Supplier Scale, Competition, and Consumer Experience in Platforms

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Abstract

The relationship between supply-side competition and platform market dynamics has long been debated, yet the specific role of supplier scale remains underexplored. In this paper, we empirically study how competition from large-scale suppliers influences platform market dynamics and, in turn, a platform's ability to compete with outside options. Using Airbnb as an example, we show that restricting large-scale suppliers yields only transient benefits to price, demand, and revenue for existing suppliers. We provide evidence that these transient gains are accompanied by a decline in average consumer experience and an increase in demand for outside options. These results suggest that the competition among small individual suppliers alone cannot maintain the quality standards necessary to rival external options, underscoring the long-term importance of large-scale suppliers for platform sustainability. Moreover, we find that such restrictions do not disproportionately benefit marginal suppliers such as Black suppliers who are often the intended beneficiaries of these interventions, potentially due to their more limited experience. Our findings highlight the critical role that large-scale suppliers play in sustaining platform growth by enhancing consumer experience. This study contributes to the literature on supply-side competition, platform governance, and supply-side disparities by clarifying the importance of large-scale suppliers in platform markets.

Key words: Economies of Scale, Competition, Platform Governance, Consumer Experience, Racial Disparity

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1 Introduction

The relationship between competition and quality provision, and its implications for market outcomes, have long been debated in both the platform literature and in markets more generally (e.g., Anderson and Simester 2013, Belleflamme and Peitz 2019, Farronato and Fradkin 2022, Fradkin 2017, Horton 2019, Tiwana 2015). While some evidence suggests that increased competition increases average platform quality that attracts buyers (Matsa 2011), other research cautions that intense competition can suppress demand and reduce suppliers' incentives to exert effort (Rossi 2024). These mixed findings motivate further examination of how supplier heterogeneity shapes the effects of competition on platform dynamics and long-term sustainability.

One important dimension of heterogeneity among suppliers is supplier scale. As shown in Bar-Gill and Brynjolfsson (2016), suppliers on a platform often scale up as the platform grows, leading to an increasing mass in the right tail of the firm size distribution over time. By operating with lower marginal costs and greater market efficiency (Li and Srinivasan 2019), large-scale suppliers have the potential to play a significant role in ensuring quality provision. Yet little is known about how competition from large-scale suppliers alters platform market outcomes such as price, demand, and revenue. In particular, if large-scale suppliers play a significant role in quality provision, reduced competition from such suppliers may shape consumer experience and, over time, consumer perceptions of platform quality — with downstream consequences for the platform's ability to compete with alternative offerings.

In this paper, we explore these dynamics by examining how restricting large-scale suppliers affects the consumer experience and market outcomes for incumbent suppliers on the platform over both the short and long term. We leverage a unique platform policy change on Airbnb, the largest home-sharing platform. The platform introduced the "One Host, One Home" policy in April 2016 in New York City and San Francisco to restrict the supply from large-scale hosts. This self-imposed policy limited each host to a single entire-home listing on the Airbnb platform, directly curtailing competition from large suppliers. We test the implications of this policy change using a difference-in-differences (DiD) approach with matching, drawing on data from eleven large U.S. cities from May 2015 to April 2019.

Our baseline results show that while restricting large-scale suppliers yields short-run gains for incumbents across price, demand, and revenue, these benefits diminish to roughly one-third of their initial magnitude in the long run. To verify our findings, we estimate an event study model and assess robustness using doubly robust machine learning methods and synthetic DiD. We provide evidence that these transient benefits are in part because restricting large-scale suppliers reduces average consumer experience on the platform and increases demand for outside options. Our findings shed new light on the long-term importance of large-scale suppliers, as small individual suppliers alone are insufficient to maintain a level of competition and quality necessary to compete with large-scale outside options. This extends prior work on intraplatform competition by clarifying the distinctive role of large-scale suppliers in sustaining platform quality and long-run competitiveness.

Restricting large-scale suppliers creates opportunities for the remaining providers to capture additional demand and revenue. A natural question, however, is whether all existing suppliers are equally positioned to take advantage of these opportunities. Koo and Eesley (2021) shows that suppliers respond differently to changes in market conditions and adopt distinct strategies due to heterogeneity in their experience, capabilities, and resources. This heterogeneity suggests that the benefits of restricting large-scale suppliers may not be distributed evenly across providers. Therefore, it is important to examine whether less-advantaged suppliers are able to benefit from such restrictions to the same extent as their more advantaged counterparts.

To examine this question, we focus on Black hosts as an example of such suppliers. Black hosts face not only the competitive pressures common to smaller individual hosts, but additional market frictions such as more limited access to resources (Bayer and Charles 2018, Chetty et al. 2020) and statistical discrimination (Alyakoob and Rahman 2025, Laouénan and Rathelot 2022, Luca et al. 2024). We find that Black hosts respond to the restriction of large-scale suppliers by charging higher prices than other competitors, yielding smaller increases in demand and revenue. Further analysis indicates that Black hosts differ from others in their use of algorithmic pricing tools and activity intensity, leading them to misinterpret platform changes, set suboptimally high prices, and fail to fully capture gains in demand and revenue.

Together, our research sheds new light on a long-standing question regarding supply-side competition in platforms. Despite the transient benefits to incumbents from restricting large-scale suppliers, our findings emphasize the importance of competition from large-scale suppliers to a platform's long-term sustainability in competitive environments. By operating at lower costs and greater efficiency, large-scale suppliers sustain the competition necessary to uphold consumer experience so that the platform can effectively compete with outside options. We further find that restricting large-scale suppliers does not disproportionately benefit the hosts who might seem most likely to gain from reduced competition — smaller, less-experienced hosts such as Black hosts, who already face additional market frictions. We show that this is partly explained by differences in experience — Black hosts are less likely to use algorithmic pricing tools, leading them to misinterpret market changes and set suboptimally high prices.

In so doing, we extend a growing body of work on competition among suppliers within a platform (e.g., Boudreau and Hagiu 2009, Parker and Van Alstyne 2005, Rochet and Tirole 2003). Prior research has documented mixed evidence on the effects of an increase in overall supply-side competition on market outcomes (Anderson and Simester 2013, Belleflamme and Peitz 2019, Farronato and Fradkin 2022, Fradkin 2017, Horton 2019, Matsa 2011, Rossi 2024, Tiwana 2015) and consumer experience (Matsa 2011, Rossi 2024), which may partly reflect differences in markets and supplier composition across settings. Our paper contributes to this literature by examining the implications of competition from large-scale suppliers, an important group on platforms, for platform market dynamics and long-term sustainability. A small literature has examined the short-run effect of restricting large-scale professional suppliers on price and demand. In particular, Chen et al. (2023) find that professional suppliers compete directly against smaller individual suppliers, and so restricting them induces new entry by nonprofessional hosts and creates higher prices for incumbents. However, our study shows that these benefits are transient: in the long run, the absence of large-scale suppliers erodes average consumer experience on the platform, shifting demand toward outside options and attenuating the gains for smaller suppliers. Moreover, we show that this attenuation is particularly pronounced for hosts who face additional market frictions — such as Black hosts — who are less equipped to interpret these market dynamics and adjust their pricing accordingly.

2 Related Literature

This paper is related to the broad literature that examines competition among suppliers in platforms and its impact on marketplace outcomes (e.g., Jullien et al. 2021, Mayya and Li 2025, Rietveld and Schilling 2021, Rochet and Tirole 2003, 2006). Although having a large number of suppliers on a platform-based marketplace has been shown to be important and beneficial for reasons such as fostering network effects (e.g., Bresnahan and Greenstein 2014, Brynjolfsson et al. 2003, 2011, Shankar and Bayus 2003), it also intensifies competition within the platform. Existing literature has found mixed effects of increased competition among suppliers within a platform. On the one hand, an increase in competition can improve suppliers' performance by accelerating the evolution of products (Belleflamme and Peitz 2019, Tiwana 2015) and increasing sales (Anderson and Simester 2013). On the other hand, the entry of an additional seller imposes negative externalities on incumbent sellers by lowering per-seller profits and reducing expected profits (Belleflamme and Peitz 2019, Fradkin 2017, Horton 2019).

Evidence on the implications of competition on quality provision has also been mixed. Within the context of the supermarket industry, Matsa (2011) shows that increased competition has a positive impact on quality. In contrast, Rossi (2024) shows that increases in competition decrease the effort of hosts on Airbnb. We add nuance to these findings by showing that the relationship between competition and quality depends critically on the *type* of competitor: restricting large-scale suppliers on Airbnb reduces consumer experience over the long run, suggesting that their presence — rather than simply competitive intensity — is a key driver of quality provision on the platform. This is a subgroup that accounts for an increasingly large share of platform activity over time (Bar-Gill and Brynjolfsson 2016) and which exerts a distinctive influence on market dynamics (Gutt et al. 2019).

Our study is also related to the stream of literature on platform governance (e.g., Boudreau 2010, Tiwana 2013). This literature highlights the implications of heterogeneity among suppliers within a platform; in particular, it shows that certain types of participants can be more beneficial to a platform's overall performance (e.g., Bakos 2001, Boudreau 2018, Gawer et al. 2002, Krishnan and Gupta 2001, Rochet and Tirole 2003, Varian 2010, Parker et al. 2016). Prior research suggests that restricting access based on specific thresholds, such as product similarity, can prevent competitive crowding and maintain suppliers' incentives to innovate and develop new products or services (Boudreau 2012). From the perspective of supplier scale, existing research has mainly focused on the trade-off between platform control and supplier autonomy as suppliers expand their operations and the market becomes more concentrated (e.g., Hagiu and Wright 2019, Jullien et al. 2021, Teh

2022). However, little is known about how large-scale suppliers alter platform market dynamics, which further affects a platform's ability to compete with outside options.

A small literature has examined the short-run implications of restricting large-scale professional suppliers for price and demand. In particular, Chen et al. (2023) exploit the same Airbnb policy change we study to show that restricting large-scale hosts increases prices and reservation days for incumbent nonprofessional suppliers in the short run. However, their analysis covers only a short-run 16 month period post-treatment and does not examine whether these gains persist, nor does it consider the role of consumer experience in shaping long-run market dynamics. Our study differs in several important ways. First, we track outcomes over a 36-month window, revealing that the short-run gains documented by Chen et al. (2023) substantially attenuate over time. Second, we show that this attenuation is driven by a deterioration in consumer experience that gradually shifts demand toward outside options such as hotels.

Our focus on heterogeneous supplier outcomes also contrasts with Chen et al. (2023) in an important way. They find that more advantaged suppliers — those with higher-priced listings — capture disproportionately larger gains from the policy in the short run. We show the mirror image for less advantaged suppliers: Black hosts, who are more likely to operate lower-priced listings and face additional market frictions, benefit less from the policy in both the short and long run. Moreover, we provide an explanation for this asymmetry that goes beyond price tier or statistical discrimination.

Supply-side racial disparities have been documented in the context of the sharing economy and efforts have been undertaken to address these disparities caused by market frictions for reasons such as statistical discrimination (e.g., Alyakoob and Rahman 2025, Laouénan and Rathelot 2022, Luca et al. 2024, Zhang et al. 2021). Existing literature has shown that suppliers' individual experiences and capabilities shape their decision-making (Afuah 2013, McIntyre and Srinivasan 2017, Wang and Miller 2020), and as a result, some may misinterpret platform changes and adopt strategies that inadvertently exacerbate their own disadvantages due to factors such as lack of access to other resources (Koo and Eesley 2021). Accordingly, disparities faced by minority suppliers may reflect differences in experience or skills (e.g., Bayer and Charles 2018, Chetty et al. 2020) that can lead them to misinterpret market conditions and encounter greater difficulties in setting optimal prices. We add empirical evidence to prior research on racial disparities by underscoring the role of limited experience among Black suppliers in interpreting shifts in competition and other market dynamics. This finding is consistent with prior literature emphasizing the importance of individual experience and skills beyond statistical discrimination.

3 Research Design

3.1 Sample

In practice, platforms choose opposite strategies when dealing with large-scale suppliers. For example, *eBay* started with small individual suppliers and later on opened platform access to large-scale suppliers to drive platform growth by boosting reliability and volume.

"We are no longer just an auction site. We are a marketplace where buyers expect a professional experience, and that requires bringing in the brands and large-scale retailers that consumers trust." - John Donahoe, former CEO of eBay, 2008 Analyst Day (Donahoe 2008)

On the contrary, as noted above, other platforms, such as *Etsy*, *Airbnb*, *Ticketmaster*, consider large-scale suppliers as a threat to the brand identity and choose to restrict them.

"We need to get back to what is truly special about Airbnb—everyday people who host their homes. We don't want to be a platform where you just book a place; we want to be a platform where you feel like you belong." — Brian Chesky, co-founder and CEO of Airbnb, 2024 Product Vision Speech (Chesky 2024)

We use Airbnb as the empirical context for our study. Founded in 2008, Airbnb has grown into the world's largest home-sharing platform, with more than 8 million active listings across 220 countries and regions. In the United States, hosting on Airbnb represents a meaningful source of supplemental income — the typical U.S. host earned approximately $15,000 annually in 2024 — making the platform an important setting in which to study the distributional consequences of supply-side policy interventions. [1]

Our study focuses on Airbnb listings in eleven major U.S. cities: Boston, Chicago, Dallas, Houston, Los Angeles, Miami, New York City, Oakland, Philadelphia, San Francisco, and Washington, DC. These cities were selected due to their high Airbnb activity and demographic diversity, making them ideal for analyzing the implications of a supply-side change on a digital platform. The

[1] `https://news.airbnb.com/en-in/about-us/`

observations in our analysis span from May 2015 to April 2019, a period that brackets the "One Host, One Home" policy. We end the sample period in April 2019 to avoid the influence of Covid and the potential similar treatment to the control cities. This leaves us with three post-treatment years in total. We consider 12 months in the post-treatment period as short-run and 36 months in the post-treatment period as long-run. We believe that a 36-month post-treatment period is sufficient to capture the long-run effects, as platform markets can change substantially within three years through supplier turnover, evolving quality and reputation, and competitive responses from outside options.[2]

3.2 Background

As mentioned in Brian Chesky's speech, Airbnb's brand identity is built on providing authentic travel experiences. However, the platform's host demographics have shifted significantly. Originally a venue for individuals to earn supplemental income from spare rooms, it has evolved into a lucrative industry for large-scale commercial operators managing dozens or even thousands of listings. According to American Hotel & Lodging Association (2016), hosts renting out multiple entire-home units generated nearly $2 billion in revenue in 2016, accounting for about half of the total market revenue in some cities; meanwhile, the number of listings by multi-unit hosts rose by more than 160%. Recent academic research shows that such multi-unit hosts have an advantage over individual hosts because of lower operating costs arising from economies of scale (Li and Srinivasan 2019). Although they played an important role in driving Airbnb's early growth and scale (Grothaus 2015), critics argue that these hosts remove housing from the local residential market to cater to short-term travelers, exacerbating housing shortages and affordability issues (Kerr 2017, Ting 2016). To address legislative concerns about housing and rental affordability and to reduce conflict with local governments, Airbnb rolled out a platform-level policy of self-regulation, the "One Host, One Home" policy, in New York City and San Francisco.[3] This policy restricts hosts in these cities from listing properties at multiple addresses and promotes home sharing in primary

[2] As a benchmark, the traditional IO literature documents annual establishment entry rates of approximately 10% and exit rates of approximately 8% in U.S. retailing (Jarmin et al. 2004). Platform markets are likely to adjust even more rapidly than traditional offline markets because suppliers face lower physical and capital constraints. Industry data shows that approximately 50% of hosts who joined Airbnb leave the platform within three years (Lighthouse 2023).

