INFERRING TAX COMPLIANCE FROM PASS-THROUGH: EVIDENCE FROM AIRBNB TAX ENFORCEMENT AGREEMENTS

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Abstract—Tax enforcement is especially costly when market participants are difficult to observe. The benefits of enforcement depend crucially on pre-enforcement compliance. We derive an upper bound on pre-enforcement compliance from the pass-through of newly enforced taxes. Using data on Airbnb listings and the platform's voluntary collection agreements, we find that taxes are paid on, at most, 24% of Airbnb transactions prior to enforcement. We also find that demand for Airbnb listings is inelastic, driving three key insights: the tax burden falls disproportionately on renters, the excess burden is small, and tax enforcement is relatively ineffective at reducing local Airbnb activity.

I. Introduction

THE rapid rise of online marketplaces such as Amazon, eBay, Craigslist, and Airbnb has created contexts in which tax obligations are ambiguous or difficult to enforce. For example, before the recent June 2018 U.S. Supreme Court decision in *South Dakota v. Wayfair Inc*., states were unable to require online sellers lacking sufficient local presence (i.e., *nexus*) to collect and remit sales taxes.[1] Because state and local government agents cannot fully observe key details of online transactions, they often rely on residents to self-report the applicable taxes owed.[2] Naturally, this enables individual market participants, some of whom may simply be unaware of their tax obligations, to evade with low probability of detection.[3]

In some cases where nexus cannot be established, policymakers have worked to shift the burden of tax collection and remittance to online platforms and retailers through the use of voluntary collection agreements (VCAs).[4] Online companies may be incentivized to enter VCAs to avoid facing strict regulations, such as the ban New York City imposed 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 show that an upper bound on pre-enforcement tax compliance can be obtained by estimating the change in price after a tax becomes fully enforced. We apply this approach to Airbnb, which offers a particularly attractive setting. Jurisdictions with legislated taxes on hotels and other short-term housing rentals, but no formal enforcement agreements, must rely on hosts (suppliers) to collect and remit the applicable taxes. If jurisdictions wish to locate and penalize evaders, they incur large enforcement costs.[5] Since 2014, however, Airbnb has entered into over 275 VCAs with jurisdictions across the United States to enforce sales and hotel taxes.[6] Once an agreement is implemented, Airbnb becomes the tax remitter and collects taxes on every applicable transaction from renters (consumers) at the point of sale, which increases compliance to 100% among transactions made on the platform. Importantly, the enforced taxes are included in the price presented on each property's main page (see figure D1 in the online appendix), which mitigates the concern that renters may underreact due to lack of salience (Chetty, Looney, & Kroft, 2009). In addition to this application, we show that our approach generalizes to contexts in which a change in statutory incidence does not accompany enforcement, such as the "Amazon tax" and new laws enabled by the *South Dakota v. Wayfair Inc*. decision.

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

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[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, Fox, and Luna (2009) conservatively projected that forgone e-commerce state tax revenue would be $11.4 billion in 2012 alone.

[4] In some cases, policymakers have been able to establish nexus and compel online companies to collect and remit sales and use taxes. For example, many states have 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, Ben-David, & Park, 2018).

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

[6] 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/.

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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 prior to full enforcement, suggesting that tax jurisdictions can increase compliance substantially by entering a VCA. We show that this result is robust across specifications and rule out potential threats to the validity of our estimated upper bound.

We use the same approach to find that the enforcement of a 10% tax reduces nights booked by 3.6%. This supports our finding of 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. Combining our estimated price and quantity effects, we find that the elasticity of demand is relatively low (-0.48). This suggests that Airbnb renters do not treat hotels and other short-term rental options as close substitutes. Moreover, it suggests that taxing Airbnb is not an effective policy lever for those seeking to reduce Airbnb market activity in a given area.

If we follow Farronato and Fradkin (2018) and assume that Airbnb hosts are price takers, we are able to derive several additional insights. First, we adapt an intuitive result explained nicely by Zoutman, Gavrilova, and Hopland (2018) to calculate that the price elasticity of supply is at least 1.5. This means that hosts are relatively price sensitive and renters bear a disproportionately large share of the economic tax burden. This is consistent with Farronato and Fradkin (2018), who estimate price elasticity of supply to be 2.16. 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 only 7%.[7] Second, we use back-of-the-envelope calculations to show that enforcement increases tax revenue by at least \$69 per property per month. Multiplying \$69 by 2,245, the average number of properties in a treated tax jurisdiction, yields a monthly increase in tax revenue of at least \$155,000 from properties in the jurisdiction. Moreover, we find that enforcement appears to impose a very small efficiency cost on the local market of \$0.03 per dollar of additional revenue.

