The Short-Run Effects of Congestion Pricing in New York City*

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Abstract

We study the impacts of New York City's Central Business District (CBD) Tolling Program, the first cordon-based congestion pricing scheme in the United States. Using a generalized synthetic controls approach that compares outcomes in NYC to contemporaneous outcomes in other cities, we find that the policy increased speeds on CBD roads by 11%, with little-to-no effect on air quality, transactions at shops and restaurants, or overall foot traffic in the CBD. Speeds also increased *outside* the CBD, especially on roads commonly traversed by drivers traveling to/from the CBD. These spillovers lead to faster trips throughout the metro area, including for many unpriced trips. We develop a simple model to bound the effects on driver welfare. If drivers have a Value of Travel Time (VOTT) of \$40/hour, then we estimate that driver welfare increased by at least \$14.3 million/week, before any revenue recycling or environmental benefits. Passenger vehicles headed to the CBD are only better off if their VOTT exceeds \$153/hour, but the modest speed improvements for the many unpriced trips reduce the overall 'break-even' VOTT of these drivers to at most \$21/hour. Finally, we show how characteristics of local travel patterns and road networks can inform the potential impacts of introducing cordon-based congestion pricing in other cities.

JEL codes: **R41, R48, R5, H2, H71**

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1 Introduction

Congestion pricing is a textbook application of Pigouvian taxation (Pigou, 1920), and, in theory, can achieve first-best traffic allocations by internalizing congestion externalities (Vickrey, 1963, 1969). In practice, however, congestion pricing programs are rare and always second-best: they price only a subset of roads, trips, or times. The welfare impact of such programs depends on how traffic flows adjust across the road network and the resulting effects on road speeds for other drivers. These spillovers are theoretically ambiguous, as traffic can either reallocate towards untolled roads and worsen their congestion or decrease in total volume and reduce congestion city-wide (Verhoef, Nijkamp and Rietveld, 1996; De Palma and Lindsey, 2000; Verhoef, 2002).[1] Despite their potential importance, empirical evidence on the spillovers of second-best congestion pricing is scarce, partly because the effects are diffuse and propagate across the road network, complicating identification.

We contribute new evidence using the introduction of congestion pricing in New York City (NYC), the first cordon-based congestion pricing program in the United States. Under the policy, passenger vehicles must pay $9 to enter NYC's central business district (CBD) between 5am-9pm on weekdays and 9a-9pm on weekends.[2] We measure the policy's impact using a generalized synthetic control (GSC) design (Xu, 2017) combined with data covering the metropolitan areas of NYC and five control cities. Our primary data include a panel of road segment and origin-destination level traffic statistics derived from anonymized and aggregated trips taken with Google Maps, which we complement with information on air quality from PurpleAir sensors, spending at shops and restaurants from a sample of credit/debit cards, and foot traffic from a sample of GPS devices. Using the GSC approach, we estimate the treatment effects of congestion on CBD outcomes, traffic conditions beyond the CBD, and overall travel times across the city. The granularity and scale of our data enable us to detect even small treatment effects—often less than a one percent change—throughout the NYC metro area.

We begin by evaluating the effects on the now-priced CBD itself, where we find that the introduction of congestion pricing led to an immediate and persistent increase in speeds. Raw average speeds in the CBD during peak hours increased from 7.1 miles per hour (mph) pre-policy to 8.2 mph in the six months following implementation. However, average speeds in most cities rose during the same period, and comparing raw average speeds conflates the treatment effect of the policy with concurrent trends in traffic conditions. Using the GSC approach, we estimate that the policy increased speeds in NYC's CBD by 11% relative to a synthetic control formed from other cities, suggesting that three-fourths of the observed change in speeds is attributable to the policy. The effects on speeds are larger during the afternoon (historically the most congested time) and persist even after peak-hour pricing ends. Introducing congestion pricing also reduced the dispersion in speeds across roads by compressing the tail of extreme delays, which has potentially important welfare implications given driver preferences for reliability (Small, Winston and Yan, 2005; Bento, Roth and Waxman, 2024).

Beyond road speeds, the policy had little-to-no effect on CBD air quality, visits to restaurants/shops,

[1] In a survey of economists before the policy's implementation, 90% of respondents agreed that congestion pricing in NYC would "lead to a substantial reduction in traffic congestion in the targeted area," but the majority were uncertain as to whether traffic would increase on roads just outside the CBD (Clark Center Forum, 2024).

[2] Prices are per-day, and are lower for motorcycles, higher for trucks, vans, and buses, and 75% lower during off-peak hours. For-hire vehicles pay a per-trip fee of $0.75 for taxis and $1.50 for ridesharing companies.

or overall foot traffic. Estimated fuel efficiency[3] improved by 2–3% for cars traveling in the CBD, but these improvements have yet to have a detectable treatment effect on ambient concentrations of fine particulates ($PM_{2.5}$). We also find no effect on the number of credit/debit card transactions at shops and restaurants in the CBD, although we do find some evidence of a small negative effect on the total amount spent. Overall foot traffic in the CBD—inferred using location data from GPS devices—has also remained steady, suggesting that any reduction in car trips to the CBD has been offset by substitution toward other travel modes.

To evaluate effects beyond the CBD, we first develop a segment-level measure of exposure to congestion pricing based on each segment's *co-occurrence* with the CBD. We define co-occurrence as the share of drivers crossing the focal segment who also cross segments in the CBD as part of their trip, and measure the co-occurrence of each segment using data on pre-period traversals. Many drivers not bound for the CBD will still traverse segments exposed to the policy, and, through this exposure to the now-priced CBD trips, may experience changes in speed after the policy's implementation. A road segment has a co-occurrence of 50% if half of the cars that typically traversed it prior to the policy were trips to or from the CBD. If the policy's main effect were to reduce CBD trips by 10% uniformly, a segment with 50% co-occurrence would see a proportional 5% decrease in cars.

We find that the policy increased speeds throughout the NYC metro area, with larger effects on roads with higher levels of co-occurrence with the CBD. For road segments with 80-100% co-occurrence—which include the Lincoln Tunnel, Holland Tunnel, and other main entries to the CBD—the policy's implementation increased speeds by an average of 15%. The effects then decline monotonically in the level of co-occurrence, but even speeds on the average road with 0-20% co-occurrence experienced a smaller 4% increase in speeds. Across road types, highways experienced larger increases in speeds—especially at lower levels of co-occurrence—while the effects on local and arterial roads were more limited. The smaller effect on roads within the CBD than on roads with 80-100% co-occurrence is partly due to the composition of road types. The CBD is comprised of local and arterial roads with auxiliary congestion from pedestrians and stopped vehicles (even at 3am, the average CBD speed is less than 15 mph), while roads leading into the CBD tend to be larger highways whose speeds respond more to changes in volumes.

The changes in road segment speeds add up to meaningful differences in travel times between different parts of the city. For trips to the CBD, the implementation of congestion pricing increased average trip speeds by about 9%. While drivers on trips to the CBD must now pay a toll for these time savings, passenger vehicle drivers on other trips within, from, and outside the CBD benefit from increased speeds without having to pay the new congestion fee. Speeds on trips within and from the CBD increased by 6–7%. Even trips that took place entirely outside the CBD experienced modest gains of 0.5–2%, depending on the distance of their origin and destination to the CBD. As we discuss shortly, the dispersed effects far from the priced zone—which, in many settings, might be difficult to detect—are first-order for evaluating the net welfare impacts of the policy.

Distributionally, the policy had similarly positive effects on speeds for Census tracts of different

[3] We infer fuel efficiency on road segments using an engineering model that transforms speed profiles and segment characteristics into an estimate of liters consumed per 100 kilometers traveled, similar to the approach in Brooker et al. (2015).

income levels and in specific areas, such as the Bronx, New Jersey, and Staten Island. We group Census tracts by the quintile of median household income and estimate the effects for each group of tracts separately. We find that introducing congestion pricing in NYC increased average speeds on segments within tracts across the household income distribution, with, if anything, larger effects for the lowest income tracts. Tracts in the bottom income quintile saw speed increases of 3.2% compared to increases of 1–2% in the higher income quintiles. Similarly, trips to the CBD originating in each income quintile have all seen speed gains of 7.5–9%. For New Jersey residents, the policy increased speeds on road segments within Hudson and Bergen counties by 4.7%, and increased speeds on trips to (from) the CBD by 10.5% (9.7%). For residents of the Bronx and Long Island, the policy increased speeds on road segments within each area by about 3% and increased speeds on trips to/from the CBD by 5–10%.

We bound the welfare gains for drivers traveling throughout the metro area using a simple model that weighs the time savings for drivers against the new congestion fee. We estimate that the 1.61 million passenger vehicles traveling to the CBD during peak hours in a typical week save an average of 3.1 minutes and pay an average of $7.9.[4] For these trips, the policy is only welfare-improving if the drivers' Value of Travel Time (VOTT) is over $153/hour. However, because of the spillovers in road speeds, many *unpriced* trips are also now faster. While we estimate that the average unpriced passenger vehicle trip saves just 11 seconds, there are far more unpriced trips than priced trips. Including unpriced trips, the 'break-even' VOTT of drivers for the policy is at most $21/hour. We repeat a similar exercise for taxi/FHV trips, which save similar amounts of time but at a lower per-trip fee. If all drivers and taxi/FHV passengers have a VOTT of $40/hour, then we estimate total weekly welfare gains—before any revenue recycling or environmental benefits—of at least $14.3 million, which come almost entirely from unpriced trips. The implied peak hour revenue is about $14 million per week,[5] and, based on the improved fuel economy on trips, we estimate a savings of about 653 tonnes of CO_2 each week, which is worth $120,900 if the social cost of carbon is $185 per tonne (Rennert et al., 2022).

Finally, we investigate the mechanisms behind the observed effects in NYC, and whether similar results are plausible in other congested cities. The impact of cordon-based congestion pricing depends on three underlying factors: 1) how pricing changes traffic volumes; 2) other drivers' exposure to those volume changes; and 3) the shape of the *congestion functions* that describe the relationship between traffic density and speed on different roads. Intuitively, if most CBD-bound trips begin just outside of the cordon area, or if the roads they traverse are already at free-flow speeds, then few other drivers will be affected by any change in the number of CBD trips. If, instead, CBD-bound trips originate from the outskirts of the metro area and travel on congested roads, then changes in CBD trip volumes will have larger spillovers on speeds for other drivers. While our data are ill-suited to speak to how demand would respond to price changes, we evaluate how the latter two ingredients contributed to

[4] The average price paid is lower than the advertised $9 toll due to crossing credits for entering the CBD through an already-tolled entrance, such as the Lincoln Tunnel.

[5] This is an approximation of the true operating revenue and does not account for other vehicle classes, low-income discounts, uncollected fares, and same-day repeat entries. The MTA reported CBD toll revenues of $159 million for January to March 2025, i.e. $13.3 million per week (MTA, 2025*b*).

the effectiveness of the policy in NYC and compare across our control cities to assess the potential for congestion pricing beyond NYC.

To measure the exposure of drivers to CBD trips, we start with our segment-level co-occurrence measures. For each origin-destination (OD) pair in each city, we calculate exposure as the average duration-weighted co-occurrence of segments traversed on trips to, from, and within the CBD. Trips to/from the CBD have average exposures of 50-65% in each city—i.e., over half of the other drivers on the road are also on CBD trips when weighted by duration—with the largest values in NYC and Chicago and the smallest values in Atlanta and Baltimore. The exposure for trips to/from the CBD are high in part because even trips that start or end far from the CBD may spend a disproportionate share of the travel time on the congested roads near the CBD itself. Trips outside the CBD are naturally less exposed on average ($< 10\%$ in all cities), but the values range from a low of about 3% in Philadelphia and NYC to over 7% in Chicago and Boston.

We then estimate congestion functions for groups of road segments using pre-policy data on speeds and corresponding density. We group road segments by their co-occurrence bin (i.e. 0-20%, 20-40%, ...) and road type (i.e., local, arterial, and highway) and estimate separate congestion functions for each set of roads in each city, using a functional form from the Bureau of Public Roads (BPR). We do not directly observe density, so, following Choudhury et al. (2024), we impute density based on the observed partial flows. Consistent with the traffic engineering literature, the estimated congestion functions are typically convex, especially on highways (Seo et al., 2017). As a result, the effects on speeds of a constant change in density depend on *where* along the congestion function pre-policy traffic operated. If the road typically operates near a steep part of its congestion function, removing even a few cars can substantially increase speed. For each city and trip type, we estimate the average local elasticity of the congestion function of road types traversed, evaluated at the pre-period density.

We find that roads in NYC were operating at more elastic parts of their congestion functions prior to the implementation of the policy than those in our five control cities. The gaps are often large: trips to, from, and outside the CBD in NYC traversed roads where the average local elasticity was about 70% higher than similar trips in Philadelphia. The closest city is Boston, where the average elasticities for roads along trips of each type were about 10% smaller than in NYC. In cities where the local elasticities are larger, the marginal effects of adding or removing drivers on speeds will also be larger. While NYC is middle of the road in terms of how exposed drivers are to CBD trips, small changes in the density of cars on the road will lead to larger increases in speeds in NYC than in any other city. Across cities, introducing cordon-based congestion pricing may be the least effective in Philadelphia (where average exposure and congestion function elasticities are generally smaller than in NYC), and most effective in Chicago (where the average exposure is higher than in NYC and the congestion function elasticities are only modestly smaller).

Our work most directly contributes to studies of cordon-based congestion pricing programs, including early evaluations of the New York City policy and a larger body of work on older policies in London, Stockholm, Milan, and elsewhere.[6] Since its launch, year-to-year comparisons in speeds

[6] Other studies conduct ex-ante simulations of potential congestion pricing policies in cities without any existing policy. See, for example, Almagro et al. (2024); Barwick et al. (2024); Durrmeyer and Martínez (2024); Hierons (2024); Ater et al. (2025); Montero, Sepúlveda and Basso (2025).

and travel times in NYC's CBD have been reported by journalists (Gordon et al., 2025; Ley, Hu and Collins, 2025), the Congestion Pricing Tracker website (Moshes and Moshes, 2025), and the Metropolitan Transportation Authority (MTA, 2025*a*).[7] Perhaps due to having access to limited data covering subsets of NYC roads, however, these analyses occasionally disagree on whether the program decreased or increased travel times (Hu, Ley and Schweber, 2025). In contrast, we evaluate the causal effects of congestion pricing using a consistent econometric strategy and large-scale data covering roads throughout the metro areas of NYC and a set of comparison cities. Outside of NYC, past research found that the 2003 implementation of a congestion charge in downtown London led to reduced traffic, lower pollution, and fewer accidents (Leape, 2006; Green, Heywood and Paniagua, 2020; Tang and van Ommeren, 2022). Similar effects on downtown traffic have been documented for congestion pricing in Stockholm (Eliasson et al., 2009; Simeonova et al., 2021) and Milan (Gibson and Carnovale, 2015).[8]

Evidence on the spillovers of congestion pricing is more mixed. Early theoretical work focused on the welfare effects of first-best pricing in a world with just a single road, setting aside any spillovers (Vickrey, 1963, 1969). Later, studies of second-best congestion pricing—where only a subset of roads are priced—incorporated the effects of spillovers, but were primarily focused on the *negative* impact on speeds of roads that were substitutes for a tolled road (Verhoef, Nijkamp and Rietveld, 1996; De Palma and Lindsey, 2000).[9] Empirically, Gibson and Carnovale (2015) finds that congestion pricing increased traffic on radial roads that were substitutes to traveling through downtown Milan, while Herzog (2024) finds that London's policy reduced traffic on certain untolled roads leading into the downtown area. For these two papers, which study programs introduced before the prevalence of Google Maps or other large data sources, the authors are limited to a handful of speed observations from only the focal city. Our more detailed data enable us to detect even small spillover effects, a level of precision that is often unavailable for analyses of many place-based policies (Neumark and Simpson, 2015).[10] We find that, in our setting, accounting for the small, diffuse spillovers is critical for measuring net welfare changes, and excluding them flips the sign of aggregate driver welfare.

2 Congestion Pricing in NYC

New York City's congestion pricing policy, officially known as the Central Business District Tolling Program, was implemented on January 5th, 2025. The initiative imposes a fee on vehicles entering Manhattan south of 60th Street, excluding the FDR Drive and West Side Highway. The tolls are

[7]Even before the policy's implementation, MTA (2022) and Hierons (2024) evaluate the policy's potential impacts on congestion, air pollution, and mode choice.

[8]In lieu of congestion pricing, some cities impose driving restrictions and car-free zones, which can reduce downtown congestion and pollution but are inefficiently targeted and do not raise any revenue (Davis, 2008; Gallego, Montero and Salas, 2013; Yang, Purevjav and Li, 2020; Galdon-Sanchez et al., 2022; Sleiman, 2024).

[9]The classic example is a tolled highway running parallel to a free highway. In the US, many roads now have 'express lanes' that are dynamically priced in response to current traffic conditions. These lanes offer drivers the option to pay for faster speeds when needed, but can increase congestion in the remaining untolled lanes (Hall, 2018; Bento, Roth and Waxman, 2024; Cook and Li, 2024).

[10]Others, such as Busso, Gregory and Kline (2013), show that focusing on the focal region is a first-order approximation to the aggregate welfare effects of a place-based policy if behavioral adjustments enter as second-order terms. However, this argument requires that prices (or, for our setting, speeds) remain unchanged beyond the focal region. Otherwise, those price changes are first-order for the welfare of residents in the non-focal regions.

collected electronically and vary based on the time of day, vehicle type, and whether the vehicle is equipped with an E-ZPass transponder. Since the policy's launch, nearly 248,000 passenger cars and 143,000 taxis/FHVs have entered the CBD on the average weekday during peak hours.[11] The Metropolitan Transportation Authority (MTA) plans to use the toll revenue to fund repairs and enhancements to the city's subway, bus, and commuter rail systems.

Toll rates are set at $9 per day for passenger cars and small commercial vehicles if paid by E-ZPass. Motorcycles pay $4.50 per day, while trucks and buses pay between $14.40 and $21.60 per day, depending on their size. These rates are reduced by 75% overnight, and are up to 50% higher if drivers do not have E-ZPass and instead pay by mail. Taxis and ridesharing vehicles pay a per trip rate of $0.75 for taxis and $1.50 for ridesharing vehicles for trips that start, end, or pass through the CBD.[12] There are a few exempted vehicles (e.g., emergency vehicles), and vehicles entering via certain bridges or tunnels that are already tolled receive a partial credit. Low-income residents can also apply to receive 50% off after their first ten trips in a month. The passenger car rate is set to increase to $12 in 2028 and $15 in 2031.

The concept of congestion pricing in New York City has a history spanning several decades. In the early 1970s, Mayor John Lindsay proposed tolling East River crossings to mitigate traffic congestion in Lower Manhattan, but the plan was eventually withdrawn after facing stiff opposition from the trucking industry, taxi drivers, and local businesses concerned about the financial impact of tolls. Subsequent mayors attempted to implement similar measures. In 2007, Mayor Michael Bloomberg's administration proposed the PlaNYC initiative, which aimed to introduce congestion pricing to promote sustainability and fund transportation infrastructure. While this proposal stalled in the state assembly, it resurfaced in 2017 when Governor Andrew Cuomo advocated for congestion pricing to raise funds for improving transit. This led to its eventual inclusion in the 2019 state budget and, after some further delays, its implementation in 2025.