[3] The policy was also implemented in Portland on January 30, 2017, but Portland is not included in our data.

residences only. [4][5]

The "One Host, One Home" policy was introduced unexpectedly on April 2, 2016. Formally implemented seven months later on November 1, the policy mandated that hosts maintain listings at only one specific address. Under these rules, a host attempting to list a second location would have to delete their original entry. Airbnb further announced strict enforcement measures, where violations could result in account deactivation or suspension.

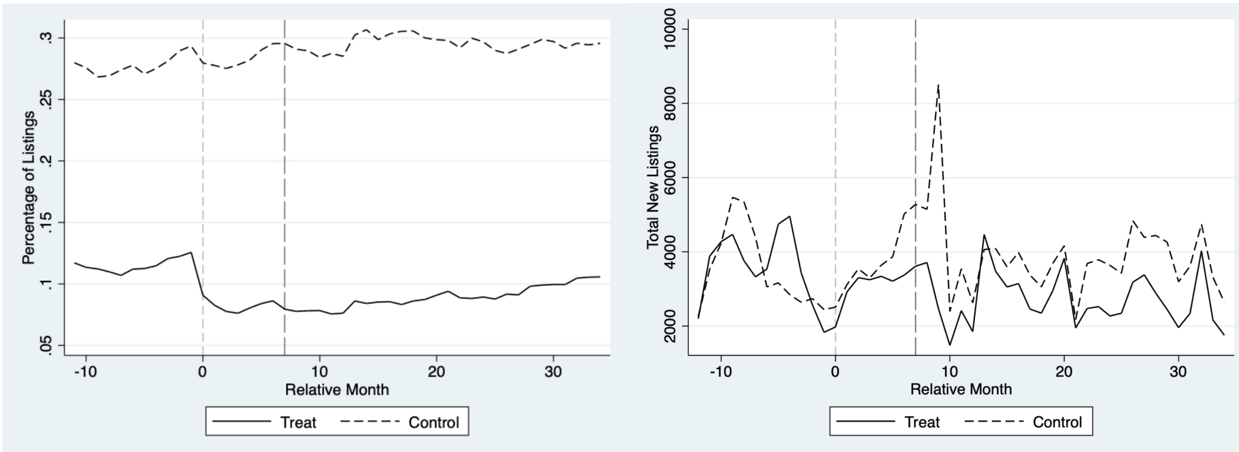
We define *large-scale hosts* as those who ever managed more than one entire-home listing *simultaneously* in a city in any month during our study period. This definition is consistent with the target group of the "One Host, One Home" policy. Using this defintion, Figure 1 shows how the supply side on Airbnb changes over time. Following the regulation, there was a significant decrease in the percentage of listings by large-scale suppliers in the treated cities (i.e., New York City and San Francisco), and this is not offset by new entrants on the platform.[6] In Figure 1, the first vertical line at relative month $t = 0$ marks the policy announcement at the beginning of that month, which corresponds to a significant decline in the supply of large-scale suppliers. The second vertical line denotes the policy implementation, which also shows a slightly further downward trend. In our main analysis, we use the policy announcement for the timing of the treatment, and consider the policy implementation as an alternative in the robustness checks.

Since market participants were largely unaware of the timing and scope of the "One Host, One Home" policy, its rollout functioned as a plausibly exogenous shock to the supply of large-scale hosts. These restrictions fundamentally shifted host behavior by altering the platform's composition and reducing the competitive pressure from large-scale hosts within the platform. Consequently, the introduction of this policy provides a unique opportunity to analyze how competition from large-scale suppliers shapes platform market dynamics.

[4] `https://www.airbnbcitizen.com/wp-content/uploads/2018/04/One-Host-One-Home-_-April-2018-Update.pdf`

[5] `https://www.airbnbcitizen.com/wp-content/uploads/2017/03/One-Host-One-Home-%E2%80%93-San-Francisco-Feb-2017.pdf`

[6] The peak in the number of total new listings in the control cities were corresponding to major events in Washington DC (*presidential inauguration*) and Houston (*Superbowl*). We re-plot Figure 1 and rerun the baseline regressions in the Appendix by excluding those two cities.



(a) Percentage of listings by large-scale suppliers

(b) Total New Listings

Figure 1: Change in supply side on Airbnb

3.3 Data and variables

We obtain data on Airbnb listings through a third-party data provider, AirDNA, which provides time-varying listing characteristics (e.g., reviews, Superhost status, number of beds, amenities, etc.) along with property performance data, such as price and reservation information. To exclude inactive listings, we restrict our analysis to listings marked as reserved or available for booking on the host's calendar. We further limit the sample to listings existing in both the pre- and post-treatment period, covering both the short and long run.

We complement the Airbnb data sets with socio-economic variables related to rental and housing markets at the zip code level. We obtain information on monthly housing value from *Research Data on Zillow*.[7] Furthermore, to examine the implications of potential competition between Airbnb and the incumbent hotel industry, we complement our dataset with zip-code level time-varying hotel performance data (e.g., hotel demand, occupancy rate, number of rooms, etc.) from *Smith Travel Research*. We merge Airbnb listing datasets with zip code level datasets at the zip code by year-month level, and thus our analysis is conducted at the year–month level.

We use three measures of market performance as our outcomes of interest in the baseline analysis: price, demand, and revenue. Information on price is directly observed from AirDNA. As for demand, we use the occupancy rate, calculated as the ratio of booked days to the total number

[7] `https://www.zillow.com/research/data/`

of available or booked days, as the proxy for demand in our main analysis. We construct alternative measures of demand, including number of reservations and number of booked days, as robustness checks. We measure revenue using RevPAR (revenue per available room), the hospitality industry's standard metric for top-line performance (Smith Travel Research 2024).

To explore mechanisms behind the long-run effects of large-scale supplier competition, we construct time-varying zip-code-level variables capturing local competition and market dynamics. To capture market entry, we calculate the number of new Airbnb listings and new Airbnb host entries in each month based on the *create date* of listings. A host is classified as a *new host* in month t if their earliest listing *create date* falls in month t. We sum up all the *new hosts* in a zip code in time t to calculate the *number of new host entries*. Additionally, we use time-varying variables on hotel performance obtained from *Smith Travel Research* to investigate whether user demand shifts to outside options after restricting large-scale players on Airbnb. Specifically, we examine the implications for *hotel occupancy rate* and *hotel demand*, defined as the number of rooms sold in a specific area during a specific time period, excluding complimentary rooms.

To understand the implication of large-scale suppliers for quality provision, we construct measures for consumer experience on the platform in both the pre- and post-stay stage. To proxy for the availability of high-quality options, we examine *Booking Lead Time* . *Booking Lead Time* is calculated as the number of days between the booked date and check-in date.[8] This variable captures the number of days a guest needs to plan in advance when making a booking. An extended *Booking Lead Time* reflects a decrease in the availability of high-quality options, as high-quality listings become harder to secure and necessitate earlier planning. We also use listing-level ratings for communication and check-in, which are considered to specifically relate to host behaviors that can be readily and quickly adjusted by Airbnb hosts.

To predict race information for hosts, we supplement the AirDNA data with host profile pictures obtained from the *host picture URL* on *InsideAirbnb*.[9] We employ the models trained by Kärkkäinen and Joo (2019) to predict the race of each host in our sample. However, data limitations prevented host race prediction in four cases: (1) *InsideAirbnb* did not collect data for Dallas, Houston, Miami, or Philadelphia during our sample period, accounting for approximately 15% of main-

[8] `https://www.airdna.co/blog/vacation-rental-metrics-booking-lead-time`
[9] `https://insideairbnb.com/`

sample listings. (2) The listing either lacks a host photo URL or the URL was inaccessible. (3) The host photo contains no human face. (4) The host photo contains more than one face. Cases (2)–(4) result in an additional loss of approximately 50% of main-sample listings. We exclude listings falling into any of these categories from the host race heterogeneity analysis. Section 5.4.4 provides evidence that listings with and without race information do not differ systematically on the outcomes of interest.

The summary statistics at the listing and zip-code level are shown in Table 1.

Table 1 Summary statistics

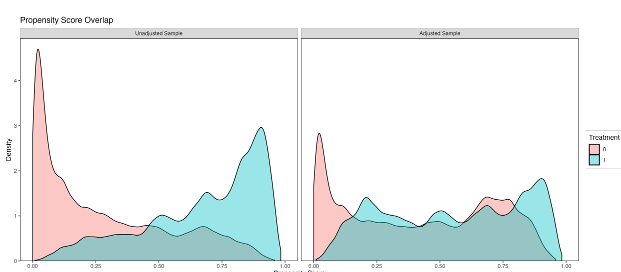
	VARIABLES	N	mean	sd	min	max
Listing	*treated*	744,460	0.464	0.499	0	1
	after	744,460	0.779	0.415	0	1
	$\log(price + 1)$	744,460	4.833	0.711	2.398	12.111
	occupancy rate	744,460	0.465	0.363	0.000	1.000
	$\log(\#reservation{+}1)$	744,460	1.190	0.823	0.000	3.434
	$\log(booked\_days + 1)$	744,460	2.049	1.341	0.000	4.344
	$\log(RevPAR + 1)$	744,460	3.087	1.955	0.000	9.669
	rating communication	744,460	8.863	2.867	0.000	10.000
	rating check-in	744,460	8.852	2.872	0.000	10.000
	Booking lead time	551,596	38.685	61.233	−191	364
	log(#listing per host + 1)	744,460	0.773	0.873	0.000	5.576
	$\log(\#review + 1)$	744,460	3.140	1.536	0.000	6.647
	Superhost	744,460	0.229	0.420	0	1
	petsallowed	744,460	0.133	0.339	0	1
	smoking	744,460	0.042	0.201	0	1
	Black	262,141	0.105	0.306	0	1
Zip-code	#new host entries	24,255	8.366	15.08	0	425
	#hotel demand	5,397	61987.9	75322.3	3537	538142
	hotel occupancy rate	5,397	0.760	0.122	0.384	0.998
	$\log(ZHVI + 1)$	744,460	13.480	0.590	12.175	14.389
	$\log(\#listing + 1)$	744,460	6.084	1.024	0.693	8.186
	log(median income + 1)	744,460	11.082	0.443	9.423	12.429
	$\log(population + 1)$	744,460	10.625	0.578	7.419	11.654
	$\log(households + 1)$	744,460	9.768	0.523	6.335	10.680

4 Identification Strategy

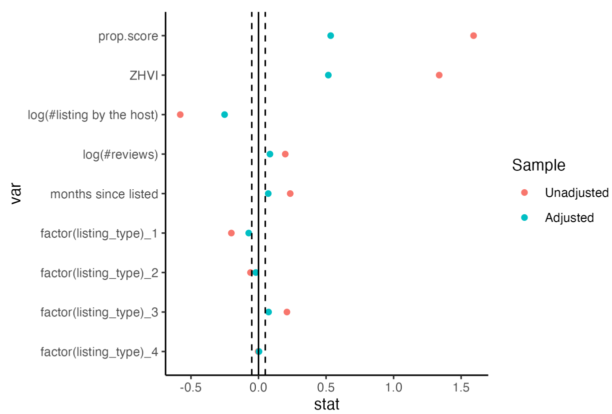
As noted above, we exploit the variation in the "One Host, One Home" policy implementation across cities over time, employing a standard difference-in-differences (DiD) approach. The treated group are the Airbnb listings in the policy-affected cities, New York City and San Francisco. In the main analysis, we use the date of the policy announcement, April 2016, as the treatment date. One concern may be that the impact of the announcement may differ from that of the actual implementation. We provide a robustness check in Section 5.4.2 in which we use the timing of the policy implementation (November 2016) as the treatment date and exclude the period between the announcement and implementation date in our analysis.

Given market and listing differences across cities in our sample, we apply the inverse probability of treatment weighting (IPTW) method to enhance the comparability between listings in the treated and the control cities (Austin and Stuart 2015, Giorcelli 2019, Thoemmes and Ong 2016, Zhang et al. 2021). To do this, we first estimate each listing's treatment probability as a function of observed covariates. We then construct the weights for each listing as the inverse of its treatment assignment probability. This approach constructs a weighted sample of treatment and control listings for which the distribution of measured baseline covariates is independent of treatment assignment (Austin and Stuart 2015, Thoemmes and Ong 2016). This approach makes the treatment and control groups more comparable, increasing the balance of the distribution of observed covariates between the treatment and control groups.

The matching is based on both listing and local market characteristics. Listing characteristics include listing type, number of bedrooms, number of reviews, month since being listed on Airbnb, and total listings managed by the same host, while market characteristics include Airbnb market size and housing values. The comparison between the unadjusted and adjusted sample is shown in Figure 2, which indicates that the treated and control listings are more balanced after implementing the inverse probability weighting method.



