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"Airbnb in the City" : assessing short-term rental regulation in Bordeaux

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Abstract
Short-term rental platforms, led by *Airbnb*, have disrupted the tourism accommodation industry over the last decade. This disruption has sometimes come along with unwanted long lasting effects on the urban dynamics of cities, and it has encouraged policy-makers to intervene. However, little is known about how effective such interventions are. This paper empirically evaluates the impact Bordeaux’s regulation has had on STR activity through both a Differences-in-differences and a spatial discontinuity design. We find that regulation has had a reductive effect of over 316 rented days per month per district on average. This equates to over half of a pre-regulation standard deviation and 27 thousand nights spent per month in STRs across the city. However, the city’s attempts to limit activity stemming from commercial listings yields mixed results as compliant home-sharing listings also seem to have modified their behaviour. Additionally, analysis at the city border points towards the existence of potential spillover effects on the suburbs, further paving the way for discussion about the effectiveness of one-size-fits-all STR policy design.

Keywords : Short-term rental, Airbnb, Regulation, Tourism, Housing, Spatial Discontinuity, Differences-in-differences

Introduction

The rise of short-term rental (STR) online platforms has drastically changed the way tourists and dwellers experience the city. Thanks to online intermediaries and platforms like Airbnb, it has become easy to lease one’s accomodation for short periods of time. This grants dwellers the opportunity for additional revenue when the accommodation is not occupied and offers tourists a large variety of lodging in different neighborhoods of the city, leading to an authentic “off-the-beaten-track” (Maitland, 2010) experience. Unfortunately the fast growth of the STR market has taken the local governments by surprise and most touristic agglomerations in the world face important negative externalities. Barcelona, Lisbon and Berlin are some examples of iconic cities where central and historic neighborhoods have been transformed under pressure from short-term rental.

STRs are blamed for many troubles, namely for a pressure on housing prices that leads to a crowding out effect on the residential rental market (Barron et al., 2020; Garcia-López et al., 2020; Horn and Merante, 2017; Koster et al., 2018), for transforming neighborhoods with for instance the old market of Boqueria in Barcelona that now serves homogeneous tourist with standardized products, for nuisance and nightlife that affects the well-being of inhabitants, and for an "unfair" competition with Hotel Industry (Zervas et al., 2017). Overall, STR comes with ambivalent net effects, as positive externalities for some individuals can be over-compensated by negative externalities for others and vice-versa. It is also of interest to local public authorities in charge of planning and development of a city as it becomes difficult to figure out why and where airbnb has to be regulated or nurtured.

As a result, a global movement of regulation is rising over concerned cities, starting with metropolitan and mostly touristic destinations(Nieuwland and Melik, 2020). However no dominant design seems to exist in the rules implemented by the governments whether they are local or national. Each city seems to pick in a bunch of heterogeneous measures which range from a limit on the number of days authorized for STR to a mandatory registration or even a strict ban in specific areas (von Briel and Dolnicar, 2020). In the end, laissez-faire remains an option for many cities. The ability of local and national authorities to enforce these regulation attempts is also very heterogeneous, which makes the assessment of the effectiveness of these public policies difficult. .

France is the most visited country in the world and faces a vast diffusion of airbnb with some listings offered for rental in many cities. Among them, the city of Bordeaux is known throughout the world for its wine production and it provides an interesting case as in july 2017, the city hall has decided to pass a rule which strongly increases the cost of using a dwelling for furnished tourist accommodation. Following a huge increase in STR supply, local authorities decided on a "compensation rule" that targets hosts who lease their primary residence for over 120 days a year as well as all hosts that don't live in the dwelling. In the continuum of regulation measures, the City of Bordeaux chose a moderate action by encouraging a targeted change in individual behaviour of leasing. The objective is to prevent or at least limit some undesired urban dynamics (nuisance, negative externalities, emergence of airbnb clusters, etc) by restricting airbnb activity. To our knowledge, this research is one of the first to evaluate the individual consequences of a targeted regulation of STR. In particular we estimate how the supply of STR is affected over the city and who are the hosts that suffer the most from novel restrictions. To this aim, we distinguish between 3 categories of hosts : those who are targeted (super-hosts), those who are not (normal-hosts or STR hosts that follow the spirit of the sharing economy by renting all or part of their dwelling for a limited period of time), and those of inhabitants who newly enter the market of STR.

We use two different empirical designs to build a robust contrafactual and identify the potential effects in the behaviour of the different renters.

Our main results are twofold :

1/ Regulation in Bordeaux has had a net negative effect on STR activity of 316 rented days per month per district on average. This equates to over half of a pre-regulation standard deviation. This aggregate result also holds in the city's peripheral neighborhoods, where any listing on Bordeaux's side of the border has rented almost 30 fewer days on average than its competitors on the other side of the border in the two years following regulation.

2/ In addition to the dip in activity of commercial listings, home-sharing listings have also sustained losses in activity following regulation, particularly near the city's border. This result is not due to a lack

of market entrants, and considering a constant Airbnb demand assumption, it points towards changes in the behaviour of existing compliant listings.

Our results suggest some new ways to regulate digital platforms devoted to STR. In particular a one size fits all policy comes with undesired and indirect effects and a more targeted and smart regulation is undoubtedly more accurate.

The remainder of this paper is as follows. In section 1 literature on STR and urban dynamics is surveyed. Section 2 is devoted to our empirical strategy with two different econometric designs. Section 3 reports our commented results which are then discussed in regard to their policy implications and smart-regulation of STR.

1 - Urban dynamics, STR and regulation

Most of the literature has focused on the effect of STR on rents and housing prices and on the hotel industry. Using a dataset of all the STR listings available in the US cities over the 2011-2016 period, Barron et al., (2020) show that a 1% increase in the supply of listings is associated with a 0.018% increase in rent rates and a 0.026% increase in housing prices. However, the estimation of this average effect covers very different realities and contexts, legitimating that other scholars studied the consequences of STR in cities and regions which were the most impacted by STR. Horn and Merante, (2017) suggest that in Boston, STR is responsible for a decline in the supply of housing offered for rent and has led to an increase in rents (0.4 percent for one standard deviation increase in Airbnb listings). Garcia-López et al. (2020) reach similar conclusions but considering the case of Barcelona, showing that both rents and prices increase with the number of Airbnb listings. Combining different empirical strategies, they have also shown that these effects are heterogeneous since in the most touristic part of the city the increase in rents could be 7% (The city-wide average is 1.9%) while the posted and transaction prices could have grown by 14% and 17% respectively. For France Ayouba et al., (2020) have shown that Airbnb could have a significant positive impact on rents in the agglomeration of Paris, Montpellier or Lyon.

Los Angeles has also been the center of all the attention as it faces a strong housing crisis. It has been one of the first and most impacted cities following the development of STR, and one of the first to try to regulate the phenomenon (Lee, 2016). These attempts of regulation inaugurate a series of papers which exploit these public policies as a natural experiment to estimate the effect of Airbnb on rents and housing prices. The work of Koster et al. (2018) takes advantage of the fact that 18 of 88 cities in the county of Los Angeles have implemented restrictions for “vacation rental”. They combine a Spatial regression Discontinuity Design (SD) at the border of the 18 cities and a Differences-in-differences (DID) estimation comparing at the zipcode level areas which have and have not implemented the regulation. Their results show that *Airbnb* could be responsible for a 10% increase in property values in the Central Business District of L.A., while regulation could have decreased the number of listings by 50% and the house prices by 3%. In Berlin, restrictions imposed by the local government have been increasing since 2014 and have led, according to Duso et al., (2020) to a large decrease in listings available on *Airbnb*. The authors use the regulation to infer the increase in rents generated by STR and highlight the heterogeneity of this effect according to the nature of the accommodation rented or the density of STR in the district. Another paper by Valentin, (2020) studies the complete ban of STR in the French quarter of New Orleans. The author estimates that this restriction depressed the property values by 30% in the most impacted neighborhoods. Interestingly,

the author also highlights the potential spillover effect on adjacent neighborhoods suggesting that restriction policies can have unintended effects for those who live close to the regulated areas.

Literature has clearly established the negative impact of STR accomodations on housing and the rental market, legitimating the need to regulate the development of these new forms of rentals. Less is known about the form that the regulation should take to be efficient and achieve the targeted objectives. The context of Bordeaux's willingness to control the development of STR and limit their negative consequences by implementing an original compensation rule provides an unique opportunity to evaluate the causal impact of the regulation.

