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The Effect of Airbnb on Housing Prices: Evidence From the 2017 Solar Eclipse

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The Effect of Airbnb on Housing Prices: Evidence from the 2017 Solar Eclipse *

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Abstract

This study estimates the causal impact of Airbnb expansion on local housing prices by exploiting the 2017 total solar eclipse as a natural experiment. The eclipse's path of totality created a exogenous temporary demand for short-term rentals that resulted in a persistent increase in area supply of Airbnb listings. The IV/2SLS results indicate that a one percent increase in Airbnb listings generates a 0.037 to 0.043 percent increase in housing prices, a magnitude consistent with other causal research on this question. However, we provide additional evidence that our result is driven by homeowners' willingness to accept (WTA) due to increased rental income from monetizing excess housing capacity, whereas previous research largely reports estimates that combine this effect with demand-driven displacements of long-term housing supply. These findings suggest that WTA effects play a major role in Airbnb's influence for more efficient utilization of housing as an asset, and that regulations that partially pan investor listings will have muted effects on housing affordability.

JEL Classification: R29, R31, H31
Keywords: Airbnb, housing market, home sharing, digital platform economy, housing price.

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1 Introduction

Peer-to-Peer ("P2P") homesharing platforms can potentially affect local housing prices through both supply and demand effects. The P2P homesharing technology has made it easier for short-term renters to effectively coordinate with investor-hosts in the marketplace for local living spaces. This increase in investor-owned properties can displace a relatively fixed supply of long-term housing, which drives up the price for long-term housing. Second, P2P platforms increase the efficiency of housing as an asset by making it easy for existing homeowners to list excess living space for rent to short term renters. Because homeowners participating in P2P homesharing value their living space for its ability to generate cash flow and its consumption services, the minimum price they would be willing-to-accept (WTA) to sell their home increases. We term the former effect of investor purchases the "demand effect," and the latter improved efficiency of housing as an asset the "WTA effect."

Recent policies directed at P2P homesharing tend to distinguish between homeowners who generate extra income by temporarily renting the excess capacity of their primary residence (Gigs) and individuals who acquire properties for investment purposes (Investors).[1] In particular, policymakers have been adopting less restrictive policies for Gigs, which implies that policymakers perceive a difference in the social value induced by the two effects.[2] This shift in policy raises an important question about the trade-off between existing homeowners being able to extract cashflow from their properties and the affordability of homeownership for new homeowners. Illuminating this tradeoff requires knowing the relative importance of the demand effect and the WTA effect in the evolution of housing prices in markets exposed to the P2P home-sharing platforms.

The empirical context of existing studies produce estimates driven by both demand and WTA effects. The reason is that scholars have relied on case studies of regulatory changes (Koster et al. 2021; Chen et al. 2022; Bibler et al. 2023) or Bartik shift-share instruments (Barron et al. 2021; Garcia-Lopez et al. 2020; Franco et al. 2021; Xu et al. 2021) to address endogeneity between housing prices and P2P expansion. So, while both sets of research recover causal estimates that Airbnb prevalence increases prices in the long-term housing market, they represent full, or combined, demand and WTA effects. We expand on the existing literature by estimating the effect of Airbnb expansion on

[1]Loosely following the terminology in the P2P literature, we will refer to homeowners listing excess capacity as "Gig" suppliers and those using the platforms to advertise properties that are used solely as short-term rentals as "Investor" suppliers.

[2]The most well-known example is probably New York City's "One Host, One Home" that requires listed properties be the primary residence of the host and that short term renters have access to the entire property. Other versions of the policies may relax the privacy elements or simply limit the number of days per year a property can be listed for rent so as to make it unattractive to investors.

housing prices using variation that is plausibly driven by WTA effects only.

Specifically, using data from 2014 to 2019, we estimate a IV/2SLS model of Airbnb on housing prices where exposure to the 2017 solar eclipse path of totality serves as an instrument for Airbnb expansion. We show that this eclipse induced a large and statistically significant increase in first time listings along the path of totality as spectators sought short-term rentals during this weekend event. Importantly, Airbnb activity in the treated areas increased for the entirety of our post period following the 2017 eclipse. As we show, this is consistent with the eclipse acting as a one-time lottery that covers the fixed start-up costs' for a new host to get a property listed on Airbnb; i.e., fixed costs that otherwise would have served as a barrier to entry were now covered by the eclipse-lottery. We also provide corroborating evidence on migration and primary residence homeownership to indicate that there was no permanent increase in demand for housing or short-term listings in these areas. Together, this evidence clearly demonstrates that the IV is both relevant and exogenous.

Our results point to Airbnb expansion causing a statistically significant increase in housing prices, with estimated elasticities in the range of 0.037 to 0.043. This finding is robust to numerous specification changes. These elasticities are sizable when compared to the existing research, which is in the range of 0.03 to 0.06.[3] While the existing studies rely on variation that triggers both the "WTA" and "demand" effects, we argue, and provide evidence, that the expansion in Airbnb activity induced by the solar eclipse caused local housing prices to rise only through the WTA effect. Entry in the Airbnb market increases the cash flow associated with homeownership, which implies that homeowners must be offered a higher amount to induce them to sell their homes; i.e., "WTA" increases. Consequently, our results suggests that the WTA effect accounts for roughly two-thirds of the previously estimated elasticities in the literature.

This finding has important implications of how we assess the likely impacts of recent efforts to regulate the short-term rental market. Policies like New York City's 2023 Local Law 18, which largely bans Investors in favor of Gigs, will have limited downward pressure on housing prices. The reason is that a significant share of the Airbnb price effect is driven by changes in WTA associated with the increased cashflow provided by Airbnb. Therefore, a policy that allows Gigs to continue their operations will keep housing prices high relative to a world with a complete ban. Airbnb remains a technology that allows for more efficient use of housing as an asset, and at least in the current setting, this translates into higher market prices for housing.

The remainder of the paper proceeds as follows. Section 2 describes the current literature and highlights our contribution. This is followed by a brief conceptual framework

[3]It is worth noting that all these elasticities are estimated in cases where the treated unit is a high income city. Therefore, our most comparable result is arguably that for high income counties, which is an elasticity of 0.08.

in Section 3, empirical strategy in Section 4, data in Section 5, and results in Section 6. We discuss the mechanism behind our findings in Section 7 and offer concluding remarks in Section 8.

2 Related Literature

The most relevant papers to our research are those studies that seek to causally identify the effect of Airbnb on housing prices. Our review of the literature highlights two important opportunities for making substantive contributions. First, the literature identifies the effect of Airbnb expansion by relying on variation induced by regulatory changes or the expansion of the market through the new P2P technology. Consequently, these papers implicitly produce estimates that combine the demand and WTA effect. From these studies, we compute comparable elasticity estimates that are plotted in Figure 1, with details in the calculation found in Appendix A. Second, the literature largely consists of case studies of large cities over a short-time horizon, whereas we explore a large geographic area over a five-year time horizon. We follow this section with a summary review of papers on other dimensions of Airbnb that provide some context for issues that may pertain to housing price capitalization (e.g. tax compliance, distribution of welfare gains, etc.).

2.1 Airbnb and the Housing Market

Estimating the causal the effect of Airbnb expansion on housing prices is challenging because of standard endogeneity concerns. In short, areas that are desirable to pay for short-term rentals tend to also be desireable for long-term residency. The existing literature relies on two sources of variation to overcome these concerns: variation induced by regulation and variation induced by tourist demand for short-term housing. Below we describe each approach and discuss how they relate to our work.

Evidence from Regulatory Policy. Several governments have implemented regulatory restrictions on Airbnb activity. The regulations tend to be local and vary substantially across jurisdictions (see Table 1 of Gauss et al. (2024)).

The least stringent regulatory actions studied are probably those examined in Bibler et al. (2023) and Koster et al. (2021). Bibler et al. (2023) examines city-level enforcement of local regulations in San Francisco (September 2017) and Chicago (December 2016) that started requiring registration that increased the cost of listing, while also ramping up the enforcement of other taxes and regulations. They estimate the effect of the policy with a differences-in-differences design that compared census tracts within city-limits to those in the same metropolitan area but outside the city limits. Using the 12 months prior through

18 months post policy, the authors find the policy restrictions on Airbnb reduced sale prices of homes in the regulated census tracts by about 4.3 percent in the most Airbnb dense portions of the metropolitan area. Similarly, Koster et al. (2021) looks at housing sales from January 2014 to October 2018 in Los Angeles County, where 18 of 88 cities implemented Home-Sharing Ordinances that demonstrably reduced the listings of Airbnb by about 50 percent and transaction prices by 2 percent.

Other policies investigated in the literature have more directly sought to restrcit the supply of Airbnb. Chen et al. (2022) investigates "One Host, One Home" policies in a staggered event-study of New York City (April 2016), San Francisco (April 2016), and Portland (February 2017). These policies capped the number of properties a host can manage in a single city. They construct a comparison group of zip codes from other U.S. cities using a coarsened exact matching design over the policy window of 2014 to 2017. They find that the policy announcement reduced housing prices by 1.4 percent, and further 1.9 percent reduction following policy enforcement. These effects are primarily driven by a significant reduction in listings owned by hosts with multiple properties. Seiler et al. (2024) studies the effect of Irvine, California's 2019 ban on short term rental platforms use in residential areas, focusing on Airbnb listings. They compare Irvine zip codes to a control group comprised of zip codes in neighboring Orange County cities from August 2018 to June 2021 using a difference-in-difference framework and find that the policy reduced contracted rents by about 2.6 percent over the two-year post-period.

Policies have been found to have similar effects in German cities. In Berlin, Duso et al. (2024) examines long term apartment rents by city-block and month from November 2014 to July 2019. They consider the staggered rollout of a city-wide prohibition on short-term rental listings. The authors estimate that each additional listing on Airbnb blocked by the restrictions reduced city-block rent per square meter by 1.3 to 2.4 percent. Similarly, Gauss et al. (2024) studied a cap on the number of permissible days that a given property can be reserved in the cities of Berlin, Hamburg, and Munich from 2015 to 2019. Though they do find from their differences-in-differences regression that the number of reservations via Airbnb decreased substantially, they found little spillover effect into other short-term markets or effects on long-term units.

All these studies support the claim that Airbnb contraction reduces housing prices and rents with estimated elasticities falling in the range of 0.01 to 0.11 (see Figure 1). Although these regulations vary in the way they work, they all have the effect of reducing the supply of short-term rentals via Airbnb by changing the nature of the property rights of those trading in the market. In restricting the terms of permissible use, both existing and prospective property owners experience a change in their respective valuations of properties.

Evidence from Instrument Variables. The remaining studies address the identification challenge through some form of Bartik shift-share instrument. These IV's include two components, with one predicting where Airbnb listings will arise among neighborhoods in a metropolitan area, and a second global aggregate index of Airbnb demand to predict when they will appear. These studies universally employ Google Trend Searches as their time predictor, but vary substantively on the locational predictor, and consequently the nature of the study results in other combined predictors via time trends, fixed effects, and other variables to satisfy the conditional independence assumption.

For the United States, Barron et al. (2021) uses the Zillow price index for US zip codes in core-based statistical areas from 2011 to 2016, and employs an IV/2SLS with fixed effects. Using the number of establishments in food services and accommodations as the spatial predictor, they estimate price elasticities of 0.018 for rents and 0.026 for housing prices. A study by Congiu et al. (2024) creates an IV by combining Google trends searches with reviews of the top 150 Tripadvisor attractions to estimate the effect of Airbnb expansion on housing prices in five Italian cities from 2014 to 2019, finding one percentage point increase in Airbnb density leads to a 0.618 percent increase in price per square meter. Using a measure of proximity to tourism amenities as their shift-share instrument, Garcia-Lopez et al. (2020) finds that a 100 unit increase in neighborhood Airbnb units increased long-term residential housing unit prices by 11 percent. Lastly, Franco et al. (2021) uses Airbnb listings at the beginning of the data period as the location share for a study of 106 Portguese municipalities from 2012 to 2016, finding a one percent increase in Airbnb units as a share of total housing units to increase housing prices by 3.74 percent.

Our study adds to the literature in several important ways. First, unlike the existing IV-based papers, our IV is arguably more exogenous and is based on a more transparent set of identifying assumptions. For example, it is not always clear whether identification in the Bartik-IV setting requires exogeneity of the shocks or the shares (Borusyak et al. 2021; Goldsmith-Pinkham et al. 2020). Secondly, the Bartik IV approach is implicitly capturing the effect of Airbnb on housing prices induced by the P2P's technology diffusing into areas. In one sense, the technology essentially lowers the transaction costs renters and hosts face in finding one-another to engage in market exchange. Lowering transaction costs increases demand for short-term rentals with little effect the fixed housing stock. Therefore, both existing property owners and prospective buyers can generate cash flows by putting the property to its most valued use. This leads to higher housing prices because buyers are willing to pay more and existing owners' willingness to accept increases. As a one-time event, the identifying variation from the solar eclipse does not represent a structural shift in the available technology affecting both buyers and sellers. Instead, the varaition comes from a loweiring of entry barriers for Gig hosts, which allows us to estimate the price effects of a change in willingness to accept by sellers; see discussion in Section 3.

