The Effect of Home-Sharing on House Prices and Rents: Evidence from Airbnb*

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Abstract

We assess the impact of home-sharing on residential house prices and rents. Using a dataset of Airbnb listings from the entire United States and an instrumental variables estimation strategy, we show that Airbnb has a positive impact on house prices and rents. This effect is stronger in zipcodes with a lower share of owner-occupiers, consistent with non-owner-occupiers being more likely to reallocate their homes from the long- to the short-term rental market. At the median owner-occupancy rate zipcode, we find that a 1% increase in Airbnb listings leads to a 0.018% increase in rents and a 0.026% increase in house prices. Finally, we formally test whether the Airbnb effect is due to the reallocation of the housing supply. Consistent with this hypothesis, we find that, while the total supply of housing is not affected by the entry of Airbnb, Airbnb listings increase the supply of short-term rental units and decrease the supply of long-term rental units.

Keywords: Sharing economy, peer-to-peer markets, housing markets, Airbnb

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1 Introduction

The sharing economy represents a set of peer-to-peer online marketplaces that facilitate matching between demanders and suppliers of various goods and services. The suppliers in these markets are often small (mostly individuals), and they often share excess capacity that might otherwise go unutilized—hence the term "sharing economy." Economic theory would suggest that the sharing economy improves economic efficiency by reducing frictions that cause capacity to go underutilized, and the explosive growth of sharing platforms (such as Uber for ride-sharing and Airbnb for home-sharing) testifies to the underlying demand for such markets.[1] The growth of the sharing economy has also come at the cost of great disruption to traditional markets (Zervas et al., 2017) as well as new regulatory challenges, leading to contentious policy debates about how best to balance individual participants' rights to freely transact, the efficiency gains from sharing economies, the disruption caused to traditional markets, and the role of the platforms themselves in the regulatory process.

Home-sharing, in particular, has been the subject of intense criticism. Namely, critics argue that home-sharing platforms like Airbnb raise the cost of living for local renters while mainly benefitting local landlords and non-resident tourists.[2] It is easy to see the economic argument. By reducing frictions in the peer-to-peer market for short-term rentals, home-sharing platforms cause some landlords to switch from supplying the market for long-term rentals—in which residents are more likely to participate—to supplying the short-term market—in which non-residents are more likely to participate. Because the total supply of housing is fixed or inelastic in the short run, this drives up the rental rate in the long-term market. Concern over home-sharing's impact on housing affordability has garnered significant attention from policymakers and has motivated many cities to impose stricter regulations on home-sharing.[3]

[1]These frictions could include search frictions in matching demanders with suppliers and information frictions associated with the quality of the good being transacted or with the trustworthiness of the buyer or seller. See Einav et al. (2016) for an overview of the economics of peer-to-peer markets including the specific technological innovations that have facilitated their growth.

[2]Another criticism of Airbnb is that the company does not do enough to combat racial discrimination on its platform (Edelman and Luca, 2014; Edelman et al., 2017) or that it generates negative externalities for neighbors (Filippas and Horton, 2018) though we will not directly address these issues in this paper.

[3]For example, Santa Monica outlaws short-term, non-owner-occupied rentals of fewer than 30 days as does New York State for apartments in buildings with three or more residences. San Francisco passed a 60-day annual hard cap on short-term rentals (which was subsequently vetoed by the mayor). It is unclear, however, to what degree to which these regulations are enforced.

Whether or not home-sharing increases housing costs for local residents is an empirical question. There are a few reasons why it might not. The market for short-term rentals may be very small compared to the market for long-term rentals. In this case, even large changes to the short-term market might not have a measurable effect on the long-term market. The short-term market could be small—even if the short-term rental rate is high relative to the long-term rate—if landlords prefer more reliable long-term tenants and a more stable income stream. Alternatively, it is possible that home-sharing simply does not cause much reallocation from the long-term rental stock to the short-term rental stock. Owner-occupiers—those who own the home in which they live—may supply the short-term rental market with spare rooms and cohabit with guests or they may supply their entire home during temporary absences,[4] but either way, the participation of owner-occupiers in the short-term rental market may not cause a reallocation from the long-term rental stock if these housing units are still primarily used as long-term rentals in the sense that the owners are renting long-term to themselves. Another type of participation in the short-term rental market that would not result in reallocation is vacation homes that would not have been rented to long-term tenants anyway, perhaps due to the restrictiveness of long-term leases causing vacation home-owners to not want to rent to long-term tenants. In this case, the vacation home units were never part of the long-term rental stock to begin with. In either case, whether owner-occupiers or vacation-home owners, these homes would not be made available to long-term tenants independently of the existence of a home-sharing platform. Instead, home-sharing provides these owners with an income stream for times when their housing capacity would otherwise be underutilized.

In this paper, we study the effect of home-sharing on residential house prices and rents using a comprehensive dataset of all U.S. properties listed on Airbnb, the world's largest home-sharing platform. The data are collected from public-facing pages on the Airbnb website between 2012 and the end of 2016, covering the entire United States. From this data, we construct a panel dataset of Airbnb listings at the zipcode-year-month level. From Zillow, a website specializing in residential real estate transactions, we obtain a panel of house price and rental rate indices, also at the zipcode-year-month level. Zillow provides a platform for matching buyers and sellers in the housing market and landlords with tenants in the long-term rental market; thus, their price measures reflect sale

[4] A frequently cited example is that of the flight attendant who rents out his or her home on Airbnb while traveling for work.

prices and rental rates in the market for long-term housing. Finally, we supplement this data with a rich set of time-varying zipcode characteristics collected from the Census Bureau's American Community Survey (ACS) and a set of variables correlated with tourism demand such as hotel occupancy rates from STR, airport travelers from the Bureau of Transportation Statistics (BTS), and hotels' online reviews from TripAdvisor.

In the raw correlations, we find that the number of Airbnb listings in zipcode i in year-month t is positively associated with both house prices and rental rates. In a baseline OLS regression with no controls, we find that a 1% increase in Airbnb listings is associated with a 0.1% increase in rental rates and a 0.18% increase in house prices. Of course, these estimates should not be interpreted as causal and may instead be picking up spurious correlations. For example, cities that are growing in population likely have rising rents, house prices, and numbers of Airbnb listings at the same time. We therefore exploit the panel nature of our dataset to control for unobserved zipcode level effects and arbitrary city level time trends. We include zipcode fixed effects to absorb any permanent differences between zipcodes while fixed effects at the Core Based Statistical Area (CBSA)-year-month level control for any shocks to housing market conditions that are common across zipcodes within a CBSA.[5]

We further control for unobserved *zipcode-specific, time-varying* factors using an instrumental variable that is plausibly exogenous to local zipcode level shocks to the housing market. To construct the instrument, we exploit the fact that Airbnb is a young company that has experienced explosive growth over the past five years. Figure 1 shows worldwide Google search interest in Airbnb from 2008 to 2016. Demand fundamentals for short-term housing are unlikely to have changed so drastically from 2008 to 2016 as to fully explain the spike in interest, so most of the growth in Airbnb search interest is likely driven by information diffusion and technological improvements to Airbnb's platform as it matures as a company. Neither of these should be correlated with local zipcode level unobserved shocks to the housing market. By itself, global search interest is not enough for an instrument because we already control for arbitrary CBSA level time trends. We therefore interact the Google search index for Airbnb with a measure of how "touristy" a zipcode is in a base year, 2010. We define "touristy" to be a measure of a zipcode's attractiveness for tourists and

[5]The CBSA is a geographic unit defined by the U.S. Office of Management and Budget that roughly corresponds to an urban center and the counties that commute to it.

proxy for it using the number of establishments in the food service and accommodations industry.[6] These include eating and drinking places as well as hotels, bed and breakfasts, and other forms of short-term lodging. The identifying assumptions of our specification are that: 1) Landlords in more touristy zipcodes are more likely to switch into the short-term rental market in response to learning about Airbnb than landlords in less touristy zipcodes and 2) ex-ante levels of touristiness are not systematically correlated with ex-post unobserved shocks to the housing market at the zipcode level *that are also correlated in time with Google search interest for Airbnb*. We discuss the instrument, its construction, and exercises supporting the exclusion restriction in more detail in Sections 5, 5.1, and in the Appendix B.

Using this instrumental variable, we estimate that for zipcodes with the median owner-occupancy rate (72%), a 1% increase in Airbnb listings leads to a 0.018% increase in the rental rate and a 0.026% increase in house prices. We also find that the effect of Airbnb listings on rental rates and house prices is decreasing in the owner-occupancy rate. For zipcodes with a 56% owner-occupancy rate (the 25th percentile), the effect of a 1% increase in Airbnb listings is 0.024% for rents and 0.037% for house prices. For zipcodes with an 82% owner-occupancy rate (the 75th percentile), the effect of a 1% increase in Airbnb listings is only 0.014% for rents and 0.019% for house prices. These results are robust to a number of sensitivity and robustness checks that we discuss in detail in Sections 5.1 and 6.2.

The fact that the effect of Airbnb is moderated by the owner-occupancy rate suggests that the effect of Airbnb could be driven by non-owner occupiers being more likely (because of Airbnb) to reallocate their housing units from the long- to the short-term rental market. We directly test this hypothesis using the same instrumental strategy described above and data on various measures of housing supply that we collected from the American Community Survey. We find that: (i) the total housing stock (which is the sum of all renter-occupied, owner-occupied, and vacant units) is not affected by the entry of Airbnb, (ii) an increase in Airbnb listings leads to an increase in the number of units held vacant for recreational or seasonal use,[7] (iii) an increase in Airbnb listings leads to a decrease in the number of units available to long-term renters, and (iv) the above effects on supply

[6]We focus on tourism because Airbnb has historically been frequented more by tourists than business travelers. Airbnb has said that 90% of its customers are vacationers but is attempting to gain market share in the business travel sector.

[7]According to Census methodology, units without a usual tenant but rented occassionally to Airbnb guests would be classified as vacant for recreational or seasonal use. We describe the data in more detail in Section 6.4.

are smaller for zipcodes with a higher owner-occupancy rate. These results are consistent with the hypothesis that Airbnb increases rents and house prices by causing a reallocation of housing supply from the long-term rental market to the short-term rental market. Moreover, the size of the reallocation is greater in zipcodes with fewer owner-occupiers because, intuitively, non-owner-occupiers may be more likely to reallocate. Finally, it is worth mentioning that we cannot rule out the possibility of other effects of Airbnb such as any of the positive or negative externalities; thus, our results should be interpreted as the estimated net effect with evidence for the presence of a reallocation channel.

2 Related literature

We are aware of only two other academic papers that directly study the effect of home-sharing on housing costs, and both of them focus on a specific U.S. market. Lee (2016) provides a descriptive analysis of Airbnb in the Los Angeles housing market while Horn and Merante (2017) use Airbnb listings data from Boston in 2015 and 2016 to study the effect of Airbnb on rental rates. Using a fixed effect model, they find that a one standard deviation increase in Airbnb listings at the census tract level leads to a 0.4% increase in asking rents. In our data, we find that a one standard deviation increase in listings at the within-CBSA zipcode level in 2015-2016 implies a 0.54% increase in rents.

We contribute—and differentiate from previous work—to the literature concerning the effect of home-sharing on housing costs in several important ways. First, we present the first estimates of the effect of home-sharing on house prices and rents that use comprehensive data from across the United States. Second, we are able to exploit the panel structure of our dataset to control for unobserved neighborhood heterogeneity as well as arbitrary city-level time trends. Moreover, we identify a plausible instrument for Airbnb supply and conduct several exercises to support its validity. These exercises reassure us that the measured association between Airbnb and house prices and rents is likely causal. Third, we show that the effect of Airbnb is strongly moderated by the rate of owner-occupiers, a finding consistent with the hypothesis that the Airbnb effect operates through the reallocation of housing supply from the long- to the short-term rental market. Fourth, we provide direct evidence in support of this hypothesis by showing that Airbnb is associated with a decrease in long-term rentals supply and an increase in short-term rentals supply while having no association with changes in the total housing supply. Fifth, by showing that the effects of Airbnb are moderated by the owner-occupancy rate, our results highlight the importance of the marginal homeowner in terms of reallocation (since owner-occupiers are much less likely to reallocate their housing to the permanent short-term rental stock). Thus, the marginal propensity of homeowners to reallocate housing from the long- to the short-term rental market is a key elasticity determining the overall effect of home-sharing.

Our paper also contributes to the growing literature on peer-to-peer markets. Such literature covers a wide array of topics, from the effect of the sharing economy on labor market outcomes (Chen et al., 2017; Hall and Krueger, 2017; Angrist et al., 2017), to entry and competition (Gong et al., 2017; Horton and Zeckhauser, 2016; Li and Srinivasan, 2019; Zervas et al., 2017), to trust and reputation (Fradkin et al., 2017; Proserpio et al., 2017; Zervas et al., 2015). Because the literature on the topic is quite vast, here we focus only on papers that are closely related to ours and refer the reader to Einav et al. (2016) for an overview of the economics of peer-to-peer markets and to Proserpio and Tellis (2017) for a complete review of the literature on the sharing economy.

Closely related to the marketing literature and this work we find papers that study the effects of the entry of peer-to-peer markets and the competition that they generate. Gong et al. (2017), for example, provide evidence that the entry of Uber in China increased the demand for new cars; Farronato and Fradkin (2018), Li and Srinivasan (2019), and Zervas et al. (2017) study the effect of Airbnb on the hotel industry; however, each one of them focuses on a different question. Zervas et al. (2017) focus on the subsitution patterns between Airbnb and hotels, and show that, after Airbnb entry in Texas, hotel revenue dropped. Moreover, the authors show that this negative effect is stronger in periods of peak demand. Farronato and Fradkin (2018) focus instead on the gains in consumer welfare generated by the entry of Airbnb in 50 U.S. markets. Finally, Li and Srinivasan (2019) study how the flexible nature of Airbnb listings affects hotel demand in different markets. The authors show that, in response to the entry of Airbnb, some hotels may benefit from moving away from seasonal pricing. Our paper looks at a somewhat unique context in this literature because we focus on the effect of the sharing economy on the reallocation of goods from one purpose to another, which may cause local externalities. Local externalities are present here because the suppliers are local and the demanders are non-local; transactions in the home-sharing market, therefore, involve a reallocation of resources from locals to non-locals.[8] Our contribution is therefore to study this unique type of sharing economy in which public policy may be especially salient.

Finally, our work is related to papers studying the consequences of what happens when a online platform lowers the cost to entry for suppliers. For example, both Kroft and Pope (2014) and Seamans and Zhu (2013) study the impact of Craiglist on the newspaper industry and find a substantial substitution effect between the two.

The rest of the paper is organized as follows. In Section 3, we discuss the economics of home-sharing and how home-sharing might be expected to affect housing markets. In Section 4, we describe the data we collected from Airbnb and present some basic statistics. In Section 5, we describe our methodology and present exercises in support of the exclusion restriction of our instrument In Section 6, we discuss the results and present several robustness checks to reinforce the validity of our results. Section 7 discusses our findings, the limitations of our work, and provides concluding remarks.

3 Theory

The market for long and short-term rentals is traditionally viewed as segmented on both the supply and demand side. On the demand side, the demanders for short-term rentals are tourists, visitors, and business travelers while the demanders for long-term rentals are local residents. On the supply side, the suppliers of short-term rentals are traditionally hotels and bed and breakfasts while the suppliers of long-term rentals are local landlords. Local residents who own their own homes (owner-occupiers) are on both the demand and the supply side for long-term rentals (they rent to themselves.)

Segmentation exists between the long- and short-term markets despite the fundamental similarity in the product being offered (i.e., space and shelter). The segmentation may exist for a few reasons. First, short-term demanders may have very different needs than long-term demanders. Short-term demanders may only require a bed and a bathroom while long-term demanders may

[8]This may not be seen as a real economic cost, though a shift of welfare from locals to non-locals is important for public policy because policy is set locally. Some have also argued that home-sharing can create a real negative spillover for neighbors (Filippas and Horton, 2018).

also require a kitchen and a living area. Second, the legal environment is very different for short and long-term demanders. Long-term tenants are typically afforded rights and protections that are not available to short-term visitors. Because of this segmentation, the unit price of renting exhibits a term structure with the price of a short-term rental typically being much higher than the price of a long-term rental. Marketplaces for long- and short-term rentals have historically remained separate due to this segmentation.

Effects of home-sharing: Housing supply reallocation and expansion

With the advent of home-sharing, segmentation on the supply side is becoming blurred. Because of home-sharing platforms like Airbnb, it is now much easier for properties that were traditionally used only for long-term rental to now also be used for short-term rental.[9]

Now that it has become easier for owners of traditionally long-term housing to supply the short-term market, what can we expect the effects to be? First, we can expect some owners of traditionally long-term housing to switch from supplying a long-term demander to supplying short-term demanders. In the short run, the supply of housing and of hotels is inelastic, so this reduces the supply of housing available in the long-term rental market and increases the supply of rooms in the short-term rental market. This, in turn, pushes up rents in the long-term rental market and pushes down rents in the short-term rental market (Horn and Merante, 2017; Zervas et al., 2017). To the extent that search and matching frictions exist in both rental markets, this should also reduce the vacancy rate in the long-term rental market and increase the vacancy rate in the short-term rental market.

In the long run, we may also expect a supply response. The quantity of homes that are able to supply both long- and short-term renters (i.e., homes traditionally built for long-term housing) would be expected to increase in the long-run, while the quantity of hotel rooms that are only able to supply the short-term market should decrease. The degree to which there will be quantity adjustments will depend on the amount of land available in the city and the stringency of land use regulations as well as the cost of construction (Gyourko and Molloy, 2015).

