The Non-Monetary Negative Externalities of Airbnb

Candidate number: 1053033

January 15, 2021

Social Data Science: Applied Analytical Statistics

Abstract

Recently, many researchers have examined the impact of short-term rentals such as Airbnb on local residents, finding that a greater increase in Airbnb supply often results in higher property and rental prices, although the effect may vary. However, nobody has yet performed a quantitative examination of non-monetary externalities, to my knowledge. I thus used a two-stage instrumental variables strategy to quantify the impact of Airbnb, as proxied by number of listings or bookings in a small geographic area, on non-monetary externalities, specifically noise, parking, and mice or insect infestations, as proxied by 311 complaints, in that same area. I used the implementation or enforcement of new local short-term rental laws, which caused a drop in supply of both Airbnb listings and bookings, as an instrument. I found that areas with a higher proportion of hosts with more than one listing or listings that rent out entire apartments as opposed to individual rooms see an increase in 311 complaints, but the overall impact of all Airbnb units on 311 complaints was mixed. These results may help policymakers further refine their regulation of Airbnb to decrease the impact of negative externalities.

1 Introduction and Literature Review

Airbnb and other short-term rental websites like Homestay or VRBO have become a lightning rod for controversy in recent years, with critics alleging that Airbnb raises rents and damages quality of life, increasing noise and traffic, for local residents (Bernal & Crisp, 2019). Anecdotal stories of noisy parties, rubbish and broken furniture left behind, and extremely rude behaviour from guests are frequently cited in news articles (Bernal & Crisp, 2019; Williams, 2016).

Researchers have recently begun examining more closely the potential impacts of Airbnb on the cities they enter. Airbnb has brought billions of dollars in tourism to cities and is extremely popular amongst its users, many of whom say that they could not have travelled without the aid of Airbnb (Lee, 2016). However, much research has found negative impacts on the long-term rental and hotel markets.

Franco et al (2019) examined the role of Airbnb on the local property and rental market for long-term residents in Portugal. By using a differences-in-differences (examining Porto and Lisbon – highly touristy cities – versus the rest of the country) model that focused on increases in Airbnb listings that occurred after a 2014 liberalization in the laws on short-term rentals, they found a 34% increase in property values and 10.9% in rents due to the reform. Although their model may still suffer from problems with endogeneity (e.g., one could imagine that Lisbon and Porto may see both increased rents and increased tourist activity for reasons other than the short-term regulatory rental reform), they support their findings with an instrumental variables approach. Barron et al. (2017) studied the same question, shifting the setting to the United States. They used an instrumental variables model, employing a combination of Google searches for Airbnb and a measure of how "touristy" a zipcode is in a base year as an instrument. They found only a small effect from Airbnb, with a 10% increase in listings leading to a 0.42% increase in rents and 0.76% increase in house prices. The effect is small due to the relatively large proportion of Airbnb hosts who occupy the homes they rent out; it grows in zipcodes with more absentee landlords, indicating that some have reallocated units from the long-term to the short-term market.

Wachsmuth and Weisler (2018) studied Airbnb in New York City, arguing that by introducing a potential new revenue flow for culturally desirable neighbourhoods that might attract tourists, Airbnb has induced "rent gap" gentrification even in the absence of significant capital reinvestment. The possibility to earn more money by renting out to tourists – who demonstrate a higher willingness to pay – rather than current residents itself serves as a sort of reinvestment in housing markets in certain neighbourhoods, thus driving up rents for everyone. A similar study also examined rent gap theory in the Palma Old Quarter in Mallorca, Spain, finding that due to the increased profitability of renting to tourists rather than residents, landlords have transferred homes from the long-term to the short-term rental market, depressing long-term supply, especially of affordable housing, and thus raising rents (Yrigoy, 2019).

Other work has examined Airbnb's role as a disruptor in traditional lodging industries; though Airbnb asserts they are not a direct competitor, claiming that their guests would have chosen to stay with friends or family or not come at all rather than stay in a hotel, one study found that an increase in Airbnb supply negatively correlated with hotel room revenues, average daily cost, and occupancy rates in Boston (Dogru et al., 2017). Another study that employed a differences-in-differences strategy to examine the impact of Airbnb entry on Texan hotels found a small but negative causal impact on hotel revenue and a much larger decrease in hotel prices, which positively impacts all consumers (Zervas et al., 2017). Airbnb's ability to instantaneously add supply during times of peak demand (like the conference South by Southwest) also put pressure on hotels to lower their prices, leading to greater consumer surplus.

Cities have responded to the pressure placed on the market by Airbnb and its competitors by restricting or regulating short-term rentals. Few have chosen to ban the service entirely, as it often provides great benefits for many residents; rather, they often limit the number of potential guests or nights that a property can be rented (Nieuwland and van Melik, 2020).

Although residents, politicians, and some academics (Williams, 2016; Bernal & Crisp, 2019; Espinosa, 2016; Morris, 2015) have asserted that Airbnb causes negative impacts on local residents beyond just rising rent prices, using the argument as evidence in favour of restrictions against long-term rentals, few academic papers have specifically examined Airbnb's role in non-monetary externalities. One exception is Gurran and Phibbs (2017), who note that long-term residents in Sydney and her suburbs have expressed discontent at increased garbage disposal and parking problems, as well as the changing character of their neighbourhoods. However, their analysis is mostly qualitative or correlational in nature.

