Stemming the Tide: An Econometric Analysis of New Orleans's 2017 Short Term Rental Regulations

By Joshua Lee, Michael Lu, Arjan Kang, Vladimir Zhuravskyy *

On December 1, 2016, the city of New Orleans passed a sweeping set of regulations on short-term rentals (STRs), with the goal of addressing locals' complaints about the spread of Airbnbs and other STRs. Despite the broad literature investigating the positive and negative consequences of Airbnbs and STRs on local economies, few studies investigate the solutions governments employ to mitigate the harms. This report employs difference-in-differences and synthetic control models to investigate whether New Orleans' regulations and unique enforcement methods affected STR listings. There is strong evidence that regulations reduced citywide listings, with heavier-regulated neighborhoods experiencing stronger declines. We posit that such regulations may have an unintended effect of "demand spillover," leading to an opposite effect of nearby neighborhoods experiencing increased demand post-regulation.

I. Background and Topic Question

While New Orleans is a city that attracts tens of millions of tourists in recent years, locals have not been quick to accept Airbnbs as a hosting mechanism for such visitors. Most complaints boiled down to Airbnbs often hosting loud, noisy parties and causing increased rent near homes that were increasingly used for tourist bookings. To balance locals' complaints with the importance of Airbnb tourism, New Orleans passed a set of regulations on short-term rentals (STRs) on December 1, 2016, with the regulations set to take effect between April 1 and June 15, 2017. Alongside establishing permit and license requirements, fees, and taxes, the framework limited investors' ability to buy properties for STRs in non-commerical zones and banned STRs in the majority of the French Quarter, widely considered to be New Orleans's most popular neighborhood for tourists.

Topic Question: How have regulations on short-term rentals affected Airbnb listings in New Orleans?

This question can impact policy design in New Orleans: as it continues to grapple with the problems posed by Airbnb and other STR services, it may look at past regulations to determine what was effective. There is also a lack of research on policy solutions for the problems posed by STRs, while the effects of STRs on rent, tourism, and gentrification are well-researched. Other cities grapple with the same issues New Orleans did with Airbnb, with arguably less of the benefit. Understanding how to regulate STRs to mitigate their harms is crucial for creating effective regulations.

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II. Executive Summary

Our research question thus had two prongs. First, how did city-wide listings change following the imposition of New Orleans's regulations? Second, how did New Orleans's neighborhood-specific regulations affect neighborhood-level listings, and did they have the unintended consequence of increasing Airbnb listings in nearby neighborhoods? We employed a difference-in-differences model to analyze the city-wide effect of New Orleans's regulations, using Nashville as a counterfactual. We found strong evidence that New Orleans's regulations led to a fixed reduction in the number of Airbnb listings, by around 1,000, with a 95% confidence of interval of [-2045.80, -6.54]. However, apart from this reduction, the data does not suggest that the regulations significantly affected the growth rate of New Orleans's Airbnb listings, suggesting that the regulations eliminated certain STR stock but did not significantly disincentivize investors and landlords from investing in STRs throughout the city.

To answer the second question, we employed a synthetic control model to analyze the neighborhood-specific effect of New Orleans's regulations on the French Quarter. We found that the regulations reduced the number of monthly listings the French Quarter by 200 by the year 2019, a statistically significant decrease. However, due to limitations in the SCM model, we believe that the reduction could have been much greater.

Finally, we investigated whether the regulations on the French Quarter led to a "spillover" affect that increased the number of listings in the French. We created a synthetic neighborhood surrounding the French Quarter, based on the neighborhoods bordering the French Quarter, and fitted another synthetic control model to this neighborhood. We found some evidence that the French Quarter led to spillover in nearby neighborhoods, with our evidence being statistically significant at the 10% level.

III. Technical Exposition

A. *Data Collection*

Because our research question concerned causal analysis, we required time-series data to analyze patterns pre- and post-treatment. This requirement eliminated the real estate, demographics, venues, and listings datasets, which all either lacked dates or had dates completely before or after the regulations' implementation. The data provided in the *calendar.csv* for New Orleans only ranged from June 2 2017 to June 1 2018, which is after the implementation of the regulations from April 1, 2017 to June 15, 2017. We later found the data could not be used to supplement our post-regulation analysis because it did not correctly indicate the number of listings for each day or month when compared to other obtained datasets. We believe that the *calendar.csv* is an indicator of the availability for the next year of every listing found on Airbnb's website on a particular day, which would explain why each listing appears either 365 or 730 times in dataset. Also, the *calendar.csv* data lacks zipcodes or neighborhoods, making it too low-fidelity for our analysis.

We thus searched for listings data that at least had quarterly data broken down by zipcodes. 'Inside Airbnb" and its archives provided much of that data for after June 2017, with files in almost the same format (i.e. the same rows) as *listings.csv* for each month of the year. Toms Lee's personal blog, which was a precursor to the Inside Airbnb project, provided much of the listings data for the period beforehand. Just like the Inside Airbnb files, these files were lists of listings, albeit with far fewer columns and details. We ultimately were able to attain csvs of listings for multiple cities from 2015 to 2021, each with columns for neighborhood, price, and reviews.

B. *Exploratory Data Analysis: Deciding on the Difference-in-Differences Model*

We began by counting the number of listings per month on the city level.

Figure 1. Monthly Airbnb Listings in New Orleans, Nashville, and Los Angeles, 2015 to 2021

In Figure 3, we depict three of the five cities we analyzed. Three patterns emerge:

1) The quality of available data varies widely. For example, Los Angeles is missing long stretches of data from July 2017 to July 2020, which made these cities difficult to use as counterfactuals.

