Platform, Anonymity, and Illegal Actors: Evidence of Whac-A-Mole Enforcement from Airbnb

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Abstract

Airbnb, a prominent sharing-economy platform, offers dwellings for short-term rent. Despite restrictions, some sellers illegally offer their accommodations, taking advantage of a degree of anonymity proffered by the platform to hide from potential enforcement. We study the extent to which enforcement works in Manhattan, one of the most active short-term rental markets, by testing the effects of two recent enforcement events. We demonstrate that prices of entire-home listings in Manhattan increase and vacancies decrease following each enforcement event, suggesting that illegal entire-home listings are being withdrawn from the market, with these effects varying depending on neighborhood characteristics. We further demonstrate that a significant portion of withdrawn listings re-enter the market under the less-enforced listing category of private rooms.

Keywords: Enforcement; anonymity; short-term rentals; platform; illegal supply

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1 Introduction

Over the past years, markets for short-term rental (STR) housing experienced a meteoric rise in popularity. Airbnb, a sharing-economy platform in the hospitality industry, enables landlords (hosts) to rent out lodging to travelers (guests) on a short-term basis. In the vast majority of cases, hosts offer one of three accommodation types: an entire home, a private room or a shared space. While STRs have become a lucrative alternative for landlords compared to traditional leasing, the influx of transient populations can cause a number of nuisance problems for neighboring residents (Guettentag, 2015; Kantz, 2015; Kim et al., 2017). Coupled with these concerns are competitive worries from the hotel industry, and the legal challenges that these markets are creating under a variety of existing regulatory structures from zoning laws to housing codes and tax policies. As a result, STR markets have been attracting considerable regulatory attention in cities around the world.

A number of both local and state governments have recently adopted or are in the process of adopting a variety of policies and regulations, ranging from increased enforcement of existing laws to outright bans. Enforcement of STRs is made difficult due to a certain degree of anonymity that is afforded to hosts as part of what the platform often cites as protecting its users' privacy.[1] For instance, the exact address of a listing is not disclosed except to confirmed guests—potential guests can only view a listing's approximate locale. In addition, the name of a listing's host can be fictitious, and the platform only advertises the host's first, possibly fictitious name.[2] Payments between guests and hosts, for which the platform acts as an intermediary, can be made to any checking account chosen by the host.

Measures have been proposed to hold platforms accountable for illegal listings, but these measures have faced strong and thus far successful legal resistance, citing the protections platforms are afforded under the Communications Decency Act of 1996, as well as securing

[1] `http://www.latimes.com/local/politics/la-me-adv-airbnb-politics-20150405-story.html`
[2] `https://at.law.com/TamQXA`.

a recent Supreme Court victory in a related privacy battle on guest information.[3] Viewed another way, the Communications Decency Act has been used by platforms to fortify the privacy—and thereby anonymity—of sellers, making enforcement of past and new regulation difficult. The literature has begun to examine the effects of greater regulatory restrictions on STRs (Kim et al., 2017); however, the effect of increased enforcement on illegal supply has received surprisingly little attention. In this paper, we seek to fill in this gap. We provide evidence on how regulation events in New York City (NYC) affected the market for STRs in Manhattan. We do so by drawing on policy experiments that took place in November 2015 and in October 2016 in NYC, one of the most active STR markets in the world. These events consisted of a monetary enforcement commitment made by NYC's Mayor and new legal restrictions on STRs passed by the state.

NYC experienced a tenfold increase in Airbnb listings between 2010 and 2014, with its Manhattan borough having one of the highest listing-to-resident ratios in the world (Schniederman, 2014). The rapid rise of the STR market in Manhattan has been coupled with a pushback from regulators and the hotel industry. In the state of New York, it is illegal to rent entire homes (dwellings) on a short-term basis in multiunit buildings, referred to as Class A in the state's Multiple Dwelling Law. However, thousands of hosts skirt these restrictions. In November of 2015, NYC's mayor announced a $10 million investment in the Mayor's Office of Special Enforcement to pursue illegal STRs. Approximately one year later, New York State enacted an amendment to its Multiple Dwelling Law to prohibit residents from advertising their homes for rent for less than 30 days. Renting a shared or spare room is generally permissible provided the host resides on the premises. Violators are to be fined $1,000 for the first offense, $5,000 for the second, and $7,500 for additional offenses.[4]

[3] http://www.govtech.com/dc/articles/Supreme-Court-Protects-Privacy-for-Hotel-Airbnb-Guests.html

[4] These enforcement activities were preceded by the following: On 4/21/2014, the state Attorney General filed an affidavit in support of a subpoena for Airbnb customer information — information which was later obtained in anonymized format. On 9/13/2014, the city's hotel union joined affordable-housing advocates to lobby the Mayor's office to enforce the state law banning STRs and expand enforcement efforts. On 3/12/2015, NYC lawmakers targeting Airbnb seek to triple the size of the specialized STR enforcement task force, seeking to add at least 25 staffers.

To evaluate the impact of these enforcement developments on illegal actors in the STR market, we construct a theoretical model to generate predictions and use a difference-in-difference framework to test them by looking at the effects of enforcement events on listing prices and vacancies. We use shared-space listings as well as listings in San Francisco as control groups. We find evidence that prices of entire-home listings in Manhattan increase and their vacancies decrease following the enforcement announcements, consistent with illegal supply being withdrawn from the market. Moreover, we further demonstrate the opposite effects for private-room listings, suggesting that at least some illegal entire-home listings that are withdrawn end up re-entering the market as private rooms, a less-enforced category. While the two enforcement events we consider are quite different in nature, we demonstrate that their market effects end up being similar, akin in some sense to taxing the entire-home short-term rental market—driving a wedge between demand and supply, raising prices and reducing vacancies. We also demonstrate evidence of a competitive effect, where those entire-home listings that have a larger number of nearby competitors are more susceptible to enforcement actions. Our model indicates that this is because, for the same amount of supply withdrawn, competition dampens the ability of listings in more competitive neighborhoods to raise prices to counteract an increased risk of enforcement.

Our findings are in line with the Beckerian theory (Becker, 1968) regarding the behavior of illegal actors. That is, laws that ease enforcement, increase punishments, and/or commit resources to enforcement can trigger a response in terms of illegal supply being withdrawn and potentially diverted. The relative anonymity that sellers enjoy on the platform acts as a facilitator for illegal operators, but those illegal operators appear to closely follow market conditions and choose profit-maximizing activities that account for their risk of both detection and enforcement in equilibrium. These findings have policy implications, not only for short-term rentals in New York and in other municipalities, but also for other markets where illegal actors can operate behind a platform-facilitated veil of anonymity.

1.1 Literature Review

The widespread illegality of STRs often results from zoning codes and other ordinances municipalities have enacted to protect residential land from commercial-leaning uses without special permits or licenses (Gottlieb, 2013). There are legitimate reasons, including security, nuisance concerns, and other negative externalities for maintaining such laws.[5] Kim et al. (2017) show that such negative externalities can be demonstrated with a decreased value of real estate when a large number of STRs in a neighborhood are present. There is also evidence that the condition and uses of individual buildings have spillover effects on other properties in a neighborhood (Campbell et al., 2011; Ellen et al., 2013; Schuetz et al., 2008). An abundance of STRs in large cities with tight housing supplies may exacerbate existing housing issues. Opponents of STRs in San Francisco, for instance, claim that STRs are reducing the city's already-limited housing supply and are leading to higher rents (Said, 2012; Gyourko et al., 2015).

While Airbnb recognizes that a significant portion of its advertised STRs violate laws or landlord-tenant contractual agreements, the company maintains that it should not bear responsibility for policing its users, instead relying on users themselves to follow local laws and lease contracts.[6] Additionally, Airbnb has been engaged in lobbying efforts in a number of geographic regions, with particular focus on NYC, and has aimed to turn the city into a 'model city' for regulations (Clampet, 2013; Guttentag, 2015). While the legal battles are still commencing and the regulatory environment remains fluid, enforcement, meanwhile, has often been negligible and complaint-driven.[7]

This paper contributes to the growing literature on the sharing economy and Airbnb.[8]

[5] See, for instance, https://nyti.ms/2C2ogND and http://www.sfgate.com/realestate/article/Short-term-rentals-disrupting-SFhousing-market-3622832.php.

[6] See, e.g., https://nyti.ms/2kd34XU.

[7] See, e.g., https://www.ft.com/content/3284eeb4-c452-11e2-bc94-00144feab7de and http://www.crainsnewyork.com/article/20130512/TECHNOLOGY/305129987.

[8] Recent works include Zervas et al. (2016), Edelman et al. (2017), Fradkin (2017), Fradkin et al. (2017), and Jia and Wagman (2017).

