Amateurs and Incidence:
Evidence From a Natural Experiment Involving Airbnb and Hotel Taxes

Elisa Cheng, Yeliz Kacamak & Eleanor Wilking

This draft: March 2023
Click here for most recent draft.

How does the obligation to remit affect consumption tax incidence? In classical tax theory, assigning the responsibility to transfer tax revenue to the government has no effect on which party bears the economic burden of a consumption tax. We explore this prediction in the context of agreements between city governments and a large digital platform firm that shifted the obligation to remit hotel taxes from independent renters to the platform firm itself. Using variation in the location and timing of these agreements, we identify a substantial increase in advertised tax-inclusive rental prices—a violation of remittance invariance—but comparatively modest declines in completed reservations. A contemporaneous increase in hotel tax revenue collections suggests that the policy was an effective tax increase, assessed on previously non-compliant renters. We explore heterogeneity in pass-through of this effective tax increase using several proxies for renter price-setting sophistication. Pass-through was lowest among full-space, frequent renters who likely faced smaller optimization frictions relative to more amateur renters. Our results indicate that shifting the remittance obligation to the platform increases after-tax prices and raises revenue, suggesting that consumers bear a greater share of the tax burden when the remittance obligation is shifted to a party with fewer evasion opportunities.

1. Introduction

A fundamental tenet of classical tax theory is that the economic burden of a tax is independent of which side of the market *remits,* or transfers tax monies to the government (Myles 1989; Weyl and Farbinger 2013). Tax incidence, so the theory goes, depends only on the relative demand and supply elasticities: the less elastic party bears more of the tax.

However, recent studies have identified at least two circumstances in which who remits affects incidence in practice. First, who remits matters when one side of the market has access to differential evasion opportunities (Kopczuk 2009; Goolsbee 2012). Second, who remits matters when agents face optimization frictions (Chetty 2009; Finkelstein 2009). 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. In situations where, on average, consumers and suppliers face different optimization frictions, shifting the remittance duty from consumers to suppliers, or vice versa, will affect tax incidence.

We extend this literature by studying a context in which both optimization frictions and differential evasion opportunities are likely present. In a simple model, we show how these characteristics separately and jointly affect equilibrium prices. To study this phenomenon empirically, I 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 VCA adoption has on consumer prices, we employ two complementary estimation techniques that rely on separate identifying assumptions. First, I exploit 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 two sets of controls: metropolitan areas that did not adopt VCAs and neighboring jurisdictions within 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, which I interpret as circumstantial evidence that failure to remit was widespread.

This result complements the main finding in Kopczuk, Marion, Muehlegger, and Slemrod (2016) 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 taxes' existence or their obligation to remit them. I therefore 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 which we find empirical support.

We 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. We do this by re-estimating event study and difference-in-difference models while interacting the policy variable with characteristics of suppliers and their surroundings. For example, we find 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, in consequence, pass-through in markets with inattentive, or amateur suppliers may be different than in markets with traditional firms. Although there are various studies which already suggest that consumers face optimization frictions that affect their responsiveness to changes in tax rate or tax administration (e.g., Chetty et. al 2010; Goldin and Homonoff 2012; Homonoff 2016; Lockwood 2017), 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 (see, e.g., Myles 1989; Weyl and Fabinger 2013), 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 (Harju, Kosonen, and Skans 2015). 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 (Kopczuk et al. 2012; Stolper 2016). However, we proceed with caution in inferring that the policy changed the tax incidence, at least incidence in the way that it is conventionally defined—as the ratio of reductions of total consumer and producer surplus that results from imposition of a tax. We interpret our results 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 in the main on data from a single platform firm, in a single industry. While we duly acknowledge that this inherently limits generalizability of our estimates, we maintain 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—lots of amateurs in the market characterizes other emerging platform or "market-maker" driven markets.

For clarity, we explicitly define key terms employed throughout the paper as follows. I 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, I 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 a host who is a casual participant in the rental market—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 which I use to motivate and interpret the empirical findings. Section 3 provides background on Airbnb rental markets and the natural experiment afforded by cities' VCA negotiations, 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 impacted 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.

[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).

2. Conceptual Framework

This section sets forth a simplified, partial equilibrium model of supplier behavior to develop intuition for how Airbnb's policy of remitting hotel taxes on behalf of consumers changes the distribution of prices and supplier composition in equilibrium. It also offers several predictions for how these changes differ based on the level of pre-policy compliance (e.g., how many hosts were remitting taxes voluntarily) and hosts' price setting sophistication.

In this model, hosts differ along two dimensions: honesty and attention. Honest hosts remit in full any known tax obligations; dishonest hosts remit nothing, or some fraction of their true liability. Hosts also vary in innate attention, which affects their price setting in two ways: inattentive hosts are less perceptive of demand for their listing—leading to errors in price-setting—and, in addition, are unaware of their remittance obligation, inhibiting optimal response when the remittance regime changes.

Comparative static analysis yields three predictions. First, in the absence of evasion, the policy will not affect tax inclusive prices. Second, in the presence of evasion, more attentive hosts pass through less of the effective tax increase. Finally, on the extensive margin, the policy may induce some non-compliant hosts to exit the market.

This model makes several assumptions that are unlikely to hold in reality. We assume the prices of other goods do not enter explicitly into the host's price-setting. However, in a general equilibrium model with a representative consumer who optimizes with respect to all goods in the economy, this choice does not *necessarily* preclude other hosts' prices from having an *indirect* effect on the host's price-setting through the parameters of her demand function. To see this, consider a typical linear inverse demand function $p_i = a - bq_i$. If the relative price of the monopolist's good increases—say, because other goods that consumers purchase become less expensive—this will be expressed through a downward shift of the entire demand function (i.e., a decrease in the value of *a*, the demand curve intercept). Thus, changes in relative prices may shift the value of *a*, which in turn would affect the monopolist's optimal price. However, in our one period, partial equilibrium model, the parameter values of an individual host's demand function are static (i.e., the values of *a* and *b* are fixed).[2]

A related limitation is that the guest's decision of which listing to choose given prices and availability is omitted from the model. While it may be the case that demand for temporary lodging

[2] The intuition still pertains if Airbnb listings were imperfect substitutes for one another (i.e., moving from a strict monopolist to a model of imperfect competition). Imagine that there are two hosts with imperfectly substitutable listings who differ along a single dimension: honesty. In a Nash-in-prices equilibrium, the optimal price of each listing takes the other listing's price as an argument. The policy triggers a series of strategic price interactions that shift the market to the new equilibrium. In contrast to the monopolist set-up, the post-policy difference in price response by type is muted. The honest host will lower her listing price, but not by the full amount of the tax, because her optimal price is a function of the dishonest host's price response. The basic logic of this simplified two host model flows through to the current model with three types of hosts. As in the example, the price responses of the attentive-honest and attentive-dishonest would be attenuated. The price response of inattentive hosts will depend on the extent to which the inattentive host accurately perceives how the policy changes the best response function of other hosts. Either the inattentive host is oblivious to the price changes of other hosts, which would appear to assume the result, or the inattentive host perceives that her competitor's prices have changed, and responds by lowering her listing price—which (a) muddles the meaning of "inattentive" and (b) is not in line with the empirical finding that a sizeable number of hosts do not adjust at all to the policy. More generally, the strategic price interactions required for an imperfectly competitive setting seem incongruent with the limited experience and resources that characterize price-setting for the average Airbnb host.

within a city is relatively inelastic in the short run, it is almost certainly more elastic in the long run. If this is the case, then the incidence of the hotel tax will be increasingly borne by hosts in the long run, as guests have their choice of visiting cities with and without remittance agreements.

2.1 Set-up

Each Airbnb host supplies a listing made unique by its location and amenities from those offered by competitors. For simplicity, each host i is assumed to be a monopolist facing a downward-sloping demand q_i in price p_i for her listing.[3] The price paid by guests is either the one set by the host if hosts are expected to remit ($\theta = 0$), or the one set by the host plus a specific tax t if guests are expected to remit ($\theta = 1$).[4] More succinctly, guests face a price of $p_i^c = p_i + t\theta$.

In the host's optimization, however, what the guest is *perceived to pay* is a function of the host's attentiveness, $a_i \in \{0,1\}$. An attentive host ($a_i = 1$) correctly perceives the price paid by guests ($p_i + t\theta$), while an inattentive host ($a_i = 0$) is both unaware that guests might be required to remit a tax *and* misestimates demand due to the inclusion of a noise term ε_i. Therefore, hosts optimize with respect to a guest price of $p_i^c = p_i + ta_i\theta + (1 - a_i)\varepsilon_i$.

