The Effects of Occupancy Taxes: A Comparative Case Study in Boston

Introduction

1. Airbnb and Occupancy Taxes

AirBnB is an American travel-tech firm that facilitates a peer-to-peer online marketplace for short term lodging experiences around the world. Compared to similar firms offering vacation rental services such as VRBO or HomeAway, AirBnB is the largest and most prominent, with more than 7 million listings worldwide and 2 million people staying in one of its listings per night in 2018. Since its founding in 2008, hosts on the platform have served more than 750 million guests and has grown at an exponential rate globally[1].

As a popular alternative to hotels, the use of AirBnB has become subject to occupancy taxes in cities around the world. Typically enforced by local governments, these taxes are applied to short term lodgings and are up to 15% of the transaction in the United States. Proponents of the tax argue that existing rules that apply to hotels should also be enforced for Airbnb. Opponents instead argue that these laws do not apply to services created out of excess capacity, and their application would stifle innovation and growth in this industry. Although occupancy taxes have been largely in place in most American cities for decades, AirBnB's original stance was that the inherent less business-like and peer-to-peer nature of the transactions it facilitated were not subject to the tax[2].

However, after years of fighting with local legislators, AirBnB retracted this view in late 2013 and announced that they 'believed that it makes sense for our community to pay occupancy taxes"[3]. Around the same time between 2014 and 2015, AirBnB collaborated with major cities including New York City, Portland, and San Francisco to allow it to help facilitate the collection and remittance of occupancy taxes from its hosts[4]. This change forwarded the statutory incidence of the tax from suppliers to consumers: guests became responsible for paying the up-front cost of the tax when a reservation is booked on AirBnB.

2. Massachussetts

Despite beginning to remit taxes in some cities, AirBnB actively attempted to lobby against similar bills specifically applying to short term rentals from passing in other cities. A similar bill would only come into effect in 2019 for Massachusetts, which is the setting of our natural experiment. In the week after Christmas in 2018, Governor Charlie Baker signed into law *An Act Regulating and Insuring Short-Term Rentals*[5], a bill that extended the state's room occupancy excise tax to short term rentals for all bookings. The law imposed a 5.7% statewide excise beginning on July 1st 2019, in addition to a local option excise ranging from 0 to 6.5% and a convention center finance fee of 2.75% used to finance the construction of convention centers statewide that only applied in select Massachusetts cities. Notably, the bill came into effect almost immediately on January 1st, allowing two different natural experiments to be conducted. The procedure will be discussed in more detail in section 3.

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[1] https://news.airbnb.com/fast-facts/

[2] https://www.austaxpolicy.com/wp-content/uploads/2016/09/Wilking.pdf

[3]https://blog.atairbnb.com/who-we-are/

[4] https://chicagounbound.uchicago.edu/cgi/viewcontent.cgi?referer=&httpsredir=1&article=1038&context=uclrev_online

[5] https://malegislature.gov/Laws/SessionLaws/Acts/2018/Chapter337

In Boston, the total occupancy tax amount introduced to AirBnB listings by the act was 14.95%. This included a 5.7% statewide excise tax, a 6.5% local option excise, and a 2.75% convention center finance fee. Immediately after the introduction of the tax, AirBnB worked with the state and local governments to help remit the tax on behalf of hosts, passing the statutory incidence of the tax directly onto guests when they make a reservation. Before the COVID-19 pandemic, AirBnB was expected to collect 27.5 million in fiscal year 2020 due to the tax on behalf of the state[6].

This paper will analyze the effects of levying an occupancy tax on short term homestay rentals booked through Airbnb in the city of Boston, a sizable market with approximately 3000 listings. It will evaluate the causal impact of the tax in terms of changes in the quantity transacted (nights reserved) and transaction price. From this, it will determine the price elasticity of demand from the introduction of a tax, which can possibly provide insight on the introduction of occupancy taxes for short term homestays in other markets.

