilize the gravity model of trade with multilateral resistances in a migration setting by following [Anderson \[2011\]](#) and [Beine, Bertoli and Fernández-Huertas Moraga \[2016\]](#). Hereby, we model bilateral flows between all U.S. states, enabled by our linked census data described in Section 2.5. This allows us to motivate our empirical approach and enables us to formalize theoretical predictions and rationalize our findings. Moreover, the model helps us to claim causality and directional properties (a challenge we discuss in Section 2.4).

Consider the migration across the contiguous United States with origin state i and destination state j .¹¹ We define the utility of living in destination j as x_j , interpreted as a collection of additive financial factors, such as wages.¹² Individual h migrating from i to j faces iceberg costs of migration $d_{i,j} := c_{i,j} / \exp(\alpha \tau_{i,j})$, where $c_{i,j}$ captures standard costs such as geographic distance between i and j . Another cost factor is the introduction of state income taxation $\tau_{i,j}(z_i, \cdot)$, where z_i is defined as an indicator function equal to one if state i enacts an income tax law.¹³ Here, $\tau_{i,j}$ is meant to capture the impact of relative tax adoptions on total iceberg costs, where a tax introduction makes the relative costs of living in state i , while making moving to j less expensive. Hence, tax adoptions are both dependent on origin state i and destination state j . The net wage at the destination is defined as $x_j / d_{i,j}$.¹⁴ Next, we define the idiosyncratic utility of migrating from i to j as $v_{h,i,j}$, which has an extreme value type I distribution. Hence, h only moves from i to j if $\exists (x_j / d_{i,j}) v_{h,i,j} \geq x_i$. Expressed as logarithmic utility, we have

¹¹Hence the number of states is $I = J = 48$. Note, that the total number of possible origins or destinations can be arbitrarily high, as long as $I = J$. This imposes a symmetric (or balanced) structure on any possible formulation of this model.

¹²To maintain simplicity of the model, we limit ourselves to these cardinal factors. However, in the empirical method, preferences over destinations depend on no particular set of factors and can include any type of push and pull factor.

¹³Note that the second argument is a placeholder for j -variation. In the identification, this comes from either an interaction with the identifier $1(i = j)$ or from $z_j = 0$.

¹⁴While the personal income tax naturally decreases the migration costs by affecting relative wages, the corporate income tax enters the model through business relocation.

$\tilde{u}_{i,j} = \ln(x_j) - \ln(d_{i,j}) - \ln(x_i)$.¹⁵ Following [Anderson \[2011\]](#), we impose the log of a CRRA utility structure such that $\exp(u_{i,j}) = (x_j/d_{i,j}x_i)^{1-\theta}/(1-\theta)$, where θ captures the rate of relative risk aversion. Note that on an aggregated level, the probability of moving from i to j is given by the share of migrants i to j . The unharmonized tax introductions require adding a discrete time subscript t . We are primarily focusing on the impact of tax adoptions. Therefore, we let the policy variable capture the short-run effects of the introductions, where $\tau_{i,j,t} = 1$ implies $\tau_{i,j,t+1} = 0$.¹⁶ We further define $d_{i,j,t} = c_{i,j}/\exp(\alpha\tau_{i,j,t})$. By denoting $p_{i,t}$ as the population of state i at link t , the migration flow from i to j can then be defined as $y_{i,j,t} = g(u_{i,j,t})p_{i,t}$ with $g(u_{i,j,t}) = \exp(u_{i,j,t})/\sum_k \exp(u_{i,k,t})$, such that this flow reconstructs the CES demand or Ricardian supply shares equivalent to the trade gravity models. With $p_t \equiv \sum_i p_{i,t}$, [Anderson \[2011\]](#) has shown that we can specify the following multilateral resistance terms

$$\omega_{i,t}^{(1-\theta)} \equiv \sum_j d_{i,j,t}^{(1-\theta)} \frac{p_{j,t}^{\gamma_2}}{\psi_{j,t} p_t} \quad \text{and} \quad \psi_{j,t}^{(1-\theta)} \equiv \sum_i d_{i,j,t}^{(1-\theta)} \frac{p_{i,t}^{\gamma_1}}{\omega_{i,t} p_t}, \quad (2.1)$$

where we augment the structural equation with migration elasticities γ_1, γ_2 . Similar to a trade framework, equation 2.1 defines the multilateral resistance. These frictions may be interpreted as the average iceberg costs with respect to all possible destinations. This includes the adoption of the state tax of state i with respect to all other $j \in J \setminus \{i\}$. [Anderson \[2011\]](#) compares these terms to "incidences of migration costs". Empirically, these costs include the inverse of push and pull factors, which are imperative to the establishment of migration flows. This allows then to define a structural gravity equation of predicted migration flow y from i to j (also referred to as pair out-migration) as a function of individuals' utility of migrating:

$$y_{i,j,t} = \left(\frac{d_{i,j,t}}{\omega_{i,t} \psi_{j,t}} \right)^{1-\theta} \frac{p_{i,t}^{\gamma_1} p_{j,t}^{\gamma_2}}{p_t}, \quad (2.2)$$

Note that the structural gravity equation 2.2 captures a partial equilibrium.¹⁷

The stochastic version of equation 2.2 can be seen as a bridge to our empirical approach. First,

¹⁵The standard migration literature then focuses on personal migration choice. [McFadden \[1973\]](#) shows that if $\ln(v)$ has a type-1 extreme value distribution, the probability of choosing destination j is given by a multinomial logit form.

¹⁶This aims to disentangle the negative first-order effects of the tax on personal income and potential second-order effects via an increase in public policy provision, which we assume to have a $t + 1$ lag. Fiscal responses to the income tax adoption during our observation period have shown to be negligible within a ten-year period [[Cassidy, Dincecco and Troiano, 2024](#)].

¹⁷Inward and outward resistance can be solved for by normalizing with respect to one state, and equation 2.1 constitute the conditional general equilibrium.

we include a stochastic error factor $\varepsilon_{i,j,t}$. The stochastic form of equation 2.2 is

$$y_{i,j,t} = d_{i,j,t}^{(1-\theta)} \frac{p_{i,t}^{\gamma_1}}{p_t \left(\omega_{i,t}^{(1-\theta)} \right)} \frac{p_{j,t}^{\gamma_2}}{\psi_{j,t}^{(1-\theta)}} \varepsilon_{i,j,t}, \quad (2.3)$$

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

$$y_{i,j,t} = \exp \left[(\theta - 1)(\alpha \tau_{i,j,t} - \ln c_{i,j}) + \ln \left(\frac{p_{i,t}^{\gamma_1}}{p_t \left(\omega_{i,t}^{(1-\theta)} \right)} \right) + \ln \left(\frac{p_{j,t}^{\gamma_2}}{\psi_{j,t}^{(1-\theta)}} \right) \right] \varepsilon_{i,j,t}. \quad (2.4)$$

We define $\beta := (\theta - 1)\alpha$. With $\alpha > 0$, the introduction of taxation in the origin state i is associated with a decrease in migration costs, essentially through a reduction in relative migration costs, which in turn should increase migration flows.¹⁸ There are two potential drivers that support a positive β . The first driver operates through the direct channel, where individuals express a reluctance to bear the burden of new taxes. This reasoning is an interpretation of Tiebout [1956] and relates to personal income taxation. The second driver operates indirectly through corporate income taxation. Corporations, much like individuals, seek to avoid the new taxation. A consequential relocation of businesses across state borders, and the accompanying labor relocations, present another incentive for increasing interstate migration flows.

For notational simplicity, we set $\delta_{i,j} := (1 - \theta) \ln c_{i,j}$, $\delta_{i,t} := \ln \left(p_{i,t}^{\gamma_1} / \left[p \left(\omega_{i,t}^{(1-\theta)} \right) \right] \right)$ and $\delta_{j,t} := \ln \left(p_{j,t}^{\gamma_2} / \psi_{j,t}^{(1-\theta)} \right)$. When assuming $\mathbb{E}(\varepsilon_{i,j,t} | \tau_{i,j,t}; \cdot) = 1$, we can write equation 2.4 as a moment condition

$$\mathbb{E}(y_{i,j,t} | \cdot) = \lambda_{i,j,t} := \exp \left[\beta \tau_{i,j,t} + \delta_{i,j} + \delta_{i,t} + \delta_{j,t} \right]. \quad (2.5)$$

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

2.4 Empirical Estimation of the Gravity Model

To identify the tax adoption, varying in origin and destination state, we follow Beverelli et al. [2018] by utilizing migration within a state, i.e. non-movers, to formulate a new variable $\tau_{i,j,t} :=$

¹⁸The empirical literature on the average U.S. relative risk aversion, such as Gandelman and Hernández-Murillo [2015], has shown that $\theta \approx 2$.

$(z_{i,t} - z_{i,t-1})m_{i,j}$, where $m_{i,j}$ is an indicator function that captures interstate migration.¹⁹ Note that we are interested in the effects of the tax on migration away from i and towards j , where $\tau_{i,j,t}$ captures the effect of tax adoptions on interstate migration relative to non-movers.²⁰

We adopt the estimation procedure by [Correia, Guimarães and Zylkin \[2020\]](#). [Silva and Tenreiro \[2006\]](#) have shown that the estimation of our parameter of interest β requires a generalized linear model. In order to avoid log-linearization, the prevailing model employed in the literature is the three-way fixed effects Poisson Pseudo-Maximum Likelihood (FE-PPML) approach using iteratively reweighted least squares. There are numerous advantages associated with this technique, above all the ability to estimate the parameters of the stochastic equation 2.5 in its multiplicative form. We do not need to log-linearize the system, including $y_{i,j,t}$. This allows us to disregard potential issues with zero migration observations, as well as biases that stems from heteroskedasticity. This approach delivers consistent estimation of the β . However, in the case of three-way gravity models, this approach introduces an asymptotic bias. To address this issue, we implement an analytical bias correction method proposed by [Weidner and Zylkin \[2021\]](#).²¹ The full set of fixed effects allows us to control for the entire universe of push and pull factors of each state. Specifically, state-specific events that may have triggered both an increase in taxation and out-migration (e.g. local economic crises) are captured by the state-time fixed effects, which supports the causal interpretation of our results. These fixed effects further capture the multilateral resistance terms to migration, captured by $\omega_{i,t}^{(1-\theta)}, \psi_{j,t}^{(1-\theta)}$, which capture average migration costs. Empirically, the interpretation of these costs can be broadened to any time-varying state-specific characteristic. [Bertoli and Moraga \[2013\]](#) show that these resistances are functions of the relative attractiveness of alternative destinations. To control for resistance varying in i, j and t , [Beine and Parsons \[2015\]](#) and [Beine, Bertoli and Fernández-Huertas Moraga \[2016\]](#) suggest to use origin-time and destination-time fixed effects, especially for low-frequency data. Next, in order to interpret the estimated results as a percentage change in migration flows, we require the following transformation

$$\hat{\beta} := \exp([\hat{\beta} - bias]) - 1 = \beta \text{ for } N \rightarrow \infty. \quad (2.6)$$

¹⁹We define $m_{i,j} = 1$ iff $i \neq j$ and 0 otherwise.

²⁰Alternatively, we can directly create a non-linear combination of the adoption in origin state i and destination state j . This second approach will be estimated in the robustness checks in Section 2.6.5.

²¹It has also been shown that cluster-robust standard error estimates are biased, leading to incorrect confidence intervals. In light of this finding, we introduce alternative estimation methods that are more conservative, such as jackknife and bootstrap confidence interval methods. [Pfaffermayr \[2021\]](#) suggests that these methods provide more accurate standard error estimates and ensure the validity of the confidence intervals.

By design, we estimate the effects on migration flows. Theoretically, these flows are directional, namely from origin state i to destination state j . Since a model with multilateral resistances requires us to employ a full set of fixed effects, including origin-time $\delta_{i,t} \equiv \delta_i \delta_t$, destination-time $\delta_{j,t} \equiv \delta_j \delta_t$ and state pair $\delta_{i,j} \equiv \delta_i \delta_j$, we cannot empirically distinguish the direction of the migration flows. In the estimation, the results of the main specification are meant to be interpreted as changes in migration flows. The model then allows to interpret them as changes in outflow. We will test for and discuss directional effects in Section 2.6.2.

2.5 Data

2.5.1 Full Count Data

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

Census Linking

Following [Abramitzky et al. \[2020\]](#), links are generated by multiple matching algorithms of different levels of intensity and enable us to track individuals across different historical census waves. The various linking methods used are carefully described in [Abramitzky et al. \[2021\]](#). In order to maintain a substantial sample size while minimizing the type I errors, we use the NYSIIS standardization matching.²² This algorithm was first introduced by [Taft \[1970\]](#) and its modern iterations have been proven to yield accurate matching results.²³ Note, that since the matching is primarily based on last names, our linked samples exclusively feature male individuals.²⁴ Drawing upon these matching techniques, we have created three interconnected data

²²[Ó Gráda et al. \[2023\]](#) show that there is a potential over-representation of movers, especially among immigrants. We argue that this is of no concern to our empirical strategy.

²³The algorithm is based on phonetic similarities and is broadly used by multiple U.S. government agencies.

²⁴Historically, married females assumed their spouses last name.

sets that provide information on the same individual²⁵ during two consecutive census waves. Since the U.S. census has historically been conducted every ten years, these data sets cover a ten-year period each. Table 2.1 summarizes the scope of each of the linked data sets and offers a brief overview of the demographic characteristics of the sample populations.

Table 2.1 – Linked data decomposition summary

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

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

It is evident that our linked samples have a higher proportion of white individuals than the overall U.S. population.²⁶ We also report labor force participation rates and average income scores, which are particularly relevant as our analysis aims to estimate the impact of personal income tax adoptions on migration. Hence, a significant proportion of (self-) employed individuals in our sample is desirable. We observe marginally higher average income scores and consistent labor force participation rates.

Dyadic Data

To conduct our empirical analysis, we utilize dyadic panel data by extracting individual information from the linked data sets to determine the number of in- and out-migrants for every possible state pair, denoted as i, j .²⁷ Specifically, we count all individuals with $Residency_r = i$ and $Residency_{r+t} = j$ for each census link $t = \{r, r + 10\}$, where $r \in \{1900, 1910, 1920\}$. Our analysis focuses on the contiguous U.S., hence the dyadic dataset contains a total of $3 \times 48 \times 48 = 6,912$ observations.²⁸ Note that our aggregated migration data is perfectly symmetric by design, i.e., the out-migration from state i to j has to be the in-migration of j from i . Hence, the average net migration has to be equal to zero. In Table 2.2 we report summary statistics for the full dyadic

²⁵ Assuming no errors in the linking mechanism.

²⁶ For example, in 1910, the population was 88.9% white, whereas our data indicates a proportion of 92.3%.

²⁷ We also include the aggregate of stayers, i.e., the number of individuals who remained in their respective state.

²⁸ We exclude the 1930s from our analysis, as we expect the Great Depression and its aftermath to overshadow any potential effect of state income tax adoptions on interstate migration. Additionally, after 1929, the predominant reason for tax adoptions became the rapidly worsening economic environment, which was consistent across all states.

data. The analysis of linked dyadic data provides us with valuable insights into interstate migration patterns, which are essential for comprehending overall migration trends when interpreting our findings. We generally confirm the migration patterns described in [Zimran \[2022\]](#).

Table 2.2 – Dyadic data summary, main panel

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

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

Figure 2.1 visually represents the Westward migration pattern, revealing significantly lower levels of out-migration in the westernmost state, California.

Figure 2.1 – State level out-migration share

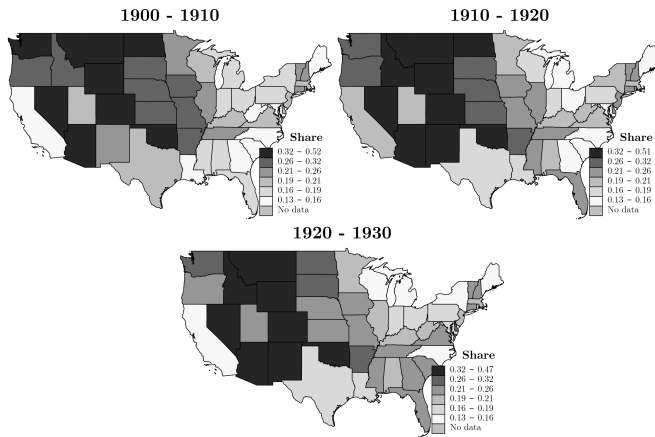
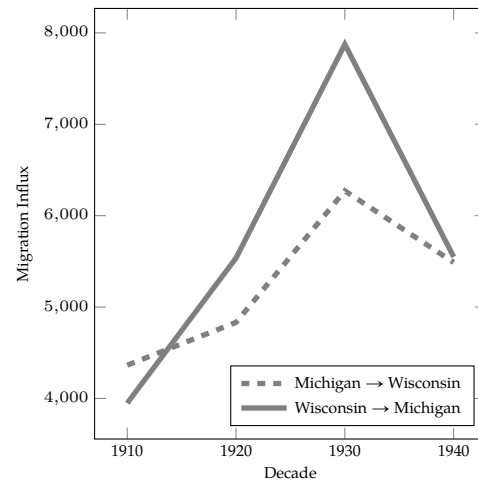


Figure 2.2 – Reciprocal migration influx between Wisconsin and Michigan



Notes: Figure 2.1 depicts the total out-migration share of population for each state. Numbers for Figure 2.2 are computed from the full count linked census data. Figures are not detrended for population growth. Influx exclusively counts realized end-of-decade reciprocal flows of individuals between the two states.

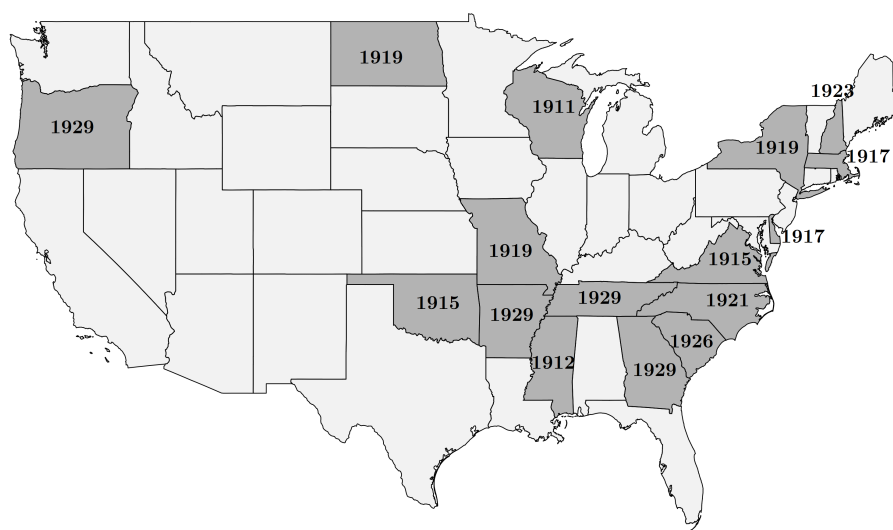
Furthermore, our results offer supportive evidence for [Derenoncourt \[2022\]](#), arguing that the Great Migration to the North did not begin before 1940. To further support our interstate migration hypothesis, Figure 2.2 presents compelling suggestive evidence focusing on the example of Wisconsin and Michigan. We have chosen these states as examples because Wisconsin became the first state to formally adopt state income taxation in 1911, whereas its neighbor and major economic partner, Michigan, with which Wisconsin shares its largest border, only introduced

such state income taxes in the late 1960s. Following the 1910s, our analysis demonstrates a remarkable increase in migration influx from Wisconsin to Michigan. In contrast, the in-migration from Michigan to Wisconsin exhibited a much flatter trend, establishing Wisconsin's positive pair net-flows for the remainder of the observation period.

2.5.2 Income Tax Data

We collect historical state income tax data from [National Industrial Conference Board \[1930\]](#) reports. This document contains a collection of comprehensive reports on state income taxes by the National Industrial Conference Board (NICB)²⁹ between 1916 and 1930. The reports contain detailed information on the taxation of personal and/or corporate income. Figures 2.3 and 2.4 show the respective adoption years of the state income taxes.

Figure 2.3 – Adoption year of personal income tax



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

²⁹ As a response to economic losses and increasing public pressure on industries caused by the First World War, eleven U.S. trade associations formed an organization to foster "harmonious relationships between employer and employees and between both labor and government." The organization provides economic records and analysis of business-relevant political decisions, such as the rollout of state income taxes. The following reports are made available by HathiTrust Digital Library.

our comprehensive dataset allows us to calculate average unemployment rates, occupational income scores, and debt per capita for both adoption and non-adoption states and hence enables us to draw descriptive comparisons.³⁰ The summarized results, presented in Table 2.4, reveal no statistically significant differences between these two groups of states. This suggests there is no economic distinction between states that have adopted income taxes and those that have not.

Table 2.4 – Statistical comparison of adoption and non-adoption states

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

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

Another related identifying assumption is whether we find parallel trends across adopters and non-adopters. This assumption essentially requires the absence of statistically significant differences in the pre-trend of the dependent variable across both treatment and control groups. We conduct separate tests for both treatment groups to ascertain this. Specifically, we examine the pre-trends for states that adopted the personal income tax in 1910 compared to those that were never treated, as well as those adopting in 1920. For states adopting in 1920, we compare pre-trends in the 1900s to those never treated, as well as to states adopting in the 1910s. Additionally, we assess states adopting in the 1920s exclusively against those never treated [[Callaway and Sant’Anna, 2021](#)]. These evaluations are reported in Table 2.4 and conducted while controlling for state-level population, where we find no statistically significant differences in the pre-trend pair-wise migration flows.

To further complement this exogeneity discussion we estimate two reduced-form regressions.

³⁰We argue that these metrics are representative proxies for the economic condition of the respective state.

