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The Causal Effect of Cycling Infrastructure on Traffic and Accidents: Evidence from Pop-up Bike Lanes in Berlin*

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ABSTRACT

This paper analyzes the effect of new bicycle lanes on traffic volume, congestion, and accidents. Crucially, the new bike lanes replace existing car lanes thereby reducing available space for motorized traffic. In order to obtain causal estimates, I exploit the quasi-random timing and location of the newly built cycle lanes. Using an event study design, a two-way fixed effects model and the synthetic control group method on geo-coded data, I show that the construction of pop-up bike lanes significantly reduced average car speed by 8 to 12 percentage points (p.p.) and up to 16 p.p. in peak traffic hours. In contrast, the results for car volume are modest, while the data does not allow for a conclusive judgment of accidents.

Keywords: congestion, urban, traffic, environment, accidents, cycling, health, COVID-19
JEL Codes: O18, Q56, R11, R41, R42

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1 Introduction

In the recent past, policy makers have aimed to reduce the negative external effects of traffic by building new cycling roads. All over the world, city governments have implemented such measures to improve the cycling infrastructure. Among the most prominent examples are Bogotá, New York, Vancouver, Mexico city, London, Paris, and Brussels. In Berlin, the bicycle traffic plan[1] proclaims that new cycling infrastructure offers benefits by reducing local air pollution, increasing the safety on the streets and using public space more efficiently by incentivizing car users to divert to cycling. Despite gaining ground in public perception, the role of cycling in cities including the change in infrastructure towards more bike-friendly urban environments and its effects on urban life is still on the fringes of academic research.

This paper investigates the effects of new bike lanes on traffic volume, congestion and accidents. Specifically, I look at new cycle infrastructure which is the result of converting street space for cars to new lanes for bicycles. The analysis uses pop-up bike lanes (PUBLs) in Berlin, which were installed after the beginning of the COVID-19 induced lock-down between March and June of 2020. My findings suggest a decrease in average car speed by between 8 and 12 percentage points, which means that congestion on these streets increased. In the main business hours of traffic the size of the effect even amounts to about 16 percentage points. Moreover, the results point towards modest changes in car volume. However, these can not be clearly attributed to PUBL installations. I also test for substitution effects on streets close-by, since the measures might merely relocate traffic in order for drivers to avoid affected routes. In close distance, the new cycling lanes increased traffic without affecting congestion. This suggests a new equilibrium with a more equal distribution of cars on the inner city road network. For accidents, I do not find any significant changes caused by pop-up lanes, which might suggest an increase in per-cyclist safety due to the rise of cyclists in the city (Kraus and Koch, 2021).

In order to identify the causal effects and the potential heterogeneity in the development of the outcomes, I use an event study approach (Clarke and Schythe, 2020), and standard two-way fixed effects models. The accidents analyses are moreover extended by a synthetic control group design (Abadie et al., 2010), which uses observable characteristics of treated and potential control units in order to designate a comparison group.

Congestion, pollution and accidents are increasing functions of traffic and among the most severe diseconomies of cities (Ahlfeldt and Pietrostefani, 2019; Borck and Schrauth, 2021; Shefer and Rietveld, 1997). Congestion is costly in various ways, especially in the form of time losses and a rise in fuel consumption (Vickrey, 1969; Treiber et al., 2008).

[1]https://www.berlin.de/sen/uvk/_assets/verkehr/verkehrspolitik/radverkehrsplan/rvp.pdf.

In Germany, congestion caused an average time loss of about 40 hours in 2021, which was an increase of more than 50 percent compared to 2020. Berlin is among the most severely affected cities with 65 hours lost. Four out of the ten most congested German streets are situated in the capital city. As a consequence, the calculated costs amounted to about 600€ per driver or more than 800 million Euro for the city in 2021.[2] There is also evidence that congestion may hinder economic growth in terms of income and employment (Jin and Rafferty, 2017; Hymel, 2009). Additionally, congestion interacts with pollution with its adverse health effects e.g. for infants (Currie and Walker, 2011; Knittel et al., 2016). Accidents also cause substantial external costs (Edlin and Karaca-Mandic, 2006). Analyzing and understanding the factors, which may cause these disadvantages of cities and inefficiencies in the public infrastructure, as well as potential solutions to them, is thus pivotal.

In order to reduce congestion in cities, different measures have been implemented by policy makers in the past. A congestion charge introduced in London, which levied a toll during prime commuting hours, was found to increase traffic speed and reduce total miles driven. A consequence of lower traffic levels was improved air quality[3] and a decrease of the amount as well as the rate of accidents (Green et al., 2016, 2020). Apart from such policies, which directly aim to target congestion, there is robust evidence that public transportation and the extension of its network may reduce congestion and lead to significant social benefits, e.g. due to higher air quality and reduced travel times (Anderson, 2014; Bauernschuster et al., 2017). Another strand of research examines whether an increased road supply could impact traffic. The extension of road infrastructure was found to proportionately increase the amount traveled in the long run, thereby not affecting congestion (Duranton and Turner, 2011).

Among the few exceptions aiming to identify causal effects of cycling on traffic related outcomes, Hamilton and Wichman (2018) found that neighborhoods with bike-sharing stations had significantly lower congestion levels compared to similar, but untreated neighborhoods in the Washington D.C. area, which hints towards a supply-driven change in commuting behavior towards more cycling if more bikes are provided. There is also research that shows that a better and safer cycling infrastructure correlates with an increase in the propensity of bicycle utilization (Buehler and Pucher, 2012; Goodman et al., 2013). Furthermore, whether cycling routes, or "cycle superhighways" in this specific case, are safer or not was found to depend on physical characteristics, e.g. whether cyclists were separated from other forms of travel (Li et al., 2017). The same type of

[2]https://inrix.com/press-releases/2021-traffic-scorecard-de/. Economic costs are calculated based on values of time as suggested in the same study.

[3]except for NO_2, because Diesel cars, which are a main contributor to this pollutant, were exempt from the charge.

cycle lanes was found to reduce traffic volume without affecting average traffic speed (Bhuyan et al., 2020).

This paper contributes to the existing literature in various ways. It is, to the best of my knowledge, the first paper to analyze a reduction of road space available for cars in order to make it available for bikes. Thereby, it connects to the much discussed "fundamental law of road congestion" (Duranton and Turner, 2011), which suggests a response of congestion as a consequence of building new infrastructure with an elasticity of one. To the best of my knowledge, there is no evidence on short to midterm effects though, and the question of what happens to traffic-related outcomes after a reduction of lane kilometers. Generally, the elasticity of traveling behavior with respect to infrastructure should hold for an extension and a reduction of lane kilometers, thus overall congestion should not be affected by a lane reduction in the long run. However, if there are sticky preferences for car utilization, we might see different outcomes. Besides, the substitution of car lanes with bike lanes provides a direct alternative in traveling mode on the respective street. By considering the effects of PUBLs on accidents, I contribute to the strand of literature, which analyses road safety (Edlin and Karaca-Mandic, 2006; Green et al., 2016; Shefer and Rietveld, 1997), and specifically in how far enhanced cycling infrastructure affects the safety of different types of road users (Li et al., 2017). Furthermore, it is among the very few papers to look at causal effects of bike infrastructure on traffic-related outcomes and to the best of my knowledge the first one to use a quasi-experimental design. In general, it is difficult to identify the causal effects of new bike lanes on city-related outcomes due to the fact that their creation[4] often is meant to be a response to outcomes like road safety or congestion. Thus, city authorities for example want to reduce street accidents by creating safer cycling infrastructure. This paper addresses this type of reverse causality by looking at (quasi) randomly built bike lanes during COVID-19 lock-down times. The roll-out of such lanes is normally very slow. This is why pop-up lanes are a chance to circumvent empirical problems like anticipation effects, because of their very fast and sudden construction. The results of the paper may serve as a valuable contribution to structural models, which consider infrastructure within cities. For policy makers, the results may hint towards potential problems accompanied with the sudden rezoning of car lanes. This allows them to address and tackle relevant problems prior to taking measures. The paper also helps to contribute to the question of costs and benefits of making a city more friendly to bikes in terms of new infrastructure.

In the remainder of the paper I first give an overview about the background and theoretical considerations regarding the installation of pop-up bike lanes in Section 2

[4]Throughout the paper, I will interchangeably refer to the installment of PUBLs on a street with the terms "treatment", "event", and "intervention".

before describing the data and providing some descriptive analysis in Section 3. In Section 4, I describe the methodological approach, and in Section 5 I present the results. Section 7 concludes.

2 Background and Theoretical Considerations

Political premises In Berlin, the coalition government of the three parties SPD, Die Grüne, and Die Linke (social-democrats, the green party and the left party) has set the goal of transforming city life by making it more friendly especially to pedestrians, cyclists, and people using public transport. The goals are written out in the mobility law, which was passed in July of 2018.[5] The law predominately contains plans regarding the city's traffic infrastructure and how these are going to be implemented. This includes aspects of organization and funding. For cycle lanes, the aim was to extend the existing infrastructure in order to make cycling more attractive and to increase the share of cycling in the modal split. More specifically, a major aim was to increase the safety of bicycle users and to reduce, and possibly avoid, cycling accidents. The plan also includes the construction of cycling highways, which predominantly are supposed to connect outer parts of the city with the city center. More goals of the plan include to save space since bikes require less street capacity compared to cars, to reduce local pollution, and to enhance healthiness by incentivizing an increase in physical activity.[6] Due to the fact that the steps of construction are planned to be implemented until the year 2030, most of the structural measures had not been realized by the end of the time frame considered in this paper.

Pop-up bike lanes during the COVID-19 lock-down in Berlin In March of 2020, political measures in form of a lock-down were taken in Germany as a consequence of the COVID-19 pandemic. The lock-down included the closure of all businesses[7], of schools and day care centers for young children, and of the gastronomy among other measures. Figure 1 shows the development of car volume and average speed in Berlin from 2019 to 2020 with its apparent negative correlation between total cars and speed. Just after the lock-down started on March 17th 2020, there was a sudden decline of overall traffic and congestion as suggested by an increase in average speed. This circumstance was taken advantage of by public authorities in Berlin and foremost the local authorities of

[5]MobG BE - Abschnitt 3: Entwicklung des Radverkehrs (development of bicycle traffic) https://gesetze.berlin.de/perma?d=jlr-MobGBEpG6 is the specific chapter in the law about bicycle traffic.

[6]Compare also https://www.berlin.de/sen/uvk/_assets/verkehr/verkehrspolitik/radverkehrsplan/rvp.pdf.

[7]Except for those necessary for daily life, like grocery stores.

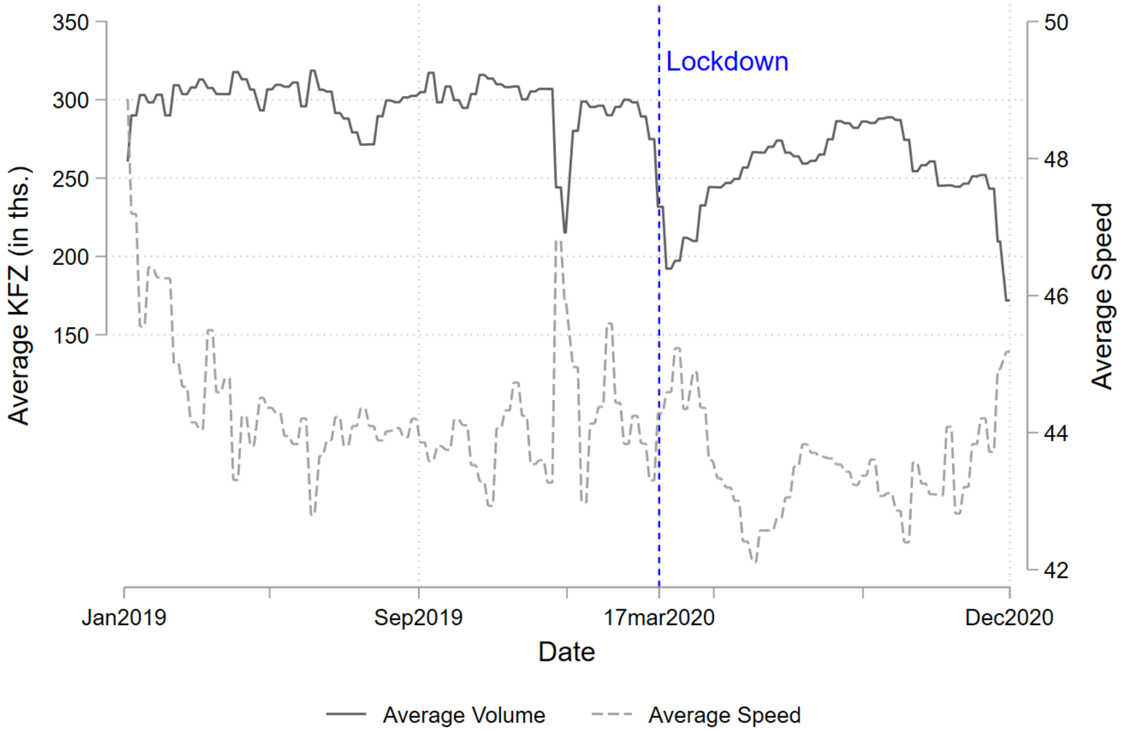


Figure 1: Traffic in Berlin 2019 - 2020

Notes: The graph shows the development of average vehicle volume and average vehicle speed in Berlin from the beginning until the end of the sampling period. The beginning of the COVID-19 induced lockdown on March 17th, 2020 is depicted by the vertical blue, dashed line.

