Better Tax Enforcement Moderates Airbnb's Pressure on Housing Costs

Jesse A. Ellis
North Carolina State University Poole College of Management
jaellis5@ncsu.edu

David G. Kenchington
Arizona State University W.P. Carey School of Business
David.Kenchington@asu.edu

Jared D. Smith
North Carolina State University Poole College of Management
jared_smith@ncsu.edu

Roger M. White
Arizona State University W.P. Carey School of Business
roger.white@asu.edu (Corresponding author)

Abstract

The growing popularity of home-sharing platforms such as Airbnb, partly fueled by hosts' ability to evade local taxes and regulations, has been shown to elevate housing costs by reallocating long-term housing units to the short-term rental market. This study assesses whether enhanced tax enforcement can mitigate this trend. We analyze staggered tax collection agreements between Airbnb and Florida counties, wherein Airbnb collects taxes from the hosts directly. Using a difference-in-differences methodology, we find these agreements significantly slow the growth of housing costs, highlighting the importance of tax policy in addressing the sharing economy's influence on housing affordability.

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I. Introduction

In this paper, we examine whether better tax enforcement moderates Airbnb's upward pressure on housing costs. House sharing platforms such as Airbnb allow a homeowner to rent out a room or house for a short period. Because these platforms lower the cost of entry in the short-term rental market, their presence should lead to increased home values as homeowners can derive cash flows from otherwise idle assets. In addition, they raise the opportunity cost of participating in the long-term rental market, which could lead to increased rents. Recent research supports these predictions (e.g., Barron, Kung, and Proserpio (2021), Garcia-López, Jofre-Monseny, Martinez-Mazza, and Segu (2020), Horn and Merante (2017), and Sheppard and Udell (2016)), confirming a narrative told in the popular press (e.g., Edwards (2016), Glink and Tamkin (2016), and van der Zee (2016)).

The magnitude of this effect can be considerable. Garcia-López et al. (2020), for example, observe that in Barcelona neighborhoods where Airbnb is popular, Airbnb boosts rents by 7% on average and home prices by 17%. Valentin (2021) similarly documents that a complete restriction on short-term rentals in New Orleans resulted in home values in touristy areas dropping by as a much as 30%. Duso et al. (2021) predict that after controlling for neighborhood and apartment characteristics, one additional Airbnb listing within 250 meters of an apartment predicts rent being higher by about 0.7%. Given this pattern of results, there is significant concern that home-sharing platforms are increasing housing costs and potentially reducing housing affordability.

The sharing economy has grown exponentially over the past decade, and partially fueling this growth has been the ability of many sharing economy participants to avoid compliance costs that burden conventional competitors (e.g., Kaplan and Nadler (2015), Migai, de Jong, and Owens (2018), Oei and Ring (2015), and Zervas, Proserpio, and Byers (2017)). In the home-sharing market, hosts on platforms like Airbnb can legally avoid much of the regulatory burden faced by competing hotels, such as the differential requirements for fire safety inspections between Airbnb properties and hotels. Although rentals on Airbnb are typically subject to the same short-term rental sales tax requirements as hotels, Airbnb providers often circumvent these taxes, as local governments struggle to fully monitor market activity and ensure compliance. For example, Bibler, Teltser, and Tremblay (2021) examine 100 of the largest Airbnb markets in the U.S. and estimate that only about 24% of listings voluntarily comply with local sales tax regulations.

In most areas, complying with local sales tax regulations requires Airbnb hosts to maintain a business tax certificate with local authorities, as well as calculate and remit taxes on a regular basis. Facing few consequences for avoiding this costly and time-consuming compliance process, many Airbnb providers evade these local taxes. To stem this tax evasion and generate revenue, some local jurisdictions have recently entered into tax enforcement agreements with sharing economy platforms, like Airbnb, wherein the platform collects and remits local taxes, as opposed to relying on individual providers/hosts to do so. Mechanically, this improves Airbnb providers' tax compliance from about 24% to 100%. Wilking (2020) and Bibler et al. (2021) document that the increased costs (due to paying taxes) in this setting are borne in part by both customers (paying higher after-tax prices) and providers (offering lower pre-tax prices). Accordingly, we expect that these tax enforcement agreements will make Airbnb a less appealing venue for property owners by making hosting via Airbnb less profitable as a function of mandatory tax compliance.

We examine whether this negative shock to the profitability of Airbnb listings reduces local housing costs. While hosts could potentially migrate to alternative home-sharing platforms without tax enforcement agreements (e.g., VRBO, HomeAway, FlipKey) [1], such platform-hopping entails switching costs, introducing non-price barriers that may deter migration between digital platforms

[1] In very recent years, these Airbnb competitors have established tax enforcement agreements with a handful of Florida counties, but none are as heavily regulated as Airbnb.

(Knittel (1997), Strombom, Buchmueller, and Feldstein (2002), and Tucker (2019)). A notable switching cost in our setting is the loss of reputational capital because host ratings do not transfer to new platforms. Perhaps more importantly, Airbnb's dominance as the leading home-sharing platform creates a mutually reinforcing expectation between hosts and renters. Hosts anticipate renters to search there, and renters anticipate hosts to list there. Simply put, switching from Airbnb to less popular platforms likely results in fewer bookings and less pricing power, thereby reducing short-term rental income.

If tax enforcement agreements diminish Airbnb's appeal by reducing providers' profits and if providers face difficulties in switching platforms, we hypothesize that this negative shock to Airbnb listing profitability could lead to a reduction in local housing costs. For instance, a decline in Airbnb profitability may prompt property owners to transition from short-term Airbnb rentals to long-term residential leases. This shift could increase the availability of long-term rental properties and reduce long-term rental prices. Moreover, the decrease in Airbnb profitability can reduce long-term rental prices without necessarily increasing the supply of long-term rentals. The high costs associated with switching to alternative platforms, along with the difficulty of replicating Airbnb's short-term rental income, could lead to a reduction in property values. Since rents are a function of property value, this reduction could consequently lower equilibrium prices in the long-term rental market.

We test whether tax agreements lessen upward pressure on housing costs using a sample of agreements between Airbnb and Florida counties, in which Airbnb agrees to collect and remit local sales taxes on behalf of providers. These "voluntary tax enforcement agreements" usually emerge from regulatory conflict between Airbnb and local governments. Airbnb is incentivized to agree to these arrangements to avoid onerous local regulations that restrict Airbnb hosts and discourage customers. Local governments are incentivized to agree to these arrangements due to the significant revenue they generate (Wilking (2020)). In a typical arrangement, while Airbnb agrees to act as tax collector, it does not provide information about the hosts, guests, or properties to protect hosts from other forms of regulation, such as zoning enforcement (e.g., Dineen (2016), Kilbride (2018), Layden (2016), and Zamost et al. (2018)).

We exploit the staggered introduction of these agreements over the 2015 to 2019 window in 41 separate Florida counties to examine rents and home values in a difference-in-differences setup. We observe that, following these agreements, counties that enact them have lower housing costs than those that do not. As housing costs in Florida increase over the course of our sample period (on average), a better interpretation of our result is that Airbnb tax enforcement agreements slow housing cost increases, likely through making Airbnb less attractive for property owners. In terms of magnitude, we document that Airbnb tax enforcement agreements slow the growth in housing costs by between 1.6% and 5.8% in our sample of Florida counties, depending on the housing type and model specification. For example, our difference-in-differences models predict that monthly rent for a three-bedroom unit is lower than otherwise expected by about $26 in the years after a county enacts an Airbnb tax enforcement agreement (2.2% of the mean county-year rent for a three-bedroom unit of $1,160). Moreover, we find that the results vary based on the level of Airbnb activity in the county: The tax agreements impact price to a greater degree in counties in which Airbnb is popular.

Although our difference-in-differences design absorbs systematic differences between treatment and control counties across time, the tax agreements (treatments) are not entered into randomly. Therefore, it is still possible that economic conditions influence both tax sharing agreements and home values. For instance, poor economic conditions could depress tax revenue and rents simultaneously, which could lead to a spurious correlation between Airbnb tax enforcement agreements and lower housing costs. To address this concern, we conduct a spillover analysis in which we examine the effect of a neighboring county's Airbnb tax enforcement agreement on housing costs in the focal county. We find that housing costs in a focal county *positively* correspond to *adjacent* counties implementing Airbnb tax enforcement agreements. That is, when Airbnb becomes less profitable in adjacent (competing) counties, housing costs in the focal county increase, potentially as a function of the focal county becoming more attractive to travelers (as Airbnb listings in adjacent counties are now marginally more expensive after the tax enforcement agreement). This result is consistent with a causal interpretation of our findings, as it is difficult to envision a correlated omitted variable that would positively predict the establishment of Airbnb tax enforcement agreements in the focal county, depress housing costs in a focal county, *and* boost housing costs in adjacent counties.

To strengthen our interpretation and enhance the robustness of our findings, we conduct several additional tests. First, we find that our results are largely unchanged when we use corrections for staggered difference-in-differences designs to address the early-versus-late reference group issue (see, for example, Baker, Larcker, and Wang (2022) for a discussion of this issue). As well, we find no evidence the parallel trends assumption is violated. Second, we implement placebo tests by assigning treatment status to counties arbitrarily, and our actual treatment effects are replicated in only a minimal fraction of these placebo iterations. Finally, we use a set of alternative housing cost measures in place of our baseline rent measure and find similar results. As a final test, we find suggestive evidence that the tax agreements slow the growth of Airbnb. That is, after the agreements go into effect, counties that have collection agreements see fewer Airbnb listings per housing unit than in counties that do not have such agreements.

We expect our result to inform both policy makers and researchers interested in the sharing economy, housing markets, and tax policy across the fields of economics, finance, accounting, and marketing. Specifically, our research corroborates recent studies in marketing and economics suggesting that Airbnb increases housing costs by shifting units from long-term residential use to the short-term rental market (e.g., Barron et al. (2021), Koster, van Ommeren, and Vokhausen (2021), Garcia-Lopez et al. (2020) Bekkerman, Cohen, Kung, Maiden, and Proserpio (2022)).

Unlike most existing research that centers on the impacts of regulatory restrictions that are subject to varying and uncertain levels of enforcement and compliance, our study is distinct in that we focus on the role of tax enforcement agreements that are implemented by Airbnb itself, ensuring 100% compliance. This perspective is particularly relevant for tax researchers, as this finding adds to the growing list of the broader consequences of stricter tax enforcement, such as businesses undertaking less aggressive income tax strategies (Hoopes, Mescall, and Pittman (2012)), businesses obtaining lower cost of debt and equity financing (El Ghoul, Guedhami, and Pittman (2011)), greater commercial lending growth (Gallemore and Jacob 2020), and improved financial statement quality (Hanlon, Hoopes, and Shroff (2014)).

For finance researchers, our study adds to the understanding of how taxes influence asset management and operations. In particular, we contribute to the growing literature on how tax policy affects housing costs (e.g., Best and Kleven (2018), De Simone, Lester, and Markle (2020), Dee (2000), Lutz (2015), Oliviero and Scognamiglio (2019), and Palmon and Smith (1998)), but more generally our results also speak to the ability of tax policy to have real effects on business operations (e.g., Atanassov and Liu (2020), Faccio and Xu (2015), Foley et al. (2007), Graham and Tucker (2006), Marcus, Jacob, and Jacob (2013), and Li et al. (2017)). Most of these studies focus on how taxes influence capital allocation, which is in line with our findings of Airbnb tax enforcement affecting not only housing costs, but also the distribution of residential vs. short-term rental housing in an area via discouraging participation in the short-term rental market.

