Clearing the Fog: Price Transparency, Market Churn, and Consumer Welfare in Short-Term Rentals*

Ginger Zhe Jin
Boston University & NBER

Liad Wagman
Rensselaer Polytechnic Institute

Jingyi Xing
University of Maryland

August 9, 2026

Abstract

We study California's short-term lodging price transparency law (AB 537), which from July 1, 2024 required Airbnb and other providers to advertise fee-inclusive prices. Using Coarsened Exact Matching to compare Airbnb listings in San Diego and San Francisco with those in Seattle and Chicago, we find that all-in pricing made guests more price-sensitive relative to control cities; yet, contrary to the standard prediction that transparency lowers prices, average daily rates rose by 4.8 percent. We also observe significant market churn: relative to control cities, treated cities saw more exits and entries, listings reliant on cleaning-fee revenue disproportionately exited, higher-quality listings were more likely to stay, and entrants offered lower fees and lower quality than incumbents. A discrete-choice welfare analysis shows that the law removed a sizable "welfare illusion" created by obscured fees and, largely through market churn, raised realized consumer surplus. The net gains, after accounting for price increases, were positive on average but unevenly distributed across treated ZIP codes.

Keywords: all-in pricing; drip pricing; partitioned pricing; price transparency; short-term rentals; Airbnb; consumer welfare; market churn; entry and exit.

JEL Codes: D12, D18, D83, L15, L85, K20, R31

*Jin: Boston University, Questrom School of Business & NBER. Email: gzjin@bu.edu. Wagman: Rensselaer Polytechnic Institute, Lally School of Management. Email: wagman@rpi.edu. Xing: University of Maryland, Department of Economics. Email: jiyxing@umd.edu. We are thankful to the University of Maryland for the generous funding for data acquisition. James C. Cooper, John Yun, Gus Hurwitz, and participants at the ICLE work in progress workshop have provided constructive comments. All errors are ours, all rights reserved.

1 Introduction

Non-transparent pricing is pervasive in consumer markets. Firms often advertise a low headline price while disclosing mandatory fees only later at checkout (drip pricing), quote a base price alongside separate mandatory surcharges rather than a single all-inclusive total (partitioned pricing), or display both the base and total prices but render the total less salient and obscure the fees that account for the difference.

Such concerns have been especially acute in short-term rentals, where platforms such as Airbnb have long highlighted a nightly rate on the search page while cleaning fees, service fees, and other charges appeared in smaller or less salient text, or were disclosed only later in the booking process. In the United States, Airbnb's cleaning fees average more than $150 per booking, making the gap between the advertised and all-in price economically significant.[1] On July 1, 2024, California Assembly Bill 537 (Ch. 805, Stats. 2023, codified at Cal. Bus. & Prof. Code § 17568.6) became operative, requiring all short-term lodging sellers to display a fee-inclusive total price upfront, before taxes.[2] This paper is among the first to study the causal effects of a statewide all-in-pricing mandate in the U.S. short-term rental market, and our findings challenge the conventional wisdom about what price transparency does to equilibrium prices and consumer welfare.[3]

The prevailing view in the drip and partitioned pricing literature is that replacing obscured prices with all-in upfront prices should make consumers more price-sensitive, stimulate competition, and reduce equilibrium prices (Mahoney, 2025; Gabaix and Laibson, 2006). Using Airbnb listing data one year before and one year after AB 537 took effect, we compare listings in San Diego and San Francisco against comparable listings in Seattle and Chicago, which were not subject to a comparable law until the final months of our sample. We employ a difference-in-differences design with Coarsened Exact Matching to account for potential differences in listing characteristics between treated and control cities.

Our finding on consumers' price sensitivity confirms the conventional wisdom: using a discrete choice demand model, we estimate that all-in pricing increased consumers' own-price elasticity by approximately 10 percent in treated California cities relative to control cities. Consistent with the prediction that transparent pricing makes total cost more salient, guests also placed greater weight on listing quality when booking, a pattern that aligns with existing field evidence on price salience and laboratory evidence on attention to quality (Blake et al., 2021; Huck et al., 2016).

Our second and more surprising finding runs counter to the prevailing predictions on equilibrium price. Rather than declining, all-in prices (average daily rates) in California Airbnb listings increased by approximately 4.8 percent relative to control cities following the law. This price increase is not concentrated in any particular segment: listings with relatively high cleaning fees and those in more competitive markets alike raised prices in absolute terms. The finding is robust to whether

[1] See an article by AirDNA (a private company specializing in Airbnb data collection) at https://www.airdna.co/blog/airbnb-cleaning-fees-what-hosts-need-to-know.

[2] See https://leginfo.legislature.ca.gov/faces/codes_displaySection.xhtml?lawCode=BPC&sectionNum=17568.6.

[3] Bradley and Feldman (2020) study a federal airfare display rule in 2012 that concerns taxes rather than seller-set fees and find higher consumer price sensitivity when a tax-inclusive price is advertised.

Coarsened Exact Matching weights are applied.

A natural question is how prices could rise even as consumers became more price-sensitive. This puzzle motivates us to examine market churn on the supply side. In a free-entry market like Airbnb, price transparency can directly shape the exit, entry, and reallocation of activity across listings. In particular, the law may disproportionately erode the profitability of listings that had relied heavily on cleaning fee revenue: by removing a key tool for extracting consumer surplus, improved price transparency lowers these listings' expected returns from remaining on the platform.

Consistent with this logic, we find that listings with one standard deviation higher pre-law cleaning fee reliance faced hazard rates of exit 17 to 24 percent higher in treated than in control cities after the law, depending on the duration model specification. Exit rates in California spiked sharply in the first month and remained elevated thereafter. Entry did not fully offset these exits: total listing supply per ZIP code fell in treated relative to control ZIP codes—though not statistically significantly—with larger declines where cleaning fee reliance had been greater. A scatter plot of ZIP code-level changes confirms that ZIP codes with larger supply contractions also experienced larger price increases. The entrants that did join differed systematically from both incumbents and exiting listings: they charged lower cleaning fees, relied less on cleaning fee revenue, and set lower all-in prices than incumbents, consistent with their lower quality as measured by superhost status, number of reviews, and overall rating.

In light of these changes, we conduct a welfare analysis that quantifies the net effect of price transparency on consumers, accounting for changes in consumer preferences, prices, and market churn. We decompose the change in consumer welfare into three margins. First, price transparency corrects a welfare illusion: price obscurity before the law led consumers to be less sensitive to the final price and thus to overestimate their surplus by approximately $77,359 per ZIP code-month, or 15.86 percent. Second, abstracting from price changes, all-in pricing generates a realized consumer surplus that is $9,324 higher per ZIP code-month, reflecting gains to marginal consumers who are no longer lured into welfare-reducing purchases on Airbnb. Third, the estimated 4.8 percent increase in the fee-inclusive price on Airbnb, together with a parallel 1.8 percent increase on Vrbo, applied to all listings at pre-law demand, would reduce consumer surplus by $67,058 per ZIP code-month—a loss borne more by inframarginal guests who continue booking at higher prices than by guests priced out of Airbnb. Netting these three effects, all-in pricing combined with the estimated price increase would, on average, *reduce* consumer welfare by $57,734 per ZIP code-month.

However, the compositional shift in the listing pool, driven by exits of high-fee listings, stays of higher-quality listings, and entries of lower-fee, lower-quality listings, generates a welfare gain of $143,750 per ZIP code-month, more than offsetting the price-induced losses. Part of this change may be driven by an overall 5.45% expansion in Airbnb and Vrbo booked nights in San Diego and San Francisco. Under the conservative assumption that this expansion is unrelated to the law (because control cities experienced similar expansion) and should therefore be excluded from the estimated welfare gain, the net welfare change remains positive relative to realized consumer surplus before the law (about 11.56 percent higher). As a result, realized consumer surplus after the law is, on

average, higher than under the pre-law regime with non-transparent pricing, although it remains below consumers' perceived surplus (after excluding market expansion effect), which was inflated by the illusion of low upfront prices.

These welfare changes, moreover, are distributed unevenly across ZIP codes. Of the California ZIP codes in our sample, 39 experienced welfare improvements after the law and 15 experienced welfare declines. Relative to welfare-reducing ZIP codes, welfare-increasing ones tend to have a relatively larger decline in cleaning fee reliance and greater increases in average daily rates, listing quality, exits, and the total count of active listings.

Taken together, our findings carry both positive and normative implications. On the positive side, they demonstrate that price transparency laws can trigger supply-side responses—price increases, market churn, and compositional change—that are at least as important as the direct demand-side effects studied in the prior literature. Whether price transparency improves or harms consumers depends critically on whether market churn can more than offset the welfare loss from price increases. On the normative side, our welfare analysis suggests that the average consumer in California benefited from AB 537, but the gains were not evenly distributed, and market churn plays an important role in this heterogeneity.

The paper proceeds as follows. Section 2 reviews the related literature and describes our contributions in more detail. Section 3 describes California's regulatory background and our data. The empirical results are organized in three parts: Section 4 presents reduced-form evidence on listing prices and bookings; Section 5 presents reduced-form evidence on listing supply, including exits, stayers, and entries; and Section 6 estimates a structural demand model and conducts the consumer welfare analysis. Although the change in consumer price elasticity is central to the conventional wisdom about drip and partitioned pricing, we defer its estimation to Section 6 because it is derived from the discrete choice demand model and is one of the most important components of the welfare analysis. Section 7 concludes.

2 Literature Review and Our Contribution

Our work is closely related to a large economics and marketing literature on drip pricing, in which mandatory fees are disclosed only at checkout rather than upfront. Both laboratory experiments and observational studies show that many consumers cannot make informed decisions from the initial price a seller displays and often grow frustrated as the final price climbs (Greenleaf et al., 2016; Abraham and Hamilton, 2018; Hossain and Morgan, 2006; Brown et al., 2010; Santana et al., 2020; Althenayyan and Morwitz, 2026). Sellers and the platforms that intermediate transactions can therefore exploit consumers' incomplete information, search costs, and accumulating frustration to sell more at higher prices (Hossain and Morgan, 2006; Brown et al., 2010; Blake et al., 2021). Seller competition does not necessarily solve the problem, and may even worsen it: a seller can lure consumers from rivals with a low initial price and capture them later, since those who have invested effort in a purchase are unlikely to start over (Gabaix and Laibson, 2006; Ellison and Ellison, 2009).

Consumer-protection concerns about drip pricing prompted the U.S. Federal Trade Commission to hold a conference on its economics as early as 2012,[4] publish a staff report on hotel resort fees in 2017 (Sullivan, 2017), and issue the Rule on Unfair or Deceptive Fees, effective May 12, 2025.[5] Similar concerns motivated the 2009 Credit Card Accountability Responsibility and Disclosure (CARD) Act, which limits certain credit card fees (e.g., over-limit and late fees) and requires monthly statements to display prominently the cost of making only the minimum payment. Agarwal et al. (2015) find that the CARD Act reduced borrowing costs by an annualized 1.6% of average daily balances, with no offsetting rise in interest charges or drop in credit volume, and that disclosing the cost of minimum payments relative to a 36-month payoff modestly raised the share of accounts paying off balances in 36 months.

As Mahoney (2025) summarizes, the prevailing view is that replacing drip pricing with all-in upfront prices should make consumers more price-sensitive and reduce demand, in turn spurring competition and lowering final prices. We highlight a countervailing effect alongside this mechanism: because all-in pricing lowers search costs and intensifies price competition, sellers can no longer rely on non-transparent pricing to extract surplus from captive consumers and thus anticipate lower profits. In a free-entry market such as short-term rentals, lower expected profits trigger exit and market churn, and the resulting supply contraction, together with compositional change in the listing pool, can reshape consumer choice sets and equilibrium prices.

Drip pricing is not the only source of policy concern about consumer-facing price transparency. A distinct but related literature on partitioned pricing offers additional insights. Whereas drip pricing concerns the sequential concealment of mandatory fees until checkout—so that its effects run through search costs, sunk effort, and frustration—partitioned pricing asks how dividing a price into a base component and one or more surcharges shapes perceptions even when the total is disclosed simultaneously and no further search is required. The seminal finding is that partitioning can raise demand relative to an equivalent all-inclusive price: consumers anchor on the salient base price and under-adjust for the surcharge, systematically underestimating and under-recalling the total (Morwitz et al., 1998; Chakravarti et al., 2002). The size of this bias depends on how the surcharge is presented—dollar amounts are underweighted less than percentage surcharges, and partitioning can redirect attention to a secondary component in ways that shift valuation (Bertini and Wathieu, 2008)—while surcharges perceived as unjustified can trigger fairness concerns that depress demand, a countervailing force absent from the drip pricing account (Xia et al., 2004). These results bear directly on our setting: before AB 537, Airbnb displayed the fee-inclusive total but rendered it in small gray font subordinate to a bold per-night rate, with cleaning and service fees itemized separately later in the search process. The partitioned pricing literature helps explain why guests under-responded to fees even though the total was technically visible, and why cleaning fees may have been especially salient targets for regulation.

On the economics side, this evidence connects to a broader literature on price salience and

[4] See conference materials at https://www.ftc.gov/news-events/events/2012/05/economics-drip-pricing.

[5] Source: https://www.ftc.gov/news-events/news/press-releases/2025/05/ftc-rule-unfair-or-deceptive-fees-take-effect-may-12-2025.

inattention, which likewise shows that price framing affects behavior even when all information is available at the point of decision. Chetty et al. (2009) find that posting tax-inclusive prices reduces demand relative to adding the tax at the register despite the tax being fully known—evidence that salience, not information per se, drives the response. This inattention is not uniform across consumers, and the resulting heterogeneity has first-order welfare consequences: Taubinsky and Rees-Jones (2018) show that when attention to a non-salient charge varies across consumers, the welfare cost of low salience exceeds what a representative-agent model implies, and welfare must be evaluated using choices made under full salience rather than the biased demand observed under low salience (see also DellaVigna, 2009). This point is central to our welfare exercise and reinforces the perceived-versus-realized distinction developed below: because we recover price sensitivity both before and after the salient total becomes the anchor, we can measure welfare from realized (attentive) choices rather than from the inflated demand observed under non-transparent pricing. Relative to the drip pricing literature's emphasis on search-cost and competitive-bait mechanisms, the partitioned pricing and salience literatures thus contribute a complementary channel—biased perception of a disclosed total—that operates at a single decision point and carries its own welfare implications.

Beyond drip and partitioned pricing, transparency concerns also arise when prices are set through bilateral negotiation and are thus opaque to prospective buyers (as in new cars, pharmaceuticals, and healthcare), when consumers must search across many sellers (see the S&P 500 index fund case in Hortaçsu and Syverson, 2004), or when retail prices change frequently. Both private and public actors can improve transparency: a trade association or platform can disseminate price summaries or distributions (Whaley, 2019); for-profit firms can incorporate client-provided prices into algorithmic pricing tools (Assad et al., 2024; Calder-Wang and Kim, 2025); and governments can require firms to report prices to a centralized public portal (Christensen et al., 2020; Luco, 2019).

The empirical evidence on these regimes is mixed. Consistent with the conventional wisdom, Whaley (2019) finds that posting healthcare prices on a leading transparency platform reduces provider prices by 1–4% for homogeneous services (laboratory tests) but produces no response for differentiated ones (office visits), which he attributes to active use of the posted information. Similarly, Christensen et al. (2020) find that when 27 U.S. states required hospitals to report charge prices on searchable public websites, charges fell by about 5% without affecting payments or consumer search, and conclude that reputational concerns about perceived overcharging drove the reduction. Grennan and Swanson (2020) find that access to peer hospitals' purchasing data lowered negotiated supply prices, concentrated among hospitals that had paid relatively high prices for high-volume branded products, suggesting peer prices serve as a negotiation reference point. Whaley et al. (2019) similarly find no price-shopping effect when a large employer introduced a transparency platform alone, but a 27% reduction per laboratory test and 13% per imaging test once it paired transparency with reference pricing. Together, these findings suggest transparency can lower prices by facilitating search, enabling buyer-side reference pricing, and imposing reputational costs on sellers who overcharge.

In contrast, Luco (2019) shows that transparency can also let firms monitor rivals more closely

and soften competition. Using a Chilean retail-gasoline price-disclosure policy, he finds disclosure raised margins by 9% on average, with effects attenuated or reversed where consumer search is more prevalent. Greater transparency among competitors (though not necessarily among consumers) can likewise facilitate algorithmic collusion, raise prices, and weaken competition, as documented in German retail gasoline (Assad et al., 2024) and U.S. apartment rentals (Calder-Wang and Kim, 2025). At the same time, Calder-Wang and Kim (2025) show that recommendation algorithms drawing on own and rival prices can help firms read market conditions and price more efficiently, gains that may arise independently of any collusive dynamic.

Our findings on how transparency affects market churn and non-price outcomes echo Bao et al. (2026), who show that disclosing restaurant quality through Yelp consumer reviews triggered entry and exit that, in turn, reshaped the distribution of quality. Like us, they find heterogeneous effects across market types.

We are not the first to study price transparency in short-term rentals. Tran et al. (2025) examine a comparable European change, where Airbnb began displaying cleaning-fee-inclusive prices to European users in January 2019. Focusing on London listings with varying exposure to European clientele, they find that hosts who had not previously charged cleaning fees raised base prices by 17 percent after the change, especially where local competitors had relied on cleaning fees. They attribute this to how sellers update beliefs about rivals' prices: when fees are obscured, inattentive hosts benchmark only against rivals' visible base prices, but once fees are disclosed upfront they recognize that competitors were effectively charging more. Because this mechanism highlights the competitive advantage zero-cleaning-fee hosts may gain after an all-in-pricing change, we refer to it as the *cleaning fee advantage*.

In light of this literature, we examine heterogeneous treatment effects and their consistency with guest search-cost reduction and with host changes in reference pricing and pricing efficiency. While we find support for some of these mechanisms, none individually explains why all-in prices in California rose relative to control cities, even for listings with limited scope for price increases under these explanations. We further show that market churn—the reallocation of activity across stayers, exiters, and entrants—is an additional mechanism reshaping competition. Using a discrete choice demand model, our welfare analysis finds that the law improved realized consumer surplus (under post-law preferences), though this gain falls short of perceived surplus (under preferences inferred from pre-law choices). This is intuitive: ex-ante perceived surplus reflects the illusion created by non-transparent pricing. More interestingly, welfare changes are heterogeneous: realized consumer surplus is more likely to improve in ZIP codes with a larger post-law reduction in average cleaning fee reliance. Entry partially offsets exit overall, but entrants differ systematically from stayers and exiters in price, ratings, and review volume, and these compositional shifts are part of what drives the overall welfare improvement across California ZIP codes in our sample.

Our structural welfare exercise builds on a small set of papers estimating the welfare effects of drip and partitioned pricing. Seim and Vitorino (2025) estimate a limited-foresight model of mandatory add-on fees in driving schools; Tran (2020) estimates a discrete-choice model of parti-

tioned shipping fees and finds a net consumer-welfare loss; and Inderst and Obradovits (2023) show theoretically that competition on headline prices can reduce welfare, rationalizing the ambiguous price and quality effects we document. Rhodes (2023) surveys this literature.

Finally, we contribute to a growing literature on short-term rentals. Zervas et al. (2017) provide early evidence on Airbnb's competitive effects on hotels, and Farronato and Fradkin (2022) show that Airbnb expanded travelers' choice sets and generated sizable consumer surplus, especially when hotel capacity is tight. Other work examines pricing frictions (Huang, 2025), adoption of pricing algorithms (Zhang et al., 2021), price discrimination (Edelman et al., 2017), the role of consumer reviews (Culotta et al., 2026; Cui et al., 2020; Fradkin and Holtz, 2023), and the effects of short-term rental regulation on local housing prices, rents, foreclosure risk, restaurant revenue, crime, noise, listing supply, and tax revenue (Barron et al., 2021; Bibler et al., 2025; Kim et al., 2020; Ozer et al., 2024; Lanfear and Kirk, 2024; Jin et al., 2024). We add price transparency regulation to this list.

3 Background and Data

Pricing Transparency and California's Regulatory Background. On July 1, 2024, California's price transparency law for short-term lodging (AB 537), which prohibits sellers from advertising or offering a room rate that excludes any fees or charges before taxes, became operative.[6] The bill took effect alongside California's broader Honest Pricing Law (SB 478), which covers more sectors. AB 537 applies specifically to short-term lodging establishments (such as hotels), websites, applications, and centralized short-term rental platforms.[7]

We use Airbnb to study the effects of this statewide law. Airbnb listings carry several add-on fees—chiefly cleaning and service charges—that were not advertised on the initial search page before the law. Cleaning fees are host-set and can be substantial: the U.S. Airbnb market has the world's highest cleaning fees, averaging more than $150 per booking in 2025.[8] Service fees, by contrast, are charged by Airbnb and typically run 14 to 17 percent of the subtotal.[9] Given this pricing structure and the size of these charges, Airbnb is a natural setting in which to study the law, and its extensive, geographically diverse U.S. listings let us construct a valid control group for California. We study cleaning fees rather than service fees, since only the former are set by hosts.

To comply, Airbnb began displaying fee-inclusive total prices (before taxes) for all California listings in July 2024.[10] Most listings outside California were not required to do so at that time. Guests searching California listings saw a pop-up explaining that prices include all fees (Figure 1(a)), and

[6] See https://leginfo.legislature.ca.gov/faces/codes_displaySection.xhtml?lawCode=BPC&sectionNum=17568.6.

[7] Beyond California, several jurisdictions adopted all-in pricing rules in 2024 and 2025: Minnesota's Uniform Deceptive Trade Practices Act (effective January 1, 2025), Massachusetts' Unfair and Deceptive Fees regulation (940 CMR 38.00, effective September 2, 2025), and the FTC's Rule on Unfair or Deceptive Fees (16 CFR Part 464, effective May 12, 2025), which covers short-term lodging and live-event ticketing nationwide. A parallel U.S. Department of Transportation airline ancillary-fee disclosure rule (2024) was vacated by the Fifth Circuit in February 2026.

[8] See an article by AirDNA at https://www.airdna.co/blog/airbnb-cleaning-fees-what-hosts-need-to-know.

[9] See an article by Airbnb at https://www.airbnb.com/resources/hosting-homes/a/how-much-does-airbnb-charge-hosts-288.

