Cleanin' It Up: Unshrouding Hidden Fees on a Peer-to-Peer Platform *

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Abstract

We examine how greater price transparency affects pricing behavior in peer-to-peer markets. When Airbnb began displaying cleaning fee-inclusive prices to European users in response to EU regulation, hosts who had not charged fees raised their base prices by 67%, especially when competitors used cleaning fees. These adjustments arise because transparency changes how sellers perceive competitors' prices: when fees are hidden, inattentive hosts benchmark only visible base prices; once fees are unshrouded, they realize competitors were effectively charging more. In contrast, hosts already charging cleaning fees reduce them by about 1.5%, particularly when serving more EU travelers. Transparency thus reduces price obfuscation for consumers but can increase prices for previously transparent sellers, revealing that regulatory efforts to enhance transparency may have unintended redistributive effects in decentralized markets.

Keywords: Hidden Fees, Obfuscation, Peer-to-Peer Platforms, Platform Regulation, Airbnb.

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1 Introduction

Shrouded prices and hidden fees are widespread in online markets. On ticket resale platforms such as StubHub or Ticketmaster, consumers are often enticed by initially attractive ticket prices, only to find mandatory fees for processing and service charges added at the final checkout stage. Similarly, food delivery services like Grubhub and DoorDash frequently advertise low delivery costs, but then add fees for service, delivery, and small orders. In the hospitality industry, platforms like Airbnb and Vrbo have historically displayed competitive base prices, with additional charges, such as cleaning and service fees, only being disclosed after a listing has been selected. In response, the U.S. federal government has launched a broad campaign against so-called "junk fees", led by the Federal Trade Commission and the Consumer Financial Protection Bureau, aiming to disclose the total price upfront, arguing that hidden fees distort competition, exploit behavioral biases, and impose substantial welfare losses on consumers.[1]

Most empirical research on hidden fees focuses on demand-side reactions and shows that consumers react less to hidden add-on charges compared to a more salient base price (e.g. Gabaix and Laibson, 2006; Ellison and Ellison, 2009; Chetty et al., 2009; Blake et al., 2021; Bakker and Datta, 2025). These results suggest that, by hiding fees, sellers can profitably *obfuscate* the total price, attracting attention with a low base price while shifting part of the total price to the less salient fees. Unshrouding hidden fees in these cases should weakly decrease fees (and hence total prices), making consumers better off.

However, the implications of fee transparency on *peer-to-peer platforms* with largely decentralized pricing is less straightforward.[2] Because many sellers in these markets are not professionals, inattention and search frictions extend to the supply side. If fees are hidden, inattentive sellers may benchmark themselves against competitors' base prices rather than their total prices, setting their own prices lower than they would if total prices were transparent. Then, unshrouding hidden fees makes previously inattentive sellers realize competitors' total prices are higher, giving them an incentive to raise their own base prices.

We study the effects of fee transparency in the context of Airbnb, a peer-to-peer platform where sellers set a *base price per night* and an *optional per-stay cleaning fee*. Following negotiations with the European Commission, Airbnb began showing transparent, fee-inclusive prices in search results to users based in the European Union (EU) in January 2019, while non-EU users continued to see fees only later in the purchasing

[1] See Federal Trade Commission (2025), *Rule on Unfair or Deceptive Fees*, Final Rule, Federal Register, Vol. 90, No. 7, January 10, 2025, available at https://www.federalregister.gov/documents/2025/01/10/2024-30293/trade-regulation-rule-on-unfair-or-deceptive-fees. See also The White House (2023), *BidenHarris Administration Announces Broad New Actions to Protect Consumers from Billions in Junk Fees*, October 11, 2023, available at https://www.whitehouse.gov/briefing-room/statements-releases/2023/10/11/biden-harris-administration-announces-broad-new-actions-to-protect-consumers-from-billions-in-junk-fees/.

[2] Recent work on hospital pricing reveals that greater transparency can have counterintuitive results (Pan and Yaraghi, 2025).

process. This EU-specific platform redesign creates plausibly exogenous variation in fee transparency by user location, both on the demand as well as the supply side.

We use listing-level data from London, the city with the highest number of Airbnb listings in Europe and subject to EU laws and regulations for the period of our analysis. We focus on data from 2018 and 2019, a two-year period around the introduction of fee transparency for European Airbnb users, to consider how greater price transparency impacts the underlying home-share market. During this time period in London, we document that 27% of listings never charge a cleaning fee even when fees are shrouded. This pattern is difficult to reconcile with optimal price-setting with inattentive consumers and is consistent with seller inattention and a default-option bias among a large share of hosts. Descriptive evidence shows that these zero-fee hosts display lower pricing sophistication: they are more likely to use fixed prices irrespective of the day of the week, are less likely to hold "Superhost" status (i.e., an Airbnb designation awarded to hosts with consistently high ratings, responsiveness, and booking volumes), and manage listings with fewer bookings and booked nights. Further evidence suggests that the zero-fee choice is not a reputation-building strategy: cleanliness ratings are not higher for zero-fee listings.

Their presence in the market has clear implications for the effectiveness of transparency policies in a peer-to-peer market. If some sellers are inattentive, a transparency shock can affect their pricing strategy by revealing competitors' total prices. To test this, we first examine how zero-fee listings react to transparency relative to nearby comparable listings with a positive fee. We find that zero-fee hosts react to fee transparency by raising their base price by 4-5 GBP relative to positive-fee hosts.

To more accurately account for the role of competitors' prices and local competition, we construct a listing-level measure of how much a listing's perceived relative affordability changes due to fee transparency. Specifically, we benchmark each listing against comparable nearby listings twice: first using prices net of fees, and then using fee-inclusive prices. Listings whose perceived affordability improves the most once fees are included are classified as "more affordable under transparency." Compared to the simple zero-fee versus positive-fee comparison, this change in perceived affordability reflects more closely how pricing incentives change due to fee transparency. For example, a zero-fee listing's perceived affordability improves much more compared to its competitors' if its competitors all have high cleaning fees which are being unshrouded. Instead, a zero-fee listing that is competing with mostly other zero-fee listings will not experience a large change in perceived affordability.

We define the top quartile of the distribution of perceived affordability changes as our treatment group, as these listings are those whose relative affordability tends to improve the most after the implementation of price transparency. All listings in the second and third quartiles serve as the control group.[3] Using this treatment definition, we implement a difference-in-differences (DiD) design around the introduction of fee transparency for EU users in January 2019. We compare the evolution of base prices for the "more affordable" listings to otherwise similar listings. We find that after fee transparency was introduced, the treatment group increases their base prices by 8.4-8.8 GBP. This increase amounts to about 6.5% of the average base price in the treatment group. This price adjustment is concentrated among non-Superhosts, who are likely less professional. These results are in line with hosts in the treatment group being inattentive to the fee when it is hidden, discovering that competitors charge higher prices than they thought once the fee is unshrouded, and adjusting their own base price upwards. In additional analyses, we show that our results are not driven by a shift in demand due to fee transparency. Furthermore, we find that the observed price changes cannot be explained by the use of algorithmic pricing. Finally, we find no analogous adjustment for otherwise similar "more affordable" listings in New York City, where cleaning fees remained shrouded over the same period.

We also examine how fee transparency affects cleaning fees for the 71% of listings that use them. Because the policy applied only to EU-based users, listings with a higher share of EU visitors are more affected and should be more likely to react. To identify this effect, we implement a DiD design that exploits heterogeneous exposure to EU guests at the listing level. We use the locations reported in guest reviews to infer their origins and construct a measure of each listings geographical exposure to EU guests. Specifically, we define listings with pre-transparency exposure to EU travelers above the median as the treatment group.

If guests were at least partly inattentive to hidden fees and hosts used them deceptively, exposed hosts should reduce their cleaning fees once transparency increases the salience of total prices. Consistent with this prediction, cleaning fees decline by about 0.7 GBP for high-exposure listings relative to low-exposure ones. Among Superhosts, the reduction reaches 1-1.1 GBP, suggesting that more professional hosts react more to the transparency change. However, the magnitude of these adjustments is relatively modest, indicating that hosts may set cleaning fees for a variety of reasons beyond strategic obfuscation or an intent to deceive users. Robustness checks, including London-New York comparisons, confirm our main findings.

Related Literature Our article contributes to the literature on price obfuscation and seller pricing behavior. Prior work has focused mainly on demand-side reactions to hidden fees: consumers under-react to hidden fees in lab experiments (Morwitz et al., 1998), field experiments (Hossain and Morgan, 2006;

[3] Listings in the second and third quartiles are those whose perceived affordability changes the least due to transparency and thus represent a suitable control group. In Appendix D, we also compare the treatment group with each quartile separately as well as other versions of control groups.

Brown et al., 2010; Blake et al., 2021; Bakker and Datta, 2025), and observational data (Einav et al., 2015). However, Dertwinkel-Kalt et al. (2020) show that inattention is not universal and may depend on the cost of abandoning search once the full price is revealed. Taken together, these studies suggest that sellers benefit from shrouding part of the price. We show that on peer-to-peer platforms, inattention to fees can extend to the supply side. This insight has important implications for understanding the potential impacts of mandatory fee transparency.[4]

Relatedly, Ellison and Ellison (2009) show how retailers post low prices on comparison sites and then steer consumers to higher-margin products. Our findings also connect to studies of mandated price transparency in gasoline markets (Luco, 2019; Martin, 2024). While Luco (2019) identifies a collusive mechanism that entails higher prices in some areas, in our setting higher prices result from inattentive hosts' reaction to fee transparency. This mechanism differs from some work that predicts price decreases with transparency (Ellison and Ellison, 2018). Indeed, our results highlight countervailing forces in peer-to-peer markets characterized by many non-professional sellers: making fees salient shifts the reference prices that inattentive sellers use for benchmarking, prompting them to increase their prices accordingly.

A complementary strand of research assesses consumers' willingness to pay for transparency and how obfuscation interacts with search. Consumers may still gain from lower search costs and the perceived fairness of upfront prices (Seim et al., 2017; Mamadehussene, 2020; Allender et al., 2021), but any increase in total prices can partially offset those gains. For example, Seim et al. (2017) estimate that consumers value upfront, fee-inclusive prices, which improves welfare by reducing search and misperception. In adjacent work on reputation, Chiles (2021) shows that shrouded pricing can backfire through reputation channels in hotels, reinforcing the policy case for transparency when platforms intermediate repeat interactions. Our findings are consistent with this logic but highlight a peer-to-peer dimension: transparency can raise base prices for inattentive sellers even as it trims add-on fees by more rationale sellers.

Finally, our work also touches on studies that analyze Airbnbs smart pricing algorithm (Ye et al., 2018; Pan and Wang, 2021; Huang, 2025; Foroughifar, 2023). We observe a subset of listings that change prices very frequently, consistent with algorithmic pricing, which could dampen short-lived policy responses. More directly on fee obfuscation, Johnen and Somogyi (2024) analyze platform vs. seller incentives to shroud prices. In our setting, limited transparency in cleaning fees appears to have encouraged only a share of hosts to use fees deceptively, but that was sufficient to result in meaningful adjustments in prices and fees once fee transparency is introduced.

[4] We also relate to work on decentralized vs. centralized pricing on platforms (Allon et al., 2012; Aouad et al., 2023). Pricing is decentralized on Airbnb and Amazon, but centralized on Uber (Castillo et al., 2025). While platforms possess superior data, their objectives may not align perfectly with individual sellers' profit maximization (Filippas et al., 2023).

The remainder of the paper proceeds as follows. Section 2 reviews the EU fee transparency intervention and our data construction. Section 3 presents descriptive evidence on how hosts on Airbnb set base prices and cleaning fees. Section 4 examines how fee transparency affects base prices. Section 5 analyzes how fee transparency affects cleaning fees. Section 6 concludes by discussing managerial and policy implications.

2 Background

2.1 Airbnb and EU Fee Transparency

Airbnb is a peer-to-peer platform connecting hosts with guests seeking short-term stays. Hosts on Airbnb have two price components that they can set: a base price per night and a cleaning fee that applies to each stay, irrespective of its length.[5] There are different degrees of pricing flexibility that hosts can use. The default on the platform is that hosts set a fixed price per night that is the same for every future date and a cleaning fee of zero. Greater flexibility is possible but requires additional effort. Hosts may specify a positive cleaning fee by adjusting the listing settings after creation. They can also differentiate prices between weekdays and weekends or manually set rates for each individual date. Finally, hosts may activate Airbnb's automatic pricing tool, which adjusts nightly rates within a price range defined by the host.

Guests typically browse available listings through a combination of search results and an interactive map, which displays approximate location and nightly rates. The map allows guests to visually compare prices across neighborhoods and assess proximity to points of interest. Reservations are made by selecting the available dates and paying the total price, which includes the nightly rate, service fees, and any other applicable charges such as the cleaning fee.[6]

Before 2018, initial search results on Airbnb displayed only the base price per night excluding any fees. Additional fees were detailed only later in the booking process. In July 2018, the European Commission, along with the Norwegian Consumer Authority, demanded that Airbnb revise its practices to comply with EU consumer protection laws. European authorities gave Airbnb until the end of August 2018 to propose a solution, warning that enforcement action would be taken if satisfactory changes were not implemented.[7]

By September 2018, Airbnb had agreed to comply with these requests. The company committed to displaying the total, all-inclusive price per night (including any fees) on all EU versions of its platform by

[5] For more information, see: `https://www.airbnb.com/help/article/2812` (last accessed: December 12, 2024).

[6] In our analysis, we focus on the cleaning fee, as it is the only fee determined directly by the host. Yet, fee transparency also extends to other mandatory charges, such as the Airbnb service fee.

[7] For the official EU declaration, see: `https://ec.europa.eu/commission/presscorner/detail/en/IP_18_4453` (last accessed: October 22, 2025).

(a) EU IP Address

(b) US IP Address

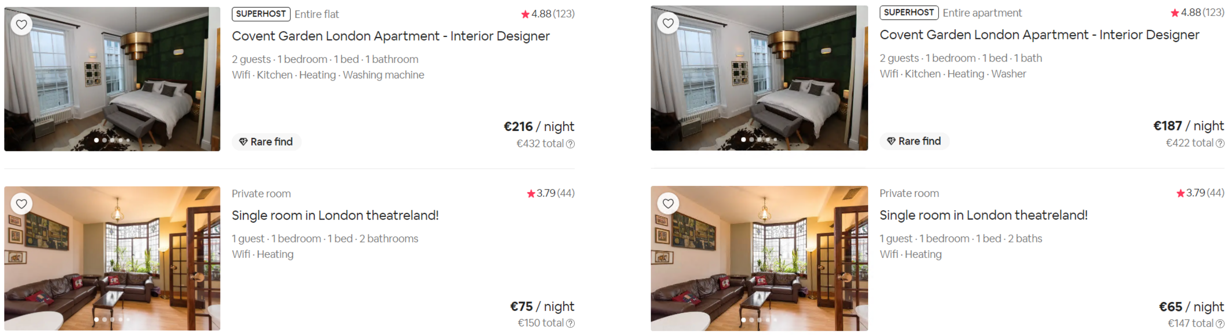

Figure 1. Airbnb search results in London as viewed by EU versus US IP addresses in February 2020.

the end of 2018.[8] As of January 2019, EU-based consumers searching for accommodations on Airbnb saw the full price of their booking upfront, including any applicable service fees, cleaning fees, and local taxes, ensuring a more transparent user experience. In July 2019, the European Commission confirmed that Airbnb was fully compliant with EU consumer protection standards and that the platform had been redesigned to ensure price transparency across all EU countries.[9]

Therefore, after fee transparency was implemented, consumers visiting the platform from the EU would see different prices in the search results than those visiting from elsewhere. To illustrate these differences, Figure 1 compares two Airbnb listings in London as viewed in the search results by users with EU-based IP addresses (left panel) and US-based IP addresses in 2020 (right panel). For the screenshot, we searched for rooms in central London for two nights. For the first listing, a European traveler would see a price of 216 euro per night, adding up to a total of 432 euro for two nights. For the same listing, a US traveler would see a price of 187 euro per night. However, the total for two nights would be 422 euro, due to additional fees that would be added on top of the base price.[10]

2.2 AirDNA and InsideAirbnb Datasets

For our analysis, we use information on Airbnb listing prices, cleaning fees, and exposure to EU travelers. We combine three sources. First, we use AirDNAs web-scraped panel of daily supply and demand, including

[8]For the official EU declaration, see: `https://ec.europa.eu/commission/presscorner/detail/en/ip_18_5809` (last accessed: October 22, 2025).

