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Should We Ban AirBnB? Short-Term Rental Regulation and Housing Prices

Duarte Gonçalves 25884

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Should We Ban AirBnB?

Short-Term Rental Regulations and Housing Prices*

Duarte Gonçalves

Nova SBE

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Abstract

In this paper, we analyze the impact of short-term rental on housing prices through a quasi-natural experiment that exploits a ban imposed in 2018 by the municipality of Lisbon in several of its neighborhoods. We rely on administrative data on the registries of short-term rental housing units and neighborhood housing prices and sold quantities in Lisbon and Porto to employ i) difference-in differences models and ii) event-study designs. Our findings document a sizeable peak in new registries during the period between the announcement of the ban and its implementation, suggesting that agents were able to avoid the restriction. We also show that the ban fell short on its claims and validate the theory that buyers in the housing market value the option to participate in the short-term rental market upon purchase.

Keywords: AirBnB, Policy Analysis, Housing Market, Short-Term Rental

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1 Introduction

It was not until early this century that, for the first time in history, the global urban population outnumbered the rural population. By 2050, the ratio of world's population living in urban areas is expected to be at two-thirds.¹ This fast-moving trend led former UN Secretary General Kofi Annan to coin this as the “Urban Millennium”, and as such, the desire to address sustainable development from an urban perspective is reflected in the goals set by the UN in the *2030 Agenda for Sustainable Development*.²

Among others, housing affordability has become a key matter in the management of this urban growth. However, dealing with the former has revealed to be everything but a smooth journey, with alarms ringing all over the world.³ Focusing on Europe, many reasons have been pointed as being in the root of this issue: from historically low borrowing costs to a booming tourism and the emergence of peer-to-peer markets such as Airbnb.⁴

In theory, these factors may contribute to a speculation-induced expansion in the housing market demand, as investors might acquire properties for potential appreciation, renting them as a source of income during the ownership (Sheppard et al. 2016). This can be quite relevant in this context, since metropolitan areas are usually land constrained and tend to be more regulated (Green et al. 2005), meaning their housing supply is characterized by lower elasticities (Saiz 2010). As a consequence, demand shocks are amplified in the absence of adequate supply side adjustments that could smoothly accommodate the former (Hilber & Vermeulen 2015). They also lead to a shift within the housing market supply, namely by generating a bias towards short-term rental agreements, as the latter become relatively more attractive to landlords, implying a reduction in the stock of properties destined to permanent housing and the consequent arising of inflationary pressures.

¹According to the UN. See un.org/development/desa/en/news/population/2018-revision-of-world-urbanization-prospects.html

²See sustainabledevelopment.un.org/post2015/transformingourworld

³See washingtonpost.com/news/wonk/wp/2018/06/13/a-minimum-wage-worker-cant-afford-a-2-bed-room-apartment-anywhere-in-the-u-s/ & ft.com/content/38f8c8e4-8227-11e9-9935-ad75bb96c849

⁴See nytimes.com/2019/12/17/business/europe-housing.html, ft.com/content/2fe06a7c-cb2a-11e9-af46-b09e8bfe60c0 & expresso.pt/economia/2016-05-29-Valores-de-imobiliario-disparam-nos-arredores-de-Lisboa (in Portuguese)

One of the countries that seems to have particularly felt the effects of the increasing tourism is Portugal. In fact, not only the number of passengers in Lisbon’s airport increased from 13 to 29 million⁵ in the last decade, as the city also tops the ratio of Airbnb listings per inhabitant across European capitals.⁶ Moreover, in 2014, the country opted for a regulatory reform, *Decreto-Lei n.º 128/2014*, which vastly simplified the bureaucratic process for landlords to list their property on hosting platforms and start accepting guests, thereby allowing the transition to short-term rental to become frictionless (Franco et al. 2019). This path seems to have generated regrets, as in 2018 the city was taking a u-turn by imposing restrictions on the very same market, resulting in the creation of areas on which the registry of new short-term rental units was suspended - *Proposta n.º 677/AML/2018*. We take advantage of the latter in order to provide quasi-experimental estimates of the impact of short-term rental marketplaces on house prices, by employing a Difference-in-Differences approach that compares regions which were affected by the registry suspension at different periods in time.

Our findings suggest the occurrence of anticipation effects, with new short-term rental registries increasing by 32.4% in affected areas right before they were suspended. Regarding the housing market, the law’s approval resulted in a decrease in prices estimated at 16% for contained zones, although this effect was less prominent after the implementation. The number of sales decreased by 23.1% in the treated areas after the law became effective, which is consistent with the theory that the option to short-term rent upon purchase is valued by the buyers. However, these effects were only felt by the smaller, most “Airbnb-ish” dwellings.

The paper is organized as it follows: the next section 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. Sections 4 and 5 explain the data and methodology. Section 6 presents the results. Finally, in section 7 we discuss our findings and provide some concluding remarks.

⁵According to data from *Instituto Nacional de Estatística* - INE

⁶See dn.pt/cidades/lisboa-e-a-cidade-europeia-com-maior-racio-de-casas-por-habitante-no-airbnb---moody-10957144.html (in Portuguese)

2 Literature Review

Empirical evidence has uncovered many negative effects that arise from the thrive of platforms such as Airbnb. As expected, hotel revenues significantly decrease as new and accessible alternatives emerge. However, this impact manifests itself primarily through less aggressive hotel room pricing (Zervas et al. 2017). This increased competitiveness in the short-term rental market implies some resulting dividends for consumers (although concerns may be developed on how fair that competition is and what kind of consumers benefits from this). 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. It is critical to disentangle the consequences of this gentrification process, as these can be potentially confounding and therefore hinder any inferences about the causal relationship between Airbnb presence and housing prices.

