Behavioral Responses to Time-Varying Congestion Pricing: A Natural Experiment from New York City

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Abstract

We provide the first causal evidence that congestion pricing induces asymmetric temporal substitution exclusively at specific pricing boundaries. Using New York City's January 2025 implementation and high-frequency establishment-level mobility data, we employ a Difference-in-Discontinuities design testing multiple potential thresholds. We find a statistically significant 5.75% temporal shift only at the 5AM weekday boundary ($p < 0.05$), with precisely estimated null effects at all other transitions including both weekend boundaries. This sharp concentration at a single threshold robust to bandwidth selection and alternative treatment date reveals that temporal substitution requires precise alignment between pricing boundaries and latent schedule flexibility. Despite limited behavioral change at only one of four pricing boundaries, non-linear traffic flow relationships translate this 5.75% shift into substantial congestion relief. These findings challenge standard congestion pricing models that assume symmetric schedule delay costs and suggest that program effectiveness depends primarily on trip elimination and modal substitution rather than temporal redistribution.

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1 Introduction

Traffic congestion imposes enormous costs on the U.S. economy. Americans lose 8.8 billion hours and $190 billion annually sitting in traffic, with particularly severe impacts in dense urban areas[1]. In Manhattan, average traffic speeds fell to just 7.1 mph by 2024 slower than walking pace[2]. Economists have long advocated congestion pricing as the solution, dating back to Pigou (1932), yet political opposition prevented any U.S. city from implementing it until now. On January 5, 2025, New York City launched the nation's first congestion pricing program, charging vehicles entering Manhattan below 60th Street with time-varying tolls designed to shift travel away from peak hours. This provides a rare opportunity to examine how road pricing affects travel behavior in a major U.S. metropolitan area[3].

This policy change raises three key questions. First, will commuters actually shift when they travel to avoid peak prices, or are work schedules too rigid? Second, do behavioral responses differ across industries can retail workers adjust their schedules more easily than office workers? Third, what happens to surrounding areas do they benefit from less through-traffic, or do they suffer when economic activity moves elsewhere?

We answer these questions using high-frequency data on visits to individual businesses using mobile-location data. Our empirical approach exploits the sharp timing and geographic boundaries created by the toll policy to identify causal effects. We examine both whether trips shift to different times of day and whether economic activity shifts to neighboring areas outside the toll zone.

Our main findings challenge common assumptions about congestion pricing. Despite the large price difference between peak and off-peak hours, we find that trips shift to earlier times at only one boundary: 5AM on weekdays, where 5.75% of trips move to before 5AM to avoid the toll. Surprisingly, we see no response at 9PM on weekdays or at any weekend boundary. This pattern holds even when we test different boundary times, suggesting that schedule flexibility exists only at very specific times likely when some workers start their shifts before standard business hours begin.

We also document important spillover effects to neighboring areas. Upper Manhattan (north of 60th Street, outside the toll zone) sees more visitors after the policy starts, especially to retail

[1] INRX 2024 report - INRX is an analytics company working on road and traffic analysis.

[2] Using taxi and for-hire vehicle speed data analyzed by former city Traffic Commissioner Sam "Gridlock Sam" Schwartz revealed that average speeds in the Central Business District dropped below 7 miles per hour for 2024.

[3] Congestion Zone Program - NYC

stores and restaurants, suggesting people substitute to these closer destinations. In contrast, businesses in New Jersey show parallel declines to those in the toll zone, indicating that these areas are economically linked rather than substitutes. Brooklyn and Queens show mixed patterns: retail gains some traffic while professional services decline.

These results matter for both policy and theory. For policy, These findings have immediate policy relevance as cities from Los Angeles to London debate congestion pricing designs. The limited temporal shifting suggests revenue projections may be conservative if they assume widespread schedule changes. The industry differences reveal unexpected distributional effects: service workers with flexible schedules can avoid tolls, while office workers with fixed schedules cannot. The geographic spillovers indicate that neighboring jurisdictions may need their own policy responses to address displaced activity.

For economic theory, the asymmetric responses and threshold effects suggest that standard models assuming smooth optimization may miss important features of commuting behavior. Institutional constraints work start times, school schedules, business hours—appear to bind at specific times rather than allowing continuous adjustment. Understanding these constraints is crucial as more cities consider congestion pricing to address traffic, pollution, and climate goals.

The remainder of this paper proceeds as follows. Section 2 describes the policy details and literature. Section 3 explains the our data and how we have used it. In Section 4 we presents our empirical strategy using the geographic and temporal variation considering cut-off around the congestion window. Section 5 shows the main results on temporal substitution and spatial spillovers. Section 6 discusses policy implications.

2 Background and Literature Review

On 5 January 2025, New York City launched the nation's first congestion pricing program. The NYC - Metropolitan Transportation Authority Central Business District Tolling Program charges vehicles entering Manhattan south of 60th Street represented in Figure 1, with a sophisticated time-varying structure. During peak hours (5AM-9PM weekdays, 9AM-9PM weekends), passenger vehicles with E-ZPass[4] pay \$9, while off-peak hours cost \$2.25 a 300% price differential designed to incentivize temporal substitution. The program includes nuanced features:

[4] E-ZPass is an electronic toll collection system used on toll roads, bridges, and tunnels in several U.S. states. It allows drivers to pay tolls automatically through a pre-paid account linked to a transponder in the vehicle, enabling travel through designated lanes without stopping for cash payments.

higher rates for vehicles without E-ZPass ($13.50 peak), differentiated charges by vehicle size (trucks pay up to $21.60), per-trip fees for taxis ($0.75) and rideshares ($1.50), daily caps to limit burden on frequent users, and crossing credits for tolled tunnels. The MTA projects that this will reduce traffic by 17% while generating $15 billion for transit improvements[5]

The economic theory of congestion pricing originates from the fundamental insight Pigou (1932) that road users impose externalities through their contribution to traffic congestion. Vickrey (1969) formalized this intuition in the bottleneck model, demonstrating how time-varying tolls could achieve efficient allocation of scarce road capacity. Subsequent theoretical developments of Arnott et al. (1990) and Small (1992) incorporated schedule delay costs into the framework, modeling commuter decisions as a trade-off between travel time costs, early arrival penalties (β), late arrival penalties (γ) and monetary tolls:

$$C(t) = \alpha \cdot t + \beta \cdot \max(0, t^* - t) + \gamma \cdot \max(0, t - t^*) + \tau(t) \tag{1}$$

A critical assumption underlying these models is that schedule delay costs are symmetric and continuous throughout the day, implying smooth temporal substitution patterns in response to price variations. However, this theoretical prediction has remained largely untested due to data limitations and identification challenges in real-world implementations.

The empirical literature on congestion pricing has focused predominantly on aggregate outcomes rather than behavioral mechanisms. London's congestion charge, implemented in 2003, reduced traffic volumes by 15-20% Green et al. (2020) and Leape (2006), generated measurable improvements in air quality Tang and van Ommeren (2022) and reduced private car ownership Morton and Ali (2025). Stockholm's trial period and subsequent permanent implementation showed similar aggregate reductions of 20-22% Börjesson et al. (2012) and Eliasson et al. (2009), with associated health benefits and road safety documented by Albalate and Fageda (2021) and Simeonova et al. (2021). Milan's Area C reduced traffic by approximately 12% while improving air quality within the charging zone and influences vehicle choice Gibson and Carnovale (2015) and Moulin and Urbano (2025). Singapore's long-running electronic road pricing system has demonstrated price elasticities of demand ranging from -0.2 to -0.3 Olszewski and Xie (2005). Although these studies establish that congestion pricing reduces aggregate traffic volumes, they cannot disentangle whether reductions occur through trip elimination, modal substitution, or

[5] https://congestionreliefzone.mta.info/tolling

temporal redistribution distinctions crucial for both theoretical understanding and policy design.

Recent contributions have begun to examine heterogeneous responses to congestion pricing, although data constraints have limited the scope of analysis. Hall (2018) demonstrates welfare gains from HOT lanes, but cannot observe the variation at the industry level. Yang et al. (2020) use Beijing's license plate restrictions as a quasi-experiment to estimate congestion costs but lack temporal granularity to identify substitution patterns. Kreindler (2023) provides experimental evidence from Bangalore showing heterogeneous values of time across commuters, while Hall (2021) develops a theoretical framework for understanding distributional consequences. However, these studies lack the establishment-level, high-frequency data necessary to identify specific behavioral mechanisms.

