

# How Short-Term Rental Regulations Reshape Urban Spending: Evidence from New York City's Restaurant Sector\*

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## Abstract

We study the spillover effects of short-term rental (STR) regulations on urban service markets. We focus on the 2023 STR regulation in New York City (NYC), which took effect in September 2023 and is among the most restrictive in the United States. The policy led to a sharp decline in the supply of Airbnb listings. Using credit and debit card transaction data at the restaurant-month level, we analyze spending patterns across 19 major US metropolitan areas from July 2022 to July 2024. To identify the causal impact of the regulation on restaurant spending, we employ a Difference-in-Differences strategy paired with Propensity Score Matching to match each NYC restaurant with a comparable non-NYC restaurant. We show that NYC's STR regulation led to a 10% reduction in restaurant spending, with stronger effects observed for higher-priced restaurants and those catering to tourists. These findings are consistent with the fact that limiting the supply of STRs increases accommodation costs, reducing the influx of tourists and the discretionary spending of those willing to incur higher accommodation costs. These results underscore the importance of considering broader economic impacts when designing urban housing policies.

**Keywords:** short-term rentals, restaurant demand, transactional data, policy

# 1 Introduction

Short-term rental (STR) platforms are today a popular option for many travelers, offering access to an extensive and diverse supply at a lower cost than traditional accommodation options such as hotels.

For example, Airbnb—one of the most popular short-term rental platforms—allows homeowners and tenants to rent out spare capacity to short-term visitors, thereby increasing the overall supply of temporary accommodations and lowering the cost of travel for tourists ([Zervas et al., 2017](#)). By expanding available lodging options, Airbnb is able to attract additional tourists who otherwise would not have switched to a hotel if no Airbnb were available, thus increasing the total volume of visitors ([Farronato and Fradkin, 2022](#)). Moreover, because Airbnb listings are located throughout residential neighborhoods rather than concentrated in hotel districts, the platform changes the spatial distribution of tourists within the city. This expansion and reallocation of visitor activity may alter local patterns of spending and employment, including demand for restaurants and other urban services ([Alyakoob and Rahaman, 2022](#); [Basuroy et al., 2020](#)).

Despite these documented benefits, the expansion of STR has generated considerable debate, particularly around housing affordability. Supporters argue that platforms like Airbnb stimulate local economies by attracting tourists and spreading spending across a wider set of neighborhoods. Opponents contend that STR platforms reduce the supply of long-term housing, raise rents, and disrupt residential communities ([Barron et al., 2021](#); [Garcia-López et al., 2020](#); [Horn and Merante, 2017](#)). In response, many cities have enacted regulations aimed at limiting or strictly monitoring STR activity, including license requirements, availability restrictions, and fines ([Bekkerman et al., 2023](#); [Foroughifar and Narang, 2025](#)).

These STR policies are designed with the primary objective of reducing STR supply and increasing (or at least preserving) housing affordability and neighborhood stability. Yet, they may also produce unintended consequences for other sectors of the urban economy. For example, [Bekkerman et al. \(2023\)](#) shows that these policies, while effective at reducing STR

supply, also reduce residential investments that could increase housing supply.

The STR regulation-driven reduction in STR supply limits stay options for potential travelers, who, confronted with costlier options, may decide not to travel at all ([Farronato and Fradkin, 2022](#)), or travel but cut back on discretionary or hedonic expenditures due to the higher cost of stay. Consequently, policies designed to restrict STR markets may generate unintended spillover effects on complementary urban sectors in cities that implement them. Whether such spillovers exist and how substantial they are remains an open empirical question with important policy implications. This paper contributes to the literature on STR policies by examining these potential spillovers and their magnitude.

In September 2023, New York City (NYC) implemented one of the most restrictive STR regulations in the United States. The regulation required hosts to register with the city, prohibited most entire-home rentals unless the host was present, and limited listings that did not meet these conditions. This regulation led to a sharp decline in the number of active Airbnb listings, making most of the Airbnb supply illegal. By early November 2023 (or about two months after the regulation took effect) the number of active Airbnb listings had fallen to approximately 4,600, down from about 23,000 in August of the same year, according to data from AirDNA, a company that analyzes performance metrics for over 10 million Airbnb and Vrbo vacation rentals worldwide.<sup>1</sup> This regulation provides a valuable setting to study how restricting STR affects the economic activity in other urban sectors.

In this paper, we utilize the 2023 NYC regulation as a natural experiment to examine the impact of STR restrictions on the local restaurant industry. Restaurants represent a large and dynamic sector of the urban economy, and are closely linked to tourism and neighborhood-level consumption ([Basuroy et al., 2020; Schiff, 2015](#)). To evaluate how the regulation affected the restaurant industry, we construct a rich panel dataset of monthly credit and debit card transactions for over 60,000 restaurants across 20 major US metropolitan areas, obtained from SafeGraph and spanning July 2022 to July 2024. Our identification strategy relies

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<sup>1</sup>See <https://www.airdna.co/>

on propensity score matching and a Difference-in-Differences (DiD) design, which compares changes in consumer spending for NYC restaurants before and after the regulation took effect, relative to a control group of similar restaurants in other major US cities.

Our primary estimates show that the short-term rental regulation enacted in NYC in 2023 led to a roughly 10% decline in consumer restaurant spending over the 14 months after the policy took effect. A simple back-of-the-envelope calculation suggests that this reduction in spending translated to a loss of approximately 3 billion dollars for the city of New York. To reinforce the causal interpretation of these results, we present several robustness and sensitivity checks. We show that our estimates are robust to the inclusion of a wide array of controls, different matching methods and estimation models, different time windows around the regulation implementation, placebo tests, and different restaurant samples.

To better understand the effects of this regulation, we next look at which establishments are most affected. We find that higher-priced restaurants and those with a higher share of non-local customers experienced the most pronounced decreases in spending. This suggests that the mechanisms behind this effect are both reduced tourist inflows in touristy neighborhoods, and higher accommodation costs that limit the consumption of travelers who were forced to stay in more expensive types of accommodations.<sup>2</sup>

Overall, the results indicate that the 2023 STR regulation in NYC, while aimed at stabilizing housing affordability and neighborhood safety, had significant negative spillover effects on the local restaurant economy. By reducing the supply of short-term rental accommodations, the regulation increased accommodation costs, constrained budget travelers, and ultimately suppressed demand for restaurants, particularly those with higher prices and those catering to tourists. These findings highlight the broader economic interdependencies within urban markets and underscore the need for policymakers to consider several factors when designing regulations to limit STRs.

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<sup>2</sup>According to news reports (e.g., <https://www.travelweekly.com/Travel-News/Hotel-News/Airbnb-crackdown-windfall-for-NYC-hotels>), NYC's average daily hotel rate surged in December 2023 (three months after the regulation) by 11% to \$393. The revenue per available room (RevPAR) increased by 15.6%. In contrast, the national RevPAR average increased by only about 0.3%.

