Assessing the Impact of Algorithmic Quantity Regulations on Sharing Platforms: Evidence from Airbnb in Paris*

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Abstract

Sharing platforms have been associated with a series of negative externalities. In recent years, several automated caps, or *algorithmic quantity regulations*, have been deployed to police such platforms. AQRs constitute a paradigm shift in platform regulation, as they enable exhaustive, and low-cost enforcement, thus comprehensively influencing interactions both within and outside the focal platform. However, their actual impact is not known, and has not been studied. In this work, we employ a series of difference-in-differences analyses to provide causal evidence on the impact of AQR both *within*, as well as *outside* a focal sharing platform - Airbnb. Within Airbnb, we find that the *quality* of platform offerings was negatively affected after the introduction of an algorithmic *quantity* regulation - marked by 6% decline in ratings. Additionally, we find that the AQR affected certain platform participants disproportionately. Providers without organic and designated trust building signals, i.e., inexperienced hosts and non-superhosts, bore the cost of the AQR, ending up worse off than their counterparts. We also identified two effects that were at play outside Airbnb as a result of the AQR. First, we identified an *anticipatory effect* in competing platforms, evidenced by the self-regulation of providers in Vrbo where occupancy rates declined by 3.6%. Second, we identified a *spillover effect*, evidenced by a reduction in housing prices by 0.402 units after the introduction of the AQR. This research provides empirically grounded insights on the implications of AQRs *within* as well as *outside* the focal platform, informing the broader regulatory discourse surrounding platform regulation.

Keywords: Sharing Platforms, Algorithmic Quantity Regulation, Anticipatory Effect, Spillover Effect, Causal Inference

JEL Codes: L51, L86, R31

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"Paris is the number one city in the world for Airbnb and we want to make sure our community grows responsibly and sustainably. While experts across the world agree that Airbnb has no significant impact on the housing market, we want to do our bit to address historic housing concerns in central Paris and help make this city a better place for everyone. We know reasonable automated caps are effective, target bad actors and are free for everyone, and we hope other platforms will follow and introduce similar measures to promote responsible and sustainable travel in Paris."
— Emmanuel Marill, General Manager, Airbnb France

1 Introduction

Sharing platforms are defined as socioeconomic systems that enable collaborative consumption, connect different groups of participants and grant on-demand temporary access to underutilized assets (Belk, 2014; Benjaafar and Hu, 2020). Sharing platforms have recently been associated with a series of negative externalities (Malhotra and Van Alstyne, 2014) leading to a slew of regulatory measures (Cohen and Sundararajan, 2015). Among these, quantity regulations — i.e. supply-side regulations that restricting the quantity of a good available (Glaeser and Shleifer, 2001; Sheshinski, 1976) — have emerged as a prominent tool in recent times. By stipulating caps on sharing, such quantity regulations aim to limit the externalities of the sharing economy. Traditionally, regulations aiming to reduce sharing have been hard to implement, monitor, and impose, leading to their weak enforcement, and leaving sharing platforms to operate in regulatory grey areas.

To meet this challenge, sharing platforms and regulators have begun to increasingly rely on the algorithmic enforcement of regulations. The algorithmic nature of these regulations constitutes a *paradigm shift* as it enables their pervasive, exhaustive, and low-cost enforcement, thereby comprehensively influencing the complex interactions that take place within platforms (Busch, 2021; Erickson and Sørensen, 2016). Despite the spurt of *Algorithmic Quantity Regulations* (henceforth AQRs) for platform regulation in several cities including Paris, London, and Amsterdam (Booth and Newling, 2016; Schechner, 2017; Lomas, 2018), and the crucial role that AQRs play in the platform regulatory landscape, their actual impact remains largely unknown. In this work, our primary goal is to further our understanding on the impact of AQRs both *within* as well as *outside* the focal sharing platforms, leading us to our research question: *How does the implementation of an Algorithmic Quantity Regulation (AQR) in a sharing platform affect outcomes both within and outside the focal platform?*

Drawing upon extant literature on sharing platforms and regulation theory, we employed

the difference-in-differences method to investigate the impact of AQRs on Airbnb's platform in the city of Paris. Utilizing a quasi-natural experiment, we scrutinized the impact of AQR both within and outside the platform. We found causal evidence that the introduction of the algorithmic *quantity* regulation led to a significant decline in the *quality* of treated offerings within the platform. In addition, we found evidence that the effect of the AQR, i.e., decline in quality ratings was not evenly distributed. Notably, offerings from experienced hosts and superhosts were better off than their counterparts. This latter finding is particularly noteworthy, as disadvantaged participants, including those from lower socioeconomic backgrounds, often lack the opportunities to quickly accumulate reviews, or meet the criteria required to earn trust-building badges (Slee, 2017; Scholz, 2016; Frenken and Schor, 2019). As a result, the disadvantaged participants that these regulations are aiming to protect, are the ones that bear the cost of the regulation.

Outside Airbnb, the introduction of the AQR led to two distinct effects. First, we found an *anticipatory effect* in a competing platforms, namely Vrbo, where providers voluntarily decreased their sharing activities, even though Vrbo did not implement an AQR. Second, we found a *spillover effect* in the housing market, evidenced by declining housing prices in the treated Parisian districts.

To the best of our knowledge, our results represent the first empirical evidence regarding the impact of AQRs in sharing platforms, emphasizing that regulating these platforms requires a delicate balancing act that accounts for the complex interactions occurring both within and outside the platform. Sharing platforms pose a significant regulatory challenge. On one hand, they create value for their participants (Einav et al., 2016; Benjaafar and Hu, 2020) and society (Greenwood et al., 2017; Greenwood and Wattal, 2017). However, they also cannibalize housing supply (Li et al., 2022), reduce profits for related markets (Li and Srinivasan, 2019; Zervas et al., 2017), and contribute to the gentrification of neighborhoods (Gant, 2016; Wachsmuth and Weisler, 2018). While experts and policymakers agree on the necessity of regulating sharing platforms (Cohen and Sundararajan, 2015), identifying effective regulatory tools and approaches remains an ongoing challenge (Miller, 2016). Our study advances this conversation by providing empirical evidence to inform this debate and, most importantly, by highlighting who ultimately bears the burden of regulation.

Three critical implications emerge from our findings. First, the decline in quality due to the AQR underscores adverse consequences for consumers seeking affordable sharing options, providers relying on supplementary income, and the platform as an entire ecosystem that sustains a diverse set of participants. Studies have shown that for some Airbnb providers, renting out their apartments is their sole source of income (Rayna, 2022), while many others — including single mothers and retirees — rely on this income to cover essential living

expenses, support their children's education, and escape poverty (Stohler, 2024). Furthermore, sharing platforms often provide the only flexible options for participants to generate secondary income (Bowman, 2019). In sum, the implications of introducing algorithmic regulations extend well beyond the boundaries of sharing platforms as business entities.

Second, the heterogeneous effects of the AQR reveal that while sharing platforms encourage competition and performance, they include non-professional hosts who seek to supplement their income and gain experiences from interacting with diverse guests. Trust-building signals are crucial in this competitive environment, but the process of acquiring these signals, such as provider experience and superhost badges, is fraught with disparities. These existing inequalities are exacerbated by opaque algorithmic rules, meaning that their uniform application does not result in uniform outcomes.

Third, our findings on external effects demonstrate how an AQR imposed on one sharing platform can result in far-reaching implications for the dynamics at play within other platforms and markets. Both the anticipatory self-regulation on competing platforms and the decline in housing prices indicate the potential positive outcomes that can be achieved by strategically balancing the interests of multiple stakeholders to enhance overall welfare.

This research contributes to three distinct streams of literature. First, this study contributes to the growing body of literature examining the regulation of sharing platforms and the challenge of balancing the interests of their diverse stakeholders (Chen et al., 2021; Han et al., 2022; Yu et al., 2019). Second, these findings inform an upcoming stream of literature that identifies that platform owners wield significant power over all platform participants, and due to their quasi-natural authority over all platform interactions (Cohen and Sundararajan, 2015), they are strategically positioned to play the role of private regulators (Hagiu and Boudreau, 2008). Finally, by highlighting the significant implications that the introduction of the AQR had for the platform and its ecosystem, this study contributes to the emerging literature on the use of algorithms in policy implementation (Danaher et al., 2017; Dourish, 2016; Gritsenko and Wood, 2022; Bucher et al., 2021; Gillespie, 2014; Möhlmann et al., 2020). Cumulatively, these findings contribute to the regulatory debate surrounding platforms in general, and sharing platforms in specific. Our results point out the nuanced effects of algorithmic quantity regulation and the diverse, conflicting interests of stakeholders and the communities involved. Beyond this, we add data from EU to the discourse. Finally, our work informs policy making and governance of platforms, with relevance for ongoing regulatory frameworks (for example, the Digital Services Act and the Data Governance Act in EU).

2 Background

2.1 Sharing Platforms and Regulating their Externalities

Sharing economy regulation has emerged as a global issue, as the negative externalities of sharing platforms have led to increasing calls for regulation (Cohen and Sundararajan, 2015; Bivens, 2019; Light, 2017). By negative externalities we refer to cases where the exchange of goods and services imposes costs on third parties, i.e., economic transactions that burden others, and as such, their total cost is not reflected in the price charged for the exchange (OECD Glossary, 2002). Specifically, sharing platforms in general, and home-sharing platforms in particular, have been associated with a series of negative externalities including increases in housing and rent prices (Lee, 2016; Barron et al., 2021) , decrease of rental housing supply (Li et al., 2022), threats to community life (Filippas et al., 2020), reduced profits for the hotel industry (Li and Srinivasan, 2019; Zervas et al., 2017), and increased gentrification of neighbourhoods (Wachsmuth and Weisler, 2018; Gant, 2016).

A slew of measures have been undertaken to keep these negative externalities at bay. These measures include complete bans of offerings (Hall, 2024), tax imposition (Cui and Davis, 2019), caps on the number of offerings that can be shared by a single provider, and the introduction of heavy fines (Chen et al., 2021). It is important to also note that the algorithmic implementation of a quantity regulation — which is the focus of this study — has led to global spurt (Booth and Newling, 2016; Lomas, 2018; Schechner, 2017) and constitutes the most prominent form of regulation to date.

Despite their negative externalities, it is important to recognize that sharing platforms do not operate in silos: in sharing platforms, both providers and consumers benefit, and platform participants are interwoven members of their local communities. Providers share their underutilized goods and services to generate alternative sources of income, identify opportunities for temporary and flexible work, and improve shared prosperity (Alyakoob and Rahman, 2022; Benjaafar et al., 2019). Consumers also benefit from their participation, as they get access to a great variety of goods and services, find unique opportunities that would not be available to them otherwise, and receive highly personalized recommendations (Einav et al., 2016; Fraiberger and Sundararajan, 2017). Sharing platforms provide consumer welfare (Farronato and Fradkin, 2022), and in fact, they contribute to consumer surplus and welfare more significantly for below median income consumers (Fraiberger and Sundararajan, 2017), and the obtained surplus increases over the long term (Filippas et al., 2020). In sum, communities within which sharing platforms operate benefit from all of the aforementioned advantages, as platform participants constitute indistinguishable parts of these communities, and maintaining the benefits associated with sharing platforms, while also diminishing their

drawbacks, has become of pivotal importance for platforms and regulators alike (Edelman and Geradin, 2015; Geist, 2018).

Sharing platforms are distinct from their traditional commerce counterparts, as consumers within them can accrue surplus by buying less, wasting less, while also being less encumbered by possessions (Benjaafar et al., 2019). However, because many typical mechanisms that foster trust are absent in these settings, sharing platforms heavily rely on trust-building systems such as user-generated reviews and the superhost badge designated by Airbnb (Einav et al., 2016). These mechanisms mitigate information asymmetry, reduce transaction-specific risks (Dellarocas, 2010), enable reputable providers to charge price premiums (Ba and Pavlou, 2002), and serve as signals of quality, safety, and trust (Tadelis, 2016). The development of these trust-building mechanisms, and the benefits for both providers and consumers, have made sharing platforms increasingly pervasive: revenues for both platforms and their providers are expected to double (Smith, 2017).

As a result of the increasing deployment of AQRs, their pervasive nature, and the need for AQRs to be implemented, monitored, and enforced within the focal sharing platform, platform owners are emerging as private regulators. This is noteworthy because platform owners are now empowered to use strategic instruments including the platform's architecture and user interface to influence the behavior of their participants (Hagiu and Boudreau, 2008), with the ramifications of these changes extending well beyond the formal boundaries of sharing platforms. Moreover, and despite the changing nature of the platform owners' role, the actual impact of AQRs has neither been examined, nor been understood to date.

