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ABSTRACT

This dissertation consists of three chapters that each study the interaction between government policy and real estate markets. All three chapters are connected by a broad interest in renters, landlords, and rental markets.

Chapter 1 investigates the relationship between place-based policies and real estate and rental markets empirically by studying a large, spatially targeted education policy change in California. Specifically, I exploit the funding formula of California's largest school finance reform in decades, the 2013 "Local Control Funding Formula" (LCFF), to estimate the effects of increased school funding at the local level. My estimates indicate an additional \$1,000 per student in annual state grants increased local house prices by approximately 9%. The policy induced migration and increased total population, the number of children, and average family size in communities that received grants. Funding increases did not capitalize into rental prices during the study period (2013-2018), likely due to compositional effects of households that migrated because of the reform. These findings provide the first causal estimates of school finance reform on both rental and real estate markets and suggest the benefits of place-based policies are not necessarily offset by higher rents.

Chapter 2 studies the effects of two of the most acute US government interventions in the rental housing markets in recent history: the COVID-19 eviction moratoria and the related emergency rental assistance (ERA) programs. Eviction moratoria and ERA programs were prominent policies implemented to assist families negatively impacted by the pandemic. I exploit the heterogeneous timings of state-level moratoria to determine their effects on tenant evictions and landlord financial distress. I find evidence eviction moratoria reduced tenant evictions, but increased

the rate of financial distress among landlords. These effects are most pronounced in states where the local moratorium expired before the ERA program could commence. In such states, rental properties were 2.1% more likely to be sold during the pandemic. This suggests the ERA program likely had an important moderating effect on landlord sales and foreclosures during the COVID-19 pandemic.

Finally, in Chapter 3, I turn my attention to policies set by homeowners and condominium associations. Approximately 60% of new US single-family construction is part of a homeowners association. These private associations collect monthly or annual fees from residents, have broad power to set rules and regulations for the community, and often own significant portions of land or common elements on behalf of the community. Membership is typically compulsory and transferred to new owners upon the sale of an existing property. I study common covenants, rights, and restrictions among New York City and Chicago associations to determine whether these non-governmental regulations have a material impact on housing availability, financing, or use. I particularly focus on restrictions that limit the ability to rent units.

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CHAPTER 1

The Local Effects of Spatially Targeted Public Policies: Evidence from California School Finance Reform

1.1. Introduction

In the United States, public K-12 education is closely tied to geographic location. California is one of many states where students are assigned to a school based on their location of residence. These school districts have strict boundaries with independent budgets, facilities, and staff. Unsurprisingly, school district quality varies across the state depending on individual district characteristics. Because of the connection between place of residence and the resulting school district education quality, local housing markets are closely linked to local school districts. Policy changes in the local education system can have significant effects on real estate markets (Hilber, 2017).

The 2013 Local Control Funding Formula (LCFF) increased state aid to districts by nearly $20 billion per year and is the largest school finance reform (SFR) in California in the last 40 years[1]. Because one major policy objective of the reform was to equalize education quality between school districts, this funding was not distributed evenly across the state. Instead, the bulk of this funding was allocated to a set of qualifying districts with high levels of student poverty and many English learners. These grants were very large compared to existing district budgets: some school districts received a windfall increase from the state of around 50% of their 2012 budget, while others received relatively little.

[1]Wolf and Sands (2016) provide a summary of previous California SFRs and the policy context of LCFF.

Because the LCFF funding was spatially restricted in how it could be used by districts, LCFF acted as a type of place-based policy. The potential real estate market outcomes of policies like LCFF have been debated in the literature (Glaeser and Gottlieb, 2008; Kline and Moretti, 2014a; Gaubert et al., 2020). LCFF may have had a large effect on local real estate markets because homeowners and renters value additional K-12 education funding. It may also have induced migration and sorting by households as housing costs and education quality adjusted. The extent of these effects is an empirical question (Chaurey, 2017; Barbieri et al., 2020).

Of particular importance in the context of LCFF are potential adverse consequences for renters: renters tend to be over-represented in the high-poverty districts targeted by LCFF[2]. Wyckoff (1995) argues that place-based aid similar to LCFF can have negative consequences for renters. The argument is that if disadvantaged students tend to be renters and the additional education funding capitalizes into rental prices, they may be forced to pay higher rents or move. If this is indeed true, then the LCFF policy may be ineffective at obtaining its distributional objectives.

To answer these questions, I estimate the effect of increased K-12 education funding on local housing and rental markets by exploiting the funding formula of LCFF. The adoption of LCFF provides a particularly clean setting to measure and analyze these real estate market effects. California has unique laws that fix property taxes at the state level, so the implementation of LCFF was not accompanied by local taxation changes. The method in which LCFF was allocated was clear, transparent, and difficult to manipulate. And the transition to LCFF was rapid and smooth.

Because the funding formula introduced by LCFF provides additional grants to districts with a large proportion of students learning English or in poverty, many districts qualified for hundreds or thousands of dollars of additional funding per student. I rely on a kink in the funding formula that determines which districts qualify for these additional grants in order to identify the effects of the policy. The kink is large, and visually represented in Figure A.1. I use a difference-in-difference design

[2]According to the 2018 US Census Bureau ACS, in the average high-poverty district approximately 40% of households are renters.

to compare the outcomes after the policy change between districts that barely qualified or barely did not qualify for a particular type of grant called a "concentration grant". Under standard assumptions, this difference-in-difference estimate identifies the policy effects. To alleviate potential concerns about confounding housing market trends, I supplement these results with a regression kink design (RKD) that uses the differences in treatment intensity above and below the kink to provide identification.

I find LCFF funding caused a large increase in house prices. This is graphically evident in Figure A.4. A $1,000 per student increase in annual funding increased house prices by approximately 9%. These house price increases seem to have been driven by the migration of households with school-aged children and improved education quality. I estimate receiving an additional $1,000 per student improved graduation rates by about 1.4%. This is a sizeable increase in education quality. Receiving additional funding also led to migration. Overall population increased in the local areas that received grants, as well as average family size and the total number of children. The share of non-English speakers also increased and household income grew more slowly in these areas after the policy. This is consistent with families who placed a higher value on education sorting into communities targeted by LCFF.

While LCFF caused house prices to rise, these price increases did not pass through to renters. Instead, I find the policy decreased rental prices in the areas that received additional funding. My negative estimates of rent prices may be explained by slow household income growth in treated areas after the policy is implemented, possibly due to migration and compositional effects. My estimates indicate qualifying for additional funding decreases local average household income growth from 2013 to 2018 by about $3,000. Considering the close relationship between local wages and local rental prices described by Moretti (2010), the slow rental price growth after the policy is implemented is likely tied to slow income growth. Regardless of the cause, the lack of measured rental price increases from the policy indicate that the policy aims were not offset by rental prices.

The motivation for analyzing the real estate effects of LCFF is related to a long line of research emphasizing the role of local public goods in real estate prices and resident-community sorting. While the initial theory was first developed by Tiebout (1956) and Oates (1969), numerous other studies have extended their theoretical work as well as estimated the effects of specific types of public goods[3]. A particular emphasis has been placed on the role of K-12 education, and Hilber (2017) provides a recent review of this literature. While much attention has been given to the value residents place on education quality, previous research does not look directly at the real estate market effects of SFRs. Instead, most studies focus on existing education quality differences between districts by, for example, comparing house prices near school district boundaries (Black, 1999; Gibbons et al., 2013; Davidoff and Leigh, 2008; Gibbons and Machin, 2003). Very little is known about how housing markets, specifically rental housing markets, react to a place-based education policy change such as LCFF.

Recent academic research has suggested reevaluating the "traditional skepticism towards place-based policies" (Austin et al., 2018) and a surge of publications have focused on the theory of spatially targeted or place-based policies (Gaubert et al., 2020; Kline and Moretti, 2014b; Fajgelbaum and Gaubert, 2020; Ehrlich and Overman, 2020). But much of this work focuses on preferential tax treatment to certain areas, such as via economic development zones[4]. Here, I empirically analyze the effects of a spatially targeted policy in the spirit of previous work, but instead consider a government transfer to residents in the form of increased education funding.

This paper also contributes to the well-developed but conflicted literature on the relationship between school funding and education quality. Hanushek (2003) notes that research prior to his study points to a weak or non-existent causal relationship between funding and quality. But more recent literature argues the opposite[5].

[3]See *inter alia* the real estate price impacts of air quality improvements (Banzhaf and Walsh, 2011; Grainger, 2012), school district infrastructure investments (Cellini et al., 2010), park renovations (Livy and Allen Klaiber, 2016), and subway accessibility (Sun et al., 2015)

[4]See, e.g., Busso et al. (2013); Ham et al. (2011)

[5]For example, Guryan (2003) finds test scores improved in low-income areas after SFRs. Card and Payne (2002) find decreased SAT score gaps between high- and low-income areas after SFRs were implemented. Jackson, Johnson, and Persico (2016) present evidence that SFRs across the United States led to improved long-run student outcomes

Overall, the empirical evidence of the effects of increased school funding on student outcomes is mixed. The estimates of the graduation rate increases from LCFF in this paper provide additional evidence that funding increases can lead to real education quality gains.

Even though this paper analyzes the regional effects of a specific SFR, many place-based public policies that result in spatially heterogeneous increases in the provision of public goods could lead to similar regional effects. The implications of this paper extend beyond the analysis of SFRs or the economic effects of an improvement in educational quality to place-based policies in general. The sizable second-order effects of place-based policies are important to consider when evaluating policy proposals.

The remainder of the paper proceeds as follows. Section 2 presents the policy context and California's education system before and after the LCFF reform. I introduce the data that I use in Section 3. I share my empirical design and results in Section 4. Section 5 concludes.

1.2. Policy Context

California's K-12 education system is similar to the rest of the United States: local school districts have considerable autonomy and are responsible for all of the teaching and day-to-day management, with the state reserving some control via mandates and regulations surrounding required curriculum and programs, administrative requirements, and finances. Most school districts span one or more towns or cities, are in charge of the educational instruction of thousands of students, and manage hundreds of employees across many campuses. They hire teachers, administrators, and staff, negotiate salaries, manage facilities, decide how to meet state curriculum requirements, and have broad discretion on how to spend their funding.

One aspect in which California's K-12 education system departs from its peers is in the way finances are collected and dispersed. School districts in California receive the bulk of their general instruction budget from local property taxes and the state. They also receive some funds from federal and state sources, much of which are restricted for specific purposes such as special education, teacher professional development, school buses, and other designated programs.

Unlike most other states, local school districts in California have virtually no authority to adjust revenues by levying taxes. Due to a series of California Supreme Court cases in the 1970s that challenged large gaps in school funding between rich and poor communities, California implemented an education finance system called "Local Revenue Limits" (LRLs). This system, in combination with a 1978 constitutional referendum called "Proposition 13" that locked the property tax at 1% statewide, resulted in an education finance system where the state has virtually exclusive control over K-12 education funding.

Under the LRL system that lasted from the 1970s until 2013, the state calculated a "revenue limit" for each school district's general instruction budget. School districts were to raise the money for their revenue limit through local property taxes (fixed at 1% of property assessment values by Proposition 13), with the important caveat that if total property tax revenues were below the revenue limit, any gap would be made up by the state in the form of a grant from the state's general budget. In this way, even school districts in poor areas with low property tax revenues would be guaranteed an amount of funding similar to the level in other districts. Revenue limits were set such that nearly all districts had a revenue limit higher than their actual property tax revenues: 90% of all school districts in 2010 required state assistance to meet their revenue limits. In essence, revenue limits had become de facto general budget entitlements for the vast majority of school districts.

Each district's revenue limit was based off a complex formula that included the number of students enrolled, historical funding levels for the district, and a local cost of living adjustment. While facially its purpose was to provide an equal amount of instruction budget to each district on a per student basis, over the decades the funding system grew into a chimera of political agendas and priorities that competed with the premise of fiscal equality (Timar, 1994).

After four decades of modifications, policymakers felt the LRL system was in need of major reform and simplification. In 2012, California predicted a surprise budget surplus due to a strengthening post-recession economy. California's governor and legislature used this as an opportunity to introduce a law overhauling the LRL system into the "Local Control Funding Formula" (LCFF) which would begin taking effect for the 2013-2014 school year.

LCFF primarily altered the formula used to compute the limit for each district and did not majorly alter the structure of the funding system created by LRL. The LCFF budget was essentially LRL but under a new name and formula: it was still funded from local property taxes, with the state making up any shortfall, still the majority of the districts' operating budgets, and still kept the proportion of school districts that relied on state grants to meet their calculated limit at around 90%.

The formula that calculated the minimum funding level guaranteed to a district underwent two major modifications with LCFF: (1) each district was now allocated a base funding amount per student that was around 50% higher than under the LRL system, and (2), essentially the only variation in funding per student between districts was via grants for districts with disadvantaged students. LCFF required each district to report the number of students who fell into at least one of the following categories: English Learners (ELs), recipients of the National School Lunch Program (NSLP) which provides food for low-income students, and foster youth. The count of the number of students who qualify in at least one of these categories is referred to by the state as the "unduplicated pupil count" (UPC) for the district. The proportion of students in the district that fall into one of these categories out of all the enrolled students is referred to as the "unduplicated pupil proportion" (UPP). Districts receive additional funding based on their UPP under LCFF.

The calculation of each district's LCFF begins with a set amount of base funding per student. This base funding in 2018 is approximately 50% higher than the average funding per student in 2012 under LRL. Then, districts with disadvantaged students are given additional LCFF allotment per student in the form of supplemental grants and concentration grants. A supplemental grant increases a district's LCFF by 20% above the per student base funding for each student that qualifies as disadvantaged.

Districts with more than 55% of students considered disadvantaged qualify for additional funding above the supplemental grant in the form of a concentration grant. Each of the students over the 55% UPP threshold qualify the district to receive an additional 50% funding for these students as a concentration grant.

In mathematical notation, the total LCFF funding amount for a district can be calculated as follows:

$$\Big(G + (20\% \times G \times UPP) + \Big(50\% \times G \times \max(0, UPP - 55\%)\Big)\Big) \times N$$

Here, G is the base grant per student and N is a measure of the number of students.

The introduction of concentration grants that phase in at a 55% UPP threshold provides a natural kink to use for identification in an RKD empirical setup. Details of the empirical work are in the following sections, but before moving on a few more notes about the policy context are important.

The base funding levels per student under LCFF were set such that all schools saw an increase in the general instruction budget funding. The change to LCFF should be viewed as giving every district at least as much general instruction budget as they had before, with the percentage increase in funding for the district essentially a function of UPP.

Finally, the change to LCFF was rolled out over time with the bulk of the transition completed by the 2016-2017 school year. Each year school districts calculated both the funding they would receive under the old LRL policy (called the ”floor”) and the funding they would receive if LCFF was fully funded (called the “target”). The state supplied a different amount of this “gap” between the target and floor each year based on state tax revenues with the goal of fully funding the target by around 2020. In 2013-2014, only 12% of this gap was funded. But by the 2016-2017 school year, the gap was virtually eliminated: 96% of the target was funded in 2016-2017 and in 2018-2019 the target was fully funded and the transition was complete. This rollout was equal across all districts but introduces a source of potential delay in the outcome variables.

1.3. Data

This paper makes use of three different sets of data: California school district statistics and reports, census data, and real estate records.

California school districts are required to publicly disclose many statistics and financial details each year. Much of this data is aggregated by the California Department of Education (CDE) and its non-profit partner, Ed-Data. I use CDE and Ed-Data as the source for all data on California school districts, such as enrollment, graduation rates, financial information, and teacher statistics. I additionally use CDE data for the GIS geographic boundary data used to determine the school district(s) associated with each ZIP code.

EdData reports 1,041 school districts in California as of the end of the 2018-2019 school year, but a large number of these school districts are not suitable for inclusion in this study due to their small size, special status, or other factors. Out of the full sample of school districts, roughly 100 are County Offices of Education or other special education agencies, which I exclude these from the study.

