# Essays in Applied Microeconomics

by

Brietta Russell

A dissertation accepted and approved in partial fulfillment of the

requirements for the degree of

Doctor of Philosophy

in Economics

Dissertation Committee:

Keaton Miller, Chair

Edward Rubin, Core Member

Mark Colas, Core Member

Rebecca Lewis, Institutional Representative

University of Oregon

Winter 2026

DISSERTATION ABSTRACT

Brietta Russell

Doctor of Philosophy in Economics

Title: Essays in Applied Microeconomics

This dissertation consists of four chapters. Chapter I provides a summary of all other chapters in the dissertation. Chapter II and III are related to short-term rentals, and Chapter IV is related to violence and banking in Mexico.

Chapter II examines the relationship between short-term rental demand and supply. Using Covid-19 state containment policies as an exogenous shock to demand, I find that cities with stricter containment policies experienced a larger reduction in travel demand, relative to cities with lower restrictions. I do not find evidence that this shift in demand systematically impacted Airbnb supply.

Chapter III analyzes the effect of short-term rental regulatory policy in Denver on Airbnb supply. I find evidence that the primary-residence condition implemented effectively reduced Airbnb supply, especially among hosts with more than two properties. However, the enforcement mechanism implemented does not sufficiently reduce non-compliant properties from participating in the market. I find evidence that the policy reduced home prices and monthly rents, particularly in areas with high short-term rental demand.

Chapter IV, co-authored with Alfredo Burlando, Lucia Dalla Pellegrina, and Josephine Kim, provides evidence regarding the spillover effects of violence on financial outcomes in Mexico. Following the 2007 spike in drug-related homicides, we find that banks contract their

overall operations. This reduces financial access in areas that are not directly impacted by violence but are serviced by banking institutions that also operate in violent areas.

This dissertation includes previously unpublished co-authored material.

# ACKNOWLEDGMENTS

I am grateful to the members of my dissertation committee for providing support and valuable insight throughout this process. I am especially thankful for my advisor, Keaton Miller, who helped push my ideas forward, kept me positive, and provided guidance and confidence during every sprint. I extend my thanks to Edward Rubin, Mark Colas, and Rebecca Lewis for their ideas and insights.

I extend my thanks to my fellow graduate students who have, at many times, pulled me through the challenges of the program. I extend special thanks to Eric Wilken, Jose Rojas-Fallas, and Dante Yasui, who have played a crucial role in this process. This journey would have been near impossible without their humor and camaraderie.

Finally, I am grateful for the University of Oregon Law School for the Consumer Protection Grant, which funded the data used in my short-term rental research.

# DEDICATION

I dedicate this work to the people who have been there for me through the highs and lows of this journey. To my parents, who have been my biggest cheerleaders. Thank you for giving me the confidence and courage to take on challenges, and for encouraging me along every step.

To my brother and my grandparents, for their excitement and support. To my Grandpa Ron, who passed during the program, whose determination and playfulness reminds me there is always an opportunity for fun. To my Grandma Chris who passed during my high school years, who instilled in me a curiosity and excitement for education, and whose kindness shaped my perspective as a teacher.

Finally, I dedicate this to the friends who have been patient with me throughout the past five years. To Kyron, thank you for making me laugh, reminding me to pause, and for keeping me rooted in adventure.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

# CHAPTER I

## INTRODUCTION

This dissertation consists of four chapters. Chapter I provides a summary of all other chapters in the dissertation. Chapter II and III are related to short-term rentals, and Chapter IV is related to violence and banking in Mexico.

Chapter I investigates the relationship between short-term rental supply and demand. During the height of the Covid-19 pandemic, state governments implemented various policies to limit social interactions and contain the virus. These policies varied in stringency and longevity, with many states continuing social restrictions as domestic travel reopened. I use the policy variation as an exogenous shock to travel demand, where we might expect that travelers prefer destinations with lower Covid restrictions. A change in demand for travel could impact the demand for overnight accommodation, resulting in a long-run supply response. I estimate the effect of a restriction-induced shift in travel demand on Airbnb supply decisions. I first observe that states with a higher Covid stringency index experience a significant decrease in travel demand. A one standard deviation increase in the stringency index is associated with a 6.0% reduction in airline destination passengers, relative to states at the average stringency level. The effect is increasing with stringency. I then use the predicted travel to estimate how Airbnb supply responds. I find no significant evidence that restriction-induced demand changes impact Airbnb supply, suggesting a need for more granular travel data.

Chapter III analyzes the effect of short-term rental policy on Airbnb supply. The growth of the short-term rental (STR) market has generated concerns about increased housing prices and negative externalities in residential neighborhoods. I study Denver's 2017 STR regulations,

which introduced a primary residence requirement aimed at preventing landlords from developing STR investment portfolios. I use a Poisson differences-in-differences model to quantify the effect of Denver's policy on Airbnb supply. Relative to a set of control cities, regulations decreased active listings by 24.3%, or 12 per zip code. Pre-existing listings were significantly more likely to exit the market, especially properties hosted by commercial hosts. Active enforcement of the policy through citations did not have an additional effect on supply. I suggest that relatively low fines did not deter non-compliance when hosts expect high revenue. Finally, the regulations caused, on average, a $30 reduction in long-term monthly rental prices, concentrated in Downtown Denver. This was driven by an increase in long-term rental supply.

Chapter IV contains previously unpublished co-authored material with Alfredo Burlando, Lucia Dalla Pellegrina, and Josephine Kim. This chapter investigates the impact of crime and insecurity on the retail banking sector in Mexico, analyzing how elevated crime rates influence banks' operational dynamics, including transaction volumes, credit risk, and overall profitability. Using data from 2001 to 2011 and employing a shift-share regression methodology, the study distinguishes between direct impacts on banks within high-crime areas and indirect impacts on banks in low-crime areas with branches located in high-crime regions. The findings reveal significant adjustments in banking operations due to crime-induced insecurity, with reductions in deposits, credit, branch numbers, and ATM points in municipalities indirectly exposed to higher crime rates. This research highlights the broader economic implications of crime on financial sector stability and contributes to understanding the mechanisms of adaptation of financial institutions to external shocks, complementing the existing literature on financial inclusion and poverty reduction.

# CHAPTER II

# COVID-19 POLICY STRICTNESS AND THE EFFECTS ON TOURISM: AIRBNB SUPPLY RESPONSE TO INCREASED TRAVEL DEMAND

## 2.1 Introduction

In 2020, the rapid spread of Covid-19 resulted in a global pandemic. By March 2020, every U.S. state had reported a confirmed case, and state governments imposed several related policies aimed at limiting the virus. These containment and closure policies restricted public gatherings, closed interstate travel, and encouraged individuals to stay home when possible. After several months of restrictions, states began to ease the closure policies while still encouraging safe behavior. By August 2020, all interstate travel bans had been lifted, and the rate at which remaining containment policies were lifted varied across states. This variation continued through July 2021. During this 'initial recovery phase,' the unfamiliarity of Covid dampened and fears surrounding Covid declined steadily (Mertens et al., 2023). Domestic travel increased, and those who felt comfortable engaged in airline travel. Those traveling for leisure may have shown preferences for destinations with lower Covid stringency levels.

Resulting from the overall lower levels of travel related to Covid, accommodation services, including Airbnb, experienced a significant reduction in demand. However, Airbnb recovered quickly and grew from pre-pandemic levels within a year (Dubois, 2021). Home-sharing platforms have become increasingly prevalent as a short-term rental (STR) accommodation option. In recent years, residents have become concerned with the impact that short-term rentals may have on long-term housing prices (Barron et al., 2021). Increasing profits

generated by short-term rentals may incentivize landlords to enter the STR market instead of the long-term rental market, which would then put upward pressure on rental prices through reduced long-term supply. To understand the relationship between profits and long-run STR supply, I exploit the variation in state Covid restrictions as a shock to overnight accommodation demand and use this to estimate the effect of a persistent demand shift on Airbnb supply.

Variation in both the longevity and stringency of state policies offers an opportunity to analyze travel decisions as a function of state restrictions, as well as the implications of those decisions on the tourism sector and residential welfare. Specifically, observing differences in state response to the same disruption provides a setting to identify disproportionate economic recovery. From there, we can establish how changes in destination travel patterns affect the supply of tourist accommodations. I hypothesize that increased containment policies reduce city amenities temporarily. Conditional on travel, the extent to which a city's amenities are available is now relevant for destination choice. I first show that the stringency of Covid restrictions systematically impacts the number of airline passengers traveling to an airport – places with higher restrictions relative to other states experience significantly lower travel demand. I find that, after omitting March – August 2020, a standard deviation increase in the stringency level reduces domestic airline passengers by 6.6%, compared to states at the average stringency for that month.

The relationship between short-term rental supply and demand is endogenous. It's possible that an increasing number of accommodation options in an area positively affects travel demand. This could happen through increased variety in lodging, reduced prices, or reducing supply constrained markets. Having established that individuals traveling prefer destinations with lower restrictions, I use predicted travel demand to identify the effect on Airbnb supply.

Covid restrictions are plausibly exogenous to Airbnb supply as the purpose of restrictions are to reduce Covid transmission. These policies target the demand for travel by restricting the extent to which people can gather. I investigate how responsive property owners are to surges in travel demand by comparing Airbnb supply in states with high Covid restrictions to states with low restrictions. Economic theory suggests that suppliers will enter the STR market in the long-run when profits are positive. To identify Airbnb host response, I implement an instrumental variable regression where I predict travel demand with Covid restrictions. Theoretically, an increase in demand would result in an increase in the supply of Airbnbs. I find that an increase in predicted travel demand has a weak but positive effect on Airbnb supply.

The rest of the paper is organized as follows. I discuss the state of existing literature in Section 2. In Section 3, I describe the data and methodology used to explore the relationship between short-term rental demand, profits, and supply decisions. I present the results in Section 4 and provide concluding remarks in Section 5.

**2.2 Literature Review**

Research addressing Covid has primarily focused on the effectiveness of policies at reducing the spread. Other angles have looked at how the interruption has affected operations and profits of firms, specifically during the pandemic. Dogru et al. (2023) regress occupancy rates on state Covid cases to see how Covid affects booking behavior for both Airbnbs and hotels. This is primarily a demand study, as they control for the supply of accommodation. Additionally, in analyzing how the effects change with response stringency, they classify states as either ‘more restrictive’ or ‘less restrictive’ according to WalletHub. Ultimately, they find a 1% increase in the number of cases decreases the hotel occupancy rate by roughly 0.01%, decreases private room Airbnb bookings by 0.011%, and has no significant impact on entire

home Airbnb bookings. For hotels, the effect is dampened in less restrictive states. Their analysis ends in December 2020 and is limited by the classification of state restrictions. I extend this by looking at an actual stringency index and differentiate from their work by examining the supply response. Other work finds that, in Italy, Airbnb consumers in August 2020 favor listings with a price premium and locations in rural areas (Filieri et al., 2023). This suggests there is a preference for safer accommodation following the reopening.

With the Airbnb platform increasing in popularity, researchers have begun attempts at identifying the effects of Airbnb presence on local housing markets and consumer welfare. Farranoto and Fradkin (2018) find that Airbnb generates consumer surplus for travelers by putting downward pressure on accommodation prices, particularly in supply constrained areas or times. They estimate that a 1% increase in the average daily Airbnb rate results in 2.2% increase in bookings. For a zip code at the median owner-occupancy rate, a 1% increase in Airbnb listings increases the rental rate by 0.018% and home prices by 0.026% (Barron et al., 2021). Areas with more listings lead to decreases in the renter-occupancy rate and vacancies for rent, suggesting there is a reallocation from the long-term rental market to the short-term rental market Several other works attempt to estimate the effects of Airbnb on home prices (Sheppard & Udell, 2016; Marque et al., 2023), on externalities (Filippas & Horton, 2017), and consumer welfare (Farhoodi, 2019; Huang, 2021).

My research contributes to existing literature in the following ways. Most research surrounding Covid and tourism looks at the effects on operations during the pandemic or the efficiency of restrictions in reducing the spread of the virus. I differentiate from this by investigation how varied Covid restrictions affected travel behavior during a period of recovery. To my knowledge, I am the first to suggest using Covid restriction stringency as an econometric

tool to model sorting during this time and extend it as an exogenous shock to travel. Although we saw politicalization of the virus in many ways, we have yet to quantify the extent to which the pandemic induced a political migration and the implications of sorting on neighborhoods. Additionally, I then use the resulting difference in destination choice to examine how short-term rental supply responded. Although it is important to understand a property owner's decision to supply to the long-term or short-term market, data limitations and simultaneity issues make answering this question a challenge. I address this issue by exploiting the change in demand caused by Covid-19 restrictions.

## 2.3 Data and Methodology

### 2.3.1 Data

The measure of state Covid restriction stringency comes from Oxford' Covid Government Response Tracker (OxCGRT). The dataset reports a daily index of various Covid-related policies for each state, categorized as containment and closure, economic policies, health systems, and vaccination policies. The containment and closure policies are of particular interest and report a measure of school closures, workplace closures, public event cancellations, limitations on gatherings, public transport closures, shelter-in-place status, domestic travel restrictions, and international travel restrictions. Oxford constructs a stringency index (SI) that captures all containment and closure policies, as well as the status of public information campaigns. I use a similar index constructed with the same methodology, but I include the state's requirement for facial coverings. I aggregate the daily state index to the monthly level and create a normalized measure of state stringency within each month. Table 1 summarizes the various measures of the stringency index used in the analysis. SI 2 is used as the primary measure throughout the analysis.

**Table 1** Stringency Index Construction

| | |
|---|---|
| SI 1 - OxCGRT Index | All containment and closure policies<br>Public health campaigns |
| SI 2 - Preferred Index | All containment and closure policies<br>Public health campaigns<br>Facial coverings |
| SI 3 | All containment and closure policies<br>Facial coverings |

The stringency index is ordinal, and larger values correspond to stricter Covid policies. There was significant variation in Covid response across states during the first 18 months of the pandemic. Figure 1 illustrates the monthly distribution of the stringency index, which includes facial covering and public health campaign indicators. By April 2020, all states had responded with a set of policies that corresponded to a large stringency index. As the initial shock of the pandemic settled, states varied in the intensity of their response. All states lifted domestic travel bans by August 2020, but the lingering restrictions continued across states. This variation in stringency converges around June 2021, at which point most states suggest safe behavior and social distancing.

At times, I categorize each state as a high restriction state, average restriction state, or low restriction state. To do so, I first calculate the average stringency index in the first 12 months of the pandemic (March 2020 - February 2021), and I observe an average index of 59.4 with a standard deviation of 11.2. If a state's average stringency index over this period is greater than 65, I consider the state to be a high restriction state.[1] If the state's average stringency index is below 53.8, I consider the state to be a low restriction state. States with an average index between 53.8 and 65 are considered average. In addition to the policy indicators and stringency index, the OxCGRT dataset includes the number of new confirmed cases and confirmed deaths

[1] The formula associated with the categorization is $avg.index \pm .5 * sd$. Using a full standard deviation does not provide enough states in either the high or low category.

associated with Covid. I account for the number of monthly cases per capita and deaths per capita in the analysis.

**Figure 1** Monthly Distribution of State Stringency

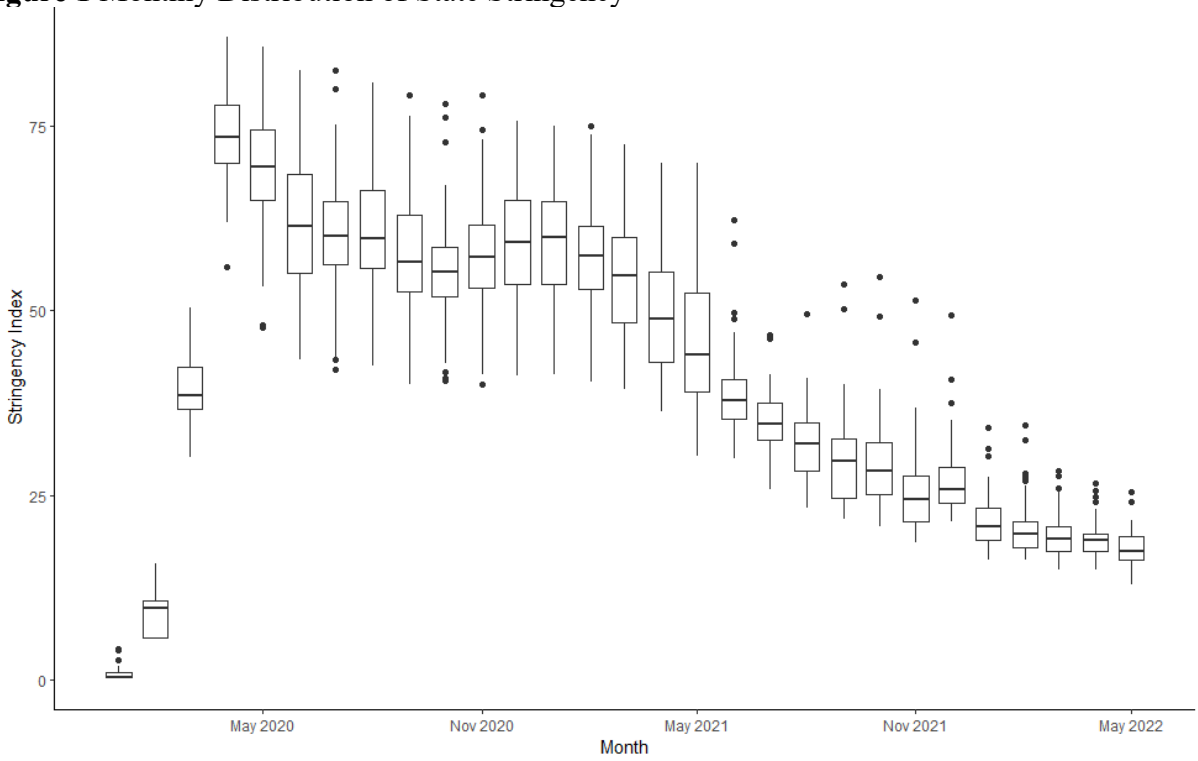



Figure 1 shows the monthly distribution of the state stringency index (SI 2), constructed with all containment and closure indicators, as well as facial coverings and public health campaign indicators.

The Bureau of Transportation Statistics (BTS) reports monthly airport passengers through the T-100 Domestic Market database. Passenger counts are provided for both domestic flights and origin flights. Importantly, origin and destination are based on the individual, not the flight. If an individual travels from Portland to Houston with a layover in Denver, the origin is Portland, the destination is Houston, and Denver is not included in the observation. Some airports service more than one city. For these instances, I combine data for the cities listed in the airport name. Additionally, some cities are serviced by more than one airport, so I conduct analysis at the airport level. City demographics come from the American Community Survey, and I aggregate

those from zip code to city using the 2019 Zip Codes Business Patterns. Although I do not use the business records from that dataset, it did provide a convenient crosswalk from zip code to city.

I use data from AirDNA to measure the number of Airbnb units in a city. AirDNA regularly scrapes the Airbnb website to collect operations data for each listing in the United States. Additionally, AirDNA defines a location type for each property.[2] The goal of this paper is to understand the extent to which demand shocks influence potential Airbnb hosts to invest in the short-term rental market As such, I only include properties that are categorized as entire homes. For each airport included in the BTS dataset, I count the number of Airbnb properties in the cities directly served by each airport. Additionally, I determine the location type of each airport region as the most common location type of the Airbnb units.

The period of analysis covers April 2015 through May 2022. Figure 2 illustrates the national trend in travel-related variables over this time. Domestic airline travel experiences an immediate and large reduction in passenger counts at the onset of Covid. The numbers recover slightly over time, but do not fully recover until 2022. The number of Airbnb reservations also falls. Airbnb supply seems to take a slight dip but appears mostly unchanged.

### 2.3.2 Methodology

The relationship between short-term rental supply and demand is endogenous. Increasing the number of accommodation options in an area will increase the amount of travel demanded. Additionally, the increased variety in lodging and the resulting investment in an area could cause a shift in demand for accommodations. Over time, the number of short-term rental suppliers will respond to the profits resulting from the demand shock. This endogenous relationship makes it

[2] Location types include coastal, mountainous/lake resort, urban, suburban, medium sized city, and rural.

challenging to causally identify the effect of demand shocks on supply. To disentangle changes in demand that are independent of supply, I use an instrumental variable strategy. Identification relies on the assumption that covid restrictions only affect Airbnb supply through its influence on demand. I exploit variation in Covid containment policies as an exogenous shock to demand, where I use domestic airline passengers as a proxy for Airbnb demand. I expect that states with higher restrictions will experience a large reduction in demand.

**Figure 2** National Travel Over the Year

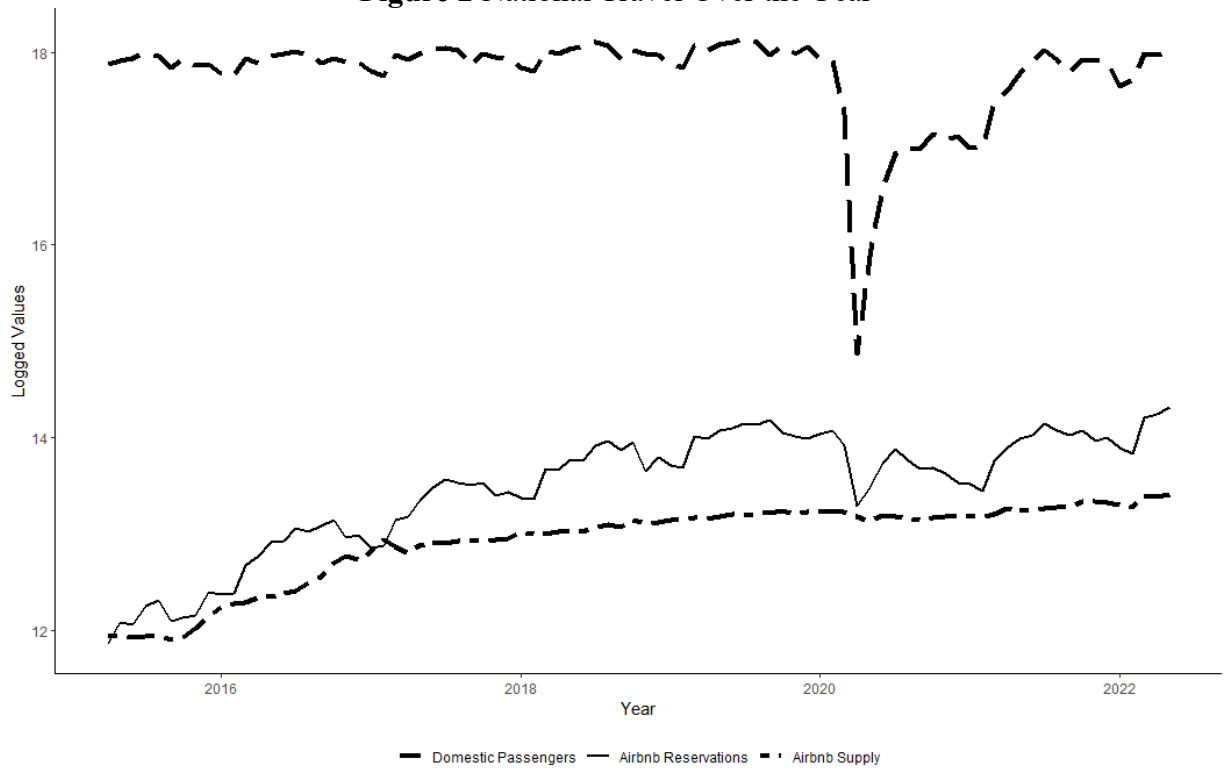



Figure 2 shows the trend in national domestic airline passengers, national Airbnb reservations, and national Airbnb supply. Airbnb supply includes all publicly viewable listings that are considered entire homes.

