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Inferring Tax Compliance from Pass-through: Evidence from Airbnb Tax Enforcement Agreements

Abstract

Tax enforcement can be prohibitively costly when market transactions and participants are difficult to observe. Evasion among market participants may reduce tax revenue and provide certain types of suppliers an undue competitive advantage. Whether efforts to fully enforce taxes are worthwhile depends on the rate of compliance in the absence of such efforts. In this paper, we show that an upper bound on pre-enforcement tax compliance can be obtained using market data on pre- and post-enforcement periods. To do this, we estimate the pass-through of tax enforcement agreements between Airbnb and state and local governments, which achieve full compliance at the point of sale. Using data on Airbnb listings across a number of U.S. metropolitan areas, as well as variation in enforcement agreements across time, location, and tax rate, we estimate that taxes are paid on no more than 24 percent of Airbnb transactions prior to enforcement. We also find that demand is inelastic, which drives several key insights: the economic burden of taxation disproportionately falls on renters, excess burden is very small, and tax enforcement is not an effective policy lever for interest groups seeking to reduce local Airbnb activity.

JEL-Codes: H200, H220, H260, L100.

Keywords: evasion, short-term housing rentals, sharing economy, voluntary collection agreements, online sales and use taxes.

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1 Introduction

Online marketplaces such as Amazon, eBay, Craigslist, and Airbnb have transformed industries by increasing competition and reducing transaction costs. However, the rapid rise of online marketplaces created contexts in which tax obligations were ambiguous or difficult to enforce. Before the recent June 2018 U.S. Supreme Court decision in South Dakota v. Wayfair Inc., states were unable to compel online sellers without sufficient local presence (i.e. *nexus*) to collect taxes on sales made to residents of that state.[1] Because state and local government agents cannot fully observe key details of online transactions, enforcing the applicable taxes was challenging.[2] In many cases, jurisdictions simply relied on residents to self-report the taxes they owed on economic activity conducted online. Naturally, this enabled individual market participants, some of whom may simply have been unaware of their tax obligations, to evade with low probability of detection.[3]

In some cases, such as the "Amazon tax," policymakers were able to establish nexus and compel online companies to collect and remit sales and use taxes.[4] In other cases where policymakers could not establish nexus, they worked to shift the burden of tax collection and remittance onto online platforms and retailers through the use of voluntary collection agreements. Such efforts can increase tax revenue if these online companies are less able, or less willing, to evade.[5] Traditional suppliers also face incentives to promote enforcement to mitigate competitive advantages enjoyed by online suppliers.[6] Finally, platforms have the

[1] The decision in Quill Corp. v. North Dakota, 504 U.S. 298 (1992), established the nexus requirement. The decision in South Dakota v. Wayfair Inc., 585 U.S. ___ (2018), overturned the earlier ruling.

[2] See Agrawal and Fox (2017) for a survey of e-commerce tax enforcement issues and policy proposals.

[3] According to Manzi (2015), in the 27 states that enable individuals to report use taxes on their income tax return, between 0.2 and 10.2% of income tax returns reported any use tax in 2012. Bruce et al. (2009) conservatively projected that foregone e-commerce state tax revenue would be $11.4 billion in 2012 alone.

[4] Many states passed laws, collectively referred to as the "Amazon tax," enabling them to cite the presence of facilities such as fulfillment centers to establish nexus (Baugh et al., 2018).

[5] While the conventional principle of tax-collection invariance states that economic tax incidence and tax revenues do not depend on who bears the statutory tax burden, Kopczuk et al. (2016) demonstrate that this principle is violated when one or more sides of the market differ in their ability to evade.

[6] See, for example, lobbying groups such as the American Hotel and Lodging Association (Benner, 2017), Alliance for Main Street Fairness (`www.standwithmainstreet.com/content.aspx?page=efairness`), and Retail Industry Leaders Association (`www.rila.org/Public-Policy/Fairness/E-Fairness/Pages/default.aspx`).

incentive to cooperate to avoid facing restrictive regulations or outright bans, such as the one imposed by New York on Airbnb in late 2016 (Benner, 2016). However, whether efforts to fully enforce taxes on online activity are effective or wasteful crucially depends on the rate of compliance among individuals in the absence of formal enforcement.

In this paper, we develop an approach to bound pre-enforcement tax compliance that relies only on observing tax rates and prices from pre-enforcement (partial compliance) and post-enforcement (full compliance) periods. The intuition, on which we elaborate in Section 3, is as follows. Suppose a tax enforcement agreement shifts the statutory burden away from suppliers and onto online platforms or retailers who fully enforce the tax on consumers at the point of sale. As a result, demand will fall by the amount of the tax. There is also a contemporaneous positive supply response, as the tax is no longer included in compliant suppliers' marginal costs and evaders no longer face the risk of being caught. Note that the magnitude of the supply response cannot be larger than the resulting decrease in the market-clearing price paid to suppliers.[7] Thus, the ratio of the price effect to the size of the tax yields an upper bound on the rate of pre-enforcement compliance.

This approach yields a valid upper bound on pre-enforcement compliance when suppliers are price-takers. However, while this condition is sufficient, it is not necessary. We relax the assumption that suppliers are price-takers and show that the bounding argument is also valid for imperfect competition under reasonable conditions. In addition, while we focus on enforcement that involves a change in statutory incidence, we show that our approach generalizes to contexts where a change in statutory incidence does not accompany enforcement. Such contexts include the "Amazon tax" and new laws enabled by the U.S. Supreme Court decision in South Dakota v. Wayfair Inc.

We apply our approach to Airbnb, which offers a particularly attractive setting to study. In jurisdictions with legislated taxes on hotels and other short-term housing rentals (STRs),

[7]This assumes that the law of supply holds. The magnitude of the supply response will be, at most, equal to the decrease in the market-clearing price paid to suppliers when supply is perfectly elastic. Note that the market clearing price consumers pay to producers is tax-inclusive before enforcement and tax-exclusive after enforcement.

but no formal enforcement agreements with platforms such as Airbnb, governing bodies must rely on hosts (suppliers) to collect and remit the applicable taxes and pay large enforcement costs to locate and penalize evaders.[8] Since 2014, however, Airbnb has entered into over 275 agreements with cities, counties, and states across the U.S. to enforce sales, hotel, transient, and other taxes.[9] Once an agreement is reached, Airbnb becomes the tax remitter and collects taxes on every applicable transaction from renters (consumers) at the point of sale, increasing tax compliance to 100% in those jurisdictions. Importantly, the taxes are salient as they are included in the price presented on the main property page (see Figure 1). This implies that consumers are unlikely to under-react to this shift in statutory tax burden, as cautioned by the work of Chetty et al. (2009).

Using data derived from Airbnb.com on over 170,000 properties spanning three years and 61 unique tax jurisdictions, we employ a difference-in-differences estimation strategy that exploits variation in Airbnb tax enforcement across time, location, and tax rate. First, we estimate the effect of tax enforcement on booking prices, accounting for location-specific shocks and unobserved heterogeneity across properties. We find that the enforcement of a 10% tax reduces the price paid to hosts by 2.4% and increases the total price renters pay by 7.6%. This yields an upper bound of 24% compliance pre-enforcement. That is, at least 76% of transactions evade taxation, suggesting that tax jurisdictions can increase compliance substantially by entering an enforcement agreement. This result is robust across specifications, and we rule out potential threats to the validity of our upper bound by testing for the presence of contemporaneous negative supply responses.

We use the same approach to find that the enforcement of a 10% tax reduces nights booked by 3.6%. Adapting an intuitive result, explained nicely by Zoutman et al. (2018), we use the estimated effects on price and quantity to infer price elasticity of demand and bound

[8]Indeed, anecdotal evidence suggests that, in the absence of formal enforcement, compliance among Airbnb hosts is low (Tuttle, 2013; Bruckner, 2016; Cohn, 2016).

[9]See `https://www.airbnbcitizen.com/airbnb-tax-collection-program-expands-has-already-collected-110-million-for-governments/` and `https://www.airbnbcitizen.com/airbnb-tax-facts/`

price elasticity of supply. In particular, the estimated effects of the enforced tax rate on the price paid to hosts and nights booked imply an average price elasticity of demand of -0.48. These results suggest that the negative demand shock caused by an Airbnb tax enforcement agreement dominates any contemporaneous positive supply shock. This is consistent with at least partial pre-enforcement evasion; in the absence of evasion, the equilibrium quantity should remain unchanged and the price paid to hosts should fall by exactly the amount of the tax. Furthermore, our estimates imply a lower bound on the price elasticity of supply of 1.5, suggesting that hosts are relatively price-sensitive and that renters bear a larger share of the economic tax incidence.

Using back-of-the-envelope calculations, we find that tax enforcement increases tax revenue by at least $69 per property per month. Multiplying this by the average number of properties in a treated tax jurisdiction, 2,245, yields a monthly increase in revenue of at least $155,000 from the jurisdiction. We also find that enforcement imposes a relatively small efficiency cost on the local market of $0.03 per dollar of additional revenue. This is driven by the relatively low price elasticity of demand, which suggests two additional insights. First, renters may not view hotels and other short-term rental options as close substitutes for Airbnb listings, suggesting that the introduction of Airbnb has substantially increased consumer surplus. Second, the low quantity effect suggests that taxing Airbnb is not an effective policy lever for those seeking to reduce Airbnb market activity in a given area.

This paper is closely related to the literature focused on detecting and estimating tax evasion.[10] One approach taken in the literature involves comparing reported and actual aggregate quantities to infer evasion.[11] Another method exploits the IRS Taxpayer Compliance Measurement Programs, which provide data on compliance from randomized audits (e.g., Feinstein (1991)). Others compare administrative records of taxes paid to actual tax liabilities, as in Dwenger et al. (2016) who find that 20% of taxpayers are intrinsically motivated

[10]See Slemrod (2016) for an overview of recent research on tax compliance and enforcement.

[11]For example, Pommerehne and Weck-Hannemann (1996) compare income reported on tax returns to national income accounts.

to comply with a church tax in the absence of deterrence. The approach we propose in this paper is most closely related to work that uncovers evidence of evasion by exploiting changes in enforcement activity.[12]

In particular, our work contributes to research studying compliance across different tax regimes. Slemrod (2008) and Kopczuk et al. (2016) are especially relevant, in that the authors show the textbook principle of tax-collection invariance can fail in the presence of evasion. Specifically, in Kopczuk et al. (2016), the authors find that economic tax incidence and tax revenues in the diesel fuel market depend on which part of the supply chain bears the statutory tax burden. Their results can be explained by heterogeneity in the ability to evade taxes throughout the supply chain, though due to data limitations the authors are unable to estimate the extent of evasion. Doerrenberg and Duncan (2014) use an experimental approach to show that, when one side of a market can evade taxes, the economic responses are small and the benefits incurred by evaders are shared with the side of the market that has no opportunity to evade. Our paper builds on this body of work by showing how researchers can exploit heterogeneity in evasion ability to estimate tax compliance, and also provide insight on supply and demand elasticities, tax incidence, and welfare effects.

This paper also contributes to the growing literature on the sharing economy and Airbnb in particular. In their research on the welfare effects of Airbnb entry, Farronato and Fradkin (2018) estimate a price elasticity of supply of 2.16, which is consistent with our estimated lower bound of 1.5. In fact, we show that combining their estimated supply elasticity with our estimated effect of enforcement on price implies a pre-enforcement compliance rate of 7% (see Section 5.5). Farronato and Fradkin (2018) also find that, while an increase in

[12]For example, Marion and Muehlegger (2008) study evasion by exploiting regulatory innovation in the diesel fuel market. Another example is Wilking (2016), a working paper in which the author finds that Airbnb hosts reduce asking prices in response to tax enforcement agreements, but do so by less than the full amount of the tax. This suggests that, indeed, some hosts do not comply in the absence of enforcement agreements. While the finding is consistent with our results, the paper only considers asking price responses in response to a much smaller number of tax enforcement agreements. As such, the author is only able to provide trace evidence of evasion and insight on incidence. We consider changes in supply-side responses and equilibrium outcomes using a much richer dataset. This enables us to make stronger claims on tax compliance rates and incidence, and to provide deeper insight on price elasticity of supply/demand and welfare implications.

the prevalence of Airbnb reduces hotel revenue, at least 70% of Airbnb bookings are "new" in that they would not have resulted in hotel bookings in the absence of Airbnb. This is consistent with earlier work by Zervas et al. (2017), which finds that an increase in Airbnb prevalence is associated with lower hotel prices and revenues. While these studies show that Airbnb is successfully competing with the hotel industry and increasing consumer surplus, particularly during periods of high demand when hotels are likely to be fully booked, there exist concerns that the growth in this market is making residential housing less affordable (e.g., Barron et al. (2017)).

Finally, our work contributes to the growing literature on the relationship between taxes, tax enforcement, and online shopping. One of the seminal papers in this literature, Goolsbee (2000), finds that consumers facing higher local sales taxes are more likely to make (untaxed) purchases online, and that taxing online purchases could significantly reduce the number of internet purchases. Other economists have also studied this relationship using different online shopping data and find similar results: Alm and Melnik (2005); Ballard and Lee (2007); Scanlan (2007); Ellison and Ellison (2009); Anderson et al. (2010); Einav et al. (2014); and Baugh et al. (2018).

2 Data

To motivate our conceptual framework and empirical strategy, we first describe our data on Airbnb and tax enforcement agreements. We start with information derived from Airbnb.com on STR listings including daily price, daily availability, daily bookings, date of booking, and various time-invariant property-specific characteristics such as number of bedrooms, number of bathrooms, maximum number of guests, and reported coordinates. The data come from a third-party source that frequently scrapes property, availability, host, and review information from the website.

These data cover 27 major metropolitan areas across the United States and include over

860,000 properties that were active anytime between August 2014 and September 2017.[13] The complete dataset consists of more than 4,800 unique city-county-state combinations, which we call tax jurisdictions. For several reasons, we initially restrict our sample to roughly the top 100 tax jurisdictions in terms of number of listings. First, there is considerable heterogeneity across jurisdictions; in particular, larger jurisdictions are much more likely to be treated. Second, the largest jurisdictions are the most relevant for welfare analyses given the size of the markets and the higher likelihood of entering into an Airbnb tax enforcement agreement. Finally, the larger jurisdictions are likely to be more competitive given their denser concentration of other STR listings and lodging options. This is important for when we use our estimated price and quantity effects to provide insights on supply elasticity and welfare. To this end, we also restrict our sample to listings that represent reasonably close substitutes to more traditional lodging alternatives.[14]

We then aggregate our property-day data to the property-month level and supplement them with the implementation dates and tax rates of all the tax enforcement agreements made between Airbnb and the relevant state/local governments. The enforced tax rates vary by jurisdiction. They also vary over time within jurisdiction, as some jurisdictions are affected by subsequent agreements or changes in tax rates. As such, we are able to exploit variation in the timing, magnitude (both within and between cross-sectional units), and location of tax enforcement.