2) In each city, the growth rate of listings within most three-to-five month periods is relatively gradual and consistent, apart from significant exogenous events such as the implementation

of New Orlean's new regulations or the COVID-19 pandemic (the beginning of which we lack data for, but we assume that listings were impacted by the pandemic.)

3) New Orleans's long-term trend in monthly listings is remarkably similar to that of Nashville, remaining near-parallel except for the six months following New Orleans's regulations.

These patterns led us to select Nashville as the counterfactual for a difference-in-differences model. First, despite missing data for 31 of the 69 time periods from August 2015 to April 2021, Nashville has few extremely-long periods of missing data. The number of listings does not vary significantly each month, so we are primarily concerned with mitigating significant, long-term interpolation, preferring Nashville over cities like Los Angeles. Second, the near-parallel trends of Nashville and New Orleans satisfy the key assumption of parallel trends pre-treatment for the difference-in-differences model.

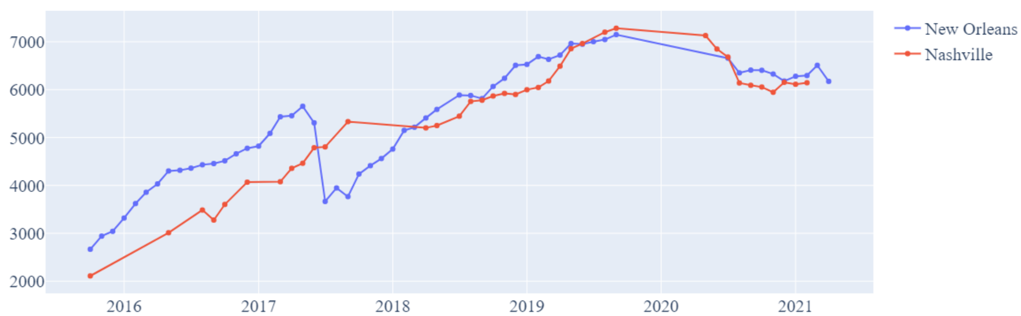

Figure 2. Monthly Airbnb Listings in New Orleans and Nashville

Thus, the dataset containing New Orleans' and Nashville's monthly listings—visualized in Figure 3—served as the basis for our difference-in-differences model. We did make one key adjustment: we linearly interpolated the data for missing months prior to the COVID-19 pandemic. For example, Nashville is missing several months of data between late 2015 and early 2016, so we interpolated the data points with numbers from the lines depicted in the figure. This increased the number of data points available and thus the power of our difference-in-differences model. We believe that the interpolated data points are relatively accurate to the actual missing numbers, which is supported by our observation that the number of listings typically follows a gradual, monotonic increase, without significant variance in the short-to-medium term.

C. *Our Difference-in-Differences Model*

The difference-in-differences model is amongst the most popular econometric models in policy analysis. It was famously used in Alan Krueger and David Card's landmark 1993 study that found New Jersey's minimum-wage increase did not decrease full-time wage employment, challenging conventional economic wisdom concerning minimum wages' effects on employment.

The model estimates an intervention effect by comparing the outcomes of the treated group to a control group post-intervention. The key assumption is that the treated group's trend, or growth rate, is the same as the control group's prior to intervention, as noted above. Then, it calculates the difference between the treated group's actual post-intervention outcome and the treated group's post-intervention outcome had it continued to parallel the control group's trajectory.

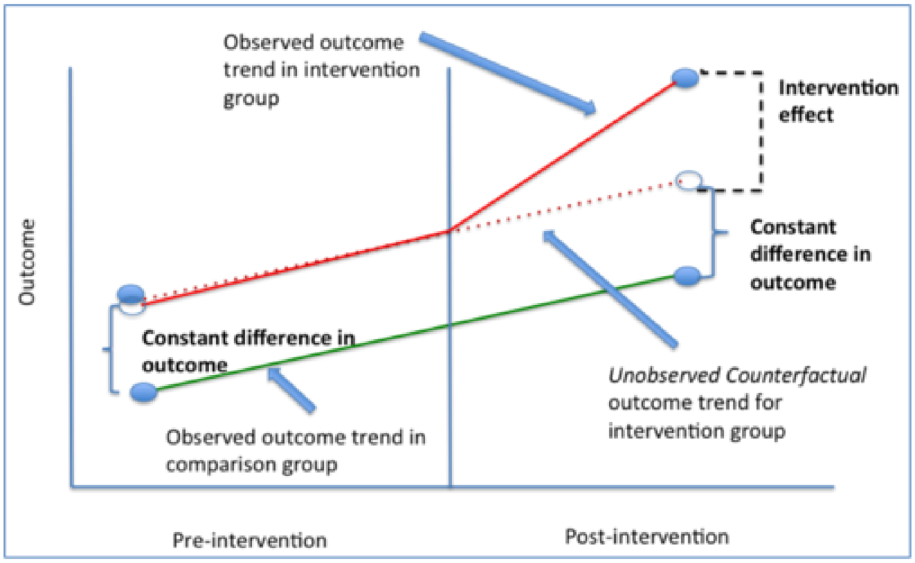

Figure 3. The Difference-in-Differences Model

Source: Columbia University School of Public Health

More formally, we can describe our difference-in-differences model as follows:

$$\hat{Y} = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_3 X_1 * X_2$$

where Y is the number of listings, β_0 is our baseline, X_1 is an indicator random variable that is 1 if the city is New Orleans (and thus 0 if the city is Nashville), X_2 is an indicator random variable that is 1 if the time of the data point is on June 1, 2017 or after, and β_2 and β_3 are coefficients. Fitting our difference-in-differences model as a least-squares regression produced the following results:

Table 1—Least-squares Difference-in-Differences Regression Results

	Coefficient	Standard Error	P> $\|t\|$	[0.025, 0.975]
Intercept	3314.55	184.84	0.000	[2948.32, 3680.78]
New Orleans	2618.11	228.36	0.000	[2165.648, 3070.568]
Post-Treatment	972.3500	261.40	0.000	[454.418, 1490.282]
New Orleans, Post-Treatment	-1026.17	322.95	0.002	[-1666.041, -386.291]

According to our model, regulations reduced the number of monthly Airbnb listings in New Orleans by around 1026 listings. The difference-in-differences model assumes a fixed, constant reduction, which is supported by the continued parallel trends in New Orleans and Nashville listings six months after regulations. In other words, while regulations reduced the number of listings in New Orleans, they have not significantly changed New Orleans's growth rate from that of the counterfactual.

However, the standard errors are likely underestimated due to significant autocorrelation between data points, as listings have grown significantly over time. This leads us to utilize heteroskedasticity and autocorrelation (HAC) robust standard errors as proposed by Newey and West (1987). Utilizing HAC-robust standard errors does not change the coefficients, but it leads to larger standard errors:

Table 2—Least-squares Difference-in-Differences Regression Results, Using HAC-robust Standard Errors

	Coefficient	Standard Error	P> $\|t\|$	[0.025, 0.975]
Intercept	3314.55	262.79	0.000	[2799.48, 3829.62]
New Orleans	2618.11	331.78	0.000	[1967.82, 3268.40]
Post-Treatment	972.3500	396.26	0.01568	[195.68, 1749.02]
New Orleans, Post-Treatment	-1026.17	520.22	0.05101	[-2045.80, -6.54]

With the larger HAC-robust standard errors, we see that New Orleans's regulations led to a statistically significant reduction in listings at the 10% level, just slightly missing the 5% level.

Overall, the difference-in-differences model offers relatively strong evidence that New Orleans's regulations reduced the number of listings in the city. Because New Orleans's long-run growth rate continued to parallel Nashville's six months after regulations, **we believe that the regulation led to a fixed reduction in the number of listings rather than in listing growth.** This may seem counterintuitive: that the licensing requirements, new taxes, and bans on different types of STRs did not act as some sort of disincentive for new owners to enter the market.

To explain how New Orleans's regulatory framework led to a fixed reduction in Airbnb listings but not in the medium-to-long term growth rate, we propose the following hypotheses. **First, the regulations invalidated a significant proportion of the STR housing stock, leading to the fixed reduction in the number of listings** that our model suggested. For instance, certain types of STR listings in non-commerical zones and most STR listings in the French Quarter were invalidated immediately following the implementation of regulations. **Second, although regulations may have stifled the growth rate in invalidated listing types, investors and landlords shifted into valid STR properties, leading to a higher number of listings in these STR property types.** The French Quarter, a popular neighborhood for tourists that was targeted in New Orleans's regulations, offers a strong case study to test our hypotheses.

D. Exploratory Data Analysis: The French Quarter

The French Quarter is a strong case study for our hypotheses. First, New Orleans directly banned hundreds of STRs in most of the French Quarter, so it may best show the fixed reduction in Airbnb listings. Second, the French Quarter is a popular tourist location, so its nearby neighborhoods may have experienced "spillover" effects post-regulations if investors rotated into them.

We grouped the listings datasets by date and neighborhood to find aggregate statistics on total listings, median reviews, and median prices per neighborhood. We eliminated all data after August 2019 because of a significant data gap from August 2019 to mid 2020. Figure 4 shows Airbnb listings fell across New Orleans neighborhoods post-regulations, most significantly in the French Quarter. And, two years later, the French Quarter's listings had grown far less than neighbors.