Literature

There exist a handful of papers that examine of occupancy taxes on hotel demand based on policies in different states and timeframes. Despite the heterogeneity of these 'natural experiments', they all generally conclude that price elasticities for demand are quite inelastic and generally passed on to hotel guests. Clay and Collins note that "although hotel taxes are now a common revenue source for state and local governments, they have been relatively little studied"[7].

One of the earliest ex-post studies on the effects was of occupancy taxes was Bonham et al's paper that examines Act 340 in Hawaii, which imposed a 5 percent transient accommodation tax beginning in 1987. Through examining the supply side revenues, they find that "the effects of the 1987 Hawaii hotel room tax did not have a significant negative impact on hotel rental receipts", but make a caveat that their findings may not be generalizable to other locations. Nonetheless, this implies that most of the occupancy tax's economic incidence is passed on to guests, indicating that demand–largely from tourists–is highly price inelastic[8]. Hiemstra and Ismail also conclude that price elasticities towards hotel occupancy taxes are indeed inelastic, but break up the heterogeneity of hotels into different sizes and price points. They find that price elasticities of demand for smaller hotels (<150 rooms) are larger in magnitude than larger hotels. In addition, across hotel sizes, hotels with larger prices also have price elasticities greater in magnitude[9]. This implies that elasticities may be greater for AirBnB's given their much smaller scale. Lastly, Clay and Collins study the effects of a $5 per night hotel tax imposed in Georgia in 2015 through a panel data setting. They find that the tax approximately decreased total bookings by 2.7%, implying a consumer elasticity of about -0.7. In addition, they observe that although the introduction of the tax did have a $1.5 decrease in daily prices, but the result was not statistically significant.

[6] https://www.patriotledger.com/news/20191204/new-rental-tax-expected-to-generate-275-million-in-revenue

[7] https://jrap.scholasticahq.com/article/3701.pdf

[8] https://ntanet.org/NTJ/45/4/ntj-v45n04p433-41-impact-hotel-room-tax.pdf?v=%CE%B1&r=5943970666266978

[9] https://www.sciencedirect.com/science/article/pii/0010880492900309

Wilking studies the effects of shifting of the statutory of an occupancy tax from producers to consumers on AirBnB, specifically with a shift in the remittance of the occupancy tax from AirBnB hosts to the AirBnB platform. She finds that "shifting the legal obligation to remit hotel taxes from small, independent hosts to AirBnB increases after-tax prices paid by consumers", but the magnitude of the effect is heterogeneous across hosts and listings. Her results contradict the traditional theory that "the incidence of a consumption tax is exclusively determined by market-wide demand and supply elasticities", and that "other factors, such as assignment of the remittance obligation, may affect the incidence in practice"[10].

This paper will examine the effects of introducing transient occupancy taxes on short term homestays and determine the corresponding price elasticities of consumers in response to the occupancy tax. The study differs from previous literature focusing on hotels as homestays typically operate at much smaller scales than hotels and serve more casual travelers. These factors combined imaginably make the demand responses to the introduction of an occupancy tax to be more price elastic.

Data

1. *Inside Airbnb* and Data Schema

The data for this paper originates from *Inside Airbnb*, an independent investigatory project that collects and hosts substantial AirBnB data on more than 100 cities around the world. The data collected by *Inside Airbnb* are web-scraped from the Airbnb website on a roughly monthly basis and can be found here. Although *Inside Airbnb* was originally started by its creators to investigate the effects of AirBnB on affordable housing and exacerbating gentrification, the data are made public for free and open for use.

Since the data on *Inside Airbnb* are completely collected through web scraping the Airbnb website, they contain only and all information that a visitor to Airbnb's site can see. Approximately every month, data are scraped for all listings in a particular city. For each listing, its availabilities for the next 365 days and quoted price per night over the next year are collected. In addition, information particular to the listing such as its location, host, review count, and room type are also collected.