1.1 The case of Bordeaux

Bordeaux is the 9th largest city of France with over 260,000 inhabitants in 2020, and a metropolitan area that gathers 1 million people. Known worldwide for its wine industry, the city of Bordeaux also benefits from a mild climate, is near the Atlantic ocean and offers a renowned gastronomy. According to official statistics of the city, in 2019, 6.35 million nights had been officially rented (in hotels and in registered STR). This figure has steadily increased over the last few years, suggesting that domestic but also international demand for STR has been growing in recent times.

This however comes along with various negative externalities as mentioned earlier. Bordeaux's response to the STR phenomenon was relatively quick as it was the second French city after Paris to adopt laws limiting it.

The motivations behind the need to regulate is summed up by the July 10th 2017 deliberation of the city council[1], which considers five main issues and negative externalities that are created by the development of STR.

1) Transformation of some neighborhoods into exclusively touristic areas with a disappearance of community life associated to a degradation of social capital ;
2) High pressure on housing prices because of the high profitability of STRs ;
3) Degradation of properties and shared spaces because of the high rotation of guests ;
4) Competition with traditional tourism accommodation ;
5) Airbnb is the only platform to automatically collect visitor's taxes and there is a loss in tax for the city ;

French law already addressed the issue and has made it mandatory for hosts to declare their listings to city councils for taxation purposes, thereby obtaining registration numbers that authorized them to lease their properties on STR platforms. On July 10th 2017, the city of Bordeaux released a deliberation regarding the declaration and registration of STRs (Bordeaux, 2017). The city, with its deliberation, decided that it would no longer authorize property owners to change the use of their dwelling from "meant for habitation" to "furnished tourist accommodation" unless they abided by extremely strict rules.

For new changes in use, the rules in Bordeaux include the need to "compensate" by purchasing another property of a similar size and in the same area that was until then used for commercial purposes. Hosts that want a change in use of their listing would then need to transform their newly-acquired commercial premise into a long-term habitation.

[1] http://www.bordeaux.fr/images/ebx/fr/CM/12461/12/acteCM/69313/pieceJointeSpec/148521/file/acte_00046189_D.pdf

The new regulations were officially enforced in Bordeaux on March 1st, 2018. It is important to note that existing listings at the time regulation was announced have had time to register for changes in use without the need for compensation. The new rules are not designed in a way that will phase commercial listings out but they will limit new entries. Similarly to Paris, public agents were hired to track down illegal listings. On January 1st, 2019, Airbnb showed willingness to cooperate as it announced that listings declared as primary residences in Bordeaux would be blocked after having been rented out for 120 days in a calendar year[2]. Together with that came laws making it easier to hand out fines to both hosts and platforms that would not respect the rules[3]. **Figure 1** serves as a recapitulative timeline for the evolution of STR regulation and enforcement in Bordeaux.

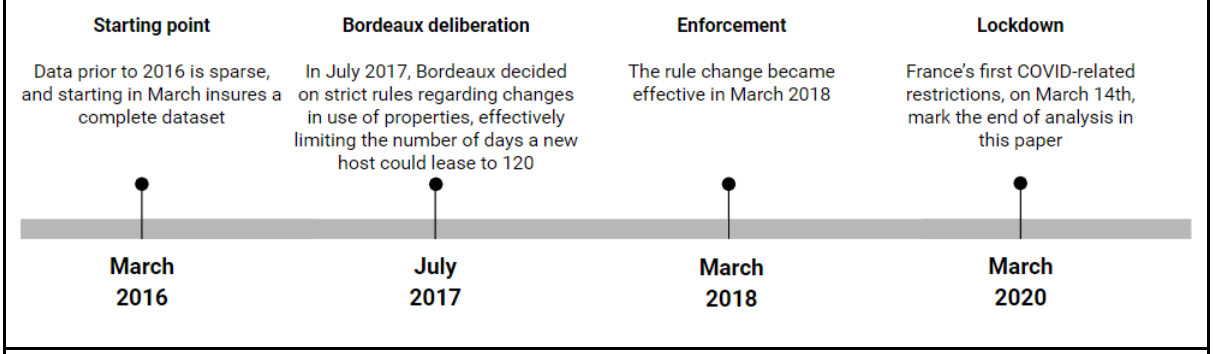

Figure 1 : Timeline of regulatory events in Bordeaux, France, and of this paper's analysis

1.3 Expected effects of the regulation

The compensation rule is designed to provide different incentives according to the intensity of the short-term rental's usage. We define type 1 (T1-non targeted) hosts those who live in their dwelling and rent it less than 120 days per year as well as those who rent only a part of their habitation,for them nothing "should" change after the regulation. As described earlier they are not the target of the regulators. However, It would be naive to believe that this new regulation has been perfectly understood and integrated by hosts who have very different objectives and face various economic and social contexts. Some could have believed that they had to stop renting their home on *Airbnb* even if they were not affected by the regulation while others could have not been aware of the risk taken in renting more than 120 days and thus capitalized on the success of their accomodation and increased their activity regardless of the new regulation.

[2] This agreement was established together with other cities and negotiated on a national level, which possibly alleviated the lack of bargaining power medium-sized cities can sometimes have with leading platforms https://immobilier.lefigaro.fr/article/airbnb-vous-ne-pouvez-plus-louer-plus-de-120-jours-par-an-a-paris_47a64ea6-0cda-11e9-ba7e-e142f78c8ed1/

[3] Up to 10.000€ for hosts and 50.000€ for platforms per non compliant listing https://www.legifrance.gouv.fr/eli/loi/2018/11/23/TERL1805474L/jo/texte

For the renters defined as type 2 (T2-targeted), the compensation "rule" applies. Those who live in their habitation and rent more than 120 days have to decrease the number of days, but those who rent a habitation where they don't live have to stop doing it or make the costly compensation. The expected decrease in the number of reservation nights in STR could be the consequence of the diminution of days available for rental but also of listings who exit the market because they became illegal or because renting one's primary residence less than 120 days a year is no longer profitable.

However their willingness to conform to the compensation rule is directly related to the ability of the local government to enforce the regulation. It is also, as exposed previously, conditional to the fact that the renters have not understood that between the announcement of the rule and its effective enforcement, a period of 8 months enabled them to change their dwelling from "meant for habitation" to "furnished tourist accommodation" without the need to compensate. This possibility even if not advertised could have lowered the effect of the compensation rule and the expected decrease in T2 renters after the regulation.

A last category of renters has to be considered, those who entered the market after the regulation (T3-entrant). The way regulation could have changed the behaviour of entrants is twofold. First a clear and transparent legal framework could favour new entry of hosts because they now know better what is legal or what is not reassuring those who were reluctant to rent their accomodation. The second explanation is driven by market dynamics. As the targeted renters, who are by definition those with the most important STR activities, leave the market or decrease the days reserved, they give extra opportunities for new "legal" entrants. At a constant demand and if the entry is costless it could be that the number of new entrants compensates "at most" the exit of T2 renters.

From the regulator perspective two non contradictory objectives seem to coexist. The first is a global decrease in the STR activities. Less STR reservations could directly address the 5 issues raised by the deliberation of July 2017. In term of the framework discussed above, the magnitude of the effect can be described as follows:

$$\Delta STR = \Delta T1 + \Delta T2 + T3 \leq 0$$
with $\Delta T1 \leq 0$; $\Delta T2 < 0$ and $0 \leq T3 \leq |\Delta T2|$

The second is a change in the composition of the STR supply which would be only composed of renters who live in their accomodations and rent it less than 120 days a year. This second objective should prevent some neighborhoods to be entirely dedicated to STR accommodations (so-called *Airbnb* clusters) and release the pressure on the residential rental market. The ratio of $\Delta T2/\Delta T1$ can evaluate this change in the nature of STR supply.

It is also worth noting that as the regulation only impacts the city of Bordeaux, some spatial spillovers with cities in the suburb of Bordeaux could exist. The discontinuity created by this localized regulation can create opportunities for renters outside Bordeaux but sufficiently closed to the border to attract the demand possibly left vacant by Bordeaux. In this regard the success of the compensation rule in Bordeaux could be misleading from a more global perspective. At least the existence of potential spillovers coud question the way dwellers in areas where the regulation does not apply change their behavior. The aim of the following empirical strategy is to evaluate the overall causal impact of this regulation and disentangle the different effects at stake. To this aim we propose two different and complementary empirical designs to assess the effect of the STR regulation in Bordeaux.

2 - Empirical strategy

The uniqueness of Bordeaux's situation both within its urban area and within its region creates an opportunity for a natural experiment setting that can help evaluate the causal impact of regulation.