The size of the price and quantity response to home-sharing will also depend on the degree to

[9] Home-sharing platforms greatly reduce traditional frictions that previously prevented some homeowners from participating in the short-term rental market such as transactional frictions associated with trust (Einav et al., 2016).

which owners of traditionally long-term rental housing reallocate to the short-term rental market. There are many reasons why an owner would choose not to reallocate. First and foremost, the owner may live in her home. Thus, the owner will not reallocate from the long-term market (where she rents to herself) to the short-term market. She may still participate in the short-term market by selling unused capacity such as spare rooms or time when she is away, but this does not constitute a reallocation from the long-term rental stock to the short-term rental stock because those spare units of capacity would not have been allocated to a long-term tenant anyway and therefore does not push up long-term rental rates. However, the allocation of spare capacity to the short-term rental market, which constitutes a pure supply expansion, can reduce prices in the short-term rental market.[10]

Second, the owner may not reallocate from the long-term market to the short-term market because the costs outweigh the benefits. There could be many costs associated with supplying the short-term rental market. Short-term renters may annoy neighbors, thus reflecting poorly on the host and reducing his social capital in the community. In some cases, an owner may be bound against renting to short-term renter by a homeowners' association. Short-term renters may also be more likely than long-term renters to cause property depreciation. A property owner may also prefer the steadier stream of payments offered by a long-term tenant over the lumpier stream of payments offered by sporadic visitors booking the home for short stays. Owners who simply choose not to use the short-term market will cause no reallocation and therefore have no effect on prices in either the long-term or the short-term rental markets.

Finally, it is worth pointing out that reallocation from the long-term rental stock to the short-term rental stock does not require that expected rents in the short-term rental market be higher than expected rents in the long-term rental market. There may be reasons for preferring to rent short-term instead of long-term even if the expected rents from short-term are lower, as may be the case according to Coles et al. (2017). One reason could be that the owner does not like the restrictiveness of a long-term lease. Even if the owner does not plan to use the property as a primary residence or a vacation home, not renting to a long-term tenant increases the option value for other uses such as letting family or friends stay or even holding out for higher long-term rents

[10] If the owner-occupier is currently allocating spare rooms to the long-term market (i.e., by having a roommate) and then decides to stop renting to a roommate and instead use Airbnb, then this would constitute a reallocation.

in the future while capitalizing on surges in short-term demand.

Effects of home-sharing: Externalities and option value

Besides reallocation of housing supply, home-sharing can affect long-term rental rates in a few other ways. First, there may be both positive and negative externalities. On the positive side, home-sharing may draw tourist money into the neighborhood, increasing revenues to local businesses and increasing the demand for space in the neighborhoods. This would have the effect of increasing both long and short-term rental rates. Farronato and Fradkin (2018) and Coles et al. (2017) document that home-sharing has drawn tourists into neighborhood that previously had very few, and Alyakoob and Rahman (2018) find a positive relation between Airbnb and restaurant employment. On the negative side, the tourists that home-sharing draws in may be unpleasant or noisy. This can make the neighborhood a more unpleasant place to live, thus decreasing rents. In local debates over Airbnb, this has proven to be an unexpectedly salient point (Filippas and Horton, 2018).

Second, if tenants themselves are able to sell unused capacity in the short-term market, even while under a long-term rental lease, then this would increase the demand for renting. In the short run where supply is inelastic, this would push up rents in the long-term rental market. The degree to which rents are increased depends on the degree to which tenants are willing and able to sell unused capacity.[11] In the long-run, this effect could lead to further expansion in housing supply.

So far, the discussion has focused on rental rates. Since buying a house can be viewed as purchasing the present value of future rental payments, house prices should be equal to the expected present value of rents for a similar unit, adjusted for any tax implications, borrowing costs, maintenance costs, and physical depreciation (Poterba, 1984). Thus, any effect of home-sharing on long-term rental rates will be directly capitalized into house prices. However, because home-sharing also allows the homeowner to sell unused capacity on the short-term market, it should have an additional effect of increasing prices even further than the direct effect on rents.

Finally, we note that it is possible that home-sharing externalities differentially affect homeowners and renters. For example, homeowners may be more sensitive to noisy neighbors than renters. If such were the case, then the net effect of home-sharing on the price-to-rent ratio could be negative

[11]In practice, this will depend on the laws of individual cities and the types of leases landlords sign with tenants, and the enforceability of any associated clauses.

even though the increased option value of using spare capacity would increase it.

Effects of home-sharing: Other effects

Finally, we note two other effects that home-sharing may have on short and long-term rental markets. First, in the long-run, home-sharing may change the characteristics of the housing stock. For example, by increasing the option value of spare capacity, home-sharing may cause future homes to be built with spare capacity in mind. There may be an increase in the supply of homes with accessory dwelling units that are optimized for delivery to short-run tenants with the main unit simultaneously being occupied by the owner.

Second, home-sharing may change the short-run supply elasticity of short-term rentals. Without home-sharing, the short-run supply of short-term rentals is inelastic because there is only a fixed number of hotel rooms in any given neighborhood. High development costs and regulations make it difficult to adjust this number quickly. Home-sharing increases the flexibility of traditionally long-term housing to freely move between the long and short-term rental markets, thus leading to a more elastic supply in the short-term market that is able to quickly expand in response to surges in demand and then quickly contract when the surge is over. Farronato and Fradkin (2018) document this phenomenon and evaluate its welfare implications.

Summary

To summarize, we have argued that home-sharing will have the following effects. First, it will cause a reallocation from the long-term housing supply to the short-term rental market. In the short-run, this will push up rental rates and house prices, and decrease vacancy rates in the long-term market. In the long-run, this could lead to an increase in housing supply, depending on the housing supply curve of the market. Second, the size of the reallocation effect will depend on the propensity of homeowners to reallocate housing from the long-term market to the short-term market in response to home-sharing. The effect of home-sharing will be smaller when there are fewer homeowners choosing to reallocate. Third, rents and prices should both increase due to the increased option value of spare housing capacity, with prices increasing more than rents, thus leading to an increased price-to-rent ratio. Countervailing these three effects (which are all positive on prices and rents) is the possibility of negative externalities. If home-sharing makes the neighborhood less desirable to live in, then this could have a negative effect on rents and prices. If homeowners are especially sensitive to these externalities, home-sharing could decrease the price-to-rent ratio. On the other hand, there could also be positive externalities that have the opposite effects.

The predicted effects of home-sharing on rental rates and house prices is therefore ambiguous. In this paper, we aim simply to test for the net effect. We will find that the net effect is positive on rental rates, house prices, and the price-to-rent ratio in a way that is consistent with both the reallocation channel and with increasing the option value of spare capacity. We also provide some direct evidence of the reallocation channel. However, we cannot rule out the potential for other effects like externalities, nor do we disentangle the size of the various channels. It is also worth mentioning that, in this paper, we focus only on short-run effects. This choice is dictated by two reasons: First, home-sharing is a relatively new phenomenon, and Airbnb itself is only a decade old. Cities are still actively grappling with how to respond to home-sharing, and so we believe that it is too early to look for long-run effects. Second, in this paper, we do not find any empirical evidence that Airbnb (as yet) is associated with changes to the total housing supply, though we do find evidence for reallocation of housing from long-term rental stock to short-term rental stock.

4 Data and Background on Airbnb

4.1 Background on Airbnb

Recognized by most as the pioneer of the sharing economy, Airbnb is a peer-to-peer marketplace for short-term rentals, where the suppliers (hosts) offer different kinds of accommodations (i.e. shared rooms, entire homes, or even yurts and treehouses) to prospective renters (guests). Airbnb was founded in 2008 and has experienced dramatic growth, going from just a few hundred hosts in 2008 to over three million properties supplied by over one million hosts in 150,000 cities and 52 countries in 2017. Over 130 million guests have used Airbnb, and with a market valuation of over $31B, Airbnb is one of the world's largest accommodation brands.

4.2 Airbnb listings data

Our main source of data comes directly from the Airbnb website. We collected consumer-facing information about the complete set of Airbnb properties located in the United States and about the hosts who offer them. The data collection process spanned a period of approximately five years, from mid-2012 to the end of 2016. We performed scrapes at irregular intervals between 2012 to 2014 and at a weekly interval starting January 2015.[12]

Our scraping algorithm collected all listing information available to users of the website, including the property location, the daily price, the average star rating, a list of photos, the guest capacity, the number of bedrooms and bathrooms, a list of amenities such as WiFi and air conditioning, etc., and the list of all reviews from guests who have stayed at the property.[13] Airbnb host information includes the host name and photograph, a brief profile description, and the year-month in which the user registered as a host on Airbnb.

Our final dataset contains detailed information about 1,097,697 listings and 682,803 hosts spanning a period of nine years, from 2008 to 2016. Because of Airbnb's dominance in the home-sharing market, we believe that this data represents the most comprehensive picture of home-sharing in the U.S. ever constructed for independent research.

4.3 Calculating the number of Airbnb listings, 2008-2016

Once we have collected the data, the next step is to define a measure of Airbnb supply. This task requires two choices: First, we need to choose the geographic granularity of our measure; second, we need to define the entry and exit dates of each listing in the Airbnb platform. Regarding the geographic aggregation, we conduct our main analysis at the zipcode level for a few reasons. First, it is the lowest level of geography for which we can reliably assign listings without error (other than user input error).[14] Second, neighborhoods are a natural unit of analysis for housing markets because there is significant heterogeneity in housing markets across neighborhoods within cities but comparatively less heterogeneity within neighborhoods. Zipcodes will be our proxy for neighborhoods. Third, conducting the analysis at the zipcode level as opposed to the city level helps with identification. This is due to our ability to compare zipcodes within cities, thus controlling for any unobserved city level factors that may be unrelated to Airbnb but that affect all neighborhoods

[12] In their paper, Horn and Merante (2017) incorrectly state that our Airbnb dataset comes form InsideAirbnb.com (probably referencing an older version of this paper), but, in fact, the current results are based on data that one of the authors of this paper scraped and collected.

[13] Airbnb does not reveal the exact street address or coordinates of the property for privacy reasons; however, the listing's city, street, and zipcode correspond to the property's real location.

[14] Airbnb does report the latitude and longitude of each property but only up to a perturbation of a few hundred meters. So it would be possible, but complicated, to aggregate the listings to finer geographies with some error.

within a city such as a city-wide shock to labor productivity.

The second choice, how to determine the entry and exit date of each listing, comes less naturally. First, our scraping algorithm did not constantly monitor a listing's status to determine whether it was active or not but rather obtained snapshots of the properties available for rent in the US at different points in time until the end of 2014 and at the weekly level starting in 2015. Second, even if it did so, measuring active supply would still be challenging.[15] Thus, to construct the number of listings going back in time, we employ a variety of methods following Zervas et al. (2017), which we summarize in Table 1.

Table 1: Methods for Computing the Number of Listings

	Listing is considered active ...
Method 1	starting from host join date
Method 2	for 3 months after host join date and after every guest review
Method 3	for 6 months after host join date and after every guest review

Method 1 is our preferred choice to measure Airbnb supply and will be our main independent variable in all the analyses presented in this paper. This measure computes a listing's entry date as the date its host registered on Airbnb and assumes that listings never exit. The advantage of using the host join date as the entry date is that for a majority of listings, this is the most accurate measure of when the listing was first posted. The disadvantage of this measure is that it is likely to overestimate the listings that are available on Airbnb (and accepting reservations) at any point in time. However, as discussed in Zervas et al. (2017), such overestimation would cause biases only if, after controlling for several zipcode characteristics, it is correlated with the error term.[16]

Aware of the fact that method 1 is an imperfect measure of Airbnb supply, we also experiment with alternative definitions of Airbnb listings' entry and exit. Methods 2 and 3 exploit our knowledge of each listing's review dates to determine whether a listing is active. The heuristic we use is as follows: A listing enters the market when the host registers with Airbnb and stays active for

[15]Estimating the number of active listings is a challenge even for Airbnb. Despite the fact that Airbnb offers an easy way to unlist properties, many times hosts neglect to do so, creating "stale vacancies" that seem available for rent but in actuality are not. Fradkin (2015), using proprietary data from Airbnb, estimates that between 21% and 32% of guest requests are rejected due to this effect.

[16]The absence of bias in this measure is also confirmed by Farronato and Fradkin (2018) where using Airbnb proprietary data resulted in the same estimates obtained by Zervas et al. (2017) (where the data collection and measures of Airbnb supply are similar to those used in this paper).

m months. We refer to m as the listing's Time To Live (TTL). Each time a listing is reviewed, the TTL is extended by m months from the review date. If a listing exceeds the TTL without any reviews, it is considered inactive. A listing becomes active again if it receives a new review. In our analysis, we test two different TTLs, 3 months and 6 months.

Despite the fact that our different measures of Airbnb supply rely on different heuristics and data, because of Airbnb's tremendous growth, all our measures of Airbnb supply are extremely correlated. The correlation between method 1 and each other measure is above 0.95 in all cases. In the Appendix, we present robustness checks of our main results to the different measures of Airbnb supply discussed above and show that results are qualitatively and quantitatively unchanged.[17]

4.4 Zillow: rental rates and house prices

Zillow.com is an online real estate company that provides estimates of house and rental prices for over 110 million homes across the U.S. In addition to giving value estimates of homes, Zillow provides a set of indexes that track and predict home values and rental prices at a monthly level and at different geographical granularities.

For house prices, we use the Zillow Home Value Index (ZHVI) that estimates the median transaction price for the actual stock of homes in a given geographic unit and point in time. The advantage of using the ZHVI is that it is available at the zipcode-month level for over 13,000 zipcodes.

For rental rates, we use the Zillow Rent Index (ZRI). Like the ZHVI, Zillow's rent index is meant to reflect the median monthly rental rate for the actual stock of homes in a geographic unit and point in time. Crucially, Zillow's rent index is based on rental *list prices* and is therefore a measure of prevailing rents for new tenants. This is the relevant comparison for a homeowner deciding whether to place her unit on the short-term or long-term market. Moreover, because Zillow is not considered a platform for finding short-term housing, the ZRI should be reflective of rental prices in the long-term market.[18] For each zipcode, we calculate the price-to-rent ratio as simply the ZHVI

[17]One additional source of error in our computations is listings that were posted and then taken down between 2008 and 2011 since we did not start scraping until 2012. However, the number of such listings is likely to be small, as shown in Figure 3. Moreover, our regressions use only data starting in 2011, so the influence on our results is likely minimal. Further, as we show in Table 15 of the Appendix, our results are robust to the exclusion of early years.

[18]Since the ZHVI and ZRI measure medians, our results only apply to the middle of the housing market. In the Appendix, we explore effects on different segments of the housing market and find that the effects are qualitatively similar.

divided by the ZRI.

4.5 Other data sources

We supplement the above data with several additional sources.

Variables used for the instrument We use monthly Google Trends data for the search term "airbnb", which we downloaded directly from Google. This index measures how often people worldwide search for the term "airbnb" on Google and is normalized to have a value of 100 at the peak month. We use the Census's Zipcode Business Patterns data to measure the number of establishments in the food services and accommodations industry (NAICS code 72) for each zipcode in 2010.

Zipcode level time-varying characteristics We collect from the American Community Survey (ACS) zipcode level annual estimates of median household income, population, share of 25-60 years old with bachelors' degrees or higher, and employment rate. From the ACS, we also obtain zipcode level annual estimates of the number of housing units occupied by their owners or renters, and the number of vacant units. The ACS also reports the reason a housing unit is vacant (for example, whether the owner is holding it vacant so that he or she can use it occassionally for recreation or whether it is vacant and currently looking for a tenant). We can therefore calculate the owner-occupancy rate as the share of occupied units that are occupied by owners and the total housing stock as the sum of owner-occupied units, renter-occupied units, and vacant units.

Proxies for tourism demand We obtained hotel occupancy rates at the CBSA-year-month level from STR, a company that tracks hotel performance worldwide. We collected the number of incoming airport travelers for all airports in the United States from the Bureau of Transportation Statistics. Finally, we collected the complete set of hotel and restaurant reviews for all the hotels and restaurants available on TripAdvisor. This data amount to about 18 million hotel reviews from 88,000 accommodation properties (hotels, inns, B&Bs) and about 25 million restaurant reviews from about 478,000 restaurants from 2001 to the beginning of 2017 (2019 for restaurant reviews).

4.6 Summary statistics

Figure 2 shows the geographic distribution of Airbnb listings in June 2011 and June 2016. The map shows significant geographic heterogeneity in Airbnb listings with most Airbnb listings occurring in large cities and along the coasts. Moreover, there exists significant geographic heterogeneity in the growth of Airbnb over time. From 2011 to 2016, the number of Airbnb listings in some zipcodes grew by a factor of 30 or more; in others, there was no growth at all. Figure 3 shows the total number of Airbnb listings over time in our dataset using methods 1-3. Using method 1 as our preferred method, we observe that from 2011 to 2016, the total number of Airbnb listings grew by a factor of 30, reaching over 1 million listings in 2016.