Despite the existence of a considerable amount of literature in the context of sharing economy, the empirical work on the specific relationship between the prominence of peer-to-peer housing markets and rents is still somewhat scarce.

Among that literature, the main focus resides in the impact of Airbnb on the North American market. Sheppard et al. (2016) employ a matched difference-in-differences to estimate the causal impact of Airbnb presence on New York City's house prices and conclude that, considering the treatment of having Airbnb units nearby, 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. 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. Barron et al. (2018) use data on Airbnb's listings between 2011 and 2016 across the United States to employ a two-stage least squares approach by constructing an instrumental variable based on google trends, but find modest effects.

Koster et al. (2018) take advantage of the Los Angeles’ Home Sharing Ordinances (namely 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 and conclude that Ordinances reduced listings by 50% and house prices by 3%. More recently, Valentin (2019) found that the regulatory reform in New Orleans (which required hosts to pay for an annual short-term rental license, limited the number of days a host could rent per year and defined prohibition zones for this type of rental) displaced landlords from short-term renting and led to a decrease in house prices.

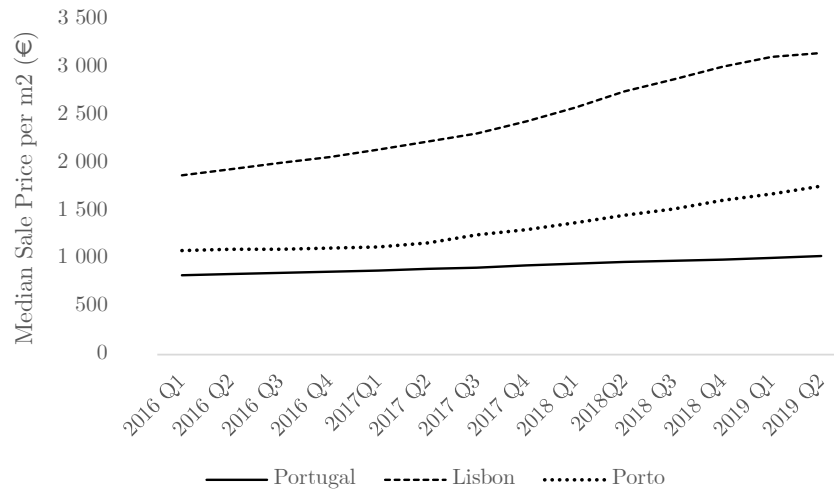
Moreover, some empirical work has been carried out in Southern Europe. Segú (2018) finds that Airbnb is responsible for a 4% increase in Barcelona’s rents between 2009 and 2016, through an instrumental approach based on a listed unit’s distance to the beach. Garcia-López et al. (2019) use an instrumental 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 tourist neighborhoods. Finally, Franco et al. (2019) took advantage of the 2014 Portuguese reform that promoted short-term housing supply by employing a matched difference-in-differences. The authors conclude that, in Portugal, a 1 p.p increase in the share of Airbnb properties increases house prices by 4.5%.

Our paper adds to the existing body of literature by taking advantage of a quasi-natural experiment to use an identification strategy that better restricts the presence of endogeneity (as explained over the following section), therefore resulting in a more robust causal inference.

3 Context & Institutional Framework

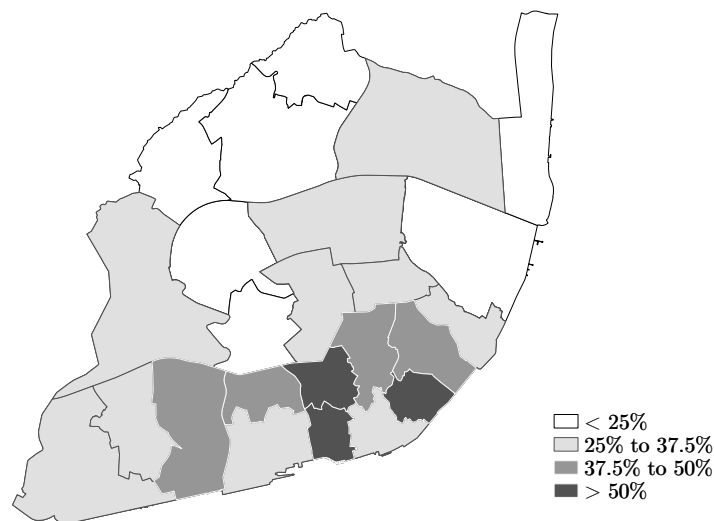
Figure 1 shows the evolution of housing prices in Lisbon and Porto, relative to the nationwide reality. From early 2016 to mid 2019, the median sale price per square meter for Lisbon and Porto increased by 68.2% and 61.9%, respectively. As for the whole country, the growth rate was more modest, sitting at 24.2%.

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



The heterogeneity of this growth was not particular to the aggregate level. Looking at Lisbon, and focusing on the time frame that preceded any sort of action to tackle the issue, *Figure 2* shows how this rise was distributed across the city. It suggests that this was not a homogeneous process, as the highest growth rates were concentrated on central downtown areas, whilst the low growth rates were mainly experienced by peripheral locations.

