ESSAYS ON THE AIRBNB MARKET: SUPERHOSTS AND LOCAL POLICY DETERMINANTS AND EFFECTS

by

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To my hero, my biggest fan, and my best friend, Kayla.

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ABSTRACT

This dissertation is composed of three distinct empirical analyses, separated by chapter. Chapter I examines the impact that salient information has on host and guest decisions in the sharing economy using data from Airbnb from December 2018 to April 2019. Airbnb's Superhost badge offers a shortcut to consumers searching for high-quality sellers. By estimating the impact of acquiring the badge separately from the conditions that Airbnb uses to merit Superhosts, I isolate the effect of salience of the information the badge carries via OLS and fixed effect panel models. The result is a negligible increase in price by sellers, but a growing effect on the number of reviews, 0.10 and 0.27, one and two months after earning the badge, respectively. I estimate the effect on revenues is between 10% to 17% increase from increased bookings by guests.

Chapter II asks why local governments pass restrictions on short-term rentals, such as Airbnb. I construct a novel classification of these laws passed by cities. I use panel binary probits and ordered models to predict the marginal effects of local economic conditions on short-term rental restrictions using data from 2012 to 2019 in nineteen U.S. cities. I find that a one standard deviation decline in housing affordability leads to a 20.57 percentage point increase in the likelihood that a city council restricts in a specific approach that only personal residences may be operated as a short-term rental. Alternatively, a one standard deviation increase in affordability predicts a 23.78 percentage point increase in the likelihood that no restrictive policy is passed.

Chapter III identifies the effect of policies aimed at reducing short-term rental supply. I use fixed effects panel models with five years of data from Airbnb to show the

casual response of professional and nonprofessional suppliers in the United States from changes enacted through city law. I find these restrictions do little to reduce professional supply but significantly affect nonprofessional hosts, reducing availability by as much as -15.7%. A 1 percent increase in permit fees leads to a -0.7 percentage point decrease in professional's supply. Yet for nonprofessionals, the same 1 percent increase leads to a percentage point increase of 1 to 1.2 percentage points in supply. My paper expands the limited and conflicting empirical research of local policy aimed at short-term rentals by offering robust methods to disentangle the heterogeneous effects by host type.

TABLE OF CONTENTS

LIST OF TABLES

CHAPTER III THE HETEROGENEOUS EFFECTS OF GOVERNMENT RESTRICTION AND FEES ON AIRBNB SUPPLY

CHAPTER I

SALIENCE IN THE SHARING ECONOMY: EVIDENCE FROM AIRBNB BADGED SUPERHOSTS

1 Introduction

Sharing economy websites, such as Airbnb.com, often offer badges for high quality sellers. These simple icons associated with products and sellers allow consumers to quickly identify differentiated sellers among a sea of options. While traditional consumer theory suggests that such a badge should not influence a consumer's demand if the information the badge conveys is already available and of low cost to obtain, behavioral economic studies show that the salience of information can have a large effect on consumer choices (Lleras et al. 2017; Bordalo, Gennaioli, and Shleifer 2013). This paper examines the impact that Superhost status of Airbnb home rentals has on revenues by isolating the effect of the salient badge.

Airbnb is an increasingly popular platform where hosts provide guests accommodations from as simple as a shared room to a complete home (Wang & Hung, 2015). Until recently, all the properties for booking on the website were provided by hosts who create an online profile of their home. Guests search for properties, henceforth referred to as listings, that match their needs for their trip by viewing photos, reading descriptions written by the host, reviewing the price and available dates, as well as feedback mechanisms such as review scores and responses from previous guests.

Airbnb offers an ideal quasi-experiment to analyze the effect of salient badges in peer to peer websites and applications, which include many forms of labor and resource sharing. Data can be collected in rich formats via web scraping providing precise estimates of statistical significance and the ability to control for relevant factors. Purchases represent significant transactions for consumers, where buyers are likely to spend a considerable amount of time searching for products that meet their demands for limited vacation time with family or friends. Airbnb is also a giant in the tourism industry, while privately held, it was estimated to be valued at $38 billion by Forbes[1] which rivals the most valuable hotel companies in the world.

Airbnb.com[2] states on its website that Superhosts are "top-rated and most experienced hosts." Consumers might believe that renting a night's stay at one of these differentiated listings provides additional protections, however, the website does not provide institutional backing or guarantees of service. Achieving the websites badged status requires that hosts must meet four conditions: an average review score of 4.8 out of 5 stars, ten bookings, limited cancellations, and high response rates with quick response times to customer inquiries. All these elements of a host are easily viewed on the listing website. Consumers can easily understand these aspects about a host without needing the badge to confirm them. I provide more detail about these conditions in section 3.

[1] Forbes - https://www.forbes.com/sites/greatspeculations/2018/05/11/as-a-rare-profitable-unicorn-airbnb-appears-to-be-worth-at-least-38-billion/#559588b22741

[2] https://www.airbnb.com/superhost

Along with Airbnb, I hypothesize that each of the four conditions are associated with a rightward shift in the demand curve and would produce a higher price and/or higher quantity sold. For example, a host who becomes more responsive to guests can be economically interpreted as an increase in the value of the stay. Alternatively, a listing that receives an increase in its review score rating can be interpreted as higher quality than before through improved customer satisfaction with the listing. I argue that the badge represents the salience of the information of an increase in the quality of the listing.

To test the impact of the Superhost badge, I estimate both cross sectional and fixed effect panel models to isolate the badge's impact on revenue. The dependent variables estimated are price and number of reviews, which serves as a proxy for quantity, i.e. frequency of booking. To compare my data to the existing literature, I draw my results from a sample 20 U.S. cities at the zip code level of observation over 5 time periods – December 2018 through April 2019. Because an increase in quality should shift the demand curve right, I isolate the increase on quality by separately estimating the coefficients of the conditions Airbnb uses to award the badge. This allows for econometric exploitation of the exogenously awarded badge, where hosts take no action in applying or being considered for the badge. The fixed effect panel regression allows me to observe the actual price and review frequency variation as measures of quality of change.

I find that the OLS regression results align with the existing literature, where both price and number of reviews have large positive coefficients for Superhost. However, like previous literature, it is unclear if these coefficients disentangle the effect of increases in quality from the salience of the badge. When I control for high quality listings in my second OLS regressions, the effect on price almost entirely disappears, which aligns with the panel results. The impact on quantity sold (proxied via number of reviews) is large and significant across both the OLS and panel models, indicating that hosts who earn the badge earn a sizeable increase in their frequency booked. In the fixed effects models, the effect is insignificant in the month in which the badge is earned, however it grows thereafter up to 0.27after the badge is earned. I estimate an effect from 10% to 17% increase in revenue solely from the increased booking from the salience of the badge for those hosts who can earn it.

This paper's unique contribution to the literature is identifying the value of salience of information in the sharing economy, all other aspects of quality held equal. To the best of my knowledge this paper is the first to use panel methods to observe actual changes at the individual listing level, observing hosts price setting behavior and consumers quantity purchasing behavior.

This paper continues as follows. Section 2 provides a brief literature review. Section 3 explains in more detail the measures and assignment of Airbnb Superhost badge. Section 4 describes the empirical methods used. Section 5 presents data. Section 6 reports results. Section 7 includes a short discussion. Section 8 concludes.

2 Literature review

Demand for quality differentiated products is a well-researched subject within the industrial organization literature. Airbnb's Superhost listings can be thought of as vertically differentiated products, where higher quality products shift the demand curve right. Philips and Thisse (1982) show that the value of vertically differentiated products for all consumers is strictly increasing in terms of quality. Higher quality goods provide increased utility to all consumers.

Mazzeo (2002) finds that differentiated products earn higher revenues through the ability to charge higher prices. From these studies consumers are rational when they are willing to pay higher prices for the characteristics that Superhosts are assessed on. Considering hotels specifically, Lockyer (2005) identifies that price is a core component of decisions of business and leisure travelers. Consumers are sensitive to price and have varying willingness to pay for increases in quality. Hung et al (2010) find that hotel demand is driven by star ratings, room attributes, cleanliness, and amenities. Byers, Proserpio, and Zervas (2017) conduct a panel analysis showing that Airbnb listings are indeed substitutes for hotel stays, most closely competing hotels that cater to leisure-oriented travelers and lower tier quality hotels.

An overnight accommodation can be viewed by its individual features, however, behavioral economics literature has shown that the salience of information can also have significant impact on purchase decisions (Masatlioglu, Nakajima and Ozbay 2012). Lleras et al (2017) identify how decision makers limit a large set of potential choices. The most relevant limiting step to my analysis is “narrowing down” where the decision maker chooses criteria to eliminate options. I theorize that this also applies in consumer searches for Airbnb listings. Rather than spend a modest amount of time searching for the listings that meets their preferences, many guests specifically rule out listings that are not

Superhosts without considering the underlying quality of the hosts who earn the badge. As Bordalo, Gennaioli, and Shleifer (2013) explain, a "fundamental feature of decision making, namely, that the consumer's attention is drawn to—and his choice is shaped by—the most salient aspects in the choice context he faces."

Airbnb broadly has been studied by several researchers and a nice summary and scope analysis of the existing literature is provided by Dann, Teubner, and Weinhardt (2019). Chen and Xie (2017) argue that the two primary drivers of Airbnb are consumers shopping for affordable products and hosts monetization of homes. Guttantag (2015) argues that Airbnb's growth is due to its distinct attributes - cost savings, household amenities, and authentic local experience.

To my knowledge, five prior studies have specifically estimated the independent relationship of Superhost on price. Chen & Xie (2017) use data from Austin Texas to create an exhaustive hedonic model based on all immediately observable characteristics of listings. They find that the badge's effect on the price is positive but not statistically significant, stating "It seems that consumers are able to isolate the influence of host quality from committing the price they are willing to pay." Alternatively, Wang & Nicou (2017), Teubner et al. (2016), and Gibbs et al. (2017) find that the badge has both a positive and significant effect on price. Wang & Nicou (2017) conduct an OLS and quantile regression, finding that 24 out of 25 of their assessed variables influence price, including Superhost status with a positive 8.37% increase at the .01 p-value significance level. Teubner et al. (2016) use cities in Germany to estimate a hedonic model where they theorize that reputation mechanisms of Superhost, pictures, verifications, etc. influence price. They find being Superhost increases price in Germany by $2.97 per night. Based on their sample mean price of $159.90, this would indicate a 1.86% increase.

The above studies use cross sectional data which are unable to show causality due to the possible bias in the quality of listings. The only study that I am aware of that uses panel data is Neumann and Gutt (2017) which provides both a theoretical model for setting optimal listing prices as well as empirical results. Their panel data analysis suggests that hosts raise prices modestly as a reaction to receiving five reviews (0.489%), ID verification (1.59%), and Superhost status (0.672%).

Examining number of reviews by using an OLS cross-sectional model with listings from Hong Kong, Liang, Schuckert, Law, and Chen (2017) find that Superhosts are more likely to receive reviews. They argue that hosts dedicate more time and energy to earn and retain the badge than a host might otherwise without the possibility of earning the badge. They, along with Teubner et al. (2016), hypothesize that the Superhost badge is a reputation mechanism, akin to a signaling model. However, from their research it is unclear what costly action is being taken to show that listings are high quality, since these same studies find positive coefficients associated with the conditions needed to achieve Superhost. Because the information is already free and available, I argue that the host is not providing any new information, but rather the fact that this information is highlighted to interested guests creates a salient information effect.

An analysis which distinguishes the effect of the increase in quality from the salience of the information that the badge conveys has yet to be conducted. The Airbnb states on its website[3] that Superhosts earn 22% higher revenues than non-badged hosts. Though, little information is given on the company's website about how this figure was calculated. The above econometric studies have used regressions to estimate the value of the badge among other listing features, yet the influence on price shows mixed results, and the cross-sectional nature of these studies makes causal impact not possible to determine. While previous economic studies argue that the badge is a reputation mechanism, where hosts are signaling value, I argue the salience of the badge is separable from the conditions it is comprised of. I isolate the effect of the badge's salience using panel models to disentangle increases in quality from the awarding of the badge.

To the best of my knowledge, no analysis has been completed specifically to separate the salience of the Superhost badge on revenues, and thus, seek to isolate the effect of its salience from the underlying components. In addition, previous research has not incorporated panel data to show causal relationships. The existing evidence is mixed regarding the impact the badge has on price and frequency of reviews when adequately controlling for its four conditions.

3 Airbnb Superhost Status

This section describes the website's badge in more precise detail. According to the website,[4] as of June 2019, hosts must meet four conditions: an average review score of 4.8 out of 5 stars, ten bookings, limited cancellations, and high response rates to customer inquiries. Over the years, these requirements have changed slightly, however

[3] https://www.airbnb.com/superhost

[4] https://www.airbnb.com/superhost

the explanations in the paper reflect the requirements during the observation periods I use. The status is evaluated quarterly for every host. Listings whose host meets the conditions within the last 12 months will have the badge appear on their page for the next quarter. Each of these aspects are explained below.

