

# 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, which was cooperatively enforced by Airbnb, on the supply of listings, nights booked, and booking prices. We find that the policy dramatically reduced Airbnb availability by 20 to 27%, bookings by 22 to 31%, and increased booking prices by roughly 3% relative to properties in untreated cities in the surrounding area. Aggregate estimates at the census tract level reveal similar results. Moreover, there is substantial heterogeneity. The most Airbnb-dense neighborhoods experience the largest reductions in supply and bookings and the largest increases in booking prices. Our heterogeneity analyses reveal that commercial hosts experience larger declines in supply and bookings and smaller increases in booking prices than relatively casual hosts. In aggregate, the enforcement of San Francisco's registration policy reduced nights booked by just over 27,000 per month and a reduction in hosts' revenue by roughly \$5.3 million per month.

**Keywords:** Airbnb, short-term housing rentals, sharing economy, registration enforcement, platform regulation

**JEL Classifications:** R31, R38, R52, Z38, L51, L83

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# 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.<sup>1</sup> Such platforms, like Airbnb, have dramatically expanded the availability of housing accommodations for travelers (Farronato and Fradkin, 2018, Li and Srinivasan, 2019, Zervas et al., 2017). This in turn affects local economic activity (Basuroy et al., 2020), amenities (Almagro and Domínguez-Iino, 2022), 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 and 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.<sup>2</sup> Indeed, a growing body of literature shows that Airbnb penetration increases long-term housing prices.<sup>3</sup> 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., 2021). Similarly, the so-called “One Host, One Home” policy has been adopted by several cities across the U.S. 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 paper, we study perhaps the largest policy-driven Airbnb shock to date and its

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<sup>1</sup>Ridesharing platforms such as Uber and Lyft have also had large impacts, affecting labor markets (Chen et al., 2019, Berger et al., 2018), transit and congestion (Agrawal and Zhao, 2023, Hall et al., 2018, Tarduno, 2021), alcohol consumption (Teltser et al., 2021), drunk driving and traffic fatalities (Anderson and Davis, 2021, Barrios et al., 2020, Brazil and Kirk, 2016, Dills and Mulholland, 2018, Greenwood and Wattal, 2017, Zhou, 2020), and more.

<sup>2</sup>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 (Khoury, 2019).

<sup>3</sup>See, for example, Barron et al. (2021), Chen et al. (2022), Duso et al. (2020), García-López et al. (2020), García et al. (2020), Horn and Merante (2017), Koster et al. (2021).

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, registration fees (\$450 every two years), reduced ability to evade applicable federal, state, and local taxes, and reduced ability to skirt San Francisco’s existing “One Host, One Home” policy. They may also facilitate enforcement of restrictions on the number of units or nights available, zoning restrictions, and additional regulatory burden and oversight.<sup>4</sup> Airbnb assisted San Francisco with enforcing the registration policy by removing listings that remained unregistered. In January 2018, four months following initial implementation, Airbnb removed almost 5,000 unregistered listings (nearly 50%).<sup>5</sup> In contrast, 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 markets, 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. To address this, we estimate not only average effects across the entire city, but also heterogeneity in these effects across relatively casual versus commercial hosts and across neighborhoods of varying Airbnb popularity. We exploit three dimensions of variation to obtain credible causal estimates. The first is temporal

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<sup>4</sup>See Airbnb (2022) for further examples.

<sup>5</sup>See the news article by Said (2018), and also research from Rossi (2023), who leverages this large shock to study the relationships between competition, reputation, and Airbnb host effort.

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, we exploit variation in treatment intensity as measured by pre-treatment 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 treated listing in the San Francisco city limits is available in any given month declines by roughly 6 to 8 percentage points (20 to 27%) and the number of nights booked per listing-month declines by 0.6 to 0.9 (22 to 31%) compared to untreated listings in the broader San Francisco metro area. The 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 for this and find that the average nightly booking price increases by a little over \$5, or roughly 3%. Using the listing count and baseline averages for the city of San Francisco, along with our preferred estimates, we find aggregate nights booked declined by 27,182 per month and hosts' monthly revenue fell by \$5.29 million.

To the extent that an overarching policy goal is to reallocate housing back to the long-term market in the neighborhoods where Airbnb is most popular, it is also useful to examine whether the registration policy's effects are largest in those neighborhoods. To address this, we aggregate to the Census tract level and present heterogeneity estimates across tracts with varying levels of pre-treatment Airbnb density (i.e., number of available listings per 1,000 population). Indeed, we find the largest effects in quartile 4; 13 to 13.3 fewer available listings (roughly 24%), 106 to 125 fewer nights booked (19 to 23%), and an increase in average booking prices by \$8.62 to \$10.66 per night (4.5 to 5.6%). In quartile 3, we find

a decrease of roughly 4.4 available listings per tract-month (17.5%), a decrease of 25.6 to 35.6 nights booked per tract-month (10 to 14%), and no statistically significant impact on booking prices. We find no statistically or economically meaningful effects among tracts in the lowest two density quartiles.

