Supplier Salience: Incidence, Market Entry & Welfare

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Abstract

This paper explores how optimization frictions in supplier price-setting affect tax incidence. We use a natural experiment that varied the effective hotel tax rate in certain cities at different times to document heterogeneity in tax pass-through using multiple proxies for price-setting acumen. Pass-through is lowest for sophisticated hosts whose prices before the policy closely follow local hotels. In contrast, less sophisticated hosts are much less likely to adjust their pre-tax price, passing the entire tax burden onto consumers. Further investigation suggests that the behavior of these hosts can be delineated into inattention (failure to change price) or lack of skill in price setting which have distinct consequences for demand. Finally, we find that this policy affected market composition, with net entry skewing toward more sophisticated hosts after the policy. We develop a model for welfare analysis which incorporates supplier salience and other price setting optimization failures.

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1 Introduction

Classical tax theory holds that a tax's economic burden is exclusively determined by relative demand and supply elasticities: the less elastic party bears more of the tax(??). However, recent studies have established that tax design considerations, such as remittance duty matter when sides of the market have access to different evasion opportunities (??). Other factors related to individual optimization frictions have also been found to influence tax incidence, in particular the extent to which consumers perceive the tax–i.e. tax salience (??). For example, a consumer who wholly ignores the presence of sales tax or incorrectly calculates the tax-inclusive price of an item will bear a larger share of a tax increase, on average, than a consumer who perceives the tax and performs this calculation correctly.

But with the rise of new technologies that lower barriers to entry for small, thinly capitalized suppliers, it is increasingly likely that these same optimization frictions exist on the supply side of the market. In this paper, we seek to understand how limited tax salience among a significant fraction of short term occupancy suppliers affects tax incidence, and ultimately market composition. We leverage these empirical findings to draw comparisons between the welfare implications of taxing mixed professional and amateur markets in contrast to traditional markets wholly dominated by professional sellers.

We extend this literature by studying a context in which the optimization frictions faced by suppliers vary substantially. In a simple model, we show how supplier tax salience separately and jointly affect equilibrium pre-tax prices. To study this phenomenon empirically, we exploit plausibly exogenous variation in the timing of bilateral remittance agreements, called Voluntary Collection Agreements (VCAs), between Airbnb and city governments in the United States. These VCAs shifted the responsibility to remit hotel taxes from individual suppliers to the Airbnb platform itself. We conclude that which side of the market remits can have an economically meaningful effect on equilibrium prices, tax collections, and the characteristics of market entrants.

The paper makes two contributions. First, we show that shifting the remittance duty substantially increased after-tax prices and that this effect likely stemmed from the elimination of a differential evasion opportunity available to suppliers. Intuitively, suppliers that previously evaded the tax adjust their pre-tax price downward by less than the amount of the tax in response to the policy, passing some or all of the tax on to consumers. In contrast, suppliers that previously complied with the tax will respond to it by lowering their pre-tax price by the amount of the tax. This practice, as classical tax theory predicts will happen when switching the remittance obligation, leaves consumer prices unchanged.

To identify the effect of VCA adoption on consumer prices,weemploy two complementary estimation techniques that rely on separate identifying assumptions. First,weexploit variation in the timing and location–both across and within metropolitan areas–of VCA adoption to estimate a triple difference specification. The identifying assumption is that, prior to the policy, consumer prices in treated cities were moving in parallel with respect to those in two sets of control geographies: other metropolitan areas that did not adopt VCAs, and neighboring jurisdictions within the same metropolitan areas that did not adopt VCAs

Second,we take advantage of detailed data on the locations of listings to estimate a geographic regression discontinuity (RD) design, comparing those listings just within the municipal border of a VCA-adopting city to listings just outside that border. Reassuringly,we find similar estimates using both methods. On average, for each one percentage point of the local hotel tax rate, the price paid by consumers rises by approximately 0.9 percent. Using the same sources of variation in timing and location,we find that hotel tax collections increase in proportion to the size of the Airbnb market before the policy, whichwe interpret as circumstantial evidence that failure to remit was widespread.

This result complements the main finding in **?**, which states that the economic incidence of a quantity tax on diesel fuel depends on the point of collection within the supply chain. As the remittance obligation moves "up" the chain from retailers to distributors and prime suppliers, the pass-through rate of diesel taxes to the retail price increases, as do tax revenues. This suggests that differential evasion opportunities afforded to these agents explain the relevance of a tax's collection point.

Our second contribution is to provide evidence suggesting that the effect of VCA adoption may be heterogeneous with respect to suppliers' attentiveness to the policy and the existence of hotel taxes. Although some suppliers may have purposely chosen not to comply with the tax prior to the adoption of the VCAs, other suppliers may have been unaware of the hotel tax's existence or their obligation to remit.wetherefore model supplier behavior as being characterized by their "attentiveness" and also allow for the possibility that inattentive hosts are not only less informed about the policy environment but may systematically err in their demand forecasts as well, a hypothesis for whichwefind empirical support.

I document heterogeneity in the effect of VCA adoption on consumer prices by several supplier characteristics, including responsiveness to local demand shocks, experience, and concentration of competitors.wedo this by re-estimating event study and difference-in-differences models while interacting the policy variable with characteristics of suppliers and their surroundings. For example,wefind that a one percentage point increase in the correlation between a host's prices and those of local hotels–a proxy for price-setting sophistication–results in a 0.2 percent reduction on the overall increase in consumer prices following adoption of the VCA.

One interpretation of this finding is that attention to local demand conditions and attention to the tax regime are related, and that, as a result, pass-through in markets with inattentive or amateur suppliers may be different than in markets with traditional firms. Although there are various studies that already suggest consumers face optimization frictions that affect their responsiveness to changes in tax rate or tax administration (e.g., **????**), there is comparatively little evidence on whether similar optimization frictions also affect suppliers.

Relatedly, while tax incidence is traditionally exclusively determined by market-level factors such as the level of competition and supply and demand elasticities (e.g.,**??**), there is some empirical evidence that differences in firms' characteristics, such as managerial resources that affect price-setting strategies, can lead to variation in tax incidence within a market where some firms have market power. For example, small, independent firms are more likely to rely on simplified pricing rules, such as round-number heuristics, and may not fully incorporate tax changes into price-setting behavior (**?**).Our empirical findings suggest that more sophisticated hosts pass on less of the tax burden resulting from the elimination of evasion opportunity, lending support to this hypothesis.

A caveat is warranted. Price changes provide direct evidence of the increased cost of maintaining consumption after the policy and can also provide indirect insight into underlying market functions (??). However,we proceed with caution in inferring that the policy changed the incidence of the tax, at least incidence in the way that it is conventionally defined–as the ratio of the reductions in total consumer and producer surplus resulting from imposition of a tax. We interpret ourresults to suggest that the tax burden on guests in these cities rose–they are now paying higher prices for identical products. Yet, if some hosts were previously evading their remittance obligation, as seems likely, their absolute tax burden rose as well (from zero). Therefore, the policy changed the tax incidence in the sense that it shifted the burden of the tax from taxpayers to the suppliers and consumers, rather than the relative burden of the tax as shared between consumers and suppliers.

Another limitation of our approach is that it relies mainly on data from a single platform firm in a single industry. While I acknowledge that this inherently limits the generalizability of our estimates,wemaintain that two key features of this context expand the project beyond a case study: first, despite obvious difficulty in valuation, hosts have full autonomy in price-setting, and second, a large contingent of hosts on Airbnb are amateurs, a feature characteristic of other emerging platform or "market-maker" driven markets.

For clarity,we explicitly define key terms employed throughout the paper as follows.we understand pass-through–distinct from incidence–as the degree to which tax-exclusive prices adjust to shift the economic burden of the tax to non-remitting parties to the taxable transaction. As is standard,we express pass-through as a percentage calibrated to the total tax liability[1]. we refer to individual suppliers who list their property on the Airbnb as hosts, and consumers or short-term renters as guests, in keeping with Airbnb's nomenclature. We define an amateur host as one who is a casual participant in the rental market–i.e., they did not secure their property interest for the purpose of short-term rental and they lack the price-setting acumen that accrues to professionals through intensive rental activity or centralized price-setting resources.

The remainder of this paper is organized as follows: Section 2 lays out a theoretical framework of the effects of shifting remittance duty that we use to motivate and interpret the empirical findings. Section 3 provides background on Airbnb rental markets and the natural experiment afforded by cities' VCA adoption, while Section 4 introduces the data and characteristics of the sample. The next three sections explore empirical claims corresponding to the predictions of the model. Section 5 studies the effect of the remittance shift on tax-inclusive prices and collection of tax revenue. Section 6 considers supplier heterogeneity in pass-through and Section 7 asks whether the policy affected market exit decisions. Section 8 summarizes the empirical results and discusses their implications for ongoing academic and policy dialogues about tax system design. Section 9 concludes.

2 Policy Variation

This section provides three types of background information relevant for subsequent analysis. First, wedescribe the characteristics of the emerging, platform-driven, short-term rental market, and Airbnb specifically. Next,we discuss a timeline of the Airbnb VCAs, which provide the plausibly exogenous policy variation needed for analysis. Finally,we provide details of Airbnb's implementation of agreements, including how and when the tax was displayed

[1]However,we refer to pass-through of the policy as if it constituted a new tax, rather than being partially constituted by a change in compliance costs.

during booking in Airbnb's interface.

Airbnb is the largest of several firms facilitating short-term, peer-to-peer residential space rentals through an online platform. Originally conceived as an online marketplace to connect couch surfers, Airbnb has experienced remarkable growth in recent years, expanding exponentially in popular tourism cities around the globe.[2] Hosts on Airbnb create listings for each of their properties. Each listing includes information about the space's characteristics, such as the number of beds, kitchen availability, and whether it is a private apartment or a shared space. Hosts can designate a listing's availability and set its price for each calendar day.

In addition to consumer safety concerns, local governments have expressed frustration with Airbnb hosts' avoidance of short-term rental taxes. In cities with significant tourism, the estimated loss of occupancy tax revenue is significant. Initially, Airbnb's position was that its rentals were not subject to occupancy taxes because transactions were "peer-to-peer" rather than commercial in nature. In May 2014, the company officially retracted this view and announced that it believed its hosts were responsible for paying occupancy taxes to local governments. It also amended its "Terms of Service" agreement to inform hosts of their obligation to research and comply with applicable local taxes and regulations.[3]

On June 28, 2014, Airbnb announced that it had reached an agreement with the city of Portland, OR to collect an 11.5% occupancy tax on all reservations booked on its site, and to pay these taxes to the city at the end of each quarter. Crucially, the agreement explicitly prohibited Portland's city government from requiring Airbnb to disclose information related to a specific taxable transaction that could individually identify hosts. As part of the exchange, the Portland City Council agreed to pass a code revision that would legalize short-term home rentals if residents obtained a $180 permit and installed fire alarms.