I thus chose to examine this topic, studying the role of Airbnb on non-pecuniary externalities. With access to detailed Airbnb data from Inside Airbnb (Cox, 2021), as well as open-source records of 311 calls in San Francisco, New York City, and Boston, I examined how changes in the number of Airbnb listings on a block impacted noise, parking, or other types of nuisance complaints reported to 311 from that same block. Because Airbnb listings are most likely endogenously related to the composition of a neighbourhood in a way that might also affect noise complaints (e.g., gentrifying neighbourhoods are more likely to call in noise complaints and are also more likely to attract more Airbnb listings), I estimated a two-squares instrumental-variables model, using recent changes in implantation or enforcement of short-term rental laws in New York City, Boston, and San Francisco as the instrument, since these changes are likely highly correlated with number of Airbnb listings but should be uncorrelated with noise complaints when excluding short-term rentals as a channel.

Hypothesis 1: Having more Airbnbs in a geographic area will result in more 311 complaints.
Hypothesis 2: Airbnbs in which the owner is not present ("entire home/apartment") will see higher 311 complaints.
Hypothesis 3: Having more Airbnbs whose owners manage more than one listing will result in more 311 complaints.
Hypothesis 4: Airbnbs in neighbourhoods that are gentrifying will significantly and positively impact the number of complaints, as past research has shown to be the case (Misra, 2018).

2 Methods

2.1 Short-Term Rental Policy Details

New York City: Although New York City has banned short-term (fewer than 30 days) rentals of apartments without the presence of the current tenant since 2011, the law was rarely and poorly enforced. In October of 2016, the governor signed a law forbidding the advertisement of any such rentals, which came into effect in November of 2016 (Wachsmuth and Weisler, 2018). As can be seen in Figures 1 and 2, this led to an immediate drop in both total number of listings and total bookings within the next 30 days, although the Airbnb market soon rebounded.

San Francisco: San Francisco imposed regulations on short-term rentals relatively early, limiting the number of days a property could be rented out per year (up to 90) in 2014. In 2015, SF began requiring hosts to register as both a short-term rental and business, which Airbnb fiercely resisted until late 2017. At this point, they began removing listings, with a large majority removed just before January 2018, the absolute latest deadline for to eliminate unregistered hosts from the platform. Figures 3 and 4 indicate that the removal in listings also led to fewer monthly bookings.

Boston: From January 2019, Boston required new short-term rental hosts to register and only rent out their primary residences, but the law did not take effect for current hosts until September 2019. Figures 5 and 6 demonstrate the large decrease in both listings and bookings that resulted. The vertical lines in all figures represent the last possible date before the regulation took place.

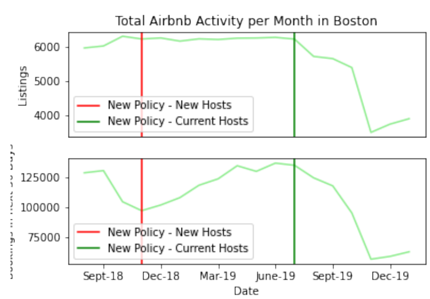

Figure 1: *Boston Total*

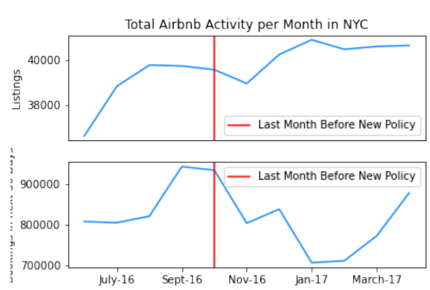

Figure 2: *NYC Total*

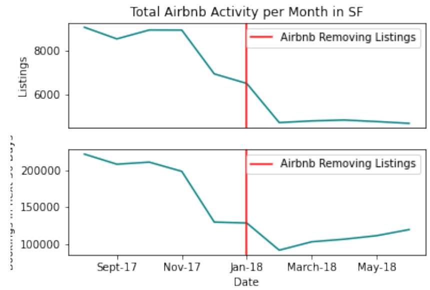

Figure 3: *SF Total*

2.2 Data Preparation

I chose to look at nuisance complaints in the five months before and after the policy took effect for everyone. Most American cities maintain a 311 hotline where residents can call to complain and ask the city to take action on issues like noise, parking, rodent infestations, and so forth.

For Boston, I included both the five-month range for the policies impacting the new hosts (Jan 2019) and the current ones (Sept 2019). My dataset thus ranged from June 2016-April 2017 for NYC, September 2018-February 2020 for Boston, and August 2017 to June 2018 for San Francisco, with November 2016, September 2018 and August 2017 as the respective cut-off points.

With the Socrata Open Data API, I downloaded data from NYC's 311 database ("311 Service Requests from 2010 to Present") corresponding to 2016 and 2017 for the complaint types "Illegal Parking," "Noise – Residential," and "Rodent", where latitude and longitude were not null, which returned 349,942 results. I separately investigated the results where latitude and longitude did not exist, to ensure their exclusion would not bias the results. 2664 observations under the above 3 categories did not have a latitude and longitude attached to them, accounting for 0.31%, 0.63%, and 1.55% of all residential noise, rodent, and illegal parking complaints, respectively.

I then used the Socrata Open Data API to download data from SF's 311 database for noise complaints (car alarms, amplified electronics sounds, generic noise issues and "other excessive noise"), rodent or insect infestations, and parking enforcement complaints. I did not include any cases that were tagged as duplicates or invalid or without valid latitude/longitude coordinates. As with NYC's 311 data, I examined the percentage of all (12,275) observations that did not include valid latitude and longitude coordinates; these 176 observations accounted for 0.43%, 8.26%, and 10.43% of all parking enforcement, noise, and rodent/insect infestations, respectively. The relatively high percentage of nulls in noise and rodent/insect infestations indicate that those results should be viewed with a grain of salt.

Finally, I downloaded Boston's 311 data from data.boston.gov, retaining only those where the reason was "Generic Noise Disturbance", "Parking Complaints" or "Mice Infestation" (no similar field for rats rather than mice existed). I checked for nulls and found none.

