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Tax-Induced Inequalities in the Sharing Economy

Yao Cui, Andrew M. Davis

Abstract. The growth of sharing economy marketplaces like Airbnb has generated discussions on their socioeconomic impact and lack of regulation. As a result, most major cities in the United States have started to collect an “occupancy tax” for Airbnb bookings. In this study, we investigate the heterogeneous treatment effects of the occupancy tax policy on Airbnb listings, using a combination of a generalized causal forest methodology and a difference-in-differences framework. While we find that the introduction of the tax significantly reduces both listing revenues and sales, more importantly, these effects are disproportionately more pronounced for residential hosts with single shared-space (nontarget) listings versus commercial hosts with multiple properties or entire-space (target) listings. We further show that this unintended consequence is caused by customers’ discriminatory tax aversion against nontarget listings. We then leverage these empirical results by prescribing how hosts should optimally set prices in response to the occupancy tax and identify the discriminatory tax rates that would equalize the tax’s effect across nontarget and target listings.

1. Introduction

The growth of the sharing economy, which allows for peer-to-peer sharing of goods and services, has led to a number of recent discussions around proper regulation. By allowing consumers to earn income by sharing personal resources with others, consumers can now act as businesses. As a result, policy makers have struggled to find the proper regulation for platforms that facilitate such entities and transactions, such as Airbnb. Indeed, Airbnb’s entry to the sharing economy has not gone unnoticed by the lodging industry. As one executive for the American Hotel and Lodging Association states, “Airbnb is operating a lodging industry, but it is not playing by the same rules” (Benner 2017). Aside from the lodging industry, the growth of platforms like Airbnb has also negatively impacted long-term rental and housing markets, as stated by one city councilman of Los Angeles: “We have lost thousands and thousands of units. We have contributed to homelessness. We have contributed to the higher prices that make the city so unaffordable, and we have to take action today to change this” (Daniels 2019). As a consequence of industry pressure and discussions around Airbnb’s socioeconomic impact, a number of municipalities recently imposed that Airbnb charge and collect an “occupancy tax” for its bookings.

Requiring Airbnb to collect an occupancy tax is not unwarranted, as it generates a more equitable market. Without such a tax, commercial hosts who list multiple hotel-like offerings through Airbnb are afforded a competitive advantage over their peers. Commercial hosts are thus encouraged to “move their properties from out of the long-term rental and for-sale markets and into the short-term rental market” (Barker 2020). As a consequence, policy makers have devoted considerable attention to certain types of “target” hosts and listings. Specifically, hosts with multiple listings, entire-space listings, or listings that are rented out a large percentage of the year (LaGrave 2018). Yet, a significant portion of hosts on Airbnb do not exhibit any of these criteria and are generally of less concern to policy makers (“nontarget” hosts and listings). For example, these latter hosts often look to rent out a portion of their primary residence in a shared-space manner. A Quinnipiac poll finds that most New York City voters support the notion of being able to earn supplemental income by renting out underutilized housing space (Quinnipiac University 2014). Considering that the largest markets for Airbnb typically have higher costs of living, such as Los Angeles and New York City, this extra income helps offset inordinate real estate expenses.
Given the heterogeneity of hosts and listings on Airbnb, it is unclear whether the occupancy tax has affected these alternative types of hosts in different ways. In this paper, we attempt to empirically investigate the effects of the occupancy tax policy on Airbnb listings and develop insights regarding its distributional implications, through the following research questions. (1) How does the occupancy tax differentially impact targeted versus nontargeted listings? (2) What analytical prescriptions can be developed to (i) help hosts and Airbnb better respond to the occupancy tax overall and (ii) mitigate any differential effects on targeted versus nontargeted listings?

To address these research questions, we obtain transaction-level listings data from one of Airbnb's largest markets in 2016, Los Angeles. Our analysis focuses on the City of Los Angeles, a large Airbnb market that imposed the tax on August 1, 2016. We use the remaining County of Los Angeles as the baseline control for contemporaneous unobservables. Thus, we have a quasi-experimental difference-in-differences design, where we compare the change in performance of listings in the City of Los Angeles, after the tax was implemented, to those outside of Los Angeles but within the same county, during the same time frame. To establish that the parallel trends assumption between the two datasets is ensured, we combine a causal forest method with the difference-in-differences framework. Specifically, the causal forest method, which stems from the random forest method in machine learning, addresses the lack of random assignment of the Airbnb tax policy by identifying a "clone" from the control group for each listing in the treatment group. One can then observe the performance of the control (the County of Los Angeles excluding the city) listing, after the tax was implemented in the City of Los Angeles, as the counterfactual for the treated listing. This method is especially advantageous compared with traditional matching approaches in that it allows us to study the heterogeneous treatment effects at the individual listing level and use the predictive power of machine learning to conduct prescriptive analytics.

Before addressing our research questions, we first examine the overall impact of the tax on listing revenues, sales, and prices. We find that the introduction of the occupancy tax, in the City of Los Angeles, leads to statistically significant decreases in listing performance: revenues decreased by 15% and sales decreased by 12%. These reductions are especially pronounced immediately after the implementation of the tax but persist in the long term. We also estimate that listing prices decreased by only 3.2% after the occupancy tax was imposed, suggesting that hosts were unable to mitigate the sales reduction of the tax by adequately decreasing prices.

We then investigate our first research question around the potential heterogeneous treatment effects of the occupancy tax on targeted versus nontargeted listings. In sum, we find that the tax affects nontargeted listings in a disproportionately negative way: the revenue decrease for nontargeted listings is nearly two times larger than that of targeted listings, with a similar effect on sales as well. Importantly, we show that this unintended consequence of tax regulation is not driven by (1) differential price responses of hosts, (2) differential exit rates of listings, or (3) differential price sensitivities of customers, between the two types of listings. Instead, we find that this difference is because of customers’ discriminatory tax aversion against nontargeted listings.

In an effort to test the robustness of these heterogeneous treatment effects, we conduct a number of further analyses that include, but are not limited to, expanding the set of control groups, replicating the analyses on a different treated city, examining strategic booking behavior, extending the time period, and aggregating the level of analysis. In all cases, we find that the introduction of the occupancy tax has an adverse impact on nontargeted listings, relative to targeted listings.

Given these empirical results, we use the causal forest method to address our second research question around developing data-driven prescriptions. In particular, we first identify the optimal price response for each listing, given the introduction of the occupancy tax. We demonstrate that by setting prices optimally (average prescribed reduction of 7.2% versus 3.2% observed), a "win-win" outcome occurs: sales increase by 18.7% on average, thereby benefiting customers, and revenues increase by 12.8%, thereby benefiting hosts. We also prescribe how much the occupancy tax, for nontargeted listings only, would need to be reduced such that it would neutralize its adverse effect on such listings. Overall, we find that a tax rate of 6%, as opposed the 14% currently applied to all listings, would effectively curb the deleterious effect on nontargeted (versus targeted) listings.

2. Related Literature

There are three streams of literature most relevant to our study: (1) empirical papers focusing on the sharing economy, notably Airbnb, (2) studies that investigate the effects of taxes on seller and consumer behavior, and (3) papers that apply causal machine learning methods.

The growth of the sharing economy has motivated academic researchers to study novel issues related to marketplaces (see Chen et al. 2020 and Narasimhan et al. 2018 for reviews of the relevant literature). In particular, there is a line of empirical research on
home-sharing marketplaces such as Airbnb. First, existing research has studied the socioeconomic impacts of Airbnb on other markets. Zervas et al. (2017) find that in Austin, the adverse impact of Airbnb’s market entry on hotel revenues is in the 8%-10% range. Barron et al. (2021) find that a 1% increase in Airbnb listings led to a 0.018% increase in rents and a 0.026% increase in house prices in the United States. Alyakooob and Rahman (2022) study the externality effect of Airbnb on local restaurants and find that a 2% increase in Airbnb activity (measured by reviews per listing) resulted in a 3% growth in restaurant employment in New York City. Moreover, Zhang et al. (2022) study how Airbnb performance depends on local ride-sharing activities and find that the exit of Uber/Lyft led to a decrease of 9.6% in Airbnb demand, which is equivalent to a decrease of $6,482 in the annual revenue of an average Airbnb host. Second, Li and Netessine (2020) study how market thickness affects platform performance and find that increased market thickness leads to lower matching rates because of increased search friction. Third, Li et al. (2019) and Cui et al. (2020b) study Airbnb hosts’ pricing behaviors. Li et al. (2019) investigate the pricing difference between professional and nonprofessional hosts and find that professional hosts price-discriminate more efficiently, which results in higher revenues and occupancy rates. Cui et al. (2020b) identify the differential pricing between listings that facilitate shared living with a local host and those that do not and investigate its implications on the sharing economy. Fourth, Edelman et al. (2017) and Cui et al. (2020a) investigate racial discrimination on Airbnb. Edelman et al. (2017) find that booking requests from guests with African-American names are less likely to be accepted than white names. Cui et al. (2020a) find that a review posted on the guest’s page significantly reduces discrimination. Our paper adds to the literature by studying a new important issue: tax regulation on the sharing economy marketplaces and its resulting distributional implications.

There are past studies that investigated how ad valorem taxes impact seller and consumer behavior. However, most of these study industries that, relative to Airbnb, are homogeneous and/or highly regulated, such as hotels, alcohol, and tobacco. They also often focus on how much of the tax is passed through to customers. For instance, Bonham et al. (1992) show that a 5% occupancy tax imposed on Hawaii hotels was fully passed through to consumers with no significant loss in revenues. Similarly, Kenkel (2005) and Shrestha and Markowitz (2016) both find that alcohol tax increases are passed through to consumers. Turning to the behavioral effects of taxes, research has found that consumers are generally tax averse. For example, Sussman and Olivola (2011) conduct five controlled experiments and find that people prefer to avoid taxes rather than avoid equal-sized increases in other types of costs. Chetty et al. (2009) conduct a field experiment in a supermarket and investigate how the presentation of taxes affects demand and revenue. They observe that when price tags explicitly list both the price plus the required tax (versus the original price by itself), demand decreases by roughly 8%. Feldman and Ruffle (2015) show, through controlled experiments, that consumers spend less money when presented with prices that exclude the tax on the tag (for a review of how consumer choices are influenced by the salience of taxes, see Greenleaf et al. (2016)). Importantly, although past studies on tax aversion focus on identifying its effect and, potentially, estimating its impact on overall demand, we provide a novel result in that tax aversion can be discriminatory. We also demonstrate the negative consequences of such discriminatory tax aversion on nontargeted listings and provide prescriptions for how alternative discriminatory tax rates can offset any unintended adverse effects.

