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Airbnb and rental markets: Evidence from Berlin

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ABSTRACT

We exploit the differential responses of Airbnb hosts to two distinct policy interventions in Berlin to shed light on the optimal design of policies targeting short-term rental platforms to mitigate rental market inflation. The first intervention, which affected commercial listings, significantly impacted long-term rental markets, unlike the second intervention, which mainly affected non-commercial listings. Leveraging these policy variations, we estimate the marginal impact of Airbnb on rental supply and rents. Each additional commercial Airbnb listing displaces 0.23 to 0.37 rental units and increases rent per square meter by 1.3 to 2.4 percent. This underscores the importance of targeting commercial listings when regulating short-term rental markets.

1. Introduction

Many cities worldwide are facing housing shortages and associated rent increases. Short-term rental platforms and the potential opportunities they offer to re-purpose housing into short-term accommodations have been targets of policies aimed at increasing the supply of housing. A flurry of regulatory measures ranging from licensing requirements to blanket bans of certain short-term rental practices have been proposed and implemented across the world in recent years (see Nieuwland and Van Melik (2020) and von Briel and Dolnicar (2021) for surveys). While regulating short-term renting in protection of the rental market has been a popular policy, proponents of peer-to-peer platforms, such as Airbnb, contend that such regulations need to account for the economic welfare that peer-to-peer platforms create. For instance, owner-occupiers who occasionally rent out their homes generate gains from trade while likely having a small impact on the housing market. Furthermore, the additional short-term accommodation supply can also benefit travelers (Farronato and Fradkin, 2022). On the other hand, if properties are offered on the short-term rental markets commercially, supply on the long-term rental market is reduced, potentially adding pressure to the rental market.

Balancing the goals of protecting the rental market and preserving the beneficial transactions enabled by short-term rental platforms requires an understanding of how different policies affect different types of Airbnb listings and how those, in turn, affect rental markets. This would allow policymakers to design policies that specifically target Airbnb listings that are likely to be repurposed as rentals while maintaining Airbnb listings that have little impact on the housing market. While empirical studies identify a positive relationship between Airbnb expansion and rents as well as house prices (e.g. Garcia-López et al., 2020; Koster et al., 2021), there is much less empirical work aimed at quantifying the substitution between different types of Airbnb listings and housing supply. Consequently, there is scarce quantitative evidence on which types of policies are likely better suited to balance harnessing the advantages of short-term renting and protecting rental markets. Understanding differential impacts of differential policies is the main focus of this paper.

We study two distinct policy changes that affect short-term rental supply in Berlin, Germany. In May 2016, a law came into force that prohibited the “misuse” of real estate property for short-term renting.

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In August 2018, this law was amended to require hosts on short-term rental platforms to publish a registration number. We first assess the impact of these policy interventions, highlighting differences in their impact on commercial and non-commercial Airbnb listings. We define commercial listings based on two alternative measures. First, we define an Airbnb listing as commercial if its annual revenue exceeds the average annual rental income of a comparable property on the long-term rental market. Second, we define an Airbnb listing as commercial if it is available for booking for more than 180 days a year. The rationale for this latter definition is that a property is assumed to be a person's main residence under tax law if they live in it for more than half of the year. We show that, while both policy changes led to a reduction in the number of entire homes listed on Airbnb, only the 2016 policy affected commercial listings. This stands in contrast to the 2018 policy, which mostly affected non-commercial listings. Next, we provide evidence that *only* the 2016 policy led to a statistically significant average increase in long-term rental supply in areas where Airbnb listings were available before the policy was implemented, compared to areas where this was not the case. We do not find such a statistically significant result for the 2018 policy. Finally, exploiting the policy changes to design an instrumental variable strategy with the aim of identifying the marginal effect of increased Airbnb supply on both rental supply and rent prices, we find that each additional commercial Airbnb listing decreases rental supply by 0.23 to 0.37 units and increases square meter rents by 0.13 to 0.24 euros. We consistently find stronger marginal effects when using *only* commercial Airbnb listings as our measure of Airbnb exposure — as opposed to using *all* Airbnb listings, which is the sum of non-commercial and commercial listings. We find a 188% to 363% larger effect on rental supply, and a 72% to 221% larger effect on rents when using only commercial listings, instead of all listings.

For our analysis, we build a novel and rich dataset containing monthly information on asked rents, rental characteristics, and information about the number of Airbnb listings in highly granular geographies. Our data allow us to control for a rich set of observable characteristics and geography-specific fixed effects and time trends. In our empirical analysis, we first document that both policy interventions substantially reduced overall Airbnb supply in the short-term. The May 2016 reform led to a 30% reduction of Airbnb listings, whereas the August 2018 reform led to an 18% reduction. Importantly, we also document that both reforms had a different impact on the type of Airbnb listings exiting the market: While we observe a sizeable exit of occasional listings both in 2016 and in 2018, only the 2016 reform led to a sizeable exit of commercial listings. We define commercial listing either based on availability — i.e. listings available for short-term renting for more than 180 days over the last twelve months – or based on revenue – i.e. listings earning more than the average rent for a similarly-sized apartment in the same area.

To assess whether the policies were effective in increasing rental supply, we run a difference-in-differences analysis where we define treated units as the geographies for which we observe any Airbnb exposure at the *beginning* of the calendar year of each reform. To account for cross-sectional and time-varying differences across geographies, we control for granular geography-specific fixed effects and time trends. While we find a statistically significant impact of the 2016 reform on the rental supply of treated geographies, this is not the case for the 2018 policy intervention. This discrepancy corroborates the hypothesis that it is mainly commercial listings that crowd out rental units: Because only the 2016 reform led to a substantial reduction in commercial listings on Airbnb, it is only for this reform that we observe an increase in rental units after the policy came into effect. The statistically significant positive effect of the 2016 reform on rental supply is not accompanied by a statistically significant negative effect on rental prices. There are several possible explanations for this, one of which is that the rent effects are moderate and that the difference-in-differences method is too imprecise to measure them.

Thus, to more precisely estimate the marginal effect of Airbnb on rental supply and rent prices, in a second step we use the policy intervention as a source of plausibly exogenous variation in Airbnb supply for an instrumental variable approach. Our instrumental variable approach relies on exploiting the correlation between pre-policy exposure to Airbnb and the reduction in Airbnb listings induced by the reform to build a relevant instrument. Essentially, we use a difference-in-differences estimator of the policies' effect on Airbnb supply as our first stage. The validity of our instrument relies on the assumption that, conditional on geographically granular fixed effects and time trends, and a rich set of observable characteristics, the pre-policy exposure to Airbnb is orthogonal to unobserved factors that might explain differences in the average variation in rental market outcomes around the policy changes between areas with and without pre-policy exposure to Airbnb.

We first apply our IV methodology to estimate the marginal effect of Airbnb on rental supply. For the 2016 reform, we find that each additional commercial Airbnb within the same city-block leads, on average, to a reduction in the supply of housing units of 0.23 to 0.37. If, instead, we use all listings (non-commercial and commercial listings) to measure Airbnb exposure, the estimated marginal effect reduces to 0.08. The approximately three- to five-fold difference in estimated effect sizes highlights the importance of accounting for the type of Airbnb listings. For the 2018 reform, we obtain imprecisely estimated null effects, consistent with this reform generating insufficient variation in commercial listings to identify the marginal effect conditional on the highly disaggregated geographic fixed effects and time trends.

To understand the effect of Airbnb on rents, we apply our IV methodology to analyze how the presence of Airbnb listings near newly listed rentals affects their asked prices. To exploit the rich set of characteristics we observe for each rental, we opt to use the individual rental as the unit of observation and measure its Airbnb exposure by counting the number of Airbnb listings within 250 m. We systematically select rental-level control variables from a rich set of variables using a Lasso-based approach proposed by Belloni et al. (2014) and Chernozhukov et al. (2015). When using all Airbnb listings (non-commercial or commercial) as the measure of Airbnb exposure, we estimate a marginal rent increase of 8 cent per square meter. Consistent with the findings from the analysis focusing on the substitution between Airbnb listings and rental units, we find a larger effect when focusing on commercial listings only. We estimate that a single additional commercial Airbnb within 250 m leads to an increase in asked rents of 13 to 24 cent per square meter, which corresponds to approximately 1.3 to 2.4 percent of the average rent (approximately 10 euro per square meter).

Our paper contributes to a growing body of literature on the effects of peer-to-peer home-sharing on the housing market, which generally finds that increased Airbnb exposure results in higher rents or house prices. Using a shift-share instrument, Garcia-López et al. (2020) find that Airbnb causes average rent and house price to increase on the order of 4%. Using a similar approach with data from the US, Barron et al. (2021) find a 0.02% increase in house and rent prices from a 1% increase in Airbnb penetration at the zip-code level (see also (Franco and Santos, 2021) and Congiu et al. (2022) for further examples employing this identification approach). Koster et al. (2021), Peralta et al. (2023), and Bibler et al. (2023) exploit spatial discontinuities in Airbnb legislation to show that these policies tend to reduce house prices with (Koster et al., 2021) additionally studying the effect on rent prices at the US-zip-code level.[1]

[1] Other approaches try to address the inherent endogeneity driven by omitted variables or potential reverse causality by controlling for a large set of observables (Horn and Merante, 2017) and comparing more-Airbnb-exposed areas to less-exposed ones (see Horn and Merante, 2017; Mindl, 2020; Franco and Santos, 2021).

We make two contributions to this literature. First, we evaluate and compare two policy changes in the same city. This is key to understanding how the design of such policies can impact Airbnb and the rental market, ultimately contributing to improving their effectiveness. Second, because these two policy changes have heterogeneous effects on Airbnb supply depending on the commercialism of the listings, we can better understand the mechanism through which Airbnb affects rental markets. Exploiting the plausibly exogenous and heterogeneous variation induced by the policies, we can precisely estimate the marginal impact of different types of Airbnb listings in a unique setting within the same geography.

Both contributions are novel, helping to better assess the relationship between peer-to-peer home-sharing platforms and the long-term rental market. The fact that we can highlight the differences between two types of Airbnb regulation in the same city distinguishes our analyses from previous work that exploits policy shocks. While Koster et al. (2021) analyze 18 policy changes in different cities of Los Angeles County, all of these policy changes are of similar nature. Therefore, their results cannot inform about differential impacts of different types of Airbnb regulation. The same is true for the analyses in Peralta et al. (2023) and Bibler et al. (2023) who investigate single policy changes in different cities. We complement these pieces of work by highlighting how two different policy changes in the same city can have a differential impact on Airbnb supply and, subsequently, the rental market.

Finally, our paper is further related to research broadly studying externalities exerted by peer-to-peer short-term rental platforms. Wachsmuth and Weisler (2018) find that Airbnb can increase gentrification, while results by Almagro and Domínguez-Iino (2022) suggest that Airbnb affects residents' location choices and availability of local amenities. In a similar vein, Calder-Wang (2021), using data from New York City, finds that rent increases caused by Airbnb result in welfare losses for renters, even after accounting for the possibility that renters might themselves generate revenues from Airbnb.

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 evaluate how the two policy changes in 2016 and 2018 affected the supply of Airbnb listings as well as rental market outcomes. In Section 4, we use the insights from Section 3 to estimate the marginal effect of nearby Airbnb listings on rental supply and rent prices. Section 5 concludes.

2. Institutional background and data

2.1. The berlin housing market

Since German reunification in 1990, the Berlin housing market has 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, with the housing price bubble bursting before the turn of the millennium (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 sharply increasing accommodation figures: In 1996, there were 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. Between 2010 and 2019, the number of beds in accommodation establishments has risen from around 100,000 to around 150,000.

At the same time, Berlin has gained considerably in population. Since reunification, the population has risen by 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 in 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 laws, and expropriation of large housing companies. Prices, between 2010 and 2022, have increased by 160%.[2] Within this period, rents surged by approximately 20%.[3]

2.2. Institutional background

Because of the increase in rent prices, in May 2014 the Berlin Senate passed a law[4] to ban the "misuse" of housing, i.e. the use of real estate property for purposes other than housing. This law did not focus exclusively on short-term renting as a form of misuse, but also included other potential forms such as speculation and commercial use and even prohibited leaving housing space unused. However, for short-term rentals specifically, the law stipulated a two-years transition period 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. In its initial version, the law remained vague on the exact definition of when a short-term rental constitutes "misuse".

On August 1, 2018, an amendment of the law took effect. This amendment defines more clearly what requirements need to be fulfilled for a short-term renting permission to be granted.[5] Another 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. Fig. 1 summarizes the different stages of the law.[6]

For our main analysis, we focus on one-year windows around the date on which the initial law and its amendment took effect. We refer to the first intervention as the "2016 reform" and to the second intervention as the "2018 reform".[7] The law and its amendment applied, unequivocally, to the entirety of the city. We are not aware of other reforms relevant to Airbnb or the rental market that were implemented in Berlin around these dates. The arguably most relevant other reform is the introduction of the "second-generation rent control" (see Mense et al., 2023, for more details) that was implemented in June 2015.

[2] For details on property price developments in German cities, see www.greix.de.

[3] According to data of the federal statistical office. As rents for sitting tenants are highly regulated, the main driver of this development are rents in new contracts, which have almost doubled between 2012 and 2022 according to the state-owned investment bank of Berlin, ibb: https://www.ibb.de/media/dokumente/publikationen/in-english/ibb-housing-market-report/ibb-housing-market-report-2022-summary.pdf.

[4] The German name of the law is Zweckentfremdungsverbot-Gesetz; we refer to it as ZwVbG or just "the law".

[5] In particular, it allows residents to rent out property short-term, as long as the property is their main residence and short-term rental is only occasional. Furthermore, it allows residents 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. There is a fee of 255 euro to be paid to start the approval process at the local district council (*Bezirksamt*) independently of the outcome of the application. The law specifies that, upon submission of all the necessary documents by the host, the local district council must issue a decision within three months.

[6] In Duso et al. (2022), we use these policy changes to discuss how event studies can be used to assess supply-side substitution.

[7] It would also be interesting to analyze the implementation of the reform in May 2014. However, our Airbnb data only start in November 2014, thus preventing us from doing so.

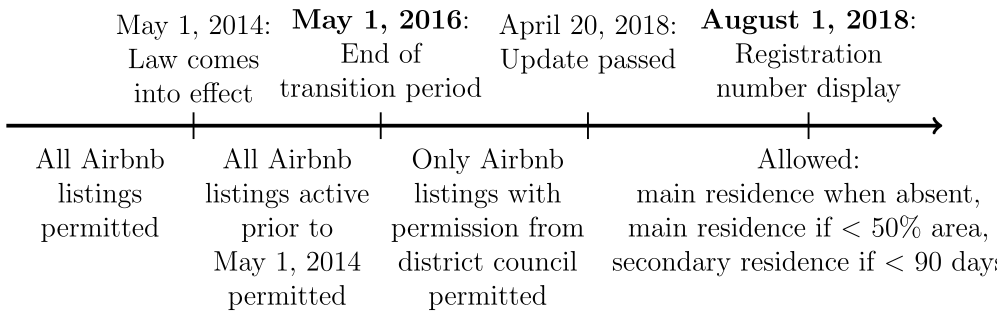


Fig. 1. Stages of the ZwVbG. Dates in bold denote the policy dates used in our analysis.

Hence, all our results are for time periods in which the rent control was already in place.

There is only limited public information about the degree to which the policies were enforced. There are three official sources of information available. The first is a report issued by the Berlin Senate (the executive organ) to the Berlin House of Representatives about the implementation of the law as of end of September 2016 (Senatsverwaltung für Stadtentwicklung und Wohnen, 2016). The other two sources are responses by the Berlin Senate to inquiries made by Member of Parliament Niklas Schenker of the Linke party to the Berlin House of Representatives in July and August 2022 (Senatsverwaltung für Stadtentwicklung, Bauen und Wohnen, 2022a,b). According to Senatsverwaltung für Stadtentwicklung und Wohnen (2016), by the end of September 2016, i.e. five months after the end of the transition period, 2636 applications for permits to use real estate as short-term rentals had been submitted. Of these, only 1161 applications had been decided by September 30, 2016. This discrepancy indicates that obtaining a permit was not an instant process, but could take some time. In most decided cases, the applications were rejected (793). Of the remainder, 316 applications were withdrawn and only 58 permits to operate a short-term rentals were granted throughout Berlin by end of September 2016. These numbers indicate that, at least shortly after the end of the transition period, it was fairly difficult to obtain a permit for short-term renting and enforcement was fairly strict, at least for Airbnb hosts who tried to comply with the law.

It is more difficult to find official statistics on the degree to which non-complying Airbnb hosts were prosecuted. According to Senatsverwaltung für Stadtentwicklung, Bauen und Wohnen (2022a), as of June 30, 2022, approximately 7.4 million euro in fines were issued related to the law. However, this figure is not broken down into the type of misuse the fines relate to, so they include fines for other types of mis-use such as speculation and commercial use as well. Furthermore, of these 7.4 million euro in fines, only about 2.1 million euro had been collected as of June 30, 2022. This discrepancy shows that enforcement of the law was a potentially lengthy process with property owners having the possibility to appeal the districts' decisions. Nevertheless, the numbers suggest that the districts did put effort into enforcing the law.

