The Impact of Short-Term Rental Regulations on Urban Spending: Evidence from Transactional Data*

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Abstract

We examine the effects of short-term rental (STR) regulations on urban service markets, focusing on the restaurant industry. Our setting is New York City's 2023 STR regulation (Local Law 18), which took effect in September 2023 and triggered an immediate 80% contraction in active Airbnb listings citywide. Using restaurant-month–level credit and debit card transaction data across 19 major US metropolitan areas, combined with a matched difference-in-differences design, we find that the policy led to a 7.5% decline in restaurant total spending, a 5.0% reduction in transaction volume, and a 2.3% decrease in average spending per transaction. These effects are consistent with multiple reinforcing mechanisms: the sharp reduction in STR supply reduced tourist inflows and raised accommodation costs, while simultaneously depressing local income through lost host revenues and reduced employment in tourism-dependent sectors. We provide empirical support for this interpretation through three complementary analyses. First, the declines in total spending and transaction volume are concentrated among restaurants catering to non-local customers and higher-priced establishments, whereas average spending per transaction falls similarly across restaurant types—a pattern consistent with reduced tourist visits and tourists' discretionary spending as the primary channel and a modest contraction in local discretionary spending as a secondary one. Second, NYC airports experienced a 3.5% to 4.3% decline in passenger arrivals relative to other major US airports, confirming reduced tourist inflows. Third, neighboring New Jersey markets experienced a 10.1% increase in restaurant spending, suggesting partial spatial displacement of economic activity rather than outright destruction of demand. Taken together, our findings highlight that STR regulations can have economically meaningful consequences well beyond the housing market, underscoring the importance of accounting for downstream impacts on local service industries in urban policy design.

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

1 Introduction

Short-term rental (STR) platforms are increasingly popular among travelers, offering access to a diverse and extensive supply at lower costs 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 the number of lodging options, Airbnb can attract additional tourists who otherwise would not have traveled, thereby increasing the total number 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 Rahman, 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 such as Airbnb stimulate local economies by attracting tourists and dispersing 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 to limit or strictly regulate STR activity, including licensing requirements, availability restrictions, and fines (Bekkerman et al., 2023; Foroughifar and Narang, 2025).

These STR policies are designed primarily to reduce STR supply and to increase (or at least preserve) 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) show 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 the number of available options for potential travelers, who, confronted with more expensive alternatives, may decide not to travel at all (Farronato and Fradkin, 2022), or travel but cut back on discretionary or hedonic expenditures due to higher stay costs. 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, if so, how substantial they are remain open empirical questions with important policy implications.

This paper addresses this question by examining how STR regulations affect urban service markets, with a focus on the restaurant sector. To do so, we exploit the implementation of New York City's Local Law 18 in September 2023—one of the most restrictive STR regulations in the United States—as a natural experiment. 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 led to a sharp decline in the number of active Airbnb listings, making most of the Airbnb supply illegal. By early November 2023 (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 (an 80% reduction), according to data from AirDNA, a company that analyzes performance metrics for over 10 million Airbnb and Vrbo vacation rentals worldwide.[1] This dramatic contraction in STR supply provides an ideal setting to study how restricting short-term rentals affects economic activity in complementary urban sectors.

We focus on the restaurant industry for several reasons. First, restaurants represent a large and dynamic sector of the urban economy, closely linked to tourism and neighborhood-level consumption (Basuroy et al., 2020; Schiff, 2015). Second, restaurant spending is a key component of the tourist experience and discretionary consumption, making it particularly sensitive to changes in visitor volumes and travel budgets. Third, the availability of high-frequency transaction data allows us to measure spending responses at the establishment

[1] See https://www.airdna.co/

level, enabling us to identify heterogeneous effects across restaurant types.

To evaluate the causal impact of the regulation on restaurant demand, we construct a rich panel dataset of monthly credit and debit card transactions for over 60,000 restaurants across 19 major US metropolitan areas, obtained from SafeGraph and spanning a period from July 2022 to July 2024. We consider only in-person, or "offline," transactions and exclude online orders from platforms such as DoorDash or GrubHub. This is because in December 2023, New York City implemented a minimum-wage policy that affected app-based food delivery platforms, likely affecting the volume and cost of online transactions.[2]

Our main identification strategy combines propensity score matching with a Difference-in-Differences (DiD) design, comparing changes in consumer spending at NYC restaurants before and after the regulation took effect, relative to a matched control group of similar restaurants in other major US cities.

We begin the analysis by documenting a substantial and immediate decline in restaurant spending following the implementation of Local Law 18. Our primary estimates indicate that the regulation led to a roughly 7.5% reduction in consumer restaurant spending over the 11 months following the policy's enactment. Decomposing this overall effect, we find that the regulation reduced offline transaction volume by approximately 5.0% and average spending per transaction by approximately 2.3%, indicating that the impact operates through both fewer customers visiting restaurants and lower spending among those who continue to visit. A simple back-of-the-envelope calculation suggests that the spending decline translated to a loss of approximately $1.85 billion in annual revenue for New York City's restaurant industry. The negative effect emerges immediately after the regulation takes effect, deepens over subsequent months as transitional grace periods expire (December 2023), and shows some signs of recovery only 9 months after the policy took effect.

Next, we provide further support for the mechanisms driving the observed effect through three complementary analyses. First, we examine which types of restaurants are most af-

[2] Including all transactions, we obtain similar results.

fected by estimating heterogeneous treatment effects across all three outcomes. We find that the declines in total spending and transaction volume are concentrated among restaurants catering to non-local customers and higher-priced establishments. Spending for restaurants with a higher share of non-local customers decreased by 14.5% (versus 0.4% for those catering to locals), driven primarily by an 8.1% reduction in transaction volume; similarly, spending for higher-priced restaurants decreased by about 12.3% (versus 2.8% for lower-priced ones), again driven by fewer transactions. In contrast, average spending per transaction declined modestly and uniformly across all restaurant types. These results, where the extensive margin effects are concentrated among tourist-oriented and higher-priced restaurants but the intensive margin effects are broadly shared, are consistent with reduced tourist visits as the primary channel and a modest contraction in local discretionary spending as a secondary one. Second, we analyze aggregate passenger traffic at NYC airports and find a statistically significant decline in passenger volumes relative to other major US airports, providing direct evidence for reduced tourist inflows. Third, we examine spatial spillovers and find evidence of modest demand reallocation to neighboring markets (New Jersey), where, after the policy, restaurant spending increased by approximately 10.1%, though not enough to offset the losses within NYC, as New Jersey's restaurant market is substantially smaller than NYC's.

Taken together, these results are consistent with several reinforcing mechanisms through which restricting STR supply affects urban consumption. The first and most direct mechanism is that LL18 eliminated over 80% of short-term rental listings, substantially reducing accommodation options for price-sensitive travelers who, faced with more expensive alternatives, may have chosen not to visit New York City at all (Farronato and Fradkin, 2022). Second, beyond the direct loss of Airbnb travelers, the contraction in short-term rental supply tightened the broader accommodation market and increased accommodation prices.[3] Visitors who did decide to visit New York City, therefore, faced higher accommodation costs,

[3]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. Revenue per available room (RevPAR) increased by 15.6%. In contrast, the national RevPAR average increased by only about 0.3%.

which left them with less of their travel budget for discretionary spending. Third, the regulation reduced local income through two related channels: thousands of Airbnb hosts lost their rental income, and the broader tourism-dependent workforce faced reduced hours, tips, and employment as visitor foot traffic decreased (prior work documents a positive causal link between Airbnb activity and local restaurant employment (Alyakoob and Rahman, 2022)). This reduction in local income likely translates into lower discretionary spending by NYC residents, including spending at local restaurants.

We conclude the paper by presenting a battery of robustness checks that reinforce the causal interpretation of our results. Our estimates remain stable across different matching methods (propensity score matching, coarsened exact matching, and synthetic difference-in-differences), alternative time windows, sample representativeness corrections, and specifications that relax the balanced panel requirement. Finally, two separate placebo tests—one shifting the treatment date to a pre-regulation period and another randomly assigning treatment status—yield null effects.

The findings presented in this paper contribute to and extend the literature on platform regulation and urban economics. First, while previous research has examined how STR expansion affects local economies (Alyakoob and Rahman, 2022; Basuroy et al., 2020; Hidalgo et al., 2024), we study how restrictive STR regulation affects complementary urban sectors. This distinction is crucial: whereas marginal changes in STR activity may generate localized effects, large-scale regulatory interventions can trigger general equilibrium responses involving spatial reallocation, behavioral adjustments, and cross-sector spillovers. Our setting allows us to identify these broader equilibrium effects.

Second, our findings have important policy implications. The existing literature on STR regulation has focused primarily on intended effects—reducing STR supply and increasing housing affordability (Barron et al., 2021; Garcia-López et al., 2020)—and on some unintended consequences within the housing market itself (Bekkerman et al., 2023). Our results demonstrate that these regulations can also affect complementary sectors of the urban economy. While policymakers designing STR restrictions focus on housing market outcomes, our results suggest that a complete welfare analysis must account for losses to tourism-dependent industries, reductions in city tax revenues, and potential employment effects in the service sector. Moreover, the spatial spillovers we document indicate that city-level regulations may simply relocate economic activity to neighboring jurisdictions rather than preserving it locally, raising questions about the appropriate geographic scope of STR policy.

