Airbnb and Rental Markets: Evidence from Berlin

Tomaso Duso, Claus Michelsen, Maximilian Schäfer and Kevin Ducbao Tran

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School of Economics

University of Bristol
Priory Road Complex
Bristol
BS8 1TU
United Kingdom

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Tomaso Duso, Claus Michelsen, Maximilian Schäfer, and Kevin Ducbao Tran[1]

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Abstract

We exploit two policy interventions in Berlin, Germany, to causally identify the impact of Airbnb on rental markets. While the first intervention significantly reduced the number of high-availability Airbnb listings bookable for most of the year, the second intervention led to the exit of mostly occasional, low-availability listings. We find that the reduction in Airbnb supply has a much larger impact on rents and long-term rental supply for the first reform. This is consistent with more professional Airbnb hosts substituting back to the long-term rental market. Accordingly, we estimate that one additional nearby high-availability Airbnb listing crowds out 0.6 long-term rentals and, consequently, increases the asked square-meter rent by 1.8 percent on average. This marginal effect tends to be smaller in districts with a higher Airbnb density. However, these district experienced a larger slowdown in rent increases following the reform due to larger reductions in Airbnb supply.

Keywords: rents, housing market, short-term rental regulation, sharing economy, Airbnb

JEL codes: R21, R31, R52, Z30

[1]Duso: German Institute for Economic Research (DIW Berlin), Technische Universität Berlin, CEPR, and CESifo. Email: tduso@diw.de. Michelsen: DIW Berlin and Universität Potsdam. Email: cmichelsen@diw.de. Schäfer: University of Bologna. E-mail: maximilian.schaefer@unibo.it. Tran: University of Bristol. E-mail: kevin.tran@bristol.ac.uk.
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1 Introduction

Home-sharing through peer-to-peer platforms such as Airbnb has provided an efficient way to match demand and supply for short-term rentals. Thus, it has become a ubiquitous and successful tool to reduce capacity under-utilization in housing markets. This is the case when owner-occupiers occasionally share their entire house or part of it for limited periods, creating benefits for themselves as well as those demanding short-term rentals. However, the expansion of peer-to-peer short-term rentals can also generate externalities on local housing markets: When landlords find it more profitable to offer their homes on the short-term market instead of supplying them as long-term rentals, supply shortages may be the consequence. This latter concern has led cities worldwide to regulate online short-term rental platforms. While some empirical studies establish that Airbnb increases rents and house prices, there is less empirical evidence on the underlying mechanism driving this result. Yet, a better understanding of this mechanism is key to guide policy-making in the design of effective regulatory tools.

In this paper, we study the impact of Airbnb on rents as well as the crowding out of long-term rental supply. To this end, we analyze the effects of two distinct Airbnb supply shocks caused by the introduction of short-term rental regulation in Berlin, Germany. In May 2016, a law came into force that prohibited the "misuse" of real estate property for short-term renting in Berlin. In August 2018, an amendment of this law took effect that, among other things, requires hosts on short-term rental platforms, such as Airbnb, to publish a registration number, obtainable only from the local council. Both the original law and its amendment led to plausibly exogenous reductions in Airbnb supply. However, the structure of these reductions was rather different across the two reforms. Exploiting the combination of these two elements – the exogeneity and heterogeneity of the variation in Airbnb supply – is the core of our identification strategy that helps us causally quantify how Airbnb affects long-term rental markets and identify the mechanism behind these effects. This joint analysis of effect and mechanism is our main contribution.

We put together a novel and rich dataset containing monthly information on asked rents, rental characteristics, and Airbnb penetration in Berlin. Importantly, we observe the exact location of Airbnb listings and rentals. One novelty of our data set, in comparison to the existing literature, is that it allows us to track the monthly evolution of both rental as well as Airbnb supply. In our empirical analysis, we first document the distinct effects both reforms had on Airbnb supply. While they both significantly reduced the number of Airbnb offers, only the first successfully reduced the number of high-availability listings, which we define as listings available for short-term renting for more than 180 days in a year. By contrast, the second reform mainly led to a decrease of low-availability listings, which are likely offered for rent on Airbnb only occasionally. This finding indicates that the first intervention was more successful at reducing professional short-term renting. These results, which we consider to be auxiliary to our main research goal, are interesting findings *per se*, since they allow us to assess the effectiveness of different regulatory tools.

Using the implementation of the reforms as instrumental variables for the Airbnb supply, we analyze Airbnb's impact on asked rents. This approach allows us to estimate the elasticity of rents to Airbnb listings and go beyond the mere estimation of the impact of the reforms on rents. For the first reform, we find that one additional home on Airbnb within 250 meters of a rental leads to a seven cents per square meter increase of the asked rent on average. For the second reform, the corresponding marginal effect is only equal to three cents. We argue that the smaller marginal effect found using the second reform is due to the reform's negligible effect on high-availability Airbnb listings. When focusing on high-availability listings only, we find a larger effect: one additional high-availability home on Airbnb within 250 meters of a rental increases asked rents by 16.5 cents per square meter – about 1.8 % of the average rent. Because the second reform did not significantly affect the supply of high-availability Airbnb listings, we can only identify this effect using the first one.

These results suggest that ignoring the type of listings when assessing the effect of Airbnb supply on the rental market likely masks important heterogeneity. The overall effect of Airbnb on rents appears to be mainly driven by high-availability listings that are more likely to return to the rental market when Airbnb is regulated. Indeed, we provide direct evidence for high-availability listings substituting back to the long-term rental market after the first reform. Using a similar approach as for the analysis of rents, we find that higher Airbnb density results in lower long-term rental density. Again, this effect is larger when focusing on high-availability Airbnb listings. Specifically, each additional high-availability entire home offered on Airbnb within the same block leads, on average, to a 0.6 unit reduction in the supply of long-term rentals. Given our estimates, we cannot reject the hypothesis that each additional high-availability entire home on Airbnb reduces long term rental supply at a ratio of one to one. Similar to the rent analysis, we can only clearly document these effects using the first reform.

In an extension, we further analyze geographic heterogeneity of the effect on rents by stratifying the sample by city districts. Our results suggest substantial effect heterogeneity with effect sizes of up to 79 cents per square meter for each additional nearby high-availability Airbnb. We find evidence that the marginal effect of nearby Airbnb listings is larger in district with a lower Airbnb density. However, due to the larger reduction in Airbnb listings, our results imply that districts with a higher Airbnb density experienced a larger slowdown in rent increases due to the policy.

Our paper contributes to a small, but growing, literature on the effects of home-sharing through peer-to-peer platforms such as Airbnb on the housing market. Common approaches to address the inherent endogeneity driven by omitted variables or potential reverse causality include controlling for a large set of observables (Horn and Merante, 2017), using shift-share-like instruments for Airbnb popularity (Barron et al., 2021; Garcia-López et al., 2020), and using policy changes as natural experiments (Koster et al., 2018; Peralta et al., 2020). We contribute new evidence relying on plausibly exogenous variation in Airbnb supply and emphasize heterogeneous effects caused by different types of Airbnb listings.

With respect to the underlying mechanism, Horn and Merante (2017) find that Airbnb supply negatively correlates with rental supply in Boston. Based on a panel data approach, Garcia-López et al. (2020) find that Airbnb supply negatively correlates with the number of owner and rental households. Barron et al. (2021) use shift-share instrumental variables to establish a negative causal link between Airbnb and rental supply. Shabrina et al. (2021) estimate that up to two percent of all properties in

London are misused on Airbnb as holiday rentals and that a higher density of misused properties positively correlates with rents. Compared to this literature, the contribution of our paper is twofold. First, we observe two different policy changes that have heterogeneous effects on Airbnb supply. Second, using this plausibly exogenous variation, we can directly test how Airbnb supply is related to rental supply. Combined, these aspects of our setting allow us to provide novel insights into the mechanism through which Airbnb affects rents.

Moreover, our analysis allows us to directly control for rental-specific characteristics when analyzing rents. We measure Airbnb exposure of individual rentals by counting the number of Airbnb listings within 250 meters. Additionally, we control for a rich set of neighborhood characteristics such as air and noise pollution, or the number of supermarkets and schools. To systematically select from the rich set of covariates, we use Lasso-based regression methods (Belloni et al., 2014; Chernozhukov et al., 2015) for model selection.

The remainder of the paper proceeds as follows. In Section 2, we present details regarding the regulation of Airbnb in Berlin. Further, we describe the different data sets used in the analysis. In Section 3, we provide a descriptive analysis motivating some of the choices we make in the econometric modeling. In Section 4, we discuss our identification strategy. Our main results are presented in Section 5. In Section 6, we examine the geographical heterogeneity of Airbnb's impact on rents. Section 7 concludes.

2 Institutional Background and Data

2.1 The Berlin housing market

Since reunification, the Berlin housing market experienced pronounced boom and bust cycles. In the 1990s, investors speculated overly optimistically on a rapid increase of Berlin's economic and political importance. House prices rose strongly, ending up in a bursting housing price bubble before the turn of the millenium (Holtemöller and Schulz, 2010). The boom period was followed by ten years of stagnation: in the 2000s rents and house prices in Berlin were moderate, opening opportunities for entrepreneurs, a vivid cultural scene, and young people to shape urban live in Germany's capital.