Finally, our research presents a compelling case to policymakers about the dual benefits of tax enforcement agreements. These agreements not only enhance tax compliance but also help temper the rise in housing costs driven by Airbnb.[2]

In the next sections, we detail the data and tests, present our results, and briefly conclude.

II. Data and Tests

To test whether tax enforcement agreements reduce housing costs, we use a difference-in-differences design focused on Florida. We choose this setting because, unlike most other states, Florida tax policy is almost completely dictated at the state and county level, which allows for a cleaner design than in other states where tax policy is directed at the municipal level. Furthermore, Florida is a major tourist destination popular on Airbnb. Accordingly, we expect that housing cost pressure induced by Airbnb is potentially strong enough in Florida to measurably react to tax enforcement agreements.

Our baseline difference-in-differences model follows, where subscripts *c* and *t* index county and year, respectively:

$$\textit{Housing Costs}_{c,t} = \beta_1 \text{ x } \textit{Airbnb Enforcement Agreement}_{c,t} + \Sigma\ \textit{Controls}_{c,t} \qquad (1)$$

This and all other models are estimated at the county-year level and include county fixed effects and year fixed effects to adjust for time-invariant county-level variation as well as state-wide time trends, respectively. Accordingly, changes in housing costs are identified within-county around the implementation of tax enforcement agreements, as opposed to between-county by

[2] Decreasing housing costs is admittedly not a universally beneficial policy objective, as homeowners and landlords benefit from increasing rents and increasing housing prices. However, given the political and societal attention given to housing affordability in the U.S. in recent years, we expect that policy makers would look favorably on interventions that mitigate housing cost increases spurred by tax evasion.

comparing counties with and without tax enforcement agreements. To address the possibility that the standard errors are correlated within-county over time, we cluster standard errors at the county level.

The independent variable of interest, *Airbnb Enforcement Agreement*, is an indicator variable that equals one when county *c* in year *t* has a tax enforcement agreement in place in which Airbnb collects and remits the county-level tourist taxes on behalf of hosts. We collect data on these agreements through newspaper articles, press releases, and tax compliance guidance provided by Airbnb to hosts. In Table 1 we list the 41 of 67 Florida counties that enact a tax enforcement agreement with Airbnb by 2019. As the first tax enforcement agreements emerge in Florida in December 2015, we begin our sample period in 2012 to allow for a pre-treatment period in our difference-in-differences tests. Our sample of 536 observations is generated from this eight-year sample of 67 counties (8 years x 67 counties = 536 county-years).

Our primary dependent variable is the county-year Fair Market Rent as reported by the United States Department of Housing and Urban Development (HUD).[3] HUD defines Fair Market Rent as the 40th percentile gross rent expense for a standard quality unit and provides this measure for several different unit types of rental units (one-bedroom, two-bedroom, three-bedroom, and four-bedroom). HUD economists calculate these rent measures using housing expense costs collected by the U.S. Census Bureau and more heavily weigh rent costs of recent movers to better approximate current market rent expenses.[4] HUD reports Fair Market Rent by year for counties and metropolitan areas, and we use the county-level data to match with Florida's county-level tax

[3] This measure gives us the best sample coverage, but we explore other housing cost measures from the FHFA and Zillow in subsection IV.C.

[4] These figures are calculated and reported as part of the federal housing subsidy regime to estimate how much housing assistance is appropriate in different areas. For example, public housing assistance recipients with Section 8 vouchers must pay 30% of their income towards their gross rent expense (rent plus utilities paid by the tenant), and then the Section 8 voucher contributes the difference between this portion of the renter's income and the HUD Fair Market Rent for their respective unit.

policy regime (i.e., Airbnb tax enforcement agreements occur at the county level). We follow prior literature in using these HUD Fair Market Rent estimates to proxy for housing costs in a panel setting. This data is, for example, used by O'Keefe (2004) to document that lower housing costs attract welfare recipients and by Saiz (2007) to examine how international migration into U.S. cities contributes to higher housing costs.

We argue that using the HUD Fair Market Rent (i.e., 40^{th} percentile gross rent expense) is a reasonable way to capture changes in rent expense due to increasing tax enforcement against Airbnb. While Airbnb heavily advertises its luxury housing, research suggests many of their offerings are mid-tier and below. For example, Guttentag and Smith (2017) compare the room quality between Airbnb listings and regular hotel rooms and find that Airbnb listings are cleaner and more comfortable than low-cost hotels, similar to mid-range hotels, but worse than expensive hotels. Similarly, Zervas et al. (2017) find that lower-priced hotels and hotels that do not cater to business travelers were the most affected when Airbnb entered the market in Austin, Texas. These results indicate that there is likely overlap between Airbnb listings and the type of units captured by HUD Fair Market Rent, which suggests that profitability shocks to Airbnb could affect housing costs in a way that is reflected in the HUD Fair Market Rent data.

In modelling Fair Market Rent at the county-year level, we control for regional trends in housing costs by including as a control variable *Adjacent County Fair Market Rent*. This control measures the population-weighted county-year average Fair Market Rent of all counties that border the focal county for the respective unit size. For example, in regression specifications estimating the Fair Market Rent for a two-bedroom unit in a focal county, *Adjacent County Fair Market Rent* corresponds to the two-bedroom unit HUD Fair Market Rent in the focal year in counties neighboring the focal county (weighted by adjacent county population).

We also control for a host of county-year demographic and economic measures, including population, average wage, and unemployment rate. We collect population data from U.S. census estimates, average annual wage data from the Quarterly Census of Employment and Wages data series published by the U.S. Bureau of Labor Statistics, and unemployment rate data from the Local Area Unemployment Statistics data series published by the U.S. Bureau of Labor Statistics.

Because our focus is on how Airbnb tax enforcement agreements affect housing costs, we are sensitive to the effects of other elements of tax policy that can influence housing costs. We include property tax millage rate, collected from Florida Department of Revenue records, as prior research establishes that higher property taxes depress real estate values (e.g., Lutz (2015), Oliviero and Scognamiglio (2019), and Palmon and Smith (1998)). Likewise, higher local sales taxes also depress property values (e.g., Man and Bell (1996) and Shon and Chung (2018)), so we similarly collect data on local sales tax rates from the Florida Department of Revenue and include this county-year measure as a control variable.

Finally, we also control for the local county-year level of county tourist development taxes (*Tourist Tax Rate*), which are sales taxes that apply only to short-term rentals like stays in hotels or Airbnb properties. These taxes are enacted by local voters and the associated revenue must be directed to tourist-related expenses (renovating convention centers, improving beaches, funding tourist bureaus, etc.) (Wenner (2020)). More importantly, this is the class of taxes subject to the Airbnb tax enforcement agreements we examine. Almost all consumption taxes in Florida are administered by the state, but county-level tourist taxes are administered at the county level. As these taxes are administered at the county level, the Airbnb tax enforcement agreements for county-level tourist taxes must be established on a county-by-county basis (between Airbnb and the county government). We use this staggered adoption setting, described in Table 1, as the foundation for our difference-in-differences design. Note that as our analysis is at the county-year level, we require an Airbnb tax enforcement agreement to be in effect for at least six months of a year for it to be considered treated (e.g., if a county establishes an agreement in December 2015, we consider 2016 to be the first treated year).

[Table 1 about here]

III. Primary Results

A. Summary Statistics

Table 2 reports summary statistics. Mean monthly *Fair Market Rent* ranges from about $700 (for one-bedroom units) to about $1,350 (for four-bedroom units). The mean *Airbnb Enforcement Agreement* of 0.27 indicates that Airbnb collects and remits taxes to the local government on behalf of hosts for about one-quarter of sample county-years.

[Table 2 about here]

Table 3 reports a correlation matrix that offers the first insight into whether *Airbnb Enforcement Agreement* affects *Fair Market Rent*, but almost no correlation exists in this pooled analysis. However, our regression specifications identify within-county effects in a difference-in-differences framework by using county and year fixed effects, to better establish causality, and we present these regression models in Table 4.

[Table 3 about here]

B. Difference-in-Differences Regression Results

We examine four measures of monthly *Fair Market Rent* in Table 4 (county-year rent estimates for one-bedroom, two-bedroom, three-bedroom, and four-bedroom units), and we estimate three different regressions for each of these four dependent variables: one model only using *Airbnb Enforcement Agreement* and fixed effects (for county and year), one model only using control variables and fixed effects, and one fully specified model using *Airbnb Enforcement Agreement*, control variables, and fixed effects. We present these first models including only the county and year fixed effects along with *Airbnb Enforcement Agreement* in predicting *Fair Market Rent* in columns 1, 4, 7, and 10. All of these specifications see *Airbnb Enforcement Agreement* load with a negative coefficient. This treatment effect is significant (p-value < 0.10) in columns 1, 7 and 10, and nearly so in column 4 (p-value < 0.13). These results suggest that voluntary tax enforcement agreements between Airbnb and local governments correspond to *lower* housing costs, consistent with the prediction that housing costs decrease when Airbnb hosting is less profitable (due to sales tax compliance being more stringently enforced on Airbnb hosts).

[Table 4 about here]

Models in columns 2, 5, 8, and 11 omit the treatment variable *Airbnb Enforcement Agreement* but include all of the county-year control variables we describe in the previous section (population, unemployment rate, average wages, tax rates, average adjacent county rent, etc.) for county-year rent estimates for one-bedroom, two-bedroom, three-bedroom, and four-bedroom units, respectively. We add our treatment effect *Airbnb Enforcement Agreement* to these baseline models in columns 3, 6, 9, and 12 to estimate fully specified models, and we again observe tax enforcement agreements are related to lower rents for all property types in our sample (all p-values < 0.105). In terms of economic significance, this predicted reduction in housing costs ranges from about 2% (for one-bedroom, two-bedroom, and three-bedroom units) to about 5.8% (for four-bedroom units). For example, the -25.933 coefficient on *Airbnb Enforcement Agreement* in column (9) indicates that after a county establishes an Airbnb tax enforcement agreement, rents for the 40th percentile three-bedroom apartment are lower than otherwise expected by about \$25.93 (which is about 2.2% of the $1,159.90 average HUD Fair Market Rent for the 40th percentile three-bedroom unit across our panel).

C. Cross-section of Airbnb Penetration

In our next test, we exploit data on Airbnb penetration to examine heterogeneity in the treatment effects. Intuitively, we would expect the tax agreements to matter more where Airbnb is more popular, similar to how Franco and Santos (2021) find that short-term rental regulations have a greater impact on housing costs in tourism centers with greater short-term rental demand. Hence, we anticipate larger impacts from tax agreements in counties with higher Airbnb usage. To examine this prediction, we model rents as a function of Airbnb tax enforcement agreements and an interaction of these agreements with *Airbnb Share*, defined as the share of housing units allocated for Airbnb use in a given county-year. Data on county-year Airbnb listings come from AirDNA, a leading firm that compiles monthly Airbnb listings.[5]

[Table 5 about here]

In line with the prediction, Table 5 shows that our treatment effects are stronger where Airbnb penetration is higher. For three out of the four columns (1 through 3), the coefficients on the interaction are negative and statistically significant at the 10% level or better. For example, results from Column 1 suggest that the introduction of Airbnb tax enforcement agreements is associated with a monthly decrease of $10.22 in rents for one-bedroom units for each 1% increase in Airbnb share. The fact that the effect intensifies in regions with greater Airbnb popularity bolsters our primary conclusion: tax enforcement reduces Airbnb's economic appeal, thereby alleviating upward pressures on local housing costs.