Lee et al. (2015) point out that host reputation, including the number of reviews, host responsiveness, and host tenure, can impact the price of a listing. Zervas et al. (2015) indicate that Airbnb listings have higher average ratings compared to the hotel industry. Wang and Nicolau (2017) document that host attributes are the most important price determinants of Airbnb listings. Our work complements the above by shedding some light on how illegal actors respond to enforcement events, when accounting for the different listing-specific attributes that these aforementioned works have documented to be important.

This paper also connects with the literature that looks at enforcement in other markets where illicit goods are sold. Also following a Beckerian theory, Chan et al. (2018), for instance, show that in the market for illicit drugs, laws that ease enforcement may not necessarily diminish supply but can instead divert it towards fewer sellers. In our context, we see a diversion of supply towards a less-enforced market category.

Our paper is most related to Kim et al. (2017) who study the effects of a new housing ordinance that prohibits STRs. They utilize a natural experiment that took place on an island off the coast of Florida. The island, comprised of three townships and several housing zones, enacted an ordinance that required housing rentals in particular residential zones of a particular township to be at minimum for 30 days (i.e., disallowing STRs). With enforcement and detection being less of an issue in that particular small locale, they identify the externalities that STRs can cause by examining changes in real-estate prices. Our focus here is quite different though complementary, in that rather than examining externalities, we are interested in the effect of enforcement on the behavior, as evidenced through changes in listing price and vacancy, of illegal actors in a much larger and denser STR market.

The remainder of the paper is organized as follows. Section 2 presents the regulatory background in New York City, and Section 3 presents the theoretical model. Section 4 describes the data set. Section 5 presents our empirical methodology, and Section 6 reports our findings. Section 7 concludes.

2 Regulatory Background

In the state of New York, it is illegal to rent entire dwellings in buildings with more than two residential units, referred to as Class A in NY Multiple Dwelling Law (passed in 2010) for less than 30 days.[9] Most residential buildings in New York City fall under the Class A classification (Office of the Attorney General of the State of New York, 2014). From the perspective of the state and city, shared-space and private-room listings are permissible provided the host is a permanent occupant of the premises.[10] In contrast, renting entire residences (entire-home listings) for less than 30 days is illegal. Despite their illegality, thousands of hosts offer entire-home listings as STRs, and many of these listings are concentrated in Manhattan. It is important to emphasize that our focus here is on illegalities that stem from state and city laws, and not violations of homeowner association rules, property management rules or violations of contractual lease agreements between landlords and long-term tenants.

Although the 2010 amendment of the Multiple Dwelling Law was originally aimed at illegal hotels and boarding houses, as the STR market expanded, opponents of STRs became increasingly vocal about the fact that the Multiple Dwelling Law also rendered nearly all entire-home listings in NYC illegal. In 2014, the state's attorney general office issued a series of subpoenas requesting data on Airbnb listings for the previous three years.[11] Subsequently, Airbnb provided the attorney general with anonymized data. In October 2014, the attorney general's office released the report '*Airbnb in the City*,' summarizing the information that was gathered.[12] The report indicated that 72% of the private STRs in NYC were noncompliant

[9]The Multiple Dwelling Law, originally adopted in 1929, was amended in 2010 by a bill sponsored by Senator Liz Krueger to clarify the definition of illegal hotels. A 'Class A' building is one that "is occupied, as a rule, for permanent residence purposes. This class shall include tenements, flat houses, maisonette apartments, apartment houses, apartment hotels, bachelor apartments, studio apartments, duplex apartments, kitchenette apartments, garden-type maisonette dwelling projects, and all other multiple dwellings except class B multiple dwellings" (New York State Assembly 2010).

[10]N.Y. Mult. Dwell. Law Art. 4, 121.1. "Permanent residence purposes" are defined as the "occupancy of a dwelling unit by the same natural person or family for thirty consecutive days or more."

[11]See Airbnb v. Schneiderman, 989 N.Y.S.2d 786, 788–89 (Sup. Ct. 2014).

[12]NY State AG Office, Airbnb in the City (Oct. 2014): https://ag.ny.gov/pdfs/AIRBNB%20REPORT.pdf

with the Multiple Dwelling Law (with the Manhattan borough standing at a significantly higher percentage due to its small number of non-class A dwellings).[13]

Since early 2015, New York City's enforcement efforts have been led by specialized task force operating out of the Mayor's of Special Enforcement Office—a quality-of-life-focused unit which includes building inspectors, police, financial investigators, and city attorneys. Their enforcement procedures had been 'passive', driven primarily by resident complaints and followed up by in-person inspections. However, in November 2015, the Mayor's office announced a surprise commitment of $10 million to empower the task force to actively sniff out illegal STRs, promising a shift from the prior passive enforcement approach. The task force could, for instance, use funds to make shill reservations at listings in order to identify their physical addresses and owners.[14] This event is the first policy treatment in our empirical analysis.

State legislators meanwhile separately pursued their own legislation to enhance the enforcement of illegal listings, an effort that faced stiff resistance from Airbnb along the way and whose outcome had been uncertain up until the Governor decided to sign it, despite ongoing negotiations between Airbnb and the Governor's office. On October 21, 2016, Governor Cuomo signed the legislation into law, amending the state's Multiple Dwelling Law to prohibit the *advertising* of "occupancy or use" of units in Class A multiple dwellings for purposes other than permanent residence. This law places the offense at the solicitation stage, instead of requiring the Mayor's Office to investigate whether a transaction had already occurred. Moreover, this law identifies the host themselves, posting the ad, as the violators, rather than the building owner or landlord. While sans cooperation from the platform it may be difficult to de-anonymize hosts, the law raises the stakes for illegal actors. This event is the second policy treatment in our analysis.

[13] Office of the Attorney General of the State of New York 2014, https://ag.ny.gov/press-release/ag-schneiderman-releases-report-documenting-widespread-illegality-across-airbnbs-nyc.

[14] The Mayor's office issues violations to building owners, or landlords, rather than offending tenants. If applicable, landlords can pursue their own resolutions with tenants in accordance with their lease agreements.

3 Model

We use a price-theory model that focuses on a no-arbitrage condition in the housing asset market. Specifically, assume that landlord-operators of rental properties have several options for renting out a dwelling: renting to long-term tenants (L), and renting the property short term either as an entire-home listing (E) or as a collection of private rooms (P).[15] Each option yields a per-period cashflow denoted by either $L(m_l)$, $E(m_e)$ or $P(m_p)$, respectively, where m_θ, $\theta \in \{l, e, p\}$, denotes the number of competing landlords in each housing category.[16] For simplicity and at no qualitative loss, we assume that these cashflow functions are continuous in the number of competitors.

Renting a property short term requires some additional periodized costs c relative to renting to long-term tenants. These added costs can encompass the monetary value of any additional time, effort, and periodized rates (e.g., rental rates, imputed or not) of necessary items such as furniture. In addition, renting a property short term as an illegal listing incurs an expected cost from potential enforcement, denoted by Γ_e and Γ_p for entire-home and private-room listings, respectively.

We assume that rental income is determined from the standard interaction between supply and demand for a given rental category in a neighborhood. Hence, holding constant the demand for rentals in any given time period, an increase in m_θ decreases rental income because of the increase in supply. The equilibrium number of landlords in each category is determined from the no-arbitrage condition:

$$L(m_l^*) = E(m_e^*) - c - \Gamma_e = P(m_p^*) - c - \Gamma_p.$$

[15] We assume that the option of renting a residence as a collection of (primarily) shared spaces is dominated by renting either to long-term tenants or as a collection of private rooms. This appears to be the case in our data, indicating that hosts who rent shared spaces in fact tend to reside on the premises.

[16] We assume that the three categories are competing independently. While this is more plausible for the long-term and short-term categories, it is not obvious for entire-home and private-room short-term listings. However, in the context of our model, as long as such an interaction is not first-order, the nature of the result is unchanged.

Consider an increase, due to legislation or enforcement resource commitments, in the probability of being subject to enforcement of illegal entire-home listings. This entails an increase in the expected cost from enforcement for those listings, denoted by $\hat{\Gamma}_e$, such that $\hat{\Gamma}_e > \Gamma_e$. Given this increase, some existing hosts of illegal entire-home listings will switch over to renting their dwellings either to long-term tenants or as private rooms.[17] In particular, the new equilibrium satisfies:[18]

$$L(m_l^{**}) = E(m_e^{**}) - c - \hat{\Gamma}_e = P(m_p^{**}) - c - \Gamma_p.$$

From this new no-arbitrage condition, it is straightforward to see that as a result of the increase in the expected cost from enforcement, $\hat{\Gamma}_e - \Gamma_e$, we have that $m_l^{**} > m_l^*$, $m_e^{**} < m_e^*$, and $m_p^{**} > m_p^*$ are satisfied.[19]

We summarize the above observations in the following proposition.