2.2 Other Dimensions of Airbnb

The rapid expansion of Airbnb has also induced studies on various other effects. For example, Basuroy et al. (2020) finds that Airbnb expansion into a zip code increases the revenues of restaurants in that same zip code. Zervas et al. (2017) studies the impact on the hotel industry and finds that Airbnb expansion into Texas led to lower hotel revenues as hotels reduced prices to compete with Airbnb. Chang et al. (2022) finds that Airbnb expansion has a heterogeneous effect on the hotel industry with budget hotels competing on price while high-quality hotels increase prices and investment in service quality. There is also evidence that Airbnb expansion increases capital investments in homes (Bekkerman et al. 2022; Xu et al. 2021) and reduces deed transfers by providing homeowners with additional income (Bibler et al. 2023).

Another set of studies focus on tax incidence and tax compliance. For example, Wilking (2020) tests the remittance in-variance principle and finds that tax-inclusive prices on Airbnb listings increases after a policy change that shifted the remittance burden from the host to Airbnb. A related study by Bibler et al. (2021) finds that compliance rate with taxes on Airbnb transactions was at most 24% prior to a policy change that shifted the remittance burden from hosts to Airbnb. They also find that renters on Airbnb are insensitive to price changes which allows hosts to shift most of their tax burdens onto renters.

Scholars have also explored competition and discrimination on Airbnb. First, Chen et al. (2023) finds that professional and nonprofessional hosts have a competitive relationship. Second, as mentioned above, there is evidence that the competition between Airbnb and hotels depends on the quality of the hotel (Chang et al. 2022). Third, there is evidence that hosts from minority backgrounds earn lower rents compared to white hosts (Kakar et al. 2018; Marchenko 2019; Laouénan et al. 2022), and that reservation requests from minority guests are less likely to be accepted (Cui et al. 2020).

There have also been important theoretical contributions aimed at quantifying the welfare implications of Airbnb. The findings from this branch of the literature suggests that Airbnb is welfare improving in general with some important distributional impacts. For example, Farronato et al. (2022) finds that Airbnb expansion generated $305 million in consumer surplus and $112 million in peer host surplus in 2014. These gains were only partly offset by reductions in hotel revenues and variable profits. Additionally, the gains were especially large in bigger cities that experience large demand shocks. Similarly, Calder-Wang (2021) finds that while Airbnb expansion had a positive impact on social welfare in New York city, longterm renters experienced a total welfare loss of $2.4 billion.

In a related study, Li et al. (2022) estimates a structural model of homeowners decisions to list on Airbnb and find that while entry hurts renters in the long-term market, it benefits Airbnb hosts, particularly those who own affordable units.

3 Theoretical framework

The purpose of this section is to provide a conceptual framework for understanding the role of willingness to accept and willingness to pay in determining the effect of Airbnb market activity on observed market transaction prices. This framework is then used to explain how the solar eclipse instrument employed in this research generates variation strictly induced by willingness to accept.

3.1 Conceptual Framework

Let the housing market be comprised of three types of buyers and sellers that differ in the way they use a house. "Investors" use a house permanently for producing income from short-term rentals, "Gigs" use a house for both consumption and short-term rental profit, and "Owners" use a house for consumption only. These uses determine current occupants' minimum willingness-to-accept (WTA) and prospective buyers' maximum willingness-to-pay (WTP). For example, the Gigs' WTP is determined by both their consumption value and projected profits from short term rental as written in Equation (1).

$$WTP_{Gj} = \overbrace{\frac{\alpha(H)}{i}}^{consumption} + \overbrace{\max(\frac{(r-k-\alpha)h}{i} - F, 0)}^{profit} \tag{1}$$

In Equation (1), H is the flow of housing services provided by house j that can be partitioned into h units for rental income on P2P platforms, α is the value of a unit of housing service consumed, i is the discount rate, r is the rental price paid by short-term renters, k is the average variable cost, and F is an initial fixed listing cost.[4]

Equation (1) nests the WTP for the three types of buyers.[5] Owners consume all of their housing units so the WTP collapses to consumption value only ($h = 0$)

$$WTP_{Oj} = \frac{\alpha(H)}{i}, \tag{2}$$

while Investors rent all their housing units so $h = H$, and Equation 1 collapses to

[4] The fixed listing costs include the cost of preparing and registering the property for listing, mental cost of accepting a stranger in personal space, as well as the cost of learning about the regulatory environment. Importantly, these are considered to be one-time costs.

[5] The above taxonomy treats each owner as a type based on their current use of the property that determines their WTP function. Specifically, we incorporate the idea that an "owner" might capitalize the valuation of Gigs even though the owner might not be interested in being a Gig herself.

profits only

$$WTP_{Ij} = \max(\frac{(r-k)H}{i} - F, 0). \tag{3}$$

Consequently, we can characterize the maximum WTP for property j as the maximum of the three functions:

$$WTP_j = \max\{WTP_{Oj}, WTP_{Ij}, WTP_{Gj}\} \tag{4}$$

The minimum willingness to accept (WTA) has a similar formulation except that the initial start-up costs, F, are now sunk for those already participating in short-term rental market. Therefore, we can represent the WTA for Gigs as

$$WTA_{Gj} = \frac{\alpha(H-h)}{i} + \frac{(r-k)h}{i}, \tag{5}$$

which nests the WTA for Investors

$$WTA_{Ij} = \frac{(r-k)H}{i}, \tag{6}$$

and Owners

$$WTA_{Oj} = \frac{\alpha H}{i}. \tag{7}$$

Thus, a property owned by type $z \in (O, I, G)$ will transact where $WTP_j \geq WTA_{zj}$. Let p^*_{mt} be the mean of these transaction prices over a given time period t in market m. Variation across communities and individuals in the parameters (i, r, k, F, α) determine the stock of properties owned by each type, which we will define as O^*_{mt}, G^*_{mt}, and I^*_{mt} for owner, gig, and investor types, respectively.

Estimating the responsiveness of housing price to changes in the stock of housing requires exogenous variation in the underlying parameters that are observable to the researcher. An additional empirical challenge is that the parameters are most likely positively correlated across communities. For example, a location on the beach would offer high α and bid-rent function for r as it would be valued by both short-term renters and long-term residents.

3.2 The Effect of Exogenous Market Shocks

The existing empirical research has employed two plausibly exogenous sources of variation to identify the casual effect of Airbnb on housing prices: local regulations and Bartik shift-share instruments. Regulations can be categorized into two groups. First, some jurisdictions increase the costs of operating in the short-term market, e.g., Chicago's registration requirements. Second, some jurisdictions impose an outright ban or limitations on short-term listings, e.g., "One Host, One Home" policies in New York City, San Francisco, and Portland, as well as full bans in Irvine, California.

Regulations will affect the WTP and WTA functions via h for those that limit or ban short-term rentals or k and F for those that impose additional operational and listing costs. The simplest case to illustrate these impacts is one of a complete and perfectly enforced ban on short-term rentals. Such a ban would mandate $h = 0$, which would cause the WTP for property j to collapse to $WTP_{Oj} = \frac{\alpha H}{i}$. In other words, Investors would be willing to pay \$0 for a property that had no cash flow and Gigs' valuation would be limited to the consumption value.

Likewise, Investors' WTA_{Ij} declines to zero, and the Gigs' WTA collapses to the owner-occupied valuation so that $WTA_{Gj} = WTA_{Oj} = \frac{\alpha H}{i}$. The resulting market clearing transaction price in the presence of the ban, p^b_{mt}, is lower than the price without the ban, p^*_{mt}, because the ban lowers both the WTA and WTP. In practice, bans are less than complete and are imperfectly enforced. Therefore, both Investors and Gigs continue to operate at levels G^b_{mt}, and I^b_{mt}, with $G^b_{mt} + I^b_{mt} < G^*_{mt} + I^*_{mt}$.[6]

Bartik shift-share instruments work through identifying plausibly exogenous increases in the ability to rent properties to short-term renters. This can be interpreted as a increase in r, which raises WTP and WTA for Investors and Gigs. The resulting transaction prices on traded properties operate both through the higher WTP and WTA such that the mean observed price is $p^s_{mt} > p^*_{mt}$. The observed use of properties also shifts to Gig and Investor users such that $G^s_{mt} + I^s_{mt} > G^*_{mt} + I^*_{mt}$.

3.3 The Effect of the Eclipse as Celestial Lottery

The key insight here is that the variation exploited in the existing literature reflects permanent changes in both willingness to pay and willingness to accept driven by permanent shifts in the expected cash-flow of the property rights over short-term rentals. We contribute to the literature by relying on variation that arguably only affects the willingness to accept. Specifically, the 2017 Solar Eclipse described in Section 4.2 caused a large, exogenous, single weekend shock to the cash flow of short-term rentals. This one-time shock expanded Airbnb activity with long term impacts on WTA. To see how the eclipse led to a long-term increase to WTA only, consider the following exercise.

Suppose it is announced at $t = 0$ that Investor and Gig type properties listed on Airbnb in future period T of selected communities will receive a cash payment of R. What effect would this lottery have on Airbnb activity and consequently housing prices?

[6] Regulations that increase listing and operation costs k & F, will also change both WTP and WTA, and have similar qualitative price effects as a ban.

First, note that this future lottery payment could have anticipatory effects that affect WTP at $t = 0$ for Investors an Gigs, as reflected in equations (8) and (9):

$$W\tilde{T}P_{Ij} = \max(\frac{(r-k)H}{i} - F + \frac{R}{(1+i)^T}, 0) \tag{8}$$

$$W\tilde{T}P_{Gj} = \frac{\alpha(H-h)}{i} + \max(\frac{(r-k)h}{i} - F + \frac{R}{(1+i)^T}, 0). \tag{9}$$

This implies the new willingness to pay for property j is higher to the extent it is more valued by Gigs and Investors.

$$W\tilde{T}P_j = \max\{WTP_{Oj}, W\tilde{T}P_{Ij}, W\tilde{T}P_{Gj}\} \tag{10}$$

Stated differently, lottery winners for whom $\frac{R}{(1+i)^T} > F$ will be induced to purchase and list in the short-term rental market prior to period T. Notice that R is only paid in period T, so it does not affect WTP valuations for any owner type in any community in periods after T.[7] Time value of money also makes it most likely to induce listing as the time period approaches T. Additionally, the higher WTP will likely translate to higher transaction prices in the period before T.

Second, the lottery will affect the WTA of existing Gigs and Investors in the pre-period as reflected in equation (11) and (12).

$$W\tilde{T}A_{Gj} = \frac{\alpha(H-h)}{i} + \max(\frac{(r-k)h}{i} - F + \frac{R}{(1+i)^T}, 0) \tag{11}$$

$$W\tilde{T}A_{Ij} = \frac{(r-k)H}{i} + \frac{R}{(1+i)^T} \tag{12}$$

The Investors are already listing and thus receive a windfall equivalent to $\frac{R}{(1+i)^T}$. The impact on Gigs depends on the type of Gig. Those Gigs who were already listing prior to the lottery announcement will receive a windfall like the Investors. Those Gigs who were not listing their properties on the short-term market prior to the lottery might be induced to list if $\frac{R}{(1+i)^T} > F$. That is, Gigs for whom entry costs were too high are induced to switch into short-term rental listings and are able to access the long-term cashflow $\frac{(r-k)h}{I}$.

All these impacts that occur between lottery announcement and lottery implementation will affect the level and composition of property owners who supply Airbnb units in the post period of the communities that win the lottery, i.e. $G^e_{mt} + I^e_{mt} > G^*_{mt} + I^*_{mt}$. Therefore, while the post-lottery period WTA and WTP functions are the same for win-

[7] In other words, the WTP will be the same across lottery winners and losers in the post period; i.e., after period T.

ners and losers of the lottery, the composition of the property owners in the winning communities will be shifted toward Gigs and Investors relative to the composition in the losing communities. That is, there will be more properties in the post-period lottery-winning zone with the WTA_{Gj} and WTA_{Ij} that determine transaction prices observed in the post-period, p^e_{mt}. Thus, the difference in prices p^e_{mt} and p^*_{mt} is strictly due to more properties with a higher WTA.