[9]For the official declaration, see: `https://ec.europa.eu/commission/presscorner/detail/en/IP_19_3990` (last accessed: October 22, 2025).

[10]For easier comparison, we switched the currency to Euro for the screenshots. The discrepancy between total prices shown to EU and US consumers is due to the foreign exchange rate used when displaying the prices.

asked prices (i.e., the price set by the host), availability, and bookings.[11] These data include daily information on each listing's price, availability for booking, and booking status. For listings that were booked, the dataset also records the time of booking and the price at which the booking occurred, which we refer to as the *booked price*. The second dataset, also from AirDNA, contains Airbnb reviews, that is, the full text plus limited guest metadata. Crucially for our analysis, reviews include the guests' self-reported home locations, which we use to proxy exposure to EU travelers. Because AirDNA does not provide time-varying cleaning fees, we supplement these sources with monthly snapshots from InsideAirbnb,[12] which report cleaning fees for most listings.[13]

We focus on London, the European city with the largest number of Airbnb listings and subject to EU regulation during our study period. We restrict the sample to listings observed at least twice between January 2018 and December 2019.[14] We restrict the analysis to private rooms and entire homes that represent the vast majority of our data. Furthermore, we only include listings in months in which they were available for at least one day.[15]

Since we analyze host behavior, we primarily focus on the *asked* price and cleaning fee, which refer to the price and fee set by the host for a given date. These prices may differ from the *booking* price and cleaning fee, which correspond to those of actual transactions.[16]

3 Fee- and Price-Setting Behavior

We begin our analysis with descriptive evidence about hosts' fee- and price-setting behavior on Airbnb. In a setting in which some consumers are inattentive to hidden fees, we would expect profit-maximizing sellers to shift part of their price to the hidden fee. However, Figure 2 presents a striking pattern: whereas 71% of listings indeed always set a positive cleaning fee, 27% of them never set a cleaning fee at all. Only 2%

[11] AirDNA is a data analytics provider that compiles and analyzes short-term rental data, primarily from Airbnb. For more information, see: https://www.airdna.co/.

[12] InsideAirbnb is an independent project that collects and shares data about Airbnb listings. For more information, see: https://insideairbnb.com/.

[13] To combine these datasets, we aggregate the daily AirDNA data to months. Then, we merge the monthly InsideAirbnb data to match the monthly cleaning fee information to the AirDNA data. For several listings, cleaning fees are always missing in the InsideAirbnb data. For these cases, we assume that the listings have not set a cleaning fee, i.e. the fee is zero. Appendix A provides further details on the data matching process and our treatment of missing cleaning fee information.

[14] The UK formally left the EU on January 2020 and continued to follow EU rules and regulations until December 2020. Airbnb maintained the same transparent price display for UK consumers even after 2020. In addition, London provides the most precise and comprehensive data among major European cities and did not experience other policy changes affecting Airbnb during our period of analysis.

[15] In additional analyses, we compare Airbnb listings in London to those in New York City.

[16] Results on booking prices and cleaning fees are provided in Appendices D.2 and E.2, respectively. There we see that our main results hold.

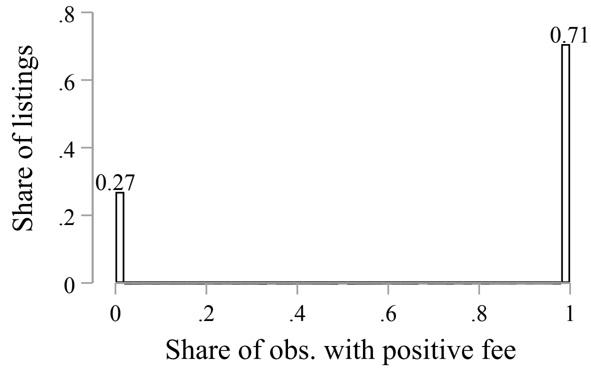

Figure 2. Share of observations with positive fee by listing. Each observation represents one listing.

of listings use a zero cleaning fee in some months and a positive cleaning fee in other months.[17] Note that each observation underlying this histogram is one listing and we calculate the share of months for which we observe a positive cleaning fee for each listing. The histogram shows the distribution of these shares over all listings in the sample. The bimodal distribution reveals that there are effectively two types of listings: those that always have a positive cleaning fee and those that never do.

Given this clear distinction in cleaning fee usage, it is important to understand if zero-fee listings and positive-fee listings are different in other observable characteristics as well. To assess and compare their pricing strategies, we create measures that capture different types of pricing strategies that hosts can use on Airbnb. Using daily price data, we identify three broad categories of pricing strategies. First, a host can set the same price every day, regardless of the day of the week. This is the default pricing when initially setting up a listing. We refer to this pricing strategy as "Day-of-Week (DoW) independent" pricing. Second, if a host sets the same price on all weekend days[18] and different prices during the week, we refer to this as "Weekend (WE) pricing." Finally, we define an aggregate "Other" category to include any strategy that does not fall into the above classifications. This classification allows us to assign a pricing strategy to each listing in every week. As listings can switch strategies from week to week (see Appendix B for details), we aggregate these classifications to the listing-month level by computing the share of weekly observations in which a listing adopts each pricing strategy.

Additionally, we create a variable that measures the frequency of price adjustments per listing-month: "Pricing changes." To capture price adjustments, we define a price change as the difference between a list-

[17] This evidence is consistent with an official Airbnb statement in 2021, which noted that "among active Airbnb listings worldwide, 45% do not charge a cleaning fee. For listings that do charge a cleaning fee, the fee averages less than 10% of the total cost of the reservation." For the full statement, see: https://news.airbnb.com/fee-transparency-on-airbnb/ (last accessed:October 22, 2025).

[18] We define the weekend as Friday to Saturday, Saturday to Sunday, or Friday to Sunday.

Table 1. Descriptive statistics for prices and fees

	Obs.	Mean	SD	p25	p50	p75
Asked base price	1,030,459	119.47	439.26	46.00	82.00	140.00
Asked cleaning fee	1,030,459	33.53	41.14	0.00	20.00	50.00
Asked cleaning fee (if > 0)	750,642	46.03	41.81	20.00	35.00	60.00
Pricing: DoW independent	1,030,459	0.57	0.41	0.05	0.70	1.00
Pricing: WE pricing	1,030,459	0.30	0.34	0.00	0.16	0.54
Pricing: Other	1,030,459	0.13	0.25	0.00	0.00	0.19
Pricing changes	1,030,459	6.77	8.64	0.00	2.00	12.00

ing's asked price on a given calendar day and its asked price on the same day-of-the-week in the preceding week. This construction filters out mechanical day-of-week patterns (e.g., higher weekend prices), so a listing that systematically charges more on Fridays is not incorrectly flagged as changing prices from Thursday to Friday every week.

Table 1 provides descriptive statistics for the entire sample. Asked listing prices in London are approximately 119 GBP per night, but there is large dispersion across listings. The mean cleaning fee is about 34 GBP (46 GBP if focusing on listings with a positive cleaning fee), again with considerable dispersion. The most prominent pricing strategy is day-of-the-week-independent pricing which we observe in 57% of observations. About 30% of observations are listings that use weekend pricing. About 13% of observations are listings that use pricing patterns that are neither DoW independent nor WE pricing. On average, listings have seven price changes per month, again with considerable dispersion.[19]

To shed light on differences between zero-fee and positive-fee listings, we next show descriptive statistics for both groups. To exclude effects from the implementation of fee transparency, we restrict the sample to the period before the European Commission's announcement, i.e. January to June 2018. Table 2 reports descriptive statistics for the price variables as well as other variables, comparing listings that never charge a cleaning fee with those that always charge a positive fee.

Table 2 shows that zero-fee listings generally set lower prices. Zero-fee listings are also more likely to use DoW independent pricing, i.e. to set a constant price independent of the day of the week, whereas positive-fee listings are more likely to use WE pricing or other, more flexible pricing strategies. Furthermore, zero-fee listings change prices much less frequently than positive-fee listings. Adding the demand side, we see that zero-fee listings are booked at lower prices and for an average of one fewer day per month.

[19]Keeping the definition of the price change variable in mind is important when interpreting it. For example, if a listing uses DoW independent pricing and changes the price from 100 GBP per night to 110 GBP per night, this would show up as seven price changes.

Table 2. Zero-fee vs positive-fee listings (before EU announcement)

	0% pos. fees			100% pos. fees			
	N	Mean	SD	N	Mean	SD	Diff
Asked price	69,955	102.38	248.52	174,734	112.17	119.67	9.784***
Pricing: DoW independent	69,955	0.73	0.37	174,734	0.53	0.42	-0.199***
Pricing: WE pricing	69,955	0.20	0.31	174,734	0.33	0.36	0.133***
Pricing: Other	69,955	0.07	0.19	174,734	0.13	0.25	0.066***
Price changes	69,955	4.09	7.39	174,734	7.28	8.88	3.190***
Booked price	33,334	94.18	131.04	110,387	114.81	217.20	20.624***
No. bookings	69,955	2.22	4.16	174,734	3.23	4.60	1.013***
Booking length	33,334	5.69	5.32	110,387	6.08	4.98	0.383***
Days booked	69,955	8.33	14.96	174,734	13.26	18.22	4.927***
Min. nights	32,340	2.46	5.49	107,382	2.41	4.87	-0.044
Reviews	69,276	15.04	35.91	173,409	18.62	32.24	3.577***
Overall rating	67,461	5.94	4.45	171,195	7.75	3.47	1.814***
Cleanliness rating	43,916	9.16	1.30	144,199	9.26	1.07	0.100***
Entire home	69,955	0.37	0.48	174,734	0.64	0.48	0.268***
Superhost	69,247	0.10	0.30	173,327	0.15	0.35	0.044***

Notes: Summary statistics for zero-fee listings vs positive-fee listings. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 % level, respectively.

Conditional on a booking, they are booked for an average of 0.4 fewer days. Zero-fee listings also have fewer reviews on average. The overall and cleanliness ratings of zero-fee listings are statistically significantly smaller. However, while the difference in the overall rating is economically meaningful, the difference in the cleanliness rating is not.[20] Turning to other observable listing and host characteristics, only about 37% of zero-fee listings are entire homes, compared to 64% of positive-fee listings. Moreover, zero-fee listings are less likely to be managed by Superhosts.[21]

Overall, the patterns in Table 2 suggest that hosts of zero-fee listings are less active and less professional. They tend to adopt simpler pricing strategies, are booked less frequently and at lower prices, receive fewer and lower-quality reviews, and are less likely to be Superhosts. In Appendix B, we provide additional results in line with this interpretation. We show that areas further away from the city center tend to have higher shares of zero-fee listing. Furthermore, we document that the differences in prices in Table 2 cannot be entirely explained by differences in observable characteristics. Finally, we explore a potential alternative

[20] The ratings are measured on a scale from 0 to 10, where each integer value corresponds to a half-star on Airbnbs five-star scale. For example, a rating of 10 represents a five-star rating, while a rating of 9 corresponds to 4.5 stars. Thus, the difference between these two ratings equals a one-star difference on Airbnb's scale.

[21] Superhost status is assigned by Airbnb and can be earned based on a high number of reservations, high response rate, low host cancellation rates, and high rating. The status is assessed every three months based on the preceding 12 months. Therefore, the status can vary within hosts. For more details, see: https://www.airbnb.co.uk/help/article/829 (last accessed: December 13, 2024).

explanation for the prevalence of zero cleaning fees: fear of backlash by consumers. We show that zero-fee listings do not have better cleanliness ratings on average which suggests that avoiding the cleaning fee out of fear for backlash is not warranted.

Taken together, we observe a clear pattern: zero-fee hosts are less active and less professional than hosts with a positive fee. They almost never switch into charging a fee, are less prevalent in central/tourist areas, are less likely to hold Superhost status, and are less likely to adopt sophisticated pricing strategies. Moreover, the zero-fee choice is not explained by a cleanliness or reputation-building strategy. All of these pieces of evidence are in line with a sizeable subset of hosts being inattentive to the cleaning fee, staying with the default fee of zero, and possibly being inattentive to their competitors' fees as well.

4 How does fee transparency affect prices?

We now examine how the policy change affected prices on Airbnb. In a setting with rational, profit-maximizing hosts and consumers who exhibit limited attention to cleaning fees, we would expect hosts to charge a positive cleaning fee when fees are shrouded. Once fee transparency is introduced, these hosts should reduce their cleaning fees, as a larger fraction of consumers now accounts for them. At the same time, hosts may have an incentive to raise the base price to partially offset the resulting revenue loss, although this second effect is theoretically ambiguous ex ante. Moreover, if shrouding constituted a deceptive practice, we would expect fee transparency to induce a sizable decrease in cleaning fees.

Contrasting this insight, in Section 3, we see that a substantial share of hosts in our sample never sets a positive fee at all. Furthermore, we show that these hosts seem to be less professional than hosts with a positive cleaning fee in many dimensions. If some hosts are inattentive to their competitors' fees when setting their own prices, this has important implications for how fee transparency affects pricing behavior on the platform.

Consider an inattentive host who benchmarks their price against nearby competitors by searching for similar listings. Before the policy change, this host observes only competitors' base prices per night, as additional fees are hidden unless they click into each listing. After the policy change, the same search displays the full price per night, inclusive of all fees. As a result, competitors with cleaning fees now appear more expensive, even if their actual total prices have not changed.

This shift in the perceived price distribution alters the hosts reference point: realizing that competitors' total prices are higher than previously thought, the inattentive host may adjust their own base price upward. Hence, fee transparency can indirectly induce price increases—not because competitors change their prices, but because previously inattentive sellers update their benchmarks once fees become salient. In contrast, no such adjustment would occur in environments where all hosts are rational and fully account for competitors' total prices.

4.1 Zero-fee Listings Increase Base Prices

To test this mechanism empirically, we first estimate the following DiD regression equation:

$$y_{it} = c + \alpha \mathbb{1}(t \geq \text{Jan } 19) + \beta \mathbb{1}(\bar{f}_i = 0)\mathbb{1}(t \geq \text{Jan } 19) + \mu_i + \gamma x_{it} + \varepsilon_{it}\,, \tag{1}$$

where y_{it} are asked prices (both with and without the fee), $\mathbb{1}(\bar{f}_i = 0)$ is the treatment dummy that is equal to one for listings that never set a positive cleaning fee, $\mathbb{1}(t \geq \text{Jan } 19)$ is the post-policy dummy, and μ_i and x_{it} denote listing fixed effects and control variables that vary across specifications, all for listing i in month t. To account for different seasonality across areas in London, we also include specifications with geography-month fixed effects. As larger geographical units, we use Local Authority Districts (LADs). As smaller geographical units, we use Lower layer Super Output Areas (LSOAs).

Table 3. Zero-fee listings increase their prices

	Outcome: Price net of fee				Outcome: Price including fee			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-policy	-10.58***	7.066***	7.191***	7.287***	-10.93***	7.481***	7.609***	7.715***
	(0.634)	(0.470)	(0.471)	(0.487)	(0.634)	(0.470)	(0.471)	(0.487)
... × Zero fees	4.968**	4.974**	4.590**	4.521**	4.664**	4.671**	4.274*	4.194*
	(1.665)	(1.665)	(1.676)	(1.695)	(1.665)	(1.665)	(1.676)	(1.695)
Linear time trend	1.479***				1.542***			
	(0.0469)				(0.0469)			
Constant	52.35***	115.3***	115.3***	115.2***	60.27***	125.9***	125.9***	125.8***
	(1.503)	(0.269)	(0.270)	(0.275)	(1.503)	(0.269)	(0.270)	(0.275)
Listing FEs	✓	✓	✓	✓	✓	✓	✓	✓
Month FE		✓	✓	✓		✓	✓	✓
LAD-month FEs			✓				✓	
LSOA-month FEs				✓				✓
Adj. R^2	0.85	0.85	0.85	0.84	0.85	0.85	0.85	0.85
Avg. total price	119.47	119.47	119.47	119.47	130.25	130.25	130.25	130.25
Obs.	1,030,459	1,030,459	1,030,459	1,030,459	1,030,459	1,030,459	1,030,459	1,030,459

Notes: Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 % level, respectively. We show event studies corresponding to columns (3) and (7) in Appendix C.