Proposition 1 *Given a relative increase in the enforcement of illegal entire-home listings, some illegal entire-home hosts withdraw their listings. Of those hosts who withdraw, a portion then proceeds to re-enter their accommodations as private-room listings, whereas others switch to renting to long-term tenants.*

Our theory thus implies that following an enforcement event, some entire-home listings will be withdrawn from the market. While an enforcement event may also have some chilling effect on demand, we believe that any such effect will be second-order relative to the impact

[17] An increase in the expected costs from enforcement for illegal private-room listings, as well as increases in punishments if caught, can also be incorporated. Provided the overall expected increase in cost is lower for private-room listings than the corresponding increase for entire-home listings, which we believe is the case in our empirical context, the proceeding result is unchanged.

[18] Some of the periodized costs c may be considered as sunk (e.g., some of the necessary home durables), and incorporating this consideration does not change the qualitative nature of the result.

[19] While it can be argued that the probability of being subject to enforcement decreases in the number of competing listings, that is, that there is safety in crowds, such that $\Gamma_e(m_e)' < 0$ and $\Gamma_p(m_p)' < 0$, incorporating this consideration does not change the result. However, it entails that, ceteris paribus, more illegal entire-home listings are withdrawn and more of those withdrawn listings are re-entered as private rooms than rented to long-term tenants. In our data, we see that any such safety-in-crowds effect is second-order and dominated by a competitive effect (i.e. of having more competition).

on supply. It thus follows that demand increases for listings that remain in the market, whereby simple profit-maximization that sets marginal revenue equal to marginal cost entails higher listing prices and lower within-listing vacancies. Consequently, as depicted in Figure 1, we predict that for entire-home listings that remain in the market after an enforcement event, listing prices increase and vacancies decrease.[20] We further predict that the opposite holds for private-room listings; that is, following an analogous argument, prices decrease and vacancies increase, due to some of the illegal supply re-entering under this listing category.

It further follows from the above no-arbitrage condition that in some neighborhoods, given a greater extent of local competition, a relative inability by listing operators to raise prices means that the withdrawal of entire-home listings is more pronounced following an increase in enforcement, whereas the change in cashflow is smaller. To see this, note that if operators face competitive pressures and the change in cashflow, $E(m_e^{**}) - E(m_e^*)$, is relatively small ceteris paribus, and insufficient to offset $\hat{\Gamma}_e - \Gamma_e$, then more listings need to be withdrawn. That is, a lower $|E'(m_e)|$ entails that $E(m_e^{**}) - E(m_e^*)$ shrinks while $m_e^{**} - m_e^*$ increases ceteris paribus, in order to satisfy the no-arbitrage condition. Intuitively, due to a relative inability to raise their prices, marginal operators in more competitive neighborhoods face greater pressures to withdraw their listings after an enforcement event. We thus hypothesize that in more competitive neighborhoods, an increase in enforcement of illegal entire-home listings entails lower relative increases in price and greater relative reductions in vacancies.

We summarize our theoretical predictions regarding the effect of increased enforcement of entire-home listings as follows:

Hypothesis 1: Prices of entire-home listings would increase and vacancies decrease.

Hypothesis 2: Prices of private-room listings would decrease and vacancies increase.

Hypothesis 3: The change in entire-home prices would tend to be lower and the change in entire-home vacancies higher in neighborhoods with more entire-home listings.

[20] While a rightward shift in demand could generate similar movements in prices and vacancies, we believe it is highly unlikely that demand—specifically demand for entire-home listings, and not other listing categories—reacted positively to the two enforcement events.

4 Data

We begin by collecting all consumer-facing information on the complete set of Airbnb listings in Manhattan between 2015 and 2017. This dataset has monthly scrapes at slightly irregular intervals.[21] Each listing is identified by a unique identifier and comes with time-invariant characteristics such as its host's unique identifier, neighborhood, approximate locale (latitude and longitude in six-digit decimal format that indicate the approximate coordinates of a listing), and property type (entire apartment, private room, or shared space). Listing information also contains a number of time-variant characteristics such as listing price,[22] the number of days during which the property is available for booking over the next 30, 60, or 90 days, number of reviews, review rating, cancellation policy, minimum nights per stay, the maximum number of guests, a measure of the host's experience (number of days since the host's first listing was created), review gap (number of days since the latest review), whether the listing is offered for instant booking (i.e., without requiring host approval), the host's average response time and response rate to guest inquiries, and whether the host has a so-called 'Superhost' badge.[23]

Since our focus is on short-term rentals, we restrict attention to listings that are offered for rent for fewer than 30 days, removing observations with a minimum number of nights that is greater than 30. We also exclude observations with listing prices per night that exceed \$1000 because some hosts may set their rates prohibitively high in lieu of blocking their calendars. We exclude observations before August 2015 because such observations do not

[21] There are 30 scrapes in total including Jan, Mar, April, May, Jun, Aug, Sep, Oct, Nov (two scrapes), Dec in 2015; and Jan Feb, April to Dec in 2016; and Jan to Aug in 2017. The data is publicly available at `InsideAirbnb.com`.

[22] Hosts may adjust prices of individual days. The listing price represents the "base price" chosen by the host for the listing, i.e., the price for days that are not specifically edited by the host. It is also the price potential guests observe when they do not enter specific dates. This price is particularly useful for our analysis because it can readily enable hosts to raise prices throughout their calendars with one action; that is, we expect to see changes in this price as a result of new enforcement events.

[23] Hosts who meet the following criteria receive a Superhost designation: (i) Hosted at least 10 guests in the past year; (ii) maintained a high response rate and low response time; (iii) received primarily 5-star reviews; (iv) did not cancel guest reservations in the past year.

contain important controls such as cancellation policies, whether instant book was offered, and host response times and rates. We use the roughly monthly scrapes between August 2015 and August 2017, comprising 456,161 observations and 10,366 entire-home listings.

Next, we construct a measure of competition for each listing by using geographical mapping software to count the total number of other listings of the same type that are located in close proximity. We define close proximity by forming a geographic circle of radius 0.1 and 0.3 miles around each listing based on its coordinates. This calculation is repeated for each time period, so these count measures are time-varying, and represent the parameter m_θ in our theory. We also calculate the number of days that are vacant for each listing in the period of 30-to-60 days ahead of each data scrape. We focus on this timeframe for two reasons. First, some guest reservations for this time window are more likely to still be forthcoming and thus may possibly be affected by an enforcement announcement that had been made since the previous data period (i.e., days 0-to-30 are more likely to have been previously booked by guests). Second, 30-to-60 days is not too far in the future, making it more likely that potential forthcoming reservations fall or partially fall in those dates, to make for a meaningful comparison among listings.

Panel A of Table 1 presents summary statistics for the three types of listings (entire-home, private-room, and shared-space) in Manhattan. We separately report public listing information and our measures of competition. The bottom two rows give the average number of same-type competitors in a 0.1-mile and 0.3-mile geographical radius. They indicate that there are, on average, 43 entire-home competitors in a 0.1-mile radius and 301 competitors in a 0.3-mile radius. Figure 2 depicts trends of the number of listings and average listing prices for each of the three listing types over time. Figure 2(a) shows that the number of entire-home listings stays roughly the same overall but contains two significant downward fluctuations, each occurring after an enforcement announcement. In contrast, the number of private-room listings increases over time. Average prices for all listing types seem to follow a similar pattern in Figure 2(b).

As an additional control group, we further collect data on listings in San Francisco and the number of nearby competitors for each of those listings. We choose San Francisco because the city made no enforcement announcements during the relevant timeframe and because of similarities in its short-term rental market conditions (i.e., a large influx of visitors and tight housing supply). Panel B of Table 1 shows summary statistics for the three types of listings in San Francisco. The average listing prices of entire-home and private-room listings are higher than those in Manhattan whereas the 30-day vacancies are similar. Trends for San Francisco are provided in Figure 3.

5 Empirical Methodology

We examine the effect of the enforcement events with a difference-in-difference methodology, using entire-home and private-room listings as separate treatment groups and, initially, shared-space listings as the control group. The basic regression specification we estimate is given by

$$y_{jkt} = \alpha_t + \alpha_j + \delta X_{jkt} + \beta EC_{kt} + \varepsilon_{jkt}, \tag{1}$$

where j identifies listings according to their unique ID, k denotes listing type, t indexes time, y_{jkt} is the dependent variable of interest (the logarithm of listing price or the logarithm of the vacancy rate), α_t and α_j are month and listing fixed effects, X_{jkt} are control variables, EC_{kt} is a dummy variable that equals one if an enforcement announcement that is applicable to listing k had been made and zero otherwise, and ε_{jkt} is an error term. This methodology controls for fixed differences between treated and untreated listings via listing fixed effects. The month dummies control for aggregate fluctuations. Our estimate of the enforcement effect is β.

