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Cycling in a Crisis: Employing Quasi-Experimental Designs to Estimate the Effects of Provisional Bicycle Infrastructure

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Abstract

During the Covid-19 pandemic, provisional infrastructure was implemented to support active transportation. However, the planning community still requires deeper understandings of the relationships between provisional infrastructure and mobility. We use the example of a roadway closure in Philadelphia to investigate how one active mode—bikeshare use—changed in response to provisional infrastructure. We employ differences-in-differences models that measure the semi-causal effect of provisional infrastructure on bikeshare trip durations and find that even during the pandemic, when trip durations increased across the bikeshare system, provisional infrastructure had an additive and statistically discernible impact above and beyond the pandemic effect.

Keywords

bikeshare, Covid-19, differences-in-differences, quasi-experiment

Introduction

Separated and safe bicycle facilities—such as bike lanes, protected bike lanes, or segregated bike paths (Furth 2021)—theoretically have the capacity to generate increased cycling in the population that can, in turn, yield positive outcomes for multiple priority areas in urban and transportation planning (Ryerson et al. 2021). Scholars have highlighted how new cyclist-serving infrastructure is associated with greater physical activity and theoretically positive health outcomes (Crane et al. 2017), reduced air pollution (Schmitz et al. 2021; Whitehurst et al. 2021), higher levels of utilitarian cycling (Heesch et al. 2016; Winters et al. 2007), and greater gender diversity among cyclists (Garrard, Handy, and Dill 2012). However, far less is known about the causal impact of safe infrastructure on cycling. The spatio-temporal nature of infrastructure investments makes measuring their impact on non-motorized transport modes quite complicated. For example, implementing a new bike lane can take months, so measuring changes in cycling requires a wide temporal window for analysis. Additionally, the provisions of that same bike line may be drawing users from nearby roadways (Parker et al. 2013; Parker, Gustat, and Rice 2011), thus requiring a broad spatial frame of reference to measure its impact accurately. Therefore, much of the scholarship that measures these impacts yield correlational rather than causal findings (Pucher, Dill, and Handy 2010).

The advent of the COVID-19 pandemic has radically shifted this landscape, in that the totality of the transportation system shifted essentially overnight (Bagdatli and Ipek 2022; Habib and Anik 2023; Haghani et al. 2024). This moment affords scholars a unique "natural experiment" under which to measure causal impacts across diverse mobility environments including public transit (Liu and Zhang 2023; Niu and Zhang 2023), walking (Angel et al. 2023), and driving (Katrakazas et al. 2021). The policy environment during the peak of the pandemic especially allowed for experimentation in terms of infrastructure provision for cyclists that were regularly deemed impossible during "normal" operations,

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including new bike lanes, safe streets, and even the closure of roadways to automobiles (Buehler and Pucher 2024).

In the following analysis, we use longitudinal, origin-destination, bikeshare data from the Indego system in Philadelphia, PA, administratively collected for every trip starting in 2016, to employ differences-in-differences models (DID) (Angrist and Pischke 2009) that measure the impact of a major provisional road closure during the pandemic on bikeshare trip durations. Indego is a docked bikeshare system, where bicycles are checked in and out from fixed locations (Fishman 2020). We therefore define treatment and control groups based on the spatial proximity of origin bikeshare stations to the road closure of interest, Martin Luther King Drive (MLK).

We employ duration as the dependent variable in this analysis for two key reasons: (a) longer trips are associated with greater physical activity (Fishman 2016), and can thereby better match planning priorities around promoting public health (Koglin and Rye 2014; McAndrews and Marcus 2014; Moudon and Lee 2003; Zahran et al. 2008), and (b) longer trip durations can lead to greater access to goods and services through bikeshare (J. Wang and Lindsey 2019). While the first justification for employing trip duration as the outcome variable is relatively location agnostic—more physical activity yields positive public health outcomes in most environments—the second justification, around accessibility to bikeshare and accessibility gains from the bikeshare system (Desjardins, Higgins, and Páez 2022), is more location specific to our case study in Philadelphia. In "docked" bikeshare systems like Philadelphia's, system users can only travel between a fixed set of geographic locations where check in/check out stations are currently active (Fishman 2020). Locations where bikeshare stations are concentrated and spatially distributed allow users to travel for a wide range of durations. In contrast, neighborhoods with sparse station implementation mean that users from those areas must travel by bike share for greater distances and longer times in order to access the nearest bikeshare station. These geographic characteristics of docked bikeshare systems suggest that longer travel times can yield greater accessibility through bikeshare, particularly for those users who live in areas with less dense station distributions (Davidson 2023).

If "exposure" to the provisional MLK road closure is associated with an increase in trip durations in the treatment group, such that increases go above and beyond the documented increase at the peak of the pandemic (Davidson et al. 2022), this would suggest that the provision of high quality bicycle facilities is associated with greater cycling trip durations. Indeed, we find such an "above-and-beyond" effect. While all trips that originated from stations in the treatment and control groups saw substantial increases in duration at the peak of the pandemic, treatment stations in close proximity to MLK saw a highly significant increase of 2.4 minutes compared to control stations. Together, treatment stations saw an increase in durations of nearly 25 percent compared to pre-pandemic conditions, as compared to 16 percent for control stations.

Empirically, infrastructure policies are generally not tested using "true" experiments—like a randomized, controlled trial (RCT) in medicine—but rather quasi-experiments (Shadish, Cook, and Campbell 2001) that can yield semi-causal inferences. The lack of researcher-designed treatment and control groups in infrastructure experiments during the pandemic means that the causal chain is not as robust as in an RCT but is still "semi"-causal—a stronger linkage between cause and effect than that derived from a correlation study. Here, we build on the examples in the literature that infer semi-causal changes in cycling levels during the pandemic (Kraus and Koch 2021; Sung 2023; Zhang and Fricker 2021) and focus on measuring the impacts of provisional infrastructure (Becker et al. 2022; Lovelace et al. 2020; Rérat, Haldimann, and Widmer 2022). Given our conservative estimation of the DID model and definition of the control group, these findings are likely underestimates of the infrastructure effect. Our findings suggest that well-defined, safe, and substantial cyclist-serving infrastructure can support an overall increase in cycling durations in the population, yielding positive outcomes across multiple urban planning dimensions. We now move to a discussion of the key literature that frames our analysis.

Literature Review

We highlight three key bodies of literature that situate the methodological and analytic framework in this paper. First, we discuss examples in the transportation planning literature that measure the impact of new infrastructure on cycling (across multiple outcome metrics), as necessary background to the unique experiments conducted during the Covid-19 period. Second, we review the extant literature on the impacts of the Covid-19 pandemic broadly on levels on cycling. Finally, we address the literature that specifically investigates the ways that provisional infrastructure instituted during the pandemic impacted bikeshare and cycling use patterns more generally. Throughout, we highlight the ways that the extant literature has generated causal or semi-causal inferences for the relationship between infrastructure and cycling. We conclude by highlighting gaps in the literature as framing for the structure of our research design.

