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ORIGINAL

Subway ridership inside and outside Manhattan's congestion relief zone during the first year of congestion pricing

Afluencia al metro dentro y fuera de la zona de congestión de Manhattan durante el primer año del peaje

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ABSTRACT

Introduction: Congestion pricing was expected to shift travel toward the subway, yet station-level evidence on ridership inside the tolled zone is scarce. This study compared subway entries at station complexes inside Manhattan's congestion relief zone with those outside it during the first year of the toll. Methods: Open administrative data from the MTA subway hourly ridership datasets were aggregated to station-day entries for 5 January to 31 December 2024 (pre-toll) and the same window in 2025 (first year of the toll). Station complexes south of 60th Street in Manhattan were classified as inside the zone. A difference-in-differences model estimated the ratio of inside to outside ridership change on the log scale. Results: Among 424 station complexes, 65 were inside and 359 outside the zone. Average daily entries rose 7.5% inside and 9.5% outside the zone. The ratio of the inside to the outside change was 0.983 (95% CI 0.966-1.000; p = 0.044), and 0.964 (95% CI 0.939-0.991; p = 0.008) on weekends. Conclusions: Subway ridership inside the congestion relief zone did not increase relative to stations outside it during the first year of congestion pricing. The toll's anticipated mode-shift dividend to the subway was not observed at the station level.

Keywords: congestion pricing; subway ridership; public transport; difference-in-differences; New York City; urban mobility.

RESUMEN

Introducción: Se esperaba que el peaje por congestión trasladara viajes al metro, pero hay poca evidencia a nivel de estación sobre la afluencia dentro de la zona tarifada. Se compararon las entradas al metro en los complejos dentro de la zona de congestión de Manhattan con las de fuera durante el primer año del peaje. Métodos: Los datos administrativos abiertos de afluencia horaria del metro de la MTA se agregaron a entradas por estación y día del 5 de enero al 31 de diciembre de 2024 (antes del peaje) y en el mismo periodo de 2025 (primer año). Los complejos al sur de la calle 60 en Manhattan se clasificaron como dentro de la zona. Un modelo de diferencia en diferencias estimó la razón del cambio de afluencia dentro y fuera en escala logarítmica. Resultados: De 424 complejos de estación, 65 estaban dentro y 359 fuera de la zona. Las entradas diarias medias crecieron 7,5% dentro y 9,5% fuera. La razón del cambio dentro respecto al de fuera fue 0,983 (IC 95 % 0,966-1,000; p = 0,044), y 0,964 (IC 95 % 0,939-0,991; p = 0,008) los fines de semana. Conclusiones: La afluencia al metro dentro de la zona no aumentó en relación con las estaciones de fuera durante el primer año del peaje. El trasvase al metro esperado no se observó a nivel de estación.

Palabras clave: peaje por congestión; afluencia al metro; transporte público; diferencia en diferencias; Nueva York; movilidad urbana.

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INTRODUCTION

Congestion pricing charges road users for entering a defined urban area at times when the road network is scarce, and it remains one of the few instruments able to reduce traffic, raise revenue, and redirect travel toward public transport at the same time.

The economic rationale treats road space as a priced input whose underpriced use produces external congestion costs, and the empirical literature on cordon and area tolls has repeatedly confirmed that prices change driving behavior.(1,2)

The same literature is less settled on the size of the transit response, which is why a program that finances transit with toll revenue still requires direct evidence on whether ridership follows the revenue. Whether the same prices also change transit ridership is a separate question, because the transfer of trips from cars to trains depends on the alternatives available to each driver and on how much of the tolled demand is discretionary.(3)

New York City introduced the first cordon toll in the United States on 5 January 2025. Vehicles entering Manhattan south of 60th Street, the zone the Metropolitan Transportation Authority calls the congestion relief zone, pay a charge whose revenue is committed by law to the subway, bus, and commuter rail network.(4,5)

The base toll for passenger vehicles during the day is nine dollars, with a lower overnight rate and credits for drivers who already paid a toll at certain river crossings, so the effective price varies by time of day and entry point.(6)The design was built around the subway, because the toll applies to the part of Manhattan where the subway is densest and its revenues address the system's capital needs.