As previously indicated, shared-space listings, due to their lower profitability both compared to private-room listings and to traditional longer-term leasing, are unlikely to suffer from illegalities at the state and city level nor from indirect effects from the two treatments in our test. We thus believe they form a reasonable control group for testing the impact of the enforcement events using a same-geography difference-in-difference specification.

To provide an additional check, we also consider same-category listings in San Francisco as alternative control groups. From the summary statistics (Table 1, Panels A and B) and listing trends (Figures 2 and 3), it is apparent that the two geographies share some similar market conditions. In particular, the two peaks of entire-home listings in Figures 2a and 3a are apparent during holiday seasons. Both geographies have tight housing supplies, high housing prices, and large inflows of tourism. Importantly, San Francisco did not experience a regulatory change in its STR market during the relevant timeframe. We thus believe that same-category listings in San Francisco form suitable control groups because the development of their members is a good proxy for that of the treatment group had neither policy event occurred. We use a similar difference-in-difference methodology given by

$$y_{iklt} = \alpha_t + \alpha_i + \delta X_{iklt} + \beta EC_{klt} + \varepsilon_{iklt}, \tag{2}$$

where i indexes listings, k denotes listing type, l denotes the city, EC_{lt} is a dummy variable that equals one if an applicable enforcement event had taken place in city l, and other terms are analogous to (1). Standard errors are clustered by room type, imposing no restrictions on the possible serial correlation of the error terms.

6 Empirical Results

6.1 Baseline Specification

We begin by testing how the two enforcement announcements affected the listing prices and vacancies of entire-home listings. The first enforcement event, the Mayor's announcement in November 2015, committed \$10 million to support enforcement activities. The second event, the newly-enacted state bill in October 2016, deemed illegal the advertisement of an entire Class A dwelling for short-term rent, independent of whether a transaction occurred. Because they are quite different in nature, we initially separate the two events. We thus initially divide our sample to the time span from August 2015 to March 2016 for the first event and April 2016 to May 2017 for the second.

Column (1) of Table 2 gives the baseline specification for the first event, using entire-home listings as the treatment group and shared-space listings as the control group, suggesting an average increase of 8.77% in listing price following the Mayor's announcement.[24] The specification in column (2) includes the number of competitors as an additional control and gives a similar increase in listing price. Columns (3) and (4) of Table 2 suggest that the vacancies of entire-home listings decrease by 5.37% to 6.34% following the Mayor's announcement, depending on the specification.[25]

As predicted by our theory, Table 3, in contrast, shows opposite effects on private-room listings, suggesting an average decrease in price of 6.05% to 6.37% and an average increase in vacancies of 3.05% to 3.41%, depending on the specification. While it can be misleading to directly compare the price and vacancy changes between entire-home and private-room listings, it is apparent that the magnitudes of changes are smaller for private-room listings, particularly since there are fewer of them. This would also be consistent with our theoretical prediction that only a portion of withdrawn entire-home listings re-enter as private rooms.

We next test the impact of the second event on prices and vacancies, applying analogous specifications to the first event. Table 4 shows results that, on first glance, parallel those in Table 2. In particular, following the state's bill enactment, listing prices of entire homes increase approximately 13%, depending on the specification. The corresponding decrease in vacancies of entire-home listings is 3.59% to 3.78%. That is, Table 4 indicates that the

[24]We omit reporting the coefficients on most of the controls as they are not our focus. They are available upon request.

[25]We note that an alternative explanation, that prices simply increase to account for the higher expected cost from potential enforcement, would entail an *increase* in vacancies via an upward movement along the demand curve, which cannot be reconciled with the fact that vacancies in fact decrease.

second event has a higher impact on listing price and a lower impact on vacancies relative to the first event. From our theoretical model, an explanation for this is that marginal hosts, who are more likely to withdraw following an increase in enforcement, have already withdrawn after the first event. Any additional entire-home listings withdrawn, due to a relative decrease in the number of nearby competitors, would have a more pronounced effect on price, entailing that fewer entire-home listings need to withdraw in order to satisfy the no-arbitrage condition.[26]

In line with our predictions, Table 5 again shows opposite effects for private-room listings, with prices decreasing 6.05% to 6.35% and vacancies increasing by 3.37% to 3.75%. Due to their smaller number, these changes also seem to indicate that only a portion of withdrawn entire-home listings re-enter as private rooms, though we again caution that a direct comparison can be misleading.

6.2 Market Dynamics

A potential concern is that the short-term rental market fallout from the Mayor's announcement may interact with the effect of the second, state law event. The specifications above, while yielding results that are consistent with our hypotheses, do not account for these dynamics—for instance, how quickly the prices of entire-home listings rise after an enforcement event and whether the first event accelerates the impact of the second, stabilizes it, or mean reverts it. That is, the preceding specifications may obscure any reverse causality relationship. To account for these dynamics, we adopt the method of Autor (2003).[27] Specifically, we add indicator variables for 1 and 2 months before the state's bill adoption, months 0 to 3 after adoption, and month 4 onward. Among these seven indicators, the first

[26]While it is also possible that any potential chilling effect on demand was more pronounced following the enactment of the state bill than following the prior event, the steep rise in price seems to suggest otherwise—in fact the relative changes seem to indicate that any chilling effect was less of a factor following the state bill than following the Mayor's announcement.

[27]Autor (2003) uses lag and lead on a yearly basis. We use the lag and lead on a monthly basis because of the rapid pace of events and the structure of our data.

six are equal to one only in their corresponding month period, while the final indicator is equal to one in each month beginning with the fourth month following the adoption of the state bill. We present results from this test for the enforcement effect on listing price; results for vacancy are analogous.

Column (1) of Table 6 presents the base specification augmented with the leads and lags. This specification suggests that the coefficients on the bill adoption leads are close to zero, showing little evidence of an anticipatory response from entire-home listing hosts, e.g., due to anticipating the bill or due to the preceding Mayor's announcement. In the month of the new bill's passage, entire-home listing prices increase substantially by 13%, after which this increase eventually settles at 11.4% in months 4 onward. Column (2) repeats this exercise, adding additional controls and the number of competitors. The pattern of coefficients is comparable in each case, providing robust evidence that the newly adopted bill led to a rise in the price of entire-home lisitngs. In column (2), our preferred specification, the impact on listing price is 12.91% in months 4 onward.

6.3 Re-entry of Illegal Supply

Since they are not as lucrative as the other listing categories, we believe shared-space listings are less likely to attract illegal operators, and thus imply the highest likelihood of legality in Manhattan from the state's and city's perspectives. As the same time, as a result of their higher likelihood of legality, shared-space listings are unlikely to be targeted to the same extent as the other listing cateogories by law enforcement, which raises the concern that in order to skirt restrictions, entire-home listing hosts may also re-enter as shared-space listings following an increase in enforcement. Hence, a potential issue is that withdrawn illegal entire-home listings re-enter as shared-space listings, which would overstate our previous estimations of the effects on prices and vacancies.

To examine the above, we use another city as a control group. Specifically we contrast price and vacancy changes in NYC listings with listings in San Francisco according to the specification in (2). We add city-specific time trends to account for housing-market trends in each city. We choose San Francisco due to its STR market similarities to NYC in terms of a large number of visitors and a tight housing supply, and because San Francisco had no legal changes to its STR market in the relevant time period. Due to data limitations, we only apply this test to the second enforcement event.

For entire-home listings, column (1) of Table 7 shows the baseline specification without city-specific time trends, and suggests an average increase of 11.26% in listing price following the adoption of the new state bill. Adding city-specific time trends, column (2) indicates a comparable increase of 11.79%. Columns (3) and (4) show comparable results on vacancy, indicating entire-home listings in Manhattan faced a 5.59% and 6.03% decrease, respectively, following the enactment of the state bill. These results reaffirm the findings from our previous specifications regarding the withdrawal of entire-home listings from the market.

Also in line with our prior estimations, panel A in Table 8 shows the opposite effects on price and vacancy for private-room listings. Under the baseline specification without city-specific time trends, column (1) suggests an average of 5.33% decrease in listing price following the adoption of the new state bill. Adding city-specific time trends, column (2) indicates a comparable 5.69% decrease. Columns (3) and (4) indicate vacancies of private-room listings in Manhattan faced a 3.01% and 3.26% increase, respectively, following the enactment of the bill. These results also reaffirm our previous findings regarding the re-entry of some illegal supply, previously listed as entire homes, as private-room listings, suggesting that at least some illegal supply is being diverted to the less-enforced private-room category.

Crucially, panel B in Table 8 indicates that no such price or vacancy effects exist for shared-space listings, in line with the assumptions underlying our model and prior estimations. That is, while we observe market indicators of re-entry by withdrawn entire-home listings, we only observe them under the private-room category.

