# Evaluating the Multi-year Mobility Impacts of Pandemic-Induced Street Experiments – The Evidence of the Curbside Café Program in Toronto

Manuscript written by Qiwei Song[1,2,3] and Meikang Li[4]

1. School of Architecture, The Chinese University of Hong Kong, Hong Kong SAR, China
2. Senseable City Laboratory, Department of Urban Studies and Planning, Massachusetts Institute of Technology, Cambridge, MA, USA
3. Computational Social Science Laboratory, Faculty of Social Science, The Chinese University of Hong Kong, Hong Kong SAR, China
4. School of Design and Innovation, Shenzhen Technology University, Guangdong, China

## Abstract

Pandemic-era street experiments rapidly reallocated road space to support local economies and active travel, yet their longer-term mobility effects remain insufficiently understood. This study examines Toronto's CaféTO curbside dining program as a city-scale, annually repeated natural experiment, linking patio design with bikeshare trips from 2020 to 2022. Using a spatial difference-in-differences framework that accounts for trip spillovers, we estimate the causal impacts of curbside cafés on cycling activity over time. We find that CaféTO significantly increased bikeshare usage in 2020 and 2021, with effects weakening in 2022 as travel patterns normalized. Impacts were most substantial after full seasonal deployment and on weekends, indicating a prominent leisure-related mechanism. Importantly, design effects evolved over the years: wider patios generated immediate cycling gains during the program's initial rollout, whereas longer café frontages became influential in later stages. Together, these findings frame curbside cafés as demand-shaping urban amenities with time-varying and design-contingent mobility co-benefits, informing when and how street experiments merit institutionalization for shifted street governance.

**Key Words**: active travel, street experiment, causal impact, curbside café, natural experiment, pandemic

# 1. Introduction

## 1.1 Pandemic Disruptions, Active Mobility, and Street Experiments

The COVID-19 pandemic constituted an unprecedented shock to urban mobility systems, rapidly destabilizing established travel practices and modal hierarchies (Hunter et al., 2024). Lockdowns and physical distancing mandates led to sharp declines in public transport use, while cycling and other active modes exhibited a faster rebound as alternatives to transit (Tabascio et al., 2023; Teixeira et al., 2024). Beyond short-term behavioral shifts, the pandemic exposed structural vulnerabilities embedded in car-oriented street systems, opening a critical window to reconsider how street space is allocated and governed in support of sustainability transitions (Hogan et al., 2022; Verhulst et al., 2023). In North America, the persistence of hybrid and remote work arrangements has further reinforced the relevance of this shift by reshaping neighborhood-scale travel patterns and raising questions about how pandemic-induced changes in active mobility may be sustained in the long term (Younes et al., 2024).

A growing body of research suggests that changes in active travel behavior during the pandemic cannot be fully understood without considering the concurrent transformation of street space itself (Hunter et al., 2024). In response to public health imperatives and rising demand for outdoor activities, cities worldwide rapidly reallocated roadway space through a wide range of temporary interventions, including pop-up bike lanes, pedestrianized streets, curbside dining, and traffic calming measures (Combs & Pardo, 2021; Zhao et al., 2024). These interventions exemplify what has been conceptualized as street experiments: intentional and temporary changes to street use, regulation, or form, aimed at testing pathways away from “streets for traffic” toward “streets for people” (Bertolini, 2020).

Street experiments are typically low-cost, temporary, rapidly implemented, and experimental in nature; they function as transition experiments within sustainability transition theory (Glaser & Krizek, 2021). Compared to pre-pandemic initiatives, COVID-era street experiments were more spatially extensive, longer lasting, and politically ambitious, enabled by emergency governance and expedited approval processes (Glaser & Krizek, 2021; Verhulst et al., 2023). Although many interventions were framed as temporary crisis responses, a substantial share (~50%) has since been retained or formalized, signaling their potential to shape post-pandemic mobility trajectories and street governance (Kutela et al., 2022; VanHoose, 2023). These developments raise a critical question: do temporary street experiments generate lasting mobility benefits that justify institutionalization?

## 1.2 Curbside Dining Program and Active Travel

Among pandemic-induced street experiments, curbside dining programs represent a particularly consequential yet understudied form of intervention (Brody et al., 2024). By converting curb lane parking spaces into outdoor dining areas, cities simultaneously addressed public health concerns, supported local economic recovery, and altered the functional and perceptual qualities of street environments. These interventions operate within curb zones—contested spaces where mobility, commerce, and social life intersect—making them especially relevant for understanding trade-offs and synergies in street reallocation. Despite early projections that pandemic-related economic recovery programs would be transient (Kutela et al., 2022), many have successfully transitioned into permanent frameworks.

Across North America, cities including Chicago, New York, Los Angeles, Seattle, and municipalities in New Jersey implemented curbside café initiatives during the pandemic, transforming underutilized parking infrastructure into vibrant “streateries” (Gregg et al., 2022; Mandhan & Gregg, 2023). A parallel development occurred in Toronto, Canada, where the CaféTO program emerged as a prominent example of scale and institutionalization (Brody et al., 2024). Launched in 2020 as an emergency economic recovery measure, CaféTO repurposed extensive curb space for outdoor patios serving hundreds of restaurants citywide, and was renewed annually before being made permanent as an annual program (Brody et al., 2024; City of Toronto, 2021), reflecting both political support and perceived public value. Beyond their immediate economic function, these installations have reshaped streetscapes by fostering social interaction and public-space activation, aligning with social capital theory (Mandhan & Gregg, 2023).

Despite the documented economic and social benefits of curbside dining (Rossmore, 2023), its implications for active mobility remain under-researched. While survey data suggest that pedestrian-oriented layouts and heightened street vitality may incentivize walking and cycling (Noland et al., 2023)—with municipal reports confirming significant cyclist patronage (City of Toronto, 2021)—empirical evidence remains fragmented and largely descriptive. Although a recent study investigated the correlation between café design and bikeshare usage, it focused solely on the 2020 implementation stage, precluding an assessment of long-term causal impacts (Song et al., 2024). Similarly, citywide analyses of pandemic-era street experiments report aggregate increases in walking but lack the resolution needed to evaluate cycling behavior and its temporal dynamics longitudinally (Hunter et al., 2024).

Consequently, there remains a critical lack of causal, longitudinal evidence linking curb space reallocation to changes in cycling behavior, as well as a limited understanding of how specific design attributes—such as patio width layout, and protective infrastructure—condition these effects. Addressing this gap is crucial for determining whether curbside dining primarily serves as a temporary placemaking response or as a meaningful catalyst for longer-term transitions toward sustainable urban mobility.

### 1.3 Research Objectives and Contributions

Building on the literature on street experiments and active mobility, this study examines Toronto’s CaféTO curbside dining program as a large-scale, real-world natural experiment to evaluate its causal impacts on bikeshare ridership from 2020 to 2022. While outdoor dining may foster active travel by reshaping the perception of street comfort (Noland et al., 2023), robust evidence based on longitudinal behavioral data remains scarce.

This study makes three key contributions. First, it provides the first city-scale, multi-year quasi-experimental assessment of a curbside dining program’s impacts on cycling, capturing how effects evolve across distinct pandemic and post-pandemic phases. Second, by accounting for spatial dependencies and spillover effects within a docked bikeshare network, the analysis advances causal inference on street interventions beyond the restrictive independence assumptions of conventional models by adopting a spatial difference-in-differences (SDID) framework. Third, by disaggregating effects by day type and integrating high-resolution design metrics, the study reveals how the physical form and temporal context of curbside cafés shape mobility responses. By positioning CaféTO as a critical case of pandemic-era street experimentation, this research offers empirical insight into when and how curb-space interventions can support active mobility and inform the institutionalization of people-centered street governance.

## 2. Data and Materials

### 2.1. Study Area

As Canada’s primary financial and technological hub, Toronto offers a high-density, compact urban environment that is conducive to active mobility research (Xu, 2019). With a metropolitan population approaching seven million by 2020, the city’s established urban form and robust docked bikeshare network offer a rigorous setting for evaluating pandemic-induced shifts in transit behavior. Specifically, Toronto’s rapid deployment of adaptive street experiments, most notably the CaféTO curbside dining program, provides a unique quasi-experimental context to examine the interplay between reallocated curb space and micro-mobility patterns during a period of systemic urban transition.

### 2.2 Active Travel Behavior - Bikeshare Ridership Frequency

We quantify active travel behavior using daily station-level ridership frequency from Bike Share Toronto. The longitudinal dataset includes geospatial coordinates (origins/destinations), durations, and high-resolution timestamps. To ensure data integrity, we excluded anomalous trips (<1 min or >45 min) and synchronized mobility data with local meteorological records to mitigate exogenous variance. Inspired by El-Assi et al. (2017), observation days with precipitation exceeding 5mm were removed to control for weather-induced bias.

To capture the divergence in travel patterns, we disaggregated the analysis into weekday and weekend cohorts. The primary outcome metric was the aggregated daily origin-destination (O-D) trip frequency per station, calculated on a monthly basis. The study area was restricted to Toronto’s core service area, resulting in a panel of 539, 558, and 595 stations for 2020, 2021, and 2022, respectively.