The first condition Airbnb specifies is that its hosts must have a 4.8 star overall rating out of 5 stars. These ratings are viewable to anyone searching online for listings. Ratings can only be written by verified guests from the Airbnb website. Guests also provide star ratings for qualities such as accuracy and cleanliness, as well provide a written review. The overall rating that is used for Superhost is a separate, non-aggregate guest measure of the listing. The rating is viewable from the listing's page, along with specific aspects of the properties such as location, value, and host communication. An important note regarding the star rating is that most consumers, who I presume lack technical knowledge about reading web pages source code, can only see the rating rounded to the nearest half star values. For example, a consumer would see 5 stars, 4.5 stars, 4 stars, etc. As a researcher, however, the web scraped data is available in a more precise format from 0 to 100 star rating. As an example, a score of 96 would mean a 4.8 out of 5 stars. During the time of my observation, Airbnb also required that 50% of guests must leave a review, though this requirement has recently been removed as of July 2019.

The second condition is that the listing must have ten or more stays within the last twelve months. An alternative condition, intended for longer term rentals, is also able to fulfill this requirement with 100 nights over at least three stays. Consumers are not able to directly view the number of stays as a metric on the listing page. However, this information can be inferred by viewing the count of reviews available on the listing page. Because reviews can only be written by guests who have completed a stay, a listing with ten reviews must have had ten stays at a minimum. Even without ten reviews, it is possible to infer the number of stays as a multiple of the number of reviews. For example, if 50% of guests leave a review, the consumer could reasonably infer that five reviews indicates ten stays have been completed.

The third condition is that the host must have a 90% response rate within 24 hours to initial customer inquiries. On the listing's page, the consumer can see this information in bucketed categories, such as the host responses "within an hour", "within a few hours", "within 24 hours". Also next to the host information is their response rate, which ranges from 0 to 100%.

The fourth and final condition is the host cannot have canceled any stays within the last 12 months, except for "extenuating circumstances," which the website lists are things including death, serious illness, severe damage, and natural disasters. This does not include reservations when guests themselves cancel. This condition, unlike the others, does take a small amount of searching to determine. Regardless of the reason, whenever a host cancels a reservation, the website automatically creates a review that cannot be removed[5] which states that "The host canceled this reservation … days before arrival." To check for previous reviews, a consumer can easily use the search feature embedded in the review section to find reviews with "cancelation" to return all instances within the past. While this quality could reasonably be harder for guests to know before choosing a

[5] https://www.airbnb.com/help/article/314/why-did-i-get-a-review-that-says-i-canceled

listing, cancelations are also rare. In my summary statistics I observe that 8% of listings have had a cancelation in the last 12 months.

Taking these four conditions, Airbnb evaluates for Superhosts on a quarterly basis - January 1st, April 1st, July 1st, and October 1st. Hosts can both gain or lose the badge based on the conditions for the last twelve months. According to their websites, the evaluation is automatic and applied to all listings managed by the host, requiring no action from hosts. There is no minimum requirement for how long the listing has been available. The evaluation is conducted on a 12-month rolling status. For example, on April 1st, 2019, the website checks if conditions were met from April 1st, 2018 to March 31st, 2019. Any listings managed by a host that meets the conditions would receive the Superhost badge displayed at the top of the listings page, as well as displayed on the listing summary available when searching for available listings. The badge is applied for an entire calendar quarter, until the next evaluation period where it is either continued or removed.

4 Empirical Methods

To test the revenue impact of the Superhost badge I use three sets of regressions - full sample OLS, a limited sample OLS, and fixed effects panel regressions. To show the impact on revenues, I estimate two dependent variables, log of price per night and number of reviews. I use log of price because I expect the independent variables to have a percentage increase effect rather than a linear increase on dollar price. Number of reviews serves as a proxy for number of bookings.

While number of bookings is the most appropriate metric because it can directly be interpreted as quantity sold, this information is not available to consumers or myself as a researcher. Using private Airbnb data Fradkin, Grewal, and Holtz (2019) observe that an average of 71.7% of guests leave reviews. I am unaware of if there are trends or changes is this frequency of reviews. A back of the envelope calculation could derive number of bookings by simply dividing reviews by 0.717 However, while the website has a standard practice of reminding guests to leave a review, it is possible for hosts to take strategic action in hopes of increasing the number of reviews. If the average listing does receive 71.7% response rate, then this may be valuable to increase reviews for some hosts. This behavior would bias the OLS estimates with Superhosts appearing to have higher than actual reviews. A weaker assumption that review rates are roughly equal on either side of the Superhost threshold is plausibility met by the fixed effects panel model. While some hosts may take strategic actions to encourage reviews, they are likely to remain consistent in this, which would be estimated in the individual intercept term of the fixed effect on number of reviews.

$$y_{imt} = \alpha_{mt} + \gamma Superhost_{it} + \beta X_{imt} + \varepsilon_{imt} \qquad (1)$$

The first two sets of regressions I run are OLS models specified in equation 1. The *i* subscript represents the specific listing and the *m* subscript represents the market of the listing. I estimate the market at the level of zip code, which allows for unique effects by location. I estimate an intercept at the market level to control for desirability of the location, as demand can vary significantly for Airbnb market (Zervas et al. 2017) and finer specificity within a city allows for smaller error. I use all times in my observation period and allow for city time cross effects which allows for changes in seasonal demand. Zervas et al. (2017) find significant variance in demand by city, such as Austin's SXSW festival.

The independent variable of interest is *Superhost*, which is a dummy taking on 1 if the listing currently has the Superhost badge. If the listing has met the conditions but not yet passed the quarterly review date, this variable takes on a value of 0. As addressed above, the salience of the information the badge can be interpreted as the γ coefficient, as the characteristics that qualify a Superhost are controlled within X matrix.

Included in the X matrix are each of the 4 conditions - the overall listing rating, a dummy for 10 or more reviews, a dummy for existence of cancellations, and dummy for response rate over 90%. The salience of the information is represented by the γ coefficient. β represents the shift in demand as a result of the quality increase. Additional controls included in the X matrix are the number of guests accommodated and a dummy for entire home (as opposed to shared or private rooms), as these variables could influence the dependent variable and may bias the outcome if they are correlated. For example, accommodating more guests would be expected to raise the price.

I estimate the two sets of these OLS regressions to contrast the coefficient results of using the full sample with a limited sample. The first set, which is the full sample is the approach taken by many researchers in the existing literature (Chen and Xie 2017; Wang and Nicou 2017; Teubner et al. 2016). These regressions are exposed to potential bias due to the endogenous effect of high-quality listings, which may influence the dependent and independent variables at the same time. The second set of regressions is the limited sample, which is taken by selecting only the listings that meet Superhost requirements. I construct this reduced sample using the conditions that Airbnb states on its website. More detail is provided in the data section. By only including the Superhost eligible listings, this eliminates the potential endogenous effect of high-quality listings. The source of variation exists for two reasons. First, a listing may have only recently achieved the metrics necessary to become a Superhost but have not yet been evaluated by the next quarterly date. Second, a qualifying listing may be managed by a host who has other listings which brings down the host's metrics such that they are not eligible for Superhost status for their host account overall. Both these sources of variation are ideal in their exogenous nature, which again removes potential bias of the results.

The final set of regressions I run are fixed effects panel models (equation 2), which use the variation across times to estimate coefficients. This is an ideal method of estimation, instead of the OLS model, as it observes the direct outcomes of listings who flip Superhost status in addition to other variables that change. Only the variables in the observations that change over the observation period are being used to estimate the coefficient.

$$y_{imt} = \alpha_i + \alpha_{mt} + \gamma Superhost_{imt-n} + \beta X_{imt} + \varepsilon_{imt} \quad (2)$$

The panel model uses the same dependent, independent, and cross time-market variables as the OLS models. An additional element which I add is a delayed effect term. Because guests can and will likely book their trips in advance, I introduce the *n* term, which allows for changing to a Superhost to effect *n* periods in the future. In my model, if *n* equals zero, it tests the effect of the badge acquisition on reviews in the same month, for example. I expect to be relatively small due to the time for increased bookings to occur. If *n* equals one, it would be the effect in the month after a host earns the badge, and when *n* equals two, it estimates is the effect 2 months after the host earns the badge. An important trade off estimated this delayed effect is the reduction in observation periods, as estimated the effect in the future truncates the number of periods that I can observe the effect.

5 Data

Airbnb provides extensive details publicly on its website about listings and their respective qualities. This allows for rich data scraping by organizations such as InsideAirbnb.com, a third party who provides rich tables on a monthly basis of all available listings in twenty two United States cities, as well as five Canadian cities and over twenty other cities in Europe and Asia. To gain enough sample size and demand variation, I use all listings within the twenty US cities[6]. As Zervas et al. (2017) importantly note, to the econometrician, it is possible to falsely identify listings who have exited the market yet not removed their respective page from Airbnb. To account for this in line with their analysis, I remove listings without a review in June 2018, 6 months

[6] Los Angeles, New York, San Diego, Austin, San Francisco, Las Vegas, Chicago, Boston, Portland, New Orleans, Nashville, Providence, Columbus, Pacific Grove, Asheville. Oakland. Rhode Island, Salem OR, San Jose, Santa Cruise

before my first period of observation.

I use 5 monthly periods, from December 2018 to April 2019. This provides two time periods with the potential highest number of Superhost qualifying but non-badged listings – December and March. These are the months immediately before badges are awarded on the quarterly cycle. To construct markets, I use the zip code on the listing's webpage. This allows me to isolate price and frequency of reviews at a finer level than using alone city can provide. To operate within computer memory constraints and still estimate market-time cross effects, I select from largest one hundred zip codes by number of Airbnb listings. This results in 64,695 unique observations. Summary statistics are provided in Table 1.1. For my regressions that only use Superhost qualifying listings, I limit the sample size by constructing the four necessary conditions based on the Airbnb website and rule out non-qualifying listings. This constitutes 37% of the total sample.

Price is taken as the listed per night price. Hosts can require a minimum number of nights booked (the median is 2), as well as an additional cleaning fee per stay. It is also possible for hosts to change their prices for specific dates. While some strategic behavior is possible, in line with previous research (Chen and Xie 2017; Wang and Nicou 2017; Teubner et al. 2016) I assume that there is no distinction between Superhosts and non-Superhosts around the behavior of price setting by individual night. Number of Reviews is estimated by observing the number of reviews from the previous month and subtracting that from the number of reviews in the current month.

Table 1.1

Summary Statistics

Variable	n	mean	sd	median	min	max
Log price	251163	4.88	0.71	4.83	2.30	9.21
total number of reviews	251163	46.42	64.46	21.00	1.00	828.00
ave. reviews per month	251162	2.18	1.92	1.63	0.02	18.00
review score rating	248677	94.97	6.82	97.00	20.00	100.00
response rate > 90%*	212278	0.91	0.29	1.00	0.00	1.00
host responds within day*	251163	0.84	0.37	1.00	0.00	1.00
cancellations in last 12 mo.*	251163	0.08	0.27	0.00	0.00	1.00
Superhost*	251163	0.39	0.49	0.00	0.00	1.00
Superhost eligible*	210583	0.37	0.48	0.00	0.00	1.00
Superhost eligible* (disregarding cancellations)	210583	0.39	0.49	0.00	0.00	1.00
ten or more reviews*	251163	0.65	0.48	1.00	0.00	1.00
entire home*	251163	0.71	0.45	1.00	0.00	1.00
no. accommodates	251163	3.93	2.58	3.00	1.00	34.00

*Notes: S*tatistics are provided for number of unique listings times the number of observed periods
An * indicates a binary variable where 1 = true and 0 = false

An additional variable that required some effort to create was the cancelations within last 12 months. There is no binary indicator on the listing page, so instead I constructed this by performing a text analysis of all cancelations within 12 months of the observation period. If there was a posting about an automated cancelation, the dummy variable was turned on. My construction of this variable is likely to be more unforgiving than Airbnb's internal methods for cancelations because I do not have purview into the extenuating circumstances when hosts can cancel without penalty, as hosts can defend a justifiable cancellation. For this reason, I construct a Superhost eligible variable that disregards the cancelation condition, and the result is 2% increase in potentially eligible listings, up to 39%.

Descriptive Statistics are provided in Table 1.2, showing the variables of listings in the first period, December 2018. The left column, All Obs, includes all listings that were present in the first period. The next three columns break down the observations into three categories – those listings that were never a Superhost during the observed five periods, those listings that switched from having the badge to not having it or vice versa, and lastly those who had the badge for the entire five periods. 4,509 observations chanced status during the observation months (December 2018 - April 2019), representing 11.2% of the sample.