A. *Related Literature*

This paper is closely related to the literature focused on detecting and estimating tax evasion.[8] One approach taken in the literature involves comparing reported and actual aggregate quantities to infer evasion.[9] Another method exploits the IRS Taxpayer Compliance Measurement Programs, which provide data on compliance from randomized audits (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 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.[10]

In particular, our work contributes to research studying compliance across different tax regimes. Slemrod (2008) and Kopczuk et al. (2016) are especially relevant: they show that the textbook principle of tax collection invariance can fail in the presence of evasion. Specifically, Kopczuk et al. (2016) 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 into 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) find that while an increase in the prevalence of Airbnb reduces hotel revenue, at least 70% of Airbnb bookings would not have resulted in hotel bookings in the absence of Airbnb. This is consistent with earlier work by Zervas, Proserpio, and Byers (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 makes residential housing less affordable (Barron, Kung, & Proserpio, 2021).

Finally, our work contributes to the growing literature on the relationship between taxes, tax enforcement, and online

[7]To elaborate, our approach yields an estimated upper bound on pre-enforcement compliance of 24%, which occurs when supply is perfectly elastic. If, however, we take 2.16 as the true price elasticity of supply, we can combine it with our estimated price effect to infer a point estimate on pre-enforcement compliance of 7%. See section VE for detailed calculations.

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

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

[10]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 data set. This enables us to make stronger claims on tax compliance rates and incidence and to provide deeper insight into price elasticity of supply and demand as well as welfare implications.

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 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 (2009b), Anderson et al. (2010), Einav et al. (2014), and Baugh et al. (2018).

II. 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 short-term rental 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 were collected by AirDNA, a third-party source that frequently scrapes property, availability, host, and review information from the website.[11]

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.[12] The complete data set 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 the top 105 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 short-term rental 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.[13]

We then aggregate our property-day data to the property-month level and supplement them with hand-collected data on implementation dates and tax rates for all tax enforcement agreements between Airbnb and the relevant state and local governments. Airbnb publishes the locations and tax rates of all their tax enforcement agreements, which enables us to search for primary and secondary sources (i.e., local government websites and news articles) detailing the timing of implementation.[14] Using these sources, we are able to confirm treatment dates for all but one location (Palo Alto, CA). 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 potential confounders, we drop treatment and control jurisdictions with regulatory changes and changes in self-enforcement efforts during the sample period. Also, since our preferred specification includes metro-month-year fixed effects (see section IV), we only keep jurisdictions if they are part of metros containing within-metro-month-year treatment variation.[15] 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 ranging from February 2015 through June 2017. Thus, we have between 6 and 34 months (18.7 on average) of preperiod data and 3 to 31 months (18.3 on average) of postperiod data for treated jurisdictions. 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 nonzero tax, the average is 11.2%. Of the 38 treated jurisdictions, 15 experience one increase in the enforced tax rate during the postperiod, and one experiences two such increases. In appendix C, we discuss in detail which jurisdictions we keep and drop and provide the relevant justifications. We also present more 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, 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 tax exclusive after the agreement is implemented. For brevity, we also refer to this

[11] This is in contrast to some papers that use administrative data from Airbnb, such as Jaffe et al. (2018) and Farronato and Fradkin (2018). AirDNA's website is https://www.airdna.co/.

[12] 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, DC.

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

[14] For details on current enforcement agreements, see https://www.airbnb.com/help/topic/1236/occupancy-tax-collection-and-remittance. For two examples of relevant news articles, see the following links that discuss the timing of enforcement in Washington, DC, and Salt Lake City: https://www.cnbc.com/2018/12/12/los-angeles-passes-regulation-targeting-airbnb-rental-hosts.html and https://www.deseret.com/2017/2/14/20606190/utah-airbnb-hosts-earned-35-5-million-welcomed-246-000-people-in-2016.

[15] To retain a few metros, we introduce within-metro-month-year treatment variation by including 13 jurisdictions that fall outside of the largest 105. See appendix C for more details.

