ESSAYS ON HOUSING MARKET AND LOCAL ECONOMY

by

Wenjing Xiao

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This thesis is dedicated to my husband and my parents,

for their endless love and support.

To my dad,

though you never got to see this, you are in every page.

Thank you for always being proud of me.

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ESSAYS ON HOUSING MARKET AND LOCAL ECONOMY

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This dissertation consists of two essays on housing market and local economy.

The first essay, included in Chapter 1, is "Real Estate Investors and Property Taxation". In this paper, we study the inequality in property taxation in the U.S. single-family home market based on a property's assessed value. Comparing the assessment ratio of properties owned by investors with those of owner-occupiers, we find a 3.0%–4.7% assessment discount nationwide for properties owned by large investors (i.e., those owning more than 100 properties) relative to owner-occupied homes in the same area. This difference translates into an estimated total annual property tax savings of $66–$104 million for large investors across the country. Further evidence based on micro-level appeals data in Cook County, Illinois, and Florida suggests that the institutional assessment discount results from a higher likelihood of appeal and more favorable outcomes upon a successful appeal for large investors. States with a fairer property taxation administration, a higher market share by large investors, and a higher property tax burden show a greater assessment discount for large investors.

The second essay, included in Chapter 2, is "Housing Stability and New Business Creation". It is a joint work with Steven Xiao. We examine whether stronger legal renter rights encourage business creation by enhancing housing stability. In California, passages of city ordinances that protect renters from arbitrary evictions increase the number of new businesses

by 9.9% and the proportion of female (8.0%) and racial-minority business owners (12.1%). These firms can survive in the long run and perform no worse than others. Household-level analysis shows that renters are less likely to move, more likely to become self-employed and generate more business income after law passages. Our evidence suggests that enhancing housing stability can benefit the local economy by promoting self-employment and job creation.

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CHAPTER 1

REAL ESTATE INVESTORS AND PROPERTY TAXATION

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1.1 Introduction

Real estate investors have become more influential in shaping the US residential housing market in recent years, with increasing activity in the sector. According to a report by Stateline, investor purchases made up 24% of the total single-family home sales across the country in 2021.[1].This marks an annual growth of around 15%-16% since 2012. The percentage of purchases by investors was particularly high in certain sun-belt states such as Georgia, Arizona, California, Nevada and Texas, where approximately one third of all properties sold in 2021 were bought by investors. These investors tend to hold on to the properties for a longer period of time to generate a steady stream of rental income, rather than reselling them quickly to capture short-term capital gains. The rise in investments in single-family rental (SFR) investment can be attributed to various factors, such as the rising prices of new construction homes, shortage of housing inventory, and the pandemic-fueled suburban growth, among others.

Institutional investment in SFR markets has also grown exponentially since the Great Recession, transforming it from a traditional mom-and-pop business into an investment asset class that offers attractive stable returns for institutional investors. The foreclosure crisis in 2008 resulted in a large number of foreclosed and bank-owned properties that were available for purchase at deep discounts. Institutional investors saw an opportunity to buy these properties at a low price, renovate them, and then rent them out for a steady stream of income. Institutional investment in SFR market has continued to grow in the recent years. A research report by MetLife Investment Management estimates that institutions own about 700,000 SFRs in 2022, or close to 5% of the nationwide market [2]. Institutional holdings have

[1]Source: https://www.kunc.org/news/2022-09-14/investors-not-homeowners-are-buying-more-and-more-colorado-homes-and-rents-are-going-up/

[2]Source:https://investments.metlife.com/insights/real-estate/the-future-of-housing-our-outlook-for-single-and-multi-family-investments/

been forecasted to reach 7.6 million homes by 2030, accounting for more than 40% of all SFRs in the United States. [3]

In this paper, we examine the implications of investment in the SFR space on local municipal finance in terms of property taxation. Property taxes are one of the most important sources of revenue for local governments. Counties, school districts, municipalities, and townships are typical jurisdictions that collect and rely on property taxes. In 2019, local governments in the U.S. collected $559 billion in property taxes, accounting for about 30% of local general revenue and nearly half of local own-source revenue, excluding transfers from federal and state government.[4] Property tax bills are calculated based on assessed value, local property tax rates, and other exemptions and credits when applicable. In principle, the assessed value, which local governments determine for taxation purposes, should reflect the fair market value of a property. Given an equitable property tax administration, the assessment-to-market value ratio (the assessment ratio hereafter) should be the same for all properties within a tax jurisdiction. However, recent studies have shown evidence of assessment gap from various perspectives, such as across race and between high- and low-priced homes (Avenancio-León and Howard, 2022, Berry, 2021). These papers argue that a key driver of property tax inequality is the appeals process, which allows owners to protest the government's assessed value and achieve a tax reduction if successful. For instance, (Avenancio-León and Howard, 2022) show that the tendency to appeal an assessment and the outcome of the appeal are different across homeowners of different races residing within the same neighborhood, leading to racial minorities taking up for a disproportionate share of the property tax burden.

[3]Source:https://www.prnewswire.com/news-releases/institutional-investment-in-single-family-rentals-is-on-the-rise-reports-yardi-matrix-301596511.html

[4]Source:https://www.urban.org/policy-centers/cross-center-initiatives/state-and-local-finance-initiative/projects/state-and-local-backgrounders/property-taxes

We postulate that property tax inequality also exists between real estate investors and owner-occupiers. The property tax appeals process is complex and time consuming. Owner-occupiers often lack the capacity and time to understand the filing procedures of property tax appeal, and, as a result, the appeals rate is generally low for homeowners. For instance, according to the Florida Department of Revenue, the property tax appeals rate in 2021 is about 1%. Ex ante, it is unclear whether all real estate investors have an advantage or disadvantage relative to owner-occupiers in the property tax assessment of their properties. For instance, mom-and-pop and micro-sized investors with a small portfolio might have limited attention and resources available to effectively monitor the tax assessment of each individual property, leading to a higher assessment ratio for their properties than for owner-occupied homes on average. However, once the scale of the portfolio exceeds a certain threshold, large investors could gain an advantage over individual and micro-sized investors because of economies of scale (Ambrose et al., 2000, Bers and Springer, 1997, Gurun et al., 2022, Yang, 2001). For example, large investors can institutionalize property tax management by developing specialized in-house personnel or hiring outside management teams, practices not feasible for individual and micro-sized investors because of the high fixed costs associated with such an endeavor. Lower marginal cost of filing would allow the large-scale investors to file tax appeal more aggressively. [5] Furthermore, large investors possess a large amount of assessment data from their own properties, many of which are located in the same area with similar property characteristics. This information allows large investors to identify overvaluation and appeal more effectively. Thus, we hypothesize that large real estate investors can achieve more favorable tax assessments on their properties than owner-occupiers, as a result of more efficient and effective property tax appealing.

[5]Often times, property owners need to pay an appeals filing fee to the county or out-of-pocket fees to hire a specialist to file the appeal, regardless of the outcome.

To test our hypothesis, we collect property-level transaction and assessment data from 31 states in the United States between 2005 and 2016, and count the number of properties held by each owner based on the recorded owner mailing address. We classify property owners into five groups based on scale: *owner-occupiers* (with only one primary residence), *micro investors* (2–20 properties), *small investors* (21–50 properties), *medium investors* (51–100 properties), and *large investors* (more than 100 properties). For each transaction, we use the assessed value in the year after the transaction and use the sales price as the market value to calculate the assessment ratio. We then examine the relationship between a property's assessment ratio and the size category that its owner belongs to. We include county×year fixed effects to account for heterogeneity due to differences in the assessment procedure and property tax policies, such as the assessment scaling factor and frequency.[6]

The empirical results support our hypothesis: large investors that own more than 100 properties enjoy a significant 3.0%–3.7% discount in the assessment ratio relative to owner-occupiers in the same county. Our analysis of properties with repeated sales shows an even greater magnitude of the institutional assessment discount: large investors have a significant 4.7% assessment discount on their properties relative to the owner-occupiers who own the same properties at a different point of time. This difference translates into an estimated total annual property tax savings of $104 million for large investors across the country.[7] Another interesting finding from our empirical analysis is that not all investors have property tax advantage on their properties relative to owner-occupied homes. Specifically, the average

[6]Each state applies a scaling factor, which ranges from 10% to 100%, to determine the assessed value based on the home's fair market value. For example, in Georgia, the scaling factor is 40%. If a county assessor in Georgia determines the fair market value for a property to be $100,000, then the assessed value is $40,000. Property assessments are most commonly conducted at the county level. See "State-by-State Property Tax at a Glance" by Lincoln Institute of Land Policy 2021.

[7]Estimates are calculated based on the national median effective property tax rate of 1.4% (Avenancio-León and Howard, 2022) and 700,000 SFRs with an average home price of $225,000 owned by institutions in 2022, according to research reports by MetLife Investment Management.

assessment ratio for micro investors that own 2—20 properties is 0.8%–2.3% higher than that for owner-occupiers. This finding suggests that small-scale investors have property tax disadvantage relative to owner-occupiers and this is likely due to limited attention and constrained resources for property tax appeals.

Next, we examine the property tax appeals process as a potential channel through which large investors achieve favorable assessment ratios. In every state, property owners have the right to undergo some type of appeals procedure if they disagree with the property tax assessments. While nationwide data on property tax appeals are not available, we use micro-level data from Cook County, Illinois, the second-largest county in United States, to examine the differences in appealing decisions and outcomes across property-owner groups. Our analysis reveals that large investors are 77.0% more likely to appeal and 15.1% less likely to win an appeal upon filing than are owner-occupiers. Large investors can also achieve a 41.0% higher value reduction than owner-occupiers. We also show that the final assessment ratios are indeed lower for large investors post-appeal. Taken together, the evidence suggests that the overall effect of property tax appeals is more favorable to large investors compared to owner-occupiers. To substantiate our findings in Cook County, we also use property tax data from Florida to show that reductions in final assessed values through tax appeals are twice as much for large investors' properties than for owner-occupied homes.

We also consider two alternative explanations for the assessment discount enjoyed by large investors. First, local assessors' practices could systematically bias toward or against large investors, relative to owner-occupied homes, resulting in a lower or higher assessment ratio for their properties. Although we could not completely rule out this possibility, we find this unlikely to explain our findings for several reasons. State and local governments usually apply standardized procedures in assessing property values, mostly by using automated valuation models or computer-assisted mass appraisals. In addition, to the best of our knowledge, no open-source information is available on the size of the portfolios held by each rental home

investor. Therefore, there is no evidence that the assessors would know if an owner is a professional large investor and then systematically create more favorable assessed values for their properties.

Second, the institutional assessment discount could be driven by the relatively higher market value (i.e., sales price) for large investors in the denominator if these investors systematically overpay for their properties. This explanation contradicts existing findings using regional samples that large investors tend to purchase properties at a discount (Mills et al., 2019, Smith and Liu, 2020). We confirm these findings by previous studies using nationwide data and show that on average large investors acquire properties at a 13.6% discount compared to owner-occupiers. Such a transaction price discount should bias the assessment ratio upward for large investors. As such, our estimate of assessment discount for large investors is likely understated and should be considered to be a lower bound.

Lastly, we explore factors that potentially contribute to the cross-state variations in the assessment discount for large investors. The Council on State Taxation (COST) publishes a scorecard on state property tax administrative practices in 2011. The COST scorecard evaluates the fairness of the property tax administration of each state along three aspects: standardized procedures, property tax appeals procedures, and tax burden of residential properties relative to business properties. A total score and a grade are given to each state, with a lower score indicating a fairer property tax administration. An important component of a good property tax administration is a fair property tax appeal procedure.[8] Our empirical evidence shows that on average large investors received a higher assessment discount in states with a fairer state property tax administration. We also find that the magnitude of the assessment discount for large investors in a state is positively correlated

[8]See "The Best and Worst Property Tax Administration - COST scorecard on state property tax administrative practices, 2011."

with their local market share, the level of local property tax burden, and the average share of votes for the Democratic candidate in presidential elections.

The paper proceeds as follows. Section 2.2 discusses our contribution to the literature. Section 2.3 introduces the institutional background behind property taxation. Section 2.4 describes the data used in the empirical analysis and discusses the summary statistics. Section 1.5 discusses our hypotheses. Section 1.6 discusses the empirical design. Section 1.7 reports the empirical results. Section 2.6 provides the concluding remarks.

1.2 Literature review

Our findings contribute to two strands of literature. First, our findings add to the emerging literature on institutional SFR investors. Recent studies examine the changing landscape of the U.S. SFR markets and the social and economic impacts of institutional investors, such as on housing prices, renters' welfare, and neighborhood stability (Allen et al., 2018, D'Lima and Schultz, 2019, Ganduri et al., 2022, Gurun et al., 2022, Lambie-Hanson et al., 2019, Mills et al., 2019, Smith and Liu, 2020). For example, activist groups, such as the ACCE Institute, Americans for Financial Reform, and the Public Advocate, have criticized "Wall Street landlords" for charging high rent and fees, poorly maintaining rentals, and ruthlessly evicting tenants, all activities that undermine renters' welfare. On the flip side, recent studies suggest that large institutional investors can play a beneficial role in improving neighborhood safety, while internalizing the cost of neighborhood amenities (Gurun et al., 2022).

Our study is the first to examine the effect of institutional SFR investment on municipal finance through property taxation. Property taxes are an important source of revenue for local governments. Additionally, property taxes, which are the single largest item of operating expenses for real estate investors, have been increasing in recent years with the booming real estate market. Recent studies show that institutional SFR investors achieve economies

of scale by building geographically concentrated portfolios and merging with rival investors (Gurun et al., 2022). Our study finds that the economies of scale also allow large investors to manage their property taxes more efficiently and effectively.

Second, our paper contributes to the literature on inequality in property taxation. Previous studies have examined property tax inequality from various perspectives. For example, some recent studies such as (Avenancio-León and Howard, 2022) and (Kahrl, 2016) examine the racial gap in property taxation . Another line of research focuses on assessment regressivity, a term that refers to situations in which low-priced (high-priced) properties have higher (lower) assessment ratios (Amornsiripanitch, 2020, Atuahene and Berry, 2018, Berry, 2021, Hodge et al., 2017, McMillen and Singh, 2020). Interestingly, the institutional assessment discount documented in our study is distinct from assessment regressivity because institutional investors typically purchase properties with a deep discount relative to local market values. Hence, any assessment regressivity would bias us against finding an assessment discount for institutional investors. Issues related to property tax assessment have received relatively little attention in municipal finance research. Our findings inform policy makers about the inequality of property taxation between owner-occupiers and investors and point out the drawbacks of the current property tax appeals system that have potentially disadvantaged owner-occupiers.

1.3 Institutional background of property taxation

In the United States, taxpayers from all 50 states and the District of Columbia pay taxes on real properties. For the most part, local governments (e.g., municipalities, counties, and school districts) instead of state governments levy annual property taxes on residential properties. The determination of the property tax amount for a property usually involves three steps: (1) assessing the market value of the property; (2) determining the taxable value of the property; and (3) applying the tax rate from the corresponding tax jurisdiction

to the taxable value of the property. In most states, the county takes the responsibility of determining the assessment value. Each state applies a scaling factor to determine the assessed value relative to the fair market value, and this scaling factor ranges from 10% to 100%. For example, in Georgia, the scaling factor is 40%. If a county assessor determines the fair market value for a property to be $100,000, then the assessed value is $40,000.

The frequency of assessments also varies across jurisdictions. Most jurisdictions conduct assessments annually, and some do so less frequently. To determine the taxable value of a property, state and local governments apply other limits, exemptions, deductions, and credits to the assessment value when applicable.[9] Local governments, such as municipalities and school districts, calculate the annual tax amount of a property by multiplying the imposed tax rate by the taxable value of the property. Since the property tax system varies along many dimensions across the country, it is crucial to focus on the comparison of assessment ratios within small geographic areas that share the same property tax system. We carefully design our empirical tests to address this issue.

We focus our analysis on assessment ratios within counties for several reasons. First, variations in the fair market value due to property-level characteristics can be accounted for by the most recent transaction price in the denominator, making the comparison between houses meaningful even without explicitly controlling for home characteristics, such as size and age. Second, most local property assessments are done at the county level. The scaling factor, which is used to determine the assessed value based on appraisal value, is also the same within counties. Third, the actual tax amount is a much more complex measure because of exemptions. The reported tax bill from the county reflects the assessed values, exemptions, and tax rates. Exemptions reflect the "legislative intent and public administration of the tax system," and thus they have been excluded for the purpose of our study (Avenancio-

[9]Source:https://www.urban.org/policy-centers/cross-center-initiatives/state-and-local-finance-initiative/projects/state-and-local-backgrounders/property-taxes

León and Howard, 2022). The most common type of exemption is the homestead exemption for owner-occupied residences and senior-citizen-occupied homes. For instance, Colorado exempts seniors and qualifying veterans from paying taxes on the first 50% of the property market value up to $200,000. Florida exempts property tax up to $50,000 of the assessed value for owner-occupied properties[10] (Lincoln Institute of Land Policy, 2021). Counties also have discretion on the exemption level. It is difficult to identify the dollar amount and the number of exemptions applied to each property to back out the preexemption tax burden. Fourth, in principle, a fair tax administration system should result in the same assessment ratio for all property owners given the same scaling factor, regardless of variations in exemptions and effective tax rates. The effective property tax rate could be different within a county because of variations in the tax rate levied at the township or school district level, and these variations largely reflect local government's financial health, budget, and the quality of local public services. These variations should not constitute a source of inequality in the assessment ratio within a county. Therefore, we consider the assessment ratio to be the most simple and transparent measure for studying the inequality of property taxation. This measure can be easily computed with little ambiguity, while controlling for potential confounding factors to a large extent.

1.4 Data

1.4.1 Data source

Our empirical analysis relies on six sources of data: (1) detailed property-level data from Zillow; (2) micro-level data on property tax appeals from Cook County, Illinois; (3) preliminary and final property tax assessment data from Florida; (4) state-level census data and government finance data from the U.S. Census Bureau; (5) state-level data on share of votes

[10]The first $25,000 of value is entirely exempt. The second $25,000 exemption applies to the value between $50,000 and $75,000 and does not include a benefit on school tax.

for the Democratic candidate at presidential elections from MIT Election Data and Science Lab; and (6) scores for state property tax administrative practices from the Council on State Taxation. We will explain each of the data sources in detail below.

We get data on real estate transactions from Zillow's Assessor and Real Estate Database (ZTRAX). Our sample period is from 2005 to 2016. This database contains two types of data: transaction data and assessment data provided by local property tax assessors. Our main data set has detailed transaction-level information, such as transaction date, deed type, sales price, identities, and mailing addresses of transaction parties, among others. We merge this data set with data on property characteristics as of 2016 and annual assessment values post-transaction date from the assessment database based on a unique parcel ID. Detailed property characteristics include property type, property location, year built, building area square footage, and the number of bedrooms and bathrooms. In summary, our transaction-level data include three sections: transaction details, property characteristics, and yearly assessment value during the owner's holding period of the property.

Our sample contains data from 31 states. There are 12 nondisclosure states[11] in the United States, where the sales price of the property is neither recorded nor publicly available. The sales price data for these states are either missing or sparsely populated in ZTRAX. Therefore, we exclude these nondisclosure states from our sample, except for Missouri, since the nondisclosure policy only applies to certain counties in Missouri. Because of data limitations with ZTRAX, we also have to exclude the District of Columbia and the following seven states from our analysis: South Dakota, Rhode Island, South Carolina, Tennessee, Maine, Vermont, and Wisconsin. Important data fields or files are either completely missing or sparsely populated for these states in the Zillow database. Lastly, we choose to exclude California from our analysis. California passed Proposition 13 in 1978. This amendment

[11]The current list of nondisclosure states includes Alaska, Idaho, Kansas, Louisiana, Mississippi, Missouri (some counties), Montana, New Mexico, North Dakota, Texas, Utah, and Wyoming.

puts limits of any ad valorem tax on properties at 1% of their assessment value, caps the annual increase of the assessment value at 2%, and establishes a new concept of the base year value for property tax assessment. Even though base year values are supposed to be reassessed upon sales of property, there exists different mechanisms and exceptions that owners are able to transfer the low base year assessment value to a replacement property. [12] The gap between market values and assessment values has widened since the passage of Proposition 13 in California. Therefore, we consider the mechanism of producing property tax assessment values in California significantly different from that in the other states.

To count the number of property holdings of the buyer at the time of purchase, we identify each property owner by the recorded mailing address as of 2016 assessment data. We aggregate the number of properties across country held by each owner as of 2016. Since the buyer's mailing address is available for each transaction, this allows us to work backward to add or deduct the number of property holdings for each owner prior to 2016. As a result, we are able to create an annual property count for each property owner during our sample period. Then we append the property count to the transaction-level data based on buyer's mailing address and transaction date. We use the property count one year prior to the transaction year to determine the property owner size group. In our empirical analysis, we set owner-occupiers as the base group and divide investors into four groups based on the number of properties they own (Table 1.1): *micro investor* (with 2—20 properties), *small investor* (with 21—50 properties), *medium investor* (with 51—100 properties), and *large investor* (with more than 100 properties). For example, a property transaction took place in June 2010, and the buyer's property count was 58 as of 2009, so the owner belongs to the *medium investor* group. Our final transaction-level sample includes 9.6 million observations in 31 states from 2005 to 2016.

[12] Source:https://assessor.saccounty.net/LowerMyTaxes/BaseYearValueTransfers/Pages/default.aspx

We conduct our analysis on the property tax appeals process in two geographic regions: Cook County in Illinois and Florida. We obtain micro-level data on property tax appeals from the Cook County government open data source. This data set contains a unique tax pin for each property, an indicator for whether an appeal is filed, an appeal outcome, and a final adjusted assessed value post-appeal, among others. We append the property location information from ZTRAX database to each property in the Cook County appeals data set based on the unique property tax pin.

We collect property tax assessment data from the Department of Revenue in Florida. This data set provides data on final assessment values, an adjustment between the preliminary and final assessment value, the reason for the adjustment, and the owner's property address.