(a) Propensity Score Overlaps



(b) Covariates Balance Check

Figure 2 Balance Checks

Our DiD regression is as follows:

$$Y_{ijt} = \beta treated_{ij} \times after_t + X_{ijt} + \psi_i + \pi_t + \rho_{jt} + \varepsilon_{ijt} \quad (1)$$

where Y_{ijt} is the dependent variable, which is either price, occupancy rate, or revenue for listing i in city j in year-month t. The key regressor is $treated_{ij} \times after_t$, where $treated_{ij}$ takes a value of one for listings located in New York City and San Francisco and zero otherwise, and $after_t$, which takes a value of one for year-months after April 2016. β is the parameter of interest, which captures impact of the policy on market outcomes. We include time-varying listing-level control variables X_{ijt}, listing fixed effects, ψ_i, and year-by-month fixed effects, π_t. We further include city-specific time trends, ρ_{jt}, to account for seasonality and any unobserved trends varying over time across cities. We construct these by interacting a set of city dummy variables with a linear count variable that increments by one for each month. The residual term, ε_{ijt}, captures idiosyncratic shocks. Standard errors are robust and clustered by listing. To account for potential error correlation across listings within the same zip code, we reestimate the models with zip-code-level clustering; results are reported in Appendix A5.3. The unit of analysis is at the listing by year-month level.

5 Empirical Findings

5.1 How do large-scale suppliers shape platform market outcomes?

We begin by examining whether and how price, demand, and revenue for existing Airbnb listings change after restricting large-scale suppliers. Table 2 reports the DiD estimates using the IPTW-weighted sample with a full set of control variables. In Appendix E, we present results including only controls for city-specific time trends, year-month fixed effects, and listing fixed effects, along with results without matching to establish a baseline relationship in the raw data.

Table 2 shows significant increases in price, demand, and revenue following the restriction of large-scale suppliers; however, these effects attenuate toward pre-treatment levels in the long run. The first two columns in Table 2 show that, on average, prices of pre-existing listings increase by 5.80% within the first year of policy implementation, but the magnitude is attenuated to 1.23%

when extending the post-treatment period to three years.[10] Columns 3 and 4 in Table 2 show that there is a 14 percentage point increase in the occupancy rate of Airbnb listings in the short run, but this change decreases to 6 percentage point in the long run. The impacts on revenue in the last two columns in Table 2 reveal a similar pattern: restricting large-scale hosts increases revenue by around 94% in the short run, but this positive effect declines to about one-third of its initial magnitude (around 30%) over time. Overall, these results suggest that, in the short run, the positive scarcity effect dominates, but the benefits for existing suppliers attenuate over time. This attenuation of gains suggests that large-scale suppliers do more than simply compete with incumbent hosts—they may also sustain the consumer experience and platform demand that small individual suppliers alone cannot maintain. We discuss this mechanism further in the next section.

Table 2 Relationship between restricting large-scale suppliers and market outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		*occupancy rate*		$\log(RevPAR+1)$	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
treated × after	0.0122***	0.0564***	0.0607***	0.1415***	0.2730***	0.6648***
	(0.0031)	(0.0035)	(0.0032)	(0.0045)	(0.0170)	(0.0234)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	744460	380584	744460	380584	744460	380584
N of listings	25401	25401	25401	25401	25401	25401

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

5.2 Mechanisms behind the transient benefits

A short-run deviation from the baseline may dissipate in the long run for two different reasons. First, on the demand side, restricting large-scale suppliers may induce an aggregate contraction if the intervention adversely impacts the platform's competitive position relative to outside options. Such a shock potentially diminishes the platform's variety or reliability, thereby triggering a substitution toward external competitors. Second, on the supply side, increased profits in the short run may induce the entry of new firms, thereby shifting the supply curve outward and normalizing prices or quantities.

[10] Percentage change is computed as $\exp(\beta) - 1$ for dummy variable in log-linear regressions.

Our empirical evidence does not support an interpretation where supply changes explain our results. As shown in Figure 1, the number of new entrants on the platform remains stagnant following the platform intervention. In Appendix Table A1, we provide regression results showing that restricting large-scale suppliers does not stimulate new entrants in the platform. Given this absence of a supply-side response, the remainder of this section motivates how demand-side dynamics can give rise to the long-run changes we observe.

5.2.1 Theoretical motivation

We develop a parsimonious model to illustrate how restricting large-scale suppliers changes consumer perceptions of the platform, shifting demand to outside options. These types of changes can in turn give rise to a long-run reversion of prices, occupancy rates, and revenue observed in our baseline analysis. A complete discussion of the model is available in the Appendix, here we provide a non-technical overview.

The model has several critical assumptions. First, we simplify the distinction between large-scale and other suppliers to depend solely on their marginal costs, as large-scale suppliers have the advantage of lower marginal costs due to economies of scale. Second, we incorporate asymmetric information on the demand side into the model. This is a valid assumption in the real world, especially in platforms for experience goods. Given that consumers cannot directly observe the true quality of the product or service ex ante, a key feature of this model is that consumers' expected utility of participating in the platform relies on their beliefs about average consumer experience. A consumer's main goal is to maximize her utility. She does this by choosing the products or services provided in the platform, balancing quality against expense.

Existing literature has shown that large-scale suppliers provide a standardized, professionalized experience (Farronato and Fradkin 2022, Li and Srinivasan 2019) and have a better brand reputation (Luca 2016). Our data aligns with these findings, suggesting that large-scale suppliers provide significantly better quality on average.[11] As a result, restricting large-scale suppliers indicates departure of high-quality suppliers and the remaining suppliers may reduce quality investments in the platform. This may inadvertently degrade the aggregate consumer experience by shifting

[11] *Rating about communication*: Mean=8.97 if large-scale suppliers, Mean=8.80 if small individual suppliers, t-stats = 23.88***.
Rating about check-in: Mean=8.97 if large-scale suppliers, Mean=8.79 if small individual suppliers, t-stats = 27.07***.

the supply mix toward less efficient and casual individual providers. Additionally, we model the consumer belief updating process using a Bayesian learning framework, in which consumers update their beliefs about the platform's average consumer experience based on two observable signals: choice availability and ratings.

Consistent with the baseline empirical results, our model formally characterizes how restricting large-scale suppliers affects the platform's ability to compete with outside options and reduces demand on the platform via consumer experience, which ultimately gives rise to the observed transient benefits. Specifically, if such restrictions do not significantly alter the average consumer experience in the short run, the positive scarcity effect dominates, leading to higher prices and increased demand for incumbents. However, if the restrictions erode the average consumer experience in the long run, consumers gradually shift to outside options, thereby attenuating the short-run gains obtained by incumbent suppliers.

5.2.2 Empirical evidence of change in consumer experience

In this subsection, we provide empirical evidence consistent with the mechanism illustrated in the theoretical framework. We begin by examining how consumers' beliefs about the platform's average consumer experience respond to the restriction of large-scale suppliers. Specifically, we examine the implication of restricting large-scale suppliers for the two observable signals consumers use to update their beliefs: choice availability and ratings. We first look into whether restricting large-scale suppliers influences consumers' pre-stay experience during the choice stage. One implication of restricting large-scale suppliers at the choice stage is that the set of available options, particularly high-quality ones, becomes more limited. As a result, consumers need to plan further in advance to secure desirable listings, thereby complicating their decision-making process. Specifically, we consider the *Booking Lead Time* as a measure for the pre-stay experience, which is calculated as the number of days between the booked date and check-in date.[12] This variable captures the number of days a guest needs to plan in advance. A longer *Booking Lead Time* reflects a negative signal, as high-quality listings become harder to secure and necessitate earlier planning.

As described in Section 3.3, this variable contains measurement error because the recorded *Booked Date* is the date AirDNA's system identified the booking through its scraping and modeling

[12] `https://www.airdna.co/blog/vacation-rental-metrics-booking-lead-time`

of Airbnb publicly visible availability calendar, rather than the exact date the guest made the reservation. Consequently, there are some reservations where *Booked Date* values fall after the corresponding *Stay Date*. About 10% observations in our sample has negative value in the *Booking lead time*. This happens when AirDNA detects a reservation retroactively or if a host updates the calendar with a delay. However, because the AirDNA data is modeled from daily scrapes, we consider *Booked Date* as a proxy of when the reservation was made. As an alternative specification, we adjusted the negative values of *Booking Lead Time* to zero.

Table 3 presents the estimated results. The sample is a subsample of the one used in the main analysis, conditioning on having a reservation in both the pre-treatment and post-treatment periods. As a result, the number of listings is smaller than in the main analysis. The estimated coefficients show that restricting large-scale hosts leads to an increase of roughly 9 days in the required planning time. This effect remains of comparable magnitude in both the short and long run.

Table 3 Relationship between restricting large-scale hosts and booking lead time

	(1)	(2)	(3)	(4)	(5)	(6)
	Including negative values		Replacing negative with zero		Excluding negative values	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
treated × after	9.6228***	9.1746***	8.9980***	9.6707***	9.6163***	9.2021***
	(0.6304)	(0.6375)	(0.6246)	(0.6337)	(0.6308)	(0.6378)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	551596	276826	551596	276826	549811	275601
N of listings	22055	22055	22055	22055	21571	21571

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
The estimations are conditional on having reservation in both pre-treatment and post-treatment, and thus the sample is a subsample of the one in the main analysis.
Column 3 and 4 replace the negative booking lead time with 0.
Column 5 and 6 exclude the negative booking lead time.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

Furthermore, we suspect that the increase in booking lead time after the platform restricts large-scale suppliers will be larger for higher-quality listings. We estimate the regressions analogous to those in Table 3 but with an interaction with *Superhost* status.[13] We define *Superhost* as a time-

[13] Airbnb states "Superhosts are experienced hosts who provide a shining example for other hosts, and extraordinary experiences for their guest."

invariant variable which is equal to one if a host has ever gained the *Superhost* status in our sample period. The estimated coefficients are shown in Table 4. On average, guests need to plan 3–5 additional days in advance to reserve listings managed by *Superhosts*. The findings in Tables 3 and 4 suggest that restricting large-scale suppliers reduces the availability of listings on the platform, particularly among high-quality options.

Table 4 Relationship between restricting large-scale suppliers and booking lead time for listings owned by *Superhost*

	(1)	(2)	(3)	(4)	(5)	(6)
	Including negative values		Replacing negative with zero		Excluding negative values	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
Superhost × treated × after	2.7701***	4.2789***	2.8372***	4.5251***	2.7577***	4.2682***
	(0.4642)	(0.6838)	(0.4624)	(0.6788)	(0.4645)	(0.6838)
treated × after	9.1091***	8.5061***	8.4840***	9.0057***	9.1053***	8.5363***
	(0.6280)	(0.6384)	(0.6220)	(0.6347)	(0.6284)	(0.6387)
linear combination	11.8792***	12.7850***	11.3212***	13.5308***	11.8630***	12.8045***
	(0.7783)	(0.9069)	(0.7729)	(0.9008)	(0.7789)	(0.9071)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	551596	276826	551596	276826	549811	275601
N of listings	22055	22055	22055	22055	21571	21571

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
The estimations are conditional on having reservation in both pre-treatment and post-treatment, and thus the sample is a subsample of the one in the main analysis.
Column 3 and 4 replace the negative booking lead time with 0.
Column 5 and 6 exclude the negative booking lead time.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

After documenting the adverse effects on choice availability, we next examine whether restricting large-scale suppliers also affects the other signal consumers use to update their beliefs: ratings. We focus on two aspects: ratings about communication and check-in, which are specifically related to host behaviors that can be readily and quickly adjusted by Airbnb hosts.[14] The estimated results are shown in the first four columns in Table 5. Consistent with the theoretical model, we find that both the communication and check-in ratings about for Airbnb listings significantly decrease in the long run after the restricting large-scale hosts.

For individual hosts, the decline in ratings arises solely from reduced effort due to less competi-

[14]Guests feedback consists of four elements: a written comment, private comments to the host, a star rating about the overall experience, and star specific ratings regarding various dimensions.

tion with large-scale counterparts. For large-scale hosts, however, the decline reflects both reduced competition and the loss of economies of scale. If supplier scale matters in platforms, the effect on rating should be more pronounced for large-scale suppliers. We test this hypothesis in the last four columns in Table 5. As expected, the effect of restricting large-scale suppliers on rating is more negative for large-scale suppliers.

These findings indicate that, after restricting large-scale suppliers, consumer satisfaction declines. This effect arises not only from reduced competition, which weakens hosts' incentives to respond to unfavorable reviews, but also from the loss of economies of scale. As shown in the theoretical framework, changes in consumer experience emerge gradually over time. Accordingly, our empirical findings show that the negative effects are more pronounced in the long run. As a result, from the consumer perspective, users may experience persistently less-satisfied stays.

Table 5 Relationship between restricting large-scale suppliers and Airbnb quality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	rating communication		*rating check-in*		*rating communication*		*rating check-in*	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
treated × after	-0.0510^{*}	0.0299	-0.0552^{*}	0.0235	-0.0088	0.0628^{*}	-0.0131	0.0548^{*}
	(0.0289)	(0.0306)	(0.0288)	(0.0306)	(0.0315)	(0.0330)	(0.0313)	(0.0330)
large-scale × treated × after					-0.1530^{***}	-0.1190^{**}	-0.1528^{***}	-0.1133^{**}
					(0.0509)	(0.0503)	(0.0510)	(0.0508)
net effect for large-scale host					-0.1619^{***}	-0.0562	-0.1659^{***}	-0.0584
					(0.0481)	(0.0487)	(0.0484)	(0.0491)
Listing FE	Y	Y	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y	Y	Y
N of obs	744460	380584	744460	380584	744460	380584	744460	380584
N of listings	25401	25401	25401	25401	25401	25401	25401	25401

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

Together, Table 3 to Table 5 reveal that restricting large-scale suppliers has negative impacts on both review ratings and the availability of high-quality options on the platform. These findings provide empirical evidence aligned with predictions from the theoretical model that restricting large-scale suppliers lowers consumer experience on the platform, with these effects becoming increasingly pronounced over time. As shown in the model, the growing magnitude of these negative effects on consumer experience over time plays an important role in the evolution of market dynamics. The delayed adjustments in consumer choice reflecting not only inertia but also gradual learning and repeated negative experiences. Our results highlight that competition from large-scale suppliers are important in delivering a superior consumer experience and sustaining platform's reputation, as individual providers alone are insufficient to maintain competitive intensity and market efficiency at a comparable level.

5.2.3 Change in demand for outside options

In the previous subsection, we provided empirical evidence that restricting large-scale suppliers reduces consumer experience on the platform, particularly in the long run. We next examine whether this deterioration in consumer experience leads to a shift in demand toward outside options, as demonstrated by the theoretical framework.

We investigate this using zip-code level hotel performance data. The estimated results are shown in Table 6. The results show that while there is little change in hotel demand or occupancy rate in the short run after the policy, in the long run there is a roughly 4% increase in the hotel demand and 2.2 percentage point increase in the occupancy rate.[15] Consistent with our model predictions, these findings suggest that the demand is not immediately redirected to outside options but instead shifts gradually over time as consumers learn and update their beliefs.