To establish causal inference via Difference-in-Differences, we defined May and June of each year (2020–2022) as the pre-treatment window to validate the parallel trends assumption. Given the implementation of CaféTO in July, the treatment effect was evaluated during the post-treatment period of August and September. The spatial distributions of ridership frequency for representative periods (e.g., June and September 2020) are illustrated in **Figure 1**, with descriptive statistics presented in **Table 1**.

(a) Weekday of June

(b) Weekend of June

(c) Weekday of September

(d) Weekend of September

**Fig. 1** Daily ridership Frequency in 2020

**Table 1**. Descriptive statistics of all variables.

| **Variable** | **Description** | **2020 Mean (SD); N = 539** | **2021 Mean (SD); N = 558** | **2022 Mean (SD); N = 595** | **Data Source** |
|---|---|---|---|---|---|
| Weekday Frequency (May) | Daily O+D value | 15.47 (18.83) | 36.74 (37.66) | 49.5 (43.58) | Bikeshare Toronto |
| Weekend Frequency (May) | | 32.13 (53.36) | 58.27 (82.77) | 58.07 (61.62) | |
| Weekday Frequency (June) | | 35.41 (41.25) | 41.77 (38.97) | 61.46 (54.77) | |
| Weekend Frequency (June) | | 59.37 (93.18) | 70.42 (92.39) | 71.99 (75.38) | |
| Weekday Frequency (August) | | 49.72 (46.34) | 49.66 (46.71) | 70.95 (64.06) | |
| Weekend Frequency (August) | | 66.28 (80.16) | 58.26 (65.21) | 83.1 (88.08) | |
| Weekday Frequency (September) | | 44.31 (36.49) | 49.28 (44.57) | 65.26 (59.77) | |
| Weekend Frequency (September) | | 62.66 (69.88) | 55.9 (58.79) | 69.16 (67.61) | |
| Sum_collector | Total length of the collector road (km) | 1.24 (0.92) | 1.23 (0.92) | 1.22 (0.91) | Toronto Open Data Portal |
| Sum_bikelane | Total length of the bike lane (km) | 1.91 (0.93) | 1.90 (0.92) | 1.92 (0.92) | |
| Sum_capacity | Total bike capacity in bikeshare stations | 19.09 (6.49) | 19.05 (6.47) | 19.01 (6.38) | |
| n_subway | Total subway stations | 7.83 (5.86) | 7.76 (5.85) | 7.78 (5.84) | |
| n_parks | Total number of parks | 7.94 (4.26) | 7.92 (4.23) | 7.87 (4.17) | |
| Pop_density | Population (1,000) | 10.27 (6.12) | 10.14 (6.13) | 10.15 (6.19) | Census Tract Data (2016) by Statistics Canada |
| Commute_bike | % of people who cycle to workplaces | 0.07 (0.04) | 0.07 (0.04) | 0.07 (0.04) | |
| Per_Wfh | % of people working from home | 0.1 (0.02) | 0.1 (0.03) | 0.1 (0.03) | |
| %Institutional | % Institutional land use | 0.05 (0.13) | 0.05 (0.13) | 0.05 (0.13) | Toronto land use map |
| %Employment | % Employment land use | 0.04 (0.1) | 0.04 (0.1) | 0.04 (0.1) | |
| %Regeneration | % Regeneration land use | 0.07 (0.16) | 0.07 (0.16) | 0.07 (0.16) | |
| %Openspace | % open space land use | 0.15 (0.18) | 0.15 (0.18) | 0.15 (0.18) | |
| %Apartment | % apartment land use | 0.07 (0.11) | 0.07 (0.11) | 0.06 (0.11) | |
| %Landuse_mix | Shannon Mix of different land use types | 1.4 (0.46) | 1.4 (0.45) | 1.4 (0.45) | |
| LENGTH_sum | Total length of curbside cafes (m) | 132.76 (158.0) | 134.38 (159.28) | 113.27 (130.75) | Provided by Urban Design and Transportation Consultants, variables were post-processed and mapped onto QGIS. |
| WIDTH_mean | Average width of curbside cafes (m) | 1.49 (1.01) | 1.68 (1.0) | 1.55 (1.01) | |
| Planter_su | Total number of movable planters | 18.24 (25.83) | 13.09 (24.74) | 3.75 (17.91) | |
| DeckArea | Total wooden deck area size (sqm) | N/A | 54.64 (95.21) | 7.43 (35.43) | |

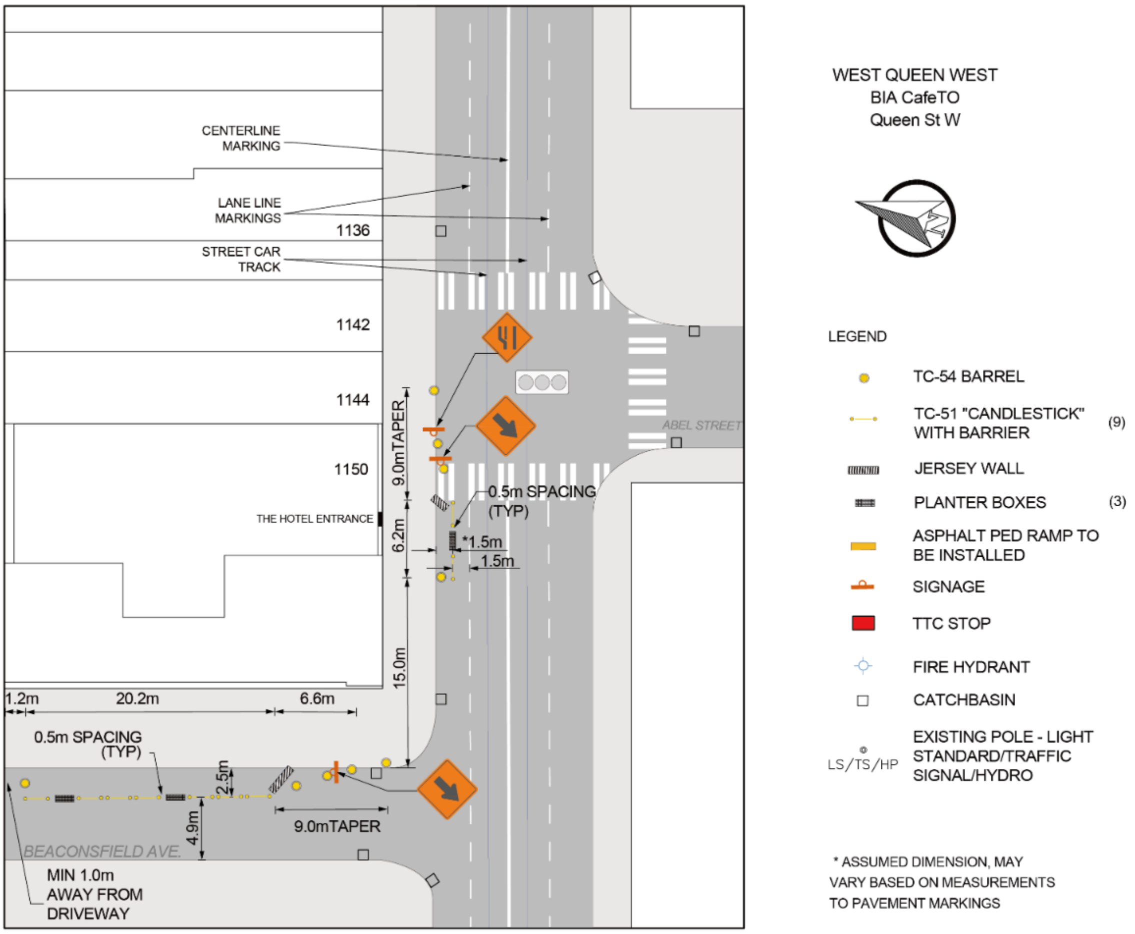
Notes: All variables have been aggregated to the 500m buffer around each bikeshare docking station.

## 2.2 CaféTO curbside dining program

The CaféTO curbside dining program is a crucial pandemic-era street experiment that repurposed empty curbside parking spaces left vacant by disrupted commuting patterns (Brody et al., 2024). The initiative was a collaboration between city planning and transportation staff, local Business Improvement Areas (BIAs), and consultants. Applications from restaurants are assessed to ensure the adjacent curb lane facing the facades can be safely converted into a temporary dining patio. In its 2020 launch, the city facilitated the program by waiving fees, expediting approvals, and providing infrastructure like safety barriers and planters. Owners were responsible for maintenance and furnishings (Mandhan & Gregg, 2023).

Regarding the timelines, the application was aimed for approvals by the end of June, and most patios were installed in July, with late applications implemented in August. Some sites may be revoked in August due to identified on-site traffic impacts, including congestion. The program was typically fully operational by September and dismantled by late October to prepare for winter snow removal.

Patio locations and dimensions were determined by consultant-led site assessments, with lengths generally restricted to restaurant frontages unless adjacent installations were consolidated (**Fig. 2**) or permission granted by adjoining property owners. To mitigate safety risks and potential confounding factors, the city implemented temporary speed limit reductions on affected road segments. We quantified the physical attributes of each curbside café, including venue dimensions and the structural elements such as planter boxes (delineation) and concrete jersey walls (safety), by manually incorporating these metrics into a GIS shapefile. These high-resolution design variables were subsequently aggregated at the bikeshare station level for subsequent analysis.