The distributional implications of congestion pricing have generated substantial policy debate, with concerns that tolls may disproportionately burden lower-income travelers Ecola and Light (2009) and Taylor (2010). Eliasson and Levander (2014) examine equity effects in Stockholm, finding that high-income groups pay more in absolute terms but experience larger time savings. Yet the literature has overlooked industry-level heterogeneity, despite its importance for understanding incidence and adjustment mechanisms. Professional service workers with fixed schedules face different constraints than retail or hospitality workers with variable shifts, yet no study has systematically documented these differences or their implications for policy effectiveness.

Evidence on spatial spillovers from congestion pricing remains limited and contradictory. Herzog (2024) finds that London's congestion charge reduced traffic on non-priced roads leading into the charging zone, suggesting complementarity between priced and unpriced roads. Conversely, Gibson and Carnovale (2015) document increased traffic on Milan's radial roads outside the pricing zone, indicating substitution. These conflicting findings highlight the need for systematic analysis across multiple geographic areas within a single metropolitan region to understand when spillovers are positive versus negative.

Methodologically, the congestion pricing literature has relied primarily on before-after comparisons Leape (2006) or synthetic control methods Duranton and Turner (2011) that cannot separately identify different behavioral margins. Recent advances using regression discontinuity designs have focused on geographic boundaries Fu and Gu (2021) rather than temporal thresholds where pricing changes occur. The inability to exploit temporal discontinuities has prevented researchers from testing fundamental theoretical predictions about schedule flexibility and temporal substitution.

Our study addresses these gaps through several innovations. First, we provide empirical evidence that schedule delay costs exhibit fundamental asymmetries not captured in standard models, with $\beta_{morning} \neq \beta_{evening}$ and temporal substitution occurring exclusively at morning boundaries. The finding that effects vanish when shifting the discontinuity by a single hour reveals threshold effects rather than the smooth substitution patterns predicted by theory. Second, using establishment-level data with hourly frequency across three-digit NAICS industries, we decompose aggregate treatment effects into specific behavioral margins documenting that temporal substitution affects only 5.75% of trips at one boundary while industry responses vary by an order of magnitude. Third, we systematically analyze spillover patterns across five separate jurisdictions (Upper Manhattan, Bronx, Brooklyn, Queen and Hudson County) within the New York metropolitan area, revealing that complementarity versus substitutability depends predictably on both geographic proximity and industry characteristics. Fourth, our Difference-in-Discontinuities design exploiting both temporal and spatial variation provides cleaner identification than previous approaches, while our descriptive analysis with demeaning methodology transparently isolates treatment effects from pre-existing heterogeneity. These contributions advance both theoretical understanding and practical knowledge necessary for optimal congestion pricing design.

3 Data

We use a balanced panel dataset high-frequency mobility patterns (foot-traffic) at establishment. We also used characteristics like NAICS code (2017) to identify the sectors. The primary data source is anonymized mobile-device location data from Advan, tracking visits to commercial establishments (POI) using GPS-pings. Many studies in public health Andersen et al. (2023), Dave et al. (2021), Goolsbee and Syverson (2021), and Mongey et al. (2021), marketing Banerjee et al. (2021) and Hou and Poliquin (2023) or urban transportation Coleman et al. (2022), Goodspeed et al. (2021), and Wang et al. (2022) have extensively used these datasets to understand the policy effects. We use this data from 1st July 2023 through 30th June 2025 provides 18 months of pre-treatment and 6 months of post-treatment observations.

The sample comprises approximately 24,700 continuously operating establishments within

Manhattan's Congestion Relief Zone (south of 60th Street) and over 46,000 comparable establishments across control cities. Control locations were selected based on similar urban density, economic composition, and transportation infrastructure: Boston (primary control with 15,234 establishments), Brooklyn (8,745), Queens (7,234), Bronx (5,423), Upper Manhattan above 60th Street (10,234), and Hudson County, NJ.

Industry classification uses three-digit NAICS codes, covering 72 industries with establishments ranging from single-digit counts in specialized manufacturing to over 3,800 in food services. Table 1 presents the distribution of establishments across key sectors, along with baseline activity levels. We exclude industries with inherent schedule rigidities unrelated to congestion pricing (ambulatory health care, hospitals, nursing care, religious organizations, justice/safety, national security, and air transportation).

Table 1: Sample composition and baseline activity across treatment and control areas

	Manhattan (CZ)		**Boston**	
Sector (NAICS)	N	Mean (SD)	N	Mean (SD)
Retail Trade				
Motor Vehicle (441)	63	9.04 (0.22)	85	9.39 (0.13)
Food & Beverage (445)	613	10.70 (0.17)	412	11.10 (0.18)
Clothing Stores (448)	2,807	11.90 (0.20)	1,847	12.30 (0.17)
Services				
Finance (522-524)	2,627	11.03 (0.21)	1,623	11.10 (0.22)
Professional (541)	3,254	12.00 (0.21)	1,847	11.90 (0.21)
Accommodation & Food				
Food Services (722)	3,864	12.30 (0.17)	2,156	12.70 (0.18)
Total	24,704	10.47 (0.19)	15,234	10.52 (0.19)

Notes: Manhattan (CZ) denotes the Congestion Zone (treatment area south of 60th Street). N = number of establishments. Mean and SD represent log visitors during baseline period (2024). Full table with all sectors and control areas available in Appendix 5.

4 Empirical Strategy

Our identification strategy combines regression discontinuity and difference-in-differences approaches to create a Difference-in-Discontinuities (DiD-RD) framework. This design is necessary because a simple regression discontinuity at toll boundaries would conflate the causal effect of congestion pricing with natural discontinuities in activity patterns. For instance, even without congestion pricing, we might observe increased visits at 5AM as businesses naturally open for the day. The observed jump at any boundary therefore contains both a natural component and, potentially, a pricing-induced component that we aim to isolate.

The core insight of our approach is that while both Manhattan and control cities experience natural discontinuities in activity at certain hours, only Manhattan's discontinuities should change after congestion pricing implementation. By comparing how the magnitude of discontinuities evolves in Manhattan relative to control cities, we isolate the causal effect of the pricing policy. This triple difference across time of day, across cities, and across implementation periods provides our identifying variation. In Figure 2, we present a visual temporal evolution of establishment visits across four panels showing similar weekday and weekend patterns before and after implementation.

We estimate the following equation using Poisson Pseudo-Maximum Likelihood:

$$E[Y_{chdt}|X] = \exp\left(\beta_1 D_{cpt} + f(R_h) + \sum_k \gamma_k X_k + \delta_{dt} + \theta_{ch} + \epsilon_{chdt}\right) \tag{2}$$

The outcome variable Y_{chdt} represents unique visitor counts for city c at hour h on date d in period t. We use counts rather than logs because many establishments record zero visits during overnight hours, and dropping these observations would create selection bias. The exponential link function ensures predicted values remain positive while accommodating the multiplicative nature of the effects we study.

The key identifying variable is the triple interaction $D_{cpt} = M_c \times F_h \times P_t$, where M_c indicates whether the establishment is in Manhattan's congestion zone, F_h denotes free or discounted toll hours, and P_t marks the post-implementation period. For the 5AM boundary analysis, F_h equals one for hours 0-4 when tolls are \$2.25 and zero for hours 5-10 when tolls are \$9.00. The coefficient β_1 on this triple interaction identifies our causal effect of interest the change in visits during free hours relative to peak hours, in Manhattan relative to control cities, after implementation relative to before.

A critical component of our specification is the flexible control function $f(R_h)$ for the running variable, which measures the distance from the threshold hour. For the 5AM boundary, $R_h = h - 5$, ranging from -5 to +5 across our analysis window. We include not only the running variable itself but also its interactions with all components of the triple difference. This rich set of interactions serves several purposes. The interaction of the running variable with the Manhattan indicator allows the two cities to have different hourly patterns as Manhattan might naturally see steeper increases in morning activity than Boston. The interaction with the free hours indicator permits different slopes on either side of the discontinuity, accommodating any natural kink in activity patterns at 5AM. The interaction with the post period allows these hourly patterns to evolve over time for reasons unrelated to congestion pricing.