Infrastructure Impacts in Bicycle Planning Research Pre-Covid-19

In order to measure the impact of a provisional road closure to automobiles during the pandemic on cycling trip durations, we first address two core behavioral models (Aldred 2013) that link new infrastructure with increased cycling in the pre-pandemic literature. First, we highlight the user preferences and empirical justifications for separated bicycles facilities, those removed from automobile traffic, and second, how cyclists will use such infrastructure based on their preferences, even when diverting from a spatially (distance) or temporally optimal route choice. The arguments in favor of separated bicycle facilities are well documented in the literature (Furth 2012; Pucher and Buehler 2008; Pucher, Buehler, and Seinen 2011) and the benefits of these facilities have been supported across multiple analytic frameworks. Foremost, studies of stated-responses from current and prospective cyclists exhibit nearly uniform convergence around preference for separated facilities, such as bike paths or protected bike lanes, away from automobiles (Aldred et al. 2017; Monsere, McNeil, and Sanders 2020; Sanders 2016). In addition, the behavioral literature on road safety for cyclists increasingly suggests that separated facilities are associated with lower rates of crashes and that when such crashes do occur, they are less harmful (P. Chen 2015; L. Chen et al. 2012; Cicchino et al. 2019; Sundstrom, Quinn, and Weld 2019). Given the users' preferences for separated facilities and the increased safety these facilities offer, we can expect that cyclists are more likely to use these facilities, rather than shared roadways. Indeed, multiple studies using GPS data have documented how cyclists will divert from the spatially/temporally optimal route choice, in order to use separated facilities (Broach, Dill, and Gliebe 2012; Menghini et al. 2010; Park and Akar 2019; Pritchard, Bucher, and Frøyen 2019; Winters et al. 2010).

While there is a clear preference for separated bicycle facilities, as opposed to shared roadways, there is a lack of consensus on how the type of separated facilities impacts perceptions of and evidence for safer cycling. Stated preference studies have shown that users prefer painted bike lanes over sharing the roadway but prefer separated bike lanes (e.g., with bollards or parked cars) over painted lanes (Foster et al. 2015; McNeil, Monsere, and Dill 2015; Smith and Sadeghpour 2022). Behavioral studies show that merely painting an unprotected bike lane can decrease nearby automobile users' speeds, which would then lead to safer cycling (LaMondia et al. 2019). However, this hierarchy of safety—where protected lanes are deemed safer than painted lanes, and so on—does not necessarily play out in the empirical evidence. Scholars have found that substantial separation, like continuous barriers or changes in the grade of the roadway, is associated with lower crash risks, but that less intensive separation, like lower bollards or parked cars, exhibits risks parallel to shared roadways (Cicchino et al. 2020).

Under the conditions described above—for example, well-separated bicycle facilities, that are ideally protected—new infrastructure has the capacity to induce greater levels of cycling (Garrard, Rose, and Lo 2008; Hull and O'Holleran 2014; Pritchard, Bucher, and Frøyen 2019). However, much of the extant literature that measures this capacity is correlational in approach, and thus is unable to address whether the new infrastructure spurred greater levels of cycling, or only spatially reorganized the patterns of existing cyclists. A pressing question is whether there is a *causal*, rather than correlational, effect of new cyclist-serving infrastructure on levels of cycling (Mölenberg et al. 2019). In two studies on the provision of new bike lanes on major thoroughfares in New Orleans, LA, Parker and colleagues (Parker et al. 2013; Parker, Gustat, and Rice 2011) find signals that new infrastructure does lead to increased cycling. By observing cyclist counts before and after adding a bike lane, the authors find that overall rates of cycling increase along roadways that experience the "treatment" of adding a bike lane, while "control" roadways saw either no change or decreases in use. These studies leave open the question of whether the bike lane street attracted users from other roadways or generated new users (Heesch et al. 2016), but do find signals that overall rates of cycling increased, at least on the bike lane street, even when accounting for dampened rates nearby (Parker et al. 2013).

In contrast, Dill and coauthors find no causal effect of the addition of a new bicycle boulevard on overall rates of cycling, which the authors suggest may be due to the unique character of this infrastructure intervention and/or issues in the study design (Dill et al. 2014). Song and colleagues (Song, Preston, and Ogilvie 2017) address a key question that remains from Parker's studies—whether there is an effect of exposure to bicycle facilities on modal shift. Using a two-stage panel design around the advent of new active transport infrastructure, the authors find that using the new infrastructure predicts modal shift away from automobiles to cycling or walking. With this background in changes in cycling behavior pre-pandemic, we now explore how cycling levels changed during the pandemic and survey examples of data-driven impact assessments of new cyclist-serving infrastructure from this time period.

Changes in Levels of Cycling and Bikeshare Use during the Covid-19 Pandemic

The literature on cycling during the Covid-19 pandemic is increasingly vast and there are a number of review papers that summarize these changes (Buehler and Pucher 2021, 2022, 2024). Rather than attempt to survey the totality of this body of work, we focus on changes in levels of cycling, with a particular focus on changes in bikeshare use patterns. Bicycle usage overall tended to decrease at the start of the pandemic but then rebounded dramatically internationally (Heydari, Konstantinoudis, and Behsoodi 2021; Nikitas et al. 2021); this change pattern differed across regions and municipalities in the United States (Buehler and Pucher 2021, 2024). Bikeshare use—as measured by either trip generation and/or duration—largely decreased at the very outset of the pandemic (Padmanabhan et al. 2021) during the United States' first lockdown. However, this was not the case in all cities, such as Philadelphia, which saw almost immediate increases in use as compared to pre-pandemic periods (Davidson et al. 2022). After the initial lockdown in summer 2020, many bikeshare systems saw use even exceeded 2019 levels of use (Padmanabhan et al. 2021; Sung 2023; Tokey 2020; H. Wang and Noland 2021a, 2021b). In addition, a number bikeshare systems in the United States even expanded their services during the pandemic either through provision of more bicycles, extending the geographic reach of their system, or both (Buehler and Pucher 2022).

The Impact of New Infrastructure during the Covid-19 Pandemic on Levels of Cycling and Bikeshare Use

One of the most dramatic mobility policy experiments during the pandemic was safe infrastructure to support cycling. Scholars have taken advantage of the near instantaneous provision of such infrastructure, in order to develop research design that measure near-causal impacts of this new infrastructure on levels of cycling (Lovelace et al. 2020; Younes et al. 2023). In case studies of Geneva and Lausanne, Rérat and coauthors find that the addition of this provisional infrastructure led to greater feelings of safety and a higher propensity to cycle among survey respondents in two low-cycling European cities (Rérat, Haldimann, and Widmer 2022). Becker and colleagues conduct a parallel analysis in Berlin (Becker et al. 2022) and find higher levels of cycling and lower exposure to pollutants along pop-up road closures. Finally, Kraus and Koch find, across multiple European examples, that provisional cycling infrastructure during the pandemic spurred higher levels of ridership in nearby areas (Kraus and Koch 2021). These studies tend to also find that these interventions are warmly received and considered to be popular. Underlying the successful provision of road closures is the unique "policy window" afforded by the pandemic, which allowed municipal governments to make dramatic changes in rapidly providing new cyclist-serving infrastructure (Harris and McCue 2022).

We now move to a discussion of the gaps in these literatures before describing our research design which is structured to respond to these gaps.

Gaps in the Literature

The studies reviewed here signal that new infrastructure provision can impact cycling levels but that this impact is context-dependent, whether on the spatial/built environment, existing infrastructure, or levels of exposure to the infrastructure. However, there is a core methodological gap in much of the extant literature pre- and post-pandemic that measures the impact of infrastructure on levels of cycling. The vast majority of studies are correlational and therefore unable to determine whether the new infrastructures caused increases in use. This gap is grounded in three core issues: (1) the cross-sectional nature of much of the literature on infrastructure impacts, (2) the generally lengthy time frame under which new cycling infrastructure is planned and ultimately engineered in the built environment, and (3) the difficulty in crafting true research experiments in cycling analyses. The primary issue facing cross-sectional research is that such studies generate correlations between infrastructure and measures of cycling activity but yield limited causal inference as there is little knowledge of cycling levels pre (or post) intervention. The policy time frame for new infrastructure only furthers this issue, in that the implementation of new facilities can take many years of planning, which makes it difficult to determine when to measure changes following "exposure" to the infrastructure (Dill et al. 2014). Finally, access to the kind of longitudinal data, well-defined treatment and control groups, and criteria to isolate semi-causal impacts are difficult to achieve using the observational studies that typify bicycle planning research (Mölenberg et al. 2019).

