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Do short-term rentals increase housing prices? Quasi-experimental evidence from Lisbon

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Do short-term rentals increase housing prices? Quasi-experimental evidence from Lisbon[1]

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Abstract:

We provide causal estimates of the impact of short-term rental regulations by exploiting a quasi-natural reform implemented in the city of Lisbon. In November 2018, the municipality of Lisbon banned the registry of new short-term rental properties in some neighborhoods. We rely on two administrative data sets on short-term rental registries, between 2015 and 2019, and house transactions between 2017 and 2019. We also use data on Airbnb rental 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 prices do not seem to react to the ban. Both the number of transactions and house prices decreased after the reform, mostly in two-bedroom flats. Our findings confirm a sizeable impact of short-term rentals on real estate prices, concentrated in a segment of smaller houses.

JEL Classification: R12, R21, R30.

Keywords: Airbnb, Policy Analysis, Housing Market, Short-Term Rental, Portugal

Note: This article is sole responsibility of the authors and do not necessarily reflect the positions of GEE or the Portuguese Ministry of Economy.

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1. Introduction

Across the globe, housing affordability has been linked to the touristic boom and the advent of short-term rental digital platforms. Airbnb, one of these platforms, was created in 2008 and grew to be present in more than 80 thousand cities and raising more than 30 billion dollars in just one decade. In June 2019, ten 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".[5]

In response to this concern, several cities across the globe have implemented measures to curb the growth of short-term holiday rentals, including Amsterdam, Barcelona, Los Angeles, and New York. In this paper, we exploit a ban on new short-term rental licences introduced by the municipality of Lisbon in November 2018 in some neighborhoods. This ban provides an ideal quasi-experimental setup to estimate the causal impact of short-term rental regulations on the real estate market.

According to the OECD Affordable Housing Database, in 2015 a median income couple with two children had to spend 10.2 years of income to buy a house in the country's capital or financial center. The equivalent figure in 1985 was 6.8 years. Prices are increasing more rapidly recently. According to the 2019 Property Index publication by Deloitte, housing prices have increased since 2015 in all but one of the 16 analysed countries.[6] Portugal is one of the four countries in the with a price increase of more than 30% between 2015 and 2018. The price surge in Portugal hides considerable heterogeneity; indeed, 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 just 4 years. In 2019, Portugal hosted more than 16 million foreign tourists, up from 9 million in 2013. Lisbon was elected World's Leading City Break Destination in 2017, 2018, and 2019 by the World Travel Awards.[7] The number of overnight stays in Lisbon alone reached almost 12 million in 2019, more than 20 times its resident population. Therefore, Portugal offers a natural setup to analyse the impact of the tourism and short-term rental blast on the real estate market.

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. Given the spectacular increase in the housing prices in Lisbon and other cities, the government legislated to allow the municipalities to enforce zoning regulations that would include rules about the supply of short-term rentals. Under this law, the municipality of Lisbon implemented a ban of new short-term rental registries in November 2018. The ban was extended one year later to some adjacent neighbourhoods, which we use as the comparison group to the 2018 treatment in our difference-in-differences approach. Therefore, this is one of the first papers in the literature to provide credible quasi-experimental estimates of the impact of short-term rental marketplaces on *i)* registries, *ii)* Airbnb prices, *iii)* numbers of transactions, and *iv)* house prices.[8]

[5] See `https://www.theguardian.com/cities/2019/jun/20/ten-cities-ask-eu-for-help-to-fight-airbnb-expansion`.

[6] See `https://www2.deloitte.com/be/en/pages/real-estate/articles/deloitte-property-index-2019.html`. The publication analyses the following 16 countries: Spain, The Netherlands, Hungary, France, Austria, Belgium, Croatia, Poland, Czech Republic, Italy, Portugal, Denmark, Latvia, UK, Norway, and Germany.

[7] See `https://www.worldtravelawards.com/profile-8079-turismo-de-lisboa`.

[8] A notable exception that also relies on quasi-experimental evidence is Koster et al. (2018).

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. As a consequence, there were no effects on short-term rents in the immediate period following the ban, or even a slight price decrease for listings owned by single-property owners. This may be explained by the surge in registrations, which may have caused a temporary excess supply.

Our results on 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-a-vis* the comparison areas. Prices decreased by 8%, showing shows 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, confirming the widespread belief that the market is dominated by relatively small houses. 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 are to be avoided. Secondly, the surge of short-term rental markets creates an upward pressure on real estate prices, concentrated in some types of properties.