These shocks offer a unique setting for understanding the supplier's behavior. Potential suppliers will observe demand for overnight accommodation in their area and decide to enter the market if they expect entry to be profitable. A persistent increase in local travel demand would increase the profitability of Airbnbs in the area, potentially causing new hosts to invest in the

market This setting offers information as to how different policy timelines affect hospitality recovery. In response to the economic disruption, it's likely that a fraction of hosts removed their listings from the market We can observe the host's decision to relist their property and when they do so. I expect that locations that lift restrictions earlier experience more hosts that return to the market.

To estimate the effect of restrictions on demand, I use the following equations:

$$demand_{ast} = \beta_1 SI_{st} * location_a + \beta_2 cases_{st} + \beta_3 deaths_{st} + \gamma_a + \gamma_t + \varepsilon_{ast} \quad (1)$$

$$supply_{ast} = \delta_1 \widehat{demand}_{ast} + \delta_2 cases_{st} + \delta_3 deaths_{st} + \gamma_a + \gamma_t + \varepsilon_{ast} \quad (2)$$

where $\beta_1$ captures the effect of a standard deviation increase in regulations on demand, relative to a city that experiences the average stringency index. The standard deviation is measured using the monthly average index across all states. I interact the stringency index with the location type of each city to explore heterogeneity along this dimension. $cases_{st}$ and $deaths_{st}$ measure the number of new cases and number of deaths in the state, respectively. Finally, I use month fixed effects and city or airport fixed effects. Standard errors are clustered at the state level. I then use the predicted values from Equation 1 to estimate the effect on Airbnb supply, using Equation 2. $\delta_1$ is the effect of restriction induced demand changes on Airbnb supply. If restrictions negatively impact the traveler's experience, then $\beta_1$ should be negative. Across location types, I expect the reduction in demand to be the largest for urban locations and smallest for low density locations (coastal, mountainous, rural). I expect $\delta_1$ to be positive, where increases in demand will increase the supply of Airbnbs.

The instrument is valid if restrictions affect Airbnb supply only through changing travel patterns. I address this in a few ways. First, although some states had imposed restrictions on the

**Figure 3** National Travel by Stringency Level

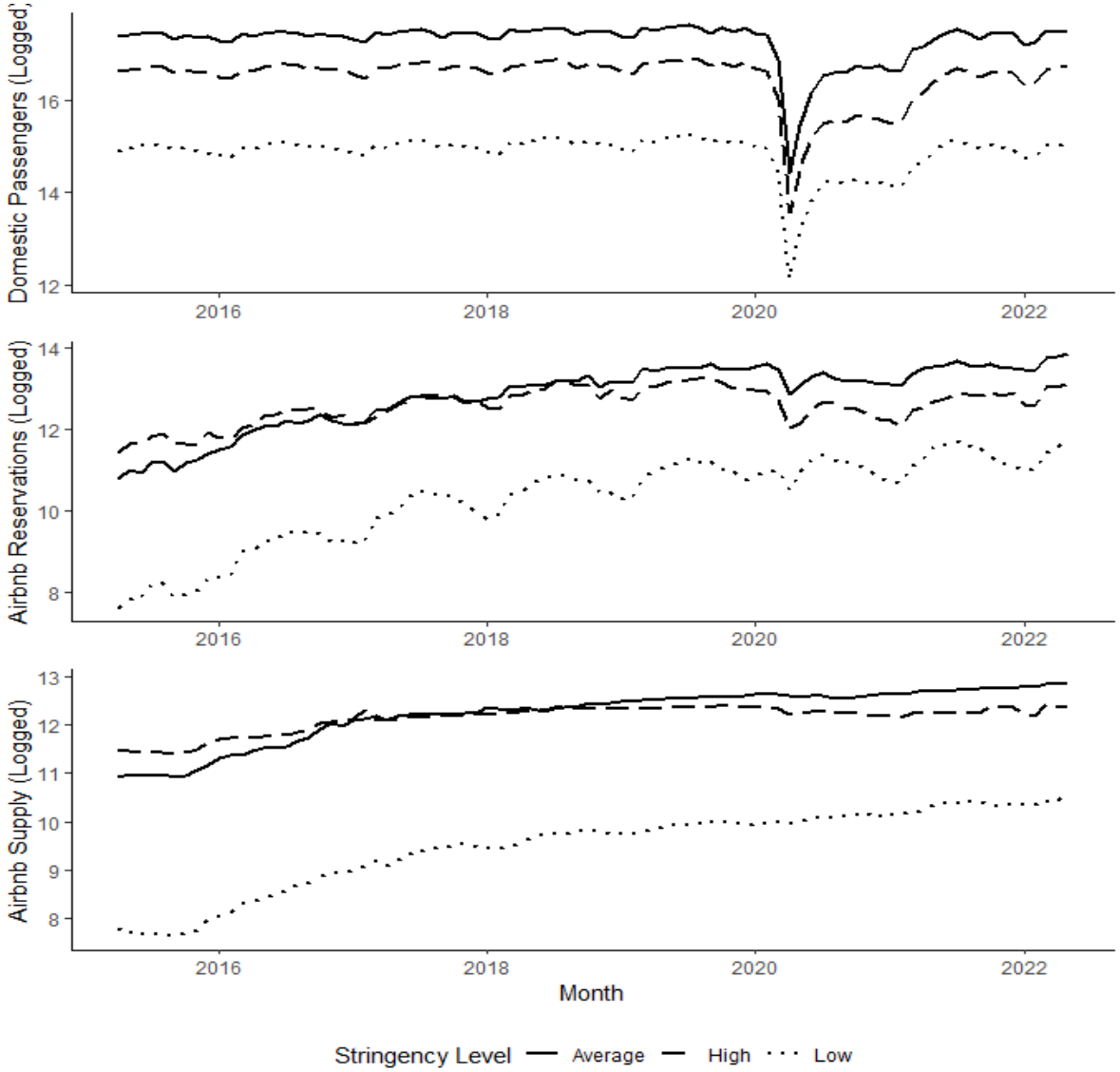



Figure 3 shows the trend in national domestic airline passengers, national Airbnb reservations, and national Airbnb supply. Airbnb supply includes all publicly viewable listings that are considered entire homes.

type of individual who could book an Airbnb (nurses, quarantining locals, etc.), there were limitations with enforcement. Hosts were responsible for screening guests, leaving opportunities for noncompliance. Second, I focus on the period when the U.S. reopened domestic travel. By August 2020, interstate travel bans had fully lifted, and direct Airbnb restrictions were no longer a concern. In my primary analysis, I exclude March 2020 through July 2020 to reduce the potential direct effect of restrictions on Airbnb supply. By this time, some households were eager

to resume travel and had disposable income. International travel was still restricted, so the U.S. experienced a surge in domestic travel. An additional threat to identification is the concern that 'touristy' cities influenced restrictions to continue fueling the tourist industry. I address the potential endogeneity issue by using state restrictions, where it is unlikely that any one tourist city can influence state response.

## 2.4 Results

I begin by visualizing the national trend in travel-related outcomes, distinguished by stringency level, in Figure 3. I categorize each state as either high restriction, low restriction, or average. Airline travel seemed to recover at the same rate regardless of the state stringency. Airbnb reservations and total supply seem to grow at a slower rate moving out of the pandemic.

### 2.4.1 Restriction Induced Demand Shift

Using Equation 1, I estimate the effect of Covid-19 restrictions on measures of demand. Table 2 reports the estimated effects of the normalized stringency on destination passenger counts. Column 1 provides the estimates associated with my preferred specification. One standard deviation increase in restrictions causes a 6.0% decrease in airline passenger counts. In Column 2, I use city fixed effects instead of airport fixed effects and observe a slightly larger coefficient.

Columns 3 and 4 report the heterogeneity in effects across location types. I find significant negative effects on airline passengers for all location type, with the exception of rural locations. The largest impacts are concentrated in coastal and mountainous locations. This is unsurprising but highlights a challenge in the analysis. Necessary travel is more likely to occur in large cities, which would dampen a negative estimate on leisure travel. With total passenger counts, I am unable to distinguish between leisure travel and necessary travel. Covid restrictions

**Table 2** Effect of Covid Stringency on Domestic Airline Travel

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Stringency | -0.062 ** | -0.064 ** | | |
| | (0.022) | (0.022) | | |
| New Cases | -0.028 | 0.004 | -0.026 | 0.006 |
| | (0.071) | (0.065) | (0.072) | (0.066) |
| Deaths | -0.948 | -1.162 | -1.303 | -1.550 |
| | (3.552) | (3.527) | (3.574) | (3.521) |
| String x Coast | | | -0.102 * | -0.099 * |
| | | | (0.040) | (0.040) |
| String x Mountain | | | -0.137 * | -0.143 * |
| | | | (0.059) | (0.058) |
| String x Suburban | | | -0.039 | -0.050 |
| | | | (0.036) | (0.036) |
| String x Urban | | | -0.068 ** | -0.087 * |
| | | | (0.022) | (0.040) |
| String x Medium | | | -0.043 | -0.038 |
| | | | (0.022) | (0.023) |
| String x Rural | | | -0.043 | -0.044 |
| | | | (0.036) | (0.034) |
| Date FE | Yes | Yes | Yes | Yes |
| Location FE | Airport | City | Airport | City |
| N | 39362 | 39362 | 39253 | 39253 |
| R2 | 0.969 | 0.865 | 0.969 | 0.864 |

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Standard errors clustered at state level and reported in parentheses. New cases and deaths are measured per capita.

should not influence necessary travel, so airline passenger counts do not make a strong proxy for Airbnb demand. In any case, the negative impact of Covid policy is increasing in magnitude with strictness.

I extend my preferred specification to other constructions of the explanatory variable. Table 3 reports the estimates using alternative measures of stringency index, including the OxCGRT's provided index (1), the index that includes face covering and public health campaign indicators (2), and an index that only includes containment indicators and facial covering indicators (3). In all cases, the effect is within .3 percentage points of the preferred specification reported in column 2.

**Table 3** Alternative Measures of Covid Stringency and Domestic Airline Travel

| | (1) | (2) | (3) |
|---|---|---|---|
| Stringency | -0.063 * | -0.062 ** | -0.059 * |
| | (0.024) | (0.022) | (0.025) |
| New Cases | -0.025 | -0.028 | -0.020 |
| | (0.071) | (0.071) | (0.068) |
| Deaths | -0.880 | -0.948 | -1.085 |
| | (3.549) | (3.552) | (3.538) |
| Date FE | Yes | Yes | Yes |
| Location FE | Airport | Airport | Airport |
| Public Health | Yes | Yes | No |
| Facial Coverings | No | Yes | Yes |
| N | 39362 | 39362 | 39362 |
| R2 | 0.969 | 0.969 | 0.969 |

*** p < 0.001; ** p < 0.01; * p < 0.05.

Standard errors clustered at state level and reported in parentheses. New cases and deaths are measured per capita.

**Figure 4** Effect of Stringency Index from m Months Ago on Travel

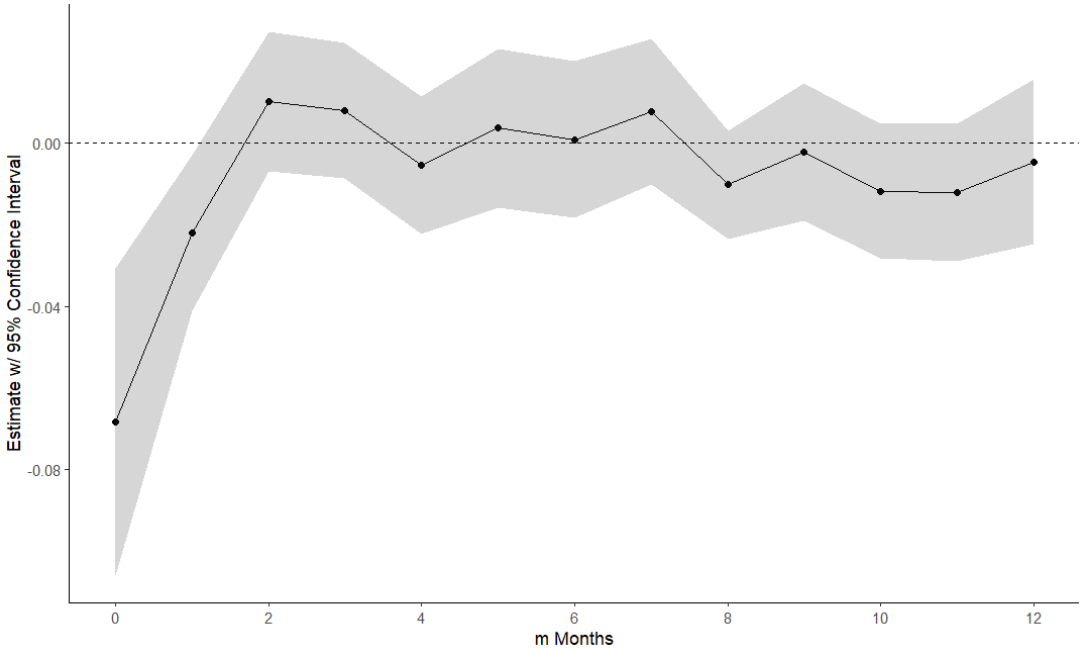



Figure 4 provides the estimates of the stringency index from m months ago on current domestic airline passengers. The 95% confidence interval is reported by the shaded area.

Travel plans, especially airline travel, are typically considered well in advance of the actual travel date. The sudden onset of Covid increased flexibility around this to some extent, as

people were able to cancel plans or bookings at the last minute. To investigate the role of timing, I estimate the effect of the lagged stringency index on airline passengers. I find a significant impact of a one-month lag on current passengers shown in Figure 4. There are no effects for lags past one month.

**Table 4** IV Regression Estimates

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Pred. Passengers | -0.996 | 3.483 | -0.164 | 0.573 |
| | (4.438) | (24.233) | (0.488) | (0.618) |
| Stringency | -0.100 | 0.184 | | |
| | (0.271) | (1.571) | | |
| String: Coast | 4.774 | 10.773 | 5.590 | 6.559 *** |
| | (10.439) | (28.029) | (3.246) | (1.613) |
| String: Mountain | | | 0.017 | 0.023 |
| | | | (0.046) | (0.026) |
| String: Suburban | | | -0.088 | -0.016 |
| | | | (0.076) | (0.060) |
| String: Urban | | | -0.070 | 0.030 |
| | | | (0.071) | (0.078) |
| String: Medium | | | -0.052 * | -0.017 |
| | | | (0.022) | (0.051) |
| String: Rural | | | -0.090 | -0.030 |
| | | | (0.072) | (0.021) |
| Date FE | Yes | Yes | Yes | Yes |
| Location FE | Airport | City | Airport | City |
| N | 39253 | 39253 | 39253 | 39253 |
| R2 | 0.910 | -1.504 | 0.956 | 0.906 |
| First-stage F-stat: | 9.770 | 8.500 | NA | NA |

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Standard errors clustered at state level and reported in parentheses. New cases and deaths are measured per capita.

### 2.4.2 Effect of Demand Shock on Airbnb Supply

Having established that states with higher restrictions experience larger reductions in airline travel compared to states with lower restrictions, I now investigate the effect of the restriction induced demand shock using Equation 2. I expect that increases in predicted demand will increase the supply of Airbnb units. The results for the instrumental variable regression are

shown in Table 4. My preferred analysis is reported in Column 1, where I include airport region fixed effects. I find a weak increase in Airbnb supply following a Covid induced increase in demand. The effect is similar, but insignificant, when I use city fixed effects in Column 2. I find no additional effect when I interact stringency with location type, shown in columns 3 and 4.

**2.5 Conclusion**

In response to Covid-19, states individually set containment policies that limited the social amenities available in the community. These policies varied widely across states in both intensity and longevity. After the first few months of uncertainty, domestic travel reopened, and individuals slowly engaged in tourism activity. I theorize that the lingering containment policies influenced a traveler's destination decision, and I provide evidence that consumers preferred travel destinations with lower restrictions. Specifically, I find that a one standard deviation increase in stringency is associated with a 6.6% decrease in airline passengers. The effect is increasing in stringency. As expected, the effects are driven by the period of August 2020-June 2021, where there is high variation in state containment policy and individuals are returning to travel. The effect is concentrated in the short-term, suggesting that travelers are responding to updates in their information set I utilize the restriction induced change in travel patterns to estimate the extent to which Airbnb supply responds to demand shocks. Potential Airbnb hosts might have observed a local increase in travel demand related to lower restrictions and choose to enter the market Although the results are weakly significant, they do suggest that demand shocks increase the supply of Airbnb properties.

This study faces some limitations. First, airline passengers seem to be a weak proxy for Airbnb demand. Airline travel did not have a strong correlation with Airbnb demand or measures of Airbnb supply. There are a few possible reasons for this. First, during Covid, short trips to

destinations within a driving distance became a popular option for travelers. This wouldn't be captured in airline data. Future work could account for this by using granular mobility data that observes vehicular travel or individual movement. Additionally, there is likely a within-state effect that I do not capture. Travelers may opt to spend a weekend in a less densely populated area, increasing the demand for that location type despite facing the same Covid restrictions.

The instrumental variable estimation strategy is valid if the restrictions only affect Airbnb supply through the resulting demand change. Most restrictions did not explicitly limit the extent to which Airbnb could operate. After the initial onset of Covid, some states requested that Airbnb hosts do not rent to travelers, with an exception for travelling nurses or other responders. There was essentially no active enforcement of this, and the states which attempted to limit Airbnb activity only did so for a short-period of time. Regardless, my primary analysis omits the months with interstate travel restrictions, March-June 2020. This would alleviate the concerns of direct influence via Airbnb specific regulations.

In my analysis, I find evidence that restrictions do impact airline travel, and, subsequently, could impact the local Airbnb market Specifically, restrictions reduce travel, as well as the demand for accommodation options. The results suggests that Covid restrictions and policy decisions shaped the tourism recovery for local regions following the pandemic. Although the study provides suggestive evidence, the findings invite further work to examine the implications of states responding differently to a national interruption. The results also establish containment restrictions as a tool to model the political sorting during this time. There are not many policies that individuals directly experience on a day-to-day basis, but Covid restrictions directly impacted an individual's day. From these restrictions, the U.S. experienced a 'political migration' of sorts, and future work should explore the effects of this sorting.

# CHAPTER III

# SHORT-TERM RENTAL REGULATIONS, ENFORCEMENT, AND HOST BEHAVIOR: EVIDENCE FROM DENVER

## 3.1 Introduction

In recent years, home-sharing as an accommodation option has become an increasingly popular option for consumers and homeowners. Platforms like Airbnb and Vrbo provide a trustworthy space for homeowners to rent vacant living space to travelers. Through online bookings, homeowners can generate additional personal income, and lodging taxes generated from reservations provide tax revenue for the city. In high-demand locations, vacation properties often generate higher revenue than long-term rentals, which can incentivize landlords to substitute towards the short-term rental (STR) market Additionally, investors may choose to expand short-term rental property portfolios. This investment behavior can reduce the supply of long-term housing and puts upward pressure on prices (Barron et al., 2021; Garcia et al., 2022). The potential crowd-out effects have prompted discussions on whether governments should regulate short-term rentals. By June 2016, there were 400 dedicated Airbnb units hosted by someone with more than two properties in Denver, CO. In the same month, Denver passed an ordinance to regulate the STR market, with the intention to limit investment behavior and protect affordable housing.

Regulations for short-term rentals vary. Some cities, like New Orleans, have banned short-term rentals in particular areas. Other cities take a more lenient approach, requiring only that listings are registered with the city, associated with application and renewal fees. Denver's

approach falls somewhere in the middle. In addition to the financial costs of registration, hosts may only operate a short-term rental if it is their primary residence. In theory, hosts may not operate more than two listings in Denver. Non-compliant listings are subject to a fine of $150 for the first offense. The ordinance passed in June 2016 with three goals: protect affordable housing, preserve social connections in neighborhoods, and create an equal regulatory environment for all entities in the accommodations industry. Regulations became effective six months later, in January 2017.

The Denver ordinance changed homeowner incentives in three ways. First, the licensing requirement increased the fixed cost of operating a short-term rental and may deter a fraction of listings from operating in the market Second, the primary residence condition prohibited investment behavior in the city. Finally, the threat of citations reduces the expected value of hosting and may push non-compliant hosts out of the market The goal of this paper is to quantify the effect of the policy on Airbnb supply, explore the challenges with enforcement, and identify the effect of reduced STR supply on housing prices. To quantify the effect of STR regulations in Denver on Airbnb supply, I utilize an extensive dataset from AirDNA. I compare the supply trends in Denver to 14 unregulated cities using a Poisson difference-in-differences model. Based on the number of properties the host has been associated with and the activity patterns of the listing, I categorize listings as either casual, professional, or commercial and explore the heterogenous response across each type.[3]

Results show that regulations slowed the growth of investment behavior in Denver but did not stop it. Over the first three years since the regulations became effective, the supply of active Airbnb listings fell by almost 25%, or 12 listings per zip code, relative to the control cities.