There has been a growing call amongst planning (Lee et al. 2022) and transportation scholars (Brathwaite and Walker 2018) to orient mobility research toward causal inference, even in the absence of controlled, experimental designs. Given the ethical, monetary, and political complications or conducting randomized controlled trials in the fields of planning and/or transportation, quasi-experiments have been increasingly deployed in the pursuit of causal inference (Winters et al. 2018). Our work here builds on select studies that have used such statistical designs, by taking advantage of the unique infrastructure provisions during the pandemic to measure changes in levels of cycling (Kraus and Koch 2021; Sung 2023).

We now move to a discussion of our research design, which we orient to fill some of gaps in the existing knowledge on the semi-causal impact of infrastructure on levels of cycling. We respond to these gaps in our analysis by specifically engaging with exposure to provisional infrastructure, a careful classification of treatment and control bikeshare stations that reflect similar built environments, and conservative estimation strategies to engage with the semi-causal impact of provisional road closures on bikeshare use patterns.

We now explicitly describe this research design before operationalizing the design under a specific model using bikeshare data.

Research Design

The Covid-19 pandemic offers a unique experimental setup in which to conduct quasi-experimental research (Shadish, Cook, and Campbell 2001) that remedies many of these inferential concerns around causality described above. As has been well documented, the pandemic impacted the totality of the transportation environment, with bicycling rates seeing a particularly dramatic increase in use during the peak of the pandemic (Buehler and Pucher 2022, 2024). Furthermore, initiatives meant to provide socially-distant, outdoor recreational opportunities, such as "pop-up" road closures to automobiles (Becker et al. 2022; Lovelace et al.

2020), established safe, cyclist-serving infrastructure almost overnight that would otherwise need to follow on years of advocacy, planning, and programmatic analysis. The joint impact of these two trends during the pandemic in Philadelphia—greater bikeshare use, and experiments in infrastructure provision—allows for a unique quasi-experimental approach to isolate the impact of new infrastructure on rates of cycling. If we assume that metrics of cycling uptake—bicycle purchases, trip generation, trip durations, and so on—increased during the pandemic, which we know they did in our case study area of Philadelphia (Cowan 2021; Davidson et al. 2022), we then ask the essential question: did these metrics increase at an even greater rate following the provisional implementation of new infrastructure?

To construct a quasi-experiment that explores this question, we require: (1) longitudinal data, (2) a metric for cycling uptake—trip duration, (3) an example of new cyclist-serving infrastructure, and (4) clearly defined treatment and control groups around exposure to this infrastructure. Together, these elements allow us to statistically evaluate if the onset of the pandemic *plus new infrastructure* yielded increased cycling trip durations. We now describe the methodological framework—the differences-in-differences model—and data inputs—longitudinal bikeshare data that describe individual-level trip durations—that inform each of these four components of our research design.

Methods and Data

The Differences-in-Differences Model

Differences-in-differences (DID) models are a powerful econometric tool used to estimate the causal or semi-causal effect of a policy intervention by comparing changes in a given outcome over time between a treatment group and a control group. This methodology hinges on the parallel trends assumption, which posits that any pre-existing differences between the treatment and control groups remain constant over time, ensuring that any observed changes can be attributed to the intervention rather than other factors. Crucially, this assumption does not imply that the groups are identical at baseline, only that their differences, in the absence of the intervention, would have followed a parallel trajectory over time. This assumption, when satisfied, controls for unobserved, time-varying factors that affect the treatment and control groups. DID models effectively remove the influence of confounding variables that vary over time, thereby strengthening the causal inference. DID models are chosen for their robustness in estimating causal effects in observational studies, especially when randomized controlled trials are infeasible. By leveraging the parallel trends assumption and controlling for time-variant confounders, DID models offer a rigorous framework for evaluating policy interventions across diverse planning areas beyond just transportation, including vacant land improvements (Heckert and Mennis 2012), the formation of service districts (An 2024), and the impact of the arts on neighborhood change metrics (Woronkowicz 2016).

Defining Treatment and Control Groups in Our Data

To isolate the semi-causal impact of the closure of MLK on bikeshare trip durations, it is necessary to define multiple indicator terms under the DID specification that describe the time-varying effect of the roadway closure, as well as a treatment and control group that captures the unique impact of being "exposed" to the MLK road closure. We define the time-varying effect as the period when MLK was fully closed to automobiles by the City of Philadelphia between March 20, 2020, and August 3, 2021.

Defining a treatment and control group to measure the impact of MLK's closure is more complex. In many cities during the pandemic, multiple provisional infrastructure interventions were instituted in tandem. New York and Paris were exemplar in this approach, blanketing their cities with new safe streets, bikeways, and closed roadways (Buehler and Pucher 2024). In such an environment, isolating the impact of a singular infrastructure intervention would be nearly impossible, as the reasonable spatial catchment for a given intervention could easily overlap with its nearest intervention neighbor. Philadelphia, however, did not undertake anywhere near this level of experimentation with provisional cycling infrastructure. Although the city did engage in drastic reductions of roadway and parking spaces to allow for outdoor dining, especially in the downtown area, MLK was the singular example of a closed major roadway for the purposes of recreation in the city. Minor roadways were closed periodically and we address this concern in the Limitations section of the Discussion and Conclusion. Therefore, we can reasonably measure the impact of the closure of MLK based on spatial proximity to this roadway, given that there were no other substantive interventions during the pandemic that explicitly would yield changes in the cycling landscape.

Figure 1 provides a visual illustration of the approach we take to spatially define treatment and control stations. We limit the study area to only those bikeshare stations in Philadelphia's network that are within 1,000 m of MLK (n = 25). We define this buffer area using Euclidean distance and highlight the benefits and limitations of using the Euclidean approach in the Discussion and Conclusion section. After limiting the study area to only these twenty-five stations, we define "treatment" stations as those that are within a 500-m buffer of MLK (n = 8; blue stations and buffer in Figure 1). The "control" group are the remaining stations (n = 17) that are between 500 and 1,000 m of MLK (red stations and buffer in Figure 1). The use of a 500-m buffer to define close spatial proximity to a feature of interest is often employed in the broader transportation literature including studies that look at public transit (X. Chen et al. 2022; Guerra, Cervero,