Firstly, a Differences-in-differences (DID) design will allow for an estimation of the aggregate net effect of regulation on activity in Bordeaux. The DID specification is indeed particularly useful when attempting to identify the global causal effect of a given policy. This global causal effect is for a large part driven by touristy neighborhoods in the city center. However, in addition to its strong assumptions, it falls somewhat short in measuring effect heterogeneity across space. It also deliberately works around the effects regulation can have on non-treated areas by selecting control cities that are far enough not to be affected by regulation.

This issue is addressed by the spatial discontinuity design (SD), our second specification. This method enables discussion over the existence of potential spillovers and provides further robustness to our original results by checking for their validity in peripheral areas of the city. It is important for regulation to be effective on the city's outskirts as these areas might experience disproportionate losses from STR activity given their residential structure. Additionally, the SD can provide further insight on host behaviour through an experiment at a per-listing level. This differentiates it from the DID, where districts are the observed units.

2.1 Data

While Murray Cox's *Inside Airbnb*[4] project provides free to use open source *Airbnb* listing data and has been used in studies relating to the housing market, its dataset would not be adapted to our designs as the only city it covers in the *Nouvelle-Aquitaine* region is Bordeaux. That is why this paper relies on STR listing and activity data acquired from specialized American company *AirDNA*, another standard for research in the field. The data includes monthly web scraps beginning in 2016 of all *Nouvelle-Aquitaine* listings from both *Airbnb* and *HomeAway*. For every listing in every month for which it is active, the dataset provides a number of variables relevant to our research. The number of nights that were booked in a given month, which will further be referred to as "Reservation days", is an indication of the influx of tourists. It is the dominant variable behind the STR-related problems raised earlier in this paper and the one upon which policy-makers have imposed a limit. The number of days for which a listing is available for rental, regardless of whether it is rented or not, is also at our disposal. Additionally we use information about the spatial coordinates of listings, their price, and their type. The "type" variable qualifies the listing as an entire home, a shared room, a private room or any other category of accommodation. In the DID setting, the aforementioned data is aggregated at the district level. This allows us to introduce covariate district[5]-level structural data[6] from the French national institute for statistical and economic studies[7].

[4] http://insideairbnb.com/

[5] IRIS are "aggregated units for statistical information", their aim is to divide cities into units of about two thousand inhabitants.

[6]
- 2016 census for the number and the share of primary residences
- 2016 "Filosofi" scheme for median available income
- 2018 facilities database ("Base permanente des équipements") for the number of restaurants

[7] Institut National de la Statistique et des Études Économiques, INSEE hereafter

The time window of our observed data closes at the beginning of March 2020 because of the strong heterogeneous impacts the COVID-19 pandemic along with its restrictions might have had on our observed units.

To evaluate the impact of the compensation rule on the different categories of renters, we attempt to differentiate the listings that are presumably targeted by the regulator from those that are not. It is clear that the rule intends to impose a very important cost to those whose listing is not used as anyone's primary residence. Regulation discriminates against the latter through the number of reservation days. This implies that a listing rented on STR platforms for over 120 days in a calendar year cannot be anyone's primary residence. We extend this principle to any day a listing is made available for rental and drop the need for calendar years. As such, a listing is defined as *targeted* (Type 2 - host) if it is an entire home that is at least available for ten days per month on average during its stay on the market. This does not follow the way regulation is enforced and is more comprehensive, as reservation days are the regulation's only observed variable, but it is a fair representation of the policy-makers' intention to cut down on non-primary residence listings. **Table 1** shows descriptive statistics for both categories of listings in Bordeaux, both before and after regulation took place. At first glance, both categories seem to have grown in quantity and in activity over time, with a stronger growth for non-targeted listings. However we have to keep in mind that this global increase is an average over a two-year period, it is thus possible that the STR activity varies along the post-regulation period.

Table 1 : Summary statistics of Bordeaux's listings according to whether we define them as targeted or not

		March 2016 to February 2018 (Pre-regulation)				**March 2018 to February 2020 (Post-regulation)**			
		N	*Mean*	*St. Dev.*	*Median*	*N*	*Mean*	*St. Dev.*	*Median*
Reservation days	Targeted	10082	97.54	113.84	56	12387	113.07	145.83	58
	Not Targeted	3869	71.36	94.70	36	4859	90.34	130.48	42

2.2 Differences-in-differences

2.2.1 Identification

Bordeaux's regulatory intervention is unique within its region, *Nouvelle-Aquitaine*. A differences-in-differences (DID) approach allows us to evaluate the causal impact of the intervention on the STR market by using other cities as reference points for what the evolution would have been without treatment, i.e. without the regulation. This research design is an efficient way of observing the aggregate impact of treatment. The most adequate dependent variable at hand for measuring the strain of STR activity on long term residents is the aggregate number of nights spent by guests in STR accommodation. The model is specified as

$$Y_{i,j,t} = \alpha + ß^{Treatment} D_j + ß^{Period} T_t + ß^{Effect} T_t D_j + ß^{Month} M_t + ß^{City} C_j + ß^{STR} X_i + ß^{Structure} Z_i + \varepsilon_{i,j,t}$$

where $Y_{i,j,t}$ is the number of reservation nights for any given district *i* in city *j* during month *m*. T_t is equal to 1 for any observation made after enforcement, i.e. if $t \geq 2018/03/01$, and to 0 otherwise. D_j is our treatment dummy. It is equal to 1 if city *j* is Bordeaux and to 0 otherwise, and the coefficient $ß^{Effect}$ of its interaction with T_t is the Differences-in-differences estimator. M_t and C_j are dummy vectors used to control for month-specific fixed effects and city-specific fixed effects respectively. Performing the analysis at the more granular district level *i* rather than only at the city level *j* allows us to better control for structural diversity in the dataset through vectors *X* and *Z*. *X* incorporates STR market specifics, namely the share of Entire Home listings in the district and the average nightly rate of listings. Only active listings are considered for these covariates. The number of restaurants per inhabitant and the median level of income are structural characteristics that are taken into account by *Z*. They are fixed in time. In order to avoid downward bias of standard errors because of serial correlation, standard errors are clustered at the city level.

This research design relies on a few key assumptions. The first is that without treatment, the difference in activity on the STR market between treated and control units would have remained constant over time. To validate this assumption, the effect of the underlying differences between the treated and control units has to be constant over time. Minimizing these differences is key when building a control group. Bordeaux is however a rather unique city in the *Nouvelle-Aquitaine* region as it is both the biggest and the most economically diverse. This makes the prospect of finding a control group that fulfills the parallel trends assumption challenging. The four cities we have selected to be a part of the control group are Anglet, Bayonne, Biarritz and La Rochelle. They all host important STR markets and range from being tourism-intensive (Biarritz) to being economically diverse (La Rochelle). All of them offer a proximity to the Atlantic ocean's seashore, as shown on the map in **Annex 1**, and they are all part of the same administrative region : Nouvelle Aquitaine. These cities are also used to simulate a random assignment for treatment. **Annex 2** offers a representation of the districts analyzed in the DID both in the treated and the control group

Figure 1 depicts the six-month rolling mean number of nights spent in STR accommodation over time in Bordeaux's (treated) districts and in other cities' (control) districts. Treatment districts are less sensitive to seasonal peaks but also record more activity in the winter. Control group districts experience growth in both peak activity and off-season activity even after the treatment, whereas the monthly number of booked nights in Bordeaux gradually declines over time once regulation is enforced. This is consistent with the expected effect of a regulation that blocks new targeted hosts from entering. Robustness checks through variations in the control group composition reinforce the idea that our results do not depend strongly on the observed structural differences between treated and untreated units. The potential influence of anticipation effects after the announcement will also be checked for with a double-DID design (cf. **Annex 3**).