Table 2.5 – Reduced form regressions to test exogeneity

	$z_{i,t}$	Out-migration: $\ln(\sum_{j=1}^{J \setminus \{i\}} y_{i,j,t})$
Out-migration: $\ln(\sum_{j=1}^{J \setminus \{i\}} y_{i,j,t-1})$	0.1199 (0.1475)	
$\ln(\text{Revenue}_{i,t})$	0.1861 (0.1421)	−0.0409 (0.0476)
Origin Population: $\ln(\sum_{i=1}^J y_{i,j,t})$	−0.8850 (0.3491)**	1.3629 (0.0864)***

State FE, Census Year FE	Yes	Yes
Observations	144	192
Adjusted R^2	0.4371	0.9864

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

The first regresses the tax adoption $z_{i,t}$ on state-level (lagged) out-migration to directly test simultaneity. A second regression analyzes the explanatory power of state-level revenue with respect to out-migration. Origin i population is added as an additional time-varying control. Results are reported in Table 2.5. We find that neither lagged out-migration nor revenues have a significant effect on the income tax adoption. This indicates no evidence for potential simultaneity. Furthermore, revenues seem to be an inadequate indicator not only for tax adoptions but also migration. These findings are in line with historical records, such as Stark [1987], noting that the early adoptions were independent of additional revenue creation.

2.6 Baseline Results

To further motivate the utilization of a gravity model, we initially estimate a standard gravity model of migration with distance (instead of state-pair fixed effects). We find that our model fits the data-generating process of bilateral migration flows very well. The remainder of this paper will use state-pair fixed effect, which controls for distance and other time-invariant confounders. For the baseline, we estimate the effects of tax adoptions for the full sample. The corresponding equations 2.5 and 2.6 define $\hat{\beta}$ as the percentage change in migration flows caused by the adoption of state income taxes in state i . Table 2.6 shows the untransformed results for Panel A: *Personal Income Tax* adoptions and Panel B: *Corporate Income Tax* adoptions. Once we account for the asymptotic bias by using the correction suggested by Weidner and Zylkin [2021] and transform the coefficient, we conclude that interstate migration flows increase by 8.70% due to the

introduction of state income taxation.³¹ The magnitude of this finding is comparable in size to Cassidy, Dincecco and Troiano [2024], who identify an increase of 11.3% of interstate migration following tax adoptions between 1900 and 2010 for a subsample of young families. Similarly, Martínez [2022] finds an inflow elasticity of 7.2% following a significant income tax cut in one Swiss canton in 2006.³² These results remain statistically significant, even after applying the most conservative estimation for standard errors (i.e. the Jackknife resampling). We also observe an 11.28% increase in migration flows following the introduction of corporate income tax. We estimate the effects of personal and corporate taxation separately in two distinct specifications. In Section 2.6.2, this allows us to conditionally disentangle the effects of these two taxes.³³

Table 2.6 – Effects of income tax adoptions on interstate migration flows (equation 2.5)

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

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

The scale of these outcomes does not come as a surprise. Previous research by Molloy, Smith and Wozniak [2011] has firmly established that the United States has historically experienced significant rates of internal migration. On average, between 1980 and 2000, around one-third of adults have relocated across states within the last five years. Their study also identifies shifts in state-to-state price differentials as a key driving force behind interstate migration. Moreover, we delve into the effects of tax changes on the extensive margin, which typically surpasses effects on the intensive margin. It is plausible to assume that if alterations in variations of cross-state tax

³¹In absolute terms, this amounts to an average increase of pair migration flows by $1,162.44 \times 0.087 \approx 97.01$.

³²Note that a direct comparison of our estimates to elasticities is to be interpreted with caution, as the baseline focuses merely on adoption effects.

³³Combining both taxes into one more particularized variable would require accounting for various scenarios, such as cases where personal taxes preceded corporate taxes, vice versa, or instances where only one of them was implemented. Nevertheless, due to overlaps between personal and corporate taxation, it is challenging to perfectly distinguish the effects of personal income tax from those of corporate income tax introductions.

rates induce internal migration, the introduction of new tax rates should have a similar effect, potentially even stronger. Aligned with this reasoning, Akcigit et al. [2022] ascertain a mobility elasticity of 1.23 for inventors in the post-1940 period. Once again, our findings reveal a significantly greater increase in internal migration, which aligns with the notion that the introduction of new tax rates serves as a more substantial shock.

While flow elasticities are insightful by themselves, the interstate migration choice literature, such as Moretti and Wilson [2017] argues the policy-relevance of stock elasticities as they ultimately present the change in the total tax base. In our case, results can be easily converted such that

$$\hat{\eta} = \hat{\beta} \left[\frac{\sum_i \sum_j \sum_t y_{i,j,t}}{\sum_i \sum_t p_{i,t}} \right] = \hat{\beta} \times \text{Average Out-Migration Share} \quad (2.7)$$

defines the stock elasticity. With our baseline estimates, this translates to an average 1.78% decrease in adopter states' population stock.³⁴

2.6.1 Heterogeneity

The advantage of granular data is that it allows us to count movers and stayers of different demographic and geographic groups. As we know from Borjas [1987], selection into migration varies across demographics. We also assume the effectiveness of the tax adoptions on internal migration to vary significantly among those characteristics. Table 2.7 summarizes the various groups we analyze. Given the substantial level of labor mobility, particularly among young adults, it is reasonable to assume that younger individuals with sufficiently high income would bear a disproportionate impact. A similar presumption can be extended to the overall labor force population. We anticipate that both these groups will not only respond to individual income taxation but also to corporate income taxation through potential firm relocation.

Research conducted by Liu [2014] demonstrates that the implementation of state-level corporate income taxes led to a reduction in the corporate share of economic activities within those states, indicating significant business relocation. Consequently, there is a possibility of workers relocating as well. Different assumptions apply to less mobile groups, such as homeowners and farmers, who are likely to be less inclined to move across state borders. Similarly, licensed professionals like lawyers and physicians, whose licenses are often state-specific, are also expected

³⁴Note that a key assumption of this exercise is that the baseline results capture changes in bilateral out-migration rather than in-migration.

Table 2.7 – Demographic and geographic groups S and their respective mean dependent variable

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

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

to exhibit limited mobility due to the dependence on local reputation.³⁵ As a result, we anticipate this group to show a higher degree of reluctance on average towards interstate migration. In line with these conjectures are the summary statistics, presented in Table 2.7. We find that young individuals and immigrants exhibit a notably high relative rate of out-migration. In contrast, licensed professionals, farmers, and homeowners observe a comparatively lower rate of out-migration.

Estimating the migrational effects on these various groups poses a central challenge, as it requires comparing different sample estimates. To make statistical inference across these groups possible, we need to stack the subsets and introduce group identifiers $\mathbb{1}_s$, where $s \in S$ defines the respective group in Table 2.7.³⁶ Further, we introduce group fixed effects δ_s . In order to then estimate the effects for each subgroup in one regression, we define

$$y_{i,j,t,s} = \exp \left[\sum_s \beta_s (\mathbb{1}_s \times \tau_{i,j,t}) + \delta_{i,j,s} + \delta_{i,t,s} + \delta_{j,t,s} \right] \xi_{i,j,t,s}. \quad (2.8)$$

Note that we include the full sample as one specific group s .³⁷

³⁵Note that around 94% of the *Licensed Professions* subsample are attorneys and physicians.

³⁶A recent application of this procedure can be found in Cengiz et al. [2019].

³⁷Hence we have 13 groups, each with 6,912 observations. The total number of observations therefore increases to 89,856. We do not include the "No West" subgroup in S as this coefficient is simply estimated to ensure that our results are not driven by the Westward migration. We abstract from interpreting the specific effect.

2.6.2 Results across Groups

To gain further insights into the demographics, we present the results for equation 2.8 in Figures 2.5 and 2.6. A characteristic decomposition of migrational responses to public policies is often motivated by [Borjas \[1987\]](#), arguing a certain level of self-selection into migration, especially along the educational and income dimension.

Figure 2.5 – Panel A: Personal Income Tax

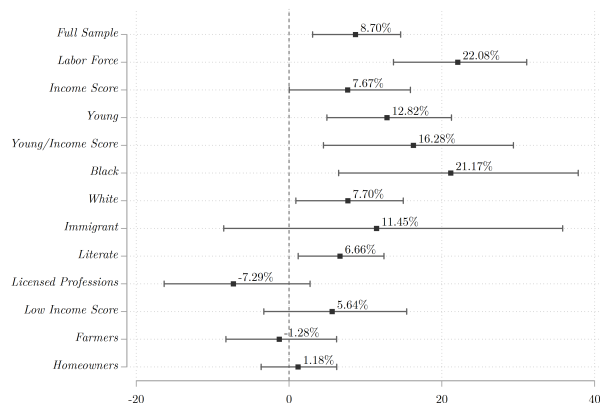
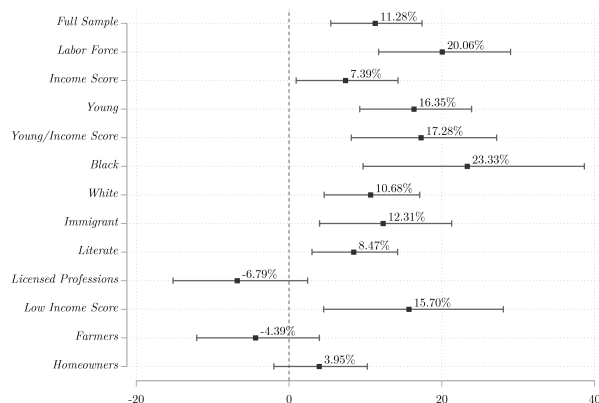


Figure 2.6 – Panel B: Corporate Income Tax



Notes: Confidence intervals and represent the 95th percentile. The point estimates are transformed using equation 2.6. Results are bias corrected using [Weidner and Zylkin \[2021\]](#). Table A.2.1 reports full results.

For Panel A (Figure 2.5), as anticipated, the largest point estimate is registered by the group of adults in the labor force. This group is effectively targeted by the new policy. We find a 22.08% increase in migration flows for this group. Note, that this point estimate is statistically different from the point estimate of the full sample. Our findings further reveal positive impacts on young adults with a yearly income of \$1,000 or above, who are expected to experience significant effects from the adoption of an income tax. For this group, we find an increase of 16.28% associated with the introduction of personal income taxes. For our "control groups", including top earners, farmers, and homeowners, we do not identify any significant effects on migration flows. These results further confirm the existence of a direct channel of interstate tax avoidance. That is, individuals react to the introduced taxation with migration. These results can be interpreted as historical evidence Tiebout's hypothesis, preceding his work in the 1950s.

We aim to identify another influential factor: the personal migration stemming from corporate relocation. Though directly estimating the impact of corporate tax adoptions on firm moves is challenging, we can utilize individual migration as an indirect indicator. [Liu \[2014\]](#) supports our rationale by illustrating reduced corporate activities following corporate income tax adoptions, hinting at businesses moving across borders to avoid taxation. In our previous discussion (Section 2.2), we highlighted a newspaper excerpt from Wisconsin, one year after the income

tax adoption, revealing that numerous firms had shifted operations to neighboring states. To illustrate this firm relocation, we intend to gauge the effects of corporate tax introduction on individual migration.

In order to partially disentangle the effects of personal and corporate income tax, we now concentrate on estimating effects within a subgroup of individuals with income below the average exemption of approximately \$1,000. This group covers around 8% of the total sample population. While we do not discern notable effects for this subgroup in Panel A regarding personal income tax adoptions, but we do observe substantial and statistically significant effects in Panel B, reflecting corporate tax adoptions.³⁸ Specifically, in Figure 2.6, we report a 15.70% surge in internal migration flows among individuals unaffected by personal state income taxation. This robustly suggests the presence of a secondary mechanism through which state income tax adoptions impact interstate migration. Note that every other group is comparable with the estimates of Figure 2.5.

Is the increase in flows driven by out-migration?

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

$$\log \left(\frac{\pi_{h,i,j}}{1 - \pi_{h,i,j}} \right) := \text{logit}(\pi_{h,i,j}) = \zeta_1 z_i + \zeta_2 z_j + \phi V_h + \rho_1 W_i + \rho_2 W_j + \nu_{h,i,j}, \quad (2.9)$$

where $\pi_{h,i,j}$ captures the probability that for individual h we have $i \neq j$, i.e. that the individual moved across the state border. The tax implementation in both the origin and destination states is represented by variables z_i and z_j respectively. If we validate our understanding of the gravity model outcomes, wherein alterations in migration patterns are instigated by out-migration trends, then it logically follows that $\zeta_1 > 0 > \zeta_2$. This scenario implies that the introduction

³⁸The positive and insignificant effect for Panel A further suggests that unaffected individuals may respond due to anticipated wage changes in the future. Note however that effects are contaminated partially by states adopting both personal and corporate income tax.

Table 2.8 – Effects of income tax adoptions on out-migration probability (equation 2.9)

	1910s				1920s			
	Personal Tax		Corporate Tax [†]		Personal Tax		Corporate Tax [†]	
	(i.)	(ii.)	(i.)	(ii.)	(i.)	(ii.)	(i.)	(ii.)
Origin z_i	0.314*** (0.003)	0.200*** (0.003)	0.183*** (0.009)	0.351*** (0.010)	0.910*** (0.004)	0.073*** (0.004)	0.486*** (0.009)***	0.027** (0.011)
Dest. z_j	-0.359*** (0.003)	-0.051*** (0.003)	-0.035*** (0.009)	-0.170*** (0.010)	-1.007*** (0.004)	-0.138*** (0.004)	-0.489*** (0.009)	-0.142*** (0.011)
Indiv. Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	12,811,480	12,811,480	1,151,534	1,151,534	15,838,897	15,838,897	972,245	972,245

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

of a tax in the origin state increases the likelihood of out-migration, whereas its introduction in the destination state diminishes said probability. This further aligns with [Cassidy, Dincecco and Troiano \[2024\]](#), finding no significant impact of the early adoptions on state revenues or spending, which could otherwise contribute to additional pull factors.

In order to mitigate endogeneity we control for individual characteristics V_h as well as state characteristics W_i, W_j .³⁹ Similar to [Boustan, Kahn and Rhode \[2012\]](#), we estimate a series of logits for the 1910s and 1920s separately. Table 2.8 presents the results of this analysis for personal and corporate income tax adoptions.

For the entire series of logits we find $\hat{\zeta}_1 > 0 > \hat{\zeta}_2$. Note that, similar to the gravity estimation above, we partially disentangle the effects of personal and corporate income tax adoptions by estimating the model for corporate tax introductions for a subset of individuals that are not directly affected by the personal income tax. In Table 2.7, we denoted this group as *Low Income Score*. The idea is to infer information on the secondary channel through which income tax adoptions affect individual migration, namely firm relocation. Here, again, $\zeta_1 > 0 > \zeta_2$ proposes that any effect on migration flows is driven by a change in out-migration. These results suggest that estimates in Sections 2.6 and 2.6.2 are indeed driven out-migration.

³⁹Individual controls include age, race, marital status, family size, labor force participation, occupational income score, educational attainment, and homeownership. All variables are measured in the base year. State controls include the logarithm of population, unemployment rate, average occupational income score as well as the latitude and longitude of the respective origin and destination state.

2.6.3 Border States

Another interesting exploration is to look at effects relative to geographic, and hence cultural and political, closeness. Specifically, we are interested in the baseline effects for bordering versus non-bordering states. A priori, it is not clear whether the baseline results obtained so far, are driven by migration into states of close proximity or a broader finding. Additionally, as the average migration flows for bordering states are presumably much larger, we can juxtapose relative and absolute effects. We would expect the effects on migration flows, at least in absolute terms, to be statistically larger for bordering states. For that, let us consider a modified version of equation 2.5

$$y_{i,j,t} = \exp \left[\beta_1 \tau_{i,j,t} + \beta_2 (\tau_{i,j,t} \times b_{i,j}) + \delta_{i,t} + \delta_{j,t} + \delta_{i,j} \right] \varsigma_{i,j,t}, \quad (2.10)$$

where the indicator function $b_{i,j} = 1$ if origin i and destination j are bordering states.⁴⁰ The separate term for $b_{i,t}$ is dropped due to multicollinearity with the pair fixed effect. For the comparison of absolute changes in the migration flows we next need to compute the mean dependent variables $\bar{y}_{i,j,t}^m$ for both values of $b_{i,j}$.⁴¹

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

Of course, as for each state i there are more non-bordering states than bordering states, we report that the number of s for $b_{i,j} = 0$ is much larger. However, as to be expected, the mean dependent variable is much larger for $b_{i,j} = 1$. Individuals migrate predominantly to states relatively close to their origin i . With these insights, we can now estimate the model of equation 2.10.

We observe a slightly reduced relative effect for state pairs sharing a border, with $\hat{\beta}_1 + \hat{\beta}_2$ being statistically significance. Notably, the marginal effect for $b_{i,j} = 1$ is 7.21%, indicating a magnitude smaller than the overall effect. While this seems contradictory to the idea that migration costs increase in distance, it highlights the complexity of migration responses. It is crucial to account for variations in the base migration, i.e. the mean dependent variable in this context. Specifically,

⁴⁰Note that we define $b_{i,i} = 1$.

⁴¹We compute the mean dependent variable for a subset of N where $i \neq j$, i.e. $m_{i,j} = 1$ we define as N^m . There are 48 states for 3 time periods, hence $N^m = 6,912 - 144 = 6,768$. For notational simplicity, can then define $\bar{y}_{i,j,t}^m = 1/N^m \sum_i y_{i,j,t} \times m_{i,j}$.

Table 2.10 – Estimates of tax adoption effects for the border dummy specification

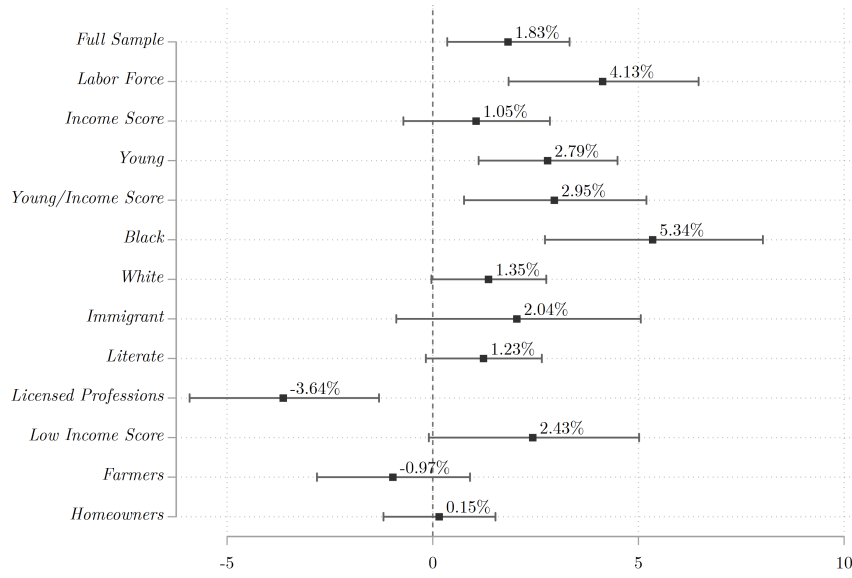
	<i>Panel A: Personal Income Tax</i>	<i>Panel B: Corporate Income Tax</i>
Tax Introduction $\tau_{i,j,t}$	0.0916 (0.0187)***	0.1390 (0.0196)***
Interaction $\tau_{i,j,t} \times b_{ij}$	-0.0220 (0.0222)	-0.0594 (0.0226)***
$\hat{\beta}_1 + \hat{\beta}_2$	0.0696 (0.0185)***	0.0797 (0.0176)***
Observations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

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

the mean absolute change in migration flows for bordering states resulting from the introduction of personal income taxation is $5,527.88 \times 0.0721 \approx 398,29$. Conversely, the mean absolute change in migration flows for non-bordering states stands at $721.42 \times 0.0959 \approx 69.20$. Unsurprisingly, we find a much larger estimated average increase in migration flows among bordering states. A similar conclusion can be drawn for the adoption of corporate taxation. Here, the mean absolute changes in migration flows for bordering states is 458.44 and 107.61 for non-bordering states. We conclude that the effect in absolute terms was significantly larger in bordering states. Note that, however, an investigation of the effects of these differences in tax rates is not equivalent to effects on the intensive margin per se. We rather analyze the effects of the "intensity" of the introduction. In order to proceed, we first have to restate our model. Recall that the variable of interest $\tau_{i,j,t} = z_{i,t}m_{i,j}$ captures the binary nature of the introduction, without considering the intensity of the treatment. We model the tax adoption as a reduction in migration costs $d_{i,j,t} = c_{i,j}/\exp(\alpha\tau_{i,j,t})$, where our empirical results from Section 2.6 established that $\alpha > 0$. Now, let us define $d_{i,j,t}^r = c_{i,j}/\exp(\alpha^r\tau_{i,j,t}^r)$, where $\tau_{i,j,t}^r = (z_{i,t}m_{i,j})r_i$.

2.6.4 Adopted Tax Rates

Another essential aspect of the tax rollouts was the adopted rates. States differed with respect to the design implemented to tax their constituents. Some states, such as Mississippi introduced a flat income tax rate, while others, such as Wisconsin, adopted a progressive tax regime. The maximum income tax rate in the various states is presented in Figure 2.7, with an average of 3.9%. We can see that there is a large heterogeneity across states. The maximum adopted tax rate differential is 9.5 percentage points.

Figure 2.8 – Maximum personal income tax rate at the time of the adoption

Notes: Confidence intervals and represent the 95th percentile. The point estimates are transformed using equation 2.6. Results are bias corrected using [Weidner and Zylkin \[2021\]](#). Table A.2.2 reports full results.

2.6.5 Additional Mechanistic Insights and Robustness

To gain further insights into the underlying mechanism as well as strengthen the robustness of our results, we explore various alternative specifications. Robustness checks will be juxtaposed against our primary model outlined in equation 2.2.

Years since adoption As the effects measured by the baseline are supposed to be interpreted as decennial changes in migration flows, it is worth exploring the development of these effects within the first ten years of the adoption. Specifically, the persistence of the introduction shock. Recall that migration flows are measured at the end of a respective decade. This allows to use the year of adoption relative to the end of the respective decade of adoption to estimate dynamic effects of the tax adoption.