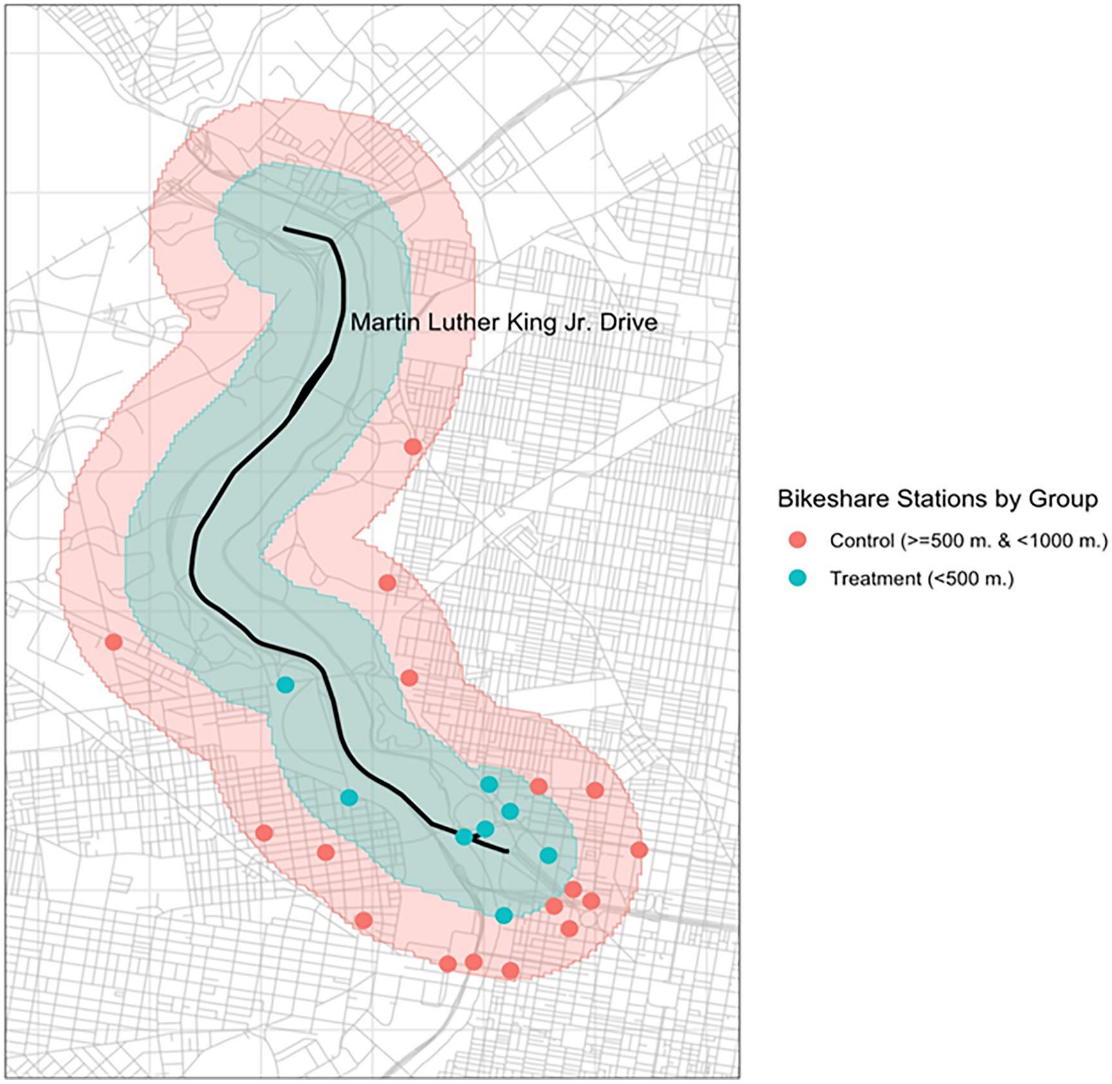

Figure 1. Study-area bikeshare stations classified by treatment and control groups.

and Tischler 2012) and car ownership (Ding and Cao 2019). This same buffer distance is regularly used to explore the effects of bicycle infrastructure (Cervero et al. 2009; Fuller et al. 2013; Madsen et al. 2014; Prins et al. 2014). Previous examples in the transportation literature explicitly employ 500-m buffers to spatially define a "treatment" area and the 500- to 1,000-m window as the "control" area for a DID model (Dai, Diao, and Sing 2024).

Although there are concerns raised around the limited empirical foundations for using 500/1,000-m buffers (Laviolette, Morency, and Waygood 2022), the benefits of using such distance thresholds for cycling research outweigh the issues. Namely, these distance thresholds are regularly employed in the literature, and, for our modeling purposes, 500 m reflects a very short cycle trip (high access), while 1,000 m reflects a slightly longer trip (lower access). It is important to note that this is a conservatively defined control group for at least three reasons: (1) 1,000 m is not a very large distance to cover by bicycle, meaning that even the most distant stations in the control group could relatively easily make use of MLK Drive, (2) many of the "control" stations are located just beyond the 500-m threshold, and (3) all of the "control" stations have reasonable access to the Schuylkill River Trail, a bike path on the opposite bank of the Schuylkill River from MLK, and Philadelphia's flagship example (before the pandemic) of separated bicycle facilities. Together these conservative features suggest that the estimated impact of closing MLK in the DID models would

Table 1. Model Variables and Definitions.

Variable	Definition	Units
Trip level variable		
Trip Duration	Time elapsed from check out at origin station to check in at destination station	Minutes
Differences-in-differences model variables		
Origin	Bikeshare station from which the bicycle was checked out for which trip durations are aggregated	Longitude/Latitude Degrees
Time	Day and year combination for which trip durations are aggregated	Day + Year
Intervention	Indicator variable defining observations in policy intervention/ provisional road closure period	Binary (1 = Time ≥ March 20, 2020 and Time ≤ August 3, 2021/ 0 = Time < March 20, 2020 OR Time > August 3, 2021)
Treatment	Indicator variable defining observations in spatial proximity to Martin Luther King Drive	Binary (1 = Origin station ≤ 500 m of MLK/0 = Origin station > 500 m and ≤ 1,000 m of MLK)

likely be an underestimate of the policy effect of new infrastructure like a closed roadway.

Inputs to the DID Model

We employ DID models to analyze the semi-causal impact of the provisional road closure of MLK in Philadelphia on average bikeshare trip durations that originated from nearby stations. This approach allows us to isolate and interpret the effect of the road closure policy by examining how trip durations changed for stations near the closed road (which make up the treatment group) compared to stations somewhat farther away (which make up the control group), before and after the intervention. The Indego bikeshare system provides origin-destination, trip-level data for all trips taken since the program's launch in Spring 2015. We include data beginning in January 2016, the first full year of service. Like many bikeshare systems (Nguemeni Tiako and Stokes 2021), Indego adjusted their policies in response to the pandemic, reducing the cost of an unlimited monthly pass to $5 from $20 for the first months of the pandemic (Hooven 2020). In addition, Indego engaged in active system expansions through the pandemic (Pulcinella 2021).

Our DID model can be represented as:

$$\text{Trip Duration} = f\begin{pmatrix}\text{Intervention, Treatment,}\\ \text{Intervention, Treatment}\end{pmatrix}$$

where *Trip Duration* is the time that elapsed for a given ride from check out at the origin station to check in at the destination station (or the same station for a circular trip), which we aggregate to the daily average for each origin station in the data; *Intervention* is an indicator that defines trips that originated within the time period when MLK was provisionally closed to automobiles (March 20, 2020–August 3, 2021); *Treatment* is an indicator that defines the treatment and control groups based on the Euclidean distance of origin stations to MLK, such that "treatment" stations are ≤500 m of MLK, and control stations are >500 m and ≤1000 m of MLK; and *Intervention* × *Treatment* is the interaction term between the temporal and spatial indicator terms. Model variables and definitions are summarized in Table 1.

Interpretation of DID Model Coefficients

The coefficient on each term provides a unique interpretation of the relationship between our predictors and the dependent variable. In this model, the intercept represents the baseline average trip duration for trips originating from control stations before the road closure. The coefficient on the *Intervention* term isolates the main effect of the road closure on the control group, analogous to the baseline pandemic effect of the intervention. The *Treatment* coefficient captures the pre-intervention differences between the treatment and control groups. The interaction term *Intervention* × *Treatment* is the crucial term in the model, as it represents the differential impact of the road closure on the treatment group relative to the control group.

For the interpretation of the interaction term to hold, the parallel trends assumption must be satisfied. In our study, the close physical proximity of treatment and control stations ensures that external factors like weather or city-wide biking trends affect both groups similarly. This uniformity allows us to attribute differences in trip durations specifically to the road closure intervention. Our treatment and control group design also removes inconsistency in factors that vary over time. Bikeshare trip durations can change over time due to many variables such as diffusion of system information to the population or changes in biking culture across the city (Fishman 2020). As observations in our

Table 2. Summary Measures for Population Context Variables.