[5] The AirDNA data begins in late 2014, so we are left with an abbreviated sample in this analysis (n=402) compared to our primary sample period which begins in 2012. Additionally, the unadjusted Airbnb Share variable from AirDNA exhibits considerable skewness and kurtosis indicating the presence of extreme values that could skew our findings. Consequently, we opted to winsorize Airbnb Share at the 1st and 99th percentiles to mitigate the impact of outliers.

D. Spillover Tests using Adjacent County Policy Changes

Tables 4 and 5 support our prediction that better tax enforcement moderates Airbnb's pressure on housing costs. We focus our next test on strengthening the case for a causal relation running from Airbnb tax enforcement agreements to lower housing costs, addressing whether correlated omitted variables present an endogeneity threat. For example, perhaps regional economic malaise predicts both declining housing costs *and* local government aggressiveness in boosting tax revenue. If this is the case, then tax enforcement agreements with Airbnb could emerge as housing costs decline, even though no causal relation connects one to the other.

We address this concern by analyzing spillover effects, or how adjacent counties' Airbnb tax enforcement agreements correspond to housing costs in a focal county. If Airbnb tax enforcement agreements decrease housing costs by curtailing Airbnb profitability, then Airbnb hosts in a focal county would *benefit* from adjacent counties implementing Airbnb tax enforcement agreements. For example, Collier County (Naples, FL) does not have a tax enforcement agreement with Airbnb, but our sample period sees neighboring Lee County (Fort Myers, FL) enact an Airbnb tax enforcement agreement in March 2016. If the Lee County agreement increases after-tax Airbnb prices in Lee County (via more stringent tax compliance), then Airbnb hosts in neighboring counties will benefit from being comparatively more affordable *after* Lee County's Airbnb tax agreement goes into effect. If this reasoning holds, then Collier County housing costs may increase as a result of neighboring Lee County enacting an Airbnb tax enforcement agreement (e.g., Barron et al. (2021), Bibler et al. (2021), Horn and Merante (2017), and Neslin and Shoemaker (1983)).

A key premise underlying our spillover effect hypothesis is that short-term Airbnb renters have greater locational flexibility, facing lower switching costs for relocation compared to long-term residential renters. Given this assumption, a regulatory crackdown on Airbnb in an adjacent county would likely divert short-term renters into the focal county. If long-term renters in the focal county are less able to relocate—perhaps due to job commitments or social ties—this influx would exert upward pressure on long-term housing costs. However, if our initial assumption is incorrect, and it is long-term renters who are more flexible in their locational choices, a neighboring county's Airbnb crackdown could lead to a decrease in long-term housing costs in the focal county. This would occur as units in the adjacent county switch to long-term rentals, possibly enticing some residents from the focal county to move, thereby reducing demand and housing costs. In this scenario, the substitutability in the long-term rental market serves as a countervailing force, making it less likely for us to observe increased housing costs in adjacent counties.[6] Ultimately, the impact of Airbnb tax agreements on long-term housing costs in adjacent counties is an empirical question.

We test our spillover hypothesis by adding a variable labeled *Adjacent County Airbnb Enforcement Agreement* to our models that already control for focal county Airbnb enforcement agreements. We define *Adjacent County Airbnb Enforcement Agreement* as the proportion, per county-year, of neighboring counties that have an Airbnb tax enforcement agreement in place (weighted by neighboring county population). We identify neighboring counties using the NBER county adjacency file,[7] and the resulting model takes the form:

$$\textit{Housing Costs}_{c,t} = \beta_1 \; x \; \textit{Adjacent County Airbnb Enforcement Agreement}_{c,t} + \Sigma \; \textit{Controls}_{c,t} \quad (2)$$

A positive β_1 coefficient would support our spillover prediction and suggest that housing costs in a focal county increase in response to Airbnb prices (after-tax) increasing in neighboring counties, as such a price hike makes Airbnb listings in the focal county more attractive by comparison.

[6] We are grateful to an anonymous reviewer for pointing out this necessary assumption for our prediction.
[7] See https://www.nber.org/research/data/county-adjacency.

We report results from this regression in Table 6, where *Adjacent County Airbnb Enforcement Agreement* is included along with *Airbnb Enforcement Agreement* (for the focal county) as well as the vector of county-year control variables (average wages, tax rates, unemployment, etc.). In all of these models, similar to our primary results in Table 4, *Airbnb Enforcement Agreement* loads with a negative and significant coefficient, suggesting that the establishment of Airbnb tax enforcement agreements corresponds to lower housing costs within a county. Additionally, the new variable, *Adjacent County Airbnb Enforcement Agreement*, loads as predicted with a positive coefficient for all rental unit types (all p-values < 0.10). This indicates that boosting Airbnb hosts' tax compliance, and subsequently Airbnb prices, in a focal county is related to higher housing costs in adjacent counties, in line with our spillover hypothesis that Airbnb listings in these adjacent counties are comparatively more attractive relative to a focal county competitor after said focal county implements an Airbnb tax enforcement agreement.

In terms of effect size, the 27.178 coefficient in column 2 suggests that if all of a county's neighboring counties in one year established Airbnb tax enforcement agreements (shifting the *Adjacent County Airbnb Enforcement Agreement* variable from 0 to 1), then the rent for the 40th percentile two-bedroom unit in the focal county would be expected to shift upwards by about \$27.18.[8] Note that having *all* neighboring counties establish such agreements in the same year is not common, and a more typical example may be a county with three equally populated neighboring counties wherein one of the neighboring counties establishes an Airbnb tax enforcement agreement. In that case, *Adjacent County Airbnb Enforcement Agreement* would

[8] The positive coefficients on *Adjacent County Airbnb Enforcement Agreement* are generally slightly larger in magnitude than the negative coefficients on *Airbnb Enforcement Agreement*. However, F-tests suggest that this difference in magnitude is not statistically different in any of the Table 6 models.

increase by 0.33, which would suggest that the rent for the 40th percentile two-bedroom unit in the focal county would be expected to shift upwards by about $9.06 (0.33 x $27.18 = $9.06).[9]

[Table 6 about here]

Conceptually, this test helps us rule out correlated omitted variable issues by illustrating that Airbnb tax enforcement agreements predict housing costs even in cases where we can abstract from within-county issues. It is difficult to envision an omitted variable that would relate positively to the incidence of focal county Airbnb tax enforcement agreements, negatively to focal county housing costs, *and* positively to adjacent county housing costs. Accordingly, we view this spillover test as providing additional evidence consistent with a causal interpretation of our results.

IV. Robustness Tests

A. Parallel Trends Tests and Staggered Event Corrections

In this section, we explore the robustness of our results. To begin, we examine whether the parallel trends assumption holds. Difference-in-differences models examine how a treatment sample reacts to a treatment *relative* to an untreated control sample. An underlying assumption of the model is that the treatment and control samples are similar in the period prior to treatment (i.e., there would have been no change absent the treatment). We examine this assumption using dynamic difference-in-differences models to test whether the rent for soon-to-be-treated counties diverges from untreated counties in the pre-treatment period. We display the coefficients and confidence intervals from these models in Figures 1 and 2. Treatment effects are plotted relative to the year before Airbnb tax enforcement agreements are implemented (i.e., year *t-1* is the baseline comparison year). In addition to the standard two-way fixed effect (TWFE) estimates we have

[9] The standard deviation of *Adjacent County Airbnb Enforcement Agreement* is 0.34, so this ad hoc example approximates the expected change in housing costs that would accompany a one standard deviation shift in *Adjacent County Airbnb Enforcement Agreement.*

been using to this point, we also show the analogous coefficients from alternative estimation methods for staggered difference-in-differences settings. Recent literature (see, for example, Baker et al. (2022)) finds that the control group used in these settings can change the results, and that early-versus-late treatment can complicate the interpretation of treatment effects. We employ corrections from Cengiz, Dube, Lindner, Zipperer (2019), Callaway and Sant'Anna (2021), and Gardner (2021) to address these concerns in our setting.[10]

Following the recommendations in Baker et al. (2022), we display the univariate results in Figure 1 and the results from the full specification from equation (2) with controls in Figure 2. Panels A-D in both figures display the results separately for one- through four-bedrooms. In short, we see no significant effects in the pre-treatment period, and this is true in the standard TWFE procedure and for the other estimation methods. Rather, treatment effects in treated counties *only* begin to emerge in year t for four-bedroom units (when the tax enforcement agreement begins) or in year t+1 for the others (the year after the tax enforcement agreement begins). Accordingly, the parallel trends assumption does not appear to be violated in our setting.

[Figures 1 and 2 about here]

In addition to this graphical evidence, we also create a summary table of the average treatment effects from the regressions with dynamic treatment effects (note that this is not the same as the treatment effect with a simple pre- and post-period indicator). This summary of the effect sizes and their significances is shown in Table 7, with the coefficients, standard errors (in brackets), and p-values (in parentheses). The coefficient estimates and their statistical significance remain relatively consistent across various methods. All of the estimates with controls and all but two of

[10] Cengiz et al. (2019) use a stacked design, Callaway and Sant'Anna (2021) average treatment effects across treatment cohorts, and Gardner (2021) uses a two-stage regression. For more information about these issues in difference-in-differences estimation, see Baker et al. (2022).

the univariate estimates are statistically significant at the 10% level or better. Overall, the TWFE OLS results align with the results from the alternative estimators with regard to sign, magnitude, significance, and the lack of pre-treatment trends. This consistency suggests that the potential issues with early-versus-late treatment, which can skew inferences in staggered difference-in-differences designs, do not significantly affect our findings.

[Table 7 about here]

B. Placebo Tests

As another robustness check, we conduct a randomization test by using placebo Airbnb tax enforcement agreements. This is potentially important, as a variety of prior work demonstrates that region-specific treatment settings such as ours can at times generate downwardly biased standard errors, even when clustering, that over-reject the null hypothesis of no treatment effect (Bertrand, Duflo, and Mullainathan (2004) and Spamann (2019)). This literature, along with MacKinnon (2019), suggests using randomization inference with placebo treatments to examine whether the observed treatment effect is as rare in randomly generated data as the cluster-robust p-value suggests.

We follow this literature, and in our placebo test we randomly assign Airbnb tax enforcement agreements to counties following the actual pattern in our data, where 27 counties establish tax enforcement agreements beginning in 2016, 12 counties establish tax enforcement agreements beginning in 2017, and two counties establish tax enforcement agreements beginning in 2018. We randomly assign this treatment pattern to counties, subject to the requirement that these placebo treatment counties have a non-zero local tourist tax rate (i.e., the type of tax subject to Airbnb tax enforcement agreements) (e.g., Athey and Imbens (2017) and Heß (2017)). We use this set of placebo treatments to estimate the full models from equation (1) for all four types of rental units (one-bedroom to four-bedroom). We then record the placebo treatment effect (regression coefficient on *Airbnb Enforcement Agreement*). We repeat this process 499 times to generate a distribution of 500 potential treatment effects for each of the four regression specifications (499 placebo treatment effects and one actual treatment effect). We report these distributions in Figure 3 Panels A-D for one- to four-bedroom units.