Between August 2014 and August 2015, similar agreements to collect and remit hotel sales taxes were signed with San Francisco, CA (14.5%), San Jose, CA (10%), Chicago, IL (4.5%), Washington, DC (14.5%), Philadelphia, PA (8.5%), Durham, NC (6%), San Diego, CA (10.5%), and Phoenix, AZ (3%), as well as several smaller municipalities. Typically, an agreement is announced two weeks before the date when Airbnb begins collecting taxes on all bookings in that jurisdiction. Airbnb notifies affected hosts of the policy change via email shortly after the announcement.

When a guest searches for a rental on Airbnb, she is presented with a set of search results that includes an image, location, and tax-exclusive estimate of the nightly fee for each listing (Figure **??**)[4]. After a guest clicks on a listing, she is shown a more detailed accounting of the

[2]Paris is thought to have nearly 40,000 active Airbnb listings, the most of any city in the world.

[3]Beginning May 1, 2014, Airbnb's Terms of Service includes the following paragraph: YOU AS A HOST UNDERSTAND AND AGREE THAT YOU ARE SOLELY RESPONSIBLE FOR DETERMINING (I) YOUR APPLICABLE TAX REPORTING REQUIREMENTS, AND (II) THE TAXES THAT SHOULD BE INCLUDED, AND FOR INCLUDING TAXES TO BE COLLECTED OR OBLIGATIONS RELATING TO APPLICABLE TAXES IN LISTINGS. YOU ARE ALSO SOLELY RESPONSIBLE FOR REMITTING TO THE RELEVANT AUTHORITY ANY TAXES INCLUDED OR RECEIVED BY YOU. AIRBNB CANNOT AND DOES NOT OFFER TAX-RELATED ADVICE TO ANY MEMBERS.

[4]The price shown in the search results is the average cost per night of the room, excluding taxes and Airbnb's service fee. For example, if a listing's rental prices for Friday, Saturday, and Sunday are $90, $100, and $110, respectively, and the listing has a $30 cleaning fee, then the price displayed in the search results will be $110 (90+100+110+30 / 3 = 110).

rental cost, including Airbnb's service fee and any occupancy tax. Figure **??** shows examples of listings from two jurisdictions: one that has a bilateral agreement with Airbnb (Chicago, IL), and one that does not (Evanston, IL). Notice that both listings appear among same set of search results. Without clicking on a listing, it is not evident whether an occupancy tax applies to it.

3 Data

Our analysis makes use of multiple datasets. Below,wedescribe each dataset's source and features, and then discuss descriptive analysis of hosts' price-setting behavior.

3.1 Data Sources and Details of Key Variables

To measure the response of hosts to Airbnb's remittance agreements,we collect information on listings for selected U.S. cities and their surrounding areas between December 2014 and August 30, 2016[5]. Our data collection focused on cities with large tourisms sectors and cities that had announced, but not yet implemented, occupancy tax remittance agreements with Airbnb. In total, 20 cities enactment agreements during the period of data collection. See Table **??** for a list of enactment dates.We also collect data for five cities that do not enact agreements during the period–these cities serve as controls. In addition to listings within the city itself,we collect data on listings in metro areas (MSAs) to which the implementing cities belong. For each listing,we obtain its approximate geographic coordinates[6], price, unit type (e.g., shared, private room, entire home), number of reviews, and whether it can be booked instantly. Listings and hosts are each identified by a unique ID, facilitating the tracking of listings over time.

Data are collected in multiple waves, based on the implementation dates of remittance agreements. To supplement these collection efforts,we purchased additional listing data from Airdna, a company that collects Airbnb listing data.Our final analysis sample includes all listings in the city and greater metro areas[7] for all cities in the study between December 2014 and August 2016.

When a guest searches for listings in a given location, Airbnb's site returns information on the price and neighborhood of up to 18 listings per page. By clicking on a listing, the user gains additional information about its amenities, reviews, and availability. Availability is displayed using a calendar that the host controls, and where days can be designated as either available for booking or not. If designated available, the default price for that day is the listing price. However, hosts have the option of overriding the listing price for a particular day, such as for a major sporting event. In the analysis that follows,we distinguish between

[5]These data are collected using an automated script or "crawler" that systematically browses Airbnb.com and collects information on listings associated with a particular geographic search term (e.g., "New York, NY"). The script mimics the browsing experience of a potential guest by clicking through each listing in the search results and obtaining its characteristics.

[6]Geographic coordinates are purposefully offset by a small distance from the street address registered by the host for privacy. Once a listing is booked, the guest is sent an email with the exact street address. Anecdotal evidence, based on discussions by hosts on internet forums, suggests that these offsets are small (less than 1/8 mile) and, importantly, according to Airbnb's website, offsets are done within neighborhoods.

[7]Listings are included based on the intersection of approximate longitude and latitude coordinates and the U.S. Census MSA boundary files.

the ‘listing price’ and the ‘booking price.’ The latter is the final consumer price, equal to the listing price plus the Airbnb service fee, the cleaning fee, and the tax if an agreement is in place. Consumers review the booking price before the transaction is completed.

3.2 Descriptive Statistics

Table **??** contains descriptive statistics for treatment and control cities. Column 1 provides the number of unique listings in the entire metro area (both treatment and control), while Column 2 contains the number of listings located within the municipal boundary. Columns 4-6 provide means of relevant variables for each MSA.

Treated cities differ in the number of listings observed in un-treated, neighboring municipalities. For example, almost one third of listings in the Washington metro area are located in neighboring municipalities, compared to a relatively smaller fraction of listings in the Chicago metro area. Washington, D.C. is perhaps uniquely well-suited for the purpose of comparing treated host behavior to that of untreated, nearby controls: more than a third of the listings returned in a search for the city were located in Arlington, VA, Falls Church, VA, or Bethesda, MD, three municipalities that did not sign remittance agreements with Airbnb. Visual evidence of this is provided in Figure **??**, which shows the spatial distribution of listings in Washington, Chicago, Oakland, and Los Angeles.

Figure **??** displays the fraction of listings that change price at least once in three of the treatment cities in each week, limited to those listings appearing at least once in both the pre- and post-agreement periods. On average over the study period, approximately 20 percent of listings change price each week, while in San Diego, closer to a third of listings observed in any given week change prices at least once.

Finally, Figure **??** displays a histogram of prices across all listings under \$250 in the data. It is evident that hosts employ a number of heuristic pricing strategies, such as choosing prices in increments of \$10 or \$5.

4 Empirical Strategy & Results

As our variation arises from a staggered adoption setting, the standard two-way fixed effects differences-in-differences estimator may be biased if effects vary across adoption cohorts (Goodman-Bacon 2021). To avoid potentially problematic comparisons of late to early adopting cities, we estimate the effect of VCA implementation on tax inclusive prices, reservations and the probability of market exit using a ”stacked” differences-in-differences model.

In an approach informed by Cengiz et al. 2019 and Deshpande and Li 2019, we estimate the following equation:

$$Y_{ictd} = \alpha_i + \gamma_{ct} + Treat_{cd} + \sum (pi^{\tau_c}_{dt}) + \omega_{ctd} + \varepsilon_{ictd} \tag{1}$$

where i is an individual listing in city c in calendar week t with VCA adoption date d. We include listing fixed effects, α_i, to control for any time invariant characteristics of the listing (e.g. type of space, number of bedrooms etc.) as well as time-fixed effects γ_{ct}. $Treat_{cd}$ is an indicator for whether city c adopted a VCA on date d (i.e., c is a treatment city in panel d), analagous to the ”Treat” indicator in a standard two by two difference-in-difference model. Similarly, π^{tau}_{dt}, an indicator for whether week t is τ weeks before or after the week in which VCA d is adopted, performs the same function as including a indicator For ”Post.”

Our coefficent of interest, ω_{ctd}, an indicator for whether city c in week t in panel d is treated or not. We cluster standard errors at the MSA level, as prices in adjacent cities seem likely to be correlated.

To understand how the effect of the VCA varies over time, we estimate the the following event-study version of this model:

$$Y_{ictd} = \alpha_i + \gamma_{ct} + treat_{cd} + \sum(pi^{\tau}_{dt}) + sum(treat_{cd} * \pi^{\tau}_{dt}) + \varepsilon_{ictd} \tag{2}$$

where $\sum(treat_{cd} * pi^{tau}_{dt})$ are the interactions between city c being treated in panel d and week t being τ weeks before or after the week in which VCA d is adopted.

The identifying assumption for both versions is that, in the abscence of the VCA, the outcome of interest would have evolved similarly as it did in non-treated cities (i.e. paralell trends assumption).

4.1 Pass Through

We report estimates for specification (1) on the log tax-inclusive price paid by consumers in Table **??** (Column 3). For example, for each one percentage point increase in the effective tax rate, the price paid by consumers rises by approximately 0.9 percent.

This price increase, in addition to violating statutory irrelevance, suggests that the burden of increased compliance falls heavily on consumers. The effects on the advertised, pre-tax price (Column 1), and on reservations (Column 2), have the opposite sign, as expected, but much more modest one tenth of one percent decrease. We also report the results of a traditional difference-in-differences specification, which restricts the sample to listings from treated metros. Estimates from the pooled diff and triple diff are appreciably similar, but diverge (in some cases, significantly) when estimated separately by metro.

As was alluded to in the introduction, it is difficult to interpret from either set of estimates whether or how economic incidence was affected by this policy.Ourestimates show that the after-tax price rose significantly after remittance was reassigned, and, at least in the short term, there is no indication that the quality of rentals increased. Therefore, it seems reasonable to infer that consumer surplus declined. However, for previously non-remitting hosts, the tax increased their absolute tax burden (from zero) and weakly reduced demand, likely decreasing producer surplus. Without strong assumptions over underlying demand and supply elasticities, and pre-policy compliance, it is difficult to estimate the comparative reduction in surplus. Incidence in this context is further discussed in Section 6.

4.1.1 Hotel Demand and Hotel Tax Receipts?

In this section,we evaluate the effects of the policy on a city's hotel market and hotel tax receipts, using monthly data from STR, a market research firm, and tax collection data obtained from municipal governments via Freedom of Information Act requests. By reassigning the duty to remit hotel taxes from hosts to Airbnb, and therefore making it more difficult for hosts to evade the tax, the policy could be expected to have at least two effects on a city's hotel market and its hotel tax receipts. First, it effectively increases the price of Airbnb listings, and may therefore increase demand for hotel rooms to the degree that those are seen as substitutes for short-term rentals. Second, even if demand for Airbnb rentals declines somewhat following the policy, it will likely increase a city's hotel tax receipts as the opportunities for evasion dwindle.

Using monthly hotel market and hotel tax receipt data from 2010 through October 2016 for four cities that enacted these policies and three that did not,[8] we estimate the following difference-in-differences specification:

$$y_{mt} = \gamma_m + \gamma_t + \pi Treat_m POST_{mt} + \varepsilon_{mt} \quad (3)$$

where y_{mt} is the outcome of interest for municipality m in month t. Characteristics invariant to municipality or time period are captured by municipality and time fixed effects, respectively. The coefficient of interest, π, captures the difference in the outcome between municipalities that adopted the policy and those that did not, both before and after its enactment.

