Journal of Urban Economics 124 (2021) 103356

Contents lists available at ScienceDirect

Journal of Urban Economics

journal homepage: www.elsevier.com/locate/jue

Short-term rentals and the housing market: Quasi-experimental evidence from Airbnb in Los Angeles☆

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A R T I C L E I N F O

JEL classification:
R21
R31
Z32

Keywords:
Short-term rentals
Souse prices
Regulation
Supply effects
Externalities

A B S T R A C T

Online short-term rental (STR) platforms such as Airbnb have grown spectacularly. We study the effects of regulation of these platforms on the housing market using a quasi-experimental research design. 18 out of 88 cities in Los Angeles County have severely restricted short-term rentals by adopting Home Sharing Ordinances. We apply a panel regression-discontinuity design around the cities' borders. Ordinances reduced listings by 50% and housing prices by 2%. Additional difference-in-differences estimates show that ordinances reduced rents also by 2%. These estimates imply large effects of Airbnb on property values in areas attractive to tourists (*e.g.* an increase in house prices of 15% within 2.5km of Hollywood's Walk of Fame).

1. Introduction

Short-term housing rentals (STRs) have become very important due to the rise of online STR-platforms, such as Airbnb, which provide opportunities for households to informally offer accommodation to visitors. The surge in popularity of STR-platforms has led to substantial opposition because of a decrease in housing affordability (Samaan, 2015; Sheppard and Udell, 2016), unfair competition, and illegal hotelization (CBRE, 2017). Negative externalities (*e.g.* noise, reduction in perceived safety) due to the presence of tourists in residential buildings are also frequently mentioned (see *e.g.* Lieber, 2015; Williams, 2016; Filippas and Horton, 2018).

Local governments around the globe have responded quite differently towards regulating STRs. Most cities have not significantly regulated these platforms, but a limited number of cities have recently put severe restrictions in place. Berlin, for instance, requires STR-hosts to occupy the property for at least 50% of the time (O'Sullivan, 2016). San Francisco imposes a 14% hotel tax (*i.e.* a *Transient Occupancy Tax*) and a cap of maximum 90 rental days per year (Fishman, 2015). Amsterdam even imposes a maximum cap of 30 rental days per year as of 2019.

In this paper, we aim to measure the impact of Airbnb, by far the largest STR-platform, on housing markets. We focus on the effects of policies that restrict the market for STRs. There are arguably three main mechanisms of how regulation of short-term renting impacts property markets:

1. *Efficient use effect*. Short-term rentals generate income from idle space, increasing value due to additional income opportunities. Moreover, residential properties can now be used by their most profitable use (*i.e.* by short-term renters). This should be an efficiency gain that spurs housing demand, which increases house prices (see *e.g.* Turner et al., 2014).
2. *Rental housing supply effect*. Short-term rentals may in turn lead to a reallocation of existing housing stock away from the long-term rental market towards privately-owned housing, which increases rents (see *e.g.* Quigley et al., 2005).
3. *Externality effect*. Short-term rentals may create negative nuisance externalities, lowering nearby property values. If neighbors fear turnover or unfamiliar people in their neighborhood, this may reduce demand for housing (see *e.g.* Filippas and Horton, 2018).

☆ We thank Jan Brueckner, Guillaume Chapelle, David Gomtsyan, Eric Koomen, Robert Elliott, Stephen Sheppard, Mariona Ségu, as well as the seminar audiences at the Higher School of Economics (St. Petersburg), the Southwestern University of Finance and Economics (Chengdu), the 13th Meeting of the Urban Economic Association (New York), University of Birmingham, Paris School of Economics, and Zhejiang University (Hangzhou) for useful comments. Koster acknowledges the support of the HSE University Basic Research Program.

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https://doi.org/10.1016/j.jue.2021.103356
Received 1 September 2020; Received in revised form 1 April 2021
Available online 15 May 2021

To identify the effects of short-term housing rentals regulation on the housing market, we exploit exogenous variation provided by the implementation of so-called Home-Sharing Ordinances (HSOs) in Los Angeles County. 18 out of 88 cities implement regulations that essentially ban *informal* vacation rentals; hosts renting out entire properties are now subject to the same formal regulations as regular hotels and bed and breakfasts. Short-term home-sharing is not always prohibited, albeit restricted in those cities.

There are several reasons why we focus on Los Angeles County. First, it is an area that is attractive to tourists and has thousands of listings on Airbnb. It is in the global top 10 of the cities with the most Airbnb listings and is the second most popular Airbnb city in the US after New York. Second, there is substantial spatio-temporal variation in the implementation of HSOs within this county. For example, HSOs have been implemented in cities that receive many tourists (*e.g.* Santa Monica), as well as in cities that are more at the edge of the Los Angeles Conurbation (*e.g.* Pasadena). We think this might add to the external validity of the results shown in the paper. Third, by focusing on 18 cities, rather than on the introduction of an HSO in one single city, we substantially reduce the likelihood that our results are contaminated by an unobserved event (*e.g.*, a change in a city-specific policy) that occurs around the same time as the introduction of the HSO. Fourth, in Los Angeles County, in contrast to for example New York, renters are (usually) not allowed to list a property on Airbnb (Lipton, 2014).[3] This facilitates the interpretation of the distributional consequences of our results: renters generally lose from Airbnb-induced higher rents (and hardly benefit from the opportunity of subletting to short-term renters).

The variation in restrictions between cities enables us to use a spatial regression-discontinuity design (RDD), which we combine with a difference-in-differences (DiD) set-up: we essentially focus on *changes* in the number of Airbnb listings, as well as in house prices, close to the borders of cities that have implemented HSOs. More specifically, we use micro-data on Airbnb listings and house prices between 2014 and 2018. Our main results are then based on observations within approximately 2 km of borders of HSO areas.

By applying the Panel RDD we identify the first effect – the efficient use effect – which is arguably the key mechanism to explain the effects on house prices. Conditional on local area fixed effects, properties close to the border of an area where an HSO is implemented are otherwise identical, except that in one area short-term renting is restricted. Theory then indicates that there is a discrete decrease in house prices at HSO borders because houses within a treated area offer less value to homeowners.

One potential issue with the Panel RDD approach is that by comparing house prices (as well as listings) in two neighboring cities – one which implemented an HSO and the other which did not implement an HSO – substitutability between houses on the two sides of the city border may inflate the effect of the HSO implementation. We provide a range of statistical tests which all show that this 'manipulation' is non-existent. The economic intuition for the absence of manipulation is that tourist demand tends not to be very local (*e.g.*, tourists are indifferent between locations which are a couple of minutes drive from each other), so tourist accommodations compete with each other over long distances. Hence, given an elastic demand function for tourist accommodation, there is no incentive to move listings just across the border.[4]

Short-term rental platforms also reduce housing supply available for local (long-term) rental markets, which increases rents (Hilber and Vermeulen, 2016) – the rental housing supply effect. When the expected economic returns on rental and privately owned properties are the same, then the housing supply effect estimated in the rental market should be the same as the efficient use effect (estimated using house prices).[5] We cannot measure the rental housing supply effect by applying a Panel RDD for rents, because properties that are next to each other, but on different sides of the HSO border experience identical changes in housing supply and offer the same value to renters (see Glaeser and Ward, 2009). This implies that there should be no discrete jump at HSO borders for rents.[6] To capture the rental housing supply effect we employ an alternative strategy: we use ancillary data on aggregate rents for zip codes and a DiD estimation strategy, while we focus on properties further away from the HSO borders. The DiD approach relies on more restrictive identifying assumptions than the Panel RDD approach. We assess the validity of the DiD approach by applying the same approach to house prices, finding very similar effects as with the Panel RDD approach.

We also test for the third effect – the externality effect – by investigating the price change of properties outside HSOs but close to areas where HSOs have been implemented. Many papers find that housing market spillovers are very local (see *e.g.* Linden and Rockoff, 2008; Autor et al., 2014; Fisher et al., 2015; Pope and Pope, 2015; Ahlfeldt and Holman, 2018; Diamond and McQuade, 2019; Koster and Van Ommeren, 2019). We therefore also test for differences between the effects of Airbnb on prices, while distinguishing between condominiums and single-family homes. One expects that local externalities are particularly important for condominiums so if the effect of Airbnb on condominium prices would be lower this means that an external effect could be present. We do not find evidence that the externality effect is important for LA County.

We obtain two main results. Our first result is that HSOs are very effective in reducing Airbnb listings. The ordinances *strongly* reduced the number of Airbnb listings of entire properties and rooms by about 50% in the long run. We further show that room listings have not been reduced when offering rooms is still allowed, which is the case in 4 out of the 18 cities with HSOs. Our second result is that the HSO reduced house prices and rents by about 2% on average. This effect is robust to a wide range of placebo-tests and specification choices. Hence, the decision to implement an HSO is a political one, with a clear group of winners and losers, and strong distributional effects: owners lose from HSO-induced house price reductions, whereas (long-term) renters benefit from lower rents.

Our setup allows us also to estimate the effect of Airbnb demand on the housing market. Causal inference of this effect is not straightforward, as Airbnb listings are concentrated in central areas that are also attractive to residents. Hence, one is predisposed to find a strong positive correlation between Airbnb listings and house prices or rents. We estimate the effect of Airbnb demand on housing prices using an IV approach. We measure demand using the Airbnb listings rate – the share of listings to the number of housing units. Using HSOs as supply-shifting instruments for the listings rate around the border, we show that short-term rental demand for accommodation increases prices of residential properties – a standard deviation increase in the Airbnb listings rate increases prices by 5.5%. Using the DiD estimation strategy, we further show that rents decrease by about the same amount as house prices, likely because of the reduced supply of rental housing.

We then show that Airbnb implies modest property value increases for LA County as a whole: the total average property value increase due to Airbnb since 2008 is 3.6%. However, this masks the fact that a large part of LA County is not very urbanized and does not attract tourists. By contrast, the effects of Airbnb on the housing market can be large

[3] The extent of illegal subletting is unknown, but note that the host is always responsible for breaking the law, rather than Airbnb (Petterson, 2018). This strongly reduces the benefits of illegal subletting because of hefty fines and potential lawsuits.

[4] In line with this line of reasoning, we will show that prices per night for Airbnb accommodations are not affected by HSOs. The latter suggests that the market for short-term rentals is highly competitive and that tourist demand for local accommodation is highly elastic.

[5] However, note that the effects of short-term rentals on house prices may be different from those on rents in the short run, because house prices may include anticipation effects towards future changes in policies. However, we do not find evidence for this.

[6] A Panel RDD analysis of rents confirms the absence of a discontinuity in rents.

in central urban areas – within 2.5km of Hollywood's Walk of Frame, property values have increased by almost 15% due to Airbnb. Within 2.5km of beaches, prices have increased by 5.8%.

Related literature. In recent years, the *sharing economy* has received increasing attention. Economists have examined home sharing from various angles such as racial discrimination in the online marketplace (Edelman et al., 2017; Kakar et al., 2016), negative externalities of tourism (Van der Borg et al., 2017; Gutiérrez et al., 2017) and its effects on the hotel industry (Zervas et al., 2017). This is not the first empirical study on the effect of short-term rentals on the housing market. Sheppard and Udell (2016) conclude that housing values increased by about 31% due to Airbnb. Horn and Merante (2017) show that a high Airbnb density increases asking rents by $1.3 - 3.1\%$. Barron et al. (2021) show that Airbnb listings increase house prices and rents in US cities.[7] Garcia-López et al. (2018) also report a positive effect on rents in Barcelona. Almagro and Domínguez-Iino (2020) develop a structural model for Amsterdam in which Airbnb is used as a shock in consumption amenities. They find that a 10% increase in listings increases rents by 0.5%. Identification of the model parameters relies on a particular structure of the unobserved component (*i.e.* an ARMA structure). A few reports (NYCC, 2015; Samaan, 2015; Lee, 2016; Wachsmuth and Weisler, 2017) – which essentially rely on correlations – have studied the impact of Airbnb as well. In contrast to these studies, we study the effect of regulation of Airbnb itself, which is of key policy interest. In addition, we exploit quasi-experimental variation provided by changes in regulation to estimate the effect of Airbnb on the housing market.

Our paper also relates to a literature studying the effects of tourism and amenities on housing markets. Carlino and Saiz (2008), for example, show that the number of tourists visiting a city is a good predictor of the growth of US metropolitan areas in the 1990s. Ahlfeldt et al. (2017) and Gaigné et al. (2018) find that the density of pictures taken by tourists and residents increases the land value and attracts the wealthy. Moreover, a large number of papers show that high amenity locations have higher housing values (see *e.g.* Van Duijn and Rouwendal, 2013; Ahlfeldt and Kavetsos, 2014; Koster and Rouwendal, 2017). In these studies, it is impossible to disentangle the effects of tourism and amenities. An exception is a recent paper by Faber and Gaubert (2019), which shows that tourism generates substantial local and national economic gains driven by spillovers on manufacturing and national integration respectively. Our paper, therefore, contributes to this literature by using a quasi-experimental research set-up, enabling us to isolate the effects of tourism demand, proxied by Airbnb listings.

Conceptually, our paper is close to a literature measuring the effect of land use regulation and zoning, as the HSO can be seen as an example of a zoning regulation. Most studies in this field show that housing supply constraints are associated with increasing housing costs, a strong reduction in new construction, and rapid house price growth (Glaeser et al., 2005; Green et al., 2005; Ihlanfeldt, 2007; Hilber and Vermeulen, 2016). However, they do not identify the underlying mechanisms that lead to price increases. Glaeser and Ward (2009) find that local constraints do not increase the price between localities, because areas that are geographically close are reasonably close substitutes. Using a spatial regression-discontinuity design, Koster et al. (2012), Turner et al. (2014) and Severen and Plantinga (2018) also study the local effects of regulation and find that the effects of regulation for homeowners may be up to 10% of the housing value. One major difference with these studies (except for Severen and Plantinga, 2018) is that our research design does not rely on cross-sectional variation in land use regulation, but rather identifies the effect based on changes in regulation over time.

Finally, our paper is related to a large literature on housing regulation, including rent-controlled or public housing (Olsen and Barton, 1983; Fallis and Smith, 1984; Moon and Stotsky, 1993; Glaeser and Luttmer, 2003; Anderson and Svensson, 2014; Autor et al., 2014), and affordable housing (Quigley and Raphael, 2004; Diamond and McQuade, 2019; Koster and Van Ommeren, 2019). In this literature, it is common to study a policy where a fixed share of houses is regulated to help poor households. Regulation creates then an efficiency effect as well as a housing supply effect. Studies typically focus either on the efficiency effect (see Glaeser and Luttmer, 2003; Anderson and Svensson, 2014) or the housing supply effect (see Fallis and Smith, 1984). In contrast to the existing literature, we study a regulation type that induces efficiency and housing supply effects for the full housing market, rather than a sub-segment of the market. Recent studies also explicitly take into account spillovers of providing subsidized housing and find that these spillovers are very local (see Autor et al., 2014; Diamond and McQuade, 2019).

This paper proceeds as follows. In Section 2 we discuss the research context. Section 3 introduces the data and provides descriptives. In Section 4 we elaborate on the identification strategy, followed by graphical evidence in Section 5. We report and discuss the main results in Section 6, which is followed by Section 7 studying the overall price effects. Section 8 concludes.

2. Context

2.1. Airbnb in Los Angeles County

In 2007, Brian Chesky and Joe Gebbia came up with the idea of putting an air mattress in their living room and turning it into a bed and breakfast, marketed through an online platform (Lagorio-Chafkin, 2010). The website – later called Airbnb and officially launched in 2008 – is a platform that connects hosts that own accommodation (rooms, apartments, houses) with guests seeking temporal accommodation. Prospective hosts list their spare rooms or entire apartments for a self-established price and offer the lodging to potential guests.[8] Airbnb charges a fee to both the host and guest.

Airbnb has grown rapidly since its launch in Los Angeles County (as in other major cities across the globe), with now more than 40 thousand listings. 60% of those listings are entire properties (Inside Airbnb, 2017).[9] Fig. 1 clearly shows that Airbnb listings are heavily concentrated in popular areas like Venice, Santa Monica, Hollywood, and Downtown LA. Nevertheless, we also record many listings in areas that are further away from the center (*e.g.* Pasadena, Hermosa Beach).

Many cities around the world have imposed some form of regulation, *e.g.* by requiring hosts to register their STR activities with the local authorities. However, an increasing number of cities also explicitly restrict short-term housing rentals, which are defined as lettings of up to 30 consecutive days. Cities that impose so-called Home-Sharing Ordinances (HSOs) typically distinguish between two types of *informal* STRs: 'rooms', whereby at least one of primary residents lives on-site through-

[7] Barron et al. (2021) focuses on US cities and applies a difference-in-differences strategy using an instrument based on both the popularity of Airbnb and how touristy an area is to address endogeneity issues. One criticism of this approach is that high-amenity areas, in particular US inner cities, have both attracted tourists and residents in recent decades (see Couture and Handbury, 2019). Reassuringly, our estimates are of a similar order of magnitude as Barron et al. (2021), despite the differences in identification strategy and focus.

[8] With more than 4 million listings – more properties than the top 3 hotel brands, Marriott, Hilton, and IHG, combined (Airbnb, 2017) – Airbnb emerged as one of the main figureheads of the sharing economy, in which technology companies disrupt well-established business models by facilitating direct, peer-to-peer exchanges of goods and services (Lee, 2016).

[9] According to Airbnb, it generated \$1.1 billion in economic activity in the City of Los Angeles. Its typical host earned \$7,200 per year from hosting and it helped 13% of its hosts to save their home from foreclosure and another 10% from losing their home to eviction (Airbnb, 2016; Inside Airbnb, 2017).

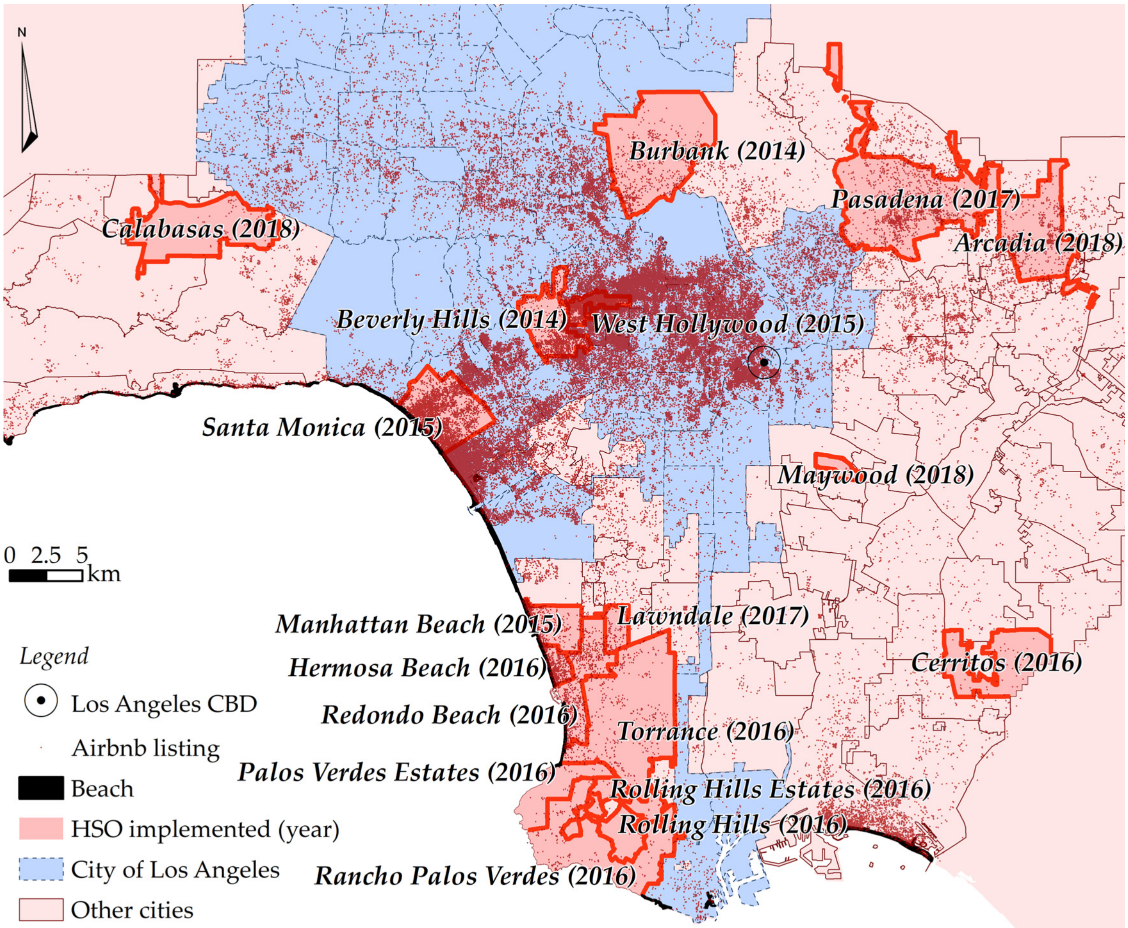


Fig. 1. Airbnb in Los Angeles County.

out the visitor's stay, and 'entire properties', which are for the exclusive use of the visitor.

