The Battle for Homes:
How Does Home Sharing Disrupt Local Residential Markets?*

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As cities debate regulations of Airbnb and other home-sharing services, we study the impacts of home sharing on local residential real estate markets. By leveraging a unique quasi-experiment on Airbnb—a platform policy that caps the number of properties a host can manage in a city, we present the first empirical evidence on the mechanism behind the disruption of home sharing on local residential markets. We first find that rents in the long-term rental market and home values in the for-sale housing market dropped after the platform policy and that the price-to-rent ratio stayed relatively constant over time. The reduction in rents and home values is attributed to excess supply in local residential markets driven by the platform policy. We further discuss the generalizability of our findings by estimating and comparing the intensity of the policy impact across cities. Lastly, we reveal that the policy had heterogeneous impacts on local residential markets by residential property types and by market characteristics (e.g., the fraction of rental housing). Our findings provide important implications for policy makers and stakeholders of home sharing platforms.

Keywords: Home sharing, Residential markets, Airbnb, Platform economics, Affordable housing, Difference-in-differences, Synthetic control

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1. Introduction

Home sharing platforms allow individuals to earn extra income by opening their spare accommodation space to travelers. The growth of home sharing platforms, particularly Airbnb.com (Airbnb), has been exponential. Starting from renting out three air mattresses in 2008, Airbnb now hosts more than 6 million properties in nearly 100,000 cities and 191 countries (Airbnb 2019). A vital driving force of this growth is the significant interest from homeowners and absentee landlords, particularly those who capitalize on short-term vocational rentals by taking homes off rental or housing markets and listing them on Airbnb (Horton and Zeckhauser 2016). Criticism began to arise that Airbnb hosts cut off the supply of homes that would otherwise have been listed on long-term residential rental markets (hereafter, rental markets) or sold on housing markets,[1] which contributes to rising rents and home values (Barron, Kung, and Proserpio 2018). In contrast, advocates insist that Airbnb does not attenuate affordability because whether Airbnb properties constitute a significant share of local rental and housing markets remains unanswered (Stulberg 2016). Given the controversies, legislators around the world are experimenting with policies to meet the goal of affordable housing while reaping the benefits of home sharing.[2] In this paper, we aim to answer the question: *Is home sharing making rental and/or housing less affordable?*

Several recent studies explore to establish the impact of home sharing on local residential markets (Barron, Kung, and Proserpio 2018; Horn and Merante 2017; Sheppard and Udell 2016). In particular, Barron, Kung, and Proserpio (2018) estimate that a 1% increase in Airbnb properties in a local residential market is associated with a 0.018% increase in rents and a 0.026% increase in home values. The mechanism, or the premise, of such

[1] We consider the residential real estate market as two relatively separate markets—the rental market and the rest, which is a market for homes for sale by either homeowners or real estate agents. The literature does not have a general name for this market. Examples are housing markets (Sommer and Sullivan 2018) and for sale markets (Barron, Kung, and Proserpio 2018). To fix terminology, we call it the "housing market" throughout the manuscript. In terms of the market prices, we unanimously use "rents" to indicate the periodic (e.g., monthly or annual) payments from tenants to landlords and use "home values" to refer to the prices paid to purchase a house.

[2] Examples are ample, such as Toronto and Vancouver in North American, London, Paris, Berlin, and Amsterdam in Europe, and Hong Kong and Singapore in Asia. The regulations taken by these cities are diverse: they limit the time for tourist rentals, collect the tourist tax directly, or require individuals who host on Airbnb to register with the local government. In some extreme cases like Berlin, the decision was drastic, and the local government banned home sharing with the support of a court ruling (O'Sullivan 2016).

impacts is that the suppliers in local residential markets—homeowners, absentee landlords, and so on—displace their properties from local residential markets to the online channel and, more importantly, the extent of the displacement is sufficiently significant to affect the local market prices. However, the literature lags in identifying the mechanism *empirically*. We aim to fill the gap and ask specifically *whether and to what extent Airbnb hosts displace their properties in local residential markets to online home sharing?*

Without directly observing the property suppliers' choices, the usual empirical strategy to study the entry of home sharing platforms suffer endogeneity challenges such as simultaneity and omitted variable biases (Burtch, Carnahan, and Greenwood 2018; Greenwood and Wattal 2017; Zervas, Proserpio, and Byers 2017). We utilize a quasi-experimental opportunity—a platform policy on Airbnb, the world' largest home sharing platform—to answer the mechanism question of how home sharing disrupts local residential markets. Specifically, Airbnb rolled out a policy that caps the number of properties a host can manage in a city in 2016 and 2017.[3] First announced in April 2016 and implemented in November 2016 in New York City and San Francisco, the policy requires a host to list properties only at a single address on Airbnb, which is the so-called "One Host, One Home" policy (the OHOH policy hereafter). Later in February 2017, Airbnb implemented the same restriction in Portland (without announcement).[4] This city-specific platform policy provides a unique opportunity to unveil the mechanism of home sharing's impacts on local residential markets. To see this, the policy, on the one hand, removes properties from the platform and, on the other hand, prevent homeowners from displacing more properties from the local residential markets if they have had one listed on the platform.[5] If home sharing platforms such as Airbnb had not been a major alternative to local residential markets, then the policy should not have significant impacts on either the long-term rental market or the for-sale housing

[3] Airbnb official site provides a detailed description of the policy (at https://www.airbnb.com/help/article/1333/why-can-t-i-have-listings-at-more-than-one-address-in-select-locations). The rolling out to Portland can be found here: https://katu.com/news/local/airbnb-launches-one-host-one-home-policy-in-portland-to-protect-long-term-housing. A blog about the policy in Barcelona, Spain is at https://www.airbnbcitizen.com/one-host-one-home-in-barcelona/.

[4] Around the same time Airbnb experimented with this policy outside the U.S. (e.g., in several areas in Barcelona, Spain). Up to the end of the study period, the cities in the U.S. where the OHOH policy was rolled out only include New York City, San Francisco, and Portland, all considered in this study.

[5] Hosts with legitimate reasons to have listings at different addresses—i.e., helping a friend or relative manage their short-term rental, traditional B&Bs, boutique hotels, long-term corporate housing, long-term rentals (for 30+ nights only), and licensed hotels (or similar) were exempt from the policy (Airbnb 2016).

market. In other words, economically meaningful impacts of the policy would serve as evidence that the suppliers in local residential markets have displaced their properties to the online channel to an extent that the displacement is sufficiently significant to affect the prices in local residential markets.

Our empirical strategy, based on the OHOH policy, hinges on the quasi-experimental nature of the policy. We construct a comparison group of zip codes from cities that can best mirror the three policy-affected cities, i.e., New York City, San Francisco, and Portland, but were not affected by the policy. Specifically, we match the policy-affected cities to the comparison cities that are closest in the size of Airbnb market and economic and demographic characteristics (population, the number of households, the median household income, the unemployment rate, the home vacancy rate, and the fractions of the White, Hispanic, and female population) using an optimal matching algorithm that minimizes a Euclidean "distance" between the comparison cities and the policy-affected cities. Our main sample includes more than 400 zip codes from ten cities across the U.S. (including the three policy-affected ones) and spans a period from October 2014 to July 2017, which covers a sufficiently wide time window to unveil the policy impacts in the local residential markets.

We combine a rich set of data sets from multiple sources. We first obtain a sample of about 284,847 Airbnb properties in the sampled zip codes. The data set contains detailed monthly transactions and property characteristics for each property, including (but not limited to) the number of reservations, nightly and monthly rates, revenue, and property type (i.e., entire home, private room, or shared room). Our data on local residential markets are mainly from the largest residential platform, Zillow.com (Zillow). We collect, for each zip code and month, the Zillow Rental Index (ZRI) and the Zillow Home Value Index (ZHVI). We calculate the price-to-rent (Price2Rent) ratio as the home value divided by annual rent (= ZHVI / (12 * ZRI)). To establish the mechanism how the OHOH policy affected the local residential markets, we further obtain the supply and equilibrium quantity in housing and rental markets, respectively, from Zillow and a third-party real estate information company. We complement the data sets of Airbnb, Zillow, and the third-party real estate information company with an extensive set of economic and demographic characteristics for each zip code from the American Community Survey (ACS) database managed by the U.S. Census Bureau.

We utilize a variety of empirical specifications to estimate the policy impacts. Our main specification is a standard regression-adjusted differences-in-differences (DID) method based on a zip code by year-month panel. In addition to zip code fixed effects and year-by-month fixed effects, we control for a rich set of ACS variables (population, the number of households, the median household income, the unemployment rate, the home vacancy rate, and the fractions of the White, Hispanic, and female population) and city-specific time trends (to control for unobserved heterogeneity in the overall trends of the local markets across cities). We further use a relative time model to verify the findings from the main specification and investigate the policy impacts over time (Angrist and Pischke 2008). Also, in order to alleviate concerns regarding the comparability between the policy-affected zip codes and the comparison zip codes, we combine a propensity score matching (PSM) method with DID and apply a synthetic control method that allows for multiple "treated" units in observational studies (Abadie, Diamond, and Hainmueller 2010; Xu 2017). Additionally, we rule out alternative explanations such as intervening events in the policy-affected zip codes using a series of placebo tests, show the generalizability of our results across cities, and discuss the unsymmetrical policy impacts across heterogenous markets and by residence property type. We have furnished Section A1 of the Internet Appendix to summarize all the empirical tests we have conducted in this study by purpose.

Our main findings are four-fold. First, we find that the policy led to a drop of both rents and home values in the policy-affected zip codes (or markets in this study). Numerically, the announcement of the policy drove down rents and home values by about 1.2% and 1.1%, respectively, and the implementation further reduced both by roughly 1.9%. These findings suggest that the policy had similar impacts in rental markets and housing markets, which manifest further in its statistically insignificant impact on the Price2Rent ratio. Similar patterns show up in relative time regressions and survive multiple additional empirical tests mentioned above.

Second, the mechanism of the policy's impacts on local market prices is that the policy may lead to excess supply in both rental and housing markets. We thus examine whether the policy affected the supply and equilibrium quantity in local residential markets. Our findings lend support to the mechanism. Specifically, we find that the OHOH policy increased the supply in both rental and housing markets. Meanwhile, the policy did not significantly affect the equilibrium quantity in housing markets, while it increased the equilibrium quantity in rental markets.[6] To further unveil the mechanism of the excess supply in local residential markets, we investigate the policy impacts on the supply on Airbnb. We find that the properties of "multi-listing" (ML) hosts, who manage multiple properties at different addresses on Airbnb, shrank significantly as governed by the policy. Meanwhile, the policy seemed to increase properties managed by "single-listing" (SL) hosts—those who have a property at only one address on Airbnb. Nevertheless, the new entries are primarily shared or private rooms that are less likely displaced from local residential markets to offset the retreat of Airbnb properties to local residential markets. We also rule out a few alternative explanations that may bias our estimation but show no evidence.

Third, we explore the generalizability of our main findings by estimating and comparing the intensity of the policy impacts across cities. Specifically, we use the (logged) number of properties managed by multi-listing hosts per a thousand population before the policy as a moderator and find that, a 1% more increase in the policy-targeted properties from multi-listing hosts in a given zip code decreased rents of the same zip code by about 0.05% for New York City and Portland. A similar intensity of the impacts on home values is also comparable between New York City and Portland. In contrast, the intensity magnitudes of the impacts are larger in zip codes of San Francisco. One may hence worry about the uniqueness of San Francisco. We conduct robustness checks by replicating the main specification with San Francisco excluded and find, not surprisingly, similar policy impacts as our main results.

Last but not least, we find some interesting heterogeneity in policy impacts by property types and by local market characteristics. In the long-term rental markets, the rents of multi-family residence (MFR) properties decreased more than the rents of single-family residence (SFR) properties after the policy. In the for-sale housing markets, in contrast, we find that the policy had slightly larger effects on the home values of SFR properties than on that of condos. We also find that the structure of local residential markets plays a moderating

[6] It is to note that first, as we later report in the empirical section of the paper, the granularity of the available data on rental supply and equilibrium quantity is different from our main sample. More importantly, we would have worried more if the policy decreased the equilibrium quantity in the local rental markets, because in that case we would not be able to distinguish between the effects of increased supply and those of decreased demand in the rental markets. We found a drop in rents despite the increased equilibrium quantity along with the increased supply. It is likely our estimation of the policy impact on rents is conservative.

role in the policy impacts. Specifically, the policy reduced rents more in zip codes with a larger fraction of rental housing than in other zip codes. In contrast, because owner-occupied housing takes up a smaller fraction in zip codes with more rental housing, the policy had smaller (in magnitude) impacts on home values in those zip codes. We conclude the empirical estimation by reaffirming the comparability between the affected and unaffected zip codes.

The findings presented in our study have important implications. The growing popularity of home sharing platforms has invited debates about how to regulate such platforms (Filippas and Horton 2017). Only if we come to a better understanding of the role of home sharing in local residential markets, can the legislators, city administration, and the marketplace itself better capitalize on home sharing. Our findings imply that home sharing has evolved to be a major alternative to the local residential markets for real estate investment, which speaks to the rising concern of housing affordability across the U.S. and, in particular, in major metropolitan areas (Metcalf 2018). Relatedly, another implication of our findings is that home sharing may accelerate inequality as local homeowners and absentee landlords displace the supply of homes from locals to travelers, compromising public affordability for private wealth. Lastly, while the impacts of the policy are certainly restricted to the U.S. cities of New York City, San Francisco, and Portland, we offer a useful formula for other cities to evaluate how introducing a platform-driven policy like "One Host, One Home" to govern home sharing participants may mitigate the pressure on housing affordability. Our study informs policy discussions in cities that have begun to experiment with various regulations upon home sharing.