2. Imputing Transactions

One key limitation of utilizing the web-scraped data from *Inside Airbnb* is that it does not reflect actual bookings and transactions made on the platform. For example, it is impossible to discern whether some dates for a unit may be 'blocked out' (unavailable to reserve) due to the unit being occupied, or it being unavailable on the market.

Thus, I resort to imputing the booking from the changes in availabilities across scraping dates. To impute transaction quantity and price, I rely on 2 key assumptions:

1. A unit that becomes unavailable across 2 consecutive scraping periods is due to the fact that it was booked. This assumption should generally be reasonable as hosts generally plan far ahead as to when their listings will be available; a host will not make a listing available if they know that they will be making it unavailable thereafter. This assumption allows me to impute a unit's dates that it was booked and the approximate time period, on the month level, when a booking was made.
2. The price of the booking for a set of dates is the same price as the latest scraping in which it was available on those dates. This may not be completely true: it is likely that hosts may decrease their prices very near the reservation date in order to maintain high occupancy and that this change is not detected by our monthly scraping. However, this limitation should not bias our analysis of changes in the transaction price, since we can reasonably

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[10] https://www.austaxpolicy.com/wp-content/uploads/2016/09/Wilking.pdf

assume that hosts adjust their prices dynamically in the same manner before and after an occupancy tax is introduced.

There is one major flaw in this model of imputation: reservations made between 2 scraping periods that also have the check-in date between the same period will go undetected. For example, if a scraping occurred on June 15th and the next scraping occurred on July 15th, a reservation that was made on June 17th for the night of June 18th will go undetected. As a result, the analysis describes a Local Average Treatment Effect on non-last minute bookings; in order for the treatment effect to be generalized, one would have to assume that last minute bookings are affected similarly by the occupancy tax as non-last minute bookings.

3. ***Reservation Date* vs *Date Booked***

Throughout this paper, there is an important subtle distinction between the terms *Reservation Date* and *Date Booked*. Specifically, the *Dated Booked* is when the reservation is made, while the *Reservation Date* is the check-in date of the reservation. For example, if a reservation for an Airbnb unit was made on April 1st for July 1st to the 7th, its reservation dates are July 1st to July 7th, but its date booked is April 1st. Although in the long run the effects of taxation affect both equally, in the short run when a tax is introduced it is better to separate these and investigate them both.

This distinction becomes particularly important in investigating the treatment effect in our natural experiment. The Massachusetts state legislature enacted the legislation for all reservation dates after July 1st, 2019, but this came into effect beginning on January 1st, 2019. Hence, only a reservation made on or after January 1st 2019 for a check in on or after July 1st was subject to a tax; if a reservation for after July 1st was booked before January 1st, or if a reservation was made after January 1st but for before July 1st, it was not subject to the tax. Thus, I will conduct 2 separate sets of analyses on the effects of the tax:

1. All reservations with reservation dates on or after July 1st. In this scenario, the treatment was ‘administered’ on January 1st, where the axis of time describes the date booked. Since we are only able to narrow down date booked to be between 2 scraping periods in an approximate period of month, there are only 5 observations in each of the pre and post treatment periods.
2. All reservations that were made after January 1st. In this scenario, the treatment was ‘administered’ effectively on July 1st, where the axis of time describes reservation dates.

4. Restrictions in Analysis

To better satisfy the first assumption, I attempt to restrict my analysis to units that are dedicated to Airbnb rentals. My hypothesis is that units dedicated to Airbnb rentals typically will only become unavailable between 2 scraping periods due to the listing being booked, while this assumption may be less dependable on units that are also the primary residence of the host. Thus, only listings with more than 100 nights available in the next year, at any stage of scraping, are considered. The cutoff of 100 nights was chosen to be a good tradeoff point between popular listings and primary residences. For primary residences, it is reasonable to assume that these listings are typically available for at most 3 months in a year; while for very popular listings, I assume that the max annual occupancy rate is around 70%, so that approximately 30%, or 100 nights a year, are available. Furthermore, hosts in the treatment group are exempted from the tax if they declare their intention to rent out the unit for no more than 14 days in a calendar year beforehand, which would likely remove units in the treatment group that are generally unavailable.