2.2 Quantity Regulations and their Algorithmic Implementation in Sharing Platforms

Socioeconomic studies have examined a range of regulatory instruments for their effectiveness in curbing negative externalities, and in relation to their impact within traditional market structures (Kaplow and Shavell, 2002; Kelly, 2005; Sheshinski, 1976). Quantity regulation, as the most prevalent regulatory instrument used, comes into play when regulators — a role traditionally assigned to governmental agencies — restrict undesirable activities by limiting supply quantities as opposed to charging taxes. They are aimed at *restricting* — but not banning or taxing — conduct between participants (Glaeser and Shleifer, 2001). Quantity regulations are abundantly practised in climate policy, where they are found to be ubiquitous, despite economists' preference for other instruments in order to reduce undesirable conduct such as price increases and tax imposition (Ellickson, 1972; Keohane et al., 2019; Kaplow and Shavell, 2002). In contrast, incumbent organizations have traditionally preferred quantity

regulations (Buchanan and Tullock, 1975).

In recent years, a marked shift toward the algorithmic implementation of quantity regulations has occurred in a number of cities grappling with platform regulation (Booth and Newling, 2016; Schechner, 2017). Algorithms allow a design-based approach to regulation and are increasingly being used to achieve social ordering across a wide variety of contexts by both private and public regulators (Yeung and Lodge, 2019). Algorithmic regulation allows an outcome-focused approach as it aims at substantive, measurable and observable outcomes(O'Reilly, 2013). The logic of control with algorithmic regulation builds upon the *code as law* paradigm, and can have a significant impact on the societies upon which it applies, especially when implemented in digital environments, such as sharing platforms, which are highly malleable through changes in their code and algorithms (Kalpokas, 2019). We refer to them as algorithmic quantity regulations (AQRs), noting that they constitute a paradigm shift for two reasons.

First, their implementation, enforcement, and compliance are all achieved algorithmically. Their algorithmic nature is particularly important because traditional quantity regulations have been weakly enforced primarily due to the increased difficulty associated with monitoring participants' activity (Katz and Shapiro, 1985). Subsequently, the introduction of AQRs constitutes an inflection point, as their algorithmic nature avails enforcement universally and at a very low operating cost.

Second, compared to quantity regulations that are typically implemented and enforced by governmental agencies, AQRs are implemented by platform owners. This constitutes a remarkable change worthy of investigation, as the locus of power moves away from governmental agencies, and platform owners are now emerging as private regulators that can use AQRs strategically to alter the behavior of participants (Hagiu and Boudreau, 2008).

While AQRs constitute a shift in regulatory practices, how such a shift should be designed, organized, and implemented is less clear: some experts have recommended self-regulation where platform owners share regulatory responsibility with governmental agencies (Cohen and Sundararajan, 2015), while others have suggested a two-pronged approach, under which public regulatory interventions complement self-regulatory mechanisms (Geist, 2018; Cusumano et al., 2021). In addition, AQRs have important managerial and strategic implications. Airbnb introduced AQRs in some of the largest markets it operates, including Amsterdam, London, Los Angeles, and Paris (Booth and Newling, 2016; Lomas, 2018; Lagrave, 2018). In sharp contrast, most of its competitors did not impose such regulations despite operating under the same legal framework. In sum, despite calls for data-driven methods (Quattrone et al., 2016) to understand the implications of the tectonic shifts that are well underway and are affecting sharing platforms and their ecosystems, the increased use

of algorithms as policy and law levers in sharing platforms is neither sufficiently investigated, nor entirely understood to date (Yeung and Lodge, 2019).

3 Hypotheses Development

3.1 The Impact of AQR on Quality Ratings

The generation of income is the single most important driver for providers to engage with a sharing platform (Benjaafar et al., 2019; Einav et al., 2016; Filippas et al., 2020; Fraiberger and Sundararajan, 2017). However, as the introduction of an AQR is likely to alter the incentive and reward structures within a sharing platform, providers are likely to experience changes in their resources, capabilities, as well as motivations to maintain the same levels of engagement with the platform. As an AQR will restrict income opportunities for providers, it can also serve as disincentive, making it difficult for them to continue offering sharing experiences that are on par with consumer expectations.

The expected income of providers from their engagement with the platform is likely to decline after the introduction of an AQR, while service and maintenance levels are also likely to drop, as providers may not any longer possess sufficient funds to sustain the same levels anymore. In addition, lowered incentives for providers may increase the likelihood of disintermediation from the platform, with participants deciding that alternative options have now become more beneficial for them, leaving fewer quality options for platform users. For example, an Uber driver in NYC may switch to providing limo services, or an Airbnb host may post their apartment on a competing platform, such as Vrbo, if the introduction of an AQR in the focal platform makes these alternative options more appealing than before. Additionally, imposing sharing limits as design rules in a platform (Yeung and Lodge, 2019) is likely to negatively affect not only the income of platform providers, but also the social capital that providers derive from their engagement with the platform.

In two-sided markets including sharing platforms, changes impacting providers can also translate into consequences for consumers. This rippling effect may be associated with two known mechanisms that generate value for participants and platforms alike: data and network effects (Katz and Shapiro, 1985; Rochet and Tirole, 2003; Gregory et al., 2022). First, platform owners intermediate two-sided sharing markets, and subsequently benefit by embedding themselves in the cross-side interactions of participants — which are typically referred to as network effects (Rochet and Tirole, 2003; Goldfarb and Tucker, 2019). However, a decrease in the number of possible interactions is likely to decrease both the value created for platform participants, as well as the value that platform owners will be able to capture.

Second, the ability of platforms to generate value through data and artificial intelligence for their participants is also likely to be impeded, as platforms will become less capable of utilizing insights derived from their data and AI capabilities to their fullest extent (Gregory et al., 2022). As a result, we expect that the introduction of an AQR will hamper the ability of platforms to both generate and capture value for their providers and consumers.

In addition, an *expectation gap* between the expected quality of an offering and its perceived quality may also be generated, leading to lower quality ratings within the platform. Consumers searching within a sharing platform right after the introduction of an AQR will be exposed to ratings that reflect the perceived quality of an offering from its past consumers. However, these new consumers — due to diminishing data effects — may perceive the quality of the same offering as lower, simply because the matches between consumers and offerings will be less optimal after the introduction of the AQR. Thus, consumers will find it even more challenging to find a *satisficing* offering (Simon, 1956). For instance, if an AQR was imposed on Tiny Library — a sharing platform where consumers can rent baby products such as strollers — many of the consumers would have to settle by renting their second-best option, and this would likely affect their perception of quality of the product at hand. Similarly, consumers are likely to provide lower quality ratings in an effort to rectify the discrepancy between the quality ratings shown on the platform, and their own perception of the quality of the offering (Kapogli and Kyriakou, 2015).

Platform architecture typically represents a challenge by itself when creating design rules that are on the one hand stable enough to constrain participants, yet versatile enough to avoid overly limiting them (Tiwana et al., 2010). As a result, a fit between a platform's architecture and its governing principles is considered a critical factor for a platform's success (Armstrong and Wright, 2007). However, we expect that the introduction of an AQR will create a mismatch not only between consumers and offerings, but also between its governing principles and its architecture.

In sum, we expect that the negative impact of an AQR on providers will also have a negative rippling effect on consumers, due to cross-side data and network effects, meaning that changes on one side of the platform will impact the benefits that the other side derives from its participation (McIntyre and Srinivasan, 2017; Gregory et al., 2022). The discrepancy between the expected quality of an offering and the quality perceived by consumers following the introduction of an AQR is likely to be amplified, as consumers will have an even harder time finding a satisficing offering, thereby increasing the search costs incurred by consumers. We anticipate a propagation of the negative consequences of the introduction of an AQR, leading to a decrease of not only the value captured by the platform, but also of the value created for its participants. Therefore, we expect that the introduction of an AQR will lead

to a decrease in the quality of the shared offerings:

Hypothesis 1: The introduction of an algorithmic *quantity* regulation in a sharing platform will lead to a decline in the *quality* ratings of platform offerings.

3.2 The Cushioning Effects of Trust-Building Signals

3.2.1 Differential Impact of Regulations

Regulation theory suggests that policy changes lead to distributive and sorting effects where some participants can end up more advantaged than others (Baldwin et al., 2011). In his seminal work, Stigler (2021) demonstrated that regulations — formerly viewed as public interest tools to correct market failures — can be "captured" by industry players, leading to outcomes that benefit incumbents over new or smaller entities. Similarly, Stiglitz and Pike (2004) highlighted how global regulations can have adverse effects on the poorest countries, which are the least able to bear the economic strain that these regulations impose.

Economists document a series of factors that contribute to the differential impact of regulations — including i) differences *regulatory intent* and *regulatory impact*, ii) structural inefficiencies, iii) unintended consequences, and iv) uneven distributional impacts (Bardhan, 1996). Past work on externality regulation has suggested that disadvantaged members of the society often end up bearing the burden of a regulation (Shapiro, 2016). Overall, both academics and policymakers have urged for considering the real-world implications of regulatory frameworks, and for calibrating regulations with precision to achieve their intended goals, so that they do not cause disproportionate harm (Bardhan, 1996; Sunstein, 2013).

Past work in sharing platforms has shown that disadvantaged participants, including those from lower socioeconomic backgrounds, often lack the resources, knowledge, and opportunities to quickly accumulate reviews, or meet the criteria required to earn trust-building signals (Slee, 2017; Scholz, 2016; Frenken and Schor, 2019). This creates a barrier to achieving trust and visibility within the platform, perpetuating a cycle of lower income and limited platform success. In addition, trust-building badges like superhosts are unevenly distributed to resource-rich and privileged participants, with these signals often also serving as cues for participants' race and financial status (Cohen and Sundararajan, 2015; Edelman and Luca, 2014; Rhue and Clark, 2022).

3.2.2 Organic & Designated Trust in Sharing Platforms

Motivated by these considerations, we examined the context of sharing platforms closely to identify heterogeneity inducing factors. Trust among strangers is the mainstay of the sharing discourse (Schor and Vallas, 2021). These trust-building signals are strongly linked to

consumption outcomes — thereby improving providers income and performance (Möhlmann, 2015). Thus, trust-building signals in a sharing platform go far beyond the social dimension of exchanges — they translate into a competitive advantage that allows providers to distinguish themselves, attract more consumers, and sustain their income (Luhmann, 2018). Technology enabled trust-building signals can even lead to price premiums, offering a competitive edge to providers who have accumulated, or have been designated, such signals (Ba and Pavlou, 2002).

In this work, we examined the heterogeneity introduced by two trust-building signals that have received notable attention from past literature — namely *host experience* and *superhost status* (Einav et al., 2016; Bockstedt and Goh, 2011). While both of them serve as important trust-building signals on sharing platforms, they do so in complementary ways. Host experience reflects the accumulated tenure of a provider on the platform, constituting an organic measure that signals trust based on historical performance. On the other hand, the superhost status is a formal designation awarded by the platform itself, which reflects not merely experience, but also adherence to specific performance criteria, including a high percentage of five star ratings, high response rates, and low cancellation rates.

3.2.3 Trust-Building Signal: Host Experience

The tenure of a sharing platform provider is strongly associated with the quality ratings received, constituting a critical factor for successful online exchanges (Einav et al., 2016). Provider experience contributes to better strategies for differentiation and improved discretion when navigating the market (Bockstedt and Goh, 2011). More prior experience on a platform is usually a favourable indicator, because those with greater experience are able to attain better revenues (Luo et al., 2021). Moreover, in online platforms, experienced providers can be strategic in adopting credibility or quality indicators (Li et al., 2009). In contrast, new providers in online marketplaces perform worse than their counterparts, and are more likely to receive negative ratings (Goes et al., 2013).

In sharing platforms, experienced providers have optimized offerings, which in turn leads to higher ratings and more reviews, making their offerings more appealing to consumers. Platform searches also favor algorithmically providers with proven track records, leading to even more visibility and revenues. New providers have a great disadvantage compared to their counterparts, as they had neither the opportunity, nor the time, to build a competitive profile. In sum, lack of experience can attenuate the ability of providers to offer quality services.

It is important to note that experience creates a positive feedback loop: more bookings lead to more reviews, which in turn lead to more bookings. The ability of an experienced

provider to provide high quality offerings is thus inherently greater compared to a new provider, who may struggle to break into the market. In addition, initial negative ratings will disproportionately affect new hosts. Early negative reviews can significantly affect an offering's total average ratings, whereas the same number of negative reviews would not affect an experienced host to the same extent, due to ratings that they have already accumulated. Finally, a very important aspect of provider inequality in sharing platforms is that providers from economically disadvantaged backgrounds may take longer to accumulate experience, due to difficulties in finding alternatives when trying to make their offerings available, or due to financial and social barriers. For instance, it may be harder for a financially strained single mother to find alternative accommodation prior to making her apartment available in Airbnb than for a resource-rich individual, and as a result, it might taker her more time to accumulate within platform experience.

