UNIVERSITY OF CALIFORNIA
SANTA CRUZ

ESSAYS IN APPLIED MICROECONOMICS

A dissertation submitted in partial satisfaction of the
requirements for the degree of

DOCTOR OF PHILOSOPHY

in

ECONOMICS

by

Jiayi Xu

June 2021

The Dissertation of Jiayi Xu
is approved:

Professor George Bulman, Co-Chair

Professor Justin Marion, Co-Chair

Professor Jonathan Robinson

Quentin Williams
Interim Vice Provost and Dean of Graduate Studies

Contents

List of Figures

List of Tables

Abstract

Essays in Applied Microeconomics

by

Jiayi Xu

This dissertations studies topics related to public economics, public policy, and financial economics. The first chapter examines how shifting the tax burden between residential and business property affects local government spending, business activities, and residential mobility. I exploit shifts in the tax burden stemming from staggered reassessment cycles. The infrequency of reassessments results in sudden and often substantial shifts in the property tax burden, while the differential timing of reassessments across towns allows for credible causal identification. The analysis reveals that local governments are not sensitive to revenue source, as shifting the tax burden between residents and businesses does not affect revenue levels or the nature of public expenditures. However, I find that higher business tax burdens reduce the number of small establishments in industries that have low profit margins, a result that is evident in data on local business counts, commercial vacancy rates, and foreclosures. Further, increased business property tax burdens appear to reduce employment levels and wages. In contrast, I do not find significant evidence of increased residential mobility. The analysis is informative for tax policy and contributes to the literatures on the residual versus strategic views of tax rate setting and the benefit versus capital views of tax incidence.

In Chapter 2, we provide new evidence of the "flypaper effect" in a context

of a budget shortfall generated by a targeted tax reduction. Specifically, we examine whether local government increases tax revenue from other sources, reduces expenditures that benefit the targeted group, and consider the net impact of these responses on income and economic productivity after China abolished its agricultural tax in 2004. Comparing nearly identical counties in adjacent provinces reveals that differences in revenue shortfall are not offset by increased taxes. However, local agriculture expenditure is disproportionately reduced, attenuating the benefits to farmers. Further, farmers in counties that experienced larger revenue shortfalls suffered losses of net income.

The final chapter investigates the impacts of firm scandals by scandal types and sectors. This chapter exploits a unique setting that generates a large set of systematically reported scandals, allowing us to measure investors' responses across scandal types. We document the recent 10-year scandals and use an event study approach to estimate the effects across product quality, personal information, and business practice scandals. We find that the effects are the largest for defective products, but there are also strong negative responses to personal information breaches. We find little stock response to the revelation of deceptive business practices. We also make comparisons across industry sectors and find that the consumer goods sector suffers the largest impacts, followed by the services sector, while the technology sector exhibits the smallest responses. These results shed light on how the market responds to various types of scandals and whether some industries are more vulnerable to negative effects.

to my husband, my parents and my grandparents

Acknowledgments

I wish to express my deepest appreciation to the people who have provided me with great help and support in completing my dissertation and the pursuit of my Doctoral Degree.

First and foremost, I would like to thank my dissertation committee members: Professor George Bulman, Professor Justin Marion, and Professor Jonathan Robinson. I want to thank my advisor Professor Bulman, who has spent countless hours advising my dissertation work. His patience, motivation, enthusiasm, and immense knowledge kept me constantly engaged with my research and inspired me to overcome difficulties during my Ph.D. studies. In addition, his deep commitment to caring for students' emotional and mental needs made me feel highly supportive during the entire journey. I would also like to thank my co-advisor, Professor Marion, who has expertly guided me through my research by carefully dissecting my research ideas and providing invaluable feedback. Besides, Professor Marion was very generous in offering me student researcher positions, co-authorship, and access to incredible data sets, helping me explore the diversity of the academic world. I am also thankful to Professor Robinson. His second-year course is what initially made me feel the power of economics. Both the course and his work set an excellent example for me by showing how economic research can benefit people and make a better world. I also feel incredibly grateful for Professor Robinson's advice on my research papers and support during my job search.

Furthermore, I would like to thank the other two members of my oral defense

committee, Professor Carlos Dobkin and Professor Jesse Cunha, for their time and valuable suggestions. I am also grateful for the advising and teaching I received from many other members of the economics department at UCSC: Ajay Shenoy, Kenneth Kletzer, Laura Giuliano, Natalia Lazzati, and all participants in the applied micro workshop and the job market practice talks. Besides, I want to express my special thanks to Sandra Reebie for her hard work and kind assistance. I am also thankful to Professor Veronica Terriquez from the Sociology Department, who provided me great opportunities to apply my economic skills to real-world problems.

Friendship is another important part that made my time at Santa Cruz become colorful and unforgettable. I want to thank my lovely cohort of Zijing Zhu, David Zink, Luka Kocic, Fernando Chertman, Ruizhi Zhang, Yifei Sheng, and Andrew Barber for all the time that we worked together. I also want to thank Jijian Fan and Duan Zhang for their support and encouragement throughout the past six years.

Last but not least, I would like to thank my family for their unconditional love and encouragement throughout my life. To my parents — thank you for always believing in me and never giving up. To my grandparents — R.I.P, and I will never let you down. To Jianfeng — thank you for all the hugs and companionship. Without you, this would not have been possible. Thank you for being here and brightening my life.

Earning a Ph.D.was challenging, and the six years were long. There were tears, complaints, and frustrations, but there were also laughs, praise, and applause. This six-year journey helped me grow so much, as the proverb says, “what does not kill you makes you stronger”. I look forward to starting the next fantastic chapter of my life.

Chapter 1

Property Taxes and Government Expenditures, Business Activities, and Residential Mobility

1.1 Introduction

Property tax is one of the most important revenue sources for local governments in the United States, and the majority of this revenue is generated from residential and business property. There has been a long debate regarding property tax incidence that is dominated by two competing views: the "benefit tax" view and the "capital tax" view (Zodrow, 2001). The benefit tax view considers property tax as a payment for public goods received (Tiebout, 1956), while the capital tax view considers it a tax on the use of capital which can drive investment out of high tax jurisdictions and can reduce wages and increase rents (Mieszkowski, 1972; Zodrow and Mieszkowski, 1986). Understanding the impact of shifting the tax burden between residential and business property is important for policy. For example, in the 2020 election, California voted on a law that would substantially shift the tax burden toward businesses by eliminating the artificially low assessed values created by Proposition 13.

To shed light on the implications of shifting the property tax burden between residential and business property, I examine: 1) whether local government revenue levels and spending patterns are sensitive to the source of property tax revenue (business or residential); 2) the impact of shifting the tax burden towards businesses on whether they move or close, their employment levels, and the wages they pay; and 3) the impact of shifting the tax burden towards residents on housing vacancies, sales, and foreclosures.

There are several challenges to identifying the causal effect of shifting the tax burden on local government, business, and residential outcomes. First, the tax burdens

faced by business and residential properties are endogenously determined by political factors and the local economic environments including the size and influence of the local business community. Second, tax levels affect the provision of public services that can alter business and residential outcomes. To overcome these issues, this paper exploits shifts in the tax burden between residential and business properties stemming from staggered reassessment cycles. The infrequency of reassessments results in sudden and often substantial shifts in the property tax burden, while the differential timing across towns allows for credible causal identification. I first implement a staggered difference-in-differences design to study whether local governments are sensitive to changes in the level and composition of the tax base. If shifting the tax burden between business and residential property does not affect total revenue and categorical expenditures, then it is possible to abstract from the role of public goods provision and isolate the pure tax burden effects on business and residential outcomes. For businesses, I examine the effect of property taxes on business exit, employment levels, and wage payments. For residents, I examine how the tax burden affects housing vacancies, sales, and foreclosures.

In the U.S., states require local governments to reassess properties on a fixed schedule every few years.[1] In non-reassessment years, the assessed values are essentially unchanged even if local economies experience dramatic changes in property values. As a result, during reassessment years, revised property values reflect cumulative market changes for the past several years. In addition, differences in business and residential

[1]For example, Ohio requires its counties to update property values every three years, and North Carolina asks its counties to conduct reassessments at least every eight years. There are five states that do not have specific reassessment cycles. They are CA, DE, NJ, NY, and PA.

property growth patterns and assessment methods can create a wedge between their values, and thus, the infrequent nature of reassessments can cause large changes in the relative tax burden of residential and business property.[2] This paper focuses on reassessments in Connecticut, which has a five-year reassessment cycle that results in large cumulative property value changes and shifts in relative tax burdens.

This paper explores how the tax burden shifts between business and residential properties affect government, business and residential outcomes. My finding shows government fiscal decisions do not vary with changes in property tax base. For businesses, I find that higher business tax burdens reduce the number of establishments in industries that traditionally have low profit margins, and it is also evident in data on local business counts, commercial vacancy rates, and foreclosures. Further, increased business property tax burdens appear to generate adverse effects on labor markets. In contrast, I do not find significant evidence of increased residential vacancies, sales, or foreclosures.

This study incorporates multiple data sources. For government outcomes, I combine administrative financial data with property assessment information, and manually collect categorical public expenditure data such as highway and transportation spending from municipal annual audit reports. To understand impacts on business activities, I gather information on establishment numbers from the U.S. County Business Patterns database, vacancy counts from the USPS Vacant Address database, and

[2] In general, residential assessments rely on a sale approach combined with a cost approach, while an income approach is often used for businesses. The differences in assessment methods will be explained in detail in section 3.2.

business property transactions from Zillow ZTRAX. For labor market outcomes, I use aggregate wage payments and employment information from the Connecticut Department of Labor. For residential mobility, I supplement USPS vacancy and Zillow ZTRAX data with vacancy information from the American Community Survey.

Specifically, the analysis generates five primary findings. First, there is no evidence that changes in the size of the property tax base or tax base composition have an effect on local revenue levels. When local property tax bases fluctuate substantially due to reassessment, one might expect significant changes in property tax revenues. However, I find that local governments adjust tax rates such that revenues are largely unaffected. While this may be surprising, it is consistent with the literature, including Lutz et al. (2011) and Alm et al. (2011), who find no relationship between assessed property values and local property tax revenue following the Great Recession. The finding also provides new evidence for the tax rate setting residual view, which argues that when the tax base falls (rises), local governments increase (decrease) the tax rate to stabilize revenue (Netzer, 1964; Ross and Yan, 2012). Having established that the tax base has little effect on local revenue, I then test whether changes in tax base composition (business versus residential) affect local revenue, and find no significant evidence that it does. That is, there is no evidence that local governments attempt to extract more revenue when a greater share of the tax burden falls on businesses.

Second, local public spending does not appear to respond to tax source changes. In particular, there is no evidence that having a larger share of taxes coming from businesses results in greater expenditures on highway and transportation and public

safety.[3] There is also no evidence that higher residential tax shares increase education spending. This result contributes to the literature on the role of revenue source on spending by distinguishing based on taxpayer identity.[4] Previous studies primarily focus on the importance of tax rates for political activities (Bordignon et al., 2003; Vermeir and Heyndels, 2006), but few have focused on the composition of the tax base. The finding that paying higher tax shares does not translate to higher public benefits is not consistent with the property tax benefit view. Further, because local governments do not appear to adjust public spending when the tax revenue source changes, I can abstract from public goods provision and isolate the pure tax effect on business and residential outcomes.

Third, higher business property taxes increase local business exit. I find that higher taxes reduce the number of local business in industries with low profit margins, which is consistent with the capital view. In the manufacturing industry, for example, there is a reduction in local small businesses (with fewer than ten employees). Specifically, a one percent increase in business tax burden leads to a decrease of 0.39% in total small business counts three years after a tax increase. Adverse effects are also observed in the retail industry. Another measure of business exit is vacancy, which is caused by firms vacating their properties. I find higher business tax burdens increase the volume of long-term (i.e., over six months) vacant business properties. By isolating the role of

[3] A recent U.S. Chamber of Commerce survey suggests businesses are more demanding for these public spending.

[4] Previous studies such as Martinez (2019) differentiate revenue sources by tax versus non-tax revenue. In addition, a series of public finance studies, such as Dahlberg et al. (2008) and Leduc and Wilson (2017), examine the impacts of local and intergovernmental revenues on local expenditures.

property taxes free from changes in public goods provision, these findings contribute to the literature on property taxation and business which has previously documented few negative effects (Bartik, 1985a; Gabe and Bell, 2004). In addition, as the property tax increases identified in this paper may be short-lived, while losing businesses (due to closing or relocation) could be long-lasting, this is of significant policy interest.[5]

Fourth, I find that firms pass the additional tax burden to employees by reducing wage payments and employment. A one percent increase in business tax burden results in a 0.06% to 0.08% reduction in annual aggregate wages, and a 0.02% to 0.05% reduction in employment levels. This finding is consistent with the capital tax view, which argues that part of the tax burden is borne by local workers through changes in their wages and employment. As most of the literature on tax incidence primarily focuses on state and federal corporate income taxes, this paper contributes by providing new evidence on the impact of local property taxes (Desai et al., 2007; Suárez Serrato and Zidar, 2016; Fuest et al., 2018).

Finally, I do not find significant evidence of increased residential vacancies, sales, or foreclosures in response to higher residential property taxes. These results are informative, as prior empirical evidence on the consequences of higher property taxes on residential mobility is inconclusive. For example, using tax changes driven by relative assessment growth and instrumented by statewide shifts in housing demand in Ohio, Fraenkel (2019) finds that higher property tax bills cause some homeowners

[5]Unlike corporate income tax reforms, which may be perceived by businesses as being permanent, the tax shocks examined by this paper are driven by periodic property reassessment, and thus, maybe perceived as short-term in nature.

to sell their properties, while Ding et al. (2020) show that higher tax bills caused by property reassessments in Philadelphia do not increase home sales among vulnerable homeowners.

The paper is organized as follows. Section 2 presents background information on property reassessments. Section 3 describes the construction of the data set. Section 4 introduces the tax shifting mechanism between business and residential properties. Section 5 presents the empirical strategy. Sections 6 to 10 report local government, business, and residential outcomes. Section 11 concludes.

1.2 Background: Property Reassessment

Reassessment is the process of reviewing the values of all properties to ensure uniformity and equality. Connecticut law require all 169 towns in the state to conduct a property reassessment every five years. By law, municipalities must assess property at 70% of its fair market value at 5-year intervals. Between reassessments, the assessed values stay constant unless there is damage to a property requiring complete demolition or total reconstruction or new construction completed on the property.[6] Figure 1.1 displays property value changes in reassessment and non-reassessment years. The left panel of the figure is a histogram of the percent change in year-to-year real property values for non-reassessment years, which are very small. Changes in reassessment years are shown in the right panel, and are substantial, reflecting cumulative real estate market variation for five years.

[6]See Connecticut CSG 12-62 and CGS § 12-62a (b).

Each year, a group of towns goes through the reassessment as scheduled by the state. Figure 1.2 shows that towns located in the same area face different reassessment schedules, revealing that assessment timing is not a function of geography. Therefore, if the property market experiences a price shock in a particular year, the changes will show up on tax bills in different years based on town-level reassessment cycles.[7]

In reassessment years, variation in assessment approaches can create wedges between business and residential property value changes. In general, there are three assessment approaches: the sales approach, the cost approach, and the income approach. Assessors consider each approach's reliability based on the type of property and the availability of market data. The most reliable method is given the greatest weight in calculating final values. In general, residential assessments rely on the sale approach combined with the cost approach, and the income approach is used only for businesses. The sales approach compares a property with similar properties recently sold in arm's length transactions. Differences in physical conditions, locations, and other aspects are adjusted according to assessors' judgments. It follows the idea that a property's real value can be reflected by the cost of purchasing an equally desirable substitute property. In applying the cost approach, an assessor first estimates the land's market value by analyzing comparable land sales. Next, the assessor calculates the cost of replacing the property at the time of reassessment, and this estimated replacement cost is depreciated using a property's current condition. Finally, the income approach estimates businesses' property market values such as retail, warehousing, offices, and industrial space. Unlike

[7]Municipalities cannot defer reassessments. Any town or city that fails to comply with the provisions forfeits ten percent of state grants-in-aid for the fiscal year following the required reassessment year.

the other two methods, it relies on the income a property can produce. It considers property as an investment, and the final value reflects what an informed investor would pay for the earning stream associated with a particular property. Typically, assessors gather information on the income and operating expenses from property owners. Then, the net income is converted to a present value using an appropriated capitalization rate.

Economic condition is another important factor that could lead to different value changes between business and residential properties. For instance, lower interest rates on business mortgages can boost urban development in downtown areas, thereby attracting more people and increasing apartment rents. As a result, it could cause commercial apartment buildings to appreciate faster than residential homes. Furthermore, business assessments heavily rely on the income approach, which considers the predictable income stream as a major factor, but residential housing assessment relies more on properties' characteristics.

Municipalities must notify property owners about reassessment results by sending written notice in November. The notice indicates the property's value before and after reassessment, states that the owner has the legal right to appeal the new assessment, and explains how they may do so.[8] For example, if 2020 is a reassessment year, property owners will receive notices of their new assessed values in November of 2020. People who disagree with the values can appeal before March of 2021.

The newly assessed values are used to prepare local budgets starting in July after the reassessment year.[9] The assessment information is submitted to the board of

[8]See Connecticut CGS § 12-62 (f).

[9]Local governments in Connecticut adopt a uniform fiscal year which begins July 1 and ends June

finance to determine property tax revenue for budget purpose after it is reviewed and approved by the tax commission. For example, the 2020 assessment values are used to prepare local budgets for the fiscal year starting in July of 2021 and ending in June of 2022. Between April and May of 2021, the board of finance will hold annual budget meetings and adopt the budget after the tax commission approves the final tax base. The property tax levy is then set, and tax bills are computed and mailed in time for payment to begin on July 1. In other words, the first tax bills impacted by the 2020 reassessment will be sent in June of 2021 and are due on July 1, 2021, which is also the start of the municipal fiscal year.

1.3 Data

The data used in this study comes from multiple novel sources. The property assessment data is obtained from the State of Connecticut Office of Policy and Management. Municipal financial data is manually collected from local annual audit reports. The County Business Patterns data set includes information on the number of establishments by industry and firm sizes, and labor outcomes are gathered from the Connecticut Department of Labor. Three more data sets, the USPS Vacant Address data, American Community Survey and Zillow ZTRAX provide information on property vacancies and transactions.

The State of Connecticut Office of Policy and Management maintains property assessment lists by property types for each of its 169 municipalities, and I use residential

30.

and business assessed values from this administrative data source. Local fiscal measures are gathered from the Municipal Fiscal Indicators data set, including data such as property tax revenues, intergovernmental revenues, and total local revenues. Detailed public spending data is scraped from local annual audit reports.[10] I use local spending on highway and transportation, public safety, and education to test whether changes in tax revenue sources affect public spending composition. The highway and transportation departments are typically in charge of road and bridge maintenance, highway signs, and parking lots; and public safety expenditure includes spending on police and fire departments.

To estimate the effect on business exit, I use the County Business Patterns database to determine the number of establishment in each town.[11] With this data, I can examine how businesses from various industries respond to property tax shocks differently. Another advantage is that it provides establishment counts by number of employees, enabling me to conduct heterogeneity analysis. In addition to business counts, I also use the number of vacant business addresses to measure business mobility and closures. I follow Immergluck (2016) and divide vacant addresses into short-term and long-term vacancies (i.e., less and over six months of vacancy). Short-term vacancies are most likely to be caused by owners' selling or leasing decisions, and long-term vacancies are likely to be a reasonable indicator for weak business demand. Vacancy

[10] Audit reports after 2011 are downloaded from Connecticut's Electronic Audit Reporting System (EARS), and earlier reports are collected from local financial departments and town clerk offices.

[11] The data comes from the U.S. Census Bureau's Business Register of U.S. companies, which includes all companies with paid employees, covering most North American Industry Classification System industries. Beginning in 2017, a data point is only published if it contains three or more establishments, and thus, this paper only uses establishment data before 2017.

data comes from the USPS Vacant Address database, and it includes information such as vacant address counts and the number of months an address has been vacant.[12] An advantage of using the USPS dataset is that it maintains vacant addresses for residential and business properties and is the only existing dataset with commercial and industrial vacancies. Previous studies such as Silverman et al. (2013) and Immergluck (2016) use the USPS data to study residential vacancies, and this paper extends the usage by focusing on business properties. I also use property transaction records from Zillow ZTRAX to examine business mobility and closures.[13] This paper examines two types of transactions: traditional sales and foreclosures. Traditional market sales are those with deed types that do not include transfers between families and inheritances. Foreclosures are defined broadly, including foreclosure deeds, deeds in lieu of foreclosure, and tax deeds.

Statistics on labor outcomes are obtained from the Connecticut Department of Labor. Connecticut has a Quarterly Census of Employment and Wages (QCEW) program serves as a near census of employment and wage information.[14] From this program, I observe annual average employment and annual aggregate wage payments by industry, which are reported by the employers on their Unemployment Insurance

[12]USPS counts vacant addresses in order to track undeliverable mail. If an address is vacant (not collecting their mail) for 90 days or longer, USPS delivery staff will label it as a vacant address. Vacant information is published at the census level by the U.S. Department of Housing and Urban Development, but I aggregate them into the town level for this study. In 2010, USPS began implementing new procedures to improve the vacant indicator's accuracy, and thus, I only use vacant data after 2010.

[13]Property level transaction records are sourced from legal recordings processed by each county recorder's offices. The transaction records are initially reported by parcel I.D. For this study, I aggregate them to town levels.

[14]The program produces a comprehensive tabulation of employment and wage information for workers covered by Connecticut Unemployment Insurance (UI) laws and Federal workers covered by the Unemployment Compensation for Federal Employees (UCFE) program.

Quarterly Contribution Returns.

For residential mobility, I use both USPS Vacant Address data and the Zillow ZTRAX data and supplement this with vacancy information from the ACS. An advantage of the ACS data is that it classifies vacant units as vacancy for rent, vacancy for sale, and seasonal vacancies. For this study, I focus on both vacancy for rent and vacancy for sale.

1.4 Business and Residential Tax Shifting

The infrequent nature of reassessments often results in substantial changes in tax base levels. For example, during the Great Recession, overall property values in Connecticut fell by over 12%. In addition, infrequent reassessments can cause large changes in the relative tax burdens of business and residential property. In this section, I explain how the tax shifting between business and residential properties affects revenue sources from the government perspective and tax burdens from the perspective of business and residents.

Property tax is determined by two components: property assessed values (V) and millage rate (m). The assessed value is the basis upon which the property tax levy is distributed among the property owners. The millage rate is the number of dollars of tax assessed for each \$1,000 of assessed values. The taxes paid by a property owner will be $T = V \times m$. For example, a rate of 20 mills results in a tax payment of \$20 for each \$1,000 assessed value. Residential and business properties are the two primary

sources of property tax revenues, and the tax paid by business is $T^B = V^B \times m$, with residential taxes similarly defined. As there is only one millage rate in Connecticut, applying to both businesses and residences, the share of tax paid by businesses is a function of assessed values, $s^B = \frac{V^B}{V^{tot}}$. Therefore, relative changes in business and residential assessed values will shift the property tax shares between them.

Figure 1.3 shows how residential and businesses values changed differently when reassessment happened after the Great Recession. As we can see from the figure, although both property types experienced depreciation, residential property experienced much larger decreases during this period. On average, residential values dropped by 12.28% and business values by 2.23%.

1.4.1 Changes in Government Tax Revenue Sources

From the government's perspectives, changes in business and residential relative assessed values reflect shifts in tax revenue sources as shown in equation 1.1:

$$\Delta s_t^B = (\frac{V_t^B}{V_t^{tot}} - \frac{V_{t-1}^B}{V_{t-1}^{tot}}) \tag{1.1}$$

Δs_t^B represents the percentage point changes in business tax shares. For example, if s_t^B is 0.15 and s_{t-1}^B is 0.1, then $\Delta s_t^B = 0.05$ suggests that a 5 percentage point tax revenue shift towards businesses from $t-1$ to t and also implies a 5 percentage point revenue shift away from residential properties.

Figure 1.4 shows the shift in property tax revenue sources caused by reassessments in the five years after the Great Recession. Year 0 is the scheduled reassessment

year, including 2009, 2010, 2011, 2012, and 2013. The top graph suggests that the tax revenue share on businesses increased by an average of 4 percentage points (on a base of 21 percentage point), which is accompanied by a commensurate drop for residential property. The magnitude of the shifts vary substantially by town. For example, the average change in business revenue share is over 8 percentage points for the highest 20% of towns, while the increase is less then 1 percentage point for the bottom 20%.

1.4.2 Changes in Business and Residential Taxes

From perspective of a business, the tax shifting from residents to businesses can lead to much large percentage changes in their tax burdens. For example, if a 5 percentage point tax burden shifts to businesses between $t-1$ and t on a base of 10 percentage points (i.e., $s^B_{t-1} = 0.1$ and $s^B_t = 0.15$), then it will result in a 50% increase in their tax burdens as expressed in equation 1.2:

$$\%\Delta s^B_t = \left(\frac{V^B_t}{V^B_{t-1}} \times \frac{V^{tot}_t}{V^{tot}_{t-1}} - 1\right) \tag{1.2}$$

The measure $\%\Delta s^B_t$ represents the percent change in business tax burden between $t-1$ and t. Specifically, this captures the change in the assessed value of businesses relative to the overall change in the tax base. Because assessed values stay constant between the 5-year intervals for reassessment, $\%\Delta s^B_t$ will approximate to zero if t is a non-reassessment year. In a reassessment year, the expression captures the percentage changes in business tax burden due to reassessment timing. For example, after the Great Recession, the average percentage change on business tax burden in a non-reassessment

year is 0.19%, which is statistically insignificant from zero. The average tax burden changes on businesses in a reassessment year during the same period is 7.6%. Figure 1.5 displays the distribution of these changes.

The size of the percentage change in business tax burden can vary substantially across towns. The variation depends in part on residential property share and in part on changes in assessed values. Equation 1.3 rewrites the $\%\Delta s_t^B$ to show the determinants of the shock size:

$$\%\Delta s_t^B \approx log(\frac{s_t^B}{s_{t-1}^B}) \approx s_{t-1}^R(1 - \frac{g_t^R}{g_t^B}) \tag{1.3}$$

where $g_t^B = \frac{V_t^B}{V_{t-1}^B}$ and $g_t^R = \frac{V_t^R}{V_{t-1}^R}$.[15] The percentage change in the share that businesses pay is approximately equal to the residential share in the town multiplied by one minus the relative growth rates in assessed values. For example, if a town experienced a 10 percent decline in residential property values and no change in business property values ($g^R = 0.9$ and $g^B = 1$), then $\%\Delta s_t^B$ would be around 8 percent if residential properties were 80 percent of the property tax base but only 2 percent if residential properties were 20 percent of the base. On average, residential properties contribute to approximately 80% of real property tax bases, while businesses make up about 20%. The wide range of shocks across towns in evident in Figure 1.5, with many towns experiencing business tax increase exceeding 15 percent.

Similar to businesses, tax shifting also impacts residents. I create the percentage change on residential tax burden between year t and $t-1$ in the same manner as

[15] More details can be found in Appendix 1.A.

in equation 1.4:

$$\%\Delta s_t^R = (\frac{V_t^R}{V_{t-1}^R} \times \frac{V_t^{tot}}{V_{t-1}^{tot}} - 1) \tag{1.4}$$

The measure $\%\Delta s_t^R$ represents the percent change in the residential tax burden between $t-1$ and t. For instance, after the Great Recession, the percentage change in residential tax burden is -0.24% during non-reassessment years, and is -4.9% during reassessment years.

1.5 Empirical Design

The endogeneity of property taxes is a primary challenge in the literature. Recent studies try to solve it in different ways, such as exploiting spatial variation in tax break policies (Mast, 2020), focusing on homeowners with low demand for public services such as retirees (Shan, 2010), or studying the spillover effects of school levies on businesses (Enami et al., 2018). This paper exploits tax burden shifts between business and residential properties due to differential reassessment timing across towns. I introduce the staggered reassessment cycles in section 1.5.1, and then discuss the empirical designs for government, business and residential outcomes in sections 1.5.2 to 1.5.5.

1.5.1 Staggered Difference-in-Differences Design

Each year, a group of towns will carry out their reassessments. Figure 1.2 shows the geographic distribution of towns based on their reassessment cycles, reveal-

ing that assessment timing is not correlated with locations. The staggered difference-in-differences identification strategy is valid under the assumption that the timing of reassessment across towns is not correlated with economic factors. That is, we have two towns with the same business and residential tax shares and that experience the same local economic trends, so the only difference between them is their reassessment schedule: one will be reassessed earlier and the other will be reassessed later.

Consider two towns, A and B, that are similar but have different reassessment schedules. During the Great Recession, they experience similar shocks. However, the shocks to assessed values and taxes are only realized afterwards in different years based on their reassessment schedules. If the reassessment year for town A is 2011, then the value changes caused by the Great Recession in town A will be reflected on 2011 tax bills and paid in 2012. If the reassessment year for town B is 2013, then the changes would be reflected on town B's 2013 tax bills and paid in 2014. Both changes reflect the lagged fluctuations of the market and are independent from things that happened in 2011 and 2013. Consequently, town B can be used as a control when town A is treated in 2011, and town A in turn can act as a control when town B is treated in 2013.

An important element of the design is that it is a short-term identification strategy based on a fiver-year reassessment cycle. In other words, the tax shocks examined by this paper may be perceived as temporary tax changes by businesses, and therefore, the effects detected by this design might be smaller. On the other hand, if the findings indicate that even short term property tax increases can generate long-lasting effects, then it should draw more attentions to local governments when determining

property tax policy.

1.5.2 Effect of Changes in Tax Bases on Local Revenue

To investigate how local governments respond to shocks in overall property assessed values, I first estimate the elasticity of the millage rate with respect to the tax base using regression 1.5:

$$lnm_{it} = \beta * ln(V_{it}^{tot}) + X_{it} + \gamma_t + \theta_i + \epsilon_{it} \tag{1.5}$$

m_{it} represents millage rates in year t for town i and V_{it}^{tot} is the total tax base. If local governments completely ignore tax base changes, we would expect β to be a very small number that is statistically insignificant from 0. On the other hand, if local governments adjust the millage rate to fully offset changes in tax bases and maintain constant revenue, then β should be insignificantly different from -1. Subscript t is the budget year, and X_{it} is a set of control variables motivated by prior literature, including share of female, share of foreign-born, and share of higher education. θ_i and γ_t indicate town fixed effects and year fixed effects. The town fixed effects control for unobserved economic characters that are constant over time, whereas year fixed effects control for statewide macroeconomic trends. Standard errors are clustered at the town level.

As infrequent reassessments can also lead to large changes in tax base compositions by changing relative assessed values between business and residential properties, I then examine how changes in these assessed values affect local revenues using regression

1.6:

$$\Delta Y_{it} = \beta_1 * \Delta V_{it}^B + \beta_2 * \Delta V_{it}^R + X_{it} + \gamma_t + \theta_i + \epsilon_{it} \tag{1.6}$$

ΔY_{it} is the change in local revenue in town i calculated between year $t-1$ and t, which includes changes in property tax revenue, other local revenue, intergovernmental transfers, and total revenue. This regression separates the influence of business (β_1) and residential (β_2) assessed values on revenue outcomes. If local governments do not response to changes in tax base composition, then we would expect both β_1 and β_2 to be insignificant.

1.5.3 Effect of Changes in Tax Revenue Sources on Public Spending

To understand whether shifts in tax revenue source affect public spending, I estimate the following regression:

$$\Delta Y_{it} = \beta * \Delta s_{it}^B + X_{it} + \gamma_t + \theta_i + \epsilon_{it} \tag{1.7}$$

Δs_{it}^B represents the percentage point change in total tax revenue coming from business properties. ΔY_{it} is the change in categorical public spending in town i calculated between year $t-1$ and t (measured both as percent change and per-capita change). Specifically, I examine whether local governments spend more on highway and transportation and public safety if more revenue comes from the business sector. I also test whether local governments reduce spending on education if residential tax shares are lower. Since local governments may not respond to revenue source changes immediately, I also examine longer-run period effects up to 3 years following tax source changes.

1.5.4 Effect of Changes in Taxes on Business Outcomes

I then estimate the effect of changes in business tax burden on business outcomes:

$$\Delta Y_{it} = \beta * \%\Delta s_{it}^{B} + X_{it} + \gamma_t + \theta_i + \epsilon_{it} \tag{1.8}$$

$\%\Delta s_{it}^{B}$ represents the percent change in property taxes on businesses stemming from reassessments. I consider two classes of outcome variables: business exit and labor market outcomes. Business exit includes measures of number of establishments, property vacancies, and property transactions. The number of establishments is differentiated by industry and firm size. For vacancies, I consider both short-term and long-term vacancies following Immergluck (2016). Transaction records include the number of business market sales and the number of foreclosures. As property taxes may not generate immediate impacts on outcomes such as moving and selling decisions, I also examine longer-run effects up to 3 years following tax changes. Labor outcomes include aggregate annual wages and average annual employment.

1.5.5 Effect of Changes in Taxes on Residential Outcomes

To examine how residential tax burdens on residents affect housing outcomes, I replace $\%\Delta s_{it}^{B}$ with $\%\Delta s_{it}^{R}$ and estimate equation 1.9:

$$\Delta Y_{it} = \beta * \%\Delta s_{it}^{R} + X_{it} + \gamma_t + \theta_i + \epsilon_{it} \tag{1.9}$$

where $\%\Delta s_{it}^{R}$ represents the percent change in tax burden on residents due to reassessment. I examine property vacancies and transaction records, and differentiate vacancies

by length (short-term vacancy and long-term vacancy). In addition, I classify vacant residential properties as vacant for sale and vacant for rent. Transaction records include the number of residential market sales and the number of foreclosures. As with business properties, I consider longer-run effects up to 3 years following reassessment.

1.6 Local Revenue

As the infrequency of reassessments can result in sudden and substantial changes in the tax base, I first investigate how local governments respond when there is a shock to overall assessed property values. When property values fluctuate substantially, one might expect significant changes in property tax revenue. For example, after the Great Recession, there were significant budgetary declines at the state and federal levels.

Previous studies suggest that when tax bases fluctuate, whether local governments experience revenue shocks is primarily determined by how they set their property tax rates. There are two different views on the role of tax rate setting. The residual view suggests that local governments can adjust millage rate to offset changes in tax base. An important implication of this view is that the elasticity of the tax rate with respect to the tax base is equal to one. Alternatively, the strategic view suggests that the tax rate is a choice variable and is selected based on local preferences toward maintaining and attracting investment (Brueckner, 2003; Ihlanfeldt and Willardsen, 2014). If the strategic view plays a dominant in tax rate setting, we should expect local gov-

ernments to reluctantly increase the tax rate when the tax base drops and experience revenue losses. Previous literature finds mixed evidence of these two views (Brueckner and Saavedra, 2001; Ihlanfeldt and Willardsen, 2014; Brien, 2018).

As shown in Table 1.1, I find that when the tax base decreases by 1%, local millage rates increase by 0.94%, offsetting changes in the tax base. The coefficient is not statistically different from negative one, which is consistent with the residual view. The estimate in column (2) results an insignificant impact of property tax base changes on property tax revenue, which is consistent with prior literature documenting property tax stability (Lutz, 2008; Alm et al., 2011; Doerner and Ihlanfeldt, 2011). Estimates in columns (3) to (5) suggest that tax base changes generate little impact on other local revenue sources, intergovernmental transfers, and total revenue.

