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How do short-term rental regulations affect market outcomes? Evidence from San Francisco's Airbnb registration requirement

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Abstract

We study the effects of San Francisco's Airbnb registration requirement on market outcomes. The policy reduced Airbnb availability by 20%–27%, nights booked by 22%–31%, and increased booking prices by 3.3% relative to listings in untreated surrounding cities. Relatively commercial listings experience larger availability and booking decreases than casual listings, and similar increases in booking prices. The fraction of available listings designated as commercial fell by 2 p.p. (15%) in the most Airbnb-dense neighborhoods. Overall, the policy reduced nights booked by 27,182/month and hosts' revenue by $5 million/month. Long-term housing prices also fell following enactment, suggesting an improvement in housing affordability.

KEYWORDS

Airbnb, platform regulation, registration enforcement, sharing economy, short-term rentals, housing affordability

1 | INTRODUCTION

Over the past decade, the sharing economy has transformed traditional markets globally. Short-term housing rental platforms are among the most impactful, and have garnered substantial attention from policymakers and researchers.[1] Such platforms, like Airbnb, have dramatically expanded the availability of housing accommodations for travelers (Farronato & Fradkin, 2022; Li & Srinivasan, 2019; Zervas et al., 2017). This in turn affects local economic activity (Basuroy et al., 2020), amenities (Almagro & Domínguez-Iino, Forthcoming), real estate investment (Bekkerman et al., 2023), housing market surplus (Calder-Wang, 2021; Farhoodi, 2021), animosity toward tourists (Fontana, 2021), and discrimination between hosts and renters (Edelman et al., 2017; Laouénan & Rathelot, 2022).

Home-sharing platforms have also drawn sharp criticism from residents and policymakers who argue that they lead to higher housing prices and displacement by reallocating long-term housing to short-term rental markets.[2] Indeed, a growing body of literature shows that Airbnb penetration increases long-term housing prices.[3] Given the tensions, it is no surprise that local governments have attempted to regulate these new and evolving markets. For example, Airbnb has enforced local lodging taxes on behalf of hosts due to regulatory pressure from local authorities to combat substantial evasion in the absence of enforcement (Bibler et al., 2024, 2021). Similarly, the so-called "One Host, One Home" policy has been adopted by several cities across the United States to limit external real estate investment and restore the peer-to-peer short-term rental market to its original "sharing" roots (Chen et al., 2022).

In this article, we study perhaps the largest policy-driven Airbnb shock to date and its effects on short-term rental supply, bookings, and booking prices to learn about how regulations impact Airbnb activity. The policy shock was generated by an agreement between Airbnb and the city of San Francisco in September 2017 to require Airbnb hosts to register their listings with the city and then post registration numbers on their listing pages. San Francisco policymakers viewed registration as crucial for enforcing existing laws and preventing the conversion of long-term rentals into "makeshift hotels" (Kerr, 2018).

Cooperatively enforced registration requirements dramatically increase the cost of hosting one's property on Airbnb, as they may entail long waiting times, reduced ability to evade applicable federal, state, and local taxes (due to a requirement to also register with the San Francisco Treasurer and Tax Collector office), reduced ability to skirt San Francisco's existing "One Host, One Home" policy, and registration fees ($450 every 2 years). They may also facilitate enforcement of restrictions on the number of units or nights available, zoning restrictions, and additional regulatory burden and oversight.[4] Airbnb assisted San Francisco with enforcing the registration policy by removing listings that remained unregistered. In January 2018, 4 months following initial implementation, Airbnb removed almost 5000 unregistered listings (nearly 50%).[5] In contrast,

[1] Ridesharing has also made large impacts, affecting labor markets (Berger et al., 2018; Chen et al., 2019), transit and congestion (Agrawal & Zhao, 2023; Hall et al., 2018; Tarduno, 2021), alcohol consumption (Teltser et al., 2021), drunk driving and traffic fatalities (Anderson & Davis, Forthcoming; Barrios et al., 2023; Brazil & Kirk, 2016; Dills & Mulholland, 2018; Greenwood & Wattal, 2017; Zhou, 2020), and more.

[2] For example, one New Orleans resident spray-painted "This Airbnb displaced 5 people" on the sidewalk in front of an Airbnb listing (Maldonado, 2018). A photo led 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 (Khouri, 2019).

[3] See, for example, Barron et al. (2021), Chen et al. (2022), Duso et al. (2024), Garcia-López et al. (2020), Garcia et al. (2021), Horn and Merante (2017), Koster et al. (2021), Seiler et al. (2024), and Wyman et al. (2022).

[4] See Airbnb (2022) for further examples.

[5] See the news article by Said (2018), and also research from Rossi (2024), which leverages this large shock to study the relationships between competition, reputation, and Airbnb host effort.

without Airbnb's cooperation, enforcement would have been much more costly and the policy would likely have had little to no bite.

For the policy to relieve pressure on long-term housing, it should induce reallocation of properties back to the long-term market, which would correspond to a reduction in Airbnb supply and bookings. Moreover, such reallocation depends in part on the extent to which the policy affects relatively commercial listings (i.e., those dedicated as full-time Airbnb rentals), as opposed to properties hosted by long-term owner-occupiers (or renter-occupiers) who host on Airbnb more casually. As Kytömaa (2024) points out, large increases in fixed costs (like those introduced by the enforcement of registration requirements) can lead to exit for both casual and commercial hosts/listings. Since San Francisco's policy effectively simultaneously enforces several short-term rental policies, it is plausible that the magnitude of the cost shock varies with host/listing type, which implies ambiguous relative magnitudes of the effects on each type. Naïvely, we might expect commercial listings to be less affected by a general registration policy, as hosts of commercial listings are likely better-equipped to overcome the costs associated with policy compliance. However, in this case the cooperatively enforced registration policy also helps the city enforce existing regulations, several of which are geared toward discouraging commercial Airbnb activity.[6] Hosts of commercial listings may also have more outside options, like pursuing long-term rental contracts or selling the property, such that they are more sensitive to the costs associated with the registration policy.

To assess how San Francisco's registration policy impacted Airbnb activity, we estimate average effects across the entire city, as well as heterogeneity in these effects across relatively casual versus commercial listings and across neighborhoods of varying Airbnb popularity. We exploit three dimensions of variation to obtain credible causal estimates. The first is temporal variation, using Airbnb and housing data before and after policy implementation and enforcement. The second is spatial variation, comparing outcomes of treated Census tracts (i.e., those within the San Francisco city limits) to untreated tracts (i.e., those outside of the city limits but within the metro area). Third, for some supplementary analyses, we exploit variation in treatment intensity as measured by pretreatment Airbnb activity.

We use data scraped from Airbnb.com by AirDNA on prices, bookings, and property characteristics of listings in the San Francisco metro area to construct a balanced listing-year-month panel. We find that, following policy enactment, the probability a listing in the city of San Francisco is available in a month falls by 6 to 8 percentage points (20%–27%) and the number of nights booked per listing-month falls by 0.6 to 0.9 (22%–31%) compared to untreated listings in the broader San Francisco metro area. Reductions in both availability and nights booked suggest that the policy meaningfully reduced the size of the market, as opposed to only reducing slack capacity (i.e., driving out listings that were rarely, if ever, booked). Because this negative supply shock reduces nights booked, we may also expect to observe a corresponding price increase. We test this and find that the average nightly booking price increases by over $5, or roughly 3%. Using listing count and baseline averages for the city of San Francisco, along with our preferred estimates, we find aggregate nights booked fell by 27,182 per month and host revenue fell by $5.29 million per month. We also find that the effects from registration enforcement persist over time, and that the timing of the effects suggests successful regulation of Airbnb relies on cooperation with the platform (consistent

[6] For example, short-term hosts are required to be permanent San Francisco residents, living in their unit for at least 275 nights per year. There is also a requirement that rentals without a host present cannot exceed 90 days per year, and that hosts who are themselves tenants in a rent-controlled unit cannot collect more in revenue than they pay their landlord on a monthly basis. See Airbnb (2022) for further details.

with Bibler et al., 2021). Our results are robust to alternate specifications, estimation, and sample composition choices, and event studies demonstrate little to no evidence of differential pretrends.

Distinguishing between commercial and relatively casual listings, we find that commercial listings are more negatively impacted by registration enforcement. We define "commercial" to mean entire-home listings in the top capacity quartile, where capacity quartiles are based on average number of bookable nights across the months in which the listing is available. The proportion of commercial listings available in a given month declines by 10.1 percentage points (26%), while among lower-capacity listings the reduction is 6.9 percentage points (26%). Commercial listings are also booked 1.7 fewer nights per month (58%), while the reduction is only 0.88 nights among lower-capacity listings (31%). Finally, booking prices for commercial listings increase by a similar amount ($5.78, or 2.3%) to lower-capacity listings ($6.03, or 3.6%). Among the third category, partial-home listings with high capacity (e.g., renter- or owner-occupiers with a spare room that is nearly always available), we find an 11.5 percentage point reduction in availability (29%) but no statistically or economically meaningful change in nights booked or booking prices, suggesting that the registration policy mainly reduced slack capacity in this segment of the market.

In total, we find that commercial listings experienced 5447 fewer nights booked per month (53%) and a $1.45 million decrease in monthly revenue (52%). Meanwhile, lower-capacity listings saw 21,077 fewer nights booked per month (33%) and a $4.2 million decline in monthly revenue (31%). While the impacts on relatively commercial listings are clearly proportionally larger, the impacts on relatively casual listings are also sizable, suggesting that registration policies may also have the unintended consequence of limiting the sort of home sharing originally envisioned when Airbnb was introduced.