The policy faced criticism and legal challenges during its development. Critics argue that this approach imposes an additional financial burden on residents, may have adverse economic impacts on business, and will shift traffic and pollution to other parts of the city (Ley, 2022). At least ten lawsuits were filed against the MTA and state officials by business coalitions, elected officials from New Jersey, and others (Hu and Ley, 2024).

3 Data & Empirical Strategy

We build data covering traffic conditions, air quality, consumer spending, and foot traffic for New York City and five other cities, which we use to form synthetic controls. The five control cities are Philadelphia, Boston, Chicago, Atlanta, and Baltimore. For each city, we define its boundaries

[11]This statistic is computed based on the publicly-available "MTA Congestion Relief Zone Vehicle Entries" dataset (source) for weekday peak hour entries by "Cars, Pickups and Vans" or "TLC Taxi/FHV" between January 5th and June 30th, 2025. These two categories account for about 85% of all entries. See Table B.1 for counts by other vehicle types.

[12]Ostrovsky and Yang (2024) evaluate the pricing by vehicle type and argue that the small per-trip charge on taxis and ridesharing companies is too low, as a single trip by a taxi likely contributes as much congestion as a trip by a private vehicle.

according to the corresponding Core Based Statistical Area (CBSA) and define its CBD using the most prominent version of a CBD or 'downtown' drawn by a city government-affiliated organization.[13] In this section, we provide an overview of each data source; Appendix A contains additional details.

3.1 Google Maps Traffic Trends

Our primary data on traffic conditions are anonymized and aggregated statistics from trips taken with Google Maps during the period of the study. The Google Maps Traffic Trends data include two primary sets of statistics: 1) hourly road segment-level outcomes, which we then further aggregate in time and space across sets of road segments; and 2) hourly origin-destination (OD) level outcomes, which are aggregated across trips based on OD Census tract characteristics. The sample covers traffic conditions from September 2024 through June 2025. Except where otherwise noted, all analyses focus on data during priced hours (5am – 9pm on weekdays and 9am – 9pm on weekends).

Segment-level outcomes. For all segment-level analyses, we group segments by either geographic location, such as Census tracts, or by shared characteristics, such as co-occurrence with the CBD. Road segments vary in length, with an average of approximately 50 meters. For each segment-level outcome, we consider aggregates composed of the harmonic mean weighted by traversal distance. To maintain a stable composition of road segments within a group, we further weight each segment by its average share of traversals within hour t between September and December 2024. That is, for a given outcome y measured across segment group j in hour t, we consider the distance-weighted average outcome:

$$\bar{y}_{j,t} = \frac{\sum_{s \in S_j} \sigma_{s,t} \times d_{s,t}}{\sum_{s \in S_j} \sigma_{s,t} \left(d_{s,t} / y_{s,t} \right)} \tag{1}$$

where S_j is the set of traversals through segments s in segment group j, $\sigma_{s,t}$ is the pre-policy segment weight, and $d_{s,t}$ is the traversal distance (equivalent to the length of each segment). We consider three types of outcomes: traversal speed, normalized traversal speed relative to the segment's speed limit, and the estimated fuel consumption rate. For fuel consumption, we estimate liters of gasoline per 100 kilometers consumed by a 'typical sedan' on a road segment based on the segment characteristics and speed profile, similar to the approach in Brooker et al. (2015). Appendix A.2 provides additional details on how we model fuel consumption.

Origin-destination (OD) level outcomes. Our second set of data covers full trips throughout the metro area, rather than individual road segments. Because of the more granular definition of these data, we aggregate trips to Census PUMAs rather than Census Tracts and to two-hour bins. We define origins and destinations based on groups of Census PUMAs, and whether the trip starts and/or ends within the CBD. For each OD pair, we measure the realized travel times and trip speeds (e.g. travel time divided by trip length), aggregated across all trips within a given OD-hour bin. To

[13] In NYC, the congestion pricing cordon area aligns with the NYC CBD boundaries defined by the City of New York before it released any plans for a lower-Manhattan congestion pricing policy (NYC Department of City Planning, 2011). Appendix A.1 documents the official sources we use to define the CBD shapes in control cities.

maintain a stable composition of trips between different PUMAs within a group, we weight each trip by the average trip volume in the same OD-hour bin between September and December 2024.

3.2 Other Outcomes

Air quality. We use air quality data from PurpleAir to estimate the effects of the policy on pollutant levels in New York City. PurpleAir is a company that sells $PM_{2.5}$ sensors as a consumer product. Its customers can opt to share the data their sensors gather with the public, and the many "community scientists" form a network of $PM_{2.5}$ monitoring with large coverage. We focus on the effects on $PM_{2.5}$ over other pollutants as this pollutant accounts for most of the adverse health effects of air pollution in the U.S. (Tschofen, Azevedo and Muller, 2019; National Institute of Environmental Health Sciences, 2024). For each city, we query the data of all outdoor PurpleAir sensors in the corresponding CBSA. The data include 22 unique outdoor PurpleAir sensors in the NYC CBD and 1,067 sensors throughout NYC and our five control cities. We calibrate the PurpleAir $PM_{2.5}$ data as recommended by the Environmental Protection Agency (EPA) (Barkjohn et al., 2022); we describe this process in Appendix Section A.3.

Consumer spending. To measure consumer spending, we use proprietary credit and debit card transaction data compiled by MBHS3 and provided through Yale University. The underlying data contain approximately 35 billion annual transactions by 180 million individuals. We receive data aggregated at the level of zipcode, day, and 3-digit North American Industry Classification System (NAICS) code for transactions between January 2024 and April 2025. We focus on transactions at restaurants or retail establishments, which we identify using their 3-digit NAICS code. For retail establishments, we restrict attention to five NAICS categories that had over a million observed transactions in the NYC CBD during 2024.[14] In NYC, the data include 165 million restaurant transactions ($4.6 billion total spend) and 286 million retail transactions ($13.2 billion total spend) during the sample period (Appendix A.4). The boundaries of NYC zipcodes almost exactly align with the CBD boundaries (Figure C.3), so we define a zipcode as treated by the policy if its centroid lies within NYC CBD.

Foot traffic. We use Advan Research's Neighborhood Patterns to measure the amount of foot traffic in the CBD over time.[15] The underlying data are passively collected from mobile applications on GPS-enabled devices (e.g., smartphones) that record a device's coordinates and timestamps whenever connected to the GPS. These data, which cover approximately 7% of the overall population (Li et al., 2024), are then stripped of personal identifiers and aggregated for research purposes. The aggregate data we receive include the number of visits to each Census tract in each hour. We further aggregate to the tract-day level by summing the number of visits to a tract during peak hours. We use data from January 2024 through June 2025, and define a tract as treated by the policy if its centroid lies within the NYC CBD (see Figure C.3 for a map).

[14] The categories are Food & Beverage Stores, Health & Personal Care Stores, Clothing & Clothing Accessories Stores, General Merchandise Stores, and Miscellaneous Store Retailers.

[15] The data was previously distributed under the name SafeGraph Patterns.

3.3 Empirical Strategy: Generalized Synthetic Controls (GSC)

Assessing the effects of New York City's congestion pricing policy is complicated by numerous confounding factors. A straightforward comparison of average outcomes before and after the policy combines the policy's effects with any other time-varying factors influencing traffic conditions. Existing evidence generally compares average traffic patterns in NYC after implementation to earlier months or to data from the prior year. Interpreting the results from such approaches as causal requires an assumption that, absent the policy, NYC traffic conditions after January 5th would have been similar to earlier periods. We introduce a method that instead compares changes in traffic conditions in NYC to contemporaneous changes in a set of comparison cities.

Specifically, we adopt the generalized synthetic control (GSC) methodology introduced by Xu (2017). The strategy relies on comparing changes in NYC before and after the policy to changes in outcomes observed in other cities, which are combined into a "synthetic control" for NYC using pre-policy data to estimate weights. The GSC method extends the classical synthetic control approach (Abadie, Diamond and Hainmueller, 2010) by incorporating interactive fixed effects, such that unobserved confounders may vary both across units (e.g., cities, Census tracts) and over time. This approach identifies the causal effect of the policy under a weaker assumption that, absent the policy, NYC's outcomes would have tracked the estimated 'counterfactual' outcomes, which are constructed from contemporaneous data in control cities weighted to match pre-policy patterns.

More formally, let $Y_{it}(0)$ denote the potential outcome for unit i at time t if untreated and $Y_{it}(1)$ denote the potential outcome if treated. We only observe

$$Y_{it} = D_{it}Y_{it}(1) + (1 - D_{it})Y_{it}(0)$$

where D_{it} is an indicator that takes the value 1 if unit i is treated at time t, and 0 otherwise. In our application, $D_{it} = 1$ for NYC after implementing the congestion pricing policy, and $D_{it} = 0$ otherwise. The set of control cities remains untreated throughout the sample period, allowing them to serve as comparisons.

Following the GSC framework, we assume that the untreated potential outcome $Y_{it}(0)$ can be decomposed into a low-rank factor structure, plus some idiosyncratic noise:

$$Y_{it}(0) = \alpha_i + \gamma_t + \boldsymbol{\lambda}_i^\top \mathbf{f}_t + \varepsilon_{it} \tag{2}$$

where α_i is a unit fixed effect, γ_t is a time fixed effect, $\boldsymbol{\lambda}_i$ is a vector of factor loadings specific to unit i, $\mathbf{f}_t$ is a vector of common factors, and ε_{it} is an idiosyncratic error term. The functional form can be extended to include time-varying unit characteristics, although we do not use such characteristics in our setting.

Estimation. We first estimate these factors and loadings from the panel data of outcomes observed in the pre-treatment period. The factors and loadings are estimated separately for each outcome variable. Intuitively, this step aims to identify the factors and loadings that best predict changes in the outcome variable over time in the soon-to-be-treated units. Using the estimated parameters, we can then predict *counterfactual* outcomes $\hat{Y}_{it}(0) = \hat{\alpha}_i + \hat{\gamma}_t + \hat{\boldsymbol{\lambda}}_i^\top \hat{\mathbf{f}}_t$ for each post-treatment period $t > T_0$, where T_0 is the time period that congestion pricing began in NYC.

The Average Treatment on the Treated (ATT) effect in period $t > T_0$ is given by:

$$\widehat{\text{ATT}}_t = \frac{1}{|\mathcal{I}|} \sum_{i \in \mathcal{I}} Y_{it} - \hat{Y}_{it}(0) \tag{3}$$

where $\mathcal{I}$ is the set of treated units. We will often report the aggregate ATT, too, which is estimated as $\widehat{ATT} = \frac{1}{T-T_0} \sum_{t>T_0} \widehat{\text{ATT}}_t$. We implement this procedure using the *gsynth* package for R provided by Xu (2017). We use cross-validation to select the dimensionality of the factors and, for inference, we use parametric standard errors.

Identification. The key assumption of the GSC approach is that the error term ε_{it} is independent of treatment assignment, time and unit fixed effects, and the unobserved cross-unit ($\boldsymbol{\lambda}_i$) and cross-time ($\mathbf{f}_t$) sources of heterogeneity. This assumption has three important implications. First, the factor loadings $\boldsymbol{\lambda}_i$ must be stable over time. If a structural break changes the fundamental relationship between cities, this stability may be violated, leading to poor counterfactual predictions during the treated period. Second, much like traditional differences-in-differences (DiD), the GSC requires that the exact timing of treatment is exogenous after conditioning on the factor structure. Finally, this assumption is violated if there are spillover effects from NYC to control cities (e.g., large-scale traffic diversions or copy-cat policy changes). This assumption is generally weaker than the standard DiD assumption that, in the absence of treatment, treated and control groups would follow parallel trends. Instead, GSC allows for multiple time-varying confounders that affect units differently. This is particularly valuable in contexts like urban traffic, where many latent factors (e.g., seasonal weather patterns) may affect different cities in varying ways.

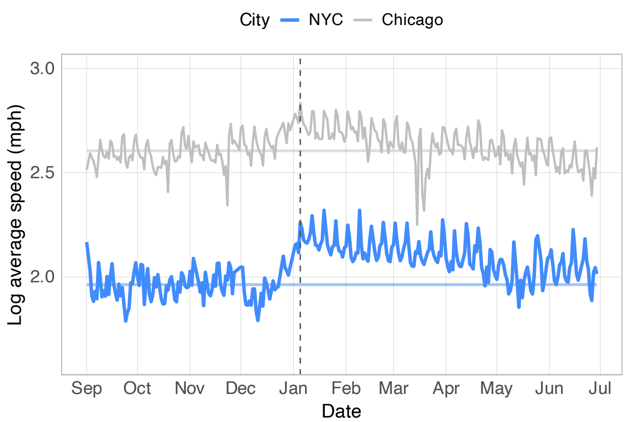
4 Direct Effects in the CBD

According to the MTA (MTA, 2025*c*), vehicle entries into the CBD decreased by approximately 10% following the introduction of the congestion pricing policy and have held steady (after accounting for seasonal fluctuations) through June, 2025. In this section, we examine the effects of congestion pricing on road conditions, air quality, consumer spending, and foot traffic in the NYC CBD. We find that the policy increased average speeds on CBD roads by 11% and improved estimated fuel efficiency by 3%. The largest effects were on the slowest roads and at the most congested times of day. Beyond traffic conditions, however, we find no evidence of impact on ambient air quality, visits to restaurants and retail establishments, or overall foot traffic.

4.1 Road Speeds within the CBD

The average speed on road segments in the NYC CBD increased from 7.1 mph in the four months preceding the policy's implementation to 8.2 mph in the six months after implementation (Figure 1). However, the speed increase before and after the policy's implementation is not unique to NYC. In fact, average speeds in the CBDs of Boston, Philadelphia, Atlanta, and Chicago were also higher after January 5th than in the preceding months (Table C.1), although the increase in NYC is the largest relative to pre-period speeds. Figure 1 shows head-to-head comparisons of CBD road speeds in NYC, Chicago, and Philadelphia.[16] In NYC, speeds increased sharply around the start of January 2025, and have remained above their pre-period average. Speeds in Chicago and Philadelphia also increased at the start of year, but have since fallen to their pre-period average in Philadelphia and below their pre-period average in Chicago.

Figure 1: Average speeds on CBD road segments



Notes: This figure documents the traversals-weighted average daily speed on road segments in the CBD of NYC, Philadelphia, and Chicago from September 2024 through June 2025. The horizontal blue line represents the average speed in NYC between September 1 and December 15, 2024.

To what extent is the observed increase in speeds in NYC's CBD attributable to the effects of the congestion pricing policy? To evaluate this, we compare speeds in the NYC CBD to speeds in the CBDs of other cities by estimating Equation (3) using a panel of average speeds. Figure 2a plots the day-level ATT on log average speeds for CBD road segments during priced hours.[17] Prior to the policy's launch, speeds in NYC were similar to the counterfactual speeds constructed from the set of comparison cities, suggesting that the GSC approach accurately captures trends across cities. After the onset of congestion pricing, average speeds in the CBD increased sharply by 10–15% relative to the synthetic control. The increase persisted through the first six months, with some reversion in May before rebounding in June. Averaging over this period, the implementation of congestion pricing increased speeds by 11%. This suggests that, under the assumptions of the GSC estimator, the policy's causal impact accounts for about three-fourths of the change in raw average speeds on CBD segments.

These average effects aggregate over both extreme stop-and-go traffic and near-free-flow traffic. Most roads in the CBD are arterial roads (e.g., 5th Avenue) or substantially narrower local roads, both of which are generally slower (and with lower speed limits) than the handful of high-volume highway segments included in the CBD area. The average speed on arterial roads prior to the policy was 7.1 mph, while on local roads it was only 5.5 mph (Table C.1). The 10th percentile of pre-policy speeds was under 3 mph for both local and arterial roads and the 90th percentile was about 20 mph

[16]See Figure C.1 for average speeds over time in our other control cities.

[17]Figure C.2 includes similar plots for other measures of CBD traffic conditions, including median speeds.

for local roads and 25 mph for arterials. The introduction of congestion pricing disproportionately impacted the slowest traffic conditions. The 10th percentile of road speeds experienced across the CBD increased by 13.8% on average, and the 90th percentile of road speeds increased by 2.8% on average. Therefore, one effect of the policy was to compress the tail of extreme delays, an outcome with potentially important welfare implications for reliability (Small, Winston and Yan, 2005). Measured directly, the dispersion of road speeds in the CBD, as captured by the ratio of 90th to 10th percentile speeds, decreased by 13.9%, and, by the ratio of 80th to 20th percentile speeds, decreased by 9%

As a complementary exercise, we consider the impact of the policy at the level of trips that start and end inside the CBD (first line of Figure 2a). This measure differs from the segment-level results above by capturing the end-to-end experience of travelers, aggregating over multiple links, intersections, and delays. For trips entirely within the CBD, we estimate that congestion pricing increased the average speed by about 5.3% since implementation, somewhat smaller than the segment-level estimates. However, this focuses only on trips that take place entirely within the CBD, while the segment-level results account for road conditions experienced by all drivers passing through the CBD regardless of origin or destination. In Section 5.3, we extend this exercise and discuss how different types of trips throughout the NYC metropolitan area were affected.

The average effects also aggregate over substantial heterogeneity across times of day. Before the policy's implementation, average speeds in the CBD ranged from 14 mph in the early morning to half that in the late afternoon (Figure 2c). In Figure 2d we present ATTs on log CBD road speeds for each two-hour time window between 3am and 11pm. This breakdown of the policy's effects shows that the increases to average road speeds were modest in the early morning (3-–5%), but rose to 15% in the weekday afternoon peak and above 20% on weekend afternoons. That is, the relative impact of congestion pricing on speeds was largest during the times of day when congestion was most severe prior to the policy. Moreover, the increases in road speeds persist after the end of peak-hour pricing at 9pm, suggesting that the decreased entry volumes throughout peak hours had spillover effects to road conditions later in the evening.

