Short-term rental bans and housing prices:
Quasi-experimental evidence from Lisbon*

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Abstract

We estimate the causal impact of short-term rentals on the real estate market by exploiting a quasi-natural zoning reform that banned new short-term rental registries in some parts of Lisbon. We rely on two administrative data sets on short-term rental registries, between 2015 and 2019, and real estate transactions, between 2017 and 2019. We also use data on Airbnb rental listings and prices since 2018. We employ a difference-in-differences estimation taking advantage of the spatial discontinuity in the registry ban. We document a spike in new registries between the announcement and the implementation of the ban, driven by domestic incumbent owners. Airbnb quantities and prices do not react. The reform decreases the number of transactions and the house prices, mostly in two-bedroom flats, with a decline of 26% in quantity, and 20% in the price per square meter. We conclude that the short-term rental market explains only partially the recent boom in property prices.

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1 Introduction

In June 2019, 10 European cities wrote to the European Commission asking for the growth of short-stay platforms to be tackled at the EU-level, with the Mayor of Amsterdam claiming that "Where homes can be rented out more lucratively to tourists, they vanish from the traditional housing market."[1] Airbnb, one of these platforms, was created in 2008 and grew to be present in more than 80 thousand cities. In response to this concern, several cities across the globe have implemented measures to curb the growth of short-term holiday platforms, including Amsterdam, Barcelona, Los Angeles, and New York. In this paper, we exploit a partial ban on new short-term rental licences introduced by the municipality of Lisbon in November 2018. This ban provides an ideal quasi-experimental setup to estimate the causal impact of short-term rental regulations on the real estate market.

Portugal is one of 4 countries (out of 16) with a price increase of more than 30% between 2015 and 2018, according the 2019 Property Index of Deloitte.[2] The price surge hides considerable heterogeneity; between the first quarter of 2016 and the last quarter of 2019, the median real estate price per square meter in the city of Lisbon increased from 1886 to 3245 euros, i.e., more than 70%.

In 2019, Portugal hosted more than 16 million foreign tourists, up from 9 million in 2013. Lisbon, elected World's Leading City Break Destination in 2017, 2018, and 2019 by the World Travel Awards, reached almost 12 million overnight stays in 2019, more than 20 times its resident population.[3] Therefore, Portugal offers a natural setup to analyze the impact of the tourism and short-term rental boom on the real estate market.

Given the spectacular increase in the housing prices in Lisbon and other cities, the parliament changed the law to allow municipalities to implement zoning regulations on the supply of short-term rentals. Under this law, the municipality of Lisbon implemented a ban of

[1] See https://www.theguardian.com/cities/2019/jun/20/ten-cities-ask-eu-for-help-to-fight-airbnb-expansion.
[2] See https://www2.deloitte.com/be/en/pages/real-estate/articles/deloitte-property-index-2019.html
[3] https://www.worldtravelawards.com/profile-8079-turismo-de-lisboa.

new short-term rental registries in November 2018. The ban was extended one year later to some adjacent neighborhoods. Our baseline specification uses the extension area as the comparison group, and the 2018 original banned area as the treatment group, and implement a difference-in-differences approach to provide credible quasi-experimental estimates of the impact of short-term rental platforms on *(a)* registries, *(b)* Airbnb listings, *(c)* Airbnb prices, *(d)* number of transactions, and *e)* house prices.

We use administrative data that covers the universe of short-term rental registries, combined with data on the housing and rental market purchased from the main real estate databank of the country, *Confidencial Imobiliário*. In addition, we exploit publicly available data from the municipality of Lisbon that lends support to our identification strategy, since treatment and comparison areas do not display statistically significant differences in a number of characteristics, including urban amenities, education and culture local public goods, mobility infrastructure, and police stations. We also use data from Inside Airbnb, a web platform that collects data about listings and prices.

We provide a number of novel results. We find convincing evidence that the incumbent owners rushed to register properties in the banned areas in the days running to the effective prohibition. Interestingly, this was done by Portuguese dwelling owners, not foreign ones, suggesting asymmetry on the awareness of the policy discussion. Moreover, we show that there were no spillover effects onto streets on the immediate border of treated areas, i.e., there was no increase in registries in comparison areas. We also show that there was no impact on the short-term rental market as measured by Airbnb data, i.e., no effect on quantities or prices, in the immediate period following the ban.

Our results for the real estate market are as follows. We find evidence of a decrease in demand for houses in the treated areas, leading to a contraction of 20% in transactions *vis-à-vis* the comparison areas. Prices decreased by 8%, showing that the option to rent the dwelling in the short-term rental market is an important demand determinant. We document that the price and quantity effects in the real estate market are mostly driven by two-bedroom properties, an expected result given that both the mean and the median number of rooms of dwelling in the short-term rental market is two. The price decrease for two-bedroom houses is almost 20%.

Our work thus suggests important policy implications. The first one is not surprising: long periods of anticipating discussions about zoning regulations that create incumbency rents stem the policy's impacts. Secondly, the surge of short-term rental markets creates an upward pressure on real estate prices, concentrated in some types of properties. Thirdly, the magnitude of our estimates indicates that the tourism and short-term rental boom, despite its salience, cannot explain the full compounded growth of 68.2% in real estate prices in the city of Lisbon between 2016 and 2019.

Our paper contributes to a nascent literature exploiting policy changes to analyze the real estate impacts of short-term rentals.[4] Koster et al. (2021) take advantage of the Los Angeles' Home Sharing Ordinances, a restriction adopted by 18 of its 88 cities that prevented landlords to short-term rent any property besides their primary one, to apply a Panel Regression-Discontinuity Design at the treatment borders. They conclude that the regulation reduced listings by 50% and house prices by 3%. Moreover, Valentin (2020) found that the regulatory reform in New Orleans (which required hosts to pay for an annual short-term rental license, limited the number of days a host could rent per year and defined prohibition zones for this type of rental) displaced landlords from short-term renting and led to a decrease in house prices. Finally, Duso et al. (2020) use two regulatory reforms in Berlin that successfully cut the number of Airbnb listings to investigate their impact on rents. They find that Airbnb listings increase average monthly rents of nearby apartments.

Other papers with causal strategies that do not rely on policy reforms include Barron et al. (2018), who use data on Airbnb's listings between 2011 and 2016 in the US, and find

[4]For other policy determinants of real estate prices, see Dachis et al. 2012, Besley et al. 2014, Kopczuk & Munroe 2015, Hilber & Lyytikäinen 2017, Slemrod et al. 2017, Best & Kleven 2018 for transaction, (Basten et al. 2017) for income, and Yinger 1982, Sirmans et al. 2008, Hilber et al. 2011, Lyytikäinen 2012, Elinder & Persson 2017, Bradley 2017 for property taxes. For the impact of short-term rentals on other markets, see Alyakoob & Rahman (2019) and Basuroy et al. (2020) for restaurants, and Zervas et al. (2017), Schaefer & Tran (2020) for the lodging industry.

modest effects, employing an instrumental variable based on google trends. Sheppard et al. (2016) employ a matched difference-in-differences strategy to estimate the causal impact of having Airbnb units nearby on New York City's house prices, and conclude that a property's sale price increases by 3.5% for weakly treated peripheral properties and by 65% for heavily treated and/or centrally located properties. A similar methodology is applied to the Portuguese real estate market by Franco & Santos (2021). Using a matched difference-in-difference strategy, they conclude that a 1 p.p increase in the share of Airbnb properties increases house prices by 4.5%, which amounts to an increase in property values of 34% due to the short-term lease regulatory reform, particularly located in the historical centers of Lisbon and Porto. Àngel Garcia-López, Jofre-Monseny, Martínez-Mazza & Segú (2020) use an instrumental variable fixed-effects model based on neighborhood proximity to tourist amenities to find that Airbnb presence in Barcelona between 2012 and 2016 raised rents by 7%, on average, in the most touristic neighborhoods.[5]

Two important studies implement structural methods to study the effects of Airbnb on the welfare of residents. Calder-Wang (2019) uses data from NY to set up a model where residents can short-rent their home, while absentee landlords can short-rent to tourists or sign long-term contracts with tenants. In this setting, Airbnb affects residents' welfare, as rent increases are widespread across the city due to spillover effects. Almagro & Domınguez-Iino (2019) build a dynamic spatial equilibrium model of residential choice with endogenous amenities, exploiting data covering the universe of residents in Amsterdam. Their results show that endogenizing how amenities are formed is central to understanding the nature and welfare consequences of spatial sorting.[6]

This paper is organized as follows. Section 2 describes the context and the legal changes that we use to design our quasi-experimental setup. Section 3 presents the data sources,

[5] See also Horn & Merante (2017), who analyze data from Boston and find that a one standard deviation increase in Airbnb density leads to a 0.4% increase in local rents.

