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ABSTRACT

Short-Term Rental Bans and Housing Prices: Quasi-Experimental Evidence from Lisbon*

We estimate the causal impact of a 2018 zoning reform that banned new short-term rental registries in some parts of Lisbon. The short-term rental licence expires when the house is sold, hence the ban removes the option value of short-term renting a property. We rely on two administrative data sets on short-term rental registries and real estate transactions, complemented with Airbnb data on listings and prices. We employ a difference-in-differences estimation taking advantage of the spatial discontinuity in the ban. We document a spike in newly registered housing units, between the announcement and the implementation of the ban. The reform decreases real estate prices by 8%, mostly in two-bedroom dwellings, for which the price drops 20%. We conclude that heterogeneous effects are key to understanding the backlash against short-term rentals.

JEL Classification: R12, R21, R30

Keywords: Airbnb, policy analysis, housing market, short-term rental, Portugal

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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 was 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 analyse the impact of the tourism and short-term rental boom on the real estate market. The short-term rental regulation in Portugal requires the house owners to register the property; this procedure is free of charge, the licence can be kept for free, and it only expires with the property sale. Therefore, owning a licence amounts to an option to earn income in the short-term rental

[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`.

market, which can capitalise into property prices.

Given the spectacular increase in the housing prices in Lisbon, the Portuguese parliament passed a law allowing municipalities to implement zoning regulations affecting the supply of short-term rentals in August 2018. The municipality of Lisbon then implemented a ban of new licences for short-term rental housing units, in November 2018.

We set up a difference-in-differences specification to estimate the causal impact of this ban on *(a)* the number of registered housing units, *(b)* the number of sold dwellings, *c)* house prices, *(d)* Airbnb listings, and *(e)* Airbnb prices. We do so by comparing the treated neighbourhoods, affected by the 2018 ban, with those in their immediate vicinity. Importantly, some of these latter were subject to a ban one year later, in November 2019, which shows that they are quite similar to the originally treated ones, lending credibility to our identification strategy.

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.* 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. Real estate prices in the treated neighbourhoods decreased by 8%, showing that the option to rent the dwelling in the short-term rental market is an important demand determinant. Heterogeneity results show that the price drop is driven by two-bedroom apartments, whose price decreases 20% in treated neighbourhoods, vis-á-vis control ones. The effect is stronger in the high-end properties in the last quartile of the price distribution. The heterogeneous effect may help explain the global political backlash against short-term rentals, which are accused of being a major driver of housing unaffordability, an impact that average effects usually estimated in the literature do not capture. The point estimates suggest a decrease in the number of sold houses, albeit non-significant.

We also show that there were no spillover effects, i.e., neither the number of short-term rental licences, nor the real estate market are affected by the policy in the control neighbourhoods. Finally, we 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 work thus suggests important policy implications. The first one is not surprising: long periods of anticipating discussions about zoning regulations may 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 analyse 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 licence, 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

[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 (2022) and Basuroy et al. (2020) for restaurants, and Zervas et al. (2017), Schaefer & Tran (2020) for the lodging industry. For the impact of the covid-19 pandemic on rental and real estate markets in Lisbon see Batalha et al. (2022).

listings increase average monthly rents of nearby apartments.

Other papers with causal strategies that do not rely on policy reforms include Barron et al. (2021), who use data on Airbnb's listings between 2011 and 2016 in the US, and find 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 centre. Ángel Garcia-López, Jofre-Monseny, Martínez-Mazza & Segú (2020) use an instrumental variable fixed-effects model based on neighbourhood 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 neighbourhoods.[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]

[5] See also Horn & Merante (2017), who analyse 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

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 data sources and handling, the empirical methodology employed in the paper, and discusses possible threats to identification. The results and robustness analyses 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 licence 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. The licence amounts to an option to rent the dwelling on short-term rental platforms, which may capitalise into the real estate price. 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

the geographical imbalances of Airbnb's revenue flows, which enhances within-cities income inequality and increases tenant displacement.

[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.

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 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 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 A1 in the Appendix), and within the city of Lisbon, where the highest growth rates were concentrated in the historical downtown areas (cfr. Figure A2 in the Appendix). The figure depicts the city of Lisbon split into its 24 civil parishes, or *freguesias* in Portuguese.[10]

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 neighbourhoods, 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 orga-

[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.

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

[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/`.

nizers.[14] Medina was re-elected in October 2017, with 42% of the votes.

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 neighbourhood, 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 control group implemented one year later. In fact, there is no noticeable increase in the number of registered housing units for short-term rentals. Figure A3 in the Appendix summarizes the timeline of these municipal regulations.

3 Empirical Strategy

In this section, we begin by presenting the four complementary data sources used in the paper. We then discuss data harmonization issues, the definition of the treatment and

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

[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*

control groups, and the treatment period. Next, we present the econometric specifications, 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, one on short-term rentals registered housing units, and the other on 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 and from Direção Geral da Administração Interna, to retrieve controls used in some robustness exercises.

The first one consists of publicly available information from the National Short-Term Rental Registry (RNAL).[18] We collect all daily new registered housing units, between January 2015 and September 2019. Our data includes the universe of legal short-term rentals registered in this period, for which we observe the registry date, address, number of rooms, 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. The data provider is *Confidencial Imobiliário*, an independent Portuguese databank specialised 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, to produce official statistics on the Portuguese housing market. We obtained the data through a paid subscription, involving a signed agreement between our institution and the databank. Therefore, we are using the most granular data on *actual* transaction prices that is available for researchers.

More specifically, we use data from *Confidencial Imobiliário*'s SIR.RU (Urban Rehabil-

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

itation) platform, which compiles information that the databank has access to thanks to a protocol with the Municipality of Lisbon. The historical centre of Lisbon contains the so-called *urban rehabilitation areas* in which, by law, the municipality has a *right of first refusal*, i.e., 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 centre areas. Therefore, the municipality has individual records of every such transaction. To preserve anonimity, the data is aggregated to the neighbourhood level. For the same reason, price data for a given type of dwelling is missing whenever there are less than four transactions, for that type of dwelling, in a given quarter and a given neighbourhood. Since the partition of the city into neighbourhoods in the data that we use comes from the information system of the municipality, it matches the areas in the law that implemented the short-term rental freeze. Therefore, we have a one-to-one correspondence between treatment and control areas and the price series.