In Fig. 1 we show the names of 18 cities that have imposed HSOs during our study period 2014–2018. The other 60 cities – including the largest one, the City of Los Angeles – did not impose regulations in this period.[10] These 18 cities, which contain close to 5 percent of the whole housing stock of this County, essentially ban informal vacation rentals by requiring hosts to have a business license and comply with health and safety laws, as well as levying a Transient Occupancy Tax on the listing price (up to 15%). Most cities completely ban short-term letting of both rooms and entire properties. 4 out of 18 cities (Calabasas, Pasadena, Santa Monica, and Torrance) still allow for room rentals, although restrictions apply. In Santa Monica, for example, the HSO allows for room rentals up to 30 days per year but prohibits hosts to operate more than one room listing at the same time. The HSOs in LA County are usually enforced. For example, the City of Santa Monica has collected more than $4.5 million in taxes from Airbnb and other short-term home rental businesses and has fined hosts violating the law for $80,000. Listings that are operating illegally may be issued fines of up to $500 per day and face criminal prosecution if they do not cease operations (City of Santa Monica, 2017).[11] In Appendix A.1 we report for each city in LA County more details regarding STR regulation.

Our estimated effect of HSOs on house prices, but not on rents, may potentially depend on future regulation changes. It seems plausible that some economic actors anticipate the introduction of future HSOs in cities that currently have none, which may affect house prices. This raises the question of whether our study captures the permanent effect of HSOs. Because we do not find evidence for anticipation effects in Section 6.2, it is plausible that the estimated effects can be interpreted as coming from permanent changes. Furthermore, if anticipation effects are present, then we would find an underestimate of the effect of the HSO on house prices. Note that we are aware of only one fundamental future change in regulation after the period analyzed by us, which is for the City of Los Angeles. This city announced in December 2018, so approximately half a year after the period for which we observe house prices, that it will introduce an HSO in July 2019 (so about 18 months after the period for which we observe house prices). It is very unlikely that this future HSO has affected house prices, also because this HSO is less restrictive than the HSOs introduced in the 18 cities analyzed by us (*i.e.* it restricts the maximum number of yearly rental days to 120, which is hardly restrictive).

Our empirical approach relies on the fundamental assumption that around the implementation of the HSOs other policies did not change

[10] In 45 cities, short-term renting is technically illegal, because it is not mentioned in the residential housing code. However, in phone interviews undertaken by the authors, local officials state that nothing is done to enforce the residential housing code and to prevent homeowners to list their properties on Airbnb. This appears to be common knowledge. We make sure that listings in those 45 cities are not lower compared to other places (see Section 6.3).

[11] Note that our estimates of the HSOs reflect the actual levels of enforcement of the cities investigated in Los Angeles County. For example, it is plausible that the effects on the number of listings as well as property prices are higher in cities where enforcement is more strict.

in the 18 cities compared to their immediate surroundings. We are not aware of such policy changes (but have actively searched for this) and offer statistical support for this assumption. One may argue that also weaker conditions may violate the main identifying assumption. For example, there may be differences in unobserved factors that might affect house price *growth*.[12] As these unobserved factors should be discrete at the spatial border, and because we focus on a relatively short study period, these factors are unlikely to play a major role. This is particularly so because we do not observe pre-trends in house prices or listings, respectively, once focusing on areas close to the borders of cities. In Section 6.3 we further perform a range of placebo tests using the information on price changes around the borders of other sets of cities and using the same borders but in other periods. All these tests indicate that there are no changes in listings and prices at the placebo borders investigated. This makes it implausible that other policies or other unobserved factors (or *e.g.* differences in school quality) changed exactly around this period.[13]

3. Data and descriptives

3.1. Data

We employ Airbnb listings data obtained from web scrapes for 15 different months from the websites www.insideairbnb.com between October 2014 and September 2018 for Los Angeles County. We double-check these data with data on listings from www.tomslee.net.[14] LA County is the most populous county in the United States (more than 10 million inhabitants as of 2018). We know the location (up to 200m) and whether a property is listed in one of the 15 months of observation.[15] For the analysis where we analyze the effects of HSOs on listings, we construct a panel dataset of all accommodations that have been listed at least once between 2014 and 2018. We create a variable that equals one when the accommodation is listed in a certain month. We refer to Appendix A.1 for more details.

We also use micro-data on housing transactions, obtained from the Los Angeles County Assessor's Office. The data provides information on sales prices and a range of property characteristics (*e.g.*, condominium, single-family home, construction year) for all transacted residential properties. We focus on transactions from January 2014 until early 2018, as these match closely to the period our Airbnb data refers to. Ancillary data on properties' locations, exact building locations, and neighborhood characteristics are obtained from Los Angeles County's GIS Data Portal. We disregard extreme outlier observations and transactions with missing information on either prices or property size or type (condominium or single-family home), as well as transactions referring to multiple parcels or units.[16]

For the analysis of the effect of Airbnb demand on house prices, there are two technical issues when matching listings data to house prices. First, the data on listings are based on 15 snapshots during our study period. Second, we do not have information on listings from January to October 2014. We deal with both issues by constructing an *imputed* measure which imputes the listing probability based on the nearest two dates for which we have information.[17]

To capture Airbnb demand, we use the Airbnb listings rate – defined by the number of listings divided by the number of housing units – within 200m of each property.[18]

We further gather monthly data on listed median rents and house prices *at the zip code level* from *Zillow*, which is a large real estate database company.[19] *Zillow* has micro-data on over 110 million homes across the United States; not just those homes currently for sale but also for rent. For each zip code in each month, *Zillow* posts the median listed rent and median listed sales price. For LA County, we have information on 114 (out of 311) zip codes.

In the econometric analysis, we will also distinguish between geographical areas within the County of Los Angeles. An area is defined by us as a City; or a neighborhood within the City of Los Angeles (which is by far the largest city); or a so-called 'unincorporated' area. In total, we have 252 areas.

3.2. Descriptives

Table 1 reports the main descriptive statistics for the Airbnb listings. We observe that, on average, rental prices per night in areas where HSOs are implemented are somewhat higher than in other areas. Hence, the HSOs are predominantly implemented in areas where there is more demand for tourist accommodation. In other observable characteristics, such as accommodation size, the number of reviews, and the share of entire properties, listings in HSO areas seem to be similar to listings in other areas. The most notable difference is that the distance to the beach is lower in areas where HSOs are implemented, as several beach towns, such as Santa Monica, Manhattan Beach, and Redondo Beach, have implemented HSOs.[20]

Fig. 2 provides information about changes in the number of listings over time (for the exact number of listings per wave, we re-

[12] City borders may sometimes intersect with natural features (*e.g.* canyons or rivers). These natural features are unlikely to cause changes in price growth because preferences usually do not change within a short time frame.

[13] This conclusion is supported by the absence of differences in (changes in) public good provisions between cities that are known to affect house prices. See for evidence on school quality Appendix A.3.2.

[14] Airbnb is not the only STR-platform available to prospective hosts. This is unlikely problematic because hosts who consider using other platforms are likely also to use Airbnb, which is the dominant platform, as the cost of advertising on Airbnb is negligible. According to www.beyondpricing.com, *HomeAway* – Airbnb's most important competitor – had 3578 listings in Los Angeles in 2016, while Airbnb had 8367 listings (which is less than observed in our data). Data on individual *HomeAway* listings is not available to us.

[15] Note that a listing always refers to the same property but properties may sometimes change listings because owners of properties have the option to remove their listing and start a new one. This has no consequence for the consistency of our estimates. Through *Inside Airbnb*, we also have information for a subset of listings on the number of reviews, which we will show for descriptive purposes.

[16] More specifically, we remove transactions referring to properties cheaper than \$50000 or more expensive than \$5 million. We also omit transactions with a m^2 price that is below \$200 or above \$20000. We further disregard repeat sales with yearly price differences larger than 50%. Additionally, we exclude properties smaller than $50m^2$ or larger than $1000m^2$ and parcels smaller than $50m^2$ or larger than 10ha.

[17] For example, when we observe that a property is listed in March, but not in May, the imputed listing probability is 0.5 in April 2015. Before October 2014 we use data on listings from October 2014.

[18] Information on the location of housing units is obtained from the *American Community Survey*, which provides information at the census block group (of, on average, 540 housing units). We draw circles around each property and calculate the area-weighted number of housing units within 200m. To avoid outliers for a low number of housing units, we replace the lowest 2.5% of the number of housing units by the value of the 2.5th percentile. In Appendix A.4.8 we show that our results are rather insensitive to outliers. As an alternative to the listings rate, we have also used the density of listings (within 200m) to calculate Airbnb demand, which provides largely similar results.

[19] The most detailed data publicly available is at the so-called *Zillow*-neighborhood. Because these data are only available for a few neighborhoods in LA County, we use the more aggregated zip code level.

[20] The condominium share of Airbnb listings exceeds the condominium share of housing transactions (see Table 2). Hence, the forbidding of Airbnb in condominium buildings in March 2015 by Owners Associations (*e.g.* to reduce within-building externalities) is unlikely widespread (see Watts v. Oak Shores Community Association, 2015).

Table 1
Descriptive statistics for Airbnb data.

Panel A: Inside HSO areas	mean	sd	min	max
Price per night *(in $)*	172.1	140.0	25	999
HSO implemented	0.769	0.421	0	1
Property type – apartment	0.515	0.500	0	1
Property type – single-family home	0.408	0.491	0	1
Property type – unknown	0.0769	0.266	0	1
Rental type – entire home/apartment	0.617	0.486	0	1
Rental type – home sharing	0.383	0.486	0	1
Accommodation size *(in number of persons)*	3.421	2.346	1	16
Number of reviews	19.27	37.62	1	602
Distance to border of HSO area *(in km)*	0.712	0.643	0.0000622	3.140
Distance to the beach *(in km)*	12.19	12.56	0	44.78
Panel B: Outside HSO areas	mean	sd	min	max
Price per night *(in $)*	147.2	132.7	25	999
HSO implemented	0	0	0	0
Property type – apartment	0.476	0.499	0	1
Property type – single-family home	0.435	0.496	0	1
Property type – unknown	0.0886	0.284	0	1
Rental type – entire home/apartment	0.597	0.491	0	1
Rental type – home sharing	0.403	0.491	0	1
Accommodation size *(in number of persons)*	3.477	2.505	1	20
Number of reviews	21.62	40.45	1	700
Distance to border of HSO area *(in km)*	4.616	4.947	0.000143	64.83
Distance to the beach *(in km)*	15.31	10.68	0	96.40

Notes: Prices are missing, unrealistically low (<$25) or high (>$1000) in 1% of the cases. The number of listings for HSO areas is 53,980. Outside HSO areas it is 344,813.

Table 2
Descriptive statistics for housing transactions.

Panel A: Inside HSO areas	mean	sd	min	max
House price *(in $)*	1,024,013	673,898	50,000	5,000,000
House price per m^2 *(in $)*	6187	2724	274.3	20,000
HSO implemented	0.391	0.488	0	1
Listings rate <200 *(in %)*	0.746	1.340	0	42.67
Property size *(in m^2)*	167.6	78.79	50	842
Parcel size *(in m^2)*	1447	3247	57	54,655
Apartment	0.371	0.483	0	1
Number of bedrooms	2.934	1.014	1	9
Number of bathrooms	2.447	0.968	1	5
Construction year of property	1971	22.07	1897	2017
Distance to border of HSO area *(in km)*	0.718	0.619	0.000137	2.992
Distance to the beach *(in km)*	14.61	14.14	0.0140	45.50
Tourist picture density *(per ha)*	5.569	7.780	0.114	31.95
Year of observations	2016	1.158	2014	2018
Panel B: Outside HSO areas	mean	sd	min	max
House price *(in $)*	610,301	476,562	50,000	5,000,000
House price per m^2 *(in $)*	4064	2189	247.5	20,000
HSO implemented	0	0	0	0
Listings rate <200 *(in %)*	0.564	1.900	0	85.64
Property size *(in m^2)*	152.6	69.39	50	921
Parcel size *(in m^2)*	2110	6333	50	95,285
Apartment	0.208	0.406	0	1
Number of bedrooms	2.980	0.948	1	10
Number of bathrooms	2.198	0.901	1	5
Construction year of property	1968	23.63	1884	2018
Distance to border of HSO area *(in km)*	11.09	12.33	0.000952	70.67
Distance to the beach *(in km)*	27.46	19.99	0.00346	107.5
Tourist picture density *(per ha)*	2.145	6.833	0	112.9
Year of observations	2016	1.169	2014	2018

Notes: The number of transactions for HSO areas is 32,971. Outside HSO areas it is 250,490.

fer to the Appendix A.1). We observe that there is a strong positive trend in the number of listings in LA County. In September 2018 the number of listings was almost 4 times higher than in October 2014. However, the growth in listings has been much lower in areas where HSOs were implemented during our study period. The trend in listings particularly diverges in 2017 once more cities implemented HSOs.

We report descriptive statistics for the housing transactions data in Table 2. The house price and the price per m^2 are substantially higher in HSO areas, respectively 52% and 68%. The listings rate is about 0.7% in HSO areas and 0.5% outside HSO areas. The spatial (see Fig. 1) and temporal (see Fig. 2) variation in the listings rate is large: for the majority of houses (65%), there are no listings within 200m.

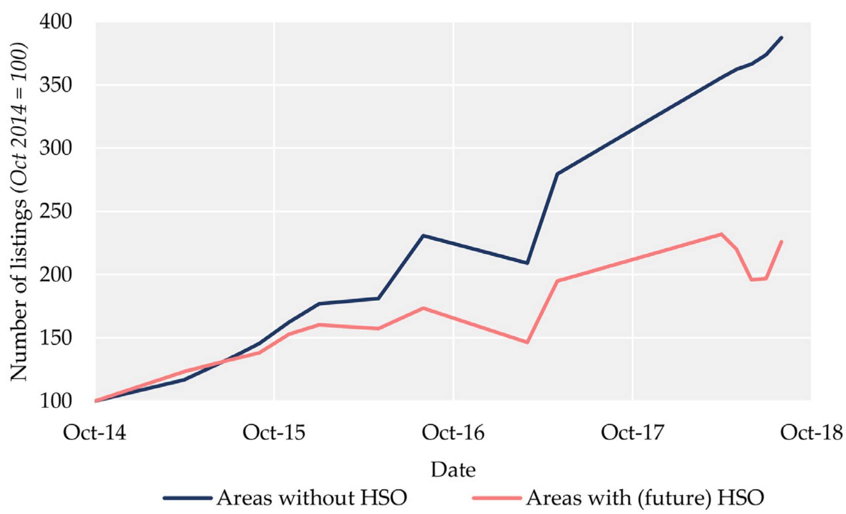


Fig. 2. Airbnb in LA County.

Table 3
Descriptive statistics for Zillow data.

Panel A: Inside HSO areas	mean	sd	min	max
Rent price per m^2 *(in \$)*	26.32	8.837	15.79	65.31
House price per m^2 *(in \$)*	6692	2464	4035	17,830
HSO implemented	0.579	0.494	0	1
Listings rate	0.826	0.790	0	4.452
Distance to border of HSO area *(in km)*	1.029	0.399	0.374	2.029
Distance to the beach *(in km)*	11.50	14.53	0.580	42.82
Distance to the CBD *(in km)*	25.54	7.136	12.85	41.08
Housing units per *(in ha)*	14.31	10.44	1.239	40.98
Year of observations	2016	1.345	2014	2018
Panel B: Outside HSO areas	mean	sd	min	max
Rent price per m^2 *(in \$)*	24.67	9.543	7.927	76.52
House price per m^2 *(in \$)*	5563	2622	1089	15,428
HSO implemented	0	0	0	0
Listings rate	1.355	1.710	0	14.26
Distance to border of HSO area *(in km)*	10.28	13.56	0.0594	58.65
Distance to the beach *(in km)*	23.86	21.54	0.137	96.28
Distance to the CBD *(in km)*	29.41	17.17	1.420	80.59
Housing units per *(in ha)*	11.48	9.730	0.320	45.66
Year of observations	2017	1.272	2014	2018

Notes: The number of observations for HSO areas is 815. Outside HSO areas it is 2676.

Properties in HSO areas are about 10% larger, but at the same time, the share of condominiums is about twice as high in these areas. This may reflect that HSOs tend to be implemented in upscale areas where prices are higher and which are inhabited by rich households that have high demands for space. These figures emphasize the need to focus on observations that are close to HSO borders to have a comparable control group. As one may expect, HSO areas tend to be more touristy: the distance to the beach is on average about half in these areas, whereas the density of tourist pictures is about twice as high, compared to non-HSO areas.

The above table also indicates that the share of housing transactions in HSO area is about 10% of total transactions. This observation is useful because it shows that the effect of HSO policies on listings in the non-HSO areas is likely small. The reason is that spillovers, which likely exist because short-term renters have an incentive to increase their demand in non-HSO areas, are expected to be of secondary importance. We come back to this later by providing empirical evidence that cross-border spillovers are non-existent (see Section 5).

Finally, we turn to the data on rents and house prices from *Zillow* for zip code areas. We report descriptives in Table 3. The average rent per m^2 is about \$26 in both areas. Although rents are very similar for both areas, we find a 17% lower average house price per m^2 outside HSO areas. The listings rate is lower in HSO areas (0.8%) than outside these areas (1.4%). Also at the zip code level, there is substantial variation in the listings rate. The zip code area with the highest rate, 14.3%, is located in Venice (City of LA), followed by a zip code in Hollywood (City of LA) with 8.9%.

A priori, it is difficult to judge the quality of the information offered by *Zillow*. Quite reassuringly, the correlation between median house prices in Zillow and median house prices using the Assessor Office's data (which we use for microanalyses) is high ($\rho = 0.941$). However, when we demean prices by zip code and month fixed effects, the correlation is only moderate ($\rho = 0.322$). This suggests that results might be dataset-specific. However, we will show that our results are not driven by the choice of the dataset.

4. Econometric framework

We are interested in the effect of short-term housing-rentals regulation on the housing market. One way to estimate this effect is to compare adjacent cities that differ in the regulation of Airbnb and then use a Spatial RDD around the cities' borders. This ignores however that cities differ in other ways than in their regulation of Airbnb. We address the latter by exploiting variation *over time* in the HSO around the borders of HSO areas. The HSOs induced exogenous changes in the propensity to list a property on Airbnb, which may have resulted in changes in house prices. Consequently, as we will use panel data (for listings as well as house prices), we will employ a Spatial Panel Regression-Discontinuity Design. In this design, we will assume that cross-border spillovers are absent (*i.e.* we assume that the Stable Unit Treatment Value Assumption (SUTVA) holds). We provide ample evidence using graphical as well as econometric analyses. Alternatively, we will also estimate difference-in-differences models (for rents, but also for prices), which do not rely on this assumption.

4.1. HSOs and Airbnb listings

The first step is to estimate the effect of the HSO on a property's probability of being listed on Airbnb. We distinguish between the probability of being listed as an entire home and the probability of being listed as rooms. We will estimate linear probability models, where we estimate the effects of the HSO on both probabilities separately.[21] We

[21] Our motivation not to estimate multinomial discrete choice models, but to estimate separate models is that, by construction, listings in our data never

use a Spatial RDD, where the running variable is the distance to the nearest border of an area where an HSO is implemented or will be implemented in the future. The effect of the HSO is captured by a discrete jump in the probability of being listed after its introduction.[22] Let ℓ_{ikt} be a dummy variable indicating whether a property i near a border of an HSO area k is listed in month t and h_{ikt} be a dummy indicating whether the HSO has been implemented. The variable d_{ik} denotes the distance to the border, where $d_{ik} > 0$.

One may argue that differences in unobservables of properties between HSO areas and neighboring areas may be correlated to the implementation of an HSO. For example, differences in the attractiveness of certain locations that are discrete at, or even further away from, the border (*e.g.*, school quality) may be present, which are correlated to h_{ikt} and influence ℓ_{ikt} at the same time. We, therefore, include property fixed effects λ_i, which control for difficult-to-observe but time-invariant differences between locations, and μ_{kt}, which capture HSO-border area by months fixed effects. More specifically, these are dummy variables that are equal to one on both sides of the shared border between two adjacent cities (or a neighborhood in the City of LA) in a specific month (hence, we include a fixed effect for each month/web scrape in each HSO-border area). This implies:

$$\ell_{ikt} = \alpha h_{ikt} + (\psi_1 + \psi_2 t) h_{ikt} d_{ik} + (\psi_3 + \psi_4 t)(1 - h_{ikt}) d_{ik} + \lambda_i + \mu_{kt} + \xi_{ikt}, \quad \text{if} \quad d_{ik} < b, \tag{1}$$

where α is the parameter of interest and ψ_1, ψ_2, ψ_3, ψ_4, λ_i and μ_{kt} are other parameters to be estimated. In this specification, ψ_1 and ψ_3 capture the possibility that distance trends in listings may be different on both sides of the border before and after the treatment. ψ_2 and ψ_4 aim to capture differences in those trends over time by including a linear interaction with time. Note that because we include property fixed effects, λ_i, we effectively only use data on properties that have been listed at least once. It also implies that $h_{ikt} d_{ik}$ and $1 - h_{ikt} d_{ik}$ are perfectly collinear with λ_i, which we address by imposing that $\psi_3 = \psi_4 = 0$. Hence, in essence, we have a regression-discontinuity design, which aims to identify a discontinuity in changes over time in listings at the border, where we allow for different distance-time trends at both sides of the border.