To alleviate concerns about endogenous treatment, we drop treatment and control jurisdictions with potentially confounding regulatory changes and changes in jurisdictions' self-enforcement efforts during the sample period. Also, since our preferred specification includes metro-month-year fixed effects (see Section 4), we drop jurisdictions if they are part

[13]The 27 metros are Anchorage, Atlanta, Austin, Boston, Charlotte, Chicago, Cleveland, Dallas-Fort Worth, Denver, Houston, Indianapolis, Los Angeles, Louisville, Miami, Minneapolis-St. Paul, Nashville, New Orleans, New York City, Oakland, Orlando, Philadelphia, Phoenix, Salt Lake City, San Diego, San Jose, Seattle, and Washington, D.C.

[14]In particular, we drop shared room listings (3.8% of the sample), properties with more than 4 bedrooms (2.9%), listings that allow more than 12 guests (1.5%), and listings with an average asking price in the bottom or top 10 percentile of their jurisdiction.

of metros devoid of within-metro-month-year treatment variation. Our resulting estimation sample includes properties from 61 jurisdictions. Of these 61 jurisdictions, 38 are treated by a voluntary collection agreement on one of 14 unique initial treatment dates. The remaining 23 jurisdictions are never treated during the sample period. The average enforced tax rate is 7.1%. However, this rate includes many property-month observations that are not affected by a tax enforcement agreement. Conditional on being subject to any non-zero tax, this average increases to 11.2%. In Appendix C, we discuss in detail which jurisdictions we keep and drop, and provide the relevant justifications. We also present information on timing of enforcement agreements and magnitudes of enforced taxes.

Table 1 displays summary statistics of the most relevant property and property-month variables. Our main outcome of interest is booking price, which is defined as the posted price (i.e. asking price) for a night that has been booked. Note that the observed booking price is tax-inclusive before an enforcement agreement is implemented, and is tax-exclusive after the agreement is implemented. For brevity, we also refer to this as the price paid to hosts. The average booking price in our sample is roughly \$134 per night. This is a few dollars lower than the average asking price, which is defined as the posted price for an available night, of roughly \$137.

Our second outcome of interest is the number of nights booked per property-month, which is 5.6 on average in our estimation sample. Note that this variable represents the number of nights that were reserved during that month for any future stay. This means that the number of nights booked in a given property-month can exceed 31. We use this measure, rather than the number of nights a listing was occupied during a particular month, because Airbnb enforces the tax on all transactions made on or after the agreement's implementation date. For example, an enforcement agreement in Los Angeles went into effect August 2016. A booking made in July 2016 for a stay in October 2016 would not have been taxed through the website, but a booking made in September 2016 for a stay in October 2016 would.

It is important to note that bookings are not directly observed. Each property's calendar of availability is scraped every one to three days to detect any changes. A change in availability suggests a booking has occurred, which can be verified when a renter writes a review of the host and property after his or her stay. The primary concern with this approach is that we may incorrectly infer that a booking occurs, and thus over-measure the number of nights booked, when a host no longer wants to rent out his or her property for a particular night and blocks that night. This type of measurement error can lead to noisier estimates on the quantity of nights booked, but would only bias our estimates if the enforced tax rate is correlated with the measurement error. This could be true if, for example, the introduction of a tax enforcement agreement causes hosts to reduce their stated availability and those reductions are incorrectly inferred to be bookings. However, given that these Airbnb tax enforcement agreements reduce hosts' marginal costs, supply responses are likely to be positive rather than negative. A related concern is the possibility that stated availability does not accurately reflect actual availability as discussed in Farronato and Fradkin (2018). In particular, the authors point out that hosts may be better at updating their stated availability during periods of high demand. If true, this implies that we might over-measure nights booked during such periods. However, in our preferred specification discussed in Section 4, we are able to alleviate this concern by including metro-month-year fixed effects to absorb the effects of idiosyncratic demand shocks.

Going through the remaining summary statistics in Table 1, we see an average availability of 19.7 nights per property-month. This variable measures the number of nights per property-month that the listing is booked or available to be booked. Table 1 also presents additional summary statistics of interest to provide a fuller picture of the additional costs associated with Airbnb bookings and the substitutability between hotels and Airbnb listings. Among the property rentals in our sample, 70% are for the entire home or apartment. The average security deposit is $156.88, the average cleaning fee is $55.40, and the average extra person fee is $8.89. The average Airbnb rental has 1.41 bedrooms, 1.35 bathrooms, supports up to 3.67 guests, and requires a minimum stay of 3.6 nights. Roughly 13% of Airbnb listings are classified *business-ready* and 18% of properties are listed by *superhosts*.[15]

3 Conceptual Framework

In this section, we illustrate the impact of tax enforcement on the STR market and derive an estimable upper bound on pre-enforcement compliance. This is simple to present when hosts are price-takers, which follows the assumption made in Farronato and Fradkin (2018). In Appendix A, we show that our bounding argument is also valid under imperfect competition when there is little to no net exit of properties from the Airbnb market following a tax enforcement agreement. Furthermore, we show in Appendix B that an analogous bounding approach is available in cases where full enforcement does not affect statutory incidence, which is particularly relevant in light of the recent South Dakota v. Wayfair Inc. U.S. Supreme Court decision.

Suppose price-taking hosts offer short-term housing rentals across two broadly-defined periods.[16] In the first period, individual hosts bear the burden of collecting and remitting any applicable sales and lodging taxes with the possibility of evading. In the second period, the statutory burden of the tax shifts away from hosts towards Airbnb who collects and remits all applicable taxes from renters at the point of sale. Neither hosts nor renters can evade under this regime.

Consider first the hosts that comply with the tax in the first period. For these hosts, the supply of accommodations is given by $S^C(P-t)$ where P denotes the price renters pay to hosts and t denotes the tax remitted by hosts.[17] Next, consider hosts that evade taxes in the first period. The supply of accommodations that evade taxes is given by $S^E(P-R)$ where $R \geq 0$ denotes the marginal costs associated with the risks of evading. Now, suppose that

[15] The requirements for a property to be classified business-ready are outlined here: https://www.airbnb.com/help/article/1185/what-makes-a-listing-business-travel-ready. The requirements to be a superhost are outlined here: https://www.airbnb.com/help/article/828/what-is-a-superhost.

[16] For simplicity, suppose that each host offers a single listing.

[17] Although sales, hotel, and use taxes are ad valorem, we model the problem using a per-unit tax throughout the paper for simplicity.

the supply curves are linear, the mass of hosts is one, and let $\lambda \in [0,1]$ denote the proportion of tax-compliant listings. This implies that the market supply of accommodations is given by $S = (1-\lambda)S^E + \lambda S^C = S(P - \lambda t - (1-\lambda)R)$.[18] The first period equilibrium price, $P = P_1$, which is tax-inclusive, satisfies $S(P_1 - \lambda t - (1-\lambda)R) = D(P_1)$. Thus, the price paid by renters in the first period is P_1 and the average price received by hosts is $P_1 - \lambda t - (1-\lambda)R$. In the second period, the statutory tax regime changes; the statutory burden of the tax now falls on renters and is perfectly enforced by Airbnb. Thus, the second period equilibrium price, $P = P_2$, which is tax-exclusive, satisfies $S(P_2) = D(P_2 + t)$. In this case, renters pay $P_2 + t$ and hosts receive P_2.

The progression from Period 0, the hypothetical initial no-tax period, through Period 2, where taxes are collected from renters at the point of sale, is presented graphically in Figures 2 and 3. The initial impact of individual hosts incurring the statutory burden of hotel and sales taxes — that is, moving from Period 0 to Period 1 — is displayed in Figure 2. The tax introduction increases hosts' marginal costs, leading to a leftward shift in the supply curve equal to $\lambda t + (1-\lambda)R$. Next, the impact of a tax enforcement being reached — that is, moving from Period 1 to Period 2 — is depicted in Figure 3. Airbnb enforcement agreements shift the statutory burden of the tax onto renters and away from hosts. Thus, hosts' marginal costs return to their Period 0 level, which is reflected by a rightward shift in the supply curve equal to $\lambda t + (1-\lambda)R$. Contemporaneously, the demand curve drops by the full magnitude of the tax, given that the tax is salient and renters are unable to evade.

If all hosts comply in Period 1, such that $\lambda = 1$, then the principle of tax-collection invariance holds meaning the equilibrium price that hosts receive, price that renters pay, and quantity of nights booked are the same in Periods 1 and 2. However, if some hosts evade in Period 1, such that $\lambda < 1$, the enforcement agreement increases the tax wedge from $\lambda t + (1-\lambda)R$ to t.[19] This implies that enforcement increases the average price renters pay,

[18] Similar to the risks faced by evaders, one might think that compliance is also costly. Compliance costs, $C \geq 0$, can be incorporated such that $S^C = S^C(P - t - C)$, which implies that $S = (1-\lambda)S^E + \lambda S^C = S(P - \lambda t - \lambda C - (1-\lambda)R)$.

[19] Note that this implicitly assumes that $R < t$. This makes intuitive sense; no host would evade if $R \geq t$.

reduces the average price received by hosts, and equilibrium quantity falls. If λ and R are observable, then we can determine the deadweight loss associated with taxing Airbnb rentals, the marginal deadweight loss due to Airbnb enforcement, and the local slope of the supply curve. However, we do not observe λ or R in our setting. This means that the magnitude of the supply shift, and thus the slope of the supply curve, are unknown.

Although we do not observe λ and R, we can use the extreme case where supply is perfectly elastic to infer an upper bound on compliance. As shown in Figure 4, the largest possible shift in the supply curve is the distance between the two observed equilibrium prices paid to hosts, P_1 and P_2, which occurs when supply is perfectly elastic. Again, note that P_1 is the tax-inclusive pre-enforcement equilibrium price, while P_2 is the tax-exclusive post-enforcement equilibrium price. This implies that $\lambda t + (1 - \lambda)R \leq P_1 - P_2$. Thus, we can derive an upper bound on the pre-enforcement compliance rate:

$$\lambda \leq \frac{P_1 - P_2 - (1 - \lambda)R}{t} \leq \frac{P_1 - P_2}{t} = \frac{\Delta p}{t} \equiv \overline{\lambda}. \tag{1}$$

The power of this approach is its simplicity, as it only requires the practitioner to observe the tax magnitude along with equilibrium prices under partial and full compliance. In practice, we estimate this directly using the reduced-form effect of tax enforcement on the price paid to hosts. A smaller difference between P_1 and P_2 implies a larger portion of the enforced tax is passed through to renters, which also implies a smaller upper bound on pre-enforcement compliance. Note also that the larger the costs associated with evading are, the more conservative the estimated upper bound will be.

We also consider the other extreme, in which there is no compliance nor risk of evading (i.e. $\lambda = R = 0$), to infer a lower bound on the elasticity of supply. This case is depicted in Figure 5. The tax enforcement agreement does not induce a supply shock when pre-enforcement compliance is 0% and there is no risk of evading, implying that any change in the average price paid to hosts and nights booked is fully attributable to a demand curve shift. Thus, we can trace out the steepest possible supply curve using the observed pre- and post-enforcement prices and quantities, as shown in Figure 5, and infer a lower bound on the price elasticity of supply. This exercise produces two key insights. First, as the price elasticity of supply approaches the lower bound, the implied estimate of pre-enforcement compliance approaches 0%. Second, as the lower bound of the price elasticity of supply approaches infinity, the upper bound of pre-enforcement compliance approaches $\overline{\lambda}$.

4 Estimation

Our primary goals are to obtain an upper bound on pre-enforcement tax compliance and provide insight on tax incidence in the sharing economy. To this end, we estimate the effects of tax enforcement agreements on average booking prices and nights booked per property-month. Although Airbnb tax enforcement policies vary at the tax jurisdiction level, we use property as our cross-sectional unit to control for property-specific observed and unobserved heterogeneity. Consider the following difference-in-differences specification:

$$ln(Y_{ijmt}) = \gamma ln(1 + \tau_{jmt}) + \alpha_i + \delta_{mt} + \mu_{ijmt}. \tag{2}$$

In Equation (2), Y_{ijmt} is the outcome of interest for property i in tax jurisdiction j and metro m in month-year t. Our treatment variable is τ_{jmt}, which is the tax rate enforced directly through Airbnb.com in jurisdiction j at time t. This variable equals zero in the absence of a formal tax enforcement agreement, and becomes positive after an agreement is implemented. Following the literature, we estimate a log-log specification in order to interpret the effects of tax enforcement on the equilibrium outcomes in terms of percentage changes.[20] We include property fixed effects, α_i, to control for time-invariant observed and unobserved property-specific characteristics. We also include flexible time effects to control for time-specific shocks to a particular area, such as metro-specific seasonality and

[20]See, for example, Marion and Muehlegger (2008) and Kopczuk et al. (2016).

idiosyncratic demand shocks.[21] Equation (2) represents our preferred specification, which includes metro-month-year fixed effects δ_{mt}.[22]

The parameter of interest, γ, represents the percent change in Y associated with a 1% increase in $(1+\tau)$, which closely approximates a one percentage point increase in the tax rate enforced through the platform. As long as supply and demand have some non-zero and finite slope, and there is less-than-full compliance pre-enforcement, then our conceptual framework yields unambiguous predictions on our two main parameters of interest. First, the effect of tax enforcement on booking price, γ_P, is negative but greater than -1. That is, the average price paid to hosts does not fall by the full amount of the tax, which in turn implies that the tax-inclusive post-enforcement price renters pay increases. Second, due to this price effect, the effect of tax enforcement on nights booked (γ_Q) is negative.

To lend credibility to our empirical strategy, we first test for pre-treatment differences in the outcomes of interest between the treatment and control jurisdictions. In Table 2, we report sample averages by treatment status and test statistics for the estimated pre-enforcement differences. To obtain these results, we use a restricted sample including only pre-treatment property-month observations. We then regress the outcome variables of interest on a dummy variable that indicates whether a property is in an eventually-treated tax jurisdiction. We report tests under two specifications. The first includes only month-year fixed effects. The second uses metro-month-year fixed effects and property-level controls. Using both specifications allows us to informally test the effectiveness of using metro-month-year fixed effects and property-level characteristics to control for observable and unobservable

[21] For example, agreements in Cleveland, OH and Santa Clara, CA preceded large sporting events. In those cases, the metro-month-year fixed effects absorb the demand shock that affected jurisdictions close to those events.