The third dataset that we analyse is obtained from Inside Airbnb, that contains monthly information about the price of active Airbnb listings in Lisbon (among other listing-level details, such as the number of reviews, and the number of different listings owned by the same owner 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.

Finally, we rely on publicly available information from *Lisboa Aberta*, a data bank of the municipality of Lisbon.[19] In particular, we retrieve information about the number of trees, which have been shown to have an effect on real estate prices in Lisbon by Franco & Macdonald (2018). We also use political data per civil parish from the government agency responsible for electoral results (*Direção Geral da Administração Interna*).

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

3.2 Data harmonization and definition of treated and control areas

We exploit the quasi-experimental nature of the legislated change to obtain estimates of the causal impact of the freeze on five outcome variables. The treatment, at the *neighbourhood* level, consists in a ban on new short-term rental registrations, implemented by the municipality decision 677/AML/2018, in the following nine neighbourhoods: *Madragoa, Bairro Alto, Bica, Príncipe Real, Santa Catarina, São Paulo/ Boavista/ Conde Barão, Alfama, Mouraria, and Sé*. In order to match the unit of observation to the unit of treatment, we aggregate all the outcome variables to the neighbourhood level (except the Airbnb listings price, due to data limitations).

Neighbourhoods, or *bairros* in Portuguese, were defined by the Lisbon Municipality during this reform to contain population with comparable socio-economic characteristics. Indeed, according to the report CML (2018), published by the Town Hall, as a basis for the quasi-experimental reform studied in our paper, neighbourhoods were delimited using historical and morphological data, retrieved from zoning plans, urban guides, and historical references, seeking a "consensual" definition of these geographical units. The definition of neighbourhood thus encompasses a special identity that differentiates them and corresponds to what tourists are looking for when deciding where to stay in the city. This level of aggregation is very similar to what is used in Ángel Garcia-López, Jofre-Monseny, Martínez-Mazza & Segú (2020).

We consider the following outcome variables: *(a)* number of registered housing units in the short-term rental national registration platform; *(b)* number of transactions, and *(c)* house prices from the SIR.RU platform of *Confidencial Imbobiliário*; *(d)* Airbnb listings, aggregated at the neighbourhood level; and *(e)* Airbnb prices, observed at the dwelling level, both from Inside Airbnb.[20]

The control group is built as follows. Firstly, we take the *updated areas of absolute contention*, i.e., those where the ban was implemented one year later; secondly, we exclude those which were already being discussed in 2018 (*Colinas de Santana* and *Graça*, to avoid

[20] We do not compute neighbourhood averages of Airbnb listings because we only have total prices, not per square meter.

anticipation effects. This leaves us with *Baixa, Liberdade, Almirante Reis*.[21] Finally, we augment the control group with a set of neighboroods in the immediate vicinity of those affected by the November 2018 and November 2019 freezes.

Figure 1 gives us a geographical display of the different areas: the treated ones, i.e., the ones that were suspended in November 2018 are in dark, while the *updated areas of absolute contention* that were unexpectedly suspended one year later are filled with a dotted pattern. The ones for which anticipation effects may have been felt are presented in light grey. Finally, the never treated neighboring civil parishes are displayed in a dashed pattern. Due to anonymity reasons that are detailed below, we loose some observations when we focus on real estate prices – panel (b).

[21] These three areas were explicitly excluded from the April 2019 discussion, given the importance of the service sector in these locations.

Figure 1: Lisbon Municipality

(a) neighbourhoods considered for Number of registered housing units and Number of Sold Houses

(b) neighbourhoods considered for Housing Prices

Note that using the areas which were frozen in 2019 is a natural choice, given their similarity to the originally treated ones; indeed, the short-term to total property ratio in the neighbourhoods covered by the 2018 freeze varies between 27% and 29%, while in those affected by the 2019 freeze it ranges from 18% to 25%.[22] As Neumark & Simpson (2015) discuss, in the context of the analysis of place-based policies, 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). Importantly, we run our analysis only until the end of the second quarter for Airbnb data and until the end of the third quarter of 2019, i.e., before the ban is extended to this second area.

In a nutshell, Table 1 summarises the different analyses conducted in the paper.

Table 1: Outcome variables

Outcome Variable	Unit	Period	Onset of Treatment Period
Number of registered housing units	Neighbourhood	2015 Q1–2018 Q4	Electoral campaign (2017 Q3)
Housing Sales	Neighbourhood	2017 Q1–2019 Q3	Parliament discussion (2018 Q3)
Housing Prices	Neighbourhood	2017 Q1–2019 Q3	Parliament discussion (2018 Q3)
Airbnb Quantities	Neighbourhood	2018 Q2–2019 Q2	Parliament discussion (2018 Q3)
Airbnb Price	Dwelling	2018 Q2–2019 Q2	Parliament discussion (2018 Q3)

3.3 Econometric specifications

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

$$\ln(y_{ipq}) = \alpha_p + \lambda_q + \beta_0 Treated_i + \beta_1 Treated_i \times Discussion_q + \beta_2 Treated_i \times Approval_q + \epsilon_{ipq} \quad (1)$$

where y_{ipq} is the number of registered units (in logs), in neighbourhood i, civil parish p,

[22] The unaffected zone with the highest short-term rental intensity had a ratio of 10%, i.e., about half the density of least short-term rental dense area of the 2019 freeze.

and quarter q, and

$$\ln(y_{ipq}) = \alpha_p + \lambda_q + \gamma_0 Treated_i + \gamma_1 Treated_i \times Approval_q + \gamma_2 Treated_i \times Implement_q + \epsilon_{ipq} \quad (2)$$

where y_{ipq} is the outcome variable (in logs) for the unit of observation i (i.e., neighbourhood for the price and quantity of sold dwellings, and for the quantity of Airbnb listings, and listing for the Airbnb price), in civil parish p in quarter q, α_p is a civil parish fixed effect, λ_q is a quarter fixed effect, and ϵ_{ipq} is an error term. 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 these 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. To account for (possible) serial correlation within the panel units, standard errors were clustered (Bertrand et al. 2004) at the geographical level of the treatment, i.e., the neighbourhood.[23]

Recent developments in the difference-in-differences literature highlight challenges in designs that exploit staggered treatments (Goodman-Bacon 2021, De Chaisemartin & d'Haultfoeuille 2020, Callaway & Sant'Anna 2021, Sun & Abraham 2021, Borusyak et al. 2021). 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 comparing early vs. late treated (sometimes augmented with other never treated areas for robustness) before the late treated group is actually treated.