Each host chooses a price to maximize her *perceived* profit:

$$\pi_i^P = [p_i - ta_i(1-\theta)(1-\gamma_i) - c_i]q_i(p_i + ta_i\theta + (1-a_i)\varepsilon_i) - F \quad (1)$$

where c_i is an exogenous, host-specific marginal cost, and F is a uniform fixed cost. In contrast, a host's *actual* profit is:

$$\pi_i = [p_i - ta_i(1-\theta)(1-\gamma_i) - c_i]q_i(p_i + t\theta) - F \quad (2)$$

Relative to attentive hosts, the perceived profit of inattentive hosts differs in two ways that affect price-setting. First, as described earlier, inattentive hosts fail to account for the prices actually paid by guests, due to being unaware of guests' possible remittance obligations and due to demand forecast error. The decision to relate hosts' attentiveness to the tax environment and their ability to accurately forecast demand is intuitive: a lack of resources or experience could potentially explain both.

Second, inattentive hosts are unable to evade their remittance obligations because they are unaware that such obligations exist. In contrast, attentive hosts are further characterized along an additional dimension of heterogeneity: honesty. An attentive host who is honest ($\gamma_i = 0$) will remit

---

[3] Though not included here for concision, I also consider a model of host price-setting under monopolistic competition, where demand, $q_i(p_i, p_{-i})$, is declining in the host's own price $\left(\frac{\partial q_i}{\partial p_i} < 0\right)$ and increasing in the prices set by all other hosts $\left(\frac{\partial q_i}{\partial p_{-i}} > 0\right)$. Assuming an equilibrium that is symmetric Nash in prices, the main results survive in sign, though the strategic price-setting attenuates their magnitude. My results also hold for a simpler model that incorporates limited strategic price-setting, in which a host's demand is partially dependent on the average price, over which an individual host has negligible influence. The results of this model fall between those of monopoly and monopolistic competition, however they impart limited additional intuition while greatly complicating exposition.

[4] For simplicity, this model assumes a two-sided market (omitting the platform firm as a potential third party to the transaction). In reality, Airbnb assumes the obligation of remitting on behalf of guests.

the tax in full, while an attentive host who is dishonest ($\gamma_i = 1$) will not.[5] Notice that this distinction is irrelevant for hosts who are inattentive to the policy ($a_i = 0$). As a result, hosts can fail to comply with the tax in two ways: either through conscious non-compliance ("evasion"), or by being unwitting non-compliers. However, neither γ_i nor a_i determines compliance with the tax when guests are obligated to remit ($\theta = 1$).[6]

2.2 *Host's Decision*

In effect, the model above describes three types of hosts: (1) attentive and honest, (2) attentive and dishonest, and (3) inattentive. All hosts choose an optimal price determined by a vector of host-specific and general parameters: $p_i^* = p_i^*(c_i, t, a_i, \theta, \gamma_i, \varepsilon_i)$. Actual profit is then determined by the fixed and variable costs, the parameters governing the tax policy, the host's attentiveness and honesty, and the quantity demanded given the true after-tax price faced by guests:

$$\pi_i^* = [p_i^* - ta_i(1-\theta)(1-\gamma_i) - c_i]q_i(p_i^* + t\theta) - F \tag{3}$$

Prior to the policy change, profit is weakly increasing in the level of honesty: inattentive hosts cannot consciously evade, while attentive hosts earn rents from being dishonest ($\frac{d\pi_i^*}{d\gamma_i} > 0$ if $a_i = 1$).[7] However, the relationship between profit and attentiveness is less straightforward. Profits are increasing in attentiveness for hosts who are dishonest, but may be decreasing for compliant hosts. Recall that inattentiveness is positively correlated with a host's tendency to misestimate the demand curve she faces. If the degree to which an inattentive host misperceives her demand is small relative to the cost of complying with her tax obligations once made aware of them, then being more attentive can actually lower profit.

2.3 *The Policy*

In response to a shift in remittance obligation from hosts to guests ($\theta \to 1$), the change in a host's optimal price will differ by her attentiveness and honesty.

Proposition 1: If a host is attentive and honest (previously complied with the tax), then shifting the remittance obligation from hosts to guests has no effect on the final prices faced by guests.[8]

---

[5] The purpose of this model is to generate comparative statics regarding price-setting and entry behavior by hosts in response to a change in remittance policy, rather than to predict changes in compliance behavior. As a result, evasion is modeled as a byproduct of hosts' exogenously determined honesty, rather than an endogenous choice, and no cost to evading is included in the model (nor does it appear, from anecdotal evidence, that many hosts who failed to comply were detected and punished).

[6] It is assumed that when guests are obligated to remit, they are unable to evade their obligation because Airbnb performs it on their behalf.

[7] Formally: $\frac{d\pi_i^*}{d\gamma_i} = \left(\frac{dp_i^*}{d\gamma_i} + a_i t(1-\theta)\right) q_i(p_i^* + t\theta) + (p_i^* - ta_i(1-\theta)(1-\gamma_i) - c_i)\frac{\partial q_i^*}{\partial p_i^*}\frac{dp_i^*}{d\gamma_i}$, where $-a_i t < \frac{dp_i^*}{d\gamma_i} < 0$ as monopolists shift only part of the perceived tax savings in evading to consumers.

[8] This proposition depends on the market structure assumptions that I make in section 2.1: namely, that hosts are monopolists whose demand curves are independent of prices set by other hosts.

A host who was attentive and honest ($a_i = 1, \gamma_i = 0$) prior to the policy change will lower her price by the full amount of the tax, $\frac{dp_i^*}{d\theta} = -t$, after the policy change goes into effect, thereby ensuring that the price faced by guests remains unchanged, $\frac{dp_i^c}{d\theta} = 0$. This is the classic result that incidence is independent of which side of the market remits a tax.

Proposition 2: If a host is attentive and dishonest or inattentive (previously non-compliant with the tax), then shifting the remittance obligation from hosts to guests increases the final prices faced by guests.

A host who was attentive and dishonest ($a_i = 1, \gamma_i = 1$) prior to the policy change will decrease her price by less than the full amount of the tax, $\frac{dp_i^*}{d\theta} > -t$, after the policy change goes into effect, thereby raising the price faced by guests, $\frac{dp_i^c}{d\theta} > 0$. Because this host was fully non-compliant with the tax previously, the policy acts as an effective tax increase by shifting the remittance obligation to a side of the market that is unable to evade it. As an effective tax increase, the host will respond by lowering her price by an amount reflective of guests' demand elasticity.

Finally, if a host was fully non-compliant with the tax previously due to being inattentive ($a_i = 0$), then she will be unaware of the policy change and therefore not adjust her price, $\frac{dp_i^*}{d\theta} = 0$. If a host does not adjust her price downward, then the policy, which mechanically adds the amount of the tax at check-out for guests, will increase guest prices by the full amount of the tax, $\frac{dp_i^c}{d\theta} = t$. Note that this is not equivalent to the incidence of the tax being fully borne by guests; failing to adjust her price downward will result in lost profits for the host.

Proposition 3. Marginal hosts who are inattentive or dishonest will exit the market.

Attentive and honest hosts ($a_i = 1, \gamma_i = 0$) in the market prior to the policy change will remain in the market after the policy change: after adjusting their prices to reflect the change in remittance obligation, their profits will remain unchanged (see Proposition 1). On the other hand, attentive and dishonest hosts ($a_i = 1, \gamma_i = 1$) and inattentive hosts ($a_i = 0$), who both previously did not comply with the tax, will see their profit decrease from what is an effective tax increase. Attentive and dishonest hosts, being aware of the policy change, will reduce prices accordingly (see Proposition 2); in doing so, they will no longer collect evasion "rents,"[9] and their profit will decrease: $\frac{d\pi_i^*}{d\theta} = \frac{dp_i^*}{d\theta} q_i(p_i^* + t\theta) + (p_i^* - c_i)\frac{dq}{dp}\left(\frac{\partial p_i^*}{\partial\theta} + t\right) < 0$, because $-t < \frac{dp_i^*}{d\theta} < 0$ and $\frac{dq}{dp} < 0$.[10] Inattentive hosts are, by definition, unaware that a policy change occurred and as a result do not adjust their prices, implicitly passing the tax through fully to guests who reduce the quantity they demand and, ultimately, hosts' profit: $\frac{d\pi_i^*}{d\theta} = t(p_i^* - c_i)\frac{dq}{dp} < 0$, because $\frac{dp_i^*}{d\theta} = 0$. From among these

[9] Evasion "rents" refer to the surplus captured by hosts from knowingly failing to remit their tax obligations. In this context, only attentive hosts can enjoy evasion rents. For examples of this terminology, see KMMS and X.