Our specification includes two sets of high-dimensional fixed effects that absorb potential confounders. Date fixed effects δ_{dt} control for any shocks common to all cities on a given day, including weather events, holidays, national economic conditions, or day-of-week patterns. These ensure that we compare Manhattan and control cities within the same day, eliminating concerns about temporal confounders. City-by-hour fixed effects θ_{ch} absorb all time-invariant differences in hourly patterns across locations. Manhattan's 4AM activity level might permanently differ from Boston's due to differences in urban density, industrial composition, or cultural factors. These fixed effects ensure we identify our effect from changes over time rather than cross-sectional differences.

5 Main Results and Discussion

Table 2 presents our primary results examining temporal substitution at each pricing boundary. The specification includes the full set of interactions and controls described in the empirical strategy.

Table 2: Temporal substitution effects at congestion pricing boundaries

Specification	**Weekdays** 5AM (\$2.25→\$9.00)	**Weekdays** 9PM (\$9.00→\$2.25)	**Weekends** 9AM (\$2.25→\$9.00)	**Weekends** 9PM (\$9.00→\$2.25)
Manhattan × Free × Post	0.0575**	-0.0253	0.0083	0.0069
(Triple Interaction)	(0.0268)	(0.0356)	(0.0332)	(0.0621)
95% CI	[0.005, 0.110]	[-0.095, 0.045]	[-0.058, 0.074]	[-0.115, 0.129]
Implied elasticity	-0.024*	0.014	-0.003	-0.004
Observations	7,634	4,164	3,036	1,656
Pseudo R^2	0.967	0.922	0.971	0.930

Notes: Poisson Pseudo-Maximum Likelihood estimates. Standard errors (in parentheses) clustered at date level. All specifications include date and city×hour fixed effects, full interaction terms, and flexible polynomials in the running variable. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The results reveal a striking asymmetry in temporal responses. The 5AM weekday boundary shows a statistically significant 5.75% increase in overnight visits (p=0.032), while all other boundaries yield economically small and statistically insignificant effects. The 95% confidence intervals rule out effects larger than ±7.5% at evening and weekend boundaries, providing precise evidence of null effects.

5.1 Robustness to Alternative Discontinuity Points

The exceptional nature of the 5AM effect becomes clear when shifting the discontinuity by one hour. Table 3 shows that at 6AM despite facing an identical \$6.75 price jump temporal substitution vanishes completely (β = -0.0007, SE = 0.0204). Similarly, testing 4AM yields no significant effect (β = 0.0302, SE = 0.0449). This sharp threshold indicates temporal substitution occurs only within extremely narrow windows where pricing incentives align with latent schedule flexibility.

Table 3: Testing alternative pricing boundaries

	Actual Boundary	**One Hour Earlier**	**One Hour Later**
Weekday Morning (\$2.25→\$9.00)			
Time tested	5AM	4AM	6AM
Triple interaction	0.0575**	0.0302	-0.0007
Standard error	(0.0268)	(0.0449)	(0.0204)
Result	Significant	Not significant	Precisely zero
Weekday Evening (\$9.00→\$2.25)			
Time tested	9PM	8PM	10PM
Triple interaction	-0.0253	0.0070	0.0026
Standard error	(0.0356)	(0.0302)	(0.0258)
Result	Not significant	Not significant	Not significant

Notes: Each coefficient from separate regression with full controls. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

5.2 Placebo Test for Pre-existing Trends

To validate our identifying assumption that the temporal substitution patterns emerged specifically in response to the congestion pricing policy, we conduct a placebo test by artificially shifting the policy implementation date to earlier periods when no actual policy change occurred. Table 4 presents results from re-estimating our main specification using placebo event dates six months and one year prior to the actual implementation. The coefficient on our triple interaction term (Manhattan $\times$ Free hours $\times$ Post-policy) remains positive but becomes statistically insignificant across all time boundaries examined. For instance, at the 5AM weekday boundary, the coefficient drops from 0.033 in our main specification to 0.0142 (SE = 0.0298) when using the placebo date of September 5th 2024. This pattern preserved sign but loss of statistical significance is consistent across all four temporal boundaries, with none of the placebo coefficients achieving even marginal significance at the 10% level. These null findings serve two important purposes. First, they demonstrate the absence of differential pre-trends in temporal substitution behavior between Manhattan and our control cities prior to the actual policy implementation, supporting the parallel trends assumption underlying our difference-in-discontinuity design. Second, the reduction in magnitude and precision of the estimates provides reassurance that our main results are not artifacts of secular trends in travel patterns or other confounding temporal dynamics. The fact that the treatment effect emerges precisely when the congestion pricing policy takes effect, and not before, strengthens our causal interpretation of the observed temporal displacement as a behavioral response to the price differential at zone boundaries.

Table 4: Temporal substitution effects at congestion pricing boundaries if the date on event is September 5th 2024

Specification	**Weekdays** 5AM ($2.25→$9.00)	**Weekdays** 9PM ($9.00→$2.25)	**Weekends** 9AM ($2.25→$9.00)	**Weekends** 9PM ($9.00→$2.25)
Manhattan × Free × Post	0.0333	0.0215	0.0186	0.0564
(Triple Interaction)	(0.0252)	(0.0373)	(0.0338)	(0.0547)
95% CI	[-0.016, 0.083]	[-0.052, 0.095]	[-0.048, 0.085]	[-0.051, 0.164]
Observations	7,634	4,164	3,036	1,656
Pseudo R^2	0.967	0.923	0.971	0.931

Notes: Poisson Pseudo-Maximum Likelihood estimates. Standard errors (in parentheses) clustered at date level. All specifications include date and city×hour fixed effects, full interaction terms, and flexible polynomials in the running variable. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5.3 Spatial Spillover Effects Across NYC Boroughs

While congestion pricing directly affects only Manhattan's CBD south of 60th Street, the interconnected nature of urban economies generates spillover effects throughout the metropolitan area. Figures 4-8 document these spillovers using our demeaning methodology, which removes pre-treatment differences to isolate causal effects. Each figure compares industry-specific responses in Manhattan's congestion zone (blue lines) to different control areas (orange lines), revealing complex patterns of substitution and complementarity. These figures display the results of our demeaning methodology across three-digit NAICS industries. Each figure compares Manhattan's congestion zone to a different control area using demeaned log visitor counts. Figure 4, Shows Brooklyn as control and industry panels with demeaned values centered at zero pre-treatment. Post-implementation divergence indicates spillover effects. Figure 5, Reveals moderate spillovers in retail and hospitality using Hudson County as control. Figure 6, Displays the strongest substitution effects, with clear post-implementation divergence in customer-facing industries with Upper Manhattan as control. Figure 7, Shows minimal spillovers, consistent with greater geographic distance with Queens as control. Figure 8, Exhibits negative spillovers in many sectors, suggesting complementarity with Bronx as control. Each panel shows, Gray horizontal line at zero (representing the pre-treatment mean), the Red vertical dashed line at January 5, 2025 (implementation date), Blue lines for congestion zone establishments aggregated at sector level. Orange lines for control area establishments. The Y-axis shows demeaned log visitors (actual values minus pre-treatment means). We discuss the demeaning methodology in detail in next section.

Industries with diverging lines post-implementation experience spillover effects, while parallel evolution indicates no impact. The heterogeneity across industries with retail and food services showing large divergences while professional services remain parallel validates our hypothesis about institutional constraints.

5.3.1 The Demeaning Methodology for Visualizing Spillovers

To visualize the heterogeneous spillover effects across industries, we employ a demeaning methodology that removes pre-treatment level differences while preserving treatment-induced changes. This approach is crucial because establishments in Manhattan and control areas have vastly different baseline activity levels that would obscure treatment effects in raw comparisons.

Our demeaning process follows these steps: For each industry (3-digit NAICS) and location (congestion zone vs. control area), we calculate the mean log visitor count during the pre-treatment period (June 2023 to December 2024):

$$\bar{Y}_{ic,pre} = \frac{1}{T_{pre}} \sum_{t \in pre} \log(Y_{ict}) \tag{3}$$

where i indexes industries, c indexes cities/zones, and t indexes time periods. For each observation, we subtract its group-specific pre-treatment mean:

$$\tilde{Y}_{ict} = \log(Y_{ict}) - \bar{Y}_{ic,pre} \tag{4}$$

We then average the demeaned values by date, location, and industry to create the comparison plots. This demeaning approach has three key advantages: By subtracting pre-treatment means rather than time-varying means, any divergence after January 5, 2025, represents the causal effect of congestion pricing. Industries with vastly different scales (e.g., Food Services with thousands of visits vs. specialized Manufacturing with dozens) can be compared on the same plot. The industry-specific panels clearly show which sectors respond to the policy and which do not.