Detailed data on Airbnb listings for all three cities came from a website called "Inside Airbnb," which scrapes Airbnb's data on a monthly basis. After downloading the data, I rounded off the latitude and longitude coordinates to the third decimal degree (0.001), which represents about 100 metres on the earth's surface (I did the same for the 311 data). I then deleted any bookings that appeared to be from a hotel, hostel, or similar business, who nowadays sometimes advertise on Airbnb, created a variable for number of bookings in the next 30 days, and recoded the string variables to be used in the analysis. I then collapsed both datasets – the Airbnb listings data and the 311 data – by date, latitude, and longitude. I merged these datasets together by city. In any given latitude-longitude pair where at least one Airbnb complaint existed but no listings and bookings or vice versa, I replaced the null with a 0.

Gentrification could potentially serve as a confounding variable. For example, gentrifying neighbourhoods are more likely to call in noise complaints (Misra, 2018) and most likely attract more Airbnb listings than they did previously. Thus, I used gentrification as a control variable. To do so, I downloaded gentrification data for San Francisco and NYC (none existed for Boston) from the "Mapping Urban Displacement" Project, which aims to "understand the nature of gentrification (increased educational attainment and income in formerly low-income neighbourhoods) and displacement in American cities" and is run by researchers at UC Berkeley (Chapple and Thomas 2020. They divide NYC's census tracts within cities into 8 typologies: not losing low-income households, at risk of gentrification (but not currently undergoing displacement or gentrification), ongoing displacement of low-income households (but not gentrification), ongoing gentrification, advanced gentrification (neighbourhoods further along in the process), stable exclusion (neighbourhoods where rent has been consistently too high for low-income households to afford), ongoing exclusion (similar to stable exclusion but with loss of low-income households), and super gentrification or exclusion (very high income neighbourhoods). SF is divided similarly, although it also includes a "at risk of becoming exclusive" (but not gentrifying) and a "high student population" category.

Using an API from the FCC that linked latitude and longitude to census tract FIPS codes, I flagged Airbnb listings located within the "ongoing gentrification" and "advanced gentrification" typologies. I then downloaded median household income by census tract from the American Community Survey 2014-2018 estimates using IPUMS, which provides census and survey data from around the world, and merged it with the full dataset, which contained count of all listings, sum of monthly bookings, and the percentage of Airbnb listings representing entire apartments (rather than just private rooms), had "super-hosts" (hosts that are rated exceptionally well by guests), or whose hosts managed more than one listing.

I finally added a binary flag accounting for before and after the policy took full effect or Airbnb removed listings. To account for the possibility of individuals reacting to the policies before they fully came into effect, I also created a categorical flag that equalled the number of months out from the policy (negative for months prior, positive for months after).

Altogether, there were 47,991 observations in 4,298 latitude-longitude-date clusters in the Boston final dataset, 245,771 in 42,521 clusters in the New York City one, and 44,841 with 6,806 clusters in the San Francisco one. I produced the regressions in R; all other data work I performed with Python.

2.3 Two-Stage Instrumental Variable

Using the timing of new short-term rental laws as an instrumental variable, I estimate the impact of Airbnb listings or bookings (X) on non-monetary externalities (Y). To be valid, an instrument Z must meet four assumptions:

1. Random assignment: Conditional on the covariates, the instrument should be randomized with respect to the outcome and treatment variables. Although the policies themselves are not applied randomly– they apply to all short-term rentals within city borders – the exact timing of policy implementation is essentially random. The data show that either Airbnb or its hosts removed listings right before these policies took effect, leading to fewer booked visits.

2. Exclusion restriction: This assumption states that the instrument should not directly impact the outcome, except through the treatment. In this case, the new policies should not impact non-monetary externalities except insofar as they reduce either the total number of listings or a certain type of listing that is directly targeted by the law. Notably, most cities' short-term rental laws directly target second homes and favour extra rooms in a host's primary residence, which may be less prone to externalities like noise complaints or rodents since the host lives there as well – although externalities like illegal parking are unlikely to vary with type of listing conditional on neighbourhood and listing covariates. However, even if the type of homes impacted by the policy is correlated with a higher likelihood of some negative externalities, this does not invalidate the instrument – it simply means that we cannot definitively distinguish between the channel of all Airbnb bookings versus only the type that are likely to be made illegal. To shed light on the distinction between the two, I added the percentage of Airbnb hosts who listed more than one room on the website as a control variable in the model.

3. Monotonicity: All those impacted by the instrument are impacted in the same direction. In other words, the new policies should not have led to more Airbnb bookings or listings than would have otherwise occurred in the counterfactual world in which they were not implemented. This assumption holds true even if some hosts now in violation of the law decided to ignore it and continue to operate as before (zero effect).

4. Existence of a first stage: The instrument Z (policy timing) must have an effect on the treatment X (Airbnb listings or bookings). Figures XX, XX, and XX, shown above, indicate that full implementation of new short-term rental laws led to a drop in Airbnb listings as well as 30-day bookings in all three cities, supporting this assumption.

I estimated the impact of Airbnb on negative externalities with a two-stage least-squares regression. In the first stage, I regressed Airbnb listings on a dummy (or categorical) variable indicating whether the policy had taken full effect or not. I then used the predicted values from this regression as the explanatory variables in the second stage.