Lastly, we acquire state-level data from various sources. First, we obtain the score that measures the fairness of property tax administrative practices for each state from the Council on State Taxation. This scorecard was published in 2011. Each state receives a score and a letter grade, with a lower score indicating a fairer property tax administration. Second, we collect data on income per capita and property tax per capita from the U.S. Census Bureau. Third, we use the state-level share of votes for the Democratic candidate in presidential elections as a proxy to measure state ideology and policy preferences, which are commonly applied in the political science literature (e.g., Carson, 2005, Gailmard and Jenkins, 2005). This data set comes from MIT's Election Data and Science Lab. We calculate the ratio of votes cast for the Democratic candidate to total votes cast in each state at the 2004, 2008, and 2012 presidential elections. A larger number corresponds to a more liberal state.

Table 2.1 reports summary statistics for the 31 states in our sample. This table shows the total count, the average assessment ratio, and large investors' local market share in each state. It is important to note that every state has a different scaling factor to determine the assessed value based on fair market value; therefore, it is not meaningful to compare

assessment ratios across states. Large investors' local market share in our full sample is about 1.3%, ranging from 0.19% to 4.21% for each state.

1.5 Hypothesis

The property tax appeals process is available in every state for property owners to challenge local governments' assessments. A standard annual process starts with mail issued by a local assessor office that notifies owners of the initial proposed assessed value. Property owners that disagree with the proposed values can file an appeal before a set deadline. Typically, an appeal can be resolved informally or through a formal hearing in which owners present evidence to support an alternative value. A successful appeal results in a reduction in the assessed value and thus a lower final tax bill. The appeals process has long been seen as complicated and time consuming by property owners.

Compared with investors, owner-occupiers in general make a more prudent choice when it comes to property tax appeals. Some first-time homebuyers are not even aware of the existence of the tax appeals process upon the purchase of their first home. For those who are aware of the option of a tax appeal, they need to weigh the costs against the benefits when deciding whether to conduct the appeal. On the cost side, it takes a considerable amount of time and effort to research and collect evidence, such as comparable home values, photos to show the conditions of subject properties, and repair invoices to support a lower value. Appearing in a board of review or court is often required for the hearing process. This appears to be a major burden for property owners in this process. The Florida Department of Revenue shows that about 41% property owners actually withdrew their filing without going to hearings between 2010 and 2016.

Additionally, financial costs could be incurred from the actual filing fees and hiring of a professional property tax or legal specialist if the owner-occupiers decide to hand over the tasks to a third party. Given the complexity of the procedure, owner-occupiers need

to evaluate the likelihood of winning the appeal and potential tax savings upon successful appeal. Therefore, owner-occupiers who decide to file an appeal are those who either are more confident they will receive a favorable outcome. For example, apparent mistakes like typos are shown in the initial assessed value, or those who have help from third-party professionals. In contrast, property tax appeals appear to be a must-do process for large investors every year as this is the only channel to reduce their largest portion of operating expenses. Having a dedicated property tax team, investors with large portfolio can take advantage of the economies of scale to lower the cost per appeal filing. The marginal cost of filing for large investors is very low and there is no consequence of an appeal failure (i.e., the worst-case scenario is no value reduction). Thus, it is a no-lose game for large investors to file an appeal for every property, regardless of their perceived likelihood of winning or estimated tax savings upon winning.

If a certain group of owners has a higher propensity to appeal and has better access to resources, such as lawyers and professional tax specialists, that can help with the appeals process, then these owners are more likely to generate disproportionate value reductions than are the others, leading to assessment inequality (Berry, 2021). In addition, a large portfolio gives investors abundant data on properties in the same neighborhood. This allows them to gather sufficient and compelling evidence to establish strong cases for appeals, helping to achieve higher value reductions. Therefore, we hypothesize that large investors tend to have the resources and incentives to proactively engage in property tax appeals and thereby receive assessment discounts relative to owner-occupiers.

1.6 Empirical design

We estimate the following regressions to test the inequality in assessment ratio across owner size groups:

$$ln(assessed\ value_{n,t+1}/market\ value_{n,t}) = \alpha + \sum_{k=1}^{4} \beta^k Owner\ group_{n,t,k} + \gamma_1' \mathbf{X} + \theta_{c,t} + \epsilon_{n,t}. \tag{1.1}$$

The dependent variable is the natural logarithm of the ratio of assessed value to market value for property n in year t. We use the sales price amount as the market value in year t and the assessed value one year after the transaction year as the assessed value in our model. $Owner\ group_{n,t,k}$ in Model 2.2 indicates the property owner size group. Specifically, we set owner-occupiers as the base group and divide investors into four size groups as shown in Table 1.1.

1.7 Empirical results

1.7.1 Baseline results

In this subsection, we first examine whether large investors receive an assessment discount for their properties relative to owner-occupiers by testing Model 2.2. The fair property tax system ought to ensure that realized assessment ratios are the same for all properties, regardless of differences in property attributes and owner attributes.

Our analysis includes all property transactions in the 31 states that have available assessed values in the year after transaction and the sales price amount is more than \$75,000.[13] We also exclude transactions with certain deed types that often result in abnormal sales price, such as foreclosures, distressed sales, and sheriff's deed. We control for unobservable time-

[13]Our baseline results are robust with different cutoffs of the property value.

varying local characteristics using county×year fixed effects as property assessments are mostly conducted at the county level. The dependent variable is the natural logarithm of the assessment ratio for each property. Column 1 of Table 1.3 shows the coefficient estimates for all four investor groups based on their scale. The estimates suggest that large investors with 100 or more properties have a significant 3% discount in assessment ratio relative to owner-occupiers.

To show that the assessment discount is not driven by heterogeneity in property attributes, we control for various property characteristics, including property age, building area square footage, the number of bedrooms, and the number of full bathrooms. In Column 2 of Table 1.3, the coefficient estimate on *Large investors* is similar: the assessment ratio is 3.7% significantly lower for large investors' properties relative to owner-occupied homes

In addition, we apply the same analysis to a subsample of properties with repeated sales during the sample period. We compare assessment ratios of the same property held by owners that belong to different property owner size groups at different points in time. The sample size reduces to 2.8 million observations as we exclude properties with only one transaction and properties with multiple transactions but owned by people from the same owner-size group. The estimates in Column 3 of Table 1.3 show that large investors have a significant 4.7% assessment discount relative to owner-occupiers on the same property owned at different point of time. These results strongly support the idea that large investors receive a lower assessment ratio for their properties relative to owner-occupiers.

A natural question that arises from the preceding discussions is whether all investors, regardless of their scale, have a disproportionate advantage over owner-occupiers regarding the property tax appeals process. Separate coefficient estimates for different size groups allow us to capture any nonmonotonic relation between investor scale and the assessment ratio. An interesting finding is that *Micro investors* with 2 to 20 properties, unlike large investors, have significantly higher assessment ratios on their properties relative to owner-occupied

homes. Column 1 shows that the assessment premium for micro investors is 0.8% with our baseline model. This coefficient estimate in Column 2 is similar after adding property-level controls. The repeated sales analysis in Column 3 shows an even higher assessment premium (2.3%) for micro investors' properties compared with owner-occupied homes. These results also provide suggestive evidence that the relation between investor scale and the assessment ratio is nonmonotonic; that is, micro investors' properties receive an assessment premium instead of a discount relative to owner-occupied homes.

To further examine the nonlinear relation between investor scale and the assessment ratio, we classify investors using a much finer grouping based on their number of property holdings. Then we reestimate our baseline model using the new 17 investor groups based on their scale. Figure 1.1 presents the estimated coefficients and 95% confidence intervals for each investor group based on their number of property holdings. This figure clearly exhibits a hump shape between investor scale and the assessment ratio. Micro investors with only two properties see higher assessment ratios than do owner-occupiers. This assessment premium for micro investors increases when they gradually scale up their businesses. Then the assessment premium shows a decline once the number of property holdings hits one dozen. Investors with more than 100 properties show a significant assessment discount on their properties relative to owner-occupied homes.

These small-scale mom-and-pop investors with only a few income properties typically self-manage their properties. These investors may have their limited attention and resources stretched thin as they increase their number of properties. Keeping track of the filing deadlines, analyzing comparable market values, making decisions on appeals filings, and preparing documents for each filing can easily turn into a daunting task for these investors as their portfolio size increases. Any geographic diversification of their portfolio will greatly increase the complexity of property tax management as it involves multiple tax jurisdictions. As a

result, micro-sized investors may be less efficient and effective in managing their property taxes, leading to a higher assessment ratio relative to that of owner-occupiers.

1.7.2 Property tax appeals

In this subsection, we explore a potential mechanism through which large investors achieve favorable assessment ratios, namely, the property tax appeals process.

We show evidence in Section 1.7.1 of an assessment gap between a large investor and an owner-occupier who hold the same property at different points in time. Hence, we surmise that any observed gap in the assessment ratio between owner size groups could be a result of dissimilarity in property tax appeals behavior and the resulting appeals outcome. Since aggregate nationwide property tax appeals data are not available, we conduct our analyses in two geographic areas: Cook County in Illinois and Florida.

1.7.2.1 Cook County property tax appeal

Cook County, Illinois, is the second-most populous county in the United States with 5.2 million population as of 2020. According to the Lincoln Institute of Land Policy, Illinois is among the states that have the highest state and local reliance on property taxes (Lincoln Institute of Land Policy, 2021). It also has the second-highest effective tax rate on medium-value residential homes in the country. Cook County has two channels for filing appeals for residential properties: one is through the county's assessor office, and the other is through a county board of review. Cook County, where Chicago is located, might have the highest number of appeals filing cases of any tax jurisdiction. For example, in 2015, Cook County processed about 166,000 appeals filings, whereas San Francisco and New York City had 4,995 and 53,000 respective filings (Berry, 2021, Grotto, 2017). This micro-level property tax appeals data set has been used extensively by prior studies to examine property owners' interaction with the property tax administration regarding property tax appeals (e.g.,

Avenancio-León and Howard, 2022, Berry, 2021, Ross, 2017, Weber and McMillen, 2010). Cook County property tax appeals data are available from 2010 to 2020. We perform the analysis using a sample that overlaps with our baseline analysis: 2010 to 2016. There are $4.1 million records that can be linked to owners with available property count data in our sample.

Cook County appeals data provide both the initial proposed assessed value mailed to the property owners and the final adjusted assessed value post-appeal. We reestimate the baseline model with the subsample from Cook County using both assessed values. Column 2 of Table 1.4 shows that the assessment ratios for large investors' properties are not significantly different from those of owner-occupied homes. This is an exception to what we document using the national data. However, this is most likely to be driven by the large purchase price discount at 10.5% in Column 3 of Table 1.4 that large investors get relative to owner-occupiers. Cook County's assessor office states that "A home's recent purchase price is sometimes, but not always, a reflection of its true market value. Sale prices of a home depend on not just the characteristics of the home itself, but also the individual buyer, seller, and even the time of year. To be fair to all properties, those that have sold recently, and those that haven't, the CCAO uses the same methods to estimate true market values." This standard assessment guideline suggests that, with a fair practice, the assessed value should not be depressed for one property that is acquired at a discounted price compared with other properties purchased at market value with similar home characteristics in the same neighborhood. Hence, the significantly large discount (10.5%) in the denominator (i.e., sales price) still implies a discount in the final assessed value for large investors' properties relative to owner-occupied homes. Comparing Columns 1 and 2 of Table 1.4, we observe that the coefficients for all four investor groups drop after the appeals process. Large investors, in particular, experience a notable decrease in the assessment premium of their properties,

dropping from 3.3% to no premium post-appeal. These results suggest that the outcomes of the appeals are more favorable for large investors than for owner-occupiers.

Next, we examine whether property tax appeals behavior differs across property owner size groups. Specifically, we study if large investors are (a) more likely to appeal, (b) more likely to win an appeal upon filing, and (c) receiving a higher value reduction conditional on a successful appeal. Two variables in the data set indicate if a property owner files an appeal through either of the channels in a given year. We identify a property with an appeal filing if either of these indicators is true. Then we define a successful appeal as one in which the final adjusted assessed value is lower than the initial proposed assessed value mailed to the property owners. The value reduction percentage is calculated as (final adjusted assessed value — initial assessed value)/initial assessed value. We merge the appeals data with the comprehensive transaction data, which includes the owners' property count based on the unique tax pin.

Table 1.5 shows summary statistics for the Cook County appeals data. The average likelihood to appeal is about 22% for all property owners. This rate is higher for all investors in general. Large investors' propensity to appeal is 14-percentage-points higher than is owner-occupiers. The overall appeal success rate is 67%, and investors are less likely to win an appeal upon filing than owner-occupiers. The mean value reduction is 15% for the whole sample. Investors also receive a higher value reduction than do owner-occupiers regardless of their portfolio size.

Table 1.6 presents the estimates for the likelihood to appeal, the likelihood to win upon filing, and the value reduction upon winning based on the Cook County appeals data. We control for unobservable time-varying local characteristics at the census tract level using census tract×year fixed effects. In Column 1 of Table 1.6, dependent variable is a binary variable that equals one if the property owner files an appeal. The estimates show that all investor groups are significantly more likely to appeal than owner-occupiers. The magnitude

of coefficients ranges from 5.5 percentage points to 21.2 percentage points, with 15.4 percentage points for large investors. It is worth noting that the economic magnitude is close to twice higher for large investors than that for micro investors. This evidence is consistent with our hypothesis that once investors' portfolio size becomes substantial, they can start dedicating more resources to property tax management and thereby take advantage of economies of scale.

In Column 2 of Table 1.6, the dependent variable is a binary variable that equals one if an owner wins the appeal conditional on filing. The estimates suggest that all investor groups are significantly less likely to win an appeal than are owner-occupiers. The magnitude of the coefficients monotonically decreases as investors' portfolio size increases. Compared with owner-occupiers, large investors are 10.4-percentage-points less likely to win an appeal upon filing. As discussed in Section 1.5, large investors tend to file appeals on their properties very aggressively, regardless of their perceived likelihood of winning or estimated tax savings upon winning. Therefore, it is reasonable to see that large investors have less chance of winning an appeal as some appeals might be filed even when the subject properties' assessed values already correctly reflect the market value. In Column 3 of Table 1.6, the dependent variable is the value reduction percentage upon a successful appeal. All investor groups have a significantly higher value reduction than do owner-occupiers upon winning an appeal. Large investors get a 5.4-percentage-point higher value reduction than do owner-occupiers. This translates to 41% higher value reduction than what owner-occupiers can receive on average (13.18%) upon a successful appeal. This is in line with our hypothesis that access to abundant data on properties allows large investors to obtain persuasive evidence, ultimately resulting in greater value reduction.

1.7.2.2 Florida property tax appeal

To substantiate our findings from Cook County property tax appeals data, we conduct a similar analysis using the Florida property tax assessment data between 2010 and 2016. Sixty-seven counties in Florida are responsible in conducting the annual assessment for real property at 100% of the market value. The property owner can challenge the initial proposed assessed value by filing a petition with the county value adjustment board or a lawsuit in circuit court. Filing an appeal through the value adjustment board is a much more common way, with a lower cost ($15 per filing) and less complicated procedures. Property owners can sometimes resolve discrepancies in the assessed value with the appraiser prior to hearing. If agreement cannot be reached prior to the hearing, property owners need to submit evidence to support the alternative value and appear in a scheduled hearing.

The Florida Department of Revenue provides property-year panel data on changes in property appraiser's opinions of a property's market value between the preliminary appraisal value and the final certified appraisal value. This data field allows us to calculate the amount of reduction in the assessed value after a property tax appeal. However, with data limitations, we are not able to identify those properties that have filed an appeal with a unsuccessful outcome (i.e., no value reduction). The Florida Department of Revenue produces statewide summary data on the number of appeals and the success rate of appeals. It shows that the average appeals filing rate is less than 1%, and the average success rate upon filing is 18.8% during our sample period.[14] Both propensities to appeal and the appeals success rate are significantly lower in Florida than in Cook County.

With the Florida data, we focus on examining whether appeals outcomes differ among the property owner size groups. Again, we define a successful appeal as one that results

[14]The appeal filing rate is estimated using the number of residential appeals filings and the number of residential parcels each year. The appeals success rate is reported for all real properties.

in assessed value reduction post-appeal. In total, there are about 12.3 million property assessment records during the sample period. Table 1.7 shows that all investors can achieve a significantly higher value reduction than can owner-occupiers upon winning. The magnitude of coefficients monotonically increases with the portfolio size, ranging from 1.9% to 11.6%. The magnitude of the coefficient for *Large investors* is about five times larger than that of *Micro investors*. The value reduction for large investors' properties is twice as much as that of owner-occupied homes (11.3%). This result is consistent with our findings from Cook County data, and collectively they suggest that large investors are more effective at reducing the assessed values of their properties through appeals than are owner-occupiers.

1.7.2.3 Discussions

Whether utilizing the appeals process could eventually lead to overall assessment discount for large investors depends on all of these three factors: the propensity to appeal, a successful appeals rate conditional on an appeals filing, and the amount of value reduction upon winning. From our empirical results in Cook County, the likelihood to appeal is 77.0% higher, the success rate is 15.1% less, and the value reduction is 41.0% higher for large investors relative to owner-occupiers. In addition, we show that the assessment premium for large investors is eliminated after the appeals process in Cook County as we discussed earlier (see Table 1.4). Collectively, the evidence indicates that the overall effect on the appeals process is favorable for large investors, likely because of the much higher likelihood to appeal and much higher value reduction upon winning than owner-occupiers. Both the Cook County and Florida case studies largely support our hypothesis that the property tax appeal is the channel for large investors relative to owner-occupiers, to achieve a lower assessment ratio.

1.7.3 Alternative explanations

In this section, we discuss two other possible drivers for large investors' assessment discount.

1.7.3.1 Assessor bias

One potential explanation for the assessment discount is that assessors have a systematic bias toward large investors, meaning that they deliberately produce a lower assessed value on the properties owned by large investors. We believe this is less likely to be the case for the following reasons. First, tax jurisdictions have widely applied computer-assisted mass appraisal or automated valuation models to determine the assessed value of properties. In-person appraisal with a site visit is simply not feasible given the large number of properties to be assessed. The International Association of Assessing Officers has developed standard guidelines for mass appraisal, which applies multivariate regressions using a small number of property-level characteristics (Avenancio-León and Howard, 2022, IAAO, 2018). Therefore, whether the owner is an investor and/or the size of the owner's property holdings should not play a role in determining the assessed value. Moreover, to the best of our knowledge, assessors have no direct information related to an owner's other property holdings in the first place; thus, they are not likely to favor investors based on their scale. Most large investors hold properties under different entities in different regions; therefore, it is a difficult and time-consuming process to identify the scale of an investor.

1.7.3.2 Sales price gap

One could argue that the denominator (i.e., sales price) of the assessment ratio instead of the nominator (i.e., assessed value) drives the assessment discount for large investors. In other words, our findings would suggest that large investors on average pay a premium in purchase price compared with owner-occupiers. However, this contradicts the widely documented empirical results that large investors in general acquire properties at a lower price. Previous studies provide ample evidence on this. For example, (Smith and Liu, 2020) shows that institutional investors in Atlanta purchased houses at a significant discount of

6.3% to 11.8% compared to owner-occupiers between 2000 and 2014. Similar findings are presented by (Allen et al., 2018) using data on Miami-Dade County, that investors in general purchase at a discount of 9.5% relative to owner-occupiers. We find similar results in our national sample. Large investors acquire properties at a significant discount of 13.6% after controlling for property-level characteristics (see Table 1.8). A significant lower purchase price would bias against finding an assessment discount for large investors. Hence, we can rule out the possibility that sales price gap between large investors and owner-occupiers drives the assessment discount for large investors. Our estimates on large investors' assessment discount are likely to be understated by over 10 percentage points, and it should be considered to be the lower bound estimate of the actual assessment discount.

1.7.4 State-level analysis

Next, we estimate Model 2.2 separately for each of the 31 states in the sample during the sample period. We control any unobservable time-varying local characteristics using county×year fixed effects. Table 1.9 reports the coefficients estimates and standard errors from these regressions. Coefficient estimates on *Large investor* are negative for 20 states, and 16 of those are statistically significant. Another eight states have significantly positive coefficients for *Large investor*. In these 16 states, large investors have a significant assessment discount ranging from 0.5% to 14%. The highest assessment discounts are seen in New York, Indiana, and New Hampshire. Other states with strong institutional investors' presence (Gurun et al., 2022) such as Georgia, Florida, and Nevada also see significant assessment discount for large investors, the discounts are 11.1%, 3.9%, and 9.2%, respectively. Among these 16 states, the mean (median) assessment discount is 7.9% (7.2%). Figure 1.2 plots the estimated coefficients and 95% confidence intervals for large investors by state.

1.7.5 Cross-state analysis

Then we explore whether state-level variations in large investors' assessment discount are driven by heterogeneity in state's property tax system, in particular the property tax appeal practices. We also examine other factors that relate to the cross-state difference in assessment discount for large investors. To measure the quality of state property tax administration, we use a score published by Council on State Taxation (COST) in 2011. The published scorecard is used to evaluate each state's property tax administration from three aspects: standardized procedures, fair property tax appeal procedures and residential property tax burdens versus business property tax burdens. A lower score indicates a fairer state property tax administration. We then use the t-value on *Large investor (100+)* from state-level estimates of Model 2.2 as dependent variable for the cross-sectional analysis. The t-value on *Large investor (100+)* takes into account both the estimated effect and estimation error of assessment discount for each state. Table 1.11 presents the estimates from our cross-state analysis. Large investors' assessment discount is significantly higher in the states with lower "fair property tax administration score" (i.e., fairer property tax administration). According to COST, one important component of a fair property tax administration is to have fair property tax appeal procedures. Typical fair property tax appeal procedures should have the following characteristics: (1) fair and reasonable deadline for the initial appeal, (2) reasonable burden of proof to sustain an appeal, (3) available of de novo review before an independent tribunal after completion of any administrative review at both local and state levels, and (4) partial tax payment or disputed portion of the property tax in escrow in case of a dispute.[15] All these factors potentially benefit large investors disproportionately given their professional approach to tax appeal and large number of filings each year. For example,

[15]See "The Best and Worst Property Tax Administration - COST scorecard on state property tax administrative practices, 2011."

a reasonable (vs. a tight) deadline on an appeal filing allows large investors to have sufficient time to prepare for quality evidence given the large number of filings they need to complete each year. In general, a simpler and fairer property tax appeals system allows large investors to scale up the appeal filings efficiently and effectively.

