Short Term Rentals and Housing Prices in Large US Cities

Jack Ohlmeier

University of Colorado Boulder

Advisor

Jonathan Hughes, PhD, *Department of Economics*

Honors Council Representative

Brian Cadena, PhD, *Department of Economics*

Defense Committee

Thomas Thibodeau, *Leeds School of Business*

Defense Date – April 5th, 2021

Abstract

In the past decade, platforms like Airbnb and VRBO have popularized Short-Term Rentals (STRs), and many cities have taken action to regulate this emerging market. But how do these restrictions regarding STRs affect housing prices in these cities? I answer this question using Housing Price Index (HPI) data from the Federal Housing Finance Agency as well as STR restriction data on 200 large cities in the United States. Using this data, I estimate the effect of STR regulations on housing prices by employing a differences-in-differences model with variable treatment timing and find that STR restrictions lead to a 6.136 percentage point increase in HPI. Additionally, I find that harsher STR restrictions lead to a 14.84 percentage point increase in HPI, while registration-only requirements do not have a statistically significant effect on HPI. However, I do have concerns with these results in regard to omitted variable bias.

Introduction

In the last 10 years, short-term housing rentals (STRs), usually defined as housing rentals less than 30 days, have become increasingly popular with travelers as an alternative to hotels and other traditional lodging choices. Online STR platforms, such as *Airbnb* and *VRBO*, allow households to easily rent out individual rooms or entire properties when not in use, and allows landlords to rent out properties on a shorter basis than for traditional leases. These STRs can offer more preferable amenities to consumers than hotels, such as more space, full kitchens, and convenient locations. However, this surge in popularity has also led to negative externalities, such as noise and security concerns for neighbors of these STR properties, as well as claims of decreased housing affordability (Greenberg 2018, Lieber 2015). This has led to a regulatory challenge for municipalities, who must balance the desires of homeowners, renters, landlords, and tourists. Many cities have decided to provide official regulation for STRs, and while each ordinance or law is different, most cities seek to restrict STRs by requiring the hosts to register

with the city, requiring the host to be present during the rental period, or by requiring the rental property to be the host's primary residence. However, these restrictions may have an effect on housing prices through various mechanisms, such as an efficient use effect, a housing supply effect, and an externality effect (Turner et al. 2014). By restricting STRs, households lose the opportunity to generate income from rooms they are not using, or from their entire house when they are not home (for example on vacation), reducing the efficiency of housing, leading to a decrease in demand and therefore prices. Additionally, restricting STRs may increase the supply for housing, since the portion of the housing stock used for STRs can be reallocated to long term housing instead, once again leading to a decrease in prices. Finally, restricting STRs could eliminate the negative nuisance and security externalities, which could increase property values.

So what has been the overall effect of these restrictions in cities that have passed them? In this thesis, I aim to measure the effect of STR regulations on the prices of single-family homes in large American cities. Studies have found that an increase in STR listings in an area leads to an increase in both house prices and rents mainly through supply effects (Barron et al. 2020, Garcia-Lopez et al. 2020), so if STR regulations are effective in reducing the number of STRs in an area, we would expect a corresponding reduction in housing prices. However, if STR restrictions are either not very stringent or not well enforced, the effect on housing prices could be very small.

In order to measure this effect, I use a differences-in-differences approach with variable treatment timing. To measure housing price, I use the Housing Price Index (HPI) from the Federal Housing Finance Agency (FHFA). The FHFA HPI is a measure of annual appreciation in single-family house prices that is calculated by reviewing repeat transactions on properties with mortgages that have been purchased or scrutinized by Fannie Mae or Freddie Mac. STR

restrictions are recorded by an indicator variable that is equal to one if a restriction in in place in a certain area in a certain year, and data for this was hand gathered, as there are no existing nationwide datasets on STR restrictions. Additionally, the type of STR restriction imposed is recorded using additional indicator variables.