The model thus took the following form:

$Y(i,t) = X(i,t) + W(i,t) + Vi|W(i,t) + V(i,t) + Z,t$

Where i denotes latitude-longitude geographic area, t represents date, Y is the outcome variable (in this case, number of 311 complaints), X is the endogenous explanatory variable (in this case, Airbnb, as proxied by either number of total Airbnb listings or the total number of bookings within the next 30 days) and W represents the time-and-place variant control variables – percentage of Airbnb listings that are an entire apartment, percentage of listings with a “super-host”, percentage of listings where the host has more than one listing. V denotes the time-invariant control variables: median household income in the zip code in which the latitude-longitude resides and the percentage of Airbnb listings in a gentrifying neighbourhood. No data on gentrification existed for Boston, so I excluded this variable from its controls. Finally, Z denotes the instrument, the binary policy flag, of short-term rental law implementation, which varies over time but not place. Notably, since individuals may begin adjusting their behaviour prior to a policy taking effect, or since Airbnb may have begun eliminating listings from its website in the months leading up to its due date, I also ran the same set of models with this categorical policy flag.

Overall, I estimated eight models per city, as shown by the table below. However, only four are shown in the results section – the categorical model results are left to the appendix.

Model Number	Controls?	Explanatory Variable	Instrument 1
No	Listings	Binary policy flag	
2	Yes	Listings	Binary policy flag
3	No	Bookings	Binary policy flag
4	Yes	Bookings	Binary policy flag
5 (Appendix)	No	Listings	Categorical policy flag
6 (Appendix)	Yes	Listings	Categorical policy flag
7 (Appendix)	No	Bookings	Categorical policy flag
8 (Appendix)	Yes	Bookings	Categorical policy flag

3 Results

3.1 Summary Statistics

Tables 1-6 show sample characteristics for the study. Tables 1, 3, and 5 show summary statistics for the dependent (number of complaints) and independent explanatory variables (total number of bookings in next 30 days, average number of bookings per Airbnb, and the mean, maximum and minimum number of listings for each unique latitude-longitude pair) for New York City, San Francisco, and Boston, respectively. The number of listings per latitude-longitude area varied greatly across all three cities, with the mean hovering around 1-2 per area, but with a long right tail – New York City and San Francisco both saw maximums of 35, while Boston had a maximum of 29. Unsurprisingly, as each latitude-longitude cluster only covers about 100 metres, each city also had areas with no listings.

Note that Inside Airbnb did not record data for June of 2018 for San Francisco, so all Airbnb listings data is not available for that month. Tables 2, 4, and 6 show summary statistics for the variables used as controls. San Francisco’s listings had the highest median household income at $120, followed by Boston and New York. Although all three cities heavily restrict second homes, the data also indicate that a relatively large proportion of listings are entire apartments, while a large number of hosts have more than one listing, perhaps indicating they might be “professional hosts” rather than individuals renting part of their homes out.

Table 1: New York City Summary Statistics

Month	Year	Number of Complaints Max	Min	Total	Bookings in 30 Days Total
June	2016	35676.0	1.499	368.0	912833.0
July	2016	34522.0	1.470	912.0	891195.0
August	2016	30834.0	1.345	293.0	901660.0
September	2016	35034.0	1.486	1407.0	1055836.0
October	2016	32045.0	1.442	78.0	1050005.0
November	2016	28783.0	1.368	99.0	887835.0
December	2016	29833.0	1.408	215.0	912391.0
January	2017	30929.0	1.439	115.0	771290.0
February	2017	27806.0	1.318	70.0	772187.0
March	2017	30121.0	1.379	110.0	851860.0
April	2017	34407.0	1.489	94.0	986451.0

Month	Year	# Bookings in 30 Days per Airbnb Mean	Listings per Latitude-Longitude Area Total	Mean	Max	Min
June	2016	20.916	41273.0	1.735	29.0	0.0
July	2016	19.694	42933.0	1.828	29.0	0.0
August	2016	19.545	43634.0	1.903	33.0	0.0
September	2016	22.194	44372.0	1.882	31.0	0.0
October	2016	22.004	44337.0	1.996	29.0	0.0
November	2016	19.029	42813.0	2.035	29.0	0.0
December	2016	19.517	43619.0	2.058	35.0	0.0
January	2017	15.919	44452.0	2.069	32.0	0.0
February	2017	15.942	43847.0	2.078	31.0	0.0
March	2017	17.516	44597.0	2.041	35.0	0.0
April	2017	19.969	45457.0	1.968	34.0	0.0

Table 2: New York City Control Variables

Month	Year	% Listings Entire Home	% Hosts 2+ Listings	% Superhost
June	2016	52.1	30.9	4.9
July	2016	51.4	31.3	4.6
August	2016	51.1	30.7	4.9
September	2016	51.4	30.8	6.3
October	2016	51.5	31.2	6.3
November	2016	49.3	31.2	6.7
December	2016	49.7	30.2	6.5
January	2017	49.3	29.7	6.3
February	2017	49.0	29.8	7.6
March	2017	48.9	29.5	8.9
April	2017	48.8	29.5	8.7

Month	Year	Neighborhood Median Household Income		
		Max	Min	Mean
June	2016	250001.0	9939.0	70325.117
July	2016	250001.0	9939.0	70406.008
August	2016	250001.0	9939.0	70220.527
September	2016	250001.0	9939.0	70162.611
October	2016	250001.0	9939.0	70168.832
November	2016	250001.0	9939.0	70137.839
December	2016	250001.0	9939.0	69793.380
January	2017	250001.0	9939.0	69788.864
February	2017	250001.0	9939.0	70091.255
March	2017	250001.0	9939.0	69885.214
April	2017	250001.0	9939.0	69785.630

Table 3: San Francisco Summary Statistics

Month	Year	Number of Complaints			Total Bookings in 30 Days
		Mean	Max	Min	
August	2017	117.0	0.027	7.0	217546.0
September	2017	98.0	0.023	4.0	204162.0
October	2017	107.0	0.025	6.0	207613.0
November	2017	68.0	0.016	3.0	195155.0
December	2017	60.0	0.016	5.0	127017.0
January	2018	71.0	0.019	2.0	125539.0
February	2018	67.0	0.022	3.0	89138.0
March	2018	144.0	0.048	5.0	99247.0
April	2018	2643.0	0.645	12.0	102297.0
May	2018	4617.0	1.026	23.0	105631.0
June	2018	4107.0	1.647	16.0	N/A