Finally, our analysis underscores the deeply interconnected nature of urban economies. Interventions in one market—even those designed with clear housing policy objectives—inevitably reshape activity in others. The restaurant sector's dependence on tourism and visitor spending makes it particularly vulnerable to policies that increase accommodation costs or reduce lodging availability. More broadly, our findings suggest that effective urban policy requires understanding not only the direct effects of regulation on targeted markets, but also the indirect effects that propagate through the broader urban ecosystem.

2 Related Literature

Most of the literature on STRs focuses on their effects on housing markets, particularly on rents and house prices. Empirical studies consistently identify a causal relationship between STR expansion and increases in rents, house prices, and neighborhood investments (Barron et al., 2021; Bekkerman et al., 2023; Garcia-López et al., 2020; Xu and Xu, 2021), which are particularly driven by commercial Airbnb listings (Duso et al., 2024). Additionally, reallocating housing to short-term markets has imposed substantial welfare losses on tenant households, including an estimated $2.7 billion increase in rents paid by NYC tenants, partially offset by $0.3 billion in gains to hosts, resulting in a net welfare loss of approximately $2.4 billion (Calder-Wang, 2021).

While the impact of STRs on housing is well-documented, less attention has been paid to their broader economic effects, particularly on urban services. This paper contributes to the literature by exploring the economic spillover effects of STR restrictions on consumer spending in local services, with a focus on the restaurant industry. STR activity can significantly impact these complementary sectors through changes in tourism and shifts in local spending patterns, but the scale and nature of these effects remain underexplored.

Recent studies have begun addressing this gap by examining the link between STRs and local economic outcomes. Alyakoob and Rahman (2022) find that a one percentage point increase in Airbnb activity in NYC leads to a 1.7% increase in restaurant employment. Similarly, Basuroy et al. (2020) show that a 1% increase in Airbnb reviews in Texas is associated with a 0.011% increase in restaurant revenue, accounting for approximately 12% of the median annual growth. These positive effects are most pronounced for independent restaurants and in less commercial areas. Additionally, Hidalgo et al. (2024) find that STR expansion in Madrid leads to positive local effects on the number of food and beverage establishments and their employees, especially in less tourist-heavy neighborhoods.

However, the expansion of STRs can also reshape the urban landscape by displacing local, resident-oriented businesses in favor of tourist-focused services. Hidalgo et al. (2023) highlight this shift, showing that STR activity often replaces neighborhood staples like drugstores and butcheries with bars and restaurants. This shift in amenity provision toward tourist-oriented services can benefit younger residents, compensating them for rising rents, but may exacerbate challenges for older residents (Almagro and Domínguez-Iino, 2025).

This paper extends the literature on STRs and local economic activity by examining the consequences of restrictive STR regulation rather than incremental STR expansion. While Basuroy et al. (2020) and Alyakoob and Rahman (2022) study how marginal increases in Airbnb activity affect restaurant outcomes, we exploit NYC's 2023 STR policy as a large, discrete, and plausibly exogenous shock that sharply reduced STR supply. This setting allows us to identify the equilibrium effects of STR withdrawal on urban consumption. Using matched difference-in-differences and high-frequency transaction data across major U.S. cities, we show that limiting STRs leads to economically meaningful declines in restaurant spending, particularly for higher-priced and tourist-oriented establishments, highlighting an important and previously underexplored cost of STR regulation.

3 Context and Data

3.1 NYC's STR Regulation: Local Law 18

NYC has regulated short-term rentals (STRs) for more than a decade, primarily through restrictions on residential unit rentals for fewer than 30 days unless a permanent resident is present. Prior to 2023, these rules were already codified in city administrative law, but enforcement capacity was limited, and compliance was uneven, allowing a substantial number of illegal listings to operate on STR platforms.

In response to growing concerns regarding housing affordability, neighborhood safety, and the conversion of long-term housing into short-term accommodations, the NYC Council passed *Local Law 18 of 2022*, also known as the Short-Term Rental Registration Law. The law became effective on September 5, 2023, following a delayed implementation period intended to allow hosts and platforms to comply with the new requirements (New York City Council, 2022; New York City Mayor's Office of Special Enforcement, 2023).

Local Law 18 substantially strengthened the city's regulatory framework through three key provisions. First, it requires all short-term rental hosts to register with the Mayor's Office of Special Enforcement (OSE) and demonstrate that the dwelling is their primary residence. Second, the law prohibits most entire-home rentals for fewer than 30 days unless the host is physically present during the stay and limits occupancy to no more than two guests at a time. Third, and critically for enforcement, booking platforms are prohibited from processing transactions for listings that are not registered with the city, effectively shifting enforcement responsibilities to STR intermediaries (New York City Mayor's Office of Special Enforcement, 2023).

The law also introduced substantial penalties for noncompliance. Hosts who violate the regulation face monetary fines, and booking platforms that facilitate unregistered rentals are subject to enforcement actions. According to the Mayor's Office of Criminal Justice, these provisions significantly increased compliance incentives and reduced the prevalence of illegal listings relative to prior regulatory regimes (Mayor's Office of Criminal Justice, 2024).

The implementation of Local Law 18 led to an immediate and pronounced contraction in short-term rental supply in NYC. Official city reports indicate that tens of thousands of illegal listings were removed following enforcement, and only a small fraction of prior STR operators successfully registered under the new rules (Mayor's Office of Criminal Justice, 2024). Industry data sources tracking STR platforms similarly document a decline of more than 80% in active Airbnb listings in the months following the law's effective date.[4]

Importantly, the implementation of Local Law 18 did not grant permanent grandfathered status to existing hosts; instead, it included a negotiated wind-down period for pre-existing bookings. In coordination with the NYC Mayor's Office of Special Enforcement (OSE), Airbnb issued guidance to hosts stating that short-term reservations made prior to the September 5, 2023 enforcement date would be permitted to proceed only if the guest check-in occurred on or before December 1, 2023. For these stays, Airbnb also refunded its platform service fees. Any reservations with check-in dates on or after December 2, 2023, were subject to mandatory cancellation and full refund. This administrative transition enabled a phased exit of non-compliant listings, effectively delaying the full contraction of New York City's short-term rental market until the end of 2023.

From an economic perspective, Local Law 18 constitutes a large, discrete, and plausibly exogenous shock to the supply of short-term accommodations in NYC. The timing of the policy was predetermined, applied citywide, and not targeted at specific neighborhoods or downstream sectors such as restaurants. This setting therefore provides a natural experiment for examining how sharp reductions in STR availability affect complementary urban markets, including consumer spending in the restaurant sector.

[4] See https://www.airdna.co/blog/nycs-short-term-rental-crackdown

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).[5] The data includes information on more than 1.1 million points-of-interest (POIs) in the United States (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 10.6 million debit/credit card users. The provided information shows the total number of transactions, the total number of customers with at least one transaction at a POI, and the total amount spent. This includes in-person transactions made at the POI as well as online transactions.[6]

Throughout the analyses presented in this paper, we restrict our spending measure to in-person (offline) transactions only, excluding online orders placed through food delivery platforms. We do so to avoid conflating the effect of the STR regulation with that of a concurrent NYC-specific policy on delivery worker pay that took effect in December 2023 and may have independently affected online food ordering.[7]

[5]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.

[6]For more details, see `https://docs.safegraph.com/docs/spend#section-online-vs-in-person-transactions` and `https://docs.safegraph.com/docs/spend-summary-statistics`.

[7]Starting in December 2023, the NYC Department of Consumer and Worker Protection began enforcing a minimum pay rate of $17.96 per hour for app-based restaurant delivery workers, which may have raised delivery costs and suppressed online orders in NYC relative to our control cities. Including online transactions in the outcome variable would therefore risk attributing any delivery-driven changes in spending to the STR regulation. See `https://www.nyc.gov/site/dca/news/018-24/mayor-adams-first-annual-increase-minimum-pay-rate-app-based-restaurant-delivery`.

From the data, we can infer economic activity associated with each POI and in each month, a widely adopted and validated approach in prior research (e.g., Chiong et al., 2025; Knight 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 11 months after. We obtain records for all POIs pertaining to Food Services and Drinking Places.[8] We also restrict the sample to restaurants that (i) are located in 19 major core-based statistical areas (CBSAs) in the United States,[9] and (ii) have no missing values for any month in the analysis period.

Because the STR regulation is implemented in NYC, some consumers or tourists affected by the restriction could shift their visitation patterns and, in turn, their 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 remaining 18 CBSAs. The NYC sample represents roughly 45% 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

[8]These places are identified by the North American Industry Classification System (NAICS) code, and their first three digits are 722.

