

In Search of Peace and Quiet: The Heterogeneous Impacts of Short-Term Rentals on Housing Prices

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Abstract

The supply of housing for short-term rental (STR) has grown dramatically with the emergence of platforms such as Airbnb. This trend has led to contradictory concerns about increasing housing prices and negative externalities. We provide evidence that in some areas, STRs can decrease housing prices. Using a parsimonious model of housing occupancy with externalities, we show that the marginal effect of STRs on housing prices depends on the net impact of STRs on local amenities. Using postal code level data from Los Angeles County, California, we show heterogeneity in the marginal effects of Airbnb listings on housing prices across localities. We then examine the consequences of a 2015 law restricting STRs within the City of Santa Monica in the coastal region of Los Angeles County. In that city, we estimate a negative relationship between the prevalence of STRs and housing prices. Using a synthetic control approach, we provide evidence that the 2015 law may have increased housing prices—and likely did not decrease housing prices—which can be rationalized by our theory. Finally, we provide evidence for a potential mechanism: public intoxication calls to the Santa Monica Police Department decreased after the policy was enacted.

JEL codes: R31, R5, L5, H7, Z38, K42

Keywords: Housing markets, peer-to-peer markets, tourism policy, nuisance crimes, local government

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1 Introduction

“The Ordinance was passed to ensure that residential rental housing remains available to long-term tenants, and because short-term rentals have undesirable impacts that threaten the stability and character of the City’s neighborhoods and result in increased rents.”

David [Martin \(2018\)](#), Santa Monica, California
Director of Planning and Community Development

Driven by the emergence of online platforms such as Airbnb and Vacation Rentals by Owner (VRBO), short-term rentals (STRs) in the housing market have experienced significant global growth over the past decade.¹ By reducing information costs, these markets enable mutually beneficial transactions between property owners and transient visitors, and thus increase the utilization of (and economic surplus created by) housing capacity. Given that they increase certain neighborhood amenities ([Basuroy et al., 2020](#)) and the option value of real property ownership, STRs have the potential to increase housing prices ([Horn and Merante, 2017](#); [Garcia-López et al., 2020](#)). Criticism of STR platforms has focused mostly on this price effect: higher housing prices means that long-term renters may be increasingly priced out of communities where they have lived for years ([Nieuwland and van Melik, 2020](#)).² As a consequence, there is an active policy debate surrounding STR regulation with an emphasis on restricting property rights on both the extensive (i.e. whether STRs are allowed in a given area) and intensive (i.e. what rules STR hosts must follow) margins.

Our contribution to this debate is an assessment of a simple point implied by the quote from a policymaker featured above: the net effect of STRs on housing prices is ambiguous due to the relationship between STRs and local amenities.³ STRs represent an extension of the capital stock available to the hospitality industry ([Zervas et al., 2017](#); [Farronato and Fradkin, 2018](#)). As traditional hospitality firms create both positive demand spillovers for

¹An STR is typically defined as the rental of a fully furnished housing unit for a period ranging between one night and several months. In contrast, long-term rentals generally involve leases with a term of at least one year.

²Furthermore, given that homes account for roughly a quarter of aggregate household net wealth, movements in housing prices can have first-order consequences for household balance sheets ([Stupak, 2019](#)).

³Throughout the paper, *amenities* are defined as “location-specific consumption goods.”

other service industries and local negative externalities such as public intoxication, petty theft, and other “nuisances” (Brunt and Hambly, 1999; Ho et al., 2009), STRs may generate similar negative externalities. Airbnb, in particular, has received negative media attention for its “party house” listings (Lieber, 2015; Coles et al., 2017).⁴ Indeed, Fontana (2021) finds that increases in Airbnb penetration in London leads to increases in complaints against tourists and a decrease in neighborhood quality. If the cost of such externalities outweighs the benefits from increased demand for local businesses—in other words, if STRs sufficiently “threaten the stability and character” of neighborhoods (Martin, 2018) in a way that is visible and relevant to potential residents (Tiebout, 1956)—the net effect of STRs on the overall level of local amenities, and potentially on housing prices, may be negative and a policymaker seeking to improve efficiency may rightfully seek to restrict STRs (Coase, 1960).⁵ In this paper, we demonstrate this both theoretically and empirically.

We formalize this idea in Section 3 with a partial equilibrium model of the housing market that builds on the work of Barron et al. (2020). In our model, home-owners choose between occupying their home themselves and listing it as a short-term rental. Having this option increases the value of owning housing. Our contribution is to consider the idea that STRs may also impose both positive and negative externalities on their neighbors. In equilibrium, an increase in the STR rental rate may *reduce* housing prices if the net effect of STRs on amenities for owner-occupiers in the neighborhood is sufficiently negative to outweigh the effect of the increased surplus earned by absentee landlords. Similarly, an exogenous change in the number of STRs in a given neighborhood may result in an increase or a decrease in housing prices, depending on the net impact of STRs on aggregate amenities and disamenities in that neighborhood.

To provide empirical evidence for the implications of our model, we turn to Los Angeles

⁴By offering “owner-absent” rentals of detached homes, STRs may host activities with negative externalities that would likely be deterred by the presence of hotel staff. Furthermore, as STRs are generally located in quieter, traditionally owner-occupied residential areas, the same activities may generate greater social costs when they take place in an STR as opposed to a hotel.

⁵Indeed, municipalities generally restrict the location of traditional hospitality firms in part to maintain the right to “peace and quiet” in areas with single-family detached homes.

(LA) County, California. This area has one of the highest levels of amenities in the United States (Albouy, 2016) but also features a high degree of income and amenity inequality across its various communities (Bobo et al., 2000; Wolch et al., 2005; Charles, 2006). As we describe in Section 4, we employ data on housing prices from Zillow, data on Airbnb participation for individual dwelling units from web scrapes, and crime data from local governments. We begin our analysis in Section 5 by considering the relationship between Airbnb listings and housing prices at the postal code level. As our model makes clear, the number of Airbnb listings is endogenous so we employ an instrument based on the level of local amenities in each postal code (ZIP Code) prior to the entry of Airbnb into the regional housing market. We first estimate the relationship for Los Angeles County in the aggregate and find a positive effect mirroring other results in the literature (Barron et al., 2020). We then estimate the relationship for each city in LA County and find significant heterogeneity. For example, in the City of Los Angeles, a 1% increase in the number of Airbnb listings in a postal code is estimated to increase housing prices by 0.25%, but in Burbank, a higher-income, lower-density community, the same increase in listings is estimated to *decrease* housing prices by 0.06%.

These results suggest that regulations restricting STRs may lead to increased housing prices. In Section 6, we examine the effects of an STR restriction enacted by the wealthy oceanfront suburb of Santa Monica—a jurisdiction for which we estimate a negative relationship between STRs and housing prices. The 2015 law, enacted in part due to concerns about increasing housing prices in the city, was arguably the strictest regulation on STR activity in effect in the United States at the time (Sanders, 2015). It is important to note that we are not claiming this policy change is exogenous to housing prices—per the contemporaneous press, it was implemented in response to a simultaneous increase in STR listings and housing prices (Logan, 2015). We first document that the law was (at least temporarily) successful in reducing the number of Airbnb listings that would be most likely to generate negative externalities. Using a synthetic control framework with the rest of California serving as

potential control units, we show that these regulations likely did not decrease housing prices and may have increased housing prices by approximately 10% in the point estimate. Finally, we provide suggestive evidence for our externality mechanism by examining detailed call data from the Santa Monica Police Department. Using a difference-in-differences framework with linear trends to account for delays in enforcement, we show that public intoxication calls decreased relative to other types of calls.⁶

Our work contributes to the recent literature examining the relationship between STR markets and housing prices. Relative to this literature, which we broadly characterize as providing average effects of STRs, our work focuses on heterogeneity in the effects of STRs. [Koster et al. \(2021\)](#) study the effects of STR regulations (which they call home-sharing ordinances, or HSOs) throughout LA County using a regression-discontinuity design around the cities' borders and find that, on average across LA County, STR restrictions decreased housing prices by 2%, which is similar to our finding that, on average across the county, additional STR listings increase housing prices. Similarly, [Almagro and Dominguez-Iino \(2019\)](#) use data from Amsterdam from 2008 to 2019 and estimate that a 10% increase in commercially operated Airbnb listings leads to a 0.393% increase in house prices. [Kim et al. \(2017\)](#) study a minimum length-of-stay requirement imposed on STRs on a small island community in Florida in 2007. They show with an OLS approach that nonresident ownership of properties on the island decreased in response to the regulation and that property values generally decreased (with potential increases in areas with a high proportion of non-resident owners), consistent with the hypothesis that the net impact of STRs on local amenities is positive. In contrast, we find evidence of a potential increase in housing prices post-restriction, and are able to link this change more directly to negative externalities associated with transient visitors. Our work extends [Barron et al. \(2020\)](#) who introduce the instrument described above and use it to estimate the average effect of Airbnb listings on housing prices

⁶In a conference paper, [Han and Wang \(2019\)](#) study the relationship between STRs and the crime rate in New York City and San Francisco using policy changes that primarily affected commercial listings and find qualitatively similar results.

across the U.S.—their result is similar to our estimate for LA County as a whole. In contrast, we extend their model and focus our empirical work on a particular geography to demonstrate significant heterogeneity. In a recent working paper, [Filippas and Horton \(2020\)](#) develop a model of home-sharing through subletting, focused on tenants and landlords with negative externalities, and using New York City (NYC) as an empirical setting.

[Calder-Wang \(2019\)](#) estimates the distributional impacts from Airbnb in NYC and finds the losses to renters (via increased prices) exceed the gains to the hosts (via an increased option value of housing). Relative to this work, we allow for the possibility that STRs generate both positive and negative externalities and explore the impact of an STR regulation empirically. [Valentin \(2021\)](#) examines the impact of STR regulation in New Orleans and finds that regulating and restricting STRs is associated with a decline in property values. In another working paper, [Fonseca \(2019\)](#) analyzes the immediate impact of Santa Monica’s law on long-term rental prices using a synthetic control approach, and finds little effect. In addition to our different focus on the effects of STRs on local amenities, we use data on home-ownership prices over a longer period to identify the effects of the law.

We also contribute to the growing literature examining the relationship between STRs and other hospitality firms, such as hotels and motels. By arguing that Airbnb listings are associated with negative externalities similar to those created by hotels, our work is complementary to other studies cited above which identify STRs as substitutes for traditional hotels. More broadly, our work contributes to the literature examining the relationship between various local externalities and housing prices (see e.g. [Nelson, 1978](#); [Bartik and Smith, 1987](#); [Glaeser et al., 2001](#)) as well as the effects of local policies on housing prices (see e.g. [Friedman and Stigler, 1946](#); [Pollakowski and Wachter, 1990](#); [Glaeser and Luttmer, 2003](#); [Glaeser et al., 2005](#); [Diamond et al., 2019](#)).

Our work further contributes to a broader literature examining the externalities of peer-to-peer markets. Within the transport sector, the rapid expansion of ridesharing apps such as Uber has led to increases in net restaurant creation, by providing restaurant patrons with

easier access to previously inaccessible locations (Gorback, 2020). However, there have been added social costs associated with the growth of ridesharing, such as increases in the number of motor vehicle fatalities as well as increases in congestion and road use (Barrios et al., 2020). Understanding the net impact of peer-to-peer markets is essential in determining whether and how to regulate these nascent industries. We conclude in Section 8 with a discussion of these externalities and suggestions for both policymakers and future researchers.

2 Background

Our analyses operate at the intersection of private housing and hospitality markets, and explore the long-standing heterogeneity between the various communities of Los Angeles County. In this section, we briefly describe this heterogeneity, provide a short history of STRs and Airbnb, and discuss the specific Santa Monica legislation ordinance restricting STRs.

2.1 Los Angeles County

Los Angeles County, with a population of more than 10 million, is the single most populous county in the United States. The county is divided into 88 incorporated cities and 76 unincorporated areas with significant heterogeneity that has been persistent through time (Bobo et al., 2000). Table 1 illustrates some of this heterogeneity in terms of median income, local characteristics, and median rents for LA County overall and for the City of LA and nine other cities within the county. Figure 1 maps these cities within LA County. The median income in the City of LA is slightly lower than the county as a whole. Malibu, a western beachside community, has a population density of only 353 residents per square mile, whereas the more centrally located West Hollywood has 13,359 residents per square mile. Other amenities such as public parks and restaurants vary widely as well—while the City of LA features 9.9 park acres per thousand residents, Pasadena offers only 2.5 park acres

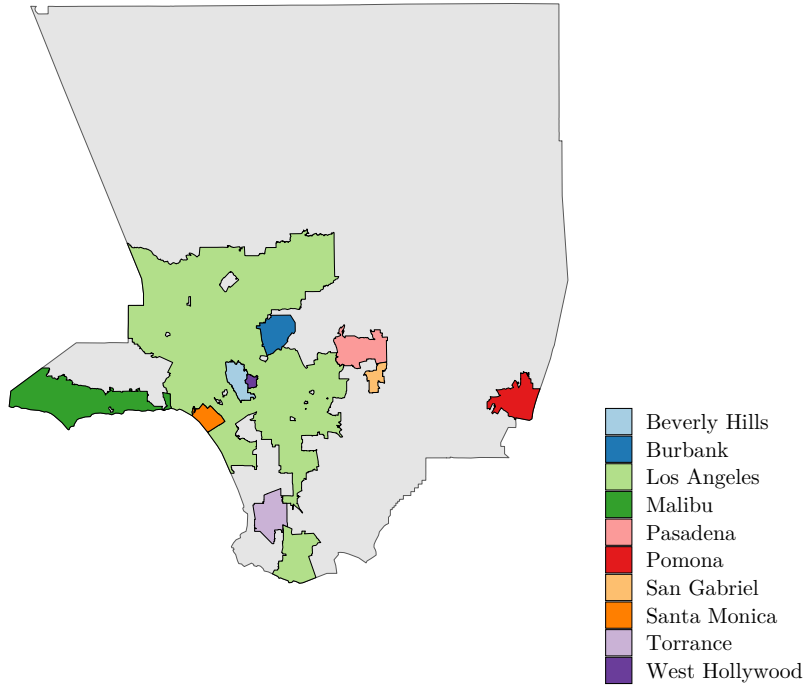
per thousand residents. Santa Monica offers 4.7 restaurants per thousand residents, while Pomona, on the far eastern border of the county, offers merely 1.3. These differences in observable amenities are likely related to the differences in average rental rates for two-bedroom apartments, which in 2016 ranged from \$1,187 in Pomona to over \$2,500 in Malibu.

Table 1: Characteristics for Selected Cities in Los Angeles County

	Income (2016 \$)	Density (res. / mi ²)	Parks (ac. / 1k res.)	Dining & lodg. (# / 1k res.)	Rent (\$ / mth)
LA (county)	62,978	13,090	3.3	1.9	1,410
LA (city)	58,504	15,637	9.9	2.0	1,473
Beverly Hills	128,985	8,164	1.9	4.7	2,339
Burbank	71,249	8,740	1.1	2.9	1,678
Malibu	125,623	353	9.1	4.0	2,529
Pasadena	79,314	8,549	2.5	3.0	1,604
Pomona	54,328	7,329	1.5	1.3	1,187
San Gabriel	63,644	9,497	0.5	3.1	1,314
Santa Monica	91,098	11,893	1.4	4.7	1,879
Torrance	80,097	9,327	2.4	2.7	1,606
West Hollywood	98,362	13,359	0.6	8.0	2,165

Notes: ‘Income’ is the 2016 median household income. ‘Population’ is the number of residents per square mile in 2010. ‘Parks’ is the number of acres of city parks per 1,000 residents in 2016. ‘Dining & lodg.’ is the number of establishments in NAICS category 72 per 1,000 residents in 2010. ‘Rent’ is the median gross rent in 2016 for renter-occupied housing with two bedrooms. All statistics from the Census Bureau except for parks, which is from the 2016 Los Angeles Countywide Comprehensive Park and Recreation Needs Assessment.

Figure 1: Select cities of LA County



This figure depicts selected city boundaries in LA County. The highlighted cities are those displayed in Table 2. Santa Catalina and San Clemente Islands are omitted.

2.2 Home sharing and the rise of Airbnb

Today’s STR market evolved from the practice of “home-sharing” which gained popularity in the U.S. in the 1950s as vacation rentals—in which visitors have private and exclusive (i.e., without the presence of a long-term resident) use of a housing unit for some period—became a viable alternative to hotels. Launched in 1995, Vacation Rentals by Owner (VRBO) provided the first online peer-to-peer platform for vacation or STR bookings; Booking.com entered a year later. Airbnb entered in 2008 and expanded the STR market by providing hosts a platform through which they could offer single rooms in their occupied homes, which gave travelers more options for lodging in residential neighborhoods. While these “owner-present” STRs differ substantially from traditional owner-absent vacation rental offerings (or hotel offerings), they quickly grew in popularity as a cheaper alternative.

2.3 Regulating STRs

Neither California nor the U.S. federal government explicitly regulates STRs—though we discuss the interaction of federal communications laws and local regulations below. Instead, STRs are regulated through local ordinances. In Section 6, we focus on Santa Monica’s Ordinance 2484CCS, which was adopted by its City Council on May 12, 2015. According to staff reports and the text of the measure, the city was roused to action because STRs removed “needed permanent housing from the market” and transient visitors could “disrupt the quietude ... of the neighborhoods and adversely impact the community” ([City of Santa Monica, 2019](#)). The measure nominally banned owner-absent STRs, while allowing owner-present STRs to continue with additional licensing, reporting, and taxation requirements.

The ordinance was debated in the months before passage and was an outgrowth of a longer-term process by the City Council to update Santa Monica’s land-use and transportation plan that began in December 2013 and continued for several months beyond the adoption of the short-term rental regulation ([Martin, 2015](#)). This “spin-off” ordinance was not unique—the City Council of Santa Monica passed other spin-off ordinances as a result of this process, including ordinances related to discrimination in long-term rental housing, water conservation policy, and commercial fitness instruction. None of these ordinances were directly related to land-use, building codes, or other regulations with first-order impacts on residential housing prices or neighborhood amenities. Furthermore, none of the ordinances passed during this period were directly related to establishments offering on-premise alcohol consumption or public intoxication, which we investigate in Section 7. The major components of the ongoing planning effort primarily concerned requirements for commercial buildings and multi-family dwellings, and thus it is unlikely that this process alone would have created significant changes in housing prices until its provisions went into effect and future construction projects were designed in compliance with those provisions. If anything, the land-use plan encouraged the construction of additional multi-family dwellings by rezoning certain areas in the city and easing restrictions on accessory dwelling units, which would

decrease housing prices compared to a counterfactual with no change in land-use policy, even if such changes were anticipated.⁷

When the ordinance went into effect in June of 2015, Airbnb (among other platforms) quickly launched a legal challenge which made enforcement difficult (Dolan, 2019). The city had no ability to prevent landlords from honoring reservations made before the ordinance went into effect. Landlords worked to circumvent the provisions of the ordinance, finding ways to offer owner-absent STRs that complied with the rules. These circumventions included modifications to listings that likely decreased the probability that renters would generate externalities. For example, the owner may be present for a short period at the beginning and end of the rental period, or be occupying an adjacent dwelling. These actions resulted in several additional legal challenges.⁸ Ultimately, the city prevailed in all cases.