Finally, recent developments in econometrics have featured causal machine learning as an approach for evaluating heterogeneous treatment effects of policies (see Athey and Imbens 2017 for a review). In this paper, we adopt a causal machine learning method, causal forest (Wager and Athey 2018, Athey et al. 2019), to investigate issues related to the tax regulation on Airbnb. Recent studies have applied causal forest in the difference-in-differences framework (Guo et al. 2021, Iyengar et al. 2021) and the instrumental variable framework (Wang et al. 2021a, b). Combining causal forest with difference-in-differences, we study the heterogeneous treatment effects of the occupancy tax policy on Airbnb listings and develop data-driven prescriptions. As we discuss in Section 4.3, causal forest is suitable for achieving such objectives.

3. Institutional Background and Data
3.1. Tax Policy on Airbnb
The Airbnb tax is similar to the occupancy tax of hotels. When a customer makes a reservation, Airbnb collects an extra tax, shown to customers as an additional line item: “occupancy taxes and fees.” This charge is calculated as a percentage (i.e., the tax rate) of the listing price and applicable surcharges (a sample screenshot is in Figure A.1 in Online Appendix A). The tax is charged only to bookings shorter than a month (i.e., 29–30 nights), with the exception that Washington, DC’s tax policy applies to bookings shorter than three months (i.e., 90 nights). This indicates that the Airbnb tax policies are intended to regulate short-term rentals only rather than long-term rentals. Moreover, whereas most municipalities charge
the same Airbnb tax rates as their hotel occupancy tax rates, some charge a higher tax rate for Airbnb than hotels, such as Chicago and its 4% surcharge for vacation rental and shared housing.

Most of the major U.S. cities have implemented tax policies on Airbnb. The first major U.S. city that implemented an Airbnb tax is San Francisco, Airbnb’s home city. Starting October 1, 2014, all customers booking an Airbnb listing in San Francisco paid a 14% occupancy tax. Across all cities, the tax rate ranges from 6% (Texas) to 14.5% (Washington, DC). The tax can also exist on different municipal levels. For instance, Texas implemented the same tax rate for the entire state and Seattle’s tax rate includes both a state tax component and a local tax component that varies by county. Among the cities where the Airbnb tax policy has been introduced, we focus on Los Angeles as our treatment group to analyze the impact of the tax policy on Airbnb listings. Los Angeles charged a 14% tax rate starting August 1, 2016, and was the largest Airbnb market in terms of listings at the start time of this study.

### 3.2. Data

Our study combines three types of data: (1) Airbnb listings’ transaction data, (2) listing attributes data, and (3) demographic data. The transaction data set was purchased from Airdna, which is a company that tracks Airbnb listings daily. In the transaction data set, for each listing and each travel date, there are three possible scenarios: (1) the listing was blocked by the host and not available for rent, (2) the listing was available for rent but not rented by a guest, and (3) the listing was available for rent and rented by a guest. The last scenario corresponds to a transaction. In this case, the data set further reports the transaction price (excluding tax), the booking date (so the advance booking days can be calculated as the difference between the travel date and the booking date), as well as the booking ID (which can be used to infer the length of stay for a booking).

As is typical in the lodging industry, customers make reservations in advance. Thus, to clarify the difference between travel date and booking date, the travel date is the date when the guest will be staying in the property, whereas the booking date is the date when the guest made the reservation for the stay. For example, if on July 20, 2016, a guest booked a stay for the night of August 30, 2016, then the booking date is July 20, 2016, and the travel date is August 30, 2016. It is easy to see the advantage of defining the timeline based on the booking date. For example, if the same listing also received a booking on August 10, 2016, for a stay on the night of August 20, 2016, then the night of August 20, 2016, was affected by the tax policy but the later night of August 30, 2016, was not. Therefore, we define the timeline based on the booking date to facilitate a clean identification of the effect of the tax policy.

We consider six months of transaction data before and after the tax policy was implemented in the City of Los Angeles (i.e., 48 weeks in total), where the first day of the first week during the posttreatment period is the tax policy date (August 1, 2016). Specifically, the booking period ranges from February 15, 2016, to January 15, 2017. We consider all listings that received at least one booking during this time period and exclude listings that entered the market after the tax treatment or exited before the tax treatment. Our final data set consists of 18,517 listings from 104 zip codes in the City of Los Angeles. While we focus on bookings that occurred up to 6 months after the tax treatment in our main analysis, we also verify that our main results are robust when extending to 12 months after the tax treatment (see Section 5.4.5). Moreover, the tax policy in the City of Los Angeles only applies to bookings of 30 nights or shorter. Long bookings that were not affected by the tax policy in the post-treatment period were negligible (0.3%).

Besides transaction data, our study also uses the listing attributes data, collected by Airdna. The data set provides each listing’s attributes related to its physical property (e.g., numbers of bedrooms and bathrooms, entire space, or shared space), service policy (e.g., check-in and check-out times, instant booking without host approval), and the host (e.g., host ID, based on which we can identify the number of listings owned by the same host, and when the host created the listing on Airbnb). The data set also includes all historical reviews. From the text content of the reviews, we can also identify which reviews were automatically generated by Airbnb when the host canceled a booking that was previously accepted. Finally, we use the zip code–level demographics data (e.g., population by race, number of housing units) from the 2016 American Community Survey in the U.S. Census to control for market-level attributes.

Table A.2 in Online Appendix A reports the summary statistics for listing-level performance metrics during the pretreatment period. The average weekly revenue has a mean of $404.9, the average weekly sales (i.e., number of future travel dates booked within a booking week) has a mean of 2.79 days, and the average daily price (net of cleaning/service fees and taxes) has a mean of $162.0. Moreover, to measure listing availability, we consider how many future travel dates during the subsequent two-month period can be booked (i.e., the date is made available for rent by the host and has not been booked so far) at any given time. The mean is 28.4 days out of two months, indicating that on average a customer would find half of the listing’s calendar available for rent. Besides listing
performance metrics, Table A.2 also reports the summary statistics for listing attributes and zip code–level market attributes. Overall, the summary statistics reveal considerable variation across listings, suggesting that the Airbnb market features substantial heterogeneity, and thus different listings may be affected differentially by the tax policy.

4. Overall Effect of Tax Policy

4.1. Research Design and Methodology

Our objective is to estimate the causal impact of the tax policy on Airbnb listings, based on which we can study the heterogeneous treatment effects of the tax policy and conduct prescriptive analytics. Such a causal impact could be easily identified if a randomized controlled field experiment is available. However, such randomization is infeasible in our context, because regulators or Airbnb cannot randomly select a group of listings to charge the tax. Therefore, we have to approach this causal inference problem using observational data, which creates the challenge of not observing how the listing performance would evolve in the absence of the tax treatment.

To predict the counterfactual, a natural way would be finding another region that shares similar time trends with the City of Los Angeles but did not receive the tax treatment during the same time period. Notice that the tax was introduced in the City of Los Angeles and not the County of Los Angeles. Because of geographic proximity, the rest of the County of Los Angeles should have similar time trends with the City of Los Angeles (Figure A.2 in Online Appendix A shows the location of the City of Los Angeles in the County of Los Angeles). Thus, the listings from the rest of the county would serve the purpose of teasing out the time trends regardless of the tax treatment. Upon further analysis, only Malibu and Santa Monica received the tax treatment before the City of Los Angeles. After excluding them, this leaves 6,328 listings from the rest of the County of Los Angeles to construct the control group, while the listings within the City of Los Angeles construct the treatment group. This constitutes a quasi-experimental difference-in-differences design, where we compare the performance change in the treatment group before and after the tax treatment to the change in the control group during the same time period.

The difference-in-differences design controls for other factors that might have affected the listings concurrently with the tax policy (e.g., demand seasonality, macro-economic factors). However, the validity of the difference-in-differences design critically hinges on the parallel trends assumption, which in turn hinges on the comparability between listings in the two groups. While we provide a detailed comparison between the treatment and control groups in Table A.3 in Online Appendix A, we note that the two groups are similar in most dimensions but that there are some meaningful differences. One primary difference is that the treatment group has a higher listing density. This is consistent with the fact that the City of Los Angeles covers more touristic areas compared with the rest of the county. Furthermore, there could be other unobserved factors (e.g., large events that only affected part of the region) that affected the two groups differentially. These differences would raise the concern of whether the counterfactual can be correctly predicted for each listing in the treatment group if the concurrent factors are controlled for only at the group level.

Recognizing that no two markets are perfectly identical (aside from the tax), to achieve comparability between the treatment and control listings in our study, we combine the difference-in-differences design with the causal forest method. In particular, although we select the City of Los Angeles as the treatment group and the remaining County of Los Angeles as the control group, we use causal forest to create a perfect “clone” from the control group for each listing in the treatment group. We then use the clone listing’s performance during the posttreatment period to construct the counterfactual for the treatment listing. Therefore, the causal forest method estimates the treatment effect for each individual listing, which is desirable given our objective to study the heterogeneous treatment effects. To provide a brief background, the causal forest method is adapted from the random forest method in machine learning and applied to the causal inference framework. It provides a nonparametric, computationally efficient approach to match treatment and control units in a multidimensional covariate space. Moreover, the predictive power of machine learning can further enable us to conduct counterfactual analyses had the listings responded differently to the tax policy, based on which we can make data-driven prescriptions.