[15] Because the variables *hotel demand* and *hotel occupancy rate* are only available for zip-codes with at least four hotels to preserve anonymity, we supplement our analysis of changes in hotel activity with *hotel rooms*, which is from the initial census information and thus available for all hotels. Although this variable measures supply rather than demand, the estimated results exhibit patterns consistent with the impact on hotel demand, showing a significant increase in the long run. These results provide further evidence that outside options, such as hotels, capture part of the demand after a reduction in supply from large-scale Airbnb hosts.

Table 6 Relationship between restricting large-scale suppliers and hotel demand

	(1)	(2)	(3)	(4)
	$\log(\#hotel\ demand + 1)$		*hotel occupancy rate*	
	$<= t + 36$	$<= t + 12$	$<= t + 36$	$<= t + 12$
treated × after	0.0439***	0.0032	0.0215***	0.0106
	(0.0119)	(0.0167)	(0.0077)	(0.0117)
Zip-code FE	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y
N of obs	5397	2676	5397	2676
N of zip-codes	117	117	117	117

Note: The sample is a subset of the main sample and an unbalanced panel because *#hotel demand* and *hotel occupancy rate* are available if a zip-code has at least four hotels.
Robust standard errors in parentheses, clustered by zip-code.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
Controlling for zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code vacancy rate, zipcode percentage black population.

5.3 Heterogeneous effects on marginal suppliers in the sharing economy

Thus far, we have focused on the role of supplier scale in driving platform long-term sustainability across multi-sided platforms, extending beyond the sharing economy. In this section, we shift our attention to the specific context of sharing economy. In the sharing economy, platform interventions often aim to ease the burden on marginal suppliers who are in economically disadvantaged positions. Those marginal suppliers are generally less efficient, face greater competitive pressure from the top-tier suppliers, and therefore bear a disproportionate burden when competing against these lower-cost (and plausibly higher-quality) providers. However, despite their lower efficiency, they may still be an important component in the sharing economy for various reasons such as preserving its peer-to-peer nature and providing locally diverse experiences. Accordingly, it is important to understand how these marginal suppliers adjust their behavior strategically after competition from large-scale providers weakens and how effective these adaptations are.

It is not ex ante clear whether these marginal suppliers can disproportionately benefit from restricting large-scale suppliers. Rationally, marginal suppliers should gain stronger benefit if they face greater competitive pressure from the more efficient suppliers, as the reduction in that pressure disproportionately eases their competitive burden. However, if their responses deviate from rational adjustment, these supply-side shifts may not benefit them. For example, suppliers may misinterpret platform changes due to lack of resources and adopt strategies that unintentionally worsen their own outcomes (Koo and Eesley 2021).

Specifically, we consider Black hosts as an example of marginal suppliers as they face more market frictions for reasons such as statistical discrimination (e.g., Alyakoob and Rahman 2025, Laouénan and Rathelot 2022, Luca et al. 2024) and limited access to education and resources (e.g., Bayer and Charles 2018, Chetty et al. 2020). We construct a dummy variable, $Black_{ij}$, equal to one if the listing is owned by a Black host, zero otherwise. We estimate the regressions analogous to those in the baseline specification (as in Table 2) but with an interaction with $Black_{ij}$. The analysis is conducted using a subsample of the sample used in the main analysis because not all hosts can be observed with race information for reasons described in Section 3.3. We find no systematic differences in our outcomes of interests between listings with and without host race information. Details are shown in Section 5.4.4.

The estimated coefficients in Table 7 show that listings owned by Black hosts experience a higher price increase but obtain a lower demand and revenue increase, relative to the ones managed by non-Black hosts. Numerically, after restricting large-scale hosts, listings owned by Black hosts experienced, on average, a 3% greater price increase than those owned by non-Black hosts, but an approximately 0.04 smaller increases in occupancy rate and an about 20% smaller increases in revenue relative to their counterparts. These initial findings suggest that, despite charging higher prices, Black hosts benefit less than non-Black hosts from restricting large-scale suppliers in terms of both demand and revenue, which exacerbates disparities.

Table 7 Relationship between restricting large-scale suppliers and market outcomes for listings owned by Black hosts

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price + 1)$		*occupancy rate*		$\log(RevPAR + 1)$	
	$<= t + 36$	$<= t + 12$	$<= t + 36$	$<= t + 12$	$<= t + 36$	$<= t + 12$
Black × treated × after	0.0357**	0.0082	−0.0428***	−0.0405***	−0.2371***	−0.1956**
	(0.0178)	(0.0134)	(0.0161)	(0.0153)	(0.0869)	(0.0808)
treated × after	−0.0060	0.0209***	0.0535***	0.1070***	0.2449***	0.5022***
	(0.0057)	(0.0059)	(0.0061)	(0.0090)	(0.0337)	(0.0455)
linear combination	0.0297*	0.0291**	0.0107	0.0665***	0.0078	0.3066***
	(0.0166)	(0.0129)	(0.0157)	(0.0159)	(0.0839)	(0.0822)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	262169	136693	262169	136693	262169	136693
N of listings	9220	9220	9220	9220	9220	9220

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
This sample is a subsample of the sample used in the main analysis, conditioning on host race information being observed.
We find no systematic difference between listings with and without host race information.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

We further investigate the role of inexperience in amplifying disparities between listings owned by Black and non-Black hosts. A plausible explanation is that, due to limited experience, Black hosts may misinterpret the extent of reduced competition from experienced large-scale hosts (Koo and Eesley 2021), failing to account for the concurrent decline in consumer experience and the reallocation of some excess demand to outside options. As a result, they may set suboptimal prices and miss out on proportional increases in demand or revenue. For example, Zhang et al. (2021) find that pricing algorithm can help with a downward price correction and thereby increase both demand and revenue, but Black hosts were 41% less likely than White hosts to adopt the algorithm. We test this hypothesis by examining whether the phenomenon of charging higher prices while gaining smaller increases in demand and revenue primarily occurs among listings owned by less experienced Black hosts. We use the underutilization of algorithmic pricing tools as a proxy for limited experience and provide additional evidence from other dimensions of inexperience as a robustness check in Section 5.4.2. Our data do not allow us to directly observe whether individual listings employ algorithmic pricing tools. Existing literature has shown that algorithmic pricing is associated with higher variability in price, as these algorithms adjust more frequently to daily market changes (e.g., Aparicio and Misra 2023, Aparicio et al. 2024, Assad et al. 2024, Brown and MacKay 2023). Therefore, we use the variability in daily listing price as a proxy of the adoption of algorithmic pricing tools, and define a dummy variable, $NoAlgorithm = 1$ if variance in daily price is below the median value.[16] In Table 8, our data provide some evidence that listings owned by less experienced hosts adjust their daily prices less frequently. We acknowledge that this is not a direct measure of whether a listing uses pricing algorithm to set the price or not, but, to some extent, variability in daily prices reflects suppliers' sophistication and responsiveness to market conditions, making it a suitable proxy in our context.

Table 8 Comparison in variability in daily price

	Mean	**t-statistic**	**p-value**
Large-scale hosts	2,368	110	0.000
Individual hosts	1,117		
Non-Black hosts	1,223	22.18	0.000
Black hosts	662		
Other hosts	1,232	30.18	0.000
Black and Individual hosts	363		

Consistent with our prior, the coefficients in Table 9 show that the significant increases in price but decreases in occupancy rate and revenue after restricting large-scale suppliers are mainly among listings owned by Black hosts who do not adopt algorithmic pricing tools. Compared to Black hosts who adopt algorithmic pricing tools, although the increase in price remains similar in both the short and long run, the decline in demand and revenue is more pronounced over the long run than in the short run. In other words, Black hosts who do not adopt algorithmic pricing tools are less likely to adjust their prices over time and consequently experience smaller gains in demand and revenue, particularly in the long run. These findings suggest that the observed larger price increase likely reflects a suboptimal adjustment to the decline in the supply of large-scale suppliers, stemming from a lack of experience, such as underutilization of algorithmic pricing tools. Hosts with less experience tend to set higher prices and make less frequent adjustments in response to changing circumstances (Wang et al. 2023, Zhang et al. 2021). This suboptimal upward price adjustment could result in constrained or even negative gains in demand and revenue for listings

[16]The results are robust to the lower threshold (i.e., 25th percentile), but are not to a higher threshold (i.e., 75th percentile).

owned by Black hosts. Because the demand for their listings is more price-sensitive than that of comparable counterparts, overestimating the reduction in competition with large-scale hosts and raising prices accordingly can disproportionately harm Black hosts (Zhang et al. 2021). These findings highlight the nuanced role of large-scale suppliers who are more experienced for marginal providers such as Black suppliers: these economically-disadvantaged suppliers may misjudge the extent of reduced competition with large-scale suppliers, as part of the excess demand shifts to outside options, leading them to adjust prices suboptimally and realize smaller gains in demand and revenue.

Table 9 Relationship between restricting large-scale suppliers and market outcomes for listings owned by Black hosts (Algorithmic pricing)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		*occupancy rate*		$\log(RevPAR+1)$	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
NoAlgorithm × Black × treated × after	0.0239	0.0209	−0.0430**	−0.0209	−0.1796*	−0.0913
	(0.0245)	(0.0192)	(0.0194)	(0.0179)	(0.0975)	(0.0883)
Black × treated × after	0.0021	−0.0131	0.0103	0.0003	0.0316	0.0035
	(0.0190)	(0.0165)	(0.0163)	(0.0150)	(0.0838)	(0.0752)
NoAlgorithm × treated × after	−0.0084	−0.0077	0.0192***	0.0177***	0.0573	0.0437
	(0.0065)	(0.0059)	(0.0064)	(0.0061)	(0.0371)	(0.0360)
treated × after	−0.0010	0.0252***	0.0431***	0.0967***	0.2095***	0.4725***
	(0.0062)	(0.0066)	(0.0064)	(0.0092)	(0.0389)	(0.0492)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	262169	136693	262169	136693	262169	136693
N of listings	9220	9220	9220	9220	9220	9220

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
This sample is a subsample of the sample used in the main analysis, conditioning on host race information being observed.
We find no systematic difference between listings with and without host race information.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

5.4 Robustness checks

5.4.1 Alternative sample

Excluding other potential shocks

As mentioned in the earlier section, Figure 1 reveals a peak in the number of total new listings in the control cities around January 2017, which corresponds to major events in Washington DC (*presidential inauguration*) and Houston (*Superbowl*). Therefore, we re-plot the change in the number of new listings over time in Figure A1 and rerun the baseline regressions in Table A2 in the Appendix by excluding those two cities. The results are consistent with our findings in the main analysis.

Entire-home apartment listing

This platform intervention is to limit the number of *entire-home listing* under each host to one. Additionally, the *entire-home listings* are more likely to be with comparable price and amenities. As a result, these listings face the most direct competition from the regulated listings and are expected to be the most affected by the restriction on large-scale hosts. We rerun our main analysis by using only *entire-home listings.* The estimated results are shown in Table A3. As expected, after limiting the sample to only entire-home listings, the effects of restricting large-scale suppliers on price, demand, and revenue are directionally consistent with the findings using the whole sample, but slightly larger in magnitude.

5.4.2 Alternative measures

Demand

In the main analysis, we use the occupancy rate of listings as a proxy of demand. Following Alyakoob and Rahman (2025), we measure demand using two alternative metrics: (1) Number of reservations, and (2) Number of booked days. *Number of reservations* refers to the number of unique reservations made for a listing in a month, which captures the number of guests who are willing to transact with a particular host. *Number of booked days* refers to the total booked days corresponding to the reservations. The estimated results in Table A4 are consistent with the main analysis, showing that there is a significant short-run increase for listings by the existing suppliers after restricting large-scale suppliers, while the magnitude of these benefits diminishes substantially in the long run. These findings provide further evidence that restricting large-scale hosts yields only temporary demand gains for the existing suppliers.

Policy implementation as alternative treatment

Although Figure 1 shows that the decline in listings from large-scale hosts began in April 2016, when the policy was announced, one might be concerned that the actual implementation could have effects different from those indicated by the announcement. To address this concern, we use the implementation month as the treatment month and excluding the period between the announcement and implementation to estimate the effect of the policy's implementation. The estimated results are shown in Table A5 which are broadly consistent with our main analysis.

Additional evidence on the role of experience

In section 5.3, we show that the disproportionately larger increase in price but smaller gains in occupancy rate and revenue after restricting large-scale suppliers are mainly among listings owned by Black hosts who are lack of experience, such as not adopting algorithmic pricing tools. These inexperienced hosts may misinterpret the reduced competition from experienced providers, as the deterioration of consumer experience and the reallocation of demand to outside options further obscure underlying market signals. In this section, we provide further evidence that the observed larger price increase likely reflects a suboptimal adjustment to a decline in large-scale suppliers because of limited experience. We consider two additional proxies of limited experience: (1) *Individual* hosts, and (2) *Less-active* listings, which is defined as listings with zero occupancy rate for six or more consecutive months. Table A6 and Table A7 present the estimated results. The coefficients in both tables reveal similar patterns that the larger increase in price but less benefits in occupancy rate and revenue after restricting large-scale suppliers are mainly among listings owned by Black hosts who are individuals or whose listings are less active. These findings provide further evidence suggesting that, due to limited experience, Black suppliers may misjudge the degree of reduced competition with large-scale suppliers because consumer experience is weakened and demand also shifts to outside options, leading them to charge higher prices but gain less in demand and revenue.

5.4.3 Alternative specifications

Alternative model specifications

First, Table A8 presents the baseline specification by only controlling for listing fixed effects, year-month fixed effects, and city-specific time trends, to establish a baseline correlation. Furthermore, Table A9 shows the estimated coefficients prior to matching, documenting the relationship in the raw data. Another concern is about the potential serial correlation within a geographical location. To address this concern, we rerun our analysis by clustering the standard error at zip-code level. The results are shown in Table A10. All the alternative model specifications reveal similar results to our main findings.

Event study model

To examine the dynamic effects of restricting large-scale suppliers, we employ an event study model to track how these effects evolve over time. We conduct the analysis on a quarterly basis to account for seasonality. We estimate the model separately for price, demand, and revenue. The estimated results are plotted in Figure A2 to Figure A4. First of all, the estimated results across the three figures show that there exist no significant effects or nearly zero effects on price, demand, and revenue during the pre-treatment period, although there is certain anticipation effect in period just before the treatment. These results provide some evidence that the treatment and control groups follow parallel trends in the outcome variables during the pre-treatment period, suggesting that the key identifying assumption of the DiD design is unlikely to be violated in our case.