[10] See https://skift.com/2024/06/20/airbnb-to-display-total-price-by-default-in-california-to-comply-with-new-law/.

on the search page Airbnb advertised the all-in price rather than the per-night rate. As Figure 1(b) and (c) show, outside California the nightly rate appeared in bold with the fee-inclusive total in smaller gray font (e.g., **$125** night · $171 total), whereas for California listings the total appeared in bold as the advertised price.[11]

Airbnb had attempted all-in pricing before the law. In December 2022 it rolled out an opt-in total-price toggle in the U.S., letting consumers display fee-inclusive totals in search results.[12] Uptake was low: over the following two years, almost 17 million guests used it, against third-party estimates of more than 275 million global users—roughly a 6% usage rate.[13] Airbnb kept the toggle opt-in rather than default to signal that its prices were displayed differently from competitors', and co-founder and CEO Brian Chesky suggested that an industry-wide shift to all-in pricing would require regulation to create a level playing field.[14] Beyond California, several states have since passed similar laws, including Minnesota's 2024 amendment to its Uniform Deceptive Trade Practices Act, effective January 1, 2025. On April 21, 2025, Airbnb announced that guests worldwide would see only the total price of stays (including all fees but not taxes) by default,[15] and three weeks later the FTC's Rule on Unfair or Deceptive Fees took effect, mandating upfront disclosure of the all-in price in live-event ticketing and short-term lodging.[16] We do not study the optional toggle, given its nationwide rollout and unclear uptake, and we set aside Minnesota's law, since Minnesota is not comparable to our treated or control cities (Seattle and Chicago). Our sample is also too short to identify the effects of Airbnb's global rollout or the FTC rule. Because control-city listings also displayed all-in prices from April 2025 onward, our estimates through July 2025 may understate the treatment effect; we re-estimate the main specifications on a sample ending in March 2025 as a robustness check.

Characterizing precisely what AB 537 changed matters, because Airbnb's pre-law display was neither pure drip nor pure partitioned pricing. Under pure drip pricing, mandatory fees are concealed until checkout; under pure partitioned pricing, the base fare and fees are shown separately with no total. On Airbnb before the law, by contrast, the fee-inclusive total already appeared on the search page, but in small gray font subordinate to a bold per-night rate (e.g., "**$125** night · $171 total"), with the fee breakdown revealed progressively later in the funnel. An opt-in total-price toggle had also been available nationwide since December 2022, though only about 6 percent of guests used it. On a spectrum from pure drip pricing (total hidden until checkout) to low-salience pricing (total visible but de-emphasized, components partitioned) to salient all-in pricing (total is the primary anchor), AB 537 moved Airbnb from the middle toward the right end. The mandate thus changed the salience and default framing of the total rather than revealing previously hidden information. Because our pre-period already contained partial disclosure, our estimates are likely a lower bound

[11] The total price $171 includes per night rate $125, cleaning fee $25, and Airbnb service fee $21.

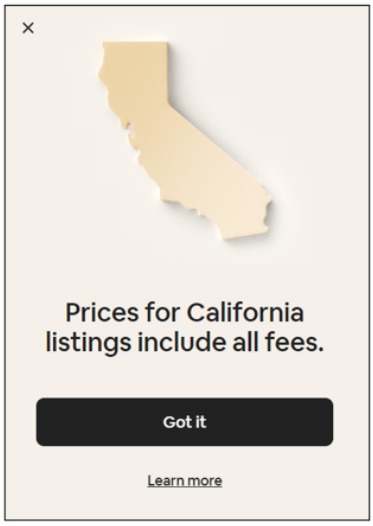
[12] See https://news.airbnb.com/airbnb-is-introducing-total-price-display-and-updating-guest-checkout/

[13] See https://news.airbnb.com/total-price-display-is-now-standard-globally/

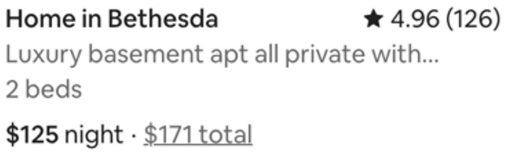
[14] See https://theescapehome.com/airbnb-founder-brian-chesky-talks-about-whats-next-for-the-short-term-rental-platform/

[15] See https://news.airbnb.com/total-price-display-is-now-standard-globally/.

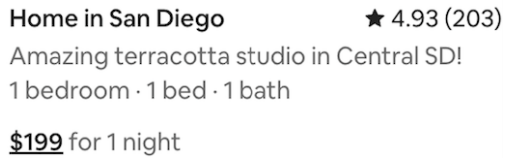
[16] See https://www.ftc.gov/business-guidance/resources/rule-unfair-or-deceptive-fees-frequently-asked-questions.



(a) Airbnb Pop-Up Message



(b) Price Display on Search Page: Non-CA



(c) Price Display on Search Page: CA

Figure 1: Airbnb Pop-Up Message and Price Display for Non-California and California Listings after California's Price Transparency Law (Screenshots Taken on April 1, 2025)

on the effect of moving from pure drip pricing to salient all-in pricing.

Data Sources. We use Airbnb listing performance data from July 2023 to July 2025 (one year before and one year after the law) for four cities in and outside California: San Diego, San Francisco, Chicago, and Seattle. We exclude Los Angeles because it introduced a new short-term-rental police-permit requirement at the same time as AB 537 (July 2024), whose effects could be confounded with those of the price transparency law.[17]

We obtain price and occupancy data from AirDNA, a private data company that collects listing-level data from Airbnb and Vrbo. The data provide price, booking, revenue, listing type, and location for each listing-month. Price—average daily rate (ADR)—is split into per-night rate (PNR) and cleaning fee, letting us examine how the law affected each component separately. One caveat is that AirDNA does not include service fees; this matters little, however, since Airbnb sets the service fee proportionally to the host-set ADR. Separating ADR and cleaning fee also lets us test whether the law had larger effects on listings that charge higher cleaning fees or rely more heavily on cleaning-fee revenue. For simplicity, we refer to ADR throughout as the all-in or fee-inclusive price, though technically the all-in price California required is ADR plus service fees.

We focus on active listings (those not blocked in a given month) and exclude the top 0.5% of ADR (above $2,000). Entire-home listings account for 84% of the sample; because other types, such as private rooms, have substantially different prices and attract different guests, we restrict attention to entire-home listings.[18]

[17] For more information on LA's police permit requirement, see https://www.lapdonline.org/police-commission/cid-rho/.

[18] The average price of entire-home listings is about twice that of private-room listings and four times that of shared-room listings (Culotta et al., 2026).

A further limitation of AirDNA is that it does not capture how a listing's attributes change over time. We therefore obtain time-varying attributes from InsideAirbnb, a website that systematically collects Airbnb data, which provides monthly listing-level characteristics including superhost status, ratings, host response rate, host acceptance rate, number of listings owned by the host, instant-booking availability, and number of reviews. We merge these with AirDNA's performance data using Airbnb's unique property ID. After merging, 87% of performance observations match directly to InsideAirbnb attributes; imputing missing values from the same listing's adjacent months raises coverage to 95% of listing-months.

AirDNA's data also include Vrbo listings, but InsideAirbnb does not collect Vrbo data, so we lack time-varying attributes for them. We therefore treat Vrbo listings as guests' outside option and use them only in the structural demand estimation.

Several other datasets supplement our main data. We use the Consumer Price Index from the Bureau of Labor Statistics to control for differing inflation across cities, and in demand estimation we use the Zillow Home Value Index by ZIP code-month to instrument for listing price.

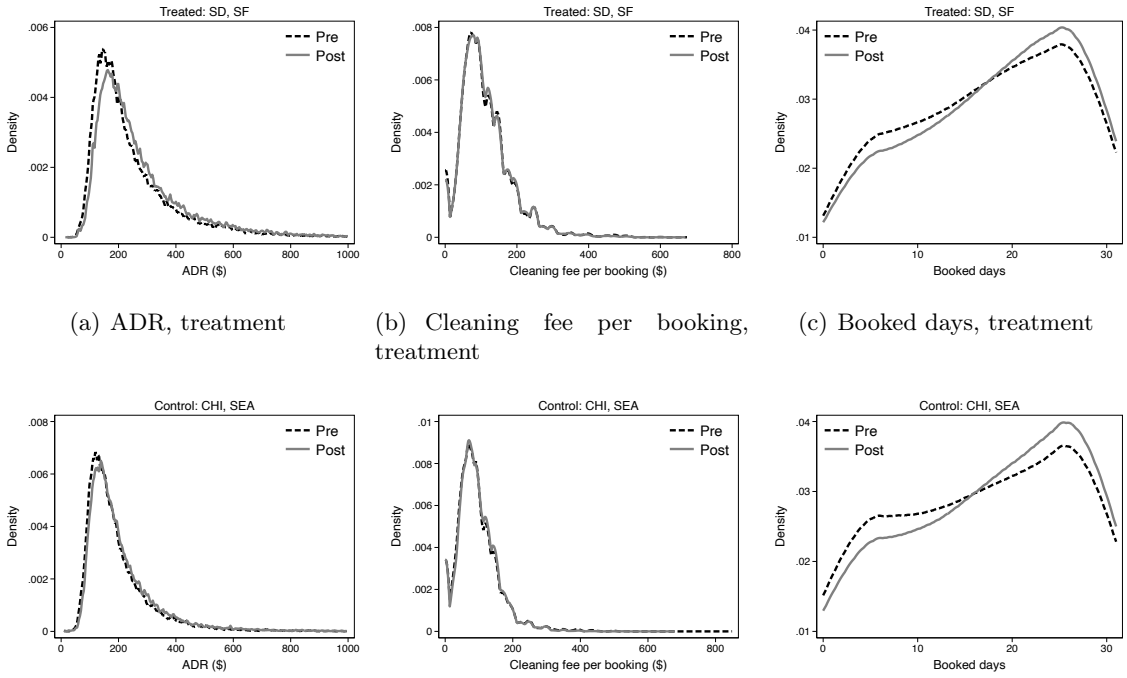
Matching Treatment and Control Groups. Identifying the treatment effect requires comparing treated units in California with control units outside it. The two may not be directly comparable: if California listings have systematically higher unobserved quality, for example, differential changes in performance could reflect quality differences rather than the law. Indeed, within our sample, listings in Chicago and Seattle averaged 0.07 more bedrooms and a superhost rate 3 percentage points higher than those in San Diego and San Francisco (Columns 1 and 4 of Table 1). We address this by (1) controling time-varying listing attributes and (2) matching treated and control listings on a broad set of observables using Coarsened Exact Matching (CEM) (Iacus et al., 2012). To avoid endogeneity, we compute pre-treatment values for all matching variables.

In particular, after coarsening the covariates and exact-matching on the coarsened values, treated and control units are grouped into strata; unmatched units are dropped, and weights are assigned to balance the two groups within each matched stratum.[19] CEM thus prioritizes balance over sample size, giving us the best chance of comparing listings that are alike on unobservables.

We match on several key attributes and performance outcomes: listings in and out of California are matched if they fall into the same bin for each variable—number of bedrooms, number of bathrooms, location type (large urban/large suburban/mid-size/resort), superhost status, number of listings managed by the host, host response rate, host acceptance rate, instant-booking availability, number of reviews, and ratings. To ensure comparable availability and performance, we also match on number of listed nights and occupancy rate, and to ensure similar local competition we match on the number of listings within 0.3 miles. For all variables we use pre-treatment averages, so matching is unaffected by changes the law induced. Our final sample includes 193,742 listing-month observations, 74,068 treated and 119,674 control.[20] Matching narrows the attribute gaps

[19] Specifically, treated units receive a weight of one, and the weight of control units is rescaled so that the weighted control group is representative of the treated group's distribution.

[20] Regressions use the 174,619 observations with complete time-varying attributes (168,085 in demand estimation,



(a) ADR, treatment

(b) Cleaning fee per booking, treatment

(c) Booked days, treatment

(d) ADR, control

(e) Cleaning fee per booking, control

(f) Booked days, control

Figure 2: Comparing Treatment and Control Listings Pre- and Post-Law

Note: Kernel density plots use CEM weights.

noted above: as Table 1 shows, the pre-treatment average number of bedrooms and superhost rate become nearly identical between San Diego/San Francisco listings and their matched counterparts in Seattle and Chicago. While matching substantially improves balance, it reduces sample size by more than half and lowers ADR in both groups.[21] We therefore report main results both with and without CEM weights and show they are robust to this choice, and that estimates remain stable when we construct CEM weights using fewer bins.

Descriptive Evidence. Figure 2 compares the distribution of listing price and bookings between treated and control units, with dashed lines for pre-law and solid lines for post-law outcomes. Defining ADR as the all-in price before tax (PNR plus cleaning fee per night), we observe an overall rise in average ADR and a heavier right tail after treatment in both groups, reflecting general price inflation over time though the shift is more pronounced among treated units. Average booked days also rose, with greater mass around 25 days a month, but booking growth was smaller for the treated group. The cleaning fee distribution changed little for either group.

where market shares are also required), and listing supply analysis uses the full unmatched panel described in Section 5.

[21] According to Table 1, unmatched listings tend to be owned by hosts with multiple listings, have higher ADRs and lower quality (as reflected in lower superhost rate, lower overall rating, and fewer reviews). This is consistent with the fact that hosts who manage multiple listings tend to have higher prices and lower quality as compared to individual hosts.

Table 1: Summary Statistics

Matched Obs.: 193,742	Treatment			Control		
	Pre (Raw)	Pre (Matched)	Post (Matched)	Pre (Raw)	Pre (Matched)	Post (Matched)
Outcome Variables:						
Avg. Daily Rate (ADR, $)	333.41	249.86	278.03	236.96	194.09	208.27
Per Night Rate (PNR, $)	285.16	213.84	242.85	199.52	163.39	177.43
Cleaning Fee Per Booking ($)	147.08	112.20	112.27	112.17	93.67	93.49
Avg. Daily Cleaning Fee ($)	48.28	36.03	35.17	37.45	30.72	30.85
Cleaning Fee Revenue ($)	688.48	541.53	557.14	514.13	460.74	496.82
Zero Cleaning Fee (0/1)	0.05	0.04	0.04	0.06	0.06	0.06
Total Revenue ($)	5246.03	4091.22	4766.07	3593.06	3177.30	3663.28
Bookings	4.72	5.06	5.16	4.79	5.11	5.45
Booked Days	16.27	17.28	17.86	15.89	16.91	17.80
Listing Attributes:						
Bedrooms	1.91	1.55	1.56	1.98	1.55	1.56
Bathrooms	1.56	1.28	1.28	1.50	1.28	1.28
Superhost	0.56	0.68	0.75	0.59	0.68	0.72
Instant-Bookable	0.35	0.26	0.27	0.34	0.26	0.26
Listings Owned by Host	15.87	6.47	6.49	24.45	10.64	8.13
Host Response Rate (0-100)	98.36	99.36	99.24	98.72	99.24	99.24
Host Acceptance Rate (0-100)	93.27	96.54	95.79	95.85	96.66	96.22
Reviews	78.44	108.84	139.71	75.77	103.86	137.25
Overall Rating (0-5)	4.80	4.87	4.87	4.80	4.86	4.86
Accuracy Rating (0-5)	4.83	4.89	4.89	4.83	4.89	4.89
Cleanliness Rating (0-5)	4.81	4.87	4.87	4.81	4.87	4.87
Check-in Rating (0-5)	4.88	4.93	4.93	4.86	4.91	4.91
Communication Rating (0-5)	4.88	4.94	4.93	4.88	4.93	4.93
Location Rating (0-5)	4.82	4.83	4.83	4.82	4.86	4.86
Value Rating (0-5)	4.71	4.78	4.78	4.71	4.79	4.79
Observations	117,805	39,247	34,821	110,497	62,788	56,886

Notes: An observation is a listing-year-month. Columns 1 and 4 do not include CEM weights; all other columns include CEM weights.

Table 1 reports summary statistics for the raw (Columns 1 and 4) and matched (Columns 2–3 and 5–6) groups; the top panel shows outcomes such as prices and bookings, the bottom panel shows attributes such as ratings and host type. After matching, pre-treatment attributes are similar across groups, since we match on pre-treatment values to reduce endogeneity: listings in Chicago and Seattle have similar bedrooms, bathrooms, and superhost rates to those in San Diego and San Francisco before the law. Some high-ADR listings in San Diego and San Francisco cannot be matched to control listings in Seattle or Chicago, and this lack of comparability could bias our estimates; focusing on CEM-matched listings helps address it. Indeed, California listings have lower ADR on average, and the treated-control ADR gap shrinks after matching. A few attributes did change after the law: (a) superhost rates rose from 0.68 to 0.75 in treated cities and from 0.68 to 0.72 in control cities; (b) the number of reviews increased by roughly 30 percent, but similarly

across groups—reflecting the cumulative nature of reviews rather than any effect of the law; and (c) the average number of listings owned by the host is 6.5 in treated and 10.6 in control units before the policy, with medians of 1 and 2, respectively.

In treated units, the average cleaning fee per booking barely moved, differing by only \$0.07 between pre- and post-law periods; the difference was \$0.18 for control units. On average, the cleaning fee accounts for about 13% to 16% of ADR. Although cleaning fees changed little, their share of ADR fell for both groups—from 14% to 13% for treated and 16% to 15% for control—because average ADR rose from \$250 to \$278 among treated units and from \$194 to \$208 among control units, driven mainly by PNR. About 5% of listing-months, on average, charged zero cleaning fees.[22]

Listing performance shows a sharp contrast: treated cities saw slower growth in bookings and booked days than control cities. Bookings rose 2 percent among treated units vs. 7 percent among control units, and booked days rose 3 percent vs. 5 percent. Because treated units' ADR increases outweighed booking declines, their total monthly revenue rose 16% vs. 15% for control units.

Taken together, the descriptive evidence shows that treated and control units are broadly comparable on observables after CEM. Treated units experienced higher ADR, a lower cleaning-fee ratio, slower booking growth, and higher revenue following the law. We next exploit cross-state variation in a difference-in-differences design with a matched control group to examine these changes formally.

4 Effects of Price Transparency on Listing Performance

Econometric Framework. Because the price transparency law applied in California but not in Seattle or Chicago, we use a difference-in-differences strategy. The treated group comprises listings in San Diego and San Francisco; the control group includes listings in Chicago and Seattle. We match the two groups using CEM weights (Section 3) to ensure balance and comparability, so that identification comes from comparing treated and control listings that are otherwise similar in quality (e.g., ratings, superhost status) and type (e.g., number of bedrooms and bathrooms, host characteristics). For listing i in city c and year-month t (calendar month m), the model is:

$$Y_{it} = \alpha_i + \mu_t + \delta_{cm} + \beta \, \text{Post}_t \times \text{CA}_i + \Gamma X_{it} + \epsilon_{it} \tag{1}$$

where Y_{it} is the outcome. Listing fixed effects α_i capture time-invariant characteristics such as square footage, neighborhood, and building age.[23] Year-month fixed effects μ_t absorb shocks common to all listings in a given year-month, and city-calendar-month fixed effects δ_{cm} capture city-specific seasonality, allowing each city its own monthly pattern. X_{it} is a comprehensive set of time-varying listing and host attributes (bottom panel of Table 1), such as ratings, reviews, and superhost status; we also include each metropolitan statistical area's (MSA) monthly Consumer Price Index to capture differing inflation across cities. ϵ_{it} is the error term, and all regressions use CEM weights.

[22] Cleaning fees are more common in the U.S. than in major markets such as the U.K. and France. For more information see https://www.airdna.co/blog/airbnb-cleaning-fees-what-hosts-need-to-know.

[23] Listing fixed effects also absorb natural disaster exposure, such as each listing's distance to wildfire locations.

The interaction coefficient β captures the change in outcomes for treated relative to control listings after the law. Conditional on the control-group restrictions, controls, and fixed effects, we interpret β as the law's effect. We cluster robust standard errors by listing to allow for arbitrary serial correlation within listings, and later report robustness to clustering by ZIP code or city.

4.1 Baseline Results

Table 2 presents the baseline estimates. ADR and PNR enter in logs, since both are strictly positive and the log form lets us read β as a percentage change. We also construct a standardized ADR that holds length of stay fixed at three days (the sample median),[24] isolating price changes from shifts in length of stay that may themselves respond to the law. For total cleaning fee, cleaning fee per booking, and revenue per listing-month, we use dollar amounts rather than logs, since these can be zero. For counts such as bookings or booked days per month, we use Poisson regressions, and to capture the extensive margin we also use an indicator for zero bookings in the focal listing-month.

The estimates imply that California listings experienced a 6.1 percent increase in PNR, a slight reduction in cleaning fee per booking, and thus a smaller 4.8 percent increase in ADR, relative to listings outside California. Standardized ADR rose slightly more (5.2 percent), as length of stay lengthened moderately. Bookings and booked days each declined in relative terms, by 3.7 and 1.2 percent respectively.[25] Consistent with this, California listings were 0.6 percentage points more likely to have zero bookings in a month (a 25 percent increase over the sample mean) and monthly cleaning fee revenue fell by \$23.8, or 4.6 percent. Because the booking decline was smaller than the price increase, average monthly total revenue rose by \$174.1, or 4.5 percent. These patterns suggest that, before the law, hosts may not have set prices high enough to maximize revenue, despite the prevailing wisdom that drip and partitioned pricing let sellers exploit consumers' information frictions within a search session and overcharge relative to all-in pricing.

Figure 3 shows quarterly event-study plots for PNR and ADR: coefficients become uniformly positive and significant after the law, and are all near zero before the law except for the first quarter of 2024.[26] The relative ADR decline in that period is likely associated with the short-term travel demand shock due to California atmospheric river storms in February 2024. It led to more than 1,000 flight delays or cancellations at airports near San Diego and San Francisco.[27] In Figure A2, after removing February and March 2024, we do not observe any pre-trends; the estimated ADR increase becomes 2.9 percent, as shown in Panel C of Table A3.[28]

[24] Standardized ADR equals PNR plus a standardized cleaning fee per night, where the latter divides the cleaning fee per booking by three (the median stay length in our sample).

[25] In Poisson regressions, the marginal effect of the coefficient is $exp(\beta)-1$, thus we have $exp(-0.038)-1 = -0.0373$ for number of bookings and $exp(-0.012) - 1 = -0.0119$ for number of booked days per month.

[26] Chicago hosted the Democratic National Convention in August 2024, one month after the law took effect, which may have temporarily affected ADR in Chicago. This may contribute to a downward bias in our estimate.

