Slow Streets and Dockless Travel: Using a Natural Experiment for Insight into the Role of Supportive Infrastructure on Non-Motorized Travel

June 2023

A Research Report from the National Center for Sustainable Transportation

Marlon G. Boarnet, University of Southern California

Seula Lee, University of Southern California

James Gross, University of Southern California

Calvin Thigpen, Lime

TECHNICAL REPORT DOCUMENTATION PAGE

1. Report No.
NCST-USC-RR-23-23

2. Government Accession No.
N/A

3. Recipient's Catalog No.
N/A

4. Title and Subtitle
Slow Streets and Dockless Travel: Using a Natural Experiment for Insight into the Role of Supportive Infrastructure on Non-Motorized Travel

5. Report Date
June 2023

6. Performing Organization Code
N/A

7. Author(s)
Marlon Boarnet, PhD, https://orcid.org/0000-0002-0890-347X
Seula Lee, https://orcid.org/0000-0002-1002-0553
James Gross, https://orcid.org/0000-0002-8405-7340
Calvin Thigpen, PhD, https://orcid.org/0000-0003-4284-2882

8. Performing Organization Report No.
N/A

9. Performing Organization Name and Address
University of Southern California
METRANS Transportation Consortium
University Park Campus, VKC 367 MC:0626
Los Angeles, California 90089-0626

10. Work Unit No.
N/A

11. Contract or Grant No.
Caltrans 65A0686 Task Order 071
USDOT Grant 69A3551747114

12. Sponsoring Agency Name and Address
U.S. Department of Transportation
Office of the Assistant Secretary for Research and Technology
1200 New Jersey Avenue, SE, Washington, DC 20590

California Department of Transportation
Division of Research, Innovation and System Information, MS-83
1727 30th Street, Sacramento, CA 95816

13. Type of Report and Period Covered
Final Research Report (01/01/2022 - 12/31/2022)

14. Sponsoring Agency Code
USDOT OST-R
Caltrans DRISI

15. Supplementary Notes
DOI: https://doi.org/10.7922/G29G5K4N
Dataset DOI: https://doi.org/10.7910/DVN/GBO9YC

16. Abstract
In the early stages of the COVID-19 pandemic, cities across the globe converted street space to non-automobile uses. This project studies four of these slow street programs in the U.S.: in Los Angeles, Portland, Oakland, and San Francisco. In each city, the slow streets (implemented in late spring to early fall 2020) are used as a treatment and compared to non-implemented control groups. The dependent variable is counts of dockless scooter trips passing a mid-block screenline for time periods both before and after slow street implementation. Those dockless scooter counts were obtained from historical data provided by Lime, a dockless scooter provider in each of the study cities. Two methodological approaches were used: differences-in-differences (DID) and panel regression analysis with block fixed effects. For the DID analysis, the researchers used networks of candidate slow streets that were not implemented as the control group. Such control networks were available in Los Angeles, Oakland, and San Francisco. For the panel analysis, they used slow street segments implemented later in the study period as control segments for earlier implemented slow street segments, including fixed effects for blocks and for time periods in the panel regressions. The findings show statistically significant associations between increased dockless scooter trips and slow street implementation in each study city, using both DID and panel analyses. The associations are robust to different specifications. The authors calculate the magnitude of the slow street treatment effect by dividing the estimated treatment effect by a 2019 baseline of dockless trip counts. In the DID analysis, they find that slow street implementation increased dockless scooter trip counts from 22.16% to 74.5%, relative to a 2019 (before slow streets) baseline. In the panel analysis, the increase in dockless trip counts on slow streets ranged from 10.77% to 16.75%, relative to a 2019 baseline.

17. Key Words
Dockless Travel, Slow Streets, COVID-19, Micromobility, Bike Infrastructure, Active Transport

18. Distribution Statement
No restrictions.

19. Security Classif. (of this report)	20. Security Classif. (of this page)	21. No. of Pages	22. Price
Unclassified	Unclassified	52	N/A

Form DOT F 1700.7 (8-72) Reproduction of completed page authorized

About the National Center for Sustainable Transportation

The National Center for Sustainable Transportation is a consortium of leading universities committed to advancing an environmentally sustainable transportation system through cutting-edge research, direct policy engagement, and education of our future leaders. Consortium members include: University of California, Davis; University of California, Riverside; University of Southern California; California State University, Long Beach; Georgia Institute of Technology; and University of Vermont. More information can be found at: ncst.ucdavis.edu.

Disclaimer

Acknowledgments

This study was funded, partially or entirely, by a grant from the National Center for Sustainable Transportation (NCST), supported by the U.S. Department of Transportation (USDOT) and the California Department of Transportation (Caltrans) through the University Transportation Centers program. The authors would like to thank the NCST, the USDOT, and Caltrans for their support of university-based research in transportation, and especially for the funding provided in support of this project. We also thank Lime, which provided dockless scooter travel data, and staff in the planning departments at each of our study cities who provided information on slow street implementation. Finally, we thank Clare Eberle, Daisy Balcazar, Kevin Ocubillo from LADOT (Los Angeles Department of Transportation), Scott Cohen, Jacob Sherman, Erin Roden from PBOT (Portland Bureau of Transportation), Brian Liang from SFMTA (San Francisco

Municipal Transportation Agency), and Noel Pond-Danchik from OakDOT (Oakland Department of Transportation) for helping us with the project.

Slow Streets and Dockless Travel: Using a Natural Experiment for Insight into the Role of Supportive Infrastructure on Non-Motorized Travel

A National Center for Sustainable Transportation Research Report

June 2023

Marlon G. Boarnet, Sol Price School of Public Policy, University of Southern California

Seula Lee, Sol Price School of Public Policy, University of Southern California

James Gross, Sol Price School of Public Policy, University of Southern California

Calvin Thigpen, Director of Policy Research, Lime

TABLE OF CONTENTS

List of Tables

List of Figures

Slow Streets and Dockless Travel: Using a Natural Experiment for Insight into the Role of Supportive Infrastructure on Non-Motorized Travel

EXECUTIVE SUMMARY

In the early stages of the COVID-19 pandemic, cities across the U.S. moved street space from automobile to pedestrian and non-motorized uses. In addition to “pop up” outdoor dining, several cities initiated slow street projects which included reducing car travel lanes, increasing areas devoted to non-motorized travel, and taking measures to slow vehicle traffic while prioritizing non-vehicle modes. Slow streets are a repurposing of existing infrastructure to support uses such as dining and shopping and to support non-car travel modes. At times these were infrastructure treatments (often temporary such as paint or signage) and in some cases policy such as allowing pedestrian use of street space. These programs were implemented at a speed and scale never before seen in the U.S. To give two examples, in the late spring and early summer of 2020, Oakland implemented 74 miles of slow street networks and Los Angeles implemented 50 miles. These and similar programs in other cities amount to what are often several years of planning and implementation compressed into weeks or months.

We use this large, and prior to COVID-19, unexpected slow street program to study the impact of slow streets on non-motorized travel. We use a quasi-experimental approach, comparing non-motorized travel on slow streets with non-motorized travel on similar “control” streets which were not converted into slow streets. We use dockless scooter travel for our empirical analysis. Throughout this report, when we use the phrase “dockless” we are referring to scooter data and hence to dockless scooter travel. We use a “before-after” approach, examining the impact of slow streets in a treatment and control group, before and after slow street implementation. We use candidate streets that were not converted to slow streets as controls for the first analysis, comparing changes from 2019 to 2020 for both the slow streets and the control group (a before-after approach with a differences-in-differences model.) In two cities, we also use later implemented slow streets as controls for the earlier implemented slow streets (a before-after approach with a fixed effects panel model). Previous studies have not had an opportunity to study the link between non-car travel and large non-car infrastructure projects. The scale of slow street implementation during the pandemic provides a rare opportunity to observe what would happen if meaningfully large street networks were adjusted to better accommodate non-car travel – not by eliminating automobiles but rather by implementing the traffic calming approaches that were common in COVID-19 slow street programs.

Because it is not possible to gather data on walking or bicycling activity retrospectively (i.e., before slow street implementation) ex post, we partnered with Lime, a provider of dockless scooters, to use their data on scooter travel on the treatment slow streets and a control group before and after slow street implementation. Lime maintains historical data on trips, allowing a before/after comparison that would not be possible for walking, bicycling, or other non-car

modes. Our results give insights into how slow streets influenced dockless scooter travel. Importantly, we use dockless scooter travel as an example of a non-car mode. We believe that our results – which show that slow streets are associated with increases in dockless scooter travel – suggest that supportive infrastructure increases non-car travel more broadly. The results likely generalize to other non-car modes, and our focus on dockless scooters should be viewed as a focus on one example of non-car travel.

We studied four cities: Los Angeles, Oakland, Portland, and San Francisco. We used two methodological approaches: differences-in-differences and panel fixed effects regression. For both approaches, our observations are mid-points of street blocks, and our dependent variable is a screenline count of the number of scooter trips passing the midpoint either in a week (panel regression) or a month (differences-in-differences). For both differences-in-differences and panel approaches, we classified blocks as treatments once a slow street network was implemented in the block. We had ready control groups in Oakland, Los Angeles, and San Francisco. Each of those three cities had networks that were considered for slow street implementation but were not implemented (Los Angeles and San Francisco) or an on-street bike-lane network that, according to the city, was prioritized for slow street implementation but not implemented (Oakland). For the panel data analysis, we used later implemented slow streets as the control group for earlier implemented slow streets. Due to timing of the implementation and the nature of the slow-street programs, the panel analysis was only possible in Portland and San Francisco.

We consistently found an association between total dockless trip counts and slow street implementation. In all cities except Los Angeles, the total trip counts increased after slow street implementation, relative to control groups, in both the differences-in-differences and panel analysis. In Los Angeles, we found positive impacts of slow streets on trip counts at specific times of day (e.g., weekday midday, weekend night time) but not in total trip counts. We compared the treatment effect (the association between slow street status and increases in dockless trips) to 2019 (pre-treatment) slow street trip counts, and find that in the differences-in-differences analysis, slow street implementation increased dockless trip counts by 54.78% in Oakland, 22.16% in Los Angeles, and 74.5% in San Francisco after controlling for the relevant covariates. These treatment effect sizes were smaller in the panel analysis but still statistically significant: 10.77% in Portland and 16.75% in San Francisco. Overall, we found statistically significant and meaningfully large associations between slow street implementation and increases in dockless scooter travel in all four study cities. We believe this is compelling evidence that networks of slow streets can increase non-motorized travel.

Introduction

In early 2020, the COVID-19 pandemic presented local governments and transportation agencies with a crisis. As stay-at-home orders were issued in order to stymie transmission of the COVID-19 virus, residents of cities faced a problem of limited access to safe spaces in which to exercise, recreate, and travel without cars. Accordingly, local governments across the United States set up temporary street closures, referred to throughout this work as Slow Streets, wherein car traffic was limited to local access and parking and streets were shared with pedestrians, cyclists, and those using other forms of active transportation such as e-scooters (Combs & Pardo 2021). While not without pre-pandemic antecedents (see, for instance, cyclovias described in Landgrave-Serrano & Stoker 2022, car-free zones more generally described in Pucher, et al. 2010), these Slow Streets proliferated at an unprecedented rate during the first year of the pandemic.

While originally implemented as a temporary measure, Slow Streets have the long-term potential to reduce traffic congestion and emissions, provide low-cost access to park-like spaces in underserved communities, and promote healthy and active lifestyles by supporting active transportation. Thus, discussion has turned to whether to keep Slow Streets as traffic levels have returned close to the pre-pandemic norm. The rapid implementation of Slow Streets programs allows for a natural experiment to assess the impact of traffic restrictions on travel behavior. However, there is still little quantitative research on Slow Streets (Kim 2022 points this out). Therefore, the current moment is crucial in empirically evaluating the impacts of Slow Streets programs around the country.

In investigating this question, we can turn to prior research on cycling-supportive street interventions. A rich literature already suggests that bicycle infrastructure provision results in increased bicycling. In a 2010 literature review, Pucher et al. (2010) found strong evidence from 139 international studies that pro-bicycling infrastructure, policies, and programs successfully increased bicycling. A study of new protected bicycle lanes in five US cities found that more trips were taken on these routes and that they induced more bicycling overall (Monsere et al., 2014). Furthermore, safe, separated bicycle infrastructure leads to improved safety outcomes, for bicyclists as well as drivers (Marshall & Ferenchak, 2019).

While the majority of this research was performed outside of the COVID-19 context, there is evidence that the relationships observed in these pre-pandemic studies hold true during the pandemic as well. A study of European cities, using a difference-in-differences modeling approach that used the abrupt implementation of temporary bicycle lanes as a natural experiment, found that the city's interventions led to large increases in cycling at the city level (Kraus & Koch, 2021).

Micromobility, defined here as small, low-speed transportation devices which are human-powered and may be electronically assisted, has seen far less attention in the literature. The bicycling literature can help provide us some insight into the relationship between supportive infrastructure and other micromobility modes, but there is currently a clear gap in the literature.