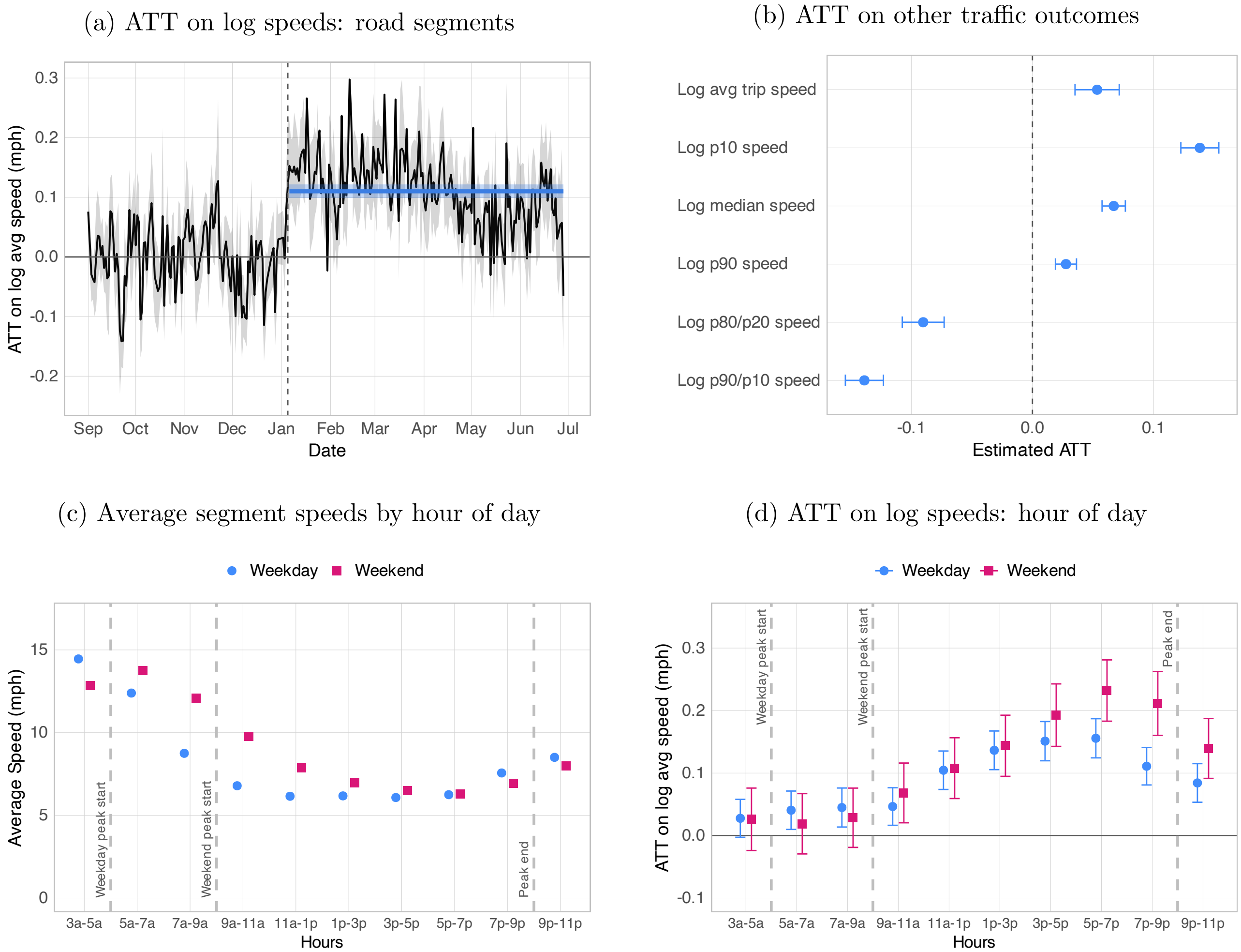
Taken together, these results reinforce the canonical prediction that congestion pricing raises speeds on priced roads (Vickrey, 1969), and provide new evidence on the magnitude, persistence, and distribution of effects across times and road types.

4.2 Fuel Consumption and Air Quality

Changes in traffic patterns also affect the quantity and location of vehicle emissions. CO_2 emissions impose externalities worldwide, while emissions of local air pollutants such as NOx, CO, and particulates impose an externality on nearby residents. Exposure to vehicle emissions specifically has been found to have negative effects on infant (Currie and Walker, 2011; Knittel, Miller and Sanders, 2016) and adult (Buckeridge et al., 2002) health outcomes.

While we cannot observe vehicle emissions directly, we use predicted fuel consumption rates as a proxy for measuring how the change in traffic patterns induced by congestion pricing affected potential CO_2 emission rates. For a given vehicle type, the primary drivers of fuel consumption are vehicle speed profiles and the segment type. On a typical road segment, fuel consumption is most efficient at speeds

Figure 2: Effects on Speeds in the CBD



Notes: Panel a) documents day-level ATTs of congestion pricing over time on peak hour CBD segment speeds. The data on average segment/trip speeds by two-hour bin in each CBD. The horizontal blue line is the aggregate ATT for all post-treatment periods. Panel b) documents the ATTs on other traffic outcomes in the CBD, including median speeds and speeds on different types of roads. Panel c) documents the average pre-period speeds by hour bin and weekday vs weekend. Panel d) estimates the treatment effect on log average speeds for each hour bin separately, using data from all other cities and hour bins as potential controls. Shading and vertical bars denote 95% confidence intervals. Standard errors are clustered at the city-level.

between 60-80 mph, and is especially inefficient at speeds below 10 mph. Cars traveling at 5 mph—close to the average speed in the CBD during the late-afternoon—will consume about four times as much fuel per mile as those traveling at 60 mph (Figure A.1). Relative to the control cities, we find that the change in vehicle speeds and segments traveled translates into a 3.4% decrease in the estimated fuel consumption of vehicles traversing CBD segments (Figure 3). This estimate corresponds to the consumption *rate* for the average vehicle in the CBD, not the total fuel consumption across all vehicles. The effect on total fuel consumption—and the corresponding emissions—would be larger if they accounted for any decreases in the total vehicle miles traveled in the CBD.

Despite the improved estimated fuel economy, we find no evidence of an effect on ambient concentrations of fine particulates measured by PurpleAir sensors. Using sensor-day level data, we compare the $PM_{2.5}$ for PurpleAir sensors inside the NYC CBD to synthetic controls formed from sensors in other cities. The estimated treatment effect on particulate matter is statistically indistinguishable from zero (Figure 3). While the estimates are noisier than our fuel consumption estimates, we can reject changes of similar magnitude to those found following the implementation in London (Green, Heywood and Paniagua, 2020) and Stockholm (Simeonova et al., 2021). The bottom end of the 95% confidence interval is -0.25$\mu g/m^3$, which would be a 3.3% decrease in the pre-period daily average $PM_{2.5}$ for sensors in NYC's CBD. One plausible explanation is that the London and Stockhom congestion charges—which began in 2003 and 2006, respectively—occurred when emissions standards were more relaxed and before the proliferation of electric vehicles, so the marginal vehicle deterred by congestion pricing in NYC may be quite different than those deterred in London and Stockholm. For example, the Euro 3 emissions standards (2000–2005) allow for over seven times more NOx per kilometer than the Tier 3 US emissions standards (2017–) allow of NOx and NMOG combined (0.15 g/km vs. 0.018 g/km). The results for London and Stockholm also consider a longer time horizon and may capture changes such as a shift towards green vehicles, which are exempt from the Stockholm fee (Nilsson, Tarduno and Tebbe, 2024) but face the same price as gas-powered vehicles in NYC.

4.3 Commerce and Foot Traffic

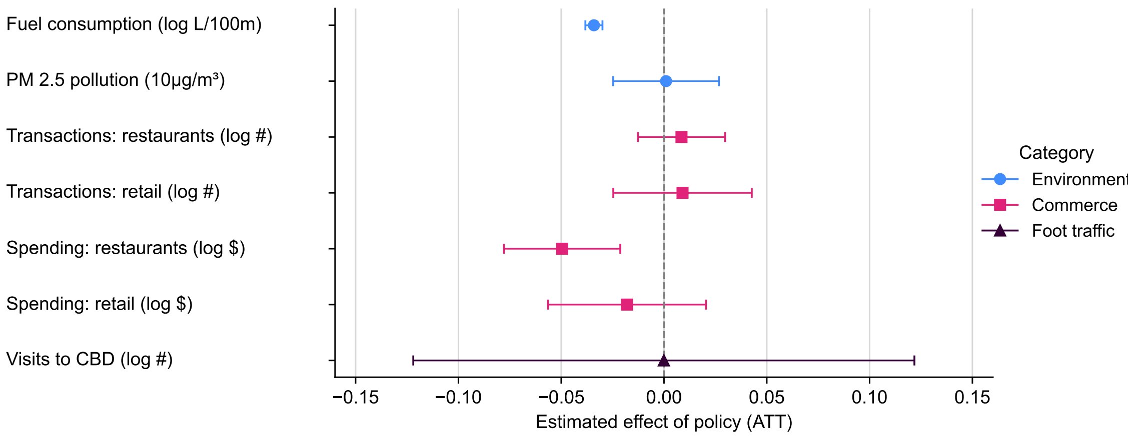
The reported decrease in vehicle entries to the CBD raises the question of whether individual *visits* to the area decreased as well. Lower vehicle volumes may indicate fewer visits if drivers substituted toward driving to different locations or staying home, but not if drivers substituted toward carpooling or traveling by transit, walking, or other modes. To examine this, we apply our synthetic controls approach to estimate the treatment effects of the congestion pricing policy on visits to restaurants and retail establishments, as well as on overall foot traffic.

First, using the aggregated data from credit/debit card spending, we compare the number of transactions and the total amount spent at restaurant and retail establishments in the NYC CBD to those in our control cities. We estimate Equation (3) using zipcode-day level data and weight observations by the number of transactions in a given zipcode-category during 2024. The data capture all transactions within a day, without distinguishing peak from off-peak hours.

We find no evidence that congestion pricing reduced the number of transactions at either restaurants or retail establishments in the NYC CBD (Figure 3). Point estimates are close to zero, and the confidence intervals rule out decreases of over 1.3% at retail establishments and 2.5% at restaurants. However, we find that the total dollars spent decreased by 5% at restaurants and 2% at retail establishments, although the latter is not statistically different from zero. With transaction counts unchanged, this suggests that either customers are spending less per visit or establishments are lowering prices relative to control cities.

We also examine impacts on foot traffic in the CBD, again estimating Equation (3) with tract-day counts of CBD visitors during peak hours and weights corresponding to pre-policy total foot traffic. Here, too, we find no clear effect: the number of CBD visitors in the GPS data remains stable relative to our control cities, implying that any reduction in car trips has been offset by substitution toward other modes. This distinction is important. Speeds in the NYC CBD rise after the policy, consistent with fewer vehicles entering the cordon, yet restaurant/retail transactions and foot traffic remain steady. Together, the results suggest that travelers primarily adjusted along the intensive margin of mode choice rather than the extensive margin of deciding whether to visit the CBD at all. However, while the point estimate for foot traffic is zero to four decimal places, the confidence interval remains wide enough to admit meaningful changes (Figure 3).

Figure 3: Effects on Other Outcomes in the CBD



Notes: This figure documents ATTs on a range of outcomes for the CBD. Average fuel consumption is estimated by Google's internal model, and we use the same specification as in Figure 2a to compute the ATT. For pollution, we use day-sensor level measures of ambient air quality (PM2.5) from PurpleAir from sensors in the NYC CBD and in our control cities. For spending, we use day-merchant zipcode level data from MBHS3 on aggregate spending from a sample of credit and debit cards. We categorize retail and restaurant businesses based on their 3-digit NAICS code. For foot traffic, we use hour-tract level data from Advan on the number of visits by GPS-enabled devices, which we subset to peak hours and then aggregate to the day level. Horizontal lines denote 95% confidence intervals. Standard errors are clustered by city.

5 Spillovers beyond the CBD

Cities are interconnected, and policies that affect one area invariably affect the entire city. A cordon toll directly prices only trips crossing into the cordon zone, but its impact on welfare depends as much on how traffic reallocates across the untolled parts of the network as on the priced zone itself (Verhoef, Nijkamp and Rietveld, 1996). If traffic diverts onto unpriced links, congestion may simply be displaced to other parts of the city. If, instead, overall volumes fall, traffic conditions may improve more broadly. Despite the importance of these spillovers, most prior evaluations of congestion pricing have primarily focused on traffic within the cordon zone only (e.g., Leape, 2006; Eliasson et al., 2009), where the effect sizes are often large enough to detect even with more limited data. In this section, we show that congestion pricing in NYC increased speeds on segments outside the CBD that had a high level of "co-occurrence" with CBD trips, resulting in lower observed travel times on trips throughout the NYC metropolitan area.

5.1 Road Speeds outside the CBD

To quantify the effects of spillovers to roads outside the CBD, we first develop a measure of the extent to which each road segment is exposed to the policy. The intuition behind our measure is simple:

segments that are frequently traversed as part of CBD trips are more exposed to reductions in the number of CBD trips than segments that are rarely part of such trips.

We formalize this idea with the following measure of *co-occurrence* between each segment and the CBD of its metropolitan area. Let S_{CBD} be the set of segments within a CBD, and let R be the set of observed trips in the relevant metropolitan area within a given time span. Each trip $R_i = \{s_1, \ldots, s_N\}$ consists of a set of segments $s_j,\ j = 1, \ldots, N$ traversed between its origin and destination. For each segment s, we define its co-occurrence with the CBD C_s as:

$$C_s = \frac{|\{R_i \in R \mid s \in R_i \wedge S_{\text{CBD}} \cap R_i \neq \phi\}|}{|\{R_i \in R \mid s \in R_i\}|}. \tag{4}$$

In other words, C_s is the fraction of trips that passed through segment s, that also passed through at least one of the CBD segments during the period of observation. We compute the co-occurrence of each segment in NYC and each of our control cities with respect to its relevant CBD using data from September to November 2024, and hold these values constant throughout our analysis. Figure C.4 plots the spatial distribution of road segments in NYC by their level of co-occurrence. As we would expect, roads closer to the CBD and highways leading in the direction of the CBD tend to have higher levels of co-occurrence.

Our definition of co-occurrence provides a natural benchmark for evaluating spillover effects. If there were no diversion of driving to nearby areas and decreases in entries were proportional to pre-policy volumes, then co-occurrence would predict the policy's impact on volumes for roads outside the CBD. For instance, if a road segment had 50% co-occurrence with the CBD and the congestion pricing policy reduced CBD entries by 10%, then we would expect that road segment to have 5% fewer vehicle traversals after congestion pricing was implemented. If speeds increased proportionally with reductions in volume, then we would expect to see a 5% increase in speeds. We do not directly observe volumes in our data, and, as we discuss in Section 7, the relationship between speeds and volumes is generally convex. However, a comparison of the treatment effects on segments with different co-occurrence levels provides several insights about the network-wide effects of congestion pricing in NYC.

Figure 4 plots average ATTs over the first six months of congestion pricing, split by co-occurrence quintile and road type (i.e., arterial, highway, and local roads, or a traversals-weighted average of all three). In each case, we estimate the treatment effects using the approach outlined in Section 3.3, where we form a synthetic control by combining segments of the same co-occurrence quintile and road type from other cities. Across all road types, the average non-CBD road segment with an 80-100% co-occurrence—that is, a segment for which 80-100% of traversals pertained to trips going to or from the CBD prior to the policy—experienced a 13.5% increase in speeds. The effects then decrease monotonically with co-occurrence, but even road segments with 0-20% co-occurrence experienced a 1.7% increase in speeds.

The estimated treatment effect on roads with 80-100% co-occurrence is larger than the estimated effect on speeds within the CBD in both relative and absolute terms. A breakdown of treatment effects by road type illustrates that this is primarily driven by the large share of highway segments in the 80-100% co-occurrence bin than in the CBD. The ATT for the highways in the highest co-occurrence bin, which include many of the major entrances to the CBD (e.g., the Lincoln and Holland Tunnels), was over 15%.[18] As the average pre-policy speed on highways with 80-100% co-occurrence was 21 mph, this effect roughly translates to a 3 mph increase in average speeds on the highway segments themselves and a 2 mph increase in average speeds on all roads with 80-100% co-occurrence overall. By contrast, the implied average increase on road speeds within the CBD, for which speeds started from a much lower baseline, was only about 0.7 mph.

At all levels of co-occurrence, the effects on speeds tended to be larger for highways and arterial roads than on local roads. Even highways and arterial roads that had only 0-20% co-occurrence with the CBD experienced a 0.8–1.5% average increase in speeds. Highly exposed local roads, such as the roads with 80-100% co-occurrence just above the northern border of the CBD at 60th St., experienced a similar increase in speeds to the local roads inside the CBD. Farther away, local roads were generally less affected than highways and arterials; however, the data is sufficiently rich to detect even a 0.5% increase in average road speeds on local roads in the 0-20% co-occurrence bin.

Overall, the evidence suggests broad improvements in traffic conditions outside the CBD, with the largest gains on the main approaches into and out of the CBD and significant effects extending across much of the network. We find no evidence of offsetting slowdowns on different road types or co-occurrence levels, suggesting that the policy reduced overall traffic volumes rather than simply displacing congestion to untolled roads. However, aggregating by co-occurrence may mask variation across neighborhoods, and concerns remain that some areas could face increased traffic. As we discuss in Section 5.3, we also find little evidence of offsetting effects in neighborhoods of different income levels or in specific areas such as the Bronx and New Jersey.

5.2 Effects on Trips throughout the Metro Area

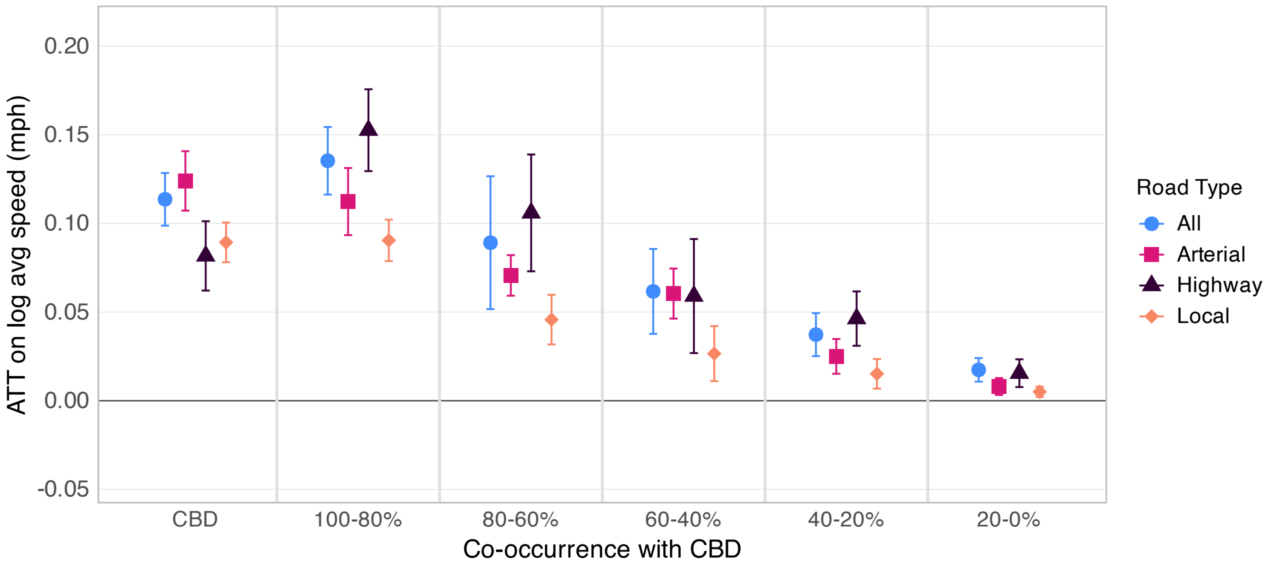
The spillover effects documented above at the segment level raise the question of how congestion pricing affected conditions for entire trips across the region. Travelers experience congestion not segment by segment but as the cumulative outcome of their journeys, and welfare ultimately depends on trip-level costs (Arnott, de Palma and Lindsey, 1993; Small, Winston and Yan, 2005). We classify trips into one of four categories depending on whether the origin and destination fall inside or outside the CBD: to CBD, from CBD, within CBD, and outside CBD. For passenger cars, only trips that start outside the CBD and enter the CBD ("to CBD") are subject to pricing, but all trips may be indirectly affected through the network spillovers documented above.[19] For each trip type, we again use the GSC approach to estimate the treatment effect of congestion pricing on log average speeds over the entire trip.

We find that congestion pricing increased speeds not only on the priced trips to the CBD, but also on unpriced trips traveling within, from, and outside the CBD. Figure 5a plots the ATT on log

[18] This is consistent with early evidence from the Congestion Pricing Tracker, which showed larger changes in speeds on bridges leading into the CBD than within the CBD itself (Moshes and Moshes, 2025). There are only a few highway segments within the CBD itself, as the FDR Drive and West Side Highway connections to West Street are exempted from congestion pricing unless drivers exit into the CBD. The CBD highway segments include just the exit/entry segments connecting highways to the CBD.

