License to chill ?
How enforcement of mandatory registration shapes Short-Term Rental markets in France

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Abstract :

The study explores the impact of mandatory registration regulations on STR markets in France, focusing on Airbnb. Using staggered difference-in-differences methodology and data from 2019–2023, we reveal that mandatory registration and platform compliance reduces the number of active STR listings but does not significantly impact overall reservation days. Our research suggests a reduction in market oversupply that has not affected demand, and has led to a further concentration of STR activity among listings. Licensed hosts have consolidated market power and expanded their activity, while regulatory measures shifted some activity towards medium-term rentals (MTR). The findings highlight challenges in balancing the evolving STR landscape.

Keywords : Short-Term Rental, Medium-Term Rental, Regulation, Airbnb, Housing Market

1.Introduction

The regulation of short-term rentals (STRs) has become a key urban policy challenge, as cities struggle to manage conflicting interests: preserving affordable housing, maintaining resident' quality of life, and supporting tourism-dependent economies. While the negative externalities of STRs—rising rents, displacement of vulnerable residents, and neighborhood disturbances—are well documented (Adamiak and Marjavaara, 2023; Bekkerman et al., 2023; Cocola-Gant and Gago, 2021; Duso et al., 2024; Hidalgo et al., 2023; Koster et al., 2021; Reichle et al., 2023; Rossi and d'Addona, 2025; Xu and Xu, 2021), the existence of an effective regulatory response is still being debated.

France offers a particularly interesting case to investigate the effects of STR regulation. The 2018 ELAN law introduced a national framework enabling municipalities to require a registration number from hosts. More importantly, it compelled platforms like Airbnb to enforce this requirement by conditioning rental activity by making it conditional on obtaining this number. While Airbnb has gradually implemented these obligations, their actual impact on rental markets remains largely unexplored.

This paper examines the effects of such platform-enforced registration systems on STR market dynamics in French cities. Specifically, we ask if this enforcement reduces the overall activity of STR or merely reallocates it among compliant hosts — An important consideration given the differences in negative externalities among hosts (Anselmi et al., 2021; Gyòdi, 2024). We also investigate the potential consequences on demand for medium-term rentals (MTRs),

which are increasingly popular in the post-COVID context of remote work and mobile lifestyles (Llaneza Hesse et al., 2023; Wachsmuth and Buglioni, 2024) and are incentivized by French licensing rules.

We take advantage of the gradual implementation of *Airbnb*'s compliance throughout French cities using a staggered difference-in-differences design. We show that the number of active listings has gradually decreased by about 20% as a result of registration enforcement in cities subject to it. This decrease in listings was however not paired with a decrease in activity, as measured by reservation days where we found no impact. In fact, we find that enforced registration has led to a decline in empty days (or unrealized supply). In doing so, it has concentrated the number of reservation days in a smaller number of listings, benefiting license holders and further incentivizing them to stay on the market. On top of this, regulation has changed the structure of the rental market by promoting longer medium-term rental instead of short-term rental.

These results underscore both the potential and the limitations of local STR regulation. If the compliance from platforms appears effective in reducing the number of listings, its impact is less convincing when it comes to reducing the overall STR activity. Accessing platform data remains difficult and limited, complicating efforts to monitor and evaluate regulatory outcomes. France's experience, and particularly Airbnb's gradual alignment with registration rules, thus provides a valuable lens through which to assess the effectiveness of local regulation within a broader urban governance framework.

The remainder of the paper is structured as follows. We begin by reviewing the literature on STR regulation and outlining why the French case is especially instructive. We then present our empirical strategy and dataset, followed by the main results. Finally, we discuss the policy implications of our findings and avenues for further research.

2. Regulated STR : challenges and results

2.1 Cities and STR regulation

The literature on the effects of regulation has expanded in recent years, paralleling the first attempts to regulate short-term rentals. Studies consistently show that regulatory measures have systematically led to a decline in short-term rental activity. Bei and Celata (2023) observed a decrease in the number of apartments listed on platforms across major European capitals following the introduction of various regulations. Similarly, Falk and Scaglione (2023), as well as Gauß et al. (2024), confirmed through more experimental empirical designs that regulation reduces host activity and listings in Geneva and three major German cities (Berlin, Hamburg, and Munich), respectively. Comparable findings are observed in U.S. cities. Koster et al. (2021) reported a 50% reduction in listings in 18 cities across Los Angeles County that adopted regulations. On the other hand, Bekkerman et al. (2023) identified a more moderate effect in a panel of 15 U.S. metropolitan areas, with an average decline of 11% in listings.

Most recently, Gyòdi et al. (2025) found that licensing-backed regulation had a strong negative impact on the market's expansion in Barcelona. The authors also show that for regulation to be effective, it must be accompanied by monitoring and verification mechanisms. In this regard, the existence of a data-sharing agreement between Airbnb and the city played a

significant role in accelerating the reduction of the STR supply. It is also worth noting that the importance of a data-sharing agreement between Airbnb and the local government was highlighted by Zhe Jin et al. (2024) in the case of Chicago. Once again, the authors emphasize the difficulty of implementing regulatory measures—such as mandatory registration—in a context where the municipality lacks both resources and access to reliable data. A similar result was also highlighted by Garz and Schneider (2023) in Norway, where the tax introduced by the government appears to have had little impact on STR activity due to the administration's limited capacity to enforce and collect it.

The diversity of local contexts and regulatory approaches also influences the expected effects of these measures (Hübscher and Kallert, 2023). In a comparative study between Barcelona and Paris, Bei (2025) shows that spatial and temporal restrictions can lead to different outcomes. For instance, Barcelona's spatial restrictions have successfully reduced STR pressure in the city center, whereas the temporal regulation implemented in Paris—while also effective—proved more difficult to enforce. The case of Bordeaux, examined by Robertson et al. (2023), further illustrates that highly restrictive measures—such as the compensation rule—can have substantial impacts, with evidence suggesting that the reduction in the number of bookings ranged between 35% and 44% of the average reservation days before the regulation.

Literature has also highlighted regulatory impacts beyond STR activity in various settings : On real estate markets (Koster et al., 2021; Gonçalves et al., 2022; Valentin, 2021; Bekkerman et al., 2023), on the market's composition (Bey and Celata, 2023; Falk and Scaglione, 2023), and on crime rate changes based on platform self-regulation (Han et al., 2022).

It is again worth noting that these results stem from varying types of regulation (Nieuwland and van Melik, 2020), making it challenging to precisely understand the effects of each measure. This complexity is further compounded by the fact that, under similar regulatory frameworks, property owners may perceive and react to regulations differently. This was demonstrated by Gauß et al. (2024) and Robertson et al. (2023), who found that, even when regulations in France and Germany targeted only the most active hosts, all hosts—regardless of whether they were directly affected—reduced their activity on platforms.

In this context, it is challenging to discuss the external validity of these findings, which are highly dependent on local environments and regulations. By studying the impact of platform participation in the implementation of registration numbers across various cities in France, we partially address the challenge of external validity—at least within the French context.