[27] See https://weather.com/news/news/2024-02-03-atmospheric-river-storm-california-flooding-updates.

[28] Because our specification includes ZIP code-by-calendar-month fixed effects and our sample spans only one year before and one year after the law, excluding February and March 2024 also effectively prevents February and March 2025 from contributing to the DID coefficient, since the effect of these two post-law months is collinear with the ZIP code-by-calendar-month fixed effects.

Table 2: Effects of California's Price Transparency Law

Treated = SD, SF; Control = SEA, CHI									
	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-23.844***	-0.428	0.061***	0.048***	0.052***	-0.038***	-0.012*	0.006***	174.072***
	(4.492)	(0.292)	(0.004)	(0.004)	(0.003)	(0.007)	(0.006)	(0.002)	(31.402)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	518.297	99.808	5.092	5.263	5.270	5.353	17.686	0.024	3858.495
Adj./Pseudo R^2	0.697	0.898	0.884	0.883	0.904	0.233	0.248	0.087	0.708
N	174,619	174,619	174,619	174,619	174,619	174,619	174,619	174,619	174,619

Notes: Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and city-month fixed effects. Regressions use CEM weights. The dependent variable in Column 5, standardized ADR, equals PNR plus a standardized cleaning fee per night, where the latter divides the cleaning fee per booking by three (i.e., the median stay length in our sample). Columns 6 and 7 estimate a Poisson model. Robust standard errors are clustered at the listing level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Robustness Checks. In Table A1, we re-estimate the main specifications without matching (since matching lowers average ADR in both groups) and, separately, after removing post-law exits and entries to isolate incumbent responses; both sets of estimates are similar in magnitude to our main results. To check whether San Diego's short-term-rental licensing framework (phased in over our sample window) drives the results, we drop San Diego and re-estimate; the ADR coefficient remains positive and significant, though smaller (2.6 percent). Table A2 truncates the sample at March 2025 (12 months before and 9 months after the law) when Airbnb rolled out a default display of all-in prices,[29] and at December 2024 (12 months before and 6 months after) when the Federal Trade Commission announced the final Rule on Unfair or Deceptive Fees.[30] We also vary CEM construction: using 3 cut points per matching variable rather than the 5 used in the main analysis and removing pre-law occupancy rate and number of listing nights from matching variables increase the sample by more than 60 percent and yields a 4.8 percent ADR increase, identical to the main estimate. Finally, Table A3 shows that most of our estimates remain statistically significant at at least the 5 percent level under clustering at the ZIP code or city level.

The finding that prices rose after the law is counterintuitive. Because consumers are price sensitive, advertising the fee-inclusive total on the search page rather than the lower nightly rate should make them less likely to book; all else equal, sellers should then lower the total price to attract marginal consumers sensitive to which price is shown upfront. Our finding contradicts this prevailing prediction. The analysis that follows explores potential explanations for the price increases in California.

4.2 Heterogeneous Effects

We next examine heterogeneous effects in a triple difference-in-differences framework and link them to explanations proposed in the literature.

Triple-Differences Framework. We use a triple-differences model:

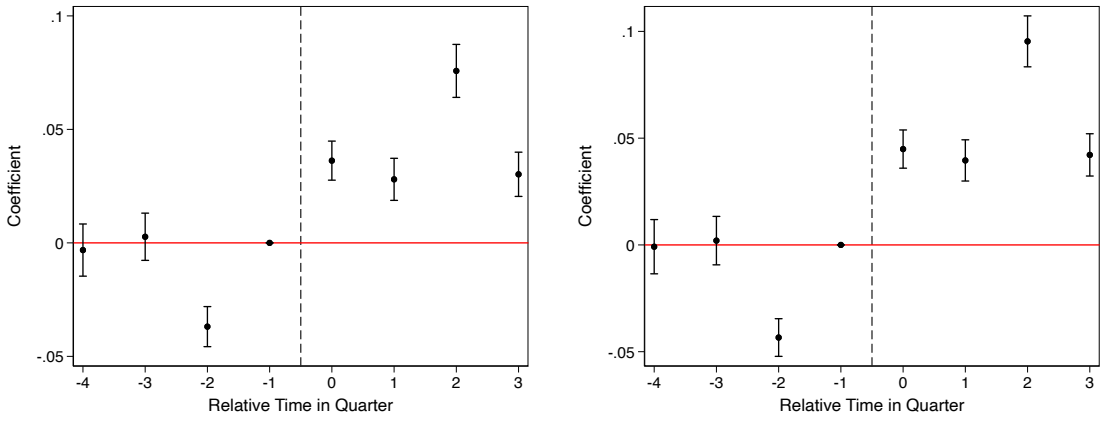
$$Y_{it} = \alpha_i + \mu_t + \delta_{cm} + \beta_1 \,\text{Post}_t \times \text{CA}_i \times \text{H}_{it} + \beta_2 \,\text{Post}_t \times \text{CA}_i + \beta_3 \,\text{CA}_i \times \text{H}_{it} + \beta_4 \,\text{Post}_t \times \text{H}_{it} + \Gamma X_{it} + \epsilon_{it} \quad (2)$$

where H_{it} denotes the type of listing i in year-month t. For variables that may fluctuate over short horizons or be affected by the law—such as competition intensity, cleaning fee difference, or price tier—we use pre-law averages as $\text{H}_{i(t)}$ to limit endogeneity. Other elements follow Equation (1). The key coefficient β_1 captures how outcomes changed across listing types after the law.

Table 3 presents the triple-differences estimates. We test heterogeneity along five dimensions—cleaning fee difference, listing quality, host sophistication, local competition, and price tier—and for each examine three outcomes: logged ADR, number of booked days, and monthly revenue. As in the main design, booked days uses a Poisson regression rather than OLS.

[29] See https://news.airbnb.com/total-price-display-is-now-standard-globally/.

[30] See https://www.ftc.gov/news-events/news/press-releases/2024/12/federal-trade-commission-announces-bipartisan-rule-banning-junk-ticket-hotel-fees.



(a) Log(ADR) (b) Log(PNR)

Figure 3: Effects of California's Price Transparency Law on Prices, Event Study

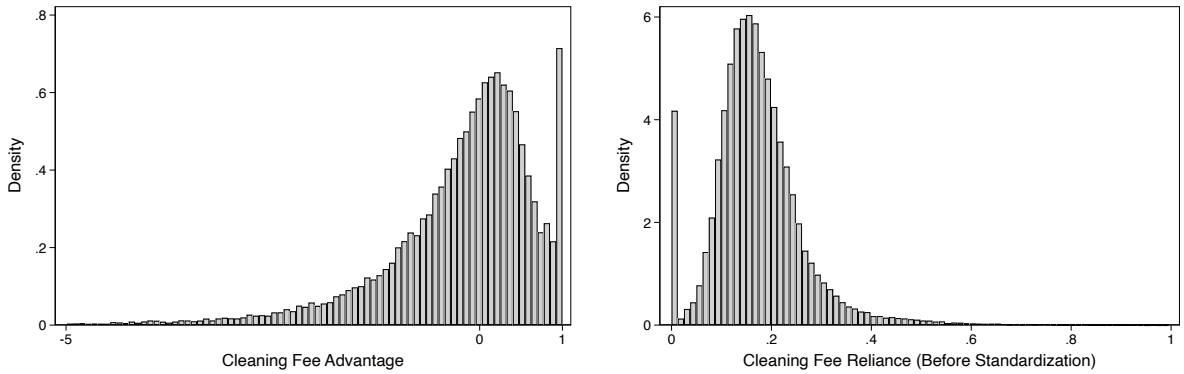
Note: Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and city-specific time trends. Regressions use CEM weights. Robust standard errors are clustered at the listing level. 95% confidence intervals are displayed as brackets.

Results by Cleaning Fee Difference. We begin with cleaning fees. Because the law promotes transparent pricing, listings with less transparent prices (higher cleaning fees) should show larger effects, so the cleaning fee can proxy for treatment intensity.

A listing's cleaning fee can be measured relative to local competitors or as a share of its own total price, so we construct two measures. The first, *cleaning fee advantage*, is equal to the ZIP code average pre-law daily cleaning fee minus the listing's own fee, divided by the ZIP code average. A listing at the local average has zero advantage; a zero-fee listing has the maximum advantage of one, as Figure 4(a) shows.

The second, *cleaning fee reliance*, is the listing's pre-law share of daily cleaning fee in ADR, with the daily cleaning fee standardized to a three-night stay so that the effect is isolated from changes in length of stay. As Figure 4(b) shows, it ranges from zero to one, with high density at zero for listings charging no cleaning fee; we then standardize it to mean zero and unit standard deviation.

Panel A reports estimates by cleaning fee difference. Columns 1–3 use cleaning fee advantage: moving from no advantage (fees at the local average) to the maximum of one (a zero-fee listing) is associated with a 1.1 percent larger ADR increase, with no decline in booked days or revenue. When fees are obscured, some hosts benchmark against rivals' more visible base prices; disclosure after the law reveals that rivals' effective prices were higher, giving these hosts room to raise their own. This parallels Tran et al. (2025), who show that lower-cleaning-fee listings appear relatively cheaper under transparent pricing and thus raise nightly prices relative to higher-fee listings. Columns 4–6 use cleaning fee reliance: the ADR coefficient is positive but small and statistically insignificant. This does not undercut the law's effect; rather, guests may find it hard to compute a listing's cleaning fee reliance (the ratio of cleaning fee to ADR), so high-reliance sellers, though more exposed to the law,



(a) Cleaning Fee Advantage

(b) Cleaning Fee Reliance

Figure 4: Distribution of Cleaning Fee Advantage and Cleaning Fee Reliance

Note: In Panel (a), cleaning fee advantage is constructed based on how a listing's average pre-law daily cleaning fee compares to the pre-law average for its ZIP code. Specifically, cleaning fee advantage is defined as the difference between these two averages, divided by the ZIP code's average. A listing with the same cleaning fee as its local competitors has zero cleaning fee advantage; a listing with zero cleaning fee has maximum cleaning fee advantage, which is equal to one. In Panel (b), cleaning fee reliance is defined as a listing's pre-law share of nightly cleaning fees in ADR, where the nightly cleaning fee is computed based on a length of stay equal to three nights (i.e., sample median).

may be unable to raise prices. Although ADR increases do not vary with reliance, Section 5 shows that reliance strongly predicts changes in aggregate listing supply and individual exit decisions.

Results by Listing Quality. We next examine quality, measured by star ratings or superhost status. To the extent that all-in upfront pricing reduces search costs, it could affect high- and low-quality listings in two opposing ways.

On one hand, in a field experiment comparing drip and all-in pricing for event tickets, Blake et al. (2021) find that drip pricing leads consumers to select and buy more expensive tickets: it makes all goods appear cheaper, and the hidden percentage fee lowers the perceived marginal cost of quality, encouraging substitution toward higher quality. As they show, sellers respond to greater price obfuscation by listing higher-quality tickets. By the same logic, since both cleaning fees and PNR rise with quality in our setting, we would expect a larger price decline among higher-quality listings in California after the law.

On the other hand, lower price-search costs may free time and attention for evaluating non-price attributes such as quality. Greater attention to quality could raise consumers' appreciation of it, implying a smaller price decline or a larger price increase for higher-quality listings. This prediction opposes Blake et al. (2021) but aligns with Huck et al. (2016), who find that consumers attend more to the quality of experience goods when they focus less on price.

To test these predictions, Panel B groups listings by overall star rating, the average response to "overall, how was the stay?" on a 1–5 scale. Six sub-category ratings exist, but the overall rating

Table 3: Heterogeneous Effects of California's Price Transparency Law

Panel A: By Cleaning Fee Difference

	Cleaning Fee Advantage			Cleaning Fee Reliance		
Dep. Var:	Log(ADR)	Booked Days	Revenue	Log(ADR)	Booked Days	Revenue
Post × CA	0.050***	-0.017***	129.701***	0.052***	-0.014**	174.283***
	(0.004)	(0.006)	(29.734)	(0.004)	(0.006)	(30.943)
Post × CA × Fee Advantage	0.011*	-0.003	33.265			
	(0.007)	(0.010)	(66.336)			
Post × CA × Fee Reliance				0.001	-0.003	-26.797
				(0.006)	(0.009)	(46.294)
Dep. Var. Mean	5.264	17.711	3868.188	5.263	17.686	3858.552
Adj./Pseudo R^2	0.884	0.248	0.709	0.884	0.248	0.708
N	173,044	173,044	173,044	174,615	174,615	174,615

Panel B: By Listing Quality

	Overall Rating			Superhost Status		
Dep. Var:	Log(ADR)	Booked Days	Revenue	Log(ADR)	Booked Days	Revenue
Post × CA	0.038***	-0.018*	83.943*	0.035***	-0.049***	-15.883
	(0.007)	(0.011)	(49.704)	(0.009)	(0.015)	(62.025)
Post × CA × High Rating	0.014*	0.011	149.901**			
	(0.008)	(0.013)	(63.266)			
Post × CA × Superhost				0.016*	0.048***	249.819***
				(0.009)	(0.016)	(72.278)
Dep. Var. Mean	5.263	17.686	3858.495	5.263	17.686	3858.495
Adj./Pseudo R^2	0.883	0.248	0.708	0.883	0.248	0.708
N	174,619	174,619	174,619	174,619	174,619	174,619

Panel C: By Host Sophistication

	Casual vs. Professional Host			New vs. Experienced Host		
Dep. Var:	Log(ADR)	Booked Days	Revenue	Log(ADR)	Booked Days	Revenue
Post × CA	0.035***	-0.016*	119.829**	0.051***	-0.006	193.797***
	(0.006)	(0.009)	(48.624)	(0.005)	(0.008)	(39.527)
Post × CA × Casual Host	0.023***	0.009	100.575			
	(0.007)	(0.012)	(62.674)			
Post × CA × New Host				-0.006	-0.012	-35.408
				(0.007)	(0.012)	(63.022)
Dep. Var. Mean	5.263	17.686	3858.495	5.263	17.686	3858.566
Adj./Pseudo R^2	0.883	0.248	0.708	0.883	0.248	0.708
N	174,619	174,619	174,619	174,614	174,614	174,614

Panel D: By Local Listing Competition and Price Tier

	Competition Intensity			Price Tier		
Dep. Var:	Log(ADR)	Booked Days	Revenue	Log(ADR)	Booked Days	Revenue
Post × CA	0.044***	-0.012*	150.248***	0.050***	-0.024**	159.572**
	(0.004)	(0.006)	(33.433)	(0.006)	(0.010)	(66.862)
Post × CA × Low Comp.	0.025**	-0.011	79.983			
	(0.010)	(0.022)	(113.870)			
Post × CA × Low Price Tier				-0.001	0.015	-12.991
				(0.007)	(0.012)	(71.174)
Dep. Var. Mean	5.263	17.686	3858.495	5.263	17.686	3858.495
Adj./Pseudo R^2	0.883	0.248	0.708	0.883	0.248	0.708
N	174,619	174,619	174,619	174,619	174,619	174,619

Notes: Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and city-month fixed effects. Regressions use CEM weights. Columns 2 and 5 use Poisson regressions. Cleaning fee advantage is defined as the difference between the local competitors' average pre-policy daily cleaning fee in a zipcode and the listing's own average, divided by the competitors' average, so a zero-fee listing has an advantage of one. Cleaning fee reliance is defined as a listing's pre-policy share of nightly cleaning fees in ADR, standardized to mean zero and unit standard deviation. A listing has high rating if its pre-policy average overall rating exceeds city-specific sample median. Casual hosts are those with a single listing. New hosts are those whose listings received their first review after 2022 (i.e., sample median). A listing is in a low-competition area if its zipcode has fewer than sample-median listing counts before the policy. A listing is in low price tier if it has below-average ADR within a zipcode before the policy. Robust standard errors are clustered at listing. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

weighs more heavily in outcomes that matter to hosts and guests.[31] Because ratings change over time and may respond to the law, we assign high-/low-rating status from each listing's pre-law values, using city-specific thresholds since some cities rate systematically higher. Listings above their city's pre-July-2024 median are "higher-rating," the rest "lower-rating." We also use superhost status, a broader measure of host quality based on multiple performance metrics over the prior 365 days, such as overall rating, cancellation rate, and response rate.

Panel B shows that higher-rated listings had 1.4 percent larger ADR increases than lower-rated ones, without greater occupancy or revenue losses. Similarly, superhosts had a 1.6 percent larger ADR increase than non-superhosts, along with relative gains in booked days and monthly revenue. Together, higher-quality listings raised ADR by more without incurring additional losses in bookings or revenue—consistent with price transparency making consumers value quality more, but inconsistent with the conjecture that higher-quality, higher-priced sellers exploited consumers more when prices were less transparent.

These comparisons are relative. In absolute terms, even non-superhosts raised ADR by 3.5 percent, though this reduced booked days with no detectable change in revenue; lower-rated listings likewise raised ADR by 3.8 percent, with no revenue decline but fewer booked days. These patterns challenge the common view that banning drip and partitioned pricing should reduce consumer frustration in price search and foster price competition.

Results by Host Sophistication. Host sophistication is another possible dimension of heterogeneity. On one hand, less sophisticated hosts may have limited knowledge of market prices and charge less than more sophisticated ones,[32] and the law could reduce this friction by letting them identify the all-in market price more easily and raise prices. On the other, more sophisticated hosts may track legal and market changes more closely and respond faster and more strongly, raising prices by more if repricing upward is the common response. Because these forces oppose, the sign is theoretically ambiguous.

We test this with two measures in Panel C. The first is listing count: casual hosts have only one listing, professional hosts more than one, with casual hosts about 35% of observations. The second is experience: a host is experienced if her listing received its first review before 2022 (the sample median), and new otherwise.

Panel C shows that casual-host listings raised ADR by 2.3 percent more than professional-host listings, without declines in booked days or monthly revenue; indeed, the coefficients for booked days and revenue are positive, though insignificant. This parallels our quality result, since casual hosts tend to have higher-quality listings.[33] We find no differential ADR effect between experienced and new hosts, though both raised ADR significantly after the law.

[31] See Airbnb community discussion at https://community.withairbnb.com/t5/Help/Difference-between-Overall-rating-in-performance-ratings-and/m-p/1055593.

[32] In our sample, average ADR is $275 among hosts with only one listing and $322 among hosts with multiple listings.

[33] The correlation between being a casual host and being a superhost is 0.06; the correlation between being a casual host and overall star rating is 0.16.

Results by Local Listing Competition and Price Tier. We next examine heterogeneity by local competition intensity. In theory, coordination (tacit or explicit) is harder to sustain in more competitive markets, so price increases should be smaller there, though transparency could lower coordination costs. At the same time, if hosts price non-cooperatively and transparency helps them price closer to the profit-maximizing level, the efficiency gain should be larger where margins are higher, that is, in less competitive markets. Both the efficiency and coordination channels thus predict larger price increases in less competitive markets.

Columns 1–3 of Panel D group listings by local competition: a listing is in a low-competition area if its ZIP code's pre-law average number of listings is below the sample median. Listings in less competitive areas raised ADR by 2.5 percent more than those in more competitive areas, with a weak decline in booked days but no corresponding revenue decline, consistent with both channels. Two pieces of evidence, however, cut against the coordination channel. First, Section 5 shows that the law increased market churn, which should make coordination harder. Second, Table A4 shows that price dispersion (measured by coefficient of variation) among listings within 0.3 miles of the focal listing did not change much after the law; if coordination among nearest competitors drove the ADR increase, we would instead expect dispersion to fall.

We also examine heterogeneity by price tier. If transparency makes it easier to benchmark against competitors, higher-priced listings should have less room, to raise prices, which predicts a larger ADR increase in the lower tier. Columns 4–6 of Panel D, where low price tier means a listing's average pre-law ADR falls below its ZIP code average, show no differential change between tiers, suggesting that price alone is not the only benchmark hosts use.

Reference Pricing. : We probe hosts' potential use of reference pricing with a revealed-preference approach. For each focal listing, we identify same-ZIP listings whose pre-law ADR falls in the same bin as the focal listing's post-law ADR ("reference listings") and test whether listings reference more similar competitors as price transparency increases. Hosts are more likely to choose reference listings like their own in size (e.g., number of bedrooms and bathrooms), quality (e.g., superhost status and star ratings), and host type (e.g., listing count and response rate). This does not produce price convergence or lower dispersion, however, because reference listings also raise prices after the law. Appendix B describes this analysis in detail.

Summary. The heterogeneous treatment effects documented above are difficult to reconcile with a demand-side-only account of the price increase. If the law operated purely by making consumers more attentive to cleaning fees, we would expect them to grow wary of high-fee listings and avoid them unless hosts lowered the cleaning fee and/or the all-in price—implying that listings with greater pre-law reliance on cleaning fees should show a different price response than those with less reliance. We find no such heterogeneity. Instead, the price increase is larger for higher-quality listings, for casual hosts, for listings that charged a lower cleaning fee than nearby competitors before the law, and for listings that faced less competition before the law. These results all point toward the supply side, suggesting either that the law changed hosts' pricing ability directly, or that

the demand-side change it induced interacts with underlying supply-side factors such as quality and competitive position. This motivates a closer look at the supply side.

The existing literature on drip and partitioned pricing has considered the supply side, but mainly through sellers' pricing incentives, namely how hosts reprice once consumer search and comparison behavior shifts, holding the set of listings fixed. In a free-entry market such as short-term rentals, however, the entry-exit margin may matter as well: to the extent the law eliminated a profit channel some hosts relied on to remain viable, it should have induced some listings to exit, a supply-side channel distinct from simple repricing. This distinction matters beyond explaining the price increase. Entry and exit affect not only the prices of surviving listings but also the choice set consumers face, and a pure price-effects analysis, which takes the set of listings as given, would miss this channel. Because entry and exit reshape the choice set independent of the price channel, they carry first-order welfare implications. This reasoning motivates anchoring our empirical analysis in a consumer welfare framework that captures both price and composition effects, rather than a price-effects analysis alone.