TABLE 1.—SUMMARY STATISTICS

	Mean	Std. Dev.	25th Percentile	Median	75th Percentile	Observations
	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

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

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 6.05 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 in 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.[16] The primary concern with this approach is that we may incorrectly infer that a booking occurs and thus overmeasure 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 bias our estimates only 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 overmeasure nights booked during such periods. However, in our preferred specification discussed in section IV, 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 and 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

[16]We purchased the scraped data from AirDNA. For roughly the first year of our sample, Airbnb disclosed whether each unavailable date was booked or made unavailable (i.e., blocked) by the host. Since Airbnb stopped disclosing this distinction, AirDNA started predicting which calendar changes are bookings and which are blocked by training an algorithm on the data they have from the full-information period. We use these data to generate our variables measuring nights booked and property availability.

business-ready and 18% of properties are listed by superhosts.[17]

III. Conceptual Framework

In this section, we illustrate the impact of tax enforcement on the short-term rental 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.[18] 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 toward Airbnb, which collects and remits all applicable taxes from renters at the point of sale. Neither hosts nor renters can evade taxes when booking through the website under this regime.[19]

Consider first the hosts who 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.[20] Next, consider hosts who 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 the supply curves are linear, the mass of hosts is 1, and let $\lambda \in [0, 1]$ denote the proportion of tax-compliant listings. This implies that the market supply of accommodations is given by $S = (1-\lambda)S^E + \lambda S^C = S(P - \lambda t - (1-\lambda)R)$.[21] 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 1a and 1b. 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 1a. 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 1b. 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, given that renters are unable to evade and assuming the tax is salient, demand falls by the full magnitude of the tax.

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.[22] This implies that enforcement increases the average price renters pay, 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 1c, 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:

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

[17]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.

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

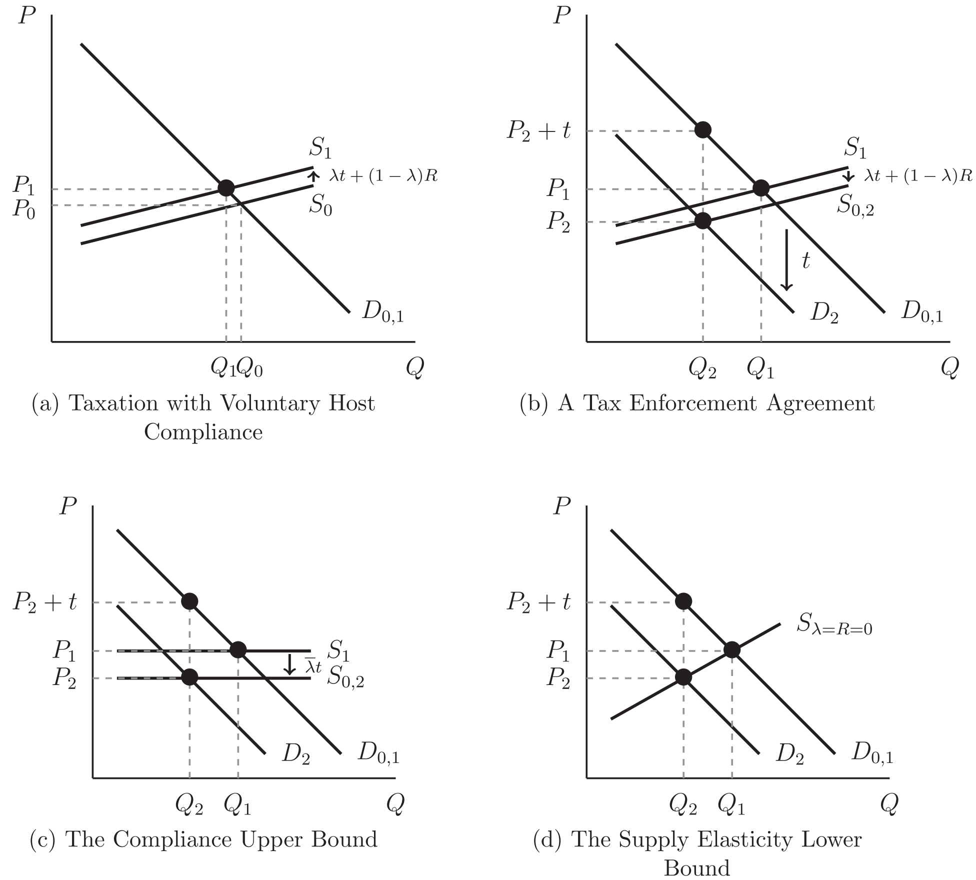
[19]While it is possible that noncompliance persists after an agreement if buyers and sellers can circumvent the website to conduct business off the platform, several mechanisms in place strongly deter such behavior. First, Airbnb blocks the ability for hosts and renters to share contact information until a booking is made. Second, Airbnb warns its customers about phishing and the risks of sending payments to hosts outside the platform. Third, hosts and renters forgo Airbnb's payment protection and legal protection if they subvert the website booking process.

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

[21]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 $S = (1-\lambda)S^E + \lambda S^C = S(P - \lambda t - \lambda C - (1-\lambda)R)$.