Santa Monica’s difficulty in enforcing their STR restriction is particularly relevant to our analyses given the results of Fontana (2021), who, using data from London, finds that “negative externalities can be explained by a lack of monitoring and co-ordination by [Airbnb] hosts”. If these legal challenges allowed absentee landlords to maintain STR listings without modification for a period, or at least to satisfy bookings made prior to the passage of the ordinance, any negative externalities generated by those listings are likely to be unaffected as well. Therefore our prior is that changes to the number (or composition) of listings and subsequent changes in negative externalities and house prices may be delayed or gradual. We return to these issues when we analyze police calls in Section 7.

3 A model of housing with type-of-use externalities

To understand the potentially heterogeneous effects of STRs on housing prices and provide stylized intuition for our empirical work, we introduce a partial equilibrium model of

⁷The conclusions in this paragraph are drawn from our review of the text of the ordinance and contemporaneous measures as well as meeting minutes and archived video of Santa Monica City Council meetings. We are unable to find any evidence to contradict these assertions.

⁸See *Diane Hayek v. City of Santa Monica*, Los Angeles Superior Court No. 17STLC02007 (May 30, 2018); *Diane Hayek v. City of Santa Monica*, Los Angeles Superior Court No. BS170950 (August 19, 2019).

housing choice that extends the work of [Barron et al. \(2020\)](#) by adding type-of-use externalities. Our goal is not to generate “sharp” falsifiable predictions—rather in the context of a literature which has both a) estimated that STRs generate increases in average housing prices ([Barron et al., 2020](#)) and b) documented significant negative externalities in particular local jurisdictions ([Fontana, 2021](#)), we show that the effect of STRs on equilibrium jurisdiction-level housing prices is ambiguous even in a parsimonious framework. Thus we abstract from several considerations—including new housing unit construction, long-term rental arrangements, and the ownership of multiple dwelling units—which do not affect the primary mechanism: the interplay between externalities and the option value (to home-owners) of offering STRs. In other words, we seek to show that one does not need to take very much more into account (relative to existing analyses) in order to obtain different results which may be of direct policy relevance.

The model environment consists of a finite number of jurisdictions J , indexed by j . Each jurisdiction offers a fixed quantity of housing H_j . The total number of agents in the housing market is exogenous and given by N . Each agent i jointly decides in which jurisdiction to purchase a home and their usage of that home, i.e., whether to be an owner-occupier or an absentee landlord. We normalize to zero the utility of not entering the market.⁹

The utility derived by individual i from owning and occupying housing in jurisdiction j is

$$u_{i,j,o} = \xi_j(k_j, f(str_j), g(str_j)) - P_j + \epsilon_{i,j,o}.$$

In this equation, $\xi_j : \mathbb{R}^3 \rightarrow \mathbb{R}$ is a function that maps three jurisdiction-specific features into a scalar amenity value. The feature k_j is a fixed, time-invariant amenity level that is unrelated to short-term rentals. $f(str_j)$ is an increasing function that maps the level of STRs in jurisdiction j to the level of *positive* amenities associated with STRs (such as extra

⁹For simplicity, we do not explicitly model long-term rental arrangements. The mechanism described here operates similarly in a model with long-term renters as long as those renters are affected similarly to owner-occupiers by location-specific amenities.

restaurants), and $g(str_j)$ is an increasing function that maps the level of STRs to the level of *negative* amenities associated with STRs (such as crime). P_j is the price of housing in jurisdiction j and $\epsilon_{i,j,o}$ is an idiosyncratic preference shock.

With a slight abuse of notation, we assume that $\frac{\partial \xi_j}{\partial f}$ is positive for all k , g and f and that $\frac{\partial \xi_j}{\partial g}$ is negative for all k , g , and f . We make no explicit assumption about second derivatives—though we note that, in general, one might expect increases in k to affect $\frac{\partial \xi_j}{\partial f}$ and $\frac{\partial \xi_j}{\partial g}$ differently, i.e. negative externalities may be “worse” if the jurisdiction is “nicer.” As a consequence, the effect of an increase in STRs on jurisdictional amenity values is ambiguous, as can be seen through the partial derivative

$$\frac{\partial \xi_j}{\partial str_j} = \underbrace{\frac{\partial \xi_j}{\partial f}}_{+} * \underbrace{f'(str_j)}_{+} + \underbrace{\frac{\partial \xi_j}{\partial g}}_{-} * \underbrace{g'(str_j)}_{+}. \quad (1)$$

Jurisdictions with different levels of amenities (and therefore different levels of f' and g') and/or different levels of k_j (and therefore different levels of $\frac{\partial \xi_j}{\partial f}$ and $\frac{\partial \xi_j}{\partial g}$) may therefore confer either greater or lesser utility when the level of STRs increases.

The utility an agent receives from being an absentee landlord is given by

$$u_{i,j,a} = \frac{R_j}{1 - \delta} - P_j + \epsilon_{i,j,a}$$

where R_j is the sum of net STR revenues in jurisdiction j net of any rental expenses and δ is the common discount rate.¹⁰ We assume for simplicity R_j is exogenous.¹¹

Also for simplicity, assume $\epsilon_{i,j,k}$ (where $k \in \{o, a\}$) is i.i.d. and follows a Type-I extreme value distribution. This implies that the probability (or choice share) $s_{j,k}$ of an individual

¹⁰We assume for simplicity there is no uncertainty in future per-period net revenues.

¹¹As STRs compete with other hospitality firms having large numbers of units, the effect of a small change in the number of STR units available in a particular jurisdiction on R_j is likely to be second-order.

choosing jurisdiction j and usage-type k is given by the familiar logit form

$$s_{j,k} = \frac{\exp(\bar{u}_{j,k})}{1 + \sum_{j'} \sum_{k'} \exp(\bar{u}_{j',k'})}.$$

where $\bar{u}_{j,k} = u_{i,j,k} - \epsilon_{i,j,k}$. In equilibrium, markets must clear: $\sum_{k \in \{o,a\}} s_{j,k} N = H_j$ for all j . Using this market-clearing condition, we can write the equilibrium price for houses in location j (see Appendix A for details):

$$P_j^* = -\log \left(\frac{(1 + \phi_{j'}) H_j}{(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j(k_j, f(str_j^*), g(str_j^*))))(N - H_j)} \right), \quad (2)$$

where $\phi_{j'}^* = \sum_{j' \neq j} \left(\exp(-P_{j'} + \frac{R_{j'}}{1-\delta}) + \exp(\xi_j(k'_j, f(str_{j'}^*), g(str_{j'}^*))) \right)$ and $str_j^* = s_{j,a} \times H_j$ is the equilibrium number of short-term rentals in jurisdiction j . We use the equilibrium housing price expression to derive an expression that guides the interaction between short-term rentals and housing prices. Our key insight comes from the relationship between the equilibrium price, the STR rental rate, and the number of STRs: $\frac{\partial P_j^*}{\partial R_j}$ and $\frac{\partial P_j^*}{\partial str_j^*}$:

$$\frac{\partial P_j^*}{\partial R_j} \approx \frac{1}{\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j(\cdot))} \left(\frac{\exp(\frac{R_j}{1-\delta})}{1-\delta} + \exp(\xi_j(\cdot)) \times \frac{\partial \xi_j}{\partial str^*} \times \frac{\partial str_j^*}{\partial R_j} \right) \quad (3)$$

$$\frac{\partial P_j^*}{\partial str_j^*} \approx \frac{\exp(\xi_j(\cdot))}{(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j(\cdot)))} \times \frac{\partial \xi_j}{\partial str_j^*} \quad (4)$$

Equation (3) approximates the effect of a change in net discounted STR rental revenues on equilibrium home prices. Equation (4) approximates the direct impact of STR listings on equilibrium housing prices. In both of these expressions, we abstract from the second-order effects of changes in model primitives on $\phi_{j'}^*$.¹² Note that $\frac{\partial str_j^*}{\partial R_j} > 0$ —the number of STRs always increases as the present value of STR revenue increases. However, the sign of $\frac{\partial P_j^*}{\partial R_j}$

¹²As neighborhoods are substitutes, there are indirect equilibrium effects of moving R_j on other neighborhoods because prices are simultaneously determined. These indirect effects may go in either direction. We thank an anonymous referee for clarifying this point.

and $\frac{\partial P_j^*}{\partial str_j^*}$ may vary. $\frac{\partial P_j^*}{\partial str_j^*}$ varies with the sign of $\frac{\partial \xi_j}{\partial str_j^*}$ —the net effect of STRs on residential amenities. For $\frac{\partial P_j^*}{\partial R_j}$, there are three non-trivial cases to consider. For clarity of exposition, consider the effect of an increase in R_j on the equilibrium price P_j^* .

Case 1. $\frac{\partial \xi_j}{\partial str_j^*} > 0$: *STRs create net-positive amenities.* In this case, as the number of STRs increases, the value of owner-occupied homes will increase, increasing demand for owner-occupation. Thus, equilibrium housing prices increase: $\frac{\partial P_j^*}{\partial R_j} > 0$.

Case 2. $\frac{\partial \xi_j}{\partial str_j^*} < 0$ and $\frac{\exp(\frac{R_j}{1-\delta})}{1-\delta} > \left| \exp(\xi_j(\cdot)) \times \frac{\partial \xi_j}{\partial str_j^*} \times \frac{\partial str_j^*}{\partial R_j} \right|$: *STRs create net-negative amenities and the magnitude of the change in the marginal benefit to absentee landlords exceeds the magnitude of the change in marginal benefit to owner-occupiers.* In this case, increasing R_j decreases amenities, but that decrease is not fully offset by the increased value realized by absentee-landlords and thus an increase in R_j leads to a net increase in the demand for houses and the equilibrium housing price increases: $\frac{\partial P_j^*}{\partial R_j} > 0$.

Case 3. $\frac{\partial \xi_j}{\partial str_j^*} < 0$ and $\frac{\exp(\frac{R_j}{1-\delta})}{1-\delta} < \left| \exp(\xi_j(\cdot)) \times \frac{\partial \xi_j}{\partial str_j^*} \times \frac{\partial str_j^*}{\partial R_j} \right|$: *STRs create net-negative amenities and the decrease in the marginal benefit to owner-occupiers exceeds the increase in the marginal benefit to absentee landlords.* Here, while the increase in R_j increases demand by absentee landlords, the increased number of STRs decreases demand by owner-occupiers by a greater amount. Thus, the net effect on demand for housing is negative and $\frac{\partial P_j^*}{\partial R_j} < 0$.

Equations (3) and (4) can be used to frame the expected impacts of STR regulations. Some STR regulations (e.g., occupancy taxes or other hospitality business taxes) may be viewed as a change to the present value of being an absentee landlord. Other regulations however may be direct shocks to the number of Airbnb listings allowed—e.g, limits on individual choices to be absentee landlords—resulting in a direct change in str_j^* .

4 Data

To explore the main implication of our model—that the relationship between STRs and housing prices is ambiguous—we collect data on housing prices, Airbnb listings, and crime reports from LA County. We choose this geography in part due to the long-term presence of Airbnb in the area. Any short-term extensive-margin competitive effects of entry are thus likely to have occurred before the period of our analyses. LA County also offers a distinct policy change in Santa Monica.

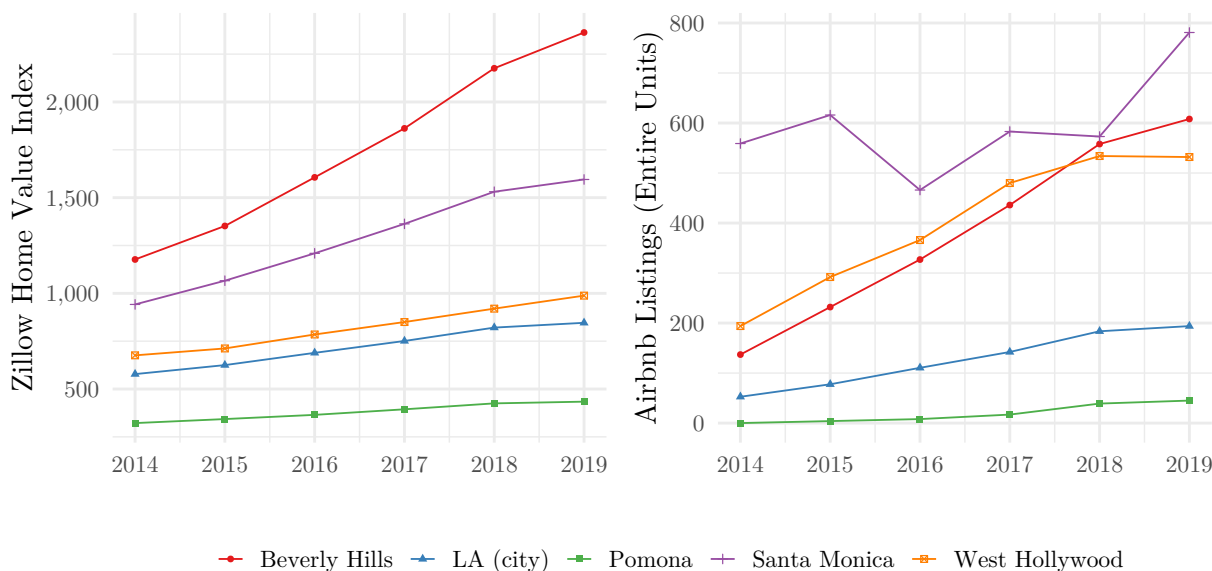
Our data on housing prices is the Zillow Home Value Index (ZHVI), which we obtain from [Zillow.com](https://www.zillow.com) for the interval 1996–2019. The ZHVI reports the estimated median home value at the postal-code-month level, adjusted for seasonality. To construct the ZHVI, Zillow estimates the sale price for all homes in each postal code based on recent sales in that area (Bruce, 2014). The ZHVI has previously been used to study a variety of issues in housing markets, including land-use regulations (Huang and Tang, 2012), strategic responses to mortgage modification programs (Mayer et al., 2014), and credit market shocks (Greenstone et al., 2020).

We obtain Airbnb listing data from [insideairbnb.com](https://www.insideairbnb.com) and tomslee.net, where each of these sources provides snapshots of consumer-facing listings available on specific days, collected by web-scraping. For each listing, we observe a unique host and room identifier, the jurisdiction for the short-term rental unit, the daily price, and the (mutually exclusive) room type: “Entire unit,” “private room,” and “shared room.” We aggregate these listings to the postal code level and construct measures of STR supply based on the total number of listings of each type. These data were scraped at irregular intervals (see Appendix Section B for more details). We obtain all scrapes from 2014 (the earliest available) to 2019. These data have previously been used to understand the relationship between Airbnb listings and housing prices in other geographies (Garcia-López et al., 2020). Finally, to construct the instrumental variable described in Section 5, we obtain Google Trends data for “Airbnb,” and also collect the number of food and accommodation establishments (NAICS 72) in each

postal code in 2010 from the ZIP Code Business Patterns data released by the U.S. Census. We discuss the details of our data cleaning procedure in Appendix Section B.

Figure 2 illustrates our data for the City of Los Angeles as well as four of the cities displayed in Table 1.¹³ Each city experienced increases in housing prices, though the degree of increase varies substantially across locations. Santa Monica is the only city in our data to experience a year-over-year decline in Airbnb listings: entire unit listings (which are generally owner-absent) decreased from 616 in 2015 to 466 in 2016 as the Santa Monica STR regulation was passed, went into effect, and was increasingly enforced. However, as STR suppliers in Santa Monica adjusted to the new regulations, the number of listings slowly increased—by 2019, the number of “entire unit” listings exceeded the pre-regulation count.

Figure 2: Home values and Airbnb listings for selected cities in LA County



Notes: Points are averages across monthly observations during each year. The Zillow Home Value Index is measured in thousands of dollars and reflects the median home value in the given jurisdiction in each year as estimated by Zillow. We divide the number of Airbnb listings in the City of Los Angeles by 100 for scaling.

¹³A fuller set of summary statistics is available in Appendix B.

5 Evidence of heterogeneous impacts of STRs on housing prices

Equation (4) suggests a testable hypothesis: at the margin, the relationship between STRs and housing prices may be positive *or negative*.¹⁴ In this section, we test this hypothesis by estimating the effects of Airbnb listings on local housing prices. Although our data contains the current per-night price of each listing, we focus on Equation (4) as opposed to Equation (3) for two reasons. First, our data represent a noisy estimate of R_j —while the lifetime expected revenue for an STR is correlated with an observed per-night price, reservations, and future prices (and therefore the time series of revenues) is unknown. Second, the observed listings represent a selected sample—we do not observe STR pricing for housing units that are not listed. As listings and housing prices are both equilibrium outcomes, we proceed by following an instrumental variable strategy in conjunction with fixed effects.

Let ZHVI_{zt} be the Zillow Home Value Index for postal code z in jurisdiction j at year-month time t . Equation (4) suggests the following estimating equation as a linear approximation to the relationship between housing prices and STRs:¹⁵

$$\log(\text{ZHVI}_{zt}) = \alpha + \beta_j \log(\text{listings}_{zt}) + FX_y + FX_j + \epsilon_{zt} \quad (5)$$

where listings_{zt} is the number of Airbnb listings, ϵ_{zt} is an unobservable, and the coefficients of interest are $\{\beta_j\}$, which vary across cities. FX_y is a set of fixed effects for the year to account for time-varying characteristics that are correlated with both Airbnb listings and housing prices, such as regional population growth and macroeconomic conditions.¹⁶ FX_j are city fixed effects that control for city-level characteristics that stay constant over time

¹⁴The null hypothesis is that the relationship between STRs and housing prices is weakly positive.

¹⁵We use a linear approximation inspired by Equation (4) in lieu of estimating the primitives underlying Equation (2) in part due to data limitations (e.g. we do not observe H_j) and in part due to the parsimonious nature of our model, which abstracts from several important issues such as changes in housing supply.