Table 1.2

Descriptive Statistics

	All Obs	Never Superhost	Switched Superhost	Always Superhost
n	40,074	22,602	4,509	12,963
ave. log price	5.158	5.122	5.201	5.203
ave. reviews/month	2.291	1.811	2.587	3.027
ave. # reviews	46.283	32.918	35.323	73.397
ave. # accommodates	3.819	3.723	3.965	3.935
% entire home	0.698	0.677	0.718	0.726
age of host (months)	52.541	51.469	48.773	55.721
age of listing (months)	26.952	24.943	24.045	31.465

There are a few interesting differences to note. Those who remained Superhosts throughout the observation period have on average much higher number of reviews. This could reflect the older age of the listing and/or more reviews earned by Superhosts. Reviews/Month shows that Superhosts earn about 1.2 more reviews per month than the

Never Superhost group. However, when comparing those who were always Superhost to those who switched to Superhost, the increase in average number of reviews is 0.44. Examining the prices of the groups, it appears there is a large high quality price premium ($181.87 for those were always Superhosts compared to $167.61 for those that never attained Superhost status during the observation periods. The same effect appears negligible when comparing those that currently have the badge with those that are soon to earn the badge, $181.72 and $181.54 respectively.

6 Results

First, I examine the OLS results for the natural log of price as the dependent variable. The results are displayed in Table 1.3, regressions (1) and (2). The sign of the coefficient is switched between the regression that uses all observations (1) and the Superhost eligible population (2). For all observations, the coefficient is 0.058 with high significance indicating that those listings that have the super host badge set price 5.8% higher. As discussed previously, however, this value may be biased due to not controlling for high quality listings. The coefficient for Superhost eligible listings is surprisingly -0.033, with high significance indicating that of the listings that are of high enough quality to achieve Superhost, hosts set price 3.3% lower ceteris paribus. Overall, the effect of Superhost on price appears ambiguous when comparing the two regressions. The panel results will provide further evidence on the badge.

Table 1.3

OLS Regression Results

	Dependent variable:			
	logged price		num. reviews	
	all obs	sh eligible†	all obs	sh eligible†
	(1)	(2)	(3)	(4)
Superhost	0.058***	-0.033***	0.395***	0.768***
	(0.002)	(0.004)	(0.014)	(0.026)
ten or more reviews	-0.108***	-0.025**	1.601***	0.802***
	(0.002)	(0.011)	(0.010)	(0.052)
host responds within day	0.048***		0.292***	
	(0.010)		(0.032)	
response rate > 90%	-0.057***		0.736***	
	(0.004)		(0.015)	
cancellations in last 12 mo.	0.043***		-0.322***	
	(0.004)		(0.019)	
review score rating	0.007***	0.047***	0.001*	-0.082***
	(0.000)	(0.001)	(0.001)	(0.008)
no. accommodates	0.139***	0.146***	-0.034***	-0.082***
	(0.001)	(0.001)	(0.002)	(0.005)
entire home	0.470***	0.417***	0.130***	0.299***
	(0.003)	(0.004)	(0.015)	(0.029)
Observations	210,583	78,189	158,654	62,256
Adjusted R2	0.992	0.993	0.533	0.581
F Statistic	55,980.700*** (df = 444; 210139)	25,472.870*** (df = 441; 77748)	512.954*** (df = 353; 158301)	247.682*** (df = 350; 61906)

Notes: Market Intercepts and Market-Time Effects not displayed.
Errors cluster at the market level. † I also run (2) and (4) using Superhost eligible disregarding the cancellation criteria. The results have virtually no impact on magnitude, sign, or significance with at most a 13% change in the magnitude.
*p<0.1; **p<0.05; ***p<0.01

The other variables that Airbnb uses to identify Superhosts have unexpected signs as well. For example, having ten or more reviews has a significant negative value for (1) and (2), though the effect is much smaller in (2). This implies that hosts with more reviews have a lower price, which could be explained by a downward sloping demand curve. Host responding within day also has a positive effect, indicating more responsive hosts set prices higher. Hosts who have a 90% response rate or higher appear to set their prices lower by 5.7%. One possible explanation is that inexperienced or bad hosts may set their price higher because of limited knowledge about optimal price setting behavior. Again, there is likely a bias in the not controlling for high quality listings in equation (1). Each of these variables are unavailable in regression (2), because Superhost eligible listings do not have variation in these variables. The control variables of entire home and number accommodates have large and significant effects, as expected from the previous literature, and are consistent across (1) and (2). Additionally, review score rating, a component of Superhost eligibility is positive and significant. For example, an increase of one-half star in rating would result in a 7% and 47% increase in price for models (1) and (2), respectively. While these estimates are very different, I suspect the value from model two is more accurate.

Next, I examine the OLS results using number of reviews as the dependent variable. Here, the sign of the coefficient for Superhost is consistent between both the all observation regression (3) and the Superhost eligible (4), with values 0.363 and 0.678 respectively. Here, the coefficient is interpreted as how many more reviews earned in the last month are explained by the independent variable. For number of reviews, it appears that Superhost has a large impact, with even more impact for high quality listings that meet the conditions for eligibility, .678 more reviews per month. Taking the review rate from Fradkin et al. (2019 of 71.7% of guests leaving reviews, this translates to 0.94 more reviews per month. However, one should use caution interpreting this as an increase in quantity sold. As mentioned in section 4 Empirical Methods, some hosts may take strategic behavior to gain more reviews, which would bias the results upward if more hosts that earn the badge go to extra lengths to increase their review rate. The panel models again will provide more evidence.

Examining the other coefficients on number of reviews, the signs are consistent with the badge having a salient effect. Those with ten or more reviews earned 1.504 more reviews in (3) and 0.722 in (4), indicating that listings with a small minimum of reviews book receive more reviews. Hosts that respond within a day receive more reviews as do those that have higher response rates. Hosts that cancel on guests receive 0.245 fewer reviews. The control variables of number of guests accommodates and entire home have relatively small magnitudes, indicating that these are unlikely to largely effect how often the listing is reviewed.

Next, I examine the results of the fixed effect panel models (5) - (8) presented in Table 1.4. Regression 5 shows the actual price setting behavior observed the same month of the variables change. Here, the acquisition of the badge leads to an increase in the price of 0.3%, a small value significant at the .05 but not .01 level despite the large sample size of over two hundred thousand observations. Interestingly, the four conditions for Superhost are all small magnitudes and not significant at the .01 level, except for ten or more reviews. Given the nature of fixed effect model and the large sample size, it appears that hosts generally do not change their price with any statistically meaningful way for changes in quality. The two control variables have the expected positive sign with significance. It appears that only upon earning the badge itself, and not its respective components, do hosts bump the price up by the price by a very modest 0.3%. This is line with the ambiguous and small magnitude of the coefficients for the OLS models.

Table 1.4

Fixed Effect Results

	Dependent variable:			
	log price (5)	Reviews Same Month (6)	Reviews 1 Month After (7)	Reviews 2 Month After (8)
Superhost	0.003**	0.002	0.133**	0.278**
	(0.001)	(0.036)	(0.055)	(0.109)
ten or more reviews	0.007***	0.944***	0.340***	0.117
	(0.001)	(0.034)	(0.046)	(0.086)
host responds within day	-0.006**	0.081	-0.072	-0.017
	(0.003)	(0.066)	(0.081)	(0.129)
response rate > 90%	-0.003**	0.130***	0.038	-0.104**
	(0.001)	(0.025)	(0.030)	(0.049)
cancellations in last 12 mo.	0.001	0.019	0.027	-0.180**
	(0.002)	(0.044)	(0.051)	(0.075)
review scores rating	-0.00001	-0.019***	-0.010*	0.002
	(0.0001)	(0.004)	(0.005)	(0.010)
no. accommodates	0.013***	-0.006	-0.046*	0.047
	(0.001)	(0.021)	(0.026)	(0.051)
entire home	0.087***	-0.038	-0.057	-0.221
	(0.005)	(0.118)	(0.150)	(0.266)
Observations	210,583	158,654	112,894	70,331
Adjusted R2	-0.377	-0.180	-0.503	-1.139
F Statistic	11.269*** (df = 373; 148691)	110.147*** (df = 276; 104094)	61.136*** (df = 179; 64161)	71.055*** (df = 83; 26984)

Note: Market Intercepts and Market-Time Effects not displayed.
*p<0.1; **p<0.05; ***p<0.01;

Regressions (6), (7), and (8) all show the fixed effect models using number of reviews as the dependent variable across different delayed time periods. Regression (6) represents the impact on reviews within the month the variable changes, (7) is one month after the change, and (8) represents two months after the change. As discussed in the Empirical Methods section, a delay in the change in rate of reviews would occur for any guests who book their trip in advance. It is also important to note the number of observations falls for each period ahead as I must drop one time. For example, a change in March 2019 (the second to last period I observe) would allow for a one month out coefficient from April 2019, but not two months out as I do not observe May 2019.

Examining the Superhost coefficient on number of reviews, (6), (7), and (8) there appears to be an upward trend. Within the same month, the impact is insignificant but one month out is grows to 0.133 and then to .278 two months out and significance at the 0.05 level holds despite the shrinking sample size. It appears there may be a further increase in future periods that I am unable to observe, which aligns with the OLS results.

The biggest impact on number of reviews is ten or more reviews, which also aligns with the OLS models which show very large and significant coefficients. The impact is initial 0.944, followed by 0.340 and 0.117, though 2 months out the variable is no longer significant at the 0.10 level. This may be due to lower number of observations. Response rate also has a small but highly significant increase of 0.130 reviews; however, the value is insignificant in the 1 month out period and unexpectedly is -0.104 two months out. As expected, cancelations in the last twelve months has a delayed but negative impact, resulting in insignificant values in the same month and one month out, but -0.180 at the .05 significance level in the two months out variable.

7 Discussion

With the above empirical results, I now return to answering the primary question of this analysis: what impact does the salience of the Superhost badge have on revenues? Examining price, despite the OLS results from regression (1) suggesting that hosts who gain the badge in turn charge 4.17% more per night, a result consistent with other researchers, the panel model shows a small magnitude of 0.3% increase in price set by hosts. With the small magnitude, I conclude that hosts make no meaningful change on price and thus prices from earning the badge have no considerable impact on revenues. One possible explanation for this is that in observing larger markets, any market power for hosts to raise prices is forgone with many competing Superhosts.

Considering the impact on the badge on quantity sold, again the OLS results suggest larger magnitude increases than the fixed effect models. Though, the panel model does show an increasing effect. Within the same month effect, there is no significance, thus the effect may be zero. However, the number of reviews one month out increases significantly by 0.133, followed by 0.278 effect two months out. As a lower bound estimate, it may be that the impact levels out around 0.278 more reviews per month. As a higher bound estimate, however, it reasonable to believe that the impact continues to grow to 0.44 more reviews per month, the variance that we see in the descriptive statistics between those that switched Superhost to those who were Superhost during all observation periods.

Taking the price impact as negligible as well as the lower and upper bound estimates on reviews, an initial number of reviews is necessary to estimate the percentage increase in quantity sold. Based on the descriptive statistics, those listings that switched Superhost during the observation periods initially had 2.587 reviews per month. At the lower bound of 0.278 reviews per month, this results in a 10.74% increase in quantity sold. At the higher bound estimate of 0.44, this would be an increase in quantity 17.01%. With a few assumptions this value can also be quantified in dollar terms. I estimate an increase based on the lower and upper bound of $211 to $334 more in revenue per month or $2,532 to $4008 annually. Note, this is assuming that on average, 71.7% of guests leave reviews as Fradkin, Grewal, and Holtz (2019) find, as well as an average price per night of $181.54 which I observe in my descriptive statics. This calculation also assumes that guests, on average, book three nights.

As argued previously, this estimate represents the salience effect. While an increase in quality should increase demand and thus revenues for hosts, each component representing high quality has been estimated out in my fixed effect regressions. While other researchers argued their results as the effect of trust building mechanisms related to Superhost, I argue that this effect is purely through behavioral economic means, where consumers rely on the badge to quickly identify sellers rather than more rationally consider their utility maximizing decision. However, from this analysis, consumers pay a negligible difference because of this salience effect. Those hosts who earn the badge are the clear winners based on the results, as their quantity sold increases from 10 to 17%. With the nature of the results, it is not possible to derive the source of the increase in bookings. It may be the quantity is ceded entirely by non-badged sellers (who would be the losers in that case), it may be that badged sellers are earning only marginal customers who otherwise would not have booked with an Airbnb listing at all, or it may be some variation between.

8 Conclusion

This paper examines the impact of salience on revenues in Airbnb. Its unique contribution is using panel methods to observe actual changes in price setting by hosts and frequency booked by consumers, proxied by number of reviews. I use OLS and fixed effect panel regressions to estimate the impact of becoming a Superhost on revenues. My analysis shows that there is indeed a salience effect, estimated to be between 10.74 and 17%, all of which comes from increased bookings for Superhosts. My results show that the change in price is negligible.