We include other covariates in the cross-state regression that help explain the state-level variations in the assessment discount for large investors. Large investors' local market share is calculated as the number of houses owned by large investors divided by total number of houses in the Zillow database in each state. The property tax burden is defined as the time-series average of property tax dollar amount as a percentage of medium personal income in a state during the sample period. We show that large investors' assessment discount is higher for states with higher large investors' market share and higher property tax burden in Table 1.11. The relation between assessment discount and large investors' local market share could be a result of selection effect or resources allocation of large investors. On the one hand, because property tax is the largest portion of their operating expenses and inflexible, large investors have strong incentives to invest more in the states with fairer property tax administrative practices. On the other hand, large investors may choose to allocate more resources to property tax management in the areas where their portfolio is highly concentrated. In this cross-state analysis, the assessment discount for large investors is higher in the state with greater large investors' presence even after controlling for local property tax administration score. Thus, the resources allocation by large investors seems to be a more plausible explanation. Similarly, in the states with higher property tax burden, large investors may dedicate more resources to their property tax management, thus receiving more favorable appeal outcome. To control for the state's political and economic environment, we include two control variables in our cross-state regression: income per capita and share of votes for Democratic candidate in the presidential elections. We use the median share of votes for Democratic candidate in 2004, 2008, and 2012 presidential elections. A larger number

corresponds to a more liberal state. The coefficient estimate for *Democratic presidential vote share* suggests that large investors' assessment discount is significantly higher in more liberal states. Our results do not show any significant relation between large investors' assessment discount and income per capita.

1.7.6 Robustness

Lastly, we perform a few robustness tests for our baseline model. First, we reestimate our baseline Model 2.2 at a much finer geography level, namely, the census block-group. Tax rates can vary within a county because of different rates levied at more local levels, such as townships and school districts. In principle, the difference in local tax rates should not be a source of variation in assessment ratios. However, one may still concern that assessors create systematic bias in assessed values based on the quality of public services or financial health of local government. For instance, local assessors might inflate (deflate) the assessed value of the properties in a neighborhood with better(worse) public amenities and/or worse(better) financial condition. If the spatial distribution of the large investors' properties correlates with those features that lead to systematic undervaluation, we might find similar results as our baseline results. To address this concern, we restrict our analysis within census block-group. Block group is a small geographic division of census tract, and typically has 600 to 3,000 people. Although this is not a definition of tax jurisdiction, we can reasonably assume that the local tax assessment practices, public amenities, local government financial condition and neighborhood attributes are fixed within the census block-group. Our estimates in Table 1.12 are qualitatively the same as the baseline estimates at the county level. Second, in our baseline results, we use the natural logarithm of the assessment ratio as the dependent variable because of the large variance in raw ratios as a result of scaling factor. Table 1.13 presents the results with the raw assessment ratio as the dependent variable. The estimates

remain robust, and the economic magnitudes of the estimates are similar to those from the baseline results.

1.8 Conclusion

In this study, we examine the effect of large SFR investors on municipal finance through the inequality of property taxation. We measure property tax inequality in terms of the assessment gap between large investors' properties and owner-occupied properties. We show that large investors that own more than 100 properties have a significantly lower assessment ratio on average than do owner-occupiers. Moreover, there is a hump-shaped relation between the portfolio size of investors and the assessment ratio of their properties. Using data from Cook County, Illinois, and Florida, we identify property tax appeals as the potential mechanism that leads to the observed assessment discount for large investors. Large investors are significantly more likely to file a property tax appeal and receive a higher value reduction upon a successful appeal relative to owner-occupiers. Professional SFR investors have advantages in the economies of scale that allow them to manage property taxation more efficiently and effectively. Our state-level analysis also shows that close to two-thirds of states exhibit an assessment discount for large investors. Furthermore, states with a fairer property taxation administration, a higher market share by large investors, and a higher property tax burden see a significantly deeper assessment discount for large investors.

Our findings inform policymakers about inequalities in the property taxation between owner-occupiers and investors and point out the drawbacks of the current property tax appeals system that owner-occupiers and micro-size investors end up taking a disproportionally larger property tax burden. Our analysis at the state level suggests that implementing supposedly "fairer" property taxation procedures may not necessarily lead to just outcomes. As a policy recommendation, we suggest that local governments should take initiatives to enhance homeowners' awareness of property tax appeals. To enable owner-occupiers and

micro-size investors to make the most of the property tax appeals system, we also recommend that local governments provide accessible resources and support to assist them in filing appeals. By offering such assistance, homeowners can gain a better understanding of the appeals process and level the playing field with large investors who have greater access to resources and expertise in this area.

However, while we provide micro-level evidence on the property tax advantage of institutional SFR investors, the overall effect of institutional investments in the SFR markets on municipal finance remains unclear because they also affect the local economy through various other channels, such as overall house prices, job creation, and the crime rate, among others. Hence, the economic and social impacts of the institutionalization of SFR markets remains an important question that demands further study.

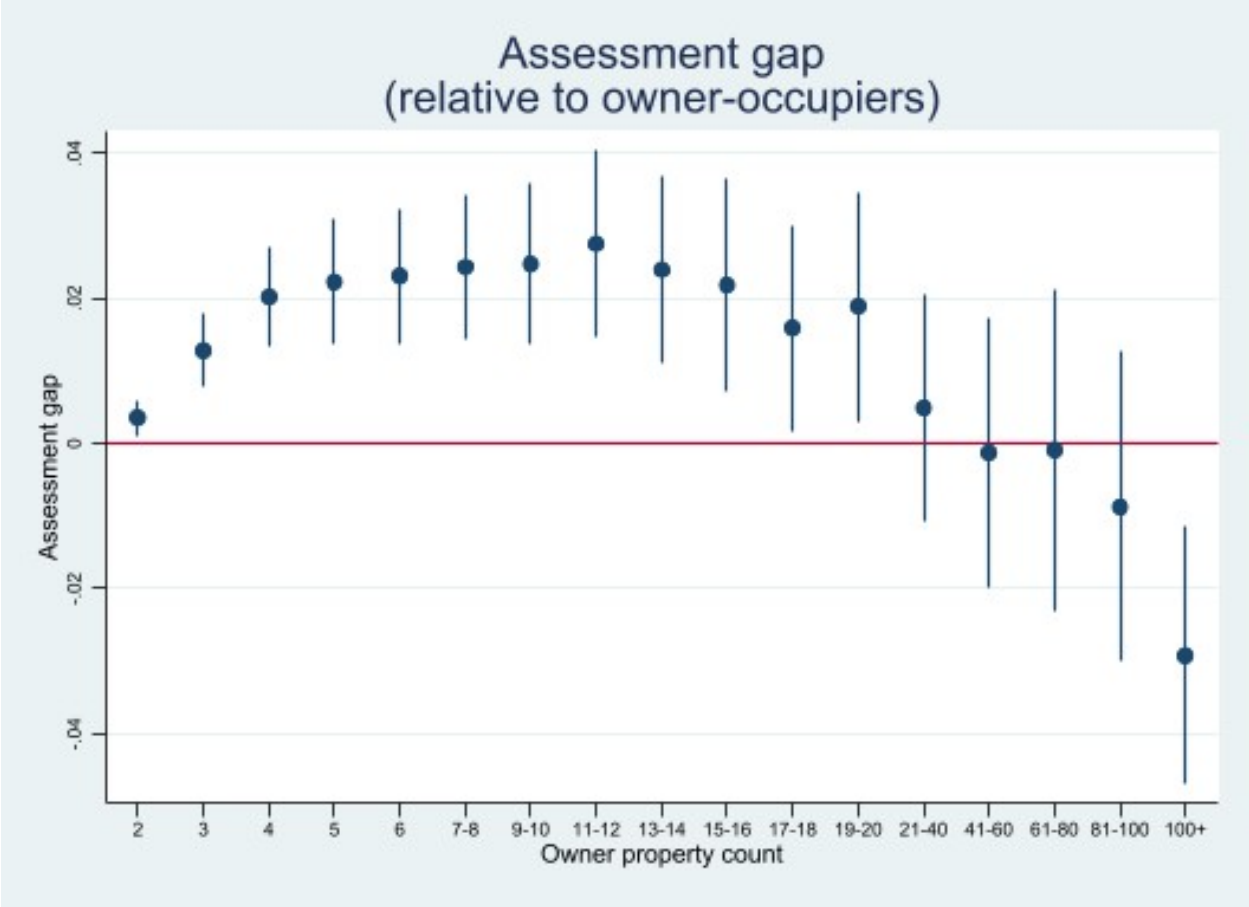

Figure 1.1: Assessment gap by investor scale

The graph plots the estimated coefficients and 95% confidence intervals for investors by their number of property holdings. We include county × year fixed effects in the regressions. The standard errors are clustered by county.

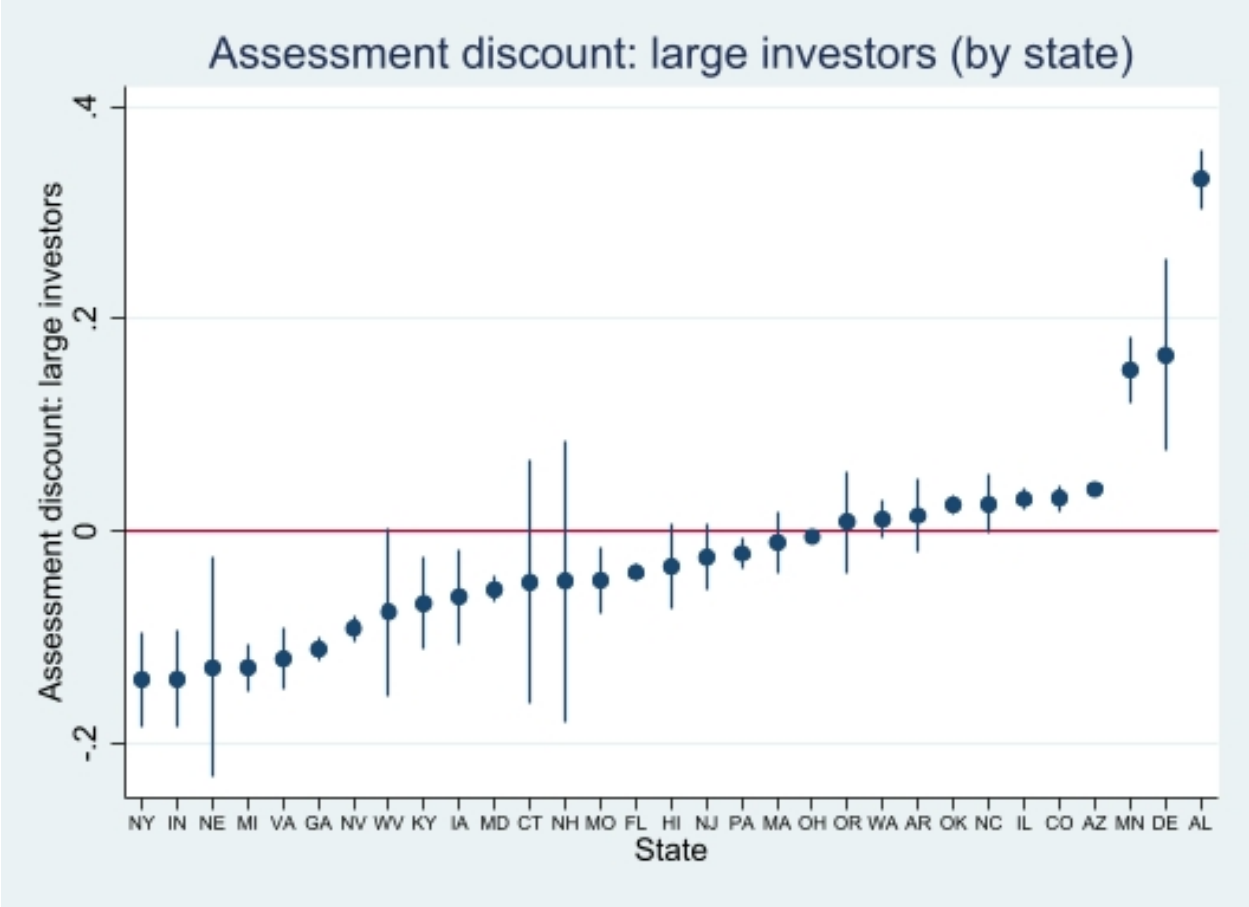

Figure 1.2: Assessment discount for large investors (by state)

The graph plots the estimated coefficient and 95% confidence interval for large investors by state. We include county × year fixed effects in the regressions. The standard errors are clustered by census tract.

Table 1.1: Owner group

This table shows how we classify the property owners based on the number of their property holdings across country.

Owner group	Number of property holdings
Owner-occupiers	1
Micro investor	2–20
Small investor	21–50
Medium investor	51–100
Large investor	100+

Table 1.2: Summary statistics

This table reports summary statistics for the number of observations, the mean assessment ratio, and large investors' local market share percentage for each state.

	Count	Assessment ratio (mean)	Large investors' market share (%)
AL	106,397	0.17	1.48
AR	114,373	0.19	0.25
AZ	453,070	0.10	1.43
CO	670,498	0.08	0.55
CT	32,472	0.69	0.19
DE	70,346	0.29	0.52
FL	2,092,711	0.81	1.40
GA	350,705	0.41	3.90
HI	11,640	0.95	0.27
IA	114,798	0.94	0.22
IL	727,301	0.24	0.63
IN	4,486	0.94	3.50
KY	99,500	0.99	0.60
MA	19,039	0.92	0.43
MD	507,939	0.82	0.62
MI	283,245	0.50	0.61
MN	139,049	0.85	0.39
MO	110,076	0.21	0.93
NC	501,403	0.90	1.21
NE	25,185	0.93	0.42
NH	2,548	0.94	0.43
NJ	379,435	0.71	0.19
NV	545,989	0.29	1.98
NY	325,969	0.72	0.65
OH	346,153	0.34	3.78
OK	175,424	0.11	1.32
OR	364,216	0.70	0.26
PA	503,908	0.48	4.21
VA	81,935	0.92	0.56
WA	423,697	0.87	0.50
WV	7,919	0.54	2.54
Total	9,591,426	0.56	1.33

Table 1.3: Assessment ratio for owner-occupiers versus investors

This table presents our baseline estimates of the assessment gap between owner-occupiers and investors (Model (2.2). The dependent variable is the natural logarithm of the assessment ratio. We classify investors into four groups by their number of properties one year prior to the transaction. *Micro investor (2–20)*, *Small investor (21–50)*, *Medium investor (51–100)*, and *Large investor (100+)* are binary variables that indicate the four groups of investors. The base group is the owner-occupier group. Column 1 includes only the binary variables for the four investor groups. Columns 2 adds additional control variables for property-level characteristics. Columns 3 re-estimates Model (2.2) by adding property fixed effects, and, thus, all time-invariant property characteristics are absorbed by the FEs and not estimated. We report t-statistics using standard errors clustered by county in parentheses. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	ln(assessed value/market value)		
	(1)	(2)	(3)
Micro investor (2-20)	0.008***	0.010***	0.023***
	(4.19)	(4.93)	(6.47)
Small investor (21-50)	0.004	0.004	0.024
	(0.48)	(0.46)	(1.49)
Medium investor (51-100)	-0.004	-0.004	0.022
	(-0.44)	(-0.41)	(1.40)
Large investor (100+)	-0.030***	-0.037***	-0.047***
	(-3.31)	(-3.89)	(-3.01)
Age		-0.001***	
		(-4.82)	
Building area sqft		0.000	
		(0.19)	
Number of bedrooms		0.004***	
		(3.03)	
Number of full bathrooms		0.001	
		(1.13)	
Property FE	No	No	Yes
County × Year FE	Yes	Yes	Yes
Adjusted R^2	0.932	0.931	0.979
Observations	9,591,426	7,683,825	2,806,854

Table 1.4: Cook County assessment ratio and sales price for owner-occupiers versus investors

This table presents estimates from Cook County appeals and transaction data. We classify investors into four groups by their number of property holdings one year prior to the transaction. *Micro investor (2–20)*, *Small investor (21–50)*, *Medium investor (51–100)*, and *Large investor (100+)* are binary variables that equal one if the investor belongs to the corresponding group. In Column 1, the dependent variable is the natural logarithm of initial assessment ratio calculated using the initial proposed assessed value mailed to the property owners as nominator. In Column 2, the dependent variable is the natural logarithm of final assessment ratio calculated using final assessed value after adjustment post-appeal as nominator. In Column 3, the dependent variable is the natural logarithm of sales price for each transaction. We include census tract × year fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

Dependent variable:	ln(Initial assessed value/market value)	ln(Final assessed value/market value)	ln(Sales price)
	(1)	(2)	(3)
Investor(2-20)	0.085***	0.075***	-0.120***
	(23.61)	(19.14)	(-31.20)
Investor(20-50)	0.224***	0.212***	-0.295***
	(11.68)	(10.67)	(-16.02)
Investor(50-100)	0.212***	0.142***	-0.271***
	(7.83)	(4.30)	(-11.47)
Investor(100+)	0.033***	0.004	-0.105***
	(3.97)	(0.47)	(-10.77)
Age	-0.000***	-0.000***	-0.001***
	(-5.49)	(-5.60)	(-8.56)
Building area (sqft)	0.000	-0.000**	0.000***
	(0.33)	(-2.39)	(34.47)
Number of bedrooms	0.002	0.004**	0.025***
	(1.11)	(2.44)	(8.61)
Number of full bathrooms	-0.012***	-0.017***	0.051***
	(-5.61)	(-7.59)	(15.39)
Cesustract × Year FE	Yes	Yes	Yes
Adjusted R^2	0.261	0.267	0.811
Observations	133,896	133,896	133,896

Table 1.5: Summary statistics for Cook County appeals

This table presents summary statistics for the Cook County appeals data. It shows the total number of observations, the total number of appeals filed, the percentage of appeal filings, the total number of successful appeals, the percentage of successful appeal conditional on an appeal filing, and the average percentage of value reduction conditional on a successful appeal.

	Total count	Appeal count	Appeal rate	Successful appeal count	Successful appeal rate	Value reduction %
Owner-occupier (1)	2,906,203	569,868	0.20	395,143	0.69	-13.18
Micro investor (2–20)	1,101,643	278,569	0.25	176,166	0.63	-18.08
Small investor (21–50)	53,367	18,880	0.35	10,678	0.57	-23.95
Medium investor (51–100)	24,872	10,692	0.43	5,698	0.53	-27.01
Large investor (100+)	58,737	19,937	0.34	10,660	0.53	-21.01
Total	4,144,822	897,946	0.22	598,345	0.67	-15.08

Table 1.6: Cook County appeals

This table presents estimates from the Cook County appeals data. We classify investors into four groups by their number of property holdings one year prior to the transaction. *Micro investor (2–20)*, *Small investor (21–50)*, *Medium investor (51–100)*, and *Large investor (100+)* are binary variables that equal one if the investor belongs to the corresponding group. In Column 1, the dependent variable is a binary variable that equals one if the property owner files an appeal unconditionally. In Column 2, the dependent variable is a binary variable that equals one if the property owner wins an appeal conditional on an appeals filing. In Column 3, the dependent variable is value reduction percentage conditional on winning an appeal. Value reduction percentage is calculated as (final adjusted assessed value - initial assessed value)/initial assessed value. We include census tract × year fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

Dependent variable:	Appeal (1)	Successful appeal (2)	Value reduction (3)
Micro investor (2–20)	0.055***	-0.034***	-0.035***
	(28.94)	(-15.15)	(-24.18)
Small investor (21–50)	0.164***	-0.058***	-0.076***
	(18.82)	(-5.73)	(-13.34)
Medium investor (51–100)	0.212***	-0.070***	-0.103***
	(14.45)	(-5.79)	(-13.65)
Large investor (100+)	0.154***	-0.104***	-0.054***
	(18.55)	(-10.94)	(-5.32)
Census tract × Year FE	Yes	Yes	Yes
Adjusted R^2	0.120	0.106	0.159
Observations	4,144,822	897,926	598,345

Table 1.7: Florida appeals

This table presents estimates from the Florida property tax assessment data. We classify investors into four groups by their number of property holdings one year prior to the transaction. *Micro investor (2–20)*, *Small investor (21–50)*, *Medium investor (51–100)*, and *Large investor (100+)* are binary variables that equal one if the investor belongs to the corresponding group. In Column 1, the dependent variable is value reduction percentage conditional on a successful appeal. Value reduction percentage is calculated as (final adjusted assessed value - initial assessed value)/initial assessed value. We include county × year fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

Dependent variable:	Value reduction from appeal
	(1)
Micro investor (2–20)	-0.019***
	(-12.12)
Small investor (21–50)	-0.081***
	(-10.52)
Medium investor (51–100)	-0.109***
	(-9.62)
Large investor (100+)	-0.116***
	(-5.08)
County × Year FE	Yes
Adjusted R^2	0.090
Observations	251,010

Table 1.8: Sales price for owner-occupiers versus investors

This table presents estimates of the sales price gap between owner-occupiers and investors. We classify investors into four groups by their number of property holdings one year prior to the transaction. The dependent variable is the natural logarithm of sales price for each transaction. *Micro investor (2–20)*, *Small investor (21–50)*, *Medium investor (51–100)*, and *Large investor (100+)* are binary variables that equal one if the investor belongs to the corresponding group. Column 1 includes only the binary variables for the four investor groups. Column 2 adds additional control variables for property-level characteristics. We include county × year fixed effects in the regressions. We report t-statistics using standard errors clustered by county in parentheses. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