The resulting graphs show demeaned log visitor counts centered around zero during the pre-treatment period. Post-implementation divergence between Manhattan's congestion zone (blue lines) and control areas (orange lines) indicates spillover effects. Positive divergence suggests substitution (control areas gaining at Manhattan's expense), while parallel evolution indicates no spillover.

The implementation of congestion pricing in Manhattan's Central Business District (CBD) below 60th Street creates powerful economic incentives for spatial and sectoral substitution that manifest as spillover effects across neighboring areas and industry sectors. Our descriptive analysis using log-transformed visitor data which notably excludes healthcare facilities, government services, and air transportation sectors to focus on discretionary commercial activity provides compelling evidence of these spillovers, though we acknowledge these patterns represent correlational rather than fully causal relationships. The exclusion of healthcare, religious, civic, and government sectors from our analysis is methodologically important, as these sectors exhibit inelastic demand characteristics that would confound the measurement of price-induced substitution effects. By focusing on commercial sectors where location choices are more elastic, we can better identify genuine spillover patterns driven by congestion pricing rather than exogenous demand shocks.

5.4 Heterogeneous Industry Effects

Our difference-in-discontinuities analysis provides robust evidence that Manhattan's congestion pricing policy successfully induces temporal reallocation of economic activity. However, The heterogeneous industry responses reveal important distributional consequences obscured by aggregate analysis presented in Figure 8. Manufacturing and transportation sectors characterized by rigid operational schedules and multi-modal supply chains experience significant negative effects even during free hours. These industries face binding constraints that prevent temporal substitution: just-in-time delivery requirements, interstate commerce regulations, and coordinated production schedules create adjustment costs that exceed the benefits of avoiding congestion charges.

Conversely, professional services and hospitality sectors show positive responses, leveraging temporal flexibility to capitalize on the pricing structure. The discontinuity creates focal points for economic coordination coffee shops opening earlier, gyms extending hours, professional services scheduling client meetings before 5 AM generating agglomeration benefits that amplify the direct pricing incentives.

Our findings necessitate fundamental revisions to standard congestion pricing theory. The bottleneck model Arnott et al. (1990) and Small (1992) assumes continuous schedule delay costs, predicting smooth temporal substitution. We find sharp discontinuities effects at 5AM but not 4AM or 6AM suggesting threshold models may better capture behavior.The asymmetry between morning and evening responses challenges the symmetric schedule delay costs assumed since Vickrey (1969). Evening rigidity likely reflects coordination requirements (family dinners, childcare) absent in early morning. Future models should incorporate these institutional constraints explicitly. Moreover, Concerns about temporal arbitrage undermining revenues De Palma and Lindsey (2004) and Lindsey (2004) appear unfounded. With substitution at only one of four boundaries affecting 5.75% of trips, revenue projections remain robust. The knife-edge nature of responses suggests precise calibration matters more than previously recognized Arnott and Small (1998). Cities should identify natural flexibility points rather than imposing arbitrary boundaries. The industry heterogeneity reveals distributional consequences beyond income effects emphasized in previous work Eliasson and Levander (2014) and Levinson (2010). Service workers can avoid tolls while professionals cannot, creating unexpected progressivity. The demeaned visualizations reveal clear patterns of industry heterogeneity. Customer-facing sectors with flexible operations (retail, food services) show strong divergence post-implementation, while sectors with fixed infrastructure or scheduling constraints (professional services, finance) maintain parallel trends despite the pricing change. This visual evidence validates our hypothesis that institutional constraints dominate individual preferences in determining behavioral responses to congestion pricing. The sharp temporal boundaries create highly salient price differences that may generate larger behavioral responses than equivalent smooth price gradients. This aligns with recent evidence on increase in speed by 10-15% in the congestion area by Cook et al. (2025). The divergence between aggregated and disaggregated effects highlights the importance of distributional analysis in policy evaluation. While aggregate efficiency improves, sector-specific losses may require complementary policies subsidies for affected industries, investment in goods movement infrastructure, or temporal zoning flexibility.

6 Conclusion

This study leverages Manhattan's congestion pricing implementation to provide causal evidence on how temporal price discontinuities affect urban economic activity. Using a difference-in-discontinuities design comparing Manhattan to Boston, we identify a 5.75% increase in visits during free hours at the 5 AM weekday threshold demonstrating successful load-shifting but through mechanisms distinct from theoretical predictions.

Our analysis yields three principal findings with direct policy relevance. First, behavioral responses concentrate exclusively at the morning commute threshold, with insignificant effects at evening and weekend discontinuities. This asymmetric pattern suggests capacity constraints during off-peak periods limit temporal substitution, implying that extending discounted hours may not proportionally increase load-shifting. Second, heterogeneous industry responses reveal that institutional constraints dominate individual optimization: manufacturing and logistics sectors experience negative effects even during free hours due to rigid operational requirements, while professional services successfully exploit temporal flexibility. Third, the divergence between positive aggregate effects and no impact on median sector highlights critical distributional consequences while total congestion may decrease, the majority of economic actors bear adjustment costs in terms of decrease in total visitors. These findings challenge standard assumptions underlying congestion pricing design. The concentration of effects at a single threshold contradicts models predicting smooth substitution across time periods. The $9 peak charge induces primarily discrete adjustments at the 5 AM boundary rather than continuous reallocation throughout discounted periods. For policymakers, this implies that graduated pricing transitions may be less effective than anticipated, as behavioral responses appear to be discontinuous rather than price-elastic at the margin.

The heterogeneous industry effects have immediate implications for policy design and political feasibility. Sectors comprising suggesting broad-based business opposition despite aggregate benefits. Transport authorities implementing congestion pricing should anticipate these distributional impacts and consider complementary policies: adjustment assistance for constrained industries, modified commercial vehicle provisions, or temporal exemptions for goods movement.

The success of professional services and hospitality sectors in adapting suggests that policies facilitating operational flexibility such as modified zoning for extended hours could enhance overall effectiveness. Several limitations qualify our findings. The six-month post-implementation window captures short-run responses but not long-term equilibrium adjustments through firm relocation or infrastructure adaptation. Manhattan's unique urban form extreme density, limited access points, extensive transit may generate larger behavioral responses than less constrained environments. While Boston provides a credible control for identification, the magnitude of treatment effects may not generalize to cities with different spatial structures or modal alternatives. Additionally, our reduced-form estimates cannot disentangle welfare implications without observing trip values or purposes.

Our results contribute to evolving understanding of congestion pricing effectiveness. The policy successfully reduces peak-period activity, but through concentrated behavioral adjustments at specific thresholds rather than smooth temporal reallocation. As global cities including London, Singapore, and Stockholm refine their pricing schemes, our evidence suggests that optimal design must account for binding institutional constraints, asymmetric temporal substitution patterns, and heterogeneous industry impacts.

Future research should examine longer-term adaptation as markets adjust to persistent price signals. The sharp behavioral responses at the 5 AM threshold may represent temporary coordination on a focal point that could dissipate as agents learn optimal responses. Alternatively, the discontinuity may reflect fundamental constraints in urban organization that persist in equilibrium. Distinguishing between these possibilities requires extended post-treatment observation and comparison across multiple cities implementing congestion pricing.

The evidence from Manhattan's early experience demonstrates that congestion pricing can achieve meaningful behavioral change and congestion reduction. However, success requires recognizing that urban transportation systems operate through complex institutional constraints rather than frictionless individual optimization. Effective policy must work within these constraints while creating incentives for their gradual relaxation where economically beneficial.