To do so, we define a variable

$$l_{i,t} = \begin{cases} r + 10 - \text{Year of Introduction}_i & \text{if state } i \text{ adopts in decade } t \\ 0 & \text{otherwise}^{43} \end{cases} \quad (2.12)$$

Since $t = \{r, r + 10\}$, with $r \in \{1900, 1910, 1920\}$, we can define $\tau_{i,j,t}^{ysa} = \tau_{i,j,t} l_{i,t}$ as a measure of treatment effects weighted by the years since the adoption. This allows to estimate the persis-

tence of the tax effects on a yearly basis, despite our data being decennial. In order to investigate the potential non-linearity in the marginal effects, we add a polynomial term to our specification such that

$$y_{i,j,t} = \exp \left[\vartheta_1 \tau_{i,j,t}^{ysa} + \vartheta_2 (\tau_{i,j,t}^{ysa})^2 + \delta_{i,t} + \delta_{j,t} + \delta_{i,j} \right] \varrho_{i,j,t}. \quad (2.13)$$

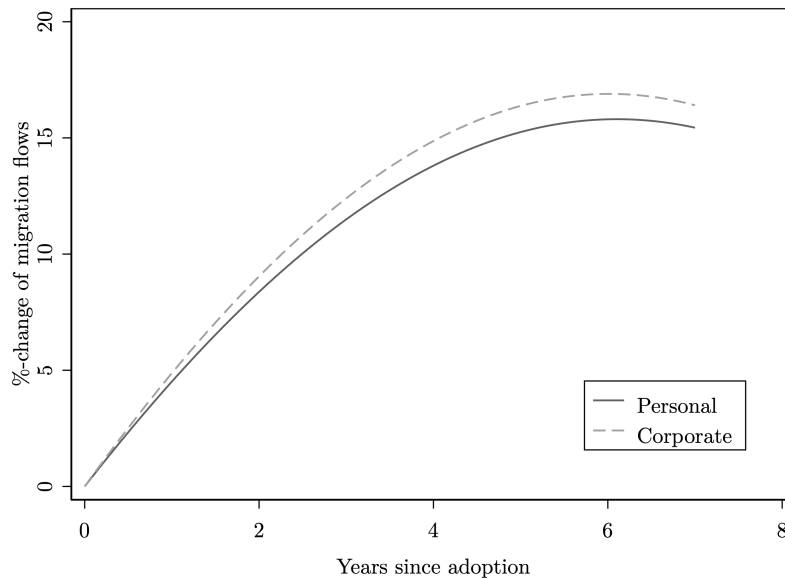
Table 2.11 – Estimates of tax adoption effects with respect to years since adoption

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

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

First, we find statistically significant non-linear effects of the tax adoptions. The policy initially had an increasing positive effect on interstate migration in the years immediately following its adoption. However, this positive impact diminishes over time, with the marginal effects decreasing as more years pass since the tax's introduction.

Figure 2.9 – Non-linear adoption effects with respect to years since adoption



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

This Mincer-like regression equation allows us to capture a curvature of the tax effects with respect to an additional year since the adoption. Here, $\tilde{\beta}_1$ can be interpreted as the percentage change in bilateral migration flows due to the tax being introduced one year earlier. We report the estimation results in Table 2.11. To graphically illustrate the concavity in the persistence of the adoption effects, we plot the effects with respect to years since the introduction in Figure 2.9. To plot these lines, we use estimates from equation 2.13. We find that the increasing positive marginal effects on migration flows last for around 6 years before they start to diminish. This exercise provides evidence that the adoption effects estimated by the baseline strategy are averages of a non-linear process over time. In line with the interpretation of the main results, the initial years after the adoption seem to be the driving force behind these average effects.

County-level analysis Another advantage of the census data is that we know not only the state of residence of every individual but also the county. This allows to augment the analysis with additional heterogeneity as we can now include within-state variation in location. Here, the most important information is the distance to the border. Section 2.6.3 has established that the absolute increase in migration flows due to the tax adoptions is larger between border states. Now we can further analyze if proximity to the border plays a role. To investigate this, we define $w_1(i)$ the origin county in state i and $w_2(j)$ the destination county in state j . Note that if $w_1 = w_2$, we have $i = j$. The county-level analysis allows us to differentiate between effects right at the border versus towards the middle of a respective adopter state. To do so, we rely on a generalized distance-to-border measure provided by Holmes [1995]. This allows us to find the shortest distance from every county center to the closest state border and denote it as $D_{w_1(i)}$.⁴⁴ We use the gravity framework to estimate the effects of the tax adoption interacted with the distance measure to assess whether the baseline results, as well as the border state results from Section 2.6.3, are driven by migration at the border. The estimation equation can be stated as

$$y_{w_1(i),w_2(j),t} = \exp \left[\varphi_1 \tau_{i,j,t} + \varphi_2 D_{w_1(i)} + \varphi_3 (\tau_{i,j,t} \times D_{w_1(i)}) + \delta_{i,t} + \delta_{j,t} + \delta_{i,j} \right] \mathfrak{I}_{i,j,t}. \quad (2.14)$$

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

⁴⁴Note that the distance is measured in miles.

direct impact of proximity to the border, or lack thereof, instead of a continuous interaction. Table 2.12 presents the results for both the continuous measure and the dummy approach.

Table 2.12 – County-level estimates of tax adoption effects with respect distance to border

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

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

First, we note that, as presumed, the bilateral migration effect of the tax adoption is clearly and significantly decreasing with the distance to the border. The fixed point is estimated to be at around 34.03 miles away from the border. Figure A.2.5 plots the negative distance measure, where we observe that county borders are coded to have a distance measure below the estimated fixed point. This lends support to the hypothesis that the main results are driven by individuals who live closer to the border. The effects seem to be even stronger geographically concentrated for the adoption of the corporate income tax. This insight is further facilitated by the findings of the indicator model. Here, the assignment to the group of counties that lie outside the fixed point distance, has a negative effect on migration flows. To summarize the results of this exercise, it seems that the baseline results are driven by out-migration of individuals living close to a state border.

Destination state tax This exercise involves estimating the migratory effects of a tax adoption indicator that takes into consideration whether the destination state has implemented the income tax. We assume that individuals aiming to evade income taxation within their own state would be less likely to relocate to another state that also enforces income taxation. Hence, we

define a new $\tau_{i,j,t}^* = 1$ iff $z_{i,t} > z_{j,t}$.⁴⁵ The purpose of this alternative investigation is to ascertain whether the outcomes presented in Table 2.6 primarily stem from individuals relocating to states that do not impose state income taxes. This serves as a supplementary measure to validate our hypothesis concerning tax avoidance. Correspondingly, the findings detailed in Table A.2.3 (column (i) and (v)) closely mirror our principal results. As a consequence, we deduce that the observed estimations are indeed influenced by significant migrations into states without state income taxation. With this newly constructed indicator, we can, similar to [Moretti and Wilson \[2017\]](#), further estimate the effects excluding the observations where $m_{i,j} = 0$, i.e. the non-movers. Ideally, our results should not be solely driven by the inclusion of these observations as they primarily serve as a support for the identification of $\tau_{i,j,t}$. However, note that the exclusion of these observations will inevitably lead to different point estimates as we drop important information from the control group. And indeed, Table A.2.3 reports smaller point estimates in columns (ii) and (vi). Qualitatively, our findings are robust to the exclusion of these observations.

Influence of westward expansion To account for the effects of the overall Westward migration, we repeat the regression analysis reported in Table 2.6 without the most Western states.⁴⁶ If the Westward migration, which was still prevalent in the early 20th century, had an effect on our results, we would expect this subset to find lower overall effects of the tax introductions.⁴⁷ We present our findings in columns (iii) and (vii) of Table A.2.3 and we observe convincing similarities between these results and our baseline estimates. It is crucial to emphasize, however, that since this pertains to a different sample, a quantitative juxtaposition of the coefficients with those in Table 2.6 is inappropriate. Hence, we find no evidence of the aforementioned bias. Hence, we conclude that the Westward migration did not alter our estimation results.

Do single states drive results? Another robustness exercise seeks to discern if the outcomes of our analysis in 2.6 are driven by specific individual states. To that end, we will replicate our main analysis, each time excluding a distinct state that implemented an income tax within the period of 1910 to 1930. We report the results in Figure A.2.3 and Figure A.2.4. We note that, qualitatively, dropping single states does not change our main results in any way.

⁴⁵Note that including the case where $z_{i,t} < z_{j,t}$ does not change the estimation outcome, as again the in- and out-migration across our sample is perfectly symmetric.

⁴⁶We define the most Western states to be Washington, Oregon, California, Idaho, Nevada, Utah, and Arizona.

⁴⁷Note that the reason for not including a bilateral Western state indicator as a control is that such geographic variables are constant over time and hence should be absorbed by the fixed effects.

End-of-decade adopters Some states implement their income taxes at the very end of a respective decade, allowing us to investigate potential anticipation effects. If anticipation significantly influences our estimates, reclassifying end-of-decade adopters into the next decade should decrease the point estimates relative to the baseline.⁴⁸ The results, presented in columns (iv) and (viii) of Table A.2.3, show that the point estimates are slightly larger compared to the baseline, suggesting that there are no significant anticipation effects regarding the tax adoptions.

Condensed tax measure Given the significant overlap in states adopting personal and corporate income taxes (see Figures 2.3 and 2.4), [Cassidy, Dincecco and Troiano \[2024\]](#) chose to condense these policies into a single introduction dummy. Unlike this condensed measure, our main specification attempts to partially disentangle the two introductions. Nonetheless, it is a good robustness exercise to estimate the effects of the condensed policy variable. We find an 11.66% increase due to the early adopted income taxes. These results are in line with our baseline estimation as well as figures for post-WWII introductions, estimated by [Cassidy, Dincecco and Troiano \[2024\]](#).

2.7 Conclusion

Tax competition is an important side effect of decentralized fiscal policy. As we have shown, this phenomenon dates back to the introduction of personal and corporate state income taxes in the U.S. in the early 20th century. Using a migration gravity model with multilateral resistance and applying it to linked U.S. census waves between 1900-1930, we causally identify the impact of state tax introductions on migration flows. We find a substantial 8.7% increase in interstate migration flows due to the introduction of personal income taxation. Additionally, using a logit model we show that our estimates are driven by out-migration, and we find larger effects among young individuals with above-median income. Our refined analysis, incorporating adopted tax rates, provides insights into the intensive margin of these effects, showing that a one percentage point increase in the introduced maximum tax rate corresponds to a 1.83% rise in interstate migration. These results extend to a secondary channel through which income taxation influences interstate migration — specifically, the adoption of corporate income taxes raises migration flows by 11.3%. This suggests that corporate taxation’s impact extends beyond financial considerations, potentially driving away industries and thereby affecting the move-

⁴⁸Besides testing for anticipation effects, this alternative definition of introduction also allows us to further check robustness.

ment of workers. Bordering states experienced a particularly pronounced impact, especially in absolute terms. Robustness checks show that our results are not driven by individual states or the broader Westward migration trend, which was notable during that period. We further show that our estimation technique is robust to the usage of alternative specifications. Finally, we confirm that estimated migration effects are indeed driven by migration into states that do not adopt an income tax. Altogether, we find significant effects of foot voting in the early 20th century United States, lending credible support to the original Tiebout Hypothesis [[Tiebout, 1956](#)]. While the transfer of these historical results to the current setting should proceed with caution, our results hold lessons for contemporary policymakers. For example, the debate over differential state income tax policies in the USA continues.⁴⁹ Likewise, modern-day Europe reveals many similarities to our historical setting, including the free movement of people. Our analysis thus helps to advance the understanding of how a fiscally federal structure affects the European Union. Contemporary policymakers should consider that the relocation effects of fiscal policy differentials might be sizeable.

⁴⁹From 2017 to 2018, the U.S. Census Bureau registered a doubling in net migration flows from California to Texas (more than 50,000), which coincides with a substantial state tax differential.

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CHAPTER 2:
APPENDIX

A.2 Tables and Figures

Table A.2.1 – Estimates of tax adoption effects across groups (stacked regressions)

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

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

Table A.2.2 – Estimates of adoption maximum tax rate effects across groups (stacked regressions)

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

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

Table A.2.3 – Robustness: Effects of income tax adoptions on interstate migration flows

	<i>Panel A: Personal Income Tax</i>				<i>Panel B: Corporate Income Tax</i>			
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Tax Introduction $\tau_{i,j,t}^*$			0.1045 (0.0152)***	0.1039 (0.0173)***			0.1084 (0.0152)***	0.1231 (0.0170)***
Tax Introduction $\tau_{i,j,t}^*$	0.0849 (0.0178)***	0.0534 (0.0288)*			0.1312 (0.0186)***	0.0683 (0.0288)**		
Bias	−0.0039	0.0066	−0.0059	−0.0088	−0.0008	0.0061	−0.0036	−0.0083
Bias Corr. St. Error (Percent.) Bootstrap Jackknife Resampling	(0.0237)*** (0.0337)** (0.0481)*	(0.0319)	(0.0187)***	(0.0222)***	(0.0255)*** (0.0407)*** (0.0528)**	(0.0316)**	(0.0195)***	(0.0217)***
Observations	6, 912	6, 768	5, 043	6, 912	6, 912	6, 768	5, 043	6, 912
Pseudo R^2	0.9988	0.9898	0.9990	0.9988	0.9988	0.9898	0.9990	0.9988

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

Figure A.2.1 – Government partisan affiliation in charge of personal income tax

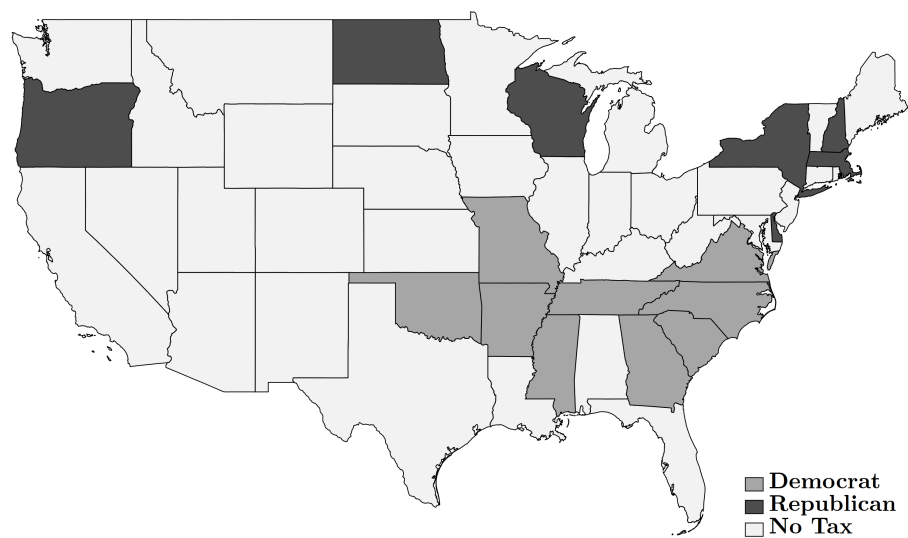


Figure A.2.2 – Government partisan affiliation in charge of corporate income tax

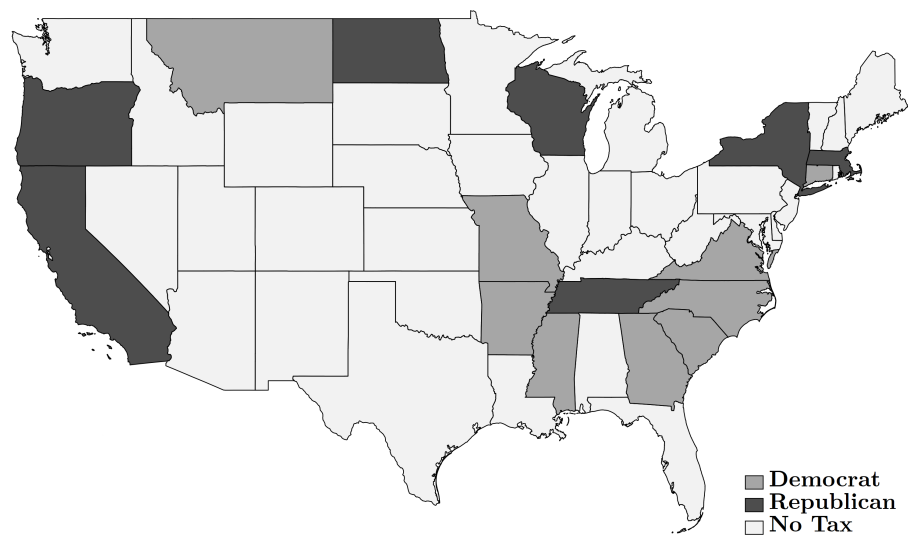


Figure A.2.3 – Panel A: Personal Income Tax

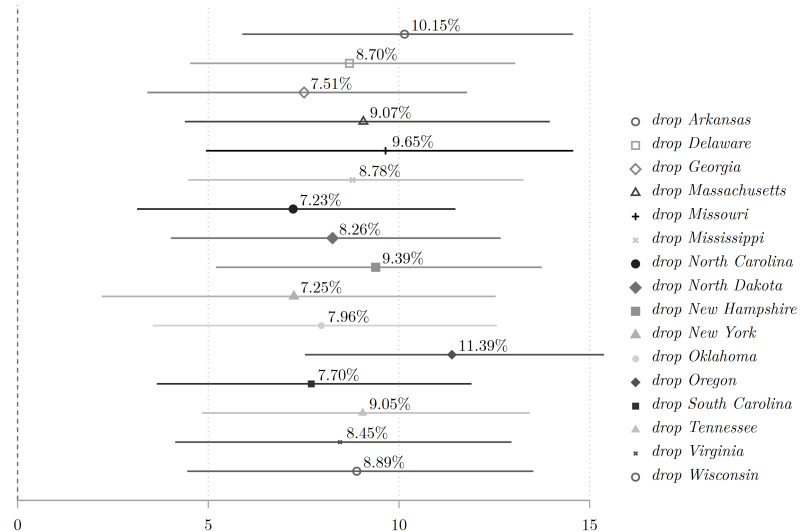
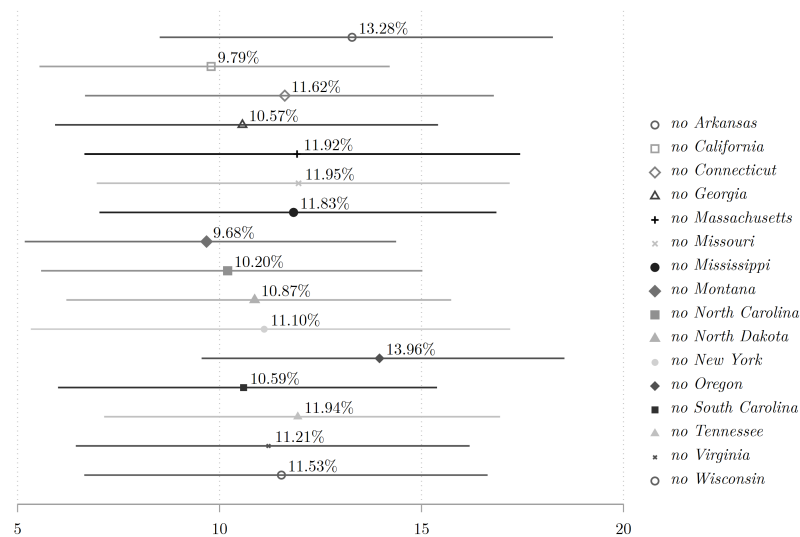
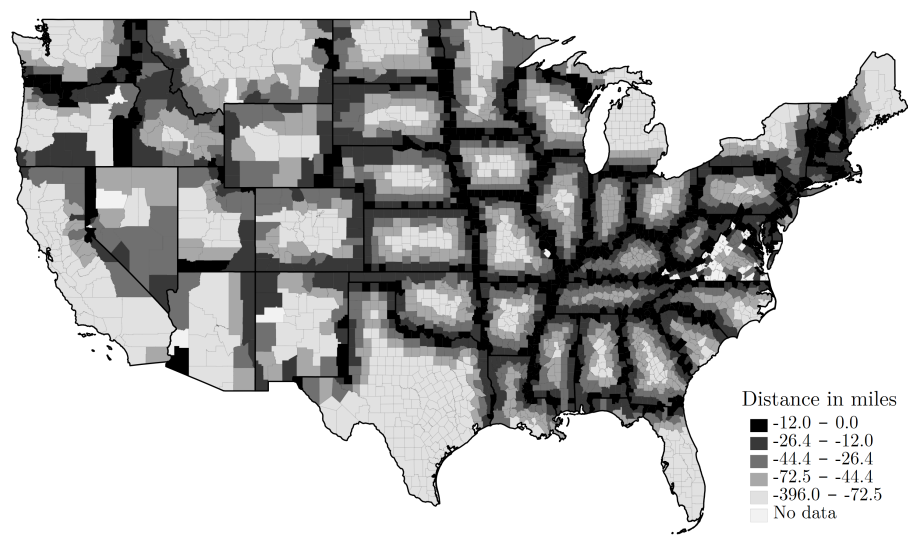


Figure A.2.4 – Panel B: Corporate Income Tax



Notes: Confidence intervals are bias corrected and represent the 95th percentile. The point estimates are transformed using equation 2.6. Number of observation for each iteration is 6,627.

Figure A.2.5 – Distance of county center to the closest state border



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

Chapter 3

Discretionary Policy under Fiscal Decentralization

Abstract

This paper examines how fiscal decentralization affects the implementation and effectiveness of discretionary fiscal policies across different levels of government. Using data from 37 OECD countries from 1990 to 2021, I estimate a system of local projections to analyze the relationship between fiscal federalism and macroeconomic stabilization policy where I introduce a new and comprehensive measure of fiscal decentralization that accounts for both quantitative subnational expenditure shares and qualitative institutional autonomy. To identify discretionary fiscal responses, I use cyclically adjusted primary budgets across different government levels. Impulse responses reveal that higher fiscal decentralization significantly constrains central governments' discretionary spending responses to economic downturns. While subnational governments in decentralized systems show increased fiscal responses, these interventions have limited and sometimes adverse effects on closing the national output gap. This suggests that decentralization may impair macroeconomic stabilization, even when they possess both the resources and autonomy to implement discretionary policies.