Dependent variable:	ln(Sales price)	
	(1)	(2)
Micro investor (2–20)	-0.019***	-0.009
	(-3.51)	(-1.34)
Small investor (21–50)	-0.129***	-0.095***
	(-4.96)	(-3.20)
Medium investor (51–100)	-0.151***	-0.125***
	(-5.47)	(-4.48)
Large investor (100+)	-0.179***	-0.136***
	(-12.80)	(-9.86)
Age		-0.003***
		(-6.19)
Building area (sqft)		0.000
		(1.20)
Number of bedrooms		0.055***
		(3.86)
Number of full bathrooms		0.271***
		(12.72)
County × Year FE	Yes	Yes
Adjusted R^2	0.272	0.469
Observations	9,591,426	7,683,825

Table 1.9: Assessment ratio for owner-occupiers versus investors (by state)

This table presents state-level estimates of assessment ratio gap between owner-occupiers and investors. The dependent variable is the natural logarithm of assessment ratio. We classify investors into four groups by their number of property holdings one year prior to the transaction. *Micro investor (2–20)*, *Small investor (21–50)*, *Medium investor (51–100)*, and *Large investor (100+)* are binary variables that equal one if the property owner is an investor that belongs to the corresponding group. The base group is owner-occupier group. We include county × year fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in parentheses. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

	Micro investor (2–20)	Small investor (21–50)	Medium investor (51–100)	Large investor (100+)
AL	0.114***	0.430***	0.242***	0.332***
	(0.01)	(0.05)	(0.03)	(0.01)
AZ	0.020***	0.048***	0.051***	0.039***
	(0.00)	(0.00)	(0.02)	(0.00)
AR	0.009***	0.025**	0.019	0.015
	(0.00)	(0.01)	(0.03)	(0.02)
CO	0.055***	0.159***	0.146***	0.031***
	(0.00)	(0.02)	(0.03)	(0.01)
CT	-0.004	-0.040	0.061	-0.048
	(0.01)	(0.04)	(0.06)	(0.06)
DE	0.051***	0.190***	0.270***	0.166***
	(0.01)	(0.06)	(0.07)	(0.05)
FL	0.000	-0.018***	-0.029***	-0.039***
	(0.00)	(0.00)	(0.01)	(0.00)
GA	-0.001	-0.046***	-0.058***	-0.111***
	(0.00)	(0.01)	(0.02)	(0.01)
HI	-0.005*	-0.048**	-0.065**	-0.034*
	(0.00)	(0.02)	(0.03)	(0.02)
IL	0.016***	0.046***	0.056***	0.030***
	(0.00)	(0.01)	(0.01)	(0.00)
IN	-0.031***	-0.214***	-0.108***	-0.139***
	(0.01)	(0.05)	(0.03)	(0.02)
IA	-0.024***	-0.055***	-0.056**	-0.061***
	(0.00)	(0.02)	(0.03)	(0.02)
KY	-0.018***	-0.064***	-0.124***	-0.068***
	(0.00)	(0.02)	(0.04)	(0.02)

	Micro investor (2–20)	Small investor (21–50)	Medium investor (51–100)	Large investor (100+)
MD	-0.028***	-0.288***	-0.249***	-0.055***
	(0.00)	(0.03)	(0.03)	(0.01)
MA	-0.006**	-0.035*	-0.046	-0.010
	(0.00)	(0.02)	(0.04)	(0.01)
MI	-0.004***	0.019	-0.057***	-0.128***
	(0.00)	(0.02)	(0.02)	(0.01)
MN	-0.013***	-0.054	-0.179**	0.152***
	(0.00)	(0.04)	(0.09)	(0.02)
MO	-0.007	0.105***	0.047	-0.046***
	(0.01)	(0.03)	(0.03)	(0.02)
NE	-0.008***	-0.062**	-0.043	-0.129**
	(0.00)	(0.03)	(0.05)	(0.05)
NV	0.007***	0.018***	0.011	-0.092***
	(0.00)	(0.01)	(0.01)	(0.01)
NH	-0.013	-0.092**	-0.307	-0.047
	(0.01)	(0.04)	(0.24)	(0.07)
NJ	0.015***	0.030	-0.136*	-0.025
	(0.00)	(0.02)	(0.07)	(0.02)
NY	0.006	0.079*	-0.114**	-0.140***
	(0.01)	(0.04)	(0.05)	(0.02)
NC	-0.006***	-0.057***	-0.098***	0.026*
	(0.00)	(0.02)	(0.03)	(0.01)
OH	0.007***	-0.018*	0.005	-0.005*
	(0.00)	(0.01)	(0.01)	(0.00)
OK	0.015***	0.050***	0.057***	0.025***
	(0.00)	(0.01)	(0.01)	(0.00)
OR	-0.007***	-0.104***	-0.067***	0.009
	(0.00)	(0.03)	(0.02)	(0.02)
PA	0.026***	0.006	0.035***	-0.021***
	(0.00)	(0.02)	(0.01)	(0.01)
VA	-0.018***	-0.023	-0.056*	-0.121***
	(0.00)	(0.04)	(0.03)	(0.02)
WA	-0.001	-0.080***	-0.048	0.011
	(0.00)	(0.02)	(0.03)	(0.01)
WV	-0.007	0.061	-0.116	-0.076*
	(0.01)	(0.06)	(0.11)	(0.04)

Table 1.10: Summary statistics for the cross-state analysis

This table presents summary statistics for all state-level variables in the cross-state regression analysis.

	Property tax administration score	Large investor market share(%)	Democratic presidential vote share	Income per capita (000′$)	Property tax burden(%)
AL	25	1.48	0.38	42,411	1.47
AR	19	0.25	0.39	42,528	1.67
AZ	26	1.43	0.45	42,414	2.85
CO	20	0.55	0.51	42,607	2.92
CT	26	0.19	0.58	42,569	4.40
DE	35	0.52	0.59	42,206	1.84
FL	14	1.40	0.50	41,734	3.22
GA	13	3.90	0.46	41,757	2.89
HI	34	0.27	0.70	41,854	2.25
IA	31	0.22	0.52	42,026	3.48
IL	35	0.63	0.58	41,989	4.26
IN	25	3.50	0.44	41,923	2.71
KY	15	0.60	0.40	42,068	1.99
MA	25	0.43	0.61	42,206	3.71
MD	11	0.62	0.62	42,428	2.79
MI	20	0.61	0.54	41,874	3.53
MN	23	0.39	0.53	41,985	3.11
MO	25	0.93	0.46	42,185	2.42
NC	19	1.21	0.48	41,826	2.45
NE	20	0.42	0.38	42,436	3.76
NH	19	0.43	0.52	42,488	5.40
NJ	31	0.19	0.57	42,279	5.35
NV	26	1.98	0.52	42,407	2.72
NY	43	0.65	0.60	42,163	4.60
OH	20	3.78	0.51	41,822	2.98
OK	18	1.32	0.34	41,951	1.57
OR	13	0.26	0.54	42,060	3.35
PA	36	4.21	0.52	42,234	3.00
VA	28	0.56	0.51	43,485	3.00
WA	21	0.50	0.56	43,194	2.75
WV	28	2.54	0.43	42,976	2.37
Total	24	1.16	0.51	42,261	3.06

Table 1.11: Large investor assessment discount: Cross-state analysis

This table presents the cross-state analysis of the assessment discount for large investors. The dependent variable is the t-value of large investors' assessment discount from the state-level results of Model 2.2. *Fair property tax administration score* is a score assigned by the Council on State Taxation (COST) to each state in 2011. Lower score indicates a fairer property tax administration for a given state. *Large investors' local market share* is calculated as the number of houses owned by large investors divided by total number of houses in the Zillow database in each state. *Democratic presidential vote share* is the median share of votes for the Democratic candidate in 2004, 2006 and 2012 presidential elections. *Income per capital* is a time-series average of per capita personal income in thousands during the sample period. *Property tax burden* is a time-series average of property tax dollar amount as a percentage of personal income during the sample period. The standard errors are bootstrapped. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

Dependent variable:	Large investor assessment discount(tvalue)
Property tax administration score	0.393**
	(2.13)
Large investors' local market share (%)	-2.951**
	(-2.03)
Democratic presidential vote share	-35.863**
	(-2.13)
Property tax burden (%)	-1.934*
	(-1.72)
Income per capita ($thousands)	-0.555
	(-0.13)
Adjusted R^2	0.298
Observations	31

Table 1.12: Robustness test of the assessment ratio for owner-occupiers versus investors (block-group)

This table presents the estimates of assessment ratio gap between owner-occupiers and investors (Model (2.2) at census block-group level. The dependent variable is the natural logarithm of assessment ratio. We classify investors into four groups by their number of properties one year prior to the transaction. *Micro investor (2–20)*, *Small investor (21–50)*, *Medium investor (51–100)* and *Large investor (100+)* are binary variables that indicates the four groups of investors. The base group is owner-occupier group. Column 1 includes only the binary variables for the four investor groups. Columns 2 adds additional control variables for property-level characteristics. We include census block-group × year fixed effects in the regressions. We report t-statistics using standard errors clustered by census block-group in parentheses. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

Dependent variable:	ln(Assessed value/market value)	
	(1)	(2)
Micro investor (2-20)	0.012***	0.013***
	(44.16)	(48.82)
Small investor (21-50)	0.015***	0.015***
	(6.59)	(6.44)
Medium investor (51-100)	0.011***	0.012***
	(3.82)	(4.26)
Large investor (100+)	-0.014***	-0.020***
	(-9.82)	(-14.63)
Age		-0.001***
		(-42.18)
Building area sqft		-0.000
		(-0.40)
Number of bedrooms		0.003***
		(13.39)
Number of full bathrooms		0.002***
		(7.31)
Census × block-group	Yes	Yes
Adjusted R^2	0.956	0.956
Observations	9,485,512	7,585,206

Table 1.13: Robustness test of the assessment ratio for owner-occupiers versus investors (ratio)

This table presents the estimates of the assessment ratio gap between owner-occupiers and investors (Model (2.2) using the raw assessment ratio as the dependent variable. We classify investors into four groups by their number of properties one year prior to the transaction. *Micro investor (2–20)*, *Small investor (21–50)*, *Medium investor (51–100)*, and *Large investor (100+)* are binary variables that indicate the four groups of investors. The base group is the owner-occupier group. Column 1 includes only the binary variables for the four investor groups. Columns 2 adds additional control variables for property-level characteristics. We include county × year fixed effects in the regressions. We report t-statistics using standard errors clustered by county in parentheses. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

Dependent variable:	Assessed value/market value	
	(1)	(2)
Micro investor (2-20)	0.003***	0.004***
	(4.39)	(5.91)
Small investor (21-50)	-0.002	-0.001
	(-0.55)	(-0.21)
Medium investor (51-100)	-0.005	-0.004
	(-1.44)	(-1.06)
Large investor (100+)	-0.011***	-0.015***
	(-4.04)	(-5.99)
Age		-0.000***
		(-6.81)
Building area sqft		-0.000
		(-0.16)
Number of bedrooms		0.002***
		(2.74)
Number of full bathrooms		0.001*
		(1.72)
County × Year FE	Yes	Yes
Adjusted R^2	0.888	0.885
Observations	9,591,426	7,683,825

CHAPTER 2

HOUSING STABILITY AND NEW BUSINESS CREATION

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2.1 Introduction

The homeownership rate in the United States has significantly declined since the Great Recession. According to the U.S. Census Bureau, in 2021, 65.5% of U.S. households owned their homes, which is 5.1% lower than its peak in 2004 (see Figure 2.1). A noteworthy trend is that middle and high-income households are becoming some of the fastest-growing groups of renters. A research report by RENTCafé shows that from 2007 to 2017, renter households earning $75,000 to $100,000 grew by 66%, while those earning less than $50,000 decreased by 0.2% (Ciuntu, 2019). Rental market policies have been a growing focus in policy debates across the country, as the COVID-19 pandemic has sparked new concerns about housing affordability and renter vulnerability.[1] Despite this trend, few discussions have focused on the implications of rental market frictions on middle-class renters, who account for an increasing proportion of the renter population and play a vital role in the economy.

This paper examines how housing stability concerns in home rental markets impact the creation of small businesses in local communities. The idea is that, even if renter households are in good standing, they are at risk of displacement due to arbitrary, retaliatory, or discriminatory evictions (Collinson and Reed, 2018, Diamond et al., 2019, Humphries et al., 2019, Pastor et al., 2018). For example, a landlord can refuse to renew a lease or terminate a month-to-month lease for no legitimate reason or offer a renewal with an unreasonable rent increase in order to displace a tenant. Such involuntary moves can severely disrupt households in terms of their financial and social well-being. Renters facing such frictions in home rental markets may lack the risk capacity and horizon to enter into entrepreneurship (Gottlieb et al., 2018, Hombert et al., 2020) and may forego potentially successful business

[1]For example, in recent years many housing advocates have pushed for stronger renter protection policies at the local level, in the hope of scaling these policies up into state legislation. During the pandemic, advocates have helped achieve the enactment of rent control or just cause eviction policies in various municipalities, such as Albany, Beacon, Kingston, Newburgh, and Poughkeepsie in New York; Baltimore, Maryland; and St. Paul, Minnesota.

opportunities. Therefore, local policies that protect renters from unjust evictions may have an unexpected benefit to the local economy by promoting household risk-taking and job creation.

We investigate this question by focusing on home rental markets in California, a state known to have a vibrant economy and relatively stronger legal renter rights. Before the passage of the state-wide just cause eviction law in 2020, renters in California are protected by eviction policies implemented at the local level. We examine heterogeneity in the degree to which renters are protected by municipal policies within the state. Since 1990, three cities in California, including Glendale (in 2002), Oakland (in 2002), and San Diego (in 2004), passed ordinances that limit the grounds upon which a landlord may evict a tenant. Under these ordinances, landlords are permitted to evict a tenant only for "good causes", such as nonpayment of rent and breach of lease provision, that are explicitly stated under the law. Importantly, the expiration of a lease agreement per se is not considered a just cause for eviction. Thus, unless landlords can provide evidence to support one of the just causes specified by these ordinances, they are expected to offer a renewal with similar terms to the renters by default. These ordinances are designed to confer home renters a sense of housing stability that is typically enjoyed by homeowners. Consistent with the intended effect of these ordinances, (Cuellar, 2019) shows that the passage of just cause eviction law in these Californian cities effectively reduces the eviction rate.

The passage of just cause eviction ordinances in these Californian cities allows us to identify exogenous variation in the risk of arbitrary and wrongful eviction for renters. To test our hypothesis, we examine the effect of just cause eviction ordinances on the subsequent creation of local new businesses by performing a difference-in-differences (DID) analysis in a sample of 120 Californian cities from 1999 to 2007. Our results show that the treated cities experienced a significant 9.9% increase in the number of new establishments after the passage of just cause eviction ordinances relative to other cities. This result is entirely

driven by new firms that hire 10 or fewer employees rather than subsidiaries or branches of large corporations, suggesting that enhanced renter housing stability is associated with an increase in small business creation. The increase in the number of new businesses did not occur until two years after the passages, supporting the parallel trend assumption of our empirical design. The post-passage increase in small business creation is manifest in a broad range of industries and is particularly pronounced in industries that are well suited for self-employment, such as transportation, trucking, business management, and freelance authors. Furthermore, legal protection against unjust eviction appears to disproportionately benefit females and racial minorities. Specifically, the number of new firms with female (non-white) owners increased by 14.2% (14.6%) after the passage of just cause eviction law in the treated cities.

We find further evidence which supports the idea that legal renter protection against unjust evictions encourages small business creation by renter households. First, we show that the passage of just cause eviction laws is associated with a higher number of newly issued small business loans but a decrease in the average size of these loans. Hence, there appears to be a higher demand for smaller business loans when there is stronger legal protection against arbitrary and wrongful evictions in place. Second, we find that the increase in the number of new firms is entirely driven by census tracts with renter occupancy rates in the top quartile. Thus, new business creation appears to occur in areas where most renters reside. Third, a higher proportion of new firms in high-renter-occupancy areas use residential addresses after the passage of just cause eviction laws. Hence, legal protection against arbitrary and wrongful evictions appears to have encouraged self-employment by renters who start their businesses at home. These results are consistent with the idea that renter protection policies, rather than other confounding events in the city, drive the increase in small business creation.

We also directly examine the relationship between legal protection from unjust evictions, renter housing stability, and renters' decisions to pursue self-employment using household

survey data that contain information on households' migration status, homeownership status, and employment status. We find that, first, renter households in the treated cities were 25.6% less likely to move (relative to the sample mean) after the passage of just cause eviction laws, consistent with an improvement in housing stability. Second, the likelihood of self-employment by renter households increased by 12.60 percentage points (or doubled from the sample mean) more than homeowners after the law passages. Third, renters in the treated cities had a significantly greater increase in income, particularly in business income, after law passages. Interestingly, both the choice of self-employment and the increase in business income in the survey sample are driven by the non-head household members, who likely generate a secondary source of income for their families. The household-level evidence suggests that improvements in housing stability due to rental market policies allow the non-primary earners of renter households to take on risky entrepreneurial activities and increase household income, while the primary earners can retain their salaried employment and provide the families with a financial safety net.

We address an alternative explanation that might drive our main empirical results. Specifically, there could be unobservable local economic shocks that confound the passage of the renter protection ordinance and also increase the local *demand* for products and services. We address this alternative explanation using three additional tests. First, if the surge in the number of new firms in the treated cities is solely driven by higher local market demand, this demand shock would also lead to an increase in the number of new establishments invested by large companies, an increase in the number of firms moving in from other areas, as well as a decrease in the number of firms moving out of the cities. However, we find no evidence supporting these predictions in the data. Second, local demand shocks are less likely to affect businesses in tradable industries that are not limited by local markets (Mian and Sufi, 2014). However, we find significant increases in the number of new businesses both in tradable and nontradable industries. Therefore, local market demand shocks cannot fully

explain the increase in small business formation in the treated cities. Third, we find no significant changes in the demand for mortgage loans in the treated cities post-passage. This evidence is inconsistent with a local economic shock which possibly drives up the demand for both mortgage loans and small business loans. Though the overall increase in new business creation may not be entirely driven by a confounding local market demand shock, certain industries can still benefit from a higher demand related to stronger legal protection for home renters and their resultant preference for self-employment. Consistent with this idea, working capital financing and apartment locating services are among the top industries that experienced the largest growth in new firm creation.

Do stronger renter rights merely encourage renters to create low-quality businesses, and thus add no real value to the local economy? If this is the case, one would expect the new firms created after the passage of just cause eviction laws to fail soon after creation. Inconsistent with this possibility, we find that the increase in the number of new firms in the treated cities after the passage of the renter protection law remains statistically significant even if we only count firms that eventually survive for up to six years. Furthermore, relative to other new firms, those created after law passages have similar performance in terms of the average sales per employee and better performance in terms of credit score. Thus, stronger renter protection induces more entrepreneurial activities without compromising the average quality of the new firms. However, we also find no association between the passage of the just cause eviction law and the number of new firms that subsequently receive venture capital (VC) financing, become acquisition targets, or go public. Therefore, legal renter protection against arbitrary and wrongful evictions appear to create businesses that generate subsistence incomes but not transformational impacts on the economy.

Our paper contributes to the policy debate among lawmakers, business groups, landlords, and housing advocates on whether to enforce stronger tenant protection laws, a debate that has been sparked by the foreclosure crisis in 2010 and the recent COVID-19 pandemic.

Lawmakers in cities and states that face the challenge of housing affordability issues have for years advocated just cause measures as part of the reform of landlord-tenant law. However, many such initiatives have failed due to conflicting opinions from different parties. For example, Boston failed to pass just cause measures as what the housing advocates called "eviction reform" after three years of continuous efforts in 2018.[2] Parties against home rental market reform commonly argue that these reforms could induce more misbehavior of tenants, and therefore discourage property investments and cause negative impacts on house values. Part of the challenges to this policy debate is the lack of evidence and analysis on the impact of renter protection policies on the local economy. Our evidence shows that strengthening renters' housing stability through policies such as just cause eviction can contribute to local economic growth by encouraging entrepreneurial activities and promoting job creation. Our study therefore aids policymakers in evaluating potential regulations that enhance tenant rights.

The paper proceeds as follows. Section 2.2 discusses our contribution to the literature. Section 2.3 introduces the legal background of local just cause eviction laws in California. Section 2.4 describes the data used in the empirical analysis and discusses the summary statistics. Section 2.5 discusses the empirical design and reports the empirical results. Section 2.6 provides a concluding remark.

2.2 Literature Review

Our study contributes to several strands of literature. First, in the literature of real estate and household finance, a series of studies examine the impact of the housing market on entrepreneurship (e.g., see Adelino et al., 2015, Black et al., 1996, Corradin and Popov, 2015, Fairlie and Krashinsky, 2012, Harding and Rosenthal, 2017, Kerr et al., 2015, Schmalz et al., 2017) For the most part, these studies find that the number of entries into entrepreneurship

[2]Source: https://www.wbur.org/radioboston/2018/05/16/jim-brooks-housing-act-recap

is positively related to local house prices. While most of these studies argue that house prices affect the rates of entrepreneurship through the collateral channel, (Kerr et al., 2015) find limited support for the collateral channel using multiple empirical settings in the U.S. and suggest that local demand for entrepreneurship likely accounts for the empirical relation between local house prices and entrepreneurship. (Jensen et al., 2014) further find that credit supply shocks due to a mortgage reform have a relatively small effect on entrepreneurship. Our study contributes to this line of research by focusing on a different aspect of the housing market that may affect entrepreneurship, namely the regulatory protection for households in home rental markets.

More broadly, our study contributes to the literature on entrepreneurship. Recent theoretical studies formulate the idea that entrepreneurship is a series of experimentations (e.g., Catherine, 2019, Dillon and Stanton, 2017, Kerr et al., 2014, Manso, 2016). This process is risky as entrepreneurs have to explore the unknown and learn about the quality of their business ideas through trial and error. Some studies examine the role of individuals' risk preferences or ambiguity aversions in the choice of entrepreneurship (Ahn, 2010, Hall and Woodward, 2010, Hvide and Panos, 2014, Koudstaal et al., 2016, Moskowitz and Vissing-Jørgensen, 2002). Other studies, such as (Babina, 2020, Barrios et al., 2020, Bellon et al., 2020, Ersahin et al., 2020, Gottlieb et al., 2018, Hacamo and Kleiner, 2020, Hombert et al., 2020), examine changes in the external risk conditions, such as labor policies, employer financial distress, personal wealth shocks, creditor rights, and the rising gig economy, that affect the cost and benefit of experimentation and hence the choice into entrepreneurship. Our study contributes to this literature by showing that rental market policies that insulate renters from the risk of wrongful evictions have the surprising benefit of encouraging entrepreneurship.