We expect experienced providers to be better equipped to develop skills that can help them adapt and position better their offerings in a rapidly evolving competitive landscape due to the advent of the AQR. As a result, we expect information asymmetries to emerge between providers (Akerlof, 1970), with experienced providers accumulating valuable tacit knowledge that can help them reduce the negative effects of the AQR on their offerings. For example, a provider that has shared her asset for a prolonged period might be better equipped to assess what she can do to differentiate her offering, how the newly established constraints will affect her offering, how to price her offering most effectively, as well as when is the optimal period for her to make her asset available for sharing. Therefore, we expect that after the introduction of an AQR in a sharing platform, the ratings of listings hosted by inexperienced hosts will disproportionately decrease:

> **Hypothesis 2A:** The introduction of an algorithmic *quantity* regulation in a sharing platform will disproportionately decrease the *quality* ratings for inexperienced hosts.

3.2.4 Trust-Building Signal: Superhost Status

High quality products yield premiums as returns to seller reputations (Shapiro, 1983).Designated trust-building signals, such as superhost status, are considered reputation badges (Einav et al., 2016) and constitute platform based institutional structures whose perceived effectiveness can contribute toward building trust (Lu et al., 2021). Provider reputation has long been considered to serve as a differentiator that drives consumer perceptions regarding an offering's quality, and contributes in differentiating higher reputation providers from their counterparts (Sun et al., 2020; Dawar and Parker, 1994). However, as earning trust-building signals turns into a competition by itself, it is important to account not only for the biases that affect these trust-building mechanisms, but also the inequalities that allow hosts to earn

these badges.

First of all, platforms give higher visibility to participants who have reputation badges (Tadelis, 2016). Platform searches typically sort incumbent providers based on consumer generated ratings. However, this can severely reduce variety on the platform, by marginalizing diverse participants (Skiti et al., 2022). Serving as trust enhancing systems, these mechanisms allow reach and access to a wider base of consumers, leading to increased transactions, and thus revenues (Bolton et al., 2013). However, trust-building systems in platform markets can suffer from two issues. First, consumers could be drawing conclusions about the quality from a limited number of interactions. Second, the trust-building system itself could be prone to biases (Nosko and Tadelis, 2015).

Taken collectively, we expect providers who have not obtained trust-building badges to be less visible, have less bookings, and earn less revenue. This can significantly impede their ability to provide higher quality services. Moreover, sharing platforms often serve as a source of secondary income for providers from financially disadvantaged backgrounds. Such providers may not have the capital to significantly invest in offering improvements significantly impacting their ratings (Zhang et al., 2022).

Anecdotal evidence and self-reported host experiences in Airbnb suggest that obtaining a *superhost* badge requires meeting stringent standards, which can be more challenging for smaller or less privileged hosts. Additionally, existing biases on home-sharing platforms can amplify when less privileged hosts navigate the introduction of an AQR and its impact on their already meager sharing income. Issues of identity, race, and disadvantage in sharing platforms have been documented by several studies that examine inequality, including taste-based and statistical discrimination (Edelman and Luca, 2014; Laouénan and Rathelot, 2022). Providers who experience such discrimination, which affects their sharing income, are likely to be further constrained by the AQR, in addition to potentially facing unjust perceptions of lower quality. Taken together, these considerations lead us to hypothesize that while *superhosts* may be better off, their counterparts will bear an uneven burden from the AQR:

> **Hypothesis 2B:** The introduction of an algorithmic *quantity* regulation in a sharing platform will disproportionately decrease the *quality* ratings for non-superhosts.

3.3 External Effects

We expect that the introduction of an AQR will have broader effects that extend beyond the boundaries of the impacted sharing platform. Specifically, we expect that the introduction of the AQR will lead to external effects both on competing sharing platforms, as well as on

the housing market. Since the supply of residential housing is finite and largely overlapping, we expect that the effects of the AQR will also impact these closely related markets. We elaborate on these hypotheses below.

3.3.1 External Effects: Impact on Competing Platforms

While AQRs are increasingly being applied to the most dominant sharing platforms, such as Airbnb, other competing platforms continue to operate without implementing any regulations. Interestingly, and within our research context, Paris had in place regulations that limited short-term home sharing, with some of these regulations significantly predating sharing platforms (Schechner, 2017). The age of these regulations suggests that efforts to sustain the housing market have been ongoing for a while, however, due to the difficulty to implement, monitor, and impose these regulations, their effectiveness remains unknown.

We expect that the introduction of the AQR in Airbnb will lead to an *anticipatory effect* by also influencing the behavior of providers in competing platforms. Past work on gig platforms has argued that in the presence of uncertainty, and when facing algorithmic management, providers may display anticipatory compliance in order to ensure their future participation in the platform (Bucher et al., 2021). This is also in line with the notion of *prevention focus* used in regulatory focus theory, under which providers may demonstrate desirable behaviors to prevent negative scrutiny (Higgins, 2012). As such, we expect that providers on Airbnb's competing platforms will also demonstrate compliant behavior despite the absence of an AQR on their platform. Moreover, since providers often multihome in sharing platforms, learned behaviors from one platform may also reflect on other platforms where they operate. Due to the aforementioned, we anticipate a broader rippling effect of the introduction of an AQR on a sharing platform to its competitors:

> **Hypothesis 3A:** The introduction of an algorithmic *quantity* regulation in a sharing platform will have an *anticipatory effect*, leading providers on competing platforms to self-regulate by making their offerings unavailable.

3.3.2 External Effects: Impact on Housing Market

Home sharing platforms such as Airbnb have transformed housing market dynamics across the world. As these platforms have grown in popularity, there is a notable decrease in the supply of housing within the markets that these platforms operate, with past work suggesting that the greater the popularity of Airbnb within a city, the greater the decrease observed in these markets (Li et al., 2022).

The shift of housing supply from the housing market to sharing platforms inevitably

leads to a decrease in the supply of housing units and drives up prices, as more units move to the short term rental market in which sharing platforms operate (Barron et al., 2021). Empirical evidence across international contexts converge with these findings. For instance, studies from London show that increasing Airbnb density is associated with an increase in rental prices (Shabrina et al., 2022). Similarly, studies from Taiwan also indicate increasing rental prices (Chang, 2020).

The impact that sharing platforms have had on the housing market has prompted regulatory interventions across cities, with past work examining the implications of such regulations for the housing market. For instance, in Los Angeles, where short term rentals are subject to home-sharing ordinances, evidence points to a decline in housing prices (Koster et al., 2021). Additional empirical analyses indicate that regulatory interventions contribute to decelerating the conversion rate of housing stock to short-term rentals, while also keeping prices at bay (Huang et al., 2021).

However, the relationship between sharing economy regulation and housing market stabilization remains complex. Despite the unintended and negative consequences of sharing platforms on prices, studies also show that regulating Airbnb to restrict home-sharing is not a clear solution to residential woes. In fact, the presence of Airbnb incentivizes residential real estate investment (Bekkerman et al., 2023).

Despite a growing body of literature surrounding the relationship between home-sharing platforms and housing markets, there remains a significant gap in understanding the direct impact of AQRs on housing prices. Since AQRs typically limit the number of days a property can be rented out on short-term sharing platforms, we expect that it will incentivize providers to move some of their properties to the housing market. As a result, we expect that the introduction of an AQR in a sharing platform will increase the supply of housing stock in the residential housing market, leading to a decrease in housing price indices:

> **Hypothesis 3B:** The introduction of an algorithmic *quantity* regulation in a sharing platform will have a *spillover effect*, leading to a *decline* in housing prices.

4 Research Methodology

4.1 Research Setting

Airbnb is the world's largest and most popular home-sharing platform worldwide, with over 150 million users, 650,000 hosts and over 7 million offerings, meaning lodging listings. Airbnb connects housing providers and consumers of their services, optimizes their matching, and facilitates their interactions. Paris is Airbnb's largest market in terms of guest arrivals

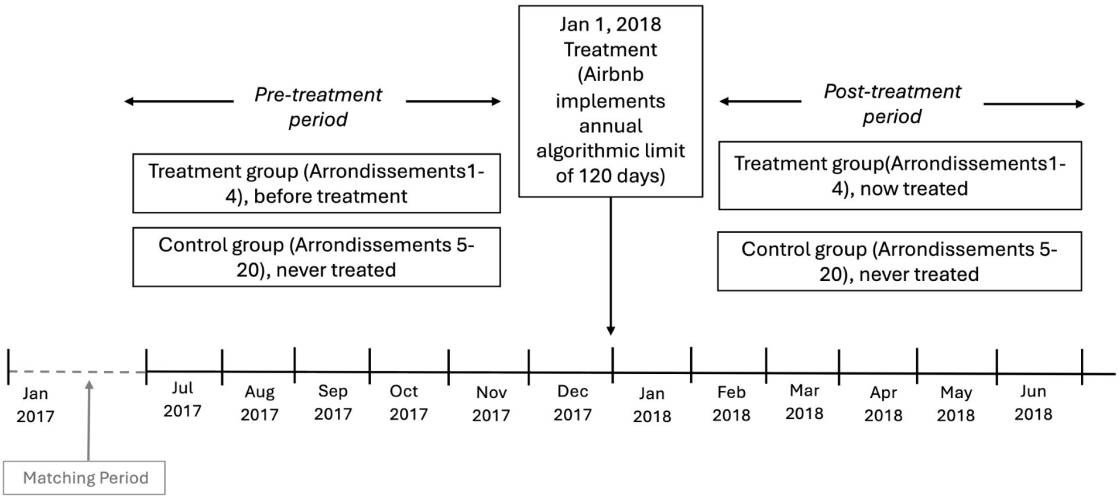
(Masson, 2023). Following a series of legal battles between the city of Paris and Airbnb (Leicester, 2018), the platform decided to automatically cap the number of days that an entire apartment could be booked to 120 days in four of the twenty Parisian arrondissements. The regulation was algorithmically enforced by Airbnb starting from January 1st, 2018 (Airbnb, 2017; Lomas, 2018). Although existing French laws stipulated that the 120-day limit should be applied across the entire city (Schechner, 2017), Airbnb chose to implement it partially, in the first four arrondissements. Similar automatic caps were also applied by Airbnb in London and Amsterdam (Schechner, 2017). However, we choose the city of Paris as our research setting because the AQR was applied to selected districts, facilitating a quasi-natural experiment.

4.2 Identification Strategy: Quasi Experimental Setup

Airbnb introduced an algorithmic quantity regulation to comply with local regulations for short-term apartment renting (Schechner, 2017), and despite the fact that its competitors in Paris did not comply with the regulation. The regulation was implemented as a feature on the platform, informing providers about the number of days remaining using a ticker on the hosts' home page, and imposing the regulation when necessary. This was an ideal setting to investigate the effect of an AQR on a leading sharing platform primarily for three reasons. First, the regulation was embedded as part of the platform's design, allowing us to directly capture its effect within the platform. Second, unlike Airbnb's approach in London and Amsterdam, where AQR was enforced comprehensively (Schechner, 2017), the AQR in Paris allows a causal research design since some listings were affected, whereas others were not. Finally, it set a precedent for algorithmically implemented quantity regulation where the responsibility of enforcement is shared between regulatory agencies and sharing platforms.

This allowed us to infer the causal effect of the AQR using a difference-in-differences specification, following exemplars of platform studies that have employed similar methodological approaches (e.g., Seamans and Zhu, 2014; Greenwood and Wattal, 2017; Burtch et al., 2018; Kircher and Foerderer, 2024). The main advantage of this statistical method is that it allows us to mimic an experimental design while using observational data, since the treatment affects only some units in the sample, while other units are not subjected to a similar change. Our dataset was sufficiently large, containing a six month of matching period, a six months of pre-treatment period, and a six month of post treatment period, making it suitable for the deployment of difference-in-differences analyses (Imbens and Wooldridge, 2007). Figure 1 illustrates our research design.

Figure 1: Research Design: Illustration



4.3 Data Description

Table 1 summarizes the datasets used in our analyses and presents summary statistics for all the variables used. We utilized three distinct datasets, each providing unique insights into different aspects of the home sharing and housing markets.

