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

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**Abstract** – Online short-term rental (STR) platforms such as *Airbnb* have grown spectacularly. We study the effects of STR-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 3%. Additional difference-in-differences estimates show that ordinances reduced rents also by 3%. These estimates imply large effects of *Airbnb* on property values in areas attractive to tourists (*e.g.* an increase of 10% within 2.5km of Downtown LA).

**Keywords** – short-term rentals, house prices, regulation, supply effects, externalities.

**JEL codes** – R21, R31, Z32.

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# 1 Introduction

Short-term housing rentals (STRs) have become very important due to the rise of online STR-platforms which provide opportunities for households to informally offer accommodation to visitors. The largest online platform is *Airbnb*. The surge in popularity of STR-platforms has led to substantial opposition because of a decrease in housing affordability (Samaan 2015, Sheppard & 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 & 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* on housing markets and the related effects of policies that restrict the market for STRs. There are arguably three main mechanisms of how short-term renting impacts property markets (see Turner et al. 2014):

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 an increase in housing demand, which increases house prices (see *e.g.* Turner et al. 2014).

2. *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, 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 & Horton 2018).

To identify the effects of *Airbnb* 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 U.S. 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 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).[1] 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. We distinguish between effects on different types of listings (home sharing, entire properties) as well as on the prices in different areas (*e.g.*,

[1] The extent of illegal subletting is unknown, but note that the host is always responsible for breaking the law, rather than *Airbnb* (Petterson 2018). Moreover, note that the increased risk also reduces benefits of illegal subletting substantially because of hefty fines and potential lawsuits.

with high and low tourist demand).

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. The reduction in the number of listings will be particularly pronounced at locations that are attractive to tourists and other visitors. Theory then indicates that there is a discrete decrease in house prices at HSO borders, because houses within an HSO 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 any 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 so-called 'manipulation' is completely absent. The economic intuition for the absence of manipulation is that tourist demand tends not to be extremely local (*e.g.*, tourists are rather indifferent between locations which are less than a couple of minutes drive from each other), so tourist accommodations compete with each other over longer distances.[2]

Short-term rental platforms also reduce housing supply available for local (long-term) rental markets, which increases rents (Hilber & Vermeulen 2016) – the housing supply effect. When the 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).[3]

We do not measure the 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 & Ward 2009). This implies that there should be no discrete jump at HSO borders for rents.[4]

[2] In line with this line of reasoning, we will show that *Airbnb* accommodation prices 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. Consistent with that, we find suggestive evidence that the number of formally registered traveler accommodations increase due to HSOs.

[3] 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.

[4] A Panel RDD analysis of rents confirms the absence of a discontinuity in rents. Another consequence is that

To capture the 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 in this context is by applying the same approach to house prices, finding very similar effects.

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. Moreover, we test for differences between effects of *Airbnb* of prices of apartments and single-family homes. We do not find evidence that the externality effect is important for LA County.

We have 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 by about 70% in the long run. Its effect on home sharing listings is smaller and about 50%. We further show that home sharing listings have not been reduced when home sharing 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 by about 3% on average. This effect is robust to a wide range of placebo-tests and specification choices. To explore this issue further, we estimate a 'structural equation' capturing the effect of demand for short-term rentals on housing prices. We measure short-term rental demand using the *Airbnb* listings rate – the share of HSO properties 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 *Airbnb* listings increases prices by about 4.2%.

Using the DiD estimation strategy, we further show that rents decrease by about the same amount as house prices.[5] This is likely because of the reduced supply of rental housing. Given that the effects on rents and prices are very comparable, this makes it plausible that the *i)* DiD approach generates causal effects and that *ii)* the *local* average treatment effect obtained in the

at the HSO side of the border, the economic return of a rental property is less than of a privately owned property used for short-term renting. This makes it plausible that the share of rental housing drops at the HSO side of the border. Our observation period is too short and the quality of housing tenure data unfortunately not good enough to quantify such a change.

[5] Furthermore, we demonstrate that there are no effects on rents *around the HSO border*, confirming that rental properties close to HSO borders are close substitutes.

Panel RDD approach can be interpreted as the average treatment effect that also holds away from HSO borders.

We then show that *Airbnb* imply modest property value increases for LA County as a whole: the total average property value increase due to *Airbnb* since 2008 is 2.7%. 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 in central urban areas – within 2.5km of Los Angeles's Central Business District (CBD), property values have increased by 10% due to *Airbnb*. Within 2.5km of beaches, prices have increased by 5.5%. 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.

**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). We are not the first empirical study on the effect of short-term rentals on the housing market, but current studies, although suggestive, do to the best of our knowledge not surmount the various endogeneity issues. Sheppard & Udell (2016) conclude that housing values increased by about 31% due to *Airbnb*. Horn & Merante (2017) show that a high *Airbnb* density increases asking rents by 1.3-3.1%. Barron et al. (2018) show that *Airbnb* increases house prices and rents in U.S. cities. Garcia-López et al. (2018) also report a positive effect on rents in Barcelona. Also a few reports – which essentially rely on correlations – have studied the impact of *Airbnb*.[6]

These studies have in common that they do not convincingly address the endogeneity issue that neighborhoods tend to become more attractive to residents and tourists at the same time. Our study is the first one that addresses this issue by exploiting quasi-experimental variation provided by changes in regulation to estimate the effect of *Airbnb* on the housing market. Furthermore,

[6] New York Communities for Change looked at correlations between *Airbnb* and neighborhood mean rent increases (NYCC 2015). Samaan (2015) looks at the rental market in Los Angeles and reports a 4 percentage points faster growth of rents in popular *Airbnb* neighborhoods. Lee (2016) argues that *Airbnb* reduces the affordable housing supply in Los Angeles, because landlords remove units from the housing market by listing on *Airbnb*. Wachsmuth & Weisler (2017) argue that *Airbnb* has introduced new revenue flows to the housing market which are systematic but geographically uneven.

and this is important, we are the only study which studies the effect of regulation of *Airbnb* itself, which is of key policy interest.

Our paper also relates to a literature studying the effects of tourism and amenities on housing markets. Carlino & Saiz (2008), for example, show that the number of tourists visiting a city is a good predictor of the growth of U.S. 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 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 & Rouwendal 2013, Ahlfeldt & Kavetsos 2014, Koster & Rouwendal 2017). In these studies, it is impossible to disentangle the effects of tourism and amenities. An exception is a recent paper by Faber & 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 & Vermeulen 2016). However, they do not identify the underlying mechanisms that lead to price increases. Glaeser & 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 & 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 (with the exception of Severen & 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 housing (Fallis & Smith 1984, Moon & Stotsky 1993, Glaeser & Luttmer 2003), public housing (Olsen & Barton 1983, Anderson & Svensson 2014) and affordable housing (Quigley & Raphael

2004). In this literature, it is common to study a policy where a fixed, but small, share of houses is regulated in order 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 & Luttmer 2003, Anderson & Svensson 2014) or the housing supply effect (see Fallis & Smith 1984). In contrast to the existing literature, we study a regulation type which induces efficiency and housing supply effects for the full housing market, rather than a sub-segment of the market. Hence, the aggregate welfare and distributional effects are expected to be much more pronounced.

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 back-of-the-envelope welfare calculations and distributional implications of HSOs and *Airbnb* in Section 7. 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.[7] *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, about 2.5% of all residential housing. 60% of those listings are entire properties (Inside Airbnb 2017).[8] Figure 1 clearly shows that *Airbnb*

[7]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).

[8]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).

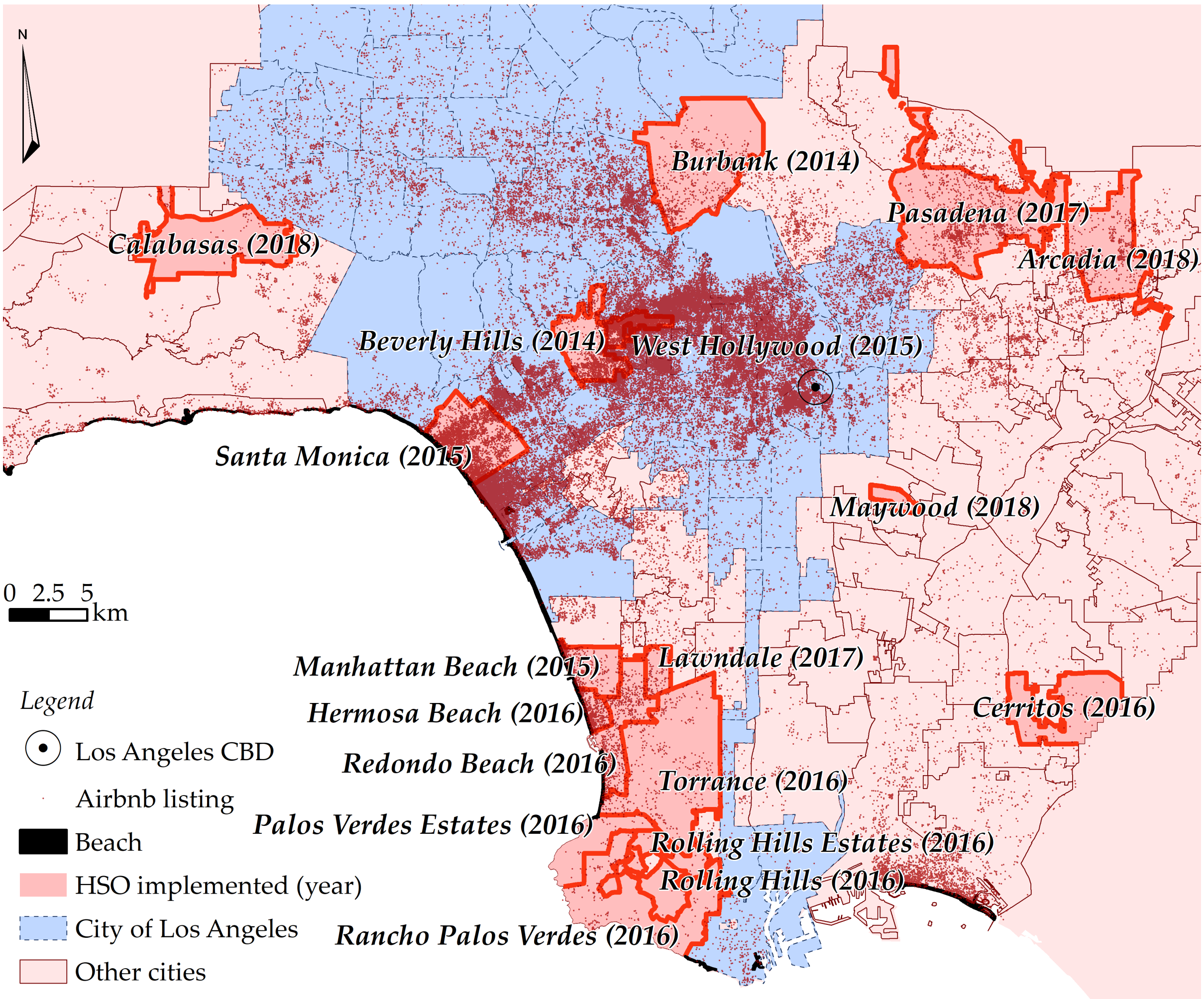



Figure 1 – Airbnb in Los Angeles County

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: 'home sharing', whereby at least one of primary residents lives on-site throughout the visitor's stay, and 'vacation rentals', which are for exclusive use of the visitor.

In Figure 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.[9] 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 both home sharing and vacation rentals. 4 out of 18 cities (Calabasas, Pasadena, Santa Monica and Torrance) still allow for home sharing, although restrictions apply. In Santa Monica, for example, the HSO allows for home sharing up to 30 days per year but prohibits hosts to operate more than one home-share 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 $\$80,000$. Vacation rentals or home-shares that are operating illegally, including informal vacation rentals, 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).[10] In Appendix A.1 we report for each city in LA County more details regarding STR regulation. We also list our data sources there.

Our estimated effect of HSOs on house prices, but not on rents, may potentially depend on future changes in regulation. 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 whether our study can be interpreted to measure the permanent effect of HSO. 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 fundamental future change in regulation after the period analysed by us, which is for the city of Los Angeles. This city announced in December 2018, so approximately one 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).

[9] 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 6.4).

[10] 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 number of listings as well as property prices are higher in cities where enforcement is more strict.

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 analysed by us (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 in the 18 cities compared to their immediate surroundings. We are not aware of such policy changes (but have actively searched for this). We also offer statistical support for this assumption. In Section 6.4 we perform a range of placebo tests using information on price changes around the borders of other sets of cities and using the same borders but in other time 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 *e.g.* differences in school quality) changed exactly around this period.[11]

# 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`.[12] 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.[13] For the analysis where we analyze the effects of HSOs on listings, we construct a panel dataset of all accommodations that have been listed between 2014 and 2018. We create a variable that equals one when the accommodation is actually listed in a certain month.

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.*, apartment, single-family home, construction year) for all transacted residential properties. We

[11] This conclusion is supported by the absence of differences of (changes in) public good provisions between cities that are known to affect house prices. See for evidence on school quality Section 5.2.

[12] *Airbnb* is not the only STR-platform available to prospective hosts. This is unlikely problematic, because hosts who consider to use 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 3,578 listings in Los Angeles in 2016, while *Airbnb* had 8,367 listings (which is less than observed in our data). Data on individual *HomeAway* listings is not available to us.

[13] Through *Inside Airbnb*, we also have information for a subset of listings on the number of reviews, which we will show for descriptive purposes.

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 is obtained from the 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 (apartment or single-family home), as well as transactions referring to multiple parcels or units.[14]

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 is 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.[15] In Appendix A.4.6 we use an alternative *approximated* measure, available for the whole period for which we observe house prices, following Zervas et al. (2017) and Barron et al. (2018). The results using this measure are essentially the same.[16] 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.[17] 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.

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.[18] *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

[14]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 also 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.

[15]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.

[16]This alternative measure is derived from information on listings on the date of their first review (if this information is missing, the date at which the host became active on *Airbnb*) and last review, while assuming that the property is continuously listed between these two reviews.

[17]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 $2.5^{th}$ percentile. In Appendix A.4.8 we show that our results are rather insensitive to outliers.

[18]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.

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.

Finally, for an ancillary analysis we gather data from Eric Fisher's *Geotagger's World Atlas*, which contain all geocoded pictures on the website *Flickr* between 2000 and 2016. To isolate the pictures taken by tourists, we exclude the users that upload pictures in 6 consecutive months during our study period. We count the number of pictures within each area as a proxy for tourist demand. The idea is that areas with a high picture density likely have more tourists visiting the area. As the number of pictures may have been taken by users of *Airbnb*, and is therefore potentially endogenous, we only use information from before the year 2014, which is the start of our study period.

### *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, 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. This difference may be relevant as distance to the beach is one characteristic that possibly affects tourist demand for accommodation. We note that the apartment share of *Airbnb* listings is about 0.5, which exceeds the apartment share of housing transactions (see Table 2). Hence, the forbidding of *Airbnb* in apartment buildings by Owners Associations (*e.g.* to reduce within-building externalities) is unlikely to be an important phenomenon.

Figure 2 provides information about changes in the number of listings over time. We observe that there is a strong positive trend in the number of listings in LA County. In September 2018

Table 1 – Descriptive statistics for Airbnb data

| *Panel A:* Inside HSO areas | mean | sd | min | max |
|---|---|---|---|---|
| Price per night *(in $)* | 171.8 | 140.1 | 0 | 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 $)* | 145.8 | 132.7 | 0 | 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*: The number of listings for HSO areas is 53,980. Outside HSO areas it is 344,813.

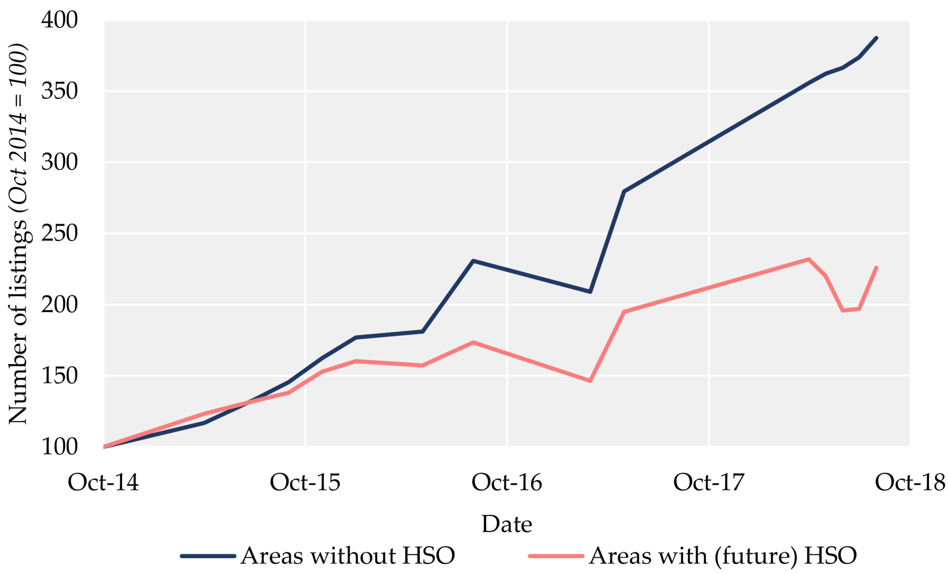



Figure 2 – Airbnb in LA County

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.