5 Effects of Price Transparency on Listing Supply

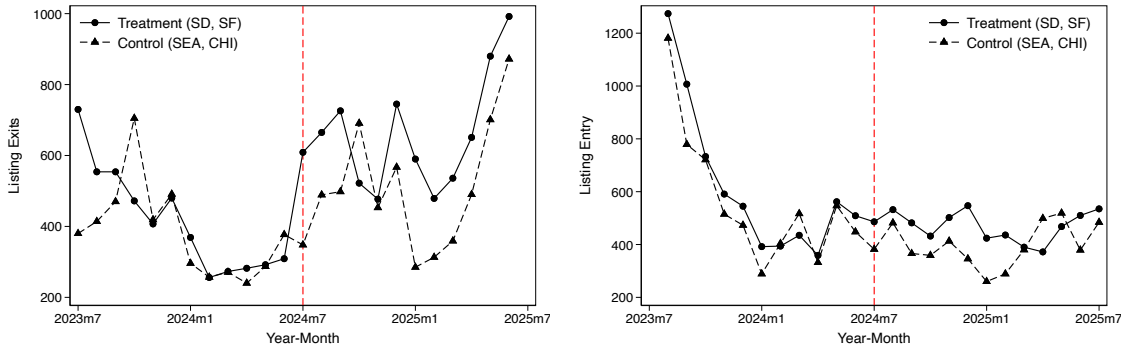
5.1 Aggregate Listing Supply

Raw Trends. Because the change in listing supply is the net of exits and entries, we plot their raw trends in Figure 5. Exits are defined by a listing's last occurrence month (except the final sample month) and entries by its first occurrence month (except the first sample month). Listings with some blocked months count as exits only if they remained inactive through the end of the sample. We include all observations, not just the CEM-matched treated and control listings.

Panel (a) shows that before the law (red dashed line), from late 2023 through mid-2024, total exits were broadly similar across treated and control cities. In the first month the law took effect, however, treated cities saw a sharp spike in exits relative to control cities, roughly doubling the previous month's count. Although control cities also saw more exits post-law, treated cities remained generally higher. This rise in exits was accompanied by more entries, as Panel (b) shows: treated cities generally had more entries than control cities, and the gap widened after the law. Combining the two margins, total listing supply declined in treated cities relative to control cities, as the increase in exits exceeded the increase in entries. These raw patterns suggest that the supply change in treated cities was driven mainly by increased exits rather than reduced entries, motivating a closer look at individual exits in Section 5.2.

Econometric Framework and Results. We next examine changes in ZIP code-level listing supply and prices after the law using a similar difference-in-differences framework. For ZIP code z in year-month t (calendar month m), the model is:

$$Y_{zt} = \mu_t + \delta_{zm} + \beta \, \text{Post}_t \times \text{CA}_z + \epsilon_{zt} \tag{3}$$



(a) Exits, Raw Trend

(b) Entry, Raw Trend

Figure 5: Changes of Listing Exits and Entry Over Time

Table 4: Effects of California's Price Transparency Law on Zipcode-Level Listing Supply and Prices

	(1) $\mathbb{1}\{\text{Exit}\}$	(2) $\mathbb{1}\{\text{Entry}\}$	(3) # of Exits	(4) # of Entries	(5) # of Listings	(6) Avg. ADR	(7) Weighted Avg. ADR
Post × CA	-0.005	0.001	0.250***	0.094	-0.016	0.028*	0.033**
	(0.008)	(0.008)	(0.071)	(0.074)	(0.014)	(0.015)	(0.014)
Dep. Var. Units	0/1	0/1	Log(#)	Log(#)	Log(#)	Log($)	Log($)
Dep. Var. Mean	0.934	0.938	2.445	2.467	4.137	5.653	5.595
Adj. R^2	0.674	0.711	0.844	0.836	0.985	0.958	0.952
N	3,661	3,661	2,620	2,712	3,661	3,661	3,655

Notes: Specifications include year-month fixed effects and zipcode-month fixed effects. An observation is a zipcode-year-month. Regressions include all observations in the sample. Dependent variables are: dummy of exit in Column 1, dummy of entry in Column 2, logged number of exits in Column 3, logged number of entries in Column 4, logged number of listings in Column 5, logged average ADR in Column 6, and logged average ADR weighted by number of booked days in Column 7, respectively for each zipcode-month. ADR, exit, and entry regressions are weighted by pre-policy average listing count per zipcode. Robust standard errors clustered at zipcode. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

where Y_{zt} denotes the dependent variable. Each observation a ZIP code-year-month. Regressions include all observations in the sample. Year-month fixed effects μ_t control for year-month trends, and ZIP code-month fixed effects δ_{zm} capture ZIP code-specific seasonality. The coefficient β represents the change in the outcome for treated relative to control ZIP codes after the law. Regressions are weighted by pre-law average ZIP code-level listing count, giving larger markets more weight in the average effects. ϵ_{zt} is an error term, and robust standard errors are clustered at the ZIP code level to allow arbitrary serial correlation within each ZIP code.

Table 4 presents the estimates. The dependent variables in Columns 1–7 are, respectively, a dummy for exit, a dummy for entry, logged number of exits, logged number of entries, logged listing supply, logged average ADR, and logged average ADR weighted by number of booked days per ZIP code-year-month. We find substantial changes on the intensive margin of exit and entry but none on the extensive margin. The number of exits rose by 25 percent in treated relative to control cities

Table 5: Changes of Zipcode-Level Listing Supply and Prices Following California's Price Transparency Law, DDD

	(1) Log(# of Listings)	(2) Log(Avg. ADR)	(3) Log(Weighted Avg. ADR)
Post × CA	-0.030	0.048***	0.054***
	(0.025)	(0.010)	(0.012)
Post × CA × Cleaning Fee Reliance	-0.109**	0.063***	0.071***
	(0.049)	(0.019)	(0.019)
Post × Cleaning Fee Reliance	0.053***	-0.010	-0.017**
	(0.020)	(0.007)	(0.007)
Dep. Var. Mean	4.137	5.653	5.595
Adj. R^2	0.985	0.961	0.955
N	3,661	3,661	3,655

Notes: Specifications include year-month fixed effects and zipcode-month fixed effects. An observation is a zipcode-year-month. Regressions include all observations in the sample. Dependent variables are logged number of listings per zipcode-month in Column 1, logged average ADR per zipcode-month in Column 2, and logged average ADR weighted by number of booked days per zipcode-month in Column 3, respectively. ADR regressions are weighted by pre-policy average listing count per zipcode. Cleaning fee reliance is defined as each zipcode's pre-policy average share of standardized daily cleaning fees in ADR, transformed to mean zero and unit standard deviation. Robust standard errors clustered at zipcode. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

after the law, alongside a statistically insignificant 9.4 percent increase in entries. Because the rise in exits exceeds that in entries, total listing supply per ZIP code declined by 1.6 percent, though this effect lacks statistical power. Average ADR rose by 2.8 to 3.3 percent depending on whether the ADR measure is weighted, consistent with the listing-level estimate in Table 2.

In Table 5, further interacting the treatment with pre-law average cleaning fee reliance shows that ZIP codes with greater reliance experienced both a substantially larger decline in total listing count and a substantially larger increase in average ADR after the law. Markets more exposed to the law thus saw greater supply contraction and stronger upward price pressure.

To illustrate the link between the ADR increase and the supply reduction, Figure A3 plots the raw changes in ADR and listing count before and after the law. Each point is a ZIP code in San Diego or San Francisco, with both axes in percent changes. The fitted line has a slope of -0.42 (standard error = 0.12), indicating a negative relationship at the ZIP code level: ZIP codes with larger declines in listing supply also saw larger ADR increases. This should not be read as the causal effect of supply changes on price, since supply and price change simultaneously and aggregate changes in listing count mask underlying compositional shifts. That said, the negative correlation is not strong enough to explain most of the price increase, since the change in total listing count is statistically insignificant (Table 4), and the heterogeneous effects in Table 3 suggest that incumbent listings may have repriced after the law without any change in listing count.

Taken together, the ZIP code-level analysis shows a large increase in exits and a smaller increase in entries in treated relative to control cities, yielding a moderate (and statistically insignificant) net decline in total listing supply, alongside an ADR increase that parallels our listing-level findings. We also find a negative relationship between changes in ADR and changes in listing supply in

treated cities. These patterns support the view that the broad price increase was partly related to heightened market churn, with the law playing a larger role in high-fee-reliance ZIP codes.

5.2 Listing Exits, Continuations, and Entries

Both Figure 5 and Table 4 suggest that supply changes in treated cities were driven mainly by increased exits. In this subsection, we examine exits at the listing level and ask which types of listings face higher exit risk after the price transparency law.

Comparing Exits, Continuations, and Entries. Table 6 compares listings across exits, continuations, and entries. Columns 1–4 are the treated and Columns 5–8 the control. For each, we report pre-law averages for exited and continuing listings and post-law averages for incumbent and new listings. We define exits as listings that stopped appearing on Airbnb after the law but before the end of our sample; continuing listings as those active through the end of the sample; new listings as those first appearing after the law; and incumbents as those that entered before the law.

Columns 1–2 and 5–6 compare exits and continuations on pre-law values. In both groups, exited listings generally had weaker performance, lower quality and thus lower ADR and lower cleaning fees than those that stayed.[34] The law, however, added a force making listings with higher cleaning fees and higher cleaning fee reliance more likely to exit, so exited listings in treated cities had higher cleaning fees and reliance on average than their control-city counterparts. Specifically, cleaning fee per booking is only \$0.15 lower among exited than continuing listings in the treated group (\$146.947 vs. \$147.095), compared to a nearly \$4 gap in the control group (\$109.265 vs. \$113.216). Similarly, while exited listings in both groups relied more on cleaning fees than continuing listings, the gap was 0.1 percentage points larger in the treated group (0.173 vs. 0.164) than in the control group (0.184 vs. 0.176). This difference is small but non-negligible; as the duration model shows later, cleaning fee reliance has sizable effects on exit risk in response to the law.

Columns 3–4 and 7–8 compare entries and incumbents on post-law values. Most quality metrics take time to accumulate—for instance, superhost eligibility requires at least 10 completed reservations, or 3 reservations totaling at least 100 nights, a bar new listings struggle to clear—so new listings generally have lower quality and lower prices than incumbents. However, post-law entrants in treated cities charged \$15 less in ADR than incumbents on average, about 1.7 times the \$9 gap in control cities, while their average overall rating was only 0.029 lower than incumbents' in treated cities, compared to 0.06 in control cities. Post-law entrants in treated cities were thus relatively more affordable and higher-quality than those in control cities, potentially expanding guests' options at better value. Cleaning fee charges also differed markedly: new listings charged \$11 *less* in cleaning fees than incumbents in treated cities but \$5 *more* in control cities. This carries through to reliance: it is 0.4 percentage points *lower* on average among new listings in the treated group but 1.7 percentage points *higher* in the control group.[35]

[34] The differences in quality and prices between exited and continuing listings are statistically significant, although we do not report the significance in Table 6.

[35] For distributions of cleaning fee and reliance across these listing types, see Figure A1.

Table 6: Comparing Exited, Continuing, and New Listings between Treated and Control Groups

	Treatment (SD, SF)				Control (SEA, CHI)			
	(1) Exited (Pre)	(2) Continuing (Pre)	(3) Incumbent (Post)	(4) New (Post)	(5) Exited (Pre)	(6) Continuing (Pre)	(7) Incumbent (Post)	(8) New (Post)
Cleaning Fee Per Booking ($)	146.947	147.095	150.503	139.725	109.265	113.216	111.847	116.793
	(115.189)	(103.122)	(105.700)	(114.422)	(83.657)	(80.940)	(77.867)	(86.072)
Cleaning Fee Reliance (0-1)	0.173	0.164	0.150	0.146	0.184	0.176	0.163	0.180
	(0.111)	(0.094)	(0.088)	(0.113)	(0.145)	(0.124)	(0.109)	(0.178)
Avg. Daily Rate (ADR, $)	329.710	334.723	375.011	359.539	223.154	241.922	258.703	249.494
	(274.440)	(249.043)	(280.013)	(268.173)	(172.824)	(176.918)	(190.556)	(175.027)
Per Night Rate (PNR, $)	281.068	286.617	326.572	311.470	187.336	203.905	220.940	209.533
	(248.405)	(225.492)	(254.881)	(244.233)	(156.458)	(159.538)	(172.860)	(159.361)
Bookings	4.071	4.954	4.927	3.799	4.329	4.960	5.215	4.239
	(3.117)	(3.178)	(3.104)	(3.058)	(3.221)	(3.310)	(3.297)	(3.200)
Booked Days	14.638	16.844	17.124	13.379	14.988	16.218	17.014	14.691
	(9.634)	(9.333)	(9.354)	(9.460)	(9.517)	(9.392)	(9.312)	(9.502)
Revenue ($)	4760.378	5418.108	6342.798	4777.685	3195.711	3736.032	4304.242	3550.089
	(5951.253)	(5324.222)	(6464.060)	(5480.767)	(3685.528)	(3598.373)	(4242.487)	(3711.201)
Superhost	0.475	0.585	0.645	0.390	0.515	0.618	0.642	0.388
	(0.499)	(0.493)	(0.479)	(0.488)	(0.500)	(0.486)	(0.479)	(0.487)
Host Experience	7.216	7.484	7.408	5.581	6.452	7.199	6.927	5.504
	(3.627)	(3.359)	(3.478)	(4.023)	(3.432)	(3.206)	(3.350)	(3.684)
Instant-Bookable	0.388	0.338	0.362	0.465	0.335	0.337	0.345	0.429
	(0.487)	(0.473)	(0.481)	(0.499)	(0.472)	(0.473)	(0.475)	(0.495)
Host Response Rate (0-100)	98.001	98.486	98.633	97.844	98.293	98.880	98.936	98.026
	(7.938)	(7.026)	(6.656)	(9.412)	(7.468)	(6.057)	(6.042)	(9.067)
Host Acceptance Rate (0-100)	92.414	93.569	92.958	89.604	94.301	96.402	95.612	92.999
	(16.813)	(15.027)	(15.769)	(20.742)	(11.922)	(9.432)	(10.604)	(15.186)
Reviews	51.584	87.963	101.709	9.328	50.554	84.820	103.768	9.805
	(86.157)	(119.395)	(129.764)	(25.123)	(77.428)	(114.719)	(123.807)	(32.996)
Overall Rating (0-5)	4.771	4.813	4.823	4.794	4.752	4.814	4.821	4.761
	(0.380)	(0.308)	(0.264)	(0.482)	(0.385)	(0.313)	(0.248)	(0.516)
Observations	30,822	86,982	106,056	24,657	29,238	81,259	99,863	23,808

Notes: This table shows pre-law averages for exited and continuing listings and post-law averages for incumbent and new listings. Cleaning fee reliance is defined as the share of standardized cleaning fees per night in ADR, shown here in raw (unstandardized) form.

Table 7: Effects of California's Price Transparency Law on Listing Exits

	Exponential		Weibull		Cox Proportional	
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.: Timing of Listing Exit	Hazard Ratio					
Post × CA	1.081**	1.084**	1.080**	1.083**	1.049	1.050
	(0.037)	(0.037)	(0.037)	(0.037)	(0.036)	(0.037)
Post × CA × Cleaning Fee Reliance		1.237***		1.232***		1.174***
		(0.045)		(0.044)		(0.041)
Post × Cleaning Fee Reliance		1.050*		1.051**		0.998
		(0.027)		(0.026)		(0.023)
CA × Cleaning Fee Reliance		0.888***		0.887***		0.874***
		(0.026)		(0.026)		(0.025)
Cleaning Fee Reliance		1.041**		1.038*		1.043**
		(0.021)		(0.020)		(0.020)
Superhost	0.527***	0.530***	0.532***	0.534***	0.574***	0.574***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)
Instant-Bookable	0.964*	0.967*	0.967*	0.970	1.001	1.003
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Host Response Rate (0-100)	0.991***	0.991***	0.991***	0.991***	0.993***	0.993***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Host Acceptance Rate (0-100)	0.997***	0.997***	0.997***	0.997***	0.999**	0.999***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Reviews	0.995***	0.995***	0.995***	0.995***	0.998***	0.998***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Overall Rating (0-5)	0.686***	0.694***	0.688***	0.696***	0.701***	0.705***
	(0.018)	(0.017)	(0.017)	(0.017)	(0.015)	(0.015)
Log-likelihood	-31,484	-31,364	-31,462	-31,348	-133,599	-133,525
N	393,117	393,093	393,117	393,093	393,117	393,093

Notes: Specifications control for time-varying listing attributes and include year-month fixed effects and city-month fixed effects. The dependent variable in the duration model reflects the timing of listing exit (i.e., the probability of listing exit at each point in time conditional on remaining active up to that point). Columns 1 to 2 assume an exponential distribution for the duration model, Columns 3 to 4 assume a Weibull distribution, and Columns 5 to 6 use a semiparametric Cox proportional hazard model. Cleaning fee reliance is defined using the average pre-policy share of standardized cleaning fees per night in ADR for each listing, transformed to mean zero and unit standard deviation. Robust standard errors are clustered at listing. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Taken together, in the treated group, listings that exited after the law charged higher cleaning fees and relied more on cleaning fee revenue than those that stayed, relative to the control group, while listings that entered charged lower cleaning fees and relied less on such revenue.

Individual Listing Exits. We next use a duration model to examine the law's effect on individual exits. Duration models handle censoring properly, so listings active through the end of the sample are not excluded, and by modeling the probability of exit in a given period conditional on surviving all previous periods, they capture whether a listing becomes more or less likely to exit the longer it stays. The analysis is at the listing-month level; the dependent variable is the probability of exiting in a given month conditional on staying through that month, capturing the timing of exit. We use a difference-in-differences specification similar to Equation (1), except that listing fixed effects are

infeasible in duration models. Because a listing's cleaning fee reliance may affect its exit decision in response to the law, we also estimate a triple-differences specification similar to Equation (2), using the same pre-law cleaning fee reliance measure from Section 4.2.

Table 7 presents estimated hazard ratios: a ratio above one indicates higher exit risk, below one lower risk. We consider several models: Columns 1–2 assume an exponential baseline hazard, Columns 3–4 a Weibull baseline hazard, and Columns 5–6 a Cox proportional hazards model that estimates the baseline hazard flexibly and nonparametrically. Columns 1, 3, and 5 report difference-in-differences estimates and Columns 2, 4, and 6 triple-differences estimates. Estimates are generally stable across specifications. Listings in treated cities were 4.9 to 8.1 percent more likely to exit early than those in control cities after the law, though statistical power falls under the Cox model. In both groups, a one-standard-deviation increase in cleaning fee reliance was associated with 3.8 to 4.3 percent higher exit risk, and the same increase raised exit risk by an additional 17.4 to 23.7 percent for treated relative to control listings after the law. Hazard ratios for listing quality (e.g., superhost status and overall star rating) are below one, so higher-quality listings are less likely to exit early. These estimates are consistent with the ZIP code-level results in Table 4 and the exit/continuation patterns in Table 6.

Compositional Changes. We further examine ZIP code-level compositional changes in price structure and listing attributes in Table 8, using a difference-in-differences framework similar to Table 4. From Columns 1 to 10, the dependent variables are logged average PNR, logged cleaning fee per reservation, cleaning fee reliance, share of zero-cleaning-fee listings, overall rating, superhost rate, host response rate, instant booking rate, logged number of reviews, and average listing age measured by first review date. Panel A reports simple averages per ZIP code-month; Panel B reports averages weighted by number of booked days per ZIP code-month.

Decomposing ADR into PNR and cleaning fee, we find a 3.8 to 4.4 percent increase in PNR alongside little change in cleaning fee, yielding a 0.6 percentage point reduction in cleaning fee reliance (the share of cleaning fee in total price). Average listing quality also improved: the average superhost rate rose by 3.2 to 4.3 percentage points, and average overall ratings and host response rates increased as well, though these estimates are statistically insignificant. This improvement reflects two factors: higher-quality listings are substantially less likely to exit, and although both entering and exiting listings tend to be lower-quality than incumbents, new entrants tend to be higher-quality than exiting listings. We also find that the average number of reviews declined by 3.9 to 4.7 percent and average listing age fell moderately, by a statistically insignificant 0.8 to 0.9 percent, reflecting an inflow of newer entrants with fewer accumulated reviews.

Table 8: Zipcode-Level Compositional Changes Following California's Price Transparency Law

Panel A: Simple Averages

	(1) PNR	(2) Cleaning Fee	(3) Cleaning Fee Reliance	(4) 0 Cleaning Fee	(5) Overall Rating	(6) Superhost Rate	(7) Response Rate	(8) Instantbook Rate	(9) Reviews	(10) Age
Post × CA	0.038**	-0.008	-0.006***	-0.005	0.004	0.032**	0.075	-0.001	-0.047***	-0.009
	(0.015)	(0.016)	(0.002)	(0.005)	(0.005)	(0.015)	(0.069)	(0.011)	(0.017)	(0.013)
Dep. Var. Units	Log($)	Log($)	0-1	0-1	0-5	0-1	0-100	0-1	Log(#)	Log(Yrs)
Dep. Var. Mean	5.490	4.828	0.165	0.063	4.804	0.576	98.532	0.356	4.303	1.045
Adj. R^2	0.960	0.924	0.795	0.884	0.735	0.753	0.530	0.832	0.960	0.917
N	3,661	3,652	3,661	3,661	3,530	3,530	3,516	3,530	3,528	3,518

Panel B: Weighted Averages by Number of Booked Days

	(1) PNR	(2) Cleaning Fee	(3) Cleaning Fee Reliance	(4) 0 Cleaning Fee	(5) Overall Rating	(6) Superhost Rate	(7) Response Rate	(8) Instantbook Rate	(9) Reviews	(10) Age
Post × CA	0.044***	0.005	-0.006**	-0.010**	0.003	0.043**	0.069	0.006	-0.039**	-0.008
	(0.014)	(0.014)	(0.002)	(0.004)	(0.005)	(0.017)	(0.055)	(0.012)	(0.018)	(0.015)
Dep. Var. Units	Log($)	Log($)	0-1	0-1	0-5	0-1	0-100	0-1	Log(#)	Log(Yrs)
Dep. Var. Mean	5.448	4.817	0.173	0.053	4.818	0.617	98.899	0.366	4.450	1.059
Adj. R^2	0.954	0.925	0.747	0.895	0.705	0.709	0.452	0.781	0.944	0.876
N	3,655	3,646	3,655	3,655	3,522	3,522	3,508	3,522	3,520	3,510

Notes: Specifications include year-month fixed effects and zipcode-month fixed effects. An observation is a zipcode-year-month. Regressions include all observations in the sample. Dependent variables are: logged average PNR in Column 1, logged cleaning fee per reservation in Column 2, cleaning fee reliance in Column 3, share of zero cleaning fee listings in Column 4, overall rating in Column 5, superhost rate in Column 6, host response rate in Column 7, instant booking rate in Column 8, logged number of reviews in Column 9, and average listing age measured by the first review date in Column 10. Regressions are weighted by pre-policy average listing count per zipcode. Robust standard errors clustered at zipcode. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Combining the ZIP code- and listing-level analyses, the law is associated with an increase in exits, a smaller increase in entries, a slight net decline in total listing supply, and an increase in average ADR, with the supply and ADR effects substantially larger in ZIP codes that relied more on cleaning fee revenue. Given the negative correlation between changes in listing supply and changes in average ADR, it is plausible that market churn and the resulting compositional changes reshaped local competition and contributed to the broad post-law ADR increase. That said, quantifying how market churn and the price increase jointly affect consumer welfare requires a structural demand model, since consumer welfare depends on both consumers' perceived price sensitivity under non-transparent pricing and their actual price sensitivity revealed after all-in pricing.