4.3.1 Dataset 1: Airbnb

Our first dataset contained public information on Airbnb listings which were obtained from a data activism organization, and included all Airbnb listings during the period examined. Our dependent variable, *Ratings*, was used as a proxy of quality, and its average of 92.85/100.00 suggested that listings were overall of high quality. The lodging capacity of each listing was captured by *Accommodates*, while *Log(Price)* reduced skewness typically present in raw data by containing log transformed values of prices. *Competition* was measured by counting the number of listings within the same arrondissement and price quartile, with an average of 216 competitive listings per observations, reflecting a highly competitive market. We accounted for the *Popularity* of a listing through the number of reviews it received per month. Using the geospatial coordinates of each listing, we calculated the distance of each listing from the city center of Paris in *Distance from City Center*. The distance of each Airbnb listing from the city centre of Paris was calculated using the Haversine formula (Inman, 1849). Used in navigation and geographic information systems, the Haversine formula computes the distance between two points on earth's surface, giving an "as-the-crow-flies" distance between two points on the globe. We computed distance d as follows:

$$d = 2r \cdot \arcsin\left(\sqrt{\sin^2\left(\frac{\Delta\phi}{2}\right)} + \cos(\phi_1) \cdot \cos(\phi_2) \cdot \sin^2\left(\frac{\Delta\lambda}{2}\right)\right) \tag{1}$$

Here, r depicts the radius of earth, approximately 6,371 kms, $\Delta\phi$ depicts the difference in latitude between two points, while $\Delta\lambda$ depicts the difference in longitude between two points. We additionally accounted for potential competition from different listing categories — namely *Private Rooms* and *Shared Apartments*, both of which were count variables capturing the number of listings within these listing categories. We further controlled for the various accommodation types within Airbnb, classified as *Apartments*, which were notably the largest group comprising of 95.31% of all listings, *Bed and Breakfast*, and *Other*.

Beyond our control variables, *Superhost* indicated listings that were provided by people that had obtained the superhost status, while *Host Experience* captured the experience of a provider in months. Both were used to test our second hypothesis in relation to the impact of trust-building signals.

Variables such as *30-day Availability*, *60-day Availability*, and *90-day Availability* were gathered from public information on the calendars of providers, and allowed estimates of the availability of a listing over the equivalent day periods. The aforementioned calendar availability measures, as well as the *Host Response Rate* and the *Instantly Bookable* measures, were used to examine the context at higher granularity and establish the proposed mechanisms.

4.3.2 Dataset 2: Vrbo

Our second dataset was obtained from a short-term rental analytics platform predominantly focusing on Vrbo, the most prominent competing platform for Airbnb. It included *Occupancy Rates*, our dependent variable, which captured the occupancy rate of a listing on the platform as a percentage variable showing how often a property was booked. In addition, we included controls such as *Average Daily Revenue (ADR)* earned from each listing, the number of *Reservations* of each Vrbo listing within a given month, and the number of days a listing was blocked off in *Blocked Days*. This dataset spanned from July 2017 to June 2018.

4.3.3 Dataset 3: Housing Price Index

Finally, we obtained longitudinal data on a housing price index in Paris from a housing analytics organization. This third dataset contained housing price indices per month and

arrondissement, depicted as *Price Index*, and was calculated by aggregating housing prices across home renting platforms operating within the city of Paris, and captured changes in residential housing prices for the entire period of interest (i.e., from January 2017 till December 2018).

Table 1: Summary Statistics

Variable	Description	Obs.	Mean	Std. Dev.	Min	Max
Dataset 1: Airbnb						
Ratings	Quality rating of a listing	102,294	92.849	6.994	20	100
Controls						
Accommodates	No. of guests accommodated	102,294	3.412	1.578	1	15
Log (Price)	Log of price per night	102,294	4.578	0.532	2.302	8.517
Competition	Listings in same price quartile, per month-arrondissement	102,294	216.263	102.575	20	797
Popularity	Popularity based on reviews received	102,294	1.186	1.785	0.01	238.46
Distance from city center	Haversine distance from city center (kms)	102,294	2.632	1.489	0.041	8.947
Private Rooms	No. of private rooms advertised, per month	102,294	273.756	152.139	61	585
Shared Apartments	No. of shared apartments advertised, per month	102,294	19.386	12.533	1	55
Category (%)						
Other	Listing type Others (base category in estimation)	4,761	4.65%			
Apartments	Listing type Apartment	97,501	95.31%			
Bed and breakfast	Listing type Bed &Breakfast	32	0.03%			
Total		102,294	100%			
Heterogeneous Effects						
Superhost	Indicator=1 if Host has reputation badge	102,294	0.0174	0.379	0	1
Host Experience	Host experience in months	102,294	44.700	18.177	6.533	118.967
Tests of Mechanisms						
30-day Availability	Calendar availability over next 30 days	102,294	5.153	8.057	0	30
60-day Availability	Calendar availability over next 60 days	102,294	12.817	17.342	0	60
90-day Availability	Calendar availability over next 90 days	102,294	22.181	27.681	0	90
Host Response Rate	Host Response Rate	71,363	95.286	13.838	0	100
Instantly Bookable	Indicator=1 if listing is instantly bookable	102,294	0.252	0.434	0	1
Dataset 2: Vrbo						
Occupancy Rate	Monthly occupancy rate for each Vrbo listing	100241	0.111	0.269	0	1
Blocked Days	No. of blocked days in a month	100241	6.556	11.692	0	31
ADR	Average Daily Revenue (USD)	18221	244.965	230.308	29.750	7450.450
Number of Reservations	No. of reservations in a month	100241	0.451	1.242	0	16
Dataset 3: Housing Price Indices						
Price Index	Housing price index	478	112.987	4.916	101.1	126.48

4.4 Comparability of Treatment & Control Groups for Valid Inferences

As mentioned, the investigated treatment of an algorithmically implemented quantity regulation was assigned to the innermost four arrondissements of Paris (1^{st}-4^{th}), which enabled us to use the listings in the remaining sixteen arrondissements (5^{th}-20^{th}) as our control group. Airbnb did not present a reason for their treatment selection, however, an argument for treating these arrondissements could potentially be that they were considered part of the most tourist-centric area of the city. We examined i) UNESCO World Heritage Sites, ii) landmarks mentioned in Wikipedia, as well as iii) landmarks and attractions on the official

Tourism Website of Paris, creating an exhaustive list of landmarks and attractions in Paris. We found that not all of the city's leading landmarks and attractions were located in the treated area. For example, the Eiffel Tower, Arc de Triomphe, Quartier Latin, Champs-Élysées, Parc des Princes, Stade Roland-Garros, Montmartre, as well as many other iconic tourist areas are not located within the treated arrondissements. This qualitative examination revealed that the city's leading attractions are not concentrated solely within the treated arrondissements, but rather distributed across the city, disproving the notion that the treatment was systematically applied to all the highly touristic areas of the city of Paris.

4.5 Coarsened Exact Matching

To further ensure that our treatment units did not have systematically different characteristics from our control units, we deployed k2k Coarsened Exact Matching (CEM) on our baseline data sample. The goal of this matching approach was to identify control and treated units that were as similar to each other as possible. Following Iacus et al. (2012), we deployed Coarsened Exact Matching (CEM) to minimize pre-treatment differences between our treated and control units. CEM bounds two factors: i) the degree of model dependence, and ii) causal effect estimation error by ex-ante user choice, while lowering bias and increasing efficiency (Iacus et al., 2012). Our stringent matching approach improved the validity of causal inference and ensured that the control units constituted robust counterfactuals to the treated units.

By implementing a k2k match, we ensured that our matching algorithm produced an equal number of treated and control units within each stratum of coarsened variables (Iacus et al., 2012). Before matching, we ensured high coverage in our dataset by including only listings for which we had data over the entire 18 months that our study focused on — from January 2017 to June 2018 — and we matched treated and control listings on the following six characteristics. First, we use *Accommodates*, since listings could differ in the number of people that they accommodate. Second, *Log(Price)* was used to account for potential differences in prices. Third, *Competition* was used to ensure that listings face similar external competition from other similarly priced listings. Fourth, we obtained listings with similar calendar availability by using *30-day Availability*. Fifth, we used *Popularity* to further reduce the risk of comparing dissimilar listings in terms of their popularity. Sixth, we used our dependent variable *Ratings* to prevent pre-treatment differences between treated and control units.

Following Kircher and Foerderer (2024), as well as Sun and Zhu (2013), we matched our observations four months prior to the announcement of the AQR. The announcement

took place on October 1st, 2017, and we matched our units on averages of covariates between January 2017 and May 2017. Following Iacus et al. (2012) we used cutoff points based on our *"knowledge of the measurement scale"*. Thus our dataset was divided into three six-month periods: i) a matching period from January 2017 to May 2017, followed by ii) a pre-treatment period from July 2017 to December 2017, which was then followed by iii) a post-treatment period from January 2018 to June 2018. Our matching strategy helped us identify the optimal units to minimize differences between our treatment and control groups, and we proceeded by using their corresponding uncoarsened values as the baseline CEM matched sample for analyses (Iacus et al., 2012). Details on the number of observations for each time period are presented in our Appendix (Table 13).

5 Results

5.1 Algorithmic Quantity Regulation's Impact on Quality

We estimated the impact of the AQR on the quality ratings of listings within the treated group using the following fixed effects estimation (Angrist and Pischke, 2009):

$$\begin{aligned} \text{Listing Rating}_{i,t} = \beta_0 + \beta_1(\text{Treat}_i \times \text{After}_t) \\ + \psi_i + \phi_t + \theta i, t + \epsilon_{i,t} \end{aligned} \tag{2}$$

Here, *Listing Rating* depicts the dependent variable in month t for listing i. β_1 depicts the key coefficient of interest, which captured the effect of the AQR as a product of the post-treatment indicator. The listing fixed effect is depicted as ψ_i, while ϕ_t indicates the month fixed effect, with these effects respectively accounting for time variant heterogeneity within and across listings. $\theta i, t$ indicates a vector containing our control variables.

Specifically, *Accommodates* controlled for the fact that listings differed in the number of guests that they could accommodate, while *Log (Price)* accounted for differences in the prices of listings across the city. We also controlled for *Competition* by other listings within the same price quartile and arrondissement, for differences in the *Popularity* of listings, and for the geospatial heterogeneity between listings by using *Distance from City Center*. Since *Shared Apartments* and *Private Rooms* rooms could potentially serve as alternatives to the listings investigated, we also controlled for the number of private rooms and shared listings advertised on the platform. Finally, we added controls for the *Category* of the listing, since different types of listings could accrue different ratings. Similar to Kircher and Foerderer (2024), we did not control for 30-day availability since it is directly affected by the AQR, and thus considered a "bad control" (Wooldridge, 2016), and it *"might just as well*

be dependent variable too" (Angrist and Pischke, 2009, p.64). We also deployed listing-level clustered standard errors to account for potential serial correlation and heteroskedacity in the residuals (Bertrand et al., 2004).

Table 2: Impact of AQR on Treated Listings in Central Paris

	(1)	(2)
	DV= Rating	
	Baseline	Controls
AQR Impact	-0.064*	-0.066*
	(0.032)	(0.032)
Treated	0.508	1.043
	(0.315)	(1.453)
After	0.115***	0.111***
	(0.023)	(0.023)
Accommodates		-0.109
		(0.061)
Log(Price)		-0.009
		(0.106)
Competition (month, arr)		0.000
		(0.000)
Popularity		0.003*
		(0.001)
Distance from city center (kms)		0.561
		(1.549)
Private Rooms		0.000
		(0.000)
Shared Apartments		0.000
		(0.001)
Category		
Apartment		-0.090
		(0.085)
Bed and Breakfast		-0.262
		(0.345)
Constant	92.674***	91.505***
	(0.083)	(4.375)
Observations	102,294	102,294
Listing FE	✓	✓
Month-Year FE	✓	✓
Adjusted R^2	0.002	0.002
F	4.194***	2.821***

Standard errors in parantheses clustered at the listing level
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2 summarizes the estimates obtained from this difference-in-differences estimation. Column (1) presents the unrestricted model, serving as the baseline specification. We found a statistically significant and negative coefficient for *AQR Impact*, providing support for our first hypothesis. On average, treated listings showed a 6.4% decline in their ratings. Column (2) includes controls and presents consistent results, with the treated listings exhibiting once

again a negative and significant decline in their ratings of 6.6%.

We further verified the observed decline in ratings as a consequence of the AQR, and demonstrated that our results were not prone to multicollinearity by examining the VIFs for both our main specification (Appendix Table 14), as well as for each of the control variables using a variance inflation matrix (Appendix Table 15).

We ensured that our work was not prone to issues related to heteroskedasticity, omitted variables, as well as autocorrelation by using robust standard errors (Appendix Table 16). First, robust standard errors account for heteroskedasticity by providing consistent standard errors even when the variance of the error terms is not constant across observations, thereby ensuring that statistical inferences remain valid. Second, robust standard errors address concerns related to omitted variables by making models less sensitive to any potential bias from variables not included in the specification. Third, robust standard errors handle autocorrelation by adjusting standard errors to account for the correlation between residuals over time. We also tested for the effect on a longer, twelve month period and found that the effect lasts over a longer duration post treatment (Appendix Table17). In sum, our results remained robust across these alternative specifications.