Table 3 presents the results of these DiD regressions using the zero-fee listings as the treatment group. It shows that zero-fee hosts increase their price by around 5 GBP per night after fee transparency, relative to listings charging a positive fee. These results suggest that more inattentive hosts, proxied by zero-fee hosts, responded to the fee transparency by raising the base price compared to hosts with a positive cleaning fee. However, these effects likely under-estimate the true reaction for two reasons. First, hosts with a positive cleaning fee also have an incentive to raise their price in response due to strategic complementarity. If that is the case, then listings in the control group may react to the treatment group's reaction, leading to a violation of the Stable Unit Treatment Value Assumption (SUTVA). Second, the extent of an inattentive host's adjustment depends on how their nearby competitors set prices and fees. If a zero-fee host is only comparing their own prices to those of other zero-fee listings, the relative affordability will not change between fee transparency regimes. If, however, many of their competitors use cleaning fees, their perceived affordability increases a lot with the introduction of fee transparency, leading to a stronger incentive to increase their own price.

4.2 Measuring Change in Perceived Affordability

To better capture the role of competitors' prices, we measure hosts' incentives to respond to fee transparency by taking into account the change in perceived affordability of competing listings. Specifically, we measure how a listing's price compares to its competitors when fees are entirely ignored as opposed to when fees are fully taken into account. This measure gives an idea of how perceived affordability changes (even absent any actual price changes) when moving from a setting in which fees are shrouded to a setting in which fees are fully transparent. Inattentive hosts whose listings become relative more affordable should have a stronger incentive to increase their prices after fee transparency is implemented.

To explore this hypothesis, we propose the following analysis. For each listing i in month t, we calculate the following price difference:

$$\Delta P_{i,t}^{shrouded} = p_{i,t} - \tilde{p}_{i,t},$$

where $p_{i,t}$ is the average asked base price per night of listing i in month t, and $\tilde{p}_{i,t}$ is the average asked base price per night of comparable listings in the same month. This difference indicates how the price (net of fees) of listing i compares to its competitors in month t. It reflects the price difference as perceived by a host who completely ignores add-on fees. Next, we define:

$$P_{i,t} = p_{i,t} + \frac{f_{i,t}}{n_{i,t}}$$

as the total price per night (including the cleaning fee $f_{i,t}$ divided by a measure of average length of booking

$n_{i,t}$) of listing i in month t, and calculate the difference:

$$\Delta P_{i,t}^{unshrouded} = P_{i,t} - \tilde{P}_{i,t}.$$

This measure captures how the total price of listing i compares to that of its competitors in month t. It reflects the price difference as perceived by a host who fully considers add-on fees. The difference

$$\delta_{i,t} = \Delta P_{i,t}^{shrouded} - \Delta P_{i,t}^{unshrouded}$$

represents the impact of price transparency on the perceived relative affordability of listing i in month t for an agent who was fully inattentive to the fee prior to price transparency and is fully attentive to it afterwards.

Notably, $\delta_{i,t} = \tilde{P}_{i,t} - \tilde{p}_{i,t} - \frac{f_{i,t}}{n_{i,t}}$ is driven by the difference between the listing's own fee and the average fee of comparable listings. The larger $\delta_{i,t}$, the more affordable listing i becomes due to fee transparency in month t.

To calculate $\delta_{i,t}$, we first obtain a per-night measure of the cleaning fee and then define relevant comparison prices $\tilde{p}_{i,t}$ and $\tilde{P}_{i,t}$. Cleaning fees on Airbnb apply to the entire stay, so we divide the total cleaning fee by a relevant number of nights. Specifically, we calculate the per-night cleaning fee by dividing the total fee by the average duration of stays in 2018 for each listing. For listings with no observed bookings, we instead divide by the minimum nights requirement. If a listing has no minimum nights requirement either, we divide by one, effectively assuming a relevant stay length of one night.[22]

We then define the benchmark comparison prices $\tilde{p}_{i,t}$ and $\tilde{P}_{i,t}$, which represent the average prices of comparable listings in the same period. We calculate these benchmark prices by estimating linear regressions of the price (net of the fee and including the fee, respectively) on whether a listing is hosted by a "Superhost", its number of reviews, whether it is instant bookable, and whether it is an entire home. We also absorb LSOA and month fixed effects. We focus on 2018 to calculate the measure based on pre-transparency prices. Based on these regressions, we obtain predicted prices for listings with similar observable characteristics. We use the residuals from these regressions as estimates of $\Delta P_{i,t}^{shrouded}$ and $\Delta P_{i,t}^{Unshrouded}$. Finally, we take the 2018 average of $\delta_{i,t}$ for each listing i, denoted as δ_i, to measure an inattentive host's incentive for price increases following the policy change.

The left panel of Figure 3 presents the distribution of δ_i for all listings in our sample. The distribution of δ_i is relatively symmetric and centered around zero with some large outliers. This is expected: within

[22] As an alternative way to address this issue, we restrict the analysis to listings with a minimum nights requirement of one night only and use the observed cleaning fee directly in $P_{i,t}$. This restriction substantially reduces the sample size, but the results remain similar. The corresponding estimates are reported in Appendix D.4.

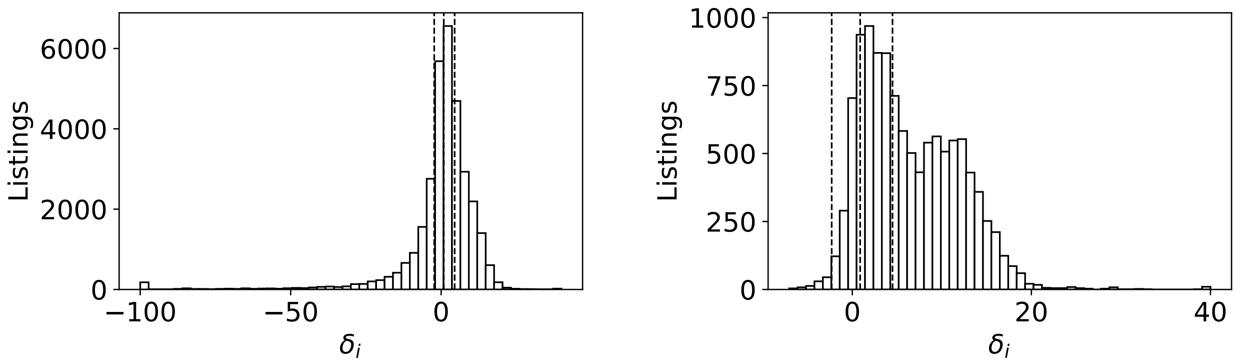

Figure 3. Distribution of δ_i for entire sample (left) and for zero fees only (right). Every point is one individual listing. Values are windsorized at -100 and 40 for better readability. The vertical lines show the 25th, 50th, and 75th percentile.

each local area and month, some listings will have higher-than-average fees and others lower-than-average fees, so relative affordability changes should balance out. Furthermore, because δ_i is based on differences between regression residuals, it is also centered around zero by construction.

The right panel of Figure 3 presents the distribution of δ_i for those listings that never set a positive cleaning fee. For these listings, δ_i is generally positive, consistent with the idea that zero-fee listings become relatively more affordable once fees are unshrouded. Yet, there is considerable variation: while some zero-fee listings display large positive values of δ_i, many exhibit values close to zero. This dispersion suggests that focusing exclusively on zero-fee listings does not fully capture hosts' incentives to react to the policy. In particular, even hosts charging small positive fees may experience similar incentives if they operate in areas where competing listings impose high cleaning fees.

For our analysis, we define the treatment group as listings in the top quartile of the δ_i distribution, that is, those with $\delta_i > 4.5$. These listings experience the largest increase in relative affordability once fees become transparent and thus are expected to react most strongly to the policy. Figure 4 plots average prices over time for the treatment group and the other quartiles of δ_i. The left panel displays prices net of the cleaning fee, while the right panel includes the fee.

In terms of levels, listings in first (Q1) and fourth (Q4) quartiles exhibit higher average prices than those in second (Q2) and third (Q3) quartiles. Q2 and Q3 correspond to listings whose perceived affordability changes least due to fee transparency, whereas Q1 listings are those whose perceived affordability decreases the most. Comparing the two panels, prices for Q1–Q3 are naturally higher when fees are included, while the difference between the two price measures is much smaller for Q4. This pattern reflects that Q4 listings (those becoming more affordable after transparency) are more likely to have zero or very small cleaning

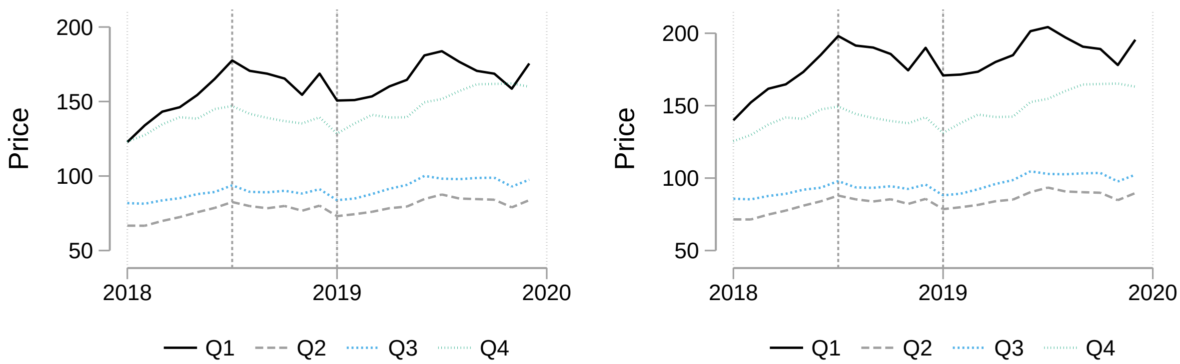

Figure 4. Prices net of fee (left) and including of the fee (right) over time by quartiles of δ_i

fees.

Turning to price dynamics, prices in Q2 and Q3 evolve in parallel and remain broadly aligned with the treatment group throughout 2018. Starting in 2019, however, Q4 prices rise and diverge from the others, consistent with these listings increasing their prices following the introduction of fee transparency. Prices for Q1 diverge somewhat in early 2018, move in parallel with Q4 during the latter half of that year, and converge again once transparency is fully implemented.

The similarity in price levels between listings in Q1 and Q4, contrasted with the substantially lower prices observed for Q2 and Q3, suggests that Q1 and Q4 listings are more likely to compete directly with each other. Figure 5 provides further evidence consistent with this interpretation by mapping the geographic distribution of listings across quartiles of the δ_i distribution. The maps report the share of observations in each quartile by Local Authority District. Listings in Q1 and Q4 are distributed in a strikingly similar manner, with both concentrated in more central areas of London. In contrast, listings in Q2 are less prevalent in these central areas, while listings in Q3 are more evenly distributed across the city.

The co-location of listings with very high and very low values of δ_i is also partly mechanical. Because nearby competitors are used to construct δ_i, listings in the same neighborhood influence each other's relative affordability. In areas with many listings charging high cleaning fees, neighboring listings with low or zero fees will, by construction, exhibit high δ_i values. Conversely, in areas where cleaning fees are generally low, the opposite holds. Finally, it is not surprising that both Q1 and Q4 listings tend to cluster in central areas and exhibit higher average prices. Listing prices and cleaning fees are typically positively correlated, and areas with higher average fees are precisely those where listings with zero or small fees appear much more affordable once fees become transparent. Thus, these areas exhibit larger changes in perceived affordability.

For our analysis, we take the fourth quartile of the δ_i distribution as the treatment group. These listings

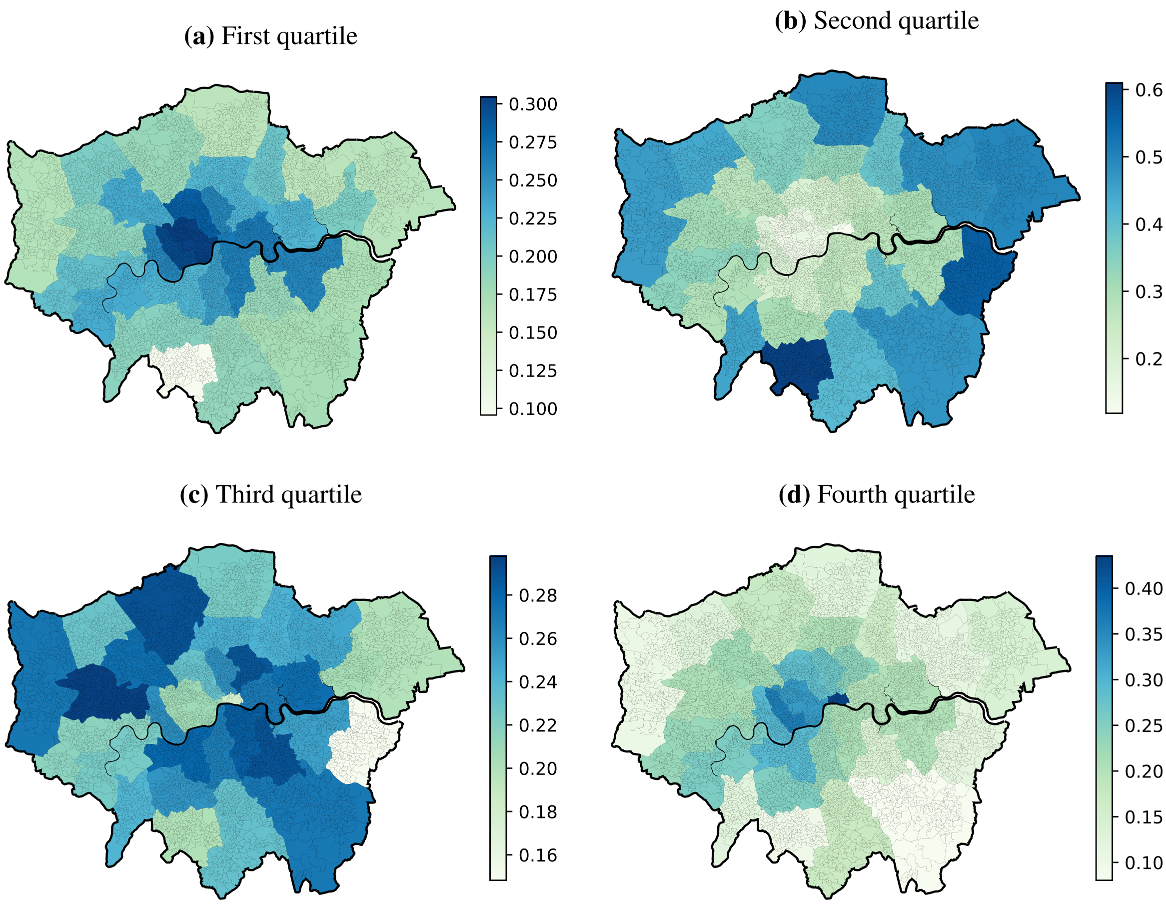

Figure 5. Share of observations belonging to a given quartile of the δ_i distribution by LAD

experience the largest increase in relative affordability once fees become transparent and are therefore expected to react most strongly to the policy. We use listings in the second and third quartiles as the control group in our preferred specifications. These listings have δ_i values close to zero (see the left panel of Figure 3), implying that their perceived affordability changes little with fee transparency. They thus provide a suitable comparison group, as they are less likely to be directly affected by the policy.

We exclude listings in the first quartile for two reasons. First, Q1 listings are often located near Q4 listings and may be indirectly affected by the policy through strategic complementarities in price setting: if Q4 listings increase prices, their close competitors could have an incentive to do the same. Second, even if Q1 listings are not located near Q4 listings, their own perceived affordability decreases with transparency, which could induce downward price adjustments. In both cases, including Q1 listings in the control group would risk violating the Stable Unit Treatment Value Assumption (SUTVA). In Appendix D.1 we present robustness checks using alternative definitions of treatment and control groups (for instance, using tertiles and quintiles to define treatment and control groups, or using Q1 as the control group). The results are broadly consistent across specifications, with a smaller magnitude of the effect when Q1 is used as the control group – consistent with some Q1 listings adjusting prices in response to Q4 listings.

4.3 The Impact of Price Transparency on Prices

Using listings in the highest quartile of the δ_i distribution as our treatment group and those in the second and third quartiles as our control group, we estimate DiD regressions to assess if "more affordable" listings adjust their prices compared to the other listings after fee transparency was implemented. Specifically, we estimate:

$$y_{it} = c + \alpha \mathbb{1}(t \geq \text{Jan } 19) + \beta \mathbb{1}(\delta_i > 4.5) \mathbb{1}(t \geq \text{Jan } 19) + \mu_i + \gamma x_{it} + \varepsilon_{it}\,, \tag{2}$$

where y_{it} are asked prices (both with and without the fee), $\mathbb{1}(\delta_i > 4.5)$ is the treatment dummy, $\mathbb{1}(t \geq \text{Jan } 19)$ is the post-policy dummy, and μ_i and x_{it} are listing fixed effects and control variables that vary across specifications, all for listing i in month t.