Table 2 gives a sense of the size of Airbnb relative to the housing stock at the zipcode level, for the 100 largest CBSAs by population in our data. Even in 2016, Airbnb remains a small percentage of the total housing stock for most zipcodes. The median ratio of Airbnb listings to housing stock is 0.21%, and the 90th percentile is 1.88%. When comparing to the stock of vacant homes, Airbnb begins to appear more significant. The median ratio of Airbnb listings to vacant homes (either for long- or short-term rental) is 2.63%, and the 90th percentile is 20%. Perhaps the most salient comparison—at least from the perspective of a potential renter—is the number of Airbnb listings relative to the stock of homes listed as vacant and for rent (which are part of the long-term rental supply). This statistic reaches 13.7% in the median zipcode in 2016 and 129% in the 90th percentile zipcode. This implies that in the median zipcode, a local resident looking for a long-term rental unit will find that about 1 in 8 of the potentially available homes are being placed on Airbnb instead of being made available to long-term residents. Framed in this way, concerns about the effect of Airbnb on the housing market do not appear unfounded.

5 Methodology

Let Y_{ict} be either the price index or the rent index for zipcode i in CBSA c in year-month t, let $Airbnb_{ict}$ be a measure of Airbnb supply, and let $oorate_{ic,2010}$ be the owner-occupancy rate in

2010.[19] We assume the following causal relationship between Y_{ict} and $Airbnb_{ict}$:

$$\ln Y_{ict} = \alpha + \beta Airbnb_{ict} + \gamma Airbnb_{ict} \times oorate_{ic,2010} + X_{ict}\eta + \epsilon_{ict} \tag{1}$$

where X_{ict} is a vector of observed time-varying zipcode characteristics and ϵ_{ict} contains unobserved factors that may additionally influence Y_{ict}. These factors could include anything that affects the underlying desirability to live in zipcode i such as changes to local labor market conditions or changes to local amenities like public school quality. If the unobserved factors are uncorrelated with $Airbnb_{ict}$, conditional on X_{ict}, then we can consistently estimate β and γ by OLS. However, ϵ_{ict} and $Airbnb_{ict}$ may be correlated through unobserved factors at the zipcode, city, and time levels. We allow ϵ_{ict} to contain unobserved zipcode level factors δ_i, and unobserved time-varying factors that affect all zipcodes within a CBSA equally, θ_{ct}. Writing: $\epsilon_{ict} = \delta_i + \theta_{ct} + \xi_{ict}$, Equation (1) becomes:

$$\ln Y_{ict} = \alpha + \beta Airbnb_{ict} + \gamma Airbnb_{ict} \times oorate_{ic,2010} + X_{ict}\eta + \delta_i + \theta_{ct} + \xi_{ict}. \tag{2}$$

Even after controlling for unobserved factors at the zipcode and CBSA-year-month level, there may still be some unobserved *zipcode-specific*, *time-varying* factors contained in ξ_{ict} that are correlated with $Airbnb_{ict}$. To address this issue, we construct an instrumental variable that is plausibly uncorrelated with local shocks to the housing market at the zipcode level, ξ_{ict}, but likely to affect the number of Airbnb listings.

Our instrument begins with the worldwide Google Trends search index for the term "airbnb", g_t, which measures the quantity of Google searches for "airbnb" in year-month t. Such trends represent a measure of the extent to which awareness of Airbnb has diffused to the public, including both demanders and suppliers of short-term rental housing. Figure 1 plots g_t from 2008 to 2016, and it is representative of the explosive growth of Airbnb over the past ten years. Crucially, the search index is *not* likely to be reflective of growth in overall tourism demand because it is unlikely to have changed so much over this relatively short time period. Moreover, it should not be reflective

[19]We use the owner-occupancy rate in 2010 to minimize concerns about endogeneity of the owner-occupancy rate. However, the results are robust to using the contemporaneous owner-occupancy rate as calculated from ACS 5-year estimates from 2011 to 2016.

of overall growth in the supply of short-term housing, *except* to the extent that it is driven by Airbnb.[20]

The CBSA-year-month fixed effects θ_{ct} already absorb any unobserved variation at the year-month level. Therefore, to complete our instrument, we interact g_t with a measure of how attractive a zipcode is for tourists in base year 2010, $h_{i,2010}$. We measure "touristiness" using the number of establishments in the food services and accommodations industry (NAICS code 72) in a specific zipcode. Zipcodes with more restaurants and hotels may be more attractive to tourists because these are services that tourists need to consume locally—thus, it matters how many of these services are near the tourist's place of stay. Alternatively, the larger number of restaurants and hotels may reflect an underlying local amenity that tourists value.

For the instrument to have power, potential hosts must be more likely to rent their property in the short-term market in response to learning about Airbnb. We can verify this assumption by examining the relationship between Google trends and the difference in Airbnb listings for more touristy and less touristy zipcodes. Figure 4 shows that such difference increases as Airbnb awareness increases, confirming our hypothesis.

In order for the instrument to be valid, $z_{ict} = g_t \times h_{i,2010}$ must be uncorrelated with the zipcode-specific, time-varying shocks to the housing market, ξ_{ict}. This would be true if either ex-ante touristiness in 2010 ($h_{i,2010}$) is independent of time-varying zipcode level shocks (ξ_{ict}) or growth in worldwide Airbnb searches (g_t) is independent of the specific timing of those shocks. To see how our instrument addresses potential confounding factors, consider changes in zipcode level crime rate as an omitted variable. It is unlikely that changes to crime rates across all zipcodes are systematically correlated in time with worldwide Airbnb searches. Even if they were, they would have to correlate in such a way that the correlation is systematically stronger or weaker in more touristy zipcodes. Moreover, these biases would have to be systematically present within all cities in our sample. Of course, we cannot rule this possibility out completely. We therefore now turn to a detailed discussion of the instrument and its validity and present some exercises that suggest that the exogeneity assumption is likely satisfied.

[20] We provide further evidence to this effect in Section 6.2.

5.1 Discussion: Validity of the instrumental variable

The construction of an instrumental variable using the interaction of a plausibly exogenous time-series (Google trends) with a potentially endogenous cross-sectional exposure variable (the touristiness measure) is an approach that was popularized by Bartik (1991) and that researchers have used in many prominent recent papers (Peri, 2012; Dube and Vargas, 2013; Nunn and Qian, 2014; Hanna and Oliva, 2015; Diamond, 2016).

The approach is popular because one can often argue that some aggregate time trend, which is exogenous to local conditions, will affect different spatial units systematically along some cross-sectional exposure variable. In the classic Bartik (1991) example, national trends in industry-specific productivity are interacted with the historical local industry composition to create an instrument for local labor demand. Such an instrument will be valid if the interaction of the aggregate time trend with the exposure variable is independent of the error term. This could happen if either the time trend is independent of the error term ($E[g_t, \xi_{ict}] = 0$) or if the exposure variable is independent of the error term ($E[h_{i,2010}, \xi_{ict}] = 0$). While this may seem plausible at first glance, Christian and Barrett (2017) point out that if there are long-run time trends in the error term, and if these long-run trends are systematically different along the exposure variable, then the exogeneity assumption may fail. In our context, a story that may be told is the following. Suppose there is a long-run trend toward gentrification that leads to higher house prices over time. Suppose also that the trend of gentrification is higher in more touristy zipcodes. Since there is also a systematic long-run trend in the time-series variable, g_t, the instrument $g_t h_{i,2010}$ is no longer independent of the error term, and 2SLS estimates may reflect the effects of gentrification rather than home-sharing.

We now proceed to make four arguments for why the exogeneity condition is likely to hold in our setting.

Parallel pre-trends

As Christian and Barrett (2017) note, the first stage of this instrumental variable approach is analogous to a difference-in-differences (DD) coefficient estimates. In our case, since the specification includes year-month and zipcode fixed effects, the variation in the instrument comes from comparing Airbnb listings between high- and low-Airbnb awareness year-months, and between high- and low-touristiness zipcodes. Because of this, Christian and Barrett (2017) suggest testing whether spatial units with different levels of the exposure variable have parallel trends in periods before g_t takes effect. This is similar to testing whether control and treatment groups have parallel pre-trends in DD analysis. To do this, we plot the Zillow house price index for zipcodes in different quartiles of 2010 touristiness ($h_{i,2010}$) from 2009 to the end of 2016.[21] The results are shown in Figure 5. The figure shows that there are no differential pre-trends in the Zillow Home-Value Index (ZHVI) for zipcodes in different quartiles of touristiness until after 2012, which also happens to be when interest in Airbnb began to grow according to Figure 1. This is true both when computing the raw averages of ZHVI within quartile (top panel) and when computing the average of the residuals after controlling for zipcode and CBSA-year-month fixed effects (bottom panel). The lack of differential pre-trends suggests that zipcodes with different levels of touristiness do *not* generally have different long-run house price trends, but they only began to diverge after 2012 when Airbnb started to become well known.

IV has no effect in non-Airbnb zipcodes

To further provide support for the validity of our instrument, we perform another test that consists of checking whether the instrumental variable predicts house prices and rental rates in zipcodes that were never observed to have any Airbnb listings. If the instrument is valid, then it should only be correlated to house prices and rental rates through its effect on Airbnb listings. So, in areas with no Airbnb, we should not see a positive relationship between the instrument and house prices and rental rates.[22]

To test this, we regress the Zillow rent index and house price index on the instrumental variable directly, using only data from zipcodes in which we never observed any Airbnb listings. The first two columns of Table 3 report the results of these regressions and show that, conditional on the fixed effects and zipcode demographics, we do not find any statistically significant relationship

[21] We cannot repeat this exercise with rental rates because Zillow rental price data did not begin until 2011 or 2012 for most zipcodes.

[22] This exercise is similar in spirit to an exercise performed by Martin and Yurukoglu (2017) to support the validity of their instrument. Martin and Yurukoglu (2017) use the channel position of Fox News in the cable line-up as an instrument for the effect of Fox viewership on Republican voting. They show that the future channel position of Fox News is not correlated with Republican voting in the time periods before Fox News. This is analogous to us showing that our instrument is not correlated with house prices and rents in zipcodes without Airbnb.

between the instrument and house prices/rental rates in zipcodes without Airbnb. If anything, we find that there is a *negative* relationship between the instrument and house prices/rental rates in zipcodes without Airbnb, though the estimates are imprecise and the sample size is considerably reduced when considering only such zipcodes. By contrast, columns 3-4 of Table 3 show that if we regress house prices and rental rates directly on the instrument for zipcodes *with* Airbnb, we find a positive and statistically significant relationship.

Of course, the sample of zipcodes that never had any Airbnb listings could be fundamentally different from the sample of zipcodes that did. In columns 1 and 2 of Table 4, we show that the sample of zipcodes with Airbnb are indeed quite different from the sample of zipcodes without Airbnb, which are richer and more educated in general. We therefore construct a third sample of zipcodes *with* Airbnb, but that are more similar to the sample of zipcodes *without* Airbnb. To do so, we use propensity-score matching. Starting with the full sample of zipcodes, we first estimate a logistic regression at the zipcode level that predicts whether or not a zipcode will be a non-Airbnb zipcode based on 2010 zipcode demographic characteristics (median household income, population, college share, and employment rate) and touristiness. For each zipcode that is observed to have no Airbnb, we find the nearest zipcode in terms of propensity score that is observed to have some Airbnb entry over the whole 2011-2016 time period. In column 3 of Table 4, we show that the propensity-score matched sample of zipcodes with Airbnb listings is (as expected) demographically similar to the non-Airbnb sample (column 1). Columns 5-6 of Table 3 report the results when we regress house prices and rental rates directly on the instrument in the propensity-score matched sample with Airbnb listings. The direct effect of the instrument is positive and statistically significant, alleviating concerns that the null effect of the instrument in the non-Airbnb sample is only because they are poorer and smaller than other zipcodes. Thus, there does not appear to be any evidence that the instrument would be positively correlated with house prices/rental rates, except through the effect on Airbnb.

Placebo test

As a final exercise, we follow Christian and Barrett (2017) to implement a form of randomization inference to test whether the instrument is really exogenous or primarily driven by spurious time trends. To do so, we keep constant touristiness, Google trends, the identity of zipcodes experiencing

Airbnb entry, observable time-varying zipcode characteristics, housing market variables, and the aggregate number of Airbnb listings in any year-month period. However, among the zipcodes with positive Airbnb listings, we randomly swap the specific number of Airbnb listings across these zipcodes. For example, we randomly assign to zipcode i the variable $Airbnb_{jct}$ (i.e., the Airbnb counts of zipcode j for CBSA c in time t). The randomized regressor preserves the overall time trends in the number of Airbnb listings but randomizes the identity of which zipcodes had *how much* Airbnb growth and thus eliminates the impact of touristiness on the *intensive margin* of Airbnb listings. If the results are primarily driven by a spurious time trends that interacts with the *extensive margin* of whether there are any Airbnb listings, then this exercise will produce 2SLS estimates that continue to be positively and statistically significant. Indeed, in their critique of the Nunn and Qian (2014) instrument, Christian and Barrett (2017) perform this test and find positive and statistically significant coefficients even using the randomized regressor. However, if the effect of touristiness on the intensive margin of Airbnb listings is really what matters, as is our argument, then the first-stage will become very weak when regressing the randomized regressor on the instrument, leading to statistically insignificant estimates. Moreover, these estimates will exhibit extremely large variance due to the weak first stage.

We estimate the 2SLS specification on this dataset for 1,000 draws of randomized allocations of Airbnb listings among zipcodes that had positive Airbnb listings. We find that the measured effect of Airbnb is statistically insignificant for over 99% of the randomized draws across our three dependent variables, i.e., rent index, price index, and price-to-rent ratio, both in the main effect and the interaction term with owner-occupancy rate. Figure 6 shows the distribution of the estimated coefficients and the associated t-statistics that we estimate for both the main effect β , for each of the three dependent variables.[23] As expected, the procedure produces a large variance of estimates that are statistically insignificant. If spurious time trends were driving our results, we would expect the Christian and Barrett (2017) procedure to give statistically significant estimates even when using the randomized regressor.[24] The results of this test are therefore consistent with an instrument that is exogenous.

[23]Results for the interaction terms γ look similar but with a different sign.

[24]See Figure 6 in Christian and Barrett (2017). In Appendix A, we discuss the test in greater detail using a Monte Carlo simulation with both a valid and an invalid instrument and show that the results of this test we obtained with our instrument are consistent with a valid instrument.

Taken together, the preceding results paint a strong picture in support of the validity of our instrument. We will therefore maintain this assumption for now, presenting results as though the instrument were valid and discuss further threats to identification in Section 6.2.

6 Results

6.1 The effect of Airbnb on house prices and rents

We begin by reporting results in which $Airbnb_{ict}$ is measured as the natural log of one plus the number of listings as measured by method 1 in Table 1.[25] Doing so, we estimate a specification similar to that used in Zervas et al. (2017) and Farronato and Fradkin (2018) where the authors estimate the impact of Airbnb on the hotel industry. Therefore, our estimates represent the elasticity of our dependent variables with respect to Airbnb supply.

In our main specifications, we consider three dependent variables: the natural log of the Zillow Rent Index (ZRI), the natural log of the Zillow Home-Value Index (ZHVI), and the natural log of the price-to-rent ratio (ZHVI/ZRI). In order to maintain our measure of touristiness, $h_{i,2010}$, as a pre-period variable, we only use data from 2011 to 2016. This time frame covers all of the period of significant growth in Airbnb (see Figure 3). We also include only data from the 100 largest CBSAs, as measured by 2010 population.[26] The data is monthly, so we deseasonalize all variables. Since the regression in Equation 2 has two endogenous regressors ($Airbnb_{ict}$ and $Airbnb_{ict} \times oorate_{ic,2010}$), we use two instruments for the two-stage least squares estimation ($g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ic,2010}$).

Table 5 reports the regression results when the dependent variable is ln ZRI. Column 1 reports the results from a simple OLS regression of ln ZRI on ln listings and no controls. Without controls, a 1% increase in Airbnb listings is associated with a 0.1% increase in rental rates. Column 2 includes zipcode and CBSA-year-month fixed effects. With the fixed effects, the estimated coefficient on Airbnb declines by an order of magnitude. Column 3 includes the interaction of Airbnb listings with the zipcode's owner-occupancy rate. Column 3 shows the importance of controlling for owner-

[25] We add one to the number of listings to avoid taking logs of zero. In Appendix B, we show that our results are robust to dropping observations with 0 listings and using ln(listings) instead.

[26] The 100 largest CBSAs constitute the majority of Airbnb listings (over 80%). In Appendix B, we show that our results are robust to the inclusion of more CBSAs.

occupancy rate, as it significantly mediates the effect of Airbnb listings. Column 4 includes time-varying zipcode level characteristics, including the ln total population, the ln median household income, the share of 25-60 years old with Bachelors' degrees or higher, and the employment rate. Because these measures are not available at a monthly frequency, we linearly interpolate them to the monthly level using ACS 5-year estimates from 2011 to 2016.[27] Column 4 shows that the results are robust to the inclusion of these zipcode demographics. Finally, columns 5 and 6 report the 2SLS results using the instrumental variable with and without time-varying zipcode characteristics as controls. Using the results from column 6 (our preferred specification), we estimate that a 1% increase in Airbnb listings in zipcodes with the median owner-occupancy rate (72%) leads to a 0.018% increase in rents.The effect of Airbnb is significantly declining in the owner-occupancy rate. At 56% owner-occupancy rate (the 25th percentile), the effect of a 1% increase in Airbnb listings is to increase rents by 0.024%, and at 82% owner-occupancy rate (the 75th percentile), the effect of a 1% increase in Airbnb listings is to increase rents by 0.014%.