Friedrichshain-Kreuzberg (FHKX)[8], who began setting up pop-up bike lanes (PUBLs) in their local district.[9] PUBLs are, contrarily to regular bicycle lanes, created spontaneously, circumventing the otherwise relatively long-lasting decision process of where and how to build a bicycle lane. While the implementation of regular bike lanes takes two to ten years, PUBLs are implemented in three to 10 days.[10] The reduced time necessary for the implementation is also due to the very simple and cheap type of construction of the lanes, which only consist of paint and temporary bollards. In the Berlin district of FHKX, which is locally governed solely by the Green party, there additionally was the political will of advancing this type of infrastructure renewal. This, according to statements by the authorities, has lead to the actual implementation in this specific district. Two days after the lock-down came into force, the first PUBL was installed on March

[8]Four out of eleven PUBLs were not set up in FHKX, but in the districts Charlottenburg-Wilmersdorf, Neukölln, Pankow and Treptow-Köpenick.

[9]The following information about the setup of pop-up bike lanes were gathered through interviews with the authorities of the Berlin Senate, conducted mainly in May 2021.

[10]Compare press release: https://www.berlin.de/ba-friedrichshain-kreuzberg/...

25th in Kreuzberg.[11] Others followed subsequently in different parts of FHKX and until July 7th there had been eight more dates with new lanes being created or lanes being extended. Overall, there is a total of 13 affected streets where PUBLs were installed or later extended on nine different dates between March 25th and July 7th.[12] Almost all streets had bike lanes set up into both directions and mostly of similar length.[13] Figure 2 shows a time-line of the exact dates of installment as well as the respective streets. By mid 2021, most of the bike lanes had been made permanent by replacing the movable bollards with fixed demarcations. Until the end of the observation period, there has been no further change in treatment status.

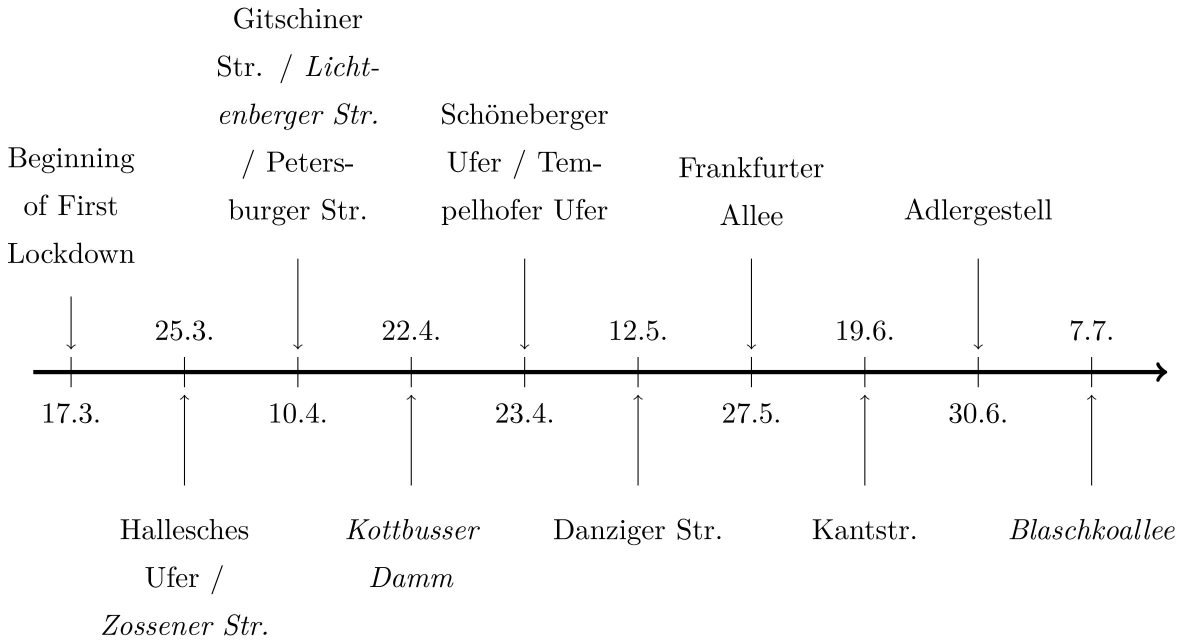


Figure 2: **Time line of PUBL installments**

Notes: The figure shows all installations of pop up lanes after the beginning of the COVID-19 induced lockdown. All street names in italic font have no observations in the traffic data set and are therefore not considered in the respective analyses of the effect on volume and speed.

Criteria of installation Following Kraus and Koch (2021) and personal interviews with local authorities, the placement and timing of pop-up bike lanes in Berlin was, conditional on certain characteristics regarding the affected streets, as good as random. The

[11]The segment on Hallesches Ufer into one direction.

[12]Three of the roads cannot be taken into account in parts of the analysis, as they happened on roads, which do not have any traffic volume/speed measuring station nearby. One of those roads is "Blaschkoallee" in Neukölln. This was the last PUBL to be established. Thus, the last date with a change in treatment status in my sample is June 30th, when the street "Adlergestell" in Treptow-Köpenick received a pop-up lane.

[13]Compare Mobycon (2020) for more information about the implementation process in Berlin.

decision on where to locate such a lane was primarily driven by the amount of available street space. Only streets with at least two car lanes were taken into consideration, such that car traffic was not blocked completely on those roads. This characteristic of a minimum lane number was the only real requirement for installing a PUBL.[14] Effectively, treated streets are a subset of roads, which are supposed to be equipped with cycling infrastructure in the future as planned in the aforementioned mobility law (compare the *Political premises* paragraph). In order to account for that restriction regarding randomness of placement, the main estimation samples will only contain streets with two or more lanes. Only in some robustness checks this sample requirement will be altered. Station fixed effects additionally control for the number of lanes implicitly. The timing of construction was influenced by factors like the availability of construction firms. Due to the fact that those were not instantaneously available for setting up all of the bollards at once, it took some time until all pop-up lanes had been placed. The quasi-experimental setup with random timing and placement addresses standard drawbacks when measuring the effects of such policy measures like reverse causality (e.g. when bike lanes are a reaction to lower demand for cars and increased demand for cycling) or omitted variable bias, which are hard to measure (e.g. local preferences for more cycling lanes in the city). In the methodology section 4, I will go into more detail and address potential threats regarding the identification strategy.

Theoretical considerations Generally, the COVID-19 era led to a change in habits due to people diverting from different forms of public transportation, like buses or metropolitan railways, to private means of transport, like cars or bicycles (Tirachini and Cats, 2020). Some European cities experienced declines in public transport ridership of over 90% following lock-downs (Vitrano, 2021). Such disruptive shocks to public transport have been found to increase car utilization and thereby congestion (Bauernschuster et al., 2017; Anderson, 2014). However, since concerns about getting infected with COVID-19 should be evenly distributed among the population within the city, these effects should be felt on all streets throughout a city simultaneously. Had PUBL streets not experienced a structural change, then the lock-down effects should have been the same compared to similar untreated streets. The question remains which explicit effects are then to be expected by the new cycling infrastructure.

The replacement of car lanes with cycle lanes means a loss of space for cars and a gain of space for bikes. Following Duranton and Turner (2011), car traffic increases with the extent of the availability of streets. This means that congestion is unit-elastic with respect to street space. The authors identified the creation of traffic as a main

[14]One characteristic of treated streets, which is observed in the data, is that most of them had no prior bicycle infrastructure. However, this is not true for all PUBL streets. This specific type of sample selection will be tackled in a robustness check in the results section.

channel and the diversion from other streets as a less important one. In general, this elasticity should apply for both, the creation of new lane kilometers as well as when lane kilometers for cars are reduced. However, apart from the change of car space provision, an alternative way of commuting was created at the same time, which may affect the elasticity. Furthermore, Duranton and Turner (2011) consider long-term developments between cities and regions, while I look at a shorter time horizon within one city.

In terms of volume, there are two potential effects I would expect in the short run. On the one hand, PUBL streets experience a decline in car volume compared to non-PUBL streets. In order to avoid traffic jams and congestion, there is the incentive to use alternative roads in the surrounding area, which did not experience the installation of a PUBL. This would mean a reduction of volume on the respective street. On the other hand, there may be no will to divert from the accustomed old route, which would be in line with time-inconsistent preferences or a status quo bias (Mattauch et al., 2016). As a consequence, volume would not be affected. In the long run, there also may be behavioral adjustments, e.g. people adapting their preferred route over time and thereby reducing volume on treated streets further. Volume also directly affects average speed. Theoretically, if there is an increase in volume without street space being changed, then this should lead to a decrease in average speed, which means more congestion. The reduction of a car lane without a change of car volume should also lead to an increase in congestion as the same number of cars now shares a lower number of lanes. Another possibility is that people just change behavior in terms of commuting time. Thus, average speed might not change even though daily/weekly volume changes because people just travel at other times of the day.

Based on these considerations, I would expect automotive volume on PUBL streets to slightly decrease or remain unchanged compared to non-treated streets, while I assume average speed to decrease and thereby congestion to increase, especially if volume is only slightly affected.

Additionally, I want to find out in how far the restructuring of road space towards a more bike-friendly environment affects road safety by looking at the development of accidents. As mentioned before, this was one of the major reasons to create such bike lanes in the first place. If car and bike travel happen on separate lanes, which was not given prior to the policy change, collisions between these two modes of travel should decrease. This would mean an overall decrease in the incidence of accidents. On the contrary, the separation of lanes could also lead to more accidents if car drivers now pay less attention to cyclists and thus oversee them when taking a turn at a crossway. Furthermore, pop-up lanes may nudge new cyclists towards using the newly created ways. This could lead to an increase in accidents, especially between cyclists. If there are more cyclists on the respective roads, while the total number of accidents is not affected due to sufficient space for cyclists, this would mean a decline in the rate of cycling accidents. Overall, the direction of the effect on accidents cannot be predicted clearly.

3 Data and Descriptive Statistics

Traffic measuring stations The outcome measures for traffic volume and average speed stem from 772 measuring stations throughout the city of Berlin. The recordings show hourly values of speed and volume of vehicles passing by one station per hour for the years 2019-2020.[15] This allows to detect within day variances as well as short-term developments of traffic within Berlin. At the same time, it is possible to differentiate between personal cars and lorries. Due to the fact that all PUBLs in my sample were set up between end of March and end of June of 2020[16], I have a post-treatment period of about 40 weeks. The pre-treatment period contains about 70 weeks. Most measuring stations cover the traffic on multiple lanes of which I build the mean over all lanes for speed and for volume.[17] The information about the number of lanes is furthermore used for specifying the control group in most analyses. It allows to compare streets of the same pre-treatment capacity with each other. Figure 3 shows a map of Berlin, depicting the net of measuring stations and the PUBLs.

Of the eleven streets, which had a PUBL installment, some lack a traffic measuring station capturing volume and speed on the respective street. Thus, I am only able to analyze a subset of affected streets regarding this outcome, ignoring the effects on five out of eleven treated streets.[18] Since all of the non-considered streets have similar characteristics to the ones in the sample and there is no correlation of the placement of measuring stations and the creation of PUBLs, I do not consider this to be a major drawback. Overall, I observe 23 measuring stations located at treated streets with two or three lanes.

For the analyses, I exclude all stations, which are situated at highways[19]. The reason is that I assume that inner-city traffic might have developed differently from traffic on the highway, which circles the city. This is because highway traffic may not be substituted as easily by cycling or different forms of public transportation. Furthermore, I exclude Saturdays and Sundays as well as legal holidays from the analyses and the daily time

[15]The Senate of Berlin also provided me with (incomplete) data for May-July in 2021. However, due to the fact that many measuring stations drop out and due to gaps in the data between 2020 and 2021, I only consider the mid-term outcomes in a subsection.

[16]As described in Section 2, the last PUBL was installed on July 7th, 2020. However, for this last treated street, there exists no measuring station nearby, such that it cannot be taken into account.

[17]Missing values are ignored in the mean calculation.

[18]Most of these affected streets are dropped due to non-existing measuring stations. One street has missing observations in the post-treatment period (the station in Danziger Str.) and therefore is also deleted from the sample.

[19]In Berlin, these are named: A100, A111, A113, A114, A115.