This paper is organized as follows. Section 2 reviews the existing literature on the effects of home-sharing markets on housing affordability. Section 3 describes the context and the institutional framework behind our empirical strategy while Section 4 discusses the empirical strategy. Section 5 presents the results. Finally, Section 6 concludes.

2. Literature Review

Metropolitan areas are land constrained and regulated (Green et al. 2005), and thus have a low housing supply elasticity (Saiz 2010) which is bound to create large price effects. According to Turner et al. (2014), there are three mechanisms through which the short term rental market impacts real estate markets. The negative externality stemming from the nuisance (increased congestion, discomfort with unfamiliar people in the neighborhood) is likely to decrease housing prices, contrary to the demand-side *efficient use effect* that increases demand for housing due to a more efficient use of the resource. Finally, the *housing supply effect* increases supply because of a reallocation of the existing housing stock away from the long-term rental market and is bound to increase rents (Sheppard et al. 2016).

The literature on the so-called sharing economy, in particular short-term rental platforms, is fairly recent. Alyakoob & Rahman (2019) show that, in New York City, an increase in the number of Airbnb reviews is associated with an increase in employees in the restaurant sector while Basuroy et al. (2020) show, in the state of Texas, that Airbnb had a positive effect on restaurant revenue. As expected, hotel revenues significantly decrease with the entry of new competitors into the market; (Zervas et al. 2017) shows that Airbnb leads hotels to price less aggressively. Although the increased competition is in principle to the benefit of consumers, 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.

A sizeable share of this literature focuses on the impact of Airbnb on real estate markets. Barron et al. (2018) use data on Airbnb's listings between 2011 and 2016 across the United States and employ an instrumental variable based on google trends, but find modest effects. Sheppard et al. (2016) employ a matched difference-in-differences strategy to estimate the causal impact of Airbnb presence 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, with the treatment defined as having Airbnb units nearby. Horn & Merante (2017) analyze data from Boston and find that a one standard deviation increase in Airbnb density leads to a 0.4% increase in local rents. A similar methodology is applied to the Portuguese real estate market by Franco et al. (2019). 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%. These amounts to an overall increase in property values of 34% due to the short-term lease regulatory reform, particularly localized in the historical centers of Lisbon and Porto. In addition, Valentin (2019) found that the regulatory reform in New Orleans (which required host to pay for an annual short-term rental license, limited the number of days an, 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. Segú (2018) finds that Airbnb is responsible for a 4% increase in Barcelona's rents between 2009 and 2016, through an instrumental approach based on a listed unit's distance to the beach. Garcia-López et al. (2019) 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.

Our paper differs from this literature in that it exploits a regulatory reform to obtain the causal impact of short-term rental in housing prices. To the best of our knowledge, the only exception that uses quasi-experimental evidence from short-term rental regulations is Koster et al. (2018). The authors 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%.

Our analysis is also related to the research agenda about other policy determinants of real estate prices. Recent contributions include Dachis et al. 2012, Besley et al. 2014, Kopczuk & Munroe 2015, Hilber & Lyytikäinen 2017, Slemrod et al. 2017, Best & Kleven 2018 on transaction taxes, (Basten et al. 2017) on income taxes, and Yinger 1982, Sirmans et al. 2008, Hilber et al. 2011, Lyytikäinen 2012, Elinder & Persson 2017, Bradley 2017 on property taxes.

3. Context and Institutional Framework

Short term rental was regulated in 2014. The decree-law 128/2014 created a simplified, straightforward registration process that can be fully conducted online. 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. Renting an unregistered unit or failing to cancel the registration of a

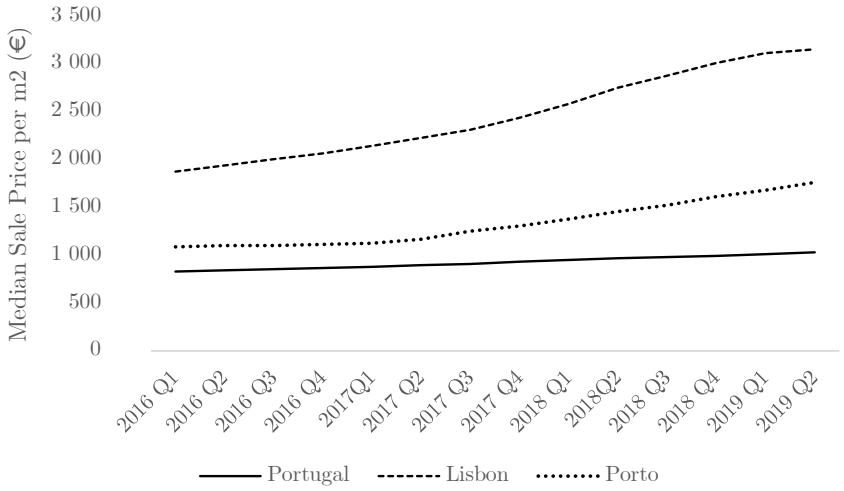
property after ceasing the rental activity is subject to a fine. Moreover, online platforms are forbidden to advertise unregistered properties. Safety regulations are verified ex-post, through random audits. Therefore, in practice, the licence is available as of the moment of registration. The license belongs to the individual, i.e., it expires when the dwelling is sold.