[22] Booking price regressions are weighted by the number of nights booked. We include another set of estimates without weights, showing that our results are not sensitive to weighting. Also, in an alternate specification, we implement county-month-year fixed effects and find similar results. However, the inclusion of county-month-year fixed effects is more restrictive since fewer tax jurisdictions are part of counties that exhibit within-county-month-year variation in tax enforcement and magnitude. In another alternate set of specifications, we replace property fixed effects with tax jurisdiction fixed effects to control for time-invariant unobserved/omitted jurisdiction-specific characteristics and control for time-invariant observed property characteristics including number of bedrooms, bathrooms, maximum guests, and more.

differences between treatment and control jurisdictions. Note that we cannot condition on tax jurisdiction or property fixed effects in these tests because the dummy variable of interest does not vary within jurisdiction or property.

Focusing on the tests that include metro-month-year fixed effects, which are analogous to our preferred specification, the estimated difference in log bookings is -0.003 (see the last column of Table 2). This is a relatively precise zero, as the cluster-robust standard error is 0.019. The estimated difference in log booking price is 0.022 with a standard error of 0.042. These tests suggest that, conditional on the included controls, neither bookings nor prices predict an eventual tax enforcement agreement. While this is also true when we only control for month-year fixed effects, the magnitudes of the differences between our treatment and control jurisdictions are considerably larger.

Next, we test for the presence of differential pre-trends in the outcomes of interest between treatment and control jurisdictions, which would threaten the credibility of our difference-in-differences estimator. We estimate the following flexible event-study specification:

$$ln(Y_{ijmt}) = \alpha_i + \delta_{mt} + \sum_{k=-5}^{7} \gamma_k D_j 1(t - T_j = k) + \mu_{ijmt} \tag{3}$$

where T_j is the month of jurisdiction j's tax enforcement agreement and D_j is a binary treatment indicator equal to 1 if jurisdiction j is ever treated. From the set of observed treatment dates, we randomly assign synthetic enforcement dates to jurisdictions that are never treated. The coefficients γ_k measure the effects of tax enforcement on the outcome variables of interest k months relative to the enforcement. For values $k < -1$, the coefficients γ_k test for the presence of pre-trends. In practice, we collapse periods more than 7 months after enforcement into period $k = 7$. We omit period $k = -1$ when estimating booking price effects, but omit period $k = -2$ when estimating the effects on nights booked to test whether renters temporally shift their booking activity to the month before an enforcement agreement goes into effect.

Figure 6 shows that there is no visual or statistically significant evidence of a pre-trend in booking prices, suggesting that the parallel trends assumption holds. The dashed lines represent the 95% confidence intervals based on standard errors robust to jurisdiction-level clustering. The figure also shows a clear decrease in booking prices starting one month after a tax enforcement agreement goes into effect. Figure 7 also shows no evidence of a pre-trend in nights booked. While the nights booked estimates are less precise than the booking price estimates, there does appear to be a reduction in nights booked following enforcement. The positive coefficient in period $k = -1$, while not statistically significant, suggests that renters may indeed be aware of the upcoming tax enforcement implementation and behave accordingly. We test this further in Section 5.2, and find that strategic behavior does not appear to undermine our central estimates.

Next, we summarize the results of the event studies using a difference-in-differences framework identical to Equation (2), except we replace the treatment variable $ln(1 + \tau_{jmt})$ with a binary indicator equal to one in a given jurisdiction-month if there is any tax enforcement agreement in place. This approach provides a straightforward comparison of booking prices and nights booked before and after enforcement, though it is limited by the fact that it eliminates useful variation in tax rates within jurisdictions across time. The estimates are reported in Table 3. In our preferred specification (column 6), the estimated average effect of enforcement on booking prices is -0.032 and statistically significant at the 1% level. This suggests that booking prices fall by 3.2% when a tax enforcement agreement is in place. Considering the average enforced tax rate is 11.2%, this suggests that booking prices fall by 28% of the enforced tax. Similarly, we estimate that the number of nights booked decreases by about 4.5%, suggesting that a one percentage point increase in the enforced tax rate reduces nights booked by roughly 0.4%.

5 Results

In this section, we present several sets of results. To start, we present our main results that allow us to bound pre-enforcement compliance, the price elasticity of supply, and estimate the price elasticity of demand in the Airbnb market. Next, we show that our main estimates are robust to alternative sample restrictions, estimation choices, and the possibility of strategic booking behavior. We also show that supply-side responses to enforcement are consistent with our main results. Finally, to provide a richer understanding of the Airbnb market, we examine heterogeneity in enforcement effects by listing type, across the distribution of asking prices, and then calculate the welfare implications of Airbnb taxation.

5.1 Main Results

Panel A of Table 4 presents our main results on the booking price paid to hosts, where each estimate can be interpreted as the percentage change in price associated with a one percentage point increase in the enforced tax rate. Columns 4 through 6 present the estimates when using property-specific fixed effects. In practice, this approach identifies the effect of interest using deviations from property-level averages. The fourth column presents the estimate when we control for a location-invariant flexible time trend using month-year fixed effects. The estimated effect is -0.196 and is significant at the 5% level. We consider this a naïve estimate because it does not control for idiosyncratic shocks, differences in seasonality, or differences in growth across location. However, the results presented in columns 5 and 6 come from specifications allowing for such location-time-specific idiosyncrasies. The column 5 specification includes county-specific month-year fixed effects, while the column 6 specification includes metro-specific month-year fixed effects. These estimates are -0.332 and -0.240, respectively, and are both statistically significant at the 1% level. We prefer the specification including metro-month-year fixed effects, since including county-month-year fixed effects absorbs a substantial amount of useful variation.[23]

In columns 1 through 3 of Table 4, Panel A, we present the estimated effects of interest from specifications that exploit deviations from jurisdiction-level, rather than property-level, mean values by replacing the property fixed effects with tax jurisdiction fixed effects. We also include time-invariant property-level characteristics as controls in these three specifications.[24] The first column presents the estimate when we only include location-invariant month-year fixed effects. The estimated effect is -0.166 but is not statistically significant at conventional levels. The results presented in columns 2 and 3 come from specifications controlling for county-month-year fixed effects and metro-month-year fixed effects, respectively. The estimates are -0.229 and -0.217, respectively, and are both statistically significant at the 1% level. Notably, the estimated price effects are similar across all six specifications, all are statistically distinguishable from full pre-enforcement compliance (-1) at the 1% level, and none are statistically distinguishable from our preferred estimate of -0.240.[25]

Our preferred estimate of $\gamma_P = -0.240$ implies that the enforcement of a 10% tax reduces the booking price paid to hosts by 2.4%. This means that the majority of the tax — the remaining 7.6% of a 10% tax — is passed through as an increase in the tax-inclusive price renters pay following enforcement. In the extreme case of zero pre-enforcement compliance among hosts (i.e. $\lambda = 0$), this estimate implies that hosts in the Airbnb market bear no more than 24% of the tax burden. In the presence of some pre-enforcement compliance among hosts, part of this estimated reduction in booking prices is driven by compliant hosts being relieved of their statutory tax obligation. If true, this means that 24% is actually an overestimate of the economic tax burden borne by hosts in the Airbnb market.

[23]Specifically, 11 of the 27 counties in our estimation sample lack within county-month-year tax variation because they contain only one sufficiently large tax jurisdiction.

[24]These controls include number of bedrooms, number of bathrooms, maximum allowed guests, cleaning fee, security deposit, fee for each additional guest, listing type, rating, strictness of cancellation policy, minimum duration, number of photos, superhost designation, and business-ready designation.

[25]Note that all of these specifications are weighted by the number of nights booked in the month of the observation. We show the unweighted estimate from our preferred specification in column 4 of Table 6, which is very similar at -0.235 and significant at the 1% level. While we don't present all six specifications, the other unweighted estimates are very similar to their weighted counterparts as well.

Next, we infer an upper bound on pre-enforcement tax compliance among Airbnb hosts from our booking price estimate.

$$\overline{\lambda} \equiv \frac{p_1 - p_2}{t} = \frac{\Delta p}{t} = \frac{\Delta p / p_1}{t / p_1} \approx \frac{\Delta p / p_1}{\tau} \approx \frac{\Delta ln(p)}{\Delta ln(1+\tau)} = -\gamma_P{}^{26} \tag{4}$$

That is, our estimated effect of the enforced tax rate on booking price paid to hosts, $\gamma_P = -0.240$, implies that taxes are paid on no more than 24% of nights booked in the absence of formal Airbnb tax enforcement agreements. If we take into account the standard error of 0.059, the 95% confidence interval ranges from 12.2% to 35.8%.

Next, we turn to Panel B of Table 4. This row of results reflects the percentage change in nights booked associated with a one percentage point increase in the enforced tax rate, estimated using the same six specifications as the booking price results in Panel A. They range from -0.340 to -0.522, but none appear to be statistically distinguishable from one another. Focusing on our preferred specification (column 6), we estimate that a one percentage point increase in the enforced tax rate reduces nights booked by 0.361% (p=0.09). This negative quantity effect suggests that the negative demand-side response to tax enforcement dominates the contemporaneous positive supply-side response, which is consistent with only partial pre-enforcement host compliance. Given that the gap between the tax-inclusive price paid by renters and booking price received by hosts must equal the size of the enforced tax rate, we can define $\gamma_{P+\tau} = 1 + \gamma_P = 0.760$ to be the relationship between the enforced tax rate and tax-inclusive price paid by renters. We can then combine our estimated price and quantity effects to calculate the average price elasticity of demand for nights booked across listings, which is relatively inelastic: $\epsilon_{demand} = \gamma_Q / \gamma_{P+\tau} = \frac{-0.361}{0.760} = -0.48$.

Even though the assumption that hosts are price-takers is not necessary for our bounding argument to hold, making the assumption allows us to identify a lower bound on the price

[26] The first approximation is used because taxes on Airbnb bookings are actually ad valorem (τ), not a fixed per-unit amount t as we model throughout the paper for convenience. To see why the second approximation is true, suppose that the tax rate enforced is a 1% ad valorem tax. Before enforcement, the enforced tax rate (τ) is zero. Thus, $\tau = 0.01 = \Delta\tau$, which is approximately equal to $\Delta ln(1+\tau) = ln(1.01) - ln(1) = 0.00995$.

elasticity of supply of Airbnb listings. Given the nature of Airbnb tax enforcement agreements, the supply curve cannot be less elastic than what the estimated effects on booking price and quantity imply in the hypothetical scenario where supply does not shift at all.[27] In this hypothetical scenario, our estimates simply represent the equilibrium effects of a reduction in willingness-to-pay equal to the magnitude of the enforced tax rate, which allows us to trace out the local supply curve. If, in fact, there is any positive supply shock, using this simple approach would lead us to underestimate the true elasticity of supply. Using the ratio of the estimated effects of the enforced tax rate on booking price and nights booked, we calculate the lower bound of the price elasticity of supply to be $\epsilon_{supply} = \gamma_Q/\gamma_P = \frac{-0.361}{-0.240} = 1.5$.

Given our lower bound estimate, supply appears to be relatively elastic in the Airbnb market. This is consistent with Farronato and Fradkin (2018), who estimate the price elasticity of supply in the Airbnb market to be 2.16. This is plausible because of the clear outside options available to many hosts, and the low costs associated with exiting the short-term rental market. In particular, we would expect that it is relatively easy for hosts supplying entire-home rentals to substitute toward the long-term rental market, or for hosts listing their primary residence to exit the rental business altogether. This hints at a natural extension, presented in Section 5.3, where we estimate heterogeneity by type of rental unit.

Finally, Table 5 presents our estimates of the effect of the enforcement agreements on the number of reservations made, rather than the number of nights booked, in order to determine how much of the reduction in nights booked is attributable to shorter stays versus a reduction in visits altogether. We estimate that a 1 percentage point increase in the enforced tax rate decreases reservations by 0.17 percent. Though this estimate is statistically insignificant, it weakly suggests that the number of visits booked through Airbnb declines with the enforcement agreement. Considering the magnitude is roughly half of the effect of tax enforcement on nights booked (-0.36), this may imply the estimated effect of tax enforcement on nights booked is composed of both intensive- and extensive-margin responses.

[27] These arguments are discussed in Section 3 and displayed graphically in Figures 3 - 5.

Unfortunately, however, neither estimate is precise enough for us to confidently infer the relative magnitudes of the intensive- and extensive-margin responses.

5.2 Robustness Checks

In this subsection, we show that our main results are robust to different sample restriction choices, strategic timing of bookings among renters, and weighting our booking price regressions by the number of nights booked per property-month. Each column in Table 6 presents the results of a robustness check using our preferred specification that includes property fixed effects and metro-month-year fixed effects. In the first column, we present the estimates we obtain when we do not impose any restrictions on the characteristics of properties included in our estimation sample. We find that the effect of the enforced tax rate on booking price is -0.218, significant at the 1% level, and statistically and economically indistinguishable from our preferred estimate of -0.240. With respect to nights booked, we find that the effect of the enforced tax rate is -0.389, significant at the 10% level, and again statistically and economically indistinguishable from our preferred estimate of -0.361.

Columns 2 and 3 present robustness checks where we deviate from our preferred empirical strategy by instead restricting our estimation sample to properties with average asking prices that fall within the middle 90% and 50%, respectively, of their jurisdictions' distributions. This is in contrast to our main sample, where we include the middle 80 percent and omit properties falling in the top and bottom 10%. Again, we obtain statistically indistinguishable estimates of the effect of enforcement on booking price and nights booked. Using the middle 90%, the booking price estimate is -0.229 and significant at the 1% level, and the nights booked estimate is -0.334 but not statistically significant at conventional levels or distinguishable from our preferred estimate. When we restrict to the middle 50% of the asking price distribution, the booking price estimate is -0.259 and significant at the 1% level. Again, the nights booked estimate of -0.316 is not statistically significant at conventional levels or distinguishable from our preferred estimate.

In column 4, we show that our preferred booking price estimate is not substantially affected by the fact that we weight the regression by the number of nights booked per property-month; the unweighted booking price estimate is -0.235, significant at the 1% level, and statistically indistinguishable from the preferred estimate. In column 5, we present the estimated effect of enforcement on nights booked after dropping properties that are never booked throughout the sample period. The estimated effect of -0.462 is slightly larger in magnitude, statistically significantly different from 0 at the 5% level, but is not statistically distinguishable from our preferred estimate.