[10] Formally: $\frac{d\pi_i^*}{d\theta} = \left(\frac{dp_i^*}{d\theta} + ta_i(1-\gamma_i)\right) q_i(p_i^* + t\theta) + (p_i^* - c_i - ta_i(1-\theta)(1-\gamma_i))\frac{dq}{dp}\left(\frac{\partial p_i^*}{\partial\theta} + t\right)$.

hosts, some will have been only marginally profitable, and the policy change will cause their profits to decrease, forcing some to exit.

3 Background

This section provides three types of background information relevant for subsequent analysis. First, we describe 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 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.[11] 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 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.[12]

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 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

---

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

[12] 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.

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 1.1).[13] After a guest clicks on a listing, she is shown a more detailed accounting of the rental cost, including Airbnb's service fee and any occupancy tax. Figure 1.2 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.

4. Data

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

4.1 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.[14] Our data collection focused on cities with large tourisms sectors and cities who 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 1.1 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,[15] 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[16] for all cities in the study between Dec. 2014 and Aug. 2016.

---

[13] 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).

[14] 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.

[15] 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.

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

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, I distinguish between 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.

4.2 *Descriptive Statistics*

Table 1.2 contains descriptive statistics for treatment and control cities. Col. 1 provides the number of unique listings in the entire metro area (both treatment and control), while Col. 2 contains the number of listings located within the municipal boundary. Col. 4-6 provide means of relevant variables for each metro.

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 1.4, which shows the spatial distribution of listings in Washington, Chicago, Oakland and Los Angeles.

Figure 1.3 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 twenty 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 1.5 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.

5. VCA Effect on Prices and Tax Collections

This section provides evidence on the first prediction of the model: in the absence of evasion, which side of the market is tasked with remitting the tax is irrelevant. To generate this in the context of cities adopting Airbnb VCAs, I answer two specific questions. First, what is the effect of shifting the hotel tax remittance duty from individual hosts to the platform firm itself on tax-inclusive prices? Second, does this policy affect revenue collection in a manner that is consistent with pre-policy evasion? I find that the policy increases both after-tax prices and the city's collections of hotel tax revenue.

5.1 *What is the Effect of Shifting the Remittance Responsibility to the Firm on Consumer Prices?*

To identify the effect on consumer prices of VCA adoption, I employ two complementary estimation techniques that rely on separate identifying assumptions. First, I exploit variation in when and where—both across and within metropolitan areas—VCAs were adopted to estimate a triple difference specification, as well as its event study analogue. I further refine both of these specifications by allowing the magnitude of treatment to scale with the local hotel tax rate.

Next, I leverage the substantial number of listings in our sample that are proximate to a municipal political border to implement a geographic regression discontinuity (RD) design. The validity of this design relies on the fact that, to consumers, listings within close proximity to a political border may otherwise be seen as close substitutes, yet those in an implementing jurisdiction may have sharply different tax liabilities and remittance obligations. To guard against a failure of the assumptions necessary for a geographic RD design to be valid, we also implement a difference-in-discontinuities, or "diff-in-disc," design.

5.1.1 Triple Difference and Event Study Estimates

Using data at the listing-date level, we estimate the following OLS equation on after-tax prices:

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

where *i* is an individual listing in metro area *m* at time t. Each metro area fully contains the boundaries of a city, and each listing is located either within that city ($C_i = 1$) or outside of it but still within the metro area ($C_i = 0$). The coefficient of interest, π, captures differences in the outcome for listings that meet three conditions: they are (1) located within a metro area that contains a city that adopts a VCA ("treated metro") ($M_i = 1$), (2) located within a city ($C_i = 1$), and (3) observed after the VCA adopted by the city within the treated metro goes into effect ($POST_{mt} = 1$). Controls include fixed effects that absorb any time-invariant listing characteristics (γ_i), national time trends (γ_t), metro-specific time trends (γ_{mt}), and trends common to listings inside or outside of cities (γ_{ct}).

Conceptually, this specification compares listings within the cities of treated metros to listings outside of those cities in the same metros, and to listings in metros that are not treated, as well as to themselves before treatment starts. Because the magnitude of the policy's effect can be expected to vary in direct proportion to the prevailing hotel tax rate (τ_m), the intensity of treatment is allowed to vary with its dose.

We report estimates for this specification (4) on the log tax-inclusive price paid by consumers in Table 1.3 (Col 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 (Col 1), and on reservations (Col 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.

Figure 1.6 visually displays the coefficients[17] of the analogue event study for Washington D.C., Chicago and San Diego. Notably, the difference in the control and treatment listings is negligible in the weeks preceding the policy, satisfying the parallel trends assumption.

5.1.2 Regression Discontinuity and Difference-in-Discontinuities

Using data at the individual listing level for all listings within two miles of a municipal border of a treated city, we first estimate the following parametric RD specification using OLS:

$$y_i = \beta X_i + \pi_1 R_i + J_i(\pi_2 + \pi_3 R_i) + \varepsilon_i \tag{5}$$

where *i* is an individual listing located R_i miles inside the municipal border, $J_i = 1[R_i \geq 0]$ is an indicator for listings within the municipal border, and X_i is a vector of time-invariant listing characteristics. The coefficient of interest, π_2, captures differences in the outcome for listings just inside the municipal border relative to those just outside of it.

Validity in this context requires that any observed and unobserved listing characteristics must vary smoothly across the cutoff, while the only factor changing sharply at the border is the tax treatment. On the one hand, Airbnb's interface encourages consumers to treat geographic search areas as contiguous, showing results without being constrained to municipal boundaries. This would suggest that, from the consumer's perspective, two listings close to but on opposite sides of a municipal border are equally attractive, and the setting is an appropriate one for an RD. On the other hand, local amenities and property taxes can differ sharply across neighboring municipalities, and these differences could manifest themselves in the quality of the housing stock—and the critical identification assumption fails.[18]

To address this concern, I take advantage of the fact that enactment of a VCA might impact the magnitude of any discontinuity that predated it. Following the approach of Grembi, Tommaso,

[17] The estimating equation is:

$$y_{imct} = \gamma_i + \gamma_t + \gamma_{mt} + \gamma_{ct} + \sum_{j>-A}^{B-1} \delta^j D_{mt}^j M_{im} C_{ic} \tau_m + \delta^B D_{mt}^B M_{im} C_{ic} \tau_m + \varepsilon_{imct}$$

where D_{mt}^j is an indicator for metro *m* in time period *t* being *j* periods away from the implementation of the policy.

[18] To empirically test whether the RD assumptions are met, I present in Table 1.4 estimates of (5) separately for a time period before and after policy enactment within select metropolitan areas containing treated cities. The results in columns 2 and 5 strongly suggest that a price discontinuity exists after the enactment of VCAs in these cities. However, in several cases—San Diego, Washington, D.C., and Phoenix—a discontinuity also exists *before* any VCA is enacted.

and Troiano (2012), we modify the RD estimating equation in (5) to measure the change in after-tax prices on either side of the border, before and after VCA enactment:

$$y_i = \beta X_i + \pi_1 R_i + J_i(\pi_2 + \pi_3 R_i) + T_t\big(\rho_1 R_i + J_i(\rho_2 + \rho_3 R_i)\big) + \varepsilon_i \tag{6}$$

This "diff-in-disc" specification shares much in common with (5), with the addition of an indicator for the post-treatment period, T_t. The coefficient of interest, ρ_2, captures any *additional* difference in the outcome for listings just inside a municipal border ($J_i = 1$) in the post-treatment period ($T_t = 1$). Estimates, reported in col. 3 and 4 of Table 1.4, confirm the existence of a price discontinuity.[19] For most cities, this discontinuity is largest when measured one month immediately before and after the policy, and diminishes when that window is broadened to two months before and after, suggesting that the competitive pressure from cross-border listings may (slowly) cause hosts inside the city to lower their prices.

Like the RD specification, the diff-in-disc specification provides a causal estimate of the effect of VCA enactment on outcomes for listings close to a municipal border. This estimate may or may not differ from that recovered by the DDD specification. For example, if the proximity of similar listings that are not (directly) affected by the VCA across a municipal border introduces additional competition that limits the degree of pass-through, then the effect of VCA enactment might vary with distance to the border. In that case, the diff-in-disc estimates will differ from the DDD estimates, which reflect the average price increase across all listings within the city. Our estimates weakly support this intuition; for most metros, the diff-in-disc estimates are lower than the triple difference estimates (col. 0, repeated from Table 1.3, col. 3).