In this setup, we only include observations that are within a small distance b of a border of an HSO. We use a uniform kernel function with a bandwidth b, and do not include higher-order polynomials of the border trends (see Imbens and Lemieux, 2008). This approach is supported by Gelman and Imbens (2016) who show that such an approach is preferred over specifications including high-order polynomials of the running variable.

In RDDs, estimated parameters are often sensitive to the choice of the bandwidth b. We, therefore, show results for different bandwidths. Our preferred specification is based on an approach proposed by Imbens and Kalyanaraman (2012) to determine the optimal bandwidth, b^*, which is calculated conditional on control variables (property fixed effects and HSO-area×month fixed effects). We discuss the procedure to determine b^* in more detail in Appendix A.2. In our context, the optimal bandwidth is about 1.8 km, so quite small. Importantly, we show that our results are rather insensitive to the choice of bandwidth, also when choosing much smaller bandwidths.[23]

4.2. HSOs and house prices

We employ a similar approach to measure the effect of the HSO on house prices. The main difference is that we include census block fixed effects rather than property fixed effects, as we have fewer repeated observations. Let p_{ijt} be the house price of property i in census block j near a border of an HSO area k in month t with time-invariant housing characteristics x_{ijk}. We estimate:

$$\log p_{ijkt} = \beta h_{ijkt} + \zeta x_{ijk} + (\omega_1 + \omega_2 t) h_{ikt} d_{ik} + (\omega_3 + \omega_4 t)(1 - h_{ikt}) d_{ik} + \eta_j + \theta_{kt} + \epsilon_{ijkt}, \quad \text{if} \quad d_{ik} < b, \tag{2}$$

where β is the parameter of interest. Similar as above, ω_1, ω_2, ω_3 and ω_4 capture parameters related to the spatial trends before and after the treatment (first difference) and over time (second difference).[24] η_j and θ_{kt} refer to census block and HSO border×month fixed effects respectively. We calculate standard errors by clustering at census blocks. This equation implies that we compare price changes along the borders of HSO areas to see if prices have changed in the treated areas due to the HSO. Again, we will show results given different bandwidths, but our preferred specification is based on the optimal bandwidth.

The above approach ignores that there may be variation over time in the effect of HSOs. This is important, as anticipation effects of new laws may underestimate the effects of HSOs. Furthermore, we wish to take into account that house prices usually adjust gradually over time (implying that long-term effects may be stronger).[25] In the empirical analysis, we estimate specifications where we allow the HSO-effect to be time-specific, so we are also able to test for anticipation and adjustment effects of HSOs.

4.3. HSOs, Airbnb listings and rents

An HSO may also affect rents. A reduction in short-term rentals may lead to a reallocation of existing housing stock towards the long-term rental market away from private housing used for short-term renting, increasing the supply of available rental stock for locals, which should decrease rents.

In contrast to house prices, given the assumption of a spatial equilibrium, long-term rents should *not* be different *at* HSO borders given two assumptions: *(i)* rental properties at different sides but very close to these borders are close substitutes and offer the same value to renters; and *(ii)* renters are not allowed to list their property on Airbnb.

We will test the first assumption by estimating regressions where we only include properties close to HSO borders (*i.e.* 1km), which should lead to a statistically insignificant rent effect. The second assumption is also likely to hold, as in Los Angeles almost all rental leases include a provision explicitly forbidding to sublet the property (Lipton, 2014).

Given that theory does not suggest a discontinuity in rents at the border and that we have information on rents at the zip code level (which would make the use of a discontinuity design in any case less convincing), we pursue a standard difference-in-differences approach where we regress rents, r_{jt}, on h_{jt}, where j refers to zip codes areas. We then have:

$$\log r_{jt} = \phi h_{jt} + \eta_j + \theta_t + \epsilon_{jt}, \tag{3}$$

where ϕ is the parameter of interest, η_j are zip code fixed effects, and θ_t are month fixed effects. This is a standard difference-in-differences spec-

switch between being listed as an entire home and rooms. Note that homeowners may switch listings, but this will be recorded as being a new listing. In any case, switching types will not affect the consistency of our methodology).

[22] Note that in our data we observe listings. A listing always refers to a certain property, but sometimes properties change listing so different listings may refer to the same property inducing a slight loss in efficiency of the estimates when using property fixed effects.

[23] When choosing very small bandwidths (<350m), the estimates become less precise. For that reason, we will also estimate (1) while imposing that $\psi_1 = \psi_2 = \psi_3 = \psi_4 = 0$. This is essentially a 'non-parametric' approach as discussed by Imbens and Lemieux (2008), and applied by Dube et al. (2010). Usually, the bias of this estimator is anticipated to be relatively high but is expected to be small in our context, because within a few hundred meters, it is plausible that the spatial variation in the listing rate within the areas at both sides of the border is absent.

[24] ω_3 and ω_4 are now identified because we include census block, rather than property, fixed effects.

[25] Moreover, we will see that the HSO-induced reduction in listings is limited within the first year after the introduction, making it more plausible that the price reaction will be slower.

ification, with the notion that we have multiple treatments at different times in our study period.[26]

The key assumption underlying a DiD strategy is that there is a common trend between the treatment and control group. This assumption cannot be tested, but, as is standard, we examine this concern by undertaking an event study in the empirical analysis and show that there is no statistically significant effect before the HSO was implemented, which suggests (but does not prove) that the common trend assumption holds. Importantly, this strategy is less convincing than the Panel RDD. We will demonstrate that when applying a DiD strategy to house prices, then the house price effects are comparable to the ones obtained using the more credible Panel RDD approach. The latter makes it plausible that the rent results are reliable.

4.4. The effects of Airbnb listings on house prices and rents

The results from Eq. (2) are informative on the *average* treatment effect of the HSO on house prices, where the average applies to estimates along the borders of HSO areas. However, it is plausible that the effect strongly varies over space depending on local tourist demand for accommodation. The latter strongly covary with the demand for Airbnb, captured by the listings rate a_{ijkt}, potentially reducing the external validity of the estimated average treatment effect. In particular, one expects that areas that are popular with tourists are more strongly affected than areas that are not.

We will therefore also estimate the effect of the listings rate in the direct vicinity, a_{ijkt}, on prices using an IV approach.[27] Because a_{ijkt} is endogenous (as listings are imputed and so are measured with error, and residents and visitors have preferences for similar locations), we use arguably-exogenous variation in the listings rate caused by HSOs.

The second stage is then given by:

$$\log p_{ijkt} = \gamma \hat{a}_{ijkt} + \zeta x_{ijk} + (\omega_1 + \omega_2 t) h_{ikt} d_{ik} + (\omega_3 + \omega_4 t)(1 - h_{ikt}) d_{ik} + \eta_j + \theta_{kt} + \epsilon_{ijkt}, \quad \text{if} \quad d_{ik} < b, \tag{4}$$

where $\hat{a}_{ijkt}$ is obtained from:

$$a_{ijkt} = \tilde{\delta} h_{ijkt} + \tilde{\zeta} x_{ijk} + (\tilde{\omega_1} + \tilde{\omega_2} t) h_{ikt} d_{ik} + (\tilde{\omega_3} + \tilde{\omega_4} t)(1 - h_{ikt}) d_{ik} + \tilde{\eta}_j + \tilde{\theta}_{kt} + \tilde{\epsilon}_{ijkt}, \quad \text{if} \quad d_{ik} < b, \tag{5}$$

where the $\sim$ refer to first-stage coefficients and $\tilde{\delta}$ is the effect of the HSOs on the listings rate. We expect $\tilde{\delta}$ to be negative. We also apply an IV approach to determine the effects of the listing rate on rents, where we control for zipcode and month fixed effects as in (5).

5. Graphical evidence

Before we turn to the regression results, we illustrate our research design graphically. In Fig. 3a, we first focus on the impact of the HSO on Airbnb listings. We include property and border segment×month fixed effects, and include a 4$^{\text{th}}$-order polynomial of distance to the border outside HSO areas and a 2$^{\text{nd}}$-order polynomial of distance to the border multiplied by the treatment inside treated areas (as we have fewer data points that are closer to the border inside HSO areas).[28] The inclusion of property and border segment×month fixed effects implies that we identify the effects *over time*. In Fig. 3a, we plot the conditional probability of listing on Airbnb. We observe a sizable drop in the type of listings in areas where HSOs have been implemented. The difference is about 8 percentage points.[29] Given a listing probability of about 0.30 (for residences that have been listed at least once), this implies a reduction in listings of 27%. Hence, in line with anecdotal evidence, this suggests that the HSO was very effective in reducing STRs.

Fig. 3a is also important, as it provides evidence of the complete absence of cross-border crossings of listings due to the HSO. We do not find any evidence that a drop in listings at the HSO side of the border is accompanied by an increase in listings just at the other side (if anything, the figure implies the opposite). We come back to this issue in Section 6.1.

Let us now investigate whether there are differences in changes of characteristics of houses listed on Airbnb between treated areas and areas in the close vicinity. Fig. 3b shows that there is essentially no difference in how Airbnb prices per night and availability changed over time between HSO areas and neighboring areas. Hence, it is not the case that properties just outside HSO areas become more expensive. The latter suggests that the demand for Airbnb listings is locally elastic and the market is extremely competitive, which is consonant with the absence of cross-border listing effects. Some HSOs still allow for room rentals. In Fig. 3c we investigate if there is a decrease in the share of listings of entire homes relative to rooms. We do not find a statistically significant jump in the change in the share of entire homes at the border. In Fig. 3d we show that the type of accommodations on offer does not seem to change due to HSOs, as the change in accommodation size is not statistically significantly different at the border.

We repeat the exercise but now focus on house prices. The results are reported in Fig. 5. Prices *decrease* by about 4% at the HSO border. It appears that this effect is highly statistically significant. One may be concerned that this result is mainly explained by the very local decrease in house prices within 500m of the border. In the next section we show that, once we include more detailed census block or property fixed effects, the estimated effect becomes more precise and is very robust to bandwidth choice. Again we do not find any evidence of cross-border effects, as house prices close to but just outside HSO areas are not higher. In Appendix A.3, we further investigate whether discontinuities in changes in housing characteristics exist at the border. We do not find evidence for this.

In spatial RDDs, one should be concerned about sorting. It might be that a discontinuity in prices due to implementation is partly caused by a discrete change in the demographic composition of the neighborhood around the border (see Bayer et al., 2007, for cross-sectional evidence on school districts). We do not find any evidence for this in Appendix A.3.2.

In a non-spatial RDD, it is common to investigate whether the density of the running variable is continuous at the threshold because a discontinuity reveals that some individuals manipulate their position around the threshold. In spatial RDDs – using data on the housing stock in built-up areas – manipulation is less of an issue because real estate hardly changes in the short term (in the absence of notable large-scale demolitions of buildings or new constructions). We investigate changes in the density of listings and transactions before and after the HSO was implemented using McCrary's (2008) methodology. In Appendix A.3.4 we do not find meaningful differences in changes in densities across borders before HSOs were implemented.

[26] We make sure that using a weighted measure based on the number of housing units per area leads to similar results.

[27] We refer to Section 3.1 for how we constructed the listings rate variable.

[28] The choice of the order of the polynomial does not make any difference. This indicates that displacement effects – Airbnb hosts that move their listings to a location just outside a treated area – are unlikely to be important, as displacement effects would have induced an increase in listings just outside treated areas.

[29] The standard error becomes smaller close to the border because the estimated effect at the border does not depend on the estimated polynomial of distance, as the distance is zero at the border given the chosen specification. In Appendix A.3 we also compare the probability of being listed *before* and *after* the HSOs were implemented on both sides of the border, without conditioning on census block group fixed effects. This analysis suggests there was essentially no difference between HSO areas and surrounding areas in terms of the number of listed entire properties before the implementation, whereas the probability is about $10 - 20$ percentage points lower after it was implemented, in line with Figs. 3a.

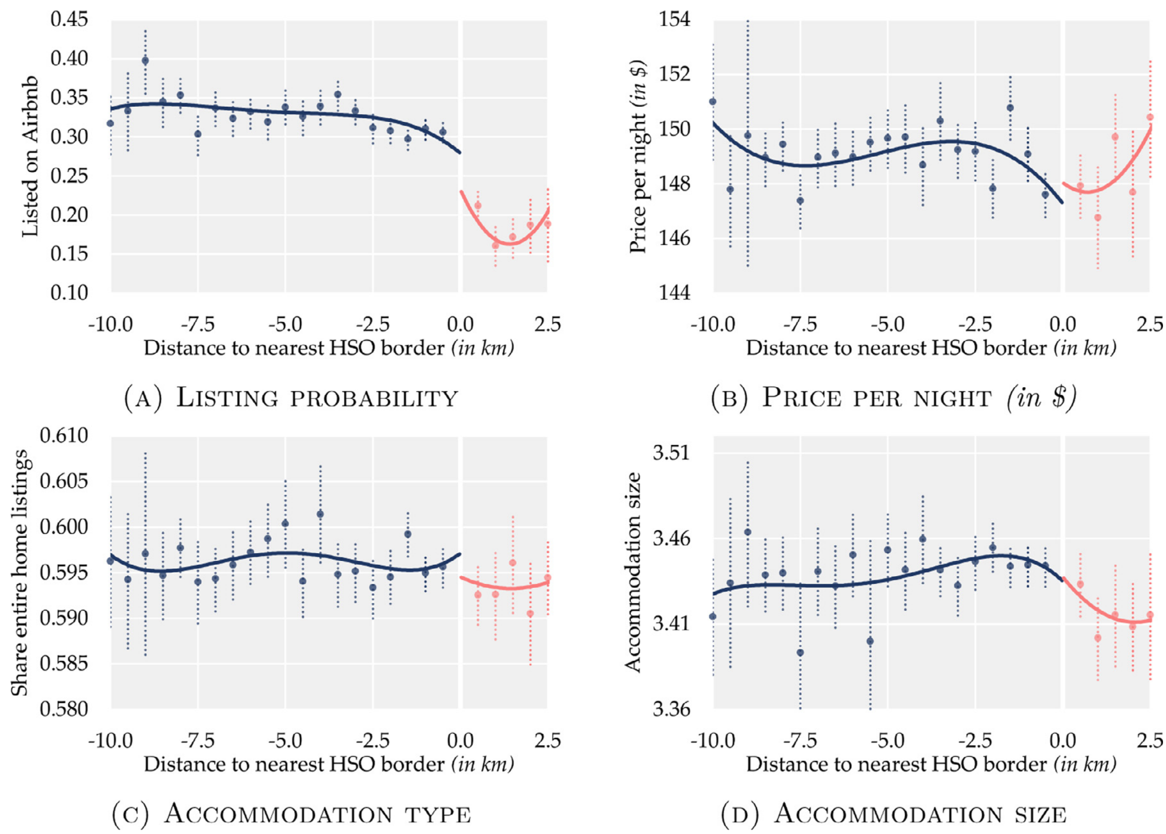


Fig. 3. Airbnb listings: variation near the HSO borders. *Notes*: Spatial differences in variables are *conditional* on property (or listing) and border segment×month fixed effects. Hence, we identify the effects *over time*. Negative distances indicate areas outside HSO areas and areas inside HSO areas but *before* treatment. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block level. We include a 4rd-order polynomial in untreated areas and a 2nd-order polynomial in treated areas.

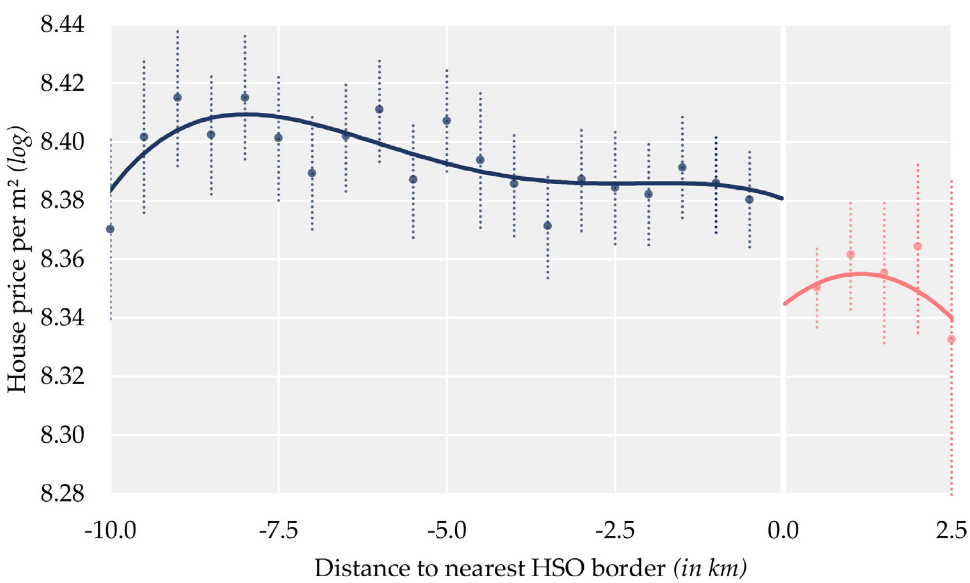


Fig. 4. House prices: variation near the HSO borders. *Notes*: Price differences are *conditional* on census block and border segment×month fixed effects. Hence, we identify the effects *over time*. Negative distances indicate areas outside HSO areas and areas inside HSO areas but *before* treatment. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block level. We include a 4rd-order polynomial of price differences in untreated areas and a 2nd

6. Results

6.1. HSOs and Airbnb listings

In Table 4 we report the baseline results of the impact of HSOs on Airbnb listings. In Panel A, we focus on listings of entire homes or apartments. In column (1) we start with the RDD using the Imbens and Kalyanaraman-bandwidth, which includes observations up to 1.67km of the nearest HSO border. The result points towards a strong reduction in Airbnb listings of 6.1 percentage points after the implementation of the HSO. Given that the share of listings around the border was about 0.3 before implementation, this implies a decrease in listings of 20%.

In column (2) we add border segment×month fixed effects. That is, we determine for each HSO area the segment of the border that is shared with another city (or neighborhood in the City of Los Angeles). In this way, we mitigate issues related to differences in the provision of public

Table 4
Baseline results for Airbnb listings.

(Dependent variable: Airbnb property is listed)							
Panel A: Entire homes/apartments	*Panel RDD* (1)	*+ Border segment f.e.* (2)	*Bandwidth: $h^* \times 2$* (3)	*Bandwidth: $h^*/2$* (4)	*Bandwidth: $h^*/5$* (5)	*Rooms not allowed* (6)	*Measurement error* (7)
HSO implemented	-0.0614***	-0.0696***	-0.0815***	-0.0444***	-0.0290		-0.1058***
	(0.0122)	(0.0123)	(0.0110)	(0.0154)	(0.0226)		(0.0168)
HSO implemented× rooms allowed						-0.0682***	
						(0.0205)	
HSO implemented× rooms not allowed						-0.0701***	
						(0.0127)	
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	270,906	270,621	425,117	154,015	80,896	270,741	253,448
Bandwidth, *b (in km)*	1.6716	1.6708	3.3416	0.8354	0.3342	1.6712	1.9639
R^2	0.3481	0.3515	0.3550	0.3481	0.3439	0.3514	0.3546
Panel B: Rooms	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HSO implemented	-0.0253	-0.0363**	-0.0436***	-0.0296	-0.0082		-0.0551**
	(0.0170)	(0.0172)	(0.0156)	(0.0226)	(0.0315)		(0.0224)
HSO implemented× rooms allowed						0.0309	
						(0.0264)	
HSO implemented× rooms not allowed						-0.0595***	
						(0.0187)	
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	171,778	171,448	259,880	94,365	45,267	171,433	156,710
Bandwidth, *b (in km)*	1.815	1.812	3.3384	0.8346	0.3338	1.8117	2.0061
R^2	0.3339	0.3438	0.3424	0.3524	0.3558	0.3439	0.3482

Notes: We exclude within 200m of the borders of HSO areas in column (7). Standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

goods. Although this implies the inclusion of 1350 instead of 270 fixed effects, this hardly impacts the results (the R^2 is not much impacted either so arguably this is not very informative on omitted variable bias, see Oster, 2019).

Imbens and Lemieux (2008) and Lee and Lemieux (2010) stress the importance of showing the robustness of the results to the choice of bandwidth. In column (3) we, therefore, multiply the optimal bandwidth by 2 and in column (4) divide it by 2. The point estimates range between 4.4 and 8.2%. One may still be concerned that the bandwidth is on the high side. We, therefore, divide the optimal bandwidth by 5 in column (5) so that we include only observations within 334m of the borders. We find a lower, albeit somewhat less imprecise, effect of 2.9 percentage points. The somewhat lower effect is not surprising as measurement error in the location is amplified when focusing only on listings close to the border. Reducing the distance even further is not informative, as the location of listings is known up to a 200m radius.[30] The finding that the effect is similar for very small bandwidths is particularly important as this implies that listings do not move just across the border of an HSO area, which would imply that we may overestimate the effects of HSOs.