There currently exists a growing body of research on STRs and home sharing, focusing mainly on the relationship between the number of STR listings in an area and housing prices (Barron et al. 2020, Garcia-Lopez et al. 2020). Studies on STR restrictions, however, are far fewer, and so far Koster et al. (2019) is the only published study on the subject. They found that STR restrictions reduced housing prices and rents by about 3% by looking at housing prices and in Los Angeles County. This thesis contributes to this literature by studying STR restrictions and their effect on housing prices on a national level. This thesis also contributes to a growing literature on the relationships between tourism and long-term residency (Hilber & Schöni 2020, Carlino & Saiz 2008), as well as a well-established literature on the effects of land use regulation (Turner et al. 2014, Ihlanfeldt 2007).

My results show that imposing STR restrictions leads to a 6.136 percentage point increase in HPI. Breaking the effect up by type of restriction, harder restrictions (here defined as requiring the STR property to be the host's primary residence or requiring the host to be present during the rental period) increase HPI by 14.84 percentage points while registration-only requirements have no statistically significant effect on HPI. However, I am concerned with a positive omitted variable bias on my estimates, particularly in that of the effect of harder STR restrictions on housing prices.

Literature Review

The research in this paper on the effect of short-term rental (STR) regulation on housing prices relates to a quickly growing literature on home sharing, short-term rental of entire houses, and housing costs (Barron et al., 2020; Garcia-Lopez et al., 2020; Koster et al., 2019). This related literature tends to focus on the number of STR listings in an area, often by using data on STRs listed on Airbnb. The findings in this literature indicate that more STR listings in a certain area increase the price of housing in that area, and this effect is stronger in areas with lower owner-occupancy rates and more STR activity. More specifically, Barron et al. (2020) found that at the median owner-occupancy rate, a 1% increase in STR listings through Airbnb led to a 0.018% increase in rents and a 0.026% increase in house prices. Garcia-Lopez et al. (2020) studied the effect of STRs in Barcelona, Spain and found that in a neighborhood with the average number of STR listings, STR activity was responsible for a 1.9% increase in rents and a 4.6% increase in home transaction prices. The consensus in this literature is that STR owners are shifting properties from the long-term to the short-term market, reducing the supply of long-term housing. This thesis contributes to this research by investigating if regulatory efforts have an effect on prices across the country.

One paper that is closely related to the analysis in this thesis is Koster et al. (2019), which studies the effects of STR platforms on the housing market in Los Angeles County using both a panel regression discontinuity design and a differences-in-differences specification, exploiting the variance of what they call “Home Sharing Ordinances” (i.e., regulations on STRs) within the county. This paper, using panel regression discontinuity design, found that STR regulations reduced listings posted on Airbnb by 50%, and reduced home prices by 3%. Using a differences-in-differences specification, they found that STR restrictions also reduced long term rents by 3%.

Additionally, they found that this effect was stronger in areas that were more popular to tourists. My research differs in that I will look at many urban areas across the United States, where STRs and regulations pertaining to them may not have as strong of an effect on housing costs. Los Angeles County is a popular tourist destination, so STR regulations in other, less travelled to cities may have a different effect on housing prices.

Additionally, my work contributes to research on the relationship on tourism and long-term residency. Carlino & Saiz (2008) found that when more pictures from tourists are geotagged in a certain location, those areas have higher levels of population growth. Essentially, population growth and tourism tend to go hand in hand. If tourism in an area is restricted somehow (by reducing the number of available STRs for example), population growth, and therefore housing prices, could be affected. More closely related to my research, Hilber & Schöni (2020) studied the effect of the Swiss Second Home Initiative, which banned the construction of second homes in certain areas. They found that the ban of second home construction lowered the price growth of primary homes and increased the price growth of second homes. The research in this thesis contributes to this literature by studying the effects of a different kind of housing regulation on long term residents.

This paper is also related to a wider literature on the effects of land use regulation (Turner et al. 2014, Ihlanfeldt 2007), which generally focuses on how land prices and house prices are affected by regulations through supply effects and the cost of regulatory constraints. Findings in this literature show that land use regulation decreases land prices due to regulatory constraints reducing the number of ways one can extract income from the land. However, home prices tend to increase with more land regulation, since home buyers' choices are restricted by these regulations, or in other words, the housing supply is reduced. However, since STR regulations

are generally imposed on areas where housing is already built, and don't restrict home buyers' choices much, it is yet to be seen what effect it will have on housing prices.

