Is Sharing Really Caring? The Effect of Airbnb on Housing Prices and Foreclosures*

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Abstract

We examine the impact of short-term housing rental platforms like Airbnb on housing affordability. Using a simple model we illustrate that, although growth in the short-term rental market leads to higher housing prices, home sharing profits can offset increased housing costs. That is, we show the net effect of the rise of platforms like Airbnb on housing affordability is theoretically ambiguous. To address this ambiguity empirically, we leverage city-level registration requirements that substantially increase the cost of hosting on Airbnb to estimate the effect of policy enforcement on the Airbnb market and the corresponding effects on home prices and foreclosures. Using data on Airbnb listings and transactions from the San Francisco and Chicago metro areas, we find that enforcing registration requirements on Airbnb listings within the city limits of Chicago and San Francisco reduced Airbnb supply and bookings by 40%, relative to untreated areas within the two metros. We find substantial heterogeneity, with the largest shocks experienced by neighborhoods with the highest per-capita density of Airbnb listings. Using transaction-level data from Zillow on home sales and foreclosures, we find that home prices decline by roughly 10% in the most Airbnb-dense census tracts while foreclosures in these same tracts *increase* by roughly 0.07 per tract-month (or 117%). These findings suggest that, while growth in home sharing platforms increases housing prices, it may also improve individuals' ability to avoid negative financial outcomes like foreclosure.

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1 Introduction

Rapidly growing sharing economy platforms have disrupted traditional markets globally over the past decade. One of the largest is Airbnb, the home sharing platform with a market capitalization of roughly \$100 billion,[1] which has substantially expanded the availability and utilization of housing accommodations for travelers (Farronato and Fradkin, 2018, Li and Srinivasan, 2019, Zervas et al., 2017). Despite massive success, home sharing platforms have been sharply criticized by residents and policymakers in urban areas who argue that the platforms negatively impact housing affordability. This criticism is driven by the concern that homeowners and real estate investors are dramatically shifting supply toward short-term rental markets, away from long-term residential markets, and thus driving up long-term housing prices and displacement rates.[2]

Indeed, home prices should unambiguously rise as the supply of long-term housing declines. However, home sharing also unlocks the opportunity for owners and long-term renters to turn underutilized housing capacity into additional income, which may more than offset the impact of increased housing prices on affordability. A notable illustration of this is provided by Caroline Lupini's Business Insider article, where she discusses how renting her home on Airbnb for 3 months pays her mortgage for the rest of the year.[3]

We argue that fully capturing the impact of home sharing on housing affordability requires examining measures of housing affordability and stability beyond prices. One such example is foreclosure, which reflects an individual's inability to consistently make their mortgage

[1]This figure reflects Airbnb's public valuation at the beginning of May 2021.

[2]For example, in a display of outrage toward Airbnb, one New Orleans resident spray-painted "This Airbnb displaced 5 people" on the sidewalk in front of an Airbnb listing: `https://thelensnola.org/2018/02/10/this-airbnb-displaced-5-people-heres-the-story-behind-that-photo-that-spread-on-facebook/`. A photo of this spurred residents to lobby for Airbnb regulation to help curb local displacement and gentrification. To combat such concerns, starting in San Francisco and Los Angeles County, Airbnb pledged \$25 million to support affordable housing. `https://www.latimes.com/business/story/2019-09-17/airbnb-pledges-25-million-to-support-affordable-housing-and-small-business`

[3]See "Renting my place on Airbnb for 3 months pays my mortgage for the rest of the year, and 4 more reasons I love it." at `https://www.businessinsider.com/renting-my-place-airbnb-for-3-months-pays-mortgage-for-the-year`.

and property tax payments. We start by theoretically illustrating that the net effect of home sharing platforms on housing affordability is ambiguous. To do this, we develop a model of home-ownership that accounts for home prices, foreclosure, and the presence of sharing. We show that all else equal a decrease in the cost of home sharing, attributable to something like the introduction of Airbnb, increases home prices but has no impact on aggregate housing affordability. Rather, the introduction of Airbnb redistributes housing market consumer surplus according to individuals' willingness and ability to profitably host short-term renters. Moreover, we show that, conditional on already owning a home, an increase in expected home sharing profits makes housing *more* affordable on average and reduces risk of negative financial outcomes like foreclosure.

To empirically address the theoretical ambiguity, we estimate (i) the Airbnb supply and booking effects of large city-level policy shocks that dramatically increase the cost of hosting one's property on Airbnb, and (ii) the corresponding effects on home prices and foreclosures. To do this, we use agreements between Airbnb and the cities of Chicago (December 2016) and San Francisco (September 2017) to require Airbnb hosts to register their listings with the city, and then post their registration numbers on their listing pages. In both cases, Airbnb assisted with enforcement by removing listings that remained unregistered a few months following initial policy implementation. For example, Airbnb removed almost 5,000 listings in San Francisco (nearly 50%) in January 2018, four months following initial implementation in September 2017.[4] In Chicago, Airbnb dropped listings from the site if they were not registered by May 1, 2017, roughly 4.5 months following initial policy implementation.[5] These registration requirements represent non-trivial cost shocks, as they may entail registration fees ($450 every two years in San Francisco, none in Chicago), reduced ability to evade applicable federal, state, and local taxes (Bibler et al., 2021), inability to list multiple units in San Francisco due to the existing "One Host, One Home" (OHOH) policy that incidentally

[4] See https://www.sfchronicle.com/business/article/Airbnb-listings-in-San-Francisco-plunge-by-half-12502075.php.

[5] See https://www.chicagotribune.com/business/ct-chicago-airbnb-rules-enforcement-0803-biz-20170802-story.html.

became enforced by Airbnb (Chen et al., 2021), notification of landlord when a long-term renter registers in San Francisco, inability to list units on Chicago's "Prohibited Buildings List" nor those not zoned as residential, as well as additional potential regulatory burdens and oversight.[6]

To credibly obtain causal estimates from these city-level policy shocks, we exploit three dimensions of variation. The first is temporal variation, using Airbnb and housing data before and after policy implementation and enforcement. The second is spatial variation, comparing outcomes from Census tracts within the San Francisco and Chicago city limits to those outside of the city limits but within the same metro areas. Third, we exploit variation in the "bite" of the policies among treated census tracts, which is driven by differing levels of pre-existing Airbnb activity.

To confirm that the registration policies substantially impact the Airbnb market, we use data scraped from Airbnb.com by AirDNA from mid-2014 through mid-2019 on prices, bookings, and property characteristics from listings in the San Francisco and Chicago metro areas. Using these data, we find that the probability a treated listing is available in any given month declines by 10 percentage points (40%) and the number of nights booked per listing-month declines by 0.9 (37%) relative to untreated listings in these two metro areas. We find the negative shocks persist over time, and the largest shocks were indeed experienced by tracts with the highest pre-policy density of Airbnb listings per capita. We show these results are robust to alternate specification choices, and event studies demonstrate the lack of systematic pre-trends leading up to treatment.

To the extent that negative supply shocks reallocate housing back to long-term use, we would expect these policy shocks to partially reverse the housing market impacts of a decade of dramatic growth in Airbnb. To test this, we use transaction-level data from Zillow on home sales and foreclosures from mid-2014 through mid-2019. Combining the policy shocks

[6]See https://www.airbnb.com/help/article/1849/san-franciscos-registration-process-frequently-asked-questions and https://www.airbnb.com/help/article/1495/chicago-homesharing-registration-frequently-asked-questions.

with tract-level variation in pre-existing density of Airbnb listings per capita, we find a decline in home prices of roughly 10% among homes sold in the most-dense quartile of tracts — those that were most impacted by the city-level policy shocks — and little to no effect in the lower-density quartiles. This pattern of results suggests some combination of a relative increase in supply and decrease in demand for homes in the most popular areas for Airbnb.

We then examine the effects of the policy shocks on foreclosures to assess the impacts on housing affordability more broadly. In the most Airbnb-dense quartile of zip codes, we find an *increase* in foreclosures of 0.07 per tract month, or 117% relative to a baseline average of 0.06 foreclosures per tract-month. This amounts to 0.24 additional foreclosures per month following a reduction of 100 available Airbnb listings. In the third quartile, we find an increase of 0.06, or 46% relative to a baseline average of 0.13 foreclosures per tract-month. This amounts to 1.3 additional foreclosures per month following a reduction of 100 available Airbnb listings. We find no effect in the second quartile. This pattern of results corresponds well to our quartile-specific estimates of the Airbnb market and housing price effects. As with the Airbnb analyses, we show that both the price and foreclosure results are robust to alternate specification choices, and event studies demonstrate the lack of systematic pre-trends leading up to treatment. Our foreclosure results emphasize that researchers ought to examine more than just prices when assessing the relationship between Airbnb and housing affordability. Indeed, while restricting the Airbnb market seems to reduce housing prices, it also seems to make housing *less* stable and affordable for property owners.

Our paper makes several contributions to our understanding of Airbnb, its regulation, and its impact on the housing market. We first document the impacts of the negative supply shocks generated when Airbnb helped San Francisco and Chicago enforce registration requirements, which represent perhaps the largest policy-driven Airbnb shocks studied in the literature to date. Unlike many studies using only information on Airbnb listings from sources like InsideAirbnb, we use much more detailed data from AirDNA to document that these negative supply shocks do more than just reduce slack. That is, we find corresponding large reductions in nights booked. Moreover, we find that the effects are stronger in areas with a larger pre-treatment Airbnb presence, and we find that the timing of the observed effects is consistent with earlier research suggesting that regulation without Airbnb cooperation is much less effective (e.g., Bibler et al., 2021).

Our findings are also consistent with earlier work documenting the positive relationship between Airbnb prevalence and housing prices (e.g., Barron et al., 2021, Chen et al., 2021, Duso et al., 2020, Garcia-López et al., 2020, Garcia et al., 2020, Horn and Merante, 2017, Koster et al., 2021, Sheppard et al., 2016). For example, Barron et al. (2021) use geographic variation in the gradually-increasing prevalence of Airbnb over time to show that a 1% increase in Airbnb listings leads to a 0.018% increase in rents and a 0.026% increase in home values. They also show that, while the total supply of housing is not affected by the entry of Airbnb, Airbnb growth reduces the supply of long-term rental units. Using similar variation, Sheppard et al. (2016), Horn and Merante (2017), Garcia-López et al. (2020), and Li et al. (2020) all find increases in housing prices and rents following Airbnb growth across several unique metro areas. An exception is Fontana (2021) who uses similar variation in London but finds no effect on housing prices. Duso et al. (2020), Garcia et al. (2020), Chen et al. (2021), and Koster et al. (2021) all leverage quasi-experimental variation resulting from policies seeking to restrict Airbnb activity, finding that a reduction in the number of Airbnb listings translated to reductions in housing and rental prices. Notably, however, these existing studies solely focus on home sale and rental prices as measures of affordability. We are the first to consider an expanded conception of affordability by including an analysis of foreclosures, which addresses the theoretical ambiguity that arises once we account for the additional income that individuals can earn once they can host on Airbnb.

By documenting geographic heterogeneity in the effects of Airbnb on housing affordability, we also contribute to a broader understanding of Airbnb's distributional welfare effects. For example, within this space, Calder-Wang (2021) estimates a structural model of residential choice to determine the effect of housing reallocation (from long-term to short-term rentals) on rents across different housing types and demographic groups, finding that Airbnb's presence in New York City reduces surplus to long-term renters by $178 million per year due to higher rental prices. Moreover, she finds that the increased burden falls most heavily on high-income, educated, and white renters whose preferences for housing and location amenities are most similar to tourists'. Similarly, Farhoodi (2021) estimates a structural model using Chicago data, and finds that welfare gains are concentrated in high-income and high-price neighborhoods. However, within neighborhoods, he finds that Airbnb increases surplus more for homeowners who are relatively low-income.

Our work also relates to literature on Airbnb and housing that models the role of Airbnb in home ownership decisions, housing-related surplus, spillover costs and benefits, and optimal policy-setting (Filippas et al., 2020, Filippas and Horton, 2020), as well as explores the effects of Airbnb on local amenities (Almagro and Domínguez-Iino, 2020) and local economic activity (Basuroy et al., 2020). Moreover, our paper contributes to a broader literature on the United States' housing affordability crisis in light of increasing news coverage and recent studies offering new explanations (e.g., Cosman and Quintero, 2019, Diamond et al., 2019).

The paper proceeds as follows. Section 2 presents our conceptual framework describing home ownership and foreclosure in the context of Airbnb. Section 3 describes the Airbnb and Zillow housing data we use. Section 4 develops our estimation strategy. Section 5 presents our results. Finally, Section 6 concludes.

2 Conceptual Framework

In this section we develop a simple framework that describes house purchasing, home sharing, and foreclosure decisions. The objective is to illustrate the ambiguous nature of the impact of home sharing on housing affordability by looking beyond the effect on housing prices.

Suppose there exists a unit mass of potential home buyers.[7] Homes are homogeneous and

[7]For simplicity, we focus on individuals considering a home purchase and ignore existing homeowners. Note that existing homeowners prior to entry of short-term rental platforms will tend to benefit, either directly or indirectly, from the new use of existing capital.

have fixed supply so that $Q_S = \overline{Q} \in (0,1)$. Note that $\overline{Q} \in (0,1)$ implies an interior solution (not all individuals will be able to purchase a home under downward sloping demand). Individual i earns the following utility from purchasing a home:

$$u_i = \alpha_i + \max\{R_i - c, 0\} - p + \epsilon_i,$$

where $\alpha_i > 0$ denotes individual i's initial willingness to pay for a home, R_i denotes the expected revenues from home sharing for individual i, $c > 0$ denotes the costs associated with home sharing, p denotes the price of the home, and ϵ_i captures a shock on individual i's willingness to pay which occurs after the home purchase decision (e.g., an income shock). Let α_i be drawn from a distribution $F(\cdot)$, R_i drawn from distribution $G(\cdot)$, and ϵ_i drawn from distribution $H(\cdot)$ with $E[\epsilon_i] = 0$ for simplicity.

There are two features of the individual's utility function that are important to highlight. First, the max-operator captures individual i's decision to share their home. If individual i purchases a home and $R_i > c$, then individual i shares their home and earns $R_i - c > 0$. This additional surplus increases individual i's willingness to pay for a home by the amount of the surplus. If individual i purchases a home with $R_i < c$, then they will not share their home and their utility from owning a home is a function of α_i, ϵ_i, and p only.