The New York subway is the largest rapid transit network in the United States, and before the pandemic it carried more than five million riders on an average weekday, with Manhattan stations accounting for a large share of the entries. The pandemic collapsed that demand, and the recovery has been uneven: ridership at Manhattan stations tied to office employment has returned more slowly than ridership in the outer boroughs, where trips are less dependent on commuting.(7,8)

Hybrid work arrangements have kept weekday office attendance below its pre-pandemic level, which depresses demand at exactly the central business district stations that lie inside the tolled zone. This incomplete recovery is the backdrop against which the toll began, and it complicates any comparison of 2024 and 2025 because the two groups of stations may have been on different recovery trajectories even without the toll.(9,10)

The question posed by the toll is therefore not only whether subway demand grew, but whether it grew differently inside the tolled zone than outside it.

Supporters of the policy projected a measurable shift from cars to trains. Simulation and scenario studies of the New York cordon estimated that a daily charge on vehicles entering the central business district would raise transit use and reduce vehicle travel, with the largest shifts concentrated among commuters bound for Manhattan.(11,12)

Health and equity analyses framed the toll as a way to expand transit access for the populations that depend on the subway, in part through the reinvestment of the revenue.(13,14)

The equity debate has been prominent in the program's public reception, and it turns on whether the groups most exposed to the toll are also the groups that gain from a stronger subway.(15,16)

These expectations rest on the assumption that a price on driving into the core translates into boardings at the stations inside that core, an assumption that is rarely tested against station-level counts after a toll goes live.

The record from other cities tempers that expectation. In London, the congestion charge reduced traffic, congestion costs, and pollution, but its effect on public transport ridership was comparatively small, and the most robust benefits appeared in travel speeds and emissions rather than in a large transfer of trips to transit.(1,17,18)

The Gothenburg cordon changed travel times and revenues more than it changed the transit mode share, and evaluations of congestion surcharges on ride-hailing in Chicago found effects concentrated in vehicle travel and pricing responses rather than in transit boardings.(19,20)

Even where mode shift has been documented, it has often been modest and conditional on the existing level of transit service.(21)

The Scandinavian cordons, such as the Gothenburg charges, are the closest European analogues in cities with mature transit, and their evaluations likewise emphasize traffic and travel-time outcomes over large transit gains.(19)

This contrast leaves open the question of how much of the mode-shift promise a cordon toll can deliver in a city where the subway already carries the majority of trips into the core.

The first evaluations of the New York program have examined traffic speeds, emissions, road safety, ride-hailing, and distributional outcomes during 2025.(4,22-24)

Those studies document faster travel and shifting toll responses, and earlier work on New York's ride-hailing surcharge showed that per-trip fees alter for-hire vehicle demand.(25)None of them, however, resolves whether subway ridership at stations inside the zone changed relative to stations outside it.

Station-level evidence matters because the dedicated revenue and the political justification of the toll rest on its capacity to move people onto the subway. A station-level comparison also captures the geographic dimension that systemwide totals obscure: the toll could raise subway use in the core, in the outer boroughs, or both, and those alternatives carry different meanings for equity and for the case for the toll.

The objective of this study was to estimate the change in subway entries at station complexes inside the congestion relief zone during the first year of the toll relative to station complexes outside the zone. The hypothesis was that entries inside the zone would increase relative to entries outside it.

METHODS

Design and study period

This was an observational quasi-experimental study using a difference-in-differences design and open administrative data, reported following the STROBE recommendations for observational studies.(26)

The pre-toll period ran from 5 January to 31 December 2024, and the post period ran from 5 January to 31 December 2025, the first year of the toll. The pre period contained 362 days and the post period 361 days. The two windows share the same calendar span and holiday structure, which removes the seasonal variation that dominates daily subway demand.