To the extent that an overarching policy goal seems to be to improve long-term housing affordability in the areas where Airbnb is most popular, we also examine whether policy-induced exit (or deterred entry) affects the long-term housing market. The first step is to confirm that the registration policy's effects are largest (in levels) in the neighborhoods where Airbnb is most popular. We aggregate to the Census tract level and estimate heterogeneity across tracts with varying levels of pretreatment Airbnb density (i.e., number of available listings per 1000 population). Indeed, we find the largest effects in quartile 4; 13 fewer available listings per tract-month, 106 fewer nights booked per tract-month, and an increase in average booking prices by $8.62 per night. In quartile 3, we estimate a decrease of roughly 4.4 available listings per tract-month, a decrease of 25.6 nights booked per tract-month, and no statistically significant effect on booking prices. We find no statistically or economically meaningful effects among tracts in the lowest two quartiles. In an aggregate analysis of composition, we also estimate a 2 percentage point (roughly 15%) decline in the share of available listings that are relatively commercial in the most Airbnb-dense quartile of tracts.

While we cannot observe long-term housing quantities, we can observe prices using the Zillow Home Value Index (ZHVI) and the Zillow Observed Rent Index (ZORI). If long-term housing supply increases, or the value of housing falls as potential income from renting one's property on Airbnb is reduced or eliminated, we would expect to see equilibrium home and long-term rental prices fall in San Francisco relative to other cities in the metro area. Moreover, we would expect the price effects to be larger in the most Airbnb-dense zip codes where the shocks were largest. Indeed, we find overall decreases in home prices (11%) and long-term rental prices (1%), and the estimated effects are larger in more Airbnb-dense areas.

Our findings contribute to the existing literature in several ways. We provide evidence that enforcing a short-term rental registration policy can generate large reductions in short-term rental activity and revenue, and potentially improve affordability of both home and long-term rental prices. In the case of San Francisco, while the registration policy itself may not be very cumbersome, it provides a means of enforcing other short-term rental regulations that would otherwise be prohibitively difficult to enforce. We also separately examine how the policy affected relatively commercial versus casual listings, in accordance with the fact that many short-term rental policies seem designed to primarily deter commercial rather than casual hosts. Finally, by documenting the impacts of this large policy shock in San Francisco, we highlight its value as a quasi-experiment that can be used to conduct further research on the social and economic effects of short-term rental platforms like Airbnb.

Our work complements existing research that examines similar cooperative enforcement of short-term rental regulations with Airbnb in 2016–2017 in a different city: New Orleans, Louisiana. For example, using a difference-in-discontinuty design, Valentin (2021) finds substantial reductions in Airbnb supply and bookings in the French Quarter relative to neighboring areas, reductions in housing values, but no clear effect on booking prices. Müller et al. (2022) also study the New Orleans regulatory shock, documenting differential effects across host types, but only examining pricing responses.

A contemporaneous working paper studies the same San Francisco regulatory shock, finding larger exit among *non*professional hosts while professional hosts appear to shift to regulation-exempt medium-term rentals (Kytömaa, 2024). In addition, Kytömaa (2024) finds similar increases in advertised nightly prices across both types. Jin et al. (2024) find a similar pattern of heterogeneous quantity effects following Chicago's cooperatively enforced short-term rental regulations introduced in 2016–2017, though no clear evidence of booking price effects. Kytömaa (2024) finds limited evidence of effects on home prices and long-term rental prices, while Jin et al. (2024) do not examine long-term housing price effects. Some additional key differences exist between our article and Kytömaa (2024) and Jin et al. (2024). Kytömaa (2024) uses Los Angeles and Portland as control cities, and Jin et al. (2024) use Atlanta, Boston, and Los Angeles as control cities, as opposed to cities in the same metro area as the treated city. Kytömaa (2024) uses Inside Airbnb data, which (unlike AirDNA) does not include information on nights booked nor the associated booking prices. Finally, both Jin et al. (2024) and Kytömaa (2024) use host-based definitions to distinguish between casual and professional listings, primarily leveraging whether a host has multiple simultaneous listings. In contrast, we use a listing-based definition in an effort to avoid misclassifications that could result from hosts gaming existing and new regulatory policies (e.g., creating multiple host accounts to circumvent the existing "One Host, One Home" policy in San Francisco), and find larger negative effects among relatively commercial listings.

Additional related papers studying the effects of regulations on Airbnb market activity include Gauß et al. (2024) and Koster et al. (2021). Gauß et al. (2024) examine the effects of regulatory shocks in a few German cities, including heterogeneity among relatively casual and commercial hosts. Their paper finds larger negative effects on relatively casual hosts, small increases in booking prices, but no resulting impact on long-term rental prices. Koster et al. (2021) study the impacts of home-sharing ordinances in 18 out of 88 cities in Los Angeles County, finding significant reductions in supply and a corresponding small decline in long-term rental prices. A couple key differences are that the policies studied by Gauß et al. (2024) and Koster et al. (2021) were not cooperatively enforced by Airbnb, and neither nights booked nor booking prices are observed in Koster et al. (2021).

Finally, our work contributes to the growing body of evidence that cooperation between the government and the platform helps enforce existing taxes and regulations more effectively (e.g., Bibler et al., 2024, 2021; Garz & Schneider, 2023a, 2023b; Jin et al., 2024). In addition, it relates to the broader Airbnb literature, including work that models and estimates the role of Airbnb in homeownership decisions, spillover costs and benefits, housing market matching frictions, and optimal policy setting (Farhoodi et al., 2021; Filippas & Horton, 2017; Filippas et al., 2020; Garcia et al., 2021).

2 | DATA

To conduct our analyses, we use public-facing information on Airbnb listings, including property characteristics and geographic coordinates, calendar availability, and implied bookings collected by AirDNA. 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 in the San Francisco metropolitan area. The sample period contains the window of time 15 months before policy enactment through the 21 months following policy enactment (i.e., June 2016 through May 2019, 36 months total). The data come from 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; Valentin, 2021).[7]

We restrict to the 10 largest cities in the metro, as measured by the total number of Airbnb listings, in an effort to avoid comparing very Airbnb-active areas to much less active areas. These 10 cities are Berkeley, Fremont, Mountain View, Oakland, Palo Alto, San Francisco, San Jose, San Mateo, Santa Clara, and Sunnyvale. Summary statistics for each city are reported in Table A.1. Next, we use reported coordinates to assign listings to Census tracts. For each tract, we calculate a measure of Airbnb density equal to the average number of Airbnb listings per 1000 population during the 15 months preceding policy enactment.[8] We use this measure to assign tracts to density quartiles, and then estimate heterogeneous treatment effects by quartile. In Figure 1, we present maps of the tracts across the 10 cities in our sample, shaded by density-quartile assignment.

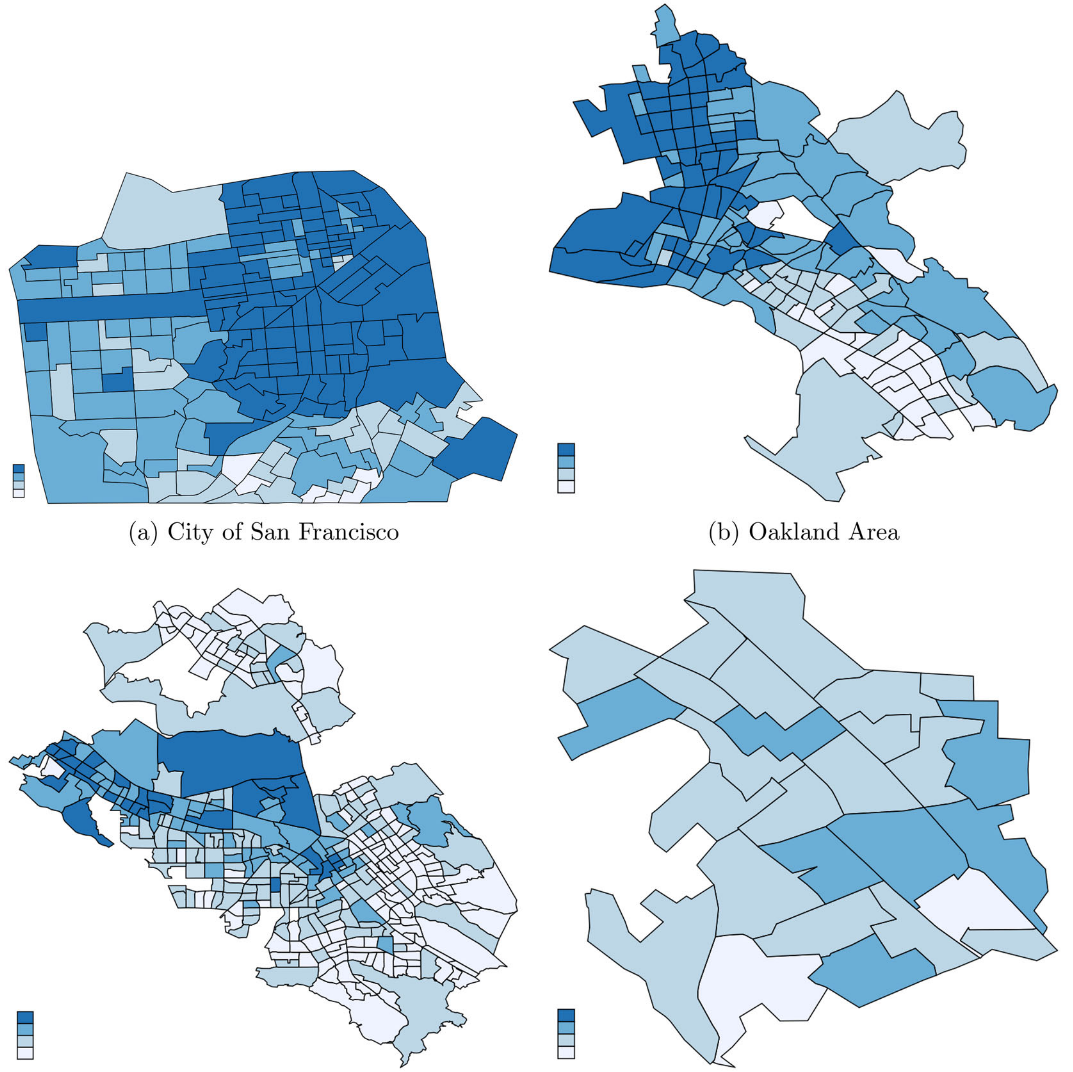
Proceeding with the restricted sample, we aggregate our property-day data to the property-month level. Our primary interest is measuring the size of the short-term rental market, and how it changes in response to the registration requirement shock. To that end, we examine availability, nights booked, and booking prices. 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.[9] Examining both is important, as a reduction in availability alone might suggest that only relatively inactive listings exit, implying little to no tangible impact on the true supply of housing allocated to the short-term rental market. We also examine posted prices associated with property nights booked to estimate the extent to which the supply shock affected booking prices.