Figure 1.9 provides visual evidence of discontinuities in after-tax prices after—and in some cases before—enactment of VCAs in Washington D.C., Chicago, Oakland and Los Angeles. The points plotted here are average residuals of after-tax price after controlling for listing characteristics and a linear time trend, for listings at different distances from the border. The left panel plots listings' average residuals for the thirty days *before* the policy, overlaid with lines of best fit estimated separately on either side of the boundary. The right panel plots the same figure for the thirty days *after* the policy. Relative to any pre-policy discontinuity that existed, the gap in after-tax prices between listings on either side of the border increases with the adoption of the policy in all four cities.

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. Our estimates 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

[19] The ten treatment jurisdictions shown are selected from the larger sample of treated cities because they have a sufficient mass of observations within a bandwidth around the municipal border. Each city's border is represented as a polygon in GIS software and divided into 0.1-mile segments, with each pair of segments connecting at a vertex. This creates between 400 and 800 vertices per city, and each listing's distance is calculated relative to its nearest vertex. Identifiers for vertices are included as fixed effects in (5), ensuring that listings on opposite sides of the same border segment are compared with one another. The regressions are estimated for both 30 and 60 days before and after policy implementation.

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.

5.2 What Effect did the Policy Have on the Municipal Hotel Market 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 re-assigning 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,[20] we estimate the following difference-in-differences specification:

$$y_{mt} = \gamma_m + \gamma_t + \pi Treat_m POST_{mt} + \varepsilon_{mt} \tag{7}$$

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 1.6 reports results from equation (7) 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 1.3 percent; however, none of these estimates are statistically distinguishable from zero.

Table 1.6 also reports results from equation (7) 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.

[20] 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).

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.

6. Heterogeneity in Pass-Through by Attention Correlates

In this section, I explore 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, I interpret this as heterogeneity in pass-through of an effective tax increase. We find 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.

6.1 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 changing (i.e., lowering) their listing price because they anticipate that consumers will be less willing to book at higher prices.[21]

I 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:

$$y_{imct} = \gamma_i + \gamma_t + \gamma_{mt} + \gamma_{ct} + \pi M_i C_i POST_{mt} \tau_m X_i + \varepsilon_{imct} \tag{8}$$

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 1.5 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.

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

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 1.8 (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 1.8 (bottom panel) plots event study coefficients estimated separately for hosts at different deciles of the host-hotel price correlation distribution; 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.

7. 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.

7.1 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,we find 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 (4) 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.[22] Table 1.7 reports the results. On average, the policy increased the likelihood of exit by one third of one percentage point (Col 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.

7.2 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.[23] 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.

I 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:

$$y_{mt} = \gamma_m + \gamma_t + \pi POST_{mt} + \varepsilon_{mt} \tag{9}$$

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.[24]

8. Results Summary and Discussion

In this section, we consider the relevance of our findings to broader academic and policy discussions on the role that statutory features play in tax compliance, the long-term collection

[22] 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.

[24] Alternative DDD specification:

$$y_{pmt} = \gamma_p + \gamma_t + \gamma_m + \gamma_{pm} + \gamma_p POST_{mt} + \gamma_m POST_{mt} + \pi M_m P_p POST_{mt} + \varepsilon_{mpt}$$

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 differences and discontinuity-in-differences 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 KKMS, 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, 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 (VCAs) 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

City	DDD*	30 Day Window			60 Day Window		
		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

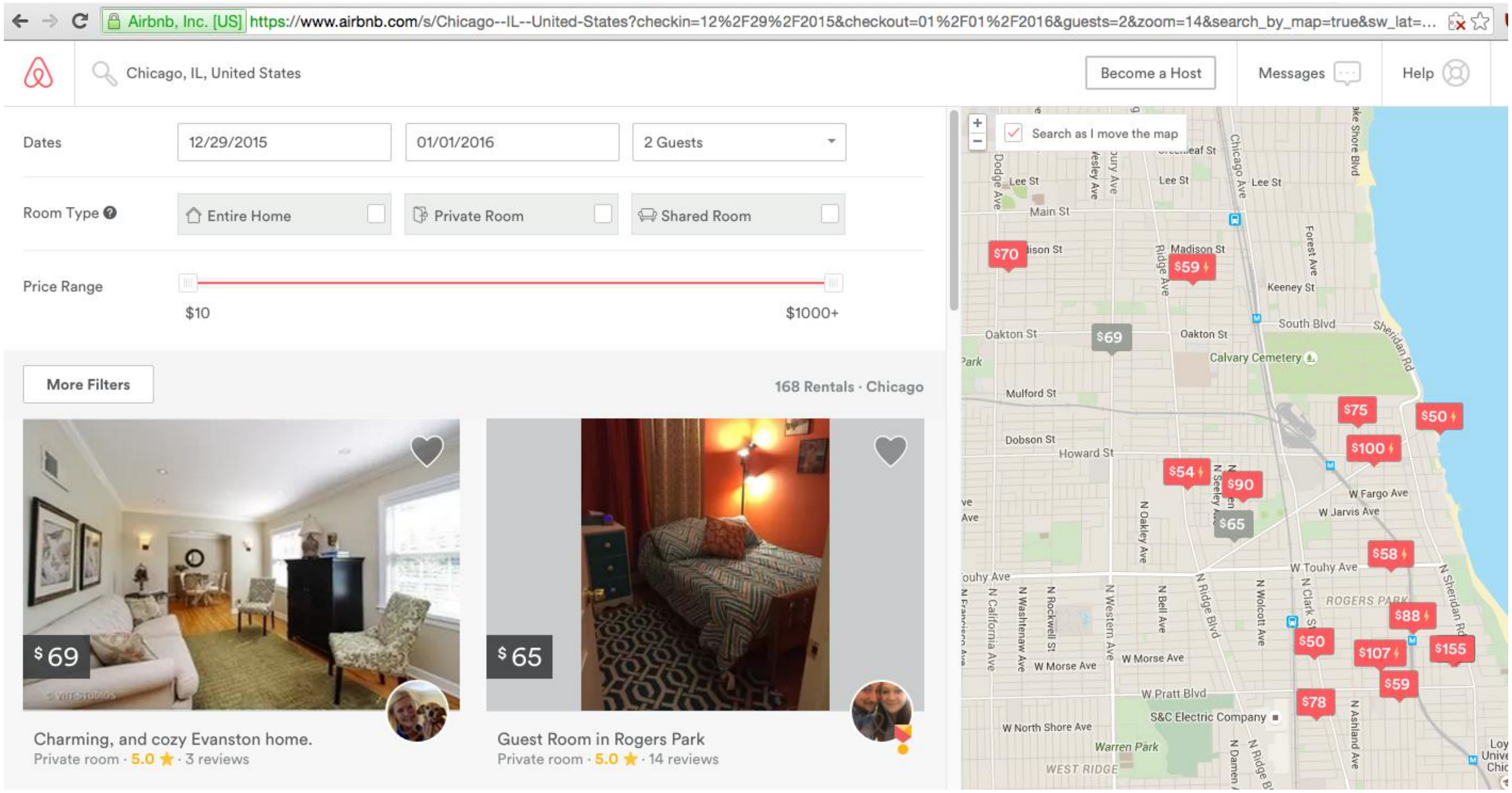
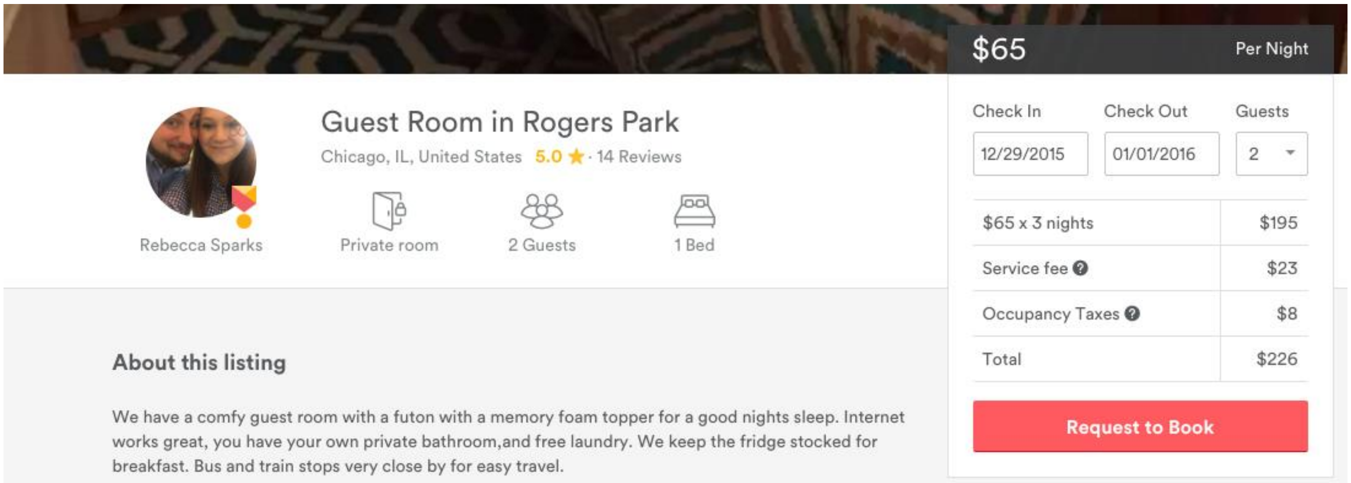
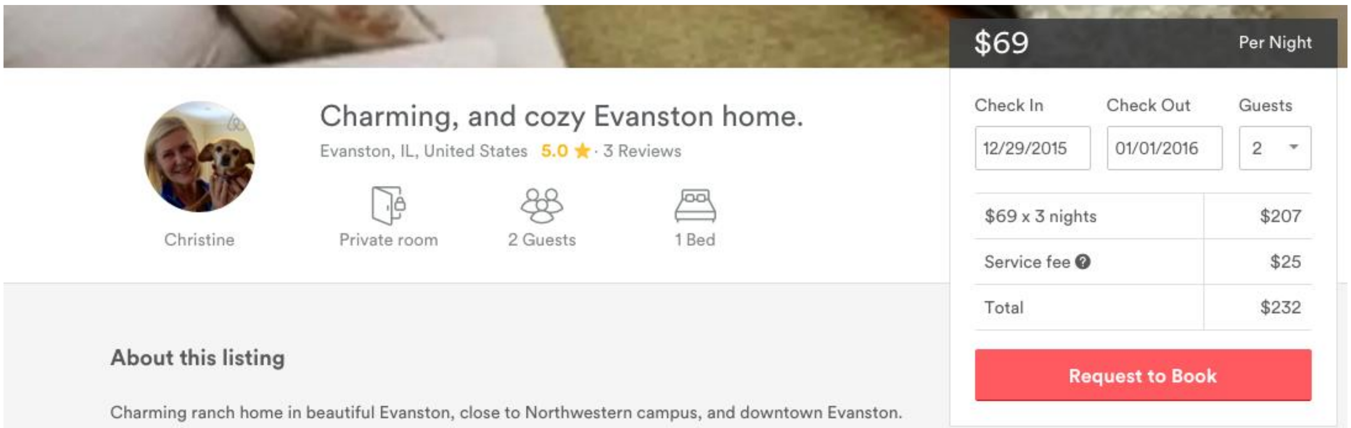