The interaction between the treatment variable and the post-policy dummy represents the treatment effect. We control for listing fixed effects μ_i to account for unobserved heterogeneity across listings that can be associated with their δ_i as well as their asked prices. In the most basic specification, we only include a linear time trend. Then, we add month fixed effects to account for city-level seasonality. The post-policy dummy is effectively a year fixed effect because we restrict the analysis to 2018 and 2019 and the dummy is equal to one for observations in 2019. Hence, the month fixed effects together with the post-policy dummy are collinear with the linear time trend, and we cannot estimate them all jointly. To allow for different patterns of seasonality in different geographies, we also include specifications which include geography-specific month fixed effects. Again, we include specifications using both larger and more granular geographic units.[23]

Table 4. DiD results for prices

	Outcome: Price net of fee				Outcome: Price including fee			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-policy	-8.591***	4.990***	4.909***	4.975***	-8.539***	5.292***	5.210***	5.280***
	(0.553)	(0.454)	(0.455)	(0.486)	(0.553)	(0.454)	(0.455)	(0.486)
... × "More affordable"	8.420***	8.407***	8.719***	8.620***	8.486***	8.473***	8.786***	8.687***
	(1.694)	(1.694)	(1.681)	(1.677)	(1.694)	(1.694)	(1.681)	(1.677)
Linear time trend	1.139***				1.160***			
	(0.0438)				(0.0438)			
Constant	51.59***	99.76***	99.75***	99.91***	54.72***	103.8***	103.8***	103.9***
	(1.917)	(0.253)	(0.254)	(0.255)	(1.917)	(0.253)	(0.254)	(0.255)
Listing FEs	✓	✓	✓	✓	✓	✓	✓	✓
Month FE		✓	✓	✓		✓	✓	✓
LAD-month FEs			✓				✓	
LSOA-month FEs				✓				✓
Adj. R^2	0.77	0.77	0.77	0.76	0.77	0.77	0.77	0.76
Avg. price	102.92	102.92	102.92	103.10	107.06	107.06	107.06	107.24
Obs.	645,531	645,531	645,531	642,862	645,531	645,531	645,531	642,862

Notes: Sample consists of quartiles 2, 3, and 4 of the δ_i distribution. Quartile 4 ("more affordable") is the treatment group. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 % level, respectively.

Table 4 shows the DiD results for different specifications.[24] We report the results for both the price net of the fee (columns (1) to (4) as well as inclusive of the fee (columns (5) to (8)). The results suggest that listings appearing more affordable under price transparency (i.e., those with a δ_i in the highest quartile of the distribution) increase their prices following the introduction of price transparency compared to other comparable listings. After the policy change, these "more affordable" listings become approximately 8 to 9 GBP more expensive. This finding holds regardless of whether we focus on the price net of the fee or the price including the fee.

To assess whether the parallel trends assumption is likely to hold, we also run event study regressions corresponding to columns (3) and (7) in Table 4. Specifically, we estimate the following equation:

$$y_{it} = \sum_s \{\alpha_s \mathbb{1}(t=s) + \beta_s \mathbb{1}(\delta_i > 4.5)\mathbb{1}(t=s)\} + \mu_i + \zeta_{g(i)t} + \varepsilon_{it}\,, \tag{3}$$

where the sum is over all the months in our sample and $\zeta_{g(i)t}$ are LAD-month fixed effects.

Figure 6 reports the estimated event study coefficients β_s. The results do not suggest a violation of the assumption of parallel conditional pre-trends. The estimates are noisy, but again suggest that prices

[23]The larger geographical units are Local Authority Districts (LADs) and the smaller geographical units are Lower layer Super Output Areas (LSOAs).

[24]We implement the regressions using the *reghdfe* package in Stata, as described in Correia (2016).

have increased relatively more for listings that appear more affordable under full price transparency. It is important to note that, in 2018, the average price per night (net of the fee) of a listing classified as "more affordable" was about 131.51 GBP. Therefore, an average increase of 8 GBP amounts to an average price increase of about 6%.

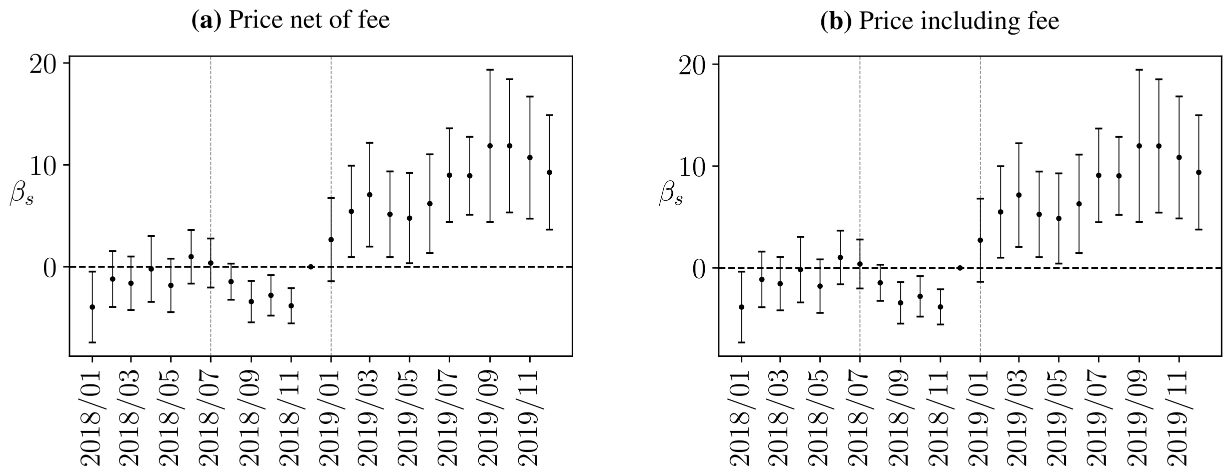

Figure 6. Event study for asked prices corresponding to columns (3) and (7) of Table 4. Includes linear time trend and listing as well as LAD-month FEs. Standard errors clustered on listing level. Bars show 95% confidence intervals.

In Appendix D, we present additional analyses. In Appendix D.1, we show results using different definitions for the control group: all listings outside the fourth quartile as well as each of the lower three quartiles separately. We also replicate the main analysis using alternative treatment and control definitions based on tertiles and quintiles of the δ_i distribution. Across these specifications, our main findings hold qualitatively. Quantitatively, the effect size is largest when using the central part of the δ_i distribution as the control group. The effect size tends to be lower when comparing to listings with more negative δ_i values (e.g. the first tertile, quartile, or quintile). These results are in line with these low-δ_i listings having an incentive to increase their prices following the implementation of fee transparency due to strategic complementarity with listings in the treatment group. In Appendix D.2, we show results for booked prices rather than asked prices. The results are similar qualitatively, but effect sizes are about half as large. This result suggests that consumers react to the price increases by "more affordable" listings by lowering demand for listings with higher price increases. In Appendix D.3, we show that a placebo analysis using data from 2017 and 2018 with January 2018 as the pseudo-policy date shows no differential price change for "more affordable" listings, ruling out end-of-year effects as a driver of our main results. Finally, our analysis requires calculating a per-night cleaning fee because the cleaning fee applies to the entire stay whereas the base price applies per night.[25] As an alternative approach, we restrict the sample to listings with a minimum nights requirement of exactly one night and use the observed cleaning fee directly when calculating the total price. While this restriction substantially reduces the sample size, the results remain qualitatively similar. We report these results in Appendix D.4.

4.4 Mechanism and Robustness

In the previous subsections, we show that zero-fee hosts react to fee transparency by raising their prices. Furthermore, we find that listings whose perceived affordability improves increase their prices after fee transparency is implemented compared to other listings.

We propose that these results are driven by hosts who are inattentive to cleaning fees when they are obfuscated. Hosts who do not set a cleaning fee may be unaware that their competitors use one and typically benchmark their own prices against nearby comparable listings. When fees are shrouded, these hosts observe only competitors' base prices on the platform, and would need to click on individual listings to learn the total price being charged. Once fees become unshrouded, the total prices of competitors using cleaning fees appear higher, leading inattentive hosts to raise their own prices even though competitors have not changed theirs. This mechanism is precisely what our δ_i measure is designed to capture.

In what follows, we provide additional evidence to support this mechanism. Specifically, we show that more experienced "more affordable" hosts do not change their price in response to transparency. This lack of response is consistent with those hosts being aware of competitors' fees even when they are shrouded. Therefore, a mere change in the display of total prices does not affect their price-setting.

In the second part of this subsection, we assess possible other explanations for our results and we provide additional robustness exercises. First, we rule out that the price increase is driven by changes in demand due to fee transparency. Second, hosts located outside the European Union should not be affected by the transparency change and the proposed mechanism. Therefore, "more affordable" listings in New York City, for example, should not increase their prices relative to the others. Third, we show that our main results are not driven by Airbnb's pricing algorithm.

Not a reaction to a demand shock. An alternative explanation for the results in Section 4.3 could be that "more affordable" listings face a positive demand shock because their fee-using competitors are perceived as

[25] In our main analysis, we do so by dividing the cleaning fee by each listing's average length of stay in 2018, i.e. the pre-treatment period. If we do not observe any bookings in 2018 for a given listing, we divide by the minimum nights requirement instead. If we do not observe a minimum nights requirement, we divide by one, effectively assuming a relevant length of stay of one night.

more expensive once fee transparency is implemented. In response to this increase in demand, these listings may find it optimal to raise their prices.

To assess this alternative mechanism, we estimate a similar event study as described in Equation (3), but using the nights booked per month as the outcome variable. Figure 7 shows the corresponding event study coefficients. These estimates do not suggest that demand for "more affordable" listings has increased with the implementation of fee transparency. If anything, they would suggest that "more affordable" listings are booked a bit less, possibly as a result of their price adjustments. Furthermore, Figure 6 shows that the price increase occurs immediately after fee transparency is implemented, whereas we would expect hosts to require a bit of time before learning that their demand has increased. Together, these results suggest that reactions to changing demand conditions are not likely to drive the patterns we see in Section 4.3.

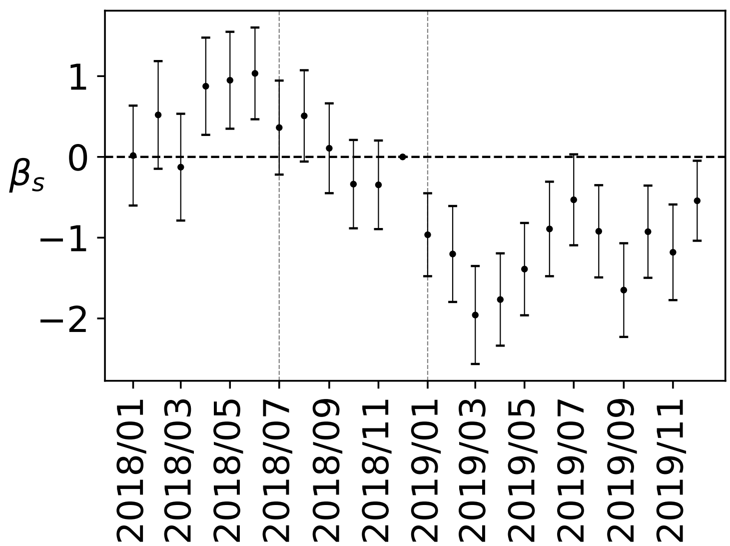

Figure 7. Event study analysis for booked nights. Sample consists of quartiles 2, 3, and 4 of the δ_i distribution. Quartile 4 ("more affordable") is the treatment group. Includes linear time trend and listing as well as LSOA-month FEs. Standard errors clustered on listing level. Bars show 95% confidence intervals.

Superhosts do not react. Our proposed mechanism requires a degree of inattention to fees by hosts of "more affordable" listings who increase prices when fees become transparent. More professional hosts should be less likely to be subject to such inattention. The Airbnb platform has a natural measure of host professionalism: the Superhost status. The Superhost status is an objective measure of professionalism assigned by Airbnb. Superhost status can be earned based on a high number of reservations, high response rate, low host cancellation rates, and high rating. The status is assessed every three months based on the preceding 12 months.[26] To assess whether "more affordable" Superhosts behave differently from "more affordable" non-Superhosts, we estimate a triple-differences regression in which we further interact the treatment and post-policy dummies with a Superhost dummy variable.

Table 5. "More affordable" Superhosts do not adjust their prices

	Outcome: Price net of fee				Outcome: Price including fee			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-policy	-8.349***	5.303***	5.217***	5.307***	-8.300***	5.601***	5.514***	5.608***
	(0.593)	(0.527)	(0.527)	(0.564)	(0.593)	(0.526)	(0.527)	(0.564)
... ×"More affordable"	9.308***	9.298***	9.636***	9.514***	9.368***	9.358***	9.697***	9.576***
	(1.895)	(1.896)	(1.881)	(1.873)	(1.895)	(1.896)	(1.881)	(1.873)
... × Superhost	-2.134***	-2.127***	-2.091***	-2.249***	-2.108***	-2.102***	-2.066***	-2.224***
	(0.581)	(0.580)	(0.580)	(0.625)	(0.581)	(0.581)	(0.581)	(0.626)
... × "More affordable" × Superhost	-8.991***	-9.019***	-9.242***	-8.995***	-8.930***	-8.957***	-9.181***	-8.938***
	(2.315)	(2.316)	(2.309)	(2.314)	(2.316)	(2.317)	(2.310)	(2.315)
Linear time trend	1.145***				1.166***			
	(0.0439)				(0.0439)			
Constant	51.33***	99.75***	99.74***	99.90***	54.46***	103.8***	103.7***	103.9***
	(1.925)	(0.253)	(0.254)	(0.256)	(1.925)	(0.253)	(0.254)	(0.256)
Listing FEs	✓	✓	✓	✓	✓	✓	✓	✓
Month FE		✓	✓	✓		✓	✓	✓
LAD-month FEs			✓				✓	
LSOA-month FEs				✓				✓
Adj. R^2	0.769	0.769	0.769	0.757	0.771	0.771	0.771	0.759
Avg. price	102.9	102.9	102.9	103.1	107.0	107.0	107.0	107.2
Obs.	645,353	645,353	645,353	642,684	645,353	645,353	645,353	642,684

Notes: Sample consists of quartiles 2, 3, and 4 of the δ_i distribution. Quartile 4 ("more affordable") is the treatment group. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 % level, respectively.

Table 5 shows the results of this triple-differences regression. The results show that the price increase of "more affordable" listings after fee transparency is implemented is almost exclusively due to those "more affordable" listings that are not hosted by Superhosts. The triple-interaction coefficient is about -9, almost entirely offsetting the interaction coefficient between the post-policy and the "more affordable" dummy. This results suggests that Superhosts whose perceived affordability increases with fee transparency (from the perspective of a previously inattentive guest) do not adjust their prices after the transparency change. This result is in line with these Superhosts having been aware of their competitors' full prices even before fee transparency was implemented. Hence, the introduction of fee transparency does not result in an incentive for these hosts to adjust prices.[27]

New York vs. London Our proposed mechanism is based on the idea that inattentive hosts of "more affordable" listings in London observe higher competitor prices once fee transparency is implemented, simply because competitors' cleaning fees are included. This mechanism should not apply to inattentive hosts

[26] For more details, see: `https://www.airbnb.co.uk/help/article/829` (last accessed: December 13, 2024).

[27] If any, their effect would be a second-order effect driven by price complementarity.

located outside of the European Union because the fee display does not change for them. To test this hypothesis, we combine our data from London with similar data from New York City. Then, we estimate a triple-differences regression in which we add a London dummy as the third interaction.[28] Table 6 shows the results of this exercise.

