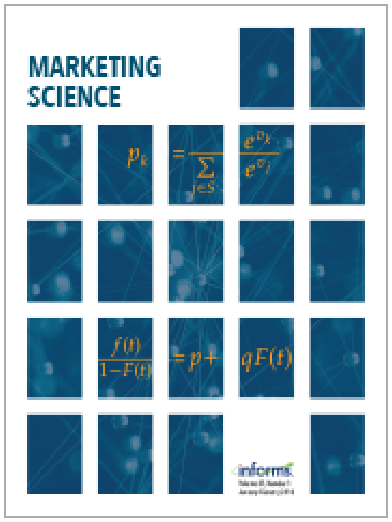

Marketing Science

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http://pubsonline.informs.org

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MARKETING SCIENCE
Vol. 40, No. 1, January–February 2021, pp. 23–47
ISSN 0732-2399 (print), ISSN 1526-548X (online)

http://pubsonline.informs.org/journal/mksc

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

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Received: August 31, 2018
Revised: June 9, 2019; November 18, 2019; January 21, 2020
Accepted: January 24, 2020
Published Online in Articles in Advance: October 2, 2020

https://doi.org/10.1287/mksc.2020.1227

Abstract. We assess the impact of home-sharing on residential house prices and rents. Using a data set 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 zip codes 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 zip code, 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. Considering the median annual Airbnb growth in each zip code, these results translate to an annual increase of $9 in monthly rent and $1,800 in house prices for the median zip code in our data, which accounts for about one-fifth of actual rent growth and about one-seventh of actual price growth. Finally, we formally test whether the Airbnb effect is due to the reallocation of the housing supply. Consistent with this hypothesis, we find that although 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.

History: Puneet Manchanda served as the senior editor and Bart Bronnenberg served as associate editor for this article.
Supplemental Material: Data and the online appendix are available at https://doi.org/10.1287/mksc.2020.1227.

Keywords: sharing economy • peer-to-peer markets • housing markets • Airbnb

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.

The rapid 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 for cities and municipalities. Because of this, in the past few years, researchers have been extensively studying these platforms and their impact on our society at large. To date, whether these platforms generate positive welfare for cities and consumers is still an open question. Those in favor of the sharing economy argue that these platforms bring several benefits for consumers such as better use of resources, lower prices, and better offerings, which in turn should increase consumer welfare (Farronato and Fradkin 2018). However, critics argue that the negative externalities generated by the sharing economy outnumber the benefits—recent research suggests that the sharing economy is increasing societal inequality (Schor 2017) and financial hardship (Daniels and Grinstein-Weiss 2018), and lowering city livability (Barrios et al. 2019, Erhardt et al. 2019).

Home-sharing, in particular, has been the subject of intense criticism. Namely, critics argue that home-sharing platforms such as Airbnb raise the cost of living for local renters while mainly benefitting local landlords and nonresident tourists. 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 nonresidents 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. Concerns over the impact of home-sharing on housing affordability have garnered significant attention from policy makers and have motivated many cities to impose stricter regulations on home-sharing.[1]

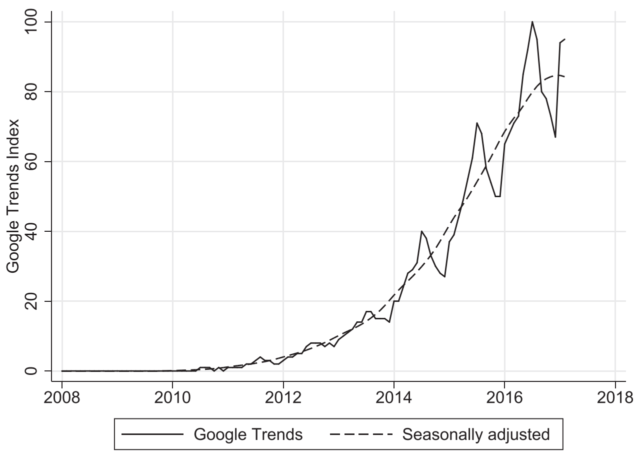
Whether home-sharing increases housing costs for local residents is an empirical question. There are a few reasons why it might not. The market for short-term rentals may be very small compared with the market for long-term rentals. In this case, even large changes to the short-term market might not 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 may supply their entire home during temporary absences, 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 because of the restrictiveness of long-term leases causing vacation homeowners 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 homeowners, these homes would not be made available to long-term tenants independent 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 data set 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 these data, we construct a panel data set of Airbnb listings at the zip code-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 zip code-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 prices and rental rates in the market for long-term housing. Finally, we supplement these data with a rich set of time-varying zip code 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 zip code i in year-month t is positively associated with both house prices and rental rates. In a baseline ordinary least squares (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 data set to control for unobserved zip code–level effects and arbitrary city-level time trends. We include zip code fixed effects to absorb any permanent differences between zip codes, whereas fixed effects at the core-based statistical area (CBSA)-year-month level control for any shocks to housing market conditions that are common across zip codes within a CBSA.[2]

We further control for unobserved *zip code–specific, time-varying* factors using an instrumental variable that is plausibly exogenous to local zip code–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 zip code–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 zip code is in a base year, 2010. We define *touristy* to be a measure of a zip code's attractiveness for tourists and proxy for it using the

Figure 1. Google Trends Search Index for Airbnb (Worldwide, 2008–2017)



Notes. Shown is the 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.

number of establishments in the food service and accommodations industry.[3] These include eating and drinking places as well as hotels, bed and breakfasts (B&Bs), and other forms of short-term lodging. The identifying assumptions of our specification are that (1) landlords in more touristy zip codes are more likely to switch into the short-term rental market in response to learning about Airbnb than landlords in less touristy zip codes, and (2) ex ante levels of touristiness are not systematically correlated with ex post unobserved shocks to the housing market at the zip code 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 and 5.1, as well as in Online Appendix B.