Finally, in columns 6 and 7 of Table 6, we compare two additional specifications with our main estimates on booking price and nights booked to test whether individuals strategically booked reservations in anticipation of upcoming tax enforcement agreements. We do this by omitting the two months around the start of the enforcement agreements. In column 6 we omit the first month that the tax goes into effect, and in column 7 we also omit the last month prior to the tax enforcement agreement. Because the enforcement agreements were generally only announced within a couple weeks of the enforcement date, omitting these two months should remove any strategic-booking bias in our estimators. The estimated effect of the enforced tax rate on booking price of -0.288, obtained after omitting both of these months, is slightly larger in magnitude but not statistically distinguishable from our preferred estimate. The estimated effect on bookings of -0.228 is smaller in magnitude, but again is not statistically distinguishable from our preferred estimate.

5.3 Heterogeneity by Listing Type and Relative Price

Next, we examine whether the estimated enforcement effects vary between entire-home and private-room listings, as well as across the distribution of asking prices. These analyses aim to provide further insight on the Airbnb market by asking which listings are more likely to evade taxation in the absence of full enforcement, and how elasticities and the incidence of taxation differ across listings.

The first two columns of Table 7 present the listing type heterogeneity results. In the first column, we present the estimated effects of the enforced tax rate on booking price and nights booked using our preferred specification and including only entire-home listings in the estimation sample; the second column repeats this but includes only private-room listings in the estimation sample. In Panel A, we show that the negative booking price effect for entire-home listings is substantially larger than for private-room listings: -0.289 (0.084) compared to -0.124 (0.037), respectively (standard errors in parentheses). This suggests that private-room listings are more likely to evade taxation before enforcement and pass a larger share of the tax onto renters after enforcement. In Panel B, we show that the negative nights booked effect for entire-home listings is also substantially larger than for private-room listings: -0.446 (0.276) compared to -0.063 (0.144), respectively. While the nights booked estimates are relatively imprecise and do not appear to be statistically indistinguishable from one another, nor zero, they do suggest that both demand and supply for private-room listings are more inelastic than for entire-home listings. This makes sense, as private-room listings tend to be cheaper and there are fewer outside options for both renters and hosts.

Columns 3 through 6 of Table 7 present the asking price heterogeneity results. Within each jurisdiction, we assign properties to quartiles based on their sample-long average asking prices. The estimates in Panel A show that the negative booking price effect is relatively small among the lowest-priced listings at -0.165 (0.040), suggesting that these properties are more likely to evade taxation before enforcement and pass a larger share of the tax onto renters after enforcement. However, the effect of enforcement on booking price is quite similar among listings in the second, third, and fourth asking price quartiles at -0.232, -0.266, and -0.267, respectively. In Panel B, we see a consistently negative effect of enforcement on nights booked ranging from -0.118 to -0.522. Listings in the second quartile of asking prices appear to experience the largest decrease in nights booked, which may suggest that these listings have the closest substitutes. That said, the estimated enforcement effects on nights booked are noisy and do not follow a clear pattern across the asking price quartiles.

5.4 Supply Responses

In this subsection, we explore whether supply-side responses to tax enforcement are consistent with our main findings. First, we examine asking prices to determine whether there is indeed a reduction in the price hosts are willing to accept after an enforcement agreement relieves them of their statutory tax burden. Second, we estimate the effect of enforcement on the number of nights available per property-month to determine whether there is an intensive-margin supply response. Third, we estimate entry and exit effects to determine whether there is evidence of a contemporaneous negative extensive-margin supply response to tax enforcement, which would threaten the validity of our estimated upper bound on pre-enforcement compliance.[28]

In column 1 of Table 8, we present the effect of enforcement on hosts' asking prices using our preferred specification including property fixed effects and metro-month-year fixed effects. We find that a one percentage point increase in the enforced tax rate reduces hosts' asking prices by an average of 0.09 percent, which is statistically significant at the 10% level. This is consistent with the notion that shifting the statutory tax burden away from hosts toward renters reduces the prices hosts are willing to accept, since they are no longer directly responsible for collecting and remitting taxes.

Turning to column 2 of Table 8, we present the estimated effect of enforcement on the number of nights available per property-month from our preferred specification. We find that a one percentage point increase in the enforced tax rate reduces nights available per month by 0.345 percent, or 0.07 nights relative to the mean of 19.7. While this estimate is statistically significant at the 10% level, it does not appear to reflect an economically meaningful negative supply response on the intensive margin.

Next, we estimate the effects of enforcement on the entry and exit of listings. We estimate entry using a binary outcome variable that equals 1 in the first month a listing is observed in our data and 0 otherwise. We find that a one percentage point increase in the enforced tax

[28] See our treatment of entry and exit in Appendix A for details.

rate increases the proportion of new listings in a given month by 0.061 percentage points, which is roughly 1% relative to the sample mean of 0.059. Similarly, we estimate exit using a binary outcome variable that equals 1 in the last month a listing is observed in our data and 0 otherwise.[29] We find that a one percentage point increase in the enforced tax rate has a positive, but economically and statistically insignificant, effect of 0.002 percentage points (0.1% relative to the sample mean of 0.021) on the proportion of listings terminated in a given month. Using an alternative definition of exit, where exit is defined as the last month a listing has at least one day available for booking, we find a positive effect on exit of 0.014 percentage points (0.4% relative to the sample mean of 0.036). However, this estimate is also statistically indistinguishable from zero. The entry and exit estimates jointly suggest that, if anything, tax enforcement has a small positive effect on supply on the extensive margin, which does not threaten the validity of our estimated upper bound on pre-enforcement compliance.

5.5 Welfare Implications

In this section, we use our estimates of the price elasticities of supply and demand to shed light on the welfare effects of Airbnb tax enforcement agreements assuming linear supply and demand. We consider this an important exercise given how little is known about STR markets and, more broadly, the excess burden from taxing online markets. In the textbook setting, the introduction of a tax will generally increase government revenue but generate a net loss in social surplus. Using information on tax rates, bookings, and booking prices, we first calculate the tax revenue generated among the treated jurisdictions in our sample. Then, we calculate the implied deadweight loss (DWL) associated with taxing these markets.

In our estimation sample there are 1,649,891 property-month observations, spanning 38 jurisdictions, with a nonzero enforced tax rate. Among these observations, the average

[29]For both our entry and exit regressions, we generate the binary entry and exit variables using the full sample period. We then omit the first and last months of our sample to estimate entry and exit, since we cannot determine month of entry among properties present in the first month of our data, nor can we determine exit among properties that are present in the final month of our data.

booking price is \$137, the average number of nights booked per month is 5.90, and the average enforced tax rate is 11.22%. Thus, the average listing subjected to a tax enforcement agreement generates \$91 in tax revenue each month.[30] Given that the average number of listings per jurisdiction-month is 2,245 across the post-treatment months, we calculate that the average treated jurisdiction received roughly \$204,000 per month in tax revenue from Airbnb listings. This translates to roughly \$149.8 million total across the 735 jurisdiction-months subjected to a tax enforcement agreement. However, this reflects all revenue collected from Airbnb listings after an enforcement agreement was implemented, which overstates the *additional* revenue generated by the agreements to the extent that taxes were paid on some transactions beforehand. Using our estimated upper bound of pre-enforcement compliance of 24%, we calculate that enforcement agreements increased revenue by at least \$69 per property-month, or roughly \$155,000 per jurisdiction-month.

Next, we turn to calculating the excess burden (DWL) from taxing the Airbnb market, as well as the marginal DWL created by the tax enforcement agreements, for three different values of pre-enforcement compliance (λ). These values include the lower bound, $\lambda = 0$, the upper bound, $\lambda = 0.24$, and the implied rate of compliance when the price elasticity of supply is 2.16, as estimated by Farronato and Fradkin (2018), $\lambda = 0.07$.[31]

The first step is to derive a linear approximation of the demand curve for the average

[30] Note that this does not include any revenue that may be generated by taxing cleaning fees and extra person fees, which are also subject to taxation under the enforcement agreements. While we can calculate the tax revenue generated from cleaning fees, we do not have information on whether a fee was paid for extra people. For the purposes of this exercise, which is comparing the tax revenue generated to the DWL generated, we consider tax revenues generated from nightly booking prices to be the most relevant consideration.

[31] To calculate this implied value of λ, we must calculate the portion of the estimated price effect that is attributable to a shift rather than a movement along the supply curve when the elasticity is 2.16. We start with the post-enforcement average booking price of \$137, average nights booked of 5.9, and average enforced tax rate of 11.22%. Next, we impute the counterfactual (i.e. partial compliance) average nights booked by calculating $5.9 \cdot (1 - 0.1122\gamma_Q) = 5.9 \cdot (1 + 0.1122 \cdot 0.361) = 6.14$. Then, assuming the price elasticity of supply is 2.16, we rearrange the elasticity equation to obtain $\Delta P = \frac{\Delta Q}{\epsilon_{supply}} \cdot \frac{P}{Q} = \frac{6.14 - 5.9}{2.16} \cdot \frac{137}{5.9} = \$2.57$. Adding this to \$137 yields the counterfactual tax-exclusive booking price of \$139.57 per night. The next step is to compare \$139.57 to the counterfactual booking price in the hypothetical case where $\lambda = 0$, enforcement only shifts demand, and $\epsilon_{supply} = 1.5$. This gives us $137 \cdot (1 - 0.1122\gamma_P) = 137 \cdot (1 + 0.1122 \cdot 0.24) = \$140.69$, which yields a difference of $\$1.12 = \lambda t$. Dividing this \$1.12 by the per-unit tax t at the average booking price of \$137, \$15.37, yields an implied $\lambda = 0.07$.

treated listing using the estimated demand elasticity of -0.48, the post-enforcement tax-inclusive average booking price of $\$137 \times (1 + 0.1122) = \$152.37$, and the post-enforcement average nights booked of 5.9. Using these three inputs, the inverse demand curve is given by $P = 440.79 - 48.88Q$. The second step is to derive three hypothetical linear approximations of the supply curve for the average treated listing: one for each $\lambda \in \{0, 0.07, 0.24\}$. This derivation uses the post-enforcement tax-exclusive average booking price of \$137, average nights booked of 5.9, and the price elasticity of supply associated with each hypothetical value of λ: 1.5, 2.16, and ∞, respectively.[32] We calculate that the inverse supply curve is given by $P = 45.92 + 15.44Q$ when $\epsilon_{supply} = 1.5$, $P = 73.57 + 10.75Q$ when $\epsilon_{supply} = 2.16$, and $P = 137$ when $\epsilon_{supply} = \infty$. With these inverse supply and demand functions, we can solve for the no-tax equilibrium in each hypothetical case, which then allows us to solve for the DWL from taxing the Airbnb market.

We summarize the results of this exercise in Table 9, which presents the hypothetical no-tax equilibrium for each price elasticity of supply, partial- and full-compliance DWL per property-month, and the implied share of the tax burden borne by consumers. In the first row, we present the calculated values of interest given $\lambda = 0$ and $\epsilon_{supply} = 1.5$, which entails a no-tax equilibrium booking price of \$140.69 and a no-tax equilibrium quantity of 6.14 nights booked per property-month. Note that in this scenario, where compliance is zero in the absence of an enforcement agreement, the no-tax equilibrium is the same as the counterfactual "partial-compliance" equilibrium. Thus, the total DWL from taxing the Airbnb market, \$1.84 per property-month, is equal to the marginal DWL associated with enforcement. In this scenario, consumers bear 76% of the economic tax burden.[33] This scenario yields a lower bound on the DWL created by taxing Airbnb listings, as well as the share of the economic tax burden borne by consumers.

In the second row, we present the calculated values of interest given $\lambda = 0.07$ and

[32]Recall that the price elasticity of supply lower bound is 1.5 when pre-enforcement compliance is zero, and that the price elasticity of supply upper bound is ∞ at the compliance upper bound of 24%. See Footnote 31 for the math that links the price elasticity of supply of 2.16 to $\lambda = 0.07$.

[33]We derive this incidence using the following calculation: $1 + \frac{\epsilon_{demand}}{\epsilon_{supply} - \epsilon_{demand}}$.

$\epsilon_{supply} = 2.16$. Here, we get a no-tax equilibrium booking price of \$139.77 and equilibrium quantity of 6.14 nights booked per property-month. Total DWL from taxing the Airbnb market is \$1.98 per property-month in this scenario. Comparing this to DWL under partial-compliance, which is only \$0.01 per property-month, \$1.97 of the total DWL is attributable to the implementation of enforcement agreements.[34] Here, the implied share of the tax burden borne by consumers is 82%. Finally, the third row presents the calculated values when supply is perfectly elastic. In this scenario, the no-tax equilibrium booking price is the same as the post-enforcement average booking price of \$137. The no-tax equilibrium quantity is 6.21 nights booked per property-month. Here, we calculate that total post-enforcement DWL due to taxation is \$2.42 per property-month. In the absence of enforcement, the total DWL is \$0.14 per property-month, implying that the marginal DWL associated with enforcement is \$2.28 per property-month.[35] In this scenario, which provides an upper bound on the DWL created by taxing Airbnb listings, the tax burden is borne entirely by consumers.

Multiplying these DWL values by the average number of listings among jurisdictions post-treatment (2,245) yields an aggregate DWL of \$4,100 to \$5,400 per jurisdiction-month. Across all three scenarios, the calculated excess burden is quite small relative to the \$204,000 in total tax revenue generated per jurisdiction-month. Moreover, most of the tax burden appears to fall on consumers. This suggests that such enforcement agreements may be politically popular at the state and local level, since they raise revenue at a relatively small efficiency cost and most of the economic burden is borne by individuals who are visiting from outside the local area. That said, caution is needed when considering these estimates. First, they do not take into account the costs of reaching a tax enforcement agreement. Second, these are partial equilibrium calculations, meaning they do not account for efficiency or revenue effects associated with renters and hosts substituting toward other markets. Finally,

[34] Counterfactual total DWL is determined using post-enforcement average nights booked per property-month, the imputed counterfactual average nights booked per-property month of 6.14, and the wedge between the tax-inclusive and tax-exclusive price of \$1.12. See Footnote 31 for the calculation of the latter two parameters.

[35] Counterfactual DWL $= 0.5 \times (6.21 - 6.14) \times (\$137 \cdot 0.1122 \cdot 0.24) = \$0.14$ per property-month.

these estimates are specific to our sample of jurisdictions, which are among the largest Airbnb markets in the U.S. and may not be representative of those not included in the sample.