Figure 1.2: Airbnb Listing Details in Chicago and Evanston, IL



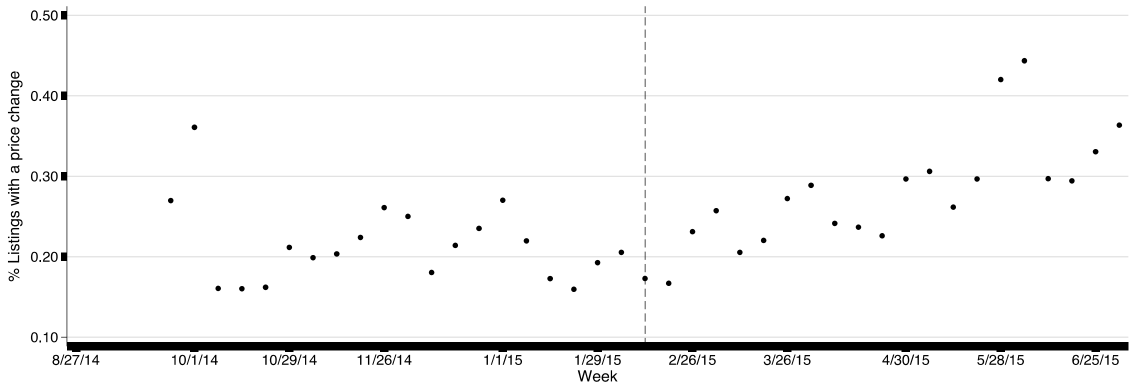
Chicago, IL



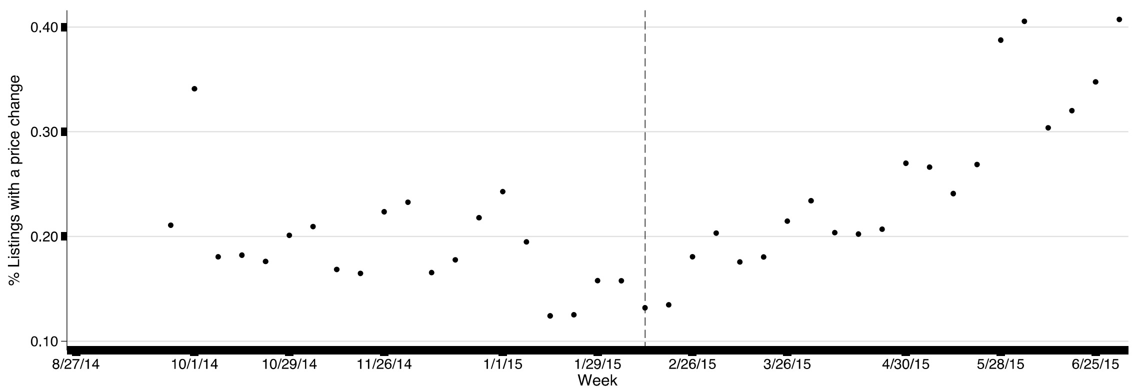
Evanston, IL

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

Washington DC



Chicago, IL



San Diego, CA

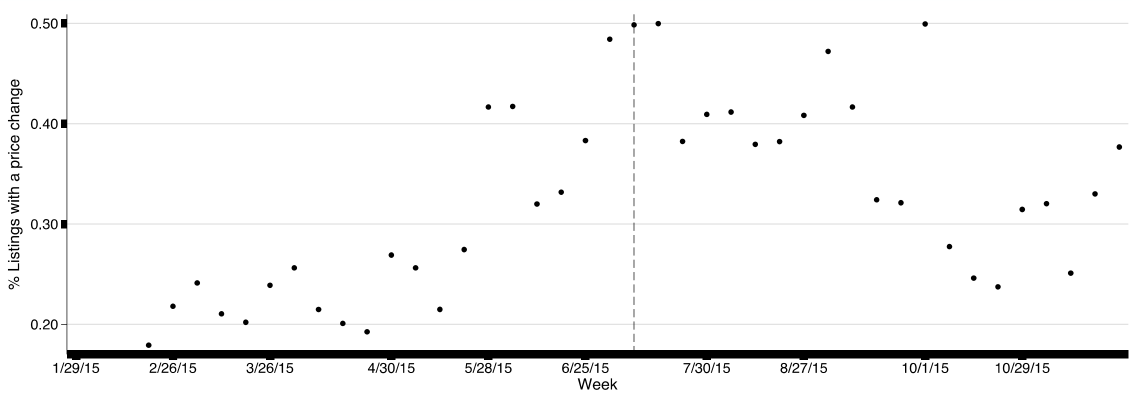


Figure 1.4. Spatial Distribution of Listings, Relative to City Boundaries (D.C.)
Note: Red dots represent Airbnb listings, Orange dots represent VRBO listings)