[6] Edelman et al. (2017) discuss the racial discrimination involved in the process of matching landlords and tenants in this kind of platforms. Wachsmuth & Weisler (2018) explore the gentrification that results from the geographical imbalances of Airbnb's revenue flows, which enhances within-cities income inequality and increases tenant displacement.

the empirical methodology employed in the paper, and discusses how possible threats to identification are handled. The results are presented and discussed in Section 4. Finally, Section 5 concludes and discusses the main implications of our work.

2 Context and Institutional Framework

Portugal embraced the touristic boom in 2014, when it created a streamlined, fully online registration system for landlords to acquire the necessary licence to list their property on hosting platforms.[7] Registering the dwelling with the Portuguese Registry Office for Short-Term Rentals is an obligatory step to advertise the property on Airbnb or similar platforms. The license does not expire, except if the dwelling is sold by the owner, irrespective of whether it is an individual or a firm. There is no cost involved in owning the licence, i.e., no fee to be paid regularly to the regulator. Therefore, there is no foreseable reason to cancel a licence, on behalf of its owner. Renting an unregistered unit is subject to a fine. Moreover, online platforms are forbidden to advertise unregistered properties. As of July 1st 2017, online platforms are liable for a fine of €32,500 if they advertise non-registered rentals, according to the decree-law 80/2017. The properties are subject to a number of regulations, including safety-related ones (e.g., fire alarms and instructions in several languages), publicity, the possibility for guests to file complaints, among others. All these are verified ex-post, through random audits from the competent independent authority.[8] Therefore, in practice, the licence is available as of the moment of registration.[9]

The simplified regulation, along with the growing number of tourists, triggered a spectacular increase in the number of short-term rental properties. In 2013 there were a total of twelve thousand properties allocated to short-term rentals in Portugal. This number multi-

[7] Decree-law 128/2014.

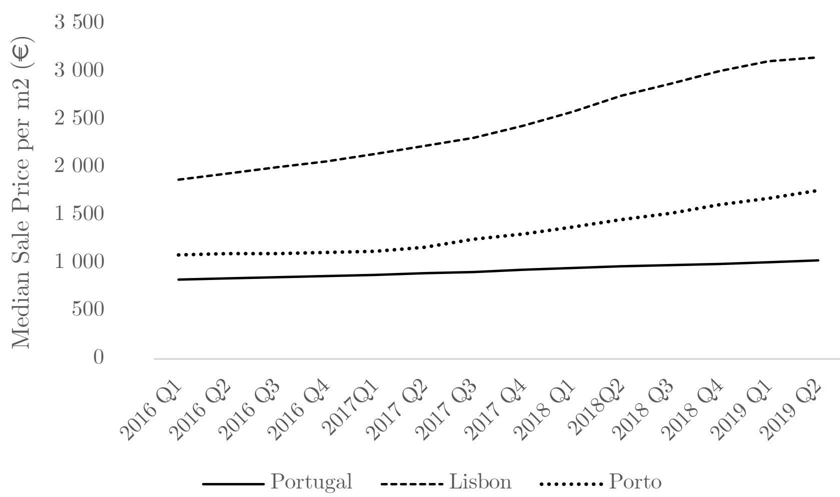
[8] The *Autoridade de Segurança Alimentar e Económica* has conducted, in 2019 (last available data) a total of 1900 audits in the whole country, and implemented 327 fines as a consequence.

[9] Short-term rental is subject to a special tax regime, according to which the owner's marginal income tax rate is applied to a fixed share of the rental income. The special tax regime does not preclude the owner from renting the property in the long-term market and paying taxes as such.

plied by eight in seven years, i.e., in July 2020 there were more than 94 thousand properties. This massive increase largely coincided with a surge in the real estate price.

From early 2016 to mid-2019, the median sale price per square meter increased by 24.2% in the country. This average change hides considerable heterogeneity both across cities, with housing price increases in Lisbon and Porto of 68.2% and 61.9%, respectively (cfr. Figure 1), and within the city of Lisbon, where the highest growth rates were concentrated in the historical downtown areas (cfr. Figure 2). The figure depicts the city of Lisbon split into its 24 civil parishes, or *freguesias* in Portuguese.[10]

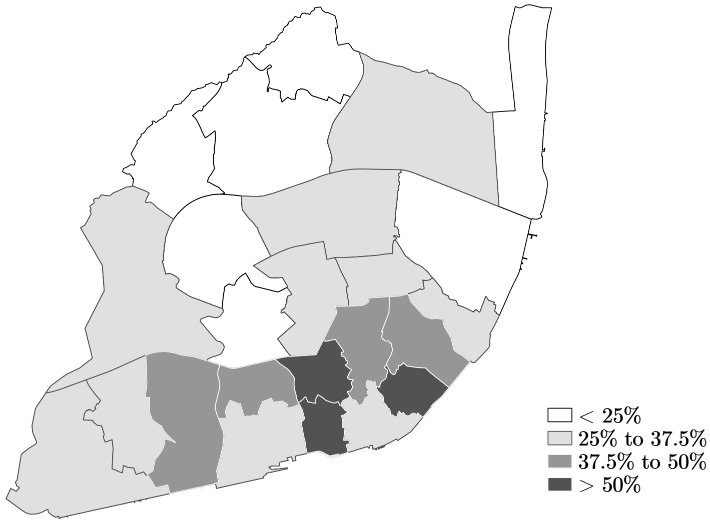
Figure 1: Median Sale Price (Dwellings) per Square Meter



Source: Statistics Portugal

[10]The civil parish is the lowest political unit in Portugal, with its own directly elected government.

Figure 2: Median Sale Price Growth Rate in Lisbon: 2016 to 2018



Source: Statistics Portugal

Reforms on the regulation of short-term rental have long been a topic of discussion. En route to the 2017 Portuguese municipal elections, the incumbent socialist mayor of Lisbon, Fernando Medina, declared his intention to enforce a cap on the short-term rental units in some neighborhoods, an ambition which was shared by the candidates on the left-wing of the political spectrum.[11] On the right, despite a common apprehension, candidates favored incentives for long-term rental rather than imposing supply restrictions.[12]

In parallel, there was mounting concern about the role of short-term rentals in the gentrification of the city and on real estate prices from several NGOs and residents' associations.[13] In January 2017, around twenty grassroots entities organized a petition calling for public policies to curb the real estate price increase in the capital, assumed excessive by the organizers.[14] Medina was re-elected in October 2017, with 42% of the votes.

[11] See publico.pt/2017/08/30/local/noticia/medina-quer-limitar-alojamento-local-nos-bairros-historicos-de-lisboa-1783830

[12] See eco.sapo.pt/2017/06/09/teresa-leal-coelho-defende-incentivos-ao-arrendamento-de-longa-duracao/.

[13] For examples of NGOs that played an active role in this debate, please refer to (in Portuguese) https://moraremlisboa.org, https://habita.info/.

[14] https://www.peticao.online/moraremlisboa

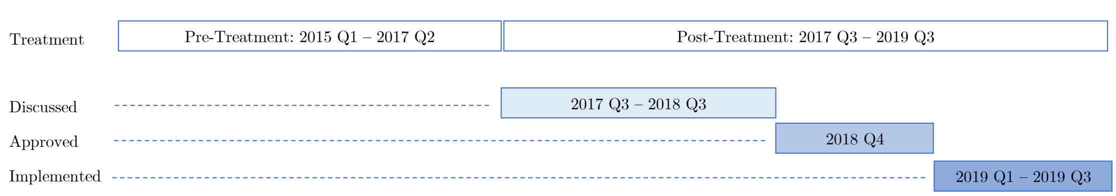
In August 2018, the Parliament legislated (Law 62/2018) to grant Portuguese municipalities the power to regulate new registries of properties in the Portuguese Registry Office for Short-Term Rentals (*Registo Nacional de Estabelecimentos de Alojamento Local* – RNAL).[15] In practice, each local government would be able to devise zoning laws regulating the density of short-term rental properties in the city. This law, however, only became effective two months later.