2.2 The case of France

According to Airbtics (2025), France is Airbnb's second-largest market after the United States. Paris is one of the most profitable cities for the platform, after London[1]. As a consequence, the national regulatory framework in France prohibits renting out primary residences for more than 120 days per year (except when only part of the residence is leased, not the entire unit, in which case no limit applies). Rules for secondary residences are more complex and depend primarily on the municipality. In cities with over 200,000 residents and in so-called "high-

[1] https://airbtics.com/best-airbnb-markets

demand" areas, landlords must obtain a change-of-use permit from the local government before renting out secondary residences as short-term rentals. While this permit may be granted upon simple declaration, stricter measures, such as "compensation" (Robertson et al., 2023), quotas, or discrimination based on the requester's characteristics may also apply on a per-city basis. Compensation requires that a housing unit removed from the long-term rental market be offset by introducing a comparable unit into the long-term rental market.

Additionally, the French Tourism Code, along with provisions in the ALUR[2] and ELAN laws, has allowed municipalities who have implemented the change of use, to implement registration numbers to monitor and control the evolution of the short-term rental market. This obligation for hosts to register is tied to the requirement that platforms only accept listings associated with a valid registration number. Platforms are also required to provide municipalities with information regarding rental activity within their jurisdiction and to block reservations that exceed the legal limits.

It is important to note that Airbnb did not immediately comply with the law upon its implementation, risking rendering the legislation ineffective. For a period, although hosts could provide a registration number, doing so was not an operational requirement to use the platform. On June 30, 2021, the day before Airbnb was fined €8 million by the Paris Judicial Court for failing to meet its obligations regarding registration numbers, the Californian company decided to comply. Initially, Airbnb made the registration number mandatory in Paris and gradually extended this obligation to other cities.

Airbnb's compliance marks the point when compliance with the law became enforceable, not the beginning of the regulation itself. Therefore, some effects of the regulation may precede Airbnb's compliance, potentially attenuating the observed impact. This is especially likely when the time between a municipality's adoption of the regulation and Airbnb's decision to comply is significant, although this gap seems to have diminished over time. Nonetheless, we argue that the effectiveness of registration-based regulation hinges on Airbnb enforcing the requirement on its platform, justifying the use of this moment as the treatment in our empirical design.

Moreover, there is a notable exception to the requirement of limiting short-term rentals of a primary residence to less than 120 days or requesting a change of use for secondary residences: the mobility lease. This provision, introduced by the ELAN law in 2018, allows for the rental of furnished properties for a period of 1 to 10 months to individuals in temporary mobility situations, such as students, interns, or remote workers. This provision serves as an incentive to shift short-term rentals toward medium-term rentals, suggesting that it could be a response to the issues caused by short-term rental practices. In any case, regulation appears to be embedded in multiple levels of governance and stakeholders (Aguilera et al., 2021) which complicates both implementation and evaluation. Put simply, there is a national-level regulation that allows for cities to require registration and authorizes them to discriminate on various factors (most notably primary residence). There is the municipal level with decision-making on registration which chooses the design of discrimination. And finally, *Airbnb* compliance is necessary and in this context, the platform recently started to prevent entire

[2] ALUR stand for Accès au Logement et un Urbanisme Rénové, is a national laws that define the rights and contexts of land use.

home listings from appearing without registration. Now, with this context in mind, we can turn more precisely to our identification strategy.

3. Expected effects

The timing of cooperation is the instrument that will be used in this paper and interpretations thus hinge on it, but we do not seek to specifically untangle the importance of compliance in the wider scope of regulation. Instead, we take platform compliance as a stricter enforcement of existing rules, and our assessment of the impact of *Airbnb*'s cooperation thus also hinges on the design of regulations itself.

We explore three main outcomes of interest in this paper. The first is the volume of STR activity, that is, the number of listings and the number of tourists they accommodate. Second, we seek to understand whether *Airbnb*'s cooperation has contributed to reshaping the STR market. Third, we assess the extent to which the mobility lease arrangement in regulation has encouraged property owners to pivot towards MTR following strict enforcement.

Our first expectation follows the literature outlined in section 2. It is that strict regulation enforcement should reduce the number of active listings on the market. Enforced licensing, as a whole, makes it more difficult for hosts to operate. As new barriers to entry are set, fewer hosts are able to enter the market, and some existing hosts might exit because of their non-compliance. This could happen through a sharp decrease in the number of active listings that lose access to the market just after regulation, but it could also happen through a gradual decrease as more and more hosts are prohibited from entering. **Table 1** shows that the median French listing was only active in a quarter of the observed months, which makes it less likely to observe a sharp decrease in active listings as regulation hits and gives more credit to the gradual hypothesis.

However, the volume of STR activity is at least as well (if not better, in most contexts) defined by the number of nights spent. Ideally, the number of nights spent (reservation days) should decrease along with active listings in order for the strain of STR on cities to be lowered. Under a hypothesis of fewer listings, this would happen if the available nights (in other words, supply of STR) that left the market led to a loss in reservation days because STR demand could no longer find fitting accommodations. The number of reservation days would however not decrease if those tourists still found other fitting STR accommodations that would have otherwise been empty. With a decrease in active listings, the expectation of change in reservation days thus hinges on the following question : Will registered listings recover the STR activity of unregistered (and thus no longer exist) listings, or will their STR activity be lost?

The answer to this question fundamentally depends on the initial state of the market, for which **Table 1** provides context. Active days are the number of days in which each listing was made available for rental. Over the whole country between April 2019 and March 2023, there is a clear pattern of oversupply. The median listing was only booked in 25% the days for which it was made active, and over three quarters of listings were empty during over half of their active days. In other words, STR listings tend to be scarcely booked compared to how much they

are made available — Probably in part because of how low the opportunity cost is for STR hosts.

Table 1 is not enough alone to determine that reservation days will be stable, but it does raise doubts over the strict enforcement of regulation's ability to reduce the number of reservation days. If listings that have a registration number and thus persist on the market are a part of the overwhelming majority that has a strong tendency to be empty, then they could be able to recover the STR activity left unattended by the absence of unregistered listings. In other words, there is a risk that *Airbnb*'s cooperation would limit existing oversupply but not actually decrease activity.

Another outcome that hinges on the same market state question is the supply-side concentration of the STR market. In the case where registered accommodations are able to recover the bookings of absent unregistered listings, their individual activity should increase. This is an important point, because it would go against rules that are broadly defined to limit market concentration by making registration more easily available to occasional STR leasers. If reservation days do not decrease, the stricter enforcement of regulation could end up benefiting *insiders* (registered hosts) without alleviating the strain of STR on cities.

A different observable outcome regarding the market structure is the prevalence of MTR. As a reminder, MTR listings were made exempt from change-of-use requirements. They thus provide an alternative for hosts to be able to operate without the need for a license. Because the opportunity for medium term rentals has existed long before *Airbnb* cooperated in any city, we can impute any cooperation-driven increase in MTR activity to an increase in the cost of presence on the STR market. In other words, causally determined gains in MTR listings in this context come as replacements for STR listings, not for long-term market rentals. This outcome is also expected to increase following mandatory licensing, as it increases the cost of operating on the STR market..