I also choose to restrict my analysis to a subset of school districts that are either unified (K-12 grades) or high school (9-12 grades). California has approximately 520 school districts that serve only elementary students. I exclude these districts for three reasons. First, high school graduation rates are not applicable to these districts, so it is difficult to measure education quality changes with my desired metric. Second, since both a high school district and an elementary district serve these residents, there is a possibility effects are confounded by the two separate treatments. Finally, I choose not to look at elementary school districts under the plausible assumption that residents place a different value on high school level education quality than elementary. After removing elementary school districts I am left with 344 unified districts and 76 high school districts (420 total).

These 420 districts exhibit wide variation in size. Los Angeles Unified School District is the largest in the state with over 500,000 students. On the other hand, 65 districts have less than 1,000 students and 8 districts have less than 100. Such districts and their communities are significantly different from the urban and suburban areas that are the focus of this paper. For example, fixed overhead costs make up a larger relative part of total budgets for these school districts. Real estate markets are illiquid and price indices are difficult to measure. With sometimes less than a dozen district employees, adjusting labor is subject to larger frictions than in an urban school district. Because of these concerns, I also exclude districts with under 1,000 students from the analysis. This ensures the sample is most representative of areas where the bulk of the population lives.

I match these districts with financial and district demographic data from the CDE and EdData. After matching and filtering on size, my dataset contains 342 districts: 282 unified and 60 high school. My final filter is to remove a small subset of districts that do not rely on state funding to meet their LCFF allocation. These "Basic Aid" districts were not affected by LCFF in the same way since they do not receive the same state assistance. After this final filter the dataset contains 49 high school districts and 262 unified districts for a total of 311 districts accounting for approximately 4.5 million of California's 6.2 million K-12 students.

I report sample statistics from my subset of districts in Table A.1. One important feature of the data is a relatively large number of students that are considered "disadvantaged" by the state. The UPP variable is the proportion of students who qualify for the National Free and Reduced Lunch Program (NFRLP), are a foster youth, or are an English Learner (EL). The requirements for each category are strict and school districts are not able to easily manipulate this count. The median UPP is 64.8%; the distribution is smooth around the 55% threshold, a necessary requirement for RKDs.

In terms of funding, school districts are required to report detailed annual budget data to the CDE. School districts receive funding from national, state, and local sources. LCFF funding comprises the bulk of a district's general instruction budget and a large proportion of the total budget. Figure A.7 plots the average proportion of total funding from each source across school districts from 2010 to 2018. I calculate the combined LCFF budget of all California school districts was approximately $58.1 billion in 2018, an increase of about 80% over the $32.2 billion LRL total budget in 2012.

In California, school districts have strict geographic boundaries, allowing me to assign each ZIP code to one or more school districts that cover the geography of the ZIP code. I match the school district data with census and real estate deed data at the ZIP code level by using CDE GIS shapefiles for the school districts and US Census shapefiles for ZCTA codes, a close approximation for ZIP code boundaries. These shapefiles allow me to construct a virtual map with the boundaries of the districts and ZIP codes from the shapefiles. I can then determine which school district is associated with each ZIP code by looking at the intersection. I calculate the proportion of the school district covered by each ZIP code by land area and use the weighted average of ZIP code house price indices or other characteristics for my analysis.

The resulting dataset of ZIP codes merged with the associated school district data contains 1,271 ZIP codes. This covers most California residents: I estimate my dataset covers the ZIP codes of 31 million Californians (roughly 80% of the total state population). Table A.2 presents summary statistics of the matched subset of California ZIP codes.

For ZIP code level data, I use two categories of data: US Census data, primarily from the American Community Survey (ACS), and real estate data, primarily from Zillow and Corelogic. My main results use Zillow House Value Indices (ZHVI) and Zillow Rent Index (ZRI) at the ZIP codes level due to their procedure that accounts for repeat sales and seasonality. Using Corelogic to construct mean and median annual house price indices at the ZIP code level produced qualitatively similar results. Because the results were similar and ZHVI and ZRI are freely available to the public while Corelogic is proprietary, I present the results using the Zillow indices.

1.4. The Local Effects of LCFF

This section presents my main empirical results. I first describe the empirical design used to identify the effects of the policy. I then examine the effects of LCFF on house and rental prices. I follow this by studying the effects of LCFF on local school district funding and education quality, to ensure the real estate market effects are a response to a real, underlying change driven by LCFF. I conclude with estimation of the local demographic effects.

1.4.1. Identification

The primary challenge to estimating the local effects of LCFF is identification. Because the intensity of treatment is a function of local characteristics, a simple ordinary least squares (OLS) approach may produce biased results if the outcome variables are correlated with school district characteristics. For example, if house prices tended to rise over this period in high-poverty areas for reasons unrelated to LCFF, an OLS regression of house prices on treatment intensity would conflate the effects of the policy with this general trend.

To identify the effects of the policy, I use two well-established methods. First, I use a difference-in-difference design and exploit the 55% UPP cutoff for concentration grants. Under the assumption that districts just below this threshold are similar to districts just above this threshold, but knowing that only districts above 55% UPP qualify for Concentration Grants, any different trends between these two groups of districts after the policy change can be attributed to the effects of the policy.

My second approach to identification is a regression kink design, or RKD. RKDs are similar to more common regression discontinuity (RD) designs. Like an RD, an RKD looks at the gap between outcomes on two sides of a breakpoint. While RDs look at the difference in levels of an outcome variable on both sides of a structural break, an RKD looks at a difference in the slope of an outcome variable on both sides of a break. The slope is estimated by fitting a polynomial to the outcome variable of interest on each side of the break, or kink. I use the RKD approach developed by Calonico et al. (2014) (CCT) to compute the optimal bandwidths for estimation.

RKDs are highly sensitivity to the selection of the bandwidth parameter (Calonico et al., 2014). Because my sample size is particularly small, my RKD estimates are especially sensitive. The RKD results, which generally agree with the difference-in-difference results, are presented as a robustness check on the difference-in-difference results.

1.4.1.1. Difference-in-difference. Both the difference-in-difference and RKD approaches I use to identify the effects of the policy change rely on the new funding formula for school districts introduced under LCFF. Under LCFF the general instruction budget for each district is calculated with the following formula:

$$\Big(G + (20\% \times G \times UPP_j) + \big(50\% \times G \times \max(0, UPP_j - 55\%)\big)\Big)N_j \tag{1.1}$$

Under the previous system, LRL, the general instruction budget for each district was:

$$(R + \varepsilon_j)N_j \tag{1.2}$$

Where $R < G$ is the base grant under LRL and ε_j is an error term for the district with a mean of zero. Because of various "adjustments", some political in nature, the base grant in LRL was not exactly equal across all districts. The error term incorporates these small and uncorrelated idiosyncratic differences. The large majority of school districts had very similar per-student budgets within a few percentage points of the mean.

Under the simplifying assumption that N_j does not change, the increase in funding for a particular district is then calculated by subtracting (2) from (1):

$$F = \Big(G + \big(20\% \times G \times UPP_j\big) + \big(50\% \times G \times \max(0, UPP_j - 55\%)\big)\Big) \times N_j - (R + \varepsilon_j) \times N_j$$

This simplifies to:

(1.3)

$$F = (G-R-\varepsilon_j)\times N_j+(20\%\times G\times UPP_j)\times N_j+\Big(50\%\times G\times \max(0, UPP_j-55\%)\Big)\times N_j$$

The expected funding change thus consisted of two parts: an increase in the base grant from R to G that is constant in expectation for all districts and an increase in funding due to supplemental and concentration grants that is proportional to a district's UPP and therefore varies by district. For a district with $UPP = 0$, the expected change in funding on a per-student basis is simply $E[G-R-\varepsilon_j] = G-R$. For a district with $UPP = 1$, the expected change in funding per student is $1.475G - R$ due to the supplemental and concentration grants.

The difference-in-difference technique compares the outcomes in two groups after the policy change. For LCFF it is natural to compare districts that barely qualified for or barely did not qualify for concentration grants. That is, by comparing the outcomes of districts somewhat above the 55% concentration grant cutoff after the policy change with districts somewhat below the cutoff, the causal impact of the concentration grants can be determined.

I estimate district-year difference-in-difference regressions of the following form on the set of districts with a 2012 UPP within a bandwith h of the 55% UPP threshold for concentration grants:

$$y_{dt} = \alpha_d + \delta_t + \beta \cdot I\{UPP_d > 0.55\} \cdot I\{t > T\} + \varepsilon_{dt} \tag{1.4}$$

The variable y_{dt} represents an outcome for state d in year t, such as the graduation rate. The model includes district fixed effects (α), year fixed effects (δ), and an error term (ε) that by assumption is uncorrelated with the outcome variable. The variable T represents the time cutoff after which the policy takes effect.

The coefficient of interest is β, which is the difference-in-difference estimate of the effect of additional funding through concentration grants. The coefficient is identified by comparing outcomes in districts that received concentration grants after LCFF took effect to outcomes in these same districts before LCFF and to other districts that did not qualify for concentration grants. This identification holds under the assumption that outcomes in districts with concentration grants would not have evolved differently from other districts in the absence of LCFF. When the bandwidth h is small, this assumption is plausible as districts in the treatment group are similar to districts in the control group.

1.4.1.2. RKD. The important feature of (3) is the kink in funding around the breakpoint of 55% UPP. As a district approaches 55% UPP, the derivative $\frac{\partial F}{\partial UPP} = 0.2$. After it passes 55% UPP, the derivative $\frac{\partial F}{\partial UPP} = 0.7$. This treatment formula lends itself particularly well to an RKD. It is also fortuitous that the median school district UPP is approximately 62%, very close to the 55% breakpoint, providing a suitable number of observations close to the kink.

For the RKD approach to be valid, there must be no manipulation by districts of their UPP. Manipulation seems infeasible, since district UPP is strictly defined and based on measures that are difficult to falsify. Additionally, the relationship between district characteristics and the outcome variables must not be kinked, with the exception of UPP and outcome variables. Simple inspection of these relationships suggest this is not the case for observable characteristics, and there is no reason to assume unobservable characteristics have such a kinked relationship.

1.4.2. House Price Effects

I use the difference-in-difference and RKD methods previously described to estimate the effect of LCFF on local house prices. Here, my unit of observation is the school district level. I use the Zillow House Price Index (ZHVI) at the ZIP code level and aggregate this index to the school district level by taking the average, weighted by the amount of geographic overlap between the ZIP code and school district.

I find that LCFF had a large and statistically significant causal effect on house prices. Table A.3 presents the results of the difference-in-difference model estimation. Here, the effect of qualifying for a concentration grant (approximately $1,000) is a house price increase of about 9.3%. This result is large and highly statistically significant using both traditional and clustered standard errors. When using a smaller bandwidth around the kink, the estimate remains approximately the same. A graphical representation of the difference-in-difference estimation is presented in Figure A.4.

The RKD estimation also indicates LCFF had a large impact on house prices. I fit the RKD model using two bandwidths: the CCT optimal bandwidth of 16.3% and a tighter bandwidth of 10%. These estimates indicate that qualifying for a concentration grant increased house prices between 4.7% and 10.2%. This is roughly similar to the effect size estimated with the difference-in-difference model and is also statistically significant. The full results of the RKD house price estimation are presented in Table A.4. A figure showing the estimated kink visually is provided in Figure A.5.

I also estimate the house price effect of LCFF for each year individually to study the dynamics of the capitalization over time. These results are presented in Figure A.6 and indicate that the capitalization proceeded slowly over at least five years.

1.4.3. Local Demographics

The theories of Tiebout (1956) and Oates (1969) predict LCFF should have significantly impacted migration and resident sorting. According to these theories, as households learned of increased education funding, those who valued these changes should be induced to move to these communities. At the same time, in-migration should cause house prices to rise and those who did not value the education funding increases should sort into different communities. It is plausible to assume that families with school-aged children are likely to sort into affected school districts since these residents are the most likely to make use of the higher levels of public goods. At the same time, seniors and families without children are likely to sort out of these communities as house prices rise and they have little use for these amenities.

To test these theories, I use the Census 5-year ACS data with the previously defined difference-in-difference model to estimate the effects of LCFF on various local demographics. My estimates confirm that qualifying for LCFF concentration grants affected migration and sorting. I report these estimates in Table A.9. I find concentration grants increased the amount of families with school-aged children living in district boundaries. Overall population in the school district boundaries increased by an average of 630 residents. Many of these residents were children: I find the percent of all residents that are children increases by 0.6%, while the percent of all residents that are seniors falls by approximately the same amount. Families also tend to be larger in areas targeted by LCFF. I estimate average household size increases by 0.05 persons.

Additionally, I find LCFF affected the *types* of families that sorted into targeted communities. The proportion of residents that identify as Hispanic rose significantly: qualifying for concentration grants increased this proportion by approximately 8%. The proportion of families that did not speak English increased by 1.3%. LCFF decreased average income growth from 2012 to 2018 by $3,055.

To understand these results, it is important to remember that LCFF funding was intended to be used to assist students living in poverty and English learners. While state controls on spending were weak, the majority of districts seem to have used the funding to improve educational outcomes for these disadvantaged children. If families with disadvantaged children learned of school districts with stronger support programs, it is reasonable to assume they may have valued these locations more highly than other residents.

These demographic results tell a compelling story that LCFF induced disadvantaged families with school-aged children to move into areas that received additional funding. Families who placed a high valuation on education quality and school funding replaced seniors, who likely placed a lower valuation on K-12 education. This sorting, in line with the Tiebout hypothesis, drove the increased house prices in areas that received additional grants after the LCFF policy change. At the same time, the disadvantaged households, who tended to be renters, saw slower income growth than their peers. This slow income growth will be important to explain the measured rent price effects in the next section.

1.4.4. Rental Markets

While the home purchasing and rental markets are closely linked, the rental market reacted differently to the transition to LCFF than the purchasing market. I find no evidence that LCFF capitalized into rental prices. On the contrary, both difference-in-difference and RKD point estimates indicate rental price growth slowed due to the funding provided by LCFF. I estimate receiving approximately \$1,000 of additional funding decreases rent growth from 2013 to 2018 by around 4.6%. Similar to the house price effects, the difference-in-difference results are presented in Table A.5, and the RKD results are in Table A.6. Figure A.10 compares the difference-in-difference treatment and control groups over time, and the size of the RKD estimate over time is shown visually in Figure A.11.

This result, that LCFF decreases rent prices, runs counter to classical capitalization theory. However previous empirical research on place-based policies such as air quality improvements and tax incentives have found limited or no effects on rent prices (Chaurey, 2017; Grainger, 2012). These other empirical findings, in combination with my results on migration, suggest the negative effects are likely driven by compositional effects. I find the areas that received additional funding tended to see an increase in non-English speaking minorities and have slower household income growth from 2013-2018. Because rental prices are closely linked to local wage growth (Moretti, 2010), the migration may have indirectly led to slower rent price growth in the short-run.

1.4.5. Funding and Education Quality

I also verify that LCFF increased funding in districts in line with Eq. (3) as planned, and that this increase improved education quality. Under the Local Revenue Limit system, each district should have received approximately the same amount of LRL funding per student, with no dependence on the district's UPP. After LCFF was implemented in 2013, districts with higher UPP should receive more funding per student. Figure A.1 indicates that this formula is correct by plotting each district's LCFF per student against the district UPP. Most districts lie on or close to a line that is relatively flat to the left of the 55% UPP threshold and steeper after the threshold[6]. Figure A.2 presents a similar graph, but instead showing the percent change in funding from 2012 to 2018. This figure bins observations by small bandwidths, a typical approach in RKDs.

The additional funding was spent roughly in line with previous spending. For each dollar of additional funding, the average school district spent 55% on instruction, 22% on services, 10% on facilities, and the remainder to various administrative and other overhead categories. This spending breakdown is approximately the same for the highest quartile of disadvantaged school districts as for the average from all the districts.

I estimate the effects of the additional funding on education quality, as measured by cohort graduation rates. I plot the change in cohort graduation rates over time for districts within 15% UPP of the concentration grant threshold in Figure A.8. The gap between the districts above and below the threshold narrows after the introduction of LCFF.

This effect is formally estimated using both a difference-in-difference and RKD model. Difference-in-difference results are reported in table A.7 for different values of h and T, the policy time cutoff. Because cohort graduation rates are likely to lag behind the policy change, I show results for various values of T. I find significant differences between the treatment and control groups in cohort graduation rates for both bandwidths.