I further restrict my analysis to listings that were both on the market in the month immediately before and after the treatment effect. This removes units that become deleted before the treatment was administered and units that enter after the treatment was administered, accounting for attrition and its opposite effect.

In addition, the analysis for reservation dates begins on February 18th, 2019, which was the latest February scraping date. This is because the reservation dates are imputed from differences in-between scraping periods, so that the earliest imputable date from the January-February scraping across all cities was February 18th. Note that only reservation before January 1st 2019 do not apply to the study, since the potential tax had not yet come into effect.

Empirical Framework

1. Synthetic Controls

To determine the causal effect of the introduction of an occupancy tax, I utilize the synthetic controls method. The synthetic controls method is a causal inference method used for comparative case studies, in which one unit in the study receives the treatment while others do not. To determine the causal effect of treatment, the method creates a non-negative linear combination of control units that represents the potential control outcome of the treated unit known as the 'synthetic control'[11].

To best fit a synthetic control, the method seeks to minimize the difference between the synthetic control and the treatment unit in the pre-treatment period as the Euclidean-norm. Thus, the method can be expressed as the following constrained optimization problem:

The package I use to calculate Synthetic Controls is AugSynth, a R package that implements the Augmented Synthetic Controls Method, developed by Ben-Michael et al at UC Berkeley[12]. Although this paper does not utilize the Augmented Synthetic Controls Method, the package implements both vanilla synthetic controls as well as synthetic controls with ridge regression, which is described below.

2. Ridge Regression

Ridge regression is a common regularization procedure to reduce overfitting, trading model variance for added bias. Ridge regression augments the minimization of sum of squared residuals with an added regularization parameter:

||Xw-b||^2 + \lambda||w||^2

Due to the relatively small amount of time periods in pre-treatment, I augment the synthetic controls method by introducing ridge regression to prevent over-fitting by keeping weights more stabilized. Ridge regression is implemented in the synthetic controls method by penalizing absolute deviations in weight from the uniform weight value. For example, if there are 10 controls, the penalization value for a weight of 0.15 would correspond to 0.15-0.1 = 0.05. Shen proposes that introducing regularization in the form of ridge regression can decrease model variance, thus reducing the post-treatment error bound despite increasing pre-treatment error[13].

Notably, introducing ridge regression deviates from the original synthetic controls method by allowing for negative weights of control units in the synthetic control. Ben-Michael et al show through simulation that this relaxation of constraints reduces the bias to ensure better balance if the potential control outcome Y_1(0) is indeed a linear combination in the control outcomes.

To tune the coefficient penalization hyperparameter lambda, leave-one-out cross validation is utilized by leaving a pre-treatment time period out in each fold. The lambda value with the lowest average error across all folds is chosen to be the correct lambda. Since the hyper parameter determined through cross validation is not exact or necessarily most optimal, one set of hyper parameters is tuned for each outcome (nights, log nights, and prices), which is then consistently applied across different analyses for better stability and replicability.

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[11] https://economics.mit.edu/files/11859

[12] https://eml.berkeley.edu/~jrothst/workingpapers/BMFR_Synth_Nov_2018.pdf

[13] https://dspace.mit.edu/bitstream/handle/1721.1/115743/1036986794-MIT.pdf?sequence=1

3. Controls

Ten control cities in North America were selected to construct the synthetic control. Notably, unlike conventional control units, these cities were already all 'treated' before the experiment timeframe, i.e. occupancy tax laws for AirBnB had already been implemented in place. However, since these cities did not see a change in treatment, we can claim that they are suitable to construct a synthetic control if we assume that the potential post-treatment outcomes in terms of quantity and price varies similarly after a while that treatment was applied (or not). The cities were selected based off of proximity to Boston and data availability on *Inside AirBnB*. Table I below lists basic information about each control city's AirBnB market and their AirBnB occupancy tax rates.