6.4 Enforcement and Competition

The effects of new enforcement events may depend on micro-level market conditions. Our belief is that, in line with Becker (1968), illegal hosts behave rationally, and, accounting for the potential costs associated with illegalities, make available listings that line up with market demand for their respective neighborhoods. For instance, entire-home hosts whose listings are located in areas of Manhattan that face high demand for STRs may be more inclined to continue hosting illegally. Similarly, those in neighborhoods with a large number of listings may feel a certain 'safety in numbers' from the risk of enforcement actions. Perhaps more importantly, hosts in more competitive neighborhoods may, on the margin, find it more difficult to raise prices despite an increase in enforcement. In line with our theory, we hypothesize that this latter effect dominates; that is, we hypothesize that hosts who face stiffer competition are less likely to raise their prices following an enforcement announcement.

To explore these dynamics, we use our measure of competition, a geographical radius of 0.1 miles around the approximate coordinates of each listing to tally up the number of competitors of each type in each period; the results with a 0.3 mile radius are similar. We conjecture that the competitive effect from having more nearby entire-home listings would dampen the impact of an enforcement event on their listing price. To test for a competitive effect, we use the median number of entire-home listings in a 0.1-mile radius to divide the treated and control groups into two subgroups each—one with a high degree and one with a low degree of competition. To stay consistent with the previous section, we focus on the second enforcement event—the state bill enactment. We use an ex-ante measure (i.e., the number of competitors before the law passed) to construct these subgroups. We estimate the following (triple) difference-in-difference-in-difference specification:

$$y_{jknt} = \alpha_t + \alpha_i + \delta X_{jlnt} + \beta_1 EC_{lt} + \beta_2 EC_{lt} * Comp_{nt} + \varepsilon_{jlnt}, \tag{3}$$

where j indexes Airbnb listings, l indexes listing location (city), n indexes listing subgroup (high or low competition), t indexes time, EC_{kt} is a dummy variable that equals one if an applicable enforcement announcement had taken place, $Comp_{nt}$ is an indicator that equals one if the listing is in a low-competition subgroup, and all other terms are analogous to (1).

This methodology controls for the fixed differences between treated and untreated listings via listing fixed effects, and it also captures the triple-difference estimates which distinguish between listings located in more and less competitive environments. The month dummies control for aggregate fluctuations. We use entire-home listings in San Francisco as our control group since, due to their small numbers, the triple-difference specification for shared-space listings in Manhattan is difficult to estimate. However, we obtain similar results (though slightly overstated, due to re-entry under the private-room category by withdrawn entire-home listings) when using private-room listings in Manhattan as the control group.

Our difference-in-difference estimate of the enforcement effect is β_1, which captures average differential changes in listing price from the pre- to post-treatment period for the more-competitive listings in the treatment group relative to changes in listing price for the more-competitive listings in the control group. The triple-difference estimate is β_2, which captures differences in the difference-in-difference estimates for less-competitive listings. The total treatment effect (i.e., the effect due to the enactment of the state bill) is $\beta_1 + \beta_2$.

Table 9 shows the results of our regressions. Column (2) indicates that high-competition listings face a 5.21% price increase following the state bill enactment, whereas prices of low-competition listings face a 8.37% increase, suggesting a more pronounced enforcement effect on price for listings in low-competition environments. Column (4) indicates that high-competition listings incur a 5.55% reduction in vacancy compared to low-competition listings which incur a vacancy reduction of 2.20%. The competition effect on vacancy suggests that marginal hosts of entire-home listings in high-competition environments, unable to raise prices as highly in response to enforcement changes, are more likely to withdraw from the market, in line with our theoretical predictions.

7 Conclusion

We constructed a model and used a difference-in-difference approach to test the effects of two recent enforcement events on the behavior of illegal short-term rental operators in Manhattan, one of the most active short-term rental markets. While the two enforcement events are quite different in nature, we demonstrated that their effects were quite similar, akin in some sense to taxing the short-term rental market for entire homes—driving a wedge between demand and supply, raising prices and decreasing vacancy by leading some entire-home listings to be withdrawn. We also showed evidence of a competitive effect, where the prices (vacancies) of entire-home listings that have a larger number of nearby competitors respond less (more) to enforcement actions. We further demonstrated evidence suggesting that at least some of the illegal supply that is withdrawn ends up re-entering the market under the less-enforced category of private rooms.

These findings raise some questions about regulators' methodologies for handling short-term rentals. While it is true that, for Manhattan, under current laws, a significant number of supply-side actors are operating illegally, it would seem possible to obtain more desired aggregate outcomes by simply taxing the market. Such an approach, due to the existing support it already receives from platforms, would not be difficult to implement.[28] Moreover, the approach has the clear benefits of generating some government revenue in lieu of directing resources to enforcement, arguably a deadweight loss, and not creating incentives to re-enter the market under different listing categories.[29]

Our findings also question the optimality of laws that protect the anonymity of sellers on a platform. In our particular context, it is unclear how the 'privacy' of sellers on a platform

[28] https://www.airbnb.com/help/article/653/in-what-areas-is-occupancy-tax-collection-and-remittance-by-airbnb-available

[29] One may argue that at the listing level, a tax would burden law-abiding hosts while potentially reducing the regulatory threat on illegal actors. At the same time, municipalities have been able to strike agreements with short-term rental platforms where licenses are issued to individual operators contingent on qualification and compliance. See, e.g., `http://www.chicagotribune.com/business/ct-biz-homeaway-settles-city-lawsuit-20180614-story.html`.

receives greater weight than the ability of local legislatures to enforce their rules, independent of arguments regarding whether a platform should be held responsible for policing its own users or not. This is perhaps a prime example of how, on the one hand, privacy—or anonymity under the guise of privacy—can create enforcement inefficiencies in the marketplace, in line with arguments by Chicago School scholars such as Posner (1978, 1981) and Stigler (1980). While, on the other hand, privacy is precisely what helps facilitate the arguably more efficient use of a resource, as evidenced through the decisions of illegal listing operators choosing to operate as short-term rentals (not accounting for potential negative externalities). That is, without privacy, illegal short-term rentals, for instance when their addresses are disclosed to enforcement agencies, would surely quickly cease to exist, which goes counter to the Chicago School arguments that argue privacy is inefficient.

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Table 1: Summary Statistics

	Entire Home Rental			Private Room Rental			Shared Space Rental		
	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N
Panel A: Summary Statistics in Manhattan									
Public Information:									
Listing Price ($)	220.36	120.34	269,863	102.04	46.99	172,832	80.04	23.44	13,508
No. Bedrooms	1.12	0.80	269,863	1.02	0.27	172,832	0.11	0.05	13,508
No. Bathrooms	1.10	0.34	269,863	1.09	0.32	172,832	1.01	0.26	13,508
No. Beds	1.75	1.05	269,863	1.14	0.47	172,832	1.09	0.31	13,508
Review Number	19.77	23.94	269,863	21.39	30.87	172,832	23.11	33.16	13,508
Review Rating	4.61	0.70	269,863	4.57	0.47	172,832	4.68	0.33	13,508
Vacancy Rate (next 30 days)	33.18%	0.40	269,863	38.67%	0.42	172,832	37.81%	0.41	13,508
Superhost Proportion	5.79%	0.22	269,863	6.77%	0.18	172,832	3.59%	0.25	13,508
Maximum guests per stay	3.48	1.82	269,863	1.98	0.89	172,832	1.56	0.98	13,508
Minimum night per stay	3.16	4.24	269,863	2.39	3.12	172,832	1.69	1.15	13,508
Appraisal Panel:									
No. of same type competitors (0.1 mile radius)	41.65	27.97	269,863	26.19	15.92	172,832	3.91	6.33	13,508
No. of same type competitors (0.3 mile radius)	301.44	181.41	269,863	152.69	87.81	172,832	31.01	16.79	45,731

Table 1: Continued

	Entire Home Rental			Private Room Rental			Shared Space Rental		
	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N
Panel B: Summary Statistics in San Francisco									
Public Information:									
Listing Price ($)	339.54	105.11	208,765	138.74	59.92	132,972	77.84	37.58	12,777
No. Bedrooms	1.17	0.88	208,765	1.05	0.29	132,972	0.12	0.04	12,777
No. Bathrooms	1.09	0.34	208,765	1.09	0.32	132,972	1.01	0.26	12,777
No. Beds	1.69	1.27	208,765	1.22	0.53	132,972	1.01	0.32	12,777
Review Number	21.33	25.06	208,765	22.89	33.10	132,972	26.33	31.55	12,777
Review Rating	4.65	0.69	208,765	4.61	0.52	132,972	4.72	0.22	12,777
Vacancy Rate (next 30 days)	29.68%	0.51	208,765	32.87%	0.75	132,972	35.97%	0.66	12,777
Superhost Proportion	8.33%	0.25	208,765	7.92%	0.17	132,972	4.29%	0.21	12,777
Maximum guests per stay	3.27	1.66	208,765	1.86	0.91	132,972	1.33	1.08	12,777
Minimum night per stay	2.95	3.78	208,765	2.66	2.39	132,972	1.51	1.22	12,777
Appraisal Panel:									
No. of same type competitors (0.1 mile radius)	28.67	27.97	188,765	19.99	15.92	132,972	3.78	6.33	12,777
No. of same type competitors (0.3 mile radius)	210.54	181.41	188,765	102.37	87.81	132,972	29.33	16.79	12,777