Data

To answer my research question, I will be using the Federal Housing Finance Agency (FHFA) House Price Index (HPI), and a hand-gathered dataset of STR restrictions in the 200 largest cities (by population) in the United States. The FHFA HPI is a weighted, repeat-sales index of single-family home prices using mortgage data from Fannie Mae and Freddie Mac that extends back to the 1990's for most areas. However, only data from 2010 to 2019 will be used for the following analysis. Specifically, HPI measures average price/value changes in sales and refinancing of the same property and is expressed as the percentage of the average house price in a certain base year (I will be using a base year of 2000). For example, if the HPI in a certain area in 2010 is 150, that means that the average house is 1.5 times more valuable in 2010 than it was in 2000. I am using the FHFA HPI data because it offers a robust measure of the change in single family home values, bases its calculations on actual home sales and refinancings as opposed to value estimates, and it uses a fully transparent methodology.

As I will be employing a differences-in-differences methodology, it is useful to look at trends in our treatment and control groups over time. Figure 1 shows average HPI over time for the treatment and control groups. Note that treatment group in this graph denotes any place that experiences an STR restriction at any time. From this graph we can see that places that implement STR restrictions have a higher HPI on average but have overall similar trends as places that do not implement STR restrictions at any point. However, the HPI in places that do implement STR restrictions does appear to grow at a faster rate after 2012 than in places that do

not implement STR restrictions. Figure 2 shows the average HPI for areas that implement an STR restriction at any point, but plotted over event time, which denotes how many years before or after an STR restriction has been put in place. Looking at this graph we can see that there does appear to be a slight decrease in the growth rate of HPI after an STR restriction is implemented, but this decrease doesn't seem to persist for longer than about 2 years.

The hand-gathered STR restriction dataset records city-level restrictions regarding STRs, in particular the answers to the following questions: Does the host have to be present during the stay? Do hosts have to register their STR with the city? Does the STR property have to be the host's primary residence? Additionally, the year regulations are implemented are recorded. This information was found by searching both local news articles and city websites. I am choosing to go with a hand gathered dataset for STR restrictions because, to my knowledge, there are not any nationwide datasets available of where and when STR regulations have been implemented. Figure 3 shows the number of cities with STR restrictions in place over time. From this we can see that STR restriction were uncommon before about 2014, and since then there has been a steady increase in the number of cities choosing to implement them. Looking at the types of restrictions cities choose to impose (Figure 4), we can see that requiring hosts to register with the city has been the most common STR restriction, and primary residence requirements and requiring the host to be present during the rental are similar in terms of popularity, but are both less common than registration requirements.

The final dataset will be a panel dataset where the unit of observation is city-year. The universe for my data is the HPI of the 200 largest cities in the US. My key variables will be HPI and the indicator variable for STR restrictions. I am going to restrict the data to the years 2010-2019, since most cities did not implement STR restrictions until the mid-2010's, and because

annual HPI data is only available through 2019 as of the writing of this thesis. Additionally, I wanted to avoid any complications in housing price data due to the Great Recession.

Methodology

The regression equation for my differences-in-differences specification is the following:

$$HPI_{ct} = \beta_0 + \alpha_c + \tau_t + \beta_1 treated_{ct} + \varepsilon_{it}$$

Where c denotes city, and t denotes year. HPI_{ct} is the Housing Price Index in city c in year t, and is our y-variable of interest. $treated_{ct}$ is an indicator variable that is equal to 1 if an STR restriction is in place in city c at time t, and is our x-variable of interest. β_1 therefore is our main coefficient of interest, as it will tell us the magnitude of the effect that STR regulations have on HPI. α_c are city level fixed effects that control for any local shocks, and τ_t are year fixed effects that control for any shocks to the housing market common to the year. ε_{ct} is the error term at the city-year level.

The key assumption essential to any differences-in-differences specification is that the control and treated groups would have the same trend in the absence of any treatment. In this case, the assumption is that if no cities ever implemented restrictions on STRs, all cities would have the same trend in housing prices, after including fixed effects. This assumption could be broken if there was something different about places that implemented STR restrictions that caused housing prices within those areas to change differently than areas that have not implemented any STR restrictions. This issue may be a threat to my analysis, and I shall discuss it later on in the discussion section of this thesis.