In November 2018, the municipality of Lisbon suspended new registries in certain pre-designated areas, known as *Zonas Turísticas Homogéneas*; more specifically, those deemed to be over-saturated, i.e., with a ratio of short-term rental to total properties above 25%.[16] This criterion was computed with information from the short-term rental registry and the 2011 census. The designated areas are bigger than a neighborhood, but smaller than a civil parish.

Following up on the November 2018 freeze, the municipality started discussing an extension of the suspension areas in April 2019.[17] This extension was approved in November 2019 (*DR n.º 214/2019, 1.º Suplemento*). However, as we make clear below when analysing the registry data, there were no anticipation effects of this ban on the comparison group implemented one year later. In fact, there is no noticeable increase in the number of registries of short-term rentals.

Figure 3 summarizes the timeline of these municipal regulations.

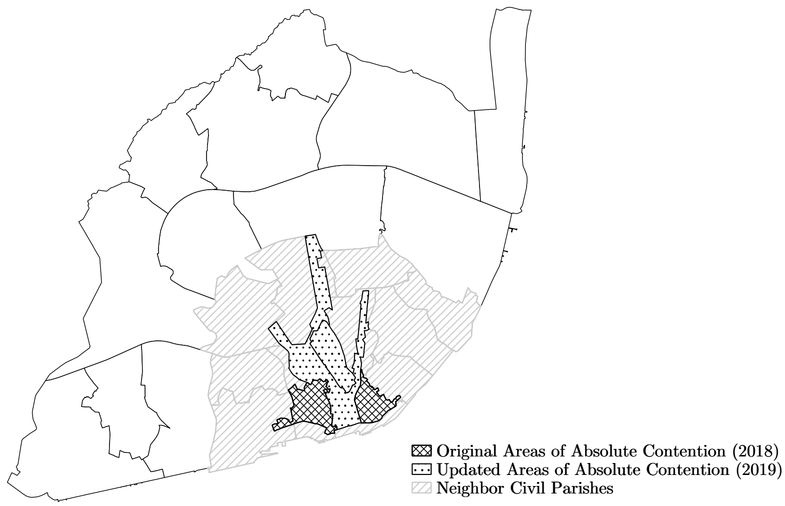
Figure 3: Analysis Timeline



[15] See `publico.pt/2018/04/05/local/noticia/autarca-de-santa-maria-maior-pede-suspensao-do-licenciamento-de-alojamento-local-na-cidade-1809305` (in Portuguese).
[16] The municipal regulation is *Proposta n.º 677/AML/2018.*
[17] *Proposta n.º 204/CM/2019*

Figure 4 gives a geographical display of the different areas. The ones that were suspended in November 2018 are in black, while those that were suspended one year later are filled with a dotted pattern. Neighboring civil parishes, used in some of our specifications, are displayed in grey.

Figure 4: Lisbon short-term rental freezes



Neighbor Civil parishes: Estrela, Campo de Ourique, Campolide, Avenidas Novas, Areeiro, Beato, Penha de França, Arroios, São Vicente, Santa Maria Maior, Santo António, and Misericórdia

3 Empirical Strategy

In this section, we begin by presenting the four complementary data sources used in the paper. We then present the methodology, compute descriptive statistics, and, finally, discuss possible threats to identification and how our empirical strategy handles them.

3.1 Data Sources

Our paper exploits two administrative sources of data on short-term rentals registries and the housing market, i.e., prices and number of traded dwellings. We complement these with non-official publicly available information on Airbnb price listings. We also use information from the municipality of Lisbon regarding urban amenities in order to characterize the treated and comparison areas.

The first one consists of publicly available information from the National Short-Term Rental Registry (RNAL).[18] We collect all daily new registries, between January 2015 and September 2019. Our data includes the universe of legal short-term rentals registered in this period, amounting to a total of 16972 dwellings for which we observe the registry date, address, number of rooms, nationality of the owner, and whether the owner is an individual or a firm.

The second administrative source contains quarterly information about the number of house sales and their average and quartile prices, per square meter, between the first quarter of 2015 and the third quarter of 2019. It comes from the SIR.RU (Urban Rehabilitation) platform, compiled by *Confidencial Imobiliário*, thanks to a protocol established with the Municipalities of Lisbon and Porto. *Confidencial Imobiliário* is an independent Portuguese databank specialized in real estate. Its data is used by almost all major credit institutions in Portugal, and by the most relevant authorities in the national and international financial system, such as Banco de Portugal, the European Central Bank, and the Bank for International Settlements.

Lisbon and Porto are, respectively, the capital and the second largest city (and also the only two metropolitan areas in the country). The historical centers of these two cities contain the so-called *urban rehabilitation areas* in which, by law, the municipality has a *right of first refusal*. This gives the municipality the right to buy the dwelling for the price agreed between the owner and the buyer for any real estate transaction in the specified city center areas. Therefore, the municipality has individual records of every such transaction. To preserve anonimity, the data is aggregated to the "neighborhood" level. For the same reason, price data is missing whenever the number of transactions in a neighborhood in a given

[18] See `https://travelbi.turismodeportugal.pt/pt-pt/Paginas/PowerBI/rnal-registo-nacional-de-alojamento-local.aspx`.

quarter is too low. Since the partition of the two cities into neighborhoods in this dataset comes from the information system of the two municipalities, it fits the areas in the law that implemented the short-term rental freeze. Therefore, we have a one-to-one correspondence between treatment and comparison areas and the price series.

Thirdly, we analyze a non-official data set (obtained from *Inside Airbnb*), that contains monthly information about the price of active Airbnb listings in Lisbon (among other individual details, such as the number of reviews of a listing and the number of different same owner's listings on the platform), between April 2018 and September 2019. Data provided by Inside Airbnb is scraped from information that is publicly available on the Airbnb site. Official data is not provided by the platform. Airbnb represents 74% of the activity among peer-to-peer housing platforms in Lisbon, 64 p.p ahead of its closest competitor, according to AirDNA, one of the largest databases on short-term rental analytics. We restrict our analysis to entire dwellings in the Airbnb platform, i.e., we remove rooms from our sample.

These three data sets provide us with our five outcome variables, namely, *(a)* registries, *(b)* Airbnb listings, *(c)* Airbnb prices, *(d)* number of transactions, and *(d)* house prices.

Finally, we rely on publicly available information from *Lisboa Aberta*, a data bank of the municipality of Lisbon, to show that treated and comparison areas are similar across a number of characteristics.[19] In particular, we retrieve information about urban amenities (architecture and heritage sites, parks, playgrounds and shopping areas), education and cultural local public goods (schools, art galleries, museums, religious cult venues, and cinemas and theaters), mobility infrastructures, and police stations.

We report descriptive statistics for the five outcome variables, and selected observables in Table 1. One interesting aspect is that, according to registration data, the average number of rooms of short-term rentals is two. The (unreported) median is also two bedrooms. This fact motivates our heterogeneity analysis, where we split the sample according to the number of bedrooms.

[19] See `http://dados.cm-lisboa.pt/dataset`.

Table 1: Descriptive Statistics on Sample Characteristics

	Mean	Stand Dev	Max	Min
(a) Registrations				
Number of Streets	590	-	-	-
Registries (per Street)	1.0	(2.3)	36	0
Rooms (per Registry)	2.0	(1.8)	73	0
% Individual	0.480	-	-	-
% Portuguese Ownership	0.953	-	-	-
(b) Airbnb Listings and (c) Airbnb Prices				
Number of Listings (per Neighborhood)	120.6	(196.5)	1121	1
Average Price (per Listing)	91.8	(105.1)	3000	9
Average Quarterly Reviews (per Listing)	2.1	(1.7)	13.8	0
% Multiple Listings	0.729	-	-	-
(d) Housing Sales				
Number of Neighborhoods	60	-	-	-
Average Sales (per Neighborhood)	18.4	(15.6)	106	1
(e) Housing Prices (m2)				
Number of Neighborhoods	42	-	-	-
Average Price (per Neighborhood)	3419.0	(1104.1)	8426	1414

3.2 Methodology

We exploit the quasi-experimental nature of the legislated change to obtain estimates of the causal impact of the freeze on five outcome variables. Treated units correspond to the areas with a freeze on new short-term rental registrations, as per the municipality decision 677/AML/2018.