Table 2: The effect of the Mayor's announcement – Entire Home vs Shared Space

	Dep: Log (listing price)		Dep: Log (vacancy)	
	(1)	(2)	(3)	(4)
# of competitors around 0.1-mile radius		-0.0024**		0.0052**
		(0.0009)		(0.0022)
Enforcement effect	0.0877***	0.0923***	-0.0634**	-0.0537**
	(0.031)	(0.026)	(0.025)	(0.027)
Competitor density	Shared Space	Shared Space	Shared Space	Shared Space
Listing attributes Controls	Yes	Yes	Yes	Yes
Neighborhood control	Yes	Yes	Yes	Yes
Listing FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
Observations	78,871	78,871	78,871	78,871
Adjusted R^2	0.6899	0.6996	0.3831	0.4272

Note: The control group is shared-space listings in Manhattan. Standard errors are clustered by neighborhood and reported in parentheses. The number of competitors is mean-centered. Controls for listing attributes not reported include cancellation policy, No. of bedrooms, No. of bathrooms, No. of beds, maximum guests per stay, and No. of listings advertised by the listing's host. ***, ** *and* * indicate significance at the 1%, 5%, and 10% levels.

Table 3: The effect of the Mayor's announcement – Private Room vs Shared Space

	Dep: Log (listing price)		Dep: Log (vacancy)	
	(1)	(2)	(3)	(4)
# of competitors around 0.1-mile radius		-0.0015**		0.0055**
		(0.0007)		(0.0025)
Enforcement effect	-0.0605**	-0.0637***	0.0305***	0.0341***
	(0.027)	(0.025)	(0.011)	(0.012)
Competitor density	Shared Space	Shared Space	Shared Space	Shared Space
Listing attributes Controls	Yes	Yes	Yes	Yes
Neighborhood control	Yes	Yes	Yes	Yes
Listing FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
Observations	41,857	41,857	41,857	41,857
Adjusted R^2	0.6571	0.6945	0.4757	0.4933

*Note: The control group is shared-space listings in Manhattan. Standard errors are clustered by neighborhood and reported in parentheses. The number of competitors is mean-centered. Controls for listing attributes not reported include cancellation policy, No. of bedrooms, No. of bathrooms, No. of beds, maximum guests per stay, and No. of listings advertised by the listing's host. ***, **, and ** indicate significance at the 1%, 5%, and 10% levels.

Table 4: The effect of the newly adopted state law – Entire Home vs Shared Space

	Dep: Log (listing price)		Dep: Log (vacancy)	
	(1)	(2)	(3)	(4)
# of competitors around 0.1-mile radius		-0.0012^{**}		0.0049^{***}
		(0.0005)		(0.0017)
Enforcement effect	0.1297^{***}	0.1322^{***}	-0.0378^{**}	-0.0359^{**}
	(0.045)	(0.038)	(0.017)	(0.015)
Competitor density	Shared Space	Shared Space	Shared Space	Shared Space
Listing attributes Controls	Yes	Yes	Yes	Yes
Neighborhood control	Yes	Yes	Yes	Yes
Listing FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
Observations	147,696	147,696	147,696	147,696
Adjusted R^2	0.7819	0.7932	0.4121	0.5101

Note: The control group is shared-space listings in Manhattan. Standard errors are clustered by neighborhood and reported in parentheses. The number of competitors is mean-centered. Controls for listing attributes not reported include cancellation policy, No. of bedrooms, No. of bathrooms, No. of beds, maximum guests per stay, and No. of listings advertised by the listing's host. ***, **, *and* * indicate significance at the 1%, 5%, and 10% levels.

Table 5: The effect of the newly adopted state law – Private Room vs Shared Space

	Dep: Log (listing price)		Dep: Log (vacancy)	
	(1)	(2)	(3)	(4)
# of competitors around 0.1-mile radius		-0.0019***		0.0063**
		(0.0007)		(0.0029)
Enforcement effect	-0.0605***	-0.0638***	0.0337***	0.0375***
	(0.023)	(0.022)	(0.011)	(0.015)
Competitor density	Shared Space	Shared Space	Shared Space	Shared Space
Listing attributes Controls	Yes	Yes	Yes	Yes
Neighborhood control	Yes	Yes	Yes	Yes
Listing FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
Observations	127,375	127,375	127,375	127,375
Adjusted R^2	0.6651	0.6837	0.4656	0.4738

Note: The control group is shared-space listings in Manhattan. Standard errors are clustered by neighborhood and reported in parentheses. The number of competitors is mean-centered. Controls for listing attributes not reported include cancellation policy, No. of bedrooms, No. of bathrooms, No. of beds, maximum guests per stay, and No. of listings advertised by the listing's host. ***, **, *and* * indicate significance at the 1%, 5%, and 10% levels.

Table 6: The estimated impact of the enacted state law on listing price

	(1)	(2)	(3)	(4)
Leads and lags:				
Bill change$_{t-2}$	0.0213	0.0261	0.0222	0.0290
	(0.056)	(0.058)	(0.053)	(0.048)
Bill change$_{t-1}$	0.0191	0.0275	0.0207	0.0341
	(0.053)	(0.061)	(0.080)	(0.080)
Bill change$_{t}$	0.1310*	0.1281*	0.1296*	0.1357
	(0.073)	(0.071)	(0.066)	(0.088)
Bill change$_{t+1}$	0.1351	0.1375	0.1377	0.1377
	(0.103)	(0.095)	(0.093)	(0.093)
Bill change$_{t+2}$	0.1511*	0.1541*	0.1522	0.1598*
	(0.086)	(0.091)	(0.093)	(0.095)
Bill change$_{t+3}$	0.0918**	0.0959*	0.0924*	0.0977*
	(0.045)	(0.051)	(0.047)	(0.050)
Bill change$_{t+4\ \text{forward}}$	0.1140**	0.1291***	0.1209**	0.1309***
	(0.043)	(0.041)	(0.049)	(0.046)
Control Group	Shared Space	Shared Space	Private Room	Private Room
# of Competitors Controls	Yes	Yes	Yes	Yes
Listing attributes Controls	Yes	Yes	Yes	Yes
Listing FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Neighborhood × Time	No	Yes	No	Yes
Neighborhood × Time^2	No	Yes	No	Yes
Neighborhood × Time^3	No	Yes	No	Yes
Observations	149,783	149,783	242,384	242,384
Adjusted R^2	0.8129	0.8133	0.8544	0.8545

*Note: Standard errors are clustered by neighborhood and reported in parentheses. Controls for listing attributes not reported include cancellation policy, No. of bedrooms, No. of bathrooms, No. of beds, maximum guests per stay, and No. of host listings. Bill change dummies $t_{-2} - t_{+3}$ are equal to 1 in only 1 month each. The Bill change$_{t+4\ forward}$ dummy is equal to 1 in every month beginning with the fourth month after adoption. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.*

Table 7: Enforcement effect on entire-home listings – rentals in SF as control group

	Dep: Log (listing price)		Dep: Log (vacancy)	
	(1)	(2)	(3)	(4)
# of competitor around 0.1-mile radius	-0.0019***	-0.0017**	0.0055**	0.0061**
	(0.0006)	(0.0007)	(0.0021)	(0.0025)
Enforcement Effect	0.1126***	0.1179***	-0.0559***	-0.0603***
	(0.039)	(0.043)	(0.012)	(0.017)
Listing attributes Controls	Yes	Yes	Yes	Yes
Listing FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
City × Time	No	Yes	No	Yes
City × Time^2	No	Yes	No	Yes
City × Time^3	No	Yes	No	Yes
Observations	200,145	200,145	200,145	200,145
Adjusted R^2	0.7142	0.7146	0.4621	0.4627

*Note: The control group is entire-home listings in SF. Standard errors are clustered by city and reported in parentheses. Controls for listing attributes not reported include cancellation policy, No. of bedrooms, No. of bathrooms, No. of beds, maximum guests per stay, and No. of host listings. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.*

Table 8: Enforcement effect on private-room and shared-space listings – SF as control group