[6]These figures leave out the small number of "basic aid" districts that would otherwise appear as outliers above the trend line. Wealthy areas that receive more tax revenues than their LRL/LCFF calculated allotment and are allowed to keep the funds (and receive no state aid) and are titled basic aid districts by the state.

My preferred model estimates the effect of receiving a concentration grant is a 1.44% increase to the cohort graduation rate. The size of the effect estimated using RKD is a similar 1.46% increase in the cohort graduation rate.

The graphical representation of the change in graduation rates is presented as Figure A.9 and demonstrates a clear kink around the threshold. Table A.8 presents the RKD results.

1.5. Conclusion

Place-based public policies target specific areas or communities as the recipients of aid, typically through funding or grants. Because the policies by definition concentrate on a geographic region, the policies are likely to have effects throughout multiple sectors of the region's economy.

Here, I analyze a 2013 school finance reform in California that targeted disadvantaged school districts. The effects on education outcomes are significant: graduation rates improved dramatically, particularly in the low-income school districts that were targeted. These effects are identified via an RKD that matches a kink in education outcomes with a kink in the funding assignment formula.

The effects of the LCFF reform do not stop with education. LCFF induced residents who placed a high valuation on K-12 education to sort into areas that were targeted by LCFF. These communities with large funding increases saw an influx of families with children, driving house prices higher. These house price increases took a number of years to manifest themselves, but seem to be large and statistically significant permanent increases in house prices. As of 2018, these house prices did not pass through to rent prices. While this indicates that higher rent prices did not offset some of the policy aims to provide educational goods to disadvantaged families, it is not clear why renters saw a decline in rent prices or if this decline will be permanent.

Overall, this paper indicates LCFF has succeeded in its primary goal of increasing education outcomes for disadvantaged students. In the process of targeting specific geographic areas for aid, it produced additional positive gains in welfare for local residents in the form of house price capitalization. Renters were not adversely affected by higher rent prices. However, it is unclear if the rental price effects found in this study hold in other policy situations. Additional future work is needed to investigate the pass-through effects of spatially targeted public goods on renters.

CHAPTER 2

Tenant Evictions and Landlord Financial Distress During the COVID-19 Crisis

2.1. Introduction

Beginning in March, 2020, state governments implemented two novel housing market interventions in response to the COVID-19 pandemic. A series of eviction moratoria and emergency rental assistance (ERA) programs became multi-billion dollar policy tools to fight the spread of COVID-19. These policies were passed rapidly, despite no prior history of implementation and almost no scholarship on expected impacts. With most of these programs now completed, it is vital to determine what, if any, effects they had. Thus, this paper focuses on the two groups most directly impacted by these new policies (tenants and landlords) and aims to understand how these interventions affected them.

An eviction moratorium is a temporary pause in the normal eviction process. The specifics of these COVID-19 eviction moratoria varied across jurisdictions, but most barred landlords from removing tenants from their properties, even when they failed to pay rent. In the United States, these moratoria covered nearly all renters across the nation: 43 out of 50 states implemented a moratorium at the state level. The federal government also passed two national moratoria.

Such dramatic policy changes to the landlord-tenant relationship has never previously occurred in modern history. Particularly striking is the suddenness of the implementation of these moratoria, the fact that they covered nearly all renters throughout the United States, the breadth of their application to almost every kind of eviction, and the large variance between states of the moratoria policy duration.

In some states, eviction moratoria ended months before the implementation of the ERA program. In others, moratoria continued well after.

In the public square and popular press, these moratoria were controversial. On one hand, some hailed the moratoria as necessary both for public health and to protect vulnerable populations from housing insecurity[1]. On the other hand, others linked such policies to adverse effects such as landlord financial distress[2] and stricter screening for renter applicants[3]. Since about one in three Americans rents their home and 41% of rental units are owned by individuals, the effects of the moratoria are potentially large. But debate continues over their value and effectiveness: at the time of writing six states continue to enforce local eviction moratoria, while 37 have chosen to let their moratoria expire.

Closely related to the eviction moratoria, beginning in December, 2020, the federal government also authorized an Emergency Rental Assistance (ERA) program. This program could pay landlords rent on behalf of renters negatively impacted by the COVID-19 pandemic. The programs were largely in response to concerns over the millions of households behind on rent and who possibly faced eviction when the moratoria expired[4]. While the moratoria prevented evictions, they did not prevent arrearages from accumulating. Total rental arrears as of January, 2021 were estimated at $57 billion[5].

This posed a problem not only for tenants behind on rent, but for landlords. Rental arrears can be difficult and slow to recover. Landlords potentially faced financial distress if they could not service debt, such as mortgages. Thus, the ERA programs were "aimed at keeping tenants in place without leaving landlords to swallow the cost."[6]

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[1]See this New York Times article from August 5, 2021

[2]See, for example, this article from the Urban Institute dated November 10, 2020, this Wall Street Journal article dated August 6, 2021 and this Los Angeles Times article dated April 5, 2021.

[3]For example, see this article from the Urban Institute.

[4]Per this Congressional Research Service Report

[5]Congressional Research Service Report

[6]Economist (2021)

On the whole, previous real estate research has little to say specifically when it comes to the potential impacts of a moratoria, to say nothing of the possible interaction between a moratorium and an ERA program. The literature is generally skeptical of government intervention into housing markets[7]. However, the unforeseen COVID 19 crisis forced consideration of such unstudied policies.

To better understand any adverse consequences of the COVID-19 eviction moratoria in the United States, I investigate their effects on both renters and landlords. I first analyze how effective the various eviction moratoria regimes were at reducing evictions in order to establish a baseline policy impact. Media accounts indicate that landlords and courts did not always comply with moratoria[8], so it is important to verify these policies were actually relevant to the eviction process. To this end, I leverage a detailed dataset assembled by the Princeton Eviction Lab on the universe of court-processed evictions in 30 large cities throughout the United States from January, 2020 to September, 2021.

I find local eviction moratoria were highly effective at reducing evictions. The presence of a local moratorium in a city was associated with 225 fewer evictions per week. When compared with the average city pre-COVID eviction baseline of 457 evictions per week, this is approximately a 50% reduction in evictions. Federal moratoria were considerably less effective: the presence of a federal, but not local, moratoria was correlated with only 29 fewer evictions per week. Against the average pre-COVID eviction baseline, this is only a 6% reduction in evictions. This suggests a large majority of evictions performed during the federal moratorium would have been prevented had a local eviction moratorium also been in place. I discuss certain policy details that may drive these differential effects in a later section.

Because an eviction moratorium eliminates eviction as a penalty for not paying rent on time, some tenants may have remained in rental units despite being unable to pay rent. Others may have strategically chosen to defer rent payments or neglect them entirely. If landlords relied on rental income to service debt obligations such as

[7]See, among many others, Sommer and Sullivan (2018), Cho and Francis (2011), Gervais (2002)
[8]See, e.g., this New York Times article and this NBC News article.

mortgages, non-paying tenants that could not legally be replaced may have induced or exacerbated financial distress among landlords.

If financial distress occurred through this channel, it is likely to have manifested itself via higher rates of foreclosures and sales among rental properties relative to rates in owner-occupied properties. Because various COVID-19 related policies limited the ability of lenders to foreclose on borrowers over this time period, I expect a relatively limited response in the foreclosure rate data. However, an increased rate of landlords selling rental properties during the pandemic may also indicate concerns over financial distress. This makes excess sales of rental properties a strong proxy for landlord financial distress.

In analyzing the relationship between these moratoria and excess sales of rental properties, I find a complex relationship. During a moratorium, a landlord may experience increased financial distress because tenants are less likely to fully pay all rent on time. However, the presence of a non-compliant and non-evictable tenant also makes it difficult for a rental property to be sold. I find suggestive evidence of this in the data, as rental properties experience a significantly increased likelihood of being sold the month following the expiration of a local eviction moratorium. This likelihood is increased according to an absolute measure, as well as relative to owner-occupied housing.

As mentioned previously, policymakers attempted to relieve this potential distress. Beginning in December, 2020, the federal government instituted an ERA program administered through the states. States were authorized to pay landlords rent on behalf of renters who could show they had been negatively affected by COVID-19. Many states allowed this funding to also be used for rental arrears. The introduction of this ERA program should have reduced landlord financial distress, since many non-paying tenants could now pay rent using state and federal funds. Thus, in areas where the moratorium ended before the ERA program began I expect to see a higher rate of rental property sales than in areas where the ERA program overlapped with the moratorium.

The empirical data supports this conclusion. I find a moratorium ending before the ERA implementation is associated with a 1.6 percentage point increase in the probability a rental property would sell during the COVID-19 pandemic. This is large and statistically significant. It suggests the ERA had beneficial effects for landlords, especially in areas where it was implemented before the moratorium ended.

These findings contribute to two distinct literatures. The first is a rapidly growing literature exploring the effects of the COVID-19 pandemic. Recent public health research by Nande et al. (2021) and Leifheit et al. (2021) suggests that the eviction moratoria reduced the spread of COVID-19 and the number of COVID-19-related deaths. Work by An et al. (2021b) indicates eviction moratoria improved household well-being, as measured by food security and mental health status. And another study by Ambrose et al. (2020) finds the CARES Act mortgage forbearance reduced tenant evictions. Many researchers also investigate the impact of the pandemic on household consumption and employment decisions (see e.g.Baker et al. (2020), Chetty et al. (2020), Bartik et al. (2020)). Related work studies the effects of government intervention during this time, including the effects of the Paycheck Protection Program (PPP) on businesses (Granja et al. (2020) and Agarwal et al. (2020)) and mortgage forbearance on households (Cherry et al. (2021) and An et al. (2021a)). However, despite this important work on other aspects of pandemic-related government intervention, this paper is among the first to look directly at the housing market effects of the eviction moratoria.

The second strand of related literature looks at the effects of government interventions in housing markets. A sizable number of studies investigate the role of the mortgage interest tax deduction on housing markets (see, e.g., Sommer and Sullivan (2018)). Other scholarship looks at the effects of rent control (Glaeser and Luttmer, 2003), regulating housing supply (Gyourko and Molloy, 2015; Quigley and Raphael, 2005), and home purchase restrictions (Sun et al., 2017). I add to this literature as one of the first to investigate the effects of eviction moratoria on renters and landlords.

The paper proceeds as follows. Section 2 outlines the context and policy specifics of COVID-19 eviction moratoria. Section 3 presents the data sources and research design. In Section 4 I report the results and conclude in Section 5.

2.2. Background

Beginning in March, 2020 the US implemented a number of measures designed to control the spread of COVID-19. Many of these policies were set at the national level, including a nationwide state of emergency. Other policies were implemented by individual states, counties, and cities. Most states issued eviction moratoria beginning in March or April of 2020: by the end of April, 43 out of 50 states had implemented some form of moratorium.

The details of these moratoria varied by state. Many moratoria were initially only executive orders by the governor[9] or, in rare occasions, courts refused to process evictions[10]. A majority were set to expire after a few weeks[11], but most states extended the initial moratoria for months or indefinitely[12]. Some states also eventually passed the moratoria legislatively[13]. Perhaps the most consistent feature across states were the policy outcomes: landlords were prohibited from evicting tenants for most reasons, including non-payment of rent.

I primarily rely on the differential timing of state-level eviction moratoria to isolate the effects of the moratoria policies. To this end, I use the COVID-19 US State Policy Database (CUSP) (Raifman et al., 2020) to track specific state-level changes to eviction moratoria policies. Figure B.1 illustrates the states that implemented eviction moratoria and those whose moratoria extended beyond December 27, 2020, when the ERA programs began. The number of states with an active moratorium is graphed over the study period in Figure B.2.

[9]e.g. Alabama, Illinois, New Hampshire, and Washington
[10]e.g. New Mexico and Idaho
[11]North Dakota's moratorium ran from March 26, 2020 through April 22, 2020
[12]e.g. California, New Mexico, and Illinois
[13]e.g. California, Massachusettes, and New York

Over this same period, the federal government passed two nationwide moratoria with limited effects. I describe their less-than complete effectiveness below. It is important to note that for my approach to identify the effects of state-level moratoria to be valid, I do not need the federal moratoria to be completely ineffective. Instead, I only need some resulting variation in the eviction moratoria treatment intensity to be driven by state-level policies. This would be satisfied if, for instance, the federal moratoria are more effective in combination with a state moratorium. I show this is the case below.

Under the CARES Act a partial federal eviction moratorium went into effect from March 27, 2020, through July 24, 2020. However, this moratorium had no enforcement mechanism and was limited to renters living in properties that participated in federal housing assistance programs or that had a federally backed mortgage[14]. The Federal Reserve Bank of Atlanta estimated it may have covered as little as 28.1% of the rental housing stock[15]. Additionally, major publications found evidence states and landlords ignored the policy and continued to initiate and process evictions[16].

Beginning in September 2020, the US Center for Disease Control (CDC) issued a national eviction moratorium with dramatically expanded scope. In theory, this should have covered nearly all renters, but in practice it was of limited effectiveness[17]. Any renter who testified they had an annual income of less than $99,000 ($198,000 if filing jointly), had been adversely affected by COVID-19, and was using their "best efforts" to make on-time rental payments was prohibited from being evicted by the CDC for non-payment of rent. But the enforcement mechanism was complex. Landlords could challenge tenants in court over the extent of their hardships. Tenants often lacked legal resources and information to defend themselves from eviction or file a countersuit. And landlords who asserted other eviction reasons besides late

[14]See the CARES Act, Pub. L. 116-136, Section 4024

[15]Federal Reserve Bank of Atlanta, "Housing Policy Impact: Federal Eviction Protection Coverage and the Need for Better Data," by Sarah Stein and Nisha Sutaria

[16]See, e.g., "Despite Federal Ban, Landlords Are Still Moving to Evict People During the Pandemic" by Jeff Ernsthausen, Ellis Simani and Justin Elliott at ProPublica

[17]See, for example, "The CDC banned evictions. Tens of thousands have still occurred" and "Renters thought a CDC order protected them from eviction. Then landlords found loopholes."

rental payments could often move forward in court. In many jurisdictions where state-level moratoria had expired, such as North Carolina, evictions continued to be processed despite the federal order[18]. Later, federal and US Supreme Court rulings ended the CDC eviction moratorium.

Because the federal eviction moratoria were not completely binding in the absence of state-level moratoria, some variation in treatment intensity is due to state-level policies. I describe in the next section how I exploit the timing of this variation to produce causal estimates of the policies' effects on landlord financial distress.

The Emergency Rental Assistance (ERA) programs consisted of two federal programs. ERA-1, part of the Consolidated Appropriations Act of 2021, provided $25 billion for rental assistance beginning in December of 2020. ERA-2, part of the American Rescue Plan Act, provided about $22 billion of rental assistance beginning in March of 2021. Both had relatively broad eligibility criteria. Essentially all renters who had income below 80% of the local area median income and could claim they faced some financial hardship due to the pandemic were eligible to receive rental assistance under ERA-1. ERA-2 expanded this eligibility set to renters who faced some financial hardship *during* the pandemic (not only *due to* the pandemic), with the same income limitations. Households could receive up to 12 months of rental assistance under ERA-1 and up to 18 months under ERA-2. This assistance could be used for rental and utility arrears, current rent and utilities, and in some cases, up to three months of prospective rent.

2.3. Data and Research Design

2.3.1. Data

To fully explore the effects of eviction moratoria and ERA programs, I combine data from multiple sources.

[18]Order of the Chief Justice of the Supreme Court of North Carolina

The details and dates of eviction moratoria policies come from the COVID-19 U.S. State Policy (CUSP) database, compiled by Raifman et al. (2020). CUSP reports the details of COVID-19 related policies enacted at the state level, including mask mandates, business restrictions, and eviction moratoria. CUSP provides granular data regarding eviction policies and captures not only details on state eviction moratoria, but also on state-level enforcement of federal moratoria.

Obtaining high quality eviction data is complicated by a number of factors, including the legal nuances of the eviction process, lack of clear and comparable data on evictions across the US, and differing interpretations of state and local eviction moratorium laws.

Evicting a tenant is a legal process with many steps. It typically begins with a landlord giving notice to tenants of a cause for eviction and a required waiting period during which tenants can correct the issue. If an issue still exists, eviction proceedings can be initiated in court. Hearings are held and, if the eviction is granted by the judge, a writ of execution is posted for the tenants. After a brief time, local law enforcement can legally assist the landlord in reclaiming property.