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. In addition, there is a tax on the capital gains stemming from the appreciation of the property during its tenure as short term rental.

The simplified regulation, together 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. The figure multiplied 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 for Lisbon and Porto increased by 68.2% and 61.9%, respectively. The growth rate for the whole country was more modest, sitting at 24.2%. This evidence is depicted in Figure 1.

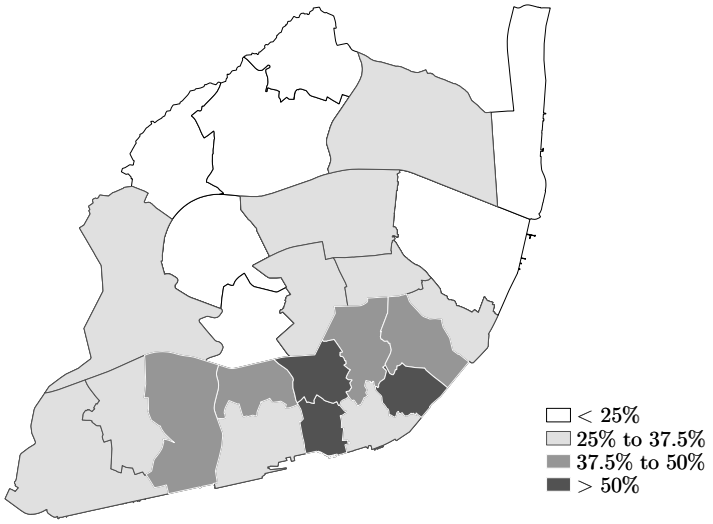
Figure 1: Median Sale Price (Dwellings) per Square Meter



Source: Statistics Portugal

The growth in real estate prices was very heterogeneous across different locations in the city of Lisbon, as shown in Figure 2. The highest growth rates were concentrated in the historical downtown areas. The figure depicts the city of Lisbon split into its 24 civil parishes, or *freguesias* in Portuguese. The civil parish is the lowest political unit in Portugal, with its own directly elected government. However, it runs a fairly small budget and has limited competences.

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 side of the political spectrum.[9] On the right, despite a common apprehension, candidates favored incentives for long-term rental rather than imposing supply restrictions.[10]

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.[11] In January 2017, some twenty grassroots organizations organized a petition calling for public policies to curb the real estate price increase in the capital, deemed excessive by the organizers.[12] Medina was re-elected in October 2017 with 42% of the votes but lost the absolute majority in the Municipal Assembly.

In August 2018, the Parliament legislated (Law 62/2018) granting 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).[13]. 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.

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

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

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

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

[13] 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).

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%.[14] 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.[15] This extension was approved in November 2019 (*DR n.º 214/2019, 1.º Suplemento*).

Figure 3 summarizes the timeline of these municipal regulations.