While districts that are more popular on Airbnb have received more applications for permission to offer properties on short-term rental platforms, the success rate exhibits no systematic difference between districts that are more or less exposed to Airbnb. This result suggests that enforcement was fairly proportional to the Airbnb presence across different districts in the city.

2.3. Data

To measure Airbnb supply in Berlin over time, we use data obtained from the data provider AirDNA. The data we use contain monthly snapshots of web scraped data of Airbnb listings in Berlin from November 2014 to July 2019. We observe three different categories of listings over time: entire homes, private rooms, and shared rooms. The majority of Airbnb listings are either an entire home or a private room. For the analysis, we focus on the supply of entire homes because these listings are the most likely to be rented out as long-term rentals in the absence of Airbnb. Furthermore, the law mostly targets short-term renting of entire properties. Consistently, our results show that this type of Airbnb supply was most affected by the reforms. Importantly for our classification of commercial Airbnb listings, the data also contain an estimate of each listing's monthly revenue and the number of days for which it was booked or available for booking.

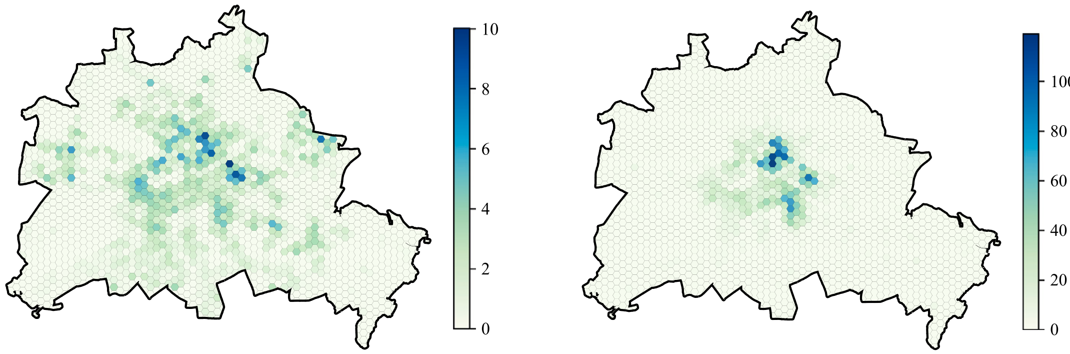
Monthly data on asked rents were provided by the economic consultancy Empirica.[8] Data from the same provider are used in previous studies analyzing the impact of rent control policies in Berlin Mense et al. (2017, 2019). The data include web scraped information on properties listed in Berlin mainly offered on three large online marketplaces: *Immonet*, *Immowelt*, and *Immobilienscout24*. The data span the period from January 2013 through July 2019 and include information on asked rents as well as various rental characteristics, such as its size and the number of rooms.[9] Each rental is only observed in the month in which it was first listed. Hence, the data set is a repeated cross-section of rentals. 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 are frequently used as proxies for real transaction information in many studies. The existing evaluations of these data show 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 match price dynamics of the entire market (Faller et al., 2009; Henger and Voigtländer, 2014). According to Henger and Voigtländer (2014), data from *Immobilienscout24* covered approximately 70 percent of the market in Hamburg, Germany, from 2007 through 2014.

To account for the heterogeneity in the attractiveness of different neighborhoods, we use geographical fixed effects where possible. When that is not possible, we use data from OpenStreetMap. The data include information about various points-of-interest such as restaurants, bars, and supermarkets.[10] The data we use are a snapshot as of February

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

[9] 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).

[10] The data are available at https://download.geofabrik.de/europe/germany/berlin.html (last accessed: March 19, 2021). In total, we use 134 variables that contain the count of a given type of point-of-interest in each city block.



(a) Long-term rentals

(b) Entire homes on Airbnb

Fig. 2. Distribution of rentals and entire homes on Airbnb in Berlin.

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.[11] Again, these data are only of cross-sectional nature. In total, we create 144 variables to capture neighborhood characteristics.

Data aggregation

We have monthly data both for Airbnb listings as well as long-term rentals. When analyzing the effect of Airbnb exposure on rental supply, we aggregate these to the smallest geographical units in our data: city blocks.[12] We count the number of rental apartments and Airbnb listings that are listed in each city block each month. When using city blocks as our unit of observation, we obtain a panel data set at the block level.

When analyzing the impact of Airbnb on rents, we use individual rental apartments in the month in which they were first listed as our basic unit of observation. This approach allows us to directly control for apartment characteristics. To obtain a measure of Airbnb exposure, we count the number of Airbnb listings within a 250-meter-radius of each listing.[13] Additionally, we use the cross-sectional data on neighborhood characteristics (e.g. points of interest or pollution measures) that we aggregate on the block level as described above to control for the characteristics of the block in which each rental is located. Thus, when analyzing the impact of Airbnb on rents, we are effectively using a repeated cross-section, thus implying we can no longer straightforwardly absorb block fixed effects. Instead, we control for postal code fixed effects and postal-code-specific trends where possible.[14]

2.4. Descriptive statistics

Table 1 shows selected descriptive statistics aggregated at the block-month level. In much of our analysis, we focus on the six months before and after each of the two policies took effect. Therefore, we show the descriptive statistics split by these two samples. The average asked rent per square meter is 9.20 euro in the May 2016 sample and 10.74 euro in the August 2018 sample. This increase reflects the general upward trend in rents. On average, each block sees 0.68 new rentals listed each month in the 2016 sample. This number is lower in the 2018 sample at 0.57 rentals. The average number of entire homes on Airbnb is roughly constant at 1.27 listings per block in both samples.

To distinguish between occasional and commercial listings, we use the availability or the revenue of a listing in the preceding year. Specifically, to classify commercialism based on availability, we distinguish listings by whether they were available for booking more than 180 days in the last twelve months (LTM) preceding each focal month. To classify commercialism based on revenues, we assess if a listing earned more than the average rent of long-term rentals with a similar number of rooms located in the same area over the LTM prior to each month.[15] The last two rows of Table 1, together with the number of entire homes staying constant across the samples, suggest that the share of commercial listings on Airbnb increased over time.

Fig. 2 shows how rentals and Airbnb listings are distributed across the city. For that purpose, we split the city into hexagons with a diameter of 500 m[16] and calculate the average number of long-term rentals and entire homes on Airbnb per month in each cell. Whereas Fig. 2(a) shows a relatively spread-out distribution throughout the inhabited parts of the city, Fig. 2(b) shows that Airbnb listings tend to be much more focused around certain areas in the city center.

3. Policy evaluation

In this section, we assess the impact of the full taking-effect of the law in 2016 and its amendment in 2018. For this purpose, we first

[11] The data are available at https://fbinter.stadt-berlin.de/fb/index.jsp (last accessed: March 19, 2021). In total, we use ten variables that contain different block-level measures.

[12] There are 13,075 city blocks in our data. They are typical units in Berlin's urban structure and can be thought of as building blocks surrounded by streets. An average block has an area of approximately 0.03 square kilometers, but this size varies across blocks.

[13] The choice of a radius of 250 m is ad-hoc. Including further away Airbnb listings will most likely result in including listings that are less relevant for the focal rental and, hence, would attenuate estimated effect sizes. We discuss the choice of the circle size and related robustness checks in Section 4.4.

[14] There are 190 postal codes in Berlin and their average size is about 4.69 square kilometer, although postal codes in the central districts where most Airbnb are located tend to be smaller.

[15] As the area of reference, we use so-called "Lebensweltlich orientierte Räume" (LOR). These are areas defined for statistical and urban planning, which are smaller than postal codes and are supposed to capture living areas in Berlin that are relatively homogeneous. In our sample, we have 447 different LORs. In each LOR, for each month, we calculate the average asked rent for long-term rentals over the LTM by the number of rooms. For each Airbnb listing, we calculate the average monthly revenue over the LTM. Then, we match each Airbnb listing with the corresponding LOR and compare their average revenue to the average rent of similarly sized long-term rentals. We match listings to rentals by using the number of bedrooms and adding one. This correction reflects the convention that a living room is typically counted as an additional room for long-term rentals, but for the Airbnb listings we only observe the number of bedrooms. Finally, an Airbnb listing is classified as commercial according to this measure if its average monthly revenue in the LTM exceeds the average rent of similarly-sized long-term rentals in the same LOR in the LTM.

[16] We use the shapefiles provided by Ahlfeldt et al. (2023).

Table 1
Block-month-level descriptive statistics.

	2016			2018			Diff
	N	Mean	SD	N	Mean	SD	
Monthly rent	26 207	664.23	389.30	25 650	752.15	436.85	87.927***
Rent per sqm	26 207	9.20	2.39	25 650	10.74	3.14	1.547***
# Rentals	60 673	0.68	1.09	70 118	0.57	1.08	−0.112***
# Airbnb (all entire homes)	60 673	1.27	2.18	70 118	1.28	2.12	0.006
# Airbnb (available > 180 days LTM)	60 673	0.45	0.96	70 118	0.60	1.21	0.150***
# Airbnb (listing rev. > comparable rents LTM)	60 673	0.29	0.73	70 118	0.47	1.06	0.178***

Notes: Descriptive statistics for selected variables on the block-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. *, **, *** indicate five, one, 0.1 percent significance.

assess how the number of different types of Airbnb listings available in Berlin has changed over time. Then, we investigate if the policy changes have led to excess exits from the platform. Here we specifically focus on whether the policies had differential effects on different segments of Airbnb supply. Finally, we analyze if the policy changes led to measurable changes in rental supply and prices.

3.1. Airbnb supply over time

Fig. 3 shows the evolution of the number of Airbnb listings in different categories using monthly data from November 2014 to July 2019.[17] The two vertical lines mark the policy dates in May 2016 and August 2018. The supply of Airbnb listings has generally been increasing in the months leading up to 2016. Then, we observe a clear transitory reduction around the policy intervention in May 2016. This drop starts a couple of months prior to the policy date, which indicates that hosts might have anticipated the policy change.[18] Following the 2016 reform, the supply of Airbnb listings increased again, but with a flatter slope than before the reform.

The August 2018 reform also resulted in a substantial decrease in Airbnb listings. However, anticipation seems to play less of a role for this policy change. A possible explanation for this difference is that the 2016 reform was preceded by a two-year transition period, whereas there were only a few months between passing and implementation of the 2018 reform. Additionally, media coverage leading up to the 2016 reform seems to have been much more pronounced than for the 2018 reform.[19]

In particular, the May 2016 drop is mostly driven by decreases in the number of entire homes. While prior to the 2016 reform, the number of entire homes on the platform was roughly twice as large as the number of private rooms, they are approximately on the same level from the 2016 reform onwards. This insight, joint with the expectation that these listings are likely the most relevant for the long-term rental market, motivates our focus on the entire homes category for much of the remaining analysis.

[17] An Airbnb listing is part of our sample if it was scraped and available for at least one day in a given month and had at least one booking over the preceding 12 months.

[18] Recall that there was a two-year transition period between the passing of the law in May 2014 and its full taking effect two years later. Therefore, it is plausible that some hosts anticipated the policy change and left the platform earlier — particularly, if they needed some time to repurpose their properties or because they were unsure about the registration process and whether they would obtain a permit.

[19] A search of German-speaking newspapers on genios.de for the term "zweckentfremdungsverbot-gesetz" reveals a peak around May 2016 with an increase in the months prior to the reform. In contrast, there is a smaller peak in August 2018 with seemingly no increase in the preceding months. See https://www.genios.de/searchResult/Alle?requestText=zweckentfremdungsverbot-gesetz&date=from_01.01.2016&date=to_31.12.2018 (last accessed: November 1, 2023).

3.2. Policy impact on Airbnb supply

The transitory nature of the decreases documented in Fig. 3 leaves open the possibility that Airbnb listings might only leave the platform temporarily and re-enter a few months after the reforms. To address this possibility, we next focus on analyzing permanent exits of the platform, both graphically as well as using Probit regressions. Doing so, we also distinguish between exits of commercial and non-commercial Airbnb listings to highlight differences in the way the two policies affected Airbnb supply.

Graphical analysis

In Fig. 4, we show the number of listings permanently leaving Airbnb. We define the month of exit as the month in which we observe a listing for the last time in the data. For each month, we then count the number of listings that exit the platform by listing type. We do the same exercise for hosts: We define the month in which we see a host for the last time in the data as the host exit month. Then, for each month, we count the number of hosts that exit the platform by listing type.

We observe spikes of exits around the two policy dates both in terms of listings as well as hosts exiting — see Figs. 4(a) and 4(b), respectively. However, there are some clear differences between the two policies. In 2016, cumulated over the three months prior to the policy date, almost 4000 listings leave Airbnb.[20] Interestingly, this figure is more than twice as high as the official number of denied permit applications by September 2016 (Senatsverwaltung für Stadtentwicklung und Wohnen, 2016). Additionally, according to Senatsverwaltung für Stadtentwicklung und Wohnen (2016), 1518 short-term rentals had been reverted back to residential use by September 2016. This discrepancy between observed change and that reported by the government is consistent with certain hosts not even attempting to obtain a permit and just removing their listings from the platform.

The number of hosts who leave the platform in the same period is smaller. The difference between the number of listings and hosts leaving the platform indicates that some of these exiting hosts own multiple listings. Since it is more likely that commercial – rather than occasional – hosts own several listings, this finding is consistent with commercial hosts exiting the market.

This intuition is also confirmed by the patterns shown in Fig. 5, where we focus on entire homes and categorize them based on two possible proxies for commercial listings. In Fig. 5(a), we distinguish listings by whether or not they were available for booking more than 180 days in the last twelve months (LTM). In Fig. 5(b), we classify

[20] 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 were built (Investitionsbank Berlin, 2016). The implementation of the law in May 2016 reduced the number of Airbnb listings by approximately 4000 units. 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 2016.

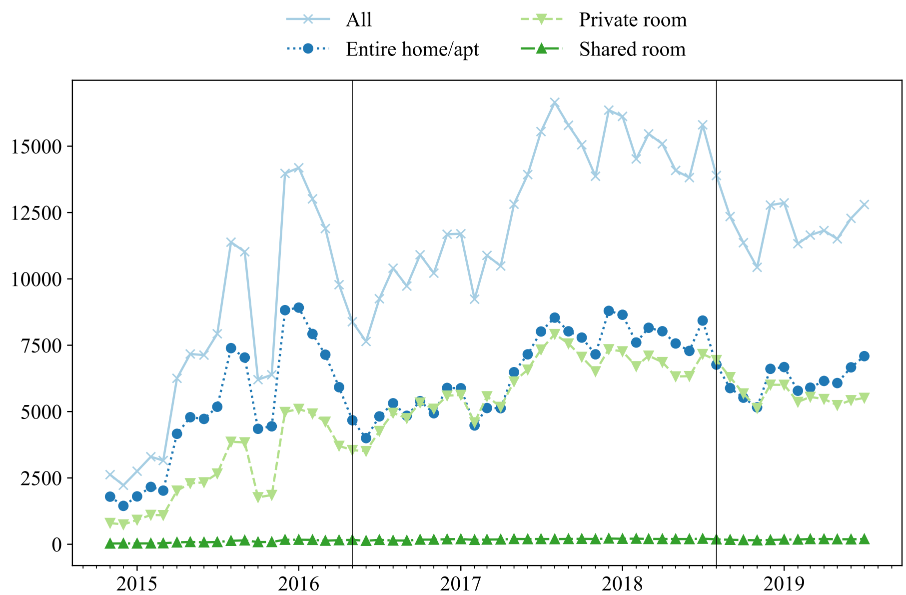
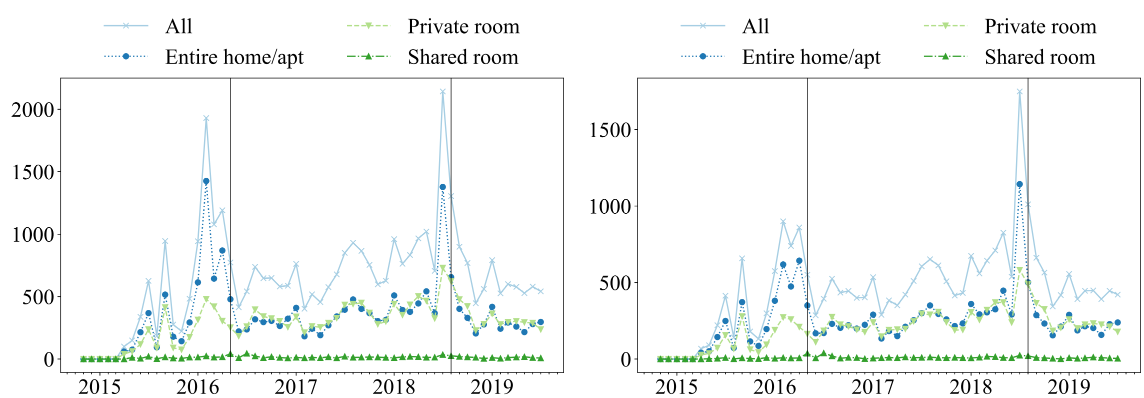


Fig. 3. Number of Airbnb listings in Berlin over time by listing type.