Second, the c variable captures many features related to home sharing platform entry and economic policy. For example, if home sharing is prohibitively difficult (e.g., sharing platforms do not exist), then $c = \infty$ and there is no home sharing. Instead, the presence of platforms like Airbnb reduces c and ensures that $c < R_i$ for some. In terms of sharing economy policy, a ban, registration fees, or taxes on short-term rentals can be represented by an increase in c. Thus, comparative statics on c will be important throughout our analysis.[8]

The timing of the game is as follows: individuals first observe c, α_i, and R_i. They then take expectations on ϵ_i and make home purchasing decisions that result in an equilibrium price that clears the market. Lastly, individuals realize their ϵ_i, and individual i goes into foreclosure if they purchased a home but end up with $u_i < 0$.[9]

Solving the game backwards, individual i purchases a home if $\alpha_i + \max\{R_i - c, 0\} - p + E[\epsilon_i] = \alpha_i + \max\{R_i - c, 0\} - p \geq 0$. This implies that the demand for homes is given by

$$
\begin{aligned}
Q_D &= \Pr(\alpha_i + \max\{R_i - c, 0\} > p) \\
&= \Pr(c \geq R_i \ \&\ \alpha_i > p) + \Pr(c < R_i \ \&\ \alpha_i + R_i - c > p). \qquad (1) \\
&= \int_{-\infty}^{c} \int_{p}^{\infty} dF(\alpha_i) dG(R_i) + \int_{c}^{\infty} \int_{p-R_i+c}^{\infty} dF(\alpha_i) dG(R_i).
\end{aligned}
$$

Setting $Q_D = Q_S = \overline{Q}$ implicitly defines the market clearing price for homes which we denote by $p^*(c)$. Naturally, housing prices decrease as short-term rental costs increase:

Proposition 1. *If home sharing costs increase, then housing prices decrease:* $\frac{\partial p^*(c)}{\partial c} \in [-1, 0)$. *Furthermore, if* α_i *is distributed uniformly, then* $\frac{\partial p^*(c)}{\partial c} = \frac{\partial E[\max\{R_i - c, 0\}]}{\partial c}$.

The natural implication here is that any short-term rental policy that increases costs will reduce local housing prices. Another important implication of this result is that moving from a world without home sharing platforms, $c = \infty$, to a world where home sharing platforms are used, $c < R_i$ for some, implies that housing prices increase. Furthermore, if housing demand is linear (i.e., α_i is distributed uniformly), then we have a straightforward narrative: the change in housing prices corresponds to the change in expected home sharing profit.

[8] In reality, both home sharing revenues and costs will differ across individuals. For example, some hosts travel more than others and can offer greater availability to generate more revenue, while some hosts may face large costs such as strong preferences for privacy. However, such a framework prevents a straightforward comparative statics approach to policy shocks. Hence we allow revenues to differ across individuals, generating idiosyncratic profits from sharing, and we maintain a homogeneous cost parameter across individuals to allow for comparative static exercises that correspond to Airbnb growth and short-term rental policies. Ultimately, the trade-offs are the same for the general setting, and so the main qualitative effects generated in this model will remain.

[9] We normalize an individual's outside option to zero.

It is important to note, however, that increases in housing prices brought on by home sharing (through a reduction in c) do not reduce housing affordability for all individuals. To determine the effect that home sharing has on home-purchasing power across individuals, we compare the set of individuals who purchase a home when $c = \infty$ with the set who purchase a home when $c < R_i$ for some i. Figure 1a captures the case where $c = \infty$. If $c = \infty$, the decision to purchase a home is completely determined by α_i such that individual i purchases a home if $\alpha_i > p^*(c = \infty)$. The set of individuals that purchase a home is the rectangle with area $\overline{Q}$. On the other hand, the presence of home sharing platforms means that $c < R_i$ so that some individuals with $\alpha_i < p^*(c = \infty)$ will participate in the market. This entry drives up housing prices, as shown in Figure 1b with $p^*(c < R_i) > p^*(c = \infty)$ so that the housing market clears by altering the set of home buyers in equilibrium.

Finally, Figure 1c depicts the winners and losers from a change in c (in terms of home-purchasing power). Not surprisingly, home sharing induces some individuals to become home buyers. In particular, those with high R_i and moderate α_i (denoted by W_1 in Figure 1c) who would not have purchased a home when $c = \infty$, will be home buyers when $c < R_i$. At the same time, individuals with $c > R_i$ and moderate α_i (denoted by L_1 in Figure 1c) are priced out of the market when home sharing becomes available. These represent extensive margin effects, because they change the set of home buyers. There are also intensive margin effects from changes in utility. In particular, those who purchase a home but do not share it are worse off because they incur a higher price without receiving the direct benefits of home sharing (denoted by L_2 in Figure 1c), while those who do share have higher surplus when home sharing is allowed (denoted by W_2 in Figure 1c).[10]

Altogether, this implies that home sharing acts as a redistribution of purchasing power within the housing market (and is neutral at the aggregate level), by reallocating surplus

[10]It is important to note that these effects assume that people only purchase one home. If a mass of individuals purchase additional homes for the purpose of home sharing (e.g., investors), then the gains accrue to a more limited set of local home buyers. In this case, the presence of so-called investors effectively shifts the vertical and diagonal lines in Figures 1b and 1c to the right. Focusing on Figure 1c we see that the presence of home sharing investors expands L_1 and contracts W_1 so that local residents purchase fewer homes and are worse off generally speaking.

from those who are unwilling to share to those who are. Furthermore, if there is correlation between income and willingness to share (e.g., if willingness to share and income are negatively correlated), then home sharing acts as a redistribution mechanism, transferring surplus from high income to low income individuals (allowing them to purchase a home which they then share). This correlation corresponds to a collapse in L_1 and W_2 in Figure 1c so that home sharing may have a redistribution effect — reducing surplus of higher-income individuals and enabling home purchases among lower-income individuals.[11]

While home-purchasing power is an important factor in housing affordability, the probability of foreclosure is also an important measure. In our model, foreclosure depends on the shock, ϵ_i, that home buyers experience after purchasing a home. Thus, if an individual i has α_i and R_i so that $\alpha_i + \max\{R_i - c, 0\} - p > 0$ but also has $\alpha_i + \max\{R_i - c, 0\} - p + \epsilon_i < 0$, then individual i's home goes into foreclosure. More formally, the probability of a foreclosure is given by:

$$\begin{aligned} \Pr(\text{FC}) \quad = \quad & \frac{\Pr(c \geq R_i \ \& \ \alpha_i > p^*(c) \ \& \ \alpha_i + \epsilon_i < p^*(c))}{\Pr(c \geq R_i \ \& \ \alpha_i > p^*(c))} \\ & + \frac{\Pr(c < R_i \ \& \ \alpha_i + R_i - c > p^*(c) \ \& \ \alpha_i + R_i - c + \epsilon_i < p^*(c))}{\Pr(c < R_i \ \& \ \alpha_i + R_i - c > p^*(c))}, \end{aligned} \tag{2}$$

where the first term captures the probability of foreclosure for homeowners that do not share their home and the second term captures the probability of foreclosure by home sharing owners.

One potential starting point to determine the impact of a home sharing shock on the probability of foreclosure is to differentiate the probability of foreclosure with respect to c. However, this approach necessarily yields an ambiguous prediction (under arbitrary distributions for the random variables), because a decrease in c leads to higher returns to home sharing and higher home prices. The new buyers who are willing to rent on home sharing platforms and the individuals who are priced out of the market (due to greater home sharing)

[11] Of course this redistribution story requires a correlation between R_i and α_i.

are all on the margin of foreclosure, so the direct effect, $\frac{\partial \Pr(FC)}{\partial c}$, is ambiguous and depends on distributional assumptions.

Another approach is to consider a home sharing cost shock that occurs *after* home purchasing decisions are made. While our model is static, this approach effectively captures a dynamic snapshot to analyze how changes in the home sharing market impact foreclosures. Considering such a shock requires an augmented version of Equation (2) where changes to c capture deviations after individuals have already made home purchase decisions. Let the post-purchase shock to home sharing costs be denoted by Δc. In this case the probability of foreclosure is given by:

$$\begin{aligned}\Pr(\mathrm{FC}|\Delta c) \quad = \quad & \frac{\Pr(\alpha_i + \epsilon_i < p^*(c) \ \&\ c \geq R_i \ \&\ \alpha_i > p^*(c))}{\Pr(c \geq R_i \ \&\ \alpha_i > p^*(c))} \\ & + \frac{\Pr(c < R_i \ \&\ \alpha_i + R_i - c > p^*(c) \ \&\ \alpha_i + R_i - c - \Delta c + \epsilon_i < p^*(c))}{\Pr(c < R_i \ \&\ \alpha_i + R_i - c > p^*(c))}.\end{aligned} \tag{3}$$

Comparing Equations (2) and (3), the only difference between the two equations is the added $-\Delta c$ in the numerator of the second probability. Thus, if there is no post-purchase shock to home sharing costs, then $\Delta c = 0$ and Equations (2) and (3) are equivalent. Instead, a shock to sharing costs that occurs after home purchasing corresponds to a change in Δc (not a change in c). We find that such a shock generates the following result:

Proposition 2. *If home sharing costs increase after individuals make their home purchase decisions, then foreclosures increase:* $\frac{\partial \Pr(FC|\Delta c)}{\partial \Delta c} > 0$.

In this case, the homeowners that share are hurt by the increase in sharing costs. This increases the likelihood of foreclosure for homeowners that host. At the same time, homeowners that are unwilling to share are not impacted by an increase in sharing costs that occurs post-purchase, so their foreclosure risk remains constant with the shock. Altogether, this post-purchase cost shock (due to something like a restrictive policy) increases the expected number of foreclosures. This suggests that caution should be taken when considering policies that limit home sharing as they may result in an increase in foreclosure.

3 Data

To test the predictions of the model, we primarily use two sources of data. The first is consumer-facing information on Airbnb listings, including property characteristics and geographic coordinates, calendar availability, and implied bookings collected from Airbnb.com by AirDNA. The second is transaction-level data on home sales and foreclosures from Zillow.com's Transaction and Assessment Database (ZTRAX), including property characteristics and geographic coordinates, home sale prices, timing of sales, foreclosures, and timing of foreclosures. In the following subsections, we describe these two data sources in more detail.

3.1 Data on Airbnb Listings

We start with information on Airbnb listings that include daily data on asking prices, availability, inferred bookings, as well as time-invariant property characteristics such as number of bedrooms, number of bathrooms, maximum number of guests, and reported coordinates for all properties listed anytime between August 2014 through June 2019 in the San Francisco and Chicago metropolitan areas. The data were collected by AirDNA, a third-party source that frequently scrapes property, availability, host, and review information from the Airbnb website. These data have been used to study Airbnb tax evasion and enforcement, along with other topics in the housing, tourism, and economics literature (e.g., Bibler et al., 2021).[12]

We then restrict our sample to the ten largest cities, as measured by the total number of Airbnb listings, in each of these two metros. Restricting in this way is useful for two reasons. First, using multiple cities from the same metro areas allows us to control for metro area by month-year fixed effects, which allows us to account for location-specific seasonality and short-term demand shocks. Moreover, the two treated cities have relatively high levels of Airbnb activity, so using only the largest Airbnb markets within those metros gives us

[12] This is in contrast to papers that use administrative data from Airbnb, such as Jaffe et al. (2018) and Farronato and Fradkin (2018).

the best set of comparable controls. The list of included cities along with relevant summary statistics for those cities are reported in Appendix Table B1.

Next, we use properties' reported coordinates to assign them into their corresponding Census tract. We then calculate a measure of Airbnb density for each tract. Specifically, we calculate the average number of Airbnb listings present during the twelve months prior to the first policy enactment per 1,000 residents using tract-level population counts from the 2010 census. Because the 25th percentile tract has 0.5 listings per 1,000 population (or about 2 total listings), and one-third of of the tracts in the bottom quartile contain zero listings, we drop the bottom quartile of tracts from our estimation sample. Practically, this makes sense, as a negative shock to Airbnb has little to no capacity to reduce Airbnb activity in areas where it does not already exist. Note that, in the bulk of our analyses to follow, we will also use density to estimate heterogeneous treatment effects.

Proceeding with the restricted sample, focusing on properties in the most Airbnb-active parts of the San Francisco and Chicago metro areas, we aggregate our property-day data to the property-month level. Our primary interest is in measuring the size of the short-term rental market, and how it changes in response to the registration-requirement shocks. To that end, we look at two main outcomes of interest: availability and nights booked. Availability is a binary variable indicating whether a listed property had at least one day of calendar availability (either booked or unbooked) in a given month. Nights booked reflects the number of calendar days in a month that a property has been reserved.[13] To conduct

[13]It is important to note that bookings are not directly observed by AirDNA. Each property's calendar of availability is scraped every one to three days to detect any changes. A change in availability suggests a booking has occurred, which can be verified when a renter writes a review of the host and property after his or her stay. The primary concern with this approach is that AirDNA may incorrectly infer that a booking occurs, and thus over-measure the number of nights booked, when a host no longer wants to rent out his or her property for a particular night and blocks that night. Because we find the policy shock substantially reduces availability, such measurement error would tend to bias us *against* finding a negative effect on nights booked, thereby suggesting that our estimated 40% reduction in bookings may be an *understimate* of the true negative shock to bookings. A related concern is the possibility that stated availability does not accurately reflect actual availability as discussed in Farronato and Fradkin (2018). In particular, the authors point out that hosts may be better at updating their stated availability during periods of high demand. If true, this implies that we might over-measure nights booked during such periods. However, in our preferred specification discussed in Section 4, we are able to alleviate this concern by including metro-month-year fixed effects to absorb the effects of idiosyncratic demand shocks.

our analyses, we construct a balanced panel of property-month observations including all listings that were booked at least once during our sample period. We balance the panel by including an observation for every month for every property, regardless of whether they were only listed for part of our full sample period. In months where a property is not listed, its outcome measures (availability and bookings) are set to zero by definition. Balancing in this way is important for measuring the overall size of the Airbnb market, as it allows us to capture both the intensive and extensive margins of Airbnb activity.