Finally, our study contributes to the literature on home rental markets. A number of studies examine the distortionary effect that rent control may have on resource allocation and

landlords' incentives to supply rental housing (e.g., see Ahern and Giacoletti, 2022, Asquith, 2019, Autor et al., 2014, Bulow and Klemperer, 2012, Diamond et al., 2019, Favilukis et al., 2019).[3] To the best of our knowledge, our study is the first to examine the impact of renter rights in the form of eviction policies on local economic activities. Some studies have examined the direct effect of eviction on renters' welfare, such as the likelihood of becoming homeless (Collinson and Reed, 2018), children's school performance (Diamond et al., 2019), and financial distress (Humphries et al., 2019), among others. Different from these studies which investigate the ex-post consequences of evictions, our study focuses on the effect of the lower ex-ante likelihood of eviction due to local regulations.

2.3 Legal Background of Just Cause Eviction

"Just cause" or "good cause" eviction laws promote stability for tenants by limiting grounds upon which a landlord can evict tenants. The law protects tenants from eviction for improper reasons. For example, without legal protection against unjust evictions, a landlord can refuse to renew a lease contract for a retaliatory purpose, such as to evict renters that complain about home condition, or with discriminatory intent, such as to evict racial or sexual minorities. Common reasons outlined in the laws that can lead to a just cause eviction can be classified into two types: one applicable to at-fault tenants and the other applicable to no-fault tenants. Examples of legal reasons to evict an at-fault tenant are non-payment of rent, illegal use of the property, and substantial damage to the property. Many of these provisions overlap with typical lease terms that permit landlords to terminate a tenancy. To evict a no-fault tenant, typical good causes include the tenant's refusal to sign an identical new lease, the landlord's move-in, and the landlord's recovery of possession under certain laws.

[3]For earlier studies, see (Early, 2000, Glaeser and Luttmer, 2003, Gyourko and Linneman, 1989, Moon and Stotsky, 1993, Olsen, 1972, Sims, 2007).

It is important to note that the expiration of a rental agreement per se is *not* considered a legal reason to evict a no-fault tenant at the end of a lease term under the enforcement of just cause eviction law. In a city without the just cause eviction law, landlords can usually evict a tenant by sending out a notice to vacate 30 or 60 days prior to the lease end to inform tenants of the termination of tenancy. Just cause eviction law substantially increases the difficulty for landlords to evict a tenant at the end of a lease term. First, landlords need to make sure the reason to vacate a tenant is on the list of just causes. Second, landlords need to provide supporting evidence to prove the good cause of the termination action. For example, a report submitted to the government of the city of Fremont, CA that evaluates the program of just cause eviction states that "One of the primary impacts of these programs is that they shift the burden of proof for a tenant eviction from the tenant to the landlord because failure to prove one of the allowable causes for eviction is an affirmative defense a tenant may use to contest the eviction."[4] In the absence of compelling evidence to justify one of the just causes listed in the law, landlords are generally obligated to renew the lease on similar terms for their tenants. As a result, just cause eviction laws aims to provide tenants with a level of stability similar to what homeowners typically enjoy.

In California, some cities had adopted just cause ordinances since the late 1970s. In some cases, just cause measures were passed as part of the rent stabilization ordinance at the city level. We collect information on renter protection measures for each city in California from the Rent Control Chart For California and verify the information on the website of each municipality (Rosenquest and Portman, 2019). Since 1990, three cities in California, including Glendale (in 2002), Oakland (in 2002), and San Diego (in 2004), passed a standalone ordinance on just cause eviction. One of the treated cities, Oakland, had a rent stabilization program in place two decades before the passage of its just cause

[4]Source:https://www.fremont.gov/home/showpublisheddocument/459/637748121565600000

eviction ordinance. The rent stabilization policy in Oakland caps the annual rent increase at the CPI. A pitfall of rent stabilization policy is that it creates incentives for landlords to arbitrarily evict existing tenants in order to circumvent the rent cap and mark the rent to market on new tenants. This leads to a higher involuntary tenant turnover initiated by landlords, harming housing stability for tenants. The adoption of just cause eviction law on top of a rent stabilization policy can largely alleviate this concern because tenants are protected by just cause clauses at the end of the lease term from arbitrary evictions by landlords who want to mark the rent to market. Thus, the just cause eviction ordinance adds an additional layer of protection to tenants.

In the Internet Appendix, we provide further details on the just cause eviction laws passed in these three cities. Table A.1 shows the list of legal reasons for just cause evictions and marks the ones that apply to each of the three treated cities. Table A.2 includes the specific ordinance that is passed in each city and the year of passage.[5]

Statewide just cause eviction laws are currently implemented in New Jersey (1974), New Hampshire (2015), Oregon (2019), California (2020), and Washington (2021). Except New Jersey, most of the statewide laws were passed in recent years in response to the enhanced public awareness of housing affordability and renter vulnerability issues. Other than these five states, just cause eviction law is mostly enforced at the municipality level. During the recent COVID-19 pandemic, the vulnerability of tenants becomes a growing issue. Various tenant activist groups and local policymakers have advocated just cause eviction protections

[5]Two other cities passed amendments to their existing just cause eviction ordinances in more recent years. East Palo Alto adopted Rent Stabilization and Just Cause for Eviction ordinance for the first time in 1988, with an exemption for units built after 1988 and single-family dwellings. In 2010, the city passed an amendment to the old ordinance to extend the coverage to almost all rental units that were previously exempt (Source:https://www.ci.east-palo-alto.ca.us/index.aspx?NID=591). Similarly, in 2010, Santa Monica amended 1979 just cause eviction law to enforce renter protection on all rental units. Before this amendment, only units built before 1979 were covered by the just cause eviction law in Santa Monica (Source:https://www.tobenerlaw.com/city-of-santa-monica-renter-protections/).

at both state and local levels. Seven cities, including five in New York state, have passed just cause eviction laws since 2021. [6]

2.4 Data

2.4.1 Data source

We obtain data on U.S. establishments from the National Establishment Time-Series Database (NETS). The NETS database provides detailed information on the physical location, the industry of operation, sales figures, the number of employees, and family trees of each establishment from 1991 to 2017. Also, the database tracks each establishment from birth to exit, including relocation. We conduct the analysis at the census-designated place (i.e., city) level. In order to map each establishment to a census-designated place, we use the longitude and latitude of each establishment provided by NETS and map them to each census-designated place using the place-level shape file for California from the Census Bureau. We discuss each data source in detail below.

We obtain eviction data in California from Eviction Lab at Princeton University. This database contains cleaned, geocoded, and aggregated records of court-ordered evictions that occurred between 2000 and 2016. Eviction Lab acquires the data from states and counties and purchases records from two independent data acquisition companies: LexisNexis Risk Solutions and American Information Research Services. The data on eviction contains aggregated eviction rate, eviction filing rate, number of evictions, and number of eviction filings, among others. Our analyses use data on eviction rates aggregated at the census-designated place (i.e., city) level. The eviction rate is defined as the percentage of renter-occupied households in an area that received an eviction judgment. Eviction judgment is the court

[6]Source:https://prismreports.org/2022/08/19/cities-experiment-eviction-prevention/

ruling that requires the renters to vacate the premises they are renting.[7] If a single address receives multiple judgments in a year, only one eviction record is counted. The eviction filing rate is defined as the percentage of renter-occupied households in an area that received an eviction filing. The total number of renter-occupied households is based on estimates from the 2000 and 2010 U.S. Censuses and ESRI Business Analyst 2016 Data Update. One concern about the eviction data in California is that some eviction cases are sealed, and are thus not available to the public. This restriction is likely to cause an underestimation of eviction rates. For this reason, all the cities located in the same counties as the treated cities are marked with "low" flags in the database, indicating that the estimates are lower than the actual. Given that our analyses focus on within-state variations, unless we assume this measurement error correlates with the treatment, this measurement error should not bias our estimates. The Eviction Lab also provides annual city-level figures on the percentage of renter-occupied homes calculated from the data available in Census Summary File and Five-year American Community Survey (ACS).

We also collect data on small business loans provided through the United States Small Business Administration (SBA) 7(a) lending program from ProPublica.[8] Using the information on borrowers' addresses from the small business loan data, we aggregate the loans at the city level and count the number of approved loans and calculate the total loan amount for each city-year. We also collect loan-level data on home mortgage applications from the database under the Home Mortgage Disclosure Act (HMDA) and match each home mortgage loan application to the corresponding city and year.

For further analysis of the outcome of new businesses, we obtain data on VC funding from VentureXpert provided by Thomson Financial. This database provides data on VC deals,

[7]Source:https://evictionlab.org/

[8]Source:https://www.propublica.org/datastore/dataset/sba-7a-business-loans

including the date of funding rounds and the funding amount. Detailed information on the companies that receive VC funding, such as company name and address, is also available. We match each company in the VentureXpert database with an establishment in NETS based on name, county, and zip code.

Lastly, to provide more direct evidence on the relationship between legal protection from unjust evictions, renter housing stability, and renters' decision to pursue self-employment, we also examine household-level data from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS, Flood et al., 2022). The CPS data are used to produce the official annual estimates of poverty and other socioeconomic and demographic characteristics such as income, health insurance coverage, school enrollment, marital status, and family structure. We will describe the data in more detail in Section 2.5.3.

Our final sample includes 120 census-designated places from 1999 to 2007 (i.e., three years before the first law passage in 2002 and three years after the last law passage in 2004). This sample covers cities listed by the California State Association of Counties in the three Californian counties where the treated cities are located. Our analysis focuses on the sample consisting of only new *standalone* establishments. We define the ownership status of each establishment using the information on parent firms from NETS. Specifically, we classify an establishment as a standalone establishment if the parent firm ID is missing in the year of inception, or if the parent firm ID is the same as the ID of the establishment in the year of inception.

We consider California a relevant and important sample to study the impact of renter protection law on entrepreneurial activities. First, California has the largest population and economy in the U.S., representing 14% of the national GDP and ranks as the fifth largest economy in the world. Second, the state has experienced a proliferation of startups in the past few decades. A culture of innovation, the presence of Silicon Valley, and an efficient legal

system that safeguards business interests, among others, have made California attractive to entrepreneurs and VC capital.

Panel A of Table 2.1 reports summary statistics for the treated and control cities in the full sample. The statistics show that the three treated cities on average have significantly more new standalone establishments per year than the average control city. This is because the control groups include many smaller cities. The standard deviation of the number of new standalone establishments is 3,037 for the control group, suggesting that the control group also includes some larger cities that are on the same scale as the treated cities. In both the treated and control groups, females and racial minorities account for less than 10% of all new business owners. Hence, these groups are under-represented among entrepreneurs. Around 36-37% of new firms use residential addresses. These firms are likely run by entrepreneurs who start their businesses at home. This is consistent with the statistics from the Census Bureau that more than half of businesses are operated primarily from someone's home. [9]

Panel B of Table 2.1 reports summary statistics for each treated city and the corresponding control cities in the same county. All three treated cities have a larger number of new firms per year compared with their counterparts in the control group. Oakland and San Diego are the largest cities in their counties in terms of the number of new firms. In Los Angeles county, there are multiple cities, such as Los Angeles and Long Beach, that have a comparable or higher number of new firms per year than Glendale. With the presence of county×year fixed effects in the DiD models, our estimates capture only the within-county-year variation in the number of new firms across the passage of just cause eviction ordinances. As such, it is important to benchmark the characteristics of the treated cities against those of the control cities in the same county to have a meaningful comparison. We do not include

[9]Source:https://www.census.gov/newsroom/releases/archives/business$_ownership/cb11-110.html$

counties other than the ones where the treated cities are located in our main analysis because they provide no incremental information to the within-county-year estimates.[10]

2.5 Empirical Design and Results

2.5.1 Just cause eviction law and eviction rate

The just cause eviction ordinance enforces stronger renter protection by limiting landlords' rights to evict except for certain legal reasons. One of our identifying assumptions is that the enforcement of just cause eviction reduces eviction risk ex-ante. We test this assumption by using data on the eviction rate at the city level provided by the Eviction Lab, and examining whether the passage of just cause eviction law reduces the eviction rate ex-post. Specifically, we estimate the following difference-in-differences (DiD) models around the passages of just cause eviction ordinances:

$$ln(1+Eviction\ rate)_{n,t} = \alpha + \beta Post_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,t}. \tag{2.1}$$

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The eviction rate is defined as the percentage of renter-occupied households that receive a final eviction judgment in an area.[11] We also use the eviction filing rate, defined as the percentage of renter-occupied households that receive an eviction filing in an area, as an alternative dependent variable. $Post_{n,t}$ is a binary variable that equals one for city-year observations of the three treated cities after the passage of just cause eviction ordinances.

[10]In Table A.4 in the Appendix, we show that the estimates remain the same after including other Californian counties or using only large cities or matched cities selected from all Californian counties as the control group.

[11]We take a log transformation of the eviction rate when using it as a dependent variable to avoid the extreme values (skewness=12.4 because log transformation) from driving the estimate and add one to the variable before the log transformation because some observations have zero eviction rate.

We control for time-varying local characteristics at the county level using county×year fixed effects ($\gamma_{c,t}$) and time-invariant city characteristics using city fixed effects (θ_n). In Table 2.2, we show that the eviction rate (eviction filing rate) declined by 7.1% (6.1%) after the passage of just cause eviction law in the treated cities.[12] The estimate is statistically significant for the eviction rate but not the eviction filing rate. Thus, just cause eviction has likely raised the barrier to a successful eviction filing. These results are consistent with the findings by (Cuellar, 2019) and support the underlying assumption that just cause eviction reduces eviction risk for renter households.[13] In Section 2.5.3, we will conduct further analysis using household survey data to examine the effect of just cause eviction laws on renter housing stability.

2.5.2 Just cause eviction law and small business creation

2.5.2.1 Baseline results

In this subsection, we examine the effect of just cause eviction law on new business creation. We estimate the following DiD models around the passages of just cause eviction ordinances:

$$ln(1+Number\ of\ new\ establishments)_{n,t} = \alpha + \beta Post_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,t}. \tag{2.2}$$

The dependent variable is the natural logarithm of one plus the number of new establishments in city n and year t.[14] $Post_{n,t}$ is a binary variable that equals one for observations

[12] $e^{(0.069)} - 1 = 7.1\%$; $e^{(0.059)} - 1 = 6.1\%$

[13] One caveat with this test is that the eviction data starts in 2000, which is two years before the passage of just cause eviction laws in Oakland and Glendale. Another issue with the eviction data is that the number of eviction in the first year is unusually higher than in the subsequent years. This is likely because the first-year data include historical eviction cases before the sample period.

[14] We take a log transformation of the establishment count when using it as a dependent variable to avoid the extreme values (skewness=10.6 because log transformation) from driving the estimate and add one to

of the three treated cities after the passage of just cause eviction ordinances. We control for time-varying local characteristics at the county level using county×year fixed effects ($\gamma_{c,t}$) and time-invariant city characteristics using city fixed effects (θ_n). In the presence of county×year fixed effects, the DiD estimator only captures within–county variation in new business creation. Hence, we only include the three counties where the treated cities are located in the sample because counties outside of the treated group provide no incremental information to the within-county-year estimates.[15]

The exogeneity assumption for our identification strategy is that the passages of just cause eviction ordinances are exogenous to the local economic conditions and/or any unobservable factors that are related to local residents' propensity to enter into entrepreneurship. To strengthen this underlying assumption, we include county-year fixed effects to account for any unobservable time-varying social and economic conditions at the county level. To the extent that the social and economic conditions are shared with neighboring cities in the same county, these time-varying factors are accounted for by the county-year fixed effects. Despite the inclusion of high-dimensional fixed effects, one can still argue that the passages of just cause eviction ordinances can be the result of a worsening economic condition at a more local level which heightens the risk of eviction for local renter households. If that is the case, one would expect a negative correlation between the passage of just cause eviction ordinance and the rate of entrepreneurship. Such a negative correlation is opposite to our empirical prediction.

The exclusion assumption for our empirical setting is that the passages of just cause eviction ordinances increase the rate of local entrepreneurship only by enhancing renters'

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the variable before the log transformation because some city-year observations have zero new establishments. Nevertheless, the estimates are robust using a Poisson regression instead of a linear regression with a log-transformed dependent variable (see Table A.4).

[15]In Table A.4 in the Appendix, we show that our results are robust to including all Californian counties or using only large cities from all Californian counties as the control group.

housing stability and thus their willingness to enter into entrepreneurship. However, a possibility that may violate the exclusion assumption is that the passages of just cause eviction ordinances may induce other outcomes, such as an influx of population, that drive up the local market demand for (as opposed to renters' supply of) goods and services. In Section 2.5.4, we perform various tests to investigate this possibility.

Table 2.3 shows the estimates of Model (2.2) for the sample period from 1999 to 2007. Column 1 shows the estimates for all new establishments. The estimates show that the total number of all new establishments increases by 9.9% after the passage of just cause eviction ordinances in the treated cities.[16] In Columns (2) and (7), we divide the new establishments into standalone establishments and subsidiary establishments, defined as those with an existing parent firm. The estimates in Column 2 show that there is a significant 9.9% increase in the number of new standalone businesses after the passages of just cause eviction laws. The magnitude of this increase is economically meaningful: it translates into 478 additional new businesses and 1,227 additional employments per year for the average treated city.[17] In contrast, Column 7 of Table 2.3 shows no significant relationship between the passage of just cause eviction law and the subsequent number of new subsidiary establishments. Hence, the increase in the number of new establishments is driven by the creation of new firms rather than new investments made by existing firms. We perform the rest of the analysis based on only new standalone establishments to measure the rate of new firm creation.

In Columns (3) to (6), we break down the new standalone establishments into four size groups based on the number of employees in the year of inception as reported in NETS. The

[16] $e^{(0.094)} - 1 = 9.9\%$

[17] The average treated city has 4,852 new establishments per year which create 14,781 employments per year. In an unreported estimate, we find that the number of new employments created by the new firms increased by 8.3% in the treated cities post-passage. Hence, the passage of just cause eviction law is related to an increase in the number of firms by $4,852 \times (e^{(0.094)} - 1) = 478$, and an increase in the number of new employment by 14,781 $\times$ 8.3% = 1,227.

estimates show that only the number of small firms (i.e., those with 10 employees or less at inception) increased in the treated cities after the passage of just cause eviction laws. Specifically, the number of firms with 1-5 employees increased by 9.4% and the number of firms with 6-10 employees increased by 12.6%. By contrast, the number of large establishments with 20 employees or more sees no significant change after the passage of just cause eviction laws in the treated cities. Hence, the enforcement of legal protection against wrongful eviction is associated with the creation of new small businesses.

Figure 2.2 presents the differences in the number of new standalone establishments between the treated and control cities around the passage of just cause eviction law. The figure confirms the parallel–trend assumption, in that the trend in the number of new firms for the treated cities does not diverge from that of the control cities until two years after the passage. Collectively, the results in Table 2.3 and Figure 2.2 suggest that enhancing renter protection is associated with an increase in small business creation.

2.5.2.2 Robustness

We also perform a number of robustness tests for our baseline results and we report the results in the Appendix. First, one may be concerned that the treated cities are fundamentally different from the other cities. To mitigate this concern, we perform the DiD analysis using census tracts located around the borders of the treated cities as control group. The assumption is that census tracts located on the two sides of the city borders should have similar social and economic characteristics, except that one side is located within the jurisdiction of the treated cities while the other side is not. Figure A.1 shows the census tracts around the city borders that are used in this analysis. In Table A.3, we re-estimate Model (2.2) using census tracts located around the borders of the treated cities. The results show a significant increase in the number of new establishments in the treated census tracts relative to those on

the other side of the city borders. This result further strengthens our causal interpretation of the relationship between renter protection on small business creation.

The second concern is that a spurious correlation between the enforcement of just cause eviction and small business creation can arise if our results are entirely driven by one city which happens to be confounded by other unobservable factors. We address this concern by decomposing the post-passage dummy into three dummies that indicate treatments for each of the three cities. In Panel A of Table A.4, we re-estimate Model (2.2) using three separate DiD estimators for each treated city. The results show that all three cities experienced a significant increase in the number of new small establishments after the passage of a just cause eviction law. These results further strengthen the causal effect of just cause eviction laws on new firm creations: while the passage of just cause eviction laws might be confounded by other events that occurred at the same time, to the extent that these confounding factors are idiosyncratic across the treated cities, they are unlikely to drive the number of new firms in the same direction.

In Panel B of Table A.4, we show that the baseline result is robust to alternative samples and model specifications. In Columns 4 and 5, we show that our baseline finding is robust if we estimate the model using negative binomial regression and Poisson regression. In Column 1, the DiD estimator of Model (2.2) remains significantly positive after including other Californian cities outside of the treated counties in the sample. In Column 2, we show a similarly strong estimate of the post-passage increase in the number of new businesses in the treated cities if we use only large cities (i.e. those with equal or greater number of new businesses than the treaties cities as of the beginning of the sample period) from all Californian counties as the control group.

In Column 3, we show that the main result holds in a matched sample based on propensity score matching. Using all Californian cities, we estimate the propensity score based on *ln(1+New standalone establishments)* in 2000 and the following city characteristics reported

by the 2000 Census: the natural logarithm of the city population (*ln(Population)*), poverty rate, the proportion of rent households (*%Renter households*), the natural logarithm of median household income (*ln(Income)*), the natural logarithm of the median property value (*ln(Property value)*), and the natural logarithm of median gross rent (*ln(Rent)*). We then match the three treated cities with ten control cities based on the propensity score. The matched control cities include Berkeley, Chula Vista, El Cajon, Goleta, Long Beach, San Francisco, San Jose, Santa Barbara, Santa Monica, and West Hollywood.[18] The estimate in Column 3 (0.097) is statistically significant and the economic magnitude is close to our baseline estimate using all other cities in the three treated counties as the control group (0.094 in Column 2 of Table 2.3). Thus, our main result is not sensitive to the choice of sample and estimation method.