(a) Listing exits

(b) Host exits

Fig. 4. Airbnb exits in 2016 and 2018 by listing type.

listings as commercial if they earned more than the average rent of long-term rentals with a similar number of rooms located in the same area over the LTM prior to each month.

Figs. 5(a) and 5(b) reveal that the 2016 reform is characterized by a higher share of commercial listings leaving Airbnb. While the total number of entire homes exiting the platform in 2018 is comparable to 2016, the fraction of commercial listings comprised in the total number of listings exiting the platform is substantially larger in 2016.

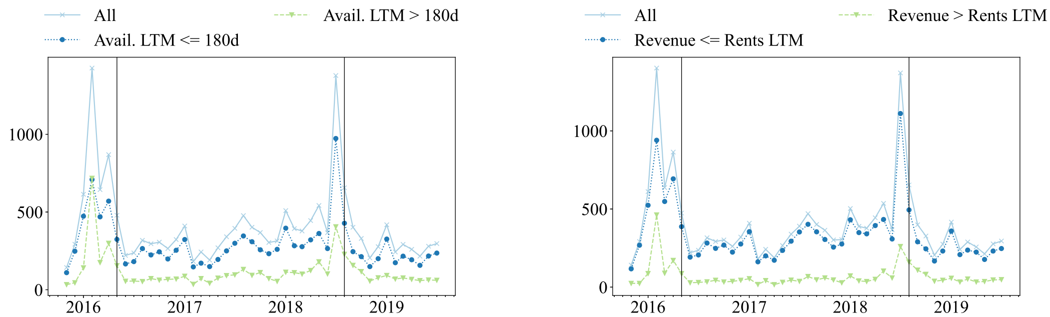
The graphical analysis of Figs. 5(a) and 5(b) provides evidence that the implementation of the policy reforms led to (i) sudden changes in Airbnb supply and (ii) that both reforms affected different types of Airbnb listings differently. The following subsection provides further evidence for these findings using a Probit regression framework.

Probit analysis

To further understand which types of listings left the platform prior to each policy date, we regress an exit dummy variable for each entire home listed on Airbnb on a constant, as well as either one of our two proxies for commercial listings: a dummy variable that indicates if the listing revenue was higher than the average rent of similarly-sized apartments in the same area in the LTM, or a dummy variable that indicates if the listing was available for more than 180 days in the LTM. We use Probit regressions and run the analysis separately for each month.

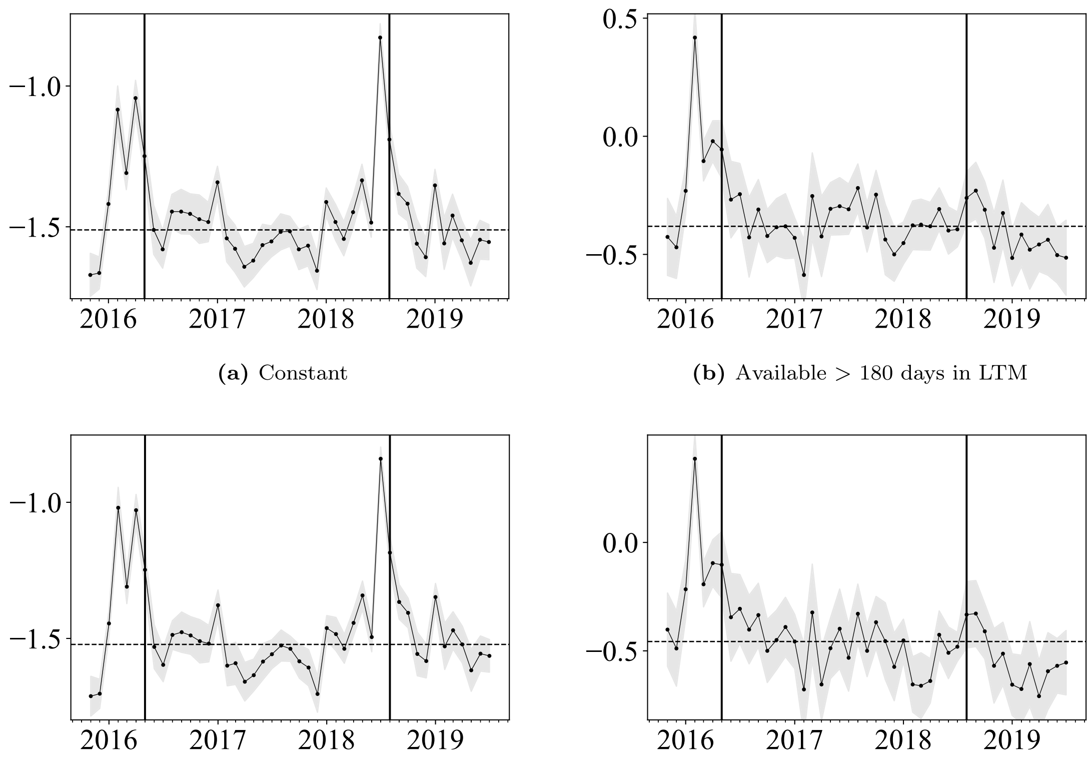
Fig. 6 reports the estimated coefficients for each month. Figs. 6(a) and 6(b) report the estimates for the regressions using a constant and the high-availability dummy variable. Figs. 6(c) and 6(d) report the estimates for the regressions using a constant and the high-revenue dummy variable. The estimated coefficients for the constants in Figs. 6(a) and 6(c) show a substantial increase in exit probability across all Airbnb listings (non-commercial and commercial) prior to both policy dates, with more anticipation prior to the 2016 reform. Additionally, Figs. 6(b) and 6(d) suggest that only the first policy had a substantial additional effect on the number of commercial listings leaving the platform.

Fig. 7 further confirms this distinction between the effects of the two policy changes. Fig. 7 reports the corresponding marginal effects, estimated at mean covariate values, for the two different proxy measures we use for commercial listings. It shows a striking difference in the impact of the two policies. While, generally, commercial listings have a smaller exit probability, the May 2016 reform causes their exit probability to increase, such that they are more likely to exit than non-commercial listings in the three-months window prior to the 2016 reform. By contrast, for the August 2018 reform, the relative probability



(a) Available > 180 days in LTM   **(b)** Listing revenue > comparable rents LTM

Fig. 5. Airbnb exits of entire homes split by commercial and non-commercial listings.
Note: Listings are classified as commercial based on Airbnb data for the 12 months prior to each focal month. Because the Airbnb sample starts in November 2014, we only show these plots starting from November 2015, a full year after our first Airbnb observation.



(a) Constant   **(b)** Available > 180 days in LTM

(c) Constant   **(d)** Listing revenue > comparable rents LTM

Fig. 6. Probit coefficient estimates.

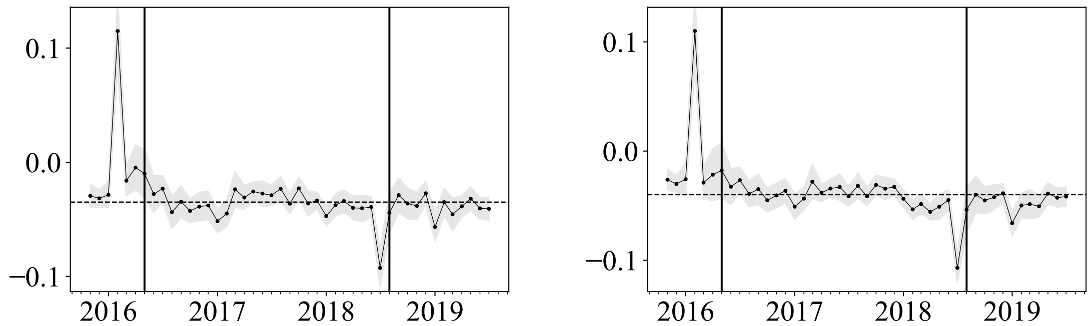
of exit for commercial listings decreases compared to that of non-commercial listings. This provides further evidence for the asymmetric impact of both policy reforms on commercial and non-commercial listings.

Thus far, the evidence suggests that a substantial number of listings exit Airbnb around both policy changes. Moreover, in 2016, a large portion of these exits accrue to what we argue are more likely to be commercial listings. This observation is relevant for our further analysis because 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 commercial Airbnb listings have a stronger impact on rents.

We conclude by noting that, while we have no direct information on the motives behind a host's decision to be active on Airbnb, we find strong evidence that the May 2016 was more successful at reducing commercial short-term renting on the Airbnb platform. Instead, the August 2018 reform, which introduced the mandatory registration number display, mostly affected non-commercial listings. One explanation might be that the small (mostly non-financial) costs to obtain such a registration number is more likely to be prohibitive for hosts who only occasionally rent out their apartments. Our hypothesis is that the exit of non-commercial Airbnb listings is less likely to result in an increase in long-term rental supply because it may just consist of, for example, people who stop offering their apartments on Airbnb while they are away on holiday.

3.3. Policy impact on the rental market

If commercial Airbnb listings are the ones most likely to be converted to long-term rental supply after exit, the 2016 reform should have had a more pronounced impact on rental market outcomes. In this subsection, we investigate whether our data support this hypothesis by assessing the policies' impact on long-term rental supply and prices.



(a) Available $>$ 180 days in LTM (b) Listing revenue $>$ comparable rents LTM

Fig. 7. Probit marginal effects.

(a) 2016 (b) 2018

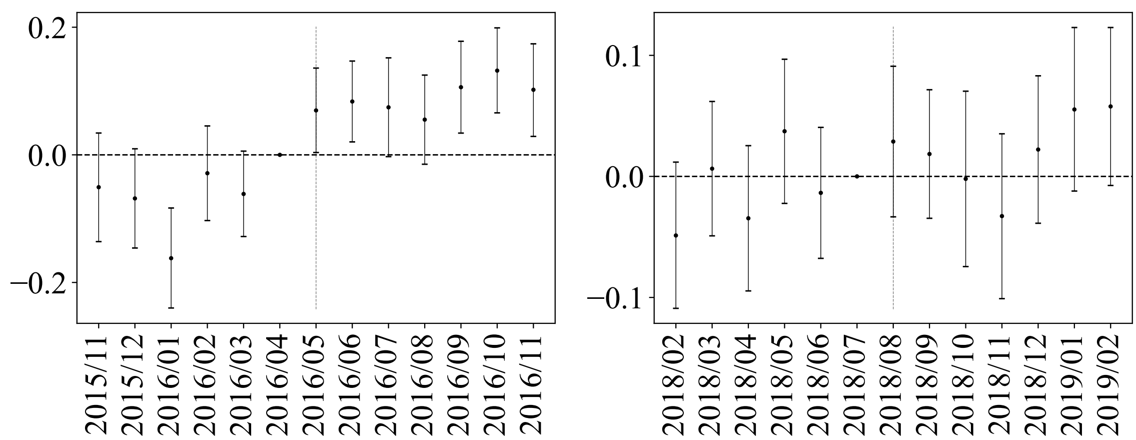


Fig. 8. Impact of policy changes on long-term rental supply. Analysis on the block-month level. Block-level fixed effects regression with postal-code-specific linear time trends. Points show point estimates of month-year fixed effects interacted with an indicator that is one if there were any entire homes present on Airbnb in a block in January before each respective policy change. Bars show 95% confidence intervals. Standard errors are clustered at the postal code level.

Methodology

We adopt a difference-in-differences type of approach in which we define treated units as units with Airbnb exposure in the first month of the year in which the policies take effect. For example, for the May 2016 reform we define a unit as treated if we observe at least one Airbnb listing in its vicinity. More precisely, we estimate equations of the following form:

$$y_{it} = \mathbb{1}(abb_{i1} > 0) * \mathbb{1}(t \geq \tau) + x'_{it}\delta + \mathbb{1}_i(g) * t + \mathbb{1}_t + \epsilon_{it}, \quad (1)$$

where y_{it} is either the number of long-term rentals in block i in month t or the asked rent per square meter of rental i listed in month t. $\mathbb{1}(abb_{i1} > 0)$ denotes the treatment dummy and $\mathbb{1}(t \geq \tau)$ the post-policy dummy. The sign $*$ describes the non-interacted and interacted terms of two variables.[21] The interaction between the treatment and the post-policy dummy represents the treatment effect. x_{it} denotes a set of time varying control variables, and $\mathbb{1}_i(g) * t$ denotes the full interaction (non-interacted and interacted terms) between time trends and geography-specific fixed effects. $\mathbb{1}_t$ denotes month-year fixed effects. The definition of the geography varies depending on the analysis we perform. We conduct the analysis using samples including the six months before and after each policy change.[22]

Eq. (1) tests whether units with pre-policy exposure to Airbnb experience a differential change in rental supply and rents compared to areas with no pre-policy exposure to Airbnb, after the policy came into effect. We estimate Eq. (1) for three measures of Airbnb exposure: all Airbnb entire homes, commercial listings defined through availability over the LTM, and commercial listings defined through revenue over LTM. We proceed by first presenting the results for rental supply and then the results for rents.

Policy impact on long-term rental supply

To analyze how the reforms affected the supply of long-term rentals in the city, we conduct an analysis in which each observation is a city-block in a given month. We define the relevant geography g in Eq. (1) as the city-block. Hence, we have $i = g$ in Eq. (1) and estimating it effectively amounts to implementing a panel-data regression with individual-specific fixed effects and individual-specific linear time

[21] For example, $\mathbb{1}(abb_{i1} > 0) * \mathbb{1}(t \geq \tau)$ denotes the full interaction between the treatment dummy and the post-policy dummy. Hence, it potentially includes a dummy for treatment units, a dummy for the post-policy time period, and a dummy for the interaction between these two variables (treated units in the post-policy period). Note that the non-interacted terms might drop out due to collinearity. For instance, the post-policy dummy is collinear with month-year fixed effects, when these are included in the analysis.

[22] We restrict our analysis to these short windows around the policy changes to mitigate the impact of potential underlying unobserved drivers of rental market outcomes. Appendix A illustrates why this is likely important in this setting.

Table 2
Policy impact on rentals.

2016	All	High availability	High revenue
Post dummy x Exposure dummy	0.086* [0.020; 0.152]	0.096** [0.030; 0.163]	0.088* [0.004; 0.172]
N	59,072	59,072	59,072
2018	All	High availability	High revenue
Post dummy x Exposure dummy	0.001 [−0.057; 0.058]	−0.017 [−0.079; 0.046]	−0.026 [−0.100; 0.047]
N	68,631	68,631	68,631

Notes: Block-month level analyses controlling for block fixed effects, month-year fixed effects, and block-specific linear time trends. Standard errors are clustered at the postal code level. The estimation is implemented following Correia (2016) to deal with the large number of fixed effects and trends. The square brackets show 95 percent confidence intervals. *, **, *** indicate five, one, 0.1 percent significance.

trends. Table 2 shows the results of estimating Eq. (1) for both samples using the different measures of Airbnb exposure.

The results reported in the first panel of Table 2 indicate that, in blocks exposed to at least one Airbnb entire home in January 2016, there are around 0.1 additional rentals listed from May 2016 onwards if compared to blocks with no Airbnb exposure. The results do not vary substantially with the measure of Airbnb exposure used, which is likely a result of the high correlation between the different variables. Fig. 8(a) shows the corresponding event-study plot for the 2016 reform when measuring exposure using all entire homes on Airbnb[23]: There is no indication for differential pre-trends prior to the reform, and the results reveal a sudden increase in rental supply starting from the month the reform took effect. The event study results when using the measures of Airbnb exposure based on availability or revenue are presented in Appendix C. We do not observe pre-trends in these specifications either.

The second panel of Table 2 and Fig. 8(b) show no clear impact of the 2018 policy change. The contrast between the 2016 and 2018 reform is consistent with the results shown in Fig. 7 and the hypothesis that mostly commercial Airbnb listings substitute back to the long-term rental market. While the 2016 reform resulted in substantial exits of commercial Airbnb listings, the 2018 reform mostly affected non-commercial ones and, therefore, did not lead to a significant increase in the supply of rentals.[24]

Policy impact on rent

To evaluate the impact of the reforms on asked rents in the city, we perform the analysis detailed in Eq. (1), using each rental property in the month it was first listed as the unit of observation. Because this data set is a repeated cross-section of properties listed in Berlin, we can no longer absorb block-level fixed effects. Instead, we define the relevant geography g as the postal code area in which rental i is located. Importantly, this approach allows us to explicitly control for apartment characteristics such as the size or number of rooms, which are relevant in determining rents. As our treatment measure abb_{i1}, we use an indicator variable that is equal to one if there were any entire homes listed on Airbnb within 250 m of rental i in January of the year of each respective policy change. To control for geography-specific time-trends and differences in those trends depending on Airbnb exposure, we include postal-code-specific time trends which we further interact with the treatment indicator for Airbnb exposure.