Additionally, I will assess my model by looking at leads and lags of the treatment. Functionally, this will look like the following regression model:

$$HPI_{ct} = \beta_0 + \alpha_c + \tau_t + \sum_{j=-m}^{q} \beta_j D_{ct+j} + \varepsilon_{it}$$

Where D_{ct} is an indicator for whether an STR restriction was imposed in year t. If HPI trends in treatment and control groups are the same pre-treatment, β_j for $j < 0$ should be zero or statistically insignificant, and β_j for $j \geq 0$ should be nonzero or statistically significant if an effect exists. I will use $m = n = 2$ leads and lags.

Results

Three regressions were run to assess the effect of STR restrictions on housing prices (Table 1). The first model looked at the effect of any STR regulation on housing prices, the second model looked at the effect of hard restrictions, and the third model looked at the effect of only requiring registration. The first regression indicates that STR restrictions lead to a 6.136 percentage point increase in HPI, and this result is statistically significant at the 1% level. Looking at the second column of Table 1, we can see that enacting hard restrictions on STRs lead to a 14.84 percentage point increase in HPI, and this result is also statistically significant at the 1% level. Finally, we can see that requiring registration only, and nothing further, leads to a 1.105 percentage point increase in HPI. However, this result is not statistically significant.

To check the robustness of my results, I also decided to perform some analysis on leads and lags. The results for this are shown in Table 2. We can see that the coefficients for our 2- and 1-year lead terms are not statistically significant, and our coefficients for our 1- and 2-year lag

terms get more positive, but once again are not statistically significant. While the lead term coefficients being not statistically significant is good to see, the rest of the coefficients not being statistically significant leads me to believe that my initial differences-in-differences model has possible issues with omitted variable bias.

Discussion

As discussed earlier, there are 3 ways that STR restrictions could affect house prices: through an efficient use effect, a supply effect, and an externality effect. Through an efficient use effect or a supply effect, we would expect an STR restriction to decrease house prices because of the reduced possible earnings from hosting an STR and an increase in long term housing supply due to less existing housing being used as STRs. However, STR restrictions may also increase housing prices because of the reduction of negative externalities associated with STRs. Looking at the results of my initial analysis (Table 1) it could be interpreted that STR restrictions, and harsh ones in particular, are successful at eliminating these negative externalities, leading to an increase in housing prices, and this effect is dominant over any efficient use or supply effects. However, I believe the magnitudes of these estimates, approximately 6 percentage points for any restrictions and 15 percentage points for harsh restrictions, are too large for the externality effect to properly explain. Additionally, this goes against the findings of research that prove that STRs lead to an increase in housing prices, unless STR restrictions are unsuccessful in reducing the number of STRs in an area. These results also go against the findings of Koster et al. (2019), who found that STR restrictions decreased housing prices in Los Angeles County. The result that registration-only requirements have no effect on housing prices, however, does make sense, as registering an STR with the city should not have a large effect on one's ability to operate or make an income from an STR, even if there are registration fees. These costs associated with

registration are small in comparison to the cost of purchasing a property to use as an STR, so only STR operators on the margin should be affected. And since the effect of this on the housing market as a whole would be very small, I would not expect there to be a statistically significant result from my analysis.

Looking at the analysis on any pre- or post-treatment trends (Table 2), there are no statistically significant lead terms or pre-treatment effects, which indicates that our treatment and control groups have similar pre-treatment trends in HPI. However, the lag terms or post treatment effects are not statistically significant. This leads me to believe that something about the cities themselves, rather than STR restrictions, is causing the statistically significant result that STR restrictions lead to higher house prices, rather than the STR restrictions being the cause of the rise is price.