These observations imply that Airbnb expansion can affect housing prices without shifting the physical housing stock from the long to short term market. The research question to be addressed using the 2017 eclipse event is whether this is empirically important.

A few additional observations are worth mentioning before the empirical analysis. First, comparing pre- and post-eclipse transaction prices will yield a lower bound estimate if the lottery raises WTP and WTA valuations prior to the eclipse; i.e., if anticipatory effects are large. Consequently, any observed increase in price in the post-period would be conservative estimates of the true local average treatment effect. It is worth noting that while it is theoretically possible that WTP increase in the pre-period, we find no evidence to suggest that it did. Second, the framework assumes that the Gig and Investor types are similarly constrained by fixed entry costs. However, fixed costs are likely less determinative for Investor types than Gig types. First, Investors might have lower fixed entry costs because they already have the experience of listing properties. Additionally, new Investors likely do not face the psychological costs that might affect a Gig type who must contend with having a stranger in their home. Second, regardless of the magnitude of the fixed entry costs, Investors entry decisions are probably less influenced by the lottery because they are less credit constrained. Therefore, it is likely that Gigs are the ones most responsive to a lottery like the one we describe above, a prospect investigated empirically in section 7.

4 Empirical Strategy

This section describes the empirical strategy used to identify the effect of Airbnb expansion on the price of houses. We begin with a two-way fixed effect model that faces obvious endogeneity problems, then describe an instrumental variables approach – informed by the conceptual framework in Section 3 – and provide evidence that it satisfies the necessary assumptions to overcome the endogeneity problem.

4.1 Model Specification

The OLS specification for estimating the effect of Airbnb on house price is:

$$ln(p_{it}) = \gamma Airbnb_{it} + \theta_i + \tau_t + \epsilon_{it}, \tag{13}$$

where p is the housing price index for area i in time t, *Airbnb* is a measure of Airbnb market penetration, θ is area fixed effect, τ is time fixed effect, and ϵ is the error term. Section 5 describes the different measures of these variables as well as the units for area and time in detail. In all cases, we begin our analysis during 2014 and end in 2019, the last complete year prior to the 2020 COVID pandemic.

The parameter of interest, γ, is the estimated effect of Airbnb expansion on housing prices. However, the estimated parameter in Equation (13) is likely biased. In particular, it is likely that unobserved shocks or heterogeneity affect general attractiveness of a location to both long-term and short-term residents. Similarly, property construction and maintenance costs are likely to be correlated within an area regardless of whether the property is ultimately used for short- or long-term occupants. Therefore, both the high housing price and increased Airbnb expansion could be jointly determined by some other factor. As described in section 2, the existing literature addresses this problem by relying on exogenous variation in Airbnb induced by policy changes or shift-share instruments. In this paper, we rely on exogenous variation induced by the path of totality of the 2017 solar eclipse to identify the local average treatment effect estimates of γ. The system of equations estimated in this paper for the main results are:

$$\text{Airbnb}_{it} = \pi_0 + \pi_1(\text{Post}_t \times \text{eclipse}_i) + \theta_i + \tau_t + \nu_{it} \tag{14}$$

$$ln(p_{it}) = \beta_0 + \beta_1 \widehat{\text{Airbnb}}_{it} + \theta_i + \tau_t + \epsilon_{it} \tag{15}$$

The rest of this section describes the solar eclipse as an instrument, providing relevant background, and investigating necessary instrument assumptions of relevancy and exogeneity.

4.2 The Eclipse IV: Background

A large section of the United States experienced a total solar eclipse on August 21, 2017. A total solar eclipse occurs when the distance between the Moon and Sun relative to the Earth is such that the Moon blocks out the entirety of the sun as viewed from a specific location on Earth. This creates a path of complete darkness called the "path of totality". The path of totality for the 2017 Eclipse was approximately 70 miles across and stretched across 287 counties in 14 states: from Salem, Oregon in the northwest to

Charleston, South Carolina in the southeast (see Figure 2).[8] While the eclipse lasted over 2.5 hours, the duration of totality ranged from 130 seconds in Oregon to 160 seconds in Tennessee.[9]

Solar eclipses are not autocorrelated across time for a given location. That is, despite the fact that there are approximately two to four solar eclipses each year, most specific locations on Earth can expect to see a total solar eclipse once every 100 years (see Figure 3). Solar eclipses are even rare at the country level; the US experienced a total eclipse in 1979, 2017, 2024, and will not experience another one until 2044. Consequently, viewing a total solar eclipse is a once in a lifetime event for most people unless they are willing to travel to the location of the event. This is precisely what happened with the 2017 eclipse. Like many other celestial events, it attracted the attention of a lot of people in America who in 2017 found themselves within traveling distance to one of the rarest events on Earth. Below we demonstrate that this event temporarily increased demand for short-term rentals, which in turn accelerated Airbnb expansion along the path of totality.

4.3 The Eclipse IV: Relevance Assumption

A valid instrument must have its own causal effect on the endogenous variable of interest. In this case, this assumption of relevancy implies that the solar eclipse must have induced an expansion of available Airbnbs in the path of totality.

Evidence for instrument relevancy can be directly provided with the data used in this study. For example, we compare trends in Airbnb activity between counties crossed by the totality ($eclipse_i = 1$) and those that were not ($eclipse_i = 0$) in the 12 months surrounding August 2017 by estimating the following regression:

$$Airbnb_{it} = \theta_i + \tau_t + \sum_{n=-12}^{-3} \gamma_t \times eclipse_i + \sum_{n=-1}^{12} \delta_t \times eclipse_i + \epsilon_{it}, \tag{16}$$

where $Airbnb_{it}$ is a measure of Airbnb activity in county i and month t. Figure 4 displays the γ and δ coefficients when using the number of Airbnb units listed as available or rented in the county for the first time. In other words, these are new property listings appearing in the Airbnb data for the first time by month and county. The figure demonstrates that there was a substantive and statistically significant increase in new

[8] The fourteen states are Oregon, Idaho, Wyoming, Montana, Nebraska, Iowa, Kansas, Missouri, Illinois, Kentucky, Tennessee, Georgia, and North and South Carolina.

[9] For example, people in Nashville, TN experienced the eclipse as follows: the moon began its trek across the sun at 11:58:31AM, fully covered the sun at 1:27:25PM, totality ended at 1:29:23, and the moon completed its trek across the sun at 2:54:02. Consequently, most of the experience is one of a partial eclipse. See NASA's eclipse2017 page for time and duration for other major cities along the path of totality.

listings in the month of and prior to the solar eclipse.[10] However, there is no persistence in new listings. This is important since a statistically significant increase in listings after the eclipse date would have suggested that the eclipse induced a longer term increase in demand to visit the areas exposed to the solar eclipse. The lack of new listings after the eclipse implies no substantive WTP effects in the post-period.

Figure 5 similarly plots the coefficients from equation (16), this time using the stock of all listings as the dependent variable. The results show a similar anticipatory effect with increases in listings coming in the month before and during the month of the eclipse. Importantly, we find that once these properties entered the short-term market, they remain available on the market after the solar eclipse. This is consistent with the eclipse as a one-time lottery that induced participation in the Airbnb platform economy by covering fixed costs that were a barrier to the initial listing. To the extent that the effect size begins to diminish over time, it is driven largely by the non-eclipse group catching up rather than those in the eclipse zone dropping out.

Relevancy is also corroborated by other research. It is estimated that over 200 million Americans viewed the 2017 eclipse directly or indirectly (Miller 2018). Miller (2018) estimates that 8% of all US adults traveled to view the eclipse and that 31% of these Americans experienced the path of totality. Further evidence that people traveled to the path of totality is provided by two studies that rely on social media activity around the time of the eclipse. First, Feng et al. (2019) finds a high concentration of tweets were posted by people along the path of totality. More importantly, they also provide evidence of both interstate and intrastate travel toward the path of totality. A second study by Ma et al. (2020) uses geotagged Instagram photos to identify 16 clusters of photographers that span the entirety of the path of totality. They also find that 65% of individuals who took a photo within the path of totality had traveled to the path of totality.

There is also circumstantial evidence indicating that visitors to the path of totality stayed at least one night. For example, according to some key statistics published on totaleclipsecolumbiasc.com, an estimated "1.6 million people traveled to or within South Carolina to witness the eclipse in the path of totality." There were over 120 events in the region, hotel occupancy was up more than 135%, Airbnb bookings were up 570%, and 40% of Columbia's Airbnb bookings were with first time hosts.[11] Wyoming, another state in the path of totality, had a similar experience. According to a study commissioned by the Wyoming Office of Tourism, 261,000 people traveled in the state of which approximately 75% were out-of-state tourists. They report that travelers stayed an average 4 days and 3.5 nights. A similar pattern is observed in Nebraska, which saw over 600,000 out-of-state travelers who stayed an average of three days. The claim that tourists stayed

[10] This is consistent with the claim made by totaleclipsecolumbiasc.com that 40% of Columbia's Airbnb bookings in the days leading up to the eclipse were with first time hosts.

[11] Website was last accessed on January 24, 2024 at 8:00PM.

multiple days and thus required lodging is further supported by traffic data analysis in the affected states. For example, Ngeni et al. (2022) shows that traffic volume peaked three days before the eclipse in the most affected areas, increased post eclipse, then returned to normal two to three days after the eclipse.[12]

4.4 The Eclipse IV: Exogeneity Assumption

The potential threat to the exogeneity assumption is that the eclipse had a direct effect on housing prices by changing the demand for long-term housing inside the path of totality. For example, it could be the case that knowledge of the impending eclipse incentivized people to buy long-term housing in the path of totality to be in a position to view the eclipse. Additionally, it could be that demand for long term housing increased because the eclipse increased awareness of the places in the path of totality. Both of these threats seem unlikely.

In a world with many competing large and attractive events, participants must decide whether the accommodation required to view a specific event should be in the form of long term housing or short-term rental. It will probably be the rare participant who offers a premium on long-term permanent residence to view one-time events instead of relying on short-term rental. Recall that observing a total eclipse from any specific location on Earth is an extremely rare event. For example, the three most recent total eclipses visible from the USA occurred in 1979, 2017, and 2024, and the next one will not be until 2044. Additionally, the path of totality of the 2017 eclipse only intersects three future total eclipses: 2024 (border of Illinois, Missouri, and Kentucky), 2052 (along the coastal edges of South Carolina), and 2078 (Carolinas and North Eastern Georgia) (see Figure 3).[13] So, while it is clear that people will visit a location to view a one-time event and thus increase demand for short term housing, we find it unlikely that they will buy long term housing to experience one-off events.

A more realistic threat to validity is the possibility that the eclipse increased awareness of the cities and towns in the path of totality. If then people become aware of the beauty of a specific location, they might respond by returning after the eclipse to buy long-term housing in that location, thus increasing demand and price. Alternatively, these eclipse viewers might systematically find the locations unappealing and become less likely to move into the area to purchase homes. A systematic bias in either direction

[12]Interested readers can visit www.eclipse2024resources.com for more information on the impact of the 2017 eclipse.

[13]Of course, the overlapping eclipses could affect the marginal home buyer who would have bought a house in the neighborhood of the overlapping area regardless of the eclipse. For example, a person who was planning to buy a house in southern Illinois might be swayed to select an area inside the path of totality rather than one outside. But these effects are most surely too small to affect our identification strategy since there are only three overlapping areas and the time between eclipses is so long.

would cause us to attribute to Airbnb presence that which is an independent effect of the eclipse. We investigate this threat by exploring migration data. If demand for long-term housing increased in response to the eclipse, then we should observe higher in or out migration flows to eclipse counties relative to non-eclipse counties in the post period. Figure 6 presents this comparison in the event study framework, and it strongly suggests that the eclipse had no effect on annual migration.[14] The point estimates are very close to zero and precisely estimated, with the 95 percent confidence intervals ranging less than $+/-50$ migrants on a mean annual base migration of 1,354.

Overall, there is strong cause to believe that the 2017 eclipse is a valid IV for this study. First, it caused an increase in demand for short-term accommodations. Second, it had no direct effect on housing prices. Third, we find that treated and control counties were statistically indistinguishable across observables during the pre-period, which is consistent with randomization according to local conditions (see Table 1).

5 Data

We use data from several sources in our empirical analysis. Data on Airbnb listings from AirDNA are used to create our variables of interest; GIS data on the 2017 solar eclipse from NASA are used to create our IV, and data on the housing price index from the Federal Housing Finance Agency is our outcome variable. We also measure housing stock with data from the U.S. Census Bureau. Our primary analyses cover the period 2014-2019 and are at the county-year level. Below we describe each of the data sources with more details on how we use them. Table 2 provides the summary statistics of the variables used in the regression analyses by eclipse and non-eclipse counties.