In both our property-level and tract-level analyses, we find that (a) the effects from registration enforcement persist over time and (b) the timing of the effects suggests successful regulation of Airbnb relies on cooperation with the platform (consistent with Bibler et al., 2021). These results are robust to alternate specification choices, and event studies demonstrate little to no differential pre-trends.

Distinguishing between the impacts on casual versus commercial hosts and listings, we find that relatively commercial hosts (i.e., the top quartile of listings based on their hosts' number of pre-treatment available listings) are more negatively impacted by registration enforcement. Relatively commercial hosts' monthly proportion of available listings declines by 14 percentage points (32%), while among casual hosts the reduction was only 5.9 percentage points (24%). Listings from relatively commercial hosts are also booked 1.7 fewer nights per month (40%), while this number is only 0.66 among casual hosts' listings (27.5%). Finally, booking prices for commercial hosts' listings increase by only \$3.85 per night (2.5%) while relatively casual hosts experience an increase of \$6.55 (3.8%).

In aggregate, we find that commercial hosts experienced 11,923 fewer nights booked per month and a decline in monthly revenue of \$2.15 million. Meanwhile, casual hosts saw 15,355 fewer nights booked per month and a \$3.07 million decline in monthly revenue. Since we define relatively commercial listings as those in the top quartile based on their hosts' pre-treatment number of available listings, their share of the overall lost revenue is disproportionately large (41%). Moreover, lost revenue among commercial listings was 39% relative to their baseline, while revenue lost by relatively casual listings was only 25%. In theory, we would expect commercial hosts to be *less* affected by the additional fixed costs imposed by the registration requirement. Thus, our pattern of results suggests that the

“bite” of the cooperatively-enforced registration policy came from helping the city enforce existing regulations such as the “One Host One Home” policy (Chen et al., 2022). That said, 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.

Our findings contribute to the existing literature in several ways. In particular, we provide evidence that registration requirements can substantially restrict the size of peer-to-peer short-term housing rental markets (Gauß et al., 2022, Hübscher and Kallert, 2023, Müller et al., 2022, Valentin, 2021). As in the case of San Francisco, this seems particularly true when they are implemented alongside other existing regulations, and they seem to disproportionately impact relatively commercial hosts. Our work also contributes to the growing body of evidence that cooperation between the government and the platform helps enforce existing taxes and regulations more effectively (Bibler et al., 2021, Garz and Schneider, 2023a,b). We also contribute to the broader Airbnb literature, including work that models and estimates the role of Airbnb in home ownership decisions, spillover costs and benefits, housing market matching frictions, and optimal policy-setting (Filippas et al., 2020, Filippas and Horton, 2017, Farhoodi et al., 2021, Garcia et al., 2020). 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.

## 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 character-

istics 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 (June 2016) through the 21 months following policy enactment (i.e., 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).<sup>6</sup>

We then restrict to the ten largest cities in the metro, as measured by the total number of Airbnb listings.<sup>7</sup> Restricting in this way is useful because the San Francisco area has relatively high levels of Airbnb activity, so using the largest Airbnb markets within that metro gives us the best set of comparable controls. The list of included cities along with summary statistics for those cities are reported in Appendix Table A1.

Next, we use reported coordinates to assign listings into Census tracts. For each tract, we calculate a measure of Airbnb density equal to the average number of Airbnb listings per 1,000 population during the 15 months preceding the first policy enactment.<sup>8</sup> We use this measure to assign tracts to quartiles based on their pre-treatment Airbnb density, and then estimate heterogeneous treatment effects by quartile.

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

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<sup>6</sup>This is in contrast to papers that use administrative data from Airbnb, such as Jaffe et al. (2018) and Farronato and Fradkin (2018).

<sup>7</sup>These cities include Berkeley, Fremont, Mountain View, Oakland, Palo Alto, San Francisco, San Jose, San Mateo, Santa Clara, and Sunnyvale.