We use two different definitions of the comparison group. For the registries, we use the *updated areas of absolute contention* in Figure 4, i.e., those where the 2019 ban was implemented. This is a natural choice, given that these neighborhoods are sufficiently close to the originally treated ones that they are included in the freeze one year later. As Neumark & Simpson (2015) discuss, in the context of place-based policies and their analysis, a reliable counterfactual, in the absence of random treatment assignment, might consist in "geographic areas that were either considered or qualified for treatment, or even designated as treatment zones in other periods" (Neumark & Simpson 2015, p.23). The comparison group which, for brevity, we call *2019 Freeze*, corresponds to the neighborhoods of *Baixa, Liberdade, Almirante Reis, Colinas de Santana, and Graça.* Treated zones had short-term rental to total property ratios of 27% and 29% (slightly above the limit), while the *2019 Freeze* ones had ratios of 18% and 25% (the latter just at the cut-off).[20]

For the remaining outcome variables, we have to make a slight modification in the comparison group. Indeed, when the possible extension of the freeze started being discussed in April 2019, the debate revolved around two neighborhoods – *Colinas de Santana and Graça* – that we remove from the comparison group because we cannot exclude that the market was influenced by anticipation effects. The remaining ones (*Baixa, Liberdade, and Almirante Reis*) were explicitly excluded from the April 2019 discussion, given the importance of the service sector in these locations, and hence we can rule out anticipation effects. These three locations are, therefore, our *Corrected 2019 Freeze* group. We augment the comparison group with a set of neighboring civil parishes, adjacent to the November 2018 and November 2019 freezes, also displayed in Figure 4. Nevertheless, to mitigate further concerns regarding possible anticipation effects, we run our analysis only until the end of the second quarter for Airbnb data and until the end of the third quarter for real estate data. This choice is motivated by the fact that there are usually some delays in real estate markets as prices and quantities observed in a quarter may reflect negotiations that took place a few months before.

In a nutshell, Table 2 summarises the different analyzes, including the comparison groups used in each sample.

[20] The 5^{th} zone with the highest short-term rental intensity had a ratio of 10%.

Table 2: Comparison Groups

Outcome Variable	Unit	Period	Onset of Treatment Period	Comparison Group
Registries	Street	2015 Q1–2018 Q4	Electoral campaign (2017 Q3)	2019 Freeze
Airbnb Quantities	Neighborhood	2018 Q2–2019 Q2	Parliament discussion (2018 Q3)	Corrected 2019 Freeze + Neighbors
Airbnb Price	Dwelling	2018 Q2–2019 Q2	Parliament discussion (2018 Q3)	Corrected 2019 Freeze + Neighbors
Housing Sales	Neighborhood	2017 Q1–2019 Q3	Parliament discussion (2018 Q3)	Corrected 2019 Freeze + Neighbors
Housing Prices	Neighborhood	2017 Q1–2019 Q3	Parliament discussion (2018 Q3)	Corrected 2019 Freeze + Neighbors

The *2019 Freeze*, and *Neighbors* areas are defined as in Figure 4.

Our main strategy is to estimate difference-in-differences specifications for each of the outcome variables, according to the following equation:

$$\begin{aligned}\ln(y_{ipq}) =& \alpha_p \times \mathbb{1}_p + \lambda_q \times \mathbb{1}_q + \beta_1 Treated_i \times Discussion_q + \beta_2 Treated_i \times Approval_q \\ &+ \beta_3 Treated_i \times Implement_q + \gamma X'_{iq} + \epsilon_{ipq}\end{aligned} \tag{1}$$

where y_{ipq} is the outcome variable for the unit of observation i, in civil parish p in quarter q, α_p is a civil parish fixed effect, λ_q is a quarter fixed effect, γ is a vector of coefficients for each of the time-varying controls X_{iq}, and ϵ_{ipq} is an error term. We use a full set of indicators for the civil parish, $\mathbb{1}_p$, and quarter, $\mathbb{1}_q$. When the unit of observation i is in the treated area, the indicator $Treated_i$ is equal to 1. In addition, $Discussion_q$, $Approval_q$, and $Implement_q$ are indicator variables that turn on when the quarter q belongs to the discussion, approval, or post-implementation period, respectively. The use of multiple interactions is motivated by the fact that the entire process, which led to the suspension of new short-term rental units, consisted on various stages that may have induced different behavioral effects.[21]

We consider the following outcome variables: *(a)* registries, aggregated to the street level; *(b)* Airbnb listings, aggregated at the neighborhood level; *(c)* Airbnb prices, observed at the

[21] Recent developments in the difference-in-differences literature highlight challenges in designs that exploit staggered treatments. Goodman-Bacon (2021), for example, shows that the OLS estimate in those cases is a weighted average of all underlying two-by-two difference-in-differences estimates. Therefore, aggregated weights might be negative even if all underlying two-by-two effects are positive. In this paper, we avoid these problems as we are mostly comparing early vs late treated (sometimes augmented with other never treated areas for robustness).

dwelling level, *(d)* number of transactions, and *(e)* house prices, both aggregated to the neighborhood level. Logs are used due to the right-skewness in the dependent variables' distributions. For robustness, we control for Civil Parishes' political alignment with the Mayor's party (i.e., a binary variable that takes value one if the civil parish is governed by the Socialist Party, and zero otherwise), and the turnout rate in 2013 and 2017 local elections. To account for (possible) serial correlation within the panel units, standard errors were clustered (Bertrand et al. 2004). This was done at a sufficiently aggregated level (Civil Parish), a conservative approach suggested by Cameron & Miller (2015).

We also explore heterogeneity results across housing sizes, by estimating (1) for sub-samples depending on the number of bedrooms of the dwellings. This is motivated by the fact that short-term rental properties are small, on average, as displayed in Table 1.

We also carry out event study exercises, according to the following dynamic effects equation:

$$\ln(y_{ipq}) = \alpha_p \times \mathbb{1}_p + \lambda_q \times \mathbb{1}_q + \sum_{q \in pre-treat} \delta_q \times Treated_i \times \mathbb{1}_q + \sum_{q \in post-treat} \delta_q \times Treated_i \times \mathbb{1}_q + \epsilon_{ipq} \quad (2)$$

where the variables and coefficients are defined as in Equation (1), and δ_q are a full set of pre- and post-treatment effects. As detailed in Table 2, the treatment period is the third quarter of 2018 for the Airbnb and real estate market specifications, and the third quarter of 2017 for the registries. The omitted quarter is the one immediately before the Treatment Period as defined in Table 2.

The conduction of these event studies is quite important as it allows us to formally test if, prior to the discussion on restrictive regulation, the concentration of short-term rental registries and the house prices displayed parallel trends.

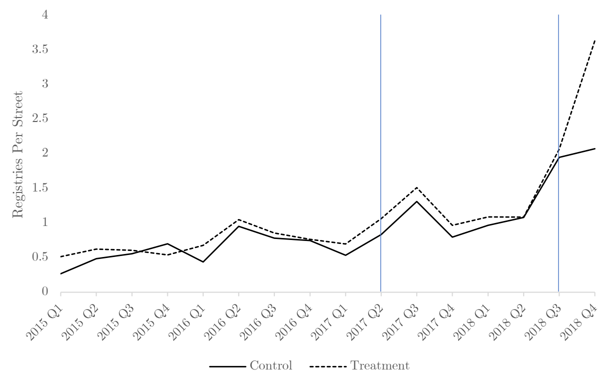
3.3 Threats to identification

In this subsection, we briefly discuss possible threats to our identification strategy, and how we mitigate these possible concerns.

One of the possible issues is anticipation, i.e., the fact that treatment assignment may have occurred before the period defined in Table 2 for each outcome variable. Note that our treatment period does not begin when the policy is implemented, but when it starts being discussed in the public domain, i.e. in the parliament discussion or municipal electoral campaign, depending on the outcomes. With this definition of treatment, we err on the side of caution.

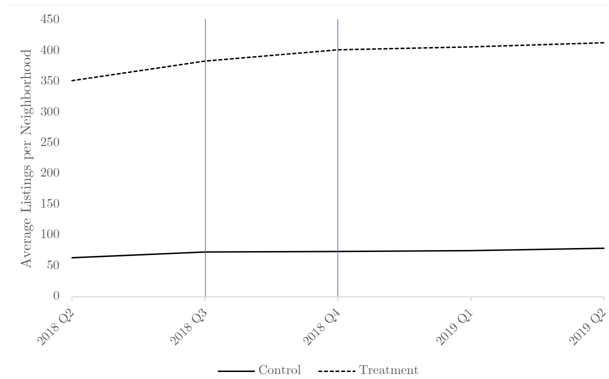
The trends for the five outcome variables, shown in Figure 5, confirm the absence of anticipatory effects. The blue lines separate the pre-treatment and the different post-treatment periods. In panel (a) we can immediately see the spike in registries in the treated areas. Importantly, there is no noticeable increase in the number of registries in control areas, showing that there were no anticipation effects of the ban introduced one year later.