Table 6. Affordable listings in London increase prices, those in NYC do not

	Outcome: Price net of fee				Outcome: Price including fee			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-policy	-12.14***	6.263***	-13.70***	6.798***	-11.96***	6.889***	-13.81***	7.455***
	(0.659)	(0.796)	(1.019)	(0.839)	(0.660)	(0.797)	(1.020)	(0.840)
... × "More affordable"	-0.578	-0.569	-0.140	0.371	-0.521	-0.511	-0.0824	0.430
	(1.244)	(1.243)	(1.246)	(1.256)	(1.245)	(1.244)	(1.247)	(1.257)
... × London	-0.270	-0.255	-1.431	-1.249	-0.532	-0.517	-1.742	-1.556
	(0.918)	(0.918)	(0.999)	(1.007)	(0.919)	(0.919)	(0.999)	(1.008)
... × "More affordable" × London	8.643***	8.649***	8.403***	7.732***	8.675***	8.681***	8.438***	7.766***
	(2.107)	(2.107)	(2.104)	(2.076)	(2.108)	(2.108)	(2.105)	(2.076)
Linear time trend	1.545***		1.719***		1.582***		1.783***	
	(0.0375)		(0.0898)		(0.0375)		(0.0898)	
Constant	83.74***	112.0***	80.60***	112.3***	89.05***	118.0***	85.42***	118.3***
	(0.776)	(0.193)	(1.710)	(0.194)	(0.777)	(0.193)	(1.711)	(0.194)
Listing FEs	✓	✓	✓	✓	✓	✓	✓	✓
Month FE		✓	✓	✓		✓	✓	✓
LAD/Borough-month FEs			✓				✓	
LSOA/NTA-month FEs				✓				✓
Adj. R^2	0.72	0.72	0.72	0.71	0.72	0.72	0.72	0.71
Avg. price	115.14	115.14	115.14	115.45	121.32	121.32	121.32	121.65
Obs.	988,422	988,422	988,422	983,235	988,422	988,422	988,422	983,235

Notes: Analysis using listings from London and NYC. Sample consists of quartiles 2, 3, and 4 of the city-specific δ_i distributions. Quartile 4 ("more affordable") is the treatment group. In specifications with fixed effects by geography-months, we use LADs in London and boroughs in New York City as the larger geographical units. In the specifications with smaller geography-month fixed effects, we use LSOAs in London and Neighborhood Tabulation Areas (NTA) for New York City. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 % level, respectively.

The results suggest that "more affordable" listings in London increase their prices after fee transparency is implemented in the European Union, whereas those in New York City do not. Again, this result is in line with our mechanism. Inattentive hosts in London whose perceived affordability changes due to fee transparency have an incentive to increase prices. Inattentive hosts in New York City do not have the same incentive, because their perceived affordability does not change as they are not subject to the transparency change.

Use of pricing algorithm Airbnb offers the possibility to let their algorithms choose an optimal price within bounds set by the host. There might be concern that our results are driven by these algorithmic pricing users. We cannot directly observe if a host uses automatic pricing. However, following the existing

[28]We retain the original currencies for each city (GBP for London and USD for New York) to avoid discrepancies arising from exchange rate fluctuations. All specifications include listing fixed effects, so any fixed differences related to currency levels do not confound the analysis.

literature (e.g., Brown and MacKay 2023; Aparicio et al. 2024; Assad et al. 2024), we can infer the use of pricing algorithms by examining the frequency of price adjustments.

We define a price change as a change between the price asked for a given night compared to the same night in the previous week. To address concerns that our results may be driven by the pricing algorithm, we exclude the top 5% of listings with the most frequent price changes. The rationale is that listings adjusting their prices very frequently are more likely to rely on pricing algorithms rather than manual adjustments. We then run the same analyses as in Table 4. Table 7 reports the results of this exercise. The findings are similar to those in Table 4, suggesting that our results are not driven by the use of algorithmic pricing.

Table 7. Excluding high frequency price changers

	Outcome: Price net of fee				Outcome: Price including fee			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-policy	-7.313***	4.819***	4.770***	4.834***	-7.321***	5.078***	5.028***	5.096***
	(0.667)	(0.491)	(0.492)	(0.511)	(0.668)	(0.491)	(0.492)	(0.511)
... × "More affordable"	8.064***	8.048***	8.298***	8.147***	8.147***	8.132***	8.384***	8.235***
	(1.765)	(1.765)	(1.760)	(1.765)	(1.765)	(1.765)	(1.761)	(1.765)
Linear time trend	1.017***				1.039***			
	(0.0483)				(0.0484)			
Constant	53.80***	96.79***	96.78***	96.94***	57.25***	101.2***	101.2***	101.3***
	(1.542)	(0.253)	(0.254)	(0.261)	(1.545)	(0.254)	(0.254)	(0.261)
Listing FEs	✓	✓	✓	✓	✓	✓	✓	✓
Month FE		✓	✓	✓		✓	✓	✓
LAD-month FEs			✓				✓	
LSOA-month FEs				✓				✓
Adj. R^2	0.77	0.77	0.77	0.76	0.77	0.77	0.77	0.76
Avg. price	99.81	99.81	99.81	99.98	104.32	104.32	104.32	104.50
Obs.	609,643	609,643	609,643	607,013	609,643	609,643	609,643	607,013

Notes: Sample consists of quartiles 2, 3, and 4 of the δ_i distribution and excludes observations with more than 23 price changes per month (95%). Quartile 4 ("more affordable") is the treatment group. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 % level, respectively.

5 How does fee transparency affect cleaning fees?

When some consumers are inattentive to shrouded fees, sellers can increase profits by shifting part of the price to the hidden price component (e.g. Gabaix and Laibson, 2006; Ellison and Ellison, 2009; Chetty et al., 2009; Blake et al., 2021; Bakker and Datta, 2025). This mechanism implies that when fees become unshrouded for a subset of consumers, sellers may find it optimal to readjust their pricing strategy by reducing the fees they charge. In the preceding sections, we documented that a large share of hosts on Airbnb do not impose cleaning fees at all. These hosts tend to increase their prices when their listings become relatively more affordable as a result of greater fee transparency.

In this section, we shift our focus to a different subset of hosts, that is those who do charge a positive cleaning fee. We investigate whether these hosts adjust their fees in response to the implementation of fee transparency. We begin by outlining the key identification challenges and our empirical strategy to address them. We then present the main results, followed by a series of robustness checks to validate the main results.

5.1 Identification Strategy

It is important to note that the treatment in this context was primarily directed at consumers. Beginning in January 2019, travelers searching from within the EU were shown transparent prices that included all applicable fees, whereas travelers searching from outside the EU continued to see prices in which such fees remained obfuscated. The extent to which a given host is affected by this policy depends on the composition of their customers, that is the share of guests originating from the EU. Listings with a higher proportion of EU travelers are therefore expected to be more exposed to the transparency requirement. A host typically hosting non-EU guests would not expect a change in their demand as a consequence of fee transparency.

Therefore, a critical empirical challenge is that we do not directly observe the country of origin of guests for each listing, which complicates the measurement of exposure to the treatment. We do observe the self-reported home location of consumers who leave reviews on the platform. Using this information, we construct a proxy for EU exposure: the share of nearby reviews from EU travelers in the pre-2019 period. Specifically, for each listing, we calculate the proportion of reviews that originate from EU travelers received by listings within a one-kilometer radius. EU versus non-EU status is determined from the self-reported home location of guests. While self-reported, this measure is available for the majority of reviews in our dataset.[29] This procedure provides us with a measure of listing-specific, time-invariant, pre-policy exposure to EU travelers which should affect the degree to which the policy change impacts any given listing.

For our analysis, we discretize the EU exposure variable. Our treatment variable is set to one for any listing where the pre-policy share of nearby reviews from EU travelers exceeds the city-wide median, and zero otherwise. According to this measure, London has an average EU traveler share of 60%.

Figure 8 presents the average exposure to EU travelers by LSOA in London. More central and touristy areas are more exposed to non-EU travelers. Similarly, the area around Heathrow airport in the West is popular among non-EU travelers. This pattern indicates that neither the EU share of travelers nor the cleaning fee is randomly distributed. Therefore, we include geography-specific month as well as listing fixed effects

[29] For this approach to be valid, it is not necessary to assume that guests based in the EU have the same propensity to report their location or leave a review as non-EU guests. These propensities may differ; however, we require that any differences are consistent across the city to ensure meaningful comparisons between listings.

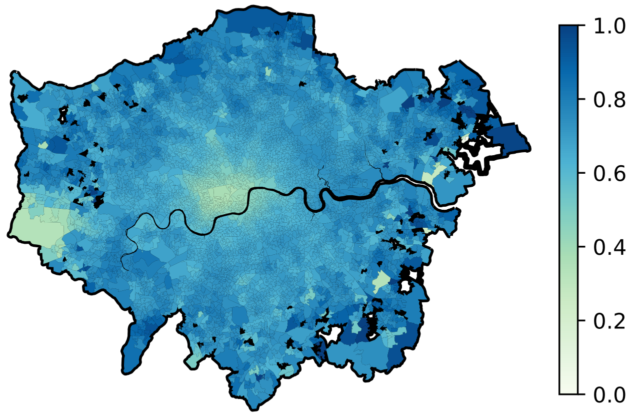

Figure 8. Average share of reviews from EU travelers by LSOA in London

in our analyses. The listing fixed effects account for cross-sectional, time-constant differences in cleaning fees and EU exposure. The geography-specific month fixed effects capture differences in seasonality in different areas of the city that are unrelated to the policy change.

5.2 Main Results

Defining listings with above-median exposure to EU travelers as the treatment group, we employ both DiD and event study regressions similar to those described in Equations (2) and (3). Our outcome variable of interest is the asked cleaning fee.

Table 8 presents our main DiD results for cleaning fees. In Column (1), we show results in the presence of a linear time trend. In Column (2), month fixed effects are added, whereas in Columns (3) and (4), we include geography-month fixed effects. In general, cleaning fees have been increasing as evidenced by the positive linear time trend and the positive post-policy coefficient. However, across all specifications, our results show that listings with above-median exposure to EU travelers have increased the cleaning fee by less after the introduction of price transparency, resulting in a decrease in cleaning fees relative to the control group. We find that listings with high EU exposure statistically significantly decrease their cleaning fees following the implementation of fee transparency requirements. The average decrease amounts to 0.6 to 0.7 GBP. At an average cleaning fee of 46 GBP among listings with a positive fee, this amounts to a decrease of about 1.5%.

To check the parallel trends assumption, we also include results from an event study regression corresponding to column (3) in Table 8. Figure 9 presents the results of the event study analysis. The findings indicate that listings began reducing their cleaning fees following the full implementation of price trans-

Table 8. Listings with high EU exposure decrease their fees post-transparency

	(1)	(2)	(3)	(4)
Post-policy	0.423***	1.949***	1.970***	1.991***
	(0.0520)	(0.0610)	(0.0625)	(0.0645)
... × High EU	-0.604***	-0.604***	-0.649***	-0.674***
	(0.0726)	(0.0726)	(0.0762)	(0.0798)
Linear time trend	0.127***			
	(0.00342)			
Constant	27.29***	32.69***	32.69***	32.69***
	(0.159)	(0.0203)	(0.0203)	(0.0207)
Listing FEs	✓	✓	✓	✓
Month FE		✓	✓	✓
LAD-month FEs			✓	
LSOA-month FEs				✓
Adj. R^2	0.98	0.98	0.98	0.98
Avg. cleaning fee (if > 0)	46.03	46.03	46.03	46.03
Obs.	1,030,459	1,030,459	1,030,459	1,030,459

Notes: DiD estimates for asked cleaning fees. Standard errors are clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 % level, respectively.

parency in January 2019. Prior to the European Commissions call in July 2018, the conditional trends in cleaning fees were parallel across the treatment and control groups. There is some evidence of a reaction starting in September 2018, before the full implementation in January 2019. These early responses are consistent with hosts anticipating the policy change or with Airbnb experimenting with parts of the website before fully implementing the new policy. Recall that in September 2018, Airbnb committed to achieving full price transparency by the end of the year. This could explain why some hosts started adjusting their cleaning fees as early as September 2018. However, the main drop occurs after January 2019, when Airbnb fully implemented the policy.[30]

The estimated effect of –0.6 to –0.7 GBP is economically modest. This magnitude is consistent with the nature of our identification strategy, which requires a certain degree of sophistication on the part of hosts. Specifically, hosts must be aware of the change in fee transparency, understand how it affects guests in different part of the world, and anticipate the share of their guests that is exposed to the policy. As such, only a subset of relatively attentive or informed hosts is likely to adjust their pricing behavior, resulting in a smaller aggregate effect.

[30]This observation suggests that the true treatment date might be September 2018. However, to be conservative, we retain January 2019 as the treatment date. This choice likely leads us to slightly underestimate the true effect of the policy change.

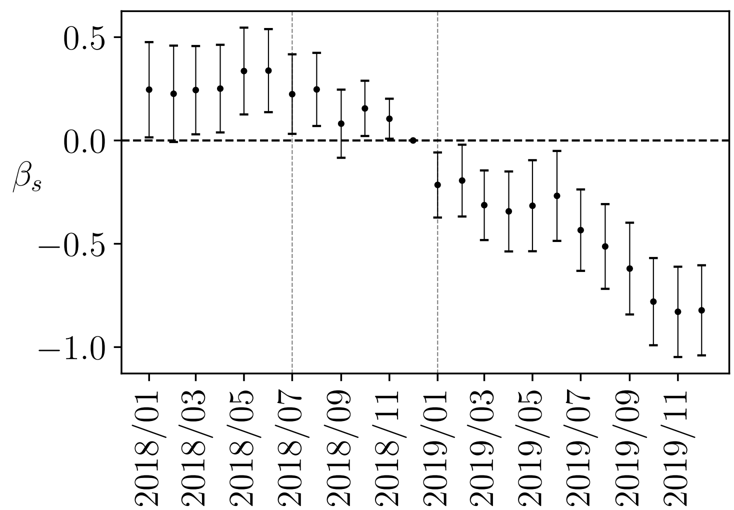

Figure 9. Event study analysis for the asked cleaning fee. The regression includes a linear time trend and listing as well as geographic area-month fixed effects. Standard errors are clustered on the listing level.

Table 9. Superhosts with high EU exposure decrease their fees by more

	(1)	(2)	(3)	(4)
Post-policy	0.362***	1.885***	1.907***	1.928***
	(0.0601)	(0.0681)	(0.0694)	(0.0716)
... × High EU	-0.525***	-0.524***	-0.572***	-0.597***
	(0.0816)	(0.0816)	(0.0850)	(0.0890)
Superhost	0.0573	0.0548	0.0479	0.0291
	(0.107)	(0.107)	(0.107)	(0.112)
... × Post-policy	0.450***	0.451***	0.448***	0.446**
	(0.133)	(0.133)	(0.133)	(0.138)
... × High EU	0.0648	0.0628	0.0812	0.134
	(0.136)	(0.136)	(0.136)	(0.142)
... × Post-policy × High EU	-0.482**	-0.483**	-0.473**	-0.483**
	(0.167)	(0.167)	(0.167)	(0.173)
Listing FEs	✓	✓	✓	✓
Month FE		✓	✓	✓
LAD-month FEs			✓	
LSOA-month FEs				✓
Adj. R^2	0.98	0.98	0.98	0.98
Avg. cleaning fee	33.57	33.57	33.57	33.57
Obs.	1,022,824	1,022,824	1,022,824	1,022,791

Notes: Triple DiD estimates for asked cleaning fees. Standard errors are clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 % level, respectively.

We would expect more professional hosts to exhibit a higher degree of sophistication and, consequently, to be more responsive to the policy intervention. To test this hypothesis, we conduct an additional analysis incorporating a Superhost indicator variable into a triple-differences regression framework. This allows us to assess whether the policy's effects differ systematically between professional and less professional hosts. Table 9 shows that Superhosts with high EU exposure decrease their cleaning fees by more than regular hosts. Non-Superhosts with high EU exposure decrease their cleaning fees by about 0.5 to 0.6 GBP after fee transparency takes effect. Superhosts with high EU exposure decrease their cleaning fees by about 1 to 1.1 GBP. These results show that more professional hosts are more responsive to the policy change in line with them being better able to adjust the fees optimally.

5.3 Robustness Checks

In Appendix E, we also consider additional robustness exercises. First, we show that our main results are robust to an alternative approach in which we first match treatment listings to control listings using propensity score matching before applying the DiD framework (see Table E1). Second, we show that our results are robust to using the booked rather than the asked cleaning fee. Because we are interested in supply-side reactions to changes in fee transparency, we analyze the *asked* cleaning fee in our main results. These fees may differ from the *booked* cleaning fee, which are the average fees for observed bookings. While the asked fees more directly measure supply-side behavior, the booked fees reflect equilibrium effects. We provide results using booked cleaning fees in Appendix E.2, where we see that the results using booked fees are in line with those using asked fees.