The hotel market data capture several monthly measures of a city's hotel market: the occupancy rate, revenue per available room, and total revenue. The occupancy rate is the number of rooms sold divided by the number of available rooms, while the revenue per available room is the total guest revenue divided by total number of available rooms. Table **??** reports results from equation (3) for log versions of these hotel market measures. These point estimates suggest that the enactment of the policy had almost no effect on the occupancy rate of hotels, though it did increase revenue per available room by 6.4 percent and total revenue by 3 percent; however, none of these estimates are statistically distinguishable from zero.

Table **??** also reports results from equation (3) for log hotel tax receipts. Enactment of the policy increased hotel tax receipts by 10 percent, though this estimate is only significant at the 10 percent threshold.

Taken together, these estimates suggest that enactment of the policy bolstered cities' hotel tax collection efforts, as evidenced by the increase in their tax receipts, but they do not provide conclusive evidence one way or the other on its effects on the local hotel market.

5 Heterogeneity in Pass-Through by Attention Correlates

In this section,weexplore how much of the observed heterogeneity in the price effect can be explained by differences among individuals in characteristics that suggest their attention to price-setting. Concluding from the previous section that non-compliance was pervasive before the policy,weinterpret this as heterogeneity in pass-through of an effective tax increase.wefind that hosts which present as "more attentive" pass-through less of the effective tax increase to consumers. This finding may generalize to pass-through of actual tax rate changes by inattentive suppliers in the absence of differential evasion opportunities.

5.1 How Does the Effect on Prices Differ by Host Observables?

As discussed in section 1.1, hosts differ in their approach to setting prices. For example, variation in price setting sophistication may cause some hosts to respond to the policy by

[8] Complete hotel market data (from STR) and hotel tax receipt data (from FOIA requests) were assembled for four cities that enacted the policy (San Diego, Palo Alto, Phoenix, Philadelphia) and three that did not (Houston, Austin, Dallas).

changing (i.e., lowering) their listing price because they anticipate that consumers will be less willing to book at higher prices.[9]

We test for heterogeneity in price response by host characteristics that may be associated with price setting sophistication: time series correlation between the host's pre-policy prices and the prices of hotel rooms; heuristic pricing, such as setting a price divisible by 5 or 10; and proxies for the intensity of rental activity, such as enabling the "instant booking" feature, listing multiple properties on Airbnb, or listing an entire unit (as opposed to a private room in what is likely an owner-occupied dwelling). For each binary host characteristic X_i,we estimate the following triple-difference specification:

$$y_{imct} = \gamma_i + \gamma_t + \gamma_{mt} + \gamma_{ct} + \pi M_i C_i POST_{mt} \tau_m X_i + \varepsilon_{imct} \tag{4}$$

The coefficient π represents the average percent difference in tax-inclusive listing prices for hosts with characteristic X_i, located within the major city ($C_i = 1$) in a "treated" metro ($M_i = 1$) after the policy is enacted ($POST_{mt} = 1$), for each percentage point of the hotel tax rate in that metro (τ_m).

Table **??** reports results of π estimated separately for each host characteristic. Taken in aggregate, hosts who are less likely to be sophisticated–who do not have instant booking turned on, who exhibit evidence of heuristic price-setting behavior, who do not rent out an entire unit, and who do not list multiple properties–usually have slightly higher tax-inclusive listing prices following the policy than hosts who are more likely to be sophisticated price-setters. For example, heuristic price-setting behavior is associated with a 0.3 or 0.4 percent higher price for every 1 percentage point of effective tax increase. Hosts who enable instant booking, on the other hand, have listing prices that are approximately 0.1 percent lower for every 1 percentage point of effective tax increase.

To further explore the relationship between "attention" and pass-through,we examine how price setting response is related to hosts' pre-policy price correlation with local hotel prices. In comparison to the previously discussed binary characteristics, this measure is continuous, and arguably more comprehensive than self-reported attributes like whether an entire unit is being rented out. To the extent that hotels and Airbnb rental properties are even imperfect substitutes, demand shocks to the hotel market should affect the Airbnb market as well. And there are a number of reasons why hotel price movements should be informative about the direction and magnitude of these shocks: hoteliers, particularly those affiliated or owned by large chains, likely set prices centrally, have extensive experience in doing so, and are pricing a largely standardized product. It is therefore likely that when hotel prices rise or fall, it is due to changes in the demand for short-term rentals that apply to Airbnb hosts as well.

Figure **??** (top panel) plots event study coefficients for hosts, estimated separately by whether hosts' pre-policy price correlations with hotel prices are above or below the median within their metro. Hosts whose prices correlated more closely with those of hotels are also more likely to adjust their prices upward by less after the policy, passing through less of the tax to consumers, at least initially. Figure **??** (bottom panel) plots event study coefficients estimated separately for hosts at different deciles of the host-hotel price correlation distri-

[9]Assuming the listing price before the policy was optimal, and demand is not perfectly inelastic, the optimal listing price after the policy is lower.

bution; here, it is even more apparent that the more "sophisticated" hosts, whose prices tracked more closely to those of hotels, pass on less of the tax in the time shortly after the policy. The subsequent convergence of prices may suggest that these sophisticated hosts, upon learning more information about the resilience of consumer demand for Airbnb rentals, bring prices up and in line with those of hosts who were inattentive to the policy's impact on prices.

6 Effect of VCA on Entry and Exit

In addition to adjusting prices, hosts can respond to the policy on the extensive margin: by deciding whether and where to list their properties. Hosts whose costs exceed their listing price in the absence of "evasion rents" have two extensive margin responses. First, they may exit the market for short term rentals altogether. Second, they may continue evading the tax by listing on an alternative platform that does not remit tax on behalf of hosts. Similarly, some prospective hosts who would have entered the Airbnb market prior to the policy may choose not to in light of it, or may choose to enter the market through an untaxed platform. We refer to this behavior as "platform jumping." To the extent that some of the tax savings are reflected in lower prices on the untaxed platform, consumers will also have an incentive to search on that platform. We examine both extensive margin behaviors in the next two sub sections.

6.1 Airbnb Exit

One plausible margin of adjustment to the policy is a host's decision to exit the Airbnb market. This decision can appear in the data in one of two ways. First, a host can delete her account, which is indicated by her listing no longer being observed after the exit date. Second, a host can "effectively exit" the market by no longer actively making her unit available. (Airbnb is set up to require that hosts actively identify dates during which their units are listed on their calendars as available.) To determine what length of continuous inactivity likely signals an effective exit,we compare the likelihood that a host exhibits subsequent activity–by making the unit available or having it reserved–after inactivity spells of varying. On the basis of this analysis,wefind that after 90 or more days of inactivity, hosts have a ten percent or smaller likelihood of becoming active again, and therefore use 90 days as a threshold for effective exit.

We then estimate the triple difference specification (1) where the dependent binary variable y_{imct} is equal to one if host i exited the market–either by deleting her account or effectively exiting–on or after time t.[10] Table **??** reports the results. On average, the policy increased the likelihood of exit by one third of one percentage point (Column 1). For comparison, the likelihood that a host in a control city leaves the market on any given day is approximately 1.2 percent, implying that the policy increased the likelihood of exit by 25 percent.

[10] To perform this analysis, the listing-date analysis dataset is extended so that each listing is observed through the end of the data window. This means that if a host deleted her account prior to the end of the data window, new records are created for which the host's listing is flagged as having exited the market.

6.2 Platform Jumping

VRBO, Airbnb's main competitor, is one such alternative (untaxed) platform. Platform jumping might be more prevalent between VRBO and Airbnb because the interfaces and requirements for the two sites are virtually identical. While creating an account for the first time on any platform takes a modest amount of effort (Airbnb advertises that it takes less than an hour), the marginal cost of creating an additional listing profile on a similar platform is likely even lower.

We test for a decline in Airbnb entries and reservations (and a corresponding increase in VRBO entrants and bookings) by estimating the following difference-in-differences equation separately for each platform:

$$y_{mt} = \gamma_m + \gamma_t + \pi POST_{mt} + \varepsilon_{mt} \tag{5}$$

where the dependent variable measures entrants/bookings in metro m in month t. $POST_{mt}$ is equal to one in treated metros after the VCA implementation date. The identifying assumption is that parallel trends in entry exist between treated and untreated metros prior to implementation of the policy. While we find that entry into Airbnb declines and that VRBO entries increase, both effects are only marginally statistically significant.[11]

7 Interpretive Framework Sketch

This section first introduces a model of supplier behavior to develop an intuition for how Airbnb's policy of remitting hotel taxes on behalf of consumers changes the distribution of prices and supplier composition in equilibrium. Afterward, we conduct a welfare analysis to investigate how supplier price setting optimization failures, such as tax salience or professionalism, affect total welfare and consumer surplus.

In this model, hosts differ along two dimensions: quality of service and tax salience. Salient hosts are fully aware of the policy that shifted the remittance obligation to hosts, whereas inattentive hosts ignore this change.

7.1 Model

We consider a market with two types of listings: good-quality and low-quality. Good quality represents listings in popular neighborhoods with better amenities, etc. Low quality corresponds to "budget" listings. Naturally, there is some substitution between the two segments. c_s and f_s denote the marginal and fixed costs of hosts in each segment, respectively. We assume $c_G > c_L$. Additionally, hosts differ in their salient level, which we denote by $\theta \in \{0, 1\}$, where the host is salient if $\theta = 1$ and non-salient otherwise.

The price faced by the consumers in each segment for each type of host is denoted by $p^c_{s,\theta} = p_{s,\theta} + t$, where $s \in \{G, L\}$ denotes the segment. Consumers vary in their preference for good versus low-quality listings, represented by the parameter $\phi \sim F(.)$ The market demand in each market segment is determined by the host prices in the relevant segment, the other segment, the average price in the hotel market, and a taste parameter. $Q_s(p^c_s, \bar{p}^c_{-s}, \bar{p}^c_H|\phi)$,

[11] Alternative DDD specification:
$y_{pmt} = \gamma_p + \gamma_t + \gamma_m + \gamma_{pm} + \gamma_p POST_{mt} + \gamma_m POST_{mt} + \pi M_m P_p POST_{mt} + \varepsilon_{mpt}$

where $p_s^c = (p_{s,1}^c, p_{s,0}^c)$. $q_{s,\theta}$ denotes the individual demand a specific firm faces in segment s with salient θ.

A host of type θ in the market segment s faces the following perceived profit function.

$$\pi_{s,\theta} = (p_{s,\theta} - c_s)q_{s,\theta} - f_s \tag{6}$$

7.1.1 Model Predictions

The following are our tentative predictions on the price-setting behavior of salient and non-salient hosts in the Airbnb market.

Proposition 1. *Salient hosts decrease their prices after the policy change. The decrease in the price is a function of hotel price changes and the elasticity of demand.*

Proposition 2. *Non-salient hosts pass the tax to the consumers fully.*

The following are our tentative predictions on the change in the market composition after the policy change.

Proposition 3. *Non-salient hosts are more likely to exit the market. This is more prominent in the higher segment as there is spillover demand from the good-quality segment to the low-quality segment with the increase in consumer price due to the effective tax increase after the policy change.*

7.2 Welfare Analysis

Under construction.