4.2. Regression Results

Before investigating the heterogeneous treatment effects of the tax on targeted versus nontargeted listings, we first examine the average treatment effect of the tax policy. We start by conducting traditional difference-in-differences regressions in this section (and proceed to causal forest results next) and analyze the impact of the tax policy on three listing performance metrics as dependent variables: revenue, sales, and price.

Revenue and sales are aggregate variables. We consider weekly revenue and sales of listings. In particular, logREVENUE is the (log-transformed) revenue that listing i earned from the bookings it received...
during week \( t \), and \( \text{logSALES}_{ib} \) is the (log-transformed) number of travel dates that were booked on listing \( i \) during week \( t \); note that \( t \) refers to booking week instead travel week. We estimate the following difference-in-differences model for weekly revenue and sales:

\[
y_{it} = \alpha \cdot \text{AFTER}_i + \beta \cdot \text{TREATED}_i \times \text{AFTER}_t + \delta \cdot X_{it} + \gamma_j + \epsilon_{it},
\]

where \( y_{it} = \log \text{REVENUE}_{it} \) or \( y_{it} = \log \text{SALES}_{it} \). The variable \( \text{TREATED}_i = 1 \) if listing \( i \) is in the treatment group and \( \text{TREATED}_i = 0 \) if it is in the control group. The variable \( \text{AFTER}_t = 1 \) for the posttreatment period and \( \text{AFTER}_t = 0 \) for the pretreatment period. The coefficient \( \beta \) is our main coefficient of interest, which captures the impact of the tax policy on the performance of the treatment listings, adjusted by the changes over time regardless of the tax policy as indicated by the control listings.

We use listing fixed effects, \( \gamma_j \), to control for any time-invariant listing-specific factors. We also control for the following time-varying factors in the covariates \( X_{it} \). First, for listing supply, \( \log \text{AVAILABILITY}_{it} \) calculates the number of days that listing \( i \) is still available for rent during the subsequent two months. Second, for reviews, \( \log \text{REVIEWS}_{it} \) tracks the cumulative number of reviews that listing \( i \) has received up to week \( t \), and \( \log \text{CANCELREVIEW}_{it} \) tracks the cumulative number of automatically generated cancellation reviews of listing \( i \) up to week \( t \) (which are excluded from \( \log \text{REVIEWS}_{it} \)). Third, for host experience, we include \( \log \text{EXISTWEEKS}_{it} \), which calculates the number of weeks that listing \( i \) has existed up to week \( t \). Fourth, for market dynamics, we include two zip code-level variables: \( \log \text{LISTINGS}_{ib} \) calculates the number of listings within the same zip code as listing \( i \) that existed in week \( t \), and \( \text{TRANSACTRATIO}_{it} \) calculates the ratio of listings within the same zip code as listing \( i \) that transacted in week \( t \). Finally, we cluster standard errors at the listing level for serial correlation.

Price can be analyzed at a more granular level. Let \( \log \text{PRICE}_{ibd} \) be the (log-transformed) price for a booking received on date \( d \) for a stay on date \( d \); thus, \( b \) corresponds to the booking date and \( d \) corresponds to the travel date. We estimate the following difference-in-differences model for price:

\[
\log \text{PRICE}_{ibd} = \alpha \cdot \text{AFTER}_i + \beta \cdot \text{TREATED}_i \times \text{AFTER}_d + \delta \cdot X_{ibd} + \gamma_j + \tau_1 \cdot \text{WEEK}_d + \tau_2 \cdot \text{DAYOFWEEK}_d + \epsilon_{ibd}.
\]

The analysis at the date level allows us to control for more factors. First, we use travel week fixed effects and travel day of week fixed effects to control for contemporaneous seasonality. Because market conditions are much more sensitive to travel time than booking time, we impose tighter controls for travel time. Second, to account for the host’s price adjustment as the travel date approaches, we include \( \log \text{ADVANCEDAYS}_{ibd} \) in the covariates \( X_{ibd} \) which controls for the advance booking days for a booking that listing \( i \) received on date \( d \) for a stay on date \( d \). Third, we control for the length of stay associated with each booking \( b \) of listing \( i \), \( \log \text{LENGTHOFSTAY}_{ib} \).

Table 1 reports the difference-in-differences regression results. Column (1) shows that the tax policy resulted in a 8.3% reduction (\( e^{-0.087} - 1 \)) in revenue (\( p < 0.01 \)). Moreover, as shown in Columns (2) and (3), this revenue reduction appears to be driven by a mixture of a demand reduction and a modest price adjustment of hosts: sales decreased by 7.3% (\( e^{-0.076} - 1 \)) and price decreased by 3.6% (\( e^{-0.035} - 1 \)) (both \( p < 0.01 \)). Because of the price adjustment, the occupancy tax was partially passed through to the hosts. However, a 3.6% pass-through does not appear to be substantial considering that the tax rate is 14%, and the tax is still largely paid by the customers.

### 4.3. Causal Forest

As mentioned previously, we use the causal forest method because of our desire to create a perfect clone for each listing in the treatment group, using the listings from the control group that did not experience the tax treatment. The idea of causal forest shares some similarity with propensity score matching. However, causal forest has several important advantages over traditional matching methods. First, traditional matching methods such as propensity score matching are not optimized for uncovering heterogeneous treatment effects (Zubizarreta et al. 2014). Causal forest, on the other hand, achieves desired consistency and asymptotic normality when estimating the heterogeneous treatment effects at the individual level. Thus, it enables us to make statistical inferences for each individual listing and study the heterogeneous treatment effects at a more granular level. Second, the machine learning nature of causal forest indicates that it is designed to optimize out-of-sample predictive accuracy by algorithmically trading off bias and variance (Hastie et al. 2009). Therefore, it enables us to more readily predict counterfactuals and make data-driven prescriptions to both Airbnb (e.g., guiding the hosts to adjust prices optimally) and policy makers (e.g., setting the optimal tax rate). Third, compared with traditional matching methods, causal forest is more robust to model misspecification, more computationally efficient with a larger covariate space, and can substantially increase the power of accurate clustering within a large covariate space (Athey and Imbens 2017, Athey et al. 2019). Given our main purpose of studying the
heterogeneous treatment effects and conducting prescriptive analytics, causal forest is the preferred method.

We now briefly review the method; the readers are referred to Athey et al. (2019) for the technical details. Suppose that the data observed by the researcher are represented by \((X_i, Y_i, W_i)\), where \(X_i\) denotes the covariates, \(Y_i\) is the outcome variable, and \(W_i\) is the treatment indicator. Following the potential outcomes framework (Rubin 1974), the goal is to estimate the (conditional average) treatment effect at any \(x\), 

\[
\tau(x) = \mathbb{E}[Y_i^{(1)} - Y_i^{(0)} | X_i = x],
\]

under the unconfoundedness assumption, \(\{Y_i^{(0)}, Y_i^{(1)}\} \perp W_i | X_i\).

The method first grows a generalized random forest. Each tree is grown on a random subsample (drawn without replacement) through recursive partitioning (Breiman 2001). In growing a tree, each step of splitting is achieved by a gradient-based greedy algorithm that selects a splitting variable from a random subset of covariates and the value of the splitting variable to divide the covariate space, with the aim of increasing the heterogeneity of the treatment effect estimates across the child nodes as fast as possible. Because of the adaptive feature of tree-based approaches, the data will be split into narrower leaves where the treatment effect is more sensitive with respect to the covariates. To achieve consistent estimation, the method requires the trees to be “honest” (Wager and Athey 2018); that is, each training sample is only used to either grow the tree or estimate the treatment effects. Correspondingly, the method achieves honesty by randomly dividing the subsample assigned to each tree into two evenly sized, non-overlapping halves, one for growing the tree and the other for estimating the treatment effects.

The method then uses the resulting forest to make out-of-bag predictions. In particular, a similarity weight \(a_i(x)\) is calculated as the frequency with which the \(i\)th training example falls in the same leaf as \(x\). The weights \(a_i(x)\) thus define the forest-based adaptive neighborhood of \(x\). The method uses these weights to define a moment condition, from which the treatment effects, \(\hat{\tau}(x)\), are identified. Athey et al. (2019) show that causal forest estimates are consistent for the true treatment effect \(\tau(x)\). Moreover, the estimates are asymptotically Gaussian, based on which an estimator for asymptotic variance is developed.

In our implementation, \(W_i = 1\) for the treatment group and \(W_i = 0\) for the control group. The outcome variable \(Y_i\) corresponds to the performance change of listing \(i\) before and after when the tax was introduced in the treatment group. Building on the primitives defined in Section 4.2, we examine the performance changes in revenue (\(Y_i^{\text{REVENUE}} = \frac{1}{T_i} \sum_{t \in T_i} \log \text{REVENUE}_{it}\)), sales (\(Y_i^{\text{SALES}} = \frac{1}{T_i} \sum_{t \in T_i} \log \text{SALES}_{it}\)), and price (\(Y_i^{\text{PRICE}} = \frac{1}{T_i} \sum_{t \in T_i} \log \text{PRICE}_{it}\)), where \(T_0\) and \(T_1\) represent the pre- and post-treatment periods, respectively. Because these outcome variables correspond to before/after changes, the estimated treatment effects, \(\hat{\tau}_i^{\text{REVENUE}}, \hat{\tau}_i^{\text{SALES}}, \text{ and } \hat{\tau}_i^{\text{PRICE}}\), all have difference-in-differences meanings. Moreover, because the outcome variables are log-transformed, we can evaluate differences across listings in relative terms.

We include a rich set of covariates \(X_i\), for unconfoundedness. First, to ensure parallel trends, we include the weekly revenue, sales, and price of each listing for the 24-week pretreatment period. Second, to achieve comparability of listings, we include the listings’ attributes regarding its physical property (e.g., numbers of bedrooms and bathrooms), service policy (e.g., check-in and check-out times), host (e.g., number of listings owned by the host), and performance.