Figure 2: Median Sale Price Growth Rate in Lisbon: 2016 to 2018 (Source - INE)



Reforms on how short-term rental is regulated have long been a topic of discussion. En route to the 2017 Portuguese municipal elections, the incumbent socialist mayor Fernando Medina declared his intention to enforce a larger public oversight on its supply, an ambition which was shared by the candidates on the left side of the political spectrum.⁷ On the right, despite a common apprehension, candidates expressed their desire to increase incentives for long-term rental rather than imposing regulation as an approach to deal with the situation.⁸

Nevertheless, public intervention was indiligent, as it was only in August 2018, and after a long period of contestation from the Lisbon's civil parishes on which Airbnb presence was sharper, that the Portuguese parliament finally decided to throw a spanner in the works by passing a reform, *Lei 62/2018*, which granted Portuguese municipalities the power to regulate new registrations of properties in the Portuguese Registry Office for Short-Term Rentals (*Registo Nacional de Estabelecimentos de Alojamento Local* - RNAL).⁹ This decentralization of the power to regulate the Short-Term Rental market was seen as being key to deal with such an heterogeneous housing affordability issue.

Subsequently, in November 2018, the municipality of Lisbon passed a legislation, *Proposta n.º 677/AML/2018*, that suspended new registrations of units belonging to certain pre-designated areas, known as *Zonas Turísticas Homogéneas*, if these were deemed to be oversaturated, i.e., if their ratio of short-term rental to total properties was above 25%. This criteria was computed with information based on the short-term rental registry and the 2011 census. The areas were delimited according to their urban layouts and types of public spaces, and taking into account the civil parishes' borders. In terms of their aggregation level, these areas are agglomerates of neighborhoods which constitute a fraction of the civil parishes.

⁷See publico.pt/2017/08/30/local/noticia/medina-quer-limitar-alojamento-local-nos-bairros-historicos-de-lisboa-1783830

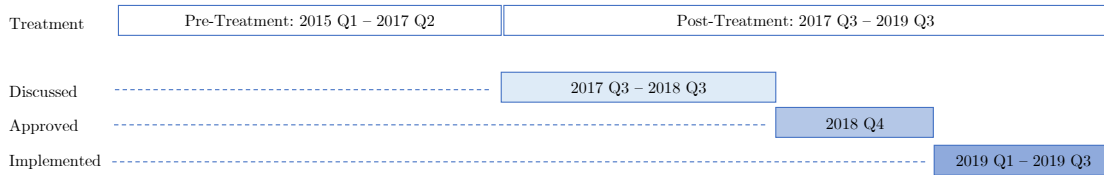
⁸See eco.sapo.pt/2017/06/09/teresa-leal-coelho-defende-incentivos-ao-arrendamento-de-longa-duracao/

⁹See publico.pt/2018/04/05/local/noticia/autarca-de-santa-maria-maior-pede-suspensao-do-licenciamento-de-alojamento-local-na-cidade-1809305. It may be worth noticing that the request for this registration is a sufficient and necessary condition for landlords to fill if they wish to list their property on a hosting platform (according to *Decreto-Lei n.º 128/2014*). This process can easily be carried out, either online or at a government office.

In April 2019, the city extended the suspension on the regulated areas and a new draft, *Proposta n.º 204/CM/2019*, was approved and submitted for public discussion. This draft intended to update the suspension areas according to new data on the short-term rental ratios. The final version was published in November 2019 (*DR n.º 214/2019, 1.º Suplemento*).

Figure 3 summarizes the timeline that guides our analysis.

Figure 3: Analysis Timeline



Overall, Lisbon seems to embody an almost ideal context to examine the effects of Airbnb on housing affordability. Besides the already mentioned prevalence of tourism-related activities, Airbnb itself represents 74% of the activity among peer-to-peer housing platforms in Lisbon, 64 p.p ahead of its closest competitor.¹⁰

In addition, issues related to the internal validity of this study does not seem to constitute a problem, as the structure of the regulatory reform allows for the provision of a satisfying control group. By comparing geographically close neighborhoods, we control for differences in possibly confounding factors, namely the tax regime and the access to amenities.¹¹ Moreover, the treated zones had short-term rental to total property ratios of 27% and 29% (slightly above the limit), while the zones in our baseline control group had ratios of 18% and 25% (the latter just at the cut-off).¹² Since the chosen threshold is somewhat arbitrary, and house-owners in treated areas cannot individually control their treatment status, the argument for selection on unobservables as a consequence of the treatment assignment should not hold.

¹⁰According to AirDNA, one of the largest databases on short-term rental analytics.

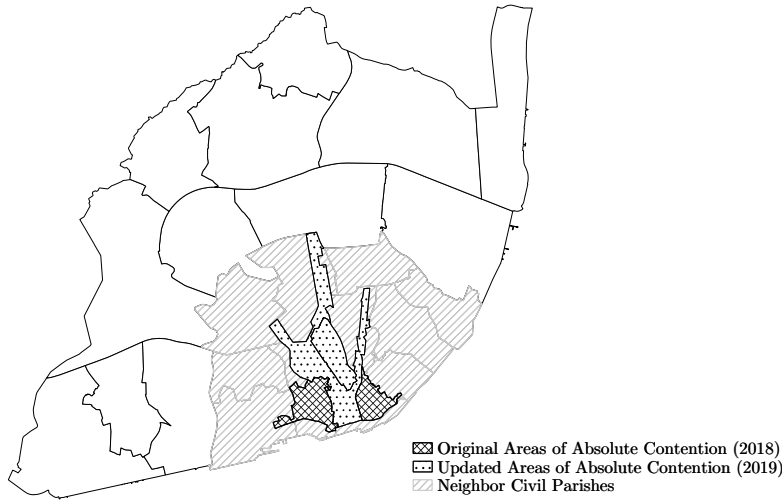
¹¹Several recent empirical studies have focused on the impact of taxation on house prices, namely the effect of transaction taxes [Dachis et al. (2012), Besley et al. (2014), Kopczuk and Munroe (2015), Hilber and Lyytikäinen (2017), Slemrod et al. (2017), Best and Kleven (2018)], income taxes (Basten et al., 2017), and property taxes [see Ross and Yinger (1999), Sirmans et al. (2008), Hilber et al. (2011), Elinder and Person (2017, Bradley (2017))].