<sup>8</sup>For reference, we use 2010 tract-level Census population counts.

that a property has been reserved.<sup>9</sup> 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 dataset’s original 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 properties in the sample (0.29), as well as only those within the San Francisco city limits (0.27), implying roughly 29% of all the listings in our balanced panel were available to be booked at least one day in any given month. The second row presents nights booked per property-month, which averages 2.87 in the full sample and 2.83 among listings in 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 full metro (\$166.82). 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 Appendix Table A2, we further summarize our Airbnb data by quartiles of tract-level

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<sup>9</sup>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 over-measure the number of nights booked, when a host decides to block out a previously-available 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 we underestimate the true negative shock to bookings.



Airbnb density. These panels provide insight on Airbnb market outcomes across areas of varying Airbnb popularity. Again, we see comparable availability and number of nights booked across properties in San Francisco and the full sample, while booking prices tend to be higher in the city limits than in the rest of the metro across all quartiles. While booking prices are higher in the more popular Airbnb tracts, availability rates and nights booked are very similar across the 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 the policies on the size of the Airbnb market at the city level, we use a standard differences-in-differences (DiD) estimator. We then test whether the effects are stronger in areas with a greater density of Airbnb listings at the tract level, and how the effects vary across types of Airbnb hosts.

The following is our core differences-in-differences 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  $\gamma$ , which measures the change in the average difference in  $Y$  between treated and control units before and after treatment. Finally,  $\eta_i$  are property-level fixed effects to control for time-invariant differences across listings,  $\delta_t$  are month-year fixed effects to control for idiosyncratic time shocks (e.g., demand shocks or seasonal effects), and  $\mu_{ijt}$  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.

Next, we go beyond our core DiD approach to examine heterogeneity in tract-level treatment effects by Airbnb market density (i.e., number of pre-treatment Airbnb listings per 1,000 tract residents), allowing us to not only exploit variation in treatment across time and place, but also variation among treated tracts that may be more/less impacted by the city-level policy implementation. We test for differential effects using the following interacted specification:

$$Y_{jt} = \sum_k \gamma_k Reg_{jt} + \eta_j + \delta_t + \mu_{jt} \quad (2)$$

where  $k$  indexes the Airbnb density group, which includes Quartiles 1 and 2 combined, Quartile 3, and Quartile 4 of the Airbnb density distribution.<sup>10</sup>

Note that in the results section to follow (Section 4), 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 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.

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

Here,  $D_j$  is an indicator for whether tract  $j$  is ever-treated, which is interacted with indicators for 5 quarters (indexed by  $q$ ) leading up to the quarter during which treatment occurs ( $Q_j$ ) as well as the 7 post-treatment quarters (0, 1, ..., 6). 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. In addition to the property-level version of the event studies, we also estimate and present tract-level event studies by quartile group.

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<sup>10</sup>Recall, this is calculated as the average monthly number of Airbnb units per 1,000 tract residents in the 15 months prior to the first policy enactment. 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.

## 4 Results

### 4.1 Main Estimates

To examine the effect of the registration requirement policy shock in San Francisco on the Airbnb market, we estimate the differences-in-differences 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 also 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.

The availability estimates range from a 5.9 to 7.8 percentage point reduction in the likelihood 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 shocks, which is 22 to 31% relative to the baseline average of 2.87 nights booked per property-month. 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. Finally, we examine the extent to which this negative supply shock increased booking prices. There, we find an increase in nightly booking price of \$5.13 to \$5.46, or a roughly 3% increase relative to the baseline mean of \$166.86. Using the 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 \$5.29 million.<sup>11</sup>

Next, in Panel B of Table 2, we present tract-level quartile-specific estimates using the same two specifications for each outcome of interest. In Airbnb density quartiles 1 and 2, we find very 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 to 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 to 23%), and an increase in average booking prices by \$8.62 to \$10.66 per night (4.5 to 5.6%).

To probe the parallel trends assumption required for our differences-in-differences estimators to yield an unbiased causal parameter, we estimate several event study specifications. In Figure 1 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 pre-trends between property listings in treated versus untreated tracts leading up to the policy enactments 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 magnitude 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 Appendix Figures A1, A2, and A3, where we again find post-treatment 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 pre-trends.

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<sup>11</sup>For reference, average nights booked per property-month is 2.83, there are 30,202 listings, and nightly booking price averages \$206.27.

## 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 hosts. If it disproportionately affects commercial hosts, 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 hosts who are more likely to be owner-occupants or long-term renters hosting on Airbnb to help make ends meet.