The toll began on 5 January 2025, so the post window starts on the first day the charge was in force, and the four days of January 2025 that preceded the toll are excluded from the post window in the same way that the first four days of 2024 are excluded from the pre window. The year 2024 was chosen as the baseline because it is the calendar year immediately preceding the charge and the last complete year before it, and because it falls within the same post-pandemic recovery phase as 2025, which limits the distance between the two comparison years. A longer pre period would reach back into earlier stages of the pandemic recovery, when the relationship between Manhattan and outer-borough ridership was changing more rapidly.

Data source

Subway entries were obtained from two open datasets published by the Metropolitan Transportation Authority on the New York State open data portal: the MTA Subway Hourly Ridership dataset for 2020 to 2024 and the continuation dataset for 2025 onward.(27,28) The two datasets are consecutive, with the first ending on 31 December 2024 and the second beginning on 1 January 2025, so the pre and post windows each fall entirely within a single dataset and no observation crosses the boundary between the two files.

Both datasets report hourly entries by station complex, payment method, and fare class, together with the borough, latitude, and longitude of each complex. Data were retrieved programmatically without a login through the Socrata Open Data API. Structured queries summed the ridership field by station complex and calendar day for rows whose transit mode is subway, using the station-day as the unit of analysis.

The ridership field counts riders who entered a station complex through OMNY or MetroCard in the hour, and it already includes free transfers, so it is a complete measure of entries. The two payment methods, contactless OMNY and the older MetroCard, are summed together because the ridership field reports entries under either method, and the fare-class breakdown is collapsed because it does not affect the station-day total. The station list used for the classification was obtained with a separate query that grouped the same source by station complex to recover the name, borough, and coordinates of every complex that recorded a subway entry.

Station classification relative to the zone

The congestion relief zone covers Manhattan south of 60th Street. Each of the 424 subway

station complexes that recorded entries during the study window was classified as inside the zone when it was located in Manhattan and south of 60th Street, identified from the station name and its coordinates. Station complexes in the Bronx, Brooklyn, and Queens, together with those in Manhattan at or north of 63rd Street, were classified as outside the zone. Two special cases were assigned explicitly.

The Roosevelt Island station complex lies in the East River, east of the central business district, and was therefore assigned to the outside group. The station complexes of the Roosevelt Island Tram and the Staten Island Railway were excluded because they are not subway stations. The classification produced 65 station complexes inside the zone and 359 outside it. Because no subway station complex sits between 59th and 63rd Streets, the boundary assignment is sharp in practice, and the stations immediately on either side of the boundary, Columbus Circle and 63rd Street, fall unambiguously on the inside and the outside respectively. The classification was applied uniformly to both years so that a station's group could not change between the pre and post periods.

Outcome

The outcome was the number of subway entries per station complex per day, obtained by summing the hourly ridership field over payment methods and fare classes. Station-day completeness was 99.8% in the pre period and 99.1% in the post period, and no station complex recorded zero entries in either period. The small number of missing station-days was treated as missing rather than imputed, and the average daily entries used each station's own count of recorded days as its denominator.

The outcome was computed separately for all days, for weekdays, and for weekends, so that each analysis used the same stations but a different day-of-week subset. This separation matters because the toll's lower overnight rate and the fixed weekday commuting pattern mean that a shift in weekday entries and a shift in weekend entries could reflect different responses, and pooling them would hide either one.

Statistical analysis

For each station complex, the average daily entries were computed in each period as the total entries divided by the number of days with records. The change in log average daily entries between the pre and post periods was then regressed on an indicator for being inside the zone. The coefficient of that indicator is the difference-in-differences estimate, and its exponential is the ratio of the inside relative change to the outside relative change. A ratio below one indicates that entries inside the zone grew less than entries outside it.