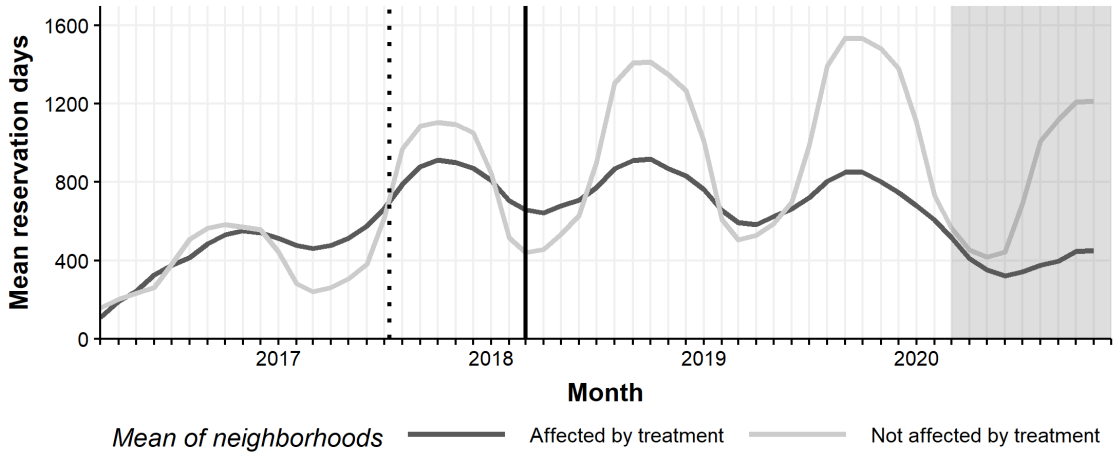

Figure 1 : Mean number of reservation days per month for treated (Bordeaux) and control districts. 6-month rolling averages are used to attenuate seasonality. The dotted vertical line indicates the announcement of regulation in Bordeaux (2017-07-10) while the straight vertical line represents regulation enforcement (2018-03-01). The grey area indicates the beginning of COVID-related travel restrictions.

Another concern in DID settings is the threat to the Stable Unit Treatment Value Assumption (SUTVA). This is the assumption that assignment of treatment has not had any effect on non-treated units over time. A violation of this assumption would invalidate the interpretation. In our setting, this would mean the lack of STR supply in Bordeaux has had a positive spillover effect on other cities. The uniqueness of Bordeaux coupled and its distance from the control group cities help validate the SUTVA as it is credible that the city is not directly competing with the seaside resorts from the control group for tourism. As such, spillover effects from treatment units to control units don't seem to threaten the validity of our results, and while spillovers are likely within the control group between Anglet, Bayonne and Biarritz, these do not threaten the assumption's validity.

2.2.2 Descriptives

As noted in the previous subsection, and as with most experiments, the DiD setting requires the control group and the treated group to be structurally homogeneous. Heterogeneous units across groups would make it difficult to attribute any effect to regulation rather than different evolution for different city types. This is a naturally difficult problem to solve. Some control group observations might be a bit more rural than any unit one could find in Bordeaux and slightly skew some of the statistics, while other districts could be more tourism-intensive than the center of Bordeaux. As shown in **Table 2**, the 76 districts from the control group are reasonably similar on average to Bordeaux's 88 districts both structurally and in terms of STR activity prior to regulation. Bordeaux is both slightly richer and slightly more populated by primary residents on average than the coastal cities, but the effect of such differences would not be significant enough to alleviate the results produced by the regressions. Treated districts are diverse in terms of STR activity, as estimated by the standard deviation of reservation days, but the same can be said about control group units. This is consistent with the idea that STR accommodation tends to be organized in hubs around cities, near tourist landmarks and in trendy neighborhoods.

Table 2 : Monthly STR activity and structural summary statistics of treatment (Bordeaux) and control group (Anglet, Bayonne, Biarritz, La Rochelle) districts, from March 2016 to March 2018 (Before regulation)

	Bordeaux districts				**Control group districts**			
	N	*Mean*	*St. Dev.*	*Median*	*N*	*Mean*	*St. Dev.*	*Median*
Reservation days	88	596.34	569.82	439.10	76	567.80	660.08	304.54
Share of Targeted listings	88	0.68	0.13	0.69	76	0.74	0.17	0.76
% Primary residences	88	0.89	0.04	0.90	76	0.81	0.18	0.87
Median income (in euros)	88	22284.35	4756.86	22402	76	20976.96	3696.19	21016

2.3 Spatial Discontinuity design

2.3.1 Identification

A Spatial regression Discontinuity (SD hereafter) design is employed along with the DID in part to make up for the latter's shortcomings. SD designs share the principles of traditional regression discontinuity designs but they use space as the running variable. This second approach will not allow us to measure the aggregate effect of regulation but it can provide further evidence of causal impact and therefore strengthen the observations made in the DID setting. It also allows us to make observations at the listing level rather than at the district level and thus to better differentiate the effect of regulation across hosts. The fact that this design relies on observations close to the border yields results that hold for peripheral less-tourism oriented neighborhoods, which could be disproportionately threatened by spikes in touristic activity.

The key assumption behind this design is that dwellings on either side of the border are not fundamentally different. If they are close enough to one another, their side of the border is initially not what would determine their probability to be listed on home-sharing platforms or to receive many guests. As such, a difference in activity of listings that depends on their side of the border would be evidence of a causal effect of regulation. The loess fit plots on **Figure 2** are encouraging in regard to this method as they depict a jump in reservation days per listing at the border after the regulation.

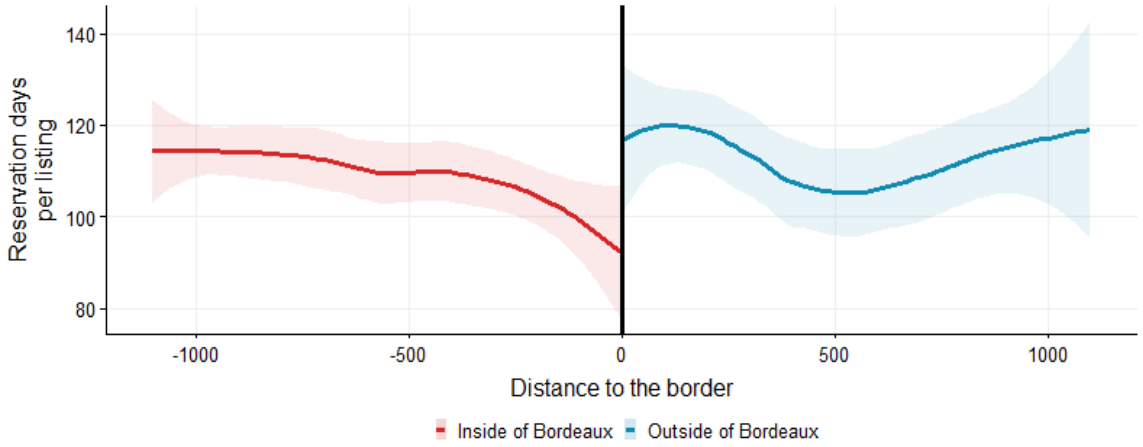

Figure 2 : Locally estimated scatterplot smoothing fits of reservation days per listing according to distance (in meters) on either side of the border, and their corresponding confidence intervals, for observations between March 2018 and February 2020 (i.e. after the regulation).

Treatment effects in this design are estimated using methods proposed by Calonico et al., (2018) and Calonico et al., (2014) in a local-linear setting. As such, and with the same dependent variable as in the DID setting, the weighted least square regression is simplified as

$$Y_k = \alpha + T_k \tau + X_k \beta^- + T_k X_k \beta^+ + Z'_k \gamma$$

where Y_k is the observed number of reservation days for every listing k in any post-regulation month. Z_k denotes unit covariates, namely whether or not the listing is an entire home listing and the proportion of primary residences in the listing's district. The distance to Bordeaux's border is denoted by X_k. It is negative if the listing is inside the city and positive otherwise. As such, the treatment cutoff occurs at $X = 0$ and for any observation where $X_k < 0$, treatment value T_k will be equal to 0. This is the opposite of usual treatment dummy assignment but will make interpretation more intuitive without affecting the estimation of the Local Average Treatment Effect (LATE) τ. Weights for every unit are determined by a triangular kernel. Regressions will be made over a bandwidth h, expressed in meters. Observations are only used in the regression if they are located within the bandwidth, i.e. if $-h < Xk < h$ for any listing i. The exclusion of data induced by bandwidth selection makes it a crucial part of a robust RDD. Tests with different bandwidths will therefore be a part of the robustness checks. The default bandwidth selection method employed in this paper is the Calonico et al., (2014) modification of the Imbens and Kalyanaraman (2012) method using a data-driven MSE-optimizing selector.