Figure 3: Analysis Timeline

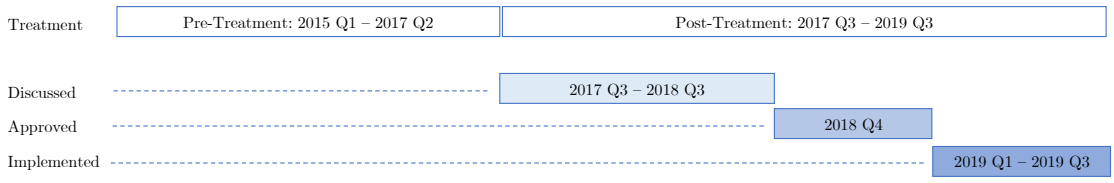
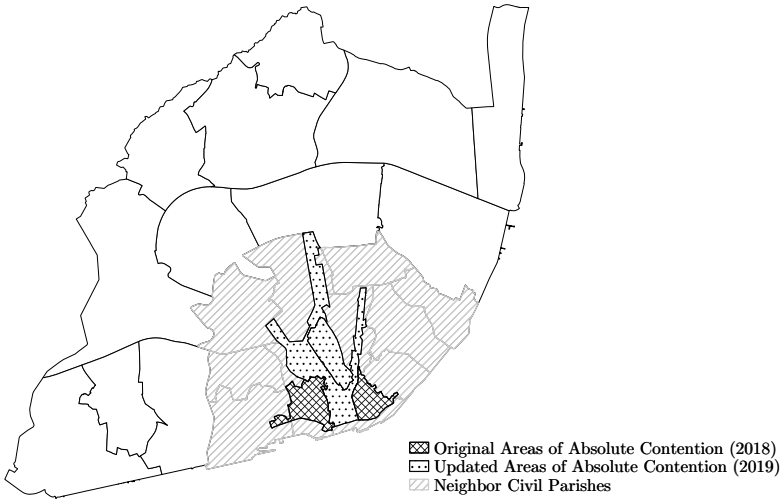


Figure 4 gives a geographical display of the different areas. The ones that were suspended in November 2018 are in black, while those that were suspended one year later are filled with a dotted pattern. Neighboring civil parishes, used in some of our specifications, are displayed in grey. In this 12 civil parishes there are 99 neighborhoods.

[14] The municipal regulation is *Proposta n.º 677/AML/2018.*
[15] *Proposta n.º 204/CM/2019*

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 e Misericórdia

4. Empirical Strategy

In this section, we begin by presenting the three complementary data sources used in the paper. We then discuss the methodology and compute descriptive statistics.

4.1. 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.

The first one consists of publicly available information from the National Short-Term Rental Registry (RNAL).[16] 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, from Banco de Portugal, to the European Central

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

Bank and 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*. The right of first refusal 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 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.

Finally, 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.

4.2. Methodology

We exploit the quasi-experimental nature of the legislated change to obtain estimates of the causal impact of the freeze on four outcome variables. The first is the number of short-term rental registries. Two variables pertain to the real estate market, namely, prices and quantities of dwellings exchanged in the market in each quarter. Finally, we look at short-term rental prices. Treated units correspond to the neighborhoods 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. 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, p.23) 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". 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).[17]

For the remaining outcome variables, we have to make a slight modification in the comparison

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

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, 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.

To fix ideas, Table 1 summarises the different analysis, including the comparison groups used in each sample.

Table 1: Comparison Groups

	Unit	Period	Treatment Period	Comparison Group
A.Registries	Street	2015 Q1–2018 Q4	Since the electoral campaign (2017 Q3)	2019 Freeze
B.Airbnb Price	Dwelling	2018 Q2–2019 Q3	Since Parliamentary discussion (2018 Q3)	Corrected 2019 Freeze + Neighbors
C.Housing Sales	Neighborhood	2017 Q1–2019 Q3	Since Parliamentary discussion (2018 Q3)	Corrected 2019 Freeze + Neighbors
D.Housing Prices	Neighborhood	2017 Q1–2019 Q3	Since Parliamentary 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.

We consider the following outcome variables: *i)* registries, aggregated to the street level, *ii)* Airbnb prices, observed at the dwelling level, *iii)* number of transactions, and *iv)* 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 the most aggregate level (Civil Parish), a conservative approach suggested by Cameron & Miller (2015).

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

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

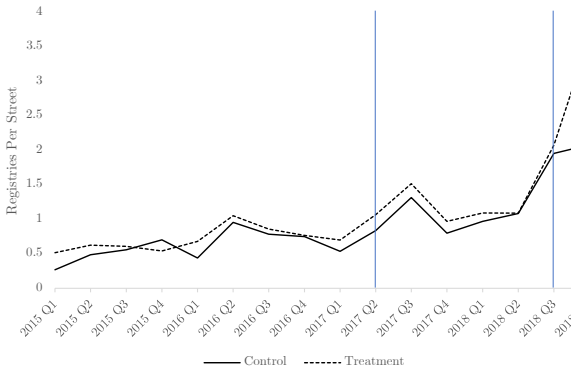
where the variables and coefficients are defined as in Equation (1), and δ_q are a full set of pre- and post-treatment effects. As detailed in Table 1, the treatment period is the third quarter of 2018 for the Airbnb and real estate market specifications, and the third quarter of 2017 for the registries.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

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.