The rest of the paper is organized as follows. We begin with summaries of related literature and highlight our contributions in the next section. Section 3 introduces and discusses the OHOH policy. Section 4 describes the data and how we construct various measures. We develop our empirical strategies and present the findings in Section 5 and provide the concluding remarks in Section 6. Lastly, we provide a plethora of additional empirical tests supporting the main results in the Internet Appendix.

2. Literature and Contributions

Studies about the impact of home sharing on local residential and short-term accommodation markets are closest in spirit to ours. Early work focuses on how home sharing affects hotels in a competitive landscape (Farronato and Fradkin, 2018; Li and Srinivasan 2018; Zervas, Proserpio, and Byers, 2017). Recently, a few studies explore the impact of Airbnb on rents (Horn and Merante, 2017), the market value of residential properties (Sheppard and Udell, 2016), and both rents and home values (Barron, Kung, and Proserpio, 2018). In particular, using an instrumental variable approach based on Google search interest for Airbnb, Barron, Kung, and Proserpio (2018) show an increase in rents and home values across the U.S. after the entry of Airbnb.[7] We add to this literature by presenting the first empirical evidence of the mechanism behind the impact of home sharing on the local residential markets. The mechanism is that property owners (e.g., homeowners and absentee landlords) displace homes in local residential markets to the online alternative channel to an extent that the displacement has affected the price levels in local residential markets.

More generally, our work contributes to the emerging literature on peer-to-peer markets (Einav, Farronato, and Levin 2016; Fradkin 2017; Hall and Krueger 2016; Horton and Zeckhouser 2016; Hui, Saeedi, Shen, and Sundaresan 2016; Li, Moreno, and Zhang, 2016; Li and Netessine, 2018). Peer-to-peer markets are characterized by sufficiently low cost (Zervas, Proserpio, and Byers 2017) and relatively low entry barriers (Einav, Farronato, and Levin 2016) through which suppliers with underutilized products (space, time, money) can meet on-demand needs flexibly. Additionally, peer-to-peer markets provide enhanced reach to customers by reducing search costs (Einav, Farronato, and Levin 2016) and facilitating arm's length transactions (Edelman, Luca, and Svirsky 2017). Among early studies, peer-to-peer markets—with ride hailing and home sharing being two heavily studied ones—are often discussed as a disruptor to incumbents who offer similar services. On ride

[7] We differ from Barron, Kung, and Proserpio (2018) in three important ways. First and fundamentally, Barron, Kung, and Proserpio (2018) assess whether home sharing disrupts local markets and try to quantify the impacts. In contrast, thanks to the (OHOH) policy change opportunity, we study whether, and more importantly, how home sharing affects local residential markets. In other words, we demystify the underlying economic mechanism through which home sharing disrupts local residential markets. Second, our findings hinge on the quasi-experiment—the removal of multiple properties a host manages on Airbnb—that is deployed through a handful of tests by devising different empirical methods. Our identification strategy teases out confounding factors such as unobserved trends in local residential markets. Barron, Kung, and Proserpio (2018) instead use an instrumental variable approach. Third, we show different evidence that home sharing affects the rental and housing markets relatively equally rather than unsymmetrically. Our findings echo the literature on housing markets, which has long established that home values and rents equilibrate in the long run (de Leeuw and Ekanem 1971, Eubank and Sirmans 1979, Rosen and Smith 1983, Smith, Rosen, and Fallis 1988, Wheaton 1990, Sommer, Sullivan, and Verbrugge 2013).

hailing, Babar and Burtch (2017), Clewlow and Mishra (2017), and Hall, Palsson, and Price (2018) examine the impact of Uber and/or Lyft on public transit services. Berger, Chen, and Frey (2018) and Wallsten (2015) investigate the impact of Uber on incumbent taxi drivers and services. Gong, Greenwood, and Song (2017) evaluate the impact of access-based Uber on vehicle ownership. We focus on home sharing and present the first direct empirical evidence of how home sharing affects local residential markets. Our findings add to the debate about the impact of the technology-driven new economy on a fundamental welfare issue—housing affordability.

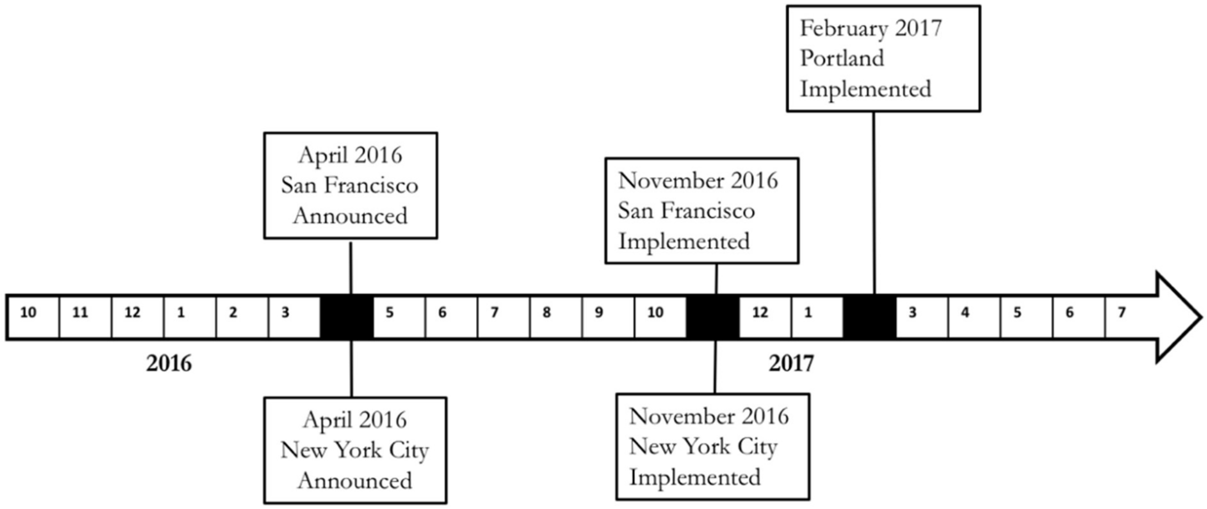
3. Airbnb and the "One Host, One Home" policy

Airbnb is known to provide an authentic experience to travelers by allowing them to "live like a local" (Benner 2016). However, the host practice of renting out a second, third, or even more than one thousand homes on Airbnb can cast a shadow on its business model.[8] Local legislators criticize absentee landlords for making housing less affordable by capitalizing on a string of for-profit properties and taking homes off the local residential markets (Kerr 2017). To smooth the relationship with local legislators, Airbnb rolled out its "One Host, One Home" policy to restrict hosts from listing multiple properties at different addresses in San Francisco, New York City, and Portland between 2016 and 2017. Figure 1 depicts the timeline of the staggered rollout of the policy in these three cities.

The policy was first announced on April 2, 2016 when Airbnb started publicizing its commitment to promoting home sharing in primary residences only by "*working with leaders in New York City and San Francisco on progressive policies*" that "*limits hosts to sharing listings at just one address on our platform*" (Airbnb 2016, 2017b). Seven months later, the policy was officially implemented. Effective on November 1, 2016, Airbnb hosts in New York City and San Francisco could list properties at only one address in their city. Under the restriction, if the hosts wanted to list a property at a different address, they would have been forced to remove the existing property. Violating the policy may lead to a host's properties and Airbnb account being suspended or

[8] The Consumerist documents a growing concern about the so-called "mega hosts" who are effectively running hotel operations, renting out dozens, hundreds, sometimes more than one thousand properties on Airbnb. Source: https://consumerist.com/2017/06/09/some-airbnb-mega-hosts-are-renting-out-more-than-1000-properties-at-once/

Figure 1. The Timeline of the "One Host, One Home" Policy



Notes.
1. The months when the policy was announced or implemented are indicated in black.
2. Because the policy was implemented in Portland on January 30, 2017 (end of the month), we mark February as the actual month for policy implementation in Portland.

deactivated.[9] Continuing its implementation in New York City and San Francisco, Airbnb unexpectedly launched the same policy in Portland to address "*Portland's housing affordability crisis and unwanted commercial operators who may be converting housing to illegal hotels on our platform*." It became effective on January 30, 2017 (Airbnb 2017a).

The staggered rollout of the policy offers an ideal setting to study whether and how home sharing impacts local residential markets. Table 1 illustrates our empirical design. We focus on both the announcement and the implementation of the policy over two years from 2016 to 2017. We use "before the policy" and "after the policy" to refer to the time periods before and after the beginning of policy announcement and implementation, collectively. Insofar as the rollout of the policy was only in these three cities: New York City, San Francisco, and Portland, zip codes in these cities serve as the "treatment" group that is affected by the policy, while those in other comparable but unaffected (by the policy) cities serve as the "control" group. We explain how we pick unaffected cities and discuss their comparability to the affected cities in the next section.

[9] Airbnb provides more information about the policy on its webpage: https://www.airbnb.com/help/article/1333/why-can-t-i-have-listings-at-more-than-one-address-in-select-locations.

Table 1. The Difference-in-Differences Design

Group	Before Policy	After Policy
Zip codes in policy-affected cities	O	X
Zip codes in unaffected cities	O	O

Note. "Policy" indicates the rollout of the OHOH policy, which includes both announcement and implementation.

4. Data and Measures

The study is based on data from four sources: Airbnb, Zillow.com, a third-party real estate information company, and the U.S. Census Bureau.

First, we collect property-level Airbnb data from a research company that provides trusted data and analytics services about Airbnb.[10] Its data service has been widely used and endorsed by leading hospitality institutions such as the American Hotel & Lodging Association, Discover Los Angeles, HVS, and CBRE Hotels.[11] The data set contains the monthly performance of individual properties. Our observation period is between October 2014 and July 2017 (a total of 34 months), which covers 18 months before and 16 months after April 2016 (the announcement of the policy). We choose this relatively large time window to observe the full impact of the policy across different phases. An advantage of the data set, compared with web-scraped Airbnb data, is that it contains both consumer-facing information, such as property characteristics (entire home, shared room, or private room), and backstage information, such as property availability (open for booking, booked by guests, or blocked on the calendar by the host), booking information (average daily price and number of reservations), and geographic coordinates of each property.[12] An Airbnb property may be unavailable for booking because the host chooses to block the calendar or other consumers have booked the property. By observing both availability and booking information, we can effectively distinguish between the two cases. We

[10] The company name is not disclosed for confidentiality and non-disclosure purposes. The company profile and details are available upon request.

[11] Introductions to the American Hotel & Lodging Association (https://www.ahla.com/who-we-are), Discover Los Angeles (https://www.discoverlosangeles.com/), HVS (https://www.hvs.com/), and CBRE Hotels (http://www.cbrehotels.com) can be found by clicking their respective links.

[12] To protect privacy and security, Airbnb does not show the exact address of properties. Customers can only get an estimated location within a small radius when browsing the listing page. The full address of a listing will only be given once booked.

follow the practice of Li and Srinivasan (2018) to define active listings as the properties that are marked as either booked or available on the host's calendar. We then use this information to calculate the presence of Airbnb in each zip code by month. We calculate the unique number of properties operated by a host and categorize hosts into multi-listing hosts (those who manage properties at multiple addresses and are hence affected by the policy) and single-listing hosts.

Second, we collect data on local housing and rental markets from two sources: Zillow.com and a third-party real estate information company.[13] The first set of data on the rental and housing market price indices is collected from Zillow.com/research. As the largest online residential real estate platform, Zillow aggregates the median estimated market rent rate and home values into smoothed, seasonally adjusted measures, namely Zillow Rent Index (ZRI) and Zillow Home Value Index (ZHVI) for each zip code by year-month.[14] To compare the relative impact of the policy on rental markets over housing markets, we calculate the price-to-rent ratio (Price2Rent hereafter) in its standard form, which is the ratio between the home value and annual rent that equals ZHVI / (12 * ZRI). Zillow also publishes price indices by conventional real estate types: condo houses, single-family residence houses, multi-family residence rentals, and single-family residence rentals. We also collect from Zillow the monthly supply and housing transactions (a measure of equilibrium quantity)[15] in the housing market. However, Zillow does not publish similar data for the rental market. Fortunately, the other set of data we collect from a third-party real estate information company complements the Zillow data with the quarterly supply and equilibrium quantity in the rental market. The data on supply and equilibrium quantity in both housing and rental markets allow us to observe the retreat of Airbnb properties to the local residential markets. We can then discover the mechanism of the policy impacts on local market prices.

Lastly, because the demographic and economic characteristics of a market (a zip code in our case) may also play a role in determining our rental and housing market price measures, we control for a comprehensive set

[13] The company name is not disclosed for confidentiality and non-disclosure purposes. The company profile and details are available upon request.
[14] Zillow's methodology of calculating the price indices, ZRI and ZHVI, can be found on their data export web page at https://www.zillow.com/research/data/.
[15] The equilibrium quantity of housing units is defined as the number of sold houses in the housing market. Similarly, the equilibrium quantity of rental units is defined as the number of occupied rentals in the rental market.

of annually compiled variables in each zip code as obtained from the U.S. Census Bureau's annual American Community Survey (ACS hereafter). These variables include population, the number of households, the median household income, the unemployment rate, the home vacancy rate, and the fractions of the White, Hispanic, and female population.