6 Effects of Price Transparency on Consumer Welfare

In this section, we estimate models of guest demand in the short-term lodging market and use the estimates to compute welfare changes for different types of consumers under non-transparent and transparent pricing.

6.1 Demand Estimation

We define a market as a ZIP code-month. Market size M_{zt} for ZIP code z in month t is set equal to the total number of booked listing-nights across all Airbnb and Vrbo entire-home listings in that ZIP code-month. A listing's share s_{jt} is its reserved nights divided by M_{zt}, and the outside share $s_{0t} = 1 - \sum_j s_{jt}$ is the fraction of Vrbo's market share. As in the reduced-form analysis, we focus only on entire-home listings. Each prospective guest i chooses a short-term lodging product j in ZIP code z and year-month t (calendar month m) to maximize her utility $u_{ijt} = \delta_{jt} + \epsilon_{ijt}$.

We first consider a logit demand model, where ϵ_{ijt} follows a Type 1 extreme value distribution. Following Berry (1994), we have:

$$\delta_{jt} \equiv \ln(s_{jt}) - \ln(s_{0t}) = \lambda_j + \mu_t + \gamma_{zm} + \theta \text{Post}_t \times \text{CA}_j + \boldsymbol{\alpha} \ln(\text{ADR}_{jt}) + \boldsymbol{\beta} \text{Quality}_{jt} + \xi_{jt} \quad (4)$$

where s_{jt} and s_{0t} denote the market shares of product j and the outside good in year-month t, respectively. As in the reduced-form specifications, we include listing fixed effects λ_j to control for each Airbnb listing's time-invariant attributes, year-month fixed effects μ_t to control for demand shocks common to all four sampled cities, and ZIP code by calendar month fixed effects γ_{zm} to control for ZIP code-specific seasonality. The DID term of $\text{Post}_t \times \text{CA}_j$ captures the effect of the law on the constant term, where Post_t is a dummy indicating post-law periods and CA_j is a dummy indicating whether listing j locates within California. The stand alone terms of $Post_t$ and CA_j are omitted because they are collinear with year-month fixed effects and listing fixed effects. ξ_{jt} represents utility derived from listing characteristics observed by consumers but not observed by econometricians. $\ln(\text{ADR}_{jt})$ represents log ADR. Since overall star rating is a comprehensive measure of listing quality, we define Quality_{jt} as a dummy for whether a listing's overall star rating

is above the sample median (4.9). In the estimation, we demean $\ln(\text{ADR}_{jt})$ and Quality_{jt} using respective sample mean so that the coefficient on $\text{Post}_t \times \text{CA}_j$ can be interpreted as the effect on a listing with average ADR and average quality, but demeaning does not change the coefficients on $\ln(\text{ADR}_{jt})$, Quality_{jt}, and their interaction terms. To allow price elasticity and consumer preference for high-quality listings to vary after the law took effect, we define:

$$\begin{aligned} \boldsymbol{\alpha} &= \alpha_0 + \alpha_1 \text{Post}_t + \alpha_2 \text{CA}_j + \alpha_3 \text{Post}_t \times \text{CA}_j \\ \boldsymbol{\beta} &= \beta_0 + \beta_1 \text{Post}_t + \beta_2 \text{CA}_j + \beta_3 \text{Post}_t \times \text{CA}_j. \end{aligned} \tag{5}$$

Generally, consumers prefer lower prices and higher quality, so $\alpha_0 < 0$ and $\beta_0 > 0$. Beyond this, the drip and partitioned pricing literature predicts that consumers should be more price-sensitive under all-in pricing, implying $\alpha_3 < 0$. Meanwhile, our reduced-form estimates suggest that consumers may also pay more attention to non-price attributes once obscurity in total price is removed, implying an increased preference for listing quality and thus $\beta_3 > 0$. We are agnostic about the coefficients for the stand-alone terms of $Post_t$ (α_1 and β_1), as they may capture nationwide changes over time rather than impacts of the law or changes specific to California listings.

Following Culotta et al. (2026), we use two instruments for ADR to address potential endogeneity. The first is the average price of private-room listings in the focal listing's ZIP code in a given month. Guests of private-room and entire-home listings differ fundamentally, but both types face similar local cost shocks; we call these the Private Room instruments ("PR" in the table). The second is Zillow's home value index (ZHVI) by ZIP code-month, which captures supply-side factors such as local property taxes and maintenance costs. Because short-term rental units are a tiny share of a city's housing stock,[36] their potential impact on ZHVI should be minimal. We instrument with 12- and 18-month lags of log ZHVI rather than contemporaneous values. These long lags serve two purposes: they precede the booking outcomes they instrument for, easing concerns that short-term rental activity itself affects local home values, and they match the horizon over which hosts set posted rates, since calendar prices are fixed in advance (up to 24 months on Airbnb). We call these the ZHVI instruments. Because both private-room prices and ZHVI are measured by ZIP code-year-month, we interact them with a listing's basic attributes (number of bedrooms and reviews) to construct property-specific instruments, which we apply to all interaction terms with log(ADR).

In addition to the simple logit, we consider a nested logit model, where a guest first chooses between Airbnb and the outside good, then chooses a listing within Airbnb. This model allows more flexible substitution patterns among Airbnb listings than between Airbnb and the outside good. Utility is defined as $u_{ijt} = \delta_{jt} + \zeta_{igt} + (1-\sigma)\epsilon_{ijt}$, where ζ_{igt} follows the unique distribution such that $\zeta_{igt} + (1-\sigma)\epsilon_{ijt}$ also follows a Type 1 extreme value distribution. The inverse market share function is given by:

$$\ln(s_{jt}) - \ln(s_{0t}) = \lambda_j + \mu_t + \gamma_{zm} + \theta \text{Post}_t \times \text{CA}_j + \boldsymbol{\alpha} \ln(\text{ADR}_{jt}) + \boldsymbol{\beta} \text{Quality}_{jt} + \sigma \ln(s_{j/g,t}) + \xi_{jt} \tag{6}$$

[36] As reported in Culotta et al. (2026), Chicago has 1.26 million housing units in total but only 5,499 Airbnb listings in an average month between July 2015 and December 2019 (of which 3,420 are entire-home listings).

Table 9: Demand Estimates

Model:	Logit OLS	Logit 2SLS			Nested Logit 2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Instruments for Price:		PR IV	L.12 ZHVI +PR IV	L.18 ZHVI +PR IV	L.12 ZHVI +PR IV	L.18 ZHVI +PR IV
Log(ADR)	-1.051***	-6.642**	-4.695***	-3.076***	-4.807***	-3.697***
	(0.029)	(3.222)	(0.910)	(0.770)	(0.893)	(0.668)
Post × CA × Log(ADR)	0.005	-0.856	-0.524**	-0.255	-0.583**	-0.406*
	(0.028)	(0.635)	(0.261)	(0.227)	(0.254)	(0.214)
CA × Log(ADR)	-0.042	0.287	-0.390	-0.965	-0.263	-0.558
	(0.042)	(1.395)	(0.734)	(0.623)	(0.740)	(0.638)
Post × Log(ADR)	0.137***	1.434*	1.000***	0.616***	0.949***	0.660***
	(0.020)	(0.753)	(0.233)	(0.155)	(0.245)	(0.148)
Quality	-0.032	0.145	0.085**	0.033	0.094**	0.067*
	(0.023)	(0.109)	(0.043)	(0.035)	(0.044)	(0.037)
Post × CA × Quality	0.077***	0.236*	0.168***	0.114**	0.180***	0.143***
	(0.029)	(0.133)	(0.059)	(0.053)	(0.058)	(0.050)
Post × Quality	0.024	-0.288	-0.183***	-0.093**	-0.188***	-0.132***
	(0.021)	(0.180)	(0.056)	(0.043)	(0.055)	(0.043)
CA × Quality	0.007	-0.066	-0.033	-0.007	-0.045	-0.032
	(0.031)	(0.082)	(0.049)	(0.042)	(0.049)	(0.042)
Post × CA	0.014	0.049	0.081	0.117*	0.096	0.115*
	(0.016)	(0.101)	(0.073)	(0.061)	(0.073)	(0.062)
Nesting parameter					0.157	0.318
					(0.346)	(0.269)
Cragg-Donald Wald F stat.		5	46	32	11	11
N	168,085	168,085	168,085	168,085	168,085	168,085

Notes: A market is defined as a zipcode-month. Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and zipcode-month fixed effects. Regressions include CEM weights. Quality is measured based on whether the overall star rating is higher than the sample median (4.9 out of 5). Column 1 uses a logit model without instruments. Column 2 uses a logit model and instruments log(ADR) using log average ADR of private-room listings in a zipcode interacted with listing attributes. Columns 3 and 4 additionally include log Zillow Home Value Index (ZHVI) interacted with listing attributes as instruments, lagged 12 and 18 months respectively. Columns 5 and 6 use a nested logit model and additionally instruments the within-zipcode market shares using number of listings in a zipcode interacted with listing attributes. Columns 5 and 6 use 12- and 18-month lagged ZHVI instruments respectively. Robust standard errors are clustered at listing. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

where $s_{j/g,t}$ represents listing j's share of the inside good (all Airbnb listings) within a market in year-month t. The nesting parameter σ, which ranges from zero to one, captures how much closer substitutes Airbnb listings are to each other than to the outside good; the model collapses to a simple logit when $\sigma = 0$. Since the log within-market share $\ln(s_{j/g,t})$ may be endogenous, we instrument it with the number of Airbnb listings in the corresponding ZIP code-month interacted with average listing attributes in that ZIP code-month. Listing counts affect each listing's market share but should not affect consumer preference.

Table 9 presents the demand estimation results. Column 1 estimates a logit model without instruments, Columns 2–4 a logit model with instruments, and Columns 5–6 a nested logit model. Column 2 uses the private room instruments; Columns 3 and 5 use 12-month-lagged ZHVI together

with the private room instruments; and Columns 4 and 6 use 18-month-lagged ZHVI together with the private room instruments. For the nested logit model in Columns 5–6, we additionally instrument the log within-market share. In Column 1, without price instruments, the price coefficient is slightly larger than 1 in magnitude, the coefficient on $\text{Post}_t \times \text{CA}_j \times \ln(\text{ADR})_{jt}$ is close to zero, and the coefficient on $\text{Post}_t \times \text{CA}_j \times \text{Quality}_{jt}$ is positive and significant. Adding the private room instruments in Column 2 yields a much larger price coefficient in magnitude, but a small first-stage F-statistic. In Column 3, we additionally instrument ADR with the ZHVI variables to improve first-stage power. The price coefficient for California markets is estimated at -5.09 and grows larger to -5.57 after we incorporate the coefficient of $Post_t \times CA_j \times \ln(ADR)_{jt}$, implying that the law raised the average own-price elasticity in magnitude by approximately 10 percent relative to control cities.[37]

Turning to quality, the coefficient of quality also grows in magnitude post the law by the coefficient of $\text{Post}_t \times \text{CA}_j \times \text{Quality}_{jt}$, indicating that consumers value quality more in California after the law relative to control cities.[38]

At the same time, the positive (but insignificant) coefficient on $\text{Post}_t \times \text{CA}_j$ suggests that demand for Airbnb listings may have expanded relative to Vrbo among consumers facing more transparent prices. This is plausible: cleaning fees were far more common on Airbnb than on Vrbo, so consumers aware of the difference may have avoided Airbnb before the law. In Column 5, the nesting parameter is estimated at 0.157 and statistically insignificant. Instrumenting ADR with 18-month-lagged ZHVI variables produces similar estimates (Columns 4 and 6).

Given the first-stage strength and the statistical insignificance of the nesting parameter, we will use the logit estimates from Table 9 Column 3 for counterfactual analysis and welfare calculation.

These results are consistent with the prevailing view in the drip and partitioned pricing literature and with our reduced-form findings. Displaying an all-in price after the transparency law raises consumers' price sensitivity, as the full cost becomes salient upfront rather than understated by non-transparent pricing at the point of search. At the same time, because consumers value quality more after the law, high-quality listings can capture additional surplus by charging higher prices than low-quality listings, again consistent with our reduced-form analysis.

Two econometric considerations are worth noting. First, the procedure above uses the full sample to construct each listing-month's market share but estimates the model with CEM weights. Table A5 reports the demand estimates without CEM weights; they continue to show high price

[37] Using the coefficient combinations in Column 3, the California price coefficient moves from $\alpha_0+\alpha_2 = -5.09$ before the law to $\alpha_0 + \alpha_2 + \alpha_3 = -5.61$ under all-in pricing; the corresponding booked-night-weighted own-price elasticities, are -5.05 and -5.57 respectively. These calculations do not include α_1 (the coefficient of $Post_t \times \ln(ADR)_{jt}$) because it indicates a change common to both CA and non-CA cities. This implies that the estimated change of elasticity due to the law is *relative* to the control cities. It does not reflect the absolute change from before to after the law within California. We also note that the estimates reported here correspond to an average Airbnb listing's own-price demand elasticity holding rivals fixed. In comparison, the reduced-form effects on price and booked days presented in Section 4 reflect how an average existing Airbnb listing responded to a market-wide change by the law, and should not be used to compute demand elasticity.

[38] Similar to price, we do not use the coefficient of $Post_j \times Quality_{jt}$ (β_1 in Equation 5) in the calculation of how the law changes consumers' sensitivity to listing quality because this coefficient captures changes common to CA and non-CA cities after July 2024.

sensitivity among consumers booking in California. This sensitivity tends to rise after the law once we instrument for price endogeneity, though the increase is statistically insignificant. One explanation is that many high-ADR California listings cannot be matched with control listings in Seattle or Chicago, and this lack of comparability distorts the estimated changes in price sensitivity; focusing on CEM-matched listings helps minimize such distortion. Under the assumption that price transparency changes consumers' (perceived) preferences for price and quality, these changes should apply to all California listings. Because we construct each listing-month's market share from the full sample, restricting the demand estimation to CEM-matched observations allows us to capture these preference changes with greater econometric rigor.

The second issue is that the law applies to all short-term-rental providers in California, including Vrbo and hotels. Our demand system therefore does not explicitly model two potential channels. One is that overall market size—total booked listing-nights on Airbnb and Vrbo—may have changed because of the law, yet we take the observed market size, both before and after, as given. The other arises because our demand model focuses on the relative difference between each Airbnb listing and the outside good of Vrbo listings in the same ZIP code: to the extent the law affects the price, attributes, and availability of Vrbo listings, these effects may enter the error term and bias our estimates. To address this, we re-estimate demand with ZIP code-year-month fixed effects in place of separate ZIP code-calendar-month and year-month fixed effects. These finer fixed effects fully absorb any effect of the law on the utility consumers derive from Vrbo listings in each ZIP code-year-month (see Appendix C for details). We can still identify the key demand parameters because price, quality, and other attributes vary within individual Airbnb listings before and after the law, and our price instruments include interactions with individual non-price listing attributes. As Table A6 shows, we continue to find increased price sensitivity in California after the law, similar in magnitude to our baseline results in Table 9.

This comes at a cost, however. Because our instrument (before interacting with individual listing attributes) draws its identifying variation largely from the ZIP code-year-month dimension, these fixed effects absorb much of that variation and substantially weaken the first stage. They also prevent us from identifying the California-wide demand response to the law (i.e., the coefficient on Post×CA in Table 9).

Given this shortcoming, we do not rely on this exhaustive specification for estimation precision or welfare analysis. Instead, we turn to a reduced-form analysis of Vrbo to measure the law's effect on Vrbo directly. In Tables A7 and A8, we replicate the reduced-form analysis for Vrbo listings and find that the ADR of California Vrbo listings rose by only 1.8 percent after the law (compared with 4.8 percent for Airbnb listings) relative to the control group, with no significant changes in bookings, booked days, revenue, listing composition, or non-price attributes, though cleaning-fee revenue declined and the probability of zero bookings rose modestly. The law thus had a much smaller effect on Vrbo, likely because a greater share of Vrbo listings charged no cleaning fee before the law (roughly 20–27% for Vrbo vs. 5–6% for Airbnb) and Vrbo listings therefore had lower average cleaning fee reliance beforehand (12–16% for Vrbo vs. 15–18% for Airbnb). Although the welfare

consequences of these Vrbo changes are likely modest, we still account for them by incorporating the estimated post-law ADR increase (1.8 percent) for Vrbo listings into our all-in-price welfare calculations, since higher outside-good prices also reduce consumer surplus and thus including them yields a more realistic picture of consumers' post-law welfare.

6.2 Welfare Analysis

In demand estimation, we compare San Diego and San Francisco (treated cities) with Seattle and Chicago (control cities) to recover the changes in consumer preference (i.e., price sensitivity and quality valuation) due to California's price transparency law. In this subsection, we use demand parameters from the above treatment-control comparison to conduct counterfactual welfare analysis for the two California cities under different pricing regimes.

Illustration of Consumer Surplus Changes. We first illustrate the welfare change from non-transparent pricing to all-in pricing in Figure 6. By the demand estimates, consumers are more price-elastic under all-in pricing than under non-transparent pricing, so the demand curve flattens when the regime switches. The demand curve under all-in pricing can be viewed as the actual, or realized, demand curve reflecting consumers' true willingness to pay when fully informed of the total price upfront, while the curve under non-transparent pricing is the perceived demand curve consumers face when the true total cost is obscured. In this illustration, we assume hosts have the same flat supply curve before and after the law.

Before the law, perceived consumer surplus under non-transparent pricing is $A+C+D+E+F+G$, while the surplus consumers actually realize is $A+C+D-B$. Under all-in pricing, by contrast, the actual and perceived demand curves coincide (shown in blue). If the total price is unchanged, actual consumer surplus under all-in pricing is $A+C+D$. Comparing actual consumer surplus under non-transparent and all-in pricing, total consumer surplus rises by area B. This gain reflects the welfare change of type 3 guests—marginal consumers who would buy under non-transparent pricing but not under all-in pricing.[39] Price transparency keeps these marginal consumers from being lured in by misleadingly low upfront prices, making them better off.

A natural objection is that any confusion created by non-transparent pricing is short-lived: the guest sees the full all-in total at checkout before paying, so no one is truly misled at the moment of transaction, and the perceived–realized surplus gap should not count as a welfare change. We reconcile this in two steps. Conceptually, non-transparent pricing distorts decisions made upstream of payment that end-of-funnel revelation does not undo. Guests decide which listings to consider, rank, and pursue based on the salient per-night rate, having already incurred search costs and coordinated with a travel party; at checkout they see the all-in total for the one listing in the cart, but not the recomputed totals of the alternatives already discarded, and re-searching is costly. The cross-listing comparison that drove the choice was thus made on non-comparable prices, and clarification before payment corrects one option, not the comparison. Empirically, if confusion

[39] Under all-in price, type 3 guests will not buy inside good but may still buy outside good.

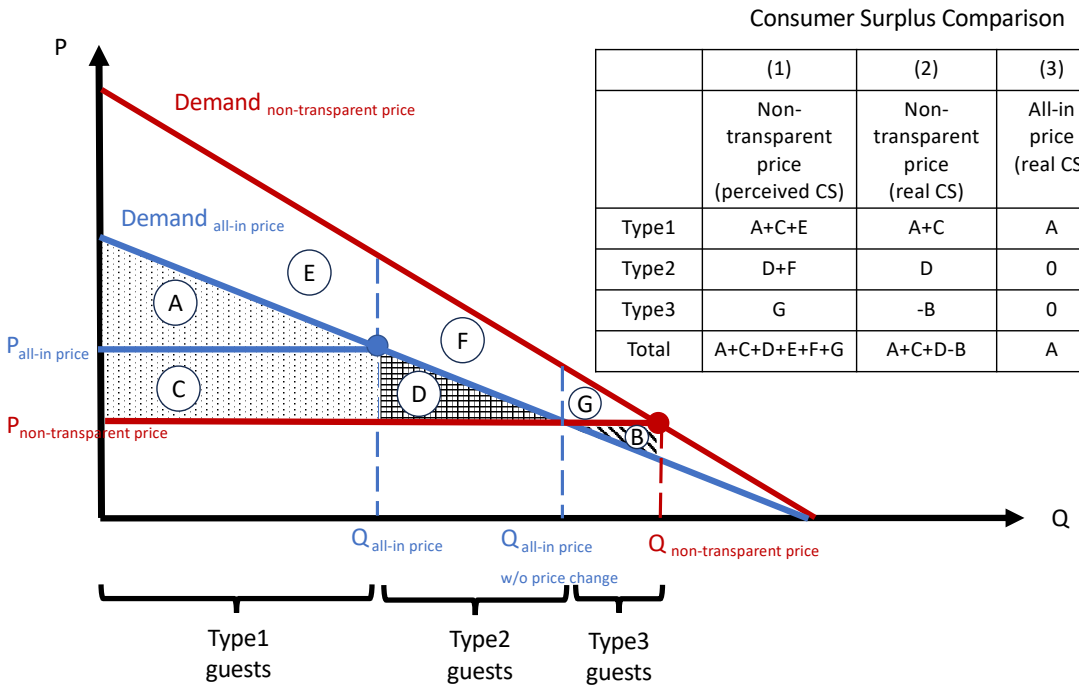
cleared fully and were inconsequential, price sensitivity would be unchanged after the law. As Table 9 shows, we instead find an 10 percent increase in own-price elasticity. Because this is revealed-preference evidence from realized bookings rather than a transient feeling, it indicates that the pre-law display materially shaped consumer choices.