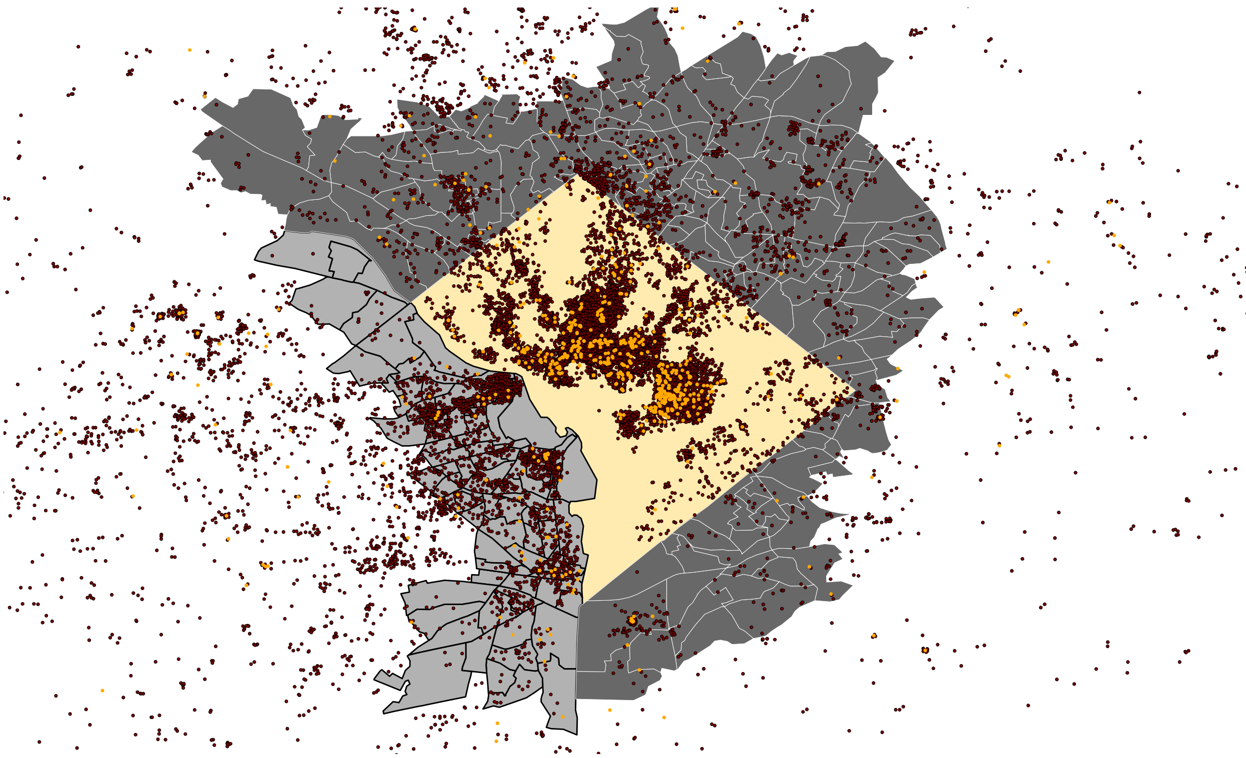
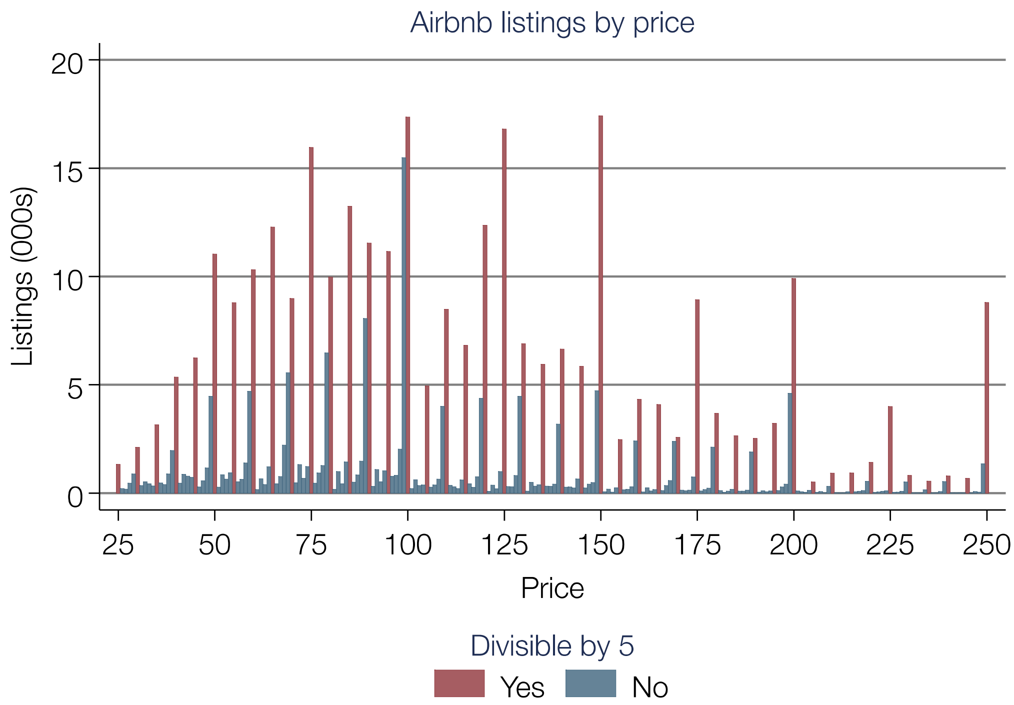


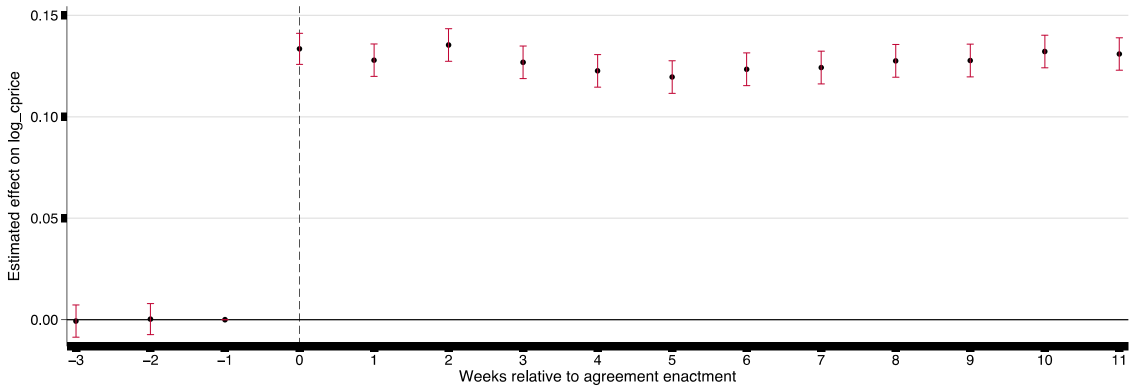
Figure 1.5: Histogram of Listing Prices



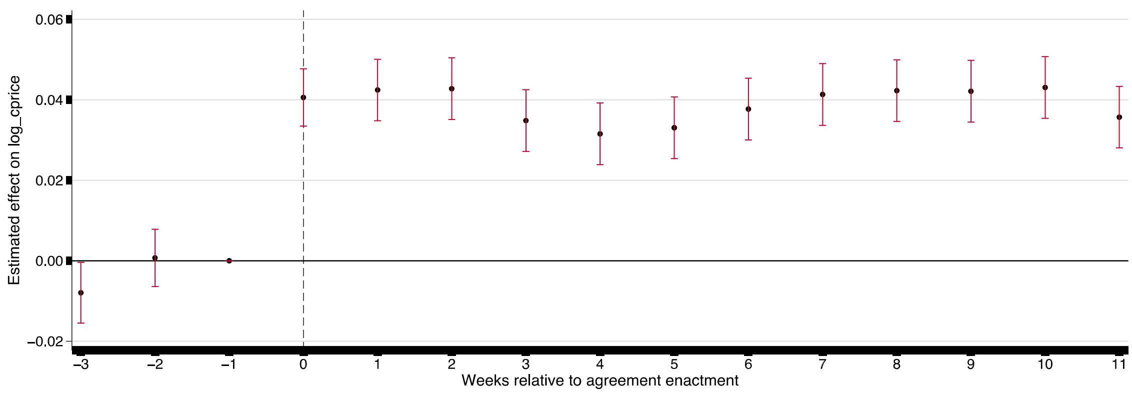
Notes: Figure displays the frequency of listings by price, for all observed listings priced under $250.