[3] I refer to professional and commercial hosts as investment hosts but distinguish between the two in reference to the analysis.

The effect occurred immediately, as listings either blocked their availability or removed themselves from the market entirely. Commercial properties that existed prior to regulations were more likely to exit the market, and regulations deterred potential professional hosts from entrance. However, non-compliant activity was common, and investment behavior continued to grow despite the regulations.

Existing literature shows that hosts are most responsive to regulations with an active enforcement mechanism (Zhe Jin, 2024). Expected value theory suggests that an agent will continue to engage in unwanted behavior until the expected costs of doing so exceed the expected benefits. As such, increasing the citation costs or the probability of detection could deter non-compliant behavior. I use administrative citations data to determine whether the threat of citation deters professional or commercial supply in Denver. An exploratory analysis suggests that the expected cost of non-compliance did not exceed the monthly revenue generated by a short-term rental. The average monthly revenue generated by a Denver host during the sample period is $1524, more than 10 times the cost of the first citation. Additionally, the City of Denver was not incentivized to issue citations that might reduce investment supply. In peak demand, investment properties could generate up to 40% of the monthly lodging revenue generated by Airbnb units in Denver.

Having observed a relative reduction in the supply of Airbnb listings, I turn my analysis to the performance of remaining listings. For each active listing, the number of monthly reservations and the monthly revenue did not change. Overall, the total amount of reservations that occurred in each zip code fell. This was driven by the supply reduction, as I do not observe a significant effect of regulations on the reservations per active listing, nor an effect on monthly revenue per active listing. The average daily rate is also not significantly affected despite the

reduction in supply, suggesting that demand for short-term rentals is elastic. Commercial properties experienced a slight increase in revenues through increased reservation nights.

A major goal motivating the STR regulations in Denver was to protect affordable housing. Investment properties often remove a potential unit from the long-term rental market, so preventing this substitution should, in theory, increase the supply of long-term rental housing and put downward pressure on prices. I find no evidence to suggest the reduction in STRs (relative to control cities) impacted house prices directly, but I observe a negative effect on the rental price index, particularly in zip codes near the downtown area. On average, this translates to a $30 decrease in monthly rent. Additional testing suggests that this was driven by an increase in rental supply, consistent with theoretical predictions.

This paper is outlined as follows. In Section 2, I provide a status of the relevant literature related to short-term rentals. I outline the regulatory background in Denver in Section 3, followed by a description of the data in Section 4. In Section 5, I show that regulations cause a significant reduction in the supply of active Airbnb listings. Commercial listings experience the largest reduction, driven by an increase in likelihood a property exits the market entirely. Results do not suggest that citations further reduce supply. Section 6 explores the effects of regulations on the performance of remaining Airbnbs, as well as the effect on housing prices. I provide concluding remarks in Section 7.

### 3.2 Literature Review

This paper contributes to the literature on short-term rentals, particularly to the growing set of analyses of regulatory efforts in this market Valentin (2020) evaluates the effects of STR regulation in New Orleans, which regulated the number of nights a host could rent their STR per year. Additionally, the policy banned listings in the French Quarter entirely. The policy led to

additional Airbnb supply and bookings in the surrounding census tracts, suggesting that hosts responded to a supply vacuum created after the ban. Chicago restricted the number of listings allowed in different types of buildings by placing restrictions based on multi-unit vs single family buildings. As a result, Zhe Jin et al. (2024) find that Airbnb supply falls, but not until the city coordinated with Airbnb to enforce the policy. They also find localized reductions in burglaries near buildings the prohibit STRs. Chen et al. (2022) finds that restricting the number of listings a host may have increases the supply and prices of nonprofessional listings. To my knowledge, the implications of a primary residence condition have not been studied.

Part of the regulatory framework in Denver is the use of fines to deter non-compliant listings. The effectiveness of citations in deterring unwanted behavior is an area of literature that is widely debated. An expected value model (Becker 1968) suggests that increasing the expected cost, through increased probability of citation or increased fines, should deter behavior when it outweighs the expected benefit of non-compliance. Some researchers find that increasing the size of fines is more effective at reducing unwanted behavior (Laske et al., 2018; Feess et al., 2018). Others find that lower fines with a higher frequency of citation is the most effective (Teodorescu et al., 2021), or that increased probability in general is an effective tool for deterrence (Matsuzawa, 2025). My work contributes to the literature by providing evidence that increasing the rate of citations is not an effective tool for deterring non-compliance when the fines associated with detection are low, relative to the revenue earned.

There is limited, but growing, research on the impact of Airbnb on residential welfare and prices. There is evidence that the presence of Airbnbs in neighborhoods increases housing costs. For a zip code at the median owner-occupancy rate, a 1% increase in Airbnb listings increases the rental rate by 0.018% and home prices by 0.026% (Barron et al., 2021). Areas with more

listings lead to decreases in the renter-occupancy rate and vacancies for rent, suggesting there is a reallocation from the long-term rental market to the short-term rental market In Santa Monica, Airbnb regulations resulted in increased house prices, suggesting a negative relationship between STR presence and prices (Garcia et al., 2022). Several other works find that Airbnb presence increases property values (Sheppard & Udell, 2016), long-term rental prices (Marque et al. 2023) and has conflicting effects on crime (Ke et al., 2021; Roth 2021). Finally, allowing STRs increases residential well-being (Filippas & Horton, 2017) and consumer welfare (Farhoodi, 2019; Huang, 2021).

Advocates for Airbnb suggest that increased accommodation locations bring tourists (and their spending) to neighborhoods that otherwise wouldn't be visited. As far as the effects of Airbnb presence on economic activity goes, Basuroy et al. (2022) find that an increase in Airbnb reviews increases restaurant revenue, especially in less commercialized zip codes. For travelers, Farranoto and Fradkin (2018) find that Airbnb generates consumer surplus for travelers by putting downward pressure on accommodation prices, particularly in supply constrained areas or times. They estimate that approximately 50% of nights booked would not have translated to a hotel booking sans Airbnb. Other work finds that Airbnb listings have become more geographically diverse and are most profitable in middle-income neighborhoods (Coles et al., 2017), and the presence of Airbnb in neighborhoods spurs business growth through commercial hosts (Xu & Xu, 2021).

My paper contributes to the established literature in the following ways. First, I provide additional evidence on the direct effects of short-term rental regulations using a Poisson distribution and difference-in-differences method. I expand on this by exploring the heterogenous effects on different host types with the expectation that professional and

commercial hosts should exit the market following regulations. Additionally, I explore the impact of active enforcement by observing the response following short-term rental citations.

### 3.3 Regulatory Background

In June 2016, the city of Denver passed Ordinance No. 262-16 to address short-term rentals, which would take effect six months later in January 2017. The timeline is shown in Figure 5. Like many other cities with STR regulations, Denver requires Airbnb hosts to register their listings with the city. This includes a financial cost for hosts, with application fees ($50), an annual registration fee ($100), and various taxes. There are also safety and building requirements that must be satisfied. Once approved, any STR advertised on a home-sharing platform must include the corresponding license number in the property description. The license is valid for one year and must be renewed to continue operations.

**Figure 5** Denver STR Regulatory Timeline


In addition to the registration requirement, Denver implemented a primary residence condition. Any property rented for less than 30 days must be the host's primary residence, defined as the person's "usual place of return." To determine whether a property is the primary residence, the city uses voting registration, tax records, utility bills, and other similar administrative records. This definition does not make allowances for partial tenancy or nomadic lifestyles. The city verifies the primary residence condition when approving the first application, and again for each yearly renewal.

By definition, a person may only have one primary residence. Consequently, compliant hosts are limited to one STR listing in the Denver municipality, and the listing must be located at their home address. Individuals whose primary residence does not fall within the Denver municipality may not list a short-term rental in the city. If someone wants to list a property that is not their primary residence, the host must apply for a lodging facility license. There are additional restrictions associated with this process, including a minimum number of rooms and zoning classifications. Anecdotal evidence suggests this is not common practice. Alternatively, the property could operate as a medium-term rental and rent the property for more than 29 days at a time, which requires separate residential registration. Hosts need not be present during the reservation, providing homeowners with an opportunity to rent their home during shorter periods of vacancy. Property owners are permitted to operate an accessory dwelling unit as a short-term rental, conditional on zoning status. In this case, the host must reside in the primary dwelling unit.

Hosts who choose to disregard regulations could avoid detection through a few avenues. First, hosts could strategically 'unlist' their property during business hours to avoid city detection. It is possible that hosts remove their listing from Airbnb entirely and reenter with the same property. The new listing would have a new id and is unable to be linked with the prior listing's id. However, this behavior could be connected through a host id and does not appear to be relevant in the data.[4] Other hosts might falsify the records used to verify primary residence or use another listing's registration number. In both cases, the listing has a registration number advertised and the city would inaccurately perceive the listing as compliant. Although the City passively monitors the Airbnb website for non-compliant listings, most insights come from

[4] I look for this by comparing the monthly unique property ids to the total number of property ids associated with a host.

neighborhood complaints. The first citation for noncompliance results in a $150 fine, and recurring fines can cost up to $999. The earliest citation was given in June 2018.

### 3.4 Data

The primary dataset I use comes from AirDNA, the leading source of comprehensive Airbnb data. AirDNA regularly scrapes the Airbnb website. I include the set of cities with no STR restrictions from the 50 largest MSAs[5] between April 2015 and December 2019. For each publicly viewable listing, AirDNA reports the monthly operations data associated with each unique property id. I observe the number of reservations booked each month, the number of days available to be booked, average reservation length, and average daily rate[6]. AirDNA also provides time consistent information, such as the unique host id, listing type, location, and date of creation. Because regulations aim to reduce the number of homes removed from the long-term rental market, I restrict the sample to listings defined as entire homes.[7] To determine the specific municipality for each listing, I use Census shapefiles and the reported latitude and longitude. The latitude and longitude provided are approximations, with an error radius of roughly 500 feet I categorize each listing as a short-term rental or medium-term rental (MTR) according to the reservation patterns observed in the data. I use the same process as Zhe Jin et al. (2024), described in Appendix A. Through this inference, I am unable to reliably determine whether a listing is a dedicated MTR if they do not have a reservation during that month. Over the sample period, 28% of listings consistently behaved as MTRs. In my analysis, I keep all Airbnb listings regardless of MTR status. Finally, I define exit date as the last month when the listing is publicly viewable.

[5] The full set of cities included is Denver, Phoenix, Dallas, Columbus, San Diego, Charlotte, Tampa, Cleveland, Salt Lake City, Richmond, Jacksonville, Baltimore, Detroit, Pittsburgh, and Raleigh.
[6] AirDNA reports the average daily rate as total revenue divided by reservation days.
[7] There are four possible listing types: entire homes, private rooms, shared rooms, and hotel rooms.

I consider a listing to be active during a month if there is at least one day when the listing is available to be booked or is booked. I focus my primary analysis on the supply of active listings, as well as the host's decision to be active. Hosts might respond to regulations by temporarily pausing their activity while observing whether the city actively enforces regulations. When hosts list a property on Airbnb, they have the option to block off availability for a chosen amount of time. Consumers can view blocked properties but are unable to request a reservation during the blocked period. There are no restrictions regarding the length or frequency of blocked schedules. Alternatively, a host may "unlist" their property for some time. In this case, the property does not show up in the AirDNA data for those months. In the analysis, I consider an unlisted property to be blocked.

Using patterns in activity, I determine a property's dedication to the STR market I observe the number of months a property is publicly viewable on Airbnb, as well as the number of months a property is active. If they have been publicly viewable for more than 6 months and have been active for 95% of those months, the listing is considered dedicated. Each listing is associated with a host id that can be linked to other listings associated with the same host. I categorize each host as one of three types: casual, professional, or commercial. All definitions are time invariant and mutually exclusive. If a host has ever been associated with more than four active listing ids in a single month, they are considered a commercial host, and their listings are considered commercial listings if they are a dedicated property. Similarly, a professional host is one that has ever been associated with three or four listings in a month, and their listings are considered professional if they are dedicated listings. A casual host has never been associated with more than two listings in a single month.

**Figure 6** Airbnb Market Composition in Denver, CO

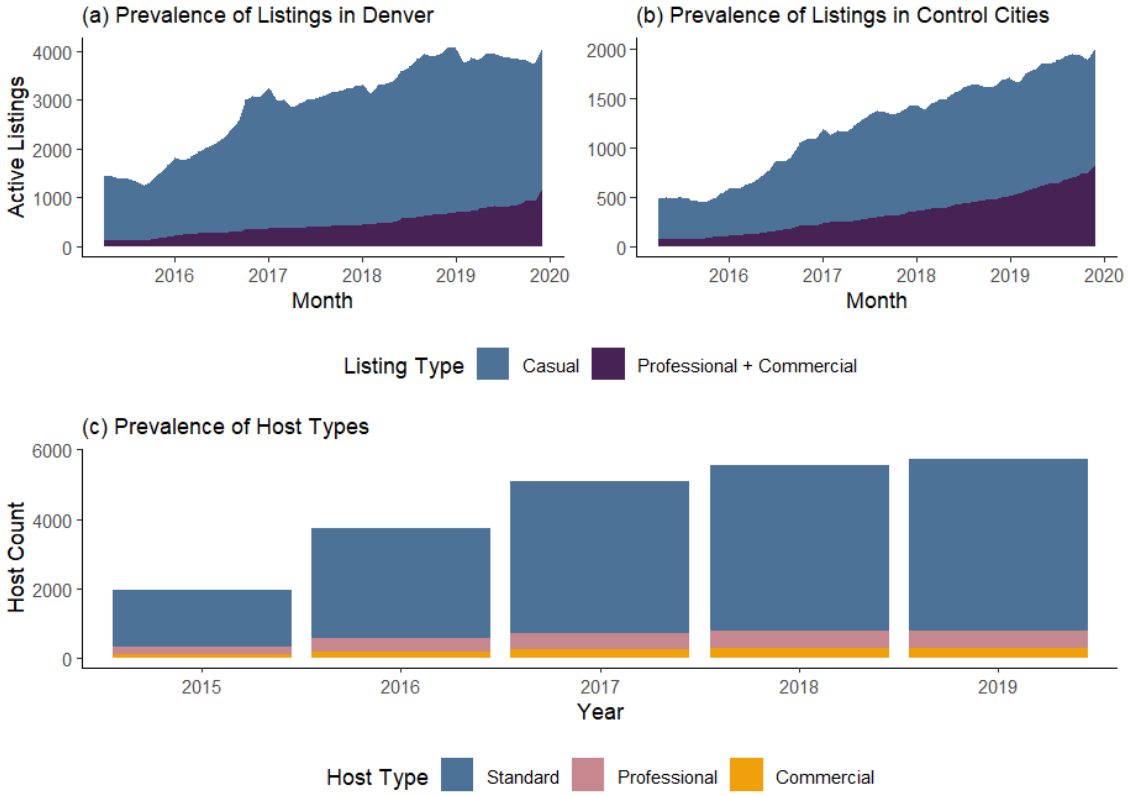



Image (a) compares the number of listings categorized as professional or commercial to the number of casual listings in Denver. Image (b) does the same for the average control city, where the values correspond to the average number of professional and commercial listings compared to average number of casual listings. Image (c) shows the composition of host type in Denver.

I chose a threshold of two properties to avoid an overestimation of true professional hosts. For example, a homeowner could list an additional dwelling unit at their place of residence as well as a designated short-term rental property. Although the second Airbnb unit is in violation of Denver regulations, there are several other cities where this situation is allowed and not considered to be a professional portfolio. This threshold is consistent with the approach by Zhe Jin et al. (2024). The supply of listings is heavily concentrated in the downtown Denver area. 51.7% of all listings and 65.6% of all professional or commercial Airbnb listings are within two miles of Union Station, a proxy for the downtown area, shown in Appendix Figure A1. All

definitions are constant across geography. By design, a host would be considered professional if they have one listing in Denver and two listings in Atlanta.

Figure 6 describes the market composition of professional hosts and their active listings in Denver. Panel (a) shows the prevalence of casual and professional/commercial properties in Denver, and panel (b) shows a similar comparison for the average control city. Panel (c) shows the composition of host types in Denver. By 2017, approximately 14% of hosts were considered professional or commercial hosts.

I aggregate the data to the zip code level and count the number of total listings, casual listings, professional listings, and commercial listings. In all categories, I only include active listings. Table 5 reports the prevalence of Airbnb properties in zip codes before and after January 2017.

**Table 5** Summary Statistics of Airbnb Presence in Denver and Control Cities

| | **Denver** | | **Control** | |
|---|---|---|---|---|
| # Cities | 1 | | 14 | |
| # Zip Codes | 39 | | 407 | |
| | Pre | Post | Pre | Post |
| **(a) Listings per Zip Code** | | | | |
| Total | 48.5 | 87.6 | 21.1 | 46.5 |
| | (79.4) | (141.2) | (34.4) | (72.7) |
| Casual | 34.3 | 59.1 | 12.7 | 24 |
| | (55.6) | (93.2) | (19) | (31.9) |
| Professional | 2.6 | 5.5 | 1.1 | 3 |
| | (4.7) | (10.4) | (2) | (4.8) |
| Commercial | 2.8 | 7.6 | 2.6 | 9.2 |
| | (5.6) | (14.5) | (6.2) | (20.4) |
| **(b) Characteristics Per Listing** | | | | |
| Bedrooms | 1.71 | 1.85 | 1.85 | 1.87 |
| Occupancy Rate | 0.364 | 0.351 | 0.254 | 0.307 |
| Average Daily Rate | 181 | 199 | 220 | 227 |
| Monthly Reservations | 2.17 | 2.63 | 1.48 | 2.31 |

The pre-period consists of April 2015-December 2016. The post-period contains January 2017-December 2019. Standard errors are provided in parentheses. Panel A reports the average number of listings per zip code, and panel B reports the average descriptive statistic per listing.

**Figure 7** Short-Term Rental Citations in Denver, CO

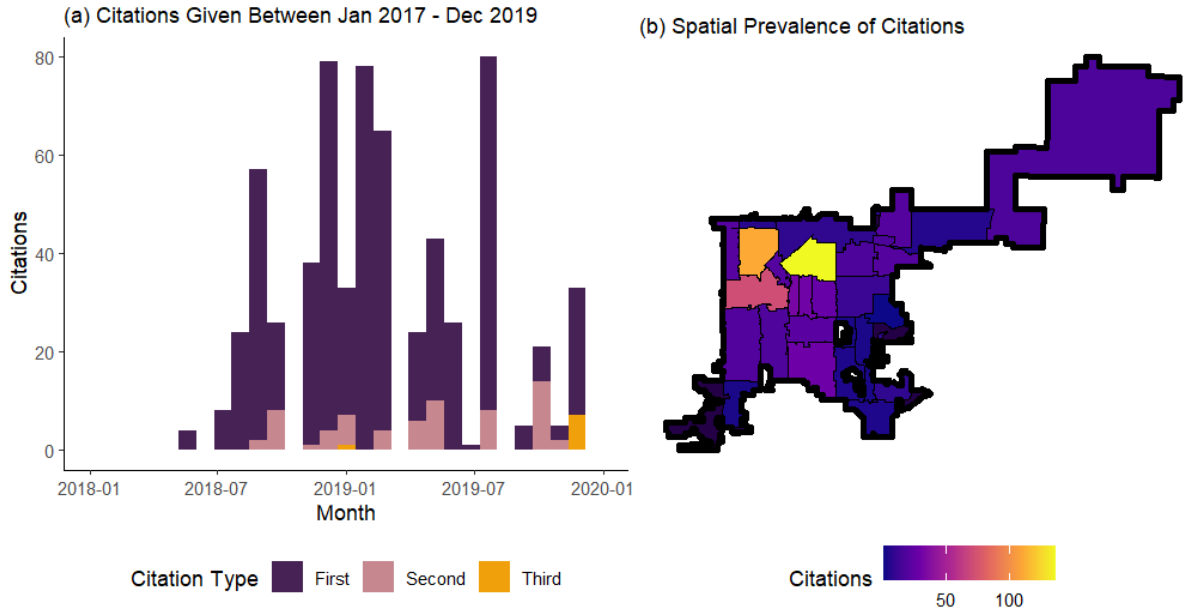



Image (a) shows the frequency of citations between January 2017 through December 2019. Prior to June 2018, there were no citations given. Image (b) illustrates the spatial frequency of total short-term rental citations between January 2017 and December 2019.

To explore the relationship between active policy enforcement and compliance, I use citations data from Denver's Department of Excise and Licenses. I include all paid citations that were mailed between January 2017 and December 2019. I observe the non-compliant property address, citation number, and recipient name. I match the address to latitude and longitude to identify the zip code associated with the citation. The first citation was not mailed until June 2018, and anecdotal evidence suggests that enforcement was limited during the first year and a half of regulations. During this time, there are a total of 807 citations written, 63% of which occurred in 2019. As seen in Figure 7, most of the citations are issued near Downtown Denver. To control for other relevant factors in the difference-in-differences analysis, I supplement the AirDNA and administrative citations data with relevant geographic and time varying trends. I include Google search trends for all included municipalities, as well as zip code demographics from the ACS 5-Year Estimates, which vary at the yearly level. I control for income, population,

age, and the number of housing units. I also use data from the Zip Codes Business Patterns to control for hospitality changes in the area, including scheduled passenger air transportation, traveler accommodation, drinking establishments, restaurants, and arts, entertainment and recreation services.

Finally, I analyze the effect of regulations on home and rental prices using data from Zillow. For long-term rental prices, I use the Zillow Observed Rent Index (ZORI) for all homes. I use the Zillow Home Value Index (ZHVI) to estimate housing prices for all homes. Both measures vary at the zip code level. I observe the number of home sales using Zillow's Sales Count. For each city, the data reports the number of unique properties sold each month. To estimate the number of long-term rental units on the market, I use American Community Survey housing vacancy and tenure data. Specifically, I define long-term rental stock as the total number of properties rented, vacant for rent, and rented but not occupied. This measure varies at the zip code level but is reported yearly.