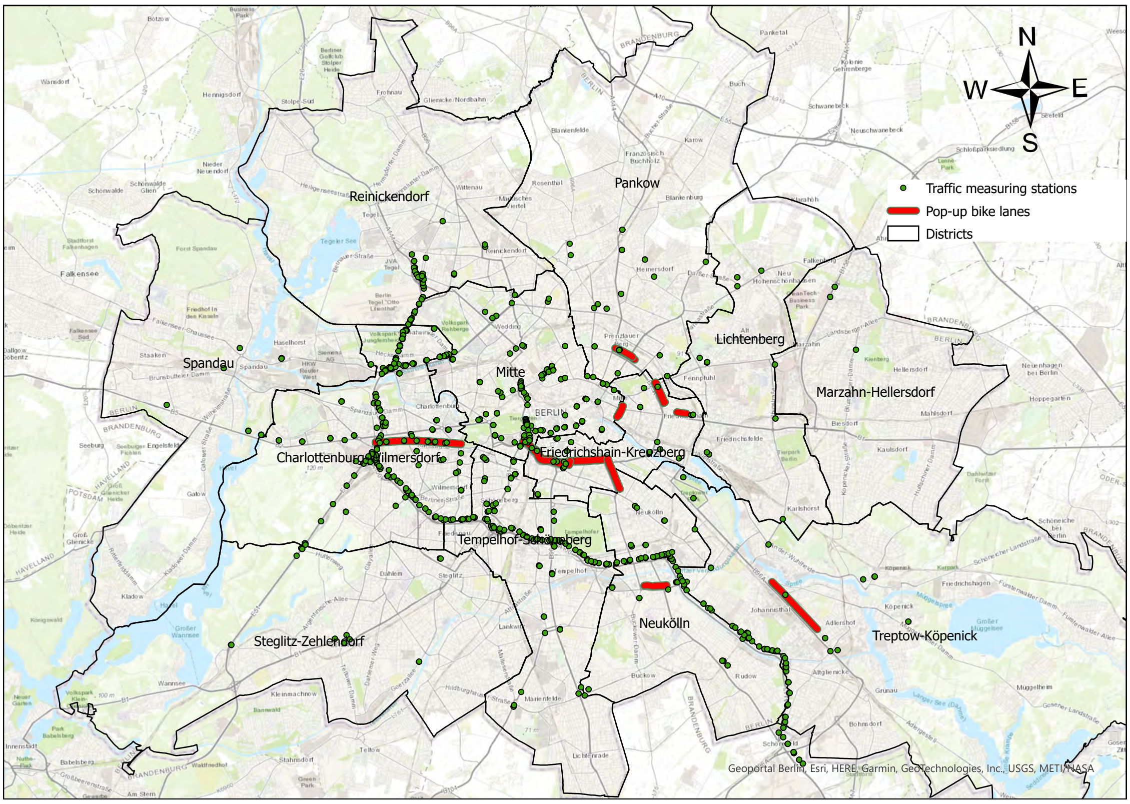


Figure 3: pop-up bike lanes and measuring stations in Berlin

Notes: The figure shows a map of Berlin. Pop-up bike lanes are marked as fat red lines and traffic measuring stations are depicted by green dots. The city is subdivided into 12 districts, which are outlined by fat black lines.

frame is limited from 5 a.m. to 8 p.m. since I want to concentrate on workday traffic. Some stations are lacking data e.g. for some hours a day or even for entire days.[20] As a consequence, in the event study analyses, which uses data aggregated to weekly values, I restrict the sample to stations, which have observations for at least five hours a day (which results in dropping about 0.04% of all observations) and then have at least 2 days of observations a week.[21] However, whenever I base the analysis on hourly data, which allows for hour and date fixed effects, I use the entire sample. Considering all these restrictions, the main sample remains with 192 measuring stations in the control group.

Figure 4 shows the natural logarithms of total weekly volume and average weekly speed by treatment status. The vertical blue lines mark the first and the last installation of a PUBL in the city. The figures already show to some extent the effect of the pop up

[20] Additionally, I had to delete partly erroneous data. Regarding traffic speed for example, there were observations with speed being over 1000. While this is a very extreme case, I cut out observations with speed of either trucks or cars being larger than 100, which is a speed level very unlikely to be reached within city boundaries. As a result, I deleted ≈ 0.0038 percent of the car speed sample and 0.00015 of the truck speed sample.

[21] All of the stations in the sample contain weeks, which only have 2 days of observations. About 1/3 of the stations have weeks with only 1 day of observations.

lanes and provide an impression regarding parallel trends between treated and untreated streets prior to the installment of pop-up lanes. As of volume, we can see that the overall development was very stable from 2019 to the beginning of 2020. The lock-down led to a sharp decline of vehicles on the streets of Berlin[22] and had returned to pre-pandemic heights by September of 2020 on untreated streets. Treated streets, however, did not return to pre-pandemic levels with respect to vehicle volume, but remained on a lower level.[23] The large spikes in the graph are the dates around new year's eve and to a lower extent during summer holidays, when traffic is significantly lower all over the city.

While between January of 2019 and March of 2020 overall speed has remained fairly constant with some minor variation on both treated and untreated streets, the lock-down led to two different developments. Treated streets experienced a decrease in speed whereas on untreated streets there initially was a small increase and then a return to normal levels. This is a first indication that treated streets experienced some sort of congestion despite the decline of vehicles traveling on them.

Overall, the figures show that spikes in traffic, either downward or upward, affected all streets across the city, even though in parts to different extents. Thus, changes in traffic in general seem to be caused by city-wide events affecting the overall traffic flow.

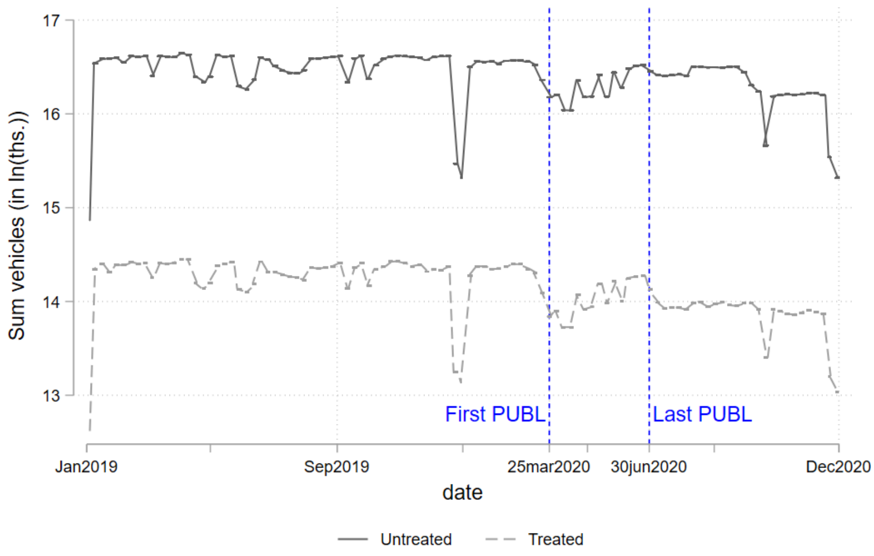
Accidents Data on accidents stems from the atlas of accidents ("Unfallatlas")[24], which locates every accident in Germany with exact point coordinates. For Berlin, this data exists for the years 2018 to 2020. It is available on a monthly basis and includes information such as the hour of the accident, day of week, whether people were killed or injured, and also which means of transport were involved (bike, pedestrian, car, etc.). It also includes lighting conditions (daylight, dawn, darkness) and road condition (wet, dry, slippery). Due to the fact that I only have monthly recordings, the post-treatment period starts the month after the implementation of the PUBL. In the accidents analyses, I use two different methodological approaches, which require two different levels of spatial aggregation. First, I run an event study design on street level within Berlin. Therefore, I match the accidents to Open Street Maps (OSM) information of Berlin such that every accident is assigned to - if possible - a single street of which I also know whether it contains a PUBL or not. The aim is to have a data set with single streets as units of observations, each of which contains the number of accidents, which occurred there each month.[25] Second,

[22]However, the decline is not as sharp as on new years eve.

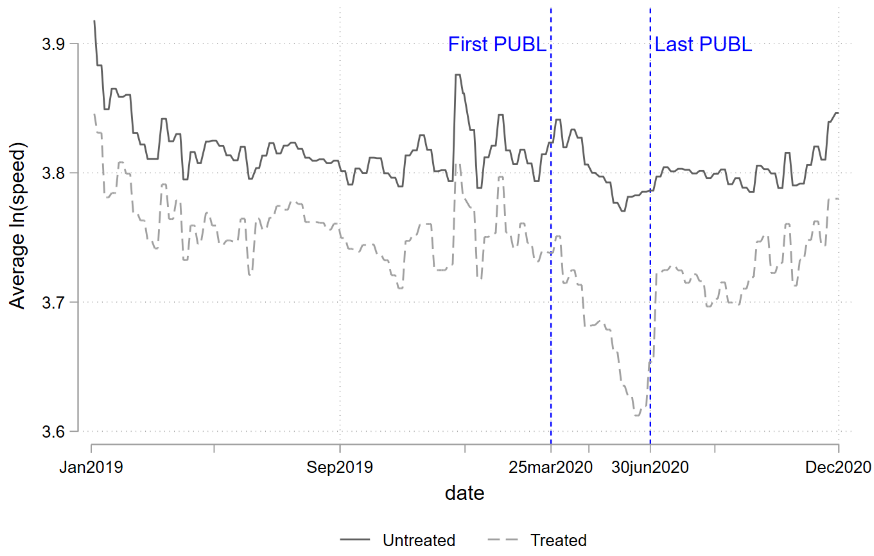
[23]The absolute difference in log values stems from the fact that there are much more untreated streets compared to treated ones.

[24]The Unfallatlas is freely downloadable at https://unfallatlas.statistikportal.de/ (last access: July 20201).

[25]In order to do so, I lay buffers of different magnitudes around the OSM-lanes and successively match those buffers to the accident data. This is necessary since the OSM data contains streets as lines and the accident data contains single coordinates, which hardly ever match exactly. Some accidents cannot be assigned unambiguously to one street, e.g. when the accident happened on a crossing. In these cases,



(a) Volume



(b) Speed

Figure 4: Development of speed and volume (pooled over cars and trucks) between Treatment and Control group

Notes: Figure 4a shows the development of vehicles (in logs) from January 2019 until December 2020 by treatment status. Figure 4b shows the development of average speed (in logs) by treatment status. In both cases, the gray dashed line represents the development of all treated streets, while the solid line shows the development of untreated streets. The two dashed blue vertical lines in each graph represent the installation of the first and the last pop-up bike lane in the sample. Streets that were treated, but do not contain a traffic measuring station, are not considered here.

I use the synthetic control group design on municipality level as a more macro-economic

the accident is assigned to both streets. However, more than 90 percent of the accidents (the exact number depends on the year) can be assigned to a single street. In order to combine street segments approach. For this data set, I take the sum of accidents within a municipality per month for all of Germany between 2018 and 2020.[26] Treated units are then the different Berlin districts in which PUBLs were installed while potential control units are recruited from municipalities in Germany outside of Berlin and for which there is data on the matching variables. These matching variables as well as additional control variables are presented in the following paragraph.[27]

Matching and control variables Since traffic speed and volume are measured locally within a time frame of about two years, most characteristics specific to streets and measuring stations are controlled for by respective fixed effects. Measuring station fixed effects capture e.g. the influence of nearby alternative modes of transport like subways, and date fixed effects account for city-wide shocks on the respective day. However, I am as well able to take into account two time-varying variables on very granular time and spatial scale. Firstly, I include geo-referenced construction works.[28] The data contains exact dates when construction works took place (mostly a time range of several days or weeks) and is differentiated by status (e.g. approved, finished, in coordination etc.) and limitations to the public caused by them (for example the closure of a traffic lane or even the entire street). I only consider construction works, which are finished, approved or ongoing. In most specifications, I will use a simple dummy variable, which indicates whether some type of construction took place or not. In additional analyses I will also account for non-binary limitations to traffic and the public. Secondly, I know about changes in the speed limit regime. Few streets, all of which in the control group, experienced a change in speed limit from a maximum speed of 50km/h to 30km/h[29] during the observation period. These changes are mandated by the Berlin Senate and realized by local authorities. Reasons for such measures are noise control, air pollution prevention, or road safety. I account for those changes using a dummy variable switching to one on the date of implementation. There are also temporary changes in the speed limit during a day, e.g. from 6 a.m. until 5 p.m., which mostly happen on streets close to a school or a kindergarten. Controlling for the latter variation in speed limit is necessary when using the entire hourly data-set in the two-way fixed effects estimations. In the case of

into entire streets, I combine the OSM data to street segment data as provided by the city of Berlin (Geoportal Berlin, 2021). Otherwise, single streets would be split into several observations.

[26]Observations for three out of 16 states drop out of the data set. The respective states are North Rhine-Westphalia, Mecklenburg-West Pomerania, and Thuringia. The reason is a lack of data for the year 2018.

[27]In order to lower the computational burden, I limit the control sample to municipalities with less than 25.000 inhabitants and thereby ignore very small and rural regions. However, I assume those not to be comparable to the treated units of interest. Changing the threshold value to 10.000 has no effects on the results.

[28]The data was provided by the Berlin Senatsverwaltung für Umwelt, Verkehr und Klimaschutz (Senate Office for Environment, Traffic and Climate protection).