We match these four sources of data at the zip code level by year-month. We exclude zip codes with no Airbnb presence or available local residential market price indices because of data unavailability. Our sample consists of 284,847 Airbnb properties listed in 200 zip codes of the three affected cities and 217 zip codes of the seven unaffected cities. Table 2 presents the definition and summary statistics of the main variables. We first translate the rents and home value measures—ZRI and ZHVI, both in $1,000—into 1990 US dollars using the inflation rate and then take the logarithm of the measures (logZRI and logZHVI).[16] Note that the price-to-rent ratio remains the same before and after the translation. We also take the logarithm of the ratio (logPrice2Rent) for consistency of interpretation. Therefore, our findings will inherit the semi-elasticity interpretation. Additionally, we take logarithms of several control variables that exhibit skewed distributions.

Next, we discuss our choice of policy-unaffected cities and the comparability with the policy-affected ones. Because the policy was announced and implemented at the city level in New York City, San Francisco, and Portland, we choose cities that are most comparable to these three affected cities. We first rank the other major U.S. cities by Airbnb market size (in terms of the number of Airbnb properties) and economic and demographic characteristics (population, the number of households, the median household income, the unemployment rate, the home vacancy rate, and the fractions of the White, Hispanic, and female population). Then, we use a Euclidean distance algorithm to find the nearest "neighbors" to the affected cities. As a result, the unaffected group consists of seven major metropolitan areas in the U.S., including Los Angeles, San Diego, San Jose, Seattle, Washington D.C., Boston, and Philadelphia. Together, the ten cities—three affected cities and seven unaffected cities—are the top metropolitan areas with a massive Airbnb presence in the U.S. Table 3 compares

[16] Not surprisingly, translating the price indices into 2000 US dollars or 2010 US dollars yields similar results in our empirical analysis. Also, note that Price2Rent is not affected by such scaling.

Table 2. Definition, Source, and Summary Statistics of Variables

Source	Variable	Definition	Mean	Median	SD	Min.	Max.
Rental and housing market price indices:							
Zillow	logZRI	Log of ZRI in $1,000 (in 1990 USD)	0.28	0.21	0.35	-0.75	1.61
	logZHVI	Log of ZHVI in $1,000 (in 1990 USD)	5.69	5.70	0.63	3.00	7.47
	logPrice2Rent	Log of Price2Rent (= ZHVI / (12*ZRI))	2.93	2.95	0.34	1.20	3.94
	logZHVI_Condo	logZHVI of condo houses	5.39	5.39	0.55	3.67	6.99
	logZHVI_SFR	logZHVI of single-family residence houses	5.93	5.90	0.70	3.48	9.22
	logZRI_MFR	logZRI of multi-family residence rental units	0.15	0.13	0.35	-0.78	1.37
	logZRI_SFR	logZRI of single-family residence rental units	0.32	0.25	0.40	-0.72	2.07
The OHOH policy:							
Airbnb	1(Announcement)	A dummy indicating the policy announcement	0.40	0	0.49	0	1
	1(Implementation)	A dummy indicating the policy implementation	0.48	0	0.50	0	1
Supply on Airbnb:							
Airbnb	log(Airbnb properties)	Log of the number of Airbnb properties	4.13	4.22	1.49	0.69	7.86
	log(ML properties)	Log of the number of Airbnb properties operated by multi-listing hosts	2.22	2.20	1.61	0	7.16
	log(SL properties)	Log of the number of Airbnb properties operated by single-listing hosts	3.96	4.04	1.45	0	7.80
	log(Entire home entry)	Log of the number of entire homes entering Airbnb	1.59	1.39	1.32	0	6.50
	log(Shared/private room entry)	Log of the number of shared or private rooms entering Airbnb	1.52	1.39	1.16	0	6.49
Supply and equilibrium quantity in the local residential markets:							
Zillow	log(Housing supply)	Log of the number of for-sale houses	4.67	4.64	0.64	2.77	6.46
	log(Sold houses)	Log of the number of sold houses	9.62	9.68	0.60	7.48	10.79
A third-party Real Estate Information Company	log(Rental supply)	Log of the number of rental units	9.08	9.16	1.40	2.57	14.10
	log(Occupied rentals)	Log of the number of occupied rental units	9.02	9.11	1.41	2.46	14.07
Economic and demographic controls:							
U.S. Census Bureau: the American Community Survey	log(Population)	Log of population	10.36	10.46	0.73	7.42	11.65
	log(Households)	Log of the number of households	9.42	9.49	0.65	6.98	10.68
	log(Med. income)	Log of the median household income	11.06	11.06	0.42	9.75	12.40
	Unemployment rate	Unemployment rate	8.06	7.30	3.54	1	29.40
	Home vacancy rate	Home vacancy rate	8.26	7.20	4.46	0.35	30.43
	% White	Percentage of the White population	39.33	41.49	23.84	0.43	88.04
	% Hispanic	Percentage of the Hispanic population	16.40	11.04	14.70	0	74.72
	% Female	Percentage of the female population	50.91	51.12	3.17	36.39	59.21

Note. The policy announcement affected only New York City and San Francisco. The policy was implemented in Portland later in February 2017.

the affected cities and the unaffected cities by observable characteristics.[17] We can see that, on average, the three policy-affected cities are not significantly different from the unaffected cities in terms of either the size of Airbnb or city economic and demographic characteristics.

Table 3. Characteristics of Policy-Affected Cities versus Unaffected Cities

	Affected Cities				Unaffected Cities			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Market size of Airbnb:								
log(Airbnb properties)	5.90	0.80	5.40	6.80	5.67	0.56	4.75	6.33
Economic and Demographic characteristics:								
log(Population)	10.30	0.30	10.10	10.60	10.17	0.42	9.32	10.57
log(Households)	9.40	0.20	9.20	9.60	9.25	0.33	8.55	9.56
log(Med. income)	11.10	0.20	11.00	11.30	11.11	0.26	10.73	11.43
Unemployment rate	7.20	0.90	6.30	8.00	7.95	1.88	5.69	10.50
Home vacancy rate	8.00	1.90	5.90	9.50	8.03	2.96	3.37	11.36
% White	46.50	16.00	34.90	64.80	41.92	14.92	23.36	64.77
% Hispanic	11.50	5.10	6.80	17.00	15.38	10.68	6.79	35.12
% Female	50.40	1.80	48.60	52.20	50.40	1.34	49.37	52.41

5. Findings

As a quick roadmap of this section, we begin with reporting the impacts of the policy (on rents and home values) by providing model-free evidence, estimates from standard DID specifications, and a battery of tests to validate the DID assumptions and to verify the robustness of our main results. We then drill down to the mechanisms behind the policy impacts by showing how the policy impacted the supply and/or equilibrium quantity on Airbnb and in local residential markets. We also rule out several alternative hypotheses. Lastly, we discuss the generalizability of our findings, reaffirm the comparability between the policy-affected group and its unaffected counterfactuals, and provide additional evidence on the heterogeneous impacts of the policy.

[17] We discuss the comparability more formally in Section 5.4. As a quick preview, we first note that our main unit of analysis is zip code by year-month (not city by year-month). It is not surprising to find highly comparable zip codes in the "treated" and "control" cities. Second, we include city-level trends in our empirical specifications to control for any unobserved trends in local residential markets at the city level. Last but not least, we adopt two additional empirical strategies—a standard DID combined with propensity score matching and a synthetic control method that allows for multiple "treated" units—to alleviate comparability concerns and to further validate our main empirical findings.

5.1. The Policy Impacts on Local Residential Market Prices

5.1.1. Model-Free Evidence

Prior to the formal regression analysis, we present the model-free evidence on an overarching prediction of our empirical framework: zip codes that were affected by the OHOH policy should have experienced less increase in rents and home values than those that were not affected. Table 4 reports the differences of the policy impact between the affected and unaffected zip codes at different phases of the policy rollout. Panel A reports the means of the pre- and post-announcement/implementation logZRI, logZHVI, and logPrice2Rent in affected zip codes. Panel B reports the same summary statistics for unaffected zip codes. We also conduct paired t-tests to compare the price indices before and after each phase of the policy.

Table 4. Average Rents and Home Values Pre- and Post-Announcement/Implementation by Group

	(1)	(2)	(3)	(4)	(5)
	Pre-announcement	Post-announcement	Paired *t*-test (2) versus (1)	Post-implementation	Paired *t*-test (4) versus (1)
Panel A. Zip codes affected by the OHOH policy					
logZRI	0.376	0.378	0.002 (0.009)	0.367	-0.009 (0.011)
logZHVI	5.919	5.993	0.074 (0.014)	6.006	0.088 (0.017)
logPrice2Rent	3.057	3.130	0.073 (0.008)	3.154	0.097 (0.009)
Panel B. Zip codes unaffected by the OHOH policy					
logZRI	0.219	0.256	0.037 (0.008)	0.256	0.037 (0.010)
logZHVI	5.505	5.597	0.092 (0.015)	5.620	0.115 (0.018)
logPrice2Rent	2.801	2.856	0.055 (0.008)	2.879	0.078 (0.009)

Notes.

1. Column (1) shows the mean values of logZRI, logZHVI, and logPrice2Rent before April 2016 (the policy announcement), Column (2) shows the means after April 2016, Column (4) shows the means after November 2016 (the policy implementation). We report paired *t*-tests for the differences between post-announcement and post-implementation with pre-announcement, respectively, in Columns (3) and (5).
2. Because zip codes in Portland did not experience the policy announcement and had a different timeline for policy implementation from zip codes in other two policy-affected cities, we do not include zip codes from Portland in the mean comparison. The results remain qualitatively consistent if we include Portland.
3. Standard errors are reported in parentheses.

We find that the means of rents and home values in both groups were, in general, higher after the policy announcement, suggesting rents and home values were on the rise in all of the sampled zip codes. However, the increase of logZRI in affected zip codes is small (0.002) and statistically insignificant, while its increase in unaffected zip codes is significantly much larger (0.037), indicating a potential hit on rents by the policy announcement in the affected zip codes. Similarly, the increase of logZHVI was larger in the unaffected zip codes than the affected ones (0.092 versus 0.074). After the policy implementation, even though not significantly, the rents decreased in the affected zip codes. The home values also increased less in affected zip codes than those in the unaffected zip codes. Overall, the model-free evidence in Table 4 suggests a negative shock of the policy on rents and home values. Additionally, even though home values continued to grow in all sampled zip codes, there was a detectable reduction in the growth momentum for affected zip codes following the policy that narrowed the price gap between the two groups.

5.1.2. Results from the Regression-Adjusted Analysis

We use a regression-adjusted DID specification to formally examine the impact of the staggered rollout of the OHOH policy, including policy announcement and policy implementation. For each zip code i in year-month t, the impact of the policy on local residential markets are specified as

$$Y_{it} = \beta_1 1(\text{Announcement})_{it} + \beta_2 1(\text{Implementation})_{it} + \gamma' Z_{it} + \mu_t + v_i + \varepsilon_{it}, \tag{1}$$

where the indicator $1(\text{Announcement})_{it}$ equals 1 if the policy was announced in zip code i by time t; $1(\text{Implementation})_{it}$ equals 1 if the policy was implemented in zip code i by month t; Z_{it} is a vector of covariates including zip code economic and demographic characteristics (ACS controls) and city-specific time trends; μ_t denotes year by month fixed effects; v_i denotes zip code fixed effects; and ε_{it} is the idiosyncratic error term. We separate the announcement from the implementation because announcing the policy may change the hosts' expectations and their property allocation. For the dependent variable Y_{it}, we use logZRI, logZHVI, and logPrice2Rent to estimate the impact of the policy on different aspects of home affordability. We cluster the standard error at the zip code level to account for potential serial correlations in our panel data (Bertrand, Duflo, and Mullainathan 2004).

The coefficients β_1 and β_2 capture the impacts of the policy on the three local residential market price measures—logZRI, logZHVI, and logPrice2Rent. Note the corresponding variables, $1(\text{Announcement})_{it}$ and $1(\text{Implementation})_{it}$, are both time and location specific. Taking $1(\text{Implementation})_{it}$ as an example, it equals one if zip code i is in New York City or San Francisco and month t is after November 2016. It is also one if zip code i is in Portland and month t is after January 2017. These two variables take values of zeros for all unaffected zip codes.[18]

Table 5 reports the estimated coefficients of the policy dummies and ACS controls per Equation (1). We omit the city-level trend coefficients for space constraint. Column (1) shows that after the policy announcement, the rents on average decreased by 1.2% and then decreased another 1.9% after the implementation in policy-affected zip codes (relative to unaffected areas). In total, the rents decreased by 3.1% in affected zip codes because of the policy. Similarly, estimates in Column (2) reveal that the home values on average decreased by 1.1% after the announcement and by another 1.9% after the implementation.[19] Overall, home values decreased by 3%. Consistent with the similar magnitudes of policy impacts between those reported in Columns (1) and (2), the policy dummy coefficients in Column (3) imply that the change in price-to-rent ratio was not statistically significant (suggesting that the policy had a similar impact on rents and home values).