6 Conclusion

In this paper, we develop a simple approach to bound of pre-enforcement tax compliance using prices before and after a change from partial to full compliance. We illustrate this approach in the context of Airbnb tax enforcement agreements with state and local governments, where full enforcement is achieved by shifting the statutory tax burden away from individual hosts toward renters via the platform. We also show that researchers can use a similar approach to study the broad range of markets affected by the recent U.S. Supreme Court decision in South Dakota v. Wayfair Inc., which enables states to fully enforce sales and use taxes on online transactions. Exploiting variation in Airbnb tax enforcement agreements, we use a difference-in-differences framework to estimate the effects of tax enforcement on booking price and quantity. We find that enforcement of a 10% tax reduces the price hosts receive by 2.4% and increases the price renters pay by 7.6%. This price effect implies an upper bound of 24% compliance in the absence of an enforcement agreement.

We also find that enforcement of a 10% tax reduces nights booked by 3.6%, which allows us to provide insight on the price elasticity of demand and supply in the Airbnb market. Combined with the estimated effect of enforcement on price, we calculate a price elasticity of demand of -0.48. If Airbnb hosts are price-takers, we can also use these estimates to obtain a lower bound on the price elasticity of supply of 1.5. This estimate is consistent with the 2.16 price elasticity of supply estimated by Farronato and Fradkin (2018) in their study of the Airbnb market. If we assume that 2.16 is the true price elasticity of supply, our results imply that taxes are only paid on roughly 7% of Airbnb transactions before an enforcement agreement is implemented.

Overall, these results indicate that tax enforcement agreements between state and local governments and Airbnb can substantially increase tax compliance, as *at least* 76% of transactions evade taxation pre-enforcement. This implies an increase in revenue of at least \$69 per property-month, or \$155,000 per jurisdiction-month. Moreover, taxing Airbnb listings imposes a relatively small efficiency cost on the local market of \$0.03 per dollar of additional revenue. Our finding that demand is less price-sensitive than supply implies that consumers bear more of the tax burden of Airbnb tax enforcement agreements. This may actually be a desirable feature from the perspective of state and local governments, as the additional revenue and inefficiency associated with taxation in this setting is disproportionately borne by visitors from outside the jurisdiction. However, our results also suggest that taxing Airbnb is a relatively ineffective policy lever for interest groups seeking to reduce Airbnb market activity.

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Tables and Figures

Table 1: Summary Statistics

	Mean	Std. Dev.	25^{th} Pctl	Median	75^{th} Pctl	Obs
	Panel A: Property-Month Level Summary					
Booking Price	133.68	78.50	83.34	115	163	963,352
Days Booked / Month	6.05	11.96	0	0	7	2,590,954
Tax Rate	0.07	0.06	0.00	0.09	0.14	2,590,954
Asking Price	137.28	88.58	83.82	117.33	167.93	1,998,846
Nights Avail.	19.67	12.88	3	28	30	2,590,954
	Panel B: Property Level Summary					
Bedrooms	1.41	0.93	1	1	2	170,619
Bathrooms	1.35	0.62	1	1	2	170,324
Max Guests	3.67	2.13	2	3	5	170,619
Entire Home/Apt	0.70	0.46	0	1	1	170,619
Rating	4.69	0.45	4.50	4.80	5.00	106,304
Security Deposit	156.88	316.12	0	0	200	170,619
Cleaning Fee	55.40	60.63	0	40	85	170,619
Extra People Fee	8.89	18.64	0	0	15	170,619
Minimum Stay (Days)	3.60	17.24	1	2	3	170,320
Business Ready	0.13	0.34	0	0	0	170,619
Superhost	0.18	0.38	0	0	0	158,074
Number of Photos	14.77	11.67	7	12	20	165,364

Notes: Sample excludes listings for shared rooms, properties that have >4 bedrooms, properties that have a >12 guest limit, or properties for which average asking price falls in bottom or top decile of the jurisdiction's distribution.

Table 2: Pre-Enforcement Differences in Outcomes

	Full Sample	Treated	Untreated	(Treated - Untreated)	
Booking Price	127.91	129.50	123.11	8.044	1.446
	(70.55)	(70.66)	(69.99)	(7.911)	(5.160)
ln(Booking Price)	4.73	4.75	4.68	0.063	0.022
	(0.48)	(0.47)	(0.50)	(0.056)	(0.042)
Nights Booked	6.47	6.64	6.01	0.481	0.260
	(12.51)	(12.60)	(12.22)	(0.366)	(0.175)
ln(1 + Nights Booked)	0.97	0.99	0.90	0.057	-0.003
	(1.35)	(1.35)	(1.32)	(0.049)	(0.019)
Asking Price	131.56	132.48	128.90	0.937	0.232
	(79.07)	(77.56)	(83.27)	(9.435)	(6.652)
ln(Asking Price)	4.75	4.77	4.71	0.032	0.024
	(0.49)	(0.47)	(0.53)	(0.062)	(0.052)
Observations	870,028	636,861	233,167		
Month-Year FE				✓	-
Metro-Month-Year FE				-	✓
Property-level Controls				-	✓

Notes: The first three columns display sample mean and standard deviations for the full, treated, and untreated samples in months when no tax enforcement agreement was in place. The last two columns display tests for whether being in a treated jurisdiction is correlated with outcomes in the pre-enforcement months. Each estimate is from a regression of the outcome variable on a dummy variable for being in a jurisdiction that is eventually treated. The regressions are restricted to observations when there was no tax enforcement agreement in place. The sample excludes listings for shared rooms, properties that have >4 bedrooms, properties that have a >12 guest limit, or properties for which average asking price falls in bottom or top decile of the jurisdiction's distribution. Standard errors are robust to clustering at the tax jurisdiction level. *** p<0.01, ** p<0.05, * p<0.10.

Table 3: Tax Enforcement, Booking Price, and Bookings:
Standard Difference-in-Differences Specification

	Panel A: ln(Booking Price)					
Tax × Post	-0.025	-0.024***	-0.024***	-0.030*	-0.038***	-0.032***
	(0.017)	(0.007)	(0.006)	(0.016)	(0.008)	(0.007)
Observations	963,352	963,344	963,352	935,691	935,683	935,691
	Panel B: ln(1+Nights Booked)					
Tax × Post	-0.033	-0.049**	-0.046***	-0.047**	-0.054**	-0.045**
	(0.024)	(0.021)	(0.016)	(0.022)	(0.027)	(0.020)
Observations	2,590,954	2,590,954	2,590,954	2,586,260	2,586,260	2,586,260
Tax Jurisdiction FE	✓	✓	✓	-	-	-
Property FEs	-	-	-	✓	✓	✓
Month-Year FE	✓	-	-	✓	-	-
County-Month-Year FE	-	✓	-	-	✓	-
Metro-Month-Year FE	-	-	✓	-	-	✓

Notes: Regressions of the natural log of booking price (Panel A) and the number of bookings (Panel B) on a dummy variable version of the treatment. Each outcome is estimated using six different specifications. Column 1 includes jurisdiction fixed effects and month-year fixed effects. Column 2 includes jurisdiction fixed effects and county-month-year fixed effects. Column 3 includes jurisdiction fixed effects and metro-month-year fixed effects. Columns 4-6 repeat the three specifications replacing tax jurisdiction fixed effects with property fixed effects. The estimation sample excludes listings for shared rooms, properties that have >4 bedrooms, properties that have a >12 guest limit, or properties for which average asking price falls in bottom or top decile of the jurisdiction's distribution. Estimates for booking price are weighted by the number of bookings contributing to the average monthly booking price observations. Standard errors are robust to clustering at the tax jurisdiction level. *** p<0.01, ** p<0.05, * p<0.10.

Table 4: Tax Enforcement, Booking Price, and Bookings:
Differences-in-Differences Specification Exploiting Tax Rate Variation

	Panel A: ln(Booking Price)					
ln(1 + tax)	-0.166	-0.229***	-0.217***	-0.196**	-0.332***	-0.240***
	(0.108)	(0.058)	(0.046)	(0.087)	(0.070)	(0.059)
Observations	963,352	963,344	963,352	935,691	935,683	935,691
	Panel B: ln(1+Nights Booked)					
ln(1 + tax)	-0.469***	-0.340	-0.431**	-0.522***	-0.392	-0.361*
	(0.120)	(0.204)	(0.186)	(0.138)	(0.290)	(0.211)
Observations	2,590,954	2,590,954	2,590,954	2,586,260	2,586,260	2,586,260
Tax Jurisdiction FE	✓	✓	✓	-	-	-
Property FEs	-	-	-	✓	✓	✓
Month-Year FE	✓	-	-	✓	-	-
County-Month-Year FE	-	✓	-	-	✓	-
Metro-Month-Year FE	-	-	✓	-	-	✓

Notes: Regressions of the natural log of booking price (Panel A) and the number of bookings (Panel B) on our treatment variable. Each outcome is estimated using six different specifications. Column 1 includes jurisdiction fixed effects and month-year fixed effects. Column 2 includes jurisdiction fixed effects and county-month-year fixed effects. Column 3 includes jurisdiction fixed effects and metro-month-year fixed effects. Columns 4-6 repeat the three specifications replacing tax jurisdiction fixed effects with property fixed effects. The estimation sample excludes listings for shared rooms, properties that have >4 bedrooms, properties that have a >12 guest limit, or properties for which average asking price falls in bottom or top decile of the jurisdiction's distribution. Estimates for booking price are weighted by the number of bookings contributing to the average monthly booking price observations. Standard errors are robust to clustering at the tax jurisdiction level. *** p<0.01, ** p<0.05, * p<0.10.

Table 5: Effect of Enforcement on Number of Reservations

	ln(1+Number of Reservations)					
ln(1 + tax)	-0.273***	-0.144	-0.185*	-0.311***	-0.199	-0.173
	(0.080)	(0.117)	(0.106)	(0.095)	(0.173)	(0.128)
Observations	2,590,954	2,590,954	2,590,954	2,586,260	2,586,260	2,586,260
Tax Jurisdiction FE	✓	✓	✓	-	-	-
Property FEs	-	-	-	✓	✓	✓
Month-Year FE	✓	-	-	✓	-	-
County-Month-Year FE	-	✓	-	-	✓	-
Metro-Month-Year FE	-	-	✓	-	-	✓

Notes: Regressions of the natural log of the number of reservations on our treatment variable. Column 1 includes jurisdiction fixed effects and month-year fixed effects. Column 2 includes jurisdiction fixed effects and county-month-year fixed effects. Column 3 includes jurisdiction fixed effects and metro-month-year fixed effects. Columns 4-6 repeat the three specifications replacing tax jurisdiction fixed effects with property fixed effects. The estimation sample excludes listings for shared rooms, properties that have >4 bedrooms, properties that have a >12 guest limit, or properties for which average asking price falls in bottom or top decile of the jurisdiction's distribution. Standard errors are robust to clustering at the tax jurisdiction level. *** p<0.01, ** p<0.05, * p<0.10.

Table 6: Robustness Checks, Booking Price and Nights Booked

	No Property Restrictions	Omit Cheapest & Most Expensive 5%	Omit Cheapest & Most Expensive 25%	Unweighted	Drop Never-Booked	Drop First Post-Tax Month	Drop First Post-Tax & Last Pre-Tax Months
				Panel A: ln(Booking Price)			
ln(1 + tax)	-0.218***	-0.229***	-0.259***	-0.235***	-	-0.273***	-0.288***
	(0.060)	(0.056)	(0.063)	(0.067)	-	(0.063)	(0.063)
Observations	1,200,885	1,054,683	633,574	935,691	-	911,595	888,661
				Panel B: ln(1+Nights Booked)			
ln(1 + tax)	-0.389*	-0.334	-0.316	-	-0.462**	-0.262	-0.228
	(0.217)	(0.221)	(0.253)	-	(0.223)	(0.219)	(0.221)
Observations	3,508,692	2,977,353	1,720,593	-	2,270,804	2,530,149	2,477,541

Notes: Regressions of the natural log of booking price (Panel A) and the number of bookings (Panel B) on our treatment variable. All regressions use the preferred specification, which includes property fixed effects and metro-month-year fixed effects. Column 1 presents the results when removing all the property-characteristic restrictions imposed in our central estimates: exclude if listing is for a shared room, property has >4 bedrooms, property has >12 guest limit, or average asking price falls in bottom or top decile of the jurisdiction's distribution. Columns 2-7 retain these restrictions, except for the varying price restrictions in columns 2 and 3, which test the robustness of the asking price restriction using the top and bottom 5th percentile and 25th percentile as cutoffs, respectively. Estimates for booking price are weighted by the number of bookings contributing to the average monthly booking price observations, except in column 4 which presents the unweighted version of the preferred booking price estimate from column 6 of Table 4. Column 5 presents the nights booked estimate after excluding properties that have never been booked from the sample. Columns 6 and 7 test for strategically-timed booking behavior among consumers by excluding observations of the first post-enforcement and last pre-enforcement months among properties in treated jurisdictions. Standard errors are robust to clustering at the tax jurisdiction level. *** p<0.01, ** p<0.05, * p<0.10.