In this project, we use data from Lime, a private shared micromobility provider, to implement a quasi-experimental design evaluating the success of Slow Streets programs in Portland, Oakland, San Francisco, and Los Angeles. Because our data are scooter trip counts, when we use the phrase “dockless travel” we are referring to scooter trips and, in the empirical analysis, to our data from Lime. Comparing scooter trip count levels before and during the pandemic between treated street segments and control segments, we find that trips along treated (Slow Streets) segments increased substantially compared to their untreated counterparts. We suggest that these results are likely to be generalizable to bicycles and other non-car and micromobility modes and that cities consider ways to expand their Slow Streets programs to induce further non-motorized travel.

Literature Review

The Covid-19 pandemic has affected many aspects of daily life, and travel is no exception. Soon after the pandemic began, a large amount of research emerged to analyze the impact of COVID-19 on travel behavior. While it is obvious that the way people move changed during the pandemic, the scope and extent of such changes differ by transportation modes. Patterson et al. (2021) evaluated the impact of COVID-19 movement restrictions on motorized and non-motorized travel using time-series models. The research found that while both motorized and non-motorized travel declined after the pandemic, motorized travel was found to recover faster than non-motorized travel.

Further, bicycling has been a more resilient mode of transit than traditional public transportation during the pandemic. Teixeira and Lopes (2020) analyzed subway and bike share system data to compare how COVID-19 differentially affected public transport and bike share ridership. According to the study, bike share has shown higher resilience than the subway with a smaller drop in its ridership and increased travel time. Teixeira and Lopes (2020) also found a modal substitution effect between the two modes – subway to bicycle. Buehler and Pucher (2021) analyzed the change in cycling levels between 2019 and 2020 among 11 European countries and regions of the USA and Canada. According to the study result, cycling levels in the study area mostly increased except for a few exceptions, with higher growth on the weekends. Buehler and Pucher (2021) expect the new cycling trend will persist even after the pandemic, necessitating continual support from governments in developing relevant infrastructures, policies, and programs.

Older literature points to a positive relationship between supportive infrastructure and micromobility. Regarding the impact of relevant infrastructure or policies on micromobility, Buck and Buehler (2012) analyzed the association between bikeshare usage and the existence of bike lanes by using Capital Bikeshare data, and the research found a statistically significant and positive correlation between the bike lanes and bikeshare ridership. In their extensive literature review, Pucher et al. (2010) also concluded that public policy plays a crucial role in encouraging cycling behaviors of people.

As many cities implemented strategies to address the pandemic, opportunities for unprecedented natural experiment settings were identified to analyze the impact of new policy or infrastructure. The implementation of temporary bike lanes provided a natural experiment setting for Kraus and Koch (2021) to investigate the association between bike-friendly infrastructure and cycling with a generalized difference in differences approach. The study used pop-up bike lanes as a treatment finding that pop-up bike lanes have led to an increase in cycling activity in European cities. Lin et al. (2021) examined the impact of COVID cycling infrastructure on the accessibility to various essential places by comparing cycling level of stress with and without COVID cycling infrastructure. The research concluded that COVID cycling infrastructure provided an increased but spatially heterogeneous accessibility to places. In Tucson, Landgrave-Serrano and Stoker (2022) performed a before-after analysis on Slow Streets using a set of controls chosen manually and inspected for balance along micromobility-supportive census characteristics. They find that, while Slow Streets and controls did not differ significantly prior to treatment, they diverged during the pandemic with a positive and significant correlation between Slow Streets treatment and both pedestrian and micromobility levels. As Kim (2022) points out, there is a need for more of such pre-post empirical studies to assess the efficacy of Slow Streets.

In addition to cycling, researchers have explored the dynamics of e-scooters. Thigpen (2020) investigated the case of 5 cities (Berlin, London, New York City, Seattle, and Seoul) with a survey, finding that respondents used shared scooters more frequently during the pandemic than before. Also, the survey result showed that the increased usage of e-scooters is likely to continue post-pandemic. The respondents also indicated a strong support for Slow Streets.

Over two years since the onset of the COVID-19 pandemic, there is an open question of whether Slow Streets should be a permanent fixture in cities. Along similar lines, Noland et al. (2022) find mixed but generally supportive survey results for COVID-19 street closures in New Jersey. Particularly in areas where the interventions happened, they find both positive opinion towards the street closures as well as increased walking activity during the pandemic.

In a theory-building work situating Slow Streets in (or against) the tactical urbanism literature, Kim (2022) notes that Slow Streets were seen as temporary measures during the pandemic, but they have been called for before the pandemic and some believe they should be permanent fixtures. Kim characterizes the argument against them along two lines: firstly, that the pandemic was already complicated and that street closures only added to the complication; and secondly, that the benefit was divided along class and race lines, since those in underserved communities were less likely to utilize Slow Streets for recreation – largely since they were still required to commute to work. At the outset of the pandemic, residents were concerned that Slow Streets would become a gathering place for residents of other communities. In Los Angeles, for example, Slow Streets locations were not divulged publicly, leaving their dissemination to the local organizations that applied for them.

At the same time, Macfarlane et al. (2022) perform a location choice model analysis in Alameda County and find both a benefit from Slow Streets as well as a greater benefit for marginalized residents. From a different angle, Slow Streets and similar initiatives may have the potential to increase equity so long as equity is a focus. Marcus, et al. (2021) highlight the Slow Streets program in San Francisco which was implemented in conjunction with other equity measures like a safety initiative at access points to essential services. Slow Streets have been argued for and against, but the preponderance of evidence appears to support them in both public opinion and micromobility outcomes. However, with a few exceptions, empirical evaluation is missing from this discussion due to their recency. With the continuation of Slow Streets in some cities and its consideration in others, it is essential to quantitatively assess the impact of Slow Streets on all forms of non-motorized travel.

The implementation of slow streets during the pandemic was intended to provide a safer, supportive infrastructure for people to walk, bike, and ride other non-motorized vehicles, and it provides a natural experiment setting. In this paper we contribute to the existing literature by providing an analysis of the impact of supportive infrastructure on e-scooter travel using the cases of San Francisco, Oakland, Portland, and Los Angeles.

Research Approach and Data

Approach A: Difference-in-Differences Analysis

We approach our research question with two quasi-experimental strategies. In the first, we compare the difference between changes in monthly trip levels from 2019 to 2020 in treated and control segments. We obtained a set of slow streets in three cities (San Francisco, Los Angeles, and Oakland), that had a set of segments which were viable for slow streets but were not ultimately implemented (San Francisco and Los Angeles) or a ready control group that was the larger target for implemented slow streets (Oakland), creating a possible set of treatment street segments (those converted to slow streets) and control street segments (those considered for slow street programs, but which were not converted into slow streets).

Since Lime was not operating in San Francisco in July 2019 nor in Oakland in November 2020, we use trip counts in July 2019 and 2020 for Oakland and November 2019 and 2020 for San Francisco, in order to preserve a yearly comparison and mitigate seasonality effects. For Los Angeles, we present the analysis for November 2019 and 2020 since the Lime scooter fleet deployment rate in November 2020 was much more stable than in July 2020.

For the treatment group, we use segments which had been implemented as slow streets by July 1st, 2020, for Oakland or November 1st, 2020, for San Francisco and Los Angeles. For controls in San Francisco, we use all slow streets which were candidates to become slow streets but were not implemented due to lack of community support or issues with the fire department and transit, as indicated in correspondence with SFMTA. In Oakland, since slow streets were chosen from the pool of neighborhood bikeways, we use the on-street neighborhood bikeways which were not chosen as slow streets.

In Los Angeles, we use a set of streets which were recommended by local organizations and discussed at neighborhood council meetings but not implemented by our study period. This information was tracked and provided by Streets for All, an advocacy group. Similar to the set of candidate streets in San Francisco, these streets followed the initial process for becoming a

Slow Street, but were ultimately not recommended by a neighborhood council or implemented by LADOT by the study period.

Since we are running a differences-in-differences approach, a crucial assumption of our first strategy is the parallel trends assumption: that in the counterfactual scenario, where no streets became Slow Streets, outcomes in both the treatment and control group would have followed the trend that we observed in the control group. Therefore, we need to be sure that our treatment and control groups are exchangeable in order to trust our effect estimates. That is, if the control streets had become Slow Streets and the treated streets had not, we would expect to see the same change in trends for the control group that we observed in the treated group.

In order to investigate the similarity between our treatment and control groups, we first inspect the pre-pandemic trip levels (Table 1) and covariate balance of each group (Table 2), with mixed results. In Oakland, we see the largest disparity between both pre-treatment trip levels and factors related to trip generation, our covariates. In San Francisco, our treatment/control group balance is more encouraging, since pre-treatment trip levels are closer and all trip-generating factors other than employment density are similar. In Los Angeles, the similarity between treatments and controls is striking, with both trip levels and trip-generating factors being nearly identical.

Since the control streets in Los Angeles were not provided by citywide agencies but suggested by local organizations to neighborhood councils, we also inspect the streets in Los Angeles for eligibility within Slow Street guidelines. Per discussion with LADOT, streets must have been classified as local, residential, or collector in order to be implemented as Slow Streets. We visually inspected each street segment in our data against an LADOT street classification layer in GIS and found one major area and three minor areas where street segments did not follow these guidelines. In Koreatown, we find nine blocks in the control group which are classified as modified secondary streets. Each of these is on Oxford Avenue between Oakwood Avenue and James M. Wood Boulevard. This street is highly residential with bike lanes to the north, and runs past the backside of a large music venue and grocery store to the south. However, this stretch in the south is otherwise unremarkable and relatively quiet. We also find five control blocks in Hollywood (Yucca Street and Bronson Avenue), six control blocks in Palms (National Boulevard), and five treatment blocks in Highland Park (Monte Vista Avenue, Avenue 59 N, Piedmont Avenue) along modified secondary streets. We include models below both with and without these streets.

Approach B: Fixed Effect Panel Regression Model

We include several covariates in our DID analysis to control for observable built environment differences in the treatment and control segments. Yet in general, the exchangeability between control and treatment segment is always a point of question for the validity of difference-in-differences approaches. For that reason, we use later implemented slow streets as the controls for earlier implemented slow streets as an additional test. Lime provided trip counts data aggregated at weekly level from May 11, 2020, to December 7, 2020, which is a 31-week timeline.

In the second strategy, we use a panel dataset of those weekly trip counts in 2020. In this strategy, instead of using a control group of segments which did not become slow streets, we leverage the staggered rollout of slow streets to use those which were implemented earlier as the treatment group and those which were implemented later as controls in a panel data analysis.

In the panel analysis, we did not use Oakland since all Slow Streets were implemented by June 2020, not providing a meaningful “later implemented” control group. Similarly in Los Angeles, the rollout of Slow Streets was not suitable for a panel analysis. Due to the size and spatial variety of Los Angeles, our analysis revealed that the panel data approach would benefit from analyzing neighborhoods within Los Angeles separately – to avoid matching “early implemented” slow streets in one part of that large city with “later implemented” slow streets in a distant location in the same city. However, most neighborhoods in Los Angeles were implemented at once or in two waves close together in time. For that reason, we did not move forward with a panel analysis of Los Angeles. However, we did add Portland to the panel analysis since they have a longer and more even rollout of slow streets, noting that Portland did not have a ready control group for DID analysis.

Data Sources and Cleaning

This project relies on two scooter trip count data aggregations provided by Lime, with which we compile two datasets. The first comprises geographic scooter trip count data for July 2019, November 2019, July 2020, and November 2020. Each segment is aggregated monthly for 4 time periods: AM peak (6 am to 9 am), midday (9:01 am to 2:59 pm), PM peak (3 pm to 6 pm), and night (6:01 pm to 5:59 am the following day). The data is further split by weekday/weekend, with weekday including Monday, 6 AM through Friday, 6 PM. Table 1 summarizes the descriptive statistics of the data.

Table 1. Descriptive Statistics of Monthly Trip Counts in Oakland, San Francisco, and Los Angeles

	Number of Segments	mean screenline trip counts	median screenline trip counts	Standard deviation of trip counts	Max trip counts
Oakland					
July 2019 Treatment	163	108.99	35	431.30	3327
July 2020 Treatment		12.73	2	61.54	477
July 2019 Control	475	289.66	79	531.66	3398
July 2020 Control		24.69	6	51.87	447
San Francisco					
November 2019 Treatment	285	32.82	9	69.77	957
November 2020 Treatment		36.29	15	59.10	625
November 2019 Control	413	61.65	6	181.53	1693
November 2020 Control		40.67	9	100.74	952
Los Angeles					
November 2019 Treatment	547	91.44	19	162.51	917
November 2020 Treatment		36.01	5	74.76	406
November 2019 Control	512	111.13	26	220.66	2243
November 2020 Control		35.43	5	69.59	511

For the monthly aggregated dataset, we also include covariates representing employment density, amenities, distance to downtown, age composition of local residents, and roadway type. Therefore, both datasets consist of observations of scooter trip counts at each segment-time along the slow streets and controls in each city during the respective study periods.

In Oakland, San Francisco, Los Angeles, and Portland Slow Streets locations and implementation dates were provided directly by Oakland Department of Transportation (OakDOT), San Francisco Municipal Transportation Authority (SFMTA), Los Angeles Department of Transportation (LADOT), and Portland Bureau of Transportation (PBOT), respectively. The scooter trip counts represent a sum of all scooter trips which pass a screenline in the middle of the block: for example, if a street runs east to west, then the total number of scooter trips which cross a north-south screenline at the middle of the block during a given time period would represent the dependent variable of scooter trips for that segment-time. For our independent variable of interest, slow street status, we use the implementation dates provided by each city's transit agency. For any segment-time observation (a street segment, i.e., block, at a given time, i.e., week or month) along the slow streets network, any observation whose implementation date is prior to the date of the observation is considered to be treated. Further, in our panel analysis, we remove segment-times that were treated during the week of observation. This preserves our stable unit treatment value assumption, since if a segment was treated midweek, then it was both treated and untreated during the week of observation. In our difference-in-difference analysis, any segments treated during the respective month of observation were similarly removed from the analysis.