### 3.5 Effects on Airbnb Supply

To study the impact of short-term rental regulations in Denver, I utilize AirDNA property data at the zip code level. I first estimate the direct effect of regulations Airbnb supply using Poisson fixed effects model. There are several zip codes that have no professional or commercial listings throughout the period, and the data follows a Poisson distribution, shown in Appendix Figure A2. After observing a reduction in the supply of Airbnb units following regulations, I test whether this is driven by hosts removing their property from the market Finally, I analyze whether active enforcement further reduces Airbnb supply. Existing literature suggests that individuals are most responsive when facing potential fines. I use administrative citations data to determine whether this is the case in Denver.

### 3.5.1 Direct Effect of Regulations on Supply

I begin by observing the trend in Airbnb supply for Denver and the control group. I present the trends in Figure 8, which graphs the number of listings per zip code for each group over time. Visually, there is no obvious decline in the number of casual or professional listings per zip code following the date regulations became effective. Compared to the average of control cities, commercial activity appears to be impacted by regulations.

**Figure 8** Trends in Airbnb Supply per Zip Code

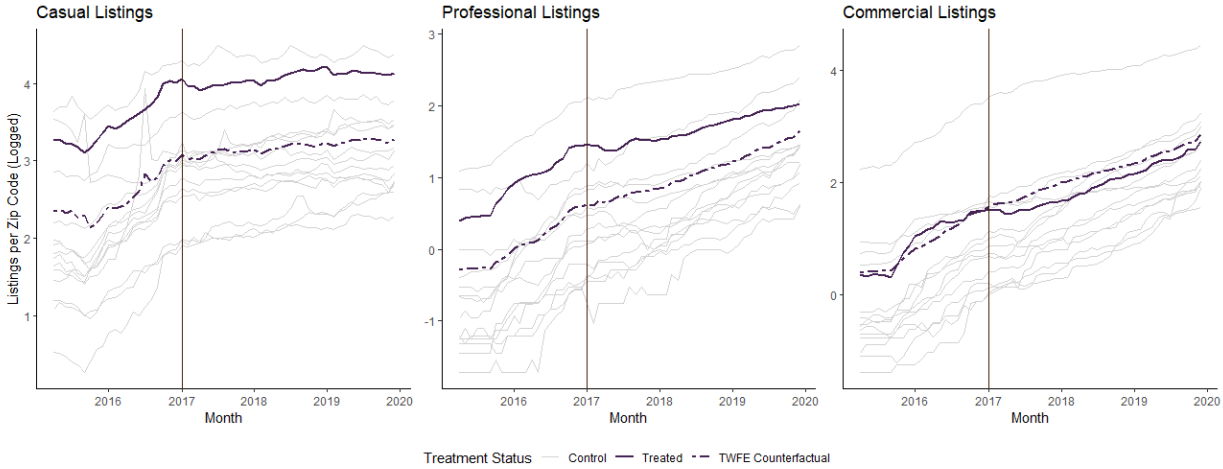



Figure 8 shows the trends in Airbnb supply per zip code over time for Denver, the control cities, and the average of the control cities. The left image shows the trend for casual listings, the middle image shows the trend for professional listings, and the right image shows the trend for commercial listings. The vertical line represents the date regulations became effective.

To identify the causal impact of regulations, I begin with a Poisson difference-in-differences analysis at the zip code level. There are 25 zip codes in the Denver municipality, all of which are treated by the short-term rental regulations. I distinguish between the date regulations passed and the date they became effective to capture policy anticipations. I am primarily interested in the effect of regulations on the prevalence of different types of Airbnb listings, so I distinguish supply as professional, commercial, or casual. Given the primary

residence condition, I expect to see a large reduction in professional and commercial listings. The direct effect of regulations on supply is given by $\beta_2$ in the following equation:

$$supply_{zt} = \beta_1 pass_{zt} + \beta_2 effective_{zt} + \delta X_{zt} + \gamma_z + \gamma_t + \varepsilon_{zt} \quad (3)$$

where the number of Airbnb listings in a zip code during month *t* is reported as $supply_{zt}$. For zip codes in Denver, $pass_{zt} = 1$ between June 2016 and January 2017. Likewise, $effective_{zt} = 1$ for Denver zip codes for all months after January 2017. $X_{zt}$ captures zip code and time-varying controls. $\gamma_z$ and $\gamma_t$ report zip code and time fixed effects, respectively.

**Table 6** Effect of Denver Regulations of Active Airbnb Supply

| | (1) **Total** | (2) **Casual** | (3) **Professional** | (4) **Commercial** |
|---|---|---|---|---|
| **(a) All Listings** | | | | |
| Pass | -0.081 | -0.026 | -0.087 * | -0.189 *** |
| | (0.046) | (0.069) | (0.037) | (0.041) |
| Effective | -0.279 *** | -0.078 | -0.272 *** | -0.383 *** |
| | (0.063) | (0.083) | (0.043) | (0.077) |
| N | 22971 | 22515 | 20007 | 19494 |
| **(b) Listings Created Before June 2016** | | | | |
| Pass | -0.057 | -0.011 | -0.048 | -0.102 *** |
| | (0.039) | (0.063) | (0.031) | (0.029) |
| Effective | -0.207 *** | -0.138 * | -0.083 | -0.365 *** |
| | (0.042) | (0.057) | (0.053) | (0.051) |
| N | 20007 | 19152 | 14136 | 14877 |

*** p < 0.001; ** p < 0.01; * p < 0.05.

The above table reports the estimates for regulations on four measures of Airbnb supply. Pass equals one between the date regulations are announced (June 2016) and the date they become effective (January 2017). Effective equals one for all months after January 2017. All regressions include month fixed effects and zip code fixed effects. Standard errors are clustered at the city level and are reported in parentheses. Coefficients are estimated using a Poisson model.

The total number of Airbnb units falls significantly after regulations, driven primarily by professional and commercial listings. Table 6 reports the results of my preferred specification in Panel A, where I use a Poisson fixed effects model and include controls for variables that change at the zip code and time level. In the three years following regulations, total active Airbnb supply

falls by 24.3%, translating to roughly 12 listings per zip code. The presence of casual listings is not significantly affected, but professional listings fall by 23.8% and commercial listings fall by 31.8%. There is a slight anticipation effect for professional listings and a significant and larger anticipation effect for commercial listings.

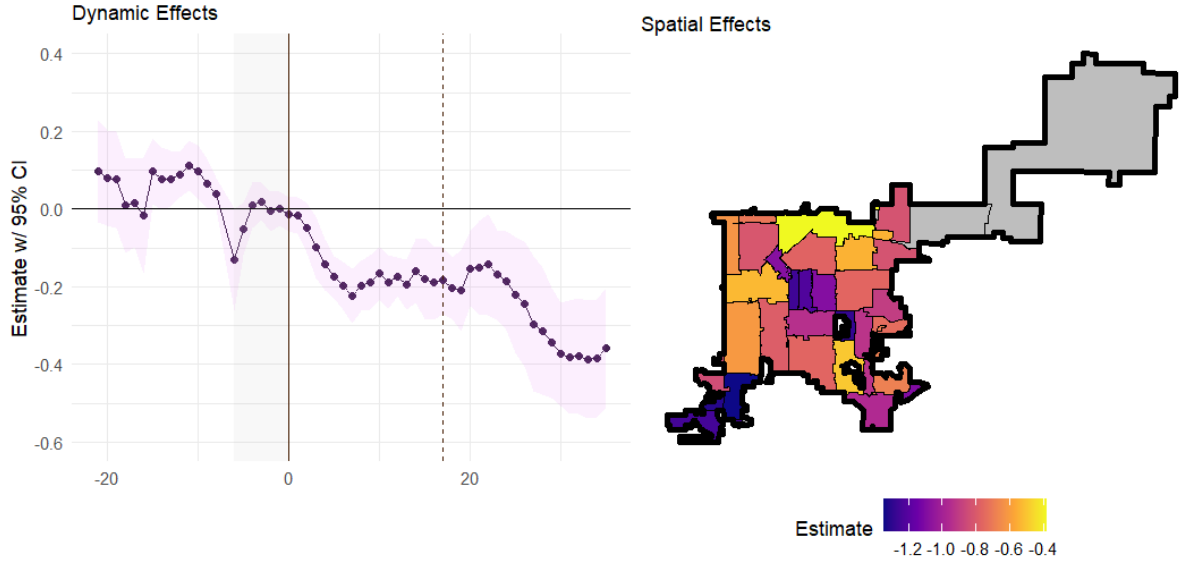
This reduction can be driven by two possible responses. Existing hosts could have observed regulations and chose to remove their listing from the market permanently, or property owners who would've joined Airbnb at some point were deterred from entering the market Panel b reports the estimates when I restrict the sample to listings that had existed prior to June 2016. The number of pre-existing professional listings did not significantly change, but a large percentage of commercial listings exited the market.

The effect of regulations on supply was increasingly negative over time, as shown in Figure 9. There was an initial, but not significant, dip in supply when regulations were announced, but there was no significant anticipation effect. Once regulations took effect, the total supply of active Airbnb properties fell steadily, until it leveled off after roughly six months of regulations. The dashed, vertical line represents the point at which Denver began to issue citations for non-compliance. Visually, it's possible that citations caused an additional reduction in supply, but the estimate is noisy. Within Denver, the largest supply reduction occurred near the downtown area.

The supply of Airbnb listings was significantly reduced following regulations, with the reduction driven by commercial listings exiting the market and an overall decline in the number of potential professional and commercial listings entering. The primary residence condition effectively targeted the property type policy makers were concerned with. The number of casual listings as unaffected, suggesting that the financial costs associated with registration did not deter

casual property owners from operating. I am unable to test whether these listings are registered with the city – it is possible that casual listings were not compliant in that sense and did not absorb the costs of registration.

**Figure 9** Effect of Regulations on Airbnb Supply




The left image in Figure 9 provides an event study of regulations on the total number of active Airbnb supply. The shaded region represents the period between the passing of the ordinance and regulations taking effect. The vertical, dashed lined represents the month in which Denver began to issue citations. The right image provides an analysis where *effective* is interacted with zip code, associated with Equation 3.

### 3.5.2 Effect of Regulations on Listing Status

I turn my analysis to the effect of regulations on the status of individual Airbnb listings. For each listing, I determine whether the listing has permanently exited the market, whether it is active, and whether it is operating as a medium-term rental. I determine whether a listing operates as a medium-term rental during a given month using reservation patterns, so I cannot explicitly state whether a listing is a committed MTR. Instead, I estimate the effect of regulations on the probability the listing operates as a medium-term rental that month, conditional on accepting a reservation. I estimate the effects with the following two-way fixed effects equation:

$$y_{it} = \beta_{1-3} treated_{it} * host.type_{it} + \delta X_{it} + \gamma_i + \gamma_t + \varepsilon_{it} \quad (4)$$

where $y_{it}$ is an indicator for whether the property has exited, is active, or is a medium-term rental. $\beta_{1-3}$ captures the separate treatment effect of regulations for casual hosts, professional hosts, and commercial listings. I control for property and time fixed effects, as well as confounding variables. I expect that the treatment effect is negative, especially for professional and commercial listings. Table 7 presents the average treatment effect across various outcomes.

**Table 7** Effect of Denver Regulations on a Listing's Active Status

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | **Exit** | **Exit** | **Exit** | **Active** | **MTR** |
| Pass | 0.018 * | 0.018 | 0.018 * | -0.028 | 0.016 * |
| | (0.006) | (0.010) | (0.006) | (0.015) | (0.006) |
| Effect | 0.066 *** | 0.316 *** | | | 0.039 *** |
| | (0.013) | (0.012) | | | (0.005) |
| Effect X Log(Rev) | | -0.038 *** | | | |
| | | (0.000) | | | |
| Effect X Casual | | | 0.064 *** | -0.101 *** | |
| | | | (0.013) | (0.016) | |
| Effect X Prof. | | | -0.007 | 0.157 *** | |
| | | | (0.013) | (0.018) | |
| Effect X Comm. | | | 0.168 *** | 0.093 *** | |
| | | | (0.013) | (0.016) | |
| N | 2277138 | 742595 | 2277138 | 1642544 | 743068 |
| R2 | 0.661 | 0.675 | 0.662 | 0.478 | 0.201 |

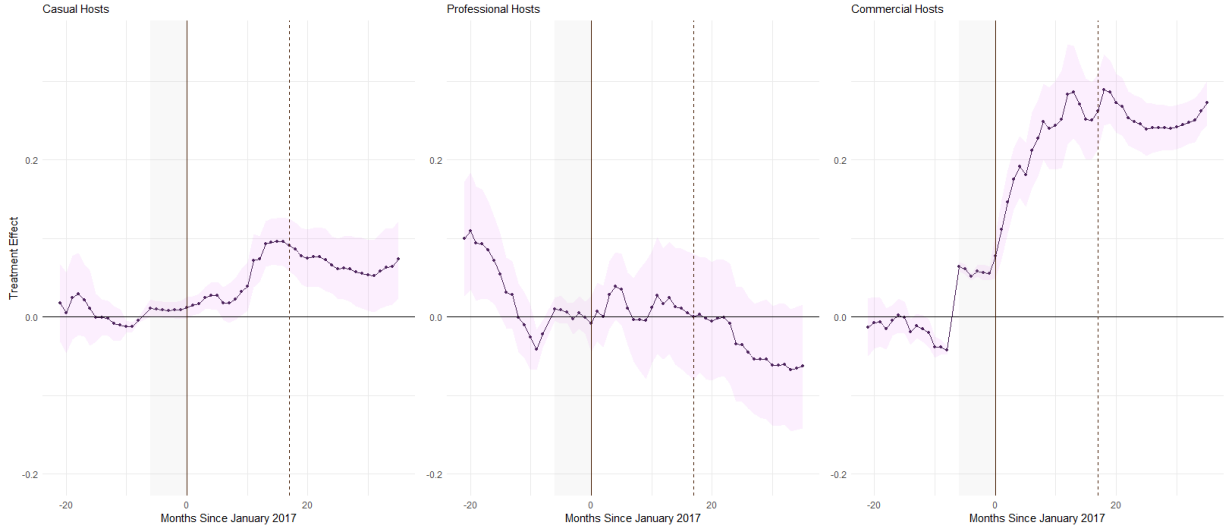
*** p < 0.001; ** p < 0.01; * p < 0.05.

All regressions include month fixed effects and zip code fixed effects. Standard errors are clustered at the city level and are reported in parentheses. Coefficients are estimated using a linear probability model.

Column 1 reports the effect on the probability a listing exits the market. On average, a listing had a 6.6 percentage point increase in the probability that it exited the market In Column 2, I interact treatment with the listing's average monthly revenue during the period before June 2016. At the average pre-regulation monthly revenue, a listing had a 5.4 percentage point increase in the probability of exit. The effect of treatment is decreasing as pre-revenue increases,

demonstrating that profitable STRs were least likely to respond. Column 3 reports the effect of regulations on exit for different property types. I observe a significant increase in the probability that a casual or commercial listing exited the market, but there was no effect for professional listings. Commercial properties had the largest response to regulations, with a 16.8 percentage point increase in the probability of exit. The response was immediate, with the probability increasing after the ordinance was announced, as seen in Figure 10. Casual listings had a 6.4 percentage point increase, which became significant roughly one year after regulations have taken effect.

**Figure 10** Effect of Regulations on the Probability a Listing Permanently Exits the Market




The images above provide event studies of treatment on the total number of casual, professional, and commercial Airbnb supply, all of which are active. The shaded region represents the period between the passing of the ordinance and regulations becoming effective. The vertical, dashed lined represents the month in which Denver began to issue citations. May 2016 is the reference month.

Column 4 of Table 7 reports the impact that regulations have on whether a listing is active. Conditional on having continued to exist in the market, casual listing were 10.1 percentage points less likely to be active in a given month. This suggests that casual properties could have pivoted towards responsive hosting, where homeowners list their home in response to an expected demand shock. Professional and commercial properties were significantly more

likely to be active. Finally, as shown in Column 5, listings were 3.9 percentage points more likely to operate as a medium-term rental in a given month. Medium-term rentals were allowed regardless of primary residence, so hosts may have changed their reservation patterns to continue operating in the market

Overall, the listing-level analysis suggests that high revenue properties were unlikely to respond to regulations. Interestingly, casual hosts were more likely to exit the market following regulations, and remaining casual properties were not as active. Professional and commercial properties that continued to operate in the market were generally more active and are more likely to act as a medium-term rental.

### 3.5.3 Active Enforcement and Citations

Despite the relative reduction in professional and commercial Airbnb supply, a substantial set of non-compliant properties continue to operate in Denver. Between January 2018 and December 2019, approximately 1,360 properties and 444 hosts were considered professional or commercial. As part of the Denver ordinance, properties that operate as a short-term rental without a valid license can face administrative citation fines. The first citation given for non-compliance is $150, and reoccurring offenses cost up to $999 at the maximum. The fines were outlined in the ordinance when it passed, but the first citation was not given until June 2018, a year and a half after regulations had been effective. Existing literature suggests that regulations are not effective until the city actively enforces the policy. I use administrative citations data from Denver to determine whether citations cause an additional reduction in supply. I run the following analysis described by Equation 5, using a probit model.

$$exit_{it} = \beta_1 pass_{it} + \beta_2 effective_{it} + \beta_3 citations_{zt} + \delta_{zt} + \gamma_i + \gamma_t + \varepsilon_{it} \qquad (5)$$

As before, I distinguish between the date the ordinance passed and the date regulations became effective. Additionally, I account for the relevance of citations in that zip code by reporting either the number of cumulative citations given by that month, or by indicating whether a citation has occurred in that zip code, reported by $citations_{zt}$. If observed enforcement effects supply, $\beta_3$ should be negative, especially for commercial or professional properties. Table 8 provides the results of the analysis. Results indicate that citations do not have an additional effect on Airbnb supply.

**Table 8** Effect of STR Citations on Exit Probability

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Citation Type | **Cumulative** | **Cumulative** | **Indicator** | **Indicator** |
| Pass | 0.018 * | 0.018 * | 0.018 * | 0.018 * |
| | (0.006) | (0.006) | (0.006) | (0.006) |
| Effect | 0.068 *** | | 0.063 *** | |
| | (0.012) | | (0.012) | |
| Effect X Casual | | 0.063 *** | | 0.057 *** |
| | | (0.012) | | (0.012) |
| Effect X Prof. | | 0.024 | | 0.044 ** |
| | | (0.013) | | (0.012) |
| Effect X Comm. | | 0.191 *** | | 0.191 *** |
| | | (0.013) | | (0.012) |
| Citations | -0.000 ** | | 0.008 | |
| | (0.000) | | (0.005) | |
| Citations X Casual | | -0.000 | | 0.016 ** |
| | | (0.000) | | (0.005) |
| Citations X Prof. | | -0.002 *** | | -0.110 *** |
| | | (0.000) | | (0.005) |
| Citations X Comm. | | -0.002 *** | | -0.049 *** |
| | | (0.000) | | (0.006) |
| N | 2277138 | 2277138 | 2277138 | 2277138 |
| R2 | 0.661 | 0.662 | 0.661 | 0.662 |

*** p < 0.001; ** p < 0.01; * p < 0.05.

All regressions include month fixed effects and zip code fixed effects. Standard errors are clustered at the city level and are reported in parentheses. Coefficients are estimated using a linear probability model. Citations are reported as either cumulative citations in a zip code or as an indicator for the zip code having ever received a citation.

Columns 1 and 2 report the estimates when using cumulative citations as the explanatory variable. Column 1 indicates that regulations had no effect on a listing's probability of exit, but

column 2 suggests that increase citations lower the probability of exit for professional and commercial listings. It's possible that there is an endogenous relationship between the prevalence of commercial properties and number of citations, so I also use an indicator variable that equals one for all months after the zip code has received a citation in Columns 3 and 4. Again, citations seemed to have a negative impact on a listing's probability of exit. This case could be capturing the growing reduction from regulations taking effect. Together, the evidence suggests that citations did not have a significant effect on the likelihood of exit. This is consistent with the earlier event studies, in which supply did not seem to respond further following the moment Denver begins to issue citations.

Using expected value theory, I theorize that a non-compliant host will continue to engage in non-compliant behavior when the expected benefit of doing so outweighs the expected cost. Fines for non-compliance start at $150 and are capped at $999. Airbnb listings in high-demand areas often generate a large profit. A property that operates as a short-term rental and earns a large monthly profit might not be concerned about a relatively low fine if they are caught. In 2018, the average monthly revenue for a listing in Denver was $1,675, associated with an average of 9 reservations day each month. In addition to the low fines relative to expected profits, the policy seemed to be minimally enforced in the first 2.5 years of the regulations.

Figure 11 highlights this trend by estimating the number of active short-term rentals that were operating in violation of the policy. By the time Denver began issuing citations, many properties were avoiding detection. This lapse in enforcement could have stemmed from difficulties with monitoring the website, or it could have been a consequence of city tax incentives. Denver imposes a 10.75% Lodger's Tax on all booked nights for short-term

accommodations. During peak demand in 2019, professional and commercial properties generated nearly $400 thousand in tax revenue for the city.