### Table 1. Difference-in-Differences Regression Results

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<th>(1) logREVENUE</th>
<th>(2) logSALES</th>
<th>(3) logPRICE</th>
</tr>
</thead>
<tbody>
<tr>
<td>AFTER ((\alpha))</td>
<td>-0.236*** (0.017)</td>
<td>-0.223*** (0.017)</td>
<td>0.023*** (0.002)</td>
</tr>
<tr>
<td>TREATED × AFTER ((\beta))</td>
<td>-0.087*** (0.018)</td>
<td>-0.076*** (0.017)</td>
<td>-0.037*** (0.003)</td>
</tr>
<tr>
<td>logAVAILABILITY</td>
<td>0.502*** (0.004)</td>
<td>0.503*** (0.004)</td>
<td>0.005*** (0.001)</td>
</tr>
<tr>
<td>logREVIEWS</td>
<td>0.473*** (0.013)</td>
<td>0.485*** (0.013)</td>
<td>0.014*** (0.002)</td>
</tr>
<tr>
<td>logCANCELREVIEWS</td>
<td>-0.512*** (0.036)</td>
<td>-0.496*** (0.035)</td>
<td>0.006 (0.004)</td>
</tr>
<tr>
<td>logEXISTWEEKS</td>
<td>-0.191*** (0.010)</td>
<td>-0.196*** (0.010)</td>
<td>0.003 (0.002)</td>
</tr>
<tr>
<td>logLISTINGS</td>
<td>-0.802*** (0.055)</td>
<td>-0.859*** (0.055)</td>
<td>-0.002 (0.007)</td>
</tr>
<tr>
<td>TRANSACTRATIO</td>
<td>7.746*** (0.069)</td>
<td>7.764*** (0.068)</td>
<td>0.019*** (0.008)</td>
</tr>
<tr>
<td>logADVANCEDAYS</td>
<td>0.020*** (0.000)</td>
<td>0.020*** (0.000)</td>
<td>0.006*** (0.000)</td>
</tr>
<tr>
<td>logLENGTHHOFSTAY</td>
<td>0.020*** (0.000)</td>
<td>0.020*** (0.000)</td>
<td>0.006*** (0.000)</td>
</tr>
<tr>
<td>(N)</td>
<td>1,020,581</td>
<td>1,020,581</td>
<td>2,107,244</td>
</tr>
<tr>
<td>Adjusted (R^2)</td>
<td>0.320</td>
<td>0.326</td>
<td>0.957</td>
</tr>
</tbody>
</table>

**Notes.** This table reports the results from difference-in-differences regressions. The revenue and sales regressions (columns (1) and (2)) are at the week level and include listing fixed effects. The price regression (column (3)) is at the date level and includes listing fixed effects, travel week fixed effects, and travel day of week fixed effects. The standard errors are clustered at the listing level.

* \(p < 0.10; **p < 0.05; ***p < 0.01.\)
during the pretreatment period (e.g., listing availability, number of reviews). Third, to correct for the differences in market environment between the treatment and control groups, we include zip code-level market attributes (e.g., per capita income, median rent). In constructing these variables, we expand out the categorical variables via one-hot encoding. In total, our causal forest implementation consists of 118 covariates; Table A.4 in Online Appendix A defines these covariates.

We grow 10,000 trees in each forest; it is recommended that the number of trees is chosen on the order of the number of training examples (Wager et al. 2014). Each tree is grown with a random subsample that contains 50% of the entire training examples. In growing a tree, the algorithm will select a random subset of covariates for each step of splitting, where the number of covariates to select each time is drawn from a Poisson distribution with mean equal to the square root of the total number of covariates. An important parameter for the tree-growing process is the minimum size of a leaf node (i.e., the minimum number of listings each leaf node is allowed to have from each group, treatment or control). The size of leaves determines the depth of a tree, which is important in governing the tradeoff between bias and variance in causal forest estimation. Smaller leaves (i.e., deeper trees) can improve the algorithm’s ability to predict heterogeneity while reducing the consistency of estimation across different subsamples. To induce the appropriate size of leaves, we tune the minimum size of a leaf node by cross-validation.

To verify the ability of causal forest to construct the perfect clone listings in our setting, we conduct a balance test on the causal forest estimation results. We first construct the clone listing for each listing in the sample by evaluating the mean of its adaptive neighborhood identified by causal forest. This involves taking the weighted average across all listings based on the similarity weights obtained from causal forest estimation. Then, we run paired t tests on the listing attributes between the original listings and the clone listings. The results of the t tests are reported in Table A.5 in Online Appendix A. In short, the clone listings are statistically indistinguishable from the original listings \((p > 0.1)\) in all dimensions, suggesting a highly reliable matching accuracy of causal forest.

### 4.4. Causal Forest Estimation Results

Continuing with the tax’s overall impact on revenue, sales, and price, Table 2 reports the causal forest estimation results. From the individual treatment effects, the doubly robust average treatment effect is estimated via augmented inverse-propensity weighting (Robins et al. 1994). As Table 2 shows, the average revenue treatment effect on the treated listings (ATT) is \(-0.162\) \((p < 0.01)\). Thus, the tax policy resulted in a 15.0\% reduction \((e^{0.162} - 1)\) in revenue and substantially hurt the listing performance. Furthermore, sales decreased by 12.0\% \((e^{0.128} - 1)\) and price decreased by 3.2\% \((e^{-0.033} - 1)\) (both \(p < 0.01)\). This latter result provides some evidence that, overall, there was a modest price response from the hosts to the tax policy, and hence a tax pass-through from the hosts to the customers. However, consistent with the regression results in Section 4.2, this effect does not appear to be substantial: with a 14\% tax, the total tax-inclusive price still increased by \((1 - 3.2\%) \times (1 + 14\%) - 1 = 10.4\%\), leaving 10.4\%\%/14\% = 74\% of the tax to the customers and 26\% to the hosts. Such an uneven distribution of tax between the two sides of the market results in a considerable demand reduction. These results indicate that the Airbnb market may not have been efficient in internalizing the tax in the market price, and another equilibrium with a more noticeable price reduction may induce a higher demand and allow the hosts to retain more revenue even though a tax is in place. We will explore this further in Section 6.1.

We also investigate how the treatment effects evolve over time using causal forest to estimate the treatment effects up to each month in the post-treatment period. Figure 1 shows the estimated monthly average treatment effects along with the 95\% confidence intervals. An examination of Figure 1 suggests that, while the demand side responded immediately to the tax policy, there was a lead time of one month for the supply side to respond. During the first month, both revenue and sales declined substantially (by 20.9\% \((e^{-0.239} - 1)\) and 20.5\% \((e^{-0.229} - 1)\), respectively), whereas price remained unchanged (the average treatment effect is insignificant both statistically and economically), indicating that the Airbnb hosts had not yet started to adjust their prices. Price started to decline during the second month, and correspondingly, the decline in revenue and sales was attenuated. The overall effect on revenue started to stabilize after three months. Although price continued to decline slightly, its marginal effect on demand recovery diminishes, making the

<table>
<thead>
<tr>
<th>Table 2. Causal Forest Estimation Results</th>
</tr>
</thead>
<tbody>
<tr>
<td>ATT estimate</td>
</tr>
<tr>
<td>------------------------------------------</td>
</tr>
<tr>
<td>Revenue (-0.162^{***}(0.009))</td>
</tr>
<tr>
<td>Sales (-0.128^{***}(0.009))</td>
</tr>
<tr>
<td>Price (-0.033^{***}(0.002))</td>
</tr>
</tbody>
</table>

Notes. This table reports the average treatment effect for treated listings (ATT) and the associated standard error (in parentheses). The ATT is doubly robust and estimated via augmented inverse-propensity weighting. The table also reports the percentiles of the individual treatment effects.

*\(p < 0.10\); **\(p < 0.05\); ***\(p < 0.01\).
overall impact on revenue insignificant. This provides some evidence that our results from analyzing the six-month post-treatment period can generalize to a longer period.

5. Heterogeneous Treatment Effects
We now turn to the heterogeneous impact of the tax. As mentioned previously, the causal forest method estimates the treatment effect for each individual listing, so we can examine the distribution of individual treatment effects across listings. Table 2 reports the percentiles of individual treatment effects for revenue, sales, and price (the entire distributions are shown in Figure A.3 in Online Appendix A). As these results show, there can be substantial variations in the individual treatment effects across listings. For example, the 10th and 90th percentiles of the revenue treatment effects are −0.484 and 0.152, respectively. Moreover, 77.2% of listings experienced a revenue reduction because of the tax, 62.3% of listings experienced a revenue reduction of more than 10%, and 32.2% of listings experienced a revenue reduction of more than 25%. Such variations warrant more detailed analyses regarding heterogeneous treatment effects, which we focus on now based on the causal forest estimation results.

5.1. Targeted vs. Nontargeted Listings
Although the original goal of home-sharing marketplaces was to serve as an efficient means to share living resources that would otherwise go to waste (e.g., a spare bedroom), high profit margins and lack of regulation have inevitably attracted other types of hosts who are more commercially driven. In particular, certain hosts own multiple listings on Airbnb and operate more like a hotel. A number of these hosts migrated from long-term rental markets or purchased new properties solely for the purpose of renting on a daily basis on Airbnb. Indeed, a short-term rental property can generate higher profit margins through a higher daily price. The downside, however, is that the occupancy rate of the property is lower compared with when it is rented on a monthly basis, which causes an inefficiency in housing resources and in turn leads to higher prices in the long-term rental and housing markets (Barron et al. 2021). This negative externality has contributed to a larger social issue, especially in larger cities where housing resources are already scarce. As a consequence, these types of listings have been of particular concern to policy makers.

On the other hand, the social impact of a local residential host, who only shares part of his/her unused space with travelers and lives in the same property, is different. In this case, Airbnb listings serve as a means to utilize housing resources more efficiently. Moreover, the entry of Airbnb has significantly impacted the profitability of hotels (Zervas et al. 2017). Although certain Airbnb listings may offer customers a similar experience with staying in a hotel room, other listings can offer considerably different experiences because of their unique features. Among all such features, the ability to live with a local host is one that fundamentally differentiates Airbnb from traditional hotels. With shared living, the customer can interact socially with the host and obtain local information and travel tips from them (Cui et al. 2020b). Thus, shared-living listings can generate consumer welfare in ways that traditional hotels are not able to and create new opportunities for tourism. Because of the differentiated service features, Airbnb’s shared-space listings are also less likely to be perceived as a direct competitor for hotels, compared with entire-space listings.