[29]This corresponds to a change of about 31mph to approximately 19mph.

daily temporary speed limits, a binary variable switching to one in the respective time frame is included in the estimations. Overall, about 12 percent of the sample is affected by speed limit changes over time or throughout the day.

I additionally gather information about traffic measuring stations and their surroundings by OSM and other sources with city-specific data. OSM allows to match the information whether a traffic measuring station lies within a certain radius (I choose a radius of 50 meter) to a tram or rail line. Thereby, it is possible to see whether a street directly "competes" with the rail line. Furthermore, for every measuring station, I match information whether it lies at a bike lane, which existed prior to the installation of the pop up bike lanes.[30] While this type of information is already captured by station fixed effects, the data still makes it possible to split outcomes by street characteristics. For example, pop-up bike lanes were predominantly installed at streets without major cycling infrastructure. I will therefore test whether outcomes are sensitive to varying control units with respect to their bike-friendliness.

In the analyses of accidents, I furthermore make use of administrative annual data of all out of the more than 10.000 municipalities in Germany, which accommodate more than 25,000 inhabitants. This data is used foremost in order to match treatment and control group in the synthetic control group design. The population restriction is made in order not to include too rural areas in the matching procedures. I use information about land use patterns (space available for traffic and for settlements), voting behavior (percentage of Green Party voters, voter turnout), and population. I also retrieve economic data (unemployment rate), as well as data regarding road safety. All this data is publicly available on a website for regional data by the Statistical Office.[31]

Sample adjustments In order to make the control group in my analyses more plausible, I apply some adjustments to the data set for my main estimations. The installation of pop-up lanes aimed at streets with more than one car lane and was realized on streets with either two or three lanes. This is why in the majority of estimations I exclude one-lane as well as four-lane streets.[32] Furthermore, most treated streets had no prior cycling infrastructure. Thus, in some estimations I will restrict the sample to streets without any sort of cycle lanes prior to the PUBL installation.[33] For the control group, I exclude all measuring stations, which lie within a 1km radius of a treated unit. This is done in

[30]The bicycle infrastructure data comes from a collection of shapefiles covering different topics in Berlin (https://www.geodaten.tu-berlin.de/menue/downloads/berlin/). I define a station to lie at a cycle lane if it is within a 15 meter reach.

[31]https://www.regionalstatistik.de/genesis/online.

[32]Four-lane streets only make up less than 0.3 percent of the overall sample, while about 25 percent of the sample are streets with one lane.

[33]Unfortunately, there was no list of streets (or the senate was not willing to provide me with such a list), which indicates streets eligible for PUBLs. Then, I could have chosen potential but not chosen streets as control group (compare e.g. Greenstone et al. (2010) for a similar setup).

order to account for potential deviation effects of traffic. Thus, if a street is treated, then surrounding streets might be affected as a consequence, because the drivers search for different, now potentially faster routes. In this case, the potential control group would be affected by the treatment itself. In further estimations, I explicitly test for these spillover effects to nearby roads.

4 Methodology

I aim to identify the causal effect of bike lanes on several outcomes like traffic volume, average traffic speed, and accidents. In my analyses, I exploit the (conditional) random timing and placement of pop-up bike lanes in Berlin during the COVID-19 pandemic in 2020. Thus, traffic measuring stations and streets, where pop-up bike lanes were set up, are handled as treatment groups, while a large number of other streets of similar size and characteristics are taken into consideration as potential control group. Firstly, I use an event study design to analyze the data aggregated to weekly levels. I primarily use it to test the common trend assumption as well as to identify the development of the outcomes over time in the post-treatment period. Secondly, I use two-way fixed effects estimations in order to receive effects in terms of single coefficients. Most importantly however, they allow me to use the entire hourly data set. Lastly, I conduct synthetic control group analyses in my accident analyses as a robustness check.

Event study design In a first step, in order to justify the common trends assumption between untreated and treated units, I estimate a flexible event study model, which takes into account the different timing of implementation and the different streets affected (Clarke and Schythe, 2020). In order to make this assumption more plausible, I impose a range of sample restrictions as described before. For estimation I use the following equation:

$$Y_{it} = \alpha + \sum_{l}^{L} \beta_l (\text{Lead } l)_{it} + \sum_{k}^{K} \gamma_k (\text{Lag } k)_{it} + \mu_i + \delta_t + \mathbf{X}_{it}\phi + \zeta_i(\text{Station} \times \text{LD}) + \epsilon_{it}. \quad (1)$$

The outcome variable Y is observed for individual monitoring station or street i at time t (which is either a running week or a running month variable). Station fixed effects are given by μ_i. They control for observable (e.g. public transport stops, topography or the number of lanes) and unobservable factors (e.g. local or political preferences in the area), which are specific to a monitoring station and its surroundings and that do not change over the time frame observed. Time fixed effects, measured by δ_t, account for shocks, which simultaneously affect the whole city, and could potentially influence travel mode and prevalence, e.g. holidays. The impact of time-varying characteristics $\mathbf{X}$, like construction works, is measured by ϕ, while ζ_i captures the effect of a station-specific lock-down dummy that I control for in the majority of two-way fixed effects specifications. Finally, ϵ_{it} represents an unobserved error term. Leads and Lags in equation 1 are dummy variables, which represent the number of periods l and k the unit is away from the event.[34] Thus, the time of opening a pop-up lane is normalized such that for each case the opening is at $l = 0$. One lead or lag variable is omitted as the baseline difference between treated and untreated units. The maximum number of Leads L (Lags K) included in the regression are then the total number of weeks before (after) the treatment. Streets without the implementation of pop-up lanes serve as pure control group, such that leads and lags are always zero. These binary variables thus capture the difference between treated and untreated streets in comparison to their difference in the base period, which by definition is zero. Without a significant difference between treatment and control group prior to the base period, the common trend assumption in the respective time frame most likely holds. The implicit assumption here is that without treatment, treated and untreated streets would have maintained differences just like in the base line period. The main advantage compared to a standard two way fixed effects model is that rather than relying on a single coefficient for post-treatment, this model captures the development of treatment effects over time via the lag coefficients and allows to inspect the common trend assumption.

Two-way fixed effects The main analyses are then conducted with a standard two-way fixed effects (TWFE) model[35] in which the lags and leads of Equation 1[36] are replaced by $\beta\text{PostTreatment}_{it}$, where $\text{PostTreatment}_{it} = \mathbf{1}\left[t \geq \text{Treatment}_i\right]$. In the estimation, all never treated measuring stations have this treatment indicator always set to zero, while it switches to one for PUBL units after the beginning of treatment. This estimation provides me with a single treatment effect pooled over all treated streets. The advantage of using the standard two-way fixed effects model is that it allows me to use the entire data-set and therefore to control for date and hour fixed effects. It is now furthermore possible to specifically control for temporary speed limit zones, which are in place e.g. between 6 a.m. and 5 p.m. on certain streets. Using the hourly data is not possible in the event study design since it requires no time gaps in the observations. This is why I aggregate observations to weekly levels in event study estimates, which solves the problem of gaps.[37]

[34] Thus, $(\text{Lead } l)_{it} = \mathbf{1}\left[t = \text{Event}_i - l\right]$ for $l \in \{1, ..., L-1\}$, and $(\text{Lag } k)_{it} = \mathbf{1}\left[t = \text{Event}_i + k\right]$ for $k \in \{1, ..., K-1\}$.

[35] I will interchangeably refer to two-way FE estimations as difference in differences (DD) model.

[36] $\sum_l^L \beta_l (\text{Lead } l)_{it} + \sum_k^K \gamma_k (\text{Lag } k)_{it}$.

[37] In the robustness section I will address potential problems regarding standard two-way fixed effects.

Synthetic control group design When analyzing the effects of PUBLs on accidents, I will furthermore apply the synthetic control group design (Abadie et al., 2010). Due to the fact that I have geo-located accidents for all of Germany, this allows me to use a macro-perspective, comparing treated districts of Berlin with similar districts all over the country.[38] Apart from pre-treatment developments of the outcome variable of interest, I use a variety of district-specific observables like population, unemployment rate, or the share of the space designated for traffic infrastructure, as matching variables to find a data-driven control group.

Main threat to identification Some aspects might influence the assumption of random assignment, which I want to specifically tackle in my analyses. In the following, I describe the potential problems and how I finally aim to solve them.

First, there is the concern of non-random selection of streets by the responsible authorities for installing a pop-up bike lane in the first place. If bike lanes were randomly assigned to any street in the city, then selection bias would not pose a problem. However, if streets are chosen based on their characteristics, e.g. that only streets with minor car traffic are chosen, then the estimated treatment effect will be biased. The choice where to locate pop-up lanes was based on Berlin-wide plans for extending the cycling infrastructure prior to the pandemic. Even though the plans existed for the whole city, all of the pop-up lanes were created in only a subset of districts[39]. In Section 2, I already argue in how far the allocation of pop-up lanes was random. However, some local district governments were more supportive in establishing PUBLs than others. This may raise the concern that there exist systematic differences between these and other districts in the city.

Second, in general the district-specific differences should be captured in the estimations by the measuring station fixed effects. Nonetheless, shocks like the lock-down after the beginning of the COVID-pandemic may lead to different behavioral adaptations in districts with PUBLS compared to districts without, e.g. with respect to commuting choices. A potential reason are political preferences and attitudes in certain areas, which may translate to differential behavioral adaptations regarding home-office or the utilization of public transportation. For example, if a district is populated with more blue-collar than white-collar workers, then home-office might be less of an option there compared to other areas of the city with a differently composed workforce. This may systematically bias the results.

[38]The measures for car volume and speed are only available for Berlin. Using the synthetic control group design on such a local scale is rather problematic due to the lack of observable control variables on street level, which are required for the matching procedure of treatment and synthetic control group. This is why the use of this method is restricted to the accident analyses.

[39]Charlottenburg, Friedrichshain-Kreuzberg, Neukölln, Treptow-Köpenick, and Pankow.

Third, some of the streets lie close to a subway line, while others are further away. As utilization of public transport has significantly changed during the Corona-crisis, those streets might have been affected differently to streets further away from public transport.

In order to control for potential differential developments after the start of the lockdown and address the aforementioned concerns, I add an interaction term to the estimation, which accounts for *lockdown* × *measuring station* effects.[40] This interaction term captures effects, which are present on a very local scale (station/street-level) after the beginning of the lockdown. Thus, it e.g. captures differential developments on streets close to public transport compared to streets further away from it. At the same time, it also subsumes district-specific changes due to local commuting preferences. Due to the fact that some pop-up lanes were installed right after the beginning of the lock-down, the interaction term may capture away parts of the actual pop-up lane effect. This is why I consider estimates, which control for the interaction term, as lower-bound outcomes.

In all estimations, I use standard errors clustered on a time and a spatial dimension. The former is a running week variable, while the latter consists of 1km × 1km grid cells spanning the entire city.[41] Thus, I tackle concerns that treatment may be spatially or temporally correlated, and therefore account for spillover effects. In alternative specifications in the robustness section, I will also test for different clusters.

5 Results

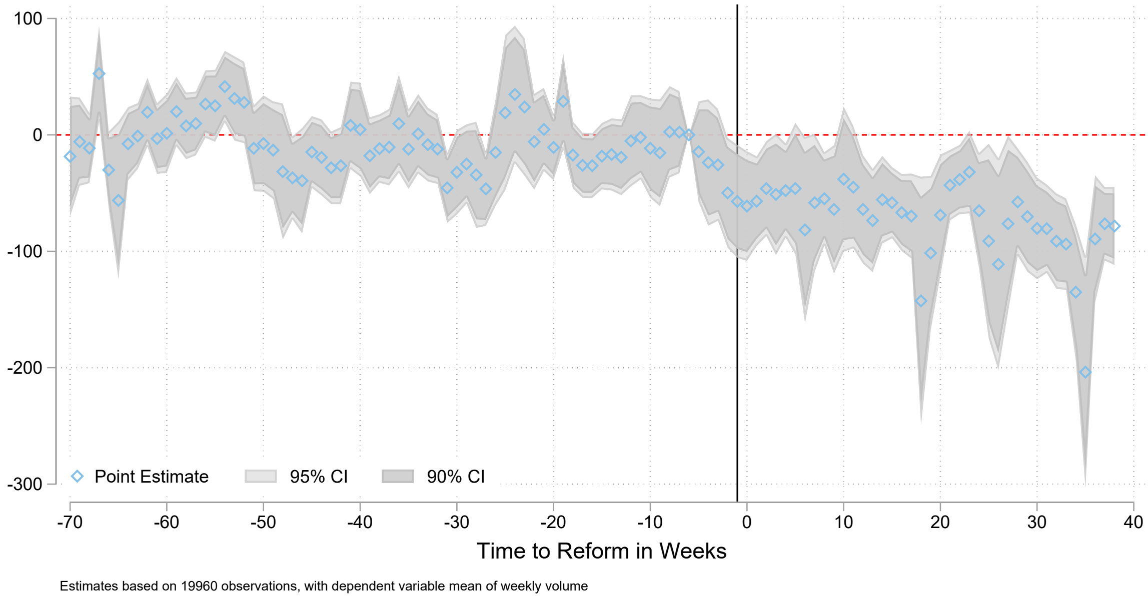
5.1 Volume and speed

Figure 5 shows the effects of the introduction of a pop-up bike lane on traffic volume and on average speed by a graphical representation of an event study design. Blue dots show the main estimation coefficients of leads and lags, while confidence intervals (CI) are depicted as area shades in light (95% CI) and dark gray (90% CI) respectively. No additional control variables apart from station and week fixed effects are added in these first estimations in order to show the "pure" common trend. Adding construction and 30-kmh-zones does not change the picture though.[42] Both graphs generally show that the common trend assumption is satisfied, however with some minor drawbacks. Considering