¹⁶As the ZHVI is seasonally adjusted, month-of-year fixed effects are not necessary.

and are correlated with both Airbnb listings and housing prices as captured by the ZHVI.¹⁷ Finally, in practice, a small number of observations have zero listings. We therefore use $\log(1 + \text{listings}_{zt})$. Our results are qualitatively robust to dropping these observations.

There may be some component of ϵ_{zt} which is correlated with listings_{zt} . For example, changes in some unobservable amenity such as the quality of local public transportation or prices of tourist attractions may be responsible for changes in both the number of STR listings and in the price of housing in a way that is not captured by our fixed effects. We employ the instrument proposed by [Barron et al. \(2020\)](#): we interact the Google Trends measure for the search term “airbnb” with the number of establishments in the food services and accommodations industry (NAICS 72) in each postal code in the base year of 2010—prior to large-scale Airbnb entry in the area. Specifically, if g_t^{air} is the Google Trends measure and b_z^{2010} is the number of NAICS 72 establishments in 2010 in postal code z , our instrument is

$$z_{zt} = g_t^{air} \times b_z^{2010}.$$

Our identifying assumption is that $E[z_{zt} \cdot \epsilon_{zt}] = 0$.¹⁸ Intuitively, b_z^{2010} acts as a proxy for the degree to which a given neighborhood attracts tourists over the long term (and therefore may be a more attractive place for entry by a potential Airbnb host). On its own, however, this variable is likely also to be correlated with housing prices, because food service establishments are probably positive neighborhood amenities ([Meltzer and Schuetz, 2012](#)). The g_t^{air} variable scales this “touristy-ness” measure by the overall market size for Airbnb. Given that the attractiveness of restaurants in a *specific neighborhood* to long-term residents (or prospective residents) of *that neighborhood* is likely not correlated with the *nationwide* market presence of Airbnb (as measured by Google Trends), the interaction between these

¹⁷For example, STR units are on average likely to be smaller than the median home size within any particular postal code. To the extent that the distribution of STR sizes conditional on the distribution of home sizes is constant across postal codes within a city, these fixed effects will account for this difference.

¹⁸In the framework of [Borusyak et al. \(2022\)](#), g_t^{air} is a single shock per period, and b_z^{2010} is the ‘exposure share’ for each postal code, which is constant across time.

two variables will likely satisfy the exclusion restriction. Furthermore, this instrument is correlated with housing prices in a given period only insofar as it is correlated with the number of Airbnb listings, conditional on the included fixed effects.¹⁹ We report summary information and first-stage estimates for our instrument in Appendix Section C.

Table 2 reports our estimates of β_{1j} for the cities listed in Table 1. In Column (1), we report an OLS estimate for the entire sample which includes every postal code in LA County with no fixed effects. In Column (2) we use our instrument, and in Column (3) we add fixed effects for city and year. In our most saturated specification, we estimate that a 10% increase in Airbnb listings *increases* average house prices in LA County by 1.12%. These estimates are similar in magnitude to those reported by Barron et al. (2020), who report that a 10% increase in Airbnb listings leads to a 0.77% increase in house prices as measured by the ZHVI.²⁰

In Columns (4) through (6), we estimate individual coefficients for each city. In Column (4) we include only city fixed effects and estimate with OLS; in Column (5) we employ our instrument. In Column (6), our preferred specification, we add year fixed effects. Appendix Table D.2 reports estimates for this specification for all cities. Across cities, the coefficients vary widely. In West Hollywood, a 10% increase in the number of Airbnb listings *increases* housing prices by 1.63%, whereas in Santa Monica, a 10% increase in the number of Airbnb listings *decreases* housing prices by 2.67%.

We have chosen to report estimates for these particular cities because they are more likely to be familiar to many readers. However, the estimated heterogeneity in the estimates of β_{1j} is similar across the entire set of estimates. Figure 3 depicts the distribution of the full set of β_{1j} coefficients for each jurisdiction using our fully saturated model. Of the 88 cities in our preferred specification, 7 have negative point estimates—5 of which are distinguishable from zero in the statistically significant sense. We take these results as broadly consistent

¹⁹One may be concerned that spillovers are not contained to the city level. To the extent that STRs are substitutes for hotels, public intoxication incidents will likely occur near the location of the STR.

²⁰This estimate comes from the unconditional effect of $\ln(\text{Airbnb Listings})$ on $\ln(\text{ZHVI})$ as reported in the first row of Table 6, Column (6) of Barron et al. (2020). Indeed, the reported confidence intervals overlap.

Table 2: The Effect of STRs on Housing Prices for Selected Cities in LA County

	(1)	(2)	(3)	(4)	(5)	(6)
log(listings) for						
LA (entire county)	0.142*** (0.002)	0.158*** (0.010)	0.112*** (0.012)			
LA (city)				0.162*** (0.003)	0.276*** (0.018)	0.252*** (0.018)
Beverly Hills				0.932*** (0.048)	1.123*** (0.054)	0.653*** (0.065)
Burbank				0.074*** (0.015)	-0.099*** (0.017)	-0.059*** (0.018)
Malibu				0.089*** (0.020)	0.318*** (0.025)	0.196*** (0.021)
Pasadena				0.021 (0.028)	0.168*** (0.035)	0.135*** (0.031)
Pomona				0.001 (0.004)	0.139*** (0.013)	0.103*** (0.016)
San Gabriel				-0.022 (0.016)	-0.140*** (0.009)	-0.135*** (0.012)
Santa Monica				-0.454*** (0.051)	-0.343*** (0.038)	-0.267*** (0.043)
Torrance				0.160*** (0.022)	0.255*** (0.019)	0.197*** (0.023)
West Hollywood				0.305*** (0.021)	0.262*** (0.027)	0.163*** (0.020)
IV	No	Yes	Yes	No	Yes	Yes
City FEs	No	No	Yes	Yes	Yes	Yes
Year FEs	No	No	Yes	No	No	Yes
R ²	0.379	0.061	0.624	0.750	0.667	0.659
Num. obs.	6800	6800	6800	6800	6800	6800

Notes: This table reports estimates of Equation (5). We estimate coefficients for each city in LA County; full estimates are available in Appendix Table D.2. An observation is a postal-code-month. The dependent variable is the log of the Zillow Home Value Index. `listings` is the number of Airbnb listings plus one, which we instrument for with an interaction of Google Trends and the number of food establishments. First stage details are reported in Appendix Section C. Heteroskedastic-robust standard errors are in parentheses; we report standard errors under alternative assumptions in Figure D.1. Stars indicate p values: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. In Columns (5) and (6) we drop cities that are collinear with the area fixed effects.

with the previous literature: for most jurisdictions, the estimated relationship between STR listings and housing prices is positive, just as the estimated relationship for the region as a whole is positive. However, the average effect for the region aggregates over a substantial degree of heterogeneity at the city level.

Figure 3: The Heterogeneous Effect of Airbnb Listings on Housing Prices



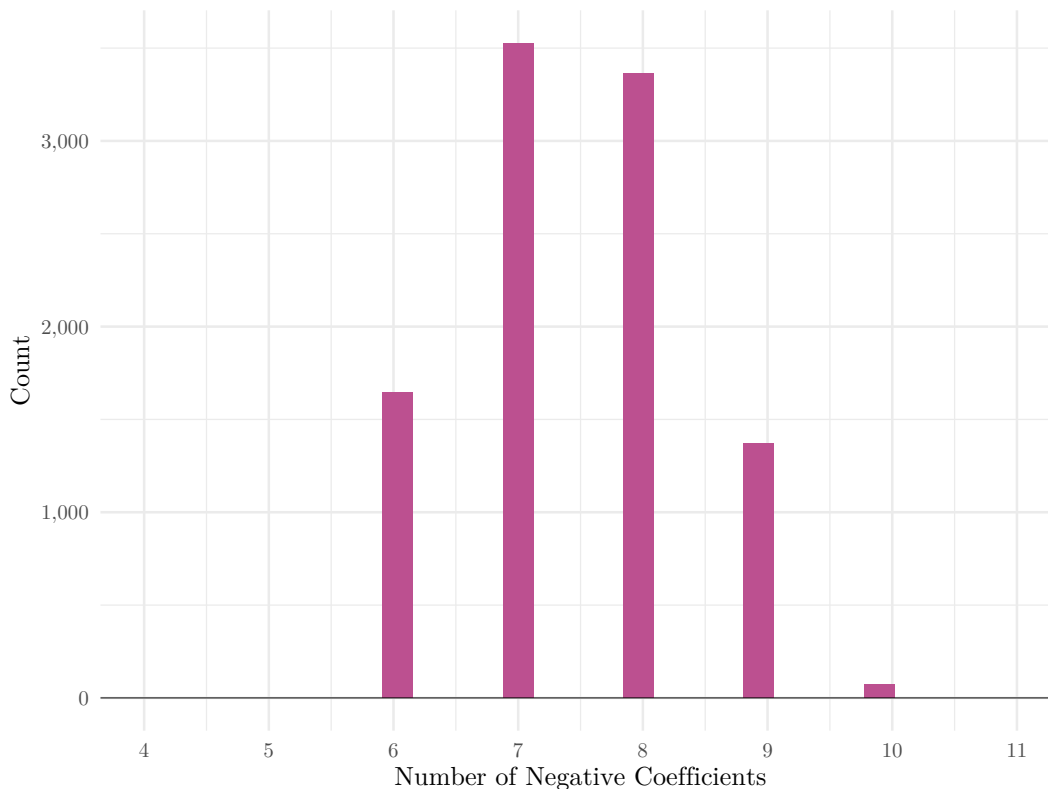
Notes: This figure depicts a histogram of the estimated β_{1j} s from Equation (5) for all cities. Estimates come from the fully saturated model: Column (6) in Table 2.

Given the number of cities in LA County, it is possible that these negative estimates are the result of measurement error or other noise, even if the true effect of additional listings on house prices is universally positive. To address this possibility, we ask “if the data was sample many times, what is the probability that we would estimate at least 7 cities with negative coefficients?” We use the Wild bootstrap (Wu, 1986) to re-estimate the coefficients of Equation (5) 10,000 times using our most saturated specification.²¹ We

²¹We thank an anonymous referee for suggesting this exercise.

plot the distribution of negative coefficients in Figure 4. We estimate at least 7 negative coefficients in 8,334 of the resamples, and in no re-sampling do we estimate fewer than 5 negative coefficients. We conclude that it is unlikely that our qualitative result (i.e. the conclusion that in some jurisdictions the marginal effect of additional STR listings on house prices is negative) is driven by measurement error or other noise.

Figure 4: The Distribution of Negative Airbnb Coefficients Across Wild Bootstrap Resamples



Notes: This figure depicts a histogram of the number of estimated β_{1j} s from Equation (5) that are negative across 10,000 Wild bootstrap resamples. For each replication, we use the fully saturated model: Column (6) in Table 2.

6 The effect of Santa Monica’s STR regulation

The evidence of the previous section suggests that the marginal impact of additional STRs on housing prices in some areas can be negative. Our framework attributes the heterogeneity in the sign of the relationship to the channel of negative amenities (for owner-occupiers)

generated by the presence of STRs. This suggests some further hypotheses: If an area in which the marginal impact of STRs on housing prices is negative implements a policy that reduces the number of STRs, either through affecting R_j or by decreasing str_j directly, we should expect to see both an increase in housing prices and a decrease in negative amenities.

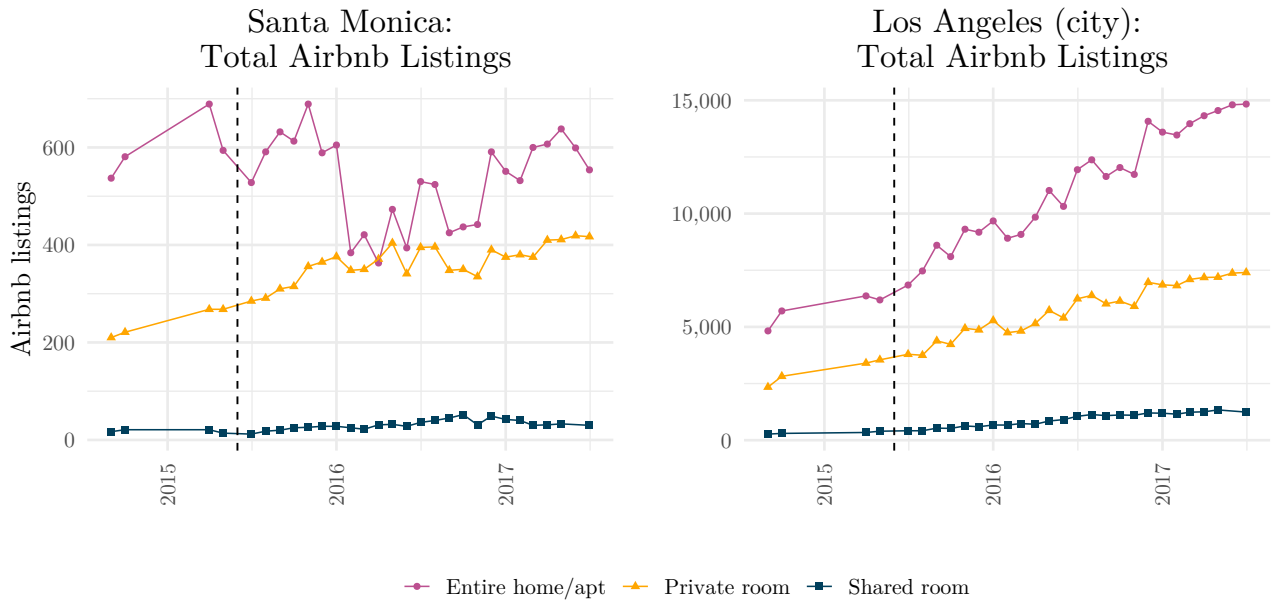
We test these hypotheses by exploring the effects of Santa Monica’s Ordinance 2484CCS, which was adopted by its City Council on May 12, 2015. According to staff reports and the text of the measure, STRs removed “needed permanent housing from the market” and transient visitors could “disrupt the quietude ... of the neighborhoods and adversely impact the community” (City of Santa Monica, 2019). The measure nominally banned owner-absent STRs, while allowing owner-present STRs to continue with additional licensing, reporting, and taxation requirements. It is important to note that given this context, we cannot claim that the policy represents a “clean” natural experiment. However, given that the stated goal of the policy was to *decrease* housing prices, any finding to the contrary provides evidence of the heterogeneous impacts of STRs.

First, we document that the reform did in fact reduce the level of Airbnb listings in Santa Monica. Figure 5 illustrates Airbnb listings in both Santa Monica and the City of LA for comparison. Each point requires a separate web scrape, and the vertical line represents the date of the passage of the reform. Only 4 scrapes were conducted before the policy was enacted, but subsequent scrapes occurred at roughly monthly intervals for the two years following. Negotiations and legal maneuvers between STR listers and the City of Santa Monica resulted in gradual enforcement which is visible in the figure; the largest drop in Airbnb listings occurred at the end of 2015.²² No similar drop appears for the

²²When the ordinance went into effect in June of 2015, Airbnb (among other platforms) quickly launched a legal challenge which made enforcement difficult (Dolan, 2019). The city had no ability to prevent landlords from honoring reservations made before the ordinance went into effect. Landlords worked to circumvent the provisions of the ordinance, finding ways to offer owner-absent STRs that complied with the rules. These circumventions included modifications to listings that likely decreased the probability that renters would generate externalities. For example, the owner may be present for a short period at the beginning and end of the rental period, or be occupying an adjacent dwelling. These actions resulted in several additional legal challenges. See *Diane Hayek v. City of Santa Monica*, Los Angeles Superior Court No. 17STLC02007 (May 30, 2018) and *Diane Hayek v. City of Santa Monica*, Los Angeles Superior Court No. BS170950 (August 19, 2019). Ultimately, the city prevailed in all cases.

City of LA. The number of entire-home listings eventually rebounded to the pre-regulation level. Conversations with city officials indicate that the policy effectively changed the nature of entire-home listings to reduce those which may generate negative externalities, though verifying those claims is difficult due to the nature of the Airbnb data.

Figure 5: Airbnb listings by room type



Notes: Each point on each graph represents a separate web scrape of Airbnb’s listings for that city. The dotted line represents May 2015, when Santa Monica’s ordinance was passed. Web scrape data was obtained from insideairbnb.com and tomslee.net and harmonized. See Section 4 for details.

We estimate the causal effect of this policy using the generalized synthetic control method of Xu (2017) inspired by Abadie et al. (2010). That is, instead of taking a single city or group of cities as an *a priori* comparison group representing the counterfactual time series of housing prices in Santa Monica, we construct counterfactual housing prices using an average of postal codes without STR restrictions in the state of California weighted according to the pre-reform outcomes.

As before, let $ZHVI_{zt}$ be the Zillow Home Value Index for postal code z at time t . Following

the notation of Xu (2017), we model ZHVI_{zt} as

$$\log(\text{ZHVI}_{zt}) = \delta_{zt}D_{zt} + \lambda'_z f_t + \epsilon_{zt} \quad (6)$$

where D_{it} is a treatment indicator which equals 1 for postal codes in Santa Monica in June 2015 or later (when the reform went into effect) and equals zero otherwise, f_t is a vector of unobserved common factors and λ_z is a vector of unknown factor loadings. ϵ_{zt} captures unobserved idiosyncratic factors influencing house prices for postal code z at time t and is assumed to have zero mean.