Month	Year	# Bookings in 30 Days per Airbnb	Listings per Latitude-Longitude Area			
		Mean	Total	Mean	Max	M
August	2017	24.369	8854.0	2.035	35.0	0
September	2017	24.173	8337.0	1.966	27.0	0
October	2017	23.451	8741.0	2.009	25.0	0
November	2017	21.978	8720.0	1.999	26.0	0
December	2017	18.712	6709.0	1.759	18.0	0
January	2018	19.645	6276.0	1.699	18.0	0
February	2018	19.564	4520.0	1.495	13.0	0
March	2018	21.812	4524.0	1.499	14.0	0
April	2018	22.090	4610.0	1.125	8.0	0
May	2018	23.387	4486.0	0.997	8.0	0
June	2018	NaN	0.0	0.000	0.0	0
July	2018	25.499	4328.0	1.496	8.0	1

Table 4: San Francisco Control Variables

Month	Year	% Listings Entire Home	% Hosts 2+ Listings	% Superhost
August	2017	60.0	35.5	22.5
September	2017	60.0	35.3	22.7
October	2017	59.8	34.9	22.2
November	2017	59.7	35.8	25.3
December	2017	59.2	43.9	31.7
January	2018	59.1	42.0	32.7
February	2018	59.9	48.2	44.4
March	2018	59.7	48.5	44.0
April	2018	58.8	49.5	42.5
May	2018	59.7	47.8	45.5
June	2018	N/A	N/A	N/A
July	2018	60.2	48.5	45.5

Month	Year	Neighborhood Median Household Income		
		Max	Min	Mean
August	2017	191750.0	14976.0	120058.420
September	2017	191750.0	14976.0	120081.755
October	2017	191750.0	14976.0	120163.677
November	2017	191750.0	14976.0	120052.096
December	2017	191750.0	14976.0	120078.410
January	2018	191750.0	14976.0	120091.686
February	2018	191750.0	14976.0	120562.251
March	2018	191750.0	14976.0	120904.326
April	2018	191750.0	14976.0	117376.651
May	2018	191750.0	14976.0	117418.720
June	2018	191750.0	14976.0	112302.207
July	2018	191750.0	14976.0	121285.669

Table 5: Boston Summary Statistics

Month	Year	Number of Complaints			Total Bookings in 30 Days
		Mean	Max	Min	
September	2018	38.0	0.014	2.0	127984.0
October	2018	57.0	0.020	3.0	129842.0
November	2018	44.0	0.015	3.0	103615.0
December	2018	22.0	0.008	2.0	95760.0
January	2019	37.0	0.013	2.0	101052.0
February	2019	48.0	0.017	2.0	107230.0
March	2019	23.0	0.008	2.0	117208.0
April	2019	26.0	0.009	1.0	122835.0
May	2019	32.0	0.011	1.0	133611.0
June	2019	26.0	0.009	2.0	129202.0
July	2019	24.0	0.008	2.0	135998.0
August	2019	28.0	0.010	2.0	134120.0
September	2019	31.0	0.011	3.0	123706.0
October	2019	32.0	0.012	2.0	116566.0
November	2019	25.0	0.009	2.0	93673.0
December	2019	15.0	0.008	1.0	55126.0
January	2020	19.0	0.010	1.0	57340.0
February	2020	32.0	0.016	2.0	61068.0

Month	Year	# Bookings in 30 Days per Airbnb	Listings per Latitude-Longitude Area		
		Mean	Total	Mean	Max
September	2018	22.803	5897.0	2.135	29.0
October	2018	22.599	5953.0	2.121	27.0
November	2018	17.164	6227.0	2.162	24.0
December	2018	16.357	6136.0	2.172	29.0
January	2019	16.738	6179.0	2.186	31.0
February	2019	17.973	6081.0	2.164	30.0
March	2019	19.464	6131.0	2.180	32.0
April	2019	20.432	6117.0	2.155	32.0
May	2019	22.372	6141.0	2.136	32.0
June	2019	21.766	6161.0	2.148	31.0
July	2019	22.777	6180.0	2.151	31.0
August	2019	22.811	6125.0	2.128	32.0
September	2019	22.588	5628.0	2.031	27.0
October	2019	21.587	5562.0	2.030	27.0
November	2019	18.602	5289.0	1.986	28.0
December	2019	17.047	3419.0	1.839	26.0
January	2020	16.076	3650.0	1.864	27.0
February	2020	16.861	3785.0	1.947	27.0

Table 6: Boston Control Variables

Month	Year	% Listings Entire Home	% Hosts 2+ Listings	% Superhost
September	2018	64.8	66.6	21.9
October	2018	64.3	67.2	22.7
November	2018	64.4	68.5	22.1
December	2018	65.4	69.6	22.4
January	2019	66.0	69.9	22.9
February	2019	65.9	69.4	23.1
March	2019	66.4	69.7	23.6
April	2019	66.7	70.0	23.1
May	2019	65.4	70.1	22.9
June	2019	64.3	70.7	22.3
July	2019	64.2	71.0	22.8
August	2019	64.6	70.9	22.8
September	2019	61.8	69.8	23.4
October	2019	61.3	70.7	22.9
November	2019	61.2	70.6	23.1
December	2019	65.3	79.2	27.5
January	2020	64.7	79.5	27.0
February	2020	64.2	80.8	28.0