In the robustness section, we conduct the following exercises: we add time-variant political covariates, and a time trend interacted with a time invariant characteristic, i.e., the number of trees per neighbourhood, to the baseline specifications (1) and (2).

[23] In Section 4.5, we show that our results are robust to clustering at the civil parish level. We also run the number of registered housing units regression disaggregated at the street level), again with robust results.

We also explore heterogeneity results across housing sizes, by estimating equations (1) and (2) 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 2.

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

$$\ln(y_{ipq}) = \alpha_p + \lambda_q + \delta_0 Treated_i + \sum_{q \in pre-treat} \delta_q \times Treated_i + \sum_{q \in post-treat} \delta_q \times Treated_i + \epsilon_{ipq} \quad (3)$$

where the variables and coefficients are defined as above, and δ_q are a full set of (quarter level) 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 number of registered housing units. The omitted quarter is the one immediately before the Treatment Period as defined in Table 1.

These event studies allow us to formally test if, prior to the discussion on restrictive regulation, the concentration of short-term rental registered housing units and the house prices and quantities sold in the real estate market displayed parallel trends.

3.4 Descriptive statistics

We report descriptive statistics in Table 2, comparing the average value of the outcome variables in the treatment and control group of neighbourhoods.

Table 2: Descriptive Statistics on Sample Characteristics

	Treatment		Control	
	Mean	Stand Dev	Mean	Stand Dev
(a) Registrations				
Number of neighbourhoods	9	-	47	-
Number of Registered Housing Units	39.31	(42.02)	6.38	(10.81)
Number of Rooms	1.94	(0.58)	2.53	(1.50)
% of Listings Individually Owned	51.79	(0.16)	60.11	(0.33)
(b) Housing Sales				
Number of neighbourhoods	9	-	47	-
Number of Sold Houses	20.29	(12.01)	18.01	(16.48)
Number of Sold Houses: 1 Bedroom	9.51	(7.71)	6.14	(6.29)
Number of Sold Houses: 2 Bedroom	5.87	(3.59)	6.42	(5.50)
Number of Sold Houses: 3 Bedroom	3.44	(2.22)	4.64	(4.35)
(c) Housing Prices (€/m2)				
Number of neighbourhoods	5	-	34	-
Average Price	4048.06	(677.31)	3322.04	(1150.29)
Average Price: 1 Bedroom	4258.36	(874.72)	3517.82	(1147.73)
Average Price: 2 Bedroom	3903.23	(952.37)	3383.98	(1308.77)
Average Price: 3 Bedroom	3932.50	(661.00)	3194.00	(1082.91)
(d) Airbnb Listings and (e) Airbnb Prices				
Number of neighbourhoods	9	-	42	-
Number of Listings	458.36	(296.09)	87.10	(112.33)
Average Price (€ per Dwelling)	82.65	(74.53)	103.70	(138.96)

As one would expect, the treated neighbourhoods have more registered housing units, more listings, a more liquid market, and also higher prices. Moreover, we observe more transactions of one- and two-bedroom flats than of bigger units, in both treated and control areas. These differences in the liquidity of the market are one of the reasons why we conduct an heterogeneity analysis along dwelling size.

The table also displays the number of treated and control neighbourhoods; one should note that the sample size for the price data is smaller, due to the anonimity reasons already discussed.

It is noteworthy that the average number of rooms of the dwellings registered for short-term rentals is 1.94 in the treated regions, and 2.53 in the control ones. Those in control areas are slightly bigger, but these two figures indicate the importance of two-bedroom properties in this market.

3.5 Threats to identification

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

First, note that our treatment period does not begin when the policy is implemented, but when it starts being discussed in the public domain, which eliminates concerns that anticipation effects plague our results. The trends for the five outcome variables, shown in Figure A4, 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 registered housing units in control areas, showing that there were no anticipation effects of the ban introduced one year later.

Moreover, the event studies defined in Equation (3) and estimated in Section 4 confirm the absence of pre-treatment differences in the treated and control areas, which lends support to our definition of the treatment period and rules out anticipatory effects.

A second concern is the possibility of spillovers between treatment and control areas.[24] Note that the evidence in Figure A4 does not suggest contagion across the two areas for any outcome. More robust evidence will be discussed Section 4.5, where we conduct a formal analysis of the possibility of contamination, comparing neighbourhoods within the control group. In any case, if there would be general equilibrium effects leading to an increase in the number of registered housing units in the control group (which we do not find), or to an appreciation in the real estate market in the neighbouring areas, this implies that our

[24] Several papers analysing the effects place-based policies find only limited evidence for housing market spillovers (see, *inter alia*, Autor et al. (2014), Koster & Van Ommeren (2019), Diamond & McQuade (2019), Koster et al. (2021)).

estimates should be interpreted as a lower bound of the true impact.

Finally, one could question the comparability of the treated and control areas. Note that they are geographically very close within the city centre, i.e., they do not vary importantly with regards to the urban amenities, which *per se* mitigates possible confounding factors. Moreover, differences in observables, when they exist, do not threat identification with a difference-in-differences strategy, unless they are correlated with *changes* in outcome variables. Regardless, we test for alternative definitions of the control group in Section 4.5.