The log transformation makes the estimate a ratio of relative changes and reduces the influence of the largest stations, whose daily entries span several orders of magnitude across the network. The two-period, two-group design does not rely on staggered treatment timing, but the modern difference-in-differences literature is the reference for the identifying assumptions of the method, in particular the assumption that inside and outside stations would have followed parallel trends in the absence of the toll.(29,30)

Heteroscedasticity-robust standard errors were used, and each estimate is reported with its 95% confidence interval before the p value. Significance was assessed at the 0.05 level with two-sided tests. Because the estimate is computed from station-level changes rather than from pooled daily counts, the standard errors reflect between-station variation in the change, which is the appropriate level of clustering for this design.

The coefficient can be read as the difference between the inside and outside growth rates, and its exponential as the factor by which the inside relative change exceeds or falls short of the outside relative change. The same model was estimated separately for weekdays, defined as Monday through Friday, and for weekends, defined as Saturday and Sunday, because the toll applies around the clock but the demand profile differs sharply between working days and the rest of the week.

Sensitivity analysis

The primary comparison uses all stations outside the zone as the control group. Because outer-borough stations may have recovered from the pandemic at a different pace than Manhattan stations, a sensitivity analysis restricted the comparison to Manhattan stations, contrasting station complexes inside the zone with those north of 60th Street. This narrower control removes borough-level recovery trends as a source of bias, at the cost of a smaller control group, and it holds constant the part of demand that depends on the density of Manhattan itself.

The Manhattan-only comparison is the most direct test of whether the toll changed ridership

inside the zone relative to a nearby control that shares the same land use and labor market. The trade-off between the two control groups is deliberate: the full outside group maximizes statistical power, while the Manhattan-only group maximizes comparability, and agreement between them is more informative than either estimate alone.

RESULTS

Station complexes and descriptive entries

The 424 station complexes that recorded entries in both years comprised 65 inside the congestion relief zone and 359 outside it (Table 1). Inside the zone, average daily entries rose from 21,294 in 2024 to 22,884 in 2025, an increase of 7.5%. Outside the zone, average daily entries rose from 5,344 to 5,854, an increase of 9.5%. The inside complexes carried substantially more riders per day than the outside complexes in both years. The outside group spanned the Bronx, Brooklyn, Queens, and upper Manhattan, whereas the inside group consisted of complexes in central and lower Manhattan.

Convergent ridership growth inside and outside the zone

Entries grew in both groups, and the weekly series followed a similar course inside and outside the zone across the two years (Figure 1). Both indexed series remained above their 2024 average through most of 2025, and the two series stayed close to one another rather than separating after the toll began. The ratio of the inside relative change to the outside relative change was 0.983 (95% CI 0.966-1.000; p = 0.044) (Table 2). The point estimate was below one, and the confidence interval excluded one.

Divergent weekend growth

The overall pattern was reproduced on weekdays, where the ratio was 0.985 (95% CI 0.968-1.001; p = 0.070). On weekends the divergence was larger. Entries inside the zone rose 8.3%, while entries outside it rose 11.3%, giving a ratio of 0.964 (95% CI 0.939-0.991; p = 0.008).

Gray zone: the Manhattan-only comparison

When the control group was restricted to Manhattan stations north of 60th Street, whose entries rose 7.6%, the ratio of the inside change to the control change was 0.999 (95% CI 0.981-1.017; p = 0.905). The inside and control groups therefore changed at almost the same rate once the comparison is confined to Manhattan.

Table 1. Subway station complexes and average daily entries by location relative to the congestion relief zone, 2024 and 2025.