[Figure 3 about here]

The Fisher p-values reported in these figures report the percentage of treatment effects from the distribution that are as or more extreme than the actual treatment effect generated using the actual data (Fisher (1935), Heß (2017), and White and Webb (2021)). The one-tailed Fisher p-value of 0.048 reported in Figure 1, for example, suggests that of the 500 potential treatment effects generated in the placebo test, only 4.8% are as or more extreme than the -15.868 treatment effect observed in the actual data. That is, a randomly assigned set of placebo Airbnb tax enforcement agreements corresponds to rent in one-bedroom units falling by $15.87 per month (or more) only 4.8% of the time. Importantly, the Fisher p-values in each of Figure 3 Panels A-D are less than 0.05, suggesting that treatment effects as large as those we observe in the actual data are unlikely to be the result of random chance (as randomly assigned placebo Airbnb tax agreements only very rarely correspond to treatment effects as large as those observed in the actual data).

C. Alternative Housing Cost Measures

Up to this point our housing cost measure has been HUD Fair Market Rent, and next we examine the results using four other measures for both rents and housing prices for single-family homes. We use two measures from Zillow, which employs artificial intelligence and highly granular neighborhood level characteristics to estimate home values. We use the *Zillow Rent Index*

(*ZRI*) as an alternative rental value measure, for which we have data on 54 Florida counties from 2012 to 2017.[11] We also use the *Zillow Home Value Index* (*ZHVI*), which is designed to estimate the typical home value for a particular county-year, reflecting homes in the 35th to 65th percentiles of value. We have ZHVI data for our entire sample period (2012-2019).[12] Our next measure is the Federal Housing Finance Agency's *House Price Index* (*HPI*), which is constructed using different valuations of the same properties over different years to estimate county-year changes in average single-family home prices.[13] Lastly, we also use data on real *Median Sales Prices* for single-family homes in Florida counties from the University of Florida's Shimberg Center for Housing Studies.

We use the same control variables here as in our previous analysis, including the adjacent county average tax agreement dummy, as well as year and county fixed effects. As with estimating rent costs, we control for regional property price trends by including an adjacent county control that measures the population-weighted, adjacent county-year average of each measure, using all counties that border the focal county. For brevity, the coefficients on the controls are not shown.

Results are displayed in Table 8, where the dependent variable is Zillow rental prices (ZRI) in column 1, Zillow home values (ZHVI) in column 2, FHFA home values in column 3 (HPI), and home selling prices in column 4. Overall, these results comport with our main results, in that our

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[11] We obtained this data from Kaggle, an online platform that hosts a wide range of datasets. This particular ZRI dataset is no longer available directly from Zillow, as Zillow changed their methodology for computing their publicly available rental price index in 2019. Zillow's current publicly available rental price index is the Zillow Observed Rent Index (ZORI) which is only available starting in 2015 for a smaller subset of the Florida counties in our study (ranging from 35-38 through time). Thus, the ZORI measure does not provide adequate coverage for our study given the timing of the tax enforcement agreements. However, the ZRI sample from Kaggle provides adequate time series and cross sectional coverage such that we can appropriately compare the pre- and post- treatment effects for the Florida counties that engaged in tax agreements with Airbnb.

[12] Monroe county is missing ZHVI data for years 2012-2014, which is why we end up with n=533, compared to our full sample where n=536.

[13] The HPI is detailed in a series of studies by FHFA economists (Bogin, Doerner, and Larson (2019a), (2019b), and (2019c)), and it is used as a proxy for home prices in a number of panel studies in the recent literature (e.g., Monras (2020) and Berger, Turner, and Zwick (2020)). The original HPI measure has a value of 100 for the baseline year, but baseline years vary by county, as data collection starts earlier in some counties than others. To correct for this inconsistency, we adjust all our HPI measures by the county-level HPI in 2011, the year before our sample period begins.

coefficient of interest, *Airbnb Enforcement Agreement*, is negative and significant at the 5% level or better in the regressions. Moreover, the economic magnitudes are similar to those we found using our main measure. For example, The -6,990.399 coefficient on *Airbnb Enforcement Agreement* in column 4 suggests that home prices are lower than expected by about 3.5% (compared to the mean of $198,256) after a county establishes a tax enforcement agreement with Airbnb, which is comparable in magnitude to the treatment effects we document in estimating HUD Fair Market Rent. Across all columns, the economic magnitudes suggest effect sizes of 2.6% to 4.5%. We view this evidence as further confirmation that stronger tax enforcement reduces the price pressure that home-sharing exerts on local housing markets.

[Table 8 about here]

V. What's Driving the Change in Housing Costs?

There are several potential mechanisms that could be causing the change in housing costs we document. First, the tax agreements may represent a shock to after-tax cash flows from short-term rental opportunities. If we consider the value of a property as partly determined by the present value of these after-tax cash flows, the tax agreements represent a negative shock to value. Second, the increased tax burdens might lead to reduced investment in Airbnb, which could decrease property quality and, by extension, neighborhood desirability and property values (Bekkerman et al. (2022) find reduced investment along the extensive margin, which might be happening along the intensive margin as well). Additionally, the value changes may stem from externalities represented by Airbnb activity, such as changes in a neighborhood's bundle of amenities (e.g., Almagro and Domíguez-Iino (2024)). Finally, the enforcement agreements might prompt marginal property owners to offer their homes to long-term residents (through sale or long-term lease) instead of listing on Airbnb, as stricter sales tax compliance on Airbnb listings makes using the platform less appealing (Wilking 2020 and Bibler et al. (2021)). Thus, Airbnb tax enforcement agreements may lower long-term housing costs in part because they decrease Airbnb listing activity and increase the relative supply of long-term housing. Importantly, these mechanisms are not mutually exclusive and may jointly contribute to the observed changes in housing costs.

While fully exploring these mechanisms is beyond the scope of the paper, in this section, we test whether tax enforcement agreements reduce Airbnb activity, using the Airbnb listing data from AirDNA in the form of the county-year *Airbnb Share* variable. We estimate the relation between tax agreements and share using the same model used in the main analysis, with the same controls and fixed effects (except that the adjacent county housing cost variable is replaced with adjacent share). Table 9 shows the results of this estimation.

[Table 9 about here]

As in Table 4, we show results with only the variable of interest (column 1), only the controls (column 2), and then full models with and without *Adjacent County Airbnb Agreement* (columns 3 and 4). We see that the coefficient estimate is -0.003 in all three relevant columns, suggesting a stable relation between the tax agreements and share, regardless of the controls. However, there are large standard errors in the model without controls, and the coefficient is not statistically significant. Adding controls in columns 3 and 4 shrink the standard errors, and with the added statistical precision, the coefficient has some significance (one-tailed p-value = 0.07).

We find an even larger and more precisely estimated effect size when we examine the treatment effects over time. Figure 4 displays results of the dynamic version of this analysis mirroring the approach in Figures 1 and 2, where we present the time-varying coefficients and confidence intervals for both the standard two-way fixed effects OLS model and the three alternative difference-in-differences methods addressing staggered treatment concerns. Panel A displays the univariate results and Panel B displays results with the full set of controls. Across the models, we see no significant pre-treatment differences in groups. The coefficient estimates post treatment are negative and increasing in magnitude, and are statistically significant in two of the four models with controls in $t+1$ and in all four models in $t+3$ (both with and without controls).

[Figure 4 about here]

To summarize the results of the dynamic treatment effects, we display the average treatment effects for each of the four models (without and with controls) in Table 10. In that table, the point estimates vary between -0.004 and -0.007, and we see that seven out of the eight treatment effects are significant at the 10% level or better. Thus, the corrected models and dynamic averages show a bit larger effect size and more statistical significance than the static effect displayed in Table 9. On the whole, it appears that tax agreements slow Airbnb uptake in treated counties, and so our results are consistent with Wilking (2020) and Bibler et al. (2021), who find that Airbnb tax enforcement agreements predict fewer Airbnb listings in a locality.

[Table 10 about here]

To get a sense for the economic magnitude of the estimated treatment effect, we note that the average Airbnb share for treated counties was 0.7%. If we take the average treatment effect across all of our dynamic models, a -0.44% reduction in share, this represents a roughly 39% reduction in Airbnb share from the counterfactual average of the treated counties had they not been treated (defined as the treated county average minus the estimated treatment effect). The estimated economic magnitude ranges from 30% to 50% across the eight dynamic models.

These economic magnitudes are in line with existing estimates in the literature. For example, Koster et al. (2021) estimate that home-sharing ordinances introduced in Los Angeles County reduced Airbnb share between 50% and 70%, and Farronato and Fradkin (2022), theorize that tax enforcement measures would lead to a 22% reduction in Airbnb supply on average.

Moreover, our effect size estimates imply price elasticities that are consistent with those found in the prior literature. For example, Koster et al. (2021) estimate that reducing Airbnb share by 1 percentage point reduces rents by 4.9%, implying a ratio of change in supply to change in price of -1%/-4.9%=0.2. In comparison, we find tax agreements reduce three-bedroom rental prices by 2.2%, which, when combined with the average share effect size estimate across all four difference-in-differences models implies a price elasticity of -0.44%/-2.2%=0.2.[14]

Admittedly, while the findings in Figure 4 and Table 10 suggest that Airbnb tax agreements lead to a reduction in Airbnb listing activity, this does not necessarily indicate an increase in the long-term housing stock. It could be that in response to the tax agreements, homeowners might have transitioned their listings to other short-term rental platforms, such as HomeAway or VRBO, which may not have had similar tax enforcement agreements during the period under study. Unfortunately, we lack the data from other platforms to validate this hypothesis. However, it stands to reason that if most hosts leaving Airbnb simply went to a different platform without suffering any significant switching costs or short-term rental income shocks, it does not seem likely that we would observe a reduction in prices as we do in the rest of the analysis.

As well, we find some suggestive evidence that long-term occupancy increases around these tax agreements in the treated counties. The U.S. Census Bureau considers a unit to be occupied if it houses an owner or a long-term resident who claims the unit as their primary residence. Conversely, units primarily utilized for short-term rentals through platforms like Airbnb are classified as vacant, even if they frequently host short-term renters. Thus, we can examine the relative shift in long-term housing by examining changes in the occupancy rates around the tax

[14] Taking the full range of coefficient estimates from Tables 9 and 10, this elasticity lies somewhere between 0.14 and 0.32. Moreover, when we restrict our analysis to only those counties with above-median Airbnb activity we find that the price effects and elasticities remain consistent with the full-sample results. This suggests the findings are robust across different levels of Airbnb penetration and are not overly sensitive to those levels.

agreements. We use the same methodology that we use with the share tests, except for swapping in long-term occupancy.

In Figure 5, we present the results of our analysis, which point to a notable uptick in long-term occupancy rates—averaging 0.8%—following the implementation of tax collection agreements. The consistent increase in coefficients in the post-treatment phase is suggestive of a shift from short-term to long-term housing after the implementation of tax agreements. Admittedly, there is considerable noise in these estimates, as evidenced by the sizeable error bars. Nonetheless, combined with all the foregoing results on price and Airbnb share, the results in the figure point to a supply effect consistent with our overall story that tax agreements make Airbnb less appealing by reducing after-tax cash flows.