Table 1 : The activity of French STR listings between April 2019 and March 2023

	Mean	*SD*	*Q1*	*Median*	*Q3*
Active months	16.46	13.44	6	12	24
Active days	420.12	359.52	141	310	615
→ Booked	30.74%	0.27	6.16%	25.31%	49.39%
→ Empty	69.36%	0.27	50.60%	74.69%	93.84%

Active months is the number of months in which listings have at least 1 active day. Per-listing data over a 48-month (1460-day) period. Booked and Empty are expressed as shares of active days.

4. Empirical strategy

We seek to investigate how *Airbnb*'s cooperation with mandatory registration might have modified STR (and MTR) activity. The adoption of Airbnb cooperation was staggered across French cities, as noted on the platform's website[3]. Paris and Bordeaux were the first to benefit from it in June and July 2021 respectively. By October 2022, forty-two major cities had also started benefiting. This gradual implementation provides grounds for a quasi-experiment across the country using a staggered difference-in-difference design.

4.1 Data

Using access to a database[4] of Airbnb listings ranging from April 2019 to March 2023, we lead staggered difference-in-difference estimations of relevant STR indicators. The data at hand include French listings' coordinates, their monthly activity and their registration numbers (if the listing has any). Here, we focus exclusively on entire home listings, as they are the only targets of the treatment. Outcome variables concentrate on reservation days, empty days and revenue. While these variables are self-explanatory, it is important to note that they are not exact observations. *AirDNA* scrapes public-facing information and has an internal model to estimate whether a listing was reserved at a given date.
Data are aggregated at the IRIS (census tract) level to add relevant covariates, all of which are openly available through *INSEE* (The French national institute for statistics). Also, estimators that draw on per-listing variables (reservation days per listing, empty days per listing and revenue per listing) all exclude census tracts that host no active listing in a given month, and are weighted by the number of active listings. These estimations are thus led on smaller populations than the aggregate census-level counterparts.

4.2 Estimation

Recent difference-in-differences (DID) literature has outlined the drawbacks of traditional dynamic two-way fixed effects (TWFE hereafter) DID designs to estimate the effect of a staggered treatment (Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021). In our setting, the effect of treatment (*Airbnb* compliance) would be expected to grow over time as fewer hosts enter the market. Treatment effects are not expected to be constant over time, nor are shifts in outcomes expected to be parallel to their initial trajectories[5].

To avoid this issue and to isolate clean comparisons, we use the flexible staggered did method outlined in Callaway and Sant'Anna (2021). This method effectively computes several fixed effects regressions using subsamples of treatment and control groups. It then aggregates these regressions for each cohort-time pair into Aggregate Treatment Effects on the Treated

[3] http://web.archive.org/web/20241024220144/https://www.airbnb.fr/help/article/1383
[4] Provider : AirDNA
[5] Using canonical dynamic TWFE designs would thus lead to "forbidden comparisons", where later-treated groups are compared to groups that have already been treated

(ATT(g,t) for each cohort *g*[6] in each time period *t*) by subtracting the expected outcome of untreated units from the expected outcome of treated units, as follows :

$$ATT(g,t) = E[Y_{i,t}|C_i = g] - E[Y_{it}|C_i < t]$$

Where the first part of the equation is the expected outcome at time *t* of census tracts *i* that are where regulated at time *g* (that are a part of cohort *g*). The second part is the expected outcome at time *t* of census tracts that were either never treated (*g = 0*) or not yet treated at time *t*.

These expected outcomes themselves depend on weighted two-way fixed effects regressions that leverage doubly robust estimators (Sant'Anna & Zhao, 2020). For absolute census tract variables, we use the number of accommodation units as weights. For "per listing" variables, we use the number of active listings. Because there are many cohorts and many time periods, we do not report individual ATT(g,t) values. All regressions are clustered at the city level (A metropolitan-area clustered version can be found in the appendix).

When presenting aggregate estimates later in this paper, we first take interest in the average ATT for each cohort across treated time periods, and then average the estimates across cohorts (in line with the recommendations in Callaway and Sant'Anna (2021)). This weighting method avoids giving more importance to cohorts that are treated for longer stretches of time, and we consider it analogous to ATTs in the canonical TWFE DID setting. The event-study-like figures presented below (Figure 2) are instead aggregations across given time periods of the ATT(g,t) of treated cohorts at that time.

As is the case in canonical DIDs, threats to the parallel trends assumption are the principal obstacle towards causal interpretations of our estimators. Such threats are particularly present in this study. First, because treatment is not being assigned randomly. Cities that have harsh regulations regarding registration (that is, those that are hostile towards hosts) have stronger incentives to negotiate enforcement with *Airbnb*[7].
Harsher registration policies are likely linked to decreases even without strict *Airbnb* enforcement (Robertson et al. 2023). If the time window between harsher registration policies and their enforcement is short, we could easily observe treatment effects that actually come as a result of legislation, not of *Airbnb*'s cooperation. Cities with the harshest policies (Lyon, Bordeaux, Paris, Saint-Malo among others) have enacted them long enough before *Airbnb* enforcement to alleviate this worry. Cities that anticipate larger undesired changes in outcomes also have more incentive to push for treatment. The threat is also attenuated by the fact policy design and its enforcement are necessarily conflated : This paper leverages the fact that the policy was not properly enforced without *Airbnb*'s cooperation. The implications we are interested in stem from the policy design itself, more so than from how necessary platform cooperation is to policy enforcement.

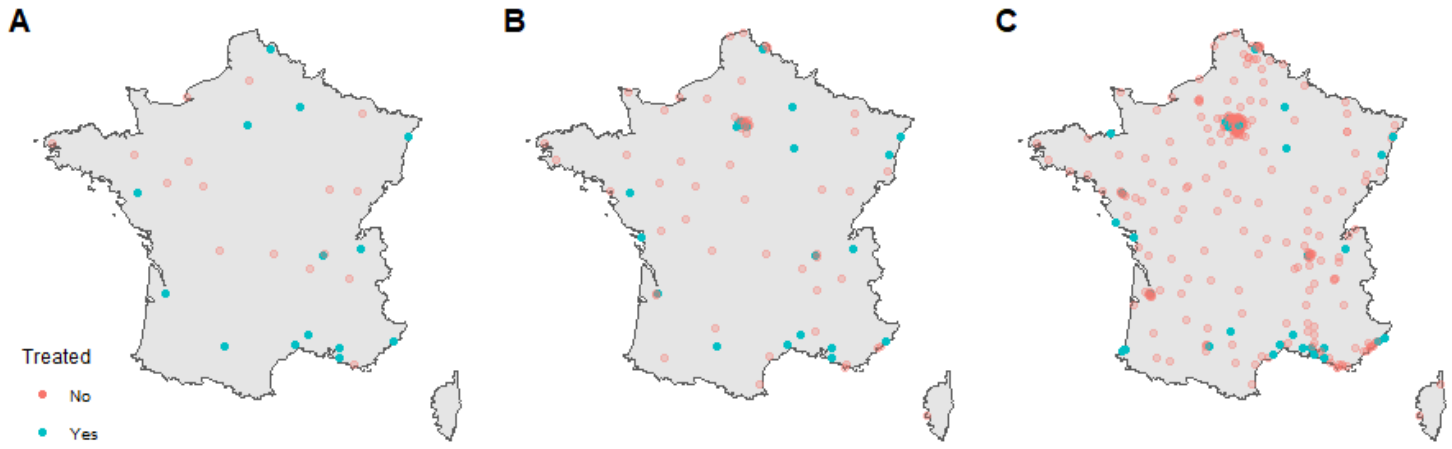
Second, cities that enact these harsher registration policies (and hence push to enforce them) are likely structurally different from non-treated cities. They are places where tourism development is less of a priority than others. Treated cities, especially in the earlier stages of

[6] Cohorts are groups of units that receive treatment at the same time
[7] Conversely, Airbnb also has stronger incentives to negotiate with them and is sometimes under judicial pressure

staggered implementation, are also those that had enough resources to develop the necessary systems to cooperate (e.g., through the transmission of authorized license numbers) and to bargain with *Airbnb*.