Table 3 and Fig. 9 report the estimated treatment effects and the corresponding event-study analysis for the 2016 and 2018 reforms. The results show no significant impact of the policy changes on asked rents in either of the samples. This contrasts with the significant increase in rental supply we find following the 2016 reform (see Fig. 8(a)) suggesting that the approach utilized herein might be inadequate for identifying price effects. The difference-in-differences method merely evaluates whether rentals in areas more likely to be affected by the reform became conditionally less expensive, instead of associating the exact variation in Airbnb caused by the reform with rent price changes. If price changes resulting from variations in Airbnb supply are subtle, this indirect approach may not detect them. One reason price changes might be subtle is that not only supply-side changes, but also the elasticity of demand play a role in determining equilibrium price adjustments. For example, with larger price elasticity, we would expect price effects of the policy to be smaller.

In the following section, we present our methodology for estimating the marginal impact of Airbnb on rental supply and rents. This approach maps the policy-induced variation in Airbnb listings more directly to the observed conditional variation in rental market outcomes. As a result, it has greater potential to identify a relation between Airbnb exposure and rents in particular.

4. Marginal impact of Airbnb on rental outcomes

The analysis in Section 3 confirms that both policies had a substantial effect on Airbnb supply with a differential impact on commercial Airbnb listings. Additionally, it suggests that only the 2016 reform had a meaningful impact on the average rental supply in geographies with pre-reform Airbnb exposure. In this section, we use these insights to estimate the marginal effect of Airbnb supply on rental supply and prices. We begin by discussing our identification strategy. Subsequently, we present our results, first for rental supply and then for rents. We particularly focus on how the estimated marginal effects change when we focus on commercial Airbnb listings instead of Airbnb listings in general.

4.1. Identification strategy

The fundamental challenge in estimating the impact of Airbnb on rental supply and prices is that there may be unobserved factors that affect both the popularity of an area on Airbnb and its popularity among residents. More specifically, typical concerns are unobserved geography-specific factors that are cross-sectionally correlated with rental outcomes and Airbnb supply, and which might also result in differential time-trends in rental outcomes and Airbnb supply. For example, in Appendix A, we provide evidence that areas with a larger Airbnb presence in the beginning of our sample are on a systematically steeper rent-price trend.

We propose an IV approach in which we instrument for Airbnb presence using a difference-in-differences equation similar to that reported in Section 3.3. By focusing on the six months before and after each

[23] Note that we use postal-code specific time trends rather than city-block-specific time trends to prevent collinearity with the event study variables.

[24] In Appendix B, we provide results using longer time windows, which corroborate that the shock to rental supply was transitory. The results look similar for the other two measures of Airbnb exposure, as we show in Appendix C.

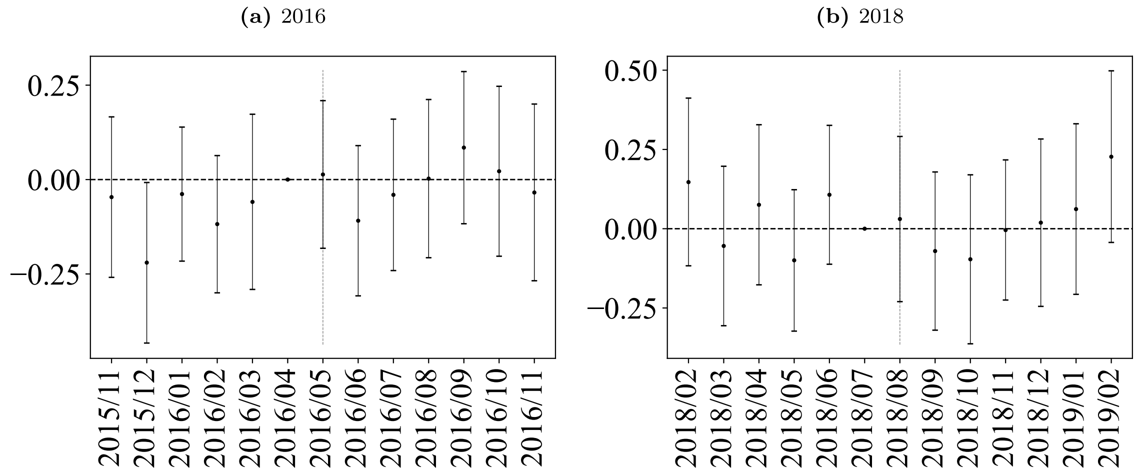


Fig. 9. Impact of policy changes on long-term rents. Analysis on the rental-month level. Points show point estimates of month-year fixed effects interacted with an indicator that is one if there were any entire homes present on Airbnb within a 250 m radius in January before each respective policy change. The estimation controls for postal code fixed effects, postal-code-specific linear time trends interacted with the Airbnb exposure indicator, and apartment characteristics. Bars show 95% confidence intervals. Standard errors are clustered at the postal code level.

policy change, we exploit short-term variation driven by the policy change, which assuages concerns related to longer-term dynamics. Furthermore, our instrument aims to de-correlate the policy-induced variation from unobserved factors that might explain both the short-term variation in Airbnb supply and the variable of interest. Compared to the difference-in-differences approach, the advantage of using an IV method is that it allows us to directly estimate and interpret the results as the marginal effect of Airbnb exposure on rental market outcomes.

The IV approach can be summarized as follows:

$$y_{it} = \alpha abb_{it} + x'_{it}\beta + \gamma_g \mathbb{1}_i(g) * t + \zeta_t \mathbb{1}_t + \epsilon_{it} \tag{2}$$

$$abb_{it} = \mathbb{1}(abb_{i1} > 0) * \mathbb{1}(t \geq \tau) + x'_{it}\delta + \kappa_g \mathbb{1}_i(g) * t + \eta_t \mathbb{1}_t + u_{it}\,. \tag{3}$$

The dependent variable in the second stage (Eq. (2)) is either the number of rentals or the rent per m^2 for the observation unit i located in geography g in month t. As in Section 3, the unit of observation i is either the block, for the rental analysis, or the individual property in the rent analysis. The coefficient of interest, α, measures the marginal effect of Airbnb exposure, abb_{it}. The time-varying control variables are denoted by x_{it}. The geographical fixed effect of location g in which observation i is located is denoted by $\mathbb{1}_i(g)$, t denotes a linear time trend, and $\mathbb{1}_t$ denotes month-year fixed effects.[25]

In the first stage (Eq. (3)), we instrument the measure of Airbnb exposure by interacting a dummy-variable for Airbnb exposure in January of the policy year, $\mathbb{1}(abb_{i1} > 0)$, with a dummy-variable for the time-periods in which the reform is in effect, $\mathbb{1}(t \geq \tau)$, where τ denotes the month in which the respective policy takes effect. The first stage can be interpreted as a difference-in-differences estimator for the effect of the policy on Airbnb variation. As such, and for the sake of exposition, we will refer to the instrument, i.e., the interaction between the dummy for pre-policy Airbnb exposure and the post-policy dummy, as the treatment effect.

The inclusion of geography-specific fixed effects absorbs unobserved cross-sectional factors typically associated with an upward bias in α, such as the attractiveness of a specific geography, which is positively correlated with abb_i and y_{it}. After controlling for geography fixed effects, the main variation within geographies stems from the policy change, which induces a drop in abb_{it}. Therefore, when using geography fixed-effects, the main identification concern stems from unobserved factors correlated with the drop in abb_{it} and the outcome variable. For example, if geographies with a larger policy-induced drop in Airbnb exposure are also on a steeper trend in rent-prices, then α is likely downward biased in magnitude, because the trends attenuate the rent decreases in geographies where the drop in abb_{it} is larger. The inclusion of granular geography-specific time trends in $\mathbb{1}_i(g) * t$ addresses such concerns.

Conditional on these geography-specific fixed effects and time tren ds, the relevance of our instrument depends on sufficient within-geography variation in Airbnb exposure that goes beyond linear time trends and that is correlated with the taking-effect of the policies. The relevance of our instrument stems from the fact that the drop in Airbnb exposure induced by the policy is correlated with whether a geography had any Airbnb exposure at the beginning of the calendar year, $\mathbb{1}(abb_{i1} > 0)$.

Arguably, there are two concerns regarding the validity of our instrument. First, the timing of the policies might be endogenous because they were introduced in response to increasing rents. This concern essentially is a reverse causality concern in which trends in the rental market are driving the policy changes. Second, even conditional on geography-specific linear trends and fixed effects, there may be unobserved factors that result in rental outcomes developing differently over time in areas with higher compared to lower Airbnb variation due to the reform.

We address the reverse causality concern through decisions in our research design. Recall that we focus on six-months windows around each policy change. This short-term perspective reduces concerns regarding reverse causality: While the law was introduced in Berlin to dampen long-term upwards pricing pressure on the rental market, the exact timing of the policy changes within these 13-months time windows should not be driven by short-term variation in rents. Especially for the May 2016 reform, the law and date were determined more than two years prior to implementation, hence, within these short-term windows, the timing of the law can be considered quasi-random. A related concern might be that these policies were introduced because of the particular pressures on the rental market in Berlin, but not in other cities. However, since our analysis exclusively focuses on Berlin, such concerns are related to the external validity of our results, and not to the validity of our instrument. In the context of our analysis, the question of the validity of our instrument is restricted to concerns related to endogeneity issues within the city of Berlin.

We address the second concern about differential conditional variation in rental market outcomes in areas with higher compared to lower pre-policy Airbnb exposure through our choice of instrument. We decide to use an indicator variable that is equal to one if there were any

[25] As in Eq. (1), the interaction terms $\mathbb{1}_i(g) * t$ indicate that we are controlling for geography-specific fixed-effects and time-trends.

Table 3
Policy impact on rents.

2016	All	High availability	High revenue
Post dummy x Exposure dummy	0.033 [−0.078; 0.143]	−0.026 [−0.142; 0.090]	0.035 [−0.098; 0.169]
N	41,159	41,159	41,159
2018	All	High availability	High revenue
Post dummy x Exposure dummy	−0.033 [−0.176; 0.109]	−0.09 [−0.303; 0.122]	−0.022 [−0.190; 0.146]
N	39,696	39,696	39,696

Notes: Analysis on the rental-month level controlling for postal code fixed effects, postal-code-specific linear time trends interacted with the Airbnb exposure indicator, and apartment characteristics. Standard errors are clustered at the postal code level. The estimation is implemented following Correia (2016) to deal with the large number of fixed effects and trends. The square brackets show 95 percent confidence intervals. *, **, *** indicate five, one, 0.1 percent significance.

nearby entire homes listed on Airbnb in January of the respective policy year. Compared to a continuous measure of pre-policy Airbnb exposure, this binary variable is mechanically de-correlated from unobserved factors that might be conditionally correlated with the Airbnb variation within the set of geographies with pre-policy Airbnb exposure, i.e., the *intensive* margin of pre-policy Airbnb exposure.

Thus, the validity of the instrument hinges on whether it is conditionally uncorrelated with unobserved factors that affect rental market outcomes and that are correlated with the *extensive* margin of pre-policy Airbnb exposure. This appears defensible, in particular in light of the fact that our IV approach controls for the potential correlation between pre-policy Airbnb exposure and geography-specific unobserved factors or time-trends by using granular fixed effects and linear time trends. The fact that Figs. 8 and 9 suggest parallel trends in rental outcomes prior to the policy dates further mitigates endogeneity concerns related to unobserved differences correlated with the instrument.[26] As in any quasi-experimental settings, it is not possible to rule out remaining endogeneity issues with certainty. However, we are confident that our approach effectively addresses the main endogeneity concerns outlined above. The described IV approach forms the backbone of our identification strategy. The nature of the two dependent variables of interest, the rental supply and the rent prices, requires adjustments to the general approach outlined here. We detail any changes in the subsequent presentation of the respective results. We start by discussing our results for rental supply and then proceed with the rent prices.

4.2. The impact of Airbnb on rental supply

To identify the causal effect of one additional Airbnb on rental supply, we rely on the IV strategy described in Eqs. (2) and (3). Each unit i represents a city-block and, thus, the data set is a panel of blocks over time. The dependent variable is the number of rentals in block i in month t, and the explanatory variable of interest is a measure of Airbnb listings in block i in month t. As our instrument, we use an indicator variable that is equal to one if there was at least one entire home listed on Airbnb in block i in the month of January preceding the respective policy change and interact it with a policy dummy that is one in time periods in which the respective policy is in effect.[27]

We control for block fixed effects, block-specific linear time trends, and month-year fixed effects. In terms of Eqs. (2) and (3), this analysis corresponds the unit of observation being at the same level as the geography fixed effects and time trends, such that $i = g$. Accordingly, the pre-policy Airbnb exposure is absorbed by the block-specific fixed effects. Furthermore, the non-interacted policy dummy is absorbed by the time fixed effects. In contrast to the general approach outlined in Section 4.1, we do not include any additional time-varying control variables x_{it}, as the rental characteristics are not defined in areas in which we do not observe and rentals.

The results of our analysis of rental supply are reported in Table 4. The upper panel focuses on the six months before and after the implementation of the law in May 2016, while the lower panel focuses on the six months before and after the reform in August 2018. For each type of Airbnb listing used to measure Airbnb exposure, we first present the OLS estimates and then report the corresponding IV estimates. The first two columns show the results obtained when using all Airbnb entire homes as a measure for Airbnb exposure, the middle and last two columns show the results for the two proposed proxy measures for commercial Airbnb listings: First, listings that are classified as commercial based on availability, and, second, listings that are classified as commercial based on revenue.

From the IV regression using all entire homes as the measure for Airbnb exposure in the 2016 sample, we estimate that, for each additional Airbnb listing in a given block, there are approximately 0.08 fewer apartments in that same block. This result suggests a limited degree of substitution between Airbnb and the long-term rental market. However, our expectation is that the estimated substitutability should be larger when focusing on commercial Airbnb listings. Indeed, according to the point estimates using the proxy-variables for the number of commercial Airbnb listings in a block, for each additional commercial Airbnb listing, there are 0.23 to 0.37 fewer long-term rentals.[28] Thus, for the 2016 sample, the estimates obtained for commercial listings are three to four times larger than the estimates obtained using all Airbnb listings.

The coefficients obtained from the OLS regressions appear to be downward biased. This indicates the presence of unobserved factors that are negatively correlated with the number of rentals and positively correlated with the number of Airbnb listings that leave the platform

[26] Indeed, one of the arguments made by Airbnb hosts challenging the law in court was that its introduction was too widespread and included areas where there was no significant pressure on the rental market (see https://www.zeit.de/politik/deutschland/2016-06/berlin-gesetz-wohnraum-illegal (last accessed: December 21, 2023)). This provides anecdotal evidence that pre-policy Airbnb exposure might not necessarily only include geographies that experienced strong rent price increases.

[27] To harmonize the treated units, we keep the instrument constant across specifications, i.e., we always use pre-policy exposure to all entire home Airbnb listings as the instrument, also when, for example, we estimate the marginal effect of high revenue Airbnb listings. By choosing this approach, we use a broad definition of treated units. This guarantees that each observation with a commercial Airbnb listing – according to either definition – is always counted as treated. In Appendix E.1, we show the results we obtain when the instrument corresponds to the explanatory variable of interest. The results are not sensitive to this choice.

[28] There are several reasons why the measured substitutability is not perfect. For one, there are transaction costs to enter the long-term rental market that might imply a delayed effect. Additionally, there are other rental platforms for furbished apartments that might offer a viable alternative to Airbnb. Finally, former Airbnb hosts might decide to sell their properties rather than enter the rental market.

Table 4
Impact of Airbnb on rental supply.

	2016 Policy					
	Entire Homes		High Availability		High Revenue	
	OLS	IV	OLS	IV	OLS	IV
Second stage						
Airbnb Exposure	−0.019*	−0.081*	−0.010	−0.234*	−0.010	−0.370*
	[−0.036; −0.003]	[−0.144; −0.018]	[−0.032; 0.013]	[−0.416; −0.051]	[−0.041; 0.021]	[−0.659; −0.080]
First stage						
Treatment Effect		−1.053***		−0.366***		−0.232***
		[−1.242; −0.864]		[−0.439; −0.294]		[−0.281; −0.182]
N	59,072	59,072	59,072	59,072	59,072	59,072
First-Stage F-Stat		119.7		99.10		84.44
	2018 Policy					
	Entire Homes		High Availability		High Revenue	
	OLS	IV	OLS	IV	OLS	IV
Second stage						
Airbnb Exposure	−0.017*	−0.001	−0.013	−0.003	0.002	−0.004
	[−0.032; −0.003]	[−0.124; 0.122]	[−0.039; 0.012]	[−0.339; 0.333]	[−0.023; 0.027]	[−0.497; 0.489]
First stage						
Treatment Effect		−0.467***		−0.171***		−0.116***
		[−0.588; −0.345]		[−0.220; −0.121]		[−0.150; −0.082]
N	68,631	68,631	68,631	68,631	68,631	68,631
First-Stage F-Stat		56.63		45.18		44.83

Notes: Block-month level analyses controlling for block fixed effects, month-year fixed effects, and block-specific linear time trends. Standard errors are clustered at the postal code level. The estimation is implemented following Correia (2016) to deal with the large number of fixed effects and trends. The square brackets show 95 percent confidence intervals. *, **, *** indicate five, one, 0.1 percent significance.

because of the reform. To understand why this might be the case, remember that areas with a larger policy-induced Airbnb reduction tend to have a higher Airbnb baseline exposure prior to the reforms and, as Fig. 2(b) suggests, these are more likely to be areas in the city center. Thus, the unobserved factors are likely associated with proximity to the city center where hosting on Airbnb is likely more profitable and where the supply of new rental units might be scarcer. Thus, the direction of the bias is in line with our expectations.