5.1 Outcome: Housing prices

The Federal Housing Finance Agency (FHFA) House Price Index (HPI) is constructed using data from mortgage transactions purchased or guaranteed by Fannie Mae and Freddie Mac. The index tracks changes in the prices of single-family homes using a repeat-sales methodology, which compares the prices of homes sold at different times but with similar characteristics, such as location, size, and age. This methodology allows for the calculation of an index that controls for the quality of the homes being sold and provides an accurate measure of price changes over time. The FHFA HPI is reported at frequencies that vary with the unit of government. They provide data from 2014 to 2019 at the annual level for counties and 5-digit zip codes, as well as quarterly level for MSA's

[14] We collect migration flows for the period 2014 to 2019 from the Internal Revenue Service Statistics of Income Division. These data are used to estimate a panel event study comparing treated counties to control counties before and after the 2017 eclipse.

and 3-digit zip codes. The HPI is considered one of the most reliable measures of changes in house prices and is used by policymakers, researchers, and the real estate industry to monitor trends in the housing market.

5.2 Variable of Interest: Airbnb Expansion

The data for Airbnb activity are obtained from AirDNA, a proprietary dataset of Airbnb listings in the US that is used in most of the previous studies described in the literature review. The data includes information about property and host attributes, as well as performance during months from 2014 through the end of 2019.

Our variable of interest measures the extent to which Airbnb has expanded into a specific geographic market, for which we consider two primary measures. The first measure, "Airbnb units", is the total number of Airbnb properties listed as available or reserved in each geographical area i divided by the U.S. Census Bureau's estimate of "Housing Units" in area i (U.S. Census Bureau 2020) for period t; see Equation (17). The Census defines a housing unit as a house, apartment, mobile home, trailer, group of rooms, or a single room designated to serve as separate living quarters. This measure takes a value of zero for geographical areas (e.g., a county-year) with no Airbnb listings but a positive number of housing units. Ideally, there would be an absence of measurement error and theoretical maximum value of this variable would be one in geography i and period t if every Census housing unit in it was reserved or listed as available for rent. However, both the numerator and denominator are measured with error. The U.S. Census is a survey-based estimate that can deviate from the true availability. Additionally, a single property can be divided into multiple Airbnb units and properties can change owners within a given time period. This means that the max value could theoretically deviate from one, albeit the actually observed maximum in the data is far below at 0.20.

$$AirbnbUnits_{it} = \frac{Unique\ Property\ Listings\ on\ Airbnb_{it}}{HousingUnits_{it}}. \tag{17}$$

While Equation (17) is similar to the measures employed in previous research, we consider an alternative measure that accounts for the intensity of Airbnb market expansion. To do this, we reweight the first measure according to the frequency with which a given unit lists as available for rent; see Equation (18). Consider two counties (A and B) with the same number of housing units and only one property listing on Airbnb during the year. Suppose the property in county A is listed every day of the year while the property in county B is listed only once during the year. Our first measure, "Airbnb units", would be the same for both counties. However, county A arguably has greater Airbnb market penetration given the greater frequency of listings. We therefore construct a measure that takes a value of zero if no properties ever listed in an area during the period and a value of one if every unit listed as available for rent during every day of the period. For each area i during period t with days d:

$$AirbnbDays_{it} = \frac{\sum_{d=1}^{D} Unique\ Property\ Listings\ on\ Airbnb_{idt}}{HousingUnits_{it} \times D}. \tag{18}$$

D is 365 or 366 for annual data and around 90 for the quarterly data. The mean of this variable is 0.018, implying that Airbnb's occupied about 1.8 percent of the theoretical maximum. An additional advantage of this measure is that it is better at handling cases where properties turnover and are relisted under different owners in the same time period. That is, a property that lists under one owner for the first half of the year and another owner for the second half would have the same contribution to the $AirbnbDay$ measure as a property that lists continuously under a single owner.

Census estimates of housing stock are only available at the county and Metropolitan Statistical Areas (MSA) level so these measures are only used when we estimate the model using county and MSA as the unit of observation. We use the numerators of Equations (17) and (18) when the geographic unit of analysis is the zip code.

5.3 IV: Solar Eclipse Exposure

The identification strategy described in Section 4 requires that we are able to identify the path of totality; the spatial locations where 100% of the sun is eclipsed. We gather this information from shape files available at the National Aeronautics and Space Administration's (NASA) Scientific Visualization Studio.[15] The shape files match each point in the US to eclipse exposure and thus allows us to create our IV. Our main analyses define the IV as an indicator variable that takes a value of 1 if any part of a geographic jurisdiction is in the path of totality and zero otherwise. Note that we count a jurisdiction as treated if any part of that jurisdiction was exposed to the path of totality. Since an area need not literally be in the path of totality to provide a visitor with access to the event, we will consider specifications where the IV is the shortest distance between the centroid of the jurisdiction and the centerline of the path of totality.

5.4 Housing Supply Zoning Stringency

We consider the potential for heterogeneous effects by the stringency of local zoning using data from two sources. First, we use the 2018 version of the Wharton Residential Land Use Regulation Index described in Gyourko et al. (2021). This index is created from responses to a land use survey of approximately 2,500 jurisdictions across the country. It is designed to capture several dimensions of regulatory stringency including state and

[15]The relevant data can be found at https://svs.gsfc.nasa.gov/4518

local political pressure, court involvement, zoning processes and rules, supply restrictions among others. The index ranges from -2.6 to 4.86 and is standardized to have mean of 0 and standard deviation of 1. This index is only available for 2006 and 2018 and based on data collected for a few large cities, suburban areas, and a few rural areas. We collapse the data at the county level to get the mean of the index for each county represented in the survey. Our final sample includes data on land use restrictions for 30% (=84/287) of treated counties and 38% (=934/2439) of control counties. We categorize counties as either lightly regulated (bottom two quartiles of the index) or highly regulated (top two quartiles).

Our second data source on housing restriction is from Ganong et al. (2017) who construct a measure of zoning stringency by counting the number or appellate and state supreme court cases that contain the word “land use.” These data are used to rank states from least to most restrictive. We similarly treat this ranking as a time invariant attribute to consider differential effects in the most restrictive states based on whether or not they are in the top 20 of the ranking (i.e. have a rank of 30 or higher).

6 Results

6.1 Main Results

For our main analysis we estimate Equations (13) and (14) using county-year as the unit of analysis and present the results in Table 3. The outcome variable is the natural log of the housing price index and we report results separately for four measures of Airbnb activity. Columns 1 and 2 report results for $AirbnbUnits$ and $AirbnbDays$ as defined in Equations (17) and (18), respectively. Columns 3 and 4 report results for the natural log of the number of listed units and days, respectively. While these latter measures deviate from our preferred measures, they provide convenient and quick interpretations that are also most analogous to the previous literature summarized in Figure 1. We also report the pre-eclipse mean and standard deviations of the Airbnb measures to aid in the interpretation of effect sizes.

First stage results. The results presented as an event study in Figure 7 show that areas treated by the eclipse experienced a statistically significant increase in Airbnb activity. Importantly, we observe that Airbnb activity remained elevated in the affected areas up to two years after the eclipse. This finding is consistent with the first stage results presented in Panel B of Table 3. We find that exposure to the eclipse had a large positive effect on Airbnb activity regardless of the measure we use. The number of Airbnb property listings per housing unit increased by one-third to one-half of the pre-period standard deviation, while the logged counts reflect increases of 0.53 and 0.457 percent when measured by units and days, respectively. The results also confirm that we have a strong IV with F-statistics above 25 when the Airbnb activity is measured in levels, and well over 150 when logged.

Second stage results. The local average treatment effects presented in Panel B of Table 3 indicate that the eclipse-induced expansion of Airbnb led to a statistically significant increase in housing prices. For example, the results in column 1 suggest that a one standard deviation increase in the number of Airbnb listings per 100 housing units (0.004) is estimated to increase the housing price index by 0.05%.[16] We get a similar estimate when we use Airbnb listed days per 100 housing-days in column 2; a one standard deviation increase in Airbnb activity leads to a 0.04% increase in housing price.[17] The log-log specifications in columns 3 and 4 provide LATE elasticities and indicate that a one percent increase in Airbnb activity increased housing price by approximately 0.037 and 0.043 percent for number of listed units and listed days, respectively. Though our preferred measure of Airbnb activity is listed days, listed units is most similar to the previous literature and is the value we report when comparing our results with the existing literature in Figure 1.

6.2 Robustness

The analyses to follow shows that the results are robust to alternative definitions of the unit of analysis, Airbnb activity, sample composition, and definition of eclipse exposure. Since these robustness checks will sometimes affect the measurement of Airbnb market penetration by changing the denominator, in the text we primarily discuss the log-log specifications that will be directly comparable to the elasticities reported in columns 3 and 4 of Table 3.

6.2.1 Unit of observation

County and year is the unit of analysis in our main results. This raises a couple of concerns. First, there might be concern that county is not the appropriate unit of analysis for housing markets. For instance, the housing market relevant to home buyers could be broader or narrower than a county. Additionally, there would be spillover effects if the housing market is too narrowly defined by our geographies (e.g. if Airbnb redistributes demand in a multi-county housing market that lowers housing prices in one county and causes them to rise in the adjacent county). Second, because the eclipse occurred on a single day and hosts can list at a daily frequency, annual data possibly misses some

[16]Calculation: $[exp(11.272/100) - 1](0.004)(100) = 0.05\%$.

[17]Calculation: $[exp(0.564/100) - 1](0.065)(100) = 0.04\%$.

important within-year variation. Below we show that our results are robust to these considerations.

In Table 4 we broaden the unit of analysis from county to County Based Statistical Area (CBSA). Because CBSAs are defined as common economic zones based on residency and employment commuting patterns, they are reasonable for consideration as a common housing market to prospective buyers. Though not all counties belong to CBSAs, the availability of housing price indexes by quarter allows us to use higher frequency data and thus capture more of the temporal variation in Airbnb activity. The results are qualitatively similar to our main results; Airbnb expansion induced by the eclipse led to a statistically significant increase in housing price, and the instruments remain very strong as gauged by the first stage F-statistics. The results indicate that a one percent increase in Airbnb activity from the solar eclipse caused local housing prices to increase by 0.017 to 0.021 percent. These effect sizes are about half of the effect sizes found in Table 3, but remain statistically significant.

We also investigate by narrowing the unit of analysis to zip codes. These are zones based on logistics for mail delivery employed by the United States Postal Service, and are therefore not restricted to county administrative boundaries. The housing price index from FHFA is available for 3-digit zip codes on a quarterly basis, and an annual basis for the geographically smaller 5-digit zip code. However, we do not have estimated housing stock inventories for these geographies, and therefore can only measure Airbnb penetration with the numerators of our measures in equations (17) and (18). Table 5 shows that a 1% increase in Airbnb activity leads to a 0.02% to 0.035% increase in housing prices depending on the Airbnb measures and zip code levels, slightly smaller than our main results in Table 3.

6.2.2 Definition of IV

The analyses presented above defines the IV as an indicator variable that is equal to one if a unit is in the path of totality and zero otherwise. However, the benefits of access to the totality is plausibly something that declines as you move further away from the center line of the path, and that this benefit does not abruptly disappear at the border. For instance, a visitor could rent lodging in the areas just outside the path of totality and then commute into the path of totality on the day of the eclipse, causing a control county to experience some dosage of the treatment. Furthermore, the duration of the full eclipse is longest at the center line, so there is perhaps reason to expect a premium for listing as you get further inside the path.[18] Both cases would bias our main estimates

[18]The center line of totality is the line that runs through the middle of the path of totality. The period of totality is longest on this line, suggesting that places on the center line might offer a higher payoff for listing on Airbnb.

toward zero.

We address these concerns in two ways. First, we create a buffer zone around the path of totality and drop these plausibly treated counties. Second, we redefine the IV with measures of distance from the line of totality.

Buffer zone. The path of totality is approximately 70 miles across. We define a buffer zone with a width of 35 miles on either side of the path of totality and drop all counties inside this buffer zone. This approach essentially excludes these potentially treated counties from the control group and should limit the incidence of contamination. Reestimating the model using this restricted sample yields results that are nearly identical to our main results, (see Table 6). Since our treatment group includes partially exposed counties, we suspect any misclassification error must be small.

Distance to Line of Totality. As an alternative strategy, we define the instrument as the inverse of the distance from the line of totality to the centroid of each county multiplied by the post-eclipse indicator. This implies that counties whose centroid is closest to the line of totality –where the duration of the eclipse was longest – receive the strongest dosage.