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 \$)* | 6,187 | 2,724 | 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 $m^2$)* | 1,447 | 3,247 | 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 | 1,971 | 22.07 | 1,897 | 2,017 |
| 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 | 2,016 | 1.158 | 2,014 | 2,018 |

| *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 \$)* | 4,064 | 2,189 | 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 $m^2$)* | 2,110 | 6,333 | 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 | 1,968 | 23.63 | 1,884 | 2,018 |
| 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 | 2,016 | 1.169 | 2,014 | 2,018 |

*Notes*: The number of transactions for HSO areas is 32971. Outside HSO areas it is $250,490$.

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 Figure 1) and temporal (see Figure 2) variation in the listings rate is large: for the majority of houses (65%), there are no listings within 200m.

Properties in HSO areas are about 10% larger, but at the same time the share of apartments 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 in order 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

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 \$)* | 6,692 | 2,464 | 4,035 | 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 | 2,016 | 1.345 | 2,014 | 2,018 |

| *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 \$)* | 5,563 | 2,622 | 1,089 | 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 | 2,017 | 1.272 | 2,014 | 2,018 |

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

the density of tourist pictures is about twice as high, compared to non-HSO areas.

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%), then 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 micro analyses) 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

The main econometric issue when aiming to estimate a causal effect of *Airbnb* on the housing market is that *Airbnb* listings are not randomly allocated across space but are concentrated in neighborhoods that are attractive to both residents and visitors with a demand for short-term letting. One way to address this issue is to compare adjacent cities that differ in 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.

### *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 home sharing. We will estimate linear probability models, where we estimate the effects of the HSO on both probabilities separately.[19] We 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. Let $\ell_{ikt}$ 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.

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 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 $\ell_{ikt}$ at the same time. We therefore include property fixed effects $\lambda_i$, which control for difficult-to-observe but time-invariant differences between locations, and $\mu_{kt}$, which capture HSO-border area specific

[19] Our motivation not to estimate multinomial discrete choice models, but to estimate separate models is that in our data properties *never* switch between being listed as an entire home to home sharing. This also implies that the HSO did not induce hosts of entire properties to shift to home sharing. Results are very similar when we estimate Logit models, or Conditional Logit Models of location choice using Poisson regressions.

months fixed effects (*i.e.* a fixed effect for each web scrape in each HSO-border area). This implies:

$$(\hat{\alpha}, \hat{\lambda}_i, \hat{\mu}_{kt}) = \underset{\alpha, \lambda_i, \mu_{kt}}{\arg\min} \sum_i \sum_t K\left(\frac{d_{ik}}{b}\right) \times (\ell_{ikt} - \alpha h_{ikt} - \lambda_i - \mu_{kt})^2, \tag{1}$$

where $\alpha$ is the parameter of interest and $K(\,\cdot\,)$ is a uniform kernel function:

$$K\left(\frac{d_{ik}}{b}\right) = 1_{d_{ik}<b}, \tag{2}$$

where $d_{ik}$ is the distance to the border and $b$ a given bandwidth. Note further that because we include property fixed effects, we effectively only use data on properties that have been listed at least once.

The bandwidth $b$ determines how many observations are included on both sides of the border. In an RDD, estimated parameters are often sensitive to the choice of the bandwidth. We therefore show results for different bandwidths. Our preferred specification is based on an approach proposed by Imbens & 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.

## *4.2 HSOs, Airbnb and house prices*

### 4.2.1 Reduced-form effects

We employ a similar approach to measure the effect of the HSO on house prices. 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:

$$(\hat{\beta}, \hat{\zeta}, \hat{\eta}_j, \hat{\theta}_{kt}) = \underset{\beta, \zeta, \eta_j, \theta_t}{\arg\min} \sum_i \sum_t K\left(\frac{d_{ik}}{b}\right) \times (\log p_{ijkt} - \beta h_{ijkt} - \zeta x_{ijk} - \eta_j - \theta_{kt})^2, \tag{3}$$

where $\beta$ is the parameter of interest, and $\eta_j$ and $\theta_{kt}$ refer to census block and HSO border× month fixed effects respectively. 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 tend to be stronger).[20] In the main analyses, we exclude transactions occurring within one year after implementation of the HSO. Second, in another analysis, we allow the HSO-effect to be time-specific, so we are also able to test for anticipation and adjustments effects of HSOs.

#### 4.2.2 Effects of the listings rate on house prices

The results from equation (3) are informative on the local *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, which strongly covaries with the demand for *Airbnb*, captured by the listings rate $r_{ijkt}$, potentially reducing the external validity of the local 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 a 'structural equation' where we regress prices on the listings rate in the direct vicinity. Because $r_{ijkt}$ 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.

In our main analysis, we instrument the listings rate with *city-specific* HSO dummies. This offers two important advantages compared to using a single dummy for HSO treatment.[21] First, the latter would lead to a misspecified first stage, because this first stage would predict the same *absolute* decrease in listings rate for all cities, whereas we show that the *relative* decrease is approximately constant across different cities.[22] Second, given city-specific HSO dummies, we

[20] 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.

[21] An approach employing a single HSO dummy provides larger effects, so our results are on the conservative side. See Appendix A.4.7 for more information.

[22] Hence this first stage would ignore that areas with large initial listings rates (such as Santa Monica or West Hollywood) face large absolute HSO-induced decreases in listings rates, while for those with low initial listings rates the absolute HSO-induced decreases are small.

are able to test for non-linear effects of the listings rate on house prices (in the second stage). The latter is important for our welfare analysis later on, as we aim to calculate counterfactuals for particular cities with high listings rates. The main disadvantage of using city-specific HSO dummies is that such a specification, although consistent, increases the chance of a bias in the estimate because we employ many instruments for one endogenous variable (Hahn & Hausman 2003, Angrist & Pischke 2008). We will show that this is not an issue.[23]

The second stage is then given by:

$$(\hat{\gamma}, \hat{\zeta}, \hat{\eta}_j, \hat{\theta}_{kt}) = \underset{\gamma,\zeta,\eta_j,\theta_t}{\arg\min} \sum_i \sum_t K\left(\frac{d_{ik}}{b}\right) \times (\log p_{ijkt} - \gamma \hat{r}_{ijkt} - \zeta x_{ijk} - \eta_j - \theta_{kt})^2, \tag{4}$$

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

$$(\hat{\tilde{\delta}}, \hat{\tilde{\zeta}}, \hat{\tilde{\eta}}_j, \hat{\tilde{\theta}}_{kt}) = \underset{\tilde{\delta},\tilde{\zeta},\tilde{\eta}_j,\tilde{\theta}_t}{\arg\min} \sum_i \sum_t K\left(\frac{d_{ik}}{b}\right) \times (r_{ijkt} - \tilde{\delta}_k h_{ijkt} - \tilde{\zeta} x_{ijk} - \tilde{\eta}_j - \tilde{\theta}_{kt})^2, \tag{5}$$

where the $\sim$ refer to first-stage coefficients and $\tilde{\delta}_k$ is a set of city-specific HSO effects.

### *4.3 HSOs, Airbnb and rents*

Short-term rentals may lead to a reallocation of existing housing stock away from the long-term rental market towards privately housing used for short-term renting, reducing the supply of available rental stock for locals, which should increase 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 offer 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, which should lead to a statistically insignificant rent effect. The second

[23] First, we show that the results are insensitive to a reduction in the number of instruments by grouping HSO-treated cities into only three different groups. Further, as an alternative to the city-specific HSO dummy specification, we use a specification where we a single HSO dummy and the interaction of HSO with the (time-invariant and therefore exogenous) indicator of tourist demand, *i.e.* the density of pictures taken by tourists. For this specification, in the first stage, the HSO dummy and the interaction variable have both highly statistically significant effects, hence any bias is likely negligible. Using this specification, we find essentially the same effects. Second, we use the LIML estimator, rather than the 2SLS estimator. The LIML estimator mitigates the bias in the estimates, but is usually less efficient than 2SLS.

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 less convincing), we pursue an alternative, but more standard, difference-in-differences approach where we regress rents, $r_{jt}$, on $h_{jt}$, where $j$ refers to zip codes areas. We then have:

$$(\hat{\phi}, \hat{\eta}_j, \hat{\theta}_t) = \underset{\phi, \eta_j, \theta_t}{\arg\min} \sum_j \sum_t (\log r_{jt} - \phi h_{jt} - \eta_j - \theta_t)^2, \tag{6}$$

where $\phi$ is the parameter of interest, $\eta_j$ are zip code fixed effects and $\theta_t$ are month fixed effects. This is a standard difference-in-differences specification, with the notion that we have multiple treatments at different times in our study period.[24] In line with the previous set-up we will also estimate a 'structural equation' by regressing rents on the listings rate.

The key assumption underlying a DiD strategy is that there is a common trend between the treatment and control group. We test and do not reject this assumption for the period before the treatment in Section 5.3, but this strategy is still less convincing than the Panel RDD, which does not require this assumption. We will however demonstrate that when applying this alternative strategy to house prices, the price effects are comparable to the ones obtained using the more credible Panel RDD approach, which makes it plausible that the rent results are reliable.

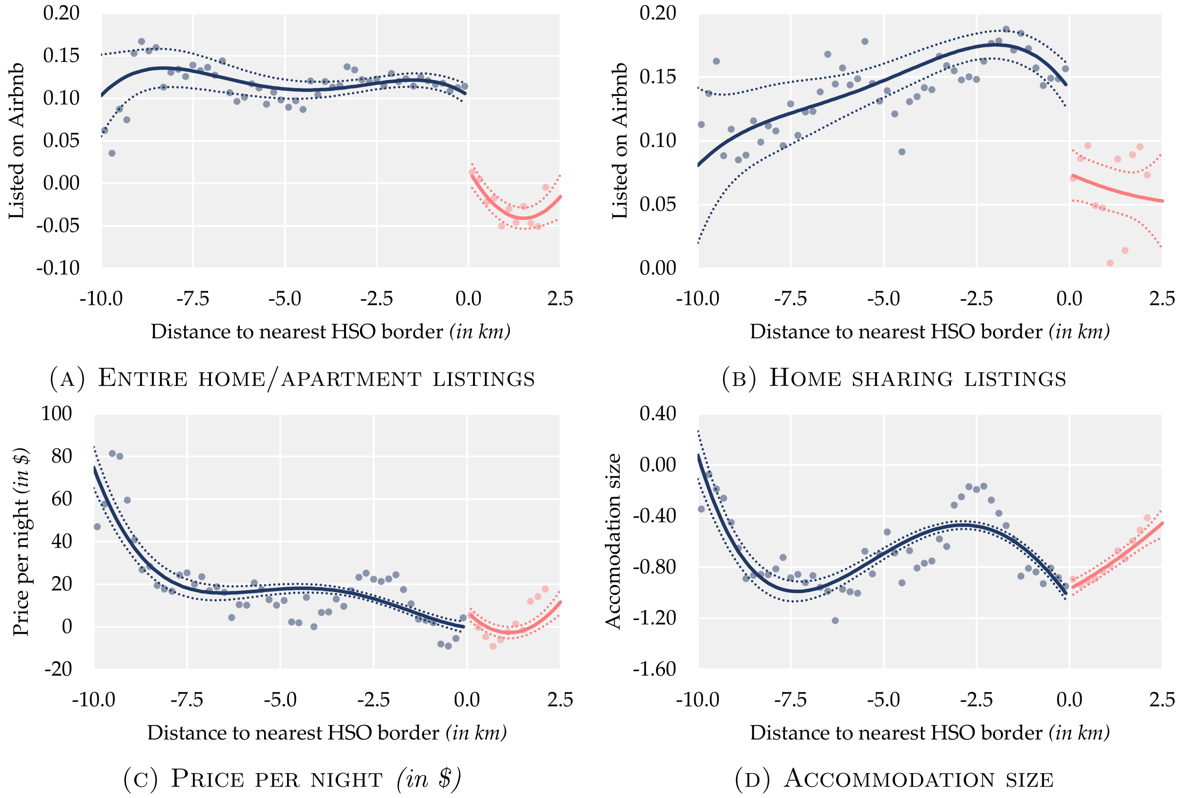
# 5 Graphical evidence

## *5.1 Treatment effects*

Before we turn to the regressions results, we illustrate our research design graphically. In Figure 3, we first focus on the impact of the HSO on *Airbnb* listings. We include census block group and HSO border×month fixed effects, and include a $4^{\text{th}}$-order polynomial of distance to the border outside HSO areas and a $2^{\text{rd}}$-order polynomial of distance to the border inside HSO areas (as we have fewer data points that are closer to the border inside HSO areas).[25] The inclusion

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

[25] 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 an HSO area – are unlikely to be important,




(a) Entire home/apartment listings (b) Home sharing listings

(c) Price per night *(in $)* (d) Accommodation size

Figure 3 – Airbnb listings: variation near the HSO borders

*Notes*: All values are demeaned by census block group and HSO border×month fixed effects. Negative distances indicate areas outside HSO areas and areas inside HSO areas *before* treatment. The dots are conditional averages at every 200m interval. The dotted lines denote 95% confidence intervals.

of census block group fixed effects implies that we identify the effects *over time*. In Figure 3a, where we report demeaned values, we plot the probability of listing 'entire properties' on *Airbnb*. We observe a sizable drop in type of listings in areas where HSOs have been implemented. The difference is about 10 percentage points.[26] Given a listing probability of about 0.30 (for residences that have been listed at least once), this implies a reduction in 'entire properties' listings of 33%. Hence, in line with anecdotal evidence, this suggests that the HSO was very effective in reducing STRs. In Figure 3b we plot the probability of 'home sharing' listings on *Airbnb*. Then, we find a smaller drop in listings of about 7.5 percentage points, corresponding to a reduction in 'home sharing' listings of about 25%.

In Figures 3c and 3d, we investigate whether there are differences in changes of characteristics of

as displacement effects would have induced an increase in listings just outside treated areas.

[26]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 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 Figures 3a and 3b.

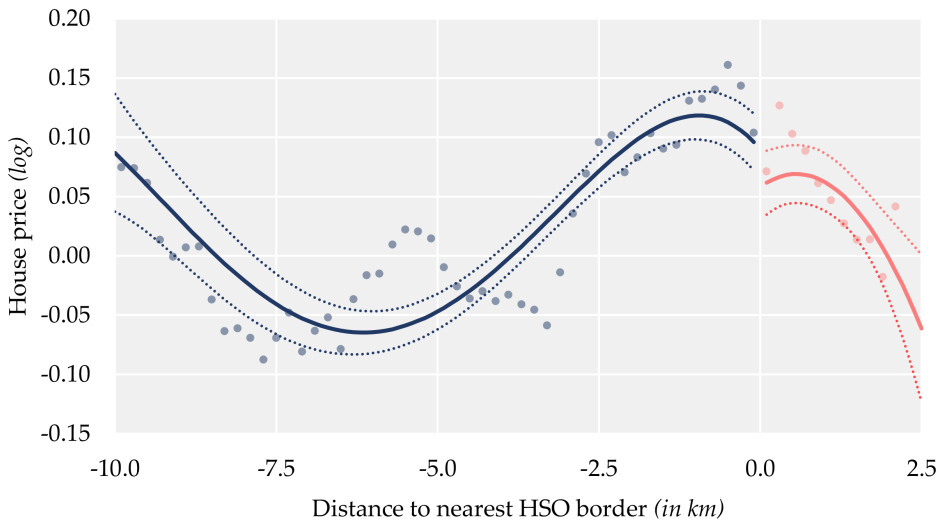



FIGURE 4 – HOUSE PRICES: VARIATION NEAR THE HSO BORDERS

*Notes*: Prices are demeaned by census block group and HSO border×month fixed effects. Negative distances indicate areas outside HSO areas and areas inside HSO areas *before* treatment. The dots are conditional averages at every 200m interval. The dotted lines denote 95% confidence intervals.

houses listed on *Airbnb* between HSO areas and areas in the close vicinity. This does not seem to be the case – there is essentially no difference in how *Airbnb* prices per night and availability changed over time between HSO areas and neighboring areas.