(a) Illustrative plan (redrawn by authors)

(b) Daytime photo; credit: Arcadis (c) Nighttime photo; credit: Arcadis

**Fig.2** Example design of curbside cafes

Figure 3 illustrates the geographic distribution of participating curbside cafés and their connection to BIAs in 2020, 2021, and 2022, with most of these cafés concentrated in downtown Toronto, particularly along major commercial thoroughfares such as Queen and Yonge Street.

(a) 2020 layout

(b) 2021

(c) 2022

**Fig.3** Layout of CaféTO curbside cafés in 2020-2022

### 2.3 Built Environment Variables

To provide an unbiased estimation of the curbside café program's effect, the analysis must control for built environment covariates known to influence cycling behavior. we adopted the widely recognized 5D framework (Ewing & Cervero, 2010), which has been previously used in bikeshare studies (Liang et al., 2023; R. Wang et al., 2020).

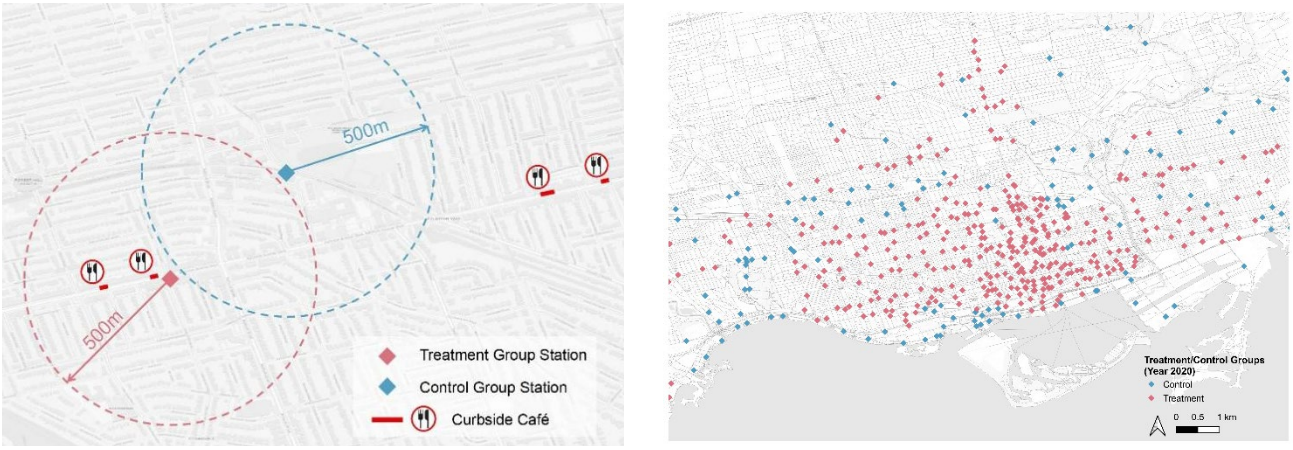
These variables were selected based on the data availability (descriptive statistics in Table 1). The variables include: 1) Density: Population density, percentage of cycling commuters, and percentage of population working from home (2016 Canadian Census). 2) Diversity: Land use proportions within a 500m walking buffer, including open space, institutional, employment, regeneration (mixed-use), and apartment neighborhoods, which can affect route choice (Fitch & Handy, 2020). 3) Design: docking capacity and the number of bikeshare stations. 4) Distance to Transit: Number of accessible subway stations. 5) Destination Accessibility: Number of parks (Faghih-Imani & Eluru, 2015).

## 3. Methodology

### 3.1 Assigning Treatment and Control Groups

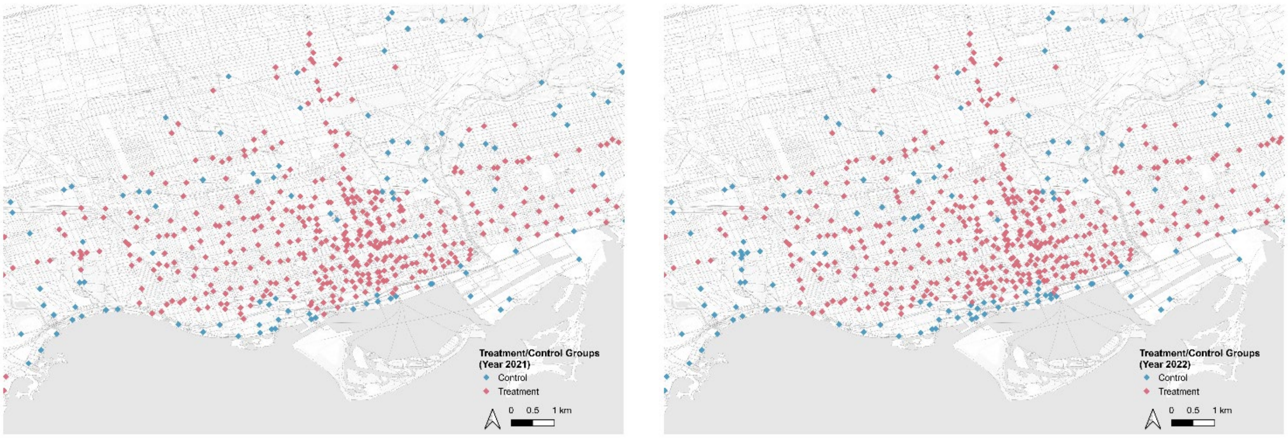
To establish causal inference between the CaféTO program and bikeshare usage, we categorized stations into treatment and control groups based on spatial proximity. Using a 500m catchment radius as a proxy for walking accessibility, we assigned a binary treatment indicator: stations with at least one curbside café within the buffer were designated as the treatment group (1), while those without were assigned to the control group (0) (see Fig. 4). The 500m buffer selection aligns with established literature on docked bikeshare catchments, which identifies 400–500 meters as the suitable threshold for walking accessibility to stations (Krizek, 2003; Wang & Akar, 2019). As shown in Fig. 4, a total of 384 treatment groups and 155 control groups were identified in 2020. In 2021, these numbers increased to 429 treatment and 129 control groups, while in 2022, 437 treatment groups and 158 control groups were defined. We hypothesize that these curbside cafés function as micro-destinations for bikeshare users, a premise supported by municipal data indicating that approximately 10% of CaféTO patrons arrived via cycling (City of Toronto, 2021).

Notably, prior studies have utilized Propensity Score Matching (PSM) to address the non-random assignment of the treatment (Salazar-Miranda et al., 2022; Yoshimura et al., 2022) and use the Difference-in-difference (DID) model to examine the causal impact. In this research, parallel trends were assessed through visual inspection of pre-intervention trends and using event-study specifications, including pre-treatment (May-June) lead terms (detailed explanations in **Appendix A**). Additionally, Welch T-tests were conducted between treatment and control groups on covariates to assess the potential disparities. Nevertheless, the presence of spatial spillover effects in bikeshare usage across nearby stations violates the Stable Unit Treatment Value Assumption (SUTVA) required by PSM, disqualifying the use of a standard DID model. Therefore, we propose an alternative spatial modeling approach to account for these spatial dependencies appropriately.



(d) Defining treatment and control stations

(e) 2020



(f) 2021

(g) 2022

**Fig.4** Illustration of treatment and control group assignment and spatial mapping

## 3.2 Model Specification- Fixed-effect DID and SDID Regression

Classical difference-in-difference regression (DID) models were first applied to evaluate the impact of temporary café interventions on bikeshare ridership before and after the implementation. DID offers a framework for causal inference in natural experiments by comparing panel data across treatment and control groups to establish a counterfactual scenario, and the model has been widely applied in prior urban studies (Choe et al., 2023; He et al., 2022). The model specification is written as Eq. (1):

$$Ridership_{it} = \beta_0 + \beta_1 Treatment_i + \beta_2 Time_t + \beta_3 Treatment_i * Time_t + \beta_4 Covariates_{ii} + \mu_i + \varepsilon_{ij} \quad (1)$$

where $Ridership_i$ is the weekday/weekend ridership frequency of the bikeshare docking station $i$, $\beta_1$ captures the net difference in ridership frequency between stations from the treatment and control groups, $\beta_2$ captures the net change of ridership frequency between before and after treatment stages, $\beta_3$ reflects the effects of the interaction term $Treatment_i * Time_t$, indicating the effects of the curbside café program, with a positive sign implying the positive causal treatment effect. $Covariates_i$ are vectors of covariates, in our case built environment variables that need to be explicitly controlled when estimating the impacts of the curbside café program. Lastly, due to the spatial distribution of curbside cafés, many treatment groups are concentrated in the downtown areas; a location fixed effect term $\mu_i$ is included to control for place-specific attributes and confounders, which may bias our estimation of the average treatment effect, while $\varepsilon_{ij}$ indicates the unobserved error term.

Nevertheless, bikeshare ridership exhibits strong spatial dependence due to the docked system design, where full or empty stations redirect users to nearby alternatives through the official app (Faghih-Imani & Eluru, 2015). Such interdependence violates the SUTVA underlying DID (Kolak & Anselin, 2020), and ignoring it can lead to inefficient and biased estimates (Diao et al., 2017). Such observations are further confirmed by Moran's I statistics, which showed positive spatial autocorrelation in bike ridership across all years at a 1% confidence interval. To account for these spillover effects between treatment and control stations, this study employs an SDID model that incorporates a spatially lagged ridership term, thereby enhancing causal inference in the presence of spatial interactions.