Given these differences, we investigate the heterogeneous treatment effects of the tax policy. We accomplish this by defining a variable which represents whether a listing is from a residential host or a commercial host, the latter of which is of special interest to regulators. To this end, we define TARGET; = 0 if the
host of listing $i$ owns only one listing and listing $i$ is a shared-space listing, and $\text{TARGET}_i = 1$ otherwise. This definition captures the previous two key characteristics that receive considerable attention by policymakers (Daniels 2019). To study how the tax policy impacted the two types of listings differentially, we estimate the following regression model:

$$
\hat{\tau}_{i}^{\text{REVENUE}} = \alpha + \beta \cdot \text{TARGET}_i + \delta \cdot X_i + \epsilon_i
$$

(1)

where $\hat{\tau}_{i}^{\text{REVENUE}}$ is the causal forest estimated revenue treatment effect for listing $i$, and $X_i$ is a vector of covariates that include listing and market attributes (Table A.7 in Online Appendix A shows the correlation matrix). The coefficient $\beta$ is our main coefficient of interest, which captures the differential impact on targeted listings relative to nontargeted listings. To be consistent with causal forest estimation, Regression Model (1) is estimated using a doubly robust estimator that generalizes the augmented inverse-propensity weighted estimator used for estimating the average treatment effect in causal forest (Semenova and Chernozhukov 2021).

Columns (1) and (2) of Table 3 report the results for heterogeneous treatment effects on revenue. We can see that $\beta$ is positive and statistically significant ($p < 0.01$), indicating that targeted listings were penalized less by the tax policy compared with nontargeted listings. Specifically, when not controlling for listing covariates (column (1) of Table 3), the treatment effect differential is $\beta = 0.166$, and the average treatment effect is $\alpha = -0.302$ for nontargeted listings and $\alpha + \beta = -0.136$ for targeted listings (recall from Section 4.4 that the overall average treatment effect is $-0.162$). When controlling for listing covariates (column (2) of Table 3; the coefficients of the covariates are omitted from Table 3 and the complete table is provided in Table A.8 in Online Appendix A), $\beta = 0.114$, which again shows a substantial differential treatment effect on the two types of listings. Therefore, the tax policy adversely affects nontargeted listings disproportionately while giving a relative advantage to targeted listings.

In addition, as a robustness check for our definition of targeted listings, we run another regression by separately considering the two factors that define targeted listings, hosts with multiple listings and entire-space listings. The results are reported in columns (3) and (4) of Table 3, which show that both factors attribute to the advantage to targeted listings, although whether a listing is an entire-space listing or not accounts for a greater portion of the effect.

To determine whether this heterogeneous treatment effect is driven by demand-side or supply-side factors, we also estimate Regression Model (1) using the sales and price treatment effects as the dependent variable. The results are reported in Table 4. The heterogeneous treatment effect on sales shows a similar result to revenue. Specifically, when controlling for listing covariates, targeted listings have a positive differential of $\beta = 0.140$ in sales reduction relative to nontargeted listings. The heterogeneous treatment effect on price shows a different result, as $\beta$ is insignificant both economically and statistically when controlling for listing covariates. Thus, although we observed a modest price response from hosts overall (Table 2), hosts of targeted and nontargeted listings did not adjust prices differentially. These results suggest that the differential impact of the tax policy on the two types of listings is not caused by the hosts’ differential pricing adjustments. Rather, regardless of the price, customers became disproportionately less likely to book a nontargeted listing. We next investigate those factors which may contribute to such a differential impact.

5.2. Discriminatory Tax Aversion

In this section, we provide further evidence to identify the underlying mechanism for the observed effect that nontargeted listings were hurt more by the tax policy. Figure 2 summarizes the possible explanations we explore, including both supply-side and demand-side factors.

<table>
<thead>
<tr>
<th>Table 3. Heterogeneous Treatment Effects: Revenue</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\hat{\tau}_{i}^{\text{REVENUE}}$</td>
</tr>
<tr>
<td>(1)</td>
</tr>
<tr>
<td>TARGET ($\beta$)</td>
</tr>
<tr>
<td>HOSTLISTINGS &gt; 1</td>
</tr>
<tr>
<td>SPACE = ENTIRE</td>
</tr>
<tr>
<td>Intercept ($\alpha$)</td>
</tr>
<tr>
<td>Controls</td>
</tr>
<tr>
<td>N</td>
</tr>
</tbody>
</table>

Notes. This table reports the results from a doubly robust regression of the causal forest estimated individual treatment effects on revenue against the covariates. In columns (1) and (2), the main independent variable is whether a listing is a targeted listing by the tax policy. Columns (3) and (4) investigate a breakdown of targeted listings into the two variables that define them. Robust standard errors are reported in parentheses. In columns (2) and (4), the coefficients of the controls (which include listing and market attributes) are omitted. The complete table is provided in Table A.8 in Online Appendix A.

*p < 0.10; **p < 0.05; ***p < 0.01.
We first consider factors related to the supply side of the market. In particular, if the hosts of targeted and nontargeted listings responded to the tax policy differently, that may cause the impact of the tax to be different for the two types of listings. This might be plausible especially given that the hosts of targeted listings are more likely to be commercial players. However, we do not find any evidence that the two types of listings responded differentially to the tax policy. One way for the host to respond is to reduce the price to maintain more demand. As we have found previously (column (4) in Table 4), the two types of hosts did not adjust prices to different degrees. The other way for the host to respond is to remove the listing from Airbnb. If a greater portion of targeted listings exited Airbnb after the tax policy was implemented, the adverse impact of the tax policy may be mitigated to a greater extent for targeted listings that remained because they would face less competition compared with nontargeted listings. To see whether this is the case, we estimate the following difference-in-differences model at the zip code level to analyze the listing composition over time:

\[
\text{PERCENT\_TARGET}_{kt} = \alpha \cdot \text{AFTER}_t + \beta \cdot \text{TREATED}_k \\
\times \text{AFTER}_t + \delta \cdot \text{X}_{kt} + \gamma_k + \epsilon_{kt}.
\]

In this case, the dependent variable \( \text{PERCENT\_TARGET}_{kt} \) is the percentage of targeted listings that existed in week \( t \) within zip code \( k \). The coefficient \( \beta \) is our main coefficient of interest, which captures the impact of the tax policy on the listing composition, adjusted by the evolvement over time regardless of the tax policy as indicated by the control group.

Figure 2. Summary of Possible Explanations for Heterogeneous Treatment Effects

Table 4. Heterogeneous Treatment Effects: Sales and Price

<table>
<thead>
<tr>
<th></th>
<th>( \hat{\tau} ) SALES</th>
<th></th>
<th>( \hat{\tau} ) PRICE</th>
</tr>
</thead>
<tbody>
<tr>
<td>TARGET (( \beta ))</td>
<td>0.227*** (0.027)</td>
<td>0.140*** (0.029)</td>
<td>-0.027*** (0.005)</td>
</tr>
<tr>
<td>Intercept (( \alpha ))</td>
<td>-0.320*** (0.025)</td>
<td>-0.009*** (0.025)</td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>N</td>
<td>18,517</td>
<td>18,517</td>
<td>12,728</td>
</tr>
</tbody>
</table>

Notes. This table reports the results from a doubly robust regression of the causal forest estimated individual treatment effects on sales (columns (1) and (2)) and price (columns (3) and (4)) against the covariates. Robust standard errors are reported in parentheses. In columns (2) and (4), the coefficients of the controls (which include listing and market attributes) are omitted. The complete table is provided in Table A.9 in Online Appendix A.

\*\( p < 0.10 \); **\( p < 0.05 \); ***\( p < 0.01 \).

The regression results are reported in Table 5. As one can see, the treatment group did not experience a differential change in the listing composition because of the tax treatment, compared with the control group (\( p > 0.1 \) for \( \beta \)). Furthermore, the market composition also remained stable after the tax treatment for the control group (\( p > 0.1 \) for \( \alpha \)). The results are robust when including time-varying zip code-level controls or not. Based on these results, we can rule out the differential host responses (i.e., supply-side factors) as the underlying mechanism for the observed heterogeneous treatment effect. We examine demand-side factors next.

Because targeted listings are relatively more similar to hotel rooms in terms of the service features offered, whereas nontargeted listings offer the unique feature of sharing living with a local host and may be perceived as less business oriented, the demand for the two types of listings may correspond to different customer segments. The impact of the tax policy will thus vary if alternative customer segments responded to the tax differentially. First, with the occupancy tax, customers must pay more overall to book a listing. If different customer segments have different degrees of price sensitivity, then the same tax rate will lead to a heterogeneous treatment effect. In particular, our observed effect of targeted listings being impacted less by the tax policy could be driven by the possibility

Table 5. Examination of Differential Listing Dropout

<table>
<thead>
<tr>
<th>PERCENT_TARGET</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>AFTER (( \alpha ))</td>
<td>0.003 (0.008)</td>
<td>-0.013 (0.008)</td>
</tr>
<tr>
<td>TREATED \times AFTER (( \beta ))</td>
<td>0.006 (0.010)</td>
<td>0.010 (0.009)</td>
</tr>
<tr>
<td>logLISTINGS</td>
<td>0.058* (0.034)</td>
<td></td>
</tr>
<tr>
<td>TRANSACTRATIO</td>
<td>0.079*** (0.028)</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>12,234</td>
<td>12,234</td>
</tr>
<tr>
<td>Adjusted ( R^2 )</td>
<td>0.828</td>
<td>0.835</td>
</tr>
</tbody>
</table>

Notes. This table reports the results from difference-in-differences regressions for weekly percentage of targeted listings per zip code, with zip code fixed effects. The standard errors are clustered at the zip code level.