These threats are difficult to alleviate in single regressions. A first step towards addressing them is opting to use not-yet-treated units as controls (on top of never-treated units) in all the presented regressions, because these not-yet-treated units are less likely to violate the assumption. Also, for transparency and to better identify economic mechanisms of the policy, we opt to present different overall ATTs on different segments of the dataset. First, Paris is a particularly unique city within France regarding tourism, and it also has the harshest STR policies. It is the largest threat to the validity of the parallel trends assumption, and we exclude it from our regressions (see the Appendix for Paris-inclusive versions). Second, we focus solely on urban areas and outline three separate data subsets based on cities' populations. The first subset only includes cities with more than 100,000 inhabitants (aged 15 or more). The second includes those with over 50,000 inhabitants (aged 15 or more). The third and broadest subsMaps of these three subsets are presented in **Figure 1** and includes cities with over 20,000 inhabitants (aged 15 or more)..

Figure 1 : Map of cities included the first subset (**A**, > 100,000 inhabitants), the second subset (**B**, > 50,000 inhabitants), and the third subset (**C**, > 20,000 inhabitants)



The subset that is limited to over 100,000 inhabitants is clearly the most balanced : 14 out of 30 observed cities are treated. It however also has a low number of observations, and many of these are concentrated on the South-Eastern coast. The second subset loses treatment balance, as 26 out of 86 cities are treated. However, it is more geographically balanced both for treated and untreated units. The third subset has very good coverage of untreated cities, but only 44 out of the 343 cities are treated.

We believe that the second subset **B** strikes the best balance between a sufficiently populated and diverse sample (for which it surpasses subset **A**), and more homogeneous treatment and control groups (a larger threat in subset **C**). We still report the overall ATTs for all three subsets of data, as doing so allows for an assessment of potential differences in outcomes for different kinds of cities.

Another step towards alleviating problems regarding the parallel trends assumption is to carefully select covariates. As a reminder, the Callaway and Sant'Anna (2021) method allows for the parallel trends assumption to be conditional on covariates. In fact, we would not expect a strong argument for the parallel trends assumption in the absence of covariates. We thus opt to aggregate listing data at the IRIS level instead of the city level. Not only are cities different across each other, they are also very heterogeneous within themselves.

We use the number of restaurants in and around[8] the census tract because STR both is attracted by and affects leisure amenities (Hidalgo et al., 2024). The relative distance[9] of the census tract to the city's center[10] is a way of defining how relatively attractive a census tract might be within its city. The number of accommodation units in 2020 serves as a proxy for STR supply potential, while the share of these accommodation units that are declared as secondary residences serves to give a measure of the tourism potential. Finally, as differences in seasonality across cities are likely to be indicative of their relative trends, we define and use a "summer intensity" of STR activity as the share of its reservation days the city experiences between June and September. In practice, presented estimates stem from regressions using all of the above as well as fixed effects.

Table 1 presents the descriptive statistics of these control variables and of the main outcome variables we use, in subset **B**. Units with no accommodation whatsoever (e.g. airports, fields) are excluded. First and foremost, IRIS units are relatively homogenous in their supply potential (number of accommodation units). Summer intensity is also balanced across treatment and control units, while the number of restaurants and the share of secondary residences is lower and more variable in the control group. The first three outcome variables of interest are all for treated units, but two encouraging elements should be noted. First, they are individually on a similar scale as their untreated counterparts. Second, the relative ratios of reservation days to active listings, empty days to active listings and empty days to reservation days are in fact very close between treated and untreated units. Third, the mean number of reservation days per listing in treated and untreated census tracts is extremely close. Untreated units see lower STR volume (which itself is not a threat to parallel trends), but they seem to have a similar structure in their STR activity.

[8] This serves to spatialize the IRIS within a broader context and to dampen border effects. Effectively, our "Restaurants" variable is the sum of restaurants in the census tracts and in those directly adjacent to it.

[9] The distance of the census tract to the center divided by the average distance of that city's census tracts to the center. In other words, how "close" it is to the center relative to other census tracts.

[10] We use the most restaurant-endowed census tract as a proxy for the city center as perceived by STR tourists

Table 2 : Descriptive statistics of the main variables of interest and of covariates, in census tracts of cities with over 50,000 inhabitants

	Treated units			Untreated units		
	Mean	*SD*	*Median*	*Mean*	*SD*	*Median*
Outcome variables						
Active listings	22.97	36.00	12	14.88	49.73	5
Reservation days	218.36	433.88	87	123.02	377.19	41
Empty days	346.02	553.92	177	250.33	1009.00	75
Reservation days per listing	8.50	4.98	8.29	8.88	5.75	8.5
Controls						
Restaurants	107.27	107.68	70	77.30	77.78	53
Share of secondary residences	4.75%	6.16%	3.10%	3.60%	8.02%	1.79%
Number of accommodations	1533.04	742.30	1426	1375.99	6787.39	1294
Summer intensity	43.07%	6.38%	39.56%	40.53%	6.55%	38.04%
Source : AirDNA and authors						

5. Results

In the first section of these results, we exclusively focus on entire home listings that do not possess a mobility lease licence (that is, those that are not analogous to MTR for most of the year). Overall ATTs are presented as shares of the sample means of each variable. We first present in **Figure 2** the estimators for all variables of interest on all three subsets of city sizes.