The proposed SDID model is a spatial regression model (see Eq. (2)) that incorporates a spatially lagged dependent variable (WY), building upon the previous DID model (Eq. (1)).

$$\mathrm{Y} = \rho\mathrm{WY} + \mathrm{X}\beta + \varepsilon \ (2)$$

In spatial regression analysis, the spatial weight matrix W must be specified a priori based on theoretical or empirical considerations. Common approaches include contiguity and k-nearest neighbors (KNN). This study adopts a KNN matrix with $k = 3$, assigning each station to its three closest neighbors. This specification reflects typical user behavior: when a station is full or empty, riders consult the bikeshare app to locate nearby alternatives. Distances to the third neighbor in 2020—approximately 391 m at the second quartile and 561 m at the third quartile—further justify that these distances fall within a reasonable range for cyclists seeking alternative docking options.

### 3.3 Model Architecture

After examining Variance Inflation Factors (VIF), variables with VIF values above 10 were removed to mitigate multicollinearity for final model fitting. The modeling framework consists of two parts. First, a set of classical fixed-effect DID models was estimated, beginning with treatment variables and subsequently adding covariates to reduce error variance (He et al., 2022). Second, we extended these models by incorporating spatial lag terms to construct SDID models that account for spatial dependence. Impacts were assessed by whether the treatment–time interaction terms remain statistically significant after covariate adjustment across six periods: 2020 weekday (Model 1d), 2020 weekend (Model 2d), 2021 weekday (Model 3d), 2021 weekend (Model 4d), 2022 weekday (Model 5d), and 2022 weekend (Model 6d).

### 3.4 Sensitivity Tests

To validate the robustness of our causal estimates against specific research design parameters, we conducted two sensitivity tests. First, we recalculated all station-level covariates and re-estimated the SDID models using an alternative 400 m (0.25 mile) walking buffer (Krizek, 2003) to ensure results were not artifacts of spatial scale. Second, to verify that identified treatment effects are independent of the spatial weight structure, we substituted the KNN specification with $K = 2$ . Consistency across these alternative spatial scales and neighborhood definitions can reinforce the robustness of our SDID framework, thereby validating the estimated impact.

### 3.5 Revealing the impact of curbside café patio design

While the SDID models provide robust causal estimates of the CaféTO program's overall impact, they do not reveal which specific patio design features shape active travel behavior, nor how these relationships may have evolved through (2020-2022) the pandemic (Gregg et al., 2022; Song et al., 2024). Understanding these micro-level design effects is essential for translating policy findings into actionable guidance for planning and urban design.

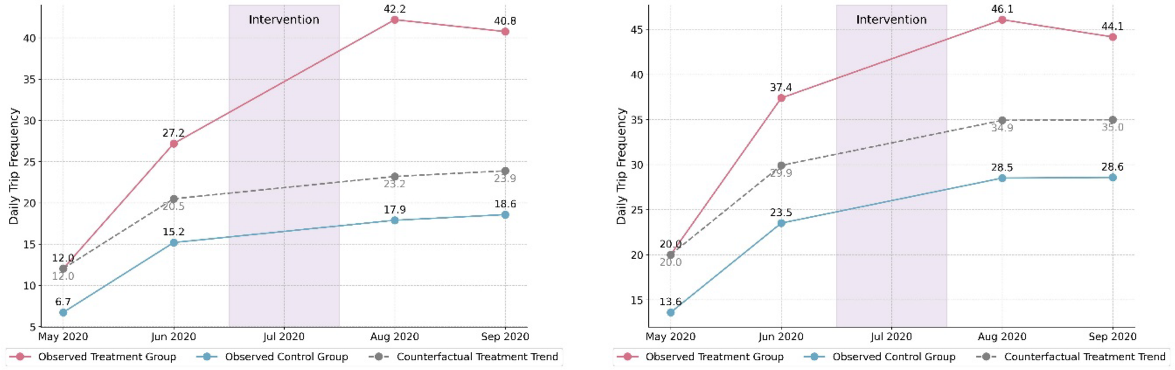
To address this gap, we conducted follow-up spatial regression analyses to examine how detailed patio design attributes, including mean patio width, total patio length, the number of planters, and the number of jersey barriers, were associated with bikeshare trip frequency in September, the post-implementation period for each year. Preliminary OLS results indicated significant spatial autocorrelation based on Moran's I, and Lagrange Multiplier (LM) diagnostics confirmed the presence of both spatial lag and spatial error dependence. Accordingly, we employed the Spatial Autoregressive Combined (SAC) model (Rey & Anselin, 2010), which incorporates both a spatially lagged dependent variable and a spatially autocorrelated error term, to more accurately estimate the relationships between patio design features and active travel outcomes.

## 4. Results

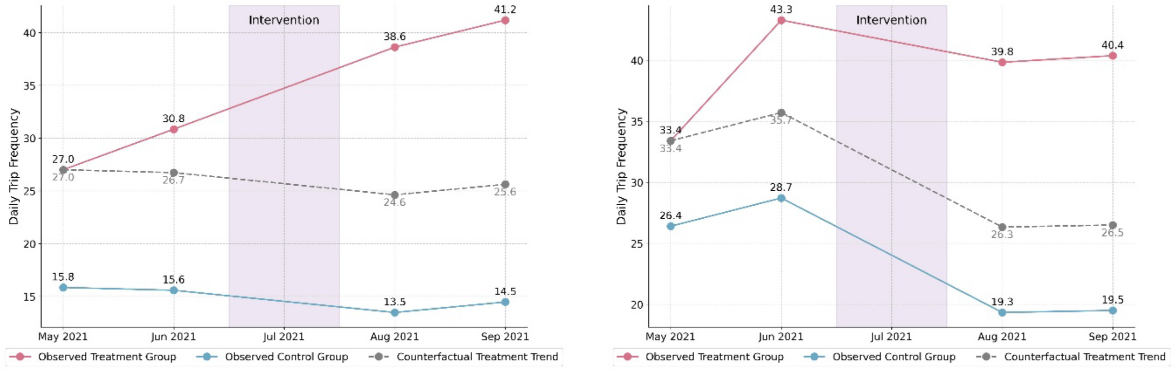
### 4.1. Descriptive Analysis

Because the compositions of the treatment and control groups varied across years, disparities between groups were assessed separately for each year. Comparisons using Welch T-tests reveal differences between groups on several, though not all, characteristics. In general, treatment groups were associated with a higher number of nearby subway stations, greater population density, a larger proportion of institutional land uses, and higher rates of bicycle commuting. By contrast, the proportion of regeneration land within the buffer areas was comparable between treatment and control groups. Control groups exhibited relative advantages in longer bike lane networks, larger bikeshare docking capacity, and higher proportions of open space and employment land.

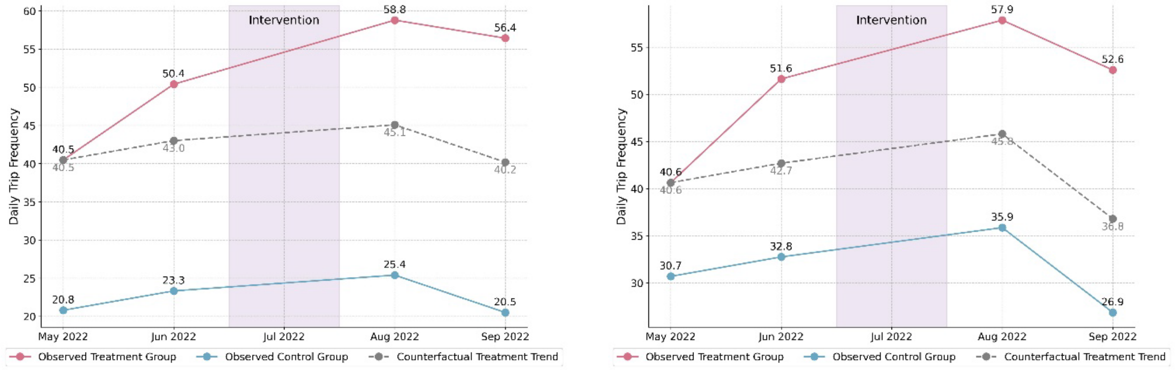
After visually inspecting the plot of trip frequencies (Fig. 5), it can be reasonably inferred that these two groups exhibit relative similarities and follow parallel trends, which is suitable for conducting DID analysis. Event study analyses were further conducted to assess the parallel-trends assumption across year–outcome combinations. **Appendix B** reports the estimated coefficients corresponding to the event-study specification described in methods. For 2020, pre-treatment lead coefficients were insignificant for weekday ridership but statistically significant for weekend ridership. In contrast, in 2021 and 2022, the pattern reversed, with insignificant leads for weekend travel and statistically significant but quantitatively small leads for weekday travel. This pattern suggests that detected pre-lead significance reflects short-term demand fluctuations rather than systematic differential trends between treated and control units.