¹²For perspective, the 5th zone with the highest short-term rental intensity had a ratio of 10%.

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”. In this case, not long after initial assignment, as soon as the data on the geographical concentration of short-term rental housing got updated, additional areas were treated.¹³ Although not intended, this lag can be exploited in order to provide quasi-experimental estimates for the impact of the suspension.

Figure 4 gives a geographical perception of the considered comparison groups. We observe, in black, the areas for which registries were initially suspended (our treatment group). Filled with a dot pattern lies our baseline control, which includes the areas that were suspended in 2019. Two additional control groups are considered. The first one, which consists on the area belonging to Neighbor civil parishes, adds the gray area to the baseline control. The second one further adds the two civil parishes from Porto that display the highest short-term rental to total units ratio, which later in 2019 experienced restrictions as well.¹⁴

Figure 4: Treated Areas & Control Groups



¹³As the data that based the eligibility was updated even before the approval, it is likely that at least part of our control group would be eligible for treatment by the time the ban was actually implemented.

¹⁴See publico.pt/2019/07/10/local/noticia/porto-suspende-novos-registos-alojamento-local-centro-historico-bonfim-1879480 (in Portuguese)

4 Data

Our paper uses two sources of data.

The first one consists on publicly available information from the National Short-Term Rental Registry (RNAL). As of September 2019, our sample included 16972 submitted registries in Lisbon and 7274 in Porto, each containing individual details, namely its date and address (among others, such as the number of rooms of the housing unit, the nationality of the ownership and its status, i.e., either if it is a singular or collective entity).

The analysis of this data set, although diverging from the main topic of this paper, is quite relevant since the law that allowed municipalities to regulate new registrations 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 landlords from targeted areas to register their properties before the registry suspension, even if they had no intention of entering the market in the short-run, potentially softening the (intended) effects of the law.

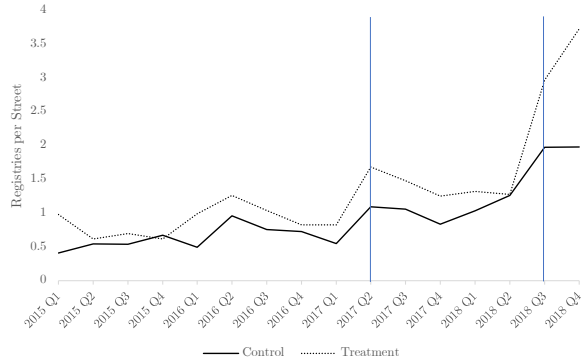
We also analyze an administrative data set (obtained from *Confidencial Imobiliário*) that contains quarterly information about the number of house sales and their average and quartile prices per square meter, for every neighborhood in Lisbon and Porto within an urban rehabilitation area, between the first quarter of 2015 and the third quarter of 2019.¹⁵

By designating the neighborhood areas in a similar fashion as the one used by the municipality of Lisbon when developing the studies that based the treatment assignment, this data set allows us to define both the originally treated areas as well as our baseline control group with a significant level of accuracy.

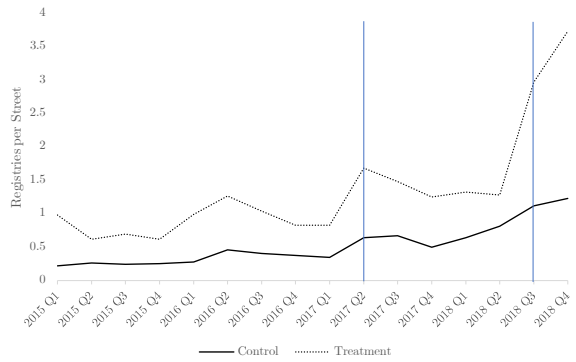
The trends in registries per street and housing prices per neighborhood are shown in *Figure 5*. The blue lines separate the pre-treatment and the different post-treatment periods. Descriptive statistics for both data sets are displayed in *Table 1*.

¹⁵Confidencial Imobiliário is a Portuguese databank specialized in real estate. The provided data set (SIR.RU) results from a protocol established with the Municipalities of Lisbon and Porto. Urban rehabilitation areas are zones in which, by law, the city is entitled to pre-emption rights. Data on prices is more restricted than data on quantities, since that, for a sufficiently low number of transactions in a given neighborhood over a given quarter, price information is omitted, in order to preserve anonymity.