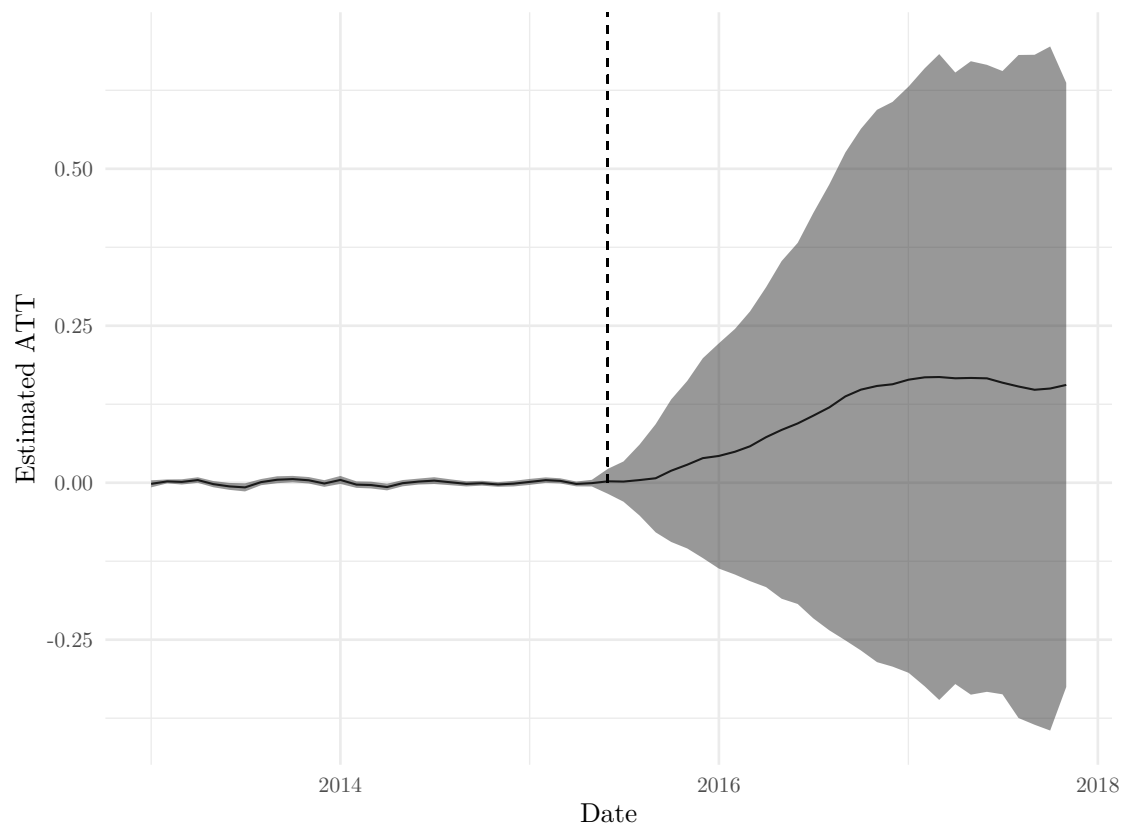
We estimate the average treatment effect on the treated (ATT) for each post-reform period $t > T_0$, $ATT_t = \frac{1}{N_{tr}} \sum_z \delta_{zt}$, and perform inference using the estimator of Xu (2017) as implemented in the R package `gsynth`. We plot these estimated ATTs along with 95% confidence intervals in Figure 6. In Appendix E we repeat this analysis using all zip codes in the U.S. as potential controls. In Appendix F we employ a traditional difference-in-differences approach using only postal codes in the City of Los Angeles as controls.

Aggregated over the post-treatment period, the estimated ATT is 0.1031, with a p-value of 0.2280 (standard error 0.0855). Though noisy, the point estimate conforms to the estimates of Table 2: using the number of entire unit listings from 2015 to 2016, $(\ln(616) - \ln(466)) \times -0.267 = -0.075$. We conclude that the reform may have increased housing prices, and probably did not *reduce* housing prices, as was the stated intent.

7 Exploring negative externalities through police calls

Our framework suggests that the results in the previous section are driven by negative externalities generated by STRs. To evaluate this mechanism, we use a difference-in-differences approach to estimate the impact of STR regulations on public intoxication calls to police relative to other police calls in Santa Monica.

Figure 6: Synthetic control estimates of the effect of Santa Monica's STR regulation on housing prices



Notes: This figure presents estimates of the average treatment effect for postal codes in Santa Monica around the time of its STR regulation using a synthetic control approach. The outcome is the log of housing prices as measured by the Zillow Home Value Index. The potential control units include all postal codes in the United States without STR regulations. The black line indicates the point estimates, while the shaded area represents the 95% confidence interval. Estimates and figure constructed using the R package **gsynth**.

We collect data on police calls from the Santa Monica Open Data Project for 2013–2019.²³ For each call, we observe the date and time, the location of the caller, and the reason for the call. Importantly, these data encode the reason for the call at a much finer level of detail than is used for either the Uniform Crime Reporting data or National Incident-Based Reporting System data compiled by the Federal Bureau of Investigation. Unfortunately, few cities in the U.S. report data at this level of detail and none do so at the same frequency; we are not aware of any cities that report data that can be harmonized with the Santa Monica data for direct comparison.²⁴ We focus on comparing calls for which the reason listed includes ‘public intoxication’ against other calls because this frequently appears in media reports about the nuisance effects of STRs (Lieber, 2015; Griffith, 2020). In Appendix G, we explore other ‘party related’ call types. These monthly data are illustrated in Figure 7. After an initial increase from 2013 to 2014, the number of public intoxication calls decreased 56% from 2014 to 2019 while all other calls decreased only 6% during the same period.

Given the delay in enforcement (in particular the fact that existing reservations were honored), we add linear trend terms to the traditional difference-in-differences specification and model the log of the number of police calls of type $i \in \{\text{intox}, \text{other}\}$ at time t (where $t = 0$ for the month when the policy went into effect) as

$$\begin{aligned} \log(\text{calls}_{it}) = & \alpha_0 + \alpha_1 \times \text{intox}_i + \alpha_2 \times \text{post}_t + \alpha_3 \times \text{intox}_i \times \text{post}_t \\ & + t \times [\alpha_4 + \alpha_5 \times \text{intox}_i + \alpha_6 \times \text{post}_t + \alpha_7 \times \text{intox}_i \times \text{post}_t] \\ & + FX + \varepsilon_{it} \end{aligned} \quad (7)$$

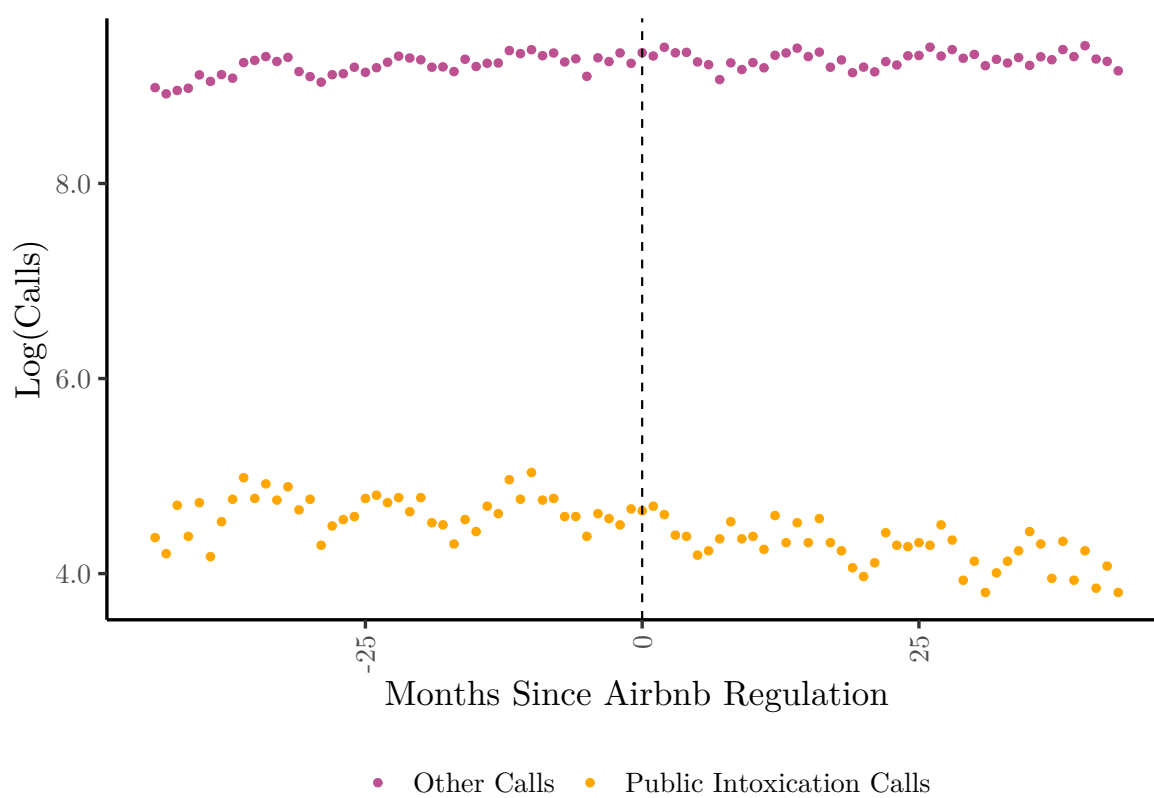
where post_t is an indicator equal to one if the observation falls after the normalized policy date. We include month-of-year fixed effects to account for the seasonality of tourism.

Table 3 reports OLS estimates of Equation (7). Our preferred specification reported in Column (2) includes month-of-year fixed effects. The relevant parameters are the level

²³<https://data.smgov.net/Public-Safety/Police-Calls-for-Service/ia9m-wspt>

²⁴For example, Eugene, Oregon, reports call reasons with high granularity, but uses categories with different definitions and aggregates to the annual level.

Figure 7: Public intoxication and other police calls in Santa Monica



Notes: Data from the Santa Monica Open Data Project. Each point is a month.

and time trend interaction terms for `post × intox calls`. Given the delay in enforcement mentioned in Section 2 and the fact that existing reservations were largely honored, it is perhaps unsurprising that there is no immediate discontinuity in police calls at the time the policy is enacted. However, post-reform, calls trend consistently downward. This effect is weaker, although more precisely estimated, when fixed effects are included. This trend persists for more than two years after the policy was enacted, despite the eventual increase in listings seen in Figure 5. As discussed in Section 2, negotiations and legal maneuvers between Airbnb listers and the City of Santa Monica resulted in post-reform listings that were qualitatively different and may have been less likely to generate public intoxication police calls. In Columns (3) and (4) we conduct a placebo test using June 2013 as a placebo reform date. There is no evidence of a difference in either the level or the time trend in calls around our placebo date. In Appendix G we explore event studies for related call types.

8 Conclusion

As the short-term rental (STR) housing market has expanded over the past several years, jurisdictions around the world have struggled to respond to its presence. Several media reports have captured policymakers’ concerns about the effect of STRs on long-term housing prices (e.g. [Henley, 2019](#); [Minder and Abdul, 2020](#)). Other reports have focused on the deleterious effects of STRs on neighborhoods, particularly from the perspective of neighboring long-term residents ([Griffith, 2020](#)), as zoning laws are generally designed to increase the value of property rights ([Fischel, 2000](#)). These different stories are potentially contradictory—if STRs sufficiently reduce local amenities, their presence could be associated with lower, not higher, housing prices.²⁵

We present a stylized model to demonstrate that STR listings at the intensive margin can reduce housing prices. Our model makes a simple point: since STRs can have both

²⁵This is particularly important as if the negative externalities associated with STRs are small, local homeowners may be sufficiently compensated by increased equity in their real property ([Coase, 1960](#)).

Table 3: Difference-in-differences evidence of Santa Monica’s STR regulation effect on police calls for public intoxication

	(1)	(2)	(3)	(4)
intox calls	−4.6667*** (0.0632)	−4.6667*** (0.0476)	−4.5308*** (0.0894)	−4.5308*** (0.0675)
post	−0.0555 (0.0372)	−0.0842*** (0.0266)	0.0262 (0.0412)	−0.0935* (0.0546)
post × intox calls	−.0960 (0.0828)	−0.0960 (0.0668)	−0.0205 (0.1073)	−0.0205 (0.0836)
Months ×				
Constant	.00057*** (0.0012)	0.0054*** (0.0008)	0.0079*** (0.0023)	0.0135*** (0.0024)
intox calls	−0.0048 (0.0030)	−0.0048** (0.0024)	−0.0034 (0.0069)	−0.0034 (0.0056)
post	−0.0055*** (0.0016)	−0.0044*** (0.0011)	−0.0054* (0.0028)	−0.0076** (0.0037)
post × intox calls	−0.0066* (0.0038)	−0.0066** (0.0032)	−0.0020 (0.0079)	−0.0020 (0.0067)
Reform date	Actual	Actual	Placebo	Placebo
Month-of-year FEs?	No	Yes	No	Yes
R ²	0.997	0.998	0.996	0.998
N	174	174	98	98

Notes: This table reports estimates of Equation (7). The bandwidth for Columns (1) and (2) is chosen per the optimal bandwidth technique of [Imbens and Kalyanaraman \(2011\)](#). An observation is a month-call-type. The dependent variable is the log of the number of police calls reported in Santa Monica. Heteroskedastic-robust standard errors are in parentheses. For Columns (3) and (4), the placebo date is June 2013 and the bandwidth is chosen to avoid including post-treatment periods. Stars indicate p-values: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

positive and negative impacts on local amenities, the impact of STRs on housing prices is an ambiguous function of the *net* effect of STRs on amenities—and therefore the effect of policies designed to curb STRs on housing prices is ambiguous as well. We illustrate this point empirically with both a panel analysis of the relationship between Airbnb listings and housing prices across jurisdictions within LA County, and a synthetic control analysis of Santa Monica’s housing prices before and after their 2015 regulation, using other cities in California without STR regulations as potential controls. In both analyses, we found evidence consistent with the hypotheses stemming from our model: STRs can lead to lower housing prices, and regulating them can increase housing prices. We provide evidence for our proposed mechanism in the form of an analysis of calls to police in Santa Monica. We find that while the policy did not have a measurable immediate (or discontinuous) effect likely due to enforcement lags, Santa Monica’s policy decreased the number of public intoxication police calls over time.

Our results have broad implications for housing policy. Many policymakers have focused on type-of-use regulations on housing units to restrict or ban STRs (Coles et al., 2017). These restrictions are expected to reduce the option value of owning a housing unit and therefore to decrease housing prices. However, in contrast to previous work, we find evidence that such a policy may have the opposite effect. Our framework suggests that local policymakers may wish to separately consider the effect of STRs on positive and negative local amenities. For example, visitors to a jurisdiction that neighbors (but does not contain) a major tourist attraction may not spend many tourism dollars within the jurisdiction but may yet generate negative externalities, in which case policymakers may wish to consider restricting STRs. This conclusion, however, stems from a qualitative thought experiment which is challenging to translate to a quantifiable form that is broadly applicable across geographies. We leave it to future work to systematically and separately quantify the effects that STRs have on positive and negative amenities across jurisdictions.

These results also point to the importance of taking into account possible additional ef-

fects of peer-to-peer transaction platforms when considering regulation. Across industries, the literature consistently finds that peer-to-peer transactions increase surplus but tend to increase variance and/or risk relative to more traditional products and services as property rights may be less well-defined. Examples include Uber ([Barrios et al., 2020](#)), Kickstarter ([Mollick, 2014](#)), Craigslist ([Kroft and Pope, 2014](#)), and Fiverr ([Hannák et al., 2017](#)), among others. Furthermore, the provisions of the Communications Decency Act imply that these platforms have the potential to create outcomes which are biased along racial and gendered lines ([Doleac and Stein, 2013](#); [Edelman and Luca, 2014](#); [Hannák et al., 2017](#)). At the same time, the potentially positive externalities that these markets may generate imply that regulations may have unintended consequences ([Cunningham et al., 2019](#)). In the case of Airbnb and other short-term housing rental platforms, our results suggest that while federal policy may help ensure more uniform treatment for consumers, local decision-makers may be best-suited to set local policy for residents ([Tiebout, 1956](#)).

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Appendices

A Equilibrium Housing Prices

In this appendix we derive Equation (2)—the equilibrium housing price equation. To simplify notation, we write $\xi_j = \xi_j(k_j, f(str_j), g(str_j))$. Furthermore, define $\bar{u}_{i,j,k} = u_{i,j,k} - \epsilon_{i,j,k}$ and $\phi_{j'} = \sum_{j' \neq j} \left(\exp(-P_{j'}^* + \frac{R_{j'}}{1-\delta}) + \exp(\xi_j) \right)$. Using the market clearing condition we can write:

$$\begin{aligned}
 & \left(\frac{\exp(\bar{u}_{j,a})}{1 + \sum_{j'} \sum_{k'} \exp(\bar{u}_{j',k'})} + \frac{\exp(\bar{u}_{j,o})}{1 + \sum_{j'} \sum_{k'} \exp(\bar{u}_{j',k'})} \right) N = H_j \\
 & \left(\frac{\exp(-P_j^* + \frac{R_j}{1-\delta}) + \exp(\xi_j - P_j^*)}{1 + \sum_{j'} \exp(-P_{j'}^* + \frac{R_{j'}}{1-\delta}) + \sum_{j'} \exp(\xi_j - P_{j'}^*)} \right) N = H_j \\
 & \left(\frac{\exp(-P_j^*)(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j))}{1 + \exp(-P_j^*)(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j)) + \phi_{j'}} \right) N = H_j \\
 & \exp(-P_j^*)(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j))N = (1 + \exp(-P_j^*)(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j)) + \phi_{j'})H_j \\
 & \exp(-P_j^*)(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j))N - H_j \exp(-P_j^*)(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j)) = (1 + \phi_{j'})H_j \\
 & \exp(-P_j^*)(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j))(N - H_j) = (1 + \phi_{j'})H_j \\
 & \exp(-P_j^*) = \frac{(1 + \phi_{j'})H_j}{(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j))(N - H_j)} \\
 & P_j^* = -\log \left(\frac{(1 + \phi_{j'})H_j}{(\exp(\frac{R_j}{1-\delta}) + \exp(\xi_j))(N - H_j)} \right)
 \end{aligned}$$

B Cleaning the Airbnb Data

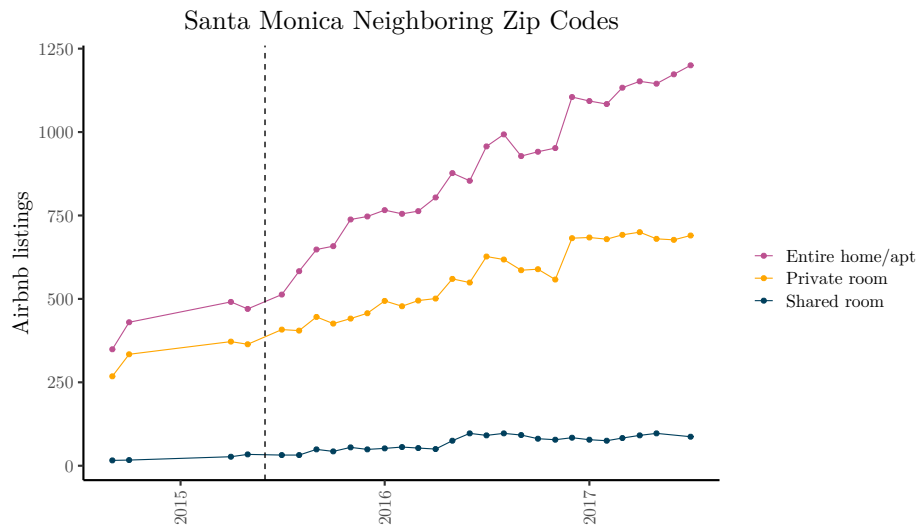
To perform our analyses, we merge the publicly available Tomslee and Inside Airbnb data to assemble the most comprehensive data possible. Both datasets contain `room id` and `scrape date` variables. The room id variable is specific to an individually listed STR.²⁶ After merging the Tomslee and Inside Airbnb data, we keep observations based on a unique

²⁶The Inside Airbnb data contains a few year-months with multiple scrapes—such as November of 2019. Since we are ultimately interested in the number of listings at the postal-code-year-month level, we keep one `room id` per `year month`.

room id and year-month pairing; which drops duplicate observations from both repeated scrapes within a month and observations that are shared by Tomslee and Inside Airbnb. Summary statistics for all of the cities included in Table 1 is reported in Table B.1.