Table 5
Control Cities

	# units	Avg price/night[1]	Est. nights/year[2]	Tax %
Asheville[†‡]	2170	168	162	13
Boston[†‡]	**3507**	**179**	**153**	**14.95**
Chicago[†]	8533	181	136	10.5
Washington DC[†‡]	9369	206	97	14.8
Denver[†‡]	5222	155	147	10.95
Montreal[†‡]	19495	109	88	3.5
Nashville[†‡]	5921	213	147	8
Quebec City[‡]	3240	111	124	3.5
Rhode Island[†]	2758	277	107	13
San Francisco[†‡]	7072	213	153	14
Twin Cities[†‡]	6680	185	62	14

[1] Data based on scraped listing prices.
[2] Data imputed by Inside Airbnb.
[†] In reservation date study.
[‡] In date booked study.

We observe that AirBnB markets differ widely in terms of size. The treatment unit Boston leaning on the smaller end, as neighboring cities such as Cambridge are not included in the analysis. Average prices across cities are somewhat similar, with Canadian cities slightly cheaper and also quoted in Canadian dollars. Note that the average price per night does not simply reflect rental-market prices, but also the type of units available: for example, a significant portion of Nashville and Rhode Island's units are entire homes and apartments, as opposed to private rooms or shared rooms, hence increasing the average price.
Boston's occupancy tax is the highest out of the cities examined, partly due to added convention center fees.
Notably, due to data availability issues, Rhode Island and Chicago were dropped from the date booked study, while Quebec City was dropped from the reservation date study.

Results

1. By Reservation Date

We begin our examination of the occupancy tax on listing reservation dates, with the treatment effect beginning on July 1st 2019. Multiple synthetic controls are utilized to conduct analyses on both the log and non-log scale for the outcome variable. The analysis is conducted at 2 granularities: on a monthly and semi-monthly basis. For ridge SCM results, the plots for cross validation errors across different lambda values are in the appendix. Notably, the same penalization hyper parameter is used for both month and semi-month level analysis for better comparability and replicability. Results that are statistically significant are marked with an asterisk.

Table 1
Treatment Effect on Reservation Dates

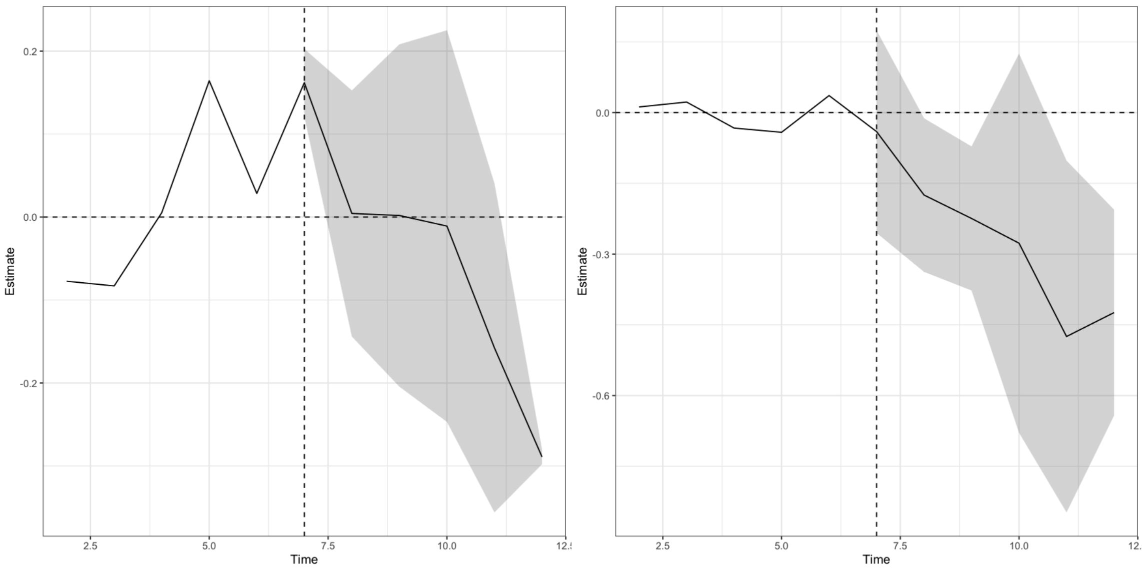
	SCM		Ridge SCM	
	Non Log	Log (%)	Non Log	Log (%)
Number of Nights				
Monthly	−5701.18	−0.269*	−6058.92	−0.238*
	(3785.49)	(0.118)	(4872.30)	(0.077)
Semi-Monthly	−2209.98	−0.245*	−2188.80	−0.236*
	(1323.32)	(0.105)	(1425.37)	(0.084)
Price				
Monthly		−0.048		−0.066
		(0.067)		(0.064)
Semi-Monthly		−0.050		−0.028
		(0.046)		(0.177)
Elasticity				
Monthly		2.65		2.85
Semi-Monthly		2.46		8.43