In addition to the standard DID specification, we estimate a relative time model to verify the findings from the main specification and check its parallel trends assumption. The trends after the policy announcement reveal how the policy has slowly but steadily affected rents and home values over time. The detailed results are presented in Section A2 of the Internet Appendix. We also address the usual "Ashenfelter's Dip" concern by dropping a few months right before the policy (Ashenfelter and Card 1985) to validate again the findings from the main specification. The results are displayed in Section A3 of the Internet Appendix. Lastly, we further ensure that it is indeed the policy driving the results using a placebo test by randomly assigning a subset of zip codes as if they were policy affected. The results are reported and discussed in Section A4 of the Internet Appendix. Not surprisingly, the main findings are quite robust to all these additional tests.

5.2. How did the Policy Affect the Local Residential Market Prices?

We have demonstrated that the policy had negative impacts on both rents and home values. Then a natural question that follows would be: what is the mechanism behind the policy impacts on the local residential market

[18] Alternatively, we could present these two variables as interaction terms between a policy-affected dummy, indicating a policy-affected zip code, and two event dummies, indicating the period is after announcement or implementation, respectively. Note that under this alternative specification, the individual terms—the policy-affected dummy and the two event dummies—are not identified because we include both zip code and year-by-month dummies.

[19] In the following discussions, even though we still present the effects of the policy announcement and implementation separately in the results tables, we often combine the two effects to discuss the overall impact of the policy.

Table 5. The Policy Impacts on Local Residential Market Prices

	(1) logZRI	(2) logZHVI	(3) logPrice2Rent
1(Announcement)$_{it}$	-0.012***	-0.011***	0.001
	(0.003)	(0.003)	(0.004)
1(Implementation)$_{it}$	-0.019***	-0.019***	0.000
	(0.003)	(0.004)	(0.004)
ACS controls: Economic and demographic characteristics			
log(Population)	0.018	0.009	-0.009
	(0.060)	(0.073)	(0.086)
log(Households)	-0.142**	-0.027	0.115
	(0.062)	(0.069)	(0.078)
log(Med. income)	-0.054**	-0.049	0.005
	(0.021)	(0.030)	(0.035)
Unemployment rate	-0.005***	-0.008***	-0.003*
	(0.001)	(0.002)	(0.002)
Housing vacancy rate	-0.003***	-0.000	0.003***
	(0.001)	(0.001)	(0.001)
% White population	0.001	0.006***	0.005***
	(0.001)	(0.001)	(0.001)
% Hispanic population	0.001	0.003*	0.002
	(0.001)	(0.002)	(0.002)
% Female population	-0.003**	-0.003**	-0.000
	(0.001)	(0.001)	(0.001)
Zip code FE	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes
City trends	Yes	Yes	Yes
Observations	13,986	13,986	13,986
R-squared	0.995	0.998	0.990
Number of zip codes	417	417	417

*** p<0.01, ** p<0.05, * p<0.1.

Note: Robust standard errors clustered at the zip code level are shown in parentheses.

prices? In this section, we hypothesize that by preventing hosts from listing properties at multiple addresses online, the OHOH policy effectively increased the supply in the offline rental and housing markets. The increase in supply may have driven down the rents and home values henceforth. We rule out a number of alternative hypotheses in this section as well.

5.2.1. The Policy Impacts on the Supply on Airbnb

We begin with the impact of the OHOH policy on the supply on Airbnb. As the OHOH policy precludes hosts who have multiple properties at different addresses, intuitively the supply from multi-listing hosts should decrease on Airbnb. But it is not clear, at least *ex ante*, how the supply from single-listing hosts and hence the total supply will change. Table 6 reports model-free evidence, in which Panels A and B report the mean values of log(Airbnb properties), log(ML properties), and log(SL properties) in affected and unaffected zip codes, respectively. We can see that, while the total number of properties and properties from single-listing hosts continued to grow in affected zip codes, the number of properties from multi-listing hosts decreased. Meanwhile, all the three measures increased in unaffected zip codes. These results seem to suggest that the policy had differential impacts on the properties of different types of hosts, which indeed led to changes in the supply on Airbnb.

Table 6. Average Supply Pre- and Post-Announcement /Implementation by Group

	(1)	(2)	(3)	(4)	(5)
	Pre-announcement	Post-announcement	Paired t-test (2) and (1)	Post-implementation	Paired t-test (4) and (1)
Panel A. Zip codes affected by the OHOH policy					
log(Airbnb properties)	4.022	4.300	0.278 (0.059)	4.380	0.358 (0.053)
log(SL properties)	3.897	4.204	0.307 (0.058)	4.290	0.393 (0.053)
log(ML properties)	2.096	2.078	-0.018 (0.055)	2.017	-0.079 (0.050)
Panel B. Zip codes unaffected by the OHOH policy					
log(Airbnb properties)	3.840	4.286	0.446 (0.038)	4.437	0.596 (0.035)
log(SL properties)	3.648	4.067	0.419 (0.036)	4.196	0.548 (0.033)
log(ML properties)	2.073	2.492	0.419 (0.044)	2.655	0.582 (0.042)

Notes.

1. Column (1) shows the mean values of log(Airbnb properties), log(SL properties), and log(ML properties) before April 2016 (the announcement), Column (2) shows the means after April 2016. Column (4) shows the means after November 2016 (the implementation). We report paired *t*-tests for the differences between post-announcement and post-implementation with pre-announcement, respectively, in Columns (3) and (5).
2. Because Portland did not experience the policy announcement and had a different timeline for implementation, we do not include Portland in the mean comparison. The results remain qualitatively consistent if we include Portland.
3. Standard errors are reported in parentheses.

We next present evidence from more formal regressions. The empirical specification follows Equation (1) with the dependent variables measuring the (ML hosts', SL hosts', and total) supply on Airbnb (keeping all ACS controls, fixed effects, and city-specific time trends). The results are presented in Table 7. Column (1) shows that the number of multi-listing properties decreased dramatically, per the goal of the policy. Quantitatively, the supply of multi-listing hosts decreased by 40% after the policy. In contrast, we observe that the number of single-listing properties increased by about 12.7% after the policy, as shown in Column (2). That is, the removal and blocking of multi-listing properties by Airbnb may have encouraged the entry of single-listing properties, while the total supply on Airbnb did not change significantly per Column (3).

Table 7. The Policy Impacts on the Supply on Airbnb

	(1) log(ML properties)	(2) log(SL properties)	(3) log(Airbnb properties)	(4) log(Entire home entry)	(5) log(Shared/private room entry)
1(Announcement)$_{it}$	-0.253***	0.065**	0.014	-0.008	-0.052
	(0.047)	(0.026)	(0.025)	(0.041)	(0.037)
1(Implementation)$_{it}$	-0.147***	0.062**	0.029	0.010	0.090**
	(0.043)	(0.026)	(0.024)	(0.030)	(0.036)
Zip code FE	Yes	Yes	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes	Yes	Yes
City trends	Yes	Yes	Yes	Yes	Yes
ACS Controls	Yes	Yes	Yes	Yes	Yes
Observations	13,986	13,986	13,986	13,986	13,986
R-squared	0.933	0.971	0.975	0.853	0.805

*** p<0.01, ** p<0.05, * p<0.1.

Notes.

1. Robust standard errors clustered at the zip code level are shown in parentheses.
2. The estimated coefficients of all ACS controls are omitted for space constraint. This applies to subsequent tables (if applicable).

Because the total supply on Airbnb did not change significantly, one may worry that the properties that were displaced back to the local residential markets may be limited if the properties of single-listing hosts who entered Airbnb may offset the removed/blocked properties of multi-listing hosts that left Airbnb. The assumption of this concern is that these new single-listing entries come directly from the local residential markets. However, if the incoming single-listing hosts are more likely people who share spare rooms in their living space rather than renting out the entire homes (with a higher chance in the market than a spare room), it may be hard to believe their new entries are all from the local residential markets. To test this hypothesis, we split the number of new entries by their types—entire home or private/shared room. We present the results in Columns (4) and (5) in Table 7. Consistent with our conjecture, the number of entire-home entries did not change significantly after the policy, while the number of private/shared properties significantly increased, which lends support to the mechanism of the policy impacts—introducing an influx of spare (shared or private) rooms from off-market living space to Airbnb without taking in significant amount of entire homes from local residential markets.

5.2.2. The Policy Impacts on the Supply and Equilibrium Quantity in Local Residential Markets

The findings reported in the last section suggest that the policy may have forced the hosts to displace their properties back to local residential markets or may retain potential entries that would have otherwise been displaced from the local residential markets to Airbnb. In this section, we use data on the supply and equilibrium quantity in the housing markets and rental markets, respectively, to demonstrate that the supply indeed increased after the policy. We also show that the equilibrium quantity either remained relatively stable or slightly increased. Hence, the negative impact of the policy on rents and home values may be attributed to the increase in the housing and rental supply.

Table 8 reports the results. We again use the same specification per Equation (1) with the dependent variables being log(Housing supply)—the available for-sale house supply, and log(Sold houses)—the sold houses that can be viewed as the equilibrium quantity, both collected from Zillow. Note that we have fewer observations due to the data unavailability in certain zip codes. Column (1) shows that the number of for-sale houses increased by about 3.4% after the policy (the impacts of announcement and implementation combined), which suggests that the policy indeed drove up housing supply. Column (2) shows that the sold houses on the housing market did not change significantly after the policy. These findings suggest that the negative policy impact on home values could be driven by the excess supply in housing markets (due to the OHOH policy).

Because Zillow does not provide similar data for rental markets, we obtained the data from a third-party real estate information company. A caveat is that the data are at a geographic area (by quarter) level that is different from zip codes, thus less granular than our main sample. Also, we cannot match the zip code-level ACS controls to the geographic areas. Table 9 shows the results. Because the two policy dummies are highly correlated in our quarter-level data, we combine them into one variable 1(After Policy) which equals one if 1(Announcement) equals one for New York City and San Francisco or if 1(Implementation) equals one for Portland. Albeit, the results are qualitatively similar if we use 1(Announcement) and 1(Implementation) separately. We can see that the rental supply increased by about 0.9% after the policy, and the equilibrium

Table 8. The Policy Impacts on the Supply and Equilibrium Quantity in Local Housing Markets

	(1) log(Housing supply)	(2) log(Sold houses)
1(Announcement)$_{it}$	-0.014	0.0002
	(0.014)	(0.0002)
1(Implementation)$_{it}$	0.048***	-0.0002
	(0.013)	(0.0002)
Zip code FE	Yes	Yes
Year by month FE	Yes	Yes
City trends	Yes	Yes
ACS Controls	Yes	Yes
Observations	7,480	7,480
R-squared	0.935	0.999

*** p<0.01, ** p<0.05, * p<0.1.

Note. Robust standard errors clustered at the zip code level are shown in parentheses.

Table 9. The Policy Impacts on the Supply and Equilibrium Quantity in Rental Markets

	(1) log(Rental supply)	(2) log(Occupied rentals)
1(After policy)$_{it}$	0.009**	0.010***
	(0.004)	(0.004)
Geographic area FE	Yes	Yes
Year by quarter FE	Yes	Yes
City trends	Yes	Yes
Observations	5,120	5,120
R-squared	0.999	0.999

*** p<0.01, ** p<0.05, * p<0.1.

Notes.

1. Robust standard errors clustered at the geographic area level are shown in parentheses.
2. Because the two policy dummies are highly correlated in our quarter-level data, we combine them into one variable 1(After Policy). We define 1(After policy) to be one if 1(Announcement) equals one in New York City and San Francisco or if 1(Implementation) equals one for Portland. The sample spans from 2014 to 2017. The results are qualitatively similar if we use 1(Announcement) and 1(Implementation) separately.
3. The zip code-level ACS controls are not included in the estimation of data at the level of geographic areas.

quantity—occupied rental units—increased by about 1%. Because the increased transactions might actually drive up the rents, this result points to even stronger evidence that the policy impact on rents is driven by the increased supply. Relatedly, we would have worried more if the results otherwise suggest that the policy led to a decrease in the equilibrium quantity. In that case, we would not be able to differentiate the source of impact between the increased supply and the decreased demand. Also, the findings—similar magnitudes in the coefficients of policy impacts on the rental supply and equilibrium quantity—might be partially due to the difference in measurement (at the much less granular geographic area by quarter level) from our main sample.

5.2.3. Excluding Alternative Explanations

The evidence speaks to the mechanism behind the policy impacts on local residential markets, that is, the OHOH policy made hosts withdraw homes from Airbnb and put them back to local residential markets to the extent that the displacement has affected the price levels (rents and home values). Nevertheless, several other alternative explanations of the decline in rents and home values after the policy merit careful study. This section investigates these alternative explanations, and we find little evidence that they are empirically important drivers of the policy impacts.

First, one alternative explanation may be that the decrease in rents and home values are due to the periodic removal of properties by Airbnb instead of the rollout of the OHOH policy. Indeed, in its attempt to appease city legislators, Airbnb was reportedly to remove some properties that "do not reflect our (Airbnb's) vision for our community" in New York City and San Francisco from June 2015 based on "regular reviews" by Airbnb staff.[20] If our results are driven by the platform's alleged removal actions in New York City and San Francisco, not the account-blocking OHOH policy, then the removal actions should also have an impact on rents and home values similar to the policy. To test this conjecture, we conduct a placebo test by including the alternative event of Airbnb's alleged removal. Specifically, we add another indicator $1(\text{After2015m6})_{it}$ for New York City and San Francisco in Equation (1) to test the impact of these alleged removal actions. The results are presented in Table 10. As Columns (1) and (2) show, the rents and home values did not have a significant change after Airbnb implemented the alleged removals. Instead, the impact of the OHOH policy announcement and implementation continue to be statistically significant. These results suggest that the estimated impacts in Table 5 are due to the policy rather than events such as alleged removals before the policy.