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

FIGURE 1.—TAX EVASION AND ENFORCEMENT ON THE AIRBNB MARKETPLACE



(a) Taxation with Voluntary Host Compliance

(b) A Tax Enforcement Agreement

(c) The Compliance Upper Bound

(d) The Supply Elasticity Lower Bound

Bold dots represent observed equilibria.

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 that the larger the costs associated with evading are, the more conservative the estimated upper bound will be. Recall that in this model, we implicitly assume that the taxes are salient. Our approach may underestimate the conservative upper bound if, due to less-than-full salience, willingness to pay does not fall by the full amount of the tax.

We also consider the other extreme, in which there is no compliance or risk of evading (i.e., $\lambda = R = 0$), to infer a lower bound on the elasticity of supply. This case is depicted in figure 1d. 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 1d, 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 $\overline{\lambda}$.

IV. Estimation

Our primary goals are to obtain an upper bound on pre-enforcement tax compliance and provide insight into 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 0 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.[23] 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 idiosyncratic demand shocks.[24] Equation (2) represents our preferred specification, which includes metro-month-year fixed effects δ_{mt}.[25]

The parameter of interest, γ, represents the percent change in Y associated with a 1% increase in $(1 + \tau)$, which closely approximates a 1 percentage point increase in the tax rate enforced through the platform. As long as supply and demand have some nonzero 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 pretreatment differences in the outcomes of interest between the treatment and control jurisdictions (see table D1 in appendix D, which also includes sample averages by treatment status). To test these differences, we use only pretreatment property-month observations and regress each outcome variable of interest on an indicator for whether a property is in an eventually treated jurisdiction. In the first set of tests, we condition on month-year fixed effects. In the second set, we condition on 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 differences between treatment and control jurisdictions.[26] 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. This is a relatively precise 0, 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.

Using the same approach, we also test for pretreatment differences in market characteristics that may be correlated with prices or pass-through rates using jurisdiction-month level aggregates. These characteristics include the number of properties listed, total supply (i.e., total nights available), nights booked, a measure of excess capacity (proportion of properties booked at least once), and the booking price distribution (25th percentile, median, and 75th percentile). We present the test results along with sample means and standard deviations in table D2 in appendix D. We find evidence that treated jurisdictions are larger on average, implying a larger number of properties, nights available, and bookings. However, because we include property and tax jurisdiction fixed effects in our analyses, the primary threat to the validity of difference-in-differences is differential rates of change between the treatment and control groups over time. Thus, for all these characteristics, we also test for differential changes in characteristics over the pretreatment months. When we condition on metro-month-year fixed effects, as we do in our preferred regression specifications, we find no statistically significant differences between treated and control jurisdictions at all.

Next, we test for the presence of differential pretrends 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:

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

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

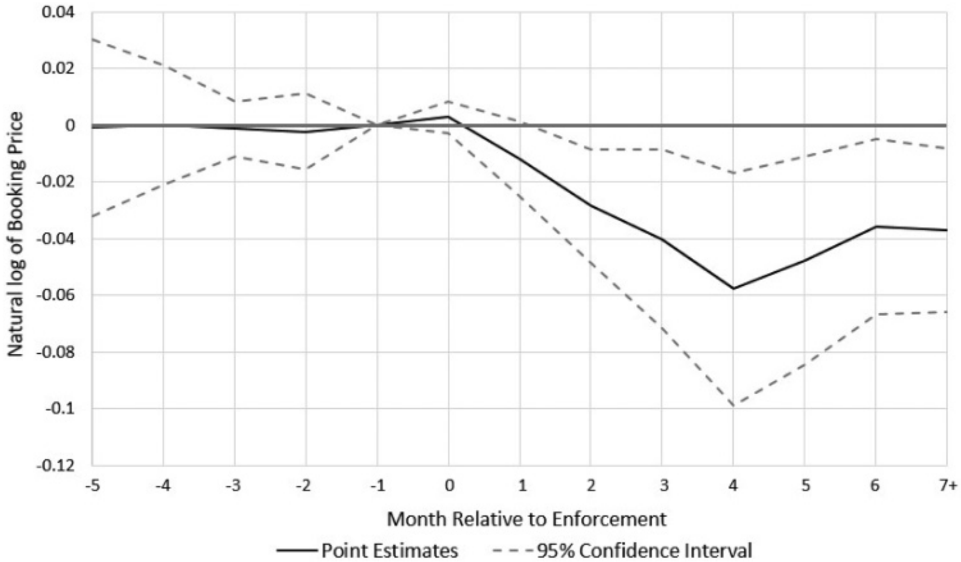
[23] See, for example, Marion and Muehlegger (2008) and Kopczuk et al. (2016).