The first row of Table 1 shows the average availability during the full sample period for all properties in the sample as well as only those within the San Francisco and Chicago city limits, both 0.25 or 25%, implying roughly a quarter of all the listings in our balanced panel of listings were available to be booked for at least one day in any given month. Row two presents the number of nights booked per property-month. Here, we see the average number of nights booked is 2.39 in the full sample, and 2.38 among those in the San Francisco and Chicago city limits. Both of these reveal very comparable activity among treated and untreated listings. To provide further evidence on the comparability of the treated and untreated listings, we present event studies in Section 5 and find essentially no evidence of differential trends leading up to the policy shocks.

In Appendix Table B2, we further summarize our Airbnb data by quartiles of tract-level Airbnb density, including means of Airbnb outcomes and characteristics for listings in the twelve months leading up to the first policy enactment. Notably, we summarize our density measure in the row labeled "Available per 1000 Pop.", where we observe 0.21 listings per 1,000 in the bottom quartile and 16.71 per 1,000 in the top quartile. While not shown in this table, approximately 31% of first quartile tracts, 56% of second quartile tracts, 52% of third quartile tracts, and 35% of fourth quartile tracts are untreated. Thus, we are able to exploit the substantial variation in density and treatment status to estimate differential Airbnb and housing effects across tracts that are more/less affected by the registration policies.

3.2 Data on Home Sales and Foreclosures

For our housing price and foreclosure analyses, we use the Zillow Transaction and Assessment Dataset (ZTRAX) which contains information on home transactions and foreclosures. Zillow sources the data from an unnamed major third-party data provider who collects the information county by county, and supplements this with internal data collection efforts to fill any gaps.[14] These data seem to cover nearly all transactions that occur. For example, Zivin et al. (2020) find that the ZTRAX covered 95% of all transactions in Florida between 2000 and 2016. For each transaction, the set of information provided includes timing of sale, transaction price, as well as assessment information such as number of bedrooms, number of bathrooms, square footage, year built, and coordinates of the home.

For the housing price analyses, we start with the set of homes with observed transaction prices during the same sample period and in the same geographic areas as in our restricted Airbnb sample (August 2014 through June 2019, the largest Airbnb jurisdictions in the San Francisco and Chicago metro areas, excluding the least Airbnb-dense quartile of tracts). The list of included cities along with relevant summary statistics for those cities are reported in Appendix Table B3. In an effort to focus primarily on normal housing transactions, we limit the sample of sales to arm's-length transactions with sale prices between $10,000 and $2 million.[15]

For the foreclosure analyses, we start with the set of observed foreclosures in the same areas and time periods. Because there are multiple observations corresponding to a single foreclosure, generated by petitions, notices, and deed transfers, we identify each foreclosure event by the date that the first piece of documentation is generated within a three-month period. That is, we treat foreclosure-related observations as unique foreclosure events only if there is no prior foreclosure-related observation for the same parcel ID within the previous

[14] See `https://www.zillow.com/research/ztrax/ztrax-faqs/`.

[15] Among sales with non-missing prices, 7% of the San Francisco metro area transactions and 1.8% of the Chicago metro area transactions are dropped due to the price restrictions. Almost all of the 7% dropped from the San Francisco metro are properties that sold for over $2 million, while roughly one-third of the 1.8% dropped from the Chicago metro area were transactions over $2 million.

three months. As with our Airbnb data, we are able to assign transacted and foreclosed properties to their corresponding Census tracts using the reported coordinates, which we use as the primary geographic unit in our empirical analyses that follow. While we use transaction-level data for our price analyses, for our foreclosure analyses we aggregate to obtain tract-month-year foreclosure counts.

In Table 1 we report the average sale price and price per square foot for observed transactions. About 39% of transactions in our sample occur in the San Francisco and Chicago city limits. Across the full set of transactions, the average transacted price is roughly $800,000. The mean of transactions occurring in the treated areas (i.e. city limits) is $766,000. The averages for price per square foot are $565 and $568, respectively. We also report the average number of foreclosures by tract-month. Here, we see that there are 0.13 foreclosures per tract-month across both treated and untreated tracts, and 0.09 foreclosures per tract-month among treated tracts. While prices and foreclosures appear to be slightly lower in the treated areas, using logged prices and tract-level fixed effects in our regressions helps to mitigate concerns about any gaps between treated and untreated areas. Moreover, we find little to no evidence of differential trends leading up to the policy shocks in the event studies presented in Section 5. In Appendix Table B4, we further summarize our Zillow data by Airbnb density quartile, including means of home prices, foreclosures, and property characteristics.

4 Estimation

First, we outline our strategy for estimating the effect of the San Francisco and Chicago registration policies on the size of the Airbnb market as measured by availability and bookings. This is the mechanism through which we expect the policy to impact the broader housing market. To estimate the effect of the policies on the size of the Airbnb market at the city level, we use a standard differences-in-differences estimator. We then test whether the effects are stronger in areas with a greater density of Airbnb listings at the tract level. After that, we turn to the impacts of the registration policies on home prices and foreclosures, where we focus primarily on estimating the differential effects across areas of varying Airbnb density.

4.1 Airbnb Market

Because our big-picture question is whether changes in the size of the Airbnb market affect housing affordability, we first test whether our policy shocks substantially affected the size of the relevant Airbnb markets. This exercise is crucial for establishing that any changes in prices and foreclosures resulting from the policy shocks can be attributed to changes in the size of the Airbnb market.

The following is our core differences-in-differences (DiD) specification:

$$Y_{ijmt} = \gamma R_j + \alpha X_{ijm} + \eta_j + \delta_{mt} + \mu_{ijmt} \tag{4}$$

where Y_{ijmt} is the outcome of interest for property i in tract j, metro m, and month-year t. We use property as our cross-sectional unit, which in certain specifications allows us to control for time-invariant property-specific characteristics X_{ijm}.[16] R_j is an indicator equal to one for tract-month-year observations where the registration policies have been enacted, and zero otherwise. Thus, the DiD parameter of interest is γ, which measures the change in the average difference in Y between treated and control units before and after treatment. Finally, η_j are tract-level fixed effects to control for time-invariant differences across tracts, δ_{mt} are metro-month-year fixed effects to control for metro-level idiosyncratic shocks (e.g., demand shocks or seasonal effects), and μ_{ijmt} reflects the idiosyncratic error term. Note that in this specification, as well as all others, we estimate standard errors that are robust to clustering at the tract level.

[16]Note that, because we construct a balanced panel of properties, the set of properties included does not change over time and thus time-invariant property characteristics are collinear with tract fixed effects. Thus, in some specifications we include only tract fixed effects, and in others we include only time-invariant property-specific characteristics. Our set of property-specific characteristics include number of bedrooms, number of bathrooms, maximum number of guests allowed, type of listing (shared room, private room, entire home or apartment), listing rating, number of photos, and number of reviews.

The two primary outcomes of interest are monthly availability and number of nights booked per month. These two outcomes capture the two margins we are interested in: the amount of housing available for short-term rentals on the platform, and the utilization of this available short-term housing. Examining both is important, as it is possible that the registration policies only cause relatively inactive listings to exit, which would have little tangible impact on the true amount of housing diverted to this short-term rental market.

Next, we go beyond our core DiD approach to examine heterogeneity in treatment effects by tract-level Airbnb market density (i.e. number of pre-treatment Airbnb listings per 1,000 tract residents). More details on the construction and summary statistics of this density measure can be found in Section 3. We test for differential effects using the following dose-response specification:

$$Y_{ijmt} = \gamma R_{jt} + \rho \cdot S_j \cdot R_{jt} + \alpha X_{ijmt} + \eta_j + \delta_{mt} + \mu_{ijmt} \tag{5}$$

where S_j is the Airbnb density measure.[17] We then test whether we can reject $\rho = 0$, which would suggest that the policy effects are larger or smaller depending on a tract's pre-existing Airbnb market size. Because the heterogeneity may not be linear and monotonic, in an alternate set of analyses, we replace the dose-response terms R and $S \cdot R$ with interactions between R and three indicators reflecting whether the tract is in the second, third, or fourth quartile of the density distribution.[18] These heterogeneity estimates allow us to not only exploit variation in treatment across timing and place, but also variation among tracts that are all nominally treated but might be more/less impacted by the city-level policy adoption.

Note that in the results section to follow (Section 5), we also estimate event studies to provide visual evidence of differences in outcomes between treated and control tracts over time. This exercise helps to compare trends in the pre-treatment periods, as well as estimate

[17]Recall, this is calculated as the average monthly number of Airbnb units per 1,000 tract residents in the year prior to the first policy enactment.

[18]Note that we assign density quartiles based on tract-level aggregates, such that 25% of the sample tracts fall into each quartile but the number of properties in each quartile differs accordingly.

time-disaggregated treatment effects. To do this, we estimate the time-specific differences in outcomes using the following specification to obtain estimates for each quarter of data both pre- and post-implementation.

$$Y_{ijmt} = \sum_{k=-7}^{8} \gamma_k D_j \cdot 1(q - Q_j = k) + \eta_j + \delta_{mt} + \mu_{ijmt} \tag{6}$$

Here, D_j is an indicator for whether tract j is ever-treated, which is interacted with indicators for 7 quarters (indexed by q) leading up to the quarter during which treatment occurs (Q_j) as well as the 9 post-treatment quarters (0, 1, ..., 8). Observations more than 7 quarters before treatment are included in period -7, and those more than 9 quarters after treatment are included in period 8. We omit quarter -2 to allow quarter -1 to capture any anticipatory effects of the policy enactment. The set of $\hat{\gamma}_k$ are then plotted to provide visual support of parallel pre-trends as well as time-disaggregated estimated treatment effects.[19]

4.2 Home Prices and Foreclosures

After establishing the relationship between registration policies and the size of the affected Airbnb markets, we turn to estimating the effect of the policies on home prices and foreclosures using transaction-level data from Zillow's ZTRAX dataset. For our home price analysis, our core specification is roughly the same as Equation (4). The key differences are that each observation now corresponds to a sale of a home, and the set of controls in X includes number of bedrooms, number of bathrooms, square footage, and the year the home was built. We continue using the same set of fixed effects (tract, metro-month-year). Again, in all specifications, we estimate standard errors that are robust to clustering at the tract level.

Turning to foreclosures, we are primarily interested in estimating the effects of policy implementation on counts of foreclosures at the tract level. Thus, we estimate the following

[19] In the event study figures presented in the next section, we include only the coefficients for quarters -6 through 7, which is the balanced set of estimates. That is, we omit the binned endpoints -7 and 8.

modified core DiD specification:

$$Y_{jmt} = \gamma R_j + \alpha X_{jmt} + \eta_j + \delta_{mt} + \mu_{jmt} \tag{7}$$

where Y is the number of foreclosures in tract j, metro m, and month-year t. Again, we include tract fixed effects η_j and metro-month-year fixed effects δ_{mt}. Here, our set of covariates include tract-level characteristics obtained from the American Community Survey (moving 5-year averages), which we report by Airbnb density quartile and treatment status in Appendix Table B5.[20]

As in the Airbnb market analysis, we estimate heterogeneous effects on prices and foreclosures across tracts of varying levels of pre-treatment Airbnb density following Equation (5). We also examine the alternative specification that estimates quartile-specific rather than dose-response effects. Finally, we also estimate event studies for prices and foreclosures following the same approach described in Equation (6).

5 Results

5.1 Effects on Airbnb Market

First, to verify that the registration requirement policy shocks in San Francisco and Chicago had a meaningful impact on the Airbnb market, we estimate the differences-in-differences (DD) parameters outlined in Section 4. The first of these results are presented in Table 2, where we present the core DD and dose-response estimates of the impact of the policy shock on availability and nights booked using our preferred specification controlling only for tract fixed effects and metro-month-year fixed effects. The first column presents the estimate of the overall average effect, -0.097, which we find to be highly statistically significant ($p<0.01$).

[20] The full set of covariates includes median age, percentage over age 65, percentage of black residents, percentage of hispanic residents, percent under age 18, unemployment, average travel time to work, percent below poverty, median household income, percent of homes built after 2010, percent of homes built before 1939, and percent of buildings containing more than 10 housing units.

This means that properties are roughly 10 percentage points less likely to be available in the time periods following the registration shocks, which amounts to a large 40% reduction in supply relative to the baseline average availability proportion of 0.25 (see Table 1). In the second column, we estimate a dose-response specification to confirm whether the treatment effects are stronger in the tracts where Airbnb listings were more prevalent pre-treatment. Here, we find the base DD parameter to be -0.074, and estimate a dose-response term of -0.001, both of which are statistically significant at the 99% level. The dose response term implies that, for each additional available Airbnb listing per 1,000 tract population, the overall negative effect on Airbnb supply grows by 0.001. For example, the most-dense quartile of tracts has a pre-treatment average of 16.71 available listings per 1,000 (see Table B2), suggesting that the total effect of the shock on availability in the most-dense quartile is roughly a 9 percentage point decrease (or 34% relative to the baseline availability rate of 0.27 in quartile 4).

It is important to note that measuring market size using availability includes both utilized and slack accommodations. Reductions in availability suggest that fewer units are offered by hosts, but this could come from slack in the supply (i.e. listings with very sparse bookings). Thus, we also estimate the effects of the policy shocks on nights booked and present the results in columns 3 and 4 of Table 2. Here we find an overall average effect of -0.89, suggesting roughly 0.9 fewer nights booked following the registration shocks, which is 37% relative to the baseline average of 2.39 (see Table 1). Turning to the dose-response estimate, we find the same pattern as with availability. The base DD parameter is -0.574 and the dose response term is -0.018. Again, considering that the most-dense quartile of tracts has a pre-treatment average of 16.71 available listings per 1,000, these estimates suggest the total effect of the shock in the most dense quartile is a 0.87 decrease in nights booked (35% relative to baseline average nights booked of 2.49 in quartile 4). These estimates confirm that the policy dramatically and meaningfully reduced the size of the Airbnb market, and did not simply cause the exit of marginally active infrequently booked listings.