In column (6) of Panel A, we make a distinction between different types of HSOs. Recall that four cities that have implemented HSOs still allow for room rentals. As we focus here on listings of entire homes, one expects that the different types of HSOs have similar effects. We, therefore, include an interaction of the HSO with a dummy indicating whether letting of rooms is allowed. In line with expectations, we do not find that the effect on listings of entire properties – which are always restricted – is different between the two types of HSOs.

The imprecise reporting of the location by Airbnb (*i.e.* the location of listings is accurate up to a distance of 200m) may affect our estimates, as it implies a misclassification error in the treatment variable if Airbnb misreports the city of each listing. This should lead to underestimates as the treatment variable is observed with measurement error.[31] To examine this, we have estimated models where we exclude observations within 200m of the border, see column (7) in Table 4. We indeed find a slightly stronger effect.[32]

[30] When we apply the 'non-parametric' approach, implying that $\psi_1 = \psi_2 = \psi_3 = \psi_4 = 0$, on observations within 334m of the borders, we find a 7.1 percentage point effect, precisely estimated with a standard error of only 1.4.

[31] One may argue that there may also be measurement error in *the running variables, i.e.* the distances to the border. Measurement error usually induces bias in the estimates, potentially even more so within a regression discontinuity framework (Davezies and Le Barbanchon, 2017). Arguably, the bias in our HSO estimates will be small, because the extent of the measurement error is small. For example, given the plausible assumption that measurement error is uniformly distributed between 200m, the measurement error variance appears only 5 – 10% of the distance variances (on both sides of the border), indicating that the attenuation bias in the effect of the running variables should be an order of magnitude smaller than the estimated effect size of the running variables. As this argument may not be entirely convincing, we have examined the importance of measurement error by estimating models, while excluding the distance to the border variables. We find then almost the same HSO effects, indicating that measurement error is unlikely to affect our estimates.

[32] We have also estimated models using an RDD, where the probability of treatment is assumed to be a function of the distance to the border, given the assumption that measurement error in the distance is uniformly distributed within 200m, which is inspired by Hullegie and Klein (2010). Again we find similar estimates.

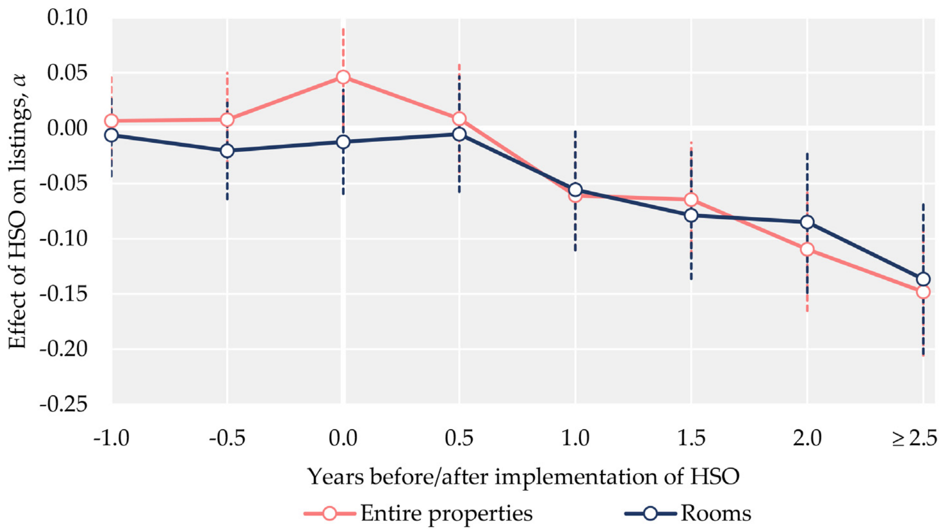


Fig. 5. An event study to the effect of the HSO on Airbnb listings. *Notes*: The optimal bandwidth $b^* = 1.6692$ for 'entire properties' and $h^* = 1.8120$ for 'rooms'. The dashed lines denote the 95% confidence bands.

In Panel B of Table 4 we analyze the effects of HSOs on listings of rooms. We repeat the same set of specifications as in Panel A. The effect is about 50% smaller than for entire homes/apartments. More specifically, the coefficient in column (1) implies that the probability to list a room has decreased by 2.5 percentage points. This effect is somewhat stronger (−3.6 percentage points) once we include border segment×month fixed effects. Given an average probability to be listed of 0.28, this implies a decrease of 13%. The finding that the percent effect on room rentals is smaller makes sense as some cities do not completely forbid room rentals (*e.g.* Santa Monica). If we include border segment×month fixed effects (column (2)) or change the bandwidth (columns (3) to (5)), this leaves the results essentially unaffected.[33]

In column (6) we again include an interaction of the HSO with a dummy indicating whether room rentals are allowed. As one expects, we do not find that rooms listings have been reduced in areas where room rentals are still allowed, whereas rooms listings have been substantially reduced in areas where short-term renting is completely banned, with a percentage point reduction that is about the same as for entire homes/apartments. We think this provides strong evidence that the changes in the listing probabilities are related to the implementation of HSOs. Column (7) highlights that measurement error is not really an issue, as the exclusion of listings within 200m of a border leads to almost the same estimate as the baseline estimate.

In Fig. 5 we show an event study on how the effect of the HSO on Airbnb listings varies *over time* by re-estimating our preferred specification shown in column (2) of Table 4, while interacting the effect of the HSO with time dummies. Our reference category then is the whole period until one year before the HSO as well as areas that are never treated. Just before and at the moment of implementation, there is no effect of the HSO. Hence, there do not seem to be pre-trends in listings related to the implementation of HSOs. However, after a year, we find a (marginally) statistically significant reduction in listings of entire properties of about 6.5 percentage points. After 2.5 years, the effect has increased to 15 percentage points for entire homes, which implies a reduction in listings of almost 50%. Therefore, in the long-run, the HSO had a very strong effect on the listings of entire properties. A similar pattern emerges for room listings, where we find that the long-run decrease in listings is 13 percentage points (or 47%). Why does the effect become stronger over time? One explanation is that, in the beginning, households/investors did not yet know whether and to what extent the ordinance would be enforced. After a while, it became clear that it was being enforced, implying potentially hefty fines.[34]

We also investigate the effects of the HSO on Airbnb rental prices of properties in Appendix A.4.2. We do not expect that *at the border* rental prices do change, because tourists are unlikely to differentiate between accommodation in a treated area and neighboring areas. We indeed find that rental prices of Airbnb properties are not significantly different at the border when applying a panel regression-discontinuity design. However, one may expect differences further away from the border if tourists have a strong preference of (not) staying in a certain area. We therefore also estimate DiD models where we exclude properties close to HSO borders (<1km). Still, we do not find any effect of HSOs on Airbnb rental prices. These results are in line with the belief that the market for short-term rentals is highly competitive: restrictions on short-term rental supply by HSOs (as well as additional Transient Occupancy taxes) do not impact the spatial equilibrium of rental Airbnb prices.[35]

6.2. HSOs and house prices

We have seen that the HSO strongly reduces the probability of using a property for short-term renting. We expect that this will have a negative effect on house prices. In Table 5 we report the results. We start with a Panel RDD, including census block and HSO area×month fixed effects, as outlined above. The results in column (1) indicate a negative effect of the policy of $\exp(-0.0178) - 1 = 1.8\%$.[36]

In column (2) we add border segment×month fixed effects leading to essentially the same result. The results do not materially change when

[33] When we exclude the spatio-temporal trend variables on the observations as in (5), we find a coefficient of 0.036 percentage point effect, with a standard error of 0.02.

[34] In Appendix A.4.1 we investigate the effects of the HSOs on the listing probability as well as prices for each city separately. We show that the coefficients are generally negative, or when positive, statistically insignificant. However, standard errors are often somewhat large, so we cannot make precise statements for individual cities.

[35] We also investigate the effects of HSOs on the number of formally registered traveler accommodations in Appendix A.4.3, using data from the *County Business Patterns*. Because we have data on only a few years and the data is only available at the zip code level, the results are imprecise. However, the point estimates seem to point towards a sizable 5% increase in the number of formal traveler accommodations after implementation of an HSO. Hence, we interpret this as suggestive evidence that HSOs have led to an increase in formal accommodation.

[36] The housing control variables either have plausible signs and magnitudes or are statistically insignificant. A 1% increase in house size leads to a price increase of 0.5%. We further find that condominiums are approximately 25–30% less expensive than single-family homes. The results are robust to the exclusion of housing controls.

Table 5
Baseline results for house prices.

(Dependent variable: log of house price)	*Panel RDD* (1)	*+ Segment month f.e.* (2)	*Bandwidth: $h^* \times 2$* (3)	*Bandwidth: $h^*/2$* (4)	*Bandwidth: $h^*/5$* (5)	*Rooms not allowed* (6)
HSO implemented	-0.0178**	-0.0177**	-0.0209***	-0.0133	-0.0195	
	(0.0071)	(0.0078)	(0.0069)	(0.0098)	(0.0150)	
HSO implemented× rooms allowed						-0.0190*
						(0.0109)
HSO implemented× rooms not allowed						-0.0173**
						(0.0082)
Property size *(log)*	0.4988***	0.4952***	0.4876***	0.5002***	0.5373***	0.4952***
	(0.0100)	(0.0090)	(0.0077)	(0.0116)	(0.0174)	(0.0090)
Parcel size *(log)*	0.0425***	0.0436***	0.0383***	0.0415***	0.0282***	0.0436***
	(0.0045)	(0.0040)	(0.0039)	(0.0053)	(0.0064)	(0.0040)
Bedrooms	0.0047**	0.0051**	0.0019	0.0083***	0.0043	0.0051**
	(0.0023)	(0.0022)	(0.0018)	(0.0029)	(0.0041)	(0.0022)
Bathrooms	0.0184***	0.0184***	0.0219***	0.0131***	0.0161***	0.0184***
	(0.0026)	(0.0026)	(0.0022)	(0.0034)	(0.0051)	(0.0026)
Apartment	-0.3170***	-0.3200***	-0.3226***	-0.3135***	-0.3377***	-0.3200***
	(0.0109)	(0.0107)	(0.0103)	(0.0129)	(0.0189)	(0.0107)
Construction year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes
Number of observations	63,487	63,275	98,594	39,192	19,110	63,276
Bandwidth, b *(in km)*	1.8029	1.8087	3.6174	0.9044	0.3617	1.8088
R^2	0.9024	0.9090	0.9052	0.9113	0.9218	0.9090

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$

we choose other bandwidths in columns (3) and (4). However, it becomes too imprecise to be statistically significant at conventional levels in column (4). We even further reduce the bandwidth to only 362m in column (5). Now, the point estimate is very close to the baseline estimate in column (2), albeit imprecise. Clearly, columns (4) and (5) indicate that cross-border effects are absent (as otherwise, the point estimates should have increased for shorter distances).[37] Column (6) tests whether HSOs that allow for room rentals have weaker price effects. This appears not to be the case: the price effect in areas that allow for room sharing is not statistically significantly different from the effect of HSOs in areas that do not allow for this. An interpretation is that most of the price effect is caused by investors buying homes and using them for short-term renting.

Back-of-the-envelope calculations indicate that these results are within a plausible range. For example, using the average list price per night and the average house price, combined with a mortgage interest rate of 3.3% and maintenance costs of 3%, implies that typical hosts who rent out their property on Airbnb for 10 nights per year earn revenue from short-term renting equivalent to 2.5% of their housing expenditure, suggesting that house prices would increase by that amount (in the absence of variable costs, such as cleaning, changing sheets). This calculation ignores the effect of professional investors, who typically outbid households, suggesting that much higher price effects are plausible if the listings rate of Airbnb properties is substantial.[38]

A well-known issue with exploiting changes in house prices over time is that one has to take anticipation effects into account. Anticipation effects may have been important as discussions on the HSO predate implementation. On the other hand, it might have taken some time before the HSO capitalized into house prices. We have tested this, with results shown in Fig. 6. We find that before implementation of the HSO there is no statistically significant price decrease (compared to the period until one year before the HSO), suggesting the absence of anticipation effects. At the moment of implementation, we find that prices are about 2% lower. The price effect is stable over time. The absence of sizable anticipation effects also implies that there is no strong evidence for pre-trends, which would potentially invalidate our research design. We test more extensively for pre-trends in Appendix A.4.4 where we include data from earlier years.

In Appendix A.4.6, we investigate to what extent negative external effects related to tourism play a role. Recall that the estimates discussed above are the *net* effects of 2 opposing mechanisms: the first is that the HSO reduces demand for housing, which decreases house prices. The second mechanism is that it reduces negative tourist externalities, which in turn increases house prices. Alternatively, because we find that the net effect of the HSO is negative, the estimates may be interpreted as underestimates of the efficient use effect, where the size of the underestimate depends on the size of the externality effect. We do not find strong evidence for the presence of a local external effect, implying that the estimated effect of the HSO almost exclusively reflects an efficient use effect.

In Appendix A.4.9 we make sure that the results also hold for median prices in the Zillow data: we show that the house price effects using the DiD estimation strategy deliver similar results as the ones reported in Table 5. This suggests that the DiD strategy which we will apply to rents is a plausible alternative estimation strategy. Moreover, these ancillary regressions highlight that the average treatment effect identified through a Panel RDD is about equal to the average treatment effect identified through a DiD strategy.

[37] Reassuringly, when we apply the 'non-parametric' approach, implying that $\omega_1 = \omega_2 = \omega_3 = \omega_4 = 0$, to the observations in column (5), we find again a similar effect of −2.2%, which is statistically significant at the 5% level, despite the strong reductions in the number of observations.

[38] Professional investors' daily revenue from renting out short-term is about twice the daily revenue from renting out long-term. Given that the renting costs (excluding the capital costs of acquiring the property) are about 20% of the revenue (here we use information from agencies that manage short-term renting for households), then the willingness to pay by professional investors exceeds those of the current marginal house owners by about 60%.

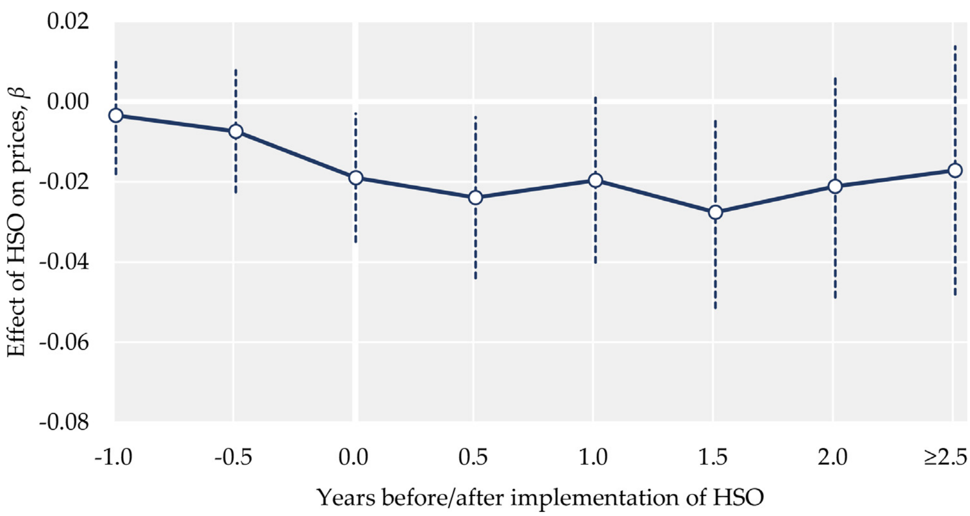


Fig. 6. An event study to the effect of the HSO on prices. *Notes*: The optimal bandwidth $b^* = 1.8089$. The dotted lines denote the 95% confidence bands.

Table 6
Placebo estimates.

Panel A: (Dep.var.: Airbnb property is listed)	*Shift border 1km outwards* (1)	*Areas with zoning code* (2)	*City of LA* (3)	*Unincorporated areas* (4)	*5 years earlier* (5)	*10 years earlier* (6)
Placebo-HSO implemented	0.0076	-0.0164	-0.0081	0.0007		
	(0.0105)	(0.0202)	(0.0076)	(0.0158)		
Spatio-temporal trend variables	Yes	Yes	Yes	Yes		
Property fixed effects	Yes	Yes	Yes	Yes		
Border segment×month fixed effects	Yes	Yes	Yes	Yes		
Number of observations	371,565	452,385	717,315	271,215		
Bandwidth, *b (in km)*	1.5145	1.3981	1.0593	1.5953		
R^2	0.3550	0.3713	0.3615	0.3786		
Panel B: (Dep.var.: log of house price in $)	(1)	(2)	(3)	(4)	(5)	(6)
Placebo-HSO implemented	-0.0058	0.0100	-0.0088	0.0164*	-0.0120	-0.0108
	(0.0186)	(0.0068)	(0.0092)	(0.0092)	(0.0104)	(0.0088)
Property characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	53,248	123,250	102,249	94,331	60,323	68,266
Bandwidth, *b (in km)*	1.8705	1.3014	1.4922	1.345	1.2936	1.9172
R^2	0.9068	0.9076	0.9109	0.9087	0.9029	0.8654

Notes: In Panel A, we exclude listings within 200 m of HSO areas (because the location is known up to 200m). In Panel B we exclude transactions in HSO areas. Standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

6.3. Placebo checks and sensitivity

It is important to show the robustness of our results. In this subsection, we will show some 'placebo'-estimates and summarize the most important robustness checks. In Table 6 we estimate regressions where we consider placebo HSOs for other areas. Panel A reports the results for the effects on listings, while Panel B investigates the effects on house prices.[39]

One obvious candidate for a placebo-test is to shift the borders of HSO areas 1km outwards to make sure that we do not capture some spatial trend that is correlated to the treatment variables. It seems that this is not an issue, as the effects of the placebo-HSOs on listings and house prices are statistically indistinguishable from zero.

In the second placebo test, we investigate the issue that in some cities Airbnb is officially not allowed because the zoning code does not allow for short-term renting, but as discussed in Section 2, these zoning codes are not enforced. We treat those cities (listed in Appendix A.1) as if an HSO would have been implemented. To determine the timing of the placebo HSOs for each of those cities, we take the timing of the nearest city that has implemented an HSO. The results in column (2) confirm that those cities do not see a decrease in listings or house prices.

As a third placebo check, we treat each neighborhood in the City of Los Angeles with a placebo HSO. This is relevant as the City of LA had plans to restrict Airbnb. At the time of writing, Airbnb is still allowed, but hosts may only operate one short-term rental at a time and will only be able to rent out their properties for 120 days a year. Again, to determine the timing, for each neighborhood in LA, we take the nearest city that has implemented an HSO. Column (3) in Table 6 shows that there is no effect of this placebo HSO on listings or prices.

Column (4) continues by checking whether 'unincorporated' areas, which have identical regulations concerning public goods and STRs, have seen changes in listings and prices. To determine the timing of the placebo HSOs we again use the date of implementation of the nearest HSO area. The coefficients clearly indicate that there is no effect of the placebo HSO. We find a slight *positive* effect on prices, but we think this is merely a Type II error, given the absence of an effect on listings.

[39] In Panel A, we exclude transactions within 200m of HSO areas because the location of listings is known up to 200m.

In the final placebo checks, we investigate whether we can detect any effect on housing prices using data from exactly 5 and 10 years earlier (from 2009 until 2013 and from 2004 until 2008) and assume that the HSO would have been implemented exactly 5 or 10 years earlier. Because Airbnb data is not available from before 2014, we cannot estimate this placebo test for listings. For house prices, we again find that estimates are economically small and statistically indistinguishable from zero. This is important because this suggests that there are not statistically significant pre-trends in prices that may explain the price effect we find in our analysis.

Therefore, the placebo-estimates reported in Table 6 confirm that the finding of a reduction in listings and house prices due to implementation of the HSO is not a statistical artifact and unlikely the result of a differential provision in the change of public goods or other regulation

We subject this conclusion to a wide range of other sensitivity checks in Appendix A.4. More specifically, in Appendix A.4.1 we report results where we estimate city-specific effects for the effects of HSOs on listings and house prices, as discussed earlier. Appendix A.4.2 investigates whether the HSO impacted rental prices of Airbnb. As mentioned earlier, we do not find that this is the case. On the other hand, we find suggestive evidence that the number of formal accommodations has increased in HSO areas (see Appendix A.4.3). Appendix A.4.4 further investigates the possibility of pre-trends in prices. In Appendix A.4.5 we investigate whether standard errors change when taking into account cross-sectional dependence. We show that standard errors are even somewhat smaller, although very comparable to the baseline estimates where we cluster at the census block level.

Appendix A.4.7 reports first-stage results of the impact of the HSO on the listings rate. In Appendix A.4.8 we examine robustness of our results if we *(i)* include property rather than census block fixed effects, *(ii)* use flexible distance to the border×year trends instead of choosing a bandwidth, *(iii)* include picture density×year trends to control for changes in attractiveness of touristy areas, *(iv)* control for changes in demographic variables, *(v)* include straight border×year fixed effects to further address any omitted variable bias, the idea being that straight border segments are likely uncorrelated to geographical features of location, as argued by Turner et al. (2014), *(vi)* exclude outliers in the listings rate. The results are generally robust.