Using this instrumental variable, we estimate that for zip codes 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 zip codes 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 zip codes 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 nonowner 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 just described 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,[4] (iii) an increase in Airbnb listings leads to a decrease in the number of units available to long-term renters, and (iv) the above-mentioned effects on supply are smaller for zip codes 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 zip codes 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

There is a growing body of research studying the effect of home-sharing on housing costs. Two papers focus on a specific U.S. market: Lee (2016) provides a descriptive analysis of Airbnb in the Los Angeles housing market, and 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 effects 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 zip code level in 2015–2016 implies a 0.54% increase in rents. A third study was recently released as a working paper (Garcia-López et al. 2019). In it, the authors study the effect of Airbnb on rental rates in Barcelona (Spain), and using several econometrics approaches, they provide evidence that Airbnb increased rental rates by 1.9%.

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 data set 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 rental supply and an increase in short-term rental 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 (because 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.

Besides its effect on housing costs, other papers studying home-sharing directly have looked at the effects of racial discrimination on the platform (Edelman and Luca 2014, Edelman et al. 2017) and the possibility of positive or negative spillovers (Alyakoob and Rahman 2018, Filippas and Horton 2018).

Our paper also contributes more generally to the growing literature on peer-to-peer markets. Such literature covers a wide array of topics, including the effect of the sharing economy on labor market outcomes (Angrist et al. 2017, Hall and Krueger 2018, Chen et al. 2019), entry and competition (Gong et al. 2017, Zervas et al. 2017, Massner et al. 2018, Li and Srinivasan 2019, Filippas et al. 2020), and trust and reputation (Zervas et al. 2015, Fradkin et al. 2017, Proserpio et al. 2018). 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 substitution patterns between Airbnb and hotels, and they 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 nonlocal; transactions in the home-sharing market, therefore, involve a reallocation of resources from locals to nonlocals. Not everyone may see this as a real economic cost, but a shift in welfare from locals to nonlocals is important for public policy because policy is set locally. 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 an 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 Craigslist on the newspaper industry and find a substantial substitution effect between the two.

The rest of this 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 and the limitations of our work, and it provides concluding remarks.

3. Effects of Home-Sharing

The market for long and short-term rentals is traditionally viewed as segmented on both the supply side and demand side. On the demand side, the demanders for short-term rentals are tourists, visitors, and business travelers, whereas 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, whereas 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 sides 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, whereas long-term demanders may 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 because of this segmentation.

3.1. 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 such as 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. Einav et al. (2016) discuss the innovations that may have given rise to these platforms, centering on reductions in transactional and information frictions associated with trust.

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 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, whereas 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 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 do 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.[5]

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 in the short term instead of the long term even if the expected rents from the short term are lower, as may be the case according to Coles et al. (2018). 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 in the future while capitalizing on surges in short-term demand.

3.2. 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. This would have the effect of increasing both long- and short-term rental rates. Coles et al. (2018) and Farronato and Fradkin (2018) document that home-sharing has drawn tourists into neighborhoods that previously had very few, and Alyakoob and Rahman (2018) find a positive relation between Airbnb entry 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.[6] In the long run, this effect could lead to further expansion in housing supply.

So far, the discussion has focused on rental rates. Because 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—or in other words, to provide the owner with an additional potential income source—it should have an additional effect of increasing prices even further than the direct effect on rents. Home-sharing may also have effects on the supply of homes listed for sale, as the option to rent on the short-term rental market may affect owners' propensity to list their homes for sale, and it may also change sellers' reservation value and marketing behavior. 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 to renters. If such were the case, then the net effect of home-sharing on the price-to-rent ratio could be negative even though the increased option value of using spare capacity would increase it.

3.3. 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.

3.4. 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 will 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 fewer homeowners are choosing to reallocate. Third, rents and prices should both increase as a result of 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 such as 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 of 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 more than 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 $31 billion, 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 and 2014 and at a weekly interval starting January 2015.[7]

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. 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. For privacy reasons, Airbnb does not reveal the exact street address of any listing until the property is booked, but the listing's city, street, and zip code information correspond to the property's real location.

Our final data set 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 these data represent the most comprehensive picture of home-sharing in the United States 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, and 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 zip code 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).[8] 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. Zip codes will be our proxy for neighborhoods. Third, conducting the analysis at the zip code level as opposed to the city level helps with identification. This is due to our ability to compare zip codes within cities, thus controlling for any unobserved city-level factors that may be unrelated to Airbnb but that affect all neighborhoods within a city such as a citywide 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 United States 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 (even for Airbnb) because of the potential presence of "stale vacancies" that are still listed but for which the host has no intention to rent out. Fradkin (2017) estimates that about 15% of guest requests are rejected because of this effect. 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.

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 zip code characteristics, it is correlated with the error term.[9]

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 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, three months and six months.

Although our 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 these different measures of Airbnb supply and show that results are qualitatively and quantitatively unchanged.

Table 1. Methods for Computing the Number of Listings

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

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 more than 110 million homes across the United States. 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 zip code–month level for over 13,000 zip codes.

For rental rates, we use the Zillow Rent Index (ZRI). Similar to 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. For each zip code, we calculate the price-to-rent ratio as simply the ZHVI divided by the ZRI.

4.5. Other Data Sources

We supplement the above-mentioned data with several additional sources.

4.5.1. 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 Bureau's ZIP Codes Business Patterns data to measure the number of establishments in the food services and accommodations industry (North American Industry Classification System (NAICS) code 72) for each zip code in 2010.

4.5.2. Zip Code–Level Time-Varying Characteristics. We collect from the American Community Survey (ACS) zip code–level annual estimates of median household income, population, share of 25- to 60-year-olds with bachelors' degrees or higher, and employment rate. From the ACS, we also obtain zip code–level annual estimates of the number of housing units occupied by their owners or renters, as well as the number of vacant units. The ACS also reports the reason a housing unit is vacant (e.g., whether the owner is holding it vacant so that he or she can use it occasionally 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.