[9]Our sample includes restaurants in Atlanta–Sandy Springs–Roswell, GA; Baltimore–Columbia–Towson, 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.

the robustness checks in Section 7.1.

The number of consumers for each restaurant and month is broken down by the inferred customers' home city.[10] 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.

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 affected 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).[11] 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.

[10] 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.

[11] https://www.ncei.noaa.gov/access/past-weather/

Summary Statistics. Table 1 reports summary statistics for the key variables used in the analysis, separately for restaurants located in NYC and those located in the remaining CBSAs. Several notable patterns emerge that are directly relevant for the interpretation of our empirical results.

Table 1: Summary Statistics

	All Restaurants			Restaurants in NYC			Restaurants in other CBSAs		
Variable	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Offline Spending	3,443.00	1,530.00	29,521.68	1,299.50	590.40	3,799.94	3,563.00	1,614.00	30,318.74
Number of Offline Transactions	180.10	67.00	2,139.76	54.23	28.00	222.92	187.10	70.00	2,197.98
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. Offline Spending, Number of Customers, Local/Non-Local Customers and Offline Transactions are at the restaurant level. Average Temperature, Precipitation, CPI, and Population are at the CBSA level. Local/Non-Local Customers are calculated based on pre-regulation months.

Restaurants in NYC operate on a smaller scale than those in other metropolitan areas, as reflected in lower average customer volumes and transaction activity. This pattern is consistent with a dense, highly competitive market in which establishments tend to be smaller and face higher operating costs, consistent with descriptive evidence of the fragmented structure of the NYC restaurant industry (Office of the New York City Comptroller, 2019).

NYC restaurants also differ markedly in customer composition. Despite lower overall customer volumes, they serve a relatively larger share of non-local customers, reflecting the city's reliance on tourism and visitor demand (González-Rivera et al., 2018). This feature is particularly relevant in our setting, as shocks to visitor accommodations—such as those induced by the contraction in short-term rental supply—are likely to have stronger effects on restaurants that depend more heavily on non-local demand (Mayor's Office of Criminal Justice, 2024).

The data further reveal substantial dispersion in customer composition and activity within both the NYC and non-NYC samples. This dispersion motivates our empirical strategy: by incorporating restaurant fixed effects and matching on pre-treatment trajectories—including the share of non-local customers—we ensure that treated and control restaurants are comparable prior to the implementation of the STR regulation and that estimated effects are not driven by pre-existing differences in restaurant scale or demand.

Finally, NYC differs from other CBSAs along several local economic and environmental dimensions. The Consumer Price Index is higher, reflecting a higher cost of living and price level faced by consumers. Population size and weather conditions, including temperature and precipitation, also vary substantially across CBSAs and can influence dining behavior. These differences underscore the importance of controlling for local economic conditions, population, and weather in the empirical analysis.

4 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 18 CBSAs. To do so, we use 1-to-1 propensity score matching based on pre-treatment time-varying covariates.[12] Specifically, we use the number of customers, the consumer total spending, and the share of 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 3,220 NYC restaurants to 3,220 in the remaining CBSAs. In Figure 5 in Appendix C, 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 matched restaurant set, we implement a DiD identification strategy. DiD compares changes in restaurant demand for treated restaurants, before and after the treatment,

[12] In Table 15 of Appendix C, we show that our findings are robust to 1-to-N matching with N as 5 and 7.

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 is usual in these settings, we partially test this assumption using an event-study design, which allows us to compare pre-treatment differences in outcomes between treated and control restaurants.

The main DiD specification takes the following form:

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

where the dependent variable Y_{it} represents our outcomes of interest for restaurant i and year-month t. We estimate this specification on three different dependent variables: (i) total consumer spending, which serves as our primary outcome; (ii) the number of customer transactions; and (iii) the ratio of (i) over (ii), i.e., average spending per transaction. This decomposition allows us to examine whether any decline in total spending is driven by reduced customer volume, lower per-customer spending, or a mix of both. $Post_t$ 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 that takes the value 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.[13] α_i are restaurant fixed effects, which control for time-invariant differences across restaurants, and τ_t are time fixed effects, which control for temporal shocks common across all restaurants. β, the coefficient of interest, measures the average treatment on the treated (ATT), that is, the average effect of the regulation on the outcome variable for

[13]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.

restaurants in NYC. We follow the recommendations of de Chaisemartin and D'Haultfœuille (2023) and cluster standard errors at the restaurant level (see Chapter 2, Page 31).[14]

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. 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:

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

where $\mathbb{1}_{\{t-t^*=k\}}$ 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 β_k, which is the ATT for time period k relative to the treatment period. As is usual, we set the baseline period to be the period just before the regulation took effect, i.e., August 2023.

5 Results

This section presents our main findings on the impact of NYC's STR regulation on restaurant demand.

5.1 Identification Check

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 using the log of total spending as outcome in Figure 1.[15] The event study considers 14 months before and 11 months after

[14] In Table 14 of Appendix C, we show estimates when clustering standard errors at the treatment level (CBSA). While, as expected, standard errors increase slightly, estimates remain statistically significant.

[15] We present the event study for the log of number of transactions in Figure 4 of Appendix C.

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 restaurant spending between treated and control restaurants before the regulation, supporting the parallel trends assumption.

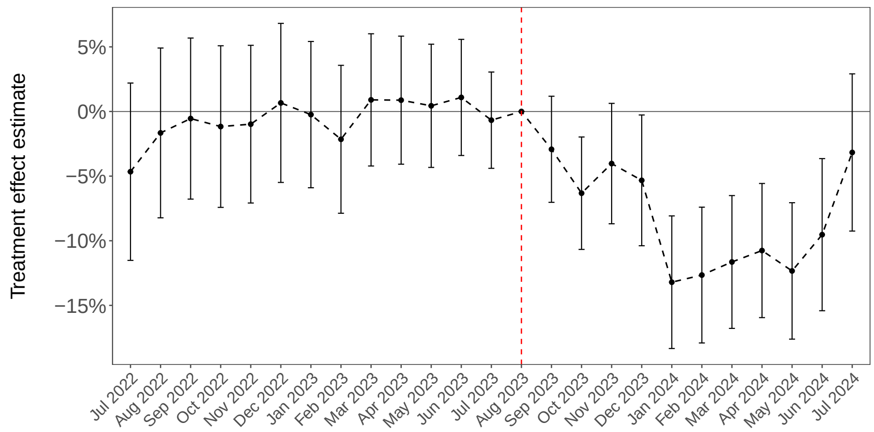
In the post-regulation period, the estimates turn negative immediately after the regulation is implemented and continue to decline over subsequent months. This sharp and immediate decrease is consistent with Airbnb's own reporting, which documents a rapid decline in Airbnb activity following the enforcement of Local Law 18 (Airbnb Inc., 2024). The treatment effects gradually deepen over time, reaching their lowest level around January 2024, when they stabilize at approximately a negative 13%.[16]

The pronounced dip observed in January 2024 aligns with the expiration of the transitional grace period established during the implementation of Local Law 18. Following the final legal decision, Airbnb, in coordination with the NYC Mayor's Office of Special Enforcement (OSE), notified hosts that they must either obtain a city registration number or convert to long-term stays (30+ days). To mitigate immediate travel disruptions, a wind-down policy was enacted: short-term reservations booked prior to September 5, 2023, were permitted to proceed if check-in occurred on or before December 1, 2023, and Airbnb refunded its service fees. Conversely, all reservations with check-in dates on or after December 2, 2023, were subject to mandatory cancellation and refund.[17] As these authorized legacy bookings concluded in December, the subsequent "hard stop" in activity can explain the sharp decline in short-term rental volume and associated consumer spending observed in early 2024. While a partial recovery begins to emerge in June 2024 (about ten months post-implementation), these results suggest that the regulation exerted a notable negative pressure on local restaurant revenue during the initial enforcement phase.

[16] We observe a very similar pattern for the number of transactions, see Figure 4 in Appendix C.

[17] See https://www.washingtonpost.com/travel/2023/09/01/nyc-airbnb-rules-local-law-18/

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 offline consumer spending at the restaurant-month level. 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. The vertical dashed red line in August 2023 marks the month preceding the regulation's implementation.

5.2 Overall Average Treatment Effect on the Treated (ATT)

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. In column 2, we report estimates that include the time-varying controls discussed in Section 4. Consistent with the event study results, we find a substantial decrease of about 7.5% ($\hat{\beta} = -0.078$, $p < 0.001$) in consumer restaurant spending after the regulation took effect.[18] The estimate remains negative and of similar magnitude to that in column 1. To contextualize the magnitude of this estimate, Appendix B presents a simple back-of-the-envelope calculation suggesting an aggregate revenue loss of approximately \$1.85 billion for the restaurant industry.

To understand whether this decrease is due to lower transaction volume or lower spending per transaction, we estimate the policy effect on the log of the number of transactions and average spending per transaction (i.e., total spending divided by the number of transactions). Table 3 presents these results. Columns 1 and 2 show that the regulation led to

[18]To interpret the coefficient $\hat{\beta}$ as a percentage change, we calculate $\exp(\hat{\beta}) - 1$. For $\hat{\beta} = -0.078$, this gives a 7.5% decrease in consumer restaurant spending after the regulation took effect.