6.4. HSOs and rents

So far, we focused on the effects of HSOs on house prices. One may wonder whether the results also hold if we extend the analysis to rents. We reiterate here that differences in rents should capture the housing supply effect – short-term rentals may lead to a reallocation of existing housing stock away from the long-term rental market towards privately-owned housing. However, there is a more fundamental difference. In contrast to owners, renters are thought to be indifferent to (otherwise identical) properties that are close to HSO borders, so the use of a Panel RDD is not the appropriate strategy to identify the housing supply effect (as it should show a zero effect of HSOs on rent). We, therefore, use a more standard difference-in-differences strategy and include observations further away from the border. Table 7 reports the results. In Panel A we test for the effect of HSOs on rents.

In column (1), Panel A, we show that due to HSOs, rents have decreased by 2.3%. Column (2) shows that the effect is similar when we exclude properties that are further away than 25km from any HSO area, which ensures that we exclude the low-density outskirts of LA County where rent trends may be very different. In column (3), we also drop observations close to (1km) but outside HSO areas. The results indicate an effect that is only slightly stronger (2.2%).

This estimate is very close to the preferred estimate for prices, reported in column (2), Table 5. Column (4) explicitly tests whether rents are continuous at the borders of HSO (within 1km of both sides). We indeed find no statistically significant difference between HSO areas and areas outside HSOs. Moreover, the point estimate is very close to zero. This suggests that properties that are close to the HSO border are indeed close substitutes, although we cannot entirely rule out the possibility that this result is driven by a small sample effect. In column (5), Panel A, we control for second-order polynomial distance to the CBD×year and distance to the beach×year trends, leading to slightly lower effects. Finally, we only keep observations in column (6) that are inside HSO areas and further away than 2.5km from any HSO border. We find that rents then decrease by 2% when an HSO is implemented.

We also test whether pre-trends and/or anticipation effects are an issue. Fig. 7 replicates the specification with distance to CBD and beach trends, zipcode and month fixed effects. This specification only includes zip codes that are further than 1 and less than 25km from a (future) treated area. We find no effect before implementation: the effect is small and statistically insignificant. After half a year, the effect of HSOs becomes statistically significant at the 5% level. The long-run effect after 2 years is about 5%, albeit somewhat imprecise.

6.5. Airbnb listings and house prices and rents

One could argue that the average treatment effect estimated around the border of HSO areas does not say much about the effect of Airbnb on house prices, because neighborhoods with a higher tourist accommodation demand are more strongly affected by the ordinances (as a relative decline in the listings probability implies a stronger absolute decrease in the listings rate in areas with a higher initial listings rate). We, therefore, estimate the direct impact of the listings rate on house prices using an IV approach. To deal with endogeneity issues – omitted variable bias and potential measurement error in the listings rate – we employ an instrumental variable approach using the HSOs in the different cities. Because we have seen that listings only gradually reduce after the introduction of the HSO, we exclude observations in the 6 months immediately after the introduction.

Table 8 reports the regression results for the two-stage Panel RDD.[40]

We observe in Table 8 that the instrument is strong in all specifications as the first-stage *F*-statistic is above the rule-of-thumb value of 10 in all specifications. The first-stage estimates are reported in Appendix A.4.7. They indicate that the listings rates have decreased by about 0.4–0.6 percentage points, which is about 50–70% of the mean. In other words, the first-stage results are comparable to what we already established in the previous subsection: the HSO has strongly reduced the number of Airbnb listings.

In column (1), we find that a 1 percentage point increase in the Airbnb listings rate increases property prices by 5.1%. In column (2) we include border segment×month fixed effects. The effect reduces to 3%. A standard deviation increase in the listings rate is associated with a $1.845 \times 0.0300 = 5.5\%$ increase in prices, so the effect of Airbnb is substantial. The elasticity of prices with respect to the average listings rate in the sample is $0.0300/0.585 = 0.0513$. When we only focus on areas where an HSO has been implemented this elasticity is very similar and equal to 0.0402.[41] Changing the bandwidth substantially does not change the results much, although the coefficient becomes imprecise for small bandwidths.[42]

In columns (5) and (6) of Table 8 we make sure that the choice to determine the listings rate within 200m is not affecting our results. When we use the listings rate within 100m, the coefficient is 0.0251, which is very similar to the baseline estimate. Moreover, when using the list-

[40] Table 8 also report the bandwidths. We obtain the bandwidth from the first stage: a regression of the listings rate on the HSO dummy.

[41] These estimates are of a similar order of magnitude as Barron et al. (2021), who use a completely different identification strategy.

[42] We also considered to further reduce the bandwidth, as in the previous tables. However, because of a weak first stage, the results are uninformative and imprecise, and available upon request. Given that both reduced-form effects of HSOs on listings and prices are statistically significant for small bandwidths, we do not consider this a major issue.

Table 7
DiD results for rents.

(Dependent variable: log of median rent per m²)						
	All obs. (1) OLS	*Outside HSO, <25km* (2) OLS	*Outside HSO, >1km, <25km* (3) OLS	*Outside HSO, <1km* (4) OLS	*Outside HSO, >1km, <25km* (5) OLS	*Outside HSO, >2.5km, <25km* (6) OLS
HSO implemented	-0.0230***	-0.0201**	-0.0223**	-0.0074	-0.0187**	-0.0202**
	(0.0087)	(0.0088)	(0.0092)	(0.0102)	(0.0082)	(0.0092)
Distance to CBD×year trends	No	No	No	No	Yes	Yes
Distance to beach×year trends	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3491	3231	2951	722	2951	2472
R^2	0.9888	0.9838	0.9829	0.9850	0.9841	0.9848

Notes: In all specifications we include observations *inside* HSO areas. Standard errors are clustered at the zipcode level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

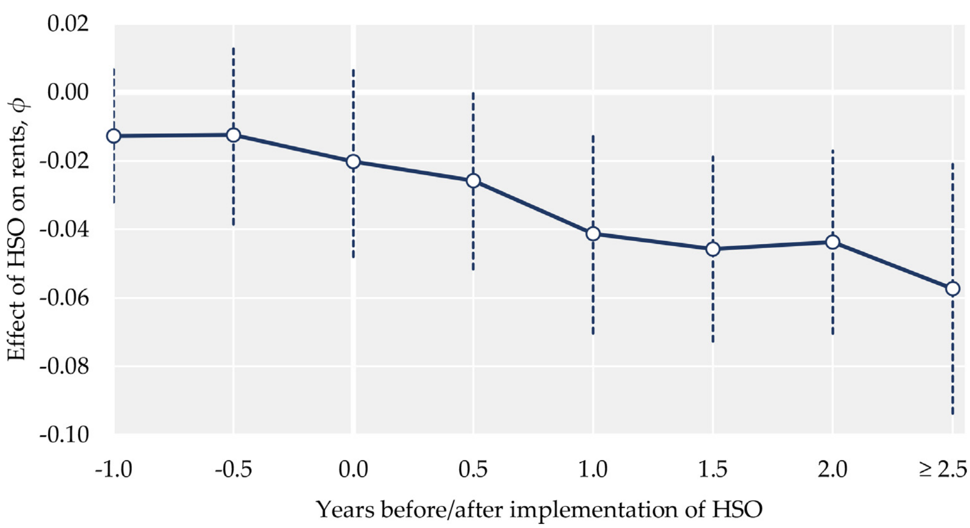


Fig. 7. An event study to the effect of the HSO on rents. *Notes*: We include observations *inside* HSO areas and observations between 1 and 25km of an HSO area. The dotted lines denote the 95% confidence bands.

Table 8
Airbnb listings and house prices: 2SLS estimates.

(Dependent variable: log of house price)								
	Panel	*+ Border*	*Bandwidth:*	*Bandwidth:*	*Different threshold*		*Selected*	*Approximated*
	RDD (1)	*segment f.e.* (2)	$h^* \times 2$ (3)	$h^*/2$ (4)	*100m* (5)	*500m* (6)	*dates* (7)	*listings rate* (8)
Listings rate <200m *(in %)*	0.0511**	0.0300*	0.0338**	0.0281			0.0934	0.0444*
	(0.0219)	(0.0156)	(0.0168)	(0.0218)			(0.0679)	(0.0228)
Listings rate <100m *(in %)*					0.0251*			
					(0.0130)			
Listings rate <500m *(in %)*						0.0372*		
						(0.0207)		
Property characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	83,766	83,037	134,074	52,115	91,623	70,232	15,509	82,076
Bandwidth, b *(in km)*	2.9429	2.9257	5.8513	1.4628	3.4276	2.297	3.8509	2.8797
Kleibergen-Paap *F*-statistic	28.45	57.57	52.25	30.13	33.24	111.6	11.73	55.36

Notes: We exclude transactions occurring within half a year after implementation of the HSO. We instrument the listings rate a dummy indicating whether an HSO has been implemented. Robust standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

Table 9
DiD results for rents.

(Dependent variable: log of median rent per m^2)	*All obs.* (1) 2SLS	*Outside HSO, <25km* (2) 2SLS	*Outside HSO, >1km, <25km* (3) 2SLS	*Outside HSO, <1km* (4) 2SLS	*Outside HSO, >1km, <25km* (5) 2SLS	*Outside HSO, >2.5km, <25km* (6) 2SLS
Listings rate	0.0491**	0.0366*	0.0497**	0.0095	0.0422**	0.0488**
	(0.0241)	(0.0185)	(0.0247)	(0.0133)	(0.0180)	(0.0216)
Distance to CBD×year trends	No	No	No	No	Yes	Yes
Distance to beach×year trends	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3491	3231	2951	722	2951	2472
Kleibergen-Paap *F*-statistic	16.61	22.30	15.88	15.65	15.23	10.39

Notes: In all specifications we include observations *inside* HSO areas. We instrument the listings rate with a dummy indicating whether an HSO has been implemented. Standard errors are clustered at the zipcode level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

ings rate within 500m the coefficient is slightly higher than the baseline estimate. Hence, our results are rather insensitive to the area choice.

Because we have to impute listings data for the months where we do not have Airbnb data, one may criticize the listings rate variable. To show robustness, we first use only the months for which we have actual Airbnb data. In column (7) we show that this leads to a very imprecise estimate and a rather weak first stage. In any case, note that the point estimate is higher than the baseline estimate.

To investigate further whether our proxy for Airbnb listings matter, we use a different proxy for Airbnb intensity, by approximating listings using the first and last review and assuming that the property is continuously listed in between, following Zervas et al. (2017) and Barron et al. (2021). The mean *approximated* listings rate is 0.54, which is very comparable to the mean *imputed* listings (0.59). The cross-sectional correlation between the imputed and approximated measures is quite high ($\rho = 0.812$). However, more relevant, as we exploit variation over time in this measure, is that the correlation over time between these two measures is much lower ($\rho = 0.416$). In column (8) we show that we also find a positive and marginally significant effect of this alternative measure. If anything, the impact is somewhat stronger, albeit not statistically significantly different from the baseline estimate.

Table 9 focuses on rents. Again we instrument for the listings rate with the HSO dummy. The results show that the instruments are sufficiently strong and in Appendix A.4.9 we show that the HSO dummy also has the expected sign and is statistically significant and negative in all cases. In column (1) we find that when the listings rate increases by 1 percentage point (0.69 standard deviations), rents increase by 4.9%. The effect is slightly lower when we only include observations within 25km of an HSO border (column (2)). This effect is comparable to the results we found in Table 8.

The effect becomes higher when we exclude zip codes outside HSO areas that are within 1km of an HSO border (column (3)). In line with previous results, we do not find any effect of the listings rate when focusing on zip codes close to HSO borders (column (4)). If we control for distance to CBD×year and distance to the beach×year trends, the effects are comparable. The estimates in columns (5) and (6) are a bit higher than the effects on prices, but note that they are not statistically significantly different from the baseline estimate for prices.

7. The overall price effects of Airbnb and HSO

We continue to calculate the overall effects of Airbnb and HSO on the housing market using our IV estimates reported in column (2) of Table 8. As we use a spatial regression-discontinuity approach, these effects reflect the preferences of local households who live around the borders of the boundaries. We will assume that the estimated local effects apply to the whole county. In principle, we do not know to what extent these local estimates are representative of the whole county. However, in additional analysis, where we do not control for the distance to the border, we get somewhat larger effects of listings on prices, suggesting that the estimate we use is likely conservative. Furthermore, when we apply a difference-in-difference strategy we obtain similar results.

In Table 10 we investigate the total effects of Airbnb and HSOs on average property prices for LA County as a whole and for specific areas, based on our estimates combined with descriptive information on house prices and the number of listings in these areas. To be more precise, we evaluate the total effect of Airbnb using the listings rate in these areas as of September 2018. We then consider two counterfactual scenarios; one where no HSOs would have been implemented and another where HSOs would apply to all cities in LA County. As the rent effect is very similar to the effects on prices (for which we provided evidence in Section 6.4), we just report price effects here, and use a discount rate of 3.3% (obtained from Koster and Pinchbeck, 2021).

In all scenarios, we assume that the effect of Airbnb listings is linear.[43] This assumption is likely innocuous when we focus on the price effects for LA County as a whole, and likely reasonable when we focus on areas with listings rates not too far from the average listing rate (equal to 1.21%), but for areas with very high listings rates (such as Venice), the predicted price effects should be interpreted with caution.

Our estimates imply that the gains of Airbnb for LA County as a whole are quite modest (3.6%). This result makes sense because many areas in LA counties have a low listings rate. However, there are also areas with higher listings rates. It is for example interesting to focus on areas within 5km of Hollywood's Walk of Fame, a major tourist hotspot, where the listings rate is more than four times the County's average. When we focus on these areas, the house price effect due to Airbnb is estimated to be 14.7%, which we consider being substantial. When we limit ourselves to areas within 2.5km of the Walk of Fame, we find even a more pronounced effect of 20%. One may wonder whether these effects are realistic and how they compare with nominal changes in prices during this period. It appears that nominal house prices within 5km of Hollywood have increased by more than 40% in the last 10 years, so it seems that our estimated effects are not unrealistically high, and explain about 1/4 of the nominal price increase.

We also consider the effects in beach towns. Within 2.5km of the beach, the price increase due to Airbnb is estimated to be equal to 4.8%.

[43] Because we have only one single instrument, this assumption is essentially non-testable given our econometric approach.

Table 10
Overall price effects of Airbnb (in 2018).

		Baseline scenario			Counterfactual scenario 1: no HSOs			Counterfactual scenario 2: only HSOs		
	Average house price (in 1000 $)	*Listings rate (in %)*	*in % of the house price*	*Yearly effect (in $)*	*Listings rate (in %)*	*in % of the house price*	*Yearly effect (in $)*	*Listings rate (in %)*	*in % of the house price*	*Yearly effect (in $)*
Total predicted price effects of Airbnb listings:										
LA county	1053	1.21	3.62	1258	1.26	3.78	1313	0.91	2.73	949
Total predicted price effects near Hollywood:										
Hollywood <10km	1688	3.07	9.22	5136	3.10	9.29	5174	2.86	8.59	4786
Hollywood <5km	1960	4.89	14.66	9483	4.92	14.77	9549	4.54	13.63	8814
Hollywood <2.5km	2446	6.68	20.05	16,182	6.70	20.10	16,225	6.17	18.53	14,955
Total predicted price effects near the beach:										
Beach <10km	1099	1.58	4.75	1723	1.64	4.93	1788	1.38	4.14	1502
Beach <5km	1128	1.93	5.79	2154	2.03	6.09	2266	1.69	5.06	1884
Beach <2.5km	1113	2.44	7.32	2691	2.57	7.73	2839	2.13	6.38	2344
Total predicted price effects for specific neighborhoods:										
Venice	1212	12.77	38.33	15,327	12.77	38.33	15,327	8.92	26.78	10,709
West Hollywood	1593	3.55	10.65	5597	5.10	15.29	8038	3.55	10.65	5597
Malibu	2193	5.89	17.67	12,791	5.89	17.67	12,791	4.15	12.45	9009
Santa Monica	1645	1.76	5.29	2870	2.80	8.40	4564	1.76	5.29	2870
Redondo Beach	888	1.17	3.51	1029	1.49	4.46	1308	1.17	3.51	1029
Pasadena	928	0.96	2.88	882	1.29	3.87	1184	0.96	2.88	882

Notes: Information is for September 2018. To estimate the yearly effects, we assume a discount rate of 3.3% (obtained from Koster and Pinchbeck, 2021). We further assume that rents are equal to discounted house prices.

If we concentrate on specific cities and neighborhoods, the price effects of Airbnb vary substantially. In one of the most popular LA neighborhoods – Venice – the total price increase is more than 30%. On the other hand, in Pasadena (which is about 15km from Downtown LA), the effects of Airbnb are modest.

Let us consider the two counterfactual scenarios. First, we consider that all HSOs are abandoned. Within 2.5km of a beach, this implies that the listings rate and house prices increase respectively by about 5% and 0.3%. For Santa Monica, which is well known for its strict HSO, the listings rate would increase by 60% and the house price by almost 2.5%, which is non-negligible. For locations near Hollywood, abandoning HSOs does not imply large changes in property values, because hardly any areas within close distance of Hollywood are targeted by HSOs.

By contrast, if all cities would implement HSOs this can have large effects in areas attractive to tourists. For example, in Venice, the listings rate would drop by 30% and house price by 11.6%. Hence, HSOs are likely to have large effects in areas attractive to tourists.

Our results also imply that in neighborhoods attractive to tourists, the distributional consequences of Airbnb are grave: in popular areas, incumbent homeowners have benefited more than $3 – 15 thousand per year due to Airbnb, whereas renters likely lost a similar amount, as renters are not allowed to list their property on Airbnb while paying higher rents at the same time.

As a consequence, there are clear distributional implications of HSOs. Homeowners will lose from the HSO, as the demand for housing will decrease. This effect is due to less efficient use of housing (because properties are not available for their most profitable use). However, (long-term) renters are likely to gain because more houses become available for rent so rents decrease. This offers a plausible explanation as to why cities around the world that have heavily restricted STRs typically have a high share of renters.[44]

[44] Conditional on income, there is a positive correlation between the share of renters and the introduction of the HSO within the Los Angeles area, see Appendix A.5. We perceive this result as suggestive only, as we do not have exogenous variation in the share of renters. For 29 other US. cities, we also find suggestive evidence as the correlation between the maximum allowed number of rental days (an inverse measure of stringency) and the share of renters is −0.25.

8. Conclusions

We have seen a spectacular growth of online short-term housing rental platforms in recent years. So, what is the effect of regulation of short-term housing-rentals on the housing market? We focus on Los Angeles County, where 18 cities have implemented Home Sharing Ordinances that restrict short-term rentals between 2014 and 2018. Using microdata for house prices, and listings, we apply a Spatial Panel Regression-Discontinuity Design around the borders of those areas and exploit the differences in the timing of the HSOs. Home Sharing Ordinances reduce Airbnb listings by about 50% and reduce house prices by 2% on average, which captures the fact that houses cannot be used for their most profitable use anymore. Using aggregate data and a difference-in-differences estimation strategy we find essentially the same effects for rents. Forbidding short-term rentals may lead to a reallocation away from privately-owned housing towards the long-term rental market – a housing supply effect.

Our estimates imply that Airbnb regulation has stark distributional implications because it induces losses for homeowners that are very substantial in areas that are popular for tourists. The opposite holds for households who typically rent and who can only gain from regulation as it increases rental housing supply and therefore reduces rents.

Our estimates imply that the total effect of Airbnb on property values in LA County is modest (3.6%). This makes sense because in large parts of this County, Airbnb is not so popular. However, in areas attractive to tourists, where the Airbnb listings rates are quite high, the effects of Airbnb are substantial. Within 2.5km of Hollywood, for example, the increase in property values is almost 15%.

Appendix

Data appendix

Below, in Table A1, we report the results of our data gathering endeavors. Ready-to-use data on Home Sharing Ordinances is not available, so we have browsed the Internet and phoned local officials to know whether the city has implemented an HSO sometime during our study period. For each city, we report whether it has implemented an HSO, whether the listing of rooms is permitted, whether an STR needs to register at the municipality and whether officially STRs are not allowed ac-

Table A1
Home sharing ordinances and STR regulations in LA County.