[Figure 5 about here]

Although we have good evidence that suggests prices declined around the tax shock to Airbnb, our evidence about changes in supply is a bit more speculative. As such, future research could do more to disentangle the underlying mechanisms causing the fall in housing costs around these tax agreements.

VI. Conclusion

The popular press has speculated for years that Airbnb and other home-sharing platforms contribute to higher housing costs for residents (e.g., Edwards (2016), Glink and Tamkin (2016), and van der Zee (2016)). This relation is intuitive, as many rental units listed on Airbnb would, absent home-sharing platforms, potentially enter the residential housing market and subsequently reduce demand (and prices) for residential units. Recent research confirms this prediction. Barron et al. (2021), for example, use a large sample of U.S. cities to document that home prices and rents increase with Airbnb growth in a zip code, and that Airbnb growth also corresponds to fewer units being made available for long-term residential use.

An Airbnb listing's profitability is what prompts property owners to shift units from long-term residential use to short-term rental use on Airbnb. Historically, at least some of this profitability comes from Airbnb hosts being able to easily evade compliance costs like sales taxes. Bibler et al. (2021), in their analysis of 100 U.S. metro areas, document that only about 24% of Airbnb listings comply with local taxes. Given this lax compliance, these authors also examine Airbnb's voluntary tax enforcement agreements with local governments, which shift the onus of tax compliance (i.e., collecting and remitting taxes) from individual Airbnb hosts to the platform itself, guaranteeing 100% tax compliance. Bibler et al. (2021), along with Wilking (2020), document that these agreements raise costs for renters and hosts, lessen the number of Airbnb nights booked, and reduce the number of properties listed on Airbnb.

Our research builds on this foundation and examines whether Airbnb tax enforcement agreements can limit the upward pressure that Airbnb exerts on housing costs. We find that after counties establish Airbnb tax enforcement agreements, housing costs are about 1.6% to 5.8% lower than otherwise predicted in the following years as measured by both rents and single-family home prices. In addition, we find a larger decrease in housing costs in counties with greater Airbnb penetration. Moreover, spillover analysis in adjacent counties reveals that tax agreements can inadvertently increase housing costs in nearby areas, as properties become more attractive for Airbnb listings due to enforced tax compliance elsewhere. Lastly, the rate of Airbnb penetration slows post-tax agreement.

The impact of these tax agreements appears sizable in total: Using Census data on numbers of dwellings and Zillow pricing data, we estimate that the aggregate decline in property value in the treated counties is perhaps about $20 billion. From the perspective of a policymaker, this reduction in values may be an intentional trade-off to mitigate externalities associated with short-term rentals, such as reduced housing affordability and strain on local infrastructure, and community disruption. Despite the loss in property tax revenue, the broader societal benefits—including removing the form of regulatory arbitrage hosts enjoyed relative to traditional hotels—could justify the changes to housing policy. These considerations highlight the importance of viewing the tax enforcement agreements as not just a fiscal tool but a policy instrument with broader social implications.

Broadly, our findings contribute to the sharing economy literature by confirming that market distortions in sharing economy products are somewhat driven by a form of regulatory arbitrage, namely sharing economy participants' evasion of regulatory compliance costs, and that stricter compliance enforcement can limit these market distortions (e.g., Kaplan and Nadler (2015), Migai et al. (2018), Oei and Ring (2015)). More directly, our results suggest that policymakers concerned with Airbnb driving up housing costs could look to tax enforcement agreements for relief, as our results provide evidence that such agreements correspond to lower housing costs. We note, however, that these policy decisions should be made with an eye to overall tax revenues. While an agreement will drive tax compliance to 100% and increase the revenue from tourist taxes, it is also associated with lower home values and thus may shrink revenue from property taxes. The overall fiscal effect will vary with Airbnb's popularity in an area (how many listings, average listing price, how often listings are booked) as well as county tax structure. Future research on the broader impact of tax enforcement agreements on revenue could aid policymakers. Regardless, our findings underscore the potential societal benefits of these agreements, particularly for communities grappling with the effects of increasing housing costs.

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Appendix 1: Variable Definitions

Fair Market Rent: Rent plus tenant-paid utilities for the 40th percentile standard rental housing unit per county-year, reported by the U.S. Department of Housing and Urban Development (HUD). Drawn from U.S. Census Bureau data on housing costs. Calculated with more weight placed on rents paid by recent movers (to better reflect current market prices).

Adjacent County Fair Market Rent: County-year mean of adjacent counties' HUD Fair Market Rent, weighted by the population of adjacent counties. County adjacency is determined using the NBER county adjacency files.

Airbnb Enforcement Agreement: County-year indicator for whether the county-level tourist tax is collected and remitted automatically by Airbnb (for Airbnb stays), or whether individual Airbnb hosts must calculate, collect, and remit the county-level tourist tax on their own.

Adjacent County Airbnb Enforcement Agreement: County-year proportion of adjacent counties with an Airbnb tax enforcement agreement in place, weighted by the population of adjacent counties. County adjacency is determined using the NBER county adjacency files.

Local Option Sales Tax: County-level sales tax rate, measured at the county-year level. Collected from Florida Department of Revenue data.

Tourist Tax Rate: County-level tourist development tax rate, measured at the county-year level. Collected from Florida Department of Revenue data.

Property Tax Rate (mills): County-year level property tax rate, measured in mills. Collected from Florida Department of Revenue data.

Population (10k): County-year level population (in 10,000s). Collected from U.S. Census Bureau data.

Average Annual Wage ($10k): County-year mean of per-worker annual wage (in $10,000s). Measured using the Quarterly Census of Employment and Wages data series published by the U.S. Bureau of Labor Statistics.

Unemployment Rate: County-year unemployment rate. Measured using the Local Area Unemployment Statistics data series published by the U.S. Bureau of Labor Statistics.

Zillow Rent Index (ZRI): County-year measure of the median estimated monthly market rental price for all homes and apartments in a county, calculated monthly by Zillow using proprietary statistical and machine learning models.

Zillow Home Value Index (ZHVI): County-year median estimated home value, computed by Zillow using a proprietary statistical model to track changes in local real estate markets.

House Price Index (HPI): FHFA's county-year index measuring changes in single family house prices using repeat-sales data, where the same property is sold or otherwise re-assessed in different years (sold, refinanced, etc.). Normalized to county-level HPI in 2011 value (year before sample period begins).

Median Sales Price: County-year median transaction price for homes in Florida counties adjusted for inflation. Data comes from the Shimberg Center for Housing Studies at the University of Florida.

Airbnb Share: The proportion of housing units allocated for Airbnb use in a given county-year. Data on county-year Airbnb listings come from AirDNA and the sample period ranges from 2014-2019. We winsorize Airbnb Share at the 1st and 99th percentiles.

Figure 1: Dynamic Difference-in-Difference Estimation for Monthly Rent (No Controls)

This figure reports treatment effect estimates from univariate dynamic difference-in-difference models of monthly Fair Market Rent around the onset of an Airbnb enforcement agreement in year t. The 90% confidence interval around the point estimates is also reported (standard errors are clustered by county). In addition to the standard two-way fixed effects estimation (TWFE OLS), this figure also reports alternative estimators that correct for staggered treatment timing from Callaway and Sant'Anna (2021), Gardner (2021), and Cengiz et al. (2019). Panels A through D display results separately for between one- and four-bedroom units.

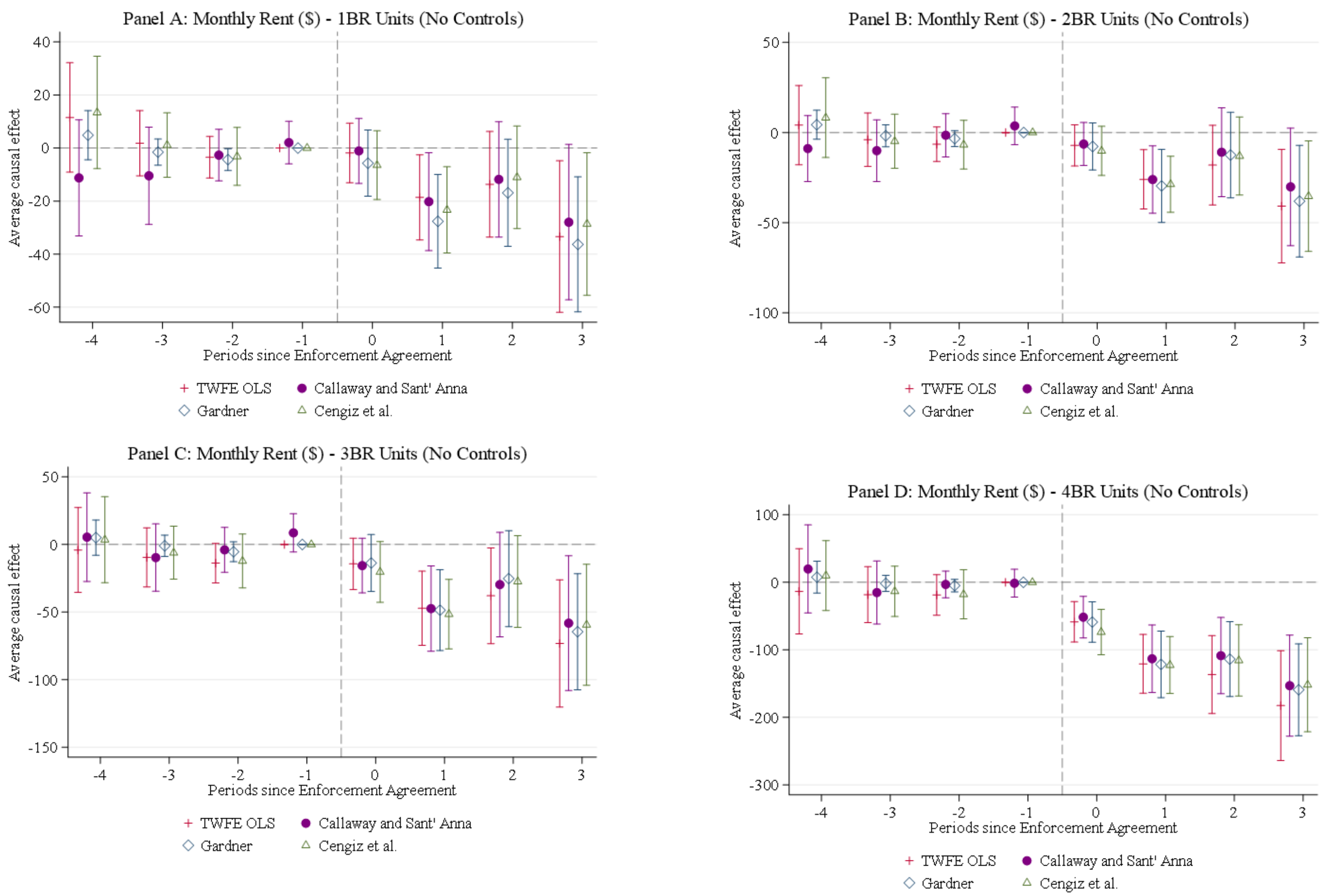

Figure 2: Dynamic Difference-in-Differences Estimation for Monthly Rent (With Controls)

This figure reports treatment effect estimates from dynamic difference-in-difference models of monthly Fair Market Rent around the onset of an Airbnb enforcement agreement in year *t*. The models include the same controls as in Table 6. The 90% confidence interval around the point estimates is also reported (standard errors are clustered by county). In addition to the standard two-way fixed effects estimation (TWFE OLS), this figure also reports alternative estimators that correct for staggered treatment timing from Callaway and Sant' Anna (2021), Gardner (2021), and Cengiz et al. (2019). Panels A through D display results separately for between one- and four-bedroom units.