## 2 Data

To study the effect of NYC’s STR regulation on consumer restaurant spending, we combine several data sources including credit/debit card transactions from SafeGraph, socio-economic indicators from the US Census Bureau and the Bureau of Labor Statistics, and weather data from the National Oceanic and Atmospheric Administration’s National Centers for Environmental Information.

**Point-of-interest and Credit/Debit Card Transaction Data.** First, we obtain data from SafeGraph via Deweydata, a data platform for academic use (SafeGraph, 2022).<sup>3</sup> The data includes information on points-of-interest (POIs) in the physical world (including restaurants), which consists of POI-level characteristics such as geolocation, sector, and debit/credit card transactions aggregated at the POI-month level. The transactions are collected from a representative sample of debit/credit card users. The provided information shows the aggregated number of transactions, the aggregated number of customers with at least one transaction at a POI, and the amount spent. This includes in-person transactions made at the POI as well as online transactions.<sup>4</sup> From the data, we can infer economic activity associated with each POI and in each month, which is widely adopted and validated in prior research (e.g., Chiong et al., 2025; Shaik et al., 2025).

We restrict the analysis to the period covering July 2022 to July 2024, which includes 14 months before the regulation took effect (September 2023) and 10 months after. We obtain records for all POIs pertaining to Food Services and Drinking Places.<sup>5</sup> We also restrict the sample to restaurants that (i) are located in 20 major core-based statistical areas (CBSAs) in the United States,<sup>6</sup> and (ii) have no missing values for any month in the analysis period.

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<sup>3</sup>Deweydata (<https://www.deweydata.io/>) is a platform that makes data more accessible for academic research. SafeGraph (<https://www.safegraph.com/>) provides updated data on points-of-interest (POI) in the physical world, including restaurants.

<sup>4</sup>For more details, see: <https://docs.safegraph.com/docs/spend#section-online-vs-in-person-transactions>

<sup>5</sup>These places are identified by the North American Industry Classification System (NAICS) code, and their first three digits are 722.

<sup>6</sup>Our sample includes restaurants in Atlanta–Sandy Springs–Roswell, GA; Baltimore–Columbia–Towson,

Because the STR regulation is implemented in NYC, some consumers or tourists who are affected by the restriction could shift their visitation patterns and thereby restaurant spending to nearby areas in New Jersey. To prevent this cross-border spillover from biasing our results, we exclude all restaurants located in New Jersey from the analysis.

Our final dataset includes 3,220 restaurants in NYC (covering all five boroughs: Manhattan, Brooklyn, Queens, The Bronx, and Staten Island) and 57,535 restaurants in the rest of the 19 CBSAs. The NYC sample represents roughly 53% chain and 55% independent restaurants, which differs from the citywide distribution of 24% chains and 76% independents. This imbalance may arise because our data requires a minimum number of monthly card transactions, making chain restaurants, which are typically larger and more stable, more likely to be included. To ensure that our results are not driven by this sample bias, we implement a downsampling and reweighting procedure that adjusts the composition of the NYC sample to match the underlying restaurant population. The main findings remain robust under this adjustment. We provide a detailed discussion of representativeness and the robustness checks in Section 6.1.

The number of consumers for each restaurant and month is broken down by the inferred customers' home city.<sup>7</sup> Using this information, we can study the differential impact across restaurants with higher vs. lower shares of non-local customers. To do so, we map each customer's home city and each restaurant location to their containing CBSA. Then, we define non-locals as those originating from any home CBSA other than the focal restaurant's CBSA, and compute the share of non-local customers.

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MD; Boston–Cambridge–Newton, MA–NH; Chicago–Naperville–Elgin, IL–IN–WI; Dallas–Fort Worth–Arlington, TX; Denver–Aurora–Lakewood, CO; Detroit–Warren–Dearborn, MI; Houston–The Woodlands–Sugar Land, TX; Los Angeles–Long Beach–Anaheim, CA; Miami–Fort Lauderdale–West Palm Beach, FL; Minneapolis–St. Paul–Bloomington, MN–WI; New York–Newark–Jersey City, NY; Philadelphia–Camden–Wilmington, PA–NJ–DE–MD; San Francisco–Oakland–Hayward, CA; Seattle–Tacoma–Bellevue, WA; St. Louis, MO–IL; Urban Alaska; Urban Hawaii; and Washington–Arlington–Alexandria, DC–VA–MD–WV.

<sup>7</sup>The customers' home city is inferred based on the activity of the debit/credit card user. The home city of each customer is reported only if at least 2 customers made a transaction at the same POI and month, to reduce privacy concerns. For more details, see <https://docs.deweydata.io/docs/safegraph-spend>

**Socio-economic Data.** We supplement the transaction data with time-varying local socio-economic indicators to account for broader economic conditions that may influence restaurant demand. Specifically, we obtain population data from the US Census Bureau at the CBSA-year level and Consumer Price Index (CPI) data from the Bureau of Labor Statistics at the CBSA-month level. Population serves as a proxy for potential local demand that is expected not to be impacted by the regulation, while the CPI is used to adjust for inflation, thereby reducing bias arising from heterogeneous price dynamics across CBSAs.

**Weather Data.** Finally, we obtain US weather data from the National Oceanic and Atmospheric Administration’s National Centers for Environmental Information (NOAA NCEI).<sup>8</sup> The dataset provides high-quality daily observations from thousands of weather stations across the United States. In our analysis, we focus on temperature and precipitation, as these variables are expected to affect restaurant demand. For each CBSA, we link restaurants to the nearest weather stations and aggregate daily observations to the monthly level to align with our restaurant spending data. We then compute the monthly average temperature and total precipitation, which serve as our primary weather covariates.

**Summary Statistics.** Table 1 reports summary statistics of the variables used, broken down by restaurants in NYC and in the other CBSAs.