	PHL (n = 374 tracts)		Treatment (n = 6 tracts)		Control (n = 11 tracts)		
	Mean	*SD*	Mean	*SD*	Mean	*SD*	Difference in means[a]
Income ($)	49,393.74	25,158.43	75,771	26,609.63	57,447.64	26,790.07	18,323.36
Population	4,217.18	1,699.18	2,883.67	1,593.93	3,914.55	1,713.04	−1,030.88
Population density (1/km^2)	7,642.49	4,402.95	9,258.45	6,405.64	8,238.73	4,334.19	1,019.72
No car access (%)	30.85	17.03	32.1	14.96	39.82	19.01	−7.73

Note: Census tracts in the "treatment" column can also appear in the "Control" as some treatment and control stations are located within the same tract.
[a]Difference between treatment and control means.

Table 3. Summary Measures for Average Daily Trip Durations for Trips Originating from Bikeshare Stations in Study Area Over the Full Time Series.

	Study area (n = 25 stations)		Treatment (n = 8 stations)		Control (n = 17 stations)		
	Mean	*SD*	Mean	*SD*	Mean	*SD*	Difference in means[a]
Trip duration (minutes)	26.44	39.28	28.04	40.45	25.8	38.78	2.24

[a]Difference between treatment and control means.

treatment and control groups were observed concurrently, effects of these time-varying factors related to cycling, and more general time-varying factors like weather, are uniform across both treatment and control groups. As a result, we can isolate and compare the effects of the MLK road closure on these trips.

The primary goal of DID models is not to maximize predicted variance in the outcome variable (reflected in metrics like the r-squared value). Rather, they are designed to provide robust causal inferences. Thus, even with a potentially low r-squared value, the interpretation of key terms in the model remains reliable.

Characteristics and Background for Model Inputs

The population-level context for our study area also suggests that the treatment and control groups reflect similar baseline conditions against which to evaluate the impact of the road closure on MLK. Table 2 shows that census tracts in the treatment and control areas both have average incomes higher than the mean across Philadelphia. While the treatment tracts feature higher incomes on average, and there is some evidence that higher income areas feature more bikeshare check outs in Philadelphia (Caspi and Noland 2019), there is not a clear link between income and bikeshare trip durations, the dependent variable of interest in our study. Treatment and control tracts feature similar populations and population densities. Importantly, both treatment and control tracts feature higher levels of households without car access than compared to the city at large (see Table 2).

It is important to note two caveats when interpreting differences in the descriptive measures in Table 2: (1) given the definition of treatment and control buffers, census tract boundaries can span the treatment and control zones, and thus can be counted in both treatment and control columns in the table, and (2) it is not reasonable to evaluate whether the differences in mean values for tract-level indicators between treatment and control areas are statistically discernible given the very small sample size of census tracts.

Moving to background characteristics for our dependent variable (see Table 3), average daily trip durations, trips that originate from our study area feature much higher durations at around twenty-six minutes, compared to the system-wide average of around fifteen minutes over the study period (see Davidson et al. 2022). Trips that originate from treatment stations reflect daily duration averages nearly three minutes higher than those that originate from control stations. We evaluate the statistical discernibility of these differences in greater detail in the Results.

Results

Both treatment and control groups display similar patterns over the time series (see Figure 2), where seasonal variation leads to relatively constant patterns in duration between 2016 and 2019, very substantial increases in duration during the spring/summer of 2020 at the outset of the pandemic, and a relative return to baseline conditions in spring 2021. The key feature of this initial plot of the time-series is that parallel trends assumption appears to generally hold. Variation in treatment and control groups tend to follow one another,

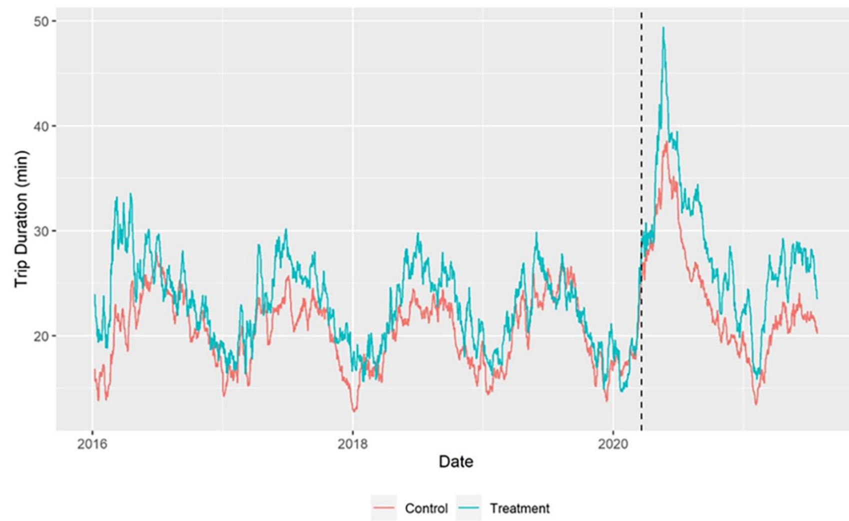

Figure 2. Time series for average daily bikeshare trip duration/day by treatment/control group.
Note: Fourteen-day rolling averages presented. Dashed line marks March 20, 2020, when MLK was closed to automobiles.

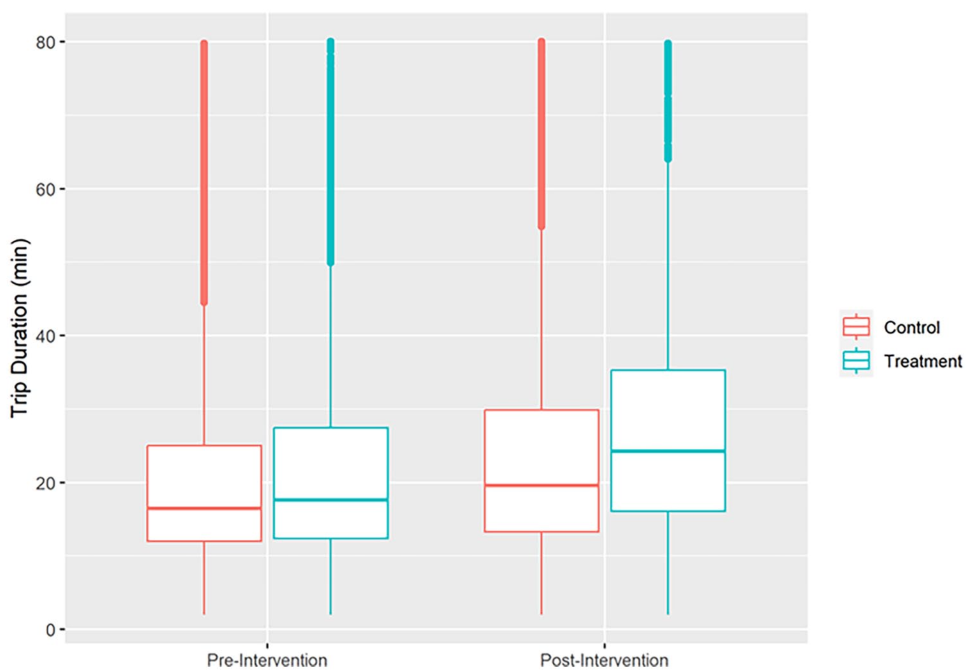

Figure 3. Boxplots that describe average trip durations by treatment and control groups months pre/post Martin Luther King Drive closure.

including both groups experiencing substantial increases post-intervention.