Next, in Figure 2, we graphically present quartile-specific estimates using the same preferred specification. In the second Airbnb density quartile, we find a slight, marginally statistically significant positive effect on availability of 0.027 percentage points. Turning to the more dense quartiles, we find a 5.6 percentage point (22%) decrease in quartile 3 and a 10.7 percentage point (40%) decrease in quartile 4, both statistically significant at conventional levels. We see the same sort of pattern for nights booked; a statistically insignificant increase of 0.293 nights booked in quartile 2, a statistically significant reduction of 0.345 in quartile 3, and a statistically significant reduction of 1.007 in quartile 4. As with the dose-response estimates, these quartile-specific estimates highlight that the treatment effects increase substantially with local Airbnb prevalence, which will prove useful for identifying the corresponding housing market effects.

Next, we examine the robustness of our central estimates to alternate sets of controls. In Table 3, we present the dose-response estimates (Panel A) from three specifications along with the corresponding quartile-specific estimates (Panel B). The first and fourth columns of estimates are from our preferred specification as discussed above. Column 2 presents an alternative specification for availability where we use month-year fixed effects instead of metro-month-year fixed effects. While the direction of the estimated effects are similar, the magnitudes are quite different. Here, our base DD estimate is -0.023 and the dose response parameter is -0.002 (see Panel A). Turning to Panel B, we find a statistically significant increase in availability in quartile 2 (0.082), an essentially null effect in quartile 3 (-0.009), and a significantly negative effect in quartile 4 (-0.069). These estimates highlight the importance of including metro-month-year fixed effects in our analyses, as we are otherwise unable to account for metro-specific idiosyncratic shocks (e.g., major sporting events) that can confound and bias our estimates.

Column 3 is another deviation from our preferred specification where we replace tract fixed effects with property-level characteristics. Note that, because we use a balanced panel of properties and our property controls are time-invariant, the property characteristics are collinear with tract fixed effects. Here, we find very similar estimates to column 1. In Panel A, the base DD estimate of the effect of the policy on availability is -0.064 and the dose response parameter is -0.001, both statistically significant at conventional levels. Turning to the quartile-specific estimates, we find a small marginally significant negative reduction in availability in quartile 2 (-0.016). As in column 1, we find a larger negative effect in quartile 3 (-0.062) and an even larger effect in quartile 4 (-0.077).

Next we repeat these three sets of specifications using nights booked as the outcome variable, and present the results in columns 4 through 6 of Table 3. We find a very similar pattern to those presented in columns 1-3. Columns 4 and 6 present very similar patterns of estimates consistent with the notions that the registration shocks had meaningful effects on Airbnb activity. Column 5, like column 2, is again somewhat of an outlier, emphasizing the importance of controlling for metro-specific idiosyncratic temporal shocks.

Finally, in Figure 3 we present the results of our event studies for both availability (Panel A) and nights booked (Panel B). Both of these use our preferred specification, including only tract and metro-month-year fixed effects as controls. Each circle in this figure represents the quarter-specific difference between treated and untreated units relative to the period two quarters before treatment, and the dashed lines represent 95% confidence intervals calculated using standard errors clustered at the tract level. In both subfigures, we see little to no evidence of systematically different pre-trends between property listings in treated versus untreated tracts leading up to the policy enactments in quarter 0, which provides support for the standard parallel trends assumption required for DD to yield an unbiased causal parameter. Then, once Airbnb begins cooperating in enforcing the registration policies (in months 4-5 following enactment), we see a clear decrease in availability and bookings among treated listings relative to their untreated counterparts, with similar magnitudes to our estimates presented in Tables 2 and 3. Moreover, the apparent delay in the impacts of policy enactment highlights the importance of cities collaborating with Airbnb to enforce their policies. Consistent with Bibler et al. (2021), this provides further evidence that policies (e.g., taxes, registration requirements) will not be effective if enacted without the platform's assistance or perhaps other substantial enforcement efforts. Note that we present quartile-specific event studies in Appendix Figures B1-B3, where we again find post-treatment effects that become larger over time, are increasing in tract-level Airbnb density, and do not exhibit evidence of differential pre-trends.

5.2 Effects on Housing Market

After establishing in Section 5.1 that the registration requirements in San Francisco and Chicago were large, meaningful policy shocks, we turn to estimating the resulting impacts on the housing market by measuring home price and foreclosure effects. In Table 4 we present the price and foreclosure estimates from our base DD and dose response specifications using our preferred set of controls (tract fixed effects and metro-month-year fixed effects). In column 1, we find that the policy shock generated an average decline in logged housing prices of 0.053, or roughly 5.3%. However, this average estimate masks considerable heterogeneity. Turning to the dose response specification in column 2, we find a statistically insignificant base DD estimate of 0.017 and a dose response term of -0.006. Given the most Airbnb-dense quartile of tracts has a pre-treatment average of 16.71 available listings per 1,000 tract population (see Table B2), the implied total effect of the shock on availability in quartile 4 is an 8.3 percent reduction in home sale prices. In Panel A of Figure 4 we plot the quartile-specific price effects, where we find a slight increase in prices (2.1%) that is statistically indistinguishable from zero in quartile 2, no change in prices (0%) in quartile 3, and a large 10.1% reduction in prices in the most Airbnb-dense quartile of tracts.

In column 3 of Table 4, we estimate the effect of the policy shock on the number of foreclosures at the tract-month level. Overall, we find the number of foreclosures per tract-month statistically significantly increase by 0.059, which represents a large 45% increase relative to the baseline average of 0.13 foreclosures in a given tract-month. In column 4, the dose response specification yields a statistically significant base DD estimate of 0.046 and a statistically significant dose response term of 0.001. This implies a total effect of 0.063 additional foreclosures in the most Airbnb-dense quartile of tracts (a 105% increase relative to the quartile 4 baseline average of 0.06 per tract-month). In Panel B of Figure 4 we plot the quartile-specific foreclosure effects, where we find no effect on foreclosures in quartile 2, a 0.061 increase in the number of foreclosures per tract-month in quartile 3 (47% relative to the baseline quartile 3 average of 0.13), and a 0.071 increase in foreclosures in quartile 4 (118% relative to the quartile 4 baseline average of 0.06).

To fully understand the magnitude of these foreclosure effects, it may also be useful to consider the change in the number of foreclosures as a fraction of the number of Airbnb listings that exit following the policy shock. For example, the baseline average number of available listings per tract in the year preceding treatment is 21.05 in quartile 3 and 73.46 in quartile 4 (see Table B2). Given a roughly 22% reduction in Airbnb listings in quartile 3 and a 40% reduction in quartile 4, as discussed in Section 5.1, this implies 4.7 fewer available listings in quartile 3 and 29.4 fewer listings in quartile 4. Thus, 0.061 additional foreclosures in quartile 3 amounts to 1.3 foreclosures per month for every 100 available listings that exit. In quartile 4, we calculate 0.24 additional foreclosures per month for every 100 available listings that exit.

Next, we examine the robustness of our central housing market estimates to alternate sets of controls. In Table 5, we present the dose-response estimates (Panel A) from three specifications along with the corresponding quartile-specific estimates (Panel B). The first and fourth columns of estimates are from our preferred specification as discussed above. Column 2 presents the price estimates when we only use month-year fixed effects instead of metro-month-year fixed effects. Here, we find a very similar pattern of estimates. Our base DD estimate is -0.8% and statistically insignificant, and the dose-response parameter is -0.5% for each additional available Airbnb listing per 1,000 population. Turning to the quartile-specific estimates in Panel B, we find no meaningful price effects in quartiles 2 or 3, and a 9.9% decrease in prices in quartile 4. In column 3, we modify our preferred specification by also including a set of property-specific covariates (see Section 4 for more detail). Again, we find a similar pattern of estimates. In particular, we find a marginally statistically significant 2.4% price increase in quartile 2, a statistically insignificant 1.4% increase in quartile 3, and a statistically significant 8% decrease in quartile 4. To summarize, following the policy shock, overall we find little evidence of any price effects in quartiles 2 and 3, but a consistently substantial 8-10% reduction in prices in quartile 4.

Turning to columns 4-6 of Table 5, we present the dose-response and quartile-specific estimates of the foreclosure effects. Column 4 restates the results of our preferred specification. Column 5, which includes tract fixed effects and month-year fixed, yields a base DD estimate of 3.8% and a dose-response estimate of 0.2%. Turning to the quartile-specific estimates, we find a remarkably similar set of coefficients to column 4. The quartile 2 effect is -0.004 and statistically insignificant, the quartile 3 effect is 0.06 and statistically significant, and the quartile 4 effect is 0.073 and statistically significant. Again, things change very little in column 6 when we add ACS covariates to our preferred specification. The only slightly meaningful difference is in the quartile 4 estimate, which is 0.064 compared to 0.071 in our preferred specification. Thus, overall we find no evidence of any foreclosure impact in quartile 2, but a consistently substantial increase in foreclosures of 0.06 to 0.07 in quartiles 3 and 4.

Finally, in Figures 5 and 6 we present the results of our event studies for both prices and foreclosures in the quartiles where we observe significant effects (quartile 4 for prices, quartiles 3 and 4 for foreclosures). Both of these use our preferred specification, including only tract and metro-month-year fixed effects as controls. Each circle in this figure represents the quarter-specific difference between treated and untreated units relative to the period two quarters before treatment, and the dashed lines represent 95% confidence intervals calculated using standard errors clustered at the tract level. As with the Airbnb market event studies, we see little to no evidence of systematically different pre-trends leading up to the policy enactments in quarter 0, which provides support for the standard parallel trends assumption required for DD to yield an unbiased causal parameter. After Airbnb begins cooperating in enforcing the registration policies (in months 4-5 following enactment), we see a decrease in logged home sale prices and an increase in foreclosures among treated listings relative to their untreated counterparts, with similar magnitudes to our estimates presented in Tables 4 and 5. Note that we present the corresponding event studies for quartile 3 and 4 price effects and quartile 2 foreclosure effects in Appendix Figures B4 and B5, where we again find no evidence of differential pre-trends.

As discussed earlier in the paper, our estimated price effects are consistent with the existing literature demonstrating a positive relationship between Airbnb prevalence and housing and rental prices. Beyond the price effects, as we hypothesized in our model, the growth of (restriction of) Airbnb appears to reduce (increase) the incidence of foreclosure. That is, despite the pressure on prices, the introduction and growth of Airbnb may improve the stability and/or affordability of home-ownership. This is perhaps especially true for those on the margin of being able afford a home and who have the capacity to earn a non-zero profit from home sharing.

6 Conclusion

As metro areas throughout the United States continue to grow and become more densely-populated, the issue of housing affordability continues to be a growing concern. Airbnb and other home sharing platforms have recently come to the forefront of the housing affordability debate as local residents worry about these platforms' impact on displacement and gentrification. To better understand how the short-term rental market affects housing affordability, we first show that the net theoretical effect is ambiguous and is thus an empirical question. That is, short-term rentals put upward pressure on housing prices but also offer an additional revenue source from a home purchase.

We estimate the impact of the short-term rental market on housing affordability by leveraging city-level Airbnb listing registration policies that dramatically decrease the size of the local Airbnb market by 40%. Following these large negative shocks, we find that home values decrease by as much as 10% in the most Airbnb-dense census tracts. However, consistent with our predictions we find that foreclosures increased by 0.07 (117%) per tract-month in the most Airbnb-dense census tracts following the negative policy shocks. That is, despite the upward pressure on housing prices, the growth of Airbnb and their competitors has improved the likelihood that homeowners in the most Airbnb-dense areas remain in good standing on their mortgage loans.

Relating our estimates back to our model, our findings indeed suggest that the growth of short-term housing rental platforms has increased housing prices. However, this is not the full story when it comes to housing affordability. First, our heterogeneity estimates imply increased housing prices attributable to Airbnb are concentrated in the most Airbnb-dense neighborhoods, while the impact of Airbnb on housing prices is negative in the least Airbnb-dense neighborhoods. Moreover, our analysis of foreclosures suggests that the past decade of growth of such platforms may have reduced foreclosures in the areas where Airbnb is most prevalent.

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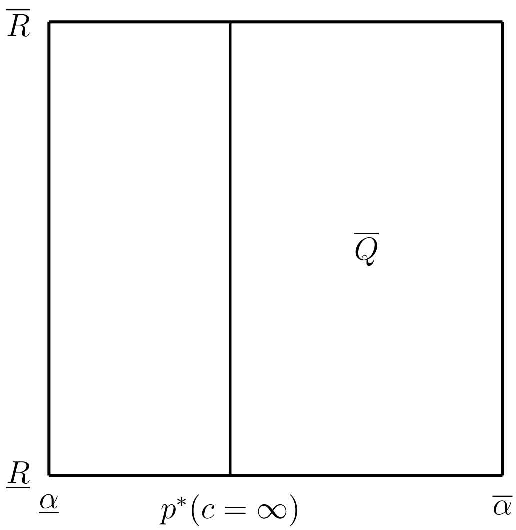
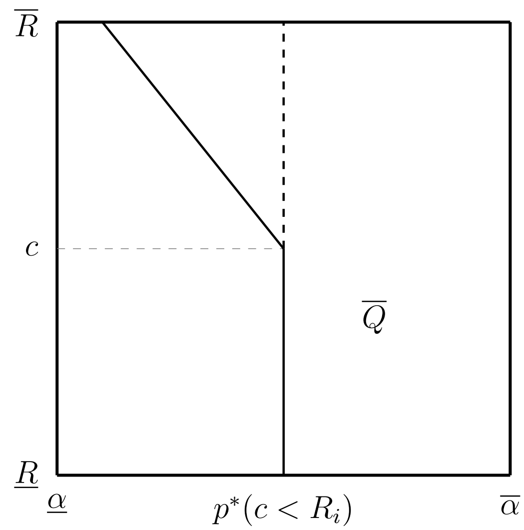
Figures and Tables

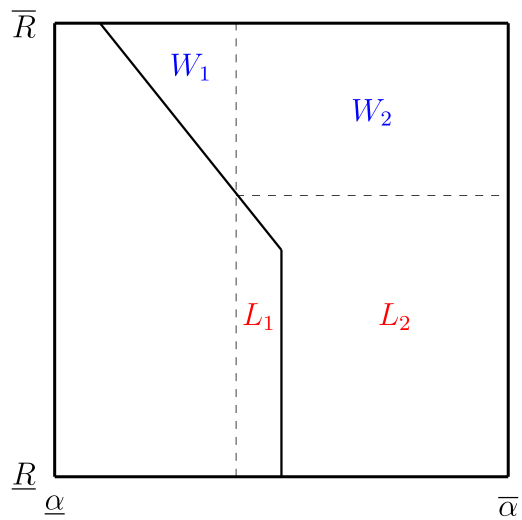
Figure 1: Graphical Representations of Home Purchasing Decisions