Measuring evictions is complicated by two factors. First, it is common for tenants to vacate the property before eviction proceedings are completed. This may occur when tenants recognize they have a low chance of prevailing at court, if tenants do not understand the eviction process and confuse initial notices for final court decisions, or successful negotiation between landlords and tenants. In these cases, court records do not include any order of eviction. However, since the landlord induces the tenant to vacate, it is not clear whether this should be included as an eviction for research purposes.

Second, eviction proceedings are typically decided by county courts. There is no standard format or single repository for this data.

To solve these problems, I rely on data compiled by the Eviction Lab[19], which provides detailed data on eviction filings throughout the US. Of particular relevance to this paper is their data tracking eviction filings at weekly intervals in 30 cities from January, 2020 to August, 2021. These 30 cities are among the largest cities in America and generally representative of a diverse set of urban locations. This data captures the initiation of eviction proceedings at court. While this measure may include some eviction proceedings that fail in court, it errs on the side of including any cases where tenants vacate before eviction orders are finalized.

To answer questions about landlord financial distress, I use real estate market microdata from CoreLogic. This dataset includes detailed records on nearly every real estate transaction and foreclosure in the United States. I combine this with CoreLogic's tax records that capture a snapshot of property characteristics each quarter. I utilize the absentee owner flags in these snapshots as a proxy for rental properties. I use a 5% random sample of the largest 20 metropolitan statistical areas, yielding about 450 million observations on 10 million subject properties tracked from 2018 through 2021. The combination of these two data sets allow me to calculate the probability of a specific property to sell or foreclose.

2.3.2. Summary Statistics

I present summary statistics of the variables I use in Table B.1. In Panel A, I report the summary statistics for the eviction moratoria policies and the resulting evictions. I use evictions tracked by Eviction Lab across 30 cities on a weekly basis. In January 2020, before the pandemic, an average of 567.6 evictions were filed weekly per city. By April 2020, this fell to an average of 44.2 evictions. This number grew to 224.4 average weekly filings in August, 2020.

Panel B presents housing summary statistics, including the number of units that were flagged as having an absentee owner (not owner occupied). These observations

[19]The Eviction Lab is a project directed by Matthew Desmond and designed by Ashley Gromis, Lavar Edmonds, James Hendrickson, Katie Krywokulski, Lillian Leung, and Adam Porton. The Eviction Lab is funded by the JPB, Gates, and Ford Foundations as well as the Chan Zuckerberg Initiative. More information is found at evictionlab.org.

are at the unit-month level. I use this variable as a proxy for rental units since the vast majority of non-owner occupied units are rented. This data represents a 5% random sample of housing units from the largest 20 metropolitan areas with quarterly data frequency from 2018Q1 to 2021Q4.

2.4. Policy Effects

This section describes how I estimate the effects of local eviction moratoria and the ERA programs on evictions and landlord financial distress. Because the COVID-19 pandemic led to many wide-ranging economic changes, care is needed to identify the direct effects of the policies without the potentially confounding effects of other economic changes.

The pandemic shifted both household preferences and income. Many households experienced reduced income, as US unemployment rate peaked at 14.7% in April, 2020[20]. At the same time, households changed purchasing patterns in both what they purchased and from where they purchased household goods (Unnikrishnan and Figliozzi, 2020). These may have caused changes to the rate of rental property sales apart from the effects of local moratoria.

In order to identify the effects of the local moratoria and the ERA, I exploit the heterogeneous timing of various state moratoria. Because the COVID-19 pandemic began so rapidly, there was little opportunity for states to coordinate policies with each other or with the federal government. The result was a wide range in the end dates of state-level eviction moratoria. I exploit the pseduo-randomness of the end dates and compare monthly outcome variables between states with active versus inactive moratoria.

2.4.1. Local Evictions

I first estimate the effects of the presence of an active eviction moratoria on the number of local evictions. I model eviction rates linearly by letting $v_{i,t}$ be the number

[20]BLS Unemployment Data

of evictions in city i in week t and assume

$$v_{i,t} = \gamma_i + \beta_1(localmor)_{i,t} + \beta_2(fedmor)_{i,t} + \varepsilon_{i,t}$$

In this regression equation, city fixed effects, essentially the city base rate of evictions, is captured by γ_i. The dummy variables $localmor$ and $fedmor$ indicate an active local or federal eviction moratorium for the city-week observation. The error term is represented by $\varepsilon_{i,t}$.

I run this regression on the sample of eviction data from thirty cities from January, 2020 through August, 2021. The results are presented in Table B.2. A local moratorium dramatically reduces the number of evictions while it is active, but a federal moratorium makes very little impact. My preferred specification (5) indicates that an active local moratorium corresponds with 225 fewer evictions per week. This is contrasted with only 29 fewer per week when only a federal moratorium is active.

The large response to local moratoria is highly robust to different specifications, the inclusion of weekly time fixed effects, and clustered standard errors. It indicates not only that local moratoria were effective at reducing evictions, but also supports the conclusion that federal moratoria were ineffective.

2.4.2. Rental Sales post-Moratorium Expiration

Next, I seek to determine the effects of eviction moratoria on landlord financial distress. I use the number of rental property sales and rental property foreclosures as proxies for landlord financial distress. This proxy relies on the hypothesis that an increase in landlord financial distress leads to an increase in the probability a rental property is sold or foreclosed. An increase in rental property sales may arise due to unrelated situations, such as a spurious trend.

While eviction moratoria were in place, landlords may have found it difficult to sell rental properties with non-paying and non-evictable tenants. Potential buyers are unlikely to view such properties favorably due to the tenant situation. Further, tenants in such situations may be non-cooperative and unwilling to show the apartment to prospective buyers or work with the landlord.

Landlords entering financial distress and wanting to sell their rental property may be unable to until the local moratorium ends. A surge in rental property sales in the first month after a local moratorium expiration is consistent with landlord financial distress and the difficulty selling during the moratorium.

I test this with a difference-in-difference model estimating the excess sales of rental properties over owner-occupied properties in the month after a local moratorium expires. I estimate the following regression equation at the property-month level:

$$sale_{i,t} = \nu_t + \alpha(absentee_{i,t-1}) + \beta(expired_{i,t}) + \gamma(absentee_{i,t-1} \times expired_{i,t}) + \varepsilon_{i,t}$$

Here, $sale_{i,t}$ is a dummy variable indicating whether the property i was sold in month t. A nation-wide month fixed effect to capture any nation-wide real estate trends is added as ν_t. A dummy variable $absentee_{i,t-1}$ indicates whether the property had an absentee owner (a proxy for rental properties) in the previous period. The $expired_{i,t}$ variable indicates whether a local moratorium expired in the immediately preceeding period. That is, $expired_{i,t} = 1$ iff the local moratorium for property i expired at $t - 1$. The difference-in-difference parameter of interest is γ, which indicates if rental properties experienced an excess amount of sales at the conclusion of a local moratorium when compared to other properties at the conclusion of a local moratorium. The error term is included as $\varepsilon_{i,t}$.

The regression estimates of this model are presented in Table B.3. Over the sample period, non-owner occupied (absentee owner) housing is more likely to sell in any given period when compared to owner occupied units. I estimate absentee owned property experiences an 11 bps higher rate of sale per month than owner occupied housing. I also find in the month after a moratorium expires, the probability a given property will sell increases by about 30 bps. The difference in difference estimator looks at the increased rate of sale for an absentee owned property in the month after a moratorium expiration. This is estimated to be about 5 bps, and highly statistically significant.

While small, this 5 bps estimate suggests absentee owned properties were uniquely affected by eviction moratoria compared to owner occupied housing. I attribute this to landlords in financial distress selling their rental properties once the local moratorium expires. This is in addition to an already elevated rate of absentee owner property sales over the sample period.

2.4.3. ERA Effects on Rental Sales and Foreclosures

The ERA programs provided billions of dollars to landlords on behalf of renters whose rent was in arrears or who would have trouble making upcoming rental payments. This money was provided as grants from the federal government to states, who administered it to landlords on behalf of renter applicants. It stands to reason that this funding would reduce landlord financial distress as it replaced missing rental payments.

The ERA programs only began in December, 2020, months after some local eviction moratoria ended. Any effects reducing landlord financial distress are therefore likely to be limited in regions where the moratoria ended months earlier.

I test this hypothesis via a difference-in-difference model that compares the number of excess sales of rental properties compared to owner-occupied properties in regions where the moratorium overlaps with the ERA program versus areas where the moratorium ends before the ERA program. I also run the same specification but look at excess foreclosures. Both estimations are done at the property level. Formally, the model is:

$$sale_i = \alpha(absentee_i) + \beta(overlap_i) + \gamma(absentee_i \times overlap_i) + \varepsilon_i$$

In this model, $sale_i$ is a dummy variable representing whether property i was sold at least once over the course of the pandemic. It counts only whether the initial owner as of February, 2020 sold the property in an arms-length transaction after

March, 2020. The $absentee_i$ variable is similar to the previous specification and represents whether property i was owned by an absentee owner before the pandemic, as of January, 2020. The variable $overlap_i$ is 1 if the end date of the local moratorium is after the beginning of the ERA programs (December 27, 2020) and 0 otherwise. In areas without a local moratorium, $overlap_i = 0$. In areas where the local moratorium has not yet expired, $overlap_i = 1$. The error term is ε_i.

The regression estimates are presented in Table B.4. Properties where the moratorium ended before the ERA program began were 2.66% more likely to sell during the pandemic. Additionally, absentee owned properties were about 3% more likely to change hands during the pandemic. Crucially, the difference in difference estimator, which indicates the additional probability of sale associated with a property both with an absentee owner and in a location with moratorium and ERA overlap, is estimated at -2.2%. Absentee owned properties were significantly less likely to be sold in areas where the ERA programs began before the expiration of local moratoria.

2.5. Conclusion

The COVID-19 pandemic led to state and federal housing policy responses that had never previously been implemented. Eviction moratoria lowered the eviction rate across the country and in turn reduced the spread of COVID-19. However, I present evidence these moratoria also increased landlord financial distress.

First, I find local eviction moratoria were generally more binding than federal moratoria. A local moratorium is associated with an average of a 50% reduction in evictions compared to the pre-pandemic baseline. In comparison, the federal moratorium was associated with only a 6% percent reduction.

Despite close to 20% of households reporting they were behind on rent during the pandemic, very few households were evicted during the moratoria. Landlords did not receive rental payments on time, suggesting some may have begun to enter financial distress.

Consistent with this theory, I find the month after the expiration of a local eviction moratorium was associated with a sudden increase in rental property sales when compared to sales of owner-occupied housing. I measure this increase is approximately 5 bps. I attribute this to landlords entering financial distress, but unable to sell their property while a non-paying and non-evictable tenant resides there. The ending of the moratorium coincides with the ability to evict the tenant, sell the property, and reduce the amount of financial distress. That this surge is larger among rental properties than owner-occupied housing indicates it was motivated by an external factor, such as financial distress.

I additionally find evidence of greater financial distress among landlords in regions where the local eviction moratorium expired before ERA programs were available. I estimate the expiration of a local moratorium before the beginning of the ERA programs is associated with a 2.16% increase in the probability an absentee owned property will sell during the pandemic. This finding is also consistent with landlords entering financial distress due to non-paying tenants and selling as the local moratorium expires. After the ERA program implementation, landlords were able to receive rental payments from the government on behalf of non-paying renters, reducing financial distress and leading to fewer distressed sales.

These findings have important implications for future housing policy. Despite initial concern over landlord financial distress, relatively few landlords seem to have been forced to foreclose or sale their rental properties. This is due in large part to ERA programs, which reduced financial distress by making rental payments to landlords on behalf of renters. The timing of ERA programs was critical, and an earlier implementation may have helped many landlords, particularly those in areas whose moratorium expired early.

CHAPTER 3

Covenants, Conditions, Rights, and Restrictions: The Incidence of Homeowners Association Regulations

3.1. Introduction

A great deal of research investigates the role of government regulations on housing supply and demand. For example, research indicates zoning requirements (Gyourko and Molloy, 2015; Mayer and Somerville, 2000; Pogodzinski and Sass, 1990), building height restrictions (Molloy et al., 2020; Pollard, 1980), permitting processes (Paciorek, 2013), housing use limitations (Shabrina et al., 2022), and financing regulations (Reher, 2021) all affect the quantity and type of supplied housing. These regulations are ubiquitous across the developed world.

The bulk of this research leaves out an important non-governmental entity that is also responsible for regulation in the housing markets: homeowners associations (HOAs). HOAs are private organizations of homeowners that manage small geographic areas, typically subdivision or neighborhood size. They generally set rules and regulations for housing units, own and manage common areas and property, hold elections for HOA leadership, and collect dues from members. Membership is automatic and mandatory for each home in the HOA and is passed on if ownership is transferred. In other words, inclusion in the HOA "runs with the land."

Condominium associations are similar to HOAs, except they are formed from condominium units instead of multiple homes in a subdivision or neighborhood. For simplicity, in this paper I use the term "HOA" to include both traditional HOAs and condominium associations.

HOAs are understudied, yet important entities in the US housing market. Approximately 20% of the US population lives in a housing unit governed by a homeowners or condominium association (McCabe, 2011). Recently, nearly 60% of all new single-family home construction and 80% of all homes in new subdivisions belonged to an HOA (Clarke and Freedman, 2019). Courts throughout the US have held their rules, restrictions, and ability to assess fees and fines are binding.

The growth of HOAs has been called by Guberman (2004), "one of the most significant privatizations of local government functions in history." Like a local government, HOAs provide public goods while also enforcing rules and regulations. Yet little formal research has actually documented these regulations in the cross-section of HOAs.

Each HOA must declare regulations formally in a document called the Covenants, Conditions, and Restrictions (CC&Rs). This document establishes limits on the use of properties in the HOA, sets forth fees and fines for violations, and sometimes details any restrictions on property sales, rentals, or transfers. But while governments typically consider the general welfare and distributional effects of policies and regulations, HOAs are commonly considered corporations, beholden only to residents.

This paper seeks to document HOA CC&Rs in New York City and Chicago. As a step toward future work to model the choice to implement specific CC&Rs, I study the various covenants that occur in the universe of New York City homeowners association agreements.

In order to study CC&Rs, I compile a novel dataset of the universe of New York City's digitized CC&Rs and amendments filed with the county recorder. I focus on these cities because they are among the largest US cities, have a high concentration of condominiums versus apartments, have few state-level laws regulating HOA restrictions, and have relatively open records. These 14,000 records begin in 2003 and end in 2018. I extract the text from these scanned documents and use a variety of techniques to capture the most relevant portions of the documents. I also include a small subset of Chicago CC&Rs. Additional data including the (near) universe of Chicago CC&Rs is pending county approval for release.

I find a number of common CC&Rs that have important implications for property financing, neighborhood composition, resident amenities, and renters. I analyze three types of specific CC&Rs from the dataset: rental restrictions, pet restrictions, and restrictions on exterior features. For each type of restriction I provide an example covenant and discussion of its effects.

The paper proceeds with more context on HOAs and a literature review in section 2. I then discuss the HOA data I compiled in Section 3. Section 4 analyzes some selected covenants and their implications, and Section 5 concludes.

3.2. Background and Literature Review

HOAs began as legal entities to preserve and maintain common areas (Hyatt, 1981). They existed in the US as far back as the mid-19th century, but surged in popularity after World War II (Esquivel and Alvayay, 2014). In the 21$^{\text{st}}$ century, the majority of new single family home constructions are part of an HOA (Clarke and Freedman, 2019).

HOAs act as a solution to the collective action problem inherent in neighborhoods and communities. Individuals own their homes, but common elements are owned as a community and maintained with the dues paid by each homeowner. These common elements can include streets, parks, pools, recreational facilities, and other infrastructure (Esquivel and Alvayay, 2014).

HOAs also solve community collective action problems by enforcing rules and restrictions intended to limit actions that produce negative externalities. For example, HOAs can require front yards to be kept tidy, paint to be certain colors, or loud noises prohibited during certain times. Stricter HOAs can set design standards for home exteriors and require any modifications to be pre-approved. These requirements can be enforced with fines or other disciplinary procedures (McKenzie, 1994).