8 Results Summary and Discussion

In this section,we consider the relevance of ourfindings to broader academic and policy discussions on the role that statutory features play in tax compliance, the long-term collection efficacy of VCAs, and the welfare and distributional consequences of taxing markets substantially populated by unresponsive sellers. In previous sections,we establish four main empirical findings:

*(1) Shifting the remittance duty substantially increased tax-inclusive prices.*we estimate this effect using both a triple difference and difference-in-discontinuities approach. Pooled triple difference estimates indicate the policy increased tax-inclusive prices by 0.9 percent for everyone percentage point of tax re-assigned to the platform, though estimates by metro vary.

(2) Shifting the remittance duty increased tax revenue collections. For every one percentage point re-assigned, tax revenues increase by 0.8 percent, scaled by Airbnb's market share, though it is not clear what portion of this effect is driven by an increase in traditional hotel prices.

*(3) Extent to which tax-inclusive prices increased correlated with attention.*we estimate the triple difference specification interacted with host characteristics likely associated with attention to price setting. Hosts whose prices closely correlate with traditional hotel prices pass on less of the effective tax increase.

*(4) Shifting remittance duty induces exit of less attentive hosts.*we find that host entry into the Airbnb market drops after VCA adoption, and, further, that entry into VRBO, Airbnb's closest competitor, increases after VCA adoption.

8.1 Location of the Duty to Remit Affects Compliance with, and Incidence of, Consumption Tax

Our results lend support to the evasion channel hypothesis of Kopczuk, Marion, Muehlegger, and Slemrod (2016), extended to a monopolistically competitive market structure. In other words, the change in tax incidence here may result, in part, from different evasion opportunities available to each side of the market. These different evasion opportunities mean that a tax levied on the demand side of the market may result in a different equilibrium outcome than a similar tax levied on the supply side of the market. Indeed, prior to the enactment of the policy, anecdotal evidence suggests very few hosts were complying with the law and remitting hotel taxes. Unlike textbook tax incidence examples, then, the policy may not merely shift tax incidence between the two sides of a market, but rather changes its overall magnitude as well.

A significant appeal of requiring firms to assess and remit taxes, such as payroll and income taxes in the U.S., is that it is more cost-effective to administer given the small number of firms relative to taxpayers. However, when there are many small "firms," each responsible for remitting a small fraction of total tax revenue–as is the case with individual Airbnb hosts and local hotel taxes–it becomes costly to monitor compliance with the tax. This situation is likely to grow more prevalent as technology and business practices lower the barriers to individuals monetizing their time or possessions; not only will many more people be subject to new tax obligations–stretching tax authorities thin–but they may also be unaware of them. If tax receipts do not keep pace with tax obligations, it will not always be clear whether sellers are making a conscious decision to evade in light of a low probability of detection, or whether they lack information about the tax and their duty to pay it. Distinguishing between these two will be crucial to designing remedies to ensure greater compliance.

8.2 Welfare and Distributional Concerns in Taxing Unresponsive Sellers

The presence of unresponsive sellers in a market, as appears to be the case with Airbnb hosts, can have significant welfare consequences. The tax salience literature has shown that, when taxes are not salient, consumers will underreact to them (Chetty, Looney, and Kroft 2009). As a result, the deadweight loss of imposing a sales tax is inversely proportional to how salient that sales tax is. Yet, in the context of Airbnb, if a host underreacts to a change in remittance obligation that functionally acts as an effective tax increase, she may end up passing through 100 percent of the tax to consumers. This, in turn, can have a large effect on consumer behavior and result in a greater deadweight loss than if the host was aware of, and responsive to, the tax. In the long run, this may be mitigated by the introduction of algorithms that assist hosts in setting prices, but in the short run, where pricing decisions are often the result of inertia or inattention, this remains a real concern.

8.3 The Promise and Peril of Government Reliance on VCAs

Voluntary compliance agreements are attractive tax collection tools for local governments for two reasons. First, in the U.S., most sales taxes are imposed by state and local governments that have limited power to compel "remote" or platform sellers to remit taxes; absent a federal solution, VCAs allow these governments to recoup some of this otherwise foregone tax revenue. Second, VCAs offer an alternative to information reporting for capacity-constrained states that may be unable to collect a tax even with identifying information about the seller.

However, the long-term effects of VCAs remain unclear, and may be potentially troubling. Platforms that negotiate VCAs with local governments often do so from a position of considerable market strength, and as a result they can secure significant concessions. For example, in exchange for remitting hotel taxes as a lump sum, Airbnb's VCAs with local governments do not require it to provide identifying information about hosts to the tax authorities. Not only does this prevent local tax authorities from recouping taxes owed on previous transactions from hosts directly, it also prevents them from monitoring their behavior on other platforms, including direct competitors to Airbnb, that have not signed VCAs. Put differently, VCAs can make local governments permanently dependent on the individual firm for significant revenues, and are signed when those firms have accrued sufficient market power to negotiate them on their terms.

9 Conclusion

In classical economic theory, the incidence of a consumption tax is exclusively determined by market-wide demand and supply elasticities. This paper contributes to an emerging empirical literature which suggests that other factors, such as assignment of the remittance obligation, may affect incidence in practice.

We find that shifting the legal obligation to remit hotel taxes from small, independent hosts to Airbnb increases after-tax prices paid by consumers. The magnitude of this effect differs by a number of host characteristics related to sophistication. While several rationalizations of our estimates are possible, this primary result is consistent with low levels of voluntary compliance among individual hosts prior to implementation of mandatory withholding, despite the existence of a paper trail and federal information reporting on Airbnb rental transactions. This finding has potentially important implications for understanding the potential revenue and distributional consequences of taxing non-employee service transactions facilitated by digital platforms.

Table 1.1: Airbnb Voluntary Collection Agreements

City	Tax Rate	Announcement Date	Implementation Date	Metropolitan Statistical Area
Treatment				
Boulder, CO	7.5		October 1, 2016	Boulder, CO Metro Area
Chicago, IL	4.5	February 1, 2015	February 15, 2015	Chicago-Naperville-Elgin, IL-IN-WI Metro Area
Cleveland, OH	3	June 20, 2015	July 1, 2016	Cleveland-Elyria, OH Metro Area
Washington D.C.	14.5	February 1, 2015	February 15, 2015	Washington-Arlington-Alexandria, DC-VA-MD-WV Metro Area
Golden, CO	7.5		November 1, 2016	Denver-Aurora-Lakewood, CO Metro Area
Kill Devil Hills, NC	6.75	May 23, 2015	June 1, 1915	Kill Devil Hills, NC
Jersey City, NJ	6	October 12, 2015	February 1, 2016	New York-Newark-Edison, NY-NJ-PA Metropolitan Statistical Area
Los Angeles, CA	14	July 18, 2016	August 1, 2016	Los Angeles-Long Beach-Anaheim, CA Metro Area
Malibu, CA	12		April 20, 2015	Oxnard-Thousand Oaks-Ventura, CA Metro Area
Newark, NJ	14.5	April 12, 2016	May 1, 2016	New York-Newark-Jersey City, NY-NJ-PA Metro Area
Oaks Island/Myrtle Beach	6.75	May 23, 2015	June 1, 2015	Myrtle Beach-Conway-North Myrtle Beach, SC Metropolitan
Oakland, CA	14	July 5, 2015	July 15, 2015	San Francisco-Oakland-Hayward, CA Metro Area
Palo Alto, CA	14	November 30, 2014	January 1, 2015	San Jose-Sunnyvale-Santa Clara, CA Metro Area
Philadelphia, PA	8.5	July 1, 2015	July 15, 2015	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD Metro Area
Portland, OR	11.5		July 1, 2014	Portland-Vancouver-Hillsboro, OR-WA Metro Area
Phoenix, AZ	5	July 1, 2015	July 1, 2015	Phoenix-Mesa-Scottsdale, AZ Metro Area
Santa Clara, CA	9.5		November 1, 2015	San Jose-Sunnyvale-Santa Clara, CA Metro Area
San Diego, CA	10.5	July 1, 2015	July 15, 2015	San Diego-Carlsbad, CA Metro Area
San Francisco, CA	16.5	August 1, 2014	October 1, 2014	San Francisco-Oakland-Hayward, CA Metro Area
San Jose, CA	10	January 1, 2015	February 1, 2015	San Jose-Sunnyvale-Santa Clara, CA Metro Area
Control				
Austin, TX	0	NA	NA	Austin-Round Rock, TX Metro Area
Dallas, TX	0	NA	NA	Dallas-Fort Worth-Arlington, TX Metro Area
Houston, TX	0	NA	NA	Houston-The Woodlands-Sugar Land, TX Metro Area
New Orleans, LA	0	NA	NA	New Orleans-Metairie, LA Metro Area
Savannah, GA	0	NA	NA	Savannah, GA Metro Area

Table 1.2: Sample Summary Statistics							
City	**N**	**N**	**N**	**Avg. Price**	**Avg. No. per Month per Host**		**Entire Apt. (%)**
	(Metro)	**(City)**	**(Listing X Days)**		**Price Changes**	**Reservations**	
Treatment							
Boulder, CO	3,657	2,446	193,878	143	2.9	4.9	24.1%
Chicago, IL	21,453	18,786	682,957	139	3.6	4.6	54.0%
Cleveland, OH	4,065	1,926	128,975	467	1.8	2.4	34.3%
Washington D.C.	22,401	11,904	769,856	148	3.0	4.9	51.8%
Golden, CO	12,927	112	373,278	124	3.6	5.4	29.8%
Kill Devil Hills, NC	151,370	3,993	2,064,362	170	1.6	4.3	81.4%
Jersey City, NJ	2,967	850	125,898	207	3.3	4.5	16.6%
Los Angeles, CA	87,598	51,793	2,437,104	198	3.5	5.0	49.2%
Malibu, CA	89,913	685	510,603	711	3.5	4.1	31.7%
Newark, NJ	151,370	503	876,406	161	1.6	4.6	77.0%
Oak Island, NC	3,454	467	106,225	198	2.7	3.9	10.6%
Oakland, CA	21,669	4,815	1,849,500	178	1.8	5.2	34.4%
Palo Alto, CA	14,720	1,813	1,323,801	415	2.1	3.9	33.0%
Philadelphia, PA	17,664	13,979	1,847,512	491	1.1	2.6	35.3%
Portland, OR	10,727	7,810	437,326	123	4.0	6.2	24.0%
Phoenix, AZ	12,219	4,438	505,783	329	2.7	3.8	22.9%
Santa Clara, CA	7,696	1,729	711,486	467	2.0	3.6	33.4%
San Diego, CA	21,096	14,995	686,205	219	3.8	4.4	29.9%
San Francisco, CA	47,623	25,954	5,558,380	218	1.7	5.2	45.7%
San Jose, CA	12,907	5,211	1,056,904	454	2.0	3.6	33.3%
Control							
Austin, TX	21997	19,250	949,109	277	3.3	3.6	25.9%
Dallas, TX	7823	3,710	274,168	142	3.5	4.7	47.6%
Houston, TX	12726	8,497	409,408	239	2.8	3.0	37.1%
New Orleans, LA	10539	9,723	424,294	187	4.3	4.6	29.2%
Savannah, GA	1847	1,082	66,989	235	5.7	6.7	22.8%