One of the main drawbacks of regression discontinuity designs is their reliance on continuous assumptions. Both reservation days per listing and the appearance of listings should be continuous across space regardless of the border. The spread of listings across space within a 1-kilometer radius of the border appears to be reasonably even given restrictions such as parks, universities or buildings of commercial use, as shown in **Figure 3**. The running variable is distance to the border, and it is not at risk of manipulation. That is to say a host cannot simply change the location of her listing in response to regulation. While the fact that treatment assignment is known 8 months in advance could lead hosts to alter their behaviour prior to our period of study, this would not be a threat to validity as

regulation would still have had a causal effect on their activity. The dependent variable in the regressions also has to be continuously distributed and reservation days per listing should not see any sharp spike or decline at the cutoff in the absence of regulation. That is to say being in Bordeaux does not make a dwelling different. This is something that could be questioned because of the attractiveness to tourists of a name like *Bordeaux* when searching for places to stay. However, placebo tests of the dependent variable during the two years leading to regulation show no significant difference in activity per listing on one side and another of the border. The continuous assumption is therefore valid (Specification (3) of **Annex 4**). It should be noted that the treatment effect observed in the SD consciously includes potential spillover effects on neighbors in addition to the negative impact of regulation on treated units.

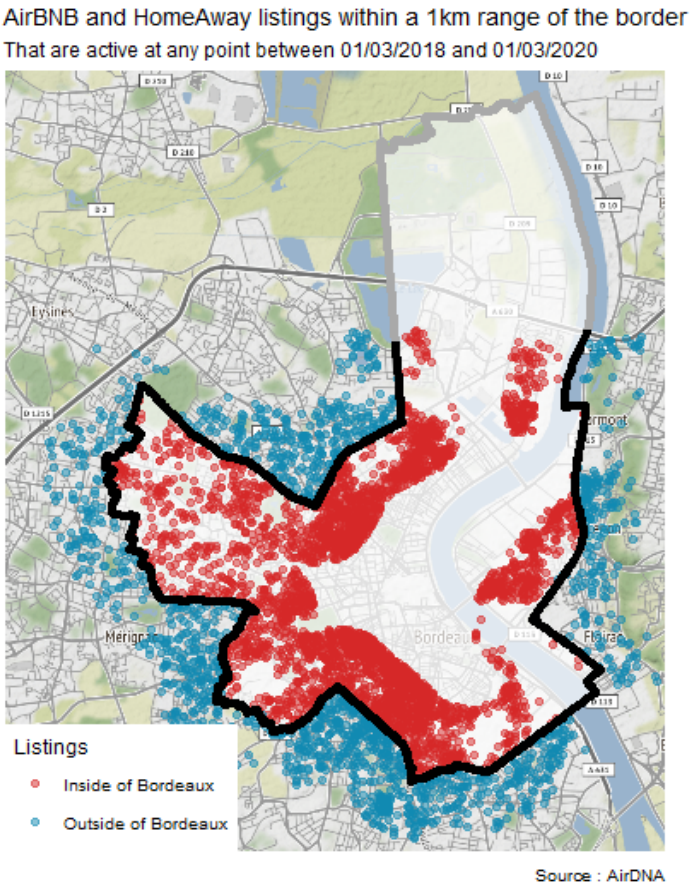

Figure 3 : *Airbnb* and *HomeAway* listings in Bordeaux and its close surroundings, within one kilometer of the city's border The black part of the border is a part of the analysis, whereas the grey part is excluded because of the lack of observations around it.

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2.3.2 Descriptives

Summary listing-level statistics for *Airbnb* and *HomeAway* listings within a 500-meter radius of Bordeaux's border for the two years preceding regulation and for the two years following it are presented in **Table 3**. These summary statistics are relevant to the spatial RDD. First of all, the mean number of reservation days is comparable across the border in the first time frame. This is consistent with the continuous assumption upon which the RDD relies.

Also, and despite regulation, per-listing activity seems to have grown on either side of the border within the 500-meter bandwidth in the second two-year window. This is to be expected given the fact that STR tourism was still growing and a number of listings would only have been active after 2017. It also serves as a reminder that regulation in Bordeaux was never meant to completely stop STR, rather to slow the part of its growth that was due to targeted listings. Both the growth of mean per-listing reservation days and of the number of observations appears to have more momentum in areas adjacent to Bordeaux.

Table 3 : Relevant STR activity summary statistics of listings located in a 500-meter radius of Bordeaux's border.

		March 2016 to February 2018 (Pre-regulation)				**March 2018 to February 2020 (Post-regulation)**			
		N	*Mean*	*St. Dev.*	*Median*	*N*	*Mean*	*St. Dev.*	*Median*
Reservation days	Inside Bordeaux	1600	86.45	103.09	46	1803	105.78	120.00	60
	Outside Bordeaux	1026	89.22	103.48	50	1459	114.43	124.63	70

3. Results

Differences-in-differences

The main findings of the DID regressions are produced in Table 4. Regulation in Bordeaux has, on average for any given district, had a strong negative impact on reservation days. Results are robust to covariate introduction both in terms of significance and in terms of scale, as shown in the preferred regression for this variable (2). Its DID estimator coefficient yields a net negative effect of 316 days. On average, for any Bordeaux district, regulation has had a negative impact of over half of a pre-regulation standard deviation on the number of nights spent per month over the two years following treatment. Over Bordeaux's 88 districts, regulation has had a cumulative impact of 27808 reservation days per month according to the DID estimation.

Table 4 : Differences-in-differences regressions results

	Reservation days				*Number of new entrants*	*Share of Targeted listings*
	(1)	(2)	(3) Targeted	(4) Not Targeted	(5)	(6)
After Enforcement	570.52*** (157.69)	508.96** (160.50)	517.41*** (147.19)	56.89** (7.29)	−33.458*** (8.082)	0.054*** (0.016)
Treatment units	109.89 (62.56)	−66.69 (70.48)	−119.73* (69.3)	21.37 (33.69)	−1.181 (0.638)	−0.047*** (0.004)
After Enforcement x Treatment units	−303.97* (124.64)	−316.01* (123.07)	−315.78* (124.12)	−19.20* (7.81)	−0.369 (1.130)	−0.025*** (0.001)
City FE	x	x	x	x	x	x
Month FE	x	x	x	x	x	x
STR market controls		x	/	x	x	/
district controls		x	x	x	x	x
N	7845	7618	7562	7418	7618	7618
Adjusted R^2	0.26	0.52	0.52	0.39	0.40	0.36
F-Statistic	51.51*** (df=52; 7792)	149.36*** (df=56; 7561)	148.30*** (df=55; 7506)	85.49*** (df=56; 7361)	90.85*** (df=56; 7561)	79.99*** (df=56; 7562)

*Note : * = p < 0.05 ; ** = p < 0.01 ; *** = p < 0.001*

Reading (2) : A district that was affected by treatment receives 316.01 less reservation days per month on average than one that was not affected, in the two years following the treatment.

STR Market control covariates in regressions (3) and (6) exclude the share of entire home listings, as all targeted listings are entire homes by definition

Regressions (3) and (4) operate on partial datasets only considering the activity of targeted listings in (3) and from non-targeted listings in (4) respectively. The significance and the scale of DID estimator's coefficient in (3) are expected and they underline how the aggregate net effect of regulation is driven by targeted listing. Regulation however seems to have also led to a dip in the activity of listings that otherwise serve as primary residences, as it has had a negative effect on non-targeted listings, albeit at a lower scale of about a seventh of a standard deviation. This goes against the hypothesis that unwanted activity might be compensated by home-sharing activity.

Regulation, by design, is not meant to kick out type-2 hosts. However, new entrants are no longer allowed to be type-2. Thus, it could have been expected for the number of new entrants on the market to be reduced, especially given the fact that non-targeted listings do not compensate for the loss in activity (4). The estimator in (5) is however not significant at a 5% confidence level, and the number of type-3 hosts in Bordeaux is therefore stable following regulation in comparison to control group cities. Further regressions showcased in the Annex 3 show that neither the number of entries of targeted listings nor that non-targeted listings have been affected in isolation.

Additionally, regression (6) yields results that are consistent in significance with our expectations. Regulation has had a negative impact on the share of active targeted listings (Type-2/Type-1) of about 2.5 percentage points on average, for any district in any given month.

These results are also robust in scale and in significance to changes in the control group, to anticipation effects and to COVID-related changes. More detail on these different robustness checks can be found in the Annex 3.

Spatial discontinuity design

The results yielded by the SD design show strong evidence of an effect of regulation on reservation days per listing in the city's peripherals, as shown in **Table 5**. Specification (2) is the most general form and is the preferred one when it comes to measuring the simultaneous effect of regulation on listing activity and on entry and exits. This regression is timeless and as such, each listing is a single observation with its number of reservation days being a sum over the two years regardless of how long it was on the market for during those two years. The effect of regulation appears to be weaker than in the DID setting as it implies a change of about a third of a pre-regulation standard deviation.