(a) Purchasing in Absence of Home Sharing

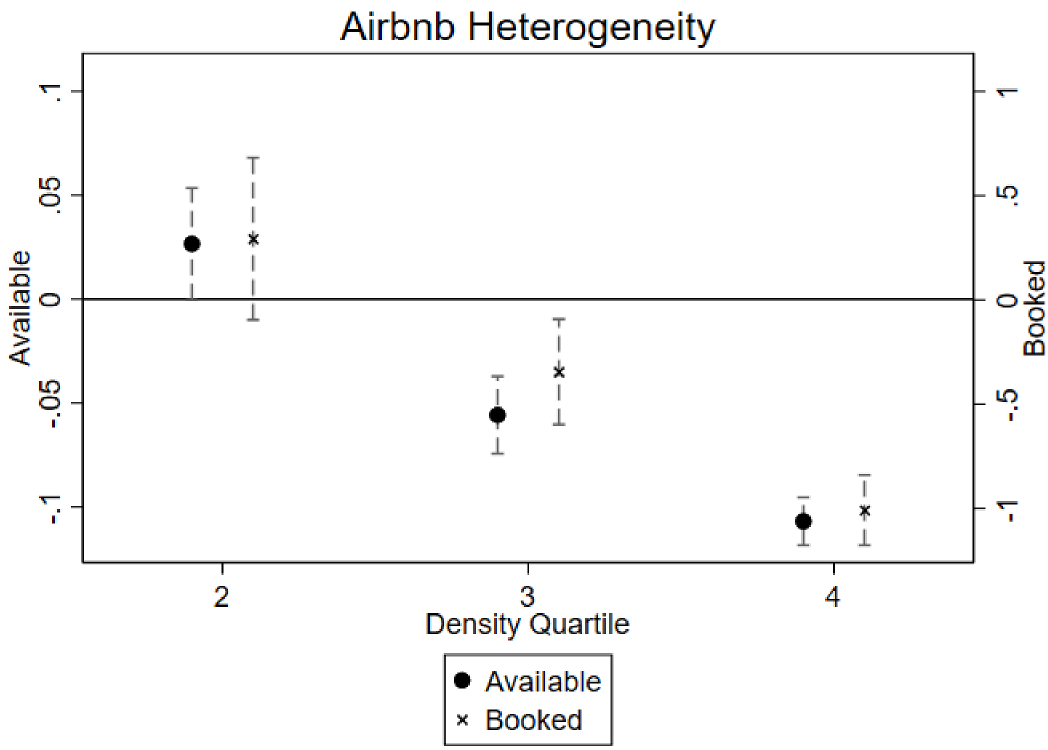


(b) Purchasing in Presence of Home Sharing



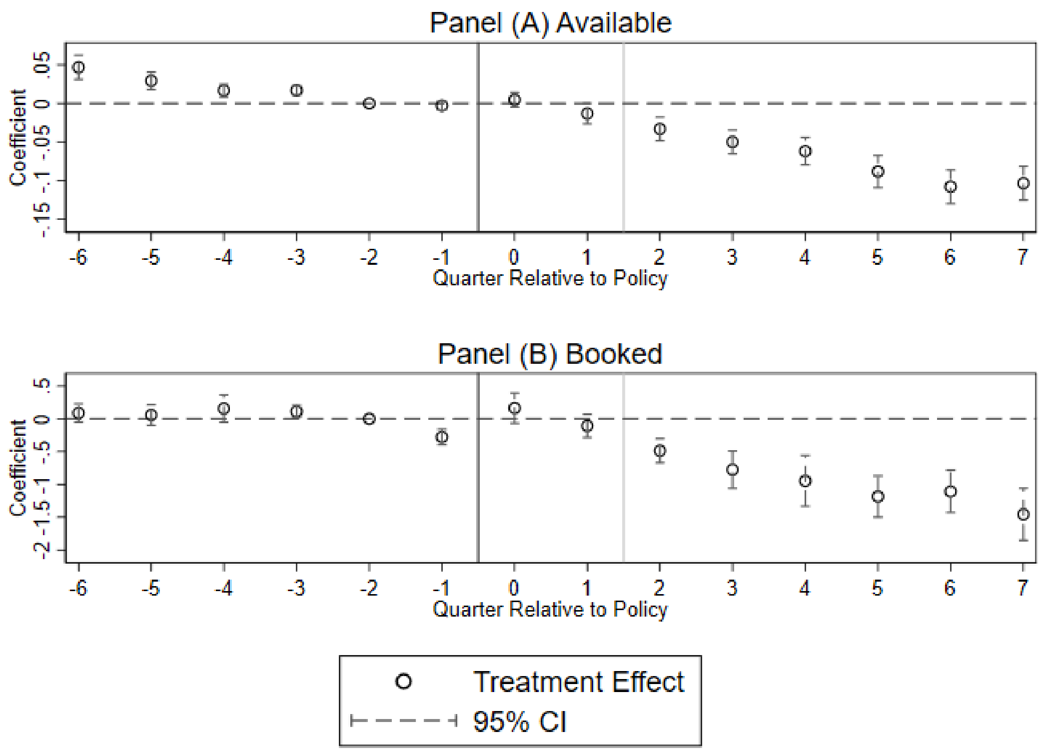
(c) Winners and Losers in Purchasing due to Home Sharing

Figure 2: Effects of Registration Shocks on Size of Airbnb Market by Airbnb Density



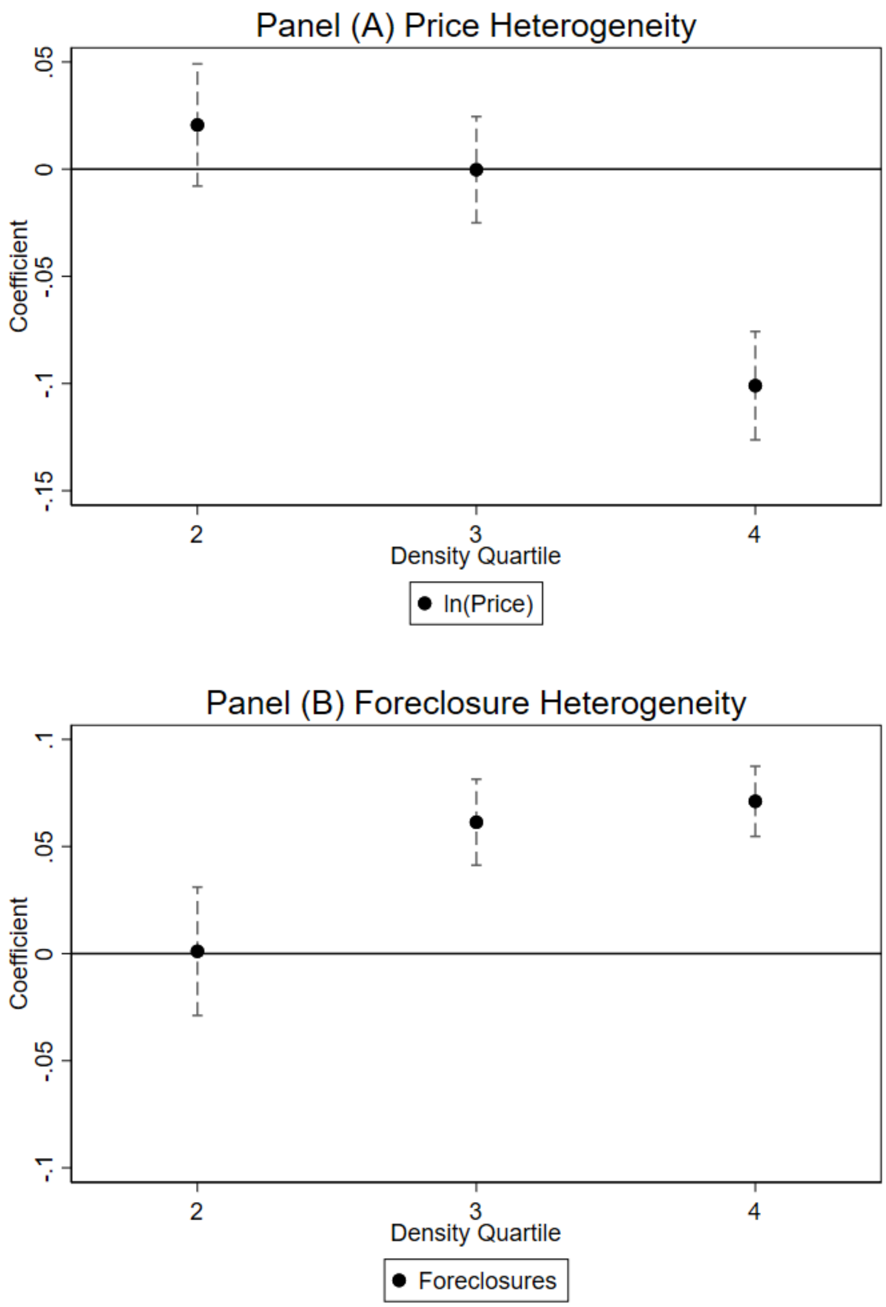
Notes: DiD estimates of the effect of the policy on probability of being available and the number of nights booked per month for each of the top 3 quartiles of the Airbnb density distribution. Both specifications include tract fixed effects and metro-month-year fixed effects. Density is measured at the tract level as the average monthly number of Airbnb listings available in the 12 months prior to the first treatment date per 1000 population in the 2010 census. Estimates are obtained by interacting the treatment-by-post indicator with indicators for each quartile of Airbnb density. Dashed vertical lines represent 95% confidence intervals. Standard errors are clustered at the tract level.

Figure 3: Event Study of Effects of Registration Shocks on Size of Airbnb Market



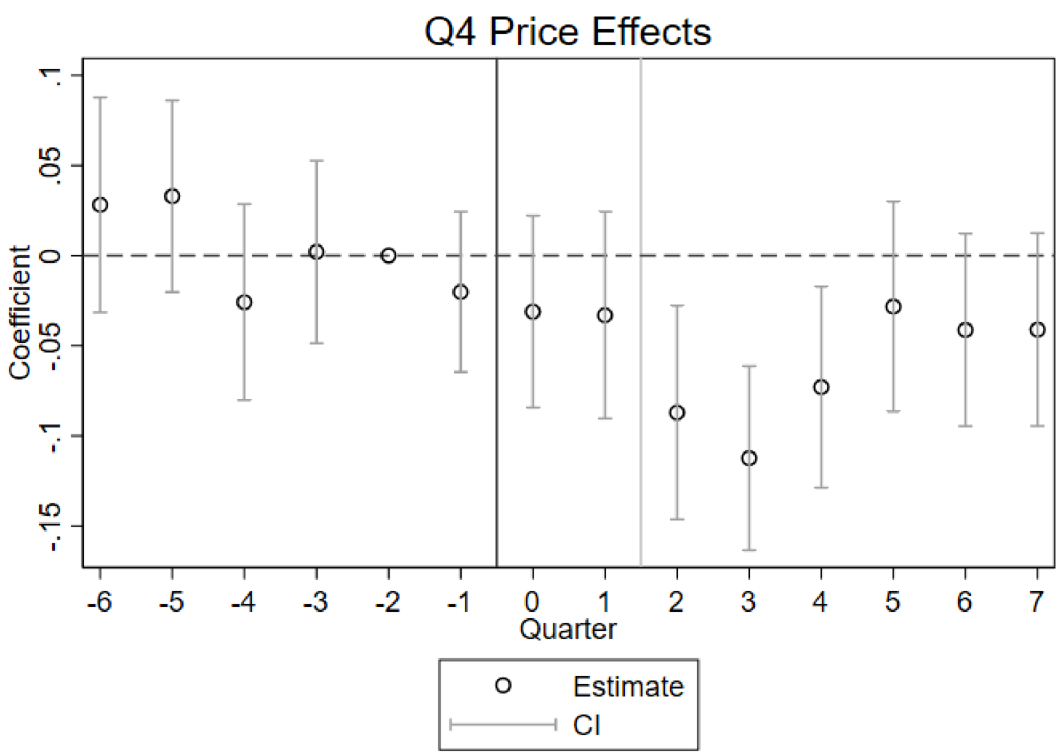
Notes: Quarterly differences in availability and nights booked around the treatment date between treated and untreated tracts. The estimation sample includes all Airbnb listings that ever appear in our sample period. The specifications include tract fixed effects and metro-month-year fixed effects. The solid vertical lines refer to the date that the policy went into effect (Sep. '17 for SF and Dec. '16 for Chicago). The lighter vertical line refers to the periods where Airbnb started enforcing the policy 4-5 months later. Hollow circles mark the quarter-specific treatment effects, i.e. the time-disaggregated DiD estimates. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level. The estimates on the binned endpoints, periods -7 and 8, are not included in this graph. We report quartile-specific event studies in Appendix B.