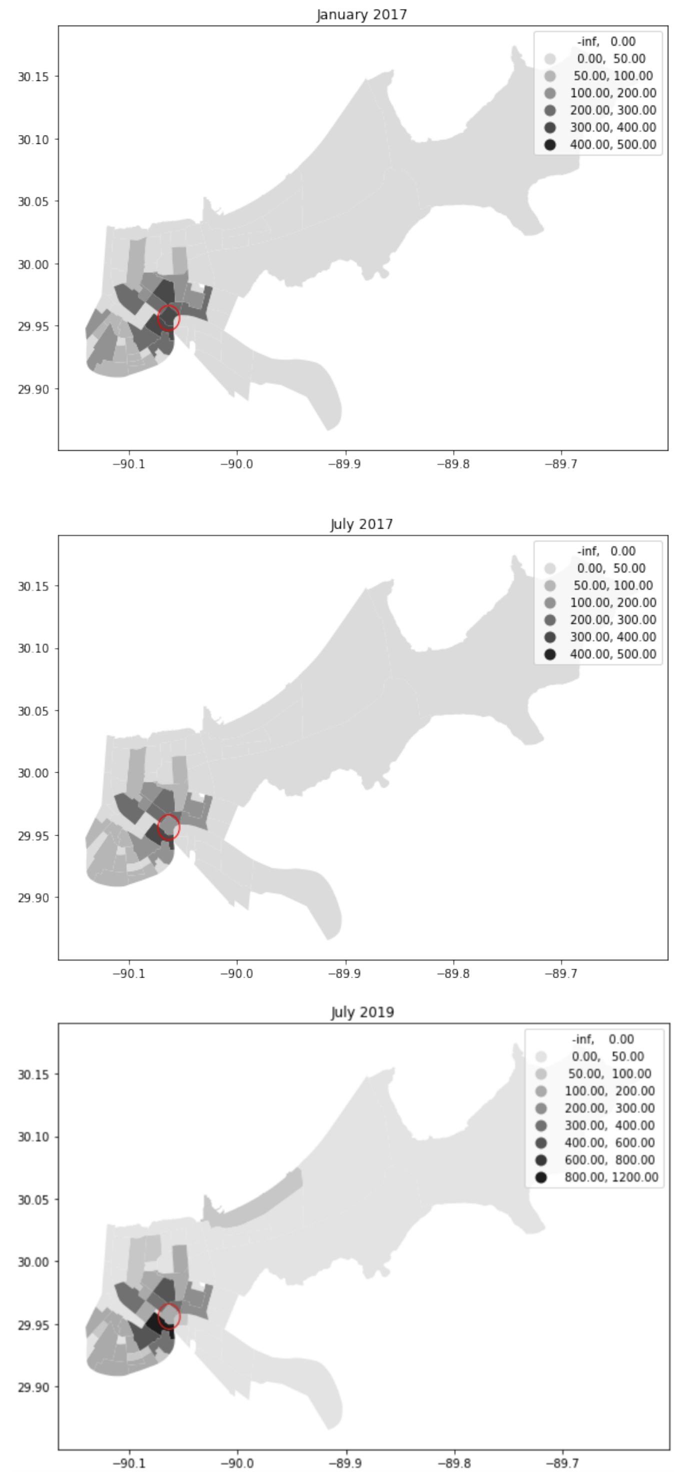

FIGURE 4. NEW ORLEANS NUMBER OF AIRBNB LISTINGS BY NEIGHBORHOOD, JANUARY 2017, JULY 2017, JULY 2019

Note: Growth in the French Quarter, circled in red, remained slow post-regulations but listings in nearby neighborhoods grew.

Although we were provided with a large amount of zipcode-level data, we decided to use neighborhoods because they are far more precise. Especially in New Orleans, zipcodes are poor indicators of regional economics, often overlapping or cutting across various neighborhoods.

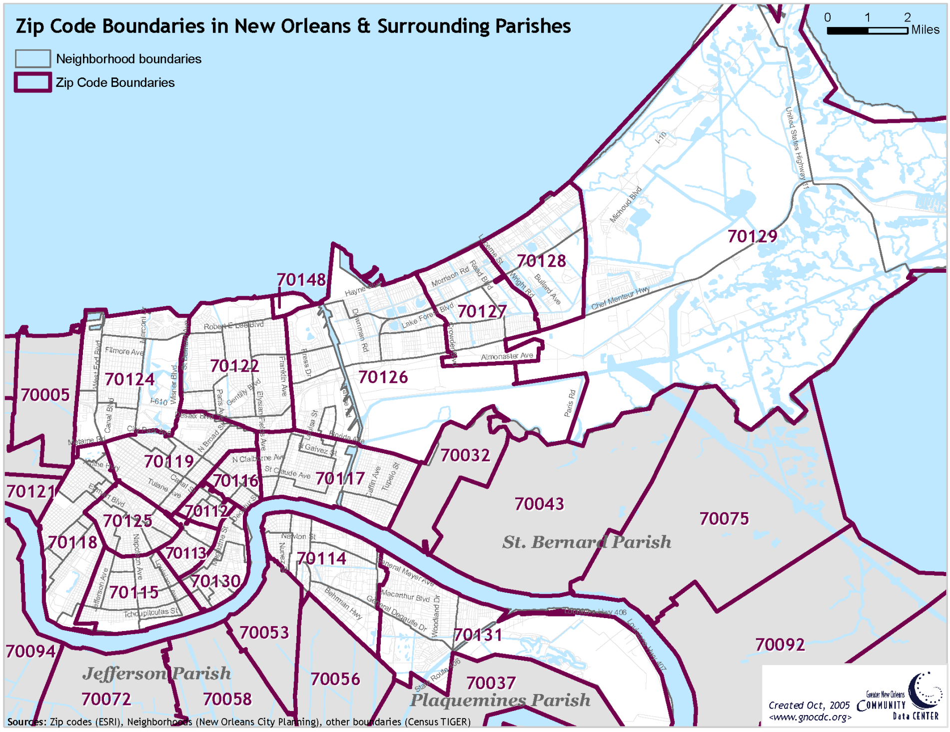

Figure 5. New Orleans Zipcodes Overlayed on Neighborhoods

Note: The French Quarter is split into three zipcodes: 70116, 70112, and 70130. This makes its regulations difficult to study with zipcodes. Source: The Data Center

Finally, we compared the French Quarter, the top Airbnb neighborhood prior to the imposition of regulations, to the next ten most popular neighborhoods for Airbnbs.