[24] 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.

[25] 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 or omitted jurisdiction-specific characteristics and control for time-invariant observed property characteristics, including number of bedrooms, bathrooms, and maximum guests.

[26] Note that we cannot condition on tax jurisdiction or property fixed effects in these tests because the indicator for eventual treatment does not vary within jurisdiction or property.

FIGURE 2.—EFFECT OF TAX ENFORCEMENT ON LOG BOOKING PRICE



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 more than 4 bedrooms, properties that have a guest limit of more than twelve, and properties for which average asking price falls in the bottom or top decile of the jurisdiction's distribution. Standard errors are robust to clustering at the tax jurisdiction level.

variables of interest k months relative to the enforcement. For values $k < -1$, the coefficients γ_k test for the presence of pretrends. In practice, we collapse periods more than seven 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 2 shows that there is no visual or statistically significant evidence of a pretrend 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 3 also shows no evidence of a pretrend 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 VB, and find that strategic behavior does not appear to undermine our central estimates.

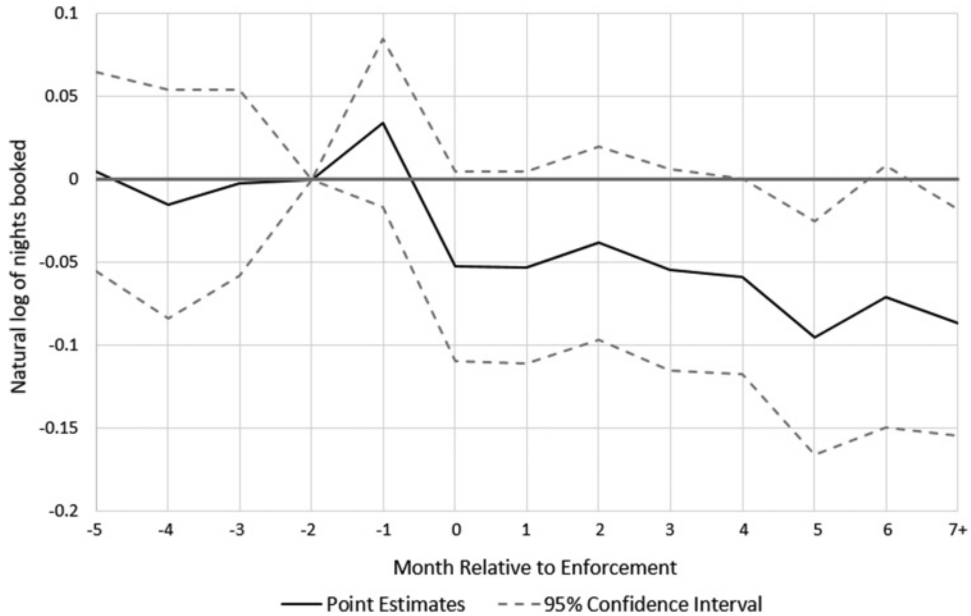
V. Results

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.

A. *Main Results*

Before discussing our main results that use differences in the size of the tax, we summarize the results using a traditional difference-in-differences framework. This is identical to equation (2), except we replace the treatment variable $\ln(1 + \tau_{jmt})$ with a binary indicator equal to 1 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 2. In our preferred specification (column 1), 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 28.6% of the enforced tax. Similarly, we estimate that the number of nights booked decreases by about 4.5%, suggesting that a 1 percentage point increase in the enforced tax rate reduces nights booked by roughly 0.4%.

FIGURE 3.—EFFECT OF TAX ENFORCEMENT ON LOG NIGHTS BOOKED



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 more than 4 bedrooms, properties that have a guest limit of more than twelve, and properties for which average asking price falls in the bottom or top decile of the jurisdiction's distribution. Standard errors are robust to clustering at the tax jurisdiction level.

TABLE 2.—TAX ENFORCEMENT, BOOKING PRICE, AND BOOKINGS: STANDARD DIFFERENCE-IN-DIFFERENCES SPECIFICATION

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

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 property fixed effects and metro-month-year fixed effects. Column 2 includes property fixed effects and county-month-year fixed effects. Column 3 includes property fixed effects and month-year fixed effects. Columns 4 to 6 repeat the three specifications replacing property fixed effects with tax jurisdiction fixed effects and including controls for property-level characteristics. The estimation sample excludes listings for shared rooms, properties that have more than 4 bedrooms, properties that have a guest limit of twelve, or properties for which average asking price falls in the 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. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.10$.