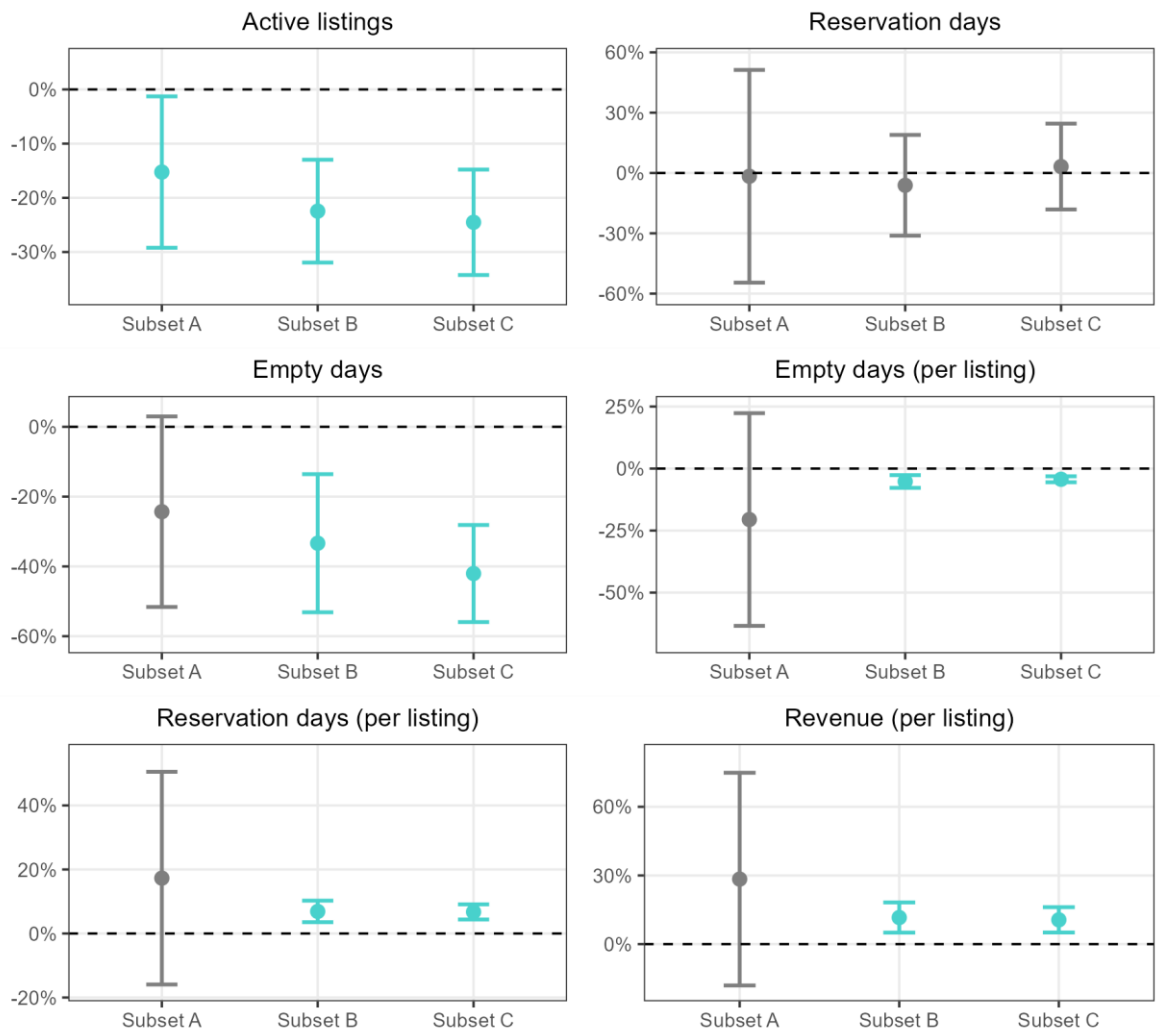
As was expected, the number of active listings has decreased in census tracts subject to *Airbnb* enforcement. It seems, at the very least, that *Airbnb*'s cooperation indeed led to policy enforcement with census tracts losing over 20% of their listings on average over their own enforcement period.

However, and importantly in the context of this paper, *Airbnb*'s enforcement of mandatory registration has had no discernible impact on reservation days. Coupled with the clear decline in empty days (or unrealized supply), this points towards the situation of oversupply outlined in section 3, and it has led registered listings to individually gain both reservation days and revenue. It appears that, in terms of its impact on STR activity, that *Airbnb*'s compliance has for now been limited to reducing the amount of empty days. Meanwhile, registered listings have recovered the demand left unattended by absent unregistered listings, and the supply on the STR market has concentrated, possibly leading to an increased professionalisation of

hosts. This is backed up by the less-affected nature of superhost listings and the increase in the share of superhost listings following treatment, as presented in Appendix Figure A.2. *Insiders* (or listings that are on the market after treatment despite the enforcement) have seen their own reservation days and revenue go up. License owners have capitalized on the loss of excess supply, the implications of which are also discussed in the next section. More information on the scale of these results can be found in Appendix Tables A.1 and A.2.

Moreover, the effect's sign is consistent across subsets. Overall ATTs that stem exclusively from very large cities (subset A) yield weaker aggregate-level effects but stronger per-listing concentration effects – albeit with lower significance. But subsets that also include medium-sized cities and lower-sized cities, even when per-listing effects are weaker, yield very significant estimators that point in the same direction as their large-city-only counterparts.

Figure 2 : Group-weighted average treatment effects on the treated for each subset of the data and for selected variables. All ATTs are expressed as shares of the sample's mean value.



The overall ATT is presented for each subset and each variable along with its 95% confidence interval. They are colored if their p-value is under 0.05. Overall ATTs are group-weighted and are based on census-level regressions that include all covariates. Subsets A, B and C are cuts of data that include cities with over 100 thousand, over 50 thousand and over 10 thousand inhabitants aged 15 and over, respectively.
Table available in the Appendix.
Reading : On average and within treated census tracts of subset B, Airbnb cooperation has reduced the number of active listings by 15% of its mean. See Appendix A1 for absolute values.

Secondly, we present event-study-like results of our preferred subset B's staggered DID estimations in **Figure 3**. These graphs aggregate, for every time *t* relative to treatment, the differences in outcomes between treated and untreated units.
While the pre-trends are not perfectly parallel, our covariate-enriched regressions are efficient at convincingly capturing the variations in the pre-treatment periods. This is extremely encouraging for our ability to accurately interpret the ATT estimators as indicative of a causal

impact. Second, and as initially expected, the number of active listings has gradually and clearly decreased in census tracts subject to *Airbnb* enforcement – But we observe little to no sharp decline in the month immediately following it. This trend is echoed by the gradual increase in empty days, and the absence of decisive movement for reservation days also serves to strengthen the point made in **Figure 2**.