Our sources contain geo-coordinates of the listings. We intersect the listing locations with U.S. census ZTCA shapefiles and aggregate within postal codes and year-month to obtain total listings by postal-code-year-month. These coordinates are often listed with noise to maintain landlord privacy. Inside Airbnb notes that the additional noise keeps the reported coordinates within a 150m radius of the listing’s true location. Thus it is likely that some listings are recorded as located in postal codes adjacent to their true postal code. We interpret this as adding classical measurement error to our sample, thus attenuating our estimates toward zero. However, for this reason (as well as the policy change in Santa Monica), it is possible that we may observe some spillovers between adjacent postal codes. In Figure B.1, we plot the number of Airbnb listings each month for postal codes immediately adjacent to Santa Monica. This graph follows the pattern displayed by the City of Los Angeles closely—and does not follow the pattern observed in Santa Monica—indicating that policy- or measurement-error-based spillovers are unlikely.

Figure B.1: Airbnb Listings in postal codes adjacent to Santa Monica



Notes: Each point on each graph represents a separate web scrape of Airbnb’s listings. The dotted line represents May 2015, when Santa Monica’s ordinance was passed. Web scrape data was obtained from insideairbnb.com and tomslee.net and harmonized.

Table B.1: Summary statistics for housing prices and Airbnb listing data

Variable	2014	2015	2016	2017	2018	2019	Avg. % Δ
<u>Zillow Home Value Index</u>							
LA (county)	541	582	637	693	755	776	8
LA (city)	578	625	689	751	821	846	7
Beverly Hills	1,177	1,352	1,606	1,862	2,176	2,363	15
Burbank	581	621	667	713	772	786	6
Malibu	1,303	1,462	1,680	1,881	2,104	2,204	12
Pasadena	605	641	689	744	811	825	7
Pomona	322	343	365	394	425	434	8
San Gabriel	637	663	689	730	781	779	5
Santa Monica	942	1,066	1,209	1,363	1,531	1,595	11
Torrance	638	664	703	749	800	805	5
West Hollywood	676	712	785	850	920	988	8
<u>Airbnb listing counts</u>							
<i>Entire unit</i>							
LA (county)	6,984	10,491	14,562	18,981	25,320	27,424	32
LA (city)	5,264	7,759	11,053	14,218	18,359	19,393	31
Beverly Hills	137	232	327	436	558	603	36
Burbank	41	80	109	141	192	218	42
Malibu	37	121	194	260	469	399	78
Pasadena	110	178	239	356	402	380	31
Pomona		4	8	17	39	45	92
San Gabriel	2	7	10	34	58	73	152
Santa Monica	559	616	466	583	573	781	9
Torrance	4	15	32	47	76	107	118
West Hollywood	194	292	366	480	534	532	24
<i>Private room</i>							
LA (county)	3,549	5,992	8,421	11,056	14,204	14,342	34
LA (city)	2,581	4,114	5,732	7,132	8,553	8,289	28
Beverly Hills	44	107	115	138	164	151	36
Burbank	30	52	89	116	136	145	40
Malibu	19	43	50	46	81	58	37
Pasadena	69	119	172	247	255	243	32
Pomona		20	30	58	91	96	51
San Gabriel	13	35	50	107	166	185	80
Santa Monica	216	307	367	398	275	290	9
Torrance	20	46	78	97	156	155	58
West Hollywood	62	84	111	142	122	127	17
<i>Shared room</i>							
LA (county)	356	606	1,139	1,524	1,849	1,793	42
LA (city)	285	481	935	1,235	1,505	1,474	43
Beverly Hills	2	11	8	9	8	8	81
Burbank	4	5	7	6	23	24	69
Malibu		1	1	1	1	2	12
Pasadena	7	9	10	9	9	5	-3
Pomona		1	5	11	12	2	76
San Gabriel		2	4	10	37	13	126
Santa Monica	19	21	35	34	15	21	13
Torrance	3	4	8	7	14	13	49
West Hollywood	7	6	6	16	7	3	10

Notes: Entries are averages across monthly observations during each year. The Zillow Home Value Index is measured in thousands of dollars and reflects the median home value in the given jurisdiction in each year as estimated by Zillow.

C Instrument Details

When estimating Equation (5), we instrument for the number of Airbnb listings by interacting the Google Trends index for the search term `airbnb` with the number of restaurants in a given postal code in 2010, before the widespread market growth of Airbnb. Figure C.1 displays Google Trends data at the monthly level from 2010 through 2019. Figure C.2 illustrates the distribution of postal codes in the U.S. according to the log of the per-capita number of food and accommodation establishments (as defined by NAICS 72). On the left graph, dots on the horizontal axis indicate the postal codes that comprise Santa Monica; the right graph illustrates the postal codes in the City of Los Angeles for comparison. While the density of restaurants in Santa Monica is clearly above the mean, the distribution of LA postal codes roughly matches the U.S. as a whole.

Figure C.1: Google Trends data used in instrument construction

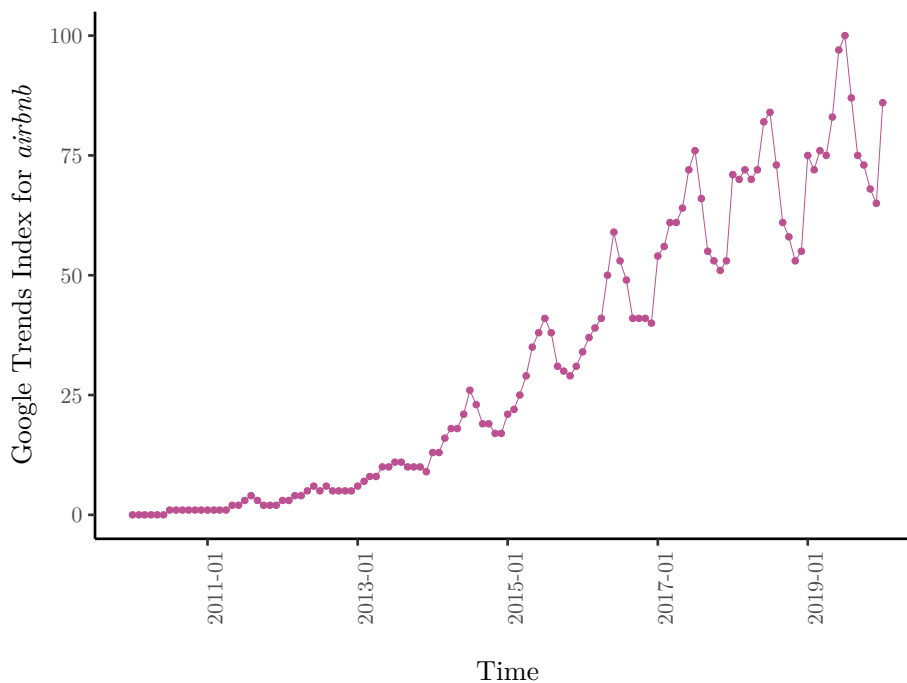
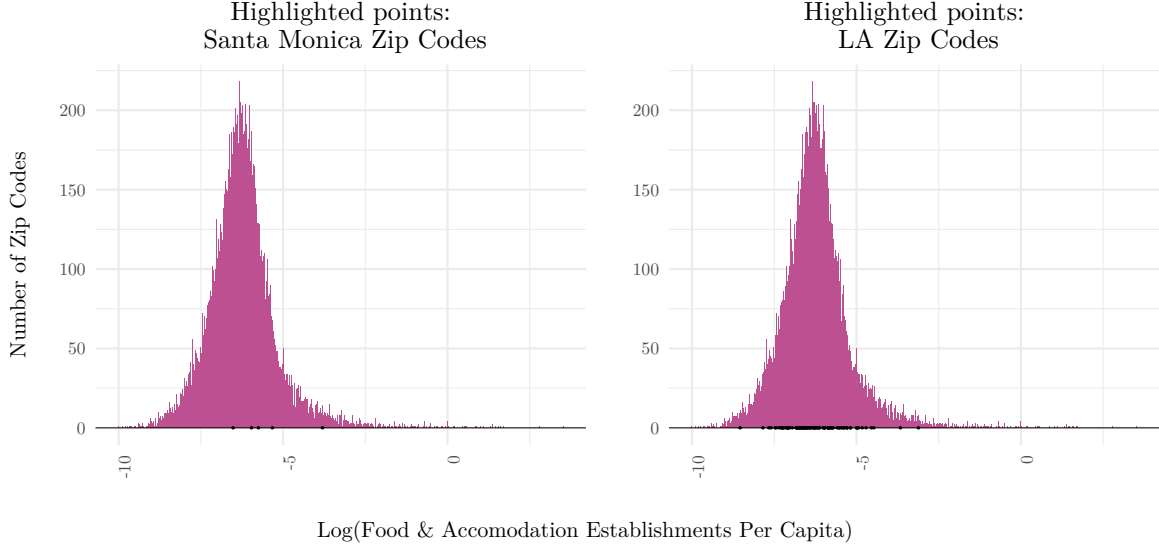


Figure C.2: Restaurant data used in instrument construction



Notes: These figures illustrate the distribution of postal codes in the U.S. according to the log of the per-capita number of food and accomodation establishments in 2010 (as defined by NAICS 72). Dots on the horizontal axis indicate postal codes for the cities in question.

D Additional IV Results, Robustness, and Alternative Specifications

In this appendix we present a number of additional results related to our analysis of the heterogeneous effects of STR listings on housing prices. We begin with additional results stemming from our preferred specification, Column (6) of Table 2. Table D.1 reports first-stage estimates i.e. the relationship between our instrument and the endogenous listings_{zt} . Our instrument enters positively and significantly. Table D.2 reports the β_{1j} coefficients for each of the cities within LA County. Finally, Figure D.1 plots the coefficients for Santa Monica and Los Angeles with confidence intervals calculated using alternative assumptions about the nature of correlations between postal-code-month-level unobservables (i.e. “clustered standard errors”). Our qualitative results are robust to these alternative assumptions.

Table D.1: First-stage instrument results

	$\log(\text{listings})$
$\log(z)$	0.643*** (0.0219)
R^2	0.6378
Num. obs.	6800

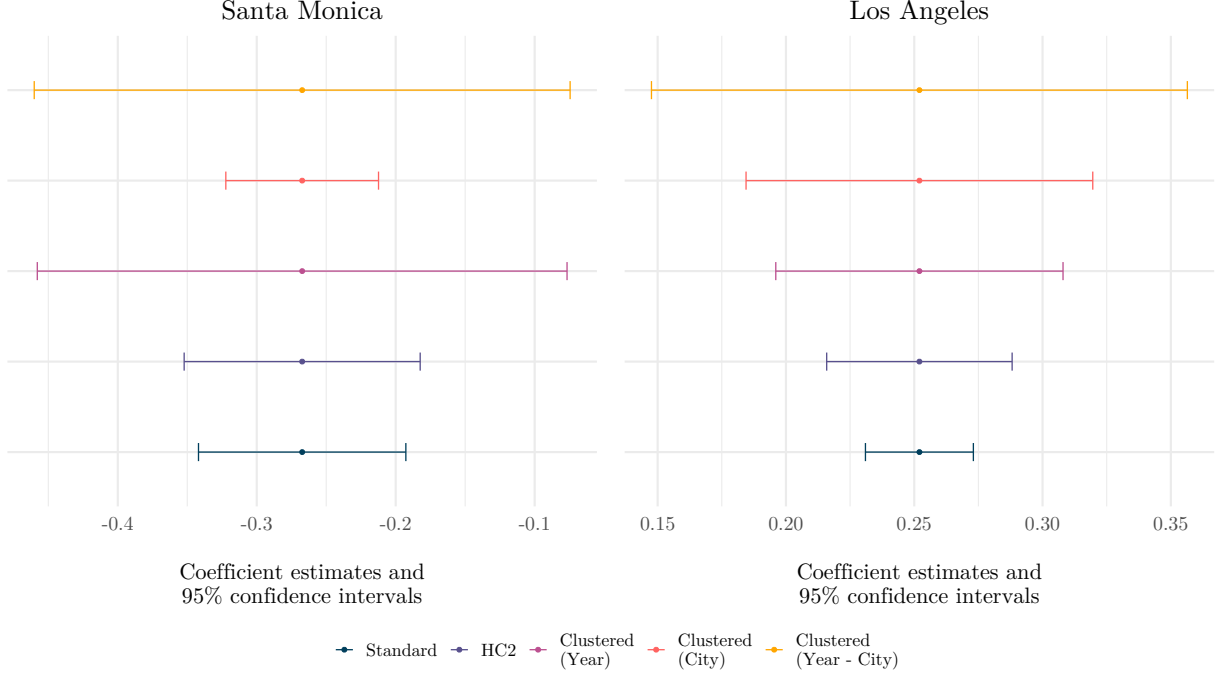
Notes: This table reports first-stage instrumental variables estimates of Equation (5) for our preferred specification, Column (6) of Table 2. An observation is a postal-code-month. Heteroskedastic-robust standard errors are in parentheses. Additional parameters of Equation (5) are included, but not reported for space. Stars indicate p values: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table D.2: The relationship between Airbnb listings and housing prices for all cities in Los Angeles County

City	Estimate	City	Estimate	City	Estimate	City	Estimate
Acton	0.0929*** (0.0185)	Covina	-0.0041 (0.0254)	La Mirada	0.106*** (0.0177)	Palmdale	0.1805*** (0.0106)
Agoura Hills	0.0723*** (0.0174)	Culver City	0.1857*** (0.0523)	La Puente	0.1098*** (0.0181)	Palos Verdes Estates	0.1453*** (0.0185)
Alhambra	0.0532*** (0.0124)	Diamond Bar	0.0645*** (0.0173)	La Verne	0.0681*** (0.017)	Paramount	0.1378*** (0.019)
Altadena	0.1207*** (0.0185)	Downey	0.0957*** (0.0265)	Ladera Heights	0.1372*** (0.0201)	Pasadena	0.1349*** (0.0311)
Arcadia	0.1397*** (0.0224)	Duarte	0.0983*** (0.0173)	Lake Hughes	0.2027*** (0.0204)	Pico Rivera	0.1218*** (0.0179)
Artesia	0.1199*** (0.0185)	East Los Angeles	0.079*** (0.0101)	Lakewood	0.1071*** (0.0121)	Pomona	0.103*** (0.0161)
Avalon	0.0368** (0.0166)	El Monte	0.1066*** (0.0184)	Lancaster	0.0014 (0.0468)	Rancho Palos Verdes	0.0873*** (0.0175)
Azusa	0.1099*** (0.0173)	El Segundo	0.1837*** (0.0213)	Lawndale	0.1458*** (0.0188)	Redondo Beach	0.2153*** (0.0231)
Baldwin Park	0.1186*** (0.0184)	Florence-Graham	0.1696*** (0.0192)	Little Rock	0.1887*** (0.0196)	Rosemead	0.1043*** (0.0175)
Bell	0.1263*** (0.0186)	Gardena	0.0581*** (0.0217)	Lomita	0.1068*** (0.0179)	Rowland Heights	0.0654*** (0.0173)
Bellflower	0.1216*** (0.018)	Glendale	-0.1373*** (0.0098)	Long Beach	0.0898** (0.037)	San Dimas	0.0823*** (0.0171)
Beverly Hills	0.6532*** (0.0654)	Glendora	-0.1534*** (0.0158)	Los Angeles	0.252*** (0.0184)	San Fernando	0.1249*** (0.0186)
Burbank	-0.0594*** (0.0177)	Hacienda Heights	0.0729*** (0.0173)	Lynwood	0.138*** (0.0186)	San Gabriel	-0.1347*** (0.0117)
Calabasas	0.0662*** (0.0176)	Hawaiian Gardens	0.1701*** (0.0204)	Malibu	0.1958*** (0.0212)	San Marino	0.302*** (0.0258)
Carson	0.084*** (0.0288)	Hawthorne	0.1415*** (0.0188)	Manhattan Beach	0.2721*** (0.0263)	Santa Clarita	-0.0539*** (0.0126)
Castaic	0.0955*** (0.0177)	Hermosa Beach	0.2349*** (0.0244)	Maywood	0.1286*** (0.0186)	Santa Fe Springs	0.1011*** (0.0177)
Cerritos	0.0646*** (0.0171)	Huntington Park	0.1371*** (0.0187)	Monrovia	0.0837*** (0.0176)	Santa Monica	-0.2673*** (0.0433)
Claremont	0.0675*** (0.0169)	Inglewood	0.0039 (0.0137)	Montebello	0.1121*** (0.0179)	Sierra Madre	0.0924*** (0.0176)
Commerce	0.1214*** (0.018)	La Canada Flintridge	0.2431*** (0.0232)	Monterey Park	0.0678*** (0.0065)		
Compton	0.0684*** (0.0068)	La Crescenta-Montrose	0.117*** (0.0183)	Norwalk	0.1159*** (0.018)		
Year FE				Yes			
City FE				Yes			
R ²				0.656			
Num. Obs				6,800			

Notes: This table reports the full set of relevant coefficients for Column (6) of Table 2. An observation is a postal-code-month. Cities include both incorporated sub-county jurisdictions and unincorporated areas as defined by the U.S. Census. The dependent variable is the log of the Zillow Home Value Index. Heteroskedastic-robust standard errors are in parentheses. Stars indicate p values: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Figure D.1: Comparing standard errors under alternative clustering



D.1 An Alternative Instrument

In this section we re-estimate Equation (5) in first-differences using a Bartik-style shift-share instrument following Goldsmith-Pinkham et al. (2020) (see also Borusyak et al., 2022). That is, we estimate the parameters of

$$\Delta \log(\text{ZHVI}_{zt}) = \beta_{1j} \Delta \log(\text{listings}_{zt}) + \Delta \mu_{zt}, \quad (8)$$

where Δ is the first-differencing operator. Let $n \in N$ denote two-digit NAICS sectors. We construct

$$z_{zt,ss} = \sum_{n \in N} \Delta g_t^{air} \times \omega_{nz}^{2010}, \quad (9)$$

where ω_{nz}^{2010} is the 2010 share of establishments located in postal code z within NAICS two-digit sector n . We use $z_{zt,ss}$ to instrument for $\log(\text{listings}_{zt})$. Intuitively, postal codes with higher shares of industries in 2010 that are correlated with Airbnb growth (after it enters) will see larger movements in Airbnb listings as national interest in Airbnb changes. However, the postal-code-level composition of a location's establishments are likely uncorrelated with

changes in national Airbnb interest.