Considering the sharing economy more broadly, there are several markets which resemble the same consumer selection model. An important distinction to consider, though, is that not all sharing economy applications allow consumers to choose their host. For example, Lyft and Uber, two of the largest ridesharing applications automatically pair a host (the driver chooses the rider) with no choice on the consumer. Among those markets where consumers shop to choose a product and/or seller, the largest examples include SnappCar and Getaround (auto rental from owners), Outdoorsy (camper auto rental from owners), Tubber (boat rentals), Origin (tech assets and services sharing), DogVacay (dog sitting), as well Spotie, FlipKey, and VRBO (competitors of Airbnb), and TaskRabbit (freelance labor).

Broadening the implications of this study towards these other sharing economy websites, it is possible that the salience effect could be even bigger based on the transaction price. Other researchers have found that reputation matters more for higher priced goods than lower priced goods (Bajari and Hortacsu 2004; and Cabral and Hortacsu 2010). Services purchased on Airbnb are likely larger than most of the other applications listed above.

Finally, a few limitations should be noted about this analysis. First, the implications may not externalize to outside the United States where cultural or geopolitical differences may exist. I only use US data. Secondly, the results may not generalize to smaller geographic markets, as I selected my sample based on the largest densities of Airbnb listings by market.

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CHAPTER II

DETERMINANTS OF GOVERNMENT RESTRICTIONS AGAINST SHORT-TERM RENTALS

1 Introduction

This paper asks why local governments implement restrictions on short-term rental markets. Homeowners provide these overnight lodging accommodations by listing their properties on Airbnb, HomeAway, and VRBO. Airbnb, the market leader, was founded in 2008 and, in December 2020, was valued at $86.5 billion [1]during its IPO despite the coronavirus pandemic. Also called peer-to-peer markets, the sharing economy has garnered a divisive place in public discourse. Compared to traditional service models, its disruptive model is led by companies with few assets and who enjoyed substantial growth before regulation. This paper examines four types of restrictive policies placed on short-term rentals – Total Bans, Only Own Residence, Max Days, and Geographical Maximums.

Three reasons plausibly motivate cities to pass laws that explicitly restrict short-term rentals. These are housing affordability, hotel profits, and public finance. Many cities defend their restrictions with their concern for the upward pressure on family homes and rental prices. A growing number of economic studies find that increased short-term rental listings increase prices for home purchases and traditional home rental

[1] https://www.cnbc.com/2020/12/10/airbnb-ipo-abnb-starts-trading-on-the-nasdaq.html

contracts. (Barron, Kung, Proserpio, 2021;Horn and Merante 2017; Segú 2018). A second argument is that hotels seek to erect barriers to reduce competition from new entry. If hotels act as oligopolists earning high profits in markets from constrained supply, they could maintain rents by lobbying for government restrictions. A third argument is cities themselves may seek to protect tax revenues. It is ambiguous if the participation of lower-priced alternatives of short-term rentals, compared to hotels, generates complements or substitutes for tourism services and revenue. Tax receipts depend upon whether short-term rentals expand the total market size of incoming tourists or cannibalize existing revenues. Restriction, therefore, may shift Airbnb customers to higher priced hotel stays and generate higher tax receipts at hotel bars, nearby dining and tourist sites, and other ancillary services.

In this paper, I examine local economy determinants on the passage of restrictions using panel binary probits and ordered panel models to predict the marginal effects of economic conditions on the outcome of restrictions placed on short-term rentals city governments. To do this, I create a novel classification of restrictions by searching and coding city law records. The result is data from nineteen U.S. cities over eight years, from 2012 to 2019.

The probit models show that a one standard deviation decline in housing affordability leads to a statistically significant 20.57 percentage point increase in the likelihood that city councils pass laws banning non-personal residence short-term rentals operation. While not significant at the 0.10 p-value level, a one standard deviation drop in affordability leads to a predicted marginal effect of 19.66, 14.65, and 4.84 percentage point increase in the likelihood of a maximum day restriction, any restrictive policy, and total ban respectively. My results do not show strong or significant effects for high seasonality and high lodging tax revenue as a percent of city budgets, suggesting that these don't motivate restrictive policies.

In the ordered models, I find that a one standard deviation increase in affordability leads to a predicted 23.78 percentage point increase in the likelihood that no restrictive policy will be passed. Alternatively, a one standard deviation decrease in affordability predicts a 10.76 to 10.09 percentage point increase in the likelihood of cities adopting a combination of any two restrictive policies. The probit and ordered models, therefore, corroborate that low affordability drives restrictions, while other plausible arguments have no sizable or clear effect.

To the best of my knowledge, my paper is the first to predict the economic determinants that lead to local restrictions against short-term rentals. My results inform us that cities are primarily motivated by affordable housing rather than lobbied by hotels or defending public finances. This research is important because it not only predicts the future policy landscape of short-term rentals but it also offers context to other sharing economy platforms that are younger in their lifecycle.

This paper is organized into seven sections. Following the introduction, Section 2 reviews relevant literature, Section 3 describes restrictive short-rental policies, and Section 4 explains the data used in this paper. Section 5 describes the empirical methods, Section 6 discusses the results, and Section 7 offers concluding remarks.

2 Literature Review

Several researchers have categorized sharing economy policy generically. The literature describes approaches as total banning, other means of restrictions, regulation through licenses and permits, and laissez-faire (Li & Ma 2019; Katz 2015; Rauch and Schleicher 2015). However, few researchers address short-term rental policy specifically. Two papers examine the effects of regulation using a report, Roomscore,[2] produced by the public policy organization R-Street (Uzunca & Borlenghi, 2019; Yang and Mao, 2019). But the goal of these works is to predict Airbnb supply rather than explain the presence of these regulations. Furukaway and Onuki (2019) investigate U.S. cities to identify a quantitative measure of short-term rental friendliness but do not seek to explain which determinants lead to restrictive policies.

In the literature, among city planners and the general public, concerns are often cited that short-term rental growth decreases affordability both for annual rental housing and housing prices. While still nascent, several studies have empirically demonstrated this upward pressure on home rents and home prices. Barron, Kung, and Proserpio (2021) find in their study of U.S. cities that a 1% increase in Airbnb leads to a 0.018% increase in rents and a 0.026% increase in house prices. Further evidence of rental price increases are shown in the cities of Boston and Barcelona (Horn & Merante, 2017: Segú, 2018). Urban planners are concerned about increasing rates of gentrification and the loss and restructuring of the rental housing stock (Wachsmuth & Weisler, 2018; Yrigoy, 2018; Amore et al., 2020).

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[2] https://www.rstreet.org/wp-content/uploads/2016/03/RSTREET55.pdf

Economic theory has long held that incumbent firms who earn positive profits have an incentive to lobby the government to restrict competition (Stigler 1971). Zervas, Proserpio, & Byers (2017) find that Airbnb is a substitute product for hotels, especially for lower-tiered and nondifferentiated hotels. This supports the motive for hotels to lobby city governments to restrict short-term rentals in any market where positive economic profits are sufficiently high.

A final argument for restriction is to protect the fiscal health of the local government. Furukaway and Onuki (2019) argue that motivates regulation. In other fields, researchers produce evidence that public finance influences state decisions to pass restrictive policies (Bradford & Bradford 2016 and Macinko & Silver 2015). However, the above research has yet to show an empirical relationship between regulation and budget concerns as it relates to the sharing economy.

3 Local Government Restrictions

Local governments have passed laws regulating short-term rentals, which apply to Airbnb in addition to other platforms such as HomeAway and VRBO. Cities may set safety standards, rules for tax collections, and restrictions regarding what types of hosts may list and in what capacities they can accommodate guests. Some cities have wholly banned the operation of any rental of a home for less than 30 days. While there are minor variations in how cities define short term rentals and refer to their policies, the laws passed by local governments to restrict the operation of an Airbnb can be grouped and classified into four categories:

1. *Total Ban:* operation of any short-term rental is explicitly outlawed.

2. *Only Own Residence:* Short-term rental hosts can only accommodate guests in their primary residence, though the host can be away from home during the stay.
3. *Max Days:* a maximum number of days per year a short-term rental can be booked by guests, usually 90 or 180 days.
4. *Geo Max:* Some or all parts of the city have an aggregate maximum number of listings that can operate within a neighborhood or other defined zone. This acts as a cap on the total number of listings that can legally be operated in an area.

4 Data

I collect data from a combination of government and industry databases, local government records, and web scraped files to construct an annual panel data set from 2012 to 2019. Nineteen U.S. cities represent a total of 152 observations across eight years. Each row represents a unique individual city and year combination.

I create a matrix of dummy variables for the four categories listed in Section 3, Local Government Restrictions, where one represents a restriction passed by city law and zero reflects the absence of the law. I also create a fifth dummy variable, *any_policy*, coded with a one when one or more of the four restrictive policies are passed. To construct this matrix, I conducted a rigorous search of city-published records of laws passed by the local government on third-party websites, such as municode.com. For this analysis, I use the year that the policy passes by the city council. I exclude any cities with complicated jurisdiction boundaries, such as Las Vegas.

In addition to the individual restriction dummy variables, I create a degree of restrictiveness variable ranging from one to five, including each of the above restrictions. The *level_restrict* reflects the number in place plus one. I use a level of restriction rather than a count because some policies apply to other categories. A *level_restrict* of one indicates that no policies are passed. A value of three indicates two restrictions are passed. Five indicates that a total ban effectively creates all restrictions.

I construct a measure of a city's dependence on hotel revenue as a percent, *city_rev_pct_lodge_tax*, by dividing lodging tax receipts by total city tax revenue. I observe lodging taxes from HVS Global Hospitality Services annual lodging tax study. I collect local government revenue data from the U.S. Census Bureau government finance database. A value of one indicates the city earns one percent of city revenue from lodging taxes.

I construct a measure of short-term rental market penetration, *airbnb_per_k_person*, by dividing the number of Airbnb listings by the city population in thousands. I count the number of Airbnb listings using web scraped data from InsideAirbnb. I observe the population of each city proper from the United States Census City and Town Populations Total (estimates).

To measure affordability, I use the National Realtors Association housing affordability index. This metric provides a score indicating if a typical family can qualify for a mortgage using local median home prices, median family incomes, and the prevailing mortgage interest rate. A score of 100 indicates a median family has sufficient income to exactly afford a median-priced home. Scores above 100 indicate higher affordability, and those below 100 indicate low affordability.

Lastly, I construct a measure of the lumpiness of demand, *lumpy_flights*, by measuring the range in disembarking passengers at the nearest airport. I do this using the T-100 Domestic Market All Carriers from the United States Bureau of Transportation Statistics (Li & Srinivasan, 2019). For each month within a given year, I calculate the percent of annual incoming passengers out of the entire year. For the annual measure of lumpiness, I measure the range by subtracting the lowest disembarking month from the highest disembarking month. Therefore, a higher number implies a larger variation between low and peak months of travel.

Table 2.1

Summary Statistics

Statistic	N	Mean	St. Dev.	Pctl(25)	Pctl(75)	Max
total_ban	152	0.039	0.195	0	0	1
max_days	152	0.197	0.399	0	0	1
only_own_res	152	0.342	0.476	0	1	1
geo_max	152	0.263	0.442	0	1	1
any_policy	152	0.461	.500	0	1	1
level_restrict	152	2.072	1.544	1	3	6
airbnb_per_k_person	152	11.261	14.127	1.271	16.948	75.885
lumpy_flights	151	1.351	0.344	1.085	1.527	2.293
city_rev_pct_lodge_tax	141	2.018	1.315	1.226	2.523	6.333
affordability	150	143.121	56.356	85.802	182.908	276.190

Table 2.1 shows summary statistics. We see that among the restrictive policies represented, *Only Own Res* and *Geo Max* are the highest number of years in place,

representing 34.2% and 26.3% of the city-year observations. *Total Ban* is the rarest, representing only 3.9% of the city-year combinations. The distribution of *airbnb_per_k_person* shows that, on average, there are 11.26 Airbnb's per thousand people. Affordability ranges widely from 49.7, very unaffordable for median families, up to 276, very affordable.

Table 2.2

Percent of Cities with Restrictions by Year

	2012	2013	2014	2015	2016	2017	2018	2019
total_ban	5.3%	5.3%	0.0%	0.0%	0.0%	5.3%	5.3%	5.3%
max_days	10.5%	10.5%	15.8%	15.8%	15.8%	21.1%	31.6%	36.8%
only_own_res	10.5%	10.5%	21.1%	21.1%	31.6%	42.1%	68.4%	68.4%
geo_max	10.5%	10.5%	10.5%	15.8%	26.3%	42.1%	47.4%	47.4%

Table 2.2 shows the percent of cities with a given restriction by year. In 2012, adoption of all policies was low, with *Geo Max*, *Max Days*, and *Only Own Residence* representing 10.5% of cities. By 2016, the adoption of these policies increased. By 2019, *Only Own Residence* is the most frequent law passed by 68.4% of cities, followed by *Geo Max* with 47.4%. The surprising change in the presence of *Total Ban* in 2012 to its absence in 2014 comes from San Francisco, a city that repealed its ban.