The LATE observed in specification (3) equates to a fifth of a standard deviation. For any given post-regulation month where a listing from the bandwidth is active, it will have rented 2.14 days fewer in Bordeaux than outside of Bordeaux. It is a monthly average effect that is net of inactivity of the listing. The listing only has an observation in a given month if it was active, i.e. rentable, for at least one day, and its absence of the market for a given month does not affect the LATE. It incorporates possible differences in demand across the border as well as differences in the behaviour of active suppliers on the STR market.

Specifications (4) and (5) attempt to capture effects that are not measured by specification(3), namely the dynamics of entry and exit from the STR market. (4) only includes listings that entered the market before March 2018. Listings outside of Bordeaux within the given bandwidths have exited the market at a lower rate than treatment units have over the observed time period, by 18 percentage points. Listings are considered to have exited the market if their last active month, the last month in which they made their listing available for rental, is in November 2019 or earlier. The probability of being an entrant measures, on aggregate, the share of new listings within the bandwidth. It is higher by 8.8 percentage points outside of Bordeaux .

Table 5 : Spatial regression discontinuity results

	Reservation days per listing		*Monthly reservation days per listing*	*Probability of exit*	*Probability of being an entrant*
	(1)	(2)	(3)	*(4)*	(5)
Local average treatment effect	26.742** [10.486 , 47.439]	29.528*** [13.295 , 50.118]	2.137*** [1.770 , 2.609]	−0.180*** [−0.287 , −0.098]	0.088** [0.027 , 0.153]
Listing type		x	x	x	x
Share of primary residencies		x	x	x	x
N Bordeaux	2176	2204	19530	1773	4328
N Control	1634	1652	15749	976	2467
Bandwidth	582.614	589.896	480.145	728.662	790.324

*Note : * = p < 0.05 ; ** = p < 0.01 ; *** = p < 0.001*

Reading (2) : On average and within a 590-meter radius of Bordeaux's border, a listing outside of Bordeaux will have accumulated 29.5 more reservation days than a listing inside of Bordeaux during the two years following regulation.

Table 6 presents attempts at nuancing the previous results by regressing on selected categories of hosts. As a reminder, regulation in Bordeaux was targeted at hosts that rented their dwelling on a STR platform enough for it not to be considered their primary residence anymore. Targeted listings as presented in **Table 6** are entire home listings that are active for at least 10 days per month on average (120 days a year). Since targeted listings are exclusively entire homes, the listing type covariate has to be dropped in the corresponding regressions. The regressions otherwise follow the same specifications as those in **Table 5**. The only situation in which no significance is established is the probability of being an entrant for non-targeted listings. This means that the share of new entrants among listings that are otherwise primary residences is comparable on both sides of the border.

Other than for specification (8), regulation appears to have affected the behaviour and the activity of listings regardless of if they were primary residences or not. Interestingly, the difference in activity and in the exit rate is even higher for home-sharing listings than for commercial-orientated listings. This is obviously not an effect that was anticipated by regulators, and it will be discussed along with our other results in the following section.

Table 6 : Further Spatial regression discontinuity results

	Reservation Days per listing		*Monthly reservation days per listing*		*Probability of having exited*		*Probability of being an entrant*	
	(1) Targeted	*(2)* Not Targeted	*(3)* Targeted	*(4)* Not Targeted	(5) Targeted	(6) Not Targeted	(7) Targeted	(8) Not Targeted
Local average treatment effect	29.602** [10.970 , 53.855]	37.793*** [18.216 , 62.781]	1.460*** [1.145 , 1.917]	2.949*** [2.526 , 3.635]	−0.163** [−0.291 , −0.051]	−0.181** [−0.341 , −0.063]	0.096** [0.029 , 0.168]	0.091 [-0.006 , 0.210]
Listing type		x		x		x		x
Share of primary residencies	x	x	x	x	x	x	x	x
N Bordeaux	1815	1157	18820	7718	1275	781	4402	1138
N Control	1188	831	14276	6779	600	448	1746	826
Bandwidth	585.167	616.468	433.428	277.000	815.616	805.608	1041.130	609.952

*Note : * = p < 0.05 ; ** = p < 0.01 ; *** = p < 0.001*

Reading (2) : On average and within a 543-meter radius of Bordeaux's border, a non-targeted listing outside of Bordeaux will have accumulated 2.95 more reservation days per month more than a listing inside of Bordeaux during the two years following regulation.

A placebo test of aggregate reservation days over the two years preceding regulation can be found in the **Annex 4** (Specification (3)) along with other checks. These include COVID-related time variation, a regression on the average stay, isolation of entire home listings, the use of a local-quadratic estimator instead of the local-linear one used in the models above, as well as modifications of the bandwidth.

4. Discussion

The results presented in the previous sections have multiple implications. First of all, both models point towards a decline in the number of reservation days induced by regulation. Reservation days are relevant to a number of objectives set by the city and results are thus consistent with these objectives. The DiD setting points towards an aggregate impact across the city of just over half of a pre-regulation standard deviation. The fact that the effect is still significant in scale at the border, as estimated in the SD, is also paramount given the structural nature of peripheral neighborhoods and their propensity to be transformed by high numbers of short term stayers. If reducing the number of reservation days was the sole criterion for evaluating regulation in Bordeaux, then it would have to be deemed a success and the net effect would be negative. The share of T2 over T1 accomodations is the other dependent variable we elected to analyze the structural change in the composition of accomodations in the city of Bordeaux. A decrease in this ratio suggests that a higher proportion of accommodations are available for residential dwellers which is a clear expectation of this regulatory attempt. In some sense, some *Airbnb* tourism clusters could have deflated, or regulation has limited a potential expansion of the tourism bubble induced by STR (Ioannides et al., 2019). On aggregate across the city, the share of T2 accomodations has decreased by 2.5 percentage points, which suggests that the regulation works even if the magnitude of this effect remains low. As explained in section 1.2, the

compensation rule mainly targets new entrants and also allowed for a period of 8 months during which the owners of T2 accommodations could freely change the official use of their dwellings. This result sheds light on the importance of the design and the timing of the regulation as it could strongly impact its effect. Listings adjacent to the border were not affected exactly in the same way those closer to the center were - it could be because they do not host the same types of tourists, or because the center has a higher concentration of listings that are targeted by regulation than the outskirts do.

Secondly, the distinction between targeted and non targeted hosts enables a look at the heterogeneous effects of regulation according to the motivations of the hosts. As a reminder, Bordeaux's regulation was aimed at STR hosts that had a commercial use of their listing, i.e. at listings that did not otherwise serve as the host, or anyone's, primary residence. The effect sought by these discriminatory measures is a reduction in activity exclusively driven by the hosts that are discriminated against, all the while keeping the benefits of home-sharing produced by hosts of primary residences. These benefits include a better use of accommodation resources as well as a better redistribution of tourism-generated revenue across the city. The regulator's decision to protect home-sharing showcases their importance. Results from both the DID and the SD setting however suggest that this implied objective was not exhaustively met, as regulation has had a reductive effect on the activity of non-targeted listings in addition to its impact on targeted listings. To put it simply, hosts who are following the tacit manifesto of the sharing economy (T1) consisting in looking for some additional income and meeting new persons are perhaps penalized. This is especially the case at the periphery of the city where we have shown that while the entry rate of non-targeted listings is not affected, their exit rate has increased and their general activity has decreased. This is contrary to the expected outcome of such regulation, as a de facto ban on commercial STR should create market opportunities for home-sharers and therefore boost their activity or the number or participants.

The designs in this paper do not allow us to establish a firm answer as to why this is not the case, but we have identified different issues this unwanted effect could stem from. Results from the SD, in which we do not observe a change in the share of market entrants, imply that this is not due to a saturation effect. The first plausible explanation is that regulation has had a direct effect on compliant hosts because it is too complicated and bureaucratic. This complexity coupled with the heavy political weight and advertisement of the policy will have made many people aware of its existence without having clear information about how it operates, hence wrongfully encouraging compliant hosts to discontinue their activity. Another reason could be the existence of additional barriers to rental set by other actors, either because they also misunderstood regulation and want to stay out of trouble or because regulation made them more aware of certain problems such as noise disturbance. Co-ownership associations and landlords can explicitly forbid STR activity in a building or a dwelling they rent to a long-term resident that uses STR platforms for home-sharing.