JEL Classification: H30, H76, H77, E63

Keywords: Fiscal Decentralization, Discretionary Policy, Local Projections

3.1 Introduction

Fiscal stabilization policies are crucial tools for managing economic fluctuations, particularly when monetary policy is constrained. While extensive research examines the effectiveness of such policies at the national level, the implications of fiscal federalism on stabilization capabilities remain theoretically contentious and empirically unexplored. In a post-zero lower bound world, this discussion is especially relevant given that government spending multipliers can be significantly higher during times of constrained monetary policy [Christiano, Eichenbaum and Rebelo, 2011]. Recent empirical evidence from Kekre [2023] underscores the macroeconomic importance of discretionary fiscal policy, showing that unemployment insurance benefit extensions generated output multipliers around one. Prud'Homme [1995] hypothesizes that fiscal decentralization may constrain central governments' ability to implement effective stabilization policies, yet the empirical validity of this claim and its underlying mechanisms remain untested. This paper examines how fiscal decentralization affects both the availability and effectiveness of discretionary fiscal stabilization policies. Specifically, I investigate two key questions: (1) how does fiscal decentralization affect central governments' ability to deploy discretionary fiscal measures, and (2) what role do subnational governments play in macroeconomic stabilization under different federal structures? The focus is on discretionary measures, such as targeted increases in unemployment benefits responding to output gaps, which are distinct from automatic stabilizers that mechanically adjust to economic conditions. These questions are particularly relevant given the global trend toward fiscal federalism and the increasing importance of fiscal policy in an era of constrained monetary options, especially in currency unions such as the Euro Area where monetary instruments lie beyond member states' jurisdiction (compare Chodorow-Reich [2019]). Within such unions, Nakamura and Steinsson [2014] show that fiscal multipliers vary significantly across regions. Moreover, since the late 1990s, developing economies have increasingly adopted federal fiscal structures, making the implications of decentralization for stabilization policy globally relevant.

Studying decentralization requires careful consideration of its multiple dimensions including fiscal, political, constitutional, structural, and geographical. I focus on fiscal decentralization, examining how subnational governments respond to economic downturns and how these responses affect national stabilization efforts. Even when subnational governments actively pursue countercyclical policies (whether by choice or institutional design), their interventions may not effectively contribute to national stabilization goals. This potential misalignment between sub-

national fiscal responses and national outcomes helps identify two key channels through which decentralization affects stabilization policy. First, a resource constraint channel, where fiscal decentralization limits central governments' spending capacity. Second, an effectiveness channel, where subnational stabilization efforts may have limited or even counterproductive effects on the national output gap due to vertical and horizontal coordination challenges and potential spatial spillovers. This tension is further supported by [Besley and Coate \[2003\]](#), showing that a decentralized provision of public goods may lead to inefficiencies from inter-jurisdictional spillovers. To analyze these questions empirically, I develop a novel measure of fiscal decentralization that combines both quantitative and qualitative dimensions of subnational fiscal authority. This measure integrates the share of subnational expenditures with institutional autonomy metrics from the Regional Authority Index [[Hooghe et al., 2016](#)], providing a more comprehensive characterization of fiscal federal structures than traditional unidimensional measures. This is further supported by the fact that fiscal policy effectiveness varies significantly in country characteristics [[Ilzetzki, Mendoza and Végh, 2013](#)]. As [Baskaran, Feld and Schnellenbach \[2016\]](#), [Blume and Voigt \[2011\]](#), and [Martinez-Vazquez, Lago-Penas and Sacchi \[2017\]](#) demonstrate, the measurement of decentralization significantly impacts empirical findings, and no single conventional measure adequately captures all relevant dimensions.

Using a dataset covering 37 countries from 1990 to 2021, I employ the cyclically-adjusted primary budget approach of [Alesina and Perotti \[1995\]](#) to identify discretionary fiscal responses across different levels of government. This methodology allows for consistent cross-country comparison of discretionary policy changes by isolating them from automatic stabilizers. I estimate impulse responses using panel local projections following [Jorda \[2005\]](#), which allows for flexible estimation of dynamic responses without imposing restrictive structural assumptions. The empirical strategy leverages both cross-sectional variation in decentralization levels and time-series variation in economic conditions and policy responses.

The results reveal three key findings. First, higher levels of fiscal decentralization significantly constrain central governments' discretionary spending responses to economic downturns, providing empirical support for [Prud'Homme \[1995\]](#)'s hypothesis. Second, I find that subnational authorities in highly decentralized systems actively respond to economic downturns, which contradicts theoretical predictions. However, while subnational governments in decentralized systems show increased fiscal responses to economic shocks, these interventions have limited and sometimes adverse effects on closing the national output gap. Third, this ineffectiveness appears

to be driven by coordination challenges among subnational entities, rather than by insufficient resources or authority, as even well-resourced and autonomous subnational governments struggle to contribute effectively to national stabilization efforts. These findings suggest that even with larger fiscal multipliers in decentralized systems (see [Brueckner, Pappa and Valentinyi \[2022\]](#)), the effectiveness of stabilization policies might be negatively affected by high degrees of decentralization. These results have important implications for the theoretical debate on fiscal federalism. While [Qian and Roland \[1998\]](#) emphasizes how fiscal competition among local governments can constrain central budgets, particularly in systems with high transfer dependence, my findings suggest that both vertical and horizontal coordination failures in stabilization efforts may be equally important. This complements the theoretical work of [Tanzi \[1995\]](#) on the potential drawbacks of extensive fiscal decentralization.

The paper contributes to several distinct strands of the literature. It extends the literature on fiscal federalism, building on [Oates et al. \[1972\]](#), by providing empirical evidence of potential negative externalities in stabilization policy. While previous empirical research, such as [Barankay and Lockwood \[2007\]](#) or [Martinez-Vazquez, Lago-Penas and Sacchi \[2017\]](#), emphasizes positive first-order effects of decentralization on public goods provision and economic growth, my findings highlight important second-order constraints on macroeconomic management. Further, the paper advances our understanding of discretionary fiscal policy effectiveness. According to [Wren-Lewis \[2000\]](#), the use of discretionary measures declined before the Great Recession, with [Taylor \[2000\]](#) advocating primary reliance on monetary policy for stabilization. However, as monetary policy became constrained by the zero lower bound, interest in discretionary fiscal policy has resurged [[Christiano, Eichenbaum and Rebelo, 2011](#)]. My results complement work by [Auerbach \[2005\]](#) and [Carlin \[2013\]](#) by identifying the federal structure as a crucial determinant of policy effectiveness, particularly relevant in the current policy environment. Finally, the methodological approach advances the measurement and analysis of fiscal decentralization. By developing a combined measure of quantitative and qualitative decentralization characteristics, the paper addresses longstanding concerns about the limitations of existing unidimensional measures. The identification strategy for discretionary spending across government levels provides a new framework for analyzing multi-level fiscal policies in federal systems.

The rest of the paper is organized as follows. Section 3.2 contextualizes fiscal decentralization and discretionary policy. Section 3.3 outlines the data, while Section 3.4 introduces the identification strategy. In Section 3.4.3, I present the results. Section 3.5 concludes.

3.2 Theoretical Context

The starting point for the theoretical discussion is an appropriate definition of fiscal decentralization. Fiscal decentralization refers to the extent of authority, both in terms of quantity and quality, granted to subnational governments. High fiscal decentralization implies that subnational governments possess significant fiscal autonomy over a substantial budget, with minimal interference from the central government. Fiscal decentralization can be subdivided into two dimensions: expenditure and revenue decentralization. As the degree of fiscal decentralization increases, power is shifted from centralized structures to lower-tier governmental institutions in at least one of these dimensions. It is important to note that this definition allows for the possibility that the other dimension may remain unchanged or even move in the opposite direction [Boadway, Shah et al., 2009]. For instance, a central authority may choose to delegate spending authority to local governments through deregulation while still maintaining control over the institutions responsible for financing these expenditures. This scenario, often referred to as "transfer dependence" in the relevant literature, is discussed by Rodden et al. [2003]. In Figure A.3.1, the approximate levels of transfer dependence in various OECD countries are depicted by plotting subnational expenditures and tax revenues as a percentage of GDP. The data suggests that transfer dependence is widespread in fiscally decentralized economies, as regional expenditures are only partially funded by subnational revenue streams. This indicates that a significant portion of subnational expenditures relies on transfers from the central government. It is worth noting that if these transfers occur after the realization of projects, subnational governments face a "soft budget" [Rodden et al., 2003]. In such cases, regional authorities may overspend on projects and run deficits, which will later be compensated by ex-post transfers from the federal government. This dynamic introduces additional spending for the central authorities and serves as an additional constraint, which explains the presence of subnational balanced budget rules. Low to moderate fiscal decentralization is usually referred to as concentration. Strict forms are called devolution and are achieved by the federal government deregulating either expenditure or funding decisions of subnational government. Hence, transfer dependence is a direct product of devolution under the assumption of central financing. Prud'Homme [1995] argues that the larger the degree of devolution, the stronger the potential implications for discretionary policies.

It is not only theoretically important to focus on the concept of fiscal decentralization and its dimensions and influencing factors. Besides offering a better understanding of the underlying

mechanisms, it is also relevant for identification purposes. Different measurements of subnational fiscal authority have been shown to result in alternating empirical conclusions for the same questions. [Baskaran, Feld and Schnellenbach \[2016\]](#) have conducted a meta-analysis on the effects of fiscal decentralization on economic growth, concluding that no definite deduction can be drawn, as there are vastly heterogeneous outcomes caused by the employment of different measures. In other words, the observed effect of fiscal decentralization is strongly driven by its measurement approach and its contained dimensions. Decentralization cannot be captured by one single indicator. As the aforementioned definition has established, fiscal decentralization is concerned with the degree of independence of fiscal decisions on expenditures and revenue. The distinction is important, as these dimensions are, both theoretically and empirically, captured by different measures. Figure A.3.2 presents an extensive overview of the respective dimensions.

This paper primarily focuses on expenditure decentralization within the frame of fiscal federalism.¹ To accurately assess a country's level of expenditure decentralization, it is necessary to distinguish between two crucial aspects. First, quantitative expenditure decentralization captures the extent of subnational government expenditure within the general government's overall budget. Second, qualitative expenditure decentralization pertains to the degree of regional authority over the allocated subnational budget, indicating the level of subnational spending autonomy.² Both dimensions of expenditure decentralization are needed to be indicative of a decentralized country. Hence, it is imperative to use a balance of both in the identification.

Besides fiscal decentralization, there are other dimensions of governmental decentralization that influence subnational fiscal autonomy.³ Note that different types of decentralization strongly interact with each other and often contain overlapping information. Nonetheless, one cannot

¹Note that federalism and decentralization are no synonyms. [Boadway, Shah et al. \[2009\]](#) argue that unitary countries, such as Denmark, can be fiscally more decentralized than many federal countries. However, federalism is a particularly influential factor in facilitating devolution, as it provides the structure needed for most decentralization.

²It is important to note that the second dimension of fiscal decentralization is referred to as revenue decentralization, which measures the total subnational revenues in relation to central revenues. Within the empirical literature on fiscal federalism, the concept of "tax autonomy" is particularly significant. Tax autonomy represents the ratio of subnational tax revenue to subnational expenditures. However, it is crucial to distinguish tax autonomy from revenue decentralization, as the latter also encompasses transfers and grants from the central government, which may be allocated to specific projects with limited subnational control. Therefore, revenue decentralization is a broader concept that encompasses transfers, while tax autonomy focuses solely on the ratio of subnational tax revenue to subnational expenditures. Consequently, the level of transfer dependence in an economy can be defined as the portion of subnational expenditures covered by transfers from the central government. This is represented by the remainder of spending not covered by subnational tax revenue, denoted as $1 - \text{tax autonomy}$. In terms of qualitative aspects, revenue decentralization depends on the extent of subnational authority, which aligns with the previous discussion.

³First, political decentralization focuses on the democratic legitimacy and voting process of regional governments. Second, administrative and structural decentralization, primarily comprises hierarchical aspects such as the number and size of the subnational bureaucratic structure. Third, constitutional decentralization. As already discussed, federalism does not necessarily imply fiscal decentralization, however, it does provide constitutionally assigned competencies to lower-tier governments and is, therefore, a strong indicator. Lastly, geographic decentralization, which contains natural "geographic fragmentation" (see [Baskaran, Feld and Schnellenbach \[2016\]](#)) but also the size of jurisdictions.

use them interchangeably. Any measure aimed at capturing the effects of fiscal decentralization on the availability and effectiveness of discretionary fiscal stabilizers needs to factor in other forms of decentralization that could potentially play a role. The subsequent stylized theoretical framework shows that constitutional decentralization can have substantial implications for the availability of additional fiscal means once stabilization policy is required. Recall that budgetary transfers from regional governments to the central government might not be feasible when the constitutional autonomy of the respective subnational governments is too large. Hence, it is important to understand the interactions between these dimensions and their synergistic effects on macroeconomic factors such as discretionary fiscal stabilization policy.

After defining various dimensions of decentralization, I proceed by assessing approaches to measure them for my context. Since different measures of the same dimension yield different results, it is important to carefully discuss different types of measures. This allows me to identify the relevant dimension for my context of stabilization policy instruments.⁴ Since the analysis focuses on the expenditure dimension, a comprehensive measure of expenditure decentralization needs to be constructed. First, quantitatively, expenditure decentralization can be directly measured through the so-called "expenditure ratio", i.e. the subnational share of government expenditures.⁵ Second, qualitatively, the scope of subnational expenditure authority, which can be captured utilizing the "Regional Authority Index" by [Hooghe, Marks and Schakel \[2010\]](#). This measure has been developed to provide an index on various factors of subnational fiscal authority and is widely used in the literature. The aggregate indicator provided by this index gives an approximation of the political and fiscal authority of subnational governments in respective countries and ranks from 0 (no authority) to 40 (complete authority).

3.2.1 Contextualize decentralization and discretionary policies

As shown by [Martinez-Vazquez, Lago-Penas and Sacchi \[2017\]](#), positive effects of fiscal decentralization on macroeconomic indicators are prevalent throughout the literature. It is only logical that many economies therefore rely on fiscally federal structures. The literature, however, lacks a comprehensive discussion of implications for discretionary policies, especially empirically. Of particular interest is how fiscal decentralization affects both the ability of central governments

⁴Table A.3.1 provides a comprehensive overview and description of several measures of the respective decentralization dimension used by the literature.

⁵The degree of transfer dependence uses "Vertical Fiscal Imbalance" which essentially measures $1 - \text{Tax Autonomy}$. There are several involved measures for tax autonomy, for instance by [Stegarescu \[2004\]](#). In its simplest form, it takes the ratio of subnational tax revenues to their respective expenditures.

to implement stabilization policies and the effectiveness of subnational fiscal responses in contributing to national stabilization goals.

The theoretical argument regarding central government constraints was first brought forward by Prud'Homme [1995], who argued that fiscal decentralization weakens macroeconomic stabilization by limiting the central government's fiscal capacity while creating a fragmented system of subnational governments that either lack the incentive or ability to implement coordinated countercyclical policies. As central governments can only choose from two sets of tools to counteract economic shocks - monetary and fiscal policy - assessing possible constraints on fiscal capacity is of high importance.

While subnational governments may respond to economic downturns with their own fiscal measures, the effectiveness of such responses in stabilizing the national economy remains theoretically ambiguous. Regional governments face distinct challenges in implementing nationally effective stabilization policies: they may focus on local rather than national objectives, struggle with cross-regional coordination, and confront significant spillover effects that could dilute the impact of their fiscal efforts. These factors suggest that even when subnational governments actively pursue countercyclical policies, their ability to substitute for constrained central government stabilization may be limited.

To formally analyze these mechanisms, I introduce a stylized model of a multilayered government budget constraint. The model enables a systematic examination of how fiscal decentralization affects the central government's fiscal capacity and the potential limitations of subnational stabilization efforts. For the following theoretical discussion, I assume that decentralized economies with advanced subnational expenditure autonomy are characterized by transfer dependence - an assumption supported by empirical evidence shown in Figure A.3.1. Under these conditions, the disposable amount of central government spending is mechanically constrained, as a portion of the total budget must be transferred to regional governments.

Central Government Constraints

I start by considering a balanced budget of a general government: $\tilde{G} - E(\cdot) = R$ where $\tilde{G}$ defines overall national spending, R denotes total revenues, and $E(\cdot)$ can be interpreted as an efficiency premium from delegating public policy objectives to regional authorities.⁶ Decisions on taxes

⁶This is meant to capture the positive impacts of subnational policies on the aggregate economy for the national economy to have quantifiable returns to fiscal decentralization.

are made prior to those on government spending. The general government spending $\tilde{G}$ can then be decomposed into a central (G^c) and subnational (G^s) fraction, such that $\tilde{G} = G^c + G^s$. For simplicity, I do not allow for government debt issuance, which can be interpreted as a very strict balanced budget rule. Since taxes are decided upon prior to spending decisions, $\tilde{G}$ has an upper bound. I can further define the (quantitative) level of decentralization as $\alpha \in [0, 1]$ such that $G^s = \alpha\tilde{G}$. Note that $E(\alpha)$ is an increasing function of the level of fiscal decentralization. I further define Φ as the decentralization multiplier such that $E(\alpha) = \Phi\alpha$ where $\Phi \in [1, \infty]$.⁷ Next, I can decompose central government spending as follows

$$G^c = (1 - \alpha)\tilde{G} = (1 - \alpha)[R + E(\alpha)] \equiv (1 - \alpha)[R + \Phi\alpha] \quad (3.1)$$

where $G^c - G^m \equiv G^f$ computes the central government's available fiscal budget G^f , as a function of central spending and G^m , representing exogenously determined "mandatory" government spending, including automatic stabilizers. The available budget can be used to finance public goods or discretionary fiscal policy measures, respectively. Note that the centrally available expenditure resources depend on the level of decentralization, which is the only choice variable in this setting, such that

$$G^f(\alpha) \equiv (1 - \alpha)\tilde{G} - G^m = (1 - \alpha)[R + \Phi\alpha] - G^m. \quad (3.2)$$

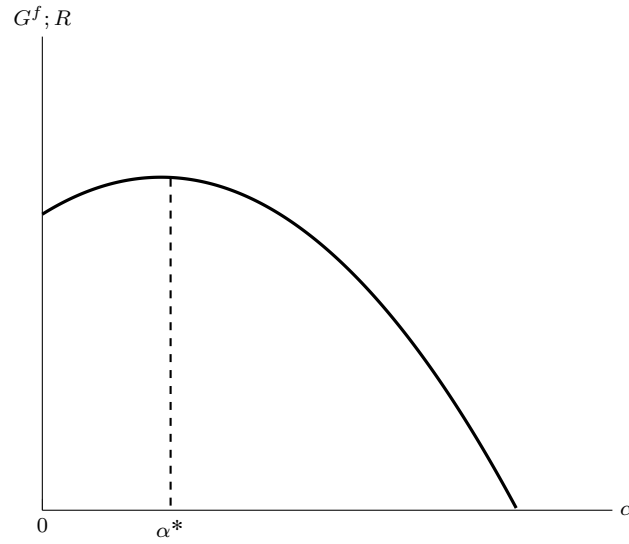
The optimal level of fiscal decentralization maximizes $G^f(\alpha)$. With α as the only choice variable, the first-order condition is $\partial G^f / \partial \alpha = 2\Phi\alpha - \Phi + R \stackrel{!}{=} 0$, yielding $\alpha^* = (\Phi - R)/2\Phi$. This balance between centralization and decentralization is motivated by [Janeba and Wilson \[2011\]](#), who demonstrate that complete centralization is rarely optimal due to political economy constraints, while complete decentralization suffers from inefficiencies arising from tax competition among regional governments. As expected, optimal fiscal decentralization depends on Φ and revenue. Note that optimality here solely refers to maximizing available central funds.⁸ Figure 3.1 illustrates the relationship between decentralization and federal capacities. Under transfer dependence, central government spending capacity increases only while the decentralization premium exceeds subnational transfers. Beyond the optimum, available central funds decrease with these transfers. The curvature shown is parameter-dependent and serves as an example.

⁷The parameter Φ captures the efficiency of subnational governments as well as potential positive externalities from local decisions. The reason I include this is to avoid the nonsensical conclusion that no decentralization is optimal.

⁸Indirectly, optimal subnational spending is determined through the exogenous Φ .

Next, I define the economy's realized output in each period as Y and the potential output as Y^p , where the latter represents the maximum sustainable output level without generating inflationary pressures. The output gap is defined as $\Upsilon = Y - Y^p$, measuring the deviation of actual from potential output. Consider a negative demand shock such that $\Delta\Upsilon < 0$, representing a decline in aggregate demand that pushes the economy below its potential. The central government can deploy expansionary fiscal policy G to close this gap, aiming for $\Upsilon = 0$. This assumes the central government's primary objective is macroeconomic stabilization, though subnational governments may also respond to economic conditions with their own fiscal measures.

Figure 3.1 – G^f -maximizing share of fiscal decentralization

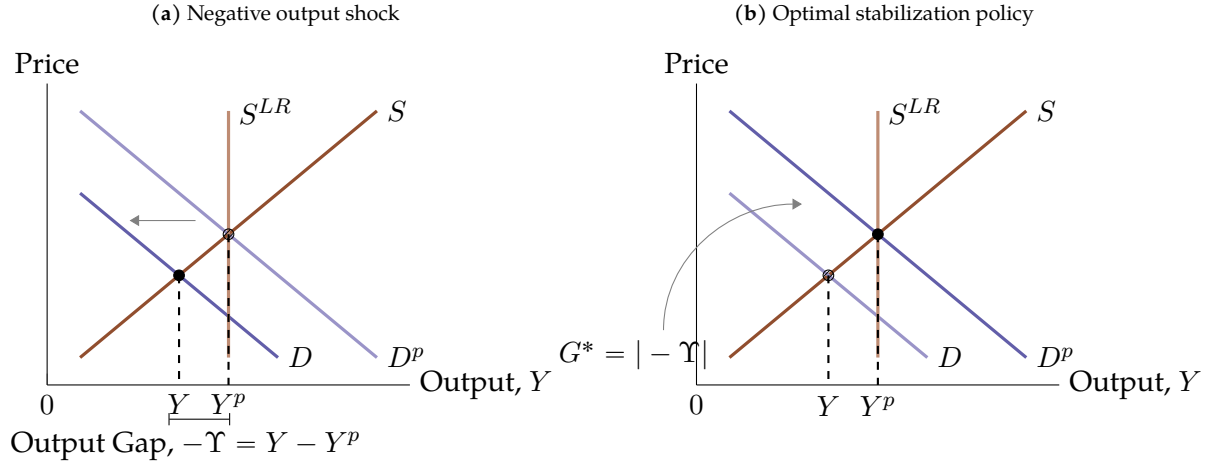


Definition 1 The optimal stabilizing amount of expansionary fiscal policy G^* is such that $\Upsilon^* = Y + G^* - Y^p = 0$, hence $G^* = |-\Upsilon|$.