This brings me to the topic of omitted variable bias, which I believe to be an issue in my analysis. In particular, I am concerned that the lack of a control for tourism has led to a bias in my results. Carlino & Saiz (2008) found that tourism is linked to population growth, and population growth increases the demand for housing, which we expect would increase the price of housing. Additionally, cities with high amounts of tourism may have local governments that are more concerned about STRs than in cities with less tourism, leading to local governments in high tourism places being more likely to impose an STR restriction, and in particular more likely to impose harsher STR restriction. With a positive correlation between tourism and housing prices, and a positive correlation between tourism and likeliness to impose an STR restriction, I would then expect to have a positive bias in my results, particularly for the effect of harsher STR restrictions on housing prices. Another source of possible omitted variable bias could be that cities with higher housing prices may have local governments that want to decrease the price of

housing (and in particular, increase the supply of affordable housing). And since STRs are often properties that would be used for more affordable long-term living otherwise, cities may impose STR restrictions (and in particular harsh ones) in an attempt to increase the supply of affordable housing. So, with a positive correlation between housing prices and a desire to increase the supply of affordable housing, and a positive correlation between likeliness to impose an STR restriction and a desire to increase the supply of affordable housing, this could be a source of positive bias in my analysis, particularly once again for the effect of harsher STR restrictions on housing prices. I am less concerned with omitted variable bias effecting my estimate of the effect of registration-only requirements on housing prices however, as motivations for enacting these kinds of regulation may be different than motivations for enacting harsher restrictions. For example, cities may want to make sure STRs are following building codes and habitability requirements or ensure that STRs are being properly taxed, but don't necessarily want to reduce the amount of STRs to increase the supply of affordable housing.

As for policy implications, I am confident in saying that cities are able to impose registration requirements for STRs without having an impact on home prices. So, a if a city wants to better regulate the STR market and make sure that STRs are fulfilling any construction or maintenance standards set by the city, requiring registration could help achieve this without adversely affecting the property values for existing homeowners. In addition, registration requirements could lead to a better experience for those using the STRs, since it could be confirmed that health and safety requirements are being met. But, if cities are specifically aiming to reduce housing prices by implementing STR restrictions, they should explore options beyond registration. Finally, with regards to harsher STR restrictions, I am not confident enough in my results to make any policy recommendations.

Conclusion

This study aims to estimate the effect of STR regulations on home prices in large cities within the United States. Using a differences-in-differences approach with variable treatment timing, I find that imposing an STR restriction leads to a 6.136 percentage point increase in HPI. Looking deeper into types of restrictions, I find that registration-only restrictions have no statistically significant effect on HPI, while harsher restrictions, in this case requiring the STR to be the host's primary residence or requiring the host to be present during the rental period, leads to a 14.84 percentage point increase in HPI. These results could indicate that STR restrictions, particularly harder ones, are successfully reducing negative externalities associated with STRs, leading to an increase in property values. However, analysis of leads and lags of the effect of STR restrictions on housing prices, as well as the magnitudes of the estimates leads me to believe that this analysis suffers from omitted variable bias with regards to tourism and city governments' reasons for imposing STR restrictions, particularly harsh ones. However, I am confident in my results with regards to registration-only requirements, as motivations for enacting these restrictions can be different than motivations for harsher requirements.

For future research, it would be useful to include a control for tourism to avoid the omitted variable bias that this study encountered. Finally, it would be useful to study the effect of STR regulations on long-term rents, as the pricing mechanisms for renting housing is different than the pricing mechanisms for purchasing housing, and STRs are often properties that would be part of the long-term rental market otherwise.

References

Barron, K., Kung, E., & Proserpio, D. (2020). The effect of home-sharing on house prices and rents: Evidence from Airbnb. *Marketing Science*.

Carlino, G. A., & Saiz, A. (2008). Beautiful city: Leisure amenities and urban growth.

Garcia-López, M. À., Jofre-Monseny, J., Martínez-Mazza, R., & Segú, M. (2020). Do short-term rental platforms affect housing markets? Evidence from Airbnb in Barcelona. *Journal of Urban Economics*, 119, 103278.

Greenberg, Z. (2018, July 18). New York City Looks to Crack Down on Airbnb Amid Housing Crisis. Retrieved December 08, 2020, from https://www.nytimes.com/2018/07/18/nyregion/new-york-city-airbnb-crackdown.html?searchResultPosition=4

Hilber, C. A., & Schöni, O. (2020). On the economic impacts of constraining second home investments. *Journal of Urban Economics*, 103266.

Ihlanfeldt, K. R. (2007). The effect of land use regulation on housing and land prices. *Journal of Urban Economics*, 61(3), 420-435.

Koster, H., van Ommeren, J., & Volkhausen, N. (2018). Short-term rentals and the housing market: Quasi-experimental evidence from Airbnb in Los Angeles.