In Table 3, we present heterogeneity analyses that distinguish between listings belonging to relatively commercial and casual hosts. Specifically, using host identifiers, we calculate how many available listing-months each host had during our 15-month pre-treatment period. Listings in the top quartile of this distribution are classified as belonging to “high-listing hosts,” and listings in the bottom 3 quartiles are classified as belonging to “low-listing hosts.” The 75th percentile is 20 property-months, implying a cutoff of  $20/15 = 1.33$  listings available for rent throughout the pre-treatment period. 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 Panel A, among properties managed by high listing (i.e., commercial) hosts, we find a 14 percentage point reduction in the proportion available to rent in a given month, or 32% relative to the baseline mean of 44 percentage points. In contrast, among casual listings, we find a reduction of 5.9 percentage points (24%). Among commercial listings, we find a reduction of 1.7 nights booked per property-month (40%), while this number is only 0.66 among casual listings (27.5%). Finally, even though commercial listings exhibit larger reductions in supply, their booking prices increase by only \$3.85 per night (2.5%) while the increase among casual listings is \$6.55 (3.8%).

The comparison between casual and commercial hosts in Panel A suggests that the en-

forcement policy reduces quantities by a relatively large amount among commercial hosts, while leading to relatively small price increases. Given the results in Table 2 that suggest substantial heterogeneity by location based on Airbnb density, we examine whether the differential effects between casual and commercial hosts are driven by location differences of host types. To do this, we re-estimate the heterogeneous effects between casual and commercial hosts 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 commercial versus casual listings. Moreover, the booking price estimates in Panels A and B are nearly identical, while the availability and nights booked estimates for both types of listings are roughly 20-25% 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 absorb the additional costs associated with registration requirements. However, this applies similarly to both commercial and casual hosts, suggesting that the differential effects between host 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 11,923 fewer nights booked per month and a decline in monthly revenue of \$2.15 million (39%).<sup>12</sup> This amounts to 41% of the total lost revenue in San Francisco, which is disproportionately large considering we define commercial listings as those in the top 25% by hosts' pre-treatment count of available listings. Meanwhile, relatively casual hosts experience a decline in nights booked per month of 15,355 and a decline in monthly revenue of \$3.07 million (25%).<sup>13</sup>

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<sup>12</sup>For reference, the average nightly booking price for commercial hosts in San Francisco is \$186.27, average nights booked is 4.24, and the number of listings is 7,026.

<sup>13</sup>For reference, the average nightly booking price for casual hosts in San Francisco is \$216.97, average nights booked is 2.4, and the number of listings is 23,160.

## 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 to 27%, the number of bookings by 22 to 31%, and increased booking prices by roughly 3%. Moreover, our findings suggest that commercial listings were disproportionately impacted, experiencing larger reductions in supply, bookings, and smaller increases in booking price relative to their relatively causal counterparts.

Moreover, unsurprisingly, we find substantial heterogeneity in aggregate-level effects across areas with more vs. less pre-existing Airbnb density. For example, in the top quartile of tracts by Airbnb density, we find that supply fell by 24%, bookings fell by 19 to 23%, and prices increased by roughly 5%. In the lowest two density quartiles, we find no statistically or economically meaningful tract-level changes in listings, nights booked, nor booking prices. We calculate the aggregate reduction in Airbnb revenue among listings in San Francisco to be approximately \$5.3 million per month, with a disproportionate 41% of the loss experienced by the top 25% of listings based on their hosts' pre-treatment number of available listings. Moreover, relative to baseline, commercial listings experienced a 39% reduction in revenue while relatively casual listings experienced a more modest 25% decline.

At first, our findings that commercial hosts are more negatively impacted may appear somewhat counterintuitive, since commercial hosts are in a better position to absorb the fixed costs associated with registering. Particularly in the San Francisco context, though, our pattern of results suggests the registration policy likely helped the city enforce regulations already on the books such as the “One Host One Home” policy.

Overall, our work provides evidence that registration requirements can substantially restrict the size of peer-to-peer short term housing rental markets. By documenting the impacts of this large policy shock in San Francisco, we also highlight its value as a quasi-experiment that can be used to conduct further research on the social and economic effects of Airbnb.