### 3 Empirical Strategy

To estimate the causal effect of NYC’s short-term rental regulation on restaurant demand, we employ a difference-in-differences (DiD) strategy combined with matching. We start with matching treated restaurants (in NYC) with control restaurants in the remaining 19 CBSAs. To do so, we use 1-to-1 propensity score matching based on pre-treatment time-varying covariates. Specifically, we use the number of customers, total spending, and the share of

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<sup>8</sup><https://www.ncei.noaa.gov/access/past-weather/>

Table 1: Summary Statistics

Variable	All Restaurants			Restaurants in NYC			Restaurants in other CBSAs		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Total Spending	3,670.00	1,706.00	29,587.99	1,507.00	712.40	3,984.10	3,791.00	1,789.00	30,385.51
Number of Customers	117.50	52.00	712.75	38.30	22.00	132.38	122.00	55.00	731.50
Number of Local Customers	82.13	39.00	134.67	24.87	16.00	30.30	85.33	41.00	137.50
Number of Non-Local Customers	41.22	17.00	617.06	23.67	12.00	120.07	42.21	18.00	633.44
Average Temperature	62.09	62.70	15.22	58.80	57.60	14.36	62.27	62.90	15.24
Precipitation Volume	3.29	2.71	2.78	4.55	4.11	2.81	3.22	2.60	2.76
Consumer Price Index (CPI)	306.80	308.20	22.71	322.80	322.50	6.75	305.90	306.30	22.95
Population	2,580,152	1,563,349	2,787,683.40	1,915,572	1,660,664	512,242.46	2,617,346	1,281,836	2,857,501.31
Number of Restaurants	60,755			3,220			57,535		

*Note.* The table reports summary statistics for the variables used. Total Spending, Number of Customers, and Local/Non-Local Customers are at the restaurant level. Average Temperature, Precipitation, CPI, and Population are at the CBSA level.

non-local customers for each one of the 14 pre-treatment periods (from July 2022 to August 2023). This procedure allows us to match each one of the 3,320 NYC restaurants to 3,320 in the remaining CBSAs. In Figure 4 in Appendix B, we show, for each matching covariate, the standardized difference in values between the treated group and control group before and after matching. After matching, the values are balanced, and there are no substantial differences across the matching variables during the pre-treatment period.

Using this set of matched restaurants, we implement a DiD identification strategy. DiD compares changes in restaurant demand for treated restaurants, before and after the treatment, with changes in restaurant demand for control restaurants over the same time period.

The key identifying assumption behind our strategy is that, in the absence of the regulation, the outcomes (restaurant demand) for restaurants in the treatment and control groups would have followed parallel trends over time. In other words, we assume that there are no restaurant-specific time-varying shocks that could simultaneously affect both the regulation and restaurant demand. As it is usual in these settings, we partially test this assumption using an event-study design.

The main DiD specification takes the following forms:

$$\log(Spend_{it} + 1) = \beta Post_t \times Treat_i + X_{ct}\gamma + \alpha_i + \tau_t + \epsilon_{it}, \quad (1)$$

where the dependent variable is the log of the monthly consumer spending at restaurant  $i$  and year-month  $t$ .  $Post_t$  is a dummy variable taking the value of 1 if period  $t$  is after the regulation took effect (i.e., from September 2023), and 0 otherwise.  $Treat_i$  is a dummy variable taking the value of 1 if restaurant  $i$  is located in NYC, and 0 otherwise.  $X_{ct}$  is a set of control variables at the level of CBSA and year-month.<sup>9</sup>  $\alpha_i$  are restaurant fixed effects, which control for time-invariant differences across restaurants, and  $\tau_t$  are time fixed effects, which control for temporal shocks common across all restaurants.  $\beta$  is the coefficient of interest, which measures the average treatment on the treated (ATT), that is, the average effect of the regulation on consumer spending at local restaurants.  $\beta$  is identified by the interaction of the treatment status and the post-treatment period  $Post_t \times Treat_i$ , comparing differences in spending at restaurants that are subject to the regulation versus those that are not, while controlling for all the aforementioned factors. Finally,  $\epsilon_{it}$  is the error term.

As we discussed above, to test the parallel trends assumption, we rely on an event-study design, which allows us to estimate the average treatment effect on the treated (ATT) for each time period relative to the treatment event. In addition to testing the parallel trends assumption, this approach allows us to observe how the treatment effect evolves over time. The specification for the event study takes the following form:

$$\log(Spend_{it} + 1) = \sum_{k=-L}^K \beta_k \mathbf{1}_{\{t-t^*=k\}} \times Treat_i + \alpha_i + \tau_t + X_{ct}\gamma + \epsilon_{it}, \quad (2)$$

where  $\mathbf{1}_{\{t-t^*=k\}}$  is an indicator that takes the value of 1 if period  $t$  is  $k$  periods relative to the treatment period  $t^*$ , and 0 otherwise.  $L$  and  $K$  are the number of examined time periods before and after the treatment, respectively. The coefficient of interest is  $\beta_k$ , which is the ATT for time period  $k$  relative to the treatment period.

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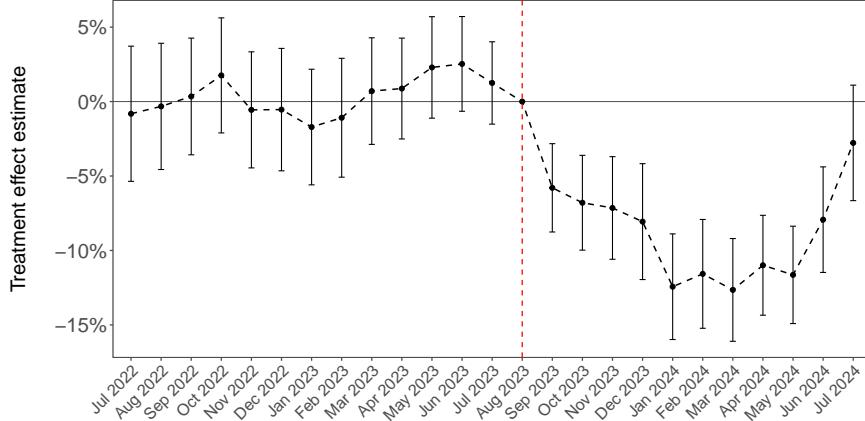
<sup>9</sup>The set of control variables includes average consumer price index, population, temperature, temperature squared, precipitation and precipitation squared. The quadratic form controls for a non-linear relationship of these covariates with the outcome variable and follows the specification in [Bollinger et al. \(2011\)](#). We log-transform these covariates to stabilize variance and reduce skewness.

## 4 Results

We start this section by assessing the identifying assumption of our identification strategy, i.e., the parallel trends assumption, by presenting the event study estimates. We present the estimates and their 95% confidence intervals of Equation 2 in Figure 1. The event study considers 14 months before and 11 months after the regulation implementation.

We observe that, in the pre-regulation period, the estimates are close to zero in all months prior to the treatment. This suggests that there was no systematic difference in outcomes for treated and control restaurants before the regulation, which supports the parallel trends assumption. In addition, in post-regulation periods, the estimates turn negative and gradually decrease over time until January 2024, when they stabilize around a negative 12%. Finally, we see a potential recovery starting in June 2024 (10 months after the regulation implementation). These results suggest that the regulation negatively affected restaurant revenue.