4 Results

In this section, we present the results of the estimation of equations (1) and (2), to obtain difference-in-differences estimates for the number of registered housing units and the remaining outcome variables, respectively, and (3), for the event studies of each of the outcome variables. In addition, we exploit heterogeneous effects.

4.1 Number of registered housing units

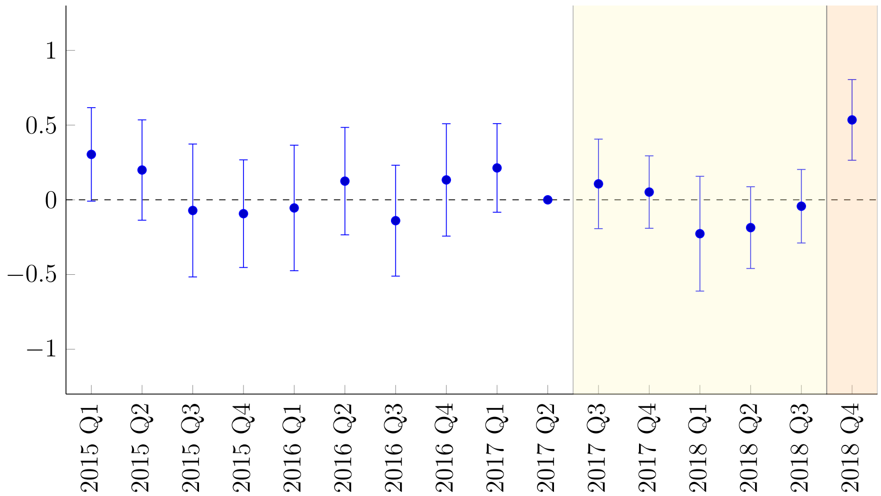
The law that allowed municipalities to regulate new registered housing units in the short-term rental market was passed in late August 2018, but only became effective two months later. This lag could, in theory, allow the owners that are considering moving into the short-term rental market to register their properties, thus curbing the intended effects of the law. In order to account for this legislative change, we aggregate registered housing units at the quarterly level, with the last quarter defined as September to November, and the remaining ones changed accordingly.[25]

The event study in Figure 2 shows that the parallel trend assumption holds in the pre-treatment period. The figure documents a sizeable spike in the number of registered housing units in the treated areas, suggesting a rush to register before the law became binding,

[25] 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.

possibly undermining its goals.

Figure 2: Event Study - ln(Registries)



Notes: N=896. It includes civil parish and quarter fixed effects as in equation (3). 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 Neighbourhood level.

The estimates of Equation (1) in Table 3 imply a similar interpretation. The baseline specification shows that neighbourhoods on the originally treated areas experienced a short-term increase of 46.4% in the quarterly number of registries as a result the law's approval. The marginally significant effect during the period of discussion is hard to interpret, and actually disappears at the more disaggregated street level, shown in Section 4.5. The effect is similar across dwelling size, and stronger for those owned by (in principle, more business oriented) firms.

Table 3: Difference-in-Differences - ln(Number of registered housing units)

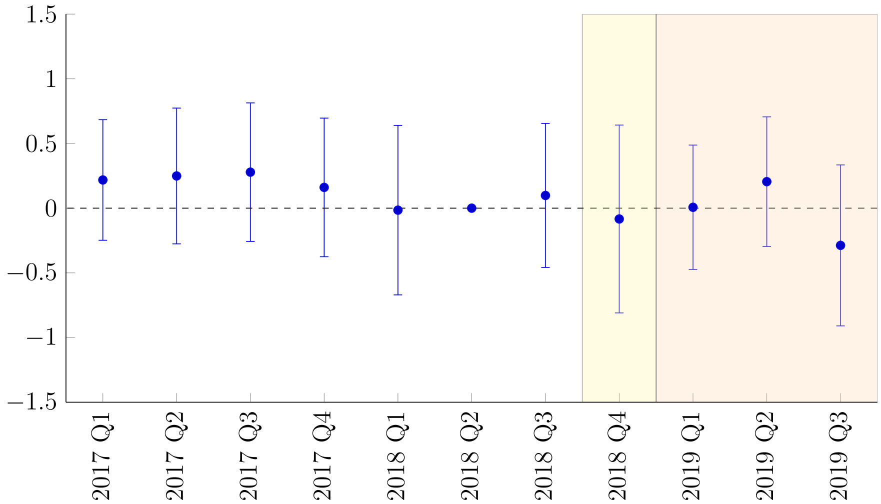
	Baseline	Heterogeneity				
		Number of Rooms			Ownership Status	
		1 Room	2 Rooms	3+ Rooms	Individual	Firm
$Treat \cdot Discussion$	-0.130*	0.149*	0.115	0.024	-0.008	0.092
	(0.07)	(0.08)	(0.09)	(0.10)	(0.05)	(0.11)
$Treat \cdot Approval$	0.464***	0.769***	0.826***	0.716***	0.593***	0.825***
	(0.12)	(0.21)	(0.14)	(0.19)	(0.13)	(0.15)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs.	896	896	896	896	896	896
Adjusted R^2	0.658	0.624	0.610	0.541	0.611	0.626

Notes: Standard errors (in parentheses) are clustered at the Street level. The Discussion period spans between 2017 Q3 – 2018 Q3, while the Approval period is 2018 Q4. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Number of Sold Houses

For the real estate market, we first analyse the effects on property transactions. The dynamic event study in Figure 3 shows convincing evidence of pre-treatment parallel trends.

Figure 3: Event Study - ln(Number of Sold Houses)



Notes: N= 616. It includes civil parish and quarter fixed effects as in equation (3). 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 Neighbourhood level.

The evidence from difference-in-differences regressions, presented in Table 4, suggests that the suspension decreased the number of transactions, although the effect is not always significant. The point estimate of the treatment effect has a higher magnitude for two-bedroom houses (albeit heterogeneous effects fail to be significant at coventional levels).

Column (2) re-estimates equation (2), but restricting the analysis to the neighbourhoods for which we observe prices, i.e., those with more than three house transactions in all quarters (five treated vs. 37 control neighbourhoods). As expected, the effect in this sub-sample is more negative and more precisely estimated, because we are eliminating the areas where the real estate market is less liquid.