Table 7: Heterogeneity Estimates

	Listing Type		Asking Price Quartiles			
	Entire Home	Private Room	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile
			Panel A: ln(Booking Price)			
ln(1 + tax)	-0.289***	-0.124***	-0.165***	-0.232***	-0.266***	-0.267**
	(0.084)	(0.037)	(0.040)	(0.056)	(0.099)	(0.105)
Observations	838,380	328,363	284,709	322,597	315,013	244,486
			Panel B: ln(1+Nights Booked)			
ln(1 + tax)	-0.446	-0.063	-0.377	-0.522*	-0.118	-0.320*
	(0.276)	(0.144)	(0.261)	(0.278)	(0.318)	(0.176)
Observations	2,329,361	1,047,633	792,078	861,504	862,029	863,933

Notes: Regressions of the natural log of booking price (Panel A) and the number of bookings (Panel B) on our treatment variable. All regressions use the preferred specification, which includes property fixed effects and metro-month-year fixed effects. The estimation samples are not restricted by number of bedrooms, guest limit, or price before the split-sample heterogeneity analyses are performed. As in our main estimation sample, we do omit shared-room listings. Columns 1-2 present the results when splitting the sample into entire home listings and private room listings, respectively. Columns 3-6 present the results when splitting the sample into jurisdiction-based quartiles of average asking prices. Estimates for booking price are weighted by the number of bookings contributing to the average monthly booking price observations. Standard errors are robust to clustering at the tax jurisdiction level. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 8: Supplier Responses to Tax Agreements

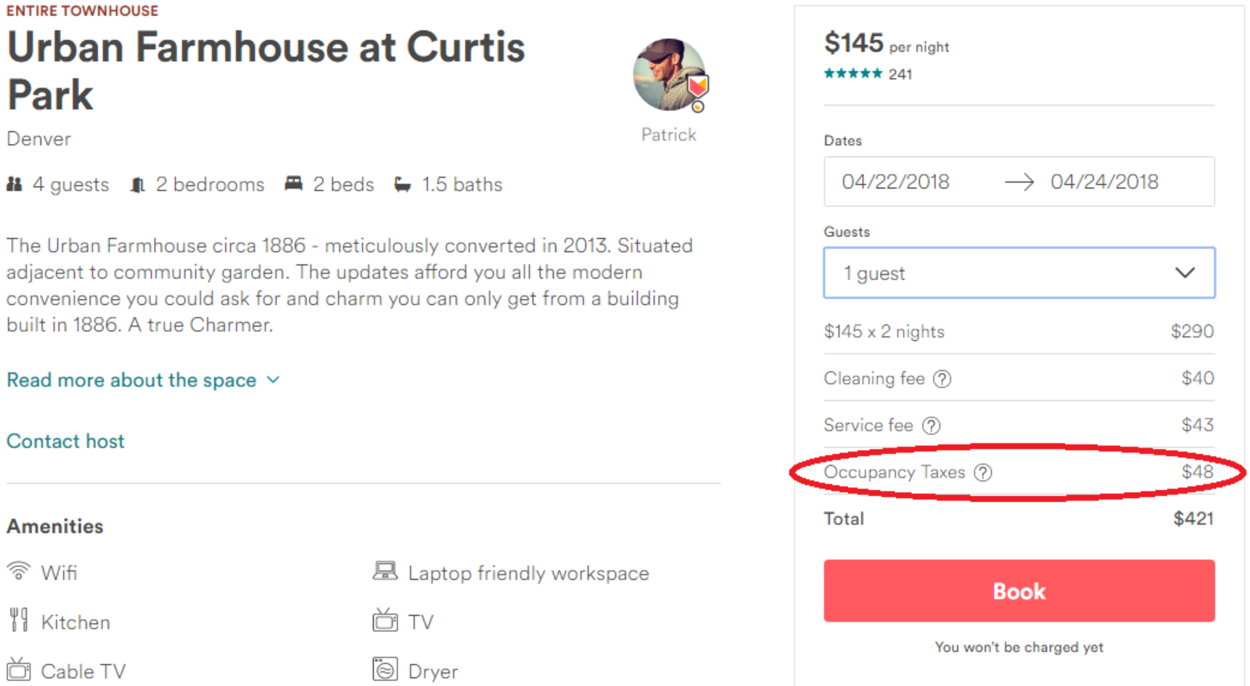
	ln(Ask P)	ln(1 + Nights Available)	Entry (1st Mo.)	Last Month Listed
ln(1 + tax)	-0.090*	-0.345*	0.061**	0.002
	(0.047)	(0.187)	(0.028)	(0.009)
Mean of DV			0.059	0.021
Std. Dev.			(0.236)	(0.143)
Observations	1,987,813	2,586,260	2,450,458	2,450,458

Notes: Estimates of potential supply side responses. All estimates are from our preferred specification that includes metro-month fixed effect and property fixed effects. Regressions are at the property-month level. *ln(Ask P)* is the log of the properties asking price in the given month. *Nights Available* is the number of nights the listing was available to be booked in each month. *Entry (1st Mo.)* is a dummy variable equal to one in the first month a property appears in our sample. *Last Month* is a dummy variable equal to one in the last month that a property is (*Listed*) on the site, or the last month that a property has more than 0 nights available for booking (*>0 Availability*). The Entry and Last Month samples omit the first and last month of our sample period. The estimation sample excludes listings for shared rooms, properties that have >4 bedrooms, properties that have a >12 guest limit, or properties for which average asking price falls in bottom or top decile of the jurisdiction's distribution. Standard errors are robust to clustering at the tax jurisdiction level. *** p<0.01, ** p<0.05, * p<0.10.

Table 9: Hypothetical No-Tax Equilibria and Deadweight Loss Per Property-Month

λ	ϵ_{supply}	No-Tax Eqm. Price	No-Tax Eqm. Quantity	Total DWL post-enforcement	Total DWL counterfactual	Consumer Tax Incidence
0	1.5	\$140.69	6.14	\$1.84	\$0.00	76%
0.07	2.16	\$139.77	6.16	\$1.98	\$0.01	82%
0.24	∞	\$137.00	6.21	\$2.42	\$0.14	100%

Figure 1: Airbnb Screenshot



Source: `https://www.airbnb.com/rooms/12365447`, accessed 4/16/2018.

Figure 2: Initial Introduction of Weakly-Enforced Taxes on Individual Hosts

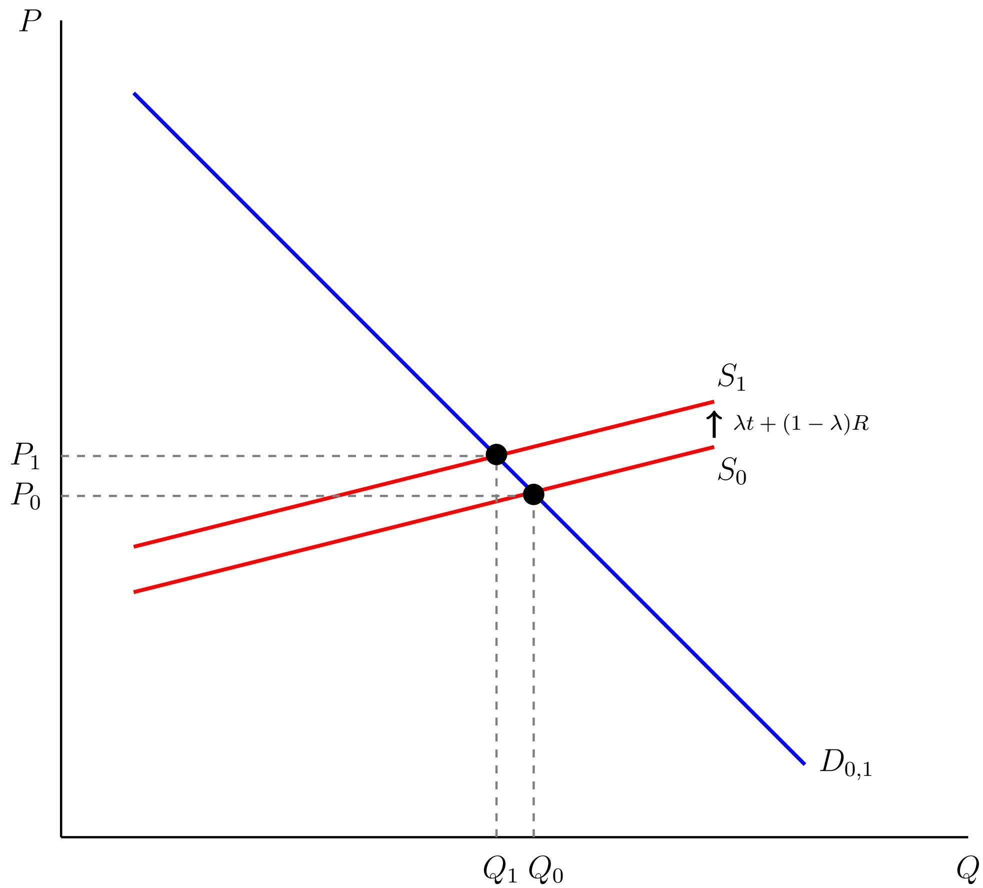
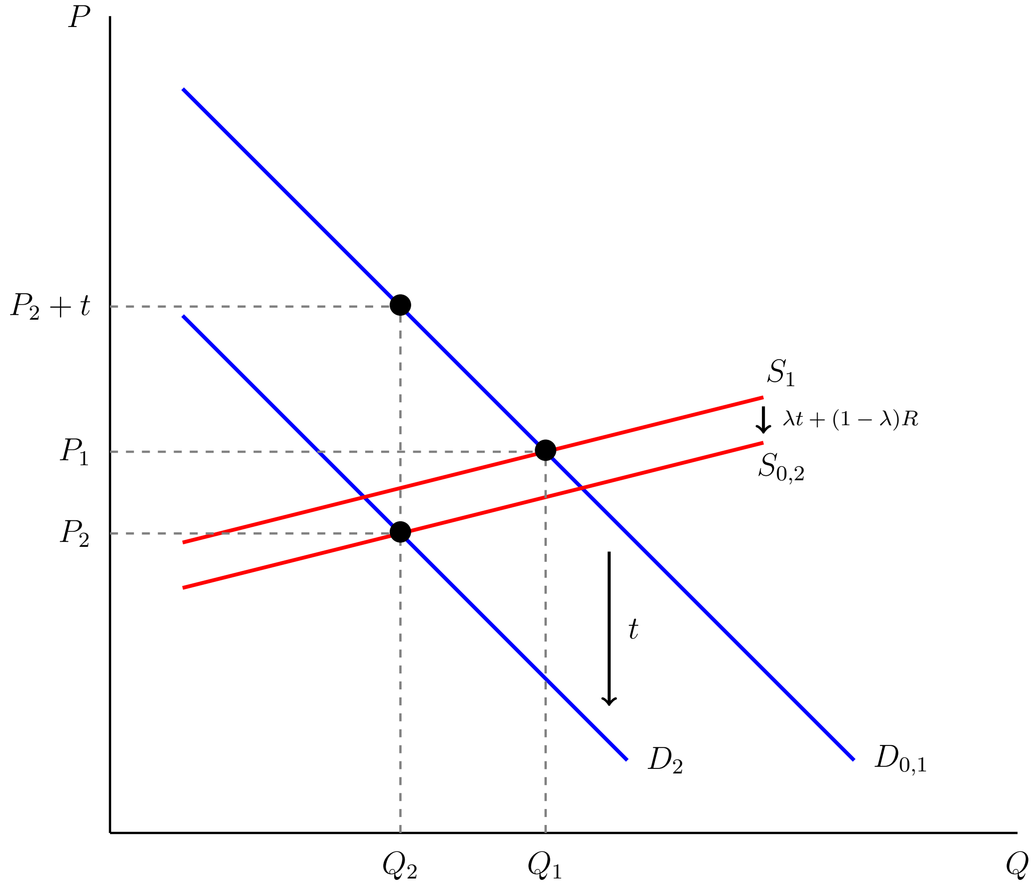
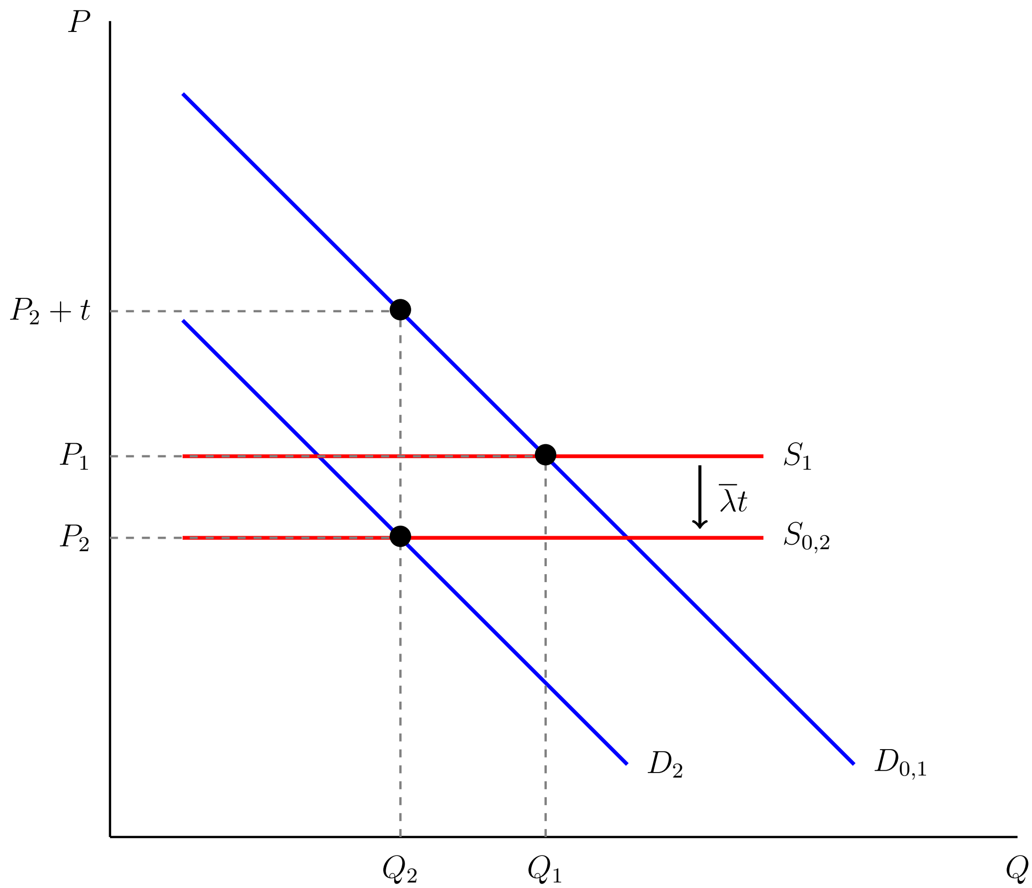


Figure 3: Impact of an Airbnb Tax Enforcement Agreement



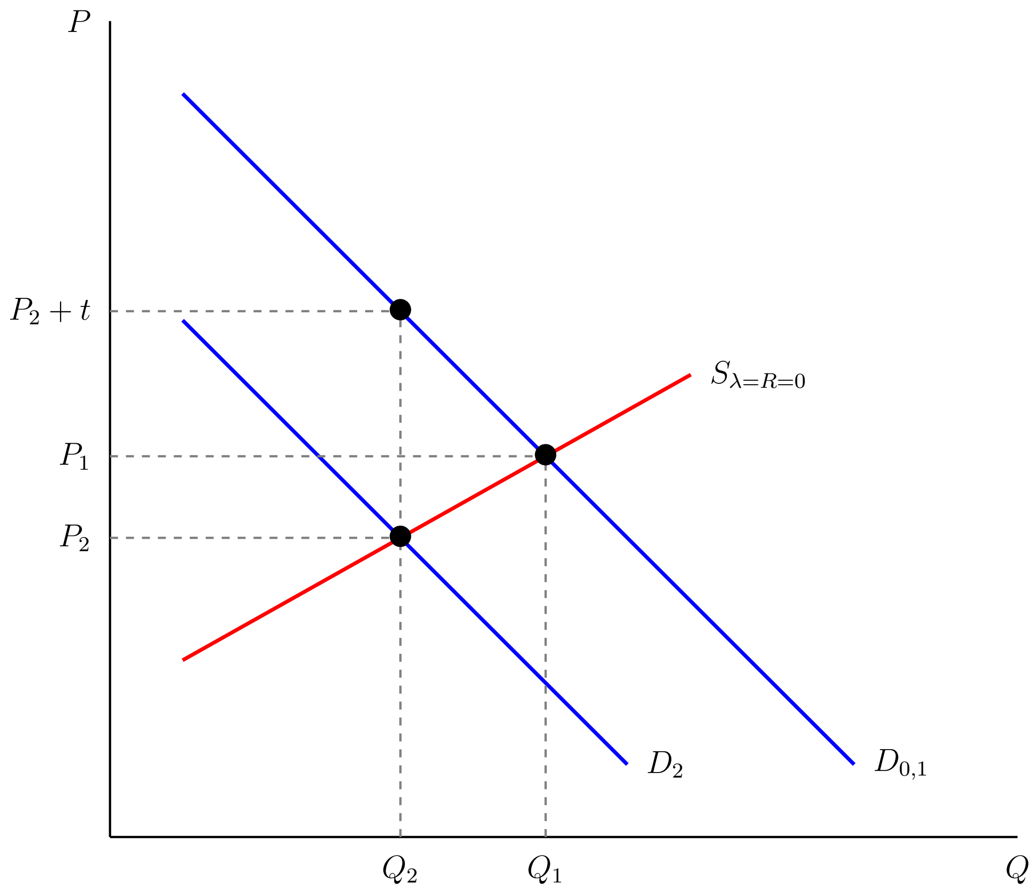
Note: Bold dots (•) represent observed equilibria.