Figure 1: The Impact of the STR Regulation on Consumer Restaurant Spending Over Time



*Note.* The plot shows the estimated treatment effect and its 95% confidence interval for each period using Equation 2. The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . The model specification includes restaurant and month fixed effects, and the control variables average consumer price index, population, temperature, temperature squared, precipitation and precipitation squared at the CBSA level. Standard errors are clustered at the restaurant level. The vertical dashed red line at August 2023 represents the month preceding the implementation of the regulation.

Having provided support for the parallel trends assumption, we present the overall ATT estimated using Equation 1. We report these results in Table 2. In column 1, we present the

results without any time-varying controls. Consistent with the event study results, we find a substantial 10% decrease ( $\hat{\beta} = -0.101$ ,  $p < 0.001$ ) in consumer restaurant spending after the regulation took effect. In column 2, we report estimates that include the time-varying controls discussed in Section 3. The estimate remains negative and of similar magnitude to that in column 1.

Table 2: The Impact of the STR Regulation on Consumer Restaurant Spending

	(1)	(2)
Post $\times$ Treat	-0.1011*** (0.0109)	-0.0915*** (0.0111)
Covariates		✓
Restaurant FE	✓	✓
Time FE	✓	✓
Adjusted R <sup>2</sup>	0.8083	0.8096
N	161,000	161,000

\*p<0.5, \*\*p<0.01, \*\*\*p<0.001

*Note.* The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . Column 2 includes the control variables average consumer price index, population, temperature, temperature squared, precipitation and precipitation squared at CBSA level. Standard errors are clustered at the restaurant level.

## 4.1 Putting the Estimates into Economic Perspective

To quantify the economic impact of the STR regulation on NYC's restaurant revenue, we provide a back-of-the-envelope estimate of the associated revenue loss. Based on transaction-level evidence, we estimate an average decline of approximately 10% in restaurant spending following the implementation of the regulation. The underlying sample includes 3,220 restaurants and debit and credit card transactions from about nine million customers. Since NYC has many more restaurants and a substantially larger consumer base than those captured in

this sample, we scale the observed effect to the city level to approximate its overall impact.<sup>10</sup> According to the Office of the NYC Comptroller, taxable sales at restaurants and other eating places totaled \$26.904 billion between September 2023 and August 2024.<sup>11</sup> Assuming that the 10% decline in restaurant spending applies to the entire industry, the counterfactual level of sales—representing what restaurant revenue would have been in the absence of the regulation—can be expressed as:

$$\text{Counterfactual Sales} = \frac{X}{1 - d},$$

where  $X$  denotes observed taxable sales (\$26.904 billion) and  $d$  represents the estimated proportional decline (0.1). Substituting these values yields:

$$\frac{\$26.904}{1 - 0.1} = \$29.893,$$

which implies that, without the regulation, restaurant sales would have reached approximately \$29.893 billion. The estimated loss in restaurant revenue is therefore:

$$\$29.893 - \$26.904 = \$2.989 \text{ billion}$$

This calculation suggests an aggregate annual loss of roughly \$3.0 billion in restaurant revenue following the implementation of the STR regulation.

The validity of these estimates is supported by independent evidence from SafeGraph, which reports a strong correlation between transaction-based spending data and corporate revenue disclosures.<sup>12</sup>

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<sup>10</sup>In 2019, NYC hosted approximately 23,650 restaurant establishments (<https://www.osc.ny.gov/files/reports/osdc/pdf/nyc-restaurant-industry-final.pdf>). In 2023, 96% of U.S. households were banked and 76.4% had at least one credit card (<https://www.fdic.gov/news/press-releases/2024/fdic-survey-finds-96-percent-us-households-were-banked-2023>), suggesting broad coverage of transaction-based spending data.

<sup>11</sup>See: <https://comptroller.nyc.gov/wp-content/uploads/documents/The-State-of-the-Citys-Economy-and-Finances-2024.pdf>

<sup>12</sup>See: <https://www.safegraph.com/blog/validating-spend-data-for-brands-against-company-reporting>

Beyond direct losses in taxable restaurant sales, this contraction likely generates secondary economic effects, including a reduction in city tax revenues and potential employment losses if affected restaurants reduce operations or close entirely.

## 5 Heterogeneity Analysis

In this section, we explore heterogeneity by allowing the treatment effect to vary across restaurant groups. We focus on two variables. The first variable is the restaurant's price. The second variable is whether the restaurant caters more to local customers than to non-local (tourist) customers. We chose these variables because we expect the regulation to reduce tourist inflows and discretionary spending, thereby disproportionately affecting higher-priced restaurants and restaurants catering to tourists.

We estimate:

$$\log(Spend_{it} + 1) = \beta_1 Post_t \times Treat_i + \beta_2 Post_t \times Treat_i \times Group_{g(i)} + X_{ct}\gamma + \alpha_i + \tau_t + \epsilon_{it}, \quad (3)$$

where  $Group_{g(i)}$  is an indicator variable that equals 1 if restaurant  $i$  belongs to group  $g$ , and 0 otherwise. In the price analysis,  $Group_{g(i)}$  is 1 if the restaurant is higher-priced (vs. lower-priced), and in the non-local customer analysis, it is 1 if the restaurant has a higher share (vs. lower share) of non-local customers. The coefficient  $\beta_1$  captures the effect for the baseline group (lower-priced restaurants or those with a lower share of non-locals),  $\beta_2$  (the interaction term) captures the differential impact for higher-priced restaurants and higher shares of non-locals, and  $\beta_1 + \beta_2$  captures the total effect for the higher-priced restaurants or those with a higher share of non-locals.

### 5.1 Heterogeneity Results

**Price Level.** We first focus on the restaurant's price level. To compute each restaurant's average price, we use the pre-treatment average spending at the restaurant. Specifically, for

each restaurant, we first aggregate the total consumer spending and total number of transactions during pre-regulation months, and then calculate the average cost per transaction. We then split restaurants by the median of this measure and estimate Equation 3 to obtain the ATT for each of the two price groups.

Column 1 of Table 3 reports the results of this heterogeneity analysis. While both lower- and higher-priced restaurants experience significant spending declines after the regulation, higher-priced restaurants face a substantially larger negative effect. The estimated decline for lower-priced restaurants is about 3.6%, whereas for higher-priced restaurants the combined effect ( $\hat{\beta}_1 + \hat{\beta}_2$ ) implies a decline of roughly 16.1%.

**Share of Non-local Customers.** Second, we focus on the origin of the restaurant's customer base (i.e., locals vs. non-locals). First, we calculate the pre-regulation share of customers whose home location (inferred by the data provider) is outside the focal restaurant's CBSA (as explained in Section 2). This measure is meant to capture whether the restaurant is more likely to cater to tourists (non-locals) vs. non-tourists (locals). Then, we split restaurants by the median of this measure and estimate Equation 3. We present these results in column 2 of Table 3. Restaurants with a lower share of non-local customers experience an estimated spending decline of about -7.1%, while those with a higher share of non-local customers experience a substantially larger decline of roughly -12.6%.