Having established that changes in the property tax base have little effect on local revenue, I next examine whether local governments respond differently when there is a change in tax base composition. Table 1.2 reports the results of separating the influence of business and residential tax base on local revenue. As we can see from column (1), neither changes in business nor residential assessed value impact property tax revenue, suggesting that local governments adjust millage rates to stabilize property tax revenue regardless of whether the change in tax base stems from business or residential property. Although a decline in total revenue is observed in column (4) for residential properties, the magnitude is economically small, and it seems to be driven by intergovernmental transfers.[16] Appendix Table 1.A also shows similar findings by

[16]One possible explanation for the changes in intergovernmental transfers could be an increase in Education Equalization Grants during and after the Great Recession.

examining the impact of changes in business tax shares on local revenue.

1.7 Categorical Public Spending

Earlier studies such as Bordignon et al. (2003) and Vermeir and Heyndels (2006) have pointed to the importance of tax rate setting for local political activities. However, little is known about whether local governments care about the source of property tax revenue when making spending decisions. If local government cares about tax revenue source, we should expect business taxpayers (or residents) to receive more public benefits if the tax burden shifts towards them. In this section, I examine how the shifting of tax revenue source affects government spending behavior.

To test whether local governments reward businesses when tax shares shift toward them, I first examine changes in local highway and transportation spending. Earlier studies have provided evidence on positive correlations between highway and transportation spending and business activity (Bartik, 1985b; Reynolds and Maki, 1990; Garcia-Mila and McGuire, 1992). A 2019 U.S. Chamber of Commerce survey also indicates that low quality of roads were a primary concern of small business owners. Highway and transportation spending also includes parking lots, which may be a priority for business owners. Highway and transportation spending may therefore disproportionately benefit business taxpayers.[17] The results in columns (1) and (2) of Table 1.3 suggest little evidence of grater highway and transportation spending after the tax bur-

[17] According to the Connecticut department of transportation, local town governments take 80% of road maintenance responsibilities on average.

den shifts toward businesses. Moreover, estimates in panel B and C imply that there is no evidence of longer-run effects.

Another public services that local businesses are likely to value is public safety. Earlier studies document the positive impact of public safety spending on business performances (Tannenwald, 1996; Papke, 1991). And, according to a recent NFIB report on small businesses, local crime is a top issue.[18] Therefore, I test whether local governments allocate more resources to public safety when businesses pay a higher tax share. The results shown in columns (3) and (4) of Table 1.3 reveal changes in public safety spending that are economically small and statistically insignificant.

An increase in the tax share for business is accompanied by a commensurate decrease for residents. As education is more likely to be a preferred public good for residents, I examine whether an increase in business tax share leads to a decline on education spending. The results in columns (5) and (6) reveal that the impact on education spending is economically small and statistically insignificant across specifications.

Overall, I find that local governments are not sensitive to changes in tax revenue sources or tax base compositions. In addition, tax payers from different interest groups do not appear to benefit from bearing higher tax shares, which is inconsistent with the property tax benefit view.[19] Further, since the shifts in tax revenue sources caused by reassessment do not lead to public service provision changes, I can rule out such a mechanism and estimate the pure tax effects on business and residential out-

[18] https://www.nfib.com/assets/NFIB-Problems-and-Priorities-2016.pdf

[19] An important condition for a tax to be benefit is who bear a higher property tax burden than others in the same jurisdiction must receive correspondingly higher benefits from public services.

comes.

1.8 Property Taxes and Business Exit

According to the capital view, high property taxes are unattractive to businesses, and may push businesses out of high tax areas and into low tax areas. However, earlier studies have mostly found a lack of adverse effects of property taxes (Carlton, 1983; Bartik, 1985a; Dye et al., 2001), and one explanation is that the benefits of public goods outweigh any negative property tax effects (Gabe and Bell, 2004). Given the finding that higher property tax burdens for businesses do not change local public services in the previous section, I test the capital view by estimating the tax effect on business mobility and closure. I consider three different outcomes to estimate business exit: the number of business establishments, business property vacancies, and transaction records.

Earlier studies examining the effect of taxes on business localities primarily focus on corporate income taxes (Ohrn, 2019; Giroud and Rauh, 2019). This paper contributes to the existing literature by examining the role of property taxes and focusing on small firms. Because business owners may not immediately respond to tax shocks, I examine the effects on business decisions 1, 2, and 3 years following the shocks.

1.8.1 Number of Establishments

Information on the number of establishments are collected from the CBP data set. Figure 1.6 shows that the majority of firms have fewer than ten employees. For

this study, I follow the most common firm size classification standard and divide establishments into micro firms (fewer than ten employees), small firms (10-49 employees), and medium and large firms (50+ employees). Micro and small firms make up nearly 90% of establishments in all industries (Appendix Table 1.B).

The estimates in column(1) of Table 1.4 indicate that a higher business tax burden is not associated with a significant decrease in establishments overall. One possible explanation for this could be that, on average, property taxes do not make up a large enough proportion of business costs to affect their profitability. Furthermore, as the identification is based on short-run tax changes, the overall impacts could be small. However, it is reasonable to believe that firms in industries with different profit margins may respond to tax changes differently. Thus, I test for heterogeneous effects.

In general, we should expect property taxes to generate larger effects in industries with lower profit margins. In the U.S., average net profit margin across all industries is 7.71%, while net profit margins for manufacturing and retail industries are around 2%.[20] Thus, I examine how the number of establishments is affected by property taxes for industries with low profit margins (i.e., manufacturing and retail).

Table 1.5 presents estimates of the effect of property taxes on manufacturing firms. The average number of manufacturing businesses in each town is 54.96, and more than half of them are operations with fewer than ten employees. I find that higher business property taxes are associated with a decreasing number of manufacturing

[20] According to a NYU Stern database, manufacturing and retails are among the sectors with the lowest profit margins. For example, the net margin for rubber& tires and grocery & food retail is 1.26% and 1.44%. Data source: http://pages.stern.nyu.edu/ adamodar/New˙Home˙Page/datafile/margin.html

establishments, as shown in column (1). The effects grow larger over time and become significant in the second year after the shock. A one percent increase in business tax burden results in a 0.12% to 0.18% decrease in manufacturing firms in the second and third years. I also find that the decrease are largely driven by micro-firms closing or moving, as indicated in column (2). The effects are significant in each of the three years following reassessment. The magnitudes suggest a 0.14% to 0.39% decrease of micro-manufacturing firms when the business tax burden increases by one percent. In addition to its lower profit margins, another explanation for manufacturing firms' high elasticity suggested by earlier literature is that manufacturing firms are more oriented to a national market, and hence local costs are a more important competitive consideration (Bartik, 1991).

Table 1.6 displays the property tax effects on retail firms. Negative results in column (1) suggest that higher tax burdens might influence retail establishments adversely, with the effects become statistically significant in the third year after the reassessment. A one percent increase in business tax burden leads to a decrease of 0.08% retail establishments in the third year after the shock. One possible explanation for the smaller effect on retail stores is that they serve local markets, and thus, may be geographically constrained.

I also examine the effects on finance and professional services industry. The results are reported in Table 1.7, and no significant effects are detected for firms in these service industries. One possible explanation could be these businesses are more location dependent. A recent study by Diodato et al. (2016) suggests that the location

preferences of services are at least as strongly driven by supply links as those of manufacturing, whereas services industries such as knowledge intensive business firms have stronger location dependency to meet labor needs than manufacturing. Besides, these service firms tend to have higher profit margins, and small changes in taxes will not have large impacts on profitability to exit current locations.[21]

1.8.2 Business Property Vacancy Rate

Higher property taxes may result in a growing volume of vacant business properties, suggesting that, on net, more firms are leaving the area, which is detrimental to local economies. This section examines the relationship between property taxes and business vacancies using USPS vacancy data. I follow Immergluck (2016) and divide vacant addresses into vacancies for a period of less than six months (short-term vacancy) and over six months (long-term vacancy). Short-term vacancies are most likely to be caused by properties listed to be sold or rented. Therefore, it can be considered as a measurement of owners' selling or leasing decisions. Previous studies also find that longer vacancies are correlated with higher crime rates (Cui, 2010; Ellen et al., 2013). Although some properties may eventually be sold or rented, vacancies exceeding six months are likely to be a reasonable indicator for weak business demand and market distress.

Table 1.8 examines the impact of property taxes on quarterly business vacan-

[21]Finance and the professional services industry have the highest profit margins, according to the NYU Stern database. For example, the net profit margins for regional banks can be as high as 30.50%, and the margins for software designs are nearly 20%. Data sources see footnote 18.

cies. Two specifications are estimated. Columns (1) and (3) report percent changes by taking log differences in vacant addresses, while columns (2) and (4) present vacancy rates by scaling the number of vacant addresses by total business addresses in the baseline year. I test the impact on short-term vacancies in the first two columns and find no statistically significant effect in the first two years. A possible explanation for this is that the USPS sample only includes properties that are vacant (not collecting their mail) for 90 days or longer. In other words, the results would not capture properties that are successfully transacted within 90 days, or properties with mail collection even if the properties are empty and listed on the market. However, I find that the effect becomes significant by the third year, with a one percent increase in the business tax burden resulting in a 1.41% increase in short-term vacancies.

Examining long-term vacancies, I find suggestive evidence that higher property tax burdens lower the demand for businesses. One year after the shock, a one percent increase in tax burden leads to the long-term vacancy rates increasing between 0.025 to 0.036 percentage points, as shown in column (4), suggesting that higher property tax burdens depress market demand and cause properties to experience longer vacancy periods.

One may argue that when demand decreases, property owners have an incentive to reduce prices in order to sell or rent properties faster rather than allowing properties to be vacant. However, a recent behavioral economics study by Bokhari and Geltner (2011) finds evidence of loss aversion among business property sellers. In a depressed market, property owners tend to hold higher reservation prices to avoid loss relative to

their purchase prices, even if it takes longer to sell. As the average lease term for retail was 7.9 years in 2013 and increased to 9.6 years in 2018, it may be worth a loss averse seller to wait to find an ideal tenant.[22] Another explanation for higher vacancy rates is price anchoring. For example, property owners may be accustomed to certain rent levels and index their expectations based on past experience, even if market conditions change.

1.8.3 Business Property Transactions

In this section, I use transaction records from Zillow to examine how owners' selling decisions respond to higher business property taxes. I first examine whether higher taxes increase traditional sales in the three years following the shock in Table 1.9. The results in column (1) indicate that higher taxes generate statistically insignificant effects on business property sales across all three years. However, the results in column (2) suggest that increasing the business tax burden causes increased business property foreclosures. This is evident in the second and third year after the shock, with a 1 percent increase in business property taxes increasing foreclosures by 0.96% to 1.03%.

As a placebo test for these results, I examine whether current business tax burden changes have predictive power for prior business outcomes. Specifically, I regress business variables calculated over earlier periods (t-1 and t-2) on percentage changes in business tax burdens between t-1 and t for towns in non-reassessment years. The results reported in Appendix Table 1.C panel A reveal insignificant coefficients, suggesting that

[22]Data source:https://ggwash.org/view/68318/why-is-that-house-or-storefront-vacant

preexisting trends in the dependent variables are unlikely to be driving the results.

Overall, I find higher property tax burdens on businesses reduce the number of establishments in industries with lower profit margins, especially for firms with fewer than ten employees. The result is evident in data on local business counts, commercial vacancy rates, and foreclosures. The findings are consistent with the capital view, and in line with Enami et al. (2018), which also finds adverse effects on the number of establishments by studying the impact of school district levies on firms. In addition, given that short-term property tax increases may have long-lasting effects on business location decisions, the results is highly relevant to policy.

1.9 Property Taxes and Wages and Employment

Higher tax bills may cause businesses to move to less expensive locations, but it is also a common view that the tax costs may be passed on to workers (Suárez Serrato and Zidar, 2016). In addition, according to the capital view, higher property taxes are not only borne by capital owners but also affect wages, land rents, and housing prices (Mieszkowski, 1972). Earlier studies examining the incidence of corporate income tax suggest that an exogenous rise of $1 in tax would reduce the wage bill between 45 and 75 cents (Randolph, 2006; Desai et al., 2007; Arulampalam et al., 2012). Recent studies have also demonstrated relationships between changes in corporate income tax and firm employment (Ljungqvist and Smolyansky, 2014; De Simone et al., 2019; Giroud and Rauh, 2019). As most of previous studies focus on corporate income taxes at the state

or federal levels, this paper contributes to the literature by providing new evidence on the impact of local property taxes on businesses and could find different results.

Table 1.10 examines the impacts of business property taxes on wages and employment for firms across all industries. Estimates in column (1) suggest that higher business property tax burdens are associated with lower annual average employment. The impacts are significant across all three years, implying a decline between 0.02% and 0.05%. Negative and statistically significant effects are also observed for wages in column (2). In the first year after the shock, a one percent increase in the business tax burden decreases the annual aggregate wage by 0.06%. The effects grow larger in subsequent years and are significant for all three years with a range between -0.06% and -0.08%. Estimates in column (3) measure the impact on annual wages per employee (i.e., annual average wage). The negative magnitudes indicate higher business tax burdens generate adverse effects on annual average wages, though the effects are only significant three years after the tax shock. Estimates in column (4) represent the influences by change on employment per capita (i.e., employment rate). A one percent increase in business tax burden leads to the employment rate dropping between 0.014 and 0.027 percentage points within three years after the shocks.

Next, I test the heterogeneous effects on wages and employment for the same division of industries presented in section 1.8. Table 1.11 presents results for manufacturing firms. Negative impacts are detected on both aggregate wage payments and average annual payments in the second and third years after the shock as shown in column (2) and (3). In particular, a one unit increase in the business tax burden leads

to a 0.26% to.29% decline in aggregate wage payments, and a 0.15% decrease in annual average wages two years after the shock. Results for retail industries are reported in Table 1.12. Although I observe some negative magnitudes on both wage and employment outcomes, they are not statistically significant. Table 1.13 presents estimates for the finance and professional services industries. In the second year after the shock, I observe adverse and significant effects on employment. I also find negative and statistically significant effects on aggregate wages and employment rates in the third year after the sock, as shown in columns (2) and (4).

Local employment trends could contaminate estimates on local labor outcomes. For example, suppose there is an increasing trend in employment that leads residential property values to rise. In this case, when a reassessment year comes, residential properties will be reassessed upwards and tax burdens on business will be reduced. Therefore, I add a town-specific year trends as shown in the Appendix Table 1.D. Coefficients reported in column (1), (3), (5), (7), (9) are from our base identifications. The results across specifications are quite similar, suggesting that preexisting trends in employment are unlikely to be driving our results. In addition, the insignificant results from the placebo test shown in Appendix Table 1.C panel B indicates that the estimates can be interpreted causally.

Overall, I find that when business property tax burdens increase, the taxes are passed through to labor by reducing wage payments and employment, which is consistent with the property tax capital view as well as earlier literature on state or federal corporate taxes.

1.10 Property Taxes and Residential Mobility

This section uses the USPS vacant address records, ACS housing vacancy data, and Zillow property transaction records to examine how residents respond to higher property tax burdens.

1.10.1 Residential Property Vacancy Rate

Similar to the previous section, I split USPS vacant addresses into short-term and long-term vacancies (i.e., less and over six months of vacancy). Recall that an increasing number of short-term vacancies might reflect a higher volume of property transactions, while long-term vacancies measure local market demand. The results reported in Table 1.14 do not reveal any meaningful effects on vacancies. One limitation of the USPS data is that it does not distinguish properties by vacancy reason. For example, a vacation house could also be counted as vacant property by USPS based on the mail collection standard. Therefore, I use ACS vacant housing data as a supplement data set to further examine the relationship between property taxes and housing vacancies.

An advantage of the ACS data is that it classifies vacant properties into several groups such as vacant for sale, vacant for rent, and vacant for recreational purposes. I use the number of vacant for-sale housing for each town to measure if higher property taxes affect homeowners' selling decisions. Table 1.15 reports the estimates based on the ACS data. Similar to the findings from USPS data, I do not detect significant

effects. Overall, I do not find any significant relationship between residential vacancies and property taxes.

1.10.2 Residential Property Transactions

In this section, I use housing transaction records from Zillow to test whether property taxes affect residential sale decisions. In Table 1.16, I test how owners of single family houses respond to property tax burden changes. The insignificant results in both columns indicate that residents are not highly sensitive to tax burden changes. A potential explanation for the insignificant results on residential foreclosures is that Connecticut has had a foreclosure mediation program for residential properties since 2008. It aims to help homeowners and banks reach fair, voluntary, and negotiated agreements to avoid foreclosure. This may help to explain why the number of residential foreclosure events does not change when the property tax burden increases.[23]

Although I do not find significant effects on residential mobility, the finding is not inconsistent with earlier research, as empirical evidence on the consequence of higher property taxes on homeowner mobility is not conclusive. For example, Seslen (2005) finds little evidence that property taxes impact elderly homeowners' decisions, while Shan (2010) finds higher property taxes increase elderly homeowners' mobility. Using variation from periodic property assessments in Ohio, Fraenkel (2019) finds that higher property taxes induce a small number of owners to sell their properties, but has no effect on foreclosures. However, Ding et al. (2020) shows that higher taxes caused

[23]The program is for residential properties only and does not apply to business properties

by property reassessments in Philadelphia do not increase home sales among vulnerable homeowners but does lead to a higher risk of tax delinquencies.

1.11 Conclusion

This paper examines how shifting the tax burden between residential and business property affects local government spending, business activities, and residential mobility. To address the endogeneity of property values and tax rates, I exploit shifts in the tax burden stemming from staggered reassessment cycles using a difference-in-differences design. This research incorporates multiple novel data sources such as financial data from local audit reports, County Business Pattern data, USPS Vacancy data, and Zillow ZTRAX.

The analysis reveals that local revenue does not respond to shocks in the size of the tax base due to reassessment as governments could adjust property tax rate to offset changes in tax base, which is consistent with the residual view of tax rate setting. In addition, local governments are not sensitive to tax base revenue source changes, as shifting the property tax burden between residents and businesses does not affect total revenue and the types of public expenditures. Thus, changes in business and residential outcomes reflect responses to changes in the property tax burden and not changes in public goods provision. For business outcomes, I find that higher business tax burdens reduce the number of establishments in industries that traditionally have low profit margins, and it is also evident in data on local business counts, commercial vacancy

rates, and foreclosures. Further, increased business property tax burdens appear to generate adverse effects on employment levels and annual aggregate wage payments. Both findings in business activities are in line with the property tax capital view of tax incidence. In contrast, I do not find significant evidence of increased residential vacancies, sales, or foreclosures.

The study contributes to the literature in several ways. First, it contributes to the literature on revenue sources, and fiscal responses by differentiating revenue sources based on taxpayers' identities, distinguishing between residents and businesses. Second, it contributes to the studies on the political implications of taxes by examining whether local governments care about the tax base composition. The analysis is informative for tax policy design, especially on the dynamics of tax sources and allocations. It also provides new empirical evidence supporting the property tax residual views and the capital view.

References

Alm, J., Buschman, R. D., and Sjoquist, D. L. (2011). Rethinking local government reliance on the property tax. *Regional Science and Urban Economics*, 41(4):320–331.

Arulampalam, W., Devereux, M. P., and Maffini, G. (2012). The direct incidence of corporate income tax on wages. *European Economic Review*, 56(6):1038–1054.

Bartik, T. J. (1985a). Business location decisions in the united states: Estimates of the effects of unionization, taxes, and other characteristics of states. *Journal of Business & Economic Statistics*, 3(1):14–22.

Bartik, T. J. (1985b). Business location decisions in the united states: Estimates of the effects of unionization, taxes, and other characteristics of states. *Journal of Business & Economic Statistics*, 3(1):14–22.

Bartik, T. J. (1991). Who benefits from state and local economic development policies?

Bokhari, S. and Geltner, D. (2011). Loss aversion and anchoring in commercial real estate pricing: Empirical evidence and price index implications. *Real Estate Economics*, 39(4):635–670.

Bordignon, M., Cerniglia, F., and Revelli, F. (2003). In search of yardstick competition: a spatial analysis of italian municipality property tax setting. *Journal of Urban Economics*, 54(2):199–217.

Brien, S. T. (2018). Compensating changes to the property tax levy? an empirical test of the residual rule. *Public Finance Review*, 46(6):949–973.

Brueckner, J. K. (2003). Strategic interaction among governments: An overview of empirical studies. *International regional science review*, 26(2):175–188.

Brueckner, J. K. and Saavedra, L. A. (2001). Do local governments engage in strategic property—tax competition? *National Tax Journal*, pages 203–229.

Carlton, D. W. (1983). The location and employment choices of new firms: An econometric model with discrete and continuous endogenous variables. *The Review of Economics and Statistics*, pages 440–449.

Cui, L. (2010). Foreclosure, vacancy and crime. *Available at SSRN 1773706*.

Dahlberg, M., Mörk, E., Rattsø, J., and Ågren, H. (2008). Using a discontinuous grant rule to identify the effect of grants on local taxes and spending. *Journal of Public Economics*, 92(12):2320–2335.

De Simone, L., Lester, R., and Raghunandan, A. (2019). Do targeted business tax subsidies achieve expected benefits? *Available at SSRN 3482207*.

Desai, M. A., Foley, C. F., and Hines, J. R. (2007). Labor and capital shares of the cor-

porate tax burden: International evidence. In *Conference on who pays the corporate tax in an open economy*.

Ding, L., Hwang, J., and Tahsildoost, M. (2020). Effects of gentrification on homeowners: Evidence from a natural experiment. *Regional Science and Urban Economics*, page 103536.

Diodato, D., Neffke, F., O'Clery, N., et al. (2016). Agglomeration economies: the heterogeneous contribution of human capital and value chains. Technical report, Utrecht University, Department of Human Geography and Spatial Planning ....

Doerner, W. M. and Ihlanfeldt, K. R. (2011). House prices and city revenues. *Regional Science and Urban Economics*, 41(4):332–342.

Dye, R. F., McGuire, T. J., and Merriman, D. F. (2001). The impact of property taxes and property tax classification on business activity in the chicago metropolitan area. *Journal of Regional Science*, 41(4):757–777.

Ellen, I. G., Lacoe, J., and Sharygin, C. A. (2013). Do foreclosures cause crime? *Journal of Urban Economics*, 74:59–70.

Enami, A., Reynolds, C. L., and Rohlin, S. M. (2018). The effect of property taxes on businesses: Evidence from a dynamic regression discontinuity approach.

Fraenkel, R. (2019). Property tax-induced mobility and redistribution: Evidence from mass reappraisals.

Fuest, C., Peichl, A., and Siegloch, S. (2018). Do higher corporate taxes reduce wages? micro evidence from germany. *American Economic Review*, 108(2):393–418.

Gabe, T. M. and Bell, K. P. (2004). Tradeoffs between local taxes and government spending as determinants of business location. *Journal of Regional Science*, 44(1):21–41.

Garcia-Mila, T. and McGuire, T. J. (1992). The contribution of publicly provided inputs to states' economies. *Regional science and urban economics*, 22(2):229–241.

Giroud, X. and Rauh, J. (2019). State taxation and the reallocation of business activity: Evidence from establishment-level data. *Journal of Political Economy*, 127(3):1262–1316.

Ihlanfeldt, K. R. and Willardsen, K. (2014). The millage rate offset and property tax revenue stability. *Regional Science and Urban Economics*, 46:167–176.

Immergluck, D. (2016). Examining changes in long-term neighborhood housing vacancy during the 2011 to 2014 us national recovery. *Journal of Urban Affairs*, 38(5):607–622.

Leduc, S. and Wilson, D. (2017). Are state governments roadblocks to federal stimulus? evidence on the flypaper effect of highway grants in the 2009 recovery act. *American Economic Journal: Economic Policy*, 9(2):253–92.

Ljungqvist, A. and Smolyansky, M. (2014). To cut or not to cut? on the impact of corporate taxes on employment and income. Technical report, National Bureau of Economic Research.

Lutz, B., Molloy, R., and Shan, H. (2011). The housing crisis and state and local government tax revenue: Five channels. *Regional Science and Urban Economics*, 41(4):306–319.

Lutz, B. F. (2008). The connection between house price appreciation and property tax revenues. *National Tax Journal*, pages 555–572.

Martinez, L. R. (2019). Sources of revenue and government performance: Evidence from colombia. *Available at SSRN 3273001*.

Mast, E. (2020). Race to the bottom? local tax break competition and business location. *American Economic Journal: Applied Economics*, 12(1):288–317.

Mieszkowski, P. (1972). The property tax: An excise tax or a profits tax? *Journal of Public Economics*, 1(1):73–96.

Netzer, D. (1964). Income elasticity of the property tax: A post-mortem note. *National Tax Journal*, 17(2):205–207.

Ohrn, E. (2019). The effect of tax incentives on us manufacturing: Evidence from state accelerated depreciation policies. *Journal of Public Economics*, 180:104084.

Papke, L. E. (1991). Interstate business tax differentials and new firm location: Evidence from panel data. *Journal of public Economics*, 45(1):47–68.

Randolph, W. C. (2006). *International burdens of the corporate income tax*. Congressional Budget Office Washington, DC.

Reynolds, P. D. and Maki, W. (1990). Us regional characteristics, new firms, and economic growth. In *presentation to Cross National Workshop on the role of Small and Medium Enterprises in Regional Economic Growth, University of Warwick*, volume 28.

Ross, J. M. and Yan, W. (2012). Fiscal illusion from property reassessment? an empirical test of the residual view. *National Tax Journal, Forthcoming*.

Seslen, T. (2005). Property tax abatement programs: Does relief really keep grandma in her home?

Shan, H. (2010). Property taxes and elderly mobility. *Journal of Urban Economics*, 67(2):194–205.

Silverman, R. M., Yin, L., and Patterson, K. L. (2013). Dawn of the dead city: An exploratory analysis of vacant addresses in buffalo, ny 2008–2010. *Journal of Urban Affairs*, 35(2):131–152.

Suárez Serrato, J. C. and Zidar, O. (2016). Who benefits from state corporate tax cuts? a local labor markets approach with heterogeneous firms. *American Economic Review*, 106(9):2582–2624.

Tannenwald, R. (1996). State business tax climate: how should it be measured and how important is it? In *Proceedings of the Annual Conference on Taxation Held under the Auspices of the National Tax Association-Tax Institute of America*, volume 89, pages 225–228. JSTOR.

Tiebout, C. M. (1956). A pure theory of local expenditures. *Journal of political economy*, 64(5):416–424.

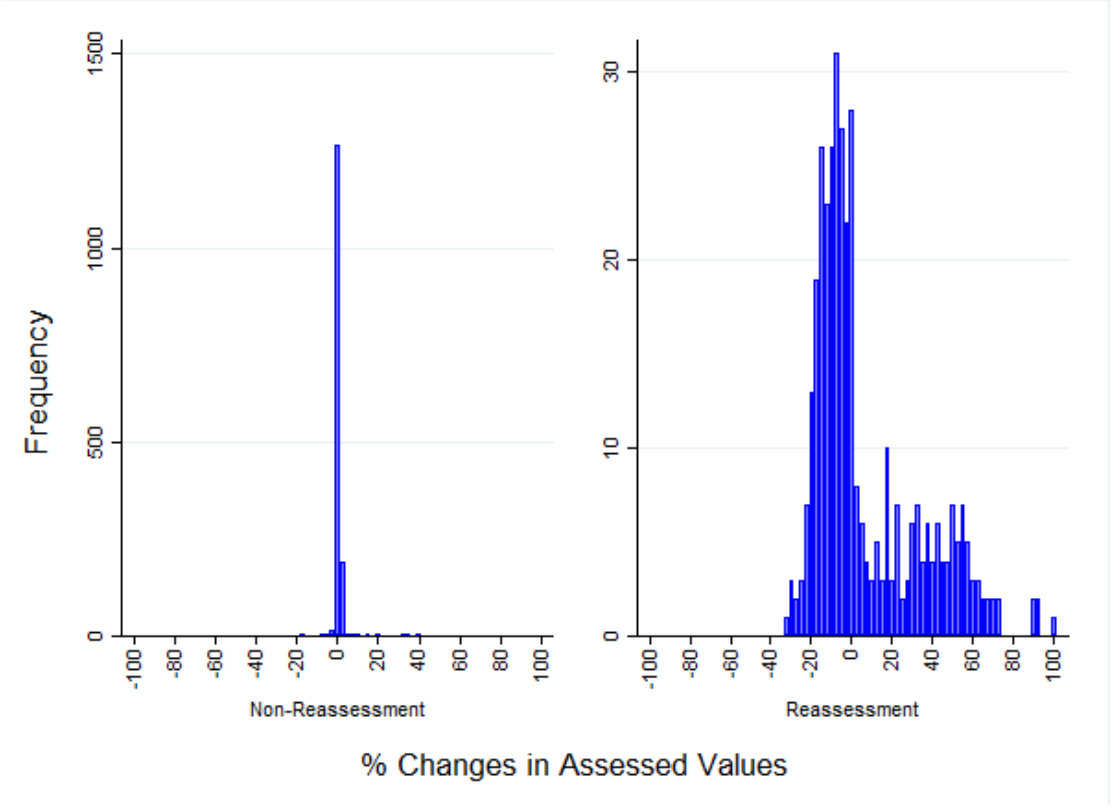
Vermeir, J. and Heyndels, B. (2006). Tax policy and yardstick voting in flemish municipal elections. *Applied Economics*, 38(19):2285–2298.

Zodrow, G. R. (2001). Reflections on the new view and the benefit view of the property tax. *Property taxation and local government finance*, pages 79–111.

Zodrow, G. R. and Mieszkowski, P. (1986). Pigou, tiebout, property taxation, and the underprovision of local public goods. *Journal of urban economics*, 19(3):356–370.

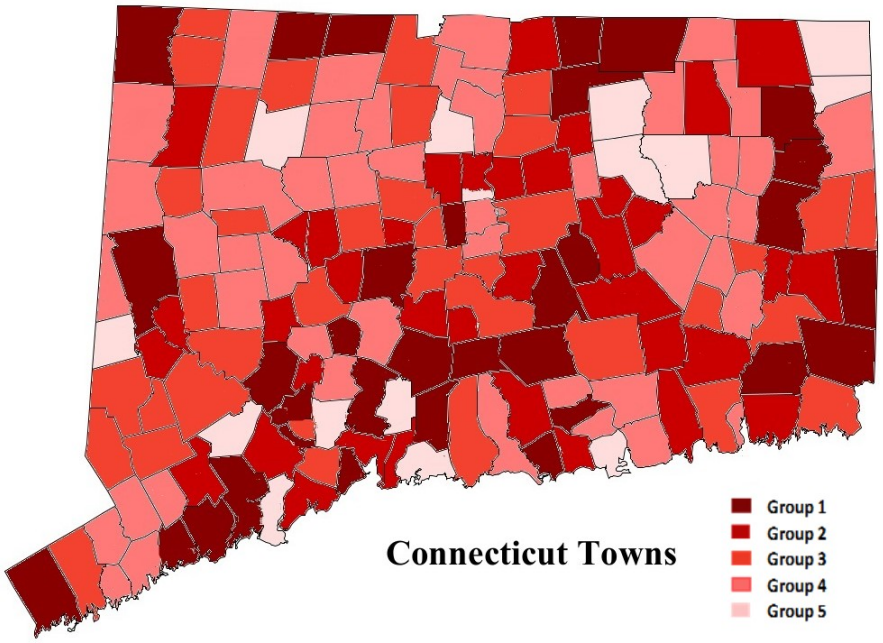
Figures and Tables

Figure 1.1: Percent Changes in Assessment Values, Non-reassessment vs. Reassessment Years



The left panel of this figure is a histogram of the percent change in year-to-year real property values for non-reassessment years, and the percent changes in reassessment years are shown in the right panel.

Figure 1.2: Town Groups by Reassessment Schedule

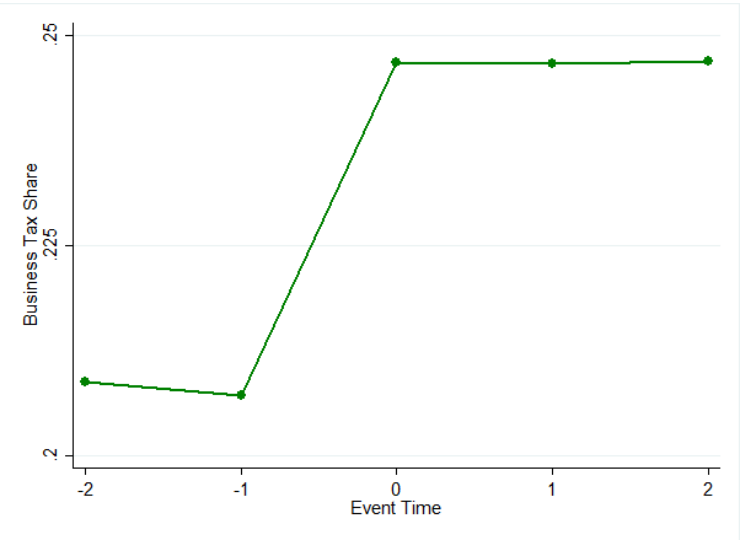


Town reassessment schedules are obtained from State of Connecticut Office of Policy and Management website.