	Dep: Log (listing price)		Dep: Log (vacancy)	
	(1)	(2)	(3)	(4)
Panel A: Private-room listings in Manhattan (treated) vs private rooms in SF (control)				
# of competitors in 0.1-mile radius	-0.0013***	-0.0015***	0.0048**	0.0053**
	(0.0003)	(0.0005)	(0.0026)	(0.0024)
Enforcement Effect	-0.0533***	-0.0569***	0.0301***	0.0326***
	(0.021)	(0.016)	(0.005)	(0.009)
Observations	142,781	142,781	142,781	142,781
Adjusted R^2	0.7701	0.7272	0.5411	0.5170
Panel B: Shared-space listings in Manhattan (treated) vs shared-space listings in SF (control)				
# of competitors in 0.1-mile radius	-0.0005	-0.006	0.0021*	0.0023*
	(0.0011)	(0.0015)	(0.0011)	(0.0012)
Enforcement Effect	0.0002	0.0004	-0.0021	0.0020
	(0.033)	(0.037)	(0.015)	(0.021)
Observations	8,361	8,361	8,361	8,361
Adjusted R^2	0.6305	0.6411	0.5337	0.5401
Listing attributes controls	Yes	Yes	Yes	Yes
Listing FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
City × Time	No	Yes	No	Yes
City × Time^2	No	Yes	No	Yes
City × Time^3	No	Yes	No	Yes

*Note: The control group is either private-room listings in SF (panel A) or shared-space listings in SF (panel B). Standard errors are clustered by city and reported in parentheses. Controls for listing attributes not reported include cancellation policy, No. of bedrooms, No. of bathrooms, No. of beds, maximum guests per stay, and No. of host listings. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.*

Table 9: Triple difference-in-difference – a competitive effect

	Dep: Log (listing price)		Dep: Log (vacancy)	
	(1)	(2)	(3)	(4)
Effect on high-competition listings	0.0594^{***}	0.0521^{***}	-0.0503^{***}	-0.0555^{***}
	(0.015)	(0.019)	(0.012)	(0.015)
Effect on low-competition listings	0.857^{***}	0.837^{***}	-0.0203^{**}	-0.0220^{**}
	(0.025)	(0.032)	(0.011)	(0.011)
Competitor density	0.1-mile radius	0.1-mile radius	0.1-mile radius	0.1-mile radius
Listing attributes Controls	Yes	Yes	Yes	Yes
Listing FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
City × Time	No	Yes	No	Yes
City × Time^2	No	Yes	No	Yes
City × Time^3	No	Yes	No	Yes
Observations	200,145	200,145	200,145	200,145
Adjusted R^2	0.7103	0.7255	0.6122	0.6311

Note: Entire-home listings in Manhattan and in SF are divided into high-competition and low-competition subgroups. Listings in SF are used as the control groups. Standard errors are clustered by city and reported in parentheses. The number of competitors is mean-centered. Controls for listing attributes not reported include cancellation policy, No. of bedrooms, No. of bathrooms, No. of beds, maximum guests per stay, and No. of host listings. ***, **, *and* * *indicate significance at the 1%, 5%, and 10% levels.*

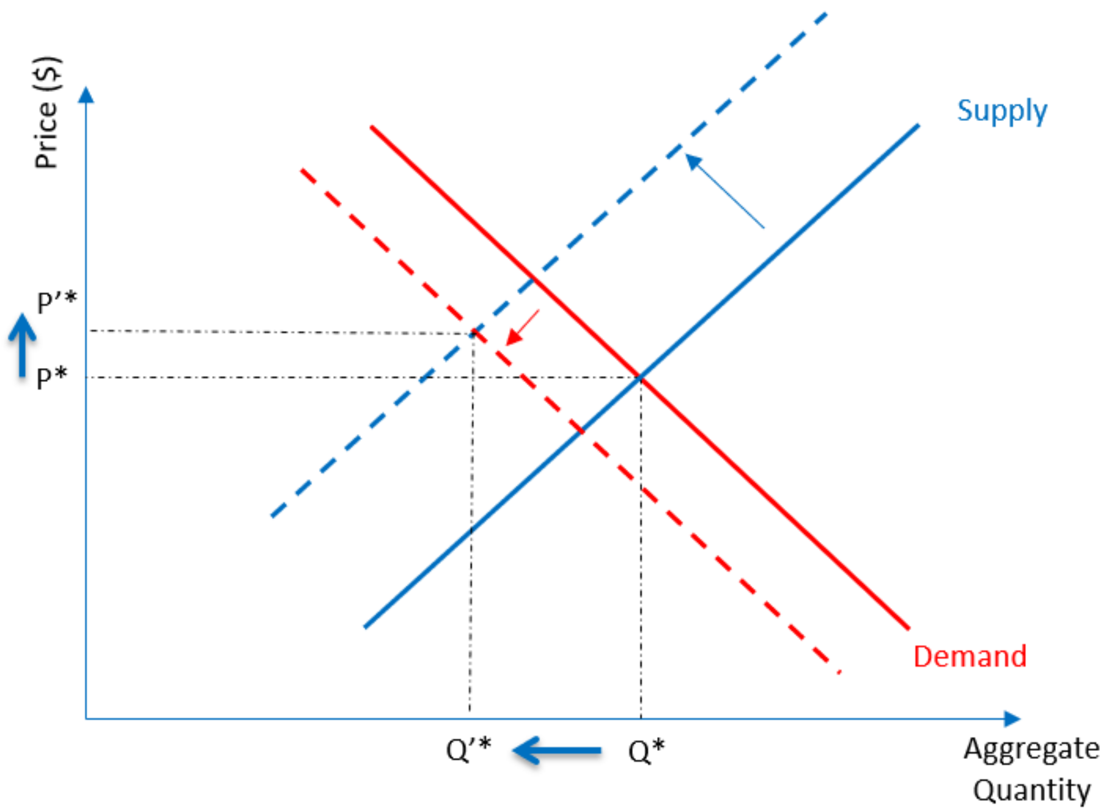
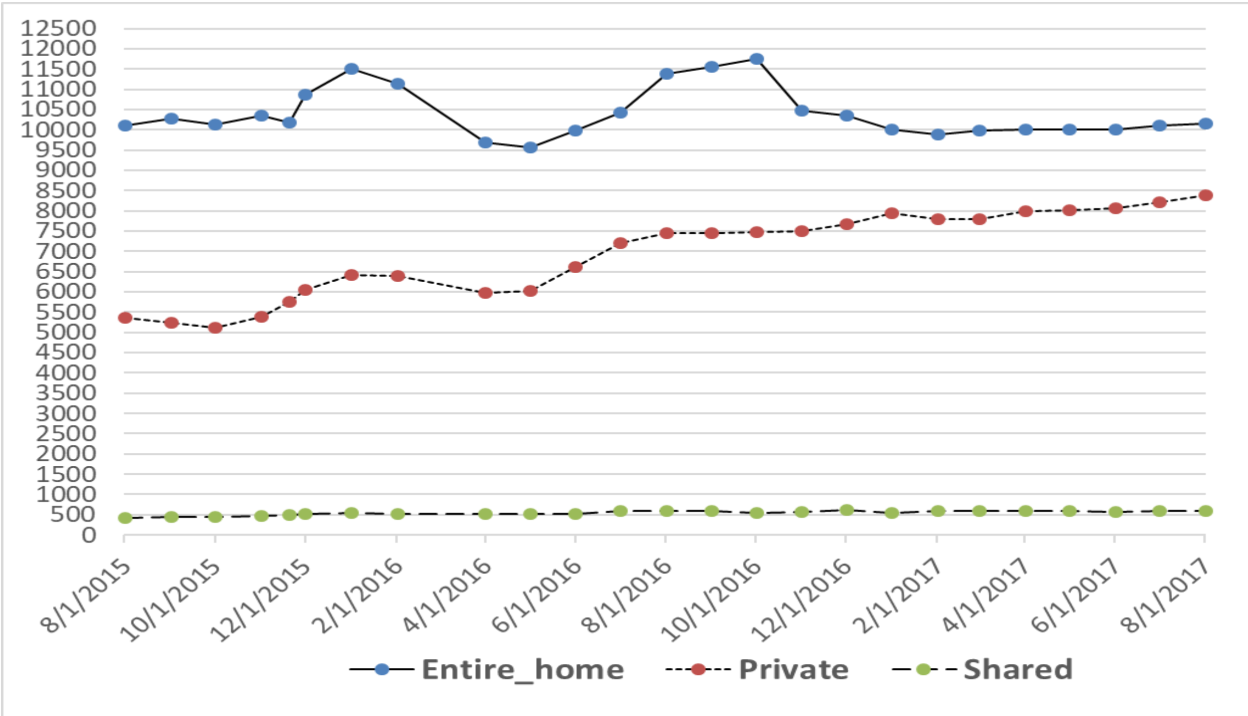
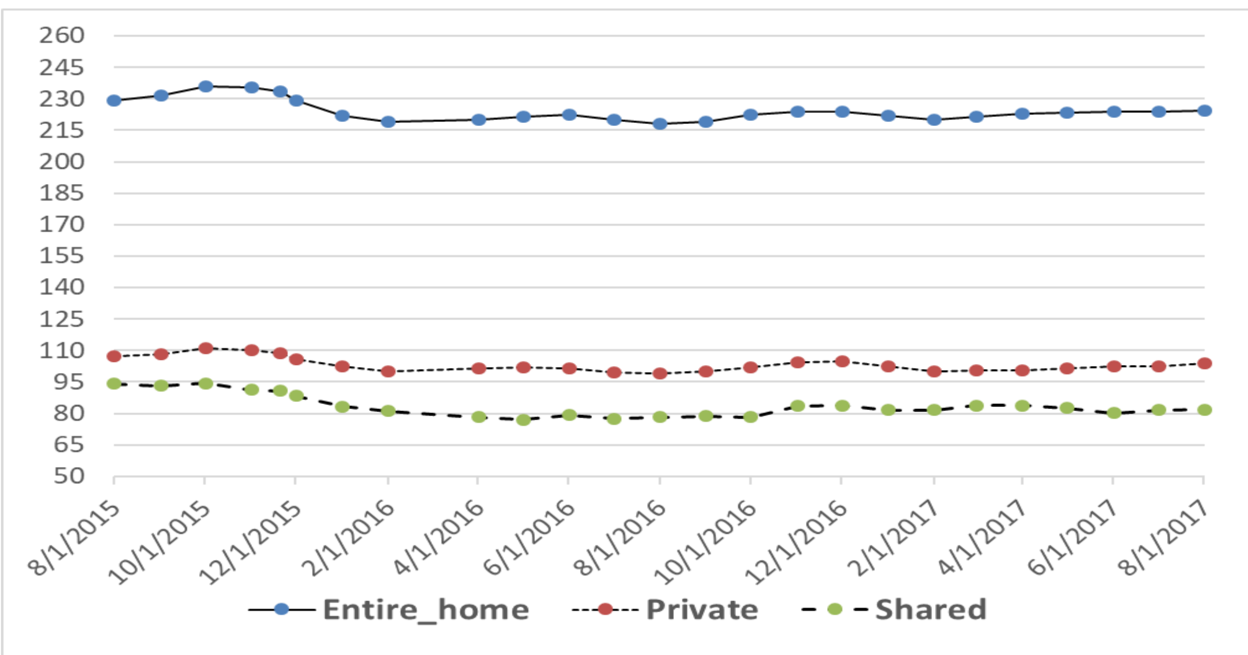