Overall, the heterogeneous effects discussed in this section are consistent with both a reduction in tourist influx and a reduction in discretionary spending by those tourists still willing to travel despite higher accommodation costs.<sup>13</sup>

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<sup>13</sup>Unfortunately, due to data issues that started in January 2024, which prevent us from clearly identifying local vs. non-local customers, we are not able to directly test the hypothesis that the number of tourists and their transactions decreased.

Table 3: The Heterogeneous Impact of the STR Regulation

	(1)	(2)
	Price Level	Non-local Customer Share
Post × Treat	−0.03581*** (0.0123)	−0.0709*** (0.0122)
Post × Treat × HigherPrice	−0.1251*** (0.0133)	
Post × Treat × HigherShare		−0.0548*** (0.0136)
Covariates	✓	✓
Restaurant FE	✓	✓
Time FE	✓	✓
Adjusted R <sup>2</sup>	0.7867	0.7864
N	161,000	161,000

\*p<0.05, \*\*p<0.01, \*\*\*p<0.001

*Note.* The table reports the estimated treatment effect using Equation 3. The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . In Column 1, the coefficient on  $Post \times Treat$  captures the effect for lower-priced restaurants, while  $Post \times Treat \times HigherPrice$  shows the differential effect for higher-priced restaurants. In Column 2, the coefficient on  $Post \times Treat$  captures the effect for restaurants with a lower share of non-local customers, whereas  $Post \times Treat \times HigherShare$  shows the differential effect for restaurants with a higher share of non-local customers. The models include control variables for average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at the CBSA level. Standard errors are clustered at the restaurant level.

## 6 Robustness Checks

### 6.1 Restaurant Representativeness and Resampling

To assess how well our estimation sample reflects the underlying composition of NYC’s restaurant market, we benchmark our analysis sample against the population of restaurants recorded in the NYC Department of Health and Mental Hygiene’s inspection data.<sup>14</sup> Because every restaurant is inspected at least once per year, these inspection records provide a near-complete census of active establishments.

A key dimension along which sample selection may operate is whether a restaurant is independent or part of a chain. Chain restaurants typically maintain higher and more stable transaction volumes, while independent restaurants often transact less and exhibit greater month-to-month variability. This distinction is central to our setting because SafeGraph’s visibility criteria require a minimum number of card transactions per month to reliably estimate revenue.<sup>15</sup> Restaurants falling below this threshold in any month appear with missing observations, and such establishments cannot be included in the balanced panel used for our main regression analysis. Thus, independent restaurants—being smaller and having lower transaction volume—are more likely to drop out of the sample.

Our analysis sample contains 3,320 NYC restaurants for which we observe at least one transaction in every month from July 2022 through July 2024. To compare this sample with the size of the citywide restaurant universe, we restrict the inspection data to restaurants inspected in each year from 2022 to 2024, yielding 6,208 establishments with sustained operations over the same period.

Using few-shot prompting with the GPT-5.1-mini large language model, we classify these restaurants into independent and chain establishments.<sup>16</sup> Within the inspection universe, 76.48% are independent and 23.52% are chains. In contrast, the 3,320 restaurants in our es-

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<sup>14</sup>See: <https://www.nyc.gov/site/doh/services/restaurant-grades.page>

<sup>15</sup>See <https://docs.deweydata.io/docs/faqs-safegraph>

<sup>16</sup>The prompts used are reported in Appendix A.

timation sample consist of 54.88% independent and 45.12% chain restaurants—a substantial overrepresentation of chains.

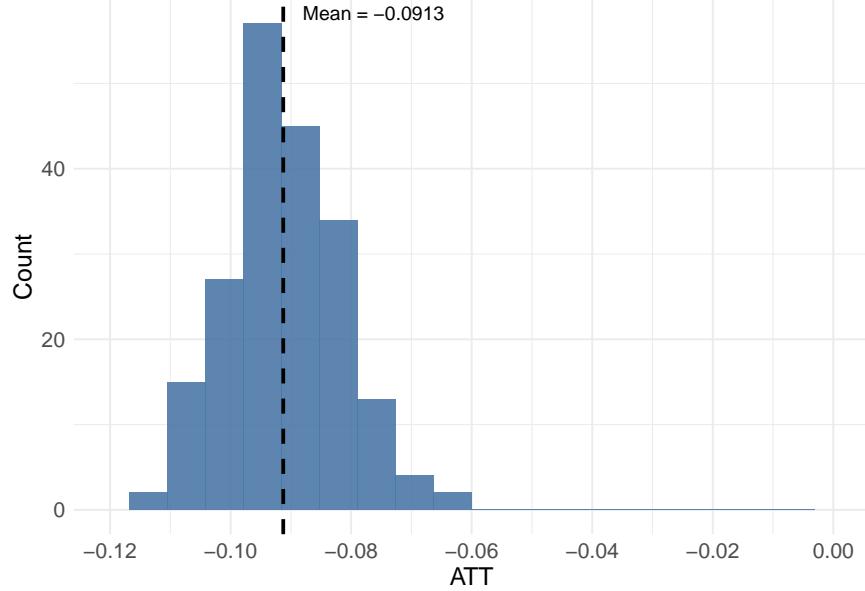
This imbalance aligns with the mechanism described above: in the pre-treatment period, chain restaurants in our data exhibit roughly 45% higher monthly transaction volumes than independent restaurants. Because higher volumes increase the likelihood that SafeGraph consistently observes sufficient transactions each month, chain restaurants are far less likely to miss months and therefore far more likely to meet the balanced-panel requirement. Independent restaurants, with lower and more variable transaction counts, are disproportionately excluded. As a result, our estimation sample overrepresents chain restaurants relative to the NYC restaurant population.

To address this mismatch and improve representativeness, we implement a downsampling-based reweighting strategy. We repeatedly draw random subsamples of 2,229 NYC restaurants whose composition matches the inspection universe, i.e., 76% independent and 24% chain establishments. For each subsample, we match these restaurants to controls and reestimate the DiD model using Equation 1, generating 200 estimates using different random seeds. The resulting distribution of average treatment effects is highly stable: the average ATT across replications is  $-0.0913$  with a 95% confidence interval of  $(-0.0926, -0.0899)$ , closely aligned with our main estimate. Figure 2 shows a very narrow dispersion across iterations, ranging from  $-0.1216$  to  $-0.0644$ . These results indicate that our findings are not driven by sample bias. Instead, the estimated treatment effect remains robust to sampling uncertainty and to alternative constructions of the estimation sample that better reflect NYC’s restaurant population.