Table 4: Difference-in-Differences - ln(Number of Sold Houses)

	Baseline		Heterogeneity		
			Number of Rooms		
	(1)	(2)	1 Room	2 Rooms	3 Rooms
$Treat \cdot Approval$	-0.227	-0.159*	0.169	-0.115	0.063
	(0.27)	(0.09)	(0.16)	(0.27)	(0.14)
$Treat \cdot Implement$	-0.168	-0.458***	-0.208	-0.289	-0.122
	(0.22)	(0.12)	(0.24)	(0.21)	(0.15)
Quarter FE	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes	Yes	Yes
Number of Obs.	616	429	540	544	517
Adjusted R^2	0.136	0.138	0.133	0.103	0.043

Notes: Standard errors (in parentheses) are clustered at the Neighbourhood level. The Approval period is 2018 Q4, while the Implement period spans between 2019 Q1–Q3. Column (2) restricts the sample to the neighbourhoods for which price information is available. We do not consider dwellings with more than 3 rooms in the heterogeneity analysis, given their low frequency in the data. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The anticipation effect that led to a surge in the number of registered housing units 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 3 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 4 lends some support to 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.

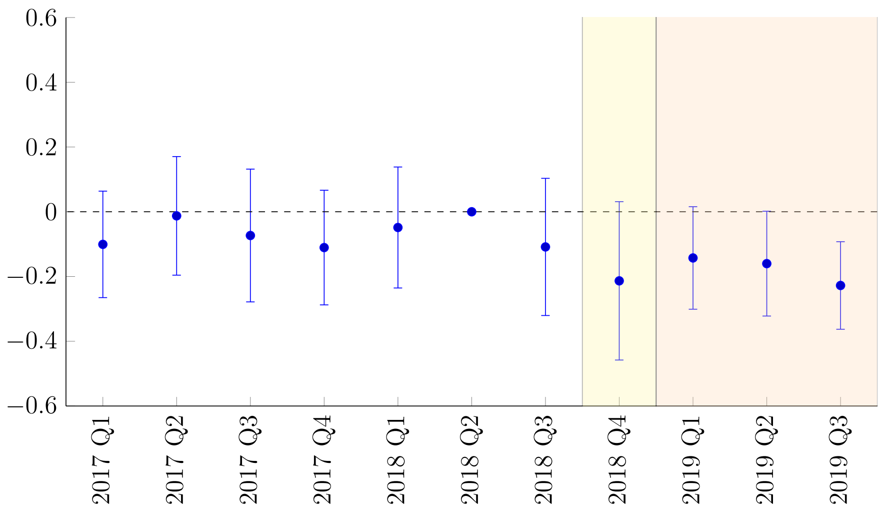
4.3 Housing Prices

We now focus on the price effects of the ban. For anonymity reasons, as explained in Section 3.2, prices are omitted in the data for neighbourhoods with less than four transactions.

These neighbourhoods are dropped from the analysis. Therefore, we rely on the control between five treated and 37 control neighbourhoods.[26] For comparison purposes, this is the sample used in column (2) of Table 4.

The event study design shown in Figure 4 highlights that prices follow parallel trends before the treatment. Moreover, prices are impacted negatively by the ban, but this effect comes with a lag, as the coefficient is statistically significant only in the third quarter of 2019.

Figure 4: Event Study - ln(Housing Prices)



Notes: N=429. It includes civil parish and quarter fixed effects as in equation (3). 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 Neighbourhood level.

We also compute difference-in-difference regressions for housing prices. The estimated coefficients from equation (2) are displayed in Table 5, and suggest that the suspension of short-term rental registered housing units induced an 8.7% decrease in prices, after the implementation of the ban. These findings confirm that the option of registering the dwelling for a short-term rental is valued by potential buyers and capitalises into real estate prices. We also observe that the price effect is twice as strong, in relative terms, for two-bedroom dwellings, in line with the stronger quantity effect obtained for quantities which was, however,

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

non significant. The results also suggest that the bulk of the effect is driven by high-end properties, in the top quartile of the price distribution.

Table 5: Difference-in-Differences - ln(Housing Prices)

	Baseline	Heterogeneity				
		Number of Rooms			Percentiles	
		1 Room	2 Rooms	3 Rooms	25%	75%
$Treat \cdot Approval$	-0.124	-0.053	-0.212	0.044	-0.044	-0.139*
	(0.08)	(0.09)	(0.14)	(0.06)	(0.12)	(0.08)
$Treat \cdot Implement$	-0.087***	-0.038	-0.196**	-0.024	-0.061	-0.104***
	(0.02)	(0.06)	(0.10)	(0.08)	(0.06)	(0.03)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs.	429	279	296	217	295	295
Adjusted R^2	0.760	0.600	0.636	0.646	0.653	0.730

Notes: Standard errors (in parentheses) are clustered at the Neighbourhood level. The Approval period is 2018 Q4, while the Implement period spans between 2019 Q1–Q3. We do not consider dwellings with more than 3 rooms in the heterogeneity analysis, given their low frequency in the data. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.4 Airbnb: Listings and Prices

We now focus on the impact of the ban on quantities of listings offered on the Airbnb platform, and their respective prices.[27] 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 6 and show that, despite the increase in the number of registered housing units in the last quarter of 2018, documented in Table 3, Airbnb listings basically did not change, at least in the short-run. Concomitantly, we find no evidence of treatment effects on Airbnb rental prices. We note that all prices are per night and dwelling. The last column includes dwelling fixed effects, to control for time-invariant characteristics of the property, including its surface, confirm that the absence of effects is not confounded by the fact that we are using the total rental price as the outcome variable. All in all, these estimates imply that it is unlikely that the registry ban limited the tourist inflow, given that the Airbnb prices and quantities did not change.

[27] For the Airbnb price data, there is no way to present it at a geographically aggregated scale, given that the website Inside Airbnb does not provide prices per square meter.