[19] Note that trips that start outside the CBD and pass through it are also priced, even if their ultimate destination is outside of the CBD. We include such trips in the definition of 'to CBD.'

Figure 4: Effect on Speeds by Co-Occurrence and Road Type



Notes: This figure documents treatment effects split by levels of co-occurrence and type of road segment. Each point is separately estimated using the average speeds in two-hour bins for segments with the corresponding level of co-occurrence and road segment type for both NYC and the comparison cities. Vertical bars represent 95% confidence intervals. Standard errors are clustered at the city-level.

average trip speeds within each origin-destination category. The policy increased speeds on trips to or from the CBD by 6–8% and speeds on trips within the CBD by about 5%. Even trips that took place entirely outside the CBD experienced modest gains of roughly 1%. While small in magnitude, these improvements apply to the majority of trips in the metro area and add up to a substantial aggregate impact.

To examine the spatial pattern in more detail, we estimate effects across a 5-by-5 origin–destination matrix based on distance from the CBD (Figure 5b).[20] We classify all origins and destinations based on the within-city percentile of their distance to the CBD (see Figure C.6 for a map). This disaggregation shows the largest gains for trips directly involving the CBD, but also nontrivial improvements for trips connecting outer areas. Trips traveling between the 0-20% distance bin and 60-100% bin experienced speed gains of 1–2% in each direction. Disaggregating ODs to this level also lets us detect small changes even in trips on the outskirts of the metro area.[21] For trips within the 60-100% distance bin, for example, we estimate a treatment effect of 0.6%. As we discuss in Section 6, these changes on unpriced trips—including the small gains on the many trips entirely outside of the CBD—have first-order implications for the policy's net welfare impact.

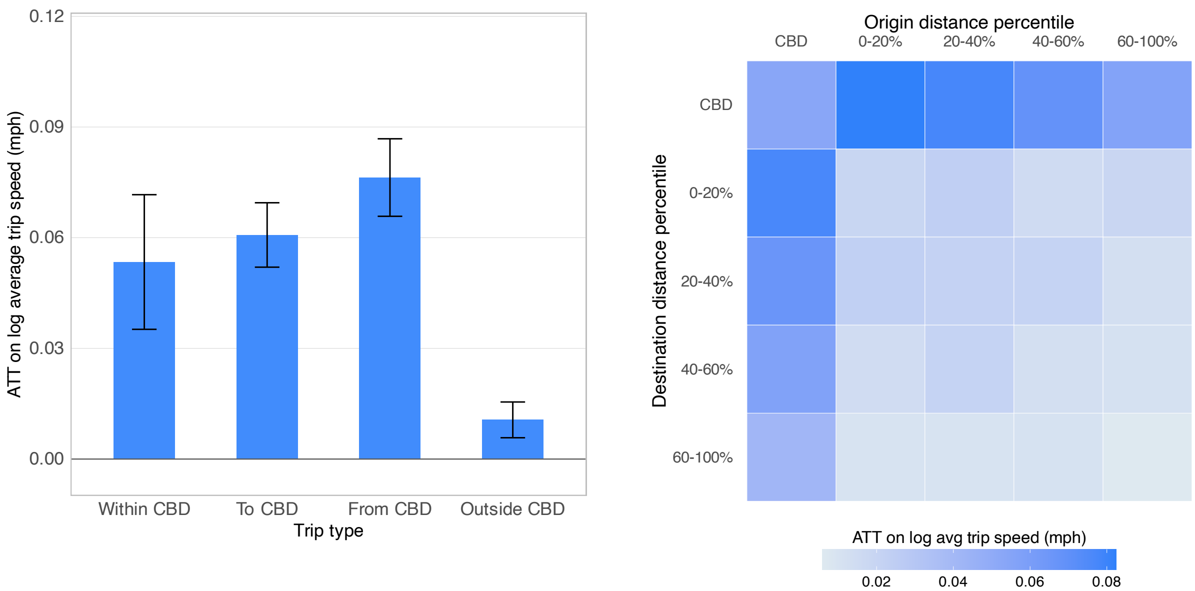
[20] Figure B.4 replicates the figure for log estimated fuel economy on trips within each OD pair. Most OD pairs saw estimated fuel economy gains in the range of 0.5–1.5%.

[21] Disaggregating the data to lower levels also creates more potential controls in other cities, giving the GSC approach more flexibility to build a synthetic control based on contemporaneous outcomes in other cities (e.g., here the outcomes in the 40-60% bin in Atlanta can enter as a control for the 60-100% bin in NYC separately from traffic outcomes on roads in the 60-100% bin in Atlanta).

Figure 5: Effect on Trip Speeds throughout NYC

(a) By trip type

(b) By OD distance to CBD



Notes: The first panel documents the ATTs for each of our four trip types. Vertical bars represent 95% confidence intervals. The second panel plots ATTs at a more spatially disaggregated level by binning trips based on their origin and destination's distance to the CBD. Each cell documents the ATT on log trip speeds for trips that originate in the bin indicated in the column labels and end in the bin indicated in the row labels. Each ATT is significantly different from zero at the 95% level, with standard errors clustered at the city-level.

5.3 Distributional Effects

Proposals to implement congestion pricing often face concerns over potential adverse impacts on specific groups, such as residents of specific areas or lower-income drivers (Ecola and Light, 2009; Taylor, 2010). We estimate heterogeneous effects across income groups and geographic areas to assess whether the effects of congestion pricing in NYC were unevenly distributed. For each region, Figure 6 documents the treatment effects on three outcomes: average speeds for road segments within the region, average speeds on trips *to* the CBD that originate in the region, and average speed on trips *from* the CBD that end within the specified region. The estimates follow the same generalized synthetic control methodology as in previous sections. Because the spatial distribution of these regions may be related to CBD travel in different ways in different cities, we use the set of all corresponding regions in each of our control cities as potential controls (e.g., the controls for the ATT on the bottom income quintile are all income quintiles in other cities).

Household income. We begin by comparing effects across the neighborhood income distribution. We group Census tracts into within-city quintiles based on their median household income in the 2019–2023 American Community Survey (ACS), which range from roughly $44,500 in the bottom quintile to $182,000 in the top.

We find broadly similar treatment effects on tracts across the income distribution. If anything, the largest gains occurred in lower-income areas – road segments in the bottom quintile experienced speed increases of 3.2%, compared with slightly smaller gains for segments in higher-income tracts.

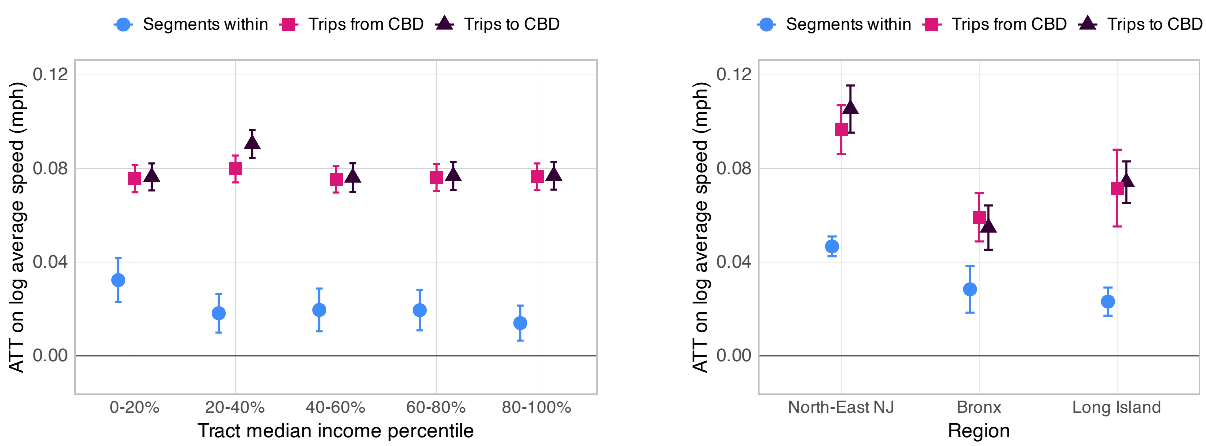
Trips traveling to or from the CBD from any income quintile experienced a 7.5-9% increase in speeds, with no significant differences in effects across quintiles.

Specific regions of interest. We next examine effects across specific geographic areas. In New Jersey, average speeds increased by 4.7% on road segments in Hudson and Bergen Counties, the two counties most directly connected to Manhattan. Trips from these counties to the CBD became 10.5% faster, while trips from the CBD to these counties became 9.7% faster. Within New York City, roads inside the Bronx became 2.8% faster, and trips to and from the CBD became 5.5% and 5.9% faster respectively. Finally, Long Island roads also became slightly faster on average, increasing in speed by 2.3%, and travel between Long Island and the CBD became significantly faster; trip speeds to and from the CBD increased by 7.4% and 7.2%, respectively. Figure C.7 further breaks out the effects on speeds for trips to the CBD based on the entrance they cross to enter the CBD. The largest effects were for trips crossing the Lincoln Tunnel, Queens-Midtown Tunnel, and Queensboro Bridge.

Figure 6: Effects on Speed by Geographies' Characteristics

(a) Tract median income

(b) Specific regions



Notes: These figures plot the ATTs on three measures of speeds: speeds on segments within a geography and speeds on trips to (from) the CBD that start (end) in the specified geography. Vertical lines represent 95% confidence intervals. Standard errors are clustered by city.

6 Driver Welfare

The implementation of congestion pricing in NYC increased speeds across the metropolitan area, such that many trips benefited from the speed increase without incurring a higher price. In this section, we conduct a simple stylized exercise to bound the welfare effects resulting from speed increases for trips between different origins and destinations. We combine these estimates to bound the net welfare impact of the congestion pricing policy on trips by private passenger vehicles and taxi/FHVs.

Pre-Policy Utility Consider an individual who would typically drive between an origin o and destination d during peak hours prior to the implementation of congestion pricing, which we will denote as period 0. On an average day, the individual would face a travel time t^0_{od}, a cost of driving

p^0_{od} that includes tolls, gas, depreciation, etc., and some baseline value for taking the trip ξ_{od}. We assume that these elements enter the driver's utility linearly:

$$u^0_{od} = \xi_{od} - p^0_{od} - \omega \times t^0_{od}. \tag{5}$$

Here, we normalize the marginal value of the cost of driving to 1 such that the coefficient on travel time ω can be interpreted as the Value of Travel Time (VOTT) in dollars per unit of time.

There may be many individuals who would take trip (o, d) if it were sufficiently attractive relative to their next best option. We assume that each individual i considering a trip had an idiosyncratic outside option utility $\varepsilon^0_{i,od}$. By revealed preference, individuals who took trip (o, d) prior to the policy preferred it to their outside option, $u^0_{od} \geq \varepsilon^0_{i,od}$, while individuals who did not take the trip preferred their outside option instead. Note that for simplicity, we define a trip by its origin and destination such that the choice of whether to take the trip or not is binary and switching modes is not explicitly considered, and we assume that the baseline value of taking the trip ξ_{od} is fixed over time. Because we do not place any restrictions on the outside option utilities, the outside options could include other modes of travel that may become more or less attractive relative to the baseline over time. As such, this assumption does not play an important role in our analysis.

Post-Policy Utility The congestion pricing policy had two major effects on the travel conditions faced by individuals: 1) if the trip required entering the CBD by vehicle, then the cost of driving increased by the congestion toll amount Δp_{od}; and 2) the average travel time t decreased by an origin-destination-specific amount Δt_{od}.[22] Because all trips—defined as the OD pairs in Figure 5b—became weakly faster on average (i.e., $\Delta t_{od} \geq 0$), the utility that an individual would obtain for taking the same trip after the policy was implemented, which we will denote as period 1, is bounded from below as follows:

$$u^1_{od} \geq \xi_{od} - (p^0_{od} + \Delta p_{od}) - \omega \times (t^0_{od} - \Delta t_{od}). \tag{6}$$

For some individuals, these changes pushed them over the margin of deciding whether to take a trip or not. Individuals for whom the post-policy utility of taking their trip was higher than their next best option, $u^1_{od} \geq \varepsilon^1_{i,od}$, may have switched to taking the trip after the policy, even if they hadn't taken the trip before. Conversely, individuals for whom $u^1_{od} < \varepsilon^1_{i,od}$ may have switched to taking their outside option even if they had been taking the trip before.

Bounding Welfare Contributions The net change in welfare from the policy is given by the difference in the sum of utilities for all affected individuals. For each trip (o, d), these individuals comprise three groups: those who took the trip before the policy, and continued to do so afterward; those who did not take the trip before the policy but started to afterward; and those who took the trip

[22] Auxiliary costs, such as gas, may have also decreased as a result of the improved fuel economy; however, we set aside any non-toll price changes for our main welfare analyses.

before the policy but stopped doing so afterward.[23] For each of the individuals who continued to make the same driving choice, the difference in utilities is just $u^1_{od} - u^0_{od}$. For the remaining individuals who switched either towards or away from their trip, the difference in their utilities relies on a comparison with their outside option.

Denote $\bar{\varepsilon}^0_{od,+}$ as the average outside option utility for individuals who only started taking the trip after the policy and $\bar{\varepsilon}^1_{od,-}$ as the average outside option utility for individuals who only took the trip prior to the policy but stopped afterwards. The average difference in utility obtained by the new trip takers is given by $u^1_{od} - \bar{\varepsilon}^0_{od,+}$. Similarly, the average difference in utility obtained by the former trip takers is given by $\bar{\varepsilon}^1_{od,-} - u^0_{od}$. We cannot credibly estimate the change in trip volumes due to the policy using our data, and so we cannot infer the values of $\bar{\varepsilon}^0_{od,+}$ and $\bar{\varepsilon}^1_{od,-}$ in order to compute an exact welfare impact. However, we can construct a simple bound on the welfare contribution of each trip based on *pre-policy* volumes, using the assumption that an individual would only start (stop) to take a given trip if that trip provided strictly higher (lower) utility after the policy than it did before. We describe the bounds formally here; Appendix B.2 offers a simple graphical illustration of the argument.

Formal Bound Derivation Let N^0_{od} be the number of individuals who took trip (o, d) prior to the policy, and let $N^{1,s}_{od}$, $N^{1,+}_{od}$, and $N^{1,-}_{od}$ be, respectively, the numbers of individuals who stuck with their driving choice, newly started taking the trip, and stopped taking the trip after the policy. The net contribution of trip (o, d) to the welfare impact of the policy is then bounded from below as follows:

$$\begin{aligned}
\Delta W_{od} &= N^{1,s}_{od} \times (u^1_{od} - u^0_{od}) + (N^{1,+}_{od}) \times (u^1_{od} - \bar{\varepsilon}^0_{od,+}) + (N^{1,-}_{od}) \times (\bar{\varepsilon}^1_{od,-} - u^0_{od}) && (7)\\
&\geq N^{1,s}_{od} \times (u^1_{od} - u^0_{od}) + (N^{1,-}_{od}) \times (\bar{\varepsilon}^1_{od,-} - u^0_{od}) && (8)\\
&\geq N^{1,s}_{od} \times (u^1_{od} - u^0_{od}) + (N^{1,-}_{od}) \times (u^1_{od} - u^0_{od}) && (9)\\
&\geq N^0_{od} \times (u^1_{od} - u^0_{od}) && (10)\\
&\geq N^0_{od} \times (\omega \times \Delta t_{od} - \Delta p_{od}). && (11)
\end{aligned}$$

Here, line (7) breaks up the net welfare contribution between individuals who took trip (o, d) prior to the policy and continued to do so afterward, individuals who only started taking the trip after the policy, and individuals who only stopped taking the trip after the policy. For individuals who newly started taking the trip, $u^1_{od} - \bar{\varepsilon}^0_{od,+}$ must be greater than zero by revealed preference, yielding the inequality in line 8. For individuals who newly stopped taking the trip, $\bar{\varepsilon}^1_{od,-}$ must be greater than u^1_{od}, yielding the inequality in line (9). Line (10) is given by the accounting identity $N^0_{od} = N^{1,s}_{od} + N^{1,-}_{od} - N^{1,+}_{od}$. Line (11) results from subtracting equation (5) from inequality (6).

[23] We assume that any trips that are unaffected by either the congestion toll price or the change in road speeds experienced no change in utility as a consequence of the policy.

Bounding Net Welfare The net welfare impact of the policy across the metropolitan area is given by the sum of welfare contributions across all OD pairs:

$$\Delta W = \sum_o \sum_d \Delta W_{od}. \tag{12}$$

Applying the inequality from line (11) we obtain the following lower bound on the net welfare impact:

$$\Delta W \geq \sum_o \sum_d \left[N^0_{od} \times (\omega \times \Delta t_{od} - \Delta p_{od}) \right]. \tag{13}$$

Connecting to Data. To estimate the bounds empirically, we limit our attention to welfare on trips by passenger vehicles and taxi/FHVs, setting aside effects on other road users (e.g., buses and commercial trucks) and how the city uses the toll revenue. In practice, the toll revenue has been committed to improvements to public transit in NYC, and speeds for other road users have also increased. Both of these suggest that our lower bound on welfare would likely increase by accounting for revenue recycling and other road users, although for the latter it depends on other road users' VOTT and the share traveling on priced trips.

As in Figure 5b, we group origins and destinations by five bins based on their distance to the CBD. For each OD pair, the effect of congestion pricing on travel time is given by

$$\widehat{\Delta t_{od}} = \bar{t}_{od} \times \left(1 - \exp(-\widehat{\mathrm{ATT}}_{od}) \right) \tag{14}$$

where $\widehat{\mathrm{ATT}}_{od}$ are the estimated ATTs on trip speed from Figure 5b and $\bar{t}_{od}$ is the pre-period average travel time. We assume that passenger vehicles and taxis are affected by road speed improvements in the same way, so that $\widehat{\Delta t_{od}}$ does not depend on the vehicle type. To estimate the change in tolls ($\widehat{\Delta p_{od}}$), we use data from the MTA on CBD entries by vehicle class and entry point to compute the average congestion fee paid by passenger vehicles and taxi/FHVs. For passenger vehicles, we assume they enter a single time per day (i.e., we do not amortize their per-day cost across entries) and compute the average price net of any rebates for entering certain already-tolled entries.[24]

To estimate the pre-period trip volumes N^0_{od} for each vehicle type, we use volumes reported by Replica, a company that combines a variety of data sources (e.g., GPS devices, sensors embedded in roads, taxi/FHV companies, and administrative data) to simulate mobility patterns throughout entire metro areas. Replica's primary customers are in the public sector, and include the MTA and other transit organizations. We use their estimated tract-to-tract travel flows by passenger vehicles and taxi/FHV for the NYC metro area for a typical weekday and weekend day in 2024 Q4, which we scale up to the full week and then sum across tracts in each of our origin-destination groups. For trips that enter the CBD, we supplement the data from Replica with data on CBD entries from the MTA

[24] Drivers entering via the Lincoln Tunnel, Holland Tunnel, Queens Midtown Tunnel, Hugh L. Carey Tunnel receive a \$3 'crossing credit' so pay an effective price of \$6 to enter the CBD, while drivers entering via the other entrances pay the full amount. We compute the weighted average price paid using the total passenger vehicle crossings for each entry (MTA, 2025*c*). We do not account for the additional 50% discount that low-income drivers can receive for trips after their first 10 trips each month.