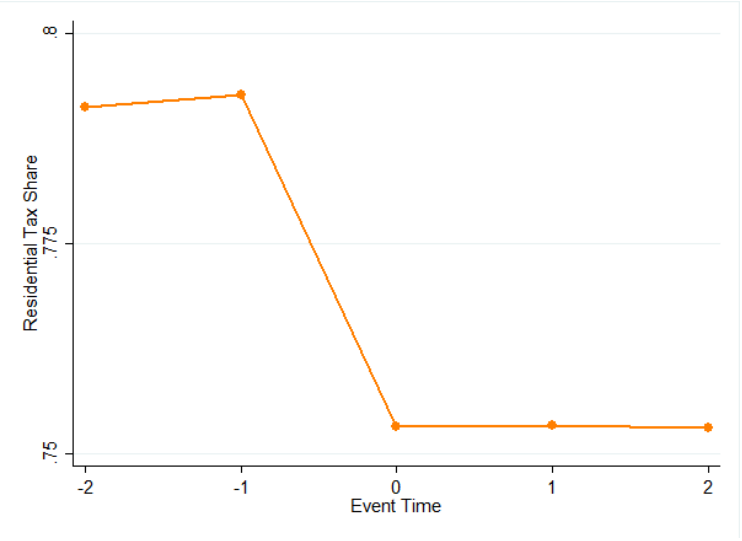
Figure 1.3: Percent Changes in Assessment Values by Property Types, Great Recession Periods

This figure shows the percentage changes of assessed values of business and residential properties due to reassessments during the great recession period. The reassessment years included in this figure are 2009, 2010, 2011, 2012, and 2013

Figure 1.4: Property Tax Revenue Shares, Great Recession Periods



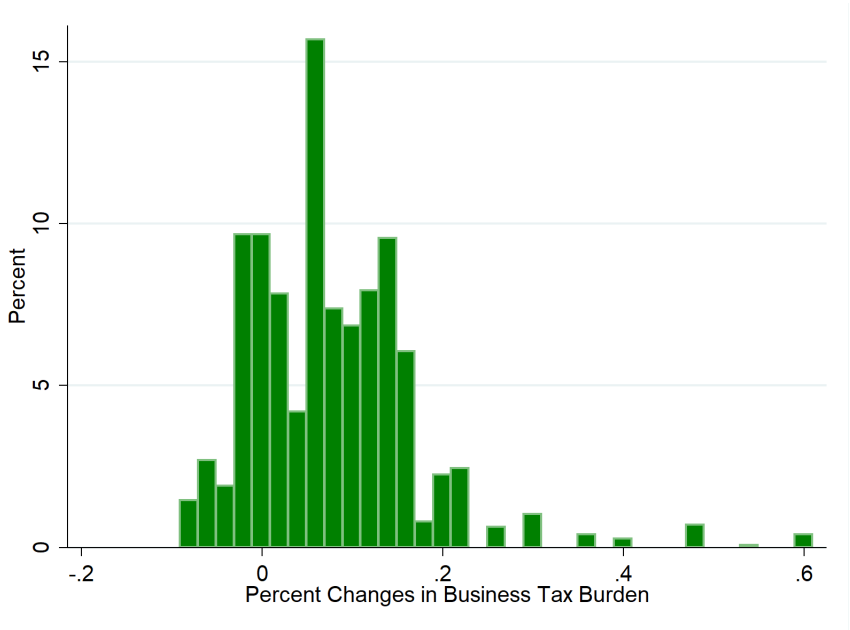
(a) Business Property



(b) Residential Property

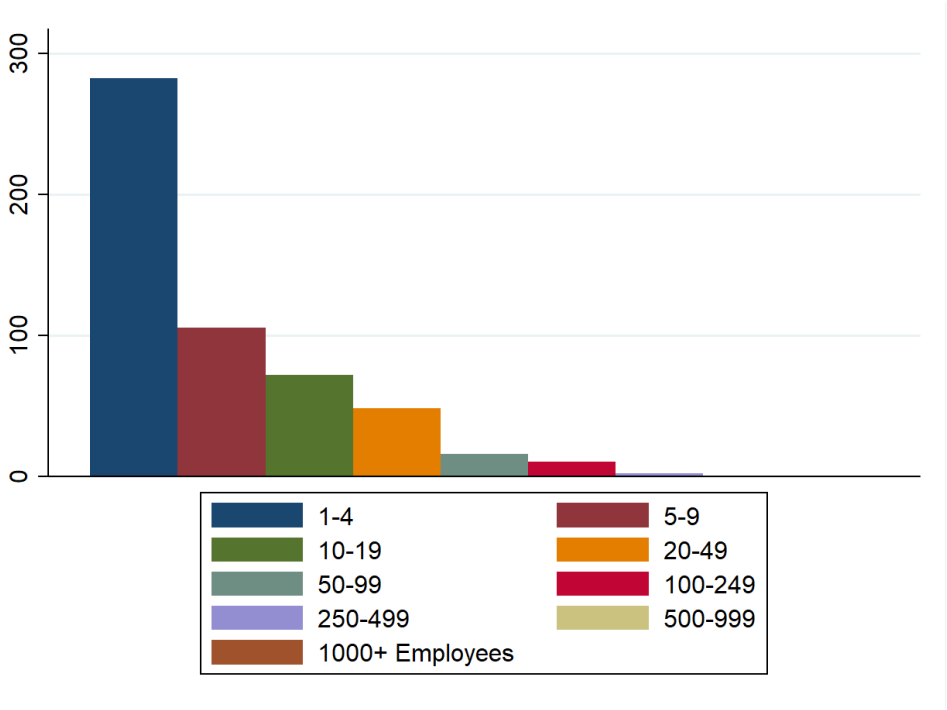
This figure shows the shifts of property tax revenue source caused by reassessment happened in and after the great recession periods. Year 0 is the scheduled reassessment year during the great recession periods, which are 2009, 2010, 2011, 2012, and 2013 for different reassessment groups. The top two panels include all the towns, and the bottom two panels include the top 20% of towns whose revenue shifted mostly towards businesses.

Figure 1.5: Percent changes on Business Tax Burdens, Great Recession Periods



This figure shows the percentage changes on business property tax burdens due to reassessments during the great recession period. The scheduled reassessment year are 2009, 2010, 2011, 2012, and 2013 for different reassessment groups.

Figure 1.6: Average Number of Establishments by Employee Size Class, Town Level



This figure shows the average number of establishments by employee size class at Connecticut's town level.

Table 1.1: The Impact of Changes in Tax Bases on Millage Rate and Revenues, 2009-2018

	(1)	(2)	(3)	(4)	(5)
	ln Millage rate	ln Tax revenue	ln Other loc. revenue	ln Intergv. revenue	ln Total revenue
ln V_{it}^{tot}	-0.9386***	0.0165	0.1774	0.0268	0.0388
	(0.0664)	(0.0371)	(0.1398)	(0.0833)	(0.0303)
Obs	1,690	1,690	1,690	1,690	1,690

All regressions are weighted by 2008 town-level population.
All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.
Standard errors in parentheses are clustered at town level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 1.2: The Impacts of Business and Residential Tax Bases on Revenues and Expenditures, 2009-2018

	(1)	(2)	(3)	(4)	(5)
	Δ Property tax pc.	Δ Other loc. revenue pc.	Δ Intergv. revenue pc.	Δ Total revenue pc.	Δ Total expend. pc.
ΔV_{it}^{B} pc.	2.9937	-0.5476	-0.3744	2.0717	3.1866
	(3.8705)	(0.8592)	(4.8540)	(2.2194)	(2.5790)
ΔV_{it}^{R} pc.	3.1738	-0.1865	-4.2070**	-1.2197*	-1.2103*
	(1.9495)	(0.1361)	(1.8750)	(0.6215)	(0.6866)
Obs	1,690	1,690	1,690	1,690	1,690

The unit for the assessment values is $1,000 per capita.
All regressions are weighted by 2008 town-level population.
All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.
Standard errors in parentheses are clustered at town level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 1.3: The Impact of Changes in Tax Revenue Sources on Categorical Public Spending, 2009-2018

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ ln Highway &Transport	Δ Highway &Transport pc.	Δ ln Public Safety	Δ Public Safety pc.	Δ ln Education	Δ Education pc.
Panel A: 1-year change [t,t-1]						
Δs_{it}^B	0.0423	-0.0790	0.1547	0.9648*	0.0528	1.0648*
	(0.1635)	(0.2159)	(0.0987)	(0.4933)	(0.0328)	(0.6091)
Obs	1,445	1,445	1,432	1,432	1,480	1,480
Panel B: 2-year change [t+1,t-1]						
Δs_{it}^B	-0.0040	-0.1240	0.0705	-0.3305	-0.0071	0.2395
	(0.2370)	(0.2130)	(0.1463)	(0.9143)	(0.1059)	(1.9149)
Obs	1,271	1,271	1,266	1,266	1,312	1,312
Panel C: 3-year change [t+2,t-1]						
Δs_{it}^B	0.0444	-0.1195	-0.0937	-0.1708	0.0743	1.8487
	(0.2178)	(0.2430)	(0.1747)	(0.9126)	(0.0826)	(1.4015)
Obs	1,109	1,109	1,103	1,103	1,144	1,144

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.4: The Impact of Business Tax Burdens on Number of Establishments, All Industries, 2009-2016

	(1)	(2)	(3)	(4)
	Δ ln number of est. all	Δ ln number of est. $<$ 10 empl.	Δ ln number of est. 10-49 empl.	Δ ln number of est. $\geq$ 50 empl.
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^{B}$	0.0040	0.0101	-0.0144	-0.0795
	(0.0106)	(0.0139)	(0.0354)	(0.0587)
Obs	1,312	1,312	1,312	1,312
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^{B}$	0.0043	0.0180	-0.0181	-0.0856
	(0.0161)	(0.0233)	(0.0462)	(0.0861)
Obs	1,148	1,148	1,148	1,148
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^{B}$	-0.0147	0.0030	-0.0554	-0.0386
	(0.0175)	(0.0234)	(0.0506)	(0.0721)
	984	984	984	984
Avg.est.	1246.05	880.93	291.21	73.90

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.5: The Impact of Business Tax Burdens on Number of Establishments, Manufacturing, 2009-2016

	(1)	(2)	(3)	(4)
	Δ ln number of est. all	Δ ln number of est. < 10 empl.	Δ ln number of est. 10-49 empl.	Δ ln number of est. ≥ 50 empl.
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^{B}$	-0.0317	-0.1335*	0.0482	-0.0987
	(0.0453)	(0.0685)	(0.1126)	(0.1185)
Obs	1,240	1,240	1,240	1,240
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^{B}$	-0.1138*	-0.2110**	-0.0743	0.0132
	(0.0628)	(0.0846)	(0.1633)	(0.1531)
Obs	1,085	1,085	1,085	1,085
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^{B}$	-0.1827**	-0.3885***	0.1820	-0.2188
	(0.0818)	(0.1291)	(0.1698)	(0.1374)
Obs	930	930	930	930
Avg.est.	54.96	30.09	17.81	7.06

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.6: The Impact of Business Tax Burdens on Number of Establishments, Retail, 2009-2016

	(1)	(2)	(3)	(4)
	Δ ln number of est. all	Δ ln number of est. $<$ 10 empl.	Δ ln number of est. 10-49 empl.	Δ ln number of est. $\geq$ 50 empl.
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^{B}$	-0.0529	-0.0361	-0.0774	-0.0075
	(0.0338)	(0.0469)	(0.0622)	(0.0689)
Obs	1,288	1,288	1,288	1,288
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^{B}$	-0.0459	-0.0635	-0.0517	0.0244
	(0.0489)	(0.0586)	(0.0975)	(0.0900)
Obs	1,127	1,127	1,127	1,127
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^{B}$	-0.0835*	-0.0752	-0.1263	-0.0207
	(0.0503)	(0.0597)	(0.0973)	(0.1025)
Obs	966	966	966	966
Avg.est.	174.96	122.32	42.86	9.77

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.7: The Impact of Business Tax Burdens on Number of Establishments, Finance and Professional Service, 2009-2016

	(1)	(2)	(3)	(4)
	Δ ln number of est. all	Δ ln number of est. < 10 empl.	Δ ln number of est. 10-49 empl.	Δ ln number of est. ≥ 50 empl.
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^B$	0.0316	0.0244	0.0132	-0.0911
	(0.0244)	(0.0355)	(0.1063)	(0.1318)
Obs	1,240	1,240	1,240	1,240
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^B$	-0.0186	-0.0096	-0.1048	-0.2639
	(0.0335)	(0.0392)	(0.1697)	(0.1614)
Obs	1,085	1,085	1,085	1,085
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^B$	-0.0275	-0.0156	-0.1839	-0.0740
	(0.0379)	(0.0471)	(0.1457)	(0.2027)
Obs	930	930	930	930
Avg.est.	228.11	176.26	41.19	10.66

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.8: The Impact on Quarterly Property Vacancies, Businesses, 2012-2018

	0-6 M. Vacancies		6 + M. Vacancies	
	(1)	(2)	(3)	(4)
	Δ ln Vacancy	Vacancy Rate	Δ ln Vacancy	Vacancy Rate
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^{B}$	0.3002	-0.0032	0.1763*	0.0252**
	(0.8694)	(0.0090)	(0.0912)	(0.0119)
Obs	1,162	1,162	1,162	1,162
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^{B}$	-0.3344	-0.0102	0.2612	0.0359*
	(1.4333)	(0.0138)	(0.1582)	(0.0198)
Obs	996	996	996	996
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^{B}$	1.4057*	0.00028	0.1875	0.0349*
	(0.8303)	(0.0099)	(0.1496)	(0.0197)
Obs	830	830	830	830

All regressions are weighted by 2008 town-level population.
All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.
Standard errors in parentheses are clustered at town level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 1.9: The Impact on Business Property Transactions, 2009-2018

	(1)	(2)
	Δ ln Tradi. sales	Δ ln Forecolsures
Panel A: 1-year change [t,t-1]		
$\%\Delta s_{it}^{B}$	0.1290	0.4490
	(0.5278)	(0.5278)
Obs	1,395	1,395
Panel B: 2-year change [t+1,t-1]		
$\%\Delta s_{it}^{B}$	0.2928	0.9584**
	(0.3023)	(0.4826)
Obs	1,247	1,247
Panel C: 3-year change [t+2,t-1]		
$\%\Delta s_{it}^{B}$	0.0059	1.0288**
	(0.2853)	(0.3023)
Obs	1,110	1,110

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.10: The Impact of Business Tax Burdens on Labor Markets, All industries, 2009-2017

	(1)	(2)	(3)	(4)
	Δ ln Employ.	Δ ln Agg. Wages	Δ ln Agg. Wages per emp.	Δ Empl. pc. × 100
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^{B}$	-0.0220*	-0.0608**	-0.0387	-0.0142*
	(0.0127)	(0.0307)	(0.0237)	(0.0074)
Obs	1,502	1,502	1,502	1,502
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^{B}$	-0.0514**	-0.0725**	-0.0212	-0.0291**
	(0.0204)	(0.0363)	(0.0242)	(0.0121)
Obs	1,334	1,334	1,334	1,334
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^{B}$	-0.0430**	-0.0847**	-0.0417*	-0.0273**
	(0.0205)	(0.0335)	(0.0252)	(0.0108)
Obs	1,168	1,168	1,168	1,168

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.11: The Impact of Business Tax Burdens on Labor Markets, Manufacturing, 2009-2017

	(1)	(2)	(3)	(4)
	Δ ln Employ.	Δ ln Agg. Wages	Δ ln Agg. Wages per emp.	Δ Empl. per capita × 100%
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^{B}$	-0.0919	-0.1671	-0.0741	-0.0033
	(0.0648)	(0.1122)	(0.0729)	(0.0023)
Obs	1,163	1,163	1,163	1,163
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^{B}$	-0.1409	-0.2898*	-0.1524*	-0.0022
	(0.0970)	(0.1550)	(0.0783)	(0.0029)
Obs	1,024	1,024	1,024	1,024
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^{B}$	-0.1056	-0.2576*	-0.1508**	-0.0017
	(0.1030)	(0.1441)	(0.0760)	(0.0036)
Obs	891	891	891	891

All regressions are weighted by 2008 town-level population.
All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.
Standard errors in parentheses are clustered at town level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 1.12: The Impact of Business Tax Burdens on Labor Markets, Retail, 2009-2017

	(1)	(2)	(3)	(4)
	Δ ln Employ.	Δ ln Agg. Wages	Δ ln Agg. Wages per emp.	Δ Empl. per capita × 100%
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^{B}$	-0.0232	-0.0134	0.0096	-0.0004
	(0.0333)	(0.0414)	(0.0278)	(0.0012)
Obs	1,394	1,394	1,394	1,394
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^{B}$	-0.0345	-0.0173	0.0174	-0.0006
	(0.0471)	(0.0641)	(0.0461)	(0.0018)
Obs	1,233	1,233	1,233	1,233
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^{B}$	-0.0378	-0.0252	0.0134	-0.0011
	(0.0472)	(0.0788)	(0.0652)	(0.0019)
Obs	1,072	1,072	1,072	1,072

All regressions are weighted by 2008 town-level population.
All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.
Standard errors in parentheses are clustered at town level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 1.13: The Impact of Business Tax Burdens on Labor Markets, Finance and Professional Services, 2009-2017

	(1)	(2)	(3)	(4)
	Δ ln Employ.	Δ ln Agg. Wages	Δ ln Agg. Wages per emp.	Δ Empl. per capita $\times$ 100%
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^{B}$	0.0089	-0.0047	0.0099	-0.0024
	(0.0415)	(0.0619)	(0.0438)	(0.0033)
Obs	1,174	1,174	1,174	1,174
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^{B}$	-0.1825*	-0.1714	0.0156	-0.0106
	(0.0934)	(0.1280)	(0.0528)	(0.0068)
Obs	1,035	1,035	1,035	1,035
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^{B}$	-0.1275	-0.2316*	-0.0849	-0.0126*
	(0.0834)	(0.1289)	(0.0745)	(0.0065)
Obs	899	899	899	899

All regressions are weighted by 2008 town-level population.
All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.
Standard errors in parentheses are clustered at town level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 1.14: The Impact on Quarterly Residential Property Vacancies, USPS, 2012-2018

	0-6 M. Vacancies		6 + M. Vacancies	
	(1)	(2)	(3)	(4)
	Δ ln Vacancy	Vacancy Rate	Δ ln Vacancy	Vacancy Rate
Panel A: 1-year change [t,t-1]				
$\%\Delta s_{it}^{R}$	-0.8994	-0.0004	0.1145	-0.0026
	(1.3833)	(0.0047)	(0.1611)	(0.0065)
Obs	1,162	1,162	1,162	1,162
Panel B: 2-year change [t+1,t-1]				
$\%\Delta s_{it}^{R}$	0.9891	0.0031	0.0625	-0.0089
	(2.4644)	(0.0054)	(0.2439)	(0.0097)
Obs	996	996	996	996
Panel C: 3-year change [t+2,t-1]				
$\%\Delta s_{it}^{R}$	-0.3618	0.0036	0.0048	0.0050
	(1.3559)	(0.0053)	(0.2323)	(0.0142)
Obs	830	830	830	830

All regressions are weighted by 2008 town-level population.
All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.
Standard errors in parentheses are clustered at town level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 1.15: The Impact on Residential Property Vacancies, ACS, 2012-2018

	Vacant for sale		Vacant for Rent		Total	
	(1)	(2)	(3)	(4)	(5)	(6)
	Δ ln Vacancy	Vacancy Rate	Δ ln Vacancy	Vacancy Rate	Δ ln Vacancy	Vacancy Rate
Panel A: 1-year change [t,t-1]						
$\%\Delta s_{it}^R$	-0.4297	-0.0025	-0.2248	0.0030	-0.0860	0.0005
	(0.6773)	(0.0052)	(0.4217)	(0.0088)	(0.2911)	(0.0117)
Obs	1,352	1,352	1,352	1,352	1,352	1,352
Panel B: 2-year change [t+1,t-1]						
$\%\Delta s_{it}^R$	0.3763	0.0003	1.2311	0.0151	0.2550	0.0154
	(1.0351)	(0.0073)	(0.9516)	(0.0106))	(0.4633)	(0.0150)
Obs	1,183	1,183	1,183	1,183	1,183	1,183
Panel C: 3-year change [t+2,t-1]						
$\%\Delta s_{it}^R$	1.2673	0.0119*	-0.1312	0.0087	0.3554	0.0206
	(1.0548)	(0.0067)	(0.7373)	(0.0151)	(0.6024)	(0.0182)
Obs	1,014	1,014	1,014	1,014	1,014	1,014

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.16: The Impact on Residential Property Transactions, Single Family House, 2009-2018

	(1)	(2)
	Δ ln Tradi. sales	Δ ln Forecolsures
Panel A: 1-year change [t,t-1]		
$\%\Delta s_{it}^R$	0.1578	-0.7403
	(0.1147)	(1.0952)
Obs	1,690	1,690
Panel B: 2-year change [t+1,t-1]		
$\%\Delta s_{it}^R$	0.0491	-0.6587
	(0.1626)	(1.3959)
Obs	1,521	1,521
Panel C: 3-year change [t+2,t-1]		
$\%\Delta s_{it}^R$	-0.2237	-0.5436
	(0.2039)	(2.0454)
Obs	1,352	1,352

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Appendices

1.A Growth of Business Tax Shares

Let V_t^B and V_t^R be the assessed values of business and residential properties. The millage rate is m_t. The taxes paid by a business property owner will be $T_t^B = V_t^B \times m_t$ with residential taxes similarly defined. As there is only one millage rate in Connecticut, applying to both businesses and residences, the share of tax paid by businesses is a function of assessed values, $s_t^B = \frac{V_t^B}{V_t^{tot}}$, with the millage rate canceling out. The growth of s_t^B is therefore given by:

$$s_t^B / s_{t-1}^B = \frac{V_t^B}{V_t^B} \frac{V_{t-1}^{tot}}{V_t^{tot}} \tag{1.10}$$

The last term decomposes as follows:

$$V_t^{tot} / V_{t-1}^{tot} = g_t^B (1 - s_{t-1}^R) + g_t^R s_{t-1}^R \tag{1.11}$$

where g_t^B is the growth in businesses property assessments: V_t^B / V_{t-1}^B. So the growth in the overall variable is the sum of the growth of the components, weighted by their shares. In conjunction with equation (8), this reveals:

$$s_t^B / s_{t-1}^B = \frac{1}{1 + s_{t-1}^R (\frac{g^R}{g^B} - 1)} \tag{1.12}$$

Taking logs, this is approximately

$$log(\frac{s_t^B}{s_{t-1}^B}) \approx s_{t-1}^R (1 - \frac{g_t^R}{g_t^B}) \tag{1.13}$$

Therefore, the growth rate in the share that businesses pay is approximately equal to the residential share in the town multiplied by one minus the relative growth rates in assessed values.

Table 1.A: The Impact of Changes in Business Tax Shares on Local Revenues, 2009-2018

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ ln Millage rates	Δ ln Property taxes	Δ ln Other loc. revenue	Δ ln Intergov. revenue	Δ ln Total revenue	Δ ln Total expenditure
Δs_{it}^{B}	1.5437***	0.0452	-0.3840	-0.5649	-0.1661	-0.1836
	(0.3249)	(0.0762)	(0.3567)	(0.5014)	(0.1332)	(0.1652)
Obs	1,690	1,690	1,690	1,690	1,690	1,690

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.B: Distribution of Establishments by Employee Size and Industry

NAICS (2-digit)	Industry Description	% of est. (1-9 employees)	% of est. (10-49 employees)	% of est. (50-99 employees)	% of establishments (100+ employees)
11	Forestry, Fishing, Hunting, and Agriculture Support	90.86%	9.01%	0.08%	0.00%
21	Mining	59.54%	35.87%	3.52%	1.07%
22	Utilities	34.06%	34.90%	17.93%	13.11%
23	Construction	88.18%	10.47%	0.89%	0.47%
31	Manufacturing	60.17%	26.61%	7.03%	6.20%
42	Wholesale Trade	76.81%	19.00%	2.38%	1.83%
44	Retail Trade	73.34%	22.14%	2.12%	2.40%
48	Transportation and Warehousing	59.54%	29.67%	7.15%	3.64%
51	Information	67.08%	26.80%	3.58%	2.55%
52	Finance and Insurance	84.28%	13.26%	1.44%	1.02%
53	Real Estate and Rental and Leasing	92.12%	7.14%	0.46%	0.29%
54	Professional, Scientific, and Technical Services	85.29%	12.93%	0.98%	0.80%
55	Management of Companies and Enterprises	52.90%	26.19%	8.31%	12.60%
56	Administrative and Support and Waste Management and Remediation Services	82.99%	13.22%	1.97%	1.82%
61	Educational Services	65.43%	22.75%	6.53%	5.29%
62	Health Care and Social Assistance	63.30%	29.92%	2.99%	3.79%
71	Arts, Entertainment, and Recreation	72.02%	22.54%	3.43%	2.01%
72	Accommodation and Food Services	60.26%	35.56%	3.43%	0.75%
81	Other Services (except Public Administration)	87.37%	11.93%	0.52%	0.18%
99	Unclassified	99.39%	0.61%	0.00%	0.00%

Note: The estimation is based on information from County Business Pattern database

Table 1.C: Placebo Test: Testing for Preexisting Trends in Main Dependent Variables

Dependent Variable	Explanatory Variable	Coefficient	Standard error
Panel A: Business Exit [t-1, t-2]			
Δ ln (no.est.) mfr., 1-9	$\%\Delta s_{it}^{B}$	0.0564	(0.0858)
Δ Vacancy rate, 6+m	$\%\Delta s_{it}^{B}$	0.0054	(0.0010)
Δ ln (Foreclosure)	$\%\Delta s_{it}^{B}$	0.0075	(0.5752)
Panel B: Employment and Wages [t-1, t-2]			
Δ ln (Employment)	$\%\Delta s_{it}^{B}$	-0.0036	(0.0635)
Δ ln (Tot. Wages)	$\%\Delta s_{it}^{B}$	0.0627	(0.1092)
Δ ln Tot. Wages per emp.	$\%\Delta s_{it}^{B}$	2.2081	(2.5131)
Δ Emply. pc. $\times 100\%$	$\%\Delta s_{it}^{B}$	0.0007	(0.0020)

All regressions are weighted by 2008 town-level population.

All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.

Standard errors in parentheses are clustered at town level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.D: Robustness Test: Testing for Preexisting Trends in Main Dependent Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ ln Employment		Δ ln Agg. Wages		Δ ln Agg. Wages per emp.		Δ Emply. pc. ×100	
1-year change	-0.0220*	-0.0209	-0.0608**	-0.0698**	-0.0387	-0.0487*	-0.0142*	-0.0117
	(0.0127)	(-0.0145)	(0.0307)	(0.0306)	(0.0237)	(0 .0237)	(0.0074)	(0.0079)
Obs.	1,502	1,502	1,502	1,502	1,502	1,502	1,502	1,502
2-year change	-0.0514**	-0.0473**	-0.0725**	-0.0889**	-0.0212	-0.0415	-0.0291**	-.0254*
	(0.0204)	(0.02278)	(0.0363)	(0.0416)	(0.0242)	(0.0273)	(0.0121)	(0.0132)
Obs.	1,334	1,334	1,334	1,334	1,334	1,334	1,334	1,334
3-year change	-0.0430**	-0.0520**	-0.0847**	-0.1244***	-0.0417*	-0.0725**	-0.0273**	-0.0302**
	(0.0205)	(0.0249)	(0.0335)	(0.0413)	(0.0252)	(0 .0277)	(0.0108)	(0.0146)
Obs.	1,168	1,168	1,168	1,168	1,168	1,168	1,168	1,168
Town FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Town-specific trend		Y		Y		Y		Y

All regressions are weighted by 2008 town-level population.
All regressions are controlled with fraction of female, fraction of white, fraction of population over 65, median household income, fraction of higher education, percent of people below poverty line, year fixed effects and town fixed effects.
Standard errors in parentheses are clustered at town level.
*** p<0.01, ** p<0.05, * p<0.1.

Chapter 2

Local Government Response to Fiscal Squeeze: Evidence from a Targeted Tax Reduction[1]

[1]This chapter is co-authored work with Jijian Fan

2.1 Introduction

The flypaper effect suggests that government responds differently to local income and intergovernmental subsidies, and is one of the most widely-documented anomalies in public finance (Henderson, 1968; Courant et al., 1978). Counter to traditional economic models which predict an equivalent response to local income and intergovernmental transfers, subsidies tend to generate a greater increase in government expenditure (Courant et al., 1978; Hines and Thaler, 1995). While a large literature has studied the effects of grant and income shocks from various sources, there is less research on the effects of shortfalls stemming from a targeted tax reduction with a designated benefited group, and little consensus about the magnitude and mechanisms of the flypaper effect and consequent economic impacts. This study considers these questions by examining the impact of an agricultural tax reduction on county-level government revenue, expenditure, and agricultural production and income. Specifically, we estimate how counties with fiscal shortfall adjust other sources of revenue, alter spending across categories, and examine how these responses impact agricultural output and farmers' net income. Understanding how local governments respond to fiscal shortfalls and alter expenditures will shed light the optimal budget policy.

The flypaper effect, originally noted by Henderson (1968) and Courant et al. (1978), describes the differential reaction of local government to income from citizens relative to an intergovernmental subsidy. Government expenditure may exhibit a high correlation with the source of revenue, even if local government could allocate it else-

where. Early studies investigated the overall expenditure change and found a crowd-in effect for intergovernmental subsidies, that is, local governments increase spending rather than reducing taxes. While a number of studies find evidence of a strong flypaper effect (Dahlberg et al., 2008; Allers and Geertsema, 2016; Suárez Serrato and Wingender, 2016; Kakamu et al., 2014), others find weak or mixed evidence (Becker, 1996; Knight, 2002; Darby et al., 2005; Brunner and Schwegman, 2017). Recent studies refine money stickiness to examine whether subsidies for specific budget categories generate disproportionate spending increases in those areas, and find evidence that revenues earmarked for health (Levaggi and Zanola, 2003), road work (Leduc and Wilson, 2017), and administration (Berg and Rattsø, 2007) tend to boost corresponding expenditure. However, Knight (2002) and Gordon (2004) find crowd-out effects on local infrastructure and education spending, respectively. The anomaly of the flypaper effect may be explained by collective decisions (Roemer and Silvestre, 2002), fiscal illusion (Turnbull, 1992), behavioral responses (Hines and Thaler, 1995), risk aversion and insurance (Vegh and Vuletin, 2015), or unobserved political preferences (Knight, 2002, 2004; Inman, 2008). Few studies have focused on the social and economic impact of the flypaper effect (Leduc and Wilson, 2017). In this study, we examine the overall and categorical flypaper effects driven by a policy-induced fiscal shortfall from a targeted agricultural tax break, and evaluate the consequent economic impact on agricultural production and farmer income.

There are a number of challenges in identifying the effect of an intergovernmental subsidy on local spending and economic outcomes. First, it is difficult to find

truly exogenous shocks to local revenue, as the amount of intergovernmental subsidy is usually not randomly determined, and is likely to correlate with unobservables at the local level (Knight, 2002; Inman, 2008). Previous studies address this issue by using various identification strategies such as regression discontinuity based on a grant formula (Dahlberg et al., 2008), political power of congressional delegations as an instrumental variable (Knight, 2002), and census calibration changes (Gordon, 2004; Suárez Serrato and Wingender, 2016). Second, local governments are inherently heterogeneous in terms of their population characteristics, natural endowments, and socioeconomic conditions, hence it is difficult to find similar local governments to use as a counterfactual. In order to generate an unbiased estimate of the impact of a specific policy, we need an observable, exogenous shock that is imposed on otherwise similar local governments.

This paper examines the effect of fiscal squeeze caused by a targeted tax reduction. In 2004, the central government of China passed a law to abolish all agricultural taxes in order to benefit the country's 900 million farmers and promote agricultural production. This tax reduction prohibits county governments from collecting agricultural taxes as one of their sources of income. As compensation, county governments were subsidized by a designated transfer from the central government, and the amount of the subsidy was determined at the province level: depending on whether a province is coastal and a major-grain producer, they receive either 0%, 50%, 80% or 100% replacement of pre-policy agricultural tax revenue. Thus, we are able to compare very similar counties that are adjacent to each other geographically but received substantially different replacement subsidies. We consider local government response in terms of revenue,

expenditure, and agricultural outcomes using a difference-in-differences design.

To allow for heterogeneous effects, we differentiate by per-capita gross product, agricultural dependence, and the political experience of county governors. To conduct the analysis, we create a merged data set from The County Public Finance Statistics Yearbook of China, China Regional Economic Statistics Yearbook, China County Social and Economic Statistical Yearbook, and a manually collected county politician profile data set that includes county governors' age, tenure, and level of education.

We find that counties on both sides of the province border follow a similar government budget trajectory prior to the agricultural tax cancellation in 2004. However, after policy implementation, when counties can no longer collect agricultural taxes, those receiving incomplete subsidies experienced a significant reduction of 45 yuan per capita, which is an 81 percent reduction in agricultural revenue, or a 10 percent reduction in overall local government revenue. In the years following policy implementation, we find that tax revenues from other sources (such as sales tax and value-added tax) and intergovernmental subsidies remain unchanged in shortfall counties relative to their neighbors. This finding is consistent with Dye and McGuire (1997), Darby et al. (2005), Bradbury and Case (2001), and Jonas (2012), each of whom find that local government does not alter taxes when there is a fiscal surplus or shortfall. Given the balanced budget requirement, the significant reduction confirms a clear first stage on revenue shortfall, and exhibits a nearly perfect overall flypaper effect.

The consequent change in government expenditure may reflect the flypaper effect in terms of categorical stickiness. Specifically, we find that the per-capita agri-

cultural spending of counties without full revenue replacement is reduced by 18 yuan, or 36 percent, which is much larger than the reductions in other categories (6 percent on average). This is not consistent with the predictions of traditional models, which suggest for proportional reductions. (Henderson, 1968; Hines and Thaler, 1995). This large reduction on local agricultural expenditure shows that, at the category level, local governments reduce the extent to which the benefits of the tax reduction accrue to the targeted group. However, the fact that agricultural expenditure reduction is less than revenue reduction still implies partial stickiness of the targeted money, which is consistent with Leduc and Wilson (2017) and Levaggi and Zanola (2003). In addition to examining the effect on agriculture, this natural experiment allows us to consider which other categories of spending are most affected by a shortfall. Apart from the agricultural spending reduction, we find significant reductions in social security spending (11.4 percent), short-run liquidity reserves (12.5 percent), and a catch-all category comprised of non-profitable public sector subsidies including designated pension and relief fees.

We examine the consequent effects of agricultural expenditure reduction on agricultural production and farmers' income. While government spending on agriculture is mostly used for fixed investment and technology promotion, the reduction on spending may also have a negative impact on agricultural production, but examining the relative changes in agricultural factor inputs (cultivated land area, agricultural labor force, and its share among population) and agricultural outputs (output of grain crops, oiling crops, and meat product) reveals no significant changes in agricultural production when counties receive incomplete subsidies. However, we find that farmers' net income in less-

subsidized counties is significantly less than that of fully subsidized counties after the agricultural tax cancellation. Net income refers to revenue from agricultural production less the costs of production minus taxes and fees. Given similar levels of agricultural tax relief, similar levels of agricultural output, and no de facto tax increase from other sources, the explanation for the income decline may be that individual agricultural producers face increased costs of production. Thus, the effect of reduced government agricultural 32 expenditure may result in higher costs of production and lower net income.

This paper contributes to the literature in several ways. First, we address the effect of a fiscal shortfall driven by a policy change instead of changes in local income or wealth (Darby et al., 2005; Dye and McGuire, 1997; Jonas, 2012). Second, as the fiscal shock is due to a targeted tax break in agriculture, we can investigate the categorical flypaper effect for the agricultural sector, enriching the existing literature by considering the extent to which local government redistributes away from the intended beneficiaries (Levaggi and Zanola, 2003; Leduc and Wilson, 2017; Berg and Rattsø, 2007). Finally, by examining the impacts on agricultural outcomes and farmers' net income, we are able to estimate the impact of local government responses on the targeted group.

The paper is organized as follows. Section 2.2 presents background information about the revenue and expenditure system in China, the agricultural tax, and its cancellation. Section 3.3 describes the construction of the data set on local revenue, expenditure, agricultural outcomes, and other local economic variables. Section 2.4 presents the empirical strategy and methods of testing its validity. Section 2.5 and 2.6

present the results of local government revenue and expenditure changes, showing the overall and categorical flypaper effects. Section 2.7 shows the consequent effects on the inputs and outputs in agricultural production, and farmer net income. Section 2.8 shows the heterogeneous effects, and Section 2.9 concludes.