Third, we perform a placebo analysis in which we shift the sample one year to include 2017 and 2018. In this placebo exercise, we use January 2018 as the placebo treatment date. Our results suggest that our main results are not driven by end-of-year effects (see Figure E2 and Table E3). Fourth, we provide DiD and event study results using the continuous measure of EU exposure rather than a discretized above-median exposure dummy. The results are consistent with the main cleaning fee results (see Table E4 and Figure E3). Fifth, we employ a different identification strategy. Rather than comparing high-EU-exposure to low-EU-exposure listings in London, we compare listings in London to listings in New York City. Listings in New York City should be less affected by the fee transparency change because they have fewer visitors from the EU and their hosts are likely less aware of the policy change as well. The results in Table E5 show that this is indeed the case. Following the implementation of fee transparency on the EU version of Airbnb, hosts in London tend to decrease their cleaning fees compared to listings in New York City.

Finally, we check for evidence that the cleaning fee is used to induce self-selection by guests based on their length of stay. Recall that the cleaning fee applies per-stay rather than per night. Therefore, a higher cleaning fee and lower base price might make a listing relatively more attractive for guests planning to stay for longer and vice versa. With the cleaning fee becoming more salient, this strategy might become more feasible as guests are now fully aware of the cleaning fee. Therefore, fee transparency might make the cleaning fee a more feasible tool to select guests based on length of stays compared to the alternative tool that hosts have: setting minimum nights requirements. In Table E6, we show results of similar DiD regressions as in the main results, but using the minimum required length of stay as the outcome variable. We do not find evidence that fee transparency has affected the minimum nights requirements of high-EU-exposure hosts. This result suggests that cleaning fees are not being used to screen consumers.

6 Conclusion

With increased scrutiny on hidden fees by regulators and policymakers, assessing how fee transparency impacts market outcomes is pivotal. Most analyses so far have focused on demand-side reactions to hidden fees. Our study provides new insights into the impact of fee transparency on a two-sided peer-to-peer platform where hosts and guests interact, and hosts cannot independently choose to obfuscate add-on fees (e.g., cleaning fees). Instead, whether these fees are shrouded or unshrouded depends on the platform owner's design choices. We find that, even with shrouded fees, many hosts do not impose a cleaning fee, resulting in fully transparent total pricing. To examine how hosts respond to changes in price transparency, we leverage a unique natural experiment: Airbnb's shift to upfront fee disclosure for consumers from the EU following European regulatory pressures.

Our analysis presents several novel findings regarding fee transparency and pricing behavior. Listings whose perceived affordability increases with fee transparency tend to increase prices after transparency is implemented. Generally, it is zero-fee listings who have the largest increase in perceived affordability, although that also depends on the competitors' fees. We propose that the mechanism operates through host inattentiveness to fees when they are shrouded, with hosts realizing true competitor prices after fee transparency is implemented. Supporting this mechanism, we find that affordable Superhosts—characterized by high booking volume, responsiveness, low cancellations, and high ratings—do not adjust their prices after transparency implementation. Additionally, affordable listings in New York City do not adjust their prices, in line with hosts in New York City not being affected by fee transparency in the same way as those in London. This is in line with our mechanism because hosts in New York City would still see prices with obfuscated fees after the transparency change. An alternative mechanism suggests that demand for low-fee listings increases with transparency as other listings now appear more expensive, leading the hosts of these listings to increase prices accordingly. However, this mechanism is not consistent with our findings. First, the price adjustment occurs immediately after the policy change. If the reason for the price increase was a shift in demand due to the policy change, we would expect a lag in which hosts learn that demand has changed. Second, we do not find evidence of a demand increase for more affordable listings after the policy change. In fact, the average number of nights booked decreases by about one night. Third, Superhosts do not adjust their prices despite potentially facing similar demand shifts.

Our analysis also presents several relevant findings about the impact of fee transparency on cleaning fees. Listings with above-median exposure to EU travelers decrease their cleaning fees by 0.6-0.7 GBP following the implementation of transparency requirements. This effect is amplified among Superhosts, who decrease their cleaning fees by approximately 1 GBP. The robustness of our findings is confirmed by an alternative identification comparing listings in London with those in New York City. Listings in London decrease their cleaning fees by 1.2 to 1.4 GBP following the implementation of fee transparency. Overall, our results show robustly that those listings that are most affected by the policy change decrease their cleaning fee compared to others.

These insights highlight that mandatory fee transparency can have unintended effects in peer-to-peer settings with decentralized pricing in which a part of the supply side may be subject to inattention to hidden fees as well. While fee transparency should still increase overall welfare by removing inefficiencies caused by inattentive consumers as well as sellers, our results highlight that fee transparency could have distributional effects. Most likely, inattentive suppliers benefit from being better able to assess competitors' prices. Attentive suppliers in turn might lose out due to not being able to increase profits via fee obfuscation as well as due to inattentive competitors adjusting their pricing. Attentive consumers may have been able to find good deals from inattentive hosts prior to fee transparency, so they could lose out with fee transparency. Finally, inattentive consumers are most likely gaining from fee transparency due to lower search costs and bias when making choices. Overall, our work highlights the potential for such redistributive welfare consequences. However, we leave the quantification of such welfare effects to future work. For policymakers, our results suggest that policies aimed at increasing consumer welfare through fee transparency may need to differentiate between peer-to-peer and centrally controlled platforms to account for these varied responses.

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Appendix

A Data Matching and Imputation of Missing Cleaning Fees

Our price and booking data come from AirDNA and have a daily frequency. Our cleaning fee data are from InsideAirbnb at a monthly frequency. To match these data sets, we first match each daily Airdna observation to the closest date for which we have an InsideAirbnb data scrape (restricting the number of days between the observation and the matched InsideAirbnb scrape to a maximum of 30 days). Next, we aggregate the data to months based on the matched InsideAirbnb date.

There are several reasons why a cleaning fee can be missing after this matching and aggregation procedure. First, even if we have InsideAirbnb data, we may be unable to match a listing we observe in the AirDNA data to the corresponding month in the InsideAirbnb data. Second, even if we can match a listing to a close enough InsideAirbnb scrape, the cleaning fee is missing for many listings in the InsideAirbnb data.

Therefore, we propose several corrections to deal with these issues. For listings for which we sometimes observe a cleaning fee and at other times it is missing, we do the following imputations: First, if the cleaning fee is missing for a listing in some months, but we observe the cleaning fee for months before and after this missing period, and if the cleaning fee is the same before and after the missing period, we impute the missing period with the value of the before and after periods. Second, if we observe a cleaning fee for a listing in some months and it never changes, then we impute the missing months with the value from the observed months. Note that it is only a minor share of listings for which the cleaning fee is missing in some months while it is observed in others. For most listings, we either always observe a cleaning fee or never. Therefore, the third type of imputation is arguably the most relevant in this setting. For listings for which we never observe a cleaning fee in the InsideAirbnb data, we impute a cleaning fee of zero. Figure A1 shows the distribution of the share of observations for each listing for which we observe a cleaning fee. The figure shows that for most listings, we either always observe a cleaning fee or never.

Note that the data do include observations for which we observe an explicit cleaning fee of zero. For listings on Airbnb, the default setting is that there is no cleaning fee. However, hosts can manually set cleaning fees in their listing's settings. We interpret an explicit zero cleaning fee as a host having explicitly entered a cleaning fee of zero, while a missing cleaning fee is a host who has not changed the default of not having a cleaning fee.

When we compare some observable characteristics between listings for which we never observe a clean-

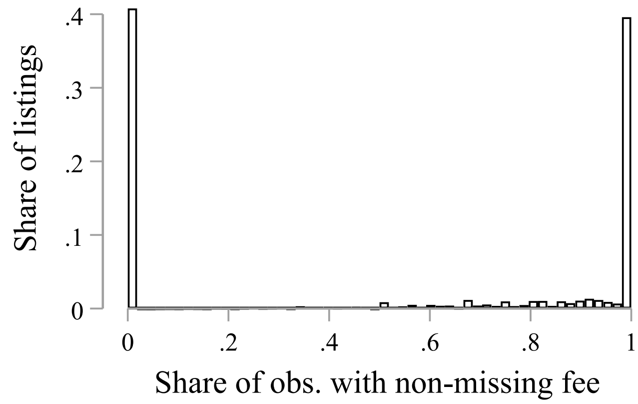

Figure A1. Share of observations with non-missing cleaning fee by listing

Table A1. Balance tables between listings for which we never observe a cleaning fees and the rest

	0% obs. fees			100% obs. fees			
London	N	Mean	SD	N	Mean	SD	Diff
Price (USD)	627,309	86.49	188.24	1,468,726	119.68	184.39	33.185***
Reviews	627,309	10.42	31.13	1,468,726	15.66	30.15	5.244***
Min. nights	627,309	3.89	25.71	1,468,726	3.62	16.75	-0.276***
Superhost	626,443	0.07	0.26	1,466,719	0.14	0.34	0.063***
Instant bookable	627,309	0.33	0.47	1,468,726	0.37	0.48	0.036***
Host listings	627,309	4.76	18.85	1,468,726	30.36	143.58	25.602***
Entire home/apt	627,309	0.32	0.47	1,468,726	0.65	0.48	0.326***
Price (USD, booked only)	232,551	108.70	129.35	617,360	157.64	151.58	48.939***
Price (USD, asked only)	847,745	144.02	415.76	996,947	165.97	285.56	21.944***
EU share	891,415	0.61	0.11	1,092,114	0.58	0.11	-0.029***
Booking length	232,554	6.24	6.06	617,360	5.96	5.11	-0.279***
Booked days	892,214	4.45	11.54	1,092,336	11.12	16.31	6.668***
Bookings	892,214	1.13	3.07	1,092,336	2.84	4.25	1.708***

ing fee to those for which we always observe one in Table A1, it seems like the ones for which we do observe a cleaning fee are more professional: They charge higher prices, have more reviews, are more likely to be instant bookable as well as hosted by Superhosts and hosts with multiple listings, are booked more frequently, and are more likely to be entire homes rather than private rooms. We think it is plausible that less professional hosts are more likely to keep the default setting of having no cleaning fee which is then not shown on the Airbnb website and appears as missing in the InsideAirbnb data.

B Additional Descriptive Statistics

As shown in Figure 2, a large share of listings (27%) always have a cleaning fee of zero.[31] In Section 3, we show that these zero-fee listings appear less professional with respect to many observable characteristics. In this appendix, we provide some more descriptive and correlational evidence to show how zero-fee and positive-fee listings differ.

Zero-fee listings are not uniformly distributed across the city. Figure B1 maps the share of listings that never charge a cleaning fee by Lower layer Super Output Areas (LSOA). The share is lower in the city center, indicating that listings in more central and touristic areas are more likely to levy a cleaning fee. However, the map also shows a large degree of heterogeneity with neighboring LSOAs having potentially very different shares of zero-fee listings.

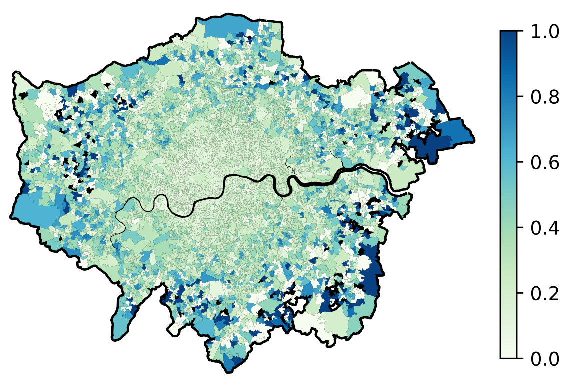

Figure B1. Share of listings without cleaning fees by LSOA

Comparing Similar Zero-fee and Positive-fee Listings Table 2 shows some important descriptive differences between zero-fee listings and positive-fee listings. Focusing on the difference in prices and performance, a part of that could be explained by the fact that positive-fee listings are on average different in many dimensions. For example, consumers likely have a higher willingness-to-pay for an entire home rather than a private room.

To more meaningfully compare the differences in prices and outcomes, Table B1 provides estimation results from linear regressions in which we regress different outcome variables on a dummy for positive-fee

[31]This evidence is consistent with an official Airbnb statement in 2021, which noted that "among active Airbnb listings worldwide, 45 percent do not charge a cleaning fee. For listings that do charge a cleaning fee, the fee averages less than 10 percent of the total cost of the reservation." For the full statement, see: https://news.airbnb.com/fee-transparency-on-airbnb/ (last accessed:October 22, 2025).

listings as well as a range of other characteristics for which we believe that they affect demand. We also include LAD-month fixed effects.

The results show that positive-fee listings set base prices that are approximately 9 GBP lower, all else equal. Factoring in the cleaning fee reduces this difference to only about 1.7 GBP.[32] However, when focusing on booked prices, the booked base prices of comparable zero-fee and positive-fee listings are not statistically significantly different. Consequently, factoring in the additional cleaning fee for positive-fee listings, the booked total price of positive-fee listings is actually about 7 GBP higher than that of zero-fee listings. This pattern is in line with consumers comparing listings based on the base price rather than the total price. Such a behavior would explain why similar listings are booked at similar base prices rather than similar total prices.

Moreover, even when controlling for these characteristics and fixed effects, positive-fee listings have approximately 0.3 more bookings per month, are booked for an additional half-day if they are booked, and for 2.7 days more per month on average. Positive-fee listings earn revenues that are on average 376 GBP higher per month than those of similar zero-fee listings. These differences in bookings and revenues are qualitatively similar to the unconditional differences shown in Table 2. However, controlling for the characteristics and fixed effects reduces the differences substantially, suggesting that a large part of the differences can be explained by differences in observable characteristics between zero-fee and positive-fee listings.

[32] To calculate the asked total price, we divide the asked cleaning fee by the average length of stay for each listing.

Table B1. Price & quantity regressions comparing zero- and positive-fee listings (before EU announcement)

	Asked base price	Asked total price	Booked base price	Booked total price	Bookings	Booked days	Booking length	Revenue
Pos. fee	-8.809***	-1.660**	-1.390	7.335***	0.322***	2.535***	0.443***	398.8***
	(0.537)	(0.551)	(1.421)	(1.434)	(0.0256)	(0.101)	(0.0354)	(31.35)
Entire home	75.48***	80.48***	83.50***	90.04***	0.602***	3.038***	0.0889**	1055.9***
	(0.478)	(0.490)	(1.223)	(1.234)	(0.0242)	(0.0954)	(0.0334)	(29.69)
Reviews	-0.155***	-0.157***	-0.104***	-0.108***	0.0218***	0.0518***	-0.00974***	2.446***
	(0.00619)	(0.00634)	(0.0142)	(0.0144)	(0.000296)	(0.00117)	(0.000356)	(0.363)
Superhost	11.12***	11.25***	12.64***	13.02***	0.0426	1.312***	0.000399	252.3***
	(0.614)	(0.629)	(1.487)	(1.501)	(0.0293)	(0.115)	(0.0371)	(35.86)
Overall rating	0.379***	0.371***	0.571***	0.576***	-0.00174	0.0571***	0.0375***	9.328***
	(0.0329)	(0.0337)	(0.0911)	(0.0919)	(0.00157)	(0.00618)	(0.00228)	(1.924)
Cleanliness rating	1.063***	1.203***	2.602**	2.835***	0.133***	0.235***	-0.199***	62.67***
	(0.296)	(0.303)	(0.812)	(0.819)	(0.0141)	(0.0554)	(0.0202)	(17.25)
Multihost	4.772***	5.569***	7.181**	7.681***	0.944***	2.683***	-0.280***	390.5***
	(0.850)	(0.871)	(2.269)	(2.289)	(0.0405)	(0.159)	(0.0565)	(49.62)
Host listings	0.0374***	0.0411***	0.0694***	0.0658***	0.00271***	0.0128***	0.000692	4.334***
	(0.00810)	(0.00830)	(0.0185)	(0.0187)	(0.000385)	(0.00152)	(0.000461)	(0.472)
Asked price					-0.00404***	-0.0166***	-0.00167***	4.179***
					(0.000110)	(0.000433)	(0.000186)	(0.135)
Observations	188,023	188,023	127,175	127,029	188,023	188,023	127,175	188,023

Notes: Sample includes all listings that either always or never set a positive cleaning fee. We restrict observations to those from 2018 to exclude effects from the transparency change. Linear regressions include LAD-month fixed effects. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

Reputation Backlash to Rationalize Zero Fees? One possible reason why Airbnb hosts might be reluctant to set a cleaning fee is that they may be worried that this could result in backlash by the consumers. In particular, having cleaning fees may result in consumers having higher expectations with respect to cleanliness and giving lower cleanliness ratings in return.[33] To test whether this concern is warranted in our setting, we regress the cleanliness rating on the asked cleaning fee as well as a dummy for zero-fee listings. The results are shown in Table B2. The first column does not include any other controls, the second column includes the shown controls as well as LAD-month fixed effects, and the third column additionally includes listing fixed effects. Note that the dummy variables for zero-fee listings, entire homes, and multihosts do not vary within listing which is why they are excluded in the third column.