Figure 4: Effects of Registration Shocks on Prices and Foreclosures by Airbnb Density



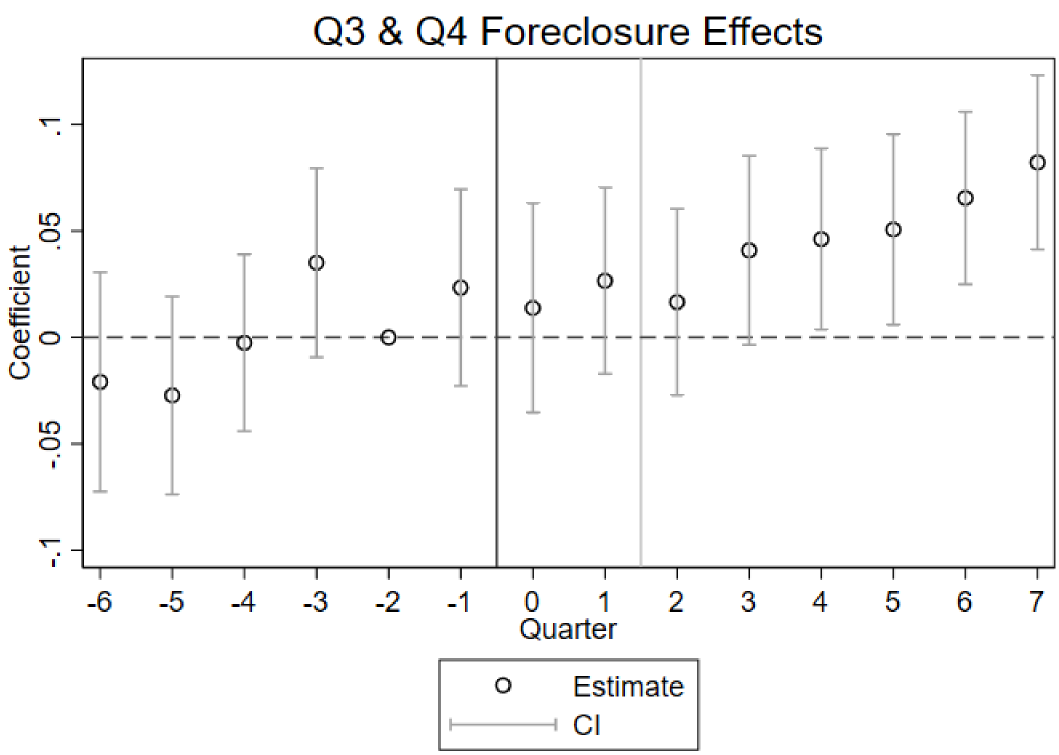
Notes: DiD estimates of the effect of the registration shocks on logged prices and foreclosures for each quartile of the Airbnb density distribution. Both specifications include tract fixed effects and metro-month-year fixed effects. Density is measured at the tract level as the average monthly number of Airbnb listings available in the 12 months prior to the first treatment date per 1000 population in the 2010 census. Estimates are obtained by interacting the treatment-by-post indicator with indicators for each quartile of Airbnb density. Dashed vertical lines represent 95% confidence intervals. Standard errors are clustered at the tract level.

Figure 5: Event Study of Effects of Registration Shocks on Prices, Quartile 4



Notes: Differences in logged home sale prices around the treatment date between treated and untreated tracts in the 4th Airbnb density quartile. The specification includes tract fixed effects and metro-month-year fixed effects. The solid vertical line refers to the date that the policy went into effect (Sep. '17 for SF and Dec. '16 for Chicago). Note, however, that Airbnb started enforcing the policies 4-5 months later. Hollow circles mark the quarter-year-specific treatment effects, i.e. the time-disaggregated DiD estimates, relative to the omitted period (quarter -2). The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level. The estimates on the binned endpoints, periods -7 and 8, are not included in this graph.

Figure 6: Event Study of Effects of Registration Shocks on Foreclosures, Quartiles 3 & 4



Notes: Differences in number of foreclosures around the treatment date between treated and untreated tracts in the 3rd and 4th Airbnb density quartiles. The specification includes tract fixed effects and metro-month-year fixed effects. The solid vertical line refers to the first quarter that the policy went into effect (Sep. '17 for SF and Dec. '16 for Chicago). Note, however, that Airbnb started enforcing the policies 4-5 months later. Hollow circles mark the quarter-year-specific treatment effects, i.e. the time-disaggregated DiD estimates, relative to the omitted period (quarter -2). The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level. The estimates on the binned endpoints, periods -7 and 8, are not included in this graph.

Table 1: Summary of Airbnb and Zillow Outcome Variables of Interest

	(1) Full Metros	(2) SF and CHI City Limits
Airbnb Outcomes		
Available	0.25	0.25
	(0.43)	(0.43)
Nights Booked	2.39	2.38
	(6.71)	(6.69)
Observations	5,250,612	3,452,889
Home Prices		
Sale Price	800,212	765,654
	(482,409)	(542,582)
Price per Sq. Ft.	565.09	568.07
	(344.97)	(431.59)
Observations	147,997	58,410
Foreclosures (Tract-month)		
Foreclosures	0.13	0.09
	(0.40)	(0.35)
Observations	75,159	41,328

Notes: *Airbnb Outcomes* include the mean and standard deviations at the property-month level. Column 1 includes the entire estimation sample including control areas, and column 2 includes only the properties in the city limits of San Francisco and Chicago. *Available* = binary variable indicating whether the property had any availability during the month and *Nights Booked* = number of nights booked in a given month. The sample contains monthly observations for every property that was ever booked during our sample period. For months in which a property is not listed or available, the outcome measures equal zero by definition. *Home Prices* include sale price and sale price per square foot of transactions. *Sale Price* based on the reported price, and *Price per Sq. Ft.* is based on our calculations using the reported price and size of home. *Foreclosures* reflect the number of foreclosures per tract-month.

Table 2: Effects of Registration Shocks on Size of Airbnb Market

	Availability		Nights Booked	
Post × Treat	-0.097***	-0.074***	-0.890***	-0.574***
	(0.006)	(0.009)	(0.089)	(0.138)
× Airbnb per 1000		-0.001***		-0.018**
		(0.000)		(0.007)
Observations	5,250,612	5,250,612	5,250,612	5,250,612
Tracts	1,193	1,193	1,193	1,193

Notes: Estimated effects of policy on availability and nights booked using linear regressions. All specifications include tract-level fixed effects and metro-month-year fixed effects. Columns 1 and 3 present estimates of the average effect of the registration policy on availability and nights booked measured at the property-month level. *Available* = dummy variable indicating whether the property had any availability in a given month, and *Nights Booked* = number of nights booked per month. Columns 2 and 4 include dose-response estimates, which present how the effects change linearly with tracts' Airbnb density. *X Airbnb per 1000* = the monthly average number of Airbnb listings in the last year pre-treatment per 1,000 population in the 2010 Census measured at the tract level, interacted with post and treatment indicators. Note that quartile 1 tracts have been omitted from the estimation sample, as discussed in Section 3. The sample contains an observation for every month for every property that was ever booked over our sample period. For months in which a property is not listed, the outcome measures are zero by definition. Standard errors are clustered at the tract level. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 3: Effects of Registration Shocks on Size of Airbnb Market, Alternate Specifications

	Availability			Nights Booked		
Panel A: Dose-Response Estimates						
Post × Treat	-0.074***	-0.023***	-0.064***	-0.574***	-0.111	-0.550***
	(0.009)	(0.008)	(0.006)	(0.138)	(0.124)	(0.080)
× Airbnb per 1000	-0.001***	-0.002***	-0.001**	-0.018**	-0.024***	-0.008**
	(0.000)	(0.001)	(0.000)	(0.007)	(0.008)	(0.004)
Panel B: Quartile-specific Estimates						
Post × Treat						
× Quartile 2	0.027*	0.082***	-0.016*	0.293	0.838***	-0.196*
	(0.014)	(0.012)	(0.009)	(0.198)	(0.179)	(0.108)
× Quartile 3	-0.056***	-0.009	-0.062***	-0.345***	0.084	-0.461***
	(0.009)	(0.008)	(0.006)	(0.129)	(0.105)	(0.074)
× Quartile 4	-0.107***	-0.069***	-0.077***	-1.007***	-0.697***	-0.751***
	(0.006)	(0.005)	(0.004)	(0.086)	(0.066)	(0.059)
Observations	5,250,612	5,250,612	5,250,612	5,250,612	5,250,612	5,250,612
Clusters	1,193	1,193	1,193	1,193	1,193	1,193
Tract FE	✓	✓	-	✓	✓	-
Metro-Month-Year FE	✓	-	✓	✓	-	✓
Month-Year FE	-	✓	-	-	✓	-
Property Controls	-	-	✓	-	-	✓

Notes: Estimated effects of policy on monthly availability and nights booked. All estimates use property-month data. Covariates and fixed effects are included as indicated at the bottom of the table. In Panel A, the estimates come from dose-response models, which reflect how the effects change linearly with tracts' Airbnb density. *X Airbnb per 1000* = the monthly average number of Airbnb listings in the last year pre-treatment per 1000 population in the 2010 Census measured at the tract level, interacted with post and treatment indicators. Panel B presents quartile-specific estimates, where the quartile groupings reflect tracts' positions in the distribution of Airbnb density. Note that quartile 1 tracts have been omitted from the estimation sample, as discussed in Section 3. In all cases, standard errors are clustered at the tract level. *** p<0.01, ** p<0.05, * p<0.10.

Table 4: Effects on Home Prices and Foreclosures

	ln($Price$)		Foreclosures	
Post × Treat	-0.053***	0.017	0.059***	0.046***
	(0.012)	(0.012)	(0.009)	(0.011)
× Airbnb per 1000		-0.006***		0.001*
		(0.001)		(0.001)
Observations	147,997	147,997	75,159	75,159
Tracts	1,193	1,193	1,193	1,193
Tract FE	✓	✓	✓	✓
Metro-Month-Year FE	✓	✓	✓	✓

Notes: Estimated effects of policy on logged home transaction prices and number of tract-level foreclosures. Estimates for price effects in columns 1 and 2 use transaction-level data. Foreclosure estimates in columns 3 and 4 use data aggregated to the census tract level. The price and foreclosure regressions include tract-level fixed effects and metro-month-year fixed effects. Columns 1 and 3 present estimates of the average effect of the registration policy on prices and foreclosures. Columns 2 and 4 include dose-response estimates, which present how the effects change linearly with tracts' Airbnb density. *X Airbnb per 1000* = the monthly average number of Airbnb listings in the last year pre-treatment per 1000 population in the 2010 Census measured at the tract level, interacted with post and treatment indicators. Note that quartile 1 tracts have been omitted from the estimation sample, as discussed in Section 3. In all cases, standard errors are clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.10.

Table 5: Effects on Home Prices and Foreclosures, Alternate Specifications

	ln($Price$)			Foreclosures		
Panel A: Dose-Response Estimates						
Post × Treat	0.017	-0.008	0.026**	0.046***	0.038***	0.047***
	(0.012)	(0.010)	(0.012)	(0.011)	(0.009)	(0.011)
× Airbnb per 1000	-0.006***	-0.005***	-0.006***	0.001*	0.002**	0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Panel B: Quartile-specific Estimates						
Post × Treat						
× Quartile 2	0.021	-0.008	0.024*	0.001	-0.004	0.008
	(0.015)	(0.012)	(0.014)	(0.015)	(0.013)	(0.016)
× Quartile 3	-0.000	-0.014	0.014	0.061***	0.060***	0.063***
	(0.013)	(0.012)	(0.012)	(0.010)	(0.008)	(0.010)
× Quartile 4	-0.101***	-0.099***	-0.080***	0.071***	0.073***	0.064***
	(0.013)	(0.012)	(0.013)	(0.008)	(0.007)	(0.008)
Observations	147,997	147,997	147,997	75,159	75,159	75,159
Tracts	1,193	1,193	1,193	1,193	1,193	1,193
Tract FE	✓	✓	✓	✓	✓	✓
Metro-Month-Year FE	✓	-	✓	✓	-	✓
Month-Year FE	-	✓	-	-	✓	-
Controls	-	-	✓	-	-	✓

Notes: Estimated effects of policy on logged home transaction prices and number of tract-level foreclosures. Estimates for price effects in columns 1 through 4 use transaction-level data. Foreclosure estimates in columns 5 through 8 use data aggregated to the census tract level. Covariates and fixed effects are included as indicated at the bottom of the table. The price regressions use property-specific covariates. The foreclosure regressions use tract-level covariates from the ACS. In Panel A, estimates come from dose-response models, which reflect how the effects change linearly with tracts' Airbnb density. *X Airbnb per 1000* = the monthly average number of Airbnb listings in the last year pre-treatment per 1000 population in the 2010 Census measured at the tract level, interacted with post and treatment indicators. Panel B presents quartile-specific estimates, where the quartile groupings reflect tracts' positions in the distribution of Airbnb density. Note that quartile 1 tracts have been omitted from the estimation sample, as discussed in Section 3. In all cases, standard errors are clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.10.

Appendix A Proofs

Proof of Proposition 1: From Equation (1) we have that

$$Q_D = \overline{Q} = [1 - F(p)] \cdot G(c) + \int_c^\infty 1 - F(p - R_i + c)\ dG(R_i). \tag{8}$$

To determine $\frac{\partial p^*(c)}{\partial c}$, note that the Implicit Function Theorem implies that $\frac{\partial p^*(R)}{\partial R} = -\ {}^{\frac{\partial Q_D}{\partial c}}/_{\frac{\partial Q_D}{\partial p}}$. To determine the numerator $\left(\frac{\partial Q_D}{\partial c}\right)$, note that differentiating Equation (8) with respect to c (using Leibniz's integral rule) implies that

$$\begin{aligned}\frac{\partial Q_D}{\partial R} &= g(c) \cdot [1 - F(p)] + 0 - [1 - F(p)] \cdot g(c) - \int_c^\infty f(p - R_i + c)\ dG(R_i) \\ &= -\int_c^\infty f(p - R_i + c)\ dG(R_i) < 0.\end{aligned}$$

Similarly for the denominator $\left(\frac{\partial Q_D}{\partial p}\right)$, differentiating Equation (8) with respect to p implies that

$$\frac{\partial Q_D}{\partial p} = -f(p) \cdot G(c) - \int_c^\infty f(p - R_i + c) dG(R_i) < 0.$$

Thus, the Implicit Function Theorem implies that

$$\frac{\partial p^*(R)}{\partial R} = -\ {}^{\frac{\partial Q_D}{\partial c}}/_{\frac{\partial Q_D}{\partial p}} = \frac{-\int_c^\infty f(p - R_i + c)\ dG(R_i)}{f(p) \cdot G(c) + \int_c^\infty f(p - R_i + c) dG(R_i)} \in [-1, 0).$$

If we assume that $\alpha_i \sim U[a, b]$, then $\overline{Q} = [1 - F(p)] \cdot G(c) + \int_c^\infty 1 - F(p - R_i + c)\ dG(R_i)$ becomes

$$\overline{Q} = \left(1 - \frac{p - a}{b}\right) \cdot G(c) + \left(1 - \frac{p - a}{b}\right) [1 - G(c)] + \frac{1}{b} \cdot \int_c^\infty R_i - c\ dG(R_i),$$

which implies that

$$p^* = (1 - \overline{Q})b - a + \int_c^\infty R_i - c\ dG(R_i) = (1 - \overline{Q})b - a + E[\max\{R_i - c, 0\}],$$

since $E[\max\{R_i - c, 0\}] = \int_c^\infty R_i - c \; dG(R_i) + \int_c^\infty 0 \; dG(R_i) = \int_c^\infty R_i - c \; dG(R_i)$. Thus, $\frac{\partial p^*(c)}{\partial c} = \frac{\partial E[\max\{R_i - c, 0\}]}{\partial c}$. $\square$