5.2 Heterogeneous Effects & the Impact of Trust-Building Signals

In order to test our second hypothesis on the potentially heterogeneous effects of the AQR across platforms participants, we interacted the difference-in-differences estimator with our two moderating variables, namely *Host Experience* and *Superhost*. Following Kircher and Foerderer (2024), our model estimates:

$$\begin{aligned} \text{Listing Rating}_{i,t} = \beta_0 &+ \beta_1(\text{After}_t \times X_{i/h}) \\ &+ \beta_2(\text{Treat}_i \times \text{After}_t) \\ &+ \beta_3(\text{Treat}_i \times \text{After}_t \times X_{i/h}) \\ &+ \psi_i + \phi_t + \phi_{i,t} + \epsilon_{i,t} \end{aligned} \tag{3}$$

This notation, including its control variables, is identical to notation (2) mentioned above, except that $X_{i/h}$ contained the moderator variable. Our key coefficients of interest were β_2 which contained the main difference-in-differences effect, and β_3, which reported the heterogeneous effect of each moderating variable.

Table 3 presents the results associated with the heterogeneous effects of both of the trust-building signals examined, namely *Host Experience* and *Superhost*. Column (1) presents the effect of *Host Experience* — captured by *Treated=1 X After=1 X Host Experience* — as positive and significant. This result confirmed Hypothesis 2A, providing evidence that

Table 3: Heterogeneous Effects by Listing & Host Characteristics

	(1)	(2)	(3)
	DV=Ratings		
	Heterogeneous Impact by		
	Host Experience	Superhost Status	Joint Model
AQR Impact	-0.154^{+}	-0.087*	-0.177^{+}
	(0.091)	(0.039)	(0.092)
Host Experience (Months)	0.002		0.002
	(0.002)		(0.002)
Treated=1 × Host Experience (Months)	-0.017**		-0.016**
	(0.006)		(0.006)
Treated=1 × After=1 × Host Experience (Months)	0.004*		0.004*
	(0.002)		(0.002)
Superhost=1		0.256***	0.254***
		(0.024)	(0.024)
Treated=1 × Superhost=1		-0.124**	-0.118**
		(0.046)	(0.046)
Treated=1 × After=1 × Superhost=1		0.125*	0.118*
		(0.053)	(0.053)
Accommodates	-0.109	-0.108	-0.108
	(0.061)	(0.061)	(0.061)
Log(Price)	-0.014	-0.027	-0.031
	(0.106)	(0.106)	(0.105)
Competition (month, arr)	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Popularity	0.003*	0.003*	0.003*
	(0.001)	(0.001)	(0.001)
Distance from city center (kms)	0.607	0.595	0.639
	(1.540)	(1.551)	(1.542)
Private Rooms	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Shared Apartments	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
Category			
Apartment	-0.083	-0.085	-0.078
	(0.085)	(0.085)	(0.085)
Bed and Breakfast	-0.259	-0.265	-0.262
	(0.345)	(0.345)	(0.346)
Constant	91.215***	91.467***	91.188***
	(4.360)	(4.382)	(4.368)
Observations	102,294	102,294	102,294
Listing FE	✓	✓	✓
Month-Year FE	✓	✓	✓
Adjusted R^2	0.003	0.005	0.006
F	3.242***	8.859***	8.309***

Standard errors in parenthesis clustered at the listing level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

$^{+}$ denotes p-values <0.10, corresponding to 0.092 for Column (1) and 0.055 for Column (3)

experienced providers were better off than their counterparts. Similarly, the positive and significant coefficient in *Treated=1 X After=1 X Superhost=1*, as presented in Column (2) provided support for hypothesis 2B, confirming that superhosts were also less impacted by the introduction of the AQR (Table 3). Finally, for robustness purposes, we included a joint specification of our results with all heterogeneous effects within a single model (Table 3, Column 3). Our results consistently showed that *Experienced* and *Superhost* providers were indeed *cushioned* from the adverse effects of the AQR, while inexperienced hosts as well as hosts that had not obtained the superhost badge were worse off.

5.3 Mechanisms

We built our arguments regarding the expected ratings decline due to the AQR in two ways. First, we expected that the supply side restriction imposed by the AQR was likely to lead to a contraction in the available market supply. This constraint could make it even more unlikely for consumers to identify high quality offerings that fully meet their preferences, resulting to an *expectation gap*, where their expectations and experiences are incongruent, which will subsequently be reflected in lower quality ratings.

Secondly, we expected that as AQRs limit sharing opportunities by design, they disincentivized providers, and due to providers' declining revenues, their available resources to maintain the same levels of quality standards would also be reduced. As a result, we expected that the providers' reduced income would be then reflected in the lowered quality of their services, and their maintenance capabilities. We proceed by providing empirical tests for both of these mechanisms. Specifically, we first illustrate the contraction in supply, by providing evidence of the declining availability within the treated area of Paris. In addition, use two split sample analyses to provide evidence of declining revenues as a mechanism for the declining ratings observed.

5.3.1 Declining Availability of Listings

A necessary precondition for the validity of our results is that the AQR indeed accomplished its intended goal — restricting sharing in Paris. We used publicly available data on listing calendars to longitudinally obtain their availability over the next 30 days. This information was based on providers' voluntary decisions to share or not share their listing during specific dates.

If the AQR was indeed successful, we should observe a decline in the availability of the treated listings. We proceeded by examining the evolution of listings 30-day availability graphically, with Figure 4 (Appendix) providing estimation-free evidence for the declining

availability of listings over time. The observed decline is indicative of the effectiveness of the AQR. Next, we used *30-day Availability* as our dependent variable, and estimated the post-treatment effect of the AQR on the treated listings (Table 4). Our results showed a negative and significant coefficient for *After AQR* in Column (1), providing evidence that the AQR led to a decline in the availability of listings. As a robustness test, we also estimated listing availability over the next 60 and 90 days, with Columns (2) and (3) confirming the declining availability of listings after the introduction of the AQR (Table 4), and providing evidence of declining revenues and a contraction in the available supply within the platform.

Table 4: Test of Availability-Based Mechanisms for Treated Listings

	(1)	(2)	(3)
	DV=*Calendar Availability*		
	30-day	60-day	90-day
After AQR	-5.947***	-13.121***	-14.938***
	(0.227)	(0.462)	(0.693)
Accommodates	-0.312	-0.236	-0.469
	(0.456)	(0.894)	(1.426)
Log(Price)	3.032**	7.297***	9.976**
	(0.948)	(2.033)	(3.258)
Competition (month, arr)	-0.006	-0.014	-0.015
	(0.004)	(0.010)	(0.016)
Popularity	-0.043*	-0.120*	-0.206*
	(0.022)	(0.052)	(0.084)
Distance from city center (kms)	3.041**	1.218	1.784
	(1.016)	(2.437)	(4.539)
Private Rooms	0.032**	0.056*	0.067
	(0.011)	(0.023)	(0.036)
Shared Apartments	-0.033	-0.077*	-0.120*
	(0.017)	(0.035)	(0.051)
Category			
Apartments	0.039	-0.017	-0.812
	(0.498)	(1.221)	(1.948)
Constant	-10.669*	-16.919	-18.523
	(4.675)	(10.039)	(16.127)
Observations	26,358	26,358	26,358
Listing FE	✓	✓	✓
Month-Year FE	✓	✓	✓
Adjusted R^2	0.152	0.157	0.154
F	104.761**	98.908**	83.860**

Standard errors in parentheses clustered at the listing level
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
This estimation drops the category Bed and Breakfast automatically.

5.3.2 Declining Revenues of Listings

We argued that the observed decline in ratings could be primarily attributed to the reduction in provider revenues following the introduction of the AQR. In the past, studies on airline

regulation have identified how declining profitability for a provider leads to declining service quality (Rose, 1990). In our setting, although the decrease in availability discussed above offers some evidence of declining revenues, it is important to recognize that publicly listed calendar availability does not differentiate between actual bookings and a host's choice to withhold the listing from the market. Therefore, to further substantiate the claim that declining revenues contribute to the observed decrease in ratings, we turn to additional evidence.

In an ideal scenario, we would have access to fine-grained panel data detailing the monthly revenues earned by each listing, which would include a comprehensive breakdown of factors such as bookings, cancellations, security and cleaning fees, charges for additional guests, as well as other charges (e.g., fees for delayed or early check-in). However, such detailed financial data are not publicly shared, neither by Airbnb, nor by its hosts. To navigate this challenge, we adopt an approach based on the precedents set by Pierce et al. (2015) as well as Kircher and Foerderer (2024). Specifically, we test predictions that should be supported if our theorized mechanism is indeed driving the observed outcomes. In other words, we test for predictions that must hold true if declining revenues are the cause of the declining ratings.

We conducted two split-sample analyses using *Host Response Rate*, and *Instant Booking* (Table 5). These two attributes were intentionally selected as they could help us differentiate between listings with low revenues, and listings with high revenues. We began by dividing our observations into subsamples based on each of these characteristics, and then estimated our regressions within each subsample. This split-sample approach enabled us to examine whether the theorized mechanism was present in subsamples where it was expected to hold, and whether it did not hold in subsamples lacking the relevant characteristic (Williams, 2015). Thus, this approach allowed us to empirically test the revenue-based mechanisms that we expected.

Host Response Rate was selected as a predictor of revenues, as hosts that are highly responsive on the platform are likely to earn more revenue than their less responsive counterparts. Similarly, we also split our sample based on whether a listing was available for *Instant Booking*, indicating that the provider of that listing allowed consumers to make an instant booking by automating the guest screening process. Anecdotal evidence suggests that instant bookings can lead to increased income from listings due to higher occupancy rates, as well as greater visibility and flexibility (Airbnb Community Center, 2017).

We first split our data based on whether the *Host Response Rate* was above or below the median level, and subsequently estimated the decline in ratings (Table 5). Our results showed that listings from hosts with a lower response rate experienced a greater decline in their

Table 5: Test of Revenue-based Mechanisms

	Dependent variable = *Ratings*			
	Split = Active Host		Split = Instantly Bookable	
	Below median	Above median	No	Yes
AQR Impact	-0.094**	-0.029	-0.054^{+}	-0.015
	(0.040)	(0.026)	(0.032)	(0.070)
Chow Test (F)	4.47**		9.65***	
Observations	71,368	30,931	76,547	25,747
Controls	✓	✓	✓	✓
Listing FE	✓	✓	✓	✓
Month-year FE	✓	✓	✓	✓
Adjusted R^2	0.000	0.022	0.014	0.009
Wald Chi^2	50.84***	137.36***	60.18***	2057.47***

Standard errors in parentheses

* p<(0.05) **p<(0.01) *** p<(0.001)

$^{+}$ denotes p-values<0.10, corresponding to -0.054 for Instantly Bookable = No

ratings, as indicated in the negative and significant coefficient of *AQR Impact*. Conversely, listings from hosts with higher response rates did not experience a significant decline in their ratings. While the two coefficients clearly differed, we further validated this difference using a Chow test to determine the statistical significance of the discrepancy between these estimates (Wooldridge, 2016), with the F-test statistic confirming the significantly different impact of the AQR across these two groups.

The analysis of our second split sample revealed that listings without *Instant Booking* experienced a steeper decline in their ratings, with the observed effect being significant at the 0.10 level. In contrast, we did not find a statistically significant decline in the ratings of listings that allowed instant bookings, with the Chow test confirming that the estimated coefficients were indeed different.

Our findings consistently showed that listings likely to be earning lower revenues were more negatively impacted by the introduction of the AQR compared to listings likely earning higher revenues. These results provide additional support for our hypothesized mechanisms related to the observed decline in ratings, while also highlighting the heterogeneous effects of the revenue-based dynamics at play.

5.4 External Effect on Competing Platform

Our quasi-natural experiment focused on the introduction of an AQR on Airbnb. Although Airbnb was the only home-sharing platform to implement such a restriction, the existing regulatory framework applies to all platforms in Paris. We investigated whether the introduction of the AQR on Airbnb had external effects by influencing competing platforms, such as Vrbo. We hypothesized that the AQR could trigger *anticipatory regulation* on competing

platforms, prompting providers to preemptively restrict the sharing of their listings before any formal restriction was introduced on these platforms. Our research design offers an ideal setting to explore the effect of the AQR on a competing platform, given that France already had laws in place limiting short-term home sharing to 120 days per year. To test this, we estimated a difference-in-differences specification using our second dataset.