As private corporations, HOAs must operate according to their bylaws, which are established at the founding and filed with the county. Typically a board is elected by members of the HOA. The board is then empowered to run the HOA, enforce regulations and policies, and sometimes enact new restrictions or rules. HOAs formally declare any regulations in an additional document also filed with the county and commonly referred to as the "CC&Rs" (covenants, conditions, and restrictions). Changes must be filed as an amendment to the CC&Rs.

The welfare effects of HOAs is naturally complex since homeowners receive amenities, but forfeit certain rights. For instance, Helsley and Strange (1998) develop a theoretical model of resource allocation in a city with and without HOAs. They find the welfare effects of an HOA are ambiguous: depending on the presence of fixed costs, HOA members may be worse off under an HOA. Hughes Jr and Turnbull (1996) develop a hedonic pricing model that incorporates neighborhood externalities and deed restrictions. Their empirical analysis suggests certain deed restrictions increase house prices, while other have the reverse effect.

These theoretical claims have naturally interested researchers. On one hand, the prevalence of HOAs continues to rise, suggesting they bring certain benefits to owners, or at least developers. On the other hand, Helsley and Strange (1998) predict fixed costs lead to inefficiently small HOAs, which may lower member welfare. The effects of HOAs on nonmembers–that is, community members who live outside the HOA–are also complex and unclear.

One line of investigation is to measure the value home buyers assign to living in an HOA. The price difference between an HOA house and non-HOA house *ceteris paribus*, should reflect the welfare gain expected from living in an HOA. This has been investigated by a number of scholars through hedonic regressions, most recently by Clarke and Freedman (2019); Hopkins (2016); Meltzer and Cheung (2014).

Another line of research is to measure the effects of specific CC&Rs. This was estimated empirically for a single CC&R by Cannaday (1994), who looked at pet restrictions. By measuring the price effects of this specific CC&Rs in one area of Chicago, Cannaday (1994) found that CC&Rs prohibiting large dogs but allowing cats were preferred over CC&Rs banning all pets or not banning any pets at all. In related research, Hughes Jr and Turnbull (1996) studies deed restrictions but not specifically CC&Rs. This paper found mixed effects of various restrictions.

Recent research and commentary have expressed concern over potential HOA impact on racial discrimination, housing affordability, and the replacement of public government with private HOAs. This shows that there are many important and unanswered questions related to HOAs that require scholarly attention. This project aims to specifically look at HOA implications for real estate markets, rental markets, and mortgage financing.

3.3. Data

Cross-sectional data on HOAs is difficult to obtain because legal documents such as CC&Rs are recorded in county offices. These offices rarely share or aggregate data between offices and often charge large fees per page for copies of documents.

To address this problem, I assemble a novel dataset of the universe of New York City HOA filings from 2003-2018. These filings include all digitized CC&Rs and CC&R amendments from this time period. I supplement this with CC&Rs from Chicago. Since the full Chicago dataset is pending release, only a small number of Chicago CC&Rs are included. I aim to construct a similar dataset with the complete set of Chicago data once it becomes available.

This dataset includes approximately 14,000 CC&Rs, totalling about 1,000,000 pages of recorded documents. I plot the number of CC&Rs filings by year in in Figure C.1. I supplement this with a breakdown of the number of CC&Rs referencing selected topics in Table C.1. The count of CC&Rs in the sample that mention each topic is provided.

3.4. Analysis of CC&Rs

To focus my research, I highlight three categories of CC&Rs that are regularly enacted by HOAs and may have important implications for real estate markets, rental markets, and mortgage financing. These categories are supplemented by example text quoted from CC&Rs in the dataset. The potential impacts of each CC&R is described, and any relevant existing research is cited.

3.4.1. Limits on Rentals

Rental restrictions in HOAs may take multiple forms. The strongest type of restriction is one barring all rentals of HOA property. Far more common is a cap on the percent of units that may be rented at any time. For example, the HOA may set this limit to 25%. Owners wishing to rent after the HOA is at this threshold must wait until units cease being rented.

An example of language setting a rental cap can be found in the assembled CC&R dataset. The below quoted provision establishes a 12% cap.

> *17. Sale or Lease by a Unit Owner*
>
> *Notwithstanding any foregoing provisions of this Declaration to the contrary, the rental or leasing of Units is limited to a total of twelve percent (12%) of the Units or three (3) Units in total, effective with the recording of this Amendment. At no time may a total of more than twenty percent (20%) of the Units or five (5) Units in total including all Hardship Exemptions be leased at any one time.... Subletting of Units is not permitted. Owners must have owned and resided in the Unit for at least two (2) years before the Unit is eligible to be leased. No lease may be for a period of less than one-year and the maximum lease period is two years while a waitlist is in place....*

Short-term rental restrictions, such as the language in the above quote forbidding leases of less than one-year, are also common. Of particular interest, limiting Airbnb has an ambiguous theoretical effect on prices. While such rules may limit externalities from inconsiderate guests, it may also drive down resale value. The adoption of these provisions is likely to be related to the composition of the neighborhood and owner characteristics.

The ability of HOAs to enforce rental restrictions is based in part on local and state law. For example, California recently passed AB 3182 in 2020, which amended state laws to prohibit HOA rules setting a rental cap at less than 25%. Under California law, the above referenced CC&Rs would not be enforceable.

Rental caps and rental restrictions may additionally be important for obtaining mortgage financing. Fannie Mae limits the availability of certain mortgage products when the number of rental units in a condominium building is above certain levels(Fannie Mae, 2022). These mortgage requirements may be part of the reason HOAs implement limits on rentals.

Restricting rentals can have affects on landlords, tenants, and residents outside the HOA. Landlords are affected by potential house price affects since they lose at least the option value of renting. Tenant welfare can be affected by constraints on the minimum and maximum lease duration, as well as potentially skewed rental prices due to an artificially smaller stock of rental housing. Finally, residents outside the HOA can be affected by changing neighborhood composition and changing rent and house prices. If the availability of rental housing drives decisions about homeownership, broader trends in homeownership and real estate may be affected by wide-spread adoption of such restrictions by HOAs (Rappaport, 2010).

3.4.2. Limits on Pets

The effects of HOA pet restrictions have been previously investigated by Cannaday (1994). He documents the existence of HOA limitations on pet ownership. These limitations describe the types of animals allowed but sometimes are specific down to prohibiting certain sizes or breeds.

My assembled CC&R dataset indicates that such restrictions continue to be common nearly 30 years later. A sample CC&R from the dataset states:

> *(e) No animals shall be raised, bred, or kept in any Unit or the Common Elements, except for dogs and cats (but not more than two (2) animals per Unit), small birds, and fish of a Unit Owner, provided said animals are of a breed or variety commonly kept as household pets, are not kept or bred for any commercial purpose,*

> *...and do not, in the judgement of the Board, constitute a nuisance to others.*

The theory proposed by Cannaday (1994) predicts HOA restrictions should "allow some level of pet ownership, but not as high a level as would be needed to achieve a stable equilibrium in the absence of negotiations among owners." Empirically, I find CC&Rs that limit pet ownership in line with his predictions.

While pet ownership restrictions on their own have no broad economic impact, such covenants likely induce resident sorting into various neighborhoods and buildings. This phenomenon as a whole provides important insight into neighborhood composition, amenities, and the spatial clustering of demographic groups.

3.4.3. Limits on External Features

Appearance based restrictions are an extremely controversial power of HOAs. Academic research confirms common sense predictions that extremely poorly maintained homes negatively affect neighborhood house prices (Han, 2014). Many HOAs attempt to prevent any negative exteralities stemming from poor maintenance or gross negligence. Some go further and regulate building style, color, yards, street parking, and ornamentation.

Controversy over limits on external residential features reached a peak in 2005 when Congress passed The Freedom to Display the American Flag Act. This bill was written in response to multiple court cases ruling HOAs had the right to prohibit residents from flying of the American Flag on their home or property (Craig, 2007).

While facially this controversy was a trivial debate on flag flying, the reality is that HOAs have broad rights to regulate and enforce nearly any policies on external features or appearance (with the exception of banning the American flag). An example of common HOA CC&R language is the following quote:

> *No unlicensed or inoperative vehicles shall be kept on the Property... [Only] outdoor grilling equipment and lawn furniture may be placed or kept in any balcony.... No plant material of any kind which*

> *overhangs the railing of any balcony may extend below the floor of such balcony.*

The effects of such restrictions is likely similar to the effects of pet restrictions: residents sort into communities based on their preferences for certain covenants and restrictions. However, the growing number of HOAs acting as *de facto* private governments may have broad implications for neighborhood composition and prices.

3.5. Conclusion

HOAs act as private governments that provide public goods and enforce regulations on neighborhoods and small communities. They have risen in prominence over the last three decades, to the point where the majority of new US single-family housing construction belongs to an HOA. Since membership "runs with the land," and owners cannot withdraw from HOAs voluntarily, the effects of the rise in HOAs on real estate markets is important to understand.

Unlike state and local governments, whose policies are subject to public notice and approval, HOAs are private. Their rules and regulations do not need to be approved by city planners or the general public. Their policy choices–typically published in a document called a CC&R–may affect members, non-members, and outside communities.

I assemble a dataset on the universe of New York City CC&Rs in order to document and study common CC&Rs and their potential effects on real estate and rental markets. I augment this data with a number of Chicago CC&Rs, pending approval to use the full Chicago dataset.

I find at least three categories of CC&Rs that may have significant economic effects. First, limits on the ability to rent the property are relatively common. These can be restrictions on short-term rentals, a maximum percent of units in the community or condominium building that can be rented at a time, or complete prohibitions on renters. These restrictions may affect the ability to receive mortgage financing, the composition of the neighborhood, and renter welfare. Second, some condominium associations restrict pets. These restrictions have been studied by

Cannaday (1994), and are reflected in price disparities. Such rules likely help achieve a more optimal welfare and eliminate negative externalities. Finally, many HOAs strictly enforce exterior features and the overall exterior image of units. Similar to rules on pets, these rules may affect property prices and resident sorting into the HOA community.

Future research is needed to determine the effects of specific HOA restrictions, particularly those dealing with renters. Since racial minorities make up a disproportionately large percentage of renters, HOA prohibitions on renters may affect neighborhood racial composition. The prevalence of such restrictions likely affect the availability of rental and low-income housing, since substitution between owner-occupied and rental housing is limited by rental caps or prohibitions. The CC&R data presented in this paper allows future researchers to incorporate HOA restriction microdata into further study on the effects of HOAs.

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APPENDIX A

Figures and Tables from Chapter 1

A.1. Tables

Table 1

Variable	Median	Mean	Std. Dev.
Enrollment	8,078	14,968	39,325
UPP	64.8%	60.9%	22.4%
LCFF Funding per Student	$10,282	$10,240	$967
Cohort Graduation Rate	90.9%	90.2%	5.42%
Number of Teachers	394	706	1,923
Average Teacher Salary	$66,850	$66,862	$6,647

Table A.1. Sample statistics for school districts in analysis subset.

Table 2

Variable	Median	Mean	Std. Dev.
Population	20,282	23,654	21,852
Mean Household Income	$72,409	$81,017	$38,348
Percent White	77.8%	73.7%	19.7%
Percent Non-English Speakers	31.4%	34.9%	23.9%
Home Value Index	$288,875	$355,583	$268,886
Pct. Increase in District Operating Budget (2012 - 2018)	75.2%	75.8%	23.6%
Pct. Increase in HVI (2012-2018)	82.4%	87.6%	35.6%

Table A.2. ZIP code level sample statistics of analysis subset.

Table 3

	(1)	(2)	(3)	(4)	(5)
Pct. Change in ZVHI	9.292***	9.292**	8.463***	8.463	5.281***
Standard Error	2.718	4.605	3.239	5.484	1.426
Bandwidth	15%	15%	10%	10%	15%
Pre-Policy Years	2011-2012	2011-2012	2011-2012	2011-2012	2009-2012
Post-Policy Years	2017-2018	2017-2018	2017-2018	2017-2018	2013-2018
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster?	No	District	No	District	No
Num. Obs.	344	344	244	244	860
Num. Districts	86	86	61	61	86

Table A.3. Difference-in-difference model estimation for percent change in district-level Zillow House Price Index (ZHVI) since 2012. Regression is estimated according to equation (4), where the outcome variable of interest is the percent change in ZHVI since 2012. District-level ZHVI is estimated using an average of ZIP ZHVI, weighted according to the intersection proportion of ZHVI and ZIP. At a threshold of 55% UPP, districts qualify for a concentration grant. The treatment and control groups are defined by whether the district is above (treatment) or below (control) the threshold, but no further away than the bandwidth for the specification. The symbols *, **, and *** denote estimates significant at the 1%, 5%, and 10% confidence levels, respectively.

Table 4

	(1)	(2)
RKD Coefficient	4.7001**	10.159*
Standard Error	2.3628	5.5408
Bandwidth	16.3%†	10%
Pre-Policy Year	2012	2012
Post-Policy Year	2020	2020

Table A.4. Regression kink discontinuity (RKD) model estimates for percent change in Zillow House Price Index (ZHVI) since 2012 as the outcome variable. The model is estimated at the ZIP-level with a kink at the 55% UPP threshold for concentration grants. The coefficient represents the change in the slope of the outcome variable when moving from below to above the kink. ZIP-level UPP is approximated by the average of district-level UPP, weighted by the amount of geographical area overlap between the district and the zipcode. The symbols *, **, and *** denote estimates significant at the 1%, 5%, and 10% confidence levels, respectively. The bandwidth with a † symbol uses the CCT optimal bandwidth estimation procedure.

Table 5

	(1)	(2)	(3)	(4)	(5)
Pct. Change in ZRI	-4.614**	-4.614	-4.164*	-4.164	-3.378**
Standard Error	1.906	3.077	2.188	3.550	1.259
Bandwidth	15%	15%	10%	10%	15%
Pre-Policy Years	2011-2012	2011-2012	2011-2012	2011-2012	2009-2012
Post-Policy Years	2017-2018	2017-2018	2017-2018	2017-2018	2013-2018
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster?	No	District	No	District	No
Num. Obs.	346	346	246	246	693
Num. Districts	87	87	62	62	87

Table A.5. Difference-in-difference model estimation for percent change in district-level Zillow Rent Price Index (ZRI) since 2012. Regression is estimated according to equation (4), where the outcome variable of interest is the percent change in ZRI since 2012. District-level ZRI is estimated using an average of ZIP ZRI, weighted according to the intersection proportion of ZRI and ZIP. At a threshold of 55% UPP, districts qualify for a concentration grant. The treatment and control groups are defined by whether the district is above (treatment) or below (control) the threshold, but no further away than the bandwidth for the specification. The symbols *, **, and *** denote estimates significant at the 1%, 5%, and 10% confidence levels, respectively.

Table 6

	(1)	(2)
RKD Coefficient	-6.368**	-4.9594***
Standard Error	2.641	1.575
Bandwidth	10.9%†	15%
Pre-Policy Year	2012	2012
Post-Policy Year	2020	2020

Table A.6. Regression kink discontinuity (RKD) model estimates for percent change in Zillow Rent Price Index (ZRI) since 2012 as the outcome variable. The model is estimated at the ZIP-level with a kink at the 55% UPP threshold for concentration grants. The coefficient represents the change in the slope of the outcome variable when moving from below to above the kink. ZIP-level UPP is approximated by the average of district-level UPP, weighted by the amount of geographical area overlap between the district and the zipcode. The symbols *, **, and *** denote estimates significant at the 1%, 5%, and 10% confidence levels, respectively. The bandwidth with a † symbol uses the CCT optimal bandwidth estimation procedure.

Table 7

	(1)	(2)	(3)	(4)	(5)
Cohort Graduation Rate	1.439**	1.439**	1.397**	1.397*	0.878**
Standard Error	0.582	0.711	0.709	0.835	0.393
Bandwidth	15%	15%	10%	10%	15%
Pre-Policy Years	2011-2012	2011-2012	2011-2012	2011-2012	2009-2012
Post-Policy Years	2017-2018	2017-2018	2017-2018	2017-2018	2013-2018
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster?	No	District	No	District	No
Num. Obs.	457	457	313	313	1,026
Num. Districts	116	116	80	80	116

Table A.7. Difference-in-difference model estimation for cohort graduation rate. Regression is estimated according to equation (4), where the outcome variable of interest is the cohort graduation rate. At a threshold of 55% UPP, districts qualify for a concentration grant. The treatment and control groups are defined by whether the district is above (treatment) or below (control) the threshold, but no further away than the bandwidth for the specification. The symbols *, **, and *** denote estimates significant at the 1%, 5%, and 10% confidence levels, respectively.