As shown in Figure A2, there is a significant increase in price immediately following the treatment occurs, but the magnitude and the statistical significance level significantly decreases at the end of the sample period. Figures A3 and A4 reveal similar patterns in the dynamic effects on demand and revenue: although there is a significant immediate increase following the treatment, these positive effects diminish in magnitude and become statistical insignificant over time. These dynamic effects also suggest that a 36-month post-treatment period is sufficient to capture both the short-run and long-run dynamics.

Additionally, as previously noted, we re-estimate these relative time models excluding listings in Houston and Washington, D.C., to mitigate potential bias from concurrent major events. The results are shown in Figure A5 to Figure A7, which exhibit consistent yet more pronounced patterns.

Doubly robust machine learning estimator

To address the potential endogeneity and misspecification issues, we re-estimate our baseline results using a doubly robust machine learning (DR-ML) approach. Without random treatment assignment, the identification of the average treatment effect depends on the unconfoundedness assumption, which requires selection on observables. This entails collecting a broad set of covariates and modeling them to approximate conditional random assignment. Traditionally, researchers have relied on linearly controlling for a set of pre-chosen covariates and linear controls, partly due to the difficulty of modeling confounders flexibly and non-parametrically. However, the assumption on the linearity of the confounders has been widely criticized as overly strong and often invalid. Building on recent advances in causal inference with doubly robust machine learning (DR-ML) (e.g., Chernozhukov et al. 2018), we are able to model confounders in a flexible and nonparametric way.

We first provide a brief outline of this method in our context. Suppose Y_{it} stands for the outcomes: price, demand, and revenue, along with a binary treatment indicator W_{it} which equals to one if the listings are located in New York City or San Francisco and the time period is after April 2016; zero otherwise, X_{it} contains a set of covariates.

Then we have a partially linear model:

$$Y_{it} = \tau W_{it} + f(X_{it}) + \epsilon_{it}, E[\epsilon_{it}|X_{it}, W_{it}] = 0 \tag{2}$$

where τ denotes the constant by which the treatment assignment shifts the outcome. We define the two intermediary objects: (1) the conditional expectation of being shocked by the "One Host, One Home" policy: $e(x) = E[W_{it}|X_{it} = x]$ and (2) the conditional mean of the outcome variable: $m(x) = E[Y_{it}|X_{it} = x] = f(x) + \tau e(x)$. The Equation (3) can be rewritten as

$$Y_{it} - m(x) = \tau(W_{it} - e(x)) + \epsilon_{it} \tag{3}$$

where τ can be estimated by a residual-on-residual regression. The machine learning method, such as random forests, can provide a flexible and better prediction for $m(x)$ and $e(x)$ (and the subsequent residuals for each observation), thereby non-parametrically controlling for confounding factors when estimating the average treatment effect and resulting in desirable statistical properties, such as unbiasedness and consistency (Athey et al. 2019). The estimator is considered to be "doubly robust" because the estimated average treatment effect remains consistent as long as either function is correctly specified, thereby offering protection against bias from model misspecification (Athey et al. 2017, Athey and Imbens 2017, Chernozhukov et al. 2018).

We apply a random forest algorithm to estimate $e(x)$ and $m(x)$ since it is nonparametric and can handle overfitting concerns well. We split the data in half and use the half without observation i to estimate the value for i. To predict the propensity of observation i being treated, we utilized the following features: housing value, number of reviews, months since being listed, number of bedrooms, listing type, pets allowed, smoking allowed. Those are the features that we think can be used to match the listings in the treated and control cities to be comparable, and do not include variables that might be changed by the listing after they get treated by the policy intervention. We also include year-month fixed effects. One key assumption is the positivity (overlapping) assumption that all units are not perfectly predicted to be in the treated or control group. Although technically the assumption only requires that all units have a non-zero probability of assignment to each treatment condition, to err on the side of caution, we restrict the range of propensity score between 0.05 and 0.95 and keep only listings having at least half of the observations with propensity score in this range (this reduces our sample size to 11,025 listings). This is critical as the DR-ML model relies on the positivity assumption in addition to unconfoundedness for identification.

To predict the conditional mean of outcome variable, we used all the variables used to predict the treatment propensity as well as additional variables that may impact a listing's market outcomes. Specifically, we added the following variables: number of listings within the zip-code, population, number of households, median income, total number of listings under the host, Superhost status. We also include listing fixed effects and city-specific time trends. We then use a Causal Forests (CF) model to identify the average and individual treatment effect. Causal Forests are adaptations of the random forest algorithm which make predictions using a weighted average of similar observations (Breiman 2001). This method estimates a generalized random forest that partitions the data by decision trees, clustering Airbnb listings that have a similar propensity to be treated into the same terminal node. The process is repeated 10,000 times to grow 10,000 causal trees. To reduce bias in tree predictions, we use *honest* forests. This requires that different subsamples be used to construct the tree and make treatment predictions. For each observation, a weighted average of treatment impact is obtained from the trees that the observation was randomly selected into (Athey et al. 2019).

The estimated results obtained from the DR-ML method, presented in Table A11, are broadly consistent with the baseline estimation, although the point estimation is smaller in magnitude and the attenuation of long-run effects appears slightly less pronounced. Moreover, the distribution of individual treatment effect shows that, in terms of price, short-run effect has a wider distribution than the long-run effect, while the distribution of long-term effects on demand and revenue are shifted to left compared with the distribution in the short term.

Synthetic differences-in-difference (SDiD)

To ensure the robustness of our findings beyond the event study model, we employ Synthetic Difference-in-Differences (SDiD), a hybrid estimator introduced by Arkhangelsky et al. (2021). SDiD addresses the inherent limitations of both traditional Difference-in-Differences (DiD) and the Synthetic Control Method (SCM) by combining unit and time fixed effects with a reweighting scheme. While traditional DiD relies on the strict parallel trends assumption, SDiD relaxes this by weighting donor units to match the pre-treatment trends of the treated group. Furthermore, it improves upon SCM by incorporating time weights that prioritize pre-treatment periods most representative of the post-treatment era, thereby providing more stable and doubly robust inference.

Applying this method requires a sufficiently long and balanced pre-treatment window to construct a valid counterfactual. At the individual listing level, many units lack the continuous observations necessary for this estimation. Simply dropping these sparse units would be problematic, as the "activeness" of a listing is highly correlated with our key research context, and thus, such a restriction would likely introduce selection bias. To mitigate this while maintaining the integrity of the SDiD requirements, we aggregate the listing-level data to the zip-code level. This spatial aggregation provides a consistent panel of observations, allowing for a more reliable construction of synthetic controls without sacrificing the representative nature of our sample.

The estimated results are shown in Figure A8 to Figure A10, which reveal similar patterns to our main findings. There is a significant increase in price, demand, and revenue immediately after restricting large-scale suppliers, but such positive effects diminish over time.

5.4.4 Other potential bias

Relisting behavior

Another concern is the potential for large-scale hosts to relist properties to bypass the regulation, which could bias our estimates if these hosts continue to manage all the listings. For example, a host who previously managed multiple listings might transfer them to friends or family members. We argue that, in theory, such a relisting strategy is costly. First, relisting carries legal and financial liabilities, as the platform requires legal documentation to verify host identities, and these verified owners remain responsible for any future incidents. Second, according to the platform design on Airbnb, transferring host ownership will lose previous review information for that listing, which would be another potential cost for the hosts. In practice, we provide some evidence from our data that such hosts consist of a negligible fraction of our sample. Because Airbnb only provides approximate geographical information of listings, rather than accurate coordinates, we examine the relisting behavior by looking into the new host entrants in the same location, proxied by a six-decimal coordinate range (no more than 0.11 meters apart) surrounding listings that were previously managed by large-scale hosts. We use the number of new host entrants in the same area surrounding listings owned by all hosts as a reference point and compare it with the number of new host entrants around listings owned specifically by large-scale hosts. If significantly more new hosts enter in approximately the same locations where listings were previously owned by large-scale hosts, it suggests the possibility of relisting behavior, as these may be the same properties under different host identities. We examined the numbers over the entire post-treatment period and within the first three months following the policy implementation, respectively. As shown in Table A12, on average, there is no significant difference in the number of new entries around all listings and around listings previously managed by large-scale hosts even after expanding the coordinate range to four-decimal (similar to 11.1 meters), and the average number of entries is relatively low. All the evidence shows that the potential relisting behavior of large-scale hosts, if any, would not be likely to bias our results.

Self-selection in posting facial information

As described in Section 3.3, we are not able to observe the information on host race for the whole sample used in the main analysis for various reasons. Existing literature has shown that business owners with better performance are more willing to disclose (e.g., Avery et al. 2023, Ho et al. 2019). As a result, one concern is that listings with and without host race information may systematically differ in their market performances such as price, demand, and revenue. To address this concern, we begin by plotting the distributions of price, occupancy rate, and revenue for listings with and without host race information. Figure A11 shows that there is no significant difference between these two subgroups with respect to these market outcomes of interests. Furthermore, we re-estimate the baseline results (i.e., Table 2) in Table A13 using only the sample with host race information. The estimated results are broadly consistent with the baseline results using the whole sample, though the magnitude is slightly smaller. These results suggest that the potential self-selection based on host race information being observed is unlikely to be a major concern.

6 Conclusions

There is a longstanding but debated body of research examining the impact of competition within digital platforms, yet the specific role of supplier scale remains underexplored. As platforms evolve, suppliers scale their operations; however, in practice platforms adopt divergent strategies regarding whether to encourage or restrict these large-scale participants. Leveraging a unique quasi-experiment policy shock of restricting large-scale suppliers on Airbnb in New York City and San Francisco, we examine the importance of supplier scale in reshaping consumer experience and incumbent supplier market outcomes over both the short and long term. On the one hand, large-scale suppliers have lower marginal cost and better operation efficiency, providing more responsive and professional service. On the other hand, restricting large-scale suppliers can create space for small individual suppliers. Collectively, these smaller participants may maintain a competitive environment that effectively rivals large-scale external alternatives. By applying a difference-in-differences combined with matching approach across eleven major U.S. cities, we shed light on the extent to which large-scale suppliers are essential to a platform's ability to sustain quality provision, retain consumers, and compete with outside options, and whether individual suppliers alone can fill this role in their absence.

Our baseline findings show that while restricting large-scale suppliers generates short-run benefits for incumbent suppliers in terms of price, demand, and revenue, these gains are not sustained in the long run. Instead, the reduction in competition from large-scale suppliers undermines the average consumer experience on the platform and increases demand for outside alternatives. These results extend prior work on the competition among heterogeneous suppliers by demonstrating that restricting large-scale suppliers yields limited benefits but substantial long-term drawbacks. Although they may crowd out individual players, they also enhance competition, maintain operation efficiency, and support the long-term sustainability of the platform. Lack of them, individual suppliers often lack the resources to maintain a competitive environment; consequently, they struggle to achieve the service quality and consumer satisfaction levels necessary to rival large-scale external alternatives.

Furthermore, we find that restricting large-scale players does not necessarily benefit marginal suppliers; instead, it may exacerbate existing disparities. In the context of the sharing economy, platform interventions are often designed to favor marginal participants, such as Black suppliers, who may be in economically disadvantaged positions. However, our findings show that Black suppliers may not benefit from such reduced competition from large-scale players due to lack of experience, such as not adopting algorithmic pricing tools. As a result, they may misjudge the extent of reduced competition if other market dynamics are not considered. For example, some of the demand may shift to alternative options due to the persistent decline in consumer experience. This can lead inexperienced Black suppliers to raise prices suboptimally, yet gain less in demand and revenue compared to their counterparts. This underscores the importance of accounting for heterogeneity in supplier characteristics, as certain subgroups may misinterpret market changes and take actions that inadvertently harm themselves.

Taken together, our study contributes not only to the growing literature on supply-side competition in digital platforms and the nuanced roles of heterogeneous suppliers, but also offers important managerial implications. For platform owners, our results highlight the critical role of large-scale suppliers in platform market dynamics and long-term sustainability, especially in a competitive environment. Limiting these suppliers may temporarily increase short-run gains for other existing players, but it can reduce overall platform quality, reliability, and its ability to compete with large-scale outside options. Large-scale suppliers maintain the competition level which individual suppliers alone cannot sustain. Without their advantages of economies of scale, platforms risk declining consumer satisfaction and losing market share to outside alternatives. To address this, platform owners should consider differentiated governance mechanisms—such as incentive schemes, quality assurance programs, or targeted support for less experienced suppliers—that preserve the benefits large-scale suppliers bring while allowing other suppliers to compete effectively.

For policymakers, the findings emphasize the complexity of managing supplier scale in digital platforms. Large-scale suppliers provide professional, reliable, and efficient services that benefit consumers, and restricting their participation can weaken competition, reduce consumer welfare, and not necessarily improve equalities among suppliers. Policymakers should adopt nuanced approaches, such as differentiated licensing or targeted support, that create space for small individual suppliers while recognizing the value large-scale suppliers contribute to the ecosystem.

This paper is not without limitations. First, our study provides additional depth and insights for the importance of large-scale suppliers in shaping platforms by examining whether and how restricting these suppliers affects market dynamics for other existing participants on the platform. Previous literature also documents the significant impact on local communities and welfare. Future research could build on this work by examining the role of large-scale suppliers in shaping these community dynamics and performing formal welfare analysis. Second, we focus on the heterogeneous effects of large-scale suppliers on market performance for marginal players, proxied by Black hosts, and show that they may not benefit from reduced competition with those large-scale suppliers due to misinterpreting market dynamics. Other important dimensions of heterogeneity may also exist, such as across geographical areas, where supplier scale could exert more nuanced influences. However, our data do not allow us to fully disentangle potential confounding factors across locations. Future research drawing on alternative data sources could further explore the heterogeneous role of supplier scale across different geographical contexts. Finally, although we provide suggestive evidence on experience or skills, such as the use of algorithmic pricing tools, our data do not allow us to directly observe whether such tools are actually employed to set listing prices. Moreover, the level of experience can be reflected in other aspects beyond what we capture here, such as individual's education level or primary occupation. Future research could benefit from more granular data and direct measures to provide more insights in how suppliers' expertise shapes their responses to changes in supply.