This combination is one of the reasons why Berlin is increasingly becoming a magnet for city travelers, trade fairs, and events. This is reflected in skyrocketing accommodation figures: In 1996, there were still around 7.5 million overnight stays in Berlin's hotels. Ten years later, there were already 15.9 million, and in 2019 34.1 million. Since 2010, the number of beds in accommodation establishments has risen from around 100,000 to around 150,000 in 2019.

At the same time, Berlin has gained considerably in population. Since reunification, the population has risen by a total of ten percent, from 3.4 million in 1990 to just under 3.8 million people in 2020. The number of inhabitants in the immediate vicinity of Berlin has also increased: for example, the population of nearby Potsdam grew from 140,000 to around 180,000 over the same period. Since 2010, this development is also reflected on the housing market, where strong increases in both rental and purchase prices of residential properties have led to a vivid debate about housing market regulation, misappropriation law, and expropriation of large housing companies.

2.2 Institutional Background

Because of the dramatic increase in rents levels, in May 2014 the Berlin Senate passed a law (Zweckentfremdungsverbot-Gesetz, henceforth ZwVbG or just "the law") to ban the "misuse" of apartments, i.e. the use of real estate property for purposes other than housing. For the initial two years after taking effect, a transition period applied during which short-term rentals that were already active before May 2014 were still permitted. The law effectively prohibited short-term rentals without explicit permission from the city council at the end of this transition period, starting May 1, 2016. While the law remained vague on the exact definition of "misuse," our data show a clear effect on Airbnb supply around May 2016.

On August 1, 2018, an amendment of the law took effect. The law defined more clearly requirements for the permission to offer short-term renting to be granted.[2] An-

[2]In particular, it allows landlords to rent out property short-term, as long as the property is their

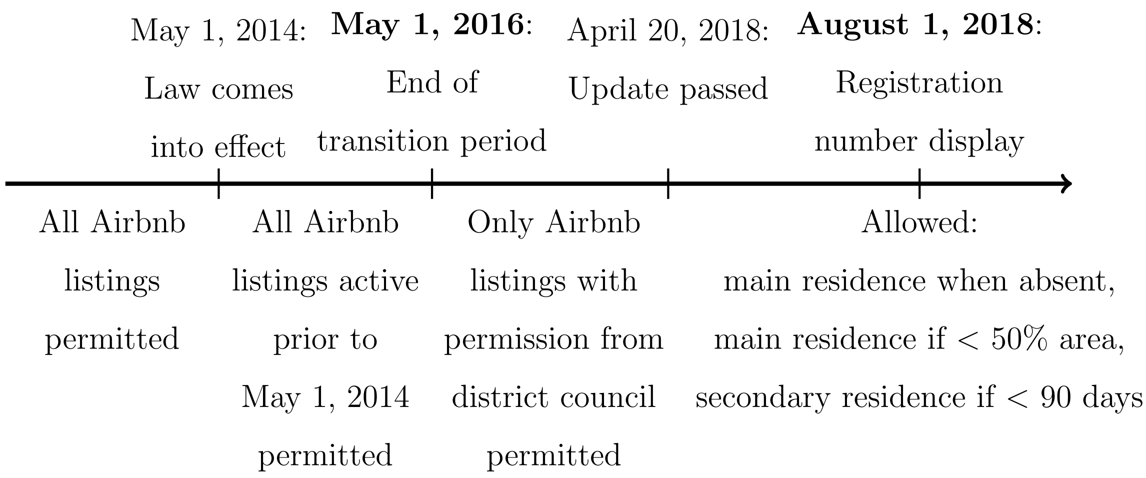


Figure 1: Stages of the Zweckentfremdungsverbot-Gesetz (ZwVbG). Dates in bold denote the policy dates used in our analysis.

other notable change was the requirement for hosts to display a registration number when renting on platforms such as Airbnb. Such a registration number can only be obtained through an application with the local council. Figure 1 summarizes the different stages of the law.

For our main analysis, we focus on seven-month windows around the date in which the initial law and its amendment took effect. We refer to the first intervention as the "May 2016 reform" and to the second intervention as the 'August 2018 reform."

2.3 Data

To measure Airbnb supply in Berlin over time, we use publicly available data from the website InsideAirbnb.com. The website provides monthly snapshots of web scraped data of Airbnb listings for various cities in the world. Data for Berlin are available in monthly intervals from May 2014 to May 2020, however with several gaps in-between. We observe three different categories of listings over time: entire homes/apartments,

main residence and short term rental is only occasional. Furthermore, it allows tenants to permanently rent out parts of their apartments, provided these parts make up less than 50 percent of the living space. Secondary residences now qualify for a permit if they are used as short-term accommodation for no more than 90 days per year.

private rooms, and shared rooms. For the analysis, we focus on the supply of entire homes/apartments. These listings are most likely to be rented as long-term rentals absent Airbnb. Furthermore, the law mostly targets short-term renting of entire apartments. Consistently, our results show that this type of Airbnb supply was most affected by the reforms.

Data on asked rents were provided by the economic consultancy Empirica.[3] Data from the same provider are used in previous studies analyzing the impact of rent control policies in Berlin (Mense et al., 2017, 2019). The data include web scraped information on apartments listed in Berlin mainly offered on three large online market-places: *Immonet*, *Immowelt*, and *Immobilienscout24*. The data span the period from January 2013 until July 2019.[4] The data include asked rents as well as various apartment characteristics, such as its size and the number of rooms. Objections against using asked prices and rents from internet ads are that they may deviate from the final, or transaction prices and that they might only cover parts of the relevant market. However, such data is frequently used as a valid substitute for real transaction information in many studies. The existing evaluations of this data shows that advertised prices do not substantially deviate from transaction prices in Germany, particularly for urban locations and during market expansions. Information on the exact market coverage of online ads is not available. However, it is shown that the data adequately matches price dynamics of the entire market (Faller et al., 2009; Henger and Voigtländer, 2014).

To account for the heterogeneity in the attractiveness of different neighborhoods, we use data from OpenStreetMap. The data include information about various points-of-

[3]See `https://www.empirica-institut.de/en/company-profile/` (last accessed: March 19, 2021).

[4]Because we want to use detailed geographical information in our main analysis, we restrict our sample to those rentals for which exact address information is available. This is the case for approximately 80 percent of the data set. Because the data do not include coordinates, we use the address information to geocode the location of each rental. This could be done without any issues for approximately 95 percent of the rentals for which we have the full address information. We exclude the rest. This leaves us with approximately 76 percent of the original observations (212,831 observations for the full sample).

interest such as restaurants, bars, and supermarkets.[5] The data we use are a snapshot as of February 2018 and, thus, offer only cross-sectional variation. Further, we use data provided by the city of Berlin including geographically disaggregated information such as the amount of noise at night or the level of particulate matter in the air.[6] Again, these data are only of cross-sectional nature.

To calculate the extent to which each of the rental apartment is exposed to Airbnb presence, we count all entire homes listed on Airbnb within 250 meters.[7] Figure 2 illustrates the logic of our calculation. In this specific example, the rental is exposed to three Airbnb listings.

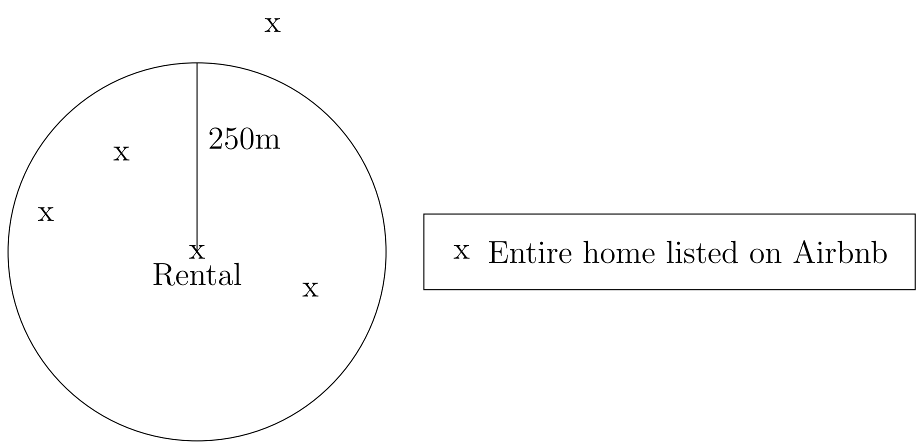


Figure 2: Illustration of our measure for Airbnb exposure. In this example, the rental would be assigned to have three Airbnb listings nearby.

Data Aggregation

To analyze the impact of Airbnb on rents, we use data at the rental level. Each observation corresponds to one rental apartment in the month in which it was first listed.

[5]The data are available at `https://download.geofabrik.de/europe/germany/berlin.html` (last accessed: March 19, 2021).

[6]The data are available at `https://fbinter.stadt-berlin.de/fb/index.jsp` (last accessed: March 19, 2021).

[7]The choice of a radius of 250m is ad-hoc. We discuss the choice of the circle size and related robustness checks in Section 5.3.

This disaggregated perspective allows us to use apartment-level characteristics in addition to neighborhood characteristics to account for potential observable determinants of rents.

To analyze the impact of Airbnb on rental supply, we instead aggregate the rental-level data to building blocks.[8] This allows us to count the number of rental apartments that are listed for rent in each block each month. With this aggregation, we obtain a panel data set at the block level. We decide to use blocks because it is the administrative unit of observation which comes closest to the level of geographic desegregation implied by the circles with 250 meter radius used in the rents analysis.[9] Because the block sizes vary, we normalize the number of Airbnb listings and rentals by the respective block area (expressed in square kilometers) for the analysis.