Next, we consider the scenario where the total price may change under all-in pricing. Our reduced-form analysis shows the law is associated with an increase in ADR, so hosts' supply curve shifts up to the blue curve in the graph, reducing total consumer surplus from $A + C + D$ to A. As a result of the price increase, some guests no longer book; we call these type 2 guests, whose surplus falls from D to zero. Others continue booking but at a higher price; we call these type 1 guests, whose surplus falls from $A + C$ to A. The total surplus loss for type 1 and type 2 guests is thus $C + D$. Taken together, the change in total consumer surplus from non-transparent to all-in pricing is $B - C - D$, which can be positive or negative, so the welfare effect of price transparency is theoretically ambiguous. Although transparency lets marginal type 3 guests make better decisions by steering them away from purchases they should not make, the price increase it induces makes both type 2 guests (priced out) and type 1 guests (who still buy, at a higher price) worse off.

Note that the comparison in Figure 6 allows for price changes but does not incorporate changes in the total count or composition of listings, nor changes in guests' willingness to pay for quality. These factors may further shift the demand and supply curves in Figure 6. Fortunately, we can incorporate all of them in the counterfactual analysis, since they are embodied in the post-law data.

We emphasize that our normative conclusions do not rest on counting the perceived surplus. We report the perceived–realized gap (the "welfare illusion") as a diagnostic of how much the pre-law non-transparent pricing led consumers to overstate their surplus. The comparison underlying our conclusion is realized-versus-realized surplus, which values every stay at true willingness to pay and charges the true total paid, and so counts no illusion.

Counterfactual Analysis. We now describe how we calculate consumer welfare under six counterfactual regimes in California. In these analyses, when calculating inclusive values or normalizing welfare estimates into dollar terms, we use pre-law coefficients for non-transparent pricing regimes and post-law coefficients for all-in pricing regimes. Specifically, according to Equation 5, we compute pre-law price coefficient as $\alpha_0 + \alpha_2$ and pre-law quality coefficient as $\beta_0 + \beta_2$; we compute post-law price coefficient as $\alpha_0 + \alpha_2 + \alpha_3$ and post-law quality coefficient as $\beta_0 + \beta_2 + \beta_3$. We do not include the coefficients of the stand alone term of *Post* (α_1 and β_1 in Equation 5) because they reflect how consumer preferences change after July 1, 2024 for both CA and non-CA cities but our counterfactual analysis aims to capture the preference that consumers *would have before the law* should consumers face all-in pricing at that time in California. Since year-month fixed effects capture common time trends in both California and non-California cities (that are potentially unrelated to the law), we deal with them in a similar way: taking the coefficient of $\text{Post}_t \times \text{CA}_j$ as the effect of the law on the constant term in California, while keeping the pre-law year-month fixed effects unchanged and using the pre-law average as the value for post-law periods, which is analogous to



Consumer Surplus Comparison

	(1)	(2)	(3)	(4)
	Non-transparent price (perceived CS)	Non-transparent price (real CS)	All-in price (real CS)	ΔCS = (3)-(2)
Type1	A+C+E	A+C	A	-C
Type2	D+F	D	0	-D
Type3	G	-B	0	B
Total	A+C+D+E+F+G	A+C+D-B	A	B-C-D

Figure 6: Illustration: Consumer Surplus Under Non-transparent Price and All-in Price

including the pre-law trend but not the coefficient of *Post*.[40]

For Counterfactual I, namely consumer-perceived welfare under non-transparent pricing before the law (corresponding to $A + C + D + E + F + G$ in Figure 6), we use the pre-law parameter estimates from Column 3 of Table 9 to calculate expected utility δ_{jt} for each listing-month, then compute the inclusive value for each ZIP code-month, $I_{zt} = 1 + \sum \exp(\delta_{jt})$. Multiplying it by market size and normalizing it by the price coefficient yield perceived consumer surplus before the law. Because demand estimation uses logged ADR rather than ADR, we multiply this value by the average pre-law ADR in each ZIP code-month, p_{zt}, so the surplus can be read in dollar terms.

Similarly, we combine pre-law data with the estimated post-law demand parameters to simulate the utility that would have been realized under all-in pricing during the pre-law period ($\delta_{jt,real}$), and compute the corresponding inclusive value $I_{zt,real} = 1+\sum \exp(\delta_{jt,real})$. Scaling it by pre-law market size and normalizing it by the post-law price coefficient yields Counterfactual III: consumer surplus that would be realized had consumers faced the pre-law choice set but chosen listings according to post-law demand parameters, as though the market had adopted all-in pricing. This corresponds to $A + C + D$ in Figure 6.

While Counterfactuals I and III capture how consumer (perceived) utility and choice translate into (perceived) consumer surplus under non-transparent versus all-in pricing during the pre-law period, a crucial remaining question is what consumer surplus is actually *realized* under non-transparent pricing before the law (corresponding to $A + C + D - B$ in Figure 6). We call this

[40] ZIP code-month fixed effects are different for California and non-California cities and thus do not need to be addressed.

Counterfactual II. It differs from Counterfactual I because perceived surplus may exceed realized surplus, as non-transparent pricing creates an illusion of lower prices that makes consumers act as though less price-sensitive than they are. It differs from Counterfactual III because consumer choice here is driven by the perceived, rather than the real, demand curve.

Denoting market size as M_{zt}, and following the correction for perceived versus realized utility of Leggett (2002) as applied by Reimers and Waldfogel (2021), the change in consumer surplus in a market moving from non-transparent to all-in pricing is:

$$\Delta CS_{zt} = \frac{p_{zt} M_{zt}}{|\alpha|} \left[\ln(I_{zt,real}) - \ln(I_{zt}) + \sum_{j \in J_z} (\delta_{jt} - \delta_{jt,real}) s_{jt} \right] \tag{7}$$

where I_{zt} is the inclusive value based on perceived utility (δ_{jt}), as in Counterfactual I; $I_{zt,real}$ is the inclusive value based on realized utility ($\delta_{jt,real}$), as in Counterfactual III; and the term $\sum_{j \in J_z} (\delta_{jt} - \delta_{jt,real}) s_{jt}$ adjusts for the possibility that what is consumed under non-transparent pricing has an ex post utility differing from its ex ante value (corresponding to $E + F + G + B$ in Figure 6). This adjustment is weighted by s_{jt}, the market share consumers chose before the law. We normalize ΔCS_{zt} by the pre-law price coefficient. Because ΔCS_{zt} captures the difference between realized surplus under non-transparent pricing ($A + C + D - B$) and under all-in pricing ($A + C + D$), namely area B, we obtain Counterfactual II by subtracting ΔCS_{zt} from Counterfactual III.

Counterfactuals I, II and III assume every listing's price is unchanged when the pre-law world shifts from non-transparent to all-in pricing. But our reduced-form analysis shows that the ADR rose by 4.8 percent on average after the law, so for Counterfactual IV we impose a 4.8 percent ADR increase for all Airbnb listings present before the law and simulate market shares for each listing-month. Using post-law parameter estimates but pre-law data, Counterfactual IV reflects consumer surplus under all-in pricing had the pre-law world experienced the estimated price change. To account for the effect of the Vrbo price increase on consumer surplus, we use the reduced-form estimates for Vrbo listings from Table A7 and compute the change in consumer utility from a 1.8-percent ADR increase in the outside good (using the post-law price coefficient). We scale this term by pre-law market size, normalize it by the post-law price coefficient, and subtract it from the welfare calculation to obtain the final estimate for Counterfactual IV. This counterfactual represents area A in the graph.

In Counterfactual V, we compute consumer surplus under all-in pricing using *post-law* data. This differs from the pre-law all-in-pricing calculation with estimated price changes (Counterfactual IV) because it uses actual post-law data, in which the composition of active listings may have fully changed, not just prices. Building on Counterfactual V, Counterfactual VI hypothetically removes post-law entries from the post-law data; contrasting the two isolates the role of entries in shaping post-law welfare. As in Counterfactual IV, both V and VI account for the post-law ADR increase for Vrbo listings.

One issue is worth noting for the post-law welfare calculation: overall market size—total booked days on Airbnb plus Vrbo—may change for reasons unrelated to the law, yet absolute post-law

Table 10: Counterfactual Simulations and Consumer Welfare

Counterfactuals (areas in the graph, using pre/post data)	Estimate	95% CI
I. Non-transparent price, perceived ($A+C+D+E+F+G$, pre data)	\$565,071	[\$554,858, \$575,283]
II. Non-transparent price, realized ($A+C+D-B$, pre data)	\$487,712	[\$479,473, \$495,950]
III. All-in price without price change ($A+C+D$, pre data)	\$497,036	[\$488,596, \$505,476]
IV. All-in price with *estimated* price change (A, pre data)	\$429,978	[\$422,863, \$437,092]
V. All-in price with *actual* price change (post data)	\$573,728	[\$561,578, \$585,878]
- After excluding average market expansion effect	\$544,076	[\$532,554, \$555,598]
VI. All-in price with *actual* price change, minus entries (post data)	\$539,185	[\$528,163, \$550,208]
- After excluding average market expansion effect	\$511,318	[\$500,866, \$521,771]

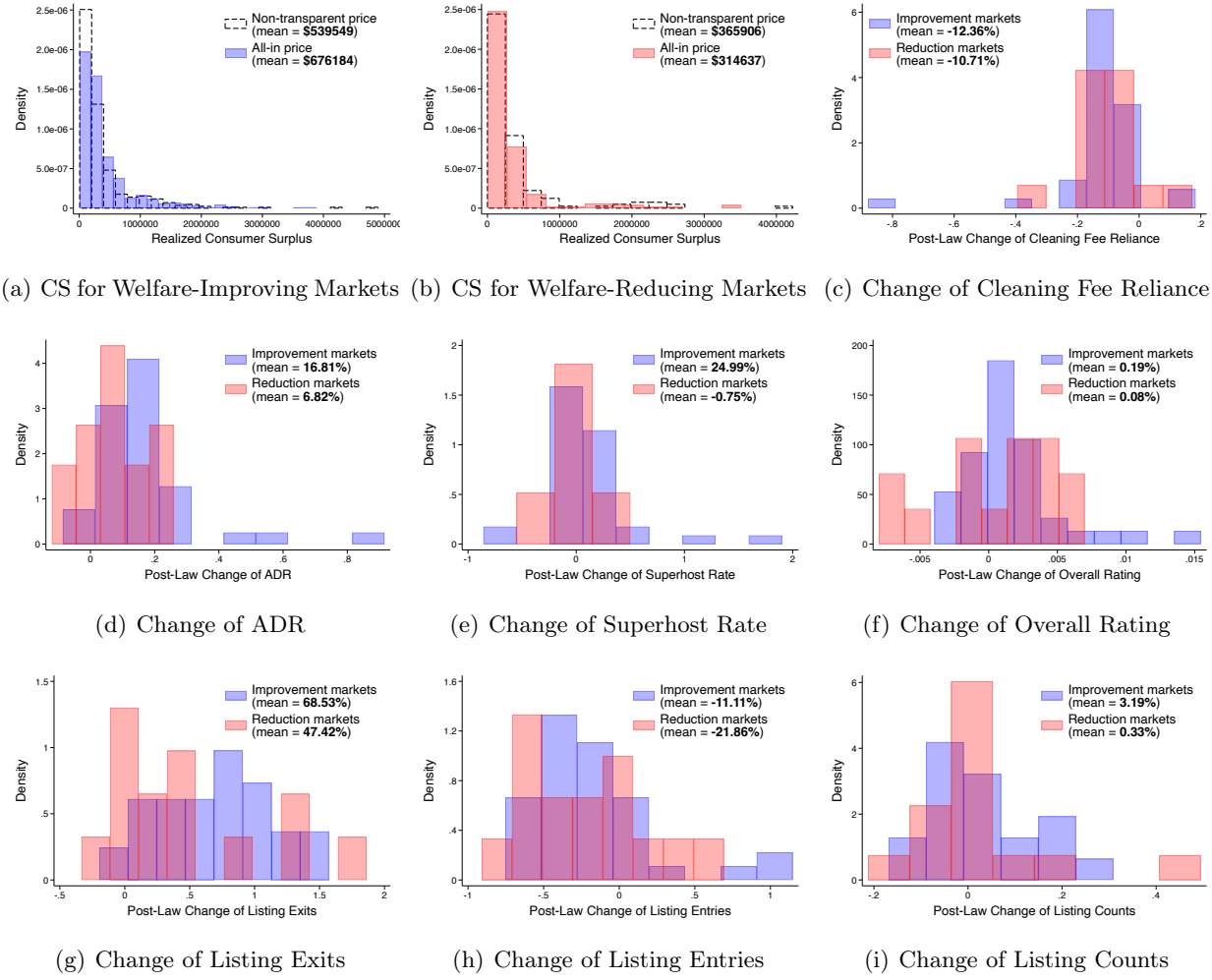
Notes: All estimates are in U.S. dollars. Welfare calculation includes all listings in San Diego and San Francisco. Counterfactual simulations are based on demand estimates from Column 3 of Table 9. Column 3 reports 95% confidence interval in brackets. To compute confidence intervals, we first calculate standard errors: we redraw 99% of the data at the zipcode-year-month level 500 times, rerun the counterfactual analysis for each redrawn sample, and report the standard deviation of the counterfactual estimates.

welfare is scaled by the market size observed after the law. Because we cannot tell how much of the market-size change the law caused, a conservative estimate should attribute none of the market-size-driven welfare change to the law. Hence, for Counterfactuals V and VI we also compute conservative post-law welfare estimates that exclude California's post-law market-size expansion.[41] For consistency, we normalize post-law consumer surplus to dollar terms using post-law ADR, since we use the post-law price to compute the corresponding inclusive value. Although a higher post-law ADR raises surplus through normalization, it also lowers surplus through the inclusive value, and so does not inflate post-law consumer surplus.

Table 10 presents consumer welfare estimates under all six counterfactuals, averaged across ZIP code-months. Column 1 identifies the counterfactual, Column 2 reports the estimated consumer welfare or welfare change in dollars, and Column 3 reports the 95% confidence interval in brackets. To compute confidence intervals, we redraw 99% of the data at the ZIP code-year-month level 500 times, rerunning the counterfactual analysis for each redrawn sample, and reporting the confidence interval of the resulting estimates.

Table 10 yields several findings. First, non-transparent pricing before the law induces a misleading overestimation of consumer welfare: perceived consumer welfare (Counterfactual I) is \$565,071, while realized consumer welfare (Counterfactual II) is only \$487,712, implying that the lack of transparency leads consumers to overestimate their welfare by \$77,359, or 15.86 percent. Second, holding prices fixed, all-in pricing generates higher realized consumer welfare (\$9,324) than before the law (Counterfactual III vs. Counterfactual II), a 1.91 percent increase. The small magnitude suggests

[41] This amounts to using pre-law market size rather than post-law market size.



(a) CS for Welfare-Improving Markets (b) CS for Welfare-Reducing Markets (c) Change of Cleaning Fee Reliance

(d) Change of ADR (e) Change of Superhost Rate (f) Change of Overall Rating

(g) Change of Listing Exits (h) Change of Listing Entries (i) Change of Listing Counts

Figure 7: Comparing Welfare-Improving and Welfare-Reducing Markets

Note: Panel (a) plots the distribution of CS by ZIP code-month for ZIP codes with higher average consumer welfare in the post-law period than before the law. Panel (b) shows the analogous plot for ZIP codes with lower average consumer welfare in the post-law period than before the law. Panels (c) through (i) plot the distributions of proportional changes in a set of variables for the two types of markets. We subtract each ZIP code's percent change in total bookings from its overall welfare change to set aside the market expansion effect.

that the direct benefit of transparency—helping marginal consumers avoid stays they should not book—is modest.[42] Third, the estimated price increase under all-in pricing (4.8 percent for Airbnb and 1.8 percent for Vrbo in Counterfactual IV) produces a welfare loss of $67,058, or 13.49 percent, relative to the no-price-change scenario (Counterfactual III). Decomposing this loss by guest type, type 1 guests bear $46,094 and type 2 guests $20,964, so type 1 guests, who continue to book on Airbnb at the higher price, account for more of the loss than type 2 guests, who are priced out of Airbnb.[43] Fourth, the welfare losses from the price increase exceed the gains from all-in pricing:

[42] By excluding demand expansion effect of all-in pricing (coefficient of $Post \times CA$) from calculation, gains for type 3 guests increase from $9,324 to $25,732.

[43] Because we define the market as booked nights on Airbnb and Vrbo, type 2 guests are those that are priced out

the $9,324 surplus increase for type 3 guests is smaller than the combined surplus reduction for type 1 and type 2 guests. As a result, the net welfare effect of all-in pricing with the estimated price increase (but without compositional change), given by area $B - C - D$, is -$57,734 per ZIP code-month on average.

As shown in Counterfactual V, actual post-law consumer welfare rises by $86,016 (from $487,712 to $573,728), or 17.64 percent, relative to before the law (Counterfactual II), and by $143,750 (from $429,978 to $573,728), or 33.43 percent, relative to all-in pricing with only the estimated price changes (Counterfactual IV).

Three factors may drive this gain. First, the entry of listings with lower prices and lower fee reliance (albeit lower quality) expands consumer's choice set. Second, the law leads consumers to value quality more and be more sensitive to price. This taste change, along with the exit of lower-quality, higher-fee-reliance listings, directs consumers towards higher-quality incumbents and lower-price entries. Third, the market for California short-term lodging has expanded over time. Our welfare calculation is scaled by observed market size before and after the law but the expansion is not necessarily driven by the law: average booked nights on Airbnb and Vrbo per ZIP code-month rose 5.45 percent in San Diego and San Francisco, similar to the increase in Seattle and Chicago (5.80 percent). Under the conservative assumption that all the observed expansion in San Francisco and San Diego is unrelated to the law and should be excluded from the estimated welfare gain, the net welfare change remains moderately positive relative to realized consumer surplus before the law (about 11.56 percent higher).[44]

Counterfactual VI highlights the role of entries in the welfare gain. Excluding post-law entries and assuming the entire market-expansion effect is unrelated to the law, post-law consumer surplus falls to $\$466,773$, about 4.29 percent below realized consumer surplus before the law (Counterfactual II). Together, the compositional shift from exits and entries offsets the negative effect of the price increase and moderately raises average (realized) consumer welfare relative to non-transparent pricing.

Although average consumer welfare rose overall after the law, some markets saw declines in consumer surplus, as Panel (b) of Figure 7 shows. In 15 such ZIP codes, average consumer surplus fell from $365,906 to $314,637. In the other 39 ZIP codes, which improved, Panel (a) shows average consumer surplus rising from $539,549 to $676,184. As before, we conservatively treat each ZIP code's post-law market expansion as unrelated to the law, subtracting each ZIP code's percent change in total bookings from its overall welfare change to set aside the expansion effect.

To better understand this heterogeneity, Panels (c) through (i) of Figure 7 plot the distributions of proportional changes in a set of variables across the two market types. Welfare-improving ZIP codes saw a larger reduction in average cleaning fee reliance after the law (-12.4% vs. -10.7% in

of Airbnb and end up booking with Vrbo. Vrbo is a much smaller short-term rental platform than Airbnb, thus it is not surprising that type 2 guests are fewer.

[44] After excluding the expansion effect from Counterfactual V ($573,728): $\frac{\$573,728}{1.0545} = \$544,076$. Comparing with realized consumer surplus before the law (Counterfactual II, $487,712), we obtain the percent change: $\frac{\$544,076-\$487,712}{\$487,712} = 0.1156$.

declining markets). Although they also saw a larger increase in average ADR (16.8% vs. 6.8%), the increase in average listing quality was even larger (a 25.0% increase in superhost rate vs. -0.8%). Welfare-improving markets saw a larger rise in exits and a smaller drop in entries than welfare-reducing markets, and on net a larger increase in listing counts. These patterns suggest that welfare heterogeneity is driven by the law's ripple effects through price changes and market churn, rather than by its direct effect of removing illusory low prices or raising consumer price sensitivity.[45]

Together, these findings imply that the consumer welfare effect of all-in pricing laws hinges critically on the supply-side response. The law's direct effects are mixed: transparency eliminates a large welfare illusion under non-transparent pricing but also triggers price increases that fall mainly on inframarginal guests and outweigh the direct gains from all-in pricing. What ultimately makes consumers better off is the compositional shift toward a mix of listings that is both more cost-effective (from entries) and higher-quality (from incumbents that choose to stay) after the law.

7 Conclusion

We provide evidence on the effects of a statewide all-in-pricing mandate in the U.S. short-term rental market. Using Airbnb listing data one year before and one year after California's short-term lodging price transparency law (AB 537) took effect, we compare listings in San Diego and San Francisco with comparable listings in Seattle and Chicago, which were not subject to a similar law until the final months of our sample.

Our findings both confirm and challenge the conventional wisdom in drip and partitioned pricing literature. First, all-in pricing raised consumers' own-price elasticity by approximately 10 percent and led them to weight quality more heavily in treated California cities relative to control cities—consistent with the view that transparent pricing makes total costs more salient, sharpening price sensitivity and shifting attention to non-price attributes. Second, all-in prices for California Airbnb listings rose by approximately 4.8 percent relative to control cities after the law, contradicting the view that upfront all-in prices should stimulate competition and lower equilibrium prices.

Besides price changes, we find significant market churn: listings with higher pre-law reliance on cleaning fees faced exit hazard rates 17 to 24 percent higher in treated than in control cities after the law; higher quality listings were more likely to stay; and new listings entered with lower price and lower quality. Moreover, entrants charged lower cleaning fees and drew less revenue from them than incumbents and exiting listings, marking a compositional shift in the listing pool toward hosts less reliant on obscured fees.

Our welfare analysis quantifies the net effect of price transparency on consumers, accounting for changes in preferences, prices, and market churn. The law corrects a welfare illusion under non-transparent pricing, generating gains for marginal consumers no longer lured into purchases they should not make on Airbnb. At the same time, the observed price increase (4.8 percent for Airbnb

[45] Indeed, when we decompose the welfare change from Counterfactual II (realized consumer surplus under non-transparent pricing before the law) to Counterfactual III (realized consumer surplus under all-in pricing before the law), every ZIP code shows some gain in consumer surplus.

and 1.8 percent for Vrbo listings) reduces consumer surplus, with the loss falling more on inframarginal guests who keep booking on Airbnb at higher prices than on guests priced out of Airbnb toward Vrbo. Netting these effects, all-in pricing with the observed price increase would, on average, reduce consumer welfare. However, compositional change in the pool of Airbnb listings—driven by both exits and entries—generates a welfare gain that more than offsets the price-induced losses. As a result, realized consumer surplus after the law exceeds that under the pre-law non-transparent pricing regime on average. These welfare changes are distributed heterogeneously across ZIP codes, depending on price changes and market churn.