For our estimation sample, we rectangularize the data to obtain a balanced panel of property-month observations for all listings that were booked at least once during our data set's original

[7] This is in contrast to papers that use administrative data from Airbnb, such as Jaffe et al. (2019) and Farronato and Fradkin (2022).

[8] For reference, we use 2010 tract-level Census population counts.

[9] Note that AirDNA does not directly observe bookings; they scrape each listing's calendar of availability every 1–3 days to detect changes. A change in availability suggests a booking has occurred, which can be verified if/when a renter leaves a review for the host/property after the stay. The main limitation is that AirDNA may incorrectly infer that a booking has occurred, and thus overmeasure the number of nights booked, when a host blocks out a previously-available night. Because we find the policy shock substantially reduces availability, such measurement error would tend to positively bias our estimated effects on nights booked, thereby suggesting that we underestimate the true negative shock to bookings.



(a) City of San Francisco (b) Oakland Area

(c) San Jose Area (d) San Mateo

FIGURE 1 Maps of tracts in sample.

Note: Tracts are shaded from light blue to dark blue based on quartiles of pretreatment Airbnb density per capita, where the lightest shade is quartile 1 and the darkest shade is quartile 4. To improve the visualization/scaling of the map, we omit five tracts that are geographically large from the periphery of the Oakland and San Jose maps.

sample period (August 2014 through August 2019). Every property has an observation for every month, regardless of whether they were only listed for part of the sample period. In months where a property is not listed, its outcome measures (availability and bookings) are zero by definition. Balancing the panel in this way allows us to capture both the intensive and extensive margins of Airbnb activity.

Table 1 shows the average availability during the full-sample period for all listings in control cities (0.31), as well as only those within the San Francisco city limits (0.27), implying roughly 31% of control city listings in our balanced panel were available to be booked at least one day in

TABLE 1 Summary of Airbnb outcome variables of interest.

	(1) Control cities	(2) SF city limits
Available	0.310	0.272
	(0.462)	(0.445)
Nights booked	2.90	2.83
	(7.31)	(7.45)
Booking price	128.72	206.27
	(115.66)	(179.37)
Number of listing-months	1,098,684	1,087,272

Note: Means and standard deviations at the property-month level. Column 1 includes the entire estimation sample for the control areas, and column 2 includes only the properties in the city limits of San Francisco. *Available* = binary variable indicating whether the property had any availability during the month and *Nights booked* = number of nights booked in a given month. *Booking price* is the average posted nightly price on nights that have been booked, weighted by number of bookings. The sample contains monthly observations for every property that was ever booked during our data set's original sample period (August 2014 through August 2019). For months in which a property is not listed or available, *Available* and *Nights booked* equal zero by definition.

any given month. The second row presents nights booked per property-month, which averages 2.9 in the control cities and 2.83 among listings in the city of San Francisco. Both measures reveal comparable activity among treated and untreated listings. The third row presents average booking prices, where we see higher booking prices in San Francisco ($206.27 per night) than in the rest of the cities in our sample ($128.72). To further inspect the comparability of the treated and untreated listings, we present event studies in Section 4 and find essentially no evidence of differential trends leading up to the policy shocks.

In Table A.2, we further summarize our Airbnb data by quartiles of tract-level Airbnb density. These panels provide insight on Airbnb market outcomes and listing characteristics across areas of varying Airbnb popularity. Availability is comparable among properties in San Francisco and the rest of the sample across all quartiles. Average capacity (number of nights available or booked per month) is also comparable across all subgroups, except for slightly lower average capacity among city of San Francisco listings in the top density quartile. Nights booked tend to be slightly lower outside of the city of San Francisco in the lower density quartiles, but in quartile 4 listings in the city of San Francisco see slightly fewer nights booked. Booking prices are higher in the city limits than in the rest of the metro across all quartiles, and tend to be higher in the more popular Airbnb tracts. Entire-home listings are more common in San Francisco relative to the rest of the cities, and more common in the higher density quartiles. That said, average number of bedrooms and maximum number of guests are similar across all subgroups. The proportion of listings designated as "High capacity" (which we define as those in the top quartile of average capacity across all months in which the property is available) is remarkably similar across San Francisco and the other cities as well as across quartiles. The proportion of "Superhosts" (defined by Airbnb as those who provide excellent hospitality based on reviews, responsiveness, and low cancellations) is lower in San Francisco relative to the rest of the cities across all quartiles, but there is little gradient across quartiles. Notably, there is sufficient variation in treatment status within each quartile, which allows us to estimate heterogeneous effects across more/less Airbnb-dense tracts.

3 | ESTIMATION

To estimate the effect of Airbnb's cooperative enforcement of San Francisco's registration requirement on the Airbnb market, we use a standard difference-in-differences (DiD) estimator. We then examine how the effects vary across types of Airbnb hosts, and test whether the effects are larger in tracts with a greater pretreatment density of Airbnb listings.

The following is our core DiD specification:

$$Y_{ijt} = \gamma Reg_{jt} + \eta_i + \delta_t + \mu_{ijt}, \tag{1}$$

where Y_{ijt} is the outcome of interest for property i in tract j, and month-year t. We use property as our cross-sectional unit, which allows us to control for property-specific time-invariant heterogeneity. Reg_{jt} 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_i are property-level fixed effects to control for time-invariant differences across listings, δ_t are month-year fixed effects to control for idiosyncratic time shocks (e.g., demand shocks or seasonal effects), and μ_{ijt} reflects the idiosyncratic error term. In this specification, as well as all others, we use standard errors that are robust to clustering at the tract level.

Next, we go beyond our core DiD approach to examine heterogeneity in tract-level treatment effects by Airbnb market density (i.e., number of pretreatment Airbnb listings per 1000 tract residents) as a sort of sanity check (or "third difference") to see whether more listings are impacted in the areas where more listings already exist. We test for differential effects using the following interacted specification:

$$Y_{jt} = \sum_k \gamma_k Reg_{jt} + \eta_j + \delta_t + \mu_{jt}, \tag{2}$$

where k indexes the Airbnb density group, which includes Quartiles 1 and 2 combined, Quartile 3, and Quartile 4 of the Airbnb density distribution.[10]

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 pretreatment periods, as well as estimate 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.

$$Y_{ijt} = \sum_{k=-5}^{6} \gamma_k D_j \cdot 1(q - Q_j = k) + \eta_i + \delta_t + \mu_{ijt.} \tag{3}$$

Here, D_j is an indicator for whether tract j is ever-treated, which is interacted with indicators for five quarters (indexed by q) leading up to the quarter during which treatment occurs (Q_j) as well as the seven posttreatment quarters (0, 1, …, 6). The set of $\hat{\gamma}_k$ are then plotted to provide

[10] Recall, this is calculated as the average monthly number of Airbnb units per 1000 tract residents in the 15 months prior to the policy enactment in September 2017. We assign density quartiles based on tract-level aggregates, such that 25% of tracts fall into each quartile but the number of properties in each quartile differs accordingly.

visual support of parallel pretrends as well as time-disaggregated estimated treatment effects. In addition to the property-level version of the event studies, we also estimate and present tract-level event studies by quartile group.

Note that, because there is only one treatment date in our setting (September 2017), the estimated γ should not suffer from the negative-weighting issue that can arise in two-way fixed effects models when there is variation in treatment timing and heterogeneity in treatment effects (e.g., Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Shapiro, 2022). That said, de Chaisemartin and D'Haultfœuille (2020) suggest that heterogeneity across groups or time can generate bias due to negative weights even without staggered treatment timing. The event study results presented in the following section appear to alleviate this concern. Moreover, in additional (aggregate-level) results available upon request, we find that using the de Chaisemartin and D'Haultfœuille (2020) estimator and ordinary least squares (OLS) produce very similar estimates.

4 | RESULTS

4.1 | Main estimates

To examine the effect of San Francisco's registration requirement policy shock on the Airbnb market, we estimate the DiD parameters outlined in Section 3. The first of these results are presented in Panel A of Table 2, where we present the main property-level DiD estimates of the impact of the policy shock on availability, nights booked, and booking prices. In all columns, we account for time-invariant property-level heterogeneity by controlling for property fixed effects. In columns 1, 3, and 5, we control for month-year fixed effects. In columns 2, 4, and 6, we instead control for density quartile by month-year fixed effects to account for the possibility that existing trends in Airbnb market outcomes differ across neighborhoods of varying Airbnb popularity.[11]

The availability estimates range from a 5.9 to 7.8 percentage point reduction in the probability that a property is available in a given month-year. This amounts to a 20%–27% reduction in supply relative to the baseline average availability proportion of 0.29. It is important to note that measuring market size using availability includes both utilized and slack supply. Reductions in availability suggest that fewer units are offered by hosts, but this could come from slack (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 roughly 0.63 to 0.9 fewer nights booked following the registration shock, which is 22%–31% relative to the baseline average of 2.87 nights booked per property-month. In Table A.4, we show that our property-level availability and booking estimates are robust to nonlinear specifications, namely, probit for the availability outcome and Poisson for nights booked, yielding nearly identical estimated marginal effects on availability and slightly larger effects on nights booked.[12]

[11] In additional results available upon request, we find nearly identical property-level results to those in Panel A columns 1, 3, and 5 when we instead include month-year fixed effects along with tract-specific month fixed effects to control for neighborhood-specific seasonality.