Table 6: Difference-in-Differences - ln(Airbnb:Listings and Prices)

	ln(Listings)	ln(Prices)	
	(1)	(2)	(3)
$Treat \cdot Approval$	0.020	-0.000	-0.000
	(0.02)	(0.00)	(0.00)
$Treat \cdot Implement$	-0.013	0.000	0.000
	(0.02)	(0.01)	(0.00)
Quarter FE	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	No
Dwelling FE	No	No	Yes
Number of Obs.	255	27460	27460
Adjusted R^2	0.671	0.062	0.007

Notes: Standard errors (in parentheses) are clustered at the Neighbourhood level. The Approval period is 2018 Q4, while the Implement period spans between 2019 Q1–Q3. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

4.5 Robustness

We now present a battery of robustness analyses. We begin by changing the baseline specification in several directions (adding political covariates, using alternative definitions of the control group, including a time trend interacted with a time invariant characteristic, and changing the clustering level of the standard errors). We then provide a formal test of the absence of contamination effects of the reform for the control areas. Finally, we show the number of registered housing units results disaggregated at the street level.

The first set of robustness tests is presented in Table A1, Table A2, and Table A3, in the Appendix, for the number of registered housing units, and transaction quantities, and prices, respectively. The first column reproduces the baseline specification, for comparison. Column (2) includes political controls (i.e., alignment of the civil parish government with the mayor party, and turnout rate at the civil parish level), while (3) and (4) change the control group, for the never treated neighbourhoods (i.e., excluding those treated in 2019), and then including the areas of *Graça* and *Colinas de Santana*, i.e., the ones that were discussed in 2018 but only included in 2019.

In column (5), we add a time trend interacted with the number of trees. This is inspired by the work of Franco & Macdonald (2018), who study the capitalization effects of urban greenness on the residential real estate market in Lisbon. They show that a square kilometer increase in the relative size of tree canopy has a positive impact of 0.20% in real estate prices, or approximately 400 per dwelling. Finally, column (6) changes the clustering of the standard errors from the neighbourhood to the civil parish level.

The signs and magnitudes of the point estimates do not change in most of the specifications, reinforcing the robustness of our results. The exceptions are a higher magnitude of the treatment effect on the number of registered housing units when the political controls are included, and the negative treatment effect on the number of transactions, which becomes significant at the 10% level when standard errors are clustered by civil parish.

Next, we turn to the potential contamination of the approved zoning regulations to the control group. We test this by implementing specifications (1) for the number of registered housing units and (2) for the number of houses and prices, while replacing the treated group of neighbourhoods by Vicinity, i.e., those in the immediate vicinity of the original treatment group. Therefore, the control group comprises all the remaining ones. The results, provided in Table Table A4 in the Appendix, show that there were no spillover effects in any of the outcome variables. The lack of contamination is further evidence of the response to the anticipated public discussion of the ban. If the ban were binding, an increase in the number of registries in the "second-best" locations, i.e, the closest possible to the banned area, would be natural.

Table A5 in the Appendix shows that the results for the number of registered housing units are robust to an aggregation at the more granular street level, which increases the number of observations. It is noteworthy that the (marginally significant) negative impact following the approval of the new legislation by the parliament (that allowed municipalities to regulate short-term rentals) disappears when the number of registires is per street; conversely, the point estimate and significance of the implementation treatment are unchanged.

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 registered housing units, and the real estate market, i.e., number and price of transactions.

We provide a number of novel results. Firstly, 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, responding to the anticipation of the law discussion.

Secondly, real estate prices decreased by 9%, on average, an effect that lags the ban by two quarters, according to the event studies. There is also a negative effect on the number of sold dwellings, which is, however, non significant. 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.

Thirdly, our findings demonstrate that that short-term rentals segment the real estate market. Two-bedroom properties become 20% cheaper, on average, in the treated areas vis-à-vis the control ones, i.e., twice the average impact. Moreover, the (non-significant) effect on the number of sold houses is also indicative of a stronger negative impact for two-bedroom houses. Two bedrooms is both the mean and the median number of rooms of the dwellings registered as short-term rentals. Moreover, the price decrease is concentrated on the upmarket properties in the last quartile of price per square meter.

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.

These price effects are likely to be a lower bound, because of the spike in the number of registered housing units before the implementation, and also because we analyse a short-run effect, while the short-term rental growth was consistent for several years. 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% (Figure A2), implying that short-term rentals seem to explain only part of the surge. Notwithstanding, the heterogeneous treatment effect for two-bedroom dwellings, which is twice as high as the average one, may help explain the backlash against short-term rentals. This is understudied in the existing literature, which focuses on average effects.

The welfare consequences of these policies are not straightforward. On the one hand, the capitalization of short-term rents into housing prices implies a welfare transfer from aspiring to incumbent owners, and from potential (or displaced) tenants to actual landlords. On the other hand, this capitalization can be interpreted as a windfall gain, at least for those who owned the houses before the boom in the short-term rental platforms; the ban also creates a windfall gain for incumbent owners of short-term rental licences. This second windfall gain can be taxed away by the personal income tax; conversely, the capitalisation effect could only be tackled with a (non-existent) property tax. The desirability of the ban and its extent hinge on the the profiles of these winners and losers, on the outside option of the displaced tenants and on the relative merits of alternative (tax) policies.