Table 1.3: Triple Difference; Dependent Variable Log Prices

	All Listings			Fixed Listing Composition		
City	Log Listing Price	Reservations	Log Price	Log Listing Price	Reservations	Log Price
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled	-0.001***	-0.001***	0.009***	-0.001***	-0.000***	0.009***
	0.000	0.000	0.000	0.000	0.000	0.000
Boulder, CO	0.039***	-0.073***	0.111***	0.021***	-0.041***	0.093***
(t=7.5%)	(0.002)	(0.003)	(0.002)	(0.002)	(0.004)	(0.002)
Chicago, IL	-0.009***	0.002	0.035***	-0.006***	-0.002	0.038***
(t=4.5%)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Cleveland, OH	0.032***	0.045***	0.062***	0.005	0.010***	0.035***
(t=3%)	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)
Washington D.C.	0.001**	-0.017***	0.137***	0.002***	-0.028***	0.138***
(t=14.5%)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Golden, CO	0.032***	-0.074***	0.104***	0.031***	-0.041***	0.103***
(t=7.5%)	(0.002)	(0.005)	(0.002)	(0.002)	(0.006)	(0.002)
Jersey City, NJ	0.009***	-0.017***	0.068***	0.014***	-0.005***	0.073***
(t=6.75%)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Kill Devil Hills, NC	0.004	0.088***	0.069***	-0.056***	0.034***	0.010***
(t=6%)	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)	(0.003)
Los Angeles, CA	0.002***	-0.023***	0.133***	-0.011***	-0.023***	0.120***
(t=14%)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Malibu, CA	0.015***	-0.005***	0.128***	0.001	-0.013***	0.114***
(t=12%)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Newark, NJ	-0.035***	-0.035***	0.023***	-0.034***	-0.019***	0.024***
(t=14.5%)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Oak Islands	-0.035***	0.016***	0.030***	-0.094***	0.027***	-0.028***
(t=6.75%)	(0.002)	(0.003)	(0.002)	(0.004)	(0.005)	(0.004)
Oakland, CA	0.006***	-0.030***	0.137***	0.002***	-0.016***	0.133***
(t=14%)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Palo Alto, CA	-0.019***	-0.008***	0.112***	-0.036***	0.017***	0.095***
(t=14%)	(0.001)	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)
Philadelphia, PA	-0.012***	-0.030***	0.070***	-0.005***	-0.012***	0.077***
(t=8.5%)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Phoenix, AZ	-0.070***	-0.019***	-0.021***	-0.028***	0.016***	0.021***
(t=5%)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Santa Clara, CA	0.045***	-0.096***	0.136***	0.037***	-0.034***	0.127***
(t=9.5%)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
San Diego, CA	-0.001	0.028***	0.099***	-0.018***	0.022***	0.082***
(t=10.5%)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
San Francisco, CA	0.028***	-0.088***	0.181***	0.026***	-0.044***	0.179***
(t=16.5%)	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.001)
San Jose, CA	-0.017***	-0.021***	0.078***	-0.048***	0.006**	0.047***
(t=10%)	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.001)

Table 1.4: Regression Discontinuity Estimate on Log Price at the Municipal Border

		30 Day Window			60 Day Window		
City	**DDD***	**Pre**	**Post**	**Diff-Disc**	**Pre**	**Post**	**Diff-Disc**
		(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Los Angeles, CA	0.133***	0.005	0.099***	0.127***	0.012	0.095***	0.110***
(t=14%)	(0.001)	(0.02)	(0.02)	(0.004)	(0.01)	(0.01)	(0.002)
		373,888	390,840	878,352	748,978	806,280	1,768,612
San Diego, CA	0.099***	0.051***	0.182***	0.099***	0.056***	0.177***	0.101***
(t=10.5%)	(0.001)	(0.00)	(0.00)	(0.010)	(0.00)	(0.00)	(0.006)
		420,674	457,678	287,399	811,026	957,586	577,025
Palo Alto, CA	0.112***	0.046	0.065***	0.057**	0.026	0.043***	-0.003
(t=14%)	(0.001)	(0.03)	(0.02)	(0.027)	(0.02)	(0.01)	(0.016)
		12,992	29,058	42,050	25,185	60,365	85,550
San Jose, CA	0.078***	-0.008	0.090***	0.093***	0.008	0.101***	0.120***
(t=10%)	(0.001)	(0.01)	(0.01)	(0.017)	(0.01)	(0.01)	(0.010)
		142,158	145,241	57,507	284,778	292,247	115,281
Santa Clara, CA	0.136***	0.098*	0.281***	0.105***	0.043	0.283***	0.097***
(t=9.5%)	(0.002)	(0.05)	(0.04)	(0.013)	(0.03)	(0.03)	(0.007)
		28,275	29,232	93,970	57,581	57,700	190,713
Oakland, CA	0.137***	0.030	0.040	0.178***	-0.001	0.032*	0.151***
(t=14%)	(0.001)	(0.02)	(0.02)	(0.024)	(0.02)	(0.02)	(0.015)
		46,951	47,019	52,229	94,933	95,780	109,935
Chicago, IL	0.035***	0.041	0.122***	0.116***	0.021	0.057*	0.131***
(t=4.5%)	(0.001)	(0.04)	(0.04)	(0.006)	(0.03)	(0.03)	(0.003)
		24,215	28,014	211,555	46,521	63,414	424,835
Washington D.C.	0.137***	-0.029***	0.095***	0.029**	-0.027***	0.092***	0.033***
(t=14.5%)	(0.001)	(0.01)	(0.01)	(0.013)	(0.01)	(0.01)	(0.008)
		105,734	105,821	316,259	210,914	213,920	647,244
Phoenix, AZ	-0.021***	0.155***	0.280***	0.131***	0.160***	0.271***	0.115***
(t = 5%)	(0.001)	(0.01)	(0.01)	(0.010)	(0.01)	(0.01)	(0.006)
		107,995	108,313	216,308	219,713	220,783	440,496
Boulder, CO	0.111***	-0.006	0.088***	0.083***	0.008	0.110***	0.068***
(t=7.5%)	(0.002)	(0.02)	(0.02)	(0.022)	(0.02)	(0.01)	(0.012)
		58,638	60,755	121,829	116,746	123,779	250,440

Notes. RD coefficients are estimated separately for the thirty day and sixty day intervals around the policy, estimates are reported in columns (1), (2) and columns (4), (5) respectively. Difference and discontinuity estimates are reported in col. (3) and (6). All specifications include for listing characteristics and time fixed effects. To ensure that like listings are being compared, I calculate the closest border vertex for each listing, and include vertex fixed effects. The sample is limited to listings within two miles on either side of the municipal border. *Triple difference estimates from Table 1.3, col (3) have been repeated for readers' convenience.

Table 1.5: Pooled Triple Difference Estimates on Log Price by Host Characteristic

Dependent Variable: Log Price	**(1)**	**(2)**	**(3)**	**(4)**
Instant Book Enabled?	-0.002***	-0.001***	-0.002***	-0.001***
	0.000	0.000	0.000	0.000
Base Divisible by 10	-0.001***	0.004***	-0.001***	0.004***
	0.000	0.000	0.000	0.000
Base Divisible by 5	-0.002***	0.003***	-0.002***	0.003***
	0.000	0.000	0.000	0.000
Entire Apartment	0.007***	0	0.008***	0
	0.000	0.000	0.000	0.000
Multiple Properties	0.002***	0	0.001***	-0.000***
	0.000	0.000	0.000	0.000
Host Fixed Effects?	No	Yes	No	Yes
Control Cities?	No	No	Yes	Yes

Table 1.6: Effect of VCA on Log of Hotel Tax Revenue

	Diff-in-Diff	Event Study
	(1)	(2)
Treat * Post* Airbnb Market Ratio	2.656*	
	(1.09)	
Pre Month 4		-0.973
		(2.32)
Pre Month 3		-2.328
		(2.53)
Pre Month 2		-1.769
		(2.59)
Post Month 0		0.599
		(3.19)
Post Month 1		-0.498
		(3.11)
Post Month 2		0.736
		(3.18)
Post Month 3		0.244
		(3.35)
Post Month 5		0.240
		(3.43)
Post Month 6		0.892
		(3.06)
Post Month 7		2.770
		(3.33)
Post Month 8		1.463
		(3.27)
Post Month 9		1.810
		(3.39)
Post Month 10		1.717
		(3.36)
Post Month 11		2.090
		(3.65)
Post Month 12		3.951
		(3.58)
Post Month>12		8.946***
		(2.40)
N	659	659

Notes. Dependent variable is log of city's monthly hotel tax revenues. Treatment is defined as the interaction between Ever Treated City*Post* Relative Airbnb Market Size at the time of treatment. Col. 1 reports difference and difference estimates, Col (2) reports event study estimates for the equivalent specification. Cities included in the sample are Palo Alto, Chicago, Washington D.C. and San Diego (treated); Austin, Dallas and Houston (Never treated). Standard errors are reported below coefficient estimates. All specifications include controls for seasonality.

Table 1.7: Effect of Policy on Entry and Exit (Difference-in-Differences)

	Airbnb Entry	**Airbnb Entry (logs)**
	(1)	**(2)**
Platform* Treat* Post	-120.743	-1.743
	(55..68)	(1.68)
N	1647	1647

Notes. Col.1 reports average effect of the policy on Airbnb hosts (absolute measure). Col. 2 reports the effect of airbnb entry in logs. Both specifications estimated with seasonal effects. Treatment is defined as the interaction between Platform*Ever Treated City*Post. Platform is equal to 1 if Platform is Airbnb. Standard errors are reported under coefficients.