Table 2.3 illustrates descriptive statistics by showing the distribution of explanatory variables by the levels of restriction. The most evident relationship we see is that lower levels of affordability are correlated with higher levels of restriction. A level of one, indicating no laws passed, has average affordability of 163.5. The lowest averages for affordability are at level five and level three, 72.7 and 88.775, respectively. We also note that the number of Airbnb listings per thousand people tends to be lower at higher restrictions. These statistics may indicate that cities that passed policies earlier successfully limited supply. The lumpiness of demand and city revenue percent from hotels show no clear pattern across restrictions.

Table 2.3

Independent Variable Averages by Level of Restriction

		level_restrict				
	all	1	2	3	4	5
N	152	96	20	22	9	5
airbnb_per_k_person	11.261	6.635	27.735	19.460	5.950	7.653
lumpy_flights	1.351	1.293	1.362	1.501	1.600	1.377
city_rev_pct_lodge_tax	2.018	1.791	2.638	2.912	0.685	2.588
affordability	143.12	163.532	136.282	88.775	107.905	72.68

5 Empirical Methods

I use two sets of models to estimate the effects of the determinants of the local economy on restrictive policy adoption. The first is a panel binary response model, which tests each restriction policy individually as the outcome variable. The second is a panel ordered model that tests the restrictive measure level, combining the effect of all restrictions passed.

The first is a panel binary response model, specified as:

$$y^*_{it} = \propto + \delta_t + \beta_1 LumpyFlights_{it} + \beta_2 CityRevPerLodge_{it} + \beta_3 Affordability_{it} + \beta_4 AirbnbPerKPerson_{it} + \varepsilon_{it} \quad (1),$$

with y^*_{it} as a latent variable where,

$$y_{it}\begin{cases} = 1, if\ y^*_{it} > 0 \\ = 0, if\ y^*_{it} < 0 \end{cases} \qquad (2).$$

I run this model for each restrictive policy separately. An outcome of one indicates that the given restrictive policy was passed into law in *i* city at *t* time. The absence of a policy being passed into law is indicated with a zero. Among the independent variables, I include *lumpy_flights,* a proxy for seasonality of demand (Li & Srinivasan, 2019), reflecting a market more likely to pay hotel rents. *City_rev_pct_lodge_tax* is a measure of how a city's dependence on lodging revenues, a public finance motive, and *affordability* reflects cities with high housing costs.

To control for the market penetration of Airbnb, I use *airbnb_per_k_person* population. The model intercept is estimated with α, and δ_t estimates a time-fixed effect to control for the tendency for laws to be passed later. The error is assumed to be distributed normally, with a mean of zero and a variance of σ^2.

My second set of estimation techniques use panel ordered models, including a random effect ordered probit model and a fixed effects ordered logit model. The advantage of using this second set of models is it combines the restrictive policies into categorical levels, from least to most restrictive. This approach allows for more testing of variation over time. The ordered probit model is specified:

$$y^*_{it} = \propto_i + \delta_t + \beta_1 LumpyFlights_{it} + \beta_2 City\_Rev\_Per\_Lodging_{it} + \beta_3 Affordability_{it} + \beta_4 Airbnb\_Per\_K\_Person_{it} + \varepsilon_{it} \quad (3).$$

The ordered logit model is specified:

$$y^*_{it} = \propto_i + \delta_t + \beta_1 LumpyFlights_{it} + \beta_2 City\_Rev\_Per\_Lodge_{it} + \beta_3 Affordability_{it} + \beta_4 Airbnb\_Per\_K\_Person_{it} + \varepsilon_{it} \quad (4).$$

For both equations 3 and 4, the latent variable y^*_{it} is given,

$$y_i \begin{cases} 5, & if\ y^*_{it} \leq \mu_1 \\ 4, & if\ \mu_1 \leq y^*_{it} \leq \mu_2 \\ 3, & if\ \mu_2 \leq y^*_{it} \leq \mu_3 \\ 2, & if\ \mu_3 \leq y^*_{it} \leq \mu_3 \\ 1, & if\ \mu_4 \leq y^*_{it} \end{cases} \quad (5).$$

I estimate the ordered logit and ordered probit models above with the same independent variables as the first model in equation 1. In addition, the ordered models estimate $\mu_1 - \mu_4$, which represent the cut variables for which level of restriction an observation *i* will have in any given period *t*. While equation 3 assumes a normal error distribution, equation 4 assumes a logit distribution. Equations 3 and 4 also provide a fixed effect α_i which estimates idiosyncratic differences by city.

6 Results

Table 2.4 shows the marginal effects and results for equation one by the outcome of the four restrictive policies. It also includes *any_policy,* indicating that any of the four policies are passed. We notice that none of the marginal effects are statistically significant, at the p 0.05 value. Only one marginal effect, *affordability,* is significant at the p 0.10 value for the only residence policy. We also notice that affordability is the only coefficient with a consistent negative sign. The other variables vary in at least one sign of their coefficient across models, and all lack statical significance. Except for affordability, we, therefore, cannot conclude if any variable certainly has a positive or negative effect.

Table 2.4

Marginal Effects from Binary Random Effects Model Results by Restriction

	(1) total_ban	(2) max_days	(3) only_own_res	(4) geo_max	(5) any_policy
lumpy_flights	-0.0165 (0.0609)	0.0756 (0.0628)	-0.0261 (0.0999)	-0.0824 (0.121)	0.0690 (0.121)
city_revpctlodgetax	0.0122 (0.0234)	-0.0173 (0.0215)	0.00606 (0.0312)	0.0197 (0.0292)	0.00236 (0.0375)
affordability	-0.000859 (0.00150)	-0.00349 (0.00310)	-0.00365* (0.00210)	-0.0000294 (0.000759)	-0.00260 (0.00224)
airbnb_per_k_person	-0.00287 (0.00619)	-0.00770 (0.00624)	-0.00302 (0.00306)	0.00259 (0.00404)	0.00628 (0.00640)
lnsig2u	4.522 (0.806)	4.261 (0.675)	4.220 (0.562)	2.860 (1.260)	4.500 (.0488)
N	86	141	141	141	141
Log lik.	-6.231	-10.330	-28.506	-25.748	-33.331

Notes: Year effects and intercept estimated but not displayed. Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

For column 3, *Only Own Res*, affordability has a significant marginal effect of -0.00365. A one standard deviation drop in affordability, 56.356, implies a 20.57 percentage point increase in the likelihood of the *Only Own Res* restriction. The same standard deviation decrease in affordability predicts a slightly smaller effect for *Max Days*, at 19.66 percentage points, though not statically different from zero at the 0.10 p-value. With smaller but still sizable magnitudes, a one standard deviation drop in affordability would result in 14.65, 4.84, and 0.16 percentage points for *any policy*, *Total Ban*, and *Max Days*, respectively.

The other variables of interest represent low magnitudes and clear significance. For example, the largest marginal effect for *lumpy_flights,* -0.0824, indicates that a one standard deviation increase of 0.344 represents only a change of 2.838 percentage points in adopting the law. The largest effect, across all models, for the variable *city_rev_pct_lodge_tax* is 0.0197 for *Geo Max*. We can contextualize this by a one standard deviation increase of 1.315, resulting in a 2.5 percentage point increase in the likelihood of adopting the policy.

We cannot accept the hypotheses that *lumpy_flights* and *city_rev_pct_lodge_tax* have statistical or economic significance. Alternatively, while the statistical significance is weak, *affordability* has a persistently negative effect on each policy type, with an economically significant magnitude. We see from before that a standard variation reduction in affordability results in a 20.57, 19.66, and 14.65 percentage point increase in the likelihood of *Only own Res*, *Max days*, and any policy, respectively.

As a robustness check, I ran each outcome with the variable *lumpy_flights* dropped. Seasonal demand could arguably affect rents for hotels and short-term rentals alike; however, the results did not change any of the statistical significance levels or magnitudes in a meaningful way.

Table 2.5

Ordered Model Results– Outcome Level of Restriction – Beta Coefficients

	(1)	(2)	(3)	(4)
	Ordered RE Probit	Ordered RE Probit w/o lumpy_flights	Ordered RE Logit	Ordered RE Logit w/o lumpy_flights
lumpy_flights	-0.205		-0.217	
	(0.907)		(1.583)	
city_rev_pct_lodge_tax	-0.0666	-0.0718	-0.138	-0.144
	(0.273)	(0.268)	(0.491)	(0.486)
affordability	-0.0317***	-0.0310***	-0.0559**	-0.0552***
	(0.00940)	(0.00873)	(0.0170)	(0.0159)
airbnb_per_k_person	-0.0118	-0.0117	-0.00840	-0.00839
	(0.0196)	(0.0194)	(0.0347)	(0.0346)
sigma2_u	1.792	1.722	5.617	5.489
_cons	(1.020)	(0.928)	(3.259)	(3.039)
N	141	141	141	141
Log lik.	-99.71	-99.74	-98.75	-98.76

Notes: Year effects and cut variables estimated but not displayed.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We examine the ordered model results in Table 2.5, which shows the beta coefficients, not the marginal effects. Columns one and two display equation 3, and columns three and four display equation 4. We see results consistent with the probit models, the coefficients for all variables except affordability are statistically insignificant and represent very small magnitudes. Though, unlike the probit results, affordability is significant at the 0.001 p-value for each model. To understand the relative impact, we need more than the coefficients of the model.

Table 2.6 displays the marginal effects of affordability as a percent likelihood for each level of restriction represented in a separate row. We see across both models, and with and without the *lumpy_flights* variable, that the marginal effects are robust across specifications. The largest and most significant marginal effect is for *level_restrict* of one, indicating no laws passed. Across the models, the effect varies only slightly, ranging from -0.00452 to -0.00449. A one standard increase in affordability leads to a 23.78 percentage point increase in the likelihood of no restriction. This result is consistent with the probit model and at a similar magnitude.

Table 2.6

Marginal Effects of Ordered Models for Affordability

Level Restrict	(1) Ordered RE Probit	(2) Ordered RE Probit w/o lumpy_flights	(3) Ordered RE Logit	(4) Ordered RE Logit w/o lumpy_flights
1	0.00452***	0.00449***	0.00452***	0.00449***
	(0.00071)	(0.000697)	(0.000720)	(0.000701)
2	-0.000705*	-0.0006954*	-0.000689*	-0.000684*
	(0.000306)	(0.000306)	(0.000301)	(0.000301)
3	-0.00179***	-0.0017988***	-0.00190***	-0.001906***
	(0.000431)	(0.000428)	(0.000467)	(0.000465)
4	-0.000740*	-0.000741*	-0.000817*	-0.000816*
	(0.000289)	(0.000292)	(0.000322)	(0.000323)
5	-0.00129	-0.00125	-0.00111	-.000109
	(0.000718)	(0.000684)	(0.000698)	(0.000665)

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The marginal effect for a level three restriction is also highly significant, with a smaller effect of -0.00191 to -0.00179. This effect indicates a one standard deviation drop in affordability would result in a 10.76 to 10.09 percentage point increase in the likelihood of adopting two restrictions. The marginal effect for a level 2 and level 4 restriction is significant at the 5% level, with a smaller effect of about -0.0007. This would mean a one standard deviation drop would increase the likelihood of adopting 2 or 3 restrictions by 3.94 percent.

It's interesting to note that there is no clear pattern indicating cities pursue the most restrictive policy combinations, and the likelihood of adopting 1 or 3 is also relatively small. However, the largest and clearest effect is the likelihood of adopting specifically two restrictions.

The marginal effects for the variables of *city_rev_pct_lodge_tax* and *lumpy_flights* are all insignificant and thus are not displayed.

7 Conclusion

This paper examines three plausible motivations for cities to pass restrictive laws against short-term rentals, such as Airbnb. These are to reduce the pressure of decreasing housing affordability, reduce competition lobbied by the hotel industry, and protect city budgets from decreased lodging and tourism tax revenues. To the best of my knowledge, my paper is the first to estimate the economic determinants that drive the passage of restrictive laws. I estimate the outcomes of a *Total Ban*, *Max Days*, *Only Own Residences*, *Geo Max*, *any_policy*, and the broader measure of the level of restriction. I do this using panel binary probit models and ordered logit and probit models for nineteen U.S. cities from 2012 to 2019.