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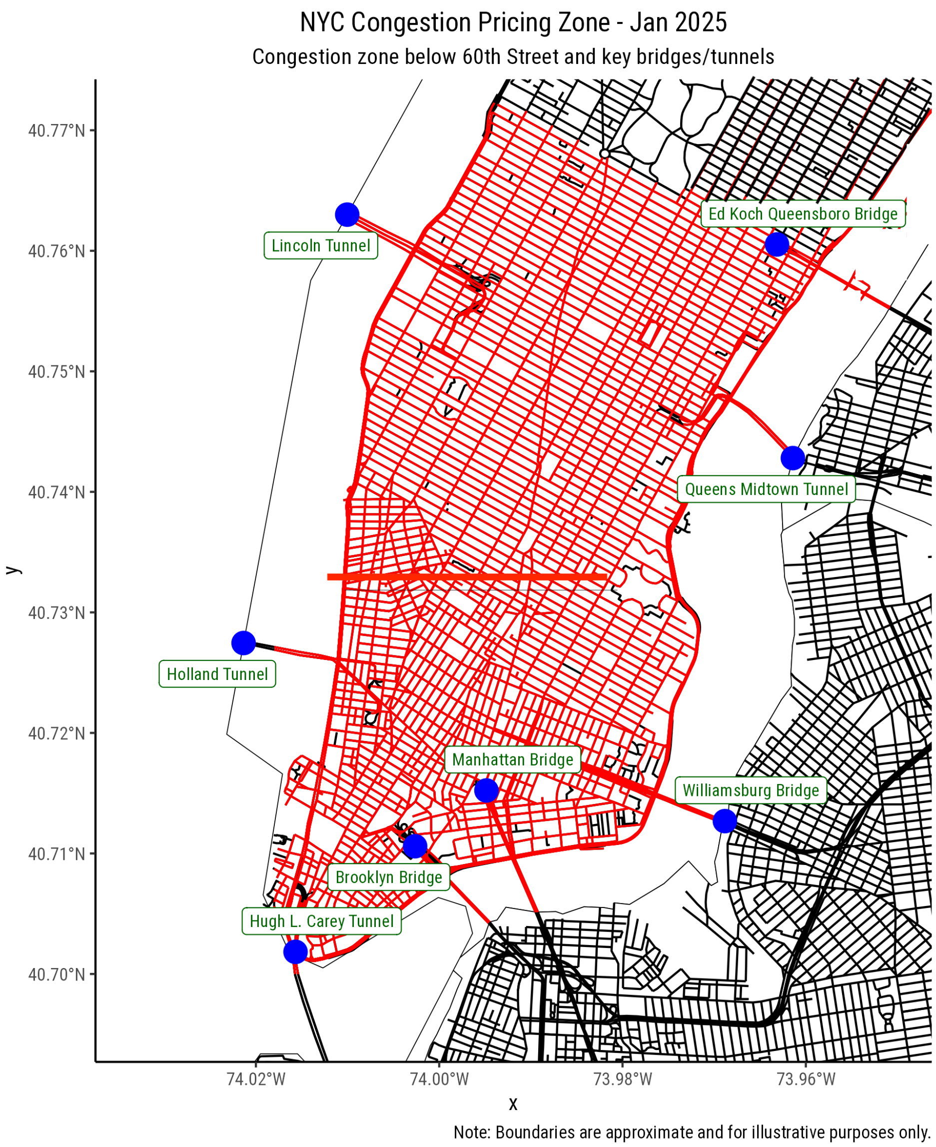
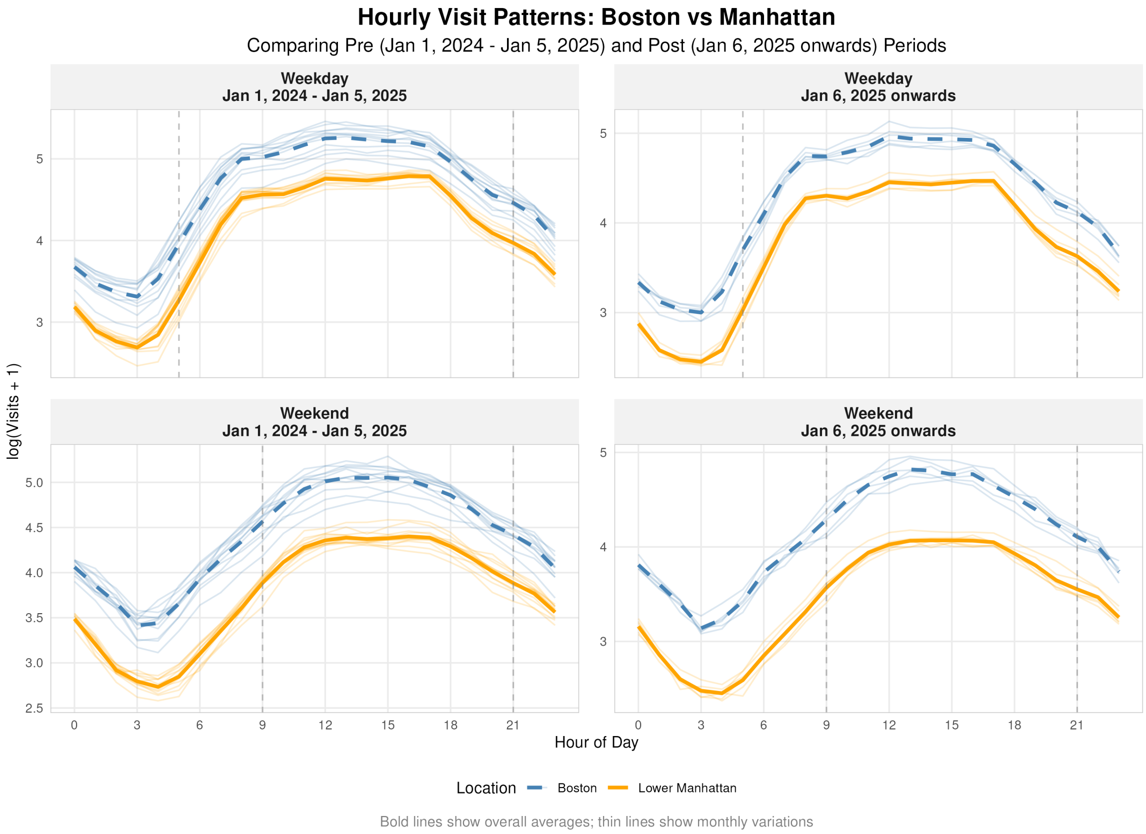
Figures

Figure 1: NYC area under Congestion Zone program. All the marked roads in red line are under congestion zone from January 5th 2025



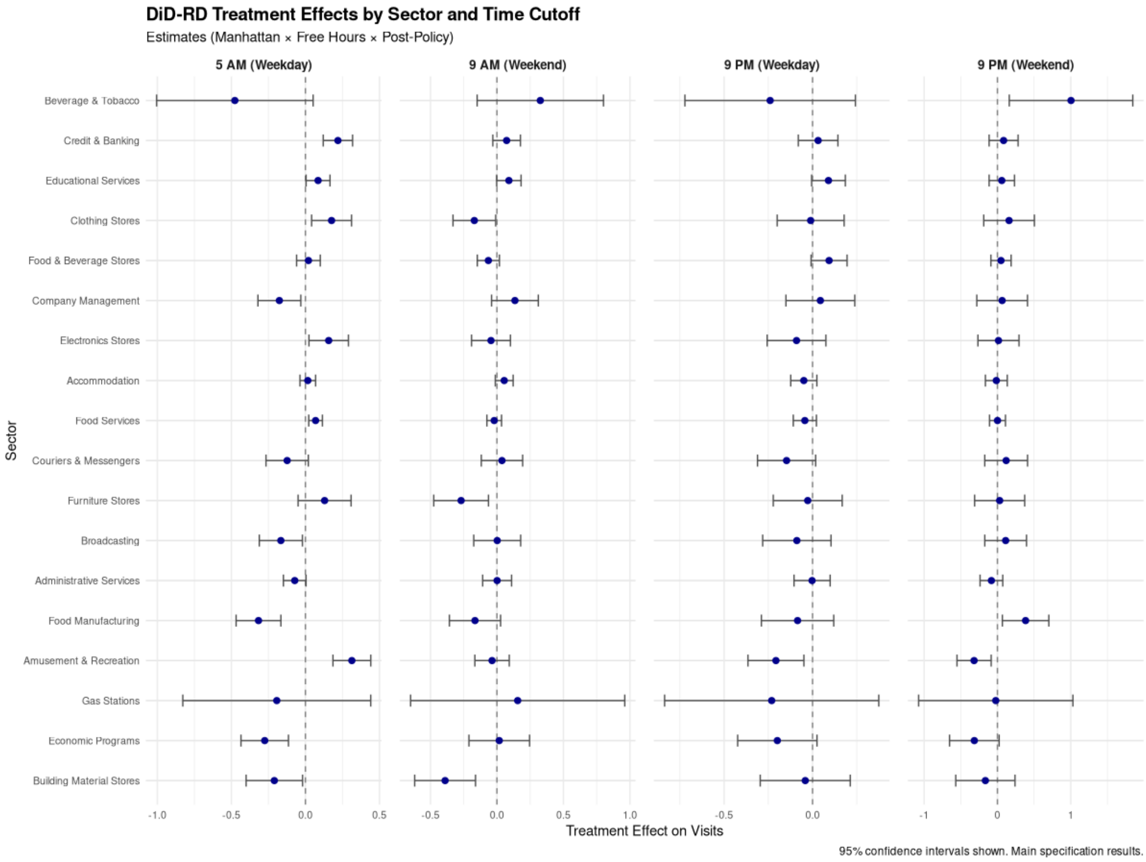
Source: Author used the road map of NYC area from the TIGRIS package in R. Note: Blue dots represent the entry to the Congestion zones form the tunnels

Figure 2: Hourly visit patterns comparing Manhattan and Boston (Control). Time series showing parallel pre-trends and post-implementation divergence at 5AM boundary.