Table B2. Explaining the cleanliness rating

	Basic OLS	With Controls + Month-Location FE	Listing + Month-Location FE
Asked cleaning fee	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
100% zero fees	-0.098***	-0.020*	
	(0.016)	(0.010)	
Entire home		0.056***	
		(0.008)	
% Reviews		0.001***	-0.001***
		(0.000)	(0.000)
Superhost		0.137***	0.008
		(0.007)	(0.005)
Overall rating		0.082***	0.066***
		(0.001)	(0.002)
Multihost		0.022**	
		(0.008)	
% Host listings		0.000***	0.000
		(0.000)	(0.000)
Constant	9.256***	1.610***	3.266***
	(0.009)	(0.077)	(0.173)
Observations	193,965	193,873	144,672
Listing Fixed Effects	No	No	Yes
LAD-Month Fixed Effects	No	Yes	Yes
Average cleanliness rating	9.24	9.24	9.26

Notes: As locations for location-month fixed effects, we use Local Authority Districts. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

The results suggest that a higher cleaning fee is not associated with a lower cleanliness rating. Specifically listings that never set a cleaning fee have slightly worse cleanliness ratings on average. If there was significant backlash against a positive cleaning fee, this coefficient should be positive, i.e. zero-fee listings should be rewarded by having better cleanliness ratings.

[33] Carnehl et al. (2025) show that increasing the base price results in lower overall and value-for-money ratings on Airbnb. A similar mechanism could exist for the cleaning fee and the cleanliness rating.

C Prices for zero-fee vs others: Event studies

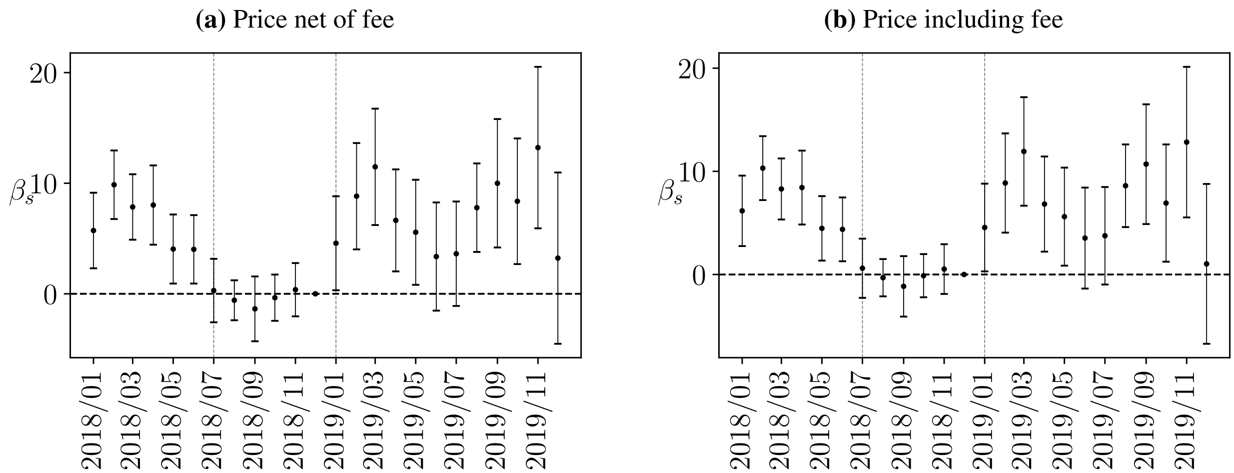

Figure C1. Event study for asked prices corresponding to columns (3) and (7) of Table 3. Includes linear time trend and listing as well as LAD-month FEs. Standard errors clustered on listing level. Bars show 95% confidence intervals.

D Price Analysis: Other Robustness

In this appendix, we present some additional analyses related to our results in Section 4.3.

D.1 Using different control groups

In our main specification, we use the second and third quartile of the δ_i distribution as the control group, the fourth quartile as the treatment group, and exclude the first quartile due to the concerns described in Section 4.2. However, using different control groups does not materially affect our results. Here, we show results in which we use the first three quartiles of the δ_i distribution as the control group as well as results in which we use each of the first three quartiles separately as our control. Furthermore, we provide results in which we split the sample into tertiles as well as quintiles of the δ_i distribution. Then, we use the highest quantile as the treatment group and each of the lower quantiles as the control separately. For brevity, we only show the results using LSOA-month fixed effects and the asked price net of the fee as the outcome variable. However, as with the main results, the choice of fixed effects and whether to analyze fee-inclusive or -exclusive prices does not materially affect our results.

Table D1. DiD results for asked net prices with different control groups

Sample split	Quartiles				Tertiles		Quintiles			
Control group (quantiles)	1+2+3	1	2	3	1	2	1	2	3	4
Post-policy	6.529***	9.526***	4.158***	6.029***	7.968***	5.131***	10.10***	4.739***	5.770***	5.243***
	(0.493)	(1.162)	(0.421)	(0.904)	(0.884)	(0.673)	(1.375)	(0.646)	(1.132)	(0.625)
... × "More affordable"	7.904***	5.276**	9.500***	7.666***	4.387**	6.409***	7.103**	11.49***	10.05***	10.85***
	(1.704)	(2.019)	(1.670)	(1.853)	(1.523)	(1.392)	(2.458)	(2.091)	(2.290)	(2.080)
Constant	115.5***	146.8***	106.4***	112.2***	132.4***	102.5***	159.6***	113.9***	111.2***	120.5***
	(0.251)	(0.419)	(0.343)	(0.379)	(0.315)	(0.285)	(0.504)	(0.433)	(0.466)	(0.425)
Adj. R^2	0.845	0.634	0.683	0.756	0.717	0.763	0.705	0.561	0.636	0.792
Avg. price	119.5	151.7	110.0	116.2	136.6	105.9	165.2	118.1	115.6	124.8
Obs.	1,030,459	425,052	425,863	424,170	569,378	569,821	338,551	338,118	338,002	338,006

Notes: Samples include the control group and the treatment group. The treatment group is always the highest quantile of the δ_i distribution and labeled as "More affordable". All specifications include LSOA-month and listing fixed effects. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

D.2 The Effect on Booked Prices

These results correspond to the ones reported in Section 4.3 except that we use booked prices rather than asked prices. Table D2 presents the DiD estimates. The effect of fee transparency on prices is qualitatively similar to the one in the main analysis, but the effect sizes are smaller. This result suggests that consumers react to increased asked prices by booking those listings less.

Table D2. DiD results for booked prices

	Outcome: Price net of fee				Outcome: Price including fee			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-policy	-8.133*	2.842***	2.703***	2.080***	-8.327*	4.857***	4.712***	4.083***
	(2.736)	(0.304)	(0.307)	(0.416)	(2.731)	(0.298)	(0.300)	(0.411)
... × "More affordable"	3.655***	3.531***	4.002***	3.287*	4.006***	3.891***	4.390***	3.681**
	(0.443)	(0.438)	(0.477)	(1.002)	(0.434)	(0.428)	(0.467)	(0.996)
Linear time trend	0.941**				1.123**			
	(0.262)				(0.261)			
Constant	56.30***	96.35***	96.35***	97.46***	54.22***	102.0***	102.0***	103.2***
	(11.28)	(0.129)	(0.141)	(0.185)	(11.26)	(0.127)	(0.138)	(0.183)
Listing FEs	✓	✓	✓	✓	✓	✓	✓	✓
Month FE		✓	✓	✓		✓	✓	✓
LAD-month FEs			✓				✓	
LSOA-month FEs				✓				✓
Adj. R^2	0.07	0.07	0.07	-0.01	0.07	0.07	0.07	-0.01
Avg. price	98.03	98.03	98.03	98.79	104.60	104.60	104.60	105.41
Obs.	393,619	393,619	393,619	386,705	393,100	393,100	393,100	386,173

Notes: Sample consists of quartiles 2, 3, and 4 of the δ_i distribution. Quartile 4 ("more affordable") is the treatment group. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

D.3 Placebo Analysis

We choose our sample period to include one year before and one year after the policy change. Because our treatment date is January 2019, this means that it would be difficult to disentangle treatment effects from possible end-of-year effects. To show that our effects are not driven by such end-of-year effects between the treatment and control group, we conduct the same analysis in a placebo setting in which we shift the sample period to 2017 and 2018 and define the treatment month as January 2018.

Table D3 shows the results of this placebo exercise. The results show that there is no detectable change between prices of "more affordable" listings and others after January 2018 compared to before. This zero result suggests that our results in Table 4 are not driven by end-of-year effects.

Table D3. Placebo results

	Outcome: Price net of fee				Outcome: Price including fee			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-policy (Placebo)	-7.706***	3.513***	3.515***	3.573***	-7.742***	3.645***	3.647***	3.707***
	(0.534)	(0.405)	(0.405)	(0.421)	(0.534)	(0.405)	(0.405)	(0.421)
... × "More affordable"	0.707	0.705	0.843	0.844	0.616	0.614	0.751	0.752
	(0.618)	(0.619)	(0.617)	(0.630)	(0.618)	(0.619)	(0.617)	(0.630)
Linear time trend	0.948***				0.963***			
	(0.0363)				(0.0363)			
Constant	67.11***	95.92***	95.90***	95.95***	70.31***	99.56***	99.53***	99.59***
	(1.170)	(0.183)	(0.183)	(0.189)	(1.170)	(0.183)	(0.183)	(0.189)
Listing FEs	✓	✓	✓	✓	✓	✓	✓	✓
Month FE		✓	✓	✓		✓	✓	✓
LAD-month FEs			✓				✓	
LSOA-month FEs				✓				✓
Adj. R^2	0.92	0.92	0.92	0.91	0.92	0.92	0.92	0.91
Avg. total price	98.11	98.11	98.11	98.19	101.80	101.80	101.80	101.89
Obs.	644,338	644,338	644,338	642,522	644,338	644,338	644,338	642,522

Notes: 2017-2018 sample. Policy date defined as January 2018. We restrict the analysis to quartiles 2, 3, and 4 of the δ_i distribution. Quartile 4 ("more affordable") is the treatment group. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

D.4 Price analysis: Minimum nights of one

This section presents the results of an analysis similar to that in Section 4.3, but with the sample restricted to listings that require a minimum stay of only one night. Under this restriction, we do not adjust the cleaning fee when calculating the total price, thereby analyzing total prices applicable exclusively to one-night stays. Apart from these modifications, the calculation of δ_i follows the approach outlined in Section 4.2.

Figure D1 displays the resulting δ_i distribution for the entire sample, as well as for listings that never impose a cleaning fee. The observed patterns are consistent with those in Figure 3. Specifically, for the entire sample, the distribution of δ_i remains centered around zero, while δ_i is positive for most zero-fee listings.

Once again, we define as "more affordable" listings all those listings whose δ_i is above the 75th percentile (9). The control group consists of listings in the second and third quartile. Figure D2 reports the average prices (both excluding and including the cleaning fee) over time for the treatment group compared to the first three quartiles of the δ_i distribution. The figure shows that prior to the policy change, net prices

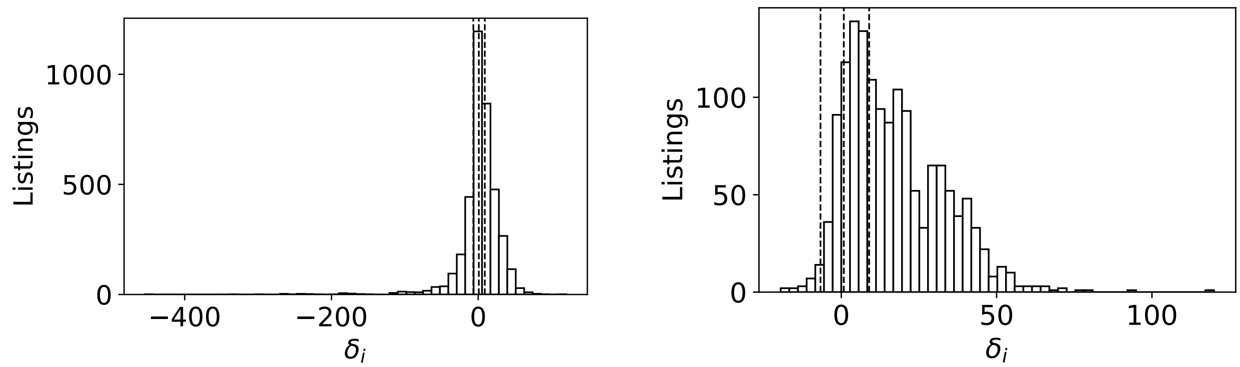

Figure D1. Distribution of δ_i for all listings (left) and for zero fees only (right)

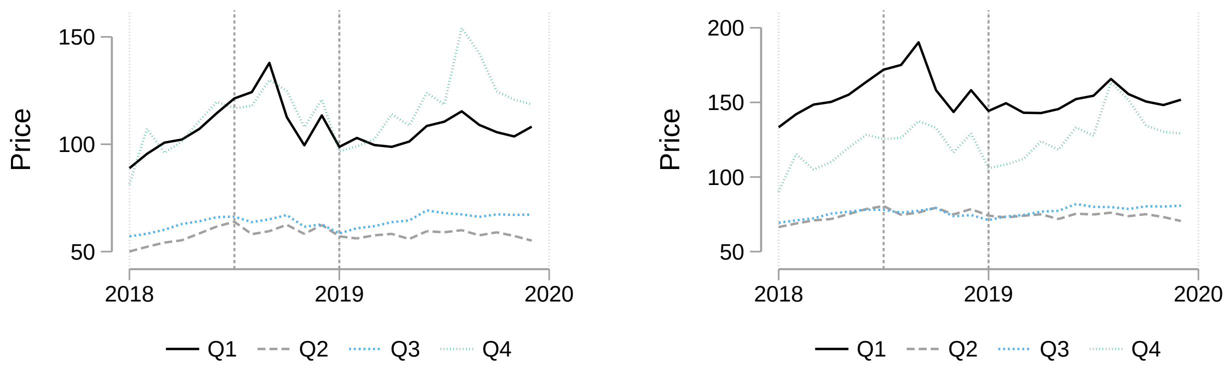

Figure D2. Prices net of fee (left) and including fee (right) over time by high and low δ_i

of listings in the first (Q1) and the last quartile (Q4) of the δ_i-distribution behave very similarly and start to diverge after the policy change. In particular, Q4 listings become more expensive in terms of net prices after the policy change. However, accounting for the fee reveals that the total price was generally lower for Q4 listings compared to the Q1 before the policy change but began to converge after the policy change. The listings in quartiles 2 (Q2) and 3 (Q3) show lower prices on average and less fluctuation in general as well.

In Table D4, we report the DiD results showing how the prices of "more affordable" listings (Q4) change compared to those of Q2 and Q3 listings after the policy change. The results are qualitatively in line with those reported in Table 4. The event study results shown in Figure D3 show that conditional pre-trends seem to develop in parallel and that prices of "more affordable" listings indeed increased after the policy change.