Another concern is that even though the passages of just cause eviction laws are not related to an increase in the number of new large businesses, it is still possible that these law passages are related to greater employment contributed by the large businesses. To consider this possibility, we re-estimate Model (2.2) using the total number of employees hired by the newly created businesses as the dependent variable. The estimates in Table A.6 show that only new businesses that hire ten or fewer people see a significant increase in the total number of employment post-law passage. Specifically, employment by new firms that hire 1-5 employees and 6-10 employees increased by 7.5% and 16.8%. Hence, just cause eviction laws appear to have promoted job creation mainly through small businesses.

Finally, one may be concerned about the confounding effect of omitted variables. We choose not to include city-level control variables for two reasons. First, most city-level economic characteristics could be "bad controls" that are themselves outcome variables with respect to the law passages we examine (Angrist and Pischke, 2009). Second, city-level

[18]Table A.5 shows that the differences in the city characteristics between the treated and control groups are statistically insignificant after matching.

social and economic variables provided by the Census Bureau are estimated over five-year windows. Thus, it is difficult to use these data to precisely control for the pre-determined level of city characteristics that are not affected by the law passage. Instead of including city characteristics as control variables, we estimate DiD models around the passages of just cause eviction law using several city-level characteristics as the dependent variables to examine whether our main findings are confounded by other changes in the city. Table A.7 presents these estimates for three dependent variables: *ln(Population)*, *ln(Median property value)*, *ln(Median household income)*, and *Poverty rate*. The sample includes two snapshots of these city-level economic characteristics based on the 2000 Census and the 5-year estimates (2005-2009) from the American Community Survey. The estimates in Columns 1-3 show no differential trends in city population, median property value, or median household income post-law passage for the treated cities relative to other cities. Thus, it is unlikely that the post-passage increase in new business creation is driven by these confounding city factors. By contrast, Column 4 shows a significant decline in the poverty rate post-law passage for the treated cities relative to the control cities. This result suggests that legal renter protection from unjust evictions might have helped improve the economic condition for households on the left tail of wealth distribution.

2.5.2.3 Top industries in new firm creation

Table 2.4 presents the top 25 industries that experienced the strongest growth in the number of new firms after the passage of just cause eviction law in the treated cities. The NETS database provides the 8-digit SIC industry classification for each establishments, allowing us to precisely identify the type of businesses that saw growth after the enforcement of a just cause eviction law. We estimate Model (2.2) using the number of new small businesses (i.e., 5 employees or fewer) in each 8-digit SIC industry as the dependent variable. Most of the top 25 industries are well suited for self-employment, such as transportation service

(SIC 47890000), trucking (SIC 42120000), business management (SIC 87410100), mortgage broker (SIC 62119901), freelance authors and songwriters (included in SIC 89990000), among others.[19]

Interestingly, apartment locating service (SIC 72999901) ranks 21st in new firm creation, consistent with a higher demand for home rentals when legal renter rights are strengthened. Furthermore, working capital financing and personal credit institutions rank 2nd and 24th in new firm creation, consistent with a higher demand for business credit when more households pursue self-employment while not having to take up a mortgage to become a home owner. Thus, while our results are broadly consistent with local residents being more willing to pursue self-employment and therefore increasing the supply of new small businesses, the increases in some types of businesses are also consistent with a higher demand conferred by stronger legal protection in rental housing markets.

Looking across industries more broadly, we find that 40 out of 82 2-digit SIC industries have a statistically significant increase while only 4 have a significant decline in the number of new firms in the treated cities post-passage. Thus, the positive relationship between legal protection against wrongful eviction and new firm creation is manifest in a broad range of industries instead of being driven by outliers.

2.5.2.4 New firms owned by females and racial minorities

Unjust eviction could be a greater risk factor for females and racial minorities. For example, (Desmond et al., 2015) show that, after an eviction, female and black renters are more likely to have long-term housing problems such as broken appliances, exposed wires, or no heat. As our data show (see Table 2.1), females and racial minorities are also underrepresented among business owners. As such, legal protection against unjust evictions may be more important

[19]See, for example, the official statistics by the U.S. Bureau of Labor Statistics on self-employment rates by industry. Source:https://www.bls.gov/spotlight/2016/self-employment-in-the-united-states/home.htm

for these renter groups when it comes to encouraging risk taking and self-employment. To test this hypothesis, in Table 2.5 we examine the impact of just cause eviction laws on the number and proportion of new firms owned by females and racial minorities. Estimates in Column 1 (3) of Table 2.5 show that the number of new firms with female (non-white) owners increased by 14.2% (14.6%) after the passage of just cause eviction law in the treated cities. These effects are larger in magnitude than that in the baseline estimates (9.9% in Column 2, Table 2.3), suggesting that legal protection against wrongful evictions has a greater effect on encouraging entrepreneurship among females and racial minorities. Consistent with this observation, the estimates in Columns 2 (4) show that the proportion of new firms created by females (non-whites) increased by 8.0% (12.1%). These results suggest that legal renter protection against wrongful evictions not only encourages small business creation but also mitigates gender and racial inequality in entrepreneurship.

2.5.2.5 Demand for small business loans

Middle-class households may have to choose between owning a home or starting a small business due to their limited credit capacity. If the rental market lacks protection against wrongful evictions, households may be forced to purchase a home to settle down in a neighborhood. By providing stronger legal renter protection, households can remain stable as renters and utilize their credit to pursue self-employment, rather than being compelled towards homeownership. We, therefore, conjecture that the passage of a just cause eviction law will increase the demand for small business loans.

To test this possibility, we examine the relationship between the passage of just cause eviction law and the number and size of approved SBA loans in the cities. In Column 1 of Table 2.6, the dependent variable is the natural logarithm of one plus the number of newly approved SBA loans for a city-year. The estimates show that the number of new SBA loans increased by 19.7% after the passage of just cause eviction laws in the treated cities. In

Columns (2) and (3), when we examine the post-passage change in the total and average SBA loan amount, we find that while the total dollar value of the SBA loan did not change significantly, the average loan size decreased by 7.7%. These results suggest that, after the passage of just cause eviction laws, there is a higher demand for smaller loans, consistent with the demand by renter households that seek to start small businesses.

2.5.2.6 Are renters the new business owners?

A key challenge to our study is to show that the increase in new businesses is indeed driven by renters becoming business owners. While we are not able to directly observe the home-ownership status of each business owner, we perform several additional tests to address this challenge. First, we explore the relationship between just cause eviction laws and the number of new firms in different areas of the treated cities based on the proportion of renter-occupied homes. The idea is that renters are more likely to start their businesses in renter-occupied areas. We sort census tracts in California into quartiles each year during our sample period based on renter occupancy rate. Quartile 1 is the bottom quartile of census tracts with the lowest percentage of renter-occupied homes, i.e., the highest homeownership rate. We then count the number of new firms separately in each quartile. The dependent variable is the natural logarithm of one plus the number of new firms in each area of a city. The estimates in Table 2.7 show that only areas in the top quartile of renter occupancy rate experienced a significant increase in the number of new firms. These are new firms in areas with the highest proportion of renter households in the state. We interpret these results as consistent with renter business owners driving the increase in new firms.

Second, we look into new firms with residential addresses. The idea is that a new firm operated in a residential property in a low- (high-)homeownership area is more likely to be owned by a renter (homeowner) who runs her business at home. Column 1 of Panel A, Table 2.8 shows that the number of new firms with residential addresses increased by 14.0%

after the passage of just cause eviction law in the treated cities. However, this increase is almost entirely driven by new firms in areas with the highest proportion of renter households. Specifically, the number of new firms using residential addresses in areas with the top-quartile level of renter occupancy rate increased by 30.6% after the passage of just cause eviction laws. This effect is significantly larger than the effect of the law passages on the number of all new firms (15.0% in Column 4, Table 2.7). In Panel B of Table 2.8, we examine changes in the relative proportion of new firms that use residential addresses. The estimates show that in areas with the top-quartile level of renter occupancy rate, the proportion of new firms using residential addresses increased by 12.9% after the passage of just cause eviction laws. The results in Tables 2.7 and 2.8 collectively suggest that the positive effect of legal protection against wrongful eviction on entrepreneurship is likely driven by renters who start their own businesses at homes.

2.5.3 Household-level evidence

To provide more direct evidence on the relationship between legal protection from unjust evictions, renter housing stability, and renters' decision to pursue self-employment, we examine household-level survey data from CPS. The survey collects information on the homeownership status of the households as well as the migration status and the employment status of each adult household member. This information allows us to examine the relationship between just cause eviction law and renter households' housing stability and their decision to become self-employed. The sample for our analysis consists of 25,419 adult individuals from 12,251 households in California that have non-missing information on age, gender, race, educational attainment, income, homeownership status, and employment status from 1999 to 2007. Table 2.9 reports the summary statistics of the household survey data. We estimate the following models to examine the post-passage changes in housing stability, employment

choice, and personal and household income in the treated cities:

$$logit(Pr(MigrationStatus)_{i,h,c,t}) = \alpha + \beta_1 Post_{c,t} \times Renter_{h,t} + \beta_2 Post_{c,t} + \beta_3 Renter_{h,t} + \gamma' X_{i,h,c,t} + \phi_h + \theta_t + \epsilon_{i,h,c,t}. \quad (2.3)$$

$$logit(Pr(EmploymentStatus)_{i,h,c,t}) = \alpha + \beta_1 Post_{c,t} \times Renter_{h,t} + \beta_2 Post_{c,t} + \beta_3 Renter_{h,t} + \gamma' X_{i,h,c,t} + \phi_h + \theta_t + \epsilon_{i,h,c,t}. \quad (2.4)$$

$$ln(Income_{i,h,c,t}) = \alpha + \beta_1 Post_{c,t} \times Renter_{h,t} + \beta_2 Post_{c,t} + \beta_3 Renter_{h,t} + \gamma' X_{i,h,c,t} + \phi_h + \theta_t + \epsilon_{i,h,c,t}. \quad (2.5)$$

Equation (2.3) is a conditional logit model where the dependent variable is a binary variable indicating the migration status of person i of household h in metro area c and year t. We estimate this model separately for three types of migration status: (1) has not moved in the past year, (2) has moved within the county in the past year, (3) has moved across counties, including moving across states and countries. If just cause eviction policies indeed enhance renter's housing stability, then we should expect a post-passage decrease in the likelihood of moving for renters in the treated cities.

Equation (2.4) is a conditional logit model where the dependent variable is a binary variable indicating the employment status of person i of household h in metro area c and year t. We estimate this model separately for two types of employment status: self-employment and wage employment. Equation (2.5) is a linear model where the dependent variable is the

natural logarithm of one plus total personal income, wage income, and business income for person i of household h in metro area c and year t.[20] We also estimate the model for total household-level income and in that case, we only keep the householders in the regression sample.

In all three models, $Post_{c,t}$ is a binary variable that equals one for all household-year observations in the treated metro areas after the passage of a just cause eviction law; $Renter_{h,t}$ is a binary variable that equals one if household h is renting the primary residence in year t; $X_{i,h,c,t}$ represents a vector of control variables including age, gender (a binary variable that equals one for female), racial minority (a binary variable that equals one for all nonwhite races), and education (a dummy variable that equals one if the person has a bachelor's degree or above); ϕ_h and θ_t denote household fixed effects and year fixed effects. [21]

Table 2.10 presents the estimates of Model (2.3) on the effect of just cause eviction law on household migration status. The estimates show that, for renter households in the treated cities, the likelihood of moving either within or across counties decreased by 3.59 percentage points after the passage of just cause eviction laws. Given that the unconditional likelihood of moving in the sample is 14 percentage points (see Table 2.9), these estimates translate into a 25.6% reduction in the likelihood of moving. This finding corroborates our results on the post-law-passage decrease in the eviction rate and strengthens the underlying premise that just cause eviction policies significantly enhance housing stability for renters.

Table 2.11 presents the estimates of Model (2.4) on the effect of just cause eviction law on household employment status. The estimates in Columns 1 and 3 show that renter

[20]We add one to the income variables before log transformation because some of the survey participants report zero business income.

[21]A limitation of the household survey data is that it provides location information only at the level of the metropolitan area, which is a time-varying geographic definition and is often broader than the definition of a city. Furthermore, the survey does not cover Glendale. Therefore, we consider all households as in the treated cities if they are reported to live in San Diego-Carlsbad-San Marcos after 2004 and in Oakland after 2002. We then include all other Californian households as the control group.

households in the treated cities are more (less) likely to enter self-employment (wage employment) after the passage of just cause eviction laws. The estimates of the marginal effects (unreported) suggest that, prior to the law passage, renter households have a lower likelihood of self-employment than homeowners by 5.80 percentage points, but their likelihood of self-employment increased by 12.60 percentage points more than homeowners after the law passage. The economic magnitude of this effect is substantial: it doubles the likelihood of self-employment from the sample mean (11 percentage points). Therefore, our estimates from household survey data are supportive of the hypothesis that legal renter protection from unjust evictions encourages renters to switch from wage employment to self-employment. In Columns 2 and 4, we separately examine the effect of law passage on the householders and nonhouseholders, i.e., the head of the household and the other adult members of the households, by adding a triple interaction between *Renter*, *Post*, and *Nonhouseholder*. Interestingly, the estimates show that the switch from wage employment to self-employment is only significant for nonhouseholders. These results suggest that housing stability allows the secondary earners of the households to take on risky entrepreneurial activities, while the primary earners can keep on their wage employment and provide a financial safety net for their families.

Table 2.12 reports the estimates of Model (2.5) on the effect just cause eviction law on household income. The estimates in Columns 1, 3, and 5 show that members of renter households in the treated cities experienced a 44.95% greater increase in personal income compared with homeowners after the passage of just cause eviction laws.[22] This includes a 34.82% increase in wage and a 49.14% increase in business income. In Columns 2, 4, and 6, we again add a triple interaction between *Renter*, *Post*, and *Nonhouseholder* to examine the differential changes in income for the householders and the other adult household members.

[22] $e^{(0.3712)} - 1 = 44.95\%$

The estimates show that the nonhouseholders of renter households had a greater increase in personal income relative to the householders, and this income increase by nonhouseholders is entirely driven by business income. This is consistent with the results in Table 2.11 which show that the increase in the renter households' propensity of self-employment is driven by nonhouseholders. In Column 7, we estimate the post-law-passage change in total household income and find that renter households in the treated cities indeed had a greater increase in aggregate income compared with homeowners. Collectively, the results in Tables 2.10 to 2.12 suggest that legal protection from unjust evictions enhances housing stability for renter households, and in turn allows these households to take the risk of self-employment and increase household income. Overall, the household-level evidence is supportive of our proposed hypothesis.

2.5.4 Alternative explanation: Local market demand shocks

An alternative explanation for our findings is that there could be a confounding trend in the local economic condition that leads to higher local demand for goods and services. As a result, new businesses are created to cater to these demands (e.g., Kerr et al., 2015). We perform several tests for this alternative explanation and report the results in the Internet Appendix. First, if this is the case, one would expect enhanced local market demand in the treated cities to also cause an increase in the number of firms that move in and a reduction in the number of firms that move out. To test this possibility, we examine the relationship between the passage of just cause eviction law and the number of existing firms moving in from and moving out to other areas, and the number of firms that go out of business in the treated cities. We identify establishments that go out of business each year using the last year in business provided in the NETS database and identify relocations of establishments based on historical location data provided by NETS. The estimates in Table A.8 show no significant changes in the number of establishments that moved in or out after the passage

of just cause eviction laws. These results do not support the notion that local demand shock is the only factor that drives the increase in the number of new firms that we observe in the treated cities.

To further examine the role of local market demand shocks, we explore the number of new firms in tradable and non-tradable sectors in the treated cities after the passage of just cause eviction laws. Non-tradable sectors typically consist of locally-rendered businesses such as restaurants, retail, and services. Local demand shocks should mostly affect the non-tradable sector in a county. On the other hand, the origin of demands for tradable sectors is more diversified, and thus firm creation in tradable sectors should not be as sensitive to fluctuations in local demands compared with the non-tradable sectors (Mian and Sufi, 2014). Hence, if local market demand is the main driver of the creation of new firms in the treated cities, one would expect the effect to be stronger for non-tradable sectors. We follow (Mian and Sufi, 2014) to identify industries in tradable and non-tradable sectors using the 4-digit NAIC code provided in the NETS database. The reported estimates in Table A.9 show that both the number of new firms in tradable and non–tradable sectors increased significantly in the treated cities after the passage of just cause eviction laws. Again, our evidence is inconsistent with local demand being the sole driving force of entrepreneurial activities in the treated cities.

Lastly, we examine changes in local demand for home mortgages after the passage of just cause eviction laws. We collect loan-level data on home mortgage applications from the database under the Home Mortgage Disclosure Act (HMDA) and match each home mortgage loan application to the corresponding city and year. Our regression estimates in Table A.10 show no significant increase in the local demand for home mortgages in the treated cities. Specifically, neither the number of applications nor the total value of applied home mortgages changed significantly after the passage of just cause eviction laws in the treated cities. Thus,

our evidence is inconsistent with a local economic shock, which possibly drives up both the demand for home mortgage loans and small business loans.

2.5.5 Quality of new firms

So far, we have documented a significant increase in new business creation associated with the passage of just cause eviction ordinance. However, one may be concerned that stronger renter protection merely draws in low-quality businesses, and creates no real value to the local economy. In this subsection, we attempt to measure the average quality of new firms created in the treated cities in several ways.

2.5.5.1 Firm survival

First, if the new entrepreneurs among renters are of low quality, one would expect these new firms to fail soon after inception. To test this possibility, we divide new firms into five groups based on years of survival since inception up to 6 years.[23] Column 1 of Table 2.13 shows that the post–passage increase in the number of new firms that only survived for 1 to 2 years is not statistically significant. By contrast, the estimates in Columns (2) and (3) show that the number of new firms that survive for 3-4 years and 5-6 years increased by 25.7% and 14.3% in the treated cities. This result is remarkable given the relatively low survival rate of small businesses. For example, according to the U.S. Bureau of Labor Statistics, the average 5–year survival rate for newly established businesses during our sample period is 48.8%.[24] These results suggest that the new firms created due to stronger renter protection are likely to survive in the long run.

[23]To perform this test, we have to truncate the sample because we do not know the survival status of firms that emerge near the end of the sample period.

[24]Source:`https://www.bls.gov/bdm/us_age_naics_00_table7.txt`

2.5.5.2 Firm operating and financial performance

Next, we measure the average quality of new firms in terms of three-year average sales per employee and three-year average financial condition. We measure a firm's financial condition based on the PayDex score (from 0 to 100) provided in the NETS database. The PayDex score is a dollar-weighted indicator assigned by Dun & Bradstreet to reflect the payment performance of a firm. NETS provides time series data on sales, employee count, and the PayDex score for each establishment.[25] For each establishment and year, NETS reports the minimum and maximum PayDex score. We take the average of the two scores to measure the financial condition of an establishment. Table 2.14 reports estimates of Model (2.2) using firm performance as the dependent variable. Specifically, the dependent variable in Column 1 is the natural logarithm of one plus three-year average sales per employee. The dependent variable in Column 2 is the three-year average PayDex score. We take the average of these two performance measures across all new firms created in a city-year. The estimates in Column 2 show that the average credit score of new firms created in the treated cities after the passage of just cause eviction laws is higher by 0.868 percentage points (or 1.2% of sample mean) relative to those in other cities or those created in the treated cities prior to the passage. These results suggest that firms created due to stronger renter protection on average have better financial performance than other firms. Thus, policies that enhance renter housing stability does not merely encourage low-quality self-employment.

[25] A caveat of our analysis on establishment sales and employment is that a large portion of the sales data from NETS are imputed. The imputation is evident from the fact that a large proportion of the establishments have identical sales figures for many consecutive years (e.g., see Crane and Decker, 2019). This data quality issue will significantly limit any analysis on the time-series dynamics of establishment performance using the sales data. Though our analysis focuses on the cross-sectional comparison across firms at the beginning of their life, which is arguably less affected by this data limitation, we should still interpret the results using the NETS sales data with caution.

2.5.5.3 VC investments, acquisitions, and going public

We also examine whether stronger renter protection leads to a higher number of "transformational" entrepreneurs, who strive to build ambitious businesses that grow beyond generating subsistence incomes and make disproportionate contributions to the economy (Hurst and Pugsley, 2011, Schoar, 2010). First, we identify new firms that later received VC financing. Using VC funding data from VentureXpert, we match startups that receive VC financing with establishments in our sample based on the company name, county, and ZIP code. Second, we identify mergers and acquisitions based on historical information on parent firms provided by NETS. Because all the establishments in our sample start off as standalone firms, we consider firms as being acquired if there is later a parent firm that does not share the same identifier as the establishment. Third, we identify establishments that eventually went public using the public/private indicator provided by NETS.[26]

In Table 2.15, we present estimates of Model (2.2) by using the number of new firms in a city-year that eventually received VC financing, became an acquisition target, or went public as the dependent variables. The estimates show that just cause eviction laws are not associated with a significantly higher number of new firms that fall into these three categories. Therefore, legal renter protection against wrongful evictions appears to create businesses that generate subsistence incomes but not transformational impacts on the economy.

2.5.6 External validity

Lastly, to ensure the external validity of our findings, we use the recent passages of just cause eviction law in four states: New Hampshire (2015), Oregon (2019), California (2020), and Washington (2021), to identify the exogenous variation in the risk of wrongful eviction for renters across states. A shortcoming of examining state-level just cause eviction laws

[26]We only consider firms that became public through an IPO rather than through an acquisition by other public firms.

is that, since these state laws are passed only recently, we do not have enough data on the post-passage period to examine their long-term impacts. Nevertheless, we examine the effect of statewide just cause eviction laws on household career choice using the latest CPS data. Specifically, we take the nationwide CPS sample from 2013 to 2022 to study the post-passage changes in the likelihood of self-employment for renter households in the treated states. Table A.11 presents the estimates of Model (2.4) using the nationwide CPS data. Column 1 includes state and year fixed effects and Column 2 includes state×year fixed effects. Consistent with our hypothesis, the estimates in both Columns show that renter households in the treated states are more likely to be self-employed after the passage of just cause eviction laws. The estimates of the marginal effects from Column 1 (unreported) suggest that in states without the law passage, renter households have a lower likelihood of self-employment than homeowners by 2.86 percentage points, but renters' likelihood of self-employment increases by 1.62 percentage points more than homeowners in states with the enforcement of just cause eviction law. These results are consistent with our findings using city-level ordinances in Californian cities.