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

	(1)	(2)
Post × Treat	-0.0909^{***}	-0.0780^{***}
	(0.0211)	(0.0206)
Covariates		✓
Restaurant FE	✓	✓
Time FE	✓	✓
Adjusted R^2	0.8450	0.8452
N	161,000	161,000

Significance levels: *p<0.05, **p<0.01, ***p<0.001

Note. The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of offline consumer spending at the restaurant-month level. Column 2 includes the control variables average consumer price index, population, temperature, temperature squared, precipitation and precipitation squared at CBSA level. Standard errors clustered at the restaurant level are reported in parentheses.

a 5.8% reduction in the number of transactions without controls ($\hat{\beta} = -0.060$) and 5.0% with controls ($\hat{\beta} = -0.051$), both $p < 0.001$, indicating that fewer customers are visiting restaurants or visiting less frequently. Columns 3 and 4 show that average spending per transaction also declined, though the magnitude is smaller at approximately 2.7% without controls ($\hat{\beta} = -0.027$, $p < 0.01$) and 2.3% with controls ($\hat{\beta} = -0.023$, $p < 0.05$).

These findings indicate that the overall decline in total restaurant spending operates through both the extensive margin (fewer transactions) and the intensive margin (lower spending per transaction), with the extensive margin accounting for the majority of the effect. The reduction in transaction volume suggests that the regulation reduced the number of visitors to NYC restaurants, while the decline in average spending indicates that those who continue to visit are also spending less per occasion, consistent with tighter travel budgets due to higher accommodation costs.

Table 3: The Impact of the STR Regulation on Consumer Restaurant # Transactions and Average Spending

	(1) # Transactions	(2) # Transactions	(3) Average Spending	(4) Average Spending
Post × Treat	−0.0595***	−0.0508***	−0.0272**	−0.0229*
	(0.0132)	(0.0131)	(0.0105)	(0.0103)
Covariates		✓		✓
Restaurant FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Adjusted R^2	0.8673	0.8675	0.8172	0.8173
N	161,000	161,000	161,000	161,000

Significance levels: *p<0.05, **p<0.01, ***p<0.001

Note. The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of the number of offline transactions for Column 1 and 2, and the logarithm of offline average spending for Column 3 and 4, at the restaurant-month level. Column 2 and 4 include the control variables average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at CBSA level. Standard errors clustered at the restaurant level are reported in parentheses.

6 Additional Analyses

The previous section established that NYC's STR regulation led to a substantial decline in restaurant spending, operating through both reduced transaction volume and lower average spending per visit.

To shed further light on the mechanisms behind this effect, we conduct three additional analyses. First, we investigate heterogeneity in treatment effects across restaurant types to identify which establishments are most affected by the regulation. Second, we provide direct evidence on changes in tourist inflows by analyzing passenger traffic at NYC airports. Third, we provide evidence of spatial displacement of tourism activity.

6.1 Heterogeneity Analysis

As discussed above, we expect the regulation-driven reduction in STR supply to reduce tourist inflows and discretionary spending, thereby disproportionately affecting higher-priced restaurants and those catering to tourists. To test whether this is indeed the case, we examine treatment heterogeneity by restaurant type: low- vs. high-priced restaurants and restaurants catering to local vs. non-local customers. To do so, we estimate a modified version of Equation 1:

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

where $Group_{g(i)}$ is an indicator variable equal to 1 if restaurant i belongs to group g, and 0 otherwise. In the price analysis, $Group_{g(i)}$ equals 1 for higher-priced restaurants (vs. lower-priced), and in the non-local customer analysis, it equals 1 for restaurants with a higher share of non-local customers (vs. a lower share). α_i and τ_t denote restaurant and month fixed effects, respectively, and ϵ_{it} is the error term. The coefficient β_1 captures the treatment effect for the baseline group, i.e., lower-priced restaurants or those with a lower share of non-local customers. The interaction term β_2 captures the differential treatment effect for higher-priced restaurants or those with a higher share of non-local customers, such that $\beta_1 + \beta_2$ represents the total treatment effect for these restaurants. The coefficient δ captures group-specific changes in outcomes in the post period that are common to both treated and control units, thereby accounting for differential time trends across groups.

Share of Non-local Customers. To identify restaurants more likely to cater to non-locals, we calculate the pre-regulation share of customers whose home location (inferred by the data provider) is outside the focal restaurant's CBSA. Then, we split restaurants by the median of this measure and estimate Equation 3. Columns 1, 3, and 5 of Table 4 report these results for total spending, number of transactions, and average spending per transaction, respectively.

The results reveal that the spending decline is almost entirely concentrated among restaurants with a higher share of non-local customers. Column 1 shows that restaurants with a lower share of non-local customers experience a negligible and statistically insignificant decline of about 0.4%, while those with a higher share experience a substantially larger decline of roughly 14.5% ($p < 0.001$). Decomposing this effect across margins, Column 3 shows that the extensive margin drives the bulk of the decline: restaurants with a higher non-local share see a significant 8.1% ($p < 0.001$) reduction in the number of transactions, compared to a small and insignificant 1.9% reduction for lower-share restaurants. Column 5 further shows that average spending per transaction declines significantly by about 1.1% ($p < 0.01$) for lower-share restaurants, and by about 2.1% ($p < 0.001$) for higher-share restaurants. The fact that the regulation's impact on total spending and transactions is concentrated among restaurants catering to non-locals provides direct evidence that the primary mechanism operates through a reduction in tourist and visitor demand. The modest decline in average spending observed even among restaurants with fewer non-local customers is consistent with a secondary channel in which the regulation reduces local income—among Airbnb hosts and tourism-related workers—thereby dampening local discretionary spending.

Price Level. To compute each restaurant's average price, we use the pre-treatment average spending at each restaurant. Specifically, for each restaurant, we first aggregate total consumer spending and the total number of transactions during the pre-regulation months, and then calculate the average cost per transaction. We then split restaurants at the median of this measure and estimate Equation 3 to obtain the ATT for each of the two restaurant types.

Columns 2, 4, and 6 of Table 4 report these results. Column 2 shows that higher-priced restaurants experience a substantially larger decline in total spending: the combined effect ($\hat{\beta}_1 + \hat{\beta}_2$) implies a significant decline of roughly 12.3% ($p < 0.001$), whereas the estimated decline for lower-priced restaurants is about 2.8% and not statistically significant. Column 4 reveals a similar pattern for the number of transactions: higher-priced restaurants see an additional decline of about 7.5% ($p < 0.01$), while lower-priced restaurants show a smaller and insignificant reduction of 1.4%. In contrast, Column 6 shows that average spending per transaction declines by a similar magnitude for both lower-priced (1.4%, $p < 0.001$) and higher-priced restaurants (1.9%, $p < 0.001$), with no significant differential effect. This pattern, where the spending and transaction declines are concentrated among higher-priced restaurants, but the per-transaction spending decline is uniform, reinforces the non-local demand channel: tourists, who disproportionately patronize higher-priced establishments, reduce their visits rather than merely scaling back spending within restaurants. The uniform decline in average spending per transaction across price tiers is again consistent with a modest reduction in discretionary spending that affects all restaurants, likely driven by lower local income associated with the contraction of the STR market.

Overall, the heterogeneous effects discussed in this section are consistent with both a reduction in tourist influx and a reduction in discretionary spending among tourists who are still willing to travel despite higher accommodation costs, as well as, to a smaller extent, among locals whose income may be affected by the regulation.

6.2 Impact on Tourist Inflows: Evidence from Airport Passenger Data

In this section, we present direct evidence of a reduction in tourist inflow.

Data. We collect monthly airport-level passenger data from the Bureau of Transportation Statistics (BTS).[19] The data report the total number of arriving passengers for each airport in each month. Our sample includes all 30 US major airports (defined by BTS) and spans the same time period as our restaurant analysis, from July 2022 through July 2024. This alignment ensures that any changes in passenger volumes are measured over the same pre- and post-regulation windows used in our main analysis.