Figure 1: Hypothesized effect on the short-term rental market for entire homes following a new enforcement event

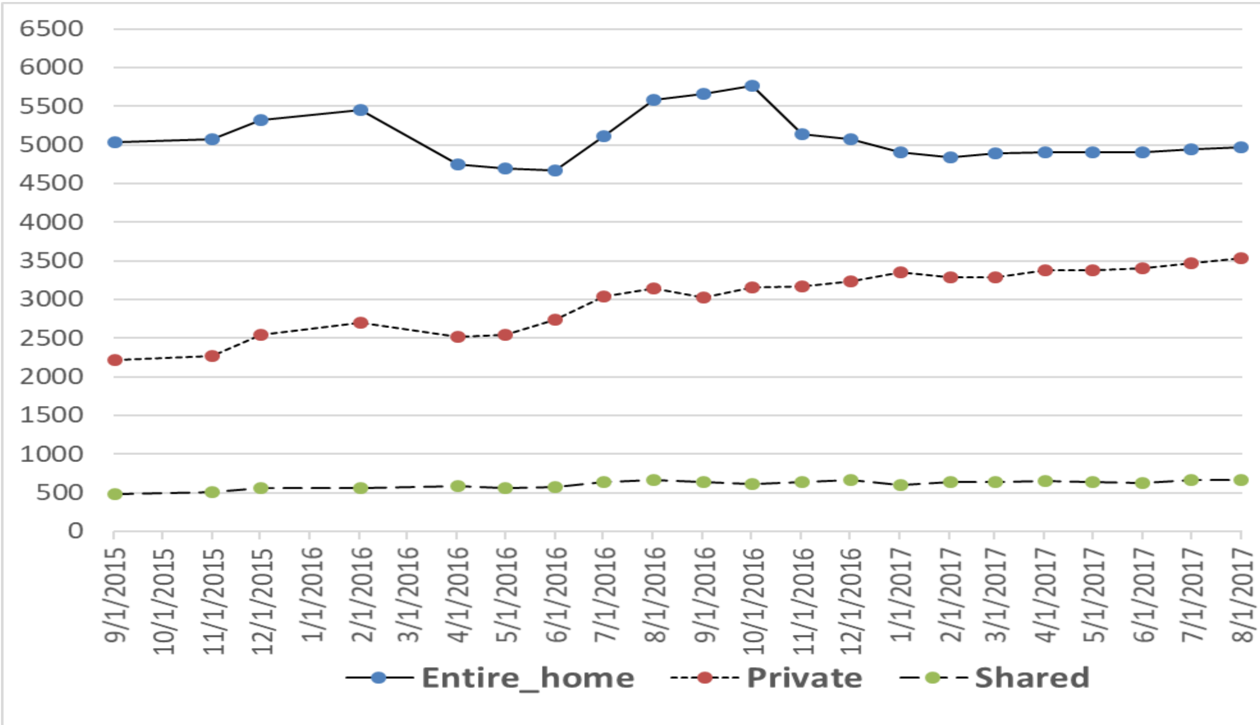


(a) Number of Airbnb listings for each listing category in Manhattan

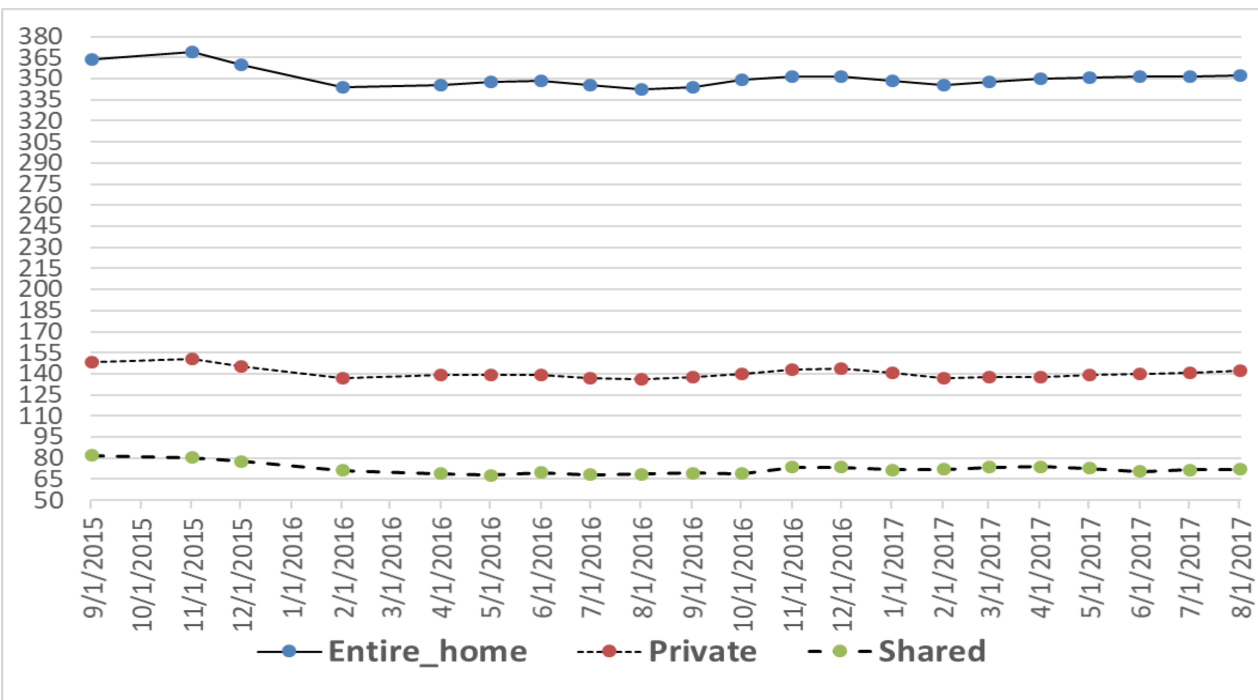


(b) Listing average price trends in Manhattan for each listing category

Figure 2: The number of listings and average price trends for the three short-term rental categories in Manhattan



(a) Number of Airbnb listings for each listing category in San Francisco



(b) Listing average price trends in San Francisco for each listing category

Figure 3: The number of listings and average price trends for the three short-term rental categories in San Francisco