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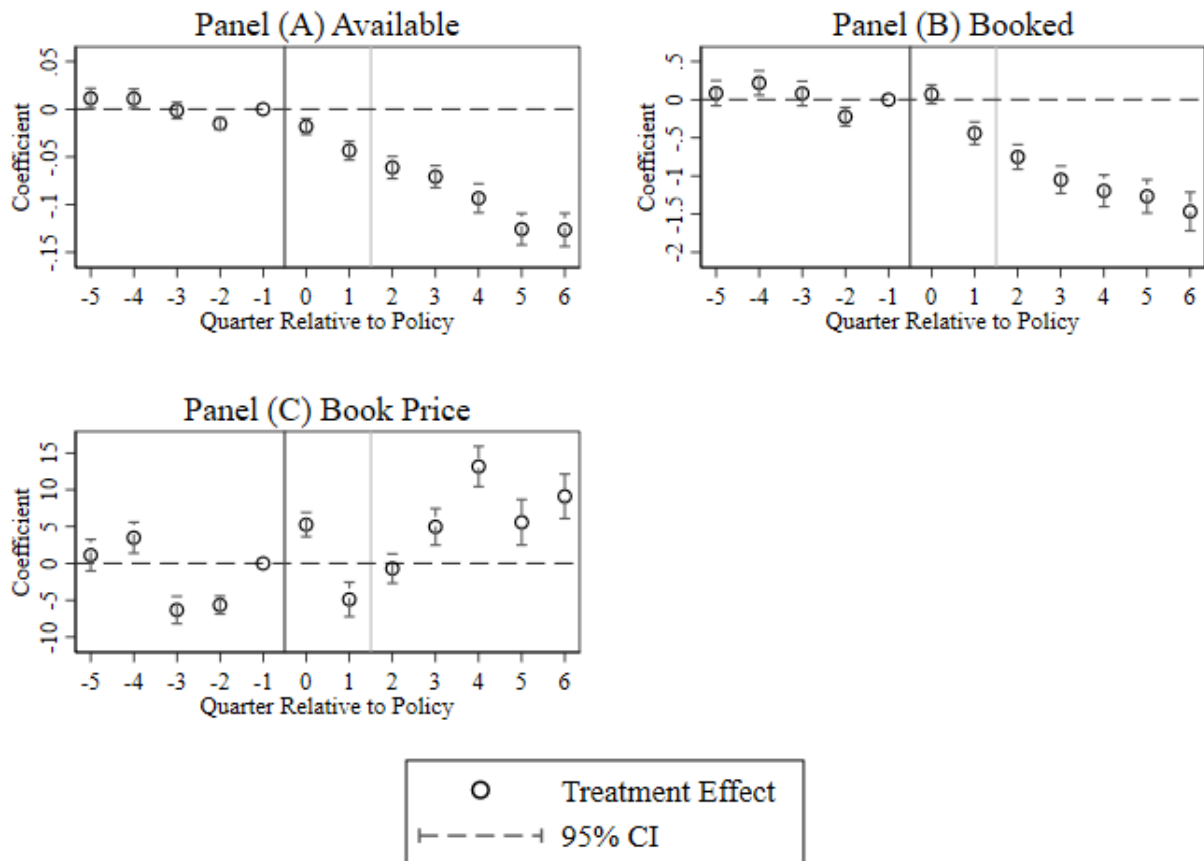
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# Figures and Tables

Figure 1: Effects of Registration Shocks on Property-Level Outcomes



Notes: 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, i.e., the time-disaggregated DiD estimates. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level.

Table 1: Summary of Airbnb Outcome Variables of Interest

	(1) Full Sample	(2) SF City Limits
Available	0.291 (0.454)	0.272 (0.445)
Nights Booked	2.865 (7.380)	2.830 (7.451)
Booking Price	166.82 (155.28)	206.27 (179.37)
Number of Listing-Months	2,185,956	1,087,272

Notes: Means 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. *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 dataset'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 2: Effects of Registration Shocks on Market Outcomes

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

Notes: 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. The property-level estimation sample contains an observation for every month for every property that was ever booked during our dataset'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 clustered at the tract level. Dependent variable means are in brackets. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