Figure 4: Compliance Rate Upper Bound



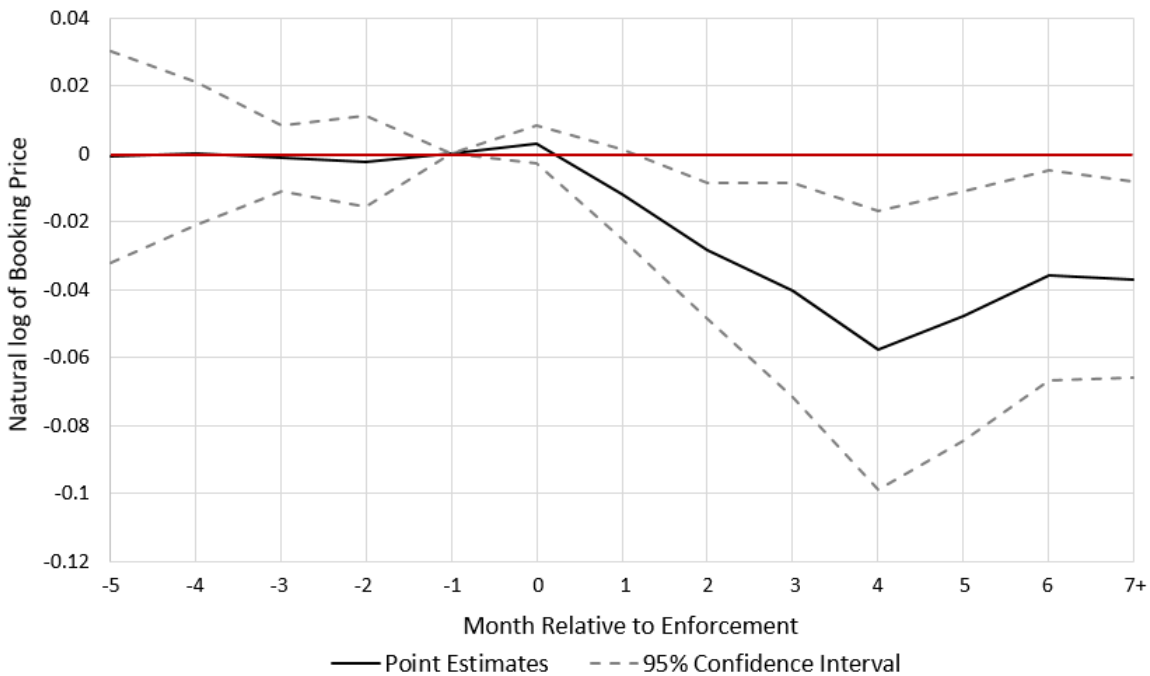
Note: Bold dots (•) represent observed equilibria.

Figure 5: Supply Elasticity Lower Bound



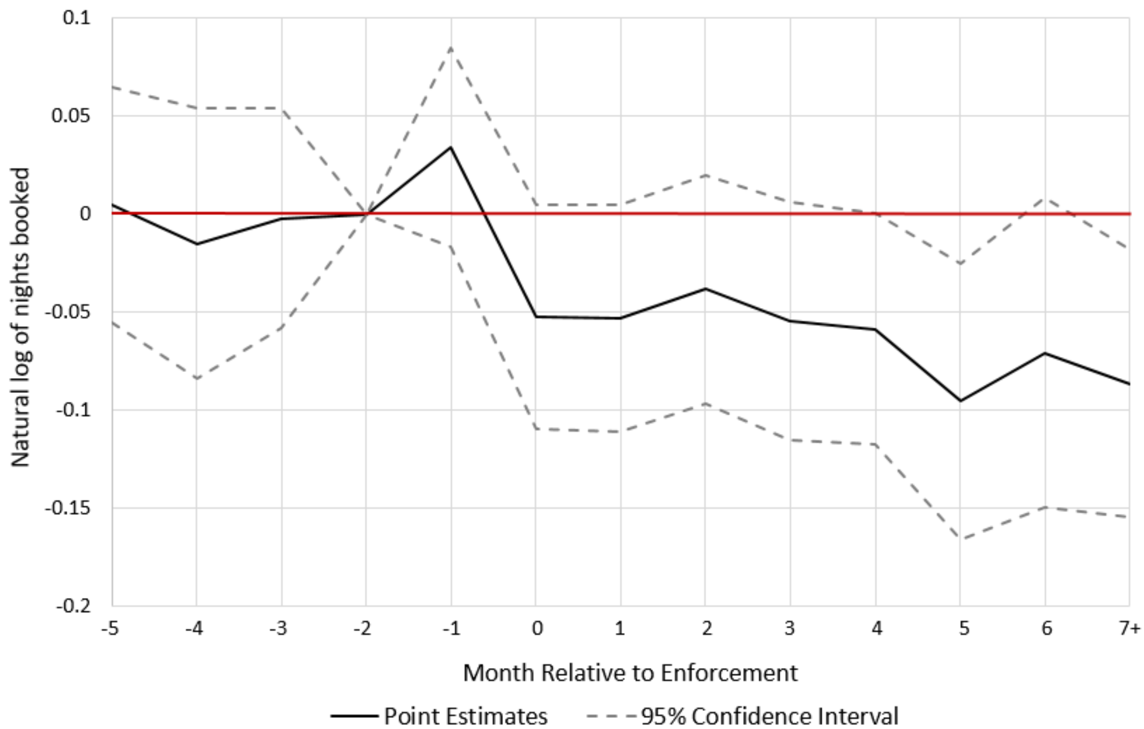
Note: Bold dots (•) represent observed equilibria.

Figure 6: Effect of Tax Enforcement on Log Booking Price



Notes: This figure presents the time-disaggregated estimated effect of enforcement agreements on the natural log of booking price. This approach interacts a binary treatment indicator with month relative to enforcement, and includes metro-month-year fixed effects as well as property fixed effects. As in our main set of estimates, this estimation sample excludes listings for shared rooms, properties that have >4 bedrooms, properties that have a >12 guest limit, or properties for which average asking price falls in bottom or top decile of the jurisdiction's distribution. Standard errors are robust to clustering at the tax jurisdiction level.

Figure 7: Effect of Tax Enforcement on Log Nights Booked



Notes: This figure presents the time-disaggregated estimated effect of enforcement agreements on the natural log of nights booked. This approach interacts a binary treatment indicator with month relative to enforcement, and includes metro-month-year fixed effects as well as property fixed effects. This figure omits month $k = -2$, instead of $k = -1$, so we can inspect whether renters appear to be strategically altering the timing of their bookings around enforcement. As in our main set of estimates, this estimation sample excludes listings for shared rooms, properties that have >4 bedrooms, properties that have a >12 guest limit, or properties for which average asking price falls in bottom or top decile of the jurisdiction's distribution. Standard errors are robust to clustering at the tax jurisdiction level.

Appendix A A Model of Imperfect Competition

Now suppose that hosts on Airbnb provide renters with differentiated listings and compete on price. For simplicity, suppose that each host is a single-unit lister. If host i complies with the tax, then a potential compliance cost ($C_i \geq 0$) exists for filing taxes. In addition, host i incurs a marginal cost c_i and a fixed cost F_i. Thus, the total profit for host i when complying is:

$$\Pi_i(\text{comply}) = (p_i - c_i - t)q(p_i, X_i; \mathbf{p}_{-i}, \mathbf{X}_{-i}) - F_i - C_i,^{36}$$

where p_i is price, X_i are the characteristics of unit i, $\mathbf{p}_{-i}$ is the vector of prices of competing units, and $\mathbf{X}_{-i}$ is the vector of characteristics of other units.[37]

If host i chooses to evade the tax, then they do not incur the compliance cost. However, evading hosts may face the risk of being caught and penalized. Let R_i denote the expected penalty associated with evading the tax. Thus, the total profit for host i when evading is:

$$\Pi_i(\text{evade}) = (p_i - c_i)q(p_i, X_i; \mathbf{p}_{-i}, \mathbf{X}_{-i}) - F_i - R_i.$$

To solve the pre-enforcement problem for host i, note that host i takes $X_i, \mathbf{p}_{-i}$, and $\mathbf{X}_{-i}$ as given when making pricing and compliance decisions. Thus, we first evaluate each profit maximization problem and then compare the profits from evading and complying at their respective optimal prices.

Solving the first-order conditions for profit maximization implies that:

$$p_i = \underbrace{c_i + \eta}_{\text{Marginal Cost}} + \underbrace{\frac{q(p_i)}{-q'(p_i)}}_{\text{Markup}}.$$

[36] Alternatively, for an ad valorem sales tax we have $(1-t)p_i$ instead of $p_i - t$. We use a unit tax for simplicity.

[37] This framework maps into a model of monopolistic competition by simply letting $\mathbf{p}_{-i}$ instead denote the pricing index corresponding to the average Airbnb market price.

Setting $\eta = t$ yields host i's optimal price when complying, p_i^C, and setting $\eta = 0$ yields host i's optimal price when evading (call it p_i^E). In equilibrium we have that $\Pi_j(p_j^E) \geq 0$ and $\Pi_j(p_j^E) \geq \Pi_j(p_j^C)$ for all j who evade, and we have that $\Pi_i(p_i^C) \geq 0$ and $\Pi_i(p_i^C) \geq \Pi_i(p_i^E)$ for all i who comply. Note that $p_i^E \in [p_i^C - t, p_i^C]$ as long as demand is not too convex.[38] Thus, if host i remits taxes, then some portion of the tax, σ_i, is passed through to renters. That is, the profit-maximizing price when complying is $\sigma_i t$ greater than the profit-maximizing price when evading: $p_i^C = p_i^E + \sigma_i t$.[39]

Next, consider how booking prices change with an Airbnb enforcement agreement that guarantees taxes are paid at the point of sale by renters. The profit-maximizing price set by a host that evades pre-enforcement falls by $(1-\sigma_j)t$, such that it equals the pre-enforcement tax-exclusive complier price $p_j^C - t$. The price renters pay for that host's property increases by $\sigma_j t$ to the pre-enforcement tax-inclusive complier price p_j^C. For compliers, neither the profit-maximizing prices they receive nor the prices renters pay change following an Airbnb enforcement agreement; there is only a change in who bears the statutory burden of the tax.

Altogether, with λ compliers and $1-\lambda$ evaders, the average decrease in the booking price paid to hosts, which is tax-inclusive before enforcement and tax-exclusive after enforcement, across all listings is given by:

$$\Delta p = \lambda t + (1-\lambda)(1-\sigma)t,$$

[38] That is, the markup is decreasing in p so that the complier bears some of the tax burden when $q''(p_i) < \frac{(q'(p_i))^2}{q(p_i)}$. Weyl and Fabinger (2013) show that pass-through can be greater than one if demand is sufficiently convex. In this case, a tax would increase the tax-exclusive price. We ignore this extreme possibility and focus on the case where pass-through, on average, is between zero and one.

[39] Because we maintain general demand functions, a closed-form solution for the pass-through rate cannot be reached. However, this pass-through rate is generated by the equilibrium pricing function above. Comparing $p_i^C = c_i + t + \frac{q(p_i^C)}{-q'(p_i^C)}$ to $p_i^E = c_i + \frac{q(p_i^E)}{-q'(p_i^E)}$ reveals how σ_i is determined. Clearly, the marginal cost when complying is larger. However, markup is smaller when complying because $\frac{q(p_i^C)}{-q'(p_i^C)} < \frac{q(p_i^E)}{-q'(p_i^E)}$ when $p_i^C > p_i^E$. Combined, these differences generate the pass-through rate $\sigma_i \in (0,1)$ such that $p_i^C = p_i^E + \sigma_i t$.

where $\sigma \in (0, 1)$ is the average pass-through rate. Solving for λ implies that

$$\lambda = \frac{\Delta p - (1 - \sigma)t}{\sigma t}.$$

Comparing this compliance rate to the proposed upper bound estimate, $\overline{\lambda}$ in Equation (1), we have that $\lambda < \overline{\lambda}$ if and only if $\sigma \in (0, 1)$.[40] Thus, the proposed upper bound on pre-enforcement compliance when hosts are price-takers, $\overline{\lambda}$ from Equation (1), is also an upper bound on pre-enforcement compliance in imperfectly competitive environments.

A.1 Entry and Exit

Now consider the case where an enforcement agreement results in hosts entering and exiting the market. After an enforcement agreement is implemented, marginal hosts are induced to enter if the pre-enforcement compliance costs (C_i) or the expected penalty for evading (R_i) is large enough. If marginal hosts enter post-enforcement, price competition generates downward pressure on prices. It is also possible that marginal evaders are no longer profitable after enforcement and exit the market. Let the net price effect from host exit be denoted by ϕ. In this case, the average change in booking price across all listings is given by:

$$\Delta p = \lambda t + (1 - \lambda)(1 - \sigma)t - \phi.$$

Solving for λ implies that

$$\lambda = \frac{\Delta p - (1 - \sigma)t + \phi}{\sigma t}.$$

Comparing this compliance rate to the proposed upper bound estimate, $\overline{\lambda}$ in Equation (1), we have that $\lambda < \overline{\lambda}$ if and only if:

$$\phi < (t - \Delta p)(1 - \sigma).$$

[40] When $\sigma = 1$ we have that $\lambda = \overline{\lambda}$. In addition, $\frac{\partial \lambda}{\partial \sigma} > 0$, which implies that $\lambda < \overline{\lambda}$ for all $\sigma \in (0, 1)$.