Table D4. DiD results for prices (min. nights of one)

	Outcome: Price net of fee				Outcome: Price including fee			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-policy	-9.628***	2.213***	2.115***	2.200***	-9.672***	3.078***	2.977***	3.059***
	(0.553)	(0.454)	(0.455)	(0.486)	(0.553)	(0.454)	(0.455)	(0.486)
... × "More affordable"	5.499***	5.415***	5.822***	5.907***	5.660***	5.577***	5.990***	6.092***
	(1.694)	(1.694)	(1.681)	(1.677)	(1.694)	(1.694)	(1.681)	(1.677)
Linear time trend	0.995***				1.071***			
	(0.0438)				(0.0438)			
Constant	34.76***	76.88***	76.87***	77.39***	43.78***	89.10***	89.09***	89.75***
	(1.917)	(0.253)	(0.254)	(0.255)	(1.917)	(0.253)	(0.254)	(0.255)
Listing FEs	✓	✓	✓	✓	✓	✓	✓	✓
Month FE		✓	✓	✓		✓	✓	✓
LAD-month FEs			✓				✓	
LSOA-month FEs				✓				✓
Adj. R^2	0.83	0.83	0.83	0.80	0.83	0.83	0.83	0.81
Avg. price	78.52	78.52	78.52	79.10	91.12	91.12	91.12	91.85
Obs.	126,193	126,193	126,193	121,363	126,193	126,193	126,193	121,363

Notes: Sample consists of quartiles 2, 3, and 4 of the δ_i distribution using listings with a minimum booking length of one night only. Quartile 4 ("more affordable") is the treatment group. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

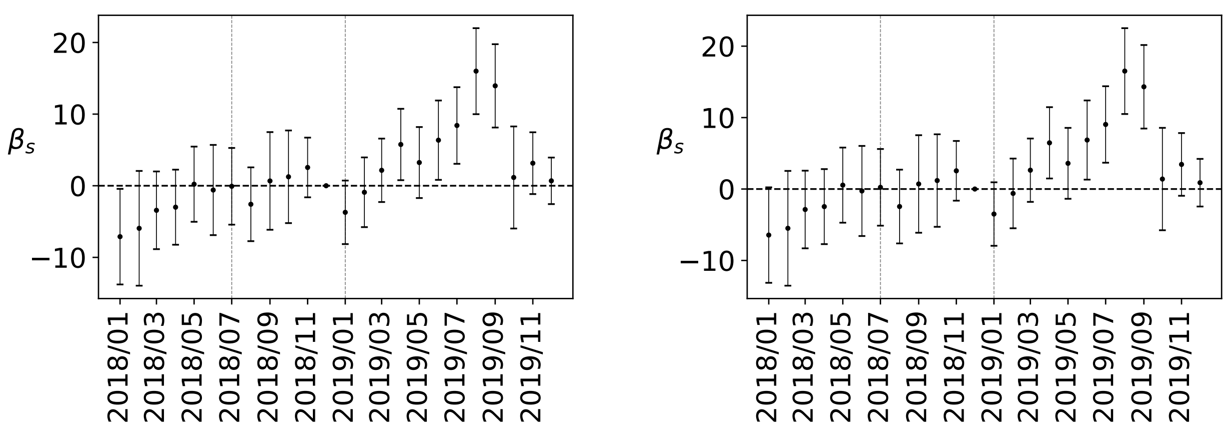

Figure D3. Event study for asked prices corresponding to columns (4) and (8) of Table D4. Includes linear time trend and listing as well as LSOA-month FEs. Standard errors clustered on listing level. Bars show 95% confidence intervals.

E Cleaning Fee Analysis: Other Robustness

In this appendix, we present some additional results related to those in Section 5.

E.1 Propensity Score Matching

In this robustness exercise, we conduct the same DiD analysis, but we match treatment listings to control listings using propensity score matching first. As variables for the propensity score matching, we use an entire home dummy, the number of reviews, a Superhost dummy, the overall rating, the cleanliness rating, a multihost dummy, and the number of listings hosted by the same host. Table E1 shows the results from this matching regression. The results are very similar to those reported in Table 8.

Table E1. DiD for cleaning fees using PSM

	(1)	(2)	(3)
Post-policy	2.105***	2.125***	2.141***
	(0.0705)	(0.0722)	(0.0750)
... × High EU	-0.629***	-0.671***	-0.689***
	(0.0844)	(0.0882)	(0.0928)
Constant	30.78***	30.79***	30.86***
	(0.0190)	(0.0190)	(0.0195)
Listing FEs	✓	✓	✓
Month FE	✓	✓	✓
LAD-month FEs		✓	
LSOA-month FEs			✓
Adj. R^2	0.98	0.98	0.98
Avg. cleaning fee	31.53	31.53	31.61
Obs.	652,027	652,027	648,893
Treated units	340,090	340,090	340,090
Control units	311,937	311,937	311,937

Notes: DiD estimates for asked cleaning fees. Control group based on PSM. Standard errors are clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

E.2 The Effect on Booked Cleaning Fees

These results correspond to the ones reported in Section 5 except that we use the booked cleaning fee rather than the asked cleaning fee. Table E2 presents the DiD estimates. The effect of fee transparency on the cleaning fee is qualitatively similar to the one in the main analysis. Figure E1 presents the event study analysis and shows that the effect of the policy is significant and negative. Moreover, also in this case, it seems that there are some anticipation effects just after Airbnb commits to fully complying with European regulators' demands. This can be due to anticipation by hosts or due to platform design experiments on Airbnb that affected a subset of hosts.

Table E2. DiD for booked cleaning fees

London	(1)	(2)	(3)	(4)
Post-policy	0.422***	1.858***	1.878***	1.896***
	(0.0476)	(0.0581)	(0.0597)	(0.0617)
... × High EU	-0.558***	-0.558***	-0.599***	-0.622***
	(0.0686)	(0.0686)	(0.0724)	(0.0761)
Linear time trend	0.120***	0	0	0
	(0.00331)	(.)	(.)	(.)
Constant	27.45***	32.53***	32.53***	32.53***
	(0.155)	(0.0194)	(0.0194)	(0.0197)
Listing FEs	✓	✓	✓	✓
Month FE		✓	✓	✓
LAD-month FEs			✓	
LSOA-month FEs				✓
Adj. R^2	0.99	0.99	0.99	0.98
Avg. cleaning fee	33.34	33.34	33.34	33.34
Obs.	1,021,866	1,021,866	1,021,866	1,021,806

Notes: Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

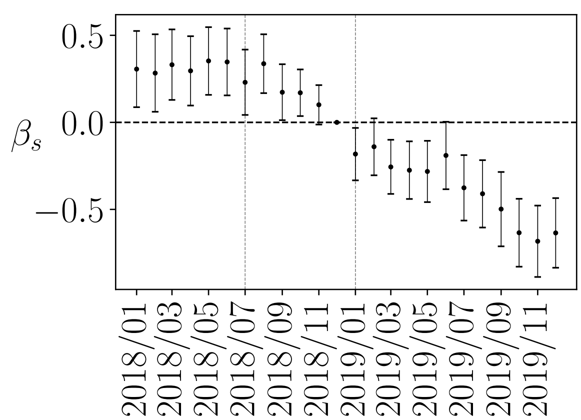

Figure E1. Event study analysis for the booked cleaning fee. The regressions include listing as well as LAD-month fixed effects. Standard errors are clustered on the listing level.

E.3 Placebo Treatment

We consider a Placebo analysis in which we shift the sample one year to include 2017 and 2018. We define the treatment date as January 2018. Then, we conduct the same analysis as reported in Table 8. Table E3 shows the results from the DiD regression using this Placebo set-up. The results suggest a small decrease in fees by high-EU-exposure listings after January 2018. However, when considering the corresponding event study regression (Figure E2), we see that there are substantial pre-trends which likely drive these results in the DiD results. Overall, these Placebo results suggest that our main results are not driven by end-of-year effects.

Table E3. Placebo analyis for cleaning fees

	(1)	(2)	(3)	(4)
Post-policy (Placebo)	-0.0682	1.177***	1.195***	1.206***
	(0.0525)	(0.0651)	(0.0666)	(0.0697)
... × High EU	-0.377***	-0.377***	-0.408***	-0.427***
	(0.0681)	(0.0681)	(0.0708)	(0.0748)
Linear time trend	0.105***	0	0	0
	(0.00325)	(.)	(.)	(.)
Constant	26.26***	29.46***	29.46***	29.45***
	(0.0756)	(0.0221)	(0.0221)	(0.0227)
Listing FEs	✓	✓	✓	✓
Month FE		✓	✓	✓
Large geo-month FEs			✓	
Small geo-month FEs				✓
Adj. R^2	0.99	0.99	0.99	0.99
Avg. cleaning fee	30.04	30.04	30.04	30.04
Obs.	861,057	861,057	861,057	861,057

Notes: DiD estimates for asked cleaning fees. 2017-2018 sample. Policy period is defined as January 2018. Standard errors are clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

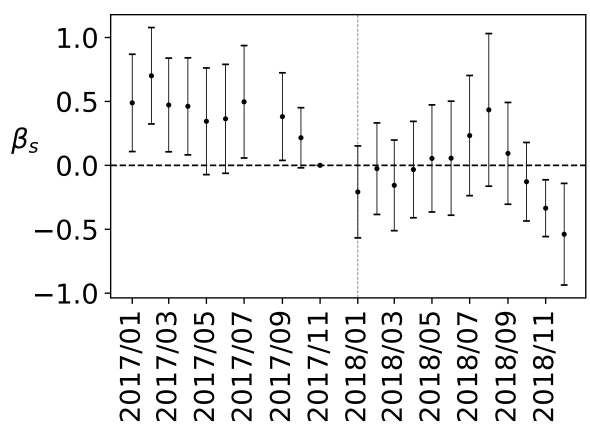

Figure E2. Event study analysis for the asked cleaning fee. 2017-2018 sample. Base period is November 2017. The regression includes a linear time trend and listing as well as LSOA-month fixed effects. Standard errors are clustered on the listing level.

E.4 Continuous EU Exposure Measure

Our measure of exposure to EU travelers is a share between 0 and 1. In our main analysis, we discretize the measure and define the treatment group as having above-median exposure to EU travelers. This discretization results in a coefficient that is easier to interpret. Here, we report a DiD regression using the same set-up as in the results in Table 8, except that we use the continuous measure of EU exposure as the treatment variable. Figure E3 shows the event study results corresponding to column (3) of Table E4. The results are qualitatively in line with our main cleaning fee results. The results suggest that a listing with 100% EU exposure decreases their cleaning fees following the implementation of fee transparency by 3 GBP compared to a listing with 0% EU exposure.

Table E4. Cleaning fee analysis with continuous EU exposure

	(1)	(2)	(3)	(4)
Post-policy	1.784***	3.309***	3.454***	3.490***
	(0.226)	(0.234)	(0.249)	(0.259)
... × EU share	-2.777***	-2.774***	-3.017***	-3.065***
	(0.366)	(0.366)	(0.390)	(0.408)
Linear time trend	0.127***			
	(0.00342)			
Constant	27.29***	32.69***	32.69***	32.69***
	(0.159)	(0.0203)	(0.0204)	(0.0207)
Listing FEs	✓	✓	✓	✓
Month FE		✓	✓	✓
Large geo-month FEs			✓	
Small geo-month FEs				✓
Adj. R^2	0.98	0.98	0.98	0.98
Avg. total price	33.53	33.53	33.53	33.53
Obs.	1,030,459	1,030,459	1,030,459	1,030,459

Notes: DiD estimates for asked cleaning fees. Standard errors are clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

E.5 London vs New York City

One concern in our design is that listings in the treatment and control groups may compete with each other. If that is the case, a competitor's treatment status could influence a focal listing's behavior, potentially violating the Stable Unit Treatment Value Assumption (SUTVA). Arguably, such spillovers are more likely to affect pricing decisions, which may indeed be affected by competitors' actions. Instead, the decision to set or adjust a cleaning fee is likely driven primarily by listing-specific factors, such as actual cleaning costs and the listing's own demand (e.g., exposure to EU travelers). In addition, information about the magnitude of competitors' cleaning fees remains relatively opaque even after the policy change. Although hosts can

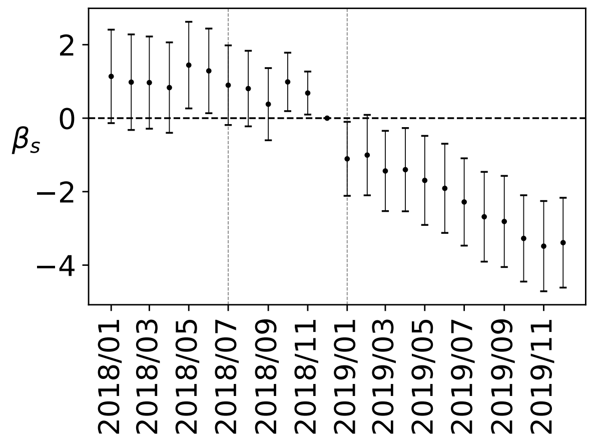

Figure E3. Event study analysis for the asked cleaning fee using the continuous measure of EU exposure. The regressions include listing as well as LAD-month fixed effects. Standard errors are clustered on the listing level.

now easily see the total price per night inclusive of the fee, in order to see the decomposition of that price into nightly base price and fees, hosts would still need to browse through their competitors' listing pages (see Figure 1).

In this robustness check, we provide additional evidence that relies on a different identification strategy. Instead of defining control and treatment based on listing-level exposure to EU travelers, we compare listings in London to New York City. The idea behind this strategy is that listings in London should be exposed to more travelers from the EU and hosts in London are likely more attentive to the policy change as well. Listings in London are not competing with listings in New York City, and the violation of SUTVA is less of a concern with this approach. The drawback of this identification strategy is that the treatment now applies on the city level. This makes it more difficult to account for city-level trends.

We estimate a similar regression as for the results shown in Table 8. The main difference is that we now define the treatment variable not based on the share of nearby reviews from EU travelers, but instead the treatment variable is equal to one for listings in London and zero for those in New York City. Table E5 presents the results for cleaning fees. Following the policy, London listings reduce their cleaning fees by about 1.2 to 1.4 GBP relative to those in New York, equivalent to roughly 3% of the average positive-fee listing in London (46 GBP; see Table 1). These estimates are consistent with our main findings in Table 8.

E.6 Cleaning Fee as a Screening Device

One important feature of the cleaning fee on Airbnb is that it applies per booking whereas the base price applies per night. As such, hosts on Airbnb can essentially set two-part tariffs and use the cleaning fee

Table E5. Listings in London decrease their fees compared to listings in NYC

	(1)	(2)	(3)	(4)
Post-policy	0.670***	2.937***	-0.988***	3.096***
	(0.0626)	(0.0744)	(0.151)	(0.0801)
... × London	-1.192***	-1.192***	-1.446***	-1.430***
	(0.0805)	(0.0805)	(0.0883)	(0.0896)
Listing FEs	✓	✓	✓	✓
Month FE		✓	✓	✓
LAD/Borough-month FEs			✓	
LSOA/NTA-month FEs				✓
Adj. R^2	0.98	0.98	0.98	0.98
Avg. cleaning fee	43.54	43.54	43.54	43.62
Obs.	1,633,621	1,633,621	1,633,621	1,629,369

Notes: Includes constant and linear trend. In specifications with fixed effects by larger geography-months, we use LADs in London and boroughs in New York City as the geographical units. In the specifications with smaller geography-month fixed effects, we use the LSOA in London and Neighborhood Tabulation Areas (NTA) for New York City. Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.

to price discriminate between travelers based on the length of their stay. Price transparency can alter this strategy: by making the cleaning fee more salient, it becomes a more effective tool for screening travelers ex ante based on their desired length of stay. As a result, the minimum-night constraints may be relaxed, and hosts might find it optimal to reduce the minimum required length of stay instead.

To test for this usage of the cleaning fee, we perform the same analysis as reported in Table 8, but use the minimum required length of stay as an outcome variable. That means we analyze how listings with above-median exposure to EU travelers changed their required minimum nights for bookings after the transparency change compared to listings with below-median exposure to EU travelers. Table E6 reports the results of the DiD regression. There is no evidence that the transparency change affects the required minimum nights on average. This null result suggests that hosts do not use the cleaning fee to screen consumers based on their desired length of stay. This finding further supports previous evidence that hosts reacted to the policy by only slightly reducing the cleaning fee and is in line with the hypothesis that the cleaning fee is mainly used to cover cleaning costs or increase revenue, rather than as a screening tool.

Table E6. DiD for minimum nights

	(1)	(2)	(3)	(4)
Post-policy	-0.0623	0.479***	0.479***	0.476***
	(0.0647)	(0.0511)	(0.0513)	(0.0526)
... × High EU	0.163	0.162	0.166	0.161
	(0.107)	(0.107)	(0.107)	(0.109)
Linear time trend	0.0456***			
	(0.00379)			
Constant	0.535**	2.465***	2.465***	2.471***
	(0.169)	(0.0204)	(0.0205)	(0.0212)
Listing FEs	✓	✓	✓	✓
Month FE		✓	✓	✓
Large geo-month FEs			✓	
Small geo-month FEs				✓
Adj. R^2	0.26	0.26	0.27	0.25
Avg. min. nights	2.69	2.69	2.69	2.70
Obs.	506,898	506,898	506,898	500,690

Notes: Standard errors clustered on the listing level. ***, **, * indicate statistical significance at the five, one, and 0.1 percent level, respectively.