(MTA, 2025c). The estimates by Replica are below the reported CBD entries by the MTA for 2024, so we scale the counts of trips to the CBD from Replica up to exactly match the aggregate entries reported by the MTA. We describe this process further in Appendix B.1.

The final component needed to estimate the welfare bounds is a VOTT (ω). We use \$40/hour as a benchmark, which is approximately the average hourly wage in the NYC metropolitan area (BLS, 2025). We also identify several threshold VOTT values that would change the qualitative interpretation of our analysis.

Estimated Welfare Bounds. For private passenger vehicles, we find that aggregate driver welfare increased by at least \$12.0 million per week, with the positive welfare gains coming entirely from *unpriced* trips. Figure 7a plots the per-trip welfare gain for each of the OD pairs. The average passenger vehicle trip to the CBD paid \$7.9 to save just 3.1 minutes, which, with a VOTT of \$40/hour, adds up to a total welfare change of at most -\$9.4 million across the 1.61 million trips to the CBD each week. For these trips, the average driver would only be guaranteed to be better off on this trip following the implementation of congestion pricing if her VOTT were above \$153/hour, although these drivers likely also take other unpriced trips (e.g., when they leave the CBD). Unpriced trips from, within, and outside the CBD save an average of just 11 seconds per trip, but there are far more unpriced trips than priced trips. Across the 180.4 million weekly unpriced passenger vehicle trips, these time savings add up to a welfare gain of at least \$21.4 million per week. Taking into account both priced and unpriced trips, the 'break-even' VOTT—i.e., the VOTT where the time savings of all drivers exactly offset the prices paid by paying drivers—for passenger vehicle trips is at most \$21/hour.

Passengers of taxi/FHVs enjoy similar time savings but at a lower price than passenger vehicles, assuming that the only effect of the policy on their cost is the congestion fee itself. For these trips, the toll on any travel to, from, or within the CBD was set at \$0.75 for taxi and \$1.50 FHV after the policy was implemented. The vast majority of priced trips are by Yellow cabs, so the average price paid across all taxi/FHV trips is \$0.78. Thanks to the cheaper congestion fees, both priced and unpriced taxi/FHV trips experienced an aggregate net welfare gain. The estimated lower bounds for per-trip welfare gains are highest for trips going to the CBD, which offered the most time savings. Welfare gains for Taxi/FHV trips within the CBD itself were small, because the ex-ante shorter trip durations translated to smaller time savings. The break-even VOTTs are \$19/hour for the average priced trip and \$17/hour for unpriced trips. There are fewer taxi/FHVs trips than private passenger vehicle trips, but the gains for taxi/FHV passengers still add up to \$2.3 million per week.

In total, our estimates suggest that the congestion pricing policy had a positive net welfare impact of at least \$14.3 million per week for passenger vehicle drivers and taxi/FHV passengers, driven almost entirely by the large welfare gains on *unpriced* passenger vehicle trips. Across both passenger vehicle drivers and taxi/FHV passengers, the net welfare is positive so long as drivers value their time at \$20/hour or more. These welfare gains do not take into account the value from the revenue that is raised or any environmental benefits. The implied peak hour revenue from the CBD entry counts and average prices is \$14 million per week, although this is only an approximation as it does not account for other vehicle classes, the low-income discount plan, payment methods (it costs more to pay by mail), or repeated entries in a single day.[25] For environmental benefits, in Appendix B.3 we show that the implied fuel economy improvements add up to approximately 74,700 gallons of gasoline saved each week. These savings correspond to 653 tonnes of CO_2 each week, which, for a social cost of carbon of \$185 per tonne (Rennert et al., 2022), is worth \$120,900 in overall welfare. In Appendix B.4, we document all of the input values for our welfare calculations so that readers can explore the results under alternative inputs.

7 Mechanisms

The effects of introducing congestion pricing on speeds and travel times depend on three underlying ingredients: 1) how pricing affects volumes throughout the network; 2) the exposure of other drivers to changes in these volumes (e.g., via the co-occurrence of segments they traverse with the CBD); and 3) the relationship between volumes and speeds on different road segments. While we cannot speak to how volumes respond to pricing in each city absent a demand model, we evaluate how the latter two ingredients contributed to the effectiveness of the policy in NYC, and whether similar effects may be expected in other cities.

Exposure to changes in volumes. Travel patterns vary across cities, such that the average trip in different cities may be more or less exposed to any changes in volumes stemming from the introduction of cordon-based congestion pricing. Consider two extreme travel patterns: one where all trips to the CBD come from the farthest reaches of the metro area, and a second where all trips to the CBD begin just outside of its boundaries. In the former case, reducing the number of trips to the CBD will have spillovers throughout the metro area, as many non-CBD trips will travel along segments with high co-occurrence. In the latter case, few segments will have high co-occurrence, and, while congestion in the CBD itself may improve, we would expect minimal spillovers beyond the CBD.

We combine our segment-level measures of co-occurrence with data from trips between origins and destination pairs to quantify how exposed different trips are to changes in CBD volumes. For each trip type t, we measure the average time spent on each type of road segment. Using the same matrix of ODs as in Figure 5b to define trips, we compute the average co-occurrence of segments traversed as

$$\bar{e}_{od} = \frac{1}{|R_{od}|} \sum_{i \in R_{od}} \sum_{s \in R_i} \frac{t_{is} \times c_s}{t_i} \tag{15}$$

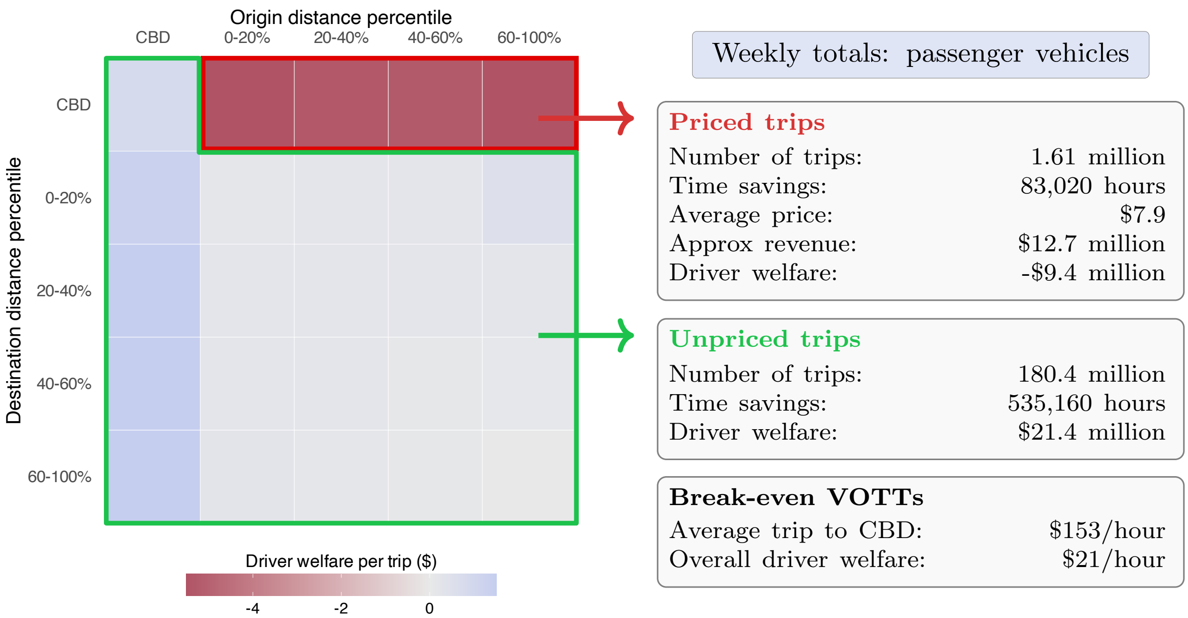
where R_{od} is the set of observed trips for a given OD pair, each trip $R_i \in R_{od}$ is composed of segments $R_i = \{s_1, s_2, \ldots, s_J\}$, c_s is the co-occurrence of segment s, and t_{is} and t_i are, respectively, the duration on segment s and the total trip duration.

We further aggregate up to trip types $l \in \{\text{To CBD}, \text{From CBD}, \text{Outside CBD}\}$, weighting by the

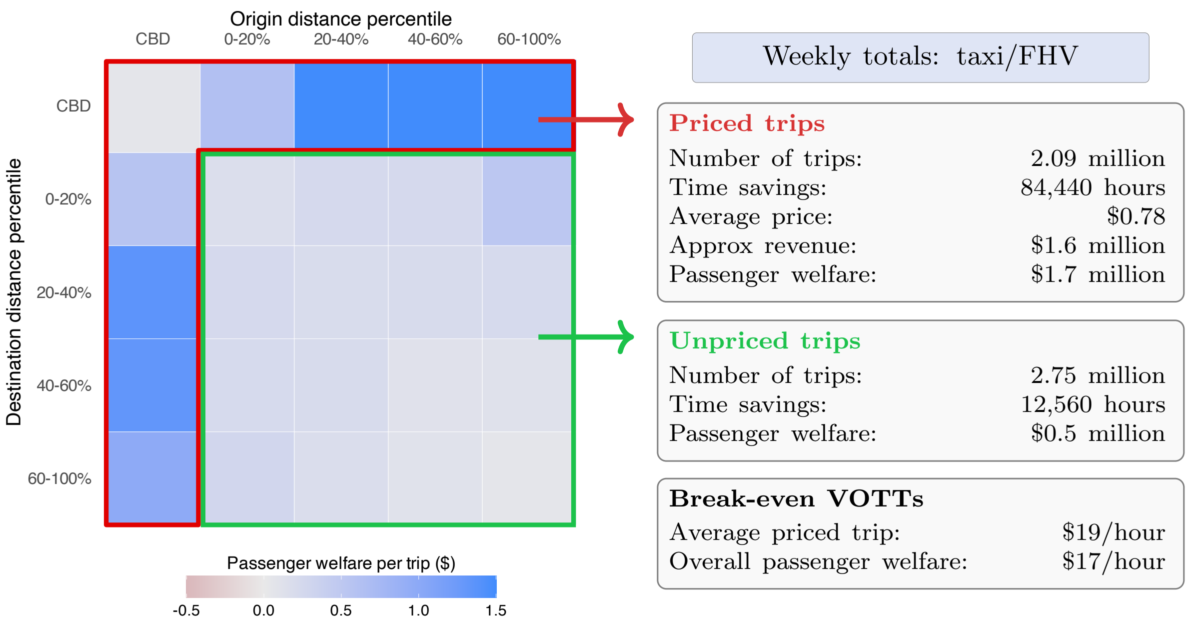
[25] The MTA reported CBD toll revenues of \$159 million for January to March 2025 and operating expenses of \$35.9 million (MTA, 2025*b*), or about \$13.3 million in gross revenue and \$10.25 million in net revenue per week.

Figure 7: Driver welfare estimates

(a) Private passenger vehicles



(b) Taxi/FHV trips



Notes: Each square is shaded by the per trip driver welfare, which are computed using the ATTs on trip speeds, the average pre-period duration, and the prices. Each ATT is significantly different from zero at the 95% level, with standard errors clustered at the city-level. The number of trips is the average tract-to-tract passenger car trips or taxi/FHV trips estimated by Replica for a typical week in 2024Q4. For trips to the CBD, we further scale the estimates from Replica to match aggregate counts of pre-period entries from the MTA (MTA, 2025*c*). Average prices are computed based on entries data. The passenger vehicle price is below $9 because of crossing credits for some entries, and the taxi/FHV price is set between the per-trip prices for taxis ($0.75) and FHV ($1.50) based on the share of trips in each. The welfare estimates are computed for each cell separately, assume a Value of Travel Time (VOTT) of $40/hour, and do not include any revenue recycling. Revenue is approximated as the number of trips multiplied by the average price, setting aside other factors such as low-income price discounts and revenue from other vehicle classes.

average weekly pre-period volumes between each OD pair (N_{od}^{PV}):[26]

$$\bar{e}_l = \frac{\sum_{o,d} \bar{e}_{od} \times N_o^{\mathrm{PV}} d}{\sum_{o,d} N_o^{\mathrm{PV}} d} \tag{16}$$

The first three columns of Table 1 document this measure of exposure for NYC and each of our control cities. Naturally, trips to and from the CBD are more exposed to the policy than trips entirely outside of the city. Including the part of the trip that is within the CBD, the weighted average segment-level co-occurrence on trips to/from the CBD ranges from about 50–65%. Across cities, NYC drivers to/from the CBD have some of the largest exposure to other CBD drivers, second only to Chicago. Outside of the CBD, however, NYC has the lowest average exposure, and is less than half that of cities like Chicago, Boston, and Atlanta. Cities with high average exposure on trips from and outside the CBD, such as Chicago, have travel patterns that suggest greater potential for positive spillovers on the speeds of unpriced trips.

Congestion functions. The next key ingredient is the local relationship between volumes and speeds. If all roads are already operating under free-flow conditions, then further decreases in CBD volumes would have no effect on travel times. The empirical relationship between speeds and the volume (or density) of cars is often used by traffic engineers to evaluate the effects of past and potential changes to traffic flows (Greenshields et al., 1935; Greenberg, 1959; Seo et al., 2017), and is referred to as the 'congestion function' of a road. Congestion functions will vary across roads based on their physical characteristics (e.g., width, curvature, and lane structure) and typical driving behaviors (e.g., spacing between cars). A curvy road full of potholes will operate at slower speeds for a given density of cars than a well-maintained highway.[27]

We estimate congestion functions for groups of road segments g using samples of simultaneous speeds v_g (in miles per hour) and densities ρ_g (in vehicles per lane-mile). Our data cover only a sample of cars on the road, so we cannot directly observe total volumes to measure road density exactly. Instead, we infer density by assuming a constant scaling factor on observed traversals and normalizing relative to the maximum density observed for a group of road segments. We then fit the following functional form, taken from the Bureau of Public Roads (BPR):

$$\frac{1}{v_g} = \frac{1}{v_{\mathrm{FF}}} \left[1 + \alpha \left(\frac{\rho_g}{\kappa} \right)^{\beta} \right]. \tag{17}$$

In this specification, v_{FF} denotes the free-flow speed, ρ_s is the inferred density of vehicles on the segment, κ is the capacity (the density at which congestion has a significant effect on speeds), α is a scaling parameter, and β governs the curvature.

We estimate separate congestion functions for each combination of road type and co-occurrence bin in each of our cities.[28] Figures 8a and 8b plot the congestion functions for the CBD and 60-80%

[26] We focus just on passenger vehicle trips for this exercise, as taxis are a small share of the non-NYC markets. The results look similar when taxi/FHV trip counts are included.

[27] See Currier, Glaeser and Kreindler (2023) for evidence of the effects of road smoothness on road speeds.

[28] These congestion functions are subtly different than those studied in traffic engineering, because we use data from

co-occurrence bins by road type in NYC; the other NYC congestion functions are shown in Figure C.8. The circles along each congestion function curve represent peak hour speeds on the average day prior to the policy's launch, and the triangles represent the post-policy speeds computed as the pre-period speeds times one plus the estimated ATT for this road type and co-occurrence bin.

This sample of congestion functions illustrates a few key points that are generally true across most cities and co-occurrence bins. First, congestion functions for local and arterial roads are lower in both levels and slopes than those for highways, so reductions in density have more limited effects on speeds. Second, congestion functions for some roads—especially highways—are non-linear, and the effects of any changes in density depend on the slope at the relevant levels of density. A highway that is already operating near the speed limit will see little gain from further reductions in density, while a highway operating at a steep point along its congestion function may see large gains from relatively small reductions in density.

Table 1: Potential for Effects in Other Cities

City	Avg. exposure ($\bar{e}_l$)			Avg. elasticity of CF ($\bar{\eta}_l$).		
	To CBD	From CBD	Outside CBD	To CBD	From CBD	Outside CBD
NYC	57.6%	58.7%	3.0%	-0.422	-0.430	-0.270
Philadelphia	54.6%	54.9%	3.2%	-0.254	-0.250	-0.215
Chicago	62.2%	62.7%	7.1%	-0.325	-0.320	-0.170
Boston	55.4%	55.5%	7.6%	-0.387	-0.384	-0.205
Atlanta	51.1%	51.8%	7.4%	-0.336	-0.334	-0.239
Baltimore	49.6%	49.8%	5.3%	-0.255	-0.254	-0.214

Notes: The first three columns document the average exposure to the policy for trips to, from, and outside the CBD, measured as the weighted average co-occurrence of segments traversed with weights corresponding to the average duration on each segment. The latter three columns document the average elasticity of the congestion functions (CF) of roads traversed on each trip type, again weighting by the average duration spent on a given segment type.

To compare the potential for volume changes to affect speeds in each city, we measure the elasticity of congestion functions on low- and high-co-occurrence roads, evaluated at the average pre-period densities. To further aggregate trips along these segments, we average across origin-destination pairs and weight by the average duration on roads corresponding to a given congestion function. More concretely, for a given OD pair we estimate the average local elasticity of the segment types that trips in that OD traverse as:

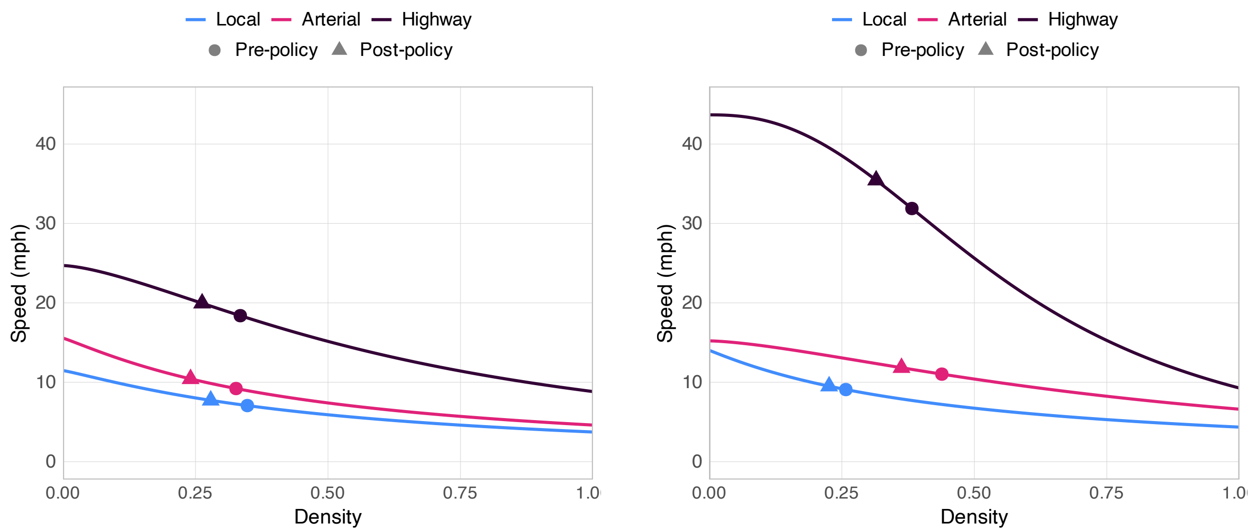
$$\bar{\eta}_{od} = \frac{\sum_c \sum_r \bar{t}^{cr}_{od} \left(v'_{cr}(\rho_{cr}) \frac{\rho_{cr}}{v_{cr}(\rho_{cr})} \right)}{\sum_c \sum_r \bar{t}^{cr}_{od}}, \tag{18}$$

where $v_{cr}(\cdot)$ is the estimated congestion function for co-occurrence bin c and road type r, ρ_{cr} is the corresponding pre-period average density, and $\bar{t}^{cr}_{od}$ is the average pre-period duration spent on this co-occurrence bin and road type combination for trips in a given OD pair. As in Equation (16), we further

a collection of heterogeneous road segments within a group rather than a single segment. As a result, different levels of density in our data may correspond to different patterns of crowding across roads – some segments may be near free-flow while others are heavily congested. The estimated function should therefore be interpreted as a reduced-form mapping between average speeds and average densities, rather than a physical law of traffic flow on a particular road.