Panel A of table 3 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 1 percentage point increase in the enforced tax rate. Columns 1 through 3 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 results presented in columns 1 and 2 come from specifications allowing for location-time-specific idiosyncrasies. The column 1 specification includes metro-specific month-year fixed effects, while the column 2 specification includes county-specific month-year fixed effects. These estimates are −0.240 and −0.332, respectively, and both are 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.[27] The third column presents the estimate when we control for a location-invariant flexible time trend using

[27] 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.

TABLE 3.—TAX ENFORCEMENT, BOOKING PRICE, AND BOOKINGS: DIFFERENCES-IN-DIFFERENCES SPECIFICATION EXPLOITING TAX RATE VARIATION

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

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 property fixed effects and metro-month-year fixed effects. Column 2 includes property fixed effects and county-month-year fixed effects. Column 3 includes property fixed effects and month-year fixed effects. Columns 4 to 6 repeat the three specifications replacing property fixed effects with tax jurisdiction fixed effects and including controls for property-level characteristics. The estimation sample excludes listings for shared rooms, properties that have more than four bedrooms, properties that have a guest limit of twelve, or properties for which average asking price falls in the 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. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.10$.

month-year fixed effects. The estimated effect is −0.196 and is significant at the 5% level. We consider this a naive estimate because it does not control for idiosyncratic shocks, differences in seasonality, or differences in growth across location.

In columns 4 through 6 of table 3, 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.[28] The results presented in columns 4 and 5 come from specifications controlling for metro-month-year fixed effects and county-month-year fixed effects. The estimates are −0.217 and −0.229, respectively, and both are statistically significant at the 1% level. The last 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. 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.[29]

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.

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

$$\overline{\lambda} \equiv \frac{p_1 - p_2}{t} = \frac{\Delta p}{t} = \frac{\Delta p/p_1}{t/p_1} \approx \frac{\Delta p/p_1}{\tau} \approx \frac{\Delta \ln(p)}{\Delta \ln(1+\tau)} = -\gamma_P.^{30} \quad (4)$$

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 3. This row of results reflects the percentage change in nights booked associated with a 1 percentage point increase in the enforced tax rate,

[28] 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.

[29] 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 D3, 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.

[30] 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 0. Thus, $\tau = 0.01 = \Delta\tau$, which is approximately equal to $\Delta \ln(1+\tau) = \ln(1.01) - \ln(1) = 0.00995$.

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.[31] Focusing on our preferred specification (column 1), we estimate that a 1 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 any contemporaneous positive supply-side response that might exist, 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: $\epsilon_{demand} = \gamma_Q/\gamma_{P+\tau} = \frac{-0.361}{0.760} = -0.48$.[32]

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 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.[33] 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.[34]

B. *Robustness Checks*

We now 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 D3 in appendix D 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 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 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% 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 not statistically distinguishable from our preferred estimate.

Finally, in columns 6 and 7 of table D3, we compare two additional specifications with our main estimates on booking

[31]Ideally, we could decompose the effect on nights booked into intensive and extensive margin effects. However, determining the number of reservations and nights booked per reservation using scraped data relies on inferring that back-to-back newly booked nights are part of a single reservation unless AirDNA can distinguish multiple reservations from review data, which may introduce measurement error. When we estimate the extensive margin effect, we find a statistically insignificant negative effect on number of reservations that is roughly half the size of our preferred estimate of the effect on nights booked.

[32]Note that if the tax is not fully salient, this calculation will tend to underestimate the true price elasticity of demand (Ellison & Ellison, 2009a; Blake et al., 2021).

[33]These arguments are discussed in section III and displayed graphically in figures 1b through 1d.

[34]This hints at a natural extension, presented in section VC, where we estimate heterogeneity by type of rental unit.

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 of 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.

In tables D4 and D5 in appendix D, we present the robustness of our central results to two additional concerns. The first concern is that our low-compliance estimate is a result of hosts operating in a legal gray area, which could limit the generalizability of our results. To address this, we reestimate our main results using a restricted sample of jurisdictions where hosts' tax obligations were clear even in the absence of an enforcement agreement.[35] Using this restricted sample, the estimated effect on booking price is −0.234. The estimated effect on nights booked is −0.270, which is slightly smaller and no longer statistically significant, but is not statistically distinguishable from our preferred estimate of −0.361. The second concern is that tax-induced substitution across jurisdictions within metro could lead us to overestimate the negative effects of enforcement agreements on price and quantity. This would mean that our compliance upper bound is too conservative and that we are overestimating the price elasticity of demand. We minimize the threat of such spillover effects by reestimating our main results using a restricted sample that includes only the largest jurisdiction from each metro area. We find that the price and quantity effects from the restricted sample are very similar in magnitude to the analogous estimates from table 3, suggesting that our results are not exaggerated by spillover effects.[36]