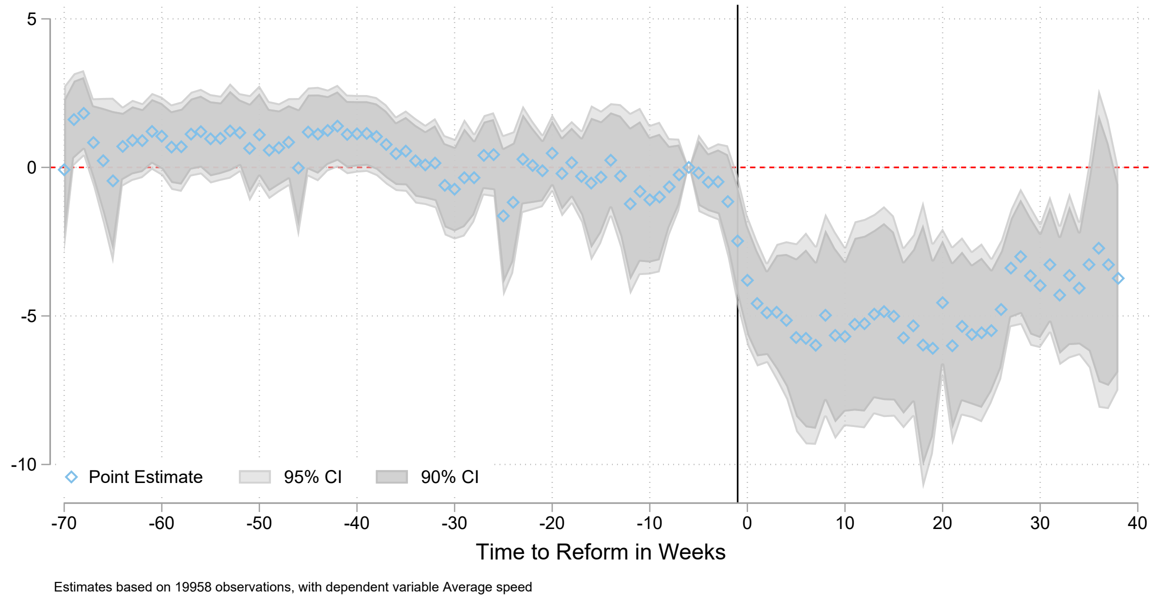
[40]Lockdown then is a dummy variable and it is defined as the time after March 22nd, 2020. The reason being is that measures were lifted from time to time and partly reinstated again. Thus, there was no clear-cut end of the lockdown. In autumn/winter of 2020, measures became increasingly strict again, resulting in another so-called "hard lockdown" in December of 2020.

[41]I use the INSPIRE grid for Germany, which is publicly available (https://gdz.bkg.bund.de/index.php/default/inspire/sonstige-inspire-themen/geographische-gitter-fur-deutschland-in-lambert-projektion-geogitter-inspire.html).

[42]The base period here is chosen to be at five weeks prior to treatment, which is the average number of weeks between the start of the lockdown and the installation of a PUBL. Whenever the *station* × *post-lockdown* interaction effect is included in the estimation, the base period is at *lead = 1*.



(a) Number of vehicles



(b) Average vehicle speed

Figure 5: Effects on traffic volume and speed

Note: The two graphs show the results of two separate event study estimations as described in Equation 1. Outcome variable Y in Figure 5a is the absolute number of vehicles while it is average vehicle speed in Figure 5b. Blue dots represent the main estimation coefficients of leads and lags. Confidence intervals are depicted as area shades in light (95% CI) and dark gray (90% CI). The vertical solid black line shows the time of treatment, which is anchored at 0. Leads and lags are the time before and after treatment in weeks. The estimations include station and week fixed effects. The sample is restricted to streets with two or three lanes and all streets within a radius of 1km to a treated street is excluded from the estimations. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.

average vehicle speed as outcome variable (Panel (b)) suggests a very stable parallel trend between treated and untreated streets prior to the PUBL installations. Only a very small number of observations about one year or more prior to treatment show marginally significant deviations in this case. For the number of vehicles as outcome, more but still very few occurrences show significant positive as well as negative differences in the pre-event time frame. However, they do not change the overall picture suggesting common trends between treated and untreated units prior to the lock-down. Additionally, one has to bear in mind that the regressions run here are based on weekly averages of speed and volume, and thereby do not take into account date or hour fixed effects, which may further adjust for unobserved differences between treated and untreated streets.[43] The main outcomes presented in this paper moreover include an interaction term, which takes into account station specific developments after the implementation of a lockdown. Figure A.1 shows the event study results considering the interaction effect. It again exhibits the validity of the parallel trend assumption.

Traffic volume The PUBL introduction exhibits a relatively small, but significant effect on the number of vehicles compared to the development on control streets when ignoring the *station* × *post-lockdown* interaction. The effect is rather modest with a few gaps to the downside and is relatively stable in size. In terms of absolute effect size, about 50-100 vehicles per week less are observed on average on treated streets compared to untreated ones. A negative effect would mean that some drivers do not want to keep using the old way and rather circumvent these high-traffic areas. The pre-trend assumption holds for the vast majority of the pre-treatment weeks, even though the few significant differences may be considered a potential backdrop in the causal interpretation of results. Including the interaction term, as was done in Figure A.1a, causes the PUBL effect on volume to become insignificant. This means that it is not possible to attribute the volume effect directly to the installation of a PUBL, but that the COVID-19 lockdown lead to a slightly differential development on treated streets compared to untreated ones.

Traffic speed Average speed of vehicles significantly decreased directly after the PUBLs were installed, which implies an increase in congestion. In absolute terms, average speed decreased by about 4-5 kilometers per hour (kmh)[44], which is about 10% of the maximum speed allowed on these streets.[45] This result is not surprising due to the fact that car-utilization behavior hardly changed in the beginning, while the number of lanes was reduced (partly from two lanes to one). The reduction of average speed compared to un-

[43] In order to run the event study design, the data requires to be balanced, which is why all event study estimates stem from data aggregated to a weekly level.

[44] This corresponds to about 2.5-3.1 miles per hour.

[45] In Germany, the standard speed within cities is 50 kmh (or 31 mph) with some exceptions.

treated streets is very stable over time, which is certified after including the interaction term as presented in Figure A.1b. This suggests that in the post-treatment time frame observed, there were no major behavioral adaptions.

Single-coefficient model While the graphical analysis provides an insight into the development of the effect over time and allows to track and reaffirm the parallel trend assumption, I also want to translate these effects into one overall estimate. The advantage of one single coefficient is that it pins down the results to an overall outcome. As discussed before, the effect on the outcome is relatively stable over time, which disburdens the concern that two-way fixed effects regressions may conceal important heterogeneities.[46] Moreover, I am now able to use the entire data set and therefore to control for date and hour fixed effects in all estimations. This means that these estimations additionally control for intra-day commuting patterns and city-wide shocks, which may have occurred on single days. The few downward spikes as observed in Figure A.1a could e.g. be attributable to such daily shocks that to some extent differently affect treated and untreated streets. While the event study design as presented in Figure 5 only includes a subset of control variables in order to show the "raw" common trend, I now control in most specifications for the interaction term between a post-lockdown dummy variable and a measuring station indicator. As discussed in Section 4, it is supposed to account for differential behavior adaptations on a local scale after the COVID-19 lock-down went into effect. Point estimates of these single-coefficient models are shown in Table 1, which presents results for outcomes in absolute values (*Panel A*) as well as for logged values (*Panel B*). All even columns include the *station* × *post-lockdown* interaction term, while uneven columns do not. As before in the event study with heterogeneous timing, I find a significantly negative effect on speed, and thus more congestion for cars. PUBLs led to an overall decrease of car speed of about 11 to 12 percentage points as *Panel B* shows. This result holds after controlling for the interaction term. In terms of volume, there is a significant decline of vehicles without the interaction term, which turns insignificant as soon as the interaction term is included in the regression. This hints towards a small decline of vehicles on treated streets compared to untreated ones, which cannot clearly be attributed to the installation of PUBLs. However, since the cycle lanes were installed only shortly after the lock-down went into effect, it is in general hard to completely disentangle these two.

Table A.1 shows outcomes of TWFE estimations using the weekly data set, which was used in the event study model. It shows that coefficients are very similar or even slightly higher compared to the regressions based on hourly data, and that inference is hardly

[46]In the robustness section I will account for more concerns about heterogeneous effects in terms of time and treatment unit.

	Volume		Speed	
	(1)	(2)	(3)	(4)
Panel A: Outcome in absolute values				
1(PU lane)	-53.57***	-7.476	-4.512***	-4.123***
	(4.876)	(7.218)	(0.306)	(0.536)
N	1546526	1546526	1543453	1543453
R^2	0.743	0.755	0.757	0.772
Stations	215	215	215	215
Interaction	No	Yes	No	Yes
Panel B: Log-transformed outcome				
1(PU lane)	-0.0228***	-0.00540	-0.113***	-0.122***
	(0.00724)	(0.0126)	(0.00878)	(0.0144)
N	1543493	1543493	1543453	1543453
R^2	0.628	0.650	0.585	0.607
Stations	215	215	215	215
Interaction	No	Yes	No	Yes

Table 1: TWFE Effect on Volume and Speed

Note: The table presents the coefficients of the treatment effects of separate two-way fixed effects estimations with vehicle volume and vehicle speed as dependent variables. Panel A shows the coefficients of interest with outcomes in absolute terms. Panel B shows the same for logged outcome variables. Even columns include an interaction term between a unique measuring station identifier and a post-lockdown dummy variable while uneven columns do not. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, and an indicator for a change in speed limits. Stations within a one kilometer radius of a treated street are excluded and the sample is restricted to streets with two or three lanes. Standard errors are clustered at 1km × 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

different. In the following, I will mainly show outcomes of single-coefficient models using hourly data in order to be able to control for the full set of controls.

5.1.1 Heterogeneity

Next, I will analyze in how far outcomes differ by treated unit, by time of the day, between type of vehicles (cars and lorries), and whether there are differences between different street sizes.

Treated unit Table A.2 presents leave-one-out analyses, which means that I conduct the main analysis several times, always leaving out one treated measuring station. This type of analyses may reveal whether results are driven by single streets or stations in the sample. All estimations include the entire set of control variables as well as the *station* × *post-lockdown* interaction term. With respect to average speed, the table shows that the overall picture holds. This means a significant increase in congestion over all specifications. However, one treated measuring seems to stand out in effect size. While the majority of coefficients lie between .12 and .13, leaving out station 15 in the table results in a coefficient of about .09. Thus, this specific station has a significant larger effect on the size of the overall result than others. The street on which this measuring station is situated is *Kantstrasse* in the western district of Berlin Charlottenburg. Besaid station lies very close to a nearby highway circling the city. A possible explanation for the difference in effect size is that this specific segment was reduced to a one-lane street. Narrowing down the space for cars to a single lane may therefore have a larger effect on speed than reducing a three-lane street to a two-lane one.[47] Looking at volume, there is no single station that drives the results. All estimates thus remain negative and insignificant. In the following, I will present results with and without this outlier station if required.

Time of day Due to the disaggregated nature of the data, I am able to differentiate traffic volume and speed by hour of the day. I am thus able to analyze whether the reduction of a street lane mostly affects commuters in traffic peak hours or also other types of trips.[48] In order to do so, I run two types of estimations: i) I clear the sample from all hourly observations except for the peak hours and ii) I run estimations for each hour of the day separately. In both cases, the estimations include the full set of control variables[49]. Results for the first approach are presented in Table A.3. Point estimates correspond to the ones in the main outcomes. This means that in the main traffic peak hours speed and volume on average do not seem to be differently affected compared to other times of the day. The second approach enables an even closer look into intraday variations. To illustrate those, coefficients of single-coefficient two-way fixed effects regressions for each hour of the day are depicted in Figures A.2 and A.3 and additionally presented in Table A.4. Unlike the main analyses, which are restricted to daytime, I now estimate the difference-in-differences model for all 24 hours of the day. Figure A.2 reveals that the difference between treated and untreated units is significant throughout the day. The size of the effect, however, varies. In the night hours (midnight until 5 a.m.), speed is significantly slower by about five to seven percentage points. From 6 a.m. onward, when traffic usually starts to pick up, the effect size gradually increases. The maximum difference is reached in the afternoon with a point estimate of about $-.16$. Thus, average car speed is about 16 percentage points lower to similar streets compared

[47] Another possibility would have been a change in speed regulation, e.g. from 50 to 30. Even though this specific street was affected by such a regulation change in 2018, there was no such change on any treated street in my sample period.