For the 2018 policy reform, we find no significant effect using the IV methodology. This is consistent with the insight that the reform in 2018 only had a limited impact on the exit of commercial listings (see Section 3), which is the variation that we expect to be relevant to identify the effect of Airbnb on the rental market. Therefore, while the first stage indicates that the instrument remains relevant for predicting the change in Airbnb listings induced by the reform, we see that the magnitudes of the first-stage coefficients halves in comparison to their respective 2016 counterparts. One possible explanation for the null results may be that the instrumented variation is insufficient to identify the marginal impact when using data around the 2018 reform.

4.3. The impact of Airbnb on rents

To analyze the impact of Airbnb on rents, we use the individual rental as the unit of observation i. This allows us to straightforwardly control for a rich set of characteristics in x_{it}, such as the number of rooms, apartment amenities (balconies, garages, etc.) and neighborhood amenities (supermarkets, schools, etc.), which are likely to explain a substantial part of the variation in rent prices.[29] Furthermore, we use a rental-specific measure for Airbnb exposure by calculating how many Airbnb listings are within 250 m of the location of a rental i in month t. As the treatment indicator, we use a variable that is equal to one if there were any entire homes listed on Airbnb within 250 m of rental i in January of the respective treatment year.

At the level of the city blocks, we encounter the problem that we often do not observe repeated cross-sectional observations within the same block, which prevents us from reliably controlling for block-specific time trends. Therefore, we instead use postal codes to control for geography-specific fixed effects and time trends (i.e., g is defined as the postal code area of observation i). Because the treatment is defined on a different geographic level than the included geographic fixed effects (250 m radii versus postal codes, respectively), the non-interacted treatment variable (any entire homes on Airbnb within 250 m in January, $\mathbb{1}(abb_{i1} > 0)$), is no longer absorbed by the geographic fixed effects. To control for cross-sectional and time-varying differences between rentals with and without pre-policy Airbnb exposure within postal code areas, we include the pre-policy Airbnb exposure dummy variable in the second stage and interact it with postal-code fixed effects and linear time trends (i.e., we include $\mathbb{1}(abb_{i1} > 0) * \mathbb{1}_i(g) * t$ in Eq. (2)). As a result, the excluded instrument again only consists of the interaction between the policy dummy and pre-policy Airbnb exposure.[30]

To systematically select from 769 potential covariates,[31] 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.[32] In

[29] We choose not to use geographies such as the block as the unit of observation because it poses the non-trivial problem of rent price aggregation over heterogeneous rental types when several rentals are present in a geography. In addition, not observing any rental in a geography in a particular period leads to missing observations. The method proposed by Ahlfeldt et al. (2023) allows to create aggregate price indexes, taking into account various control variables. However, their approach projects prices over space and time, so in order to capture a potential policy impact, we would need to include a structural break at the time of the policy change. Instead, we opt to use the disaggregated data and include the potential policy impact in the first stage.

[30] As in the rental analysis where the non-interacted policy dummy is absorbed by the time fixed effects.

[31] These 769 variables include 49 property characteristics, 134 block-level types of points of interest, nine block-level pollution and noise variables, 13 month-year fixed effects, 188 postal-code fixed effects, 188 postal-code-specific linear trends, and 188 postal-code-specific linear trends interacted with the pre-policy Airbnb exposure.

[32] Arguably, the most import assumption for this estimator is that the true underlying model is "approximately sparse". This assumption requires that the

Table 5
Impact of Airbnb on rents.

	2016 Policy					
	Entire Homes		High Availability		High Revenue	
	PDS OLS	Lasso IV	PDS OLS	Lasso IV	PDS OLS	Lasso IV
Second stage						
Airbnb Exposure	0.037***	0.075***	0.099***	0.129***	0.119***	0.241***
	[0.030; 0.043]	[0.046; 0.104]	[0.081; 0.118]	[0.090; 0.169]	[0.097; 0.142]	[0.208; 0.274]
First stage						
Treatment Effect		−5.7***		−1.494***		−0.861***
		[−7.314; −4.086]		[−1.971; −1.017]		[−1.169; −0.553]
N	41,159	41,159	41,159	41,159	41,159	41,159
Selected Xs	74	154	61	202	65	209
First-Stage F-Stat		560.7		186.8		32.70
	2018 Policy					
	Entire Homes		High Availability		High Revenue	
	PDS OLS	Lasso IV	PDS OLS	Lasso IV	PDS OLS	Lasso IV
Second stage						
Airbnb exposure	0.044***	0.083***	0.086***	0.17***	0.102***	0.176***
	[0.036; 0.053]	[0.041; 0.126]	[0.072; 0.101]	[0.085; 0.255]	[0.084; 0.120]	[0.093; 0.259]
First stage						
Treatment Effect		−4.415***		−1.696***		−0.797***
		[−5.630; −3.199]		[−2.213; −1.178]		[−1.095; −0.499]
N	39,696	39,696	39,696	39,696	39,696	39,696
Selected Xs	76	157	64	185	56	192
First-Stage F-Stat		3223		1965		641.6

Notes: Rental-month level analyses. Regressions potentially include apartment characteristics, neighborhood characteristics, month-year fixed effects, the high-availability Airbnb exposure indicator, postal-code fixed effects, and postal-code-specific linear time trends interacted with the Airbnb exposure indicator. The estimation without instruments (columns with header "PDS OLS") uses the double-Lasso estimator proposed by Belloni et al. (2014). The estimation with instruments (columns with header "Lasso IV") uses the Chernozhukov et al. (2015) estimator. Standard errors are clustered at the postal code level. The square brackets show 95 percent confidence intervals. *, **, *** indicate five, one, 0.1 percent significance.

Appendix F, we present the set of variables selected by the double-lasso estimator for each specification.

Table 5 reports the results of the analysis focusing on rent prices. The upper panel shows the results for the 2016 sample, the lower panel the results for the 2018 sample. For both samples, the estimated marginal effect when using all entire homes as a measure of Airbnb exposure is approximately 8 cent per m^2. When focusing on commercial listings, the estimated effect on rent prices is between 1.6 to 3.1 times larger: One additional commercial Airbnb listing increases the square-meter rent by 13 to 24 cent.

Unlike for the rental regressions, the estimates using data around the 2018 reform are broadly consistent with those obtained for the 2016 reform, albeit being noisier. The first-stage estimates for both 2016 and 2018 are statistically indistinguishable, as are the second-stage estimates, which increases our confidence that we are identifying the causal marginal effect of Airbnb on rents. We offer two possible explanations for why we obtain similar estimates in both samples in the rents analysis, but not in the rental analysis. First, focusing on more disaggregated data – the individual property – allows us to directly control for characteristics relevant to explain variation in rent prices, which helps identifying the effect of Airbnb exposure. Second, the use of the double Lasso estimator also improves the performance of the estimator by automatically dropping irrelevant variables and thereby reducing noise and overfitting (Chernozhukov et al., 2015).

For each definition of Airbnb exposure and for both policy reforms, the OLS estimates appear to be downward biased. As discussed in Section 4.1, this could be explained by unaccounted-for time trends, which are associated with larger rent increases in areas where the policy leads to a larger reduction in Airbnb supply. The bias observed for rent prices is consistent with the bias observed for rental supply. Unobserved factors that lead to underestimate the magnitude of the effect of Airbnb on rental supply should also lead to underestimate the effect of Airbnb on rent prices: Underestimating the supply reduction caused by Airbnb will, *ceteris paribus*, lead to underestimate the increase in prices caused by the supply reduction.

The results from the rent price analysis are in line with what we observed in the previous section. It is the presence of commercial Airbnb listings that drives the impact of Airbnb on rents. With an average per square meter rent price of approximately ten euro, our estimates imply that the marginal effect of one commercial Airbnb leads to a square meter rent increase between 1.3% and 2.4%.

4.4. Robustness

In this subsection, we briefly discuss several robustness checks that we perform with respect to our rental and rents analyses.

Seasonality. While we control for geography-specific linear-time tren ds, a remaining concern is potential seasonality. Because we limit our samples to help identification, this prevents us from controlling for seasonality using seasonal or month fixed effects. To address seasonality, we deseasonalize the data using the entire time span available to us and use the deseasonalized residuals in our main analysis. In Appendix E.2, we show that our results are mostly robust to accounting for seasonality. The only results that change slightly are the marginal effects of Airbnb on rents in 2018, which are qualitatively the same, but with smaller effect sizes. This result could be driven by the fact that the 2018 policy did not induce the same degree of exit of commercial Airbnb listings as the 2016 policy did.

Rent analysis at the city-block level. In Appendix E.3, we present results in which we replicate the rent analysis using city-blocks as the basic unit of observation. As discussed in Section 4.3, we face a missing value problem for city-blocks in which we do not observe a rental at a specific point in time. As a result, we have an unbalanced panel. Additionally, using blocks as the unit of observation raises the question of how to

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, we refer to Appendix D.

aggregate rent prices and rental characteristics for blocks in which we observe more than one rental in a given month. We opt for using simple averages. The results at the city-block level are not significant at the conventional levels. However, it is worthwhile pointing out that a relative comparison between the point estimates would lead to the same conclusions as the one presented here. The primary difference lies in the less precise estimates obtained from the block-level analysis. As previously discussed, this might be due to the block-level analysis being less precise and efficient in utilizing the variation induced by the reform to assess the impact of Airbnb exposure on rents.

Radius for Airbnb exposure. When analyzing the relationship between Airbnb and rents, we use a radius of 250 m to count the number of Airbnb listings nearby each rental. This choice is somewhat arbitrary. To assess the sensitivity of the results in the rent price analysis, we conduct robustness checks using circle size of 500 m and 1000 m. Qualitatively, our results remain similar: Nearby Airbnb listings increase rents and the estimated marginal effect is larger when focusing on commercial Airbnb listings. Quantitatively, the effect sizes are smaller with larger circles. We note that this pattern is in line with further away Airbnb listings affecting rents less. We present the results in Appendix E.4.

4.5. Discussion

We provide evidence that hosts of commercial Airbnb listings appear to be homeowners who substitute away from the long-term rental market in an environment where short-term renting on Airbnb allows for generating income comparable or higher to conventional long-term renting. This behavior exerts an externality on the long-term rental market by reducing supply and increasing rents.

While the point estimate for the rental analysis show no effect for the 2018 reform, we find significant effects for rents. This discrepancy is likely due to the fact that the level of data aggregation used for the rent analysis allows to more directly control for factors relevant in explaining the dependent variable. Additionally, the smaller impact of the 2018 reform on rental supply is consistent with this reform having led to fewer commercial listing exits, which is also the likely reason for why the marginal effect of Airbnb listings on rent prices is less precisely estimated for the 2018 reform. Nonetheless, the finding that commercial listings are the main driver of rent increases is robustly established by our analysis.

We can gauge the relevance of the estimated 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.[33] According to our estimates, one additional commercial Airbnb listing increases asked square meter rents by 13 to 24 cents.

5. 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 limited causal evidence on the mechanisms underlying Airbnb's effect on rental markets as well as direct evidence on the impact of such regulations. Our study contributes to this discussion twofold.

First, it provides new evidence on the short-term effectiveness of policy interventions in Berlin, Germany, targeting short-term rentals and aiming at limiting the spread of platforms such as Airbnb. The taking effect of this regulation in 2016 and its amendment two years later had heterogeneous effects on the behavior of Airbnb listings. The 2016 reform resulted in significant exits of both commercial as well as non-commercial listings in the short run. The 2018 reform seems to have mostly resulted in non-commercial listings leaving the platform. This distinction is important as we would expect properties that are substituted away from the long-term rental market to engage on Airbnb as commercial listings. Therefore, these listings are more likely to represent a negative externality that leads to a reduction of long-term rental supply and an increase in rents. In line with this intuition, we find evidence that the 2016 reform resulted in an increase in rental supply, while the 2018 reform did not.

Second, we use these policy interventions as a source of quasi-exogenous variation to identify the causal effect of Airbnb on rentals and rents. Our results help to highlight the mechanism at play. Listings that are available for relatively long periods of time or earning high revenues from short-term renting – those we label commercial listings – crowd-out rental supply, which can increase rents in turn. According to our estimates, one additional nearby commercial Airbnb listing decreases the long-term rental supply by 0.2 to 0.4 units. In turn, this supply decrease seems to induce an increase in the average asked rent per square meter ranging between 13 to 24 cent, which corresponds to roughly 1.3 to 2.4 percent of the average square meter rent.

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 commercial short-term renting is key. By contrast, our results indicate that non-commercial short-term renting through Airbnb is less likely causing serious and significant negative externalities on the rental market. The latter arguably generates positive value both to the hosts and the guests — see, for example, Calder-Wang (2021) and Farronato and Fradkin (2022) for a quantification of the welfare gains generated by Airbnb. Thus, regulatory efforts should also acknowledge the benefits created by Airbnb through the reduction in capacity under-utilization.

In the specific case of Berlin, our results suggest that the registration number display introduced in August 2018 most likely had a substantial impact on non-commercial Airbnb hosts' decisions to leave the platform, but this impact did not translate into large changes in the rental market. Hence, if the policy objective is to lessen pressures on the rental market while allowing for welfare gains from short-term renting, the May 2016 reform was more successful than the August 2018 reform. A lesson for policymakers is to be wary of introducing small (non-financial) costs that may be prohibitive for non-commercial Airbnb hosts, but do not affect commercial hosts as much.

While we use two different and reasonable proxies for commercialism, more precise information on whether a listing is commercial or non-commercial would allow to more cleanly disentangle the differential effect of both types. Additionally, our analysis only identifies the short-term effects of Airbnb on the rental market. An identification of long-term effects would need to account for responses in housing supply and to control for the confounding effects of other policy events, such as the introduction of rent controls. Despite these limitations, the present study provides useful insights into which type of hosting activity affects the rental market most and which types of policies may be more or less suited to regulate these.

CRediT authorship contribution statement

Tomaso Duso: Writing – review & editing, Writing – original draft, Visualization, Resources, Methodology, Funding acquisition, Conceptualization. **Claus Michelsen:** Writing – review & editing, Writing – original draft, Resources, Conceptualization. **Maximilian Schaefer:** Writing – review & editing, Writing – original draft, Visualization, Validation, Software, Methodology, Investigation, Formal analysis, Conceptualization. **Kevin Ducbao Tran:** Writing – review & editing, Writing – original draft, Visualization, Validation, Software, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

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

Declaration of competing interest

None.

Data availability

The authors do not have permission to share data.

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Appendix A. Evidence for long-term trends

To illustrate the need to restrict the analysis to relatively short time windows around the policy changes, we conduct an analysis inspired by the main specification used in Huber et al. (2021). We first need a measure of pre-treatment exposure to Airbnb. For this purpose, we count the number of entire homes in each block, as of February 2016. To account for the different sizes of the blocks, we normalize these counts by the block area to get an Airbnb density per square kilometer. For block 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 rental outcomes between areas with lower and higher Airbnb density in February 2016 changes over time. This regression amounts to estimating the following equation:

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

Each observation is one block in a given quarter. y_{iq} is either the number of long-term rentals per square kilometer or the average asked rent per square meter. $abb(Feb2016)_i$ denotes the number of entire homes per square kilometer in city-block 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 Fig. A.10, we report the estimates of β_τ for all quarters. These estimates capture the difference in rental outcomes between blocks with higher and lower Airbnb density in February 2016, conditional on the fixed effects.

Fig. A.10 reports the coefficient estimates for the quarterly dummies interacted with the number of entire homes per square kilometer on Airbnb in each block in February 2016. These coefficients represent the partial correlation between Airbnb entire home density in February 2016 and the number of rentals per square kilometer and the asked rents over the entire period for which we have rent data. The results illustrate that blocks with higher Airbnb entire home density in February 2016 experience systematically steeper trends in rental supply and rent prices than areas with lower Airbnb density in February 2016.

The results represented in Fig. A.10 highlight a fundamental difficulty in identifying the causal impact of Airbnb on rental outcomes. 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.

Appendix B. Event studies with longer samples

See Figs. B.11 and B.12.

Appendix C. Event studies: other treatment variables

C.1. Rental supply

See Figs. C.13 and C.14.

C.2. Rents

See Figs. C.15 and C.16.