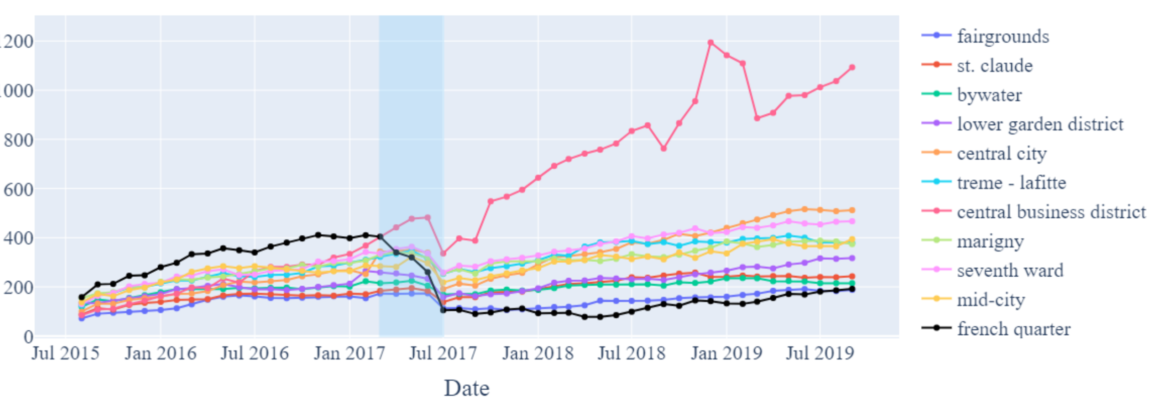

Figure 6. Listings in the French Quarter Next Ten Most Popular Neighborhoods, August 2015 to August 2019

Note: The French Quarter is split into three zipcodes: 70116, 70112, and 70130. This makes its regulations difficult to study with zipcodes. Source: The Data Center

The graph shows data for most months from August 2015 to July 2019 for the top 10 neighborhoods in Airbnb listings. Moreover, while the French Quarter is a clear leader before regulations, several other neighborhoods with many listings also showed significant growth prior to regulations. The Central Business District, which seemingly grew fastest even prior to regulations, is a clear outlier. It borders the French Quarter, so it lends credence to our "spillover" hypothesis. However, it remains to be seen whether that growth was a result of spillover or a continuation of past growth. The data makes a strong case to use the synthetic control model to identify causal relationships for our fixed drop and spillover effect hypotheses, with the 72 neighborhoods having significant Airbnb listings and consistent growth in top neighborhoods. However, one potential issue is that the neighborhoods are not necessarily independent (our spillover hypothesis), violating a key assumption of the synthetic control model. Still, the degree of correlation between most neighborhoods may be far smaller, and we later show that our synthetic control method's estimates are robust.

E. Our Synthetic Control Model: The French Quarter

The synthetic control model (SCM) is a recent development in econometrics, yet already very popular in policy analysis. Famously used in Abadie, Diamond, and Hainmueller (2010) to study the effects of California's Proposition 99 on state cigarette sales, it was deemed "the most important innovation in the policy evaluation literature " in Journal of Economic Perspectives (Spring 2017).

The model creates a "synthetic control" to compare against the treated group. It is often used in place of difference-in-differences when there are multiple counterfactuals to compare the treated group against, but none of the counterfactuals can be used as an exact control. New Orleans's neighborhoods thus offer a relatively large pool of counterfactuals for our SCM.

The synthetic control is fitted such that it follows the treated group's trend as closely as possible prior to the treatment, typically through minimizing least squares. Given a vector of the treated group for a certain time period, say August 2015 to May 2017, and a matrix of counterfactuals with the data for the same time period, the synthetic control assigns weights to each counterfactual. To reduce overfitting, the SCM limits the sum of the squares of the weights to 1, although others use Lasso regression or set the limit to a different number.

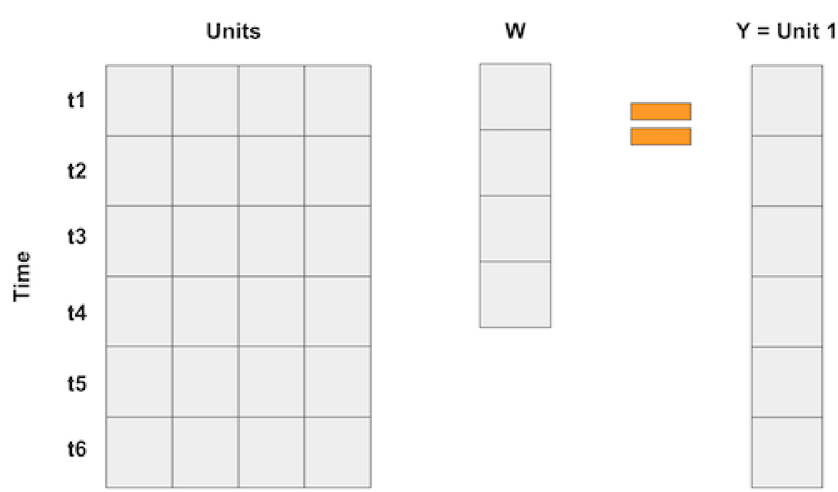

Figure 7. Counterfactuals, Vector of Weights, Vector of Treatment Group for Synthetic Control Model

Source: Causal Inference for The Brave and True, By Matheus Facure Alves

To choose how to reduce overfitting, we cross-validated Ridge, Lasso, and limited-sum-of-squares methods with different parameters for regularization and sum-of-squares limits on the 20th to 10th most popular Airbnb neighborhoods in New Orleans. For each of the neighborhoods, we trained a SCM and summed the squared errors for the model's estimates compared to the actual outcomes for each of the times from June 1, 2017 to July 1, 2019.