We repeat the exercise, but now focus on house prices. The results are reported in Figure 4. Prices *decrease* by about 3% at the HSO border. Although it is somewhat hard to conclude from the figure, it appears that this effect is highly statistically significant. 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. 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.

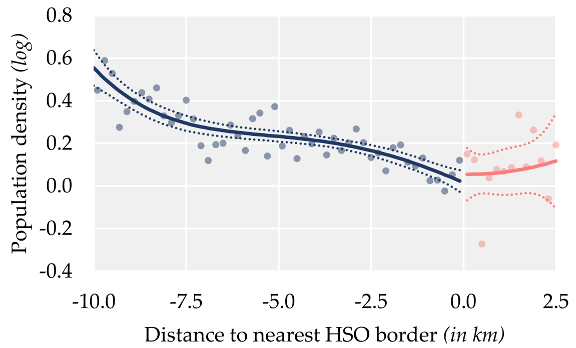
### *5.2 Sorting and public goods*

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, Figure 5 shows that all household characteristics are continuous at the border. Importantly, changes in population density and the share of owner-occupied housing is the same on both sides of the border (Figure 5a). 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, 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.

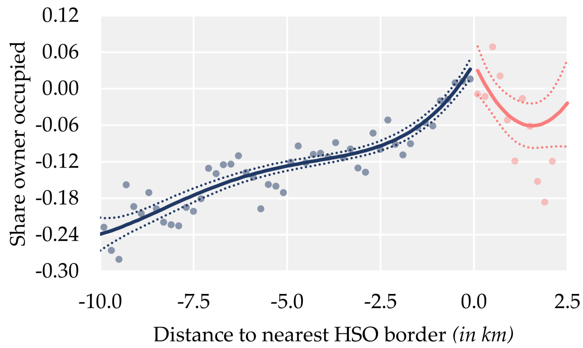
One could also be concerned that a discontinuity in prices arises because of a differential provision of public goods. While temporal changes in the quality of public goods are usually not abrupt, large cross-sectional differences in public good quality may provoke sorting. An important public good is school quality (see Black 1999, Bayer et al. 2007).[27] Using 2017 test score data of students between the 3$^{\text{rd}}$ and 11$^{\text{th}}$ 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. Figures 5g and 5h show that no such discontinuity exists, indicating that the HSO is unlikely to be correlated to school quality.

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 – see Figures 5a and 5b). 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.3.3 we do not find meaningful differences in changes in densities across borders before HSOs were implemented.

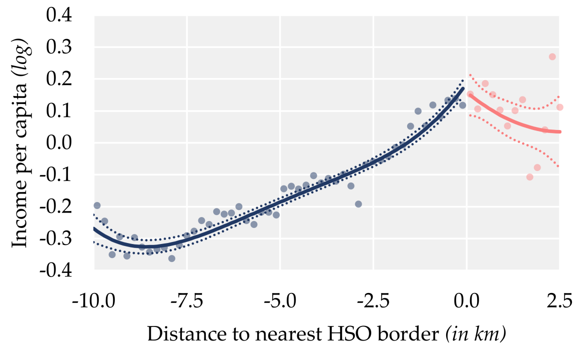
[27] We also checked for other spatial differences in *e.g.* property taxes, but we did not find any meaningful difference.



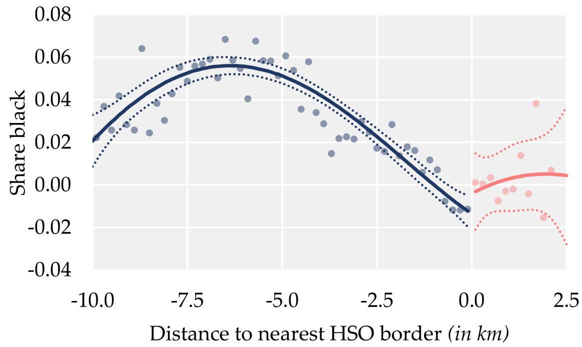

(A) Population density *(log)*



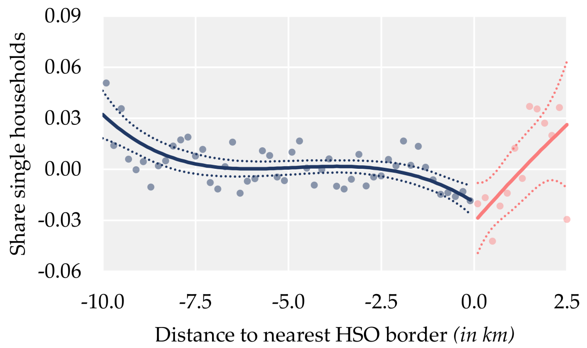

(B) Share owner-occupied housing



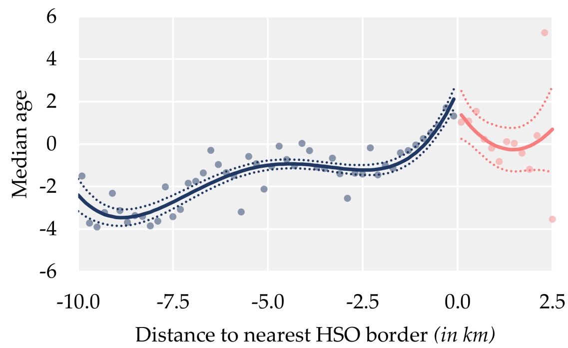

(C) Income per capita *(log)*



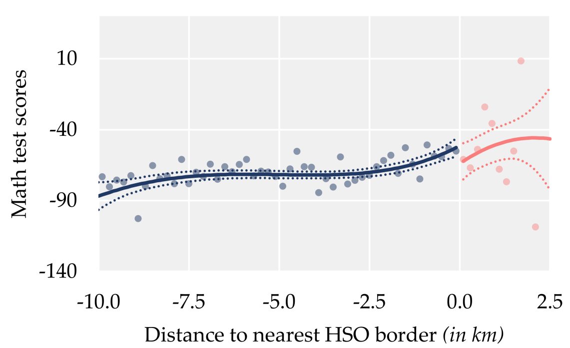

(D) Share black



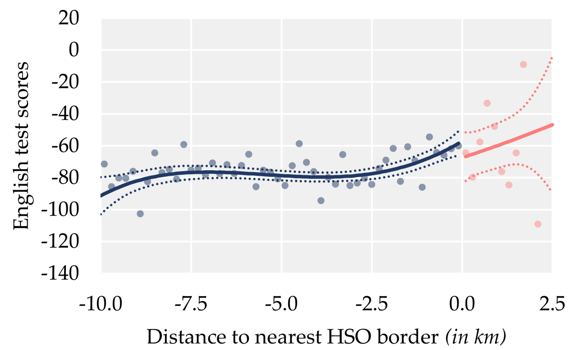

(E) Share single households




(F) Median age



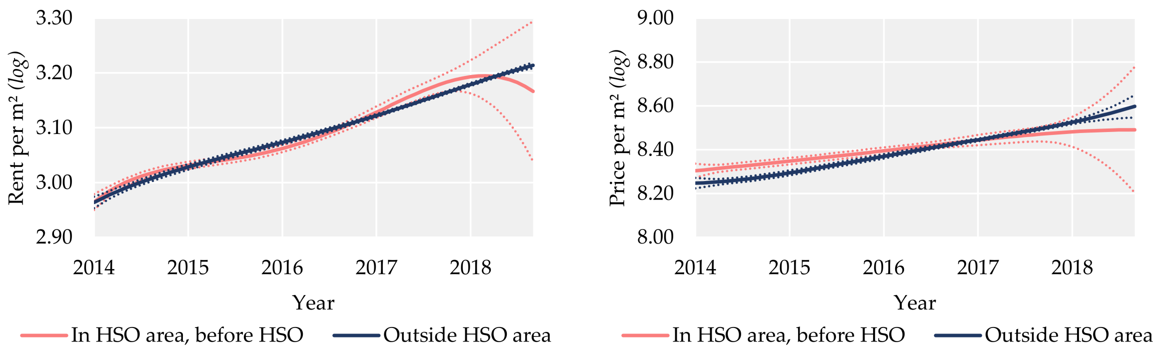

(G) Math test scores




(H) English test scores

Figure 5 – Sorting along the border

*Notes*: All values are demeaned by neighborhood and HSO border×month fixed effects. Negative distances indicate areas outside HSO areas and areas inside HSO areas *before* treatment. The dots are conditional averages at every 200m interval. The dotted lines denote 95% confidence intervals.




(A) Trends in rents (B) Trends in *Zillow* prices

Figure 6 – Common pre-trends

*Notes*: We estimate regressions with zip-code fixed effects and a 4$^{\text{th}}$-order polynomial of months. We compare observations *before* the HSO was implemented, but inside a future HSO area (red line) to observations outside HSO areas (blue line). The dotted lines denote 95% confidence intervals.

### 5.3 *Testing for pre-trends in the Zillow data*

Given the rent data from *Zillow*, we employ a DiD estimation strategy.[28] An important prerequisite for this strategy to be valid is that pre-trends (so before implementation of the HSO) are reasonably similar between treated and untreated areas. We test for this in Figure 6 using *Zillow* information on rents as well as house prices.

We first show in Figure 6a that the rent trends in areas *before* implementation of an HSO compared to rent trends *outside* HSO areas are statistically indistinguishable. This also holds for house prices in Figure 6b.

In Appendix A.3.4 we show that rent and price trends after treatment are clearly below the general rent or price trends.

## 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 & 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 9.7 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 32%.

[28] Nevertheless, we have tested for a discrete jump using a Spatial RDD, and, in line with these considerations, it is impossible to detect such a jump.

Table 4 – Baseline results for Airbnb listings

*(Dependent variable: Airbnb property is listed)*

| | *Panel RDD* | *+ Border segment f.e.* | *Bandwidth: $h^* \times 2$* | *Bandwidth: $h^*/2$* | *Home-sharing not allowed* |
|---|---|---|---|---|---|
| *Panel A:* Entire homes/apartment | (1) | (2) | (3) | (4) | (5) |
| HSO implemented | -0.0969*** | -0.0985*** | -0.1066*** | -0.0929*** | |
| | (0.0086) | (0.0088) | (0.0080) | (0.0107) | |
| HSO implemented× | | | | | -0.1063*** |
| home sharing allowed | | | | | (0.0137) |
| HSO implemented× | | | | | -0.0939*** |
| home sharing not allowed | | | | | (0.0112) |
| Property fixed effects | Yes | Yes | Yes | Yes | Yes |
| HSO area×month fixed effects | Yes | Yes | Yes | Yes | Yes |
| Border segment×month fixed effects | No | Yes | Yes | Yes | Yes |
| Number of observations | 270,906 | 270,621 | 425,117 | 154,015 | 270,741 |
| Bandwidth, $b$ *(in km)* | 1.6716 | 1.6708 | 3.3416 | 0.8354 | 1.6712 |
| $R^2$ | 0.3460 | 0.3496 | 0.3534 | 0.3458 | 0.3496 |
| *Panel B:* Home sharing | (1) | (2) | (3) | (4) | (5) |
| HSO implemented | -0.0537*** | -0.0587*** | -0.0655*** | -0.0489*** | |
| | (0.0117) | (0.0125) | (0.0115) | (0.0152) | |
| HSO implemented× | | | | | -0.0173 |
| home sharing allowed | | | | | (0.0178) |
| HSO implemented× | | | | | -0.0872*** |
| home sharing not allowed | | | | | (0.0172) |
| Property fixed effects | Yes | Yes | Yes | Yes | Yes |
| HSO area×month fixed effects | Yes | Yes | Yes | Yes | Yes |
| Border segment×month fixed effects | No | Yes | Yes | Yes | Yes |
| Number of observations | 171,778 | 171,448 | 259,790 | 94,305 | 171,433 |
| Bandwidth, $b$ *(in km)* | 1.815 | 1.812 | 3.3348 | 0.8337 | 1.8117 |
| $R^2$ | 0.3325 | 0.3428 | 0.3416 | 0.3511 | 0.3428 |

*Notes*: Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 goods. Although this implies the inclusion of 1350 instead of 270 fixed effects, this hardly impacts the results.

Imbens & Lemieux (2008) and Lee & Lemieux (2010) stress the importance of showing 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. This produces similar results. It can be seen that the results are essentially unaffected if we reduce the bandwidth to only 830m in column (4). In the final column of Panel A we make a distinction between different types of

HSOs. Recall that four cities that have implemented HSOs still allow for home sharing. 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 home sharing 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 and a $4^{\text{th}}$-order polynomial of months.

In Panel B of Table 4 we analyze the effects of HSOs on listings of home sharing. 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 5.37 percentage points. Given an average probability to be listed of 0.28, this implies a decrease of 19%. The finding that the percent effect on home sharing is smaller makes sense as some cities do not completely forbid home sharing (*e.g.* Santa Monica). If we include border segment×month fixed effects (column (2)) or change the bandwidth (columns (3) and (4)), this leaves the results essentially unaffected. In column (5) we again include an interaction of the HSO with a dummy indicating whether home sharing is allowed. As one expects, we do not find that home sharing listings have been reduced in areas where home sharing is still allowed, whereas home sharing 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.

In Figure 7 we show 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. At the moment of implementation there is no effect. However, after half a year, we already find a statistically significant reduction in listings of entire properties of about 6 percentage points. After 2.5 years, the effect has increased to 20 percentage points for entire homes, which implies a reduction in listings of almost 70%. Therefore, in the long-run the HSO had a very strong effect on listings of entire properties. A similar pattern emerges for home sharing, where we find that the long-run decrease in listings is 15 percentage points (or 53%). Why does the effect become stronger over time? One explanation is that, in the beginning, households did not yet know whether and to what extent the ordinance would be enforced. After

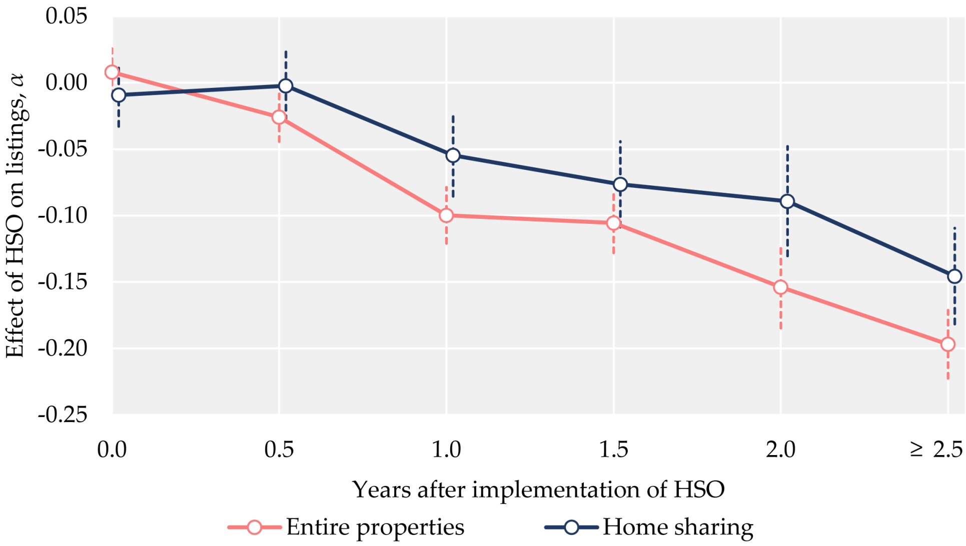



FIGURE 7 – THE EFFECT OF THE HSO ON AIRBNB LISTINGS OVER TIME

*Notes*: The optimal bandwidth $b^* = 1.6692$ for 'entire properties' and $h^* = 1.8120$ for 'home sharing'. The dashed lines denote the 95% confidence bands.

a while, it became clear that it was being enforced, implying potentially hefty fines.

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 in general negative, or when positive, statistically insignificant (except for one city, but this positive effect appears not to be robust to specification, and becomes even negative in an alternative specification). However, standard errors are often somewhat large, so we cannot make precise statements for individual cities.

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 an apartment in an HSO area and neighboring areas. We indeed find that rental prices of *Airbnb* properties are not significantly different at the border. 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 (<2.5km). 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.

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 zipcode level, the results are imprecise. However, the point estimates seem to point towards a sizable 10% 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.