**Figure 11** Low Policy Enforcement in Denver

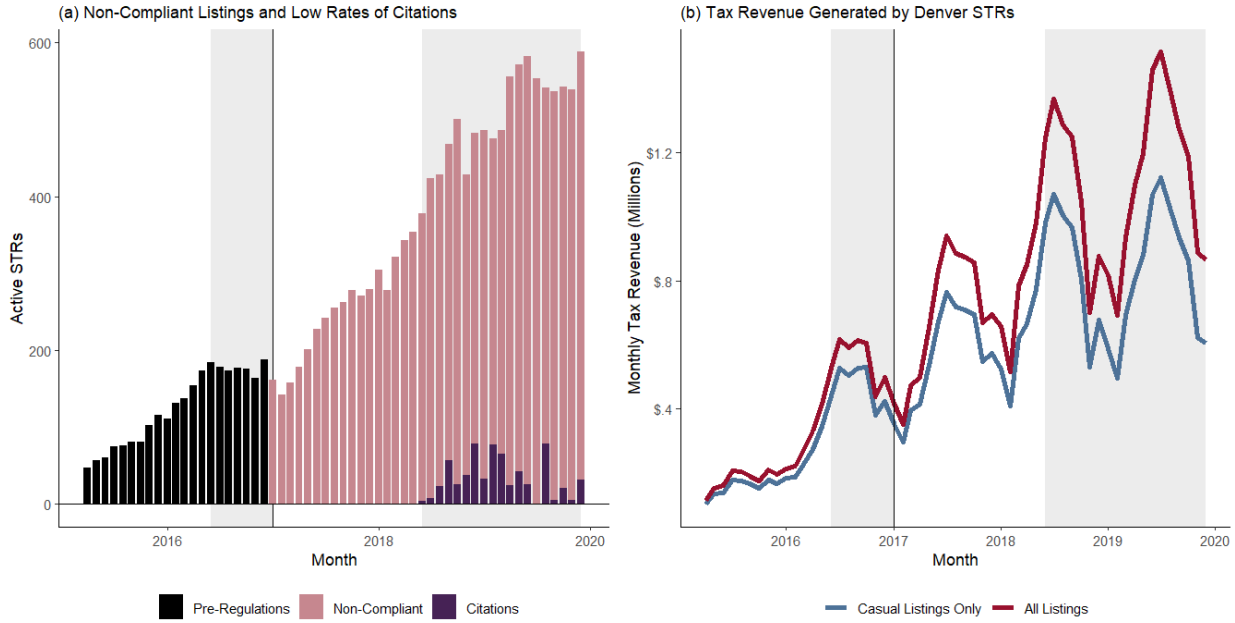



Image (a) shows the number of Airbnb properties that are considered non-compliant are behaving as a short-term rental each month. Included properties must have accepted at least one reservation to be considered an STR. The vertical line represents January 2017. After regulations take effect, the number of citations each month are overlayed the non-compliant listings. Image (b) shows the estimated monthly tax revenue generated Airbnb reservations, based on a Lodger's Tax of 10.75%. The difference between revenue generated by all listings and by casual listings represents the monthly revenue generated by professional and commercial properties.

### 3.6 Other Outcomes

Overall, Airbnb supply falls by 24.3% in Denver. Regulations shift the supply curve left as properties exit the market This changes the equilibrium in Denver and could have effects on prices, as well as other outcomes connected to the Airbnb market I expect that the average daily rate of STRs should increase if demand remains unchanged. Additionally, if hosts remove a listing from the STR market, they could either enter the long-term rental market or sell the home. This could have an effect on home prices as well.

### 3.6.1 Airbnb Performance

The short-term rental regulations cause a significant reduction in the supply of Airbnb in Denver. This could affect market outcomes in a few possible ways. The increase in costs associated with supplying a listing could put upward pressure on prices. Additionally, a shift left in the supply curve would result in higher prices, assuming no change in demand. It's possible that a reduction in supply could reduce the demand for STRs in Denver through decreased variety and a change in the type of available listings. In this case, prices could increase or decrease depending on the magnitude of the supply and demand shift. The following section discusses the impact of regulations on the general equilibrium, as well as the economic performance of remaining listings that operate in Denver. I use the following two-way fixed effects equation to estimate the effect on equilibrium outcomes:

$$\log(y_{zt}) = \beta_1 pass_{zt} + \beta_2 effective_{zt} + \delta X_{zt} + \gamma_z + \gamma_t + \varepsilon_{zt} \quad (4)$$

where $y_{zt}$ reports the following: the average daily rate, number of monthly reservations, monthly reservations per active listing, total monthly revenue, or monthly revenue per active listing. I run the analysis at both the property level and the zip code level. Table 9 reports the estimates for each outcome. Column 1 reports the effect of regulations on total reservations booked and revenue generated for each zip code. Column 2 reports the effects on reservations, ADR, and revenue generated per active listing in each zip code. Columns 3 reports the effect of regulations on property level outcomes, and Column 4 explores heterogeneity across property type.

As seen in panel (a) of Table 9, regulations reduced the total number of reservations booked in Denver by 28.9%. This effect seemed to be driven by an absence of supply – when I observe the effect of regulations on reservations per active listing, I see no significant change.

**Table 9** Economic Performance of Remaining Properties in Denver

| | (1) Zip Code - Total | (2) Zip Code - Per Listing | (3) Listing Level | (4) Listing Level |
|---|---|---|---|---|
| **A. Reservations** | | | | |
| Pass | 0.204 ** | 0.194 ** | 0.052 | 0.050 |
| | (0.056) | (0.051) | (0.034) | (0.034) |
| Effective | -0.341 *** | -0.110 | -0.060 | -0.090 |
| | (0.071) | (0.052) | (0.058) | (0.058) |
| Effect x Prof. | | | | 0.209 *** |
| | | | | (0.005) |
| Effect x Comm. | | | | 0.199 *** |
| | | | | (0.004) |
| N | 10939 | 10939 | 557199 | 557199 |
| R2 | 0.922 | 0.634 | 0.526 | 0.526 |
| **B. Average Daily Rate** | | | | |
| Pass | | 0.026 | -0.009 | -0.009 |
| | | (0.027) | (0.024) | (0.024) |
| Effective | | 0.007 | -0.039 | -0.037 |
| | | (0.030) | (0.020) | (0.020) |
| Effect x Prof. | | | | -0.024 *** |
| | | | | (0.003) |
| Effect x Comm. | | | | -0.012 *** |
| | | | | (0.001) |
| N | | 10939 | 557199 | 557199 |
| R2 | | 0.681 | 0.922 | 0.922 |
| **C. Revenue** | | | | |
| Pass | 0.232 ** | 0.222 ** | 0.056 | 0.055 |
| | (0.065) | (0.062) | (0.057) | (0.057) |
| Effective | -0.238 ** | -0.007 | -0.064 | -0.084 |
| | (0.066) | (0.070) | (0.082) | (0.082) |
| Effect x Prof. | | | | 0.094 *** |
| | | | | (0.005) |
| Effect x Comm. | | | | 0.173 *** |
| | | | | (0.005) |
| N | 10939 | 10939 | 557199 | 557199 |
| R2 | 0.906 | 0.553 | 0.568 | 0.568 |
| **D. Booked Days** | | | | |
| Pass | | 0.183 ** | 0.065 | 0.064 |
| | | (0.051) | (0.033) | (0.033) |
| Effective | | -0.036 | -0.025 | -0.047 |
| | | (0.057) | (0.064) | (0.064) |
| Effect x Prof. | | | | 0.117 *** |
| | | | | (0.004) |
| Effect x Comm. | | | | 0.184 *** |
| | | | | (0.004) |
| N | | 10939 | 557199 | 557199 |
| R2 | | 0.495 | 0.400 | 0.400 |
| Month FE | Yes | Yes | Yes | Yes |
| Property FE | No | No | Yes | Yes |
| Zip Code FE | Yes | Yes | No | No |
| MOY-City FE | Yes | Yes | No | No |

*** p < 0.001; ** p < 0.01; * p < 0.05.

The above table reports the estimates for regulations on listing performance. All regressions include month fixed effects and zip code fixed effects. Standard errors are clustered at the city level and are reported in parentheses. Coefficients are estimated using a TWFE model. Column 4 reports the estimate relative to casual properties.

Remaining professional and commercial properties experienced a weakly significant increase in the number of reservations booked, 12.6% and 11.5% , suggesting that remaining properties capture the displaced demand. Generally, I see no effect of regulations on overall STR prices in the market, shown in panel (b)[8]. However, the price for professional properties fell by 5.9% (almost $12 a day), and prices for commercial properties fell by 4.8% ($13 a day). This indicates that the market had a surplus of supply, as a supply shift at a market clearing equilibrium would have increased prices. The reduction was minimal and likely caused by less competition. It's possible that enough properties exited the market in a way that caused demand to fall in response to lower variety in accommodation options.

Overall, total revenue generated per zip code fell by 16.8%. Again, this is likely due to the reduction in supply, as the revenue generated per listing was unchanged. There was a modest increase, 9.5%, in the revenue generated by commercial properties. Additionally, the number of days booked by remaining commercial listings increased by 14.7%. Altogether, demand for short-term rentals continued to result in booked nights, despite the reduction in properties.

Regulations do not affect the economic performance of casual listings that operate in Denver, but evidence shows that remaining commercial properties are better off from decreased competition. With less competition, commercial properties can capture a larger share of the STR demand. Remaining commercial properties benefited the most through increased reservations and revenue. Even with the increased number of bookings, commercial properties have lower daily rates following regulations, suggesting that demand responds negatively to reduced options.

[8] The data source, AirDNA, defines average daily rate as the total revenue generated by the total days booked. Revenue is constructed using the daily rate, cleaning fees, and additional fees included in the cost of booking.

### 3.6.2 Housing Prices

Given that regulations were passed with the intention to protect affordable housing, I now explore the potential effects that regulations may have on home prices and supply. A reduction in commercial properties might shift properties to long-term use instead, increasing long-term supply and putting downward pressure on rental prices. Alternatively, property owners could choose to sell the home. I use two-way fixed effects and synthetic differences-in-differences to evaluate the effect of regulations on housing.

Results in Table 10 show that regulations do not cause a change in home prices when using TWFE (column 1), but under SDID (column 2), home prices fall by 3.6%. Regulations could affect prices in a few ways. It's possible that restricting the potential options for a property could reduce the option value of housing. Alternatively, existing hosts might sell their property, or potential investors might not be attracted to the investment option and choose not to purchase property in Denver. I test whether regulations cause a change in the sale of homes. Zillow reports home sales counts at the city level, so I am unable to explore heterogeneity along the zip code dimension. However, in both the TWFE and the SDID analysis, there is a negative effect of regulations on the number of home sales. Column 3 and 4 report that regulations caused an 11.6% reduction in home sales and a 19.0% reduction in lagged home sales, respectively. The SDID analysis shows a smaller negative estimate. Because there is only one treated unit in this analysis, the standard errors are estimated with the Placebo method. Consequently, the reported standard errors are most likely larger than the true standard error. This reduction suggests that investors could have pivoted away from purchasing homes with the intention to list them as vacation rentals.

**Table 10** Effect of Denver Regulations on Housing Market

| | (1) ZHVI | (2) ZHVI | (3) Sales | (4) Lag Sales | (5) Sales | (6) ZORI | (7) ZORI | (8) LTR Stock | (9) Lag LTR Stock | (10) LTR Stock |
|---|---|---|---|---|---|---|---|---|---|---|
| | TWFE | SDID | TWFE | TWFE | SDID | TWFE | SDID | TWFE | TWFE | SDID |
| Pass | 0.006 | | -0.033 * | -0.078 ** | | -0.002 | | -0.002 | 0.055 *** | |
| | (0.007) | | (0.015) | (0.020) | | (0.004) | | (0.002) | (0.011) | |
| Effect | 0.002 | -0.037*** | -0.123 *** | -0.198 *** | -0.058 | -0.020 * | -0.002 | -0.009 | 0.164 *** | 0.018 |
| | (0.017) | (0.007) | (0.023) | (0.033) | (0.056) | (0.009) | (0.013) | (0.006) | (0.034) | (0.015) |
| City FE | No | No | Yes | Yes | Yes | No | No | No | No | No |
| Zip Code FE | Yes | Yes | No | No | No | Yes | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 12880 | | 798 | 797 | | 7022 | | 13035 | 12957 | |
| R2 | 0.995 | | 0.989 | 0.958 | | 0.995 | | 0.998 | 0.928 | |
| Control Units | | 223.2/397 | | | 13.8/14 | | 51.5/93 | | | 394.7/397 |
| Control Periods | | 1.0/23 | | | 4.6/23 | | 1.0/23 | | | 1.0/3 |

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

The above table reports the effect of regulations on home prices, home sales, and long-term rents. Pass equals one between the date regulations are announced (June 2016) and the date they become effective (January 2017). Effective equals one for all months after January 2017. All regressions include month fixed effects and zip code or city fixed effects. Standard errors are reported in parentheses. For columns 1, 3, 4, 6, 8, and 9, standard errors are clustered at the city level. Standard errors in columns 2, 7, and 10 are estimated using the jackknife method, and standard errors in column 5 are estimated using the placebo method. Lag variables are lagged by one month.

There was a modest effect on the observed rent index. On average, the monthly rent fell by around 2%, reported in column 6. Column 7 reports the estimate under the SDID method, which is not significant. The ZORI data is somewhat inconsistent in the sense that Zillow begins reporting the rent index for zip codes in different months. Because SDID does not handle missing values well, I remove any zip code that has missing values at any point. This significantly reduces the sample size. For this reason, I prefer TWFE.

**Figure 12** Effect of STR Regulations on LTR Market

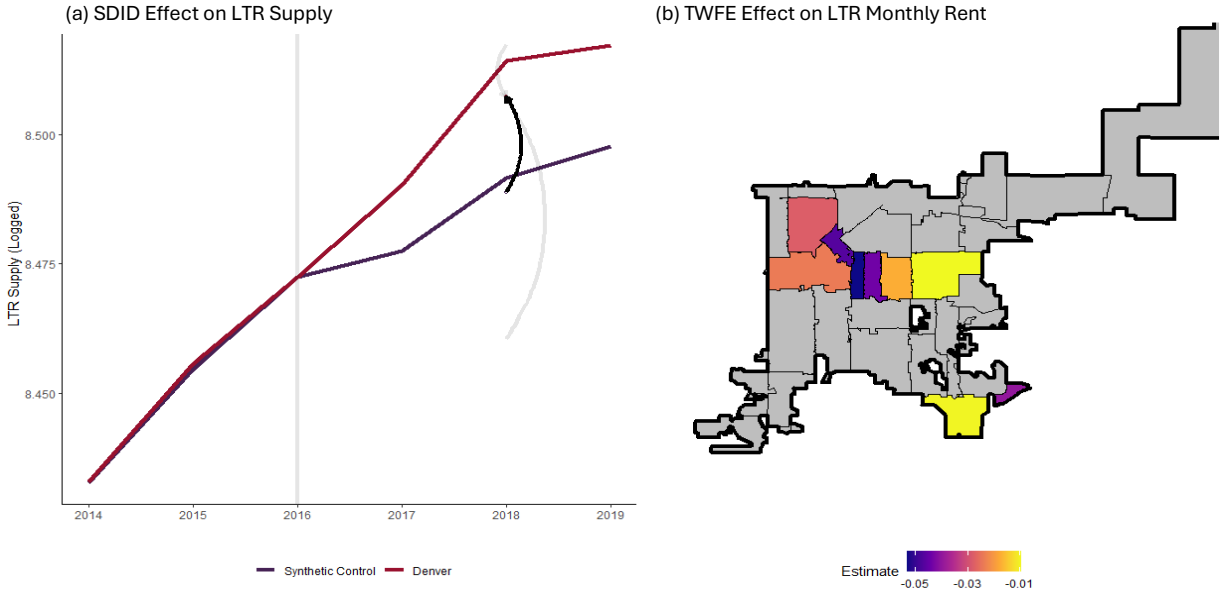



Image (a) shows the synthetic difference-in-differences estimated effect of regulations on long-term rental supply. The vertical line represents the year prior to regulations taking effect, and the arrow represents the treatment effect. The synthetic control has been shifted down to compare pre-trends. Image (b) shows the spatial effects of regulations on monthly rental prices, as measured by ZORI.

It's possible that landlords were moving away from the STR market and participating in the long-term rental instead. This substitution would increase LTR supply and reduce the prices. I test whether this is the case by estimating the impact of regulations on long-term rental supply, as reported by the American Community Survey. Using the total number of properties that are renter occupied, rented but not occupied, and vacant for rent, I observe a lagged increase in in the

long-term rental inventory, shown in column 9, and an insignificant but positive effect on LTR inventory using an SDID estimation in column 10. Figure 12 visualizes the SDID estimated effect of regulations, relative to the control cities. This positive shift in LTR supply could explain the slight reduction in rent prices. Having established this change in supply and prices, I break the effects up across zip code. I observe the largest reduction in monthly rents located near Downtown Denver. The average zip code experienced a $30 reduction in monthly rents. The results from the earlier spatial analysis on supply (Figure 9) show that the largest reduction in supply was concentrated Downtown.

### 3.7 Conclusion

To address the growing concerns regarding possible negative externalities associated with short-term rentals, Denver passed Ordinance No. 262-16 to regulate the industry. One unique element of the regulations is the primary residence condition, which, in theory, limited each Denver host to one property in Denver. We might expect that this reduced the number of investment properties operating as dedicated short-term rentals. I expand on the existing literature by investigating the direct effect of regulations and enforcement on Airbnb supply, and the indirect effects of the ordinance on housing costs. I use a Poisson fixed-effects estimation strategy to quantify the direct effect, and I show that regulations significantly reduced total active supply relative to the control cities. This effect is significant despite an inconsistent enforcement mechanism.

The effects of STR regulations were primarily concentrated on commercial listings in Downtown Denver. This is a function of the primary residence condition, which explicitly aimed to prevent commercial listings from operating in the market Immediately following the date that

regulations were announced, the probability that a commercial listing would permanently exit the market increased by nearly 9 percentage points.

Following regulations, there were non-compliant properties that continued to operate in Denver. The enforcement mechanism used by Denver in the first 18 months of regulations was mostly ineffective. Immediately following regulations, there were no instances of citations for non-compliant listings. The city began issuing citations a year and a half later, with the largest concentration of citations occurring in the zip codes with a large prevalence of professional and commercial listings. After this increase in the number of citations, non-compliant properties did not appear to respond to the threat of a fine. Between the low rates of citations and the high revenues generated through Airbnb, the expected value of operating in the market exceeded the expected cost of receiving a citation.

Despite the reduction in commercial properties, there were still notable levels of non-compliant listings that operated in the market The remaining commercial properties were positively affected by reduced competition. There was a modest increase in monthly revenue, monthly reservations, and reservation days for active commercial properties. Interestingly, the average daily rate of commercial listings fell by around $13 a day. This result suggests that demand for short-term rentals is inelastic, and that less variety of accommodation options could shift demand to the left.

One important goal of the Denver regulatory framework was to protect affordable housing. If regulations reduce the supply of commercial listings, we might expect that properties were sold or leased to the long-term rental market Using data from Zillow, I show that regulations reduced rental prices by 2%. Further testing suggests that this could have occurred through an increase in the supply of long-term rentals. I do not observe a significant effect of

regulations on housing prices. This is likely because the effects of regulations were primarily concentrated in the downtown area, and properties owners may have shifted towards the long-term rental market instead of selling the property.

In this paper, I provide evidence that regulations in Denver did reduce STR commercial activity. Using 14 control cities, I observe a reduction that was driven by casual and commercial listings exiting the market, as well as potential professional listings not entering. The equilibrium price of Airbnb did not change, suggesting that demand for STRs is elastic. Even more, regulations reduced the price of long-term rentals, concentrated in Downtown Denver. Evidence suggests the price reduction was driven by increased long-term rental stock. Despite the non-compliance that occurred following regulations, the primary residence condition did relieve housing strains in high-demand areas.

# CHAPTER IV

# INSTITUTIONAL EXPOSURE TO VIOLENCE AND THE SPILLOVER EFFECTS ON FINANCIAL OUTCOMES IN MEXICO

This chapter is co-authored with Alfredo Burlando, Lucia Dalla Pellegrina, and Josephine Kim.

## 4.1 Introduction

Access to stable financial institutions has important implications for financial inclusion (Armendáriz and Morduch, 2005; Cull et al., 2007, 2009), poverty alleviation (Banerjee et al., 2015; Khandker, 2005), and impacts on small and medium enterprises (Beck et al., 2005; Detotto and Otranto, 2010). Existing contributions, particularly within the microfinance field, predominantly concentrate on the direct impact of instability on consumers. However, little research has been done regarding the indirect effects of institutional instability on outcomes in locations serviced by institutions experiencing a negative shock.

This paper extends the literature by investigating the impact of crime and insecurity on the retail banking sector in Mexico. Specifically, it examines how elevated crime rates influence banks' operational dynamics, including transaction volumes, credit risk, and overall profitability. The study delineates the effects into direct impacts (banks in high-crime areas) and indirect impacts (banks within low-crime areas with branches connected to high-crime regions). This exploration into the banking sector's fragility due to external crime shocks introduces a novel dimension to the existing body of literature.

Our study capitalizes on the exogenous increase in crime rates triggered by Mexico's 2007 war on drugs. This unique context provides an invaluable opportunity to analyze the financial sector's responses to heightened insecurity. The research uniquely scrutinizes the intersection of crime and financial market stability, emphasizing the geographical relationship between crime rates and banking operations. Utilizing data from Mexico spanning 2001-2011, we employ a shift-share regression methodology (Bartik, 1991) to disentangle local crime shocks from direct effects of crime in a municipality and indirect effects on banks' activities through banking networks. This methodological approach represents a significant contribution, differing from traditional methodologies employed in prior studies.

Mexico's dramatic rise in crime rates, fueled by cartel competition for markets and resources, led to substantial heterogeneity in crime incidence across the country. This variation facilitates the construction of a shift-share regression methodology that leverages spatial and temporal heterogeneities in crime rates to identify local crime shocks and their consequences. We exploit the spatial variation in the local market share of financial institutions to identify variation in institutions' exposure to crime.

Our empirical findings reveal a pronounced effect on bank branches' activities, with an overall reduction in scale due to increasing risk in areas experiencing significant crime surges. More concretely, we show that, on the one hand, municipalities served by financial institutions with direct exposure to crime (due to multiple branches in high-crime areas) suffer from decreased deposits, credit, branch numbers, and ATM points. On the other hand, branches in safer areas likely experience indirect risk exposure due to the presence of high-risk branches within the same bank.

This dynamic demonstrates that crime-induced insecurity precipitates substantial adjustments in banking operations, affecting the spatial distribution of financial services. These findings shed light on the transmission mechanism of shocks within banks' branch networks and the broader economic implications of crime on the financial sector's stability and operational strategies.

Overall, this study contributes to the literature by elucidating the nuanced relationship between crime and the financial sector, particularly the transmission process between bank branches. It underscores how indirect effects of crime-induced insecurities in branches with significant crime exposure spill over across regions due to intra-bank risk-sharing mechanisms. This research enhances our understanding of how financial institutions adapt to external shocks, complementing broader inquiries focused on social objectives such as financial inclusion and poverty reduction.

The paper is structured as follows: Section 2 provides a state of related literature regarding institutional fragility and finance. Section 3 outlines our hypothesis, and Section 4 describes the data and methodology used to explore the question. Section 5 presents our main findings. We provide concluding remarks in Section 6.