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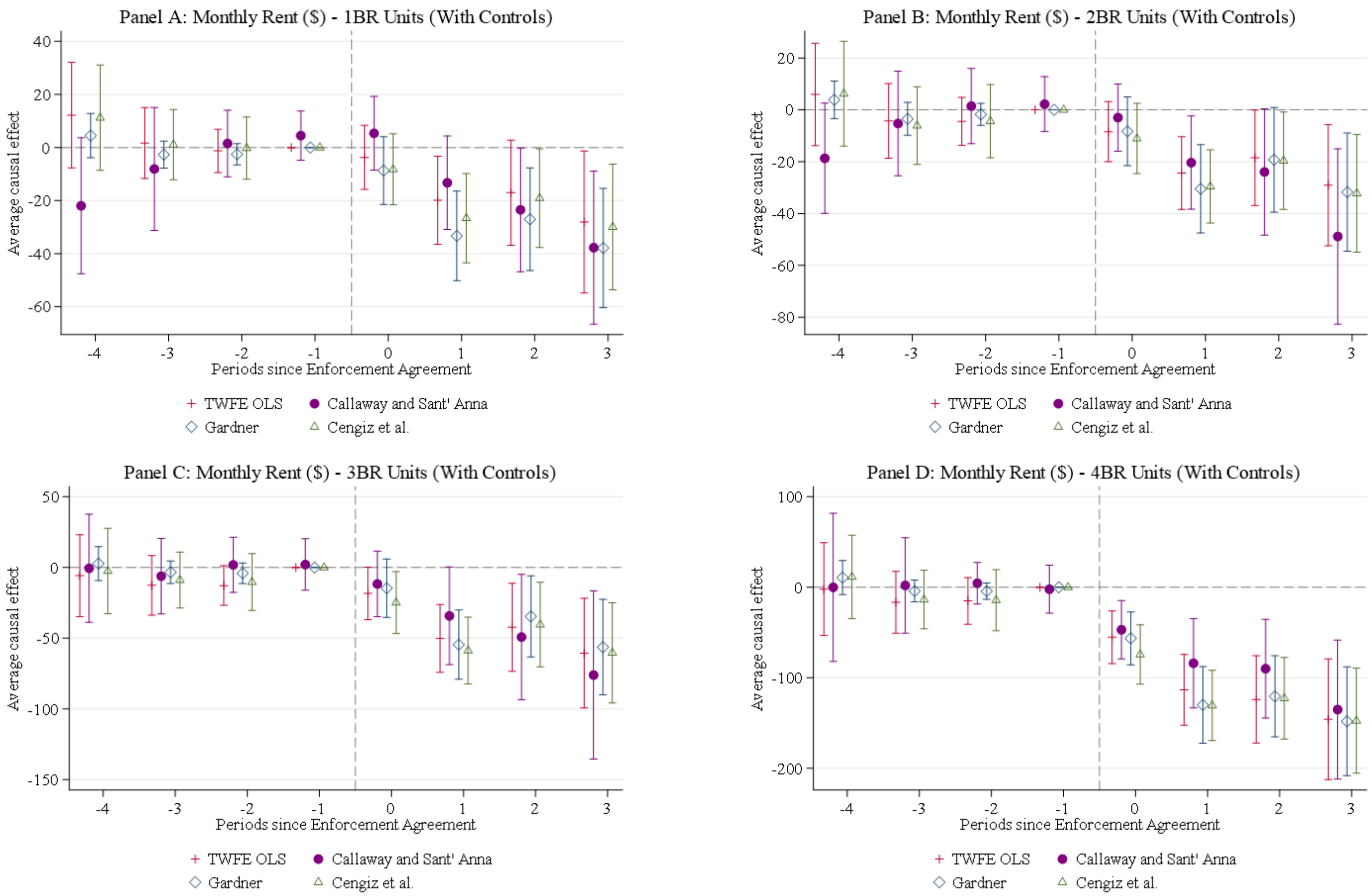
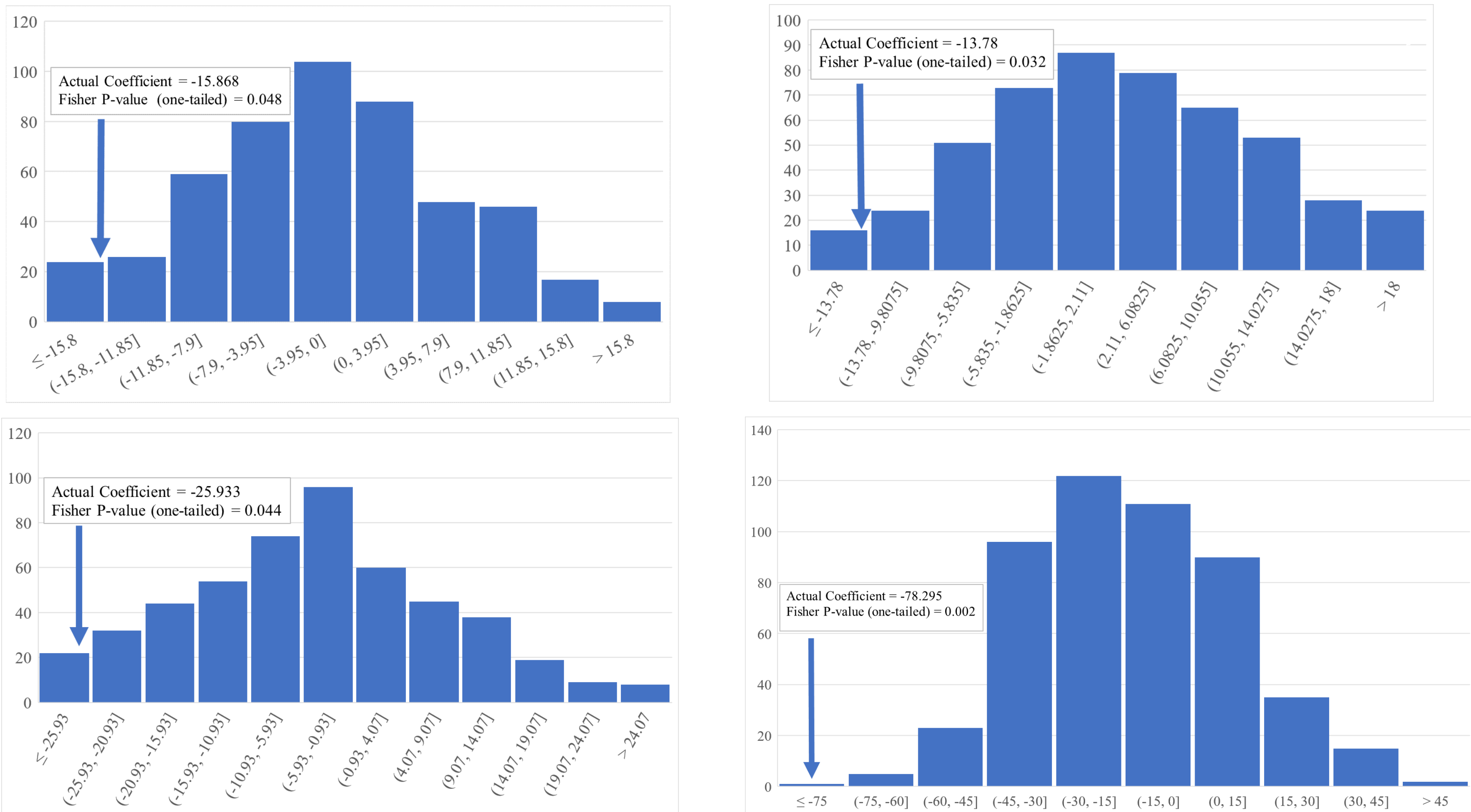


Figure 3: Distribution of Placebo Treatment Effects for *Airbnb Enforcement Agreement* in Estimating Fair Market Rent

This figure reports the distribution of 500 potential treatment effects for *Airbnb Enforcement Agreement* as generated from equation (1) (the model using all of the control variables, fixed effects, etc.). Panels A-D display results for one to four bedrooms. For each graph, one treatment effect reflects the actual data and 499 are generated from placebo Airbnb tax enforcement agreements. The Fisher p-value reports how many of these placebo treatment effects are as or more extreme than our observed treatment effect in the actual data.

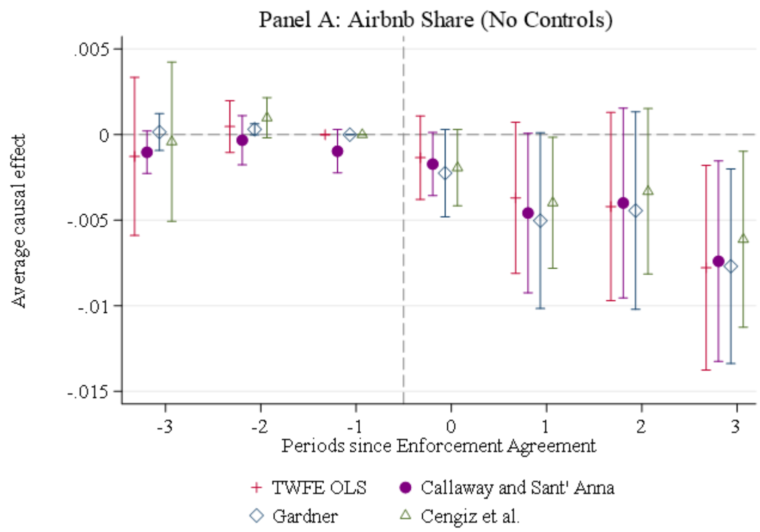


Panel A: 1 Bedroom Units Panel B: 2 Bedroom Units

Panel C: 3 Bedroom Units Panel D: 4 Bedroom Units

Figure 4: Dynamic Difference-in-Difference Estimation for Airbnb Share

This figure reports treatment effects from dynamic difference-in-differences regressions that estimate the trend in Airbnb Share (Airbnb units divided by total housing units per county-year) around the onset of an Airbnb enforcement agreement in year *t* (year *t-1* is the omitted base level equal to 0). The 90% confidence interval around the point estimates is also reported. Panel A reports the univariate estimates, and Panel B reports estimates from the models with the same controls as in model 4 of Table 9. In addition to the standard two-way fixed effects estimation (TWFE OLS), this figure also reports estimates and confidence intervals estimated using staggered difference-in-difference corrections from Callaway and Sant'Anna (2021), Gardner (2021), and Cengiz et al. (2019). Note, we only estimate back to t-3 because Airbnb share data begins in 2014 and we only have 2 counties that are treated after 2017.