[12] The nights booked estimates are likely slightly larger because Poisson estimation drops listings that are never booked during the sample period.

TABLE 2 Effects of registration shocks on market outcomes.

	Available		Nights booked		Booking price	
Panel A: Property-level						
Treat × Post	−0.0782***	−0.0585***	−0.9033***	−0.6363***	5.4649***	5.1311***
	(0.0059)	(0.0064)	(0.0774)	(0.0872)	(0.8491)	(0.8650)
	[0.29]	[0.29]	[2.87]	[2.87]	[166.86]	[166.86]
Property FE	x	x	x	x	x	x
Observations	2,185,956	2,185,884	2,185,956	2,185,884	374,600	374,591
Panel B: Tract-level						
Treat × Post × Q1 and Q2	−1.0245	−1.0811	−14.2747	−6.0958	−2.4328	−2.0532
	(0.9609)	(0.9486)	(11.8906)	(11.7143)	(5.9913)	(6.3030)
	[8.83]	[8.83]	[80.30]	[80.30]	[113.61]	[113.61]
Treat × Post × Q3	−4.3717***	−4.4781***	−25.6270**	−35.6501***	1.0022	−2.6085
	(0.8754)	(1.1158)	(10.4042)	(12.1888)	(3.6955)	(4.2612)
	[25.17]	[25.17]	[247.51]	[247.51]	[149.69]	[149.69]
Treat × Post × Q4	−13.3107***	−12.9535***	−106.2576***	−125.3870***	8.6185***	10.6601***
	(1.3520)	(1.6672)	(14.4785)	(21.7288)	(3.2408)	(3.5158)
	[54.23]	[54.23]	[547.76]	[547.76]	[190.15]	[190.15]
Tract FE	x	x	x	x	x	x
Month-year FE	x		x		x	
Quartile-month-year FE		x		x		x
Observations	26,244	26,208	26,244	26,208	24,079	24,070
N of tracts	729	728	729	728	721	720

Note: Estimated effects of policy on availability, nights booked, and booking prices using linear OLS regressions. In Panel A (property-level analyses), *Available* = dummy variable indicating whether the property had any availability in a given month, *Nights booked* = number of nights booked per property-month, and *Booking price* = average price per night booked weighted by number of nights booked. In Panel B, these outcomes are aggregated to the Census tract level and stratified by tract-level Airbnb density quartiles, which are determined by the average per-capita number of available Airbnb listings in each tract across the 15 pretreatment months. The property-level estimation sample contains an observation for every month for every property that was ever booked during our data set's original sample period (August 2014 through August 2019). For months in which a property is not listed, the outcome measures are zero by definition for availability and nights booked, and missing for booking price. Standard errors are in parentheses, and are robust to clustering at the tract level. Dependent variable means are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $* \ p < 0.10$.

Our quantity estimates confirm that policy enforcement dramatically reduced the size of the Airbnb market, rather than simply inducing the exit of marginally-active infrequently booked listings. We also examine whether this negative supply shock increased booking prices, and estimate an increase in nightly booking price of $5.13 to $5.46, or a 3% increase relative to the baseline mean of $166.86. Using San Francisco baseline averages and listing count, along with the estimates in columns 3 and 5 of Panel A, we find aggregate nights booked declined by 27,182 per month and hosts' monthly revenue fell by over $5 million.[13]

Next, in Panel B of Table 2, we present tract-level quartile-specific estimates using the same two specifications for each outcome of interest as a sort of sanity check to confirm that effects are largest in the areas with more Airbnb listings. In Airbnb density quartiles 1 and 2, we find very

[13] For reference, average nights booked per property-month is 2.83, there are 30,202 listings, and the nightly booking price average is $206.27.

small and statistically insignificant decreases in available listings, nights booked, and booking prices. Turning to quartile 3, we start to see statistically significant effects. Specifically, we find a decrease of roughly 4.4 available listings per tract-month (17.5% relative to baseline mean of 25.17), a decrease of 25.6 to 35.6 nights booked per tract-month (10%–14% relative to baseline mean of 247.51), and no statistically significant impact on booking prices. In quartile 4, we see the largest effects; 13 to 13.3 fewer available listings (roughly 24%), 106 to 125 fewer nights booked (19%–23%), and an increase in average booking prices by $8.62 to $10.66 per night (4.5%–5.6%).

We caution against interpreting the ratio of price and quantity effects as an implied elasticity of demand. First, we do not observe changes in fees over time, so the estimated effects on booking prices may understate the effect on total prices. Second, the policy creates heterogeneous shocks to fixed costs, which induces selective exit. In this setting (as opposed to the tax enforcement case in Bibler et al. (2021), which induces little to no net exit), policy-induced exit is the primary driver of the decline in nights booked, while price effects are estimated based on the fraction of the sample that is undeterred by the policy shock (i.e., those that do not exit). Thus, the estimated overall change in quantity is larger than the change in bookings among listings undeterred by the new fixed costs.

To probe the parallel trends assumption required for our DiD procedure to yield unbiased estimators of causal parameters, we estimate several event study specifications. In Figure 2, we present the event study figures for our property-level analyses, from the specification that includes property fixed effects and month-year fixed effects as controls. In all three subfigures, we find little to no evidence of differential pretrends between property listings in treated versus untreated tracts leading up to the policy enactment in quarter 0, which provides evidence in support of the parallel trends assumption. In Panels (a) and (b), we find clear reductions in availability and nights booked, and it appears the magnitudes of the reductions grow over time. In Panel (c), while the estimates bounce around a bit, they show fairly clear evidence of an increase in booking prices that also grows over time. We also present quartile-specific event studies when aggregating to the census tract level in Figures A.1, A.2, and A.3, where we again find posttreatment effect magnitudes that (a) become larger over time (particularly in quartiles 3 and 4), (b) are increasing in tract-level Airbnb density, and (c) exhibit little to no evidence of differential pretrends.[14]

Next, we address the concern that spillovers from San Francisco to the control cities induced by the policy shock could lead us to overestimate the true causal effects of interest. While none of the control cities in our sample are geographically contiguous to the city of San Francisco, it is still important to probe the assumption of no spillovers. In Table A.5, Panel A, we reestimate Panel A of Table 2 after excluding the three geographically-closest control cities (Oakland, Berkeley, and San Mateo). The estimates turn out to be slightly larger across the board, which is the opposite of what we would expect if spillovers were contaminating the control group.

Finally, we address the concern that similarly timed introductions of short-term rental regulations in the control cities could lead us to underestimate the true causal effects of interest. While we did not find regulations in any of the control cities that were cooperatively enforced

[14] One may notice a jump in some quarter-1 coefficients (i.e., June, July, and August 2017). First, we note that this jump is only meaningfully present in the aggregate-level analyses, and primarily in density quartile 3 and the nights booked event study for the bottom two density quartiles. The underlying raw data reveal spikes in activity in San Francisco and the control cities, which is larger in levels in San Francisco but similar in proportion. This could be attributable to seasonality effects, and/or the Golden State Warriors' (San Francisco's National Basketball Association team) finals participation and championship in June 2017. We confirmed robustness by estimating alternate event studies using Poisson models for availability and bookings and a log transformation of booking prices, which are available upon request.

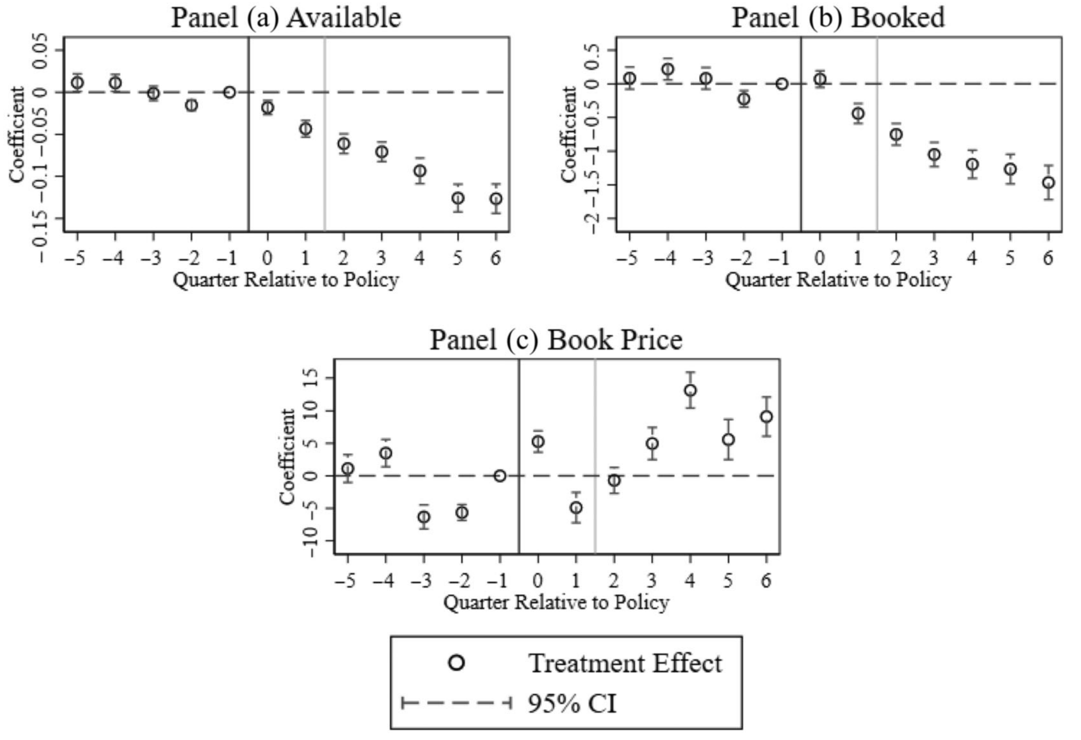


FIGURE 2 Effects of registration shocks on property-level outcomes.