Name of city	Year and month of implementation		HSO	Home sharing not allowed	Register STR	STR not in zoning code	Source
Agoura Hills			0	0	0	0	phone interview
Alhambra			0	0	0	0	phone interview
Arcadia	2017	7	1	1	1	0	phone interview
Artesia			0	0	0	0	web search
Azusa			0	0	0	1	web search
Baldwin Park			0	0	0	1	phone interview
Bell			0	0	0	1	web search
Bell Gardens			0	0	0	1	web search
Bellflower			0	0	1	0	phone interview
Beverly Hills	2014	9	1	1	1	0	web search
Bradbury			0	0	0	1	web search
Burbank	2014	6	1	1	1	0	web search
Calabasas	2018	1	1	0	1	0	web search
Carson			0	0	0	1	phone interview
Cerritos	2016	8	1	1	1	0	web search
Claremont			0	0	0	1	phone interview
Commerce			0	0	0	1	web search
Compton			0	0	1	0	web search
Covina			0	0	0	1	phone interview
Cudahy			0	0	0	1	web search
Culver City			0	0	0	1	phone interview
Diamond Bar			0	0	0	1	web search
Downey			0	0	0	1	phone interview
Duarte			0	0	0	1	web search
El Monte			0	0	0	1	phone interview
El Segundo			0	0	0	0	web search
Gardena			0	0	0	1	web search
Glendale			0	0	0	0	phone interview
Glendora			0	0	0	1	web search
Hawaiian Gardens			0	0	0	0	web search
Hawthorne			0	0	0	1	web search
Hermosa Beach	2016	6	1	1	1	0	web search
Hidden Hills			0	0	0	1	web search
Huntington Park			0	0	0	1	web search
Industry			0	0	0	0	web search
Inglewood			0	0	0	1	web search
Irwindale			0	0	0	1	web search
La Canada Flintridge			0	0	0	1	web search
La Habra Heights			0	0	0	1	web search
La Mirada			0	0	0	0	web search
La Puente			0	0	0	1	web search
La Verne			0	0	0	1	web search
Lakewood			0	0	0	0	web search
Lancaster			0	0	0	1	web search
Lawndale	2017	7	1	1	0	0	web search
Lomita			0	0	0	0	web search
Long Beach			0	0	0	0	web search
Los Angeles			0	0	0	0	web search
Lynwood			0	0	0	1	web search
Malibu	2016	10	0	0	1	0	web search
Manhattan Beach	2015	6	1	1	1	0	web search
Maywood	2018	4	1	1	0	0	web search
Monrovia			0	0	0	0	web search
Montebello			0	0	0	1	web search
Monterey Park			0	0	0	1	web search
Norwalk			0	0	0	1	web search
Palmdale			0	0	0	1	web search
Palos Verdes Estates	2016	9	1	1	1	0	web search
Paramount			0	0	0	1	web search
Pasadena	2017	10	1	0	1	0	web search
Pico Rivera			0	0	0	1	web search
Pomona			0	0	0	1	web search
Rancho Palos Verdes	2016	7	1	1	1	0	web search
Redondo Beach	2016	6	1	1	1	0	web search
Rolling Hills	2016	12	1	1	1	0	web search
Rolling Hills Estates	2016	12	1	1	1	0	web search
Rosemead			0	0	0	1	web search
San Dimas			0	0	0	1	phone interview
San Fernando			0	0	0	0	phone interview
San Gabriel			0	0	0	1	phone interview

(*continued on next page*)

Table A1 (*continued*)

Name of city	Year and month of implementation		HSO	Home sharing not allowed	Register STR	STR not in zoning code	Source
San Marino			0	0	0	1	web search
Santa Clarita			0	0	0	0	phone interview
Santa Fe Springs			0	0	0	0	web search
Santa Monica	2015	6	1	0	1	0	web search
Sierra Madre			0	0	0	0	web search
Signal Hill			0	0	0	0	web search
South El Monte			0	0	0	1	web search
South Gate			0	0	0	0	web search
South Pasadena			0	0	0	1	phone interview
Temple City			0	0	0	1	phone interview
Torrance	2016	4	1	0	1	0	web search
Vernon			0	0	0	0	web search
Walnut			0	0	0	1	web search
West Covina			0	0	0	0	web search
West Hollywood	2015	9	1	1	1	0	web search
Westlake Village			0	0	0	0	phone interview
Whittier			0	0	0	1	phone interview
Unincorporated			0	0	1	0	web search

Note: We obtain information from the internet from: https://la.lawsoup.org/legal-guides/laws-by-topic/short-term-vacation-rentals/, https://www.dailybreeze.com/2016/03/02/redondo-beach-becomes-latest-south-bay-city-to-crack-down-on-short-term-rentals/, https://www.newportbeachca.gov/government/departments/finance/revenue-division/short-term-rentals,https://la.curbed.com/2014/3/24/10126966/the-few-places-in-los-angeles-where-airbnbs-might-be-legal, https://www.latimes.com/tn-blr-burbank-changes-housing-rules-20140628-story.html, https://beverlyhills.granicus.com/, https://www.beverlyhills.org/cbhfiles/storage/files/5614863821749456971/ShortTermRentals-Enforcement.pdf, https://www.pasadenastarnews.com/2017/07/08/new-rules-are-coming-for-la-airbnb-hosts-heres-what-the-city-is-planning/, https://www.mykawartha.com/news-story/8796058-rolling-hills-unhappy-with-status-quo-on-short-term-rentals/, https://www.rpvca.gov/DocumentCenter/View/8725/Agenda-Item-2_RPV_SR_2016_07_12_-Short-Term-Vac-Rentals?bidId=, https://tbrnews.com/news/redondo_beach/why-redondo-beach-wants-to-get-rid-of-airbnb-in/, https://www.lakewoodcity.org/civicax/filebank/blobdload.aspx?BlobID=27108, https://www.cerritos.us/NEWS_INFO/news_press_releases/2016/september/rentals.php, https://cerritos.granicus.com/, https://www.huntingtonbeachca.gov/announcements/announcement.cfm?id=917, https://qcode.us/codes/lawndale/revisions/1139-17.pdf, https://www.lomita.com/cityhall/government/ccMeetings/minutes_2016-09-06.pdf, https://www.longbeach.gov/press-releases/community-to-help-shape-plans-for-a-short-term-rental-ordinance/, https://cityofmaywoodpark.com/wp-content/uploads/2015/09/2018-2-ORDINANCE-2018-short-term-rentals-bnbs.pdf, https://sausalito.granicus.com/, https://sireagendas.westcovina.org/sirepub/cache/2/0n1f34d04rmjm3ook0naprwr/27509409222018040358852.PDF, https://ttc.lacounty.gov/othertaxes/docs/FAQsforOnlineHostingPlatformFINAL.pdf."

Fig. A1. Evolution of number of listings per wave.

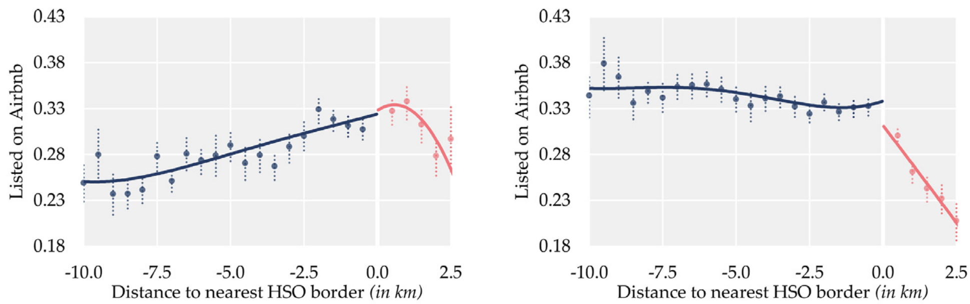
cording to the residential zoning code. Furthermore, we list the sources from which we get the information.

In Fig. A1, we provide the number of active listings per wave. The Figure also indicates that we have approximately 90 thousand unique listings.

A2. Bandwidth selection

We use the approach proposed by Imbens and Kalyanaraman (2012), who show that the optimal bandwidth can be estimated as:

$$b^* = C_K \cdot \left(\frac{\hat{\sigma}^2_-(c) + \hat{\sigma}^2_+(c)}{\hat{f}(c) \times \left((\hat{m}^{(2)}_+ - \hat{m}^{(2)}_-)^2 + (\hat{r}_+ + \hat{r}_-) \right)} \right)^{\frac{1}{5}} \times N^{-\frac{1}{5}}, \tag{A1}$$



(A) AIRBNB LISTING, BEFORE HSO

(B) AIRBNB LISTING, AFTER HSO

Fig. A2. Variation near HSO borders before and after the HSO. *Notes*: Spatial differences in variables are *conditional* on month fixed effects. Hence, we identify the effects *over space*. Negative distances indicate areas outside HSO areas and areas inside HSO areas. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block level. We include a 4rd-order polynomial in untreated areas and a 2nd-order polynomial in treated areas.

where the constant $C_K = 3.4375$ and N is the number of observations. $\hat{\sigma}^2_-$ and $\hat{\sigma}^2_+$ are the conditional variances of respectively ℓ_{ikt} or $\log p_{ijt}$ given $d_i = c$ on both sides of the threshold (indicated with ' - ' and ' + '). $\hat{f}(c)$ denotes the estimated density of d_i at c. $\hat{m}^{(2)}_-$ and $\hat{m}^{(2)}$ are estimates of the second derivatives of a function of the dependent variable on the distance to the boundary d_i. $\hat{r}_+$ and $\hat{r}_-$ are estimated regularization terms that correct for potential error in the estimation of the curvature of $m(d)$ on both sides of the threshold.

Because we exploit variation in prices and the HSO over time to determine the bandwidths, we first demean the variables by month and property or census block fixed effects. In many specifications we add additional covariates (*e.g.* housing characteristics). We then determine the conditional variance of the dependent variable given all covariates and fixed effects at the threshold, so $\hat{\sigma}^2_-(c \mid x_{ikt}, \lambda_i, \theta_k t)$ and $\hat{\sigma}^2_+(c \mid x_{ikt}, \lambda_i, \theta_k t)$. Usually, adding covariates does not affect the optimal bandwidth much (Imbens and Kalyanaraman, 2012). Indeed, adding a wide array of controls barely influences the optimal bandwidth in our specifications.

A3. Other graphical evidence

In this Appendix, we review ancillary graphical evidence that supports the identifying assumptions we make in our research design. In Appendix A.3.1 we first consider cross-sectional variation in the listing probability and house prices around the borders of HSO areas. In Appendix A.3.2 we investigate sorting and the provision of public goods. Appendix A.3.3 considers discontinuities in housing characteristics and Appendix A.3.4 investigates jumps in densities of key variables after the HSO has been implemented.

A3.1. Cross-sectional differences in listings

In Fig. A2 we illustrate cross-sectional differences in listings *before* and *after* the HSOs were implemented. In Fig. A2a, we compare the probability of being listed *before* an HSO was implemented on both sides of the border. It is clear that there was essentially no difference between HSO areas and surrounding areas. However, after the HSO was implemented, the probability of being listed is approximately 4 percentage points lower (see Fig. A2b).

A3.2. Sorting and public goods

In spatial RDDs, one should be concerned about sorting. It might be that a discontinuity in prices due to implementation is partly caused by a change in the demographic composition of the neighborhood (see Bayer et al., 2007, for cross-sectional evidence on school districts). Using Census Block Group level data from the *American Community Survey* (ACS) 2014–2016, Fig. A3 shows that all household characteristics are continuous at the border. Importantly, changes in population density and the share of owner-occupied housing are the same on both sides of the border (Fig. A3a). The latter is noticeable as one might expect to see a relative increase in home-ownership (as to be able to rent out to tourists) in the areas where Airbnb is still allowed if rents do not change. The reason may be that in the short run it may be hard to evict long-term renters. Hence, HSOs did not seem to have led to a fundamental change in housing tenure. We also do not detect changes in the household composition, measured by income, the share of blacks, single households, or median age. Nevertheless, in sensitivity analyses (see Appendix A.4.8) we will control for changes in the housing stock and demographic characteristics and show that this does not affect the results.

One could also be concerned that a discontinuity in prices arises because of a differential provision of public goods. While temporal changes in the quality of public goods are usually not abrupt, large cross-sectional differences in public good quality may provoke sorting. An important public good is school quality (see Black, 1999; Bayer et al., 2007).[45] Using 2017 test score data of students (observed at the individual school level) between the 3rd and 11th grade on English and Mathematics from the California Assessment of Student Performance and Progress (CAASPP), we checked for possible discontinuities in changes of student performance around the HSO borders. Fig. A3g and h show that no such discontinuity exists, indicating that the HSO is unlikely to be correlated to school quality.[46]

A3.3. Discontinuities in housing characteristics

An important assumption in the panel regression-discontinuity design is that changes in covariates, except for the treatment variable, are continuous at the border. We, therefore, investigate in Fig. A4 whether changes in housing characteristics over time do not show discontinuities.

Fig. A4a highlights that the change in the share of condominiums is not statistically significantly different at the border of HSO areas. Fig. A4b further shows that there is small discrete jump in the change in construction year at the border, but this jump is only statistically significant at the 10% level. This would imply that after an HSO has been implemented, slightly newer properties are traded in HSO areas. To control for this effect we include construction decade dummies in the regressions.

For property size, we also find a small jump, but the difference is again only marginally statistically significant. To the extent the price effect partly captures changes in the houses on offer in HSO areas, we control for the property size in the house price regressions. Finally, we do not find a statistically significantly different jump in the number of bedrooms (Fig. A4d).

A3.4. Conditional McCrary tests

A test for discontinuities in densities of the running variable *before the introduction of the HSO* might be informative, as a discontinuity might be indicative of unobserved housing or household traits (*e.g.* different

[45] We also checked for other spatial differences in *e.g.* property taxes, but we did not find any meaningful difference.

[46] Note that not all school districts are pertaining to one city. For example, the City of Carson is served by the Los Angeles and Compton school districts. West-Hollywood is also part of the LA school district.

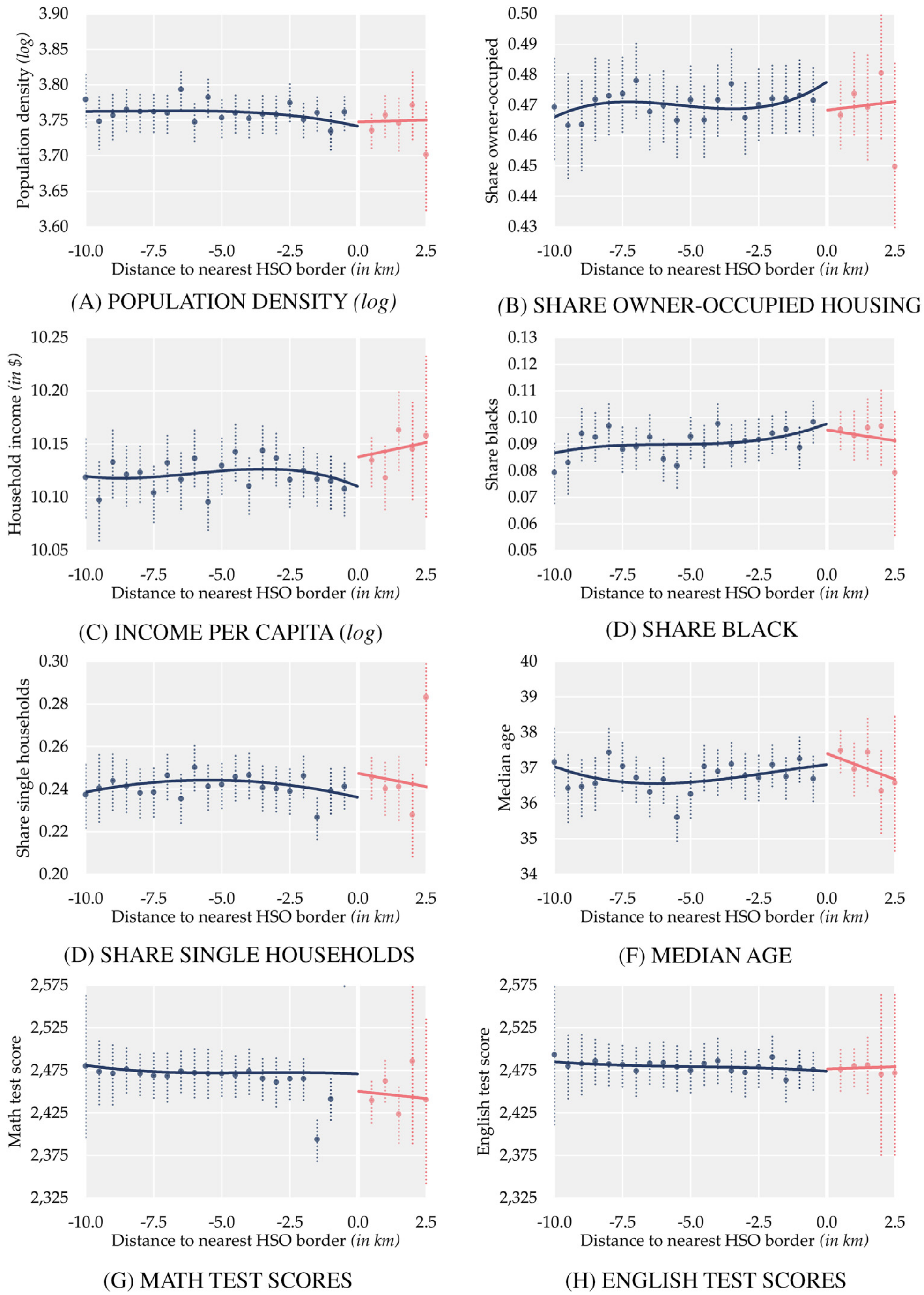


Fig. A3. Sorting along the border. *Notes*: Spatial differences in variables are *conditional* on census block group and border segment×month fixed effects. Hence, we identify the effects *over time*. Negative distances indicate areas outside HSO areas and areas inside HSO areas but *before* treatment. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block group level. We include 3rd-order polynomials in untreated areas and a linear function in treated areas.

types of households sorting themselves into treated areas) that are potentially correlated with the treatment. However, this test should take into account the geography of the area and borders of the areas, as discontinuities in listings or housing transactions may also indicate that some areas border mountainous areas, parks or the sea.

We, therefore, estimate a two-step density test in the spirit of McCrary (2008). In the first step, we estimate the spatial distribution for buildings employing McCrary's methodology. In the second step, we estimate this distribution for listings and housing transactions respectively. Our test is then the difference in the estimated densities between the second and first step. Hence, a negative (or positive) density differential would indicate that there are fewer (or more) listings/transactions than expected given the spatial distribution of buildings.

The results are reported in Fig. A5. Fig. A5a tests for the continuity of the density differential of listings *before* an HSO was implemented. We find that there is no difference in the density for listings at the HSO border. We repeat the same exercise, but now for housing transactions in Fig. A5b. This test indicates a discontinuity due to a higher density of housing transactions just across the border in HSO areas. Note however that the discontinuity is economically very small, so we do not consider this as a problem.

We repeat this exercise by estimating the adapted McCrary's density test *after* an HSO was implemented, but given the spatial distribution of buildings in 2014. In Fig. A6a we show that Airbnb listings are now discontinuous after the HSO. The density is much lower in treated areas, which is in line with the finding that listings have been reduced due to the implementation of HSOs. For house prices (Fig. A6b) we find

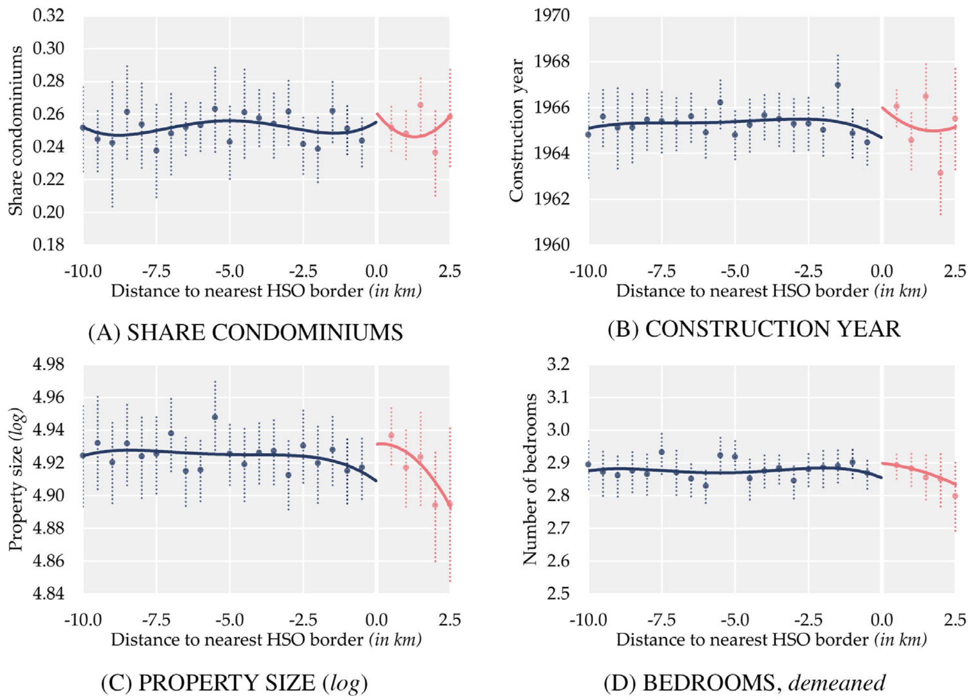


Fig. A4. Housing transactions: variation near HSO borders. *Notes:* Spatial differences in variables are *conditional* on census block and border segment×month fixed effects. Hence, we identify the effects *over time*. Negative distances indicate areas outside HSO areas and areas inside HSO areas but *before* treatment. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block level. We include a 4rd-order polynomial in untreated areas and a 2nd-order polynomial in treated areas.