[48] Traffic peak hours are defined as the time frame between 6 a.m. and 9 a.m. in the morning as well as 4 p.m. and 7 p.m. in the evening.

[49] This includes date fixed effects, measuring station fixed effects, a constructions dummy, and the station times lockdown-dummy interaction.

to times before treatment at the main traffic peak time of the day. Until the evening, the effect size gradually decreases. Regarding vehicle volume, there is significantly more traffic on PUBL streets compared to non-PUBL streets in the night hours, but not during the day when we consider outcomes in log terms. Absolute values as presented in Figure A.3, however, suggest that this difference is driven by a very small absolute effect of about 20 vehicles. Since at night hours there is a significantly lower number of cars on the streets, effects at that time of the day should not be over-interpreted. With respect to speed, traffic is relatively free-flowing at night, which explains the smaller effect size. The size of the volume effect is also very small in absolute terms at night times with an absolute effect of about 20 vehicles.

Cars and Trucks In the data it is possible to differentiate traffic volume and speed by type of vehicles. I thus analyze whether there are differences in the outcome when differentiating between cars and trucks.[50] Table A.5 shows that the entire result is driven by cars. A look into the data reveals that only about six percent of vehicles observed on the streets in the unrestricted sample are trucks. As a consequence, also the variation of observed trucks is much lower compared to cars. This reduced variation may therefore lack power to detect significant results.

Street size Apart from differences analyzed so far, there may exist heterogeneities regarding different street sizes. For example, streets with two lanes prior to the installment of a PUBL may show different results compared to those with three lanes ahead of treatment because they may be harder or easier to substitute for commuters. Table A.6 shows results for two- and three-lane streets separately, restricting also the control group to streets with the respective number of lanes. Apparently, volume is not differently affected on the different types of streets. The effect size for average speed indeed shows slightly different results. Smaller streets are affected more severely compared to larger ones. From a policy perspective, this speaks for the installment of new cycle lanes on larger streets, while it may be advisable to spare streets with less space. However, one has to take into account the much smaller sample size, which is about five times bigger for two lane streets. Furthermore, the difference of the effect size is only marginally significant. Taking all aspects into account, heterogeneities of results are, if existent, rather small.

[50]Trucks are defined as vehicles longer than 7.5 meters. This subsumes buses and the majority of delivery trucks.

5.1.2 Spillover effects to surrounding streets

Results so far suggest that traffic on treated streets decreased slightly - even though the decrease cannot be unambiguously attributed to the installation of a PUBL - and congestion increased. Thus, even though there are in total less vehicles on the streets, the remaining ones are slower due to the reduction in space for motorized traffic. Since the utilization of public transport significantly declined during the lockdown, this might have put additional pressure on the streets by people changing their preferred mode of transport from train to car or bike. These people might want to avoid congested streets by choosing alternative roads close-by. Thus, commuters leaving their old accustomed routes as well as new car users might divert to roads in the vicinity of PUBL streets. In order to test this hypothesis, I repeat the main analysis with measuring stations, which lie within different distance ranges of a PUBL. Thereby, I assign the respective starting date of the treatment to each nearby station and delete the actually treated streets from the sample.[51] Table A.7 shows the corresponding estimates. While speed and therefore congestion seems not to be affected on the surroundings of treated streets, there is a significant increase of car volume on streets within a 750 meter and a 1 kilometer radius of PUBL streets. One would expect the effect size to decrease, the larger the radius drawn from the PUBL street. The table suggests otherwise with the 1km-coefficient being larger than the 750 meter one. However, the difference in effect size is not significant, which means that streets up to 1km away from a PUBL street are equally affected. Moving further away then shows the expected development with the coefficient size tending to zero and being insignificant.[52] This result suggests that drivers are actually nudged away to some extent from their accustomed routes, which however does not negatively affect the surrounding streets in terms of congestion. Rather, a new equilibrium with a more equal distribution of traffic seems to be established.

5.1.3 Medium run effects

Additionally to the data set running until the end of 2020, the Berlin Senate also provided me with additional traffic data for the time period between March and May of 2021. The reason not to include the entire time frame into the main estimations is that many measuring stations drop out of the observation network in 2021, and therefore the sample becomes less balanced. Additionally, there are more than three months missing between my observation period and the data from 2021, which does not allow to track potential developments in between. Results for the entire sample as well as for the sample without

[51] Since I want to know about the effect on all types of lanes, I abstain from the restriction of limiting the sample to streets with a specific number of lanes. This explains the larger overall sample size.

[52] Note that treated stations between the distances are not mutually exclusive. This means that all stations that are handled as treated in the 750m regressions are also treated in the 1km regressions.

the outlier from the leave-one-out analysis are presented in Table A.8. All coefficients slightly increase in size. The volume effect now becomes significant and larger, while the speed effect is only marginally larger, and does not significantly change. Thus, about one year after the installation of the PUBLs, drivers are apparently nudged away to a larger extent from treated streets, which are still more congested than control streets. The combination of less volume combined with similar congestion may be explained by an overall city-wide increase in motorized traffic, which more evenly distributed on the street network compared to pre-treatment times. One reason for the overall traffic increase potentially is a larger share of workers returning to offices rather than working at home. Those now re-entering the streets have an incentive to use streets without a new cycle lane that replaced a car lane.

5.1.4 Robustness

I run a variety of tests in order to assess the robustness of the results with respect to aspects like sample composition, clustering, placebo treatments and variations of control variables.

Standard Errors In my main results I cluster standard errors on level of 1km $\times$ 1km grids spanning the city and weeks in order to account for errors to be spatially and temporally correlated. In alternative specifications, I alter this cluster specification by using 1) the twelve local districts of Berlin and weeks, 2) a station/week cluster, 3) standard errors only clustered on grid level, and 4) standard errors only clustered on weekly level. These different types of clustering thus take into account different variations of spatial clusters and of the time component. This means that the data then is treated as independent across the respective cluster (Cameron et al., 2011). Results in Table A.9 show that inference is not affected. The effect on speed is still significant on at least a 5-percent, and mostly on a 1-percent level. The exclusion of the station with a larger impact on the results (station 15 in Table A.2) in Panel B, also does not alter the results, independent from the cluster specification.

Sample adjustments In my main estimations, I make some restrictions to the sample, e.g. regarding the number of lanes.[53] I vary the sample composition to check if and to what extent different sampling structures may play a role. In the case of significantly different outcomes, estimates are likely biased due to sample selection. Results with different sample restrictions are shown in Table A.10. Firstly, columns 1 and 2 show the results using the full sample (including one-lane-streets and four-lane-streets) and the full set of 24 hours. While the volume coefficient is still not significant, the average

[53] I exclude all one- and four-lane streets.

speed estimate becomes a little smaller in size. The most likely reason is that part of the effect is offset in night hours, where there is not much traffic on the roads in general. Secondly, I look at the full sample again, but now only for non-night times, i.e. from 5 a.m. until 8 p.m. as shown in columns 3 and 4. The formerly made presumption regarding speed and its effect being slightly offset during night hours is confirmed, since the coefficient in column 4 jumps back to the result found before. Taking into account one-lane-streets and four-lane-streets, however, renders the volume coefficient significant with a slightly larger coefficient size. Thus, comparing volume to the entirety of the road system within Berlin shows that volume actually declined. The effect is driven by one-lane-streets, which account for about 25 percent of the sample size.[54] Thirdly, I only consider streets without any type of prior cycling infrastructure. Then, treated streets are such with a PUBL as the first type of cycling infrastructure and control streets are such without any type of cycling infrastructure throughout the sampling period. The reason to look at this sample restriction is twofold. Firstly, almost all treated stations had no prior cycling infrastructure. Therefore, the control group is even more harmonized in terms of characteristics. Secondly, while treated streets receive a direct alternative as mode of transport, bikes do not have an own lane to use in the comparison group. If the volume effect was e.g. larger, this might indicate that car drivers were nudged away from using their car and potentially switched to using the bike. Alternatively, the lack of a change in the effect might be interpreted as such that the new bike lane diverted cyclists from other cycling routes or incentivized commuters to use the bicycle rather than public transport. The sample size is considerably smaller in these specifications. While the volume effect remains insignificant indicating that it is not former car drivers now using the new cycle lanes, the coefficient for average speed becomes slightly larger. This means that restricting the sample to streets with very similar characteristics shows a slightly stronger effect than allowing for a more generalized set of streets in Berlin to be part of the control group. Lastly, in my main estimations I exclude all stations, which lie on a highway, thereby only comparing inner-city streets with each other. The last two columns check whether results hold in the case of including highways into the control group. Results are very close to the full-sample outcomes in the first two columns. The size of the sample is now about twice as large compared to the standard sample and coefficient sizes are similar. Though economically small, the effect on volume is now positive and significant. One potential reason is that home office regulations in times of the COVID-19 lockdown had a larger effect on the highways circling the city and this effect is not entirely subsumed by the *post-lockdown* $\times$ *stations* interaction. That would mean that highway travel was significantly reduced compared to inner-city commuting.

[54] The differentiation between results including streets with one, two, and three lanes compared to results including streets with two, three, and four lanes are not shown here, but are available upon request. Four-lane-streets themselves do not influence coefficients.

However, since I would not consider highways to be part of an adequate control group, this difference should not be over-interpreted. Overall, different sample specifications suggest that results are not prone to selection bias.

Placebo tests A common approach in difference-in-differences models to assess the robustness of results is the performance of placebo tests. Table A.11 presents the results of such tests with respect to treatment timing. Therefore, I deleted all observations in the actual treatment period starting in mid March, 2021. Then, treatment status was assigned to all treated measuring stations for three different placebo-treatment dates. The dates were chosen at intervals of about four months, starting in January of 2020. The table shows that all placebo treatment effects are insignificant except for volume in the case of treatment starting in April of 2019, which was about one year prior to actual treatment. In this case, the coefficient is marginally significant with an economically very small effect. Due to the large time interval between actual and placebo treatment as well as the fact that the pre-trend assumption was more fuzzy in the case of volume, I conclude that placebo tests generally support the main results.

Construction control In my main estimations I have so far used a construction dummy as time-varying control variable, which switched to one, whenever there was any type of construction during the observation period. This means that the dummy does not take into account which kind of restriction was imposed on traffic participants. However, there exist many different types of construction and accompanying restrictions. It is likely that imposing a temporary stopping restriction on a street differently affects the average speed of vehicles compared to blocking an entire lane. The reason why I use a dummy variable is that it is not straight-forward to interpret a categorical construction variable, since there does not exist a natural order, which tells about the severeness of restriction. Table A.12 compares the results between using a construction dummy and allowing the type of restriction to vary. The effect on volume remains insignificant and very small in absolute size. Car speed is still significantly affected with about the same coefficient size. The table also reveals that it is important to control for construction, which significantly affects both, volume and speed. Thus, construction works with the implication of increased congestion actually may nudge drivers away from their accustomed routes.

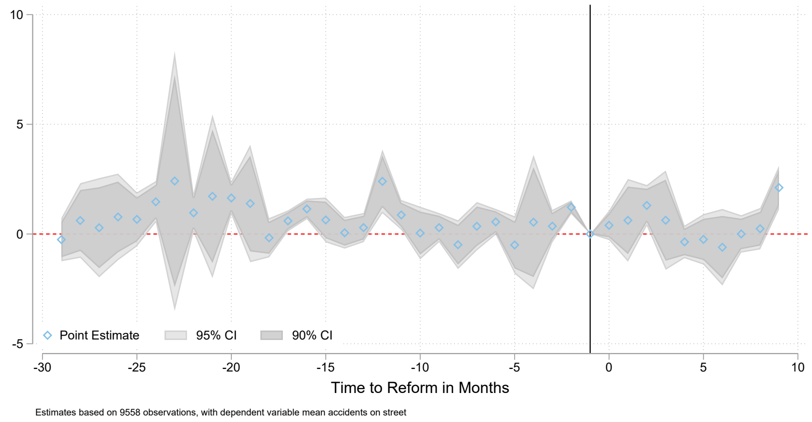
Concerns about Standard TWFE Standard two-way FE estimations assume a constant treatment effect, which causes potential biases, when it varies over affected unit and time. This bias is caused by potentially negative weights assigned to the treatment effect of single treated units, which then compose a weighted sum over all DID estimations.

This becomes a problem if the average treatment effects are heterogeneous across groups or periods (De Chaisemartin and d'Haultfoeuille, 2020). With respect to treated units, I show in the leave-one-out analyses, as presented in Table A.2, that the results are homogeneous except for one station. Moreover, the initial event study design exhibits not much variation of the effect over time after treatment. Therefore, when accounting for the outlier, the respective bias does not seem to weigh heavy. However, I still compute the weights as suggested by De Chaisemartin and d'Haultfoeuille (2020). All weights in my regressions are positive, which suggests that this bias is of no relevance in this setting. Apart from checking the weights, I also repeat my main analyses without heterogeneous timing, which again addresses the potential problem when streets are treated at different points in time. Now, all treated roads are assigned the same pre-treatment and post-treatment period. As a consequence, all observations between March, 25th and June, 30th are deleted and the treatment indicator switches to one after that period for all treated measuring stations. Table A.13 shows results for which I delete all observations between the first and the last treatment date. The effect on volume and speed are very similar to my main outcomes, which suggests that the staggered timing of treatment in the main analyses does not pose a major problem.