Second, some may worry that other home sharing platforms, such as HomeAway and VRBO that are

[20] See the reports on Airbnb's removal starting June 2015 in New York City at https://2sqy5r1jf93u30kwzc1smfqt-wpengine.netdna-ssl.com/wp-content/uploads/2016/07/OneHostOneHomeNewYorkCity-1.pdf and in San Francisco at https://www.businessinsider.com/airbnb-vows-to-crack-down-on-illegal-hotels-in-san-francisco-2016-4.

Table 10. A Placebo Test of an Alternative Event

	(1) logZRI	(2) logZHVI	(3) logPrice2Rent
1(Announcement)$_{it}$	-0.014***	-0.009***	0.005
	(0.002)	(0.003)	(0.003)
1(Implementation)$_{it}$	-0.022***	-0.017***	0.004
	(0.003)	(0.003)	(0.004)
1(After2015m6)$_{it}$	-0.005	0.004	0.009**
	(0.004)	(0.004)	(0.004)
Zip code FE	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes
City trends	Yes	Yes	Yes
ACS Controls	Yes	Yes	Yes
Observations	13,986	13,986	13,986
R-squared	0.995	0.998	0.990

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Note. Robust standard errors clustered at the zip code level are shown in parentheses.

considered the second and third to Airbnb,[21] may follow Airbnb and implement similar policies during the same time period, which may make our estimation an overestimate of the effects. We first conducted a diligent search for the news about these two competing platforms and did not find similar policies. Also, if there happened to be such policies that contaminate our estimates, they would have to overlap precisely with the OHOH policy—in the same three cities (New York City, San Francisco, and Portland) at exactly the same time (the staggered rollout of the policy announcement and the policy implementation). We argue that there were unlikely such policies. Lastly, we check the number of properties in the ten sampled cities on HomeAway and VRBO (properties on other platforms are either overlapped or fairly negligible compared to these two) and find that the market share of Airbnb is over 90% in nearly all the sampled cities. The results are presented in Section A5 of the Internet Appendix. Therefore, the intervening impact of other platforms, if any, might be small and negligible in our estimates. Another related concern is that the removed Airbnb properties may have been displaced by hosts to other short-term rental platforms. We argue that if that is the case, our estimates

[21] VRBO and HomeAway—both owned by Expedia Group and display identical properties—are the second and third most popular home sharing services. Source: https://www.airgms.com/vacation-rental-sites/. Other platforms such as Booking.com and TripAdvisor.com also offer home sharing services, yet at a much smaller scale. Marriott reportedly plans to create a platform for home sharing, which may be a viable competitor to Airbnb in the future. Source: https://www.wired.com/story/airbnb-marriott-each-want-what-other-has/

would be a conservative estimation (an underestimate) of the policy impact on local residential markets, because our findings so far have demonstrated unanimously significant effects on supply and prices in the local markets.

5.3. The Intensity of the Policy Impacts

One may argue that two policy-affected cities in our sample, New York City and San Francisco, may be quite unique and generally have higher rents and home values than other cities in the sample. Even though the DID method only requires that the affected and unaffected zip codes have parallel rather than identical trends before policy, it is a valid concern whether our estimated policy impacts can generalize to other cities. In this section, we contemplate the generalizability of our results and also evaluate to what extent Airbnb affects local residential markets. Studies have emerged to quantify these impacts (Barron, Kung, and Proserpio 2018; Horn and Merante 2017; Sheppard and Udell 2016). Barron, Kung, and Proserpio (2018), in particular, estimate that a 1% increase in Airbnb properties in an average owner-occupied area leads to about a 0.018% increase in rents and a slightly larger effect on home values. The OHOH policy we study here provides a unique opportunity to estimate similar "elasticities." Instead of using an instrumental variable approach as they do, we rely on the staggered rollout of the OHOH policy. Estimating the DID specification in each city separately for generalizability is not reasonable because each zip code may have its unique market characteristics. Instead, we use the following specification:

$$\begin{aligned} Y_{it} = \beta_1 1(\text{Announcement})_{it} \times \log(\text{ML density})_i + \beta_2 1(\text{Implementation})_{it} \times \log(\text{ML density})_i \\ + \gamma' Z_{it} + \mu_t + v_i + \varepsilon_{it}, \end{aligned} \tag{2}$$

where the dependent variable is still logZRI, logZHVI, or logPrice2Rent for zip code i at month t. We introduce a moderator, $\log(\text{ML density})_i$, to measure the number of properties by multi-listing hosts per one thousand population in zip code i *before* the policy. Because the policy specifically aims for these properties by multi-listing hosts, the intuition is that in zip codes that have a higher multi-listing property density, the policy impacts are expected to be higher (in magnitudes). The coefficients β_1 and β_2 take the interpretation of the percentage change in Y (on average) after the policy announcement and implementation when the number of multi-listing property density increases by 1% in a zip code. In this sense, we interpret them as similar "elasticities" in the literature.

We report the regression results on rents in Table 11. Using the full sample, Column (1) shows that a 1% increase in ML density before the policy magnified the policy's negative impact on rents by about 0.06% (= 0.022 + 0.038). Columns (2) - (4) report the by-city results. In each of the regressions, we use the zip codes in one affected city along with all other unaffected cities. For example, the sample used in Column (2), the "New York City" regression, contains New York City and the other seven unaffected cities (excluding San Francisco and Portland). Note because Airbnb implemented the policy in Portland without announcement, Portland has only one policy dummy, 1(Implementation). We can observe that the marginal impact of 1% higher ML density on rents is about 0.052% in New York City, 0.123% in San Francisco, and 0.051% in Portland. Despite a relatively high marginal impact in San Francisco, the estimates are quite close between New York City and Portland.

Table 11. The "Marginal Effect" of Multi-listing Property Density on Rents

	(1)	(2)	(3)	(4)
DV: logZRI	Full Sample	New York City	San Francisco	Portland
1(Announcement)$_{it}$ * log(ML density)$_i$	-0.022***	-0.021***	-0.047***	
	(0.004)	(0.004)	(0.008)	
1(Implementation)$_{it}$ * log(ML density)$_i$	-0.038***	-0.031***	-0.076***	-0.051***
	(0.004)	(0.005)	(0.008)	(0.012)
Zip code FE	Yes	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes	Yes
City trends	Yes	Yes	Yes	Yes
ACS Controls	Yes	Yes	Yes	Yes
Observations	13,986	12,082	8,076	8,348
R-squared	0.995	0.995	0.997	0.996
Number of zip codes	417	361	241	249

*** p<0.01, ** p<0.05, * p<0.1.

Notes.

1. Because Airbnb implemented the policy in Portland directly, Portland has only one policy dummy, 1(Implementation).
2. Robust standard errors clustered at the zip code level are shown in parentheses.

The results seem reasonable, as the policy impact in widely spread and diverse zip codes of a large city like New York City and the zip codes in an average city like Portland likely speak more generalizability than the impact found in San Francisco which may have its specific trend. Further analyses also show that our main results are robust after excluding San Francisco. Additionally, the marginal effect of San Francisco becomes close to New York City and Portland after accounting for its specific trend after 2016. The detailed results can be found in Section A6 of the Internet Appendix.

We also report the results on home values in Table 12. We can see that the estimates for New York City (0.034%) and Portland (0.042%) are also relatively close, while the estimates for San Francisco are again relatively larger. The results on price-to-rent ratio are omitted because they continue to be insignificant.

The similar magnitudes of estimates from New York City and Portland suggest that our estimates of the marginal effect of ML density on local residential markets might be generalizable. A 1% increase in ML density before the policy may deepen the policy's negative impact on rents by about 0.05% and the home values by about 0.03% - 0.04%.

Table 12. The "Marginal Effect" of Multi-listing Property Density on Home Values

DV: logZHVI	(1) Full Sample	(2) New York City	(3) San Francisco	(4) Portland
$1(\text{Announcement})_{it}$ * $\log(\text{ML density})_i$	-0.007	-0.000	-0.041***	
	(0.005)	(0.006)	(0.014)	
$1(\text{Implementation})_{it}$ * $\log(\text{ML density})_i$	-0.036***	-0.034***	-0.050***	-0.042***
	(0.005)	(0.005)	(0.008)	(0.014)
Zip code FE	Yes	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes	Yes
City trends	Yes	Yes	Yes	Yes
ACS Controls	Yes	Yes	Yes	Yes
Observations	13,986	12,082	8,076	8,348
R-squared	0.998	0.998	0.998	0.998
Number of zip codes	417	361	241	249

*** p<0.01, ** p<0.05, * p<0.1.

Notes.

1. Because Airbnb implemented the policy in Portland directly, Portland has only one policy dummy, 1(Implementation).
2. Robust standard errors clustered at the zip code level are shown in parentheses.

5.4. Discussions: Comparability

In this section, we formally discuss and try to alleviate the concern of comparability. As we briefly mentioned earlier in Section 4, one may worry that the affected cities are different from the unaffected cities of our choice (based on a Euclidean-distance matching method). First, it is to note that our empirical tests are based on a zip code by year-month panel, not a city by year-month panel. Although the affected and unaffected cities are potentially different, we argue that it would not be surprising to find highly comparable zip codes in unaffected cities that mirror zip codes in affected cities. Second, as in most regressions, we include city-specific time trends to control for any unobserved city-level heterogeneity in terms of the overall trend in the local residential markets.[22] Last but not least, we conduct two more empirical tests using two other specifications. Specifically, we first combine the usual DID with a propensity score matching (PSM) method and report the findings in Section 5.4.1. Then, in Section 5.4.2, we utilize a synthetic control method (Xu 2017) that allows for multiple "treated" units (or zip codes in our case) to further alleviate the comparability concern and verify our main findings.

5.4.1. DID with Propensity Score Matching

We first use propensity score matching to construct a matched sample with similar attributes on all the observable measures for the affected and unaffected groups before the policy (Abadie and Imbens 2006). Specifically, we ran a logistic regression on the zip code level using the mean values of all observables to predict the probability of experiencing the policy. These variables include the Airbnb market size (in terms of the number of Airbnb properties) and economic and demographic characteristics (population, the number of households, the median household income, the unemployment rate, the home vacancy rate, and the fractions of the White, Hispanic, and female population).[23] In the matching process, New York City has 19 zip codes out of support. We also only keep one control zip code for each of the affected zip code. In the end, we have 181 zip codes from both the affected and unaffected cities. We present the balance check of covariates of the PSM sample in Table A7 in Section A7 in the Internet Appendix. After matching, the covariates are quite balanced. There is no significant difference in covariates between the policy affected and unaffected groups.

We then estimate the same model in Equation (1) on the PSM sample and report the results in Table 13. As Columns (1) and (2) show, the rents and home values decreased by 3.5% and 3.6% after the policy,

[22] For example, San Francisco has witnessed an overall trend in the growth of new constructions since 2011 according to the San Francisco Planning Department (see, e.g., their annual reports at https://sfplanning.org/project/san-francisco-housing-needs-and-trends-report).

[23] We have also tried to include the residential market price measures before the policy in the PSM process. The results are qualitatively similar, just with more zip codes from New York City out of support. We report the results in Section A7 in the Internet Appendix.

respectively. And the policy did not have a significant impact on the price-to-rent ratio, as presented in Column (3). Therefore, we find a consistently negative effect of the OHOH policy on both the rents and home values on our PSM sample. Moreover, the magnitudes of the estimates are even slightly higher in the PSM sample than our main results in Table 5. Hence, we keep the more conservative estimates in the main results.

Table 13. Replicating the Main DID Regressions using a DID with PSM Method

	(1) logZRI	(2) logZHVI	(3) logPrice2Rent
1(Announcement)$_{it}$	-0.015***	-0.015***	-0.000
	(0.003)	(0.003)	(0.004)
1(Implementation)$_{it}$	-0.020***	-0.021***	-0.001
	(0.003)	(0.004)	(0.004)
Zip code FE	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes
City trends	Yes	Yes	Yes
ACS Controls	Yes	Yes	Yes
Observations	12,162	12,162	12,162
R-squared	0.629	0.840	0.632
Zip codes	362	362	362

*** p<0.01, ** p<0.05, * p<0.1.

Note. Robust standard errors clustered at the zip code level are shown in parentheses.

5.4.2. A Synthetic Control Method

We further utilize a synthetic control method to alleviate the concern that zip codes in unaffected cities may not be a good "counterfactual" comparison to the zip codes in the affected cities. This method involves, for each affected zip code, the construction of a weighted combination of zip codes in unaffected cities to form a "synthetic control" zip code. Then, the constructed "control" zip codes are used to estimate what would have happened to the affected zip codes if they were not affected by the policy (Athey and Imbens 2017). Compared to the usual DID approach that essentially takes a simple average of all zip codes in unaffected cities, the main advantage of the synthetic control method is that it accounts for the effects of confounders changing over time by weighting the "control group" to better match the "treatment group" before the policy (Abadie, Diamond, and Hainmueller 2010). Because we have multiple policy-affected zip codes, we rely on a variant of the usual synthetic control method, a generalized synthetic control (GSC) method developed by Xu (2017), which allows for multiple "treated" units and estimates the average treatment effects on the treated (ATT) based on an interactive fixed effects model (Bai 2009).