2.6 Conclusion

In this study, we examine the hypothesis that regulations that enhance renters' housing stability can encourage new business creation by insulating renter households from the risk of an involuntary move due to arbitrary and wrongful evictions. Utilizing the passages of just cause eviction ordinances in three cities in California as an identification strategy, we show that stronger renter protection leads to a significantly higher number of new firms, especially firms owned by females and racial minorities. These firms tend to survive in the long run and have similar performance to those created prior to law passages. Renter households appear to have benefited from just cause eviction by enjoying better housing stability and increasing household income through self-employment. Our evidence shows that rental mar-

ket policies that enhance renters' housing stability have the surprising benefit of encouraging small business creation, which contributes to local employment and economic growth. As the recent declining trend of the homeownership rate and the COVID-19 pandemic sparked new concerns about housing stability and renter vulnerability, our study provides a new perspective to the policy debate on rental housing market regulations.

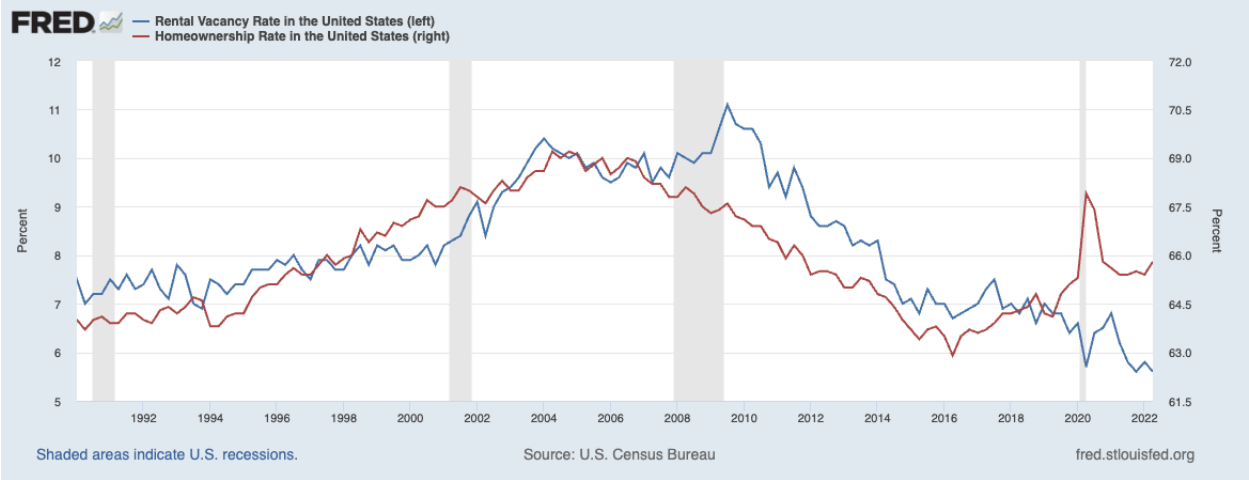


Figure 2.1: Homeownership rate and rental vacancy rate in the U.S
This figure shows the historical homeownership rate and rental vacancy rate in the U.S. Source: the U.S. Census Bureau

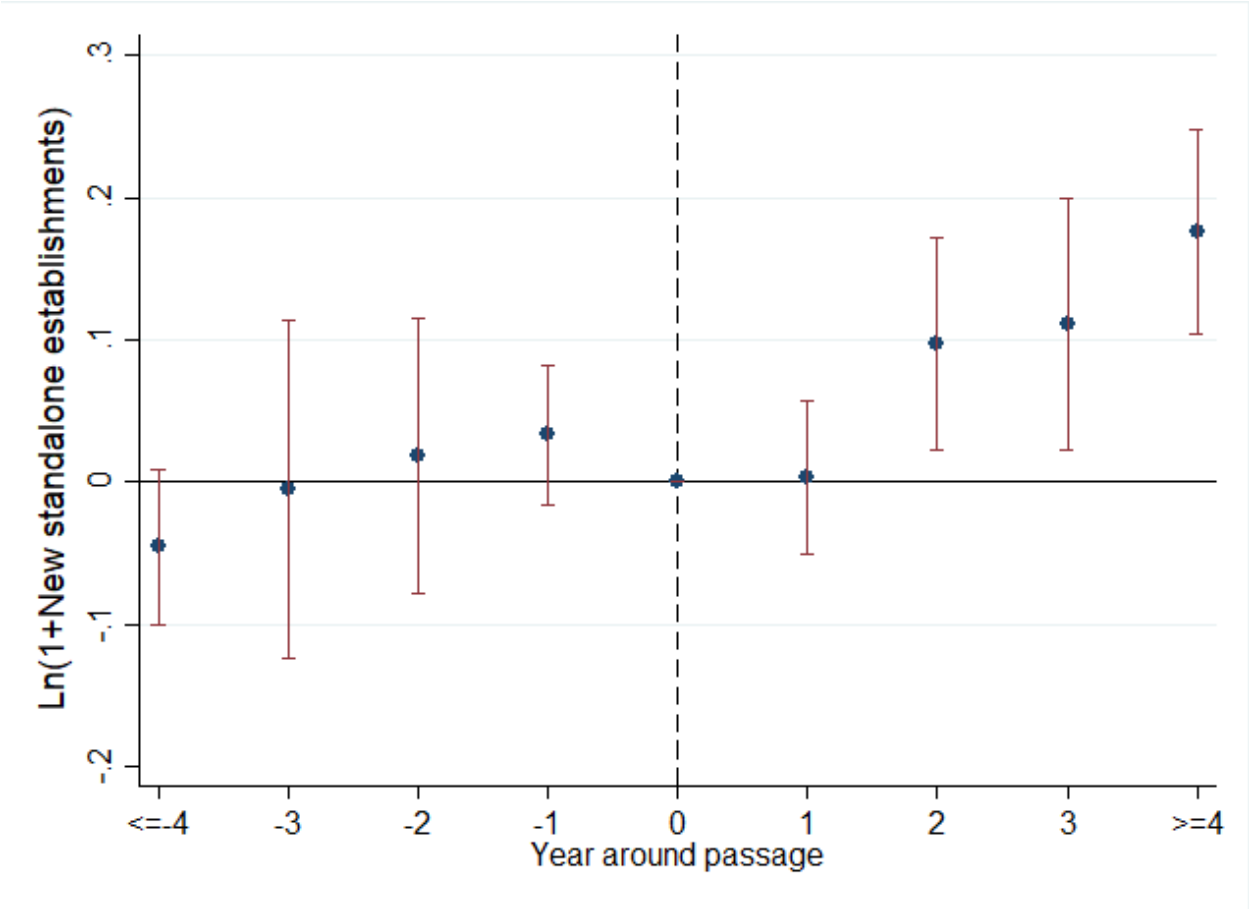


Figure 2.2: The number of new standalone establishments in California around the passage of just cause ordinance

This figure presents the estimates for a dynamic DiD model of the number of new standalone establishments (Model (2.2)). Each node represents the coefficient estimate for the difference between the treated and control cities in years around the passage of just cause ordinance. Observations for the treated cities that are four years or more before the passage are set as the baseline for the estimates.

Table 2.1: Summary statistics

This table reports summary statistics on the number of new establishments and eviction rates for the treated and control cities.

Panel A: Full-sample

Statistic:	N	Mean	P25	P50	P75	S.D.
Treated group (3 cities in 3 counties)						
Eviction rate (%)	24	1.17	0.29	1.49	1.56	0.71
Eviction filing rate (%)	24	1.41	0.32	1.75	2.09	0.87
New standalone establishments	27	4,852.44	1,943.00	2,758.00	7,232.00	4,354.55
New establishments with female owners (%)	27	7.57	4.72	6.32	8.69	4.24
New establishments with racial minority owners (%)	27	3.04	1.61	2.23	3.93	2.22
New establishments with residential address (%)	27	37.02	33.60	36.91	39.86	4.29
Control group: 117 cities						
Eviction rate (%)	936	1.90	0.84	1.48	2.31	2.97
Eviction filing rate (%)	936	2.08	0.93	1.69	2.53	3.13
New standalone establishments	1,053	708.21	159.00	319.00	578.00	3,037.25
New establishments with female owners (%)	1,053	7.19	3.81	5.56	9.09	5.72
New establishments with racial minority owners (%)	1,053	3.07	1.13	2.08	3.81	3.32
New establishments with residential address (%)	1,053	36.47	26.44	35.98	45.71	17.61

Panel B: Counties of treated cities

Statistic:	N	Mean	P25	P50	P75	S.D.
Treated city: Glendale in Los Angeles county						
Eviction rate (%)	8	0.29	0.09	0.11	0.29	0.41
Eviction filing rate (%)	8	0.30	0.10	0.12	0.32	0.42
New standalone establishments	9	1,862.78	1,311.00	1,840.00	2,591.00	736.74
New establishments with female owners (%)	9	6.44	4.63	4.78	7.38	3.96
New establishments with racial minority owners (%)	9	2.05	0.96	1.58	2.23	1.71
New establishments with residential address (%)	9	37.74	36.52	37.52	38.48	3.32
Control cities (87) in Los Angeles county						
Eviction rate (%)	696	2.09	0.91	1.56	2.48	3.39
Eviction filing rate (%)	696	2.27	1.00	1.76	2.72	3.57
New standalone establishments	783	779.45	151.00	287.00	525.00	3,512.05
New establishments with female owners (%)	783	6.82	3.54	5.19	8.58	5.97
New establishments with racial minority owners (%)	783	3.20	1.06	2.14	3.94	3.60
New establishments with residential address (%)	783	34.62	24.06	33.27	44.17	18.51

Statistic:	N	Mean	P25	P50	P75	S.D.
Treated city: Oakland in Alameda county						
Eviction rate (%)	8	1.57	1.36	1.53	1.67	0.28
Eviction filing rate (%)	8	2.13	1.91	2.17	2.34	0.24
New standalone establishments	9	2,639.44	2,009.00	2,712.00	3,558.00	907.09
New establishments with female owners (%)	9	8.39	5.90	7.30	8.06	4.91
New establishments with racial minority owners (%)	9	4.54	2.73	3.93	5.40	2.73
New establishments with residential address (%)	9	39.14	36.98	39.32	41.21	4.64
Control cities (13) in Alameda county						
Eviction rate (%)	104	0.88	0.36	0.88	1.28	0.57
Eviction filing rate (%)	104	1.16	0.54	1.09	1.63	0.79
New standalone establishments	117	492.51	192.00	394.00	681.00	413.26
New establishments with female owners (%)	117	8.13	4.74	6.94	10.48	4.94
New establishments with racial minority owners (%)	117	3.18	1.54	2.73	4.08	2.27
New establishments with residential address (%)	117	44.32	36.17	41.81	49.57	15.45
Treated city: San Diego in San Diego county						
Eviction rate (%)	8	1.65	1.50	1.52	1.77	0.26
Eviction filing rate (%)	8	1.80	1.61	1.75	1.99	0.24
New standalone establishments	9	10,055.11	7,232.00	9,896.00	13,419.00	3,774.29
New establishments with female owners (%)	9	7.88	5.31	6.32	8.69	4.06
New establishments with racial minority owners (%)	9	2.52	1.79	2.12	2.47	1.33
New establishments with residential address (%)	9	34.17	32.70	33.97	35.41	3.53
Control cities (17) in San Diego county						
Eviction rate (%)	136	1.73	0.95	1.74	2.36	0.85
Eviction filing rate (%)	136	1.83	1.03	1.84	2.44	0.88
New standalone establishments	153	508.58	192.00	417.00	728.00	394.64
New establishments with female owners (%)	153	8.34	5.02	7.27	10.20	4.63
New establishments with racial minority owners (%)	153	2.37	0.97	1.78	2.92	2.27
New establishments with residential address (%)	153	39.99	31.66	41.26	48.36	11.14

Table 2.2: Just cause eviction law and eviction rate

This table presents estimates of DiD models of eviction rate (Model (2.1)). The dependent variable is the natural logarithm of one plus eviction rate or eviction filing rate in percentage. *Post* is a binary variable that equals one after the passage of the just cause ordinance. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

	Ln(1+eviction rate)	Ln(1+eviction filing rate)
Post	-0.069**	-0.059
	(-2.22)	(-1.64)
County×Year FE	Yes	Yes
City FE	Yes	Yes
Adjusted R^2	0.738	0.719
Observations	960	960

Table 2.3: Just cause eviction law and new establishments

This table presents estimates of DiD models of the number of new firms (Model (2.2)). The dependent variable is the natural logarithm of one plus the number of new establishments. Column 1 includes both standalone establishments and subsidiaries. Column 2 includes only standalone establishments. Columns (3) to (6) include the standalone establishment of different size groups, with size defined by the number of employees in the year of inception. Column 7 includes only subsidiary establishments. *Post* is a binary variable that equals one after the passage of just cause ordinances. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	All	Standalone					Subsidiary
		All standalone	Small (1-5)	Small (6-10)	Medium (11-20)	Large (20+)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	0.094***	0.094***	0.092***	0.122**	0.071	-0.104	-0.125
	(3.55)	(3.70)	(3.87)	(2.54)	(1.21)	(-1.20)	(-1.42)
County×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.982	0.982	0.981	0.916	0.861	0.834	0.924
Observations	1,080	1,080	1,080	1,080	1,080	1,080	1,080

Table 2.4: Top industries in small business creation post passage

This table presents the top 25 8-digit SIC industries that experienced statistically significant growth in the number of new firms post-passage. Columns 3 and 4 present the coefficient and t-statistics on estimates of Model (2.2) using the natural logarithm of one plus the number of new standalone establishments in each 2-digit SIC industry as the dependent variable. We exclude industries for which the coefficient is statistically insignificant, and non-classifiable establishments (SIC=99). *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Rank	SIC code (1)	Coefficient (2)	Industry Name (3)
1	76991103	1.469	Locksmith shop
2	61530100	1.094	Working capital financing
3	47890000	0.908	Transportation services, not elsewhere classified
4	56210101	0.873	Boutiques
5	55990000	0.823	Automotive dealers, not elsewhere classified
6	62119901	0.812	Mortgages, buying and selling
7	56219902	0.794	Ready-to-wear apparel, women's
8	67990000	0.778	Investors, not elsewhere classified
9	48990000	0.775	Communication services, not elsewhere classified
10	89990900	0.769	Scientific consulting
11	67190000	0.757	Holding companies, not elsewhere classified
12	73890200	0.756	Inspection and testing services
13	50990000	0.746	Durable goods, not elsewhere classified
14	79910103	0.730	Spas
15	73810100	0.730	Guard services
16	84120100	0.711	Art gallery
17	89990000	0.678	Services, not elsewhere classified
18	59990601	0.662	Audio-visual equipment and supplies
19	73890600	0.660	Interior design services
20	87410100	0.654	Business management
21	72999901	0.646	Apartment locating service
22	67330000	0.632	Trusts, not elsewhere classified
23	80119904	0.630	Physicians' office, including specialists
24	61410000	0.622	Personal credit institutions
25	42120000	0.616	Local trucking, without storage

Table 2.5: New firms owned by females and racial minorities

This table presents estimates of DiD models of the number of new firms. In Column 1 (2), the dependent variable is the natural logarithm of one plus the number (proportion in percentage) of new standalone establishments with female owners. In Column 3 (4), the dependent variable is the natural logarithm of one plus the number (proportion in percentage) of new standalone establishments with racial minority (non-white) owners. *Post* is a binary variable that equals one after the passage of the just cause ordinance. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

X:	Number of new standalone establishments by			
	female owners		racial minority owners	
Dependent variable:	Ln(1+X)	Ln(1+%X)	Ln(1+X)	Ln(1+%X)
	(1)	(2)	(3)	(4)
Post	0.133***	0.077***	0.136	0.114*
	(4.97)	(2.87)	(1.27)	(1.85)
County×Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.950	0.681	0.899	0.681
Observations	1,080	1,080	1,080	1,080

Table 2.6: Just cause eviction law and small business loans

This table presents estimates of DiD models of small business loan issuances. For each city-year, we count the number and the amount of newly issued small business loans provided through the SBA 7(a) lending program. In Column 1, the dependent variable is the natural logarithm of one plus the number of new loans. In Columns 2 and 3, the dependent variable is the natural logarithm of one plus the total (average) loan amount. The sample in Column 3 is smaller because the average loan value is missing if there are no SBA loans issued in a city–year. *Post* is a binary variable that equals one after the passage of the just cause ordinance. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	Ln(1+Number of new loans)	Ln(1+Total loan amount)	Ln(1+Average loan amount)
	(1)	(2)	(3)
Post	0.180***	-0.280	-0.080*
	(2.79)	(-0.93)	(-1.83)
County×Year FE	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Adjusted R^2	0.917	0.727	0.413
Observations	1,080	1,080	1,035

Table 2.7: Just cause eviction law and new firms: partitioned by renter occupancy rate

This table presents estimates of DiD models of the number of new firms (Model (2.2)). We sort census tracts into quartiles based on the renter occupancy rate. The dependent variable is the natural logarithm of one plus the number of new standalone establishments in each renter occupancy quartile within a city. *Post* is a binary variable that equals one after the passage of the just cause ordinance. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	Ln(1+New standalone establishments)			
Renter occupancy rate:	Quartile 1	Quartile 2	Quartile 3	Quartile 4
	(1)	(2)	(3)	(4)
Post	-0.104	0.092	0.139	0.140***
	(-0.91)	(0.57)	(0.85)	(3.37)
County×Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.953	0.944	0.946	0.976
Observations	1,080	1,080	1,080	1,080

Table 2.8: New firms with residential addresses

Panel A presents estimates of DiD models in which the dependent variable is the natural logarithm of one plus the number of new standalone establishments with residential addresses. Panel B presents estimates of DiD models in which the dependent variable is the natural logarithm of one plus the proportion (in percentage) of all new standalone establishments that use residential addresses. In Columns 2 to 5 of both panels, we sort census tracts into quartiles based on renter occupancy rate and count the number of new establishments in each quartile. *Post* is a binary variable that equals one after the passage of the just cause ordinance. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively

Panel A: Number of new firms with residential addresses

Dependent variable:	Ln(1+New standalone establishments with residential address)				
Renter occupancy rate:	Full sample	Quartile 1	Quartile 2	Quartile 3	Quartile 4
	(1)	(2)	(3)	(4)	(5)
Post	0.131**	-0.036	0.045	0.189	0.267***
	(2.57)	(-0.37)	(0.37)	(1.28)	(4.82)
County×Year FE	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.970	0.948	0.947	0.939	0.966
Observations	1,080	1,080	1,080	1,080	1,080

Panel B: Proportion of new firms with residential addresses

Dependent variable:	Ln(1+%New standalone establishments with residential addresses)				
Renter occupancy rate:	Full sample	Quartile 1	Quartile 2	Quartile 3	Quartile 4
	(1)	(2)	(3)	(4)	(5)
Post	0.027	-0.158**	-0.067	0.086	0.121***
	(0.93)	(-2.00)	(-0.96)	(0.66)	(2.88)
County×Year FE	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.872	0.955	0.913	0.919	0.947
Observations	1,080	1,080	1,080	1,080	1,080

Table 2.9: Summary statistics for household survey data

This table presents the summary statistics of household survey data from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey. The sample consists of adult individuals in California covered by the survey from 1999 to 2007.