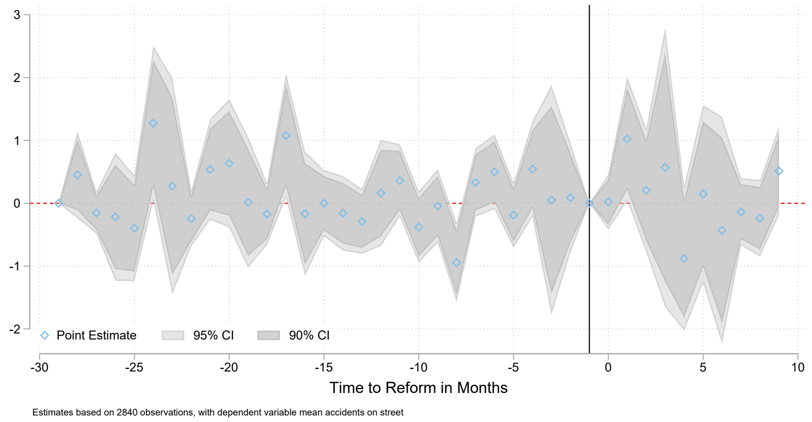
5.2 Accidents

To get a first visual impression of the accidents data within Berlin, I plot the monthly mean development of total accidents by treatment status in Figure A.4. Since about 40 percent of overall observations are zeroes and the maximum number of total accidents per street and month is 20, the monthly mean is relatively low and ranges between .5 and 2. This means that the time variation of accidents is not very high. However, the development of accidents between treated and non-treated streets in Berlin is similar before and after treatment. Panels (b) and (c) show that the result is independent from the means of transport involved. Apparently, the occurrence of accidents is cyclical with a higher incidence in summer months.

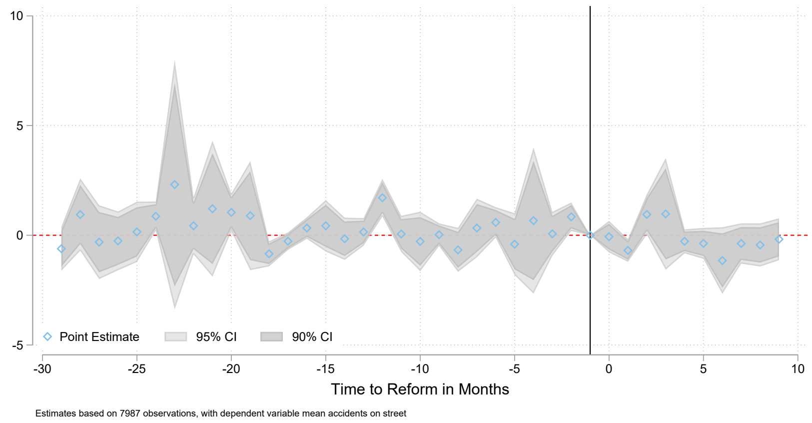
Event study design I repeat the event study analysis with accidents, where the outcome variable is the total number of accidents, the number of bike accidents or the number of car accidents respectively. Accidents are aggregated on street level and are only available on a monthly basis. Thus, the point estimates depict the differentials between PUBL streets and non-PUBL streets per month. Figure 6 shows that the variation between treated and untreated streets is relatively small in absolute terms and that there is no clear change in accidents after treatment. For the time before and after the installation of a PUBL, the average difference between treated and untreated units is never larger than three accidents. For bike accidents, the variation is even smaller. I also fail to detect differences if I take into account the severeness of injuries, which means that for neither accidents with death or heavily injured persons nor for those without such casualties, there is a change in cases (results not shown here). Thus, at least in the short term of observations available, PUBLs did not lead to a significant decline of accidents on those streets, but they also did not significantly contribute to other types of accidents. However, this development does not take into account the number of cyclists on these streets due to a lack of data available. If there was a significant increase in cyclists, then the possibility of a decline in per-cyclist accidents could not be eliminated. Repeating the analysis separately for each treated streets or the leave-one-out analysis (not shown here) do not alter these results.



(a) Overall accidents



(b) Bike accidents



(c) Car accidents

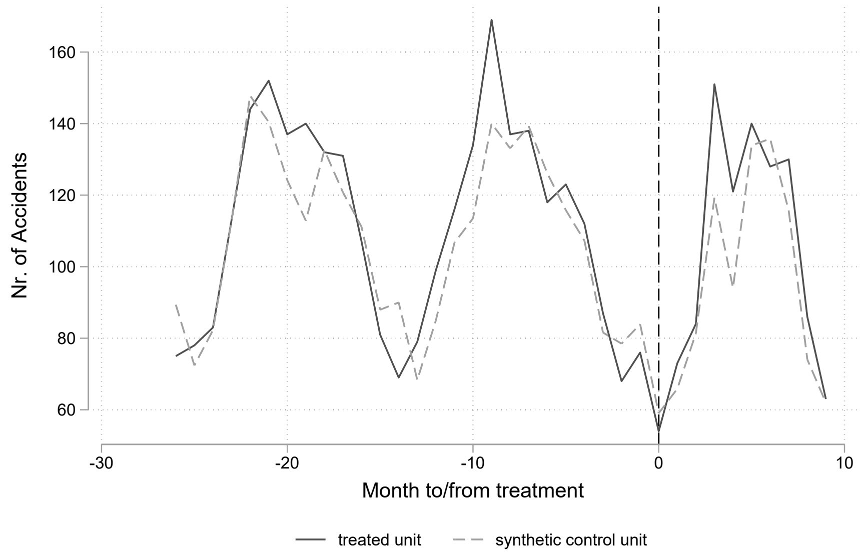
Figure 6: Effects on Accidents

Note: The graphs show the results of separate event study estimations as described in Equation 1. Outcomes are overall accidents (Figure 6a), accidents with bicycles involved (Figure 6b), and accidents with cars involved (Figure 6c). Blue dots represent the main estimation coefficients of leads and lags. Confidence intervals are depicted as area shades in light (95% CI) and dark gray (90% CI). The vertical solid black line shows the time of treatment, which is anchored at 0. Leads and lags are the time before and after treatment in months. The estimations include street and month fixed effects as well as controls for road condition at the time of the accident and the type of street (primary, secondary etc.). Standard errors are clustered at 1km × 1km grid cell level spanning the city times a running month variable.

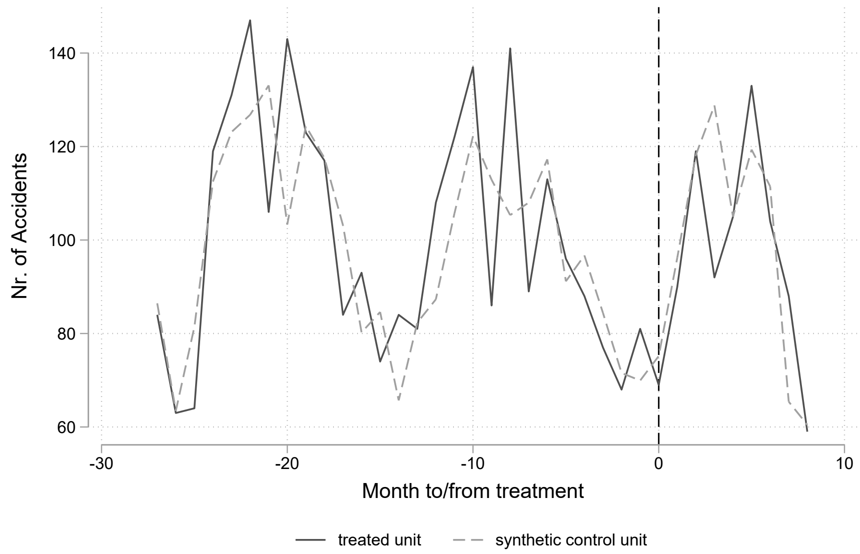
Synthetic control group design Green et al. (2016) analyzed the effects of a congestion charge in London on traffic accidents and used the synthetic control group method (Abadie et al., 2010) to identify the effects of interest. Their unit of treatment was the city of London while other cities in the country served as synthetic control units. In order to make my results more robust, I follow their approach using municipality level data for Germany as a whole. Now, the outcome variable is the number of accidents (and subgroups of such) per month and municipality.[55] Berlin is subdivided into its 12 districts and treatment status is assigned in the month when the first street within the district receives a PUBL. Since I exploit the variation of accidents between municipalities over time, the outcome of this approach also subsumes potential spillover effects on other streets rather than only actually treated ones. Due to the fact that treatment months differ between Berlin's districts, I run the synthetic control group method separately for each district that was treated. The respective treatment unit and synthetic control units are matched on various economic and socio-demographic indicators, which might influence the decision to use certain modes of traffic and could influence the amount of accidents in a region. Among those are the population of the municipality, the share of Green Party voters, the share of space used for settlements and for traffic respectively, and the number of unemployed in the region. Furthermore, the matching procedure uses the annual number of accidents from 2018 to 2020.[56] Tables A.14 to A.17 show the predictor balances between each treated and the corresponding synthetic control group. For all five municipalities the predictor matches are very close. Thus, in terms of predictors the synthetic control group resembles the treated municipalities. Actual results by means of a graph are shown in Figure 7. All sub-graphs support the conjecture that the synthetic control groups are good matches for the treated units since the pre-treatment outcomes of both follow parallel paths. Just like in the event study design, I fail to find significant differences between treated units and controls after the installation of PUBLs. This means that taking a more macro-economic view by considering entire treated districts leads to the same outcome as the within city street-level evaluation. This strengthens the finding of PUBLs not having an effect on total accidents. However, as noted earlier, this ignores the number of cyclists on the streets and potentially implied decreases (in case of an increase of bicycle users) of per-cyclist accidents.

[55]The reason not to run a synthetic control group design with traffic data is because I do not have available traffic data for the whole of Germany, but only for Berlin.

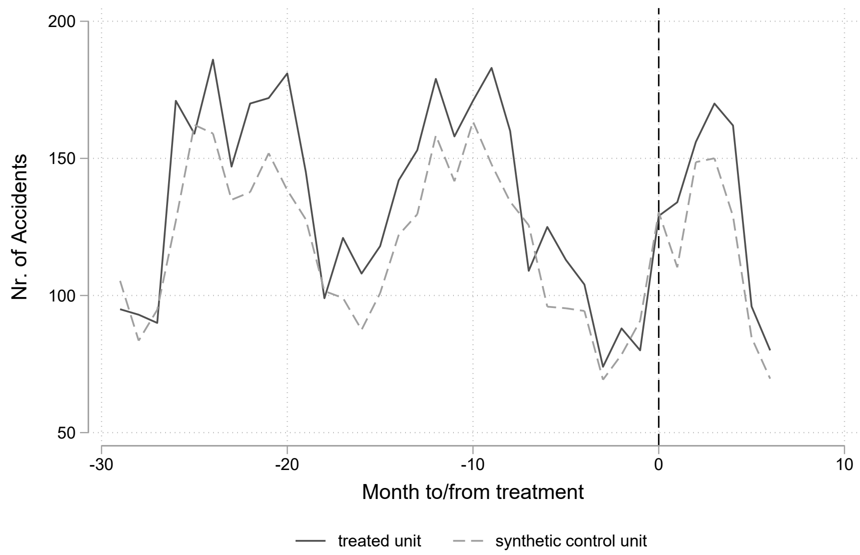
[56]In Germany, there are more than 10.500 municipalities in about 400 districts. While most variables, like population, voting behavior, and land use designation, are available on municipality level, some information like the unemployment rate, is only publicly available on district level. Since municipalities are administrative sub-divisions of districts, I assign the numbers of the district to the respective municipality.