### *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. Because house prices usually adjust only slowly and Figure 7 shows that the reduction in listings is limited in the first year after the introduction of the HSO, we exclude transactions occurring within one year of implementation of the HSO.[29] 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 $\exp(-0.0421) - 1 = 4.1\%$.[30]

In column (2) we add border segment×month fixed effects. This reduces the coefficient somewhat. An HSO is now associated with a 2.9% decrease in house prices. The results do not materially change when we choose other bandwidths in columns (3) and (4).

Back-on-the-envelope calculations indicate that these results are within the range of estimates which are plausible. For example, using the average list price *Airbnb* per night and the average house price, combined with a mortgage interest rate of 2% 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 4% of their housing expenditure, suggesting that house prices increase by about 5% (given 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.[31]

[29] We test later whether anticipation and adjustment effects are important.

[30] 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 apartments are approximately $25 - 30\%$ less expensive than single-family homes. The results are robust to the exclusion of housing controls.

[31] 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 (we use information here 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%.

Table 5 – Baseline results for house prices
*(Dependent variable: log of house price)*

| | *Panel RDD* | *+ Segment month f.e.* | *Bandwidth: $h^* \times 2$* | *Bandwidth: $h^*/2$* |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| HSO implemented | -0.0421*** | -0.0297*** | -0.0317*** | -0.0227** |
| | (0.0067) | (0.0080) | (0.0070) | (0.0101) |
| Property size *(log)* | 0.4973*** | 0.4957*** | 0.4869*** | 0.4987*** |
| | (0.0098) | (0.0092) | (0.0080) | (0.0118) |
| Parcel size *(log)* | 0.0456*** | 0.0465*** | 0.0388*** | 0.0444*** |
| | (0.0045) | (0.0041) | (0.0041) | (0.0054) |
| Bedrooms | 0.0043* | 0.0044* | 0.0008 | 0.0083*** |
| | (0.0023) | (0.0023) | (0.0019) | (0.0029) |
| Bathrooms | 0.0184*** | 0.0181*** | 0.0228*** | 0.0123*** |
| | (0.0027) | (0.0027) | (0.0023) | (0.0036) |
| Apartment | -0.3162*** | -0.3201*** | -0.3240*** | -0.3148*** |
| | (0.0116) | (0.0115) | (0.0109) | (0.0141) |
| Construction year dummies | Yes | Yes | Yes | Yes |
| Census block fixed effects | Yes | Yes | Yes | Yes |
| Property fixed effects | No | No | No | No |
| HSO area×month fixed effects | Yes | Yes | Yes | Yes |
| Border segment×month fixed effects | No | Yes | Yes | Yes |
| Number of observations | 58,316 | 58,285 | 93,881 | 35,269 |
| Bandwidth, $b$ *(in km)* | 1.8447 | 1.8594 | 3.7188 | 0.9297 |
| $R^2$ | 0.9025 | 0.9098 | 0.9050 | 0.9119 |

*Notes*: We exclude transactions occurring within one year after implementation of the HSO. Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In Table 6 we investigate heterogeneity in the effect of the HSO on house prices. In column (1) we first investigate whether HSOs have stronger effects in places where tourist demand is high. As a first proxy for 'touristy places' we use distance to the beach. In column (1) we show that the interaction effect of HSO with beach distance is not statistically significant at conventional levels. Its point estimate indicates that the effect of HSOs reduces by just 0.5 percentage points when the distance to the beach doubles ($\ln(2) \times 0.0071$), so the effect is small. Distance to the beach is likely a noisy proxy for touristy places, as popular places like Hollywood and Downtown LA are far from the beach. We therefore use additional information on the density of geocoded pictures by tourists. We calculate the log of picture density in a neighborhood. Column (2) shows that HSOs do not have a stronger effect in places with high tourist demand.

Column (3) tests whether HSOs that allow for home sharing have weaker price effects. This appears not to be the case: the price effect for HSOs that allows for home sharing is not statistically significantly different from the effect of HSOs in areas that do not allow for this.

Table 6 – HSOs and house prices: heterogeneity

*(Dependent variable: log of house price)*

| | *Distance to beach* | *Tourist pictures* | *Home sharing not allowed* | *House type interactions* | *External effect* |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| HSO implemented | -0.0414*** | -0.0414*** | | | -0.0320*** |
| | (0.0130) | (0.0113) | | | (0.0084) |
| HSO implemented× distance to beach *(log)* | 0.0071 | | | | |
| | (0.0056) | | | | |
| HSO implemented× picture density *(log)* | | 0.0067 | | | |
| | | (0.0052) | | | |
| HSO implemented× Home-sharing allowed | | | -0.0329*** | | |
| | | | (0.0116) | | |
| HSO implemented× No home-sharing allowed | | | -0.0266*** | | |
| | | | (0.0095) | | |
| HSO implemented×single-family | | | | -0.0238** | |
| | | | | (0.0100) | |
| HSO implemented×apartment | | | | -0.0335*** | |
| | | | | (0.0097) | |
| Share of HSO area 0-200m× outside HSO area | | | | | 0.0219 |
| | | | | | (0.0498) |
| Share of HSO area 200-500m× outside HSO area | | | | | -0.0409 |
| | | | | | (0.0342) |
| Property characteristics | Yes | Yes | Yes | Yes | Yes |
| Census block fixed effects | Yes | Yes | Yes | Yes | Yes |
| HSO area×month fixed effects | Yes | Yes | Yes | Yes | Yes |
| Border segment×month fixed effects | No | Yes | Yes | Yes | Yes |
| Number of observations | 57,844 | 57,930 | 57,961 | 58,000 | 57,903 |
| Bandwidth, $b$ *(in km)* | 1.8403 | 1.8446 | 1.8467 | 1.8432 | 1.8432 |
| $R^2$ | 0.9097 | 0.9097 | 0.9098 | 0.9098 | 0.9097 |

*Notes*: We exclude transactions occurring within one year after implementation of the HSO. Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In column (4) of Table 6 we include an interaction term with housing type. If local negative externalities of *Airbnb* listings (*e.g.*, noise) *within buildings* are important, we might expect to see that prices of apartments have decreased less due to the HSO. This is not what we find. If anything, the effect of the HSO is slightly stronger for apartments. However, the difference in the effects for apartments and single-family homes is not statistically significant.

Column (5) investigates to what extent negative external effects related to tourism that spread out to other areas play a role. Recall that theory indicates that an HSO has a negative effect on house prices, because of a decrease in the demand for housing, but a positive effect because of a reduction in negative tourist externalities. It is possible that reductions in tourism within the HSO border also reduce negative effects of tourism across the border, potentially increasing prices across borders. To investigate this, we calculate the share of land within 200m and between 200 and 500m, just outside HSO areas. If there are negative external effects of *Airbnb*, one expects to see price increases close to the HSO borders, because of a reduction in those negative externalities. We do not find evidence for this.[32] Furthermore, the main effect related to HSOs is about the same as in the baseline specification. Overall, this implies that negative external effects of *Airbnb*, if present, do not spread out to adjacent area and are unlikely to be very large.

In Appendix A.4.1, we investigate heterogeneity between different cities in Los Angeles County by estimating separate effects for each city. We exclude cities for which there are a limited number (<100) of transactions after implementation of the HSO (*e.g.* in Pasadena, Calabasas) or when there are fewer than 1000 transactions in or near the HSO area over the whole period (*e.g.* in Rolling Hills, Hermosa Beach). We are left with 8 HSO cities. The results are not always precise. Nevertheless, we find that in 6 cities the effect is negative (and for two cities are highly statistically significant). For most of the cities, these effects are not statistically different (at the 5% level) from the baseline estimate, which suggests that the variation in estimates between cities might be entirely due to random variation and not due to more fundamental factors (*e.g.*, the extent the HSO is enforced).

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. Another issue is that it might have taken some time before the HSO capitalized into house prices. We have tested this, with results shown in Figure 8. We find that before implementation of the HSO there is no statistically significant price decrease, hence there is no anticipation effect. At the moment of implementation we find that prices are about 2.5% lower. The price effect becomes somewhat stronger over time, in line with Figure 7 in Section 6.1. 1.5 years after implementation the effect of HSOs stabilizes at around 4.5%.

### *6.3 Airbnb listings and house prices*

One could argue that the local average treatment effect of the HSO as estimated above 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

[32] We play around with different thresholds and include different rings, but the conclusion that external effects of *Airbnb* into adjacent areas are absent is unaffected.

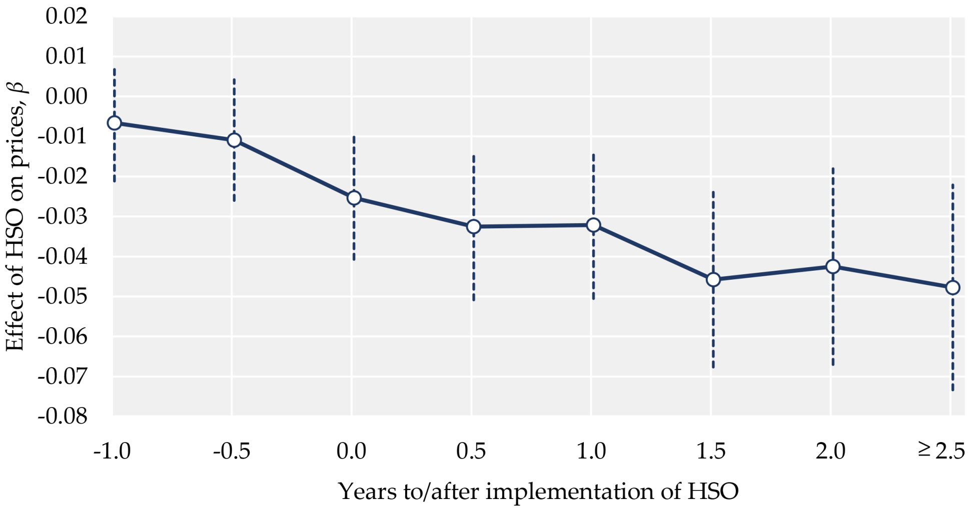



Figure 8 – The effect of the HSO on house prices over time

*Notes*: The optimal bandwidth $b^* = 1.8236$. The dotted lines denote the 95% confidence bands.

the listings probability implies a stronger absolute decrease in the listings rate in areas with a higher initial listings rate). We therefore estimate a 'structural equation' wherein we estimate the direct impact of the listings rate on house prices. To deal with endogeneity issues – omitted variable bias and potentially measurement error in the listings rate – we employ an instrumental variable approach using the HSOs in the different cities.

Table 7 reports the regression results for the two-stage Panel RDD.[33] We observe in Table 7 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.4.5. They indicate that the listings rates have decreased differently in different HSO areas. In areas popular by tourists, such as West-Hollywood, Santa Monica and Beverly Hills, the listings rate has decreased by 0.5-1.5 percentage points. This is in line with what we already established in the previous subsection: the HSO has strongly reduced the number of *Airbnb* listings.

In column (1), Panel A, we find that a 1 percentage point increase in the *Airbnb* listings rate increases property prices by 3.5%. In column (2) we include border segment×month fixed effects. The effect reduces to 2.3%. A standard deviation increase in the listings rate is associated with a $1.845 \times 0.0226 = 4.2\%$ increase in prices, so the effect of *Airbnb* is substantial. The elasticity

[33] Table 7 also report the bandwidths. We obtain the bandwidth from the first stage: a regression of the listings rate on the city-specific HSO dummies. We group all HSO areas with fewer than 500 transactions after the implementation of the HSO into one group.

Table 7 – Airbnb listings and house prices: 2SLS estimates
*(Dependent variable: log of house price)*

| | *Panel RDD* | *+ Border segment f.e.* | *Bandwidth: $h^* \times 2$* | *Bandwidth: $h^*/2$* | *Non-linear effect* | *Other Instruments* |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Listings rate <200 *(in %)* | 0.0351*** | 0.0226** | 0.0301*** | 0.0114 | 0.0203*** | 0.0231** |
| | (0.0100) | (0.0088) | (0.0097) | (0.0087) | (0.0078) | (0.0093) |
| Listings rate$^2$ <200 *(in %)* | | | | | 0.0001** | |
| | | | | | (0.0000) | |
| Property characteristics | 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 | 81,672 | 80,905 | 132,571 | 49,984 | 80,905 | 80,687 |
| Bandwidth, $b$ *(in km)* | 2.9847 | 2.9628 | 5.9256 | 1.4814 | 2.9628 | 2.9521 |
| Kleibergen-Paap $F$-statistic | 18.49 | 18.15 | 17.47 | 16.94 | 18.15 | 64.24 |

*Notes*: We exclude transactions occurring within one year after implementation of the HSO. We estimate the specifications in columns (1)-(4) and (6) by Limited Information Maximum Likelihood, while the specification in column (5) uses a Control Function approach. In columns (1)-(5), the listings rate is instrumented with city-specific dummies indicating whether an HSO has been implemented. In column (6) we instrument the listings rate with the HSO dummy and an interaction term of HSO with log picture density. Robust standard errors are clustered at the census block level and in parentheses. We use cluster-bootstrapped standard errors (250 replications) in column (5). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

of prices with respect to the average number of listings in the sample is $0.0226/1.1523 = 0.0386$. When we only focus on areas where an HSO is implemented this elasticity is very similar and equal to 0.0303.[34]

Changing the bandwidth substantially does change the results a bit, as shown in columns (3) and (4) in Panel A. Notably, the effect is smaller when we focus on areas closer to the borders. One explanation for this is that we use a smoothed value of the listings rate (<200m), so it is harder to identify the effect of interest close to the border. In any case, the result is not statistically significant from the baseline estimate.

In column (5) we test for the non-linearity of the listings rate on house prices by including a second-order effect of the listings rate. Because we have a non-linear endogenous variable we use a control function approach rather than the LIML estimator, implying that we include the first-stage residual and the square of the residual as a control function in the second stage. We find evidence for a slight convex effect of the listings rate on house prices. For example, for a listings rate of 0.5%, the marginal effect is 0.0204, while for a listings rate of 25% it is 0.0253. From this, we conclude that non-linearity in the effect of listings rate on house prices is not an

[34] These estimates are of a similar order of magnitude as Barron et al. (2018), who use a completely different identification strategy.

important issue.

Column (6) considers the use of other instruments. More specifically, we include a dummy whether an HSO has been implemented and interact the HSO dummy with log tourist picture density in the neighborhood or city, to allow for the fact that the HSO has a heterogeneous effect on the listings rate. The first-stage results in Appendix A.4.5 indeed show that HSOs have a more substantial effect on the listings rate in areas with a higher tourist picture density. The second-stage coefficient is almost identical to the preferred specification reported in column (2).

### *6.4 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 8 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.[35]

One obvious candidate for a placebo 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. Again, to determine the timing, for each neighborhood in LA we take the nearest city that has implemented an HSO. Column (3) in Table 8 shows that there is no effect of this placebo HSO on listings or prices.

[35] In Panel A, we exclude transactions in HSO areas. In Panel B we further exclude transactions within one year of the placebo treatment, in keeping with the results reported in Table 5.

Table 8 – 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 | -0.0036 | -0.0187 | -0.0066 | -0.0159 | | |
| | (0.0064) | (0.0192) | (0.0068) | (0.0135) | | |
| Property fixed effects | Yes | Yes | Yes | Yes | | |
| Border segment×month fixed effects | Yes | Yes | Yes | Yes | | |
| Number of observations | 406,245 | 476,250 | 746,130 | 276,075 | | |
| Bandwidth, $b$ *(in km)* | 1.5361 | 1.331 | 1.0459 | 1.588 | | |
| $R^2$ | 0.3530 | 0.3683 | 0.3602 | 0.3780 | | |
| *Panel B: (Dep.var.: log of house price in $)* | (1) | (2) | (3) | (4) | (5) | (6) |
| Placebo HSO implemented | 0.0093 | -0.0009 | -0.0119 | -0.0091 | 0.0011 | -0.0046 |
| | (0.0173) | (0.0095) | (0.0110) | (0.0104) | (0.0111) | (0.0101) |
| Property characteristics | 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 | 53,220 | 113,456 | 88,324 | 86,111 | 55,138 | 62,709 |
| Bandwidth, $b$ *(in km)* | 1.8688 | 1.3031 | 1.5688 | 1.2558 | 1.3421 | 1.9594 |
| $R^2$ | 0.9067 | 0.9111 | 0.9125 | 0.9104 | 0.9035 | 0.8669 |

*Notes:*In Panel B, we exclude transactions occurring within one year after implementation of the placebo HSO. Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Column (4) continues by checking whether 'unincorporated' areas, which have identical regulation with respect to 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.

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.

Therefore, all placebo-estimates reported in Table 8 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). In Appendix A.4.4 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.4.5 reports first-stage results of the impact of the HSO on the listings rate, followed by results where we use an alternative proxy for the listings rate based on the first and last review in Appendix A.4.6. The results are very similar.