For a robustness check of the analysis, we also added covariates to the difference-in-differences analysis. Those covariates include employment density, distance to downtown, number of amenity facilities within 200 meters, percentage of population aged between 15 to 35, and type of roadway. Those covariates were chosen based on previous studies in the literature that showed associations between those variables and dockless scooter trip generations (e.g., Bai and Jiao, 2020.)

For employment density, we use an aerial interpolation to represent the weighted average of jobs per square kilometer near each block midpoint. We start by joining the total jobs figure from each block group in the Longitudinal Employer-Household Dynamics (LEHD) Origin-Destination Employment Statistics (LODES) Workplace Area Characteristics 2018 data to National Historic Geographic Information System (NHGIS) geographical block group boundaries. Next, we divide total jobs by land area in square kilometers. Finally, we create a 1-kilometer circular buffer around each block midpoint and take the weighted average of each block group's employment density, with weights representing the percentage of the circular buffer made up by each block group. This method results in a weighted average of jobs per square kilometer such that the weight of each block group is directly proportional to the amount of land it covers within 1 kilometer of the segment midpoint in question.

Using the employment density data above, we calculate a downtown in each city representing the centroid of the block group with the highest employment density. Then, a distance in meters to the nearest downtown was computed from each segment midpoint. This represents our distance to downtown covariate.

Next, we pulled all amenities within the four cities from the OpenStreetMap (OSM), a free, online geographic database, as of October 2021 a python package that helps to download geospatial data from OSM (OSMnx). We filter the amenities to include only establishments serving food or beverages with at least 50 observations between all four cities, comprising 'restaurant', 'cafe', 'fast_food', 'bar', 'pub', 'ice_cream', 'nightclub', 'marketplace', and 'food_court'. Finally, we create a 200-meter circular buffer around each segment midpoint and take the sum of the amenities contained within. Therefore, our amenities covariate represents the total number within 200 meters of each block midpoint.

For the age composition of local residents, we use a similar aerial interpolation method to our employment density covariate. Instead of calculating employment density, we use American Community Survey (ACS) 2018 5-Year estimates to calculate the percentage of residents in each block group aged 15-35. We then use a 1-kilometer circular buffer to calculate a weighted average of the percentage of young people in each block group in the same manner as employment density. Therefore, our age composition covariate represents the weighted average of the percentage of residents between ages 15-35 within 1 kilometer of each segment midpoint.

Finally, since the Slow Streets locations were drawn on the OSM network, we kept the 'highway' category in the OSM data to use as an ordinal variable representing roadway type. These categories include motorway (e.g., freeways), trunk (e.g., undivided highway), primary (e.g., large, multilane roads), secondary (smaller roads), tertiary (smaller streets), residential (smaller, quieter streets linking to housing), and unclassified (small streets similar to residential but not linking to housing). The descriptive statistics of covariates can be found in the Table 2 and Table 3 below.

Table 2. Descriptive Statistics of Covariates in Oakland, San Francisco, and Los Angeles

Oakland					
	Variable	**Employment density**	**Distance to downtown (meters)**	**Number of amenities**	**Percentage of population aged between 15 to 35**
Treatment	Mean	3396.94	3677.94	1.40	33.54
	Min/ Max	373.53/ 23397.92	243.95/ 7119.25	0/ 28	22.35/ 42.81
	Std. dev	5025.76	1745.24	3.84	4.53
	# of obs	163			
Control	Mean	4947.31	4108.48	4.21	31.19
	Min/ Max	72.05/ 23882.28	235.17/ 14290.9	0/ 53	11.56/ 45.80
	Std. dev	6255.65	3110.56	8.12	7.16
	# of obs	475			
San Francisco					
Treatment	Mean	3552.61	6347.64	3.73	32.15
	Min/ Max	608.59/ 30579.09	1487.76/ 10377.57	0/ 31	21.49/ 49.10
	Std. dev	4328.14	2225.32	5.72	6.91
	# of obs	285			
Control	Mean	4264.94	6189.61	3.73	31.10
	Min/ Max	637.01/ 64418.06	826.51/ 10323.64	0/ 97	22.05/ 46.78
	Std. dev	7214.93	2337..62	8.30	6.42
	# of obs	413			
Los Angeles					
Treatment	Mean	2934.03	10815.08	0.95	31.74
	Min/ Max	573.14/ 9782.31	3178.97/ 20534.34	0/ 22	15.83/ 43.78
	Std. dev	2577.52	5215.52	2.50	4.71
	# of obs	512			
Control	Mean	2390.07	10517.24	0.70	32.02
	Min/ Max	217.91/ 10477.39	4300.83/ 19347.58	0/ 13	23.49/ 46.87
	Std. dev	2329.16	4373.58	1.97	4.21
	# of obs	547			

Table 3. Roadway Type Classification in Oakland, San Francisco, and Los Angeles

Oakland							
	Motorway	**Primary**	**Residential**	**Secondary**	**Tertiary**	**Unclassified**	**Total**
Treatment	0	0	157	5	1	0	163
Control	19	25	102	188	135	6	475
San Francisco							
Treatment	0	0	243	1	41	0	285
Control	0	0	403	0	10	0	413
Los Angeles							
Treatment	0	1	528	7	11	0	547
Control	0	3	427	12	70	0	512

The second dataset comprises data aggregated similarly, but at the week level for the 31 weeks starting Monday, May 11, 2020, and ending Sunday, December 13, 2020.

Prior to running our analysis, we inspect both datasets for accuracy and outliers. In San Francisco, we found two streets which were included in both candidate and treated data from SFMTA. We remove those segments (accounting for 27 and 34 segments in treatment and control groups, respectively) from the difference-in-difference analysis since treatment status could not be firmly established. In the panel analysis, we remove two segments due to island and outlier status. In the northeast of San Francisco, we remove one island segment since the rest of the segments follow a linear pattern. In the east, we remove one segment due to outlier status – this is likely since the segment midpoint in OSM places it close to an intersection, and it is likely to catch both east-west (slow street) and north-south (non-slow-street) scooter traffic. We also checked our difference-in-difference dataset for outliers but did not identify any. This is feasible since the two datasets are aggregated at a different temporal scale and in different time periods.

In Oakland, we remove any control segments which are classified as category one by the Oakland DOT from the difference-in-difference analysis. These segments are bike lanes which are not on-street and are likely to be geographically inaccurate since segments were snapped to the OSM drive network, as well as qualitatively different from on-street treatment and control segments. We also remove any bikeways which had not been implemented yet, as well as any treatment segments which were not implemented as Slow Streets by the study period. These account for 123 treatment segments and 375 control segments.

In Portland and Los Angeles, we first used a set of unofficial shapefiles since we had not yet received official shapefiles from local agencies. In the case of Los Angeles, we did not find any discrepancies between our official and unofficial data. However, in Portland, we identified several segments we had hand-coded based on unofficial data which were geographically inaccurate. Upon receiving detailed official data, we flagged and removed all incorrectly-drawn segments (accounting for 95). Therefore, we are confident that all segments we include in both panel and difference-in-differences analyses are accurate and without outliers, but in Portland there are some slow streets segments (about 10 percent) which are not included in our analysis. Appendix A2 shows the number of segments in the original data and the number in each model, by city.

Methods

Approach A: Difference-in-Differences Analysis

For our difference-in-differences approach, we compare dockless trip volumes on treatment and control streets before and after the slow street implementation. Using our screenline dockless trip count data, we estimate the following regression:

$$Trip\ Volume = a + b_1 * Treatment + \ b_2 * After + \ b_3 * (Treatment \cdot After) + u$$

where $Trip\ Volume =$ screenline counts of dockless trips that cross the mid-point of the segment for the entire month,

$Treatment = 1$ if the segment had a slow street implemented, 0 otherwise

$After = 1$ for the month after the slow street was implemented (July or November 2020 for Oakland and San Francisco, respectively), 0 otherwise

$Treatment \cdot After =$ Interaction of the Treatment and After indicator variables

$u =$ regression error term

$a, b_1, b_2, and\ b_3$ are coefficients to be estimated, and b_3 is our main interest.

For Oakland, we use July of 2019 and 2020, and for San Francisco and Los Angeles, we use November of 2019 and 2020 for the DID analysis. For the months in use, the months are not affected by any closure dates in each city's slow street implementation.

Approach B: Fixed Effect Panel Regression Model

Our fixed effect model based on the different implementation dates controls for the issue of control group validity. We use later implemented slow streets as the controls for earlier implemented slow streets. We estimate a fixed effect panel regression model for San Francisco and Portland.

For the analysis of San Francisco, we are only using the data after week 8 (June 29) for several reasons. First, Lime officially restarted its operation during June and July, which makes the early period data unreliable. If the number of deployed fleets are too small and the coverage is not large enough, we cannot be sure that our estimates are reliable. In San Francisco, normalized fleet counts, which measure the deployed scooter fleet each week relative to the maximum fleet count over the study period, recovers to about 60% starting from week 8. Moreover, as Figure 1 shows, the average trip counts per segment for all our segments showed a sharp increase between week 7 and 8. Thus, we consider that week 9 a reasonable starting point for panel the analysis.

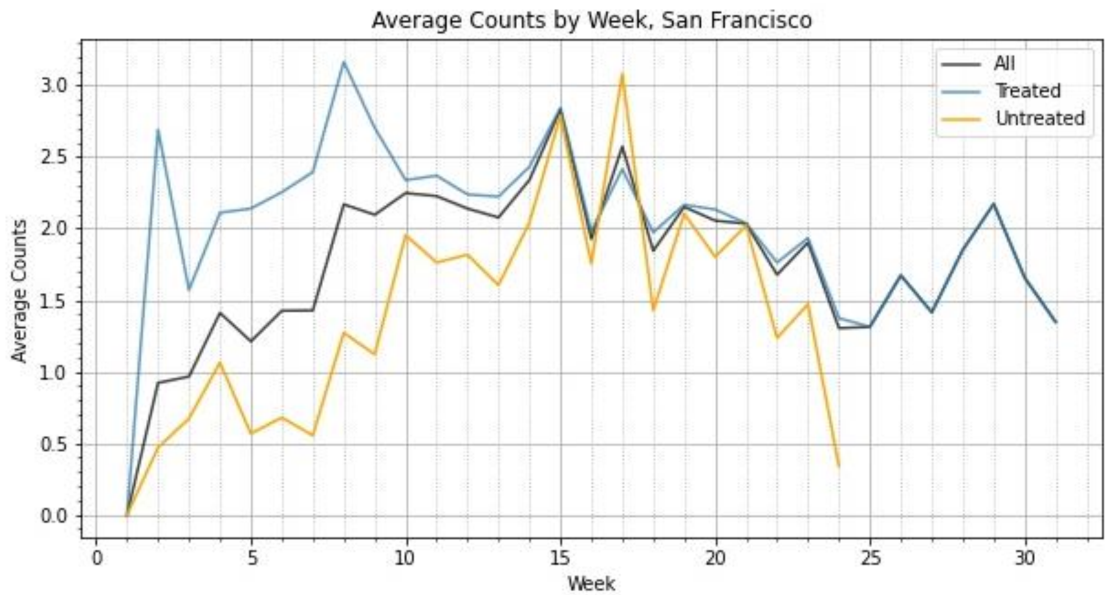

Figure 1. Average Trip Counts by Week in San Francisco

For Portland, our second study area for the panel model, a large amount of street segments was implemented at an early stage (Table 4). However, since the provided Lime data is available between May 11th of 2020 and December 13th of 2020, slow streets that were implemented before week 1 (May 11) should be removed from our analysis. Moreover, if street segments were converted to slow streets on week 1, the segments would not have any changes in the treatment status. Since we want to use the 'before' treated as controls for the slow street implementation, the slow street segments that were implemented in week 1 should also be excluded from our analysis.