Our study is subject to several caveats. Because of confounding policies and data limitations, we study two treated California areas over one year following the law, leaving open how the findings generalize to longer horizons or other markets. Our structural model also focuses on the demand side and makes no assumptions about how listings adjust prices after the law. Indeed, our reduced-form results suggest hosts may have changed pricing strategy not because consumers became more price sensitive, which would predict price reductions, but because transparency may help hosts better observe rivals' prices and price more efficiently. Some evidence is inconsistent with greater price coordination among competing listings after the law, but why hosts reprice remains an open question for future work.

Another caveat is that we observe an expansion in total short-term-rental bookings (Airbnb plus Vrbo) across all four sampled cities, but we cannot tell whether, or to what extent, it is related to AB 537; our welfare comparison conservatively excludes all of it from the law's effects. We also lack data on hotels, which fall under the same California law and can account for a substantial share of short-term lodging, especially in downtown ZIP codes. Further analysis is warranted to understand the law's true impact on total market size across all types of short-term lodging.

References

Abraham, Ajay T. and Rebecca W. Hamilton (2018) "When Does Partitioned Pricing Lead to More Favorable Consumer Preferences?: Meta-Analytic Evidence," *Journal of Marketing Research*, 55 (5), 686–703.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel (2015) "Regulating Consumer Financial Products: Evidence from Credit Cards," *The Quarterly Journal of Economics*, 130 (1), 111–164.

Althenayyan, Abdullah and Vicki G. Morwitz (2026) "The Impact of Surcharges on Consumers: The Role of Consideration Set Formation," *Working paper*, Columbia University.

Assad, Stephanie, Robert Clark, Daniel Ershov, and Lei Xu (2024) "Algorithmic Pricing and Competition: Empirical Evidence from the German Retail Gasoline Market," *Journal of Political Economy*, 132 (3), 723–771.

Bao, Ying, Limin Fang, and Matthew Osborne (2026) "The Effect of Quality Disclosure on Firm Entry and Exit Dynamics: Evidence from Online Review Platforms," *SSRN Working Paper #4804928*.

Barron, Kyle, Edward Kung, and Davide Proserpio (2021) "The Effect of Home-Sharing on House Prices and Rents: Evidence from Airbnb," *Marketing Science*, 40 (1), 23–47.

Berry, Steven T. (1994) "Estimating Discrete-Choice Models of Product Differentiation," *The RAND Journal of Economics*, 25 (2), 242–262.

Bertini, Marco and Luc Wathieu (2008) "Research Note—Attention Arousal Through Price Partitioning," *Marketing Science*, 27 (2), 236–246.

Bibler, Andrew J., Keith F. Teltser, and Mark J. Tremblay (2025) "How Do Short-Term Rental Regulations Affect Market Outcomes? Evidence from San Francisco's Airbnb Registration Requirement," *Real Estate Economics*, 53 (4), 841–868.

Blake, Tom, Sarah Moshary, Kane Sweeney, and Steven Tadelis (2021) "Price Salience and Product Choice," *Marketing Science*, 40 (4), 619–636.

Bradley, Sebastien and Naomi E. Feldman (2020) "Hidden Baggage: Behavioral Responses to Changes in Airline Ticket Tax Disclosure," *American Economic Journal: Economic Policy*, 12 (4), 58–87.

Brown, Jennifer, Tanjim Hossain, and John Morgan (2010) "Shrouded Attributes and Information Suppression: Evidence from the Field," *The Quarterly Journal of Economics*, 125 (2), 859–876.

Calder-Wang, Sophie and Gi Heung Kim (2025) "Algorithmic Pricing in Multifamily Rentals: Efficiency Gains or Price Coordination?" *SSRN Working Paper No. 4403058*, https://ssrn.com/abstract=4403058, Revise and resubmit, Journal of Political Economy.

Chakravarti, Dipankar, Rajan Krish, Pallab Paul, and Joydeep Srivastava (2002) "Partitioned Presentation of Multicomponent Bundle Prices: Evaluation, Choice and Underlying Processing Effects," *Journal of Consumer Psychology*, 12 (3), 215–229.

Chetty, Raj, Adam Looney, and Kory Kroft (2009) "Salience and Taxation: Theory and Evidence," *American Economic Review*, 99 (4), 1145–1177.

Christensen, Hans B., Eric Floyd, and Mark Maffett (2020) "The Only Prescription Is Transparency: The Effect of Charge-Price-Transparency Regulation on Healthcare Prices," *Management Science*, 66 (7), 2861–2882.

Cui, Ruomeng, Jun Li, and Dennis J. Zhang (2020) "Reducing Discrimination with Reviews in the Sharing Economy: Evidence from Field Experiments on Airbnb," *Management Science*, 66 (3), 1071–1094, 10.1287/mnsc.2018.3273.

Culotta, Aron, Ginger Zhe Jin, Yidan Sun, and Liad Wagman (2026) "Safety Reviews on Airbnb: An Information Tale," *Marketing Science*, 45 (2), 280–317.

DellaVigna, Stefano (2009) "Psychology and Economics: Evidence from the Field," *Journal of Economic Literature*, 47 (2), 315–372.

Edelman, Benjamin, Michael Luca, and Dan Svirsky (2017) "Racial Discrimination in the Sharing Economy: Evidence from a Field Experiment," *American Economic Journal: Applied Economics*, 9 (2), 1–22, 10.1257/app.20160213.

Ellison, Glenn and Sara Fisher Ellison (2009) "Search, Obfuscation, and Price Elasticities on the Internet," *Econometrica*, 77 (2), 427–452.

Farronato, Chiara and Andrey Fradkin (2022) "The Welfare Effects of Peer Entry: The Case of Airbnb and the Accommodation Industry," *American Economic Review*, 112 (6), 1782–1817.

Fradkin, Andrey and David Holtz (2023) "Do Incentives to Review Help the Market? Evidence from a Field Experiment on Airbnb," *Marketing Science*, 42 (5), 853–865, 10.1287/mksc.2023.1439.

Gabaix, Xavier and David Laibson (2006) "Shrouded Attributes, Consumer Myopia, and Information Suppression in Competitive Markets," *The Quarterly Journal of Economics*, 121 (2), 505–540.

Greenleaf, Eric A., Eric J. Johnson, Vicki G. Morwitz, and Edith Shalev (2016) "The Price Does Not Include Additional Taxes, Fees, and Surcharges: A Review of Research on Partitioned Pricing," *Journal of Consumer Psychology*, 26 (1), 105–124.

Grennan, Matthew and Ashley Swanson (2020) "Transparency and Negotiated Prices: The Value of Information in Hospital-Supplier Bargaining," *Journal of Political Economy*, 128 (4), 1234–1268.

Hortaçsu, Ali and Chad Syverson (2004) "Product Differentiation, Search Costs, and Competition in the Mutual Fund Industry: A Case Study of S&P 500 Index Funds," *The Quarterly Journal of Economics*, 119 (2), 403–456.

Hossain, Tanjim and John Morgan (2006) "... Plus Shipping and Handling: Revenue (Non) Equivalence in Field Experiments on eBay," *The B.E. Journal of Economic Analysis & Policy*, 6 (2), 1–30.

Huang, Yufeng (2025) "Pricing Frictions and Platform Remedies: The Case of Airbnb," *Working Paper*, https://ssrn.com/abstract=3767103, SSRN No. 3767103.

Huck, Steffen, Gabriele K. Lünser, and Jean-Robert Tyran (2016) "Price Competition and Reputation in Markets for Experience Goods: An Experimental Study," *The RAND Journal of Economics*, 47 (1), 99–117.

Iacus, Stefano M., Gary King, and Giuseppe Porro (2012) "Causal Inference without Balance Checking: Coarsened Exact Matching," *Political Analysis*, 20 (1), 1–24.

Inderst, Roman and Martin Obradovits (2023) "Excessive Competition on Headline Prices," *International Economic Review*, 64 (2), 783–808.

Jin, Ginger Zhe, Liad Wagman, and Mengyi Zhong (2024) "The Effects of Short-Term Rental Regulation: Insights from Chicago," *International Journal of Industrial Organization*, 96, 103087.

Kim, Yongseok, Davide Proserpio, and Suman Basuroy (2020) "Sleeping with Strangers: Estimating the Impact of Airbnb on the Local Economy," in *Proceedings of the European Marketing Academy*, 49.

Lanfear, Charles C. and David S. Kirk (2024) "The Promise and Perils of the Sharing Economy: The Impact of Airbnb Lettings on Crime," *Criminology*, 10.1111/1745-9125.12383, Published online.

Leggett, Christopher G. (2002) "Environmental Valuation with Imperfect Information The Case of the Random Utility Model," *Environmental and Resource Economics*, 23, 343–355.

Luco, Fernando (2019) "Who Benefits from Information Disclosure? The Case of Retail Gasoline," *American Economic Journal: Microeconomics*, 11 (2), 277–305.

Mahoney, Neale (2025) "Why Regulate Junk Fees?" *Journal of Economic Perspectives*, 39 (4), 203–220.

Morwitz, Vicki G., Eric A. Greenleaf, and Eric J. Johnson (1998) "Divide and Prosper: Consumers' Reactions to Partitioned Prices," *Journal of Marketing Research*, 35 (4), 453–463.

Ozer, Gorkem Turgut, Brad N. Greenwood, and Anandasivam Gopal (2024) "Noisebnb: An Empirical Analysis of Home-Sharing Platforms and Residential Noise Complaints," *Information Systems Research*, 35 (4), 1824–1847.

Reimers, Imke and Joel Waldfogel (2021) "Digitization and Pre-purchase Information: The Causal and Welfare Impacts of Reviews and Crowd Ratings," *American Economic Review*, 111 (6), 1944–1971.

Rhodes, Andrew (2023) "A Survey on Drip Pricing and Other False Advertising," TSE Working Paper 23-1434, Toulouse School of Economics, Available at SSRN 4430453.

Santana, Shelle, Steven K. Dallas, and Vicki G. Morwitz (2020) "Consumer Reactions to Drip Pricing," *Marketing Science*, 39 (1), 188–210.

Seim, Katja and Maria Ana Vitorino (2025) "Drip Pricing When Consumers Have Limited Foresight: Evidence from Driving School Fees," *RAND Journal of Economics*, 56 (4), 444–475.

Sullivan, Mary W. (2017) "Economic Analysis of Hotel Resort Fees," economic issues paper, Federal Trade Commission, Bureau of Economics, https://www.ftc.gov/system/files/documents/reports/economic-analysis-hotel-resort-fees/p115503_hotel_resort_fees_economic_issues_paper.pdf.

Taubinsky, Dmitry and Alex Rees-Jones (2018) "Attention Variation and Welfare: Theory and Evidence from a Tax Salience Experiment," *The Review of Economic Studies*, 85 (4), 2462–2496.

Tran, Kevin Ducbao (2020) "Partitioned Pricing and Consumer Welfare," Discussion Paper 1888, DIW Berlin, Available at SSRN 3676900.

Tran, Kevin Ducbao, Leonardo Madio, Michelangelo Rossi, and Mark J. Tremblay (2025) "Cleanin' It Up: Unshrouding Hidden Fees on a Peer-to-Peer Platform," *SSRN Working Paper No. 5642430*, https://ssrn.com/abstract=5642430, Earlier version: CESifo Working Paper No. 11574, 2024.

Whaley, Christopher, Timothy T. Brown, and James C. Robinson (2019) "Consumer Responses to Price Transparency Alone versus Price Transparency Combined with Reference Pricing," *American Journal of Health Economics*, 5 (2), 227–249.

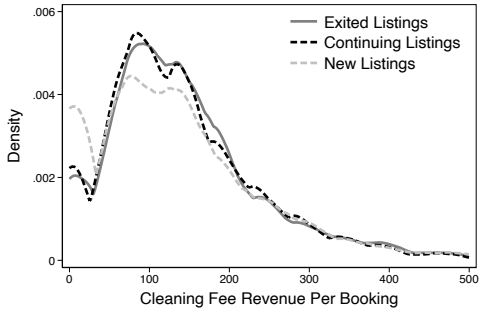
Whaley, Christopher M. (2019) "Provider Responses to Online Price Transparency," *Journal of Health Economics*, 66, 241–259.

Xia, Lan, Kent B. Monroe, and Jennifer L. Cox (2004) "The Price Is Unfair! A Conceptual Framework of Price Fairness Perceptions," *Journal of Marketing*, 68 (4), 1–15.

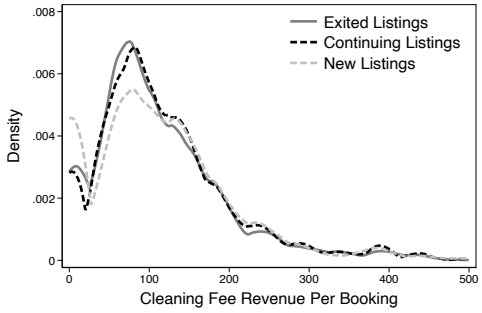
Zervas, Georgios, Davide Proserpio, and John W. Byers (2017) "The Rise of the Sharing Economy: Estimating the Impact of Airbnb on the Hotel Industry," *Journal of Marketing Research*, 54 (5), 687–705.

Zhang, Shunyuan, Nitin Mehta, Param Vir Singh, and Kannan Srinivasan (2021) "Frontiers: Can an Artificial Intelligence Algorithm Mitigate Racial Economic Inequality? An Analysis in the Context of Airbnb," *Marketing Science*, 40 (5), 813–820, 10.1287/mksc.2021.1295.

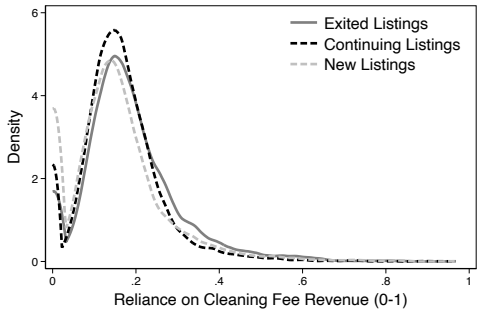
A Supplemental Figures and Tables



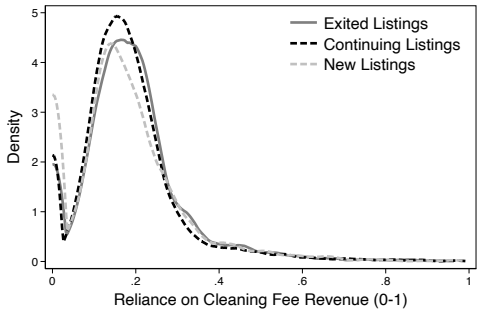
(a) Cleaning Fee Per Booking, Treatment



(b) Cleaning Fee Per Booking, Control



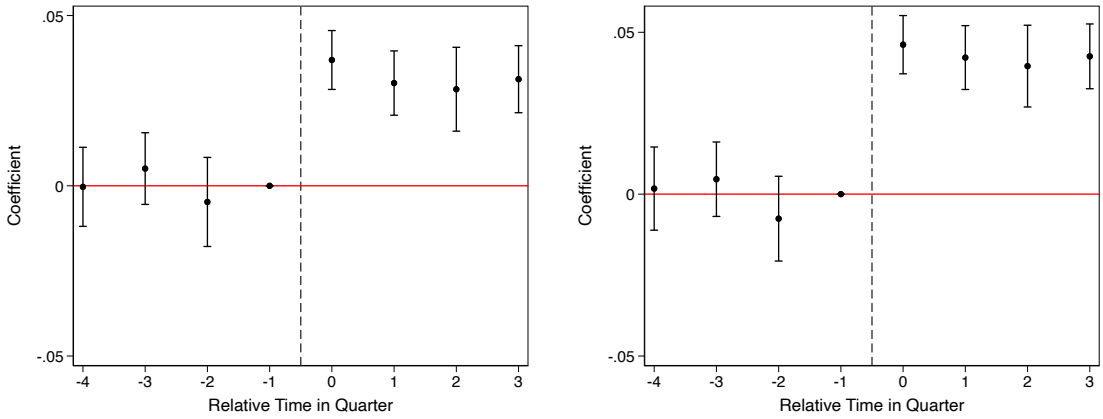
(c) Cleaning Fee Reliance, Treatment



(d) Cleaning Fee Reliance, Control

Figure A1: Distribution of Cleaning Fee and Cleaning Fee Reliance between Treatment and Control

Note: Panels (a) and (b) plot the average cleaning fee per booking for exited listings (using pre-law data), continuing listings (using pre-law data), and new listings (using post-law data) in the treatment and control groups respectively. Similarly, Panels (c) and (d) plot the average share of cleaning fee revenue in total revenue for exited listings (using pre-law data), continuing listings (using pre-law data), and new listings (using post-law data) in each group respectively.



(a) Log(ADR) (b) Log(PNR)

Figure A2: Effects of California's Price Transparency Law on Prices, Event Study (Removing February and March 2024)

Note: Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and city-specific time trends. Regressions use CEM weights. February and March 2024 are excluded from estimation. Robust standard errors are clustered at the listing level. 95% confidence intervals are displayed as brackets.

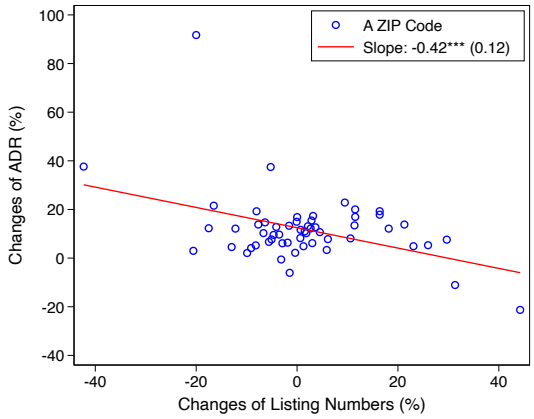


Figure A3: Relationship between Changes of Listing Supply and Changes of ADR in California

Note: Each point represents a ZIP code in San Diego or San Francisco. The vertical axis measures the percent change in average ADR within each ZIP code after the policy, and the horizontal axis measures the percent change in the number of listings within each ZIP code.

Table A1: Effects of California's Price Transparency Law, Robustness Checks (I)

Panel A: No CEM Weights

	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-11.413***	-0.706***	0.061***	0.049***	0.051***	-0.022***	-0.009**	0.004***	294.404***
	(3.721)	(0.226)	(0.003)	(0.002)	(0.002)	(0.005)	(0.004)	(0.001)	(26.830)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	641.831	130.822	5.332	5.504	5.510	4.989	16.661	0.031	4959.310
Adj./Pseudo R^2	0.710	0.921	0.899	0.901	0.917	0.235	0.267	0.103	0.716
N	426,972	426,972	426,960	426,972	426,972	426,972	426,972	426,972	426,972

Panel B: No CEM Weights & Removing San Diego

	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-7.664	-0.673	0.033***	0.026***	0.030***	-0.027***	0.001	0.002	164.452***
	(4.913)	(0.415)	(0.004)	(0.004)	(0.003)	(0.008)	(0.007)	(0.002)	(41.171)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	535.604	113.577	5.214	5.389	5.394	4.891	16.229	0.036	4109.127
Adj./Pseudo R^2	0.684	0.898	0.892	0.889	0.908	0.251	0.276	0.103	0.686
N	254,871	254,871	254,861	254,871	254,871	254,871	254,871	254,871	254,871

Panel C: No CEM Weights & Removing Post-Law Exits and Entries

	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-11.529***	-0.791***	0.069***	0.056***	0.057***	-0.026***	-0.015***	0.002**	325.540***
	(3.837)	(0.229)	(0.003)	(0.003)	(0.002)	(0.005)	(0.004)	(0.001)	(28.059)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	685.059	131.728	5.358	5.527	5.533	5.322	17.525	0.026	5280.279
Adj./Pseudo R^2	0.721	0.928	0.904	0.907	0.922	0.217	0.258	0.099	0.726
N	304,655	304,655	304,646	304,655	304,655	304,655	304,655	304,655	304,655

Notes: Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and city-month fixed effects. The dependent variable in Column 5, standardized ADR, equals PNR plus a standardized cleaning fee per night, where the latter divides the cleaning fee per booking by three (i.e., the median stay length in our sample). Columns 6 and 7 estimate a Poisson model. Robust standard errors are clustered at listing. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Effects of California's Price Transparency Law, Robustness Checks (II)

Panel A: Truncating Sample at March 2025

	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-24.161***	0.199	0.081***	0.065***	0.069***	-0.036***	-0.009	0.004*	215.970***
	(5.108)	(0.333)	(0.005)	(0.004)	(0.004)	(0.009)	(0.007)	(0.002)	(35.086)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	511.196	99.702	5.072	5.247	5.254	5.286	17.424	0.026	3726.889
Adj./Pseudo R^2	0.697	0.900	0.883	0.883	0.904	0.239	0.260	0.090	0.701
N	150,768	150,768	150,768	150,768	150,768	150,768	150,768	150,768	150,768

Panel B: Truncating Sample at December 2024

	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-33.603***	0.317	0.059***	0.046***	0.050***	-0.051***	-0.021***	0.007***	79.163*
	(6.233)	(0.395)	(0.005)	(0.005)	(0.005)	(0.010)	(0.008)	(0.002)	(41.696)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	524.198	100.018	5.076	5.249	5.257	5.406	17.834	0.024	3820.941
Adj./Pseudo R^2	0.703	0.904	0.887	0.888	0.907	0.239	0.258	0.091	0.707
N	132,271	132,271	132,271	132,271	132,271	132,271	132,271	132,271	132,271

Panel C: CEM with Fewer Cutpoints & Fewer Matching Variables

	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-23.022***	-0.547	0.064***	0.048***	0.053***	-0.043***	-0.015***	0.003*	188.222***
	(4.340)	(0.349)	(0.004)	(0.004)	(0.003)	(0.006)	(0.006)	(0.002)	(27.769)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	532.811	110.289	5.173	5.350	5.354	4.994	16.414	0.035	3961.075
Adj./Pseudo R^2	0.704	0.903	0.883	0.881	0.902	0.250	0.276	0.109	0.701
N	291,826	291,826	291,815	291,826	291,826	291,826	291,826	291,826	291,826