### 4.2 Literature Review and Background

The impact of structural and contextual shocks on financial institutions has been extensively explored in the finance and development literature. A substantial portion of this scholarship has traditionally centered on microfinance and financial inclusion, often employing randomized controlled trials, surveys, and case studies to assess the effects of financial access on poverty alleviation and small enterprise development (Duflo et al., 2007; Pitt and Khandker, 1998; Karlan and Zinman, 2011; Morduch, 1999). Within this body of work, financial inclusion

has been identified as a key mechanism for promoting socioeconomic development (Armendáriz and Morduch, 2005; Cull et al., 2007, 2009), with positive effects observed on household welfare and business outcomes (Banerjee et al., 2015; Beck et al., 2005).

However, as financial services expand geographically and demographically, the exposure of banks to external threats—including crime and violence—has gained prominence as an emerging field of inquiry. Scholars have begun to investigate how criminal activity can compromise the stability, profitability, and strategic orientation of financial intermediaries. Detotto and Otranto (2010) establish that crime exerts a negative influence on economic growth through increased uncertainty and transaction costs, a mechanism that can be extended to the financial sector. Thai-Ha (2021) similarly argues that elevated crime undermines the expansion of financial inclusion by deterring bank activity and client participation in formal finance.

In more recent literature, the nexus between crime and banking has gained increasing attention, as scholars seek to understand how insecurity and violence disrupt financial intermediation across both formal and informal sectors. A growing consensus identifies crime as a critical external shock that affects multiple dimensions of bank operations. First, crime significantly increases operational costs, as financial institutions must invest more heavily in security infrastructure, such as armored transport, surveillance systems, insurance, and private guards. These costs, while necessary to mitigate physical risk, impose a financial burden that can erode margins, especially in regions where the profitability of branches is already marginal (Asiedu and Lien, 2011; De Juan, 1996).

Second, crime elevates credit risk and uncertainty in lending environments. In areas affected by violence, the reliability of collateral declines—either because physical assets are more likely to be destroyed or stolen, or because enforcement of contracts becomes more

difficult in weak institutional contexts. Borrowers in violent areas may also experience income volatility due to disrupted local labor markets, extortion, or forced displacement, thereby increasing the likelihood of default. These dynamics complicate credit risk assessment and make standard lending models less effective (Tella et al., 2010). As a result, banks may respond by tightening credit conditions, raising interest rates, or withdrawing lending altogether.

Third, the presence of crime adversely affects deposit mobilization and liquidity management. Households and firms may reduce their use of formal banking channels for fear of being targeted, or shift their behavior toward more liquid, informal, or mobile alternatives. Deposits may become more volatile, and customer confidence may decline, which constrains the ability of banks to maintain stable funding sources. In extreme cases, depositor runs or sharp withdrawals can occur in response to surges in local violence, especially where perceptions of institutional protection are weak.

These adverse impacts often trigger strategic retrenchment by banks. Institutions may opt to close branches, reduce staff, or scale back services in high-risk areas to contain exposure (Aldama-Navarrete, 2021). The closure of branches and ATMs not only reflects a rational cost-benefit response but also reshapes the geography of financial access, exacerbating inequality between safe and unsafe areas. Vulnerable populations, especially in rural or marginalized urban zones, face increasing difficulty accessing formal financial services. This can have long-term implications for poverty dynamics, as these communities are forced to rely on informal financial mechanisms, often characterized by higher costs, lower transparency, and limited consumer protection.

This process initiates a negative feedback loop between crime and financial exclusion. As banks withdraw from violent areas, households and businesses lose access to savings

instruments, credit, and secure payment systems. In turn, the lack of financial services can stunt local economic activity, foster informal or illicit transactions, and increase dependence on cash-based economies that are more difficult to monitor or tax. Over time, this reinforces informality and economic vulnerability, creating a vicious cycle that can further entrench poverty and social exclusion (De Juan, 2019; Alesina et al., 2019).

Moreover, the effects of crime on banking are not purely economic; they are deeply institutional and political. In contexts where criminal organizations exert territorial control, they may directly influence the operation of financial services through extortion, coercion, or infiltration. This can distort banks' allocation decisions and affect the credibility of the financial system. In extreme cases, criminal actors may use financial institutions for money laundering or to gain political leverage, thereby compromising the integrity of the banking system and weakening regulatory oversight.

Thus, crime emerges not only as a barrier to financial development but also as a systemic risk factor. Its influence spans from micro-level customer behavior to macro-level institutional strategies, affecting everything from lending decisions to capital allocation and network structure. This literature underscores the need for a multidimensional understanding of crime's impact on finance—one that incorporates spatial heterogeneity, institutional constraints, and the socio-political environment in which banks operate.

A growing strand of empirical research distinguishes between direct and indirect effects of crime on banking activities. Direct effects materialize in municipalities with high violence, where bank branches experience higher rates of closure, reduced loan disbursement, and increased non-performing loans. These are typically driven by local economic stagnation and security threats to both personnel and clients. In contrast, indirect effects arise in municipalities

not directly exposed to violence but connected through institutional channels—such as shared ownership structures or integrated balance sheets—to high-crime areas. Through risk management adjustments or internal capital reallocations, branches in safe areas may still face tightened liquidity, staff reductions, or restricted lending as a result of portfolio-wide risk exposure (Berger et al., 2010; Dell'Ariccia and Marquez, 2006).

Mexico's drug war offers a compelling empirical setting to observe both types of effects. The rapid escalation and spatial heterogeneity of cartel-related violence in the late 2000s created exogenous variation in crime exposure, allowing researchers to isolate its causal impact on economic and financial activity. Mejia and Restrepo (2013) show that organized crime undermines state capacity and formal markets, encouraging informalization and reducing trust in institutions. In the financial sector, these disruptions are transmitted through bank networks, resulting in changes in branch operations, staff allocation, and customer behavior, even in areas not directly affected by violence.

This study contributes to this emerging literature by applying a shift-share regression methodology to identify the causal effects of both local and networked crime shocks on banking outcomes. In doing so, it provides new evidence on how banks adapt to insecurity, and how institutional interconnectivity shapes the transmission of exogenous shocks across the financial system.

### 4.3 Empirical Hypothesis

We hypothesize that banks may suffer increasing fragility when a substantial share of their branches is located in areas experiencing spikes in crime episodes. Fragility likely consists of reduced transactions and changes in the primary economic activities of their customers (shifting from legal to shadowed, often illegal, activities) in high-crime areas. Bank branches in

high-crime areas might also incur higher operating costs due to the surge of uncertainty and credit risk, and lack of long-term deposits. This direct channel eventually and negatively affects the overall profitability of the bank.

An indirect (and relatively unexplored) effect occurs if the reduction in branches located in violent areas spills over to other branches operated by the same bank in safer areas. Given the greater risks, bank branches located in areas with high crime incidence will either move some of their activities to safer places (substitution effect) or cause a general reduction of the overall bank's activities (complementarity effect), including those conducted in safer areas.

The size of the spillover will be driven by: (1) how severe is a crime shock in violent areas; (2) the risk "exposure" of a given bank in those areas (i.e., the incidence of crime on the overall business of the bank); and (3) the degree of indirect exposure of branches in safer areas (the extent to which risk in branches located in violent areas spills over to other branches operated by the same bank in other areas).

The direction of change of the spillover is ambiguous. On the one hand, a "complementarity effect" might lead banks to reduce their overall activities throughout all their branches, to cope with a sort of aggregate (average) shock. In this case, branches located in relatively safe areas would suffer from a reduction in their activity to the same extent to which the overall activities of the bank are generally affected. On the other hand, a "substitution effect" could lead banks to move resources from riskier branches to safer branches, increasing their activities in the latter. The net effect must be empirically estimated.

To this purpose, we advance the following null hypothesis:

H0: *As the indirect exposure to crime risk increases (due to a relatively high share of branches of a given bank located in high-crime areas), bank branches in low-crime areas experience a contraction in their activities (complementarity effect).*

H0 will be tested against alternative hypotheses, that is: i) as the indirect exposure to crime risk increases, bank branches in low-crime areas take over the activities of those established in high-crime areas (substitution effect), or ii) the (indirect) exposure to crime risk is not affected by bank activities in high-crime areas.

## 4.4 Data and Methodology

### 4.4.1 Data

We combined two primary data sources to construct a unique panel of information at the municipal-year level spanning the years 2001 through 2011.

Crime at the local level was obtained from the statistical agency INEGI, which collects annual records of murders. From this measure, we constructed the variable (log) murders per 100,000 inhabitants of a municipality, which represents our measure of local crime incidence. Between 2007 and 2008, the homicide rate increased substantially and continued to grow through the end of the sample. Violence was primarily concentrated along the coast and in Northern Mexico. Figure 13 shows the homicide rate for each municipality in 2009.

From the Comisión Nacional Bancaria y de Valores (CNBV), we obtained detailed financial intermediation records, both at the municipal and institutional (bank) level. CNBV reports savings deposits and operational data at the quarterly level, and reports credit balances at the monthly level. We aggregate these data to construct the key outcome variables in municipality m in year *t*. In particular, we observe the credit and default balances related to commercial credit, consumer loan balances, total deposit amounts, number of savings contracts,

**Figure 13** Direct Crime Exposure by Municipality

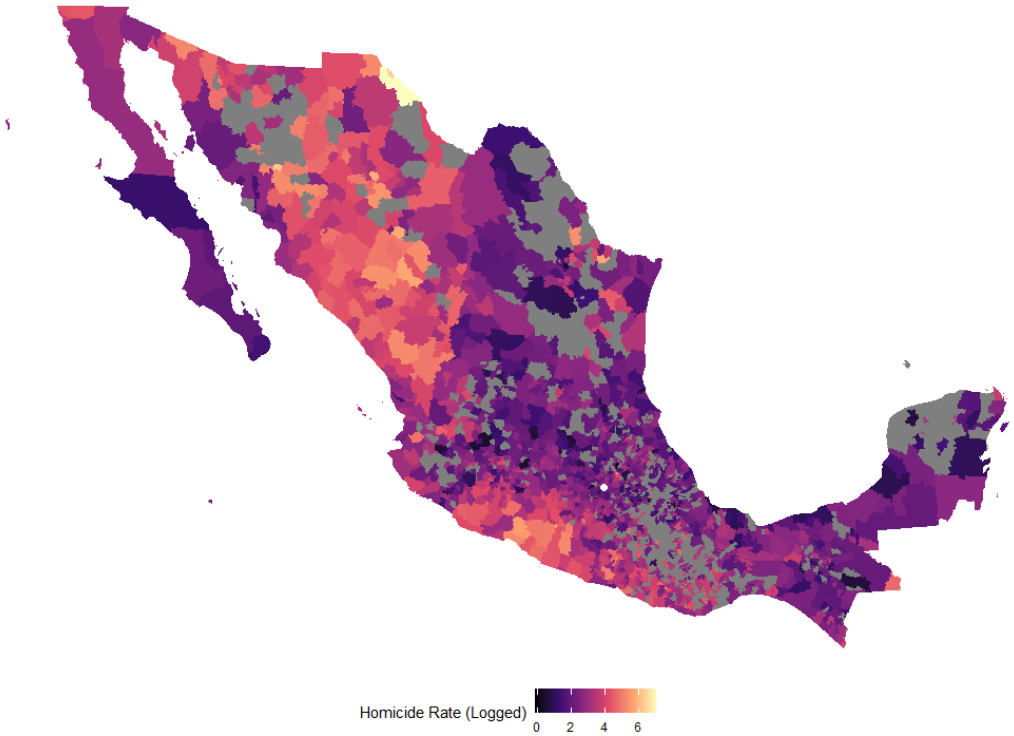



Figure 13 presents each municipality's logged homicide rate in 2009, measured as the number of homicides per 100,000 people. Mexico City is indicated by the white point.

branches, staff, and ATMs, as well as indicators for whether the municipality has any staff or any ATMs.

We illustrate the national trend in homicides, credit balances, and savings deposits over the sample period in Figure 14. Between 2000 and 2007, both measures of banking activity remained relatively stable, and the homicide rate slowly grew. By 2008, the amount of savings increased significantly and persisted through 2011. The homicide rate grew at a faster rate during this time, as did the national credit balance. As mentioned earlier, the increase in cartel competition during this period could account for illegal banking activity, particularly in high-crime areas.

We construct weights by observing the institution's total banking activity, defined as total savings and loans. An institution's total savings consists of demand deposits, time deposits, and

**Figure 14** Homicide Rate and Banking Activity in Mexico

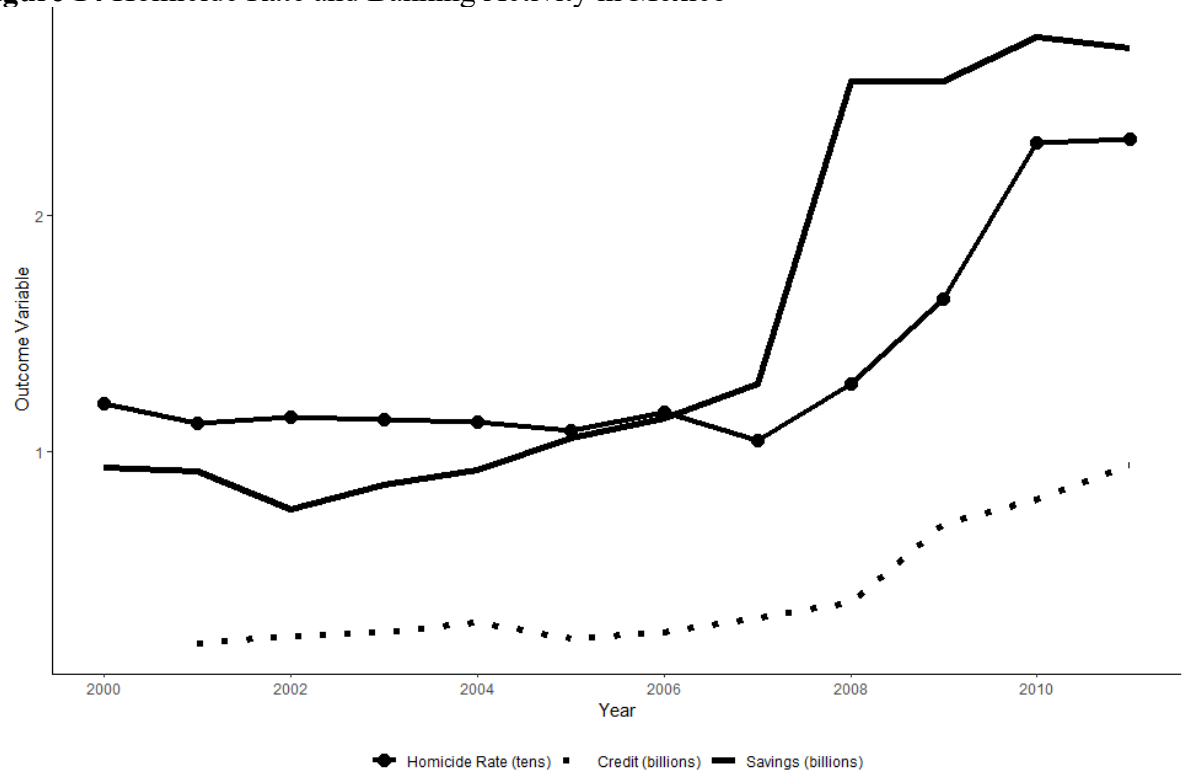



Figure 14 shows the evolution of violence and banking activity in Mexico between 2000 and 2011. The homicide rate is averaged over all municipalities. Credit and savings are averaged over all municipalities with a positive value, including only active institutions in the measure. Credit and savings are both reported in hundreds.

banking bonds, and total loans measure the institution's total outstanding credit balance. In some instances, banks that were used to construct the weights were later acquired by other institutions later in the sample. We treat the institutions involved as one institution, more specifically, we categorize them as the institution that acquired the original banks.

We present summary statistics for banking institutions and municipalities in Mexico in Table 11. Across the entire sample, roughly one quarter of institutions acted as a savings only institution, engaging in no lending behavior. Throughout the sample, around 75% of municipalities had any credit balance or savings balance, but no more than 40% of municipalities had access to a branch within the municipality. We normalize the measure of indirect crime exposure across the period of analysis. As such, exposure is growing overtime.

**Table 11** Summary Statistics for Banking Institutions and Mexican Municipalities

| | 2006 | 2007 | 2008 | 2009 | 2010 | 2011 |
|---|---|---|---|---|---|---|
| **A. Banking Institution Characteristics** | | | | | | |
| % Savings Only | 23.9% | | | | | |
| % Credit Only | 8.7% | | | | | |
| Total Deposits | 24.7 | 24.9 | 25.1 | 25.2 | 25.3 | 25.2 |
| Credit Balance | 23.5 | 23.7 | 23.9 | 24.6 | 24.7 | 24.9 |
| Total Deposits per Municipality | 20.6 | 20.7 | 20.8 | 20.8 | 20.9 | 20.9 |
| Credit Balance per Municipality | 19.3 | 19.6 | 19.6 | 20.3 | 20.4 | 20.6 |
| # Branches | 163 | 171 | 221 | 223 | 228 | 230 |
| # of Municipalities Served | 61.2 | 62.8 | 75.1 | 78.2 | 79 | 78.3 |
| Crime Exposure | 11.6 | 7.98 | 11.7 | 16.5 | 29.9 | 41.2 |
| **B. Municipality Characteristics** | | | | | | |
| Number of Municipalities | 2393 | | | | | |
| Total Deposits | 5.97 | 6.09 | 6.89 | 6.95 | 7 | 6.98 |
| Credit Balance | 5.11 | 5.36 | 5.57 | 6.31 | 6.4 | 6.55 |
| % Municipalities with Savings or Credit | 73.2% | 73.5% | 75.0% | 80.2% | 76.8% | 75.1% |
| # Branches | 3.13 | 3.28 | 4.24 | 4.29 | 4.39 | 4.41 |
| % Municipalities with a Branch | 35.1% | 35.1% | 38.3% | 39.9% | 39.1% | 38.9% |
| # ATMs | 0 | 0 | 12.4 | 16.7 | 15.1 | 15.2 |
| # Staff | 34.3 | 36.2 | 64.3 | 64.9 | 64.5 | 66.9 |
| Indirect Crime Exposure | -0.238 | -0.398 | -0.118 | 0.25 | 0.791 | 1.18 |
| Homicide Rate | 11.7 | 10.4 | 12.9 | 16.5 | 23.1 | 23.3 |

Panel A provides the average characteristics of banking institutions in Mexico between 2006 and 2011. Panel B provides the average characteristics of municipalities in Mexico. In both panels, total deposits and credit balances are reported in log values.

### 4.4.2 Methodology

Our analysis relies on the following first-difference regression:

$$\Delta Y_{mt} = \beta\ \Delta CrimeExposure_{mt} + \gamma\ \Delta X_{mt} + \delta_m + \delta_t + \varepsilon_{mt} \quad (6)$$

where $\Delta Y_{mt}$ is the change in the outcome variable from one year to the next in the period 2007 to 2011. We control for time-invariant differences across municipalities through municipal fixed effects $\delta_m$, and aggregate shocks through year fixed effects $\delta_t$. Finally, we account for the level of crime in municipality *m* by including the change in murders per 100,000 inhabitants from one year to the next in the control set $X_{mt}$.

The key parameter of interest is $\beta$, the coefficient on the differenced shift-share variable that measures exposure to crime through the banking sector (described below). A negative coefficient supports H0, suggesting that the spillover effects are negative, i.e., controlling for the local amount of crime, banks located in municipalities served by bank branches that are indirectly exposed to risk due to the concentration of same-bank branches in high-crime rate areas suffer from a reduction of their overall activities). Instead, a positive or non-significant $\beta$ would support the alternative hypothesis, i.e., controlling for the local amount of crime, banks located in municipalities served by bank branches that are indirectly exposed to risk due to the concentration of same-bank branches in high-crime rate areas benefit -or is not affected by- the volume of the overall activities conducted by the bank.

We construct the exposure variable $CrimeExposure_{mt}$ as a shift-share instrument that captures the weighted average of crime faced by each financial institution operating in the municipality. That is:

$$CrimeExposure_{mt} = \sum_{i \in B_m} \sigma_{im} CrimeShock_{it} \quad (7)$$

The explanatory variable is composed of the "shift" component $CrimeShock_{it}$ related to the overall exposure of bank *i* in a given period *t*, and a "share" component $\sigma_{im}$, averaged over the presence of bank *i* in $B_m$, which is the set of all institutions operating in municipality *m*.

The "shift" component $CrimeShock_{it}$, is defined as follows:

$$CrimeShock_{it} = \sum_{m \in M_b} \xi_{im} Hom_{mt} \quad (8)$$

Each institution *i* operating in the set of municipal markets *m* faces a level of crime that is the weighted average of the (log) of homicide rate in each municipality, $Hom_{mt}$. The weights $\xi_{im}$ are characterized by $\sum_{i \in M_b} \xi_{im} = 1$ and represent the share of total bank activities (i.e., savings

and credit) by bank $i$ in municipality m. Thus, if a significant amount of bank $i$'s business comes from municipality $m$ ($\xi_{im}$ is high), and there is a large increase in crime in municipality $m$, the banking institution faces a large crime shock. This can translate to a large (indirect) exposure to crime in another municipality if bank $i$ holds a large share of activities in that municipality.