We include quadratic and cubed specifications of the distance instrument, but find little substantive difference in the alternative first stage specifications that are presented in Table 7. The elasticities of Airbnb expansion range from 0.02 to 0.027 and are statistically significant with first stage F-statistics in the 41 to 143 range. However, the instrument variable is weaker when using the level measures of Airbnb market penetration, with F-statistics below 12 in all cases.

6.2.3 Sample composition

Our main results use an unbalanced panel due to missing observations for the housing price index and housing stock inventories. One concern with this approach is that the results could be driven by changes in the composition of counties over time. We address this concern by re-estimating our main specification using a fully balanced sample. Specifically, we restrict the analysis to counties that were present in our sample in 2014 (i.e., the beginning of our sample). This restriction causes us to lose over 3,300 county-year observations, but nevertheless the results in Table 8 are extremely similar to those in Table 3.

6.3 Heterogeneity

This section examines heterogeneity in the effects described in our main results by regressing the model on subsamples of the full data. We consider two types of heterogeneity:

zoning restrictiveness and household income.

6.3.1 Zoning restrictions

Places with restrictive policies on housing supply expansion may experience larger price increases. We explore this possibility by re-estimating our models on substamples with two measures of land use restrictions, a state-level ranking of zoning restrictions provided in Ganong et al. (2017) and the Wharton Residential Land Use Regulation Index of 2018 (WRLURI) Gyourko et al. (2021).

The results presented in Table 9 provide mixed evidence on whether zoning restrictiveness results in larger effects of Airbnb on logged housing prices. In the Zone Rank methodology of Panel A, the elasticities are substantively larger for counties that belong to a state in the top half of the restriveness ranking relative to the bottom half. For the WRLURI measure, the results are pretty comparable. This pattern in the elasticities is the same regardless of how we measure Airbnb activity. In the Zone Rank approach, a one percent increase in units listed on Airbnb increases housing prices by 0.078 percent in the most restrictive states, but just 0.018 percent in the least restrictive. For the WRULRI measure, these elasticities are 0.04 and 0.035 for units and days, respectively. Moving from less restrictive to more restrictive increases the size of the point estimates, but the differences are not statistically distinguishable.

6.3.2 Household Income

We group counties into high or low income groups based on their pre-eclipse period median household income. Table 10 presents the results which indicate the LATE is driven primarily by above median income counties. In fact, a one percent increase in Airbnb expansion increases housing prices in high income counties by 0.08 to 0.10 percent depending on the measure of Airbnb market penetration used. Corresponding estimates in low income counties range from 0.018 to 0.022. A potentially important implication of this finding is that the previous literature's findings have been mostly case studies of areas whose incomes are high by national standards (Los Angeles, Chicago, Barcelona, New York, San Francisco, Berlin, Florence, etc.). Arguably, we should use the elasticity of 0.081 when comparing our results to the existing literature.

7 Mechanism

Section 3.3 argues that the eclipse LATE is driven by changes in the WTA of those who were induced to enter the short-term rental market. This is in contrast to previous research where the price effects reflect changes in both the WTP and WTA. Below we present several pieces of evidence to support our claim that the mechanism behind the higher housing prices in our study is driven by higher reservation prices in the WTA of existing owners, and that there was no displacement in the longterm housing market.

First, if the post-period was marked by a permanent increase in demand for short-term rentals, we would observe continued increases in new listings driven by higher WTP buyers and a possible shift of housing units from the long-term to short-term market. But this is not what we observe in the data. We already showed that the effect of the eclipse on new listings was a one-time event leading up to the solar eclipse (see Figure 4). Similarly, Figure 6 showed that there was no increase in migration to the area that would be consistent with a change in WTP from post-eclipse permanent residents.

Second, we argue in Section 3.3 that the eclipse was akin to a lottery where residents of a county were offered a one-time cash payment if they listed an Airbnb unit on the weekend leading to the Eclipse on Monday, August 21, 2017. This one-time payout likely induced some residents in the path of totality to enter the Airbnb market because the one-time payout covers the fixed entry costs. These fixed costs are likely larger and play a more substantive role in the entry decision of casual renters ("Gigs") than experienced renters ("Investors").[19] Therefore, we should expect to find that the increase in new listings is driven by Gigs with no effect on the long-term housing supply.

In Figure 8, we use an event study to support the claim that the eclipse offered a one-time abnormal return. To do this, we use AirDNA data on daily listings to calculate "weekend revenue" defined as the revenue collected on Fridays to Mondays. For example, the weekend of the eclipse ($t = 0$) is from Friday, August 18 to Monday, August 21, 2017. The previous and subsequent Friday-to-Mondays represent the surrounding weekends in the event study, where we calculate the accumulated revenue from these daily reservations. As expected, properties in treated counties collected an average of $3,150 more in rental income than properties in control counties over the eclipse weekend. This could be interpreted as the expected cash value of the eclipse lottery that induced participation into the local Airbnb markets.

We also have evidence that the eclipse primarily induced Gigs rather than Investors to enter the market.[20] Figure 9 shows that the number of properties listed by Gig hosts substantively increased during and following the eclipse for counties in the path of totality,

[19]Investors would have already incurred much of the fixed costs for their existing properties and are less likely to face liquidity or credit constraints. Additionally, investors are less likely to face psychological costs associated with sharing their property with strangers.

[20]To do so we first define Investor hosts as those hosts that have multiple properties and each property is listed in its entirety. Gig hosts are hosts that have a single property that is only partially available (i.e. the renters share the space with the owners). These two restrictions constitute a small fraction of the total Airbnb listers, but likely succeed in accurately identifying properties that have left the long-term owner market to become lodging for short-term visitors (Investors) and those that remain in the longterm market (Gigs).

but investor hosted properties show no such effect. This finding suggests that there was no crowding-out of long-term residential housing supply. In other words, long-term residents began to participate in the short-term market on a part-time basis rather than being displaced by full time listings.

A third piece of evidence that our results are not driven by the demand effect complements our earlier analysis of county migration (see Figure 6). If housing units are being taken off the market for long-term housing and are no longer used for permanent residences, then there should be a decline in the number of properties associated with a homestead tax exemption. If people are looking for vacation homes or investment properties then we should see a decrease in the number of properties that are claiming property tax homestead exemption in the treated counties.[21] We obtain data on the homestead eligibility of properties from Corelogic and use these data to calculate the number and share of parcels that had the homestead exemption in each county year. Figure 10 presents the result of an event study on these outcomes and finds that homestead became no more or less prevalent in counties exposed to the totality following the eclipse.

Another signal of a shift in the long-term housing market would be changes in long-term rents. Specifically, if the eclipse induced landlords to shift from the long-term to short term market, then we would expect to see changes in long-term rents as these units are removed from the market. We explore this possibility using data on rents from Zillow Observed Rent Index (ZORI).[22] The rental data we use reflect the mean of listed rents that fall into the 35th to 65th percentile range for all homes (single family, multi-family and condo) and are reported at the county-month level. We find no evidence that the solar eclipse affected long-term rental prices in a reduced form event study (see Figure 11). This null effect is confirmed by our IV/2SLS model with the natural log of rents as the dependent variable (see Table 11). The existing evidence that Airbnb exposure increases rents in the long-term market is likely driven by the fact that these studies rely on policy events that also affect the WTP of prospective property buyers.

To summarize, our analysis suggests that the eclipse provided a one-time cash payment that was large enough to overcome fixed entry costs for a significant number of Gig hosts. Once these hosts entered the market, they were able to increase the cash flow of their property, which increased the minimum price at which they would be willing to sell their property. Therefore, the resulting price increase reflects higher WTA by Gigs due to a more efficient use of their existing housing stock rather than a shift of the housing stock from the long-term to the short-term market.

[21] The property tax homestead exemption is only available on a home owner's primary residence.

[22] Zillow defines ZORI as "a repeat-rent index that is weighted to the rental housing stock to ensure a representative sample across the entire market, not just those homes currently listed for-rent."

8 Conclusion

We estimate the effect of Airbnb expansion on housing prices using the 2017 solar eclipse as an instrument to overcome identification challenges. Our results indicate that the expansion of Airbnb induced by the solar eclipse caused housing prices to increase with elasticity ranging from 0.037 to 0.043. The estimates are statistically significant, robust to several alternative specifications, and represent about two-thirds of the effect size found in previous studies.

A key difference between our study and the existing literature is that our setting does not include a change in property rights or technology access that would affect the willingness to pay for properties. Instead, our setting reflects a one-time lottery for joining the Airbnb market supply that allows existing homeowners to produce income from excess housing capacity. We exploit this feature to show that the estimated effects reflect a more efficient use of the existing housing stock without displacing permanent residents.

This form of efficiency gives reasons to expect that partial bans on Airbnb (or other P2P platforms) will have minimal effects on housing prices. In a counterfactual landscape where policy permits all Airbnb listings, some housing stock will surely be devoted full time to listing on Airbnb. However, expansions in the supply of Airbnb through Gig listings likely meet the demand from short-term renters, and this will continue to increase housing prices through the willingness-to-accept mechanism. It therefore would probably be best to consider partial bans as a means to gentrify the Airbnb supply on the margins of Gig versus Investor type listings, rather than as a strategy for increasing the stock of affordable housing. Our results suggest that a partial ban that limits the conversion of properties to full-time use as short-term rentals will keep prices elevated because of the more efficient use of housing among home owners.

Lastly, our paper provides the literature a novel instrument for studying the effect of short-term rental listings on various outcomes. Airbnb has been used to study the effects on the hotel industry, tax evasion, residential investment, and many other choices described in section 2.2. Our discovery of the solar eclipse as a relevant first stage predictor of Airbnb market expansion could aid in additional causal research designs, provided the eclipse does not have another pathway to affecting those outcomes. Furthermore, the fact that the LATE is by expansion of Gig participation without resident displacement would be an important caveat to the inference in such research.

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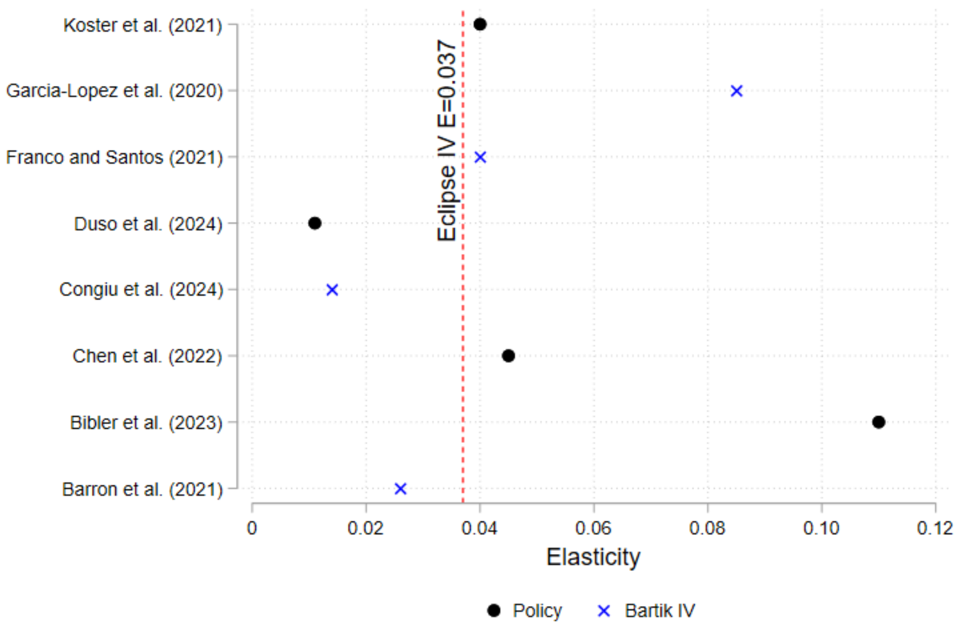
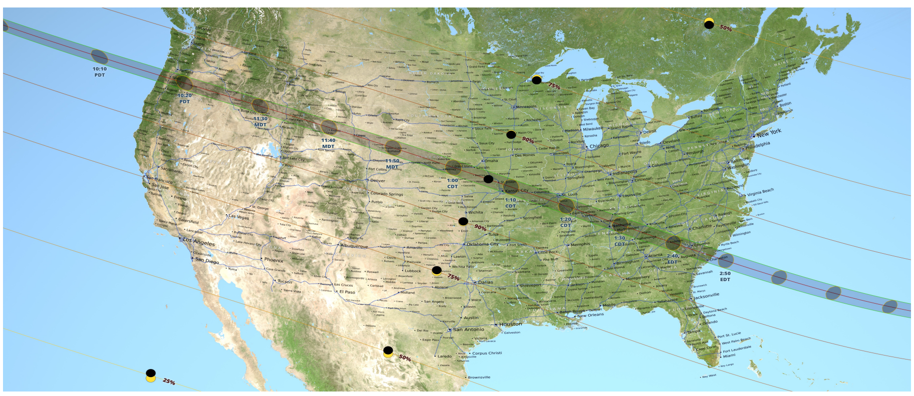
9 Figures

Figure 1: Estimates of Elasticity of Airbnb Listings on Housing Prices



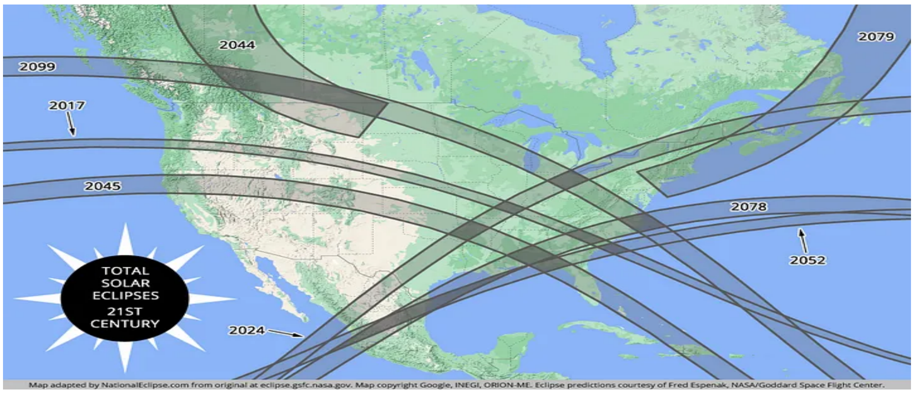
Notes: Markers distinguish between studies identifying from policy ban or Bartik shift-share instrument, as discussed in Section 2. Description of these point estimates are reported in Appendix A.