Month	Year	Neighborhood Median Household Income		
		Max	Min	Mean
September	2018	180694.0	12759.0	76148.578
October	2018	180694.0	12759.0	76126.279
November	2018	180694.0	12759.0	76527.249
December	2018	180694.0	12759.0	76608.555
January	2019	180694.0	12759.0	76286.246
February	2019	180694.0	12759.0	76209.592
March	2019	180694.0	12759.0	76679.397
April	2019	180694.0	12759.0	76635.776
May	2019	180694.0	12759.0	76210.031
June	2019	180694.0	12759.0	75809.587
July	2019	180694.0	12759.0	75948.193
August	2019	180694.0	12759.0	76190.490
September	2019	180694.0	12759.0	76019.586
October	2019	180694.0	15703.0	75945.974
November	2019	180694.0	15703.0	75650.916
December	2019	180694.0	15703.0	77280.176
January	2020	180694.0	15703.0	76864.297
February	2020	180694.0	12759.0	76364.331

3.2 Correlations

Table 7 shows correlations between the explanatory (total number of listings or total number of bookings in the next 30 days) and dependent variables (number of total complaints, broken into each type of complaints – noise, infestation, and parking). The data indicate a negative and significant but small correlation between the number of total complaints and both explanatory variables, versus a positive and significant but still small correlation between the explanatory variables and the number of noise or infestation complaints.

Table 7: Correlations

	Total Number of Listings	Bookings in Next 30 Days
Number of Total Complaints	-0.04*	-0.03*
Number of Noise Complaints	0.01*	0.01
Number of Infestations Complaints	0.06*	0.06*
Number of Parking Complaints	-0.0	-0.01

3.3 Regressions

Tables 8, 9 and 10 show Models 1-4 (varying the explanatory variable between number of total Airbnb listings and number of actual bookings in the next 30 days and choosing to include or not include controls) for NYC, SF, and Boston respectively. Models 5-8, those that use the categorical policy flag as an instrument, are shown in the appendix. They show very similar results, with very small coefficient or significance differences and none in sign, supporting the use of a binary policy flag as an instrument.

3.3.1 Table 8: NYC Regressions

	Instrumental variable: Binary Policy Flag			
	Dependent variabe: Number of 311 Complaints			
	(1)	(2)	(3)	(4)
# Listings	−0.274***		−0.589***	
	(0.105)		(0.162)	
# 30-Day Bookings		0.031**		0.007***
		(0.013)		(0.002)
Med HH Income			−0.000***	−0.000***
			(0.000)	(0.000)
% Gentrified			0.463***	0.102***
			(0.080)	(0.026)
% Whole Apt			0.663***	−0.122***
			(0.167)	(0.046)
% Superhost			0.229***	−0.011
			(0.056)	(0.017)
% 2+ Listings			0.592***	−0.050*
			(0.146)	(0.030)
Constant	1.961***	0.170	1.528***	0.999***
	(0.207)	(0.535)	(0.124)	(0.026)
Observations	245,771	245,771	144,493	144,493
R^2	−0.021	−0.259	−0.136	0.012
Adjusted R^2	−0.021	−0.259	−0.136	0.012
Residual Std. Error	4.530 (df = 245769)	5.031 (df = 245769)	2.530 (df = 144486)	2.359 (df = 144486)

*p<0.1; **p<0.05; ***p<0.01

3.3.2 Table 9: SF Regressions

	Instrumental variable: Binary Policy Flag			
	Dependent Variable: Number of 311 Complaints			
	(1)	(2)	(3)	(4)
# Listings	-0.629^{***}		-0.569^{***}	
	(0.013)		(0.022)	
# 30-Day Bookings		-0.026^{***}		-0.022^{***}
		(0.001)		(0.001)
Med HH Income			-0.00000	0.00000
			(0.00000)	(0.00000)
% Gentrified			0.108^{***}	0.127^{***}
			(0.009)	(0.010)
% Whole Apt			0.386^{***}	0.390^{***}
			(0.016)	(0.018)
% Superhost			0.112^{***}	0.093^{***}
			(0.006)	(0.006)
% 2+ Listings			0.291^{***}	0.198^{***}
			(0.012)	(0.009)
Constant	1.253^{***}	1.172^{***}	0.407^{***}	0.313^{***}
	(0.021)	(0.020)	(0.017)	(0.016)
Observations	44,841	44,841	39,243	39,243
R^2	-0.582	-0.676	-0.596	-0.927
Adjusted R^2	-0.582	-0.676	-0.597	-0.927
Residual Std. Error	1.038 (df = 44839)	1.068 (df = 44839)	0.610 (df = 39236)	0.670 (df = 39236)

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

3.3.3 Table 10: Boston Regressions

	Instrumental variable: Binary Policy Flag			
	Dependent Variable: Number of 311 Complaints			
	(1)	(2)	(3)	(4)
# Listings	0.004		0.008	
	(0.006)		(0.008)	
# 30-Day Bookings		0.0001		0.0002
		(0.0002)		(0.0002)
Med HH Income			-0.00000^{***}	-0.00000^{***}
			(0.000)	(0.000)
% Whole Apt			−0.003	−0.002
			(0.004)	(0.002)
% Superhost			−0.001	−0.001
			(0.002)	(0.002)
% 2+ Listings			−0.005	−0.001
			(0.005)	(0.001)
Constant	0.002	0.006	0.002	0.004
	(0.013)	(0.008)	(0.005)	(0.003)
Observations	47,991	47,991	46,896	46,896
R^2	−0.016	−0.008	−0.003	−0.004
Adjusted R^2	−0.016	−0.008	−0.003	−0.004
Residual Std. Error	0.115 (df = 47989)	0.114 (df = 47989)	0.077 (df = 46890)	0.077 (df = 46890)

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

4 Discussion

Hypotheses 2, 3, and 4 are strongly supported by the data. All the models indicate that a higher percentage of listings that are entire homes or apartments, have hosts that rent out more than one listing, or are in gentrifying neighbourhoods significantly increase the probability of 311 complaints. These results are also in line with the popular perception, often the cause behind regulation of short-term rentals, that some hosts use Airbnb much like a hotel booking agency and their guests are accordingly less respectful of the place and the neighbourhood.