As this expansionary fiscal policy measure is part of the total amount of government spending, the federal government can use any amount $G \leq G^f(\alpha)$ to stabilize the economy. As shown before, there is an optimal level of decentralization α^* that maximizes central spending. I continue by considering different scenarios and the respective possible central response for various levels of fiscal decentralization. Let me assume that an optimal central fiscal budget is sufficient to stabilize the economy, i.e. $G^* \leq G^f(\alpha^*)$. This condition, $G^* \leq G^f(\alpha^*)$, is crucial as it links the optimal level of fiscal decentralization to stabilization capacity. When satisfied, the central government retains sufficient fiscal space to counteract negative demand shocks even at the optimal level of decentralization. However, if $G^* > G^f(\alpha^*)$, tension emerges between the efficiency gains from decentralization and the central government's ability to stabilize the economy. In this case,

the actual level of decentralization may need to deviate from α^* to maintain adequate stabilization capacity. This trade-off becomes particularly relevant during severe economic downturns when $|\Upsilon|$ is large. Figure 3.2 illustrates this mechanism through standard AD-AS, showing both the initial negative demand shock and the stabilizing effect of fiscal expansion G^* .

Figure 3.2 – Graphical depiction of optimal fiscal stabilization policy



Scenario 1 Consider an economy where the level of fiscal decentralization is set at its optimum α^* . By assumption, the amount of freely available government spending is sufficiently large such that $G^f(\alpha^*; E(\alpha^*)) \geq G^*$. Then the economy can be satisfactorily stabilized such that $\Upsilon^* = 0$, as depicted in Figure 3.2b.

Scenario 2 First, let me consider a fiscally centralized economy where the level of fiscal decentralization is $0 \leq \alpha^L < \alpha^*$. This means that $G^f(\alpha^L; E(\alpha^L)) < G^f(\alpha^*; E(\alpha^*))$. Of course, if $G^f(\alpha^L; E(\alpha^L)) \geq G^*$ we are back to Scenario 1, although, the economy would need a larger share of G^c . However, when we have $G^f(\alpha^L; E(\alpha^L)) < G^*$, central authorities are unable to fully close the output gap. In this stylized setting, the economy is clearly too centralized and misses out on additional revenue streams via delegating public policy decisions to lower-tier governments. Converging closer to the optimal α^* will reduce the output gap where ideally we end up at α^* and $G^f(\alpha^*; E(\alpha^*))$. However, the central government needs to be careful to not over-correct. Scenario 3 is addressing the case where the level of decentralization is too high.

Scenario 3 Now, I consider a strongly fiscally decentralized economy where the level of fiscal decentralization is $\alpha^H > \alpha^*$. This leads to larger transfers $E^H > E^*$, but due to a much larger α^H , we have that $G^f(\alpha^H; E(\alpha^H)) < G^f(\alpha^*; E(\alpha^*))$. Again, considering that $G^f(\alpha^H; E(\alpha^H)) \geq G^*$, we are back to Scenario 1, although we would once again need a larger share of G^f to close the

output gap. Now, in case of $G^f(\alpha^H; E(\alpha^H)) < G^*$, we can only partially close the output gap. This is precisely the case brought up by Prud'Homme [1995]. While subnational governments may respond with their own fiscal measures, the central government's constrained capacity remains a significant concern, particularly assuming the limited effectiveness of subnational stabilization efforts at the national level. The central authorities would need to reallocate some of the fiscal budget back to the federal level. The idea of fiscally centralizing in order to stabilize the economy, especially in the context of a currency union, has also been discussed by Bilbiie, Monacelli and Perotti [2021]. There has been empirical evidence for fiscal centralization by imposing subnational budgetary consolidation in a variety of countries after the Great Recession. Ahrend, Curto-Grau and Vammalle [2013] depicts consolidation during the Euro crisis for multiple transfer-dependent economies. Ahrend, Curto-Grau and Vammalle [2013] discuss that this is not always feasible, especially when governments are heavily decentralized. Demonstrably, a high level of fiscal decentralization is accompanied by a high level of constitutional and administrative decentralization. This results in a constitutionally motivated lower bound of fiscal decentralization. Roughly 7 out of 10 highly fiscally decentralized economies also exhibit strong constitutional decentralization.⁹ In this case, any *ad hoc* centralization efforts will be extremely difficult.

While subnational governments may respond to economic downturns with expansionary fiscal measures, the effectiveness of these responses in stabilizing the national economy remains limited. This compounds the challenge posed by constrained central government capacity, as subnational responses may not effectively substitute for reduced federal fiscal space. The next section examines why subnational stabilization efforts, even when present, may have limited impact on national output stabilization.

Limitations of Subnational Stabilization

The previous analysis demonstrated how fiscal decentralization mechanically constrains central government capacity for stabilization policy. A natural question is whether subnational governments could effectively fill this gap. Even in a highly decentralized system where regional governments actively respond to economic downturns, several factors may limit their effectiveness in stabilizing the national economy. To analyze this formally within the model framework, consider the case where subnational governments attempt to contribute to stabilization. The combined stabilization effort would be $G^c(\alpha^H; E(\alpha^H)) + \beta G^s$, where β represents the fraction

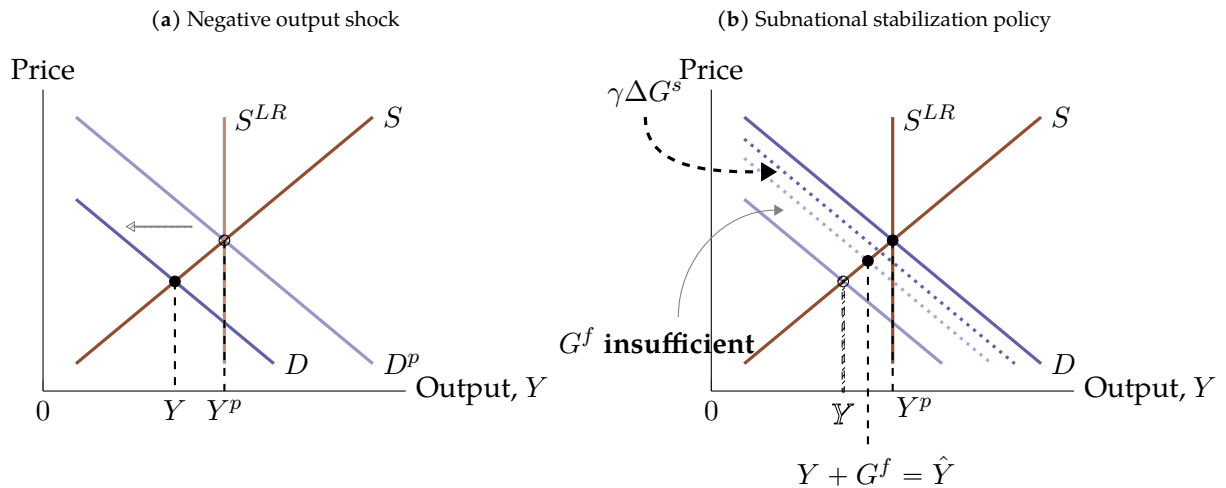
⁹Based on own calculations with the sample described in Section 3.3

of subnational fiscal capacity devoted to stabilization policy. However, the national effectiveness of subnational spending may be diminished by a factor $\gamma \in (0, 1]$, such that the effective stabilization becomes:

$$G^{\text{eff}} = G^c(\alpha^H; E(\alpha^H)) + \gamma\beta G^s \quad (3.3)$$

The parameter γ captures several factors that may reduce the national impact of subnational fiscal efforts. These may include (1) spatial spillovers that dilute the impact of local spending, (2) horizontal coordination failures across regions that lead to inefficient allocation, (3) focus on local rather than national objectives (i.e. vertical coordination failures), and (4) limited fiscal multipliers at the subnational level. This aligns with [Gordon \[1983\]](#) showing how independently made fiscal decisions by subnational governments may create externalities that affect other jurisdictions and often necessitate central government intervention to address resulting inefficiencies. This suggests that even if $G^c(\alpha^H; E(\alpha^H)) + \beta G^s = G^*$ in nominal terms, the effective stabilization G^{eff} may fall short of closing the output gap when γ is sufficiently small. Figure 3.3 illustrates this mechanism: while subnational governments may increase spending in response to the shock ($\Delta G^s > 0$), the resulting shift in aggregate demand may be inadequate to compensate for constrained central capacity.

Figure 3.3 – Graphical depiction of discretionary policy on the central and subnational level



These theoretical channels raise several empirical questions: How do subnational governments actually respond to economic downturns? To what extent do their fiscal efforts translate into national output effects? The empirical analysis that follows will address these questions directly, examining both the presence of subnational responses and, crucially, their effectiveness in contributing to national stabilization.

3.3 Data

The annual dataset consists of 37 economies from Asia and Oceania, Europe, and the Americas. However, note that the dataset contains a substantial fraction of developed Western countries. The time horizon of the investigation spans from 1990 to 2021. The time frame is constrained by data availability. However, I argue that this is a sufficiently long time series as it covers several major global economic recessions as well as presumably many smaller national recessions.¹⁰ For standard macroeconomic variables, I use the IMF International Finance Statistics database. For forecast variables such as the output and employment gap, one possible source is the OECD Outlook database. Alternatively, these variables can also be computed manually. The main data source for public finance data at the central and subnational level is the International Monetary Fund's Government Finance Statistics. This unique source allows me to observe different types of expenditures and revenues and their respective sources for various levels of government. Note that provided data does not distinguish between different subnational governments (e.g. for the U.S., I only have data on the aggregate of all states rather than state-wise data). These aggregate data points are however sufficient for the course of this analysis. This data will then be used to estimate the discretionary spending behavior of various levels of government, as well as to quantify fiscal decentralization. Additionally, I am able to check the precision of the estimates produced with the IMF data by using the OECD Fiscal Decentralization Database.¹¹ For other categories of decentralization, for instance, constitutional aspects, I utilize a dataset provided by Daniel Treisman¹² at UCLA. Besides fiscal decentralization, the data contains information on structural, political, and constitutional decentralization. However, note that indicators provided by this data source are time-invariant and have not been updated since 2008. For the qualitative dimension of decentralization, I use the Regional Authority Index.¹³ This unique index provides annual indicators for almost every country worldwide from 1950-2018. The Regional Authority Index data then consists of multiple indicators, with the most relevant for this study being the degree of authority exercised by subnational governments. It consists of several scores on political, fiscal as well as legal authority.¹⁴ In Table A.3.2, I provide an overview of data on cross-country expenditure decentralization and authority. We can observe that these two dimensions do not

¹⁰The NBER estimates a typical business cycle to last for about 4.7 years.

¹¹The OECD Data consists of only a handful of Western economies and is very incomplete, hence I can only use it as a reference for the precision of my own calculations.

¹²2008, available at: <http://www.sscnet.ucla.edu/polisci/faculty/treisman/>

¹³<https://www.arjanschakel.nl/index.php/regional-authority-index>

¹⁴For more details on the index please refer to the Codebook RAI-Country, Regional Authority Index (RAI v. 3)

always coincide. This further demonstrates the importance of having a measure that captures the quantitative and qualitative components of fiscal decentralization.

3.4 Empirical Investigation

This investigation aims to shed light on an understudied claim that fiscal decentralization can cause constraints to discretionary fiscal policy. In order to do so, I intend to empirically investigate the differences in discretionary reactions among countries with different levels of fiscal decentralization. Therefore, I estimate a structural VAR-type model by employing panel local projection methods similar to [Jorda, Schularick and Taylor \[2013\]](#). The identification strategy requires attention along several dimensions. First, I describe various ways to identify a country's level of fiscal decentralization. Second, I must identify the level of fiscal decentralization, as well as changes in discretionary spending for multiple levels of government. This allows me then to assess the effects of crisis spending on the persistence of the output gap and vice versa. First, I am going to estimate this system for several levels of decentralization. Later, I will directly use continuous measures of expenditure decentralization to estimate marginal effects.

3.4.1 Degree of fiscal decentralization

I introduce fiscal decentralization into my empirical model in two distinct ways. The first approach groups economies into separate bins corresponding to their level of fiscal decentralization, loosely based on [Jorda, Schularick and Taylor \[2013\]](#). This enables direct estimation of the two channels by comparing these "decentralization groups". To identify groups, I establish a classification strategy based on both quantitative and qualitative expenditure decentralization. The quantitative measure uses the expenditure ratio $G_{i,t}^s/\tilde{G}_{i,t} \equiv \alpha_{i,t}$ for each country i in year t , while the Regional Authority Index (RAI) serves as a qualitative measure. For the grouping, I classify countries into three categories: high fiscal decentralization, some fiscal decentralization, and centralization. To ensure tractability, I use averages across time denoted as $\overline{\text{RAI}}_{i,\cdot} = (R)^{-1} \sum_{t=1}^R \text{RAI}_{i,t}$ and $\alpha_{i,\cdot} = (R)^{-1} \sum_{t=1}^R \alpha_{i,t}$ respectively. I define the cut-off levels for high decentralization $\{\overline{\text{RAI}}_{i,\cdot}, \overline{\alpha}_{i,\cdot}\}$ and some decentralization $\{\underline{\text{RAI}}_{i,\cdot}, \underline{\alpha}_{i,\cdot}\}$. It must hold that $\overline{\text{RAI}}_{i,\cdot} > \underline{\text{RAI}}_{i,\cdot} > 0$ and $\overline{\alpha}_{i,\cdot} > \underline{\alpha}_{i,\cdot} > 0$. These definitions allow the separation of countries into the aforementioned three groups, such that $\{F_i^H, F_i^L, F_i^C\}$ indicate high fiscal decentralization, some fiscal decentralization, and centralization respectively. Further note that these groups must be, by design, mutually exclusively, i.e. $F_i^H + F_i^L + F_i^C = 1 \forall i$. Formally, I define the three

groups as

$$\begin{aligned}
 F^H &= \mathbb{1}(\text{RAI}_{i,\cdot} \geq \overline{\text{RAI}}_{i,\cdot} \text{ and } \alpha_{i,\cdot} \geq \overline{\alpha}_{i,\cdot}), \\
 F^L &= \mathbb{1}(\overline{\text{RAI}}_{i,\cdot} > \text{RAI}_{i,\cdot} \geq \underline{\text{RAI}}_{i,\cdot} \text{ and } \overline{\alpha}_{i,\cdot} > \alpha_{i,\cdot} \geq \underline{\alpha}_{i,\cdot}), \\
 F^C &= \mathbb{1}(\text{RAI}_{i,\cdot} \leq \underline{\text{RAI}}_{i,\cdot} \text{ and } \alpha_{i,\cdot} \leq \underline{\alpha}_{i,\cdot})
 \end{aligned} \tag{3.4}$$

The second approach allows for measures of decentralization to enter the model directly, allowing estimation of marginal effects. Initially, I use the expenditure ratio and RAI in turn to estimate the marginal effects separately. Then I estimate the effects in a combined measure. As discussed in [Martinez-Vazquez, Timofeev et al. \[2010\]](#) and [Baskaran, Feld and Schnellenbach \[2016\]](#), this requires careful consideration of the functional form through which different decentralization indicators enter the specification.

Preliminary evidence in Table 3.1 suggests that fiscal decentralization affects the coordination of fiscal responses across government levels. While centralized countries show a strong correlation between central and subnational spending growth during recessions (correlation of 0.90), this relationship weakens substantially in decentralized systems (correlation of 0.35). This pattern motivates our investigation of how fiscal decentralization affects both the implementation and effectiveness of stabilization policy across government levels.¹⁵

Table 3.1 – Correlations of central and subnational spending growth

	All times	Recessionary regimes	Non-recessionary regimes
High decentralization (F^H)	0.3284	0.3546	0.3519
Some decentralization (F^L)	0.6384	0.3315	0.7598
Centralization (F^C)	0.6063	0.9017	0.4643

Notes: The correlations are obtained by $\text{Corr}(\Delta G_{i,t}^c, \Delta G_{i,t}^s)$. The groups are defined as outlined in the text. I can see that, for centralized economies, central and subnational spending growth strongly coincide. For highly decentralized economies this concurrence disappears.

3.4.2 Identifying Discretionary Fiscal Policy Across Government Levels

The empirical analysis requires identifying discretionary fiscal responses at different levels of government and distinguishing them from automatic stabilizers, whereas discretionary instru-

¹⁵To identify recessionary periods, I employ the [Bry and Boschan \[1971\]](#) algorithm which approximates the NBER methodology. The algorithm identifies local extrema in GDP per capita $y_{i,t}$, with peaks indicated by $\mathbb{1}_{P_{i,t}}(\Delta y_{i,t+1} < 0, \Delta y_{i,t} > 0)$ and troughs by $\mathbb{1}_{R_{i,t}}(\Delta y_{i,t+1} > 0, \Delta y_{i,t} < 0)$. A recession spans $R_{i,t} = 1 \forall t : t - m \leq t \leq t + n$ where $P_{i,t-m} = 1$ and $R_{i,t+n} = 1$.

ments as non-automatic fiscal actions aimed at stabilizing the economy (e.g. 2020 CARES Act in the U.S.). More generally, discretionary fiscal policy represents a governmental reaction to macroeconomic shocks that deviates from automatic responses. The standard approach for identifying exogenous fiscal policy shocks in the literature, the so-called narrative approach (pioneered by [Romer and Romer \[2010\]](#) and [Ramey \[2011\]](#)) is highly data-intensive, even for one country. Considering that my investigation is based on a cross-country comparison of 37 countries and the objective is to identify discretionary policy changes for multiple levels of government, a narrative approach is beyond the scope of this analysis. Instead, I employ the cyclically-adjusted primary balance approach developed by [Alesina and Perotti \[1995\]](#) and [Alesina and Ardagna \[1998\]](#), which has been more recently applied by [Yang, Fidrmuc and Ghosh \[2015\]](#) and [Carnazza, Liberati and Sacchi \[2020\]](#). This method allows consistent identification across countries and government levels. For notational tractability, let $G_{i,t} = \{\tilde{G}_{i,t}, G_{i,t}^c, G_{i,t}^s\}$ represent expenditures at different government levels. With $U_{i,t}$ denoting the national unemployment rate, I first estimate

$$G_{i,t} = \rho_0 + \rho_1 U_{i,t} + \text{ttrend} + \nu_{i,t}, \quad (3.5)$$

where `ttrend` adds a linear time trend. This allows to evaluate predicted government spending under the assumption of constant economic conditions, approximated with the lagged unemployment rate, i.e. $\hat{G}_{i,t}(U_{i,t-1})$. In the absence of automatic stabilizers, discretionary spending changes $g_{i,t}$ are then identified as

$$\hat{G}_{i,t}(U_{i,t-1}) = \hat{\rho}_0 + \hat{\rho}_1 U_{i,t-1} + \text{ttrend} \quad \text{such that} \quad (3.6)$$

$$g_{i,t} := \hat{G}_{i,t}(U_{i,t-1}) - G_{i,t-1}. \quad (3.7)$$

3.4.3 Impulse responses

I aim to estimate a system of equations to investigate the interdependency between discretionary fiscal actions and output/employment gaps for various levels of fiscal decentralization. I utilize the local projection method introduced by [Jorda \[2005\]](#). Here, I can directly estimate impulse responses by re-estimating the contemporaneous specification for $H - 1$ periods of the respective dependent variable. This setting allows for a tractable estimation procedure through OLS such that I can focus on the economics that drive my results. To set up my system, I need to define a few variables. So far, I captured discretionary spending changes in their absolute size. This

leads to larger average changes for larger countries. To cater to this issue, I normalize changes in discretionary spending with the respective country's GDP. Therefore, I define $Y_{i,t-1}$ as the one-period lagged GDP of country i such that $\mathcal{G}_{i,t} := g_{i,t}/Y_{i,t-1}$, where I inherit the aforementioned notational tractability such that $g_{i,t} = \{\tilde{g}_{i,t}, g_{i,t}^c, g_{i,t}^s\}$. Furthermore, I define $Y_{i,t}^p$ as the potential GDP and $\mathcal{Y}_{i,t} := \Upsilon_{i,t}/Y_{i,t}^p$ as the output gap ratio. I will first write down the contemporaneous model before generalizing it to the full dynamic system. The two following equations

$$\begin{aligned}\Delta\mathcal{G}_{i,t} &= \beta(-\mathcal{Y}_{i,t-1}) + \delta_i + \text{ttrend} + \sum_{p=1}^P [\phi\Delta\mathcal{G}_{i,t-(p+1)} + \Gamma_p\mathbf{X}_{i,t-p}] + \varepsilon_{i,t} \\ \Delta\mathcal{Y}_{i,t} &= \eta\mathcal{G}_{i,t} + \delta_i + \text{ttrend} + \sum_{p=1}^P [\lambda\Delta\mathcal{Y}_{i,t-(p+1)} + \Xi_p\mathbf{X}_{i,t-p}] + \varepsilon_{i,t},\end{aligned}\tag{3.8}$$