## 6.2 Allowing Unbalanced Panel Data

As discussed in Section 6.1, restaurants with lower and more variable transaction volumes—particularly independent establishments—are more likely to fall below SafeGraph’s monthly visibility threshold. When this happens, revenue is unobserved for that month, which gen-

Figure 2: Robustness Check: Distribution of ATTs Using Different Restaurant Samples



*Note.* The plot shows a distribution of treatment effects using Equation 2 and 200 different restaurant samples. Each sample randomly draws NYC restaurants such that the proportion of independent and chain establishments matches the composition observed in the restaurant universe based on inspection data. The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . The model specification includes restaurant and month fixed effects, and the control variables average consumer price index, population, temperature, temperature squared, precipitation and precipitation squared at the CBSA level. Standard errors are clustered at the restaurant level.

erates missing values in the panel. In our main analysis, we restrict the estimation sample to a fully balanced panel: we include only restaurants with complete monthly observations from July 2022 through July 2024. While this restriction enhances consistency of the DiD estimation, it may disproportionately exclude smaller restaurants and thus affect generalizability.

To assess the sensitivity of our findings to this sample-selection choice, we relax the balanced-panel requirement and allow restaurants to enter the analysis even if they have a limited number of missing observations. Specifically, we construct an unbalanced panel in which a restaurant is included as long as it has no more than  $X \in \{1, 2, 3\}$  missing months over the pre-treatment periods of the analysis period.

To include treated restaurants with at most  $X \in \{1, 2, 3\}$  missing months in the 14 pre-treatment periods and their matched controls, we proceed as follows. For  $X = 1$ , there are

14 possible single-month combinations. For each combination, we identify restaurants with missing data in that focal month. Since we match on pre-treatment outcomes, we temporarily exclude the focal month from the panel and perform PSM between these treated restaurants and a control pool consisting of restaurants with no missing values over the full analysis period. After matching, we modify each selected control by introducing a missing value in the corresponding focal month to ensure consistency with the matched treated unit, and then remove the matched control from the pool (matching without replacement). For  $X = 2$ , we consider all possible pairs of missing months and iterate the same procedure. This process is repeated for  $X = 3$  to include all restaurants with up to three missing months and their matched controls. Once all treated restaurants and matched controls are included, for each  $X$ , we re-estimate the DiD specification using all available observations for each restaurant.

Table 4 summarizes the results. The estimated treatment effects for  $X = 1$ ,  $X = 2$ , and  $X = 3$  are -0.1220, 0.1243, and -0.1294, respectively, which are larger than but close in magnitude to our baseline estimate based on the balanced panel. These findings indicate that our results are not driven by conditioning on restaurants with perfect month-to-month visibility, nor by excluding establishments with modest amounts of missing data.

### 6.3 Alternative Time Windows

Our main analysis uses the period from July 2022 to July 2024, comprising 14 pre-treatment months and 11 post-treatment months. Changing this window may affect (1) the matching procedure, which depends on pre-treatment revenue paths, and (2) the composition of the balanced panel, since the number of months required for inclusion determines which restaurants are observed in all periods within the specified window. We therefore assess the robustness of our results to alternative time windows.

We first shorten the pre-treatment period while holding the restaurant sample from the main analysis and the number of post-treatment months fixed. Because matching relies on pre-treatment time-varying covariates, reducing the number of pre-treatment months may

Table 4: Robustness Check: Allowing Unbalanced Panel Data

	(1) 1 Missing	(2) 2 Missing	(3) 3 Missing
Post $\times$ Treat	-0.1220*** (0.0106)	-0.1243*** (0.0103)	-0.1294*** (0.0102)
Covariates	✓	✓	✓
Restaurant FE	✓	✓	✓
Time FE	✓	✓	✓
Adjusted R <sup>2</sup>	0.7762	0.7695	0.7637
N Restaurants	7,856	8,944	9,834
N Obs	192,825	215,649	232,908

\*p<0.5, \*\*p<0.01, \*\*\*p<0.001

*Note.* The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . Columns 1, 2, and 3 allow treated restaurants with at most 1, 2, and 3 missing values in the pre-treatment periods, respectively, to enter the estimation. The models include control variables for average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at the CBSA level. Standard errors are clustered at the restaurant level.

affect the quality of the match. Columns 1–3 of Table 5 report results using 12, 10, and 8 pre-treatment months, respectively, instead of 14 of the main analysis. Across all specifications, the estimated treatment effects remain close to the baseline, indicating that our findings are not sensitive to the exact length of the pre-treatment period used for matching.

Next, we allow additional restaurants—those not included in the main balanced panel—to enter the analysis when they meet the observability criterion for the shorter pre-treatment windows. Columns 4–6 of Table 5 present the results. The estimates remain stable, suggesting robustness both to matching on fewer pre-treatment periods and to changes in the composition of restaurants that satisfy the balanced-panel requirement.

Finally, we shorten the post-treatment period while keeping the full 14-month pre-treatment window constant. Shorter post-treatment windows allow more restaurants to meet the balanced-panel criterion. To isolate the effect of changing the time window from changes in sample composition, we estimate each specification twice: once using the origi-

nal restaurant sample and once allowing the expanded set of eligible restaurants to enter. Columns 1–3 of Table 6 examine 9, 7, and 5-month post-treatment windows with the original sample, respectively, while columns 4–6 examine the same post-treatment windows (9, 7, and 5 months) with the new sample, respectively. Within each pair of columns with the same number of post-treatment periods (i.e., column 1 vs. 4; column 2 vs. 5; and column 3 vs. 6), the estimated treatment effects remain similar (i.e., -0.0940 vs. -0.0727; -0.0917 vs. -0.0798; and -0.0720 vs. -0.0635), suggesting that the results are robust to changes in the post-treatment period and to shifts in sample composition due to the shorter windows.

Table 5: Robustness Check: Shortening Pre-treatment Time Windows

	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.0940*** (0.0108)	-0.0917*** (0.0107)	-0.0720*** (0.0110)	-0.0727*** (0.0101)	-0.0798*** (0.0102)	-0.0635*** (0.0104)
Covariates	✓	✓	✓	✓	✓	✓
Restaurant FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Adjusted R <sup>2</sup>	0.7909	0.7958	0.8005	0.7835	0.7869	0.7970
N Restaurants	6,440	6,440	6,440	7,370	7,474	8,058
N Obs	148,120	135,240	122,360	169,510	156,954	153,102

\*p<0.5, \*\*p<0.01, \*\*\*p<0.001

*Note.* The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . Columns 1, 2, and 3 use 12, 10, and 8 pre-treatment periods, respectively, while holding the restaurant sample of the main analysis constant. Columns 4, 5, and 6 also use 12, 10, and 8 pre-treatment periods, respectively, but allow restaurants that meet the balanced panel criterion to enter the estimation. The models include control variables for average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at the CBSA level. Standard errors are clustered at the restaurant level.