We classify NYC's three major airports—John F. Kennedy International Airport (JFK), LaGuardia Airport (LGA), and Newark Liberty International Airport (EWR)—as treated

[19] https://www.transtats.bts.gov/data_elements.aspx

Table 4: The Heterogeneous Impact of the STR Regulation

	Total Spending		# Transactions		Average Spending	
	(1) Non-local Share	(2) Price Level	(3) Non-local Share	(4) Price Level	(5) Non-local Share	(6) Price Level
Post × Treat	-0.0044	-0.0282	-0.0190	-0.0141	-0.0112^{**}	-0.0139^{***}
	(0.0239)	(0.0189)	(0.0165)	(0.0154)	(0.0039)	(0.0034)
Post × Treat × HigherShare	-0.1520^{***}		-0.0657^{*}		$-0.0105^{\dagger}$	
	(0.0419)		(0.0262)		(0.0058)	
Post × Treat × HigherPrice		-0.1031^{*}		-0.0752^{**}		-0.0051
		(0.0414)		(0.0261)		(0.0057)
After × Group	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
Restaurant FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.8454	0.8461	0.8676	0.8680	0.9382	0.9382
N	161,000	161,000	161,000	161,000	161,000	161,000

Significance levels: $^{\dagger}p<0.1$, $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$

Note. The table reports the estimated treatment effect using Equation 3. The outcome variable is the logarithm of offline consumer spending in Columns 1-2, the number of offline transactions in Columns 3-4 and average spending per transaction in Columns 5-6 at the restaurant-month level. In Columns 1, 3, and 5, the coefficients on *Post* × *Treat* captures the effect for restaurants with a lower share of non-local customers, whereas *Post* × *Treat* × *HigherShare* shows the differential effect for restaurants with a higher share of non-local customers. In Column 2, 4 and 6, the coefficients on *Post* × *Treat* captures the effect for lower-priced restaurants, while *Post* × *Treat* × *HigherPrice* shows the differential effect for higher-priced restaurants. The models include *Post* × *Group* and control variables for average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at the CBSA level. Standard errors clustered at the restaurant level are reported in parentheses.

units. All other US airports serve as potential controls. While EWR is geographically located in New Jersey, it is a primary gateway for travelers to NYC and serves a substantial share of visitors to the city; accordingly, we include it in the treated airport system.

Empirical Strategy. To estimate the effect of the STR regulation on tourist inflows, we proceed in three steps. First, we estimate a baseline DiD specification similar to Equation 1, replacing restaurant with airport fixed effects, and using the full sample of treated and control airports (3 and 27 units, respectively).

Second, similar to the restaurant spending analysis, we combine propensity score matching (PSM) with DiD to improve comparability between treated and control units. We implement 1:2 matching, pairing each of the three treated airports with two control airports (six controls in total) that exhibit similar pre-regulation passenger volume trajectories. The matching variables consist of 1) the total number of arriving passengers in each pre-regulation month, which captures both the level and seasonal dynamics of airport traffic prior to the regulation, and 2) the average share of international passengers across the pre-regulation months, which captures differences in the extent to which airports serve international travelers.[20] Using this matched sample, we re-estimate the DiD specification in Equation 1.

Third, we use the Synthetic Difference-in-Differences (SynthDiD; Arkhangelsky et al., 2021) estimator, which generalizes the DiD framework by assigning weights to both control units and pre-regulation time periods. This method places greater weight on control airports whose pre-regulation passenger outcomes are most similar to those of the treated airports, as well as on pre-regulation months that are most comparable to post-regulation periods. By jointly reweighting units and time periods, SynthDiD further relaxes parallel trends assumptions and provides a complementary estimate of the regulation's impact.

Across all specifications, we consider two outcome measures: (i) the logarithm of monthly

[20] Matched control airports include Charlotte Douglas International Airport (CLT), Ronald Reagan Washington National Airport (DCA), Washington Dulles International Airport (IAD), Chicago O'Hare International Airport (ORD), Phoenix Sky Harbor International Airport (PHX), and San Francisco International Airport (SFO).

passenger volume and (ii) the level of monthly passenger volume at the airport level.

Results. Table 5 reports the estimated effects of the STR regulation on airport passenger volumes using the baseline DiD, PSM-DiD, and SynthDiD specifications. Columns 1–3 present results using the logarithm of monthly passenger volume, while columns 4–6 report estimates using the raw number of passengers.

Across all three estimators, we find consistent evidence of a decline in passenger traffic at NYC airports following the implementation of the STR regulation. Using the log specification (columns 1–3 of Table 5), the estimated treatment effects range from 3.5% to 4.3% and are statistically significant across the DiD, PSM-DiD, and SynthDiD models. These estimates indicate a meaningful contraction in air travel demand relative to the corresponding control airports.

Results using passenger volumes in levels (columns 4–6 of Table 5) are economically similar. The baseline DiD and PSM-DiD estimates imply reductions of approximately 78,000 to 94,000 arriving passengers per airport-month, respectively, and are statistically significant. The SynthDiD estimate is of comparable magnitude but less precise and not statistically significant. Taken together, the log and level specifications point to a sizable and robust decline in airport passenger traffic associated with the STR regulation. Interestingly, these magnitudes are consistent with an Airbnb report documenting a similar decline in Airbnb guest activity in NYC following the regulation (Airbnb Inc., 2024).

6.3 Spatial Spillovers to Neighboring Markets

In addition to lower tourist inflow, the spatial displacement of travelers can also reduce restaurant demand in NYC. This would be the case if the reduction in NYC's STR supply increased the relative attractiveness of nearby and cheaper locations. In particular, in New Jersey, accommodations are less expensive than in NYC while remaining within commuting distance of Manhattan.

Table 5: The Impact of the STR Regulation on Airport Passenger Volume

	Log Passenger Volume			Passenger Volume		
	(1) DiD	(2) PSM-DiD	(3) SynthDiD	(4) DiD	(5) PSM-DiD	(6) SynthDiD
Post × Treat	−0.0359**	−0.0443*	−0.0380**	−77,850.0***	−93,522.7*	−51,987.2
	(0.0119)	(0.0190)	(0.0123)	(20,411.10)	(40,478.1)	(41,961.02)
Airport FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
N Airport	30	9	30	30	9	30
N Obs	750	225	750	750	225	750

Significance levels: *p<0.05, **p<0.01, ***p<0.001

Note. The table reports estimated treatment effects from the baseline DiD and PSM-DiD specifications (columns 1–2 and 4–5) using Equation 1, and from the Synthetic DiD specification of Arkhangelsky et al. (2021) (columns 3 and 6). The outcome variable is the logarithm of total arriving passengers in columns 1–3 and the number of total arriving passengers in columns 4–6, measured at the airport-month level. Standard errors clustered at the airport level are reported in parentheses.

To examine whether spatial displacement exists, we study the effect of NYC's regulation on restaurants located in New Jersey (specifically, in Hudson and Essex counties, which contain Jersey City and Newark, respectively).[21] We estimate a DiD specification analogous to Equation 1, comparing changes in restaurant spending in New Jersey before and after the implementation of Local Law 18 to matched (using PSM) restaurants in other metropolitan areas that are not plausibly affected by NYC's regulation. The identifying assumption is that, absent the policy, restaurant spending in NJ would have followed the same trends as those in the control group.

Table 6 reports these results. In contrast to the negative effect observed within NYC, we find a positive effect on restaurant spending in New Jersey following the regulation. The pooled 10.1% increase ($p < 0.001$) across both counties suggests that a non-trivial share of restaurant consumption is reallocated to nearby markets rather than eliminated entirely.

The results discussed in this section give us a clearer picture of how NYC's regulations affected the restaurant industry. We find evidence consistent with less discretionary spending, fewer travelers visiting NYC, and spatial displacement of some of NYC's restaurant demand to neighboring markets.

[21] Note that due to this type of spillovers, New Jersey restaurants were excluded from the main analysis.

Table 6: The Spillover Effect of the STR Regulation on Consumer Restaurant Spending in New Jersey

	(1)
Post × Treat	0.0960***
	(0.0275)
Covariates	✓
Restaurant FE	✓
Time FE	✓
Adjusted R^2	0.8773
N Restaurant	1,634
N Obs	40,850

Significance levels: $^+$p<0.1, *p<0.05, **p<0.01, ***p<0.001

Note. The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of offline consumer spending at the restaurant-month level. The coefficient of *Post* × *Treat* captures the effect for restaurants in both Hudson County (Jersey City) and Essex County (Newark). The model includes control variables for average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at CBSA level. Standard errors clustered at the restaurant level are reported in parentheses.

7 Robustness Checks

In this section, we present a set of sensitivity and robustness checks to reinforce the causal interpretation of our results.[22] We address six potential concerns: (1) sample representativeness, (2) sensitivity to the balanced panel requirement, (3) robustness to alternative time windows, (4) consistency across different matching methods and estimators, (5) STR policies affecting other cities, and (6) spurious correlations.

7.1 Restaurant Representativeness

To assess how well our estimation sample reflects the underlying composition of NYC's restaurant market, we benchmark it against the population of restaurants recorded in the NYC Department of Health and Mental Hygiene's inspection data.[23] Because every restau-

[22]For brevity, we focus on only consumer total spending.

[23]See: https://www.nyc.gov/site/doh/services/restaurant-grades.page

rant is inspected at least once per year, these inspection records provide a near-complete census of active establishments. We evaluate representativeness along two dimensions: the chain–independent composition of the sample, and the geographic distribution of restaurants across NYC neighborhoods. In both cases, we find that our estimation sample deviates modestly from the inspection universe and show that correcting for these imbalances leaves our main estimates largely unchanged.