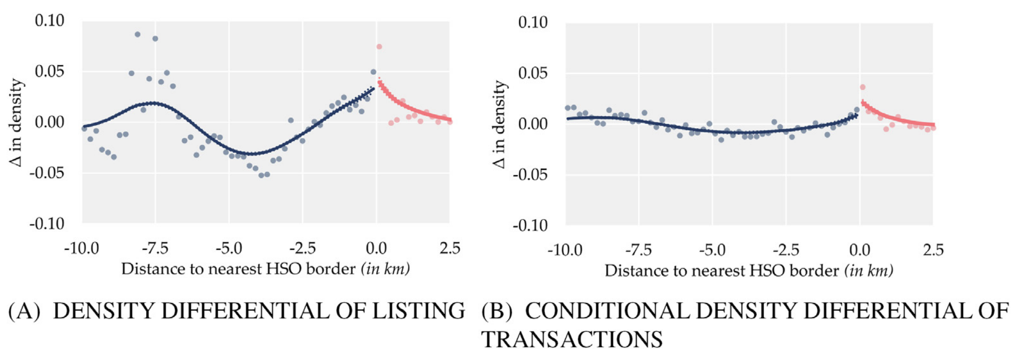


Fig. A5. Conditional McCrary density tests before HSOs. *Notes*: We focus on observations *before* implementation of the HSO. Negative distances therefore indicate areas outside HSO areas. The dots are conditional densities at every 200m interval. The dotted lines denote 95% confidence intervals. On the *y*-axis we ploy the difference in densities of McCrary's density test between respectively listings and housing transactions and the density of buildings.

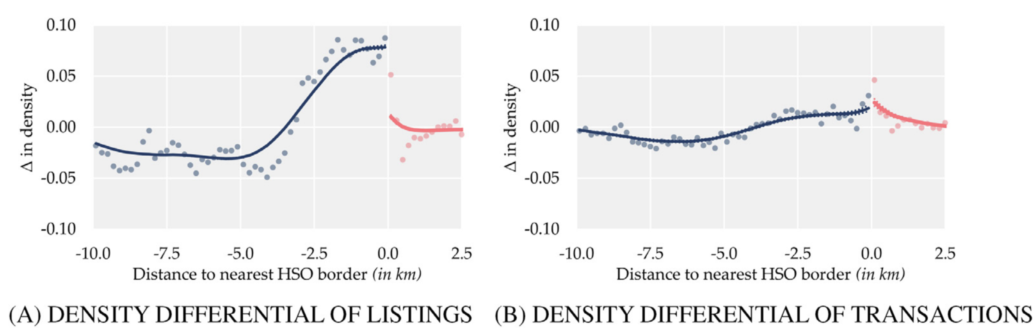


Fig. A6. Conditional McCrary density tests after HSOs. *Notes*: We focus on observations *before* implementation of the HSO. Negative distances therefore indicate areas outside HSO areas. The dots are conditional densities at every 200m interval. The dotted lines denote 95% confidence intervals. On the *y*-axis we ploy the difference in densities of McCrary's density test between respectively listings and housing transactions and the density of buildings.

essentially the same difference in density of transactions as in A5 b, which we think is reassuring: the HSO did not lead to a different market turnover on both sides of the HSO borders.

A4. Other regression results and robustness

In this part of the Appendix, we will subject our results to a wide range of robustness checks and report some additional results. Appendix A.4.1 first investigates the effects of HSOs in different cities on the listing probability and prices. In Appendix A.4.2 we investigate whether the HSO influenced Airbnb rental prices, close to and further away from the border of HSO areas. Appendix A.4.3 further investigates whether the supply of hotels has changed due to HSOs. Appendix A.4.4 further investigates the possibility of pre-trends in listings, prices, and rents. In Appendix A.4.5 we examine whether the standard errors change when accounting for cross-sectional and temporal

Table A2
City-specific number of observations.

	Airbnb – homes		*Airbnb – rooms*		*Housing transactions*	
	Before HSO (1)	*After HSO* (2)	*Before HSO* (3)	*After HSO* (4)	*Before HSO* (5)	*After HSO* (6)
Arcadia	1930	967	3590	1793	1819	262
Beverly Hills	0	13,644	0	4821	174	670
Burbank	0	4212	0	4863	373	2848
Calabasa	570	288	720	357	1118	11
Cerritos	112	98	320	280	974	502
Hermosa Beach	2315	2666	996	1118	528	294
Lawndale	450	227	590	293	480	105
Manhattan Beach	696	4542	242	1555	553	882
Maywood	33	12	0	0	146	0
Palos Verdes	178	163	142	117	508	256
Pasadena	7463	3733	5327	2662	5328	297
Rancho Palos Verdes	485	553	509	583	1277	774
Redondo Beach	2376	2733	2293	2603	2183	1320
Rolling Hills	24	21	0	0	59	17
Rolling Hills Estates	80	70	72	63	361	126
Santa Monica	4170	27,041	2178	14,221	928	1457
Torrance	799	1209	1469	2193	2567	2108
West Hollywood	6709	18,522	2803	7636	719	949

dependence. We then proceed by reporting the first-stage results in Appendix A.4.7. We subject our results to a wide array of additional robustness checks in Appendix A.4.8. In Appendix A.4.9 we check for sensitivity of the results using the *Zillow* data, so the results using a DiD estimation strategy.

A4.1. City-specific effects

Here we analyze city-specific effects. We re-estimate the preferred specification where we include border segment×month fixed effects. Given that the number of observations for many cities is limited, one expects that only for a handful of cities the coefficient is statistically significant. On the other hand, if there is a substantial number of coefficients with the wrong sign, and these coefficients are statistically significant, then our identification strategy would be less convincing. We first show for each HSO city the number of treated and untreated observations in Table A2. It is shown that some cities, such as Cerritos, Maywood, Rolling Hills, and Rolling Hills Estates have very few observations. Hence, we should be careful not to take these results too seriously. We report the results in Table A3.

In columns (1) and (2) we report the results for listings of entire properties and rooms, respectively. Column (1) shows that the point estimates related to HSOs are almost always negative and in three cases highly statistically significant. Column (2) also shows that most coefficients are statistically significant and negative and comparable to the results in column (1). For Maywood and Rolling Hills, we find statistically significant effects with an incorrect sign, but this is due to very low number of observations (see Table A2).

In column (3) we focus on house prices. The results show that the effect is in most cases negative, although often imprecise. We find statistically significant negative effects, whereas the positive effects are far from being statistically significant.

A4.2. HSOs and Airbnb short-term rental prices

Did HSOs have an impact on short-term rental prices of Airbnb properties? We explore this in Table A4. These are hedonic price analyses using observations of properties that are listed (in our dataset). We emphasize that spatial equilibrium theory indicates that *at the border* short-term rental prices would not change, because tourists are unlikely to differentiate between accommodation in HSO areas and immediately adjacent areas and are therefore unlikely to be willing to pay higher prices in areas that have implemented HSOs.

In column (1) we estimate the Panel RDD and do not find a statistically significant effect of an HSO on Airbnb rental prices. This also holds if we include border segment×month fixed effects in column (2) and change the optimal bandwidth in columns (3), (4), and (5). In column (6) we include property fixed effects. In all cases the effect of an HSO on prices is economically negligible and statistically insignificant, confirming spatial equilibrium theory.

We extend these results by using the same difference-in-differences approach as in Section 6.4. In Table A5 we report the results. In column (1) we include all observations in LA County. The effect of HSOs is small and statistically insignificant. This also holds if we only include observations within 25km of any HSO border. In column (3), we exclude observations that are close to (<1km) a border. Column (4) further controls for distance to CBD and distance to the beach trends. In the final column we exclude observations within 5km of an HSO border. All results are economically small and far from being statistically significant.

All in all, we do not find any evidence that Airbnb rental prices are affected by HSOs, which is in keeping with the notion that the market for Airbnb properties is competitive and tourist demand for local accommodation is elastic.

A4.3. HSOs and formal accommodation

We investigate here how the formal hotel industry benefited from the implementation of HSOs. Again, at the border, we expect few effects. However, when comparing HSO areas with areas further away from the border, we might expect to see an increase in the number of officially registered traveler accommodations, which we investigate here (we do not have information on hotel rates).

We obtain yearly data from the *County Business Patterns* at the zip code level and keep NAICS-sector 72111, which are traveler accommodations, including hotels, casino hotels, and other traveler accommodations. Because the latest County Business Pattern data is from 2016, we also include 2012 and 2013, so that we have data for 5 years. We take the same approach as in Section 6.4, where we use a DiD design. Table A6 reports the results of several Poisson regressions.

In column (1) we include all zipcodes in LA County. The point estimate suggests that the number of traveler accommodations increases due to HSOs by $\exp(0.0557) - 1 = 5.7\%$, which is sizable. However, the coefficient is quite imprecisely estimated. This also holds for the other specifications, where we include zip codes that are further away from HSO borders (>1km or >2.5km). Hence, we think Table A6 provides suggestive evidence that the number of travelers accommodations has increased due to the HSO.

Table A3
City-specific effects for listings and prices, all observations.

	(Dep.var.: entire property is listed) (1)	*(Dep.var.: home sharing is listed)* (2)	*(Dep.var.: log of house price in $)* (3)
HSO implemented×Arcadia	-0.0471	-0.0346	-0.0683**
	(0.0644)	(0.0487)	(0.0278)
HSO implemented×Beverly Hills			0.0195
			(0.0266)
HSO implemented×Burbank			-0.0600**
			(0.0272)
HSO implemented×Calabasas	0.0529	0.1003	-0.1876***
	(0.0932)	(0.0825)	(0.0417)
HSO implemented×Cerritos	-0.1834	-0.1716	-0.0496**
	(0.2053)	(0.1160)	(0.0224)
HSO implemented×Hermosa Beach	-0.2177***	-0.3009***	-0.0542
	(0.0551)	(0.0752)	(0.0398)
HSO implemented×Lawndale	0.0907	0.1008	0.0094
	(0.1022)	(0.1101)	(0.0511)
HSO implemented×Manhattan Beach	-0.0642*	-0.0557	-0.0632**
	(0.0339)	(0.0437)	(0.0257)
HSO implemented×Maywood	0.6983***		
	(0.0268)		
HSO implemented×Palos Verdes Estates	-0.0090	-0.0504	-0.0520
	(0.1320)	(0.1307)	(0.0326)
HSO implemented×Pasadena	0.0508	0.0630	-0.0141
	(0.0429)	(0.0496)	(0.0298)
HSO implemented×Rancho Palos Verdes	-0.1633	0.0371	-0.0124
	(0.1151)	(0.0878)	(0.0174)
HSO implemented×Redondo Beach	-0.0272	-0.0418	-0.0192
	(0.0479)	(0.0549)	(0.0195)
HSO implemented×Rolling Hills	0.5544**		-0.0819
	(0.2218)		(0.0559)
HSO implemented×Rolling Hills Estates	-0.1772	-0.1196	-0.0551**
	(0.1872)	(0.0953)	(0.0263)
HSO implemented×Santa Monica	-0.1712***	-0.0224	0.0022
	(0.0220)	(0.0259)	(0.0227)
HSO implemented×Torrance	0.0783*	0.0915*	-0.0379***
	(0.0462)	(0.0513)	(0.0147)
HSO implemented×West-Hollywood	-0.0702***	-0.0622***	0.0263
	(0.0132)	(0.0211)	(0.0246)
Property characteristics	No	No	Yes
Spatio-temporal trend variables included	Yes	Yes	Yes
Listing fixed effects	Yes	Yes	No
Census block fixed effects	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes
Number of observations	270,336	171,448	63,277
Bandwidth, b (in km)	1.6674	1.8255	1.8089
R^2	0.3523	0.3446	0.8447

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

A4.4. Anticipation effects and pre-trends

Here we investigate pre-trends of listings, house prices, and rents in more detail. In Fig. A7 we focus on listings. Clearly, we do not observe a pre-trend even if we extend the period of observation to two and a half years before the implementation of the HSO. The effects turn negative after about a year, although these are only statistically significant at the 5% level two years after the treatment.

Let us now turn to the effect of HSOs on prices. Note that the Panel RDD would imply that price trends in HSO areas and neighboring areas are the same before the HSO. However, if anticipation effects are important because HSOs are announced or anticipated, prices may already adjust before the actual treatment. We did not find evidence for this in Fig. 6 in Section 6.2, but we investigate this further in Fig. A8 by allowing for price changes 2.5 years before treatment. This implies that the reference category is composed of transactions in areas that will be treated in 2.75 years or later; and of transactions in areas close to treated areas.

We do not find evidence for pre-trends. That is before the treatment year there are generally no statistically significantly lower prices in treated areas if we focus on our baseline sample that includes housing transactions between 2014–2018. One may be concerned that this is an issue of precision, as the point estimate is still negative and around 2% the year before treatment. We emphasize that this may be due to the announcement of HSOs before actual implementation.[47] To the extent anticipation effects are important we are inclined to find an underestimate. Still, the long-run effect after 3 years is still about 1– 2%, albeit imprecise because of few observations. However, if we focus on a longer sample based on data since 2004, we find no evidence whatsoever for pre-trends, as the effect is not statistically significant and close to zero before treatment.

We test for even longer pre-trends (respectively 5 and 10 years before the treatment) in Section 6.3 by taking samples of house prices preceding the current sample. By running placebo regressions we show that there is no evidence that prices were already decreasing in areas where HSOs are to be implemented.

[47] For example, in the City of Los Angeles regulation was announced about half a year before it was implemented in July 2019. However, discussions about what type of regulation should be implemented in the City of LA have taken even longer.

Table A4
HSOs and Airbnb prices.

(Dependent variable: log of price per night)	*Panel RDD* (1)	*+ Border segment f.e.* (2)	*Bandwidth: $h^* \times 2$* (3)	*Bandwidth: $h^*/2$* (4)	*Bandwidth: $h^*/5$* (5)	*Property f.e.* (6)
HSO implemented	0.0051	0.0061	0.0052	0.0054	0.0081	0.0040
	(0.0090)	(0.0093)	(0.0089)	(0.0115)	(0.0093)	(0.0066)
Private room	-0.3534***	-0.3527***	-0.3569***	-0.3489***	-0.2140***	-0.2242***
	(0.0086)	(0.0086)	(0.0069)	(0.0119)	(0.0327)	(0.0193)
Shared room	-0.7756***	-0.7770***	-0.7658***	-0.8469***	-0.3083***	-0.3310***
	(0.0305)	(0.0306)	(0.0215)	(0.0447)	(0.0842)	(0.0430)
Accommodation size *(log)*	0.5197***	0.5164***	0.5055***	0.5081***	0.0992***	0.1114***
	(0.0092)	(0.0088)	(0.0073)	(0.0116)	(0.0194)	(0.0112)
availability	0.1921***	0.1934***	0.1920***	0.1885***	0.0242***	0.0263***
	(0.0065)	(0.0065)	(0.0054)	(0.0086)	(0.0038)	(0.0024)
Minimum of required nights *(log)*	0.0229***	0.0227***	0.0260***	0.0209***	-0.0155***	-0.0132***
	(0.0034)	(0.0034)	(0.0029)	(0.0044)	(0.0031)	(0.0024)
Maximum of required nights *(log)*	0.0057***	0.0057***	0.0050***	0.0060***	-0.0005	-0.0004
	(0.0016)	(0.0016)	(0.0013)	(0.0022)	(0.0024)	(0.0016)
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Listing fixed effects	No	No	No	No	No	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes
Number of observations	137,487	137,351	211,883	79,503	38,415	104,833
Bandwidth, b	1.7842	1.7826	3.5652	0.8913	0.3565	1.3353
R^2	0.7745	0.7772	0.7769	0.7843	0.9771	0.9768

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

Table A5
HSOs and Airbnb prices, DiD results.

(Dependent variable: log of price per night)	*All obs.* (1) OLS	*Outside HSO, <25km* (2) OLS	*Outside HSO, >1km, <25km* (3) OLS	*Outside HSO, >1km, <25km* (4) OLS	*Outside HSO, >2.5km, <25km* (5) OLS
HSO implemented	-0.0008	-0.0007	-0.0010	0.0011	-0.0000
	(0.0045)	(0.0045)	(0.0046)	(0.0046)	(0.0051)
Airbnb property characteristics	Yes	Yes	Yes	Yes	Yes
Distance to CBD×year trends	No	No	No	Yes	Yes
Distance to beach×year trends	No	No	No	Yes	Yes
Listing fixed effects	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	339,322	336,468	286,193	286,193	214,113
R^2	0.9782	0.9778	0.9779	0.9779	0.9782

Notes: In all specifications we include observations *inside* HSO areas. Standard errors are clustered at the zipcode level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

Table A6
HSOs and traveler accommodations.

(Dependent variable: number of accommodations)	*All obs.* (1) Poisson	*Outside HSO, <25km* (2) Poisson	*Outside HSO, >1km, <25km* (3) Poisson	*Outside HSO, <1km* (4) Poisson	*Outside HSO, >1km, <25km* (5) Poisson	*Outside HSO, >2.5km, <25km* (6) Poisson
HSO implemented	0.0557	0.0511	0.0550	0.0709	0.0508	0.0566
	(0.0500)	(0.0502)	(0.0505)	(0.0534)	(0.0541)	(0.0560)
Distance to CBD×year trends	No	No	No	No	Yes	Yes
Distance to beach×year trends	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1183	1121	1051	149	1051	872
Log-likelihood	-1,952	-1,851	-1,733	-256.8	-1,731	-1,426

Notes: In all specifications we include observations *inside* HSO areas. Standard errors are clustered at the zipcode level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

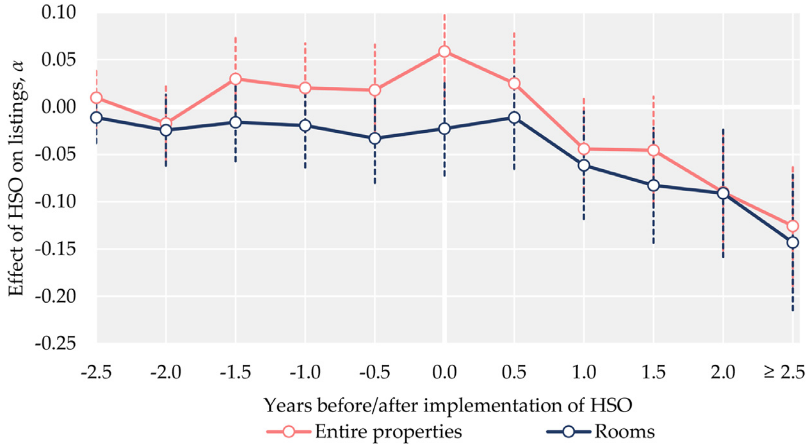


Fig. A7. An event study to the effect of the HSO on listings. *Notes*: The optimal bandwidth $b^* = 1.6692$. The dotted lines denote the 95% confidence bands.

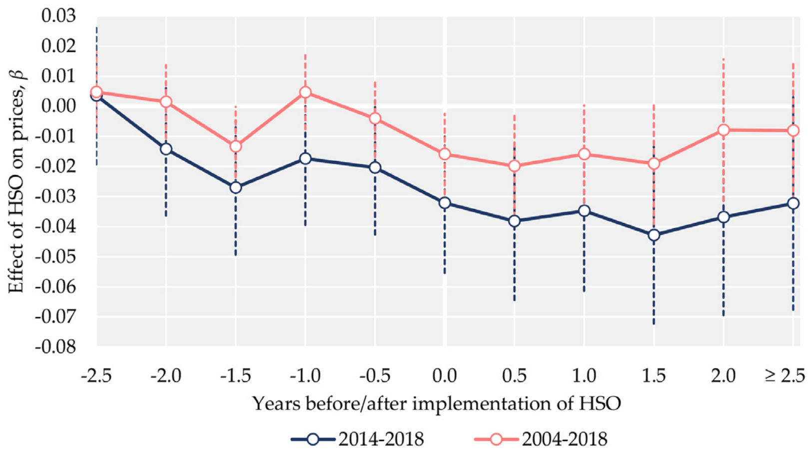


Fig. A8. An event study to the effect of the HSO on prices. *Notes*: The optimal bandwidth $b^* = 1.8089$. The dotted lines denote the 95% confidence bands.

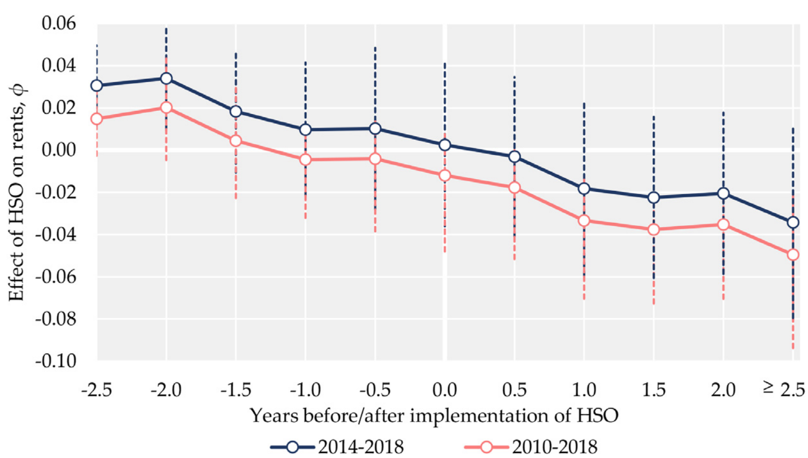


Fig. A9. An event study to the effect of the HSO on rents. *Notes*: In all specifications we include observations *inside* HSO areas and observations between 1 and 25km of an HSO area. The dotted lines denote the 95% confidence bands.