2.2 Background

2.2.1 Revenue and Expenditure System

China, one of the largest countries in the world, has 34 provinces, 333 prefectures, and 2,862 counties.[2] County government is the lowest level of government that has independent authority, above which there are prefecture, provincial, and central governments.[3]

Tax revenue and other government income are shared between central and local (including county, prefecture, and provincial) governments by methods detailed in the Tax Sharing System (TSS). This system classifies three types of taxes: central taxes, local taxes, and shared taxes. Examples of each type of tax can be found in Table 2.1.[4] While part of central government tax revenue is distributed as subsidies for local governments, all revenue from local personal income, land use, property, and agricultural taxes, goes directly to the local budget. In 2000, local revenue accounted for

[2]Data comes from the China government website in 2014. The numbers include equivalent administrative divisions, for example, the number of provinces includes number of autonomous regions and municipalities.

[3]There are also 44,741 township-level governments below the counties, however, due to their dependency on county government, few fiscal studies had focused on township government behavior.

[4]The detailed tax sharing is listed in Appendix Table 2.A.

half of the local budget, while the rest came from central subsidies.[5] In less developed areas, central subsidies account for an even higher fraction of revenue. For example, in our sample, which does not include wealthy coastal cities, intergovernmental transfers finance about two-thirds of local budgets.

In conjunction with revenue sharing between central and local governments, public expenditure in China is also shared. Local governments have high levels of expenditure responsibility that exceed those in most countries. In most countries, it is the central government's responsibility to provide social security and welfare, while education and health are often shared between state/provincial and central governments. In China, county and township governments provide the majority of public services, including 70 percent of budgetary expenditures for education, and 60 percent of those for health (Bank, 2002). As of 2006, China did not have national social security legislation. At that time, most social security revenue (pensions and medical insurance, etc.) was also administrated at the county level.[6] Appendix Table 2.B shows the assignments of expenditure responsibilities between central and local governments from 2003 to 2006. It shows that local governments have primary responsibility for most public services, including agriculture, education, and public health.

[5]Including the local share from shared tax revenue.

[6]The central government, for instance, the Ministry of Labor and Social Security, may provide general guidance and ensure that local regulations follow central government guidelines. Provincial or city/county social insurance agencies administrate their respective social security pools and individual accounts. Provincial labor and social security authorities are responsible for regulatory funds to which locally pooled funds in the jurisdiction must pay a percentage of their revenue. Notably, all revenues and expenditures of social security are managed under a specific account called "Social Security Fund", and the fund has an independent budget which is separately prepared from the general budget. Moreover, governments always subsidize the funds with general public budget to make ends meet.

2.2.2 Agricultural Tax

China has long been an agricultural nation, and agricultural taxes have existed for over 2,600 years. All individuals and organizations who receive revenue from certain agricultural products are required to pay the tax. The agricultural tax is a proportional tax on perennial production under normal climate, soil, and cultivating conditions. Therefore, by definition, it is a lump-sum tax based on past production levels instead of current revenue (Wang and Shen, 2014). Counties are the smallest administrative division in China that has authority to determine the agricultural tax rate and collect agricultural taxes. The statutory highest agricultural tax rate by the central government is 15.5% , but the average tax rate in practice is 8.4%.[7] As all agricultural taxes go to county governments, in this paper, we study the policy effects on county-level governments.

In the 1950s, when the People's Republic of China was newly founded, agricultural taxes accounted for more than 40% of total government revenue. While the importance of agricultural tax revenue has been decreasing during recent decades, it still accounts for 30% of local revenue for counties and 10% of total revenue. Nevertheless, the implementation of agricultural taxes has potentially harmful implications. First, agricultural taxes may discourage agricultural production. Second, the burden of the agricultural producer may be high: in some provinces, aggregate tax burdens on farmers are as high as 30 percent, which is higher than for the nation's highest earners (Chen,

[7]The statutory tax rate is based on the historical production level, while the practical rate is calculated on the actual production level.

2003). Third, agricultural taxes collected in less-developed areas will aggravate spatial inequality, which is a critical issue in China, and accelerate conflicts between farmers and local governments (Bernstein and Lü, 2000; Lin et al., 2007).

In 2004, in order to encourage agricultural production, reduce farmers' economic burdens, and narrow spatial development gaps, China's central government decided to abolish agricultural taxes.[8] The nationwide agricultural taxes were immediately reduced.[9] In 2006, all local governments had fully stopped collecting agricultural taxes. This policy affected 900 million Chinese farmers, exempting them from over 100 billion yuan in agricultural taxes. Previous studies on the abolishment of China's agricultural taxes focus on agricultural productivity and the economic outcomes for farmers (Xu and Wang, 2009; Wang and Shen, 2014), but find little effects.

As agricultural tax was one of the major sources of local government revenue in China, and all revenue from it went to local governments, the abolishment could lead to large local deficits, especially for agriculture-dependent counties. In order to offset this deficit, China's central government offered a permanent, annual lump-sum grant called "Subsidy for Agricultural Tax Cancellation" to local governments. This grant is differentiated by province so that counties in some provinces got full offset while others did not. The amount of central government subsidy is determined by each county's 2002

[8]The agricultural taxes being canceled included three types of taxes: *Agricultural Tax*, *Agricultural Specialty Tax* and *Livestock Tax*. Other relevant taxes, including *Land Occupation Tax*, *Tobacco Product Tax* and *Deed Tax*, even if accounted under the category of "Agricultural Taxes", are still in effect. Therefore, the value of revenue categorized as "agricultural tax" does not fall to zero after the abolishment.

[9]In the beginning of 2004, the agricultural specialty tax was immediately canceled, and the tax rate of agricultural tax was reduced by three percentage points for MGP provinces and one percentage point for non-MGP provinces.

agricultural tax revenue, but varies in compensation rate. The subsidy rate from the central government was set as: a) 100% for major-grain producing (MGP) provinces in central and western regions, b) 80% for non-MGPs in central and western regions, c) 50% for MGPs in eastern (coastal) regions, and d) 0% for non-MGPs in eastern (coastal) regions.[10] We acquire the level of total offsetting subsidies from both central and provincial governments, and categorize provinces by whether this total subsidy is equal to or less than the baseline level. Figure 2.1 shows the geographical distribution of provinces with balanced revenue and with net revenue loss due to the abolishment.[11] As the figure shows, there is a clear border, meandering from the Southwest to East Coastal Area, that separates areas that got full compensations and those that did not.

2.3 Data

The data used in this study comes from multiple sources. The primary data is converted from The County Public Finance Statistics Yearbook of China. Two more data sets, the China Regional Economic Statistics Yearbook and the China County Social and Economic Statistical Yearbook, provide county-level economic variables. In addition, we manually collect a data set of the background and careers of counties' local

[10] However, provincial governments may also provide subsidies to county-level governments. This happens in areas with incomplete subsidies, and thus reduces the policy differences induced by central policy. When we define less-subsidized counties, we focus on the 50% and 0% cases since there are no de facto first stage differences between 100% and 80% provinces.

[11] Precisely speaking, given the increasing trend in agriculture tax revenue, counties getting full subsidies of 2002 agricultural tax revenue will still expect relative fiscal losses after 2004. We focus on the loss of insufficient baseline subsidy, i.e., the missing part in 2002 baseline subsidy, assuming that counties in different provinces had same trajectories of agricultural tax revenue if the agricultural taxes were not canceled.

governors.

The primary data set used for analysis is The County Public Finance Statistics Yearbook of China, for the years 2001 to 2006. This yearbook includes government revenues and expenditures at the county level. The data set is available in the form of print books through Department of Budget, Ministry of Finance of China. We manually digitized the yearbooks from 2001 to 2006 by scanning each page and then using Optical Character Recognition (OCR) software to convert scanned images to spreadsheets. We exploit the balancedness of the budget to manually check for and correct errors that occurred during data conversion.[12]

The resulting data reveal revenue sources: agricultural revenue (including agricultural tax revenue and the subsidy for agricultural tax cancellation after 2004), other non-agricultural revenue (including revenue from value-added taxes, sales taxes, city maintenance taxes, local personal income taxes, firm taxes, and other non-tax local revenue such as fees), and subsidies (including all intergovernmental subsidies, except for the agricultural subsidy mentioned above). Importantly, the yearbooks also include expenditures, including: agriculture (forestry, livestock and fishing are included), infrastructure, education, governmental administration, social security, public safety, miscellaneous, and changes in reserves. A detailed description of expenditures can be found in Section 2.6.

To study the effects of the cancellation of agricultural tax on outcomes such as

[12] A technical notes regarding error fixing in OCR can be found in Appendix Section 2.A. There are other studies, such as Yin and Zhu (2012), using the same data set with a shorter year span from The County Public Finance Statistics Yearbook, however, the quality of data is limited and thus is not adopted by our study.

agricultural production and farmers' income, we also use data from the China Regional Economic Statistics Yearbook. This book is available in electronic form, and we extract variables that capture agricultural inputs and outputs, such as agricultural labor force, area of cultivated land, farmers' income, and the amount of agricultural production (grain crops, oil crops, and meat product). The third data set used in this study is the China County Social and Economic Statistical Yearbook. This data set is also in electronic form, and provides information on both the production and welfare sectors, such as by-sector gross regional product for each county.

Furthermore, to study how local governors may impact the revenue and expenditure changes when the agricultural taxes are canceled, we manually collect information for county-level officials. We focus on the top local governor (County Party Secretary, i.e., Xian Wei Shu Ji) for counties located on province borders in 2004, and collect their age, education level, the time when they started to serve as County Party Secretary, and whether they were promoted afterwards.[13] This information was collected from the Baidu Encyclopedia, which is a commonly used website that contains profiles for noteworthy individuals. For officials not included in the Baidu Encyclopedia, we searched county newspapers, and made phone calls to county governments. Our data is new, as there are no existing data sets on county-level governors. Existing data sets, such as Jiang (2018), are focused on profiles of prefecture or higher level governors.[14]

[13]The border counties will be discussed in Section 2.4.

[14]In China, officials in prefecture or higher level are counted as high-level officials. Their profiles are managed and supervised by the central government, so access to these profiles is readily available. However, county-level officials are counted as low-level officials, and their profiles are not uniformly managed.

We capture per-capita values by dividing each county's revenue and expenditure by its population in the corresponding year. The variable "Other Income" is the summation of per-capita local revenues except for revenue from agricultural taxes. We also keep track of its sub item, value added tax and sales tax, in order to compare with previous studies that find income substitution from sales and value-added taxes (Burge et al., 2012; Zhao and Hou, 2008; Chen, 2017). The variable "Other Intergovernmental Subsidy" is the summation of per-capita subsidies. We do not provide the sub-items, as their categories are changing over time during the sample period.

2.4 Empirical Framework

In this section, we detail the empirical strategy used to estimate the effect of the targeted agricultural tax cancellation and incomplete subsidy on county fiscal outcomes on revenue and expenditure. We introduce a baseline difference-in-difference design first, discuss the shortcomings of this approach, and then develop a border design and present evidence of its validity.

2.4.1 Baseline Difference-in-Difference Design

The endogeneity of grant levels is a common issue in the literature, and it is resolved in different ways, such as using power of congressional delegations or census calibration changes as instruments (Knight, 2002; Gordon, 2004; Suárez Serrato and Wingender, 2016), or exploiting discontinuities in grant determination (Dahlberg et al., 2008). This paper exploits an exogenous policy difference in compensation rates across

provinces. A baseline version of the specification can be written as follows:

$$Outcome_{it} = \beta_0 + \beta_1 Post04_t + \beta_2 Incomplete_i + \beta_3 Post04_t \times Incomplete_i + \vec{\eta}\vec{X}_{it} + \epsilon_{it} \quad (2.1)$$

where the subscript i and t denote county and year respectively. The *Outcome* represents a county's revenue from agricultural tax and subsidy, revenue from other local sources, intergovernmental subsidies, and expenditure on agriculture, infrastructure, education, social security, administration, public safety, and change in financial resources from special government funds. $Post04$ is a dummy variable that takes value 1 if the year is equal to or after 2004 when the policy shock occurred, and *Incomplete* equals to 1 if the county is in treatment group, i.e., in those provinces that are expected to have insufficient subsidy due to the policy. We also add other county-level control variables $\vec{X}$, including county population, and the per-capita regional domestic product in each sector. The coefficient β_1 captures the changes in the outcome between the pre period (2001–2003) and the post period (2004–2006). β_2 captures the initial difference between county governments on each side of the treatment border. Our interest is in the coefficient β_3, which reveals the effect of an incomplete subsidy.

However, simply using a difference-in-difference design that includes all counties in all provinces of China may generate biased estimates. As shown in Figure 2.1, provinces that receive insufficient subsidies are, by design, located in coastal areas, with high levels of regional development. Wealthier counties may have fundamentally different trends in revenue, expenditure, farmer's income, and other, unobserved factors over time than the poorer, more agricultural counties. To generate more credible estimates,

we implement a border design.

2.4.2 Border Design

China has more than 2,000 counties with diverse geographic and socioeconomic conditions. We strengthen our design by restricting the sample to adjacent counties on either side of provincial borders. The border design has been implemented in various studies, such as Card and Krueger (2000); Dube and Reich (2010); Duranton et al. (2011); Thompson and Rohlin (2012); Turner and Blagg (2017). Counties on the border of adjacent provinces are likely to be similar in terms of natural and socioeconomic conditions, such as population density, geographical endowment, road system, economic development, etc. They may also experience similar shocks to weather and economic conditions. The identification strategy is valid under the assumption that the pre-post change on each side of the border would be similar in the absence of the change in tax law. This assumption would be violated if any other policy changes are implemented at the same time and introduce asymmetric effects across the border, or if there exists different trends in outcome variables for treated and control counties. This results in a sample of 1,002 observations from 168 counties over 6 year.

As an alternative design, we extend the definition of "border counties" to include counties that are contiguous to border counties. This extended sample includes 1,855 observations from 311 counties. Using the extended sample helps control for geographic trend. Further discussion about this extended sample and matching results can be found in Section 2.5.3.

We estimate the following equation using restricted border contiguous sample and extended border sample, respectively:

$$Outcome_{it} = \delta_i + \gamma_t BorderSegment_i + \beta_3 Post04_t \times Incomplete_i + \vec{\eta}\vec{X}_{i,t} + \epsilon_{it} \tag{2.2}$$

Pre-existing county differences are absorbed by county fixed effects. We include year-by-segment fixed effect as suggested in Dube and Reich (2010). As the province border extend across all of Southern China Mainland, the differences between the east and west counties can be large. Having year-by-segment fixed effects allow for locally varied year effects, reducing the potential for bias, and generating greater precision in estimation. A segment is defined as a section of border that separates two distinct provinces. There are 15 segments along the border. Counties are clustered into segments exclusively. At the junction area of multiple provinces, if a county is contiguous to more than one province, it is clustered to whichever is closer to the county administrative center. The standard errors are clustered on province level, assuming error independence across provinces. (Cameron and Miller, 2015).

We further estimate the year-by-year specification using restricted border contiguous sample:

$$Outcome_{it} = \delta_i + \gamma_t BorderSegment_i + \sum_{k=2004}^{2006} \beta_k \mathbf{1}(Year = k) Post04_t \times Incomplete_i + \vec{\eta}\vec{X}_{it} + \epsilon_{it} \tag{2.3}$$

This alternative specification may show the partial implementation of the policy, and capture any effects faded or increased over time.

2.4.3 Design Validity

We define counties as treated if they are located in provinces that, by national law, are set to receive less than a full offsetting subsidy, and counties as part of the control group if they are in provinces that re provided with full compensation. Identification is based on contiguous counties that are on opposite sides of the provincial borders, as shown in Figure 2.1.[15] This results in a sample with 86 counties in 8 provinces as the control group and 82 counties in 7 provinces as the treatment group. Table 2.2 shows that counties on either side of the border are quite similar in terms of population and gross regional product in each sector before the agricultural tax abolishment (2001—2003).[16] The overall fiscal structures for treated and control counties are also similar: while the majority revenue (70 percent of total revenue) comes from intergovernmental subsidies, agricultural tax income accounts for about 7 percent of total revenue. The shares of other income and expenditure are similar as well.[17] Appendix Figure 2.A and 2.B show the distribution of key variables. These figures show that counties in the border sample are more similar than those in full sample.

To confirm that contiguous counties on opposite sides of the border follow the same trajectories prior to the policy, we implement a series of timing placebo tests by estimating Equation 2.2 using observations in pre- and post-treatment periods, but a

[15]The distribution of counties inn the extended sample is shown in Figure 2.2. In China, county-level administration unit includes *district*, *county-level city*, and *county*. A *district* is part of a city which is most developed, and a *county-level city* is smaller and less industrialized than a city, but larger and developed than a typical county. We exclude these two and only study *counties*.

[16]For comparison, in Appendix Table 2.C we present the summary statistics for all sample.

[17]The share of intergovernmental subsidy is large in our sample, mainly as a consequence of the fact that most of border counties are relatively poor and less developed.

false treatment time dummy one year before and two years after the actual year of agricultural tax abolishment. Likewise, we implement a geographical placebo test using the true timing of the policy, but false borders within provinces with and without full subsidy replacement. These placebo tests help to rule out the cases that the effect comes from a time or geographical trend.

2.5 The Effect of a Subsidy Shortfall on Local Revenue

The agricultural tax cancellation along with different fiscal subsidy creates an exogenous shock to county government budget. In this section, we first discuss how agricultural revenue is changed due to the agricultural tax cancellation, then show the effect on net revenue change, and provide robustness tests in the end.

2.5.1 Agricultural Revenue

We first investigate the effect of the cancellation of the agricultural tax and incomplete replacement subsidies on local government revenue. Figure 2.3 shows the population-weighted trends of agricultural revenues per-capita: counties on both sides of the border have similar trends in agricultural tax income prior to the policy change. After the abolishment of agricultural taxes, counties in provinces with incomplete replacement experience significant revenue losses compared with counties in the control group. Specifically, in 2001, 2002 and 2003, counties on each side of the border have nearly identical per-capita agricultural tax revenue. In 2004, revenue in treated counties drops to about 10 yuan per capita, while control counties remain closer to 40 yuan. This

gap grows even larger in 2005 and 2006.

To examine whether these differences are driven by trends around the border, we examine how the effects vary with distance from the border. Figure 2.4 plots the average differences in agriculture revenue against the distance from the the province border.[18] Counties on the treated side are assigned positive distances, while control group counties are assigned negative distances. This reveals a strong first stage in agricultural revenue income difference across the border.[19] The graph shows that there is no noticable geographical trend, so the results appear to stem from the policy shock.

Table 2.3 presents the estimates of the policy effects on local government revenue. Column (1) shows that treated counties get an agricultural tax subsidy of 32.47 yuan per capita less than control counties on average due to the policy.[20] Consequently, in column (2) we find that net agricultural revenue for local governments is reduced by 34.79 yuan per capita.[21] Taking into account that the agricultural revenue has been cut by more than 80 percent, and that the aggregate effect is a reduction of about 17 million yuan per county (which is equivalent to 2.5 million USD), the first-stage effect is strong in both economic magnitude and statistical significance.

While the first two columns in Table 2.3 show average changes in agricultural

[18]The distance is measured from the county administrative center, which is defined by the location of the county government building, to the nearest province border.

[19]Note that, this is exhibition similar to, but not a regression discontinuity design, in that we do not regard distance from the border as a running variable to explain the differences in fiscal outcome.

[20]Based on the fact that both treated and control counties get no agricultural tax subsidy prior to the agricultural tax cancellation.

[21]The agricultural revenue for local governments is defined by adding agricultural subsidy to agricultural tax revenue. Despite that most agricultural taxes are canceled, there are some taxes in effect post to the policy, such as land use tax and tobacco tax. This explains why the difference in agricultural revenue is slightly different from that of agricultural subsidy.

tax revenue before and after policy implementation, columns (1) and (2) in Table 2.4 examine how these subsidy gaps vary by year. The agricultural subsidy for treatment counties is partially reduced in 2004 (21.72 yuan per capita, a 47.2 percent change), which is consistent with the fact that the policy of agricultural tax cancellation was partially implemented in that year. In 2005 and 2006, treated counties experienced a full reduction in agricultural revenue, totaling 40.58 and 42.43 yuan per capita, respectively, or a roughly 100 percent change, indicating full policy compliance.

2.5.2 Net Local Revenue

Given the fiscal shortfall caused by the loss of agricultural revenue, and the balanced budget requirement, local government must either increase revenue from other sources, reduce expenditures, or use reserve funds.[22] The overall flypaper effect would suggest that the expenditure will decrease. While most studies investigate the income and expenditure effects of increased fiscal income and find positive net revenue effects (Sjoquist et al., 2005; Allers and Geertsema, 2016; Dahlberg et al., 2008; Brunner and Schwegman, 2017), there is little consensus about the revenue response when government faces a fiscal squeeze. Jonas (2012) shows that local governments in the U.S. simultaneously increased their income from other sources and decreased local government expenditure after the 2008 economic recession. Other studies find that local governments tend to maintain their levels of expenditure by raising money from other

[22] Yu suan fa [Budget Law] (promulgated by the National People's Congress, Aug. 31, 2014) art. 12, 28 (P.R.C.) The reserve funds come from the general local reserve, and the surplus in special government funds.

sources Lago-Penas (2008); Melo (2002); Gamkhar and Oates (1996). The practice of revenue raising differs by the institutional context. In countries where local governments have the authority to determine tax rates, they can simply increase the tax rate, while in countries where local governments do not have the authority to change tax rates freely, this tax revenue increase can be done by strengthening tax enforcement.[23] In China, local governments have limited authority to change tax rates, but they have the freedom to change the extent of tax enforcement.[24]

In Table 2.3, column (3), we find no evidence that local governments facing fiscal squeeze increase revenue from other sources, which exhibits perfect overall money stickiness. Lost the other potential sources, column (4) shows no difference in other intergovernmental subsidies, which rules out the case that treatment counties get insufficient agricultural subsidies but are compensated through other intergovernmental transfers. Therefore, as shown in column (5), the aggregate effect on net revenue change is about -44.87 yuan per capita, corresponding to an average reduction of about 9.6 percentage in total local revenue.

Column (3) to (5) in Table 2.4 shows the net revenue effects by year. As shown in column (3), the percentage changes of other sources of local revenue are insignificant and negligible, while the changes, if anything, are negligible in 2005 and 2006. Column (4) shows that in neither post-treated year local governments with incomplete replace-

[23] In developing countries, the tax enforcement level is usually low, leaving enough room for revenue raising Khan et al. (2016).

[24] Chen (2017) finds that local governments increased income from value-added and sales taxes by imposing higher de facto tax rates. Notably, Chen (2017) exploits the same shock but uses a difference-in-differences design to compare counties within the same prefecture.

ment got abnormal subsidies from other sources. The aggregated changes in government revenue are shown in column (5), revealing that the per-capita revenue reduction in 2004 is half as large as in the following two years.[25]

2.5.3 Robustness Tests

In this section we present the results of various robustness tests, including using the extended sample, and using a sample matched on county characteristics.

The goal of the border design is to find the counterfactual that is the most similar to treated counties. However, in the border design, the sample size is limited, so we use an enlarged sample as a robustness test. Besides border-contiguous counties, we also include counties that are adjacent to border-contiguous counties, i.e., the closest off-border counties. Therefore, the sample size is roughly doubled. Another advantage of using the extended border sample is that it allows us to control for geographical trends that may otherwise contaminate the estimates.

By including additional county observations, we replicate our primary estimates on agricultural revenue and net revenue change. Table 2.5 shows the effects on local revenues for the extended sample. Compared with the main results on local revenue (Table 2.3), we find that the effects are slightly smaller, and the precision is slightly greater, but there is no statistical difference between the results using the extended sample and those using the border county-only sample. This suggests that the

[25]The fact that local governments did not raise revenue from other sources may be explained by the institutional context in China. Local government officials are often appointed by higher levels government, so there is no voting pressure for local government to maintain expenditure level.

results are robust to the selection of counterfactual counties and that the border design is likely to be valid.

While the border design assumes that contiguous counties along the border are similar, we cannot rule out the possibility that non-adjacent counties may be a better counterfactual. Specifically, using matched, non-adjancent counties eliminates concerns about potential spillover effects. Therefore, as an alternative approach, we implement propensity score matching (PSM) as a method of choosing counterfactual counties that are most likely to experience similar outcomes. Propensity score matching is used to estimate an ex-ante probability of being treated, i.e., the propensity score, and to use counties with a similar propensity score as the counterfactual.(Rosenbaum and Rubin, 1983; Imbens, 2000; Gelman and Imbens, 2014) This strategy is commonly used to select a control group when a subset of individuals are affected by an exogenous shock in a panel data context. Counties in fully and less subsidized provinces are matched by population, the share of revenue from intergovernmental subsidies (a proxy for the dependence on central government), and the share of agricultural tax income in local revenue (a proxy for agricultural dependence).[26] These two variables are crucial in determining how a county will be affected by the policy. Appendix Figure 2.C shows the density of propensity scores, revealing that the propensity scores in provinces with and without full compensation are distributed differently. We use propensity score matching to compare the sample from the common support of distributions using caliper matching, as there are often multiple potential control counties with similar propensity

[26]We use fiscal data from County-level Public Finance Statistics Yearbook of 2002.

scores.

Table 2.D presents the changes in revenue estimated with the propensity score matched sample. Compared with main result in Table 2.3 and the extended sample in Table 2.5, we find the net effect on agricultural revenue reduction (-19.96 yuan per capita, 38.8 percent) is smaller and imprecisely measured, while the effects on outcomes in natural logs are larger. Overall, the similarity in magnitude and percentage change shows that the treatment effects are robust to the selection of counterfactual, and also indicates that the cross-border spill-over effect of public spending is not a major concern that might dampen the estimate.

2.5.4 Placebo Tests

One may argue that the estimated treatment effect may come from not the policy but pre-determined differences between opposite sides of the province border, or geographical or time-related confounding treatments. In order to rule out these cases, we conduct a series of placebo tests in both time and space. Four different tests are implemented, in which we a) let the year 2003, one year prior to the actual cancellation of agricultural tax be the treatment time, and only use observation from 2002 to 2003, i.e., use 2002 observations as pre-treat and 2003 observations as post-treated; b) let year 2006, i.e., two years after the agricultural tax cancellation, be the treatment time, and only use observation from 2005 to 2006; c) fabricate a border by using the original treated counties as the control group, while using border contiguous counties also on the treated side as the treatment group, i.e., move the whole policy border to the south

by one county so that we are comparing within provinces with insufficient subsidy; d) fabricate a border by using the original control group counties as the treatment group, while using border contiguous counties as the new control group, i.e., move the border to the north by one county, and thus compare within provinces with full revenue replacement.[27]

Table 2.E shows the revenue effects for each of these placebo tests. The results of the tests are shown in Panel A to D, respectively. We find no significant changes in any sources of local government revenues in any test, which strongly suggests that pre-existing differences and post-treatment trends and confounders are not driving the results.

2.6 The Effect of a Subsidy Shortfall on Expenditure

Given the significant reduction in agricultural revenue and the lack of increased revenue from other sources, the agricultural tax cancellation provides an opportunity to examine how local governments adjust their expenditure. Specifically, we examine how agricultural expenditure and other categories of expenditure are affected by the fiscal shortfall.

[27] In test b), the reason for not using one year post to actual treatment time is that year of 2004 is partially treated. If we use the faked treatment time which is one year after the true time, it will capture the partial implementation of policy.

2.6.1 Agricultural Expenditure

For the expenditure effects, first and foremost, we are interested in the consequent change in agricultural spending. The literature examining how an increase in governmental subsidy changes local spending reveals mixed evidence. Levaggi and Zanola (2003), Berg and Rattsø (2007) and Leduc and Wilson (2017) find that intergovernmental subsidies targeted to a specific sector boosts the spending in that sector. However, crowd-out effects are found in Knight (2002) and Gordon (2004), that is, federal funds for a specific category of expenditure will reduce the spending of local government on that category. In our case, agriculture is the targeted benefited sector. If the reduction of local government spending on agriculture is proportional to overall revenue reduction, it implies perfect categorical money stickiness. On the contrary, if the reduction of local government spending on agriculture is as much as overall revenue reduction, it implies that local governments fully offset the targeted policy intended by the central government.

Table 2.6 shows the policy effects on local government expenditure on agriculture. Column (1) shows that the per-capita agricultural expenditure is reduced by 17.61 yuan, which is 35.6 percent of agricultural spending. Recall that in Table 2.3 we find an 81 percent reduction in agricultural revenue, this implies an elasticity of 0.44, implying a partial categorical flypaper effect. When the negative income shock stems from the agricultural sector, the expenditure on agriculture is significantly reduced, yet the multiplier effect is limited compared to other studies, indicating that local govern-

ments partly reduce the extent to which the benefits of the tax reduction accrue to the targeted group.

In column (1) of Table 2.7 we present the expenditure effects over time. The pattern of changes is interesting. Unlike the revenue effect, that is, partial reduction in 2004 and full effect in 2005 and thereafter, agricultural spending in counties with incomplete subsidy is reduced immediately in 2004, the first year of the policy change. Per-capita agricultural spending is reduced by 22.75 yuan, which is a 44.6 percent decline. The reductions in 2005 and 2006 are 27.1 and 32.9 percent, respectively.

2.6.2 Other Expenditures

In addition to expenditure on agriculture, we are interested in how expenditure in other categories responds to fiscal squeeze. Previous theoretical studies claim that a neutral government may adjust its expenditure proportionally (Barro, 1990; Yin and Zhu, 2012).[28] Mahdavi (2004) compares the elasticity of expenditure in different categories and finds that spending on luxury goods (such as park and recreation) is more volatile than that on necessity goods (such as public safety).

Table 2.6 shows the effect of each category of expenditure. In general, different categories of spending are not proportionally changed. Specifically, column (7) shows that one of the most significantly reduced categories of spending is for a broad, catch-all category consisting of hospitals and libraries, designated pensions and relief

[28] While previous studies do not differentiate the sources of money and merely focus on revenue from local taxation, in Appendix Section 2.B we present a simplified model, which particularly shows how productive spending and welfare-related expenditure are proportionally adjusted in response to change in intergovernmental subsidy.

fees for veterans, science and technology expenditure, public health and medic expenditure, general government fund expenditure, cultural and sports undertakings, militia and military reserve expenditure, lottery and community fund operation cost, and government donation and humanitarian assistance.[29] Treated counties have experienced a per-capita reduction of 21.23 yuan in miscellaneous spending, which is a 12.5 percent reduction. Local governments also reduce payments into liquid reserves.[30] Column (3) shows that Social security spending is also reduced by 2.96 yuan per capita. Despite the fact that the magnitude is relatively small compared with agricultural spending, this reduction accounts for 11 percent of social security spending. Other categories of expenditure, such as infrastructure, education and public policy, do not respond significantly to the reduction in revenue.[31]

The pattern of changes in expenditure indicates that reduction in agricultural expenditure accounts for about half of the revenue change in magnitude. Apart from that, the miscellaneous spending and social security expenditure are vulnerable to fiscal deficit. However, spending on infrastructure, education and public safety are less elastic, perhaps because they are more likely to be guided and supervised by the higher-level governments.

[29]Each of the sector accounts for a small share in the category of miscellaneous spending, however, we are unable to decompose and estimate sector-by-sector changes from the data.

[30]In China, there are general government budget and special government funds. The former can be freely allocated by the local government, but the latter is for specific use, and is controlled or guided by central government. When there is a surplus in government funds, it is allowed to use them to fill the deficit gap of general spending, but it is not allowed to move the money in the opposite direction. Therefore, apart from the general liquidity reserve fund (which is included in miscellaneous spending), the government funds serve as another source of local government reserve.

[31]The administration expenditure is rather increased by 4.08 yuan per capita in counties that got insufficient subsidies. However, considering that the base level of administrative spending is large, this increase accounts for a negligible percentage change.

Table 2.7 shows the effect over time. Contrary to agricultural spending, which shows a stable reduction, treated counties are more inclined to decrease their miscellaneous expenditure and reserves from special government funds in 2005 and 2006: the reduction in miscellaneous spending and special government funds reserves are around 30 and 20 yuan per capita, respectively, much larger than the effects in 2004 (7.95 and 12.71 yuan per capita). The shift in expenditure reduction from agricultural spending to miscellaneous spending and special government funds reserve indicates that the expenditure pattern changes are different in short- and long-run: most of the short-run effect comes from the agricultural sector, which means local governments partially offset the targeted policy by the central government. However, as time goes by, the long-run effect is more likely to be determined by expenditure elasticity, as miscellaneous expenditure seems to be the most elastic among all expenditure categories. We also observe a persistent reduction in social security spending.

2.6.3 Robustness Tests

Similar to Section 2.5.3, we examine the robustness of estimates to using an extended border sample and a propensity score matched sample. Table 2.8 shows the expenditure effects estimated with the extended sample. Agricultural expenditure is decreased by 18.08 yuan per capita (38.0 percent) for less-subsidized counties. Miscellaneous spending and social security spending are reduced by 7.0 and 12.0 percent, respectively. Apart from a 5.7 percent reduction in public safety spending (though the magnitude effect is subtle), these results are similar to the primary estimates.

Table 2.F presents the expenditure effects estimated with the propensity score matched sample. Column (1) shows that the agricultural expenditure is reduced by 16.61 yuan per capita, which is 37.2 percent of agricultural expenditure. In column (3) and (7), we find that the relative reduction of miscellaneous expenditure is about 10.9 percent, and the percentage reduction of social security spending is 15.9 percent. These results are similar to the main regression results, but are less precisely estimated.

2.7 Agricultural Production and Farmers' Income

Now that we have observed the categorical flypaper effect in agricultural spending, an important concern is that what will be the resulting impact of lost expenditure on agricultural production and farmer income. Previous literature has shown that government spending plays a crucial role in agricultural production Griliches (1964); Chavas (2001). In China, Zhou and Chen (2005) finds that farmers' income is significantly increased by benefiting policies, such as tax-for-fee reform. Wang and Shen (2014) studies how does agricultural tax cancellation affect farmers, but finds little effects on agricultural inputs, outputs, or farmer income. We re-examine these effect under a difference-in-differences framework on border counties.

For agricultural production, we measure factor inputs in three dimensions: the area of cultivated land, local agricultural labor force, and the share of agricultural workforce among total labor force.[32] Table 2.9 shows the policy effects on factor inputs

[32]Some provinces provide additional county-level data on agricultural production, such as electricity used in agricultural production and number of registered agricultural machinery. However, the sample size is too limited to draw any informative result.

in agricultural production. Column (1) to (3) shows the average treatment effect, while column (4) to (6) shows the effect by year. Despite the fact that the cultivated land size in counties with insufficient budget replacement tends to be lower, no significant results in terms of magnitude or percentage change are found in either dimension, indicating that there is no factor input difference between counties getting full subsidies or not. For agricultural output, as we have found no notable changes in agricultural inputs, we would expect no output change as well. Table 2.10 shows the changes in agricultural outputs, measured in the grain crops yield, oil crops yield, and meat production.[33] We find the grain crops yield and per-capita production are slightly reduced in 2005 and 2006, but other agricultural production outputs are unaffected.