Figure 1.1: Airbnb Search Results

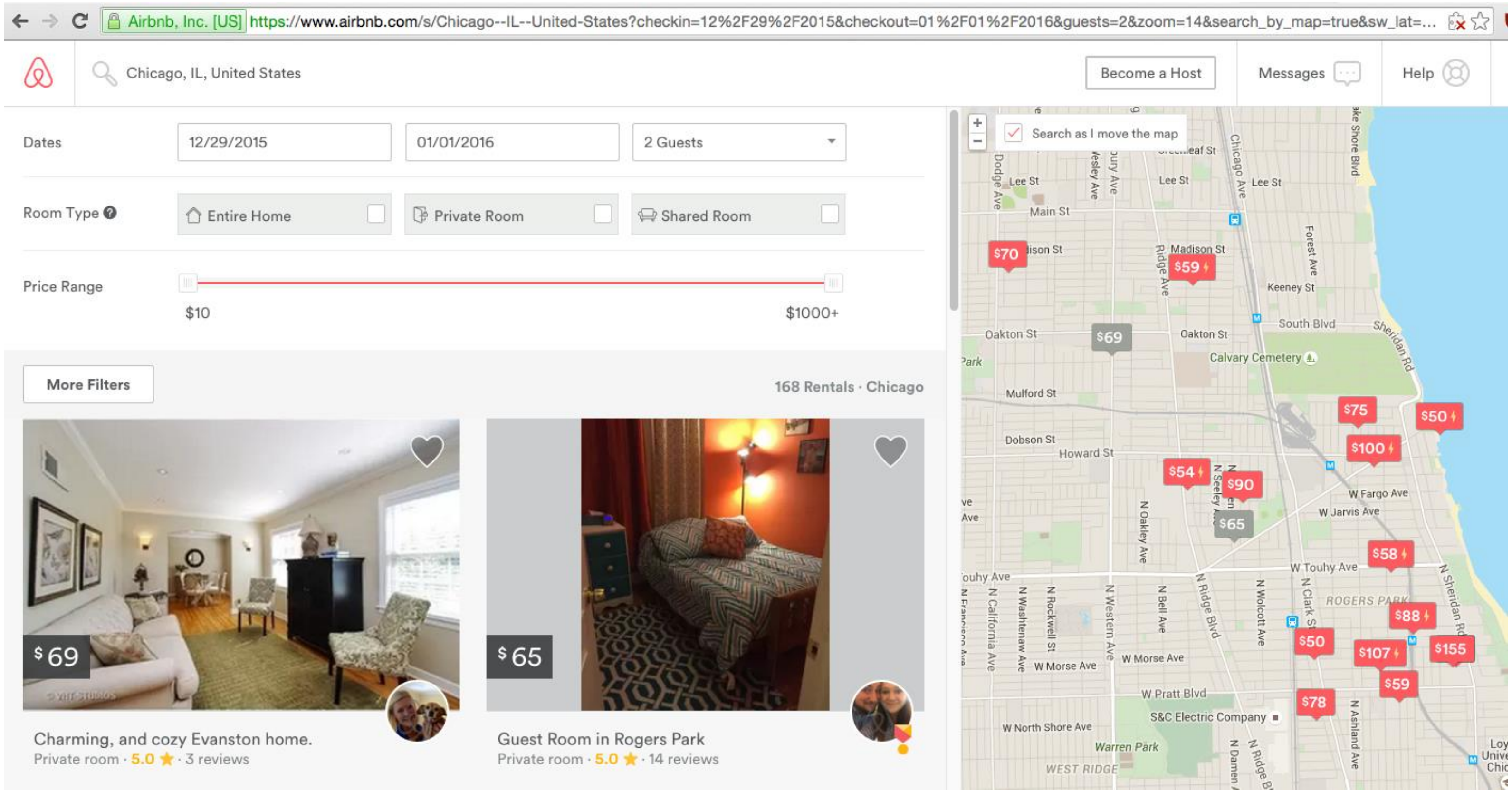
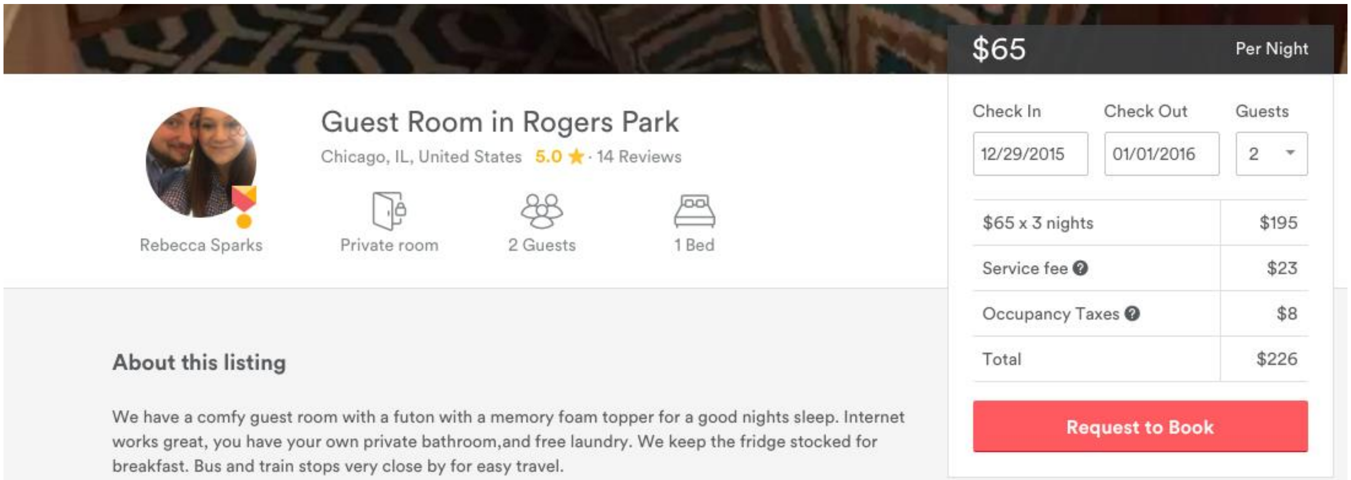
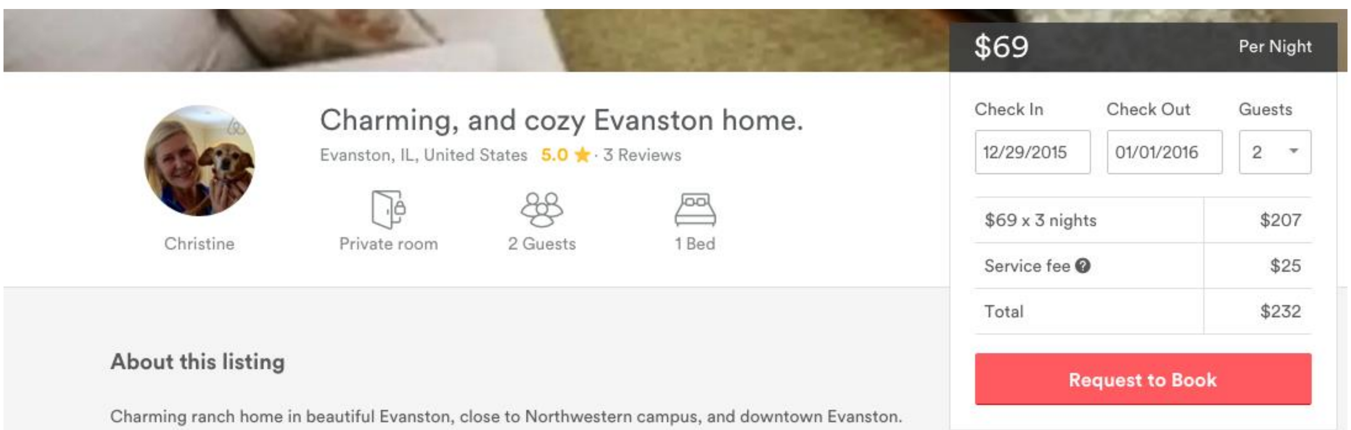


Figure 1.2: Airbnb Listing Details in Chicago and Evanston, IL



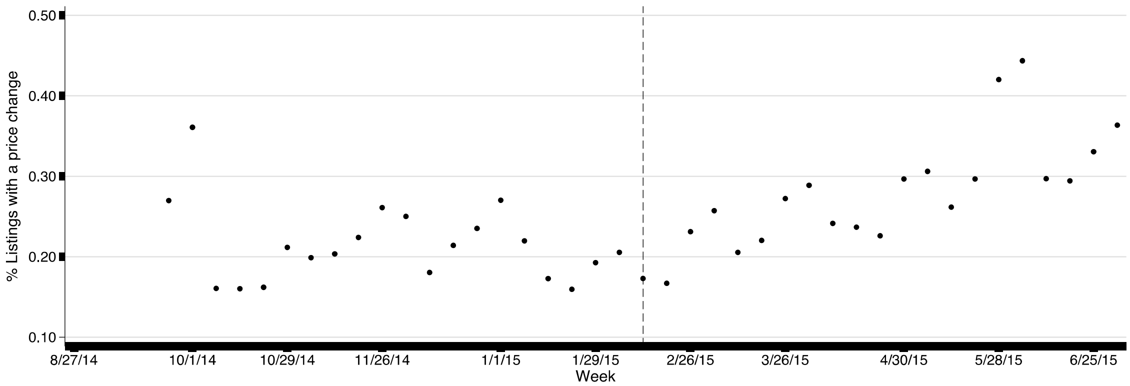
Chicago, IL



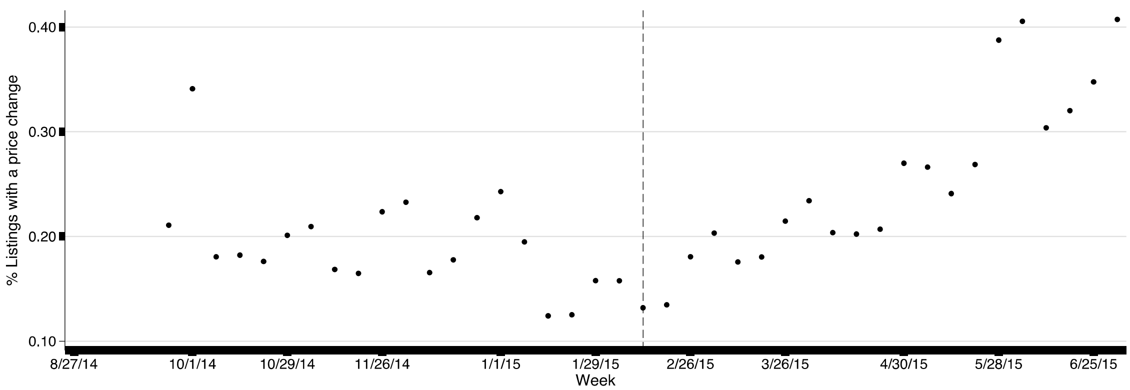
Evanston, IL

Figure 1.3. Percent of Listings with Price Changes (Weekly)

Washington DC



Chicago, IL



San Diego, CA

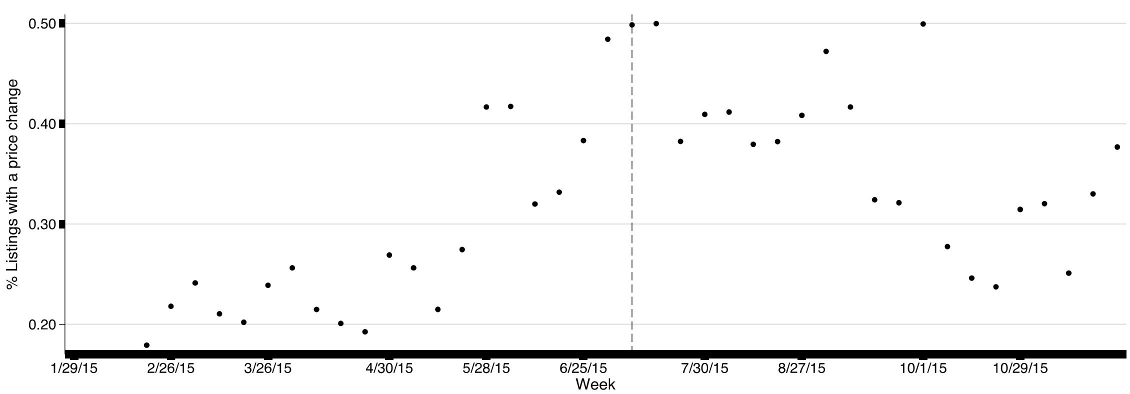


Figure 1.4. Spatial Distribution of Listings, Relative to City Boundaries (D.C.)
Note: Red dots represent Airbnb listings, Orange dots represent VRBO listings)