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

Washington DC



Chicago, IL



San Diego, CA

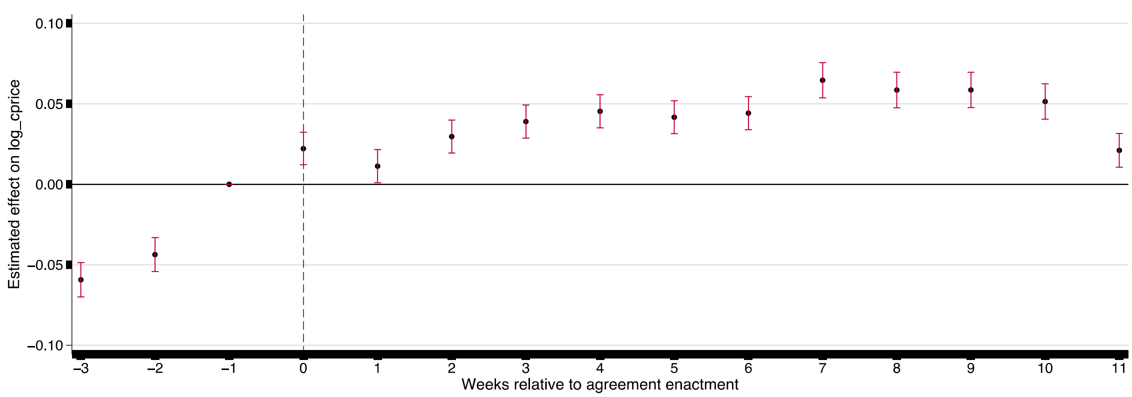
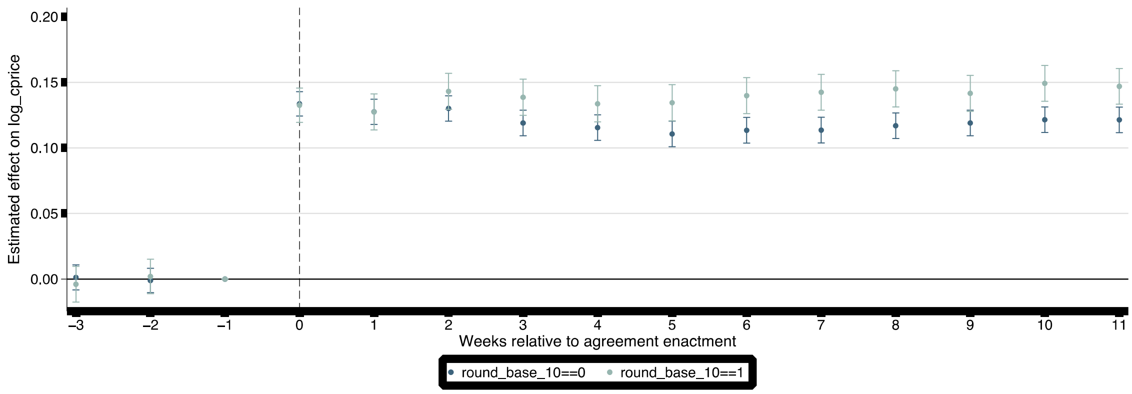
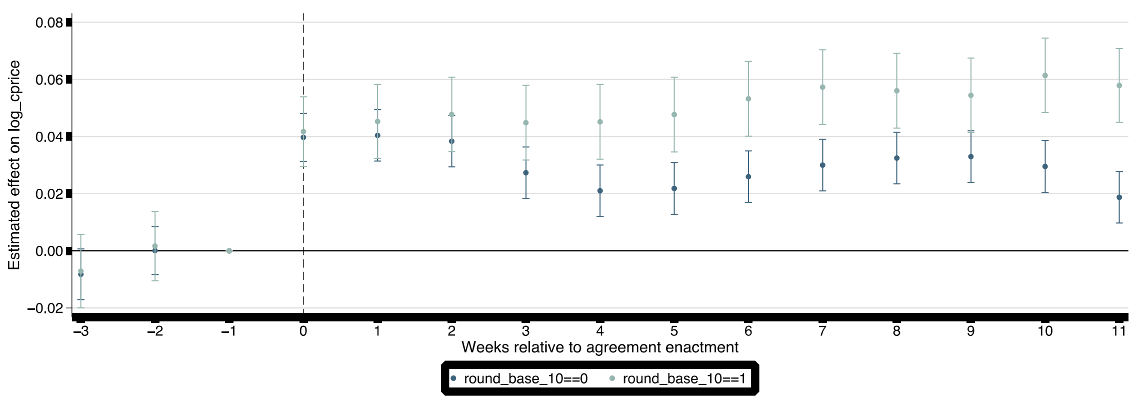


Figure 1.7. Event Study Estimates for Listings with Round Base (Divisible by 10)

Washington, D.C.