**Figure 15** Variation in an Institution's Crime Exposure

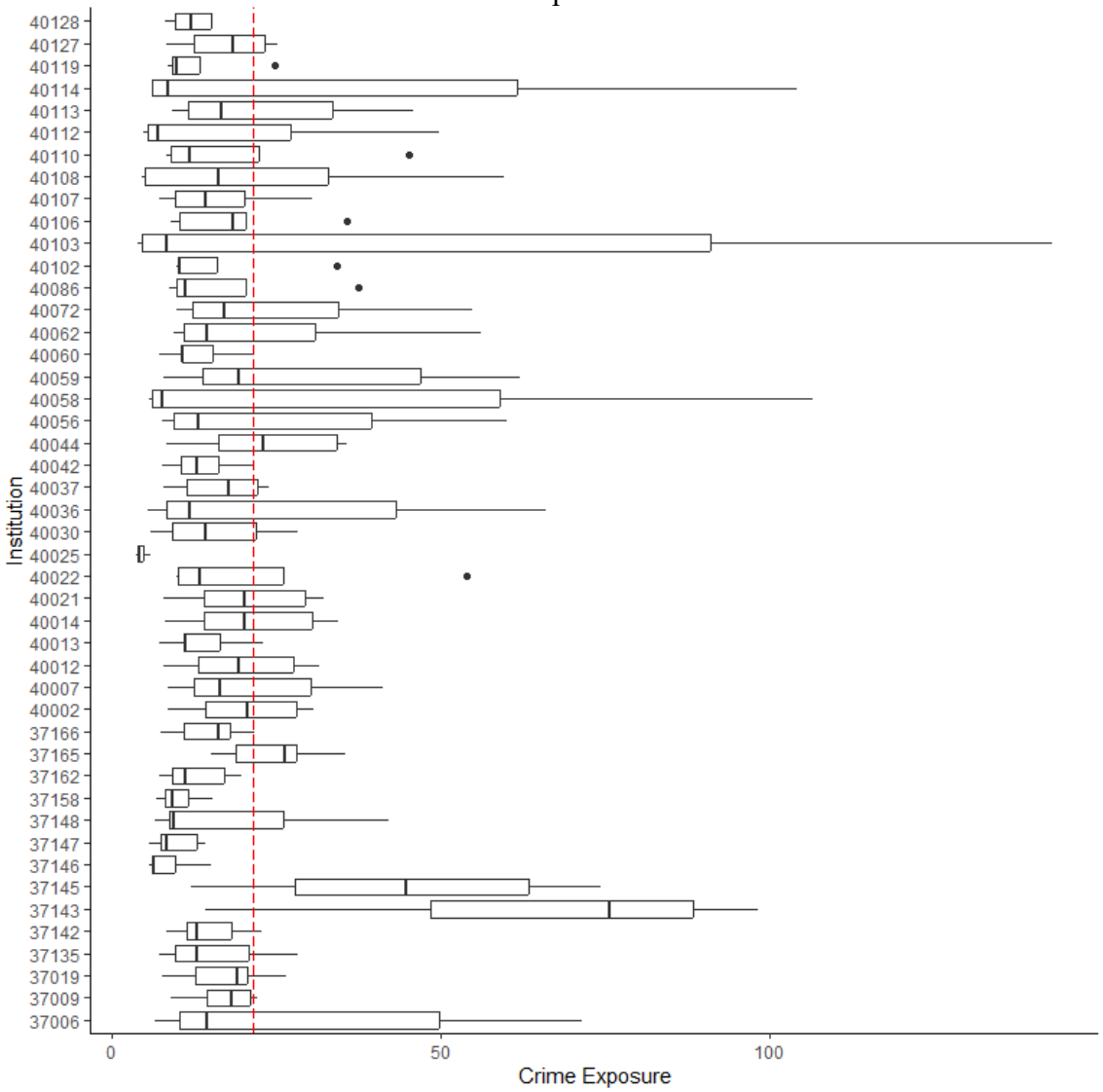



Figure 15 illustrates the variation in the yearly crime exposure within an institution and across institutions between 2007 and 2011. The y-axis contains each institution's id, coded by CNBV. The crime exposure is the weighted average of the homicide rate per 100,000 people, where the weights are constructed by the share of total deposits each municipality contributes to the institution for each year. The vertical dashed line indicates the average crime exposure for the sample.

Between 2007 and 2011, some institutions were more affected by the spike in violent crime than others. Figure 15 shows the variation in each institution's exposure to crime across the sample period and the municipalities they serve. Institutions with lower levels of crime exposure conducted a substantial amount of their operations in municipalities with low homicide rates. While many institutions are centered around the sample average, there are several institutions that consistently operate in high-exposure environments. For these institutions, we might expect a geographic shift in their banking activity. This could be an overall reduction in their banking activity, which would reduce activity in the municipalities it serves. Alternatively, banks could shift their activity and substitute towards low crime municipalities.

The "share" of activities in municipality *m* held by institution *i* $\xi_{im}$ could be affected by the current amount of crime and is therefore potentially endogenous. Therefore, it is measured at the baseline period (i.e., the average between the years 2001 and 2006). As is standard in shift-share regression models, the identifying assumption relies on the exogeneity of either shifts or shares (Bartik, 1990). We assume that the shares are exogenous, i.e., the relative importance of a financial institution in a municipality was not influenced by the spike in crime, and a financial institutions' operations in the pre-crime period were not influenced by the spike in crime.

**4.5 Results**

Our primary analysis investigates the effect of institutional exposure to crime shocks on the municipalities they serve. As such, we begin by visualizing each municipality's indirect exposure to violence in Figure 16. Municipalities with a larger value rely on institutions that have a large exposure to violent crime, relative to the average crime shock over sample period. Across Mexico, there is variation in the indirect exposure to violent crime.

**Figure 16** Indirect Crime Exposure by Municipality (2009)

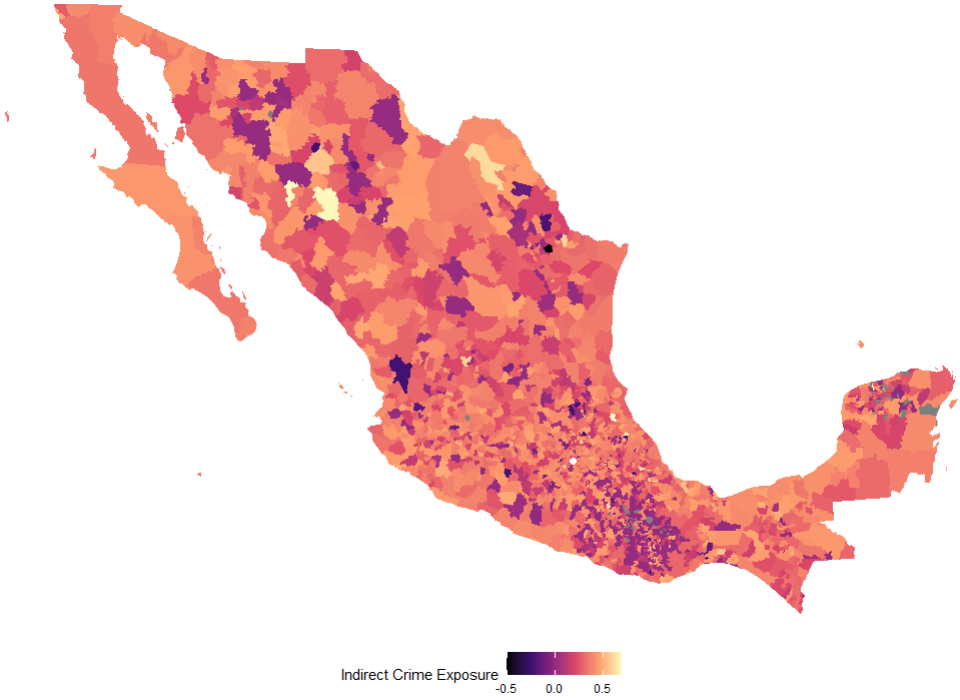



Figure 16 presents each municipality's indirect crime exposure in 2009. Indirect crime exposure is the weighted average of the direct crime exposure (normalized) for each institution operating in the municipality. The weights are constructed by measuring how relevant each institution is to the municipality, measured by the percentage of total deposits the institution accounts for.

Exploiting this variation in indirect exposure to crime, we estimate the spillover effects of an institution's violent crime exposure on financial outcomes in municipalities the institution serves. Table 12 reports the estimated spillover effects of institutional crime exposure on year-to-year changes in consumer financial outcomes at the municipality level. The coefficients capture how a banking institution's exposure to violence translates into changes in municipal loan markets, default behavior, and deposit activity. Across all outcome variables, an increase in institutional violence exposure negatively impacts the banking sector in municipalities. We include the direct homicide rate of the municipality but acknowledge that the relationship is endogenous.

Panels B and C provide robustness tests for our estimates. In Panel B, we conduct a leave-one-out methodology, in which we measure the weighted average of a municipality's

**Table 12: Effect of Violence Exposure on Consumer Financial Outcomes**

| | (1) Credit | (2) Default | (3) Deposits | (4) Contracts |
|---|---|---|---|---|
| **Panel A: Main Specification** | | | | |
| Δ Crime Exposure | -0.660 | -0.088 | -22.745 *** | -0.001 ** |
| | (2.329) | (0.067) | (6.252) | (0.000) |
| Δ Homicide Rate | -0.028 | 0.000 | -0.026 | 0.000 |
| | (0.019) | (0.000) | (0.045) | (0.000) |
| $R^2$ | 0.167 | 0.105 | 0.145 | 0.197 |
| N | 11965 | 11965 | 11965 | 11965 |
| **Panel B: Leave-One-Out** | | | | |
| Δ Crime Exposure | -0.905 | -0.086 | -22.406 *** | -0.001 ** |
| | (2.556) | (0.067) | (6.356) | (0.000) |
| Δ Homicide Rate | -0.028 | 0.000 | -0.026 | 0.000 |
| | (0.019) | (0.000) | (0.045) | (0.000) |
| $R^2$ | 0.167 | 0.105 | 0.145 | 0.197 |
| N | 11965 | 11965 | 11965 | 11965 |
| **Panel C: Endogenous Weights** | | | | |
| Δ Crime Exposure | 1.675 | -0.106 | -18.762 *** | -0.003 *** |
| | (1.926) | (0.081) | (4.329) | (0.000) |
| Δ Homicide Rate | -0.029 | 0.000 | -0.024 | 0.000 |
| | (0.019) | (0.000) | (0.045) | (0.000) |
| $R^2$ | 0.167 | 0.105 | 0.145 | 0.201 |
| N | 11965 | 11965 | 11965 | 11965 |

*** p < 0.001; ** p < 0.01; * p < 0.05.

Table 12 reports the estimates of a municipality's weighted crime shock on consumer financial outcomes, measuring the spillover effect of violence. Dependent variables are reported in millions of pesos for columns 1-3 and millions of contracts for column 4. All estimates include municipality and year fixed-effects. Standard errors are clustered at the municipal level and reported in parentheses. Panel A reports the estimates when using the exogenous, pre-2007 weights. Panel B reports the estimates using the exogenous, pre-2007 weights and a leave-one-out approach. Panel C reports the estimates using the potentially endogenous yearly weights.

exposure to violence through its institutions while excluding the municipality's contribution to the institutional exposure. Doing so alters the $CrimeShock_{it}$ constructed in Equation 8 such that now:

$$CrimeShock_{it} = \sum_{n \neq m} \xi_{in} Hom_{nt} \quad (9)$$

where $CrimeShock_{it}$ is the weighted sum over all municipalities *n* that are not *m*. The results are robust to this measure. In Panel C, we allow the shares in Equation 8 to vary during the current

year of interest, $\xi_{imt}$. This provides us with the endogenous measure of indirect crime exposure that we aim to instrument for.

**Table 13: Effect of Violence Exposure on Financial Access**

| | (1) Any Branch | (2) Any Staff | (3) # Staff | (4) Any ATM | (5) # ATMs |
|---|---|---|---|---|---|
| **Panel A: Main Specification** | | | | | |
| Δ Crime Exposure | -0.010 ** | -0.008 ** | -16.580 ** | -0.012 * | -3.013 *** |
| | (0.003) | (0.003) | (5.517) | (0.005) | (0.800) |
| Δ Homicide Rate | 0.000 | 0.000 | -0.001 | 0.000 | 0.003 |
| | (0.000) | (0.000) | (0.006) | (0.000) | (0.002) |
| $R^2$ | 0.133 | 0.122 | 0.227 | 0.305 | 0.163 |
| N | 11965 | 11965 | 11965 | 11965 | 11965 |
| **Panel B: Leave-One-Out** | | | | | |
| Δ Crime Exposure | -0.009 ** | -0.008 ** | -16.377 ** | -0.012 * | -2.838 *** |
| | (0.003) | (0.003) | (5.471) | (0.005) | (0.760) |
| Δ Homicide Rate | 0.000 | 0.000 | -0.001 | 0.000 | 0.003 |
| | (0.000) | (0.000) | (0.007) | (0.000) | (0.002) |
| $R^2$ | 0.133 | 0.122 | 0.227 | 0.305 | 0.163 |
| N | 11965 | 11965 | 11965 | 11965 | 11965 |
| **Panel C: Endogenous Weights** | | | | | |
| Δ Crime Exposure | -0.008 *** | -0.010 *** | 3.106 | -0.130 *** | -3.121 *** |
| | (0.002) | (0.003) | (3.080) | (0.005) | (0.697) |
| Δ Homicide Rate | 0.000 | 0.000 | -0.003 | 0.000 | 0.003 |
| | (0.000) | (0.000) | (0.006) | (0.000) | (0.002) |
| $R^2$ | 0.132 | 0.123 | 0.226 | 0.337 | 0.163 |
| N | 11965 | 11965 | 11965 | 11965 | 11965 |

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Table 13 reports the estimates of a municipality's weighted crime shock on consumer access to financial services, measuring the spillover effect of violence. All estimates include municipality and year fixed-effects. Standard errors are clustered at the municipal level and reported in parentheses. Panel A reports the estimates when using the exogenous, pre-2007 weights. Panel B reports the estimates using the exogenous, pre-2007 weights and a leave-one-out approach. Panel C reports the estimates using the potentially endogenous yearly weights.

While all outcomes experienced a negative spillover effect, the coefficients on commercial credit loans and default balances are not significant. However, a municipality's savings deposit balance and number of contracts slowed down significantly when the municipality was served by institutions with high crime exposure. A standard deviation increase in indirect crime exposure from one year to the next results in 22.745 million less pesos and

around 1000 less contracts. Between 2009 and 2010, the average municipality in Mexico experienced an increase in the standardized indirect crime exposure of .814.[9] The corresponding change in savings deposits was around 18.5 million pesos, or 1 million USD.

We turn to the spillover effects of institutional violence on financial supply in indirectly affected municipalities, reported in Table 13. We observe that institutions withdraw operations in municipalities in response to violence. Municipalities that rely on institutions with large violence shocks experience slower growth in and lower probabilities of being served by baking institutions. A standard deviation increase in the municipality's indirect banking exposure to violence reduces the year-to-year operational numbers by nearly 17 staff and 3 ATMs and slightly lowers the likelihood that a municipality has access to a banking branch or ATM. The estimates are robust when using a leave-one-out approach.

To explore the indirect effects of violence on consumer credit outcomes in a municipality, we explore auto loans, credit cards, loans for durable goods, payroll loans, and personal loans. For this purpose, Equation 6 becomes:

$$\Delta ConsumerCredit_{mt} = \beta \Delta CrimeExposure_{mt} * CreditType_{mt} + \gamma \Delta X_{mt} + \delta_m + \delta_t + \varepsilon_{mt} \quad (10)$$

where $CreditType_{mt}$ is a factor variable. We report the point estimates and 95% confidence interval in Figure 17. Across most measures, we observe a significant and negative spillover effect. Auto loans and loans for durable goods experienced the largest reduction in response to indirect crime exposure, followed by personal and payroll loans. Credit card loans, however, experienced an increase in the credit balance following indirect crime exposure. This could occur

[9] We normalize institutional crime exposure across the entire sample period, so the measure is increasing in time. In 2007, the average municipality's year to year change in indirect crime exposure is -0.240. In 2008, it was 0.422. In 2009, 2010, and 2011, it was 0.553, 0.814, and 0.580, respectively.

through decreased payments through reduced banking operations or an increase in existing credit usage because of reduced access to alternatives.[10]

**Figure 17** Spillover Effect of Crime Exposure on Consumer Credit Balance

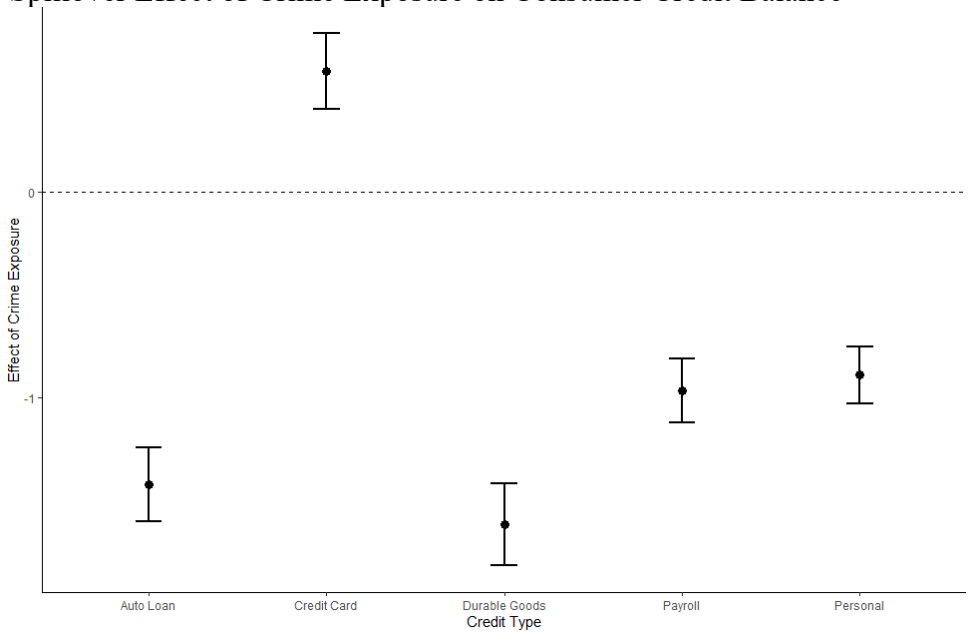



Figure 17 reports the indirect effect of violent crime exposure (leave-one-out) on different types of credit balances. Included are auto loans, credit card balances, loans for durable goods, payroll loans, and personal loans. All estimates are found using municipality and year fixed effects. Standard errors are clustered at the municipality level.

Taken together, the results show that violence is experienced not only in areas with large rates of crime, but the effects are transmitted through banking networks as well. Most notably, access to financial services falls in municipalities that are served by institutions with high exposures to violence. Additionally, the number of savings contracts and savings deposits falls. Consumer credit balances fall, except for credit card balances. This relationship reflects the complementary effect that violence has on banking: affected institutions will contract their overall activity in response to violence.

[10] Online banking could provide an avenue for existing credit card users to continue usage. While online banking grew between 2005 and 2015, only 10% of the internet-using population had regularly engaged in online banking services by 2015. Not quite 40% of individuals in Mexico had consistent access to the internet by 2015, making the population of online banking users quite small (Adame et al., 2016).

Identification relies on exogeneity of the shares, where the relative importance of an institution in a municipality should not be influenced by crime shocks. We present Rotemberg weights associated with the Bartik instrument, as outlined in Goldsmith-Pinkham et al. (2020). We aggregate the yearly weights to the institution level and report the weights in Table 14.

To address the sensitivity to misspecification, we run the analysis excluding the institutions associated with the three highest and lowest Rotemberg weights, reported in Table B1 in the Appendix. Across all outcomes, the estimates are within the 95% confidence interval of the point estimate from our main analysis in Tables 12 and 13. The estimate on default balances becomes insignificant. Additionally, we provide context regarding the size and spread of each the institutions with the highest and lowest Rotemberg weights in Figure B1 of the Appendix. Notably, four of the six institutions are concentrated regionally.

**Table 14** Rotemberg Weights

| Institution | Alpha | Beta |
|---|---|---|
| Banca Quadrum | 2.66 | 0.474 |
| American Express Bank (México) | 1.11 | 0.56 |
| Banco Santander (México) | 1.09 | 0.106 |
| Banco Mercantil del Norte | 0.363 | 1.06 |
| Banco Interacciones | 0.289 | 0.432 |
| | | |
| Banco del Bajío | -0.46 | 0.0821 |
| Banco Nacional del Ejército, Fuerza Aérea y la Armada | -0.542 | 0.452 |
| Banco de Crédito Rural del Pacífico-Sur | -0.693 | 2.36 |
| Banco del Sureste | -1.01 | 0.304 |
| Banco de Crédito Rural del Pacífico Norte | -1.09 | 0.448 |

We report the five highest and lowest Rotemberg weights associated with our analysis, aggregated to the institution level as recommended in Goldsmith-Pinkham et al. (2020).

### 4.6 Conclusion

This study explores the relationship between crime and insecurity and the retail banking sector in Mexico. We contribute to existing literature that focuses on financial inclusion, poverty,

and development. While prior studies primarily focus on the direct impacts of insecurity on financial inclusion, we focus on the spillover effects of violence and insecurity on financial outcomes in other locations. This provides new insights regarding the consequences of violent crime shocks on financial markets, more specifically, the contraction of overall activity in response to fragility.

We employ a shift-share estimation strategy and exploit the variation in banking exposure to violent crime shocks. Through this, we disentangle the direct and indirect of institutional fragility on financial outcomes in Mexican municipalities between 2007 and 2011. By constructing measures of both direct exposure (branches operating in high-crime municipalities) and indirect exposure (branches operating in low-crime areas but serviced by institutions that operate in high-crime municipalities), we gain a deeper understanding as to how crime shocks affect banking activity. This study provides evidence that a bank's increased exposure to violence results in complementary effects for the municipalities served by the institution. Specifically, institutions reduce the number of savings contracts and balances, as well as their overall banking activity in response to violent crime exposure.

Our findings show that rising crime and insecurity cause banking institutions to restrict financial activity across their network, not just in municipalities affected by the violence. Municipalities served by institutions with high crime exposure experience reductions in deposits, loan supply, branch presence, and ATM availability. These results highlight a previously unexplored mechanism through which crime shocks affect banking and financial activity.

Future research should engage in a deeper understanding of the long-term implications of crime on banking operations, while also assessing the robustness of findings across different contexts and investigating potential policy interventions to mitigate the negative impacts of

crime on the financial sector. Additionally, expanding the analysis to include other financial institutions and different types of crime could provide a more comprehensive understanding of the financial spillovers of crime.