Figure 5: Trends for Outcome Variables



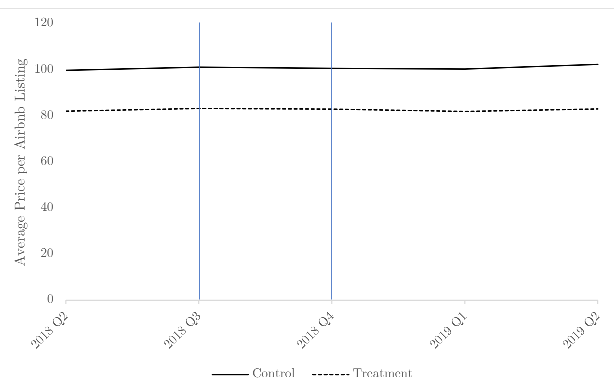
(a) Registries

N= 9440. Control: 2019 Freeze



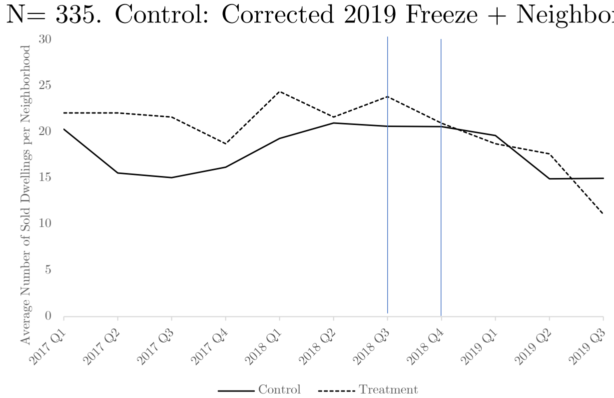
(b) Airbnb Listings

N= 335. Control: Corrected 2019 Freeze + Neighbors



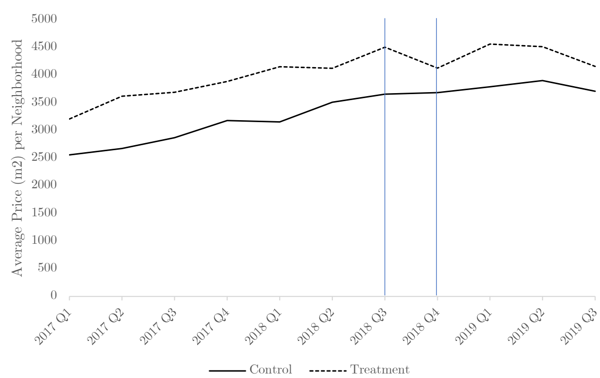
(c) Airbnb Prices

N= 32675. Control: Corrected 2019 Freeze + Neighbors



(d) House Sales

N= 660. Control: Corrected 2019 Freeze + Neighbors



(e) Housing Prices

N= 462. Control: Corrected 2019 Freeze + Neighbors

Moreover, the event studies defined in Equation (2) and estimated in Section 4 confirm the absence of pre-treatment differences in the treated and comparison areas, which lends support to our definition of the treatment period and rules out anticipatory effects.

Another possible concern is related to the differences in treated and comparison areas that may confound the estimation of causal effects. We provide a battery of balance tests about the characteristics of the treatment and comparison areas in Table 3. For the sake of simplicity, we focus on the comparison areas defined by the 2019 Freeze. Note that all these variables are measured at the geographical unit of observation considered in the analysis, which is far smaller than the civil parish. This also ensures that the treatment and comparison areas are geographically very close, which *per se* mitigates possible confounding factors.

The first set of characteristics pertains to urban amenities. There is evidence suggesting that both cultural heritage amenities (which includes includes churches, palaces, and other historic buildings) and the proximity to large urban forests and smaller neighborhood parks are capitalized through residential prices in Lisbon (Franco & Macdonald 2018*b*,*a*). We consider all these characteristics. More precisely, we use the aesthetic quality of the buildings, proxied by the number of buildings with the prestige architectural prize *Prémio Valmor*, and the number of municipal public interest buildings, such as libraries, churches, historical hotels or schools. We also look at parks and the number of trees, together with children playgrounds. In addition, we consider shopping areas. Reassuringly, none of these urban amenities is statistically different across treatment and comparison areas.

Next, we consider education and culture local public goods, namely, the number of art galleries, museums, cinemas and theaters, religious cult venues, and schools. Again, we find no statistically significant difference between treatment and comparison areas in any of these characteristics.

The third attribute that we analyze is the urban mobility infrastructure of each area. We show that the number of metro stations and electric car charging facilities (MobiE) is also not statistically different across the treatment and comparison neighborhoods.

The fourth comparison pertains to the number of police stations, and again we do not find a statistically significant difference between the number of these facilities in treated and comparison areas.

Finally, we discuss the possibility of spillovers between treatment and comparison areas. Note that the evidence in Figure 5 does not suggest, for all outcomes, contagion across the two areas. More robust evidence will be discussed in Section 4.1, where we show that there was no increase in registrations in the streets at the border of the treated (frozen) areas, when the ban was implemented.

Table 3: Balance Tests: characteristics of the areas

	Pre-Treatment		
	Treatment	Control	Difference (p-value)
Urban amenities			
Buildings with *Prémio Valmor*	1.250	1.700	-0.450
			(0.567)
Public interest buildings	2.500	2.650	-0.150
			(0.883)
Parks	0.917	0.650	0.267
			(0.202)
Number of trees	103.500	154.800	-51.300
			(0.481)
Children playgrounds	0.667	0.100	0.567
			(0.121)
Shopping areas	0.250	0.400	-0.150
			(0.393)
Education and culture public goods			
Schools	0.500	0.350	0.150
			(0.526)
Art galleries	2.167	0.800	1.367
			(0.140)
Museums	1.500	0.600	0.900
			(0.109)
Cinemas and theaters	0.750	0.500	0.250
			(0.413)
Religious cult venues	0.417	0.400	0.017
			(0.858)
Urban mobility			
Metro stations	0.083	0.450	-0.367
			(0.113)
Electric car charging facilities (MobiE)	0.750	0.400	0.350
			(0.278)
Police			
Police stations	0.167	0.250	-0.083
			(0.388)
Observations	12	20	32

Notes: The control area is defined in *2019 Freeze*. *Prémio Valmor* is the most prestigious architectural honor in Portugal to be awarded to a building.

4 Results

In this section, we present the results of the estimation of equations (1) and (2) for each of the outcome variables. We also exploit possible heterogeneity in all samples.

4.1 Registries

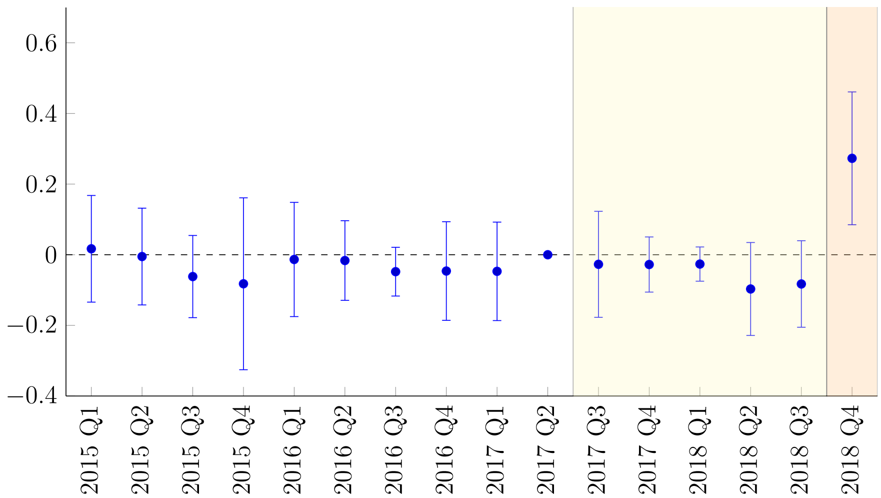
The law that allowed municipalities to regulate new registries in the short-term rental market was passed in late August 2018, but only became effective two months later. This lag could, in theory, allow the owners that are considering moving into the short-term rental market to register their properties. This may curb the intended effects of the law.