3 Descriptives

In this section, we document the heterogeneous impact of the implementation of the law in 2016 and its amendment in 2018 on Airbnb supply. We also relate the observed variation in Airbnb supply to the year-over-year increase in the housing stock in Berlin to give a sense of the overall importance of Airbnb for the housing market. Additionally, we present stylized facts on rental characteristics and evolution of rent prices over time to motivate and qualify the findings from the main analysis.

[8]The "building block" is a typical unit in Berlin's urban structure. The development of the block with one or more courtyards emerged between the end of the 19th and the beginning of the 20th century. "The building activity was regulated by development plans and building codes, in which street limit lines, the size of the blocks, the minimum size of courtyards and the floor spaces of buildings were stipulated." (`https://www.stadtentwicklung.berlin.de/umwelt/umweltatlas/edc607_01.htm`).

[9]While the circles have an area of approximately 0.2 square kilometers, an average block has an area of approximately 0.03 square kilometers.

3.1 Heterogeneous Impact of Reforms on Airbnb

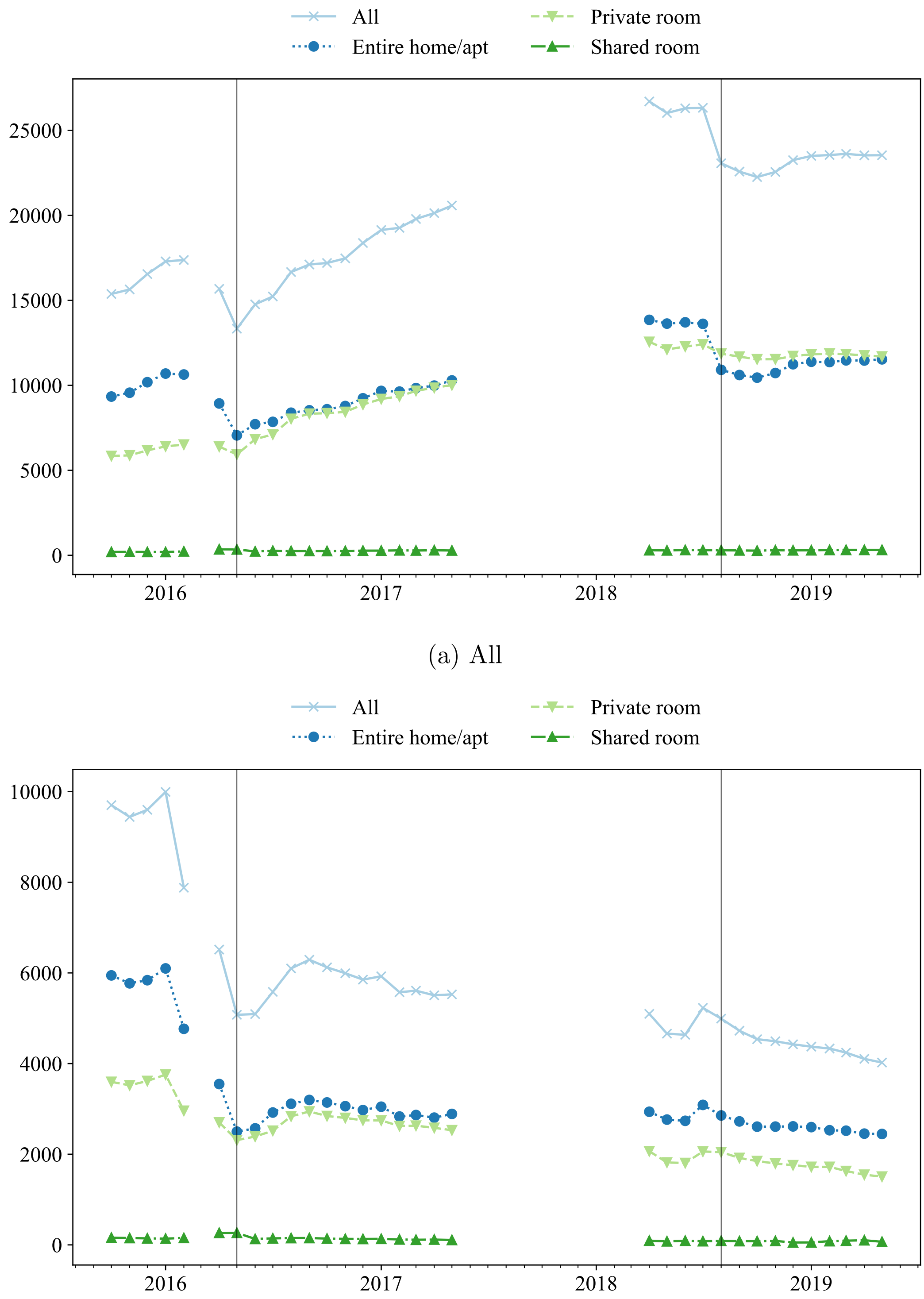
Figure 3a shows the evolution of the number of Airbnb listings in different categories for the available monthly snapshots from 2015 to 2019.[10] We observe a clear transitory reduction around the two policy interventions in May 2016 and August 2018. Both drops are mostly driven by decreases in the number of entire homes, which motivates our focus on this listing category for the remaining analysis.

Figure 3b shows the evolution of the number of high-availability Airbnb listings (i.e. listings available for booking more than 180 days a year). The 2016 and 2018 reforms appear to have very different effects on high-availability listings. While the number of high-availability listings decreased substantially in 2016, the August 2018 reform seems to have no noticeable impact on the number of high-availability listings.

The observation that the May 2016 reform had a large impact on the supply of high-availability Airbnb listings while the August 2018 reform did not is relevant for our analysis. The main mechanism through which Airbnb is hypothesized to affect rents is that landlords decide to list apartments on Airbnb rather than renting them out long-term (e.g. Yrigoy, 2019). If this is the case, we would expect that high-availability Airbnb listings have a stronger impact on rents. While we have no direct information on the motives behind a host's decision to rent on Airbnb, it appears plausible that the May 2016 was more successful in reducing "professional" short-term renting on the Airbnb platform, which are likely to be available for short-term renting for longer periods of time.

To gain a sense of the relative importance of Airbnb for the housing market in Berlin, we compare the number of Airbnb listings to the evolution of the housing stock. In 2016, Berlin's housing stock reached 1.916 million apartments. In the same year, 10,722 new apartments where build. According to Figure 3b, the implementation of the law in May 2016 reduced the number of high-availability Airbnb listings in Berlin by approximately 4,000. While this number is a mere 0.02 percent of the total housing stock, it represents a noteworthy 37 percent of the newly build housing capacity in

[10] An Airbnb listing is part of our data set if the listings is set to be available for booking at any time in the future. Note that Airbnb data is missing in March 2016.



(a) All

(b) Available > 180 days

Figure 3: Airbnb supply over time

2016.[11]

3.2 Rental Descriptives

Table 1 shows selected descriptive statistics calculated for rentals we observe in the two samples used for the analysis. The "May 2016" sample includes almost 20,000 apartments listed from February 2016 to August 2016. The "August 2018" sample includes approximately 21,000 apartments listed from May 2018 to November 2018. The average asked rent per square meter is 9.27 euro in the May 2016 sample and 10.86 euro in the August 2018 sample. The increase reflects the general positive trend in rents. In terms of non-price characteristics, like the apartment size and the number of rooms, the rentals in both samples appear similar. While, statistically, apartments seem to be a significantly smaller in 2018, the differences are arguably negligible economically.

Table 1 also reports the level of Airbnb exposure before the respective reforms and the reduction in Airbnb exposure caused by the policy interventions. The overall exposure to entire home listings is higher in 2018. However, the exposure to high-availability listings is slightly smaller in 2018. Additionally, the reduction in exposure to high-availability entire homes is substantially larger in the 2016 sample.

Figure 4 illustrates that areas with higher Airbnb exposure in February 2016 experience a systematically steeper price trend than areas with less Airbnb exposure in February 2016. Specifically, we regress rents on quarterly dummies interacted with the initial level of Airbnb exposure in February 2016 and controlling for district-specific time trends. The figure reports the coefficient estimates for these interactions, which represent the partial correlation between Airbnb exposure in February 2016 and the asked rents over the entire period for which we have rent data.[12]

For example, according to Figure 4, one additional nearby Airbnb listing in February 2016 is associated with an additional two cents increase in asked rent per square meter until the end of the sample. This additional increase is measured net of district-specific

[11] See https://www.ibb.de/media/dokumente/publikationen/in-english/ibb-housing-market-report/ibb_housing_market_report_2016_summary.pdf (last accessed: March 18, 2021).

[12] Details on the estimation method are provided in Appendix A.

Table 1: Rental-level descriptive statistics

	2016			2018			
	N	Mean	SD	N	Mean	SD	Diff
Rent per sqm	19683	9.27	2.58	21356	10.86	3.30	1.597***
Area (sqm)	19683	70.29	30.25	21356	68.88	29.74	-1.419***
Rooms	19666	2.38	0.96	21332	2.34	0.94	-0.041***
# Airbnb (entire homes, 250m)	19683	9.97	17.04	21356	13.56	22.66	3.587***
...available > 180 days	19683	3.63	6.30	21356	2.91	5.16	-0.720***
Post-pre avg. # entire homes	19683	-1.94	0.42	21356	-2.31	0.60	-0.369
...available > 180 days	19683	-1.40	0.24	21356	1.05	0.22	2.452***

Notes: Descriptive statistics for selected variables on the rental-month level. The left panel shows the results for the sample surrounding the May 2016 reform. The right panel shows the results for the sample surrounding the August 2018 reform. The "Diff" column shows the differences in the means in both samples. For the "Post-pre" rows, we regress the Airbnb count on a post-law dummy, a dummy for the 2018 sample, and their interaction. The reported coefficient for the 2016 sample is just the estimated coefficient for the post-law dummy. The reported estimate for the 2018 sample is the sum of post-law dummy and interaction coefficient. *, **, *** indicate five, one, 0.1 percent significance.

quarterly fixed effects.