(a) 2020 Weekday (b) 2020 Weekend



(c) 2021 Weekday (d) 2021 Weekend



(e) 2022 Weekday (f) 2022 Weekend

**Fig.5** (Median) trip frequency of the treatment and control groups

## 4.2. Estimating Average Treatment Effects

In terms of the results of the statistical models (full details refer to **Appendix Table C1-C3**), when accounting for covariates and location fixed effects (Model 1d-6d), the SDID model achieves a decent fit, explaining close to or more than half of the total variance ($R^2$ ranging from 0.439 to 0.562). For simplicity, the results for the d-series models are also presented in Table 2. The SDID models (Model 1d-6d) outperform the DID models (Model 1b-6b) in terms of explanatory power, considering spatial spillover effects, and improve variance explanation on average by 15.2% for weekdays and 14.1% for weekends. Furthermore, the SDID model exhibits increasing explanatory power over the years, demonstrating slightly better performance in explaining weekday trips compared to weekends in 2020. However, the pattern reverses for 2021 and 2022.

Regarding the causal effect size (see **Fig. 6** for grouped coefficients plot) of the curbside café interventions on bikeshare trip frequency between 2020 and 2022, the SDID models consistently identify a positive and statistically significant treatment effect on bikeshare activity across weekdays and weekends in 2020 and 2021, while no statistically significant effects are reported in 2022.

**Table 2.** SDID results (extracted from Appendix Table C1-C3)

| | **Model 1d** | **Model 3d** | **Model 5d** | **Model 2d** | **Model 4d** | **Model 6d** |
|---|---|---|---|---|---|---|
| **Attributes** | **Weekday** | | | **Weekend** | | |
| Year | 2020 | 2021 | 2022 | 2020 | 2021 | 2022 |
| Treatment | -0.36 | -1.35 | -4.57 | -5.19 | -18.15*** | -8.55 |
| T1 (August) | 1.32 | -1.63 | 4.16 | -4.63 | -21.02*** | 7.93 |
| T2 (September) | -4.34 | -4.64 | -2.85 | -9.01 | -24.01*** | -6.40 |
| T1 * Treatment | 6.61 | 5.64 | -1.08 | 10.05 | 20.02*** | -5.33 |
| T2 * Treatment | 10.75*** | 9.24** | 5.74 | 13.44* | 22.81*** | 7.02 |
| Sum_bikelane | -0.95 | -0.61 | -0.77 | -3.07* | -2.25 | -3.37** |
| Sum_capacity | 1.66*** | 1.66*** | 2.20*** | 3.17*** | 2.93*** | 3.14*** |
| n_subway | 0.004 | 0.75 | 2.65** | -2.23 | -0.65 | 0.23 |
| Pop_density | 0.86*** | 0.85*** | 0.75*** | 1.15*** | 1.08*** | 0.98*** |
| Commute_bike | 65.10*** | 30.47 | 82.45*** | 156.05*** | 93.41*** | 164.51*** |
| %Institutional | 15.90** | 23.04*** | 43.98*** | 18.09 | 14.05 | 24.95** |
| %Employment | 4.54 | -6.27 | -15.37 | 22.07 | -0.19 | -13.93 |
| %Regeneration | 22.02*** | 18.84*** | 39.18*** | 43.44*** | 31.60*** | 52.45*** |
| %Openspace | 35.14*** | 15.78*** | 0.28*** | 105.75*** | 59.52*** | 44.81*** |
| Constant | -41.19*** | -36.29*** | -40.97*** | -79.33*** | -50.98*** | -64.45*** |
| Spatial lag (wY) | 0.09*** | 0.63*** | 0.66*** | 0.63*** | 0.65*** | 0.67*** |
| Location Fixed effect (μ) | 0.56*** | 0.09*** | 0.08*** | 0.20*** | 0.18*** | 0.14*** |
| R-squared | 0.439 | 0.516 | 0.562 | 0.449 | 0.496 | 0.511 |
| N (sample) | 539 | 558 | 595 | 539 | 558 | 595 |

Notes: p value *<0.1, ** p<0.05, *** p<0.01.

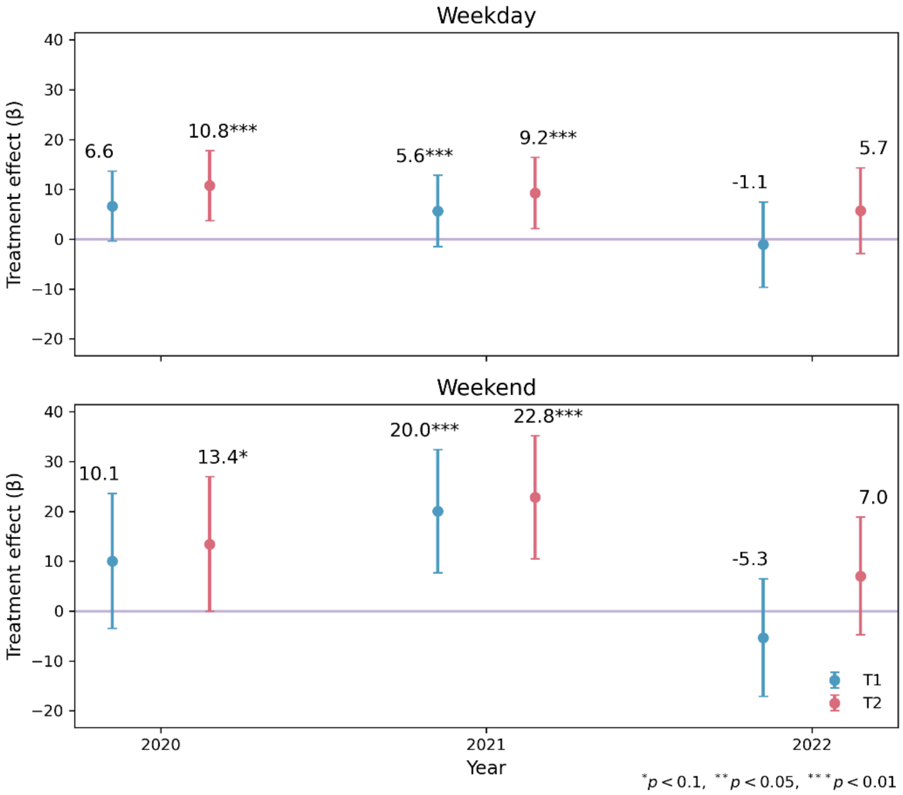


**Fig.6** Treatment effect size from 2020 to 2022.

Temporal heterogeneity further reveals that program impacts were more pronounced in the late-season implementation period (T2), following the completion of installations and subsequent design adjustments across all three years. In August (T1), statistically significant effects were largely absent, except for weekends in 2021 (20.02, $p < 0.05$). During September (T2), weekday impacts are pronounced in 2020, with daily trips increasing by 10.75 ($p < 0.01$), but diminish in 2021 (9.24, $p < 0.05$) and become statistically insignificant by 2022 (5.74, n.s.). Weekend responses are consistently stronger than weekday effects within the same year, reaching 13.44 ($p < 0.10$) in 2020 and peaking at 22.81 ($p < 0.01$) in 2021, before declining sharply in 2022 (7.02, n.s.).

Additionally, accounting for spatial dependence substantially reduces estimated effect sizes relative to conventional DID models (b series), underscoring the importance of spatial adjustment for accurate causal inference. For instance, for September (T2) weekdays in 2020, the average treatment effect declines from 19.67 ($p < 0.01$) in the DID model to 10.75 ($p < 0.01$) once spatial spillovers are incorporated (Model 1d). A similar pattern is observed on weekends, where effect estimates decrease from 23.38 ($p < 0.05$) to 13.44 ($p < 0.10$).

## 4.3. Effects of Covariates

Regarding covariates (**Table 2**), the proportions of open space and regeneration were strongly and positively associated with bikeshare trip frequency across all years, with open space showing the highest magnitude among all land use types in 2020, but exhibiting a reduced impact in subsequent years. Given evidence that access to large parks is positively associated with life satisfaction and lower mental stress (Mouratidis & Yiannakou, 2022), the pandemic heightened the importance of accessible open spaces, which have also been shown to encourage cycling within and around urban cores (Shaer et al., 2021). Institutional land use significantly influenced active travel during weekdays, although its effect on weekends was less consistent. Conversely, employment areas remained statistically insignificant throughout the study period.

Among other built environment factors, as expected, population density, bike station capacity, and the bike-commuting ratio are consistently positive predictors of trip frequency. This aligns with the findings that people who often cycle continued to do so during the pandemic (Qu et al., 2022). Nevertheless, infrastructure-related variables, such as bike lane length, largely show inconsistent and insignificant effects during the pandemic, while the number of subway stations exhibits a significantly positive association with trip frequency only on weekdays in 2022.