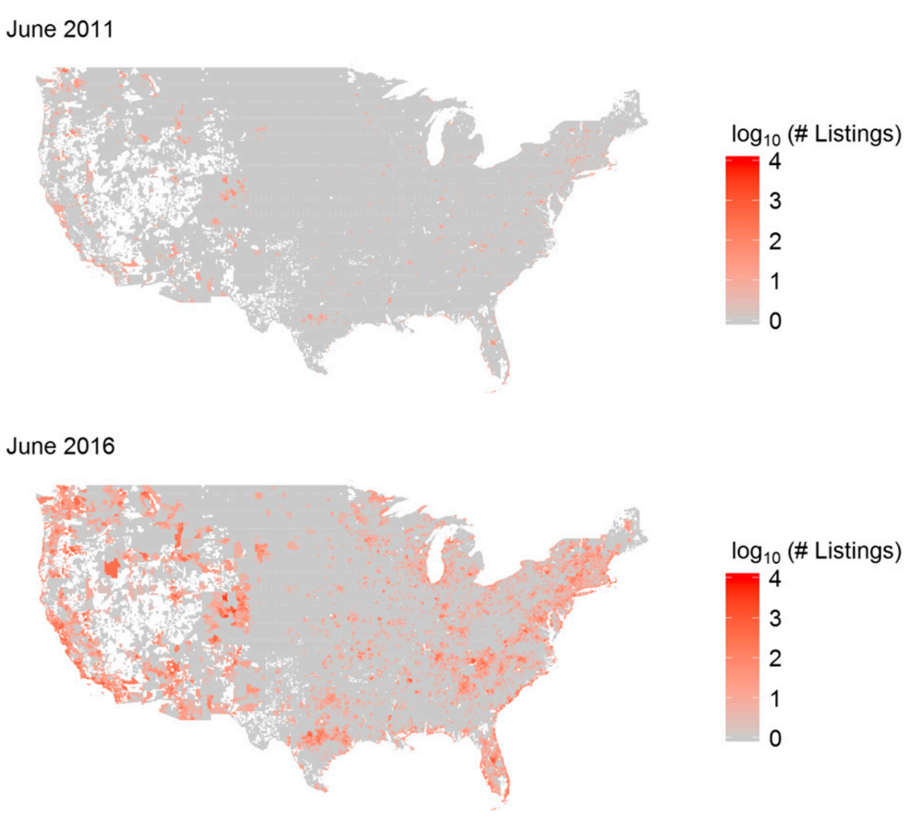
4.5.3. Proxies for Tourism Demand. We obtained hotel occupancy rates at the CBSA-year-month level from STR, a company that tracks hotel performance worldwide. We collected the number of incoming airport travelers for all airports in the United States from the Bureau of Transportation Statistics. Finally, we collected the complete set of hotel and restaurant reviews for all the hotels and restaurants available on TripAdvisor. These data amount to about 18 million hotel reviews from 88,000 accommodation properties (hotels, inns, and 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 zip codes 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 data set 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.

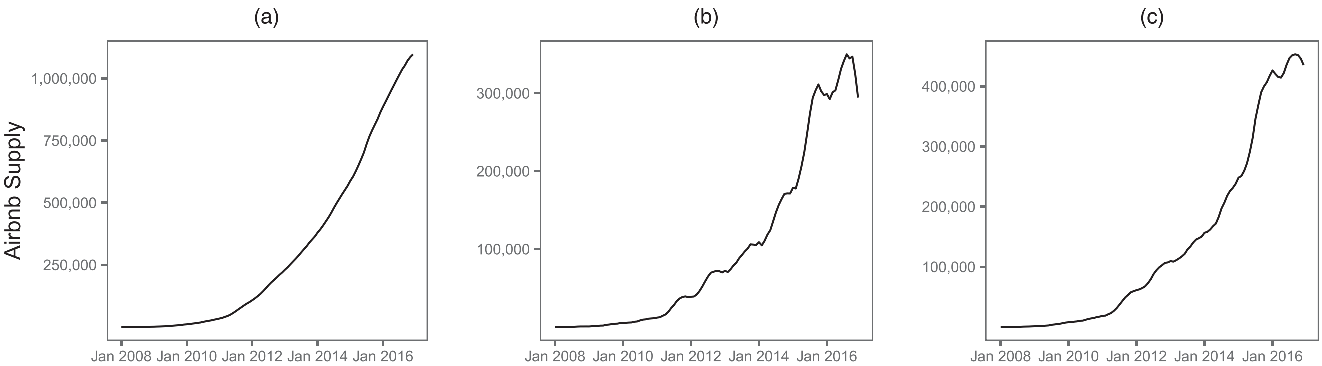
Table 2 gives a sense of the size of Airbnb relative to the housing stock at the zip code level for the 100 largest CBSAs by population in our data. Even in 2016, Airbnb remains a small percentage of the total housing stock for most zip codes. The median ratio of Airbnb listings to housing stock is 0.18%, and the 90th percentile is 1.72%. When compared with 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 2.22%, and the 90th percentile is 18%. Perhaps the most salient comparison—at least from the perspective of a

Figure 2. (Color online) Map of Airbnb Listings by Zip Code, 2011–2016



Notes. 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 a log listing is set to 0 if there are zero listings. Geographic areas without zip code boundary information are colored white.

Figure 3. Total Number of Airbnb Listings (United States, 2008–2016)



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

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 11.77% in the median zip code in 2016 and 121% in the 90th percentile zip code. This implies that in the median zip code, a local resident looking for a long-term rental unit will find that about 1 in 9 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 to be unfounded.