Group	Station complexes (n)	Average daily entries 2024	Average daily entries 2025	Change (%)
Inside the congestion relief zone	65	21294	22884	7.5
Outside the congestion relief zone	359	5344	5854	9.5

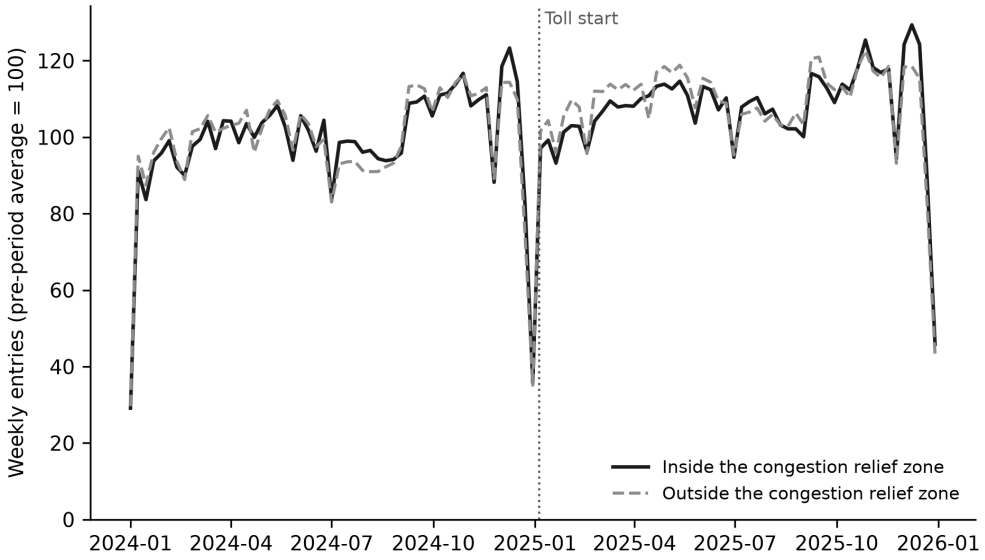
Note: The congestion relief zone comprises Manhattan station complexes south of 60th Street, excluding Roosevelt Island.

Table 2. Change in subway entries inside versus outside the congestion relief zone between 2024 and 2025.

Stratum	Inside change (%)	Outside change (%)	Ratio inside/outside	95% CI	p
All days	7.5	9.5	0.983	0.966-1.000	0.044
Weekdays	7.2	9.0	0.985	0.968-1.001	0.070
Weekends	8.3	11.3	0.964	0.939-0.991	0.008
Sensitivity: inside versus Manhattan north of 60th Street	7.5	7.6	0.999	0.981-1.017	0.905

Note: The ratio inside/outside is the exponentiated difference-in-differences coefficient from a regression of the station-level change in log average daily entries on an indicator for stations inside the zone, with heteroscedasticity-robust standard errors. CI = confidence interval.

Figure 1. Weekly subway entries inside and outside the congestion relief zone, 2024 to 2025.



Note: Weekly entries are indexed to the 2024 pre-period average of each group, set to 100. The dotted vertical line marks the start of the toll on 5 January 2025.

DISCUSSION

The results run against the expectation that congestion pricing would lift subway ridership inside the tolled zone relative to the rest of the city. Inside the zone, entries rose over the first year of the toll, but they rose slightly less than entries outside it, and the confidence interval around the overall ratio excluded one. This reading does not support a mode-shift dividend to the subway during the first year, and it shifts attention from the question of whether the toll moved people onto trains to the question of what the subway's own recovery path looks like in the years after the toll.

This finding sits alongside, rather than against, the early evaluations of the New York program. Studies of 2025 reported faster travel in the zone, reduced ride-hailing activity, and behavioral responses to the time-varying charge, all of which are consistent with fewer cars but do not require a proportionate increase in subway boardings.(4,22)

Road safety and emissions work similarly identified the vehicle side of the ledger as the place where the toll acted first.(23,31)

Distributional analyses describe compensatory transit strategies precisely because the transit benefits of the toll were uncertain.(6,24)

This contrasts with the pre-implementation simulations, which projected mode shift from calibrated models rather than from observed station counts.(11,12)The gap between those projections and the observed station counts is the concrete divergence the present study quantifies.