## 4.4. Sensitivity Tests of SDID regression

To confirm robustness, we adjusted the treatment assignment using a 400m buffer (**Appendix Table C4**). This reclassification led to the following station counts (Treatment/Control): 334/205 in 2020, 372/186 in 2021, and 390/205 in 2022. Sensitivity analyses confirm the robustness of our causal estimates across varying spatial specifications. Although the 400m buffer models exhibit a marginal decrease (~ 5%) in goodness-of-fit and a slight reduction in effect magnitude—for instance, the 2021 weekend average treatment effect shifts from 22.81 ($p < 0.01$) to 16.13 ($p < 0.05$)—the directionality and temporal trends remain entirely consistent. The positive causal impact continues to show a downward trajectory on weekdays (2020–2021) and a peak-then-fade pattern on weekends (2021–2022). Furthermore, re-estimation using an alternative spatial weight structure ($k = 2$) yields significant interaction terms of comparable magnitude. These consistent results across alternative spatial scales and neighborhood definitions validate our primary research design and reinforce the stability of the identified treatment effects.

## 4.5. The impact of CaféTO design configurations

This research further elaborated on four curbside café design metrics to investigate how the configuration and characteristics may contribute to cycling behavior while controlling the impact of built environment variables (**Table 3**). Among design metrics, patio width shows the most immediate effect, with positive and significant associations in 2020 for both weekdays and weekends, where a one-meter increase in width corresponds to approximately 9–10% higher cycling activity. However, this effect attenuated and became insignificant in later years, suggesting diminishing marginal returns as mobility patterns stabilize and its simple presence is no longer enough to sustain bikeshare usage. In contrast, patio length exhibits a delayed influence, remaining insignificant in 2020 but becoming positive and significant in 2021 and 2022. An additional 100 linear meters of café frontage is associated with a 5–7% increase in cycling, indicating that larger and more continuous café footprints begin to affect travel behavior as the program matures. Planters and deck platforms exhibit consistently weak or insignificant effects across all years, suggesting a limited direct influence on cycling demand. Overall, these results highlight the temporal evolution of design effects, shifting from immediate spatial reallocation to longer-term corridor and destination functions.

**Table 3.** Regression results and diagnosis (2020-2022).

| | **Model 9a** | **Model 9b** | **Model 9c** | **Model 10a** | **Model 10b** | **Model 10c** |
|---|---|---|---|---|---|---|
| **Attributes** | **Weekday** | | | **Weekend** | | |
| Year | 2020 | 2021 | 2022 | 2020 | 2021 | 2022 |
| Length_sum | 0.0003 | 0.00067*** | 0.0006*** | 0.0003 | 0.00071** | 0.00046* |
| Width_mean | 0.0882*** | 0.03652 | -0.0215 | 0.0976*** | 0.0500 | -0.01538 |
| Planter_sum | 0.0011 | -0.00106 | -0.00136 | 0.00159 | -0.0016 | -0.00056 |
| DeckArea | / | -0.00006 | 0.00028 | / | -0.00017 | -0.00015 |
| Sum_bikelane | 0.0464* | 0.00004 | 0.00002 | 0.00003 | 0.00002 | 0.00003 |
| Sum_capacity | 0.0255*** | 0.02232*** | 0.0024*** | 0.0272*** | 0.02664*** | 0.0193** |
| n_subway | 0.0564* | 0.06774** | 0.0683** | 0.0248 | 0.00186 | 0.0134 |
| Pop_density | 0.0189*** | 0.00001** | 0.00001** | 0.0001*** | 0.00001 | 0.0000 |
| Commute_bike | 2.8398*** | 2.10126*** | 2.2471*** | 2.791*** | 1.41105 | 0.9408 |
| %Institutional | 0.5903*** | 0.53393*** | 0.618*** | 0.452** | 0.18691 | 0.21336 |
| %Employment | 0.27706 | 0.10404 | 0.1004 | 0.4403 | -0.55855* | 0.3262 |
| %Regeneration | 0.6332** | 0.49812** | 0.6266*** | 0.7108*** | 0.2095 | 0.21292 |
| %Openspace | 0.6249*** | 0.2576* | 0.03745 | 1.2965*** | 0.56815** | 0.21642 |
| Constant | 0.1231 | -0.073 | -0.011 | 0.126 | -0.281 | 0.112 |
| Spatial lag (wY) | 0.5628*** | 0.721*** | 0.721*** | 0.5808*** | 0.8158*** | 0.909*** |
| lamda | -0.208*** | -0.381*** | -0.345*** | -0.201*** | -0.759*** | -0.99*** |
| Pseudo R-squared | 0.568 | 0.681 | 0.762 | 0.535 | 0.365 | 0.385 |
| N (sample) | 539 | 558 | 595 | 539 | 558 | 595 |

Notes: p value * <0.1, ** p <0.05, *** p <0.01. All dependent variables were log-transformed.

## 5. Discussion

### 5.1. Causal effects of curbside café on active travel through the pandemic

This study offers the first longitudinal, causal evidence that the CaféTO curbside café program influenced bikeshare usage during the COVID-19 pandemic, while revealing pronounced temporal heterogeneity in its effects over time. Consistent with the rationale of the pandemic-induced street experiments literature (Glaser & Krizek, 2021; VanHoose, 2023), reallocating curb space for outdoor dining was found to causally increase active travel, particularly cycling during periods of disrupted mobility in 2020 and 2021. It enriches the existing empirical evidence, which solely reported the positive impact on walking (Hunter et al., 2024). However, these effects were not stable over time and varied systematically across weekdays and weekends, reflecting distinct behavioral mechanisms.

The stronger weekday effects observed in 2020 likely reflect the reconfiguration of daily mobility under widespread work-from-home arrangements and reduced peak-hour commuting in the early pandemic era (Younes et al., 2024). Under these conditions, weekday travel became more discretionary and less constrained by rigid commuting schedules, rendering cyclists more responsive to changes in street environments. In this context, curbside cafés may have supported cycling primarily by shaping optional leisure or non-mandatory trips (Teixeira et al., 2024) rather than influencing routine, necessity-driven commuting behavior. Its attenuation in 2021 and eventual disappearance of impacts by 2022 likely reflect the gradual normalization of weekday mobility in Canada, during which cycling increasingly faced competition from other transport modes, reducing the relative salience of curbside cafés in shaping travel choices.

In contrast, the most pronounced weekend effects observed in 2021 point to a different underlying mechanism. As vaccination uptake increased and restrictions on indoor dining were gradually eased—albeit with intermittent extensions during successive waves of the virus (McNee, 2022)—previously suppressed leisure and social activities began to rebound. At the same time, curbside cafés benefited from institutional learning and iterative refinement following their initial deployment in 2020, resulting in more standardized designs, improved infrastructure aesthetics, and greater public familiarity. Under these conditions, curbside cafés increasingly served as focal points for activity, thereby enhancing the destination's attractiveness. In this phase, cycling was encouraged as part of broader social and recreational activities, rather than as a purely utilitarian means of transportation.

Notably, the subsequent attenuation of bikeshare usage effects by 2022 does not necessarily indicate a failure of policy relevance. Instead, it suggests that the mobility impacts of curbside cafés were most salient under conditions of constrained choice and heightened sensitivity to public space quality. As everyday mobility patterns normalized and provincial restriction orders were lifted in April 2022 (McNee, 2022), the travel behavior in 2022 became more structured and predictable, a shift also reflected in the progressively improving explanatory performance of the models over time. Under these more stable conditions, the role of curbside cafés shifted toward their primary objectives—supporting local businesses, fostering social interaction, and enhancing street-level vibrancy—within which cycling benefits may be understood as an added co-benefit rather than a sustained core outcome. This interpretation aligns with the program's continued institutionalization and annual implementation, underscoring its broader contribution to urban livability beyond measurable changes in cycling demand (Mandhan & Gregg, 2023). Taken together, these findings suggest that curbside cafés operate less as direct mobility infrastructure and more as demand-shaping urban amenities, whose influence on cycling depends on broader temporal, social, and institutional contexts.

## 5.2. Practical Implications

Our findings provide actionable design insights for policymakers and urban designers by quantifying how specific curbside café metrics influence active travel, particularly cycling, which remains comparatively marginal in many North American cities compared to European contexts. Beyond their causal impacts, curbside cafés demonstrate measurable spatial leverage: a 1-m increase in patio width is associated with an approximately 10% increase in bikeshare usage in 2020. Achieving a comparable weekday vitality gain through land-use interventions would require roughly an 8% expansion of parkland within the walking buffer, a process typically constrained by lengthy approvals, high costs, and land acquisition challenges. In contrast, curbside cafés, as a key example of street experiments, are characterized by low-cost, rapidly deployable, and spatially efficient—capable of complementing longer-term urban regeneration strategies than revising land uses. As participation is largely voluntary through local BIAs, prioritizing streets with sufficient right-of-way capacity and active commercial frontage may maximize gains in active mobility. More importantly, when paired with the city's ambitious infrastructure expansion plans, reallocating curb space for outdoor dining can advance the complete streets vision by creating more pedestrian-friendly street profiles and helping to reduce inequities in active travel that stem from uneven infrastructure provision while potentially strengthening residents' intentions to adopt cycling over time (Tabascio et al., 2023).