7.1.1 Chain vs. Independent 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.[24] 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 3,220 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.[25] Within the inspection universe, 76.48% are independent and 23.52% are chains. In contrast, the 3,220 restaurants in our estimation sample consist of 54.88% independent and 45.12% chain restaurants. This means

[24] See https://docs.deweydata.io/docs/faqs-safegraph

[25] The prompts used are reported in Appendix A.

that in our data, chain restaurants are overrepresented.

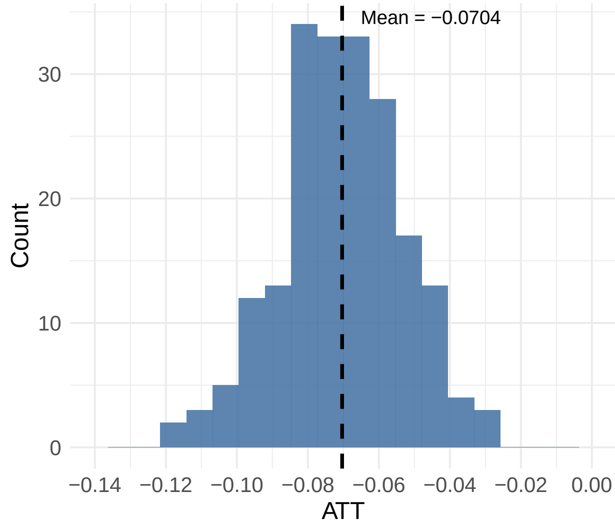
This imbalance aligns with the mechanism described above: in the pre-treatment period, chain restaurants in our data exhibit roughly 45% higher monthly transaction volumes than independent restaurants. Because higher volumes increase the likelihood that SafeGraph consistently observes sufficient transactions 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 re-estimate 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 –0.0704 with a 95% confidence interval of (-0.0727, -0.0680), closely aligned with our main estimate. Figure 2 shows a very narrow dispersion across iterations, ranging from –0.1159 to –0.0287. 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 estimation samples that better reflect NYC's restaurant population.

7.1.2 Spatial Reweighting

Beyond the chain–independent dimension, geographic composition is another channel through which our SafeGraph sample may deviate from the broader NYC restaurant universe. If certain neighborhoods are systematically over- or underrepresented in SafeGraph relative to their true share of the citywide restaurant population, our estimates could reflect the spending patterns of spatially unrepresentative restaurants rather than those of the average NYC

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



Note. The plot shows a distribution of treatment effects using Equation 1 and 200 different restaurant samples. Each sample randomly draws NYC restaurants so that the proportions of independent and chain establishments match the composition observed in the restaurant universe based on inspection data. The outcome variable is the logarithm of offline consumer spending at the restaurant-month level. 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.

establishment.

To assess and correct for this spatial selection, we construct inverse-probability weights that align the geographic distribution of our SafeGraph sample with that of the inspection universe, using the 2023 inspection records as the population benchmark. For each census tract s, we compute the share of inspected restaurants $\hat{P}_s^{\text{insp}} = n_s^{\text{insp}}/N^{\text{insp}}$ and the share of SafeGraph restaurants $\hat{P}_s^{\text{sg}} = n_s^{\text{sg}}/N^{\text{sg}}$, where n denotes counts within unit s and N denotes the respective totals. The raw spatial weight for a SafeGraph restaurant located in unit s is then

$$w_s = \frac{\hat{P}_s^{\text{insp}}}{\hat{P}_s^{\text{sg}}}.$$

Weights greater than one upweight restaurants in census tracts where SafeGraph undersamples relative to the inspection universe, while weights less than one downweight restaurants in oversampled census tracts. Restaurants located in census tracts with no corresponding inspected establishment are assigned a missing weight and excluded from the weighted estimation. We normalize the raw weights by their mean so that the weights have a unit mean across the NYC sample, and winsorize at the 99th percentile to limit the influence of extreme values.

We re-estimate our main specification (Equation 1) using these census-tract-level spatial weights. Given the 1-to-1 matching structure, each non-NYC control restaurant is assigned the same weight as its matched NYC treated restaurant, so that matched pairs contribute equally to the estimator. Table 7 reports the results. The weighted treatment effect is −0.0785 (column 1, without covariates) and −0.0673 (column 2, with covariates), both statistically significant at the 0.1% level and closely in line with our baseline estimates in Table 2. The stability of the coefficient across the unweighted and spatially weighted specifications indicates that geographic selection in the SafeGraph sample does not meaningfully distort our main finding.

Table 7: The Weighted Impact of the STR Regulation on Consumer Restaurant Spending

	(1)	(2)
Post × Treat	-0.0785^{***}	-0.0673^{***}
	(0.0237)	(0.0232)
Covariates		✓
Restaurant FE	✓	✓
Time FE	✓	✓
Weights	✓	✓
Adjusted R^2	0.8452	0.8453
N	159,800	159,800

Significance levels: *p<0.05, **p<0.01, ***p<0.001

Note. The table reports the spatial-weighted estimated treatment effect using Equation 1. The outcome variable is the logarithm of offline consumer spending at the restaurant-month level. Column 2 includes the control variables average consumer price index, population, temperature, temperature squared, precipitation and precipitation squared at CBSA level. Standard errors clustered at the restaurant level are reported in parentheses.

7.2 Allowing Unbalanced Panel Data

As discussed in Section 7.1, 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 generates missing values in the panel. In our main analysis, we restrict the estimation sample to a fully balanced panel, including only restaurants with complete monthly observations from July 2022 through July 2024. While this restriction enhances the consistency of DiD estimation, it may disproportionately exclude smaller restaurants, thereby affecting 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 at most $X \in \{1, 2, 3\}$ missing months during the analysis's pre-treatment period.

To include treated restaurants with at most $X \in \{1, 2, 3\}$ missing months in the 14 pre-treatment periods, along with 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, along with their matched controls. Once all treated restaurants and matched controls are included, we re-estimate the DiD specification for each X using all available observations for each restaurant.

Table 8 summarizes the results. The estimated treatment effects for $X = 1$, $X = 2$, and $X = 3$ are -0.0651, -0.0724, and -0.0744, respectively, which are slightly smaller than our baseline estimate from 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.

Table 8: Robustness Check: Allowing Unbalanced Panel Data

	(1) 1 Missing	(2) 2 Missing	(3) 3 Missing
Post × Treat	-0.0651^{**}	-0.0724^{***}	-0.0744^{***}
	(0.0200)	(0.0193)	(0.0187)
Covariates	✓	✓	✓
Restaurant FE	✓	✓	✓
Time FE	✓	✓	✓
Adjusted R^2	0.8345	0.8265	0.8241
N Restaurants	7,856	8,944	9,834
N Obs	192,825	215,649	232,908

Significance levels: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$

Note. The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of offline consumer spending at the restaurant-month level. 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 clustered at the restaurant level are reported in parentheses.

7.3 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 affect the quality of the match. Columns 1–3 of Table 9 report results using 12, 10, and 8 pre-treatment months, respectively, instead of the 14 in the main analysis. Across all specifications, the estimated treatment effects remain close to the baseline, indicating that our findings are not sensitive to the length of the pre-treatment period used for matching.

Next, we expand the sample to include additional restaurants. These restaurants did not have balanced observations over the full 14-month pre-treatment period, but do have balanced observations when we use the shorter pre-treatment windows (12, 10, or 8 months). We report these results in columns 4–6 of Table 9. The estimates remain stable, suggesting that our results are robust to both matching on fewer pre-treatment periods and to changes in restaurant composition 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 original restaurant sample and once allowing the expanded set of eligible restaurants to enter. In columns 1–3 of Table 10 we report the results using 9, 7, and 5-month post-treatment periods and keeping the original sample of restaurants. In columns 4–6 of Table 10, we report the results using 9-, 7-, and 5-month post-treatment periods and including all restaurants for which observations are balanced. Again, the results are consistent with our main analysis.

Table 9: Robustness Check: Shortening Pre-treatment Time Windows

	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.0491^{*}	-0.0695^{***}	-0.0568^{**}	-0.0647^{***}	-0.0667^{***}	-0.0457^{**}
	(0.0207)	(0.0195)	(0.0198)	(0.0196)	(0.0173)	(0.0170)
Covariates	✓	✓	✓	✓	✓	✓
Restaurant FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.8538	0.8639	0.8671	0.8479	0.8681	0.8777
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

Significance levels: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$

Note. The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of offline consumer spending at the restaurant-month level. 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 clustered at the restaurant level are reported in parentheses.

Table 10: Robustness Check: Shortening Post-treatment Time Windows

	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.0860^{***}	-0.0796^{***}	-0.0635^{**}	-0.0746^{***}	-0.0627^{***}	-0.0540^{**}
	(0.0204)	(0.0205)	(0.0205)	(0.0192)	(0.0186)	(0.0185)
Covariates	✓	✓	✓	✓	✓	✓
Restaurant FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.8479	0.8496	0.8522	0.8482	0.8517	0.8467
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

Significance levels: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$

Note. The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of offline consumer spending at the restaurant-month level. 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 clustered at the restaurant level are reported in parentheses.