Table 8

	(1)	(2)
Coefficient Estimate	1.4636*	2.3661*
Standard Error	0.82408	1.2969
Bandwidth	15%	10%
Pre-Policy Year	2012	2012
Post-Policy Year	2018	2018

Table A.8. Regression kink discontinuity (RKD) model estimates the change in cohort graduation rate since 2012 as the outcome variable. The model is estimated at the district-level with a kink at the 55% UPP threshold for concentration grants. The coefficient represents the change in the slope of the outcome variable when moving from below to above the kink. The symbols *, **, and *** denote estimates significant at the 1%, 5%, and 10% confidence levels, respectively.

Table 9

	Coeff. Est.	Std. Dev.
Pop	627.7***	(241.0)
Avg. HH Size	0.0538***	(0.0157)
% Seniors	-0.609***	(0.194)
% Children	0.604**	(0.240)
Mean HH Inc.	-3055.7***	(937.9)
Med. HH Inc.	-2,151.1***	(773.5)
% Non-English	1.290***	(0.381)
% Hispanic	8.089***	(2.979)

Table A.9. Difference-in-difference model estimation for district-level demographics. Regression is estimated according to equation (4), where the outcome variable of interest is listed at the top of each column. The pre-period is from 2011 to 2012, and the post-period is 2017-2018. At a threshold of 55% UPP, districts qualify for a concentration grant, so the coefficient estimate represents the effect of receiving a concentration grant on district demographics. The treatment and control groups are defined by whether the district is above (treatment) or below (control) the threshold, but no further away than the bandwidth for the specification. The symbols *, **, and *** denote estimates significant at the 1%, 5%, and 10% confidence levels, respectively.

A.2. Figures

Figure 1

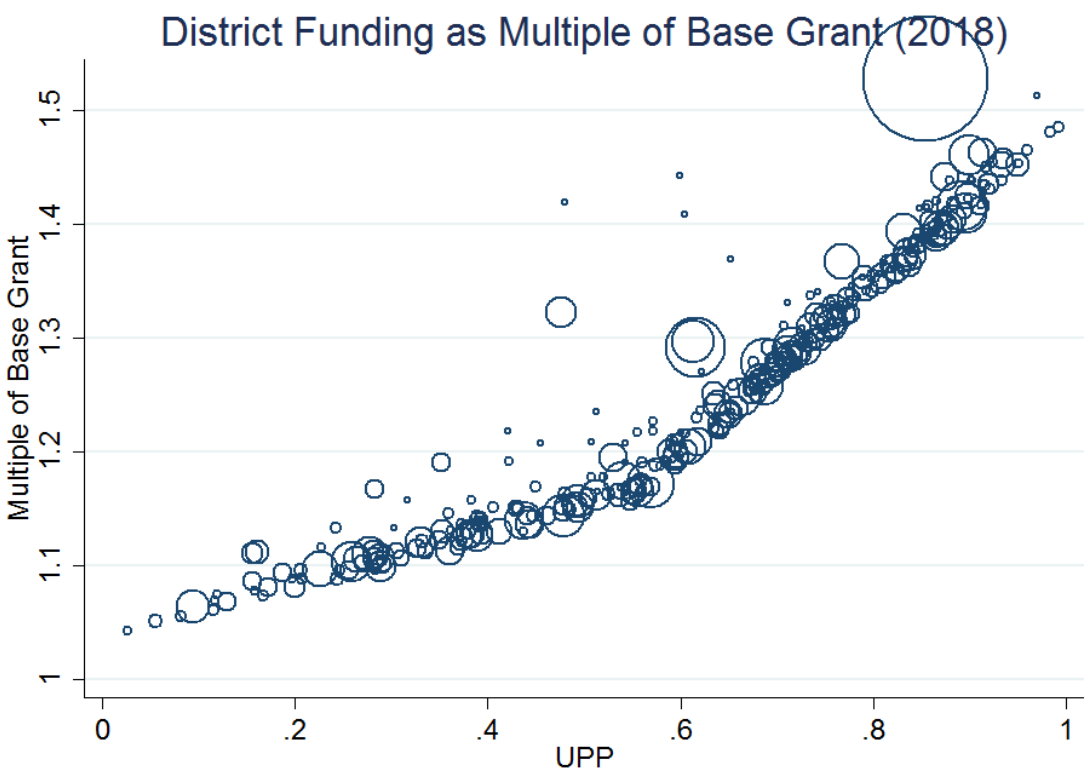

Figure A.1. Plot of per-student district budgets in 2018. The per-student budget is expressed as a multiple of the expected base-grant per student, adjusted by grade-level composition of the district. The unduplicated pupil proportion (UPP) represents the proportion of the district that the state considers disadvantaged. At a UPP of 55%, a district is eligible to begin receiving concentration grants, a source of additional funding from the state.

Figure 2

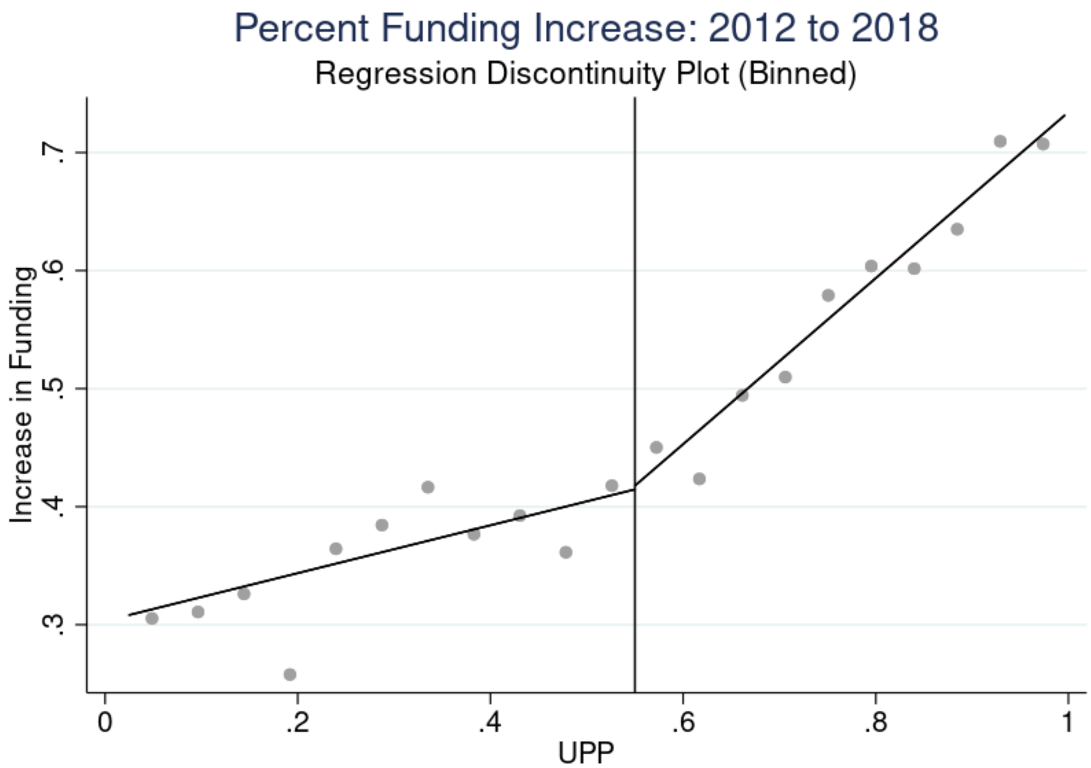

Figure A.2. Binned plot of the percent change in funding per student from 2012 to 2018. Funding change from 2012 to 2018 in percent terms is kinked around 55% UPP. Low UPP (low poverty) districts saw only a moderate increase in funding per student. High UPP (high poverty) districts saw a very large increase in funding per student.

Figure 3

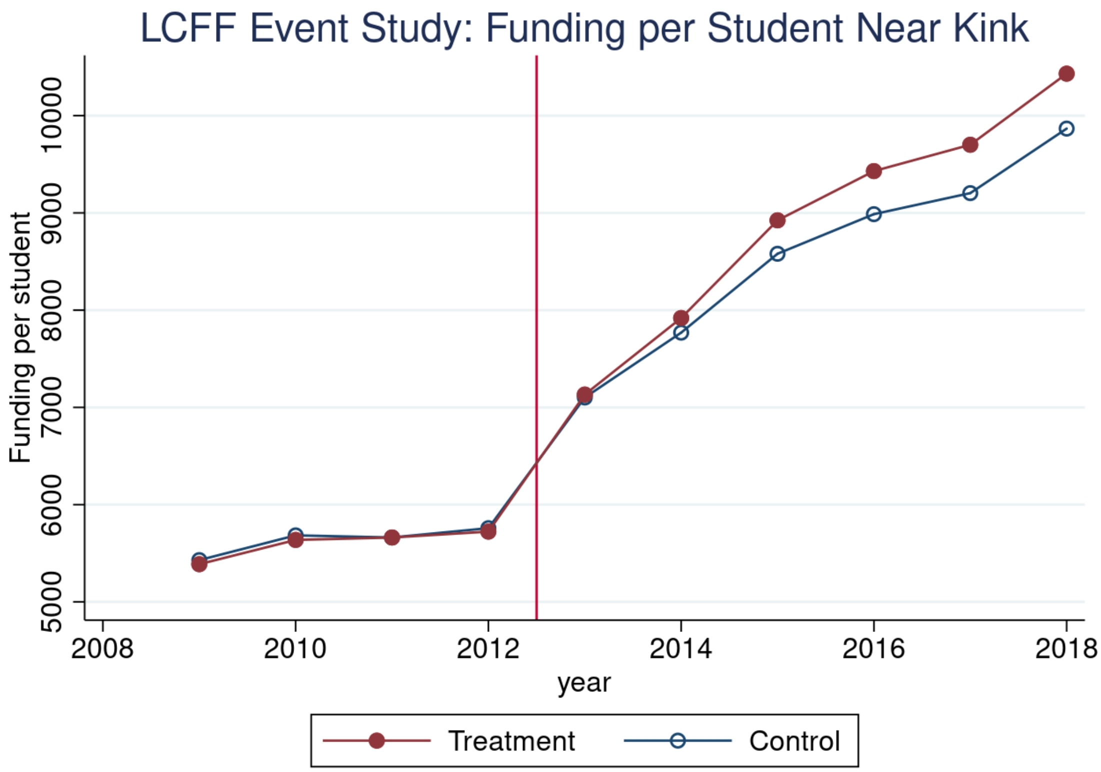

Figure A.3. Plot of the per-student budgets over time of two groups of districts: those who were above vs. below the 55% concentration grant threshold UPP, within a 15% bandwidth..

Figure 4

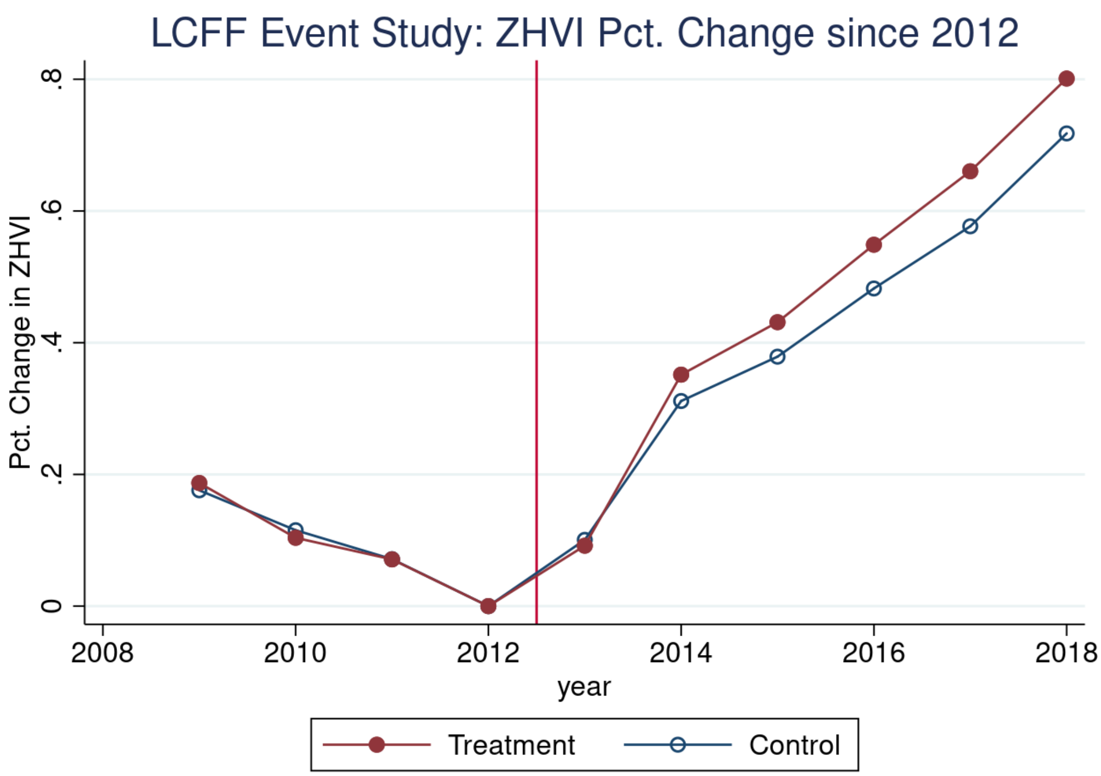

Figure A.4. Plot of the percent change in ZHVI since 2012 for the difference-in-difference specification around the concentration grant threshold. The bandwidth here is 15%; districts above (below) the 55% UPP threshold but within 15% of the cutoff are considered treated (control).

Figure 5

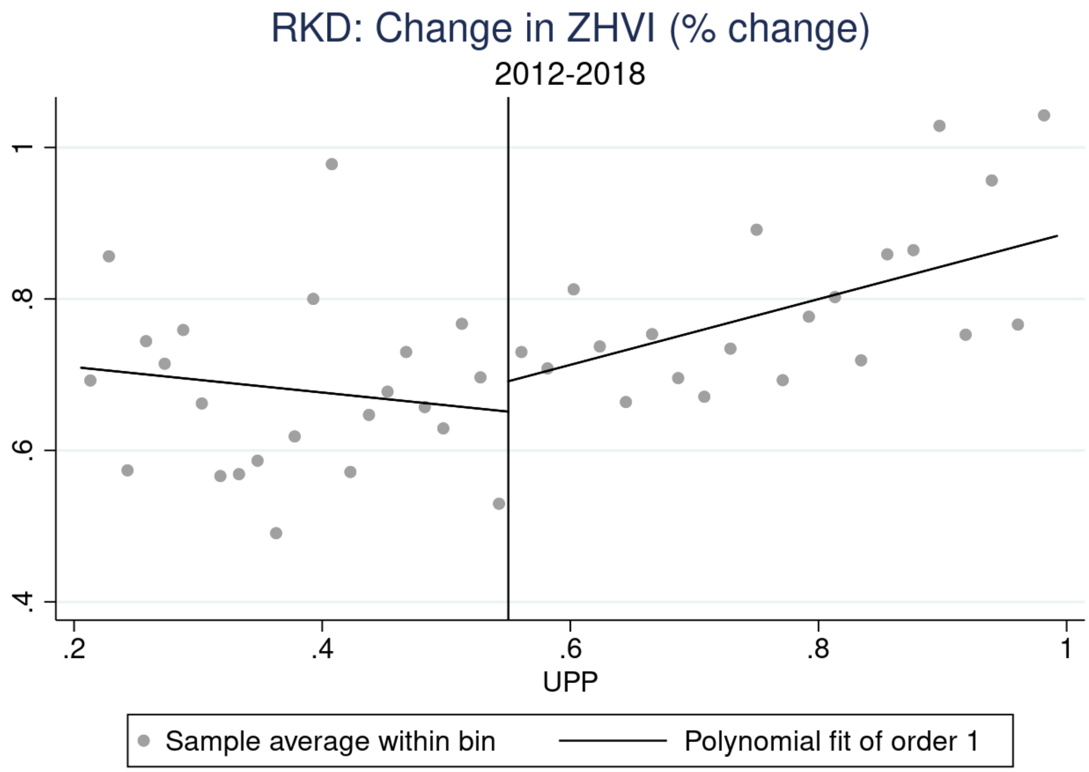

Figure A.5. Plot of changes in the house price index by ZIP code UPP. The observations are binned according to UPP and averaged, a standard technique in visualizing RKDs.