We settled on limiting the sum of squares of the weights to 1. This is the most common regularization method for economists and achieved by far the lowest error, but there are several issues. Most prominent is that the French Quarter, which had—by a significant amount—was the clear leader in listings prior to regulations. The limit makes fitting to the largest quantity difficult because each of the counterfactual weights are limited to less than 1. Nevertheless, we believe its extremely-low cross-validation error justified our choice.

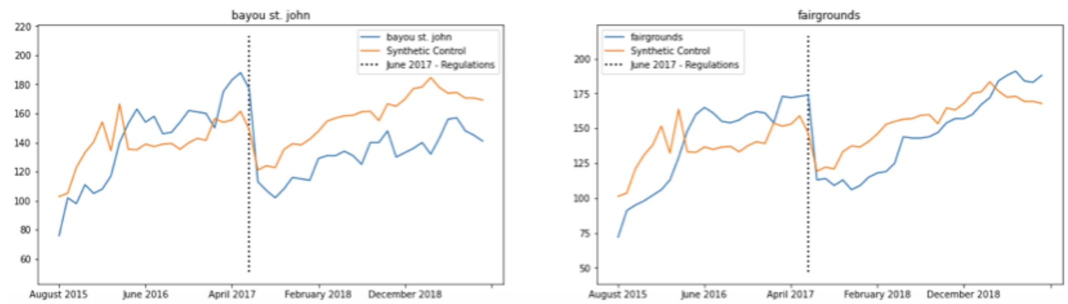

FIGURE 8. CROSS VALIDATION ON TOP 20-TO-10 NEIGHBORHOODS AGAINST THE SYNTHETIC CONTROL MODEL

Note: For most neighborhoods in the CV set, the synthetic control model is remarkably accurate at predicting listings over time. However, it sometimes struggles with underfitting due to the sum-of-squares limit of 1.

Having selected our SCM and its parameters, we fit the model to the French Quarter to evaluate the effect of regulations.

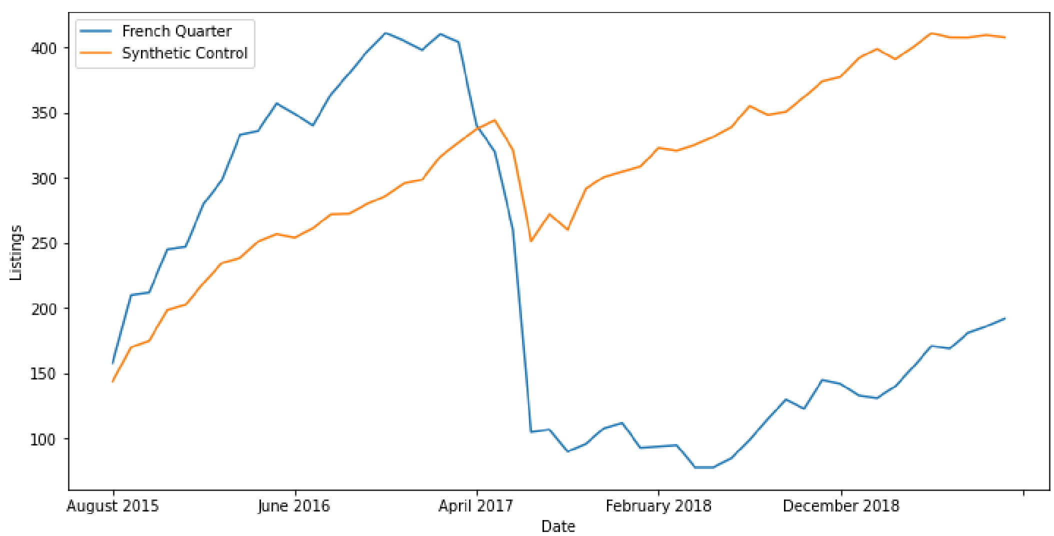

FIGURE 9. LISTINGS IN THE FRENCH QUARTER VERSUS THE SYNTHETIC CONTROL

Note: The limits of our synthetic control model's regularization method become apparent with the French Quarter, the leader in listings prior to treatment.

According to our SCM, regulations reduced the total listings in the French Quarter by around 200, by August 2019. Note that the model struggles with fitting to the French Quarter pre-regulations, likely due to the sum-of-squares limit of 1. However, this underfitting is mitigated by the model underestimating the listings in the French Quarters pre-regulations. This leads us to believe that, if anything, the model has underestimated the reduction in listings from the regulations.

We use Fischer's exact test to test the statistical significance of our numbers. To do this, we create synthetic models for the next 30 most popular Airbnb neighborhoods prior to the regulations. Limiting to the top 30 eliminates neighborhoods with insignificant numbers of listings pre-regulations, since neighborhoods with near-insignificant quantities are also quite difficult to model.

Then, we calculate our model's errors by subtracting the actual number of listings in each neighborhood by the numbers predicted by the synthetic control model. If the error is positive, the neighborhood has outperformed the model, if negative, the neighborhood has underperformed, and if 0, the model was exactly correct.

Again the model is remarkably accurate in predicting the total listings in each neighborhood even two years after regulations. Apart from the Central Business District and French Quarter, most errors cluster around 0. Applying Fischer's exact test to the error model, we find that the reduction in listings is statistically significant, with a p-value of .032, which is significant at the 5% level.

Additionally, for a year after regulations, the gap between total listings and the synthetic control continues to grow. This suggests that the growth rate in highly-regulated neighborhood was hindered for at least a year after regulations. However, the statistical significance of this trend relies mostly on the subjective selection of time periods to evaluate. For several months afterwards, invalidated properties may have continued to be taken off the market as Airbnb and New Orleans enforced regulations. As such, we can at most conclude that there is strong evidence there was a significant fixed reduction in listings following regulations and there is some evidence that regulations hindered the French Quarter's growth afterwards. This validates our first hypothesis that the regulations led to a fixed reduction in the number of listings in the French Quarter.