We present some robustness checks regarding the choice of instruments for the listings rate in Appendix A.4.7. For example, we further test for robustness by including only the single-best instrument (see Angrist & Pischke 2008, pp. 212-213). We also test for non-linearity of the listings rate effect when using other instruments. Our results are very robust.

In Appendix A.4.8 we examine robustness of our results if we *(i)* include propery rather than census block fixed effects, *(ii)* use distance to the border×year trends, *(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, *(vi)* exclude outliers in the listings rate and *(vii)* measure the listings rate within 100m or 500m, rather than within 200m. The results are very robust.

### *6.5 HSOs, listings and rents*

So far, we focused on the effects of HSOs and *Airbnb* listings 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. Because renters should be indifferent to properties that are close to HSO borders, we cannot use a Panel RDD to identify the housing supply effect. We therefore use a more standard difference-in-differences strategy and include observations further away from the border. Table 9

Table 9 – DiD results for rents

*(Dependent variable: log of median rent per $m^2$)*

| | *All obs.* | *Outside HSO, <10km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, <2.5km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, >5km, <10km* |
|---|---|---|---|---|---|---|
| *Panel A:* Effects of HSOs | (1) OLS | (2) OLS | (3) OLS | (4) OLS | (5) OLS | (6) OLS |
| HSO implemented | -0.0347*** | -0.0372*** | -0.0496*** | -0.0192 | -0.0376*** | -0.0440*** |
| | (0.0118) | (0.0120) | (0.0134) | (0.0128) | (0.0124) | (0.0132) |
| 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 | 3,313 | 2,564 | 1,805 | 1,396 | 1,805 | 1,266 |
| $R^2$ | 0.9893 | 0.9802 | 0.9806 | 0.9832 | 0.9816 | 0.9854 |
| *Panel B:* Effects of listings | (1) 2SLS | (2) 2SLS | (3) 2SLS | (4) 2SLS | (5) 2SLS | (6) 2SLS |
| Listings rate | 0.0365* | 0.0321** | 0.0575** | 0.0112 | 0.0278* | 0.0300 |
| | (0.0199) | (0.0148) | (0.0274) | (0.0099) | (0.0165) | (0.0265) |
| 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 | 3,313 | 2,564 | 1,805 | 1,396 | 1,805 | 1,266 |
| Kleibergen-Paap $F$-statistic | 78.32 | 72.48 | 89.51 | 49.86 | 214.7 | 87.20 |

*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 city-specific dummies indicating whether an HSO has been implemented. Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

reports the results. In Panel A we test for the effect of HSOs on rents, while in Panel B we test for the direct effect of listings, while instrumenting the listing rate with the dummy indicating whether an HSO has been implemented in the area.

In column (1), Panel A, we show that due to HSOs, rents have decreased by 3.4%. Column (2) shows that the effect is essentially the same when we exclude properties that are further away than 10km 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 (2.5km) but outside HSO areas. The results indicate an effect that is slightly stronger (4.9%).

This estimate is slightly higher than 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 2km of both sides). We indeed find no statistically significant difference between HSO areas and areas outside HSOs, which is in line with the idea that properties that are close to the

HSO border are (likely) close substitutes. 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 that are inside HSO areas and further away than 5km from any HSO border. We find that house prices then decrease by 4.4% when an HSO is implemented.

In Panel B, Table 9, we use the listings rate. 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 city-specific HSO dummies also have the expected sign and are statistically significant and negative in all case. In column (1) we find that when the listings rate increases by 1 percentage point (0.69 standard deviations), rents increase by 3.7%. This effect is comparable to the results we found in Table 7. The effect is slightly lower when we only include observations within 10km of an HSO border (column (2)), while it almost doubles when we exclude zipcodes outside HSO areas that are within 2.5km of an HSO border (column (3)). In line with previous results, we do not find any effect of the listings rate when focusing on zipcodes close to HSO borders (column (4)). If we control for distance to CBD×year and distance to the beach×year trends, the effects are very comparable to the effect on house prices, although the coefficients are not very precise.

In Appendix A.4.9 we make sure that the results also hold for median list prices: we show that the house price effects using the DiD estimation strategy deliver similar results as the ones reported in Tables 5 and 7. This suggests that the DiD strategy is a plausible alternative estimation strategy. Moreover, these ancillary regression highlight that the local treatment effect identified through a Panel RDD is about equal to the average treatment effect identified through a DiD strategy.

## 7 Welfare implications: back-of-the-envelope calculations

We showed that the HSO leads to lower house prices and rents. This effect is due to a less efficient use of housing and a reduction in supply of rental housing respectively. We now aim to calculate the back-of-the-envelope welfare effects of the HSO as well as the distributional implications of the policy. We then continue to calculate the overall effects of *Airbnb* on the housing market.

To be able to comment on the quantitative welfare effects of the HSO, we make several restrictive assumptions. We refer to Appendix A.5 for more details on these assumptions, but we discuss here the most important ones. First, welfare effects of tax avoidance are assumed to be absent. This assumption is not too restrictive because the effects of tax avoidance by *Airbnb* suppliers tend to be very small in LA County, given the relatively low tax rates in California.[36] Second, we focus on the comparison between observations that are either in or far from the HSO border (which implies that we avoid the complication that rental properties close to the border are not affected by the HSO, see Section 6.5). Third, we consider the case where outside investors buy properties to rent out via STR-platforms and assume that the investors' willingness to pay slightly exceeds the highest willingness to pay of incumbent households. Fourth, we assume that the marginal utility of income is the same for owners and renters.

Our preferred estimate in Table 5 indicates a 2.9% decrease in property values due to HSOs. We calculate then the welfare effect using the average house price in HSO cities and obtain an annual welfare loss of HSOs of about $680 per property. Given the substantial benefits of *Airbnb*, this seems a reasonable number.[37] The intuition for such a substantial loss is that the investors' WTP is likely much higher than the WTP of the marginal incumbent household being priced out of the market.

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 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 6.5), we just report price effects here.

Our estimates imply that the gains of *Airbnb* for LA County as a whole are quite modest (2.7%). This is only true because many areas in LA counties have a low listings rate. It is therefore

[36]Nevertheless, because tax avoidance of suppliers of *Airbnb* is likely present by the HSO, our decreases in welfare because of efficiency losses are likely overestimates, but this does not have any consequence for the distributional implications.

[37]In non-HSO areas this loss is slightly lower, because house prices are lower. The welfare loss of HSOs would be about $610 a year in these areas if HSOs would have been implemented.

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 | 1,053 | 1.21 | 2.72 | 717 | 1.26 | 2.84 | 748 | 0.91 | 2.05 | 541 |
| *Total predicted price effects near the CBD:* | | | | | | | | | | |
| CBD <5km | 2,457 | 2.84 | 6.40 | 3,935 | 2.84 | 6.40 | 3,935 | 2.45 | 5.52 | 3,393 |
| CBD <2.5km | 5,054 | 4.56 | 10.30 | 13,011 | 4.56 | 10.30 | 13,011 | 3.91 | 8.84 | 11,164 |
| *Total predicted price effects near the beach:* | | | | | | | | | | |
| Beach <5km | 1,128 | 1.93 | 4.36 | 1,228 | 2.11 | 4.77 | 1,345 | 1.49 | 3.36 | 0,947 |
| Beach <2.5km | 1,113 | 2.44 | 5.51 | 1,534 | 2.68 | 6.06 | 1,688 | 1.87 | 4.23 | 1,177 |
| *Total predicted price effects for specific neighborhoods:* | | | | | | | | | | |
| Venice | 1,212 | 12.77 | 28.84 | 8,736 | 12.77 | 28.84 | 8,736 | 8.92 | 20.15 | 6,104 |
| West Hollywood | 1,593 | 3.55 | 8.01 | 3,190 | 5.10 | 11.51 | 4,581 | 3.55 | 8.01 | 3,190 |
| Malibu | 2,193 | 5.89 | 13.30 | 7,291 | 5.89 | 13.30 | 7,291 | 4.15 | 9.36 | 5,135 |
| Santa Monica | 1,645 | 1.76 | 3.98 | 1,636 | 2.80 | 6.32 | 2,601 | 1.76 | 3.98 | 1,636 |
| Redondo Beach | 888 | 1.17 | 2.64 | 587 | 1.49 | 3.36 | 0,745 | 1.17 | 2.64 | 587 |
| Pasadena | 928 | 0.96 | 2.17 | 503 | 1.29 | 2.91 | 675 | 0.96 | 2.17 | 503 |

*Notes*: Information is for September 2018. To estimate the yearly effects, we assume a discount rate of 2%. We further assume that rents are equal to discounted house prices.

interesting to focus on areas within 5km of the Los Angeles's CBD, where the listings rate is more than twice the County's average. When we focus on latter areas, the total house price effect due to *Airbnb* is estimated to be 6.4%. When we limit ourselves to areas within 2.5km of the CBD, we find an effect of 10.3%, which is substantial. 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 the CBD have increased by more than 40% in the last 10 years, so it seems that our estimated effects are not unrealistically high.

We also consider the effects in beach towns. Within 2.5km of the beach, the price increase due to *Airbnb* has been 4.4%. If we concentrate on specific cities and neighborhoods, the price effects of *Airbnb* do vary substantially. In one of the most popular LA neighborhoods – Venice – the total price increase is almost 30%.[38] On the other hand, in Pasadena (which is about 15km from Downtown LA), the effects of *Airbnb* are small.

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 10% and 0.4%. For Santa Monica, which is well known for its strict HSO, the listings rate would increase by 60% and the house price by almost 2.3%, which is substantial. For locations near the CBD, abandoning HSOs does not imply changes in property values, because no areas within the CBD are targeted by HSOs.

By contrast, if all cities would implement HSOs this has implications for prices near the CBD. Within 5km of the CBD, the listings rate would decrease by almost 15% and prices by 0.9%. For Venice the listings rate would drop by 30% and house price by 8.7%. 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 $3-8 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

[38] Of course, we should take this prediction with a pinch of salt, because we extrapolate a linear effect to large non-marginal changes in the listings rate. However, we do not find strong evidence that non-linearity is important (see column (5), Table 7).

from the HSO, as the demand for housing will decrease. This effect is due to a 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.[39]

## 8 Conclusions

We have seen a spectacular growth of online short-term housing rental platforms in recent years. However, it is yet unknown how these platforms affect the housing market.

We exploit quasi-experimental variation in *Airbnb* listings to test the impact of short-term rentals on house prices and rents. 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 timing of the HSOs. Home Sharing Ordinances reduce *Airbnb* listings by about 70%, and reduce house prices by 3% 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 of away from privately owned housing towards the long-term rental market – a housing supply effect.

On average, the total effect of *Airbnb* on property values in LA County is modest (2.7%). 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 the CBD, for example, the increase in property values is almost

[39]We have analyzed the conditional relationship between the share of renters and the probability to implement an HSO using data for 88 incorporated cities and two unincorporated places in the Los Angeles area. Slightly surprisingly, for these data, there is no correlation between the share of renters and the introduction of the HSO, but as shown in Appendix A.6, when we condition on income per capita (and, less importantly, a range of demographic indicators), then there appears to be a strong effect of the share renters on the probability to implement an HSO: the results suggest that there is proportional relationship between these two variables. We perceive this result as suggestive only, as we do not have exogenous variation in the share of renters. We also gathered some data for 29 other U.S. cities. We find again suggestive evidence that a majority of renters is associated with more stringent *Airbnb* regulations using a sample of 29 major U.S. cities. We use the maximum number of days per year allowed for short-term renting as an (inverse) measure of stringency. The maximum allowed number of rental days is 45 for cities with a majority of renters, while it is 246 for all other cities (the correlation between maximum allowed number of rental days and the share of renters is $-0.25$).

10%. In Venice, which is a popular tourist destination, *Airbnb* has increased property values by almost 30%.

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.

Ignoring the distributional consequences, our results suggest that *Airbnb* regulation has a negative effect on overall welfare, given the important proviso that tax avoidance by suppliers of *Airbnb* listings is limited. Rather than regulating short-term rental platforms quantitatively, it seems feasible to address current tax avoidance more directly. For example, for several cities in the world, including San Francisco and Amsterdam, *Airbnb* agreed to collect an ad-valorem tourist tax for the city (without revealing information about *Airbnb* hosts).

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Sheppard, S. & Udell, A. (2016), 'Do AirBnB Properties Affect House Prices?', *Williams College Department of Economics Working Papers* .

Turner, M., Haughwout, A. & Van der Klaauw, W. (2014), 'Land Use Regulation and Welfare', *Econometrica* **82**(4), 1341–1403.

Van der Borg, J., Camatti, N., Bertocchi, D. & Albarea, A. (2017), 'The Rise of the Sharing Economy in Tourism: Exploring Airbnb Attributes for the Veneto Region', *Mimeo* .

Van Duijn, M. & Rouwendal, J. (2013), 'Cultural Heritage and the Location Choice of Dutch Households in a Residential Sorting Model', *Journal of Economic Geography* **13**(3), 473–500.

Wachsmuth, D. & Weisler, A. (2017), 'Airbnb and the Rent Gap: Gentrification Through the Sharing Economy', *Mimeo, McGill University* .

Williams, L. (2016), 'When Airbnb Rentals Turn into Nuisance Neighbours', *The Guardian* (September 18, 2016).

Zervas, G., Proserpio, D. & Byers, J. (2017), 'The Rise of the Sharing Economy: Estimating the Impact of Airbnb on the Hotel Industry', *Journal of Marketing Research* **54**(5), 687–705.

# Online Appendix

## *A.1 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 some time during our study period. For each city we report whether is has implemented an HSO, whether home sharing is permitted, whether a STR needs to register at the municipality and whether officially STRs are not allowed according the residential zoning code. Furthermore, we list the sources from which we get the information.

## *A.2 Bandwidth selection*

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

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

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 $\ell_{ikt}$ 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 & Kalyanaraman 2012). Indeed, adding a wide array of controls barely influences the optimal bandwidth in our specifications.

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 |

Table A1 – *continued*

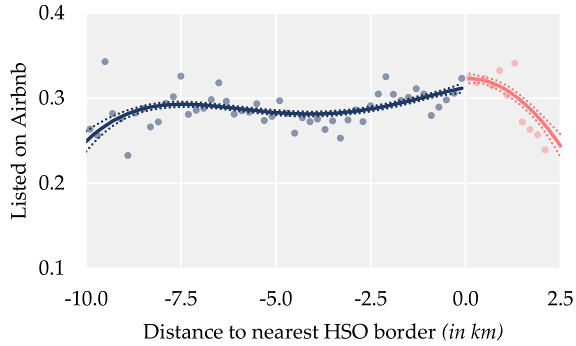
| Name of city | Year and month of implementation | | HSO | Home sharing not allowed | Register STR | STR not in zoning code | Source |
|---|---|---|---|---|---|---|---|
| Paramount | | | 0 | 0 | 0 | 1 | web search |
| Pasadena | 2017 | 10 | 1 | 0 | 1 | 0 | web search |
| Pico Rivera | | | 0 | 0 | 0 | 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 |
| 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:

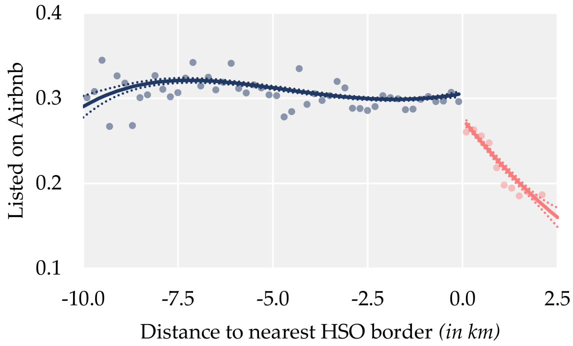
- https://la.lawsoup.org/legal-guides/laws-by-topic/short-term-vacation-rentals/,
- https://www.dailybreeze.com/2016/03/02/redondo-beach-becomes-latest-south-bay-city-to-crack-down-on-short-term-rentals/,
- https://www.newportbeachca.gov/government/departments/finance/revenue-division/short-term-rentals,
- 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/561486382174945697I/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/2750940922018040358852.PDF,
- https://ttc.lacounty.gov/othertaxes/docs/FAQs%20for%20Online%20Hosting%20Platform%20FINAL.pdf."