Figure 6

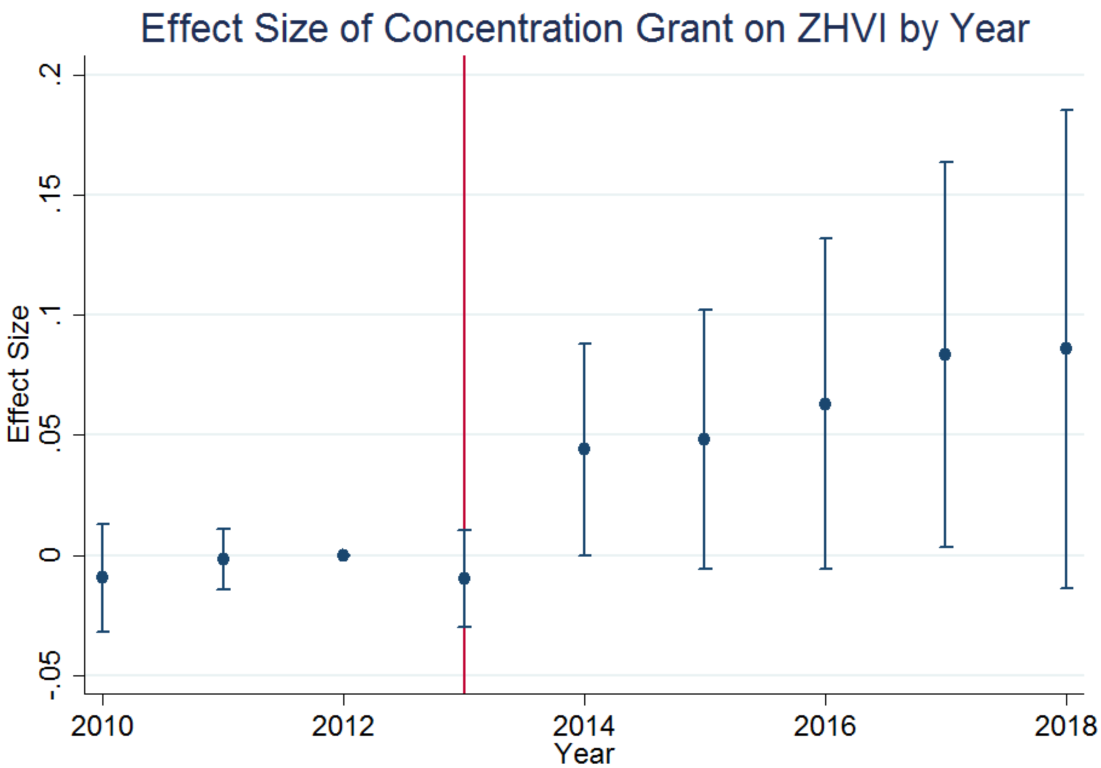

Figure A.6. Plot of the difference in the percent change in ZHVI by year between the treatment and control groups (15% bandwidth around the 55% concentration grant cutoff).

Figure 7

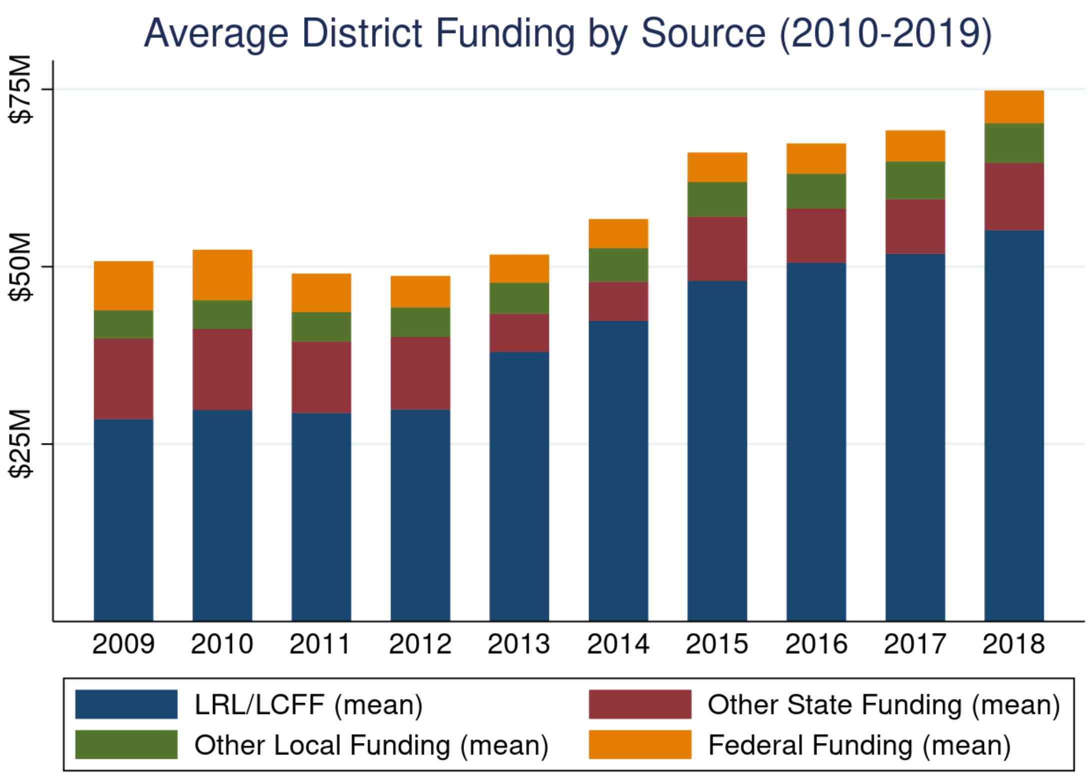

Figure A.7. The average proportion of school budget sources by year.

Figure 8

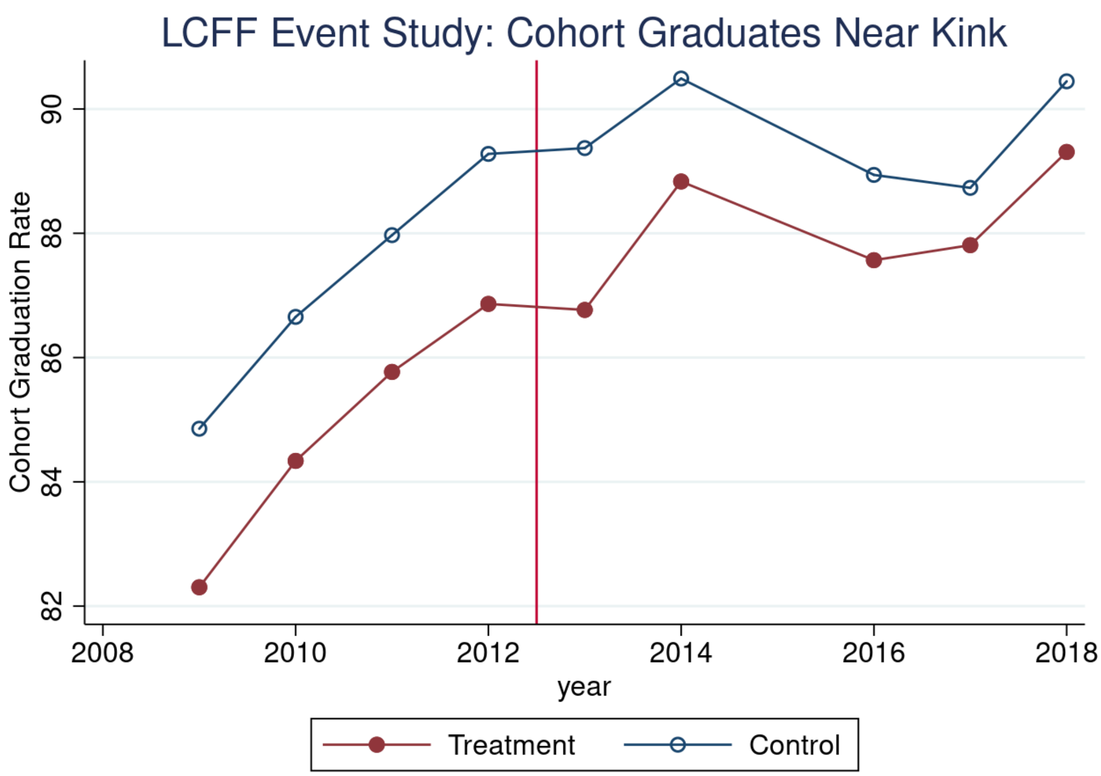

Figure A.8. Plot of the change in graduation rates over time for districts within a bandwidth $h = 15\%$ of the concentration grant threshold. The base year is 2012, and the policy went into effect the following school year.

Figure 9

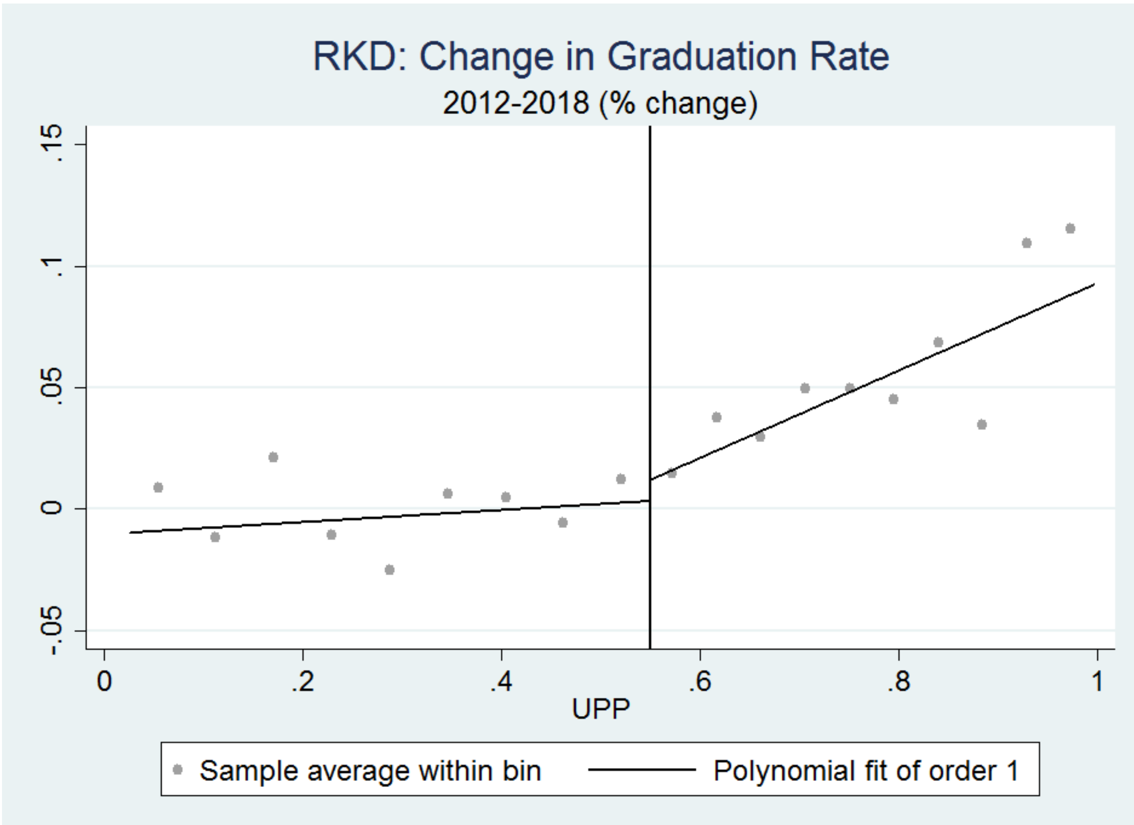

Figure A.9. Plot of changes in graduation rate for each district from 2012-2018 against UPP. Observations are binned and presented as the averages for the bin. A distinct transition occurs around the threshold where concentration grants begin at $UPP = 55\%$.

Figure 10

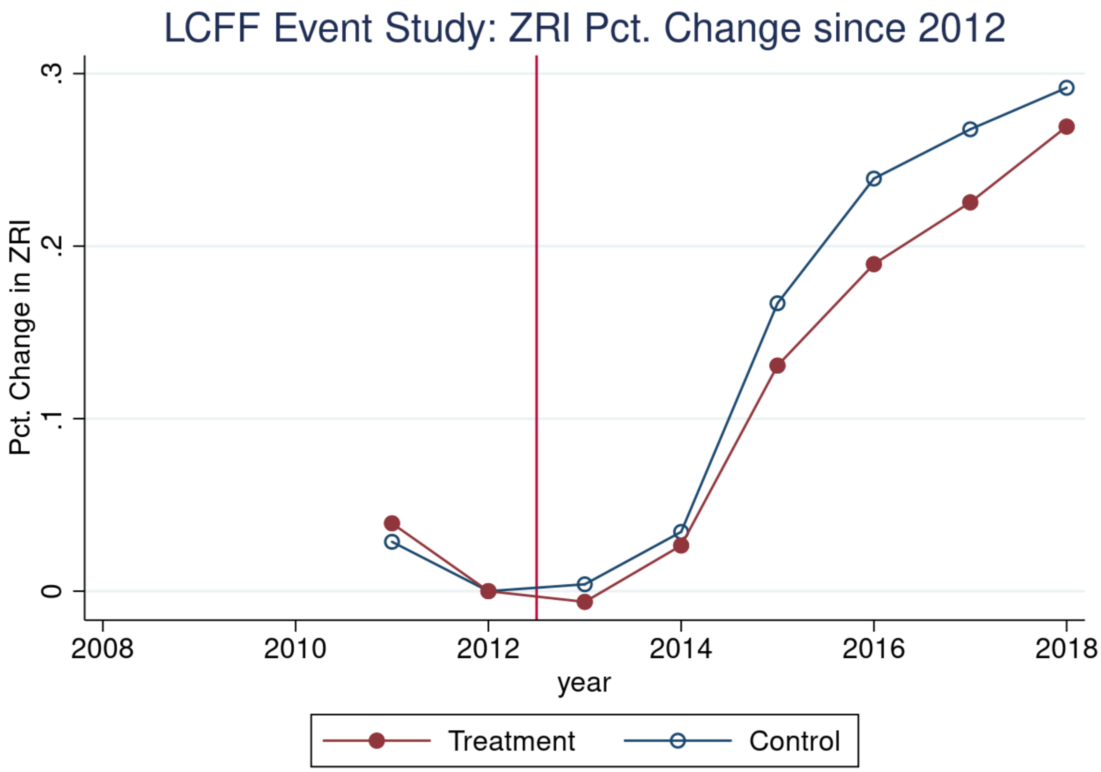

Figure A.10. Plot of the percent change in ZRI since 2012 for the difference-in-difference specification around the concentration grant threshold. The bandwidth here is 15%; districts above (below) the 55% UPP threshold but within 15% of the cutoff are considered treated (control).