5. Methodology

Let Y_{ict} be either the price index, the rent index, or the price-to-rent ratio for zip code i in CBSA c in year-month t over the time period 2011–2016; let $Airbnb_{ict}$ be a measure of Airbnb supply over the same time period; and let $oorate_{ic,2010}$ be the owner-occupancy rate in 2010.[10] We assume the following causal relationship between Y_{ict} and $Airbnb_{ict}$:

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

where X_{ict} is a vector of observed time-varying zip code 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 zip code i, such as changes to local labor market conditions or changes to local amenities such as 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 zip

Table 2. Size of Airbnb Relative to the Housing Stock (Zip Codes, 100 Largest CBSAs)

	p10	p25	p50	p75	p90
June 2011					
Airbnb listings	0	0	0	1	6
Housing units	919	2,249	6,518	12,250	17,515
Airbnb listings as a percentage of					
Total housing units	0.00	0.00	0.00	0.01	0.08
Renter-occupied units	0.00	0.00	0.00	0.05	0.29
Vacant units	0.00	0.00	0.00	0.16	0.79
Vacant-for-rent units	0.00	0.00	0.00	0.86	4.55
June 2016					
Airbnb listings	0	2	10	39	128
Housing units	944	2,334	6,679	12,583	18,000
Airbnb listings as a percentage of					
Total housing units	0.00	0.06	0.18	0.53	1.72
Renter-occupied units	0.00	0.23	0.73	2.18	6.63
Vacant units	0.00	0.67	2.22	6.49	18.17
Vacant-for-rent units	0.51	3.49	11.77	38.50	120.93

Notes. This table reports the size of Airbnb relative to the housing stock by zip codes for the 100 largest CBSAs as measured by the 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 zip code–level five-year estimates. p, percentile.

code, city, and time levels. We allow ϵ_{ict} to contain unobserved zip code–level factors δ_i and unobserved time-varying factors that affect all zip codes within a CBSA equally, θ_{ct}.[11] Writing $\epsilon_{ict} = \delta_i + \theta_{ct} + \xi_{ict}$, Equation (1) becomes

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

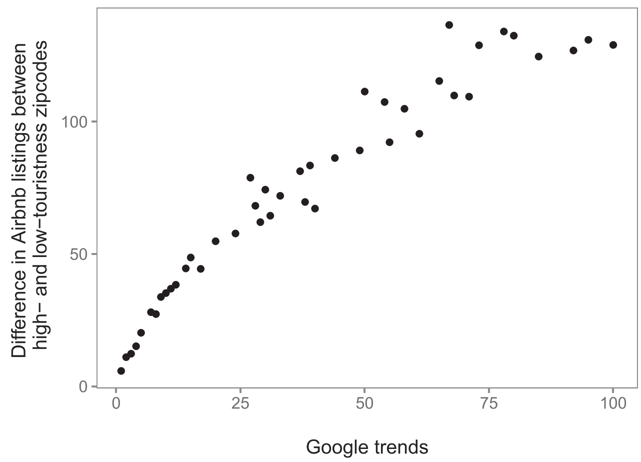
Even after controlling for unobserved factors at the zip code and CBSA-year-month levels, there may still be some unobserved *zip code-specific*, *time-varying* factors contained in ξ_{ict} that are correlated with $Airbnb_{ict}$. To address this issue, we construct an instrumental variable that is plausibly uncorrelated with local shocks to the housing market at the zip code 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 10 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 of overall growth in the supply of short-term housing, *except* to the extent that it is driven by Airbnb.

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 zip code 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 zip code. Zip codes 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 zip codes. Figure 4 shows that such a difference increases as Airbnb awareness increases, confirming our hypothesis.

Figure 4. Testing the IV Operating Assumption



Notes. This figure plots the difference in the number of Airbnb listings for high- and low-touristiness zip codes over the Google Trends values. We use the sample median value of touristiness to create two equally sized groups of high- and low-touristiness zip codes.

In order for the instrument to be valid, $z_{ict} = g_t \times h_{i,2010}$ must be uncorrelated with the zip code–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 zip code-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 zip code–level crime rate as an omitted variable. It is unlikely that changes to crime rates across all zip codes 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 zip codes. 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 turn now to a detailed discussion of the instrument and its validity and present some exercises that suggest that the exogeneity assumption is likely satisfied.

5.1. Discussion: Validity of the Instrumental Variable

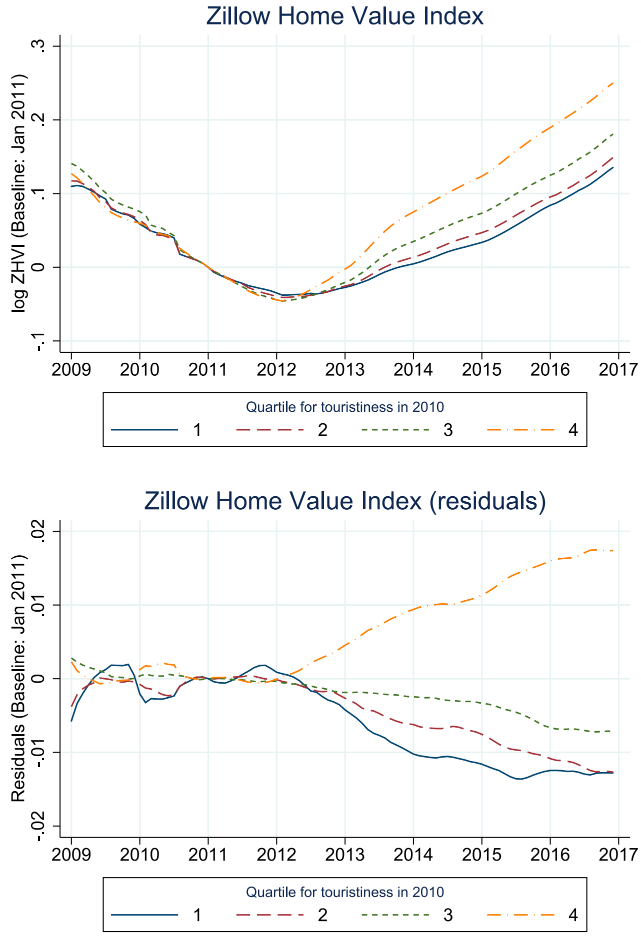
The construction of an instrumental variable (IV) using the interaction of a plausibly exogenous time series (Google trends) with a potentially endogenous cross-sectional exposure variable (the touristiness measure) is an approach that was popularized by Bartik (1991) and that researchers have used in 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 the exposure variable is independent of the error term ($E[h_{i,2010} \xi_{ict}] = 0$). Although 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 zip codes. Because 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 two-stage least squares (2SLS) estimates may reflect the effects of gentrification rather than home-sharing.