Figure 2: Path of Totality for 2017 Total Solar Eclipse



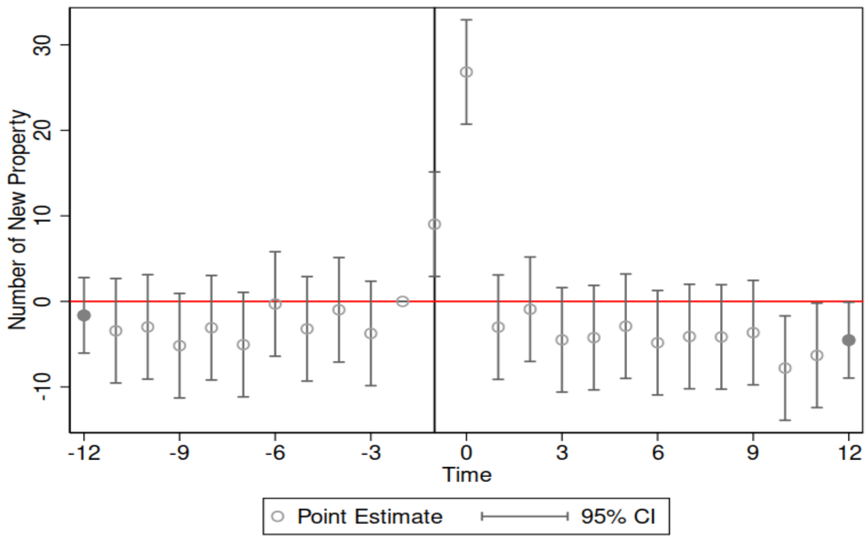
Source: The image was produced by Ernie Wright at NASA's Scientific Visualization Studio.

Figure 3: Paths of Totality for North American Total Solar Eclipses, 2017-2099



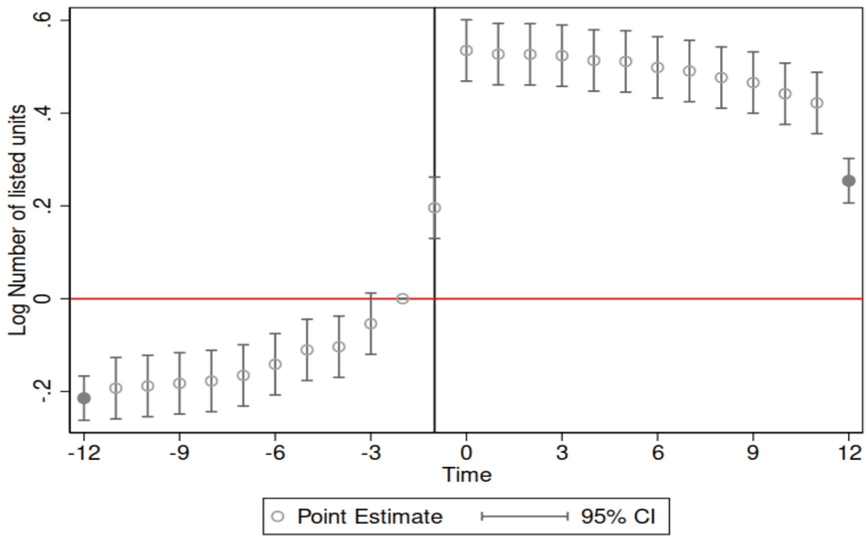
Source: The image was produced by Ernie Wright at NASA's Scientific Visualization Studio.

Figure 4: Number of New Airbnb Listings by Month



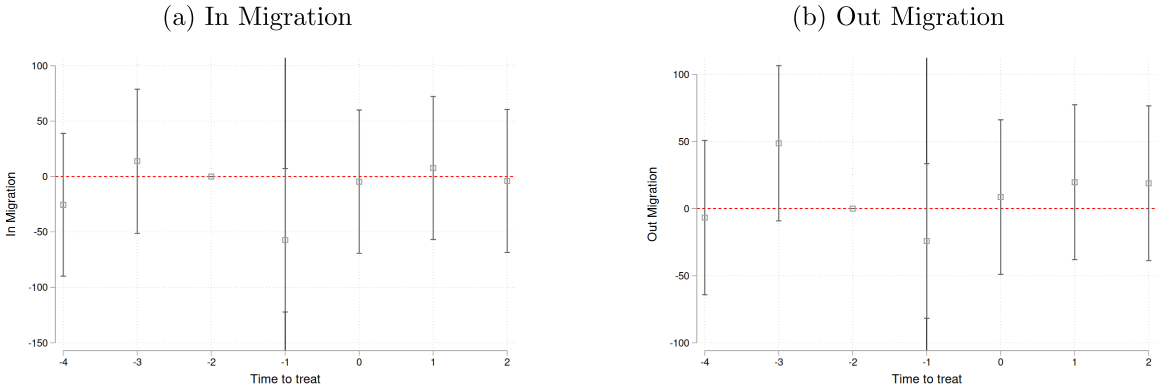
Notes: Reported are the coefficients of a panel event study comparing Airbnb activity in the path of totality to those outside the path of totality. The dependent variable is the the number of property listings appearing for the first time in the AirDNA data by county-month. $t = 0$ indexes the month of the eclipse (August, 2017).

Figure 5: Total Airbnb Listings by Months



Notes: Reported are the coefficients of a panel event study comparing Airbnb activity in the path of totality to those outside the path of totality. The dependent variable is the natural log of the total number of listings. The unit of observation for the analysis is $county - month$. $t = 0$ indexes the month of the eclipse (August, 2017)

Figure 6: Effect of Solar Eclipse on County Household Migration Flow

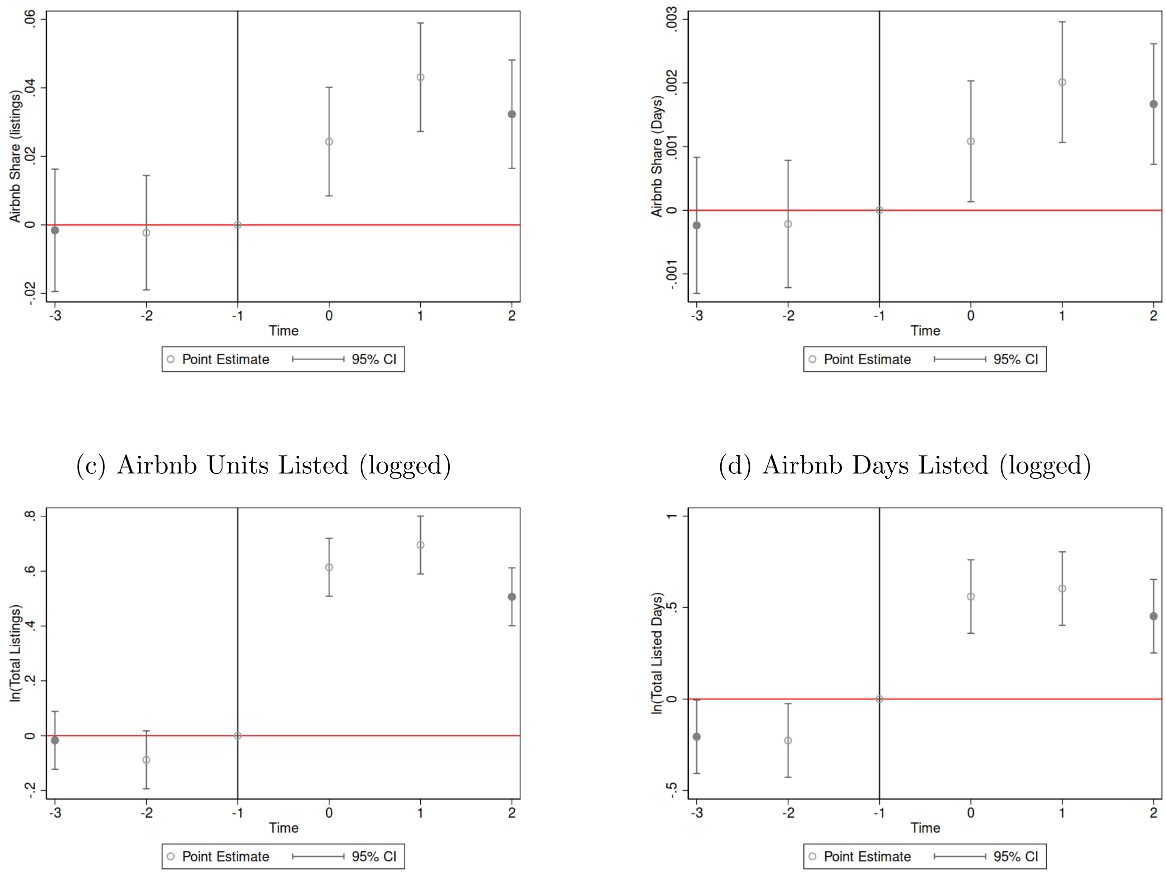


Notes: Reported are the coefficients of a panel event study comparing migration flows in treated and control counties before and after 2017 ($t = 0$). The data on migration flows are from the county-to-county migration data from the Internal Revenue Service Statistics (IRS) of Income Division. Household migration estimates are based on year-to-year address changes reported on the individual income tax returns filed with the IRS. We restrict to county-year observations between the 5th and 95th percentile.

Figure 7: Event Study of Eclipse on Airbnb Units and Days Listed

(a) Airbnb Units Listed

(b) Airbnb Days Listed

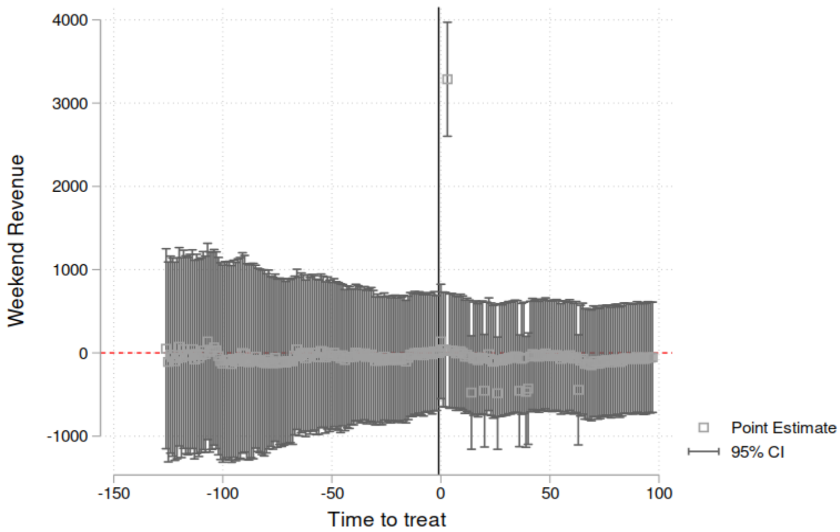


(c) Airbnb Units Listed (logged)

(d) Airbnb Days Listed (logged)

Notes: The figure plots coefficients from an event study of solar eclipse exposure on above listed outcomes in counties by year, with $t = 0$ indexing 2017.