The results represented in Figure 4 highlight a fundamental difficulty in identifying the causal impact of Airbnb on rent prices. Despite controlling for a rich set of location-time fixed effects, there remain unobserved factors that cause differential price increases in areas with a larger baseline Airbnb density. This complicates any analysis aimed at estimating the causal long-term impact of the transitory reduction in Airbnb supply triggered by the reforms. To mitigate the potential impact of unobserved trends and because of the transitory effect of the policies on Airbnb supply, we focus our analysis on short time windows around both policy interventions.

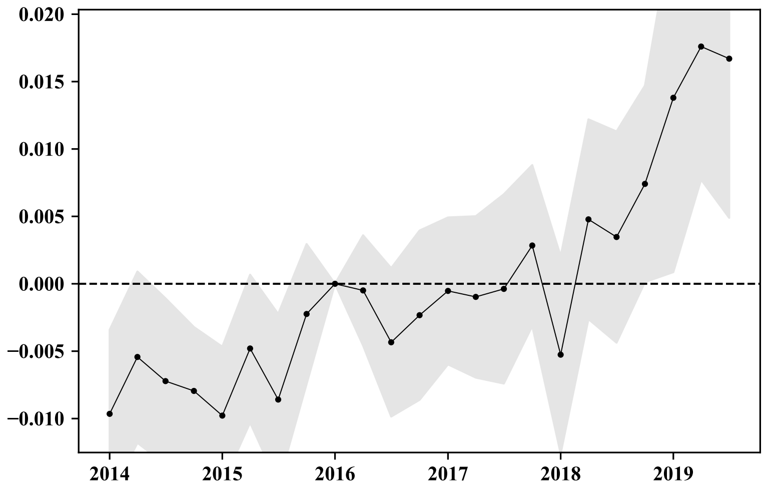


Figure 4: Partial correlation between February 2016 Airbnb exposure and rent prices.

4 Identification Strategy

For our empirical analysis, we exploit the timing of the policies as an instrument for Airbnb supply. More specifically, we use the following instrumental variable approach:

$$y_{it} = \alpha abb_{it} + x'_{it}\beta + \epsilon_{it} \tag{1}$$

$$abb_{it} = \gamma \mathbb{1}(t \geq \tau) + x'_{it}\delta + u_{it}\,, \tag{2}$$

The dependent variable y_{it} is either the asked monthly rent for rental i in month t or the rental density per square kilometer in block i in month t. When analyzing Airbnb's impact on rents, abb_{it} denotes a measure of Airbnb listings within 250 meters of the rental. When analyzing Airbnb's impact on rental supply, abb_{it} denotes a measure of Airbnb density per square kilometer in block i.

The variables contained in x_{it} denote the set of exogenous controls. When analyzing Airbnb's impact on rents, we control for a potentially very large set of covariates such as rental and neighborhood characteristics, postal-code fixed-effects, as well as postal-code-specific linear time trends. When analyzing Airbnb's impact on rental supply, by using blocks as the unit of analysis, we can employ panel estimation methods. Thus, we opt for a more parsimonious specification by differencing out block fixed-effects and including only postal-code-specific linear time trends.

Local variation in Airbnb supply cannot be assumed to be orthogonal to unobserved factors. Therefore, estimating Equation (1) by OLS, would result in a biased estimator. Consequently, we use the introduction of the reforms as an instrument. The indicator $\mathbb{1}(t \geq \tau)$ is a dummy variable that takes on the value of one for observations after the respective law took effect in τ (we refer to this variable as the "post-dummy"). We run separate regressions for the May 2016 and August 2018 sample. Thus, τ denotes either May 1, 2016, or August 1, 2018.[13]

Since we focus on short time windows around both reforms and control for geographic fixed effects, the variation we exploit for our analysis mainly corresponds to within-geography *drops* (i.e. a negative change in the number of Airbnb listings). The reduction in the number of Airbnb listings is mechanically larger in geographies with more Airbnb listings before the reform. At the same time, it is also likely that geographies with higher Airbnb density before the reform experience steeper trends in rents (see Figure 4). Thus, the steeper price trend confounds the effect of the Airbnb reduction we exploit. As a result, OLS coefficients are likely to be downward biased in magnitude because a larger drop in the number of Airbnb listings correlates with steeper price trends. This steeper price trend will dampen the reduction in rents caused by reducing the number of Airbnb listings.

The instrument proposed in Equation (2) is, by definition, orthogonal to unobserved confounders that vary at the level of the geographies within the city. This provides a compelling rational for why our instrumental variable strategy is likely to fulfill the validity assumption: the instrumented drop in Airbnb supply caused by the reform is orthogonal to trends in rents. However, for this insight to be true, the variation in Airbnb supply during the short time windows around the reforms must itself be unaffected by time trends. Otherwise, the instrumented number of Airbnb listings does

[13]We have no indication for parallel policy changes that might have simultaneously affected rents during the periods we use for our analysis. However, it might be interesting to note that a rent control policy has been active in Berlin since June 2015. We refer the interested reader to Mense et al. (2017, 2019) for a more detailed description of the policy.

not just capture variation caused by the reform but also by the trends in the Airbnb supply, which might be correlated with rent price trends. We address this threat to our identification strategy by controlling for postal-code-specific trends. Additionally, in Appendix B, we show that we find no indication for systematic trends in the observed Airbnb variation around the reforms.

5 Results

5.1 The Impact of Airbnb on Rents

To analyze the impact of Airbnb on rents, we use the IV-estimator described in Equations (1) and (2). The dependent variable is the asked monthly rent per square meter. The explanatory variable of interest is the number of entire homes listed on Airbnb within 250 meters (either including all or only high-availability listings) of the focal rental. Our approach allows us to control for individual rental-level characteristics and a rich set of neighborhood and geographical characteristics that are likely to influence both rents as well as the number of Airbnb listings. To systematically select from our rich set of covariates, we employ the "double-Lasso" estimator proposed by Chernozhukov et al. (2015) for instrumental variables estimation. The general idea is to use Lasso regression to select covariates that are most important in explaining the dependent variable as well as the explanatory variables of interest.[14]

Table 2 reports the results of our IV approach and contrasts them with non-instrumented OLS estimators. Columns (1) and (4) show, for both reforms, the results based on non-instrumented OLS when using *all* Airbnb entire homes as the measure of Airbnb exposure. Columns (2) and (5) show results based on all entire homes as the measure of Airbnb exposure when using the IV estimator. Finally, columns (3) and (6) show the results based on the IV estimator when using only high-availability entire

[14]Arguably, the most import assumption for this estimator is that the true underlying model is "approximately sparse." This assumption requires that the true model can be approximated with a small number of variables with only little approximation error. For more details on how we apply the estimator in our context, please refer to Appendix C.

Table 2: Impact of Airbnb on rents

	(1)	(2)	(3)	(4)	(5)	(6)
	2016	2016	2016	2018	2018	2018
	PDS OLS	Lasso IV	Lasso IV	PDS OLS	Lasso IV	Lasso IV
Second stage						
Entire homes (250m)	0.022***	0.068**		0.018***	0.034***	
	[0.013; 0.031]	[0.027; 0.109]		[0.008; 0.028]	[0.015; 0.054]	
Entire homes, available > 180 days (250m)			0.165***			0.162***
			[0.089; 0.241]			[0.071; 0.254]
First stage						
Post-dummy		-2.733***	-1.518***		-3.284***	0.056
		[-3.581; -1.885]	[-1.953; -1.083]		[-4.362; -2.206]	[-0.159; 0.270]
N	19,657	19,657	19,657	21,319	21,319	21,319
Rent/m^2	9.26	9.26	9.26	10.86	10.86	10.86
Selected Xs	78	67	62	52	71	64
First-stage t-stat		-6.318	-6.826		-5.97	0.506

Notes: Rental-month level analyses. Regressions potentially include apartment characteristics, neighborhood characteristics, postal-code-specific linear time trends, and postal-code fixed effects. The estimation without instrument (columns (1) and (4)) uses the double-Lasso estimator proposed by Belloni et al. (2014). The estimation with instrument uses the Chernozhukov et al. (2015) estimator. The square brackets show 95 percent confidence intervals. *, **, *** indicate five, one, 0.1 percent significance.

homes as the measure for Airbnb exposure. The first-stage results are reported in the third row of Table 2.

The estimated marginal effect using all entire homes is more than twice as large in the 2016 compared to the 2018 sample. As discussed in Section 3, the introduction of the law in 2016 was more successful in reducing the number of high-availability listings than the second reform. Thus, the difference between the coefficients in columns (2) and (5) might be a result of the different types of Airbnb listings leaving as a consequence of the different reforms. Effectively, for the August 2018 reform, we could see a similar reduction in entire home on Airbnb, but a lower impact on rents because the entire homes leaving the platform in 2018, being low-availability homes, are less likely to return to the long-term rental market. As a result, the coefficient's estimate suggests a lower impact of Airbnb on rents.