On average, a pre-intervention trip originating from a control group station lasted 20.5 minutes, compared to 21.9 minutes in the treatment group (difference in means significant at $p < .001$). While the average trip duration for control group stations increased by more than three minutes post-intervention to 23.7, treatment station duration averages increased to 27.5 minutes (difference in means significant at $p < .001$). The change in these differences is described in the boxplots in Figure 3.

The coefficients of the DID model mirror the results described above (see Table 4). The intercept term describes average trip durations for control group stations pre-intervention, which were around 20.5 minutes. The coefficient on *Intervention* describes change in duration over time for trips

Table 4. Differences-in-Differences Model Results.

	Dependent variable:
	Trip duration
Intervention	3.179***
	(0.186)
Treatment	2.154***
	(0.183)
Intervention × Treatment	2.403***
	(0.343)
Constant	20.536***
	(0.097)
Observations	37,376
R^2	0.027
Adjusted R^2	0.027
Residual *SE*	13.506 (df = 37,372)
F statistic	341.396*** (df = 3; 37,372)

***$p < .001$.

originating from control group stations of around 3.2 minutes. This change can be thought of as the baseline pandemic effect absent the policy intervention of the road closure. The coefficient on *Treatment* suggests that trips originating from treatment stations, those within 500 m of MLK, feature average durations 2.2 minutes higher than control stations pre-pandemic. Finally, the coefficient on the interaction term suggests that trips originating from treatment stations saw an increase of 2.4 minutes above control stations following the closure of MLK. This change can be considered the change in duration *above* the expected increase due to change over time, or, put differently, the "policy effect" of the road closure.

Robustness Check on Initial Model Specification

We conduct a robustness check on the DID model that extends data included in the time-series to December 31, 2021. While the provisional road closure of MLK lasted nearly 1.5 years, the roadway partially reopened for vehicle use on August 4, 2021, at which point, a portion of MLK constituting roughly half of the roadway's total length was reopened to vehicular traffic. Three out of 29 stations in the study area are within 1,000 m of the reopened portion of MLK, two in the control group and one in the treatment group. The remaining twenty-six stations are located closest to the portion of MLK Drive that remained closed to vehicles through the end of 2021.

In response to this development in the policy intervention, we fit a second model that extends the time-series to add in data from August 4 to December 31, 2021, but retains the definitions of the *Intervention* and *Treatment* indicators as defined in the original DID model. All stations in the 500-m buffer of MLK are still considered "treated," even those within the buffer of the reopened roadway, and all data post March 20, 2020, are part of the "intervention" period, even though the roadway was no longer fully closed for days after August 4, 2021. This robustness check allows us to investigate how supportive infrastructure interventions for cyclists can yield impacts as the infrastructure itself changes over time.

The time-series plot when including data through the end of 2021 looks similar to the initial time-series (see Figure 4). Given that this addition of data comes from fall and early winter of 2021, there is an expected decline in durations, as the conditions for cycling during these seasons in Philadelphia worsen. Nonetheless, it appears that the treatment group continues to exhibit higher trip durations as compared to the control group. These visual patterns are reinforced in the results of the DID model when including the additional data. The sign and statistical discernibility of the *Intervention, Treatment*, and *Intervention* × *Treatment* coefficients are consistent from the original DID model (see Table 5). This size of the *Treatment* and intercept terms are unchanged from the original model given the nature of the DID model specification. The size of the *Intervention* coefficient is smaller compared to the initial model, due to the inclusion of later date from fall/winter when cycling conditions are worse in Philadelphia. Finally, the interaction term in the robustness check model is slightly smaller than in the original DID model.

Figure 5 visually summarizes the results of the DID and robustness check models as well as the differences between the two models. The three lines presented reflect the change in average trip durations over time for the control group (in red), the treatment group (in blue), and the counterfactual (in light blue, dashed). The counterfactual reflects the expected change in duration for the treatment group, absent exposure to the "treatment" of the MLK road closure. The slope for the change in the treatment group is steeper than the counterfactual, even when including data following the partial reopening of MLK.

Discussion and Conclusion

Interpretation of Results

As expected, we find substantial increases in average daily trip durations for both the treatment and control groups around the time that MLK was closed to automobiles, given that the beginning of this policy experiment coincided with the start of the pandemic lockdowns. In the time series plots (Figures 2 and 4) we see a sharp vertical increase in our dependent variable during spring/summer of 2020, a trend which is mirrored by the positive and highly statistically discernible coefficient on the *Intervention* term in our models (Tables 4 and 5). This initial finding reinforces earlier research (Davidson et al., 2022) that suggests strong increases in trip durations across the bikeshare system in Philadelphia at the outset of the pandemic.

Although these overall trends of increasing trip durations during the pandemic are found in both the treatment and

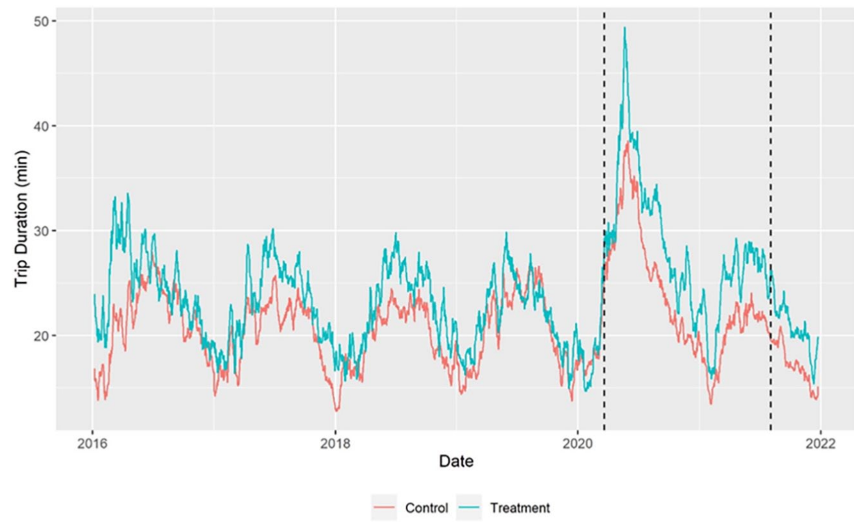

Figure 4. Time series for average daily bikeshare trip duration/day by treatment/control group; including data post-partial reopening of Martin Luther King Drive.

Note: Fourteen-day rolling averages presented. Dashed line marks period between March 20, 2020, and August 3, 2021, when MLK was fully closed to automobiles.

Table 5. Robustness Check Model Results.

	Dependent variable:
	Trip duration
Intervention	1.544***
	(0.167)
Treatment	2.154***
	(0.182)
Intervention × Treatment	2.082***
	(0.308)
Constant	20.536***
	(0.096)
Observations	40,903
R^2	0.016
Adjusted R^2	0.016
Residual *SE*	13.431 (df = 40,899)
F statistic	227.465*** (df = 3; 40,899)

***$p < 0.001$.

control groups, it is important to highlight the differences between these groups before MLK was closed to automobiles. While the average trip durations in Figure 2 appear to be visually quite similar between the treatment and control groups, we find differences in the pre-intervention period, as described by the statistical analyses. Trips that originate from treatment stations exhibit average durations approximately 1.5 minutes longer than control stations before the MLK closure. We believe this baseline difference may be due to the fact that during non-pandemic periods, MLK was still regularly closed to automobiles on the weekends. Although both treatment and control stations are located in a bicycle-infrastructure rich area of Philadelphia, namely in close proximity to the Schuylkill River Trail, the periodic closure of MLK may have spurred longer travel times pre-pandemic.