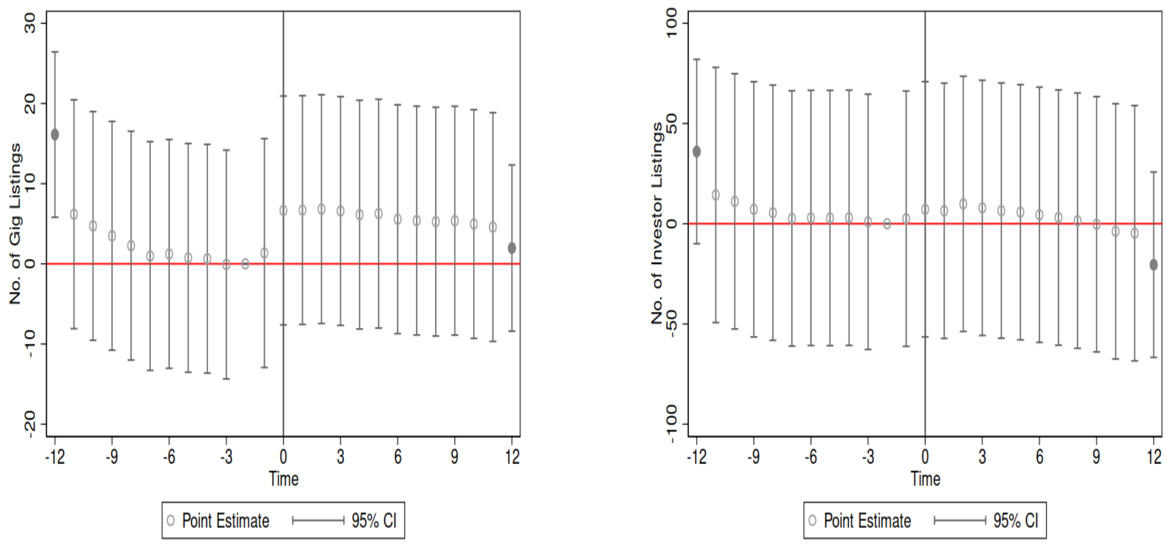
Figure 8: Event Study for Property Weekend Revenue Generated



Notes: Reported are the coefficients of a panel event study comparing 'weekend revenue' in treated and control counties before and after the weekend of August 21, 2017 (eclipse weekend, $t = 0$). We define weekend revenue as the total revenue earned over the four days Friday-Monday and we calculate this value for each weekend in each year. The unit of observation is the property and weekend, and includes property fixed effects.

Figure 9: Event Study of Effect of Solar Eclipse on Number of Airbnb Property Listings by Host Type

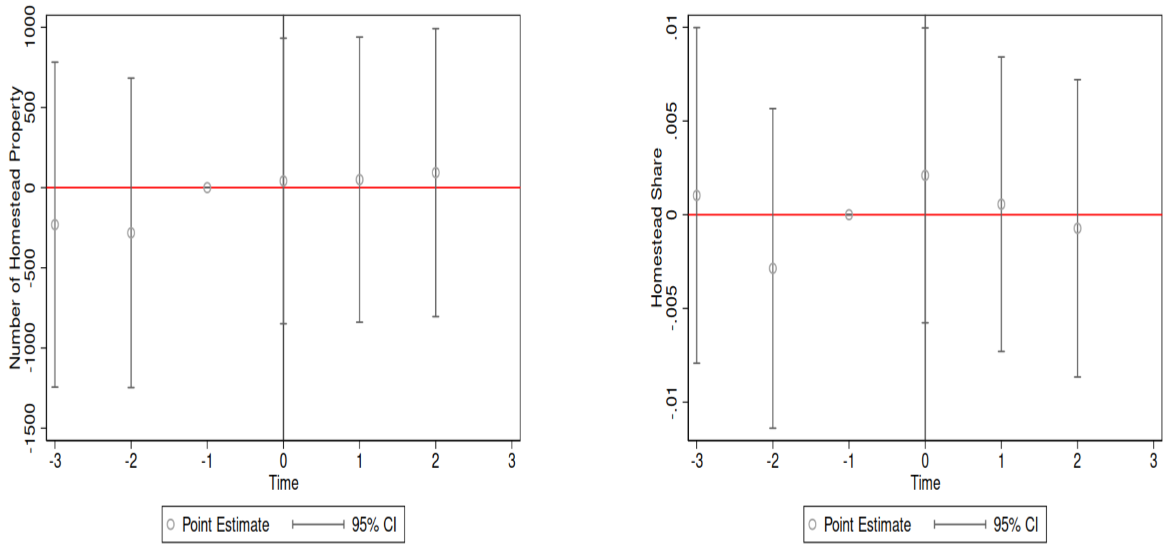
(a) Effect on Gig Listings (b) Effect on Investor Listings



Notes: Reported are the coefficients of a panel event study comparing number of listings in treated and control counties before and after August, 2017 ($t = 0$). Gig hosts are those hosts who have a single property that is shared with the guests. Investor hosts are those host with multiple properties that are listed in their entirety.

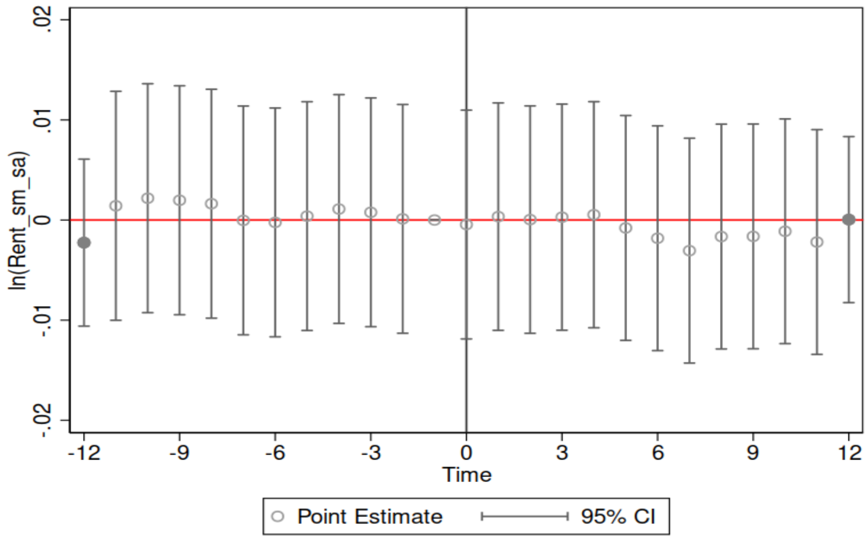
Figure 10: Event Study of Effect of Solar Eclipse on Homestead Exemptions

(a) Number of Homesteads (b) Homestead Share of Total Properties



Notes: Reported are the coefficients of a panel event study comparing homestead properties in treated and control counties before and after 2017 ($t = 0$). Unit of observation is county-year

Figure 11: Effect of Eclipse on Zillow Rents



Notes: Reported are the coefficients of a panel event study comparing rents in the longterm market in treated and control counties before and after August, 2017 ($t = 0$). The rent data are from Zillow's Observed Rent Index (ZORI) and reflects the "mean of listed rents that fall into the 35th to 65th percentile range for all homes and apartments in a given region".

10 Tables

Table 1: Descriptive Statistics by Treatment and Period Status

	2014-2016		2017-2019	
	Control	Treat	Control	Treat
Share of listings	0.001	0.001	0.004	0.005
	(0.004)	(0.003)	(0.011)	(0.011)
Share of days	0.018	0.016	0.070	0.097
	(0.067)	(0.045)	(0.186)	(0.185)
ln(number of listings)	4.492	4.148	6.107	6.221
	(2.191)	(1.947)	(2.121)	(1.845)
ln(number of days)	7.593	7.247	9.165	9.162
	(2.262)	(2.053)	(2.125)	(1.908)
eclipse	0.000	1.000	0.000	1.000
	(0.000)	(0.000)	(0.000)	(0.000)
HPI	287.262	243.213	307.115	263.226
	(180.781)	(129.604)	(209.076)	(153.782)
lnhp	5.522	5.384	5.573	5.453
	(0.497)	(0.449)	(0.519)	(0.457)
HousingUnits	60.043	32.233	53.199	27.897
	(152.261)	(53.314)	(144.125)	(50.236)
N	6082.000	681.000	7209.000	858.000

Notes: Reported are means with standard deviations in parentheses. All statistics are calculated using pre-treatment data 2014-2016 at the county-year level. Housing units reported in thousand of units. HPI is housing price index.

Table 2: Summary Statistics

	Mean	SD	Min	Max	N
Share of listings	0.003	0.009	0.000	0.208	14830
Share of days	0.048	0.147	0.000	3.493	14830
ln(number of listings)	5.361	2.284	0.693	13.751	14830
ln(number of days)	8.432	2.312	0.000	16.569	14830
eclipse	0.104	0.305	0.000	1.000	14830
HPI	293.499	192.515	71.840	2116.550	14830
lnhp	5.536	0.506	4.274	7.658	14830
HousingUnits	53579.4	141266.2	765	3579329	14830

Notes: All statistics are calculated at the county and year level. Housing units reported in thousand of units. HPI is housing price index.

Table 3: Effect of Airbnb Expansion on Logged Housing Prices

	Units	Days	ln(Units)	ln(Days)
	Panel A: TWFE			
Treatment Effect	1.847***	0.116***	-0.001	-0.003***
	(0.074)	(0.004)	(0.001)	(0.001)
	Panel B: IV			
Firststage	0.002***	0.034***	0.530***	0.457***
	(0.000)	(0.005)	(0.030)	(0.033)
Treatment Effect	11.272***	0.564***	0.037***	0.043***
	(2.223)	(0.097)	(0.006)	(0.007)
Obs.	14808	14808	14808	14808
Mean	0.001	0.018	4.458	7.559
Std. Dev	0.004	0.065	2.170	2.244
F(first)	27.650	39.837	316.025	193.677

Notes: Reported are the estimated effect of Airbnb expansion on housing price using county-year as the unit of observation. The dependent variable is the housing price index and Airbnb expansion is measured as 'Days', which is the number of days listed expressed as a share of the number of housing unit days, and 'Units', which is the number of units listed expressed as a share of the number of housing unit. We measure housing unit days as the number of housing units times the number of days in the year. 'TWFE' reports results from a two-way fixed effects model while 'IV' reports the results from the two-stage least squares model. The IV is an indicator variable that takes a value of 1 if any part of a county was in the path of totality and 0 otherwise. 'Mean' and 'Std. Dev' are pre-period statistics for the measures of Airbnb expansion, and 'F(first)' is the first stage F-statistics. Robust standard errors are reported: $*0.10 * *0.05 * * * 0.01$.

Table 4: CBSA x Quarter

	Units	Days	ln(Units)	ln(Days)
Treatment Effect	1.160***	8.244***	0.017***	0.021***
	(0.393)	(3.012)	(0.006)	(0.008)
Obs.	7654	7654	7654	7654
Mean	0.008	0.002	5.810	8.904
Std. Dev	0.014	0.003	1.981	1.962
F(first)	37.046	18.567	225.333	125.179

Notes: Reported are the estimated effect of Airbnb expansion on housing price using CBSA-quarter as the unit of observation. The dependent variable is the logged housing price index and Airbnb expansion is measured as 'Days', which is the number of days listed expressed as a share of the number of housing unit days, and 'Units', which is the number of units listed expressed as a share of the number of housing unit. We measure housing unit days as the number of housing units times the number of days in the year. 'TWFE' reports results from a two-way fixed effects model while 'IV' reports the results from the two-stage least squares model. The IV is an indicator variable that takes a value of 1 if any part of a county was in the path of totality and 0 otherwise. 'Mean' and 'Std. Dev' are pre-period statistics for the measures of Airbnb expansion, and 'F(first)' is the first stage F-statistics. Robust standard errors are reported: $*0.10 * *0.05 * * * 0.01$.

Table 5: Zipcode

	3-digit Zipcode		5-digit Zipcode	
	ln(Units)	ln(Days)	ln(Units)	ln(Days)
Treatment Effect	0.029***	0.035***	0.021***	0.024***
	(0.004)	(0.005)	(0.004)	(0.004)
Obs.	18402	18402	86422	86422
Mean	5.753	8.856	3.778	6.799
Std. Dev	1.960	1.937	1.715	1.847
F(first)	483.253	366.931	700.199	425.095

Notes: Reported are the estimated effect of Airbnb expansion on housing price. Units of observation are 3-digit zip code by quarter and 5-digit zip code by year. The dependent variable is the logged housing price index and Airbnb expansion is measured as the natural log of 'Days', which is the number of days listed, and natural log of 'Units', which is the number of units listed. 'TWFE' reports results from a two-way fixed effects model while 'IV' reports the results from the two-stage least squares model. The IV is an indicator variable that takes a value of 1 if any part of a county was in the path of totality and 0 otherwise. 'Mean' and 'Std. Dev' are pre-period statistics for the measures of Airbnb expansion, and 'F(first)' is the first stage F-statistics. Robust standard errors are reported: $*0.10 * *0.05 * * * 0.01$.