Proof of Proposition 2: From Equation (3) we have that

$$\begin{aligned}\Pr(\text{FC}|\Delta c) &= \frac{\int_{p^*(c)}^\infty H(p^*(c) - \alpha_i) - H(-\infty) dF(\alpha_i)}{1 - F(p^*(c))} \\ &\quad + \frac{\int_c^\infty \int_{p^*(c)-R_i+c}^\infty H(p^*(c) - \alpha_i - R_i + c + \Delta c) - H(-\infty) dF(\alpha_i) dG(R_i)}{\int_c^\infty 1 - F(p^*(c) - R_i + c) dG(R_i)}.\end{aligned}$$

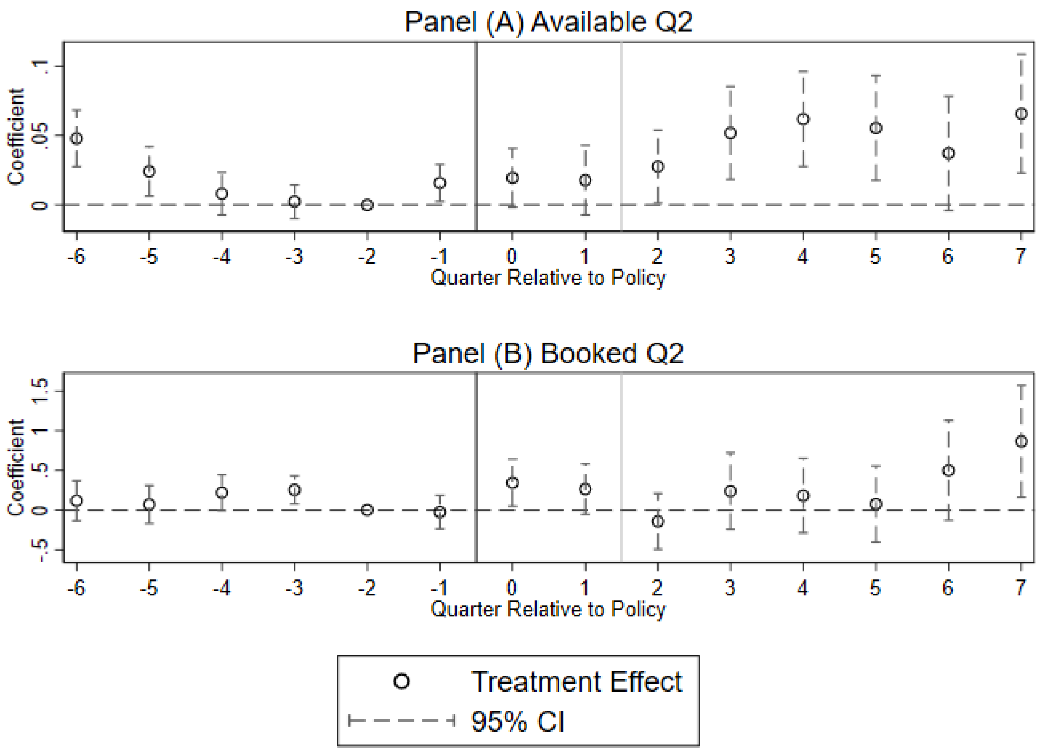
Differentiating implies that

$$\frac{\partial \Pr(\text{FC}|\Delta R)}{\partial \Delta R} = \frac{\int_c^\infty \int_{p^*(c)-R_i+c}^\infty h(p^*(c) - \alpha_i - R_i + c + \Delta c) dF(\alpha_i) dG(R_i)}{\int_c^\infty 1 - F(p^*(c) - R_i + c) dG(R_i)} > 0.$$

$\square$

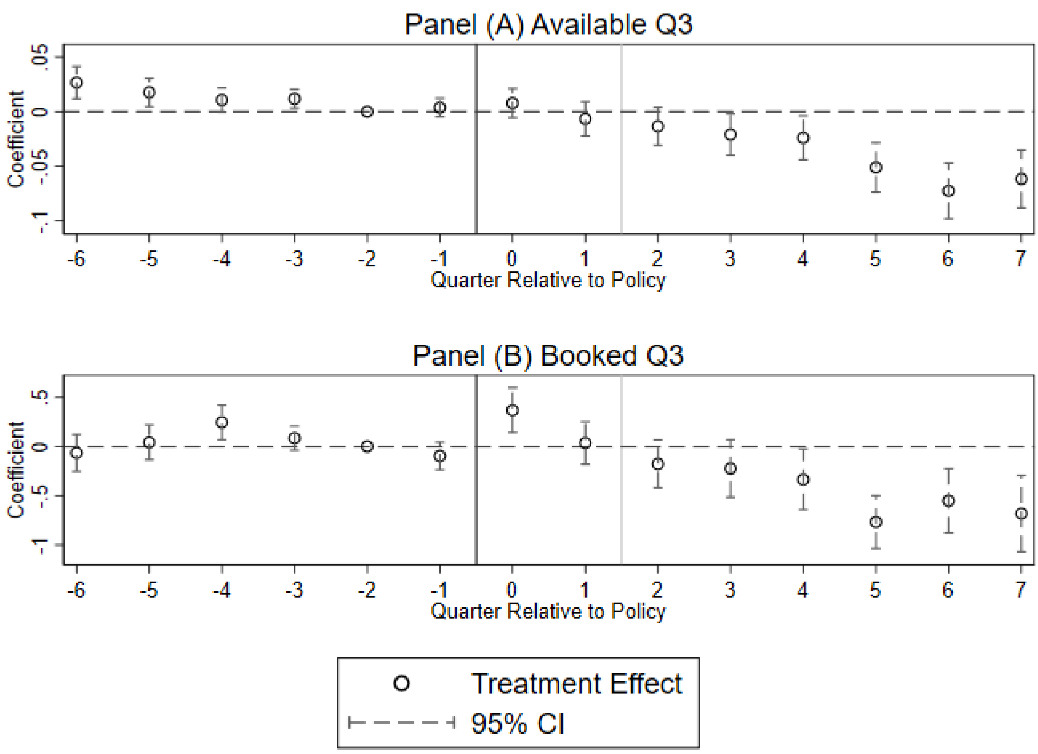
Appendix B Supplemental Tables and Figures

Figure B1: Event Study of Effects of Registration Shocks on Size of Airbnb Market, Quartile 2



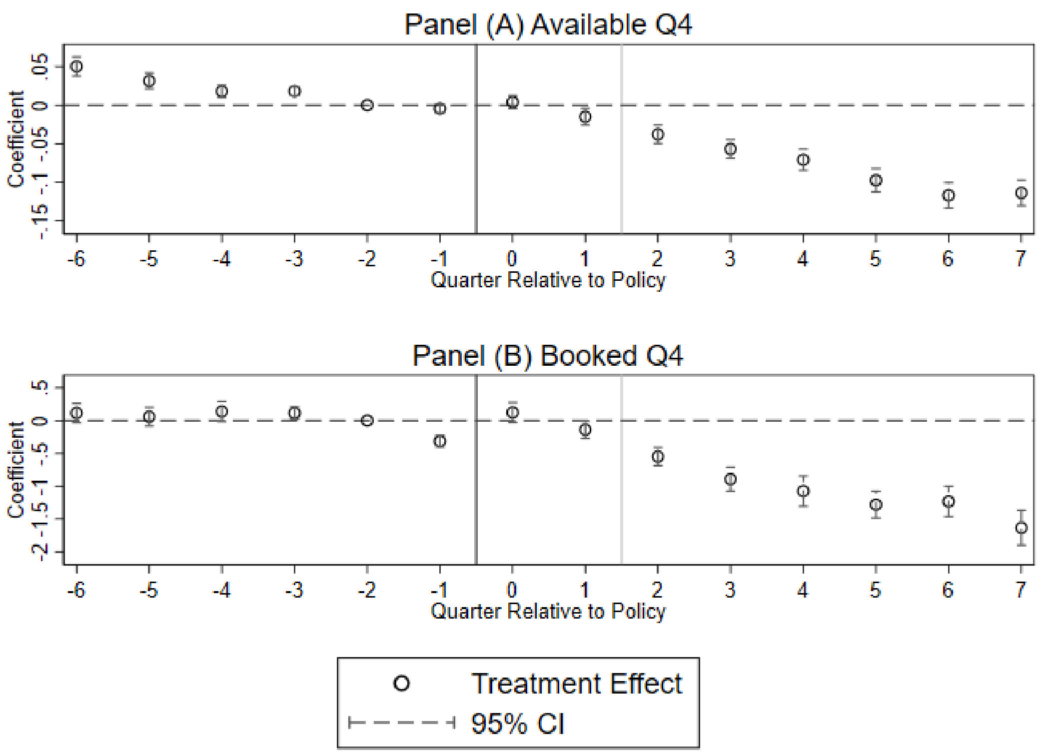
Notes: Quarterly differences in availability and nights booked around the treatment date between treated and untreated tracts in quartile 2 (25th-50th percentile) of the distribution of pre-treatment Airbnb listings per 1,000 tract population. The estimation sample includes all Airbnb listings that ever appear in our sample period. Controls consist of tract fixed effects and metro-month-year fixed effects. The solid vertical lines refer to the date that the policy went into effect (Sep. '17 for SF and Dec. '16 for Chicago). The lighter vertical line refers to the periods where Airbnb started enforcing the policy 4-5 months later. Hollow circles mark the quarter-specific treatment effects, i.e. the time-disaggregated DiD estimates. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level. The estimates on the binned endpoints, periods -7 and 8, are not included in this graph.

Figure B2: Event Study of Effects of Registration Shocks on Size of Airbnb Market, Quartile 3



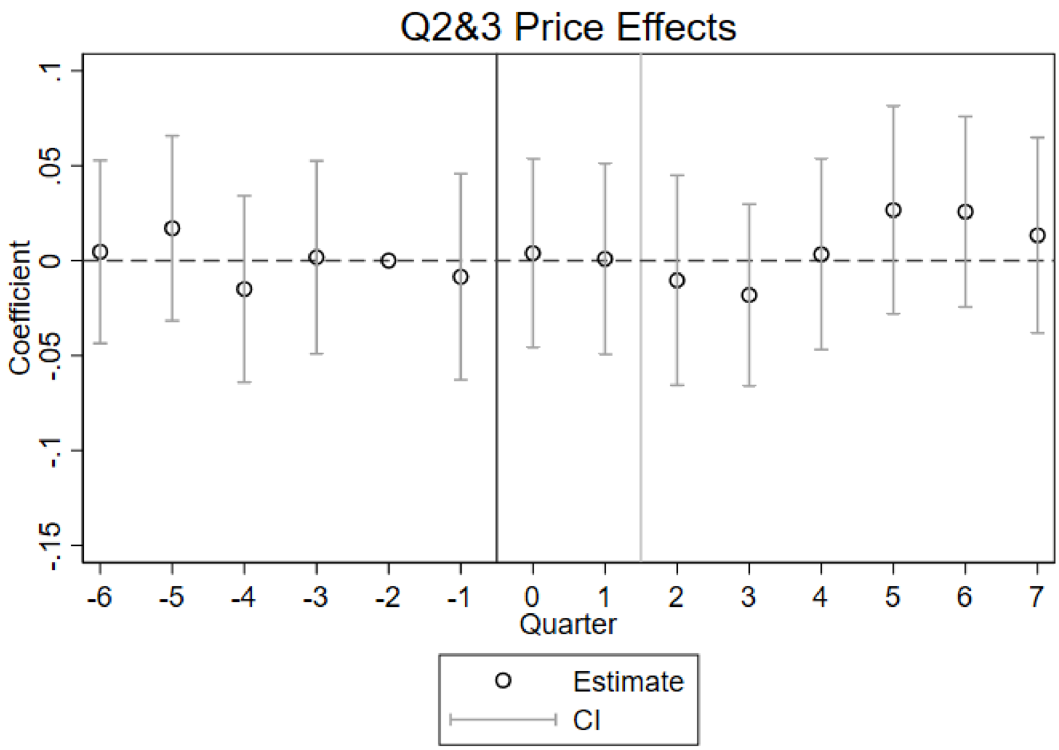
Notes: Quarterly differences in availability and nights booked around the treatment date between treated and untreated tracts in quartile 3 (50th-75th percentile) of the distribution of pre-treatment Airbnb listings per 1,000 tract population. The estimation sample includes all Airbnb listings that ever appear in our sample period. Controls consist of tract fixed effects and metro-month-year fixed effects. The solid vertical lines refer to the date that the policy went into effect (Sep. '17 for SF and Dec. '16 for Chicago). The lighter vertical line refers to the periods where Airbnb started enforcing the policy 4-5 months later. Hollow circles mark the quarter-specific treatment effects, i.e. the time-disaggregated DiD estimates. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level. The estimates on the binned endpoints, periods -7 and 8, are not included in this graph.