This shows that our estimate of $\overline{\lambda}$ is valid if net exit ($\phi > 0$) is not too large. In fact, we find in Section 5 that, if anything, enforcement has a net entry effect ($\phi < 0$).

Appendix B Compliance in the Case of Use Taxes

In many online markets, the statutory tax burden falls on consumers rather than producers even in the absence of enforcement agreements. For example, before the June 2018 Supreme Court decision in South Dakota v. Wayfair (585 U.S. ___, 2018), consumers in many states were obligated to self-report use taxes when purchasing goods from online retailers or platforms. After full enforcement is implemented by law or a collection agreement, consumers pay the applicable tax at the point of sale. In this example, unlike Airbnb, the effective statutory burden is imposed on the same side of the market (consumers) before and after enforcement. In this appendix, we show that researchers can estimate an upper bound on pre-enforcement compliance in this scenario as well. We also show that the price elasticity of supply is point identified, and that we can estimate a lower bound on the magnitude of the price elasticity of demand.

For simplicity, consider this case under the assumption that suppliers are price-takers. Suppose there are three periods. In period 0, there are no use tax obligations associated with online purchases. In the first period, individual hosts bear the burden of collecting and remitting applicable use taxes but are able to evade relatively easily. In the second period, the statutory burden again falls on consumers while evasion is no longer possible.

Consider first the consumers that comply with the tax as introduced in period 1. For these consumers, demand is given by $D^C(P+t)$ where P denotes the price paid to the seller and t denotes the tax remitted by the consumer. Now consider the consumers that evade taxes. The demand from evading consumers is given by $D^E(P+R)$ where $R \geq 0$ denotes the costs associated with evading. Suppose that the demand curves are linear, the mass of consumers is one, and $\lambda \in [0, 1]$ denotes the proportion of tax-compliant consumers in period 1. This implies that market demand is given by $D = (1-\lambda)D^E + \lambda D^C = D(P + \lambda t + (1-\lambda)R)$.

The first period equilibrium is given by the equilibrium tax-exclusive price, $P = P_1$, that satisfies $S(P) = D(P + \lambda t + (1-\lambda)R)$. Thus, the price paid by consumers in the first period is $P_1 + \lambda t + (1 - \lambda)R$ and the average price received by sellers is P_1. In the second period, the tax is automatically applied to each transaction at the point of sale. In this case, evasion is impossible. Thus, the second period equilibrium tax-exclusive price, $P = P_2$, satisfies $S(P) = D(P+t)$. In this case, consumers pay $P_2 + t$ and sellers receive P_2. This is displayed graphically in Figure 8, where D_0 is demand in period 0, D_1 is demand in period 1, and D_2 is demand in period 2.

If all consumers comply in the first period (i.e. $\lambda = 1$), then demand and the equilibrium price that sellers receive is the same across the periods 1 and 2: $D_1 = D_2$ and $P_1 = P_2$. However, when some consumers evade in the first period (i.e. $\lambda < 1$), then tax enforcement shifts demand further downward. This further reduces equilibrium quantity and the price received by sellers, and increases the average price paid by consumers.

When λ and R are unobserved, researchers can use the extreme case of perfectly elastic demand to derive an upper bound on pre-enforcement compliance. Figure 9 highlights that the largest possible shift in the demand curve from period 0 to 1 is the distance between P_1 and $P_2 + t$, which occurs only when demand is perfectly elastic. This implies that $\lambda t \leq P_2 + t - P_1$. Thus, one can estimate the following upper bound on pre-enforcement compliance λ:

$$\lambda \leq \frac{P_2 + t\text{–}P_1}{t} = \frac{t\text{–}\Delta p}{t} \equiv \hat{\lambda}. \tag{5}$$

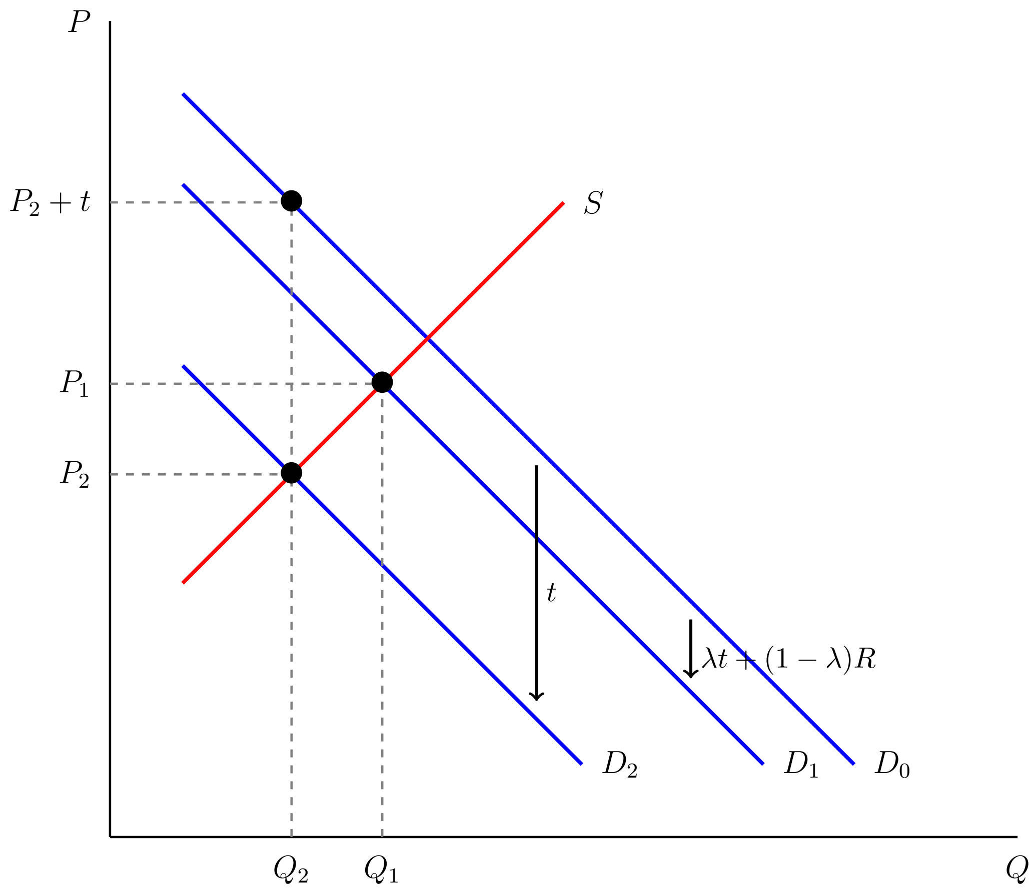
Note that this upper bound differs from the Airbnb case where the statutory burden shifts from hosts to consumers. Here, the compliance upper bound is such that:

$$\hat{\lambda} \equiv = \frac{t\text{–}\Delta p}{t} = 1 - \overline{\lambda}, \tag{6}$$

where $\overline{\lambda}$ is the upper bound from the Airbnb case. While the upper bounds differ depending on how enforcement affects the statutory burden of taxation, the fact that each upper bound is derived from the pass-through rate is consistent across contexts. This reinforces that the power of this approach is its simplicity, as it only requires the practitioner to observe the tax magnitude along with market prices under partial and full compliance.

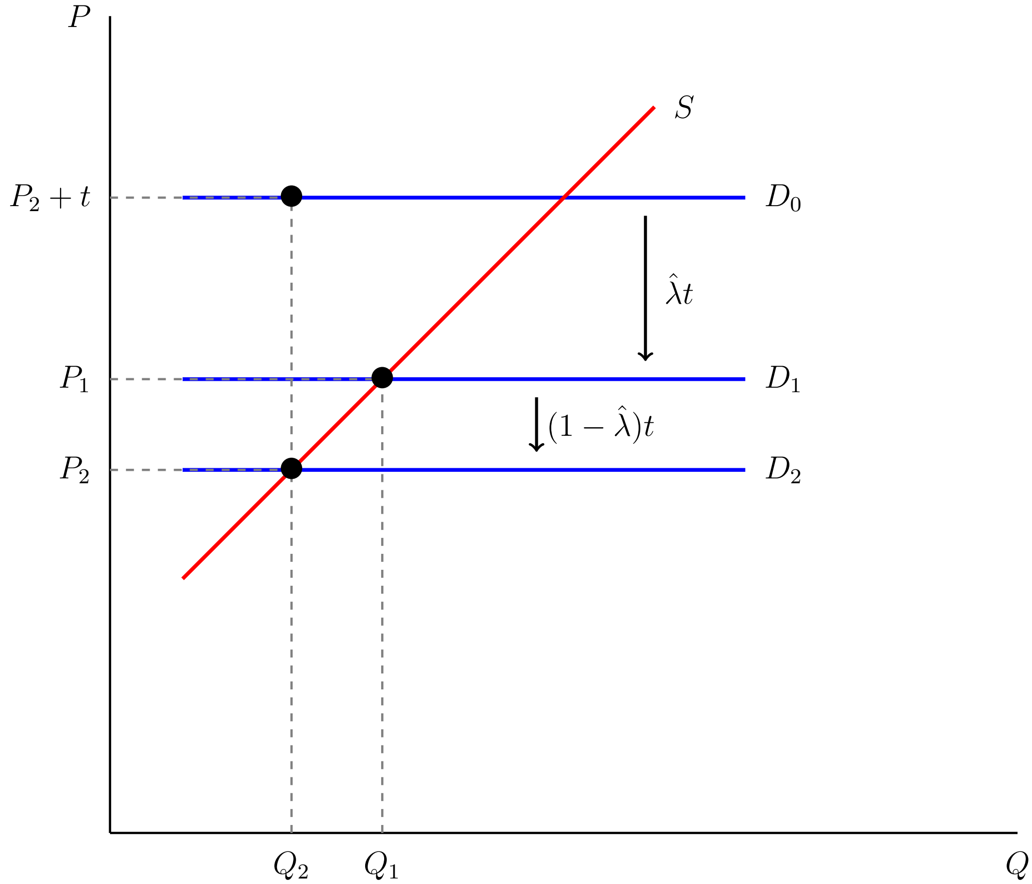
In the use tax case, the researcher can point identify the price elasticity of supply using the change in equilibrium between periods 1 and 2. The researcher can also derive a lower bound on the magnitude of the price elasticity of demand. That is, the price elasticity of demand cannot be less elastic than when $\lambda = 0$, as shown graphically in Figure 10. In this case, there is no shift in demand from period 0 to 1, meaning that tax enforcement in period 2 results in a downward demand shift by the full amount of the tax. Thus, we can trace out the steepest possible demand curve using the observed pre- and post-enforcement quantities and tax-inclusive prices to derive a lower bound on the magnitude of the price elasticity of demand.

Figure 8: Impact of Use Tax Enforcement



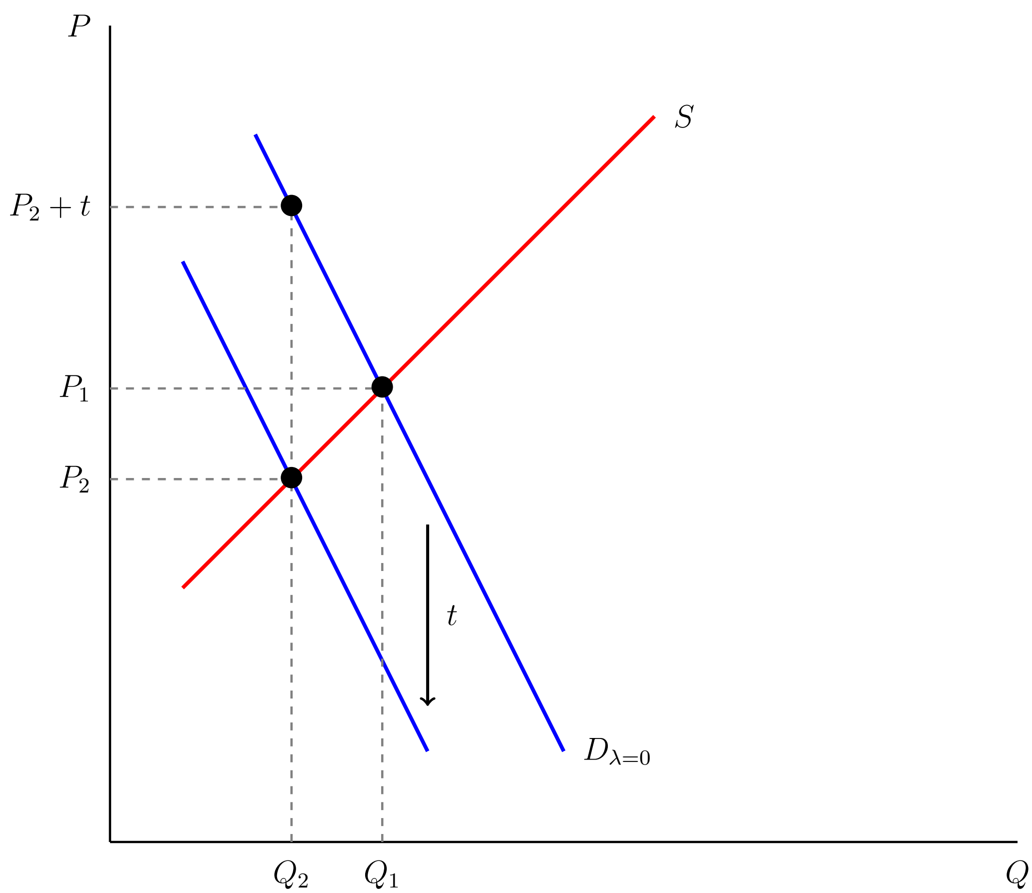
Note: Bold dots (•) represent observed equilibria.

Figure 9: The Use Tax Compliance Rate Upper Bound



Note: Bold dots (•) represent observed equilibria.

Figure 10: The Use Tax Demand Elasticity Upper Bound



Note: Bold dots (•) represent observed equilibria.

Appendix C Data Appendix - Sample Restrictions

Available upon request.