Table 6 repeats the regressions with ln ZHVI as the dependent variable. As with the rental rates, we find that controlling for owner-occupancy rate is very important as the estimated direct effect of Airbnb listings increases by an order of magnitude when controlling for the interaction vs. not. Further, including demographic controls still does not affect the results. Using the coefficients reported in column 6 of Table 6, we estimate that a 1% increase in Airbnb listings leads to a 0.026% increase in house prices for a zipcode with a median owner-occupancy rate. The effect increases to 0.037% in zipcodes with an owner-occupancy rate equal to the 25th percentile and decreases to 0.019% in zipcodes with an owner-occupancy rate equal to the 75th percentile.

It is worth noting that in both the rental rate and house price regressions, the 2SLS estimates (columns 5 and 6 of Tables 5 and 6) are about twice as large as the OLS estimates (columns 3 and 4 of Tables 5and 6). This goes against our initial intuition that omitted factors (such as gentrification) are most likely to be positively correlated with both Airbnb listings and house prices/rents, thus creating a positive bias. However, we note that the OLS estimate may also be negatively biased or biased toward zero for two reasons. First, there may be measurement error in the true amount of home-sharing, leading to attenuation bias. Measurement error may arise from the fact that we only estimate the number of Airbnb listings, and we do not know their exact entry and exit, nor do

[27] Results are not sensitive to different types of interpolations.

we know their availability for bookings. Measurement error may also arise from the fact that there are other home-sharing platforms besides Airbnb that we do not measure.[28] Our estimate for the number of listings is therefore a noisy measure of the true number of short-term rentals. Second, simultaneity bias may be negative if higher rental rates in the long-term rental market would cause a decrease in the number of Airbnb listings, *ceteris paribus*. This could happen if an increase in the long-term rental rate causes fewer landlords to choose to supply the short-term market and more to supply the long-term market.

Finally, Table 7 reports the regression results when ln ZHVI/ZRI is used as the dependent variable. Column 6 shows that the effect of Airbnb listings on the price-to-rent ratio is positive, and that, similarly to rents and prices, the effect is declining in owner-occupancy rate. At the median owner-occupancy rate, a 1% increase in Airbnb listings leads to a statistically significant 0.01% increase in the price-to-rent ratio.

To summarize the results reported in Tables 5-7, we show that: 1) An increase in Airbnb listings leads to both higher house prices and rental rates, 2) the effect is slightly higher for house prices than it is for rental rates, and 3) the effect is decreasing in the zipcode's owner-occupancy rate. These results are consistent with the hypothesized effects of reallocation discussed in Section 3, namely that Airbnb causes some landlords to reallocate housing from the long-term rental stock to the short-term rental stock, pushing up prices and rents in the long-term market, and the effects are attenuated in areas with more owner-occupiers because owner-occupier usage of Airbnb is less likely to represent true reallocation. We provide further, more direct evidence of reallocation in Section 6.4. The finding that the effect of Airbnb on price-to-rent ratio is positive suggests that home-sharing may have increased homeowners' option value for utilizing spare capacity. Finally, if there are negative externalities generated by the use of Airbnb that spill over to house prices and rental rates, they do not appear to be large enough to override the effects of reallocation.

[28]Our results are robust, however, to the inclusion of controls reflecting the popularity of other home-sharing websites like HomeAway and VRBO. We do so by using Google Trends index, a widely used proxy for demand in several settings (Choi and Varian, 2012; Ghose, 2009; Li et al., 2016), as a proxy for demand for such platforms. We report these results in Table 18 of Appendix B.

6.2 Threats to identification

As in any study using observational data without experimental variation, endogeneity is always a concern. Even though we conducted a number of exercises in Section 5.1 that support the validity of the instrument, one might still be concerned that the instrument is picking up spurious correlation. In this section, we discuss three potential threats to our identification strategy, and provide evidence that they do not affect our results.

Gentrification One may be concerned that post-2012, touristy and non-touristy zipcodes experienced differential trends in gentrification or neighborhood change. However, columns 5-6 of Tables 5-7 show that the main results are unchanged by the inclusion of time-varying zipcode demographic controls. Because the included demographic controls (population, household income, share of college-educated, and employment rate) are fairly basic measurements of zipcode level economic outcomes, they are likely to be highly correlated with other unobserved factors that affect zipcode level housing markets such as local amenities or local labor market conditions. Therefore, the fact that our results are not affected by these controls suggests that it is unlikely that the instrument is correlated with other unobserved zipcode level factors that affect housing markets.[29]

Tourism demand Another endogeneity concern would be that our instrument is actually picking up changes in tourism demand, which would naturally increase the demand for space in more vs. less touristy zipcodes and thus affect house prices and rents. A priori, we see no obvious reason to think that, after controlling for city-year-month fixed effects, the time-variation in Google searches for Airbnb should be correlated with aggregate tourism demand. Further, a simple comparison shows that Google trends for Airbnb is uncorrelated with Google trends for other tourism-driven websites (Figure 7). Despite this, we address this concern directly by controlling for various measures of tourism demand. First, we control for annual counts of the number of food & accommodations establishments in each zipcode as reported by the Census Bureau's Zipcode Business Patterns data. Second, we control for the total number of airport passengers arriving at each U.S. city each month

[29] A related concern would be that demographics respond slowly to an underlying change in the desirability of a neighborhood, but rental rates and prices respond more quickly. Then, contemporaneous measures of zipcode demographics may not control for the unobserved factors. To account for this possibility, we try controlling for 1-year ahead and 2-year ahead measures of zipcode demographics instead of contemporaneous measures and find that the results are unchanged. We show these results in Appendix B.

and then allocate these arrivals to zipcodes based on the zipcode's share of hotel rooms in each city. Data on airport passengers come from the Bureau of Transportation Statistics and data on hotel rooms come from STR, a company that tracks the hotel industry worldwide. Third, we control for monthly hotel occupants in each zipcode using occupancy rates data we obtained from STR. STR only provides the number of hotel occupants at the city level, so again we assign hotel occupants to zipcodes based on the zipcode's share of hotel rooms in each city. Finally, we control for the monthly number of reviews written for accommodation properties (hotels, Inns, B&Bs) and restaurants in each zipcode on the website TripAdvisor, a website specializing in reviews for tourist attractions, restaurants, and accommodations. We report the results from these regressions in columns 1-4 and 6-9 of Table 8. We find that controlling for any of these factors does not change our main results, either qualitatively or quantitatively, so it does not appear that unobserved changes to tourism demand are driving spurious correlation in our estimates.

High and low-touristy zipcodes Finally, we rule out any differential effects between high- and low- touristy zipcodes that are linear in time by directly controlling for the interaction of a linear time trend with zipcode touristiness: $t \times h_{i,2010}$. The results are reported in columns 5 and 10 of Table 8. The main results are robust even to the inclusion of these touristiness-specific time-trends.

The results reported in this section, combined with the exercises supporting the validity of the instrument we discussed in Section 3, strongly support a causal interpretation of our main estimates. Any potential confounder would have to (i) begin to differentially affect high- and low-touristy zipcodes in 2012 (just when Airbnb started taking off), (ii) affect zipcodes with low owner-occupancy rate more than zipcodes with high owner-occupancy rate, (iii) be uncorrelated with house prices and rents in zipcodes that never had any Airbnb, but correlated with house prices and rents in zipcodes that did—even among zipcodes that ex ante look demographically similar, and (iv) it would have to be correlated over time with the Airbnb Google search index beyond the linear time trend. Moreover, the potential confounder would have to be unrelated to changes in zipcode demographic characteristics and unrelated to our measured changes in tourism demand. While we cannot completely rule out the possibility of such a confounder, it does appear that most of the plausible sources of spurious correlation are accounted for in our analysis.

Finally, in the Appendix, we show that our results are robust to a number of sensitivity and specification checks, such as using different measures of Airbnb supply and running the regression on different subsamples of the data. For example, we show that our results hold for: (i) zipcodes that are close and far from the city center, (ii) early (2011-2013) and late (2014-2016) time periods, (iii) more or less populous cities, and (iv) different housing segments.

6.3 Effect magnitudes

In this section, we consider the economic significance of our estimated effects. Our baseline result is that a 1% increase in Airbnb listings leads to a 0.018% increase in rents and a 0.026% increase in house prices at a median owner-occupancy rate zipcode. The median year-on-year growth rate in Airbnb listings was 28% across zipcodes in the top 100 CBSAs. Taken at the sample median, then, Airbnb growth explains 0.5% in annual rent growth and 0.7% of annual price growth.

Another way to calculate effect size is to calculate the Airbnb contribution to year-over-year rent and house price growth for each zipcode by multiplying median year-over-year changes in log listings by the estimated coefficients $\hat{\beta} + \hat{\gamma} \times oorate_{i,2010}$. We report these effects in Table 9 for the median zipcodes in the 10 largest CBSAs as well as for the median zipcode in our sample of 100 largest CBSAs. We also include the average year-on-year rent and price growth for comparison. While the size of the Airbnb contribution may seem large, we caution that estimating the effect at the sample median masks substantial heterogeneity in the actual experiences of different zipcodes, and ignores the very likely possibility of heterogeneous treatment effects, such as between different quality segments of the housing market.[30] We also note that our estimated effects are consistent with those found in Horn and Merante (2017) who study the effect of Airbnb on rents in Boston from 2015-2016. They find that a one standard deviation increase in Airbnb listings leads to a 0.4% increase in rents. In our data, the within-CBSA standard deviation in log listings is 0.27 for 2015-2016, which at the median owner-occupancy rate implies a 0.54% increase in rents using our estimates.

[30]Our main results only speak to the effect of Airbnb on the median housing unit as we use median rents and prices. In the Appendix, we explore the effects of Airbnb on other measures such as house prices separately for 1 through 4 bedroom homes, and rents for multifamily vs. single-family homes. The results are not very different from each other, so we opt to only report median effects in the paper.

6.4 The effect of Airbnb on housing reallocation

In Section 6, we showed that Airbnb has a positive effect on house prices and rents and that this effect is moderated by the owner-occupancy rate. This latter finding suggests that the effect of Airbnb on the housing market is likely due to non owner-occupiers reallocating their properties from the long- to the short-term rental market. As we explained in Section 3, assuming that the total housing supply is inelastic in the short-run, this reallocation would decrease long-term supply, thus increasing both rental rates and house prices.

In this section, we present direct evidence of this mechanism. To do so, we investigate the effect of Airbnb on four measures of housing supply: (i) the number of homes that are vacant for seasonal or recreational use, (ii) the number of homes vacant and for rent, (iii) the number of homes that are rented to long-term tenants (renter-occupied units), and (iv) the total housing stock, which is the sum of all renter-occupied, owner-occupied, and vacant units. We obtain this data from the American Community Survey, an annual survey administered by the U.S. Census that randomly samples individual housing units. Housing units that are found to be unoccupied, or occupied by anyone who is not the usual resident (such as an Airbnb guest), are classified as vacant. The Census then either asks the owner of the vacant unit (or the current occupant, or neighbors, if the owner cannot be reached) why the unit is vacant. Thus, homes that are held vacant for use as short-term rentals or are occupied by home-share guests at the time of the survey would be classified as vacant for seasonal or recreational use. Homes that are vacant but in which the owner is seeking a long-term tenant would be classified as vacant and for rent.[31] To summarize, short-term rental properties would be contained in measure (i) while long-term rental properties are contained in measures (ii) and (iii). Measure (iv) is the sum of all housing units.

We run regressions of the form given in equation (2) using the four housing supply variables discussed above as dependent variables. One issue with this measure is that housing supply data is not available at the zipcode level at a monthly frequency. We therefore have to use annual data, so the time-period in the regressions is a year. Moreover, to smooth out annual fluctuations due to sampling error, the ACS reports 5-year running averages of these variables. Therefore, there is

[31] Other possible reasons for vacancy include being vacant and for sale, vacant for migrant workers, either rented or sold but not yet occupied, or "other". For more information, see the U.S. Census Bureau's report titled "American Commnunity Survey Design and Methodology (January 2014)."

serial correlation in the dependent variable, which we account for by clustering standard errors at the zipcode level.

If, as we hypothesized, the effect of Airbnb is mainly due to the reallocation effect discussed above, then we would expect that Airbnb listings are associated with an increase in the short-term rental supply (measure (i)) and with a decrease in long-term rental supply (measures (i) and (ii)). Further, these changes should not be due to changes in the total housing supply. Thus, there should not be any association between Airbnb listings and such variable (measure (iv)).

Table 10 reports the results of these regressions. Column 1 shows that higher Airbnb listings lead to more homes that are vacant for seasonal or recreational use, which is consistent with an increase in the short-term rental stock. Columns 2 and 3 show that higher Airbnb listings lead to fewer homes that are vacant and for-rent and fewer homes that are renter occupied, which is consistent with a decrease in the long-term rental stock. As with the results on rents and prices, the effects are strongly moderated by the owner-occupancy rate of the zipcode with Airbnb having stronger effects in zipcodes with fewer owner-occupiers. This makes sense because, as we discussed in Section 3, non-owner-occupiers should be more likely than owner-occupiers to reallocate. Finally, there is no short-run effect of Airbnb on the total supply of housing, which is consistent with housing supply being very inelastic in the short-run (we did not test for long-run effects for reasons we discussed in Section 3).

The results reported in this section provide strong evidence that are consistent with our hypothesis that the effect of Airbnb is, at least in part, due to the reallocation of the housing stock form the long- to the short term rental market.

7 Discussion & Conclusion

The results presented in this paper suggest that the increased ability to home-share has led to increases in both rental rates and house prices. The increases in rental rates and house prices occur through at least two channels. In the first channel, home-sharing increases rental rates by inducing some landlords to switch from supplying the market for long-term rentals to supplying the market for short-term rentals. The increase in rental rates through this channel is then capitalized into house prices. In the second channel, home-sharing increases house prices directly by enabling homeowners to generate income from excess housing capacity. This raises the value of owning relative to renting and therefore increases the price-to-rent ratio directly.

The results of this paper contribute to the debate surrounding home-sharing and its impact on the housing market. While Airbnb and proponents of the sharing economy argue that the platform is not responsible for higher house prices and rental rates,[32] critics of home-sharing argue that Airbnb does raise housing costs for local residents. This paper provides evidence supporting the latter hypothesis, and it does so using the most comprehensive dataset about home-sharing in the US available to date.

Moreover, by showing that the effects of Airbnb are moderated by the owner-occupancy rate, this paper highlights the importance of the marginal homeowner in terms of reallocation (since owner-occupiers may be less likely to reallocate their housing to the permanent short-term rental stock). Thus, this paper demonstrates that the marginal propensity of homeowners to reallocate housing from the long- to the short-term rental market is a key elasticity determining the overall effect of home-sharing.

Turning to how cities and municipalities should deal with the steady increase in home-sharing, our view is that regulations on home-sharing should (at most) seek to limit the reallocation of housing stock from long-term rentals to short-term rentals without discouraging the use of home-sharing by owner-occupiers. One regulatory approach could be to only levy occupancy tax on home sharers who rent the entire home for an extended period of time or to require a proof of owner-occupancy in order to avoid paying occupancy tax.

Of course, this research does not come without limitations. First, we must recognize that our Airbnb data is imperfect: While we observe properties listed on Airbnb, we do not observe exact entry and exit of these properties. However, using Airbnb proprietary data, Farronato and Fradkin (2018) obtain very similar elasticity estimates to Zervas et al. (2017) who use a similar approach to ours to obtain Airbnb data and measure Airbnb supply. This, along with our extensive set of robustness checks, reassures us about the validity of our results.

Second, we need to keep in mind that in settings where the effects are likely to be heterogeneous, a 2SLS estimate does not represent the Average Treatment Effect (ATE) but instead a Local

[32]For example, Airbnb disputed the findings of a recent report on the effects of the platform on the housing market in New York City. See: `https://www.citylab.com/equity/2018/03/what-airbnb-did-to-new-york-city/552749/`.

Average Treatment Effect (LATE) or the effect of Airbnb on the subset of "complier" zipcodes–those zipcodes that are induced by the instrument to change the value of the endogenous regressor. Thus, our estimates do not necessarily reflect the average effect of Airbnb on any zipcodes. Despite this limitation, however, we estimate magnitudes that are similar to those obtained by Horn and Merante (2017) for the city of Boston. Finally, our results do not take into account possible spillover effects the neighboring zipcodes can have on each other.

To summarize the state of the literature on home-sharing, research (including this paper) has found that home-sharing: 1) raises local rental rates by causing a reallocation of the housing stock, 2) raises house prices through both the capitalization of rents and the increased ability to use excess capacity, and 3) induces market entry by small suppliers of short-term housing who compete with traditional suppliers (Zervas et al., 2017). More research is needed, however, in order to achieve a complete welfare analysis of home-sharing. For example, home-sharing may have positive spillover effects on local businesses if it drives a net increase in tourism demand (Alyakoob and Rahman, 2018). On the other hand, home-sharing may have negative spillover effects if tourists create negative externalities such as noise or congestion for local residents (Filippas and Horton, 2018). Moreover, home-sharing introduces an interesting new mechanism for rapidly scaling down the local housing supply in response to negative long-term demand shocks and a mechanism for rapidly scaling up the short-term accommodations supply in response to a short-term demand serge (Farronato and Fradkin, 2018). Understanding the full impact of such a mechanism on the housing market is an open question to date. We leave these research questions for future work.