Figure 8: Example Congestion Functions

(a) CBD roads (NYC) (b) 60-80% co-occurrence roads (NYC)



Notes: This figure plots estimated congestion functions for roads in NYC. The congestion functions are estimated using pre-period observed speeds and densities, as shown in Equation (17). Circles are added to each line based on the raw average speed before the policy's implementation. Triangles are based on the post-period speed, which is computed using the estimated ATT on speeds.

aggregate up for each trip type $l \in \{\text{To CBD}, \text{From CBD}, \text{Outside CBD}\}$ with weights corresponding to the number of pre-period passenger vehicle trips in a given OD pair (N_{od}^{PV}).

The last three columns of Table 1 document $\hat{\eta}_l$ for each city and trip type. Prior to the implementation of congestion pricing, NYC roads traversed by drivers on trips to, from, and outside the CBD were operating at steeper parts of their respective congestion functions than roads traversed by drivers on similar trips in each of the other cities. This is especially true of roads on trips to and from the CBD, which were about 10% more locally elastic than the next closest city (Boston) and nearly 70% more elastic than the lowest ranked city (Philadelphia). Roads traversed on trips outside the CBD were also operating at more elastic parts of their respective congestion function in NYC than in any other city, often by similarly large relative differences (although smaller absolute magnitudes).

These average elasticities of the congestion functions suggest that the effects on time savings of introducing a cordon-based congestion price may have been uniquely large in NYC relative to our control cities. However, while roads outside the CBD in NYC were modestly more elastic than in other cities, the relative differences are smaller than the differences in exposure to CBD trips for drivers on trips outside the CBD. For these unpriced trips entirely outside of the CBD—which contribute substantially to overall welfare—the average exposure in NYC was half that of some other cities. Thus, while speeds on trips to/from the CBD in other cities would likely be less affected by any change in volumes than in other cities, spillovers on trips outside their respective CBDs may be larger if higher exposure offsets the lower elasticities.

Finally, the influence of exposure to CBD trips and congestion function elasticity depends critically on the nature of the demand response to congestion pricing. Although we cannot observe this directly in New York, our results are consistent with a largely *extensive margin* response in which drivers to the CBD substituted to other modes of transportation rather than driving to other nearby locations. If drivers in other cities were to respond in other ways—for instance, because alternative modes of transit are less appealing—then the nature of both the direct effects and the spillover effects of cordon-based congestion pricing might be quite different.

8 Conclusion

The introduction of congestion pricing in New York City offers new evidence on how cordon-based pricing can reshape urban mobility in ways that extend well beyond the tolled area. While the policy had the expected direct effect of increasing road speeds inside the CBD, we find that the greatest welfare gains arise from spillovers onto unpriced trips *outside* of the CBD. Although the impact on travel times for any single trip outside of the CBD was small, the number of trips outside the CBD is much larger than the number of trips to, from, or within the CBD. As such, our findings suggest that evaluations limited to the cordon area itself would significantly understate the welfare gains of congestion pricing.

The welfare implications also underscore a tension central to the political economy of congestion pricing. Priced drivers face salient, concentrated losses unless their value of time is very high, while unpriced travelers enjoy diffuse, less visible benefits. Distributionally, we observe broadly similar speed improvements across neighborhoods with varying household income levels, as well as in specific areas of interest, such as New Jersey.

Whether similar effects can be expected in other cities depends, in part, on the distribution of travel patterns in each city and the centrality of roads leading to and from each city's CBD in its road network. On one hand, many roads in NYC operated on steep portions of their congestion functions prior to the policy, so even small reductions in volumes could produce outsized speed gains that affect a large share of unpriced trips. On the other hand, trips outside the CBD in NYC also tend to be less exposed to CBD-bound trips than similar trips in other cities. Cities like Chicago and Boston—where trips outside the CBD are over twice as exposed to CBD trips—may see larger spillovers throughout the metro area. Moreover, the influence of exposure and congestion function elasticity depends on whether driving trips affected by congestion pricing are replaced entirely or modified to use other roads. Whether a cordon-based congestion price similar to NYC would generate a similar type of demand response in other cities remains an open question.

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Appendices

A Data Appendix

A.1 CBD Definitions

Table A.1 contains references to the CBD definitions we use for the control cities and New York City. Each links to a snapshot of the corresponding web page on the Internet Archive. This avoids links breaking due to website changes of the city-affiliated organizations from which we derive the CBD definitions.

Table A.1: CBD Definitions Overview

Link	City name	Defined by	CBD shape
🔗	Atlanta	Atlanta Downtown	The "Downtown Boundary".
🔗	Baltimore	Downtown Partnership of Baltimore	The "Downtown Management Authority" shape on page 5.
🔗	Boston	City of Boston	The outline of the "Downtown" neighborhood.
🔗	Chicago	City of Chicago	The "Downtown Zone Area".
🔗	New York City	Metropolitan Transportation Authority	The outline of the "Congestion Relief Zone".
🔗	Philadelphia	Center City District Philadelphia	The boundaries of the district on page 8.

A.2 Estimating Fuel Efficiency

We evaluate the environmental impact of this experiment by measuring fuel consumption rates on road segments. CO_2 emission rates then naturally arise from these estimates of fuel consumption, as carbon emissions are typically modeled as proportional to the fuel consumption in transportation settings (Department for Energy Security and Net Zero, 2023). Since direct measurements of fuel or CO_2 emissions are impractical, we use scalable methods for estimating fuel consumption rates instead.

Fuel consumption modeling has been widely studied in the literature (Faris et al., 2011). Models of this form roughly fall into two categories: principled models (Faris et al., 2011), which aim to model the physics underlying energy usage, and empirical models (Department of Energy , DOE; Ersal et al., 2012; Department of Energy , DOE), which fit non-parametric models to ground-truth fuel consumption data. For our purposes, we integrate models from the *National Renewable Energy Laboratory* (NREL), which fall roughly under both categories. These models at their core rely on FASTSim (Brooker et al., 2015), a physics-based simulator (hence a principled model) which calculates the power required to meet a given drive cycle speeds provided other inputs such as road grade and vehicle specifications such as drag, transmission, and rolling resistance. Its methodology and data are validated from dynamometer testing data via collaboration with other labs (e.g., Argonne National Laboratory), so this is a high-fidelity model with many parameters to calibrate towards specific vehicle models. However, FASTSim requires significant computational power and high-frequency GPS location data, which makes it challenging to run for all segments or trips. To address this issue, we use an empirical machine learning model on top of FASTSim, similar to NREL's RouteE model (Holden, Reinicke and Cappellucci, 2020). This family of models significantly reduces the computational burden and works well with segment-level speeds, eliminating the need for high-fidelity GPS location data. The ML-based model takes as features the properties from segments and estimates the fuel consumption for each segment. Features commonly used by these models include the segment-level speeds, road grade, and length.

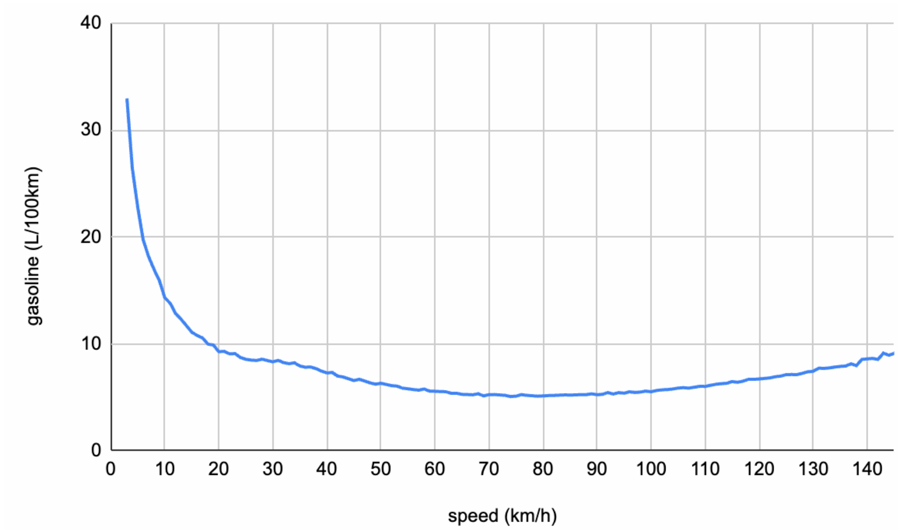
Figure A.1 depicts the empirical relationship between speed and fuel consumption on one class of roads, which is used to model the overall relationship. Notably, the convex shape of the model is a commonly known feature by energy modeling practitioners, and denotes that vehicles operating at intermediate driving speeds generally experience the highest levels of fuel efficiency.

A.3 PurpleAir Pollution Measures

For each city, we collect hourly $PM_{2.5}$ CF1 and relative humidity readings for all PurpelAir sensors in the corresponding CBSA. For redundancy, each sensor includes two independent measurement channels, labeled as "A" and "B" channels. We fill in missing relative humidity readings of any one channel with the readings from the other channel and censor any outlier $PM_{2.5}$ values larger than 300 $\mu g/m^3$.

We follow EPA's PurpleAir data calibration methodology to improve the quality of our air pollution data (Barkjohn et al., 2022), taking the following steps to build a dataset of sensor-day level $PM_{2.5}$ level.

Figure A.1: Example Speed-Fuel Consumption Rate Curve



Notes: This figure documents an example of the empirical relationship between speeds and fuel consumption for one class of roads. Similar data are used to model the overall relationship on various roads and road conditions.

1. We drop records for which the difference between the A and B channel $PM_{2.5}$ values are greater than 5 µg/m^3 or the relative percentage difference is greater than 70% (computed with the lowest value in the denominator).

2. Still following Barkjohn et al. (2022), we compute the quantities

$$
\begin{aligned}
x_1 &= 0.524 \text{ PM}_{2.5,\text{ raw}} - 0.0862 \text{ RH} + 5.75 \\
x_2 &= 4.21 \cdot 10^{-4} \text{ PM}^2_{2.5,\text{ raw}} + 0.392 \text{ PM}_{2.5,\text{ raw}} + 3.44 \\
x_3 &= 0.0244 \text{ PM}_{2.5,\text{ raw}} - 13.9,
\end{aligned}
\tag{A.1}
$$

   where $\text{PM}_{2.5,\text{ raw}}$ is the raw hourly CF1 $PM_{2.5}$ channel reading and RH is the relative humidity channel reading;

   We then use these values to compute the estimated hourly $PM_{2.5}$ for a specific channel in a given sensor as

$$
\text{PM}_{2.5} = \begin{cases} x_1 & \text{if PM}_{2.5,\text{ raw}} < 570 \\ x_1(1 - x_3) + x_2 x_3 & \text{if } 570 \leq \text{PM}_{2.5,\text{ raw}} < 611 \\ x_2 & \text{if } 611 \leq \text{PM}_{2.5,\text{ raw}} \end{cases}
\tag{A.2}
$$

3. We aggregate to the sensor-hour level by taking the average of the channel-specific adjusted levels of $PM_{2.5}$, or taking the value of just one of the two channels' hourly adjusted $PM_{2.5}$ levels when it is missing for the other channel.

4. Finally, we compute the daily adjusted $PM_{2.5}$ averages, dropping any comprising of fewer than 18 hours of observations.

The outcome variable we use in our analysis of the air pollution effects in the NYC CBD is that of sensor-date level $PM_{2.5}$. Some sensors are missing information for one or more days. We impute missing sensor-date $PM_{2.5}$ values using a regression with sensor $\times$ month-of-sample and sensor $\times$ day-of-week fixed effects.

A.4 Consumer Spending Data from MBHS3

The data from MBHS3 are aggregated to the zipcode, date, and 3-digit NAICS code level. The zipcode is based on the merchant's physical location. The data exclude online transactions. Table A.2 documents the total number of transactions and aggregate transaction amount by category. The final three columns subset to zipcodes within the CBD. The average transaction size in both restaurant and retail categories tends to be higher for merchants in the CBD than in the overall city.

We define zipcodes as lying within a city's CBD if its centroid is contained by the CBD boundaries. For NYC, we manually include zipcode 10004. This zipcode includes Governor's Island, which pulls its centroid out into the river, but most of its merchants are in the southern portion of the Financial District. Figure C.3 Panel a) maps the 'treated' zipcodes in the NYC CBD. The Baltimore CBD is small and there are no zipcodes whose centroids lie within it, so we do not report any spending for Baltimore's CBD.

Table A.2: Transaction Counts and Amounts: January 2024 - April 2025

		All			**CBD-only**		
City	Category	Count (millions)	Amount (millions $)	Avg. size ($/trans.)	Count (millions)	Amount (millions $)	Avg. size ($/trans.)
NYC	Restaurant	164.7	4613.2	28.0	26.1	941.7	36.0
	Retail	286.1	13247.0	46.3	39.6	2196.0	55.4
PHL	Restaurant	88.5	2291.9	25.9	3.9	125.3	32.1
	Retail	168.6	7549.0	44.8	3.2	115.0	35.5
CHI	Restaurant	114.2	2773.8	24.3	5.8	172.5	29.7
	Retail	148.2	7425.8	50.1	3.7	137.5	37.3
BOS	Restaurant	59.2	1433.1	24.2	0.8	25.6	32.4
	Retail	68.4	3390.8	49.6	0.2	15.9	79.2
BAL	Restaurant	61.7	1066.6	17.3	–	–	–
	Retail	58.4	2644.8	45.3	–	–	–
ATL	Restaurant	141.7	3368.8	23.8	3.6	90.3	25.2
	Retail	190.0	8995.0	47.3	1.2	43.2	37.2
Total	Restaurant	629.9	15547.4	24.7	40.2	1355.6	33.7
	Retail	919.7	43252.4	47.0	47.9	2507.5	52.3

Notes: This table documents the aggregate number of transactions and total spending in the MBHS3 data for zipcodes within each of the sample cities between January 2024 and April 2025. The Baltimore CBD is small and there are no zipcodes whose centroids lie within it.

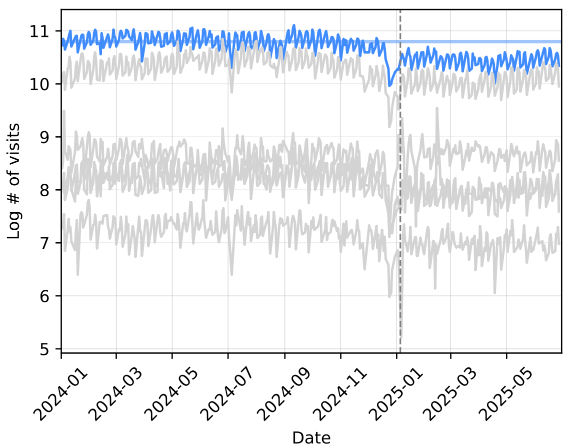
A.5 Foot Traffic Data from Advan

The raw data from Advan Neighborhood Patterns include the number of stops by GPS devices at the hour-block group level. We restrict to peak hours and combine across all block groups within the same Census tract. Figure A.2 plots the average daily foot traffic in the CBDs of our sample cities. There is a clear drop in all cities at the start of 2025. The drop is substantial in magnitude—about

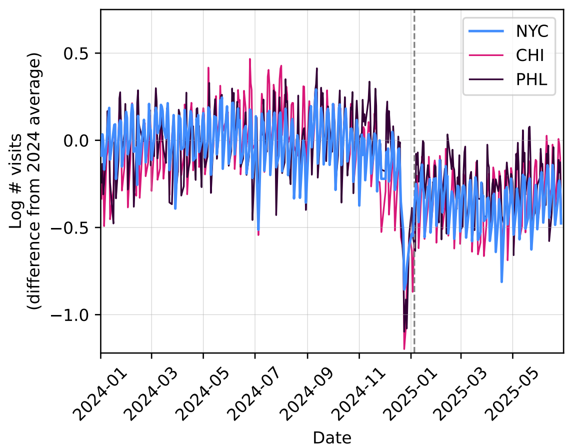
30%—and is likely due to a change in the sources from which Advan purchases location data. The decline in measured foot traffic is similar in magnitude and timing across all of our sample cities.

Figure A.2: CBD Foot Traffic

(a) Log average stops per day



(b) Log stops relative to 2024 average



Notes: These figures plot daily CBD foot traffic, restricted to weekday peak hours (5am to 9pm). The first panel plots the log total number of visits. The blue horizontal line is the pre-period average for the NYC CBD. The second panel restricts attention to the three largest cities and plots the difference in log trips between a given date and the 2024 average.

B Welfare Calculations Appendix

B.1 Origin-Destination Volumes

For computing the aggregate welfare effects in Section 6, we need the number of personal vehicle and taxi/FHV trips between each set of origins and destinations. One limitation of the data from Google Maps is that we cannot reliably observe volumes, much less changes in volumes over time. We instead estimate these values for the pre-period (September to December 2024) using a combination of data from Replica and the MTA.

We start with estimated tract-to-tract flows for a 'typical' Thursday and Saturday in the fourth quarter of 2024 from Replica, an urban data platform company that spun out of Google in 2019. Replica ingests data from dozens of sources, including location data from GPS devices, traffic patterns from in-road sensors, administrative data on local demographics, parcel land use, public transit usage, taxi/FHV trips, travel surveys, and more. Replica then builds full-population simulations that aim to accurately capture the population and travel patterns of metro areas. Underlying these simulations are activity-based models trained to match the 'ground truth' data (e.g., the number of cars driving over a specific road segment with an embedded sensor). Key for our use case, Replica estimates the mode choice for each trip, including separately estimating use of private autos and taxi/FHVs. We aggregate to typical weekday and weekend flows to the weekly level by multiplying the weekday flows by five and the weekend flows by two.

We supplement data from Replica with data from the MTA on CBD entries. CBD entries are an especially important source of welfare in our setting, and the MTA provides realized flows. First, we use data on realized entries since congestion pricing launched, split by entry point, 10-minute interval, and vehicle class. We use these data to compute the number of taxi/FHVs ("TLC Taxi/FHV") and passenger car ("Cars, Pickups and Vans") entries during peak hours in the average week. Together, these two categories account for 85% of all entries (Table B.1). We scale the estimated entries by Replica to exactly match the passenger vehicle and taxi/FHV counts. Second, as these entries are for 2025 but our desired volumes are those in the absence of congestion pricing, we further scale each CBD entry estimate up by 13% to match the baseline CBD entries reported by in MTA (2025*c*). This approach essentially combines Replica data on the relative distribution of origin tracts for CBD trips with MTA data on the overall levels. Note that the MTA entries data include priced trips that pass through the CBD en route to other destinations, which may explain some of the discrepancy in CBD trip levels between the MTA and Replica counts.

While we do not use within-day variation in CBD entries, Figure B.2 shows the number of entries to the CBD by 10-minute interval on the average weekday (including all vehicle classes) reported by the MTA. There is some clear bunching just before and after the peak pricing time.