Overall, we observe that the introduction of the occupancy tax had a statistically significant result on log number of nights booked, both in the ridge and normal synthetic controls method cases. However, the model does not find statistically significant results when using non-log outcome variables, suggesting that the 15% tax had a proportional decrease to the number of nights booked. Thus, in subsequent sections regarding the robustness of the results, only log outcomes will be examined.

For log prices, the model does not possess sufficient statistical power to determine statistically significant results for changes in price. Across log differences semi-monthly and monthly, the treatment effects are generally similar for both prices and nights, providing some robustness to the results.

We can calculate price elasticities of demand from the changes in quantity transacted and price. The percentage of tax offloaded to consumers is the difference between the tax levied and the change in price. For example, for the non-ridge analysis conducted on a monthly level, a price hike of 14.95% - 4.8% = 10.15% was bore by consumers. With a quantity decrease in the number of nights by 26.9%, this in turn indicates that the consumer elasticity is 2.65.

Overall, we observe that the price elasticity of demand is quite elastic, which is not consistent with the inelastic results concluded from previous ex-post studies of occupancy taxes on hotels. The ‘direction' of this difference in elasticities however matches our intuition, since AirBnB consumers are likely to be more tourist-dominated and thus more elastic to changes in price. Furthermore, prices are likely to be more dynamic and less sticky for hosts on AirBnB as opposed to traditional hotels.



Change in log nights (left) and log price (right) on a month granularity, no ridge

2. By Date Booked
A similar analysis is conducted for the effects of the occupancy tax on listing dates of booking of reservations occurring after July 1st 2019, with the treatment effect beginning on January 1st 2019. However, due to the limitation of the method, the date booked can only be narrowed down to be between 2 scraping periods, which is approximately a 1-month time frame. Thus, only a month level analysis can be conducted.

Table 2
Treatment Effect on Date of Booking

	SCM		Ridge SCM	
	Non Log	Log (%)	Non Log	Log (%)
Number of Nights	-361.22	-0.200	13.60	-0.327^{*}
	(235.71)	(0.346)	(2904.95)	(0.148)
Price		-0.127		-0.064
		(0.251)		(0.147)
Elasticity		8.89		3.82

The nature of the experiment leads to large variance in the data, since the number of nights after July 1st booked grows significantly as the period gets closer to July 1st. Thus, the analysis does not possess sufficient statistical power to determine any results of statistical significance, except for the number of nights in the the ridge SCM case, which is likely due to simply having a ‘lucky’ penalization hyperparameter.
Since most results are not of statistical significance, not much can be concluded from the analysis. However, it is worthy to note that the results appear to be in a similar ballpark as the results in the cut-off by reservation date analysis above. It is also likely that elasticities are higher in this case, since guests are able to adjust their plans months in advance.