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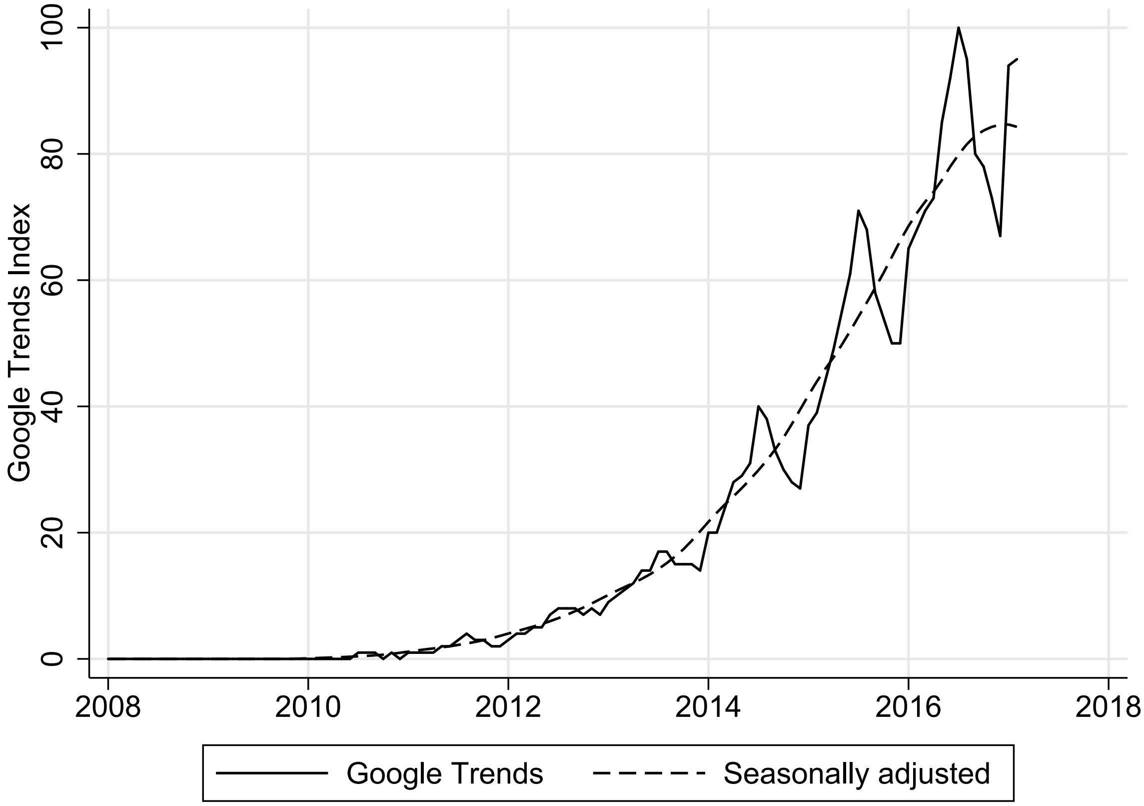
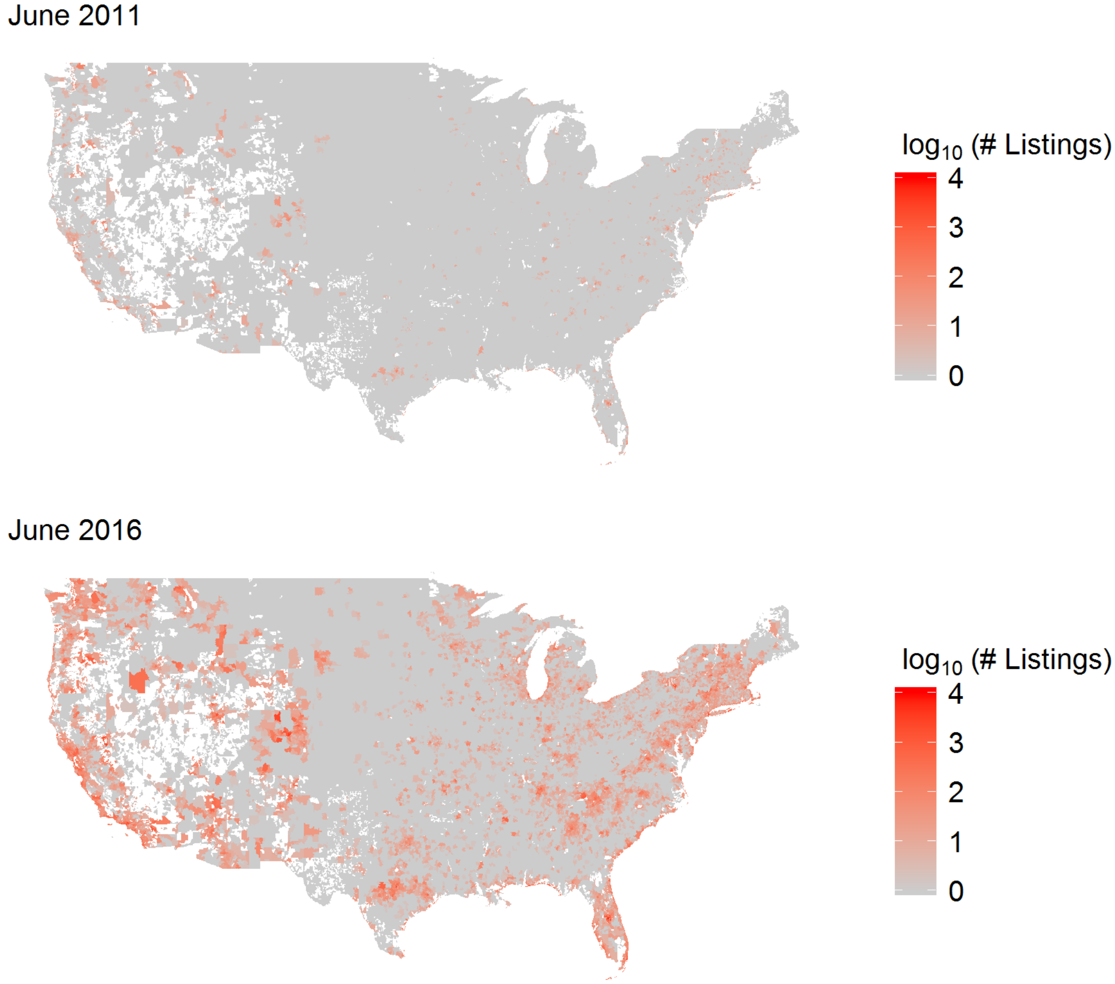
Figure 1: Google Trends Search Index for Airbnb (Worldwide, 2008-2017)



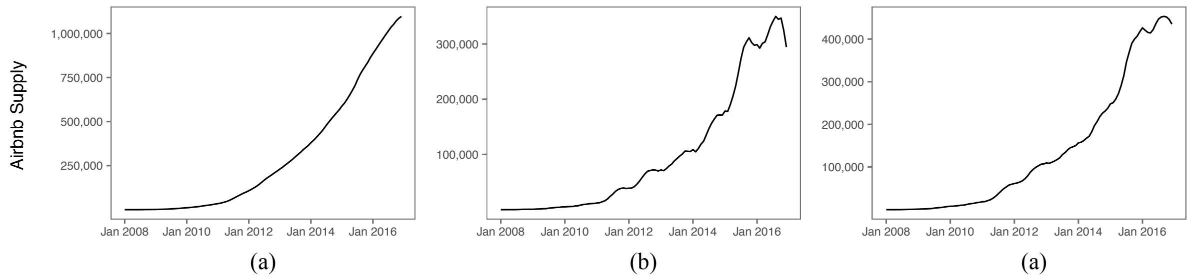
Note: Monthly Google Trends index for the single English search term "Airbnb", from any searches worldwide. Google Trends data are normalized so that the date with the highest search volume is given the value of 100. Seasonal adjustment is done using a local polynomial smoother.

Figure 2: Map of Airbnb Listings by Zipcode, 2011-2016



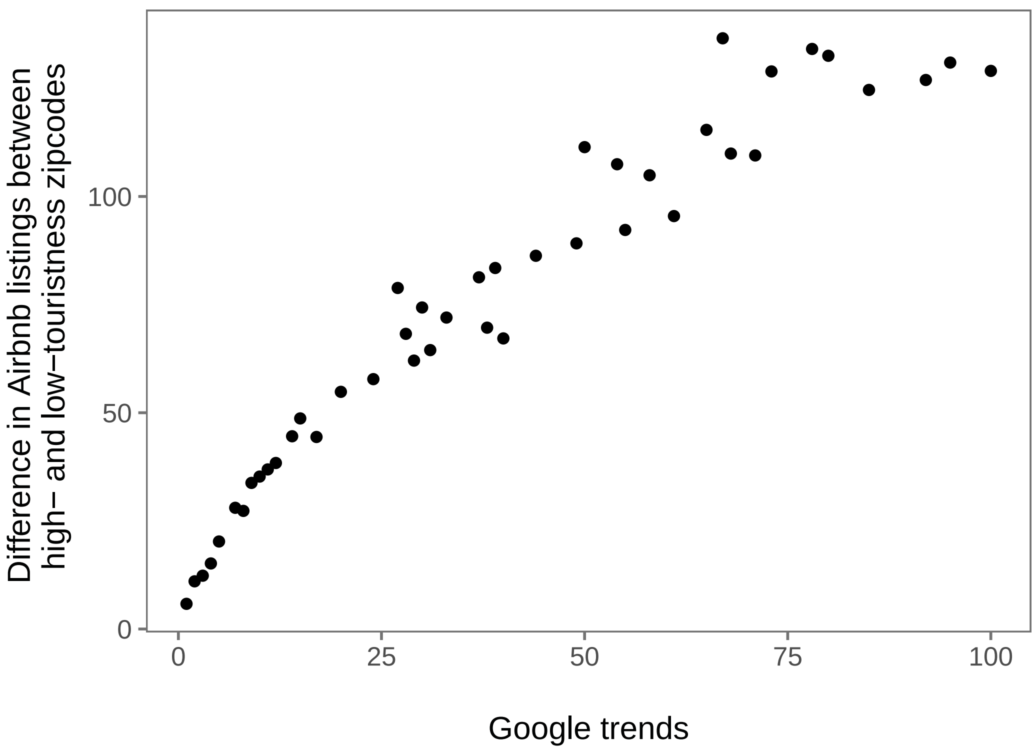
Note: The figure shows the spatial distribution of Airbnb listings in June 2011 and June 2016, where the number of listings is calculated using method 1 in Table 1. Listings are reported in logs, and log listings is set to zero if there are zero listings. Geographic areas without zipcode boundary information are colored white.

Figure 3: Total Number of Airbnb Listings (US, 2008-2016)



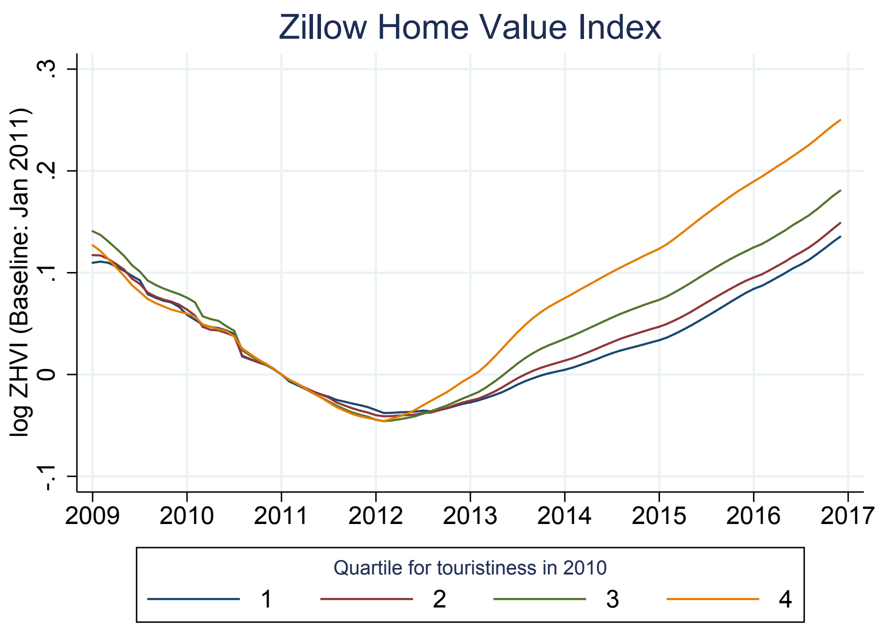
Note: This figure plots the number of Airbnb listings over time, using each of the 3 methods described in Table 1: (a) method 1 – cumulative supply, (b) method 2 – TTL = 3 months, (c) method 3 – TTL = 6 months.

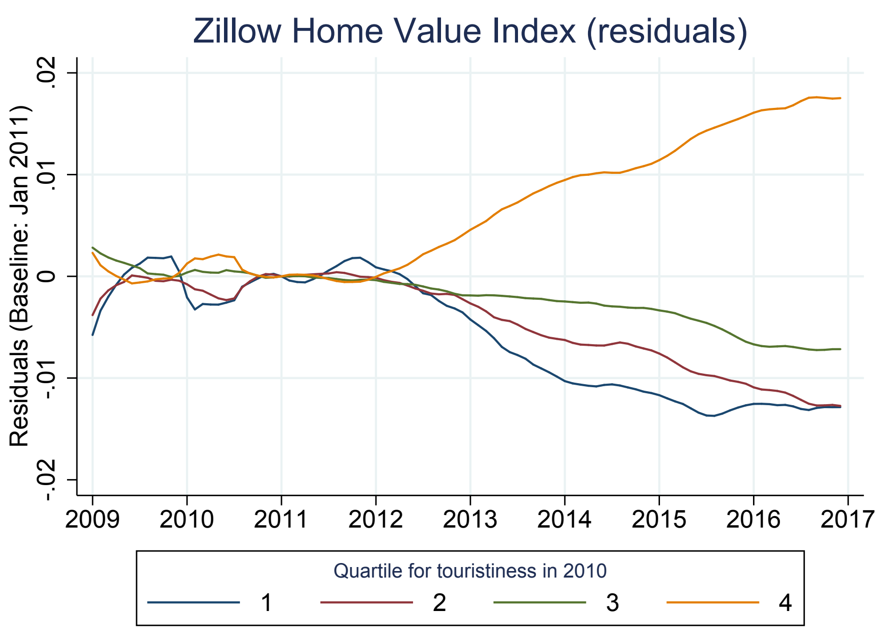
Figure 4: Testing the IV Operating Assumption



Note: This figure plots the difference in the number of Airbnb listings for high- and low-touristiness zipcodes over the Google trend values. We use the sample median value of touristiness to create two equally sized groups of high- and low-touristiness zipcodes.

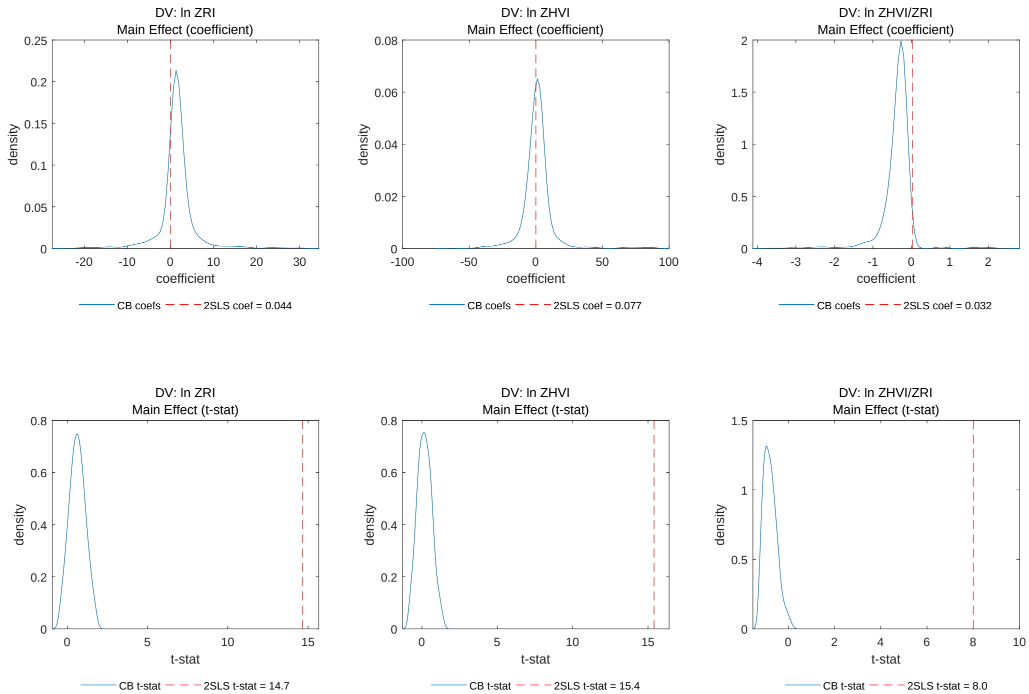
Figure 5: Trends in Zillow Home Value Index by "Touristiness" of Zipcode





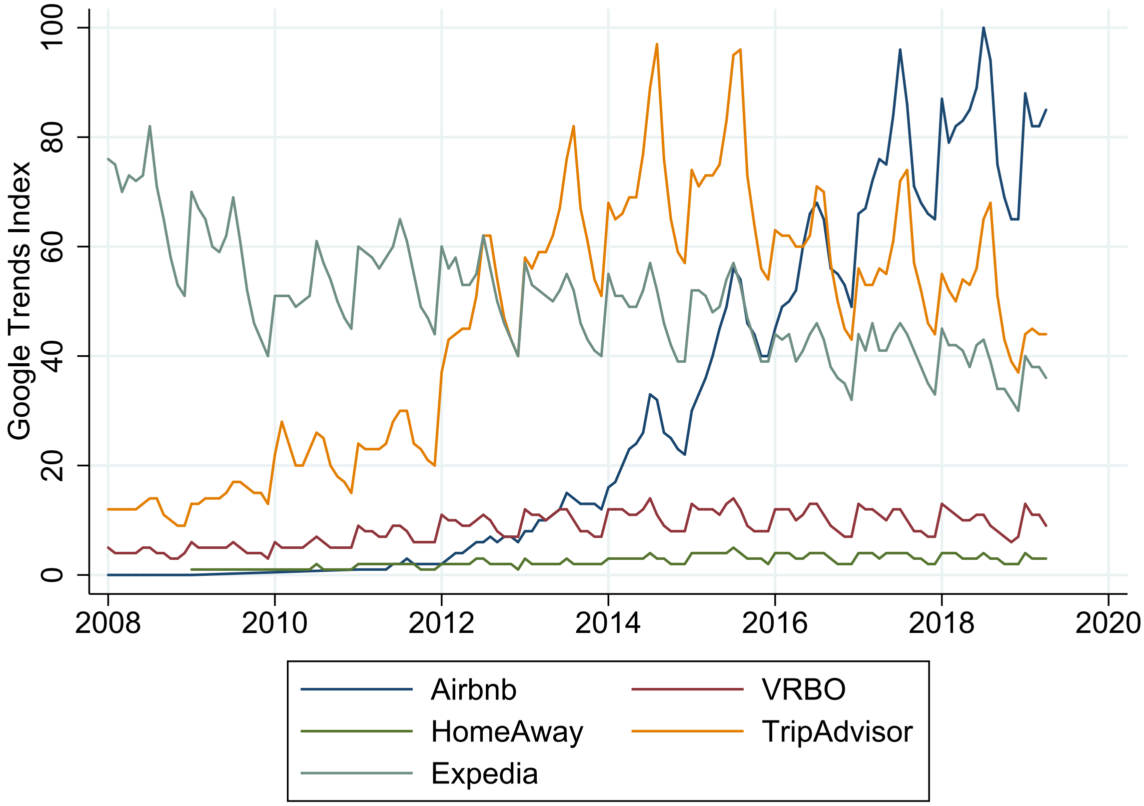
Note: The top panel plots the ZHVI index, normalized to January 2011=0, averaged within different groups of zipcodes based on their level of "touristiness" in 2010. Touristiness is measured as the number of establishments in the food services and accommodations sector (NAICS code 72) in 2010, and the zipcodes are separated into four equally sized groups. The bottom panel plots the residuals from a regression of the ZHVI on zipcode fixed effects and CBSA-month fixed effects.