Table 2.11 shows the effect of agricultural tax cancellation on farmer net income. The net income is defined as revenues minus costs minus government transfers, which, precisely speaking, is the summation of wages, productive income, and business income, minus expenditure and depreciation in family production and business, minus taxes and other fees. Column (1) in Panel A implies that farmers' average net income in counties with insufficient subsidy is 86.24 yuan less than that in counties with full replacement. This accounts for 2.5 percent of farmer's total income, which is both economically and statistically significant, indicating that farmers are worse off when their local government receives incomplete subsidy replacement. Column (2) shows the decomposed income effects by year: in 2005 when the policy is fully implemented, farmers

[33]Grain crops include rice, wheat, cereal, corns, etc. Oil crops include beans, peanuts, sunflower seeds, rape seeds, etc. Meat product includes pork, beef, chicken, lamb, etc. Note that beans are usually accounted as oil crops in China, which is different from American and European practice.

in counties with insufficient subsidy earn around 100 yuan less than those in control counties; while in 2004 when the policy is partially implemented, the income effect is also in proportion. Panel B shows the resulting income effect in natural logarithm: the income difference accounts for about three percentage in farmers' total income. This pattern is similar to the revenue change of local government.

The effects on farmers' net incomes are unexpected but interesting. After agricultural tax cancellation, no matter how the government subsidizes local governments, farmers are directly better off, and thus farmers' income is expected to rise. The significant results and consistent pattern in our estimation indicate that counties with deficit pass their fiscal shortfall to farmers, from whom they previously collected agricultural taxes. Recall that the net income is determined by revenues, costs, and transfers, and the revenue effect is supposed to be the same across the border since there are no significant differences in levels of agricultural outputs, and the agricultural product purchase price is set by the central government thus there is no price effect across the border. The taxes and fees should also be the same, in that agricultural taxes for farmers in treatment and control counties are equally exempted, and that in Table 2.3 we do not observe governments on either side exhibiting abnormal revenue increases, which rules out the case of transferring tax burden to farmers by collecting from other sources. Therefore, the only possible change must come from costs.

The treatment effects on farmer's net income and agricultural outcomes reveal that when local governments reduce their expenditure on agriculture, the actual agricultural production is not significantly affected. However, farmer's net income is reduced

in counties with insufficient tax compensation, roughly equal to the agricultural revenue change. One explanation for this is a transferring effect: when local governments spend less on agricultural expenditure that may promote agricultural productivity, the responsibility falls to farmers and thus they, as individual producers, spend more on production materials (such as fertilizer, agricultural machine, technology investment, etc.)

2.8 Heterogeneous Effects

In the previous sections, we have shown the pattern of local government revenue and expenditure in response to the loss of an agricultural subsidy. We further our study by examining the effects for different types of counties, as average effects may obscure interesting heterogeneity. Specifically, we examine how local government revenues and expenditures are affected by the tenure of the local governor, as well as by the level of of county agricultural dependence and wealth.

First, we are interested in how a county's local governor affects its fiscal behavior. On one hand, as discussed in Li and Zhou (2005), central government uses personnel control to induce desired local economic performance, thus local governors have an incentive to generate economic growth in order to get a promotion. On the other hand, extensive existing evidence about political connections (Faccio, 2006; Li et al., 2008; Fisman and Wang, 2015) indicates that local governors may build stronger connections if he or she stay in the office for longer time. Therefore, we are testing whether counties

with newly-appointed governors and with experienced governors implement a similar response when facing fiscal deficit.

Table 2.12 presents the revenue effects. We categorize counties by whether their top local governor (County Party Secretary) was appointed within the last two years or not.[34] When counties have fiscal deficit due to insufficient revenue replacement, columns (1) and (2) show that treated counties are exposed to similar fiscal shocks, regardless of the tenure of of local governors. However, column (3) and (5) show a large (but less precisely estimated) difference, where new governors accept the revenue shortfall, while longer-tenured governors tend to collect revenue from other sources. The decomposed effects are fuzzy, but in aggregate, counties with new governors exhibit larger average deficits. This finding is consistent with previous literature, as politicians with longer tenure are more likely to increase revenue from other sources (Li and Zhou, 2005).

Table 2.13 presents the expenditure effects. We find that new governors tend to reduce local expenditure, especially reducing agricultural and miscellaneous spending. On the contrary, the expenditure reduction in counties with experienced governors is subtle: they even increase their administration and public safety spending. Instead, their major adjustments come from using reserves. These results, to some extent, indicate that experienced governors are more likely to use their fiscal sources when facing an unexpected deficit, including raising additional revenue and using government funds,

[34]In China, each county has its own election period, however, the time is correlated, in that county-level election and governor appointment are usually right after the election of higher level government. In our sample, governors of nearly two thirds of counties are categorized as "newly-appointed", while the longer-tenured accounts for just 30 percent. Particularly, to make this variation exogenous, we exclude those who are on their second term, i.e., have served more than five years. Thus, all politicians in our sample are on their first term, and thus the variation of politician tenure is exogenous.

while newly-appointed governors reduce local spending.

Additionally, we examine heterogeneous effects by the agricultural tax dependence of each county. Agricultural tax dependence is measured by the share of agricultural tax income among local government's total revenue prior to the cancellation of the agricultural tax. We separate counties into two groups based on the median level. In the high-dependency group, agricultural taxes accounted for 37.7 percent of local tax revenue on average, while in the low-dependence group, this number is 17.3 percent. When the agricultural tax is abolished, agriculture dependent counties will be affected more than counties that are less dependent on such tax revenue. Appendix Table 2.G shows the revenue effects by agricultural tax dependence. In column (1), we see that agricultural tax-dependent counties lose relatively more subsidy from tax cancellation compared to those counties also receiving an incomplete subsidy but that are less dependent on agricultural taxes. However, in column (2), we find that high-dependence counties, apart from the loss due to the incomplete subsidy, lose even more in their revenue from land use taxes, tobacco taxes and deed taxes (which are categorized as "agricultural income" but not canceled by the policy). The percentage of agricultural revenue reduction for agriculture-dependent counties is about 100 percent, while this number for less agriculture-dependent counties is 24.1 percent. For the expenditure effects, Appendix Table 2.H column (1) shows that, both agricultural dependent and independent counties experience similar reduction in agricultural spending in terms of magnitude and percentage change. Other expenditure effects are less precisely estimated.

We also study how the local government behavior varies by local wealth, i.e., gross regional product per capita. Similar to the heterogeneity tests for agricultural dependence, we split based on the median level and separate counties into rich and poor counties. Rich counties have average per-capita gross product of 5,866.24 yuan, while poor counties have 2,828.67 yuan. Appendix Table 2.I shows the heterogeneous effects in local revenue changes. Column (1) and (2) show that treated counties with lower per-capita GDP experience a bit more loss in agricultural revenue than treated but rich counties. The correlation to the previous result shown in Appendix Table 2.G is intuitive, as rich counties usually have more development in industry and business sector, but poor counties are usually agricultural-based. The net revenue change in column (7) also reflects such differences. However, given the difference in net revenue shortfall, when we turn to the expenditure effects, Appendix Table 2.J shows that rich and poor counties exhibit similar reduction in agricultural spending.

The heterogeneous effects by agricultural dependence and wealth indicate that, despite the flypaper effect, i.e., reduction in government agricultural spending, the magnitude and percentage change is not highly sensitive to how much it decreased the total revenue of the county. A potential explanation is that county governments perform revenue and expenditure accounting at the sector level, providing additional insight into the presence of a categorical flypaper effect.

2.9 Conclusion

In 2004, China's central government abolished agricultural taxes and provided annual subsidies to offset local budget losses. However, the amount of the subsidy varied by province, and otherwise similar counties on opposite sides of provincial borders experienced significantly different revenue shocks. Using a merged data set from the County Public Finance Statistics Yearbook of China, the China Regional Economic Statistics Yearbook, the China County Social and Economic Statistical Yearbook, and a manually collected county politician profile data set, a difference-in-difference design on border counties shows that agricultural tax revenue was reduced by 81 percent when counties received no subsidy replacement, accounting for a 10 percent reduction in total local revenue. We find no evidence that local governments offset the loss by increasing taxes on other sectors on the economy. Instead, we find that local agricultural spending is reduced by 36 percent. Social security and other expenditures were also reduced, while expenditures on infrastructure, administration, and education experienced little or no change. We further find that agricultural factor inputs and production were not significantly affected, but the net income of farmers in less-subsidized counties decreased, suggesting that the cost pressure of public goods supply reduction is transferred to agriculture producers.

This study contribute to the literature in several ways. First, it addresses the effect of fiscal squeeze driven by a policy change and finds no crowd-in effect from other sources of local income, which exhibits the flypaper effect in terms of overall revenue.

Second, it supplements the existing categorical flypaper effect literature by examining the consequent effects for the agricultural sector, revealing that local government reduces expenditures that benefit farmers. Specifically, we find that government spending is reduced for farmers, and farmers' net income declines in less-subsidized counties due to the increased cost. Our findings imply that local government is reluctant to shift the tax burden to other residents, and partially offsets the benefits intended for the group receiving the tax reduction through reduced expenditure. This highlights the potentially unintended consequences of a targeted tax reduction imposed by the central government when expenditures are determined at the local level.

References

Alesina, A. and Rodrik, D. (1994). Distributive politics and economic growth. *The Quarterly Journal of Economics*, 109(2):465–490.

Allers, M. A. and Geertsema, J. B. (2016). The effects of local government amalgamation on public spending, taxation, and service levels: evidence from 15 years of municipal consolidation. *Journal of Regional Science*, 56(4):659–682.

Bahl, R. W. (1999). *Fiscal Policy in China: Taxation and Intergovernmental Fiscal Relations*. University of Michigan Press.

Bank, W. (2002). China national development and sub-national finance: A review of provincial expenditures.

Barro, R. J. (1990). Government spending in a simple model of endogeneous growth. *Journal of Political Economy*, 98(5, Part 2):S103–S125.

Becker, E. (1996). The illusion of fiscal illusion: Unsticking the flypaper effect. *Public Choice*, 86(1-2):85–102.

Berg, E. and Rattsø, J. (2007). Do grants to state and local governments stimulate economic development?

Bernstein, T. P. and Lü, X. (2000). Taxation without representation: peasants, the central and the local states in reform china. *The China Quarterly*, 163:742–763.

Bradbury, Katharine L., C. J. M. and Case, K. E. (2001). Property tax limits, local fiscal behavior, and property values: Evidence from massachusetts under proposition 2(1/2). *Journal of Public Economics*, 80(2):287–31.

Bräuninger, T. (2005). A partisan model of government expenditure. *Public Choice*, 125(3):409–429.

Brunner, E. J. and Schwegman, D. J. (2017). The impact of georgia's education special purpose local option sales tax on the fiscal behavior of local school districts. *National Tax Journal*, 70(2):295.

Budge, I. and Hofferbert, R. I. (1990). Mandates and policy outputs: Us party platforms and federal expenditures. *American Political Science Review*, 84(1):111–131.

Burge, G. S., Piper, B., et al. (2012). Strategic fiscal interdependence: county and municipal adoptions of local option sales taxes. *National Tax Journal*, 65(2):387–416.

Cameron, A. C. and Miller, D. L. (2015). A practitioner¡¯s guide to cluster-robust inference. *Journal of Human Resources*, 50(2):317–372.

Card, D. and Krueger, A. (2000). Minimum wages and employment: A case study of the

fast-food industry in new jersey and pennsylvania: Reply. *The American Economic Review*, 90(5):1397–1420.

Chavas, J.-P. (2001). Structural change in agricultural production: Economics, technology and policy. *Handbook of Agricultural Economics*, 1:263–285.

Chen, S. X. (2017). The effect of a fiscal squeeze on tax enforcement: Evidence from a natural experiment in china. *Journal of Public Economics*, 147:62–76.

Chen, X. (2003). A study on china's county and township public finance and farmer income growth. *Shangxi Economic Press*.

Courant, P. N., Gramlich, E. M., and Rubinfeld, D. L. (1978). *The stimulative effects of intergovernmental grants: Or why money sticks where it hits*. University of Michigan, Institute of Public Policy Studies.

Dahlberg, M., Mörk, E., Rattsø, J., and Ågren, H. (2008). Using a discontinuous grant rule to identify the effect of grants on local taxes and spending. *Journal of Public Economics*, 92(12):2320–2335.

Darby, J., Muscatelli, V., and Roy, G. (2005). How do sub-central government react to cuts in grants received from central governments? evidence from a panel of 15 oecd countries.

Devarajan, S., Swaroop, V., and Zou, H.-f. (1996). The composition of public expenditure and economic growth. *Journal of Monetary Economics*, 37(2):313–344.

Drazen, A. and Eslava, M. (2010). Electoral manipulation via voter-friendly spending: Theory and evidence. *Journal of Development Economics*, 92(1):39–52.

Dube, Arindrajit, T. W. L. and Reich, M. (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *The Review of Economics and Statistics*, 92(4):945–964.

Duranton, G., Gobillon, L., and Overman, H. G. (2011). Assessing the effects of local taxation using microgeographic data. *The Economic Journal*, 121(555):1017–1046.

Dye, R. F. and McGuire, T. J. (1997). The effect of property tax limitation measures on local government fiscal behavior. *Journal of Public Economics*, 66(6):469–487.

Faccio, M. (2006). Politically connected firms. *The American Economic Review*, 96(1):369–386.

Fisman, R. and Wang, Y. (2015). The mortality cost of political connections. *The Review of Economic Studies*, 82(4):1346–1382.

Gamkhar, S. and Oates, W. (1996). Asymmetries in the response to increases and decreases in intergovernmental grants: Some empirical findings. *National Tax Journal*, pages 501–512.

Gelman, A. and Imbens, G. (2014). Why high-order polynomials should not be used in regression discontinuity designs. *NBER working paper*, No.20405.

Gonzalez, M. d. l. A. (2002). Do changes in democracy affect the political budget cycle? evidence from mexico. *Review of Development Economics*, 6(2):204–224.

Gordon, N. (2004). Do federal grants boost school spending? evidence from title i. *Journal of Public Economics*, 88(9):1771–1792.

Griliches, Z. (1964). Research expenditures, education, and the aggregate agricultural production function. *The American Economic Review*, pages 961–974.

Henderson, J. M. (1968). Local government expenditures: a social welfare analysis. *The Review of Economics and Statistics*, pages 156–163.

Hines, J. R. and Thaler, R. H. (1995). Anomalies: The flypaper effect. *The Journal of Economic Perspectives*, 9(4):217–226.

Imbens, G. W. (2000). The role of the propensity score in estimating dose-response functions. *Biometrika*, 87(3):706–710.

Inman, R. P. (2008). The flypaper effect. *National Bureau of Economic Research*.

Jiang, J. (2018). Making bureaucracy work: Patronage networks, performance incentives, and economic development in china. *American Journal of Political Science*, forthcoming.

Jonas, J. (2012). Great recession and fiscal squeeze at us subnational government level.

Kakamu, K., Yunoue, H., and Kuramoto, T. (2014). Spatial patterns of flypaper effects for local expenditure by policy objective in japan: A bayesian approach. *Economic Modelling*, 37:500–506.

Khan, A. Q., Khwaja, A. I., and Olken, B. A. (2016). Tax farming redux: Experimental

evidence on performance pay for tax collectors. *The Quarterly Journal of Economics*, 131(1):219–271.

Khemani, S. (2000). *Political Cycles in a Developing Economy: effect of elections in the Indian States*, volume 2454. World Bank Publications.

Knight, B. (2002). Endogenous federal grants and crowd-out of state government spending: Theory and evidence from the federal highway aid program. *The American Economic Review*, 92(1):71–92.

Knight, B. (2004). Parochial interests and the centralized provision of local public goods: evidence from congressional voting on transportation projects. *Journal of Public Economics*, 88(3-4):845–866.

Lago-Penas, S. (2008). Local governments' asymmetric reactions to grants: Causes and consequences. *Public Finance Review*, 36(2):219–242.

Leduc, S. and Wilson, D. (2017). Are state governments roadblocks to federal stimulus? evidence on the flypaper effect of highway grants in the 2009 recovery act. *American Economic Journal: Economic Policy*, 9(2):253–92.

Levaggi, R. and Zanola, R. (2003). Flypaper effect and sluggishness: evidence from regional health expenditure in italy. *International Tax and Public Finance*, 10(5):535–547.

Li, H., Meng, L., Wang, Q., and Zhou, L.-A. (2008). Political connections, financing

and firm performance: Evidence from chinese private firms. *Journal of Development Economics*, 87(2):283–299.

Li, H. and Zhou, L.-A. (2005). Political turnover and economic performance: the incentive role of personnel control in china. *Journal of Public Economics*, 89(9):1743–1762.

Lin, J. Y., Tao, R., and Liu, M. (2007). Rural taxation and local governance reform in china's economic transition: Origins, policy responses, and remaining challenges.

Mahdavi, S. (2004). Shifts in the composition of government spending in response to external debt burden. *World Development*, 32(7):1139–1157.

Melo, L. (2002). The flypaper effect under different institutional contexts: The colombian case. *Public Choice*, 111(3-4):317–345.

Roemer, J. E. and Silvestre, J. (2002). The ¡°flypaper effect¡± is not an anomaly. *Journal of Public Economic Theory*, 4(1):1–17.

Rosenbaum, P. R. and Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, 70(1):41–55.

Sjoquist, D. L., Walker, M. B., and Wallace, S. (2005). Estimating differential responses to local fiscal conditions: A mixture model analysis. *Public Finance Review*, 33(1):36–61.

Suárez Serrato, J. C. and Wingender, P. (2016). Estimating local fiscal multipliers. *NBER working paper*, No.22425.

Tan, J. (2010). *Study on the Relationship between Financial Right and the Power of Central Government and Local Authorities*. China Financial and Economic Publishing House.

Thompson, J. P. and Rohlin, S. M. (2012). The effect of sales taxes on employment: New evidence from cross-border panel data analysis. *National Tax Journal*, 65(4):1023.

Turnbull, G. K. (1992). Fiscal illusion, uncertainty, and the flypaper effect. *Journal of Public Economics*, 48(2):207–223.

Turner, T. M. and Blagg, B. (2017). The short-term effects of the kansas income tax cuts on employment growth. *Public Finance Review*, pages 1025–1043.

Vegh, C. A. and Vuletin, G. (2015). Unsticking the flypaper effect in an uncertain world. *Journal of Public Economics*, 131:142–155.

Wang, X. and Shen, Y. (2014). The effect of china's agricultural tax abolition on rural families' incomes and production. *China Economic Review*, 29:185–199.

Xu, Cuiping, Q. S. and Wang, H. H. (2009). The impact of rural taxation reform on farm household income: Evidence from the yangtze river delta. *Chinese Economy*, 42(4):75–90.

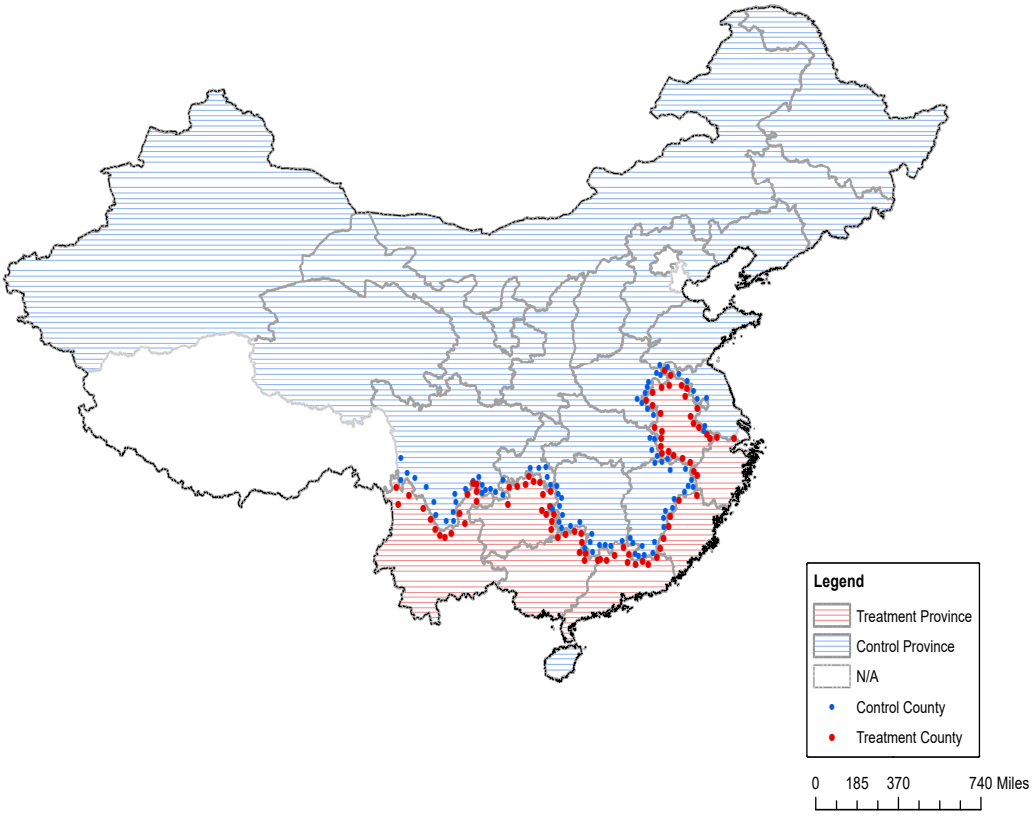
Yin, H. and Zhu, H. (2012). A study of productive expenditure bias in county-level finance in china. *Social Sciences in China*, 33(1):127–147.

Zhao, Z. J. and Hou, Y. (2008). Local option sales taxes and fiscal disparity: The case of georgia counties. *Public Budgeting and Finance*, 28(1):39–57.

Zhou, L.-A. and Chen, Y. (2005). The policy effect of tax-and-fees reforms in rural china: A difference-in-differences estimation. *Economic Research Journal*, 8:5.

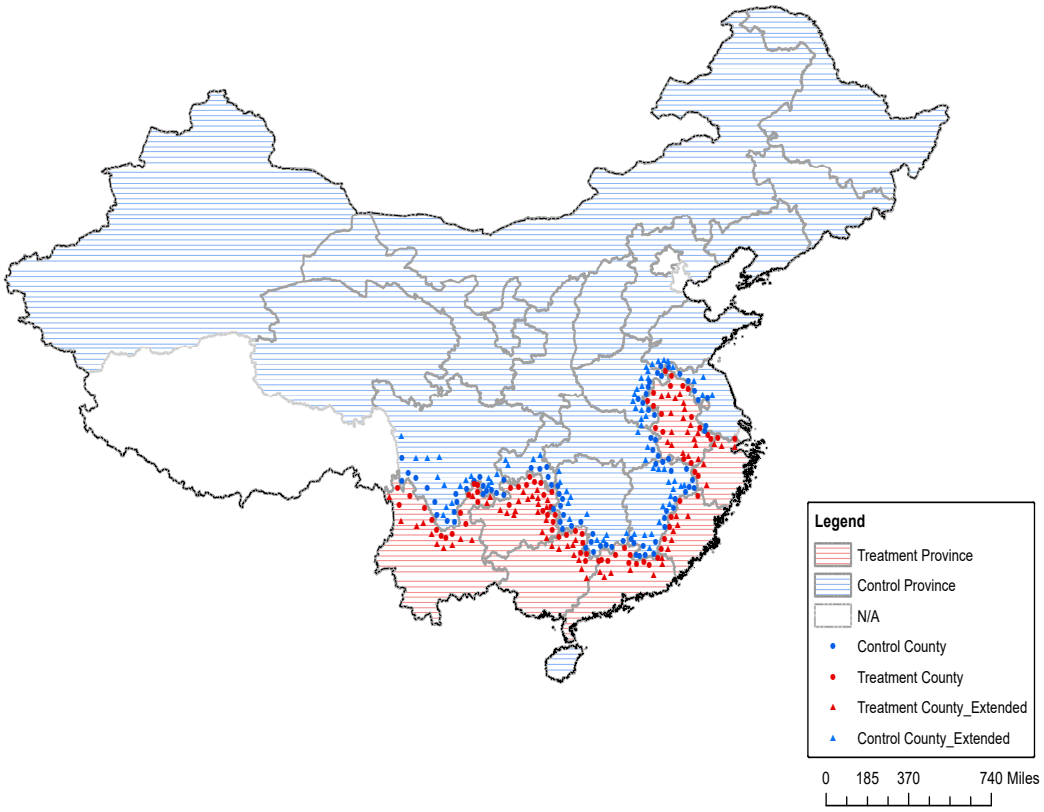
Figures and Tables

Figure 2.1: Geographical Distributions of Policy Effects: Border Sample



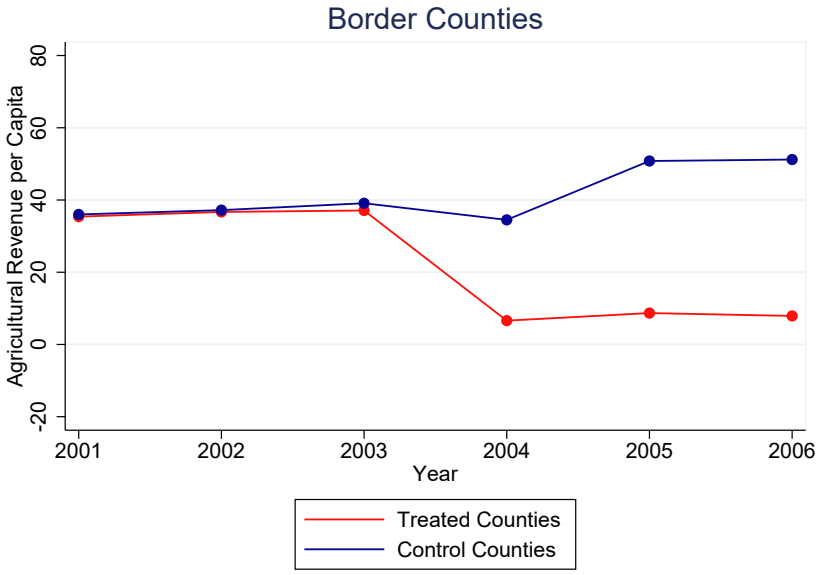
This figure shows geographical distribution of China Mainland provinces with different policy effects. Counties in blue-colored provinces (Sichuan, Chongqing, Hunan, Jiangxi, Hubei, Henan, Shandong, Jiangsu, and other northern provinces which are not included in the sample) get full amount of baseline subsidy, and counties in provinces in red (Yunnan, Guizhou, Guangxi, Guangdong, Fujian, Zhejiang and Anhui) get insufficient level compared to baseline. We exclude Municipal Cities (Beijing, Tianjin and Shanghai), Special Autonomous Regions (Hong Kong, Macau and Taiwan), and Tibet (where there is agricultural tax exemption since 1950). All the counties labeled on map are adjacent to the border. There are 90 counties in control group (blue) and 87 in treatment group (red).

Figure 2.2: Geographical Distributions of Policy Effects: Extended Sample



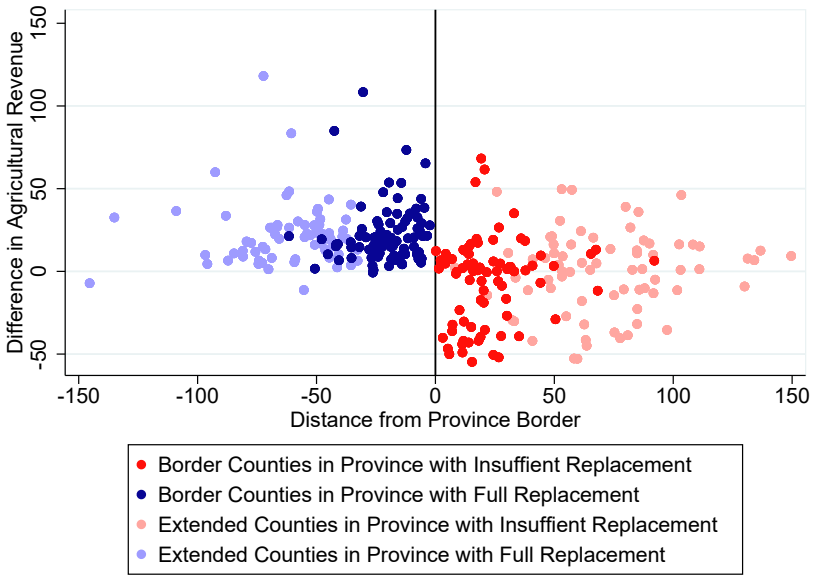
This figure shows geographical distribution of China Mainland provinces with different policy effects. Counties in blue-colored provinces (Sichuan, Chongqing, Hunan, Jiangxi, Hubei, Henan, Shandong, Jiangsu, and other northern provinces which are not included in the sample) get full amount of baseline subsidy, and counties in provinces in red (Yunnan, Guizhou, Guangxi, Guangdong, Fujian, Zhejiang and Anhui) get insufficient level compared to baseline. We exclude Municipal Cities (Beijing, Tianjin and Shanghai), Special Autonomous Regions (Hong Kong, Macau and Taiwan), and Tibet (where there is agricultural tax exemption since 1950). By including off-border counties to the original sample, the extended sample includes 1,978 observations in total, with 164 counties in control group (blue) and 166 in treatment group (red).

Figure 2.3: Policy Effects on Agriculture Revenues by Year



This figure shows the population-weighted trends of local government's agricultural revenue (tax+subsidy), adjusted for agricultural importance (using share of agricultural tax income) as covariates. Their pre-treatment trend looks matched after the adjustment. Agricultural revenue includes agricultural tax (Agricultural Tax, Agricultural Specialty Tax and Livestock Tax) income and specific subsidies from central government.

Figure 2.4: Policy Effects on Agriculture Revenues by Distance From Border



This scatter plot shows spatial distribution of changes in agriculture revenue after the agricultural tax abolishment. Distance is measured from county administrative center (i.e., the location of county government) to the nearest border and is in unit of kilometer. Positive distance represents counties in provinces with full subsidy, and negative distance represents counties in provinces with incomplete replacement.

Table 2.1: Revenue Sharing between the Central and Local Governments

I.Taxes exclusively assigned to the Central and Provincial Governments
1. Excise taxes
II. Taxes shared between the central and local governments
1. Value-added tax
2. Enterprise income tax
3. Natural resource tax
4. Construction tax
5. Industrial and commercial tax
III.Taxes exclusively assigned to local governments
1. Agricultural tax
2. Personal income tax
3. Land use tax
4. Property tax

Notes: For a detailed description of Revenue Sharing System, please refer to Appendix Table 2.A.

Table 2.2: Descriptive Statistics for the Border Sample

Unit: RMB per capita

	Control Group		Treatment Group	
	Mean	Std.dev.	Mean	Std.dev.
Total Revenue	532.99	215.51	661.09	387.93
Local Revenue	182.02	99.74	174.15	98.00
Agricultural Tax	44.42	22.58	41.37	21.74
Value Added Tax	19.59	19.44	26.09	31.13
Personal Income Tax	7.91	5.62	9.87	10.95
Sales Tax	32.17	26.88	32.63	27.44
Other Income	137.60	88.18	132.78	97.00
Intergovernmental Subsidy	350.97	189.21	486.95	351.78
Total Expenditure	501.48	192.45	615.99	370.53
Agriculture	46.94	33.69	73.78	76.56
Infrastructure	33.95	58.60	21.15	44.28
Education	120.99	36.82	159.45	65.09
Administration	72.91	38.15	90.00	54.81
Social Security	18.17	12.56	14.72	14.71
Public Safety	29.77	13.92	33.32	19.56
Miscellaneous	170.84	68.39	211.91	139.48
Gross Regional Product in				
Primary Sector	1,444.30	464.20	1,537.08	643.07
Secondary Sector	1,484.04	1,271.77	1,239.29	928.95
Tertiary Sector	1,630.06	4,405.61	1,378.58	816.82
Population	561,261	362,629	492,549	354,556
Number of Counties	86		82	
Number of Observations	257		244	

Notes: This table shows the pre-treatment descriptive statistics of counties in treatment group and control group, restricted to border sample. Revenue, expenditure and gross regional product are in unit of RMB per capita (2004 real price).

Table 2.3: Policy Effects on Local Government Revenue

Unit: RMB per capita

Outcome Variable	(1) Agr. Subsidy	(2) Agr. Revenue (Tax+Subsidy)	(3) Other Local Revenues	(4) Other Central Subsidies	(5) Net Revenue Change
Panel A: Reduced form, OLS estimate					
Post04×Incomplete	-32.468***	-34.788***	-8.934**	-1.151	-44.873***
	(4.948)	(8.293)	(4.049)	(16.469)	(9.774)
Mean Dep.Var.	12.365	46.870	135.390	406.007	588.267
Observations	1,002	1,002	1,002	1,002	1,002
R-squared	0.826	0.556	0.718	0.865	0.878
Number of Counties	168	168	168	168	168
Panel B: Natural-logs Reduced form, GLM estimate					
Post04×Incomplete		-0.810***	-0.023	-0.056	-0.096***
		(0.229)	(0.031)	(0.050)	(0.022)
Observations		1,002	1,002	1,002	1,002
Number of Counties		168	168	168	168

Agricultural revenues include agricultural tax revenues and agricultural subsidy revenues (which only exists after 2004).

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects, year-by-segment fixed effects, and covariates of population and per-capita GDP in each sector.

Standard errors in parentheses are clustered at province-border level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.4: Policy Effects on Local Government Revenue by Year

Unit: RMB per capita

Outcome Variable	(1) Agr. Subsidy	(2) Agr. Revenue (Tax+Subsidy)	(3) Other Local Revenues	(4) Other Central Subsidies	(5) Net Revenue Change
Panel A: Reduced form, OLS estimates					
Post04×Incomplete×Year=04	-21.939***	-21.722***	-1.938	-2.116	-25.776*
	(2.626)	(5.235)	(3.684)	(18.509)	(13.952)
Post04×Incomplete×Year=05	-38.455***	-40.580***	-11.988**	-0.927	-53.496***
	(6.385)	(9.863)	(4.127)	(18.569)	(12.617)
Post04×Incomplete×Year=06	-37.335***	-42.432***	-13.072*	-0.386	-55.891***
	(6.383)	(10.187)	(6.995)	(14.310)	(9.751)
Mean Dep.Var.	12.365	46.870	135.390	406.007	588.267
Observations	1,002	1,002	1,002	1,002	1,002
R-squared	0.848	0.591	0.719	0.865	0.879
Number of Counties	168	168	168	168	168
Panel B: Natural-logs Reduced form, GLM estimates					
Post04×Incomplete×Year=04		-0.472***	-0.024	-0.042	-0.072***
		(0.103)	(0.028)	(0.050)	(0.024)
Post04×Incomplete×Year=05		-0.986***	-0.027	-0.048	-0.102***
		(0.353)	(0.032)	(0.054)	(0.027)
Post04×Incomplete×Year=06		-1.042***	-0.019	-0.073	-0.110***
		(0.339)	(0.041)	(0.049)	(0.020)
Observations		1,002	1,002	1,002	1,002
Number of Counties		168	168	168	168

Agricultural revenues include agricultural tax revenues and agricultural subsidy revenues (which only exists after 2004).