Table B.1: CBD entries by vehicle class

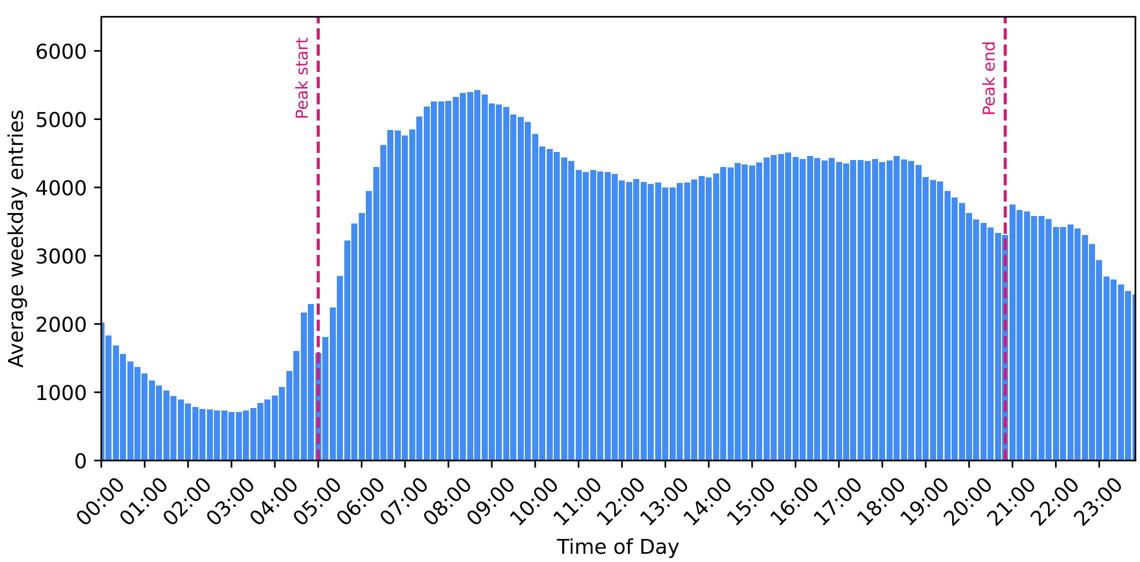
Vehicle class	Avg. daily entries	Frac. of all entries
Cars, Pickups and Vans	226351	0.596
Taxi/FHV	127678	0.336
Single-Unit Trucks	15492	0.041
Buses	7504	0.020
Motorcycles	1516	0.004
Multi-Unit Trucks	1029	0.003

Notes: This table documents the average entries into the CBD for peak hours between Jan 5, 2025 to June 30, 2025.

Figure B.1: Estimated Weekly Trip Counts

Notes: These figures plot the log weekly estimated trip counts between bins of origins and destinations. The trip counts are estimated using a combination of data from Replica and the MTA and reflect pre-congestion pricing flows. The red polygons indicate priced trips and the green polygons indicate unpriced trips.

Figure B.2: CBD Entries by Time of Day (MTA)



Notes: This figure plots the average number of weekday CBD entries by 10-minute intervals. The underlying data are from the MTA and include all vehicle classes.

B.2 Illustrative Example

We provide an illustrative example of the argument underlying the welfare bounds derived in Section 6. Figure B.3 illustrates the effects of congestion pricing for a representative priced and unpriced trip. The y-axes are the cost of the trip, inclusive of the value of the travel time $\omega t(Q)$ and the congestion fee p, setting aside all other costs. Here, the travel time depends linearly on the quantity Q—which is often not the case for real-world congestion functions—so the average private cost line is described by $c = \omega t(Q) + p$. The demand curve $Q(c)$ can be thought of as the CDF of outside option values (ε_i) across the population of prospective drivers for a trip. Although we allow the outside option values to change before and after the policy in the formal bounds, here we assume that the outside option value for each driver is fixed such that demand is unchanged pre- and post-policy.

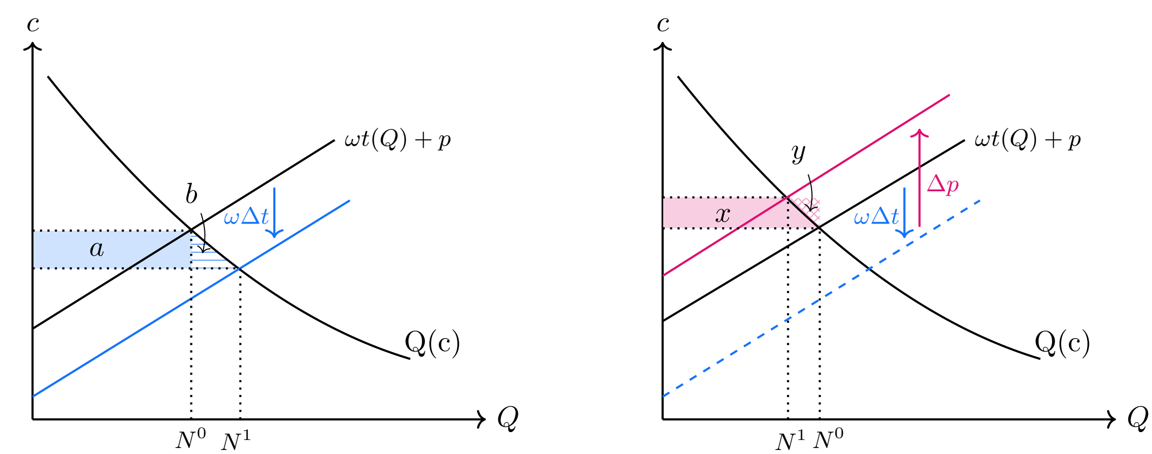
For unpriced trips (Figure B.3a), introducing congestion pricing reduces travel time costs by $\omega\Delta t$. With elastic demand, this would encourage additional driving and move the equilibrium volume from N^0 to N^1. The true change in welfare stemming from the policy change is the area $a + b$. By using fixed volumes in our welfare calculation, we report welfare of just $\widehat{\Delta W}_{\text{unpriced}} = a$ for unpriced trips, which understates the true welfare gains by the area of b.

For priced trips (Figure B.3b), the countervailing price increase (Δp) offsets the improved travel times ($\omega\Delta t$) and, in this illustration, reduces total volumes. The true change in driver welfare is the area x. Here, by using fixed volumes, our welfare estimate of $\widehat{\Delta W}_{\text{priced}} = -x - y$ overstates the true welfare loss by the area y. If the gains due to time savings are greater than the losses due to paying the toll—as is the case for many taxi trips—then the figure would look more similar to Figure B.3a and we would understate the net gains of priced trips.

Figure B.3: Illustration of Driver Welfare Calculations

(a) Unpriced trip

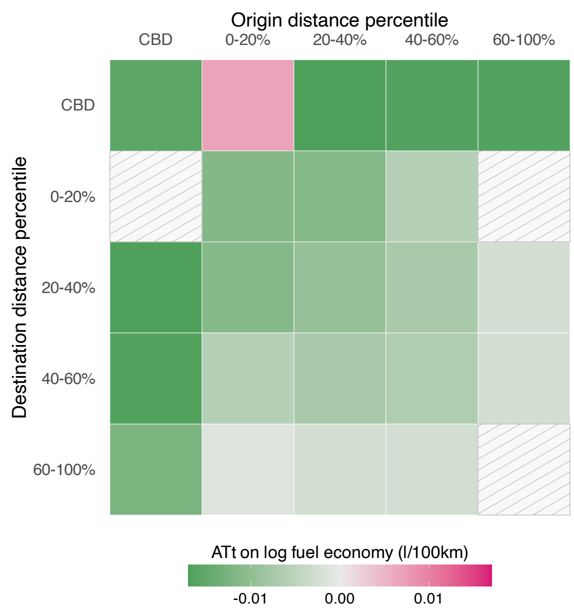
(b) Priced trip



B.3 Fuel savings

Figure B.4 documents the ATTs on fuel economy (in liters of gasoline per 100km) for trips between each OD pair. We compute the aggregate effect on fuel consumption in a similar manner to how we compute time savings for the welfare calculations. Specifically, for each OD pair, we multiply the average pre-period estimated fuel consumption (in liters) by the estimated treatment effect (in percent), excluding ATTs that are not significantly different from zero. We then sum across all OD pairs, weighting by the number of pre-period passenger vehicle trips from Replica (N_{od}^{PV}). In total, we compute weekly fuel savings of 74,700 gallons per week (on a baseline of 25.9 million total gallons per week). Using a conversion rate of one gallon of gas to 8.75 kilograms of CO_2 , the implied CO_2 savings are 653 tonnes each week. At a social cost of carbon of $185 per tonne (Rennert et al., 2022), this has a social value of $120,900.

Figure B.4: Treatment effect on fuel economy by OD distance to CBD



Notes: This figure documents the ATT on log fuel economy (l/100km) for trips in each OD pair. Grey hash marks represent estimates that are not significantly different from zero at the 95% level, with standard errors clustered at the city-level.

B.4 Welfare Calculation Inputs

Table B.2 documents the primary inputs for the welfare estimates presented in Section 6. The columns correspond to estimated treatment effect on log speeds ($\widehat{\mathrm{ATT}}_{od}$), the average pre-period duration ($\bar{t}_{od}$), and the volumes (N_{od}^{j}) and average price (p_{od}^{j}). Note that we assume that prices are equal across all priced trips within a vehicle class and that each vehicle class faces the same travel times and ATTs conditional on an origin-destination pair.

Table B.2: Welfare estimate inputs

				Passenger Vehicles		Taxi/FHVs	
Origin	Destination	$\widehat{\text{ATT}}_{od}$	$\bar{t}_{od}$	N_{od}^{PV}	p_{od}^{PV}	N_{od}^{FHV}	p_{od}^{FHV}
CBD	CBD	0.053	23.2	643104	$0	607838	$0.78
CBD	0-20%	0.077	25.4	501032	$0	262957	$0.78
CBD	20-40%	0.067	46.6	295583	$0	100202	$0.78
CBD	40-60%	0.057	53.8	247297	$0	83728	$0.78
CBD	60-100%	0.042	62.2	317983	$0	123129	$0.78
0-20%	CBD	0.082	24.8	610386	$7.90	322985	$0.78
0-20%	0-20%	0.019	12.6	7738585	$0	812093	$0
0-20%	20-40%	0.024	15.0	2627617	$0	203010	$0
0-20%	40-60%	0.015	23.3	1508345	$0	129397	$0
0-20%	60-100%	0.010	41.1	1122931	$0	82589	$0
20-40%	CBD	0.078	47.1	207671	$7.90	121089	$0.78
20-40%	0-20%	0.024	15.0	2562395	$0	148876	$0
20-40%	20-40%	0.022	13.5	9768322	$0	265182	$0
20-40%	40-60%	0.021	14.9	4112082	$0	133374	$0
20-40%	60-100%	0.010	26.3	1469808	$0	40717	$0
40-60%	CBD	0.069	54.5	221190	$7.90	145707	$0.78
40-60%	0-20%	0.016	23.3	1479622	$0	78476	$0
40-60%	20-40%	0.021	14.8	4248147	$0	98916	$0
40-60%	40-60%	0.013	13.6	22665226	$0	385637	$0
40-60%	60-100%	0.011	15.7	6202699	$0	78086	$0
60-100%	CBD	0.056	61.5	572317	$7.90	319255	$0.78
60-100%	0-20%	0.019	40.9	1085775	$0	56038	$0
60-100%	20-40%	0.013	26.1	1562846	$0	12879	$0
60-100%	40-60%	0.011	15.6	6613899	$0	31220	$0
60-100%	60-100%	0.006	14.1	103590251	$0	197165	$0

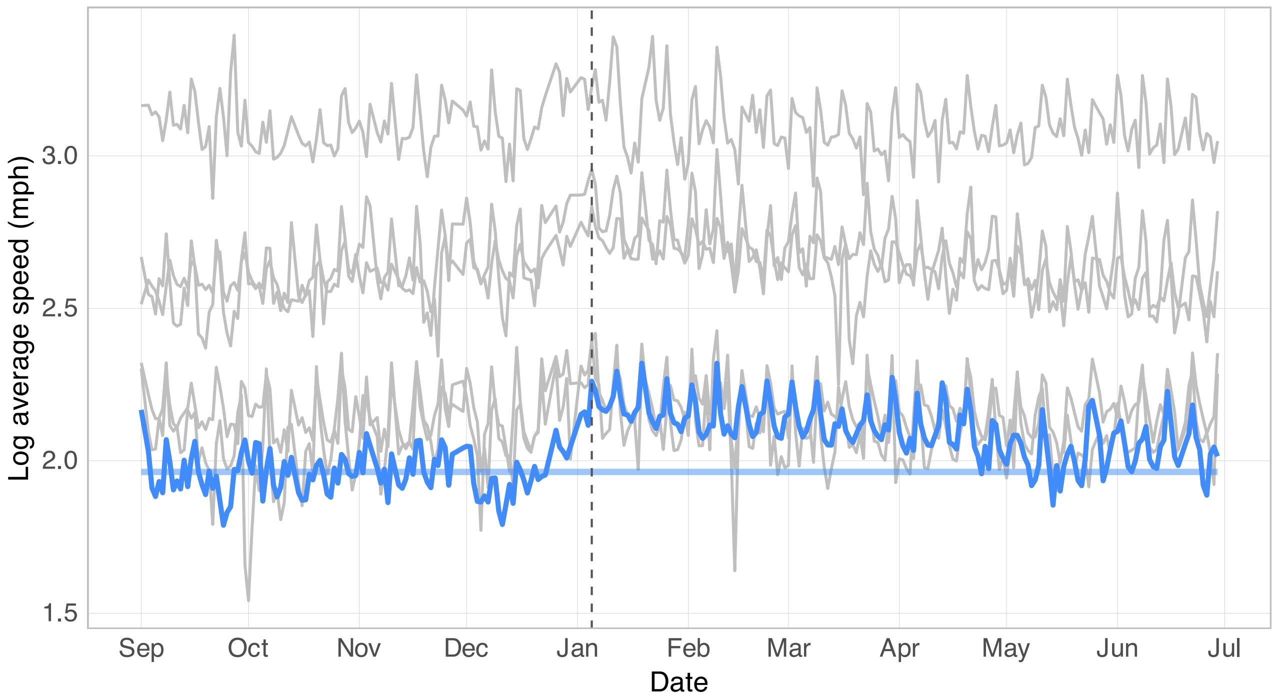
C Supplementary Tables and Figures

Table C.1: Average CBD speeds before and after congestion pricing

		Average speeds (mph)	
City	Road type	Before Jan 5, 2025	After Jan 5, 2025
New York	All	7.14	8.17
	Highway	16.95	18.45
	Arterial	7.19	8.31
	Local	5.54	6.24
Atlanta	All	22.37	22.26
	Highway	33.86	33.57
	Arterial	11.51	11.36
	Local	10.22	10.2
Baltimore	All	8.19	8.29
	Highway	8.54	8.83
	Arterial	8.3	8.28
	Local	7.6	7.84
Boston	All	13.99	14.82
	Highway	32.16	33.11
	Arterial	7.09	7.37
	Local	6.61	6.79
Chicago	All	13.57	13.88
	Highway	25.21	26.68
	Arterial	10.32	10.43
	Local	8.69	8.88
Philadelphia	All	8.56	8.77
	Highway	35.77	35.01
	Arterial	7.93	8.15
	Local	6.59	6.71

Notes: This table reports the average volume-weighted speeds on segments in each of the cities used throughout our analyses for weekday peak hours. The "before" period data starts September 1st, 2024 and the "after" period data ends on June 30th.

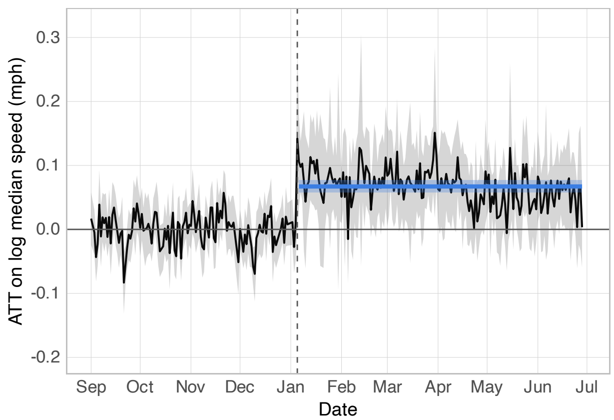
Figure C.1: Average CBD speeds



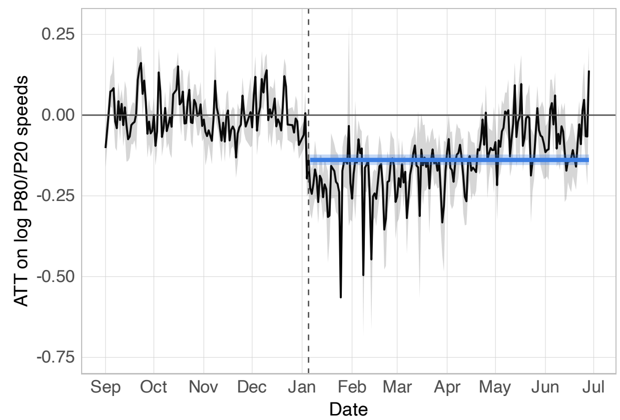
Notes: This figure documents the volume-weighted average daily speed on road segments in the CBD of each city. The horizontal lines represent the average speed between September 1 and December 15, 2024.