In order to account for this legislative change, we aggregate registries at the quarterly level, with the last quarter defined as September to November, and the remaining ones changed accordingly.[22]

The event study in Figure 6 shows that the parallel trend hypothesis holds in the pre-treatment period. The orange shaded area spans a period before the ban and another one after the ban is implemented. The figure documents a sizeable spike in the number of registries in the treated areas, suggesting a rush to register before the law became binding, possibly undermining its goals.

[22] That is, each quarter begins one month earlier, and therefore the first quarter of each year actually begins in December of the previous year. This correction is done only in this part of the analysis (i.e., registries).

Figure 6: Event Study - ln(Registries)



Notes: N= 9440. The comparison group is the Freeze 2019 area. This regression includes civil parish and quarter fixed effects. Each quarter is corrected to begin one month earlier. The shaded area corresponds to the post-treatment period, namely to the discussion (in yellow) and the approval (in orange). The 95% confidence levels are clustered at the civil parish level.

The estimates of Equation (1) in Section 4.1 imply a similar interpretation. Looking at the specification in Column (2), which is our baseline, the coefficients suggest that, although there was no significant reaction to the initial public discussion over short-term rental regulation, streets on the originally treated areas experienced a short-term increase of 30.9% in the quarterly number of registries as a result the law's approval. Results remain stable if we add the neighboring civil parishes in Lisbon (Column 4) and touristic areas in Porto (Column 5) to the comparison area.[23]

We find strong evidence of treatment heterogeneity across different types of dwellings and ownership status. Table 5 shows that the effect was higher for owners of smaller units, confirming the fact, already highlighted, that the latter are the main segment of the short-term rental market. There is no difference regarding the ownership of the dwelling, i.e., if it belongs to an individual or a firm.

[23] In Porto, we only consider the civil parish *União de Freguesias do Centro Histórico do Porto.* This is comprised by the previous (prior 2013-reform) civil parishes of *Cedofeita, Santo. Ildefonso, Sé, Miragaia, S. Nicolau, and Vitória.* We cannot use this Porto area in the comparison group in future periods since short-term rental regulations were also implemented in this area in 2019 (See `publico.pt/2019/07/10/local/noticia/porto-suspende-novos-registos-alojamento-local-centro-historico-bonfim-1879480` (in Portuguese)).

The most important result in Table 5 is the stronger reaction by domestic owners, who presumably were more aware of the anticipated discussion of the ban. The point estimate for foreign owners is one third of the magnitude and it is not significant. We take this as additional evidence that the law was ineffective at curbing the share of short-term rental properties in the designated areas, at least in the short run, due to anticipation effects by the informed parts of the market.

Table 4: Difference-in-Differences - ln(Registries)

Comparison:	2019 Freeze			+ Neighbors	+ Porto
	(1)	(2)	(3)	(4)	(5)
$Treat \cdot Discussion$	-0.016	-0.016	0.006	0.010	-0.017
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
$Treat \cdot Approval$	0.309***	0.309***	0.337***	0.383***	0.397***
	(0.04)	(0.04)	(0.05)	(0.03)	(0.03)
Quarter FE	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	No	Yes	Yes	Yes	Yes
Political Controls	No	No	Yes	No	No
Number of Obs.	9440	9440	9440	20288	27008
Adjusted R^2	0.085	0.093	0.093	0.122	0.108

Notes: The treated and comparison areas, as well as Neighbor Civil Parishes, are defined as in Figure 4. The vector of controls consists of Civil Parishes' political alignment with the Mayor's party and turnout rate. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Heterogeneous Effects - ln(Registries)

	Rooms (Median)		Ownership Status		Nationality	
	Below/=	Above	Individual	Firm	Domestic	Foreign
$Treat \cdot Discussion$	-0.003	-0.015	-0.015	0.000	-0.024	0.020
	(0.02)	(0.04)	(0.01)	(0.04)	(0.03)	(0.03)
$Treat \cdot Approval$	0.296***	0.196**	0.251***	0.246***	0.304***	0.092
	(0.06)	(0.03)	(0.05)	(0.04)	(0.04)	(0.07)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes	Yes	Yes	Yes
Political Controls	No	No	No	No	No	No
Number of Obs.	8704	6544	7920	8352	9376	3504
Adjusted R^2	0.095	0.073	0.081	0.080	0.090	0.064

Notes: The comparison group is the Freeze 2019 area. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The magnitude of the effects in Section 4.1 and Table 5, along with the evidence that this result is driven by domestic incumbent owners, suggests that the anticipation period effectively suppressed the impact of the ban in the short run. We now provide a further test of this hypothesis, by re-estimating Equation (1) with a treatment group that consists of the streets located right outside the suspension border. If the ban is binding, we expect to see an increase in the number of registries in the "second-best" locations, i.e, the closest possible to the banned area, after the law becomes effective.

Table 6 exhibits the results. Our baseline specification shows that there were no statistically significant spillovers, even after the suspension became binding, ruling out any displacement effects. This evidence also constitutes an important validation of our identification strategy, as discussed in Section 3.3, because it confirms that there was no contagion of comparison areas.

Table 6: Spillover Effects - ln(Registries)

Comparison:	2019 Freeze		
	(1)	(2)	(3)
$Treat \cdot Discussion$	0.005	0.005	0.008
	(0.05)	(0.05)	(0.05)
$Treat \cdot Approval$	-0.008	-0.008	-0.004
	(0.08)	(0.08)	(0.08)
$Treat \cdot Implement$	-0.002	-0.002	0.001
	(0.04)	(0.04)	(0.05)
Quarter FE	Yes	Yes	Yes
Civil Parish FE	No	Yes	Yes
Political Controls	No	No	Yes
Number of Obs.	4218	4218	4218
Adjusted R^2	0.050	0.080	0.079

Notes: The comparison group is the remaining Freeze 2019 area. The vector of controls consists of Civil Parishes' political alignment with the Mayor's party and turnout rate. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Airbnb Listings

For the remainder of this paper we focus our attention on the effect of the ban on both the short-term rentals and the real estate market. The comparison group is the *corrected 2019 freeze*, i.e., we remove the neighborhoods that were banned in 2019 but whose inclusion in the ban was discussed beforehand. We also add the areas in the neighboring civil parishes.

We start by focusing on the impact on Airbnb quantities, aggregated at the neighborhood level, in this subsection, and on Airbnb prices in the next. One important limitation of our analysis is that data is available as of the third quarter of 2018; therefore, we only have one pre-treatment quarter. This does not allow us to explicitly test the parallel trend assumption.

The difference-in-differences results can be seen in Table 7 and show that, despite the increase in registries in the last quarter of 2018, listings in the market basically did not change, at least in the short-run.

Table 7: Difference-in-Differences - ln(Airbnb Listings)

	Corrected 2019 Freeze + Neighbors
	(1)
$Treat \cdot Approval$	0.007
	(0.03)
$Treat \cdot Implement$	-0.021
	(0.03)
Quarter FE	Yes
Neighborhood FE	Yes
Political Controls	No
Number of Obs.	335
Adjusted R^2	0.395

Notes: The comparison group is the Corrected 2019 Freeze + Neighbors. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

We also inspect (possible) heterogeneity effects by re-estimating Equation (1) splitting the sample by the number listings of the owner on the platform (single or multiple) and below and above the median number of reviews. This latter is a proxy of quality, as more reviews signal a property that is rented more often. In all cases, presented in Table A1 in the Appendix, point estimates are very close to zero and not statistically significant.

4.3 Airbnb Prices

We now present the results for the rental prices of Airbnb listings. All prices are per night and dwelling. Furthermore, since our unit of observation is the dwelling, we estimate the equations with dwelling fixed effects. This is important as these control for the surface of the property and other time-invariant factors, and allows us to run the regression on total rental price.

The difference-in-differences results are presented in Table 8; the magnitude of the effect is negligible and, moreover, not statistically significant. This constitutes very strong evidence that the ban of new registries had no impact on the prices of Airbnb listings, at least in the short-run.

Table 8: Difference-in-Differences - ln(Airbnb Prices)

	Corrected 2019 Freeze + Neighbors	
	(1)	(2)
$Treat \cdot Approval$	-0.002	-0.002
	(0.01)	(0.01)
$Treat \cdot Implement$	-0.005	-0.004
	(0.01)	(0.01)
Quarter FE	Yes	Yes
Dwelling FE	No	Yes
Political Controls	No	No
Number of Obs.	32675	32675
Adjusted R^2	0.013	0.008

Notes: The comparison group is the Corrected 2019 Freeze + Neighbors. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In addition, the results in Table A2 in the Appendix point to the absence of effects in the various subgroups. However, there is a slight decrease of 0.8% in the rental price of dwellings belonging to single-property owners. This small effect may signal excess supply given the surge in registries, but it is too small to be economically meaningful.