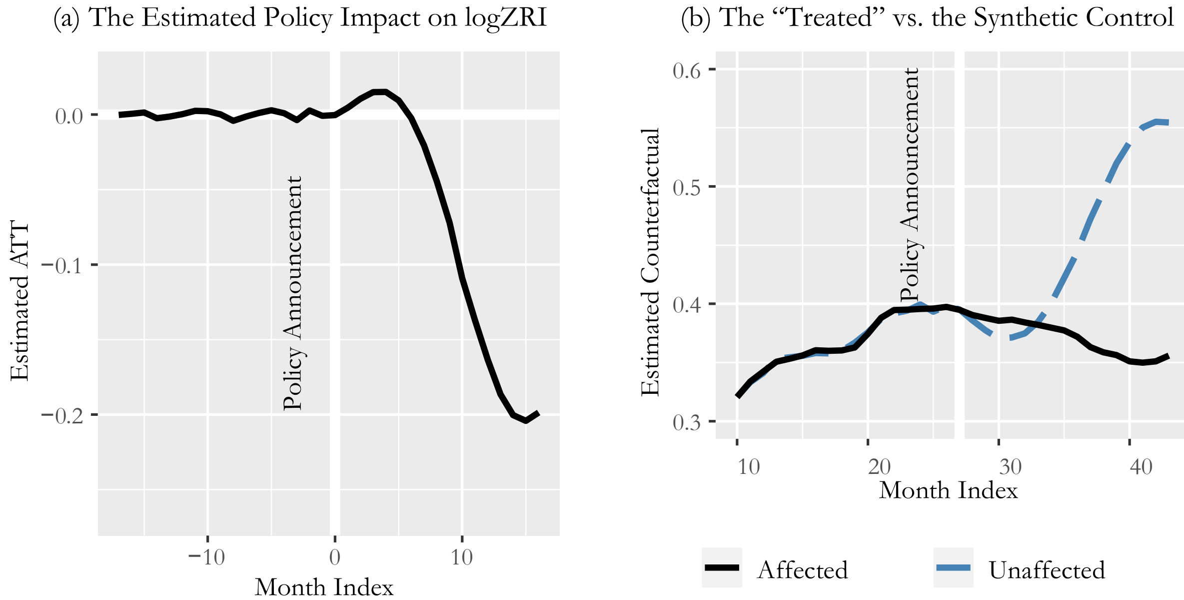
Figure 2 presents the findings of the policy impact on rents from the GSC method. We use only policy announcement that happened in New York City and San Francisco at the same time for a clear demonstration. Panel (a) illustrates the estimated coefficient of the policy announcement, where the x-axis is the relative months to the announcement (with zero being the announcement month) and the y-axis represents the difference between the affected and unaffected zip codes. The grey area in panel (a) is the 95% confidence interval. As Panel (a) shows, first, the difference between the affected zip codes and the synthetic control zip codes centers around zero before the treatment. In other words, the two groups are similar in terms of rents before the announcement. However, after the policy announcement, the rents in the affected zip codes start to decrease significantly compared with those in the synthetic control counterfactuals. Panel (b) illustrates the different policy impact between the affected and unaffected zip codes. We can see that the solid line, representing the affected zip codes, overlaps with the dotted line (for the synthetic unaffected zip codes) before the policy announcement but falls below the latter afterward, which reaffirms that the policy led to a reduction in rents in affected zip codes. Figure 3 shows the graph for the policy impact on home values based on the GSC method. We can see a similar pattern that verifies our main findings.

In summary, both the PSM results presented in Section 5.4.1 and the evidence from the GSC method in Section 5.4.2 suggest that our findings are less likely driven by the difference between the zip codes in policy-affected cities and those in unaffected cities. Instead, the estimated effects from these two sections are both qualitatively and quantitatively similar to our main results as presented in Table 5, which gives us more confidence in the comparability of the two groups and the robustness of our DID estimations.

5.5. Discussions: Heterogeneity

In this section, we study heterogeneous policy impacts by local residential market characteristics and by property type. Specifically, we are first interested in whether the structure of local residential markets plays a moderating role in the policy impacts and focus on the fraction of rental housing in a zip code. We also study (potentially) differential impacts of the policy on prices of different types of residential properties: (1) condos

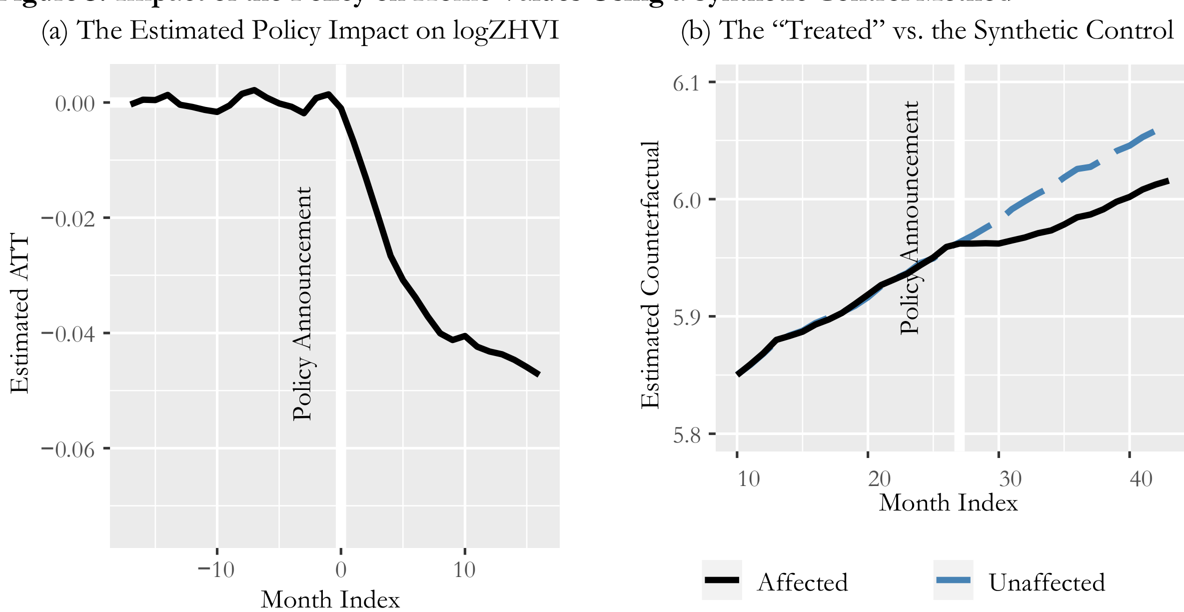
Figure 2. Impact of the Policy on Rents Using a Synthetic Control Method



Notes.

1. Panel (a) draws the gap plot—the average treatment effect on the treated (ATT) of the policy. Panel (b) depicts the counterfactuals for affected zip codes.
2. We use only policy announcement that happened in New York City and San Francisco at the same time for a clear demonstration.

Figure 3. Impact of the Policy on Home Values Using a Synthetic Control Method



Notes.

1. Panel (a) draws the gap plot—the average treatment effect on the treated (ATT) of the policy. Panel (b) depicts the counterfactuals for affected zip codes and others separately.
2. We use only policy announcement that happened in New York City and San Francisco at the same time for a clear demonstration.

and single-family residence properties in rental markets and (2) multi-family and single-family residence homes in housing markets.

5.5.1. Differential Policy Impacts by Fraction of the Rental Market

In a local area that has a larger fraction of rental housing (relative to other local markets), the mechanism of the policy impacts governs that the policy would have a greater hit on its rents, simply because a larger fraction of housing would be displaced back to or retained in the rental market in that area. By analogy, we predict that the policy would have a relatively smaller effect on home values in such areas with a larger fraction of rental housing, meaning that a smaller fraction of housing would be subject to the OHOH policy. We test these predictions using two approaches: (1) subsample analysis comparing zip codes with larger fractions of rental housing to others and (2) using the fraction of rental housing as a moderator of the policy impacts.

Table 14 reports the results. Both the subsample and moderator analyses lend support to our conjecture. Operationally, we code a zip code as having a larger fraction of rental housing if the zip code is among the top 50% in the rental percentage distribution in the data. Likewise, we define a zip code being a market with a smaller fraction of rental housing if it belongs to the bottom 50%. Columns (1) - (3) report the findings of the differential impacts on rents. Consistent with our predictions, the coefficient estimates in Columns (1) and (2) suggest that rents experienced a policy-driven drop by only 2.3% (announcement and implementation combined) in zip codes with a smaller fraction of rental housing, and in contrast, the policy led to an almost 4% drop in rents in zip codes with a larger fraction of rental housing. The regression with moderators in Column (3) speaks for a similar pattern. Columns (4) - (6) show the results on home values. Numerically, the policy led to about 3.2% decrease in home values in zip codes with a smaller fraction of rental housing, which is slightly larger in magnitude than the policy impact in zip codes with a larger fraction of rental housing (a 2.7% drop). The results from the moderator analysis in Column (6) support similar conclusions.

5.5.2. Differential Policy Impacts by Property Type

We further study the heterogeneity in the policy impacts by the type of properties. Zillow provides the rent and home value indices for differential types of residential properties. In the rental market, the main property types are multi-family residence housing (building with 5+ housing units) and single-family residence housing.

Table 14. The Differential Policy Impacts by the Fraction of Rental Housing

	DV: logZRI			DV: logZHVI		
	Subsample: % Rental			Subsample: % Rental		
	Lower	Higher	Moderator	Lower	Higher	Moderator
	(1)	(2)	(3)	(4)	(5)	(6)
$1(\text{Announcement})_{it}$	-0.012***	-0.011***	-0.006	-0.015***	-0.007	-0.020***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
$1(\text{Implementation})_{it}$	-0.011**	-0.027***	-0.010***	-0.017***	-0.020***	-0.013***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.006)	(0.004)
$1(\text{Announcement})_{it}$			-0.011**			0.014***
x $1(\text{Higher rental})_{i}$			(0.005)			(0.005)
$1(\text{Implementation})_{it}$			-0.017***			-0.011**
x $1(\text{Higher rental})_{i}$			(0.004)			(0.005)
Zip code FE	Yes	Yes	Yes	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes	Yes	Yes	Yes
City trends	Yes	Yes	Yes	Yes	Yes	Yes
ACS controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,034	6,952	13,986	7,034	6,952	13,986
R-squared	0.681	0.603	0.634	0.882	0.792	0.834
Zip codes	211	206	417	211	206	417

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Note. Robust standard errors clustered at the zip code level are shown in parentheses.

In the for-sale housing market, Zillow summarizes for each zip code by month the ZHVI for condos (defined as the condominium and co-operative homes) and single-family residence homes separately. Thus, the data allow us to explore whether there exist any differences in policy impacts on local residential prices for different property types.

Table 15 reports the subsample regression results by property type. We focus on the differential impacts on rents in Columns (1) and (2). The coefficient estimates suggest that, first, the policy had statistically significant impacts on the rents for both multi-family and single-family residence properties. Second, the policy had greater impacts on multi-family properties than on single-family ones, suggesting that, potentially, larger fractions of multi-family residence properties were displaced from the online channel to rental markets. Columns (3) and (4) report regression results of heterogeneous policy impacts on home values. We find that, despite similar negative impacts on the value of both condos and single-family residence homes, the policy seems to have slightly heavier influence on single-family residence homes. This indicates that single-family homes may have been more displaced to the housing markets.

Table 15. The Differential Policy Impacts by Property Type

	Rental property type		Housing property type	
	logZRI_MFR	logZRI_SFR	logZHVI_Condo	logZHVI_SFR
	(1)	(2)	(3)	(4)
1(Announcement)$_{it}$	-0.020***	-0.008***	-0.010***	-0.018***
	(0.002)	(0.002)	(0.003)	(0.002)
1(Implementation)$_{it}$	-0.025***	-0.011***	-0.017***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.002)
Zip code FE	Yes	Yes	Yes	Yes
Year x month FE	Yes	Yes	Yes	Yes
City trends	Yes	Yes	Yes	Yes
ACS controls	Yes	Yes	Yes	Yes
Observations	13,449	13,449	9,196	9,196
R-squared	0.995	0.994	0.997	0.998

*** p<0.01, ** p<0.05, * p<0.1

Note. Robust standard errors clustered at the zip code level are shown in parentheses.

6. Concluding Remarks

The progress in information technologies has given rise to a host of online marketplaces that facilitate transactions between buyers and sellers without traditional intermediations such as real estate brokers or agents. The lower transaction costs and search costs provide these platforms with advantages over traditional channels (Goldfarb and Tucker 2019). Such advantages are especially salient in local residential markets, in which home sharing services arise with the proliferation of Internet-related technologies. Suppliers in local residential markets, be they homeowners or absentee landlords, now have an alternative channel to allocate assets. Yet, the impact of home sharing on local residential markets is largely underexplored. In particular, because the emergence of these platforms leads to a natural concern that they might harm housing affordability, understanding the impact on local residential markets and the mechanism behind the impact is important both theoretically and practically.

We respond to the call by answering whether and how the home sharing platforms make rental and housing less affordable. We find that the "One Host, One Home" policy implemented by Airbnb drove down both rents and home values, and the impacts are quite comparable across the policy-affected cities, in particular between New York City and Portland. We further drill down to the mechanism of how the policy affected the prices in local residential markets. Our findings show that the policy led to supply increases in rental and housing markets, suggesting that the policy's impact on prices may be driven by excess supply in both markets—namely, the policy forced local hosts to put their properties back to the residential markets and retained future entries because of the "One Host, One Home" restriction. This mechanism prevails even after we take into consideration a few intervening events. Lastly, we identify some interesting heterogeneities in the policy impacts across markets with different characteristics and types of properties. One advantage of our study is that we utilize a handful of empirical methods (to cross validate our findings) that include, e.g., a standard DID as the main specification, a relative time model, DID combined with a PSM method, and a state-of-the-art synthetic control method that allows for multiple policy-affected zip codes as in our setting. Our main findings survive multiple specification tests, robustness checks, and placebo tests, consistently pointing to similar results.