Table 3: Effects of Registration Shocks on Casual vs. Commercial Airbnb Listings

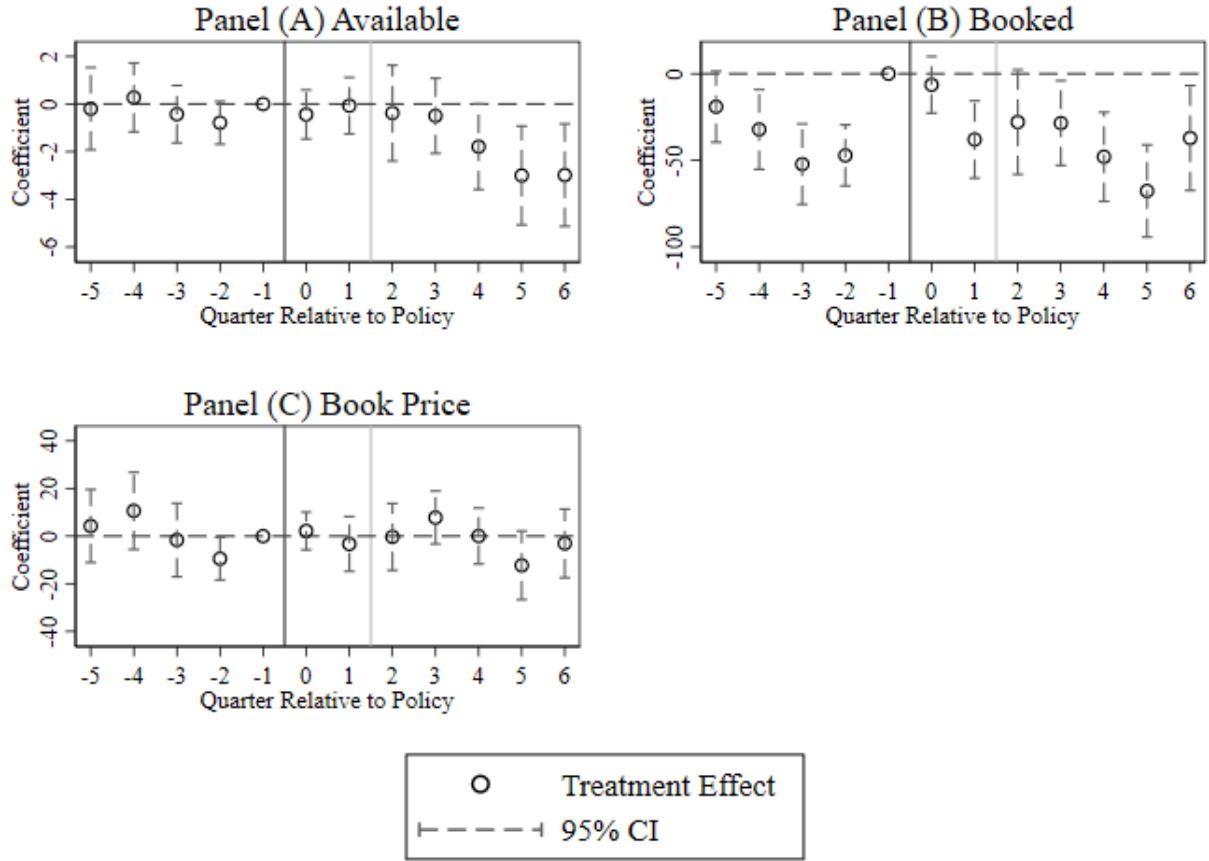
	Available	Nights Booked	Booking Price
Panel A: Main Estimation Sample			
High List Host x Treat x Post	-0.1428*** (0.0122) [0.44]	-1.6974*** (0.1330) [4.20]	3.8511*** (1.3306) [155.93]
Low List Host x Treat x Post	-0.0586*** (0.0065) [0.24]	-0.6630*** (0.0855) [2.41]	6.5536*** (0.9077) [173.47]
Panel B: Airbnb Density Quartile 4 Only			
High List Host x Treat x Post	-0.1055*** (0.0148) [0.43]	-1.2827*** (0.1753) [4.27]	3.8719** (1.7563) [176.15]
Low List Host x Treat x Post	-0.0474*** (0.0082) [0.23]	-0.5227*** (0.1225) [2.36]	6.9692*** (1.2827) [198.97]

Panel A of this table presents estimated average effects of the enforced registration requirements on availability, nights booked, and booking prices across hosts with a relatively high vs. low number of available listing-months during the pre-treatment period. Observations are at the property-month level. Panel B does the same, except restricting the estimation sample to properties in the top Airbnb density quartile. All specifications include month-year 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. “High List Host” refers to the top 25 percent of properties with respect to the number of property-months their hosts had available to rent during the 15-month pre-treatment period (>20 property-months), and “Low List Host” refers to those in the bottom 75 percent. The sample contains an observation for every month for every property that was ever booked during our dataset’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 clustered at the tract level. Group-specific dependent variable means are presented in brackets. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