The binary probit models I estimate consistently show that housing affordability is negatively related to restrictive policies. That is, cities with unaffordable housing are the most likely to restrict short-term rentals. While the only statistically significant result is for the *Only Own Residence* policy, the economic significance is high for two others. The marginal effects show that a one standard deviation decrease in affordability predicts an increased probability of 20.57, 19.66, and 14.65 percentage points for *Only own Residence*, *Max Days*, and *any policy*, respectively. I do not find statistical significance or economic significance supporting hotel lobbying or public finance rationales, measured by the variables of *lumpy_flights* and *city_rev_pct_lodge_tax.*

The ordered logit and ordered probit models corroborate the significance of affordability. High affordability predicts a low likelihood of adopting restrictive policies, and specifically, low affordability indicates a high probability of adopting a level three restriction. A one standard deviation rise in affordability predicts a 23.78 percentage point increase in the likelihood to pass no restrictions and a 10.76 to 10.09 percentage point decrease in the likelihood of passing specifically two restrictions. We can reasonably conclude that affordability drives cities to pass laws against short-term rentals such as Airbnb.

Limitations of my research arise because my study is limited to only U.S. cities, and I use a small sample which may lead to the lack of statistical significance of some independent variables. Despite this, I show large and significant effects driven by housing affordability. My results support the anecdotal evidence that cities with high affordability are unlikely to pass restrictive policies, and examples of so many of the largest cities in the United States limit short-term rentals.

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CHAPTER III

THE HETEROGENEOUS EFFECTS OF GOVERNMENT RESTRICTION AND FEES ON AIRBNB SUPPLY

1 Introduction

Since the introduction of Airbnb in 2008, local governments have sought to regulate the market to achieve a balance of social and market goals. Many cities have sought to restrict all short-term rentals through local laws. Other policies focus specifically on professional hosts, those who manage more than one short-term rental listing. It's unclear if the intended supply reductions are effective, and it remains opaque when the policy aims at professional hosts if the law targets these hosts successfully. To estimate this information accurately is to show what effect targeted local restrictions have. Anecdotal media reports and public sentiment suggest that these policies have not stopped many Airbnb listings' illegal operation, such as a New York lawsuit [1] alleging a firm illegally operated over 250 listings to 76,000 guests.

My paper studies this question of whether local government restrictions and permit fees reduce supply as intended. It further identifies how heterogeneously aimed policies affect professional and nonprofessional hosts. I specifically study the policies aimed at all hosts - complete outlawing (Total Bans), maximums on the number of days a listing can be booked (Max Days), and zone-based aggregate listing maximums (Geo

[1] https://www.businessinsider.com/21-million-lawsuit-alleges-real-estate-brokers-used-airbnb-illegally-2019-1

Max). In addition, I examine those policies aimed directly at professional hosts, including a maximum number of listings one host can manage (Max Host) and instances where hosts can only list their primary residence (Only Own Residence). In addition, I test if increases in annual permit fees have significant effects on Airbnb supply.

I use two fixed effects models to measure both the aggregate and heterogeneous effects of each of the above policies by estimating the percent availability of listings as an outcome variable. My data include seventeen U.S. cities over five years, from 2015 to 2019. The data come from web-scraped Airbnb listings availability and other demographics, the city council passed laws in effect, and control variables including supply and demand factors. With the combination of fixed effects for individual listings, markets, and time, my model allows for casual interpretations of hosts' responses to policy changes.

I find that policies aimed at professional hosts and those that restrict all host types have no clear effect on reducing supply. It may contribute to a mild increase. Alternatively, I find that both broad restrictive policies and those focused on professional hosts reduce the supply of nonprofessional listings, those managed by a host with only one listing, by anywhere from -6.5% to -15.7%.

I also find that annual permit fees have sizable and opposing effects by host type. I estimate that nonprofessional hosts respond to a 1% increase in fees with a 1 to 1.2 percentage point increase in availability. One explanation for this is amateur hosts are not profit-maximizing but instead pursue a target-based income approach. Professional hosts, however, respond to a 1% increase in annual fees with a -0.7 percentage point reduction availability.

My paper expands the existing literature, which has not yet combined models estimating Airbnb supply (Dogru et al., 2020b; Li et al., 2016) with the varied effects of professional and nonprofessional hosts in response to restrictive local policies (Uzunca & Borlenghi, 2019; Yang and Mao, 2019). The existing inconsistent evidence that regulation has led to either decreasing or increasing supply can be explained by my research of the opposing effect of professional hosts' mild positive increase in supply and nonprofessional hosts' decrease. To the best of my knowledge, my paper is also the first to use panel data to estimate changing restrictions over time, while previous research estimates Airbnb regulation's effects relying on a one-time categorization of regulation ((Uzunca & Borlenghi, 2019; Yang and Mao, 2019).

The evidence of this paper suggests that city laws passed to restrict short-term rentals such as Airbnb, VRBO, and HomeAway, do little to affect professional hosts but do affect nonprofessional hosts. Cities should acknowledge that further efforts are necessary to achieve the desired reductions, and among them, increasing permit fees is likely to reduce professional supply based on my analysis. This research may also illuminate other sharing economy markets, such as Turo, where suppliers provide autos for rent.

The organization of this paper is typical of current empirical economic research. Section 2 reviews current literature. Section 3, Local Government Restrictions and Fees, explains the unique restrictions passed by local cities. Section 4 stages my hypotheses, Section 5 presents the data used, and Section 6 articulates the empirical methods. Section 7 displays and discusses the results, and Section 8 concludes.

2 Literature Review

The foundational research for this paper relies on three veins in the literature. The first vein regards models that seek to estimate the supply of Airbnb listings, the second is how regulation and policy impact the market, and the third is those that research the distinctions of professional and nonprofessional hosts.

Dogru et al. (2020b) estimate Airbnb supply with specific emphasis on the impact of macroeconomic conditions using data from all 50 states annually from 2010 – 2017. They find that hotel room rates or Average Daily Rate (ADR), house prices, and GDP contribute to the increased supply of Airbnb. They use a generalized method of moments model using dynamic panel data. As their goal is to study macroeconomic conditions, they do not examine local policy.

Yang and Mao (2019) research the determinants of Airbnb supply, including regulation. They use a mixed-effects negative binomial model to examine two outcomes – the total number of listings on Airbnb and the total number of available days within one year. They find that the number of visitors, hotel ADR, monthly housing costs, and percentage of occupied renter housing units increase Airbnb participation. Alternatively, average household size reduces Airbnb supply. To examine the effects of regulation, they use a report, Roomscore,[2] produced by the public policy organization R-Street. The

[2] https://www.rstreet.org/wp-content/uploads/2016/03/RSTREET55.pdf

researchers find that a local tailored legal framework and hostile enforcement decrease supply. The use of a reliable source to categorize policy is a strength of this study, though a limitation arises because the report was only produced in 2016. The authors are therefore unable to estimate the effects of policy changes over time.

Uzunca & Borlenghi (2019) also use the 2016 Roomscore report to assess a broad measure of 'regulation strictness' using an OLS model comprising of 59 United States cities. Contrary to Yang and Mao (2019), they find that higher levels of regulation across the board led to statistically and economically significantly higher rates of supply of actively listed units. They argue that "people are naturally averse to uncertainty and an increase in legislation diminishes legal uncertainty."

The above research doesn't examine if a heterogeneous nature exists between amateur and experienced sellers. The literature specifies a professional host as any host who manages two or more listings on Airbnb. Therefore, a nonprofessional host is any host who manages a single listing. (Li et al., 2016; Dogru et al., 2020a).

Li, Moreno, Zhang (2016) use fixed effect panel data from one city in 2012 and 2013. They argue that other researchers observe non-profit maximizing behavior from amateur sellers elsewhere and that this amateur behavior is observed on Airbnb as well. Indeed, they find that professional hosts earn 16.9% higher daily revenue and 15.5% higher occupancy rate at the same frequency of offerings. They point to professional hosts' use of strategic seasonal pricing. Their evidence suggests that nonprofessional hosts miss opportunities to vary prices in seasonal spikes in demand.

Looking beyond Airbnb, Camerer, Babcock, Loewenstein, and Thaler's (1997) research shows unexpected heterogeneous effects between experienced and amateur taxi drivers. They find that inexperienced cab drivers don't respond to higher rates by increasing their quantity supply; instead, amateur drivers 'set a loose daily income target and quit working once they reach that target.' This same income target approach may also apply to unprofessional Airbnb hosts rather than the traditional profit-maximizing model.

Dogru et al. (2020a) refer to the increasing rates of professionalization. Their descriptive study from November 2017 to October 2018 shows that 12 states generate more revenue on Airbnb than the other 38 states and that high tourism demand correlates with professionalism. The growing industry trend for investment guidance to investors in real estate for use in Airbnb, such as the website AirDNA, supports these observations.

The unique nature of Airbnb forces hosts to categorize their listing as one of three types: entire home (entire house or apartment for guests), shared space (sleeping areas shared with either guests or the host), and private space (sleeping rooms are separated but common areas are shared). Researchers use room type categorization as both an independent variable in estimations and to segment the sample to estimate regression results by listing type (Yang and Mao, 2019; Dogru et al., 2019; Zervas et al., 2017)

For a broad review of the nascent literature on Airbnb, Guttentag (2019) provides an excellent summary and content analysis of the significant findings in Airbnb research.

3 Local Government Restriction and Fees

Local governments have passed laws regulating short-term rentals, which apply to Airbnb in addition to other platforms such as HomeAway and VRBO. Cities may set safety standards, rules for tax collections, and restrictions regarding what types of hosts may list and in what capacities they can accommodate guests. Some cities have wholly banned the operation of any rental of a home for less than 30 days. While there are minor variations in how cities define short term rentals and refer to their policies, the laws passed by local governments to restrict the operation of an Airbnb can be grouped and classified into five categories:

1. *Total Ban:* operation of any short-term rental is explicitly outlawed.
2. *Max Days:* a maximum number of days per year a short-term rental can be booked by guests, usually 90 or 180 days.
3. *Geo Max:* Some or all parts of the city have an aggregate maximum number of listings that can operate within a neighborhood or other defined zone. This acts as a cap on the total number of listings that can legally be operated in an area.
4. *Only Own Residence:* Short-term rental hosts can only accommodate guests in their primary residence, though the host can be away from home during the stay.
5. *Maximum Host:* Hosts may only operate at most three separate short-term rentals. Thus, making it illegal for a host to manage four or more listings, therefore limiting or reducing the professionalized scale of the market

A final policy that may not intend to restrict listings directly but could decrease market participation is

6. *Permit Fees*: Annual fees to acquire or maintain an annual permit to legally operate a short-term rental.

One concern for this analysis is that state policy governing short-term rentals would influence local policies and endogenously affect local policy. Only one state restricts Airbnb operations for the cities observed in this paper within the years surveyed in the panel data. New York State in 2011 passed the Multiple Dwelling Laws,[3] which acts as an Only Own Residence policy for New York City. As this policy has not varied since 2011, a fixed effect analysis will not estimate this non-varying effect.

4 Hypothesis

This paper seeks to determine the impact of local restrictions on the supply of Airbnb listings. I hypothesis for each type of policy that the intended host type will reduce their market supply. First, I hypothesize that those policies intended to restrict all listings operation will reduce the supply of both host types:

- Hypothesis 1a: Policies that intend to restrict the supply of all hosts -- Total Ban, Max Days, and Geo Max -- will reduce the supply of professional hosts.
- Hypothesis 1b: Policies that intend to restrict the supply of all hosts -- Total Ban, Max Days, and Geo Max -- will reduce the supply of nonprofessional hosts.

[3] https://ny.curbed.com/2013/3/25/10260752/an-introduction-to-new-yorks-short-term-rental-laws

Second, I hypothesize that those policies explicitly aimed at professional hosts will only reduce supply for professional hosts.

- Hypothesis 2a: Policies that intend to restrict the supply of only professional hosts -- Only Own Residence and Maximum host -- will reduce the supply of professional hosts.
- Hypothesis 2b: Policies that intend to restrict the supply of only professional hosts - Only Own Residence and Maximum host -- will not affect the supply of nonprofessional hosts.

Finally, while it is unlikely that annual permit fees will be sufficiently large to change market participation, these fees could serve as a barrier to entry if sufficiently high. In addition, the fees could result in market exit if marginal costs rise sizably. I hypothesize that they will have no economically significant effect.

- Hypothesis 3a: Increases in annual permit fees will not affect the supply of professional hosts who remain in the market.
- Hypothesis 3b: Increases in annual permit fees will not affect the supply of nonprofessional hosts who remain in the market.