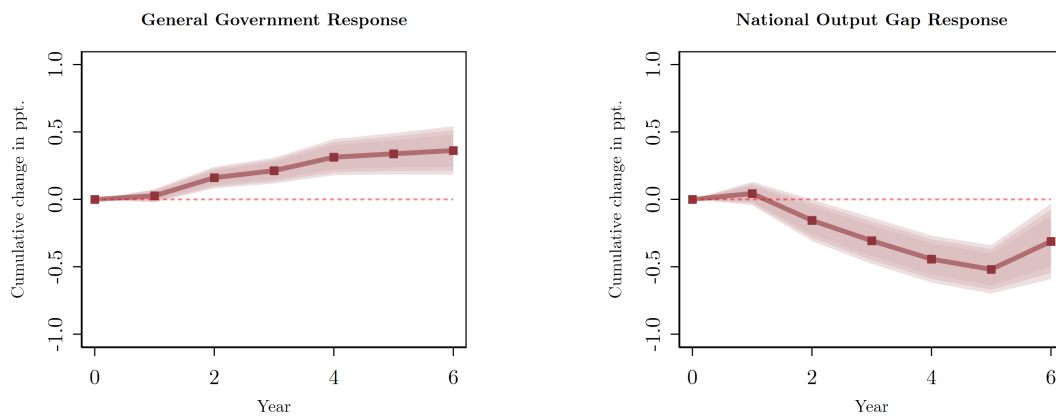
represent the main specification. Here, vector $\mathbf{X}$ collects an array of time- and country-varying control variables.¹⁶ Additionally, δ_i controls for any time invariant country-specific effect and t adds a linear time trend. Note that, similar to [Ramey and Zubairy \[2018\]](#), the model employs timing restrictions mimicking a Cholesky identification, where a change in the output gap is assumed to have a lagged effect on discretionary spending. The advantage of the local projection method is that I can compute the impulse responses for H periods, by simply estimating a system OLS regressions where the matrices $\mathbf{B} = [\hat{\beta}^0, \hat{\beta}^1, \dots, \hat{\beta}^{H-1}]^T$ and $\mathcal{N} = [\hat{\eta}^0, \hat{\eta}^1, \dots, \hat{\eta}^{H-1}]^T$ yield the estimated impulse responses of the respective shock to either the relative output gap or discretionary spending. For tractability, I re-write the system of equations (Appendix B.3, equation B.3.1) such that I estimate cumulative response to the shocks

$$\begin{aligned}\Delta_h\mathcal{G}_{i,t+h} &= \beta^{(h)}(-\mathcal{Y}_{i,t-1}) + \text{ttrend} + \text{control}(\mathcal{G}) + \varepsilon_{i,t+h} \\ \Delta_h\mathcal{Y}_{i,t+h} &= \eta^{(h)}\mathcal{G}_{i,t} + \text{ttrend} + \text{control}(\mathcal{Y}) + \varepsilon_{i,t+h}\end{aligned}\tag{3.9}$$

for $h = 0, 1, \dots, H-1$. I further define $\text{control}(\mathcal{G}) = \delta_i^{(h)} + \sum_{p=1}^P [\phi^{(h)}\Delta\mathcal{G}_{i,t-(p+1)} + \Gamma_p^{(h)}\mathbf{X}_{i,t-p}]$ and $\text{control}(\mathcal{Y}) = \delta_i^{(h)} + \sum_{p=1}^P [\lambda^{(h)}\Delta\mathcal{Y}_{i,t-(p+1)} + \Xi_p^{(h)}\mathbf{X}_{i,t-p}]$. Note that the left-hand side variable now captures the cumulative variable $\Delta_h\mathcal{G}_{i,t+h} = \mathcal{G}_{i,t+h} - \mathcal{G}_{i,t-1}$ and $\Delta_h\mathcal{Y}_{i,t+h} = \mathcal{Y}_{i,t+h} - \mathcal{Y}_{i,t-1}$ respectively.

As mentioned before, the local projections' environment allows for a straightforward interpretation of the results. Therefore, $\mathcal{N}$ captures the cumulative percentage point change in the output gap ratio associated with a 1 percentage point increase in the change of discretionary spending

¹⁶As of now, this control vector includes changes in GDP per capita, unemployment rates, and short-run interest rates. Additionally, I add a Euro Area dummy to control for a country's participation in the currency union.

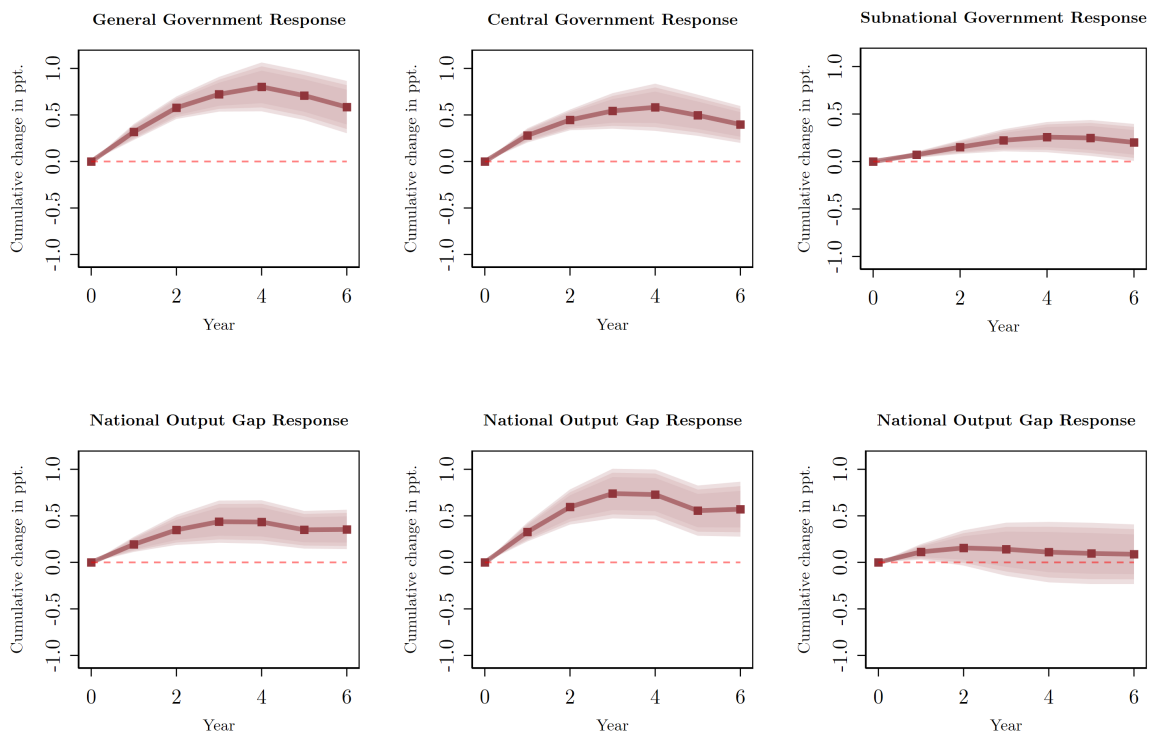
Figure 3.4 – Cumulative responses for total spending

Notes: The left graph shows cumulative response of changes in the total spending to a 1 ppt drop in the output gap ratio. The right graph depicts the cumulative impulse response of the output gap ratio to a 1 ppt change in total spending growth of the general government. Shaded areas represent the 80%, 90%, and 95% confidence intervals respectively.

ratio. Similarly, β defines the cumulative percentage point deviation in the change of discretionary spending ratios related to a 1 percentage point decrease in the relative output gap. To evaluate the model, I report the impulse responses estimated with equation 3.9 for general government total spending in Figure 3.4 and then different levels of discretionary spending in Figure 3.5. As expected, I find a sharp and quite persistent incline in general government discretionary spending associated with a 1 ppt drop in the relative output gap. I further estimate a positive effect of expansionary fiscal policy on the relative output gap. These initial response estimations are in support of my methodology to fit this investigation. Figure 3.4 indicates the importance of identifying the discretionary changes of government spending as opposed to simply total spending. I find that using total expenditures I estimate inverse responses for the output gap ratio, indicating that my explanatory variable is cyclically behaved. Hence, it is crucial to apply discretionary spending to my model.

I repeat these computations for general, central, and subnational spending separately. I present the results in the three columns of Figure 3.5. The juxtaposition allows to compare central and subnational discretionary responses to a decline in the relative output gap. Additionally, I estimate potential differences in the output gap's reaction to an increase in the central and subnational discretionary expenditures to capture the respective average effectiveness of the measures. The fiscal responses are qualitatively similar across levels of government. That is, discretionary spending is increasing after a widening in the output gap. The magnitude of the responses is visibly larger for the central government. As these effects are global averages across different levels of decentralization, this could be either driven by a lack of subnational autonomy or a

Figure 3.5 – Cumulative responses for discretionary spending



Notes: The upper graphs show cumulative response of changes in the discretionary spending to a 1 ppt drop in the output gap ratio. The lower graphs depict the cumulative impulse response of the output gap ratio to a 1 ppt change in discretionary spending growth of the respective level of government. Shaded areas represent the 80%, 90%, and 95% confidence intervals respectively.

lack of subnational agency, depending on the composition of the sample. To investigate this further, I must estimate the effects with respect to various levels of decentralization.

A similar observation can be drawn for the output gap response to an increase in discretionary spending. The output gap closing is driven by the central interventions. An increase in subnational discretionary policies has minor and statistically insignificant implications for the output gap. Note that, as subnational expenditures are measured as an aggregate of all local and regional governments, a change in this variable could be driven by $n \in [1, N]$ subnational governments.¹⁷ This finding therefore expands Prud'Homme [1995] argument of insignificant subnational discretionary policy measures to a potential aggregate level. In other words, even the combination of multiple subnational governments has little effect on the national economy's output gap. The takeaway of this aggregate exercise is that while subnational fiscal policies do respond to negative output shocks, they do not significantly influence the overall output of the economy. This reinforces the model, suggesting that subnational governments employ inadequate policies to close the output gap.

¹⁷For a country with N subnational governments.

By decentralization groups

The most straightforward approach to investigate the influence of fiscal decentralization on the interdependency of changes in discretionary spending and the relative output gap is by estimating equation 3.9 for the different subgroups $\{F_i^H, F_i^L, F_i^C\}$. To categorize economies according to the identification strategy, I must parameterize $\{\overline{\text{RAI}}_{i,\cdot}, \bar{\alpha}_{i,\cdot}\}$ and $\{\underline{\text{RAI}}_{i,\cdot}, \underline{\alpha}_{i,\cdot}\}$. I choose a fairly restrictive main parametrization, which is presented in Table 3.2.¹⁸

Table 3.2 – Parametrization for decentralization grouping

	Cut-off	Closest value Above	Closest value below
$\overline{\text{RAI}}_{i,\cdot}$	19	19.84 (Italy)	17.11 (Netherlands)
$\underline{\text{RAI}}_{i,\cdot}$	10	10.13 (Norway)	9.39 (Hungary)
$\bar{\alpha}_{i,\cdot}$	28%	28.67% (Italy)	27.54% (Latvia)
$\underline{\alpha}_{i,\cdot}$	10%	11.16% (Israel)	9.37% (Turkey)

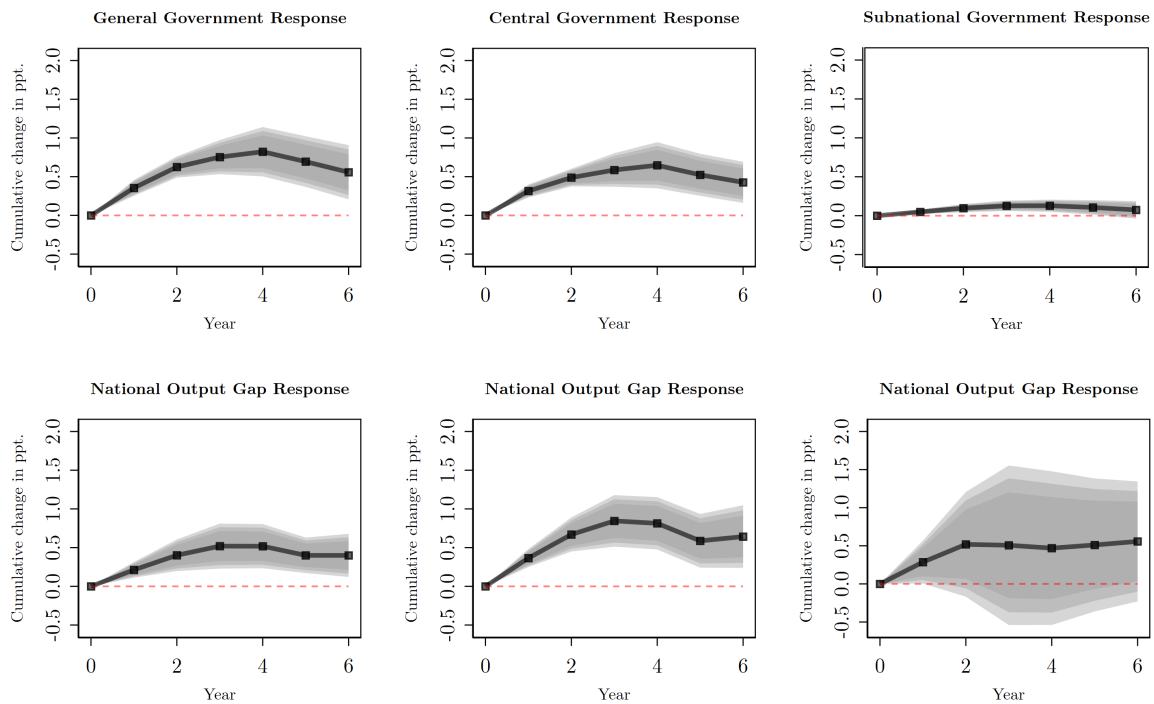
Notes: The maximum possible level of $\text{RAI}_{i,t}$ is 40, defining complete independence of all regional authorities. The subnational share of total government expenditures is bounded such that $\alpha_{i,t} \in [0, 1]$.

Figure A.3.3 depicts a world map showing the geographic distribution of different levels of decentralization. Clearly, geographic decentralization plays an important role in the development of fiscally federal structures. As previously mentioned, in this case, federalism arises out of necessity, which further motivates the inclusion of a country fixed effect in the regression analysis. Corresponding summary statistics can be found in Table A.3.3. In the absence of a shock, there are no systematic differences among these groups in the main variables of interest, i.e. changes in discretionary policies and the relative output gap. Similarly, other confounders appear to be balanced, with the exception of the unemployment rate, which is found to be slightly elevated in centralized economies. Lastly, to little surprise, fiscally decentralized are on average more federal, have more tiers of government, and observe a larger level of subnational autonomy. This overview lends credible support to the overall validity of the grouping mechanism.

Centralization Starting off, I estimate equation 3.9 for centralized economies. The results are shown in Figure 3.6.

Unsurprisingly, stabilization policies are mainly driven by the central government. Subnational governments neither have enough funds nor the fiscal autonomy to react to a national shock. Instead, the central government is solely responsible for discretionary measures. The national

¹⁸The table also includes the closest observed values to the respective cut-offs.

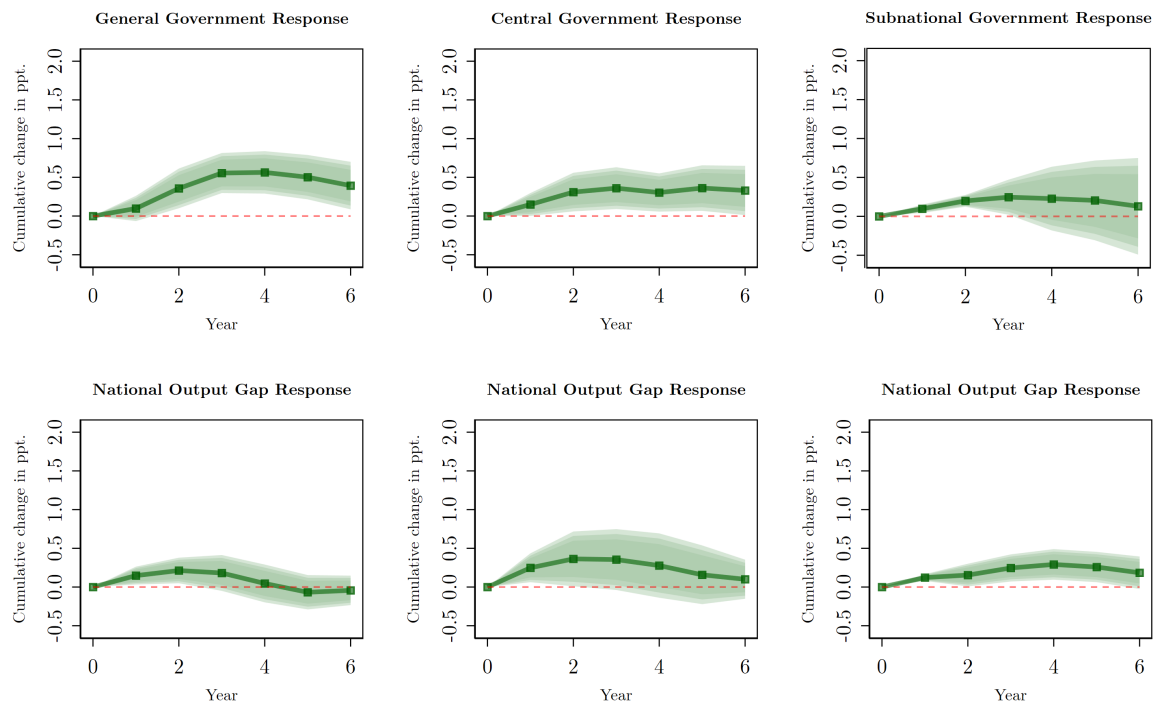
Figure 3.6 – Cumulative responses for centralized economies

Notes: The upper graphs show cumulative response of changes in the discretionary spending to a 1 ppt drop in the output gap ratio. The lower graphs depict the cumulative impulse response of the output gap ratio to a 1 ppt change in discretionary spending growth of the respective level of government. Shaded areas represent the 80%, 90%, and 95% confidence intervals respectively.

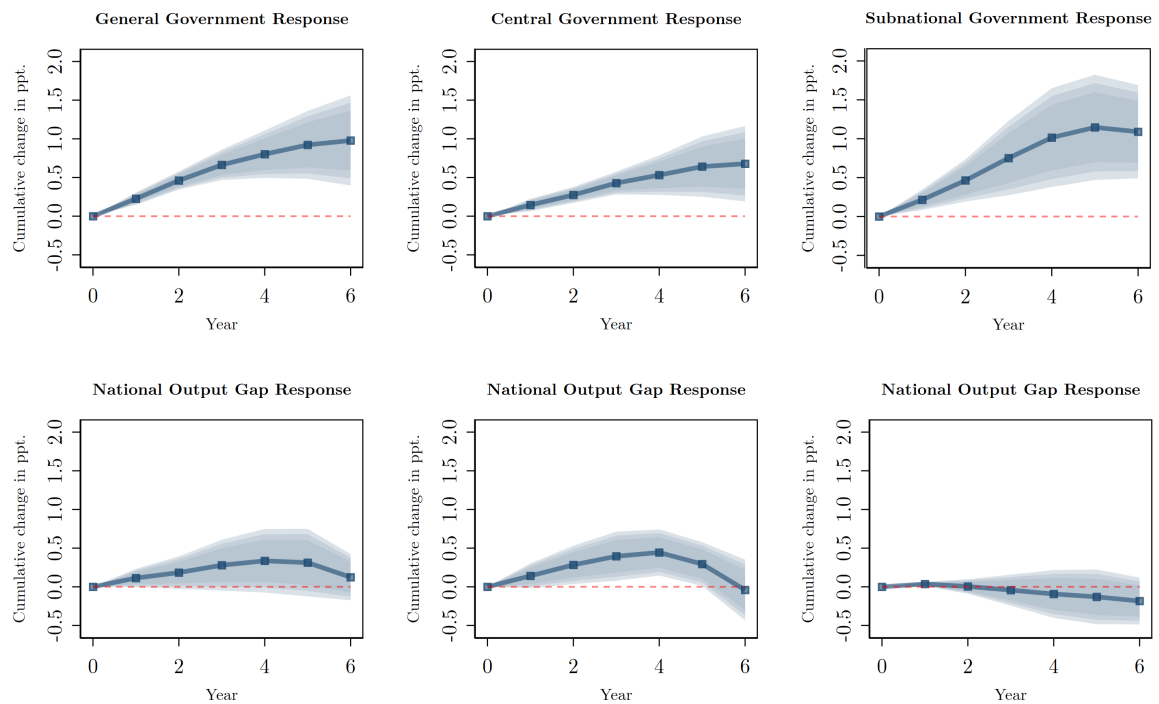
output gap response is similar among discretionary measures of different levels of government. This indicates that there is a high alignment in the employed measures. The effects of subnational discretionary spending changes are insignificant due to a lack of power as the economies of this subset observe little to no variation in subnational spending.

Some Decentralization I repeat this analysis for economies with some level of decentralization, i.e. decentralized economies, albeit their subnational governments not being as autonomous and not controlling as many funds as for the previous group.¹⁹ Results are presented in Figure 3.7. I start by analyzing the right panel of subnational responses and their effects on the national output gap. The point estimates are slightly higher than the case before, however also less significant. The effect on the national output gap is also considerably smaller. The central government also increases discretionary spending following an output gap shock. This means that, opposing theoretical claims by Prud'Homme [1995], different layers of government in fiscally decentralized economies do not offset each other's policies. The reaction is however smaller than for centralized economies.

¹⁹ As the values for $\{\overline{RAI}_{i,\cdot}, \overline{\alpha}_{i,\cdot}\}$ are defined somewhat randomly, these cut-offs should not be interpreted as definitive.

Figure 3.7 – Cumulative responses for economies with some decentralization

Notes: The upper graphs show cumulative response of changes in the discretionary spending to a 1 ppt drop in the output gap ratio. The lower graphs depict the cumulative impulse response of the output gap ratio to a 1 ppt change in discretionary spending growth of the respective level of government. Shaded areas represent the 80%, 90%, and 95% confidence intervals respectively.

Figure 3.8 – Cumulative responses for highly decentralized economies

Notes: The upper graphs show cumulative response of changes in the discretionary spending to a 1 ppt drop in the output gap ratio. The lower graphs depict the cumulative impulse response of the output gap ratio to a 1 ppt change in discretionary spending growth of the respective level of government. Shaded areas represent the 80%, 90%, and 95% confidence intervals respectively.