Hypothesis 1 has mixed support. Boston's models show little either way, as they are not significant. In New York City, a higher number of Airbnb listings results in a higher number of nearby 311 complaints, but the opposite is true when it comes to number of days booked in Airbnb units in the next 30 days, which might be a more accurate measure of guest stay. In San Francisco, a higher number of Airbnb listings or bookings results in a lower number of nearby 311 complaints, although the effect is small.

These results seem to contradict the popular narrative that Airbnb writ large is a vehicle for rude and disrespectful tourists; however, they indicate that when guests are allowed an entire home to themselves, rather than just a private room, they are more likely to cause a nuisance to the neighbourhood. The data also implies that Airbnb's well-known impacts on rental and property prices may not carry over to the non-monetary realm, although many have asserted otherwise (Williams, 2016; Bernal Crisp, 2019; Espinosa, 2016; Morris, 2015). The only other study on this topic found a negative impact of Airbnb on non-monetary externalities (Gurran and Phibbs 2017) but it mostly relied on qualitative and anecdotal methods, and took place in Sydney, rather than the United States. The context may change the results greatly.

4.1 Limitations and Extensions

The instrument may not fully meet all four assumptions, especially the exclusion restriction, as discussed in Methods. It is possible that the type of short-term rentals that the law is intended to restrict (usually homes that hosts do not live in) are also the type that are more likely to cause nuisances; of course, this does not mean the instrument is invalid, simply that it may not distinguish well between the impact of all Airbnbs on non-monetary externalities versus the impact of second homes.

Another potential method for estimating the causal impact of Airbnb listings on non-monetary externalities would be to use differences-in-differences, examining 311 complaints that occur near listings on either side of city limits before and after the laws came into effect. However, there is currently no publicly available data on Airbnb listings outside of the large cities and a few counties as provided on the Inside Airbnb website. Internal access to Airbnb's data would be very helpful for this. Furthermore, few cities publish their 311 data (and even fewer include noise complaints in 311 data).

Finally, the noise complaints in 311 data often do not include the kind of data we might care about – residential noise complaints, which are often taken care of by the police rather than the city – but instead include construction or other types of noise complaints. The noise complaints data from SF seems particularly untrustworthy, since it is unclear if it includes residential noise complaints such as parties.

5 Conclusion

In general, the popular narrative that Airbnb causes non-monetary externalities is not supported by the data; however, areas with a higher proportion of hosts with more than one listing or listings that rent out entire apartments as opposed to individual rooms see an increase in 311 complaints. Many of these listings are most likely illegal, however, and it is possible that those who chose to defy the law and continue renting out such listings are less likely to care about the impact of their short-term rental units on their neighbours. Moreover, the data is not as strong as it could be, particularly around type of complaints; only NYC clearly states that residents should call 311 rather than the police to take care of residential noise complaints like loud parties or music.

This study is the first of its kind, to the best of my knowledge, to use an empirical and quantitative strategy to study the effects of Airbnb on non-monetary externalities. It has important implications for policymakers, indicating that they should focus their energy on particular kinds of Airbnb units – notably entire homes and apartments and those run by hosts with more than one listing – if they wish to decrease the negative externalities from Airbnb. However, as enforcing bans on such rentals have proven difficult, as shown by the still-large numbers of entire homes and apartments or second homes on the platform despite their alleged illegality, policymakers should consider other options to reduce externalities, like better enforcing noise complaints to decrease the likelihood of their happening, a Pigouvian tax on short-term rentals that can be redistributed to current residents, or allowing more hotels and hostels, especially budget ones, to be built to undermine the intense demand for short-term rentals.

6 Appendices

6.1 Appendix 1: Categorical Policy Flag

	Instrumental variable: Categorical Policy Flag			
	Dependent Variable: Number of 311 Complaints			
	(1)	(2)	(3)	(4)
# Listings	-0.205^{*}		-0.348^{**}	
	(0.105)		(0.156)	
# 30-Day Bookings		0.329		0.006^{**}
		(0.873)		(0.003)
Med HH Income			-0.00001^{***}	-0.00000^{***}
			(0.00000)	(0.00000)
% Gentrified			0.347^{***}	0.109^{***}
			(0.076)	(0.035)
% Whole Apt			0.415^{***}	-0.105
			(0.160)	(0.071)
% Superhost			0.148^{***}	-0.007
			(0.054)	(0.021)
% 2+ Listings			0.376^{***}	-0.039
			(0.140)	(0.045)
Constant	1.825^{***}	-11.940	1.345^{***}	1.006^{***}
	(0.205)	(35.496)	(0.119)	(0.035)
Observations	245,771	245,771	144,493	144,493
R^2	-0.009	-26.223	-0.049	0.014
Adjusted R^2	-0.009	-26.223	-0.049	0.014
Residual Std. Error	4.504 (df = 245769)	23.394 (df = 245769)	2.432 (df = 144486)	2.358 (df = 144486)

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

	Instrumental variable: Categorical Policy Flag			
	Dependent Variable: Number of 311 Complaints			
	(1)	(2)	(3)	(4)
# Listings	−0.692***		−0.640***	
	(0.012)		(0.023)	
# 30-Day Bookings		−0.029***		−0.026***
		(0.001)		(0.001)
Med HH Income			−0.00000	0.00000
			(0.00000)	(0.00000)
% Gentrified			0.119***	0.150***
			(0.009)	(0.011)
% Whole Apt			0.435***	0.472***
			(0.016)	(0.020)
% Superhost			0.124***	0.108***
			(0.006)	(0.006)
% 2+ Listings			0.327***	0.239***
			(0.012)	(0.011)
Constant	1.351***	1.293***	0.447***	0.360***
	(0.020)	(0.020)	(0.018)	(0.018)
Observations	44,841	44,841	39,243	39,243
R^2	−0.762	−0.942	−0.762	−1.353
Adjusted R^2	−0.762	−0.942	−0.762	−1.353
Residual Std. Error	1.096 (df = 44839)	1.150 (df = 44839)	0.640 (df = 39236)	0.740 (df = 39236)