Table 6: External Effects: Impact of AQR on Competing Platform

	(1)	(2)
	DV=Occupancy Rate	
AQR Impact	-0.009**	-0.036**
	(0.004)	(0.014)
Before/After	-0.329***	0.015
	(0.006)	(0.013)
ADR (USD)		-0.000**
		(0.000)
Blocked Days		0.004***
		(0.001)
Reservations		0.047***
		(0.002)
Constant	0.397***	0.580***
	(0.004)	(0.013)
Observations	100,241	18,221
Listing FE	✓	✓
Month FE	✓	✓
Adjusted R^2	0.160	0.124
F	409.70 ***	121.75***

Standard errors in parentheses clustered at the listing level

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

Treatment Indicator fully absorbed and dropped off

Table 6 summarizes the effect that the introduction of the AQR on Airbnb had on Vrbo, i.e., a competing unregulated platform. The negative and significant coefficient for *AQR Impact* in Column (1) confirmed a decline in occupancy rates of Vrbo offerings in the baseline model on the full dataset spanning six months (N=100,241). Next, we included controls. The results presented in Column (2) reaffirmed the declining occupancy rate, even after accounting for these controls. Although the effect size was relatively small (-0.036), it provides evidence of the anticipatory regulation effect driven by providers' potential self-regulation in response to the AQR (Table 6). We examine the VIF matrix for all covariates and confirmed that our models did not suffer from multicollinearity (Appendix Table 18).

5.5 External Effect on Housing Prices

Using data on housing price indices in the city of Paris (Dataset 3), we estimated an additional difference-in-differences model to evaluate the effect of AQR on housing prices. The index was benchmarked to a base value of 100, which represented a reference property in

April 2011 with a total surface of 40m^2, two rooms, one bathroom, and located on the second floor. Thus, an index value greater than 100 indicates an increase in housing prices relative to the base period, while a value less than 100 indicates a decrease.

Table 7: External Effects: Impact on Housing Price Index

	(1)
	DV=Housing Price Index
AQR Impact	-0.402*
	(0.174)
Before/After Indicator	14.680***
	(0.243)
Constant	104.916***
	(0.170)
Observations	478
Adjusted R^2	0.970
F	633.890 ***

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The housing price index was calculated using the hedonic method, which adjusts for variations in the quality and characteristics of properties, ensuring that the index reflects true market trends. Unlike other methods which can be distorted by changes in the types of properties sold, such as a shift toward higher or lower-end properties in the market, the hedonic method isolates the impact of specific features (e.g., location, surface, and age) on the price. This allows the index to reflect pure price changes in the housing market, rather than changes in the composition of sales.

Table 7 summarizes our findings. The negative and significant coefficient for *AQR Impact* suggests a decline in housing prices within the treated area, indicating that the introduction of the AQR had a significant effect on housing prices. This decline was consistent with our hypothesis 3B, and the anticipated market response to the introduction of the AQR.

5.6 Robustness and Falsification Tests

5.6.1 Parallel Trends

We conducted a series of parallel trends tests by specifying treatment periods before the AQR was implemented, to ensure that there were no pre-existing differences in our covariates, paying particular attention to our dependent variable — *Ratings* (Table 8). The non-significant coefficients for the time periods preceding the introduction of the AQR confirm the presence of parallel trends for our dependent variable in Column (1). We also tested for the presence of parallel trends in Columns (2), (3), and (4) for time varying listing level covariates, and confirmed that no significant pre-treatment existed.

We proceeded by examining the presence of parallel trends for the dependent variables used in our external effects analyses. Specifically, for the anticipatory effect on competing platforms, we estimated the parallel trends based on the *Occupancy Rates* of Vrbo (Table 8). We note that in our unrestricted model without controls, a difference in trends appears in pre-treatment periods. Following advice on conditional parallel trends by Sant'Anna and Zhao (2020), we added control variables to obtain conditional parallel trends (Table 8). The introduction of these controls yielded a conditional parallel trend that goes back up to five time periods before the treatment period, providing additional support to our results. Finally, we tested for parallel trends in our third difference-in-differences specification, which used Paris housing prices data. Table 8 also confirmed the presence of parallel trends in the six months preceding the introduction of the AQR for our dependent variable, namely *Price Index*, providing once again support for the results reported.

Table 8: Test of Parallel Trends

	Dataset 1 (Airbnb)				Dataset 2 (Vrbo)		Dataset 3 (Housing)
	Ratings	Accommodates	Log(Price)	Popularity	Occupancy	Occupancy *(Conditional PT)*	Price Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat × t-1	0.036	0.002	0.001	0.006	-0.030***	0.013	0.299
	(0.023)	(0.004)	(0.002)	(0.023)	(0.005)	(0.023)	(0.441)
Treat × t-2	0.045	0.002	-0.000	-0.013	-0.016***	-0.024	0.271
	(0.029)	(0.004)	(0.002)	(0.018)	(0.004)	(0.021)	(0.444)
Treat × t-3	0.061	-0.000	-0.000	-0.013	-0.014***	-0.012	0.076
	(0.033)	(0.005)	(0.002)	(0.018)	(0.005)	(0.017)	(0.444)
Treat × t-4	0.057	-0.000	-0.000	-0.002	-0.012	0.026	-0.083
	(0.039)	(0.005)	(0.002)	(0.022)	(0.006)	(0.016)	(0.441)
Treat × t-5	0.070	-0.004	-0.001	0.004	-0.014*	0.021	-0.139
	(0.044)	(0.005)	(0.002)	(0.030)	(0.005)	(0.016)	(0.441)
Treat × t-6	0.116*	-0.002	0.000	-0.032	0.006	0.036 **	-0.132
	(0.049)	(0.006)	(0.002)	(0.019)	(0.007)	(0.013)	(0.441)
Constant	92.775 ***	3.424***	4.572***	1.225***	0.396***	0.580***	104.916***
	(0.018)	(0.003)	(0.001)	(0.006)	(0.004)	(0.013)	(0.172)
Observations	102,294	102,294	102,294	102,294	100,241	18,221	478
Listing FE	✓	✓	✓	✓	✓	✓	✓
Month-year FE	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.002	0.003	0.014	0.001	0.160	0.124	0.969
F	3.276***	3.753***	19.998***	26.670***	298.030***	93.500***	513.413***

Standard errors in parentheses clustered at listing level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.6.2 Matching Effectiveness & Handling Imbalance

We also report our test of imbalance before and after using a CEM k2k matching approach (Table 9). While this matching approach reduces imbalance for the dependent variable, residual imbalances remain. Following Blackwell et al. (2009, p.537), we control for the remaining imbalance by adding these variables to our regression specifications, with the exception of 30-day availability, which as mentioned before, acts as a "bad control" (Wooldridge, 2016).

Table 9: k2k Coarsened Exact Matching: Imbalance & t-tests

	Unmatched Sample				Matched Sample			
	Control N=78,864	Treatment N=13,296	Diff. (Means)	t-test p-val	Control N=37,966	Treatment N=13,182	Diff. (Means)	t-test p-val
Rating	0.026	0.055	-0.279	0.000	92.810	92.859	-0.049	0.490
Accommodates	3.281	3.467	-0.186	0.000	3.409	3.446	-0.03	0.019
Log (Price)	0.706	1.063	-0.356	0.000	4.512	4.748	-0.236	0.000
30-day Av	5.613	6.683	-1.249	0.000	6.147	6.849	-0.702	0.000
Competition	296.547	200.087	96.460	0.000	221.679	199.156	22.523	0.000
Popularity	0.921	1.436	-0.515	0.000	1.122	1.435	-0.313	0.000

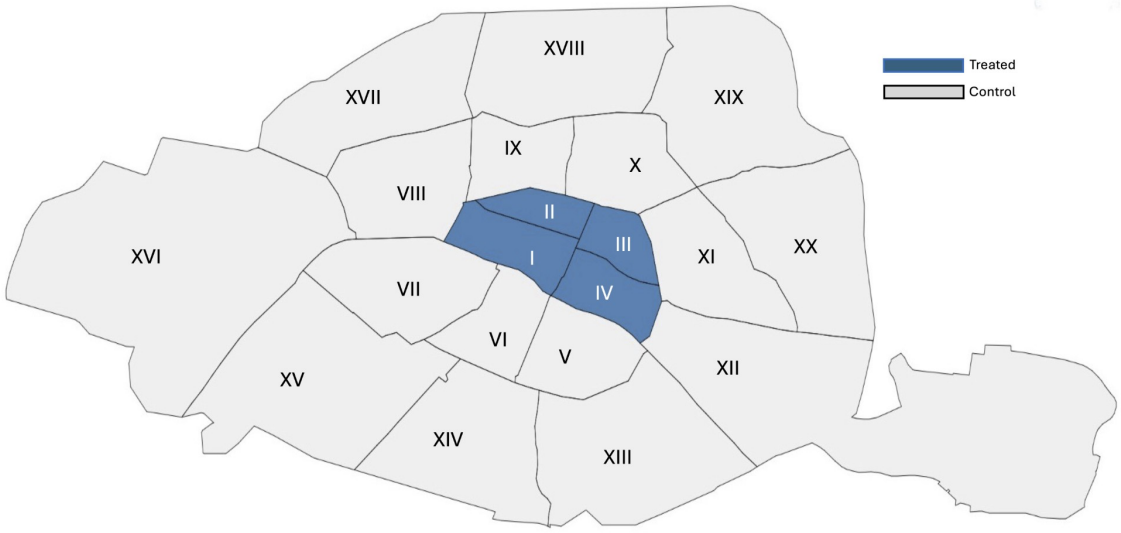
5.6.3 Stable Unit Treatment Value Assumption (SUTVA)

We test for the Stable Unit Treatment Value Assumption (SUTVA) (Rubin, 1980) in our research design to ensure the validity of our causal inferences. SUTVA requires that there are no unmodeled spillovers and rests on two key assumptions. First, the treatment must be consistent across all listings, without variation in its application regarding both the *extent* and *timing* of the intervention, which is true in our research setting. Second, the outcome for each treated listing must depend solely on its own treatment status, with no influence from the treatment status of other listings, thereby eliminating any potential spillover effects (Rubin, 1980).

We proceeded by examining the SUTVA assumption in our context. Assuming that the demand for Airbnb listings stayed the same before and after the introduction of the AQR, consumers whose preferences were geared toward i) location and ii) entire apartments, could shift their demand to entire apartments listed in the arrondissements directly adjacent to the ones that were impacted by the AQR. Therefore, we expected a *demand spillover* to occur. This would lead such listings to gain new bookings and earn increased revenues. Incentivized by these gains, such listings are likely to receive both, an increased number of ratings in a month, and potentially increase their ratings by improving the quality of services. Should this happen, it would imply a compositional shift in our control group. In longitudinal studies, particularly those utilizing difference-in-difference methodologies, a critical assumption is that the composition of the control group stays stable over time. A compositional shift would indicate changes in the characteristics of the control group, which can potentially introduce biases and confound results (Heckman et al., 1997).

We proceeded to test for SUTVA violations by first excluding our actual treated group from the analysis, i.e., listings in the 1^{st}-4^{th} arrondissements from our CEM matched sample. Next, after visually inspecting spiral layout of the Paris arrondissements as depicted in Figure 2, we found that the 5^{th}-12^{th} arrondissements are directly adjacent to the treated ones. Next, we examined whether listings in these adjacent arrondissements indeed experienced

Figure 2: Map of Paris



any significant changes to their ratings by deploying a difference-in-differences estimation, using the listings in adjacent arrondissements as our treated group, and the remaining listings (13^{st}-20^{th} arrondissements) as our control group. Thus, we estimated the effect of the AQR on a placebo treatment group, i.e, listings whose location was near the treated area, but were not actually treated. If there were indeed significant spillover effects, the ratings to these listings should show a significant difference. The results of our analysis, showed that there were no significant changes in the ratings of these listings as a result of the introduction of the AQR (Table 10, Column (1)). To the best of our understanding, we expect that this may be a consequence of demand diffusion throughout the control group, so that even though a demand spillover occured, it did not significantly affect the quality ratings in adjacent arrondissements.

As an additional test to ensure that our results were not prone to SUTVA violations, we use the distance from the city center on our baseline matched dataset to deploy a k2k CEM match. Our strict geospatial matching resulted in a reduced sample of 20,698 listings, with all listings selected by our algorithm being within a maximum distance of 2.4 kms from the city center. This created a second placebo group of listings in adjacent arrondissements that were geospatially closest to the actual treated listings. We then compared the listings that were close to the city center and in adjacent but not affected arrondissements, with listings that were also within a similar distance from the city center but were not located in adjacent arrondissements. The results once again confirmed that there was no spillover effect from the treated to the control group (Table 10, Column (2)).