Statistic:	N	Mean	P25	P50	P75	S.D.
Total personal income	37,280	41,438.77	14,400.00	29,120.00	51,100.00	50,523.95
Wage income	37,280	35,123.27	10,000.00	24,960.00	46,000.00	45,567.01
Business income	37,280	3,443.78	0.00	0.00	0.00	20,683.68
Total household income	17,703	81,997.88	37,440.00	63,900.00	102512.00	74,079.89
Not moved	37,280	0.86	1.00	1.00	1.00	0.35
Moved within county	37,280	0.10	0.00	0.00	0.00	0.30
Moved across county	37,280	0.05	0.00	0.00	0.00	0.21
Self employment	37,280	0.11	0.00	0.00	0.00	0.32
Wage employment	37,280	0.88	1.00	1.00	1.00	0.32
Renter	37,280	0.37	0.00	0.00	1.00	0.48
Age	37,280	39.31	29.00	39.00	49.00	12.95
Female	37,280	0.47	0.00	0.00	1.00	0.50
Nonwhite	37,280	0.19	0.00	0.00	0.00	0.40
Education	37,280	0.29	0.00	0.00	1.00	0.46

Table 2.10: Just cause eviction law, renter household, and housing stability

This table presents estimates of Model (2.3) using household survey data from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey. The sample consists of adult individuals in California covered by the survey from 1999 to 2007. The dependent variable in Column 1 is a binary variable that equals one if the person has not moved in the past year. The dependent variable in Column 2 (3) is a binary variable that equals one if the person has moved within (across) counties in the past year. *Renter* is a binary variable that equals one if the household rents its residence. *Post* is a binary variable that equals one for person-year observations in Oakland or San Diego after the passage of just cause ordinances. We include household fixed effects and year fixed effects in the regressions. We report t-statistics using standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	Not moved	Moved within county	Moved across county
	(1)	(2)	(3)
Renter × Post	1.8223***	-1.5464***	-15.1127***
	(6.77)	(-3.93)	(-17.08)
Renter	0.1007	-0.1497	0.0285
	(0.38)	(-0.37)	(0.04)
Post	0.8546	-0.7240	0.7484*
	(0.72)	(-0.46)	(1.94)
Age	0.0503***	-0.0404***	-0.0488***
	(12.67)	(-7.98)	(-8.49)
Female	0.0144	0.0712	-0.1393
	(0.27)	(1.06)	(-1.42)
Nonwhite	-0.1066	-0.1337	0.4461**
	(-0.53)	(-0.63)	(2.03)
Education	0.0623	-0.0351	0.0100
	(0.45)	(-0.22)	(0.09)
Household FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Pseudo R^2	0.211	0.213	0.126
Observations	5,731	3,972	2,693

Table 2.11: Just cause eviction law, renter household, and employment status

This table presents estimates of Model (2.4) using household survey data from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey. The sample consists of adult individuals in California covered by the survey from 1999 to 2007. The dependent variable in columns 1 and 2 is a binary variable that equals one if the person is reported as self-employed. The dependent variable in Columns 3 and 4 is a binary variable that equals one if the person is reported to have a wage employment. *Renter* is a binary variable that equals one if the household rents its residence. *Nonhouseholder* is a binary variable that equals one if the person is not the head of the household. *Post* is a binary variable that equals one for person-year observations in Oakland or San Diego after the passage of just cause ordinances. We include household fixed effects and year fixed effects in the regressions. We report t-statistics using standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	Self employment		Wage employment	
	(1)	(2)	(3)	(4)
Renter × Post	0.9947***	0.5292	-1.1979***	-0.6953
	(2.92)	(1.36)	(-3.96)	(-1.62)
Renter × Post × Nonhouseholder		0.9624***		-1.0357***
		(5.69)		(-8.92)
Renter	-0.4577**	-0.3834*	0.4611**	0.3929*
	(-2.30)	(-1.92)	(2.32)	(1.95)
Post	0.4538***	0.5918***	-0.4681***	-0.6140***
	(2.88)	(5.31)	(-2.90)	(-5.24)
Nonhouseholder		-0.3330***		0.3294***
		(-4.76)		(4.66)
Renter × Nonhouseholder		-0.1477		0.1350
		(-1.33)		(1.26)
Post × Nonhouseholder		-0.2539**		0.2690**
		(-2.24)		(2.42)
Age	0.0724***	0.0672***	-0.0714***	-0.0663***
	(30.44)	(24.28)	(-29.40)	(-22.37)
Female	-0.5809***	-0.5251***	0.5576***	0.5020***
	(-6.72)	(-6.86)	(6.58)	(6.66)
Nonwhite	-0.2762	-0.2629	0.2624	0.2445
	(-1.38)	(-1.27)	(1.32)	(1.20)
Education	-0.2443*	-0.2876**	0.2750**	0.3185**
	(-1.87)	(-2.25)	(2.11)	(2.52)
Household FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pseudo R^2	0.147	0.158	0.143	0.153
Observations	7,128	7,128	7,060	7,060

Table 2.12: Just cause eviction law, renter household, and income

This table presents estimates of Model (2.5) using household survey data from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey. The sample in Columns 1 to 6 consists of adult individuals in California covered by the survey from 1999 to 2007. The dependent variable is the natural logarithm of one plus total personal income in Columns 1 and 2, wage income in Columns 3 and 4, and business income in Columns 5 and 6. *Renter* is a binary variable that equals one if the household rents its residence. *Post* is a binary variable that equals one for person-year observations in Oakland or San Diego after the passage of the just cause ordinance. *Nonhouseholder* is a binary variable that equals one if the person is not the head of the household. The dependent variable in Column 7 is the natural logarithm of one plus total household income and the sample includes only the householders. We include household fixed effects and year fixed effects in the regressions. We report t-statistics using standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	ln(Total personal income)		ln(Wage income)		ln(Business income)		ln(Total household income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Renter × Post	0.3712***	0.2012*	0.2988***	0.3653	0.3997**	0.2713	0.1310***
	(3.67)	(1.70)	(3.30)	(1.56)	(2.22)	(1.38)	(3.07)
Renter × Post × Nonhouseholder		0.3704***		-0.1159		0.2654***	
		(11.58)		(-0.35)		(3.22)	
Renter	-0.0933	-0.0667	0.1381	0.1321	-0.2226**	-0.2386**	-0.0476
	(-1.34)	(-0.88)	(1.22)	(0.94)	(-2.40)	(-2.74)	(-0.88)
Post	0.0281	0.0696	-0.2730***	-0.4052***	0.2338	0.3216	0.0405*
	(0.26)	(1.26)	(-5.09)	(-2.76)	(1.04)	(1.61)	(1.84)
Nonhouseholder		-0.2282***		-0.0928		-0.1919***	
		(-6.53)		(-1.38)		(-6.03)	
Renter × Nonhouseholder		-0.0481		0.0145		0.0358	
		(-1.31)		(0.13)		(0.42)	
Post × Nonhouseholder		-0.0857		0.3142		-0.2100***	
		(-0.60)		(0.85)		(-2.79)	
Age	0.0637***	0.0598***	0.0341***	0.0328***	0.0283***	0.0252***	
	(21.48)	(20.01)	(11.52)	(10.65)	(25.42)	(23.57)	
Female	-0.5105***	-0.4716***	-0.2891***	-0.2755***	-0.3398***	-0.3085***	
	(-25.74)	(-21.72)	(-5.25)	(-4.99)	(-7.57)	(-7.47)	
Nonwhite	-0.1151	-0.1025	-0.0228	-0.0222	-0.1118	-0.1011	
	(-1.57)	(-1.40)	(-0.14)	(-0.14)	(-1.02)	(-0.91)	
Education	0.6087***	0.5877***	0.7616***	0.7540***	0.0312	0.0137	
	(15.36)	(13.87)	(11.70)	(11.70)	(0.40)	(0.18)	
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.246	0.250	0.279	0.279	0.328	0.329	0.548
Observations	37,093	37,093	37,280	37,280	36,961	36,961	12,888

Table 2.13: Just cause eviction law and survival of new firms

This table presents estimates of DiD models of the number of new firms by years of survival. The dependent variables are the natural logarithm of one plus the number of new standalone establishments grouped by the number of years survived since inception. *Post* is a binary variable that equals one after the passage of just cause ordinance. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	ln(1+New standalone establishments)		
Number of years survived:	1-2 years	3-4 years	5-6 years
	(1)	(2)	(3)
Post	0.020	0.229***	0.134**
	(0.44)	(3.81)	(2.08)
County×Year FE	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Adjusted R^2	0.954	0.959	0.959
Observations	1,080	1,080	1,080

Table 2.14: Just cause eviction law and performance of new firms

This table presents estimates of DiD models of firm performance. The dependent variable in Column 1 (2) is the natural logarithm of three-year average sales per employee (PayDex score) of the new firms. *Post* is a binary variable that equals one after the passage of just cause ordinances. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

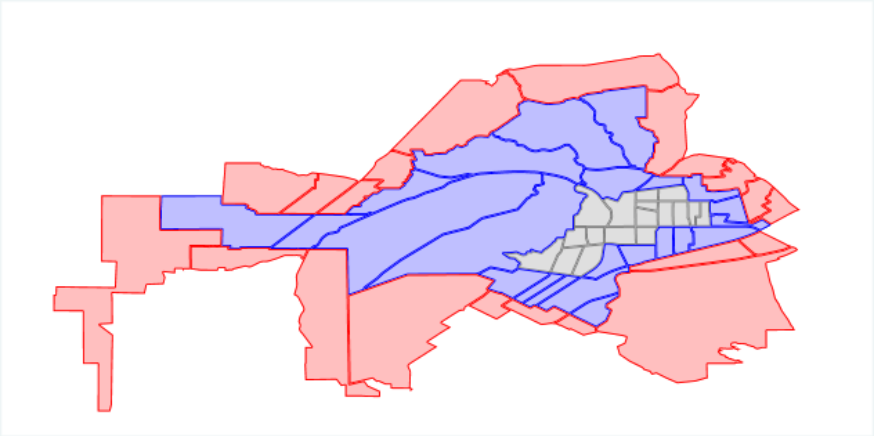
Dependent variable:	Ln(Average 3-year sales per employee)	Average 3-year PayDex
	(1)	(2)
Post	-0.012	0.868***
	(-0.92)	(2.95)
County×Year FE	Yes	Yes
City FE	Yes	Yes
Adjusted R^2	0.670	0.292
Observations	1,080	1,041

Table 2.15: Just cause eviction law and successful firm outcomes

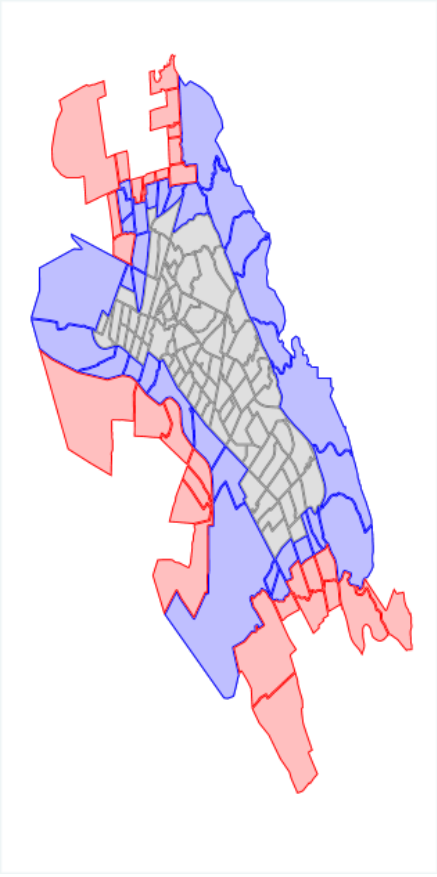
This table presents estimates of linear probability DiD models. The dependent variables are the natural logarithm of one plus the number of new standalone establishments that eventually received VC financing (Column 1), became an acquisition target (Column 2), or went public (Column 3). *Post* is a binary variable that equals one after the passage of just cause ordinances. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	ln(1+New standalone establishments)		
	VC funding	Acquisition	Going public
	(1)	(2)	(3)
Post	0.011	-0.098	0.128
	(0.16)	(-0.95)	(0.75)
County×Year FE	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Adjusted R^2	0.797	0.852	0.632
Observations	1,080	1,080	1,080

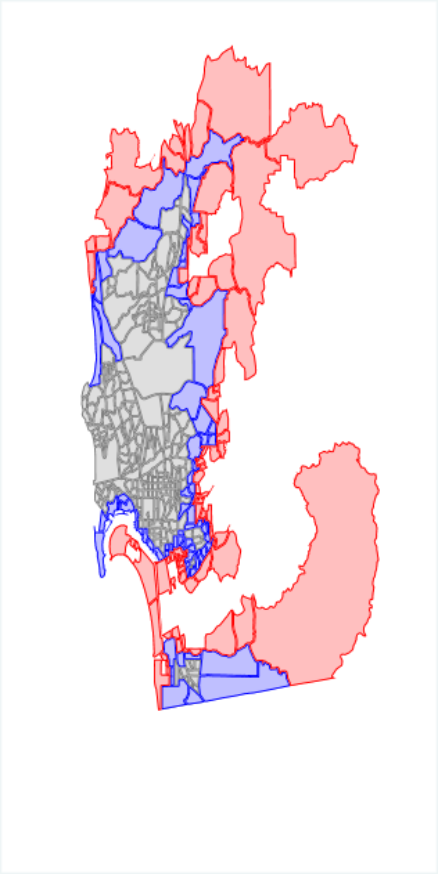
2.7 Appendix



(a) *Glendale*



(b) *Oakland*



(c) *San Diego*

Figure A.1: Census tracts around the borders of treated cities

These figures show the census tracts around the borders of treated cities that are included in the analysis in Table A.3. The census tracts in blue (red) color are in the treated (control) group.

Table A.1: Legal reasons for eviction

Legal reason for eviction	Glendale	Oakland	San Diego
Failure to pay rent	✓	✓	✓
Violations of rental agreement	✓	✓	✓
Substantial damage to the unit	✓	✓	✓
Illegal use	✓	✓	✓
Landlords' right to make substantial repairs	✓	✓	✓
Landlord or immediate family to occupy	✓	✓	✓
Refusal to sign identical new lease		✓	✓
Refused the landlord access		✓	✓
Removal of unit from market by demolition	✓		
Landlord to recover possession under Ellis Act		✓	
Failure to vacate under a temporary lease			
Landlord to recover possession to comply with government's order to vacate	✓		
Landlord to recover possession to comply with contractual agreement relating to the qualifications of tenancy	✓		
Tenant at the end of the lease is a subtenant not approved	✓		
Tenant continues to smoke when prohibited	✓		
Remove the unit permanently from rental housing use	✓		✓
Tenant has continued to disturb neighbors after written notice		✓	

Table A.2: Summary of just cause eviction law by city

Treated cities	Ordinance passed	Passage year	Existing renter protection measures
Glendale	Just Cause and Retaliatory Evictions Ordinance	2002	NA
Oakland	Just Cause for Eviction Ordinance	2002	Rent Adjustment Program Ordinance 1980
San Diego	Just Cause Eviction Ordinance	2004	NA

Table A.3: Just cause eviction law and new establishments around city borders

This table presents estimates of DiD models of the number of new firms (Model (2.2)) using census tracts around the borders of treated cities. The dependent variable is the natural logarithm of one plus the number of new standalone establishments. Column 1 includes all standalone establishments. Columns (1) and (2) include standalone establishments with 1-5 employees and 6-10 employees, respectively. *Post* is a binary variable that equals one after the passage of just cause eviction ordinances. We include county × year fixed effects and census tract fixed effects in the regressions. We report t-statistics using standard errors clustered by census tract in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	Ln(1+New standalone establishments)		
	All standalone	Small(1-5)	Small(6-10)
	(1)	(2)	(3)
Post	0.069**	0.065**	0.029
	(2.11)	(1.97)	(0.89)
County × Year FE	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes
Adjusted R^2	0.950	0.946	0.634
Observations	2,673	2,673	2,673

Table A.4: Robustness: Alternative specifications

This table checks the robustness of our key results to alternative specifications. In Panel A, we re-estimate Model (2.2) by decomposing the DiD estimator to those for each individual treated city. We report t-statistics using standard errors clustered by city. In Panel B, we re-estimate Model (2.2) using alternative samples and models. Column 1 includes all Californian cities. Column 2 includes only large Californian cities that have at least as many new businesses as the treated cities as of the beginning of the sample period. Column 3 includes a matched sample with the three treated cities and ten control Californian cities selected based on the propensity score. Column 4 presents estimates of a negative binomial regression. Column 5 presents estimates of a Poisson regression. We report t-statistics using standard errors clustered by city in Columns 1 and 5 and robust standard errors in Columns 2 to 4. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Panel A: DiD for individual treatments

Dependent variable:	Ln(1+New standalone establishments)		
	All	Small (1-5)	Small (5-10)
	(1)	(2)	(3)
Post (Glendale)	0.162***	0.162***	0.153***
	(9.49)	(9.06)	(5.25)
Post (Oakland)	0.091*	0.088*	0.122
	(1.93)	(1.81)	(1.64)
Post (San Diego)	0.057**	0.066**	0.033
	(2.30)	(2.47)	(0.84)
County×Year FE	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Adjusted R^2	0.982	0.981	0.916
Observations	1,080	1,080	1,080

Panel B: Alternative sample and models

Dependent variable:	Ln(1+New standalone establishments)			New standalone establishments	
Sample:	All Californian counties	Large cities	Matched cities	Treated counties	
Model:	Linear	Linear	Linear	Negative binomial	Poisson
	(1)	(2)	(3)	(4)	(5)
Post	0.094***	0.123***	0.097**	0.076*	0.069**
	(3.57)	(2.90)	(2.01)	(1.83)	(2.42)
County × Year FE	Yes	No	No	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
Year FE	Absorbed	Yes	Yes	Absorbed	Absorbed
Adjusted R^2	0.982	0.987	0.991		
Log Likelihood				-4936.556	-6900.284
Observations	4,194	153	117	1,080	1,080

Table A.5: Propensity score matching

This table compares the treated cities with the control cities in the matched sample used for estimation in column 3, Panel B of Table A.4. Using all Californian cities, we estimate the propensity score based on $Ln(1 + Newstandaloneestablishments)$ in 2000 and the following city characteristics reported by the 2000 Census: the natural logarithm of the city population (*ln(Population)*), poverty rate, the proportion of rent households (*%Renter households*), the natural logarithm of median household income (*ln(Income)*), the natural logarithm of the median property value (*ln(Property value)*), and the natural logarithm of median gross rent (*ln(Rent)*). We then match the three treated cities with ten control cities based on the propensity score. The matched control cities include Berkeley, Chula Vista, El Cajon, Goleta, Long Beach, San Francisco, San Jose, Santa Barbara, Santa Monica, and West Hollywood. Columns 1 and 2 report the mean value of city characteristics for the treated and control groups. Column 3 reports the differences. The p-values of the differences are reported in brackets.

Sample:	Treated	Control	Difference
Number of cities:	3	10	
	(1)	(2)	(3)
Ln(1+New standalone establishments)	7.879	6.839	1.040
			(0.17)
ln(Population)	13.032	11.955	1.077
			(0.16)
Poverty rate	16.497	13.216	3.281
			(0.31)
%Renter households	56.880	55.867	1.013
			(0.91)
ln(Income)	10.656	10.768	-0.111
			(0.41)
ln(Property value)	12.474	12.753	-0.279
			(0.29)
ln(Rent)	6.604	6.706	-0.101
			(0.38)

Table A.6: Just cause eviction law and employment created by new establishments

This table presents estimates of DiD models of the number of new firms (Model (2.2)). The dependent variable is the natural logarithm of one plus the total number of employees hired by the new establishments. Column 1 includes both standalone establishments and subsidiaries. Column 2 includes only standalone establishments. Columns (3) to (6) include the standalone establishment of different size groups, with size defined by the number of employees in the year of inception. Column 7 includes only subsidiary establishments. *Post* is a binary variable that equals one after the passage of just cause ordinances. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	All	Standalone					Subsidiary
		All standalone	Small (1-5)	Small (6-10)	Medium (11-20)	Large (20+)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	0.039	0.080	0.072**	0.155**	0.109	0.204	0.142
	(0.98)	(1.45)	(2.57)	(2.25)	(0.78)	(0.64)	(0.44)
County×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.952	0.960	0.980	0.880	0.754	0.665	0.803
Observations	1,080	1,080	1,080	1,080	1,080	1,080	1,080

Table A.7: Just cause eviction law and other city economic characteristics

This table presents estimates of DiD model of other city economic characteristics. The sample consists of two snapshots of city-level economic characteristics based on the 2000 Census and the 5-year estimates (2005-2009) from the American Community Survey. The dependent variables include *ln(Population)*, *ln(Median property value)*, *ln(Median household income)*, and *Poverty rate*. *Post* is a binary variable that equals one after the passage of just cause ordinances. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	ln(Population)	ln(Median property value)	ln(Median household income)	Poverty rate
	(1)	(2)		
Post	-0.014	0.042	0.006	-2.020***
	(-0.66)	(0.64)	(0.54)	(-3.94)
County×Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.999	0.963	0.994	0.955
Observations	240	240	240	240

Table A.8: Just cause eviction law and firm movements

This table presents estimates of DiD models of firm relocation. The dependent variables are the natural logarithm of one plus the number of establishments that moved in and moved out in a city-year. *Post* is a binary variable that equals one after the passage of just cause ordinances. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	Ln(1+Number of establishments)	
	Move in	Move out
	(1)	(2)
Post	-0.007	0.011
	(-0.11)	(0.18)
County×Year FE	Yes	Yes
City FE	Yes	Yes
Adjusted R^2	0.958	0.957
Observations	1,080	1,080

Table A.9: Just cause eviction law and new firms: tradable v.s. non-tradable

This table presents estimates of DiD models of the number of new firms (Model (2.2)). The dependent variables are the natural logarithm of one plus the number of new standalone establishments in tradable and non-tradable industries. *Post* is a binary variable that equals one after the passage of just cause ordinances. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	Ln(1+New standalone establishments)	
	Tradable	Non-tradable
	(1)	(2)
Post	0.136**	0.111***
	(2.38)	(3.08)
County×Year FE	Yes	Yes
City FE	Yes	Yes
Adjusted R^2	0.919	0.960
Observations	1,080	1,080

Table A.10: Just cause eviction law and demand for home mortgage

This table presents estimates of DiD models of demand for home mortgage loans. For each city-year, we count the number and the amount of new home mortgage applications provided by the HMDA database. In Column 1, the dependent variable is the natural logarithm of one plus the number of new home mortgage applications. In Column 2, the dependent variable is the natural logarithm of one plus the total value of the applied home mortgage. *Post* is a binary variable that equals one after the passage of just cause ordinances. We include county × year fixed effects and city fixed effects in the regressions. We report t-statistics using standard errors clustered by city in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	Ln(1+Number of home mortgage loans)	Ln(1+Total loan amount)
	(1)	(2)
Post	-0.061	-0.114
	(-0.62)	(-1.05)
County×Year FE	Yes	Yes
City FE	Yes	Yes
Adjusted R^2	0.889	0.884
Observations	934	934

Table A.11: State-wide just cause eviction law, renter household, and employment status

This table presents estimates of Model (2.4) using household survey data from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey. The sample consists of all adult individuals in the U.S. covered by the survey from 2013 to 2022. The dependent variable is a binary variable that equals one if the person is reported as self-employed in the survey year. *Renter* is a binary variable that equals one if the household rents its residence. *Post* is a binary variable that equals one for person-year observations in states that have passed a state-wide just cause eviction law. We include state and year fixed effects in Column 1 and state×year fixed effects in Column 2. We report t-statistics using standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent variable:	Self employment	
	(1)	(2)
Renter × Post	0.1808**	0.1829**
	(2.19)	(2.10)
Renter	-0.3192***	-0.3201***
	(-6.32)	(-6.27)
Post	-0.0852**	0.5768***
	(-2.27)	(5.64)
Age	0.0371***	0.0371***
	(68.43)	(67.44)
Female	-0.5304***	-0.5307***
	(-55.54)	(-55.41)
Nonwhite	-0.3695***	-0.3690***
	(-10.63)	(-10.80)
Education	0.0669*	0.0669*
	(1.85)	(1.85)
State FE	Yes	No
Year FE	Yes	No
Household FE	No	No
State × Year FE	No	Yes
Pseudo R^2	0.065	0.066
Observations	584,601	584,601

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