## 6.4 Alternative Matching Method and Estimator

In our main analysis, we use propensity score matching as the matching method and the difference-in-differences as the estimation model.

**Coarsened Exact Matching (CEM).** As a first robustness check, we replace the propensity score matching used in the main analysis with CEM (Iacus et al., 2012). Unlike propen-

Table 6: Robustness Check: Shortening Post-treatment Time Windows

	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.1020*** (0.0113)	-0.0989*** (0.0117)	-0.0878*** (0.0120)	-0.0901*** (0.0108)	-0.0829*** (0.0112)	-0.0780*** (0.110)
Covariates	✓	✓	✓	✓	✓	✓
Restaurant FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Adjusted R <sup>2</sup>	0.7911	0.7919	0.7931	0.7821	0.7837	0.7857
N Restaurants	6,440	6,440	6,440	7,484	7,840	8,118
N Obs	148,120	135,240	122,360	172,132	164,640	154,242

\*p<0.5, \*\*p<0.01, \*\*\*p<0.001

*Note.* The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . Columns 1, 2, and 3 use 9, 7, and 5 post-treatment periods, respectively, while holding the restaurant sample of the main analysis constant. Columns 4, 5, and 6 also use 9, 7, and 5 post-treatment periods, respectively, but allow restaurants that meet the balanced panel criterion to enter the estimation. The models include control variables for average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at the CBSA level. Standard errors are clustered at the restaurant level. \*p<0.5, \*\*p<0.01, \*\*\*p<0.001

sity score matching—which balances treated and control restaurants on a scalar propensity score—CEM directly enforces balance on coarsened versions of the covariates, ensuring exact matching within bins of observed characteristics. This approach is more transparent, less model-dependent, and more robust to misspecification than the propensity score estimation.

We implement Coarsened Exact Matching (CEM) using the same set of baseline characteristics as in the main analysis. Specifically, we coarsen restaurants into strata based on (i) quarterly total spending (bin size = 13), (ii) the average number of customers in the 14 months prior to treatment (bin size = 13), and (iii) the average proportion of non-local customers over the same period (bin size = 2). After forming strata, we perform 1:1 nearest-neighbor matching within each stratum, ensuring that each treated restaurant is matched to exactly one control restaurant using the Mahalanobis distance metric<sup>17</sup>. Units in strata without overlap, as well as treated or control units without an available match, are dropped. The

<sup>17</sup>Mahalanobis distance is a measure of the distance between two points in multivariate space, which takes into account the covariance of the variables. This distance metric is often used in matching methods to account for the correlation between covariates and ensure that treated and control units are comparable based on their joint distribution of covariates (Iacus et al., 2012).

resulting matched sample consists of 5,996 restaurants (2,983 treated and 2,983 controls), retaining 92.64% of restaurants relative to the PSM-matched sample. We then re-estimate the DiD specification in Equation 1 on this CEM-matched sample. As shown in column 1 of Table 7, the estimated treatment effect of -0.0946 closely mirrors our baseline result in magnitude, indicating that our findings are robust to the use of CEM with 1:1 matching.

**Synthetic Difference-in-Differences (SynthDiD).** As a second robustness exercise, we estimate the treatment effect using the SynthDID estimator of [Arkhangelsky et al. \(2021\)](#). SynthDID combines elements of synthetic control and traditional DiD by constructing weighted averages of control restaurants to form a synthetic comparison group whose pre-treatment path closely approximates that of treated restaurants.

Although our main approach already matches pre-treatment outcomes, SynthDID provides an additional robustness check because it (i) assigns data-driven weights to control units based solely on pre-treatment outcomes, and (ii) places more weight on pre-treatment periods that are closer in outcome level to the post-treatment period. This produces a counterfactual comparison group via a different outcome-based weighting scheme than the one implied by our matching and DiD framework. For more details, see [Arkhangelsky et al. \(2021\)](#).

To keep consistency with our main analysis, we include the same set of covariates in SynthDID estimates. Following [Arkhangelsky et al. \(2021\)](#), we incorporate the adjustment by applying SynthDID to the residuals  $\log(Spend_{it} + 1)^{res} = \log(Spend_{it} + 1) - X_{ct}\hat{\gamma}$  of the regression of  $\log(Spend_{it} + 1)$  on  $X_{ct}$ . The estimated treatment effect of -0.0966, reported in column 2 of Table 7, is nearly identical to the baseline DiD estimate. This further confirms that our findings are not sensitive to the choice of estimator or outcome-based weighting scheme.

Table 7: Robustness Check: Alternative Matching and Estimation Methods

	(1) CEM	(2) SynthDiD
Post $\times$ Treat	-0.0946*** (0.0104)	-0.0966*** (0.0079)
Covariates	✓	✓
Restaurant FE	✓	✓
Time FE	✓	✓
N Restaurants	5,966	60,755
N Obs	149,150	1,518,875

\*p<0.5, \*\*p<0.01, \*\*\*p<0.001

*Note.* Column 1 reports the estimated treatment effect using Equation 1 and the Coarsened Exact Matching (CEM). Column 2 reports the estimated treatment effect using the Synthetic Difference-in-Differences (SynthDiD) model. Both are described in Section 6.4. The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . The model specifications include the control variables average consumer price index, population, temperature, temperature squared, precipitation and precipitation squared at CBSA level. Standard errors are clustered at the restaurant level.

## 6.5 Placebo Tests

We conduct two placebo tests to assess whether the main results may be driven by spurious correlations or chance.

**Placebo Treatment Timing.** We first shift the treatment date to a placebo month occurring 5 months before the actual implementation. For this placebo date, we repeat the matching procedure and re-estimate the DiD model using only the periods between the placebo date and the period preceding the true treatment month. Because no regulation changes occurred in these intervals, the estimated effects should be close to zero. Table 8 reports the results, which show no meaningful effect ( $\hat{\beta} = -0.006$ ,  $p = 0.633$ ).

**Placebo Treated Units.** Second, we assign treatment randomly by selecting restaurants from the 19 CBSAs to form a placebo treatment group of the same size as in the main analysis. We then re-match and re-estimate the DiD specification over the same analysis window. We repeat this procedure 200 times to get a distribution of ATTs. Since these units were not subject to the regulation, we expect a distribution centered on zero.

Figure 3 shows the distribution of the 200 ATTs. We observe that the distribution is, as expected, centered around zero (with a mean of -0.0007 and a 95% confidence interval of (-0.0019, 0.0004)). These tests suggest that the main findings are not driven by pre-existing trends or random variation.