## Appendix A   Online Appendix

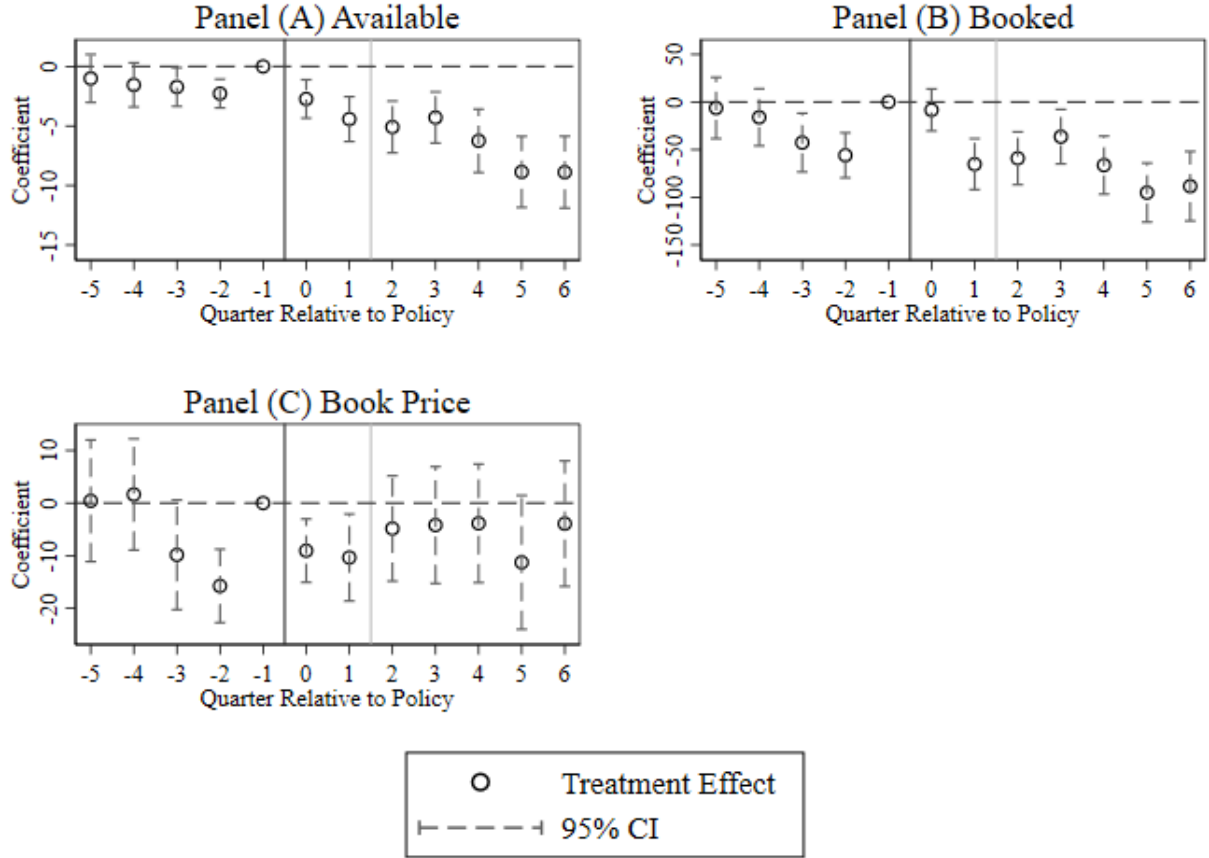


Figure A1: Effects of Registration Shocks on Tract-Level Outcomes, Quartiles 1 & 2