High Decentralization Lastly, I run my analysis for highly decentralized economies. The results are collected in Figure 3.8. Two results are worth noting. Here, subnational governments respond heavily to a nationwide widening of the output gap. Changes in this discretionary subnational spending however translate to no significant effect on the output gap. Together with the insights from centralized economies (Figure 3.6), this indicates that subnational governments reacting autonomously to a negative nationwide output shock, have no impact on the national economy. Based on [Prud'Homme \[1995\]](#), there are two potential drivers. First, subnational economies, and hence their spending, are often too small to affect nation-wide output. Second, a lack of coordination among subnational governments can render even their collective efforts ineffective, as uncoordinated or contradictory policies may cancel each other out or create regional imbalances.

Visually, there is little evidence for the restriction argument. In other words, with the crude grouping measure, it is difficult to infer whether an elevated level of decentralization constrains the central government's response to negative output shocks. Hence, a more nuanced analysis is warranted. To estimate these effects marginally with respect to the level of decentralization, I need to introduce continuous measures into equation 3.9.

Continuous decentralization measures

Following [Martinez-Vazquez, Timofeev et al. \[2010\]](#) and [Baskaran, Feld and Schnellenbach \[2016\]](#), I separately consider quantitative and qualitative measures of fiscal decentralization. Given the inherent correlation between these measures, I start by estimating two distinct systems where $FD_{i,t}$ represents a variety of continuous decentralization measures, which will be explained in the subsequent analysis. The estimation system can be adapted to include an interaction term with the respective continuous measure of decentralization. This specification allows estimation and visualization of marginal effects across different levels of decentralization.

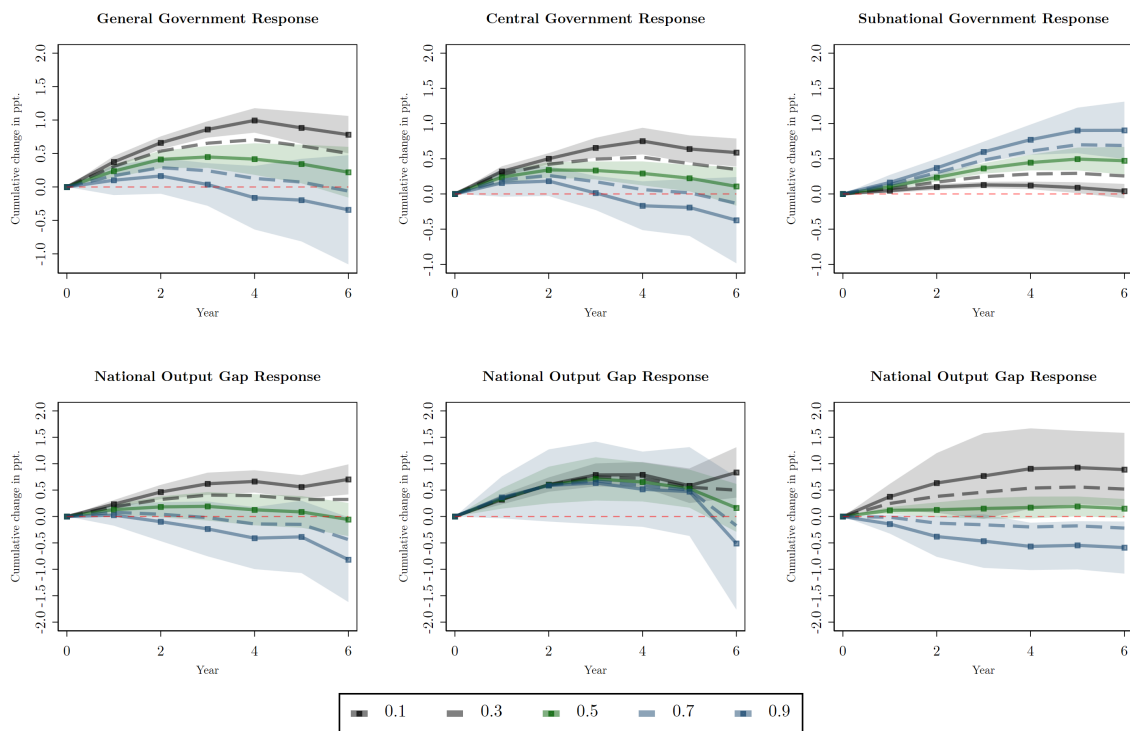
$$\begin{aligned}\Delta_h \mathcal{G}_{i,t+h} &= \beta_{1,1}^{(h)}(-\mathcal{Y}_{i,t-1}) \times FD_{i,t-1} + \beta_{2,1}^{(h)}(-\mathcal{Y}_{i,t-1}) + \beta_{3,1}^{(h)}FD_{i,t-1} + \text{ttrend} + \text{control}(\mathcal{G}) + \varepsilon_{i,t+h} \\ \Delta_h \mathcal{Y}_{i,t+h} &= \eta_{1,1}^{(h)}\mathcal{G}_{i,t} \times FD_{i,t} + \eta_{2,1}^{(h)}\mathcal{G}_{i,t} + \eta_{3,1}^{(h)}FD_{i,t} + \text{ttrend} + \text{control}(\mathcal{Y}) + \varepsilon_{i,t+h}.\end{aligned}\tag{3.10}$$

While this approach offers tractable estimation and straightforward interpretation, it requires caution when comparing results across specifications. The structural interdependence between quantitative and qualitative decentralization suggests these estimates should be interpreted as

alternative measurements of the same underlying effects rather than complementary dimensions. Thus, using the measures separately represents two approaches to capturing similar marginal effects through different measurement lenses. As argued before, using only one measure ignores the multidimensionality of expenditure decentralization. Hence, I subsequently combine these measures into a single index to provide a comprehensive view of decentralization effects.

Quantitative I begin with analysis using the expenditure ratio $\alpha_{i,t}$, measuring the quantitative aspect of fiscal decentralization.²⁰ The patterns broadly align with the discrete grouping results discussed above.

Figure 3.9 – Cumulative responses using the expenditure ratio as a measure of fiscal decentralization



Notes: The upper graphs show cumulative response of changes in the discretionary spending to a 1 ppt drop in the output gap ratio. The lower graphs depict the cumulative impulse response of the output gap ratio to a 1 ppt change in discretionary spending growth of the respective level of government. Shaded areas represent the 90% confidence intervals respectively.

The upper panel of Figure 3.9 reports the response estimates of discretionary spending for all layers of government. The results indicate heterogeneous central government reactions to negative output shocks across different levels of expenditure ratio. The pattern is very much in alignment with the previous results. That is, the higher the level of decentralization, the smaller

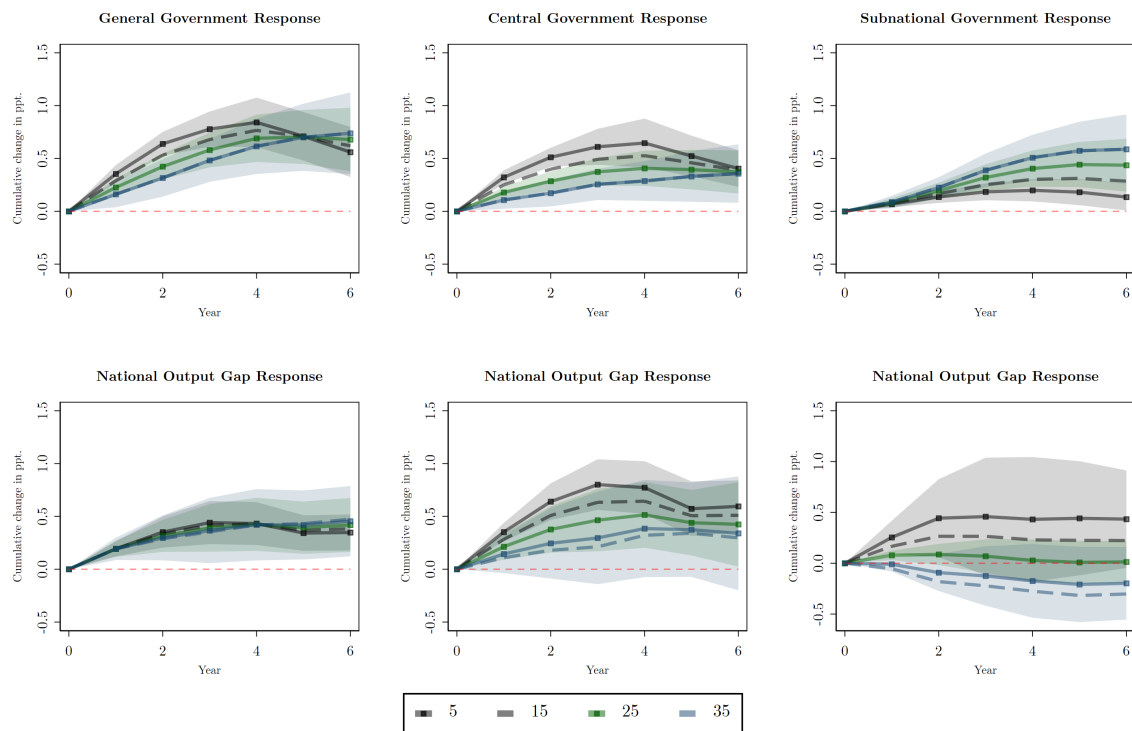
²⁰Note that, in my sample, $\max(\alpha) = 0.76$ hence marginal effects beyond this threshold are out-of-sample extrapolations and should be interpreted as such.

the response to the output shock by the central government. Similar to before, responses by subnational governments are flipped. This is a mechanical relationship further providing support to the empirical strategy.

The results for the output gap response (Figure 3.9) are more striking. Beyond the findings from the discrete grouping, I find that the output response to subnational discretionary spending turns negative for a high enough level of decentralization. Figure 3.8 was hinting at a potential adverse relation between the level of decentralization and the output gap response. These effects were however not strong enough to offset the overall positive impact of the central government. However, isolating the very top quantitatively decentralized economies reveals that the central government is unable to offset the negative effect of the subnational governments. This finding supports the theoretical framework: while decentralized systems enable stronger subnational fiscal responses, the lack of coordination and potential inefficiencies in local-level stabilization efforts suggest resulting in discretionary spending that, while possibly beneficial at the regional level, fails to effectively contribute to aggregate macroeconomic stabilization. Beyond that, at a very high level, it seems to actively hinder stabilization.

Qualitative Next, I examine marginal effects using the Regional Authority Index to capture qualitative expenditure decentralization. The specification follows the structure of equation 3.10, now using $RAI_{i,t}$. A priori, the line of argument I have taken so far requires similar results from this exercise as both quantitative and qualitative decentralization are required for subnational governments to divert from central stabilization policies. The impulse responses are presented in the upper panel of Figure 3.10.²¹ The ordering of the magnitudes with regard to the level of decentralization is identical to the case above. A noteworthy difference is the central response for economies with a very high level of qualitative decentralization. Here, contrary to the previous findings, central and therefore general spending responses are above zero. This in turn also leads to non-negate output responses in the lower panel of (Figure 3.10). These findings highlight that subnational autonomy, capturing qualitative decentralization, is a necessary but not sufficient condition for the theoretical channel suggested by the theory. This is not surprising, as the constraint argument heavily relies on the scope of expenditures on a subnational level. Hence, I need to find a functional form to let both measures enter the system at the same time.

²¹Note that since this uses a different measure of fiscal decentralization, the marginal effect groupings differ from those in Figure 3.9, precluding statistical comparison of response patterns.

Figure 3.10 – Cumulative responses using the RAI as a measure of fiscal decentralization

Notes: The upper graphs show cumulative response of changes in the discretionary spending to a 1 ppt drop in the output gap ratio. The lower graphs depict the cumulative impulse response of the output gap ratio to a 1 ppt change in discretionary spending growth of the respective level of government. Shaded areas represent the 90% confidence intervals respectively.

Combined The parallel analyses using quantitative and qualitative measures reveal consistent patterns, suggesting a robust relationship between decentralization and stabilization effectiveness. However, while both dimensions interact in determining a government's actual capacity for fiscal stabilization, the expenditure ratio (α) serves as the foundational constraint. This perspective is supported both theoretically and empirically: theoretically, because the share of resources controlled at subnational levels creates the fundamental structure within which authority can be exercised; and empirically, as demonstrated in Figure 3.11, where the RAI tends to overstate decentralization in economies with high authority but relatively low expenditure shares. To account for this interaction and provide a more comprehensive assessment, I construct a combined measure that captures both the actual spending authority (through α) and the institutional capacity for autonomous decision-making (through RAI). A combined index allows to examine whether the patterns observed in the separate analyses persist when considering both dimensions simultaneously, while potentially offering more precise estimates by incorporating the full scope of fiscal decentralization. The challenge is to decide on a functional form of the combined measure. I will offer two approaches: a naive but hence tractable and a

more advanced measure built to capture the ordered relevance for the effects on macroeconomic stabilization. I define the combined measure as $FD_{i,t}$.

The naive approach ignores the relative importance of the two measures and takes the geometric mean such that

$$FD_{i,t}^n = \left[\alpha_{i,t} \times \frac{RAI_{i,t}}{\max\{RAI_{i,t}\}} \right]^{\frac{1}{2}} \in [0, 1]. \quad (3.11)$$

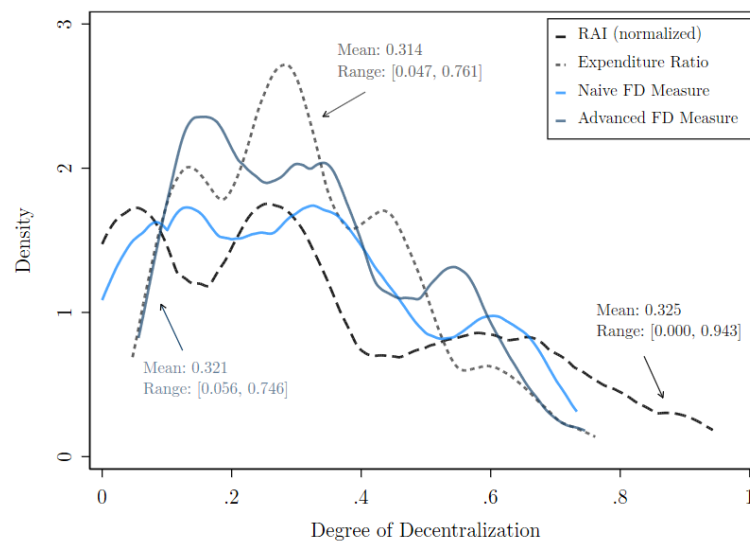
Because the RAI ranges from 0 to 40 while α is bounded by $[0,1]$, normalizing RAI by dividing by 40 and then taking the geometric mean, will lead to the RAI variations dominating the measure. This happens because small absolute changes in RAI translate to larger relative changes after normalization compared to similar changes in α . Hence, the result from this estimation should resemble Figure 3.10. Figure A.3.4 indeed shows similar insights confirming that the naive combined measure overstates the relative importance of the RAI.

To create a more sophisticated measure I account for the fact that while the level of authority is an important factor, decentralization is ultimately manifested through actual spending patterns, captured by the expenditure ratio. To do so, I create a quantitative decentralization measure weighted by the authority of subnational governments. This new measure can then be seen as a substitute for the α share in the model, capturing the effective decentralized share of expenditures.²² To capture information from the expenditure ratio, while also adjusting the measure for subnational autonomy, I propose a nuanced combined measure that allows to account for the difference in the variances. I use inverse variance weighting to identify a weighted average of the two measures. Formally, the advanced measure then defines

$$FD_{i,t} = \frac{\alpha_{i,t} \times [\text{Var}(\alpha_{i,t})]^{-1} + \Omega_{i,t} \times [\text{Var}(\Omega_{i,t})]^{-1}}{[\text{Var}(\alpha_{i,t})]^{-1} + [\text{Var}(\Omega_{i,t})]^{-1}} \quad (3.12)$$

where $\Omega_{i,t} = RAI_{i,t} / \max\{RAI_{i,t}\}$. The kernel density is again plotted in Figure 3.11. The new measure drastically decreases the variance relative to the naive approach. An important feature of this measure is that the range is nearly identical to the expenditure ratio. This allows $FD_{i,t}$ as a theoretical substitute for $\alpha_{i,t}$ from the model, as the measure is empirically bounded by the same expenditure shares. Figure 3.11 plots the densities of the respective decentralization measures to provide a visual comparison of the alternative continuous measures.

²²Note that autonomous subnational spending control is a key assumption of the model.

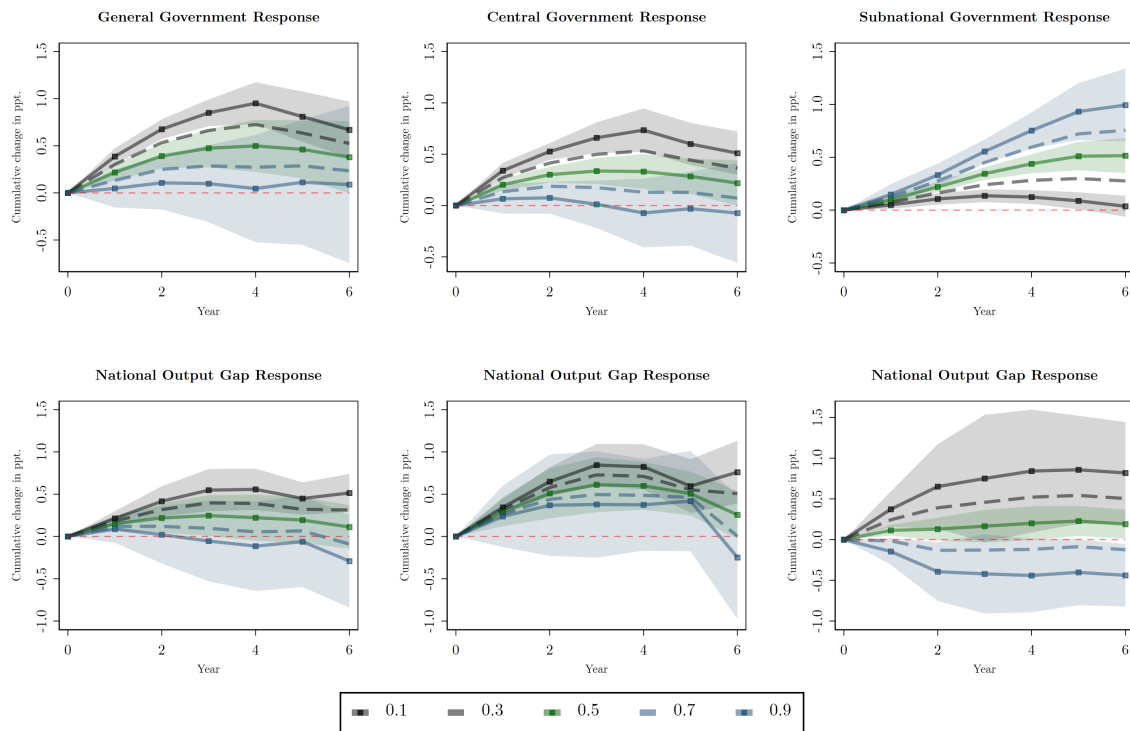
Figure 3.11 – Kernel densities of different continuous decentralization measures

Notes: The dotted gray lines show the separate indicators, while blue solid lines show the two combined measures. The RAI measure is normalized by the maximum to create a distribution bounded by 0 and 1.

There are visually prominent differences in the variances of the quantitative and the normalized qualitative measures. The density plots demonstrate the key disadvantage of the RAI: it highly overstates the level of decentralization for economies where the authority might be relatively high however the share of expenditures is relatively low. This issue persists for the naive measure, providing further support for the overweight of the RAI. The new combined measure of fiscal decentralization can be again used the estimate relative effects of output and discretionary spending shocks. Impulse responses are plotted in Figure 3.12.

I begin by interpreting the findings for the central government, which are presented in the middle panel. Discretionary responses decrease drastically with the level of fiscal decentralization. This finding provides empirical support to the theoretical claim that advanced fiscal federalism constrains the availability of central discretionary measures. This argument is supported by the finding that the output gap response to a change in central spending does not significantly depend on the level of decentralization. A different picture emerges when focusing on the subnational governments in the right panel. Here, we see that subnational governments increase discretionary spending more, the larger the level of decentralization. Hence, more fiscal decentralization does not only reallocate funds to the subnational government but these funds are also activated during economic downturns. Hence, the central and subnational government responses are inversely related.²³ Turning to the output responses, the level of fiscal decentraliza-

²³ Although this is partially due to the mechanical composition of available funds, it is important to keep in mind, that these are changes in discretionary responses which do not have to be positively correlated with overall expenditures.

Figure 3.12 – Cumulative responses using a combined measure of fiscal decentralization

Notes: The upper graphs show cumulative response of changes in the discretionary spending to a 1 ppt drop in the output gap ratio. The lower graphs depict the cumulative impulse response of the output gap ratio to a 1 ppt change in discretionary spending growth of the respective level of government. Shaded areas represent the 90% confidence intervals respectively.

tion seems to matter a lot. While increases in subnational discretionary spending in centralized economies have a positive effect on the relative output gap, these effects decrease and turn negative as the level of decentralization rises. Hence, while I do not find empirical support for offsetting discretionary policies, I find that even if the direction of subnational discretionary expenditures aligns with the central government, it has opposing effects on the national economy. This suggests that subnational governments focus on discretionary measures that may locally benefit the economy, but not globally.

3.5 Conclusion

This paper empirically investigates the implications of high levels of fiscal decentralization for macroeconomic stabilization. The analysis can be seen as a long overdue data-driven analysis of the theoretical predictions by Prud'Homme [1995] and Tanzi [1995]. I formalize these predictions in a tractable framework to flesh out several first-order interactions between decentralization and discretionary fiscal policies. On the one hand, decentralized spending, including discretionary measures, may enhance a country's stabilization toolkit through a higher fiscal

multiplier [[Brueckner, Pappa and Valentinyi, 2022](#)]. On the other hand, particularly high levels of decentralization may negatively affect the availability and effectiveness of discretionary fiscal policy.