Indeed, when only using high-availability entire homes, the estimated marginal effect for the 2016 reform in column (3) is more than twice as large. The coefficient for the 2018 reform in column (6), while being similar in size, cannot be interpreted due to the weak instrument problem – the policy dummy is not significant in the first-stage. High-availability Airbnb listings are likely to capture professional landlords who choose to permanently rent to short-term renters instead of regular tenants. When also including occasional hosts in the measure for Airbnb exposure (i.e., when using all Airbnb entire homes), we introduce proxy-variable bias by using an imperfect measure for the relevant Airbnb listings driving rents. The proxy-variable bias is known to result in underestimating the real magnitude for the coefficient of interest. Note that the bias from using OLS is consistent with areas with higher Airbnb exposure experiencing steeper rent price trends as discussed in Section 4.

5.2 The impact of Airbnb on Rental Supply

In this subsection, we provide direct evidence that high-availability Airbnb listings affect long-term rental supply. Again, we rely on the IV strategy described in Equations (1) and (2). Due to the nature of the question we address in this section, we are forced to choose a different level of aggregation. To remain as close as possible to the level of geographical disaggregation chosen for the rents analysis, the unit of analysis is defined at the level of the smallest geographical unit available to us: blocks.[15]

The dependent variable is the density per square kilometer of rentals in the focal block. The main explanatory variable of interest is the corresponding Airbnb density per square kilometer. Compared to the rents analysis, we now have a panel based on granular geographies, which motivates us to run conventional panel-IV regressions with block fixed-effects.

The results of our analysis of rental supply are reported in Table 3. Columns (1) and (4) show the results using a non-instrumented OLS estimator for both reforms, respectively. As in the previous analysis of rents, the OLS estimators rely on all Airbnb entire homes as the measure for Airbnb exposure. Columns (2) and (5) repeat the same analysis but rely on the IV strategy outlined in Section 4. Columns (3) and (6) show

[15] As a robustness check, we also estimate the model using larger geographies, the so-called "Lebensweltlich orientierte Räume (LOR)." These are areas defined for statistical and urban planning that are supposed to have homogeneous structural as well socio-economic internal structures. These areas are smaller than postal code areas. Results are quite similar to those reported here.

Table 3: Impact of Airbnb on rental supply

	(1) 2016 OLS	(2) 2016 IV	(3) 2016 IV	(4) 2018 OLS	(5) 2018 IV	(6) 2018 IV
Second stage						
Entire homes per km^2	0.121	-0.409*		-0.107	0.123	
	[-0.119; 0.360]	[-0.788; -0.030]		[-0.322; 0.108]	[-0.142; 0.388]	
Entire homes (> 180 days) per km^2			-0.602*			12.28
			[-1.153; -0.052]			[-84.740; 109.300]
First stage						
Post-dummy		-21.27***	-14.45***		-32.66***	-0.327
		[-25.320; -17.230]	[-17.190; -11.700]		[-38.290; -27.040]	[-2.711; 2.056]
N	12,489	12,489	12,489	13,831	13,831	13,831
First-stage t-stat		-9.944	-10.21		-11.05	-0.24

Notes: Block-month level analyses. Block-level fixed effects regressions including a linear time trend. The square brackets show 95 percent confidence intervals. *, **, *** indicate five, one, 0.1 percent significance.

the results based on the IV estimator when using only high-availability entire homes as the measure for Airbnb exposure.

For the 2016 reform, we find a significant negative effect of entire homes Airbnb supply on rental supply in column (2). We do not find a significant effect in the 2018 sample in column (5). Again, this result is in line with the second reform mostly leading to the exit of occasional non-professional Airbnb hosts.

Also consistent with the hypothesis that high-availability listings drive the effect measured for overall supply, the marginal effect obtained when only using high-availability listings appears to be larger in 2016 (column (3)). Notice that, in the latter regression, we cannot reject the hypothesis that this coefficient is equal to minus one. This means that we cannot reject the hypothesis that each additional high-availability home listed on Airbnb reduces long term supply by one apartment. Like in the analysis for rent prices, the weak instrument problem impedes an interpretation of the second-stage estimate in column (6).

Again, the OLS coefficients appear to be downward biased in magnitude. The OLS coefficients imply that areas which experienced a larger reduction in Airbnb supply due to the reform did not experience a larger increase in rental supply. To understand why this might be the case, it is important to remember that areas that experienced a larger Airbnb reduction also have a higher Airbnb baseline density prior to the reforms. Thus, these areas are likely to be more touristic and, consequently, more lucrative for short-term renting. It is likely that the number of Airbnb hosts who decide to only superficially abide to the law (i.e. reduce availability) but who in reality continue to engage in professional short term renting (i.e. do not return the apartment to the long-term rental market) after the reforms is higher in popular tourist areas.[16]

5.3 Robustness

In this subsection, we briefly discuss two robustness checks that we perform with respect to our main analysis.

Seasonality: While we control for postal-code-specific linear-time trends, we cannot directly address the effect of month-specific effects. Our focus on short time windows prevents estimation of month fixed effects. To address seasonality, we opt for an approach in which we deseasonalize the data using the entire time span and use the deseasonalized residuals for our main analysis.[17] In Appendix D.1, we show that our results remain robust. Interestingly, the IV results for rents for the August 2018 sample are no longer statistically significantly different from zero, which is consistent with the

[16]The 2016 reform specified "misuse" in a loose sense. It is unclear how the law was enforced. It is reported that enforcement relied partly on neighbors reporting likely misuse. Additionally, an easy way for authorities to detect likely misuse would have been to check availability calendars of hosts on the Airbnb website to see if hosts rent throughout the year, which would be an immediate indication for misuse. Landlord who did not wish to comply with the law could reduce activity to raise less suspicion but continue to exclusively rent to tourists. It is important to note that prices on the long-term rental market in Berlin are regulated but that the Airbnb regulation did not include any price regulation. Thus, renting for 90 days on Airbnb might still be more attractive than renting for 365 days in a price-regulated long-term rental market. Addtionally, alternative short-term rental platforms that are subject to less scrutiny might offer a viable alternative for landlords who do not want to comply with the law.

[17]Another way to assess the role of seasonality would be to run placebo test in which we shift the time window of our analysis and define placebo control and treatment periods. This approach is problematic in our setting because the instrument is likely only relevant around the actual date of policy interventions: It is not clear how to interpret placebo results with a weak first stage.

hypothesis that only high-availability listings impact rents.

Measures of Airbnb Exposure: The choice to use a 250 meter radius circle to count the number of Airbnb listings nearby a rental is somewhat arbitrary. To assess the sensitivity of the results in the rent price analysis, we conduct robustness checks using circle size of 500 meters and 1000 meters. Qualitatively, our results remain unaffected in the sense that we only find significant effects on rents during the 2016 reform. The stronger marginal effect for high-availability entire homes in the 2016 reform vanishes if we use larger circle sizes. This might be caused by the lower geographic precision of a measure based on larger circle sizes. We present the results in Appendix D.2.

5.4 Discussion

High-availability Airbnb listings are likely to capture homeowners who substitute away from the rental market in an environment where short-term renting on Airbnb allows for generating income comparable or higher to conventional long-term renting.

Altogether, we believe that our analysis provides convincing evidence that rents are mostly affected by high-availability listings: Our data suggest that only the 2016 reform had a noticeable impact on the number of high-availability listings active on Airbnb. At the same time, we find that only the first reform was successful at reducing rents. Additionally, the estimated marginal effect when only using high-availability Airbnb entire homes is larger than the marginal effect obtained when using all Airbnb entire homes. Finally, we provide direct evidence that the reduction in Airbnb listings only led to an increase in rental supply following the 2016 reform.

We consider it unlikely that other factors might explain the differences in the effects between the 2016 reform and the 2018 reform. Our descriptive analysis suggests that the apartments in both samples are comparable: while we find statistically significant differences between apartment characteristics between both samples, the differences do not appear economically relevant.

We can gauge the relevance of the Airbnb effect by comparing the magnitude of our effect with the overall development of asked rents in Berlin. According to Statista, asked square meter rents in Berlin increased on average by 65 cents per year between 2012 and 2018.[18] According to our estimates, one additional high-availability listing in 2016 increases asked square meter rents by 16.5 cents. With, on average, 3.63 high-availability entire homes in the vicinity of a rental in 2016, the cumulative average effect of Airbnb on rent prices corresponds to 55.5 cents. Thus, the average impact of Airbnb on rents in 2016 in Berlin corresponds to 85 percent of the average yearly increase in rents in Berlin between 2012 and 2018.