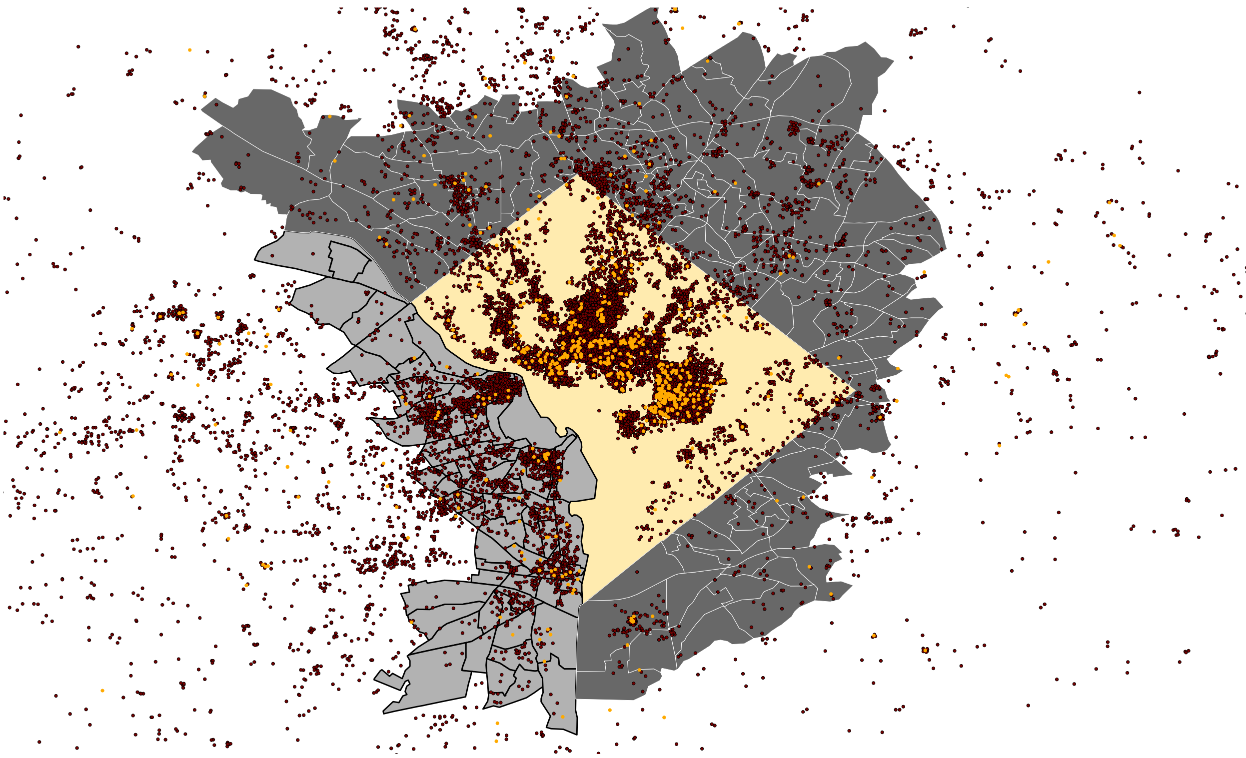
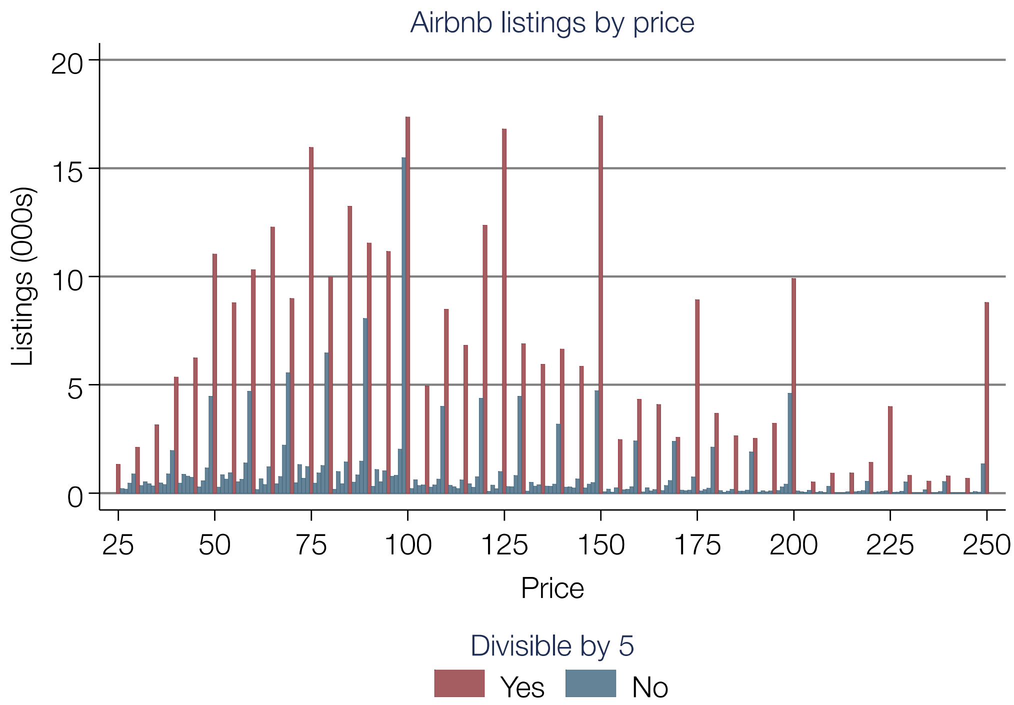


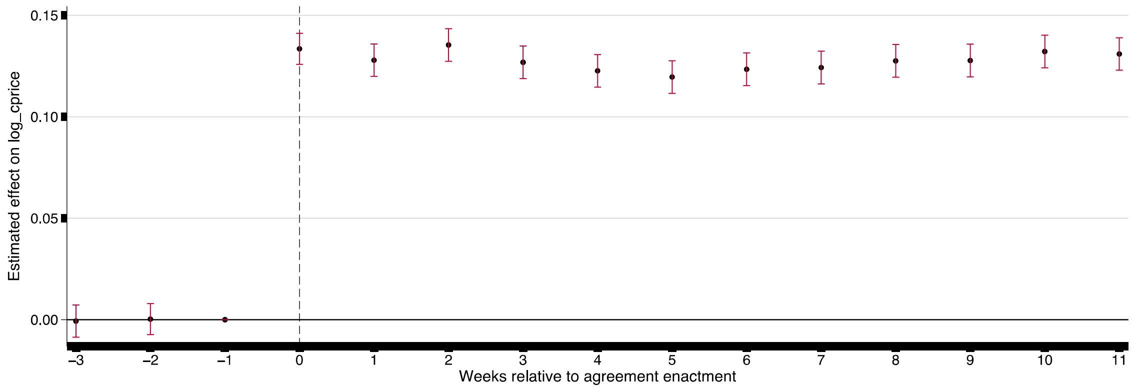
Figure 1.5: Histogram of Listing Prices



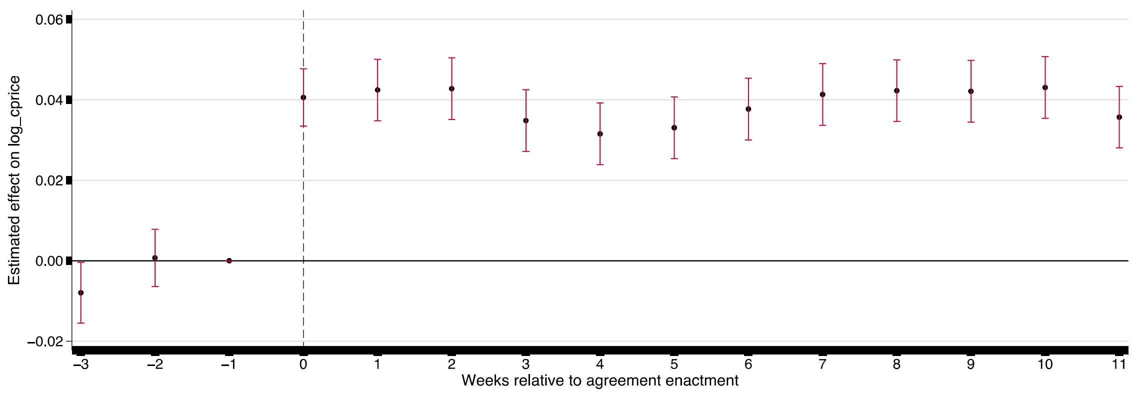
Notes: Figure displays the frequency of listings by price, for all observed listings priced under $250.