Table 10: SUTVA Assumptions & Geospatial Spillover

	DV=Ratings	
	Baseline CEM Sample	Distance based k2k CEM sample
	(1)	(2)
AQR Impact	-0.020	0.704
	(0.027)	(0.576)
Before/After	0.149***	-0.568
	(0.028)	(0.576)
Accomodates	-0.090	0.030
	(0.057)	(0.183)
Log(Price)	-0.070	-0.756
	(0.117)	(0.406)
Competition (month, arr)	-0.000	0.000
	(0.001)	(0.001)
Popularity	0.003	0.002
	(0.002)	(0.008)
Distance from city center (kms)	4.025	-3.797
	(5.227)	(7.952)
Private Rooms	0.000	-0.001
	(0.000)	(0.001)
Shared Apartments	0.000	0.008*
	(0.001)	(0.003)
Category		
Apartments	-0.055	0.029
	(0.068)	(0.111)
Bed and Breakfasts	-0.250	0.000
	(0.348)	(.)
Constant	79.948***	101.647***
	(17.752)	(10.764)
Observations	75,936	10,349
Listing FE	✓	✓
Month-Year FE	✓	✓
Adjusted R^2	0.004	0.004
F	2.902***	1.378***

Standard errors in parantheses clustered at the listing level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.6.4 Effects on Shared Apartments

When entire apartment listings in the treated area were limited to being shared up to 120 days, their limited availability led to a contraction in market supply. An alternative hypothesis could be that guests who had a strong preference in terms of location - i.e., those that prioritized accommodation within the 1^{st}-4^{th} arrondissements, could simply shift demand to other type of apartments that do not face any restrictions, such as *Shared Apartments* and *Private Rooms*. We expected that such a demand spillover would significantly affect the ratings of these untreated units that were located within the treated area. Using our difference-in-differences setup, we examined the impact of the AQR on a dataset of shared apartments and private rooms in Paris. We found no significant impact of the AQR on the

untreated units, even though their geospatial location lied within the treated area. This result confirmed that only listings that were subject to the treatment showed the observed decline in quality (Table 11), serving as a falsification test.

Table 11: Falsification Test: Effect on Shared Apartments

	DV=Ratings	
	(1)	(2)
AQR Impact	-0.118	-0.106
	(0.078)	(0.078)
Treated/Control Indicator	0.181	-0.716
	(0.823)	(0.801)
Before/After Indicator	-0.038	-0.043
	(0.049)	(0.051)
Accomodates		0.072
		(0.200)
Log (Price)		-0.494*
		(0.195)
Competition(month, arr)		-0.002
		(0.001)
Popularity		0.001
		(0.009)
Distance from city center (kms)		-0.816*
		(0.395)
Category		
Apartments		0.054
		(0.164)
Bed and Breakfasts		-0.140
		(0.133)
Constant	92.538***	97.215***
	(0.075)	(1.592)
Observations	70,224	70,205
Listing FE	✓	✓
Month-Year FE	✓	✓
Adjusted R^2	0.000	0.001
F	0.593***	1.167***

Standard errors in parentheses clustered at the listing level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Since this estimation is run on shared apartments and private rooms, their count is not used as a control.

Competition = shared and private rooms listed in a given month and arrondissement in the same price quartile.

5.6.5 Dormancy of Incumbent Listings

We deployed an additional robustness test to examine the activity levels of incumbent listings on the platform. It may so happen that sharing restrictions through AQR may motivate platform disintermediation, thus leading to *platform exits*. However, we expected that the sharing restrictions placed by the AQR lead to *dormancy* — in the sense that providers could simply choose to become inactive and stop making their listings available within the platform

— as opposed to leading to *exits* of incumbent listings. This expectation was informed by the fact that while the AQR limits the number of days a listing can be shared within the platform, it does not ban providers. In addition, due to its sheer size, Airbnb remains the largest home-sharing platform, giving hosts the opportunity to supplement their income, and due to its current market dominance, providers have limited options to benefit from their assets outside Airbnb. For providers, exiting Airbnb would also entail losing the reputation signals that they had previously accumulated within the platform.

Table 12: Likelihood of Listings turning Dormant

	DV=Dormant Listing Indicator	
	(1)	(2)
AQR Impact	-0.129***	-0.108**
	(0.034)	(0.034)
Treated/Control Indicator	0.085	-0.163
	(0.221)	(0.244)
After	3.710***	3.686***
	(0.034)	(0.035)
Accommodates		0.057*
		(0.026)
Log(Price)		0.342***
		(0.065)
Competition (arr-Month)		0.001***
		(0.000)
Distance city center (kms)		-0.172
		(0.137)
Private Rooms		-0.001*
		(0.000)
Shared Apartments		-0.002
		(0.001)
Category		
Apartments		0.702***
		(0.084)
Bread and Breakfast		2.110*
		(1.044)
Observations	278,813	278,792
Listing FE	✓	✓
Month-Year FE	✓	✓
Chi^2	33581.494	33718.149

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Therefore, we expected that the disincentivized providers of the treated arrondissements could either become *dormant*, or remain active and distribute their listing availability over time. We proceeded by examining the likelihood that incumbent listings became dormant due to the introduction of the AQR. To achieve this, we used our full dataset of Airbnb listings, and using the date since a listing received its last review, we created the *Dormant* variable, which indicated listings that had received their last review more than six months ago

from the time that they are observed in the dataset. We used a six month window to ensure that we had sufficient time to compare both before and after the introduction of the AQR, and — after eliminating *Popularity* from our specification to avoid multicollinearity issues — we estimated a difference-in-differences model using logit estimations for the likelihood of a listing becoming dormant (Table 12). Contrary to expectations — and in line with the aforementioned results providing evidence that incumbent providers were better off after the introduction of the AQR — we found that incumbent listings became less dormant, and hence more likely to receive bookings after the introduction of the AQR. Since this was also a difference-in-difference estimation, we confirmed the parallel trends of our dependent variable - *Reviewed (Last Six Months)* in the Appendix (Table19).

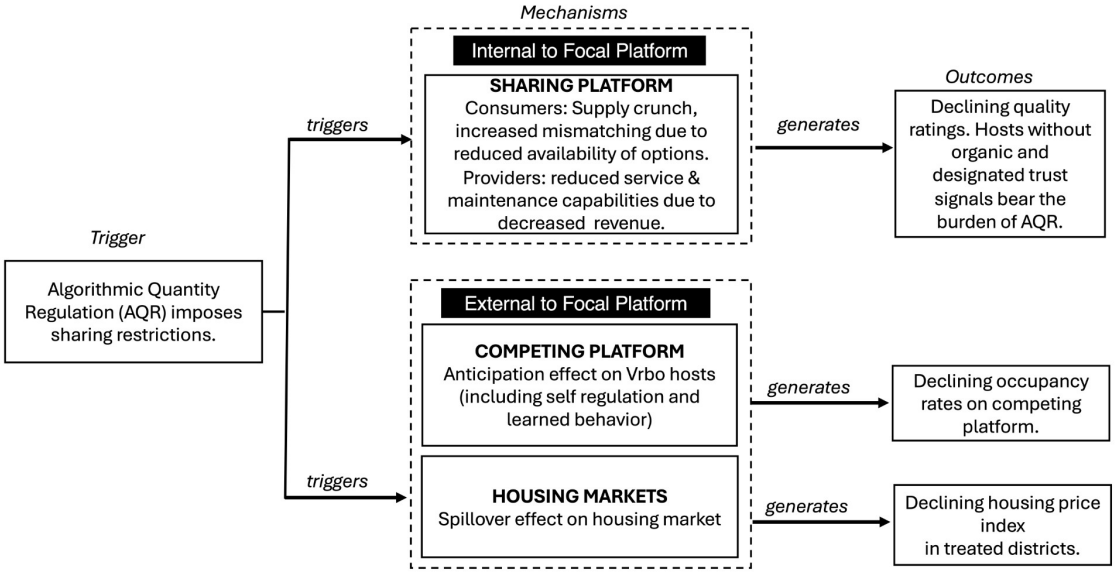
6 Discussion

6.1 Summary of Findings

Using a quasi-natural experiment on Airbnb in Paris, we investigated the impact of the AQR both *within* and *outside* the focal platform. Within Airbnb, we identified three adverse effects as a result of the AQR's introduction. Specifically, we found that the algorithmic quantity regulation negatively impacted the quality of the offerings. Additionally, we provided evidence that the AQR affected platform participants *unequally*, and causally demonstrated that inexperienced providers, as well as providers that had not obtained the superhost status, were worse off than their counterparts. We also identified two effects that were external to Airbnb. First, we identified an *anticipatory effect* on competing platforms, reflected in the self-regulation of providers on Vrbo. Second, we identified a *spillover effect* in the housing market, evidenced by a decline in the housing price index within the treated area following the introduction of the AQR. Figure 3 depicts the potential mechanisms that are triggered by AQR. The implementation of AQR triggers a supply crunch in the focal platform - leading to suboptimal matches of consumer preferences. Additionally, providers are earning less revenues than before and are unable to offer and maintain satisfactory experiences to their guests. Taken together, both mechanisms generate adverse outcomes for the platform and its participants in terms of quality ratings. Meanwhile, providers on competing platforms anticipate the imposition of similar restrictions, resulting in declining occupancy rates. Finally, as AQR limits income from short term rentals, it may lead owners to housing markets, where a declining price index is observed.

We proceed by elaborating on the implications of our work for research and practice, and conclude by discussing the limitations of this work as well as future research directions.

Figure 3: Potential Mechanisms



6.2 Implications for Research

AQRs are designed to mitigate the negative externalities associated with sharing platforms. While these interventions are well-intended, the lack of empirical evidence on their effects has left their significant implications insufficiently understood. This study makes five key contributions to the growing body of literature on platform regulation in general, and on the role of algorithmically implemented regulations in sharing platforms in particular.

First, while past studies have documented some positive welfare effects of regulating sharing platforms, such as enhanced safety of neighbourhoods (Han et al., 2022), our research provides strong causal evidence that the introduction of an AQR on a leading sharing platform led to a significant, unintended, and counterproductive outcome: a decline in quality on the platform. Our findings demonstrate that even well-intended platform regulation carry significant costs. The observed decline in quality ratings has adverse implications for all platform stakeholders. For price-sensitive *consumers*, particularly those seeking affordable accommodation options — including students, young professionals, displaced individuals immigrants, and gig economy workers — sharing platforms often represent their only viable housing option, and declining quality directly diminishes their welfare. In line with previous findigs, for instance in the context of airline regulations (Borenstein and Rose, 1994), quality loving, but price inelastic consumers are worse off post regulation.

For *providers* trying to cover living costs and make ends meet, declining quality ratings can trigger a negative feedback loop — lower ratings lead to fewer bookings, which in turn

result in reduced income and further declines in ratings. Beyond directly treated providers, this negative consequence can also have a cascading effect on *platform dynamics* — poor quality indicators on Airbnb can erode trust across the entire platform. This is particularly detrimental for vulnerable providers who depend on the platform for their essential income. In a trust-driven market, quality indicators serve both as a source of competitiveness, and as a critical source of sustained income for *providers*. Survey data reveal that as inflation and cost of living have risen across the world, approximately 35% of hosts across the world admitted that income from Airbnb helps them cover their costs of living, while approximately 40% reported that Airbnb income helped them make ends meet (Airbnb, 2022). Future studies should explore these dynamics further, particularly by investigating how platform and host-level interactions evolve in the presence of AQRs, especially in cities with similar regulatory frameworks, such as London and Amsterdam.

Second, our study contributes toward understanding who bears the burden of the regulation, putting the unintended effects of AQR in the limelight. Our results show that in the face of AQR, consequences for superhosts and experienced providers are limited, while in sharp contrast, their counterparts face an increasingly uneven playing field. The introduction of the AQR shrinks their already limited revenues, making it harder for them to compete for bookings and trust-building signals. While trust-building signals help providers differentiate themselves in the market, they can also be discriminatory, as the very process that allows providers to acquire them is riddled with inequalities. In doing so, our study contributes to the growing body of literature examining the regulation of sharing platforms and the challenge of balancing the interests of their diverse stakeholders (Chen et al., 2021; Han et al., 2022; Yu et al., 2019).

The current regulatory approach neither manages to restrict large scale operators, nor does it preserve opportunities for smaller, individual hosts who are less likely to have accumulated trust-building signals.

Third, as platform owners wield significant power over all platform participants, and due to their quasi-natural authority over all platform interactions (Cohen and Sundararajan, 2015), they are strategically positioned to play the role of private regulators (Hagiu and Boudreau, 2008). Our work identifies levers that platform owners and policymakers can strategically use to alleviate some of the negative effects associated with the introduction of an AQR. Specifically, platform-based trust signals through in-platform incentives as well as regulatory interventions contribute toward creating a level playing field. Future work should strive to identify optimal behavioral nudges and interventions that could be used to benefit disadvantaged participants.