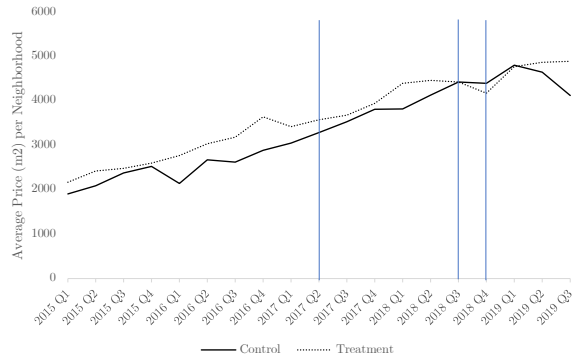
Figure 5: Trends for Short-Term Rental Registries and Housing Prices



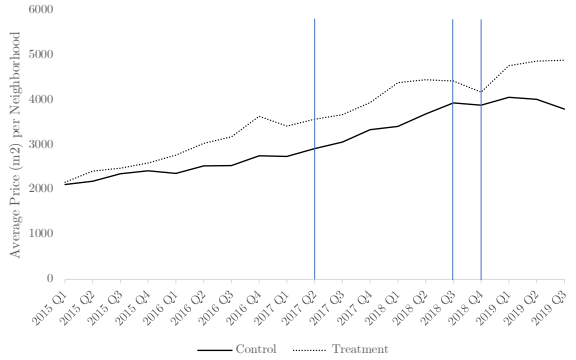
(a) Registries: Baseline Control Group



(b) Registries: Neighbors Control Group



(c) Housing Prices: Baseline Control Group



(d) Housing Prices: Neighbors Control Group

Table 1: Descriptive Statistics on Sample Characteristics

	Pre-Treatment				Post-Treatment			
	Treatment	Baseline Control	+ Neighbors	+ Downtown Porto	Treatment	Baseline Control	+ Neighbors	+ Downtown Porto
<i>A.Registrations</i>								
Number of Streets	300	161	653	931	310	174	761	1133
Registries (per Street)	10.4 (9.9)	8.2 (10.3)	4.7 (6.6)	5.5 (8.1)	12.7 (12.9)	9.1 (11.5)	5.8 (7.7)	6.7 (11.0)
% Singular	0.530	0.499	0.529	0.528	0.482	0.405	0.521	0.521
% National	0.961	0.964	0.965	0.970	0.940	0.953	0.950	0.950
Rooms (per Registry)	1.8 (2.0)	2.0 (2.1)	2.1 (2.1)	2.0 (2.1)	1.9 (1.4)	2.5 (2.1)	2.3 (2.0)	1.9 (2.0)
% Alignment	0.519 (0.022)	0.484 (0.027)			0.441 (0.071)	0.399 (0.073)		
% Turnout	0.577 (0.013)	0.571 (0.021)			0.563 (0.030)	0.525 (0.066)		
<i>B.Housing Prices (m2)</i>								
Number of Neighborhoods	9	18	57	60	9	18	57	60
Average Price (per Neighborhood)	2928.5 (322.9)	2668.7 (1002.5)	2507.7 (709.9)	2496.8 (1439.3)	4417.6 (412.9)	4154.0 (1118.1)	3708.9 (948.2)	3688.4 (934.5)
% Alignment	0.523 (0.041)	0.488 (0.050)			0.474 (0.052)	0.411 (0.065)		
% Turnout	0.546 (0.053)	0.552 (0.055)			0.523 (0.060)	0.538 (0.069)		

Notes: “% 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.

5 Methodology

5.1 Difference-in-Differences Estimations

Our econometric specifications were based on the following equation:

$$\begin{aligned} \ln(Y_{s,p,q}) = & \alpha_{s,p} + \delta Treat_{s,p} + \beta_1 Treat_{s,p} \cdot Discussion_q + \beta_2 Treat_{s,p} \cdot Approval_q \\ & + \beta_3 Treat_{s,p} \cdot Post_q + \gamma X_{p,q} + \lambda_p + \tau_q + \epsilon_{s,p,q} \end{aligned} \quad (1)$$

Where the outcome variable takes the value of (the logs of) $R_{s,p,q}$, the number of registrations belonging to street s from civil parish p in quarter q , $AP_{n,p,q}$, the average sale price of housing units belonging to neighborhood n from civil parish p in quarter q and $S_{n,p,q}$, the total number of sold houses in neighborhood n from civil parish p in quarter q . The independent variables include $Treat_{s,p}$ as a treatment dummy that takes the value of 1 if the street is contained in the original absolute contention areas, $Treat_{s,p} \cdot Discussion_q$ as an interaction term between the treatment variable and the period between the electoral campaign and the proposal's approval (2017 Q3 to 2018 Q3), $Treat_{s,p} \cdot Approval_q$ as an interaction term between the treatment variable and the period ranging from the approval of the proposal to its implementation (2018 Q4), $Treat_{s,p} \cdot Implement_q$ as an interaction between the treatment variable and the periods after which the law became effective (2019 Q1 to 2019 Q3), $X_{p,q}$ as time-varying civil parish specific controls, which include the abstention level and political alignment with Lisbon's mayor, λ_p and τ_q as civil parish and quarter fixed effects (respectively), that account for time-invariant characteristics. $\epsilon_{s,p,q}$ is an error term.

Logs are used due to the right-skewness in the dependent variables' distributions and for interpretation purposes. The least count value is also added in the log-transformation of $R_{s,p,q}$ due to the existence of zero-values in the observations. 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.

To account for serial correlation within the panel units, standard errors were clustered (Bertrand et al. 2004). This was done at the most aggregate level (Civil Parish), in an attempt “to be conservative and avoid bias” (Cameron & Miller 2015, p.17).

5.2 Event Study Plots

Event study exercises were also carried out, according to the following equation:

$$\ln(Y_{s,p,q}) = \lambda_p + \tau_q + \sum_{q \neq 2017Q2} \beta_q (Treat_{s,p} \cdot Quarter_q) + \epsilon_{s,p,q} \quad (2)$$

Where the outcome variable takes the value of (the logs of) $R_{s,p,q}$ and $AP_{n,p,q}$. The regressor $Treat_{s,p} \cdot Quarter_q$ is an interaction term between the treatment and the quarter dummies. λ_p and τ_q are civil parish and quarter fixed effects, just as in equation (1). $\epsilon_{s,p,q}$ is an error term. Log-transformations are carried out in a similar way as in the difference-in-differences model. Standard errors were again clustered at the civil parish level.