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.5: Policy Effects on Local Government Revenue: Extended Sample

Unit: RMB per capita

Outcome Variable	(1) Agr. Subsidy	(2) Agr. Revenue (Tax+Subsidy)	(3) Other Local Revenues	(4) Other Central Subsidies	(5) Net Revenue Change
Panel A: Reduced form, OLS estimates					
Post04×Incomplete	-31.291***	-32.150***	-4.734	0.231	-36.654***
	(5.412)	(7.744)	(3.754)	(19.568)	(11.026)
Mean Dep.Var.	12.553	47.651	139.239	398.354	585.244
Observations	1,855	1,855	1,855	1,855	1,855
R-squared	0.822	0.564	0.690	0.832	0.853
Number of Counties	311	311	311	311	311
Panel B: Natural-logs Reduced form, GLM estimates					
Post04×Incomplete		-0.760***	-0.021	-0.042	-0.084***
		(0.216)	(0.030)	(0.059)	(0.026)
Observations		1,855	1,855	1,855	1,855
Number of Counties		311	311	311	311

Agricultural revenues include agricultural tax revenues and agricultural subsidy revenues (which only exists after 2004).

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects,year fixed effects× border-segment fixed effects, and fraction of agriculture tax revenue in local total revenue(subsidy not included, proxy for importance of agricultural tax to local economy).

Those counties not on the province border is clustered into the same border segment as its closest neighbor county, where the distance is calculated using county seat-to-county seat straight distance.

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.6: Policy Effects on Local Government Expenditures

Unit: RMB per capita

Outcome Variable	(1) Agriculture	(2) Infrastructure	(3) Social Security	(4) Administration	(5) Education	(6) Public Safety	(7) Miscellaneous	(8) Total	(9) Reserves
Panel A: Reduced Form, OLS estimate									
Post04×Incomplete	-17.608***	4.623	-2.963**	4.083***	3.839	-0.611	-21.231***	-25.836**	-19.037***
	(5.545)	(7.644)	(1.209)	(1.189)	(3.203)	(1.106)	(4.380)	(10.873)	(4.942)
Mean Dep.Var.	53.636	24.683	17.422	72.443	141.253	29.763	188.395	545.768	42.499
Observations	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002
R-squared	0.913	0.680	0.807	0.950	0.932	0.924	0.939	0.963	0.815
Number of Counties	168	168	168	168	168	168	168	168	168
Panel B: Natural-logs Reduced Form, GLM estimate									
After×Treatment	-0.356***	-0.000	-0.114**	0.015	-0.016	-0.026	-0.125***	-0.064**	-0.216
	(0.062)	(0.432)	(0.057)	(0.026)	(0.013)	(0.018)	(0.019)	(0.027)	(0.133)
Observations	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002
Number of Counties	168	168	168	168	168	168	168	168	168

All regressions are weighted by 2005 county-level population

All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.7: Policy Effects on Local Government Expenditures by Year

Unit: RMB per capita

Outcome Variable	(1) Agriculture	(2) Infrastructure	(3) Social Security	(4) Administration	(5) Education	(6) Public Safety	(7) Miscellaneous	(8) Total	(9) Reserves
Panel A: Reduced Form, OLS estimate									
Post04×Incomplete ×Year=04	-22.754**	3.309	-3.304**	2.896**	10.977***	0.083	-7.946**	-13.070	-12.706*
	(8.658)	(5.819)	(1.254)	(1.336)	(1.857)	(0.756)	(3.678)	(10.331)	(6.476)
Post04×Incomplete ×Year=05	-12.000***	3.649	-1.979	3.066**	0.879	-1.971*	-29.525***	-34.317**	-19.178***
	(3.550)	(7.829)	(1.126)	(1.405)	(4.530)	(1.094)	(4.536)	(12.301)	(4.763)
Post04×Incomplete ×Year=06	-17.861***	6.919	-3.581*	6.291***	-0.537	0.015	-26.646***	-30.537**	-25.354**
	(3.581)	(8.710)	(1.971)	(1.360)	(4.163)	(1.530)	(5.967)	(11.211)	(9.282)
Mean Dep.Var.	53.636	24.683	17.422	72.443	141.253	29.763	188.395	545.768	42.499
Observations	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002
R-squared	0.634	0.217	0.454	0.727	0.810	0.689	0.809	0.879	0.431
Number of Counties	168	168	168	168	168	168	168	168	168
Panel B: Natural-logs Reduced form, GLS estimate									
Post04×Incomplete ×Year=04	-0.446***	-0.213	-0.139***	0.015	0.055***	-0.014	-0.068***	-0.042*	-0.244*
	(0.102)	(0.352)	(0.046)	(0.020)	(0.014)	(0.022)	(0.017)	(0.023)	(0.126)
Post04×Incomplete ×Year=05	-0.271***	0.035	-0.082	0.009	-0.019	-0.062**	-0.151***	-0.064**	-0.213
	(0.049)	(0.438)	(0.065)	(0.028)	(0.023)	(0.026)	(0.024)	(0.028)	(0.189)
Post04×Incomplete ×Year=06	-0.329***	0.195	-0.121	0.034	-0.056***	-0.011	-0.140***	-0.069**	-0.245
	(0.047)	(0.485)	(0.102)	(0.031)	(0.013)	(0.020)	(0.021)	(0.031)	(0.168)
Observations	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002
Number of Counties	168	168	168	168	168	168	168	168	168

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.8: Policy Effects on Local Government Expenditures: Extended Sample

Unit: RMB per capita

Outcome Variable	(1) Agriculture	(2) Infrastructure	(3) Social Security	(4) Administration	(5) Education	(6) Public Safety	(7) Miscellaneous	(7) Total	(8) Reserves
Panel A: Reduced Form, OLS estimate									
Post04×Incomplete	-18.038***	4.806	-3.105**	3.750**	3.738*	-1.232	-10.449***	-16.770	-19.884***
	(5.083)	(6.789)	(1.287)	(1.276)	(1.812)	(0.754)	(3.472)	(13.599)	(6.276)
Mean Dep.Var.	55.168	23.253	16.574	77.250	139.786	31.718	202.530	564.756	54.431
Observations	1,855	1,855	1,855	1,855	1,855	1,855	1,855	1,855	1,855
R-squared	0.896	0.672	0.770	0.934	0.908	0.922	0.924	0.956	0.731
Number of Counties	311	311	311	311	311	311	311	311	311
Panel B: Natural-logs Reduced Form, GLM estimate									
Post04×Incomplete	-0.380***	0.056	-0.120**	0.014	-0.017	-0.057***	-0.070***	-0.047	-0.437*
	(0.059)	(0.402)	(0.049)	(0.029)	(0.024)	(0.009)	(0.023)	(0.032)	(0.231)
Observations	1,855	1,855	1,855	1,855	1,855	1,855	1,855	1,855	1,855
Number of Counties	311	311	311	311	311	311	311	311	311

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.

Standard errors in parentheses are clustered at province-border level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.9: Policy Effects on Factor Inputs in Agricultural Production

Outcome Variable	(1) Actual Cultivated Area Size	(2) Agricultural Laborforce	(3) Share of Agr. Laborforce	(4) Actual Cultivated Area Size	(5) Agricultural Laborforce	(6) Share of Agr. Laborforce
Unit	Km^2 (100 Hectare)	1,000 Person	% (in total population)	Km^2 (100 Hectare)	1,000 Person	% (in total population)
Panel A: Reduced Form						
After×Treat	-3.125	-0.337	0.005			
	(7.875)	(5.207)	(0.006)			
After×Treat×Year==2004				11.623	-5.699	-0.001
				(14.174)	(5.190)	(0.006)
After×Treat×Year==2005				-13.582	3.995	0.009
				(14.782)	(7.227)	(0.009)
After×Treat×Year==2006				-10.956	1.841	0.011
				(14.413)	(6.784)	(0.008)
Mean Dep.Var.	527.82	365.29	0.48	527.82	365.29	0.48
Observations	888	859	859	888	859	859
R-squared	0.111	0.282	0.191	0.115	0.284	0.194
Number of n_county	168	168	168	168	168	168

All regressions are weighted by 2005 county-level population.
All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.
Standard errors in parentheses are clustered at province level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 2.10: Policy Effects on Gross and Per-capita Agricultural Production

Outcome Variable	(1) Grain Crops	(2) Oil Crops	(3) Meat Product	(1) Grain Crops	(2) Oil Crops	(3) Meat Product
Panel A: Gross Product						
						Unit: 1,000 ton
After×Treat	-3.366	-0.548	-3.441*			
	(7.386)	(2.796)	(1.881)			
After×Treat×Year==2004				8.089	-0.930	-4.427**
				(7.892)	(1.932)	(1.645)
After×Treat×Year==2005				-15.149*	-0.134	-2.407
				(7.725)	(5.477)	(2.123)
After×Treat×Year==2006				-0.009	-0.558	-3.502
				(7.792)	(1.529)	(3.092)
Mean Dep.Var.	323.63	24.648	53.908	323.63	24.648	53.908
Observations	903	816	840	903	816	840
R-squared	0.601	0.349	0.506	0.605	0.349	0.507
Number of n_county	168	145	151	168	145	151
Panel B: Per-Capita Product						
						Unit: Kg per capita
After×Treat	-14.826	3.150	-2.734			
	(10.412)	(3.904)	(3.103)			
After×Treat×Year==2004				0.327	0.656	-3.370*
				(8.256)	(1.805)	(1.735)
After×Treat×Year==2005				-26.523**	8.195	-1.611
				(11.508)	(9.693)	(3.537)
After×Treat×Year==2006				-17.150	-1.743	-3.842
				(11.954)	(2.162)	(6.088)
Mean Dep.Var.	431.758	30.467	72.688	431.758	30.467	72.688
Observations	903	816	840	903	816	840
R-squared	0.495	0.230	0.519	0.500	0.235	0.520
Number of n_county	168	145	151	168	145	151

All regressions are weighted by 2005 county-level population.
All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.
Standard errors in parentheses are clustered at province level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 2.11: Policy Effects on Farmers' Income

Unit: RMB per capita

	(1)	(2)
Outcome Variable	Farmer's Income	Farmer's Income
Panel A: Change in Magnitude		
After×Treat	-86.237***	
	(26.046)	
After×Treat×Year=2004		-54.287
		(36.759)
After×Treat×Year=2005		-97.054***
		(26.408)
After×Treat×Year=2006		-106.319***
		(27.827)
Mean Dep.Var.	2424.153	2424.153
Observations	1,770	1,770
R-squared	0.674	0.675
Number of n_county	311	311
Panel B: Change in Natural-logs		
After×Treat	-0.025*	
	(0.012)	
After×Treat×Year=2004		-0.017
		(0.012)
After×Treat×Year=2005		-0.030**
		(0.013)
After×Treat×Year=2006		-0.028
		(0.016)
Mean Dep.Var.	7.721	7.721
Observations	1,770	1,770
R-squared	0.646	0.647
Number of n_county	311	311

All regressions are weighted by 2005 county-level population.
All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.
Standard errors in parentheses are clustered at province level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 2.12: Policy Effects on Local Government Revenue by Tenure of Politicians

Unit: RMB per capita

Outcome Variable	(1) Agr. Subsidy	(2) Agr. Revenue (Tax+Subsidy)	(3) Other Local Revenues	(4) Other Central Subsidies	(5) Net Revenue Change
Panel A: Reduced form, OLS estimate					
Post04×Incomplete×NewGovernor=0	-36.163***	-41.148**	12.107	1.381	-27.660
	(5.952)	(15.322)	(16.198)	(26.123)	(30.426)
Post04×Incomplete×NewGovernor=1	-30.726***	-34.271**	-6.378	-0.038	-40.688***
	(4.580)	(12.021)	(8.139)	(17.977)	(10.153)
Observations	856	856	856	856	856
R-squared	0.800	0.412	0.681	0.841	0.852
Number of Counties	143	143	143	143	143
Panel B: Natural-logs Reduced form, GLM estimate					
Post04×Incomplete×NewGovernor=0		-0.853**	0.034	-0.049	-0.085
		(0.372)	(0.137)	(0.108)	(0.060)
Post04×Incomplete×NewGovernor=1		-0.806**	-0.028	-0.088	-0.112***
		(0.345)	(0.058)	(0.097)	(0.039)
Observations	856	856	856	856	856
Number of Counties	143	143	143	143	143

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects,year fixed effects× border-segment fixed effects, and fraction of agriculture tax revenue in local total revenue(subsidy not included, proxy for importance of agricultural tax to local economy).

Those counties not on the province border is clustered into the same border segment as its closest neighbor county, where the distance is calculated using county seat-to-county seat straight distance.

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.13: Policy Effects on Local Government Expenditures by Tenure of Politicians

Unit: RMB per capita

Outcome Variable	(1) Agriculture	(2) Infrastructure	(3) Social Security	(4) Administration	(5) Education	(6) Public Safety	(7) Miscellaneous	(8) Total	(9) Reserves
Panel A: Reduced Form, OLS estimate									
Post04×Incomplete×NewGovernor=0	-12.901	1.322	0.063	10.227***	2.260	4.139	-12.876	-0.942	-26.717**
	(11.762)	(10.932)	(2.735)	(3.074)	(3.408)	(2.666)	(11.645)	(27.560)	(10.332)
Post04×Incomplete×NewGovernor=1	-16.602**	6.863	-3.360	1.734	3.501	-1.391	-18.602***	-23.405*	-17.283
	(6.100)	(9.837)	(2.189)	(5.410)	(4.871)	(1.666)	(5.476)	(11.167)	(10.564)
Observations	856	856	856	856	856	856	856	856	856
R-squared	0.507	0.132	0.309	0.652	0.785	0.630	0.787	0.855	0.186
Number of Counties	143	143	143	143	143	143	143	143	143
Panel B: Natural-logs Reduced Form, GLM estimate									
Post04×Incomplete×NewGovernor=0	-0.284**	0.573	0.040	0.111*	-0.028	0.120*	-0.104*	-0.023	-0.649***
	(0.119)	(0.416)	(0.183)	(0.062)	(0.058)	(0.063)	(0.061)	(0.065)	(0.190)
Post04×Incomplete×NewGovernor=1	-0.389***	0.495	-0.091	-0.015	-0.031	-0.062	-0.134***	-0.082**	-0.236
	(0.082)	(0.665)	(0.093)	(0.040)	(0.029)	(0.041)	(0.025)	(0.035)	(0.191)
Observations	856	856	856	856	856	856	856	856	856
Number of Counties	143	143	143	143	143	143	143	143	143

All regressions are weighted by 2005 county-level population.
All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.
Standard errors in parentheses are clustered at province level.
*** p<0.01, ** p<0.05, * p<0.1.

Appendices

2.A Notes on Economic Data Converting with OCR

Optical Character Recognition (OCR) is the technology that converts images of typed, handwritten or printed text into machine-encoded text in designated forms. With the development of digital image scanning and processing technology, using OCR software becomes a common method for converting spreadsheet in printed materials (such as books, archives, and reports) to data set that can be directly used in economic research and analysis. A printed spreadsheet consists of the contents (most are numbers), the horizontally and vertically aligned structure, and the frame lines that separate cells. Therefore, apart from the general converting errors, the structure of spreadsheets may introduce specific errors. In this part, we briefly discuss three types of conversion errors in the environment of digitizing spreadsheets, the consequent effects on data quality in terms of measurement errors in estimating average treatment effect, and some ways to fix them.

The first type of error comes from single characters. In most cases OCR works and provides accurate results. However, when the condition of source materials is poor, a single character can be wrongly read. For example, number "0" with a black stain on its bottom right could be read as letter "Q", and number "1" with a horizontal, dirty crease could be read as number "4". Since this type of error occurs randomly, it introduces same bias to data of treatment and control groups, and thus statistically only reduce the precision of estimates.

The second type of error is due to the poor quality of scanning. This is more likely to happen at certain position of pages, even if we place and scan the material in the correct way. For example, when we scan the County Public Finance Statistics Yearbook of China, which is a book of 1,500 pages, the quality of contents near the hinge is poor, because it is hard to flatten that part of a thick book on a book scanner. The impact of this type of error depends on how the spreadsheet is ordered. If variables are listed in columns, scanned variables close to the hinge may have larger measurement errors. On the contrary, if variables are listed in rows but observations are in columns, such measurement errors may fall to certain observations, and we need to test whether it is correlated with treatment assignment.

The last but most critical type of error comes from the frame line. If the numbers are too close to the frame line, the vertical frame line could be converted as part of numbers, leading to an additional digit "1" at the beginning or the end of the original number. For example, a right-aligned number "23456" can be converted to "234561". The lead "1" changes the value by adding 10^N where N is the number of digits in original value, while the lag "1" changes the value by multiplying the original value by 10. This type of error, if not being corrected, usually leads to asymmetric changes in variable distribution of treatment and control group.

Besides manual proofreading, we exploit the balancedness of the budget balance sheet to check for and correct errors. As all tables of revenue, expenditure and subsidy have their total and sub items, we simply check whether the summation of sub items equals to total item. This implementation can rule out most errors listed above.

The only remains are those with two same errors in one observation, but such cases are very rare.

2.B Government Spending and Political Incentives

2.B.1 Baseline Model

In this part we set up a model for local government behavior in response to the changes in intergovernmental subsidy. Using this model, we are able to develop some testable hypotheses in local government behavior.

Consider a two-period economy with a representative agent and a (local) government. At the very beginning, the government allocates its revenue on public expenditures. Following Alesina and Rodrik (1994), Barro (1990), Devarajan et al. (1996) and Yin and Zhu (2012), we assume that there are two different sector of spending: production-related expenditure (g_1) and welfare-related expenditure (g_2). In order to focus on the role of intergovernmental transfer, different from previous studies, we assume that the government collects no local taxes and the only source of revenue comes from an exogenous transfer, G_0, and the government's budget balance condition is therefore:

$$g_1 + g_2 = G_0$$

In the first period, the representative agent allocate her endowment k_0 onto consumption c_1 and capital stock k, and in the second period, the agent consumes c_2, which comes from the production of capital. Assume that labor supply is fixed, and further assume

that the production function is given by:

$$y = \tilde{A}k^{\gamma}(g_1\bar{l})^{1-\gamma} = Ak^{\gamma}g_1^{1-\gamma}$$

Assume that the agent has intertemporal discount factor β, and in each period she utilizes consumption and government's *durable* welfare goods, assume that the utility function is given by:

$$u_i(c_i, g_2) = \ln c_i + \alpha \ln g_2$$

Therefore, given government expenditures, the agent chooses each period's consumption c_1, c_2, and the capital stock k to maximize:

$$\max_{c_1,c_2,k} U^C = u_1 + \beta u_2 = \ln c_1 + \beta \ln c_2 + (1+\beta)\alpha \ln g_2$$

subject to

$$c_1 + k = k_0$$

and

$$c_2 = Ak^{\gamma}g_1^{1-\gamma}$$

we can solve for the equilibrium

$$c_1^* = \frac{1}{1+\beta\gamma}k_0$$

$$k^* = \frac{\beta\gamma}{1+\beta\gamma}k_0$$

$$c_2^* = Ag_1^{1-\gamma}(\beta\gamma)^{\gamma}\frac{k_0^{\gamma}}{(1+\beta\gamma)^{\gamma}}$$

Now, given the representative agent's response function, the government allocates g_1 and g_2 to maximize its utility function. We assume that the government is neutral and benevolent, i.e., maximizes the consumer's utility:

$$\max_{g_1,g_2} U^G = u_1^* + \beta u_2^* = \ln c_1^* + \beta \ln c_2^* + (1+\beta)\alpha \ln g_2$$

subject to

$$g_1 + g_2 = G_0$$

First-order condition implies that

$$\beta\left(\frac{1-\gamma}{g_1}\right) + (1+\beta)\alpha\left(\frac{1}{g_1 - G_0}\right) = 0$$

which solves the optimal share of production-related expenditure

$$\frac{g_1}{G_0} = \frac{1}{1 + \frac{\alpha}{1-\gamma}\left(1 + \frac{1}{\beta}\right)}$$

alternatively,

$$\frac{dg_1}{dG_0} = \frac{1}{1 + \frac{\alpha}{1-\gamma}\left(1 + \frac{1}{\beta}\right)}$$

or

$$\frac{d\ln g_1}{d\ln G_0} = \frac{1}{1 + \frac{\alpha}{1-\gamma}\left(1 + \frac{1}{\beta}\right)}\frac{G_0}{g_1}$$

The derivation implies that, when there is an exogenous change in intergovernmental transfer, local governments are expected to reduce productive expenditure and welfare-related expenditure proportionally, keeping their relative sizes unchanged. It is expected to see identical percentage changes.

Note that, with constant elasticity of substitution (CES) production function and utility function, the results may apply to general cases with multiple sectors. Therefore, if we compare the percentage changes of different categories of expenditure, without flypaper concern and political incentive concern, those estimates are expected to be the same.

2.B.2 Generalized Case with Political Incentive Concern

One strong assumption in the baseline model is that, the decision maker, i.e., local politician, are neutral and benevolent to maximize local representative agent's utility. In reality, this assumption is usually violated. Existing literature has found empirical evidence that governments' expenditure composition can reflect their ideology. Budge and Hofferbert (1990) examines the relation between U.S. party programs and federal government expenditures, and concludes that parties do stick to policies on which they are elected. Bräuninger (2005) finds that higher weight on social welfare spending in a party's policy agendas does lead to a significant increase in social security spending. Moreover, a variety of studies discuss about how political incentives or career concerns affect expenditure composition. Drazen and Eslava (2010) finds that under the competitive election system in Colombia, voter-targeted spending rises in election years relative to other categories of spending. Similar results are also discovered by Khemani (2000) for a study in India and Gonzalez (2002) in Mexico.

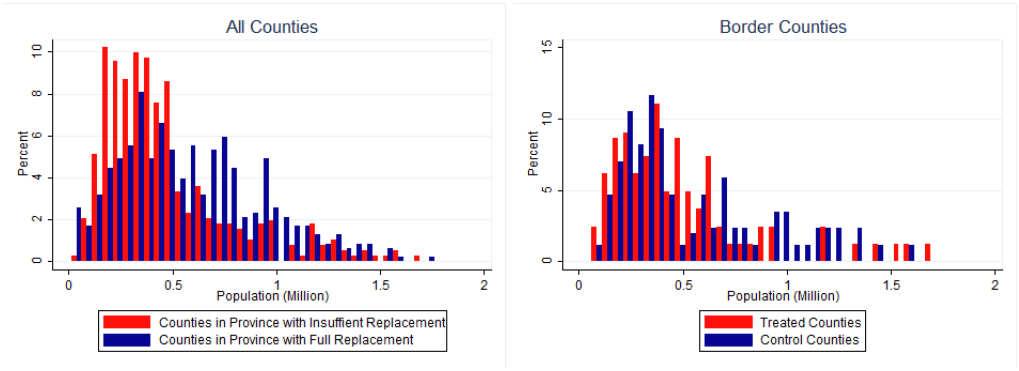
Different from most republic countries, China's county-level governors are usually appointed or dismissed by upper government. Therefore, for the local governors,

they consider about their evaluation from higher level government. Li and Zhou (2005) and Yin and Zhu (2012) find that under the central appointment system in China, local officials put higher priorities on production related spending (for example, agricultural and infrastructure expenditure) over welfare expenditure as their turnovers are largely based on the economic performance (mainly GDP) of their regions. However, this concern, if exists, should apply to counties on both sides of province border. Therefore, in a triple-differences design, it is expected to see no difference in percentage changes between groups.

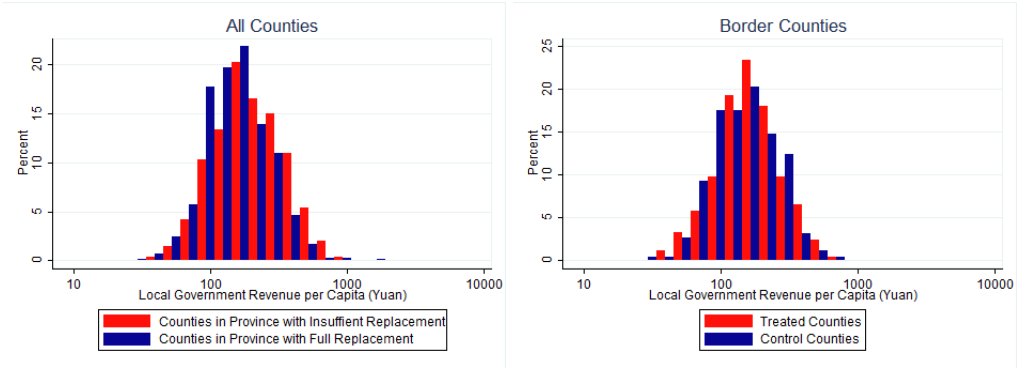
Appendix Figures and Tables

Figure 2.A: Variable Distribution

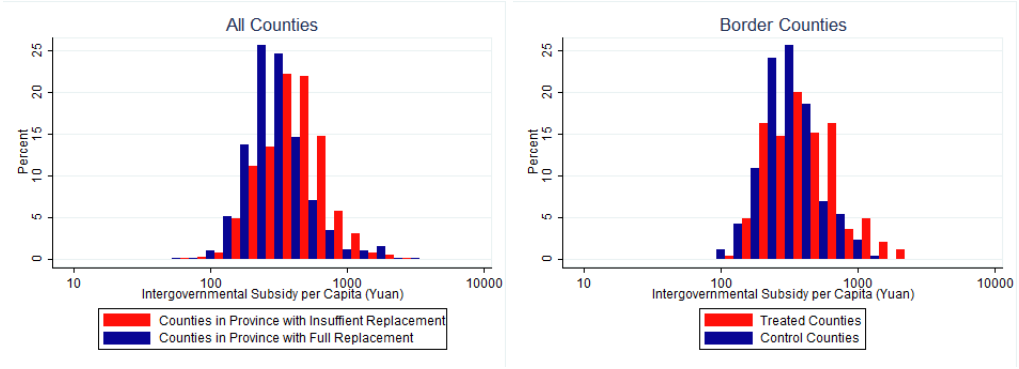
(a) Population



(b) Local Revenue



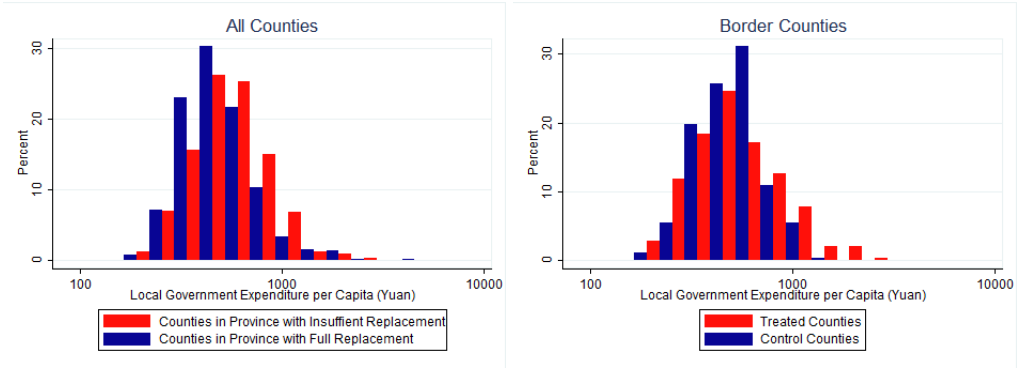
(c) Intergovernmental Subsidy



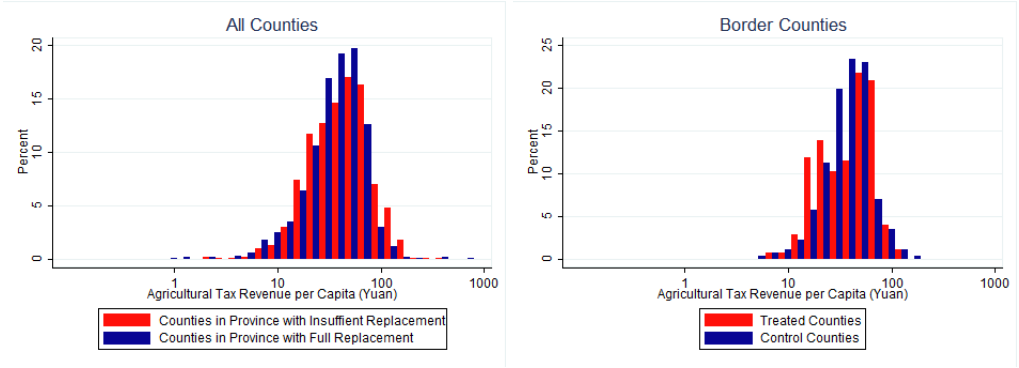
These figures show the distribution of key variables (population, local revenue, and intergovernmental subsidy) on average before 2004 for counties in province with full and incomplete subsidy replacement. By comparing the distributions, it shows that treatment and control counties in border sample are more similarly distributed than those of full sample.

Figure 2.B: Variable Distribution, Continued

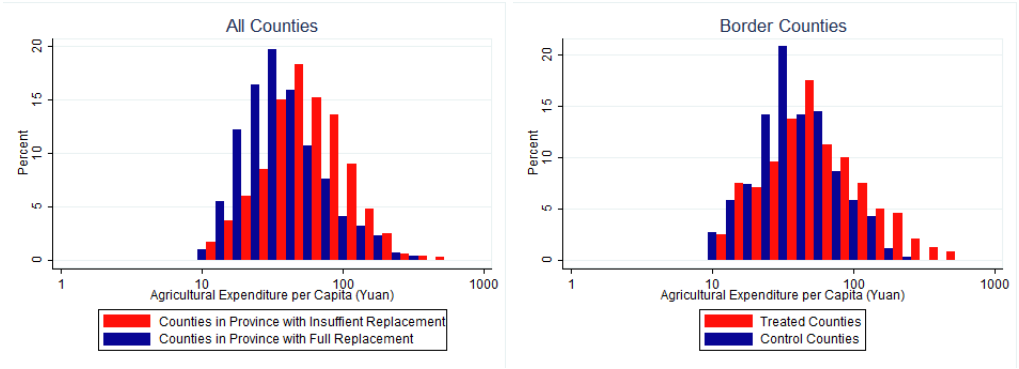
(a) Local Expenditure



(b) Agricultural Tax Income

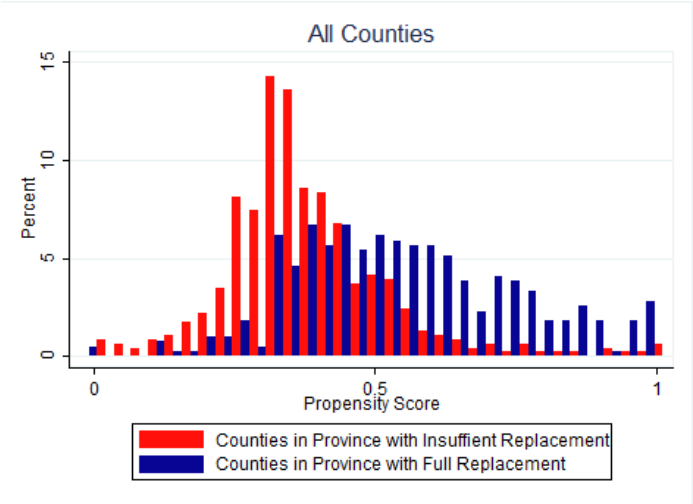


(c) Agricultural Expenditure



These figures show the distribution of key variables (local expenditure, agricultural tax income, and government expenditure on agriculture) on average before 2004 for counties in province with full and incomplete subsidy replacement. By comparing the distributions, it shows that treatment and control counties in border sample are more similarly distributed than those of full sample.

Figure 2.C: Propensity Score Distribution



This figure shows the distribution of propensity score of treatment and control group counties. Counties in treated and control provinces are matched by population, share of intergovernmental subsidy, and share of agricultural tax income in local revenue. The figure shows unbalanced distributions, which implies the importance of matching to find a comparable counterfactual.

Table 2.A: Revenue Sharing between the Central and Local Governments, Detailed

I. Taxes exclusively assigned to the Central and Provincial Governments

1. Excise taxes
2. Income tax of all central government enterprises
3. Taxes collected from the Ministry of Railroads and from the headquarters of banks and insurance companies
4. Income taxes, sales taxes and royalties from offshore oil activities of foreign companies and joint ventures
5. Energy and transportation fund contribution
6. Seventy percent of the three sales taxes collected from enterprises owned by the Ministry of Industry, the Ministry of Power, SINOPEC (petrochemicals), and the China nonferrous metals companies.
7. All customs duty, VAT and excise taxes on imports
8. Enterprise income tax collected from banks and other financial institutions.

II. Taxes shared between the central and local governments

1. Value-added tax (75 percent central, 25 percent provincial)
2. Natural resource taxes (coal, gas, and other minerals if the enterprises are fully State-owned.)
3. Construction tax on the cost of construction of buildings that are outside the plan and financed from retained earnings
4. Salt tax
5. Security and exchange tax (50 percent central, 50 percent provincial)
6. Industrial and commercial tax, and income tax levied on foreign and joint venture enterprises.

III. Taxes exclusively assigned to local governments

1. Income tax and adjustment tax of locally owned state enterprises, collectives, and private enterprises (including agricultural tax)
2. Business (gross receipts) tax falling on sectors not covered by VAT (transportation and communications, construction, finance and insurance, post and telecommunications, culture and sports, entertainment, hotels and restaurants, and other)
3. Rural market (stall rental) trading tax
4. Urban maintenance and construction tax (a surcharge on the tax liability of enterprises for business tax, consumption tax, and VAT)
5. The urban land use tax
6. Vehicle and vessel utilization tax
7. Thirty percent of the product and VAT revenues collected from enterprises owned by the Ministry of Industry, Ministry of Power, SINOPEC, and the China nonferrous metals companies
8. Individual income tax
9. Value-added tax on land
10. Education surtax
11. Entertainment and slaughter taxes
12. Property tax
13. Surtax on collective enterprises
14. Resources tax
15. Fixed asset investment tax (discontinued in 1999)
16. Fines for delinquent taxes.

Source: Bahl (1999).

Table 2.B: Expenditure responsibilities between Central and Local Governments, 2003-2006

	Central Exp/Sub-Item Exp	Local Exp/Sub-Item Exp	Central Exp/Central Total Exp	Local Exp/Local Total Exp
Agriculture	9.4%	90.6%	1.93%	6.60%
Infrastructure	38.8%	62.2%	17.79%	10.26%
Education	7.8%	93.2%	3.11%	15.05%
Health	2.3%	97.7%	0.28%	4.17%
Social Security	12.2%	87.8%	2.55%	6.33%
Pension	0.9%	99.1%	0.07%	2.85%
Administration	16.7%	83.3%	5.44%	9.60%
Public Defense	98.8%	1.2%	29.43%	0.12%

Notes:This table shows partial expenditure responsibilities between central and local government in China. The first two columns represents the percentages in terms of a specific sub item expenditure, while the last two columns show the percentages of total expenditure for that level of government. All the percentages are averaged between 2003-2006. A complete table can be found from Tan (2010).