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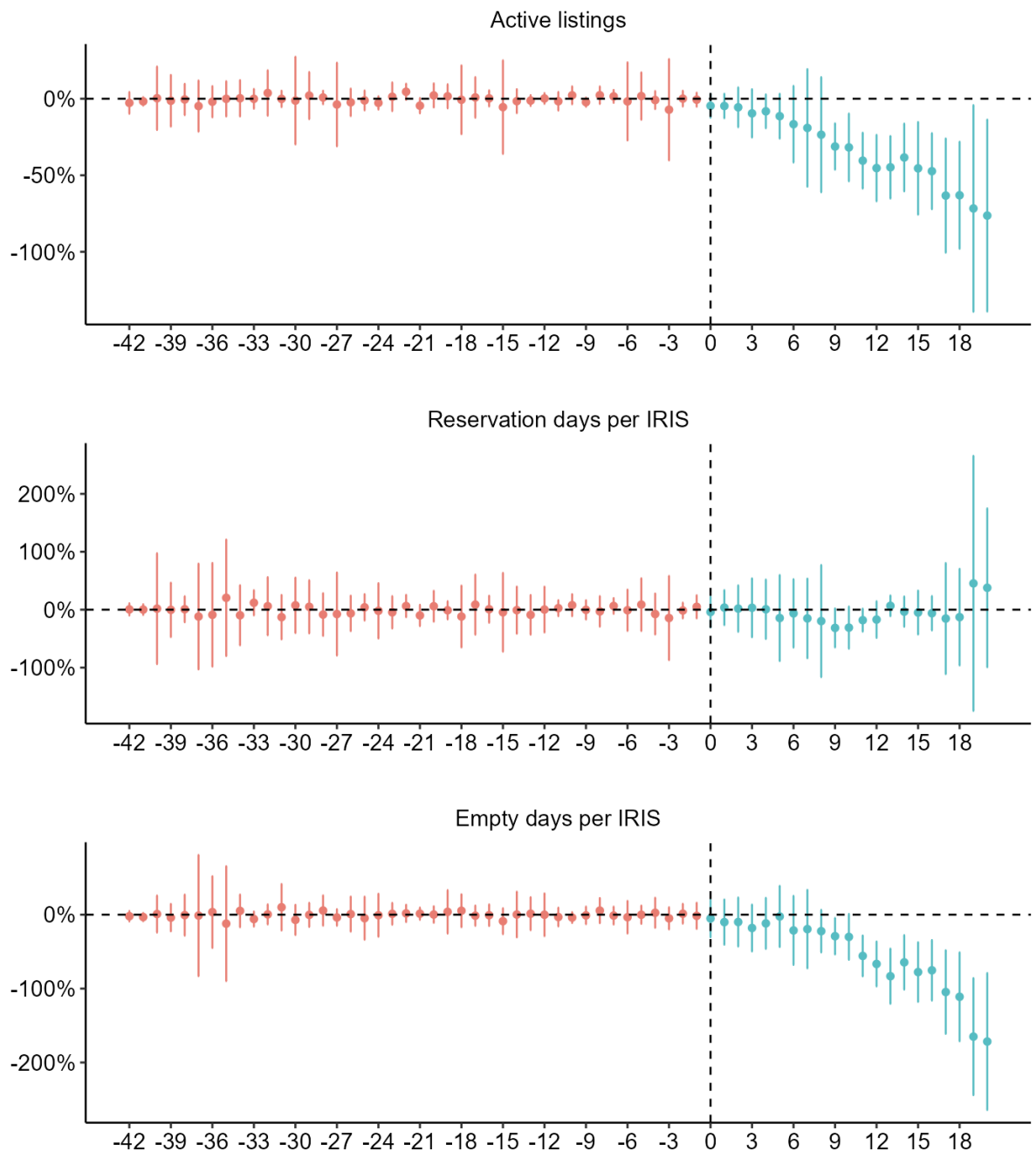
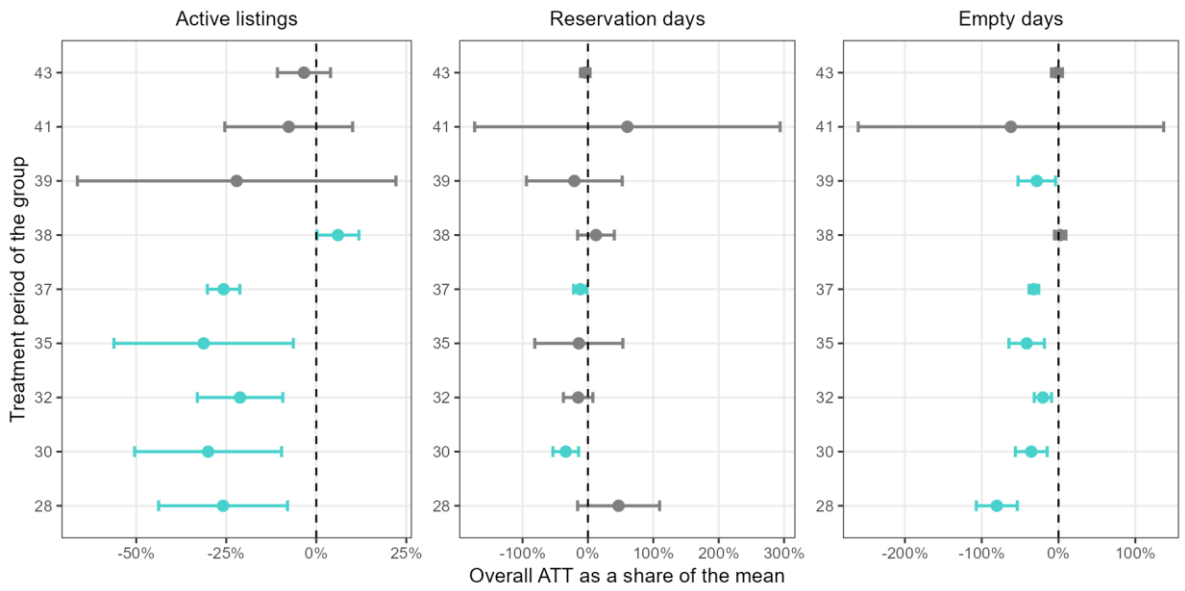
Figure 3 : Event-study like reporting the average ATT(g,t) of three selected variables in every time period, using subset B. All ATTs are expressed as shares of the sample's mean value.



These are period-wise aggregations of ATT(g,t)s presented for subset B along with their 95% confidence intervals. They are colored red before treatment, and blue after treatment. Because units can be treated in different months, values on the x-axis may represent different calendar months for different units.
Reading : On average, 12 months after regulation and within treated census tracts of subset B, Airbnb cooperation has reduced the number of active listings by about 50% of its mean.

Figure 4 also details and reinforces the results found in **Figure 2**. The results tend to be consistent across different treatment cohorts and showcase the gradual effect of *Airbnb*'s cooperation on the number of active listings — Although perhaps suggesting a plateau of its impact. Both the number of active listings and of empty days are consistently negative (except for Troyes, the only city of group 38), whereas no conclusion can be made regarding the number of reservation days.
It is worth reiterating that earlier cohorts tend to comprise larger cities, and that different cohorts are composed of cities with (sometimes very) different regulations.

Figure 4 : Average ATT for each treatment group (or cohort) for variables of interest using subset B. Average effects over the treatment period are outlined on the x-axis for each cohort (y-axis).



The group-wise average ATT is presented for subset B (cities with >50,000 inhabitants aged 15 and over) along with its 95% confidence interval for selected variables. They are colored if their p-value is under 0.05.
Reading : Units treated in period 28 saw an average decrease of a quarter of their mean active listings over the observed treated period.

The first section of these results was devoted to assessing the effect of treatment on the STR market, and excluded mobility-lease listings (MTR-hybrid arrangements) from the dataset. Here, we show in **Figure 5** that mobility-lease listings appear to have gained traction,as expected. We begin by separating listings that declare a mobility lease as a license,

and count them in each IRIS. We then double-check by observing the post-treatment evolution of the share of mobility-lease listings within each IRIS. Because we only have access to the most recent license declaration (that is, the license variable is not reported monthly), we are likely underestimating the effect here. Listings that did not have a license beforehand (and have since switched) will be considered to have been active as a mobility-lease listing before treatment, hence lowering pre-and-post treatment differences. Still, we observe an increase both in the number of mobility-lease listings (or an increased activity of those listings) and in the relative share of listings that are mobility-leases in treated census tracts.

Figure 5 : Group-weighted overall ATTs for each subset of data. The left-hand estimates focus on the absolute number of mobility-lease listings per census tract, whereas the right-hand estimates focus on their share in the overall supply of listings. All values are expressed as shares of the sample's mean value.