Chicago IL



San Diego, CA

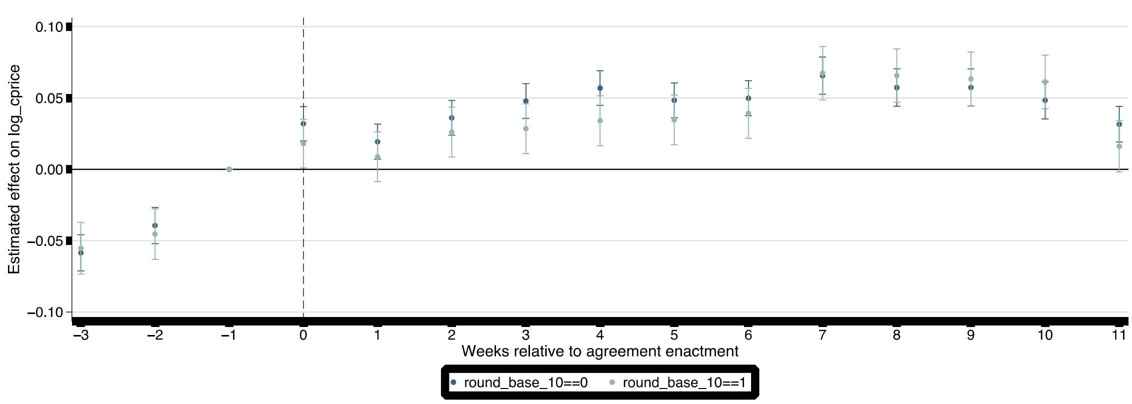


Figure 1.8: ES on Log Price by Pre-policy Correlation with Hotel Price

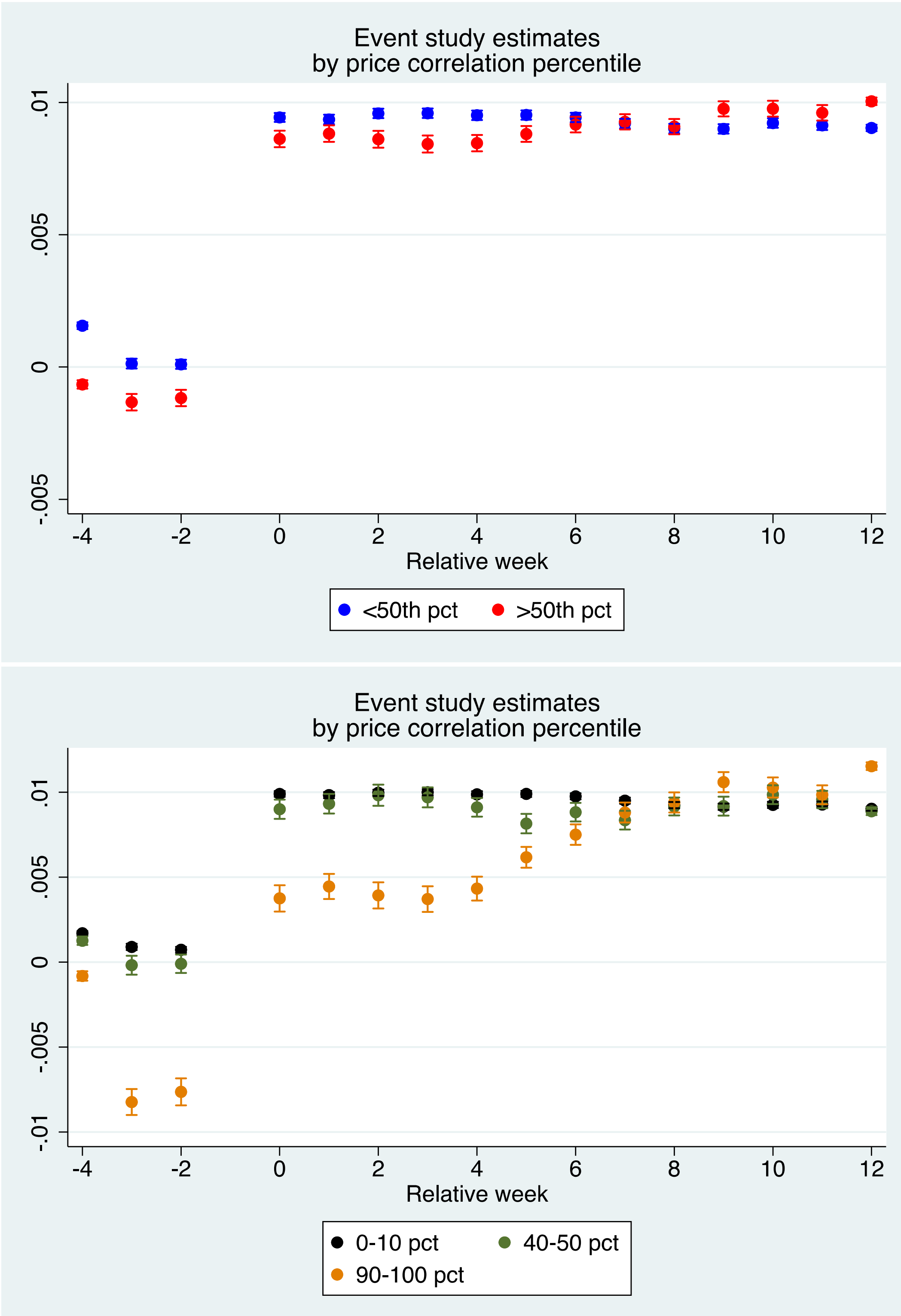
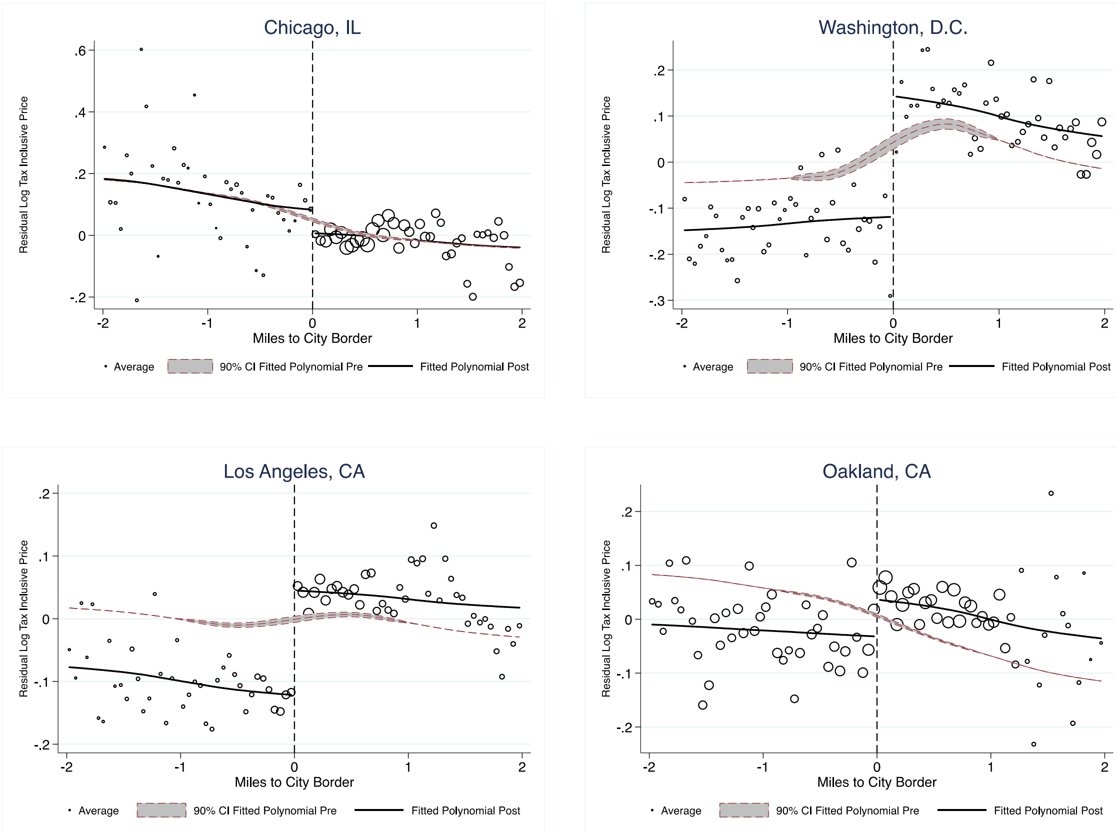


Figure 1.9: Difference in Discontinuity Residuals



References

Agostini, Claudio A. and Claudia Martinez A. (2012), "Tax Credits Response to Tax Enforcement: Evidence from a Quasi-Experiment in Chile," working paper, Universidad Adolfo Ibanez and Universidad da Chile.

Alm, James, Edward Sennoga, and Mark Skidmore. (2009) "Perfect competition, urbanicity, and tax incidence in the retail gasoline market," Economic Inquiry 47:1, p.118-134.

Balke, Nathan S. and Grant W. Gardner. (1991) "Tax Collection Costs and the Size of Government," working paper, Southern Methodist University.

Barnett, Paul G., Theodore E. Keeler, and Teh-wei Hu. (1995) "Oligopoly structure and the incidence of cigarette excise taxes," Journal of Public Economics 57:3, p. 457-470.

Besley, T. (1989) "Commodity taxation and imperfect competition: A note on the effects of entry," Journal of Public Economics 40, p. 359-367.

Besley, Timothy J. and Harvey S. Rosen. (1999) "Sales taxes and prices: an empirical analysis," National Tax Journal 52, 157-178.

Chernick H. and A. Reschovsky. (1997) "Who pays the gasoline tax?" National Tax Journal 50, 157-178.

Chetty, Raj. 2009. "Is the Taxable Income Elasticity Sufficient to Calculate Deadweight Loss? The Implications of Evasion and Avoidance." *American Economic Journal: Economic Policy*, 1(2): 31–52.

Chouinard, Hayley and Jeffrey M. Perloff. (2004) "Incidence of federal and state gasoline taxes," Economics Letters 83, p. 55-60.

De Paula, Aureo and Jose A. Scheinkman. (2010) "Value-Added Taxes, Chain Effects, and Informality," American Economic Journal: Macroeconomics 2:4, p. 195-221.

Delipalla S, O'Donnell O. (2001) "Estimating tax incidence, market power and market conduct: The European cigarette industry," International Journal of Industrial Organisation 19, p. 885-908.

Doyle, Joe J. and Krislert Samphantharak. (2008) "$2.00 Gas! Studying the effects of a gas tax moratorium," Journal of Public Economics 92, p. 869-884.

Finkelstein, Amy. 2009. "EZ-Tax: Tax Salience and Tax Rates." *Quarterly Journal of Economics*, 124(3): 969-1010.

Fullerton, Don and Gilbert E. Metcalf, "Tax Incidence," in Alan J. Auerbach and Martin S. Feldstein, eds., Handbook of Public Economics, volume 4, New York: North-Holland, 2002, p. 1787-1872.

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano. "Policy responses to fiscal restraints: A difference-in-discontinuities design." (2012).

Harju, Jarkko, Tuomas Kosonen, and Oskar Nordström Skans. 2015. "Strategists, generalists and firm-level price setting: Evidence from consumption tax reforms." Unpublished manuscript. http://www.sv.uio.no/econ/english/research/news-and-events/events/guest-lectures-seminars/ofs-seminar/2015/swefin_reforms_05032015.pdf

Kau, James B. and Paul H. Rubin. (1981) "The Size of Government," Public Choice 37:2, p. 261-274.

Kopczuk, Wojciech. (2005) "Tax bases, tax rates and the elasticity of reported income," Journal of Public Economics 89, p. 2093-2119.

Kopczuk, Wojciech, Justin Marion, Erich Muehlegger, and Joel Slemrod. Forthcoming. "Does Tax-Collection Invariance Hold? Evasion and the Pass-through of State Diesel Taxes." *American Economic Journal: Economic Policy*.

Myles, Gareth D. 1989. "Ramsey tax rules for economies with imperfect competition." *Journal of Public Economics*, 38(1): 95-115.

Pomeranz, Dina. (2011) "No Taxation without Information: Deterrence and Self- Enforcement in the Value Added Tax," working paper.

Poterba, James M. (1996) "Retail Price Reactions to Changes in State and Local Sales Taxes," National Tax Journal 49:2, p. 165-76.

Rothstein, Jesse. (2010) "Is the EITC as Good as an NIT? Conditional Cash Transfers and Tax Incidence," American Economic Journal: Economic Policy 2:1, p. 177-208.

Saez, Emmanuel, Manos Matsaganis, Panos Tsakloglou. (2012) "Earnings Determination and Taxes: Evidence from a Cohort-Based Payroll Tax Reform in Greece," The Quarterly Journal of Economics 127, p. 493-533.

Sidhu, Nancy D. (1971) "The Effects of Changes in Sales Tax Rates on Retail Prices," Proceedings of the Sixty-Fourth Annual Conference on Taxation p. 720-33.

Slemrod, Joel. (2001) "A General Model of the Behavioral Response to Taxation," Inter- national Tax and Public Finance 8:2, p. 119-128.

Slemrod, Joel. (2008) "Does It Matter Who Writes the Check to the Government? The Economics of Tax Remittance," National Tax Journal 61:2, p.251-275.

Weyl, E. Glen, and Michal Fabinger. 2013. "Pass-through as an economic tool: Principles of incidence under imperfect competition." *Journal of Political Economy*, 121(3): 528-583.