*p<0.1; **p<0.05; ***p<0.01

	Instrumental variable: Categorical Policy Flag			
	Dependent Variable: Number of 311 Complaints			
	(1)	(2)	(3)	(4)
# Listings	0.019**		0.023***	
	(0.008)		(0.007)	
# 30-Day Bookings		0.001**		0.001***
		(0.0003)		(0.0004)
Med HH Income			−0.00000**	−0.00000***
			(0.000)	(0.000)
% Whole Apt			−0.010***	−0.012***
			(0.003)	(0.004)
% Superhost			−0.003**	−0.007***
			(0.001)	(0.003)
% 2+ Listings			−0.014***	−0.005***
			(0.004)	(0.002)
Constant	−0.028*	−0.023*	−0.007*	−0.009*
	(0.016)	(0.014)	(0.004)	(0.005)
Observations	47,991	47,991	46,896	46,896
R^2	−0.186	−0.143	−0.031	−0.137
Adjusted R^2	−0.186	−0.143	−0.032	−0.137
Residual Std. Error	0.124 (df = 47989)	0.122 (df = 47989)	0.078 (df = 46890)	0.081 (df = 46890)

*p<0.1; **p<0.05; ***p<0.01

7 References

"311 Cases: San Francisco." (2021) DataSF. Retrieved from https://data.sfgov.org/City-Infrastructure/311-Cases/vw6y-z8j6.

"311 Service Requests." (2021). Analyze Boston. Retrieved from https://data.boston.gov/dataset/8048 ad64-4bfc-b090-ee00169f2323.

"311 Service Requests from 2010 to Present: New York City." (2021). NYC OpenData. Retrieved from https://data.cityofnewyork.us/Social-Services/311-Service-Requests-from-2010-to-Present/erm2-nwe9.

"Area API." Federal Communications Commission. Retrieved from https://geo.fcc.gov/api/census/!/bl

Barron, K., Kung, E., and Proserpio, D (2018) The sharing economy and housing affordability: evidence from Airbnb. Proceedings of the 2018 ACM Conference on Economics and Computation. https://doi.org/10.1145/3219166.3219180.

Bernel, N., and Crisp, J. (1 May 2019). Airbnb's rapid growth has changed the face of Europe. Here's how some cities are fighting back. The Telegraph. Retrieved from https://www.telegraph.co.uk/technology/2019/04/30/airbnbs-rapid-growth-has-changed-face-europe-cities-fighting/.

Chapple, K. Thomas, T. (2020). Berkeley, CA: Urban Displacement Project. Retrieved from https://www.urbandisplacement.org/.

Cox, M. (2021). Inside Airbnb: get the data [Data sets]. Retrieved from http://insideairbnb.com/get-the-data.html.

Dogru, T., Mody, M., and Suess, C. (2017). The hotel industry's Achilles heel? Quantifying the negative impacts of Airbnb on Boston's hotel performance. Boston Hospitality Review, 5(3).

Espinosa, T. P. (2016). The cost of sharing and the common law: How to address the negative externalities of homesharing. Chapman Law Review, 19(1), 597–627. Gurran, N., and Phibbs, P. (2017). When tourists move in: How should urban planners respond to Airbnb? Journal of the American Planning Association, 83(1), 80–92.

Manson, S., Schroeder, J., Van Riper, D., Kugler, T., and Ruggles, S. (2020) Median Household Survey from Tract: IPUMS National Historical Geographic Information System: Version 15.0 [dataset]. http://doi.org/10.18128/D050.V15.0 Misra, T. (18 October 2018). Yes, 311 nuisance calls are climbing in gentrifying neighborhoods. Bloomberg City Lab. Retrieved from https://www.bloomberg.com/news/articles/2018-10-18/in-new-york-city-gentrification-brings-more-311-calls.

Morris, S.L. (22 January 2015). Airbnb is infuriating the neighbors. Is it time for new rules? LA Weekly. Retrieved from https://www.laweekly.com/airbnb-is-infuriating-the-neighbors-is-it-time-for-new-rules/.

Nieuwland, S. and van Melik, R. (2020). Regulating Airbnb: how cities deal with perceived negative externalities of short-term rentals. Current Issues in Tourism 23(7): 811-825.

Simmons, A. (5 February 2020). Short-term rental laws in major U.S. cities. 2nd Address Research. Retrieved from https://www.2ndaddress.com/research/short-term-rental-laws/.

Wachsmuth, D., and Weisler, A. (2018). Airbnb and the rent gap: Gentrification through the sharing economy. Economy and Space 50 (6): 1147-1170.

Williams, L. (18 Sep 2016). When Airbnb rentals turn into nuisance neighbours. The Guardian. Retrieved from https://www.theguardian.com/technology/2016/sep/17/airbnb-nuisance-neighbours-tribunal-ruling.

Yrigoy, Ismael. (2019). Rent gap reloaded: Airbnb and the shift from residential to touristic rental housing in the Palma Old Quarter in Mallorca, Spain. Urban Studies 56(13): 2709-2726.

Zervas, G., Prosperpio, D., and Byers, J. (2017). The rise of the sharing economy: estimating the impact of Airbnb on the hotel industry. Journal of Marketing Research 54: 687-705. http://dx.doi.org/10.1509/jmr.15.0204.