These explanations however fall short when attempting to understand the difference in the scale of impact on non-targeted hosts between the DID setting and the SD setting. On aggregate across the city, the reduction in reservation days of non-targeted hosts was of about one seventh of a standard deviation per neighborhood, which is understandably significantly smaller than the effect on targeted listings. However, on the outskirts, the SD yields results that show non-targeted listings are disproportionately affected by regulation. This brings us to a third point of discussion regarding our results. One of the assumptions we hold in our DID estimation is that control cities are far enough not to have been directly affected by regulation in Bordeaux. This assumption is relaxed in the Spatial

Discontinuity design, and as such, the LATE incorporates both the direct reductive effect of regulation on activity in the city and potential indirect spillover effects of regulation on the surrounding suburbs.

Regulation in Bordeaux has presumably not had a strong effect on the demand for accommodation in the area, only on the supply in the city. With a reasonable assumption of a constant demand for STR, this decrease in supply might have led some tourists to opt for hotels instead of STR options, but it has likely also created market opportunities for adjacent hosts to provide accomodation for the more spatially flexible guests. As a result of regulation, non-targeted hosts in the center might benefit from a slight advantage given the increasing scarcity of targeted hosts on the one hand. On the other hand, non-targeted hosts close to the border operate in a market environment that is more competitive than it used to be because of targeted supply in the center spilling over towards adjacent neighborhoods. This would help explain the effects observed on type 1 (non-targeted) hosts near the border, who are now operating within the same space as the "spilled over" type 2 hosts and therefore face tougher competition. These results also highlight threats posed by such city-wide regulatory efforts. The low spatial scale of the decision appears to push potential problems linked to STR activity slightly further away rather than suppress them. It pushes them onto spaces that are an extension of Bordeaux's urban area and that likely share the problems faced by residential neighborhoods on the city's inner outskirts. To put it simply, the city center exports its externalities related to STR to adjacent neighborhoods. These externalities can be positive when they come along with a spread of income linked to tourism, new dynamism stimulating new localized amenities (restaurants, bakeries, etc) but they can also be negative when nuisance, high rotation of guests, and increases in rent can push away some residents or damage their well-being.

Moreover, affected peripheral cities also share a political space and decision-making process with Bordeaux regarding a variety of fields through the *Métropole*, which is relevant when it comes to urban planning policy-making. This is an institutionalized indication of the common interests Bordeaux has with neighboring cities, and it casts questions over the decision to regulate at the city level given the need for STR policies to be in line with the wider frame of housing and economic policies. It is difficult to make the argument that urban planning is relevant to the *Métropole*'s interests but that STR regulation is only relevant to Bordeaux's interests, as our results show that Bordeaux's regulation has a direct effect on potentially disturbing STR activity in the rest of the *Métropole*. In any case, a uniform and *one-size-fits-all regulation*, both in spatial terms i.e related to geographical characteristics of neighborhood and in behavioural terms, i.e related to type of hosts seems partially efficient to keep the control of urban transformations associated with *Airbnb*-like platforms. Indirect and undesirable effects also stem from this uniformity and a more targeted or *smart-regulation* is probably something that should be designed.

Conclusion

The aim of this paper was to assess the effect of the attempted regulation of short-term rental accomodations in Bordeaux. The so called "compensation rule" provided an opportunity to evaluate the causal impact of the regulation on aggregate in the city but also on the different categories of renters.

Our main result shows that the number of nights spent in a district of Bordeaux has been reduced by 316 nights per month in the post-regulation period. While this effect is mainly driven by the most touristic and central district of the city, we also show using a spatial discontinuity design that a listing in Bordeaux lost 29 days worth of rental as compared to its equivalent outside of Bordeaux. We then

demonstrate that this decrease in the activity of short-term rental is also driven by those that were not “à priori” affected by the regulation, suggesting the existence of non-intended effects in the consequences of the regulation. Finally the compared evolution of short-term rental accommodations supply in Bordeaux’s adjacent cities suggests that the regulation may have had spillover effects, creating new opportunities for hosts outside of the regulated area and thus potentially harming the net effect of the measure. Our research obviously has some limitations, the main one being that the results are place-dependent and contextualized. These results are also time-dependent and sensitive to the local institutions in charge of the regulation design. Further assessments are needed in various contexts and for more cities to understand what the interrelated external effects on the short-term rental market are.

The debate on regulating digital platforms, the sharing economy and their related issues is beyond the scope of this paper but it is still open. It is a concern for many governments, local, national or continental facing many multi-dimensional but interrelated effects (on competition, on geography, on labour, on technology, on mobility, on society as a whole, etc). There is a growing need to design and adapt better regulations (Frenken, et al., 2015). This contribution is a first step towards designing what we suggest as a smart-regulation, considering that a one size fits all approach produces many undesired and ambivalent externalities. It also paves the way for a research agenda that calls for further work on the way planners in charge of urban transformations, digital platforms, local institutions and regulators should go hand in hand for mutual benefits.

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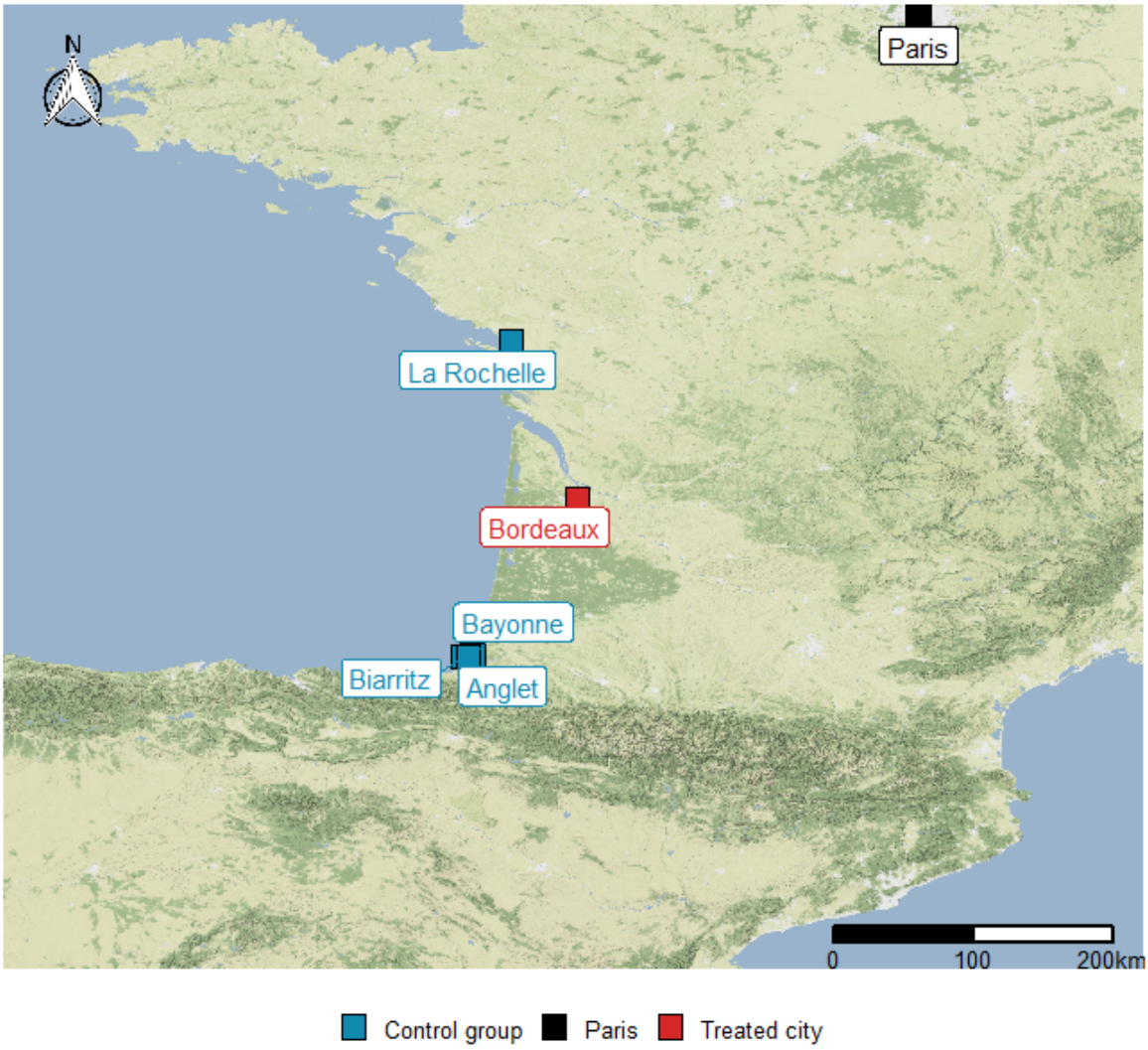
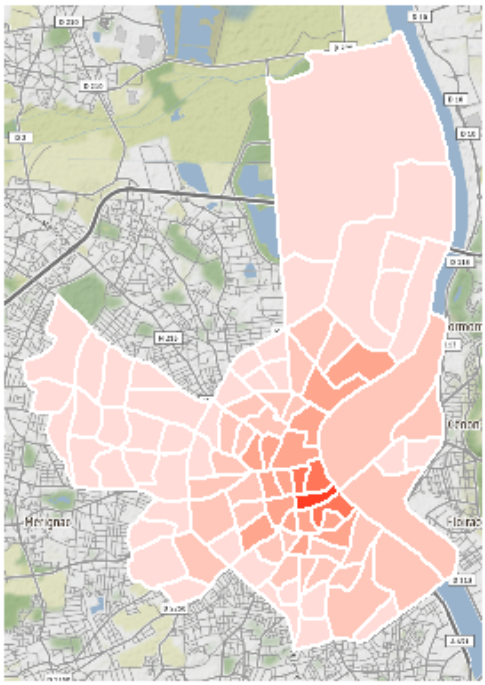
Annex