In the post-intervention period, after MLK was fully closed to automobiles, the difference between treatment and control average durations swelled to almost four minutes, and this difference is highly statistically discernible. It is important to note that both treatment and control groups saw substantial increases in the median duration and a positive movement of the interquartile range post-MLK closure (Figure 3). These two summary measures signal that the changes in duration are robust across multiple measures of central tendency, beyond evaluating only the differences in means (Bills and Walker 2017). These additional metrics also help to illustrate that the difference between treatment and control durations is substantially greater at a range of values—the 25th, 50th, and 75th percentile values for duration are all farther apart between treatment in control during the post-closure period than in the pre-closure one.

Policy Implications

The results of the DID model (Table 4) extend these initial results and strongly suggest the positive, semi-causal impact of supportive infrastructure on bikeshare trip durations. Beginning with the intercept term in the model, we find that, pre-pandemic trips that originated from control stations in our study area featured average durations of more than twenty minutes. This value highlights that trips in this study appear to

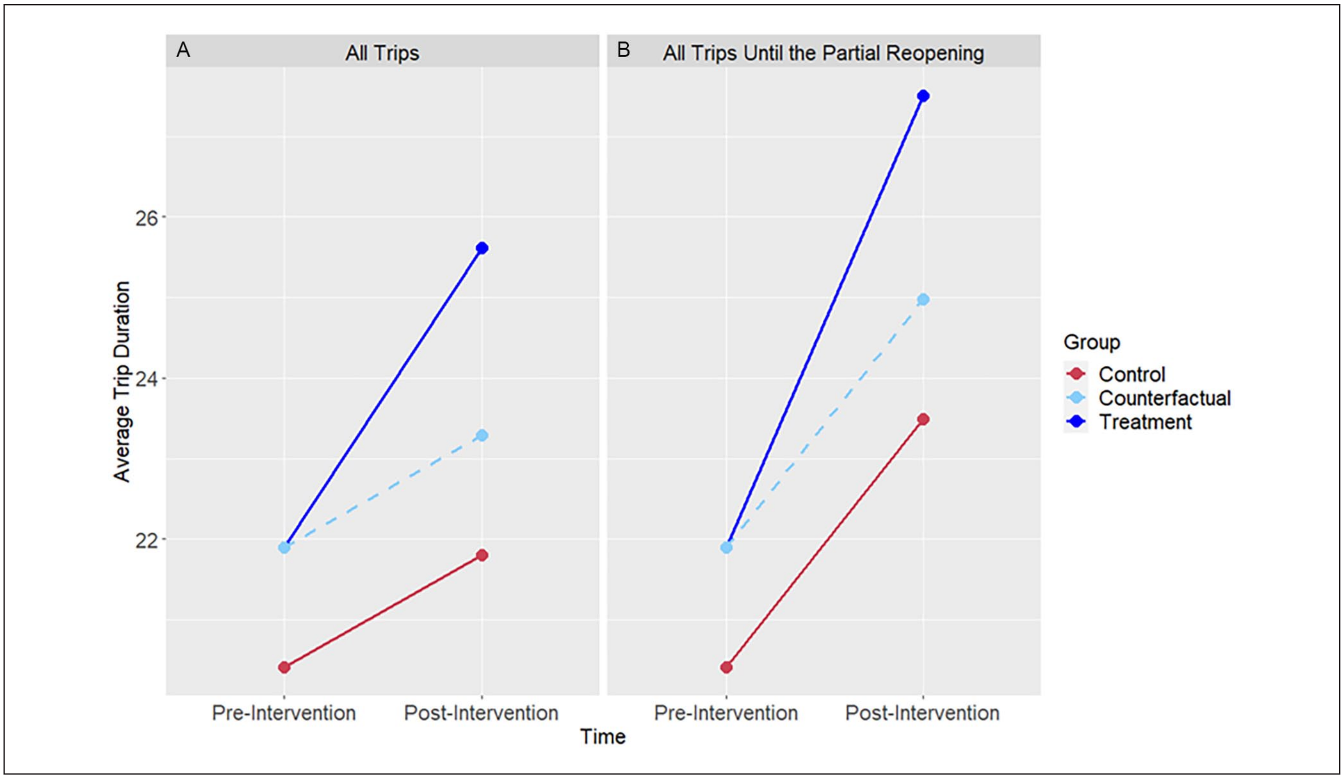

Figure 5. Illustration of robustness check (A) and differences-in-differences (B) model results.

be longer at baseline than the system-wide average of around fifteen minutes (see Davidson et al. 2022). This is important to note, given that any changes in duration in the models responding to the MLK closure are starting from an already high baseline pre-intervention. The coefficients on the *Intervention* and *Treatment* term reinforce the findings described visually in Figures 2 and 3. The baseline difference between treatment and control groups pre-pandemic is around 2.4 minutes, as described by the coefficient on *Treatment*. The "pandemic" effect, as described by the coefficient on *Intervention* is a quite substantial, at around 3.2 minutes, suggesting an increase of nearly 16 percent compared to the pre-intervention baseline. Both findings mirror the bivariate analysis, but do not, on their own, suggest a semi-causal impact of the MLK closure. Rather they serve as the key control variables for the interaction between *Intervention* and *Treatment*, which allows for semi-causal inference.

The core policy implications are derived from the interaction term in the DID model, the key indicator in our study. The interaction between *Treatment* and *Intervention*, is large at 2.4 minutes, and highly statistically discernible. Taken on its own, this value would suggest 11 percent increase above the pre-intervention baseline for the treatment group. Recall, however, that the interaction term reflects the additive effect of being in the treatment group on top of the baseline effect described by the coefficient on *Intervention*, meaning that the temporal plus policy effect is closer to an increase in durations of 25 percent compared to baseline. In this light, the coefficient on the key interaction term presents a very strong indication that supportive infrastructure can generate increases in cycling trip durations in the population, at least for bikeshare users. Recall, as well, that the stations in both the treatment and control groups reflect higher than average durations at pre-pandemic baseline.

This suggests that supportive infrastructure can not only yield positive impacts on use, but also that there does not appear to be a "ceiling effect" to this increase. Even in areas typified by baseline higher cycling trip durations, new infrastructure can yield increases in use. Finally, the coefficient on the interaction term in our robustness check model (Table 5) is still large and statistically discernible, signaling the persistence of the effect of new infrastructure on treatment group stations. The results of this robustness check model only strengthen the semi-causal impact of supportive, cyclist-serving infrastructure, leading to increased trip durations. The strength of our findings is described succinctly in Figure 5, where, when compared to the counterfactual, the semi-causal, positive impact of supportive infrastructure on bikeshare use is strong and clear, displaying a much steeper slope across both the DID and robustness check models.

Limitations

While these findings are robust, and suggest a semi-causal impact of new, supportive infrastructure on trip duration, there are several limitations to our study. First, given the administrative nature of the data, we have no way of knowing if users in the treatment group rode on MLK. The Indego system does not provide any geolocated route data for trips taken on their bicycles. It could be possible that trips originating at treatment stations traveled for longer periods of time, but on other roadways than MLK. Second, we define the spatial buffers for treatment and control groups using Euclidean as opposed to network distance. While this has several advantages for measuring the lived experience of cyclists (who do not traverse the road network in the same way as automobiles—for example, using sidewalks, traveling against traffic, etc.), it may not capture certain areas where stations are practically inaccessible to MLK given geographic barriers and/or the nature of the street network.