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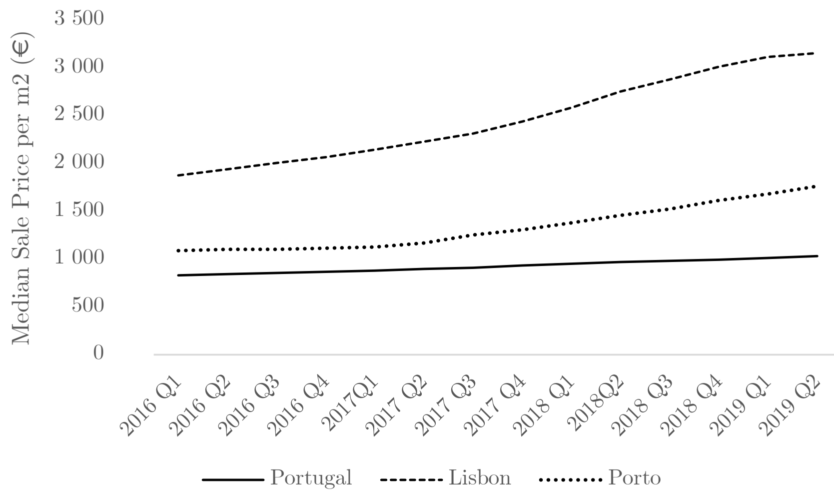
A Appendix

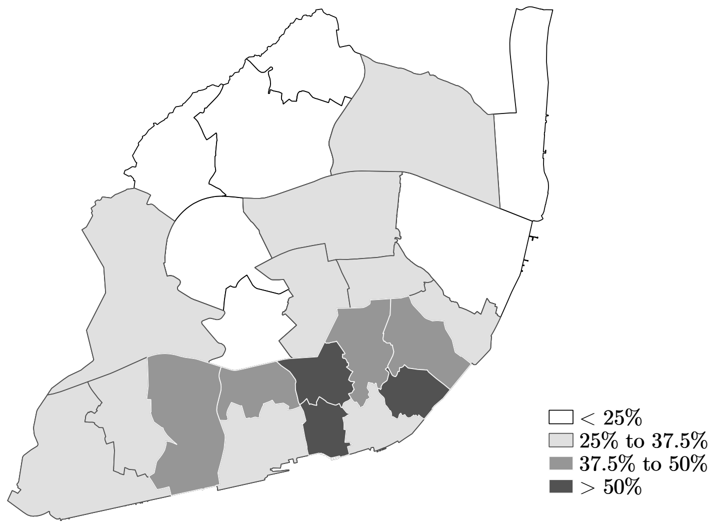
Figures

Figure A1: Median Sale Price (Dwellings) per Square Meter



Source: Statistics Portugal

Figure A2: Median Sale Price Growth Rate in Lisbon: 2016 to 2018



Source: Statistics Portugal

Figure A3: Analysis Timeline

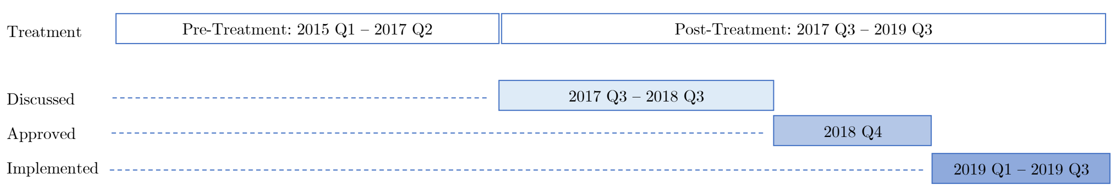
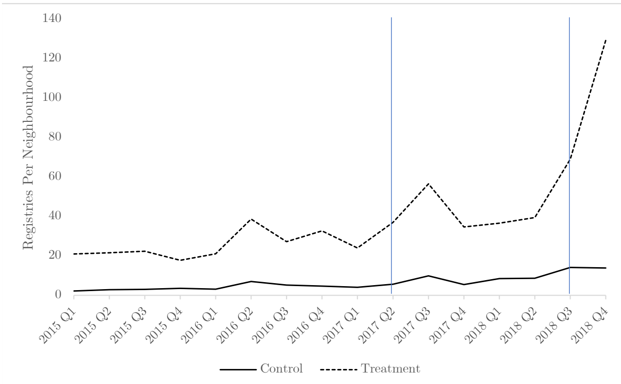
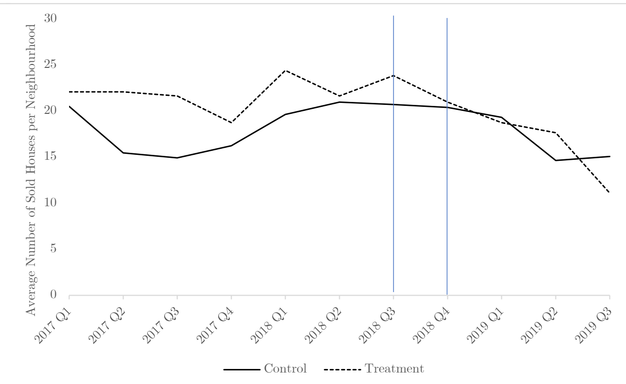


Figure A4: Trends for Outcome Variables



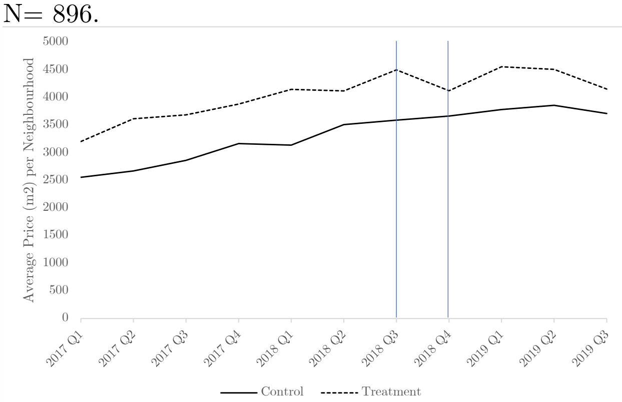
(a) Number of registered housing units

N= 896.



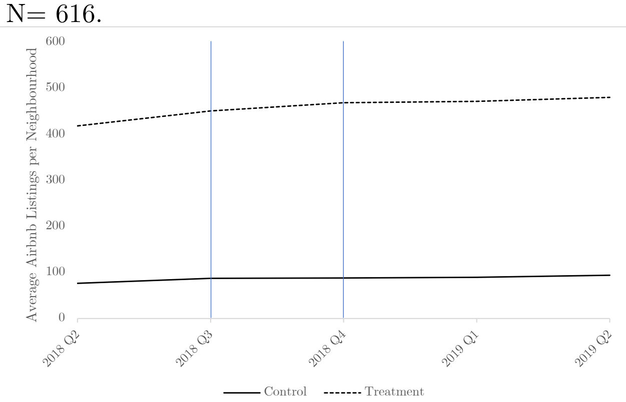
(b) House Sales

N= 616.