Table 2.C: Descriptive Statistics of All Sample

Unit: RMB per capita

	Control Group		Treatment Group	
	Mean	Std.dev.	Mean	Std.dev.
Total Revenue	549.01	323.72	679.70	323.09
Local Revenue	189.70	118.50	215.25	131.16
Agricultural Tax	45.21	35.08	47.33	33.00
Value Added Tax	22.59	22.64	34.38	37.22
Personal Income Tax	8.90	8.47	13.37	18.20
Sales Tax	30.88	26.52	41.97	36.48
Other Income	144.48	100.74	167.91	123.13
Intergovernmental Subsidy	359.32	280.88	464.46	277.62
Total Expenditure	507.19	290.04	625.72	305.78
Agriculture	46.90	41.49	68.22	55.51
Infrastructure	30.39	69.14	23.17	41.36
Education	120.89	52.11	164.13	59.17
Administration	76.95	63.51	92.82	50.49
Social Security	16.67	14.44	12.29	12.17
Public Safety	28.52	17.17	34.96	19.85
Miscellaneous	178.63	103.22	218.30	114.80
Gross Regional Product in				
Primary Sector	1,615.24	968.35	1,634.12	843.9669
Secondary Sector	1,961.14	1,630.95	1,660.58	1,766.45
Tertiary Sector	1,645.70	2,144.5	1,280.19	1,148.22
Population	607,388	341,593	454,708	313,375
Number of Counties	470		391	
Number of Observations	1,409		1,174	

Notes: This table shows the pre-treatment descriptive statistics of counties in treatment group and control group. Revenue, expenditure and gross regional product are in unit of RMB per capita (2004 real price).

Table 2.D: Policy Effects on Local Government Revenues: Matching

Unit: RMB per capita

Outcome Variable	(1) Agr. Subsidy	(2) Agr. Revenue (Tax+Subsidy)	(3) Other Local Revenues	(4) Other Central Subsidies	(5) Net Revenue Change
Panel A: Reduced form, OLS estimate					
Post04×Incomplete	-28.323***	-19.960**	5.002	-11.599	-26.557
	(5.064)	(8.312)	(12.938)	(20.692)	(24.055)
Mean Dep.Var.	13.996	49.295	164.313	415.435	629.043
Observations	4,321	4,321	4,321	4,321	4,321
R-squared	0.761	0.321	0.613	0.698	0.763
Number of Counties	730	730	730	730	730
Panel B: Natural-logs Reduced form, GLM estimate					
After×Treatment		-0.388**	-0.123**	-0.092	-0.120***
		(0.197)	(0.054)	(0.059)	(0.030)
Observations		4,321	4,321	4,321	4,321
Number of Counties		730	730	730	730

Agricultural revenues include agricultural tax revenues and agricultural subsidy revenues (which only exists after 2004).

All regressions are compounded-weighted by 2005 county-level population and caliper matching weights.

All regressions are controlled with county fixed effects, year fixed effects, and fraction of agriculture tax revenue in local total revenue(subsidy not included, proxy for importance of agricultural tax to local economy).

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.E: Placebo Test: Policy Effect on Local Government Revenues

Unit: RMB per capita

Outcome Variable	(1) Agr. Revenue (Tax+Subsidy)	(2) Other Local Revenues	(3) Other Central Subsidies	(4) Net Revenue Change
Panel A: Fake Treatment Time: 1 year before				
Post03×Incomplete	-1.6317	-5.0401**	12.7681	6.0963
	(1.3570)	(2.2005)	(7.3006)	(8.7913)
Mean Dep.Var.	47.831	109.461	336.766	494.058
Observations	335	335	335	335
R-squared	0.327	0.367	0.509	0.585
Panel B: Fake Treatment Time: 2 year after				
Post06×Incomplete	-3.0386*	-5.2265	-7.5228	-15.7878
	(1.685)	(7.393)	(14.066)	(17.421)
Mean Dep.Var.	48.735	174.799	542.473	766.006
Observations	335	335	335	335
R^2	0.984	0.987	0.986	0.988
Panel C: Fake Border of Treatment: Within Treatment Group				
Post04×FakeBorder(T)	4.7991*	7.4672	-14.1102*	-1.8438
	(1.9664)	(3.9921)	(6.7973)	(7.3537)
Mean Dep.Var.	39.671	141.283	427.873	608.827
Observations	934	934	934	934
R^2	0.661	0.924	0.958	0.960
Panel D: Fake Border of Treatment: Within Control Group				
Post04×FakeBorder(C)	3.2584	7.6816	15.3143	26.2543
	(2.6468)	(6.7471)	(14.0052)	(19.0888)
Mean Dep.Var.	54.631	137.451	372.534	564.617
Observations	923	923	923	923
R^2	0.845	0.913	0.916	0.923

Agricultural revenues include agricultural tax revenues and agricultural subsidy revenues (which only exists after 2004).

All regressions are weighted by 2005 county-level population.

All regressions are controlled for county fixed effects, year fixed effects, border-specific trends, and fraction of agriculture tax revenue of local total revenue(subsidy not included, proxy for importance of agricultural tax to local economy).

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.F: Policy Effects on Local Government Expenditures: Matching

Unit: RMB per capita

Outcome Variable	(1) Agriculture	(2) Infrastructure	(3) Social Security	(4) Administration	(5) Education	(6) Public Safety	(7) Miscellaneous	(8) Total	(9) Reserves
Panel A: Reduced Form, OLS estimate									
Post04×Incomplete	-16.606*	0.668	-3.870	1.798	1.167	0.291	-5.408	-16.783	-9.774
	(8.820)	(8.426)	(2.982)	(3.513)	(4.517)	(2.487)	(10.155)	(20.670)	(12.702)
Mean Dep.Var.	55.992	25.235	16.046	79.627	144.256	32.453	202.646	575.569	53.475
Observations	4,321	4,321	4,321	4,321	4,321	4,321	4,321	4,321	4,321
R-squared	0.426	0.062	0.266	0.591	0.599	0.521	0.725	0.770	0.205
Number of Counties	730	730	730	730	730	730	730	730	730
Panel B: Natural-logs Reduced Form, GLM estimate									
Post04×Incomplete	-0.372***	0.167	-0.159	-0.035	-0.057	-0.070*	-0.109***	-0.098***	-0.137
	(0.076)	(0.392)	(0.138)	(0.023)	(0.036)	(0.042)	(0.042)	(0.034)	(0.091)
Observations	4,321	4,321	4,321	4,321	4,321	4,321	4,321	4,321	4,321
Number of Counties	730	730	730	730	730	730	730	730	730

Agricultural revenues include agricultural tax revenues and agricultural subsidy revenues (which only exists after 2004).

All regressions are compounded-weighted by 2005 county-level population and caliper matching weights.

All regressions are controlled with county fixed effects, year fixed effects, and fraction of agriculture tax revenue in local total revenue(subsidy not included, proxy for importance of agricultural tax to local economy).

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.G: Policy Effects on Local Government Revenue by Level of Agricultural Dependence

Unit: RMB per capita

Outcome Variable	(1) Agr. Subsidy	(2) Agr. Revenue (Tax+Subsidy)	(3) Other Local Revenues	(4) Other Central Subsidies	(5) Net Revenue Change
Panel A: Reduced form, OLS estimate					
Post04×Incomplete×DepHigh=0	-28.182***	-16.737***	-1.064	-13.376	-31.177
	(3.432)	(4.367)	(15.825)	(24.130)	(29.949)
Post04×Incomplete×DepHigh=1	-35.168***	-45.432***	-10.225	8.750	-46.906***
	(4.695)	(12.500)	(7.809)	(14.761)	(13.354)
Observations	1,002	1,002	1,002	1,002	1,002
R-squared	0.804	0.461	0.645	0.836	0.847
Number of Counties	168	168	168	168	168
Panel B: Natural-logs Reduced form, GLM estimate					
Post04×Incomplete×DepHigh=0		-0.241*	-0.040	-0.181***	-0.147***
		(0.132)	(0.066)	(0.065)	(0.041)
Post04×Incomplete×DepHigh=1		-1.039***	-0.023	0.031	-0.059*
		(0.349)	(0.062)	(0.095)	(0.032)
Observations		1,002	1,002	1,002	1,002
Number of Counties		168	168	168	168

Agricultural Dependence is measured by share of agricultural tax income (pre-treatment) among total local income. Low and high group have mean of 0.171 and 0.376, respectively.

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects,year fixed effects× border-segment fixed effects, and fraction of agriculture tax revenue in local total revenue(subsidy not included, proxy for importance of agricultural tax to local economy).

Those counties not on the province border is clustered into the same border segment as its closest neighbor county, where the distance is calculated using county seat-to-county seat straight distance.

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.H: Policy Effects on Local Government Expenditures by Level of Agricultural Dependence

Unit: RMB per capita

Outcome Variable	(1) Agriculture	(2) Infrastructure	(3) Social Security	(4) Administration	(5) Education	(6) Public Safety	(7) Miscellaneous	(8) Total	(9) Reserves
Panel A: Reduced Form, OLS estimate									
Post04×Incomplete×DepHigh=0	-12.479	1.930	-3.855	6.416	8.192	2.685	-19.672*	-5.489	-25.687
	(8.292)	(10.354)	(3.584)	(4.871)	(5.557)	(2.265)	(10.692)	(29.269)	(17.086)
Post04×Incomplete×DepHigh=1	-18.621**	8.531	-2.233	2.512	2.333	-2.105	-19.644***	-28.966*	-17.940*
	(8.243)	(9.151)	(1.858)	(4.732)	(3.999)	(1.438)	(4.787)	(15.532)	(8.599)
Observations	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002
R-squared	0.494	0.093	0.312	0.648	0.780	0.633	0.770	0.848	0.194
Number of Counties	168	168	168	168	168	168	168	168	168
Panel B: Natural-logs Reduced Form, GLM estimate									
Post04×Incomplete×DepHigh=0	-0.393***	0.198	-0.220	-0.035	-0.056	-0.025	-0.164***	-0.114***	-0.435
	(0.089)	(0.446)	(0.143)	(0.030)	(0.037)	(0.046)	(0.028)	(0.038)	(0.292)
Post04×Incomplete×DepHigh=1	-0.324***	0.861	0.051	0.059**	0.007	-0.031	-0.086***	-0.019	-0.299*
	(0.082)	(0.586)	(0.095)	(0.029)	(0.025)	(0.029)	(0.028)	(0.032)	(0.164)
Observations	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002
Number of Counties	168	168	168	168	168	168	168	168	168

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.

Standard errors in parentheses are clustered at province-border level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.I: Policy Effects on Local Government Revenue by Level of Wealth

Unit: RMB per capita

Outcome Variable	(1) Agr. Subsidy	(2) Agr. Revenue (Tax+Subsidy)	(3) Other Local Revenues	(4) Other Central Subsidies	(5) Net Revenue Change
Panel A: Reduced form					
Post04×Incomplete×Rich=0	-33.398***	-38.478**	-9.758	1.131	-47.105***
	(4.971)	(13.460)	(8.983)	(15.250)	(13.514)
Post04×Incomplete×Rich=1	-31.409***	-29.379**	-1.692	0.763	-30.309
	(5.113)	(12.555)	(17.686)	(30.355)	(33.124)
Observations	1,002	1,002	1,002	1,002	1,002
R-squared	0.801	0.403	0.645	0.835	0.847
Number of Counties	168	168	168	168	168
Panel B: Natural-logs Reduced form, GLM estimates					
Post04×Incomplete×Rich=0		-0.948**	-0.018	-0.030	-0.078**
		(0.408)	(0.079)	(0.100)	(0.037)
Post04×Incomplete×Rich=1		-0.550*	-0.045	-0.120	-0.129**
		(0.288)	(0.069)	(0.094)	(0.052)
Observations		1,002	1,002	1,002	1,002
Number of Counties		168	168	168	168

Richness is measured by county GDP.

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects,year fixed effects× border-segment fixed effects, and fraction of agriculture tax revenue in local total revenue(subsidy not included, proxy for importance of agricultural tax to local economy).

Those counties not on the province border is clustered into the same border segment as its closest neighbor county, where the distance is calculated using county seat-to-county seat straight distance.

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

j

Table 2.J: Policy Effects on Local Government Expenditures by Level of Wealth

Unit: RMB per capita

Outcome Variable	(1) Agriculture	(2) Infrastructure	(3) Social Security	(4) Administration	(5) Education	(6) Public Safety	(7) Miscellaneous	(8) Total	(9) Reserves
Panel A: Reduced Form, OLS estimate									
Post04×Incomplete×Rich=0	-16.811*	3.826	-2.519	3.185	6.784	-1.683	-18.915***	-24.577	-22.528**
	(8.463)	(9.850)	(1.919)	(6.027)	(5.541)	(1.737)	(4.457)	(18.025)	(9.090)
Post04×Incomplete×Rich=1	-15.812*	10.876	-3.348	5.228	-0.282	2.011	-21.090*	-13.305	-17.004
	(8.425)	(11.406)	(3.844)	(5.230)	(4.421)	(2.150)	(10.374)	(31.667)	(13.695)
Observations	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002
R-squared	0.492	0.093	0.311	0.647	0.780	0.630	0.770	0.847	0.193
Number of Counties	168	168	168	168	168	168	168	168	168
Panel B: Natural-logs Reduced Form, GLM estimate									
Post04×Incomplete×Rich=0	-0.328***	0.617	0.013	0.037	0.023	-0.026	-0.103***	-0.035	-0.376**
	(0.088)	(0.766)	(0.094)	(0.028)	(0.032)	(0.039)	(0.031)	(0.037)	(0.175)
Post04×Incomplete×Rich=1	-0.399***	0.464	-0.188	-0.018	-0.085**	-0.030	-0.151***	-0.102*	-0.329
	(0.088)	(0.419)	(0.147)	(0.057)	(0.037)	(0.050)	(0.039)	(0.053)	(0.210)
Observations	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002
Number of Counties	168	168	168	168	168	168	168	168	168

Richness is measured by county GDP.

All regressions are weighted by 2005 county-level population.

All regressions are controlled with county fixed effects, year-by-border-segment fixed effects, and covariates of per-capita GDP in each sector and share of elementary school/middle school students in all population.

Standard errors in parentheses are clustered at province level.

*** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

Market Sensitivity to Product Quality, Personal Information, and Business Practice Scandals[1]

[1]This chapter is co-authored work with Jijian Fan

3.1 Introduction

Contaminated food, faulty automobile engines, deceptive advertisements and personal information data breaches make the news on a regular basis. In 2017, 20 million pounds of food were recalled by the U.S. Department of Agriculture (USDA) and 30 million vehicles were affected by National Highway Traffic Safety Administration (NHTSA)'s recalls. In 2018, there was a breach of more than 50 million Facebook users' private information. These facts are not only harmful to consumers but can also be highly problematic for firms (Van Heerde et al., 2007). Existing literature has shown that firm scandals such as defective products, corporate fraud, and layoff announcements can impose costs on firms, pose a threat to firm reputations, and hurt stock prices (Dawar and Pillutla, 2000; Marsh et al., 2004; Korkofingas and Ang, 2011; Crutchley et al., 2007; Farber and Hallock, 2009; Falkheimer and Heide, 2015). The paper contributes to the literature by examining if certain types of scandals generate larger shocks and if some industries are more exposed to negative effects.

A major challenge to answering these questions is that scandals are highly heterogeneous, receive different levels of media coverage, and may only be identified ex-post as scandals due to the level of attention they receive. Moreover, variation in the timing of a scandal being exposed further complicates comparisons due to changes in the market environment. A third challenge is that a variety of scandal types and industries are required. In practice, for example, the USDA announces only food safety issues and the NHTSA announces only automobile recalls, so most existing studies

have only considered a single type of scandal for a single industry and therefore, does not make comparisons across different scandal classifications (Govindaraj et al., 2004; Korkofingas and Ang, 2011; Ni et al., 2016). By utilizing a unique event in China, this paper can systematically examine the effects of firm-level scandals under a uniform treatment. The "3.15 Night" is an annual television program held on China's consumer rights day. It is hosted by China's central government, the China Consumer Association and the predominant state television broadcaster, China Central Television (CCTV). The TV program first aired on March 15^{th}, 1991 and has been broadcast annually for 28 years (1991-2018). As the show is hosted by the central government and airs on CCTV, a TV channel watching by 95.5% of the Chinese population, the program has high viewership. The two-hour program aims to expose a range of companies whose violations of laws affect Chinese consumers. More than 200 violations by 250 companies have been broadcast over the past 28 years. The scandal-hit companies vary from small local farms to Fortune Global 500 companies, and the violations range from bank employees selling personal information to fast food restaurants serving expired food. In the program, each scandal is revealed in a similar form as a 15-minute documentary. The documentary consists of both undercover investigations and interviews with victims, experts, and government agents. As the "3.15 Night" provides a platform that systematically exposes various scandals, we can consider both their average effects and differentiate effects on the extensive and intensive margins by scandal type and industry.

This paper examines the financial impacts of various scandal exposures on firms. Following the existing literature, we calculate the daily firm-level abnormal re-

turns based on the market model (McWilliams and Siegel, 1997; Wang et al., 2014), and apply the standard event study approach to explore the changes in cumulative abnormal returns relative to the event day (Farber and Hallock, 2009; Nguyen and Nielsen, 2010). Using each episode of "3.15 Night" from 2007 to 2018, we document 178 scandal-hit Chinese and global firms. In order to conduct a heterogeneous analysis based on scandal types, we group the scandals into three categories: product quality, personal information breach, and deceptive business practices. Additionally, to make across sector comparisons, we divide all the firms into three sectors: consumer goods, consumer services, and technology. As our primary interest is studying stock price reactions, we evaluate publicly traded firms or firms whose parent companies are publicly traded.

The preliminary analysis reveals several interesting results. Existing studies estimating the average effects of negative firm-level shocks on stock market prices find effects range from -29% to nearly 0, and our estimate of the average effect is -4.1%, which is within this range. Regarding scandal types, we find that the market cares about the product quality scandal most, with a sharp drop of 9% right after the exposure and an average decline of 12%. However, this estimation is relatively larger than earlier studies focusing on those types of scandals. The larger magnitude may be attributed to three reasons. First, the severity of the scandal can affect the magnitude (Gokhale et al., 2014). As the scandals revealed in the show are all very significant and representative, the impacts could be larger. Secondly, the format of scandal revelations is different. Product hazard issues examined by prior studies, for example, are typically revealed by a recall announcement posted on the Wall Street Journal (Chen et al., 2009; Hsu

and Lawrence, 2016; Ni et al., 2016), while in our settings, all scandals are exposed by undercover investigations and interviews broadcast on national televisions, and thus, may provide more direct evidence and be more salient to consumers. Thirdly, the scandal firms in our sample are made up of industry-leading companies. It is possible that an unexpected violation from those companies generates a larger market response (Rhee and Haunschild, 2006).

In today's digital world, businesses establish large databases with all kinds of personal information, from credit card numbers to the number of kids in a house, and therefore, breaches of privacy events are increasingly attracting researchers' attention. Recent studies by Malhotra and Kubowicz Malhotra (2011) and Martin et al. (2017) find such breaches have negative effects on firms' performances. Similarly, we also find breaches of personal information generate strong responses by the market. The scandal exposure causes an immediate value loss of 3.2% and an average value loss of 4.4%. Our results are in line with Campbell et al. (2003) that shows a 5.5% value loss due to breaches involving unauthorized access to customer personal data or proprietary firm data. However, we do not find any significant effects from deceptive business practices. Concerning different sectors, we find the consumer goods sector experiences average losses of 9% and appears to be the most vulnerable sector to scandals. The services sector is the second most impacted with an average loss of 6.5%. The market responds little to scandals in the technology sector. A possible explanation for this result could be problems that occur in this sector are less directly harmful, or markets tend to have a higher tolerance for this growth sector.

Our paper contributes to the literature in three ways. First, we utilize a unique event in which scandals are revealed in a systematic way. All firms are treated uniformly, enabling us to make comparisons across scandal types and sectors to uncover heterogeneity in market response. Although numerous empirical works have examined the consequences of adverse firm-level shocks, most of them only consider a single type of shock (Worrell et al., 1991; Farber and Hallock, 2009; Nguyen and Nielsen, 2010; Knittel and Stango, 2013) arising from a single industry (Thomsen and McKenzie, 2001; Govindaraj et al., 2004; Thirumalai and Sinha, 2011; Gokhale et al., 2014; Ni et al., 2016). Secondly, by using the unique event, we solve selection bias that occurs in the process of identifying scandals to consider. Media exposure of a scandal may increase the probability that an event is included in a study. However, we are able to consider all scandals reported by the show, so this type of selection is not an issue. Finally, under a uniform treatment allowing systematic scandal exposures for the same media coverage in similar timing via similar methods of reporting, the selection issue does not exist. Additionally, our study provides new empirical evidence on the effects of personal information breaches, which few studies have considered previously (Campbell et al., 2003; Acquisti et al., 2006).

The paper is organized as follows. Section 3.2 provides an overview of the "3.15 Night" show, its history, and the detail of its contents. Section 3.3 discusses the data sources and summarizes sample characteristics. Section 3.4 introduces the empirical strategies used in this paper and section 3.5 presents empirical evidence on the effects of scandals. Section 3.6 concludes.

3.2 Background

In the early 1990s, China's market was flooded with fake and defective products. At that time, as the market system, mechanism and laws were underdeveloped, consumers played extremely weak roles, and their rights were frequently ignored and violated(Yin, 1998). In order to protect consumers' rights and suppress harmful business activities, on March 15^{th}, 1991, a TV program called"3.15 Night" aired. The "3.15 Night" has been broadcast for 28 years(1991-2018) and is hosted by over 15 central government departments including the Supreme People's Court, the Supreme People's Procuratorate, the Ministry of Public Security, the Ministry of Justice,the Ministry of Commerce, the Ministry of Agriculture, the Ministry of Public Health,etc., together with the China Consumer's Association and the state television broadcaster, CCTV. During the past 28 years, the program successfully exposed business scandals from a variety of industries such as food, medical, automobile, finance, information technology, etc. More than 250 companies were accused of violating consumers' rights. These accusations include selling infant food with overdose additives, misleading consumers by fraudulent online transactions and reviews, producing poor quality tiers, and collecting personal information, etc.

The "3.15 Night" show airs annually on March 15^{th} on the China's Consumer Rights Day. Every year, it has a unique theme and all scandal cases are organized around that theme. For example, the theme in 2012 was "Consumption in the Sun" and "Make Consumers More Dignified" in 2014. After deciding the theme, the core

team starts to collect consumers' complaints and reports from various platforms and selects the most representative or promising leads to further investigate. Sometimes, the investigations could last as long as 6 months. All procedures are carried out under confidential agreements. Based on an interview from Huaxia Times, the security measures include 1) requiring all people to sign confidential agreements; 2) having closed-off management where staff from different cases are not allowed to communicate with each other; 3) building closed working environment that restricts personal activity; 4) prohibiting phone usage in-studio and 5) destroying all related drafts and manuscripts at least once a day.

During the show, a 15-minute documentary is played for each offensive behavior. The documentary typically consists of three parts 1) undercover investigations; 2) victim interviews, and 3) expert/government agent interviews.

Undercover InvestigationsJournalists from CCTV hide in targeted firms as employees, disguise themselves as business partners or pretend to be consumers to obtain trust and collect evidence. Secret cameras will record all the pictures and conversations used in the investigations. For example, in 2012, to collect evidence of McDonald's serving expired food, a journalist worked as an employee in McDonald's and recorded images of other employees using expired meat to cook burgers.

Victim Interviews In this part, victims voluntarily tell their own experiences with and stories of certain products. For instance, in 2014 Mr. Wan shared his story in the documentary about how Nikon kept refusing to replace his camera lens although it was still under warranty. In 2015, Mr. Liu shared a video of his Land Rover suddenly

losing power when he was driving on an expressway.

Experts/Government Agents Interviews In the last part of the documentary, experts or government agents are invited to comment on the case. Typically, they point out what type of legislation those activities violated, how severe the violation is and how customers should protect themselves from such violations.

Hosted by the central governments and supported by authentic evidence, the "3.15 Night" generates huge impacts not only to Chinese firms but also to other global firms. For example, in 2013, the show criticized Apple of its discriminatory after-sales service in China. Early in 2012, the China Customer Association had warned Apple twice of its problematic service policy, but Apple did not give out any clear response. However, after the exposure on "3.15 Night", Apples CEO Tim Cook issued an apology letter to Chinese customers and made adjustments to the policy very quickly.

3.3 Data

The data used in this study comes from a variety of sources. First, scandal-hit firms are obtained by watching each episode of "3.15 Night" from 2007 to 2018. Second, scandal-hit companies, as well as their parents companies, are linked to their daily closing prices and the corresponding market or industry sector indices. Finally, we collect business data such as total revenue, sector revenue, and other important information for each exposed firm. This section describes detailed steps in dealing with all the data.

In order to get a complete list of scandal-hit firms, we watched each episode of "3.15 Night" from 2007 to 2018 and created a list that includes 178 investigated firms. Meanwhile, for each firm, we document its offenses, affected brands, product categories, or service, the legislation it violated, the length of each documentary, whether there was an undercover investigation, whether there was any interview, and whether there was any enforcement inspection, etc. As our primary interest is studying stock price reactions, we further narrow the sample to publicly traded firms or firms' parent companies are publicly traded. This gives us 44 different firms traded on seven global stock markets[2]. Table 3.1 displays all the publicly traded scandal firms in our sample. In some cases, firms are dual-listed across markets, and we use stock prices from their primary markets. It is also important to notice that firms such as the Agriculture Bank of China and Volkswagen AG are exposed more than once across years, and therefore, their stocks are counted more than once in our sample. We examine 52 affected stocks in total.

The daily closing stock prices of each firm are obtained from the Wind Financial Database. As our study includes firms from seven stock markets, we follow the existing literature on international stock markets and collect the most frequently used indices for each market. The indices we used in this paper are: 1) Shenwan Index for China mainland stock exchanges; 2) Heng Seng Index for the Hong Kong Stock Exchange; 3) S&P 500 Index for the United States; 4) Nikkei 500 for Japan; 5) KOSPI for South Korea; 6) DAX for Germany, and 7) CAC40 for France.

[2]We exclude trading firms from China secondary stock markets or insufficient pre-event data were available to calculate abnormal returns

Finally, we gather business-related information from firms' financial statements and annual reports. As the scandal may or may not come from firms' primary sectors, we adjust the stock price responses into a comparable measure across scandals by collecting revenue shares of the scandal-hit sectors. Additionally, because the scandals arise from the Chinese market, the regional revenue also helps us measure the importance of the Chinese market and gets unbiased estimates. We also account for ownership shares of joint ventures in the relevant few cases. Other business information from the annual reports such as R&D spending, advertising spending, and political connections will also benefit our further analysis.

3.4 Methodology

3.4.1 The Market Model

In an efficient market, stock prices can immediately reflect the financial impact of an unanticipated event (Malkiel and Fama, 1970). A rich literature has examined stock market reactions to understand the effects of firm-level scandals. For example, Ni et al. (2016) estimate firms' cumulative abnormal returns after recalls of tainted toys and Fisman and Wang (2015) study the negative stock returns after disclosures of workers' fatal accidents. paper examines changes in the cumulative abnormal returns of scandal-hit firms relative to the scandal exposure time. We follow MacKinlay (1997) and use an estimation window of the 250 prior trading days to estimate normal returns as shown in equation 3.1 and use the market model as McWilliams and Siegel (1997) to

estimate the abnormal returns shown in equation 3.2. Prior literature has widely applied the market model to study firm-level scandals (i.e.Thirumalai and Sinha (2011); Wang et al. (2014); Ni et al. (2016)). The market model is defined in the following way,

$$R_{it} = \alpha_i + \beta_i R_{It} + _{i,t} \tag{3.1}$$

where R_{it} is firm i's daily return at day t and R_{It} is the market or industry index for that day. Next, the daily abnormal return is calculated as

$$AR_{it} = R_{it} - (\hat{\alpha}_i + \hat{\beta}_i R_{It}) \tag{3.2}$$

and the cumulative abnormal return over an event window of [t_1,t_2] is defined as

$$CAR_{it}(t_1, t_2) = \sum_{t=t_1}^{t_2} AR_{it} \tag{3.3}$$

The R_{It} plays the role of the control group in estimating the abnormal returns. Instead of using market indices as the control group, we compare scandal-hit stocks to their sector indices, because it helps to further isolate noises beyond the scandal impacts on the sector. A concern of using such indices is if most of the targeted firms come from the same sector or industry and make up significant parts of an index, then the sector index could be contaminated. However, in our settings, within a sector, only a tiny proportion of firms are investigated each year. Additionally, we do not observe any significant changes in the sector indices during the event windows.

3.4.2 Empirical Strategy

An event study framework has been traditionally used in understanding the financial impacts of events such as policy announcements or elections. It also has been

widely employed in learning the effects of firm-level shocks such as product recalls (Govindaraj et al., 2004; Thirumalai and Sinha, 2011), layoff announcements (Farber and Hallock, 2009) and CEO deaths (Nguyen and Nielsen, 2010). Our baseline model follows the standard event study approach and assumes a single factor model. That is, for stock i at time t, we estimate the equation 3.4,

$$CAR_{it}^{adjusted} - CAR_{i0}^{adjusted} = \beta_0 + \beta_1 Post_t + stock_i + year_t + _{it} \tag{3.4}$$

where

$$CAR_{it}^{adjusted} = \frac{CAR_{it}}{ProductShare \times ChinaRevenueShare} \tag{3.5}$$

$CAR_{it}^{adjusted}$ is the adjusted cumulative abnormal returns. In reality, the magnitude of the stock responses will depend on whether the scandals arise from firms' major products and primary businesses. Additionally, as the scandals stem from China, the importance of the Chinese market should also be taken into account. For example, in 2017, NIKE, Inc. was accused of false advertising its basketball shoes in China. Therefore, in order to estimate the full scandal-hitting impacts on NIKE, we adjust the CAR by the share of revenues obtained from the footwear sector (0.61) and the Chinese market (0.12) in 2016. As a result, NIKE's CAR is scaled up by a multiplier of 13.7. The baseline day is set as March 14^{th} (day 0), which is a day before the scandal announcement. As we are interested in changes of the CAR relative to the baseline day, we further subtract the $CAR^{adjusted}$ at day 0 from day t to get the differences. $Post_t$ is a dummy variable for scandal exposure on or after March 15 every year. $stock_i$ is the fixed effects for scandal stocks and $time_t$ is the year fixed effects. The coefficient of

interests is β_1 which represents the overall effects of scandal exposure.

We group scandals into three categories: product safety, personal information breach, and deceptive business practices. To get the magnitudes of different scandal types and make comparisons among them, we interact the $Post_t$ dummy with the scandal categorical variable,

$$CAR_{it}^{adjusted} - CAR_{i0}^{adjusted} = \alpha_0 + \alpha_1 Post_t + \alpha_2 Post_t \times ScandalType_i + \alpha_3 ScandalType_i + stock_i + year_t + _{it} \quad (3.6)$$

where α_2 presents stock responses to various scandal types, and α_1 and α_3 will be absorbed by the fixed effects. Similarly, in order to find how firms from different sectors react distinctly to adverse shocks, we interact the $Post_t$ dummy with the sector categorical variable, which includes consumer goods, consumer services, and technology. The coefficient in front of the interaction term reveals the vulnerability of those sectors to scandal.

3.5 Results

3.5.1 Average Effects

Figure 3.1 displays the changes in daily cumulative abnormal returns relative to day 0 based on an event window of 20 trading days before and after the exposure. The regression results in Table 3.2 echo the pattern observed in Figure 3.1. As shown in panel A, the CAR between Day -20 and Day -1 are all insignificantly different from 0, which, to some extent, mitigates the concerns of information leakage. Panel B exhibits

the daily effects after the exposures. It is clear that the descent increases gradually and becomes rather stable around day 7. Although existing literature has suggested that the event window should be set as short as possible (McWilliams and Siegel, 1997) and the efficiency market hypothesis implies the financial impact of an unanticipated event will be immediately reflected in stock prices(Malkiel and Fama, 1970), event windows selected by prior studies seem to vary. For example, Farber and Hallock (2009) use a two-day event window to study market reactions on layoff announcement while Fisman and Wang (2015) allow a 30-day event window after disclosures of workers' death. In this paper, we choose a relative longer event window for two reasons. First, as trading for some scandal-hit firms was immediately suspended following the exposure, it may put off the effects by one to two days. Secondly, as China is an offshore market for global companies, it may take time for information to spread. Thus, we use a 15-day event window [-7,7] as the baseline time length and conduct robustness tests on other time lengths.

3.3 shows the average effects of the scandal exposures. In panel A, a 4.1% decline is detected, and the result is quite robust to the inclusion of various fixed effects. In panel B and C, we extend the analysis to the 10^{th} and 20^{th} trading day after the revelation, and find an average loss of 5.4% and 8.6% respectively. Existing studies estimating the average effects of negative firm-level shocks on the stock market various from -29% (Dowdell et al., 1992) to nearly zero, and our estimation is within this range.

3.5.2 Effects by Scandal Types

Figure 3.2 displays the distinct stock responses based on scandal types: product quality, personal information breaches, and deceptive business practices. Product quality includes violations such as selling expired food, defective household products, and problematic vehicles. Personal information breach refers to activities like collecting and selling private information without permission. Firms with false advertising, tricky warranties or that are charging hidden fees are accused of performing deceptive business practices. Table 3.1 provides details on the violations that each firm is accused of. From Figure 3.2, we can see that after the exposure, the product quality group drops sharply in the first three days, the privacy breach group gradually declines over time, and the deceptive businesses seem to be rather sticky.

Table 3.4 shows the daily effects by scandal types. Column 1 shows the daily impacts on the product quality group. Although during the pre-event period, the product quality group had three non-zero CAR, it should not be our primary concerns because the CAR for that group exhibited a first increasing and then decreasing trend shown in Figure 3.2. At day 1 and day 2, the market appeared to delay its reactions as we expect, but it generated immense responses afterward. At day 3, there was a sharp drop of 9.4%, and the declines continued to 20% in the subsequent days. Although the estimation is relatively larger than some previous studies, we are not the first one that captures such strong impacts. The larger magnitude may be attributed to three reasons. First, the severity of the scandal can affect the magnitude. For example, Gokhale

et al. (2014) shows Toyota's floor mat recall generated zero effect to the stock price, the Saylor highway accident recall made Toyota's CAR reach a value of -7%, and the 2010 major recall caused its CAR to fall by 19.9%. As the scandals revealed in the show are all very significant and representative, the impacts could be larger. Second, the format of scandal revelations is different. Product hazard issues examined by prior studies, for example, are typically revealed by recall announcement posted on the Wall Street Journal (Chen et al., 2009; Hsu and Lawrence, 2016; Ni et al., 2016), while in our settings, all scandals are exposed by undercover investigations and interviews broadcast on national televisions, and thus, may provide more direct evidence and be more salient to the consumers. Third, the scandal firms in our sample are made up of the leading companies with higher reputation. Previous studies suggest that firms with strong reputation suffer severer market value losses than do firms with a weaker reputation (Rhee and Haunschild, 2006), so it is possible to have larger market responses than other studies due to the sample compositions. Column 3 presents the impacts of personal information breaches on the stock prices. Right after the announcement, the market responded strongly to the scandal with a drop of 3.2%. The magnitude is similar with Cavusoglu et al. (2004) that finds a loss of 2.1% of value over two days following the event, and the subsequent pattern is in line with Ko and Dorantes (2006) and Malhotra and Kubowicz Malhotra (2011) which imply firms' market value is negatively affected by a breach in both the short and long runs. Column 5 includes the effects of deceptive business practices and shows zero financial impacts.

Table 3.5 includes the results of the average effects by scandal types. Panel A

presents the baseline findings, and the results are robust to the inclusion of various fixed effects. The product quality scandal generates the largest effects with an average drop of 12%. The market also shows strong responses to the breaches of privacy. Our work detects an overall loss of 3.6% of value over the 15-day event window, and the magnitude is within the range of earlier studies. However, it seems that the market does not react to deceptive business scandals. Additionally, our findings are robust to different event windows as shown in panel B and C. To sum up, investors believe that product quality scandals cause the largest drop in expected future profits, personal information breaches also generate significant value losses, while deceptive business practices seem to have minimal impacts.