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

5.1.1. Parallel Pretrends. 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, because the specification includes year-month and zip code 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 zip codes. 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 pretrends in DD analysis. To do this, we plot the Zillow house price index for zip codes in different quartiles of 2010 touristiness ($h_{i,2010}$) from 2009 to the end of 2016.[12] The results are shown in Figure 5. The figure shows that there are no differential pretrends in the ZHVI for zip codes 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 zip code and CBSA-year-month fixed effects (bottom panel). The lack of differential pretrends suggests that zip codes 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.

Figure 5. (Color online) Trends in Zillow Home Value Index by "Touristiness" of Zip Code



Notes. The top panel plots the ZHVI index, normalized to January 2011 = 0, averaged within different groups of zip codes 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 zip codes are separated into four equally sized groups. The bottom panel plots the residuals from a regression of the ZHVI on zip code fixed effects and CBSA-month fixed effects.

5.1.2. IV Has No Effect in Non-Airbnb Zip codes. 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 zip codes 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.[13]

To test this, we regress the Zillow rent index and house price index on the instrumental variable directly, using only data from zip codes 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 zip code demographics, we do not find any statistically significant relationship between the instrument and house prices/rental rates in zip codes without Airbnb. If anything, we find that there is a *negative* relationship between the instrument and house prices/rental rates in zip codes without Airbnb, though the estimates are imprecise and the sample size is considerably reduced when considering only such zip codes. By contrast, columns (3) and (4) of Table 3 show that if we regress house prices and rental rates directly on the instrument for zip codes *with* Airbnb, we find a positive and statistically significant relationship.

Of course, the sample of zip codes that never had any Airbnb listings could be fundamentally different from the sample of zip codes that did. In columns (1) and (2) of Table 4, we show that zip codes with Airbnb are indeed quite different from zip codes without Airbnb, which are richer and more educated in general. We therefore construct a third sample of zip codes *with* Airbnb but that are more similar to the sample of zip codes *without* Airbnb. To do so, we use propensity score matching. Starting with the full sample of zip codes, we first estimate a logistic regression at the zip code level that predicts whether a zip code will be a non-Airbnb zip code based on 2010 zip code demographic characteristics (median household income, population, college share, and employment rate) and touristiness. For each zip code that is observed to have no Airbnb, we find the nearest zip code 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 zip codes with Airbnb listings is (as expected) demographically similar to the non-Airbnb sample (column (1)). Columns (5) and (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 zip codes without Airbnb are poorer and smaller than zip codes with Airbnb. 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.

5.1.3. 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

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

	Sample: Zip codes w/o Airbnb ever		Sample: Zip codes 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)
Zip code FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	61,854	50,875	587,141	520,797	51,800	42,082
R^2	0.979	0.994	0.992	0.996	0.982	0.993

Notes. This table reports regression results when outcomes of interest are regressed on the instrumental variable directly for three samples of zip codes: (1) zip codes that were never observed to have any Airbnb listings, (2) zip codes that were observed at some point to have Airbnb listings, and (3) zip codes that had Airbnb listings and were propensity score matched to zip codes that did not have Airbnb listings. Because zip code demographic characteristics are not available at a monthly frequency, zip code-month measures for household income, population, college share, and employment rate are interpolated from the 2011–2016 ACS five-year estimates. Clustered standard errors at the zip code level are reported in parentheses. DV, dependent variable; FE, fixed effects.

Table 4. Comparing Airbnb and Non-Airbnb Zip Codes

	Sample: Zip codes w/o Airbnb	Sample: Zip codes 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
# Zip Codes	999	9,356	999

Notes. This table reports mean zip code characteristics from three samples: (1) zip codes that were never observed to have any Airbnb listings, (2) zip codes that were observed at some point to have Airbnb listings, and (3) zip codes that had Airbnb listings and were propensity score matched to zip codes that did not have Airbnb listings.

***indicates that the difference in means compared with the non-Airbnb sample is statistically significant, with $p < 0.01$.

spurious time trends. To do so, we keep constant touristiness, Google trends, the identity of zip codes experiencing Airbnb entry, observable time-varying zip code characteristics, housing market variables, and the aggregate number of Airbnb listings in any year-month period. However, among the zip codes with positive Airbnb listings, we randomly swap the specific number of Airbnb listings across these zip codes. For example, we randomly assign to zip code i the variable $Airbnb_{\text{jct}}$ (i.e., the Airbnb counts of zip code 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 zip codes 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 trend 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 as a result of the weak first stage.

We estimate the 2SLS specification on this data set for 1,000 draws of randomized allocations of Airbnb listings among zip codes 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 in 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 the main effect β for each of the three dependent variables. 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.[14] The results of this test are therefore consistent with an instrument that is exogenous.

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.