7.4 Alternative Matching Method and Estimator

In this section, we show that the results are robust to different matching methods and estimators.

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 propensity 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 CEM using the same set of baseline characteristics as in the main analysis. Specifically, we coarsen restaurants into strata based on (i) quarterly offline spending (bin size = 12), (ii) the average number of offline transactions in the 14 months prior to treatment (bin size = 12), and (iii) the average proportion of 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. Units in strata without overlap, as well as treated or control units without an available match, are dropped. The resulting matched sample comprises 6,090 restaurants (3,045 treated and 3,045 controls), retaining 94.57% of restaurants relative to the PSM-matched sample. We then re-estimate Equation 1 on this CEM-matched sample. We report these results in column 1 of Table 11. The estimated treatment effect of -0.1041 closely matches our baseline result in magnitude, indicating that our findings are robust to using CEM.

Synthetic Difference-in-Differences (SynthDiD). As a second robustness exercise, we estimate the treatment effect using the SynthDID estimator (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, we refer the reader to Arkhangelsky et al. (2021). We report these results in column 2 of Table 11. The estimated treatment effect of -0.0734 is nearly identical to the baseline DiD estimate.

Table 11: Robustness Check: Alternative Matching and Estimation Methods

	(1) CEM	(2) SynthDiD
Post × Treat	-0.1041^{***}	-0.0734^{***}
	(0.0183)	(0.0152)
N Restaurants	6,090	60,755
N Obs	152,250	1,518,875

Significance levels: *p<0.05, **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 methods are described in Section 7.4. The outcome variable is the logarithm of offline consumer spending at the restaurant-month level. Standard errors clustered at the restaurant level are reported in parentheses.

7.5 Excluding Control CBSAs with Regulated Cities

Given that STR regulations are today pervasive, a potential concern with our analyses is that some of the control CBSAs' cities implemented them during our observation period. If this is the case, these regulations could introduce bias into our estimates. For example, if some cities introduced STR regulations at the same time as NYC, our estimates could be downward-biased if these regulations have effects similar to those identified in NYC. To address this concern, we perform a robustness check in which we exclude CBSAs that contain cities that introduced STR regulations during our observation period. Specifically, we identify and exclude four CBSAs that implemented STR regulations during our observation period: Philadelphia-Camden-Wilmington (PA-NJ-DE-MD), St. Louis (MO-IL), Atlanta-Sandy Springs-Roswell (GA), and Urban Hawaii. Philadelphia introduced comprehensive STR regulations that became effective January 1, 2023, requiring all hosts to obtain permits regardless of rental frequency and began aggressive enforcement in July 2023 by delisting approximately 1,500-1,700 unlicensed properties from booking platforms (City of Philadelphia, 2023). St. Louis enacted Ordinance 71729 in November 2023, which took effect on November 6, 2024, establishing the city's first comprehensive STR permit system, including requirements for designated agents and limits on the number of rental units per owner (City of St. Louis, 2024). Atlanta implemented its STR ordinance in March 2022, with enforcement beginning in March 2023, restricting hosts to a maximum of two properties with one required to be a primary residence (City of Atlanta, 2023). Finally, Hawaii passed Senate Bill 2919 in May 2024, granting counties the authority to regulate, and potentially phase out, STRs in residential zones, with Maui County proposing to eliminate over 7,000 vacation rental units (Office of the Governor of Hawaii, 2024).

In doing so, we exclude 298 restaurants in these four CBSAs that were originally matched to treated NYC restaurants in our main analysis. Specifically, among the 3,220 treated restaurants, 90 were matched with controls from the Philadelphia CBSA, 123 from the Atlanta CBSA, 45 from the St. Louis CBSA, and 40 from the Hawaii CBSA. We then reconstruct our estimation sample by re-matching the entire set of treated NYC restaurants to controls drawn exclusively from the remaining 14 CBSAs without STR regulations during our observation period. We then re-estimate Equation 1, and report these results in Table 12. The estimated treatment effects are nearly identical to our baseline results in Table 2, confirming that our findings are not driven by the inclusion of control restaurants in markets that experienced regulatory changes during our observation period.

Table 12: The Impact of the STR Regulation on Consumer Restaurant Spending: Excluding Control CBSAs with STR Regulations

	(1)	(2)
Post × Treat	−0.0814***	−0.0690**
	(0.0215)	(0.0211)
Covariates		✓
Restaurant FE	✓	✓
Time FE	✓	✓
Adjusted R^2	0.8448	0.8450
N	161,000	161,000

Significance levels: *p<0.05, **p<0.01, ***p<0.001

Note. The table reports the estimated treatment effect using Equation 1, excluding CBSAs that experienced much less strict short-term rental regulations during the sample period (Philadelphia-Camden-Wilmington, PA-NJ-DE-MD; Atlanta-Sandy Springs-Roswell, GA; St. Louis, MO-IL; and Urban Hawaii). The outcome variable is the logarithm of offline consumer spending at the restaurant-month level. Column 2 includes the control variables average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at the CBSA level. Standard errors clustered at the restaurant level are reported in parentheses.

7.6 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 13 reports the results, which show no meaningful effect ($\hat{\beta} = -0.007$, $p = 0.730$).

Placebo Treated Units. Second, we assign treatment randomly by selecting restaurants from the 18 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 around 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 0.0006 and a 95% confidence interval of (-0.0016, 0.0028)). These tests suggest that the main findings are not driven by pre-existing trends or random variation.

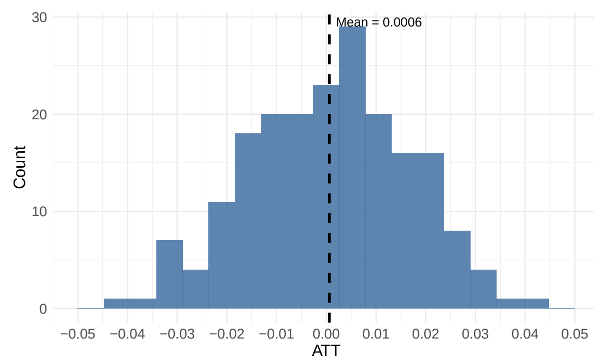
Table 13: Placebo Treatment Timing

	(1)
Post × Treat	−0.0073
	(0.0213)
Covariates	✓
Restaurant FE	✓
Time FE	✓
Adjusted R^2	0.8530
N Restaurants	6,440
N Obs	122,360

Significance levels: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$

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 offline consumer spending at the restaurant-month level. The model specifications include the control variables average consumer price index, population, temperature, temperature squared, precipitation and precipitation squared at CBSA level. Standard errors clustered at the restaurant level are reported in parentheses.

Figure 3: Placebo Treated Units



Note. The plot shows a distribution of treatment effects using Equation 1 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 offline consumer spending at the restaurant-month level. 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.

8 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 of roughly 7.5% in monthly consumer spending at restaurants. This decline translates into an estimated annual revenue loss of approximately 1.85 billion dollars for the city's restaurant industry. Decomposing this overall effect, we find that the regulation led to a 5.0% reduction in transaction volume and a 2.3% decrease in average spending per transaction, indicating that the impact operates through both fewer customers visiting restaurants and lower spending among those who continue to visit, as well as among locals.

The effects are not evenly distributed across establishments. Restaurants catering to tourists and those at higher price points experience the largest declines in total spending and transaction volume. These declines are driven primarily by fewer customer visits rather than by lower spending per visit, which falls modestly and uniformly across all restaurant types. This pattern points to reduced tourist visits as the primary mechanism. Independent evidence from airport passenger data confirms a reduction in tourist inflows, with NYC airports experiencing a 3.5% to 4.3% decline in passenger volumes compared with other major US airports. Finally, we document modest spatial spillovers to neighboring markets where restaurant offline spending increased by approximately 10.1%. Together, these patterns indicate that the decline in restaurant demand is driven primarily by lower visitor volumes, with a secondary contribution from constrained discretionary budgets among both visitors who continue to travel despite higher accommodation costs and locals whose income is affected by the contraction of the STR market.

Taken together, the findings show that STR regulations designed to increase housing affordability can generate substantial unintended consequences 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 impact business performance, employment, and tax revenues. The spatial spillovers we document further suggest that city-level regulations may shift economic activity to neighboring jurisdictions rather than preserving it locally, underscoring the importance of accounting for cross-border effects in policy design.

The policy implications are therefore twofold. First, cities considering restrictive STR policies should weigh the housing benefits against potential losses to tourism-dependent industries. A complete welfare analysis must account not only for improvements in housing affordability and neighborhood stability, but also for reduced revenues in complementary sectors, lower tax receipts, and possible employment effects. Second, complementary policies may be needed to mitigate negative spillovers, such as expanding the supply of legal, affordable lodging options or providing targeted support to the restaurant and hospitality sectors during regulatory transitions. More broadly, our study underscores the importance of evaluating urban housing policies through a broader economic lens, recognizing that interventions in one market inevitably shape outcomes in other markets. Effective urban policy requires understanding not only the direct effects of regulation on targeted markets, but also the indirect effects that propagate through the interconnected urban economy.