Table 6: Exclude Buffer Zone

	Units	Days	ln(Units)	ln(Days)
Treatment Effect	0.686***	13.999***	0.036***	0.040***
	(0.132)	(3.199)	(0.005)	(0.006)
Obs.	9263	9263	9263	9263
Mean	0.020	0.001	4.704	7.809
Std. Dev	0.061	0.004	2.230	2.289
F(first)	28.711	18.988	351.285	230.086

Notes: Reported are the estimated effect of airbnb expansion on housing price using county-year as the unit of observation. The dependent variable is the housing price index and Airbnb expansion is measured in 'Days' and 'Units'. 'Share' indicates that our Airbnb measures are expressed relative to the number of housing unit days or the number of housing units. 'log' indicates that we used the natural log of our Airbnb measures. We measure housing unit days as the number of housing units times the number of days in the year. The IV is an indicator variable that takes a value of 1 if any part of a county was in the path of totality and 0 otherwise. The analysis excludes counties in a buffer zone on either side of the eclipse. 'Mean' and 'Std. Dev' are pre-period statistics for the measures of Airbnb expansion, and 'F(first)' is the first stage F-statistics. Robust standard errors are reported: $*0.10 * *0.05 * * * 0.01$.

Table 7: Check Distance to Centroid

	Units		Days	
	Squared	Cubed	Squared	Cubed
	Panel A: Levels			
Treatment Effect	0.469***	0.442***	14.586***	15.176***
	(0.141)	(0.136)	(5.234)	(5.460)
Mean	0.018	0.018	0.001	0.001
Std. Dev	0.065	0.065	0.004	0.004
F(first)	7.337	5.088	4.525	3.137
	Panel B: ln of Levels			
Treatment Effect	0.020***	0.021***	0.025***	0.027***
	(0.006)	(0.006)	(0.008)	(0.008)
Obs.	14808	14808	14808	14808
Mean	4.458	4.458	7.559	7.559
Std. Dev	2.170	2.170	2.244	2.244
F(first)	63.154	143.519	41.477	103.928

Notes: Reported are the estimated effect of airbnb expansion on housing price using county-year as the unit of observation. The dependent variable is the housing price index and Airbnb expansion is measured in 'Days' and 'Units'. Panel A reports results when Airbnb activity is measured in 'Share', which indicates that our Airbnb measures are expressed relative to the number of housing unit days or the number of housing units. We measure housing unit days as the number of housing units times the number of days in the year. Panel B reports results when Airbnb activity is measured in 'log', which indicates that we used the natural log of our Airbnb measures. The IV is the inverse of the distance between the line of totality and the centroid of the county. The column 'Quadratic' reports results when a quadratic specification of the IV is used, and 'Cubic' reports results when a cubic specification of the IV is used. 'Mean' and 'Std. Dev' are pre-period statistics for the measures of Airbnb expansion, and 'F(first)' is the first stage F-statistics. Robust standard errors are reported: $*0.10 * *0.05 * * * 0.01$.

Table 8: Balanced sample

	Units	Days	ln(Units)	ln(Days)
Treatment Effect	0.488***	9.341***	0.037***	0.042***
	(0.092)	(1.940)	(0.006)	(0.007)
Obs.	11539	11539	11539	11539
Mean	0.021	0.001	4.750	7.858
Std. Dev	0.070	0.004	2.178	2.237
F(first)	34.233	25.360	300.766	187.932

Notes: Reported are the estimated effect of airbnb expansion on housing price using county-year as the unit of observation and a fully-balanced panel. The dependent variable is the housing price index and Airbnb expansion is measured in 'Days' and 'Units'. Panel A reports results when Airbnb activity is measured in 'Share', which indicates that our Airbnb measures are expressed relative to the number of housing unit days or the number of housing units. We measure housing unit days as the number of housing units times the number of days in the year. Panel B reports results when Airbnb activity is measured in 'log', which indicates that we used the natural log of our Airbnb measures. 'TWFE' reports results from a two-way fixed effects model while 'IV' reports the results from the two-stage least squares model. The IV is an indicator variable that takes a value of 1 if any part of a county was in the path of totality and 0 otherwise. 'Mean' and 'Std. Dev' are pre-period statistics for the measures of Airbnb expansion, and 'F(first)' is the first stage F-statistics. Robust standard errors are reported: $*0.10 * *0.05 * * * 0.01$.

Table 9: Land use restrictions

	ln(Units)		ln(Days)	
	High	Low	High	Low
	Panel A: Zone Rank			
Treatment Effect	0.078***	0.018***	0.104***	0.020***
	(0.012)	(0.006)	(0.018)	(0.006)
Obs.	3309	11475	3309	11475
Mean	5.041	4.274	8.129	7.379
Std. Dev	2.112	2.151	2.171	2.233
F(first)	119.350	200.157	86.797	119.297
	Panel B: WRLURI18			
Treatment Effect	0.040***	0.035***	0.045***	0.045***
	(0.012)	(0.011)	(0.014)	(0.015)
Obs.	2991	2864	2991	2864
Mean	5.770	4.698	8.879	7.809
Std. Dev	2.355	2.069	2.358	2.127
F(first)	94.781	65.786	67.359	40.266

Notes: Reported are the estimated effect of airbnb expansion on housing price using county-year as the unit of observation. Panel A reports results when we split the sample by the restrictiveness of land use policy as determined by the Zone Rank index. A county receives a 'High' Zone rank if it is located in a state that is ranked 1 to 29, and a 'Low' zone rank otherwise. Panel B reports results for the WRLURI2018 index. A county receives a 'High' WRLURI2018 rank if it is in the top two quartiles of the WRLURI2018 distribution, and a 'Low' WRLURI2018 rank otherwise. The dependent variable is the housing price index and Airbnb expansion is measured as the natural log of 'Days', which is the number of days listed, and natural log of 'Units', which is the number of units listed. The IV is an indicator variable that takes a value of 1 if any part of a county was in the path of totality and 0 otherwise. 'Mean' and 'Std. Dev' are pre-period statistics for the measures of Airbnb expansion, and 'F(first)' is the first stage F-statistics. Robust standard errors are reported: $*0.10 * *0.05 * * * 0.01$.

Table 10: Effect by County Median HH income

	ln(Units)		ln(Days)	
	High	Low	High	Low
Treatment Effect	0.081***	0.018	0.104***	0.022
	(0.019)	(0.013)	(0.027)	(0.017)
Obs.	2426	2423	2426	2423
Mean	6.319	5.408	9.413	8.536
Std. Dev	2.190	2.080	2.181	2.078
F(first)	62.558	66.596	47.132	44.439

Notes: Reported are the estimated effect of Airbnb expansion on housing price using county-year as the unit of observation. The dependent variable is the housing price index and Airbnb expansion is measured as the natural log of 'Days', which is the number of days listed and 'Units', which is the number of units listed. We classify counties as high or low based on the average of their pre-eclipse household income. 'High' shows results among counties in the top 2 quartiles of the pre-eclipse household income distribution. 'Low' shows results among counties in the bottom 2 quartiles of the pre-eclipse household income distribution. Data on household incoem is only available for 800+ counties and 62 of these counties are treated. All estiamtes are from a 2SLS where the IV is an indicator variable that takes a value of 1 if any part of a county was in the path of totality and 0 otherwise. 'Mean' and 'Std. Dev' are pre-period statistics for the measures of Airbnb expansion, and 'F(first)' is the first stage F-statistics. Robust standard errors are reported: $*0.10 * *0.05 * * * 0.01$.

Table 11: Effect of AirBNB on Zillow rents

	Smoothed		Seasonality	
	ln(Units)	ln(Days)	ln(Units)	ln(Days)
Treatment Effect	-0.002	-0.003	0.004	0.005
	(0.004)	(0.005)	(0.004)	(0.005)
Obs.	29820	29820	29820	29820
Mean	5.698	8.703	5.698	8.703
Std. Dev	1.739	1.707	1.739	1.707
F(first)	205.519	160.061	205.519	160.061

Notes: Reported are the estimated effect of Airbnb expansion on rents using county-month as the unit of observation. The dependent variable is the Zillow Observed Rent Index (ZORI) that measures the "mean of listed rents that fall into the 35th to 65th percentile range for all homes and apartments in a given region". Airbnb expansion is measured as 'Days', which is the number of days listed and 'Units', which is the number of units listed. 'Smoothed' indicates the rent index has been smoothed using a three-month simple moving average. 'Seasonality' indicates the index has been smoothed and seasonally adjusted using Zillow's proprietary algorithm. All estimates are from a 2SLS model where the IV is an indicator variable that takes a value of 1 if any part of a county was in the path of totality and 0 otherwise. 'Mean' and 'Std. Dev' are pre-period statistics for the measures of Airbnb expansion, and 'F(first)' is the first stage F-statistics. Robust standard errors are reported: $*0.10 * *0.05 * * * 0.01$.

A Calculation of Elasticities from Literature

Figure 1 reports elasticities derived from the previous literature employing causal identification strategies, which are discussed in the literature review section 2. We made every effort to use the original authors own elasticities that were comparable to those of this paper, and favored calculations that required as few of our inferential calculations as possible.

- Garcia-Lopez et al. (2020): Provides coefficient estimates of $dln(price)/d(Listings \times 100) = 0.110$ in columns 1 and 2 of Table 3, so the approximate increase in housing price of a 100 unit increase is 11.6 percent. The regression is at the neighborhood (BSA) level, of which there are 233. Table 1 gives the total Airbnb counts in 2015 for Barcelona as 16,951, so an approximate average neighborhood level is 72.5. Since a 100 unit increase is 137 percent of 72.5 units, the elasticity is $11.67/137 = 0.085$.

- Barron et al. (2021): On page 25 reports that they find a a one percent increase in Airbnb listings leads to a 0.026 percent increase in housing prices for zip codes at the median owner-occupancy rate.

- Franco et al. (2021): On page 10, discussing the results from column (3) of Table 4, reports that a 1 percentage point increase in parish Airbnb share results in a

3.74 percent price increase. Based on table A1, the number of Airbnb properties is 812 and the total number of dwellings is 72,277 at the mean. This implies that a 1% increase in number of Airbnb properties is 8.12, which implies that the share of Airbnb properties increases by 0.0001123. Therefore a 1% increase in Airbnb properties results in a 0.04% ($= exp\{0.0001123 \times 3.74\} - 1$) increase in housing price; i.e., the estimate elasticity is 0.04.

- Koster et al. (2021): On page 2 summarizes their main finding as the policy restricting reducing Airbnb listings of properties and rooms by about 50 percent in the long run, and that it reduced house prices and rents by about 2 percent on average. This implies a elasticity of $-0.02/-0.5 = 0.04$. Similarly, their Table 8 reports the results of using the policy as an instrument on Airbnb listings to estimate the effect on housing prices, and the main 2SLS results (columns 1 and 2) are 0.05 and 0.03.

- Chen et al. (2022): On page 8591 reports that their "estimates suggest that the policy may further decrease home values and rents by about 0.03%-0.06% if the density is 1% higher in a zip code." On the figure we list the midpoint of that range, 0.045.

- Bibler et al. (2023): On page 20, they find the policy effect overall is to reduce Airbnb listings by 0.06 percentage points, a 21 percent reduction in the pre-period supply. In column 1 of Table 5, they find the same policy to have increased the log of housing prices by 0.024. We calculate the elasticity then as $(exp\{0.024\}-1)/21 = 0.11$.

- Congiu et al. (2024): In Table 4-column 3 reports a 0.63 increase in the log of housing prices per square meter (approximately 87.8 percent) in neighborhoods that were induced by the Bartik instrument to increase Airbnb density by 1 unit in the previous period. The mean density is reported in Table 3 to be 0.016, so one unit increase is 62.5 times the increase, and the elasticity is $87.8/6250 = 0.014$.

- Duso et al. (2024): Reports the effect of one additional Airbnb listing in an area on on long term apartment rent per square meter in Table 5. For the 2018 policy, they find this local average treatment effect to be 0.083 for listing of entire homes. Using the formula $\varepsilon = \frac{\partial price}{\partial unit}\frac{units}{price}$ and the means of the descriptive statistics from their Table 1, this calculation is $\varepsilon = 0.083\frac{1.27}{9.2} = 0.011$.

- Not enough information to calculate: Gauss et al. (2024) and Seiler et al. (2024)

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