We report first-stage results in Table D.3. Relative to our original instrument, this instrument is weaker. This is likely because many NAICS two-digit sectors are simply not strongly correlated with STR listings in either direction. Similarly, to the extent that this instrument captures the overall intensity of business activity in a postal code and postal codes with higher levels of business activity are likely to have a lower number of STR listings, the negative coefficient is unsurprising.

We illustrate the distribution of β_{1j} in Figure D.2; the full set is reported in Table D.4. Relative to the estimates in our preferred specification in Table 2, we estimate that, on the margin, additional Airbnb listings reduce housing prices in many more jurisdictions within LA County. One potential explanation for this is a substantial time persistence in the number of NAICS 72 establishments: if NAICS 72 establishments in 2010 (at the postal code level) are correlated with some underlying amenity x_{zt}^o at the postal code level (not captured by our city fixed effects) which also generates Airbnb listings once Airbnb enters, then our exclusion restriction may not hold.²⁷ If so, the sign of the bias is equal to the sign of $\beta^o \times Cov(\text{listings}_{zt}, x_{zt}^o)$ where β^o is the marginal effect of x_{zt}^o on housing prices. If x_{zt}^o is a positive amenity, such as access to major transportation infrastructure or large tourist attractions within a city, then it is reasonable that β^o and $Cov(\text{listings}_{zt}, x_{zt}^o)$ are both positive. As a consequence, the estimates of Section 5 would be biased upwards. To the extent that our first-differenced specifications purge this correlation, these estimates are sensible.

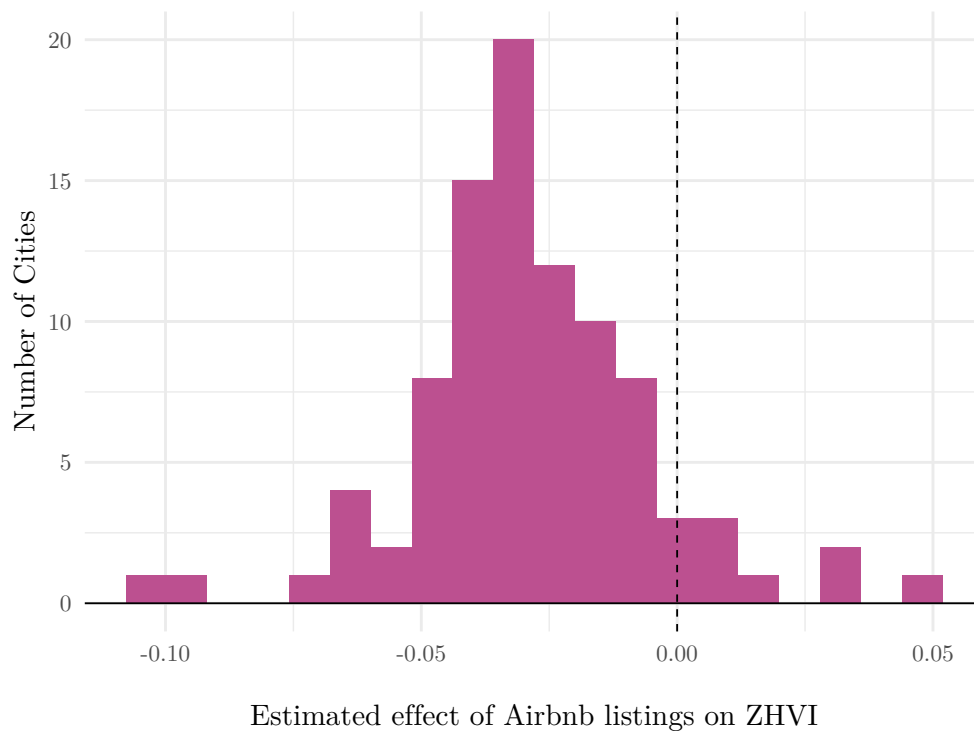
²⁷Indeed, across all US postal codes, the correlation between the number of NAICS 72 establishments in 2010 and the number of NAICS 72 establishments in 2017 is 0.97. We thank an anonymous referee for this point.

Table D.3: Alternative Instrument First Stage Results

	$\Delta \log(\text{listings})$
z_{ss}	-0.0259*** (0.0066)
R^2	0.0077
Num. obs.	5,712

Notes: This table reports first-stage instrumental variables estimates of Equation (8). An observation is a postal-code-month. Heteroskedastic-robust standard errors are in parentheses. Additional parameters of Equation (5) are included, but not reported for space. Stars indicate p values: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Figure D.2: The Heterogeneous Effect of Airbnb Listings on Housing Prices with an Alternative Instrument



Notes: This figure depicts a histogram of the estimated β_{1j} s from Equation (8) for all cities.

Table D.4: The relationship between Airbnb listings and housing prices for all cities in Los Angeles County using an alternative instrument

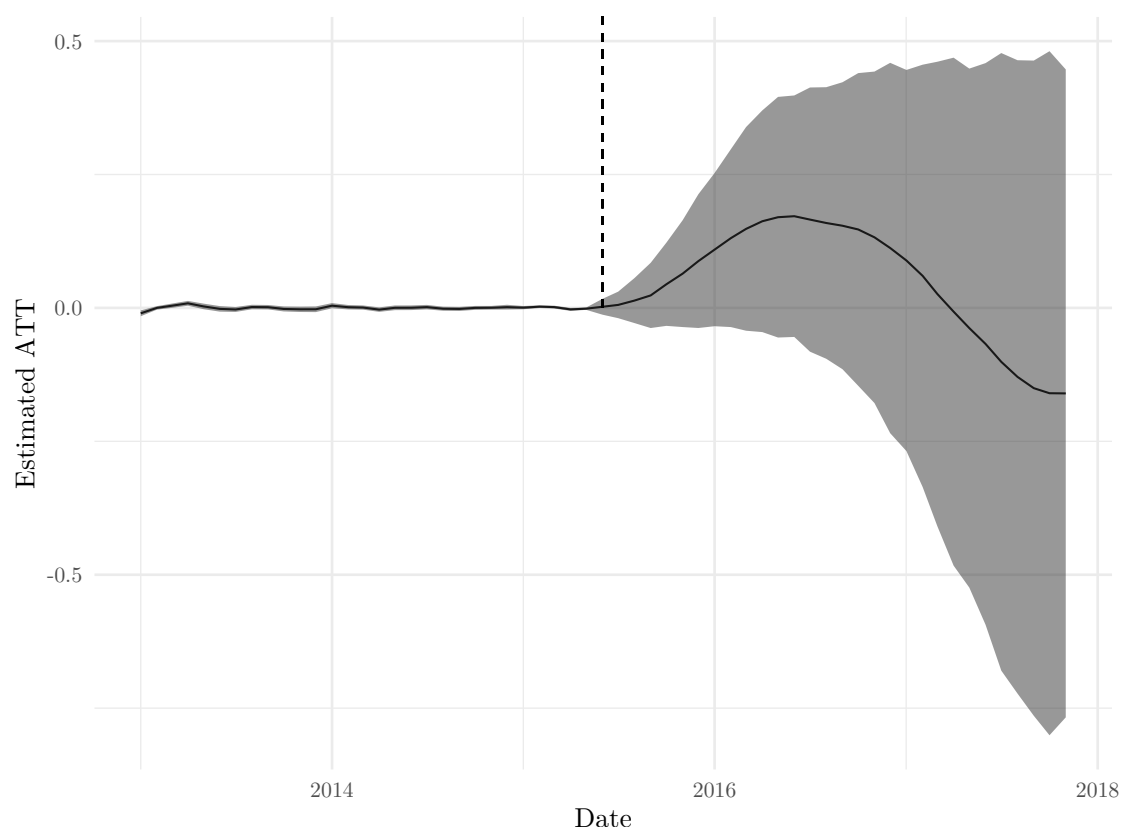
City	Estimate	City	Estimate	City	Estimate	City	Estimate
Acton	-0.036 (0.0231)	Covina	-0.0222*** (0.005)	La Mirada	-0.0293** (0.0134)	Palmdale	-0.0271*** (0.0079)
Agoura Hills	-0.0438*** (0.0168)	Culver City	0.0042 (0.0175)	La Puente	-0.0143* (0.0082)	Palos Verdes Estates	-0.0155 (0.0315)
Alhambra	-0.0537*** (0.0097)	Diamond Bar	-0.0479*** (0.0138)	La Verne	-0.0428*** (0.0142)	Paramount	-0.0087 (0.0149)
Altadena	-0.0615*** (0.0222)	Downey	-0.0313*** (0.0088)	Ladera Heights	-0.0748*** (0.0237)	Pasadena	-0.0437*** (0.0096)
Arcadia	0.0312** (0.0148)	Duarte	-0.0226 (0.0152)	Lake Hughes	-0.0299 (0.0209)	Pico Rivera	-0.0094 (0.0112)
Artesia	-0.0426** (0.0171)	East Los Angeles	-0.0321*** (0.0076)	Lakewood	-0.0226*** (0.0059)	Pomona	-0.0326*** (0.0078)
Avalon	-0.0302 (0.0246)	El Monte	-0.026*** (0.0075)	Lancaster	-0.0281*** (0.008)	Rancho Palos Verdes	-0.0419*** (0.0148)
Azusa	-0.034** (0.0146)	El Segundo	-0.0082 (0.0236)	Lawndale	-0.0197* (0.0113)	Redondo Beach	-0.0462*** (0.0114)
Baldwin Park	-0.0135 (0.0118)	Florence-Graham	-0.0376** (0.0167)	Little Rock	-0.0357 (0.0221)	Rosemead	-0.0453*** (0.0162)
Bell	-0.0212** (0.0095)	Gardena	-0.0062 (0.0097)	Lomita	-0.047*** (0.0181)	Rowland Heights	-0.0457*** (0.0139)
Bellflower	-0.0162 (0.0103)	Glendale	-0.034*** (0.0071)	Long Beach	-0.0417*** (0.0058)	San Dimas	-0.033*** (0.0118)
Beverly Hills	0.0095 (0.0112)	Glendora	-0.028*** (0.0105)	Los Angeles	-0.0324*** (0.0024)	San Fernando	-0.0011 (0.0093)
Burbank	-0.0344*** (0.0061)	Hacienda Heights	-0.0386*** (0.0123)	Lynwood	-0.0289*** (0.011)	San Gabriel	-0.0673*** (0.0135)
Calabasas	-0.0653*** (0.017)	Hawaiian Gardens	-0.0259 (0.0272)	Malibu	-0.0202 (0.0165)	San Marino	0.0013 (0.0193)
Carson	-0.0395*** (0.0127)	Hawthorne	-0.0497*** (0.0145)	Manhattan Beach	0.0448** (0.0208)	Santa Clarita	-0.0308*** (0.004)
Castaic	-0.0268** (0.0114)	Hermosa Beach	0.0359 (0.0233)	Maywood	-0.0303*** (0.0097)	Santa Fe Springs	-0.0058 (0.0084)
Cerritos	-0.0447*** (0.0159)	Huntington Park	-0.0089 (0.01)	Monrovia	-0.0571*** (0.02)	Santa Monica	-0.0102 (0.0149)
Claremont	-0.0235** (0.0114)	Inglewood	-0.0376*** (0.0091)	Montebello	-0.0198** (0.0096)	Sierra Madre	-0.0356* (0.0192)
Commerce	-0.0368*** (0.0087)	La Canada Flintridge	0.0154 (0.0155)	Monterey Park	-0.0408*** (0.0122)		
Compton	-0.0334*** (0.0091)	La Crescenta-Montrose	-0.0342* (0.0196)	Norwalk	-0.0139 (0.0092)		
R ²				0.7158			
Num. Obs				5,712			

Notes: This table reports the full set of city-level estimated coefficients of Equation (8). An observation is a postal-code-month. Cities include both incorporated sub-county jurisdictions and unincorporated areas as defined by the U.S. Census. The dependent variable is the first-differenced log of the Zillow Home Value Index. Heteroskedastic-robust standard errors are in parentheses. Stars indicate p values: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

E Using All US Postal Codes as Potential Synthetic Controls

In this appendix we repeat the analysis of Section 6 using all of the postal codes in the US as potential donor units for the synthetic control exercise. The estimate average treatment effects are plotted in Figure E.1. Aggregated over the post-treatment period, the estimated ATT is 0.0455, with a p-value of 0.4949 (standard error 0.0667).

Figure E.1: Synthetic control estimates of the effect of Santa Monica's STR regulation on housing prices



Notes: This figure presents estimates of the average treatment effect for postal codes in Santa Monica around the time of its STR regulation using a synthetic control approach. The outcome is the log of housing prices as measured by the Zillow Home Value Index. The potential control units include all postal codes in the United States without STR regulations. The black line indicates the point estimates, while the shaded area represents the 95% confidence interval. Estimates and figure constructed using the R package `gsynth`.

F Using Differences-in-Differences to Estimate the Effect of Santa Monica’s STR Regulation

In this section we estimate the causal effect of Santa Monica’s STR regulation on housing prices using a differences-in-differences approach with the City of LA as a control. Let P_{zt} be the housing price (as measured by the ZHVI) for postal code z at time t . We estimate the parameters of

$$\log(P_{zt}) = \beta_0 + \beta_1 \times trt_z + \beta_2 \times post_t + \beta_3 \times (trt_z * post_t) + FX + \epsilon_{zt} \quad (10)$$

where we define trt_z as an indicator equal to one if the postal code z falls in Santa Monica and zero otherwise (the City of LA), and $post_t$ as an indicator equal to one if the year-month is later than May of 2015 (when the policy was enacted). The coefficient on the interaction between these two terms (β_3) provides an estimate of the local average treatment effect. As before, FX are fixed effects for county region and year.

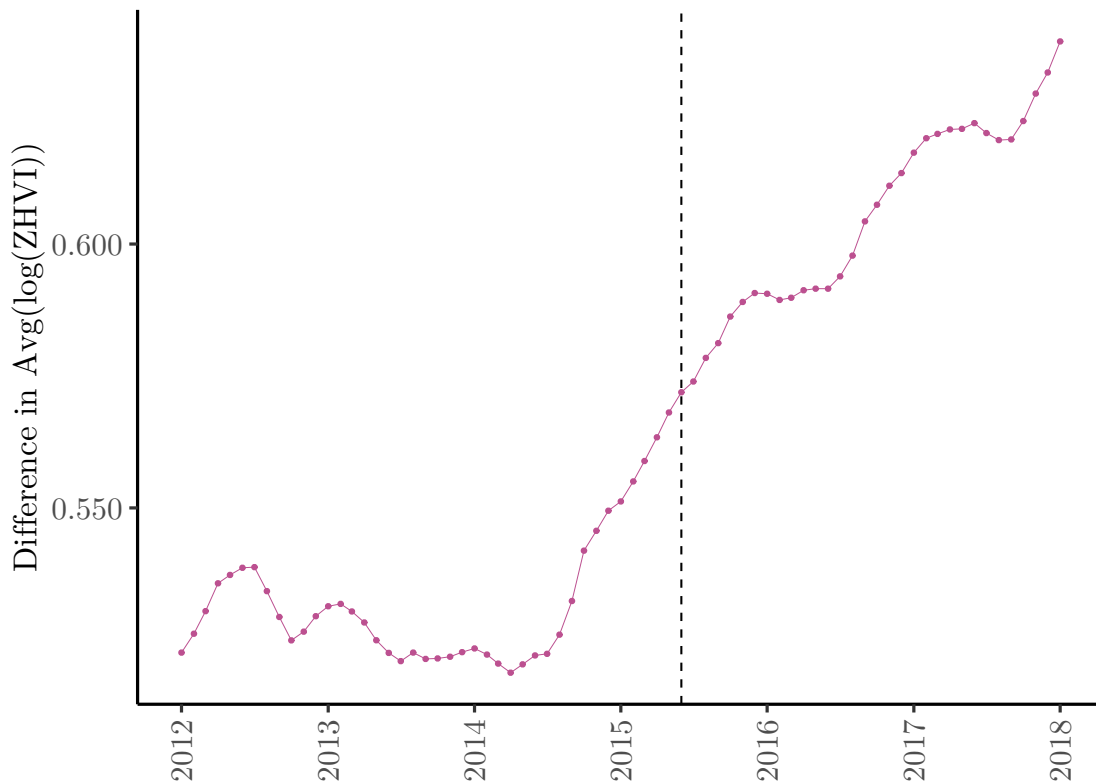
The parameter β_3 is identified if the pre-treatment differences in the outcome variable are the same (parallel pre-trends), and if the treatment itself did not generate spillovers that affect the control groups (see, for example, [Hansen et al., 2020](#)). We note that Santa Monica rests at the western edge of LA County, and, per [Table B.1](#), the number of Airbnb listings within Santa Monica before the reform numbered approximately 1/16th of those in the City of Los Angeles. We thus conclude that it is unlikely that Santa Monica’s reform affected the number of STRs in the City of Los Angeles (and thus could have affected LA house prices through the STR channel).²⁸

To evaluate the parallel pre-trend assumption, we plot the difference between Santa Monica and Los Angeles in the average of the log of housing prices over time, where the average is taken over the postal codes that comprise each city, in [Figure F.1](#). We begin our analysis in 2012, the “trough” of housing prices in the region in the wake of the Great Recession. The difference is relatively constant for several years prior to the reform, but increases starting in mid-2014 (in levels, both cities were experiencing increases in housing prices at this time). It is for this reason that we pursue a synthetic control approach as our

²⁸In [Appendix Figure B.1](#) we plot the time series of listings in postal codes immediately bordering Santa Monica. Though the number of observations prior to the reform is limited, significant spillovers are not apparent.

primary method for investigating the effects of Santa Monica’s STR regulation.