4.3. Descriptive Statistics

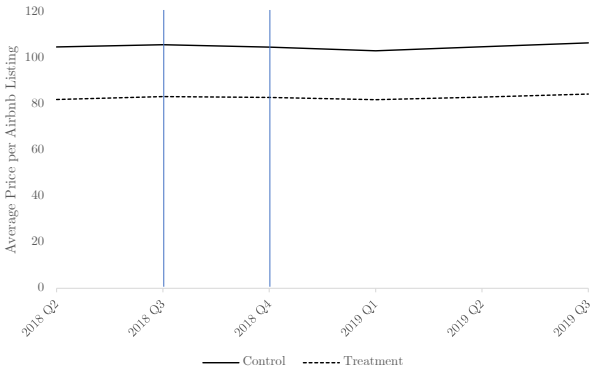
The trends for the four outcome variables are shown in Figure 5. The blue lines separate the pre-treatment and the different post-treatment periods. In panel (a) one can immediately see the spike in registries. Additionally, descriptive statistics for the three data sets are displayed in Table 2.

Figure 5: Trends for Outcome Variables



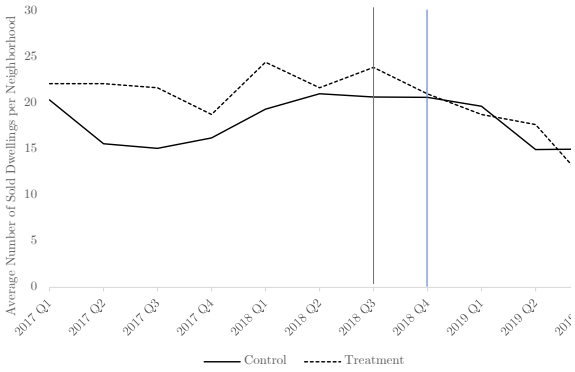
(a) Registries

N= 9440. Control: 2019 Freeze



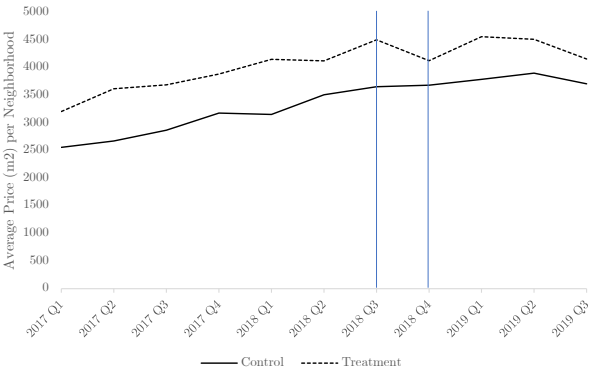
(b) Airbnb Prices

N= 39336. Control: Corrected 2019 Freeze + Neighbors



(c) House Sales

N= 660. Control: Corrected 2019 Freeze + Neighbors



(d) Housing Prices

N= 462. Control: Corrected 2019 Freeze + Neighbors

Table 2: Descriptive Statistics on Sample Characteristics

	Mean	Stand Dev	Max	Min
A.Registrations				
Number of Streets	590	-	-	-
Registries (per Street)	1.0	(2.3)	0	36
Rooms (per Registry)	2.0	(1.8)	73	0
% Singular	0.480	-	-	-
% National	0.953	-	-	-
B.Airbnb Listings				
Number of Listings	6556	-	-	-
Average Price (per Listing)	92.2	(105.8)	3000	9
Average Quarterly Reviews (per Listing)	2.1	(1.7)	0	12.64
% Multiple Listings	0.731	-	-	-
C.Housing Sales				
Number of Neighborhoods	60	-	-	-
Average Sales (per Neighborhood)	18.4	(15.6)	106	1
D.Housing Prices (m2)				
Number of Neighborhoods	42	-	-	-
Average Price (per Neighborhood)	3419.0	(1104.1)	8426	1414

In Table 3 we report on pre-treatment mean differences between treatment and comparison areas for the four outcome variables. Moreover, 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 neighbourhood parks are capitalized through residential prices in Lisbon (Franco & Macdonald 2018*b*,*a*). For that reason we show that amenities such as buildings of public interest, museums, and gardens/parks are not substantially different between treatment and comparison areas. Overall, we argue that, by comparing geographically close areas, we mitigate possible confounding factors, namely the tax regime (which is the same for all the Lisbon municipality) and the access to local amenities.