Lieber, R. (2015, October 09). New Worry for Home Buyers: A Party House Next Door. Retrieved December 08, 2020, from https://www.nytimes.com/2015/10/10/your-money/new-worry-for-home-buyers-a-party-house-next-door.html

Turner, M. A., Haughwout, A., & Van Der Klaauw, W. (2014). Land use regulation and welfare. *Econometrica*, 82(4), 1341-1403.

Figure 1: Housing Price Index Over Time for Treatment and Control Cities

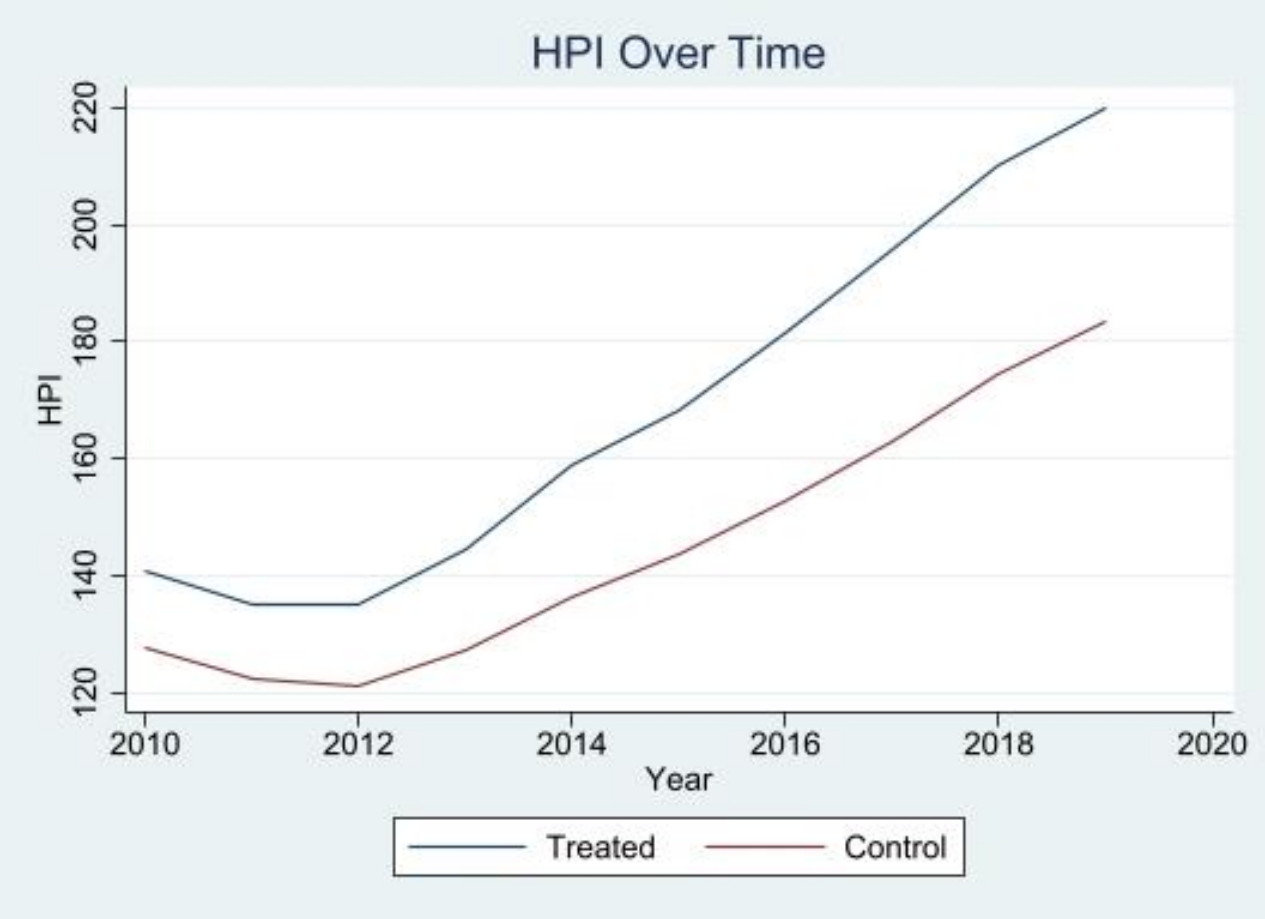

Figure 2: Average HPI vs. Event Time for Treatment Group Cities