Finally, note that the French Quarter begins to underperform the model by a statistically significant amount just prior to regulations. Since regulations were passed in late 2016, that could have affected the French Quarter as investors pulled out of the STR properties prior to of regulations.

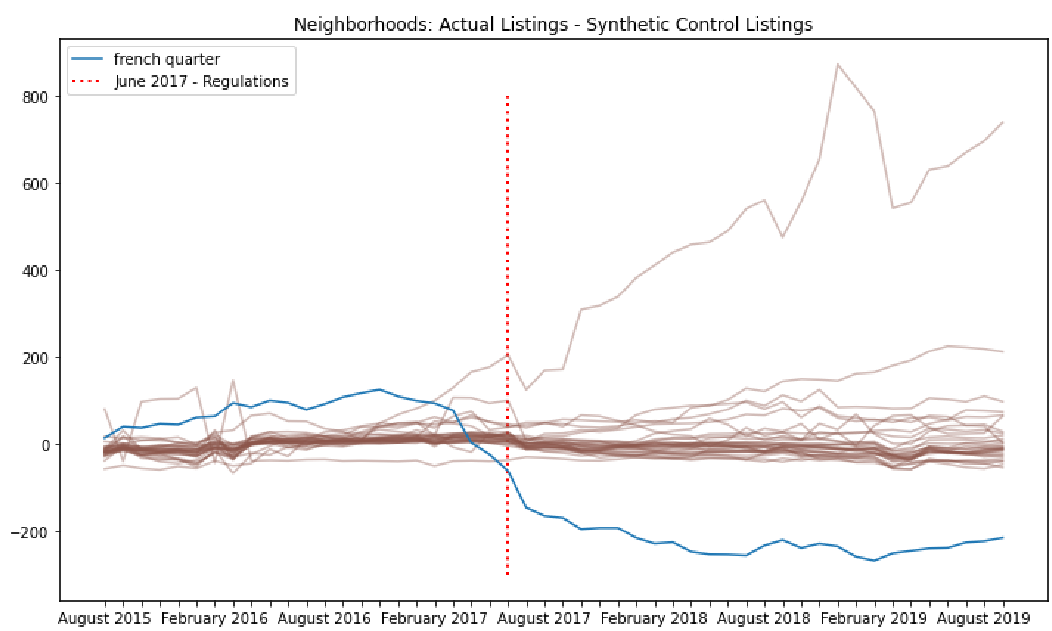

Figure 10. Errors in the Synthetic Control Model for the Top 30 Neighborhoods and the French Quarter

Note: The limits of our synthetic control model's regularization method become apparent with the French Quarter, the leader in listings prior to treatment.

F. The Spillover Effect: Did Investors Shift to Nearby Neighborhoods?

As previously mentioned, the French Quarter is a strong candidate to examine the spillover effect because its popularity makes nearby neighborhoods quite appealing for STR investors. We can thus apply the SCM to bordering neighborhoods to evaluate the spillover effect.