Figure 6: Christian and Barrett (2017) Exercise Results



Note: This figure shows the distribution of the t-statistics for both the main coefficient on *Airbnb* and the coefficient on the interaction term $Airbnb \times oorate$ for our three dependent variables using the Christian and Barrett (2017) randomized regressors described in Section 5.1.

Figure 7: Google Trends Index for Related Websites (Worldwide, 2008-2019)



Note: Monthly Google Trends index for various tourism related websites, from any searches worldwide. Google Trends data are normalized so that the highest search volume over all the compared terms and time-periods is equal to 100.

Table 2: Size of Airbnb Relative to the Housing Stock (zipcodes, 100 largest CBSAs)

	p10	p25	p50	p75	p90
June 2011					
Airbnb Listings	0	0	0	2	7
Housing Units	1,058	2,813	7,437	12,829	18,037
Airbnb Listings as a Percentage of					
Total Housing Units	.00	.00	.00	.02	.09
Renter-occupied Units	.00	.00	.00	.06	.33
Vacant Units	.00	.00	.00	.20	.92
Vacant-for-rent Units	.00	.00	.00	1.01	5.06
June 2016					
Airbnb Listings	1	4	13	44	144
Housing Units	1,097	2,926	7,610	13,219	18,443
Airbnb Listings as a Percentage of					
Total Housing Units	.03	.08	.21	.60	1.88
Renter-occupied Units	.13	.33	.87	2.50	7.31
Vacant Units	.37	.99	2.63	7.19	20.00
Vacant-for-rent Units	1.72	4.65	13.70	42.80	129.00

Note: This table reports the size of Airbnb relative to the housing stock by zipcodes for the 100 largest CBSAs as measured by 2010 population. The number of Airbnb listings is calculated using method 1 in Table 1. Data on housing stocks, occupancy characteristics, and vacancies come from ACS zipcode level 5-year estimates.

Table 3: IV Validity Check: Correlation Between Instrument and Rents/Prices in Zipcodes Without Airbnb

	Sample: Zipcodes w/o Airbnb ever		Sample: Zipcodes w/ some Airbnb		Sample: Propensity-Score matched sample w/ Airbnb	
	(1) DV: ln ZRI	(2) DV: ln ZHVI	(3) DV: ln ZRI	(4) DV: ln ZHVI	(5) DV: ln ZRI	(6) DV: ln ZHVI
$g_t \times h_{i,2010}$	−1.63E−06	−3.92E−06	3.17E−06***	5.38E−06***	9.88E−06***	8.77E−06*
	(3.17E−06)	(4.48E−06)	(2.22E−07)	(3.43E−07)	(3.46E−06)	(4.52E−06)
ln Population	0.011	0.045***	0.055***	0.087***	0.052***	0.077***
	(0.013)	(0.016)	(0.007)	(0.011)	(0.019)	(0.029)
ln Median HH Income	−0.002	−0.001	0.027***	0.017*	0.013	0.008
	(0.011)	(0.016)	(0.006)	(0.009)	(0.015)	(0.023)
College Share	0.054*	0.120***	0.057***	0.058***	0.052	−0.053
	(0.032)	(0.038)	(0.014)	(0.020)	(0.034)	(0.063)
Employment Rate	0.045	−0.017	0.044***	0.126***	0.011	0.133**
	(0.031)	(0.033)	(0.015)	(0.023)	(0.036)	(0.060)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	61854	50875	587141	520797	51800	42082
R^2	0.979	0.994	0.992	0.996	0.982	0.993

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: This table reports regression results when outcomes of interest are regressed on the instrumental variable directly for three samples of zipcodes: 1) zipcodes that were never observed to have any Airbnb listings, 2) zipcodes that were observed at some point to have Airbnb listings, and 3) zipcodes that had Airbnb listings and were propensity-score matched to zipcodes that did not have Airbnb listings. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis.

Table 4: Comparing Airbnb and Non-Airbnb Zipcodes

	Sample: Zipcodes w/o Airbnb	Sample: Zipcodes w/ some Airbnb	Sample: Propensity-Score matched sample w/ Airbnb
Touristiness	7.40	43.73***	7.34
ln Median Income	10.90	11.05***	10.90
ln Population	8.25	9.44***	8.28
College Share	0.19	0.34***	0.19
Employment Rate	0.72	0.73***	0.72
# Zipcodes	999	9356	999

Note: This table reports mean zipcode characteristics from three samples: 1) zipcodes that were never observed to have any Airbnb listings, 2) zipcodes that were observed at some point to have Airbnb listings, and 3) zipcodes that had Airbnb listings and were propensity-score matched to zipcodes that did not have Airbnb listings. *** indicates that the difference in means compared to the non-Airbnb sample is statistically significant with p<0.01.

Table 5: The Effect of Airbnb on Rental Rates

	(1)	(2)	(3)	(4)	(5)	(6)
ln Airbnb Listings	0.098***	0.008***	0.022***	0.021***	0.046***	0.044***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)
... × Owner-occupancy Rate (2010)			−0.023***	−0.022***	−0.038***	−0.036***
			(0.002)	(0.002)	(0.003)	(0.003)
ln Population				0.050***		0.042***
				(0.007)		(0.007)
ln Median HH Income				0.021***		0.017***
				(0.005)		(0.006)
College Share				0.063***		0.057***
				(0.013)		(0.013)
Employment Rate				0.048***		0.036***
				(0.014)		(0.014)
Zipcode FE	No	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	No	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	No	No	No	No	Yes	Yes
Observations	649841	649841	649841	649697	649841	649697
R^2	0.169	0.991	0.991	0.991	0.991	0.991
Kleinbergen-Paap F Statistic					820.0	807.0

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. To avoid taking the log of a zero, one is added to the number of Airbnb listings before taking logs. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 6: The Effect of Airbnb on House Prices

	(1)	(2)	(3)	(4)	(5)	(6)
ln Airbnb Listings	0.175***	0.009***	0.040***	0.038***	0.080***	0.077***
	(0.004)	(0.001)	(0.002)	(0.002)	(0.005)	(0.005)
... × Owner-occupancy Rate (2010)			−0.048***	−0.046***	−0.074***	−0.071***
			(0.003)	(0.003)	(0.006)	(0.006)
ln Population				0.077***		0.063***
				(0.010)		(0.010)
ln Median HH Income				0.012		0.005
				(0.008)		(0.008)
College Share				0.073***		0.061***
				(0.018)		(0.018)
Employment Rate				0.099***		0.070***
				(0.020)		(0.020)
Zipcode FE	No	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	No	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	No	No	No	No	Yes	Yes
Observations	572858	572858	572858	572805	572858	572805
R^2	0.188	0.996	0.996	0.996	0.996	0.996
Kleinbergen-Paap F Statistic					661.9	646.7

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. To avoid taking the log of a zero, one is added to the number of Airbnb listings before taking logs. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 7: The Effect of Airbnb on Price-to-Rent Ratio

	(1)	(2)	(3)	(4)	(5)	(6)
ln Airbnb Listings	0.077***	0.002**	0.016***	0.015***	0.032***	0.032***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)	(0.004)
… × Owner-occupancy Rate (2010)			−0.022***	−0.022***	−0.032***	−0.032***
			(0.003)	(0.003)	(0.005)	(0.005)
ln Population				0.030***		0.025**
				(0.010)		(0.010)
ln Median HH Income				−0.013		−0.016*
				(0.009)		(0.009)
College Share				0.011		0.006
				(0.019)		(0.019)
Employment Rate				0.046**		0.034
				(0.022)		(0.022)
Zipcode FE	No	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	No	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	No	No	No	No	Yes	Yes
Observations	537157	537142	537142	537089	537142	537089
R^2	0.154	0.979	0.979	0.979	0.979	0.979
Kleinbergen-Paap F Statistic					629.8	616.9

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. To avoid taking the log of a zero, one is added to the number of Airbnb listings before taking logs. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 8: Controlling for Measures of Tourism Demand

	DV: ln ZRI					DV: ln ZHVI				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ln Airbnb Listings	0.040***	0.044***	0.044***	0.044***	0.044***	0.071***	0.078***	0.077***	0.085***	0.046***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.008)
... × Owner-occupancy Rate (2010)	−0.034***	−0.035***	−0.036***	−0.036***	−0.036***	−0.069***	−0.071***	−0.071***	−0.076***	−0.056***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)
ln Population	0.043***	0.044***	0.041***	0.042***	0.042***	0.069***	0.071***	0.063***	0.062***	0.073***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
ln Median HH Income	0.018***	0.018***	0.018***	0.017***	0.017***	0.003	0.004	0.005	0.005	0.006
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
College Share	0.052***	0.055***	0.056***	0.057***	0.057***	0.053***	0.057***	0.061***	0.060***	0.066***
	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.018)	(0.019)	(0.018)	(0.018)	(0.018)
Employment Rate	0.038***	0.038***	0.037***	0.036***	0.036**	0.079***	0.078***	0.071***	0.067***	0.087***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Food & Accommodations Estabs.	3.48E−04***					5.83E−04***				
	(7.15E−05)					(1.25E−04)				
ln Hotel Occupancy		4.83E−04***					4.31E−04			
		(1.83E−04)					(2.77E−04)			
ln Airport Travelers (arrivals)			3.36E−04***					1.37E−04		
			(1.18E−04)					(1.79E−04)		
# Trip Advisor Reviews				1.38E−07					−3.58E−05***	
				(3.55E−06)					(1.08E−05)	
$t \times h_{i,2010}$					−7.94E−08					3.58E−06***
					(9.31E−07)					(1.07E−06)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	639400	639400	648905	649697	649697	564157	564157	572085	572805	572805
R^2	0.991	0.991	0.991	0.991	0.991	0.996	0.996	0.996	0.996	0.996
Kleinbergen-Paap F Statistic	650.0	761.5	802.8	791.9	167.1	516.1	611.9	642.5	579.2	218.3

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. To avoid taking the log of a zero, one is added to the number of Airbnb listings before taking logs. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 9: Effect Magnitudes for 10 Largest CBSAs

	Year-over-Year Airbnb Contribution		Year-over-Year Growth	
CBSA	Rent	Price	Rent	Price
Top 100 CBSAs	0.59%	0.82%	3.18%	5.70%
New York-Newark-Jersey City, NY-NJ-PA	0.60%	0.83%	3.64%	3.55%
Los Angeles-Long Beach-Anaheim, CA	1.14%	1.79%	4.92%	9.66%
Chicago-Naperville-Elgin, IL-IN-WI	0.34%	0.44%	2.25%	3.98%
Dallas-Fort Worth-Arlington, TX	0.70%	1.01%	4.18%	8.21%
Miami-Fort Lauderdale-West Palm Beach, FL	1.02%	1.51%	4.51%	11.72%
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	0.54%	0.73%	1.94%	2.05%
Houston-The Woodlands-Sugar Land, TX	0.95%	1.37%	4.67%	8.34%
Washington-Arlington-Alexandria, DC-VA-MD-WV	0.70%	0.96%	1.28%	4.41%
Atlanta-Sandy Springs-Roswell, GA	0.75%	1.07%	3.11%	8.42%
Detroit-Warren-Dearborn, MI	0.16%	0.21%	2.41%	8.54%

Note: Airbnb contribution is calculated as $\hat{\beta} + \hat{\gamma} oorate_{ic,2010}$ multiplied by the median year-over-year growth in log Airbnb listings for each zipcode, and then taken at the median zipcode. Estimates from columns 6 of Tables 5 and 6 are used.

Table 10: The Effect of Airbnb on Housing Supply

	(1) ln Vacant Seasonal	(2) ln Vacant For-Rent	(3) ln Rental Stock	(4) ln Housing Stock
ln Airbnb Listings	0.078***	−0.048*	−0.036***	−0.002
	(0.025)	(0.025)	(0.006)	(0.002)
... × Owner-occupancy Rate (2010)	−0.018	0.045*	0.053***	−0.002
	(0.032)	(0.027)	(0.005)	(0.003)
ln Population	−0.212***	−0.225***	0.871***	0.547***
	(0.055)	(0.079)	(0.032)	(0.019)
ln Median HH Income	0.051	0.151***	−0.457***	−0.074***
	(0.046)	(0.055)	(0.026)	(0.011)
College Share	−0.016	−0.052	−0.177***	0.100***
	(0.116)	(0.152)	(0.066)	(0.024)
Employment Rate	0.093	−0.465***	0.315***	0.146***
	(0.123)	(0.152)	(0.068)	(0.033)
Zipcode FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes
Observations	49282	49580	61435	61720
R^2	0.913	0.927	0.993	0.999
Kleinbergen-Paap F Statistic	742.4	587.2	1082.9	1099.4

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: Definitions of the dependent variables are given in Section 6.4. The number of Airbnb listings is calculated using method 1 in Table 1. To avoid taking the log of a zero, one is added to the number of Airbnb listings before taking logs. Clustered standard errors at the zipcode level are reported in parentheses.

For Online Publication: Appendix

A Monte Carlo Simulation of Placebo Test

In Section 5.1, we describe the test proposed by Christian and Barrett (2017) (CB exercise) in which a randomized regressor is constructed by keeping the identity of which zipcodes had non-zero Airbnb listings but swapping the actual number of Airbnb listings across zipcodes. The randomized regressor preserves the overall time trends in the number of Airbnb listings but randomizes the intensive margin of Airbnb growth experienced by each zipcode, thus eliminating the impact of touristiness on the intensive margin of Airbnb listings. If the results are primarily driven by spurious time trends that interact with the *extensive margin* of Airbnb listings, then the exercise will produce 2SLS estimates that continue to be positive and statistically significant. This is what happens when Christian and Barrett (2017) apply their exercise to the Nunn and Qian (2014) instrument for U.S. Food Aid, which they were critiquing. In our case, if the effect of touristiness on the intensive margin of Airbnb listings is what really matters, then the first-stage will become very weak when regressing the randomized regressor on the instrument. The weak first stage results in a high variance of second stage estimates when performing 2SLS using the randomized regressor and statistically insignificant estimates.

Here, we demonstrate using a Monte Carlo simulation with an instrument that is known to be valid. We will show that the CB exercise leads to a large variance of point estimates that are statistically insignificant. Consider a model in which X_{it} is our endogenous explanatory regressor and Y_{it} is our outcome of interest. The causal relation is:

$$Y_{it} = \beta X_{it} + \epsilon_{it} \tag{3}$$

and X_{it} is a non-negative regressor given by:

$$X_{it} = \begin{cases} \gamma_0 + \gamma_1 H_i \times G_t + \nu_{it} & \text{if } c_{it} = 0 \\ 0 & \text{if } c_{it} = 1 \end{cases} \tag{4}$$

The censoring indicator, c_{it}, controls whether X_{it} is zero. For simplicity, we assume that γ_0 is large enough so that $X_{it} > 0$ when $c_{it} = 0$. Crucially, we assume that:

$$P(c_{it} = 1) = \frac{1}{1 + \exp(3 + \nu_{it})} \tag{5}$$

so that c_{it}, which controls the extensive margin of X_{it}, is exogenous to $H_i \times G_t$ but endogenous to ν_{it}. c_{it} is analogous to whether the zipcode i had zero Airbnb listings in time t.