While planters and wooden platforms showed limited direct impacts on travel behavior, these elements should not be evaluated solely in terms of mobility outcomes. Planters, for instance, enhance visual quality, spatial definition, and perceived safety while providing buffering from adjacent traffic. Survey evidence indicates that users value decorative and protective features for improving comfort, accessibility, and inclusivity (City of Toronto, 2021); otherwise, such street experiments might be perceived as a "depressing eyesore," a concern raised in cities like New York City that may negatively affect housing premiums (O'Brien, 2021). As the CaféTO program has been institutionalized and repeated annually since the pandemic, integrating these elements through refined and durable design—rather than ad hoc deployment—may enhance their long-term contribution to street quality and indirectly support active travel. This interpretation aligns with evidence from Barcelona, where structurally integrated interventions have been shown to foster more sustained social interaction than temporary tactical measures that are vulnerable to vandalism and limit long-term community appropriation (Morales-Flores & Marmolejo-Duarte, 2025). Overall, these results suggest that curbside cafés function most effectively when designed as adaptable public space systems, where mobility benefits emerge as co-benefits of broader improvements in urban livability, rather than as standalone objectives.

As suggested by VanHoose et al. (2022), translating temporary street arrangements into permanent change requires assessing their transitional capacity through a multi-level perspective that links local experiments to broader institutional, behavioral, and infrastructural regimes. In this sense, the pandemic shock did not simply disrupt urban mobility but created a critical window to rethink urbanism through feasible, street-level interventions that could enhance long-term resilience (Jasiński, 2022). Arguably, within automobile-prioritized street environments shaped by modernist planning paradigms, pandemic-induced street experiments—such as curbside cafés—have demonstrated their potential to challenge entrenched norms of traffic dominance and to inform longer-term systemic shifts in urban mobility governance and street design, particularly when embedded within supportive policy frameworks and repeated over time (Bertolini, 2020).

## 6. Conclusion

In this paper, we utilize Toronto's CaféTO program as a natural experiment to investigate whether reallocating curb space for outdoor dining can influence active travel throughout the COVID-19 pandemic. Using a longitudinal causal framework that accounts for spatial dependence, our proposed spatial difference-in-difference model shows that curbside cafés can generate measurable benefits in promoting bikeshare usage, but that these impacts are strongly time- and context-dependent. The pattern of effects suggests that street reallocations matter most when everyday mobility is disrupted and people are susceptible to the quality of public space, whereas their direct influence on cycling becomes less salient as travel routines stabilize.

Beyond exploring whether curbside dining "works", our findings highlight when and under what design conditions such interventions are most likely to matter. Design parameters that determine the amount and configuration of reallocated curb space appear to condition observed activity responses, suggesting that street experiments may be most effective when treated as iteratively refined public-realm interventions that complement longer-horizon investments in parks, streetscapes, and mobility infrastructure. While questions of institutionalization and system change exceed the empirical scope of this study, scholars suggest that repetition and policy embedding may help translate temporary reallocations into more durable outcomes in promoting active travel in the long run.

While our study offers an evaluation of the causal impacts of the curbside dining program on bikeshare usage, we acknowledge several limitations. First, the sidewalk café, as another critical category of outdoor dining, could have also contributed to active travel during the pandemic (Brody et al., 2024) and its impact was likely reflected in the omitted variables in this study. Second, our SDID specification focuses on average treatment effects; future studies should examine heterogeneity across neighborhoods and street contexts. Finally, replication in other cities with comparable curbside dining programs is necessary to test generalizability and clarify how design and governance jointly shape the impacts of equity, livability, and active travel over time.

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# Appendix

## Appendix A Event Study Analysis

To examine the dynamic effects of the CaféTO intervention and to formally assess the parallel-trends assumption underlying the difference-in-differences (DID) framework, we estimated an event-study specification that allows treatment effects to vary over time relative to the intervention.

Let $i$ index bike stations and $t$ index time periods. The event-study model is specified as:

$$Y_{it} = \alpha + \beta \cdot Treat_i + \sum_{k \neq -1} \gamma_k \cdot D_t^k + \sum_{k \neq -1} \delta_k \cdot \left(Treat_i \times D_t^k\right) + \varepsilon_{it}$$

where $Y_{it}$denotes bikeshare ridership at station $i$ in period $t$, estimated separately for weekday and weekend travel for different years ; Treat$_i$is an indicator equal to 1 for stations exposed to the CaféTO program and 0 otherwise; $D_t^k$is a set of event-time dummy variables indicating the number of months relative to the intervention; $k = -1$(June) is omitted as the reference (baseline) period; $\delta_k$captures the dynamic treatment effect at event time $k$; $\varepsilon_{it}$is an idiosyncratic error term.

Given in our case that the intervention occurred annually in July, which is not directly observed in the outcome data. Accordingly, event time is defined as:

- $k = -2$: May (two months before intervention),
- $k = -1$: June (one month before intervention, baseline),
- $k = +1$: August (one month after intervention),
- $k = +2$: September (two months after intervention).

Standard errors are clustered at the station level to account for serial correlation in station-specific ridership patterns.

Formally, identifying assumption requires lead term:

$$\delta_{-2} = 0,$$

indicating no differential change between treated and control stations from May to June.

# Appendix B Event Study Result

**Table B1.** Event Study results for 2020-2022

| | **2020 Weekday** | **2020 Weekend** | **2021 Weekday** | **2021 Weekend** | **2022 Weekday** | **2022 Weekend** |
|---|---|---|---|---|---|---|
| Treat × May (Lead) | 4.13 (3.03) | 18.56*** (5.98) | -10.29*** (1.26) | -2.55 (3.56) | -4.05** (1.62) | -2.91 (2.85) |
| Treat × August | 13.81*** (1.66) | 18.80*** (3.80) | 10.33*** (1.28) | 35.47*** (5.97) | 0.16 (1.47) | -8.12*** (2.76) |
| Treat × September | 19.67*** (2.66) | 23.38*** (4.62) | 18.38*** (2.19) | 42.17*** (7.14) | 12.85*** (1.80) | 12.96*** (3.42) |
| Treat (baseline) | −6.20 (5.16) | -38.93***(12.34) | -2.87 (5.29) | -52.14*** (14.56) | -8.20 (5.78) | -19.71** (9.46) |
| May | −22.88*** (2.90) | −40.46*** (5.85) | 2.88** (1.17) | -10.28*** (3.47) | -8.98*** (1.50) | -11.78*** (2.74) |
| August | 4.46** (1.36) | -6.48* (3.60) | -0.05 (1.69) | -39.42*** (5.89) | 9.37*** (1.19) | 17.07*** (2.58) |
| September | −5.11** (2.48) | −13.36*** (4.43) | -6.62*** (2.01) | -46.94*** (7.02) | -5.64*** (1.38) | -12.34*** (3.23) |
| Constant | 39.83*** (4.94) | 87.10*** (12.06) | 43.97*** (5.07) | 110.50*** (14.36) | 55.42*** (5.27) | 86.47*** (9.10) |
| R-squared | 0.119 | 0.053 | 0.028 | 0.052 | 0.028 | 0.030 |

Notes: p value *<0.1, ** p<0.05, *** p<0.01. Treat × May serves as a pre-treatment lead to assess the parallel-trends assumption. June is the omitted reference period.

# Appendix C DID and SDID Results

**Table C1.** Regression results and diagnosis (2020, N=539).

| | **Model 1a** | **Model 1b** | **Model 1c** | **Model 1d** | **Model 2a** | **Model 2b** | **Model 2c** | **Model 2d** |
|---|---|---|---|---|---|---|---|---|
| **Attributes** | **Weekday** | | | | **Weekend** | | | |
| **Method** | DID | DID | SDID | SDID | DID | DID | SDID | SDID |
| **Intervention (curbside dining program)** | | | | | | | | |
| Treatment | -5.74 | -0.61 | -5.35* | -0.36 | -37.80*** | -7.00 | -19.88*** | -5.19 |
| T1 (August) | 4.46 | 4.46 | 0.23 | 1.32 | -6.48 | -6.48 | -3.89 | -4.63 |
| T2 (September) | -5.11 | -5.11 | -4.08 | -4.34 | -13.36 | -13.36* | -7.25 | -9.01 |
| T1 * Treatment | 13.81** | 13.81*** | 4.09 | 6.61 | 18.80* | 18.80** | 6.52 | 10.05 |
| T2 * Treatment | 19.67*** | 19.67*** | 7.64* | 10.75*** | 23.38** | 23.38*** | 9.43 | 13.44* |
| **Covariates** | | | | | | | | |
| Sum_bikelane | / | -1.93* | / | -0.95 | / | -6.38*** | / | -3.07* |
| Sum_capacity | / | 2.07*** | / | 1.66*** | / | 4.01*** | / | 3.17*** |
| n_subway | / | 1.69 | / | 0.004 | / | -2.25 | / | -2.23 |
| Pop_density | / | 1.68*** | / | 0.86*** | / | 2.12*** | / | 1.15*** |
| Commute_bike | / | 100.27*** | / | 65.10*** | / | 223.81*** | / | 156.05*** |
| %Institutional | / | 38.73*** | / | 15.90** | / | 37.27*** | / | 18.09 |
| %Employment | / | 5.43 | / | 4.54 | / | 36.32** | / | 22.07 |
| %Regeneration | / | 55.37*** | / | 22.02*** | / | 107.46*** | / | 43.44*** |
| %Openspace | / | 60.07*** | / | 35.14*** | / | 184.83*** | / | 105.75*** |
| Constant | 26.92*** | -51.66*** | 7.57** | -41.19*** | 55.40*** | -98.33*** | 16.09*** | -79.33*** |
| Location Fixed effect (μ) | 0.18*** | 0.15*** | 0.08*** | 0.09*** | 0.44*** | 0.33*** | 0.18*** | 0.20*** |
| **Spatial terms** | | | | | | | | |
| Spatial lag (wY) | / | / | 0.76*** | 0.56*** | / | / | 0.80*** | 0.57*** |
| **Overall Performance** | | | | | | | | |
| R-squared | 0.056 | 0.305 | 0.349 | 0.439 | 0.064 | 0.341 | 0.354 | 0.449 |

Notes: p value *<0.1, ** p<0.05, *** p<0.01.