Note: Quarterly differences in property-level availability, nights booked, and booking prices 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 property fixed effects and month-year fixed effects. The solid vertical lines refer to the date that the policy went into effect (September 2017). The lighter vertical line refers to the periods where Airbnb started enforcing the policy 4 months later. Hollow circles mark the quarter-specific treatment effects, that is, the time-disaggregated difference-in-differences (DiD) estimates. The dashed vertical lines are 95% confidence intervals using standard errors robust to clustering at the tract level.

by Airbnb during our sample period, we found that San Jose, Berkeley, and Sunnyvale may have had regulations enacted at some point during or near our sample period. In Table A.5, Panel B, we exclude those three cities from the analyses, and again find very similar estimates to those in Panel A of Table 2.

4.2 | Casual versus commercial Airbnb listings

In the previous subsection, we find that registration policy enforcement has a large negative effect on Airbnb market size, while increasing average booking prices. It is also important to separately examine the extent to which the policy affects commercial and casual listings. If it disproportionately affects commercial listings, then this registration policy likely achieves some of the intended consequences (i.e., to discourage/reduce Airbnb as a new form of commercial real estate investment). Otherwise, the registration policy would have the unintended consequence of hurting casual listings that are more likely to be hosted by owner-occupants or long-term renters hosting on Airbnb to help make ends meet.

TABLE 3 Effects of registration shocks on casual versus commercial Airbnb listings.

	Available	Nights booked	Booking price
Panel A: Main estimation sample			
High capacity × Entire × Treat × Post	−0.1012***	−1.7135***	5.7840**
	(0.0165)	(0.1728)	(2.8186)
	[0.39]	[2.93]	[248.22]
High capacity × Partial × Treat × Post	−0.1154***	−0.2639	−0.3614
	(0.0187)	(0.1784)	(1.5126)
	[0.40]	[2.62]	[102.77]
Low capacity × Treat × Post	−0.0687***	−0.8821***	6.0304***
	(0.0058)	(0.0834)	(0.9817)
	[0.26]	[2.89]	[166.11]
Panel B: Airbnb density quartile 4 only			
High capacity × Entire × Treat × Post	−0.0913***	−1.2608***	7.9491***
	(0.0261)	(0.2451)	(2.8160)
	[0.38]	[2.82]	[265.60]
High capacity × Partial × Treat × Post	−0.1195***	−0.1631	−0.3296
	(0.0269)	(0.2521)	(2.3882)
	[0.41]	[3.18]	[116.48]
Low capacity × Treat × Post	−0.0459***	−0.6569***	6.4865***
	(0.0079)	(0.1280)	(1.3587)
	[0.25]	[2.81]	[191.16]

Note: Panel A of this table presents estimated average effects of the enforced registration requirements on availability, nights booked, and booking prices across listings that are relatively commercial versus casual using the same property-level estimation sample as our main analyses in Panel A of Table 2. "High capacity" refers to a property being in the top 25% of all properties in terms of average nights available per month when the property is available (and "Low capacity" is the bottom 75%). "Entire home" is an indicator for whether the listing is for an entire housing unit, as opposed to a private/shared room. We consider high-capacity entire-home listings to be relatively commercial. Panel B does the same, except restricting the estimation sample to properties in the top Airbnb density quartile. All specifications include group-specific month-year fixed effects and property fixed effects. *Available* = number of properties that had any availability in a given tract-month, *Nights Booked* = number of nights booked per tract-month, and *Booking price* = average price per night booked weighted by number of nights booked. The sample contains an observation for every month for every property that was ever booked during our data set's original sample period (August 2014 through August 2019). For months in which no properties are listed, the outcome measures are zero by definition for availability and nights booked, and missing for booking price. Standard errors are in parentheses, and are robust to clustering at the tract level. Group-specific dependent variable means are presented in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $*\ p < 0.10$.

In Table 3, we present heterogeneity analyses that distinguish between relatively commercial and casual listings. Specifically, we calculate each listing's number of nights booked and nights that were available to be booked per month across the months in which the listing was available. Listings in the top quartile of this distribution are classified as "high capacity," and listings in the bottom three quartiles are classified as "low capacity." The 75th percentile of capacity is 28.3 nights available per month across available months.[15] We then interact this high-capacity indicator with whether the listing was for an entire housing unit (separating out owner- or renter-occupiers with a spare room or bed that is nearly always available) to arrive at our "commercial" designation.

[15] We probe the sensitivity of our results to this threshold in Panel A of Table A.6, designating the top 40% as high capacity rather than the top 25%, and find very similar estimates.

We use a listing-based rather than host-based definition (e.g., hosts with multiple simultaneous listings) in an effort to avoid misclassifications or changes in status that could result from hosts gaming existing and new regulatory policies (e.g., creating multiple host accounts to circumvent the preexisting "One Host, One Home" policy in San Francisco). Moreover, we view the interaction between "high capacity" and "entire home" as crucial, since neither indicator individually does a good job of distinguishing between relatively casual or commercial listings. For example, a host who lists their entire unit one or two weekends a month would likely be considered casual. Similarly, a host who lists a spare room in their home every day would also likely be considered casual.[16] We conduct these analyses at the property level, controlling for property and month-year fixed effects, and clustering standard errors at the tract level. In Table A.3, we present summary statistics stratified by high-capacity entire-home listings, high-capacity partial-home listings, and lower-capacity listings.

In Panel A of Table 3, among relatively commercial listings, we find a 10 percentage point reduction in the proportion available to rent in a given month, or 26% relative to the baseline mean of 39 percentage points, and a reduction of 6.9 percentage points (26%) among lower-capacity listings. Among commercial listings, we find a reduction of 1.7 nights booked per property-month (58%), while this number is only 0.88 among casual listings (31%). Finally, booking prices increase similarly in levels across commercial and lower-capacity listings ($5.78 and $6.03, respectively), but are larger in percentage terms among lower-capacity listings. Looking at high-capacity partial-home listings, we find similar reductions in availability as relatively commercial listings (0.12 percentage points, or 29%), though it appears as though this is entirely slack capacity, as this group experiences no statistically or economically significant change in nights booked or booking prices.

The comparison between casual and commercial listings in Panel A suggests that the enforcement policy reduces quantities by a relatively large amount among commercial listings. Given the results in Table 2 that suggest substantial heterogeneity by location based on Airbnb density, we examine whether the differential effects between lower-capacity and commercial listings are driven by location differences of listing types. To do this, we reestimate the heterogeneous effects including only the most Airbnb-dense quartile of tracts. We report these estimates in Panel B, where we find a very similar pattern of heterogeneity across all listing categories. That said, the availability and nights booked estimates for both types of listings range from 10% to 50% smaller in magnitude in quartile 4 compared to the full sample. This suggests that listings in more popular neighborhoods may have a slightly better ability to overcome the additional costs associated with registration requirements. However, this applies similarly across all listing categories, suggesting that the differential effects between listing types is not simply due to differences in geographic location.

Overall, using listing counts and baseline means from the city of San Francisco, we calculate that commercial listings experienced 5447 fewer nights booked per month (a 53% reduction relative to a baseline mean of 10,205) and a decline in monthly revenue of $1.45 million (52%).[17] This amounts to 26% of the total revenue lost by both commercial and lower-capacity listings in San Francisco, which is disproportionately large as commercial listings comprise just 12% of the combined set of commercial and lower-capacity listings. Meanwhile, relatively casual listings

[16] We include heterogeneous estimates by entire/partial home and high vs. low capacity separately in Panels B and C of Table A.6.

[17] For reference, the average nightly booking price for commercial listings in San Francisco is $272.11, average nights booked is 3.21, and the number of listings is 3179.

experience a decline in nights booked per month of 21,077 (33%) and a decline in monthly revenue of $4.2 million (31%).[18]

Finally, in Table A.7, we examine whether the aggregate composition of commercial listings changed as a result of the registration policy. We define the proportion as the number of available commercial listings (i.e., high capacity and entire home), defined in the same way as in the property-level analyses, divided by all available listings. We also stratify these estimates by quartiles. We find evidence that commercial listings comprise a smaller share of available listings in density quartile 4 (approximately 1.6 to 2.1 percentage points relative to a baseline mean of 13.2 percentage points) following registration policy enforcement. We also find a reduction in the proportion of commercial listings in quartile 3 (1.2 to 1.9 percentage points, relative to a baseline of 11.1 percentage points), though these estimates are not statistically significant at conventional levels.

4.3 | Long-term housing prices

To further assess whether the San Francisco registration policy likely achieved some of the intended consequences, we directly examine how the policy affected the long-term housing market. While we cannot observe quantities of home sales and long-term rentals, we can observe price indices using the ZHVI and the ZORI, which index long-term housing prices at the zip code level over time. Given that the policy shock induced exit from the Airbnb market, we may expect that some of that housing was reallocated back to the long-term housing market. It is also possible that demand falls, as potential income from renting one's property on Airbnb is reduced or eliminated by the policy. Both potential explanations would cause equilibrium home and long-term rental prices to fall (or grow less) in San Francisco relative to other cities in the metro area. Moreover, we would expect the price effects to be larger in the most Airbnb-dense zip codes where the shocks were largest. To estimate these housing price effects, we regress log housing prices on the policy indicator, month-year fixed effects, and zip code fixed effects, and use standard errors that are robust to clustering at the zip code level, similar to Equation (2).