Figure B3: Event Study of Effects of Registration Shocks on Size of Airbnb Market, Quartile 4



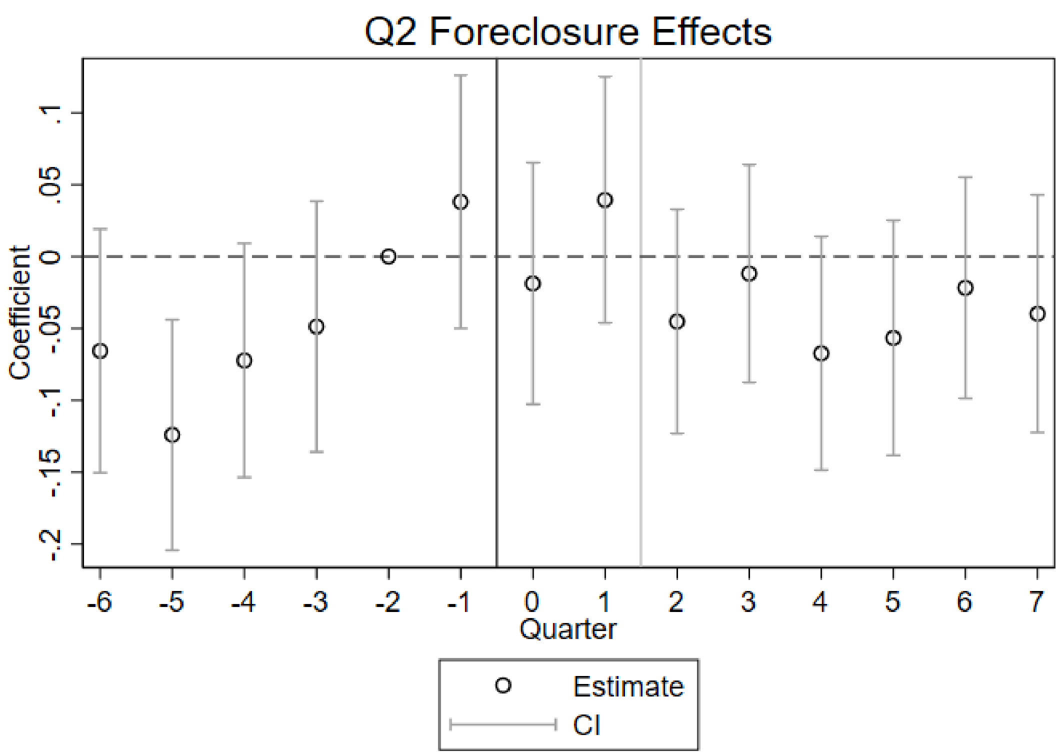
Notes: Quarterly differences in availability and nights booked around the treatment date between treated and untreated tracts in quartile 4 (75th-100th percentile) of the distribution of pre-treatment Airbnb listings per 1,000 tract population. The estimation sample includes all Airbnb listings that ever appear in our sample period. Controls consist of tract fixed effects and metro-month-year fixed effects. The solid vertical lines refer to the date that the policy went into effect (Sep. '17 for SF and Dec. '16 for Chicago). The lighter vertical line refers to the periods where Airbnb started enforcing the policy 4-5 months later. Hollow circles mark the quarter-specific treatment effects, i.e. the time-disaggregated DiD estimates. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level. The estimates on the binned endpoints, periods -7 and 8, are not included in this graph.

Figure B4: Event Study of Effects of Registration Shocks on Prices, Quartiles 2 & 3



Notes: Differences in logged home sale prices around the treatment date between treated and untreated tracts in the 2nd and 3rd Airbnb density quartiles. The specification includes tract fixed effects and metro-month-year fixed effects. The solid vertical line refers to the date that the policy went into effect (Sep. '17 for SF and Dec. '16 for Chicago). Note, however, that Airbnb started enforcing the policies 4-5 months later. Hollow circles mark the quarter-year-specific treatment effects, i.e. the time-disaggregated DiD estimates, relative to the omitted period (quarter -2). The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level. The estimates on the binned endpoints, periods -7 and 8, are not included in this graph.

Figure B5: Event Study of Effects of Registration Shocks on Foreclosures, Quartile 2



Notes: Differences in number of foreclosures around the treatment date between treated and untreated tracts in the 2nd Airbnb density quartile. The specification includes tract fixed effects and metro-month-year fixed effects. The solid vertical line refers to the first quarter that the policy went into effect (Sep. '17 for SF and Dec. '16 for Chicago). Note, however, that Airbnb started enforcing the policies 4-5 months later. Hollow circles mark the quarter-year-specific treatment effects, i.e. the time-disaggregated DiD estimates, relative to the omitted period (quarter -2). The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level. The estimates on the binned endpoints, periods -7 and 8, are not included in this graph.

Table B1: Summary of Airbnb Data For Each City in Sample

City	Metro	Total Listing-Month Obs.	Avg Tract Pop (2010 Census)	Pre-Treat Listings Avail / 1,000 (Tract)	Pre-Treat Mthly Book Per Listing
Berkeley	SF	246,924	3,825	12.35	3.16
Berwyn	Chicago	3,591	5,721	0.83	1.35
Chicago	Chicago	1,732,059	4,322	11.36	2.17
Des Plaines	Chicago	2,736	4,070	0.88	2.22
Evanston	Chicago	41,268	4,444	3.65	2.29
Forest Park	Chicago	7,467	4,815	3.63	2.76
Fremont	SF	71,478	5,459	1.78	1.33
Mountain View	SF	141,303	4,346	13.30	2.36
Naperville	Chicago	3,420	4,940	0.88	1.69
Oak Park	Chicago	25,821	3,871	3.02	2.30
Oakland	SF	407,835	3,629	8.41	2.38
Palo Alto	SF	134,805	4,914	11.08	2.18
San Francisco	SF	1,720,830	4,655	18.24	2.70
San Jose	SF	394,896	5,129	5.77	1.79
San Mateo	SF	73,473	4,383	4.00	1.90
Santa Clara	SF	106,134	6,541	7.05	1.73
Schaumburg	Chicago	2,565	5,149	0.69	0.89
Skokie	Chicago	1,539	5,133	1.38	4.47
Sunnyvale	SF	132,183	5,632	5.75	1.86
Wheaton	Chicago	285	2,890	0.84	4.62

Notes: Summary of Airbnb data by city for tracts included in our main estimation sample. Note that quartile 1 tracts have been omitted from the estimation sample, as discussed in Section 3. "Pre-Treat" refers to only the 12 months before the first policy enactment (i.e. Chicago's registration policy in December of 2016).

Table B2: Summary of Airbnb Data by Airbnb-Density Quartile

	Q1	Q2	Q3	Q4
Airbnb Outcomes				
Available	0.10	0.21	0.25	0.27
	(0.30)	(0.41)	(0.44)	(0.44)
Nights Booked	0.77	1.57	2.27	2.49
	(3.84)	(5.38)	(6.51)	(6.78)
Nights Available	2.69	5.37	6.46	6.70
	(8.34)	(11.10)	(11.84)	(11.93)
Property Characteristics				
Entire Home	0.40	0.35	0.48	0.61
	(0.49)	(0.48)	(0.50)	(0.49)
Superhost	0.80	0.78	0.79	0.82
	(0.40)	(0.41)	(0.41)	(0.39)
Bedrooms	1.53	1.41	1.39	1.34
	(1.52)	(1.22)	(1.23)	(1.56)
Bathrooms	1.36	1.32	1.29	1.31
	(0.70)	(0.70)	(0.68)	(0.76)
Max Guests	3.48	3.13	3.25	3.26
	(2.82)	(2.54)	(2.42)	(2.23)
Number of Photos	12.60	13.19	13.40	13.65
	(10.55)	(11.14)	(10.98)	(11.83)
Number of Reviews	17.06	18.12	21.53	20.72
	(32.02)	(34.78)	(43.29)	(44.49)
Tract Level Characteristics				
Mean Nightly Price	106.77	123.67	135.02	190.60
	(94.93)	(131.92)	(61.87)	(62.51)
Available per 1000 Pop.	0.21	1.45	4.69	16.71
	(0.17)	(0.59)	(1.23)	(11.49)
# of Listings Available	0.98	6.87	21.05	73.46
	(0.85)	(3.77)	(9.09)	(56.76)
Population (1000s)	4.42	4.71	4.54	4.50
	(1.63)	(1.56)	(1.66)	(2.45)
Observations	25524	98892	265728	740772

Notes: Summary of Airbnb listings by quartile over the 12 months before the first treatment date. Quartiles are determined at the tract level, so that each quartile includes one-quarter of tracts but the proportion of properties differs across quartiles. *Q1* refers to the least dense quartile, reflecting the bottom 25% of tracts as measured by number of Airbnb units per 1,000 residents, and *Q4* includes the 25% most-dense tracts. Note that, while quartile 1 is included in this table for completeness, it is excluded from our main estimation sample.

Table B3: Summary of Zillow Data For Each City in Sample

City	Metro	Transactions	Average Sale Price	Foreclosures
Berkeley	SF	3,476	$1,049,024	192
Berwyn	Chicago	1,754	$204,921	196
Chicago	Chicago	32,255	$485,411	2713
Des Plaines	Chicago	1,211	$253,382	117
Evanston	Chicago	2,782	$555,239	173
Forest Park	Chicago	636	$269,369	54
Fremont	SF	10,079	$935,679	380
Mountain View	SF	1,612	$1,160,918	39
Naperville	Chicago	2,156	$329,548	87
Oak Park	Chicago	2,786	$456,020	148
Oakland	SF	16,530	$730,038	1376
Palo Alto	SF	1,486	$958,296	48
San Francisco	SF	26,162	$1,111,142	1122
San Jose	SF	30,437	$839,467	2306
San Mateo	SF	6,340	$1,127,483	168
Santa Clara	SF	3,902	$992,629	143
Schaumburg	Chicago	747	$342,217	42
Skokie	Chicago	436	$331,099	41
Sunnyvale	SF	3,193	$1,228,928	136
Wheaton	Chicago	31	$460,796	6

Notes: Summary of Zillow outcome data by city for tracts included in our main estimation sample. Note that quartile 1 tracts have been omitted from the estimation sample, as discussed in Section 3.

Table B4: Summary of Zillow Data by Airbnb-Density Quartile

	Q1	Q2	Q3	Q4
Zillow Outcomes and Property Characteristics				
Sale Price	249,935	566,305	810,542	970,275
	(221,060)	(403,480)	(434,883)	(454,297)
Price per Sq. Ft.	184.25	375.30	527.98	752.49
	(144.40)	(226.42)	(260.25)	(361.87)
Number of Bedrooms	2.88	3.06	2.88	2.22
	(0.86)	(0.99)	(1.19)	(1.32)
Number of Bathrooms	1.54	1.90	2.00	1.87
	(0.66)	(0.77)	(0.88)	(0.94)
Square Feet	1355.10	1528.66	1632.15	1458.54
	(532.43)	(625.56)	(682.37)	(737.58)
Year Built	1951.23	1959.13	1954.98	1967.02
	(26.34)	(29.34)	(34.33)	(40.96)
Observations	9,949	11,515	9,686	8,394
Tract Level Characteristics				
Foreclosures	0.48	0.26	0.13	0.06
	(0.86)	(0.58)	(0.41)	(0.27)
Observations	4,764	4,776	4,776	4,764

Notes: Summary of Zillow transactions by quartile over the 12 months before the first treatment date. Quartiles are determined at the tract level, so that each quartile includes one-quarter of tracts but the proportion of properties differs across quartiles. *Q1* refers to the least dense quartile, reflecting the bottom 25% of tracts as measured by number of Airbnb units per 1,000 residents, and *Q4* includes the 25% most-dense tracts. Note that, while quartile 1 is included in this table for completeness, it is excluded from our main estimation sample.

Table B5: ACS Summary by Quartile and Treatment

	Tract Level Means and SDs							
	Q1		*Q2*		Q3		*Q4*	
	SF/Chi		SF/Chi		SF/Chi		SF/Chi	
Tract Pop. (1,000s)	3.78	4.79	3.92	4.61	3.60	4.63	3.54	3.78
	(1.70)	(1.39)	(1.72)	(1.33)	(1.61)	(1.61)	(1.80)	(1.48)
Unemployment	19.44	8.23	14.19	8.44	11.04	7.70	6.69	7.26
	(9.65)	(3.76)	(9.01)	(4.05)	(6.54)	(3.58)	(4.78)	(3.49)
Mean Travel to Work	37.40	29.13	35.31	29.44	33.30	27.26	31.28	27.06
	(5.09)	(3.16)	(4.52)	(3.74)	(4.21)	(4.19)	(3.68)	(4.40)
Pct. Below Poverty	27.06	11.21	23.21	10.75	20.57	10.22	14.40	12.99
	(14.51)	(8.59)	(14.87)	(9.01)	(12.79)	(8.40)	(9.61)	(10.59)
Med. HH Inc. (1,000s)	40.68	75.41	46.48	91.54	58.85	94.20	83.31	92.26
	(18.68)	(28.89)	(19.86)	(39.77)	(30.65)	(36.46)	(34.37)	(43.44)
Pct. Built Post 2010	0.25	0.63	0.28	0.64	0.88	0.79	1.09	1.40
	(0.80)	(1.45)	(0.82)	(1.64)	(2.23)	(1.80)	(2.25)	(3.36)
Pct. Built Pre 1939	44.28	8.39	49.65	10.34	50.09	17.43	51.14	29.98
	(24.45)	(13.38)	(22.90)	(19.11)	(20.68)	(23.49)	(20.86)	(24.53)
Pct. >10 Unit Build.	7.14	15.14	13.69	14.13	22.20	20.56	34.39	30.79
	(8.92)	(16.65)	(16.82)	(17.00)	(23.02)	(19.04)	(28.55)	(25.44)
Med. Value of Owner-	157.08	317.35	221.58	532.33	410.70	660.90	647.36	729.22
Occupied (1,000s)	(63.81)	(139.80)	(94.14)	(283.02)	(285.84)	(317.05)	(367.13)	(374.87)
Med. Rent	907.08	1346.49	931.53	1659.19	1140.75	1641.63	1465.46	1606.49
	(165.65)	(377.09)	(167.92)	(543.40)	(419.45)	(451.31)	(477.72)	(491.43)
Med. Age	34.48	37.71	35.99	38.12	35.81	38.19	36.20	38.41
	(6.45)	(6.37)	(6.07)	(4.92)	(6.04)	(5.32)	(6.09)	(6.53)
Pct. Over 65	11.78	12.54	12.06	12.35	11.48	12.91	11.46	13.67
	(5.29)	(6.42)	(4.97)	(4.29)	(5.38)	(4.84)	(7.44)	(6.93)
Pct. Under 18	26.90	24.45	24.64	24.24	20.45	22.04	13.61	17.08
	(6.37)	(4.84)	(5.95)	(3.98)	(6.90)	(4.58)	(5.99)	(6.10)
Pct. Black	52.45	7.01	34.36	7.87	26.51	9.76	11.39	12.29
	(43.55)	(9.80)	(41.28)	(11.10)	(33.52)	(13.58)	(18.82)	(14.58)
Pct. Hispanic	31.14	26.23	29.37	25.56	22.99	21.34	17.26	15.54
	(34.52)	(24.09)	(29.33)	(19.76)	(23.53)	(16.41)	(16.89)	(11.63)
Observations	774	345	489	636	534	591	732	390

Notes: Summary of tract-year measures by density quartile and treatment status from 2014 to 2016.