Notes: Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and city-month fixed effects. Regressions use CEM weights. The dependent variable in Column 5, standardized ADR, equals PNR plus a standardized cleaning fee per night, where the latter divides the cleaning fee per booking by three (i.e., the median stay length in our sample). Columns 6 and 7 estimate a Poisson model. We use 5 cutpoints to construct CEM weights in the main analysis and in Panel C of this table, we use only 3 cutpoints and remove pre-law occupancy rate and number of listed nights from matching variables. Robust standard errors are clustered at listing. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Effects of California's Price Transparency Law, Robustness Checks (III)

Panel A: SEs Clustered at Zipcode

	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-23.844***	-0.428	0.061***	0.048***	0.052***	-0.038***	-0.012	0.006***	174.072***
	(6.619)	(0.306)	(0.007)	(0.006)	(0.005)	(0.012)	(0.008)	(0.002)	(32.688)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	518.297	99.808	5.092	5.263	5.270	5.353	17.686	0.024	3858.495
Adj./Pseudo R^2	0.697	0.898	0.884	0.883	0.904	0.233	0.248	0.087	0.708
N	174,619	174,619	174,619	174,619	174,619	174,619	174,619	174,619	174,619

Panel B: SEs Clustered at City

	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-23.844**	-0.428	0.061**	0.048**	0.052**	-0.038***	-0.012	0.006***	174.072***
	(5.096)	(0.302)	(0.016)	(0.012)	(0.012)	(0.013)	(0.010)	(0.000)	(14.764)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	518.297	99.808	5.092	5.263	5.270	5.353	17.686	0.024	3858.495
Adj./Pseudo R^2	0.697	0.898	0.884	0.883	0.904	0.233	0.248	0.087	0.708
N	174,619	174,619	174,619	174,619	174,619	174,619	174,619	174,619	174,619

Panel C: Removing February and March 2024

	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-26.487***	-0.432	0.039***	0.029***	0.033***	-0.042***	-0.014**	0.007***	88.973***
	(4.689)	(0.309)	(0.004)	(0.004)	(0.004)	(0.007)	(0.006)	(0.002)	(33.048)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	524.724	99.853	5.102	5.271	5.279	5.419	17.874	0.023	3932.168
Adj./Pseudo R^2	0.699	0.908	0.883	0.883	0.903	0.233	0.248	0.089	0.711
N	159,570	159,570	159,570	159,570	159,570	159,570	159,570	159,570	159,570

Notes: Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and city-month fixed effects. Regressions use CEM weights. The dependent variable in Column 5, standardized ADR, equals PNR plus a standardized cleaning fee per night, where the latter divides the cleaning fee per booking by three (i.e., the median stay length in our sample). Columns 6 and 7 estimate a Poisson model. Robust standard errors are clustered at zipcode in Panel A, at city in Panel B, and at listing in Panel C. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Effect of California's Price Transparency Law on ADR Dispersion

	(1) SD of ADR	(2) CV of ADR
CA × Post	8.809***	-0.001
	(0.895)	(0.002)
Dep. Var. Mean	160.068	0.581
adj. R^2	0.795	0.677
N	171,862	171,862

Notes: Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and city-month fixed effects. Regressions use CEM weights. Column 1's dependent variable is the standard deviation of ADR within a 0.3-mile radius for each listing-month. Column 2's dependent variable is coefficient of variation within a 0.3-mile radius for each listing-month, which is computed by dividing the standard deviation of ADR by the mean of ADR. Robust standard errors are clustered at the listing level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Demand Estimates (No CEM Weights)

Model:	Logit OLS	Logit 2SLS			Nested Logit 2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Instruments for Price:		PR IV	L.12 ZHVI +PR IV	L.18 ZHVI +PR IV	L.12 ZHVI +PR IV	L.18 ZHVI +PR IV
Log(ADR)	-1.028***	-0.813	-1.337*	-0.227	-1.748	-0.458
	(0.016)	(0.822)	(0.802)	(0.466)	(1.085)	(0.492)
Nesting parameter					-0.280	-0.008
					(0.291)	(0.152)
Post × CA × Log(ADR)	0.051***	-0.008	-0.113	0.106*	-0.174	0.072
	(0.014)	(0.123)	(0.094)	(0.062)	(0.131)	(0.065)
CA × Log(ADR)	0.092***	-2.568***	-2.990***	-2.124***	-3.629***	-2.354***
	(0.022)	(0.726)	(0.815)	(0.652)	(1.240)	(0.737)
Post × Log(ADR)	0.075***	0.233*	0.322**	0.116*	0.453**	0.121*
	(0.011)	(0.140)	(0.133)	(0.061)	(0.215)	(0.064)
Post × CA × Quality	-0.002	-0.003	0.018	-0.026	0.028	-0.018
	(0.016)	(0.032)	(0.030)	(0.021)	(0.038)	(0.022)
Post × Quality	0.052***	0.035	0.013	0.061***	0.006	0.056**
	(0.012)	(0.035)	(0.034)	(0.020)	(0.042)	(0.023)
CA × Quality	0.050***	0.057***	0.050**	0.064***	0.056*	0.062***
	(0.017)	(0.022)	(0.023)	(0.019)	(0.029)	(0.021)
Quality	-0.086***	-0.076***	-0.065***	-0.089***	-0.077***	-0.088***
	(0.013)	(0.021)	(0.021)	(0.015)	(0.028)	(0.020)
Post × CA	0.075***	0.298***	0.378***	0.217***	0.451***	0.262***
	(0.010)	(0.077)	(0.063)	(0.061)	(0.110)	(0.061)
Cragg-Donald Wald F stat.		13	45	54	10	19
N	407,096	407,096	407,066	407,066	407,066	407,066

Notes: A market is defined as a zipcode-month. Specifications control for time-varying listing attributes and include listing fixed effects, year-month fixed effects, and zipcode-month fixed effects. Quality is measured based on whether the overall star rating is higher than the sample median (4.9 out of 5). Column 1 uses a logit model without instruments. Column 2 uses a logit model and instruments log(ADR) using log average ADR of private-room listings in a zipcode interacted with listing attributes. Columns 3 and 4 additionally include log Zillow Home Value Index (ZHVI) interacted with listing attributes as instruments, lagged 12 and 18 months respectively. Columns 5 and 6 use a nested logit model and additionally instruments the within-zipcode market shares using number of listings in a zipcode interacted with listing attributes. Columns 5 and 6 use 12- and 18-month lagged ZHVI instruments respectively. Robust standard errors are clustered at listing. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Demand Estimates (Zipcode-Year-Month Fixed Effects)

Model:	Logit OLS	Logit 2SLS		
	(1)	(2)	(3)	(4)
Instruments for Price:		PR IV	L.12 ZHVI +PR IV	L.18 ZHVI +PR IV
Log(ADR)	-1.050***	-3.838***	-3.429***	-1.117
	(0.025)	(1.191)	(1.085)	(0.690)
Post × CA × Log(ADR)	0.015	-0.424**	-0.365**	-0.173
	(0.020)	(0.181)	(0.168)	(0.113)
CA × Log(ADR)	0.034	-0.053	0.006	-0.869*
	(0.038)	(0.783)	(0.721)	(0.485)
Post × Log(ADR)	0.036**	0.871***	0.808***	0.270***
	(0.015)	(0.189)	(0.174)	(0.089)
Post × CA × Quality	0.019	0.107**	0.094**	0.040
	(0.018)	(0.050)	(0.047)	(0.031)
Post × Quality	0.036***	-0.150***	-0.132**	-0.002
	(0.013)	(0.057)	(0.052)	(0.030)
CA × Quality	0.013	-0.019	-0.013	0.010
	(0.022)	(0.039)	(0.036)	(0.026)
Quality	-0.043***	0.051	0.040	-0.030
	(0.016)	(0.039)	(0.036)	(0.024)
Post × CA	absorbed	absorbed	absorbed	absorbed
Cragg-Donald Wald F stat.		13	12	14
N	167,976	167,976	167,976	167,976

Notes: A market is defined as a zipcode-month. Specifications control for time-varying listing attributes and include listing fixed effects and zipcode-year-month fixed effects. Regressions include CEM weights. Quality is measured based on whether the overall star rating is higher than the sample median. Column 1 uses a logit model without instruments. Column 2 uses a logit model and instruments log(ADR) using log average ADR of private-room listings per zipcode interacted with listing attributes. Columns 3 and 4 additionally include log Zillow Home Value Index (ZHVI) interacted with listing attributes as instruments, lagged 12 and 18 months respectively. Robust standard errors are clustered at listing. Nested logit models cannot be identified together with the zipcode-year-month fixed effects due to collinearity. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effects of California's Price Transparency Law, For Vrbo Listings

Treated = SD, SF; Control = SEA, CHI									
	(1) Total cleaning fee	(2) Cleaning fee per booking	(3) PNR	(4) ADR	(5) ADR (Std)	(6) # of Bookings	(7) # of Booked days	(8) Zero Booking	(9) Revenue
Post × CA	-17.984**	0.231	0.020**	0.018**	0.018**	-0.012	0.007	0.012***	88.237
	(6.984)	(0.985)	(0.008)	(0.008)	(0.007)	(0.019)	(0.015)	(0.004)	(80.539)
Dep. Var. Units	$	$	Log($)	Log($)	Log($)	# (Poisson)	# (Poisson)	0/1	$
Dep. Var. Mean	333.137	96.106	5.588	5.677	5.707	3.825	13.872	0.064	4337.888
Adj./Pseudo R^2	0.704	0.893	0.898	0.896	0.897	0.277	0.341	0.094	0.544
N	61,992	61,992	61,972	61,992	61,992	61,992	61,992	61,992	61,992

Notes: Specifications include listing fixed effects, year-month fixed effects, and city-month fixed effects. The dependent variable in Column 5, standardized ADR, equals PNR plus a standardized cleaning fee per night, where the latter divides the cleaning fee per booking by three (i.e., the median stay length in our sample). Columns 6 and 7 estimate a Poisson model. Robust standard errors are clustered at the listing level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Summary Statistics for Vrbo Listings

	Treatment (SD, SF)		Control (SEA, CHI)	
	Pre	Post	Pre	Post
# of Exits per ZIP Code	7.166	5.908	1.591	2.216
	(8.333)	(6.944)	(1.929)	(3.542)
# of Entries per ZIP Code	8.331	8.811	3.352	2.572
	(11.607)	(9.191)	(5.558)	(3.801)
Overall Rating (0-100)	92.866	93.172	89.781	90.099
	(12.994)	(13.473)	(15.202)	(16.114)
Cleaning Fee Per Booking ($)	118.556	92.382	98.840	78.501
	(121.170)	(112.675)	(93.477)	(82.790)
Cleaning Fee Reliance (0-1)	0.117	0.088	0.157	0.126
	(0.117)	(0.116)	(0.177)	(0.275)
Zero Cleaning Fee (0/1)	0.273	0.405	0.203	0.309
	(0.445)	(0.491)	(0.402)	(0.462)
Avg. Daily Rate (ADR, $)	413.423	446.302	298.601	315.994
	(312.354)	(334.431)	(246.922)	(251.492)
Per Night Rate (PNR, $)	380.977	421.232	271.709	293.747
	(300.124)	(325.379)	(240.327)	(245.804)
# of Bookings	3.415	3.723	3.792	4.229
	(2.812)	(3.113)	(3.166)	(3.396)
# of Booked Days	12.970	12.923	14.647	14.881
	(9.537)	(9.629)	(9.714)	(9.797)
Revenue ($)	4496.245	4889.040	3654.825	3992.674
	(4389.525)	(4800.945)	(3247.844)	(3676.070)
N	14,396	19,201	12,440	16,724

Notes: This table presents summary statistics for Vrbo entire-home listings.

B Reference Pricing

In this appendix, we describe a revealed preference approach to examine how hosts anchor their own prices to those of local competitors.

We use a revealed preference approach to recover the weights hosts assign to each listing characteristic when they select their reference listings. We first compute the average ADR and average attributes for each listing in the pre- and post-law periods. Then we match each focal listing to reference listings in the same ZIP code whose pre-law ADR falls in the same bin as the focal listing's post-law ADR. These matched listings are identified as the chosen references for each focal listing. We use a conditional logit model for all California's listings, given by the following form:

$$Y_{ij} = \sum_{a \in \mathcal{A}} \beta^a \left(|\text{Attribute}_i^a - \text{Attribute}_j^a| \right) + \epsilon_{ij} \tag{8}$$

where i denotes the focal listing and j denotes the candidate listing within the same ZIP code as i. The dependent variable Y_{ij} is a choice indicator equal to one for the chosen reference listings and zero for all other listings within a ZIP code. The independent variables are the absolute differences between the focal listing and each candidate listing for listing attribute $a \in \mathcal{A}$. The coefficients β^a indicate the weight for listing attribute a in the reference listing selection process.

Table B1 presents the estimates from the conditional logit model. A negative coefficient on attribute a means that listings with less distance on attribute a are more likely to be chosen as the reference listings. Most coefficients are negative and significant, suggesting that hosts tend to reference listings that are similar to their own in terms of size, quality, and host type.

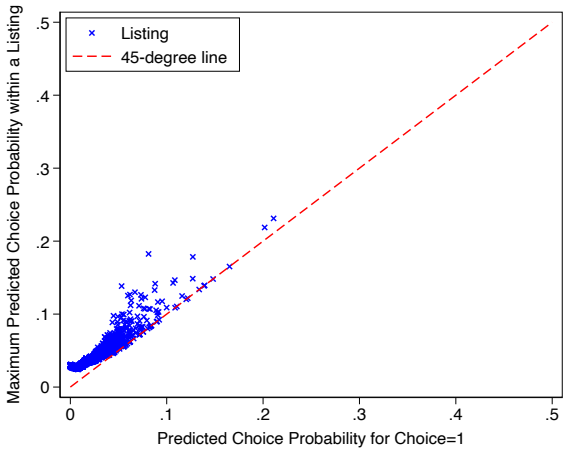
Using these estimates, we plot for several post-estimation outcomes in Figure B1. In Panel (a), we compute the predicted choice probability of each candidate listing for a given focal listing and plot the highest predicted probability against the predicted probability of the chosen reference listings. We observe that most points cluster near the 45-degree line and a number of them above the 45-degree line, indicating that the model fits the data reasonably well. Hosts whose listings lie on the 45-degree line select reference listings exactly as the model predicts, while those above it place slightly different weights on listing attributes when making their selection.

Panel (b) shows the average ADR between focal listings and reference listings before and after the policy. The correlation between focal and reference listings' ADR is similar between pre- and post-law periods. The slope of the post-period fitted line is moderately steeper, suggesting slightly larger ADR variations. Panel (c) compares the changes of ADR between focal listings and reference listings, generating several interesting observations. First, changes of ADR between the focal and reference listings are highly correlated: a listing's ADR tends to move together with the ADR of its reference listings. Second, the correlation is positive, implying that the focal and reference listings do not appear to move toward convergence. In particular, when a focal listing raised its ADR, its reference listings typically also increased ADR rather than lowering them to narrow the gap.

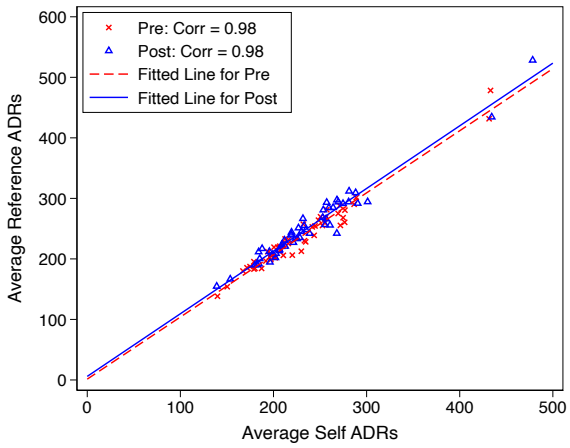
Table B1: Recovering Listing Attribute Weights using a Revealed Preference Approach

Dep. var.:	Choice Indicator
diff_superhost	-0.025
	(0.041)
diff_host_listcnt	-0.005***
	(0.001)
diff_bedroom	-0.993***
	(0.020)
diff_bathroom	-0.809***
	(0.029)
diff_response_rate	-0.003
	(0.002)
diff_acceptance_rate	-0.009***
	(0.001)
diff_instantbook	0.020
	(0.042)
diff_review	-0.002***
	(0.000)
diff_r_overall	-0.103
	(0.101)
diff_r_accuracy	-0.064
	(0.095)
diff_r_cleanliness	-0.137*
	(0.082)
diff_r_checkin	-0.180**
	(0.083)
diff_r_communication	0.123
	(0.091)
diff_r_location	-0.202***
	(0.075)
diff_r_value	-0.097
	(0.082)
Pseudo R^2	0.103
Observations	1,481,845

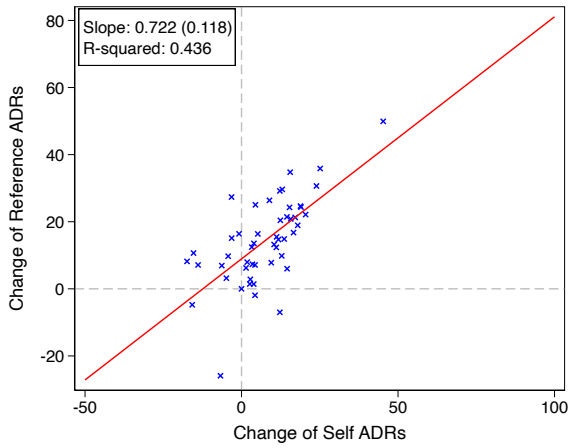
Notes: This table shows estimates from a conditional logit model for treated listings. We first compute the average ADR and average attributes for each listing in the pre- and post-policy periods. We then match each focal listing to reference listings in the same zipcode whose pre-policy ADR falls in the same bin as the focal listing's post-policy ADR. These matched listings are identified as the chosen references. The dependent variable is a choice indicator equal to one for the chosen reference listing and zero for all other listings in the same zipcode. The independent variables are the absolute differences in pre-policy attributes between the focal listing and each candidate. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.



(a) Model Fitness



(b) Pre- and Post-ADR



(c) Change of Self and Reference ADR

Figure B1: Post Estimation Outcomes

Note: In Panel (a), the choice probability predictions are obtained from a conditional logit model specified in Equation 8. Panel (b) plots the average self ADRs and the average reference ADRs pre- and post-law per ZIP code. Panel (c) plots the difference in average self ADRs and the difference in average reference ADRs before and after the policy by ZIP code.

C Outside Good Utility Function in the Demand Model

In this appendix, we allow for a flexible outside good utility function in the demand model to account for the potential impact of the price transparency law on utility for Vrbo listings. We show that this impact is fully absorbed by the ZIP code-year-month fixed effects that we include in Table A6.

Define the market as a ZIP code-month. For Airbnb listing j in ZIP code z, year y and month m (year-month t), consumers expect utility:

$$U_{jt} = \lambda_j + \mu_{ym}^{Airbnb} + \gamma_{zm}^{Airbnb} + \underbrace{\boldsymbol{\alpha}\ln(\text{ADR}_{jt}) + \boldsymbol{\beta}\text{Quality}_{jt}}_{\phi_{jt}} + \xi_{jt} + \underbrace{\eta_{zym}^{Airbnb}}_{\text{zip-t factors}} + \underbrace{\epsilon_{jt}}_{\text{logit errors}}. \tag{9}$$

Taking the pool of Vrbo listings as the outside good in the focal ZIP code-month, the utility of the outside good is:

$$U_{0t} = \lambda_0 + \mu_{ym}^{Vrbo} + \gamma_{zm}^{Vrbo} + \underbrace{\boldsymbol{\alpha}\ln(\text{ADR}_{0t}) + \boldsymbol{\beta}\text{Quality}_{0t}}_{\phi_{0t}} + \xi_{0t} + \underbrace{\eta_{zym}^{Vrbo}}_{\text{zip-t factors}} + \underbrace{\epsilon_{0t}}_{\text{logit errors}}. \tag{10}$$

Following Berry (1994), we have:

$$\begin{aligned} \ln(s_{jt}) - \ln(s_{0t}) =& E(U_{jt}) - E(U_{0t}) \\ =& \lambda_j - \lambda_0 + \mu_{ym}^{Airbnb} - \mu_{ym}^{Vrbo} + \gamma_{zm}^{Airbnb} - \gamma_{zm}^{Vrbo} \\ &+ \phi_{jt} - \phi_{0t} + \xi_{jt} - \xi_{0t} + \eta_{zym}^{Airbnb} - \eta_{zym}^{Vrbo}. \end{aligned} \tag{11}$$

where s_{jt} and s_{0t} denote the market shares of product j and the outside good in year-month t, respectively. As in the reduced-form specifications, we include listing fixed effects λ_j, year-month fixed effects μ_t, and ZIP code-month fixed effects γ_{zm}. ξ_{jt} captures consumers' valuations of unobserved product characteristics, and $\ln(\text{ADR}_{jt})$ denotes logged ADR. Since overall star rating is a comprehensive measure of listing quality, we define Quality_{jt} as a dummy for whether a listing's overall star rating is above the sample median (4.9).

Rearrange and some items can be absorbed by ZIP code-year-month fixed effects:

$$\begin{aligned} \ln(s_{jt}) - \ln(s_{0t}) =& E(U_{jt}) - E(U_{0t}) \\ =& \phi_{jt} + \underbrace{\lambda_j - \lambda_0}_{\text{Airbnb listing } j \text{ FE}} + \underbrace{[\xi_{jt} - \xi_{0t}]}_{\text{error term}} \\ &+ \underbrace{[-\phi_{0t} + \mu_{ym}^{Airbnb} - \mu_{ym}^{Vrbo} + \gamma_{zm}^{Airbnb} - \gamma_{zm}^{Vrbo} + \eta_{zym}^{Airbnb} - \eta_{zym}^{Vrbo}]}_{\text{absorbable by ZIP code-year-month FE}} \end{aligned} \tag{12}$$

The terms in the last row are absorbable by ZIP code-year-month fixed effects, so including the ZIP code-year-month fixed effects can fully account for the law's potential impact on utility derived from Vrbo listings.