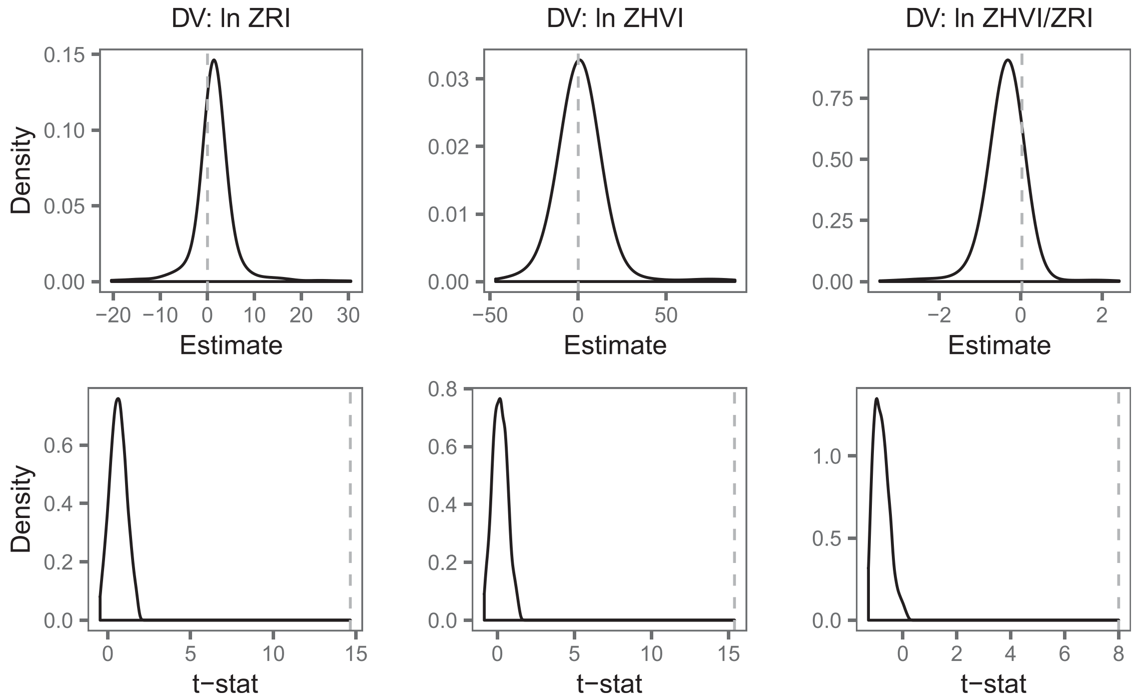
6. Results

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 1 plus the number of listings as measured by method 1 in Table 1.[15] 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 preperiod variable, we only use data from 2011 to 2016 in our estimation. 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.[16] The data are monthly, so we deseasonalize

Figure 6. Christian and Barrett (2017) Exercise Results



Notes. This figure shows the distribution of the estimates and *t*-statistics for the main coefficient β of Equation (2) for our three dependent variables (rental rates, house prices, and price-to-rent ratio) using the Christian and Barrett (2017) exercise described in Section 5.1. The gray dashed lines represent the main 2SLS estimate and *t*-statistic obtained using the nonrandomized data.

all variables. Because 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(*AirbnbListings*) and no controls. Without controls, a 1% increase in Airbnb listings is associated with a 0.1% increase in rental rates. Column (2) includes zip code 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 zip code's owner-occupancy rate. Column (3) shows the importance of controlling for owner-occupancy rate, as it significantly mediates the effect of Airbnb listings. Column (4) includes time-varying zip code–level characteristics, including the natural log of total population, the natural log of the median household (HH) income, the share of 25- to 60-year-olds with bachelor's 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 five-year estimates from 2011 to 2016.[17] Column (4) shows that the results are robust to the inclusion of these zip code demographics. Finally, columns (5) and (6) report the 2SLS results using the instrumental variable with and without time-varying zip code characteristics as controls. Using the results from column (6) (our preferred specification), we estimate that a 1% increase in Airbnb listings in zip codes 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 an increase in rents by 0.024%, and at 82% owner-occupancy rate (the 75th percentile), the effect of a 1% increase in Airbnb listings is an increase in 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 versus not. Furthermore, 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 zip code with a median owner-occupancy rate. The effect increases to 0.037% in zip codes with an owner-occupancy rate equal to the 25th percentile and decreases to 0.019% in zip codes with an owner-occupancy rate equal to the 75th percentile.

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)
ln(*Airbnb Listings*) × *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)
Zip code 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	649,841	649,841	649,841	649,697	649,841	649,697
R^2	0.169	0.991	0.991	0.991	0.991	0.991
KP *F*-statistic					820.0	807.0

Notes. The number of Airbnb listings is calculated using method 1 in Table 1. To avoid taking the log of 0, 1 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 zip code demographic characteristics are not available at a monthly frequency, zip code-month measures for household income, population, college share, and employment rate are interpolated from the 2011–2016 ACS five-year estimates. Clustered standard errors at the zip code level are reported in parentheses. All variables are seasonally adjusted. FE, fixed variables; KP, Kleibergen and Paap (2006).
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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 5 and 6). This goes against our initial intuition that omitted factors (such as gentrification) are most likely to be

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)
ln(*Airbnb Listings*) × *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)
Zip code 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	572,858	572,858	572,858	572,805	572,858	572,805
R^2	0.188	0.996	0.996	0.996	0.996	0.996
KP *F*-statistic					661.9	646.7

Notes. The number of Airbnb listings is calculated using method 1 in Table 1. To avoid taking the log of 0, 1 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 zip code demographic characteristics are not available at a monthly frequency, zip code-month measures for household income, population, college share, and employment rate are interpolated from the 2011–2016 ACS five-year estimates. Clustered standard errors at the zip code level are reported in parentheses. All variables are seasonally adjusted. FE, fixed effects; KP, Kleibergen and Paap (2006).
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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 0 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 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.[18] 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, similar 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 zip code'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 the 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.

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

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)
ln(*Airbnb Listings*) × *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)
Zip code 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	537,157	537,142	537,142	537,089	537,142	537,089
R^2	0.154	0.979	0.979	0.979	0.979	0.979
KP *F*-statistic					629.8	616.9

Notes. The number of Airbnb listings is calculated using method 1 in Table 1. To avoid taking the log of 0, 1 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 zip code demographic characteristics are not available at a monthly frequency, zip code-month measures for household income, population, college share, and employment rate are interpolated from the 2011–2016 ACS five-year estimates. Clustered standard errors at the zip code level are reported in parentheses. All variables are seasonally adjusted. FE, fixed effects; KP, Kleibergen and Paap (2006).

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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.