## *A.3 Other graphical evidence*

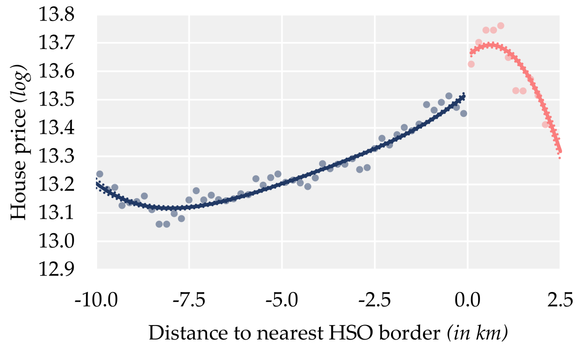
In this Appendix we review ancillary graphical evidence that supports the identifying assumptions we make in our research design. In Appendix A.3.1 we first consider cross-sectional variation in the listing probability and house prices around the borders of HSO areas. Appendix A.3.2 considers discontinuities in housing characteristics and Appendix A.3.3 investigates jumps in



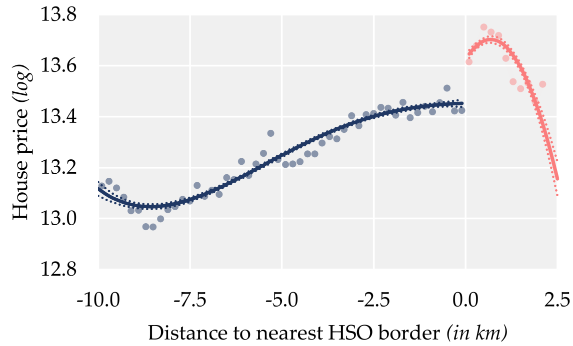

(A) Airbnb listings, before HSO




(B) Airbnb listings, after HSO




(C) House price, before HSO




(D) House price, after HSO

Figure A1 – Variation near HSO borders before and after the HSO

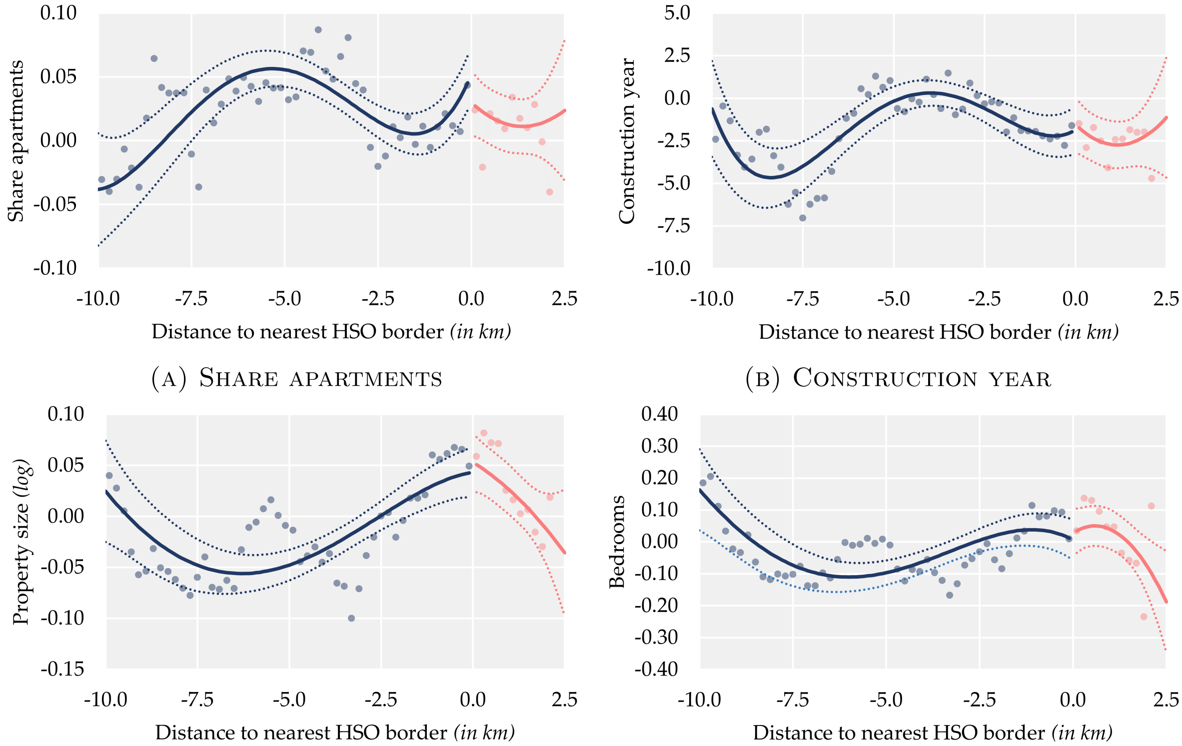
*Notes*: All values are demeaned by HSO border×month fixed effects. Negative distances indicate areas outside HSO areas and areas inside HSO areas *before* treatment. The dots are conditional averages at every 200m interval. The dotted lines denote 95% confidence intervals.

densities of key variables after the HSO has been implemented. Appendix A.3.4 investigates trends in rents and house prices after HSOs have been implemented.

### A.3.1 Cross-sectional differences in listings and prices

In Figure A1 we illustrate cross-sectional differences in listings and house prices *before* and *after* the HSOs were implemented. In Figure A1a, we compare the probability of being listed *before* a 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 Figure A1b). Note that this effect is somewhat stronger when focusing on listings of entire properties (in keeping with the results reported in Figure 3).

In Figures A1c and A1d, we consider cross-sectional variation in house prices on both sides of the border before and after implementation of the HSO. This exercise clearly illustrates the difficulty of obtaining valid estimates for the effects of *Airbnb* on house prices using a cross-sectional




(A) Share apartments (B) Construction year

(C) Property size *(log)* (D) Bedrooms, *demeaned*

Figure A2 – Housing transactions: variation near HSO borders

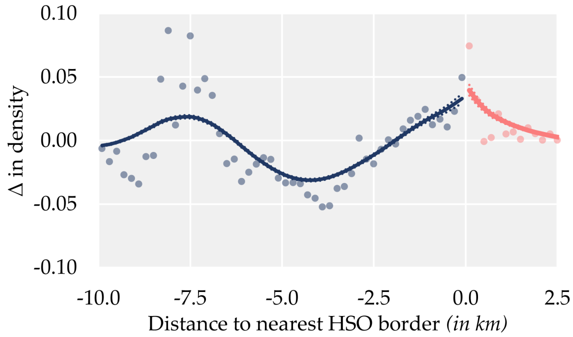
*Notes*: All values are demeaned by census block group and HSO border×month fixed effects. Negative distances indicate areas outside HSO areas and areas inside HSO areas *before* treatment. The dots are conditional averages at every 200m interval. The dotted lines denote 95% confidence intervals.

approach, as there is substantial variation in prices around the border. Moreover, there is a discrete jump in house prices *at* the HSO border. This is not too surprising, as there may be cross-sectional differences in provision of public goods or taxes. These differences are unlikely to play a role when comparing house price changes across the border over time.

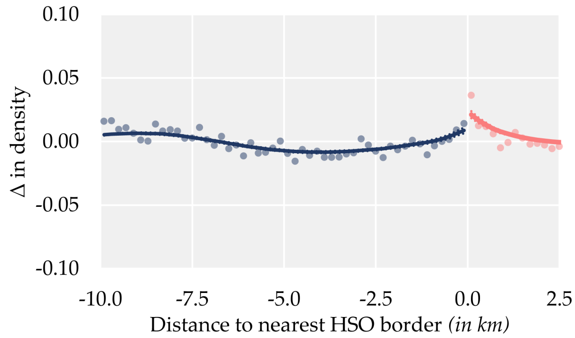
### A.3.2 Discontinuities in housing characteristics

An important assumption in our Panel Regression Discontinuity Design is that changes in covariates, except for the treatment variable, are continuous at the border, for which we provided evidence in Figure A2. We therefore investigate in Figure A2 whether changes in housing characteristics over time do not show discontinuities.

Figure A2a highlights that the change in the share of apartments is not statistically significantly different at the border of HSO areas. Figure A2b further shows that the change in construction year is not statistically significantly different between HSO areas and areas that are not targeted. This also holds for property size (Figure A2c) and the number of bedrooms (Figure A2d).




(A) Density differential of listings




(B) Conditional Density differential of transactions

Figure A3 – Conditional McCrary density tests before HSOs

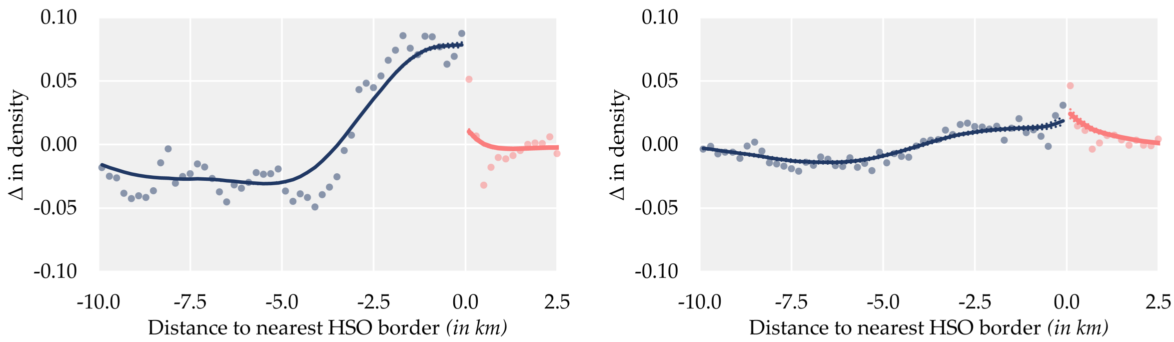
*Notes*: We focus on observations *before* implementation of the HSO. Negative distances therefore indicate areas outside HSO areas. The dots are conditional densities at every 200m interval. The dotted lines denote 95% confidence intervals. On the $y$-axis we ploy the difference in densities of McCrary's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

### A.3.3 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 types of households sorting themselves into HSO 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 for 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 Figure A3. Figure A3a 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 Figure A3b. 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.




(A) Density differential of listings (B) Density differential of transactions

Figure A4 – 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.

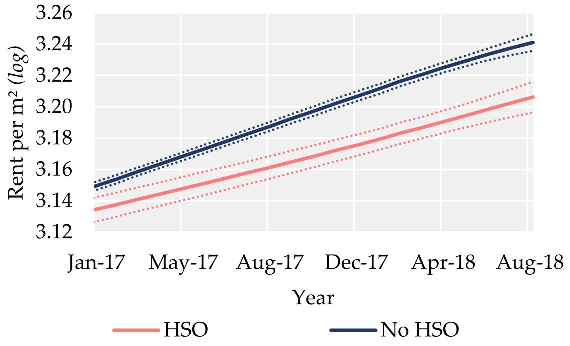
We repeat this exercise by estimating the adapted McCrary's density test *after* an HSO was implemented, but given the spatial distribution of buildings in 2014. In Figure A4a we show that *Airbnb* listings are now discontinuous after the HSO. The density is much lower in treated areas, which is in line with the finding that listings have been reduced due to the implementation of HSOs. For house prices (Figure A4b) we find essentially the same difference in density of transactions as in A3b, which we think is reassuring: the HSO did not lead to a different market turnover on both sides of the HSO borders.

#### A.3.4 Trends in property values

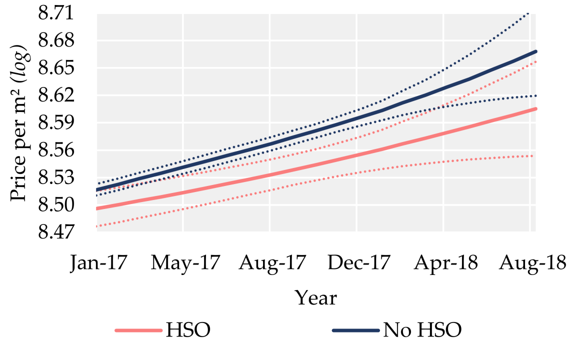
In Figure A5a we report trends in rents for untreated and treated observations. We consider the trend for treated observations only after January 2017 to have a sufficient number of areas where HSOs were implemented. We can clearly see that the rent in treated areas is always lower (about 3%) than in areas where HSOs have not (yet) been implemented. We repeat this exercise for *Zillow* prices in Figure A5b, leading to the same conclusion.

### *A.4 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




(A) Trends in rents




(B) Trends in *Zillow* prices

Figure A5 – Trends in property values

*Notes*: We estimate regressions with zip-code fixed effects and a $4^{\text{th}}$-order polynomial of months in not (yet) treated areas and a $2^{\text{nd}}$-order polynomial of time inside treated areas. We focus on observations from 2017 onwards because we have few observations in treated areas before 2017. The dotted lines denote 95% confidence intervals.

HSOs. In Appendix A.4.4 we investigate whether the standard errors change when accounting for cross-sectional and temporal dependence. We then proceed by reporting the first-stage results in Appendix A.4.5. Appendix A.4.6 we use another proxy for the listings rate, followed by robustness checks on the choice of instruments for the listings rate 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.

### A.4.1 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 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 is less convincing. We report the results in Table A2.

In columns (1) and (2) we report the results for respectively listings of entire properties and home sharing. We exclude observations in and near cities that have implemented HSOs before the first observation in the data and cities for which we have fewer than 1000 listings over the whole sample period. 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).

Table A2 – City-specific effects for listings and prices

| | (Dep.var.: entire property is listed) | (Dep.var.: home sharing is listed) | (Dep.var.: log of house price in $) |
|---|---|---|---|
| | (1) | (2) | (3) |
| HSO implemented×Arcadia | -0.0999 | -0.0933** | |
| | (0.0626) | (0.0465) | |
| HSO implemented×Beverly Hills | | | 0.0371 |
| | | | (0.0293) |
| HSO implemented×Burbank | | | -0.0710*** |
| | | | (0.0274) |
| HSO implemented×Calabasas | | | |
| HSO implemented×Cerritos | | | -0.0340 |
| | | | (0.0322) |
| HSO implemented×Hermosa Beach | -0.1707*** | -0.3095*** | |
| | (0.0556) | (0.0768) | |
| HSO implemented×Lawndale | | | |
| HSO implemented×Manhattan Beach | -0.0344 | -0.0758* | -0.0362 |
| | (0.0315) | (0.0423) | (0.0301) |
| HSO implemented×Maywood | | | |
| HSO implemented×Palos Verdes Estates | | | |
| HSO implemented×Pasadena | -0.0177 | -0.0173 | |
| | (0.0396) | (0.0454) | |
| HSO implemented×Rancho Palos Verdes | -0.2101 | 0.1133 | |
| | (0.1691) | (0.1054) | |
| HSO implemented×Rolling Hills | | | |
| HSO implemented×Rolling Hills Estates | | | |
| HSO implemented×Redondo Beach | 0.0099 | -0.0632 | -0.0376 |
| | (0.0532) | (0.0626) | (0.0336) |
| HSO implemented×Santa Monica | -0.1713*** | -0.0500*** | -0.0236 |
| | (0.0123) | (0.0160) | (0.0240) |
| HSO implemented×Torrance | 0.0994** | 0.0482 | -0.0451** |
| | (0.0503) | (0.0529) | (0.0202) |
| HSO implemented×West-Hollywood | -0.0909*** | -0.0778*** | 0.0234 |
| | (0.0121) | (0.0200) | (0.0267) |
| Property characteristics | No | No | Yes |
| Property 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 | 264,605 | 163,969 | 33,680 |
| Bandwidth, $b$ *(in km)* | 1.6674 | 1.8255 | 1.9269 |
| $R^2$ | 0.3496 | 0.3393 | 0.8574 |

*Notes*: In column (3), we exclude transactions occurring within one year after implementation of the HSO. Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

There is only one estimated effect in these two columns, which is in contrast to expectations: in column (1) the HSO seems to have had a positive effect on the probability that an entire property is listed in Torrance and it is (only) just statistically significant at the 5% level. This turns out to be a statistical artifact (as we estimate 18 coefficients in columns (1) and (2), there is a 60% chance that at least one coefficient is significant when using significance levels of 5% in the absence of any effect). In another analysis, *i.e.*, the first stage of the structural equation results, where we estimate the city-specific effect of the HSO on the listings rate, we find that the effect of HSO for Torrance is negative (albeit statistically insignificant) (see Appendix A.4.5).

In column (3) we focus on house prices. Again, we exclude observations for which we have fewer than 1000 transactions in or near the respective city. This leaves us with 8 cities for which we can estimate the effect. The results show that the effect is in most cases negative, although somewhat imprecise. We find statistically significant effects for Burbank and Torrance. The two positive point estimates observed in Beverly Hills and West-Hollywood are far from being statistically significant.