The overall ATT is presented for each subset and each variable along with its 95% confidence interval. They are colored if their p-value is under 0.05. Overall ATTs are group-weighted and are based on census-level regressions that include all covariates. Subsets A, B and C are cuts of data that include cities with over 100 thousand, over 50 thousand and over 10 thousand inhabitants aged 15 and over, respectively.

***note** : The mean share of MTR listings was just under 1% in all three subsets. The share of MTR listings was thus subject to just over half a percentage point in all three subsets, and they remain a strong minority.*

6. Discussion and conclusion

Our findings provide a way to assess the impact of Airbnb's commitment to complying with authorities to control and limit the expansion of short-term rental properties. Before discussing them, we feel it is important to reiterate that the heterogeneity of regulations across cities does not allow us to directly impute our findings to a given type of regulation or another. We leveraged their one largest common tool and its stricter enforcement in cooperation with a platform to better understand how this tool affects the STR market. In assessing the impact of stricter enforcement, these results are a proxy to better understand the aggregate effects of France's diverse license-based regulations, but these licenses are not equivalent across cities. The implications we primarily focus on in this discussion are those related to adverse STR (and MTR) effects.

At first glance, enforced mandatory licenses appear efficient towards lowering the strain of STR on cities, as the number of listings decreases in cities that have implemented regulation. This result alone is positive and certainly has importance regarding the impacts of STR on housing : There are less units for which long-term-rental seekers are competing with STR demand. However, this positive is mitigated by the nature of that competition for the remaining units and by other non-housing-related concerns for residents.

The most important factor towards this mitigation is that overall reservation days appear to have been unaffected by regulation. This means that just as many STR users are spending nights in these cities. To understand why this is the case, it is essential to recognize that hosts on STR markets have face relatively low opportunity costs. They do not need high occupation rates for their presence on the platform to be worthwhile. Nevertheless, our results show a decrease in empty days along with stable reservation days. We are thus able to deduce that the demand for STR listings is still met, answering the key question of section 3 : Licensed STR listings have recovered the loss in STR activity leftover by the absence of unlicensed listings. Thus, the enforcement of mandatory licensing has, for now, only had the effect of reducing oversupply.

Part of why this is important is that the number of reservation days, or the amount of STR activity, is a key factor for the negative residential sentiment towards *Airbnb*. Unless there are significant changes in the composition of demand, there is little reason to expect a reduction in STR related negative externalities. After mandatory licensing, there are still just as many STR users, and they keep the same relative power over the urban space. On top of this, it raises a critical question regarding housing : Does the stability of reservation days counteract the decrease in active listings ?

Listings that have remained might have been more profitable than those that have not before enforcement, but the enforcement of mandatory licensing has made them *even more* profitable than they already were. In other words, there are fewer listings, but licensed listings have benefited from being protected from competition by the regulation : They have a *license to chill*. It is clear that their incentive to switch to the long-term market has thus gotten lower, and there is a real risk that listings that are licensed are more cemented as STR accommodations than they were before. Licensed listings concentrate more STR activity, they are likely larger individual threats to the LTR market and likely produce stronger negative externalities (Anselmi et al., 2021; Gyòdi, 2024). The benefits of the decrease in the number of active listings are definitely mitigated by this concentration of STR activity.

Another issue raised by our results is that if STR activity is more concentrated listing-wise, it is likely to also be more concentrated spatially. The spatial concentration of STR activity is an important matter, because there are threshold effects to how STR tourists are able to change the urban space. Hidalgo et al. (2024), for example, estimate these threshold effects on restaurants using the number of listings – But the amount of STR bookings is analogous to it, if not more representative.

The results presented in this paper are consistent across different types of cities, from larger to smaller, outlining the fact that STR-related problem-solving is not only the worry of global metropoles and tourist hubs. Medium-sized and smaller cities, while at lower scales and volumes, face similar market patterns of STR.

We also find that the regulation's enforcement has also, as intended, created incentives regarding the development of MTR listings. The MTR phenomenon is still relatively new and its effects are difficult to envision, but the enforcement of mandatory STR registration by *Airbnb* is helping it gain ground. It is worth noting that we are also likely underestimating their presence by focusing solely on *Airbnb*, as hosts of MTR listings might be using other traditional LTR platforms or means to lease their accommodation. In the context of this license-based design framework, their presence comes at the cost of STR oversupply, which is, from the local government's perspective, probably a desirable solution. However, it is difficult to tell whether these listings would have been a part of the LTR or of the STR marker without the MTR-specific provision in the licensing rule. The question of the arbitrage between their impact on the LTR market and on the precarity of long-term renters and their apparent efficiency is not the subject of this paper, and it provides encouraging grounds for future research. Recently, we have also onserved the emergence of new intermediaries that enable hosts to reduce the transaction costs associated with regulation. In this evolving context, these *concierges* offer to take care of the marketing of STR or MTR offers. Their role undoubtedly needs to be studied more closely.

Our results also provide insight from the perspective of another stakeholder : platforms themselves, in this case, *Airbnb*. Beyond its initial regulatory entrepreneurship phase (Pollman and Barry, 2017), *Airbnb* has participated in public and behind-closed-doors discourse and has contributed to shaping regulation (Aguilera et al., 2021). It is one of the reasons for which treatment is seen as *Airbnb*'s compliance in this paper, rather than coercion. In fact, our empirical results point towards platform compliance not having directly hurt the platform in the short-to-medium term because reservation days were not affected. *Airbnb* seems to have found common ground with policy-makers on the licensing matter that allows for stable activity, for now at least. The policy's design also provides additional opportunity for the platform to compete with smaller existing MTR alternatives (Cocola-Gant and Malet Calvo, 2023) by creating an enabling framework.

Finally, this paper's empirical results are fundamentally limited by the three-step design of regulation. Also, the internal validity of the empirical design is threatened by the fact the largest and most STR-threatened cities tend to have regulated more and earlier. However, the data points in interesting directions that follow, in our view, intuitive mechanisms given the oversupply on the STR market. We believe that the external validity of these results is higher as a consequence.

To conclude, we show that French cities subject to *Airbnb* enforcement of the registration number regulation, losing over 20% of their listings on average over their own enforcement period. However, *Airbnb*'s enforcement of mandatory registration has had no discernible impact on reservation days, while we observe clear decline in empty days (or unrealized supply). All in all, what we have shown is that it is difficult to regulate the short-term rental market fairly and equitably. The embeddedness of public decisions, the diversity of the territories concerned, and the particularly fluctuating and constantly renewing demand at a time when labor and tourism markets are being reorganised, call for an extension of this work. In our opinion, obtaining more detailed, first-hand data (from STR platforms and Airbnb in particular) remains an essential objective for measuring and ensuring fair regulation

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Appendix

Regression tables

Table A.1 : Group-weighted average treatment effects on the treated for each subset of the data and for selected variables (analogous to **Figure 2**)