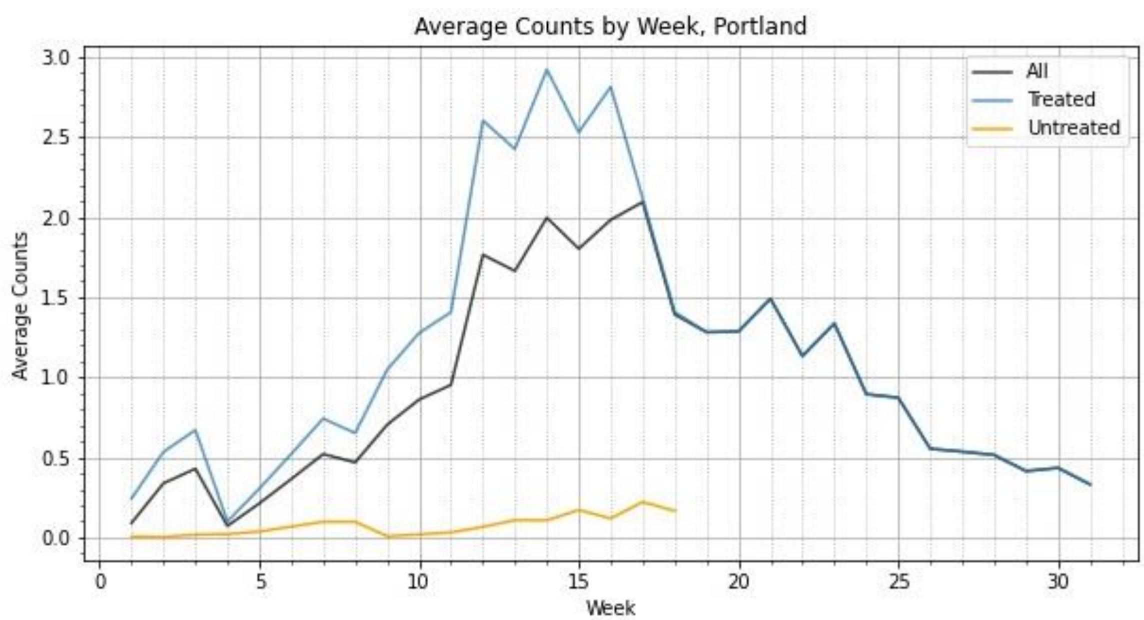

Figure 2. Average Trip Counts by Week in Portland

Table 4. Week Identification and Slow Street Implementation by Week, Portland

Week of Implementation	Week Starts	Week ends	Number of segments (Total number of observation)	%	Cum. %
Week 0		2020-05-10	227 (7037)	35.69	35.69
Week 1	2020-05-11	2020-05-17	174 (5394)	27.36	63.05
Week 3	2020-05-25	2020-05-31	11 (341)	1.73	64.78
Week 4	2020-06-01	2020-06-07	5 (155)	0.79	65.57
Week 7	2020-06-22	2020-06-28	9 (279)	1.42	66.98
Week 12	2020-07-27	2020-08-02	1 (31)	0.16	67.15
Week 14	2020-08-10	2020-08-16	13 (403)	2.04	69.18
Week 16	2020-08-24	2020-08-30	188 (5828)	29.56	98.74
Week 18	2020-09-07	2020-09-13	8 (248)	1.26	100
TOTAL			636 (19716)	100	

Using our data on dockless trip volumes in San Francisco and Portland, we estimate the following fixed effect panel regression model:

$$Trip\ Volume_{i,t} = a + b_1 * Treatment_{i,t} + \sum\beta_t * Week_t + u_i + v_{i,t}$$

where i indicates street segment id and t indicates the week id,

$Trip\ Volume =$ screenline counts of dockless trips that cross the mid-point of the segment for each week,

$Treatment = 1$ if the segment had a slow street implemented, 0 otherwise,

$u =$ fixed effect, and

$v\ =$ pure residual

$a, b_1, and\ \beta s$ are coefficients to be estimated, and our main interest is b_1 since the βs only capture the weekly trends.

Results

Approach A: Difference-in-Differences Analysis

Oakland

In Table 1, treatment segments in Oakland show decline in counts, from a sample mean of 108.99 in July 2019 to a sample mean of 12.73 in July of 2020. The control segments in Oakland show both a higher mean level of counts and a larger decline, from a sample mean of 289.66 in July of 2019 to a sample mean of 24.69 in July 2020.

Table 5 presents the results of the difference-in-differences (DID) regression fit on the full sample in Oakland. The data we originally received includes observations of trip counts for the entire month (pre- and post-treatment, respectively) at each segment midpoint during each time period on both weekdays and weekends. In our models, we aggregate the data in several ways. We have four different times of day observation by weekday and weekend, leading to 8 models (AM weekday, AM weekend, MD weekday, MD weekend, PM weekday, PM weekend, NT weekday, and NT weekend). In the TOTAL column, we sum trip counts for each time of day on both weekdays and weekends together, such that each observation represents the full sum of trips at a given segment midpoint over the full month. The Overall column represents a model where counts are disaggregated such that there is a monthly observation for each combination of weekday/weekend and time period at each segment midpoint.

The coefficient on the Interaction of treatment and after indicator variable (in bold) is the effect of the treatment on treated – the impact of slow street implementation relative to the counterfactual of the control group. In Oakland, the treatment effect of slow street implementation for total trips for the month is an increase in counts of 168.7 per segment, which is equivalent to 154.8 percent of July 2019 mean value among those segments (108.99). For all times-of-day (AM, MD, PM, NT) and for weekday and weekend, the treatment effect of slow street turned out to be positive and statistically significant, as highlighted in Table 5.

In Oakland, treatment effect was the largest during PM and MD weekday. And the observed treatment is weaker in weekend and during morning hours. During weekdays of PM peak hours, the treatment segments observed increase in scooter traffic volume of 46.31 trips on average per segment, which is 42.5 percent of the pre-treatment mean trip value (108.99). In the morning hours, the treatment effects were an increase of 27.93 and 0.82 trips on average per segment, for weekday and weekend respectively, which are equivalent to 25.62 percent and 0.75 percent of the pre-treatment mean (108.99). For all four different times-of-day, the treatment effect was greater for weekday than weekend.

We note that the after-observation period in Oakland is July 2020, still early in the pandemic and during what was, at that time, California's highest COVID case load peak. We believe the large drop in trip volumes in Oakland might in part reflect the early stage of the pandemic, including concerns about fomite transmission of the disease which could have dissuaded dockless users in ways that would have been less common in November 2020. We note that the purpose of the control segments is to provide a control for time trends which include the state of the pandemic. The changes in the trip volumes between two periods for Oakland, San Francisco, and Los Angeles are shown respectively in Figure 3 and Figure 4 and Figure 5. The labels represent the raw changes in the trip counts in each segment. The legend and categories for changes in trip counts is the same for each map, to give comparability across the three cities. For that reason, not all categories are populated equally in each map.

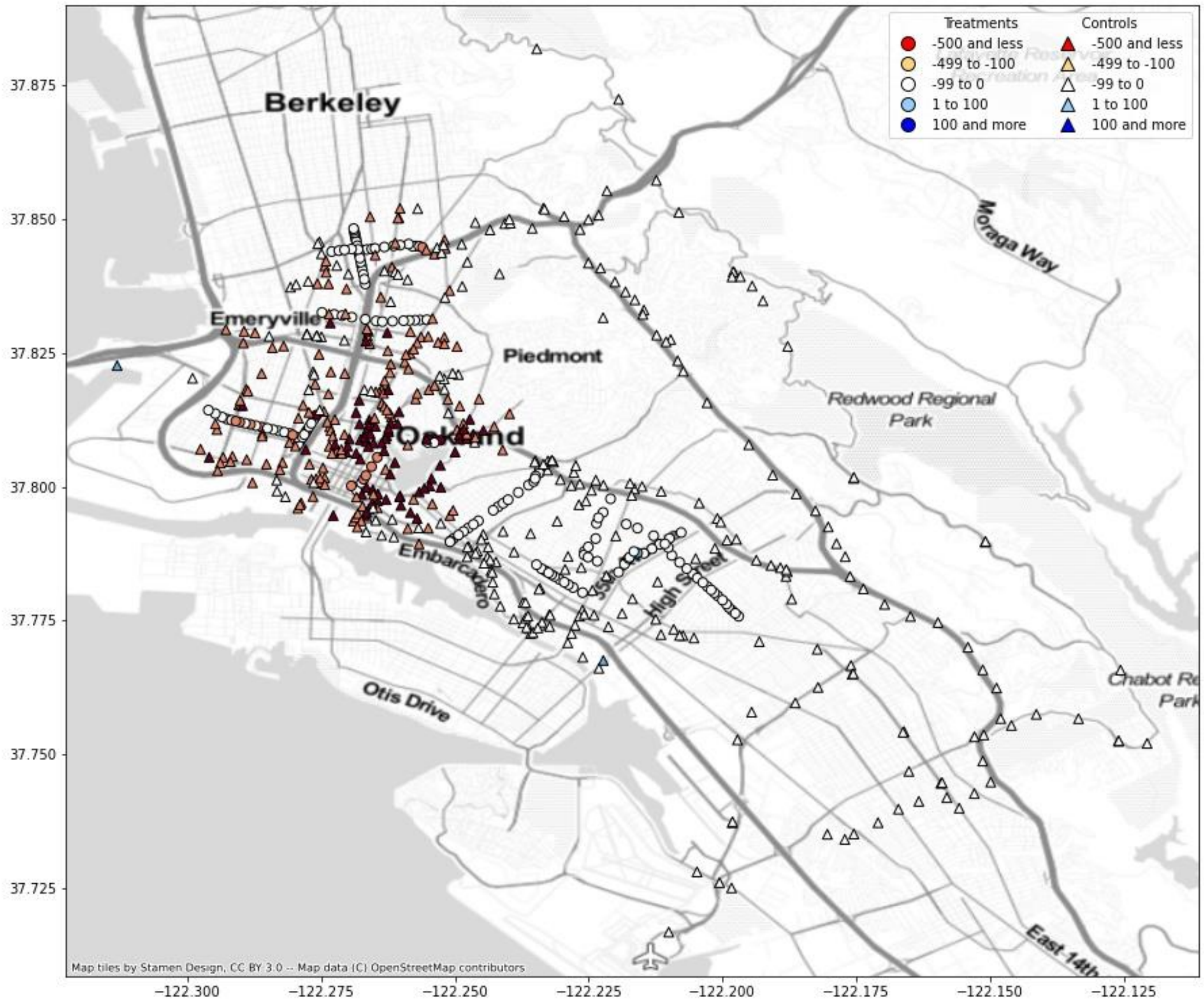

Figure 3. Changes in Trip Volumes from Pre-treatment to Post-treatment in Oakland

Table 5. Difference-in-Differences Analysis Results, Oakland

	Overall	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
treatment	-22.58***	-180.67***	-28.62***	-0.94***	-40.00***	-8.93***	-48.26***	-5.47***	-34.84***	-13.61***
	(2.33)	(41.64)	(5.96)	(0.24)	(8.33)	(2.78)	(10.27)	(1.80)	(9.87)	(3.48)
After	-33.12***	-264.96***	-39.90***	-1.21***	-58.87***	-13.18***	-69.60***	-7.59***	-57.11***	-17.49***
	(1.45)	(24.53)	(4.34)	(0.14)	(5.13)	(1.47)	(6.64)	(1.05)	(5.23)	(2.07)
1.treatment #1.after	**21.09*** **	**168.70*** **	**27.93*** **	**0.82*** **	**37.82*** **	**7.98*** **	**46.31*** **	**4.26** **	**32.60*** **	**10.99*** **
	(2.34)	**(41.98)**	**(5.96)**	**(0.25)**	**(8.38)**	**(2.85)**	**(10.33)**	**(1.89)**	**(9.93)**	**(3.62)**
Constant	36.21***	289.66***	41.01***	1.46***	63.43***	15.57***	74.19***	10.05***	61.65***	22.29***
	(1.45)	(24.41)	(4.34)	(0.14)	(5.12)	(1.44)	(6.62)	(1.01)	(5.21)	(2.00)
Observations	10,208	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276
R-squared	0.06	0.11	0.09	0.07	0.12	0.07	0.10	0.05	0.10	0.07

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 6. Difference-in-Differences Analysis Results, San Francisco

	Overall	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
1.treatment	-3.60***	-28.83***	-1.26**	-0.26**	-8.09***	-4.50***	-5.40***	-1.50	-4.15***	-3.67***
	(0.52)	(9.84)	(0.52)	(0.11)	(2.41)	(1.69)	(1.70)	(0.95)	(1.45)	(1.41)
1.after	-2.62***	-20.98**	-2.14***	-0.21*	-5.55**	-4.38***	-2.94	-0.82	-3.45**	-1.49
	(0.54)	(10.22)	(0.46)	(0.11)	(2.49)	(1.66)	(1.84)	(1.07)	(1.46)	(1.61)
1.treatment #1.after	**3.06*** **	**24.45** **	**1.25** **	**0.19**	**9.00*** **	**4.49** **	**3.22**	**0.67**	**3.19* **	**2.43**
	(0.61)	**(11.56)**	**(0.55)**	**(0.12)**	**(2.69)**	**(1.88)**	**(1.99)**	**(1.22)**	**(1.79)**	**(1.91)**
Constant	7.71***	61.65***	3.37***	0.55***	13.44***	9.77***	10.34***	5.25***	9.95***	8.98***
	(0.47)	(8.93)	(0.45)	(0.10)	(2.33)	(1.55)	(1.61)	(0.85)	(1.20)	(1.11)
Observations	11,168	1,396	1,396	1,396	1,396	1,396	1,396	1,396	1,396	1,396
R-squared	0.01	0.01	0.03	0.01	0.01	0.01	0.01	0.00	0.01	0.00

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

San Francisco

In San Francisco, the treatment segments increased from a sample mean of 32.82 to a sample mean of 36.29 between November 2019 and November 2020. In contrast, the control segments decreased in volumes, from a sample mean of 61.65 to a sample mean of 40.67 during the same period (Table 1).

The pattern in San Francisco is more intuitive: on average, scooter segment volumes decreased on control segments while increasing on treatment segments. The pattern in Oakland shows that the control segments have higher trip volumes than the treatment segments, in both the before and after time periods, and that both the treatment and control groups have, on average, large declines after slow streets were implemented.