### A.4.2 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 A3. 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 apartments 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 changing the optimal bandwidth in columns (3) and (4). In column (5) we include property fixed effects. In all cases the effect of an HSO on prices is economically negligible and statistically insignificant.

We extend these results by using the same difference-in-differences approach as in Section 6.5. In Table A4 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

TABLE A3 – HSOS AND AIRBNB PRICES

*(Dependent variable: log of price per night)*

| | *Panel RDD* | *+ Border segment f.e.* | *Bandwidth: $h^* \times 2$* | *Bandwidth: $h^*/2$* | *Property f.e.* |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| HSO implemented | -0.0069 | -0.0057 | -0.0070 | -0.0024 | -0.0052 |
| | (0.0089) | (0.0089) | (0.0092) | (0.0106) | (0.0105) |
| Private room | -0.2326*** | -0.2329*** | -0.2380*** | -0.2377*** | -0.2337*** |
| | (0.0139) | (0.0141) | (0.0195) | (0.0194) | (0.0259) |
| Shared room | -0.3790*** | -0.3791*** | -0.4025*** | -0.4024*** | -0.4092*** |
| | (0.0334) | (0.0350) | (0.0440) | (0.0439) | (0.0665) |
| Accommodation size *(log)* | 0.1012*** | 0.0988*** | 0.1040*** | 0.1038*** | 0.0916*** |
| | (0.0077) | (0.0078) | (0.0106) | (0.0105) | (0.0136) |
| availability | 0.0310*** | 0.0309*** | 0.0300*** | 0.0302*** | 0.0295*** |
| | (0.0018) | (0.0018) | (0.0021) | (0.0021) | (0.0023) |
| Minimum of required nights *(log)* | -0.0109*** | -0.0114*** | -0.0146*** | -0.0147*** | -0.0160*** |
| | (0.0021) | (0.0022) | (0.0028) | (0.0028) | (0.0027) |
| Maximum of required nights *(log)* | -0.0006 | -0.0003 | -0.0003 | -0.0003 | -0.0003 |
| | (0.0011) | (0.0011) | (0.0016) | (0.0016) | (0.0017) |
| Census block fixed effects | Yes | Yes | Yes | Yes | Yes |
| Property fixed effects | No | No | No | No | Yes |
| HSO area×month fixed effects | Yes | Yes | Yes | Yes | Yes |
| Border segment×month fixed effects | No | Yes | Yes | Yes | Yes |
| Number of observations | 126,742 | 126,475 | 198,745 | 71,409 | 94,990 |
| Bandwidth, $b$ | 1.823 | 1.8191 | 3.6381 | 0.9095 | 1.3594 |
| $R^2$ | 0.7728 | 0.7758 | 0.7763 | 0.7825 | 0.9782 |

*Notes*: We exclude transactions occurring within one year after implementation of the HSO. Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

observations within 10km of any HSO border. In column (3), we exclude observations that are close (<2.5km) within a border. Column (4) further controls for distance to CBD and distance to the beach trends. 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 systematic 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 tourists demand for local accommodation is elastic.

### A.4.3 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).

Table A4 – HSOs and Airbnb prices, DiD results

*(Dependent variable: log of price per night)*

| | *All obs.* | *Outside HSO, <10km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, >5km, <10km* |
|---|---|---|---|---|---|
| | (1) OLS | (2) OLS | (3) OLS | (4) OLS | (5) OLS |
| HSO implemented | -0.0069 | -0.0057 | -0.0070 | -0.0024 | -0.0052 |
| | (0.0089) | (0.0089) | (0.0092) | (0.0106) | (0.0105) |
| 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 |
| Property fixed effects | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 3,491 | 2,742 | 1,983 | 1,983 | 1,444 |
| $R^2$ | 0.9888 | 0.9796 | 0.9794 | 0.9805 | 0.9836 |

*Notes*: In all specifications we include observations *inside* HSO areas. We exclude observations occurring within one year after implementation of the HSO. Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A5 – HSOs and traveler accommodations

*(Dependent variable: number of accommodations)*

| | *All obs.* | *Outside HSO, <10km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, <2.5km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, >5km, <10km* |
|---|---|---|---|---|---|---|
| | (1) Poisson | (2) Poisson | (3) Poisson | (4) Poisson | (5) Poisson | (6) Poisson |
| HSO implemented | 0.0978 | 0.0966 | 0.1109 | 0.0603 | 0.1086 | 0.1223 |
| | (0.1001) | (0.1002) | (0.1012) | (0.1025) | (0.1028) | (0.1038) |
| 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 | 1,171 | 965 | 716 | 401 | 716 | 511 |
| Log-likelihood | -1,933 | -1,592 | -1,168 | -679.3 | -1,168 | -835.8 |

*Notes*: In all specifications we include observations *inside* HSO areas. We exclude observations occurring within one year after implementation of the HSO. Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

We obtain yearly data from the *County Business Patterns* at the zipcode 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 6.5, where we use a DiD design. Table A5 reports the results of several Poisson regressions.

In column (1) we include all zipcodes in LA County. The point estimate suggests that the

Table A6 – Spatial HAC standard errors
*(Dependent variable: log of house price in $)*

| | Baseline | $sw = 1 \times b^* km$ | $sw = 2 \times b^* km$ | $sw = 5 \times b^* km$ | $sw = 10 \times b^* km$ |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| HSO implemented | -0.0297*** | -0.0297*** | -0.0297*** | -0.0297*** | -0.0297*** |
| | (0.0080) | (0.0066) | (0.0067) | (0.0069) | (0.0070) |
| Housing characteristics | Yes | Yes | Yes | Yes | Yes |
| Census block fixed effects | Yes | Yes | Yes | Yes | Yes |
| Distance to border×year trends | Yes | Yes | Yes | Yes | Yes |
| Month×property type fixed effects | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 58,284 | 58,284 | 58,284 | 58,284 | 58,284 |
| $R^2$ | 0.9097 | 0.9097 | 0.9097 | 0.9097 | 0.9097 |
| Bandwidth, $b$ *(in km)* | 1.8594 | 1.8594 | 1.8594 | 1.8594 | 1.8594 |
| Spatial cut-off *(in km)* | — | 1.8594 | 3.7188 | 9.2969 | 18.5938 |

*Notes*: We exclude transactions occurring within one year after implementation of HSOs. We estimate standard errors corrected for cross-sectional dependence using a Bartlett kernel and given the indicated spatial cut-offs. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

number of traveler accommodations increase due to HSOs by $\exp(0.0978) - 1 = 10.3\%$, which is sizable. However, the coefficient is quite imprecisely estimated. This also holds in the other specifications, where we include zipcodes that are further away from HSO borders (>2.5km or >5km). When we only include zipcodes close to borders, the point estimate is slightly lower. Hence, we think Table A5 provides suggestive evidence that the number of travelers accommodations have increased due to the HSO.

### A.4.4 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 A6 we report the baseline specification with standard errors clustered at the census block level. If we then allow for 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.

#### A.4.5 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 7. The dependent variable is the *Airbnb* listings rate within 200m of the property in Table A7.

In column (1), Table 7, the coefficients imply that the HSO has reduced listings on average by about 0.5 percentage points, which is 85% of the mean listings rate. However, there is substantial heterogeneity, as expected. We find substantial reductions in the listings rate of above 1 percentage point in Santa Monica and West-Hollywood. In Burbank and Rancho Palos Verdes the reduction is respectively 0.26 and 0.39 percentage point. In other cities we do not find statistically significant effects, but the signs are generally correct. The effects of HSOs on the listings rate tend to become somewhat stronger once we include HSO border segment×month fixed effects, but they are largely in the same ballpark. Columns (3) and (4) in Table 7 show similar effects once we respectively increase or decrease the bandwidth. Column (5) replicates column (2), as the first stage for the non-linear effects is identical to column (2). In column (6) in Table 7 we test for another set of instruments. We then use the HSO dummy as well as an interaction effect of the HSO with the log of tourist picture density. We find that indeed HSOs have a larger (absolute) effect in areas that are popular by tourists. For example, for an area with the mean picture density, the reduction in the listings rate is 0.5 percentage points, while in the HSO area with the highest picture density, the decrease in the listings rate is 1.4 percentage points.

Table A7 – Listings and house prices: First-stage results

*(Dependent variable: listings rate (imputed))*

| | *Panel RDD* | *+ Border segment f.e.* | *Bandwidth: $h^* \times 2$* | *Bandwidth: $h^*/2$* | *Nonlinear effect* | *Other Instruments* |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| HSO implemented× Beverly Hills | -0.2647 | -0.4464*** | -0.2953** | -0.5329*** | -0.4464*** | |
| | (0.1697) | (0.1646) | (0.1504) | (0.1922) | (0.1646) | |
| HSO implemented× Burbank | -0.3856*** | -0.1590 | -0.0419 | -0.1014 | -0.1590 | |
| | (0.1397) | (0.1261) | (0.1010) | (0.1246) | (0.1261) | |
| HSO implemented× Cerritos | 0.0183 | -0.0085 | -0.0554 | -0.0235 | -0.0085 | |
| | (0.0471) | (0.0513) | (0.0579) | (0.0501) | (0.0513) | |
| HSO implemented× Manhattan Beach | 0.4480 | 0.1180 | -0.1049 | 0.6593 | 0.1180 | |
| | (0.3946) | (0.3434) | (0.2902) | (0.4488) | (0.3434) | |
| HSO implemented× Rancho Palos Verdes | -0.2612*** | -0.3667*** | -0.3812*** | -0.3156** | -0.3667*** | |
| | (0.0936) | (0.1194) | (0.1172) | (0.1276) | (0.1194) | |
| HSO implemented× Redondo Beach | -0.8969 | 0.1537 | 0.1285 | 0.3170** | 0.1537 | |
| | (0.7934) | (0.1158) | (0.1037) | (0.1580) | (0.1158) | |
| HSO implemented× Santa Monica | -1.2740*** | -1.4434*** | -1.3213*** | -1.4689*** | -1.4434*** | |
| | (0.1072) | (0.1463) | (0.1348) | (0.1577) | (0.1463) | |
| HSO implemented× Torrance | -0.0105 | -0.0351 | -0.0190 | 0.0548 | -0.0351 | |
| | (0.0460) | (0.0554) | (0.0480) | (0.0854) | (0.0554) | |
| HSO implemented× West-Hollywood | -1.0878*** | -1.3360*** | -1.0336*** | -1.6556*** | -1.3360*** | |
| | (0.2287) | (0.1957) | (0.1566) | (0.2435) | (0.1957) | |
| HSO implemented× Other cities | -0.0897 | -0.3713*** | -0.4109*** | -0.2064 | -0.3713*** | |
| | (0.1396) | (0.1397) | (0.1279) | (0.1763) | (0.1397) | |
| HSO implemented | | | | | | -0.1817*** |
| | | | | | | (0.0654) |
| HSO implemented× Tourist picture density *(log)* | | | | | | -0.3508*** |
| | | | | | | (0.0392) |
| Property characteristics | 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 | 81,672 | 80,905 | 132,571 | 49,984 | 80,905 | 80,687 |
| Bandwidth, $b$ *(in km)* | 2.9847 | 2.9628 | 5.9256 | 1.4814 | 2.9628 | 2.9628 |
| $R^2$ | 0.6968 | 0.7265 | 0.6949 | 0.7458 | 0.7265 | 0.7258 |

*Notes*: Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

### A.4.6 An alternative proxy for the listings rate

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 use a somewhat 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. (2018). 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

Table A8 – Listings and house prices: First-stage results, approximated listings rate

*(Dependent variable: listings rate (approximated))*

| | *Panel RDD* | *+ Border segment f.e.* | *Bandwidth: $h^* \times 2$* | *Bandwidth: $h^*/2$* | *Nonlinear effect* | *Other Instruments* |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| HSO implemented× | -0.1519 | -0.4101*** | -0.3540*** | -0.4099*** | -0.4101*** | |
| Beverly Hills | (0.1278) | (0.1343) | (0.1229) | (0.1570) | (0.1343) | |
| HSO implemented× | -0.3754** | -0.1302 | -0.0098 | -0.0600 | -0.1302 | |
| Burbank | (0.1498) | (0.1039) | (0.0846) | (0.1099) | (0.1039) | |
| HSO implemented× | 0.0600* | 0.0418 | 0.0506 | 0.0663* | 0.0418 | |
| Cerritos | (0.0323) | (0.0335) | (0.0459) | (0.0364) | (0.0335) | |
| HSO implemented× | 0.0366 | -0.1470 | -0.1328 | 0.2572* | -0.1470 | |
| Manhattan Beach | (0.1513) | (0.1331) | (0.1189) | (0.1512) | (0.1331) | |
| HSO implemented× | -0.1934*** | -0.4296*** | -0.4247*** | -0.3719*** | -0.4296*** | |
| Rancho Palos Verdes | (0.0615) | (0.1133) | (0.1118) | (0.1113) | (0.1133) | |
| HSO implemented× | 0.5540 | -0.0816 | -0.0619 | 0.0200 | -0.0816 | |
| Redondo Beach | (0.3948) | (0.0782) | (0.0753) | (0.0891) | (0.0782) | |
| HSO implemented× | -0.9895*** | -1.1945*** | -1.0954*** | -1.1497*** | -1.1945*** | |
| Santa Monica | (0.0936) | (0.1087) | (0.1031) | (0.1241) | (0.1087) | |
| HSO implemented× | 0.0440 | 0.0050 | 0.0225 | 0.0803* | 0.0050 | |
| Torrance | (0.0341) | (0.0384) | (0.0363) | (0.0486) | (0.0384) | |
| HSO implemented× | -0.5165*** | -0.7981*** | -0.7589*** | -1.0869*** | -0.7981*** | |
| West-Hollywood | (0.1678) | (0.1352) | (0.1142) | (0.1642) | (0.1352) | |
| HSO implemented× | -0.0189 | -0.6282*** | -0.6112*** | -0.5307*** | -0.6282*** | |
| Other cities | (0.1296) | (0.1088) | (0.1063) | (0.1148) | (0.1088) | |
| HSO implemented | | | | | | -0.1858*** |
| | | | | | | (0.0397) |
| HSO implemented× | | | | | | -0.2371*** |
| Tourist picture density *(log)* | | | | | | (0.0280) |
| Property characteristics | 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 | 80,466 | 80,017 | 130,994 | 49,480 | 80,017 | 80,203 |
| Bandwidth, $b$ *(in km)* | 2.9256 | 2.9202 | 5.8404 | 1.4601 | 2.9202 | 2.9202 |
| $R^2$ | 0.6834 | 0.7120 | 0.6672 | 0.7479 | 0.7120 | 0.7117 |

*Notes*: Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

($\rho = 0.416$). This indicates that relying merely on one measure may provide non-robust results, so we will also examine the alternative approximated measure.

We first report first-stage results in Table A8. The predicted changes in the listings rates due to HSOs are somewhat lower than when using the imputed measure, which makes sense as the approximated listings underestimate changes in listings over time. However, the qualitative implications are very much in line with the results reported in Table A7.

Table A9 report the second-stage results. We show that using an alternative measure for the listings rate does not matter much for the results. We find coefficients that are slightly stronger:

Table A9 – Listings and house prices: Second-stage results, approximated listings rate

*(Dependent variable: log of house price)*

| | *Panel RDD* | *+ Border segment f.e.* | *Bandwidth: $h^* \times 2$* | *Bandwidth: $h^*/2$* | *Non-linear effect* | *Other Instruments* |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Listings rate <200 (approximated) *(in %)* | 0.0617*** | 0.0325*** | 0.0368*** | 0.0181 | 0.0302*** | 0.0300** |
| | (0.0167) | (0.0118) | (0.0124) | (0.0125) | (0.0111) | (0.0122) |
| Listings rate$^2$ <200 (approximated) *(in %)* | | | | | -0.0000 | |
| | | | | | (0.0000) | |
| Property characteristics | 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 | 80,466 | 80,017 | 130,994 | 49,480 | 80,017 | 80,203 |
| Bandwidth, $b$ *(in km)* | 2.9256 | 2.9202 | 5.8404 | 1.4601 | 2.9202 | 2.9274 |
| Kleibergen-Paap $F$-statistic | 14.60 | 20.77 | 20.85 | 18.41 | 20.77 | 81.68 |

*Notes*: We exclude transactions occurring within one year after implementation of the HSO. We estimate the specifications in columns (1)-(4) and (6) by Limited Information Maximum Likelihood, while the specification in column (5) uses a Control Function approach. In columns (1)-(5), the listings rate is instrumented with city-specific dummies indicating whether an HSO has been implemented. In column (6) we instrument the listings rate with the HSO dummy and an interaction term of HSO with log picture density. Robust standard errors are clustered at the census block level and in parentheses. We use cluster-bootstrapped standard errors (250 replications) in column (5). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

a 1 percentage point increase in the listings rate is associated with a price increase of 3-3.25%. A standard deviation increase in the approximate listings rate implies a price increase of 6.1. Hence, our results are robust regarding the proxy used for *Airbnb* listings in the vicinity.