We present our results in Table 4, where we find an overall reduction in home prices in San Francisco relative to control cities of 0.109 log points or roughly 11%. For reference, the median value of owner-occupied housing units in San Francisco was nearly $1.4 million between 2019 and 2023 (U.S. Census Bureau, 2025). Stratifying zip codes into Airbnb density quartile, we find home price reductions of 4%–5% in quartiles 1 and 2, 5%–6% in quartile 3, and 10%–14% in quartile 4. Turning to long-term rental prices, we find smaller negative effects of 1% across all quartiles and 1.3 to 1.6% in quartiles 3 and 4. For reference, approximately 61.5% of occupied housing units were renter-occupied at a median monthly rental price of $2419 between 2019 and 2023 in San Francisco (U.S. Census Bureau, 2025). A key limitation, however, is that the rental price index data are quite sparse, such that they should be taken with a grain of salt. It is also why we are unable to reliably estimate effects by Airbnb density quartile.[19]

[18] For reference, the average nightly booking price for casual listings in San Francisco is $211.34, average nights booked is 2.66, and the number of listings is 23,894.

[19] For example, we only observe rental price index data for one treated zip code in quartile 2, and all 6 months of its observations fall in the posttreatment period. Also, there are only 36 observations from five zip codes outside of San Francisco in quartile 4, all of which are in the posttreatment period.

TABLE 4 Effects of registration shocks on zillow indices of home values and long-term rental prices, zip code level.

	Ln(Zillow Home Value Index)			Ln(Zillow Long-Term Rental Price Index)		
	All quartiles			All quartiles	Q3 and Q4	Q3 and Q4
Treat × Post	−0.109***			−0.010***	−0.013***	−0.016***
	(0.012)			(0.004)	(0.004)	(0.004)
Treat × Post × Q1 and Q2		−0.043***	−0.054***			
		(0.007)	(0.007)			
Treat × Post × Q3		−0.058***	−0.049***			
		(0.011)	(0.018)			
Treat × Post × Q4		−0.140***	−0.104***			
		(0.010)	(0.017)			
Zip code FE	x	x	x	x	x	x
Month-year FE	x	x		x	x	
Quartile-month-year FE			x			x
Observations	3888	3888	3888	1393	777	777
N of zip codes	108	108	108	64	36	36

Note: Estimated effects of the registration policy in San Francisco on logged zip code by month home price and long-term rental price indices from Zillow using linear OLS regressions. In columns 2 and 3, the estimates are stratified by zip code-level Airbnb density quartiles, determined by the average per-capita number of available Airbnb listings in each zip code across the 15 pretreatment months. In columns 5 and 6, the sample is restricted to only zip codes in the top two Airbnb density quartiles. Coefficients can roughly be interpreted as percentage changes (e.g., −0.14 ≈ decline of 14%). The estimation sample includes the same 36-month window as our Airbnb analyses. Standard errors are in parentheses, and are robust to clustering at the zip code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $*\ p < 0.10$.

5 | CONCLUSION

We analyze the market impacts of a city-level Airbnb listing registration policy in San Francisco. We find that the policy, which was cooperatively enforced with help from Airbnb, reduced Airbnb listing availability by 20%–27%, the number of bookings by 22%–31%, and increased booking prices by roughly 3%. We calculate the aggregate reduction in Airbnb revenue among listings in San Francisco to be approximately $5.3 million per month.

We find that commercial listings were disproportionately impacted, experiencing larger reductions in supply and bookings than their more casual counterparts. In total, we calculate that enforcement of the registration policy led to a 53% reduction in revenue among commercial listings and a 31% reduction in revenue among relatively casual listings. Aggregate analyses of market composition suggest that the fraction of available listings considered to be relatively commercial declined by roughly 15% postenforcement. Our findings that commercial listings were more negatively impacted may be somewhat surprising at first, since we might generally expect that their hosts are in a better position to absorb the fixed costs associated with a registration requirement. However, in the case of San Francisco, the registration policy likely helped the city enforce several "anticommercial" regulations already on the books (e.g., residence requirements, the "One Host, One Home" policy, and more). Another (complementary) potential explanation is that hosts of commercial listings may have more outside options, like pursuing long-term rental contracts or selling the property, such that their threshold for exiting the short-term rental market is lower.

Further analyses reveal that reductions in availability and bookings were largest (in level terms) in the most Airbnb-dense areas of San Francisco, suggesting that either reallocation of housing back to long-term markets and/or reductions in demand for housing units was largest in those same areas. We present additional evidence to support this by examining home price and long-term rental price indices, where we estimate a 10%–14% reduction in home prices in the highest density quartile (relative to only a 4%–5% reduction in the lowest two density quartiles) and a 1.3%–1.6% reduction in rental prices in the highest two density quartiles.

Overall, our work provides evidence that registration requirements can substantially restrict the size of peer-to-peer short-term housing rental markets, while disproportionately driving out relatively commercial activity and potentially reallocating housing back toward long-term markets. That said, such policies may also impose the unintended consequence of substantially reducing revenue (i.e., income) received by hosts of relatively casual listings.

By documenting the impacts of this large policy shock in San Francisco, we highlight its value as a quasi-experiment that can be used to conduct further research on the social and economic effects of home sharing. A limitation of our study is that we cannot directly observe the extent to which Airbnb hosts and renters substituted toward different markets or platforms as a result of the policy shock, or whether there was an overall reduction in tourism to San Francisco. We leave these questions for future research.

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APPENDIX A

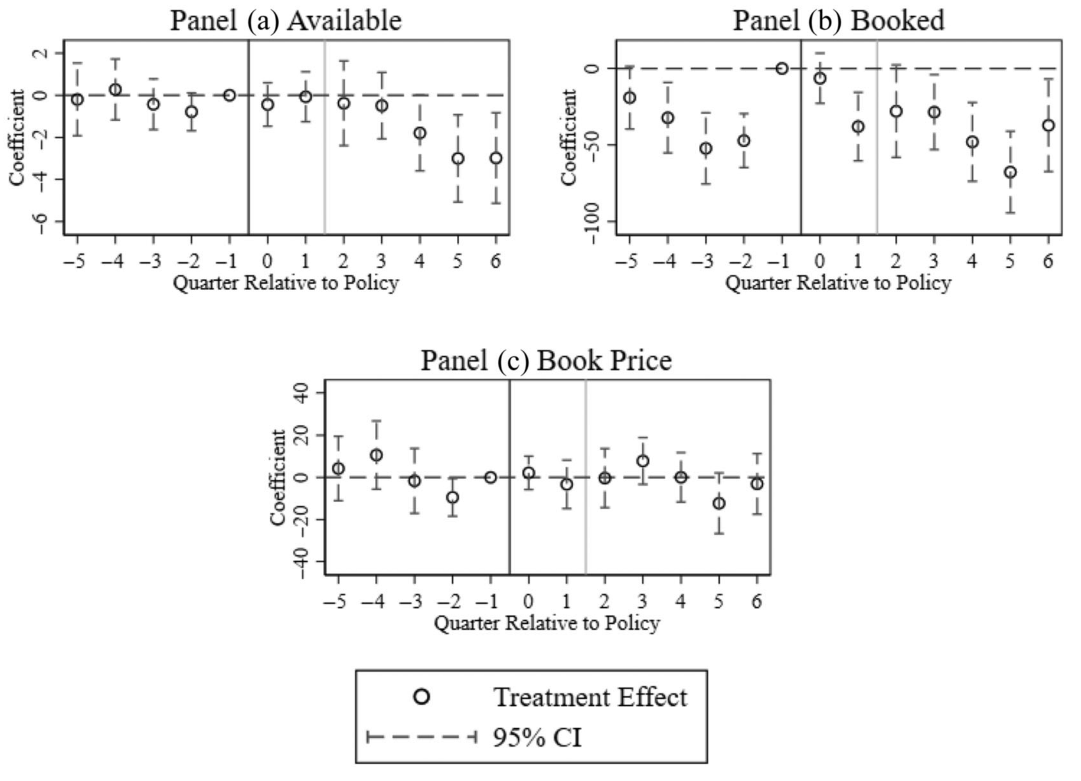

FIGURE A.1 Effects of registration shocks on tract-level outcomes, quartiles 1 & 2.

Note: Quarterly differences in availability, nights booked, and booking prices around the treatment date between treated and untreated tracts in quartiles 1 and 2 (0th–50th percentile) of the distribution of pretreatment Airbnb listings per 1000 tract population. The estimation sample includes all Airbnb listings that ever appear in our sample period. The specifications include tract fixed effects and month-year fixed effects. The solid vertical lines refer to the date that the policy went into effect (September 2017). 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, that is, the time-disaggregated difference-in-differences (DiD) estimates. The dashed vertical lines are 95% confidence intervals using standard errors that are robust to clustering at the tract level.