5 Data

I collect data from a combination of government, industry, and web scraped sources to construct an annual panel data set from 2015 to 2019. Seventeen U.S. cities[4]

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[4] Austin, TX; Boston, MA; Chicago, IL; Columbus, OH; Denver, CO; Los Angeles, CA; Minneapolis, MN; Nashville, TN; New Orleans, LA; New York, NY; Oakland, CA; Portland,

represent a total of 492,104 observations across five years. Each observation reflects an individual listing on Airbnb, a unique rental space with more beds to accommodate overnight guest stays. I combine details from each listing with local economic conditions and government policy

The listings data is web scraped directly from the Airbnb website by a third party, Inside Airbnb. The web scraped method effectively takes a snapshot of every listing and all qualitative and quantitative information provided at one moment in time. This snapshot is taken roughly every year in May. There are instances where the website was scraped in varied months, especially in 2015 when some cities were web scraped in June, August, and September. This paper uses the unique identifier of the listing, its city, the nightly price to book, and the number of nights listed as available to be booked in the next twelve months. In addition, I observe the number of listings that the host operates. The host has a parent identifier. Across all five years, 54.2% of the listings are managed by a host with only one Airbnb (nonprofessional), while 45.8% of listings are managed by hosts with more than one Airbnb (professional).

To create a supply measure, I divide the number of days available in the next twelve months by three hundred sixty-five. This calculation produces a metric of annual percent availability. It answers what portion of the year the host is willing to provide accommodations. A complication of the percent available measure is that nights that guests have already booked will show as unavailable (Li et al., 2016). For example, if

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OR; Providence, RI; San Diego, CA; San Francisco, CA; San Jose, CA; Seattle, WA; Washington, DC guests have already booked the upcoming three weekends, six days will show as unavailable, despite being available and purchased by consumers. A strong assumption is that all listings are booked ahead by guests at a universal rate. However, this is not necessary for a fixed effects analysis. Instead, I make the weaker assumption that the rate at which guests book in advance does not vary within an individual listing.

I create a measure of professional hosts by assigning a factor variable. Listings whose host manages more than one listing are indicated as *prof* (Li et al., 2016; Dogru et al., 2020a), while those listings whose host manages only one listing are given the factor label *nonprof*.

I local demographic data collect from government sources as annual measures. I observe the American Community Survey (Yang and Mao, 2019) urban area data to control for average home prices and average monthly rent for long-term leases. I observe market demand from incoming flights data from the Bureau of Transportation Statistics, the T-100 Domestic Market All Carriers to sum the incoming passengers to the nearest airport to the city. To control for the closest substitute to Airbnb, I collect the average daily rate (ADR) for local hotels from Business Travel News. ADR is a simple average nightly price for all hotels in the urban area, including non-business-oriented hotels.

To construct local government restriction and fees measures, I conducted a rigorous search of city published records of laws passed by the local government on third-party websites, such as municode.com. For this analysis, I use the effective year that the policy begins rather than the time the city council passed it. I exclude any cities with complicated jurisdiction boundaries, such as Las Vegas. Most often, cities provide one to six months for a given policy to go into effect. During the periods observed, San Diego passed a law but repealed it before it went into effect. Therefore, following the above rule, this law would not affect the data I construct because the policy never went into effect.

Using the above findings, I create a matrix of restrictions as dummy variables where a one indicates a given local government restriction is in effect, and a zero is the absence of that restriction. As explained in section 3, Local Government Restriction and fees, these are *Total Ban*, *Only Own Residence*, *Max Days*, *Max Host*, and *Geo Max*. Finally, I input the logged annual fee for any city with a permit in place. For any locality without a fee, I replace the permit fee with one so that the log equals 0.

Table 3.1

Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl (25)	Pctl (75)	Max
Total.Ban	492,104	0.005	0.074	0	0	0	1
Max.Days	492,104	0.125	0.331	0	0	0	1
Geo.Max	492,104	0.2	0.4	0	0	0	1
Only.Own.Res	492,104	0.464	0.499	0	0	1	1
Max.Host	492,104	0.104	0.305	0	0	0	1
Log_fee	492,104	2.038	2.341	0	0	4.489	6.215
percent_available	492,101	0.445	0.377	0	0.049	0.847	1
log_home_rent	492,104	7.249	0.173	6.822	7.179	7.376	7.62
log_home_price	492,104	13.007	0.39	12.117	12.716	13.323	13.81
prof	492,068	0.458	0.498	0	0	1	1
log_ADR	492,104	5.446	0.249	4.847	5.212	5.706	5.898
log_passengers	492,104	16.709	0.603	14.478	16.269	17.164	17.596
log_price	492,104	4.906	0.785	0	4.382	5.298	10.309
Entire_home_apt	492,104	0.657	0.475	0	0	1	1
Private_room	492,104	0.314	0.464	0	0	1	1
Shared_room	492,104	0.029	.0168	0	0	0	1

Table 3.1 shows summary statistics. I take the log of all non-dummy variables to show elasticities (Dogru et al., 2020b; Yang and Mao, 2019). Each observation indicates an individual listing on Airbnb at one time. For example, the mean measure for a dummy variable of *Total Ban* at 0.005 demonstrates that 0.5% of all listings in the sample face that local policy restriction. The mean for log fee represents the average of all listings the log of any required permit fee.

Table 3.2 illustrates the change in policy adoption across time. We see that the *Only Own Res* and *Annual Fees* vary frequently across the years observed, while *Total Ban*, *Max Days*, and *Geo Max*, and *Max Host* have infrequent variation. By the final year observed, *Total Ban* is rare at 5.6%, but *Only Own Residence* and *Annual Fee* are common, at 44.4% and 61.1%, respectively.

Table 3.2

Percent Policy Adoption by Year

	Year					
Policy	2015	2016	2017	2018	2019	All
Total Ban	0.0%	0.0%	0.0%	5.6%	5.6%	2.2%
Max Days	11.1%	11.1%	11.1%	11.1%	16.7%	12.2%
Geo Max	11.1%	11.1%	22.2%	22.2%	22.2%	17.8%
Only Own Res	22.2%	27.8%	33.3%	33.3%	44.4%	32.2%
Max Host	5.6%	5.6%	5.6%	11.1%	11.1%	7.8%
Annual Fee	22.2%	22.2%	38.9%	50.0%	61.1%	38.9%

Table 3.3 shows the distribution of availability by policy across the columns. For example, nonprofessional hosts have 36.8% availability in markets without *Total Ban*, and professional hosts have 53.7% availability. Markets with a *Total Ban* in effect have lower availability, with nonprofessional hosts averaging 25% and professionals with

39.7%. With one exception, we see that cities with restrictive policies always have lower availability, ceteris paribus. *Geo Max* is the exception, with professional hosts at a slightly higher availability in markets with this policy, 54.4% compared to 53.4%. One explanation is that an aggregate geographical maximum leads to higher levels of availability among the reduced number of listings who participate in the market.

Table 3.3

Percent Availability Variance by Policy in Place

	Total Ban		Max Days		Geo Max		Only Own Res		Max Host	
	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
nonprof	36.8%	25.0%	37.1%	33.8%	36.0%	39.7%	41.0%	32.6%	37.1%	32.6%
prof	53.7%	39.7%	54.3%	49.6%	53.4%	54.4%	56.1%	49.9%	54.5%	46.2%

While this broad tendency in Table 3.3 towards lower availability in cities with restrictive policies may lead us to believe that the policies achieve their intended effect, Table 3.4 shows that any casual interpretation may be confounded by time trends. We see that percent availability tends to decrease across time. Both host types reduce availability each year, except that professional hosts modestly increased availability in 2019, from 48.1% to 48.4%. We also see that professional hosts consistently offer a higher rate of availability.

Table 3.4

Percent Availability by Year

	2015	2016	2017	2018	2019	All
nonprof	63.0%	44.3%	35.3%	28.5%	27.2%	36.7%
prof	73.1%	64.0%	54.7%	48.1%	48.4%	53.6%

Finally, Table 3.5 shows the increasing rate of professionalization of listings. In 2015, professional hosts managed 36.6% of all listings. In 2019, this metric rose to 53.4% of all listings. The data corroborate evidence from Dogru et al. (2020) that Airbnb is increasingly professionalized.

Table 3.5

Percent of Listings Managed by Professional Host

Year	Percent Professional
2015	36.6%
2016	41.0%
2017	41.3%
2018	47.4%
2019	53.4%
All Years	45.8%

6 Empirical Methods

I use fixed effect models to estimate local government policy and fees' effects on the supply of Airbnb listings that remain in the market. To contrast the heterogeneity of professional and nonprofessional hosts, I estimate two sets of models. The first model assumes that all listings are homogenous and tests hypotheses 1a, 1b, 3a, and 3b. The second model introduces interaction terms to distinguish local policy effects on professional hosts and nonprofessional and will test all hypotheses, including 2a and 2b. All other aspects of the two models are the same.

The first specification tests aggregate policy effects assuming homogenous responses to policy by host type. Therefore, I test hypotheses 1a, 1b, 3a, and 3b using:

$$y_{itm} = \propto_i + \gamma_m + \delta_t + \beta_1 Prof_{imt} + \beta_2 TotalBan_{imt} + \beta_3 MaxDays_{imt} + \beta_4 GeoMax_{imt} + \beta_5 OnlyOwnRes_{imt} + \beta_6 MaxHost_{imt} + \beta_7 LogFee_{imt} + \beta_8 X_{imt} + \varepsilon_{itm} . \quad (1)$$

The outcome variable is percent available, a measure of the percent of days available in one year. Therefore, we interpret the coefficients of the fixed effect model as the percent of days added or decreased in response to an observed change in the independent variables. For example, a coefficient of 0.01 indicates an increase of 3.65 more days available. I index the outcome by i for the individual listing, t for the time observed, and m for the market.

As stated in the data section, a complication of the percent available measure is that nights that guests have already booked will show as unavailable (Li et al., 2016). I make the assumption that the rate at which guests' book in advance does not vary within an individual listing. If true, the individual fixed effect will capture the idiosyncratic propensity to book in advance. Therefore, measuring the change in percent availability will effectively demonstrate changes in the quantity of listing supply.

I use the calendar year for time periods and formal city boundaries as a market. The fixed effect model differences out the individual effects, α_i, of each listing using the within estimator. Additionally, market and time effects are estimated with α_m and δ_t to control for the unique nature of each city and for any evolution across time. β_1 captures the impact of any listing that changes management from a nonprofessional to a professional host. I apply the error term epsilon at the individual listing level. As standard errors are likely to be biased (Abadie et al., 2017), I cluster errors by listing.

The restrictive policies are estimated by β_1 through β_7. These include each dummy variable for local government restrictions - *Total Ban*, *Only Own Residence*, *Max Days*, *Max Host*, *Geo Max* - and the continuous variable of the logged fee to hold a permit. I index each variable by individual listing, the time observed, and the market. The estimated coefficients, therefore, show the effect of implementing a new restriction or a change in the permit fee on the percent of nights available.

The matrix X is a set of control variables that could plausibly interfere with the variables of interest. These include logged average home rent (annual), logged average home price, logged average daily revenue (ADR), logged incoming passengers, and logged nightly price to book the listing. I also include a factor variable for the type of listing – entire home, private space, or shared space. Home rent is a supply factor for hosts who could substitute their Airbnb for a traditional annual rental. Alternatively, a homeowner may choose to sell their home and remove their property from Airbnb. Incoming flight passenger totals serve as a proxy for demand (Dogru et al.,2020b; Yang & Mao, 2019). Hotels substitute for Airbnb stays (Zervas et al. 2017); therefore, I include logged ADR by the market as Airbnb hosts will likely consider hotel rates to set price and quantity supplied. Lastly, price per night will theoretically be positively related to nights supplied.

The second model is identical to equation one, except for an interaction term by host type applied to each restrictive policy. This change enhances the model by distinguishing the heterogeneous effects of professional and nonprofessional hosts and therefore allows us to test each hypothesis, including 2a and 2b, which was not possible with equation 1. The specification is:

$$
\begin{aligned}
y_{itm} = \propto_i + \gamma_m + \delta_t + \beta_1 Prof_{imt} + \beta_2 Prof_{it} Total.Ban_{imt} + \beta_3 Prof_{it} Max.Days_{imt} \\
+ \beta_4 Prof_{it} Geo.Max_{imt} + \beta_5 Prof_{it} Only.Own.Res_{imt} \\
+ \beta_6 Prof_{it} Max.Host_{imt} + \beta_7 Prof_{it} Log.Fee_{imt} \\
+ \beta_8 NonProf_{it} Total.Ban_{imt} + \beta_9 NonProf_{it} Max.Days_{imt} \\
+ \beta_{10} NonProf_{imt} Geo.Max_{imt} + \beta_{11} NonProf_{it} Only.Own.Res_{imt} \\
+ \beta_{12} NonProf_{imt} Max.Host_{imt} + \beta_{13} NonProf_{it} Log.Fee_{imt} + \beta_{14} X_{imt} \\
+ \varepsilon_{itm}. \quad (2)
\end{aligned}
$$

7 Results

Table 3.6 shows the results of equation 1, the aggregate effects of restrictive policy. Four specifications are estimated. The first represents all listings, while two through four use three subsamples of the data - entire home, private space, and shared space, respectively. Because the first specification represents the sample as a whole, we see βs estimated effect for the factor variables of private space and shared space within the X matrix. The entire home factor acts as the omitted category.