\*\( p < 0.10 \); **\( p < 0.05 \); ***\( p < 0.01 \).
that the demand for targeted listings is less sensitive to price changes.

To examine price sensitivity, we use the causal forest estimated treatment effects on both price and sales and analyze the relationship between them. Moreover, a regression that relies on cross-listing variations could suffer from omitted factors that influence both the demand response to the tax and the price adjustment of the host. We thus consider fixed effects regressions which rely on within-listing variations over time and run this regression on the causal forest estimates for the treatment effects on price and sales within each month. By using listing fixed effects to control for cross-listing unobservables, we analyze how the hosts’ price adjustments over time affect listing demand.\(^1\) The regression model is specified as follows:

\[
\hat{\imath}_{\text{im}} \text{SALES} = \beta_1 \cdot \hat{\imath}_{\text{im}} \text{PRICE} + \beta_2 \cdot \hat{\imath}_{\text{im}} \text{PRICE} \times \text{TARGET}_i + \delta \cdot X_{\text{im}} + \gamma_i + \epsilon_{\text{im}},
\]

where \(\hat{\imath}_{\text{im}} \text{SALES}\) and \(\hat{\imath}_{\text{im}} \text{PRICE}\) are the causal forest estimated treatment effects on sales and price during month \(m_i\), respectively, \(\beta_1\) corresponds to the price sensitivity for nontargeted listings, and \(\beta_2\) corresponds to the price sensitivity differential between targeted and nontargeted listings.

The regression results are reported in Table 6. While the market demand is sensitive to listing prices in general (\(\beta_1\) is negative and statistically significant; \(p < 0.01\)), the two types of listings do not have statistically distinguishable price sensitivities (\(p > 0.1\) for \(\beta_2\)). However, the magnitude of \(\beta_2\) may be considerable. A negative \(\beta_2\) (which is the case after including time-varying listing covariates; see column (2) of Table 6) indicates that targeted listings may actually face a (statistically insignificant) higher degree of price sensitivity but were hurt less by the tax policy regardless. Therefore, we do not find any evidence that supports differential price sensitivity as the factor that drives the heterogeneous treatment effect.

Another factor that may affect demand during the post-treatment period is the tax aversion of customers. That is, demand may decrease if customers must pay more because of a supplementary tax rather than a simple increase in the listing price itself. If the degree of customers’ tax aversion is correlated with whether a listing is targeted or not (i.e., customers’ tax aversion is discriminatory), then the heterogeneous treatment effect will be affected by the tax aversion.

To examine tax aversion, we compare a listing that received the tax treatment to its counterfactual if it did not receive the tax treatment, but its price was increased to the same tax-inclusive level. For example, we compare the performance of a listing with price $100 plus a 14% tax to its counterfactual performance if its price were $114 without any tax. Such a comparison will tell us whether customers are less willing to pay the extra $14 when it is paid as tax. We use causal forest to mimic such an experiment in the posttreatment period. For each listing in the treatment group, we predict the counterfactual listing performance in the absence of the tax treatment using listings from the control group. We match listings based on the tax-inclusive price, so that the total price paid by the customer is the same. For the covariates, we include weekly tax-inclusive price trends during the posttreatment period, as well as listing and market attributes to control for the quality of listings. The outcome variable is the sales of each listing. The causal forest estimate then tells us the demand difference when tax is charged versus when the same amount of tax is built into the price and not charged as tax.

From this analysis, the average treatment effect of tax aversion on sales is \(-0.065\) (standard error = 0.006; \(p < 0.01\)), which accounts for about half of the overall sales reduction (recall from Section 4.4 that the average treatment effect is \(-0.128\) for sales). Specifically, listing sales dropped by 6.3\(\%\) (\(e^{-0.065} - 1\)) because the additional amount that customers had to pay was a tax. Although the magnitude of this effect is smaller than in laboratory studies (e.g., 30\% in Feldman and Ruffe 2015), we note that it is similar to a well-known field experiment on tax aversion in supermarkets, which estimates an 8\% reduction in demand (Chetty et al. 2009).

Turning to whether targeted and nontargeted listings have different degrees of tax aversion, we now estimate Regression Model (1) using the tax aversion estimates as the dependent variable. The results are reported in Table 7. We find that targeted listings face a lower degree of tax aversion (i.e., \(\beta\) is positive). The effect is significant statistically (\(p < 0.01\)), and the magnitude of \(\beta\) is similar to that in the regressions for

| Table 6. Examination of Differential Price Sensitivity |
|----------------|----------------|----------------|----------------|
|       |       | \(\hat{\imath}_{\text{im}} \text{SALES}\) | \(\hat{\imath}_{\text{im}} \text{PRICE}\) |
| \(\hat{\imath}_{\text{im}} \text{PRICE} \times \text{TARGET}\) | \(\beta_1\) | \(-0.666*** (0.230)\) | \(-0.782*** (0.246)\) |
| logAVAILABILITY | \(\beta_2\) | \(0.048 (0.245)\) | \(-0.058 (0.249)\) |
| logREVIEWS | \(\gamma_i\) | \(0.044*** (0.005)\) | \(0.061** (0.019)\) |
| logCANCELCREVIEWS | \(\delta\) | \(-0.028 (0.025)\) | \(-0.028 (0.025)\) |
| logEXISTWEEKS | \(\epsilon_{\text{im}}\) | \(-0.087 (0.068)\) | \(-0.087 (0.068)\) |
| logLISTINGS | \(\delta\) | \(0.495*** (0.043)\) | \(0.495*** (0.043)\) |
| TRANSACTRATIO | \(\gamma_i\) | \(0.782*** (0.246)\) | \(0.782*** (0.246)\) |
| N | \(N\) | 37,216 | 37,216 |
| Adjusted \(R^2\) | \(R^2\) | 0.295 | 0.302 |

Notes. This table reports the results from panel regressions using monthly treatment effects on sales and price, with listing fixed effects. The standard errors are clustered at the listing level.

\(\cdot p < 0.10; **p < 0.05; ***p < 0.01.\)
Table 7. Heterogeneous Treatment Effects: Tax Aversion

<table>
<thead>
<tr>
<th></th>
<th>TAX_AVERSION (1)</th>
<th>TAX_AVERSION (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>TARGET (β)</td>
<td>0.173*** (0.020)</td>
<td>0.146*** (0.021)</td>
</tr>
<tr>
<td>Controls</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>N</td>
<td>15,402</td>
<td>15,402</td>
</tr>
</tbody>
</table>

Notes. This table reports the results from a doubly robust regression of the causal forest estimated individual treatment effects on tax aversion against the covariates. Robust standard errors are reported in parentheses. In column (2), the coefficients of the controls (which include listing and market attributes) are omitted. The complete table is provided in Table A.10 in Online Appendix A. *p < 0.10; **p < 0.05; ***p < 0.01.

This table reports the results from a doubly robust regression of the causal forest estimated individual treatment effects on tax aversion against the covariates. Robust standard errors are reported in parentheses. In column (2), the coefficients of the controls (which include listing and market attributes) are omitted. The complete table is provided in Table A.10 in Online Appendix A.

5.3. Expanded Control Pool

While we will later demonstrate the ability of causal forest to achieve unconfoundedness, a control group that is more comparable to the treatment group would enable causal forest to achieve a higher matching accuracy. Because of the quasi-experimental nature of our study, potential concerns could arise from the fact that there are systematic differences between the treatment and control groups. To alleviate this concern, we repeat our analysis on the heterogeneous treatment effects of the tax, when expanding the control group beyond the County of Los Angeles. In particular, we sequentially include listings from San Francisco, San Diego, and New York City.

We note that San Francisco and San Diego have the advantage of being geographically closer to Los Angeles, whereas New York City is located on the east coast and may have different seasonality than Los Angeles. However, both San Francisco and San Diego have introduced the tax policy before Los Angeles, indicating that the listings in the treatment and control groups would be under different tax regimes during our pretreatment period, whereas New York City has the advantage of not having introduced the tax policy before Los Angeles.

We now perform a series of robustness checks for our findings around heterogeneous treatment effects. All detailed result tables from these analyses, including two further checks omitted here for space (on alternative listing categorization and tax aversion), are provided in Online Appendix B.

5.4. Robustness Checks

5.4.1. Unconfoundedness Assumption. Although the causal forest method has advantages over traditional matching methods, its validity hinges on the unconfoundedness assumption, that is, conditional on the matching covariates, the treatment status is independent of the outcome. Although the unconfoundedness assumption is not directly testable, we now conduct a placebo test to provide some evidence that it is likely satisfied in our setting. The idea is that if causal forest is effective in achieving unconfoundedness, we would expect the clone listings created from the control group to be indistinguishable from the treatment listings during a time period without the tax treatment. To this end, we focus on the six-month pretreatment period and place a placebo treatment at the middle of this period. We then run our causal forest difference-in-differences analysis between the first three months and the second three months. The estimated average treatment effects are 0.002 (standard error = 0.028) for revenue, −0.007 (standard error = 0.028) for sales, and 0.001 (standard error = 0.003) for price. All estimates are statistically insignificant (p > 0.1), thus verifying that causal forest is effective in achieving comparability between the treatment and control listings.

5.4.2. Ruling out Locational Spillover of Treatment Effects. Recall that we construct the control group based on geographic proximity. Although geographic proximity is useful to ensure that the treatment and control groups share common time trends, a potential concern may arise in our case if the tax treatment within the City of Los Angeles can spill over to areas outside the city. For example, if some customers who originally wanted to book a listing within the city borderline of Los Angeles switched to book a listing slightly outside the city because they found the tax unacceptable, the demand for the control group would also be impacted by the tax policy, especially for areas close to the city borderline. We conduct two robustness checks to address this issue. First, we
extend the control region to also include Orange County (which includes 4,362 listings), making the geographic region under study the entire Los Angeles metropolitan area. By extending the control region, we include more listings that are not too close to the treatment region, hence alleviating the concern of spillover. Second, we remove listings that are located in zip codes along the city borderline of Los Angeles, so that the treatment and control regions are no longer contiguous, and spillover is less likely. In this analysis, 11,333 listings remain, including 7,604 treatment listings and 3,729 control listings. The results for these two robustness checks are reported in Tables B.1 and B.2, respectively, in Online Appendix B. In both analyses, our previous findings regarding the heterogeneous treatment effects around revenue, sales, and price, continue to persist.