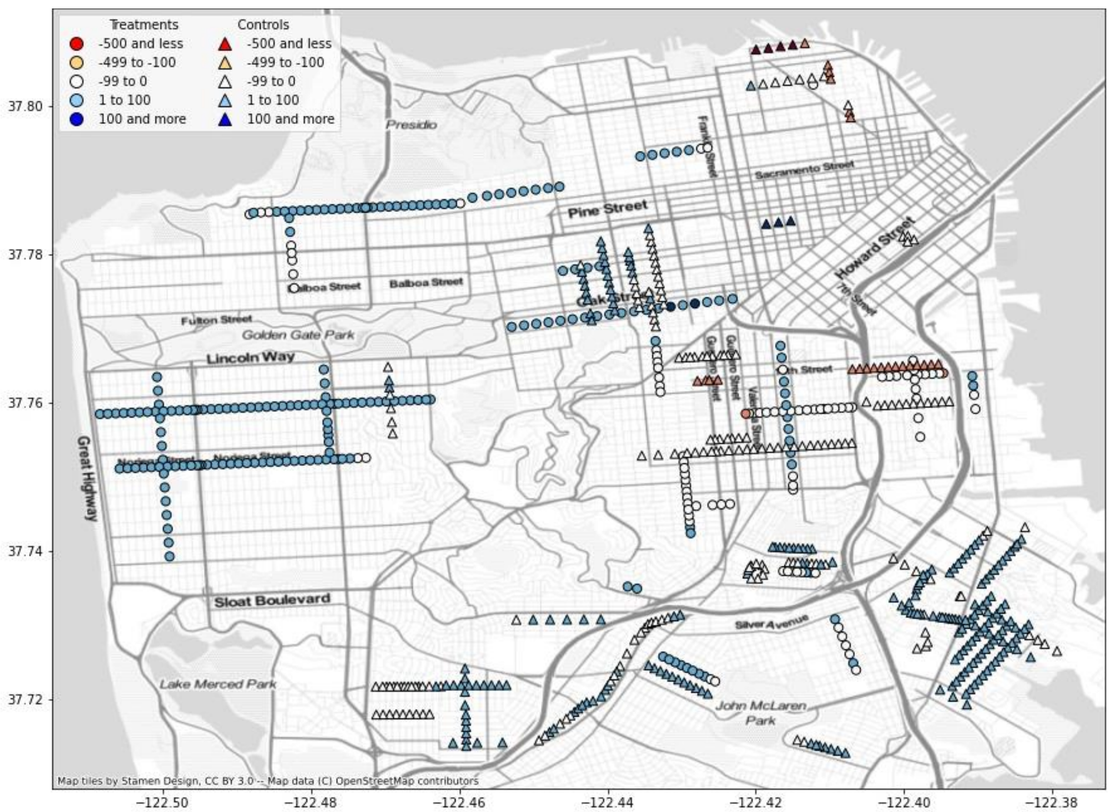

Figure 4. Changes in Trip Volumes from Pre-treatment to Post-treatment in San Francisco

The treatment effect in San Francisco is a volume increase of 24.45, on average, per segment relative to control segments, which is equivalent to 75 percent increase relative to November 2019 mean value of segment trip volumes among the treatment in San Francisco (32.82). Similar to Oakland, even when we look closer to different times-of-day (AM, Mid-day, PM, Night time) and to weekend and weekday, the treatment effect of slow street implementation was positive and statistically significant for AM weekday, MD weekday, MD weekend, and NT weekday. The treatment effect of slow streets was found to be strongest during mid-day hours in weekday with coefficient value of 9.0, which is 168.2 percent increase compared to 2019 mid-day hour weekday average trip counts per treatment segment (5.35) as shown in Appendix A1. The impact was smallest during morning hours weekday, with volume increase of 1.25 on average per segment, which is 59 percent increase compared to November 2019 weekday morning mean (2.11).

Los Angeles

In Los Angeles, treatment segments showed a decline in trip counts (Table 1), from a sample mean of 91.44 in November 2019 to a sample mean of 36.01 in November of 2020. During the same period, the control segments also showed decline in trip volumes, from a sample mean of 111.13 to a sample mean of 35.43. While both treatment and control showed decline in scooter trips, the drop in control segments (75.7) was larger than the drop in treatment segments (55.43).

The DID analysis results for Los Angeles can be found in Table 7. In Los Angeles, we did not find statistically significant impact of slow street implementation on total trip counts, which aggregates all different times-of-day and weekend/weekday. However, more refined analysis showed some positive and statistically significant impact of slow street implementation. The impact of treatment was largest during night-time of weekdays, with coefficient value of 4.55 meaning that in November 2020, the treatment segments had 4.55 more trips compared to November 2019, relative to control group. The results also suggest positive and statistically significant impact of slow street implementation during morning peak hours in weekday, mid-day weekend, and PM peak hours in weekend. Figure 5 shows the changes in trip volumes after treatment in Los Angeles.

Table 7. Difference-in-Differences Analysis Results, Los Angeles

	Overall	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
1.treatment	-2.46***	-19.69	-2.30**	-0.36*	-4.74*	-2.03*	-2.32	-1.53**	-3.92*	-2.50
	(0.64)	(11.97)	(0.91)	(0.20)	(2.59)	(1.15)	(1.76)	(0.73)	(2.30)	(2.61)
1.after	-9.46***	-75.71***	-5.90***	-1.14***	-15.24***	-8.25***	-11.24***	-4.76***	-14.29***	-14.87***
	(0.54)	(10.23)	(0.80)	(0.17)	(2.29)	(0.97)	(1.52)	(0.62)	(1.97)	(2.10)
1.treatment #1.after	**2.53*** **	**20.27**	**2.43** **	**0.24**	**4.53**	**2.44** **	**1.67**	**1.73** **	**4.55* **	**2.69**
	(0.68)	**(12.77)**	**(0.95)**	**(0.22)**	**(2.80)**	**(1.23)**	**(1.86)**	**(0.80)**	**(2.49)**	**(2.75)**
Constant	13.89***	111.13***	7.81***	1.84***	24.03***	11.60***	16.40***	7.12***	20.93***	21.40***
	(0.52)	(9.75)	(0.78)	(0.15)	(2.17)	(0.93)	(1.44)	(0.59)	(1.87)	(2.01)
Observations	16,944	2,118	2,118	2,118	2,118	2,118	2,118	2,118	2,118	2,118
R-squared	0.03	0.05	0.05	0.04	0.04	0.06	0.06	0.05	0.04	0.04

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

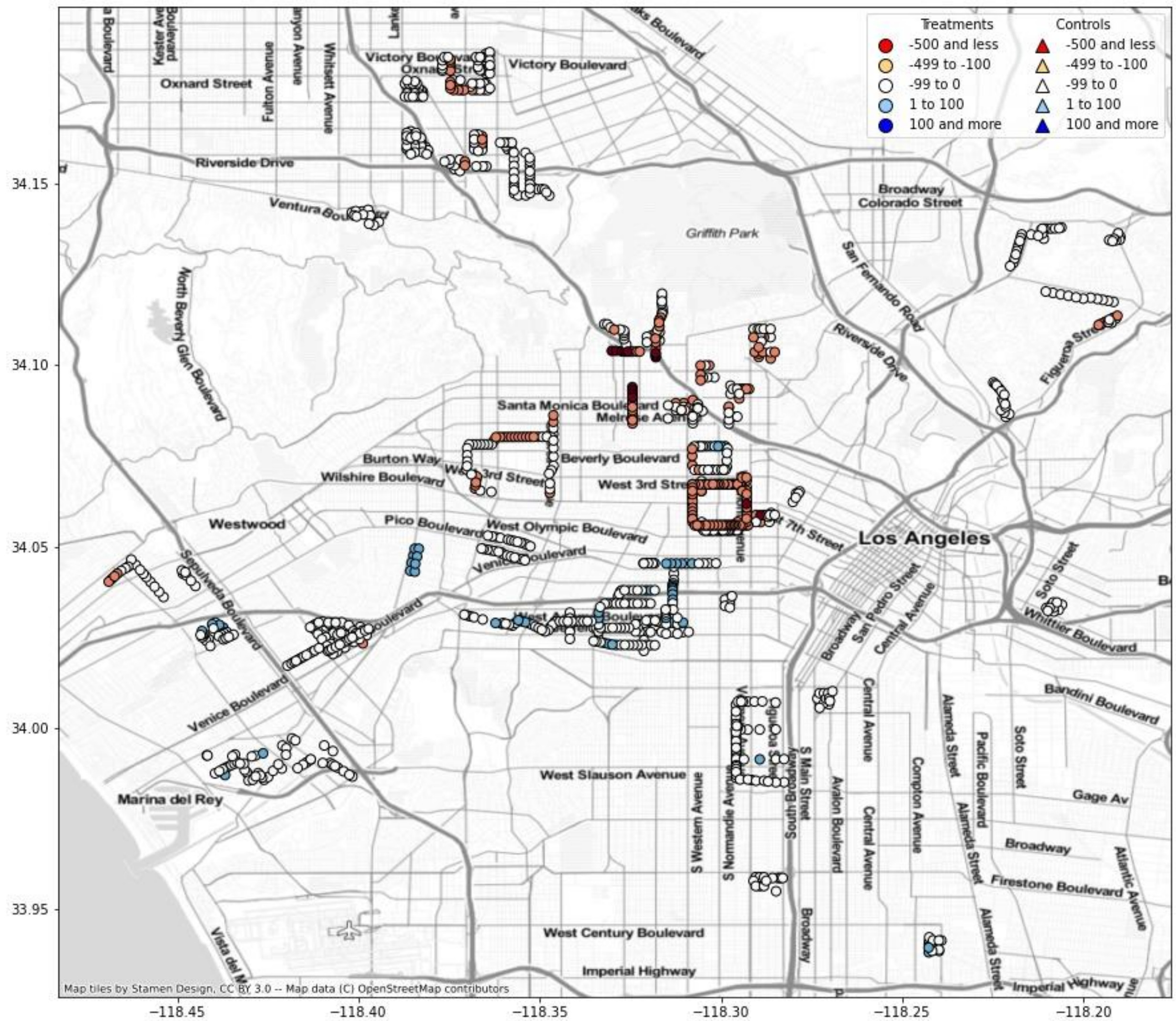

Figure 5. Changes in Trip Volumes from Pre-treatment to Post-treatment in Los Angeles

As discussed in our research approach section, we identified several segments in the Los Angeles control group and a few in the treatment group which were classified by LADOT as modified secondary streets. Table 8 below shows tabulation of roadway type classification of Open Street Map and LADOT. Among 1,059 segments, 38 segments are classified as secondary streets based on LADOT classification. However, only 2 among 38 segments are classified as secondary streets according to OSM roadway type classification. Most of those 38 segments are either residential or tertiary, based on OSM classification.

Table 8. Secondary Street Classification of All Segments in Los Angeles

	OSM Roadway Type Classification				
LADOT classification	Primary (Control/ Treatment)	Secondary (Control/ Treatment)	Tertiary (Control/ Treatment)	Residential (Control/ Treatment)	Total (Control/ Treatment)
Non-Secondary	3 (2/1)	17 (10/7)	67 (59/8)	934 (409/525)	1,021 (280/541)
Secondary	1 (1/0)	2 (2/0)	14 (11/3)	21 (18/3)	38 (32/6)
Total	4 (3/1)	19 (12/7)	81 (70/11)	955 (427/528)	1,059 (512/547)

Under OSM classification, 'Primary' refers to primary highway or arterial road and 'Secondary' refers to a highway which is not part of a major route, but nevertheless forming a link in the national route network, major arterial roads. 'Tertiary' roads are defined to be roads connecting smaller settlements, and within large settlements for roads connecting local centers. OSM tertiary roads commonly also connect minor streets to more major roads. Finally, 'Residential' streets are those serve as an access to housing, without function of connecting settlements. Residential roads are often lined with housing and they provide access to, or within, residential areas but they are not normally used as through routes.

LADOT Geohub provided roadway classification data, and they classified streets as Alley, Collector, Collector Street, Divided Major Highway – Class II, Local, Local Street, Major Highway – Class I, Major Highway – Class II, Major Highway Class III, Modified Collector Street, Modified Local Street, Modified Major Highway, Modified Major Highway Class II, Modified Secondary, Modified Secondary Highway, Private Street, Proposed Collector, Proposed Modified Secondary, Scenic Collector Street, Scenic Divided Major Highway – Class II, Scenic Divided Secondary Highway, Scenic Major Highway – Class I, Scenic Major Highway – Class II, Scenic Secondary Highway, Secondary Highway, Und. Or Prop. Collector Street, Und. Or Prop. Local Street, Und. Or Prop. Major Hwy – Class II, Und. Or Prop. Private Street, Und. Or Prop. Scenic Mjr Hwy – Class II, Und. Or Prop. Scenic Secondary Hwy, Unknown Type or Closed Street.

According to LADOT, 'Local Street', 'Collector Street', 'Secondary Highway', 'Major Highway-Class II' takes up 60.87%, 16.47%, 8.88% and 8.81% respectively, which accounts for more than 95% of all street segments in LA, and all remaining categories accounts for less than 5% of street segments in LA.