### A.4.7 Other instruments

In this subsection we investigate the robustness to the use of different instrumentation strategies. We report the results in Table A10.

First we report the results in column (1) when we ignore heterogeneity in the first stage. This leads to a misspecified first stage because the absolute reduction in the listings rate due to the implementation of HSOs is unlikely to be the same for areas with low and high listings rates.[40] We find that prices increase by 5.3% for a 1 percentage point increase in the listings rate if we only use one HSO dummy as an instrument. Hence, the effect is somewhat stronger. Given that the first stage is likely misspecified, we prefer our baseline estimates.

Column (2) follows a suggestion by Angrist & Pischke (2008), pp 212-213, to only use the

[40] Consistent with that, the increase in the $R^2$ of the instruments is only 0.0014 when we use the single HSO dummy, whereas the increase more than doubles when we use city-specific dummies.

Table A10 – Robustness for the use of instruments
*(Dependent variable: log of house price)*

| | *only HSO instrument* | *HSO in Santa Monica* | *3 HSO instruments* | *HSO× log pictures* | *Non-linear effect* |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Listings rate <200m *(in %)* | 0.0513*** | 0.0189* | 0.0176** | 0.0197** | 0.0209** |
| | (0.0118) | (0.0106) | (0.0085) | (0.0087) | (0.0089) |
| Listings rate$^2$ *(in %)* | | | | | 0.0001 |
| | | | | | (0.0001) |
| 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 | 80,910 | 80,907 | 65,904 | 65,904 | 80,687 |
| Bandwidth, $b$ | 2.963 | 2.9629 | 2.9574 | 2.9629 | 2.9521 |
| Kleibergen-Paap $F$-statistic | 90.81 | 97.28 | 48.72 | 117.7 | 81.68 |

*Notes*: We exclude transactions occurring within one year after implementation of the HSO. We estimate the specifications in column (3) by Limited Information Maximum Likelihood, while the specification in column (5) uses a Control Function approach. In column (1) we instrument the listings rate with the HSO dummy, in column (2) we instrument the listings rate with a dummy indicating whether the HSO is implemented in Santa Monica. Column (3) uses three instruments, which are dummies for HSOs implemented in respectively cities bordering the city of LA, South Bay cities and other cities. In column (4) we use solely the interaction term of HSO with the log of picture density as an instrument and column (5) instruments the listings rate with the HSO dummy and an interaction term of HSO with log picture density. Robust standard errors are clustered at the census block level and in parentheses. We use cluster-bootstrapped standard errors (250 replications) in column (5). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

single-best instrument as to avoid potential biases caused by the use of multiple instruments. According to column (2), Table A7, the HSO in Santa Monica is the strongest instrument. When we only use that instrument, we find a high Kleibergen-Paap $F$-statistic of 97 (which is also a bit higher than when using only one HSO dummy, see column (1)). The second-stage effect is similar, although this effect is now less precisely estimated (this is unsurprising as we only use variation in the listings rate caused by the HSO implemented in Santa Monica).

In column (3), Table A10, we reduce the number of instruments by making a distinction between HSOs implemented in cities bordering the city of LA, South Bay cities, and other cities. The coefficient of the listings rate on house prices is similar, albeit slightly lower than the preferred baseline specification. Nevertheless, it is not statistically significantly different from the baseline specification.

Column (4) again uses the strategy to only use the single best instrument. We therefore use solely the best alternative instrument, which is the HSO interacted with the log of tourist picture density. We now have a very strong first stage (with a Kleibergen-Paap $F$-statistic of 118). The second stage coefficient is very similar to the baseline estimate again.

Table A11 – Sensitivity analysis for reduced-form effects

*(Dependent variable: log of house price)*

| | *Property fixed effects* | *Distance to border trends* | *Picture trends* | *Neighborhood characteristics* | *Straight segment trends* |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| HSO implemented | -0.0696*** | -0.0398*** | -0.0271*** | -0.0258*** | -0.0379*** |
| | (0.0237) | (0.0065) | (0.0089) | (0.0081) | (0.0147) |
| Property characteristics | Yes | Yes | Yes | Yes | Yes |
| Distance to border×year trends | 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 | 8,691 | 265,912 | 58,291 | 58,495 | 52,783 |
| Bandwidth, $b$ *(in km)* | 2.2235 | — | 1.8602 | 1.9557 | 1.8293 |
| $R^2$ | 0.9749 | 0.9120 | 0.9102 | 0.9101 | 0.9251 |

*Notes*: We exclude transactions occurring within one year after implementation of the HSO. Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Finally, in the last column, we investigate robustness for the finding that non-linear effects are not of particular importance. We use here the alternative instruments, HSO and HSO interacted with tourist pictures, rather than the city-specific HSO dummies. Using a control function approach, we get essentially the same result as reported in column (5), Table 7.

### A.4.8 Sensitivity analysis

Here, we subject our results to an additional range of robustness checks. We report the reduced-form results for prices in Table A11.

The first column improves on identification by including property fixed effects rather than census block fixed effects. Because we look at a relatively short time 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 6.1%.[41] 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.

[41] 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. Indeed, the long-run effect shown in Figure 8 is closer to the effect obtained here.

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 using a bandwidth we include a second-order polynomial of distance to the nearest HSO border interacted by year, 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 transaction to the log of population density, 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 A11, 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. If anything, it seems that the effect of HSOs is somewhat stronger, but not statistically significantly different from the baseline, given the somewhat higher standard error.

We repeat a similar set of specifications when estimating the 'structural equation' as to evaluate the impact of *Airbnb* listings on house prices. In all specifications we instrument the listings rate with city-specific HSO dummies. The results are reported in Table A12.

Column (1) uses property fixed effects. In line with the reduced form effects, the estimated effect is considerably stronger. The effect is in our opinion unrealistically strong. This may be due to a rather weak first stage (the Kleibergen-Paap $F$-statistic is below 10) and the particular selection of observations that are sold twice in our study period.

Table A12 – Sensitivity analysis for the impact of listings rate 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 <100m* | *Listings rate <500m* |
|---|---|---|---|---|---|---|---|---|
| Listings rate <200m | 0.0966** | 0.0364*** | 0.0201** | 0.0200** | 0.0295** | 0.0276*** | | |
| | (0.0382) | (0.0094) | (0.0082) | (0.0087) | (0.0132) | (0.0096) | | |
| Listings rate <100m | | | | | | | 0.0170** | |
| | | | | | | | (0.0072) | |
| Listings rate <500m | | | | | | | | 0.0228** |
| | | | | | | | | (0.0097) |
| Property characteristics | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Distance to border×year trends | Yes | No | No | No | No | No | No | No |
| Pictures×year trends | No | Yes | No | No | No | No | No | No |
| Neighborhood characteristics | No | No | Yes | Yes | No | No | No | No |
| Straight border segment×year fixed effects | No | No | No | Yes | No | Yes | No | No |
| Census block fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Border segment×month fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 12,087 | 265,908 | 76,778 | 78,319 | 74,593 | 75,835 | 89,001 | 68,096 |
| Bandwidth, $b$ *(in km)* | 3.1318 | — | 2.7533 | 2.9516 | 2.8927 | 2.7197 | 3.4463 | 2.3348 |
| Kleibergen-Paap $F$-statistic | 9.00 | 19.14 | 18.17 | 18.13 | 12.27 | 18.90 | 12.93 | 39.33 |

*Notes*: We exclude transactions occurring within one year after implementation of the HSO. We estimate the specifications by Limited Information Maximum Likelihood. The listings rate is instrumented with city-specific dummies indicating whether an HSO has been implemented. Robust standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In column (2) we also find a somewhat stronger effect: a 1 percentage point increase in the listings rate is associated with a price increase of 3.6%. When we control flexibly for differential price trends between more and less touristy areas in column (3), the coefficient of listings rate is almost the same as the preferred specification. The same holds in column (4) when we control for changes in neighborhood characteristics.

In column (5) we use straight border-segment×year fixed effects. We find a somewhat stronger effect, but the effect is far from being statistically significantly different from the baseline specification.

Column (6) we make sure 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 very comparable.

In the final two columns of Table A12 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 0.0170, which is very similar to the baseline estimate. Moreover, when using the listings rate within 500m the coefficient is essentially identical to the baseline estimate. Hence, our results are insensitive to the area choice.

#### A.4.9 Sensitivity analysis 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 A13, corresponding to the second-stage results reported in Panel B of Table 9. It can easily be seen that HSOs reduce the listings rate by about 1.25-2.00 percentage points, which is comparable in magnitude as reported in Table A7, albeit slightly stronger.

In Table A14 we repeat the DiD analysis, but now we take the median list price in the *Zillow* data as 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 2km in column (4) we find a strong negative impact of HSOs. This is in contrast to the insignificant effect of HSO on rents within 2.5km, 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

Table A13 – DiD results for rents, first-stage results
*(Dependent variable: listings rate)*

| | *All obs.* | *Outside HSO, <10km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, <2.5km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, >5km, <10km* |
|---|---|---|---|---|---|---|
| | (1) OLS | (2) OLS | (3) OLS | (4) OLS | (5) OLS | (6) OLS |
| HSO implemented× | -0.9575*** | -1.1942*** | -0.8325*** | -1.5966*** | -1.3012*** | -0.8174*** |
| Palos Verdes Estates | (0.1399) | (0.1565) | (0.1527) | (0.1953) | (0.2542) | (0.1247) |
| HSO implemented× | -1.0052*** | -1.2423*** | -0.8772*** | -1.6524*** | -0.8290*** | -0.8343*** |
| Rancho Palos Verdes | (0.1425) | (0.1586) | (0.1539) | (0.1970) | (0.0589) | (0.1369) |
| HSO implemented× | -0.4625*** | -0.6995*** | -0.3336** | -1.1116*** | -0.7865*** | -0.2237** |
| Redondo Beach | (0.1558) | (0.1705) | (0.1659) | (0.2070) | (0.2478) | (0.0947) |
| HSO implemented× | -1.2403*** | -1.4427*** | -1.1250*** | -1.7893*** | -1.5977*** | -1.0691*** |
| Santa Monica | (0.1351) | (0.1446) | (0.1318) | (0.1835) | (0.1776) | (0.0942) |
| HSO implemented× | -0.8370*** | -1.0718*** | -0.7080*** | -1.4798*** | -0.8359*** | -0.5082*** |
| Torrance | (0.1455) | (0.1601) | (0.1537) | (0.1977) | (0.1475) | (0.0722) |
| HSO implemented× | -0.4451*** | -0.6506*** | -0.3613** | -0.9299*** | 0.0445 | 0.1740 |
| Other cities | (0.1405) | (0.1394) | (0.1703) | (0.1694) | (0.1731) | (0.3886) |
| 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 | 3,313 | 2,564 | 1,805 | 1,396 | 1,805 | 1,266 |
| $R^2$ | 0.9622 | 0.9667 | 0.9499 | 0.9772 | 0.9714 | 0.9792 |

*Notes*: In all specifications we include observations *inside* HSO areas. We exclude observations occurring within one year after implementation of the HSO. Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 (5). Nevertheless, the point estimates are similar to the baseline results reported in Table 7, in particular once we control for distance to the CBD-trends and distance to the beach-trends (columns (5) and (6)).

### *A.5 Theoretical welfare considerations*

What are the effects of implementing an HSO on welfare? Let us assume that welfare effects are captured by changes in house prices and rents, in line with a large urban economics literature (see *e.g.* Brueckner 1990).[42] To be able to comment on the quantitative welfare effects of the HSO, we make the following assumptions. *(i)* All agents have a discount rate of 2% and *(ii)* the same

[42] The latter assumption can be derived from more primitive assumptions, including that markets are competitive, except for the housing market where homeowners own the land and have market power.

Table A14 – DiD results for prices, Zillow data
*(Dependent variable: log median list price)*

| | *All obs.* | *Outside HSO, <10km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, <2.5km* | *Outside HSO, >2.5km, <10km* | *Outside HSO, >5km, <10km* |
|---|---|---|---|---|---|---|
| *Panel A:* Effects of HSOs | (1) OLS | (2) OLS | (3) OLS | (4) OLS | (5) OLS | (6) OLS |
| HSO implemented | -0.0404** | -0.0460** | -0.0310* | -0.0516** | -0.0406** | -0.0386 |
| | (0.0189) | (0.0180) | (0.0182) | (0.0250) | (0.0199) | (0.0258) |
| 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 | 3,257 | 2,508 | 1,749 | 1,340 | 1,749 | 1,210 |
| $R^2$ | 0.9937 | 0.9875 | 0.9875 | 0.9856 | 0.9892 | 0.9900 |
| *Panel B:* Effects of listings | (1) 2SLS | (2) 2SLS | (3) 2SLS | (4) 2SLS | (5) 2SLS | (6) 2SLS |
| Listings rate | 0.0511 | 0.0447* | 0.0436 | 0.0351* | 0.0289 | 0.0332 |
| | (0.0340) | (0.0228) | (0.0399) | (0.0197) | (0.0242) | (0.0354) |
| 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 | 3,257 | 2,508 | 1,749 | 1,340 | 1,749 | 1,210 |
| Kleibergen-Paap $F$-statistic | 78.74 | 72.84 | 91.04 | 49.69 | 210.2 | 94.61 |

*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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

marginal utility of income. *(iii)* Welfare effects of tax avoidance are assumed to be absent.[43] *(iv)* The demand functions for housing are linear and the total housing stock is given.[44] *(v)* We focus on locations far from the HSO border. *(vi)* Home-ownership and renting are perfect substitutes. *(vii)*. We consider the case where outside investors buy properties to list on *Airbnb* and assume that the investors' WTP slightly exceeds the highest WTP of incumbent households.[45]

In Figure A6 we show the demand effects of the HSO. We have drawn housing supply $H^*$ and the equilibrium quantity of STR listings. In the initial situation, in the absence of regulation, the equilibrium quantity of STRs is given by $H_{\text{STR}}$ and the units used for residential housing is

[43]Because tax avoidance of suppliers of *Airbnb* is likely present by the HSO, our decreases in welfare because of efficiency losses are likely overestimates, but this does not have any consequence for the distributional implications.

[44]This assumption is not essential, as non-linear demand functions implied by our log-linear hedonic price functions, provide almost identical welfare results.

[45]Note that when the investors' WTP does not exceed the highest WTP of incumbent households, then our welfare estimates are overestimates. We believe, however, that the assumption is defensible. Note further that because the proportion of STRs is small compared to the other houses, about 2.5% in our data, the slope of the investors' demand function is essentially irrelevant.

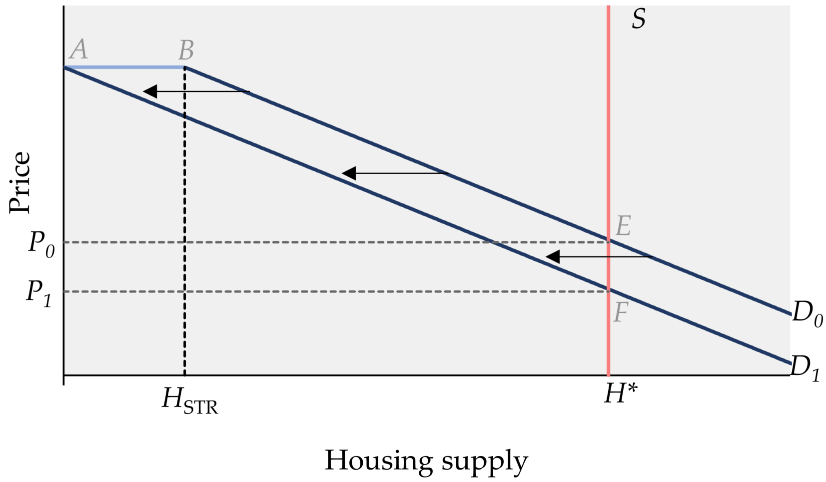


Figure A6 – Demand effects of HSOs

$H^* - H_{\text{STR}}$. In Figure A6, the HSO implies that $H_{\text{STR}} = 0$, which means a reduction in the total surplus equal to $A$-$B$-$E$-$F$. Per property, the reduction in surplus is $E$-$F$.

## *A.6 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 A15 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 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 a set of other demographic controls. There seems to be a proportional increase of 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.

Table A15 – 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.