5.4.3. Ruling out Contamination from Strategic Booking Behavior. Another potential issue is that if certain customers were aware of the implementation of the tax policy beforehand and strategically booked earlier than the tax date of August 1, 2016, to avoid the tax, whereas without being aware of the tax policy, they would have booked after August 1, 2016, then the treatment effect will be overestimated for the period shortly after the tax treatment. To rule out this potential concern, we conduct two additional analyses. First, we verify that the advance booking days did not change prior to the tax treatment. To this end, we focus on the two-month period immediately before the treatment and run our causal forest difference-in-differences analysis on the listings’ (log-transformed) advance booking days between the first month and the second month. The estimated average treatment effect is 0.007 (standard error = 0.031) and is statistically insignificant ($p > 0.1$). Second, we exclude the month immediately before and after the treatment and repeat our main analysis. The details are reported in Table B.3 of Online Appendix B and show similar results as our original findings around heterogeneous treatment effects. Combining these two pieces of empirical evidence, we conclude that strategic booking to avoid the tax is unlikely to have an impact.

5.4.4. Alternative Treatment Group. We next test the robustness of our findings regarding the choice of the treatment group. We replicate our main analysis for Los Angeles on a different city, San Diego, which introduced the tax policy on July 15, 2015. The results are reported in Table B.4 of Online Appendix B. In short, even though we switch to a completely different market, our main findings around revenue, sales, and price are robust in that we observe the same heterogeneous treatment effects between targeted and nontargeted listings.

5.4.5. Longer Time Period. To better understand the impact of the tax policy over a longer time horizon, we extend the analysis to 12 months after the tax treatment. Table B.5 of Online Appendix B reports the detailed results for this robustness check and indicates that the heterogeneous treatment effects persist in the longer term.

5.4.6. Zip Code–Level Analysis. We also evaluate our main findings when conducting the analysis on the zip code level instead of the listing level. For each zip code, we compute the difference between its targeted listings’ average performance change before and after the tax treatment and that of its nontargeted listings. We then run our causal forest estimation on these zip code–level differentials between targeted and nontargeted listings and use zip code–level covariates (which include market attributes and the aggregated listing attributes) for matching. The estimated average treatment effects, which now tell us the heterogeneous treatment effects between the two types of listings, are 0.135 (standard error = 0.081; $p < 0.1$) for revenue, 0.144 (standard error = 0.087; $p < 0.1$) for sales, and

Table 8. Heterogeneous Treatment Effects: Expanded Controls

<table>
<thead>
<tr>
<th></th>
<th>$\tau^{\text{REVENUE}}$</th>
<th>$\tau^{\text{SALES}}$</th>
<th>$\tau^{\text{PRICE}}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>TARGET</td>
<td>0.114*** (0.027)</td>
<td>0.140*** (0.029)</td>
<td>0.005 (0.005)</td>
</tr>
<tr>
<td>TARGET (+SF)</td>
<td>0.103*** (0.027)</td>
<td>0.128*** (0.029)</td>
<td>0.004 (0.005)</td>
</tr>
<tr>
<td>TARGET (+SF, +SD)</td>
<td>0.107*** (0.028)</td>
<td>0.125*** (0.03)</td>
<td>0.003 (0.006)</td>
</tr>
<tr>
<td>TARGET (+SF, +SD, +NYC)</td>
<td>0.104*** (0.031)</td>
<td>0.131*** (0.034)</td>
<td>–0.001 (0.006)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>N</td>
<td>18,517</td>
<td>18,517</td>
<td>12,728</td>
</tr>
</tbody>
</table>

Notes. This table reports the main heterogeneity results, where the listings in San Francisco (SF), San Diego (SD), and New York City (NYC) are added to the control pool one at a time. The table reports the results from a doubly robust regression of the causal forest estimated individual treatment effects on revenue (column (1)), sales (column (2)), and price (column (3)) against the covariates. Robust standard errors are reported in parentheses. The coefficients of the controls (which include listing and market attributes) are omitted. The complete tables are provided in Tables A.12–A.14 in Online Appendix A.

*p < 0.10; **p < 0.05; ***p < 0.01.
–0.002 (standard error = 0.027; p > 0.1) for price. Thus, we obtain substantively similar results when aggregating the level of analysis to the zip code level.

6. Prescriptive Analytics

Our empirical analyses have found that the original way of implementing the tax policy has led to an unintended consequence of overpenalizing the type of Airbnb listings that were meant to be protected while giving an advantage to the type of listings that the tax policy aimed to target. We have further uncovered that the main driver of this unintended consequence is customers’ discriminatory tax aversion. Thus, to achieve the goal of tax regulation more effectively, policy makers should take the customers’ discriminatory tax aversion into consideration and find better ways to curb the impact of such customer behavior. Moreover, the impact of the tax policy also depends on how effectively Airbnb hosts were able to respond by adjusting prices. It is important for Airbnb hosts to know what is the right price to charge after the tax regulation, and the resulting optimal tax pass-through is of interest to policy makers.

In this section, we provide data-driven prescriptions to improve these two kinds of decision making. In Section 6.1, we devise a tool that can yield customized price optimization for each listing to respond optimally to the tax regulation. In Section 6.2, we analyze what tax rate should be set for nontargeted listings, if the tax rate can be discriminatory, to offset the differential impact caused by the original tax policy. These prescriptive analytics will depend on correctly predicting the counterfactuals of policy making and market response. We use the predictive power of causal forest to achieve this.

6.1. Customized Price Optimization

Thus far, we have found that Airbnb hosts were generally reluctant to reduce prices after the occupancy tax was introduced. The price reduction was only ~3.2% during the six months after the tax treatment, resulting in only 26% of the tax being incurred by the hosts. This suggests the need to investigate whether the adverse impact of the tax policy can be alleviated by a more significant price response by hosts. Furthermore, because listings are heterogeneous, and hence having different optimal prices, it is important to conduct this analysis on the listing level and provide a tool for customized price optimization.

To find the optimal price response for each listing, we need to know the demand function (i.e., how the listing sales would react to different prices) at the listing level. To estimate the demand function, we make use of our causal forest approach. Recall that in our main analysis, we used the listings’ pretreatment performance to predict the treatment effects on sales and price simultaneously. We now include the previously obtained price treatment effects to the covariates set and predict the sales treatment effects. This allows us to obtain each listing’s sales treatment effect as a function of its price response and all other factors we previously considered. We then use the obtained causal forest to predict counterfactuals if each listing were to adjust its price differently. In particular, for each listing, we generate 31 counterfactuals with the price adjustment ranging from ~20% to 10% (with a step size of 1%). Using the obtained causal forest, we make out-of-sample predictions for sales under the counterfactuals. Finally, combining the obtained sales treatment effects and the associated prices, we can compute the revenue treatment effect under each counterfactual, and identify the optimal price response for each listing.

We obtain that the average optimal price response across all listings is ~7.2%. Compared with the actual price reduction of ~3.2%, the result suggests that on average a listing should have reduced its price by 4% more. With an average pretreatment price of $162.0, a 4% reduction would correspond to reducing the price by $6.5 more on average. Moreover, note that a 7.2% price reduction would indicate that the total tax-inclusive price is increased by (1 – 7.2%) × (1 + 14%) − 1 = 5.8%, leaving 5.8%/14% = 41% of the tax to the customers and 59% to the hosts. Thus, the optimal price indicates that a greater portion of the tax should be incurred by the host, which is in contrast with the hosts’ actual price responses. Importantly, such an overall price reduction would lead to a significant recovery of demand: under the optimal customized prices, sales would increase by 18.7% on average and revenue would increase by 12.8% on average.

Figure 3(a) shows the distribution of the difference compared with the actual price response across all listings is ~7.2%. Compared with the actual price reduction of ~3.2%, the result suggests that on average a listing should have reduced its price by 4% more. With an average pretreatment price of $162.0, a 4% reduction would correspond to reducing the price by $6.5 more on average. Moreover, note that a 7.2% price reduction would indicate that the total tax-inclusive price is increased by (1 − 7.2%) × (1 + 14%) − 1 = 5.8%, leaving 5.8%/14% = 41% of the tax to the customers and 59% to the hosts. Thus, the optimal price indicates that a greater portion of the tax should be incurred by the host, which is in contrast with the hosts’ actual price responses. Importantly, such an overall price reduction would lead to a significant recovery of demand: under the optimal customized prices, sales would increase by 18.7% on average and revenue would increase by 12.8% on average.

Figure 3(a) shows the distribution of the difference between each listing’s optimal price response and its actual price response. Most listings (91.5%) should have reduced their prices more (or by a similar amount), whereas a small portion of listings (8.5%) should have reduced their prices less. Among the listings that should have reduced prices more, the conditional mean of the optimal price response is ~7.8%. Correspondingly, the mean improvement compared with the actual price response is 20.6% and 13.6% for sales and revenue, respectively. Among the listings that should have reduced prices less, the conditional mean of the optimal price response is ~0.4% (i.e., these listings do not generally need to reduce prices). In this case, the optimal price response would lead to a mild reduction in sales (with a mean of 1.6% sales reduction compared with the actual price response), suggesting that demand is not sensitive to price for these listings. The overall revenue, however, would increase by 3.2%. Figure 3(b) shows the distribution of the
listings’ percentage revenue improvement by using the optimal price adjustments (truncated at 100%), which exhibits an exponential pattern.

6.2. Discriminatory Tax Rates
Although improved price responses are one way for Airbnb, hosts, and consumers to capture additional surplus after the introduction of the occupancy tax, recall that we also found that the primary driver for the overpenalization of nontargeted listings is the customers’ discriminatory tax aversion against such listings. One way to mitigate this effect is to charge different tax rates for the two types of listings. We now calibrate such discriminatory tax rates using a causal forest approach.