The base period is the second quarter of 2017, as it is the last one that preceded the electoral campaign for the 2017 Portuguese municipal elections, which kicked off the debate around prospective suspension on short-term rental in specific areas of the city, therefore enacting any possible reactions.

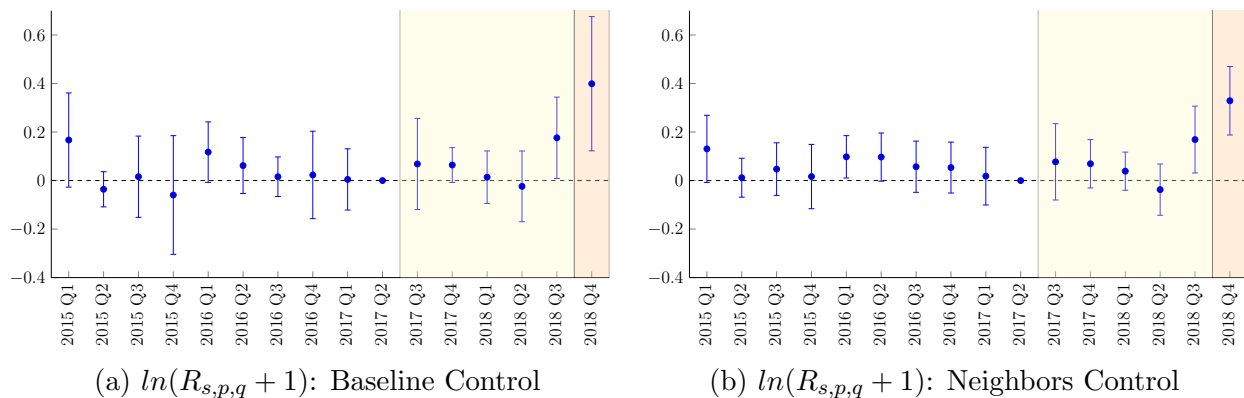
The conduction of these event studies is quite important as it allows us to formally test if, prior to the discussion on restrictive regulation, the concentration of short-term rental registries and the house prices displayed parallel trends.

6 Results

6.1 Registrations

Looking at the results related to the registry data, *Figure 6* shows that the parallel trend hypothesis is not rejected in the pre-treatment period.¹⁶ However, during the approval period (in orange), home owners from targeted areas which had not registered their property appear to have rushed that procedure before the law became binding, possibly undermining its goals.

Figure 6: Event Studies - Registrations



The estimates of equation (1), which can be observed in *Table 2*, 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 32,4% in the quarterly number of registrations as a result the law’s approval.

However, this treatment effect was not homogeneous. *Table 3* shows that this reaction was much stronger for domestic house owners, relative to foreign ones. Reasons may include increased awareness by nationals in respect to the law and the registration process, allowing a faster reaction. Moreover, the effect was higher for owners of smaller units, so we confirm the widespread belief that the latter are the main segment of the short-term rental market.

¹⁶The shaded area corresponds to the post-treatment period, namely to the discussion (in yellow) and the approval (in orange).

Table 2: Difference-in-Differences - Registrations

	Baseline Control			+ Neighbors	+ Downtown Porto
	(1)	(2)	(3)	(4)	(5)
<i>Treat · Discussion</i>	0.008 (0.03)	0.008 (0.03)	0.023 (0.03)	0.036 (0.02)	0.014 (0.02)
<i>Treat · Approval</i>	0.324*** (0.06)	0.324*** (0.06)	0.343*** (0.08)	0.385*** (0.05)	0.417*** (0.05)
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.	8352	8352	8352	19392	26160
Adjusted R^2	0.095	0.105	0.105	0.143	0.120

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 3: Heterogeneous Effects - Registrations

	Rooms (Median)		Ownership Status		Nationality	
	Below/=	Above	Singular	Colective	Domestic	Foreign
<i>Treat · Discussion</i>	0.010 (0.03)	-0.009 (0.04)	-0.007 (0.02)	0.016 (0.04)	-0.002 (0.04)	0.014 (0.02)
<i>Treat · Approval</i>	0.297*** (0.08)	0.126** (0.05)	0.234*** (0.06)	0.235*** (0.04)	0.309*** (0.06)	0.094 (0.06)
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.	7824	6256	7328	7744	8320	3504
Adjusted R^2	0.101	0.065	0.078	0.077	0.099	0.056

Notes: The comparison group is our baseline control.

Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Finally, to assess any potential spillover effects, equation (1) was estimated again, this time with a treatment group that consisted on the streets located right outside the suspension border. Besides possible contagion, a rationale for spillover effects may be the notion that, after the law becomes effective, the establishment of new short-term rental units might start to concentrate in the “second-best” locations, i.e, the allowed areas which are closer to the suspended ones. *Table 4* exhibits the results. Our baseline specification suggests the absence of statistically significant spillovers, even after the suspension became binding, ruling out any displacement effects. This is likely a consequence of the anticipation observed in the treated areas, which may have simply decreased the local need for new registrations instead of shifting them to the outside borders.