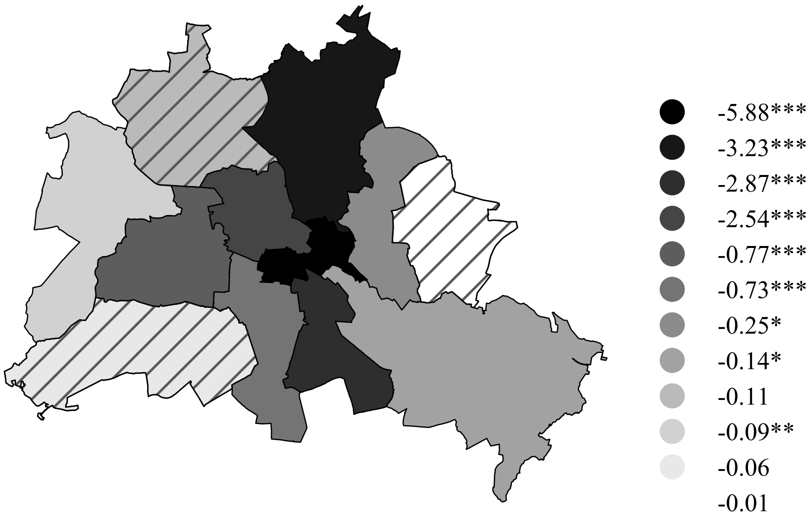
6 Geographic Heterogeneity

In this section, we explore geographic heterogeneity of the effect of Airbnb on rents. To do so, we stratify the sample by district and conduct the same analysis as reported in column (3) in Table 2. Therefore, we focus on high-availability entire homes as our Airbnb measure and use the May 2016 sample. Figure 5 graphically reports our results. Figure 5a shows the first-stage estimates for each district. Districts for which the first-stage estimates are insignificant are overlaid with diagonal stripes. Central districts experience the largest decreases in high-availability entire homes due to the reform.

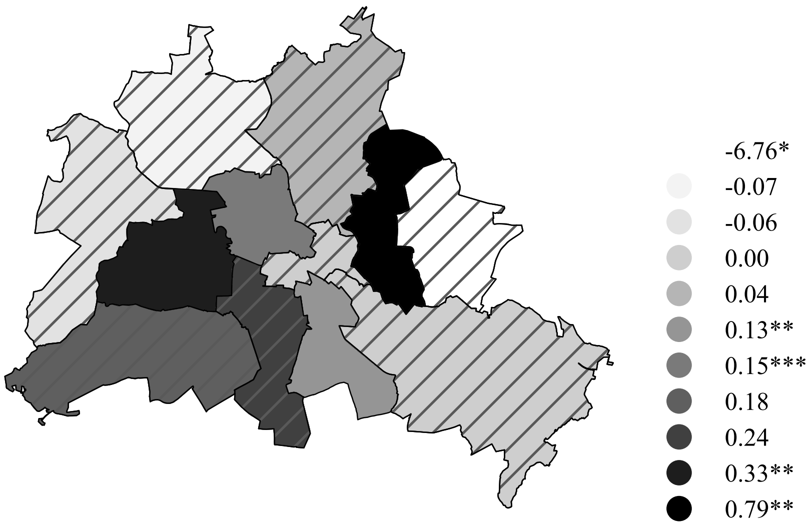
Figure 5b shows the second-stage estimates of the effect of Airbnb. Districts for which the first- or second-stage estimate is statistically insignificant are overlaid with diagonal stripes. The estimates suggest substantial effect heterogeneity across districts with marginal effects on the per square meter rent ranging from 13 to 79 cents (focusing on the districts with significant first- and second-stage estimates).

Figure 6 plots the second-stage results against the mean number of Airbnb listings within 250 meters of a rental by district. The figure only shows results for those districts for which the first stage is significant. Perhaps surprisingly, we find weak indication for decreasing marginal effects, i.e., the impact of Airbnb is smaller the larger the number of nearby Airbnb listings. One possible explanation for this pattern might be that with high Airbnb density, the negative externalities on residents (e.g. noise at night) might

[18]See https://de.statista.com, (last accessed: March 18, 2021).



(a) First-stage estimates



(b) Second-stage estimates

Figure 5: District-stratified Lasso IV regressions for rent prices.

Notes: Results refer to the May 2016 sample. The specification is identical to the one reported in column (3) of Table 2. We use only high-availability entire homes as the measure of Airbnb exposure. *, **, *** indicate five, one, 0.1 percent significance. In Figure 5a, districts for which the first-stage coefficient is not statistically significant at the 95 percent confidence level are marked with diagonal stripes. In Figure 5b, districts for which either the first- or the second-stage coefficient is not statistically significant at the 95 percent confidence level are marked with diagonal stripes.

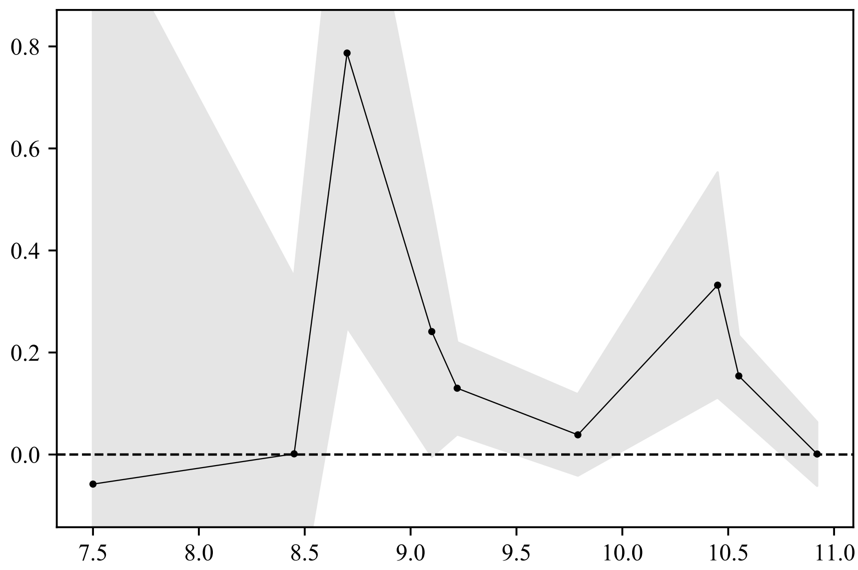


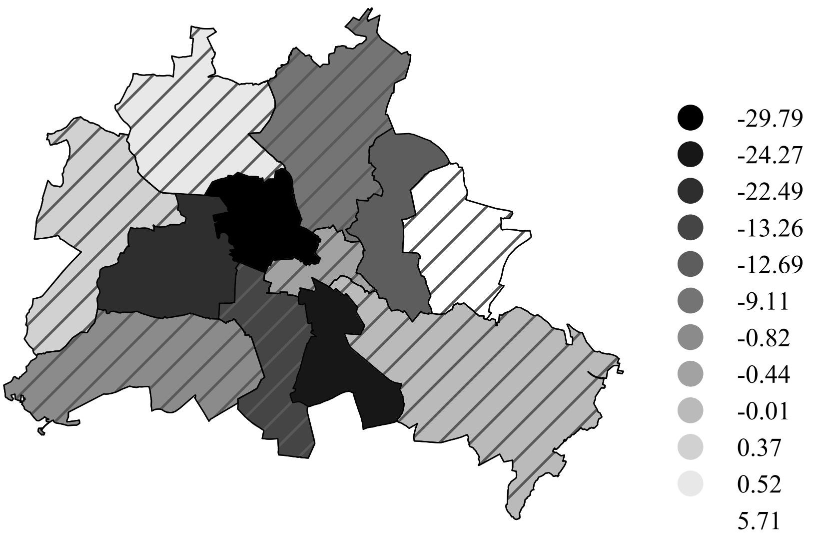
Figure 6: Second Stage Estimates by mean Airbnb exposure

Note: We only display estimates for districts in which the first stage is significant (see Figure 5a). The estimates are plotted against the mean number of entire home listings within 250 meters of a rental. The shaded area shows 95 percent confidence intervals.

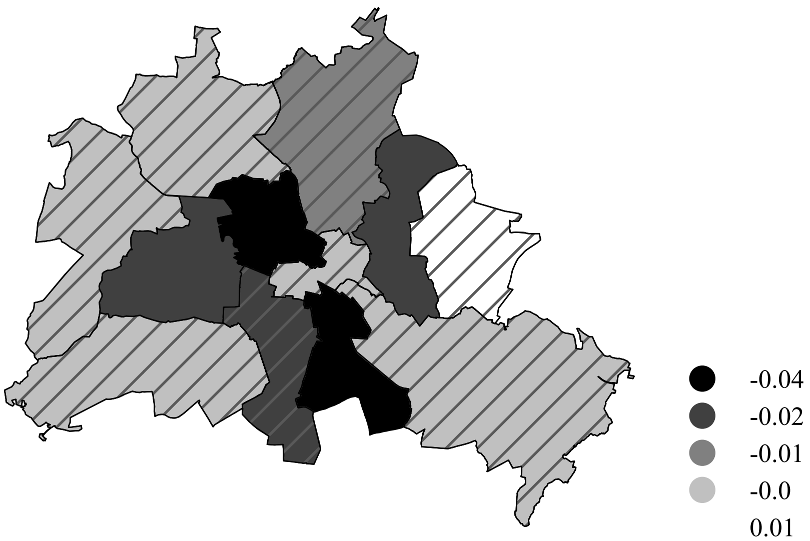
become large enough to reduce residential demand in the area.

These results are, of course, not conclusive. First, the estimates are noisy and the insight is based on the results for nine districts only, out of which only four second-stage estimates are actually statistically significantly different from zero. Second, the effects might be driven by unobserved district-level characteristics that drive both Airbnb density and the marginal effect of Airbnb on rents.

According to Figure 5a, more central districts experienced larger decreases in the number of entire homes listings on Airbnb. At the same time, however, Figure 5b suggests that these same districts are subject to lower marginal effects of Airbnb on rents. To assess the overall cumulative effect of the May 2016 legislation on rents, we look at the product of both effects. To gauge the district-specific cumulative effect of the May 2016 reform, we multiply the estimates shown in Figures 5a and 5b. Further, we calculate the average size of rentals for each district and multiply it with the product of the two stages. The result can be interpreted as the monthly rent that an



(a) Absolute decrease



(b) Relative decrease

Figure 7: District-specific cumulative decrease in monthly rents

.