Third, it may be that the closure of MLK attracted more recreational cyclists to nearby treatment stations, for example, individuals who would drive or walk to a station near the roadway and check out a bike for a long-duration, recreational trip. Furthermore, if the changes in duration were due only to increases in recreational use, the semi-causal impact of infrastructure we find in the models cannot speak to changes in utilitarian cycling (Winters et al. 2007) or commuting (Guerra et al. 2020). Fourth, in a parallel concern, it may be the case that the increased trip durations were derived in part from expanded time budgets/greater "time affluence" (Giurge, Whillans, and West 2020; Kasser and Sheldon 2009) for higher income users to devote to recreation. As has been documented during the pandemic, individuals of higher incomes had more flexibility in their schedules, more ability to limit their travel for essential needs (shopping, work, etc.), and more capacity to expand their recreation-based trips, than did lower income populations (Kar et al. 2022). Fifth, our data only reflect changes for bikeshare users. While bikeshare riders tend to more closely reflect characteristics of the overall population than do cyclists generally (Buck et al. 2013), our findings may not speak as well to the impact of provisional infrastructure for the general cycling population.

Sixth, and finally, our data are only drawn from one case study in Philadelphia, which featured one example of major provisional, cyclist-serving infrastructure during the pandemic, but did feature a number of smaller interventions that may have also impacted bikeshare trip durations. One prominent example are Philadelphia's "Playstreets" program, where certain roadway segments are closed to traffic when school is not in session to allow for expanded children's recreation. These closures have been ongoing for the last half century and continued through the pandemic (https://www.phila.gov/programs/playstreets/). Although these streets theoretically support cycling, there were only limited examples from the Summers of 2020 and 2021 in close proximity to MLK, and the vast majority of these closures occurred within the "control" area, as opposed to the "treatment" area (see Figure A1). This suggests that such closures are only limited confounders in our model, if they are confounders at all.

Future Work

These limitations, in tandem with the strong signals in our analysis, chart a course for future research on this topic. First and foremost, other data sources, such as those collected by phone-based applications or other trackers that describe geographic locations and perhaps demographic backgrounds of individual users (Fischer, Nelson, and Winters 2022; Nelson et al. 2021) could be employed to measure more granular use on and impacts of MLK during the pandemic. Primarily, these data would allow us to see who actually rode on MLK, as opposed to other roadways in the next. Initial results using automated counters suggest that use on MLK increased dramatically during the pandemic (Cowan 2021), but more spatially granular, user-level information would help to validate the causal inferences in our models. These data would also allow us to expand our findings beyond the bikeshare population and toward describing cyclists more generally. Second, individual-level data could help to remediate the concerns around the confounding distribution of time affluence. Given that treatment tracts in our study exhibit higher incomes than the city average, but also display a very large amount of variability as displayed by the standard deviation in income (see Table 2), tract level proxies are likely not sufficient controls in the model, and individual-level variation in income is necessary to delineate the differences between the impacts of infrastructure and income on trip duration.

Third, the models could be fitted with additional response variables, namely trip generation. While we argue that duration is an important indicator of cycling levels, it is important to learn about how infrastructure impacts the number of trips, not only the length of trips. Fourth and finally, additional case studies could be introduced to validate the results from Philadelphia. Other case studies from a diverse array of cities and regions could explore infrastructure impacts in areas that engaged with multiple major interventions, or those that generated no new infrastructure during the pandemic. In addition, other case studies that have geotagged bicycles, could allow for more robust inference around trip purpose, which would allow us to distinguish between utilitarian and recreational purposes in the model specifications.

With these limitations and future research directions in mind, the findings in this study support the argument that new, supportive, and robust infrastructure for cyclists yields greater bikeshare trip durations. Even during an environment characterized by increased levels of use across the bikeshare system due to the pandemic, the road closure of MLK yielded semi-causal and very substantial increases in trip durations for those stations in immediate proximity to the road closure, as compared to those stations within reasonable distance, but not immediately proximate to MLK. It will benefit the bicycle planning community to use this example as an impetus to spatially grow the network of safe and supportive infrastructure, given that such improvements can yield positive impacts.

Appendix

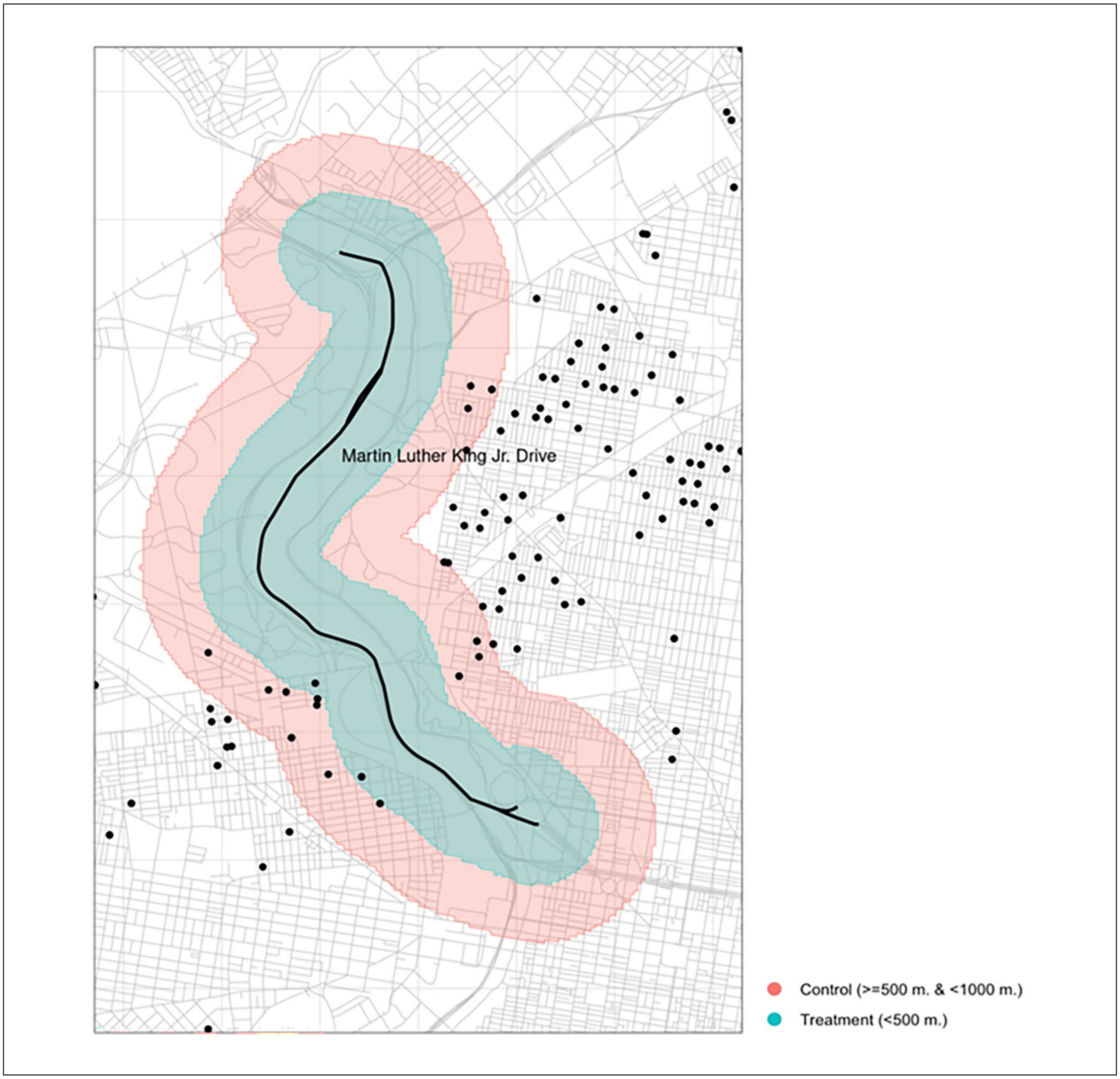

Figure A1. Location of street segment centroids for Philadelphia "Playstreets" closed to automobiles in Summer 2020 and/or Summer 2021, overlaid on control and treatment buffers used in differences-in-differences models.

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