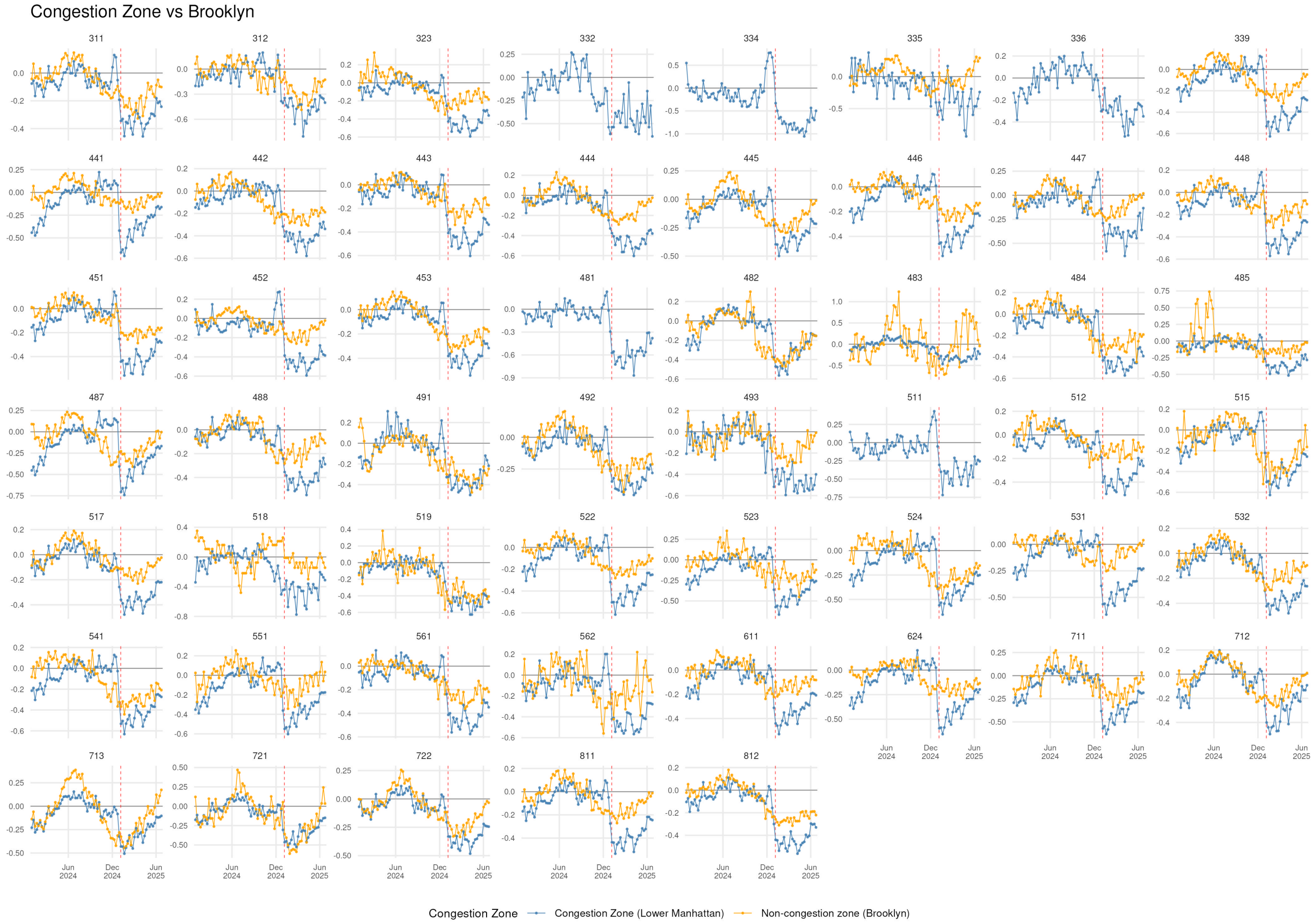
Source: Using balanced-panel for the placekeys (POI) in lower-Manhattan and Boston area, we extracted hourly visitors count from the weekly foot traffic data from Advan. The Congestion prizing program was implemented started from January 6th 2025.

Figure 3: Industry-specific effect at before 5AM (Weekday), before 9AM (Weekend), after 9 PM (Weekday) and after 9 PM (Weekend).



Source: Using balanced-panel for the placekeys (POI) in lower-Manhattan and Boston area, we extracted hourly visitors count from mobile-device location data (Advan). We used NAICS Code - 2017 to identify the sectors.

Figure 4: Brooklyn and lower-Manhattan, Industry-specific spillover effects across control cities. Detailed panels showing heterogeneous responses across NAICS industries for each control city comparison.

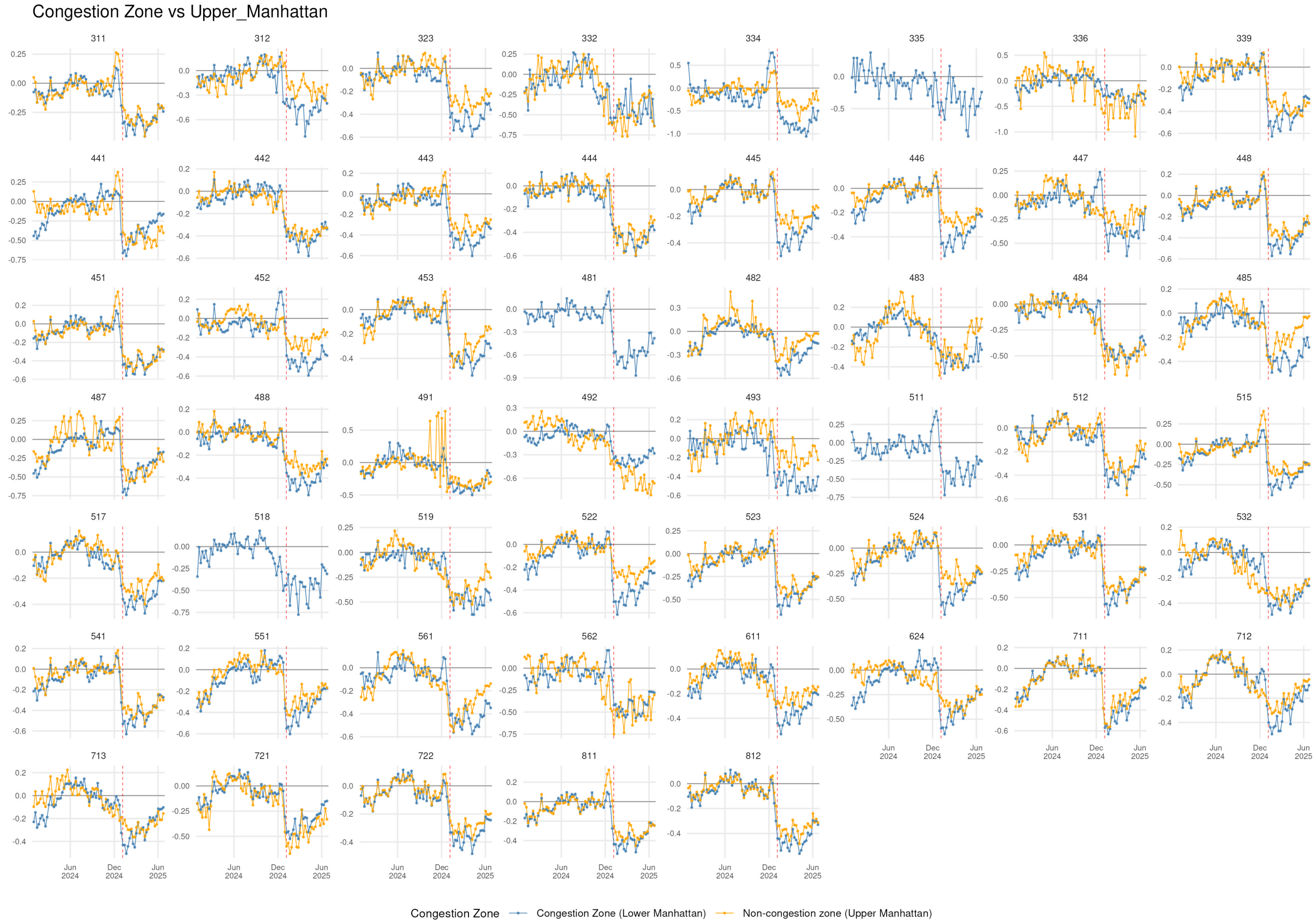


Source: Using balanced-panel for the placekeys (POI) in lower-Manhattan and Brooklyn area, we extracted weekly visitors count from mobile-device location data (Advan). We used NAICS Code - 2017 to identify the sectors.