Notes: Cumulative effects of the reform are calculated as the product of the estimates of the first stage (Figure 5a) and second stage (Figure 5b). For Figure 7a, we multiply these cumulative effects with the average apartment size in each district. For Figure 7b, we divide the cumulative effects by the average monthly rent per square meter in each district. Districts for which the either the first- or the second-stage coefficient is not statistically significant at the 95 percent confidence level are marked with diagonal stripes.

average apartment in each district saved due to the May 2016 reform.[19] The results of this exercise are shown in Figure 7a and suggest that the effect of the law on rents is heterogeneous across districts and amounts to a reduction in rents of up to approximately 30 euro per month in the more popular Airbnb districts. Figure 7b expresses the results of Figure 7a as a percentage of the average rent by district. According to Figure 7b, the cumulative effect amounts to up to four percent of the total monthly asked rent.

7 Conclusion

The impact of short-term rental platforms such as Airbnb on the long-term rental market is a controversially debated topic. Although cities around the world have already introduced policies to regulate short-term rental platforms, there is still little causal evidence on Airbnb's effect on rental markets and the underlying mechanisms. Our study is among the first to use exogenous variation generated by policy interventions targeting short-term rentals to yield causal evidence consistent with Airbnb negatively impacting long-term rental markets. The richness of our data allows us to provide supportive empirical evidence not only on Airbnb's effects on rents but also on the mechanism at play. Landlords listing their home on Airbnb for long periods of time exceeding half a year create a negative externality on the long-term rental market by crowding-out rental supply, thus increasing rents in turn.

According to our estimates, one additional nearby Airbnb listing available for more than 180 days a year increases the average asked rent per square meter by approximately 16.5 cents. This corresponds to roughly 1.8 percent of the average square meter rent. We also document substantial effect heterogeneity across geographies: The May 2016 reform, which substantially decreased high-availability Airbnb supply led to reductions of up to 30 euro in terms of the total monthly rent for an average apartment in the city's most trendy districts. These price effects are driven by the contraction in the long-term rental supply. Indeed, for each additional nearby high-availability

[19] Clearly, these results are also affected by the average apartment size in the various districts.

Airbnb listing there are 0.6 fewer apartment offered on the long-term rental market.

These findings have important policy implications. Our results help inform the design of effective short-term rental regulations. They suggest that, for a policy intervention with the aim of relieving the housing market to be effective, reducing professional short-term renting is key. By contrast, our results indicate that occasional short-term renting through Airbnb is unlikely causing negative externalities on the rental market. Because the latter arguably generates positive value both to the hosts and the guests, a smart regulation should consider this efficient reduction of capacity under-utilization in housing markets.

Our study has some limitations. First, and foremost, despite being the best available data, the Airbnb data used is noisy. While we use high-availability as a proxy of the host type, more precise information on whether the host is a professional or occasional short-term renter would allow for more cleanly disentangling Airbnb effects. Second, while our data on the rental markets is exceptionally rich and precise, it only covers asked rental prices and quantities. Finally, short-term rental platforms generate additional externalities for neighborhoods such as noise and pollution. Quantifying these negative effects would be a further important element in the design of more effective regulations that balance the benefits and costs of short-term rental platforms.

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Appendix

A Estimation details for Figure 4

We conduct an analysis inspired by the main specification used in Huber et al. (2020). We first need a measure of pre-treatment exposure to Airbnb. For this purpose, we count the number of entire homes within 250 meters, as of February 2016, for the location of each rental in the data. For rental i, denote this measure as $abb(Feb2016)_i$. The basic idea is to interact this measure with a full set of quarter fixed effects and use these interactions in a regression to assess how the discrepancy in rents between areas with fewer and more Airbnb listings in February 2016 changes over time. This regression amounts to estimating the following equation:

$$\begin{aligned} y_{iq} = &\sum_{\tau=Q1,2014}^{Q3,2019} \beta_\tau abb(Feb2016)_i \times \mathbb{1}(q(i)=\tau) \\ &+ \mathbb{1}(q(i)=\tau) + District_i \times \mathbb{1}(q(i)=\tau) \\ &+ DistrictFE_i + c + \epsilon_{it} \,. \end{aligned} \tag{3}$$

Each observation is one rental apartment in the quarter that it was first listed. y_{iq} is the asked rent per square meter. $abb(Feb2016)_i$ denotes the number of entire homes within 250 meters of the location of rental i in February 2016. We interact this cross-sectional measure of pre-treatment Airbnb exposure with a full set of quarter fixed effects. We use the first quarter of 2016 (the quarter containing February 2016) as the base quarter. Further, we include a full set of quarter fixed effects, district fixed effects, as well as the full set of interactions between quarter and district fixed effects. In Figure 4, we report the estimates of β_τ for all quarters. These estimates capture the difference in rents between rentals in locations with more and less Airbnb exposure in February 2016, conditional on the fixed effects.

B Identification

As outlined in Section 4, for our instrumental variable strategy to be valid, it is important that the instrumented Airbnb variation is not affected by trends. If trends play an important role in the Airbnb variation we exploit, it is likely that our instrumental variable strategy would not be valid because our instrument would then not only capture the variation caused by the reform but also trends. To the extent that geography-specific trends in Airbnb are correlated with rent price trends, this would invalidate our identification strategy. For our main results, we directly control for granular postal-code-specific trends. In this Appendix, we provide further evidence that systematic trends that might threaten our identification strategy do not appear to be present.

Table 4: Robustness trends

	Dependent variable:			
	Relative importance of drop			
	2016	2016	2018	2018
	(1)	(2)	(3)	(4)
Entire homes (250m)	0.0003		0.001	
	(−0.001, 0.001)		(−0.0001, 0.001)	
Entire homes, available > 180 days (250m)		0.001		−0.012
		(−0.001, 0.003)		(−0.036, 0.011)
Constant	0.579***	0.686***	0.911***	0.876***
	(0.328, 0.830)	(0.503, 0.870)	(0.713, 1.110)	(0.583, 1.169)

Notes: *, **, *** indicate five, one, 0.1 percent significance.

In Table 4, we assess how much of the variation in the number of Airbnb listings observed in both samples is caused by the reform. The analysis is performed on the postal code level. The analysis aims to assess how much of the total variation at the postal code level can be explained by the sudden drop in Airbnb listings caused by both reforms. If only a small fraction of the total variation in each sample is caused by the sudden drop, we might be concerned that other unobserved factors play an important role in explaining the variation in Airbnb exposure we observe.

To assess how much of the variation in each sample is caused by the reform, for each postal code, we calculate (i) the difference in the number of Airbnb listings between the beginning and the end of each sample and (*ii*) the difference in the number of Airbnb listings between the beginning and the middle of each sample. The ratio between (*i*) and (*ii*) is a measure for the importance of the reform in the total variation we observe. For example, if $(i)/(ii) = 1$, this indicates that all the variation is due to the drop in Airbnb. By contrast, if $(i)/(ii) < 1$, this indicates that the number of Airbnb listings recovered after experiencing an initial shock and that the total variation we observe is not only explained by the drop caused by the reform.

In Table 4, we regress the ratio for all entire homes and high-availability entire homes on the baseline number of Airbnb in each postal code (i.e. the number of Airbnb listings in the first month of each window). The constant is indicative of the general importance of the drop in the overall variation. The coefficients for the baseline number of Airbnb listings indicates whether there is a systematic relationship between the importance of the drop and the baseline number of Airbnb. For example, a negative coefficient would indicate that the number of Airbnb listings is recovering more strongly in areas with a higher Airbnb density at the beginning of the sample. Such a result would be worrying because it would indicate that areas with a higher Airbnb density, which have systematically steeper rent price trends, experience a stronger recovery in the number of Airbnb listings after the shock caused by the reform. The results in Table 4 indicate that the relative importance of the drop does not depend on the baseline Airbnb density, which is reassuring for the validity of our instrument.

C Our Application of Chernozhukov et al. (2015)

We briefly outline the algorithm of the estimator applied to our problem here. For a more detailed discussion, please refer to Chernozhukov et al. (2015). Consider the moment condition

$$E[(\tilde{\rho}^{y}_{it} - \tilde{\rho}^{abb}_{it}\alpha)\tilde{\nu}_{it}] = 0\,, \tag{4}$$

where $\tilde{\rho}^{y}_{it} = y_{it} - x'_{it}\theta$, $\tilde{\rho}^{abb}_{it} = abb_{it} - x'_{it}\vartheta$, and $\tilde{\nu}_{it} = x'_{it}\delta + \gamma\mathbb{1}(t \geq law) - x'_{it}\vartheta$.

Chernozhukov et al. (2015) show that this moment condition is valid around the true parameter values, even for small deviations from the true parameter values. Because of this result, the moment condition is "immune" to small selection errors. This moment condition corresponds to an exogeneity assumption when regressing $\tilde{\rho}^y_{it}$ on $\tilde{\rho}^{abb}_{it}$ using $\tilde{\nu}_{it}$ as an instrument. Therefore, the authors propose to estimate exactly this instrumental variable regression in order to obtain an estimate for α, the coefficient of interest.