According to Table 8 above, among 38 secondary streets (based on LADOT classification), 32 segments are control segments. However, only two of them are classified as secondary street based on OSM classification. Many of them are either residential or tertiary on OSM network. For treatment segments, although LADOT said that they did not implement slow streets on secondary streets, 6 treatment segments turned out to be on secondary streets. None of those streets were classified as secondary streets in OSM network. Rather, they were classified as residential streets or tertiary streets in OSM.

Based on the roadway type classifications of OSM and LADOT, we perform a second difference-in-differences analysis after removing the street segments that are classified as 'secondary' streets by both OSM and LADOT (2 segments). The results can be found in Table 9. Although only two segments were removed, which are both from the control group, the magnitude of coefficient dropped for all models, and the coefficient of interest for NT weekday regression lost its statistical significance.

Table 10 below shows DID regression results after removing secondary streets that are both classified as secondary streets by OSM and LADOT (2 segments) and those that are classified as primary by OSM and secondary by LADOT (1 segments). While mid-day weekend model lost its statistically significance, the coefficients of overall, AM weekday, and PM weekend model remained statistically significant and positive with small decrease in the size of coefficients.

Table 9. Difference-in-Differences Analysis Results after removing secondary streets, Los Angeles

	Overall	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
1.treatment	-1.67***	-13.36	-1.74**	-0.30	-3.02	-1.54	-1.29	-1.21*	-2.83	-1.44
	(0.59)	(11.12)	(0.81)	(0.19)	(2.29)	(1.10)	(1.61)	(0.70)	(2.17)	(2.50)
1.after	-8.77***	-70.19***	-5.39***	-1.10***	-13.67***	-7.84***	-10.33***	-4.51***	-13.39***	-13.96***
	(0.49)	(9.20)	(0.70)	(0.16)	(1.95)	(0.91)	(1.33)	(0.58)	(1.81)	(1.96)
1.treatment #1.after	**1.84*** **	**14.75**	**1.92** **	**0.19**	**2.96**	**2.03* **	**0.75**	**1.47* **	**3.65**	**1.78**
	(0.64)	**(11.97)**	**(0.86)**	**(0.22)**	**(2.53)**	**(1.18)**	**(1.71)**	**(0.76)**	**(2.37)**	**(2.65)**
Constant	13.10***	104.80***	7.25***	1.78***	22.32***	11.12***	15.37***	6.80***	19.84***	20.33***
	(0.46)	(8.69)	(0.67)	(0.14)	(1.80)	(0.87)	(1.25)	(0.55)	(1.70)	(1.87)
Observations	16,912	2,114	2,114	2,114	2,114	2,114	2,114	2,114	2,114	2,114
R-squared	0.04	0.05	0.05	0.04	0.04	0.06	0.06	0.05	0.04	0.04

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 10. Difference-in-Differences Analysis Results after removing secondary and primary streets, Los Angeles

	Overall	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
1.treatment	-1.42**	-11.39	-1.60**	-0.27	-2.67	-1.34	-1.06	-1.09	-2.40	-0.97
	(0.59)	(10.96)	(0.80)	(0.19)	(2.27)	(1.08)	(1.59)	(0.69)	(2.13)	(2.46)
1.after	-8.61***	-68.91***	-5.29***	-1.08***	-13.47***	-7.68***	-10.18***	-4.43***	-13.12***	-13.65***
	(0.48)	(8.98)	(0.68)	(0.16)	(1.92)	(0.89)	(1.31)	(0.57)	(1.76)	(1.90)
1.treatment #1.after	**1.68*** **	**13.47**	**1.82** **	**0.17**	**2.75**	**1.88**	**0.61**	**1.39* **	**3.38**	**1.47**
	(0.63)	**(11.80)**	**(0.85)**	**(0.22)**	**(2.50)**	**(1.16)**	**(1.70)**	**(0.75)**	**(2.33)**	**(2.60)**
Constant	12.85***	102.83***	7.11***	1.76***	21.97***	10.92***	15.14***	6.68***	19.41***	19.86***
	(0.45)	(8.48)	(0.66)	(0.14)	(1.77)	(0.84)	(1.23)	(0.54)	(1.65)	(1.82)
Observations	16,896	2,112	2,112	2,112	2,112	2,112	2,112	2,112	2,112	2,112
R-squared	0.04	0.05	0.05	0.04	0.04	0.06	0.06	0.05	0.04	0.04

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Robustness Check

For all three cities, we perform a robustness check in Table 11, Table 12, and Table 13 by including the trip-generating covariates jobs per square kilometer (jobdensity), distance to downtown (dtdis), number of amenities within 200 meters (amenities), percentage of residents aged 15-35 within 1 kilometer, and OSM roadway type (rwtype). After including these covariates, we see nearly identical results in effect size and significance. In San Francisco, the AM weekend and PM weekday treatment effect becomes significant at the $p < .1$ significance level. In Los Angeles, we use all street segments including those classified modified secondary as in our initial DID model. The treatment effect in the TOTAL model, representing monthly aggregated counts including all time periods at each street segment, becomes significant at the $p < .05$ level. The mid-day weekday treatment effect also becomes significant at the $p < .05$ level. Even after adding covariates which contribute to dockless trip levels, we find a similar treatment effect for Slow Street implementation on dockless trips.

Table 11. Robustness check with covariates, Oakland

	Overall	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
1.treatment	-5.40	-43.19	-15.23*	-0.19	-11.77	0.39	-15.20	1.69	-3.26	0.38
	(3.38)	(59.93)	(8.49)	(0.35)	(11.58)	(4.12)	(14.35)	(2.57)	(14.55)	(4.91)
1.after	-33.12***	-264.96***	-39.90***	-1.21***	-58.87***	-13.18***	-69.60***	-7.59***	-57.11***	-17.49***
	(1.38)	(22.45)	(4.13)	(0.13)	(4.56)	(1.37)	(6.16)	(0.98)	(4.85)	(1.89)
1.treatment #1.after	**21.09*****	**168.70*****	**27.93*****	**0.82*****	**37.82*****	**7.98*****	**46.31*****	**4.26****	**32.60*****	**10.99*****
	(2.24)	**(39.27)**	**(5.73)**	**(0.23)**	**(7.76)**	**(2.64)**	**(9.80)**	**(1.74)**	**(9.30)**	**(3.32)**
jobdensity	0.00***	0.01***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
dtdis	-0.00***	-0.02***	-0.00***	-0.00***	-0.00***	-0.00***	-0.01***	-0.00***	-0.01***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
amenities	0.43***	3.42	0.31	0.02**	1.20**	0.26*	0.46	0.15	0.67	0.36*
	(0.15)	(2.29)	(0.37)	(0.01)	(0.52)	(0.14)	(0.62)	(0.10)	(0.49)	(0.19)
per15to35	0.05	0.42	0.16	0.01*	0.31	-0.11	0.29	-0.10**	-0.04	-0.09
	(0.06)	(1.08)	(0.17)	(0.01)	(0.22)	(0.07)	(0.27)	(0.05)	(0.24)	(0.09)
1.rwtype (motorway)	1.45	11.58	6.69	0.46	5.65	-0.40	2.14	-1.24	-2.93	1.21
	(3.52)	(57.00)	(9.50)	(0.48)	(12.33)	(2.48)	(18.56)	(1.82)	(11.43)	(3.70)
2.rwtype (primary)	9.87**	78.94	14.97	0.66*	20.16	4.12	14.45	1.99	13.20	9.39**
	(3.95)	(62.11)	(11.40)	(0.38)	(13.67)	(2.85)	(18.48)	(2.07)	(12.68)	(4.21)
3.rwtype (residential)	-11.30***	-90.44	-3.97	-0.40	-14.23	-6.47**	-27.43	-5.88***	-23.85*	-8.21**
	(3.45)	(57.32)	(9.28)	(0.34)	(12.17)	(2.99)	(16.98)	(2.10)	(12.61)	(4.06)
4.rwtype (secondary)	9.96***	79.64	13.39	0.63**	16.55	6.25**	11.24	3.61*	17.99	9.99***
	(3.17)	(52.25)	(8.36)	(0.31)	(11.18)	(2.71)	(15.80)	(1.99)	(11.43)	(3.76)
5.rwtype (tertiary)	7.37**	58.98	8.04	0.40	13.12	3.23	13.22	1.52	12.58	6.86*
	(3.22)	(52.10)	(7.98)	(0.30)	(11.19)	(2.44)	(16.67)	(1.88)	(11.35)	(3.62)
Constant	31.53***	252.25***	28.86***	0.98**	36.42***	19.77***	63.26***	14.03***	65.37***	23.55***
	(3.99)	(69.08)	(10.42)	(0.42)	(14.11)	(4.28)	(19.19)	(2.94)	(15.73)	(5.56)
Observations	10,208	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276
R-squared	0.15	0.25	0.17	0.18	0.30	0.20	0.23	0.20	0.23	0.23

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 12. Robustness check with covariates, San Francisco

	Overall	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
1.treatment	-5.21***	-41.68***	-1.56***	-0.39***	-11.09***	-6.62***	-7.85***	-3.08***	-5.45***	-5.63***
	(0.64)	(11.57)	(0.51)	(0.13)	(3.05)	(2.05)	(2.05)	(1.12)	(1.54)	(1.58)
1.after	-2.62***	-20.98**	-2.14***	-0.21**	-5.55***	-4.38***	-2.94*	-0.82	-3.45***	-1.49
	(0.48)	(8.37)	(0.41)	(0.09)	(2.13)	(1.40)	(1.53)	(0.87)	(1.17)	(1.30)
1.treatment #1.after	**3.06*****	**24.45****	**1.25****	**0.19***	**9.00*****	**4.49*****	**3.22***	**0.67**	**3.19****	**2.43**
	(0.57)	**(10.36)**	**(0.50)**	**(0.11)**	**(2.47)**	**(1.72)**	**(1.81)**	**(1.11)**	**(1.55)**	**(1.73)**
jobdensity	-0.00	-0.00	0.00	-0.00	-0.00	-0.00**	0.00	-0.00**	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
dtdis	-0.00***	-0.01***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
amenities	0.41***	3.27***	0.05	0.04***	0.67***	0.46***	0.56***	0.32***	0.53***	0.64***
	(0.05)	(0.98)	(0.03)	(0.01)	(0.24)	(0.16)	(0.17)	(0.10)	(0.14)	(0.16)
per15to35	0.20***	1.61***	0.17***	0.02***	0.31***	0.25***	0.24***	0.13***	0.31***	0.16**
	(0.03)	(0.51)	(0.04)	(0.01)	(0.12)	(0.08)	(0.09)	(0.05)	(0.08)	(0.08)
2.rwtype (primary)	4.56	36.44	7.22	0.19**	3.41	1.57	16.65*	-2.09	4.25	5.24
	(2.96)	(41.51)	(5.74)	(0.07)	(7.40)	(3.40)	(9.74)	(2.53)	(5.02)	(7.86)
3.rwtype (residential)	13.49***	107.91***	1.59*	0.96**	25.47***	17.11***	20.55***	13.34***	11.38***	17.52***
	(1.91)	(34.04)	(0.84)	(0.37)	(8.83)	(5.79)	(6.26)	(3.60)	(4.21)	(5.42)
Constant	9.41***	75.24***	0.51	0.15	18.46***	14.09***	11.54**	8.90***	8.58**	13.01***
	(1.40)	(25.08)	(1.31)	(0.26)	(6.23)	(4.31)	(4.50)	(2.64)	(3.62)	(3.74)
# of obs	11,168	1,396	1,396	1,396	1,396	1,396	1,396	1,396	1,396	1,396
R-squared	0.19	0.27	0.21	0.20	0.22	0.23	0.25	0.26	0.30	0.27

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 13. Robustness check with covariates, Los Angeles

	Overall	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
1.treatment	-4.60***	-36.84***	-3.43***	-0.63***	-8.27***	-3.75***	-4.78***	-2.64***	-7.26***	-6.07***
	(0.53)	(8.79)	(0.73)	(0.15)	(1.97)	(0.79)	(1.36)	(0.54)	(1.65)	(1.95)
1.after	-9.46***	-75.71***	-5.90***	-1.14***	-15.24***	-8.25***	-11.24***	-4.76***	-14.29***	-14.87***
	(0.45)	(7.65)	(0.64)	(0.13)	(1.77)	(0.69)	(1.18)	(0.46)	(1.46)	(1.60)
1.treatment	**2.53*** **	**20.27** **	**2.43*** **	**0.24**	**4.53** **	**2.44*** **	**1.67**	**1.73*** **	**4.55** **	**2.69**
#1.after	**(0.57)**	**(9.48)**	**(0.75)**	**(0.17)**	**(2.13)**	**(0.88)**	**(1.44)**	**(0.59)**	**(1.84)**	**(2.10)**
jobdensity	0.00***	0.03***	0.00***	0.00***	0.01***	0.00***	0.00***	0.00***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Dtdis	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
(100m)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
amenities	0.31***	2.48*	0.19	-0.02	0.49	0.33**	0.47**	0.24***	0.28	0.48
	(0.09)	(1.46)	(0.12)	(0.02)	(0.32)	(0.13)	(0.23)	(0.09)	(0.28)	(0.33)
per15to35	0.10***	0.83	0.07	0.00	0.23*	0.04	0.23***	0.04	0.10	0.10
	(0.03)	(0.54)	(0.05)	(0.01)	(0.12)	(0.05)	(0.08)	(0.03)	(0.10)	(0.11)
3.rwtype	-32.00***	-256.04**	-16.96**	-3.42**	-49.82**	-21.65*	-35.34**	-14.48**	-54.06**	-60.31**
(residential)	(8.06)	(123.65)	(8.54)	(1.52)	(22.34)	(12.34)	(17.29)	(7.31)	(27.23)	(28.95)
4.rwtype	-13.95	-111.61	-6.60	-1.28	-13.13	-9.13	-15.06	-5.84	-26.63	-33.94
(secondary)	(8.82)	(137.43)	(10.00)	(1.70)	(27.66)	(13.28)	(19.85)	(7.94)	(29.15)	(30.85)
5.rwtype	-33.73***	-269.88**	-17.94**	-3.57**	-52.66**	-22.18*	-38.37**	-15.28**	-56.87**	-63.00**
(tertiary)	(8.12)	(124.22)	(8.57)	(1.53)	(22.43)	(12.42)	(17.37)	(7.34)	(27.38)	(29.09)
Constant	35.08***	280.62**	18.36**	4.41***	53.23**	25.36**	35.78**	16.11**	60.87**	66.50**
	(8.22)	(126.34)	(8.79)	(1.55)	(22.98)	(12.58)	(17.70)	(7.48)	(27.71)	(29.51)
# of obs	16,944	2,118	2,118	2,118	2,118	2,118	2,118	2,118	2,118	2,118
R-squared	0.33	0.48	0.40	0.44	0.45	0.51	0.44	0.47	0.48	0.44