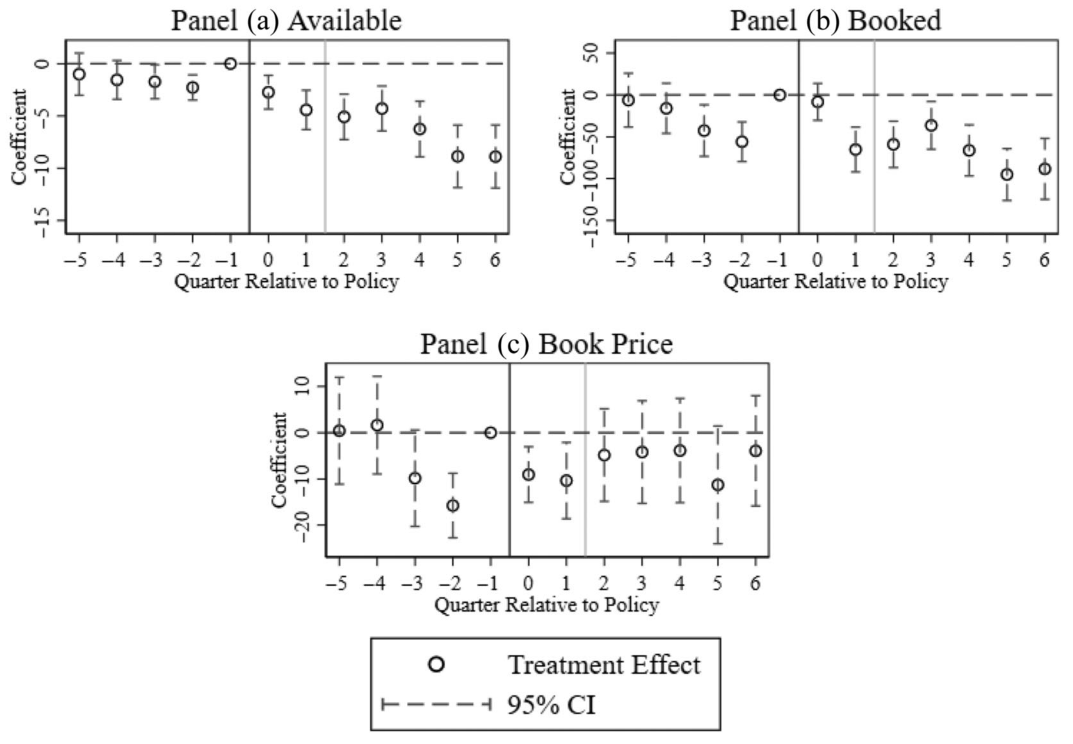

FIGURE A.2 Effects of registration shocks on tract-level outcomes, quartile 3.

Note: Quarterly differences in availability, nights booked, and booking prices around the treatment date between treated and untreated tracts in quartile 3 (50th–75th percentile) of the distribution of pretreatment Airbnb listings per 1000 tract population. The estimation sample includes all Airbnb listings that ever appear in our sample period. The specifications include tract fixed effects and month-year fixed effects. The solid vertical lines refer to the date that the policy went into effect (September 2017). 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, that is, the time-disaggregated difference-in-differences (DiD) estimates. The dashed vertical lines are 95% confidence intervals using standard errors that are robust to clustering at the tract level.

TABLE A.1 Summary of Airbnb data for each city in sample.

City	Total listing-Month obs.	Avg tract pop (2010 Census)	Pretreat listings Avail / 1000 (Tract)
Berkeley	156,096	3407	13.41
Fremont	48,528	4945	1.72
Mountain View	89,424	4055	9.75
Oakland	263,088	3445	7.18
Palo Alto	85,140	4215	9.59
San Francisco	1,087,416	4126	13.78
San Jose	259,344	4900	2.83
San Mateo	46,404	3988	4.01
Santa Clara	67,032	5158	5.21
Sunnyvale	83,484	5165	5.32

Note: Summary of Airbnb data by city for tracts included in our main estimation sample. Pretreatment listings available per 1000 population reflects the density of Airbnb listings in the 15 months leading up to policy enactment.

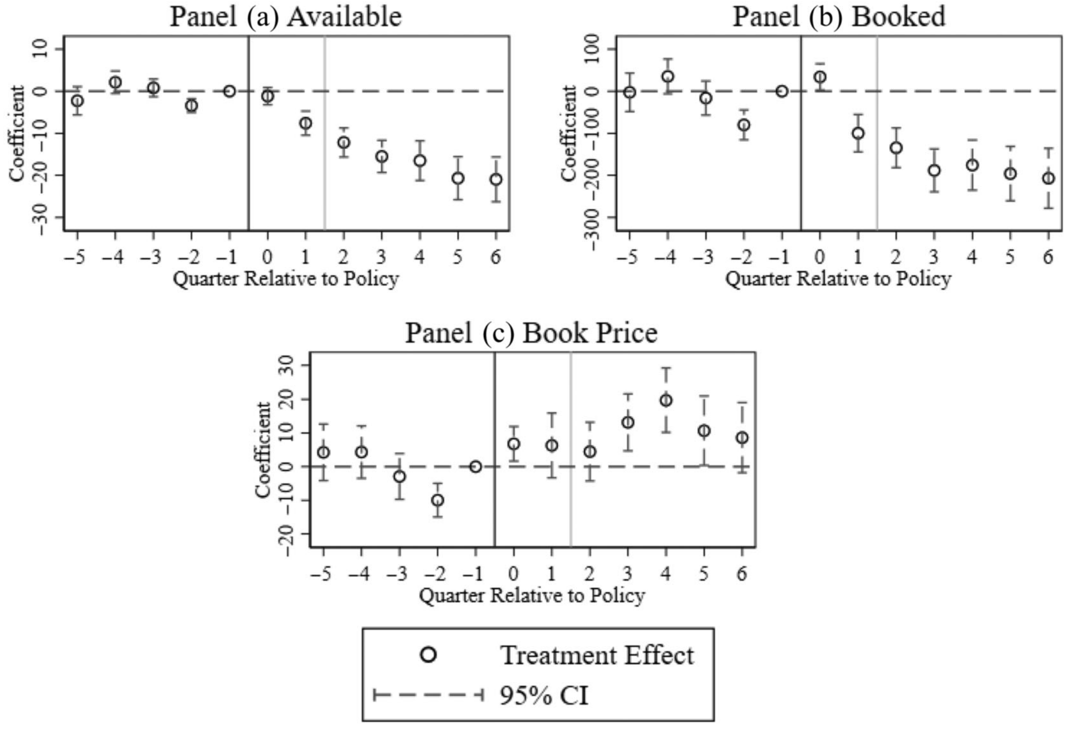

FIGURE A.3 Effects of registration shocks on tract-level outcomes, quartile 4.

Note: Quarterly differences in availability, nights booked, and booking prices around the treatment date between treated and untreated tracts in quartile 4 (75th–100th percentile) of the distribution of pretreatment Airbnb listings per 1000 tract population. The estimation sample includes all Airbnb listings that ever appear in our sample period. The specifications include tract fixed effects and month-year fixed effects. The solid vertical lines refer to the date that the policy went into effect (September 2017). 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, that is, the time-disaggregated difference-in-differences (DiD) estimates. The dashed vertical lines are 95% confidence intervals using standard errors that are robust to clustering at the tract level.

TABLE A.2 Summary statistics by density quartile and treatment status.

	Available	Capacity (Nights/Month)	Nights booked	Booking price	Entire home	High capacity	Bedrooms	Max guests	Superhost	Number of listing-months
Quartiles 1 and 2										
All other cities	0.303	7.64	2.61	106.49	0.346	0.229	1.40	2.98	0.202	327,420
	(0.459)	(12.49)	(6.98)	(105.88)	(0.476)	(0.42)	(1.3)	(2.48)	(0.402)	
San Francisco City limits	0.291	7.52	3.48	144.26	0.416	0.238	1.34	3.14	0.124	57,060
	(0.454)	(12.51)	(8.24)	(109.11)	(0.493)	(0.426)	(0.91)	(2.21)	(0.329)	
Quartile 3										
All other cities	0.311	7.69	2.80	134.35	0.477	0.206	1.37	3.02	0.178	341,964
	(0.463)	(12.45)	(7.14)	(127.72)	(0.499)	(0.404)	(1.35)	(2.26)	(0.382)	
San Francisco City limits	0.295	7.51	3.34	171.91	0.497	0.218	1.40	3.24	0.133	198,072
	(0.456)	(12.46)	(8.10)	(132.87)	(0.500)	(0.413)	(1.34)	(2.29)	(0.34)	
Quartile 4										
All other cities	0.313	7.64	3.20	138.62	0.524	0.182	1.33	2.98	0.165	429,228
	(0.464)	(12.38)	(7.67)	(110.65)	(0.499)	(0.386)	(1.14)	(1.99)	(0.371)	
San Francisco City limits	0.265	6.56	2.66	222.10	0.594	0.205	1.29	3.06	0.119	832,140
	(0.441)	(11.86)	(7.22)	(193.35)	(0.491)	(0.403)	(1.47)	(2.04)	(0.323)	

Note: Means and standard deviations at the property-month level, stratified by treatment status and Airbnb density quartile. Density quartiles are determined by the average per-capita number of available Airbnb listings in each tract across the 15 pretreatment months. *Available* = binary variable indicating whether the property had any availability during the month. *Capacity* = number of nights available or booked per month. *Nights booked* = number of nights booked in a given month. *Booking price* is the average posted nightly price on nights that have been booked, weighted by number of bookings. *Entire home* is an indicator for whether a listing is for an entire unit as opposed to private/shared room. *High capacity* refers to a property being in the top 25% of all properties in terms of average nights available per month when the property is available. *Bedrooms* = number of bedrooms in the unit. *Max guests* = maximum number of guests allowed in the rental. *Superhost* is defined by Airbnb as a host who provides excellent hospitality, based on reviews, responsiveness, and avoiding cancellations. The sample contains monthly observations for every property that was ever booked during our data set's original sample period (August 2014 through August 2019). For months in which a property is not listed or available, the outcome measures equal zero by definition.

TABLE A.3 Summary statistics by commercial/casual status and treatment status.