Chernozhukov et al. (2015) propose to obtain the sample equivalents of the necessary expressions using the following algorithm (adapted for our setting):

1. Conduct a first-stage regression of abb_{it} on $\mathbb{1}(t \geq law)$ and x_{it} and denote the corresponding coefficients as $\hat{\gamma}$ and $\hat{\delta}$. Obtain predicted Airbnb counts using $\hat{abb}_{it} = \hat{\gamma}\mathbb{1}(t \geq law) + x'_{it}\hat{\delta}$.

2. Conduct a regression of y_{it} on x_{it} and denote the corresponding coefficient as $\hat{\beta}$.

3. Conduct a regression of $\hat{abb}_{it}$ on x_{it} and denote the corresponding coefficients as $\hat{\upsilon}$.

4. Calculate $\hat{\rho}^y_{it} = y_{it} - x'_{it}\hat{\beta}$, $\hat{\rho}^d_{it} = \mathbb{1}(t \geq law) - x'_{it}\hat{\upsilon}$, and $\hat{\nu}_{it} := \mathbb{1}(t \geq law)\hat{\gamma} + x'_{it}\hat{\delta} - x'_{it}\hat{\upsilon}$. Use IV regression of $\hat{\rho}^y_{it}$ on $\hat{\rho}^d_{it}$ using $\hat{\nu}_{it}$ as an instrument to obtain $\hat{\alpha}$.

The authors propose to use either Lasso or Post-Lasso (OLS using variables previously selected by Lasso) to run the three regression steps and obtain the parameter estimates. Asymptotically, the choice of the estimator makes no difference. We use Lasso for the estimation. Estimation is implemented in Stata using a package provided by Ahrens et al. (2018).

Chernozhukov et al. (2015) show that standard inference methods for IV regression are valid for $\hat{\alpha}$. As mentioned above, the authors show that using the IV regression of the transformed prediction errors in step 4 amounts to using a moment restriction that makes the estimator robust to small model selection mistakes.[20]

[20] The authors also discuss that if perfect model selection were possible, then the transformation were not necessary. Instead, it would be valid to use the union of the x_{it} that were selected in steps 1 and 2, together with the instrument $\mathbb{1}(t \geq law)$, in a regular IV framework.

Table 5: Results for rents analysis using deseasonalized variables

	(1)	(2)	(3)	(4)	(5)	(6)
	2016	2016	2016	2018	2018	2018
	PDS OLS	Lasso IV	Lasso IV	PDS OLS	Lasso IV	Lasso IV
Entire homes (250m)	0.026	0.107		0.013	0.007	
	[0.016; 0.037]	[0.065; 0.155]		[0.007; 0.019]	[-0.017; 0.035]	
Entire homes, available > 180 days (250m)			0.495			-0.038
			[0.253; 0.611]			[-0.121; 0.049]
Draws	200	200	200	200	200	200

Notes: Rental-month level analyses. We deseasonalize the rent per square meter and the number of nearby entire homes on Airbnb by regressing them on month fixed effects and a constant first. We then use the resulting residuals in the estimations. All other estimation details are equivalent to those reported in Table 2. For inference, we draw bootstrap samples before conducting the deseasonalization and estimation. The square brackets show 95 percent confidence intervals calculated as the 2.5 and 97.5 sample percentiles of the bootstrapped coefficient estimates.

D Robustness Analyses

D.1 Seasonality

Because we focus on seven-month windows around the policy changes in our main analyses, we cannot account for seasonality by including month fixed effects. Therefore, we conduct an analysis in which we first deseasonalize both the rent per square meter as well as the count of nearby entire homes on Airbnb by regressing both on a constant and month fixed effects. We then use the residuals of these regressions and implement the main analyses as reported in Table 2. To account for the additional variation from the deseasonalization preceding the main analysis, we use a bootstrapping procedure for inference.

Table 5 reports the results of this exercise for our rents analysis. The results show that seasonality does not seem to drive our results in the May 2016 sample. The results using IV for the August 2018 sample are no longer statistically significantly different from zero.

Table 6 reports the results of this exercise for our rental supply analysis. The point estimates using the May 2016 sample are qualitatively in line with our main results. However, due to the deseasonalization, the estimates are very noisy which prevents us from making statistically precise statements.

Table 6: Results for rental supply analysis using deseasonalized variables

	(1)	(2)	(3)	(4)	(5)	(6)
	2016	2016	2016	2018	2018	2018
	OLS	IV	IV	OLS	IV	IV
Entire homes per km^2	0.109	-0.625		-0.161	0.473	
	[-0.086; 0.377]	[-1.487; -0.091]		[-0.537; 0.038]	[0.173; 0.952]	
Entire homes (> 180 days) per km^2			-3.925			1.142
			[-49.409; 13.370]			[0.187; 2.567]
Draws	200	200	200	200	200	200

Notes: Block-month level analyses. We deseasonalize the rental and Airbnb densities by regressing them on month fixed effects and a constant first. We then use the resulting residuals in the estimations. All other estimation details are equivalent to those reported in Table 3. For inference, we draw bootstrap samples before conducting the deseasonalization and estimation. The square brackets show 95 percent confidence intervals calculated as the 2.5 and 97.5 sample percentiles of the bootstrapped coefficient estimates.

Details of Bootstrapping Procedure: For the results reported in Tables 5 and 6, we draw bootstrap samples before conducting the deseasonalization to account for the variation introduced by the deseasonalization procedure. The procedures are slightly different for the two tables.

For the deseasonalization, we use the entire data set of rents and Airbnb listings available to us. For the results in Table 5, this procedure implies that we are drawing bootstrap samples from a larger sample than we end up using in the main estimation in each bootstrap iteration. Let N denote the total number of rentals in our data. In each bootstrap iteration s, we then follow the following steps:

1. Draw N rentals with replacement from the full data set.

2. Use this bootstrap sample and regress

$$y_{it} = \alpha + \beta MonthFE_t + \epsilon_{it}\,, \tag{5}$$

where y_{it} is either the rent per square meter or the measure of nearby Airbnb listings of rental i listed in month t. This regression yields coefficient estimates $\hat{\alpha}$ and $\hat{\beta}$.

3. Calculate $\hat{\epsilon}_{it} = y_{it} - \hat{\alpha} - \hat{\beta} MonthFE_t$ for both variables.

4. Use only those rentals out of the N bootstrap rentals that are within the sample time window around May 2016 or August 2018. Denote the number of corre-

sponding rentals as N_s. Note that while N is constant for all bootstrap iterations, N_s can vary. For these N_s rentals, we run the main specifications replacing rents per square meter and the Airbnb measure with the corresponding estimated residuals.

For the results reported in Table 6, we use a panel data set of city blocks by month. Therefore, we use a cluster bootstrap in which we draw blocks rather than individual observations. For each drawn block, we include the entire time series available to us for the deseasonalization and use only the sample period for the main analysis, similar to the procedure for the rents regressions.

As point estimates, we report the results from the estimation using the original sample. Denote these point estimates as $\hat{\beta}_0$. For inference, for each coefficient, we save all estimates from each of the bootstrap iterations. To calculate 95 percent confidence intervals, we simply use the 2.5 and 97.5 percentiles of the sample distribution of these estimates. To calculate p-values, we shift all of these estimates by their mean to center them around zero. We then calculate the probability to obtain estimate $\hat{\beta}_0$ given that the true parameter distribution is the distribution of estimates centered around zero. To do so, we calculate the percentage of parameter estimates that are below $0 - |\hat{\beta}_0|$ or above $0 + |\hat{\beta}_0|$.

D.2 Measure of Airbnb Exposure

The choice to use a distance of 250 meters to count the number of Airbnb listings nearby a rental is ad-hoc. The results of robustness checks using circle sizes with radii of 500 and 1000 meters are reported in Table 7. The main result that we find no effect for the 2018 reform while we find a significant effect for the 2016 reform remains intact irrespective of the circle size chosen.

However, for the 2016 reform, we have no indication that the high-availability listings exert a stronger effect. We note that a smaller circle size should be more desirable in the sense that it captures more precisely how exposed a rental is to Airbnb. Larger circle sizes could result in a high count of Airbnb listings, which might, however, be far away from the listing of interest.

Table 7: Main results using different circle sizes

	(1)	(2)	(3)	(4)	(5)	(6)
	2016	2016	2016	2018	2018	2018
	PDS OLS	Lasso IV	Lasso IV	PDS OLS	Lasso IV	Lasso IV
Entire homes (500m)	0.003*	0.037***		0.004	0.007	
	[0.000; 0.007]	[0.023; 0.051]		[-0.000; 0.008]	[-0.001; 0.015]	
Entire homes, available > 180 days (500m)			0.034***			0.130**
			[0.017; 0.051]			[0.050; 0.211]
Selected Xs	115	109	56	59	64	74
Avg. # Airbnb	34.09	34.09	12.47	46.88	46.88	10.23
Entire homes (1000m)	0.000	0.008***		0.001	0.002	
	[-0.001; 0.002]	[0.004; 0.012]		[-0.001; 0.002]	[-0.000; 0.004]	
Entire homes, available > 180 days (1000m)			0.008**			0.145***
			[0.002; 0.014]			[0.069; 0.222]
Selected Xs	109	102	38	36	89	55
Avg. # Airbnb	116.70	116.70	42.94	160.60	160.60	35.33

Notes: Rental-month level analyses. We use the number of nearby entire homes on Airbnb within 500 meters and 1000 meters as our Airbnb measure. All other estimation details are equivalent to those reported in Table 2. The square brackets show 95 percent confidence intervals. *, **, *** indicate five, one, 0.1 percent significance.