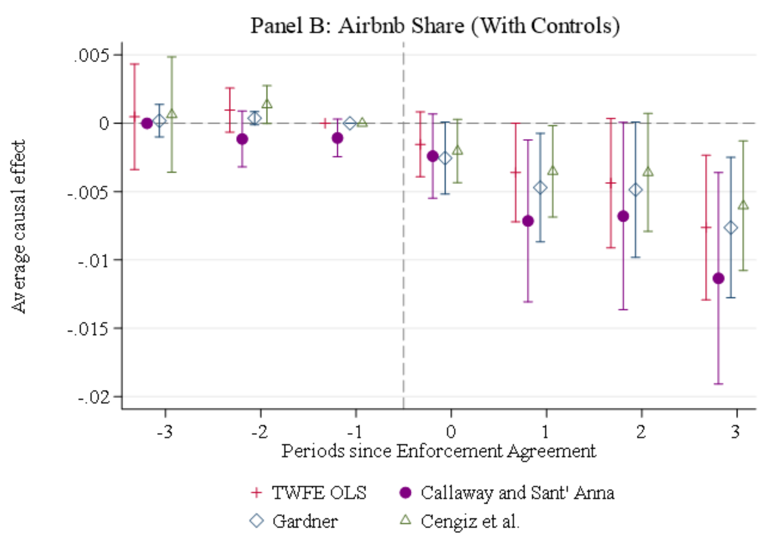
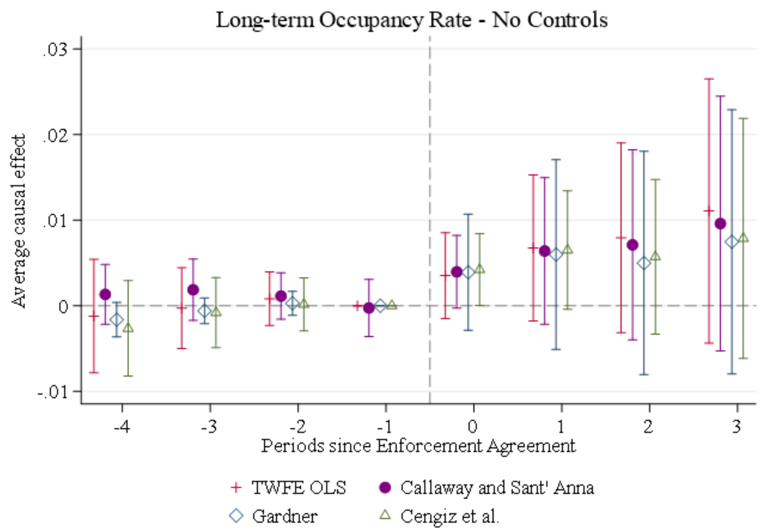


Figure 5: Dynamic Difference-in-Difference Estimation for Long-term Occupancy

This figure reports treatment effects from dynamic difference-in-differences regressions that estimate long-term occupancy (number of units occupied full-time by total housing units per county-year) around the onset of an Airbnb enforcement agreement in year *t* (year *t-1* is the omitted base level equal to 0). The 90% confidence interval around the point estimates is also reported. Panel A reports the univariate estimates, and Panel B reports estimates from the models with similar controls to model 4 of Table 9. In addition to the standard two-way fixed effects estimation (TWFE OLS), this figure also reports estimates and confidence intervals estimated using staggered difference-in-difference corrections from Callaway and Sant'Anna (2021), Gardner (2021), and Cengiz et al. (2019).



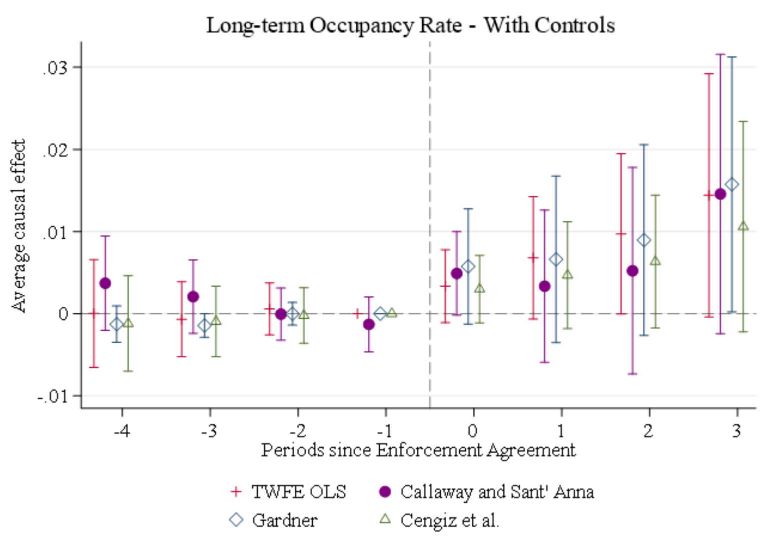


Table 1: List of County-level Airbnb Tax Enforcement Agreements

County	Tax Agreement Effective as of:
Bradford	12/1/2015
Citrus	12/1/2015
Columbia	12/1/2015
Desoto	12/1/2015
Dixie	12/1/2015
Flagler	12/1/2015
Franklin	12/1/2015
Gadsden	12/1/2015
Gilchrist	12/1/2015
Glades	12/1/2015
Hamilton	12/1/2015
Hendry	12/1/2015
Holmes	12/1/2015
Jackson	12/1/2015
Jefferson	12/1/2015
Levy	12/1/2015
Madison	12/1/2015
Okeechobee	12/1/2015
Pasco	12/1/2015
Pinellas	12/1/2015
Sumter	12/1/2015
Wakulla	12/1/2015
Washington	12/1/2015
Brevard	3/15/2016
Lee	3/15/2016
Orange	3/15/2016
Hernando	5/1/2016
Putnam	7/1/2016
Taylor	7/1/2016
Hardee	2/1/2017
Hillsborough	2/1/2017
Okaloosa	2/1/2017
Broward	5/1/2017
Indian River	5/1/2017
Miami-Dade	5/1/2017
Polk	5/1/2017
Santa Rosa	5/1/2017
Sarasota	5/1/2017
Leon	7/1/2017
Highlands	4/1/2018
Charlotte	5/1/2018

Table 2: Summary Statistics

County-year level measures for Florida counties from 2012 to 2019.

	N	Mean	Std Dev	Minimum	1st Quartile	Median	3rd Quartile	Maximum
Monthly Fair Market Rent for 1BR	536	704	146	463	586	706	787	1,266
Monthly Fair Market Rent for 2BR	536	863	190	584	694	862	960	1,682
Monthly Fair Market Rent for 3BR	536	1,160	257	698	963	1,167	1,283	2,157
Monthly Fair Market Rent for 4BR	536	1,348	345	718	1,059	1,353	1,580	2,761
Airbnb Enforcement Agreement	536	0.272	0.446	0	0	0	1	1
Adjacent County Airbnb Enforcement	536	0.241	0.337	0	0	0	0.486	1
Local Option Sales Tax	536	0.892	0.438	0	1	1	1	2.5
Tourist Tax Rate	536	3.596	1.516	0	3	4	5	6
Property Tax Rate (mills)	536	14.433	2.473	6.382	12.915	14.557	16.182	20.112
Population (10k)	536	30.447	48.023	0.824	2.724	11.954	34.381	271.694
Average Annual Wage ($10k)	536	3.816	0.602	2.77	3.366	3.707	4.148	5.662
Unemployment Rate	536	5.681	1.995	2.1	4.05	5.3	7	12.8
Zillow Rent Index	324	1290	334	748	1112	1205	1367	3242
Zillow Home Value Index	533	168,280	83,951	53,005	106,833	151,584	208,117	658,378
House Price Index	510	1.219	0.263	0.829	1.004	1.149	1.378	2.102
Median Sales Price	536	198,526	91,003	69,182	134,139	178,995	236,923	636,543
Airbnb Share	402	0.006	0.013	0	0	0.001	0.006	0.079

Table 3: Pearson Correlations

County-year level measures for Florida counties from 2012 to 2019.

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
(1)	Monthly Fair Market Rent for 1BR	1.00																
(2)	Monthly Fair Market Rent for 2BR	0.98	1.00															
(3)	Monthly Fair Market Rent for 3BR	0.96	0.98	1.00														
(4)	Monthly Fair Market Rent for 4BR	0.94	0.95	0.96	1.00													
(5)	House Price Index	0.56	0.58	0.60	0.60	1.00												
(6)	Zillow Home Value Index	0.74	0.77	0.77	0.75	0.62	1.00											
(7)	Zillow Rent Index	0.69	0.75	0.74	0.63	0.49	0.93	1.00										
(8)	Airbnb Enforcement Agreement	-0.04	-0.02	-0.02	-0.02	0.33	0.01	-0.04	1.00									
(9)	Adjacent County Airbnb Enforcement	0.23	0.22	0.24	0.24	0.57	0.25	0.10	0.53	1.00								
(10)	Local Option Sales Tax	-0.24	-0.23	-0.26	-0.27	-0.08	-0.22	-0.05	0.11	0.20	1.00							
(11)	Tourist Tax Rate	0.54	0.54	0.55	0.56	0.43	0.44	0.30	0.05	0.08	-0.34	1.00						
(12)	Property Tax Rate (mills)	-0.52	-0.52	-0.53	-0.49	-0.37	-0.71	-0.58	-0.01	-0.17	0.17	-0.39	1.00					
(13)	Population (10k)	0.59	0.60	0.60	0.60	0.36	0.36	0.31	0.02	0.07	-0.24	0.38	-0.21	1.00				
(14)	Average Annual Wage ($10k)	0.64	0.65	0.66	0.66	0.60	0.55	0.41	0.14	0.29	-0.24	0.57	-0.34	0.73	1.00			
(15)	Unemployment Rate	-0.30	-0.31	-0.33	-0.36	-0.66	-0.49	-0.44	-0.43	-0.57	-0.11	-0.20	0.32	-0.10	-0.40	1.00		
(16)	Airbnb Share	0.32	0.30	0.30	0.31	0.49	0.24	0.08	0.02	0.30	0.01	0.33	-0.26	0.16	0.24	-0.30	1.00	
(17)	Long-term Occupancy Rate	0.07	0.05	0.04	0.13	0.00	-0.21	-0.35	0.09	0.01	-0.06	0.09	0.32	0.26	0.22	-0.04	-0.04	1.00

Table 4: Airbnb Enforcement Agreements and Rent

This table reports county-year regressions estimating the county-year HUD Fair Market Rent as a function of whether the county has a tax enforcement agreement in place with Airbnb. All variables are measured at the county-year level, and fixed effects are included for county and year (2012-2019). Subscripts *c* and *t* index county and year, respectively. Standard errors are clustered at the county level and reported in brackets beneath coefficients. Two-tailed statistical significance at the $p < 0.01$ level, $p < 0.05$ level, and $p < 0.10$ level are represented by ***, **, and *, respectively.