Annex 1 : Bordeaux and the control group cities are located in sunny and touristic southwestern France.

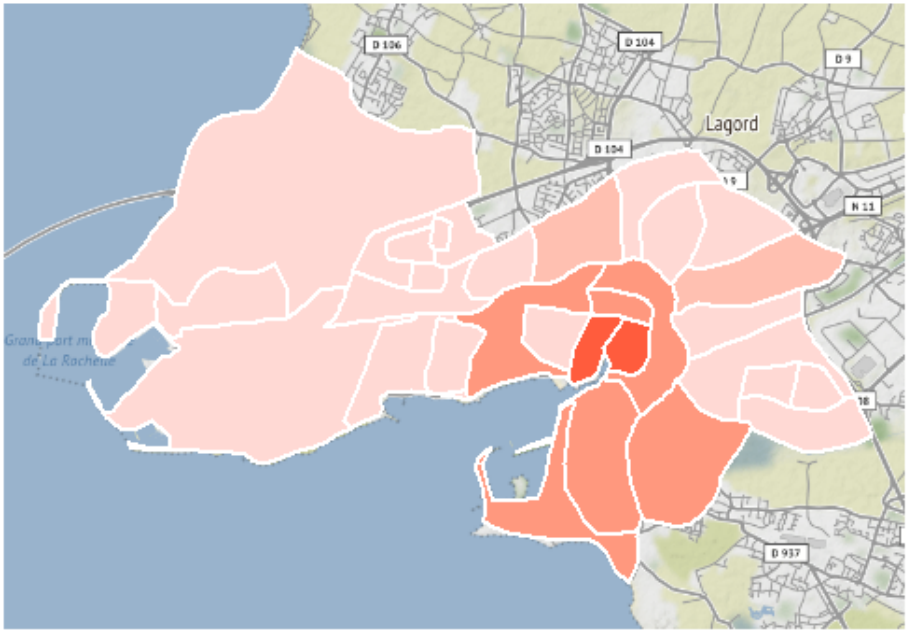


Annex 2 : Districts of Bordeaux and of the control group cities, coloured according to the STR reservation days they hosted in the two years leading up to regulation (March 2016 - March 2018)

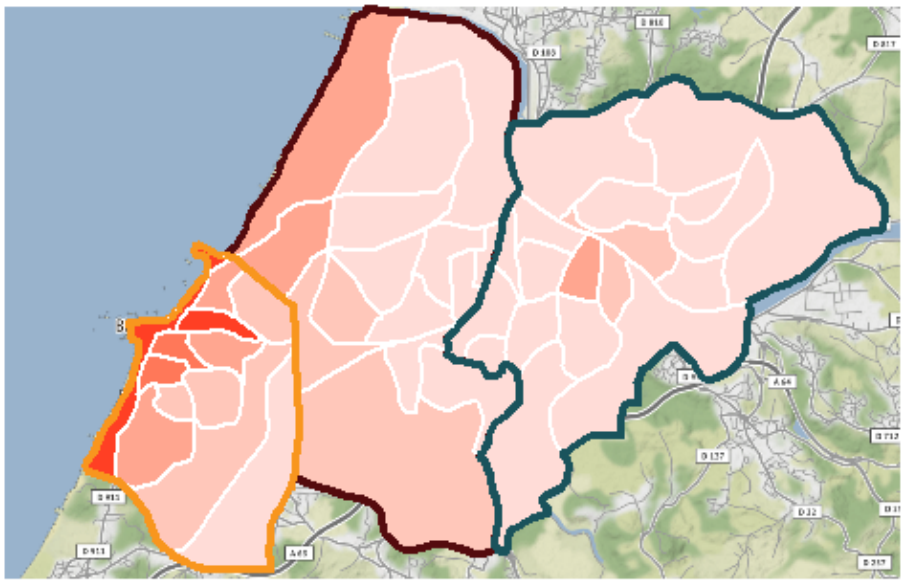
Bordeaux

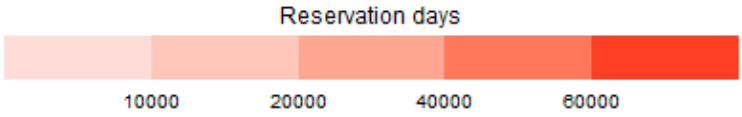


La Rochelle



Biarritz, Anglet and Bayonne





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Annex 3 : Tests regarding the Differences-in-differences results

	Reservation days			Share of targeted listings
	(1) No COVID time restriction	(2) Anticipation effect	(3) Different control group	(4) Dynamic
After Enforcement	278.45 (149.09)	518.50** (174.27)	535.08** (172.81)	0.153*** (0.035)
Treatment	−65.46 (74.89)	−49.56 (87.75)	−28.80 (85.51)	0.033*** (0.008)
After Enforcement x Treatment	−384.68*** (116.04)	−274.51*** (66.49)	−325.30* (148.99)	−0.034* (0.015)
After Announcement x Treatment		−58.72 (80.72)		
City FE	x	x	x	x
Month FE	x	x	x	x
STR market controls	x	x	x	/
Neighborhood controls	x	x	x	x
N	9045	7618	6992	5380
Adjusted R^2	0.51	0.52	0.50	0.10
F-Statistic	146.33*** (df=65; 8979)	146.77*** (df=57; 7560)	127.60*** (df=56; 6935)	11.63*** (df=55; 5324)

As shown in regression (1) of **Annex 3**, COVID-19 restrictions have not evened out the effects linked to regulation. If anything, the gap between Bordeaux's districts and control group districts has gotten larger. Regression (2) intends to check for the existence of an anticipation effect through a twofold DID and finds no evidence of such an effect.

Biarritz and Anglet were replaced by Royan and Arcachon in regression (3) in order to prove that the previous results don't strongly depend on control group selection. In regression (4), listings are allowed to become targeted or switch to home-sharing after regulation kicks in. The principle of defining targeted listings remains but it is done in isolation for the two time periods (pre and post regulation).

Annex 4 : Tests regarding the Spatial regression discontinuity results

	Reservation days per listing				
	(1) Local-quadratic regression	(2) No COVID time restriction	(3) Two previous years	(4) Bandwidth −200 meters	(5) Bandwidth + 200 meters
Local average treatment effect	30.571** [9.848 , 51.173]	39.543*** [21.715 , 62.331]	7.109 [−8.450 , 22.651]	31.775* [1.349 , 55.706]	26.580** [12.253 , 51.634]
Listing type	x	x	x	x	x
Share of primary residencies	x	x	x	x	x
N Bordeaux	4230	2442	2767	1382	3227
N Control	2228	1835	1365	1185	2022
Bandwidth	936.817	583.592	760.846	389.896	789.896

Annex 4 presents models that are complementary to the SD results in subsection 2.3.3. Specification (3) is perhaps the most important. It serves as a placebo test that, given the insignificance of the LATE, upholds the assumption of initial homogeneity across the border.

It is also common and recommended when it comes to RDDs to test for robustness through changes in the bandwidth (Lee and Lemieux, 2010). This is done through specifications (4) and (5) that adapt the bandwidth obtained in the preferred general specification (2) of **table 5**.

Given the nature of regulation and the relatively low distance at which bandwidths are selected in our setting, we opted for a local-linear regression in our SD. specification (1) shows that our results are upheld when switching to a local-quadratic regression.