## Appendix A

**Figure A1** Spatial Distribution of Airbnb Listings, January 2017

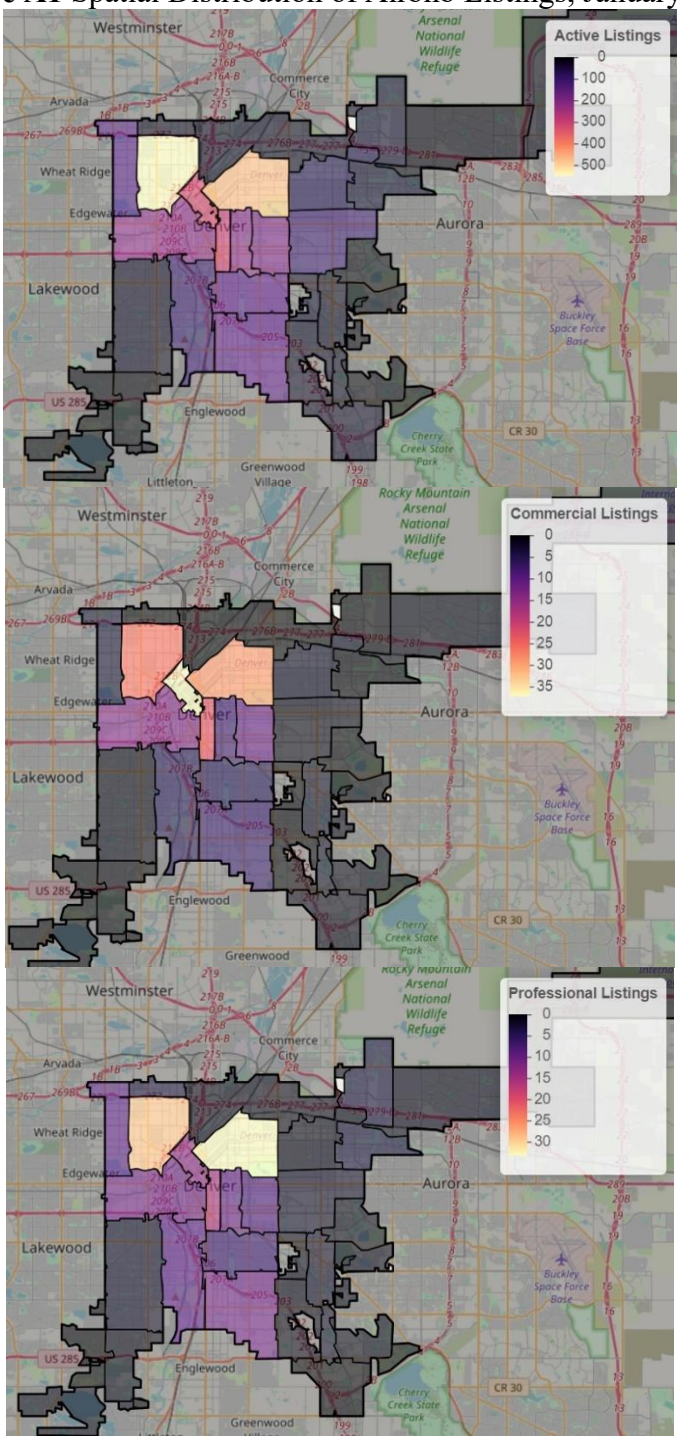
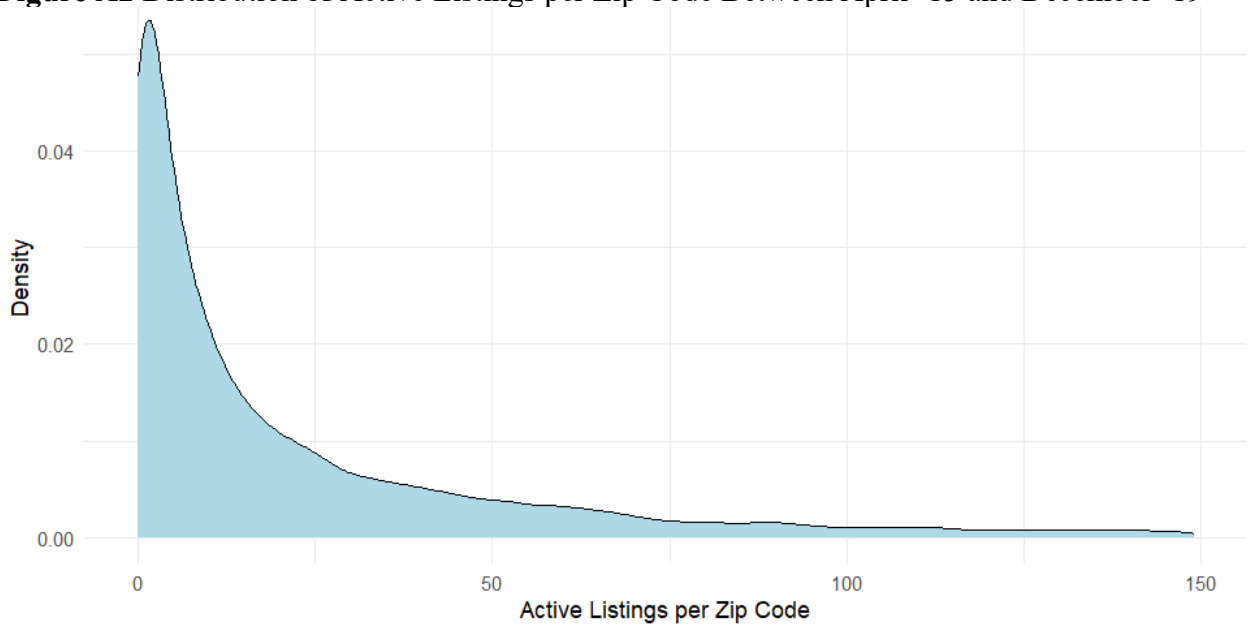


**Figure A2** Distribution of Active Listings per Zip Code Between April ‘15 and December ‘19



**MTR Definition as Described by Zhe Jin et al., 2024**

"If there is at least 1 reservation in month t but no reservation in months t − 1 and t + 1, then the listing is offered as an STR listing in month t. If the number of reservations in month t is 1 and there are reservations in month t + 1 and no reservation in month t −1, and the sum of the reservation days of month t and month t +1 is less than or equal to 31, then the listing is offered as an STR listing in month t. Suppose that the number of reservations in month t is 1, and there are reservations in both month t −1 and month t +1. In that case, the listing is offered as an STR listing in month t if the sum of reservation days in month t −1 and t is less than or equal to 31 and the sum of reservation days in month t and month t +1 is less than or equal to 31. If the number of reservations in month t is 2, and there is no reservation in month t +1 (same for t −1), then the listing is offered as an STR listing in month t if month t is not fully booked. If the number of reservations in month t is 2, and there are reservations in both month t −1 and t +1, then the listing in month t is offered as an STR listing if the sum of reservation days in month t and t +1 is less than or equals 31. If the number of reservations in month t is greater than or equal to 3, then the listing is offered as an STR listing in month t regardless of reservation patterns in adjacent months."

## Appendix B

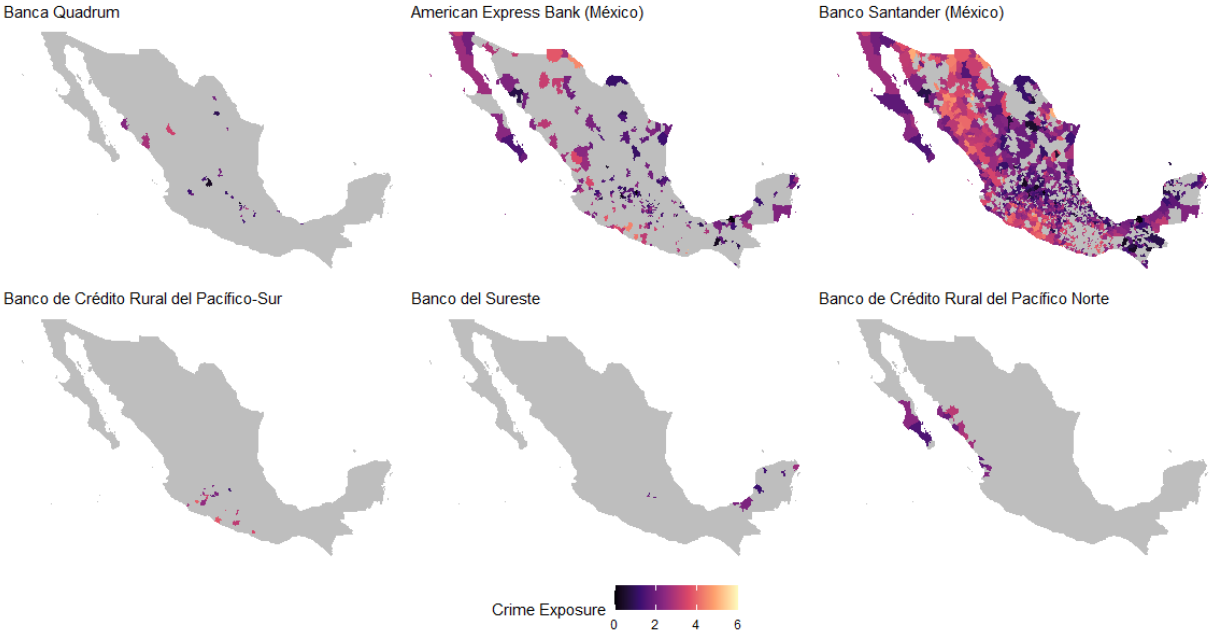
**Table B1** Robustness of Main Results

| | (1) Credit | (2) Default | (3) Deposits | (4) Contracts | (5) Any Branch | (6) Any Staff | (7) # Staff | (8) Any ATM | (9) # ATM |
|---|---|---|---|---|---|---|---|---|---|
| Panel A: Main Specification | | | | | | | | | |
| Δ Crime Exposure | 1.368 | -0.035 | -17.779 *** | -0.001 ** | -0.007 ** | -0.010 *** | -13.347 ** | -0.014 ** | -2.321 *** |
| | (1.576) | (0.053) | (4.541) | (0.000) | (0.003) | (0.003) | (4.667) | (0.005) | (0.535) |
| Δ Homicide Rate | -0.029 | 0.000 | -0.020 | 0.000 | -0.000 | 0.000 | -0.000 | 0.000 | 0.002 |
| | (0.019) | (0.000) | (0.037) | (0.000) | (0.000) | (0.000) | (0.006) | (0.000) | (0.002) |
| $R^2$ | 0.165 | 0.105 | 0.157 | 0.227 | 0.142 | 0.127 | 0.225 | 0.315 | 0.224 |
| N | 11965 | 11965 | 11965 | 11965 | 11965 | 11965 | 11965 | 11965 | 11965 |
| Panel B: Leave-One-Out | | | | | | | | | |
| Δ Crime Exposure | 1.456 | -0.029 | -17.868 *** | -0.001 ** | -0.007 ** | -0.010 *** | -13.286 ** | -0.014 ** | -2.193 *** |
| | (1.581) | (0.053) | (4.546) | (0.000) | (0.003) | (0.003) | (4.650) | (0.005) | (0.489) |
| Δ Homicide Rate | -0.029 | 0.000 | -0.020 | 0.000 | -0.000 | 0.000 | -0.001 | 0.000 | 0.002 |
| | (0.019) | (0.000) | (0.037) | (0.000) | (0.000) | (0.000) | (0.006) | (0.000) | (0.002) |
| $R^2$ | 0.165 | 0.105 | 0.157 | 0.226 | 0.142 | 0.127 | 0.225 | 0.315 | 0.224 |
| N | 11965 | 11965 | 11965 | 11965 | 11965 | 11965 | 11965 | 11965 | 11965 |

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

We exclude the institutions associated with the three highest and lowest Rotemberg weights. All regressions include municipality and year fixed-effects, and standard errors are clustered at the municipality level.

Figure B1 Institutional Crime Exposure in 2007 for 3 Highest & Lowest Rotemberg Weights




We present spatial distribution of the six institutions with the highest and lowest Rotemberg weights between 2000 and 2007. Within each municipality served, we report the direct crime exposure from 2007, measured as the homicide rate. Certain institutions had exited the market prior to our direct period of analysis: Banca Quadrum (2001), Banco de Crédito Rural del Pacífico-Sur (2002), and Banco de Crédito Rural del Norte (2002).

## References Cited


Adame, V., Arellano, A., Pérez, M. L., & Oliveros, R. M. (2016, June 9). *Digital Context in Mexico*. BBVA Research. https://www.bbvaresearch.com/wp-content/uploads/2016/09/310816-c-digital-MEXICO-ing_vf.pdf

Aldama-Navarrete, D. (2021). Dark Banking? Banks and illicit deposit flows. Banks and Illicit Deposit Flows (March 24, 2021).

Alesina, A., Piccolo, S., and Pinotti, P. (2019). Organized crime, violence, and politics. The Review of Economic Studies, 86(2), 457-499.

Armendáriz, B., and Morduch, J. (2005). The economics of microfinance. Cambridge: MIT press.

Asiedu, E., and Lien, D. (2011). Democracy, foreign direct investment and natural resources. Journal of international economics, 84(1), 99-111.

Banerjee, A., Duflo, E., Glennerster, R., and Kinnan, C. (2015). The miracle of microfinance? Evidence from a randomized evaluation. American economic journal: Applied economics, 7(1), 22-53.

Barron, K., Kung, E., & Proserpio, D. (2021). The effect of home-sharing on house prices and rents: Evidence from airbnb. *Marketing Science*, *40*(1), 23–47. https://doi.org/10.1287/mksc.2020.1227

Basuroy, Suman and Kim, Yongseok and Proserpio, Davide, Estimating the impact of Airbnb on the local economy: Evidence from the restaurant industry (July 7, 2020). http://dx.doi.org/10.2139/ssrn.3516983

Beck, T., Demirguç-Kunt, A., and Levine, R. (2005). SMEs, growth, and poverty: Cross-country evidence. Journal of economic growth, 10, 199-229.

Becker, Gary S. "Crime and punishment: An economic approach." *The Economic Dimensions of Crime*, 1968, pp. 13–68, https://doi.org/10.1007/978-1-349-62853-7_2.

Berger, A. N., Hasan, I., and Zhou, M. (2010). The effects of focus versus diversification on bank performance: Evidence from Chinese banks. Journal of Banking and Finance, 34(7), 1417-1435.

Chen, W., Wei, Z., & Xie, K. (2023). Regulating professional players in peer-to-peer markets: Evidence from Airbnb. *Management Science, 69*(5), 2893–2918. https://doi.org/10.1287/mnsc.2022.4480

Coles, P. A.,Egesdal, M., Ellen, I. G., Li, X., and Sundararajan, A. (2017). *Airbnb Usage Across New York City Neighborhoods: Geographic Patterns and Regulatory Implications*

Forthcoming, Cambridge Handbook on the Law of the Sharing Economy. http://dx.doi.org/10.2139/ssrn.3048397

Cull, R., Demirguç-Kunt, A., and Morduch, J. (2007). Financial performance and outreach: A global analysis of leading microbanks. The Economic Journal, 117(517), F107-F133.

Cull, R., Demirgüç-Kunt, A., and Morduch, J. (2009). Microfinance meets the market Journal of Economic perspectives, 23(1), 167-192.

De Juan, A. (2019). From good bankers to bad bankers. In From Good to Bad Bankers: Lessons Learned from a 50-Year Career in Banking (pp. 1-16). Cham: Springer International Publishing.

Dell'Ariccia, G., and Marquez, R. (2006). Competition among regulators and credit market integration. Journal of Financial Economics, 79(2), 401–430.

Detotto, C., and Otranto, E. (2010). Does crime affect economic growth?. Kyklos, 63(3), 330-345.

Dogru, T., Hanks, L., Suess, C., Line, N., & Mody, M. (2023). The resilience of the lodging industry during the pandemic: Hotels vs. Airbnb. *International Journal of Hospitality Management*, *109*, 103406. https://doi.org/10.1016/j.ijhm.2022.103406

DuBois, Dillon. "How Covid-19 Has Impacted the Supply of Short-Term Rentals." *AirDNA*, 26 Mar. 2021, www.airdna.co/blog/covid-19-pandemic-impacted-the-supply-of-strs.

Duflo, E., Glennerster, R., and Kremer, M. (2007). Using randomization in development economics research: A toolkit. Handbook of development economics, 4, 3895-3962.

Farhoodi, A. (2019). Welfare estimation in peer-to-peer markets with heterogenous agents: The case of airbnb. *SSRN Electronic Journal*. https://doi.org/10.2139/ssrn.3480518

Farronato, C., & Fradkin, A. (2018). *The Welfare Effects of Peer Entry in the Accommodation Market: The Case of Airbnb*. https://doi.org/10.3386/w24361

Feess, Eberhard, et al. "The impact of fine size and uncertainty on punishment and deterrence: Theory and evidence from the Laboratory." *Journal of Economic Behavior & Organization*, vol. 149, May 2018, pp. 58–73, https://doi.org/10.1016/j.jebo.2018.02.021.

Filieri, R., Milone, F. L., Paolucci, E., & Raguseo, E. (2023). A big data analysis of COVID-19 impacts on Airbnbs' bookings behavior applying construal level and signaling theories. *International Journal of Hospitality Management*, *111*, 103461. https://doi.org/10.1016/j.ijhm.2023.103461

Filippas, A., & Horton, J. J. (2017). The tragedy of your upstairs neighbors. *Proceedings of the 2017 ACM Conference on Economics and Computation*. https://doi.org/10.1145/3033274.3085097

Garcia, B., Miller, K., & Morehouse, J. M. (2022). *In Search of Peace and Quiet: The Heterogeneous Impacts of Short-Term Rentals on Housing Prices*.

Hale,T., Angrist, N., Goldszmidt, R., Kira, B., Petherick, A., Phillips, T., Webster, S., Cameron-Blake, E., Hallas, L., Majumdar, S., & Tatlow, H. (2021). A global panel database of pandemic policies (Oxford COVID-19 Government Response Tracker). *Nature Human Behaviour*. https://doi.org/10.1038/s41562-021-01079-8

Goldsmith-Pinkham, P., Sorkin, I., and Swift, H. (2020). "Bartik instruments: What, when, why, and how." *American Economic Review*, vol. 110, no. 8, pp. 2586–2624.

Huang, Y. (2021). Seller-pricing frictions and platform remedies. *SSRN Electronic Journal*. https://doi.org/10.2139/ssrn.3767103

Karlan, D., and Zinman, J. (2011). Microcredit in theory and practice: Using randomized credit scoring for impact evaluation. Science, 332(6035), 1278-1284.

Ke, L., O'Brien, D. T., & Heydari, B. (2021). Airbnb and neighborhood crime: The incursion of tourists or the erosion of local social dynamics? *PLOS ONE, 16*(7), e0253315. https://doi.org/10.1371/journal.pone.0253315

Kerr, J., Panagopoulos, C., & van der Linden, S. (2021, September). *Political polarization on covid-19 pandemic response in the United States*. Personality and individual differences. https://www.ncbi.nlm.nih.gov/pmc/articles/PMC8631569/

Khandker, S. R. (2005). Microfinance and poverty: Evidence using panel data from Bangladesh. The world bank economic review, 19(2), 263-286.

Laske, K., Saccardo, S., & Gneezy, U., Do Fines Deter Unethical Behavior? The Effect of Systematically Varying the Size and Probability of Punishment (2018). Available at http://dx.doi.org/10.2139/ssrn.3157387

Marque, R., Morales-Alonso, G., Núñez, Y. M., & Hidalgo, A. (2023). Dealing with the externalities of the sharing economy: Effect of airbnb in long-term rental prices in the city of Madrid. *Lecture Notes in Management and Industrial Engineering*, 263–270. https://doi.org/10.1007/978-3-031-29382-5_26

Matsuzawa, Kyutaro. "The deterrent effect of targeted and salient police enforcement: Evidence from bans on checkpoints for driving under the Influence." *The Journal of Law and Economics*, vol. 68, no. 2, 1 May 2025, pp. 311–360, https://doi.org/10.1086/732666.

Mejia, D., and Restrepo, P. (2013). Bushes and bullets: Illegal cocaine markets and violence in Colombia. Documento CEDE, (2013-53).

Mertens, G., Lodder, P., Smeets, T., & Duijndam, S. (2023). Pandemic panic? results of a 14-month longitudinal study on fear of covid-19. *Journal of Affective Disorders*, *322*, 15–23. https://doi.org/10.1016/j.jad.2022.11.008

Morduch, J. (1999). The microfinance promise. Journal of economic literature, 37(4), 1569-1614.

Pitt, M. M., and Khandker, S. R. (1998). The impact of group-based credit programs on poor households in Bangladesh: Does the gender of participants matter?. Journal of political economy, 106(5), 958-996.

Robertson, C., Dejean, S., & Suire, R. (2023). "Airbnb in the city": Assessing short-term rental regulation in Bordeaux. *The Annals of Regional Science*, *72*(2), 647–682. https://doi.org/10.1007/s00168-023-01215-4

Roth, J. J. (2021). Home sharing and crime across neighborhoods: An analysis of Austin, Texas. *Criminal Justice Review, 46*(1), 40–52. https://doi.org/10.1177/0734016820948947

Sheppard, S., & Udell, A. (2016), Do Airbnb Properties Affect House Prices?, No 2016-03, Department of Economics Working Papers, Department of Economics, Williams College, https://EconPapers.repec.org/RePEc:wil:wileco:2016-03.

Tella, R. D., Galiani, S., and Schargrodsky, E. (2010). Crime distribution and victim behavior during a crime wave. In The economics of crime: Lessons for and from Latin America (pp. 175-204). University of Chicago Press.

Teodorescu, Kinneret, et al. "Frequency of enforcement is more important than the severity of punishment in reducing violation behaviors." *Proceedings of the National Academy of Sciences*, vol. 118, no. 42, 13 Oct. 2021, https://doi.org/10.1073/pnas.2108507118.

Thai-Ha, L. (2021). Microfinance and Social Development: A Selective Literature Review. Asian Development Outlook.

Valentin M. Regulating short-term rental housing: Evidence from New Orleans. *Real Estate Econ*. 2021; 49: 152–186. **https://doi.org/10.1111/reec.12330**

Xu, Minhong, and Yilan Xu. "What happens when Airbnb comes to the neighborhood: The impact of home-sharing on Neighborhood Investment." *Regional Science and Urban Economics*, vol. 88, May 2021, p. 103670, https://doi.org/10.1016/j.regsciurbeco.2021.103670.

Yang, Y., & Mao, Z. (Eddie). (2018). Welcome to my home! an empirical analysis of Airbnb supply in US cities. *Journal of Travel Research*, *58*(8), 1274–1287. https://doi.org/10.1177/0047287518815984

Zhe Jin, G., Wagman, L., & Zhong, M. *The Effects of Short-Term Rental Regulation: Insights from Chicago*, June 2024, https://doi.org/10.3386/w32537.