To induce correlation between X_{it} and ϵ_{it}, which necessitates an instrument, we introduce a spurious time trend D_t, which interacts with H_i, that simultaneously affects ν_{it} and ϵ_{it}:

$$\nu_{it} = \theta D_t \times H_i + \xi_{it} \tag{6}$$

$$\epsilon_{it} = \phi D_t \times H_i + \eta_{it} \tag{7}$$

We simulate the above system using $\beta = 1$, $\gamma_0 = 10$, $\gamma_1 = \theta = \phi = 1$. We draw H_i, G_i, D_t, ξ_{it}, and η_{it} from iid standard normal distributions, which implies that $E\left[\epsilon_{it} | H_i \times G_t\right] = 0$ and thus $Z_{it} = H_i \times G_t$ is a valid instrument for X_{it}.

To summarize this model, Y_{it} is analogous to rent, X_{it} is analogous to Airbnb, H_i is analogous to touristiness, and G_t is analogous to the Google trends. In the Monte Carlo, X_{it} is endogenous through the correlation between ν_{it} and ϵ_{it}, which is driven by a spurious time trend that interacts with touristiness: $D_t \times H_i$.

OLS estimation of equation (3) results in a biased estimate of $\hat{\beta}_{OLS} = 1.151^{***}(0.002)$. 2SLS estimation of equation (3) using $Z_{it} = H_i \times G_t$ as the instrument for X_{it} results in an estimate of $\hat{\beta}_{IV} = 0.993^{***}(0.007)$. The true parameter, $\beta = 1$, is contained in the 95% confidence interval of the 2SLS estimate, which is expected since Z_{it} is known to be a valid instrument.

We then perform the CB exercise using the simulated data, which essentially amounts to randomly swapping X_{it} among the i's with $c_{it} = 0$, without changing any other variables. We do this 500 times. The top left panel of Figure 8 plots the density of the resulting estimates along with the baseline 2SLS estimate from the non-randomized Monte-Carlo data. The variance of the CB estimates is very large and the 2SLS estimate for β as well as the true parameter are well within the 5th-95th-percentile-range of the CB estimates. For comparison, in the top right panel of Figure 8, we reproduce the same plot using the data from our paper when the dependent variable is ln

ZRI. We can observe that the results of the CB exercise using our data and our instrument are very similar to those obtained with a valid simulated instrument.

The bottom left panel of Figure 8 then shows the density of the t-statistics of the CB estimates using the Monte Carlo data along with the t-stat of the baseline 2SLS estimate. In the bottom right panel, we reproduce the same plot using the data from our paper. As before, we observe that the results of the CB exercise using the data from our paper is very simlar to those obtained in the Monte Carlo with a valid simulated instrument; that is, the t-stats in the CB exercise are small and centered around zero, whereas the t-stat of the main 2SLS estimate is large and implies statistical significance.

Finally, we use our Monte Carlo simulation to explore what happens when we use an invalid instrument. We note that $\tilde{Z}_{it} = D_t \times H_i$ will be correlated with ϵ_{it} through a spurious time trend and is thus not a valid instrument. Indeed, when we perform 2SLS using $\tilde{Z}_{it}$ as an instrument for X_{it}, we estimate $\hat{\beta}_{invalidIV} = 1.671^{***}(0.006)$, which is not close to the true parameter. Moreover, $\tilde{Z}_{it}$, by design, affects the extensive margin of X_{it}, and therefore we expect to find statistically significant estimates when we perform the CB randomization. The top left panel of Figure 9 shows the resulting coefficient estimates from the CB exercise, and indeed they are shifted to the right of main estimate with non-randomized data. Moreover, the bottom left panel shows the t-stats of the CB exercise and shows that all of the CB estimates are statistically significant. These results are consistent with what Christian and Barrett (2017) show in Figure 6 of their paper (reproduced in the top right panel of Figure 9), which they use as an argument for the invalidity of the instrument they are critiquing.

Given these results, our conclusion is that, given a valid instrument, the CB exercise should produce estimates with a very large variance that encompasses the baseline estimate, and such estimates should be statistically insignificant. These are exactly the results we obtain using our data and instrument.

B Robustness Checks

Alternative measures of Airbnb supply

In this Appendix, we show that our results are robust to a number of specification and sensitivity checks. First, we show that our main results are robust to alternative methods of calculating Airbnb supply. As discussed in Section 4, we can never know the exact number of active Airbnb listings at any given point in time, and we therefore construct Airbnb supply using the three methods of Table 1. In Table 11, we report regression results when methods 2 and 3 are used to measure Airbnb supply instead of method 1. The results are barely changed, which is not surprising given the high correlation between the three measures, despite level differences.

In our main results, we use a log-log specification to measure the effect of Airbnb listings on house prices and rental rates because such specification provides us with easily interpretable coefficients in the form of elasticity that is often used in competitive settings and has been used in the past in the context of Airbnb (Zervas et al., 2017; Farronato and Fradkin, 2018). However, as Zervas et al. (2017) observe, the log-log specification implies constant elasticity, an assumption that might not hold in our setting. To make sure that our results are not driven by the log-log choice, we use an alternative specification in which $Airbnb_{ict}$ in equation (2) is measured as the number of Airbnb listings divided by the total occupied housing stock.[33] We call this measure "Airbnb density." One of the downsides of the log-density specification is that Airbnb density is extremely skewed[34], and using $g_t \times h_{i,2010}$ as the instrument, the first stage becomes very weak and we fail to reject underidentification.[35] We therefore report results using an augmented set of instruments. First, we try $g_t \times h_{i,2010}/stock_{i,2010}$ interacted with $oorate_{i,2010}$, where $stock_{i,2010}$ is the total occupied housing stock in 2010. Second, we try a third order polynomial of $g_t \times h_{i,2010}$ (our original instrument), interacted with $oorate_{i,2010}$. Third, we try fully interacted second order polynomials of g_t, $h_{i,2010}$, and $oorate_{i,2010}$. We report the results in Table 12. The main results hold qualitatively: (i) higher Airbnb density leads to higher house prices and rental rates, (ii) the effect is higher for house prices than for rental rates, and (iii) the effect is decreasing in owner-occupancy

[33] Data on total occupied housing stock is from ACS 5-year estimates from 2011 to 2016.

[34] The skewness is 129.58 compared to a mean of 0.007 and variance of 0.06.

[35] In the rent regression, an underidentification test using the Kleibergen and Paap (2006) rk LM statistic fails to reject underidentification with a p-value of 0.6650.

rate. However, the coefficients are somewhat sensitive to the choice of instruments. Thus, the log-log specification, which has proven to be very robust, remains our preferred specification.

Finally, recall that in our main specification we added one to the number of Airbnb listings to avoid taking logs of zero. We now show that the results are robust to this choice. Table 13 reports regression results when instead of adding one to the number of listings, we instead simply drop all zipcode-year-month observations in which the number of listings is zero. We also try dropping all observations where the number of listings is less than 5. The results remain qualitatively and quantitatively similar to the main results, suggesting both that adding one to the number of Airbnb listings does not affect the results but also that the results are not primarily being driven by zipcodes with very few Airbnb listings.

Heterogeneous effects across subsamples

We now test whether the results are heterogeneous or homogeneous across various subsamples of our data. We find that the effects may be heterogeneous but that the main qualitative result that Airbnb has a positive effect on prices and rents and is moderated by the owner-occupancy rate holds across subsamples.

First, we test whether Airbnb has different effects by distance to the city-center. We run the 2SLS regressions for two subsamples: the sample of zipcodes below the median distance to the city center, and the sample of zipcodes above the median distance to the city center, where the median is taken within CBSA. The results are reported in Table 14. The qualitative results hold in both the near and far samples, though it seems that the effects are actually larger in the far group. This confirms that the results are not being solely driven by a few zipcodes close to downtown areas and that home-sharing is having an impact even on zipcodes that are further from the city center.

Second, we test whether Airbnb has different effects in two time periods: 2011-2013 and 2014-2016. The results are reported in Table 15. Again, the main qualitative results can be seen in both time periods though the effect of owner-occupancy rate seems to be a lot weaker in the earlier period than in the later period, and the results on price-to-rent ratio are not significant in the earlier period. We speculate that this could be due to the possibility that Airbnb first attracted those users with spare rooms or houses not in the long-term market (i.e., vacation homes) and that only recently did Airbnb become an attractive option for landlords that previously rented in the long-term market.

Third, we repeat the regressions separately for the 30 largest CBSAs and for the CBSAs ranked 31-100 in terms of 2010 population. Table 16 reports the results. The qualitative results hold for both samples, though the results are not statistically significant in the rank 31-100 sample when the dependent variable is price-to-rent ratio. The effects of Airbnb appear to be stronger in the larger cities, which could be driven by a number of factors, including differences in housing demand and the tightness of the rental market.

Effects on subsegments of the housing market

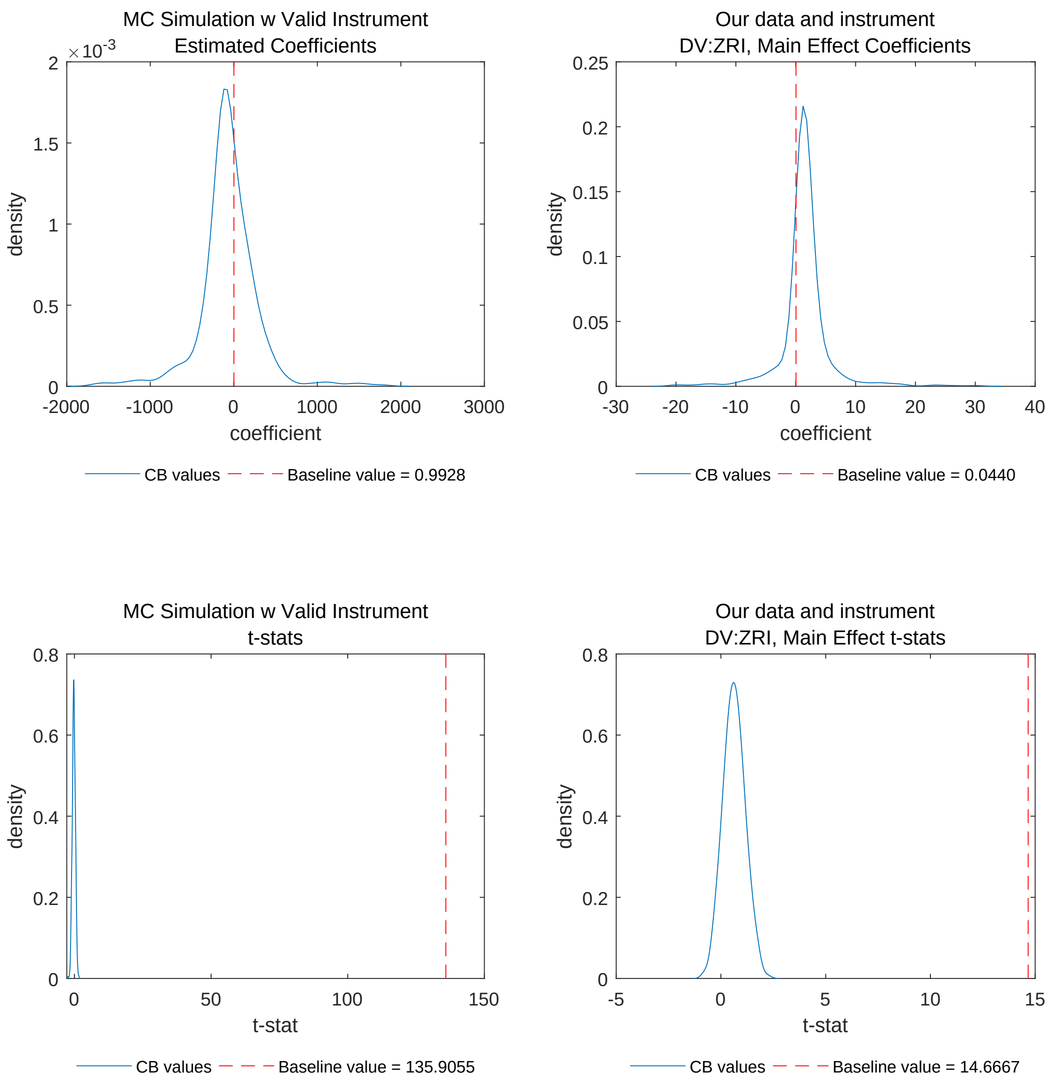
Now we test whether Airbnb has different effects on different subsegments of the housing market. In our main regressions, we used the ZRI and the ZHVI for our dependent variables, both of which measure the median rent/home value for the stock of homes in a zipcode. Thus, the results do not speak to whether or not Airbnb can have differential effects on different quality segments of the housing market. To test this, we now run the regressions using six additional rent and price measures. For rents, Zillow provides a separate index for rentals of multifamily and single-family units. For prices, Zillow provides a separate house price index for homes with 1, 2, 3, and 4 bedrooms (including both condos and single-family detached units.) The results are reported in Table 17. The results show that the effects are not too different across different subsegments of the housing market.

Additional tests

Now we show that our results are robust to the inclusion of the Google Trends index for other tourism-based websites interacted with baseline touristiness. This is an extension of the controls for tourism discussed in Section 6.2 and also addresses the extent to which our results reflect home-sharing in total or primarily the effect of Airbnb (though it is hard to fully separate as many listings are cross-listed across multiple platforms and Google searches are not a perfect proxy for actual usage.) Table 18 shows these results and shows that our results are robust to the inclusion of any of these controls.

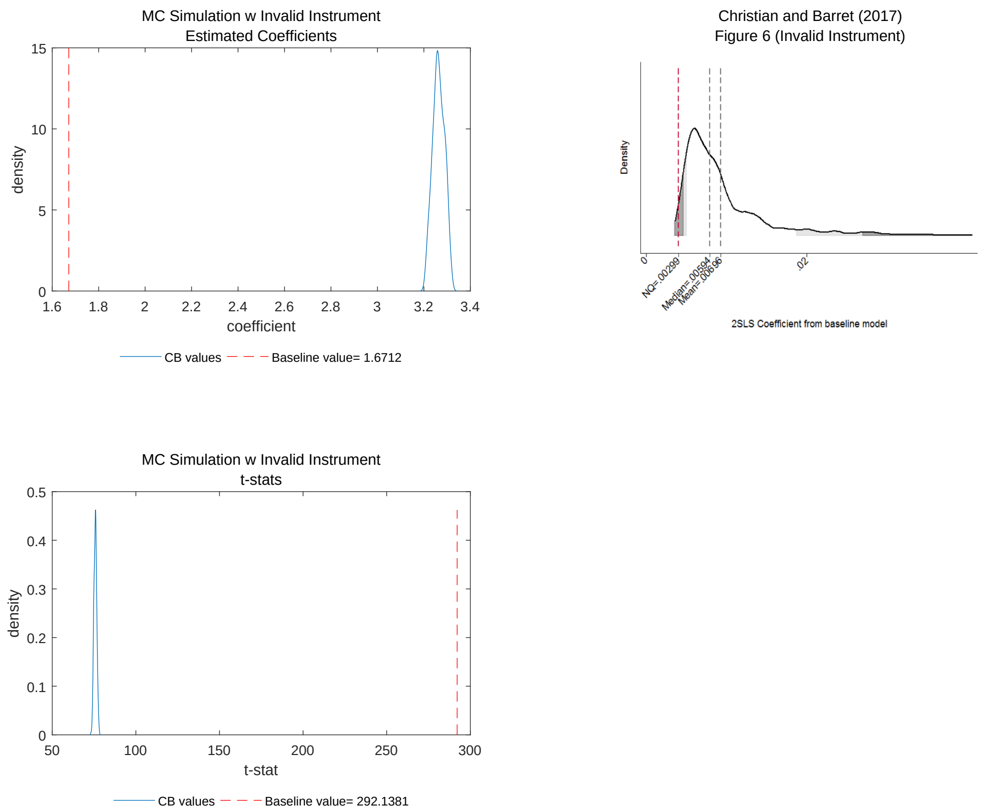
Finally, we show that our results are robust to using forward-looking measures of zipcode demographics. This addresses the concern that house prices and rental rates adjust more quickly than demographic composition. In Table 19, we control for contemporaneous, 1-year ahead, and 2-year ahead measures of demographic characteristics. The results are robust to either choice. Note that when we use the 2-year ahead demographic variables, the results look more like the results from the 2011-2013 sample in Table 15 because we only use data up to 2014 since our demographic variables are only measured until 2016. Thus, the differences in controlling for 1-year ahead and 2-year ahead demographic variables can be attributed more to changes to the sample than the choice of when to measure the demographics.

Figure 8: Monte Carlo Results for CB Exercise Using Valid Instrument, Compared with Our Paper



Note: The Christian and Barrett (2017) exercise is described in Section 5.1 of the paper and Appendix A. The top left panel shows the distribution of CB estimates along with the main 2SLS estimate using the non-randomized Monte Carlo data. The top right panel reproduces this using the data from the paper when ln ZRI is the dependent variable. The bottom left panel shows the distribution of the t-stats of the CB estimates along with the t-stat of the main 2SLS estimate using the non-randomized Monte Carlo data. The bottom right panel reproduces this using data from the paper.