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Approach B: Fixed Effect Panel Regression Model

San Francisco

Table 14 shows the results of San Francisco Fixed Effect Panel Regression model. When we compare the trip volumes of slow street segments before and after slow street implementation controlling for street segment fixed effects and time fixed effects, we find 1.283 increase of trips on average, per segment per week. The largest effect was found for mid-day during weekend, with an average of 0.474 increase in trip volumes per segment after slow street implementation. We also found positive and statistically significant impact of slow street implementation on trip volumes for weekday mornings, weekday PM peaks, and weekend nighttime. However, we saw slight decrease in trip volumes after treatment in weekend mornings. In the mornings of weekend, scooter traffic decreased 0.063 trips on average per segment after slow street implementation.

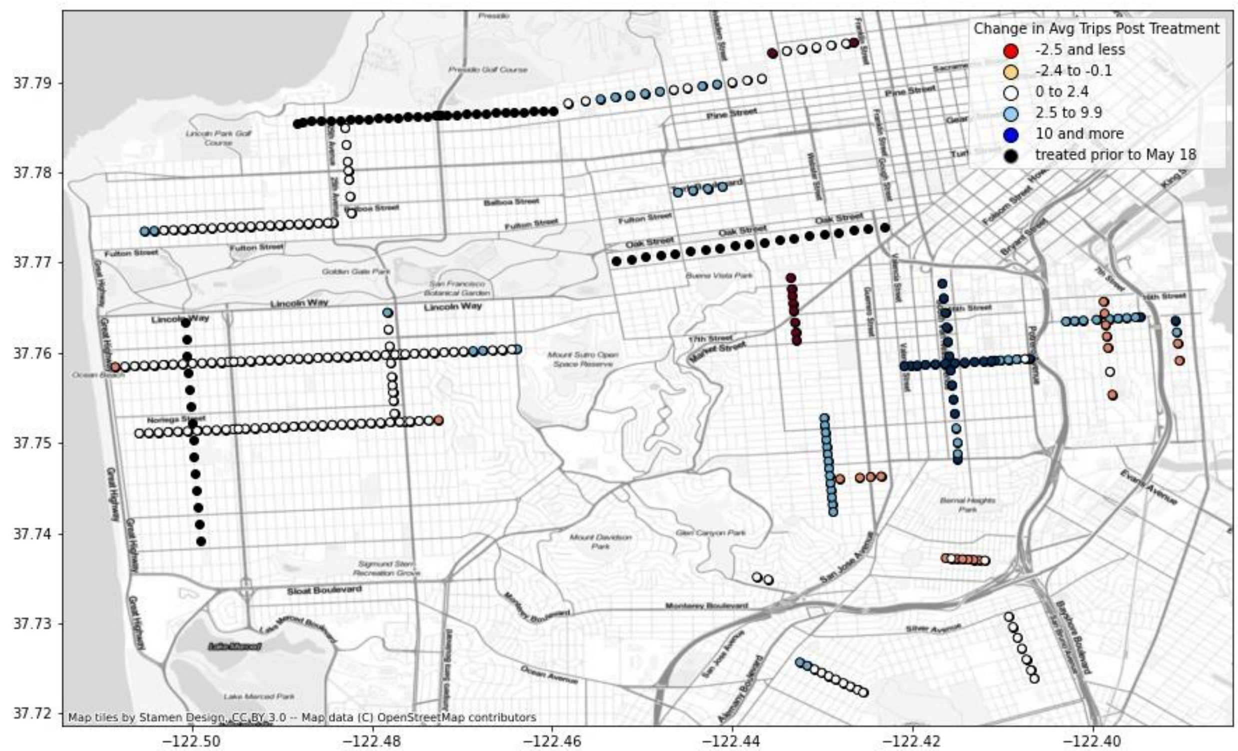

Figure 6. Change in Average Weekly Trip Counts After Treatment, San Francisco

Portland

Table 15 shows the results of the Fixed Effects Panel Regression model for Portland. Similarly, we find an overall weekly increase of .982 trips on average per segment. In the disaggregated models, we find the largest increase during the weekend night time period of about .462 trips per week per segment. We also find increases of 0.268, 0.162, and 0.126 on weekday middays, weekday PM peak, and weekend PM peak, respectively. We do find a significant decrease in trips along Slow Streets relative to control streets on both weekday middays of 0.163 trips and weekend AM peak of 0.0203 trips.

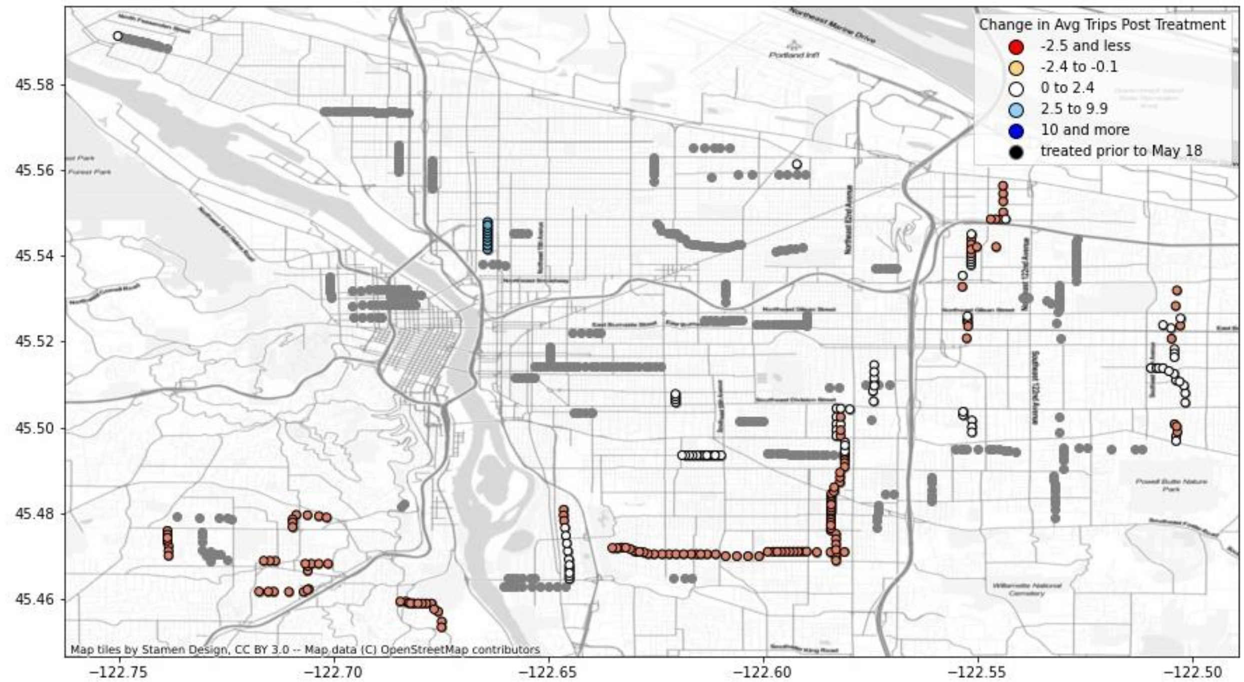

Figure 7. Change in Average Weekly Trip Counts After Treatment, Portland

Table 14. Fixed Effect Panel Regression Model Results, San Francisco

	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
treatment	1.283**	0.0613*	-0.0631***	0.0429	0.474***	0.227*	0.103	0.170	0.267*
	(0.618)	(0.0327)	(0.0203)	(0.141)	(0.0938)	(0.130)	(0.103)	(0.111)	(0.149)
Constant	8.555***	0.350***	0.193***	1.642***	1.389***	1.193***	0.891***	1.404***	1.494***
	(0.582)	(0.0415)	(0.0352)	(0.151)	(0.115)	(0.133)	(0.0965)	(0.179)	(0.136)
Observations	7,011	7,011	7,011	7,011	7,011	7,011	7,011	7,011	7,011
R-squared	0.116	0.051	0.031	0.056	0.100	0.047	0.073	0.055	0.093
# of orig_fid	310	310	310	310	310	310	310	310	310

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 15. Fixed Effect Panel Regression Model Results, Portland

	TOTAL	AM weekday	AM weekend	MD weekday	MD weekend	PM weekday	PM weekend	NT weekday	NT weekend
treatment	0.932***	-0.002	-0.0203***	0.268***	-0.163***	0.162**	0.126***	0.101	0.462***
	(0.165)	(0.0144)	(0.00762)	(0.0676)	(0.0464)	(0.0700)	(0.0309)	(0.0622)	(0.0718)
Constant	0.00426	-0	-0	0	-0	0	0	-0	0.00426
	(0.0654)	(0.00199)	(0.000711)	(0.0130)	(0.00714)	(0.0115)	(0.00389)	(0.0165)	(0.0160)
Observations	7,050	7,050	7,050	7,050	7,050	7,050	7,050	7,050	7,050
R-squared	0.114	0.030	0.143	0.055	0.049	0.038	0.058	0.070	0.078
# of orig_fid	235	235	235	235	235	235	235	235	235

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Discussion

The implementation of slow streets in Oakland, San Francisco, Los Angeles, and Portland provides a unique natural experiment opportunity that is rarely possible, to observe how travel changes when a large infrastructure support is provided unexpectedly and rapidly. We find fairly consistent evidence across two empirical approaches – difference in differences and fixed effects panel regressions – in a total of four cities.

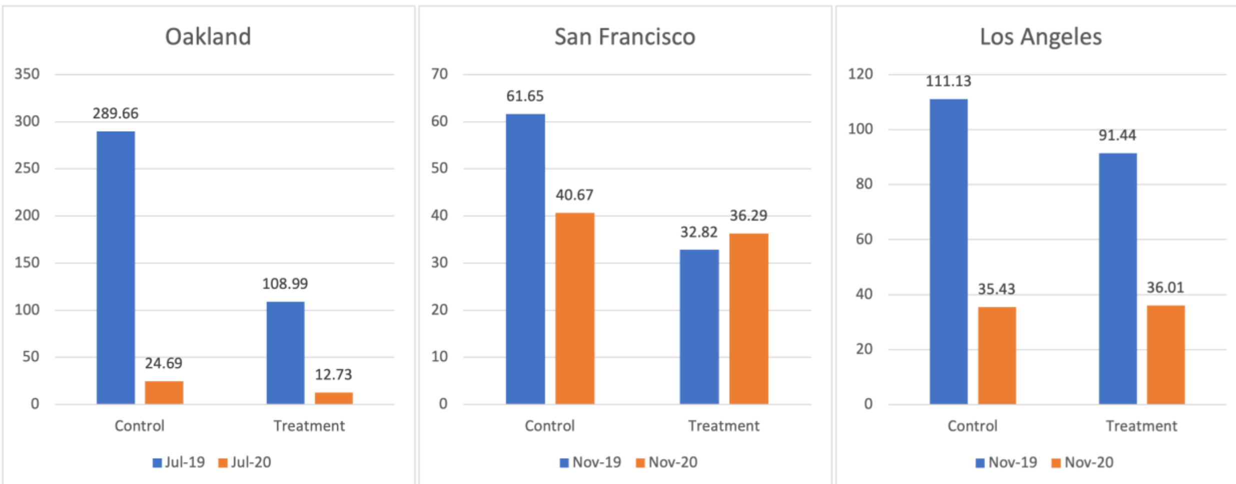

Figure 8. E-scooter Screenline Counts Before and After Slow Street Implementation

As Figure 8 shows, mean screenline counts for both treatment and control groups decreased in 2020 in Oakland and Los Angeles. However, the decrease was larger for the control group in both cities. Part of that decrease, particularly in Oakland, is due to the early (July 2020) measurement which is during a time when travel was reduced across many modes in the early stages of COVID-19 restrictions. We note that the larger decline in the control group in Oakland and Los Angeles might suggest differences across those two cities – although that difference could simply be the effectiveness of the slow street program in reducing the magnitude of dockless trip reductions. In case of San Francisco, screenline trip counts on treatment segments indeed increased in 2020 relatively to 2019.