## 7 Conclusions

This paper examines how New York City’s 2023 short-term rental regulation reshaped spending in the local restaurant sector. Using detailed credit and debit card transaction data and a difference-in-differences strategy, combined with propensity score matching, we document a sizable decline in restaurant spending following the implementation of one of the most restrictive STR policies in the United States. Our estimates indicate an average reduction

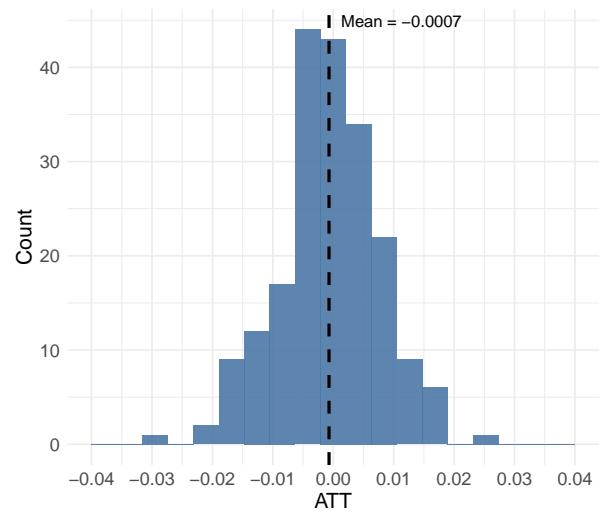
Table 8: Placebo Treatment Timing

	(1)
	Timing
Post × Treat	-0.0060 (0.0126)
Covariates	✓
Restaurant FE	✓
Time FE	✓
N Restaurants	6,440
N Obs	122,360

\*p<0.5, \*\*p<0.01, \*\*\*p<0.001

*Note.* The table reports the estimated treatment effect using Equation 1 where we assign April 2023 to be the placebo treatment timing instead of September 2023. The placebo post-treatment periods are from April 2023 to August 2023 (inclusive). The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . The model specifications include the control variables average consumer price index, population, temperature, temperature squared, precipitation and precipitation squared at CBSA level. Standard errors are clustered at the restaurant level.

Figure 3: Placebo Treated Units



*Note.* The plot shows a distribution of treatment effects using Equation 2 and 200 different restaurant samples. For each sample, we randomly assign restaurants to placebo treatment status, ensuring the number of treated restaurants matches that in the main analysis. The outcome variable is the logarithm of total consumer spend, i.e.,  $\log(Spend_{it} + 1)$ . The model specification includes restaurant and month fixed effects, and the control variables are the average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at the CBSA level. Standard errors are clustered at the restaurant level.

of roughly ten percent in monthly consumer spending at restaurants. This decline translates into an estimated annual revenue loss of approximately three billion dollars for the city's restaurant industry.

The effects are not evenly distributed across establishments. Restaurants that cater to tourists and those at higher price points experience the most substantial declines, suggesting that the regulation reduced both the number of visitors and the discretionary budgets of those who still chose to travel. These heterogeneous patterns are consistent with mechanisms related to higher accommodation costs and fewer available lodging options, both of which can reduce consumption in complementary urban sectors.

Taken together, the findings show that STR regulations designed to promote housing affordability can generate unintended spillovers on local service markets. While policymakers often focus on the direct effects of STR activity on rents, housing supply, and neighborhood stability, our results highlight the need to account for the broader ecosystem of urban consumption that depends on visitor flows and accommodation choices. Urban economies are deeply interconnected, and regulations targeted at one sector can affect others in ways that materially affect business performance, employment, and tax revenues.

The policy implications are therefore twofold. First, cities considering restrictive STR policies should weigh the housing benefits against potential losses to tourism-dependent industries. Second, complementary policies may be needed to mitigate negative spillovers, such as improving the supply of legal and affordable lodging or supporting the restaurant and hospitality sectors during periods of regulatory transition. More broadly, our study underscores the importance of evaluating urban housing policies through a wider economic lens, recognizing that interventions in one market inevitably shape outcomes in others.

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## Appendix

### A Few-shot Prompting with the GPT-5.1-mini Large Language Model

[System]

You are a helpful assistant with knowledge of the U.S. restaurant industry.

Your task is to determine whether each restaurant in a provided list is a chain or an independent establishment.

Please return your answer as a two-column table with:

Column 1: Restaurant name

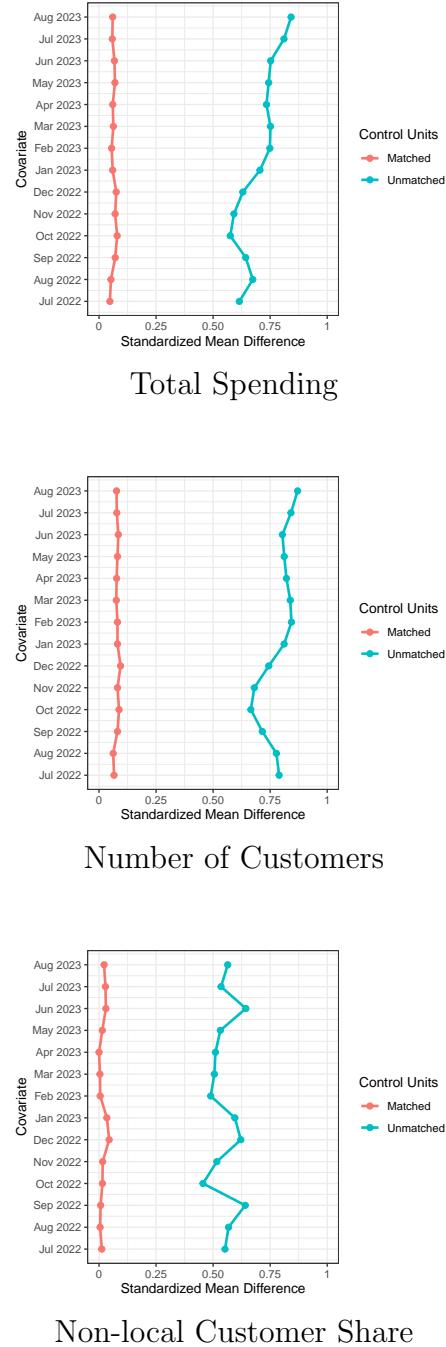
Column 2: Chain indicator (1 = chain restaurant, 0 = not a chain)

The output format should follow this example:

```
'Restaurant | Chain (1=chain, 0=not)',  
'--- | ---',  
'Surrender Cafe | 0',  
'KFC | 1'
```

## B Additional Figures and Tables

Figure 4: PSM Covariate Balance



*Note.* The figure reports standardized mean differences (SMDs) between treated and control restaurants before and after matching for three covariates: (a) Total Spending, (b) Number of Customers, and (c) Non-local Customer Share, over each pre-treatment period. The average SMDs before matching are 0.7011 for total spending, 0.7867 for the number of customers, and 0.5521 for non-local customer share. After matching, SMDs reduce to 0.0635, 0.0790, and 0.0169, respectively, suggesting satisfactory covariate balance.