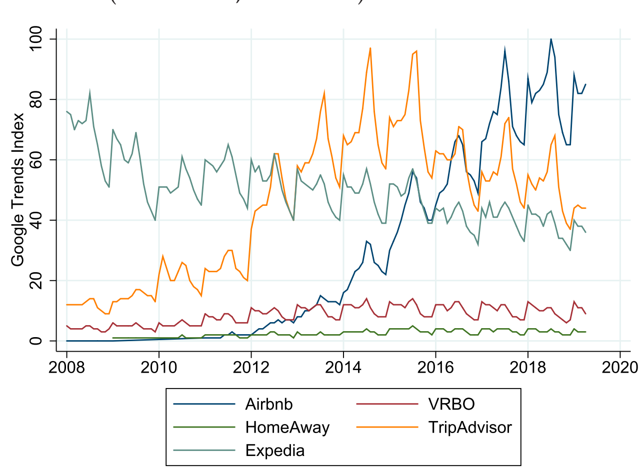
6.2.1. Gentrification. One may be concerned that post-2012, touristy and nontouristy zip codes experienced differential trends in gentrification or neighborhood change. However, columns (5) and (6) of Tables 5–7 show that the main results are unchanged by the inclusion of time-varying zip code demographic controls. Because the included demographic controls (population, household income, share of college-educated, and employment rate) are fairly basic measurements of zip code–level economic outcomes, they are likely to be highly correlated with other unobserved factors that affect zip code–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 zip code–level factors that affect housing markets.

6.2.2. 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 versus less touristy zip codes 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. Furthermore, 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 and accommodations establishments in each zip code as reported by the Census Bureau's ZIP Codes Business Patterns data. Second, we control for the total number of airport passengers arriving at each U.S. city each month and then allocate these arrivals to zip codes based on the zip code'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 zip code 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 zip codes based on the zip code's share of hotel rooms in each city. Finally, we control for the monthly number of reviews written for accommodation properties (hotels, inns, and B&Bs) and restaurants in each zip code 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.

6.2.3. High- and Low-Touristy Zip Codes. Finally, we rule out any differential effects between high- and low-touristy zip codes that are linear in time by directly controlling for the interaction of a linear time trend with zip code 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 only estimate that is significantly affected is the estimated effect of Airbnb listings on the home-value index, though not enough to eliminate the effect. Moreover, we should emphasize that it is highly plausible that the differential linear time trend between high and low touristy zip codes may indeed be caused by Airbnb, as perhaps suggested by Figure 5.

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 zip codes in 2012 (just when Airbnb started taking off), (ii) affect zip codes with low owner-occupancy rate more than zip codes with high owner-occupancy rate, (iii) be uncorrelated

Figure 7. (Color online) Google Trends Index for Related Websites (Worldwide, 2008–2019)



Notes. Shown is the 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 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)
ln(*Airbnb Listings*) × *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)		
# *TripAdvisor 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)
Zip code 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	639,400	639,400	648,905	649,697	649,697	564,157	564,157	572,085	572,805	572,805
R^2	0.991	0.991	0.991	0.991	0.991	0.996	0.996	0.996	0.996	0.996
KP *F*-statistic	650.0	761.5	802.8	791.9	167.1	516.1	611.9	642.5	579.2	218.3

Notes. The number of Airbnb listings is calculated using method 1 in Table 1. To avoid taking the log of 0, 1 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 zip code demographic characteristics are not available at a monthly frequency, zip code-month measures for household income, population, college share, and employment rate are interpolated from the 2011–2016 ACS five-year estimates. Clustered standard errors at the zip code level are reported in parentheses. All variables are seasonally adjusted. DV, dependent variable; FE, fixed effects; KP, Kleibergen and Paap (2006).

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

with house prices and rents in zip codes that never had any Airbnb but correlated with house prices and rents in zip codes that did—even among zip codes that ex ante look demographically similar, and (iv) 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 zip code demographic characteristics and unrelated to our measured changes in tourism demand. Although 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) zip codes 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 zip code. The median year-on-year growth rate in Airbnb listings was 28% across zip codes 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 zip code 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 zip codes in the 10 largest CBSAs as well as for the median zip code in our sample of 100 largest CBSAs. We also include actual year-on-year rent and price growth for comparison. To represent our estimates in dollar terms, we apply the percentage change estimates to median monthly rent and house prices, as measured by the ZRI and ZHVI, respectively. For the median zip code in the 100 largest CBSAs, our results imply that Airbnb growth can account for an annual increase of \$9 in monthly rent and \$1,800 in house price growth. In comparison with actual rent and price growth, the results imply that Airbnb can explain about one-fifth of annual rent growth and about one-seventh of annual house price growth.

Our effect magnitudes are consistent with those found in Horn and Merante (2017), who study the effect of Airbnb on rents in Boston from 2015 to 2016.

Table 9. Effect Magnitudes for 10 Largest CBSAs

	Monthly rent (*ZRI*)					House price (*ZHVI*)				
		Annual increase from Airbnb		Annual increase (actual)			Annual increase from Airbnb		Annual increase (actual)	
CBSA	Level (\$)	%	\$	%	\$	Level (\$)	%	\$	%	\$
Top 100 CBSAs	1,524	0.60	9.07	3.18	48.46	219,000	0.81	1,778	5.70	12,474
New York-Newark-Jersey City, NY-NJ-PA	2,199	0.61	13.33	3.64	80.10	391,000	0.82	3,201	3.55	13,865
Los Angeles-Long Beach-Anaheim, CA	2,441	1.16	28.34	4.92	120.22	522,700	1.80	9,389	9.66	50,494
Chicago-Naperville-Elgin, IL-IN-WI	1,611	0.34	5.54	2.25	36.32	188,500	0.44	826	3.98	7,495
Dallas-Fort Worth-Arlington, TX	1,420	0.71	10.10	4.18	59.42	174,300	1.01	1,760	8.21	14,312
Miami-Fort Lauderdale-West Palm Beach, FL	1,722	1.03	17.71	4.51	77.59	192,400	1.50	2,890	11.72	22,549
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	1,573	0.55	8.59	1.94	30.52	213,600	0.73	1,553	2.05	4,389
Houston-The Woodlands-Sugar Land, TX	1,476	0.97	14.28	4.67	68.92	125,900	1.35	1,703	8.34	10,502
Washington-Arlington-Alexandria, DC-VA-MD-WV	2,162	0.71	15.34	1.28	27.71	388,400	0.95	3,693	4.41	17,113
Atlanta-Sandy Springs-Roswell, GA	1,226	0.76	9.31	3.11	38.18	161,700	1.06	1,721	8.42	13,617
Detroit-Warren-Dearborn, MI	1,188	0.17	1.99	2.41	28.64	137,400	0.21	288	8.54	11,731

Notes. 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 zip code and then taken at the median zip code. Estimates from columns (6) of Tables 5 and 6 are used. Median zip code *ZRI* and *ZHVI* are calculated in each CBSA from 2012 to 2016, with zip codes weighted by the number of occupied housing units.