Appendix D. Double Lasso estimator

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

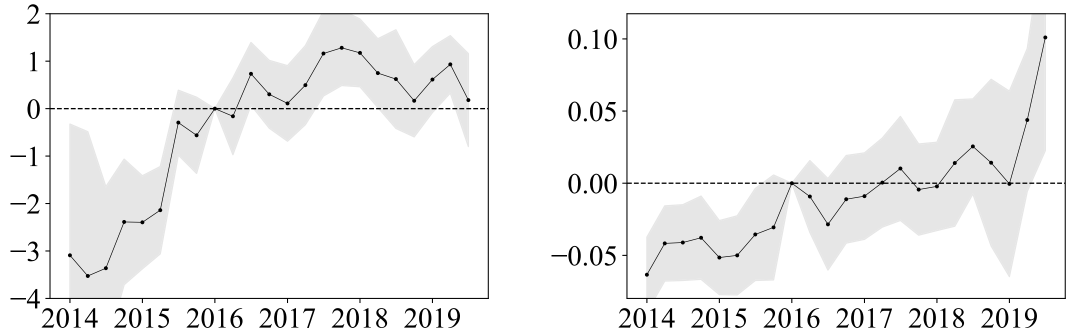
$$E[(\tilde{\rho}^y_{it} - \tilde{\rho}^{abb}_{it}\alpha)\tilde{v}_{it}] = 0\,, \tag{D.1}$$

where $\tilde{\rho}^y_{it} = y_{it} - x'_{it}\theta$, $\tilde{\rho}^{abb}_{it} = abb_{it} - x'_{it}\vartheta$, and $\tilde{v}_{it} = x'_{it}\delta + \gamma_0\mathbb{1}(t \geq \tau) + \sum_{l=1}^{3}\gamma_l\mathbb{1}(t=\tau-l) - x'_{it}\vartheta$.

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 $\tilde{v}_{it}$ 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 (adapt ed for our setting):

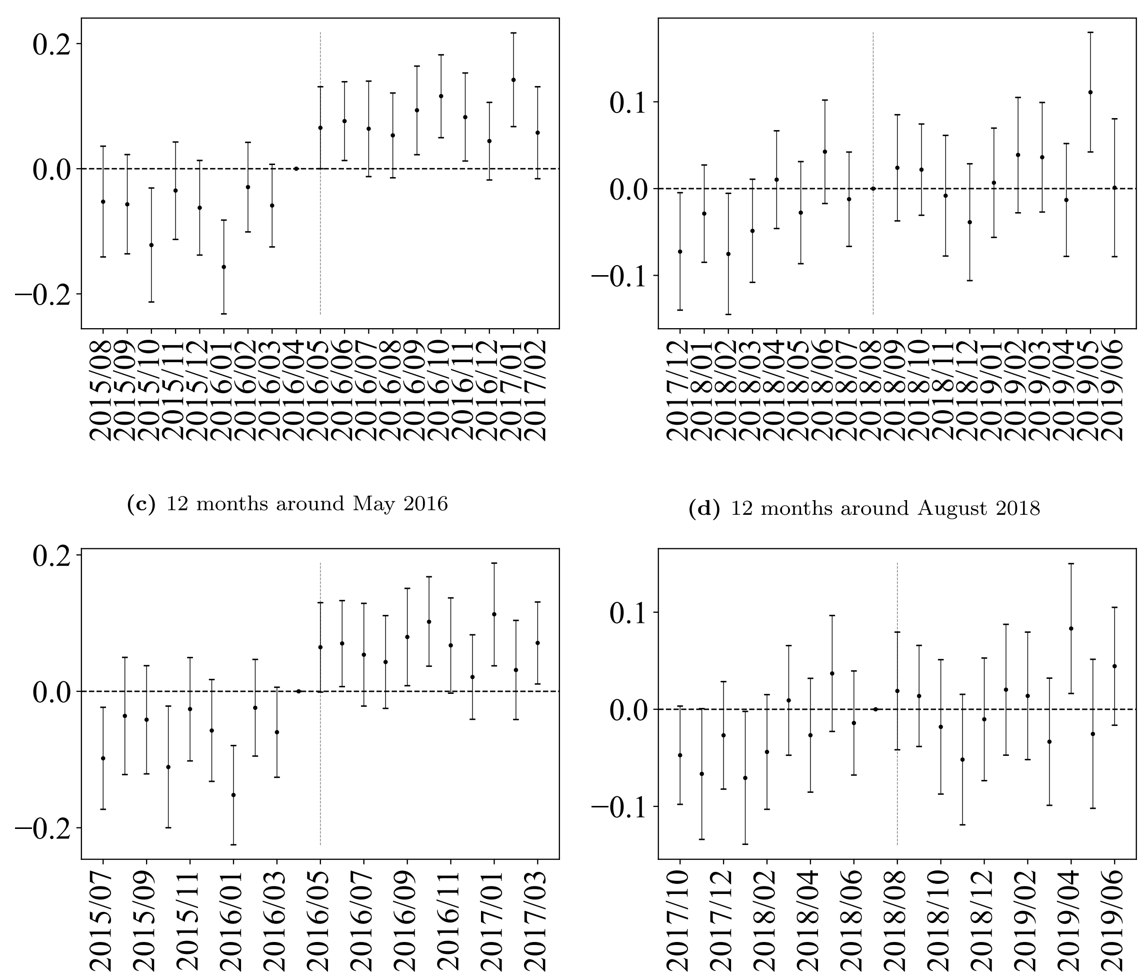
1. Conduct a first-stage regression of abb_{it} on the policy dummies and x_{it} and denote the corresponding coefficients as $\hat{\gamma}$ and $\hat{\delta}$. Obtain predicted Airbnb counts using $\hat{abb}_{it} = \hat{\gamma}_0\mathbb{1}(t \geq \tau) + \sum_{l=1}^{3}\hat{\gamma}_l\mathbb{1}(t=\tau-l) + x'_{it}\hat{\delta}$.
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 $\hat{\vartheta}$.
4. Calculate $\hat{\rho}^y_{it} = y_{it} - x'_{it}\hat{\beta}$, $\hat{\rho}^d_{it} = \mathbb{1}(t \geq \tau) + \sum_{l=1}^{3}\mathbb{1}(t=\tau-l) - x'_{it}\hat{\vartheta}$, and $\hat{v}_{it} := \hat{\gamma}_0\mathbb{1}(t \geq \tau) + \sum_{l=1}^{3}\hat{\gamma}_l\mathbb{1}(t=\tau-l) + x'_{it}\hat{\delta} - x'_{it}\hat{\vartheta}$. Use IV regression of $\hat{\rho}^y_{it}$ on $\hat{\rho}^d_{it}$ using $\hat{v}_{it}$ as an instrument to obtain $\hat{\alpha}$.



(a) Rental supply

(b) Rent prices

Fig. A.10. Partial correlation between February 2016 Airbnb density and rental market outcomes.



(a) 9 months around May 2016

(b) 9 months around August 2018

(c) 12 months around May 2016

(d) 12 months around August 2018

Fig. B.11. Impact of policy changes on long-term rental supply. The specifications are identical to those reported in Fig. 8, except that the samples used are longer.

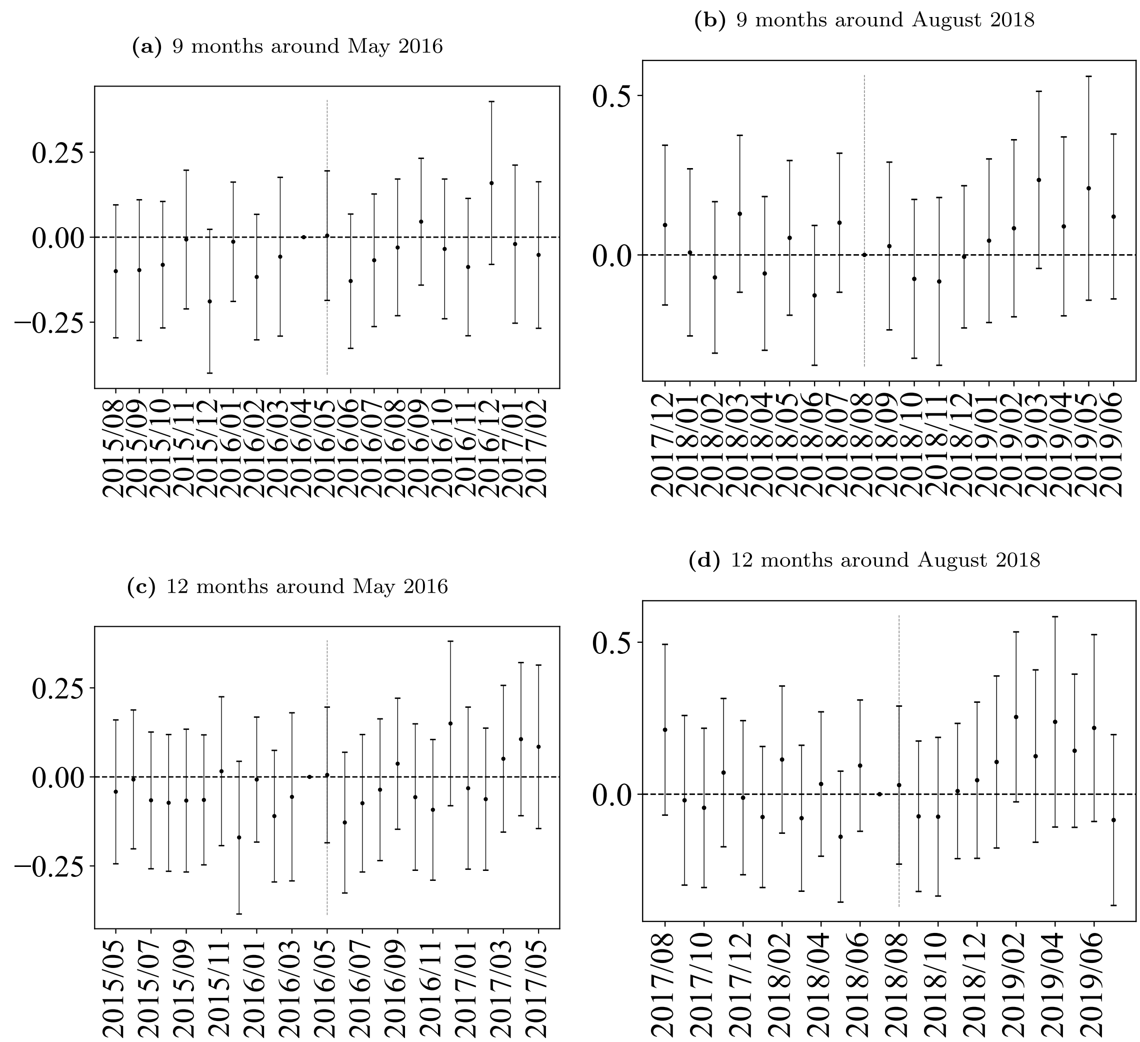


Fig. B.12. Impact of policy changes on long-term rents. The specifications are identical to those reported in Fig. 9, except that the samples used are longer.

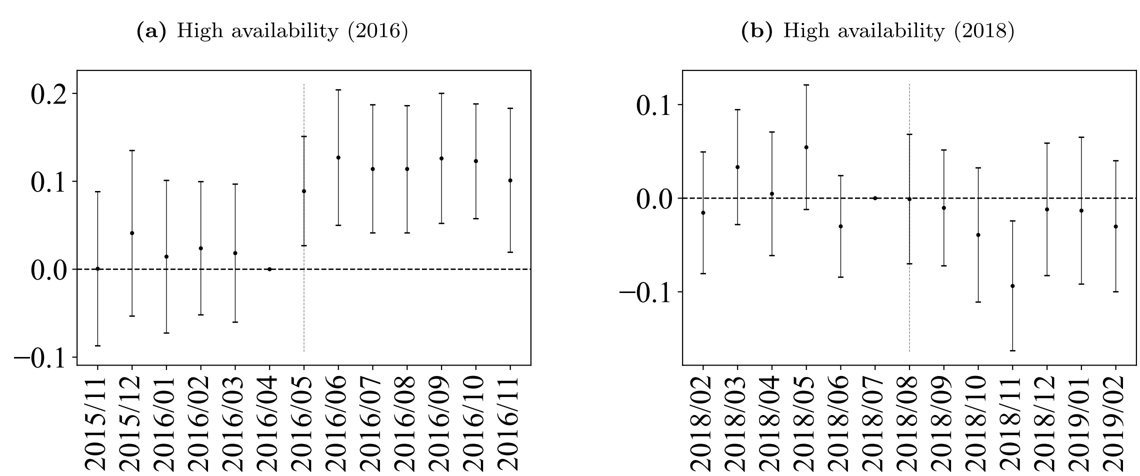


Fig. C.13. Impact of policy changes on long-term rental supply. The specifications are identical to those reported in Fig. 8, except that the points show point estimates of month-year fixed effects interacted with an indicator that is one if there were any *high-availability* Airbnb listings present in a block in January before each respective policy change.

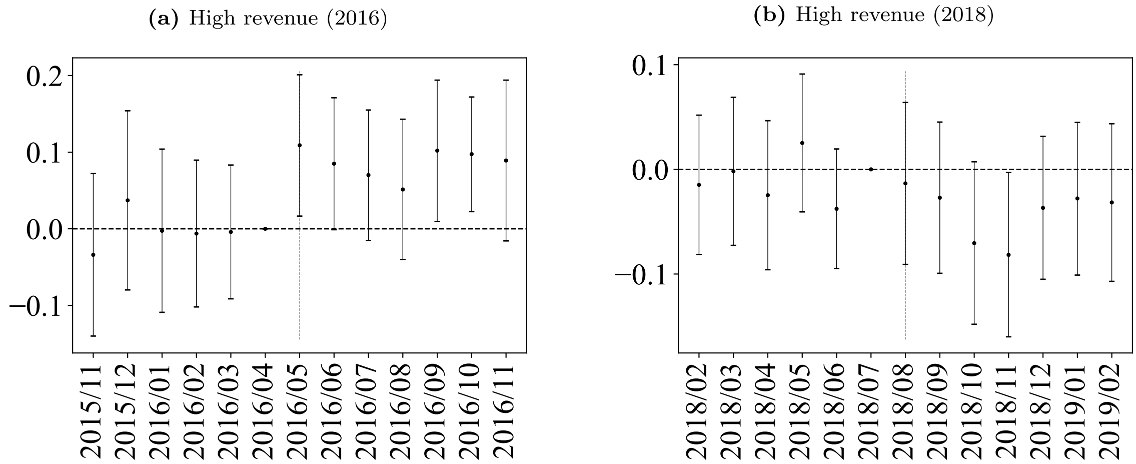


Fig. C.14. Impact of policy changes on long-term rental supply. The specifications are identical to those reported in Fig. 8, except that the points show point estimates of month-year fixed effects interacted with an indicator that is one if there were any *high-revenue* Airbnb listings present in a block in January before each respective policy change.

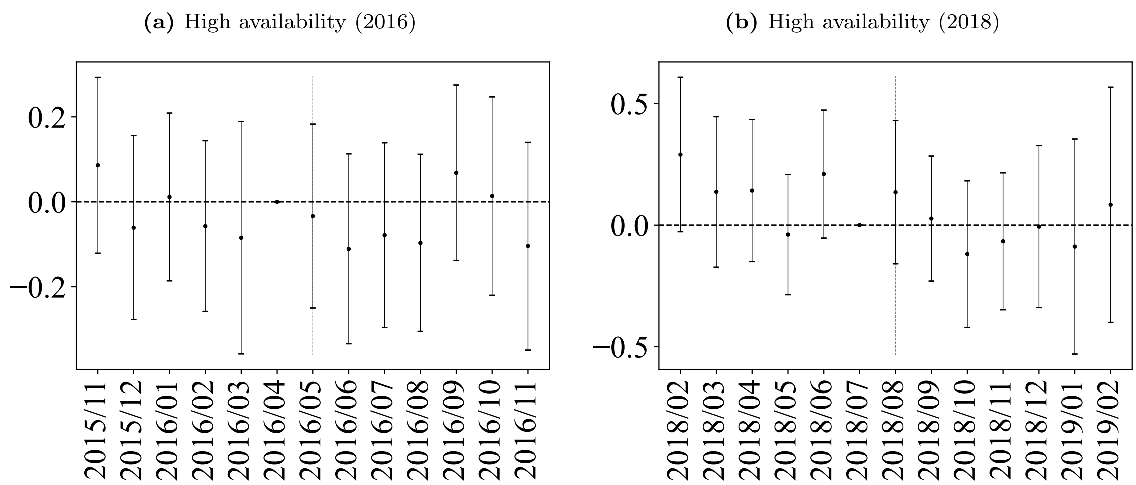


Fig. C.15. Impact of policy changes on long-term rents. The specifications are identical to those reported in Fig. 9, except that the points show point estimates of month-year fixed effects interacted with an indicator that is one if there were any *high-availability* entire homes present on Airbnb within a 250 m radius in January before each respective policy change.