This paper suggests that the negative implications can outweigh the increases in the fiscal multiplier associated with fiscal decentralization. The findings emerge from a two-step empirical approach using a dataset covering 37 countries from 1990 to 2021. First, the cyclically-adjusted primary budget method is employed to isolate discretionary fiscal responses across multiple levels of government. Second, panel local projections are used to estimate dynamic effects of changes in expenditures and the output gap. This methodological approach deliberately focuses on discretionary policy actions rather than automatic stabilizers such as standard unemployment insurance programs, which operate through predetermined formulas rather than active policy decisions. The discretionary measures examined here represent deliberate fiscal actions, such as changes to benefit levels or eligibility criteria, rather than the automatic functioning of existing programs.

Contrary to [Prud'Homme \[1995\]](#)'s prediction of an agency problem where subnational governments would remain passive during economic downturns, my research demonstrates that subnational governments in highly decentralized economies actively respond to widening national output gaps. This finding challenges the theoretical assumption that local authorities would free-ride on central government stabilization efforts. However, while subnational governments do act, their interventions often produce the adverse macroeconomic stabilization outcomes predicted by the theory, though through different mechanisms than originally anticipated. First, I find that discretionary responses of the central government decrease with the level of fiscal decentralization. This suggests constraints to the availability of central budgets to close the output gap. Second, the effectiveness of subnational discretionary measures decreases in decentralization, suggesting that central discretionary policies are more suited for closing the national output gap. If subnational governments have substantial fiscal authority, they are allowed to divert from centralized measures. I find that in such cases, these subnational policies may not only not assist the central government in closing the output gap, but proactively hinder it. A potential explanation for this is a lack of vertical and horizontal coordination in highly decentralized economies.

These results significantly contribute to the theoretical discourse on fiscal federalism. While [Qian and Roland \[1998\]](#) highlights how fiscal competition among local governments can re-

strain central budgets, particularly with subnational governments that are highly dependent on transfers, my findings indicate that both vertical and horizontal coordination failures in stabilization efforts may be equally consequential. This extends the theoretical framework regarding the potential limitations of extensive fiscal decentralization. Beyond extending the fiscal federalism literature, this research contributes to our understanding of discretionary fiscal policy effectiveness in contemporary macroeconomic environments. It further advances the methodological approach to measuring and analyzing fiscal decentralization through a novel combined measure that integrates both quantitative and qualitative dimensions of subnational fiscal authority.

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CHAPTER 3:
APPENDIX

A.3 Tables and Figures

Table A.3.1 – Decentralization Measures

<i>Dimension</i>	<i>Name</i>	<i>Description</i>
Fiscal Decentralization	Revenue Rate ¹	$\frac{\text{SNG own revenue} + \text{CG transfers and grants}}{\text{CG revenue}}$
	Tax Autonomy ²	Stegarescu [2004]
	Expenditure Rate ³	$\frac{\text{SNG expenditures (self + transfer financed)}}{\text{CG expenditures}}$
	Vertical Fiscal Imbalance	$\frac{\text{transfer financed SNG expenditures}^4}{\text{SNG expenditures (self + transfer financed)}}$
	Composite Ratio	$\frac{\text{Revenue Ratio}}{1 - \text{Expenditure Ratio}}$
	Grant Ratio	1 - Vertical Fiscal Imbalance
	R-Scale	$\frac{\text{CG revenue} + \frac{\text{SNG own revenue}}{\text{Number of SNG}}}{\text{CG} + \text{SNG revenue}}$
	E-Scale	$\frac{\text{CG expenditures} + \frac{\text{SNG own expenditures}}{\text{Number of SNG}}}{\text{CG} + \text{SNG expenditures}}$
	Regional Authority Index	Authority exercised by regional governments
Political Decentralization⁵	Political Autonomy	Existence of contiguous autonomous regions
	Locally Elected	Local election of SNG authorities
	Fiscal Authority	SNG authority over taxing/spending
	Upper-House Representation	Constituencies of upper-house are SNG
	Electoral Cycle	Synchronously running CG and SNG elections
Administrative Decentralization	Vertical Decentralization	Number of government tiers
	Employment	Share of SNG personnel
Geographic Decentralization	Territorial Fragmentation	Number and Size of SNG
	Jurisdictional Remoteness	Spatial expansion of SNG and distance to CG
Constitutional Decentralization	Federalism Indicator	Elazar [1995] ⁶
	Constitutional Authority	Statutory warranty over residual authority
	Weak Autonomy	Minimum federalism requirement ⁷
	Override	legal inability to override SNG authority

¹Alternatively, we have Revenue Ratio = $\frac{\text{SNG own revenue} + \text{CG transfers and grants}}{\text{CG revenue} + \text{SNG own revenue} + \text{CG transfers and grants}}$

²Multiple measures tracking the degree of revenue autonomy by accounting for autonomous own taxes.

³Alternatively, we have Expenditure Ratio = $\frac{\text{SNG expenditures (self + transfer financed)}}{\text{SNG expenditures (self + transfer financed)} + \text{CG expenditures}}$

⁴Locally administrated expenditures.

⁵Dummy variables, see Blume and Voigt [2011]

⁶Based on the definition of federalism by Riker [1964].

⁷Whether SNG have exclusive right to legislate at least one policy.

Table A.3.2 – Country averages of some fiscal decentralization measures

<i>Country</i>	$G_{i,\cdot}^s/\bar{G}_{i,\cdot} = \alpha_{i,\cdot}$	$RAI_{i,\cdot}$	$R_{i,\cdot}^s/\bar{R}_{i,\cdot}$	<i>Country</i>	$G_{i,\cdot}^s/\bar{G}_{i,\cdot} = \alpha_{i,\cdot}$	$RAI_{i,\cdot}$	$R_{i,\cdot}^s/\bar{R}_{i,\cdot}$
Australia	46.67%	24.00	49.21%	Kyrgys Republic	13.53%	-	13.95%
Austria	32.08%	22.15	36.11%	Latvia	27.54%	1.60	28.02%
Azerbaijan	3.12%	-	1.85%	Lesotho	8.73%	-	1.53%
Bahamas	0.00%	0	0.00%	Lithuania	25.45%	2.63	25.76%
Bangladesh	-	8.70	-	Luxembourg	11.33%	0.00	12.84%
Belarus	44.72%	-	46.46%	Malta	1.28%	0	1.67%
Belgium	32.29%	29.00	34.08%	Mauritius	5.55%	-	6.12%
Belize	0.00%	0	0.00%	Mexico	44.83%	16.67	32.06%
Bolivia	24.81%	6.66	37.60%	Mongolia	29.40%	3.67	27.75%
Bosnia & Herzegovina	14.90%	30.09	16.37%	Myanmar	8.67%	5.10	14.17%
Brazil	46.70%	18.13	51.86%	Nepal	39.97%	6.83	54.04%
Bulgaria	21.10%	1.42	19.68%	Netherlands	31.15%	17.11	33.00%
Cambodia	12.18%	3.05	12.56%	New Zealand	9.70%	8.46	10.65%
Canada	71.78%	28.01	72.35%	North Macedonia	13.16%	2.66	16.45%
Chile	13.29%	2.80	9.52%	Norway	34.54%	10.13	30.05%
Colombia	4.85%	0.71	7.15%	Palau	9.12%	-	11.88%
Croatia	27.30%	8.84	30.34%	Paraguay	8.96%	3.75	10.09%
Cyprus	3.90%	1.00	5.49%	Peru	26.05%	11.71	32.97%
Czech Republic	28.62%	8.31	30.38%	Poland	28.07%	8.78	31.11%
Denmark	59.96%	11.16	60.26%	Portugal	11.52%	5.31	14.09%
Estonia	25.08%	0.00	24.90%	Romania	19.40%	9.04	23.02%
Finland	40.86%	4.53	41.62%	Rwanda	22.48%	-	19.91%
France	18.18%	16.26	19.79%	Senegal	2.88%	-	3.44%
Georgia	20.22%	-	25.65%	Slovakia	13.35%	6.69	14.51%
Germany	45.13%	36.20	45.55%	Slovenia	16.50%	1.93	19.34%
Greece	6.78%	8.03	8.87%	South Africa	39.72%	-	45.56%
Honduras	7.43%	1.00	11.31%	Spain	38.33%	22.97	42.03%
Hungary	22.71%	9.39	25.07%	Sweden	43.00%	14.11	44.17%
Iceland	22.69%	0.00	23.67%	Switzerland	60.37%	24.96	61.74%
Ireland	22.67%	3.62	23.67%	Tajikistan	29.49%	-	27.13%
Israel	11.16%	2.00	13.66%	Thailand	11.16%	4.58	17.29%
Italy	28.67%	19.84	28.29%	Turkey	9.37%	8.04	11.40%
Japan	40.65%	13.30	47.97%	United Kingdom	26.81%	9.21	29.44%
Jordan	4.49%	-	7.06%	United States	48.86%	29.65	54.51%

Table A.3.3 – Summary statistics

	<i>All</i>		$F^H = 1$		$F^L = 1$		$F^C = 1$	
	<i>Mean</i>	<i>Stand. Dev.</i>	<i>Mean</i>	<i>Stand. Dev.</i>	<i>Mean</i>	<i>Stand. Dev.</i>	<i>Mean</i>	<i>Stand. Dev.</i>
Δ Discr. general spend.: $\tilde{\mathcal{G}}_{i,t}$	0.020	0.036	0.014	0.024	0.015	0.024	0.025	0.045
Δ Discr. central spend.: $\mathcal{G}_{i,t}^c$	0.012	0.025	0.006	0.032	0.010	0.015	0.017	0.031
Δ Discr. subnat. spend.: $\mathcal{G}_{i,t}^s$	0.006	0.021	0.007	0.032	0.006	0.018	0.005	0.013
Relative output gap: $\mathcal{Y}_{i,t}$	-0.359	3.268	-0.407	2.364	-0.109	1.996	-0.437	4.043
Δ SR rate	-0.329	2.378	-0.136	1.806	-0.290	2.933	-0.490	2.420
Euro Area	0.158	0.365	0.200	0.400	0.097	0.296	0.163	0.369
Unemployment rate	7.349	4.175	7.300	4.285	5.161	2.722	8.510	4.265
Δ GDP per capita	0.022	0.030	0.017	0.019	0.019	0.022	0.0279	0.036
Subnational autonomy	0.235	0.424	0.882	0.323	0.143	0.350	0.070	0.256
Federal	0.285	0.452	0.882	0.323	0.143	0.350	0.000	0.000
Tiers of government	3.391	0.589	3.764	0.425	3.143	0.350	3.290	0.659
Regional Authority Index	12.987	10.247	25.717	7.054	14.107	4.199	4.854	4.183
Expenditure ratio	0.314	0.161	0.444	0.148	0.395	0.131	0.211	0.096
Number of countries	37		9		7		21	

Notes: The table summarizes important key parameters for all groups according to the main parameterization. The first segment presents the averages of the respective dependent variables. The second section summarizes the control variables across groups. Note that all variables are presented as they enter the model with the exception of the unemployment rate. The last segment compares several factors of fiscal decentralization.

Figure A.3.1 – Transfer dependence according to OECD measures

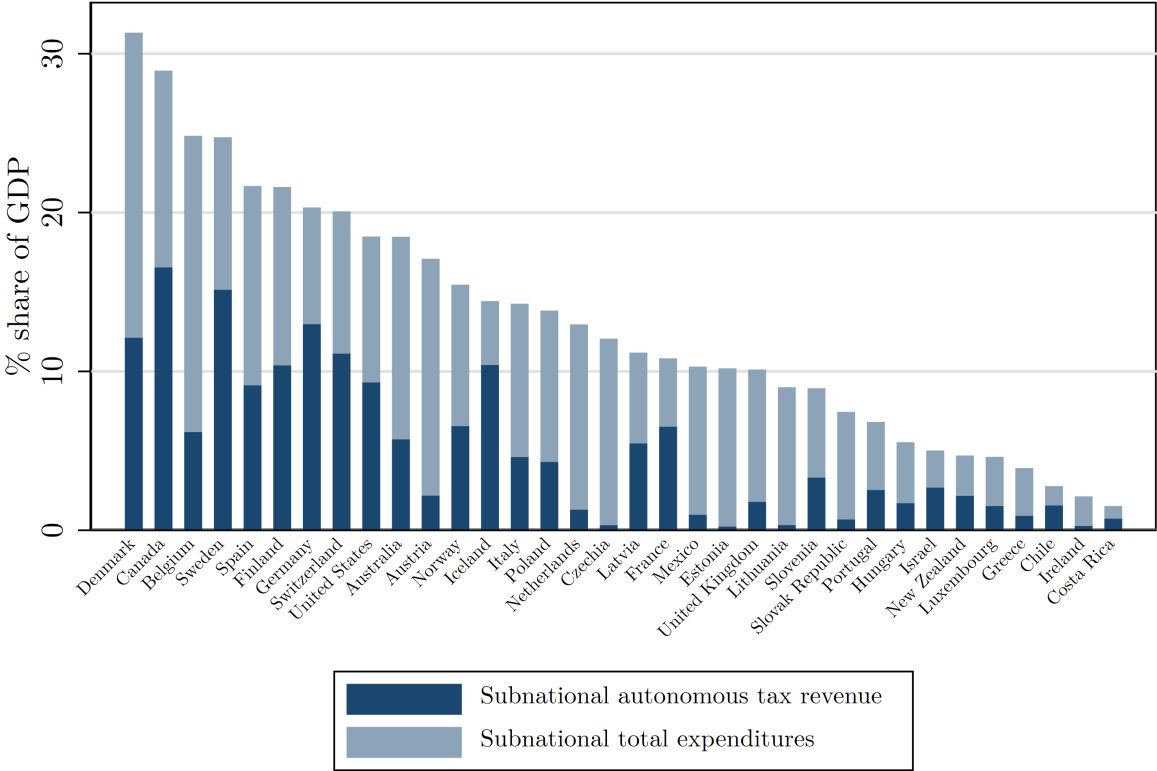


Figure A.3.2 – Organization of different types of governmental decentralization

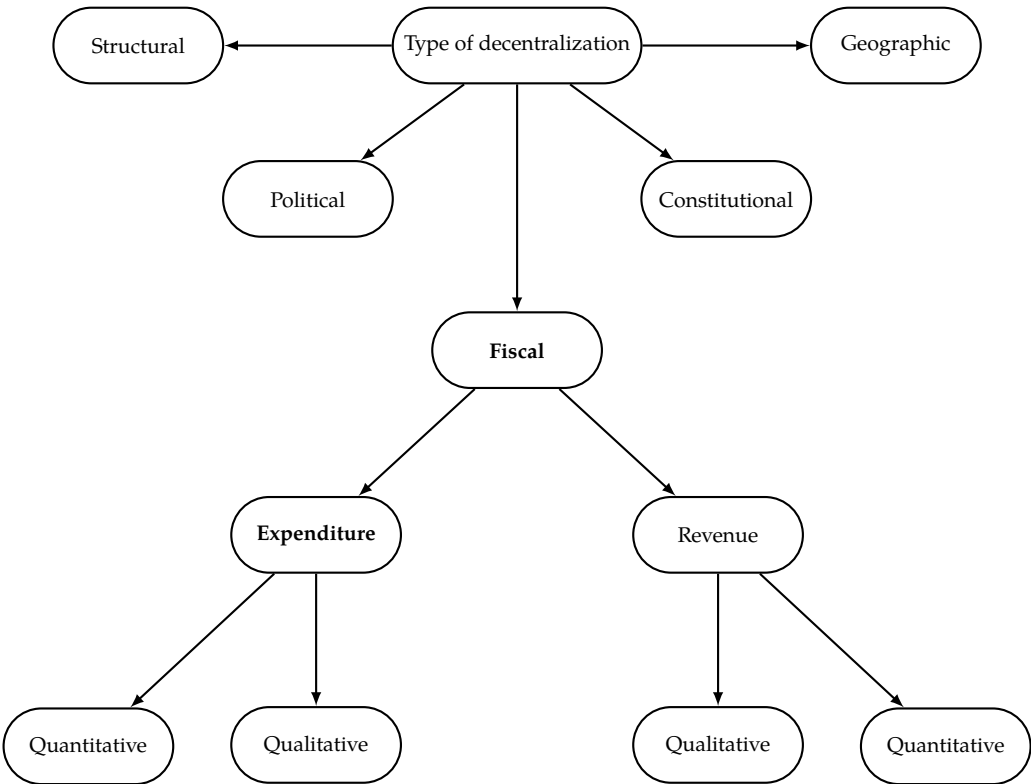
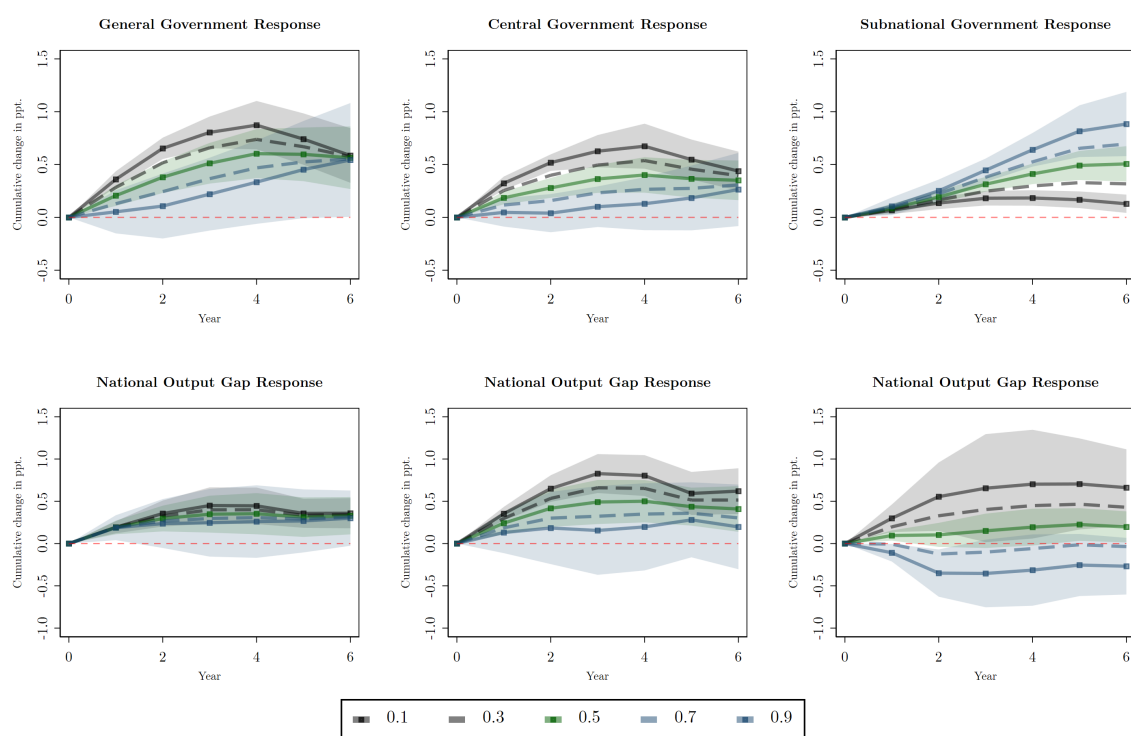


Figure A.3.4 – Cumulative responses using the geometric mean as a measure of fiscal decentralization

Notes: The upper graphs show cumulative response of changes in the discretionary spending to a 1 ppt drop in the output gap ratio. The lower graphs depict the cumulative impulse response of the output gap ratio to a 1 ppt change in discretionary spending growth of the respective level of government. Shaded areas represent the 90% confidence intervals respectively.

B.3 Technical

The full system of equations for the local projection estimation is

$$\left\{ \begin{array}{lcl} \Delta \mathcal{G}_{i,t+0} & = & \beta^{(0)}(-\mathcal{Y}_{i,t-1}) + \delta_i^{(0)} + t + \sum_{p=1}^P \left[\phi^{(0)} \Delta \mathcal{G}_{i,t-(p+1)} + \Gamma_p^{(0)} \mathbf{X}_{i,t-p} \right] + \varepsilon_{i,t+0} \\ \Delta \mathcal{Y}_{i,t+0} & = & \eta^{(0)} \mathcal{G}_{i,t} + \delta_i^{(0)} + t + \sum_{p=1}^P \left[\lambda^{(0)} \Delta \mathcal{Y}_{i,t-(p+1)} + \Xi_p^{(0)} \mathbf{X}_{i,t-p} \right] + \varepsilon_{i,t+0} \\ \Delta \mathcal{G}_{i,t+1} & = & \beta^{(1)}(-\mathcal{Y}_{i,t-1}) + \delta_i^{(1)} + t + \sum_{p=1}^P \left[\phi^{(1)} \Delta \mathcal{G}_{i,t-(p+1)} + \Gamma_p^{(1)} \mathbf{X}_{i,t-p} \right] + \varepsilon_{i,t+1} \\ \Delta \mathcal{Y}_{i,t+1} & = & \eta^{(1)} \mathcal{G}_{i,t} + \delta_i^{(1)} + t + \sum_{p=1}^P \left[\lambda^{(1)} \Delta \mathcal{Y}_{i,t-(p+1)} + \Xi_p^{(1)} \mathbf{X}_{i,t-p} \right] + \varepsilon_{i,t+1} \\ & \vdots & \\ \Delta \mathcal{G}_{i,t+H-1} & = & \beta^{(H-1)}(-\mathcal{Y}_{i,t-1}) + \delta_i^{(H-1)} + t \\ & & + \sum_{p=1}^P \left[\phi^{(H-1)} \Delta \mathcal{G}_{i,t-(p+1)} + \Gamma_p^{(H-1)} \mathbf{X}_{i,t-p} \right] + \varepsilon_{i,t+H-1} \\ \Delta \mathcal{Y}_{i,t+H-1} & = & \eta^{(H-1)} \mathcal{G}_{i,t} + \delta_i^{(H-1)} + t \\ & & + \sum_{p=1}^P \left[\lambda^{(H-1)} \Delta \mathcal{Y}_{i,t-(p+1)} + \Xi_p^{(H-1)} \mathbf{X}_{i,t-p} \right] + \varepsilon_{i,t+H-1} \end{array} \right. \quad (\text{B.3.1})$$

where the first equation is identical to equation 3.8 of the main specification.