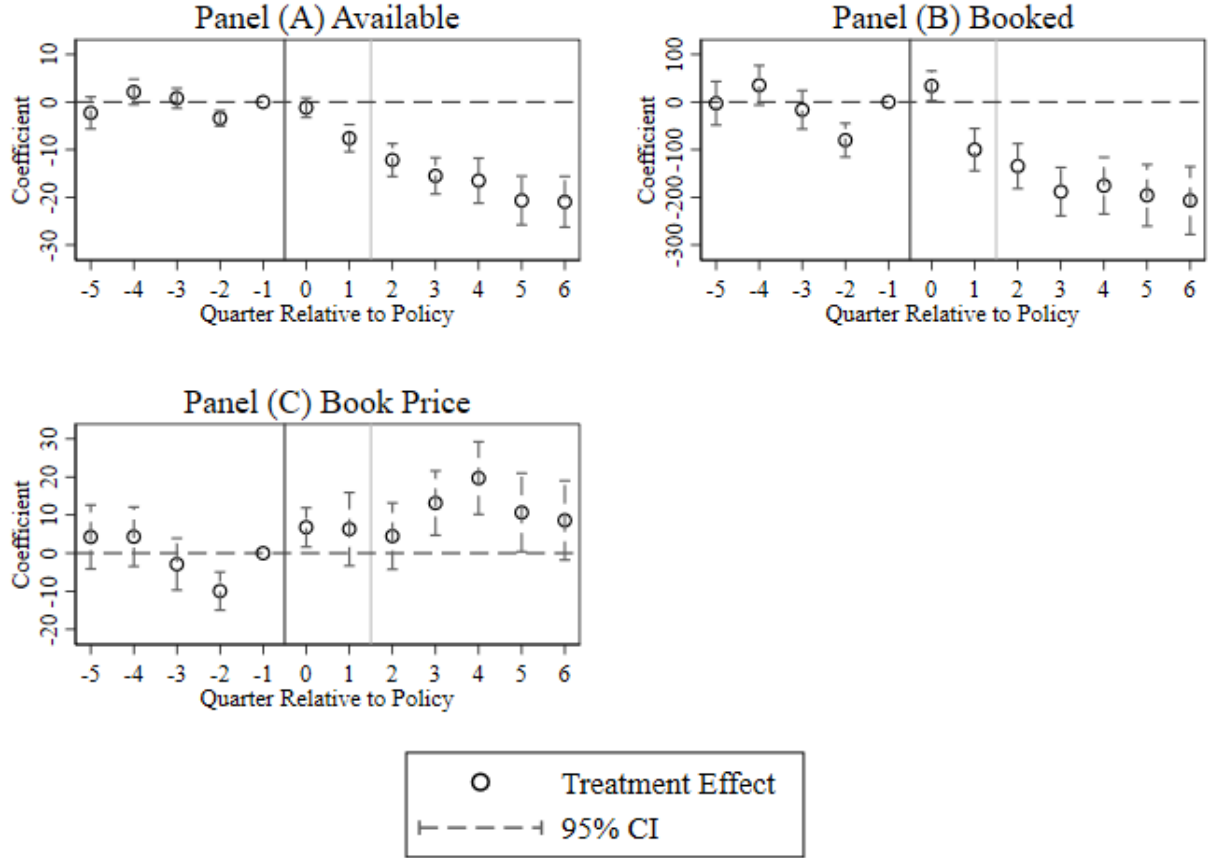
Notes: Quarterly differences in availability, nights booked, and booking prices around the treatment date between treated and untreated tracts in quartiles 1 and 2 (0-50th percentile) of the distribution of pre-treatment Airbnb listings per 1,000 tract population. The estimation sample includes all Airbnb listings that ever appear in our sample period. 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, i.e., the time-disaggregated DiD estimates. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level.

Figure A2: Effects of Registration Shocks on Tract-Level Outcomes, Quartile 3



Notes: Quarterly differences in availability, nights booked, and booking prices around the treatment date between treated and untreated tracts in quartile 3 (50-75th 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. 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, i.e., the time-disaggregated DiD estimates. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level.

Figure A3: Effects of Registration Shocks on Tract-Level Outcomes, Quartile 4



Notes: Quarterly differences in availability, nights booked, and booking prices around the treatment date between treated and untreated tracts in quartile 4 (75-100th 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. 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, i.e., the time-disaggregated DiD estimates. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the tract level.

Table A1: Summary of Airbnb Data For Each City in Sample

City	Total Listing- Month Obs.	Avg Tract Pop (2010 Census)	Pre-Treat Listings Avail / 1,000 (Tract)
Berkeley	156,096	3,407	13.41
Fremont	48,528	4,945	1.72
Mountain View	89,424	4,055	9.75
Oakland	263,088	3,445	7.18
Palo Alto	85,140	4,215	9.59
San Francisco	1,087,416	4,126	13.78
San Jose	259,344	4,900	2.83
San Mateo	46,404	3,988	4.01
Santa Clara	67,032	5,158	5.21
Sunnyvale	83,484	5,165	5.32

Notes: Summary of Airbnb data by city for tracts included in our main estimation sample. Pre-treatment listings available per 1,000 population reflects the density of Airbnb listings in the 15 months leading up to policy enactment.

Table A2: Summary of Airbnb Outcome Variables of Interest

	(1) Full Sample	(2) SF City Limits
Quartiles 1 and 2		
Available	0.301 (0.459)	0.291 (0.454)
Nights Booked	2.737 (7.189)	3.476 (8.236)
Booking Price	113.61 (107.52)	144.26 (109.11)
Number of Listing-Months	384,480	57,060
Quartile 3		
Available	0.305 (0.461)	0.295 (0.456)
Nights Booked	3.003 (7.512)	3.345 (8.103)
Booking Price	149.69 (131.15)	171.91 (132.87)
Number of Listing-Months	540,036	198,072
Quartile 4		
Available	0.282 (0.45)	0.265 (0.441)
Nights Booked	2.845 (7.38)	2.663 (7.221)
Booking Price	190.15 (171.49)	222.10 (193.35)
Number of Listing-Months	1,261,368	832,140

Notes: Means 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. *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 dataset'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.