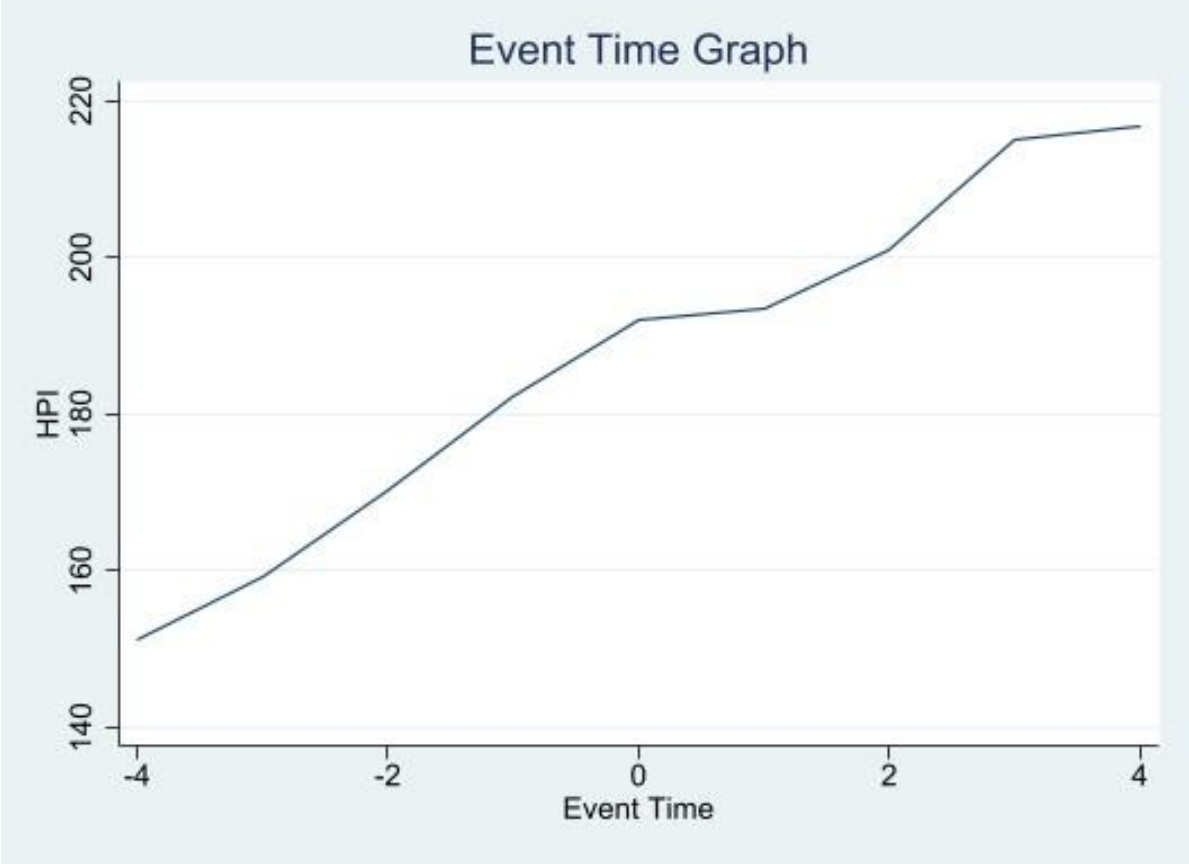

Figure 3: Cities With Active STR Restrictions Over Time

Figure 4: Types of STR Restrictions Implemented by Cities Over Time

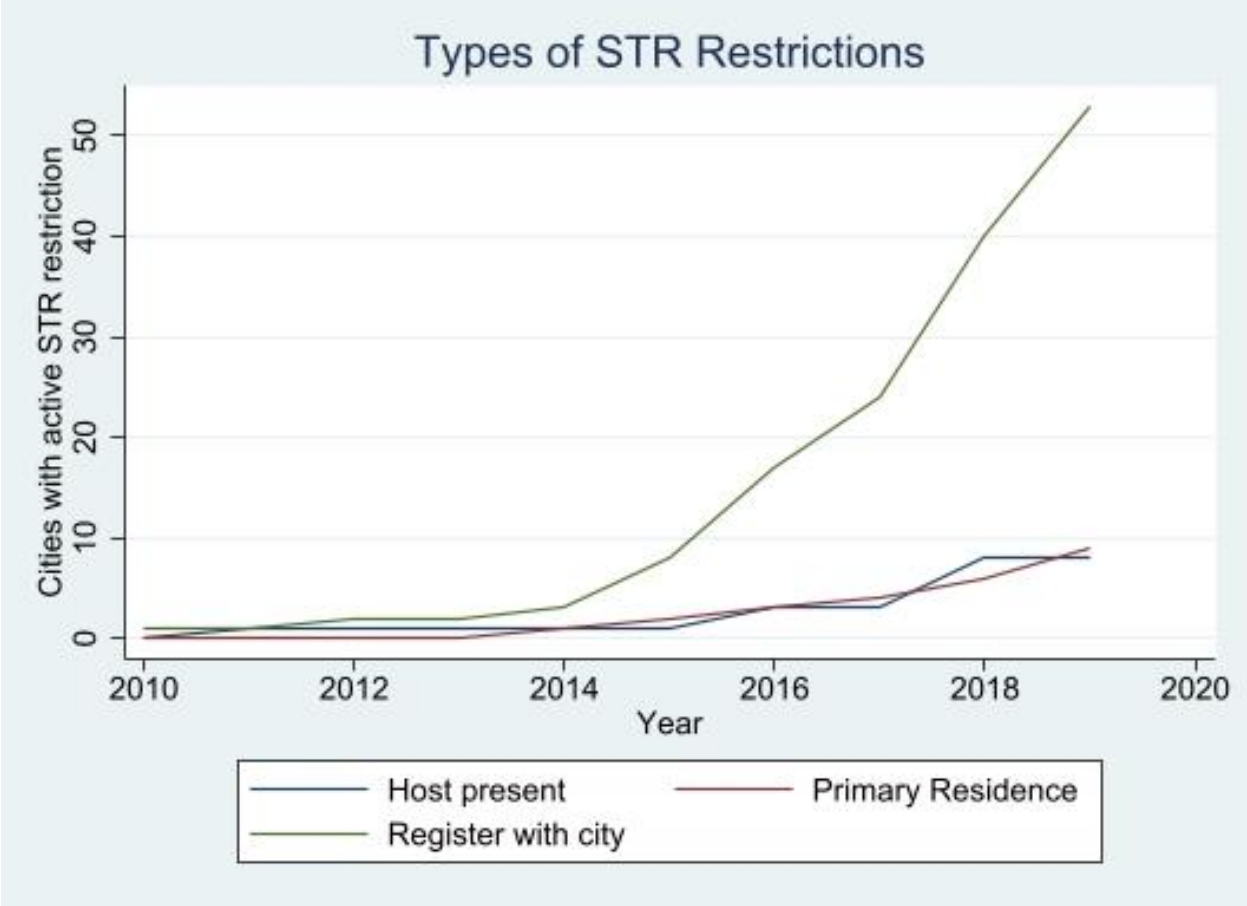

Table 1: Effects of STR Regulations on HPI

VARIABLES	(1) Any Restrictions	(2) Hard Restrictions	(3) Registration Only
STR Restriction	6.136***		
	(1.944)		
Hard Restriction		14.84***	
		(4.042)	
Registration Only			1.105
			(2.249)
Constant	69.67***	69.49***	69.17***
	(6.653)	(6.727)	(6.827)
Observations	1,959	1,959	1,959
R-squared	0.878	0.879	0.877
Year FE	YES	YES	YES
City FE	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Leading an Lagging Effects Analysis

VARIABLES	(1) city hpi2000
Restriction $_{t-2}$	-0.186
	(1.780)
Restriction $_{t-1}$	1.146
	(2.180)
Restriction $_{t}$	1.775
	(2.496)
Restriction $_{t+1}$	2.869
	(2.790)
Restriction $_{t+2}$	5.761
	(3.742)
Constant	69.34***
	(6.779)
Observations	1,959
R-squared	0.877
Year FE	YES
City FE	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1