	Available	Capacity (Nights/Month)	Nights booked	Booking price	Entire home	Bedrooms	Max guests	Superhost	Number of listing-months
High capacity × Entire home									
All other cities	0.390	11.56	2.53	205.92	—	1.89	4.71	0.129	80,820
	(0.488)	(14.59)	(7.08)	(167.16)	—	(2.20)	(2.96)	(0.335)	
San Francisco City limits	0.393	11.64	3.21	272.11	—	1.65	4.31	0.083	114,444
	(0.488)	(14.59)	(7.99)	(195.07)	—	(2.16)	(2.76)	(0.276)	
High capacity									
All other cities	0.393	11.66	2.01	131.56	0.362	1.34	2.95	0.113	223,380
	(0.488)	(14.62)	(6.31)	(135.82)	(0.481)	(1.54)	(2.49)	(0.317)	
San Francisco City limits	0.399	11.83	3.48	191.62	0.504	1.33	3.26	0.093	227,088
	(0.49)	(14.64)	(8.34)	(160.89)	(0.500)	(1.58)	(2.47)	(0.29)	
Lower capacity									
All other cities	0.288	6.63	3.13	128.2571	0.480	1.37	3.00	0.198	875,304
	(0.453)	(11.59)	(7.53)	111.9914	(0.500)	(1.17)	(2.16)	(0.398)	
San Francisco City limits	0.238	5.45	2.66	211.3362	0.584	1.31	3.06	0.130	860,184
	(0.426)	(10.84)	(7.19)	185.065	(0.493)	(1.38)	(1.98)	(0.336)	

Note: Means and standard deviations at the property-month level, stratified by commercial/casual status and Airbnb density quartile. Density quartiles are determined by the average per-capita number of available Airbnb listings in each tract across the 15 pretreatment months. "High capacity" refers to a property being in the top 25% of all properties in terms of average nights available per month when the property is available (and "Lower capacity" is the bottom 75%). "Entire home" is an indicator for whether the listing is for an entire housing unit, as opposed to a private/shared room. We consider high-capacity entire-home listings to be relatively commercial. *Available* = binary variable indicating whether the property had any availability during the month. *Capacity* = number of nights available or booked per month. *Nights booked* = number of nights booked in a given month. *Booking price* is the average posted nightly price on nights that have been booked, weighted by number of bookings. *Bedrooms* = number of bedrooms in the unit. *Max guests* = maximum number of guests allowed in the rental. *Superhost* is defined by Airbnb as a host who provides excellent hospitality, based on reviews, responsiveness, and avoiding cancellations. The sample contains monthly observations for every property that was ever booked during our data set's original sample period (August 2014 through August 2019). For months in which a property is not listed or available, the outcome measures equal zero by definition.

TABLE A.4 Nonlinear estimates of effects of registration shocks on market outcomes, property-level.

	Available (Probit)		Nights booked (Poisson)	
Treat × Post	−0.2293***	−0.1726***	−0.3200***	−0.2355***
	(0.0175)	(0.0187)	(0.0282)	(0.0288)
Marginal effects	−0.078	−0.059	−1.20	−0.88
	[0.29]	[0.29]	[3.75]	[3.75]
Property FE	x	x	x	x
Month-year FE	x		x	
Quartile-month-year FE		x		x
Observations	2,185,956	2,185,884	1,670,292	1,670,256
N of tracts	729	728	724	723

Note: Estimated effects of policy on availability and nights booked using probit estimation to measure availability effects and Poisson estimation to measure effects on nights booked. For the probit regressions, we use the approach proposed by Wooldridge (2010) in Section 15.8.2, where we include time dummies, the usual binary treatment variable, and the (property-level) demeaned treatment variable. *Available* = dummy variable indicating whether the property had any availability in a given month, and *Nights booked* = number of nights booked per property-month. The estimation sample contains an observation for every month for every property that was ever booked during our data set's original sample period (August 2014 through August 2019). For months in which a property is not listed, the outcome measures are zero by definition for availability and nights booked. Standard errors are in parentheses, and are robust to clustering at the tract level. Dependent variable means are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $* \ p < 0.10$.

TABLE A.5 Effects of registration shocks on market outcomes, property-level, robustness checks.

	Available		Nights booked		Booking price	
Panel A: Excluding three geographically-closest cities to address potential spillovers (Oakland, Berkeley, San Mateo)						
Treat × Post	−0.0980***	−0.0788***	−1.1021***	−0.8406***	6.9669***	6.6154***
	(0.0068)	(0.0078)	(0.0810)	(0.0944)	(0.9734)	(1.1157)
	[0.29]	[0.29]	[2.78]	[2.78]	[180.54]	[180.54]
Observations	1,720,368	1,720,296	1,720,368	1,720,296	285,701	285,692
N of tracts	548	547	548	547	542	541
Panel B: Excluding cities with potentially-confounding regulations (San Jose, Berkeley, Sunnyvale)						
Treat × Post	−0.0840***	−0.0693***	−1.0505***	−0.8520***	5.3323***	5.0238***
	(0.0063)	(0.0068)	(0.0857)	(0.0918)	(0.9560)	(0.9660)
	[0.29]	[0.29]	[2.89]	[2.89]	[179.35]	[179.35]
Property FE	x	x	x	x	x	x
Month-year FE	x		x		x	
Quartile-month-year FE		x		x		x
Observations	1,687,032	1,686,960	1,687,032	1,686,960	287,407	287,398
N of tracts	458	457	458	457	450	449

Note: Estimated effects of policy on availability, nights booked, and booking prices using linear OLS regressions. *Available* = dummy variable indicating whether the property had any availability in a given month, *Nights booked* = number of nights booked per property-month, and *Booking price* = average price per night booked weighted by number of nights booked. The estimation sample contains an observation for every month for every property that was ever booked during our data set's original sample period (August 2014 through August 2019). For months in which a property is not listed, the outcome measures are zero by definition for availability and nights booked, and missing for booking price. Standard errors are in parentheses, and are robust to clustering at the tract level. Dependent variable means are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $* \ p < 0.10$.

TABLE A.6 Effects of registration shocks on casual versus commercial Airbnb listings, additional analyses.

	Available	Nights booked	Booking price
Panel A: Entire home × High capacity (based on 60th percentile cutoff)			
High capacity × Entire × Treat × Post	−0.1286**	−2.0896**	6.0817**
	(0.0132)	(0.1601)	(1.9662)
	[0.42]	[4.08]	[238.66]
High capacity × Partial × Treat × Post	−0.0966**	−0.1813	1.7680
	(0.0179)	(0.2040)	(1.1104)
	[0.42]	[3.42]	[100.92]
Low capacity × Treat × Post	−0.0635**	−0.8319**	6.0554**
	(0.0054)	(0.0755)	(1.1405)
	[0.23]	[2.45]	[166.03]
Panel B: High vs. Low capacity (standard, based on 75th percentile cutoff)			
High capacity × Treat × Post	−0.1105**	−0.9354**	2.1565
	(0.0124)	(0.1227)	(1.6335)
	[0.40]	[2.75]	[169.94]
Low capacity × Treat × Post	−0.0685**	−0.8875**	6.2470**
	(0.0058)	(0.0827)	(0.9946)
	[0.26]	[2.89]	[166.11]
Panel C: Entire vs. Partial home			
Entire home × Treat × Post	−0.0781**	−1.2444**	8.4127**
	(0.0067)	(0.0902)	(1.2798)
	[0.29]	[2.98]	[230.42]
Partial home × Treat × Post	−0.0809**	−0.5356**	1.9050**
	(0.0098)	(0.1149)	(0.8226)
	[0.30]	[2.75]	[94.76]

Note: Panel A revisits our main casual versus commercial estimates from Panel A of Table 3, checking whether our findings are robust to using the top 40% rather than top 25% to determine whether a listing is "High capacity." Panel B of this table presents estimated average effects of the enforced registration requirements on availability, nights booked, and booking prices across listings that are relatively high versus low capacity. "High capacity" refers to a property being in the top 25% of all properties in terms of average nights available per month when the property is available (and "Low capacity" is the bottom 75%). Panel C examines heterogeneity across entire home versus partial home listings. "Entire home" is an indicator for whether the listing is for an entire housing unit, as opposed to a private/shared room. All analyses use the same property-level estimation sample as our main analyses in Panel A of Table 2 and Panel A of Table 3. All specifications include group-specific month-year fixed effects and property fixed effects. *Available* = number of properties that had any availability in a given tract-month, *Nights booked* = number of nights booked per tract-month, and *Booking price* = average price per night booked weighted by number of nights booked. The sample contains an observation for every month for every property that was ever booked during our data set's original sample period (August 2014 through August 2019). For months in which no properties are listed, the outcome measures are zero by definition for availability and nights booked, and missing for booking price. Standard errors are in parentheses, and are robust to clustering at the tract level. Group-specific dependent variable means are presented in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $* \ p < 0.10$.

TABLE A.7 Effects of registration shocks on fraction of available commercial Airbnb listings.

	Avail. high cap. Entire-home/ All available listings	
Treat × Post × Q1 and Q2	0.0070	0.0112
	(0.0235)	(0.0242)
	[0.091]	[0.091]
Treat × Post × Q3	−0.0115	−0.0186
	(0.0141)	(0.0144)
	[0.111]	[0.111]
Treat × Post × Q4	−0.0157**	−0.0210**
	(0.0077)	(0.0077)
	[0.132]	[0.132]
Tract FE	x	x
Month-year FE	x	
Quartile-month-year FE		x
Observations	25,316	25,282
N of tracts	723	722

Note: Estimated effects of the San Francisco registration policy on the tract-month-year fraction of available relatively commercial listings using linear OLS regressions. The outcome is calculated as the number of available listings in a given month classified as commercial (i.e., high capacity, entire-home) divided by the number of all available listings in that tract-month-year. "High capacity" refers to a property being in the top 25% of all properties in terms of average nights available per month when the property is available (and "Lower capacity" is the bottom 75%). "Entire home" is an indicator for whether the listing is for an entire housing unit, as opposed to a private/shared room. We consider high-capacity entire-home listings to be relatively commercial. Estimates are stratified by tract-level Airbnb density quartiles, determined by the average per-capita number of available Airbnb listings in each tract across the 15 pretreatment months. Standard errors are in parentheses, and are robust to clustering at the tract level. Group-specific dependent variable means are presented in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $*\ p < 0.10$.