The robustness of pre-trends for the dataset on rents is investigated in Fig. A9. Here we use either the baseline data or extend the data to 2010. We do not find strong evidence for pre-trends, confirming earlier evidence, particularly when we use data since 2010 (*i.e.* the first year for which the *Zillow* data are available).

A4.5. Spatial HAC standard errors

Spatial data is usually not interdependent. More specifically, unobserved characteristics of a property (*e.g.* crime, maintenance quality) are likely correlated over space and time. Although these variables are unlikely to be correlated with the HSO and therefore do not affect the consistency of the estimated coefficients, spatial dependence may imply that the estimated standard errors are biased.

In this paper we cluster at the census block level to partly address this issue (see Moulton, 1990), but clustering implies strong parametric assumptions as to how observations relate to other observations. We aim to allow for more general forms of dependence. We therefore use Conley's (1999) procedure to allow for spatial dependence. We use a linear Bartlett kernel to determine kernel weights, indicating how one observation relates to the other. We use an initial spatial window, denoted by sw, equal to the bandwidth used in the RDD.

In column (1) of Table A7 we report the baseline specification with standard errors clustered at the census block level. If we then allow for

Table A7
Spatial HAC standard errors.

(Dependent variable: log of house price in \$)	*Baseline* (1)	$sw = 1 \times b^{*}km$ (2)	$sw = 2 \times b^{*}km$ (3)	$sw = 5 \times b^{*}km$ (4)	$sw = 10 \times b^{*}km$ (5)
HSO implemented	-0.0177***	-0.0177***	-0.0177***	-0.0177***	-0.0177***
	(0.0078)	(0.0067)	(0.0068)	(0.0069)	(0.0070)
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes
Property characteristics	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	63,275	63,275	63,275	63,275	63,275
R^2	0.9090	0.9090	0.9090	0.9090	0.9090
Bandwidth, b *(in km)*	1.8087	1.8087	1.8087	1.8087	1.8087
Spatial cut-off *(in km)*	—	1.8087	3.6174	9.0435	18.0872

Notes: We estimate standard errors corrected for cross-sectional dependence using a Bartlett kernel and given the indicated spatial cut-offs. *** $p < .01$, ** $p < .05$, * $p < .10$.

Table A8
HSOs and house prices: external effect.

(Dependent variable: log of house price)	*Share HSO 0-500m*	*Share HSO 0-100m*	*House type*	
	(1)	(2)	(3)	(4)
HSO implemented	-0.0243	-0.0424		
	(0.0152)	(0.0427)		
Share of land in HSO $0-500$m	0.0095			
	(0.0183)			
Share of land in HSO $0-100$m		0.0264		
		(0.0444)		
HSO implemented×single-family			-0.0155*	-0.0153*
			(0.0084)	(0.0084)
HSO implemented×condominium			-0.0209**	-0.0205**
			(0.0091)	(0.0091)
HSO implemented×condominium× before Watts v. Oak Shores				-0.0197
				(0.0169)
Property characteristics	Yes	Yes	Yes	Yes
Spatio-temporal trend variables	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes	Yes
Number of observations	63,261	63,340	63,297	63,297
Bandwidth, b *(in km)*	1.8081	1.8103	1.8103	1.8103
R^2	0.9090	0.9090	0.9090	0.9090

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

cross-sectional dependence within 1.86km in column (2), we find very similar, and even slightly smaller, standard errors. In the following specifications, we increase the spatial window to up to 10 times the optimal bandwidth (almost 20km) in column (5). If anything, the standard errors become slightly smaller but are very comparable to the results clustered at the census block level. Hence, we conclude that spatial dependence is not an issue of major concern.

A4.6. Negative external effects related to tourism.

In Table A8, we investigate to what extent negative external effects related to tourism play a role. This is important because the estimates discussed above are the *net* effects of 2 opposing mechanisms. One mechanism is that the HSO reduces demand for housing, which decreases house prices, whereas the other mechanism is that it reduces negative tourist externalities, which increases house prices. Alternatively, because we find that the net effect of the HSO is negative, the estimates may be interpreted as underestimates of the efficient use effect, where the size of the underestimate depends on the size of the externality effect.

To investigate the importance of the externality effect, we consider two approaches. The first approach is based on the idea that if an HSO reduces negative effects of tourism locally (*e.g.*, up to 500m of a property which is not allowed to use short-term letting), then this has two consequences: *(i)* an HSO reduces tourism externalities for properties just outside treated areas; *(ii)* the reduction in negative externalities due to the HSO is less for properties just inside HSO borders compared to properties that are fully surrounded by treated locations. Consequently, when the externality effect is substantial, the price effect may be different close to HSO borders.

To investigate this, we calculate the share of land within a 500m ring of a treated area. Hence, for houses further away than 500m from the border, the share is either zero (when located outside an HSO area or inside an HSO area but before treatment) or one (when located in a treated area), whereas for houses within 500m, the share is between zero and one. If there are substantial negative external effects of Airbnb, *conditional on the treatment dummy*, one expects to see price increases when the share of land in HSOs is higher (see for a similar approach in the context of measuring the external effects of land use regulation,

Table A9
Listings and house prices: First-stage results.

(Dependent variable: listings rate in %)	*Panel RDD* (1)	*+ Border segment f.e.* (2)	*Bandwidth: $h^* \times 2$* (3)	*Bandwidth: $h^*/2$* (4)	*Different thresholds 100m* (5)	*Different thresholds 500m* (6)	*Selected dates* (7)	*Approximated listings rate* (8)
HSO implemented	-0.3919***	-0.5882***	-0.5229***	-0.4875***	-0.7245***	-0.4519***	-0.5577***	-0.4148***
	(0.0735)	(0.0775)	(0.0723)	(0.0888)	(0.1257)	(0.0428)	(0.1628)	(0.0528)
Property characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend variables included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	83,766	83,037	134,074	52,115	91,623	70,232	15,509	99,284
Bandwidth, b *(in km)*	2.9429	2.9257	5.8513	1.4628	3.4276	2.297	3.8509	2.8797
R^2	0.6955	0.7248	0.6954	0.7432	0.5504	0.8886	0.7082	0.7087

Notes: We exclude transactions occurring within half a year after implementation of the HSO. We instrument the listings rate a dummy indicating whether an HSO has been implemented. Robust standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

Turner et al., 2014). As the added measure is quite collinear with the HSO measure, we do not find statistically significant effects for the HSO and share of HSO land within 500m. However, the point estimates have the expected signs: the treatment effect is now −2.4%, so slightly more negative, in line with the idea that we now only capture the effect on demand, while the effect of the share of land in treated areas is positive, 0.9%, in the direction suggested by theory. In addition, to examine whether the effect is more local (because the externalities may not spread out over 500m), we have as an alternative included the share of land in treated areas within 100m in column (2). This blows up standard errors even more because of severe collinearity, so not much can be learned from the latter exercise.[48]

It is imaginable that the externality effect of Airbnb is even more local, such that it only shows up within buildings. To investigate this, in column (3) we include an interaction term with housing type. If local negative externalities of Airbnb listings (*e.g.*, noise) *within buildings* are important, one expects that prices of condominiums have decreased less due to the HSO. This is not what we find (the difference in the effects for condominiums and single-family homes is not statistically significant). If anything, the effect of the HSO is slightly more pronounced for condominiums.

It may be the case that the latter estimates are affected by a court decision in March 2015 (see Watts v. Oak Shores Community Association, 2015). This decision empowered homeowner associations to impose limits and fees on short-term rentals and therefore affected condominiums, but not any other form of housing. As a substantial share of housing is subject to homeowner associations nowadays (Clarke and Freedman, 2019), and this decision may have affected our HSO estimate for condominiums, we have added an interaction term of the HSO for condominiums with a dummy indicating whether the transaction took place before the court decision in March 2015. It appears that this additional control variable does not change our results; if anything, the coefficient has the opposite sign.

All in all, we do not find strong evidence for the presence of a local external effect, implying that the estimated effect of the HSO only reflects an efficient use effect.

A4.7. Airbnb listings and house prices: first-stage results

In this part of the Appendix, we consider the first-stage results. The second-stage results are reported in Table 8. The dependent variable is the Airbnb listings rate within 200m of the property in Table A9.

[48] We have played around with different thresholds, but the conclusion that the external effect of Airbnb is too imprecise to pin down still holds.

In column (1), Table 8, the coefficients imply that the HSO has reduced listings on average by about 0.4 percentage points, which is 67% of the mean listings rate. However, there is substantial heterogeneity, as expected. The effect of HSOs on the listings rate tends to become somewhat stronger once we include HSO border segment×month fixed effects. Columns (3) and (4) in Table 8 show similar effects once we respectively increase or decrease the bandwidth. Columns (5) and (6) show that the first-stage coefficient becomes somewhat stronger if we calculate the listings rate within 100m, while it is somewhat lower if we take the listings rate within 500m.

Column (7) only considers the months for which we have listings data. This leads to a very similar first stage. Also if we consider an alternative approximated measure for listings in column (8), the first stage is very similar.

A4.8. Additional sensitivity analyses

Here, we subject our results to an additional range of robustness checks. We report the reduced-form results for prices in Table A10.

The first column improves on identification by including property fixed effects rather than census block fixed effects. Because we look at a relatively short period, this greatly reduces the number of degrees of freedom because most properties are sold only once between 2014 and 2018. Still, we find a negative and statistically significant effect of HSOs that is even somewhat higher: an HSO seems to be associated with a price decrease of 4.9%.[49] However, using a Hausman T-test, it appears that this coefficient is not statistically significantly higher than the baseline estimate where we include census block fixed effects.

In this paper, we use a Panel RDD to identify the house price effects based on an optimal bandwidth. As a sensitivity check instead of choosing a bandwidth, we include a second-order polynomial of distance to the nearest HSO border interacted with the treatment variable and time, as well as a 4^{th}-order polynomial of distance for the non-treated observations interacted with the treatment variable and time while including all observations. In column (2) we see that this has limited repercussions for the results. If anything, the effects of HSOs are slightly stronger.

One may still be worried that the effects of Airbnb are partly determined by locational attractiveness. Column (3) aims to further alleviate these concerns by including flexible second-order trends of pictures and year. The results are hardly affected.

In column (4) we match the transactions data to neighborhood characteristics (at the census block group level). That is, we match each

[49] Note that using property fixed effects implies that identification mainly occurs based on transactions sold both in 2014 and 2018, because properties are usually not transacted in subsequent years. This implies that we identify here a long-run effect of HSOs.

Table A10
Sensitivity analysis for reduced-form effects.

(Dependent variable: log of house price)	*Property fixed effects* (1)	*Distance to border trends* (2)	*Picture trends* (3)	*Neighborhood characteristics* (4)	*Straight segment trends* (5)
HSO implemented	-0.0486*** (0.0155)	-0.0257*** (0.0081)	-0.0192** (0.0080)	-0.0149* (0.0079)	-0.0155 (0.0105)
Property characteristics	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes
Flexible spatio-temporal trend variables	No	Yes	No	No	No
Pictures×year trends	No	No	Yes	No	No
Neighborhood characteristics	No	No	No	Yes	No
Straight border segment×year fixed effects	No	No	No	No	Yes
Property fixed effects	Yes	No	No	No	No
Census block fixed effects	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	10,120	272,485	63,297	61,719	58,453
Bandwidth, b *(in km)*	2.1616	—	1.8101	1.8218	1.8148
R^2	0.9730	0.9132	0.9090	0.9095	0.9240

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

transaction to the log of population density, the share of blacks, Hispanics, and Asians, household compositions, the share of renters, and the median age, in the previous year. Given that the effects are then very similar, this suggests that the effect of the HSOs (Airbnb) is predominantly due to a reduction (increase) in demand, rather than due to changes in the neighborhood composition.

Column (5), Table A10, further improves on identification by including straight border segment×year fixed effects in spirit of Turner et al. (2014). The idea is that straight border segments are likely uncorrelated to geographical features of a location, which may impact price trends (*e.g.* through the propensity to build on the land). Because the average length of a straight border segment is just below 50m, we cannot include border segment×month fixed effects, as this will lead to a too low number of degrees of freedom. We do not find that the price effects of the HSO are very different. The estimate is similar but imprecisely estimated because of the high number of fixed effects.[50]

We repeat a similar set of specifications when estimating the impact of Airbnb listings on house prices. In all specifications, we instrument the listings rate with the HSO dummy. The results are reported in Table A11. Column (1) uses property fixed effects. The estimated effect is similar but very imprecise.

In column (2) we find a considerably stronger effect of the listing rate when we include trends instead of selecting a particular bandwidth: a 1 percentage point increase in the listings rate is associated with a price increase of 8.7%, which seems to be unrealistically strong. This suggests that using a local linear approach is preferred over including all observations (see Gelman and Imbens, 2016). When we control flexibly for differential price trends between more and less touristy areas in column (3), the coefficient of listings rate is somewhat higher than in the preferred specification. The same holds in column (4) when we control for changes in neighborhood characteristics, and column (5) where we use straight border-segment×year fixed effects.

Column (6) that the results where we test the impact of the listings rate on prices are not driven by a few, potentially unrealistic, outliers. Indeed, when we exclude observations with a rate above 15%, the results are, if anything, stronger.

A4.9. Sensitivity analyses for difference-in-differences estimation strategy

In this Appendix section we check for sensitivity of the results using the *Zillow* data, so the results using a DiD estimation strategy. We first report first-stage regression results in Table A12, corresponding to the second-stage results reported in Panel B of Table 7. It can easily be seen that HSOs reduce the listings rate by about 0.45–0.55 percentage points, which is comparable in magnitude as reported in Table A9.

In Table A13 we repeat the DiD analysis, but now we take the median list price in the *Zillow* data as the dependent variable. We find negative effects of the HSO in all specifications, with magnitudes that are very comparable as previously reported. Note that if we only include observations within 1km in column (4) we find a strong negative impact of HSOs, although the coefficient is somewhat imprecise. This is in contrast to the absence of any effect of HSOs on rents within 1km, and in line with the idea that long-term rents will not be discontinuous at the HSO border, while prices are. The reason is that two rental properties will be close substitutes and people are unlikely to be willing to pay more for a property that is just inside an HSO area.

In Panel B we report the results when instrumenting the listings rate with the city-specific HSO dummies. We find stronger effects than the baseline, but the coefficients are quite imprecise and usually only marginally statistically significant. This particularly holds for columns (3) and (4). Nevertheless, the point estimates are similar to the baseline results reported in Table 8.

A5. Renters, income and HSOs

Using data from the Community Survey on demographics in 2013, we regress a dummy indicating whether a city will implement an HSO on the share of renters. Table A14 reports the results.

When only including the share of renters, there is no effect. However, the share of renters is strongly negatively correlated to (log) neighborhood income ($\rho = 0.551$). If we control for log income, we find a strong positive association between the share of renters and the probability to have an HSO implemented. Also, income is positively correlated to this probability, likely because rich people do not care so much about the potential revenues from Airbnb, while poorer households could use the money. This is confirmed in column (3) where we further include

[50] Note that it is not entirely obvious how these freeways and mountains could invalidate our research design. First, amenity differences or preferences for these amenity differences need to change over time because we include census block fixed effects. We have a short time window, so this does not seem to be a major issue. Second, and more unlikely, these amenity differences over time and space should be correlated to the treatment variable. This implies that for HSO, there should be an improvement in amenities throughout, but only in HSOs, not on the other side of the border. Hence, we do not see how freeways or mountain ranges could invalidate our research design because they do not generate an amenity change, let alone an amenity change always at one side of the border.

Table A11
Sensitivity analysis: the impact of listings on house prices.

(Dependent variable: log of house price)	*Property fixed effects* (1)	*Distance to border trends* (2)	*Picture trends* (3)	*Neighborhood characteristics* (4)	*Straight segment trends* (5)	*Listings rate <15%* (6)
Listings rate <200m *(imputed)*	0.0225	0.0839***	0.0528***	0.0450**	0.0638*	0.0641**
	(0.0605)	(0.0299)	(0.0204)	(0.0215)	(0.0367)	(0.0267)
Property characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Distance to border×year trends	No	Yes	No	No	No	No
Pictures×year trends	No	Yes	No	No	No	No
Neighborhood characteristics	No	No	Yes	Yes	No	No
Straight border segment×year fixed effects	No	No	No	Yes	No	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	13,672	272,465	86,858	81,166	79,900	82,191
Bandwidth, b *(in km)*	3.0898	—	2.9319	2.7671	2.8364	2.7134
Kleibergen-Paap F-statistic	29.12	31.31	42.53	43.01	25.28	34.62

Notes: The listings rate is instrumented witha dummy variable indicating whether an HSO has been implemented. Robust standard errors are clustered at the census block level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

Table A12
DiD results for rents, first-stage results.

(Dependent variable: listings rate)	*All obs.* (1) OLS	*Outside HSO, <25km* (2) OLS	*Outside HSO, >1km, <25km* (3) OLS	*Outside HSO, <1km* (4) OLS	*Outside HSO, >1km, <25km* (5) OLS	*Outside HSO, >2.5km, <25km* (6) OLS
HSO implemented	-0.4690***	-0.5494***	-0.4485***	-0.7722***	-0.4437***	-0.4141***
	(0.1151)	(0.1163)	(0.1125)	(0.1952)	(0.1136)	(0.1283)
Distance to CBD×year trends	No	No	No	No	Yes	Yes
Distance to beach×year trends	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3491	3231	2951	722	2951	2472
R^2	0.9594	0.9619	0.9645	0.9554	0.9772	0.9589

Notes: In all specifications we include observations *inside* HSO areas. Standard errors are clustered at the zipcode level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

Table A13
DiD results for prices, Zillow data.

(Dependent variable: log median list price)	*All obs.*	*Outside HSO, <25km*	*Outside HSO, >1km, <25km*	*Outside HSO, <1km*	*Outside HSO, >1km, <25km*	*Outside HSO, >2.5km, <25km*
Panel A: Effects of HSOs	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
HSO implemented	-0.0315**	-0.0246*	-0.0172	-0.0312	-0.0285**	-0.0266**
	(0.0136)	(0.0134)	(0.0132)	(0.0228)	(0.0115)	(0.0114)
Distance to CBD×year trends	No	No	No	No	Yes	Yes
Distance to beach×year trends	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3429	3169	2889	660	2889	2410
R^2	0.9935	0.9895	0.9894	0.9869	0.9915	0.9919
Panel B: Effects of listings	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Listings rate *(in %)*	0.0676**	0.0450*	0.0385	0.0411	0.0645**	0.0653**
	(0.0319)	(0.0238)	(0.0294)	(0.0263)	(0.0325)	(0.0324)
Distance to CBD×year trends	No	No	No	No	Yes	Yes
Distance to beach×year trends	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3429	3169	2889	660	2889	2410
Kleibergen-Paap F-statistic	16.30	21.91	15.51	14.08	14.97	9.860

Notes: In all specifications we include observations *inside* HSO areas. We exclude observations occurring within one year after implementation of the HSO. In Panel B we instrument the listings rate with a dummy indicating whether an HSO has been implemented. Standard errors are clustered at the zipcode level and in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

Table A14
Renters and HSOs.

(Dependent variable: HSO will be implemented)	(1) Probit	(2) Probit	(3) Probit	(4) Probit
Share of renters	0.0007	0.6158***	0.9598**	1.2009**
	(0.2138)	(0.1936)	(0.3839)	(0.6089)
Average income per capita *(log)*		0.3606***	0.2793*	0.2686*
		(0.0546)	(0.1445)	(0.1473)
Share of blacks			0.2161	0.4103
			(0.6995)	(0.7339)
Share of Asians			0.1405	0.0953
			(0.2122)	(0.2001)
Share of other ethnicity			-1.2536	-1.5453
			(0.8508)	(0.9831)
Share of families			1.7343*	0.9631
			(0.9781)	(1.0725)
Share of couples			5.0080	3.8216
			(3.1254)	(3.1757)
Median age			0.0060	-0.0021
			(0.0124)	(0.0126)
Share single-family homes				0.6254
				(0.5887)
Share other homes				-2.3543
				(2.1849)
Observations	90	90	90	90
Pseudo-R^2	0.0000	0.3011	0.3703	0.4014

Notes: We report average marginal effects. Standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

a set of other demographic controls. There seems to be a proportional increase in the share of renters with respect to the probability to receive an HSO. In column (4) where we control for house type, the coefficient becomes even somewhat stronger. Although we refrain from giving a causal interpretation to these regressions, we think the correlations are in line with the idea that renters have more incentives to vote for the implementation of an HSO.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2021.103356.

CRediT authorship contribution statement

Hans R.A. Koster: Conceptualization, Methodology, Data curation, Writing - review & editing. **Jos van Ommeren:** Conceptualization, Data curation, Writing - original draft, Writing - review & editing. **Nicolas Volkhausen:** Methodology, Writing - original draft, Writing - review & editing.

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