Figure C.2: Effects on Additional Road Speed Outcomes

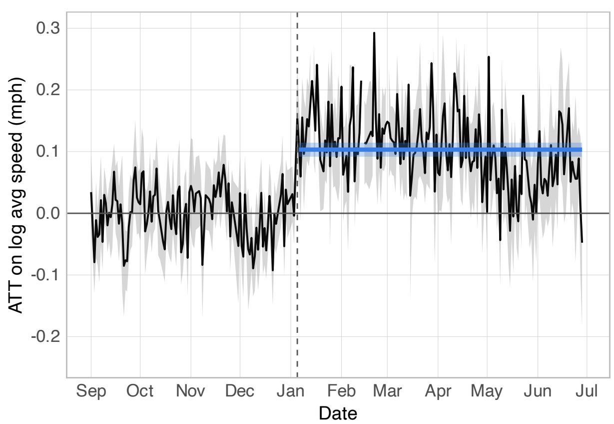
(a) ATT on median speed



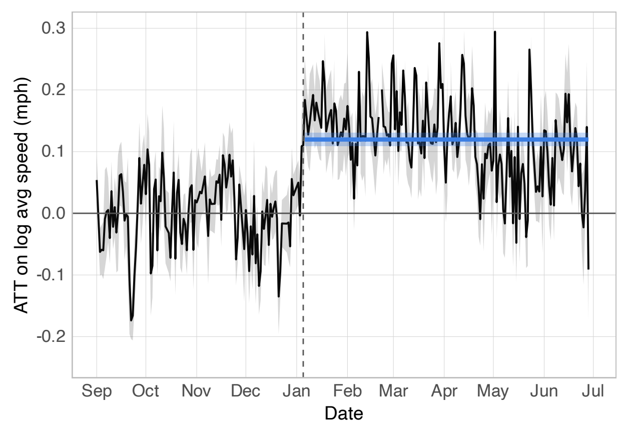
(b) ATT on p80/p20 speeds



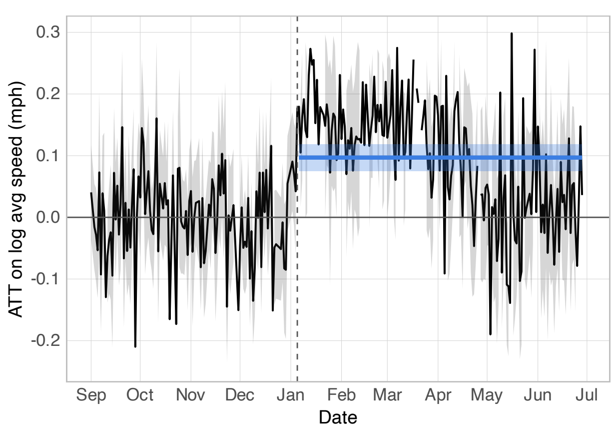
(c) Local roads: ATT on avg speeds



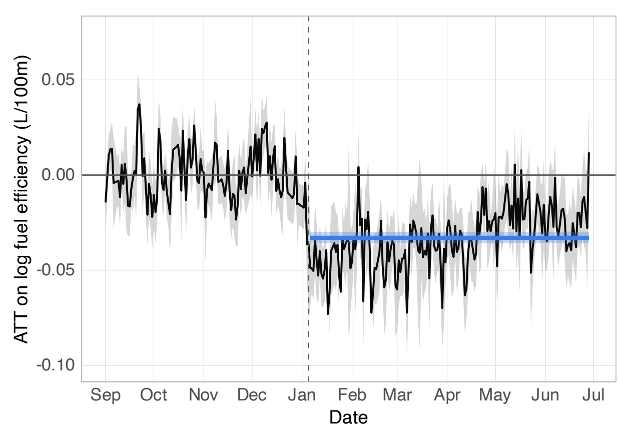
(d) Arterials: ATT on avg speed



(e) Highways: ATT on avg speed



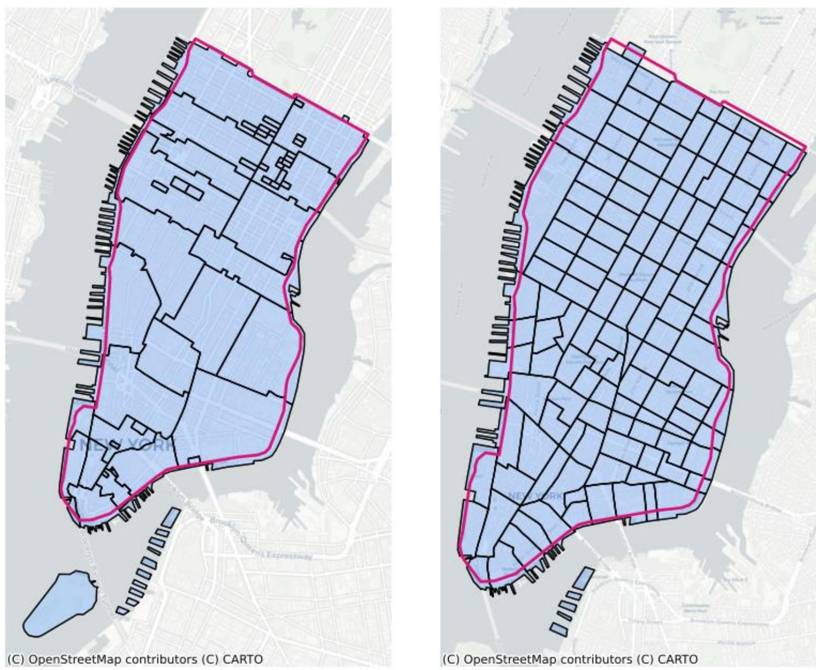
(f) ATT on fuel economy (L/100m)



Notes: This figure replicates Figure 2a for additional CBD traffic measures. Each figure plots day-level ATTs of congestion pricing over time. The underlying data are two-hour bin outcomes in each CBD. The horizontal blue line is the aggregate ATT for all post-treatment periods. Shading denotes 95% confidence intervals. Standard errors are clustered at the city-level.

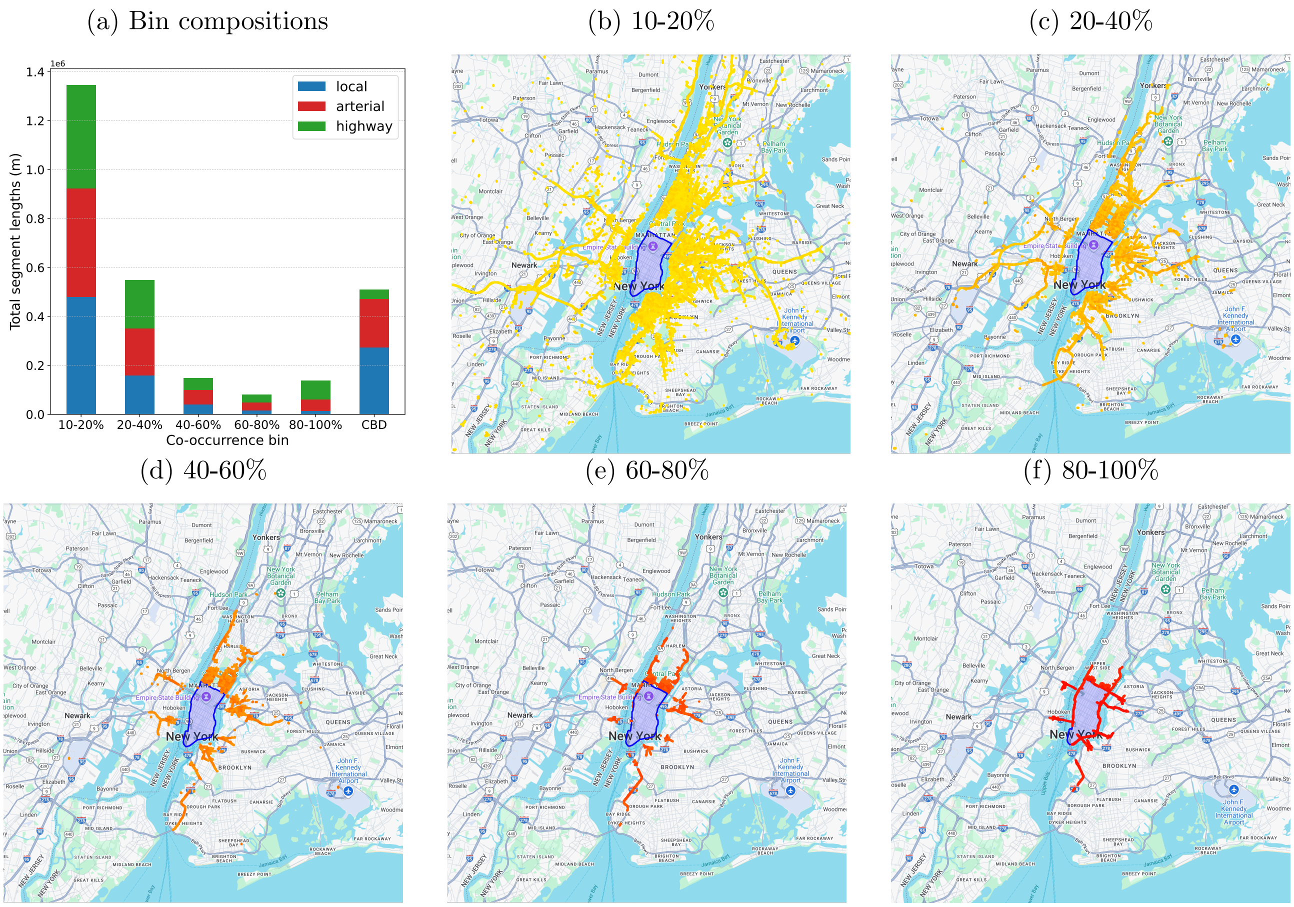
Figure C.3: Intersection of CBD and Zipcodes/Tracts

(a) Treated zipcodes (b) Treated tracts



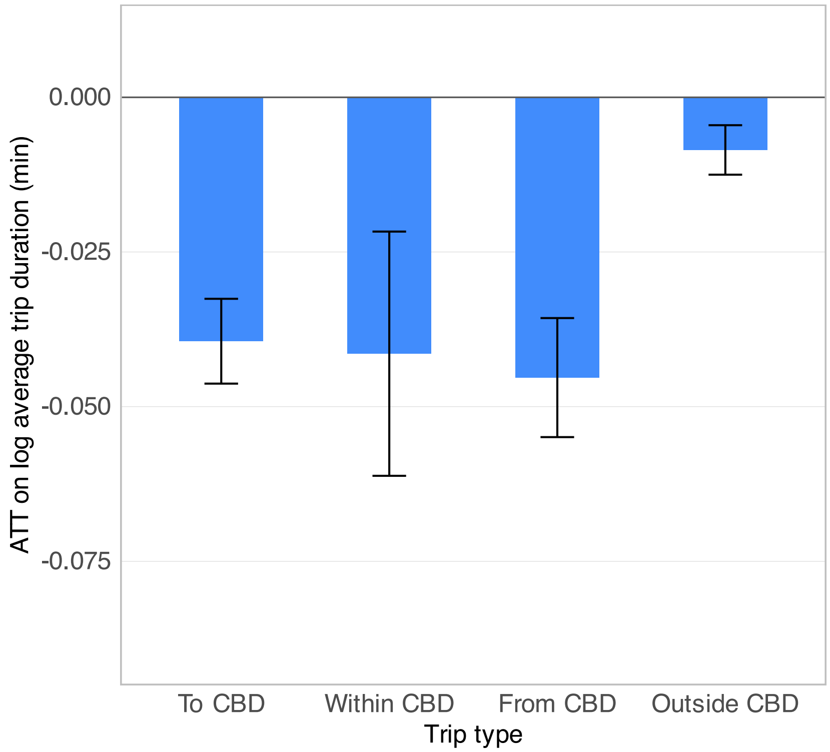
Notes: This figure documents the Census tracts and zipcodes that are defined as 'treated' by the policy because their centroids lie within the CBD. Treated units are shaded blue and the boundary of the CBD is in pink. We manually include zip 10004, which includes Governor's Island and parts of the Financial District as its centroid lies outside the CBD but the vast majority of its merchants are within the CBD.

Figure C.4: Segments by bins of co-occurrence



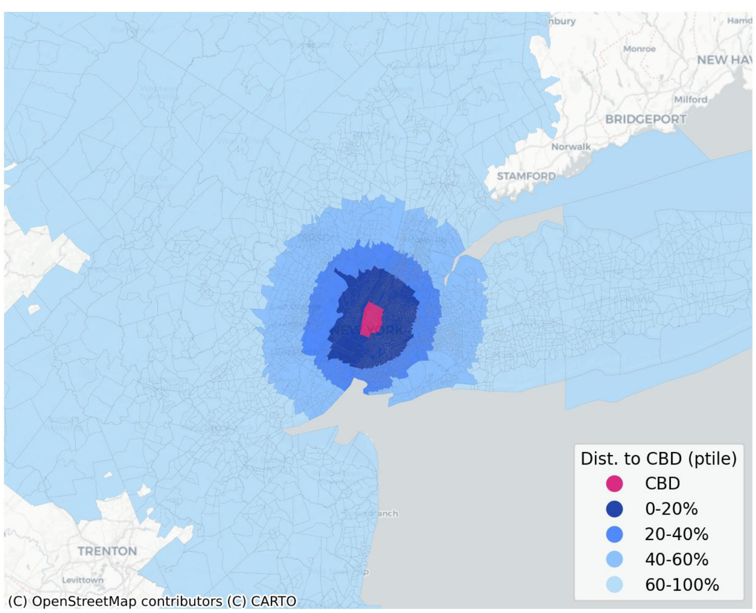
Notes: These figures map the road segments by their level of co-occurrence with the CBD, which is defined as the share of trips crossing the road segment that ultimately enter the CBD. The first panel plots the distribution of road types by co-occurrence.

Figure C.5: Treatment effect on trip durations



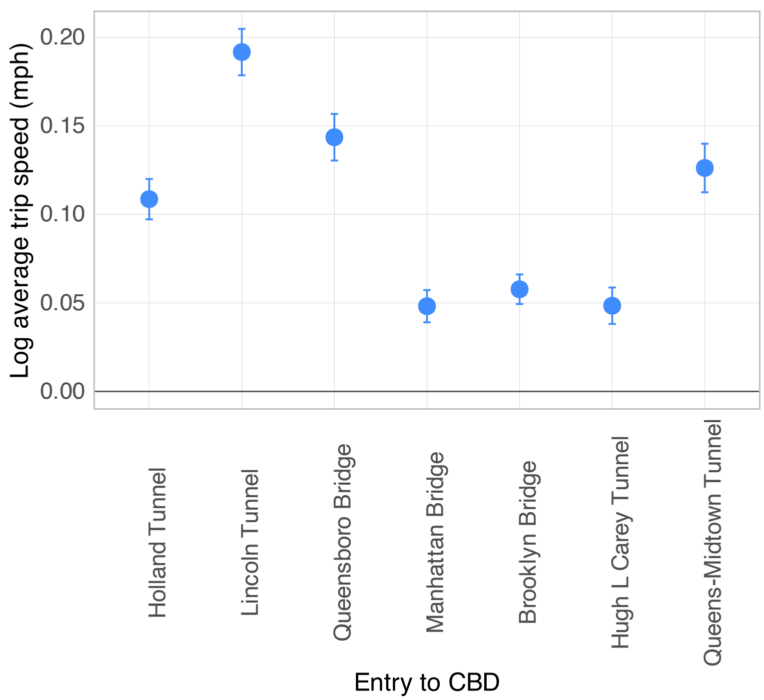
Notes: This figure documents day-level ATT on average realized trip durations split by whether the trip starts and/or ends in the CBD. The underlying data are the aggregate speed for each city, OD type, and two-hour bin. The horizontal blue lines are the aggregate ATTs for all post-treatment periods. Shaded areas represent 95% confidence intervals. Standard errors are clustered at the city-level.

Figure C.6: Bins of distance to CBD



Notes: The boundaries are based on the Core Based Statistical Area boundaries. The map is zoomed in slightly to make the CBD more visible; the CBSA extends further in each direction, especially east down Long Island. Distance to CBD is based on the distance between centroid and the closest point of the CBD.

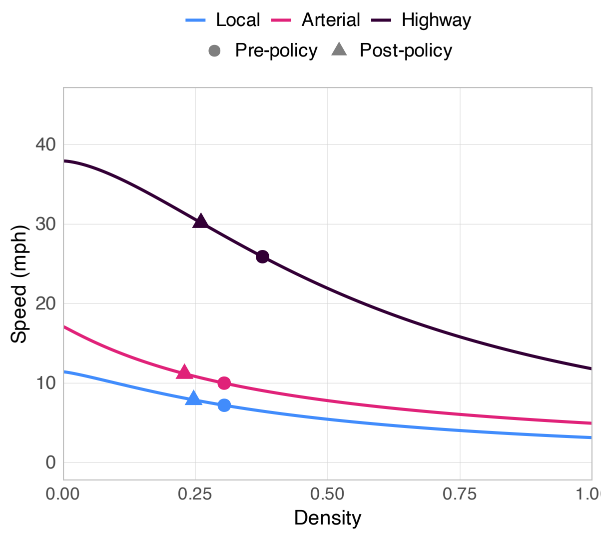
Figure C.7: ATTs on trip speeds by CBD entrance



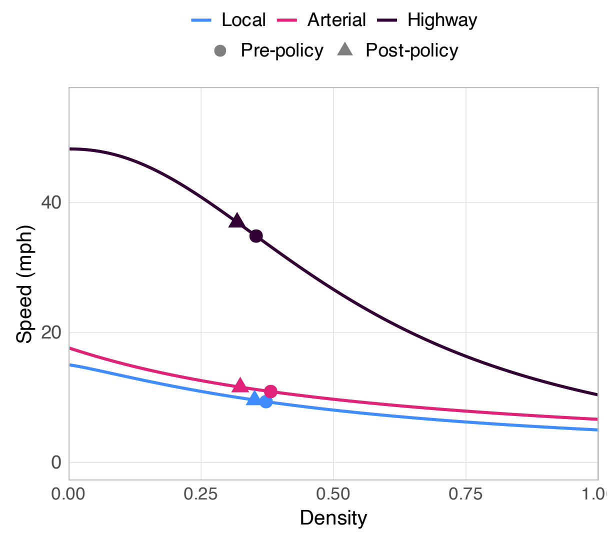
Notes: This figure plots the estimated ATT on log trip speeds for trips entering the CBD, split by which entrance they cross to enter the CBD. The set of potential controls include two-hour aggregates of trip speeds on trips to the control city CBDs from any origin. Vertical lines represent 95% confidence intervals. Standard errors are clustered at the city-level.

Figure C.8: Other congestion functions: NYC

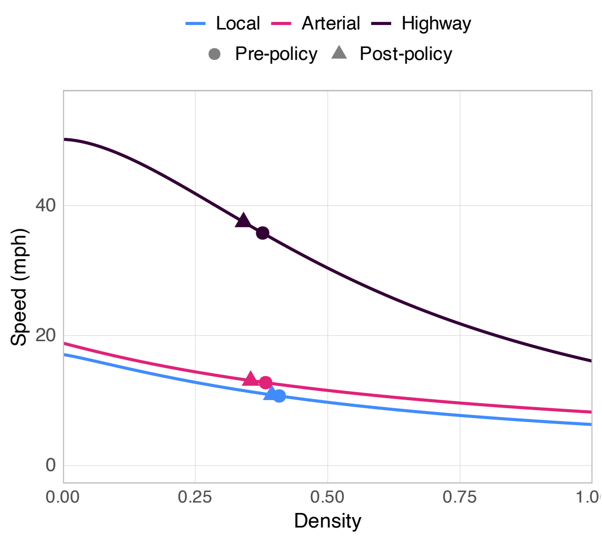
(a) 80-100% co-occurrence roads



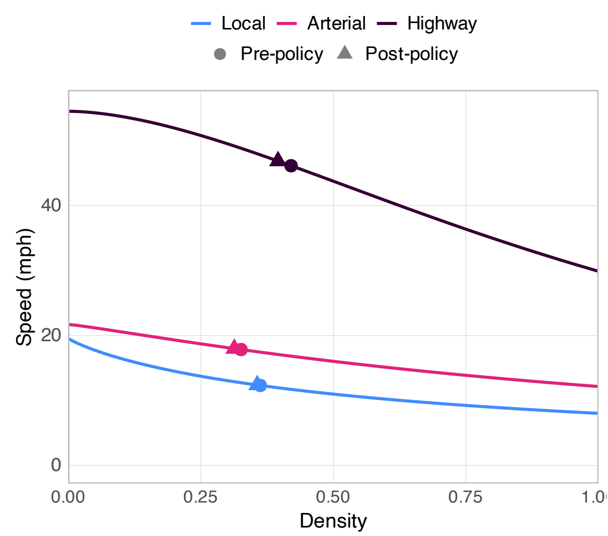
(b) 40-60% co-occurrence roads



(c) 20-40% co-occurrence roads



(d) 0-20% co-occurrence roads



Notes: This figure plots the remaining estimated congestion functions for roads in NYC; see Figure 8 for the CBD and 80-60% co-occurrence congestion functions. The congestion functions are estimated using pre-period observed speeds and densities, based on Equation (17). Circles are added to each line based on the raw average speed before the policy's implementation. Triangles are based on the post-period speed, which is computed using the estimated ATT on speeds.