3.5.3 Effects by Sectors

We follow the Industry Classification Benchmark(ICB) launched by Dow Jones and FTSE in 2005 to classify all the firms into three sectors: consumer goods, consumer services, and technology. The consumer goods sector includes firms that produce food, vehicles and household products. Units such as banks, telecommunication companies, and restaurants are assigned to the services sector. Firms like Samsung and Apple are grouped into the technology sector. As most existing studies only consider scandals from a single industry, few of them has tried to compare the vulnerability to scandals across industries. Farber and Hallock (2009) is among the few existing papers that have addressed this. They find that between 1970-1999, job loss announcements generated more substantial impacts on firms in the manufacturing industry. Another related

paper is Brounen and Derwall (2010) which shows that terrorist attacks have more substantial effects on firms in the services industry such as airlines and hotels. However, after adjusting that with systematic risk differences across industries, any divergence disappeared.

Figure 3.3 contains the market reactions of different sectors to the scandal exposures. It seems that both the goods and the services sectors experience significant drops after the revelations. Results in Tale 3.6 reflect these patterns. As we can see from column 1 and 3, both the goods and services sector experienced a 7% loss at day 3, and the effects lasted more than 20 trading days, while in column 5, the technology sector showed zero effect. The average impacts on these three sectors are displayed in Table 3.7. The consumer goods sector suffered the largest hit with a decline of 7.6% during the 15-day window, and the services sector was the second most with a loss of 4.7%. It is not surprising that the consumer goods sector was the most impacted because violations from that sector can always lead serious and immediate issues. It seems that the technology sector survives in the scandal exposures, as problems that occur in this sector are less directly harmful, and since this sector is a growth sector, markets tend to have a higher tolerance for it.

3.6 Conclusion

Financial impacts of firm-level scandals have been widely studied in the literature. An interesting question is which scandals the market is most sensitive to. Using a

unique event that created systematic scandal exposure, this paper estimates the average effects of scandals and compares effects across scandal types and industry sectors. We find that scandal exposures cause an adverse effect on average. The effects are largest for firms revealed to have defective products. The market also exhibits a strong response to personal information breaches. However, we detect no impact on firms using deceptive business practices. When examining different industry sectors, the consumer goods sector suffers the largest effects, the services sector also experiencing losses. In contrast, the market exhibits higher tolerance to scandals within the technology sector. Our paper contributes to the literature in three ways. First, we are among the very first papers to make comparisons across scandal types and sectors. This is feasible due to the uniform nature of the treatment of each scandal. Secondly, the unique setting allows us to abstract from the issue of the selection bias in identifying scandals. Lastly, our study provides new empirical evidence on the effects of personal information breaches, which few studies have considered. The findings of this paper shed light on how the market responds to various types of scandals and whether some industries are more exposed to negative effects.

References

Acquisti, A., Friedman, A., and Telang, R. (2006). Is there a cost to privacy breaches? an event study. *ICIS 2006 Proceedings*, page 94.

Brounen, D. and Derwall, J. (2010). The impact of terrorist attacks on international stock markets. *European Financial Management*, 16(4):585–598.

Campbell, K., Gordon, L. A., Loeb, M. P., and Zhou, L. (2003). The economic cost of publicly announced information security breaches: empirical evidence from the stock market. *Journal of Computer Security*, 11(3):431–448.

Cavusoglu, H., Mishra, B., and Raghunathan, S. (2004). The effect of internet security breach announcements on market value: Capital market reactions for breached firms and internet security developers. *International Journal of Electronic Commerce*, 9(1):70–104.

Chen, Y., Ganesan, S., and Liu, Y. (2009). Does a firm's product-recall strategy affect its financial value? an examination of strategic alternatives during product-harm crises. *Journal of Marketing*, 73(6):214–226.

Crutchley, C. E., Jensen, M. R., and Marshall, B. B. (2007). Climate for scandal: corporate environments that contribute to accounting fraud. *Financial Review*, 42(1):53–73.

Dawar, N. and Pillutla, M. M. (2000). Impact of product-harm crises on brand equity: The moderating role of consumer expectations. *Journal of marketing research*, 37(2):215–226.

Dowdell, T. D., Govindaraj, S., and Jain, P. C. (1992). The tylenol incident, ensuing regulation, and stock prices. *Journal of Financial and Quantitative Analysis*, 27(2):283–301.

Falkheimer, J. and Heide, M. (2015). Trust and brand recovery campaigns in crisis: Findus nordic and the horsemeat scandal. *International Journal of Strategic Communication*, 9(2):134–147.

Farber, H. S. and Hallock, K. F. (2009). The changing relationship between job loss announcements and stock prices: 1970–1999. *Labour Economics*, 16(1):1–11.

Fisman, R. and Wang, Y. (2015). The mortality cost of political connections. *The Review of Economic Studies*, 82(4):1346–1382.

Gokhale, J., Brooks, R. M., and Tremblay, V. J. (2014). The effect on stockholder wealth of product recalls and government action: The case of toyota's accelerator pedal recall. *The Quarterly Review of Economics and Finance*, 54(4):521–528.

Govindaraj, S., Jaggi, B., and Lin, B. (2004). Market overreaction to product recall

revisited—the case of firestone tires and the ford explorer. *Review of Quantitative Finance and Accounting*, 23(1):31–54.

Hsu, L. and Lawrence, B. (2016). The role of social media and brand equity during a product recall crisis: A shareholder value perspective. *International journal of research in Marketing*, 33(1):59–77.

Knittel, C. R. and Stango, V. (2013). Celebrity endorsements, firm value, and reputation risk: Evidence from the tiger woods scandal. *Management Science*, 60(1):21–37.

Ko, M. and Dorantes, C. (2006). The impact of information security breaches on financial performance of the breached firms: an empirical investigation. *Journal of Information Technology Management*, 17(2):13–22.

Korkofingas, C. and Ang, L. (2011). Product recall, brand equity, and future choice. *Journal of Marketing Management*, 27(9-10):959–975.

MacKinlay, A. C. (1997). Event studies in economics and finance. *Journal of economic literature*, 35(1):13–39.

Malhotra, A. and Kubowicz Malhotra, C. (2011). Evaluating customer information breaches as service failures: An event study approach. *Journal of Service Research*, 14(1):44–59.

Malkiel, B. G. and Fama, E. F. (1970). Efficient capital markets: A review of theory and empirical work. *The journal of Finance*, 25(2):383–417.

Marsh, T. L., Schroeder, T. C., and Mintert, J. (2004). Impacts of meat product recalls on consumer demand in the usa. *Applied Economics*, 36(9):897–909.

Martin, K. D., Borah, A., and Palmatier, R. W. (2017). Data privacy: Effects on customer and firm performance. *Journal of Marketing*, 81(1):36–58.

McWilliams, A. and Siegel, D. (1997). Event studies in management research: Theoretical and empirical issues. *Academy of management journal*, 40(3):626–657.

Nguyen, B. D. and Nielsen, K. M. (2010). The value of independent directors: Evidence from sudden deaths. *Journal of Financial Economics*, 98(3):550–567.

Ni, J., Flynn, B. B., and Jacobs, F. R. (2016). The effect of a toy industry product recall announcement on shareholder wealth. *International Journal of Production Research*, 54(18):5404–5415.

Rhee, M. and Haunschild, P. R. (2006). The liability of good reputation: A study of product recalls in the us automobile industry. *Organization Science*, 17(1):101–117.

Thirumalai, S. and Sinha, K. K. (2011). Product recalls in the medical device industry: An empirical exploration of the sources and financial consequences. *Management Science*, 57(2):376–392.

Thomsen, M. R. and McKenzie, A. M. (2001). Market incentives for safe foods: an examination of shareholder losses from meat and poultry recalls. *American Journal of Agricultural Economics*, 83(3):526–538.

Van Heerde, H., Helsen, K., and Dekimpe, M. G. (2007). The impact of a product-harm crisis on marketing effectiveness. *Marketing Science*, 26(2):230–245.

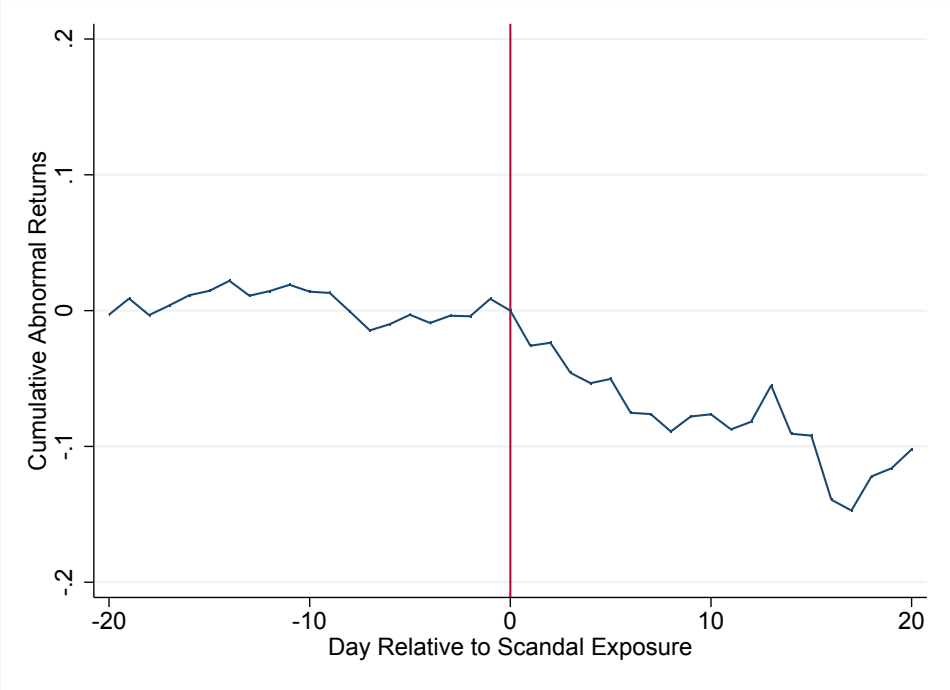
Wang, Y., Liu, S., and Du, J. (2014). Contagion effects vs. competitive effects in credence goods markets:theory and event study on china's food markets. *Economic Research Journal*, 2(11).

Worrell, D. L., Davidson III, W. N., and Sharma, V. M. (1991). Layoff announcements and stockholder wealth. *Academy of management Journal*, 34(3):662–678.

Yin, Q. (1998). Consumer education in china. *Consumer Interests Annual*, 44:168–171.

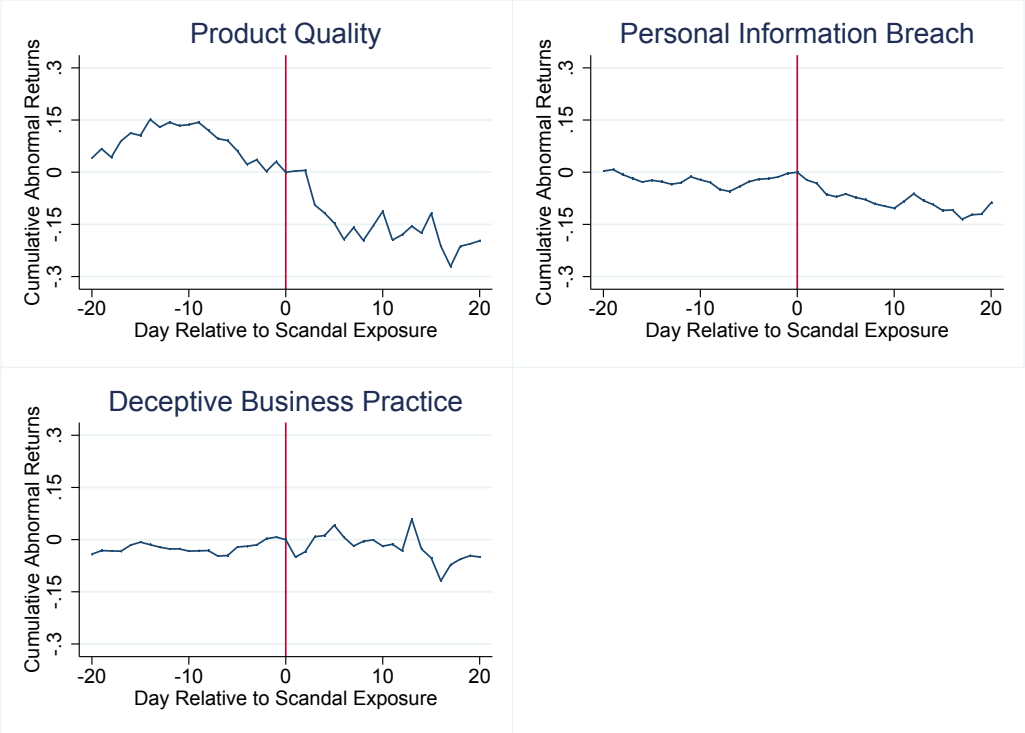
Figures and Tables

Figure 3.1: Changes in Cumulative Abnormal Returns, By Day



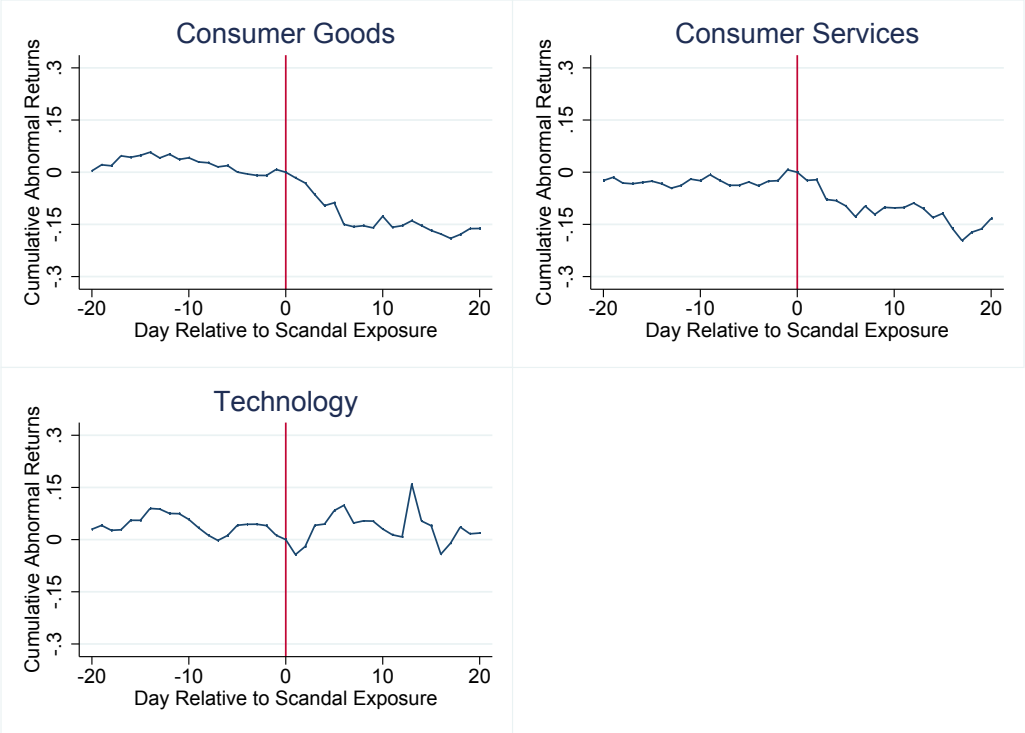
This figure plots the average CAR changes relative to the day before the scandal exposures (day 0) at an event window of [-20,20]. We line up all the daily CAR and winsorize them at the 5th and 95th percentiles. All the firms are equally weighted.

Figure 3.2: Changes in Cumulative Abnormal Returns, By Scandal Type



This figure plots the average CAR changes relative to the day before the scandal exposures (day 0) by scandal types at an event window of [-20,20]. We line up all the daily CAR and winsorize them at the 5th and 95th percentiles. All the firms are equally weighted.

Figure 3.3: Changes in Cumulative Abnormal Returns, By Sectors



This figure plots the average CAR changes relative to the day before the scandal exposures (day 0) by industry sectors at an event window of [-20,20]. We line up all the daily CAR and winsorize them at the 5th and 95th percentiles. All the firms are equally weighted.

Table 3.1: Scandal-Hit Publicly Traded Firms (2007-2018)

Firm	Scandal-Hit Year	Scandal Types
Aeon Co.,Ltd	2017	Deceptive Business Practices
Alibaba Group	2016	Deceptive Business Practices
Apple	2013	Deceptive Business Practices
Agriculture Bank of China*	2012&2015	Personal Information Breaches
Bank of China*	2015	Personal Information Breaches
BAIC Group	2015	Deceptive Business Practices
Carrefour S.A.	2012	Product Quality
China Merchants Bank*	2012	Personal Information Breaches
China Mobile*	2010&2015	Personal Information Breaches
China Telecommunications*	2012&2015	Personal Information Breaches
China Unicom	2014	Personal Information Breaches
Daimler AG	2015	Deceptive Business Practices
Dongfeng Motor Group	2017	Deceptive Business Practices
Dun & Bradstreet	2012	Personal Information Breaches
Focus Media Holding	2008	Personal Information Breaches
AutoNavi	2013	Personal Information Breaches
Gohigh Data Networks Technology Co,Ltd	2014	Personal Information Breaches
GOME Retail	2011	Deceptive Business Practices
HITACHI	2011	Deceptive Business Practices
Hewlett-Packard (HP)	2010	Deceptive Business Practices
Industrial and Commercial Bank of China*	2012&2015	Personal Information Breaches
Jianghuai Automobile	2013	Product Quality
Kumho Tier, Inc	2011	Product Quality
LG Corp.	2011	Deceptive Business Practices
Mcdonald's	2012	Product Qualities
NetEase,Inc	2013	Personal Information Breaches
Nike	2017	Deceptive Business Practices
Nikon	2014	Deceptive Business Practices
Nissan Motor Company Ltd	2015	Deceptive Business Practices
Nokia*	2007	Deceptive Business Practices
Panasonic	2011	Deceptive Business Practices
Koninklijke Philips N.V.*	2011	Deceptive Business Practices
RYOHIN KEIKAKU CO.,LTD	2017	Product Quality&Safety
SAIC Motor Corporation Limited	2013&2015	Product Quality&Safety
Samsung Electronics	2011	Deceptive Business Practices
Sharp Corp	2011	Deceptive Business Practices
SONY Corp*	2011	Deceptive Business Practices
Tata Motor	2015	Deceptive Business Practices
Toshiba Corp	2011	Deceptive Business Practices
Utstracom	2010	Deceptive Business Practices
VOLKSWAGEN AG	2013&2018	Product Quality
VOLKSWAGEN AG	2015	Deceptive Business Practices
Xiamen Kiongdomay Group Company	2017	Product Quality

Firms from secondary stock markets or with insufficient pre-event data available to calculate abnormal returns are excluded. Firms labeled with * are dual-listed across markets, and we use the stock prices from their primary markets. Exceptions are applied to Chinese firms such as Agriculture Bank of China, Bank of China, China Merchants Bank, and Industrial and Commercial Bank of China. These firms are dual-listed in both the China mainland stock market and the Hong Kong stock market. As the Chinese mainland stock market does not open to foreign investors, the Hong Kong stock market can be regraded as a supplementary stock market. Thus, we weight them by the volumes traded on each market.

Table 3.2: Changes in Cumulative Abnormal Returns, by Day

Panel A: Days before the exposure					
Days Before	(1)	(2)	Days Before	(3)	(4)
Day -20	-0.003	(0.037)	Day -10	0.014	(0.033)
Day -19	0.009	(0.035)	Day -9	0.013	(0.030)
Day -18	-0.003	(0.035)	Day -8	-0.001	(0.027)
Day -17	0.004	(0.033)	Day -7	-0.015	(0.027)
Day -16	0.011	(0.032)	Day -6	-0.010	(0.028)
Day -15	0.015	(0.034)	Day -5	-0.003	(0.027)
Day -14	0.022	(0.033)	Day -4	-0.009	(0.026)
Day -13	0.011	(0.034)	Day -3	-0.004	(0.021)
Day -12	0.014	(0.034)	Day -2	-0.004	(0.016)
Day -11	0.019	(0.033)	Day -1	0.009	(0.011)

Panel B: Days after the exposure					
Days After	(1)	(2)	Days After	(3)	(4)
Day 1	-0.026	(0.017)	Day 11	-0.087**	(0.033)
Day 2	-0.024	(0.020)	Day 12	-0.082**	(0.034)
Day 3	-0.046*	(0.025)	Day 13	-0.055*	(0.032)
Day 4	-0.053*	(0.027)	Day 14	-0.091**	(0.038)
Day 5	-0.050	(0.034)	Day 15	-0.092**	(0.039)
Day 6	-0.075**	(0.034)	Day 16	-0.139***	(0.038)
Day 7	-0.076**	(0.030)	Day 17	-0.139***	(0.038)
Day 8	-0.089**	(0.033)	Day 18	-0.147***	(0.043)
Day 9	-0.078**	(0.034)	Day 19	-0.147***	(0.043)
Day 10	-0.076**	(0.032)	Day 20	-0.102**	(0.039)
Observations	2,087		R-squared	0.071	

The dependent variables are winsorized at the 5th and 95th percentiles of the pooled distribution to reduce reliance on outliers.
Standard errors are clustered at the stock level.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.3: Average Effects of Scandal Exposures on Cumulative Abnormal Returns

	(1)	(2)	(3)	(4)
Panel A: Event Windows [-7,7]				
Post	-0.039**	-0.041**	-0.040**	-0.041**
	(0.019)	(0.019)	(0.019)	(0.019)
Constant	-0.003			
	(0.013)			
Mean Dep.	-0.021	-0.021	-0.021	-0.021
Observations	758	758	758	758
R-squared	0.024	0.435	0.133	0.435
Panel B: Event Window [-10,10]				
Post	-0.052**	-0.054**	-0.053**	-0.054**
	(0.021)	(0.022)	(0.022)	(0.022)
Constant	-0.000			
	(0.015)			
Mean Dep.	-0.024	-0.024	-0.024	-0.024
Observations	1,065	1,065	1,065	1,065
R-squared	0.030	0.452	0.128	0.452
Panel C: Event Window [-20,20]				
post	-0.086***	-0.086***	-0.087***	-0.086***
	(0.030)	(0.030)	(0.030)	(0.030)
Constant	0.004			
	(0.025)			
Mean Dep.	-0.037	-0.037	-0.037	-0.037
Observations	2,087	2,087	2,087	2,087
R-squared	0.036	0.524	0.123	0.524
Stock FE		X		X
Time FE			X	X

The dependent variables are winsorized at the 5th and 95th percentiles.
Standard errors are clustered at the stock level.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Daily Cumulative Abnormal Returns, by Scandal Types

	Product Quality		Personal Info. Breaches		Deceptive Business Practices	
	(1)	(2)	(3)	(4)	(5)	(6)
Day -20	0.041	(0.107)	0.003	(0.037)	-0.042	(0.066)
Day -19	0.067	(0.093)	0.008	(0.038)	-0.032	(0.066)
Day -18	0.043	(0.095)	-0.007	(0.036)	-0.033	(0.067)
Day -17	0.090	(0.086)	-0.018	(0.036)	-0.033	(0.063)
Day -16	0.113	(0.085)	-0.018	(0.036)	-0.016	(0.059
Day -15	0.106	(0.092)	-0.023	(0.036)	-0.007	(0.060)
Day -14	0.152*	(0.081)	-0.027	(0.038)	-0.007	(0.060)
Day -13	0.130	(0.086)	-0.034	(0.038)	-0.022	(0.062)
Day -12	0.143	(0.084)	-0.030	(0.038)	-0.027	(0.064)
Day -11	0.134	(0.085)	-0.013	(0.034)	-0.026	(0.060)
Day -10	0.137	(0.082)	-0.022	(0.034)	-0.033	(0.063)
Day -9	0.143**	(0.064)	-0.029	(0.032)	-0.032	(0.058)
Day -8	0.121**	(0.053)	-0.049	(0.033)	-0.031	(0.051)
Day -7	0.096	(0.060)	-0.055	(0.033)	-0.047	(0.050)
Day -6	0.091	(0.076)	-0.042	(0.027)	-0.046	(0.049)
Day -5	0.061	(0.077)	-0.027	(0.024)	-0.021	(0.052)
Day -4	0.022	(0.068)	-0.020	(0.020)	-0.019	(0.056)
Day -3	0.036	(0.034)	018	(0.019)	-0.015	(0.052)
Day -2	0.003	(0.035)	-0.014	(0.016)	0.002	(0.037)
Day -1	0.030	(0.031)	-0.003	(0.008)	0.007	(0.020)
Day 1	0.004	(0.027)	-0.023	(0.015)	-0.050	(0.040)
Day 2	0.005	(0.054)	-0.032***	(0.011)	-0.035	(0.042)
Day 3	-0.094	(0.072)	-0.064**	(0.025)	0.008	(0.040)
Day 4	-0.117	(0.072)	-0.064**	(0.025)	0.012	(0.051)
Day 5	-0.146	(0.088)	-0.063**	(0.028)	0.041	(0.064)
Day 6	-0.193**	(0.084)	-0.072**	(0.032)	0.007	(0.061)
Day 7	-0.159*	(0.075)	-0.079**	(0.035)	-0.018	(0.057)
Day 8	-0.196**	(0.079)	-0.091**	(0.040)	-0.005	(0.061)
Day 9	-0.155*	(0.082)	-0.097**	(0.043)	-0.001	(0.061)
Day 10	-0.113	(0.069)	-0.103**	(0.041)	-0.019	(0.063)
Day 11	-0.195**	(0.074)	-0.084**	(0.039)	-0.013	(0.062)
Day 12	-0.180**	(0.074)	-0.062	(0.037)	-0.032	(0.069)
Day 13	-0.155*	(0.076)	-0.081**	(0.039)	0.059	(0.053)
Day 14	-0.175*	(0.083)	-0.093*	(0.045)	-0.027	(0.079)
Day 15	-0.118	(0.091)	-0.110**	(0.046)	-0.053	(0.079)
Day 16	-0.213**	(0.086)	-0.109**	(0.043)	-0.119	(0.087)
Day 17	-0.271***	(0.082)	-0.135**	(0.054)	-0.072	(0.086)
Day 18	-0.213**	(0.094)	-0.122**	(0.048)	-0.056	(0.086)
Day 19	-0.206*	(0.095)	-0.120**	(0.044)	-0.046	(0.082)
Day 20	-0.197**	(0.090)	-0.088**	(0.035)	-0.050	(0.084)
Mean Dep.	-0.031		-0.052		-0.023	
Observations	527		834		726	
R-squared	0.222		0.160		0.022	

The dependent variables are winsorized at the 5th and 95th percentiles. Standard errors are clustered at the stock level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3.5: Average Effects of Scandal Exposures on Cumulative Abnormal Returns, by Scandal Types

	(1)	(2)	(3)	(4)
Panel A: Event Window [-7,7]				
Post×Product Quality	-0.126**	-0.129**	-0.127**	-0.129**
	(0.052)	(0.054)	(0.052)	(0.054)
Post×Personal Information Breach	-0.035	-0.036*	-0.035*	-0.036*
	(0.021)	(0.020)	(0.021)	(0.020)
Post×Deceptive Business Practices	0.017	0.017	0.017	0.017
	(0.020)	(0.021)	(0.021)	(0.021)
Mean Dep.	-0.021	-0.021	-0.021	-0.021
Observations	771	771	771	771
R-squared	0.104	0.465	0.170	0.465
Panel B: Event Window [-10,10]				
Post×Product Quality	-0.167***	-0.170***	-0.168***	-0.170***
	(0.059)	(0.061)	(0.059)	(0.061)
Post×Personal Information Breach	-0.043*	-0.044*	-0.043*	-0.044*
	(0.022)	(0.022)	(0.022)	(0.022)
Post×Deceptive Business Practices	0.018	0.019	0.018	0.019
	(0.023)	(0.023)	(0.023)	(0.023)
Time FE			X	X
Mean Dep.	-0.024	-0.024	-0.024	-0.024
Observations	1,065	1,065	1,065	1,065
R-squared	0.094	0.509	0.185	0.509
Panel C: Event Window [-20,20]				
Post×Product Quality	-0.239***	-0.239***	-0.238***	-0.239***
	(0.085)	(0.087)	(0.086)	(0.087)
Post×Personal Information Breach	-0.064**	-0.065**	-0.066**	-0.065**
	(0.027)	(0.027)	(0.027)	(0.027)
Post×Deceptive Business Practices	-0.000	0.001	0.000	0.019
	(0.032)	(0.033)	(0.032)	(0.023)
Mean Dep.	-0.037	-0.037	-0.037	-0.037
Observations	2,087	2,087	2,087	2,087
R-squared	0.081	0.566	0.168	0.566
Stock FE		X		X
Time FE			X	X

The dependent variables are winsorized at the 5th and 95th percentiles.
Standard errors are clustered at the stock level.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Daily Cumulative Abnormal Returns, by Industry Sectors

	Consumer Goods		Consumer Services		Technology	
	(1)	(2)	(3)	(4)	(5)	(6)
Day -20	0.004	(0.070)	-0.024	(0.050)	0.030	(0.092)
Day -19	0.021	(0.060)	-0.015	(0.049)	0.041	(0.088)
Day -18	0.018	(0.058)	-0.033	(0.047)	0.027	(0.091)
Day -17	0.047	(0.050)	-0.029	(0.047)	0.028	(0.088)
Day -16	0.043	(0.047)	-0.026	(0.047)	0.056	(0.085)
Day -15	0.048	(0.048)	-0.033	(0.048)	0.055	(0.091)
Day -14	0.057	(0.044)	-0.046	(0.049)	0.090	(0.086)
Day -13	0.041	(0.041)	-0.038	(0.048)	0.088	(0.090)
Day -12	0.052	(0.035)	-0.020	(0.046)	0.075	(0.097)
Day -11	0.037	(0.035)	-0.024	(0.046)	0.075	(0.094)
Day -10	0.042	(0.037)	-0.007	(0.039)	0.058	(0.095)
Day -9	0.037	(0.035)	-0.023	(0.039)	0.034	(0.090)
Day -8	0.027	(0.025)	-0.038	(0.041)	0.013	(0.076)
Day -7	0.015	(0.034)	-0.037	(0.044)	-0.002	(0.070)
Day -6	0.019	(0.028)	-0.028	(0.043)	0.012	(0.070)
Day -5	0.000	(0.027)	-0.028	(0.043)	0.044	(0.072)
Day -4	-0.005	(0.026)	-0.039	(0.038)	0.044	(0.071)
Day -3	-0.009	(0.025)	-0.026	(0.021)	0.040	(0.048)
Day -2	-0.009	(0.025)	-0.024	(0.018)	0.040	(0.048)
Day -1	0.008	(0.024)	0.007	(0.011)	0.013	(0.028)
Day 1	-0.016	(0.012)	-0.023	(0.015)	-0.043	(0.067)
Day 2	-0.031	(0.023)	-0.022	(0.024)	-0.020	(0.068)
Day 3	-0.065**	(0.029)	-0.078**	(0.035)	0.041	(0.070)
Day 4	-0.096**	(0.038)	-0.082**	(0.036)	0.045	(0.074)
Day 5	-0.088	(0.061)	-0.097**	(0.037)	0.084	(0.094)
Day 6	-0.150**	(0.057)	-0.128***	(0.039)	0.099	(0.078)
Day 7	-0.157**	(0.055)	-0.097***	(0.032)	0.048	(0.079)
Day 8	-0.154**	(0.059)	-0.121***	(0.041)	0.054	(0.086)
Day 9	-0.160**	(0.063)	-0.101**	(0.041)	0.053	(0.084)
Day 10	-0.127**	(0.048)	-0.103**	(0.039)	0.031	(0.089)
Day 11	-0.158**	(0.058)	-0.102**	(0.042)	0.014	(0.084)
Day 12	-0.154**	(0.058)	-0.089**	(0.038)	0.008	(0.095)
Day 13	-0.139**	(0.059)	-0.104**	(0.038)	0.159**	(0.057)
Day 14	-0.154**	(0.062)	-0.130***	(0.045)	0.054	(0.103)
Day 15	-0.168**	(0.064)	-0.118**	(0.049)	0.040	(0.101)
Day 16	-0.178**	(0.071)	-0.162***	(0.049)	-0.042	(0.106)
Day 17	-0.190**	(0.075)	-0.197***	(0.058)	-0.010	(0.102)
Day 18	-0.179**	(0.079)	-0.173***	(0.051)	0.036	(0.103)
Day 19	-0.162*	(0.080)	-0.163***	(0.050)	0.017	(0.099)
Day 20	-0.162*	(0.079)	-0.133***	(0.043)	0.019	(0.102)
Mean Dep.	-0.053		-0.066		-0.023	
Observations	570		997		520	
R-squared	0.256		0.156		0.038	

The dependent variables are winsorized at the 5th and 95th percentiles.
Standard errors are clustered at the stock level.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Average Effects of Scandal Exposures on Cumulative Abnormal Returns, by Industry Sectors

	(1)	(2)	(3)	(4)
Panel A: Event Windows [-7,7]				
Post×Consumer Goods	-0.076*	-0.076*	-0.076*	-0.076*
	(0.038)	(0.039)	(0.038)	(0.039)
Post×Consumer Services	-0.045	-0.047	-0.045	-0.047
	(0.030)	(0.031)	(0.030)	(0.031)
Post×Technology	0.012	0.011	0.010	0.011
	(0.016)	(0.015)	(0.016)	(0.015)
Mean Dep.	-0.021	-0.021	-0.021	-0.021
Observations	758	758	758	758
R-squared	0.100	0.451	0.164	0.451
Panel B: Event Window [-10,10]				
Post×Consumer Goods	-0.104**	-0.105**	-0.104**	-0.105**
	(0.045)	(0.046)	(0.045)	(0.046)
Post×Consumer Services	-0.056	-0.058*	-0.056	-0.058*
	(0.034)	(0.035)	(0.034)	(0.035)
Post×Technology	0.011	0.012	0.009	0.012
	(0.017)	(0.016)	(0.017)	(0.016)
Mean Dep.	-0.024	-0.024	-0.024	-0.024
Observations	1,065	1,065	1,065	1,065
R-squared	0.101	0.471	0.159	0.471
Panel C: Event Window [-20,20]				
Post×Consumer Goods	-0.158**	-0.159**	-0.158**	-0.159**
	(0.062)	(0.063)	(0.062)	(0.063)
Post×Consumer Services	-0.086*	-0.086*	-0.087*	-0.086*
	(0.047)	(0.048)	(0.046)	(0.048)
Post×Technology	-0.007	-0.006	-0.008	-0.006
	(0.022)	(0.023)	(0.023)	(0.023)
Mean Dep.	-0.037	-0.037	-0.037	-0.037
Observations	2,087	2,087	2,087	2,087
R-squared	0.087	0.538	0.147	0.538
Stock FE		X		X
Time FE			X	X

The dependent variables are winsorized at the 5th and 95th percentiles. Standard errors are clustered at the stock level.
*** p<0.01, ** p<0.05, * p<0.1