3. Weights
We can examine the weights that composed the synthetic control as a sanity check. For brevity in this section and the next, we will focus on the synthetic control for the outcomes log nights

and prices on a month level.

Table 3
Synthetic Control Weights (Log Outcomes, Month Level)

	Nights (SCM)	Price (SCM)	Nights (Ridge)	Price (Ridge)
Asheville	0.00	0.00	0.015	−0.186
Chicago	0.00	0.129	0.072	0.240
Washington DC	0.00	0.00	0.069	0.075
Denver	0.127	0.00	0.152	−0.087
Montreal	0.00	0.00	0.085	0.104
Nashville	0.449	0.641	0.259	0.434
Rhode Island	0.091	0.00	0.056	0.133
San Francisco	0.270	0.00	0.258	−0.021
Twin Cities	0.064	0.230	0.034	0.309

Overall, the regular synthetic control method tends to shrink weights of 'irrelevant' control cities to 0, while adding ridge regression does not. The results confirm the behavior of ridge regression that it provides control weights with less deviation from the uniform weight scenario (in this case, 0.111 is the uniform weight value). Furthermore, ridge regression outputs negative weights for control cities, which Abadie et al argue that negative weights are not as interpretable and come at the cost for extrapolation[14].

Cities that contribute significantly to the non-ridge synthetic control when examining the number of nights include Nashville, San Francisco, and Denver. These results are similar when with ridge regression, but due to the penalty in weights we observe that cities like Chicago or Montreal that had not contributed to the synthetic control now providing a minor weight. These cities have a somewhat similar number of listings as Boston, and also have quite similar average estimated occupancies per listing per year. Notably these cities also have very similar population sizes of around 700,000 residents in its city proper.

Except for Nashville, a somewhat different set of cities create the synthetic control when examining changes in prices. This discrepancy between weights can be partially explained by the fact that the quantity transacted in a market may be due to different factors that those for changes in price. Notably, only 3 cities were selected in the non-ridge analysis: Chicago, Nashville, and Twin Cities. These cities had somewhat similar average prices per night.

4. Robustness of Results

We begin by analyzing the L2 imbalance, which describes the extent to which the synthetic control matches the treated unit's pre-treatment outcomes. The L2 imbalance measures the "root sum of squared errors" in the pre-intervention period:

We can compare the L2 imbalance of the fitted synthetic control with the L2 imbalance in the case if uniform weights were used to see the method's improvement.

[14] https://economics.mit.edu/files/11859

Table 4
L2 Imbalance

	SCM	Ridge SCM
Number of Nights		
L2 imbalance	0.069	0.096
% improvement over uniform weights	83.9%	77.7%
Price		
L2 imbalance	0.202	0.177
% improvement over uniform weights	67.2%	71.3%

Note that the L2 imbalance may be reduced in the case of ridge regression since the constraint on non-negative weights is relaxed.

Next, we attempt to see if a similar effect can be observed if we utilized the weights from the analysis for log nights into the analysis for log price, and vice versa. Since default weights was not an available argument, I had to 'hack' the source package to retrieve the analysis, but was unable to produce standard errors or plots from the results.
Fitting weights from the SCM analysis on log nights (column 1) onto log price yielded a decrease of 0.020, less than half the 0.048 decrease reported in table 1. On the other hand, fitting weights from the SCM analysis on log price (column 2) onto log yield yielded a decrease of -0.580, which is slightly more than double the reported results of -0.269 in table 1. This indicates that the synthetic control for log nights fitted for Boston is not consistent with that fitted for log price. Nonetheless, the signs of the change appear to be consistent and match our intuition for the occupancy tax.

5. Potential Issues
Look into: heterogeneous responses from different hosts/host sizes (pros vs casual hosting)
More months of data, overfitting issues
Adding controls units.
Issues with multi channeling hosts: could be ATE on all home stays

Conclusion
Results
Policy?