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A Theoretical Framework

A.1 Model setup

On the supply side, there is a continuum of potential options, normalized to a total mass of N. In each period t, each option i has its own price p_{it} to maximize the corresponding profit. There is heterogeneity on the supply side and each option i is associated with a marginal cost c_{it}. The cost c_{it} is distributed over the interval $[c_{min}, c_{max}]$ according to a cumulative distribution function $F(c)$ and a probability density function $f(c)$. Critically, we make the assumption that an option's true quality, q_{it}, is a decreasing function of its marginal cost. This means that options with low marginal costs are the high-quality ones. As noted in Li and Srinivasan (2019), large-scale suppliers in the sharing economy have lower costs of managing per asset due to economies of scale and specialization. By operating at scale, these suppliers achieve greater operational efficiency in areas such as dynamic pricing, maintenance, and staffing (Kemmis 2020, Li and Srinivasan 2019).[17]

Assumption 1: $q_{it} = q(c_{it})$, *with* $q'(c) < 0$.

On the demand side, there is a continuum of potential consumers, normalized to a total mass of M. A consumer j browses all the goods or services $\{i\}$ in each period $t \in \{0, 1\}$ and gets a specific utility from each one. For simplicity, period 0 denotes the pre-treatment period, period 1 denotes the post-treatment. The utility for consumer j from choosing option i in period t is:

$$U_{ijt} = \alpha q_{it} - p_{it} + \varepsilon_{ijt}$$

However, because consumers could not observe the true quality of each option, they can only obtain a perception on the quality of the option based on observed signals. As a result, we re-write a consumer's utility function as

$$U_{ijt} = \theta r_{it} + \gamma \mu_t - p_{it} + \epsilon_{ijt}$$

where r_{it} is option i's rating in period t, μ_t is the expected consumer experience from using the platform in period t (reputation), ϵ_{ijt} is an idiosyncratic taste shock. To get a closed form solution,

[17] In our data, on average, suppliers managing multiple listings (i.e., large-scale suppliers) have higher ratings. *Rating about communication*: Mean=8.97 if large-scale, Mean=8.80 if individual, t-stats = 23.88***. *Rating about check-in*: Mean=8.97 if large-scale, Mean=8.79 if individual, t-stats = 27.07***.

we follow the standard logit model assumption.

Assumption 2: *ϵ_{ijt} follows an i.i.d. Type I Extreme Value (Gumbel) distribution. The market share for option i in period t is*

$$s_{it} = \frac{e^{\theta r_{it}+\gamma\mu_t-p_{it}}}{e^{\delta_0} + \sum_{k=1}^{N} e^{\theta r_{kt}+\gamma\mu_t-p_{kt}}}$$

where e^{δ_0} represents the value of the outside option.

Denoting $N = N_1 \cup N_2$, we can rewrite the market share (demand) for supply i in period t as

$$\begin{aligned} s_{it} &= \frac{e^{\theta r_{it}+\gamma\mu_t-p_{it}}}{\underbrace{\sum_{m\in N_1} e^{\theta r_{mt}+\gamma\mu_t-p_{mt}}}_{\text{High-cost Supply}} + \underbrace{\sum_{n\in N_2} e^{\theta r_{nt}+\gamma\mu_t-p_{nt}}}_{\text{Low-cost Supply}} + \underbrace{e^{\delta_0}}_{\text{Outside Option}}} \\ &= \frac{e^{\gamma\mu_t} \cdot e^{\theta r_{it}-p_{it}}}{e^{\gamma\mu_t} \cdot (\sum_{m\in N_1} e^{\theta r_{mt}-p_{mt}} + \sum_{n\in N_2} e^{\theta r_{nt}-p_{nt}}) + e^{\delta_0}} \end{aligned} \tag{4}$$

Each option i chooses their price p_{it} in period t to maximize their own profit, π_{it}:

$$\pi_{it}(p_{it}) = (p_{it} - c_{it}) \cdot D_{it}(p_{it}, \mathbf{p_{-it}})$$

where $\mathbf{p_{-it}}$ represents the set of prices from all other options. To find the profit-maximizing price, we take the derivative of the profit function π_{it} with respect to p_{it} and set it to zero.

$$\text{F.O.C. 1: wrt } p_{it} \text{ yields: } \frac{\partial \pi_{it}}{\partial p_{it}} = D_{it} + \left[(p_{it} - c_{it}) \cdot \frac{\partial D_{it}}{\partial p_{it}}\right] = 0$$

As established in the model setup above, the demand for option i in period t, D_{it}, is their share s_{it} of the total market M:

$$D_{it} = M \cdot s_{it}$$

A standard result from the Logit model is that

$$\frac{\partial D_{it}}{\partial p_{it}} = -M \cdot s_{it}(1 - s_{it}) = -D_{it}(1 - s_{it})$$

Substituting this back into the F.O.C. 1, we get:

$$D_{it} + (p_{it} - c_{it}) \cdot [-D_{it}(1 - s_{it})] = D_{it}[1 - (p_{it} - c_{it})(1 - s_{it})] = 0$$

For any active option (i.e., $D_{it} > 0$), we have $1-(p_{it}-c_i)(1-s_{it}) = 0$. This gives us the equilibrium condition for every option i in period t:

$$p^*_{it} = c_{it} + \frac{1}{1 - s^*_{it}} \tag{5}$$

where $\frac{1}{1-s_{it}}$ is the price markup.[18]

A.2 Bayesian belief and consumer learning process

Consumers' initial belief λ is the likelihood of successfully choosing a high-quality option on the platform, which is updated to form the prior belief $\mu(\lambda, y)$ using consumers' own previous consumption experience on the platform or word-of-mouth from others about their experience on the platform summarized by the index y. That index y could be any function of the information available to date about the options on the platform. It could be considered as a known function that is increasing in the positive experiences by either consumer herself or others, and decreasing in the negative ones.

To update her initial belief λ in a Bayesian sense, the consumer specifies probability $\sigma_h = Pr(y|p = p_h)$ that consumer's or others' past experiences are quantified as y given that the likelihood of choosing a high-quality option on the platform is high, and $\sigma_l = Pr(y|p = p_l)$ given that the likelihood of choosing a high-quality option on the platform is low. The Bayesian belief that the likelihood of choosing a high-quality option on the platform is high is then given by

$$\mu_0(\lambda, y) = \frac{\lambda\sigma_h(y)}{\lambda\sigma_h(y) + (1 - \lambda)\sigma_l(y)}$$

This shows that $\mu_0(\lambda, y)$ is increasing in λ. A better own or others' previous experience on the platform increase the belief $\mu_0(\lambda, y)$ is specified by the monotone likelihood ratio property (MLRP)

[18] An option with a high market share s_{it} has a very small $(1 - s_{it})$, which creates a very large markup. An option with almost no market share ($s_{it} \to 0$) has a markup of 1, so their price is $p^*_{it} \approx c_i + 1$.

commonly assumed in models of Bayesian updating.

Assumption 3 (MLRP): $\frac{\sigma_h(y)}{\sigma_l(y)}$ *is strictly increasing in* y.

Intuitively, a past experience by consumer herself or others is the more likely to reflect a high likelihood the better it is. To simplify notation, we now write μ_0 for the consumer's prior belief about the expected consumer experience on the platform before making a purchasing decision. The reviews of the options r and the number of presumably high-quality options on the platform to date s are considered informative.[19] Denoting the new signal d as an increasing function of r and s, we have $d(s,r)$, $d'(s) > 0$, $d'(r) > 0$. Considering $\tilde{d}$ as a threshold and $d > \tilde{d}$ for a good signal that consumers will regard this brand with good reputation, the probabilities $\rho_h \equiv Pr(d > \tilde{d}|p = p_h)$ and $\rho_l \equiv Pr(d > \tilde{d}|p = p_l)$ satisfy

Assumption 4 (Signal): *(i)* $\rho_h > \rho_l$ *and (ii)* $\rho_h > 1/2$.

The consumer uses the signal $\rho_i, i \in \{h, l\}$ to form a posterior belief μ about the expected consumer experience on the platform. By Bayes' rule, the posterior belief μ_t is formed as

$$\mu_t \equiv Pr(p = p_h|d > \tilde{d}) = \frac{\rho_h \mu_{t-1}}{\rho_h \mu_{t-1} + \rho_l \cdot (1 - \mu_{t-1})} = \frac{L \cdot \mu_{t-1}}{L \cdot \mu_{t-1} + (1 - \mu_{t-1})} \tag{6}$$

where $L = \frac{\rho_h}{\rho_l}$.

A.3 Effect of restricting large-scale suppliers

Given the empirical results in the baseline analysis, the shock of restricting large-scale suppliers will lead to a direct negative impact on the number of high-quality options on the platform and indirect consequences of reduced competition from the high-quality supply such as exerting less effort and having lower ratings. This is an open empirical question, for which we provide empirical evidence in the main text.

Initially, when the signals from s and r are not sufficiently negative to push d below a critical threshold, L does not yet decline and μ_t remains not being updated. The positive scarcity effect dominates. The denominator in the Equation (1) shrinks as N_2 decreases to N_2', $N_2' \subset N_2$. This

[19] Carrera et al. (2025) describe the consumption of experiential goods as a two-stage (choice-outcome) process: First, the consumer selects a good from the available choice set; second, they consume it and derive satisfaction from the experience.

mechanically increases the market share s_{it} and the price p_{it} for every remaining supply i as they are faced with less competition.

Over time, once s and r decrease below a certain threshold, the new signal d becomes negative enough to have $\rho_h < \rho_l$. In this case, L decreases, which in turn causes the posterior belief μ_t to decrease. According to Equation (1), the market share of consumers who choose the outside option becomes

$$s_{0t} = \frac{e^{\delta_0}}{e^{\gamma\mu_t} \cdot (\sum_{m \in N_1} e^{\theta r_{mt} - p_{mt}} + \sum_{n \in N_2'} e^{\theta r_{nt} - p_{nt}}) + e^{\delta_0}}$$

As $\mu_t < \mu_{t-1}$, the denominator becomes smaller and s_{0t} increases. The outside option becomes relatively more appealing. Consequently, this causes the market share s_{it} for every option on the platform to decrease as consumers leave for the outside option.

Each option i now faces this new, relatively lower demand in posterior. They re-solve their profit-maximization problem again:

$$p_{it}^* = c_{it} + \frac{1}{1 - s_{it}^*}$$

Since s_{i1}^* has decreased relative to the short run, s_{it}^*, the term $(1 - s_{it}^*)$ increases. This causes the markup, $\frac{1}{1-s_{it}^*}$, to decrease. After their reputations are compromised, these options can no longer sustain their previous high prices and need to compete by reducing their prices.

To summarize, as consumers learn about the new information of the platform and update their belief about the expected consumer experience on the platform based on the new negative signals, their demand for all remaining options is undermined. This forces providers to slash their prices from the immediate rise.

B Effect on new entrants in platform

Another possible explanation from an equilibrium perspective is that entry on the supply side dissipates the positive scarcity effect and re-establishes competition at its pre-equilibrium level. We aggregate the number of new listings and new hosts at zip-code level to test this alternative explanation. Consistent with Figure 1, in Table A1 we observe that new supply-side entrants (both measured by listings and by hosts) do not offset the reduction and competition has yet to return to its pre-treatment level, especially in the long run. These patterns provide some suggestive evidence of a slowdown in platform growth and a possible deterioration in the platform's brand and reputation over time.

Table A1 Relationship between restricting large-scale suppliers and Airbnb entry

	(1)	(2)	(3)	(4)
	$\log(\#new\ hosts + 1)$		$\log(\#new\ listings + 1)$	
	$<= t + 36$	$<= t + 12$	$<= t + 36$	$<= t + 12$
treated × after	−0.0587**	−0.0260	−0.0751***	−0.0460
	(0.0284)	(0.0442)	(0.0266)	(0.0414)
Zip-code FE	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y
N of obs	24255	12375	24255	12375
N of zip-codes	495	495	495	495

Note: Robust standard errors in parentheses, clustered by zip-code. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels. Controlling for zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code vacancy rate, zipcode percentage black population.

C Alternative Sample

C.1 Excluding listings in Houston and Washington DC

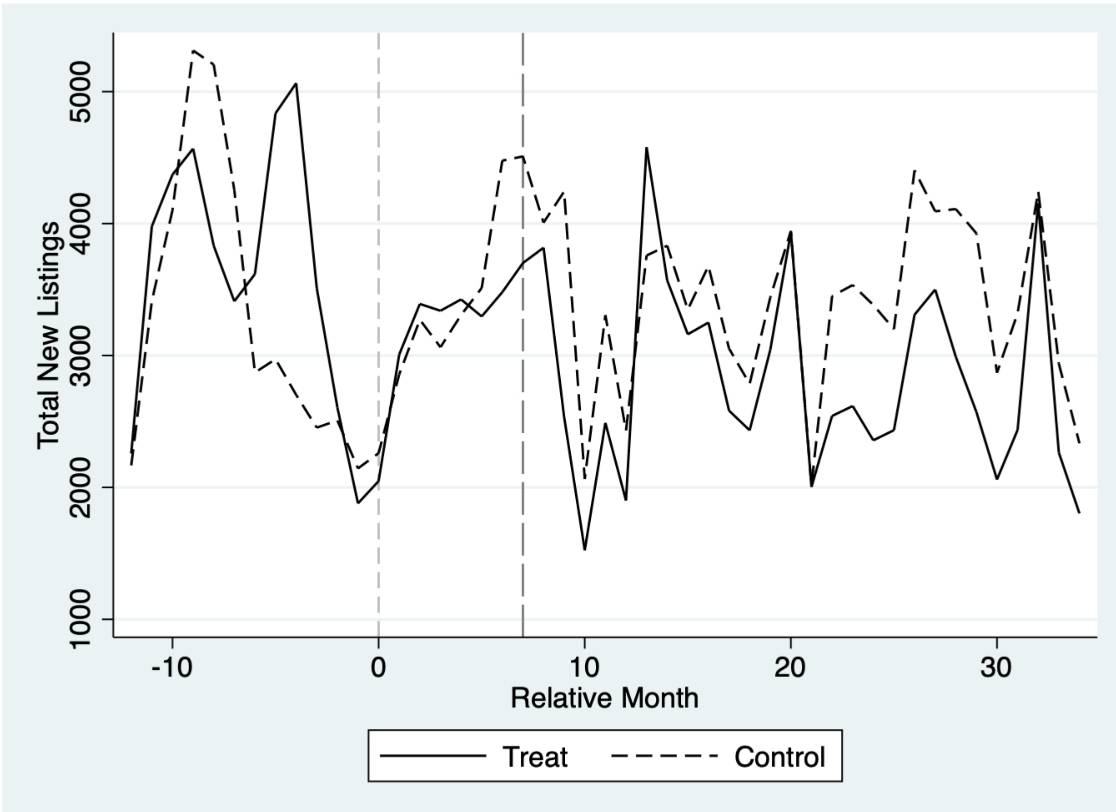

Figure A1 Total new listings (excluding listings in Houston and Washington DC)