We are among a few pioneers to explore the impact of home sharing on local residential markets (Sheppard and Udell, 2016; Horn and Merante, 2017; Barron, Kung, and Proserpio, 2018), yet the first and only to carefully explain the mechanisms behind the impact. Specifically, using a unique platform policy that caps the properties a host can manage on Airbnb, our findings point out the mechanism of how home sharing disrupted local residential markets—suppliers on rental and housing markets displace their properties to the online channel to an extent that the displacement has had a major impact on the supply in local residential markets, which in turn translated into the price levels (rents and home values). According to our knowledge, we are also the first to study how a platform initiated and implemented policy could affect the housing affordability amid many studies that predominantly focus on government regulatory policies (e.g., Valentin 2019). We show clearly that platforms have the ability to self-govern, if directed right by the legislators through, for example, requesting a system-facilitated policy. As platforms continue to rise, scale, and transform the offline markets, we suggest stakeholders of the platforms be mindful of unexpected societal impact (e.g., housing affordability studied in this paper) and proactively self-govern for goodwill.

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The Internet Appendix for "The Battle for Homes: How Does Home Sharing Disrupt Local Residential Markets?"

Section A1. A Summary of Empirical Tests

In this section, we furnish a table to present empirical tests conducted in this paper by purpose. As a roadmap, we begin with reporting the impacts of the policy (on rents and home values) by providing model-free evidence, estimates from standard DID specifications, and a battery of tests to validate the DID assumptions and to verify the robustness of our main results, including a relative time model, several tests regarding the "Ashenfelter's Dip," and a placebo test of randomized assignment.

We then drill down to the mechanisms behind the policy impacts by showing how the policy impacted the supply and/or equilibrium quantity on Airbnb and in local residential markets through model-free evidence and formal DID analyses. We also rule out several alternative explanations using another placebo test and supporting summary statistics. We further show the generalizability of our results across cities using extended DID analyses.

In order to alleviate concerns regarding the comparability, we compare characteristics between affected cities and unaffected cities, as well as formally reaffirm the comparability by combining a propensity score matching (PSM) method with DID and applying a Synthetic Control method that allows for multiple "treated" units in observational studies. Lastly, we provide additional evidence of heterogeneous policy impacts across heterogenous markets and by residence property type.

Table A1. A Summary of Empirical Tests

Purpose	Test	Location
Main estimation: Policy impacts	Model-free evidence comparing rents and home values pre- and post-policy by group	Table 4
	A difference-in-differences (DID) estimation	Table 5
Robustness checks of main results	A relative time model to validate the assumption of parallel trends	Table A2
	An "Ashenfelter's Dip" analysis to alleviate the concern of pre-treatment trend	Table A3
	A placebo test of the randomized assignment	Figure A1 Figure A2
Mechanisms behind the policy impacts	Model-free evidence comparing the residential supply pre- and post-policy by group	Table 6
	A DID estimation of the supply on Airbnb (properties by ML/SL hosts and total properties)	Table 7
	A DID estimation of the supply and equilibrium quantity in housing markets	Table 8
	A DID estimation of the supply and equilibrium quantity in rental markets	Table 9
Intervening events	A placebo test of the alleged Airbnb removal	Table 10
Intervening platforms	Summary statistics of the market share by platform and discussions on the conservative estimation of policy impacts	Table A4
Generalizability	A DID estimation of the ML intensity moderation on the policy impacts for the marginal effect on rents	Table 11
	A DID estimation of the ML intensity moderation on the policy impacts for the marginal effect on home values	Table 12
	Generalizability analyses using only NYC and Portland results to rule out the uniqueness of San Francisco	Table A5
	Generalizability analyses controlling the specific trend after 2016 in San Francisco	Table A6
Comparability	Model-free evidence comparing characteristics between affected and unaffected cities	Table 3
	DID estimations combined with propensity score matching (PSM)	Table 13 Table A9
	Balance checks for the DID-PSM estimation	Table A7 Table A8
	A generalized synthetic control (GSC) method allowing for multiple "treated" units in estimating policy impacts on rents	Figure 2
	A GSC method allowing for multiple "treated" units in estimating policy impacts on home values	Figure 3
Heterogeneity by market characteristics	A DID estimation using subsample analyses to compare the policy impact across markets with different fractions of rental housing	Table 14
	A DID estimation using the fraction of rental housing as a moderator	
Heterogeneity by residential property type	A DID estimation using subsample analyses to compare the policy impact across different types of residential properties	Table 15

Section A2. A Relative Time Model

One important assumption of the DID specification in Equation (1) is that the affected and unaffected zip codes should have parallel trends before the policies. We adopt the relative time model (Angrist and Pischke 2008) to check whether a parallel pre-policy trend exists for the affected and unaffected zip codes. The relative time model is specified as:

$$Y_{it} = \sum_{\tau=-T}^{N} \lambda_\tau D_{i\tau} + \gamma' Z_{it} + \mu_t + \nu_i + \varepsilon_{it}, \tag{A1}$$

where $D_{i\tau}$ is a dummy variable indicating whether month t is the τ month before (for negative τ's) or after (for positive τ's) the policy announcement in April 2016. Equation (A1) is similar to Equation (1), with just $1(\text{Announcement})_{it}$ and $1(\text{Implementation})_{it}$ replaced by a set of dummy variables $D_{i\tau}$ for the affected zip codes. Note that these dummy variables are equal to zero for all unaffected zip codes. The set of coefficients λ_τ can help us identify whether there exists a similar pre-policy trend in the affected and unaffected zip codes and how the policy impact changes over time at each phase.

We estimate Equation (A1) with T = 6 and N = 15 for our relative time model to inspect the trends from six before the announcement and eight months after the policy implementation. We also include a dummy for before six months, which is dropped because of collinearity and can be seen as the base for the estimates. Because zip codes in Portland have the policy implementation only after February 2017, we also excluded them from this analysis. All the other control variables (i.e., ACS controls, city trends, and zip code and month FE) are the same as the main specification.

As Columns (1) and (2) in Table A2 demonstrate, the rents and home values have no declining trends before the policy. After the announcement, we see a sudden and steady decrease in rents and home values (with the estimates significantly smaller than zero from three months after the announcement). After the policy implementation, we see that the rents and home values continue to drop towards the end of the sample. Meanwhile, as shown in Column (3), the coefficients λ_τ for price-to-rent ratio fluctuate around zero and are mostly not statistically different from zero. This finding is consistent with our main estimation in Table 5, where both the rents and home values decline after the policy announcement and drop further after the policy implementation, while the price-to-rent ratio has no significant changes.

Table A2. Results from a Relative Time Model

	(1) logZRI	(2) logZHVI	(3) logPrice2Rent
D_{-6}	0.005*	0.001	-0.004
	(0.003)	(0.004)	(0.004)
D_{-5}	0.009***	0.002	-0.007
	(0.003)	(0.004)	(0.004)
D_{-4}	0.011***	0.002	-0.009**
	(0.003)	(0.004)	(0.005)
D_{-3}	0.014***	0.006	-0.008
	(0.003)	(0.004)	(0.005)
D_{-2}	0.015***	0.009**	-0.006
	(0.003)	(0.004)	(0.005)
D_{-1}	0.013***	0.008*	-0.004
	(0.004)	(0.004)	(0.005)
D_{0}	0.008**	0.005	-0.004
	(0.004)	(0.004)	(0.005)
D_{1}	0.002	0.001	-0.001
	(0.004)	(0.005)	(0.006)
D_{2}	-0.005	-0.003	0.002
	(0.004)	(0.005)	(0.006)
D_{3}	-0.010**	-0.008*	0.002
	(0.004)	(0.005)	(0.006)
D_{4}	-0.015***	-0.011**	0.004
	(0.004)	(0.005)	(0.006)
D_{5}	-0.018***	-0.013**	0.005
	(0.005)	(0.005)	(0.007)
D_{6}	-0.018***	-0.016***	0.002
	(0.005)	(0.006)	(0.007)
D_{7}	-0.016***	-0.018***	-0.002
	(0.005)	(0.006)	(0.007)
D_{8}	-0.015***	-0.018***	-0.003
	(0.005)	(0.006)	(0.007)
D_{9}	-0.015***	-0.020***	-0.004
	(0.005)	(0.006)	(0.008)
D_{10}	-0.017***	-0.022***	-0.005
	(0.006)	(0.006)	(0.008)
D_{11}	-0.021***	-0.022***	-0.001
	(0.006)	(0.007)	(0.008)

(The table continues in the next page)

D_{12}	-0.027***	-0.024***	0.004
	(0.006)	(0.007)	(0.008)
D_{13}	-0.034***	-0.026***	0.008
	(0.006)	(0.007)	(0.009)
D_{14}	-0.039***	-0.028***	0.011
	(0.006)	(0.007)	(0.009)
D_{15}	-0.041***	-0.031***	0.010
	(0.007)	(0.008)	(0.009)
Zip code FE	Yes	Yes	Yes
Year x month FE	Yes	Yes	Yes
City trends	Yes	Yes	Yes
ACS controls	Yes	Yes	Yes
Observations	12,898	12,898	12,898
R-squared	0.594	0.816	0.610

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes.

1. In this table, we estimate Equation (A1) excluding zip codes from Portland because it had only the policy implementation and a different timeline.
2. Robust standard errors clustered at the zip code level are shown in parentheses.

Section A3. An "Ashenfelter's Dip" Analysis

One concern regarding the relative time results might be that both rents and home values seem to have a slight increase trend three months before the policy, even though the trend is quite parallel in 4-6 months before the policy.[24] In the literature, this pre-treatment trend is commonly termed the Ashenfelter's dip (Ashenfelter and Card 1985) in the difference-in-differences literature. We follow the common practice to correct for Ashenfelter's dip by excluding the sample periods around the policy shock. Specifically, we test the robustness of our results by excluding (1) the observations three months both before and after the policy announcement, and (2) only the observations three months before the announcement. By removing the pre-policy trends, the decrease in rents and home values should be smaller. Our results after correcting for Ashenfelter's dip are reported in Table A3. The coefficients indeed become smaller in magnitude compared to the main results in Table 5, but the change is very small. Meanwhile, both rents and home values still decrease significantly, and the change of the price-to-rent ratio is not significant.

Table A3. DID Results by Excluding Three Months Prior to the Policy Announcement

	(1) logZRI	(2) logZHVI	(3) logPrice2Rent	(4) logZRI	(5) logZHVI	(6) logPrice2Rent
$1(\text{Announcement})_{it}$	-0.003	-0.007	-0.004	-0.011**	-0.014**	-0.003
	(0.003)	(0.004)	(0.005)	(0.004)	(0.006)	(0.006)
$1(\text{Implementation})_{it}$	-0.015***	-0.017***	-0.002	-0.012***	-0.014***	-0.002
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Zip code FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x month FE	Yes	Yes	Yes	Yes	Yes	Yes
City trends	Yes	Yes	Yes	Yes	Yes	Yes
ACS Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,746	12,746	12,746	11,505	11,505	11,505
R-squared	0.995	0.998	0.989	0.994	0.998	0.989

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes.

1. We drop three months right before policy announcement in Columns (1) – (3) and three months both before and after policy announcement in Columns (4) – (6).
2. Robust standard errors clustered at the zip code level are shown in parentheses.

[24] The positive coefficients λ_τ also suggest that affected zip codes have higher rents than unaffected zip codes before the policy.

Section A4. An Extra Placebo Test

To ensure that it is the policy driving the results, we conduct another placebo test in which we randomly assign a subset of unaffected zip codes as if they were affected by the policy. Specifically, we focus on only unaffected zip codes in this test, and in each draw, we randomly treat 50% of those zip codes as if they were subject to the OHOH policy and replicate our main regressions to obtain the coefficients of the announcement and implementation dummy. We repeat this process multiple times in our simulation. Figures A1 and A2 depict the distributions of the estimated coefficients from simulations with 1,000 draws each. Figure A1 presents the coefficient for the announcement dummy, and Figure A2 displays the coefficient for the implementation dummy. As we can see from the figures, in all four cases, most of the mass (of the distributions) center around zero between -0.01 and 0.01 at worst. This suggests that all the estimated coefficients are not significantly different from zero (in a statistical sense) and implies that the "counterfactual" policy constructed in the simulations does not have any effects on either the rents or the home values. The results from this placebo test lend further support to our findings that it is the policy driving the differences in local market prices (between affected and unaffected zip codes).