[35]To assess clarity of tax obligations, we searched for existing legislation, policy memos, local news articles, or other government documents providing guidance to Airbnb hosts. We dropped jurisdictions for which we could not find evidence that Airbnb hosts' tax obligations were clear. These jurisdictions are Phoenix, AZ; Scottsdale, AZ; Salt Lake City, UT; New Orleans, LA; Hoboken, NJ; Tempe, AZ; Huntington Beach, CA; Mesa, AZ; Fremont, CA; Weehawken, NJ; Culver City, CA; Sandy, UT; Costa Mesa, CA; Lakewood, CO; Bethesda, MD; Cleveland Heights, OH; Silver Spring, MD; Newark, NJ; West New York, NJ; Millcreek, UT; Lakewood, OH; Golden, CO; and Metairie, LA. Tax obligations in Jersey City, NJ; and San Diego, CA, were clarified in our sample period, so we dropped the preclarification observations in those two jurisdictions.

[36]Because we cannot use metro-month-year or county-month-year fixed effects when restricting the sample in this way, the estimates presented in table D5 are most comparable to those in column 3 of table 3.

C. *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 into 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 D6 in appendix D 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 or 0, 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 D6 present results that allow for heterogeneity by asking price. 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.

D. *Supply Responses*

In this section, we explore whether supply-side responses to tax enforcement are consistent with our main findings.

TABLE 4.—SUPPLIER RESPONSES TO TAX AGREEMENTS

	Panel A: Property-level Supply Estimates			
	ln(Asking Price)	ln(1 + Nights Available)	Entry	Exit
Mean of DV			[0.059]	[0.021]
ln(1 + tax)	−0.090*	−0.345*	0.061**	0.002
	(0.047)	(0.187)	(0.028)	(0.009)
Observations	1,987,813	2,586,260	2,450,458	2,450,458
	Panel B: Jurisdiction-level Supply Estimates			
	Entry	Exit	Net Entry	Net Entry as % of Properties
Mean of DV	[68.59]	[23.57]	[45.02]	[0.046]
ln(1 + tax)	6.274	11.235*	−4.961	0.002
	(7.138)	(5.710)	(4.414)	(0.003)
Observations	2,196	2,196	2,196	2,196

Estimates of supply-side responses. Estimates in panel A include metro-month-year fixed effects and property fixed effects. ln(Ask P) is the ln of the asking price. *Nights Available* is the number of nights the listing was available. *Entry* is a dummy variable equal to 1 in the first month a property appears in our sample. *Exit* is a dummy variable equal to 1 in the last month that a property is listed on the site. In panel B, we aggregate to the jurisdiction-month level. We include jurisdiction fixed effects and metro-month-year fixed effects. Entry measures the number of new listings added to the site in a given jurisdiction-month-year. Exit measures the number of listings removed from the site in a given jurisdiction-month-year. *Net Entry as % of Properties* is measured as *Net Entry* divided by the number of properties listed in the previous month. The Entry and Exit samples omit the first and last month of our sample period. We apply the same sample restrictions as in the main sample. Standard errors are robust to clustering at the tax jurisdiction level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.10$.

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.[37]

In column 1 of table 4 (panel A), 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 1 percentage point increase in the enforced tax rate reduces hosts' asking prices by an average of 0.09%, 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 in panel A of table 4, we present the estimated effect of enforcement on the number of nights available per property-month from our preferred specification. We find that a 1 percentage point increase in the enforced tax rate reduces nights available per month by 0.345%, or 0.07 nights relative to the mean of 19.7. While one may be concerned that reduced availability is contributing to the negative effect of tax enforcement on nights booked, the large excess capacity present in the market suggests that this is not the case. In particular, the average number of nights booked per property-month (6.05) is less than a third of the number of nights available per property-month (19.7). Moreover, looking at the daily-level data by tax jurisdiction, the 75th percentile of the proportion of properties booked in a given day in a given jurisdiction is 35%, the 99th percentile is 61%, and the highest observed value is 83%.

Then 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. In column 3 (panel A), we find that a 1 percentage point increase in the enforced tax 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.[38] In column 4 (panel A), we find a relatively precisely estimated null effect on exit.