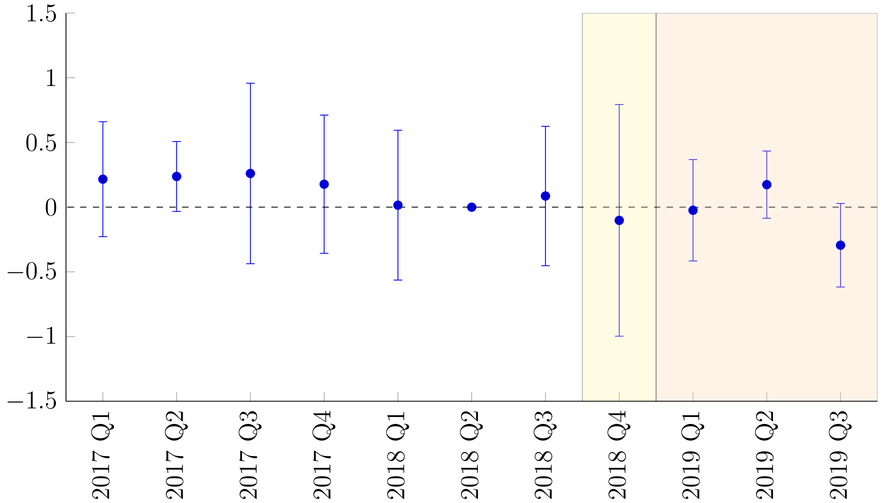
4.4 Number of Sold Houses

For the remainder of the paper, our unit of observation is the neighborhood and the comparison group is the *corrected 2019 freeze*, augmented with neighboring civil parishes, as explained in Section 3.2. We removed all neighborhoods for which future bans on short-term rentals were expected. In any case, our period of analysis excludes the last quarter of 2019. Recall that the 2019 ban was introduced in November, and was discussed as of the Spring. In order to limit possible confounding effects, our analysis stops in the third quarter of 2019, that is, one more quarter than the Airbnb ones, to accommodate the lag in the negotiation and contracting of housing sales. Our final sample includes nine treated neighborhoods, that we compare with 54 in the comparison group.[24]

We first analyze the effects on property transactions. The event study in Figure 7 shows convincing evidence of pre-treatment parallel trends. Moreover, in the third quarter of 2019, the number of houses sold in treated areas decreases *vis-à-vis* houses in the comparison area, but the difference is, albeit just marginally, significantly different from zero at a 95% confidence level.

[24] Treated neighborhoods are *Madragoa, Bairro Alto, Bica, Príncipe Real, Santa Catarina, São Paulo/ Boavista/ Conde Barão, Alfama, Mouraria, and Sé.*

Figure 7: Event Study - ln(Number of Sold Houses)



Notes: N= 660. The comparison group is the Corrected 2019 Freeze + Neighbors. The shaded area corresponds to the post-treatment period, namely to the approval (in yellow), and the implementation (in orange). The 95% confidence levels are clustered at the civil parish level.

Evidence from difference-in-differences regressions, presented in Table 9, suggests that the suspension had a negative effect in the number of sold houses, although this is only statistically significant after its implementation, with an estimated decrease of 19.6% in the number of transactions after the law became effective. Column (4) re-estimates (1), restricting the analysis to the neighborhoods for which we observe prices, i.e., those with more than three house transactions in all quarters (five treated vs. 37 comparison neighborhoods). The neighborhood level is a fairly small geographical scale, which increases the likelihood that in some quarters there are not enough transactions. As expected, the effect in this sub-sample is even more negative, because we are eliminating the areas where the real estate market is less liquid.

Table 9: Difference-in-Differences - ln(Number of Sold Houses)

Comparison:	Corrected 2019 Freeze + Neighbors			
	(1)	(2)	(3)	(4)
$Treat \cdot Approval$	-0.250	-0.250	-0.255	-0.166
	(0.24)	(0.25)	(0.24)	(0.11)
$Treat \cdot Implement$	-0.196**	-0.196**	-0.201**	-0.465***
	(0.08)	(0.08)	(0.07)	(0.05)
Quarter FE	Yes	Yes	Yes	Yes
Civil Parish FE	No	Yes	Yes	Yes
Political Controls	No	No	Yes	No
Number of Obs.	660	660	660	462
Adjusted R^2	0.025	0.156	0.153	0.148

Notes: The vector of controls consists of Civil Parishes' political alignment with the Mayor's party and turnout rate. In Column (4) we restrict our sample to neighborhoods that witnessed more than 3 house transactions in all quarters. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The anticipation effect that led to a surge in the number of registries is not expected to impact the real estate market, as the short-term rental licence belongs to the owner and is lost with the property transaction. Potentially, there could be two effects of the ban on property sales. On the one hand, there could have been a rush to buy houses in the period before the ban, which the evidence in Figure 7 rules out. On the other hand, the elimination of the option value of the short-term rental licensing may depress demand. The evidence in Table 9 supports the argument that the option to participate in the short-term rental market is an important determinant of the housing market demand in these areas.

We re-estimate the difference-in-differences specification, splitting the sample according to the number of rooms of each dwelling. The results in Table 10 show that the effect is driven by housing units of smaller dimensions, confirming the segmentation of the market along this dimension. The effect is the strongest in two-bedroom dwellings, along with the evidence on Table 1 that this is the average and median short-term rental property.

Table 10: Heterogeneous Effects - ln(Number of Sold Houses)

	Number of Rooms		
	1 Room	2 Rooms	3 Rooms
$Treat \cdot Approval$	0.110	-0.105	-0.036
	(0.15)	(0.17)	(0.10)
$Treat \cdot Implement$	-0.219	-0.259***	-0.142*
	(0.12)	(0.06)	(0.07)
Quarter FE	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes
Political Controls	No	No	No
Number of Obs.	584	584	557
Adjusted R^2	0.165	0.116	0.059

Notes: The comparison group is the Corrected 2019 Freeze + Neighbors. We do not consider dwellings with more than 3 rooms given their low frequency in the data. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

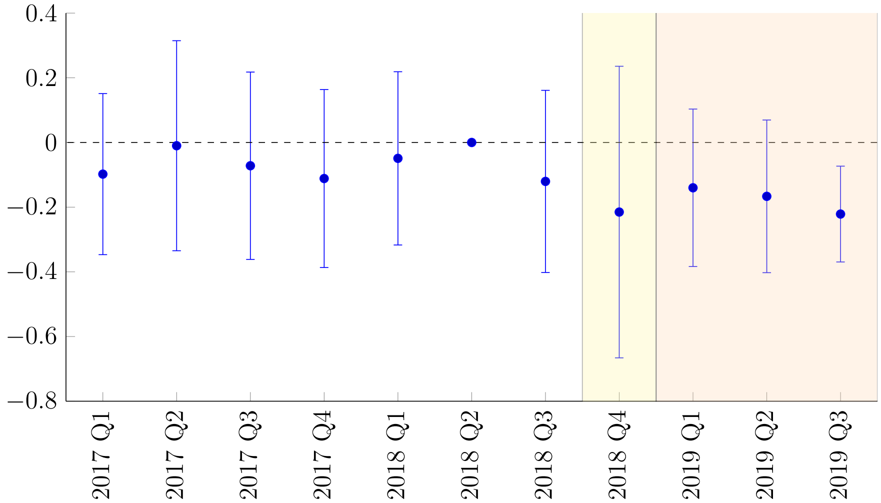
4.5 Housing Prices

We now focus on the effects on house prices. For anonymity reasons, as explained in Section 3.2, prices are omitted in our data for neighborhoods with less than four transactions. These neighborhoods are dropped from our analysis. Therefore, we rely on the comparison between five treated with 37 comparison neighborhoods.[25] For comparison purposes, this is the sample used in Column (4) of Table 9.

The event study design shown in Figure 8 highlights that prices follow parallel trends before the treatment. Moreover, prices are impacted negatively, but this effect comes with a lag, as the coefficient is statistically significant only in the third quarter of 2019.

[25] The five treated neighborhoods are *Madragoa, Bairro Alto, Santa Catarina, Alfama, and Mouraria.*

Figure 8: Event Study - ln(Housing Prices)



Notes: N= 462. The comparison group is the Corrected 2019 Freeze + Neighbors. The shaded area corresponds to the post-treatment period, namely to the approval (in yellow), and the implementation (in orange). The 95% confidence levels are clustered at the civil parish level.