Table 4: Spillover Effects - Registrations

	Baseline Control			+ Neighbors	+ Downtown Porto
	(1)	(2)	(3)	(4)	(5)
<i>Treat · Discussion</i>	0.005 (0.05)	0.005 (0.05)	0.008 (0.05)	0.046 (0.05)	0.023 (0.03)
<i>Treat · Approval</i>	-0.008 (0.08)	-0.008 (0.08)	-0.004 (0.08)	0.067 (0.09)	0.098 (0.08)
<i>Treat · Implement</i>	-0.002 (0.04)	-0.002 (0.04)	0.001 (0.05)	-0.116 (0.09)	-0.196* (0.11)
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.	4218	4218	4218	17480	25916
Adjusted R^2	0.050	0.080	0.079	0.083	0.070

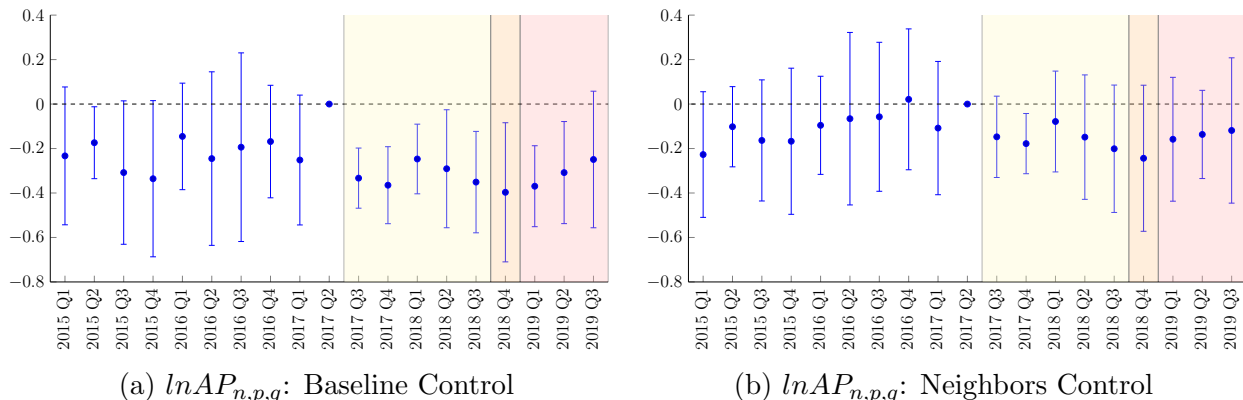
Notes: Controls include Civil Parishes’ political alignment with the Mayor’s party and turnout rate. Standard errors (in parentheses) are clustered at the Civil Parish level.

Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6.2 Housing Prices

Focusing on the effects on house prices, the event-studies shown in *Figure 7* do not appear to reject the pre-treatment parallel trend assumption. Moreover, it is interesting to observe how prices seem to react during the discussion period (in yellow), even before the approval.¹⁷ Comparing to *Figure 6*, it looks that prices had a faster response, relative to the registrations.

Figure 7: Event Studies - Housing Prices



The estimated coefficients from equation (1) (which can be observed in *Table 5*) yield a rather curious result. Although the suspension of short-term rental registrations seems to have induced a decrease in the housing prices, this effect is higher (both in magnitude and statistical significance) for the period between the approval of the law and its implementation. In this case, prices decreased by 16% shortly after approval, and 7.7% after implementation (with this last effect not being statistically significant under stricter confidence intervals).

To infer the heterogeneity in these effects, we re-estimate the difference-in-differences model by the number of rooms and energetic efficiency. The results (in *Table 6*) are consistent with the findings in *Subsection 6.1*, as they show that the more “Airbnb-marketable” properties are the ones affected by the suspension, meaning that the restriction is unable to provide a general increase in housing affordability for households that inhabit their property.

¹⁷The shaded area corresponds to the post-treatment period, namely to the discussion (in yellow), the approval (in orange) and the implementation (in red).

Table 5: Difference-in-Differences - Housing Prices

	Baseline Control			+ Neighbors	+ Downtown Porto
	(1)	(2)	(3)	(4)	(5)
<i>Treat · Discussion</i>	-0.095 (0.05)	-0.082 (0.04)	-0.089 (0.05)	-0.039 (0.04)	-0.035 (0.04)
<i>Treat · Approval</i>	-0.179*** (0.03)	-0.160*** (0.03)	-0.178*** (0.04)	-0.132*** (0.04)	-0.124*** (0.04)
<i>Treat · Implement</i>	-0.088** (0.03)	-0.077* (0.03)	-0.079* (0.03)	-0.023 (0.02)	-0.026 (0.02)
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.	459	459	459	1037	1078
Adjusted R^2	0.441	0.647	0.544	0.631	0.626

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 6: Heterogeneous Effects - Housing Prices

	Number of Rooms			Energy Efficiency (Median)	
	1 Room	2 Rooms	3+ Rooms	Above	Below/=
<i>Treat · Discussion</i>	-0.077 (0.15)	-0.024 (0.05)	-0.133 (0.18)	0.013 (0.10)	-0.015 (0.07)
<i>Treat · Approval</i>	-0.295** (0.10)	-0.186 (0.13)	0.040 (0.04)	-0.110 (0.13)	0.135 (0.10)
<i>Treat · Implement</i>	-0.063 (0.13)	-0.174*** (0.05)	-0.041 (0.07)	0.018 (0.12)	0.060 (0.09)
Quarter FE	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes	Yes	Yes
Political Controls	No	No	No	No	No
Number of Obs.	244	210	192	240	310
Adjusted R^2	0.373	0.607	0.388	0.391	0.473

Notes: The comparison group is our baseline control.

Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Finally, we test how the ban of new short-term rental registrations affected the dispersion of the prices in the housing market. To do this, equation (1) is re-estimated, with the quartile coefficient of dispersion as the outcome variable. This (relative) measure is computed by dividing the difference between the third and the first quartiles by their sum. *Table 9* shows the results.¹⁸ We find no significant effects in terms of housing prices' dispersion as a consequence of the suspension.