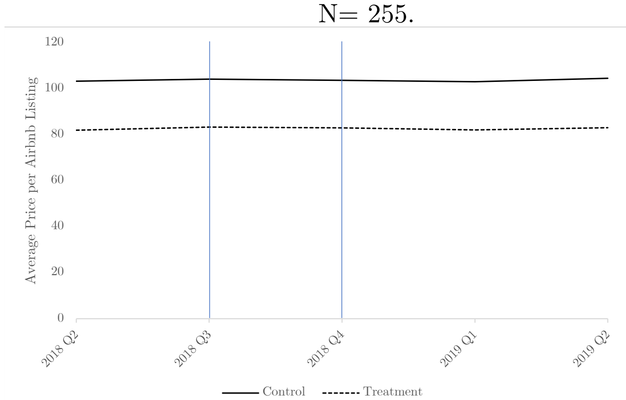
(c) Housing Prices

N= 429.



(d) Airbnb Listings

N= 255.



(e) Airbnb Prices

N= 27460.

Tables

Table A1: Difference-in-Differences - ln(Number of registered housing units)

	Baseline	Robustness				
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat \cdot Discussion$	-0.130*	0.038	-0.105	-0.119*	-0.131*	-0.130**
	(0.07)	(0.09)	(0.07)	(0.06)	(0.07)	(0.06)
$Treat \cdot Approval$	0.464***	0.674***	0.486***	0.419***	0.462***	0.464**
	(0.12)	(0.13)	(0.12)	(0.11)	(0.12)	(0.12)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes	Yes	Yes	Yes
Political controls	No	Yes	No	No	No	No
No of Trees* Trend	No	No	No	No	Yes	No
Control group	Baseline	Baseline	Never treated	+ Graça and Colinas	Baseline	Baseline
Number of Obs.	896	896	736	992	896	896
Adjusted R^2	0.658	0.659	0.674	0.642	0.693	0.658
Cluster	neighbourhood	neighbourhood	neighbourhood	neighbourhood	neighbourhood	Civil Parish

Notes: The vector of Political controls consists of Civil Parishes' political alignment with the Mayor's party and turnout rate in the local elections of 2013 and 2017. The Discussion period spans between 2017 Q3 – 2018 Q3, while the Approval period is 2018 Q4. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Difference-in-Differences - ln(Number of Sold Houses)

	Baseline	Robustness				
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat \cdot Approval$	-0.227	-0.231	-0.277	-0.253	-0.228	-0.227
	(0.27)	(0.26)	(0.27)	(0.26)	(0.27)	(0.25)
$Treat \cdot Implement$	-0.168	-0.172	-0.203	-0.184	-0.170	-0.168*
	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.09)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes	Yes	Yes	Yes
Political controls	No	Yes	No	No	No	No
No of Trees* Trend	No	No	No	No	Yes	No
Control group	Baseline	Baseline	Never treated	+ Graça and Colinas	Baseline	Baseline
Number of Obs.	616	616	506	682	616	616
Adjusted R^2	0.136	0.133	0.139	0.148	0.276	0.136
Cluster	neighbourhood	neighbourhood	neighbourhood	neighbourhood	neighbourhood	Civil Parish

Notes: The Approval period is 2018 Q4, while the Implement period spans between 2019 Q1–Q3. The vector of Political controls consists of Civil Parishes' political alignment with the Mayor's party and turnout rate in the local elections of 2013 and 2017. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Difference-in-Differences - ln(Housing Prices)

	Baseline	Robustness				
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat \cdot Approval$	-0.124	-0.119	-0.115	-0.121	-0.123	-0.124
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)
$Treat \cdot Implement$	-0.087***	-0.083***	-0.096***	-0.084***	-0.087***	-0.087***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes	Yes	Yes	Yes
Political controls	No	Yes	No	No	No	No
No of Trees* Trend	No	No	No	No	Yes	No
Control group	Baseline	Baseline	Never treated	+ Graça and Colinas	Baseline	Baseline
Number of Obs.	429	429	352	473	429	429
Adjusted R^2	0.760	0.759	0.726	0.710	0.774	0.760
Cluster	neighbourhood	neighbourhood	neighbourhood	neighbourhood	neighbourhood	Civil Parish

Notes: The Approval period is 2018 Q4, while the Implement period spans between 2019 Q1–Q3. The vector of Political controls consists of Civil Parishes' political alignment with the Mayor's party and turnout rate in the local elections of 2013 and 2017. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Difference-in-Differences - Spillovers

	ln(Registered housing units)	ln(Sold Houses)	ln(Housing Prices)
$Vicinity \cdot Discussion$	0.073	-	-
	(0.13)	-	-
$Vicinity \cdot Approval$	0.162	0.192	0.034
	(0.13)	(0.24)	(0.05)
$Vicinity \cdot Implement$	-	0.150	-0.051
	-	(0.18)	(0.04)
Quarter FE	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes
Number of Obs.	752	517	374
Adjusted R^2	0.506	0.137	0.783

Notes: Standard errors (in parentheses) are clustered at the Neighbourhood level. Vicinity comprises the group of neighbouring areas to the treated group. The Discussion period spans between 2017 Q3 – 2018 Q3. The Approval period is 2018 Q4, while the Implement period spans between 2019 Q1–Q3. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Difference-in-Differences - ln(Number of registered housing units) aggregated by street

	Baseline
$Treat \cdot Approval$	0.024
	(0.02)
$Treat \cdot Implement$	0.479***
	(0.05)
Quarter FE	Yes
Civil Parish FE	Yes
Number of Obs.	13664
Adjusted R^2	0.131

Notes: Standard errors (in parentheses) are clustered at the Street level. The Approval period is 2018 Q4, while the Implement period spans between 2019 Q1–Q3. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.