Figure 1.6. Event Study Estimates of Policy on Log of Booking Price

Washington DC



Chicago, IL



San Diego, CA

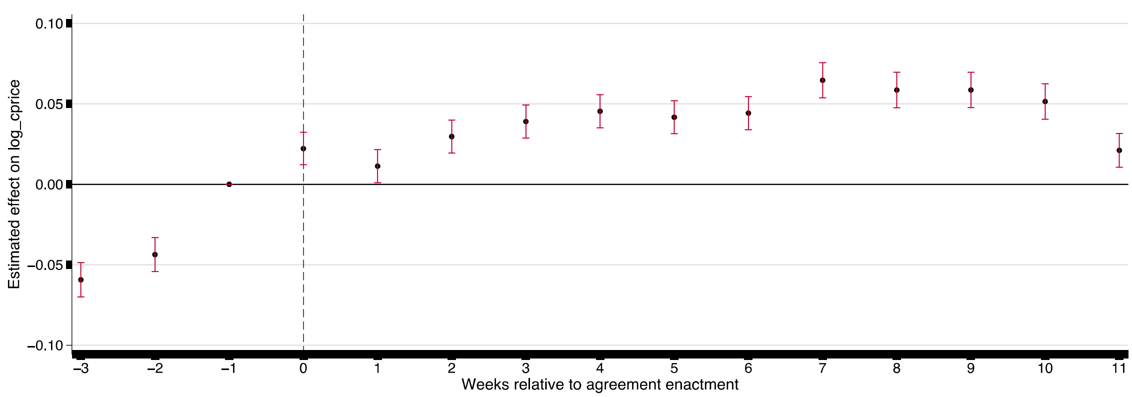
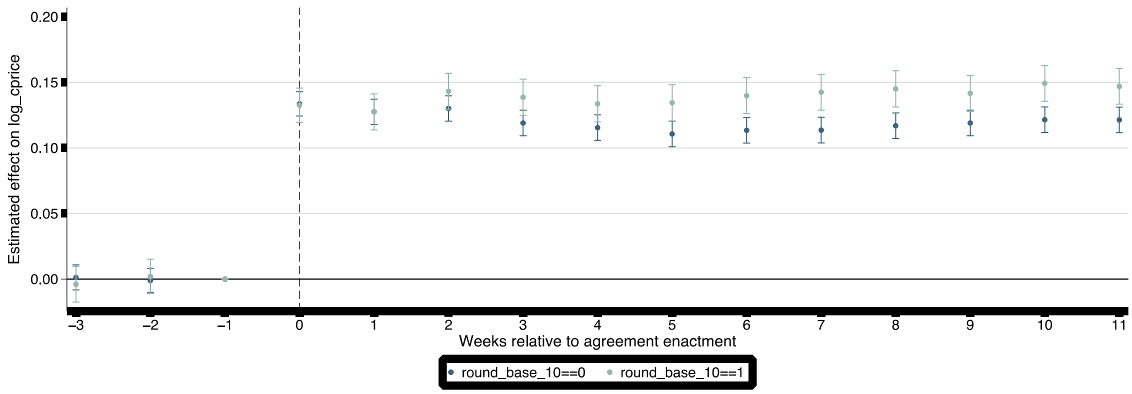
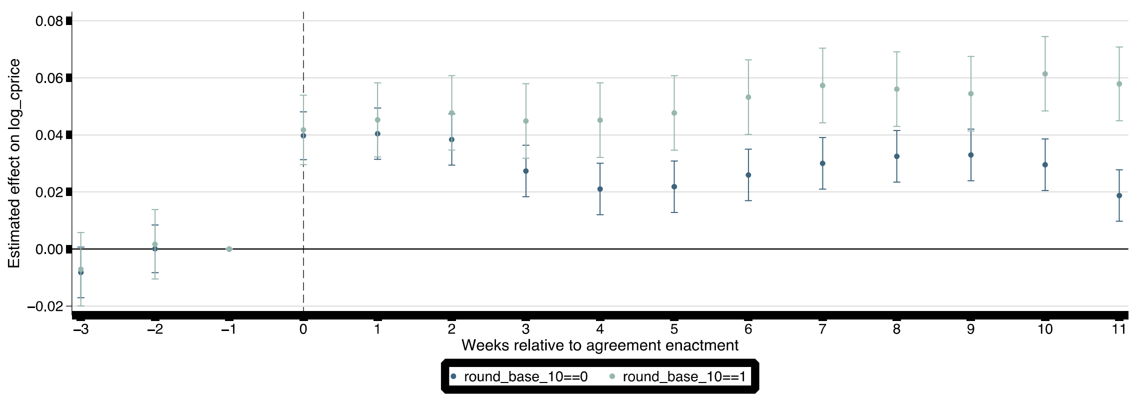


Figure 1.7. Event Study Estimates for Listings with Round Base (Divisible by 10)

Washington, D.C.



Chicago IL



San Diego, CA

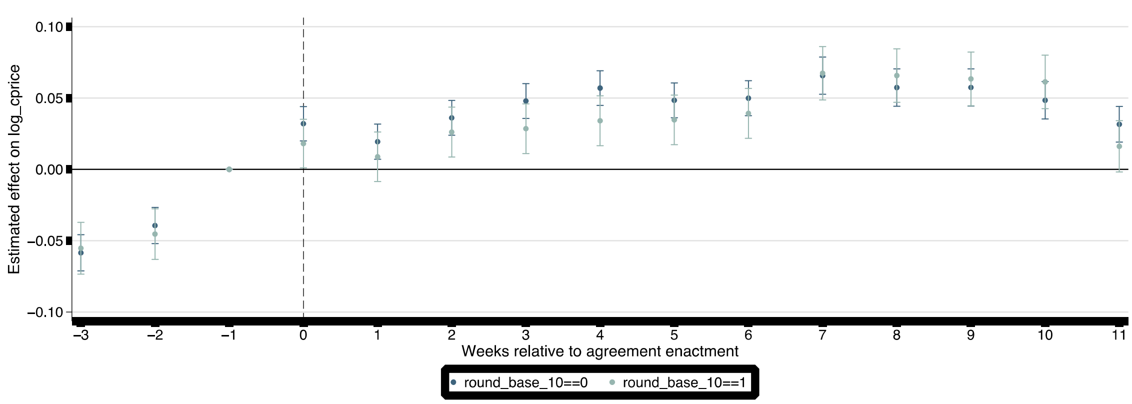


Figure 1.8: ES on Log Price by Pre-policy Correlation with Hotel Price

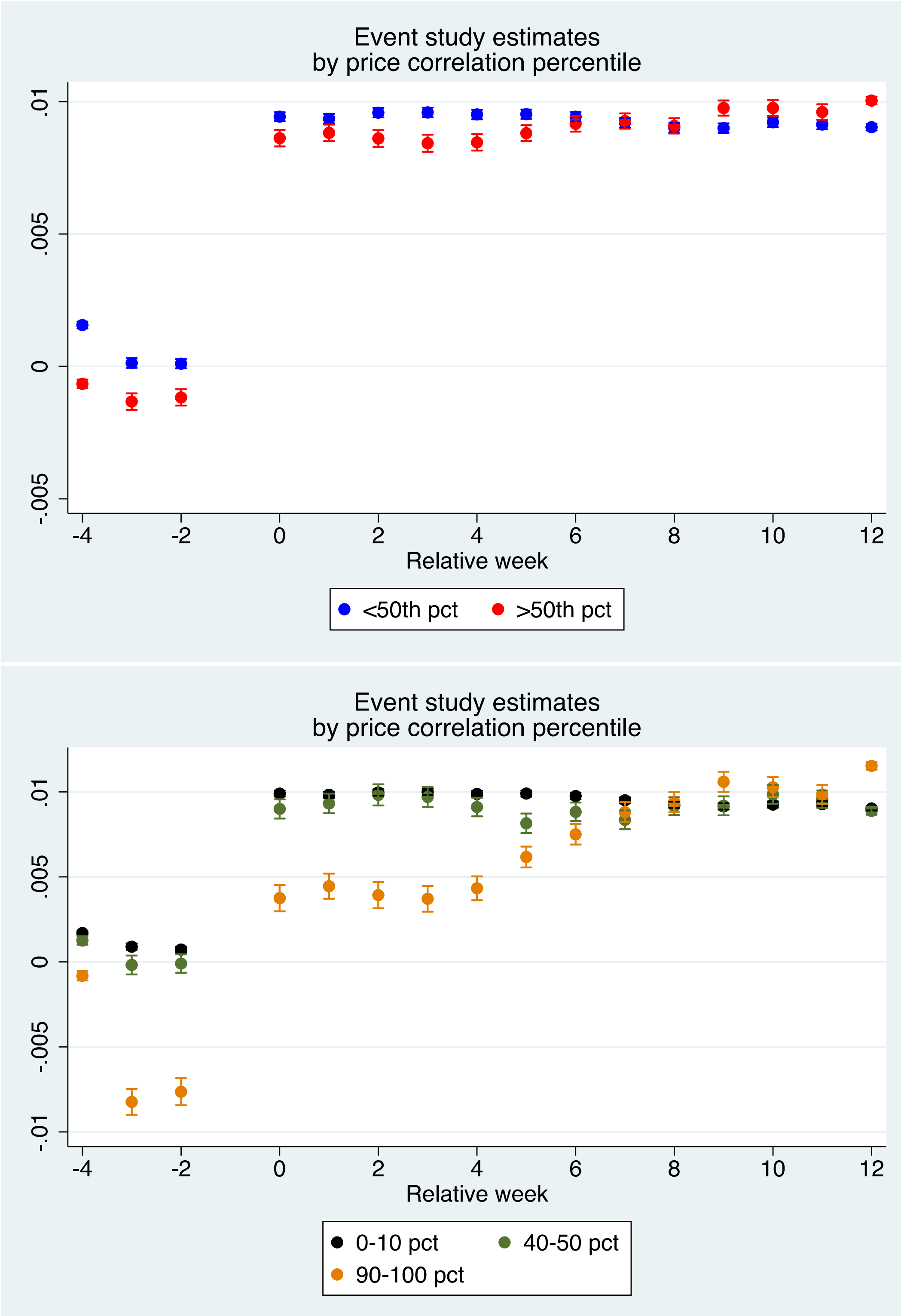
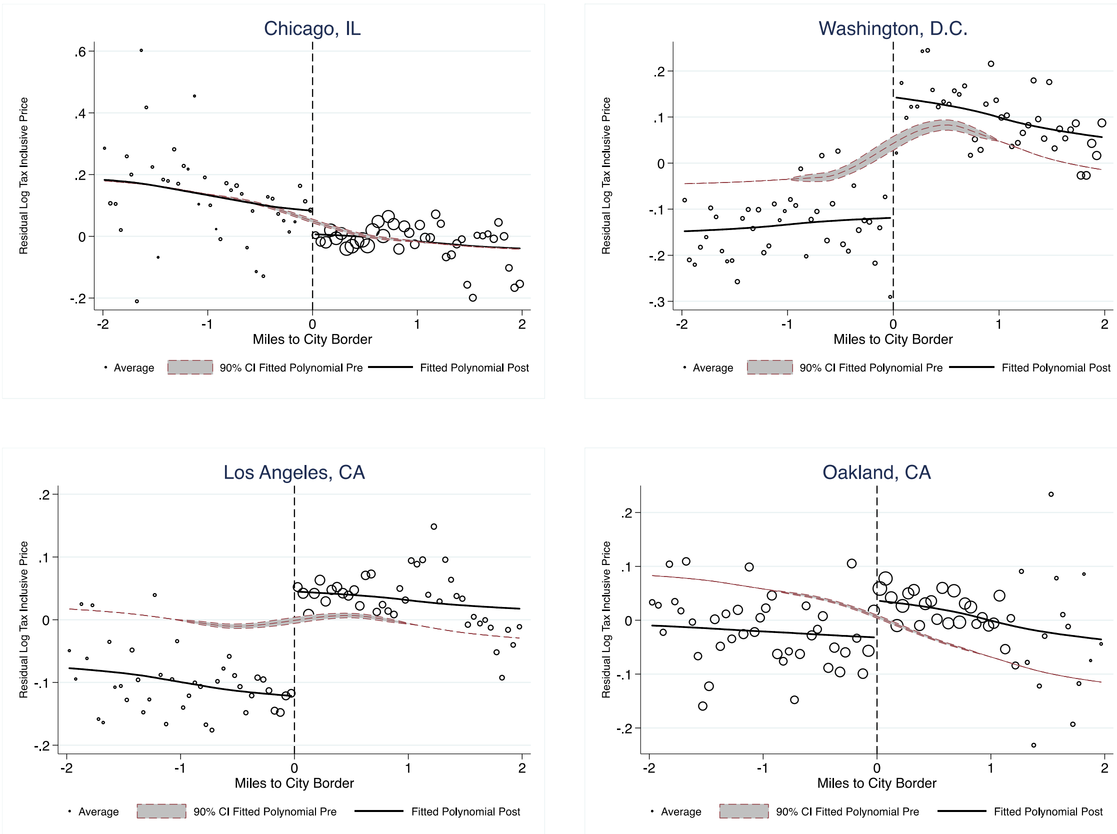


Figure 1.9: Difference in Discontinuity Residuals



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