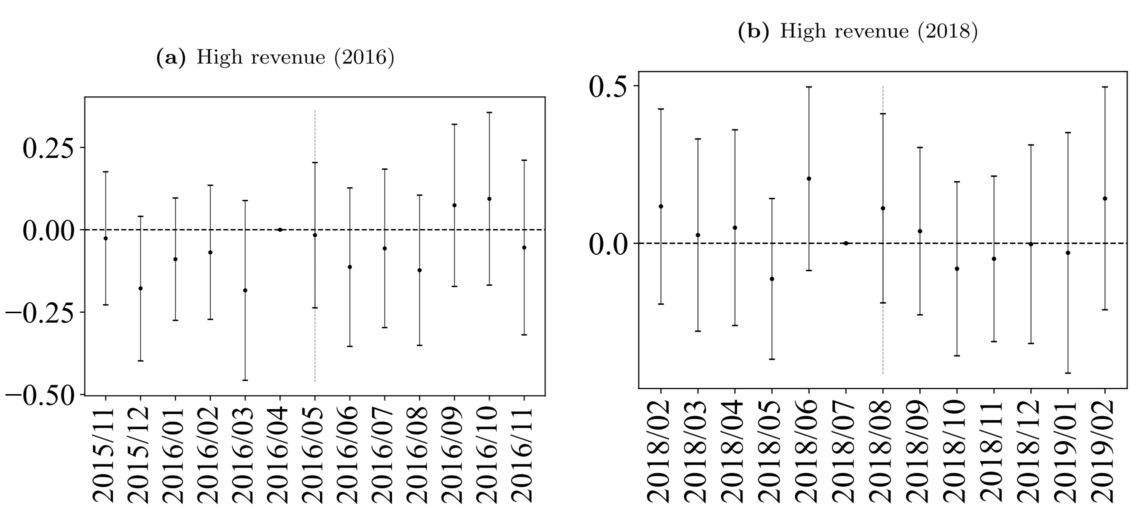


Fig. C.16. Impact of policy changes on long-term rents. The specifications are identical to those reported in Fig. 9, except that the points show point estimates of month-year fixed effects interacted with an indicator that is one if there were any *high-revenue* entire homes present on Airbnb within a 250 m radius in January before each respective policy change.

(a) 2016 **(b)** 2018

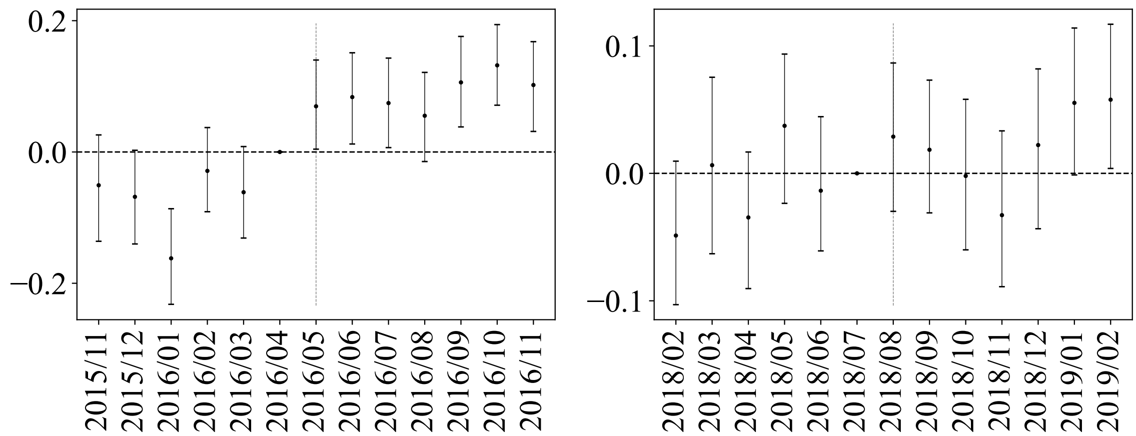


Fig. E.17. Event study: Impact of policy changes on long-term rental supply. Block-month level analyses. We deseasonalize the number of rentals by regressing them on month fixed effects first using the full sample. We then use the resulting residuals in the estimation. All other estimation details are equivalent to those reported in Fig. 8. The point estimates report the point estimates using the base sample. 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.

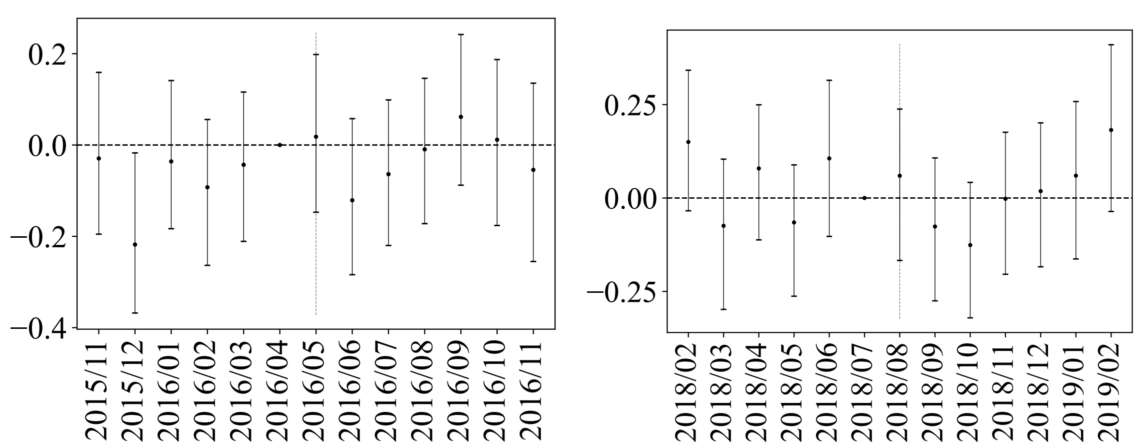


Fig. E.18. Event study: Impact of policy changes on long-term rents. Analysis on the rental-month level. We deseasonalize rents by regressing them on month fixed effects and a constant first. We then use the resulting residuals in the estimation. All other estimation details are equivalent to those reported in Fig. 9. The point estimates report the point estimates using the base sample. 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.

Table E.6
Impact of Airbnb on rental supply with matching treatment variables.

	2016 Policy					
	Entire Homes		High Availability		High Revenue	
	OLS	IV	OLS	IV	OLS	IV
Second stage						
Airbnb Exposure	−0.019*	−0.081*	−0.01	−0.147**	−0.01	−0.167*
	[−0.036; −0.003]	[−0.144; −0.018]	[−0.032; 0.013]	[−0.249; −0.046]	[−0.041; 0.021]	[−0.324; −0.011]
First stage						
Treatment Effect		−1.053***		−0.656***		−0.527***
		[−1.242; −0.864]		[−0.747; −0.564]		[−0.605; −0.449]
N	59,072	59,072	59,072	59,072	59,072	59,072
First-Stage F-Stat		119.7		197.2		176.3
	2018 Policy					
	Entire Homes		High Availability		High Revenue	
	OLS	IV	OLS	IV	OLS	IV
Second stage						
Airbnb Exposure	−0.017*	−0.001	−0.013	0.075	0.002	0.126
	[−0.032; −0.003]	[−0.124; 0.122]	[−0.039; 0.012]	[−0.210; 0.360]	[−0.023; 0.027]	[−0.226; 0.478]
First stage						
Treatment Effect		−0.467***		−0.223***		−0.209***
		[−0.588; −0.345]		[−0.291; −0.156]		[−0.262; −0.157]
N	68,631	68,631	68,631	68,631	68,631	68,631
First-Stage F-Stat		56.63		42.06		60.66

Notes: Block-month level analyses similar to those reported in Table 4 but with treatment defined on the same Airbnb type as the marginal Airbnb measure. For example column (4) uses a treatment indicator that is equal to one if there were any high-availability entire homes listed on Airbnb in block i in January of the respective policy year.

Table E.7
Impact of Airbnb on rents with matching treatment variables.

	2016 Policy					
	Entire Homes		High Availability		High Revenue	
	PDS OLS	Lasso IV	PDS OLS	Lasso IV	PDS OLS	Lasso IV
Second stage						
Airbnb Exposure	0.037***	0.075***	0.09***	0.097***	0.094***	0.198***
	[0.030; 0.043]	[0.046; 0.104]	[0.072; 0.109]	[0.076; 0.119]	[0.069; 0.118]	[0.163; 0.232]
First stage						
Treatment Effect		−5.7***		−2.026***		−1.423***
		[−7.314; −4.086]		[−2.563; −1.489]		[−1.792; −1.053]
N	41,159	41,159	41,159	41,159	41,159	41,159
Selected Xs	117	177	114	227	72	222
First-Stage F-Stat		3994		309.8		216.9
	2018 Policy					
	Entire Homes		High Availability		High Revenue	
	PDS OLS	Lasso IV	PDS OLS	Lasso IV	PDS OLS	Lasso IV
Second stage						
Airbnb Exposure	0.044***	0.083***	0.076***	0.127***	0.083***	0.168***
	[0.036; 0.053]	[0.041; 0.126]	[0.060; 0.091]	[0.091; 0.162]	[0.063; 0.103]	[0.141; 0.196]
First stage						
Treatment Effect		−4.415***		−1.784***		−0.993***
		[−5.630; −3.199]		[−2.320; −1.247]		[−1.359; −0.628]
N	39,696	39,696	39,696	39,696	39,696	39,696
Selected Xs	93	164	76	211	64	193
First-Stage F-Stat		2620		22.47		38.59

Notes: Rental-month level analyses similar to those reported in Table 5 but with treatment defined on the same Airbnb type as the marginal Airbnb measure. For example column (4) uses a treatment indicator that is equal to one if there were any high-availability entire homes listed on Airbnb within 250 m of the location of rental i in January of the respective policy year.

Table E.8
Impact of Airbnb on rental supply using deseasonalized variables.

	2016 Policy					
	Entire Homes		High Availability		High Revenue	
	OLS	IV	OLS	IV	OLS	IV
Second stage						
Airbnb Exposure	−0.019***	−0.081***	−0.010	−0.234*	−0.010	−0.370***
	[−0.031; −0.009]	[−0.138; −0.032]	[−0.033; 0.011]	[−0.390; −0.041]	[−0.037; 0.021]	[−0.627; −0.139]
First stage						
Treatment Effect		−1.053***		−0.366***		−0.232***
		[−1.131; −0.997]		[−0.405; −0.326]		[−0.257; −0.202]
Draws	200	200	200	200	200	200
	2018 Policy					
	Entire Homes		High Availability		High Revenue	
	OLS	IV	OLS	IV	OLS	IV
Second stage						
Airbnb Exposure	−0.017*	−0.001	−0.013	−0.003	0.002	−0.004
	[−0.028; −0.005]	[−0.119; 0.113]	[−0.042; 0.007]	[−0.314; 0.320]	[−0.022; 0.023]	[−0.536; 0.444]
First stage						
Treatment Effect		−0.467***		−0.171***		−0.116***
		[−0.516; −0.407]		[−0.197; −0.140]		[−0.139; −0.094]
Draws	200	200	200	200	200	200

Notes: Block-month level analyses. We deseasonalize the rental and Airbnb counts by regressing them on month fixed effects first. We then use the resulting residuals in the estimations. All other estimation details are equivalent to those reported in Table 4. The point estimates report the point estimates using the base sample. 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.

Table E.9
Impact of Airbnb on rents using deseasonalized variables.

	2016 Policy					
	Entire Homes		High Availability		High Revenue	
	PDS OLS	Lasso IV	PDS OLS	Lasso IV	PDS OLS	Lasso IV
Second stage						
Airbnb Exposure	0.010***	0.032***	0.026***	0.137***	0.025***	0.227***
	[0.007; 0.013]	[0.023; 0.042]	[0.016; 0.035]	[0.107; 0.168]	[0.011; 0.038]	[0.162; 0.271]
First stage						
Treatment Effect		−4.017***		−1.172***		−0.644***
		[−4.616; −3.579]		[−1.277; −0.975]		[−0.721; −0.549]
Draws	200	200	200	200	200	200
	2018 Policy					
	Entire Homes		High Availability		High Revenue	
	PDS OLS	Lasso IV	PDS OLS	Lasso IV	PDS OLS	Lasso IV
Second stage						
Airbnb exposure	0.011***	0.028***	0.018***	0.072***	0.015***	0.148***
	[0.004; 0.015]	[0.011; 0.045]	[0.009; 0.027]	[0.041; 0.108]	[0.004; 0.025]	[0.111; 0.207]
First stage						
Treatment Effect		−2.425***		−0.958***		−0.357***
		[−2.714; −2.079]		[−1.065; −0.772]		[−0.442; −0.232]
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 first. We then use the resulting residuals in the estimations. All other estimation details are equivalent to those reported in Table 5. The point estimates report the point estimates using the base sample. 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.

Table E.10
Rent analysis at the city-block level.

	2016 Policy					
	Entire Homes		High Availability		High Revenue	
	OLS	IV	OLS	IV	OLS	IV
Second stage						
Airbnb Exposure	−0.005	0.03	0.008	0.082	0.029	0.127
	[−0.035; 0.026]	[−0.090; 0.149]	[−0.077; 0.093]	[−0.250; 0.414]	[−0.069; 0.127]	[−0.388; 0.643]
First stage						
Treatment effect		−1.524***		−0.546***		−0.353***
		[−1.839; −1.208]		[−0.670; −0.423]		[−0.451; −0.255]
N	24,180	24,180	24,180	24,180	24,180	
Rent/m^2	9.220	9.220	9.220	9.220	9.220	9.220
First-Stage F-Stat		89.66		75.44		49.99
	2018 Policy					
	Entire Homes		High Availability		High Revenue	
	OLS	IV	OLS	IV	OLS	IV
Second stage						
Airbnb Exposure	0.062*	0.115	0.075	0.317	0.044	0.518
	[0.005; 0.120]	[−0.214; 0.444]	[−0.040; 0.189]	[−0.587; 1.221]	[−0.132; 0.220]	[−0.971; 2.007]
First stage						
Treatment effect		−0.817***		−0.298***		−0.182***
		[−1.078; −0.556]		[−0.399; −0.196]		[−0.249; −0.114]
N	23,692	23,692	23,692	23,692	23,692	23,692
Rent/m^2	10.77	10.77	10.77	10.77	10.77	10.77
First-Stage F-Stat		37.68		33.08		27.92

Notes: Block-month level analyses controlling for average apartment characteristics, block fixed effects, month-year fixed effects, and block-specific linear time trends. Standard errors are clustered at the postal code level. The square brackets show 95 percent confidence intervals. *, **, *** indicate five, one, 0.1 percent significance.

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.[34]

Appendix E. Robustness analyses

E.1. IV analyses with matching treatment variable

See Tables E.6 and E.7.

E.2. Seasonality

Because we focus on one-year 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 rental outcome variable as well as the count of nearby entire homes on Airbnb by regressing both on month fixed effects using the full sample. We then use the residuals of these regressions and implement the event studies as shown in Figs. 8 and 9 and the IV analyses as reported in Tables 4 and 5. To account for the additional variation from the deseasonalization preceding the main analysis, we use a bootstrapping procedure for inference.

Event studies. Figs. E.17 and E.18 replicate the results shown in Figs. 8 and 9 using the deseasonalization approach described above. The results are very similar to the non-deseasonalized results.

Marginal impact of Airbnb. Table E.8 reports the results of this exercise for our rental supply analysis. The estimates are very similar to our main results. More Airbnb listings nearby reduce the supply of long-term apartments. This effect is larger for commercial Airbnb listings.

Table E.9 reports the results of this exercise for our rents analysis. The results are qualitatively and quantitatively mostly in line with our main results for the 2016 sample. The estimated effect sizes are smaller for the professional Airbnb regressions in the 2018 sample, but the results remain qualitatively similar. This instability of the results could be driven by the fact that the 2018 policy did not affect professional Airbnb listings to the extent that the 2016 policy did.

Details of Bootstrapping Procedure: For the results reported in Tables E.8 and E.9, 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 rentals and Airbnb listings available to us. For the results in Table E.9, 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

$$y_{it} = \alpha + \beta MonthFE_t + \epsilon_{it}\,, \tag{E.1}$$

[34] 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 instruments $\gamma_0 \mathbb{1}(t \geq \tau) + \sum_{l=1}^{3} \gamma_l \mathbb{1}(t = \tau - l)$, in a regular IV framework.

Table E.11
Impact of Airbnb on rents using different circle sizes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	2016 All entire homes PDS OLS	2016 All entire homes Lasso IV	2016 High availability PDS OLS	2016 High availability Lasso IV	2016 High revenue PDS OLS	2016 High revenue Lasso IV	2018 All entire homes PDS OLS	2018 All entire homes Lasso IV	2018 High availability PDS OLS	2018 High availability Lasso IV	2018 High revenue PDS OLS	2018 High revenue Lasso IV
Second stage												
Airbnb Exposure (500 m)	0.012***	0.011***	0.038***	0.027***	0.052***	0.04***	0.016***	0.014***	0.036***	0.027***	0.043***	0.04***
	[0.011; 0.015]	[0.006; 0.016]	[0.033; 0.043]	[0.018; 0.035]	[0.046; 0.059]	[0.028; 0.051]	[0.013; 0.019]	[0.009; 0.019]	[0.031; 0.041]	[0.018; 0.036]	[0.036; 0.050]	[0.032; 0.048]
First stage												
Treatment Effect (500 m)		−17.32***		−4.826***		−2.829***		−12.6***		−4.767***		−2.514***
		[−22.350; −12.290]		[−6.342; −3.309]		[−3.745; −1.912]		[−16.430; −8.762]		[−6.295; −3.239]		[−3.469; −1.560]
Selected Xs	54	138	54	185	53	194	61	198	46	181	43	171
Second stage												
Airbnb Exposure (1000 m)	0.004***	0.004***	0.012***	0.011***	0.018***	0.014***	0.006***	0.005***	0.013***	0.009***	0.016***	0.011***
	[0.003; 0.005]	[0.003; 0.005]	[0.010; 0.015]	[0.008; 0.014]	[0.015; 0.021]	[0.011; 0.017]	[0.005; 0.006]	[0.003; 0.006]	[0.011; 0.015]	[0.005; 0.012]	[0.014; 0.019]	[0.007; 0.015]
First stage												
Treatment Effect (1000 m)		−49.77***		−13.59***		−7.583***		−50.88***		−20.25***		−9.934**
		[−64.620; −34.930]		[−17.930; −9.236]		[−10.110; −5.060]		[−73.080; −28.690]		[−29.990; −10.510]		[−16.640; −3.229]
Selected Xs	56	207	55	215	59	203	62	200	40	183	38	184

Notes: Rental-month level analyses. We use the number of nearby entire homes on Airbnb within 500 m and 1000 m as our Airbnb measure. All other estimation details are equivalent to those reported in Table 5. The square brackets show 95 percent confidence intervals. *, **, *** indicate five, one, 0.1 percent significance.