Figure 5: Husdon County - NJ and lower-Manhattan, Industry-specific spillover effects across control cities. Detailed panels showing heterogeneous responses across NAICS industries for each control city comparison.

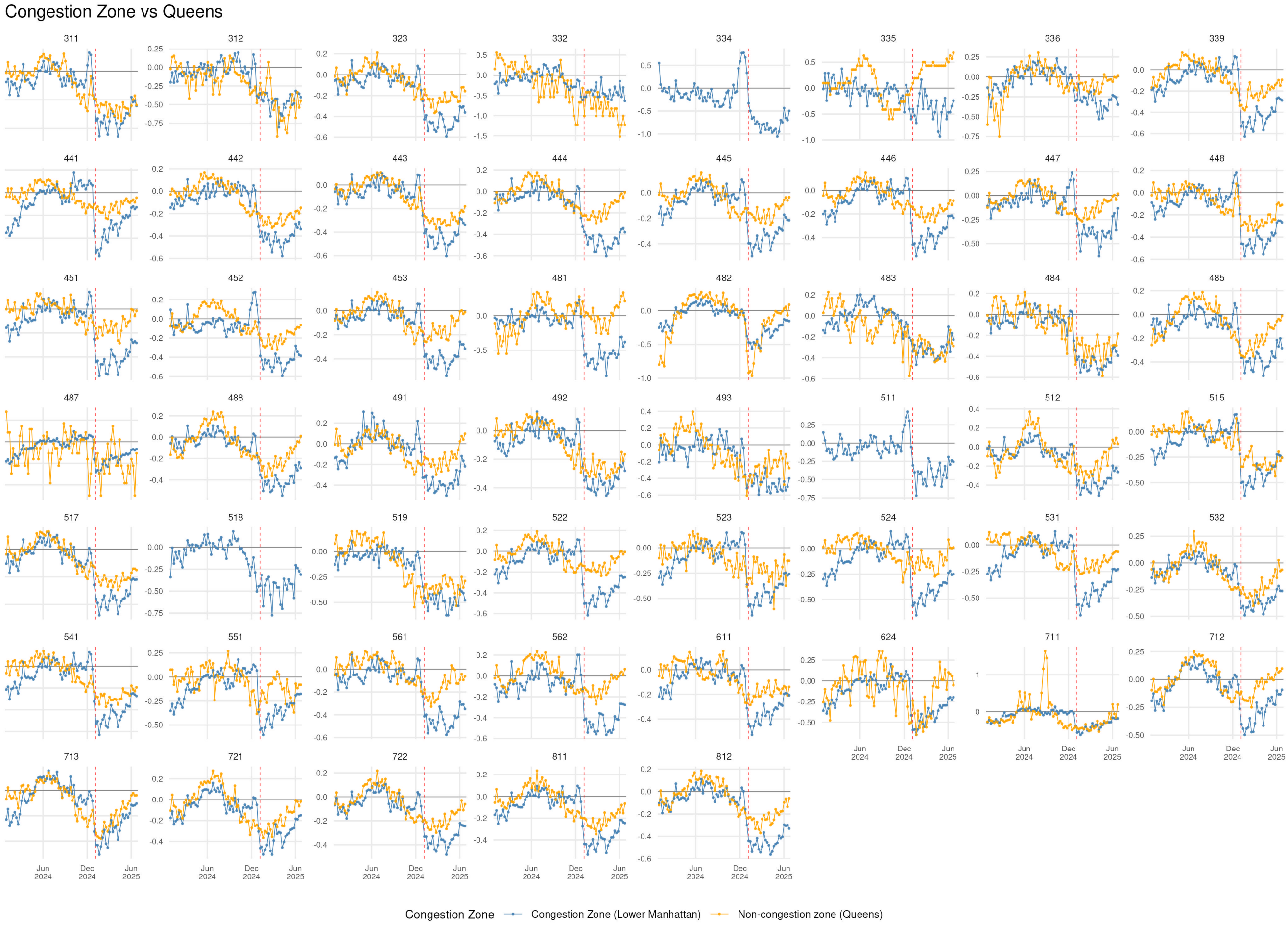
Source: Using balanced-panel for the placekeys (POI) in lower-Manhattan and Hudson County-NJ area, we extracted weekly visitors count from mobile-device location data (Advan). We used NAICS Code - 2017 to identify the sectors.

Figure 6: Upper and lower Manhattan, Industry-specific spillover effects across control cities. Detailed panels showing heterogeneous responses across NAICS industries for each control city comparison.



Source: Using balanced-panel for the placekeys (POI) in lower-Manhattan and Upper-Manhattan area, we extracted weekly visitors count from mobile-device location data (Advan). We used NAICS Code - 2017 to identify the sectors.

Figure 7: Queen and lower Manhattan, Industry-specific spillover effects across control cities. Detailed panels showing heterogeneous responses across NAICS industries for each control city comparison.



Source: Using balanced-panel for the placekeys (POI) in lower-Manhattan and Queen area, we extracted weekly visitors count from mobile-device location data (Advan). We used NAICS Code - 2017 to identify the sectors.

Figure 8: Bronx and lower Manhattan, Industry-specific spillover effects across control cities. Detailed panels showing heterogeneous responses across NAICS industries for each control city comparison.

Source: Using balanced-panel for the placekeys (POI) in lower-Manhattan and Bronx area, we extracted weekly visitors count from mobile-device location data (Advan). We used NAICS Code - 2017 to identify the sectors.

Declaration of Generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used Anthropic to improve readability and language. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

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those of the IHS, nor can the IHS be held responsible for them.

Data availability

The data that has been used is confidential.

Appendix

Table 5: Sample composition and baseline activity across treatment and control areas