Next, we estimate entry and exit effects at the jurisdiction-month level. The outcomes in the first two columns of panel B are the number of entries and exits. Here, we find statistically insignificant evidence of increased entry and marginally statistically significant evidence of increased exit: enforcing a 10% tax increases the number of new listings by 0.6 per jurisdiction-month and the number of listings that are removed by 1.1 per jurisdiction-month. While these estimates suggest that the net effect on supply along the extensive margin is negative (−0.5, as shown in column 3 of panel B), the estimate is not statistically significant at conventional levels. It also appears to be economically insignificant considering that in our main estimation sample, 1,118 properties are listed in a given jurisdiction-month on average. To address the concern that the distribution of listing flows across jurisdictions may be positively skewed, we also estimate the effect on the ratio of net entry to the number of listings in

[37]See our treatment of entry and exit in appendix A for details.

[38]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 or determine exit among properties that are present in the final month of our data.

the previous month. The estimate presented in column 4, which is not statistically significant at conventional levels, suggests that enforcing a 10% tax increases the rate of net entry by 0.02 percentage points (or $0.0002 \times 1{,}118 = 0.22$ new listings per jurisdiction-month). Overall, the very small estimates and weak statistical significance suggest that the enforcement agreements have no meaningful negative effect on supply along the extensive margin.

E. *Welfare Implications*

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 short-term rental 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 associated deadweight loss (DWL).

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 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.[39] Given that the average number of listings per jurisdiction-month is 2,245 across the posttreatment 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), $\lambda = 0.07$.[40]

The first step is to derive a linear approximation of the demand curve for the average 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.[41] 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 D7 in appendix D, 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 0 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.[42] This scenario yields a lower bound on the DWL created by taxing

[39] 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.

[40] To calculate this implied value of λ, we must calculate the portion of the estimated price effect attributable to a shift rather than a movement along the supply curve where the "true" elasticity is 2.16. Figure 1b illustrates that λt is less than or equal to the difference between the actual effect of enforcement on price and the corresponding change in price from moving along the pre-enforcement supply curve by ΔQ. Both price changes can be calculated as movements along alternative supply curves with different elasticities. For the former, we use our supply elasticity lower bound of 1.5 (the implied elasticity when $\lambda = 0$; see figure 1d). For the latter, we use the 2.16 estimate. Then, rearranging the elasticity formula ($\epsilon_s = (\Delta Q/\Delta P)(P/Q)$), we obtain $\lambda t = (\Delta Q/1.5)(P/Q) - (\Delta Q/2.16)(P/Q)$. Substituting $t = \tau P$ and $\Delta Q = Q\tau\gamma_q$ reduces this to $\lambda = \gamma_q/1.5 - \gamma_q/2.16 = 0.361/1.5 - 0.361/2.16 = 0.07$.

[41] Recall that the price elasticity of supply lower bound is 1.5 when pre-enforcement compliance is 0, and that the price elasticity of the supply upper bound is ∞ at the compliance upper bound of 24%. See note 40 for the math that links the price elasticity of supply of 2.16 to $\lambda = 0.07$.

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

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 $\epsilon_{supply} = 2.16$. Here, we get a no-tax equilibrium booking price of $139.77 and equilibrium quantity of 6.16 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.[43] 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.[44] 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 by Airbnb 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 levels, 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.

Note that caution is needed when considering our welfare 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 (taxed) markets. Third, these estimates are specific to our sample of jurisdictions, which are among the largest Airbnb markets in the United States and may not be representative of those not included in the sample. Finally, our calculations should be interpreted in the context of the actual tax salience. That is, estimates of the associated excess burden would need to be revised if the taxes were made more or less salient.

[43]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 note 40 for the calculation of the latter two parameters.

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

VI. Conclusion

In this paper, we develop a simple approach to bound pre-enforcement tax compliance using prices before and after a change from partial to full compliance. We illustrate this approach using 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 agreements' effects on booking price and quantity. We find that enforcing 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%. Combining this with the estimated price effect, we calculate a price elasticity of demand of −0.48. If Airbnb hosts are price takers, these estimates yield a lower bound of 1.5 on the price elasticity of supply. This estimate is consistent with the estimated Airbnb price elasticity of supply of 2.16 from Farronato and Fradkin (2018). If we assume that 2.16 is the true supply elasticity, our results imply that taxes are only paid on 7% of Airbnb transactions before an enforcement agreement is implemented.

Overall, these results suggest that Airbnb tax collection agreements can substantially increase tax compliance, as at least 76% of transactions evade taxation pre-enforcement. This implies an increase in tax 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. Because demand is much less elastic than supply, consumers (tourists) bear more of the tax burden than hosts (locals). This may be a desirable feature for state and local policymakers, as the additional revenue and inefficiency associated with taxation is disproportionately borne by visitors. However, our results also suggest that taxing Airbnb is a relatively ineffective policy lever for interest groups seeking to stifle the Airbnb market.

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