	Active listings			Reservation days			Empty days		
Subset	A	B	C	A	B	C	A	B	C
Overall ATT	-2.93* (1.49)	-4.15*** (0.82)	-3.91*** (0.74)	-3.02 (47.91)	-10.13 (20.05)	4.45 (15.39)	-69.90 (41.32)	-97.72*** (31.90)	-108.58*** (19.55)
As a share of mean	-15.25%	-22.46%	-24.51%	-1.63%	-6.12%	3.21%	-24.32	-33.36%	-42.04%
As a share of std. dev.	-9.75%	-9.36%	-8.53%	-0.84%	-2.49%	1.08%	-15.21%	-11.65%	-12.35%
N (cities)	30	86	343	30	86	343	30	86	343
N (treated cities)	14	26	44	14	26	44	14	26	44
In all of the above, observations at the census tract level, covariates are included, and the number of accommodations is used as weights									

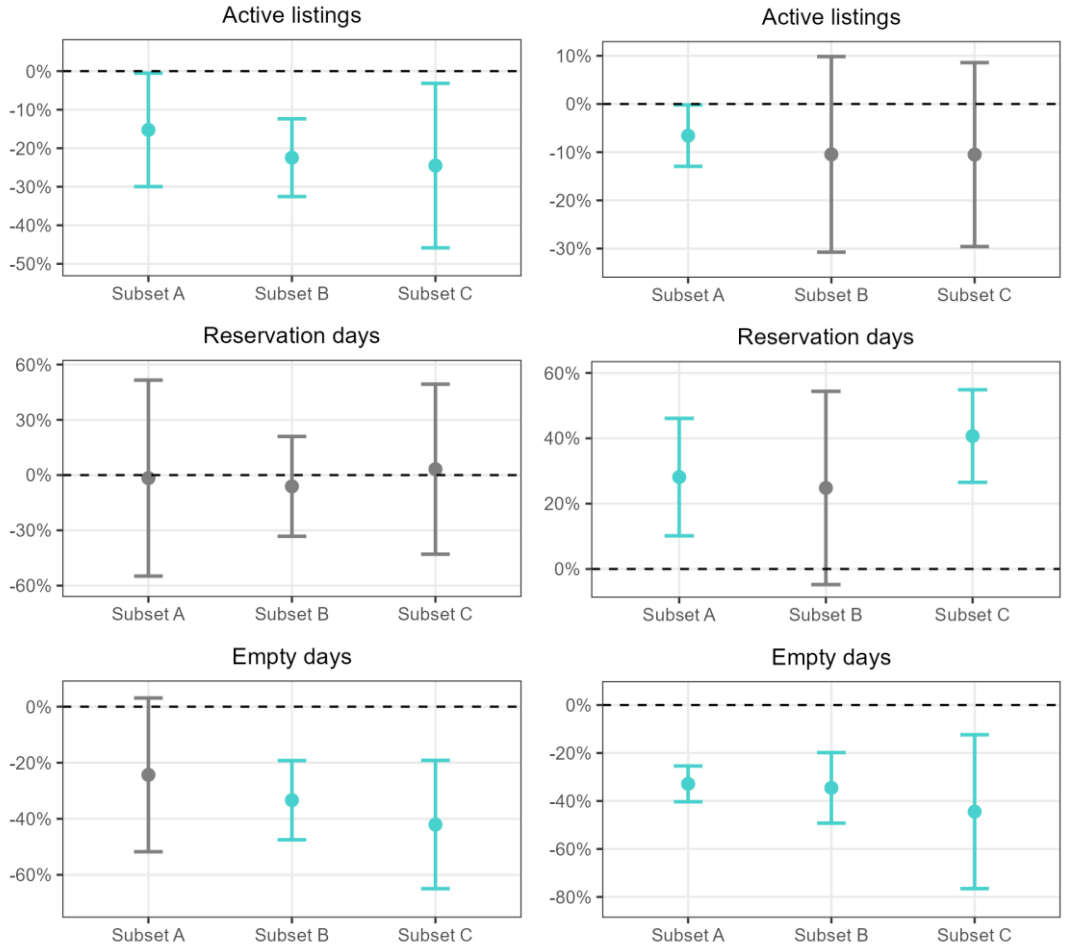
Table A.2 : Group-weighted average treatment effects on the treated for each subset of the data and for selected variables (analogous to **Figure 2**)

	Empty days per listing			Reservation days per listing			Revenue per listing		
Subset	A	B	C	A	B	C	A	B	C
Overall ATT	-3.14 (3.08)	-0.81*** (0.18)	-0.67*** (0.11)	1.56 (1.59)	0.61*** (0.16)	0.60*** (0.11)	216.58 (192.72)	86.54*** (25.73)	79.50*** (20.79)
As a share of mean	-20.54%	-5.28%	-4.32%	17.29%	6.89%	6.74%	28.40%	11.62%	10.61%
As a share of std. dev.	-58.06%	-14.25%	-11.31%	34.14%	12.89%	12.06%	37.33%	15.14%	13.27%
N (cities)	30	86	340	30	86	340	30	86	340
N (treated cities)	14	26	44	14	26	44	14	26	44

In all of the above, observations at the census tract level, covariates are included, and the number of active listings is used as weights

Robustness tests

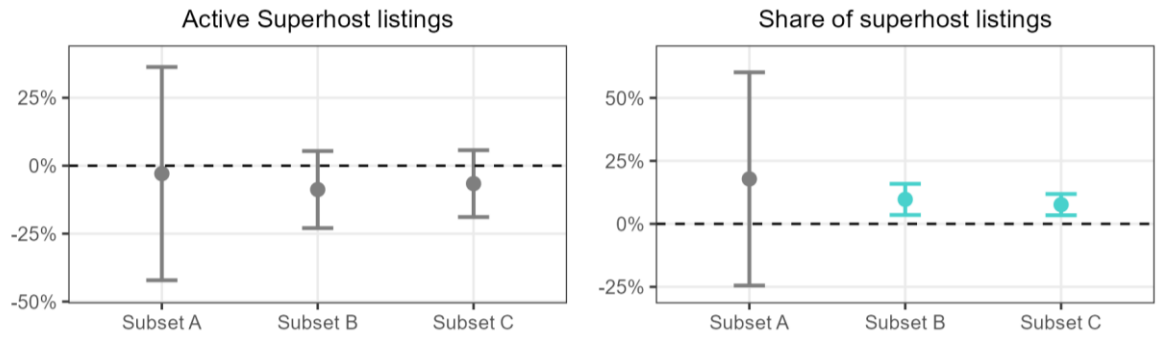
Figure A.1 : Overall ATTs using an alternative bootstrap cluster variable (Urban areas) (**left** column) and with the inclusion of Paris in each subset (**right**). All ATTs are expressed as shares of the sample's mean value.



Apart from the differences mentioned in the figure's title, the regressions were led with the same methodology as those from the main text.

Superhost listings

Figure A.2 : Group-weighted overall ATTs for each subset of data. The left-hand estimates focus on the absolute number of superhost listings per census tract, whereas the right-hand estimates focus on their share in the overall supply of listings. All values are expressed as shares of the sample's mean value.



The regressions were led with the same methodology as those from the main text.
***Important to note** : The mean share of superhost listings was 11% in subsets A and B, and 12% in subset C. The share of superhost listings was thus subject to a (non-significant) 2 percentage point change in subset A, and to a 1 percentage point change in subsets B and C.*