Figure F.1: Differences in housing prices between Santa Monica and Los Angeles



Notes: Data come from the Zillow House Value Index. Observations are the difference in the average log of housing prices between Santa Monica and Los Angeles, where the average is taken over the postal-code-month observations for each city. The date of the reform is shown by a vertical dashed line.

The top panel of Table F.1 reports estimates of Equation (10) using a bandwidth of 36 months to avoid the effects of the Great Recession.²⁹ In Column (1), we do not include fixed effects—in Column (2) we add area code fixed effects, and in Column (3) we add year fixed effects. The point estimates are statistically identical across all three specifications—we estimate that the reform increased housing prices in Santa Monica by 7.7% relative to a counterfactual of no reform. This estimate roughly conforms to the estimates in Section 5; the reform decreased Airbnb listings in Santa Monica by approximately 15%, which per Table 2 should generate an increase in housing prices of approximately 4%.

In the bottom panel of Table F.1, we perform a falsification test by altering the timing

²⁹In an earlier draft of this paper, we used the optimal bandwidth technique of Imbens and Kalyanaraman (2011) to perform this analysis, which suggested a longer bandwidth overlapping the Great Recession.

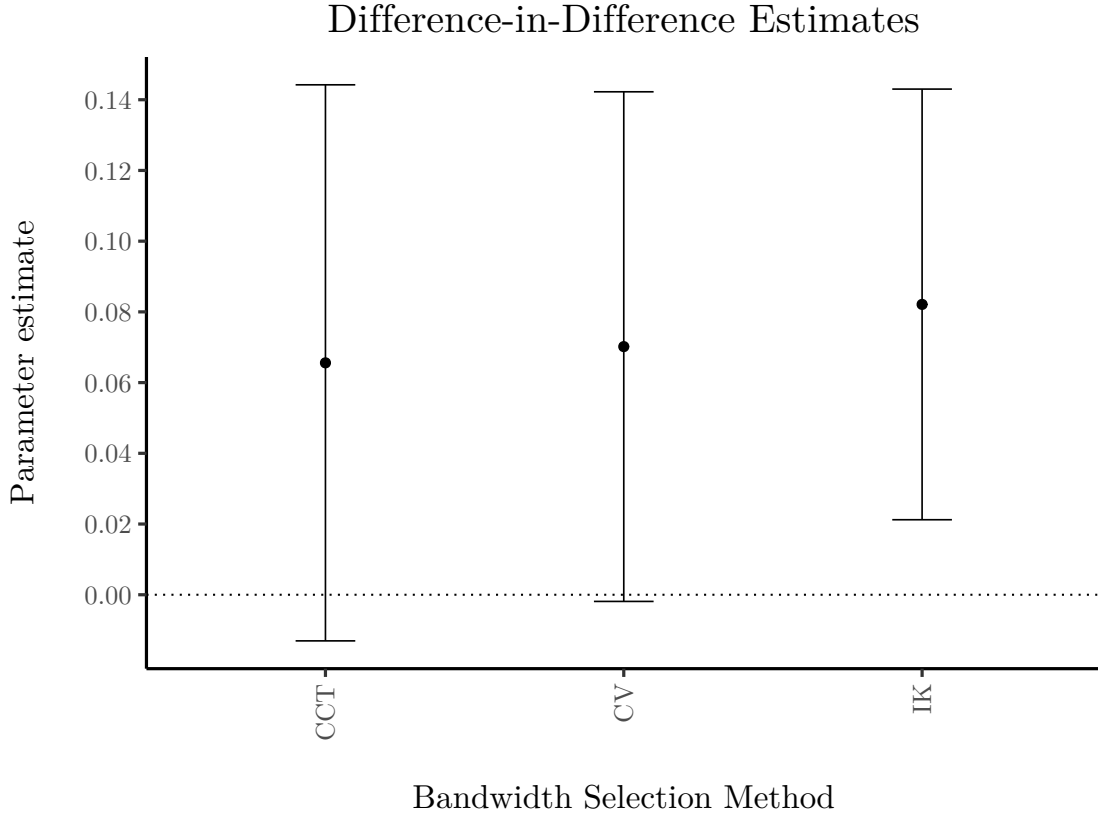
of the STR regulation to 24 months after the true date as a placebo reform. The point estimates are both smaller in magnitude than the estimates for the true reform and are imprecisely estimated. As an additional robustness check, we re-estimate Equation (10) explore using the methods of Calonico et al. (2014), Ludwig and Miller (2007), and Imbens and Kalyanaraman (2011) to select bandwidths. The coefficient estimates are presented in Figure F.2 along with 95% confidence intervals. Note that using the selection method of CCT and CV, we cannot reject the null hypothesis that the effect of the policy was different than zero. Hence, we view our results as providing evidence that the policy *may* have increased housing prices—but more conservatively—could have also done nothing. In no specification do we find a reduction in housing prices from the policy.

Table F.1: The effect of Santa Monica’s STR regulation on housing prices

	(1)	(2)	(3)
<i>True reform</i>			
Santa Monica \times post reform	0.077** (0.035)	0.077** (0.035)	0.077** (0.033)
R ²	0.179	0.283	0.328
Area FEs?	No	Yes	Yes
Year FEs?	No	No	Yes
Num. obs.	7690	7690	7690
<i>Placebo reform</i>			
Santa Monica \times post reform	0.039 (0.047)	0.039 (0.047)	0.039 (0.046)
Area FEs?	No	Yes	Yes
Year FEs?	No	No	Yes
R ²	0.130	0.249	0.259
N	5029	5029	5029

Notes: This table reports estimates of Equation (10) using a bandwidth of 36 months (see text for details). An observation is a postal-code-month. The dependent variable is the log of the Zillow Home Value Index. Heteroskedastic-robust standard errors are in parentheses. Stars indicate p values: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Figure F.2: Alternative bandwidth techniques



Notes: This figure illustrates point estimates and 95% confidence intervals for estimates of the effect of Santa Monica’s STR regulation on housing prices per Equation (10) using different bandwidth selection techniques. CCT refers to [Calonico et al. \(2014\)](#), CV refers to [Ludwig and Miller \(2007\)](#), and IK refers to [Imbens and Kalyanaraman \(2011\)](#).

G Exploring Other Police Call Types

In this section we explore trends in other types of police calls that may be generated by STR activity. We explore party complaints and loud music complaints. Summary statistics on calls in these categories are reported in Table [G.1](#). For each of these, we estimate an event study (for comparison, we include a similar estimation for public intoxication calls). For call type i in time t we estimate the parameters of

$$\log(Y_{it}) = \alpha_{i0} + \alpha_{i1} \times \text{post}_t + \alpha_{i2} \times t + \alpha_{i3} \times \text{post}_t \times t + FX_i + \varepsilon_{it} \quad (11)$$

Table G.1: Summary statistics for Santa Monica police calls

Variable	2013	2014	2015	2016	2017	2018	2019	% Δ 14-19
Loud music	121 (26)	147 (29)	134 (29)	139 (26)	137 (33)	122 (13)	113 (28)	-23.1
Party complaint	100 (35)	93 (24)	86 (22)	84 (28)	71 (17)	58 (18)	57 (21)	-38.7
Public intoxication	103 (19)	110 (23)	91 (17)	82 (8)	69 (14)	62 (12)	48 (11)	-56.4
All others	9,832 (758)	10,712 (726)	10,622 (996)	10,531 (837)	10,707 (807)	10,731 (735)	10,054 (647)	-6.14

Notes: Entries are averages across monthly observations over each year. Standard deviations are reported in parentheses.

where FX_i refer to month-of-year fixed effects.

The results are reported in Table G.2. We illustrate these estimates with the raw data in Figure G.1.

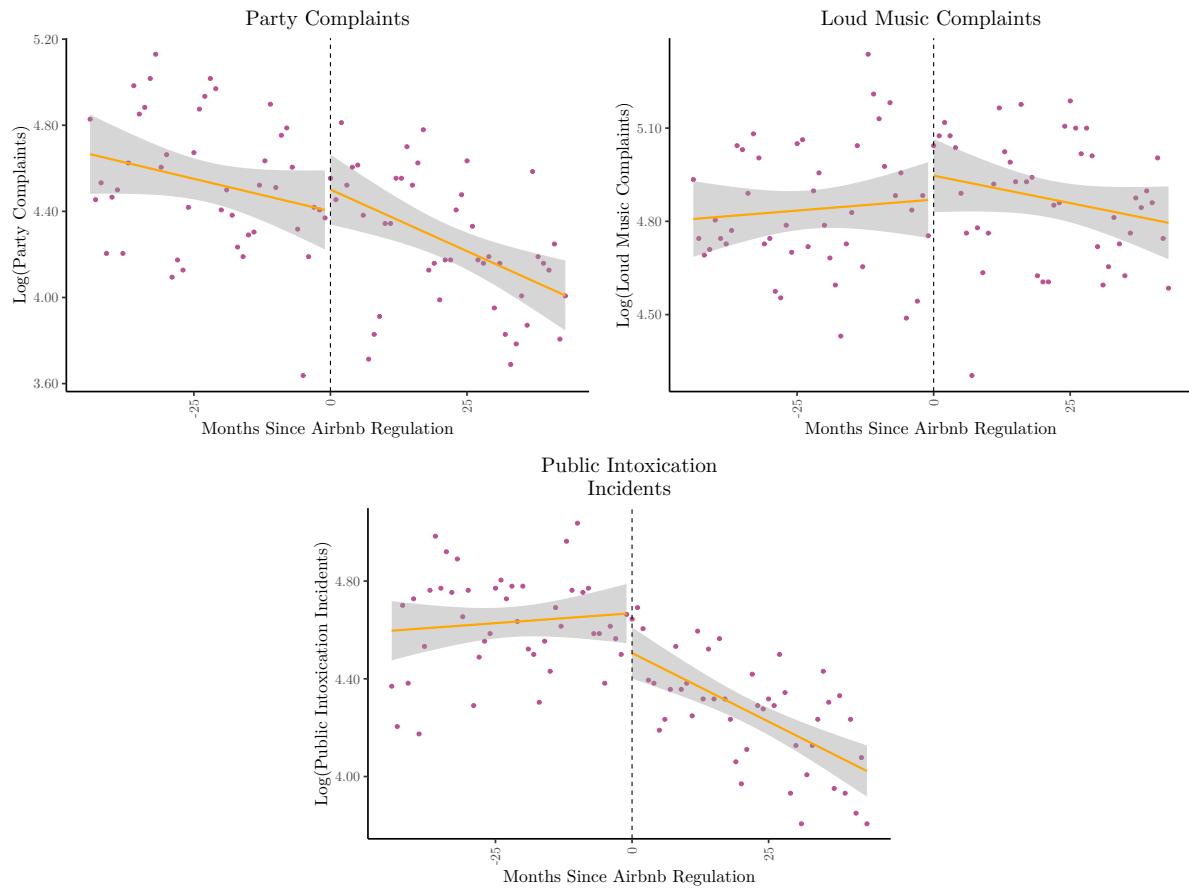
Table G.2: Event study evidence of Santa Monica’s STR regulation effects on various police call types

	Party	Party	Noise	Noise	Intox	Intox
post	0.093 (0.127)	0.016 (0.082)	0.071 (0.094)	0.023 (0.072)	-0.151** (0.074)	-0.183*** (0.066)
t	-0.005 (0.004)	-0.006*** (0.002)	0.002 (0.002)	0.001 (0.002)	0.001 (0.003)	0.000 (0.002)
$t \times \text{post}$	-0.006 (0.005)	-0.004 (0.003)	-0.005* (0.003)	-0.004 (0.002)	-0.012*** (0.003)	-0.011*** (0.003)
Month-of-year FEs?	No	Yes	No	Yes	No	Yes
R ²	0.282	0.761	0.040	0.610	0.572	0.738
N	87	87	87	87	87	87

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Notes: This table reports estimates of Equation (11). The bandwidth is chosen per the optimal bandwidth technique of Imbens and Kalyanaraman (2011). An observation is a month-year. The dependent variable is the log of the number of police calls reported in Santa Monica for one of three call types: party complaints, noise complaints, or public intoxication complaints. Heteroskedastic robust standard errors are in parentheses. Stars indicate p-values: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Figure G.1: Event studies for nuisance police calls in Santa Monica



Letter to the Editor

Dear Editor –

Thank you very much for the opportunity to revise our paper, *In Search of Peace and Quiet: The Heterogeneous Impacts of Short-Term Rentals on Housing Prices*, RSUE-D-21-00204, for further consideration at *Regional Science and Urban Economics*. We appreciate the care and thoughtfulness with which you and the referees considered our original submission. The paper is immeasurably better for that feedback.

We appreciate your flexibility with respect to the resubmission deadline – as two members of our team were on the job market this year, that flexibility was crucial. We note that the outcome of this market involved one member of our authorship team (Brett Garcia) accepting and starting a position at Booking.com during the revision process. Booking.com played no role in the funding, design, analysis, or authorship of either the original submission or this revision.

We proceed by summarizing the major changes to the paper, and then respond in detail to each referee’s report. We include the original comments in italics and respond in-line.

Summary of major changes

1. We have substantially updated our analysis of heterogeneous effects of Airbnb listings on housing prices in LA County. Our specifications include city-level fixed effects to account for factors influencing housing prices which are constant over time and within a city but vary across zip codes. We have added a test of our primary qualitative result (that the marginal impact of Airbnb may be negative in certain jurisdictions) suggested by a referee: we re-sample the data using the Wild bootstrap and re-estimate our preferred specification. In each re-sample, we estimate at least five negative coefficients. In an appendix, we follow the suggestions of [Borusyak et al. \(2022\)](#) and [Goldsmith-Pinkham et al. \(2020\)](#) and use a first-differenced specification with a shift-share instrument. Across specifications, we find evidence of substantial heterogeneity between jurisdictions within LA County.
2. We have re-analyzed the effect of Santa Monica’s STR regulation using a synthetic

control approach using all other California cities (alternatively, all U.S. cities) without STR regulations as potential ‘donor’ control units. The synthetic control unit is able to match the pre-trend in Santa Monica much more closely. The estimated average treatment effects are positive and consistent with our previous city-level estimates of the effect of marginal STR listings on housing prices, though inference is weak. We conclude that the exercise provides suggestive evidence that Santa Monica’s regulation may have increased local housing prices, and likely did not decrease them (as was the stated goal of the policy). We present this approach in the main body of the paper, and move our previous difference-in-differences approach to an Appendix.

3. We have added a placebo test to our analysis of police calls: we repeat our analysis using June 2013 as a placebo reform date. The placebo test gives a null result, increasing our confidence in our findings in Section 7.
4. We have substantially edited the text throughout, including substantial re-organizations of Section 2 and the Appendices.

Thank you again for considering our work at *Regional Science and Urban Economics*.

Sincerely,
Brett Garcia
Economist, Booking.com



Keaton Miller (corresponding author)
Assistant Professor of Economics, University of Oregon

John Morehouse
Ph.D. Candidate, University of Oregon

Responses to Referee 1

First: thank you! Your comments as well as those of the other referee have significantly improved our work and we are grateful for the time and care you invested in our original submission. We proceed below by summarizing the major changes to the paper and then responding to your comments. We reproduce your original comments in italics and respond in-line.

Summary of major changes

1. We have substantially updated our analysis of heterogeneous effects of Airbnb listings on housing prices in LA County. Our specifications include city-level fixed effects to account for factors influencing housing prices which are constant over time and within a city but vary across zip codes. We have added a test of our primary qualitative result (that the marginal impact of Airbnb may be negative in certain jurisdictions) suggested by a referee: we re-sample the data using the Wild bootstrap and re-estimate our preferred specification. In each re-sample, we estimate at least five negative coefficients. In an appendix, we follow the suggestions of [Borusyak et al. \(2022\)](#) and [Goldsmith-Pinkham et al. \(2020\)](#) and use a first-differenced specification with a shift-share instrument. Across specifications, we find evidence of substantial heterogeneity between jurisdictions within LA County.
2. We have re-analyzed the effect of Santa Monica’s STR regulation using a synthetic control approach using all other California cities (alternatively, all U.S. cities) without STR regulations as potential ‘donor’ control units. The synthetic control unit is able to match the pre-trend in Santa Monica much more closely. The estimated average treatment effects are positive and consistent with our previous city-level estimates of the effect of marginal STR listings on housing prices, though inference is weak. We conclude that the exercise provides suggestive evidence that Santa Monica’s regulation may have increased local housing prices, and likely did not decrease them (as was the stated goal of the policy). We present this approach in the main body of the paper, and move our previous difference-in-differences approach to an Appendix.

3. We have added a placebo test to our analysis of police calls: we repeat our analysis using June 2013 as a placebo reform date. The placebo test gives a null result, increasing our confidence in our findings in Section 7.
4. We have substantially edited the text throughout, including substantial re-organizations of Section 2 and the Appendices.

Response to comments

Main comments

1. *Identification strategy of Section 5:*

- (a) ***Regression equation:*** *The equation that should be taken to the data is the last equation on page 40. Equation (4) only informs about the sign of the regression coefficient of STR. When looking at equation on page 40, we see that the authors are missing many explanatory variables and their estimates may suffer from omitted variable bias that I doubt is fixed with their IV approach. An example is housing supply, which is positively and mechanically correlated with STR as well as negatively correlated with prices. Other variables that can lead to the same problem are other characteristics of neighborhoods that comove with STR and rental prices, such as services offered in such locations.*

Response: Thank you for pointing this out. We agree that our model abstracts from many important issues in this market including (as you mention) the construction of housing units. Both our modelling and empirical approaches are designed to be responsive to the literature: in the context of (a) published results that STRs generate increases in average housing prices in the aggregate (Barron et al., 2020) and (b) documented significant negative externalities on particular local jurisdictions (Fontana, 2021), our aim is to show in a parsimonious framework that the net effect of STRs on equilibrium jurisdiction-level housing prices is ambiguous. In other words, we seek to show that one does not need to take very much more into account (relative to existing analyses) in order to obtain different

results with direct policy-relevance to the jurisdictions in question.

We have updated the text on Page 12 and added a footnote on Page 18 to make this point more clear.