Table F.12
Selected control variables in each specification of Table 5. Columns correspond to the columns in Table 5.

	All 2016		H-Av. 2016		H-Rev. 2016		All 2018		H-Av. 2018		H-Rev. 2018	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Constant	x		x		x		x		x		x	
Airbnb exposure	x	x	x	x	x	x	x	x	x	x	x	x
Apartment characteristics												
Rooms	x		x		x		x		x		x	
Shower	x	x	x	x	x	x	x	x	x	x	x	x
Bathroom with window							x	x	x	x	x	x
Bath tub		x		x		x						
Balcony/terrace		x		x	x	x	x	x		x		x
Tile flooring		x		x		x						
Laminate flooring							x		x		x	
Parquet flooring	x	x	x	x	x	x	x	x	x	x	x	x
Carpet flooring	x		x		x		x		x		x	
Garden	x	x	x	x	x	x		x		x		x
Uility room	x	x	x	x	x	x		x		x		x
Floor heating	x	x	x	x	x	x	x	x	x	x	x	x
No heating										x		x
Kitchen	x	x	x	x	x	x	x	x	x	x	x	x
Open-plan kitchen	x	x	x	x	x	x	x	x	x	x	x	x
Pantry		x		x		x						
Elevator		x		x		x		x		x		x
Conservatory							x	x	x	x	x	x
Building characteristics												
Old building	x	x	x	x	x	x	x	x		x		x
Under preservation		x		x		x	x	x	x	x	x	x
Basement							x		x		x	
New-build	x	x	x	x	x	x	x	x	x	x	x	x
Janitor		x		x		x	x	x	x	x	x	x
Social housing	x	x	x	x	x	x	x	x	x	x	x	x
Block characteristics												
Noise		x		x		x		x		x		x
Particulate matter PM10 (total)							x		x		x	
Particulate matter PM2.5 (traffic)	x		x		x							
Particulate matter PM2.5 (total)	x	x	x	x	x	x		x	x	x	x	x
Nitrogenmonoxid (total)				x								
Nitrogenmonoxid (traffic)	x	x	x	x	x	x	x	x	x	x	x	x
Block points-of-interest												
Atm	x	x	x	x			x	x	x	x	x	x
Bar	x	x	x	x	x	x	x	x	x	x	x	x
Bicycle Shop	x	x	x	x	x	x	x	x	x	x	x	x
Bus Stop	x		x		x							
Cafe	x	x	x	x	x	x	x	x	x	x	x	x
Caravan Site		x		x		x						
Convenience	x	x		x	x	x	x	x				x
Furniture Shop		x		x		x						
Gift Shop				x		x						
Hairdresser				x				x				
Hospital		x		x		x						
Hotel	x		x		x							
Kindergarten		x		x		x		x		x		x
Monument		x										
Nightclub		x										
Prison				x		x						
Pub	x	x	x	x	x	x	x	x	x	x	x	x
Restaurant	x	x	x	x	x	x	x	x	x	x	x	x
School		x		x								
Track		x		x		x						
Vending Machine		x		x		x		x		x		x
Water Well						x		x		x		x
Water Works	x		x		x							
Windmill								x		x		x
Zoo		x		x		x						

(*continued on next page*)

Table F.12 (*continued*).

	All 2016		H-Av. 2016		H-Rev. 2016		All 2018		H-Av. 2018		H-Rev. 2018	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Time fixed effects												
2015–12	x	x	x	x	x	x						
2016–01	x	x	x	x	x	x						
2016–02	x	x	x	x		x						
2016–03		x		x		x						
2016–04		x		x		x						
2016–05		x		x		x						
2016–06	x	x	x	x		x						
2016–07		x		x		x						
2016–08	x	x	x	x	x	x						
2016–09	x	x	x	x	x	x						
2016–10	x	x	x	x	x	x						
2016–11	x	x	x	x	x	x						
2018–03							x	x		x		x
2018–04								x		x		x
2018–05								x		x		x
2018–06								x		x		x
2018–07								x		x		x
2018–08								x		x		x
2018–09							x	x		x		x
2018–10							x	x	x	x		x
2018–11							x	x	x	x		x
2018–12								x		x		x
2019–01								x		x		x
2019–02							x	x	x	x	x	x
Postal code fixed effects												
10117			x	x				x				
10119					x	x						
10179		x										
10243		x		x		x						x
10245								x		x		x
10249		x		x		x						
10318								x		x		x
10369		x		x		x		x		x		x
10407								x		x		x
10409								x		x		x
10435		x		x		x						
10437			x	x								
10551								x		x		x
10555		x		x		x		x		x		x
10557								x		x		x
10559		x		x		x						
10585		x		x		x						
10587		x		x		x						
10623		x		x		x						
10625						x		x		x		x
10629										x		
10707		x		x		x		x		x		x
10717		x		x		x						
10777		x										
10779		x		x		x						
10781	x		x		x			x		x		x
10783						x						
10789	x	x		x	x	x		x		x		x
10825		x		x		x						x
10961										x	x	x
10963								x		x		x
10965								x		x		x
10967												x
10969						x		x		x		x
10997												x
10999							x	x	x	x		x

(*continued on next page*)

Table F.12 (*continued*).

	All 2016		H-Av. 2016		H-Rev. 2016		All 2018		H-Av. 2018		H-Rev. 2018	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
12043												x
12049		x		x		x						
12051								x		x		x
12055		x		x		x						x
12057		x		x		x		x		x		x
12059												x
12103		x		x		x		x		x		x
12109		x		x		x		x		x		x
12167								x		x		x
12203		x		x		x						
12205		x		x		x						
12207		x		x		x		x		x		x
12209		x		x		x		x		x		x
12249								x		x		x
12277						x						
12279		x		x		x					x	
12307								x				
12309							x		x		x	
12347								x		x		x
12349								x		x		x
12355	x	x				x	x	x		x		x
12357							x	x				
12359	x		x		x							
12459	x		x		x							
12489		x		x		x						
12524							x	x	x	x	x	
12527		x		x		x						
12555		x		x		x						
12559										x		
12589		x		x		x		x		x		x
12619		x		x		x						
12623		x		x		x		x		x		x
12627		x		x		x		x		x		x
12629	x		x		x			x				
12683		x		x		x						x
12685							x	x		x	x	x
12689		x		x		x	x	x	x	x		x
13055								x		x		x
13057							x		x		x	x
13059								x		x		x
13086								x		x		x
13125	x		x		x			x		x		x
13127		x		x		x						
13156								x		x		x
13159		x		x		x						
13351		x		x		x		x		x		x
13353								x		x		x
13357								x		x		x
13435							x					
13437		x		x		x						
13465								x		x		x
13467		x		x		x						
13469							x	x				
13505	x		x		x							
13581	x		x		x							
13585						x						
13587			x	x	x	x						
13593			x									
13597	x		x		x							
13599	x	x	x		x			x		x		x
13629	x	x	x	x	x	x		x		x		x
14050		x		x		x						
14055		x		x		x						

(*continued on next page*)

Table F.12 (*continued*).

	All 2016		H-Av. 2016		H-Rev. 2016		All 2018		H-Av. 2018		H-Rev. 2018	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
14057								x		x		x
14059		x		x		x						
14089							x	x				
14109								x		x		x
14129								x		x		x
14165		x		x		x						
14169		x		x		x						
14197		x		x		x		x		x		x
Postal code trends												
10117 x Trend						x						
10243 x Trend								x				
10247 x Trend								x		x		x
10315 x Trend		x		x		x						
10318 x Trend								x		x		x
10409 x Trend		x										
10435 x Trend								x		x		x
10437 x Trend								x		x		x
10439 x Trend		x		x		x						
10551 x Trend		x		x		x		x				
10559 x Trend						x		x		x		x
10625 x Trend				x								
10629 x Trend							x		x		x	
10711 x Trend		x		x		x						
10713 x Trend								x		x		x
10719 x Trend		x		x		x		x		x		x
10779 x Trend		x		x		x						
10787 x Trend		x		x		x						x
10789 x Trend	x		x		x			x				
10825 x Trend										x		
10827 x Trend								x		x		x
10829 x Trend		x		x								
10961 x Trend							x	x	x			
10965 x Trend		x						x				
10967 x Trend										x		
10969 x Trend		x		x		x						
10997 x Trend		x		x		x		x				
10999 x Trend		x		x		x		x		x		
12043 x Trend										x	x	
12051 x Trend				x								
12053 x Trend								x		x		x
12055 x Trend								x		x		x
12059 x Trend										x		
12099 x Trend								x		x		x
12105 x Trend								x		x		x
12107 x Trend		x		x		x						
12109 x Trend		x		x		x						
12157 x Trend								x		x		x
12161 x Trend								x		x		x
12163 x Trend								x		x		x
12167 x Trend		x		x		x						
12247 x Trend		x		x		x		x		x		x
12249 x Trend		x		x		x						
12277 x Trend		x		x	x	x		x		x		x
12279 x Trend		x		x		x	x	x	x	x	x	x
12305 x Trend	x	x	x	x	x	x	x	x				
12307 x Trend		x		x		x	x	x		x		x
12309 x Trend		x		x		x						
12347 x Trend								x		x		x
12349 x Trend	x	x		x	x	x			x	x	x	x
12351 x Trend		x		x		x						
12353 x Trend		x		x		x		x		x		x
12355 x Trend		x		x		x		x		x		x
12357 x Trend								x		x	x	x
12435 x Trend		x		x		x						

(*continued on next page*)

Table F.12 (*continued*).

	All 2016		H-Av. 2016		H-Rev. 2016		All 2018		H-Av. 2018		H-Rev. 2018	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
12439 x Trend		x		x	x	x		x		x		x
12459 x Trend		x		x		x		x		x		x
12487 x Trend		x		x		x						
12524 x Trend		x		x		x		x		x		x
12526 x Trend		x		x		x		x		x		x
12555 x Trend		x		x		x						
12557 x Trend		x		x		x		x		x		x
12559 x Trend		x		x	x	x	x	x	x	x	x	x
12587 x Trend		x		x		x		x		x		x
12619 x Trend	x	x										
12621 x Trend		x		x		x		x		x		x
12623 x Trend								x		x		x
12627 x Trend			x	x			x	x	x	x	x	
12629 x Trend		x		x	x	x	x	x	x	x		x
12679 x Trend		x		x		x						
12681 x Trend		x		x		x						
12683 x Trend		x		x		x	x	x	x	x		
12685 x Trend		x		x		x		x				
12687 x Trend		x		x		x		x		x		x
12689 x Trend		x		x		x						
13051 x Trend		x		x		x		x		x		x
13053 x Trend		x		x		x		x		x		x
13057 x Trend		x		x		x						
13125 x Trend		x		x		x						
13127 x Trend		x		x		x						
13129 x Trend										x	x	x
13158 x Trend		x		x		x		x		x		x
13159 x Trend		x		x		x		x		x		x
13347 x Trend		x		x		x						
13351 x Trend								x		x		x
13353 x Trend		x		x		x						
13405 x Trend								x		x		x
13409 x Trend		x		x		x						
13435 x Trend		x		x		x		x	x	x	x	x
13437 x Trend	x	x						x		x		x
13439 x Trend		x		x		x						
13465 x Trend		x		x		x		x		x		x
13467 x Trend		x		x		x		x		x		x
13469 x Trend		x		x		x		x		x		x
13503 x Trend		x		x		x		x				
13505 x Trend		x		x		x				x		x
13507 x Trend		x		x		x						
13509 x Trend		x		x		x						
13581 x Trend		x		x		x		x		x		x
13583 x Trend	x	x		x	x	x						
13585 x Trend		x	x	x	x	x						
13587 x Trend		x		x	x	x						
13589 x Trend		x		x		x		x		x		x
13591 x Trend		x		x		x	x	x				
13593 x Trend	x	x		x	x	x		x		x		x
13595 x Trend								x		x		x
13597 x Trend		x		x		x						
13599 x Trend		x	x	x	x	x	x	x	x		x	x
13629 x Trend		x		x		x						
14055 x Trend								x		x		x
14089 x Trend	x	x		x		x		x		x		x
14109 x Trend		x		x		x						
14129 x Trend		x		x		x						
14193 x Trend								x		x		x
14197 x Trend		x		x		x						

(*continued on next page*)

Table F.12 (*continued*).

	All 2016		H-Av. 2016		H-Rev. 2016		All 2018		H-Av. 2018		H-Rev. 2018	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Postal code trends x Airbnb exposure												
10117 x Trend x Airbnb Exposure				x								
10317 x Trend x Airbnb Exposure		x		x		x						
10365 x Trend x Airbnb Exposure		x		x		x		x		x		x
10367 x Trend x Airbnb Exposure								x		x		x
10369 x Trend x Airbnb Exposure								x		x		x
10409 x Trend x Airbnb Exposure		x		x		x		x		x		x
10553 x Trend x Airbnb Exposure								x		x		x
10585 x Trend x Airbnb Exposure								x		x		x
10781 x Trend x Airbnb Exposure								x		x		x
10965 x Trend x Airbnb Exposure		x		x								
10999 x Trend x Airbnb Exposure									x			
12051 x Trend x Airbnb Exposure		x		x								
12057 x Trend x Airbnb Exposure		x		x		x						
12059 x Trend x Airbnb Exposure		x		x		x						
12099 x Trend x Airbnb Exposure		x		x		x		x		x		x
12101 x Trend x Airbnb Exposure								x		x		x
12163 x Trend x Airbnb Exposure								x		x		x
12167 x Trend x Airbnb Exposure								x		x		x
12203 x Trend x Airbnb Exposure								x		x		x
12207 x Trend x Airbnb Exposure								x		x		x
12209 x Trend x Airbnb Exposure								x		x		x
12247 x Trend x Airbnb Exposure		x		x		x						
12249 x Trend x Airbnb Exposure								x		x		x
12279 x Trend x Airbnb Exposure								x		x		x
12347 x Trend x Airbnb Exposure								x		x		x
12349 x Trend x Airbnb Exposure								x		x		x
12435 x Trend x Airbnb Exposure		x		x		x	x	x	x	x	x	x
12439 x Trend x Airbnb Exposure								x		x		x
12587 x Trend x Airbnb Exposure		x		x		x						
12621 x Trend x Airbnb Exposure								x		x		x
13053 x Trend x Airbnb Exposure		x		x		x						
13127 x Trend x Airbnb Exposure								x		x		x
13129 x Trend x Airbnb Exposure								x				
13359 x Trend x Airbnb Exposure		x		x		x						
13409 x Trend x Airbnb Exposure								x		x		x
13437 x Trend x Airbnb Exposure								x		x		x
13465 x Trend x Airbnb Exposure							x		x		x	
13469 x Trend x Airbnb Exposure								x		x		x
13585 x Trend x Airbnb Exposure								x		x		x
13599 x Trend x Airbnb Exposure								x		x		x
14055 x Trend x Airbnb Exposure								x		x		x
14169 x Trend x Airbnb Exposure								x		x		x
14193 x Trend x Airbnb Exposure		x		x		x		x		x		x
14195 x Trend x Airbnb Exposure							x		x		x	
14197 x Trend x Airbnb Exposure		x		x		x						
14199 x Trend x Airbnb Exposure		x		x		x						

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 corresponding 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 the rent and the Airbnb measure with the corresponding estimated residuals.

For the results reported in Table E.8, 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 rentals. 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 as above.

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 $0 + |\hat{\beta}_0|$.

E.3. Rent analysis on the city-block level

See Table E.10.

E.4. Circle size for rents analysis

The choice to use a distance of 250 m to count the number of Airbnb listings nearby a rental for the analysis in Section 4.3 is ad-hoc. The results of robustness checks using circle sizes with radii of 500 and 1000 m are reported in Table E.11.

We find qualitatively similar results in terms of sign of the effect and larger effect size when focusing on commercial Airbnb listings. However, with larger circle sizes, effect sizes are generally smaller. This result is in line with larger circle sizes capturing further away Airbnb listings that have less of an impact on rents.

Appendix F. Selected control variables in rents IV

See Table F.12.

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