The design of the toll itself helps explain the null result. The charge targets vehicles entering the zone, and the population that already enters Manhattan by subway, commuters bound for the densest part of the network, faced no new price and had no reason to change behavior. The margin where the toll could add subway riders is the group of drivers who could substitute a train for a car trip, and that group is small relative to the base of existing riders.(3)

The lower overnight rate and the credits for tunnel crossings further soften the price signal at exactly the times and entry points where driving into the zone was least likely to be replaced by a subway trip. In this sense the station-level null is not evidence that the toll failed, but evidence that its first-year effect was concentrated on vehicles rather than on the subway that the revenue is meant to fund.

The magnitude of the estimate is small in practical terms even where it is statistically distinguishable. The overall ratio of 0.983 corresponds to inside growth that was roughly one and a half percentage points below the outside growth of 9.5%, and the weekend ratio of 0.964 corresponds to a difference of about three percentage points.

These are not the large mode-shift effects that the pre-implementation projections antici-

pated, and they are consistent with a toll that reshaped vehicle travel while leaving the subway's recovery largely untouched. The distinction matters for interpretation: a null ridership effect is compatible with a successful traffic effect, and the two outcomes should not be collapsed into a single measure of program success.

The comparison with other cities clarifies why a null station-level result is plausible. The London congestion charge delivered clear reductions in traffic, congestion costs, and pollution, but its measurable effect on public transport ridership was modest, and the strongest effects were in vehicle outcomes.(1,17,18)

Studies of the same charge also found effects on road casualties and on equity that operated through the vehicle side rather than through transit.(32-34)

The Gothenburg cordon changed travel times and revenues more than it changed the transit mode share, and a review of road pricing and equity likewise found the strongest effects on driving rather than on transit use.(16,19)

In Chicago, a congestion surcharge affected ride-hailing demand rather than transit boardings, and a scoping review of congestion pricing and safety reported the same concentration of effects on the road network.(20,35)

Even in Seoul, a dense-transit city comparable to New York, subway ridership after the pandemic responded to the compactness of the built environment more than to any single demand-management instrument.(36)

The common methodological cause across these studies is that cordon tolls act first on the price of driving, so traffic and emissions effects appear immediately, while any transit response depends on the alternatives available to former drivers.

The transit response also operates on a longer time scale than the traffic response, because changing travel mode involves adjustments to schedules, fares, and household routines that do not happen in the first weeks of a toll. A first-year evaluation is therefore better suited to detecting the traffic effect, which is immediate, than the transit effect, which may accumulate only as drivers reorganize their routines.

The weekend divergence has a concrete explanation in the demand structure of the New York subway. Weekday ridership is dominated by commuters who already use the subway into Manhattan, so there is little remaining car travel to convert, whereas weekend trips are more discretionary and more likely to start outside the zone.(7,37)

The toll, which is lower outside peak hours, may have reduced weekend driving into the zone without an equivalent increase in weekend subway entries, leaving outside stations, many of them in outer boroughs, to grow faster. Weekend travel also responds more readily to fare levels and to leisure destinations outside the core, which are exactly the trips that the outer-borough stations serve.

This pattern parallels the post-pandemic observation that subway ridership in Manhattan recovered more slowly than ridership in the outer boroughs, where trips are less tied to office attendance.(8,10)

Bike sharing programs in New York show the same substitution logic, in which a new travel option draws riders away from the subway at particular stations rather than adding to the network.(38)

The weekday estimate, in contrast, is the closest the analysis comes to the commuting margin, and its confidence interval comfortably contains one, which indicates that the toll did not measurably change weekday commuting by subway either way.

The gray zone of the sensitivity analysis strengthens this interpretation. When the control group is confined to Manhattan stations north of 60th Street, the inside and control groups change at almost the same rate, and the difference disappears. This reading attributes the overall gap to faster recovery of outer-borough ridership rather than to a toll effect inside the zone.