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.

We can also put the magnitude of our results in the context of demand elasticity in the long-term rental market. If Airbnb causes a reduction in long-term rentals supply, this causes a movement backward and up along the demand curve for long-term rentals, resulting in an increase in rental rates. For the median zip code in our data, Airbnb is about 0.73% of the rental stock in 2016. A 1% increase in Airbnb, if all of it represents a reallocation of the rental stock, is therefore about a 0.0073% reduction in total rental supply. If this results in a 0.018% increase in rental rates, as is our estimate for the median zip code, then the implied demand elasticity for long-term rentals is 0.41. This is in the same ballpark of 0.45 to two-thirds found in other studies of housing demand elasticity (see Albouy et al. (2016) for a review), though direct comparisons to other studies should be treated with caution because of differences in time horizon and market definition.

Finally, it is worth noting that our main results only speak to the effect of Airbnb on the median housing unit. In the appendix, we explore the effects of Airbnb on subsegments of the housing market, such as separate estimates for one- to four-bedroom homes and rents for multifamily versus single-family units. The results are not very different from each other, so we opt only to report median effects in the paper.

6.4. The Effect of Airbnb on Housing Reallocation

So far, 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-term 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 these data from the American Community Survey, an annual survey administered by the U.S. Census Bureau 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.[19] To summarize, short-term rental properties would be contained in measure (i), whereas 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 previously as dependent variables. One issue with this measure is that housing supply data are not available at the zip code 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 as a result of sampling error, the ACS reports five-year running averages of these variables. Therefore, there is serial correlation in the dependent variable, which we account for by clustering standard errors at the zip code level. Because we now have only annual data, and thus less variation to exploit, we include CBSA fixed effects and year fixed effects separately, without interacting them as we did in our previous analysis. Including fully interacted CBSA and year fixed effects would cause us to lose some statistical power, but the qualitative results do not change, and we cannot reject that the estimated effects are equal to including the fixed effects separately.

If, as we hypothesized, the effect of Airbnb is mainly due to the reallocation effect previously discussed, 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 (ii) and (iii)). Furthermore, 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

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)
ln(*Airbnb Listings*) × *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)
Zip code FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Instrumental variable	Yes	Yes	Yes	Yes
Observations	49,282	49,580	61,435	61,720
R^2	0.913	0.927	0.993	0.999
KP F-statistic	742.4	587.2	1,082.9	1,099.4

Notes. 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 0, 1 is added to the number of Airbnb listings before taking logs. Clustered standard errors at the zip code level are reported in parentheses. FE, fixed effects; KP, Kleibergen and Paap (2006).

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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 zip code with Airbnb having stronger effects in zip codes 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 the 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 is consistent with our hypothesis that the effect of Airbnb is, at least in part, due to the reallocation of the housing stock from the long-term to the short-term rental market.

7. Discussion and 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.

Despite the sharing economy being in its infancy, there is a growing body of research studying these platforms. So far, marketers have been focusing on traditional questions such as competition (Zervas et al. 2017, Li and Srinivasan 2019), welfare (Farronato and Fradkin 2018), platforms design (Fradkin 2017), and trust and reputation (Fradkin et al. 2017, Proserpio et al. 2018). However, the entry of these new platforms is affecting our society in unexpected ways, thus giving marketers opportunities to weigh in on topics not traditionally studied by the discipline, including the one studied in this paper.

The results of this paper contribute to the debate surrounding home-sharing and its impact on the housing market. Although Airbnb and proponents of the sharing economy argue that the platform is not responsible for higher house prices and rental rates, 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 data set on home-sharing in the United States 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 (because 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 are imperfect: Although we observe properties listed on Airbnb, we do not observe the 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 ATE or the effect of Airbnb on the subset of "complier" zip codes—those zip codes 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 zip codes. 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 zip codes 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, 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 supply of short-term accommodations in response to a short-term demand surge (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.

Endnotes

[1] 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 these regulations are enforced.

[2] 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.

[3] 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.

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

[5] 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.

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

[7] In their paper, Horn and Merante (2017) incorrectly state that our Airbnb data set comes from 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.

[8] 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.

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

[10] We use the owner-occupancy rate in 2010 to maintain it as pre-estimation period variable in order 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 five-year estimates from 2011 to 2016.

[11] Controlling for fully interacted CBSA-year-month fixed effects may oversaturate the model, as we might expect some of the effect of Airbnb to occur at the city level and not just zip code level. Nevertheless, we maintain Equation (2) as our main specification to offer the most conservative estimate of Airbnb's effects that we can. In unreported results, we verified that not controlling for fully interacted CBSA-year-month fixed effects leads to larger effect sizes.

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

[13] 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 lineup 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 zip codes without Airbnb.

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

[15] We add 1 to the number of listings to avoid taking logs of 0. In Online Appendix B, we show that our results are robust to dropping observations with zero Airbnb listings.

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

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

[18] Our results are robust, however, to the inclusion of controls reflecting the popularity of other home-sharing websites such as HomeAway and VRBO. We do so by using the Google Trends index, a widely used proxy for demand in several settings (Ghose 2009, Choi and Varian 2012, Li et al. 2016), as a proxy for demand for such platforms. We report these results in Table 19 of Online Appendix B.

[19] 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 Community Survey Design and Methodology (January 2014)” (accessed March 20, 2020, https://www.census.gov/programs-surveys/acs/methodology/design-and-methodology.html).

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