	Manhattan (CZ)			**Boston**			**Brooklyn**			**Queens**			**Bronx**			**Upper Manhattan**		
Sector (NAICS)	**N**	**Mean**	**SD**	**N**	**Mean**	**SD**	**N**	**Mean**	**SD**	**N**	**Mean**	**SD**	**N**	**Mean**	**SD**	**N**	**Mean**	**SD**
Retail Trade																		
Motor Vehicle (441)	63	9.04	0.22	85	9.39	0.13	145	7.79	0.11	98	7.37	0.11	73	7.15	0.14	45	6.28	0.23
Building Material (442)	119	9.58	0.20	112	9.71	0.24	167	7.85	0.14	112	7.41	0.15	84	6.31	0.15	73	7.36	0.17
Electronics (443)	489	9.77	0.20	298	9.66	0.19	156	7.59	0.12	134	7.55	0.14	98	7.56	0.17	100	8.19	0.15
Food & Beverage (445)	613	10.70	0.17	412	11.10	0.18	312	9.75	0.14	289	9.83	0.11	245	9.30	0.13	391	9.55	0.11
Health & Personal (446)	526	10.60	0.18	287	10.20	0.19	267	9.27	0.11	234	9.67	0.11	187	8.72	0.11	147	9.41	0.12
Clothing Stores (448)	2,807	11.90	0.20	1,847	12.30	0.17	489	9.60	0.12	378	9.63	0.13	312	8.99	0.12	619	10.10	0.16
Sporting Goods (451)	673	10.30	0.19	396	10.30	0.20	223	7.73	0.11	189	8.48	0.10	134	6.38	0.15	132	8.47	0.19
General Merch. (452)	78	8.36	0.21	98	9.58	0.14	89	9.12	0.10	78	9.00	0.14	67	7.92	0.12	29	8.16	0.13
Misc Store (453)	778	10.30	0.18	487	10.80	0.21	234	8.29	0.14	189	8.38	0.12	145	7.38	0.11	294	9.10	0.16
Services																		
Information (511-519)	1,216	8.73	0.20	765	8.51	0.22	289	6.42	0.16	223	6.54	0.17	167	6.78	0.18	345	7.23	0.19
Finance (522-524)	2,627	11.03	0.21	1,623	11.10	0.22	456	7.04	0.15	367	7.92	0.13	289	6.23	0.17	734	8.86	0.16
Real Estate (531)	1,557	11.70	0.21	896	11.90	0.18	567	10.70	0.10	489	10.70	0.12	423	10.20	0.11	722	9.24	0.18
Professional (541)	3,254	12.00	0.21	1,847	11.90	0.21	678	8.08	0.17	534	8.15	0.15	356	7.25	0.20	879	9.94	0.19
Admin Services (561)	560	10.00	0.20	412	11.60	0.21	189	6.82	0.13	178	7.91	0.12	123	6.41	0.22	576	10.00	0.18
Educational (611)	772	11.00	0.17	487	11.20	0.18	423	10.50	0.10	367	10.30	0.12	312	10.90	0.15	310	10.90	0.14
Accommodation & Food																		
Accommodation (721)	421	10.70	0.18	287	10.60	0.21	189	7.89	0.24	167	8.01	0.17	89	6.82	0.11	165	8.76	0.22
Food Services (722)	3,864	12.30	0.17	2,156	12.70	0.18	1,234	11.10	0.14	1,023	11.30	0.13	789	10.20	0.15	2,004	11.20	0.14
Other Services																		
Repair & Maint. (811)	943	10.80	0.18	598	11.20	0.19	267	8.84	0.12	234	8.87	0.14	156	7.95	0.16	342	8.98	0.17
Personal (812)	2,059	11.10	0.20	1,284	12.20	0.19	567	9.06	0.14	478	9.59	0.14	367	8.46	0.16	1,191	9.63	0.17
Transportation																		
Transit (485)	105	9.15	0.17	85	9.91	0.17	78	6.73	0.21	89	8.58	0.16	67	7.19	0.13	118	9.77	0.14
Transport Support (488)	469	10.10	0.19	298	8.40	0.12	112	6.34	0.12	167	10.20	0.16	98	6.41	0.16	62	8.43	0.14
Manufacturing																		
Food Mfg (311)	88	9.02	0.15	56	8.64	0.21	67	6.84	0.12	56	6.97	0.14	45	6.13	0.16	30	8.43	0.15
Printing (323)	808	10.20	0.21	487	11.00	0.21	189	7.36	0.15	145	7.46	0.14	112	6.56	0.14	295	8.53	0.15
Misc Mfg (339)	674	10.50	0.21	412	9.55	0.22	178	7.60	0.13	156	7.69	0.14	134	6.57	0.16	118	8.12	0.17
Total	**24,704**	**10.47**	**0.19**	**15,234**	**10.52**	**0.19**	**8,745**	**8.42**	**0.14**	**7,234**	**8.61**	**0.14**	**5,423**	**7.89**	**0.15**	**10,234**	**9.32**	**0.17**

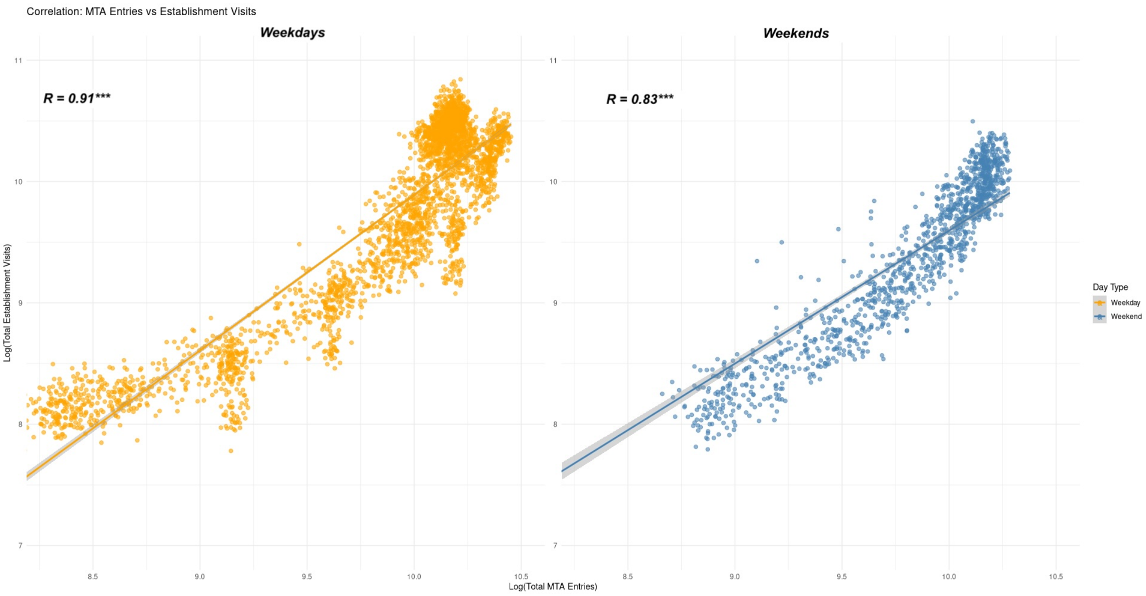
Notes: Manhattan (CZ) denotes the Congestion Zone (treatment area south of 60th Street). Upper Manhattan represents areas north of 60th Street. N = number of establishments. Mean and SD represent log visitors during baseline period (2024). Excluded sectors: ambulatory health care, hospitals, nursing care, religious organizations, justice/safety, national security, and air transportation.

Table 6: Regression Estimates for hourly visitors by Weekend and Weekday Cutoffs

	Weekend		**Weekday**	
	9PM	9AM	9PM	5AM
Dependent var.	Unique visitor			
Manhattan × Post	0.0032	-0.0054	0.0196	0.0485***
	(0.0289)	(0.0249)	(0.0177)	(0.0173)
Free hours × Post	0.0208	0.0175	0.0227	-0.1464***
	(0.0592)	(0.0253)	(0.0296)	(0.0243)
Post × Running	-0.0059	0.0123**	-0.0268***	0.0036
	(0.0087)	(0.0060)	(0.0064)	(0.0037)
Manhattan × Free hours × Post	**0.0069**	**0.0083**	**-0.0253**	**0.0575****
	(0.0621)	**(0.0332)**	**(0.0356)**	**(0.0268)**
Free hours × Post × Running	-0.0277	-0.0118	-0.0127	-0.0066
	(0.0379)	(0.0095)	(0.0209)	(0.0090)
Manhattan × Post × Running	0.0036	0.0007	0.0198***	-0.0100**
	(0.0103)	(0.0066)	(0.0065)	(0.0042)
Manhattan × Free hours × Post × Running	0.0223	0.0059	0.0078	0.0131
	(0.0444)	(0.0122)	(0.0252)	(0.0092)
Fixed Effects				
Date	Yes	Yes	Yes	Yes
Hour × City	Yes	Yes	Yes	Yes
Observations	1,656	3,036	4,164	7,634
Squared Correlation	0.93191	0.96706	0.92135	0.96889
Pseudo R^2	0.92970	0.97052	0.92245	0.96681
BIC	545,370.8	801,388.6	1,547,551.9	3,176,518.2

Notes: Manhattan, is 1 if the POI is located in the congestion prizing zone, 0 otherwise. Post, is equal to 1 if the date is later than January 5th 2025. Free Hour, is equal to 1 if the visit hour is before the congestion prizing starts for morning cut-off and after the congestion zone pricing ends for the evening cut-off, 0 otherwise. Standard errors in parentheses. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. The Standard errors are clustered at the date.

Figure 9: Correlation between MTA's vehicle entery data and visitor's from mobile-device data on weekend and weekdays



Source: Author used geolocations of the POIs to identify the congestion zone area in NYC from the TIGRIS package in R and MTA data from the website.