Table 3: Balance

	Pre-Treatment	
	Treatment	Control
A.Registrations		
Registries (per Street)	0.74	0.63
	(1.6)	(1.7)
Rooms (per Registry)	1.8	2.0
	(2.0)	(2.1)
% Singular	0.530	0.499
% National	0.961	0.964
% Alignment	0.519	0.484
	(0.022)	(0.027)
% Turnout	0.577	0.571
	(0.013)	(0.021)
B.Airbnb Listings		
Number of Listings	3257	3299
Average Price (per Listing)	82.6	100.5
	(69.1)	(127.5)
Average Quarterly Reviews (per Listing)	2.3	1.9
	(1.7)	(1.6)
% Multiple Listings	0.771	0.691
C.Housing Sales		
Number of Neighborhoods	9	51
Average Sales (per Neighborhood)	22.1	18.3
	(12.7)	(16.2)
D.Housing Prices (m2)		
Number of Neighborhoods	5	37
Average Price (per Neighborhood)	3881.8	3084.7
	(659.8)	(1035.8)
E.Amenities		
Buildings of Public Interest	56	71
Prémio Valmor	30	41
Gardens/Parks	15	13
Museums	28	13

Notes: The control area is defined in Table 1. % Alignment is the share of voters that voted on the Mayor's party. % Turnout is given by 1 minus the turnout rate. Both are computed at the civil parish level. In the data on *Tourist Amenities*, the values are normalized according to the geographic areas. *Prémio Valmor* is the most prestigious architectural honor in Portugal to be awarded to a building.

5. 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.

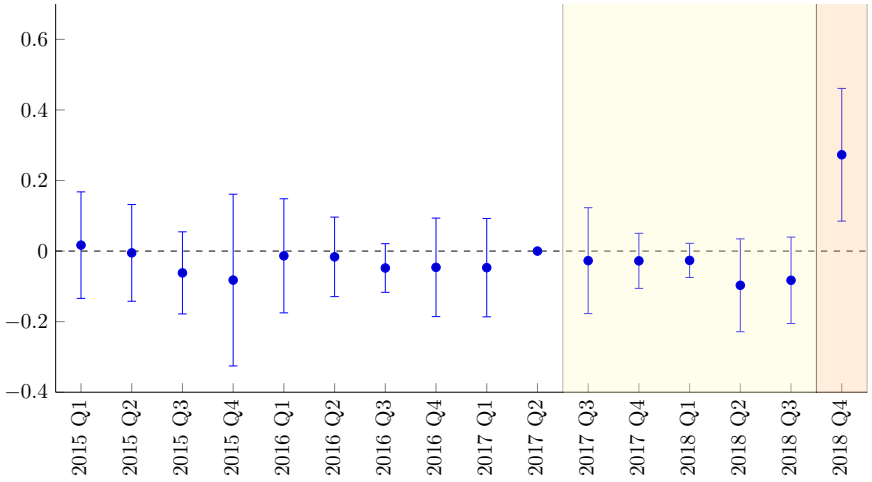
5.1. Registries

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 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.[18]

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.

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 5.1 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

[18] That is, each quarter begins one month earlier, and therefore the first quarter of each year actually begins in December of the previous year.

in Lisbon (Column 4) and touristic areas in Porto (Column 5) to the comparison area.[19]

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 widespread belief 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.

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 is non 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.

[19] 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)).

Table 5: Heterogeneous Effects - ln(Registries)

	Rooms (Median)		Ownership Status		Nationality	
	Below/=	Above	Singular	Colective	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 5.1 and Table 5, together 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 is strong evidence that the period of discussion prior to the implementation of the law was long enough that it suppressed the effectiveness of the ban, at least in the short run.

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

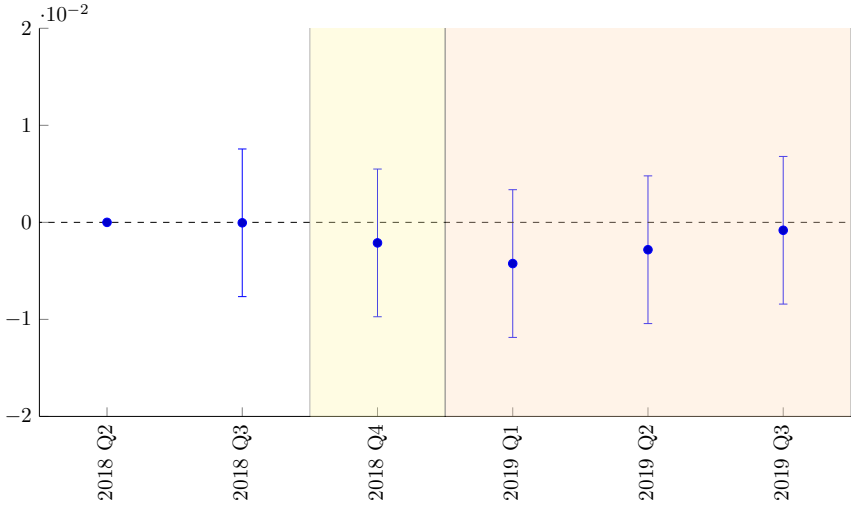
Notes: The comparison group is the remaining Freeze 2019 area. The vector of controls consists of Civil Parishes' political alignment with the Mayor's party and turnout rate. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * p < 0.10, ** p < 0.05, *** p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