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The authors declare no competing interests. This research received no specific grant from any funding agency in the public, commercial, or not-for-profit sectors. The credit and debit card transaction data used in this study were obtained from SafeGraph via Dewey Data under an academic license. Airport passenger data are publicly available from the Bureau of Transportation Statistics. Socioeconomic data are publicly available from the U.S. Census Bureau and the Bureau of Labor Statistics. Weather data are publicly available from NOAA's National Centers for Environmental Information.

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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 Putting the Estimates into 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 7.5% in restaurant spending following the implementation of the regulation (using the exact transformation $\exp(\hat{\beta}) - 1$ with $\hat{\beta} = -0.078$ from our preferred specification). 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 customer base than those captured in this sample, we scale the observed effect to the city level to approximate its overall impact.[26] 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.[27] Additionally, offline revenue accounts for around 85% of NYC restaurant sales, which is $\$26.904 \times 0.85 = \$22.8684$ billion.[28] Assuming that the 7.5% decline in restaurant spending applies to the entire industry, the counterfactual level of offline 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 offline sales (\$22.8684 billion) and d represents the estimated proportional decline (0.075). Substituting these values yields:

$$\frac{\$22.8684}{1-0.075} = \$24.722,$$

which implies that, without the regulation, restaurant offline sales would have reached approximately \$24.722 billion. The estimated loss in restaurant revenue is therefore:

$$\$22.8684 - \$24.722 = -\$1.85 \text{ billion}$$

This calculation suggests an aggregate annual loss of roughly \$1.85 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.[29]

Beyond direct losses in taxable restaurant sales, this contraction likely generates sec-

[26] In 2019, NYC hosted approximately 23,650 restaurant establishments (see, 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 (see, 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.

[27] See: https://comptroller.nyc.gov/wp-content/uploads/documents/The-State-of-the-Citys-Economy-and-Finances-2024.pdf

[28] App deliveries accounted for \$3.6 billion of the \$24.7 billion in NYC restaurant sales (15%). (See https://www.nyc.gov/assets/dca/downloads/pdf/workers/Delivery-Worker-Study-November-2022.pdf.) We also observe a similar share of offline sales of 86.23% in our data.

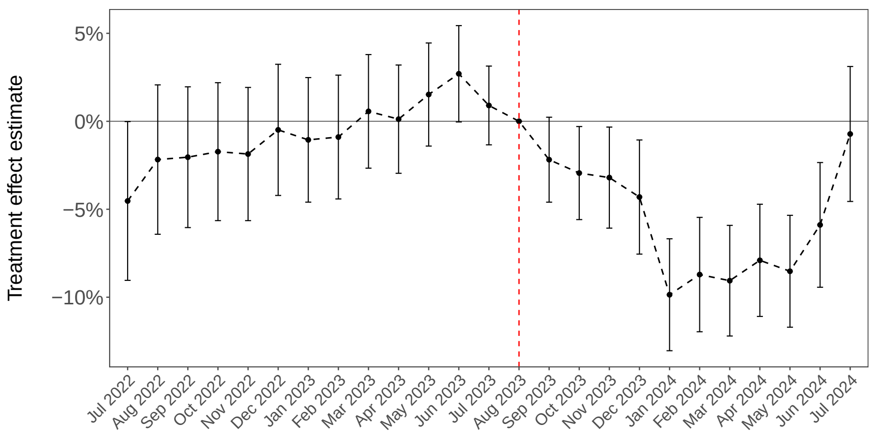
[29] See: https://www.safegraph.com/blog/validating-spend-data-for-brands-against-company-reporting

ondary economic effects, including reduced city tax revenues and potential employment losses if affected restaurants scale back operations or close entirely.

These results establish a substantial and economically significant decline in restaurant spending following NYC's STR regulation. The magnitude of the effect—approximately 7.5% reduction in spending, translating to roughly \$1.85 billion in annual losses—underscores the importance of understanding how this impact materializes across different types of restaurants and customer segments.

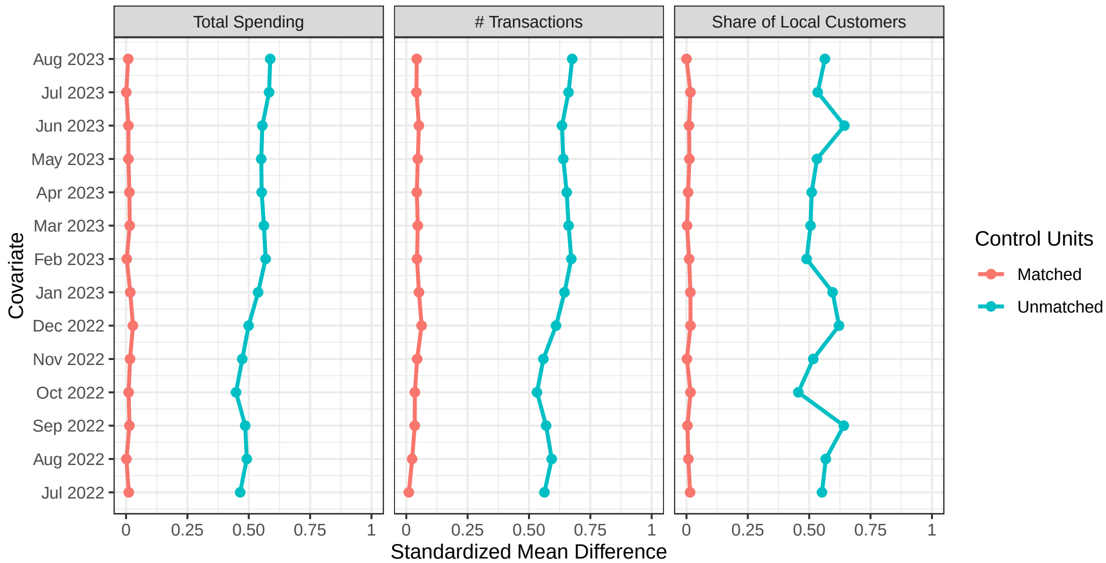
C Additional Figures and Tables

Figure 4: The Impact of the STR Regulation on Consumer Restaurant Transactions 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 the total number of transactions at the restaurant-month level. 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. The vertical dashed red line at August 2023 represents the month preceding the implementation of the regulation.

Figure 5: PSM Covariate Balance



Note. The figure reports standardized mean differences (SMDs) between treated and control restaurants before and after matching for three sets of covariates: (a) Offline Spending, (b) Number of Offline Transactions, and (c) Share of Local Customers. The average SMDs before matching are 0.526 for offline spending, 0.619 for the number of offline transactions, and 0.552 for the share of local customers. After matching, SMDs reduce to 0.011, 0.041, and 0.010, respectively, suggesting satisfactory covariate balance.

Table 14: The Impact of the STR Regulation on Consumer Restaurant Demand: Clustering Standard Errors at the CBSA Level

	(1) Total Spending	(2) # Transactions	(3) Average Spending
Post × Treat	−0.0780**	−0.0508*	−0.0229*
	(0.0234)	(0.0208)	(0.0102)
Covariates	✓	✓	✓
Restaurant FE	✓	✓	✓
Time FE	✓	✓	✓
Adjusted R^2	0.8452	0.8675	0.8173
N	161,000	161,000	161,000

Significance levels: *p<0.05, **p<0.01, ***p<0.001

Note. The table reports the estimated treatment effect using Equation 1. The outcome variable is the logarithm of offline consumer spending for Column 1, the logarithm of the number of offline transactions for Column 2, and the logarithm of the average offline spending per transaction for Column 3, at the restaurant-month level. The models include control variables for average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at the CBSA level. Standard errors clustered at the CBSA level are reported in parentheses.

Table 15: The Impact of the STR Regulation on Restaurant Offline Consumption with One to Many Matching

	(1) Five Control Units			(2) Seven Control Units		
	Total Spending (1)	# Transactions (2)	Avg Spending (3)	Total Spending (4)	# Transactions (5)	Avg Spending (6)
Post × Treat	−0.0634***	−0.0429***	−0.0171*	−0.0680***	−0.0447***	−0.0198*
	(0.0165)	(0.0099)	(0.0084)	(0.0160)	(0.0096)	(0.0082)
Covariates	✓	✓	✓	✓	✓	✓
Restaurant FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.8568	0.8770	0.8121	0.8578	0.8769	0.8150
Observations	483,000	483,000	483,000	644,000	644,000	644,000

Significance levels: *p<0.05, **p<0.01, ***p<0.001

Note. The table reports the estimated treatment effect using Equation 1 with five and seven control units. The outcome variable is the logarithm of offline spending (columns 1 and 4), the logarithm of the number of offline transactions (columns 2 and 5), and the logarithm of average spending per transaction (columns 3 and 6) at the restaurant-month level. All models include control variables for average consumer price index, population, temperature, temperature squared, precipitation, and precipitation squared at the CBSA level. Standard errors clustered at the restaurant level are reported in parentheses.