Our objective is to identify how much to reduce the tax rate for nontargeted listings to offset the tax’s differential adverse impact on demand for nontargeted listings. To find such a tax rate, we need to know how the demand for nontargeted listings would change if a different tax rate is imposed. We use causal forest to predict the listing counterfactuals under different tax rates. We now calibrate such discriminatory tax rates using a causal forest approach.

Our objective is to identify how much to reduce the tax rate for nontargeted listings to offset the tax’s differential adverse impact on demand for nontargeted listings. To find such a tax rate, we need to know how the demand for nontargeted listings would change if a different tax rate is imposed. We use causal forest to predict the listing counterfactuals under different tax rates. In this case, we include the tax-inclusive price (computed as the causal forest estimated price treatment effects plus the tax) to the covariates set and use causal forest to predict the sales treatment effects. This allows us to obtain each listing’s sales treatment effect as a function of its tax amount and all other factors we previously considered. We use the obtained causal forest to predict tax counterfactuals for nontargeted listings, where we generate 15 counterfactuals for each listing with the tax rate ranging from 14% to 0% (with a step size of 1%).

This analysis thus gives us the sales treatment effect \( \hat{\tau}_{\text{SALES}} \) for listing \( i \) when the tax rate is \( j \). Using these causal forest predictions, we run the following fixed effects regression to examine the impact of the tax rate on nontargeted listings:

\[
\hat{\tau}_{\text{SALES}} = \sum_{\eta=0}^{13} \beta_{\eta} \cdot 1(j = \eta) + \gamma_i + \epsilon_{ij},
\]

where \( \eta \) indexes the tax rate, and the \( \beta_{\eta} \) coefficients reflect the improvement on nontargeted listings’ sales treatment effects when the tax rate is adjusted to \( \eta \)% for them compared with the baseline case of a 14% tax rate.

Figure 4 plots the coefficients \( \beta_{\eta} \); the complete regression results are reported in Table A.15 in Online Appendix A. We can see that nontargeted listings will be able to recover more demand (i.e., \( \beta_{\eta} \) becomes more positive) when their tax rate is set at a lower level. Recall from column (2) of Table 4 that the sales treatment effect differential is 0.140 between the two types of listings (depicted by the horizontal line in Figure 4). To improve the sales treatment effect by 0.140 for nontargeted listings, the tax rate needs to be
decreased to 6% for them. Then, the differential tax rate would be able to offset the differential effect caused by the tax policy. Therefore, our recommendation is to reduce the tax rate to 6% for nontargeted listings (about half of the tax rate for targeted listings) to curb the discriminatory tax aversion against nontargeted listings.

7. Conclusion and Managerial Implications

In this paper, we investigate the effects of the occupancy tax policy on Airbnb listings in the Los Angeles market. Using a causal forest machine learning technique, we first find that the occupancy tax decreases revenues by 15% and sales by 12%. Furthermore, while the reductions of revenue and sales are more pronounced immediately succeeding the tax policy, the negative effects persist under longer-time horizons. We also find that hosts fail to reduce prices optimally in response to the tax.

Turning to our first primary research question, we then investigate the heterogeneous treatment effects of the tax policy. In particular, we determine how the tax policy affects targeted listings, defined as a host with more than one listing or a listing that is for an entire property, relative to other nontargeted listings. Our results suggest that the tax policy adversely affects nontargeted listings disproportionately, compared with targeted listings, in both revenues and sales. We then proceed to explore alternative explanations for this result, on both the supply side (price adjustments and listing exits) and demand side (price sensitivity and tax aversion) of the market. We identify a novel result in that customers’ discriminatory tax aversion appears to be the reason for nontargeted listings being more penalized by the introduction of the occupancy tax, relative to targeted listings. We posit that this may be because, by displaying the tax as a separate line item expense, Airbnb’s most unique listings (i.e., nontargeted) exhibit a feature that is identical to that of a traditional hotel, which diminishes their competitive advantage. Therefore, one immediate managerial recommendation from our study is that Airbnb may investigate alternative ways to diminish the salience of the occupancy tax (e.g., as part of the listing price or only in the final payment stage). This may not only mitigate the gap between the tax’s effect on targeted and nontargeted listings, but also lead to further increases in revenue and sales.

Leveraging our empirical results, we then turn to our second research question regarding prescriptive analytics. Specifically, because hosts were unable to adequately adjust their prices after the introduction of the tax, we take a data-driven approach to identify the optimal price response for each individual listing. By implementing such prices, we demonstrate that revenues would increase by 12.8% and sales by 18.7%. Managerially, this indicates that Airbnb may consider providing price support systems for hosts, which should directly contribute to a win-win outcome: hosts (and Airbnb) earn higher revenues while customers achieve higher welfare through increased sales.

Continuing with prescriptive analytics, we also investigate how discriminatory tax rates can offset the undue negative effects on nontargeted listings. That is, further utilizing causal forest we calibrate how alternative (lower) occupancy tax rates, applied to nontargeted listings, affect sales. We show that a discriminatory tax rate of 6% would nullify the inordinate effect for nontargeted listings. From a practical standpoint, while there may be challenges with implementing such tax rates, there is precedent for discriminating among types of listings. For instance, in Los Angeles there is recent legislation aimed at certain “rogue hotels” that rent more than 120 days per year (Daniels 2019). In addition, New York City recently started collecting the occupancy tax automatically through Airbnb for specific listings (e.g., hotel rooms) (Honan 2018). In this sense, our study provides an example of operations management research that generates implications for both managers and policy makers (Serpa and Krishnan 2016, Calvo et al. 2019).

It is important to note the limitations of our study. For one, despite the similarities between the City of Los Angeles and the remaining County of Los Angeles, we recognize that differences do exist (i.e., no two markets are identical). Although we attempted to mitigate this potential effect by (1) using causal forest to match at the listing level, (2) including a large set of covariates, and (3) including additional markets to demonstrate the robustness of effects, we cannot guarantee that unobserved factors do not play any factor. Another limitation is that we identify and prescribe a discriminatory tax rate for nontargeted listings, to offset the tax’s adverse impact on such listings. Yet, we do not explore other mechanisms (e.g., manipulating the display of the tax for certain listings) that might lead to similar outcomes. We consider this an exciting opportunity for future work, perhaps through experimental methods.

Last, recall that our definition of target listings is based on those characteristics that regulators appear to be most closely monitoring. During the latter stages of conducting our study, the City of Los Angeles began enforcing a law in late 2019 that specifically addresses such listings by requiring that, among other things, “only the host’s primary residence can be rented out, defined as the place where a host lives for at least six months per year,” which effectively bans targeted listings from operating on Airbnb in the City of
Contrary to removing these listings entirely, our study prescribes other potential solutions, such as imposing discriminatory tax rates. Importantly, we believe that these are not only applicable to Los Angeles, but other major cities struggling with the best ways to manage the Airbnb market. Aside from this, when it comes to identifying solutions, we note that there need not be an incentive conflict between Airbnb and policy makers. For example, Kaplan and Nadler (2015) show that in the New York City market, Airbnb and the Attorney General’s office were able to come to an agreement that allows Airbnb to continue to protect the privacy of its hosts while also providing the state with enough data to identify those listings in violation of state law. With regards to the occupancy tax, by collaborating and discussing some of our recommendations, we believe that both Airbnb and regulators may be able to advance their own interests and identify further win-win outcomes.

Acknowledgments
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Endnotes
1 Airbnb discloses the areas worldwide where it has made agreements with governments to collect and remit local taxes on behalf of hosts, as well as the tax policy details; see https://www.airbnb.com/help/article/2509/in-what-areas-is-occupancy-tax-collection-and-remittance-by-airbnb-available. Table A.1 in Online Appendix A summarizes relevant Airbnb occupancy tax policies in major U.S. cities (up to 2017, the start time of this study).
2 However, the weekly ratio of transacted listings is identical between the two groups (0.25 for both), suggesting that the market competitiveness (as measured by the ratio between demand and supply) is comparable.
3 To account for zero revenue/sales in weeks with no bookings, we take the log transformation in the form of log(REVENUEt+1) and log(SALESIt+0.01) for weekly revenue and sales, respectively, throughout the paper. The results are robust if a Poisson regression specification is used.
4 Table A.6 in Online Appendix A reports the variable importance returned by causal forest (i.e., the probability of a variable being selected in the tree-growing process). This allows us to examine which covariates have higher predictive power. First, all trend variables have high predictive power, consistent with achieving parallel trends for difference-in-differences estimation. Second, market attributes carry higher weights than listing attributes. Given that the main differences between the treatment and control listings arise from market differences, this indicates that causal forest may be effective in constructing comparable listings. Third, performance attributes carry higher weights than other types of attributes, suggesting that causal forest may help tease out the potential demand differences.
5 We further find that price started to stabilize after 7 months, and the average price treatment effect during the 12-month post-treatment period is -0.042 (standard error = 0.002). The average treatment effect during the 12-month post-treatment period is -0.150 (standard error = 0.010) for revenue and -0.121 (standard error = 0.010) for sales, both of which are highly consistent with the 6-month average treatment effect.
6 During our time period of study, 26.4% of Airbnb hosts in the City of Los Angeles owned more than one listing, who altogether controlled 51.3% of listings in the city.
7 During our time period of study, 35.3% of Airbnb listings in the City of Los Angeles were shared-space listings.
8 Under this definition, 84.7% of listings are categorized as targeted listings.
9 Another variable of interest is whether a listing is booked for a significant portion of the year. We consider an alternative definition for targeted listings by taking this into account in Online Appendix B.1.
10 We exclude the first month during the post-treatment period when the hosts have not started to adjust prices.
11 We use San Francisco as the control group for this analysis. Because of data availability, we are not able to use a more ideal control group that has not been treated before San Diego and is also geographically closer.
12 Other restrictions are in place as well. For instance, a host may not rent out their property for more than 120 days a year. Also, non-residential structures such as trailers and RVs cannot be listed.

References