- (b) **Location fixed effects:** *I do not see how the identification of β_{1j} is precluded if zip-code fixed effects are included (or other geographies such as census tracts or census blocks) as its identification should come from temporal variation within a geographical unit. If zip-codes are varying over time for LA over this period (the same way census tracts do), the authors can simply fix geographies at some point in time. A possible approach, given they have individual data on STR, is to construct their own geographies (polygons on a shapefile) and perform a spatial merge. Moreover, adding only 11 area codes to control for fixed-geographical characteristics implies an identification assumption that feels a bit herculean. Concretely, with this design that controls for such large geographies, we are comparing neighborhoods within the city of LA and assuming there are no differences in unobservable fixed characteristics. For example, access to public transit or distance to the ocean/mountains/valleys may differ substantially across LA neighborhoods and correlate with the presence of STR.*

Response: The challenge with zip-code fixed effects in this context is that zip codes are generally coterminous with city boundaries (see e.g. <http://www.laalmanac.com/communications/cm02a90001-90899.php>). As you say, we do wish to control for some set of unobservable fixed characteristics in geography. We agree that area codes are far from ideal for this purpose. In our revision, we use city fixed effects.

- (c) **Instrument:** *Its exclusion restriction feels implausible for several reasons, despite that similar strategies have been used in many other papers in the past. As a mentioned before, I think their identification strategy is a point of the paper that the authors need to think very carefully. First, if there is time persistence in the number of NAICS 72 establishments, which can be tested in the data, their instrument would not be valid as STR would be correlated with concurrent NAICS*

72 establishment counts. Second, there has been a lot of recent development in the literature of these type of interaction instruments, that are a subset of the shift-share instruments. Concretely, when shares are incomplete (the share part does not sum up to one), the IV estimate will be biased. See Borusyak, Hull, and Jaravel (2020), in particular Section 4.2 for a more formal discussion of these incomplete shares.

Response: Thank you for this insightful comment. Note that in this discussion we refer to the December 2020 version of the Borusyak et al. paper available via <https://sites.google.com/view/borusyak/research>, which we refer to here as BHJ. Our setting is panel data and estimation, and so the discussion of Section 4.3 is key. To map our notation to BHJ’s: our z (postal code) is their l (observation). As we use a single shock per period, our use of the framework mirrors Nunn and Qian (2014) and the other papers cited in BHJ’s Footnote 30 (Page 17). Following the discussion in the fourth paragraph of Section 4.3, we note that the sum of our ‘exposure shares’ (s_{lnt} in their notation, b_{zj}^{2010} in ours) is constant across time periods (i.e. as they discuss on Page 17, $s_{lnt} \equiv s_{ln0}$) and thus the incomplete shares problem of Section 4.2 does not apply in our setting.

Furthermore, as BHJ discuss on Page 17, since our exposure shares are fixed across time periods, our use of geographic fixed effects purge “both time-invariant unobservables ($\frac{1}{T} \sum_{\tau} \epsilon_{l\tau}$) from the residual and the time-invariant component of the shocks ($\frac{1}{T} \sum_{\tau} g_{n\tau}$).” In other words, to the extent that the correlation between 2010 NAICS 72 establishments and present-day NAICS 72 establishments is a time-invariant function of the city, our fixed effects purge that correlation from the residual. This argument mirrors the discussion of BHJ’s Footnote 32 – we are “fixing the shares in a pre-period [i.e. using 2010 NAICS 72 establishments]” because “the current shares [i.e. the current number of NAICS 72 establishments] are affected by lagged shocks [i.e. last month’s/year’s interest in STRs] in a way that is correlated with unobservables”. We have added a footnote discussing the comparison to BHJ on Page 19.

That said, it is possible that there are time-varying within-city unobservables that are nonetheless correlated with b_{zj}^{2010} , threatening identification. We therefore re-estimate our model in first differences using a more traditional shift-share instrument in Appendix D.1. Relative to the estimates in the main body of the paper, we find that additional Airbnb listings reduce housing prices in more jurisdictions within LA County; our qualitative result thus remains unchanged. We discuss the possible bias you identify on Page 46.

2. *Diff-in-Diff strategy for rental prices of Section 6:*

- (a) *I simply do not understand what's new thing we learn from Section 6.1 that we have not already learnt in Section 5.*

Response: We apologize for the confusion. Section 6 and 6.1 (now Section 7) are intended to provide corroborating evidence for our underlying thesis that STRs can generate net negative pressure on housing prices. After estimating negative effects in certain jurisdictions, one could conclude that a policy banning STRs in those jurisdictions would lead to increases in housing prices. From our perspective it is an additional contribution to provide evidence that an actual STR restriction implemented in a jurisdiction in which we estimate a negative relationship between STRs and housing prices *did indeed* lead to an increase in housing prices. It is furthermore valuable to provide evidence of the negative externality mechanism we propose in the form of police calls. By way of an imperfect analogy to the epistemology of medicine, it is one thing to demonstrate in a model that an RNA vaccine may be effective against SARS-Cov-2 (our Section 3), it is another thing to demonstrate that the vaccine leads human systems to generate antibodies and that those antibodies have neutralizing effects against the virus (approximately our Sections 5 and 7), and it is yet another thing to demonstrate that the vaccine leads to protection against COVID-19 illnesses (our Section 6).

Or, using a more negative/skeptical frame: a reader may reasonably come away

from Section 5 still somewhat unconvinced by our identification strategy and therefore may not believe the policy implication. Our direct tests of the policy implication and associated mechanism, though of course themselves imperfect, may help convince the reader of the broader point.

- (b) *I think pre-trends are a real problem here. I worry that the effect that they find is simply capturing the divergence between LA and Santa Monica that started almost a year before the policy was implemented. Even the argument that authors have about anticipation effects feels a bit contradictory with their other argument about lagged enforcement. Moreover, they find an effect that is almost twice as large compared to Section 5 (7.7% increase versus 4%).*

Response: We agree that LA is an imperfect control. In our revision, we replace the difference-in-differences approach with a synthetic control exercise using other cities across the entire state of California that did not implement an STR policy as potential control units. As we write on Page 26, “Aggregated over the post-treatment period, the estimated ATT is 0.1031, with a p-value of 0.2280 (standard error 0.0855). Though noisy, the point estimate conforms to the estimates of Table 2: using the number of entire unit listings from 2015 to 2016, $(\ln(616) - \ln(466)) \times -0.267 = -0.075$. We conclude that the reform may have increased housing prices, and probably did not *reduce* housing prices, as was the stated intent.” In other words, in comparing our original submission to our revision, we are able to match the pre-trend in Santa Monica much more precisely, our qualitative conclusion did not change, and the difference in the point estimates between the two exercises narrowed somewhat.

We have moved the traditional difference-in-differences exercise to Appendix F on Page 50.

- (c) *To separate anticipation effects, the authors could in principle design two treatments: one when the policy is discussed/announced and the other one when it’s actually implemented. It would be easy to find information about the announcement or discussion dates doing some simple internet search.*

Response: The synthetic control approach makes this comment largely moot as we are able to generate a synthetic control group with better pre-trend fit.

- (d) *Another placebo test is to compare LA with a city where there was no STR ordinance.*

Response: The synthetic control approach makes this comment largely moot.

- (e) *Why don't you need linear trends for regression (6) but you do for regression (7) if we expect a lag in the ordinance enforcement for both?*

Response: Thank you for pointing this out. If STRs affect housing prices directly through the option value of renting, then passing the ordinance is a strong signal that the option value is smaller than in the pre-ordinance world, even if the implementation takes some time. On the other hand, the negative externalities we analyze are generated by the presence of STRs “on the ground” (so to speak), and thus any delay in implementation/enforcement will be mirrored in the externality data.

- (f) *Overall, my recommendation is to remove 6.1 and put it in an Appendix, given the length of the paper, given the issues it brings and that the contribution in the presence of Section 5 is minimal.*

Response: Thank you for this suggestion. In light of our above comments, we have left this in the main body of the paper for the time being. However, we have worked to update the exposition of the section to make the contribution more clear.

Minor comments

1. **Other papers:** *I am very surprised that the authors do not cite “Short-term rentals and the housing market: Quasi-experimental evidence from Airbnb in Los Angeles” by Koster, van Ommeren, & Volkhausen (2021). They only not use similar data, but also study the effects of STR on rental prices. In fact, this paper finds very small or null effects of STR on prices. Thus this new analysis can help shed-light of Koster et al. (2021) counter-intuitive results. Another paper that can help guide the theoretical*

discussion and deals with a similar externality issue is Almagro & Domniguez-Iino (2020). In that paper, the authors use a structural model to tease out the different mechanisms through which STR may affect rental prices. They also find evidence that STR are associated with negative externalities.

Response: We apologize for the omission of these papers from our original draft. We have added discussions of these papers in the Introduction (see Page 5).

2. **Section 2** feels rather long. I don't know how much we learn from 2.1 other than Los Angeles County is very heterogeneous. Section 2.2 can be condensed to one paragraph. Section 2.3 contains many details that are not necessary at that point and may be more helpful in the main analysis of the paper of Sections 5 and 6.

Response: Thank you for this suggestion. We have simplified Section 2 along the lines you propose.

3. **Table 2:** This table can be better summarized in a couple of graphs.

Response: Thank you for this suggestion. We have moved the original Table 2 to an Appendix and replaced it with a pair of graphs in the new Figure 2.

4. **Model:** One disconcerting issue is that the authors are completely ignoring cross-elasticities, despite laying out a model where neighborhoods are substituted of each other. Concretely, changes in exogenous parameters such as R_1 will also have effects on ϕ_1 . It is easy to see this by looking at equation (3) on page 40. An increase in R_1 affects P_1 , which in turn affects Φ_j for all $j \neq 1$. As Φ_j moves, P_j will also need to be adjusted to restore market clearing in other locations $j \neq 1$. In other words, as neighborhoods are substitutes, there are indirect equilibrium effects of moving R_1 in other neighborhoods because prices are simultaneously determined. These indirect effects may go in either direction, contributing to the general line of thought of the paper. In general, as one can expect these to be small relative to the direct effects and that a more formal analysis may be cumbersome, as a referee I would advice to make a note of the presence of indirect effects but that they are ignored in the current analysis for the reasons presented above.

Response: Thank you for pointing this out. We have updated our discussion of Equations (3) and (4) on Page 14 to reflect this point.

5. *Further comments on regression 5:*

- Something that the authors are not controlling for is that STR units tend to be of smaller size. I believe that both Zillow as well as their web-scrapes contains data on the size of the units. Observe that ignoring this feature would bias downward the coefficients.

Response: Thank you for this comment. We agree that STR units are likely to be smaller than the median home size within any particular zip code. To the extent that the positioning of STR units within the distribution of homes is the same within a region of LA County, our fixed effects should account for that positioning. Our estimates could be biased if, for a rough example, STR units in Manhattan Beach were on average at the 25th percentile of size for homes in Manhattan Beach while STR units in Rancho Palos Verdes (both in the South Bay region of LA County) were on average at the 50th percentile for homes in Rancho Palos Verdes. Unfortunately, our Zillow data does not contain information about the average size of homes at the zip code level. We have added a footnote discussing this point on Page 19.

- On the second paragraph of page 20, the authors should explain or give examples of what type of unobservables may bias the OLS coefficients. I would also report OLS coefficients of regression 5, in a similar way to Table 4, to see the size and direction of the bias of OLS coefficients.

Response: Thank you for this suggestion – we have added examples of potential sources of bias when discussing the instrument (now on Page 19) and added OLS results to Table 2 on Page 21.

Responses to Referee 2

First: thank you! Your comments as well as those of the other referee have significantly improved our work and we are grateful for the time and care you invested in our original submission. We proceed below by summarizing the major changes to the paper and then responding to your comments. We reproduce your original comments in italics and respond in-line.

Summary of major changes

1. We have substantially updated our analysis of heterogeneous effects of Airbnb listings on housing prices in LA County. Our specifications include city-level fixed effects to account for factors influencing housing prices which are constant over time and within a city but vary across zip codes. We have added a test of our primary qualitative result (that the marginal impact of Airbnb may be negative in certain jurisdictions) suggested by a referee: we re-sample the data using the Wild bootstrap and re-estimate our preferred specification. In each re-sample, we estimate at least five negative coefficients. In an appendix, we follow the suggestions of [Borusyak et al. \(2022\)](#) and [Goldsmith-Pinkham et al. \(2020\)](#) and use a first-differenced specification with a shift-share instrument. Across specifications, we find evidence of substantial heterogeneity between jurisdictions within LA County.
2. We have re-analyzed the effect of Santa Monica’s STR regulation using a synthetic control approach using all other California cities (alternatively, all U.S. cities) without STR regulations as potential ‘donor’ control units. The synthetic control unit is able to match the pre-trend in Santa Monica much more closely. The estimated average treatment effects are positive and consistent with our previous city-level estimates of the effect of marginal STR listings on housing prices, though inference is weak. We conclude that the exercise provides suggestive evidence that Santa Monica’s regulation may have increased local housing prices, and likely did not decrease them (as was the stated goal of the policy). We present this approach in the main body of the paper, and move our previous difference-in-differences approach to an Appendix.

3. We have added a placebo test to our analysis of police calls: we repeat our analysis using June 2013 as a placebo reform date. The placebo test gives a null result, increasing our confidence in our findings in Section 7.
4. We have substantially edited the text throughout, including substantial re-organizations of Section 2 and the Appendices.

Response to comments

1. *I am very sympathetic to the thesis of the paper. Based on feedback from communities, many people are concerned about the negative amenities brought by Airbnb induced tourism. However, I am not convinced that the empirical results in the paper are meaningful. Figure 1 shows the distribution of estimated coefficients for cities in LA county. Only a handful of cities have a negative estimated coefficient. Moreover, the distribution of coefficients approximates a slightly skewed bell curve. In any empirical setting with measurement error or other noise, this distribution is what we would expect, even if the true effect is always positive. Thus, it's not clear to me that a few estimated negative coefficients are evidence that Airbnb may decrease house prices in some neighborhoods. To address this issue, the authors should conduct multiple hypothesis testing, e.g. "If the data was sampled many times, what is the probability that we would estimate 7 cities with negative coefficients?"*

Response: Thank you very much for this suggestion. In our revision, we address this concern by conducting 10,000 Wild Bootstrap resamples for our most saturated specification, Column (6) of Table 2. The results are reported in Figure 4 on Page 23. In 83% of trials, we estimate at least 7 negative coefficients. Indeed, there are no resamples for which we estimate uniformly positive coefficients.

2. *I am also not convinced by the analysis of Santa Monica's STR regulation. Figure 3 shows a clear pre-trend happening prior to the implementation of the regulation. It is very unlikely that this is due to an anticipation effect, given that the effect preceded the regulation by over a year, in which there was great uncertainty about passage of the law and degree of enforcement. Santa Monica was one of the first cities in the country to*

pass and enforce Airbnb regulation. Without other cities to look to for precedent, it is hard to believe that anticipation of the law would have an anticipatory effect of over 5% in housing values.

Response: We agree that LA is an imperfect control. In our revision, we have replaced this exercise with a synthetic control approach using all cities in California without STR regulations as potential control units. This large set of potential control units means that we are able to get a much closer pre-treatment match (see Figure 6). Our qualitative result is similar, though the inference is weaker. We conclude that the policy may have increased housing prices in Santa Monica and almost certainly did not decrease them.

3. *Moreover, Figure 3 shows a deceleration in house prices starting in 2016, about half a year after the passage of the STR ordinance. This coincides with the timing of the slowdown in Airbnb listings as shown in Figure 2. Couldn't we also interpret this as evidence that the STR regulation reduced house prices?*

Response: The replacement of our difference-in-differences approach with a synthetic control approach renders this comment moot.

4. *Koster et al. (2020), "Short-Term Rentals and the Housing Market: Quasi-Experimental Evidence from Airbnb in Los Angeles" also looks at the effect of STR regulations across cities in Los Angeles county. They find that STR regulations reduced rents and house prices, in contrast to your finding that house prices rose in Santa Monica. Can you reconcile the two findings?*

Response: Thank you for pointing this out – the omission was an oversight. We have added brief discussions of these papers in the Introduction (see Page 5). Koster et al. (2020) examine STR regulations across cities using a regression-discontinuity design around the cities' borders. They estimate that *on average across the county*, STR restrictions decrease housing prices by 2%, which is similar to our finding that on average across the county, additional STR listings increase housing prices.

5. *Figure 4 doesn't show a convincing change in public intoxication after the regulation,*

either in levels or in trends. It actually looks to me like public intoxication complaints were already trending downwards, though the scale of the graph makes it difficult to see. An easy test is to do a placebo regression where the regulation date is taken to be 2013 instead of 2015.

Response: In our original submission, that figure was mislabelled. Instead of representing only public intoxication calls, it included all “party related” calls. You can see this in the original submission by comparing the level of the data in that figure (often near 6) to the level of the data in Figure A.10, which was restricted to public intoxication calls and correctly labelled (around 4.4). We apologize for the error and are grateful for the opportunity to fix it.

The revised Figure (now Figure 7 on Page 29) now features only public intoxication calls. That said, we agree that even this revised figure does not show an obvious discontinuous drop that sometimes features in these exercises. After conversing with local officials, our understanding is that enforcement lags are responsible for this feature of the data – in particular the fact that existing reservations were honored, and so the number of STRs actually in use at any particular point post-reform may have been larger than the number of listings for future bookings. We discuss this on Page 30. We account for the possibility of a long-run trend in public intoxication calls in our analysis with the addition of linear time interactions. Our results indicate that while there was indeed a downward trend for these calls in the pre-period, the trend accelerated in the post-period.

Per your suggestion, we have added a placebo regression to Table 3. The relevant coefficients are indistinguishable from zero, increasing our confidence in our result.

6. *The discussion of Santa Monica, Burbank, and San Gabriel seems too speculative. For San Gabriel and Burbank it is argued that Airbnb has a negative effect on their prices because they are adjacent to the actual tourist destinations. But Airbnb can still boost local businesses in tourist-adjacent cities, so why should we automatically expect a negative effect on amenities? Moreover, for Santa Monica that explanation doesn't work because Santa Monica is the tourist destination. In light of comment #1,*

it doesn't seem like we should draw too much conclusion about the fact that negative coefficients were found for a handful of cities.

Response: Thank you for this comment. We have removed this discussion from the draft.

7. *This may be more a matter of taste, but I do not think the model adds much to the paper, and the authors could consider removing it in favor of a more descriptive discussion. The insights of the model seem fairly straightforward and intuitive and it doesn't seem like the machinery of the model was needed.*

Response: In general we have found that the inclusion of the model has helped some readers understand the intuition behind our results, especially in the context of existing models in which the relationship between STRs and housing prices is unambiguous. We believe there is some benefit to readers to illustrate that the existing result can be overturned with a small plausible change to the model.