Figure 9: Monte Carlo Results for CB Exercise Using Invalid Instrument, Compared with Christian and Barrett (2017)



Note: The Christian and Barrett (2017) exercise is described in Section 5.1 of the paper and Appendix A. The top left panel shows the distribution of CB estimates along with the main 2SLS estimate using the non-randomized Monte-Carlo data, *when the instrument is not valid* (the Monte Carlo simulation is described in Appendix A). The bottom left panel shows the distribution of the t-stats. The top right panel shows the distribution of CB estimates from Christian and Barrett (2017) produced using data from Nunn and Qian (2014). The red line labeled “NQ” is the main 2SLS estimate from Nunn and Qian (2014).

Table 11: 2SLS Results with Alternative Measures of Airbnb Supply

	Airbnb Measure: Method 2			Airbnb Measure: Method 3		
	(1) DV: ln ZRI	(2) DV: ln ZHVI	(3) DV: ln ZHVI/ZRI	(4) DV: ln ZRI	(5) DV: ln ZHVI	(6) DV: ln ZHVI/ZRI
ln Airbnb Listings	0.048***	0.088***	0.037***	0.049***	0.088***	0.036***
	(0.003)	(0.006)	(0.005)	(0.003)	(0.006)	(0.005)
... × Owner-occupancy Rate (2010)	−0.040***	−0.083***	−0.038***	−0.041***	−0.085***	−0.038***
	(0.004)	(0.008)	(0.006)	(0.004)	(0.008)	(0.006)
ln Population	0.045***	0.067***	0.026**	0.044***	0.066***	0.026**
	(0.007)	(0.010)	(0.010)	(0.007)	(0.010)	(0.010)
ln Median HH Income	0.015***	0.000	−0.018**	0.016***	0.002	−0.017*
	(0.006)	(0.008)	(0.009)	(0.006)	(0.008)	(0.009)
College Share	0.053***	0.052***	0.002	0.055***	0.056***	0.004
	(0.013)	(0.018)	(0.019)	(0.013)	(0.018)	(0.019)
Employment Rate	0.035**	0.065***	0.032	0.035**	0.066***	0.032
	(0.014)	(0.020)	(0.022)	(0.014)	(0.020)	(0.022)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes
Observations	649697	572805	537089	649697	572805	537089
R^2	0.991	0.996	0.979	0.991	0.996	0.979
Kleibergen-Paap F Statistic	916.3	718.8	704.4	893.7	700.2	686.5

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using either method 1 or method 2 in Table 1. To avoid taking the log of a zero, one is added to the number of Airbnb listings before taking logs. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 12: 2SLS Results for Log-Density Specifications

	Instrument Set 1			Instrument Set 2			Instrument Set 3		
	(1) DV: ln ZRI	(2) DV: ln ZHVI	(3) DV: ln ZHVI/ZRI	(4) DV: ln ZRI	(5) DV: ln ZHVI	(6) DV: ln ZHVI/ZRI	(7) DV: ln ZRI	(8) DV: ln ZHVI	(9) DV: ln ZHVI/ZRI
Airbnb Density	1.003***	2.525***	1.520***	1.891***	3.606***	1.685***	1.569***	2.658***	1.058***
	(0.219)	(0.310)	(0.330)	(0.218)	(0.373)	(0.284)	(0.185)	(0.322)	(0.270)
... × Owner-occupancy Rate (2010)	−1.102*	−3.874***	−3.070***	−3.894***	−6.693***	−3.134***	−2.549***	−3.440***	−1.644**
	(0.605)	(0.888)	(0.989)	(0.698)	(1.118)	(0.731)	(0.555)	(0.906)	(0.679)
ln Population	0.054***	0.064***	0.010	0.034***	0.041**	0.011	0.045***	0.070***	0.023*
	(0.008)	(0.013)	(0.015)	(0.011)	(0.016)	(0.012)	(0.008)	(0.013)	(0.012)
ln Median HH Income	0.014**	−0.002	−0.016*	0.008	−0.006	−0.018**	0.010*	−0.005	−0.016*
	(0.006)	(0.008)	(0.010)	(0.008)	(0.009)	(0.009)	(0.006)	(0.009)	(0.009)
College Share	0.053***	0.045**	0.002	0.068***	0.039*	−0.001	0.058***	0.042**	−0.001
	(0.015)	(0.018)	(0.019)	(0.019)	(0.021)	(0.019)	(0.014)	(0.020)	(0.019)
Employment Rate	0.044***	0.097***	0.053**	0.053***	0.111***	0.049**	0.046***	0.087***	0.044**
	(0.015)	(0.021)	(0.023)	(0.016)	(0.022)	(0.022)	(0.015)	(0.022)	(0.022)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	613245	538990	504260	613245	538990	504260	613245	538990	504260
R^2	0.991	0.996	0.979	0.990	0.996	0.979	0.991	0.996	0.979
Kleibergen-Paap F Statistic	15.26	10.75	9.347	5.797	5.813	5.666	9.369	10.27	10.19

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1, and is then divided by the total occupied housing stock to calculate "Airbnb Density". "Instrument Set 1" is $g_t \times h_{i,2010}/stock_{i,2010}$ interacted with $oorate_{i,2010}$. "Instrument Set 2" is a third order polynomial of $g_t \times h_{i,2010}$ (our original instrument), interacted with $oorate_{i,2010}$. "Instrument Set 3" is a fully interacted second order polynomial of g_t, $h_{i,2010}$, and $oorate_{i,2010}$. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 13: 2SLS Results When Dropping Observations with Low Listings

	Drop obs w/ 0 listings			Drop obs w/ <5 listings		
	(1) DV: ln ZRI	(2) DV: ln ZHVI	(3) DV: ln ZHVI/ZRI	(4) DV: ln ZRI	(5) DV: ln ZHVI	(6) DV: ln ZHVI/ZRI
ln Airbnb Listings	0.049***	0.092***	0.041***	0.034**	0.080***	0.045**
	(0.006)	(0.010)	(0.009)	(0.014)	(0.022)	(0.020)
... × Owner-occupancy Rate (2010)	−0.041***	−0.085***	−0.040***	−0.043***	−0.097***	−0.050***
	(0.004)	(0.007)	(0.005)	(0.005)	(0.009)	(0.008)
ln Population	0.037***	0.046***	0.006	0.006	0.012	0.011
	(0.010)	(0.015)	(0.016)	(0.013)	(0.021)	(0.020)
ln Median HH Income	0.035***	0.011	−0.021	0.039***	0.023	−0.009
	(0.008)	(0.012)	(0.013)	(0.009)	(0.015)	(0.016)
College Share	0.069***	0.044	−0.019	0.095***	0.073*	−0.018
	(0.018)	(0.028)	(0.027)	(0.024)	(0.041)	(0.040)
Employment Rate	0.011	0.071**	0.044	0.029	0.102**	0.029
	(0.020)	(0.031)	(0.031)	(0.030)	(0.045)	(0.044)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes
Observations	444992	396821	380148	272298	245181	236996
R^2	0.993	0.996	0.983	0.995	0.997	0.986
Kleinbergen-Paap F Statistic	150.3	119.4	119.6	44.82	38.44	38.97

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 14: 2SLS Results by Distance to City Center

	Zipcodes near city center			Zipcodes far from city center		
	(1) DV: ln ZRI	(2) DV: ln ZHVI	(3) DV: ln ZHVI/ZRI	(4) DV: ln ZRI	(5) DV: ln ZHVI	(6) DV: ln ZHVI/ZRI
ln Airbnb Listings	0.030***	0.058***	0.027***	0.059***	0.098***	0.036***
	(0.003)	(0.006)	(0.006)	(0.005)	(0.008)	(0.006)
... × Owner-occupancy Rate (2010)	−0.022***	−0.048***	−0.024***	−0.052***	−0.098***	−0.041***
	(0.004)	(0.007)	(0.007)	(0.005)	(0.009)	(0.007)
ln Population	0.036***	0.056***	0.017	0.051***	0.068***	0.029**
	(0.010)	(0.013)	(0.014)	(0.010)	(0.014)	(0.015)
ln Median HH Income	0.014**	−0.002	−0.016	0.021**	0.018	−0.011
	(0.007)	(0.010)	(0.011)	(0.009)	(0.012)	(0.014)
College Share	0.046***	0.037	0.014	0.070***	0.086***	−0.002
	(0.017)	(0.024)	(0.027)	(0.020)	(0.028)	(0.029)
Employment Rate	0.036**	0.050**	0.003	0.042**	0.073**	0.051
	(0.018)	(0.024)	(0.028)	(0.021)	(0.032)	(0.033)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes
Observations	317636	275627	255794	331959	297034	281079
R^2	0.991	0.996	0.977	0.991	0.996	0.981
Kleinbergen-Paap F Statistic	465.1	376.2	365.5	322.0	265.6	252.4

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. "Near" is defined as zipcodes that are below the median distance to CBD, and "far" is defined as zipcodes that are above the median distance where the median is taken within CBSA. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 15: 2SLS Results by Time Period

	Time Period: 2011-2013			Time Period: 2014-2016		
	(1) DV: ln ZRI	(2) DV: ln ZHVI	(3) DV: ln ZHVI/ZRI	(4) DV: ln ZRI	(5) DV: ln ZHVI	(6) DV: ln ZHVI/ZRI
ln Airbnb Listings	0.034***	0.047***	0.005	0.035***	0.097***	0.067***
	(0.003)	(0.004)	(0.004)	(0.006)	(0.010)	(0.010)
... × Owner-occupancy Rate (2010)	−0.005	−0.004	0.012*	−0.036***	−0.140***	−0.105***
	(0.005)	(0.006)	(0.006)	(0.007)	(0.011)	(0.012)
ln Population	0.044***	0.094***	0.050***	0.016*	−0.002	−0.021*
	(0.009)	(0.012)	(0.015)	(0.009)	(0.010)	(0.013)
ln Median HH Income	−0.013	0.005	0.013	0.020***	0.012	−0.009
	(0.010)	(0.010)	(0.014)	(0.007)	(0.009)	(0.011)
College Share	0.025	0.097***	0.067**	0.035**	0.016	−0.008
	(0.021)	(0.023)	(0.030)	(0.017)	(0.021)	(0.023)
Employment Rate	0.038*	0.094***	0.064**	−0.002	0.008	0.018
	(0.022)	(0.025)	(0.032)	(0.018)	(0.023)	(0.027)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes
Observations	319757	286104	264100	329940	286701	272989
R^2	0.992	0.998	0.984	0.995	0.998	0.988
Kleinbergen-Paap F Statistic	586.8	478.4	463.9	356.1	269.3	255.9

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 16: 2SLS Results by City Size

	Population Rank 1-30			Population Rank 31-100		
	(1) DV: ln ZRI	(2) DV: ln ZHVI	(3) DV: ln ZHVI/ZRI	(4) DV: ln ZRI	(5) DV: ln ZHVI	(6) DV: ln ZHVI/ZRI
ln Airbnb Listings	0.055***	0.097***	0.040***	0.022***	0.032***	0.009
	(0.004)	(0.007)	(0.005)	(0.003)	(0.006)	(0.006)
... × Owner-occupancy Rate (2010)	−0.041***	−0.084***	−0.040***	−0.016***	−0.025***	−0.004
	(0.004)	(0.008)	(0.006)	(0.004)	(0.008)	(0.008)
ln Population	0.031***	0.058***	0.043***	0.052***	0.060***	−0.009
	(0.009)	(0.014)	(0.013)	(0.011)	(0.013)	(0.016)
ln Median HH Income	0.022***	0.024**	−0.002	0.015	−0.015	−0.033**
	(0.007)	(0.011)	(0.011)	(0.009)	(0.011)	(0.015)
College Share	0.077***	0.023	−0.028	0.019	0.096***	0.047*
	(0.017)	(0.025)	(0.025)	(0.020)	(0.025)	(0.028)
Employment Rate	0.036*	0.054*	0.012	0.027	0.082***	0.062*
	(0.019)	(0.028)	(0.028)	(0.021)	(0.027)	(0.033)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes
Observations	403927	369158	351216	245770	203647	185873
R^2	0.991	0.996	0.980	0.987	0.996	0.973
Kleinbergen-Paap F Statistic	410.0	346.6	339.6	413.1	333.9	309.6

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 17: 2SLS Results by Housing Subsegment

	(1) DV: ln ZRI (MF)	(2) DV: ln ZRI (SF)	(3) DV: ln ZHVI (1BR)	(4) DV: ln ZHVI (2BR)	(5) DV: ln ZHVI (3BR)	(6) DV: ln ZHVI (4BR)
ln Airbnb Listings	0.033***	0.048***	0.032	0.043***	0.046***	0.048***
	(0.005)	(0.004)	(0.025)	(0.005)	(0.004)	(0.005)
... × Owner-occupancy Rate (2010)	−0.028***	−0.043***	−0.019*	−0.030***	−0.046***	−0.072***
	(0.003)	(0.005)	(0.011)	(0.005)	(0.005)	(0.005)
ln Population	0.053***	0.040***	0.102**	0.056***	0.058***	0.031**
	(0.011)	(0.007)	(0.043)	(0.016)	(0.011)	(0.013)
ln Median HH Income	0.034***	0.017***	0.048*	0.028**	0.019*	0.016
	(0.009)	(0.006)	(0.028)	(0.013)	(0.010)	(0.011)
College Share	0.027	0.060***	0.014	0.048	0.048**	0.054**
	(0.021)	(0.014)	(0.068)	(0.032)	(0.024)	(0.022)
Employment Rate	0.087***	0.037**	0.252***	0.111***	0.063***	0.073***
	(0.023)	(0.015)	(0.085)	(0.033)	(0.023)	(0.026)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes
Observations	451934	645025	103716	381042	471714	431394
R^2	0.986	0.990	0.993	0.994	0.996	0.996
Kleinbergen-Paap F Statistic	202.4	777.7	29.71	368.0	567.0	463.7

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. ZRI (MF) is the Zillow Rent Index for multifamily rentals while ZRI (SF) is the Zillow Rent Index for single family rentals. ZHVI (XBR) is the Zillow Home Value Index for X bedroom homes (including both condos and single-family). The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 18: 2SLS Results with Controls for Other Websites

	DV: ln ZRI				DV: ln ZHVI			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln Airbnb Listings	0.044***	0.040***	0.043***	0.054***	0.091***	0.084***	0.084***	0.095***
	(0.004)	(0.003)	(0.004)	(0.006)	(0.008)	(0.007)	(0.007)	(0.009)
... × Owner-occupancy Rate (2010)	−0.035***	−0.033***	−0.035***	−0.041***	−0.078***	−0.075***	−0.075***	−0.080***
	(0.004)	(0.003)	(0.004)	(0.005)	(0.007)	(0.007)	(0.007)	(0.008)
ln Population	0.042***	0.043***	0.042***	0.039***	0.060***	0.062***	0.061***	0.058***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)	(0.010)
ln Median HH Income	0.017***	0.017***	0.017***	0.017***	0.005	0.005	0.005	0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)
College Share	0.057***	0.058***	0.057***	0.056***	0.059***	0.060***	0.060***	0.058***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.018)	(0.018)	(0.018)	(0.019)
Employment Rate	0.036***	0.037***	0.036***	0.032**	0.064***	0.066***	0.067***	0.060***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.020)	(0.020)	(0.020)	(0.021)
$VRBO_t \times h_{i,2010}$	4.75E−07				−3.83E−05***			
	(7.77E−06)				(9.73E−06)			
$HomeAway_t \times h_{i,2010}$		1.79E−05**				−2.58E−05***		
		(7.01E−06)				(9.76E−06)		
$TripAdvisor_t \times h_{i,2010}$			2.45E−07				−1.84E−06***	
			(4.78E−07)				(5.92E−07)	
$Expedia_t \times h_{i,2010}$				6.10E−06***				9.68E−06***
				(2.32E−06)				(2.59E−06)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	649697	649697	649697	649697	572805	572805	572805	572805
R^2	0.991	0.991	0.991	0.991	0.996	0.996	0.996	0.996
Kleinbergen-Paap F Statistic	336.0	486.6	487.3	237.2	289.4	373.4	385.6	199.1

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.

Table 19: 2SLS Results with Forward-Looking Demographic Variables

	DV: ln ZRI		DV: ln ZHVI		DV: ln ZHVI/ZRI	
	(1)	(2)	(3)	(4)	(5)	(6)
ln Airbnb Listings	0.049***	0.045***	0.070***	0.059***	0.019***	0.009**
	(0.003)	(0.003)	(0.005)	(0.005)	(0.004)	(0.004)
... × Owner-occupancy Rate (2010)	−0.038***	−0.026***	−0.050***	−0.023***	−0.008*	0.009*
	(0.004)	(0.004)	(0.006)	(0.006)	(0.004)	(0.005)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes
1-year Ahead Demographics	Yes	No	Yes	No	Yes	No
2-year Ahead Demographics	No	Yes	No	Yes	No	Yes
Observations	539717	429737	477288	381696	446140	355120
R^2	0.991	0.991	0.997	0.997	0.980	0.982
Kleinbergen-Paap F Statistic	790.7	700.7	646.0	572.2	616.9	546.5

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Note: The number of Airbnb listings is calculated using method 1 in Table 1. The instrumental variables are $g_t \times h_{i,2010}$ and $g_t \times h_{i,2010} \times oorate_{ict}$. Because zipcode demographic characteristics are not available at a monthly frequency, zipcode-month measures for household income, population, college share, and employment rate are interpolated from the 2011 thru 2016 ACS 5-year estimates. Clustered standard errors at the zipcode level are reported in parenthesis. All variables are seasonally adjusted.