Figure 11

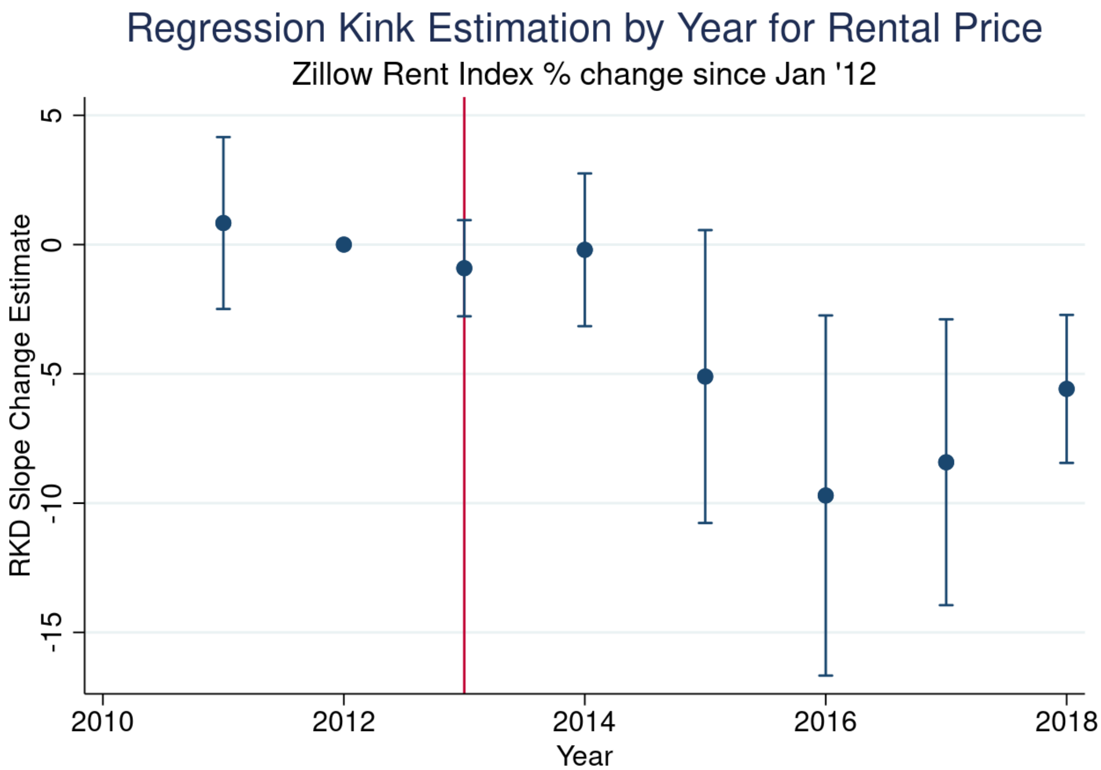

Figure A.11. Plot of the difference in the percent change in ZRI by year between the treatment and control groups (15% bandwidth around the 55% concentration grant cutoff).

APPENDIX B

Figures and Tables from Chapter 2

B.1. Tables

Table 1

	Obs	Mean	Std. Dev.
Panel A - Moratoria Policies			
State Active Policy (Dummy, as of April 30, 2020)	50	0.84	0.37
State Active Policy (Dummy, as of April 30, 2021)	50	0.24	0.43
State Moratoria Overlaps ERA (Dummy)	50	0.36	0.48
Evictions (per city, weekly)	2,807	217.5	348.0
Panel B - Housing			
Absentee Owner (Dummy)	517,803,188	0.1100	0.3129
Sale (Dummy)	517,803,188	0.0045	0.0665
Foreclosure (Dummy)	517,803,188	0.0002	0.0143
Current Moratoria (Dummy)	517,803,188	0.2568	0.4369
Sale Price, USD (Winsorized at 0.05%)	2,304,500	588,289	1,646,835

Table B.1. Summary Statistics

Table 2: Eviction Regression Results

	(1)	(2)	(3)	(4)	(5)
Active State Moratorium	-217.43***	-	-94.90***	-225.08***	-225.08***
	(12.62)		(15.43)	(12.82)	(65.26)
Active Federal Moratorium	-	0.733	-	-29.48***	-29.48
		(9.55)		(9.22)	(18.11)
Regression Constant	282.71	217.14	119.87	299.00	299.00
City FE	Yes	Yes	Yes	Yes	Yes
Time FE	No	No	Yes	No	No
Clustered?	No	No	No	No	Yes
Num. Obs.	2,807	2,807	2,807	2,807	2,807
Num. Cities	30	30	30	30	30

Table B.2. *Notes*: Regression of eviction filings in 30 selected cities tracked by the Eviction Lab from Jan 1, 2020 through June 30, 2021 on dummy variables representing the presence of an active state or federal moratorium. The outcome variable is eviction filings at the city-week level. Standard errors are in parentheses. A *, **, or *** denotes statistical significance at the 0.1, 0.05, or 0.01 level, respectively.

Table 3: Moratorium Expiration Results

	(1)	(2)
Dependent Variable:	Sale Indicator	Foreclosure Indicator
Absentee Owner	0.00112***	0.00010***
	[116.66]	[51.75]
Moratorium Expiration	0.00305***	4.07×10^{-5}***
	[97.12]	[6.01]
Absentee × Expiration	0.00049***	1.62×10^{-5}
	[6.05]	[0.94]

Table B.3. *Notes*: $N = 517,803,188$. The sample contains quarterly tax filings from a 5% random subset of properties from the largest 20 metropolitan statistical areas. These properties were matched with transaction and foreclosure data. The outcome variables, Sale Indicator and Foreclosure Indicator, are dummy variables indicating a sale or foreclosure occurred in the given month. Absentee Owner is an indicator variable representing whether the property is not owner occupied (a proxy for rental properties). Moratorium Expiration is an indicator variable representing if the local eviction moratorium ended in the previous month. Absentee × Expiration is the difference in difference estimator of interest. Numbers in brackets are p-values.

Table 4: ERA Overlap Results

	(1)	(2)
Dependent Variable:	Sale Indicator	Foreclosure Indicator
Absentee Owner	0.0296***	0.000738
	[6.76]	[1.27]
ERA Overlaps Moratorium	-0.0266***	-0.000356
	[-12.66]	[-1.28]
Absentee × Overlap	-0.0216***	0.000171
	[-3.49]	[0.21]

Table B.4. *Notes*: $N = 105,943$. The sample contains quarterly tax filings from a 5% random subset of properties from the largest 20 metropolitan statistical areas. These properties were matched with transaction and foreclosure data. The outcome variables, Pandemic Sale and Pandemic Foreclosure, are dummy variables indicating a sale or foreclosure occurred after March 1, 2020. Absentee Owner is an indicator variable representing whether the property is not owner occupied (a proxy for rental properties). ERA Overlaps Moratorium is an indicator variable representing if the local eviction moratorium was still active when the ERA programs began in Decemeber, 2020. Absentee × Overlap is the difference in difference estimator of interest. Numbers in brackets are p-values.

B.2. Figures

Figure 1

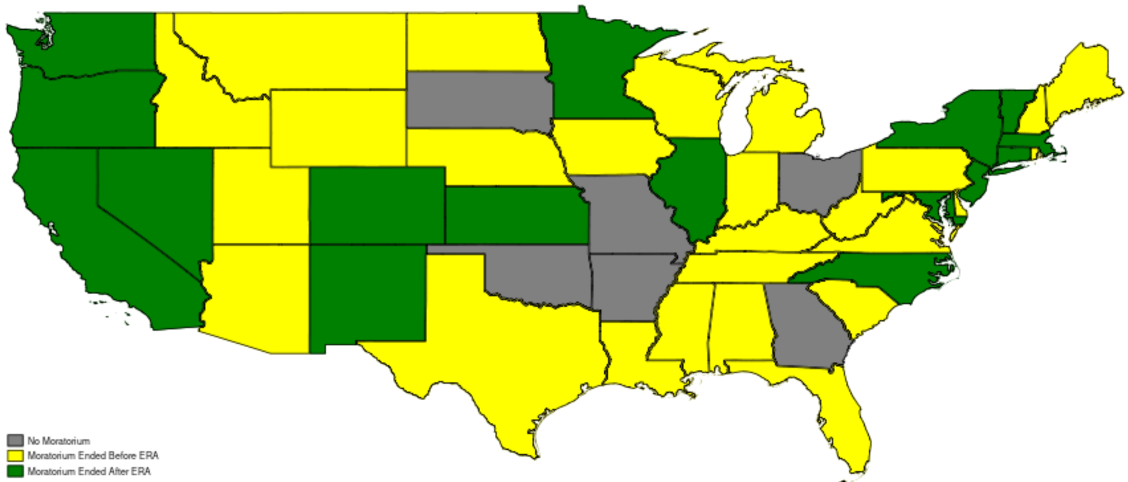

Figure B.1. State moratoria durations, with an indication of whether the moratoria expired before or after the implementation of ERA programs (Dec. 27, 2020) or whether no moratorium was implemented at a state level.

Figure 2

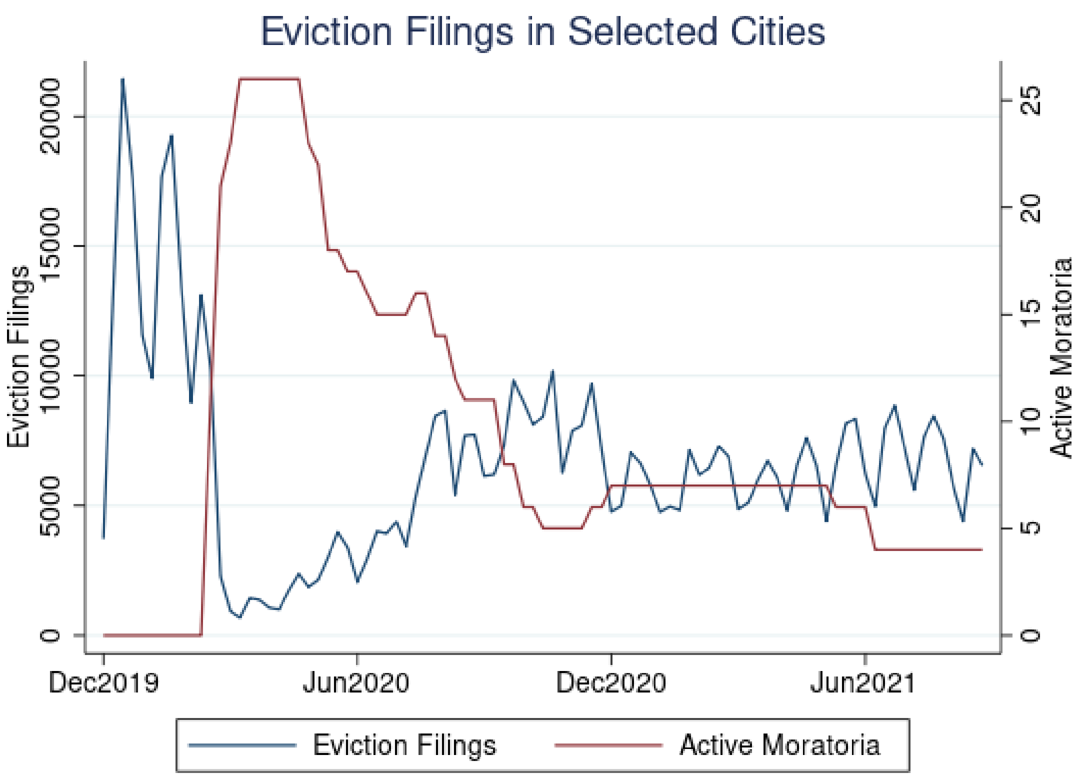

Figure B.2. The number of eviction filings by week in thirty selected cities tracked by the Eviction Lab. The eviction filing frequency is overlaid with the number of cities in the sample with an active local eviction moratorium.

Figure 3

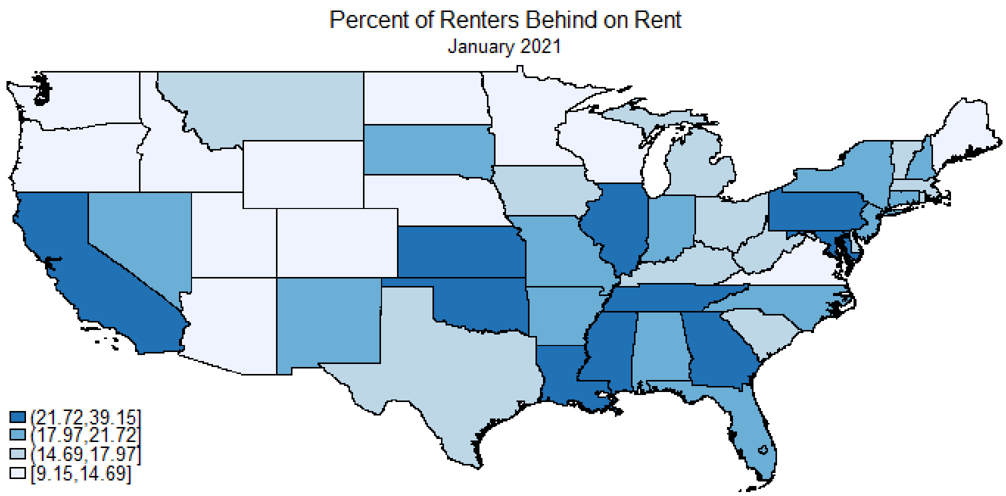

Figure B.3. Percent of renters behind on rent by state, as of January, 2021. Data from the US Census Household Pulse Survey.

APPENDIX C

Figures and Tables from Chapter 3

C.1. Tables

	Number of Records	Percent of Sample
Renters	9,391	65%
Pets	7,571	53%
Flags	157	1%
Yard	4,394	31%
Parking	6,717	47%
All	14,360	100%

Table C.1. Number of CC&Rs in sample mentioning specific topics. The sample was formed from the universe of digitized New York City CC&R records filed in 2003-2018 and a subset of digitized Chicago CC&R filings.

C.2. Figures

Figure 1

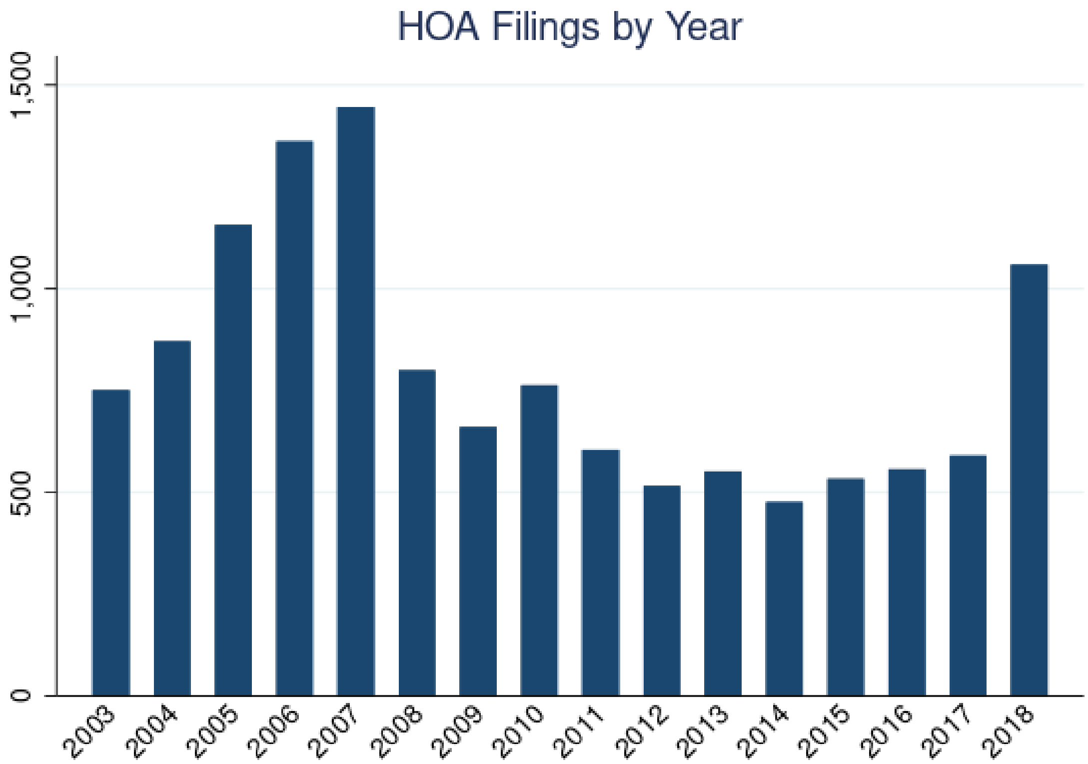

Figure C.1. Number of HOA filings in sample by year. The sample was formed from the universe of digitized New York City CC&R records filed in 2003-2018 and a subset of digitized Chicago CC&R filings.