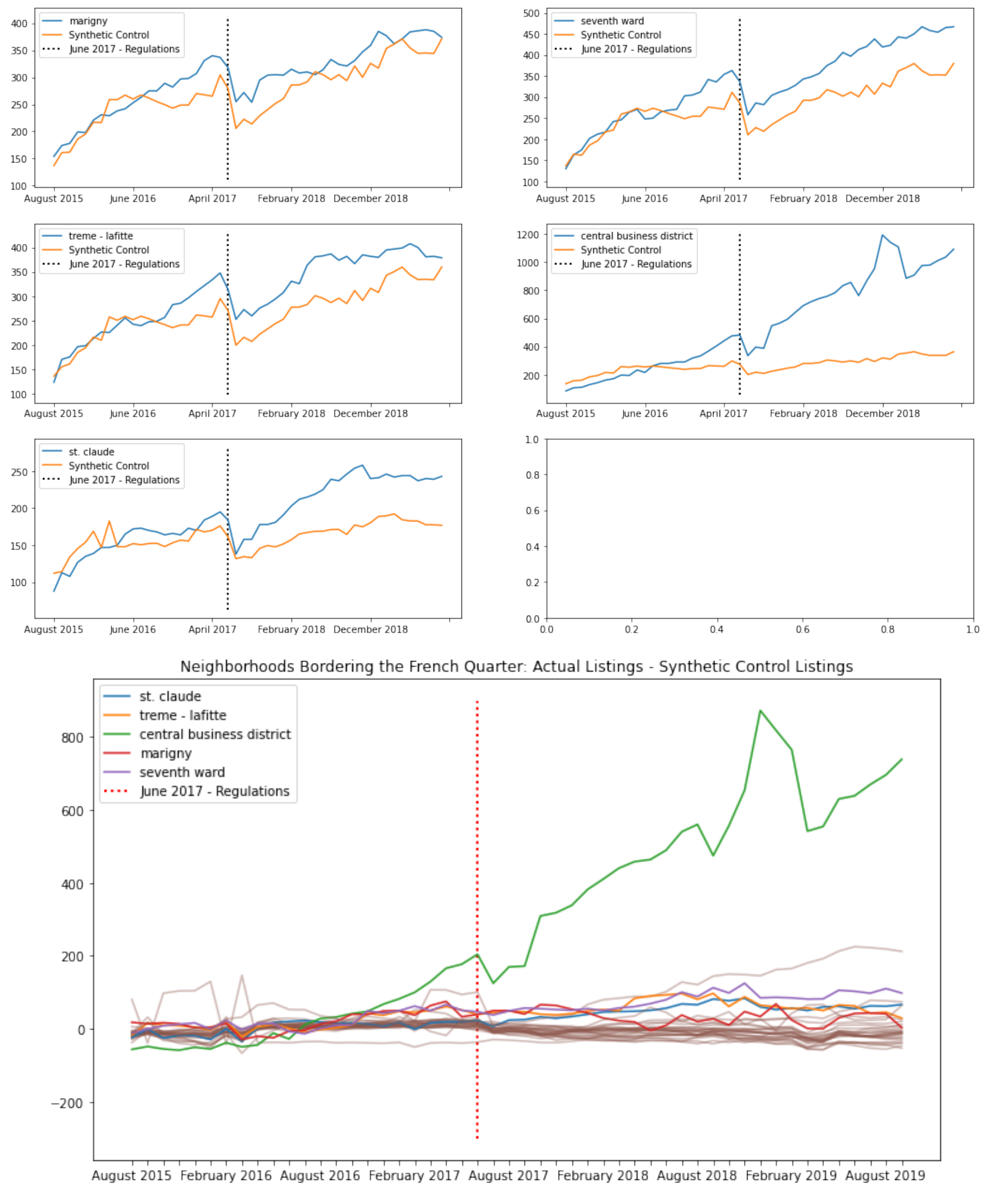

Figure 11. Neighborhoods in the Top 30 Bordering the French Quarter, Actual vs Synthetic

Note: The neighborhoods bordering the French Quarter generally outperformed the synthetic control models, but apart from the central business district, it is difficult to see whether the effect is statistically significant.

The data posed us a challenging question: how would we prove that New Orleans's regulations led to spillover elsewhere? Would spillover act as more of a fixed effect or a growth-rate effect? Investors could have quickly shifted into nearby neighborhoods leading to a fixed increase in the number of listing or the shift could have been more gradual, leading to a longer period of elevated growth. How can we be sure that the spillover was equal in each of the nearby neighborhoods? Certain neighborhoods may have been more appealing "second options" to the French Quarter. Should the quantity of spillover be evaluated as a percentage increase or an absolute increase?

We propose the following modification to the synthetic control model to capture the spillover effect. First, we create a synthetic neighborhood that is the average of the bordering neighborhood listings. This allows us to capture all potential spillover from the French Quarter into bordering neighborhoods. Second, we fit a synthetic control model to the synthetic neighborhood, excluding the neighborhoods that border the French Quarter from the training set. This allows us to compare our spillover neighborhood to other, non-spillover neighborhoods. Third, we test for statistical significance by evaluating the number of monthly listings in August 2019. This number should be relatively generalizable, since the number of listings do not vary significantly month-to-month. While this does not allow us to analyze the trends in growth rates, it is sufficient to show whether the regulations led to a net positive spillover effect in bordering neighborhoods. Finally, we can analyze the errors in the synthetic control model to our hypothesis that growth rates in spillover areas—at least for a brief period—increased enough to offset the decrease in the French Quarter's rate.

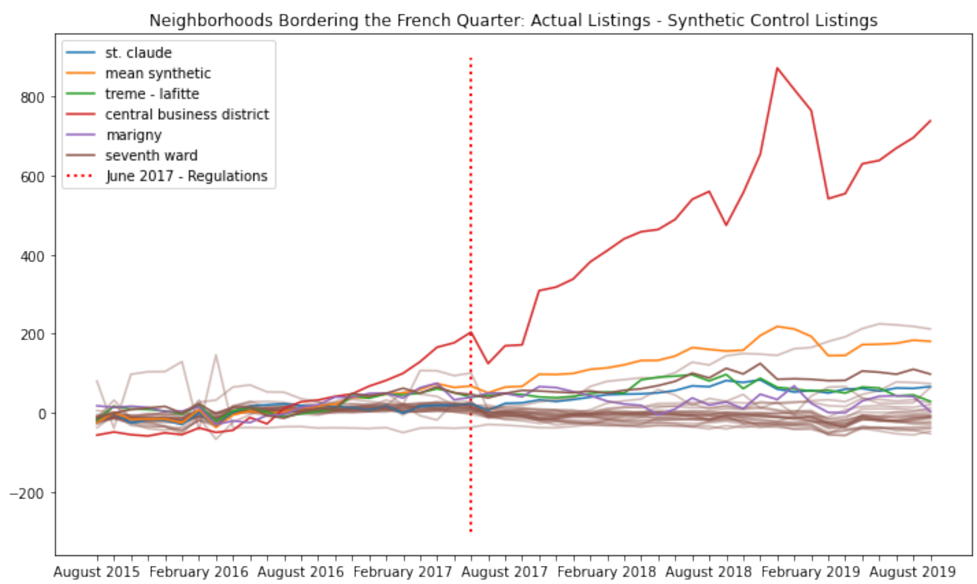

FIGURE 12. MEAN SYNTHETIC AND TOP 30 NEIGHBORHOODS, SCM ERRORS

When excluding the actual neighboring neighborhoods, the mean synthetic neighborhood has a statistically significant increase, at the 10% level. Moreover, there is evidence that its growth accelerates through February 2019, as the positive gap between it and the synthetic model continues to grow.