5.2. Airbnb Prices

We now present the results about the rental prices of Airbnb listings. All prices are per night and dwelling. The event study is shown in Figure 7. The comparison group in this case 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. Furthermore, since our unit of observation is the dwelling, we estimate the equations with dwelling fixed effects. These also control for the surface of the property, therefore we run the regression on total rental price.

One important limitation of our analysis is that data is available as of the third quarter of 2018, therefore we only have on pre-treatment quarter. This does not allow us to explicitly test the parallel trend assumption. Our results indicate that, at least in the short term, the ban had no impact on Airbnb prices.

Figure 7: Event Study - ln(Airbnb Prices)



Notes: N= 39336. 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.

In addition, we estimate difference-in-differences coefficients. The results are presented in Table 7 and confirm that the ban of new registries did not change the prices of Airbnb listings in the short-run.

We also inspect (possible) heterogeneity effects by re-estimating (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. The results in Table 8 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.

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

	Corrected 2019 Freeze + Neighbors	
	(1)	(2)
$Treat \cdot Approval$	-0.002	-0.002
	(0.01)	(0.01)
$Treat \cdot Implement$	-0.003	-0.003
	(0.01)	(0.01)
Quarter FE	Yes	Yes
Dwelling FE	No	Yes
Political Controls	No	No
Number of Obs.	39336	39336
Adjusted R^2	0.013	0.037

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 8: Heterogeneous Effects - ln(Airbnb Prices)

	Number of Listings		Number of Reviews (Median)	
	Single	Multiple	Above	Below/=
$Treat \cdot Approval$	-0.002	-0.001	0.000	-0.002
	(0.00)	(0.01)	(0.01)	(0.00)
$Treat \cdot Implement$	-0.008**	-0.001	-0.000	-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.	10572	28764	19668	19668
Adjusted R^2	0.038	0.042	0.034	0.037

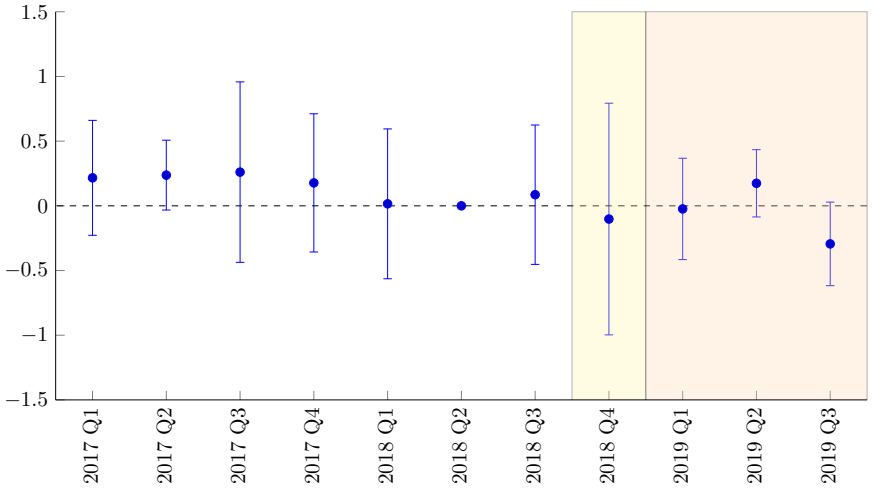
Notes: The comparison group is the Corrected 2019 Freeze + Neighbors. Standard errors (in parentheses) are clustered at the Civil Parish level. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

5.3. 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 neighbouring civil parishes, as explained Section 4.2. We removed all neighborhoods for which future bans on short-term rentals were expected. This leave us with 9 treated neighborhoods that we compare with 54 the comparison group.[20]

We first analyze the effects on property transactions. The event study shown in Figure 8 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 not statistically significantly different from zero.