Table A2 Relationship between restricting large-scale suppliers and market outcomes (Excluding listings in Houston and Washington DC)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		*occupancy rate*		$\log(RevPAR+1)$	
	$<=t+36$	$<=t+12$	$<=t+36$	$<=t+12$	$<=t+36$	$<=t+12$
treated × after	0.0211***	0.0601***	0.0697***	0.1504***	0.3188***	0.6938***
	(0.0033)	(0.0037)	(0.0033)	(0.0047)	(0.0178)	(0.0251)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	668,722	342,294	668,722	342,294	668,722	342,294
N of listings	22,794	22,794	22,794	22,794	22,794	22,794

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

C.2 Entire-home listing

Table A3 Relationship between restricting large-scale suppliers and market outcomes (Entire-home listings)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		*occupancy rate*		$\log(RevPAR+1)$	
	$<=t+36$	$<=t+12$	$<=t+36$	$<=t+12$	$<=t+36$	$<=t+12$
treated × after	0.0150***	0.0523***	0.0674***	0.1546***	0.3199***	0.7684***
	(0.0039)	(0.0050)	(0.0042)	(0.0058)	(0.0243)	(0.0328)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	443983	225967	443983	225967	443983	225967
N of listings	15160	15160	15160	15160	15160	15160

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

D Alternative Measures

D.1 Alternative demand measures

Table A4 Relationship between restricting large-scale suppliers and alternative demand measures

	(1)	(2)	(3)	(4)
	$\log(\#reservation+1)$		$\log(\#booked\ days+1)$	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
treated × after	0.0767***	0.1839***	0.2027***	0.4367***
	(0.0071)	(0.0093)	(0.0119)	(0.0162)
Listing FE	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y
N of obs	744460	380584	744460	380584
N of listings	25401	25401	25401	25401

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

D.2 Alternative treatment timing

Table A5 Relationship between restricting large-scale suppliers and market outcomes (policy implementation as treatment timing)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		$occupancy\ rate$		$\log(RevPAR+1)$	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
treated × after	0.0035	0.0162**	0.0305***	0.0316***	0.1383***	0.1853***
	(0.0043)	(0.0051)	(0.0042)	(0.0060)	(0.0236)	(0.0336)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	625943	365988	625943	365988	625943	365988
N of listings	25401	25401	25401	25401	25401	25401

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
N of obs is different from that in the baseline analysis because of excluding the time period between the announcement and implementation date.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

D.3 Alternative proxy for supplier experience

Table A6 Relationship between restricting large-scale suppliers and market outcomes for listings owned by individual Black hosts

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		*occupancy rate*		$\log(RevPAR+1)$	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
Individual × Black × treated × after	0.0651**	0.0503*	−0.0225	0.0130	−0.0718	0.0655
	(0.0236)	(0.0197)	(0.0221)	(0.0206)	(0.1149)	(0.1036)
Black × treated × after	−0.0312	−0.0382*	0.0027	−0.0194	−0.0193	−0.0938
	(0.0183)	(0.0168)	(0.0197)	(0.0186)	(0.1044)	(0.0947)
Individual × treated × after	−0.0227*	−0.0274**	0.0065	−0.0017	0.0045	−0.0284
	(0.0090)	(0.0084)	(0.0081)	(0.0072)	(0.0526)	(0.0478)
treated × after	0.0126	0.0423***	0.0468***	0.1066***	0.2317***	0.5153***
	(0.0094)	(0.0092)	(0.0088)	(0.0107)	(0.0563)	(0.0606)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	262169	136693	262169	136693	262169	136693
N of listings	9220	9220	9220	9220	9220	9220

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
This sample is a subsample of the sample used in the main analysis, conditioning on host race information being observed.
We find no systematic difference between listings with and without host race information.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

Table A7 Relationship between restricting large-scale suppliers and market outcomes for listings owned by less-active Black hosts

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price + 1)$		*occupancy rate*		$\log(RevPAR + 1)$	
	$<= t + 36$	$<= t + 12$	$<= t + 36$	$<= t + 12$	$<= t + 36$	$<= t + 12$
LessActive × Black × treated × after	0.1095***	0.0661**	−0.0102	−0.0024	0.0388	0.0811
	(0.0294)	(0.0211)	(0.0185)	(0.0168)	(0.1017)	(0.0911)
Black × treated × after	−0.0219*	−0.0228*	−0.0071	−0.0074	−0.0705	−0.0638
	(0.0096)	(0.0090)	(0.0115)	(0.0107)	(0.0535)	(0.0500)
LessActive × treated × after	−0.0232**	−0.0253***	−0.0481***	−0.0359***	−0.2294***	−0.1866***
	(0.0075)	(0.0067)	(0.0068)	(0.0063)	(0.0414)	(0.0389)
treated × after	0.0020	0.0289***	0.0656***	0.1156***	0.3014***	0.5479***
	(0.0060)	(0.0060)	(0.0064)	(0.0091)	(0.0360)	(0.0465)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	262169	136693	262169	136693	262169	136693
N of listings	9220	9220	9220	9220	9220	9220

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
This sample is a subsample of the sample used in the main analysis, conditioning on host race information being observed.
We find no systematic difference between listings with and without host race information.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

E Alternative Model Specifications

E.1 Alternative controls

Table A8 Relationship between restricting large-scale suppliers and market outcomes (Controlling for fixed effects and time-trends)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		*occupancy rate*		$\log(RevPAR+1)$	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
treated × after	0.0070***	0.0574***	0.0561***	0.1395***	0.2454***	0.6531***
	(0.0017)	(0.0022)	(0.0024)	(0.0035)	(0.0131)	(0.0188)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	744460	380584	744460	380584	744460	380584
N of listings	25401	25401	25401	25401	25401	25401

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.

E.2 Without matching

Table A9 Relationship between restricting large-scale suppliers and market outcomes (without matching)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		*occupancy rate*		$\log(RevPAR+1)$	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
treated × after	0.0102***	0.0555***	0.0573***	0.1452***	0.2605***	0.6733***
	(0.0028)	(0.0034)	(0.0028)	(0.0040)	(0.0151)	(0.0216)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	744460	380584	744460	380584	744460	380584
N of listings	25401	25401	25401	25401	25401	25401

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

E.3 Location-level clustering

Table A10 Relationship between restricting large-scale suppliers and market outcomes (standard error clustered at location level)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		*occupancy rate*		$\log(RevPAR+1)$	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
treated × after	0.0122	0.0564***	0.0607***	0.1415***	0.2730***	0.6648***
	(0.0075)	(0.0134)	(0.0071)	(0.0136)	(0.0395)	(0.0735)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	744460	380584	744460	380584	744460	380584
N of listings	25401	25401	25401	25401	25401	25401

Notes: Robust standard errors in parentheses, clustered by zip-code.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.

E.4 Event study model

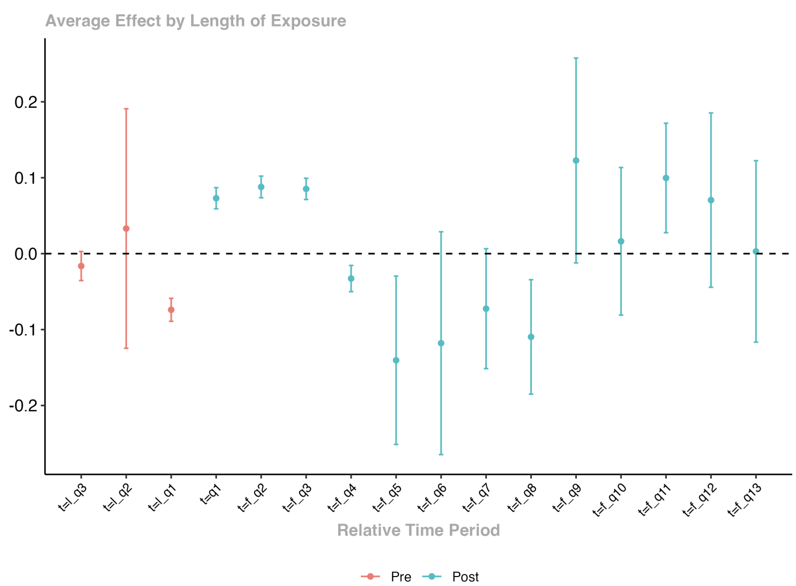

Figure A2 Event study for price

Note: The figure is plot by quarter.

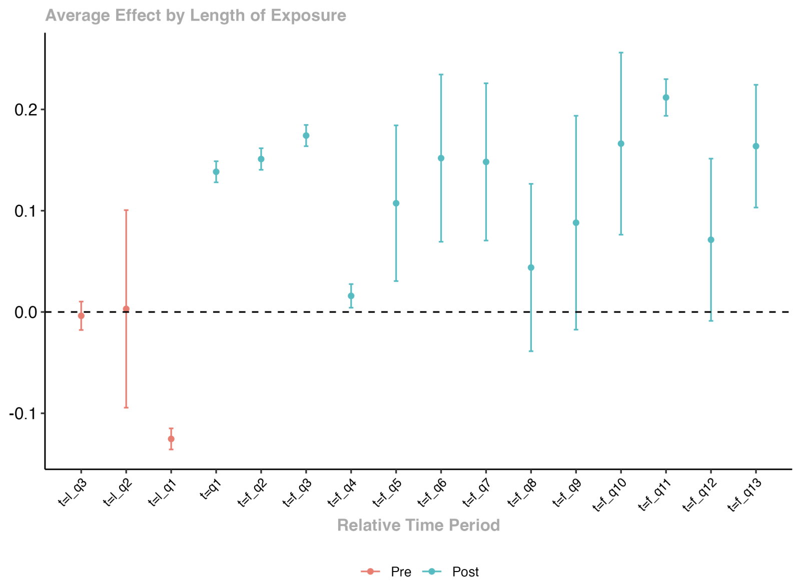

Figure A3 Event study for demand

Note: The figure is plot by quarter.

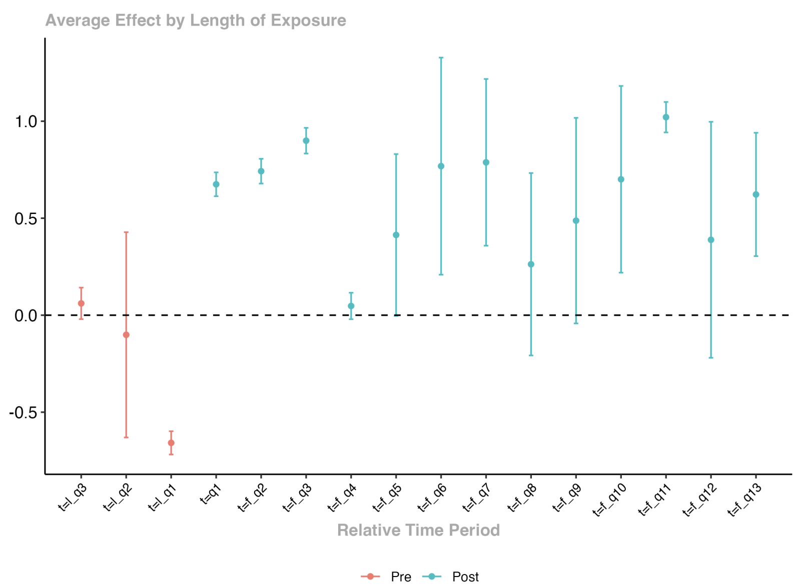

Figure A4 Event study for revenue

Note: The figure is plot by quarter.

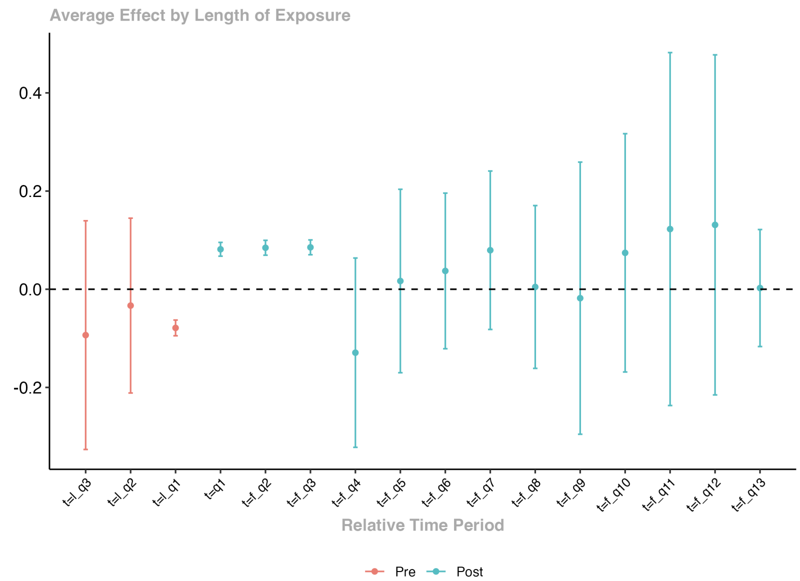

Figure A5 Event study for price (excluding Houston and DC)

Note: The figure is plot by quarter.

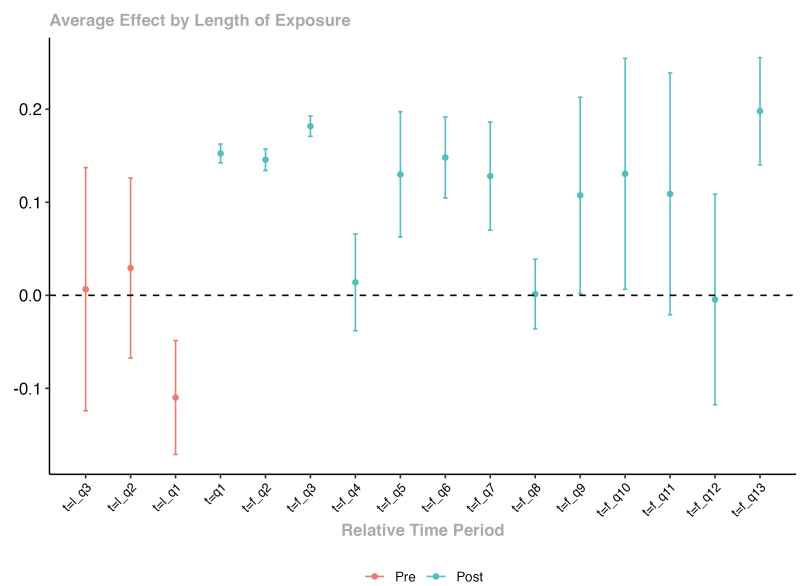

Figure A6 Event study for demand (excluding Houston and DC)

Note: The figure is plot by quarter.