**Table C2.** Regression results and diagnosis (2021, N=558).

| | **Model 3a** | **Model 3b** | **Model 3c** | **Model 3d** | **Model 4a** | **Model 4b** | **Model 4c** | **Model 4d** |
|---|---|---|---|---|---|---|---|---|
| **Attributes** | **Weekday** | | | | **Weekend** | | | |
| **Method** | DID | DID | SDID | SDID | DID | DID | SDID | SDID |
| **Intervention (curbside dining program)** | | | | | | | | |
| Treatment | -2.14 | -3.57 | -4.09 | -1.35 | -50.61*** | -34.55*** | -25.04*** | -18.15*** |
| T1 (August) | -0.05 | -0.05 | -2.07 | -1.63 | -39.42*** | -39.42*** | -16.05** | -21.02*** |
| T2 (September) | -6.62 | -6.62 | -4.09 | -4.64 | -46.94*** | -46.94*** | -17.81** | -24.01*** |
| T1 * Treatment | 10.33* | 10.33** | 4.33 | 5.64 | 35.47*** | 35.47*** | 15.85** | 20.02*** |
| T2 * Treatment | 18.38*** | 18.38*** | 6.70 | 9.24** | 42.17*** | 42.17*** | 17.81** | 22.81*** |
| **Covariates** | | | | | | | | |
| Sum_bikelane | / | -1.26 | / | -0.61 | / | -5.91*** | / | -2.25 |
| Sum_capacity | / | 2.12*** | / | 1.66*** | / | 3.82*** | / | 2.93*** |
| n_subway | / | 4.53*** | / | 0.75 | / | 2.38 | / | -0.65 |
| Pop_density | / | 1.93*** | / | 0.85*** | / | 2.24*** | / | 1.08*** |
| Commute_bike | / | 84.46*** | / | 30.47 | / | 187.57*** | / | 93.41*** |
| %Institutional | / | 65.53*** | / | 23.04*** | / | 39.66*** | / | 14.05 |
| %Employment | / | -10.19 | / | -6.27 | / | -0.75 | / | -0.19 |
| %Regeneration | / | 66.25*** | / | 18.84*** | / | 115.58*** | / | 31.60*** |
| %Openspace | / | 32.28*** | / | 15.78*** | / | 114.59*** | / | 59.52*** |
| Constant | 29.54*** | -47.54*** | 6.87** | -36.29*** | 79.99*** | -52.77*** | 34.73*** | -50.98*** |
| Location Fixed effect (μ) | 0.19*** | 0.19*** | 0.07*** | 0.09*** | 0.41*** | 0.33*** | 0.15*** | 0.18*** |
| **Spatial terms** | | | | | | | | |
| Spatial lag (wY) | / | / | 0.81*** | 0.63*** | / | / | 0.79*** | 0.65*** |
| **Overall Performance** | | | | | | | | |
| R-squared | 0.045 | 0.355 | 0.440 | 0.516 | 0.085 | 0.345 | 0.414 | 0.496 |

Notes: p value *<0.1, ** p<0.05, *** p<0.01.

**Table C3.** Regression results and diagnosis (2022, N=595).

| | **Model 5a** | **Model 5b** | **Model 5c** | **Model 5d** | **Model 6a** | **Model 6b** | **Model 6c** | **Model 6d** |
|---|---|---|---|---|---|---|---|---|
| **Attributes** | **Weekday** | | | | **Weekend** | | | |
| **Method** | DID | DID | SDID | SDID | DID | DID | SDID | SDID |
| **Intervention (curbside dining program)** | | | | | | | | |
| Treatment | 10.01* | -5.60 | -3.92 | -4.57 | -16.65** | -15.01*** | -13.85** | -8.55 |
| T1 (August) | 9.37 | 9.37* | 2.69 | 4.16 | 17.07** | 17.07** | 5.56 | 7.93 |
| T2 (September) | -5.64 | -5.64 | -2.07 | -2.85 | -12.34* | -12.34** | -4.86 | -6.40 |
| T1 * Treatment | 0.16 | 0.16 | -1.43 | -1.08 | -8.12 | -8.12 | -4.60 | -5.33 |
| T2 * Treatment | 12.85* | 12.85** | 3.75 | 5.74 | 12.96 | 12.96 | 5.48 | 7.02 |
| **Covariates** | | | | | | | | |
| Sum_bikelane | / | -0.66 | / | -0.77 | / | -6.70*** | / | -3.37** |
| Sum_capacity | / | 2.75*** | / | 2.20*** | / | 3.98*** | / | 3.14*** |
| n_subway | / | 10.08*** | / | 2.65** | / | 4.93*** | / | 0.23 |
| Pop_density | / | 2.29*** | / | 0.75*** | / | 2.63*** | / | 0.98*** |
| Commute_bike | / | 186.37*** | / | 82.45*** | / | 340.05*** | / | 164.51*** |
| %Institutional | / | 130.59*** | / | 43.98*** | / | 76.93*** | / | 24.95** |
| %Employment | / | -22.05* | / | -15.37 | / | -20.76 | / | -13.93 |
| %Regeneration | / | 112.97*** | / | 39.18*** | / | 157.54*** | / | 52.45*** |
| %Openspace | / | 10.24 | / | 0.28*** | / | 92.23*** | / | 44.81*** |
| Constant | 29.54*** | -55.69*** | 7.09* | -40.97*** | 55.10*** | -80.86*** | 12.89** | -64.45*** |
| Location Fixed effect (μ) | 0.24*** | 0.17*** | 0.08*** | 0.08*** | 0.41*** | 0.27*** | 0.15** | 0.14*** |
| **Spatial terms** | | | | | | | | |
| Spatial lag (wY) | / | / | 0.85*** | 0.66*** | / | / | 0.84*** | 0.67*** |
| **Overall Performance** | | | | | | | | |
| R-squared | 0.037 | 0.400 | 0.494 | 0.562 | 0.057 | 0.347 | 0.432 | 0.511 |

Notes: p value *<0.1, ** p<0.05, *** p<0.01.

**Table C4.** SDID results using 400m-buffer

| | **Model 7a** | **Model 7b** | **Model 7c** | **Model 8a** | **Model 8b** | **Model 8c** |
|---|---|---|---|---|---|---|
| **Attributes** | **Weekday** | | | **Weekend** | | |
| Year | 2020 | 2021 | 2022 | 2020 | 2021 | 2022 |
| Treatment | -4.98 | 0.74 | -3.83 | -9.05 | -8.50 | -4.28 |
| T1 (August) | 1.81 | -0.55 | 2.96 | -4.05 | -14.71** | 5.81 |
| T2 (September) | -3.12 | -2.70 | -2.82 | -8.00 | -16.49*** | -5.03 |
| T1 * Treatment | 6.03 | 4.71 | 1.00 | 9.90 | 14.52** | -3.47 |
| T2 * Treatment | 9.86** | 7.59* | 6.55 | 13.39* | 16.13** | 5.93 |
| Sum_bikelane | 0.15 | 1.36 | 0.95 | -1.30 | 0.56 | -1.38 |
| Sum_capacity | 0.05*** | 0.05*** | 0.12*** | 0.04 | 0.05** | 0.07*** |
| n_subway | -1.17 | -0.85 | -1.42 | -1.43 | -1.21 | -1.43 |
| Pop_density | 0.72*** | 0.73*** | 0.47** | 1.09*** | 0.94*** | 0.74*** |
| Commute_bike | 57.83*** | 16.85 | 78.20*** | 116.23*** | 48.54 | 117.83*** |
| %Institutional | 7.66 | 15.07** | 30.04*** | 7.40 | 4.69 | 8.09 |
| %Employment | 6.16 | 1.93 | -4.36 | 23.17 | 13.92 | -1.19 |
| %Regeneration | 16.98*** | 11.21** | 22.22*** | 37.69*** | 19.34** | 32.80*** |
| %Openspace | 40.26*** | 28.20*** | 17.26*** | 116.98*** | 80.43*** | 67.21*** |
| Constant | -12.83*** | -13.99*** | -9.63* | -27.16*** | -15.92** | -19.83*** |
| Spatial lag (wY) | 0.53*** | 0.64*** | 0.63*** | 0.63*** | 0.68*** | 0.72*** |
| Location Fixed effect (μ) | 0.19*** | 0.19*** | 0.08*** | 0.20*** | 0.19*** | 0.16*** |
| R-squared | 0.386 | 0.469 | 0.516 | 0.401 | 0.441 | 0.451 |
| N (sample) | 539 | 558 | 595 | 539 | 558 | 595 |

Notes: p value *<0.1, ** p<0.05, *** p<0.01.

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