Even though trip counts dropped for slow streets and control groups in many (but not quite all) cases (Figure 8), the evidence from the regression analysis indicates a positive impact of Slow Streets on dockless travel relative to the control groups. Recall that the logic of both the DID and the panel analysis is to measure changes in trip counts on slow streets relative to the changes in the control group. The positive treatment effects that we found indicate that trip counts either increased on slow streets or decreased less that did the control group, but either outcome is an increase in trip counts on slow streets relative to the control group. The magnitude of the impact ranges from 22.16% to 74.5% percent according to the DID regression model (Table 16), using the 2019 mean trip counts for the treatment group as a baseline.

Table 16. DID Analysis Summary

	DID treatment effect	2019 mean trip counts (treatment group)	% Change 2019-2020
Oakland (TOTAL, July)	168.7	108.99	+54.78%
San Francisco (TOTAL, Nov)	24.45	32.82	+74.50%
Los Angeles (TOTAL, Nov)	20.27	91.44	+22.16%

Table 17. Fixed Effect Panel Analysis Summary

	Panel treatment effect	Pre-treatment mean trip counts	% Change	2019 mean trip counts (treatment group)	% Change
San Francisco	1.283	5.0337	+25.49%	7.658	+16.75%
Portland	0.932	0.2692	+346.21%	8.652	+10.77%

Note: 2019 mean trip counts are adjusted to weekly basis (monthly mean*7/30), 2019 mean trip counts calculated using all the treatment segments in San Francisco and Portland

Table 17 shows effect sizes for the panel analysis treatment effects. The estimated treatment effect is shown relative to the pre-treatment mean trip counts (from 2020 data, before slow streets were implemented) and from July 2019 data for the treatment group. We prefer using the July 2019 mean trip counts (converted to a weekly basis) as the baseline, because that gives the treatment effect relative to the generally larger pre-Covid trip counts. We also note that comparing the treatment effect to a pre-Covid baseline is more conservative. The DID treatment effects in Table 16 are the coefficient on total counts (the specification TOTAL) in Table 5 and Table 6 for Oakland and San Francisco respectively and the same coefficient from the robustness test for Los Angeles (Table 13). Table 17 shows the treatment effects from the panel models for San Francisco and Portland, from the TOTAL specification in Table 14 and Table 15 respectively. In the far right of Table 17 we show that the panel estimates give treatment effects which range from 10.77% to 16.75% of a 2019 weekly baseline. The estimated treatment effect sizes, relative to a 2019 baseline, are also shown in Figure 9.

Figure 9. Model Results Summary

These results are consistent with the larger literature that supportive infrastructure results in increases in non-motorized travel. The implementation of Slow Streets occurred quickly and unexpectedly, and our use of control groups (either non-implemented candidate streets or late implemented slow street segments) provides an opportunity for stronger causal inference than is often available. On net, the evidence indicates that Slow Street implementation results in an increase in dockless trips.

However, the analysis has some limitations. The nature of screen line count data does not allow an examination of whether the observed increase in scooter traffic on slow streets compared to the candidate streets represents new trips or route detouring of trips that would have gone elsewhere. Thus, the analysis does not help to understand if people are replacing other travel modes, specifically cars, with e-scooter or if people are choosing slow streets over other streets. However, either way, the findings provide valuable insights to understand the importance of supportive infrastructure on e-scooter usage, and more generally on active transport.

Moreover, for both DID and panel analysis, we did not explicitly consider the time lag between treatment and behavioral change. For the DID model which used monthly aggregated trip count data, because the analysis is based on July (Oakland) and November (San Francisco and Los Angeles), which is after most slow street implementation, any lagged effect might have occurred by the time of the “after” measurements. However, for the panel model, even though we included time fixed effects, the model does not measure time lags if activity patterns take longer to reflect the changes in built environment than the span of the data. Future research should consider the effect of such time lags.

Furthermore, e-scooters might not be effective proxies for other non-car travel modes. As the goal of all four cities states, slow streets programs are expected to provide a more supportive, safer environment for people to enjoy street spaces including pedestrians, bicyclists, and all the other travel modes. We are not aware of literature correlating bicycle or pedestrian activities with e-scooter activities, so e-scooter trip counts cannot be a definitive indicator of all non-car modes. However, e-scooter data was the only available data source for empirical analysis, and the findings still highlight the importance of supportive infrastructure. If data allows, future research should examine the impact of such supportive infrastructure on pedestrian and bicyclist activities.

We believe that this evidence illuminates more than dockless travel. We used dockless trips counts as our dependent variable because Lime was able to provide data on dockless travel extending back in time to a “before implementation” time period. Thinking about dockless scooter trips as an indicator of non-motorized travel generally, we infer that Slow Streets likely will increase walking and bicycling. The evidence here supports the hypothesis that infrastructure which supports non-motorized travel can (and likely will) lead to increases in non-motorized travel.

Looking forward, we note that the control segments used in the DID analysis in some of the cities (particularly Oakland) had higher trip counts than the experimental in the “before” period, raising questions about the comparability of the treatment and control groups. Yet we note that our robustness tests, including covariates in the DID regression and the panel regressions, give results consistent with the uncontrolled DID analysis. While more study in other cities would be a welcomed addition to the literature, the evidence here supports the importance of Slow Streets in increasing dockless scooter trips.

References

Bai, Shunhua and Junfeng Jiao. (2020). Dockless E-scooter usage patterns and urban built Environments: A comparison study of Austin, TX, and Minneapolis, MN. Travel Behavior and Society, 20: 264-272.

Buck, D., & Buehler, R. (2012). Bike Lanes and Other Determinants of Capital Bikeshare Trips Transportation Research Board 91st Annual Meeting

Buehler, R., & Pucher, J. (2021). COVID-19 Impacts on Cycling, 2019–2020. Transport Reviews, 41(4), 393-400. https://doi.org/10.1080/01441647.2021.1914900

Combs, T. S., & Pardo, C. F. (2021). Shifting streets COVID-19 mobility data: Findings from a global dataset and a research agenda for transport planning and policy. Transportation Research Interdisciplinary Perspectives, 9. https://doi.org/10.1016/j.trip.2021.100322

Kim, J. (2022). COVID-19's impact on local planning and urban design practice: focusing on tactical urbanism and the public realm with respect to low income communities, Journal of Urbanism: International Research on Placemaking and Urban Sustainability, DOI: 10.1080/17549175.2022.2146155

Kraus, S., & Koch, N. (2021). Provisional COVID-19 infrastructure induces large, rapid increases in cycling. Proc Natl Acad Sci U S A, 118(15). https://doi.org/10.1073/pnas.2024399118

Landgrave-Serrano, M. & Stoker, P. (2022). Increasing physical activity and active transportation in an arid city: Slow Streets and the COVID-19 pandemic, Journal of Urban Design, DOI: 10.1080/13574809.2022.2112512

Lin, B., Chan, T. C. Y., & Saxe, S. (2021). The Impact of COVID-19 Cycling Infrastructure on Low-Stress Cycling Accessibility: A Case Study in the City of Toronto. Findings. https://doi.org/10.32866/001c.19069

Macfarlane, G., Turley Voulgaris, C., & Tapia, T. (2022). City parks and slow streets: A utility-based access and equity analysis. Journal of Transport and Land Use, 15(1), 587–612. https://doi.org/10.5198/jtlu.2022.2009

Marcus, M. L., Flores, F. P., Roberts, M. J., & Johnson, A. (2021). Why attention to complete streets implementation is so important in serving historically disadvantaged communities. Institute of Transportation Engineers. ITE Journal, 91(5), 38-43.

Marshall, W. E., & Ferenchak, N. N. (2019). Why cities with high bicycling rates are safer for all road users. Journal of Transport & Health, 13. https://doi.org/10.1016/j.jth.2019.03.004

Monsere, C., Dill, J., McNeil, N., Clifton, K., Foster, N., Goddard, T., Berkow, M., Gilpin, J., Voros, K., van Hengel, D., & Parks, J. (2014). Lessons from the green lanes: Evaluating protected bike lanes in the U.S. Transportation Research and Education Center (TREC), NITC-RR-583. https://doi.org/10.15760/trec.115

Noland, R., Iacobucci, E. & Zhang, W. (2022) Public Views on the Reallocation of Street Space Due to COVID-19, Journal of the American Planning Association, DOI: 10.1080/01944363.2022.2058595

Patterson, R., Ogilvie, D., & Panter, J. (2021). Travel Levels Before and After COVID-19 Control Measures in Cambridge, UK. Findings. https://doi.org/10.32866/001c.24854

Pucher, J., Dill, J., & Handy, S. (2010). Infrastructure, programs, and policies to increase bicycling: an international review. Prev Med, 50 Suppl 1, S106-125. https://doi.org/10.1016/j.ypmed.2009.07.028

Teixeira, J. F., & Lopes, M. (2020). The link between bike sharing and subway use during the COVID-19 pandemic: The case-study of New York's Citi Bike. Transp Res Interdiscip Perspect, 6, 100166. https://doi.org/10.1016/j.trip.2020.100166

Thigpen, C. (2020). Rethinking Travel in the era of COVID-19: Survey Findings and Implications for Urban Transportation.

Data Summary

Products of Research

Data for the location and implementation dates of slow streets were provided by LADOT (Los Angeles), SFMTA (San Francisco), Oakland DOT (Oakland), and PBOT (Portland). Los Angeles neighborhood definitions were provided by LADOT. ACS 2018 5-Year Estimates data for resident ages as well as a block group GIS layer containing land area data were downloaded as an NHGIS extract. LODES 2018 WAC data was used to represent the number of jobs in each block group. Downtowns were considered to be the block group (NHGIS) with the highest number of jobs (LODES) divided by land area (NHGIS). Amenities data was pulled from OpenStreetMap using the OSMnx module in Python on October 5th, 2021. Roadway type classifications were pulled from OpenStreetMap using OSMnx as well. Finally, trip count data for each segment-time were provided by Lime.

Data Format and Content

The research team released a file with the data used for the regression analysis which is available from the repository. Variable names are listed in the top row, as well as more detailed definitions in variable definition worksheets. Since the data was provided by Lime via an agreement which prohibits sharing of sensitive data, we provide a description of the scooter trip count variables but do not include them in the public dataset.

Data Access and Sharing

Upon a final review and confirmation, the research team uploaded the public dataset to the Dataverse depository. The data can be accessed at https://doi.org/10.7910/DVN/GBO9YC.

Reuse and Redistribution

Data that is restricted will not be released. The following citation is suggested for reuse of the data:

> Boarnet, Marlon; Lee, Seula; Gross, James; Thigpen, Calvin, 2023, "Replication Data for: Slow Streets and Dockless Travel: Using a Natural Experiment for Insight into the Role of Supportive Infrastructure on Non- Motorized Travel", https://doi.org/10.7910/DVN/GBO9YC, Harvard Dataverse, V1

Appendix A1: Mean trip counts of control and treatment group, before and after slow street implementation by city and times-of-day

Oakland	2019 Control	2020 Control	2019 Treatment	2020 Treatment
AM weekday	41.01	1.11	12.39	0.41
AM weekend	1.46	0.25	0.53	0.13
MD weekday	63.43	4.55	23.42	2.37
MD weekend	15.57	2.39	6.63	1.43
PM weekday	74.19	4.59	25.93	2.64
PM weekend	10.05	2.46	4.58	1.25
NT weekday	61.65	4.54	26.82	2.3
NT weekend	22.29	4.8	8.69	2.19
TOTAL	289.66	24.69	108.99	12.73
# of observation	475	475	163	163
San Francisco	**2019 Control**	**2020 Control**	**2019 Treatment**	**2020 Treatment**
AM weekday	3.37	1.23	2.11	1.22
AM weekend	0.55	0.34	0.29	0.27
MD weekday	13.44	7.89	5.35	8.8
MD weekend	9.77	5.39	5.27	5.38
PM weekday	10.34	7.4	4.95	5.22
PM weekend	5.25	4.43	3.75	3.6
NT weekday	9.95	6.5	5.8	5.54
NT weekend	8.98	7.48	5.31	6.25
TOTAL	61.65	40.67	32.82	36.29
# of observation	413	413	285	285
Los Angeles	**2019 Control**	**2020 Control**	**2019 Treatment**	**2020 Treatment**
AM weekday	7.81	1.91	5.51	2.03
AM weekend	1.84	0.70	1.49	0.58
MD weekday	24.03	8.79	19.30	8.58
MD weekend	11.60	3.35	9.58	3.77
PM weekday	16.40	5.16	14.08	4.50
PM weekend	7.12	2.35	5.59	2.55
NT weekday	20.93	6.64	17.01	7.27
NT weekend	21.40	6.53	18.89	6.71
TOTAL	111.13	35.43	91.44	36.01
# of observation	512	512	547	547

Appendix A2: Number of segments received, in DID, and in panel analysis

		Received	DiD	Panel
San Francisco	Treatment	312	285	310
	Control	447	413	-
Oakland	Treatment	286	163	-
	Control	850	475	-
Los Angeles	Treatment	547	547	-
	Control	512	512	-
Portland	Treatment	731	-	636
	Control	-	-	-