Fourth, this study explored the external impact of the AQR by investigating its effects

beyond the focal platform. We presented evidence of an *anticipatory effect* on Airbnb's most prominent competing platform, where providers voluntarily reduced their sharing activities, even though Vrbo did not implement an AQR. Additionally, we identified a *spillover effect* in the housing market, evidenced by a decline in housing prices within the treated Parisian districts. These findings open up rich avenues for future research from both behavioral and market dynamics perspectives. While our work uncovered the first order direct implications for related markets and demonstrated their responsiveness to sharing platform regulations, additional investigation is needed to understand the broader, long-term impacts of algorithmic regulations. This includes exploring how these regulations shape participant behavior, influence market dynamics, and affect overall welfare across different contexts.

Finally, by highlighting the significant implications that the introduction of the AQR had for the platform and its ecosystem, this study contributes to the emerging literature on the use of algorithms in policy implementation (Danaher et al., 2017; Dourish, 2016; Gritsenko and Wood, 2022).It poses that scholars have a unique opportunity to provide valuable insights into the complex sociotechnical dynamics of algorithmic regulation. While this work underscores the need for more nuanced and well-considered regulations, it also raises the potential risk of developing increasingly complex algorithmic frameworks. Future research should focus on ensuring that such regulations remain transparent to participants without compromising their fairness.

6.3 Limitations and Future Scope

As with all studies, this work has some limitations which offer opportunities for future studies on the regulation of sharing platforms. While past work has already shown that disadvantaged participants often lack the opportunities to quickly accumulate experience, or meet the criteria required to earn trust-building badges (Slee, 2017; Scholz, 2016; Frenken and Schor, 2019), due to their public nature, our datasets did not contain detailed demographic information, such as gender, race, or income. Also, while our work reported main results that were based on listings that remained active for the entirety of the period examined and were replicated using the full dataset, they did not contain data on exact entry and exit days for listings. Future research could provide more fine-grained insights into the precise effects of the regulation across different demographics, and analyze the impact of AQR on entry or exit similar to Conti and Valentini (2018), in order to augment the ability of decision-makers to develop fair interventions. While we examined whether AQR led listings to turn *dormant* on the focal platform, future studies can examine boundary dynamics using aggregated data - such as entries and exits of listings on the platform.

Data limitations from our Vrbo data (Dataset 2) prevented any inferences on cross-listed properties, or identifying hosts that were multihoming. Future studies should examine host's voluntary self-regulation in greater detail, while it might also be interesting to compare and contrast the strategic behaviors of multihoming versus singlehoming hosts. The implications for competing platforms in terms of their overall equilibrium, earnings, and market growth constitute also promising future research directions. With regard to our findings on housing prices, we note that our empirical strategy computes the average differences before and after treatment. However, housing price indices are known to be complex. Future studies should examine in greater detail, how the use of supply restricting AQRs in the sharing platforms influences housing and rental prices over time, and whether the evolution of these prices is geospatially heterogeneous.

Qualitative work could also shed light on the internal processes that led to the decision of introducing an AQR in Airbnb, how dynamics between platform owners, politicians, and regulators affected this decision, as well as who ultimately bears the responsibility for its algorithmic enforcement. In addition, our study did not include micro-level data on booking requests and host acceptance behavior, which could generate additional insights into how participants' behavior changed as a response to the introduction of the AQR.

We examined how Airbnb — the most prevalent sharing platform in terms of its market capitalization — was affected by the most prevalent type of regulation imposed on sharing platforms, namely a quantity regulation. Other administrations in New York (Lung, 2023) and Los Angeles adopted (Board, 2023) licence-based regulations, whereas Barcelona's local government banned several listings (McClanahan, 2021). Future studies should compare different types of sharing platforms, as well as different regulatory approaches to provide additional insights on their efficacies, while also considering the role that cultural and contextual differences may play toward exacerbating the associated negative externalities.

Finally, while this work remained focused on the implications of the introduction of an AQR for a sharing platform and its ecosystem, future work could compare and contrast the generalizability of the findings to other types of platforms.

7 Conclusions

Sharing platforms have been linked to a series of negative externalities, including spikes in housing prices (Lee, 2016), deterioration of community safety (Guttentag et al., 2018), as well as racial discrimination (Cui et al., 2017). However, they also benefit their participants and have significant positive spillover effects on society, including the creation of income opportunities, the promotion of local entrepreneurship, and the reduction of alcohol-related

motor vehicle fatalities (Zervas et al., 2017; Li and Srinivasan, 2019; Greenwood and Wattal, 2017). Amid the lack of clear evidence, and as their radically different nature poses difficulties to the traditional regulatory apparatus, sharing platforms have been the epicentre of a regulatory debate, leading to the introduction of regulatory interventions.

This study established a causal relationship between the introduction of the AQR on a sharing platform and the decline in platform offerings' quality. In addition, it provided evidence that signals of trust, or lack thereof, such as experience and superhost status, can attenuate this relationship. By drawing attention to platform participants as regulated agents, it demonstrated that some participants were particularly susceptible to the imposed regulatory interventions. Platform participants constitute part of the population that these regulatory interventions aim to protect in the first place. As a result, the effective regulation of a sharing platform will inevitably require striking a balance between supporting not only disadvantaged individuals outside the platform, but also protecting disadvantaged individuals within the platform.

There is much work yet to be done to understand how sharing platforms can be best designed and regulated in a way that is deemed just to all actors. As sharing platforms become increasingly pervasive and ubiquitous, understanding how interventions can be best designed to protect the interests of stakeholders both within and outside the platform becomes of ever-increasing importance. More broadly, this work might serve as the basis for a new research discourse focused on understanding, designing, and developing algorithmically implemented regulations that are nuanced, balanced, and equitable.

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Appendix A

Figure 4: Average 30-day availability in Central Paris

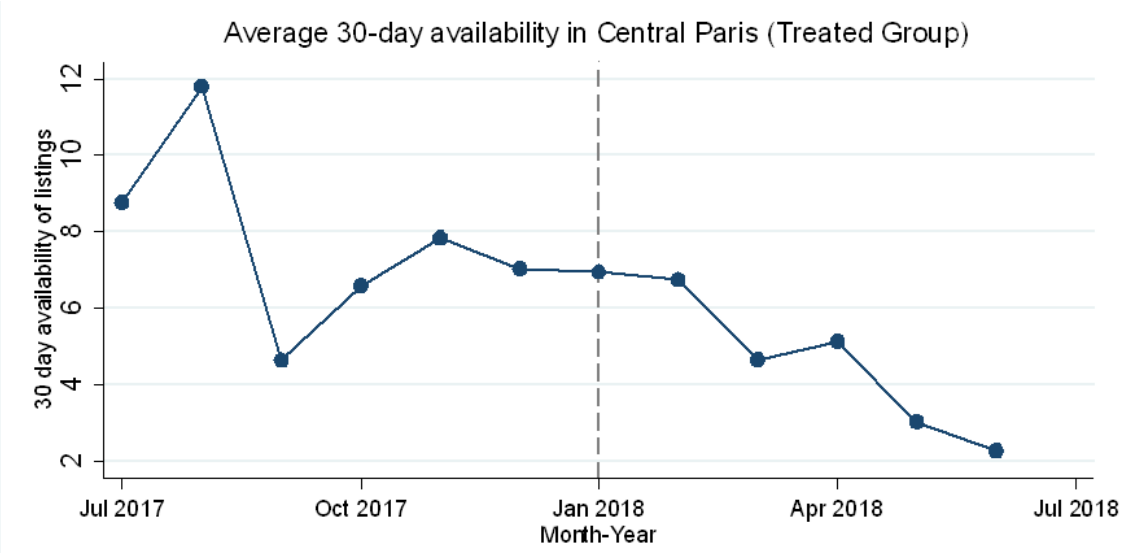


Table 13: Dataset 1: Slightly Imbalanced Panel, Observations per Time

Month-Year	Freq	Percent	Cum Percent
Jul 2017	8,525	8.33	8.33
Aug 2017	8,524	8.33	16.67
Sep 2017	8,524	8.33	25.00
Oct 2017	8,525	8.33	33.33
Nov 2017	8,525	8.33	41.67
Dec 2017	8,525	8.33	50.00
Jan 2018	8,523	8.33	58.33
Feb 2018	8,525	8.33	66.67
Mar 2018	8,525	8.33	75.00
Apr 2018	8,523	8.33	83.33
May 2018	8,525	8.33	91.67
Jun 2018	8,525	8.33	100.00
Total	102,294	100	

Table 14: VIF for main regression specification, DV=Quality Ratings

Variable	VIF	1/VIF
Accomodates	1.75	0.571
Log (Price)	1.91	0.522
Competition (arr, month)	1.27	0.789
Popularity	1.01	0.987
Distance from city center (kms)	1.39	0.721
Private Rooms	3.08	0.324
Shared Rooms	2.43	0.411
Listing Category		
Apartment	1.05	0.952
Bread and Breakfast	1.01	0.989
Heterogeneity Factors		
Superhost	1.07	0.931
Host Experience	1.06	0.939

Table 15: Variance Inflation Matrix for all controls

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Accommodates (1)		1.02	1.74	1.75	1.74	1.73	1.75
Log(Price) (2)	1.11		1.88	1.91	1.89	1.87	1.90
Competition (3)	1.25	1.24		1.26	1.24	1.08	1.26
Popularity (4)	1.01	1.01	1.01		1.01	1.01	1.01
Distance (5)	1.38	1.37	1.36	1.38		1.26	1.38
Private Rooms (6)	3.03	3.01	2.64	3.07	2.79		1.76
Shared Rooms (7)	2.42	2.40	2.41	2.42	2.41	1.38	
LC Apartment	1.05	1.04	1.04	1.05	1.05	1.04	1.01
LC Bread and Breakfast	1.01	1.01	1.01	1.01	1.01	1.01	1.01

We exclude the two categorical variables from our OLS regression specification.

Table 16: Alternative Specifications

	(1)	(2)
AQR Impact	-0.064***	-0.064*
	(0.013)	(0.032)
Treated	0.981*	0.981
	(0.493)	(1.450)
After	0.067***	0.067***
	(0.007)	(0.013)
Accomodates	-0.112***	-0.112
	(0.020)	(0.061)
Log(Price)	0.009	0.009
	(0.053)	(0.106)
Competition (month, arr)	0.000	0.000
	(0.000)	(0.000)
Popularity	0.003	0.003*
	(0.003)	(0.001)
Distance from city center (kms)	0.563*	0.563
	(0.287)	(1.552)
Private Rooms	-0.000	-0.000
	(0.000)	(0.000)
Shared Apartments	0.000	0.000
	(0.001)	(0.001)
Apartments	-0.107**	-0.107
	(0.034)	(0.085)
Bed and Breakfast	-0.282	-0.282
	(0.274)	(0.344)
Constant	91.506***	91.506***
	(0.866)	(4.378)
Observations	102,294	102,294
Month-Year FE	x	✓
Listing FE	✓	✓
Adjusted R^2	-0.089	0.002
F	13.786***	2.957***

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 17: Impact of AQR - 12 month post-treatment period

	(1)	(2)
AQR Impact	-0.133***	-0.125***
	(0.033)	(0.033)
Treated/Control Indicator	0.479	1.510
	(1.079)	(1.196)
Before/After Indicator	-0.277***	-0.284***
	(0.024)	(0.025)
Accomodates		-0.004
		(0.035)
Log (Price)		-0.086
		(0.075)
Competition (month, arr)		0.000
		(0.000)
Popularity		-0.018**
		(0.007)
Distance from city center (kms)		0.447
		(0.339)
Private Rooms		-0.000
		(0.000)
Shared Apartments		0.001
		(0.001)
Category		
Apartment		-0.041
		(0.070)
Bed and Breakfast		0.326
		(0.319)
Constant	92.591***	91.703***
	(0.162)	(1.154)
Observations	744,082	743,863
Listing FE	✓	✓
Month-Year FE	✓	✓
Adjusted R^2	0.001	0.001
F	14.569	10.320

Standard errors in parentheses clustered at the listing level
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18: VIF Matrix for Spillover Effect Estimation on Vrbo Dataset

Variable	(1)	(2)	(3)	(4)
Occupancy Rate (1)		1.02	1.02	1.00
ADR (USD) (2)	1.00		1.00	1.00
Blocked Days (3)	1.02	1.02		1.00
Reservations (4)	1.02	1.04	1.03	

Table 19: Parallel Trends for Dormancy Effect

	(1)
Treat × t-1	-0.097
	(0.063)
Treat × t-2	-0.091
	(0.063)
Treat × t-3	-0.084
	(0.063)
Treat × t-4	-0.101
	(0.063)
Treat × t-5	-0.077
	(0.062)
Treat × t-6	-0.053
	(0.064)
Observations	278813
Listing FE	✓
Month-Year FE	✓
Chi^2	33574.533

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$