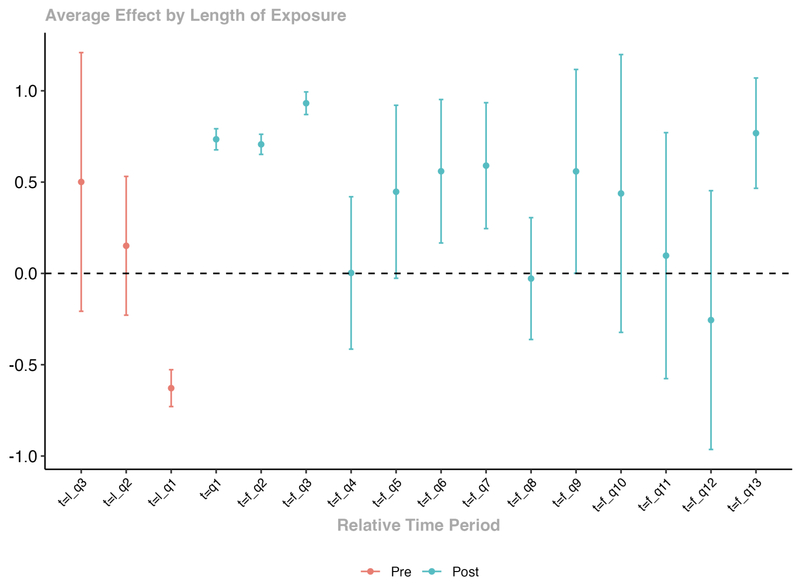

Figure A7 Event study for revenue (excluding Houston and DC)

Note: The figure is plot by quarter.

E.5 Doubly robust machine learning estimators

Table A11 Relationship between restricting large-scale suppliers and market outcomes (DRML)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price + 1)$		*occupancy rate*		$\log(RevPAR + 1)$	
	$<= t + 36$	$<= t + 12$	$<= t + 36$	$<= t + 12$	$<= t + 36$	$<= t + 12$
Coefficient	0.0109	0.0297	0.0439	0.0579	0.1802	0.3036
Std. Error	(0.0106)	(0.0103)	(0.0041)	(0.0037)	(0.0182)	(0.0179)
Distributional Statistics						
10%	-0.0390	-0.0470	-0.0330	-0.0367	-0.1730	-0.1597
25%	0.0124	-0.0159	0.0007	0.0023	-0.0074	0.0268
50%	0.0158	0.0201	0.0389	0.0450	0.1666	0.2136
75%	0.0500	0.0646	0.0812	0.0974	0.3670	0.4445
90%	0.0894	0.1132	0.1247	0.1484	0.6085	0.7244
N of obs	383,395	189,007	383,395	189,007	383,395	189,007
N of listing	11,025	11,025	11,025	11,025	11,025	11,025

Notes: Standard errors in parentheses, clustered by listing.
Threshold for the expectation of treatment: 0.05 - 0.95. Keep the whole unit if at least half of the observations with treatment probability within the threshold.

E.6 Synthetic DiD

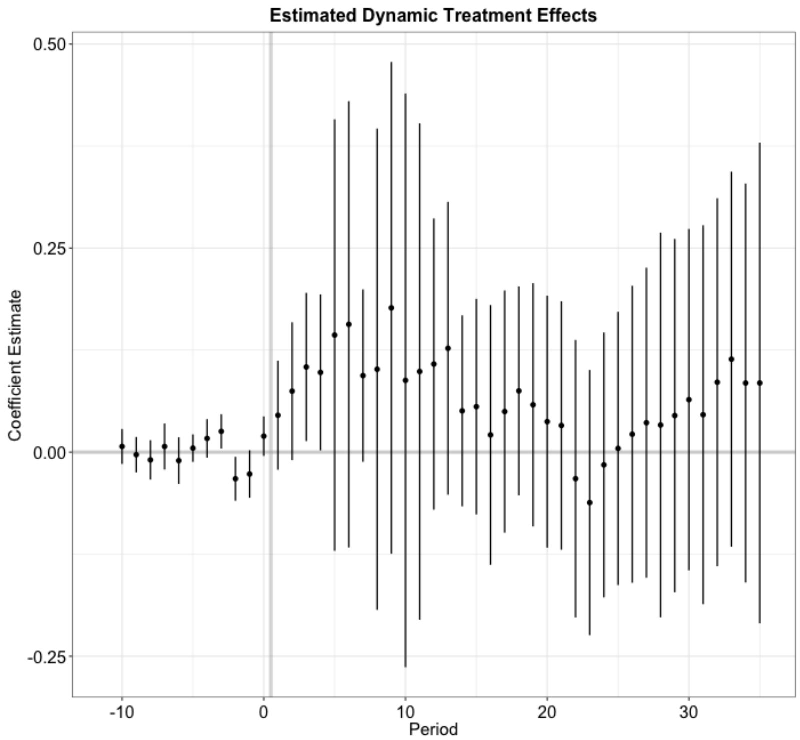

Figure A8 Synthetic DiD estimation for price

Note: The figure is plot at zip-code level. The dependent variable is the logarithm transformation of average prices at the zip-code by month level.

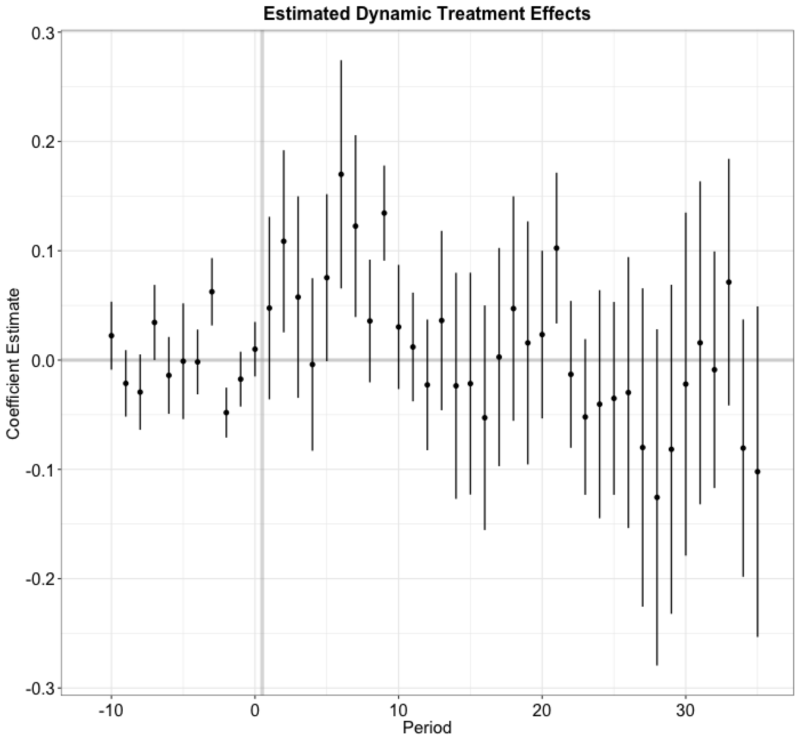

Figure A9 Event study for demand

Note: The figure is plot at zip-code level. The dependent variable is the average occupancy rate at the zip-code by month level.

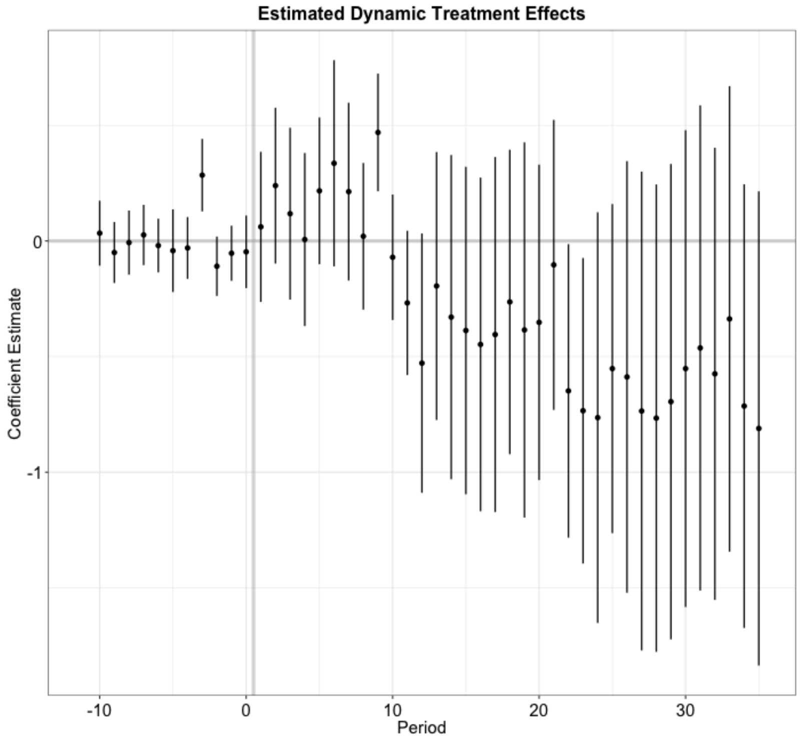

Figure A10 Event study for revenue

Note: The figure is plot at zip-code level. The dependent variable is the logarithm transformation of average revenue at the zip-code by month level.

F Other potential bias

F.1 Relisting behavior

Table A12 Distribution of new host entrants around listings

	Coordinate Level	Host Type	Mean	Std	Min	Median	Max
entire post-treatment	6-decimal coordinate	large-scale hosts	1.0465	0.8906	1	1	40
		all	1.0088	0.2755	1	1	40
	5-decimal coordinate	large-scale hosts	1.0468	0.8875	1	1	40
		all	1.0094	0.2766	1	1	40
	4-decimal coordinate	large-scale hosts	1.2395	0.8595	1	1	41
		all	1.0974	0.4315	1	1	41
first 3-month post-treatment	6-decimal coordinate	professional hosts	1	0	1	1	1
		all	1.0003	0.0162	1	1	2
	5-decimal coordinate	large-scale hosts	1	0	1	1	1
		all	1.0003	0.0162	1	1	2
	4-decimal coordinate	large-scale hosts	1.0218	0.1463	1	1	2
		all	1.0085	0.0921	1	1	2

F.2 Self-selection in posting facial information

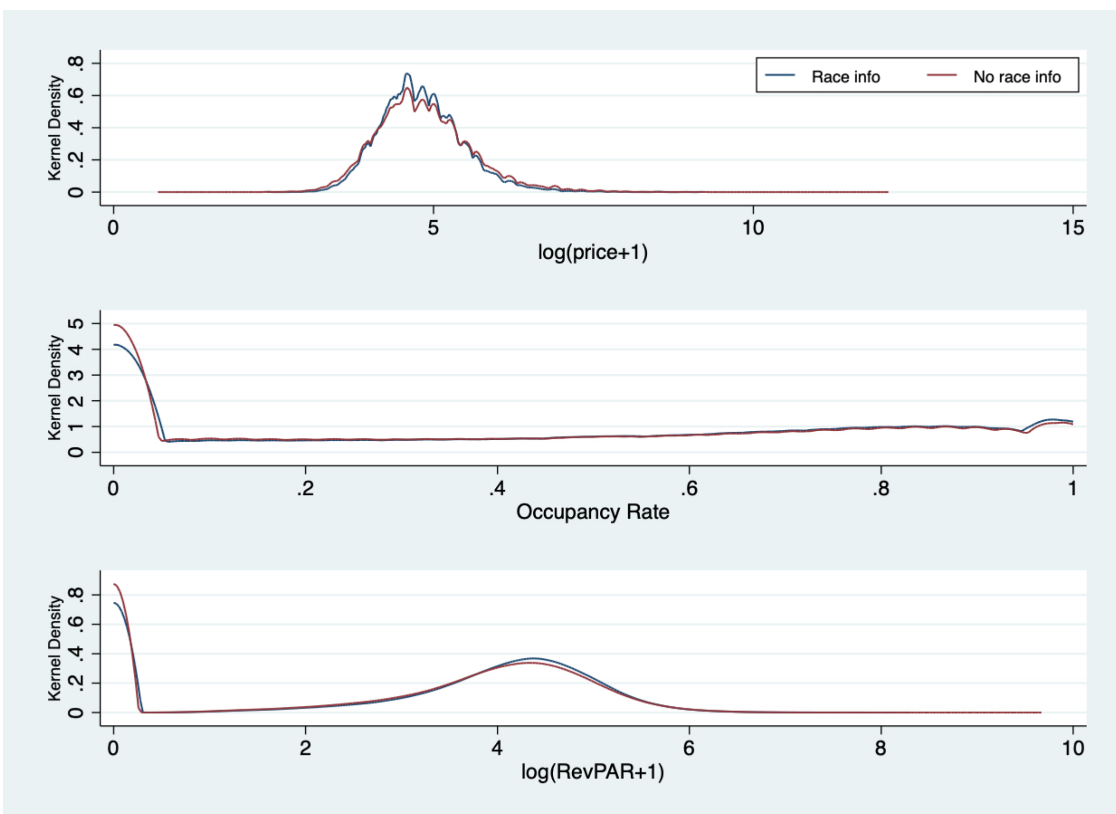

Figure A11 Distribution of outcomes between listings with and without race information

Table A13 Relationship between restricting large-scale suppliers and market outcomes (Subsample with race information)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(price+1)$		*occupancy rate*		$\log(RevPAR+1)$	
	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$	$<= t+36$	$<= t+12$
treated × after	−0.0021	0.0212***	0.0495***	0.1032***	0.2233***	0.4844***
	(0.0030)	(0.0056)	(0.0059)	(0.0088)	(0.0322)	(0.0439)
Listing FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
City-specific time trend	Y	Y	Y	Y	Y	Y
N of obs	262169	136693	262169	136693	262169	136693
N of listings	9220	9220	9220	9220	9220	9220

Notes: Robust standard errors in parentheses, clustered by listing.
***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.
Controlling for number of listings under the host, number of reviews, Superhost status, allowing pets, smoking, zip code property value (ZHVI), number of listings within the zipcode, zip code population, zip code median income, zip code households.