Fair Market $Rent_{c,t}$ (monthly in \$) = β_1 x AirBnB Enforcement $Agreement_{c,t}$ + Σ $Controls_{c,t}$

	1	2	3	4	5	6	7	8	9	10	11	12
	Rent for 1 BR	Rent for 1 BR	Rent for 1 BR	Rent for 2 BR	Rent for 2 BR	Rent for 2 BR	Rent for 3 BR	Rent for 3 BR	Rent for 3 BR	Rent for 4 BR	Rent for 4 BR	Rent for 4 BR
AirBnB Enforcement Agreement	-16.267*		-15.868*	-16.479		-13.78	-28.450*		-25.933**	-92.475***		-78.295***
	[8.827]		[8.628]	[10.575]		[8.304]	[15.818]		[12.743]	[25.206]		[20.958]
Local Option Sales Tax		19.612*	15.511		29.881**	26.334**		23.014	16.334		-0.389	-20.106
		[11.219]	[10.218]		[13.649]	[12.965]		[18.574]	[17.722]		[32.831]	[33.260]
Tourist Tax Rate		1.238	2.339		3.897	4.812		10.665	12.397		8.479	13.627
		[6.993]	[6.916]		[8.163]	[8.025]		[8.297]	[8.697]		[10.417]	[10.315]
Property Tax Rate (mills)		3.257	2.977		3.973	3.759		8.966	8.588*		9.853	8.904
		[3.202]	[3.133]		[3.460]	[3.351]		[5.497]	[5.053]		[7.884]	[6.945]
Population (10k)		4.959***	5.096***		7.393***	7.511***		8.597***	8.838***		8.281*	9.205**
		[1.796]	[1.734]		[2.120]	[2.039]		[2.935]	[2.763]		[4.723]	[4.125]
Average Annual Wage (\$10k)		-28.084	-31.772		-40.449	-43.579		18.743	13.084		18.489	3.801
		[34.934]	[34.686]		[34.234]	[34.304]		[53.449]	[51.943]		[86.416]	[79.941]
Unemployment Rate		5.894	3.602		9.366	7.413		7.672	4.025		39.742**	28.558*
		[7.564]	[7.728]		[8.956]	[8.747]		[11.158]	[10.708]		[17.391]	[16.671]
Adjacent County Fair Market Rent		0.474***	0.486***		0.423***	0.427***		0.430***	0.432***		0.562***	0.546***
		[0.104]	[0.106]		[0.097]	[0.096]		[0.099]	[0.100]		[0.125]	[0.112]
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	536	536	536	536	536	536	536	536	536	536	536	536
R^2	0.9359	0.9458	0.9465	0.9559	0.9661	0.9664	0.9471	0.9574	0.9581	0.9294	0.9425	0.9458

Table 5: Airbnb Enforcement Agreements, Airbnb Share, and Housing Costs

This table reports county-year regressions estimating the county-year rents as a function of county-level Airbnb tax enforcement agreement and local Airbnb share. Column 1 reports results for one-bedroom rentals, Column 2 for two-bedroom, and so on. All variables are measured at the county-year level, and fixed effects are included for county and year (2014-2019 due to availability of Airbnb share). The same control variables from Table 4 are included but suppressed for brevity. Subscripts *c* and *t* index county and year, respectively. Standard errors are clustered at the county level and reported in brackets beneath coefficients. Two-tailed statistical significance at the $p < 0.01$ level, $p < 0.05$ level, and $p < 0.10$ level are represented by ***, **, and *, respectively.

$$\text{Housing Costs}_{c,t} = \beta_1 \text{ x Airbnb Enforcement Agreement}_{c,t} \text{ x Airbnb Share}_{c,t} + \Sigma \text{ Controls}_{c,t}$$

	1	2	3	4
	Rent for 1BR	Rent for 2BR	Rent for 3BR	Rent for 4BR
Airbnb Enforcement Agreement	-2.320	2.263	-3.385	-62.233***
	[8.404]	[6.853]	[11.866]	[22.525]
Airbnb Share	431.983	833.980**	1,543.698**	-1,083.897
	[330.453]	[397.952]	[585.700]	[698.832]
Airbnb Enforcement Agreement x Airbnb Share	-1,022.334*	-1,327.825*	-2,211.000**	245.041
	[582.744]	[679.615]	[989.990]	[1,175.556]
Other Controls Included but Suppressed	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	402	402	402	402
R^2	0.9668	0.9801	0.9733	0.9618

Table 6: Adjacent County Airbnb Enforcement Agreements and Fair Market Rent

This table reports county-year regressions estimating the county-year HUD Fair Market Rent as a function of whether adjacent counties have tax enforcement agreement in place with Airbnb. All variables are measured at the county-year level, and fixed effects are included for county and year (2012-2019). The same control variables from Table 4 are included but suppressed for brevity. Subscripts *c* and *t* index county and year, respectively. Standard errors are clustered at the county level and reported in brackets beneath coefficients. Two-tailed statistical significance at the $p < 0.01$ level, $p < 0.05$ level, and $p < 0.10$ level are represented by ***, **, and *, respectively.

Fair Market Rent$_{c,t}$ (monthly in \$) = β_1 x Adjacent County AirBnB Enforcement Agreement$_{c,t}$ + Σ Controls$_{c,t}$				
	1	2	3	4
	Rent for 1BR	Rent for 2BR	Rent for 3BR	Rent for 4BR
Adjacent County AirBnB Enforcement Agreement	23.985*	27.178**	38.570**	54.677*
	[13.148]	[13.149]	[18.243]	[29.790]
AirBnB Enforcement Agreement	-18.073**	-16.329*	-29.604**	-83.318***
	[8.377]	[8.293]	[13.268]	[20.612]
Other Controls Included but Suppressed	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	536	536	536	536
R^2	0.9473	0.967	0.9587	0.9465

Table 7: Average Treatment Effects for Monthly Rent

This table reports average treatment effects from the various dynamic difference-in-differences models displayed in Figures 2 and 3 (standard two-way fixed effects (TWFE OLS), Callway and Sant'Anna (2021), Gardner (2021), and Cengiz et al. (2019)). Panel A (B) displays results from the regressions without (with) controls. Treatment effect coefficients are averaged from the year the Airbnb Enforcement agreement was enacted (year t) through year t+3. Standard errors are clustered at the county level and reported in brackets beneath coefficients. Two-tailed statistical significance at the p < 0.01 level, p < 0.05 level, and p < 0.10 level are represented by ***, **, and *, respectively.

Panel A: No Controls

	TWFE OLS	Callaway and Sant'Anna	Gardner	Cengiz
Rent for 1BR	-16.881*	-15.272	-21.649**	-17.354*
	[9.948]	[10.716]	[9.729]	[9.365]
Rent for 2BR	-23.013**	-18.379	-21.991*	-21.808**
	[10.858]	[11.694]	[11.730]	[10.059]
Rent for 3BR	-43.181**	-37.737**	-38.021**	-39.661**
	[17.533]	[18.982]	[17.258]	[16.195]
Rent for 4BR	-124.702***	-106.636***	-113.363***	-115.955***
	[29.182]	[28.897]	[27.414]	[26.227]

Panel B: With Controls

	TWFE OLS	Callaway and Sant'Anna	Gardner	Cengiz
Rent for 1BR	-17.167*	-17.286*	-26.713***	-20.957**
	[10.125]	[9.580]	[9.402]	[9.502]
Rent for 2BR	-20.091**	-24.045**	-22.432**	-23.105***
	[8.838]	[10.533]	[9.703]	[8.816]
Rent for 3BR	-42.829***	-42.775**	-40.034***	-46.053***
	[15.361]	[20.375]	[14.408]	[14.649]
Rent for 4BR	-109.591***	-89.024***	-113.799***	-118.792***
	[24.737]	[27.597]	[23.277]	[22.903]

Table 8: Alternative Housing Cost Measures

This table reports results from county-year regressions using alternative housing cost measures as the dependent variable. The dependent variable in column 1 is the Zillow Rent Index (ZRI), which is available until 2017. The dependent variable in column 2 is the Zillow Home Value Index Index (ZHVI), which is available until 2019. The dependent variable in column 3 is the FHFA House Price Index (HPI). The dependent variable in column 4 is the median home sale price for homes in that county from the Shimberg Center for Housing Studies at the University of Florida. All models include the full set of controls used in Table 6, but the results are suppressed for brevity. All variables are measured at the county-year level, and fixed effects are included for county and year (2012-2019). Subscripts *c* and *t* index county and year, respectively. Standard errors are clustered at the county level and reported in brackets beneath coefficients. Two-tailed statistical significance at the $p < 0.01$ level, $p < 0.05$ level, and $p < 0.10$ level are represented by ***, **, and *, respectively.

	1	2	3	4
	ZRI	ZHVI	HPI	Median Sale Price
Airbnb Enforcement Agreement	-34.027***	-7,489.133***	-0.041**	-6,990.399**
	[12.628]	[2,467.512]	[0.019]	[3,205.592]
Other Controls Included but Suppressed	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	324	533	510	536
R-squared	0.9748	0.9919	0.9632	0.9816

Table 9: Airbnb Enforcement Agreements and Airbnb Share

This table reports county-year regressions estimating Airbnb Share, the county-year share of housing units listed on Airbnb (as of December 1st of the given year) winsorized at the 1% and 99% levels. All variables are measured at the county-year level, and fixed effects are included for county and year (2014-2019). Subscripts *c* and *t* index county and year, respectively. Standard errors are clustered at the county level and reported in brackets beneath coefficients. Two-tailed statistical significance at the $p < 0.01$ level, $p < 0.05$ level, and $p < 0.10$ level are represented by ***, **, and *, respectively. One-tailed statistical significance at the $p < 0.10$ level is represented by †.

$$\text{Airbnb Share}_{c,t} = \beta_1 \times \text{Airbnb Enforcement Agreement}_{c,t} + \Sigma\ \text{Controls}_{c,t}$$

	1	2	3	4
Airbnb Enforcement Agreement	-0.003		-0.003†	-0.003†
	[0.003]		[0.002]	[0.002]
Adjacent County Airbnb Enforcement Agreement				0.002
				[0.003]
Local Option Sales Tax		0.002	0.001	0.001
		[0.005]	[0.004]	[0.004]
Tourist Tax Rate		-0.002	-0.002	-0.002
		[0.001]	[0.001]	[0.001]
Property Tax Rate (mills)		0.001*	0.001**	0.001**
		[0.000]	[0.000]	[0.000]
Population (10k)		0.002*	0.002**	0.002**
		[0.001]	[0.001]	[0.001]
Average Annual Wage ($10k)		-0.008	-0.009	-0.009
		[0.008]	[0.008]	[0.008]
Unemployment Rate		0.001	0.000	0.001
		[0.002]	[0.001]	[0.002]
Adjacent County Airbnb Share		-0.105	-0.105	-0.098
		[0.091]	[0.091]	[0.096]
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	402	402	402	402
R^2	0.7363	0.7630	0.7670	0.7677

Table 10: Average Treatment Effects for Airbnb Share

This table reports average treatment effects from the various dynamic difference-in-differences models displayed in Figure 4 (two-way fixed effects estimation (TWFE OLS), Callway and Sant'Anna (2021), Gardner (2021), Cengiz et al. (2019)). Panel A (B) displayed the results without controls (with controls). Treatment effect coefficients are averaged from the year the Airbnb Enforcement agreement was enacted (year t) through year t+3. Standard errors are clustered at the county level and reported in brackets beneath coefficients. Two-tailed statistical significance at the $p < 0.01$ level, $p < 0.05$ level, and $p < 0.10$ level are represented by ***, **, and *, respectively.

Panel A: No Controls

	TWFE OLS	Callaway and Sant' Anna	Gardner	Cengiz
Airbnb Share	-0.004	-0.004*	-0.005*	-0.004*
	[0.003]	[0.003]	[0.003]	[0.002]

Panel B: With Controls

	TWFE OLS	Callaway and Sant' Anna	Gardner	Cengiz
Airbnb Share	-0.004*	-0.007**	-0.005**	-0.004*
	[0.002]	[0.003]	[0.002]	[0.002]