That interpretation matches the documented uneven recovery of New York transit after the pandemic, in which Manhattan-oriented demand recovered more slowly than travel elsewhere in the city, partly because of behavioral inertia in the return to transit.(8,9)

Differential recovery is therefore a specific confounding mechanism, not a generic contextual difference, and it is exactly the kind of mechanism a borough-confined control group can detect.(39)

The agreement between the primary estimate and the Manhattan-only sensitivity does not resolve the mechanism with certainty, but it shifts the burden toward an explanation that has nothing to do with the toll, namely the continuing return of outer-borough ridership toward its pre-pandemic level. What remains open is whether the inside stations, which still carry far more entries per day than the outside stations, are simply closer to their pre-pandemic ceiling and therefore have less room to grow.

The strengths of the study are the completeness of the underlying data, which covers all station complexes that recorded entries in both years, and the pre and post windows, which are identical in calendar span and in holiday composition. The analysis separates weekdays from weekends, which are the two demand regimes of the New York subway, and it includes a Manhattan-only sensitivity that isolates the comparison from borough-level recovery trends.

The estimates come from a reproducible script over public data, so the station classification and the aggregation can be audited, and the same script was run twice with identical results. The use of all station complexes rather than a sample removes selection bias from the comparison, and the measurement of entries, rather than modeled ridership, keeps the outcome close to the raw administrative record. The weekly figure adds a visual check on the identifying assumption, because any abrupt separation of the two series at the start of the toll would be visible as a break in the lines, and no such break appears.

Several limitations warrant attention. The station-level design is ecological, so it cannot identify the travel choices of individual drivers, and the single treated zone precludes the multi-unit comparisons available to synthetic control designs.(30)

The assignment of stations to the zone rests on the 60th Street boundary, which is sharp but may misclassify complexes near the edge, although only the stations between 59th and 63rd Streets are affected. The first-year window is short relative to the years needed for residential and employment responses to mature, and the pre period falls within an ongoing pandemic recovery, so the parallel-trends assumption cannot be tested far into the past.(2)

The analysis also cannot distinguish the toll's effect from coincident changes in service frequency, fare policy, or office attendance during 2025, and the entry counts cannot reveal the trip origin of riders, so it is not possible to observe directly whether former drivers became subway riders. Each limitation suggests a specific remedy: individual-level travel diary or tap data would recover mode substitution, a longer follow-up would capture adjustment, boundary-based sensitivity would test the classification, and service-change data would separate the toll from other shocks.

For policy, the results counsel against counting on automatic mode shift from cordon tolls. The New York toll achieved its traffic goals in the first year while subway ridership inside the zone tracked the citywide recovery, which implies that transit gains, if they materialize, will require complementary investments in service and fares rather than the toll alone.(6,13)

Agencies elsewhere that are considering cordon tolls should treat the subway as the instrument of the zone's success only if the service is expanded in step with the toll, and they should evaluate the policy against traffic outcomes first and transit outcomes over a longer horizon.(21,36)

The revenue committed by the New York program remains a transit dividend even when the ridership dividend is not yet visible, and the distinction between the two deserves more attention in the public debate about the toll. An evaluation that finds no first-year ridership shift is not a reason to undo the toll, but it is a reason to monitor the subway separately from the road network and to report both outcomes when the program is reviewed. The two outcomes are produced by different margins of behavior, and a policy that is judged only by its traffic results will never see the subway's slower, longer response.

CONCLUSIONS

Subway entries at station complexes inside Manhattan's congestion relief zone grew during the first year of the toll but did not increase relative to stations outside the zone, and the differential was driven by slower growth inside the zone on weekends. The overall difference-in-differences estimate was close to one, and a Manhattan-only sensitivity comparison showed no divergence, which indicates that the toll was not associated with a station-level mode-shift dividend to the subway in its first year. The weekly evolution of entries inside and outside the zone followed the same course across the study window, and the toll's first year produced no separation between the two series that the station-level comparison could detect.

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