We observe *Total Ban*, *Max Days*, and *Geo Max* to test hypotheses 1a and 1b. The results for *Total Ban* in specifications 1, 2, and 3 show small magnitudes at less than 0.1% that are insignificant. While specification 4 has a large magnitude of 9%, the effect increases the percent available. The *Max Day* policy is highly significant with a modest effect in specifications 1 and 2, between 1 and 2%; however, the positive coefficients

Table 3.6

Equation 1 - Fixed Effect Regression Results

	Outcome Variable: Percent Availability			
	all listings	entire home	private space	shared space
	(1)	(2)	(3)	(4)
prof	0.001	0.005*	-0.007	0.003
	(0.002)	(0.003)	(0.004)	(0.022)
Total.Ban	0.0001	0.007	-0.008	0.090***
	(0.009)	(0.010)	(0.016)	(0.029)
Only.Own.Res	-0.050***	-0.056***	-0.036**	-0.018
	(0.008)	(0.009)	(0.017)	(0.084)
Max.Days	0.012***	0.019***	-0.009	0.031
	(0.004)	(0.005)	(0.006)	(0.029)
Max.Host	-0.140***	-0.144***	-0.136***	-0.043
	(0.011)	(0.013)	(0.023)	(0.096)
Geo.Max	-0.066***	-0.067***	-0.065***	-0.128
	(0.011)	(0.013)	(0.021)	(0.108)
log_fee	0.008***	0.007***	0.009***	-0.007
	(0.002)	(0.002)	(0.004)	(0.017)
log_home_rent	0.00004	-0.002	0.012	1.169***
	(0.043)	(0.051)	(0.081)	(0.435)
log_home_price	0.153***	0.176***	0.127*	-0.971**
	(0.039)	(0.047)	(0.073)	(0.384)
log_ADR	0.015*	0.015	0.022	-0.049
	(0.008)	(0.010)	(0.014)	(0.056)
log_passengers	-0.125***	-0.118***	-0.194***	0.524***
	(0.019)	(0.023)	(0.037)	(0.164)
log_price	0.021***	0.014***	0.046***	-0.008
	(0.003)	(0.003)	(0.006)	(0.017)
Observations	492,065	323,525	154,267	14,273
Adjusted R^2	-1.021	-1.038	-1.057	-1.788
F Statistic	1,233.555*** (df = 20; 218758)	824.860*** (df = 18; 143922)	533.074*** (df = 16; 66465)	27.621*** (df = 16; 4677)

Notes: Robust errors reported in parenthesis. Market and time effects are estimated but not displayed. Column 1 includes non-displayed factor variables by listing type.
*p<0.1; **p<0.05; ***p<0.01

indicate that listings increase their availability. Finally, *Geo Max* does show the hypothesized negative coefficient of high significance and a large magnitude, between negative 6.5% and 6.7% across the first three specifications. The effect is even larger for specification 4 for shared spaces but not significant likely due to the small sample size.

Looking at these aggregate effects, our understanding of hypotheses 1a and 1b are mixed. We cannot reject the null for *Total Ban* and *Max Days*. Despite the massive sample, hosts do not reduce their supply in response to these policies intended to restrict market participation. Geo Max supports rejecting the null and accepting the alternate hypothesis; however, we can combine analysis with equation 2 to make a complete conclusion.

Considering Hypotheses 2a and 2b, we can make initial observations from Table 3.6 regarding the aggregate effects. We see that *Max Host* leads the largest aggregate effect of any policy, -14.0% for all listing types, -14.4% for whole home and -13.6% for private rooms with no significant effect for shared spaces. Hosts appear relatively very responsive to this change overall. *Only Own Res* is a smaller yet highly significant effect of -0.050% for all listing types, -0.056% for whole homes, and -0.036% for private rooms.

As we next test hypotheses 3a and 3b, Table 3.6 shows the opposite of the theoretical effect. The log of permit fees shows a large and positive significant effect at the 0.01 p-value. We interpret the result as a 1% increase in the annual fee leads to a 0.7% to 0.9% increase in percent availability for all listings, entire homes, and private space. The effect for shared space is negative as expected in equally sized magnitude, -

0.7% though not significant. We again cannot reject the null hypothesis for 3a and 3b as the sign for all types except shared spaces is positive.

Turning to Table 3.7, we observe the results from equation 2, which expands upon the first equation by estimating heterogeneous effects by host type on the policy variables. These results allow us to test 2a and 2b, policies that aim to influence only professional hosts. We also note an unexpected and persistent pattern of opposing coefficient signs between professional and nonprofessional hosts, complicating our previous analysis of hypotheses 1a, 1b, 3a, and 3b.

We test hypothesis 2b with *Only Own Res* and *Max Host* effects on nonprofessionals. The coefficient for nonprofessional hosts' response to *Only Own Res* is large, highly significant, and unexpectedly negative. The effect for all listings of -6.5% is carried by entire home listings, with a coefficient of -7.4%. The *Max Host* policy creates an even larger magnitude of -15.7% for nonprofessional entire home listings and -13.9% for private spaces, with both effects significant at the 0.01 p-value. We can reject the null hypothesis. These policies have large and negative effects on nonprofessional hosts who reduce their supply dramatically.

We test 2a by looking at the same policies with interactions by professional hosts. *Only Own Res* has a smaller, significant mirrored effect compared to nonprofessional hosts. Entire homes carry the effect (specification 2) with a 4.3% increase in percent available, while private space and shared spaces have smaller and not significant effects. *Max Host* is also positive for entire home professional hosts in slightly smaller magnitude, at 3.6% and significant at the 0.01 p-value. The hypothesized decrease in the supply of professional hosts does not appear, and therefore, we cannot reject the null. Instead, the effect is modest and positive for entire listings.

We can further understand hypotheses 1a and 1b using equation 2 in Table 3,7. While the effect for *Total Ban* doesn't vary much, the signs of the coefficients for both *Max Days* and *Geo Max* are opposite by host type. The effect of *Max Days* for nonprofessional hosts is small yet highly significant for entire homes at 3.1%. The effect is negative and smaller for professional hosts of entire homes, at -2.4%. For shared spaces, the magnitudes are massive and significant at the 0.05 p-value, 10% for nonprofessional and -12.9% for professionals.

Combining these results with equation 1 in Table 3.6, we can now offer conclusions on hypotheses 1a and 1b that examine laws aimed at all hosts. We cannot reject the null for 1a because professional hosts do not convincingly reduce supply. Professional hosts are mainly unresponsive to total bans and show weak evidence of increasing supply to geographical maximums. While the negative signs for *Max Day*s suggest a reduction in supply, the small magnitudes are uncompelling. However, we can accept the alternate hypothesis for 1b because the effect of Geo Max policies is large and highly significant for entire homes and private spaces. Nonprofessional hosts do cut back in the face of this policy, if not *Total Bans* and *Max Days*.

One possible explanation of these unexpected results is nonprofessional hosts are unsure about the expectations of these laws and cut back their supply in the face of ambiguity in line with Uzunca & Andrea's study (2019). The evidence suggests that professional hosts actually increase their supply modestly across many restrictions,

Table 3.7

Equation 2 - Fixed Effects Regression Results

	Outcome Variable: Percent Availability			
	all listings	entire home	private space	shared space
	(1)	(2)	(3)	(4)
nonprof:Total.Ban	-0.0004	-0.005	0.020	0.037
	(0.010)	(0.011)	(0.019)	(0.045)
prof:Total.Ban	0.003	0.046*	-0.071**	0.086*
	(0.019)	(0.024)	(0.033)	(0.049)
nonprof:Max.Days	0.020***	0.031***	-0.001	0.100**
	(0.005)	(0.006)	(0.008)	(0.041)
prof:Max.Days	-0.019***	-0.024***	-0.015	-0.129**
	(0.006)	(0.007)	(0.011)	(0.061)
nonprof:Geo.Max	-0.081***	-0.081***	-0.082***	0.046
	(0.012)	(0.014)	(0.023)	(0.119)
prof:Geo.Max	0.032***	0.033***	0.030*	-0.239**
	(0.009)	(0.011)	(0.017)	(0.097)
nonprof:Only.Own.Res	-0.065***	-0.074***	-0.046**	-0.011
	(0.009)	(0.010)	(0.018)	(0.095)
prof:Only.Own.Res	0.034***	0.043***	0.017	0.016
	(0.007)	(0.008)	(0.014)	(0.091)
nonprof:Max.Host	-0.151***	-0.157***	-0.139***	-0.122
	(0.012)	(0.014)	(0.025)	(0.141)
prof:Max.Host	0.026***	0.036***	0.005	0.058
	(0.008)	(0.010)	(0.015)	(0.089)
nonprof:log_fee	0.011***	0.010***	0.012***	-0.022
	(0.002)	(0.002)	(0.004)	(0.019)
prof:log_fee	-0.007***	-0.007***	-0.005*	0.022
	(0.002)	(0.002)	(0.003)	(0.017)
prof	-0.0001	0.001	-0.003	0.052**
	(0.003)	(0.004)	(0.006)	(0.026)
Observations	492,065	323,525	154,267	14,273
Adjusted R^2	-1.021	-1.037	-1.057	-1.780
F Statistic	916.141*** (df = 27; 218751)	596.445*** (df = 25; 143915)	371.982*** (df = 23; 66458)	20.139*** (df = 23; 4670)

Table 3.7 Notes: Robust errors are reported in parenthesis. Control variables, market effects, and time effects estimated but not displayed. Column 1 includes non-displayed factor variables by listing type.
$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

including *Geo Max*, *Only Own Res*, and *Max Host*. These mismatching sign coefficients appear throughout equation 2 in Table 3.7. As nonprofessional hosts scale back, professional hosts increase supply in smaller increments, perhaps to absorb the exposed capacity.

Finally, examining our last set of hypotheses, 3a and 3b, we again see the signs are opposed by host type. While the nonprofessional coefficients are small, representing a one percentage point for entire homes and 1.2 percentage points for private spaces. We see that a 1% increase in annual permit fees leads to a one percentage point increase in the supply for nonprofessional hosts. We can reject the hypothesis for 3b, as supply increases significantly, rather than remaining unresponsive.

The effectiveness for professionals is milder, yet still highly significant at -0.7 percentage points for entire homes. We also reject 3a, as professionals cut supply in response to higher annual permit fees; a 1% increase in fees leads to a 0.7% decrease in supply.

8 Conclusion

This paper seeks to illuminate the mixed evidence of studies further to show the effect local government policy has on the supply of Airbnb listings. By using a fixed effect model to observe the changes in supply in direct response to changing policy, the results here offer, to the best of the author's knowledge, the first-panel models to assess local policy effects. This paper contributes to the literature by distinguishing the heterogeneous effects of professional and nonprofessional hosts. Across the board of local policy and increases in permit fees, these distinct types of hosts have opposite effects whenever there is a significant effect.

Taken as a whole, professional hosts appear unresponsive to policies that intend to reduce their supply, either targeted or for all hosts. We cannot reject the null hypothesis for 1a or 2a as the evidence is weak that professionals reduce their supply to restrictive policy. Alternatively, nonprofessional hosts do appear to reduce supply both in the face of policies aimed at all host types and unexpectedly even in response to restrictions that do not intend to affect them. We accept the alternate hypothesis 1b that nonprofessionals indeed cut back supply to *Geo Max* policies. We instead maintain the null hypothesis for 2b. Nonprofessionals do cut back supply in response to *Max Host* and in large magnitudes.

Examining permit fees, both types of hosts do appear to react strongly. For a 1% increase, professional hosts of entire homes decrease their intensive supply by an estimated -0.7%. In contrast, a 1% increase causes nonprofessional hosts to increase their supply by an estimated 1% and 1.2% for entire homes and private spaces, respectively. This evidence supports Camerer et al. (1997) finding of amateur taxi drivers. It appears that nonprofessional hosts also vary their supply to meet a quota of earnings rather than acting to maximize profits.

The evidence of this paper suggests that city laws passed to restrict short-term rentals such as Airbnb, VRBO, and HomeAway, do little to affect professional hosts but do affect nonprofessional hosts. Cities should acknowledge that further efforts are necessary to achieve the desired reductions, and among them, increasing permit fees is likely to reduce professional supply based on my analysis. This research may also illuminate other sharing economy markets, such as Turo, where suppliers provide autos for rent.

One limitation of this research is it's a modest sample of cities and local policy changes; with only seventeen municipalities represented, the estimates here may not extrapolate to other United States cities or cities of other countries. Another limitation is that this research only illustrates the responses of existing market participants supply.

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