We also compute difference-in-difference regressions for housing prices. The estimated coefficients from equation (1) are displayed in Table 11 and suggest that the suspension of short-term rental registries induced a decrease in prices, after the law became binding. The results are quite robust across specifications and point to a 8.6% price decrease following the implementation. These results confirm that the option of registering the dwelling for a short-term rental was valued by potential buyers.

To infer (possible) heterogeneity in these effects, we re-estimate the difference-in-differences specification by the number of rooms in each dwelling. The results, displayed in Table 13, again confirm that the effect is mostly driven by dwellings with two bedrooms, which experienced a price decrease which is 20% above the ones in the comparison areas.

Table 11: Difference-in-Differences - ln(Housing Prices)

Comparison:	Corrected 2019 Freeze + Neighbors		
	(1)	(2)	(3)
$Treat \cdot Approval$	-0.125	-0.125	-0.121
	(0.09)	(0.09)	(0.10)
$Treat \cdot Implement$	-0.086***	-0.086***	-0.082***
	(0.03)	(0.03)	(0.03)
Quarter FE	Yes	Yes	Yes
Civil Parish FE	No	Yes	Yes
Political Controls	No	No	Yes
Number of Obs.	462	462	462
Adjusted R^2	0.244	0.745	0.744

Notes: Controls include Civil Parishes' political alignment with the Mayor's party and turnout rate. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01

Table 12: Heterogeneous Effects - ln(Housing Prices)

	Number of Rooms		
	1 Room	2 Rooms	3 Rooms
$Treat \cdot Approval$	-0.062	-0.214	0.048
	(0.10)	(0.18)	(0.08)
$Treat \cdot Implement$	-0.035	-0.197**	-0.019
	(0.05)	(0.08)	(0.06)
Quarter FE	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes
Political Controls	No	No	No
Number of Obs.	304	318	232
Adjusted R^2	0.585	0.633	0.643

Notes: The comparison group is the Corrected 2019 Freeze + Neighbors. We do not consider dwellings with more than 3 rooms given their low frequency in the data. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Finally, we test how the ban of new short-term rental registries affected the dispersion of the prices in the housing market. To do this, Equation (1) is re-estimated substituting mean prices by prices in percentile 25 (Columns 1 to 3), and in percentile 75 (Columns 4 to 6). Recall that all the prices are per square meter. Table 13 shows the results. We find that most substantial decreases (between 8% and 10%) are concentrated in the most expensive dwellings. Properties in percentile 25 do not seem to be affected by the ban.

Table 13: Difference-in-Differences - ln(Price Percentiles)

	Percentile 25			Percentile 75		
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat \cdot Approval$	-0.020	-0.043	-0.39	-0.113	-0.140	-0.135
	(0.10)	(0.12)	(0.13)	(0.09)	(0.10)	(0.10)
$Treat \cdot Implement$	-0.034	-0.056	-0.053	-0.076*	-0.099***	-0.096***
	(0.05)	(0.05)	(0.04)	(0.04)	(0.02)	(0.02)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	No	Yes	Yes	No	Yes	Yes
Political Controls	No	No	Yes	No	No	Yes
Number of Obs.	319	319	319	319	319	319
Adjusted R^2	0.254	0.642	0.639	0.213	0.713	0.712

Notes: The comparison group is the Corrected 2019 Freeze + Neighbors. Controls include Civil Parishes' political alignment with the Mayor's party and turnout rate. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Conclusion

We exploit the quasi-experimental evidence from a zoning regulation implemented in Lisbon in 2018, which banned the registry of new short-term rental properties in some areas of the city. Our empirical strategy provides causal evidence about the impact of the short-term rental market regulation on the number of registries, short-term listing prices and quantities, and the real estate market, i.e., number and price of transactions.

We dismiss possible threats to identification, namely, anticipation effects, contagion to comparison areas, and pre-treatment differences in treatment and comparison areas, with a comprehensive set of descriptive evidence and regression results.

We provide a number of novel results. The first pertains to the effects of the anticipation of the law discussion. We find convincing evidence that the incumbent owners rushed to register properties in the banned areas in the days leading up to the effective prohibition. Interestingly, the spike is noticeable for Portuguese owners, but not for foreign ones, showing that the anticipatory effect depends on the awareness about policy discussions. Our second result is a consequence of the former: there were no effects on listings and prices of short-term rents in the immediate period following the ban. This confirms that the incumbent owners who had the intention to register their dwelling as short-term rental did so in the weeks before the ban was effective.

Our next set of results pertains to the real estate market. We find evidence of a decrease in demand for houses in the treated areas, shown by the event study to lag the ban by two quarters. In addition, there was no impact on housing sales in the period of discussion of the ban. Therefore, the announced ban did not trigger a run to buy houses in the designated areas. The causal impact of the short-term registry ban in the number of sales was a contraction of 20% in treated areas *vis-à-vis* the comparison ones. Prices decreased by 8%, an effect that lags the ban by two quarters, according to the event studies. This shows that the option to rent the dwelling in the short-term rental market is an important determinant of the demand for houses in the treated areas.

Finally, our findings demonstrate that that short-term rentals segment the real estate market. The price and quantity effects in the real estate market are mostly driven by two-bedroom properties, which is both the mean and the median number of rooms of the dwellings registered as short-term rentals. The price decrease for two-bedroom houses is almost 20%. Moreover, the price decrease is concentrated on the upmarket properties of the last quartile of price per square meter.

Our work suggests the following policy implications. Firstly, it is important to exert caution regarding long periods of anticipating discussions about zoning regulations that create incumbency rents. Importantly, there are asymmetric effects in the reaction to the anticipatory discussions, depending on the awareness about the policy discussion, as proxied by the nationality of the dwelling owners. Secondly, we provide causal evidence that a short-term rental ban curbs the housing market. This effect is concentrated in some types of properties, namely, two-bedroom apartments, size-wise, and upmarket ones, price-wise.

Our results point to a decrease of between 8% and 20%, depending on the specification, of housing prices. What can we infer about the impact of short-term rentals on housing prices? We cannot claim that short-term rental increased prices by *only* this amount for two reasons. On the one hand, we analyze short-run effects in this paper, while the short-term rental growth was consistent for several years. On the other hand, the spike in the registries before the implementation implies that our estimates are a lower bound of the *true* impact of short-term rental density. With these limitations in mind, we recall that the surge in housing prices in treated areas between 2016 and 2018, i.e., before the ban, was above 37.5%, as displayed in Figure 2. Short-term rentals seem to explain part of the surge, but our results suggest that the challenge of housing affordability ought to be tackled with a more comprehensive set of policies. The tourism and short-term rental boom, despite its salience, is only one of the factors behind the compounded growth of 68.2% in real estate prices in the city of Lisbon between 2016 and 2019. Other public policies that may have an impact in real estate prices are interesting topics for future research.

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A Appendix

Table A1: Heterogeneous Effects - ln(Airbnb Listings)

	Number of Listings		Number of Reviews (Median)	
	Single	Multiple	Above	Below/=
$Treat \cdot Approval$	-0.022	0.032	-0.013	0.043
	(0.02)	(0.03)	(0.03)	(0.03)
$Treat \cdot Implement$	-0.028	-0.036	-0.060	0.017
	(0.03)	(0.04)	(0.04)	(0.03)
Quarter FE	Yes	Yes	Yes	Yes
Neighborhood FE	Yes	Yes	Yes	Yes
Political Controls	No	No	No	No
Number of Obs.	325	333	329	330
Adjusted R^2	0.252	0.332	0.259	0.267

Notes: The comparison group is the Corrected 2019 Freeze + Neighbors. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A2: Heterogeneous Effects - ln(Airbnb Prices)

	Number of Listings		Number of Reviews (Median)	
	Single	Multiple	Above	Below/=
$Treat \cdot Approval$	-0.002	-0.002	0.002	-0.002
	(0.00)	(0.01)	(0.01)	(0.00)
$Treat \cdot Implement$	-0.009***	-0.002	-0.004	-0.004
	(0.00)	(0.01)	(0.01)	(0.01)
Quarter FE	Yes	Yes	Yes	Yes
Dwelling FE	Yes	Yes	Yes	Yes
Political Controls	No	No	No	No
Number of Obs.	8870	23805	16335	16340
Adjusted R^2	0.008	0.009	0.016	0.005

Notes: The comparison group is the Corrected 2019 Freeze + Neighbors. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.