Table 7: Difference-in-Differences - Quartile Coefficient of Dispersion

	Baseline Control			+ Neighbors	+ Downtown Porto
	(1)	(2)	(3)	(4)	(5)
<i>Treat · Discussion</i>	-0.004 (0.01)	-0.005 (0.01)	-0.005 (0.01)	0.003 (0.01)	N/A
<i>Treat · Approval</i>	-0.007 (0.01)	-0.006 (0.01)	-0.008 (0.01)	-0.030 (0.02)	N/A
<i>Treat · Implement</i>	-0.009 (0.02)	-0.008 (0.02)	-0.009 (0.02)	-0.012 (0.01)	N/A
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.	180	180	180	468	N/A
Adjusted R^2	0.047	0.032	0.037	0.078	N/A

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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

¹⁸Estimates considering neighborhoods in downtown Porto as part of the control group are not available due to lack of data

6.3 Number of Sold Houses

In the previous section, we have seen that the ban imposed by the municipality of Lisbon led to a decrease in house prices on the treated areas. It becomes interesting to test the mechanism behind it, as basic microeconomic theory tells us that the decrease could be justified by either a contraction in demand (as happens if potential buyers value the possibility of short-term renting upon purchase) or a supply expansion (which is consistent with house owners being stranded as a result of the suspension, unable to participate in the short-term rental market). To find out which channel predominates, we can examine the change in the transacted quantities, since a supply-side reaction would result in a higher number of sold houses, whereas a demand-side reaction would result in a lower number of transactions.

The evidence in *Table 8* 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 23.1% in the number of transactions after the law became effective.

Table 8: Difference-in-Differences - Number of Sold Houses

	Baseline Control			+ Neighbors	+ Downtown Porto
	(1)	(2)	(3)	(4)	(5)
<i>Treat · Discussion</i>	-0.180 (0.18)	-0.182 (0.18)	-0.183 (0.19)	-0.222 (0.16)	-0.253 (0.17)
<i>Treat · Approval</i>	-0.307 (0.39)	-0.296 (0.39)	-0.298 (0.40)	-0.464 (0.35)	-0.575 (0.36)
<i>Treat · Implement</i>	-0.230** (0.07)	-0.231** (0.07)	-0.231*** (0.08)	-0.433*** (0.12)	-0.467*** (0.12)
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.	509	509	509	1244	1318
Adjusted R^2	0.186	0.266	0.232	0.192	0.208

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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This goes along with the argument that the option to participate in the short-term rental market after a house purchase is a key determinant of the housing market demand. Moreover, it also reflects the deficiencies in the structure of the regulatory process caused by the gap between the announcement and the implementation, which prevented a shift within the market supply that would increase the availability of dwellings.

In line with the analysis carried out in *Subsection 6.2*, we re-estimate the difference-in-differences model by the number of rooms and energetic efficiency. The results (in *Tables 9*) are consistent with the heterogeneity found in the effects on the other variables. They also suggest that the decrease in the number of transacted houses is mostly motivated by the energetically efficient units.

Table 9: Heterogeneous Effects - Number of Sold Houses

	Number of Rooms			Energy Efficiency (Median)	
	1 Room	2 Rooms	3+ Rooms	Above	Below/=
<i>Treat · Discussion</i>	-0.061 (0.14)	-0.153** (0.05)	-0.157 (0.09)	-0.418 (0.27)	0.028 (0.16)
<i>Treat · Approval</i>	0.222 (0.23)	-0.235 (0.26)	0.207 (0.13)	0.226 (0.28)	-0.295 (0.28)
<i>Treat · Implement</i>	-0.116 (0.26)	-0.384** (0.11)	-0.316 (0.20)	-0.594*** (0.12)	-0.118 (0.20)
Quarter FE	Yes	Yes	Yes	Yes	Yes
Civil Parish FE	Yes	Yes	Yes	Yes	Yes
Political Controls	No	No	No	No	No
Number of Obs.	458	427	449	448	490
Adjusted R^2	0.307	0.186	0.039	0.238	0.178

Notes: The comparison group is our baseline control.

Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7 Final Remarks

Overall, evidence suggests that *Proposta n.º 677/AML/2018* fell short on its claims. Although housing prices appear to have been negatively affected by the law, this effect is restricted to a specific segment of the housing market, and its magnitude may not be sufficient to attract middle-class people to the targeted zones, meaning that, ultimately, the ban might have failed to accomplish its goal of reducing the compositional socioeconomic asymmetry that characterizes specific areas of the city.

The main reason for this is the fact that the suspension was impaired even before it was implemented, by allowing considerable anticipation by the agents (as demonstrated in this paper), who shielded themselves against the restriction, undermining its ambitions. This is a consequence of a combination of factors, namely i) the law only limiting new registries and ii) the entire bureaucratic process behind its implementation being slow and predictable.

In practice, this meant that, unlike the desired incentives that were induced in the demand side (more specifically the discouragement for speculative house purchases), the intended supply side effects (in this case a shift in its composition that would increase the available quantity in the long-term market) were simply absent.

Moreover, our results also imply that, in areas which are affected the most by platforms such as Airbnb, the expectation of future capital gains are a relevant factor behind the demand for housing units. This can constitute a problem in the context of an urban society which thrives for a sustainable socioeconomic development, as its dynamics may be severely affected by the gentrification that may arise from this process.

Therefore, the findings from this paper, irregardless of causing some apprehension regarding the used instrument, do point to the idea that there is scope for intervention.

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