Figure A1. A Placebo Test of Randomized Assignment—1(Announcement)

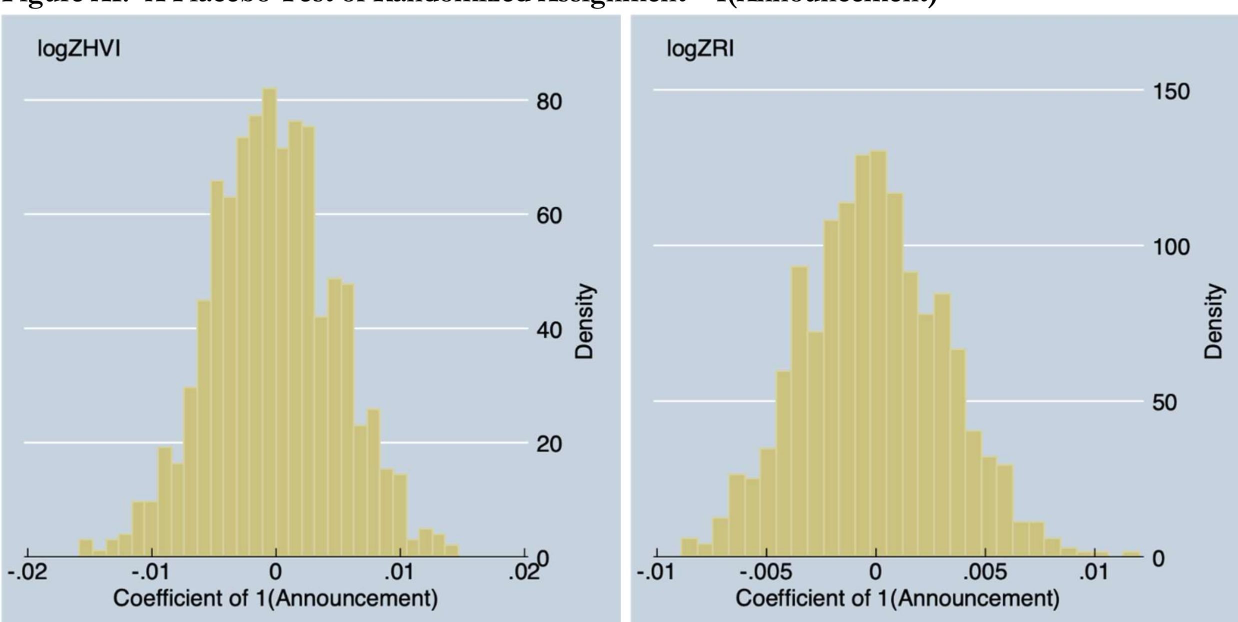
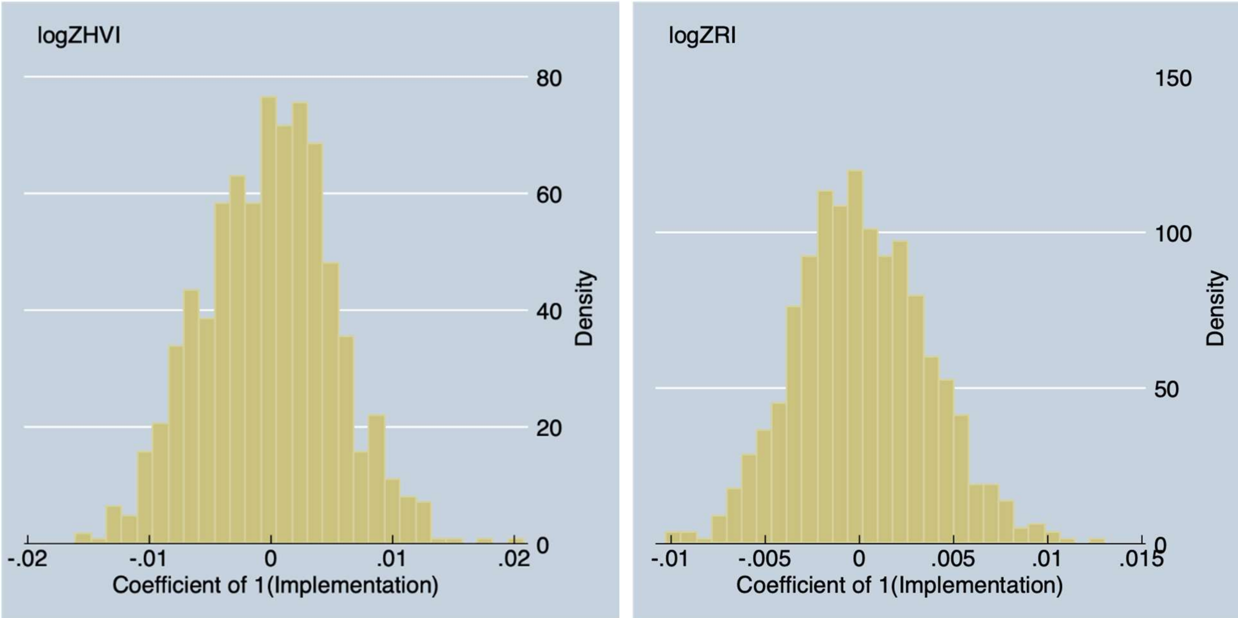


Figure A2. A Placebo Test of Randomized Assignment—1(Implementation)



Section A5. The Market Share by Home Sharing Platform

In this section, we compare the number of properties in the ten sampled cities on Airbnb, HomeAway and VRBO (properties on other platforms are either overlapped or fairly negligible compared to these two). Table A4 suggests that the share of Airbnb is over 90% in nearly all the cities sampled. Thus, the intervening impact of HomeAway, VRBO, and other platforms, if any, might be small and negligible and would not likely bias our estimation.

Table A4. The Market Share by Platform

City	Airbnb properties	HomeAway properties	VRBO properties	Airbnb as of the home sharing market
New York City	202,000	11,000	11,000	94.84%
Boston	11,200	1,600	1,600	87.50%
Philadelphia	21,000	900	900	95.89%
Washington DC	26,500	2,000	2,000	92.98%
Los Angeles	73,900	5,600	5,600	92.96%
San Diego	28,900	5,500	5,500	84.01%
San Jose	9,100	400	400	95.79%
San Francisco	36,700	2,700	2,700	93.15%
Seattle	22,200	3,000	3,000	88.10%
Portland	14,700	1,500	1,500	90.74%

Notes.

1. The estimate is rounded down to the nearest hundred/thousand of the total listings counts for each city.
2. All VRBO listings are on HomeAway as well by default since HomeAway acquired VRBO in 2006. Therefore, properties on HomeAway and VRBO are identical: https://www.tripadvisor.com/ShowTopic-g1-i10700-k9509653-Same_property_on_VRBO_Homeaway_and_FlipKey_Which_to_use-Timeshares_Vacation_Rentals.html

Section A6. Uniqueness of San Francisco

As we have shown in Section 5.3 of the main text, the policy seems to have a higher impact in San Francisco. We first examine whether our main results hold after excluding San Francisco. The results are presented in Table A5. We find that even though the magnitude slightly decreases, the main insights from our main results hold.

Table A5. The Policy Impacts on Local Residential Market Prices (without San Francisco)

	(1) logZRI	(2) logZHVI	(3) logPrice2Rent
$1(\text{Announcement})_{it}$	-0.004*	-0.003	0.001
	(0.002)	(0.003)	(0.004)
$1(\text{Implementation})_{it}$	-0.011***	-0.016***	-0.005
	(0.003)	(0.004)	(0.004)
Zip code FE	Yes	Yes	Yes
Year x month FE	Yes	Yes	Yes
City trends	Yes	Yes	Yes
ACS Controls	Yes	Yes	Yes
Observations	13,170	13,170	13,170
R-squared	0.643	0.842	0.629
Number of zip codes	393	393	393

*** p<0.01, ** p<0.05, * p<0.1.
Note. Robust standard errors clustered at the zip code level are shown in parentheses.

Moreover, we further explore what could have caused the higher policy impact in San Francisco. It seems that San Francisco may have experienced a big increase of housing supply that might have driven down the home values in 2016.[25] To address this concern, in regressions reported in Table A6, we add another time trend variable for San Francisco to control for this trend. Specifically, SF_trend_after2016 starts from 1 in January 2016, increases after that, and is zero before 2016. We can see that indeed the coefficients for this time trend after 2016 are negative and significant, suggesting that the increase of supply in San Francisco has driven down the price. Meanwhile, the marginal effects of ML density on rents and home values are still negative and statistically significant. When we evaluate the magnitude, a 1% increase in ML density will deepen the policy's

[25] See for example a news article that reports an influx of new constructions in 2016: https://www.bizjournals.com/sanfrancisco/news/2016/12/21/san-francisco-rent-decline.html.

impact on rents by 0.046% and the home values by 0.058%, which are quite comparable to the marginal effects in New York City and Portland (as in Table 11 and 12 in the paper).

Table A6. The "Marginal Effects" of ML Density for San Francisco

	(1)	(2)	(3)
	logZRI	logZHVI	logPrice2Rent
1(Announcement)$_{it}$ * log(ML density)$_i$	-0.037***	-0.037***	0.000
	(0.007)	(0.014)	(0.016)
1(Implementation)$_{it}$ * log(ML density)$_i$	-0.009	-0.021**	-0.012
	(0.007)	(0.009)	(0.010)
SF_trend_after2016	-0.014***	-0.006***	0.008***
	(0.001)	(0.001)	(0.002)
Zip code FE	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes
City trends	Yes	Yes	Yes
ACS controls	Yes	Yes	Yes
Observations	8,076	8,076	8,076
R-squared	0.997	0.999	0.992
Number of zip codes	241	241	241

*** p<0.01, ** p<0.05, * p<0.1.

Note. Robust standard errors clustered at the zip code level are shown in parentheses.

Section A7. Balance Checks of the PSM Sample

Following our PSM procedure in Section 5.4.1, we obtain a matched sample using the covariates as presented in Table A7. In the matching process, 19 zip codes from New York City are out of support, which leaves us with 181 zip codes in the affected group. We further keep only the nearest neighbor for each of the affected zip code, resulting in 181 zip codes in the control group. As shown in Table A7, the mean values of the covariates in the affected and unaffected groups are very close, with none of the pairs having a statistically significant difference. Note that we use only the average values of these covariates *before* the policy announcement for each zip code in the matching process.

Table A7. Balance Check of Covariates in the PSM Sample

	Unaffected Zip Codes			Affected Zip Codes			T-Test	
	Mean	SD	N	Mean	SD	N	diff	p-value
log(Airbnb Properties)	3.816	1.438	181	3.830	1.776	181	0.014	0.934
log(Population)	10.310	0.711	181	10.408	0.735	181	0.098	0.200
log(Households)	9.372	0.610	181	9.459	0.678	181	0.088	0.197
log(Med. income)	11.047	0.427	181	11.051	0.367	181	0.004	0.922
Unemployment rate	8.615	3.955	181	8.424	2.959	181	-0.191	0.603
Home vacancy rate	7.862	4.248	181	8.090	3.552	181	0.228	0.580
% White	41.170	24.377	181	40.065	22.632	181	-1.105	0.655
% Hispanic	15.276	15.263	181	15.038	10.979	181	-0.237	0.865
% Female	50.892	2.817	181	51.171	2.901	181	0.279	0.354

In the above matching process, we do not use the price indices (ZRI, ZHVI, and the price-to-rent ratio) in the matching process. We have also attempted to incorporate the logZRI, logZHVI, and logPrice2Rent in the matching process. While doing so, 42 zip codes from New York City are out of support. Following the same procedure as above, we have 158 zip codes both in the affected and unaffected groups. The balance check is presented in Table A8, which shows that the covariates are similar between the two groups *before* policy, even for the rents and home values. Table A9 reports the DID estimates of policy announcement and implementation on the new matched sample. Though the magnitudes of estimates are slightly different for rents and home values, we find a consistently negative impact in both rents and home values. Also, the estimated impacts, 3.4% and 2.5%, are close to our main results in Table 5, which gives us more confidence in our estimated impacts.

Table A8. Balance Check of Covariates and Price Indices in the PSM Sample

	Unaffected Zip Codes			Affected Zip Codes			T-Test	
	Mean	SD	N	Mean	SD	N	diff	p-value
logZRI	0.283	0.318	158	0.302	0.360	158	0.020	0.605
logZHVI	5.649	0.566	158	5.727	0.540	158	0.078	0.210
logPrice2Rent	2.881	0.278	158	2.940	0.234	158	0.058	0.044
log(Airbnb Properties)	3.903	1.458	158	3.912	1.732	158	0.008	0.964
log(Population)	10.314	0.717	158	10.378	0.744	158	0.064	0.438
log(Households)	9.369	0.628	158	9.440	0.683	158	0.071	0.339
log(Med. income)	11.048	0.453	158	11.066	0.381	158	0.018	0.695
Unemployment rate	8.515	3.801	158	8.352	3.071	158	-0.164	0.674
Home vacancy rate	7.617	4.217	158	8.234	3.865	158	0.617	0.176
% White	39.856	24.108	158	39.875	23.868	158	0.019	0.994
% Hispanic	16.225	15.231	158	15.517	11.510	158	-0.708	0.641
% Female	50.596	3.215	158	51.140	3.098	158	0.544	0.127

Table A9. DID with the PSM Sample Matched with Pre-Announcement Prices

	(1) logZRI	(2) logZHVI	(3) logPrice2Rent
$1(\text{Announcement})_{it}$	-0.013***	-0.012***	0.002
	(0.003)	(0.004)	(0.004)
$1(\text{Implementation})_{it}$	-0.021***	-0.013***	0.008*
	(0.004)	(0.004)	(0.005)
Zip code FE	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes
City trends	Yes	Yes	Yes
ACS Controls	Yes	Yes	Yes
Observations	10,628	10,628	10,628
R-squared	0.647	0.852	0.640
Zip codes	316	316	316

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Note. Robust standard errors clustered at the zip code level are shown in parentheses.