Figure 8: 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. If we restrict our sample to neighborhoods that witnessed more than 3 house transactions in all quarters (5 treated vs. 37 comparison neighborhoods), the impact is even more negative as portrayed in Column (4).

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

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. The effect of the ban on the market, if it exists, comes from the option value of the short term rental licensing, which is eliminated with the ban. 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 confirm the evidence that the effect is driven by housing units of smaller dimensions, confirming the segmentation of the market along this dimension suggested by the results in Table 5.

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.

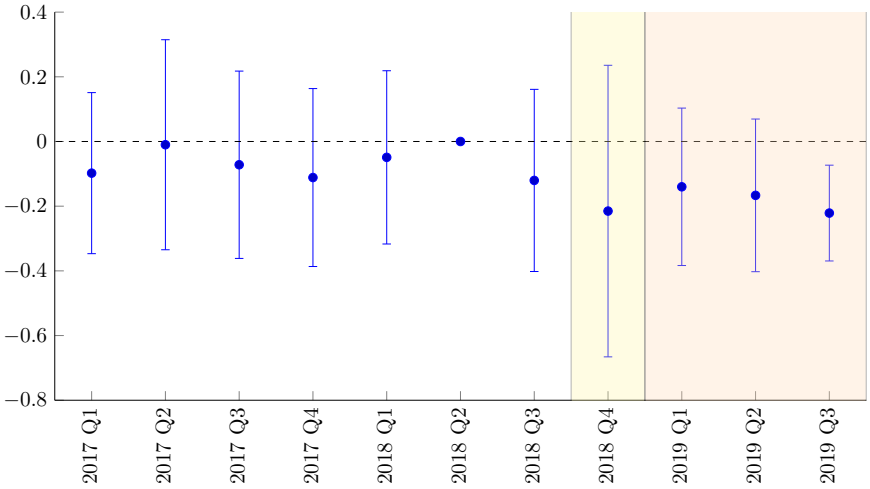
5.4. Housing Prices

Focusing on the effects on house prices, we again consider a balanced sample of neighborhoods. However, prices are omitted in our data for neighborhoods with less than 4 transactions. Therefore, we rely on the comparison between 5 treated with 37 comparison neighborhoods.[21] For comparison purposes, this is the sample used in Column (4) of Table 9.

The event study design shown in Figure 9 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.

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

Figure 9: 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 those for the number of transactions, that the option of registering the dwelling as short-term rental was valued by potential buyers.

To infer (possible) heterogeneity in these effects, we re-estimate the difference-in-differences model by the number of rooms in each dwelling. The results, displayed in Table 13, again confirm that the more "Airbnb-marketable" properties are the smallest ones. In this case, dwellings with 2 rooms experienced a price decrease of 20%.

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). 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***	-0.96***
	(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

6. Conclusion

Short-term rental platforms such as Airbnb have grown spectacularly in recent years. Lisbon seems to embody an almost ideal laboratory to examine the effects of short-term rental services on housing prices. The city experienced a tourism boom soaring the local economy, but also put added strains on municipal services and affordable housing for local residents. We exploit the quasi-experimental evidence from the introduction of a regulation banning the registry of new short term rental properties in some areas of the city. Our empirical strategy provides causal evidence about the short-term rental market regulation. To our knowledge, there is only another paper that analyses the causal impact of short term rental regulations (Koster et al. 2018). We rely on two administrative data sets on short term rentals registries and real estate prices, complemented with data on Airbnb listing prices. We analyse the impact of the ban on the number of registries, short-term listing prices, and the real estate market, i.e., number and price of transactions.

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 running to the effective prohibition. Our second result is that there were no effects on short-term rents in the immediate period following the ban. If anything, we identify a slight decrease for listings from single-listing owners. This may be explained by the surge in registrations, which may have caused a temporary excess supply.

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. The causal impact of the short-term registry ban in the number of sales was a contraction of 20% in treated areas *vis-a-vis* the comparison ones. Prices decreased by 8%, that the event studies also suggest to lag the ban by two quarters. 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, we show that the price and quantity effects in the real estate market are mostly driven by two-bedroom properties, confirming the widespread belief that the short-term rental market is dominated by relatively small houses. The price decrease for two-bedroom houses is almost 20%.

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. Secondly, the surge of short-term rental markets does create an upward pressure on real estate prices. However, this effect is concentrated in some types of properties. Therefore, 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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