(a) Accidents Kreuzberg-Friedrichshain

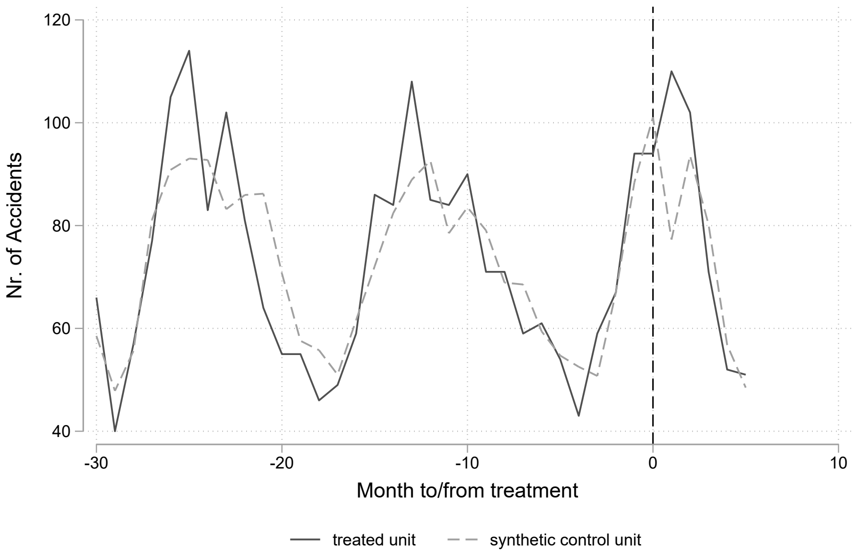


(b) Accidents Pankow

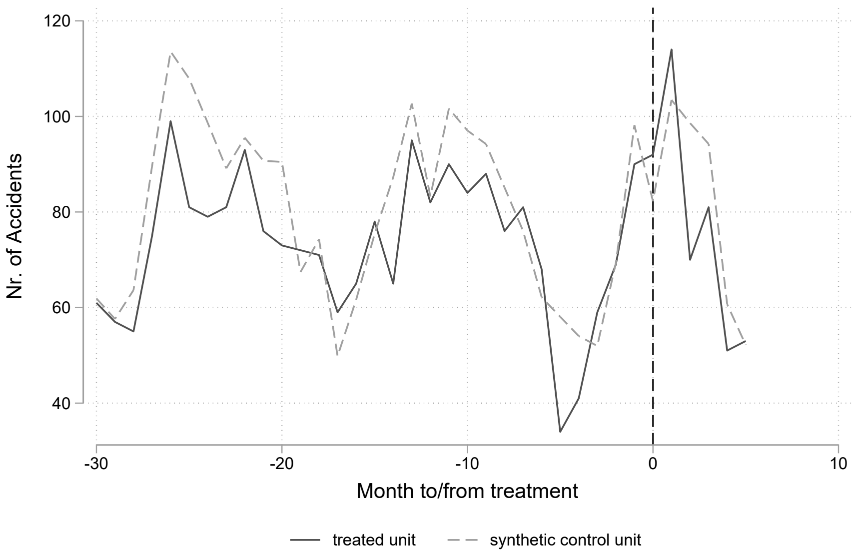


(c) Accidents Charlottenburg-Wilmersdorf

Figure 7: Effects on Accidents using synthetic control method



(d) Accidents Treptow-Köpenick



(e) Accidents Neukölln

Figure 7: Effects on Accidents using synthetic control method

Note: The graphs show the results of separate synthetic control group estimations following Abadie et al. (2010). Outcome variable is the total number of accidents. Treated units are the respective Berlin districts as mentioned in each sub-caption in which a PUBL was installed. The time of treatment is the month of the first placement of a PUBL within the respective district. The synthetic control unit consists of potentially all municipalities in Germany outside of Berlin that did not receive a PUBL. The matching between treatment and control units is based on the respective monthly outcome variable prior to treatment, traffic space, space used for settlements, population, election participation, the share of green party voters, the unemployment rate, and the absolute number of accidents from 2018 until 2020.

6 Interpretation and discussion

Accidents The results indicate that accidents in absolute terms did not change as a consequence of installing PUBLs. The only comparable paper to identify the effect of bike lanes on accidents by Li et al. (2017) found a contradictory result with a total increase of collisions of about 40% in the aftermath of new cycling ways in London. Due to the fact that Li et al. (2017) have data about the number of cyclists on the treated routes, they are also able to directly estimate an effect on the accident rate, which is not possible in my case. As a consequence of an increase of cyclists, the authors do not find a significant impact for this measure. With respect to PUBLs in general, Kraus and Koch (2021) found an increase of cycling in European cities after the installment of PUBLs of about 40% on average. The authors only looked at cycling in entire cities, not taking into account the type of streets specifically affected. If the increase of cyclists transfers to streets with PUBLs, that would mean that accident rates on PUBL streets in Berlin actually have decreased by about 40%. One potential reason for the difference between the London and the Berlin case may lie in the nature of the cycling lanes. While the lanes I consider are separated from car traffic by physical barriers, many bike lanes in London are merely indicated by blue paint on the streets. Taking all aspects into account, my results suggest that cycling actually has become safer for users of PUBLs. However, additional research on the matter is required due to the rather small sample size and thereby limited variation of the accidents data.

Traffic I find a significant reduction of average speed on PUBL streets, which means higher congestion levels. This increase in congestion seems to be primarily driven by the reduction of space available for cars rather than a significant change of total traffic on these streets. If anything, traffic on PUBL streets has slightly declined. In the economic literature, in many cases there is no differentiation between congestion and traffic volume since higher congestion levels in most instances are caused by an increase in traffic rather than a change in infrastructure. In theory, both, the increase in car volume as well as an increase in congestion, may lead to higher levels of local pollution. More cars mean more combustion engines to pollute the air, while higher congestion may increase pollution as a consequence of stop-and-go driving, which leads to higher fuel consumption or increased tire wear (Tu et al., 2022; Sommer et al., 2018). Since the results found in this paper hint toward increased congestion with slightly reduced traffic, conclusions about the effects on air pollution cannot be drawn, and the link found e.g. between congestion, traffic pollution, and infant mortality (Knittel et al., 2016; Currie and Walker, 2011) cannot be applied without further ado.

One unambiguous external cost factor of PUBLs, which is borne by car drivers, is the price paid in terms of increased travel times. Based on my estimation coefficients, a driver with an hypothetical one hour commute to work would need about one hour and five to six minutes after the installation of cycle lanes on the routes she uses. Given economic time costs, which are based on estimated values of travel time savings, of 9.37€ per hour in Germany (INRIX, 2021), this would lead to an increase of time costs up to approximately 10.3€ per one-way commute. This corresponds to a loss of about 2€ per day given that travel times from and to work do not differ. Assuming about 250 days of work a year, this would add up to costs of about 500€ for that specific driver. However, these back-on-the-envelope calculations are very hypothetical, since this would require all streets within the city to be equipped with new cycle lanes, which replace an existing car lane. The longest PUBL, which was installed in Berlin, had a length of about 3.5 kilometers (Kantstr.). If you needed five minutes to pass this specific street before the establishment of the cycle lane, then a 10-percent decrease in average car speed would lose you about 30 seconds, and then take 5.5 minutes. The economic costs in this specific case are therefore limited.

7 Conclusion

This paper is among the first to analyze causal effects of bike lanes on various outcomes, which determine some of the most important aspects of life-quality in cities. My source of exogenous variation are pop-up bike lanes in Berlin and I analyze their effect on congestion, traffic volume, and accidents. While the number of cars experienced modest but mostly insignificant declines, I find a significant reduction of average speed by between 8 and 12 percentage points. This effect reaches its maximum in peak travel hours with average speed being slower by 16 percentage points. Accidents were not affected by the installation of pop-up bike lanes. Due to the fact that I do not have street-specific data on the number of cyclists, I cannot eliminate the possibility of a decline of accidents per cyclist, which remains an open question for future research. Determining the effects of the newly installed cycle lanes on local air quality is also beyond the scope of this paper and remains an open question for future research. Overall, since economic costs of increased travel times are rather modest, the benefits are likely to outweigh them in the long run.

My findings have to be considered in the light of other consequences of the COVID-19 pandemic. Public transport has experienced a significant decline in trust and a decrease in ridership numbers (Vitrano, 2021). While some commuters might have replaced the tram or subway with their bikes, there also may exist the tendency to use the car instead. Street-specific data on bicycle as well as public transport utilization would allow for a thorough analysis of the change in the modal split. Moreover, the development of the modal shift in the long run, after the end of the COVID-19 pandemic and its limitations, is unclear. Further research should therefore tackle the question of long-term effects of replacing a car with a bike lane. On the basis of my research I furthermore conclude that the fundamental law of road congestion, which suggests a unitary elastic relationship between lane kilometers available and miles driven, does not necessarily apply in the short run. In the original paper by Duranton and Turner (2011), a reduction of vehicle lane kilometers does not relieve the streets sufficiently and as a consequence there is an elevated level of congestion over the course of several decades. My findings suggest that infrastructural changes do not directly lead to such changes but rather require a relatively long time span.

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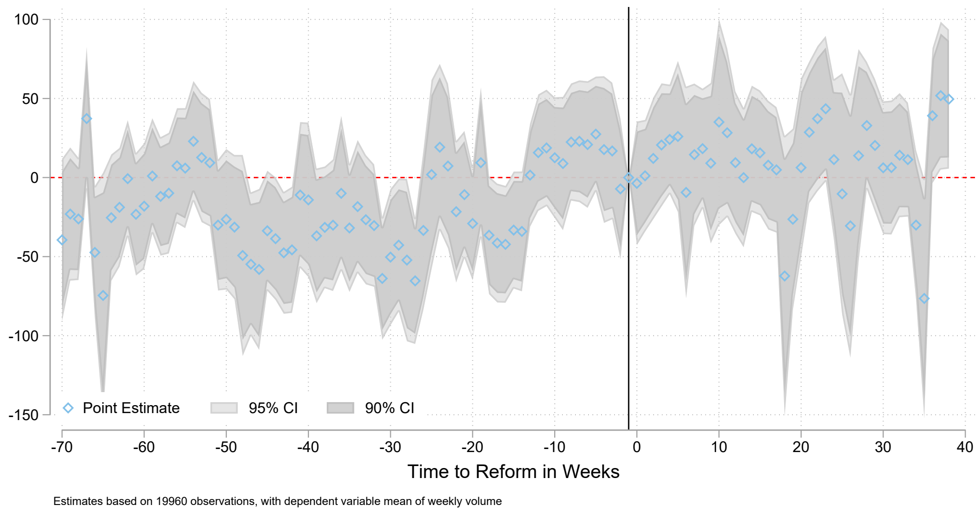
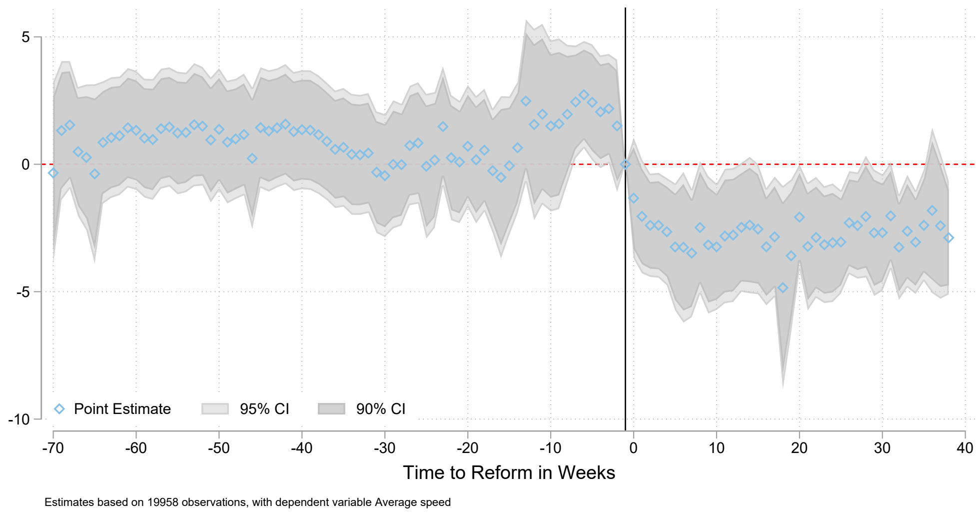
A Appendices

A.1 Appendix Figures



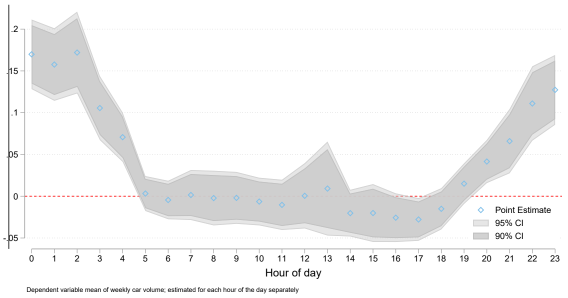
(a) Number of vehicles



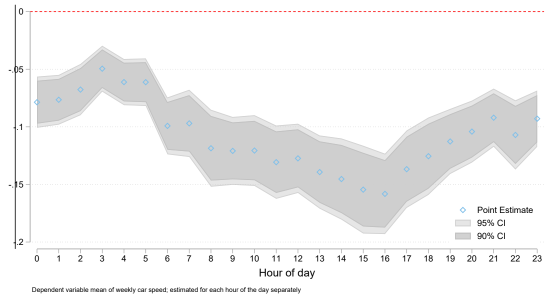
(b) Average speed

Figure A.1: Main results including an interaction between Station ID and Lock-down dummy

Note: The graphs show the results of separate event study estimations. The outcome variable in Figure A.1a is the absolute number of vehicles while it is average vehicle speed in Figure A.1b. Blue dots represent the main estimation coefficients of leads and lags. Confidence intervals are depicted as area shades in light (95% CI) and dark gray (90% CI). The vertical solid black line shows the time of treatment, which is anchored at 0. Leads and lags are the time before and after treatment in weeks. The estimations include station and week fixed effects as well as an interaction variable between each station and a post-lockdown dummy. The sample is restricted to streets with two or three lanes and all streets within a radius of 1km to a treated street are excluded from the estimations. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.



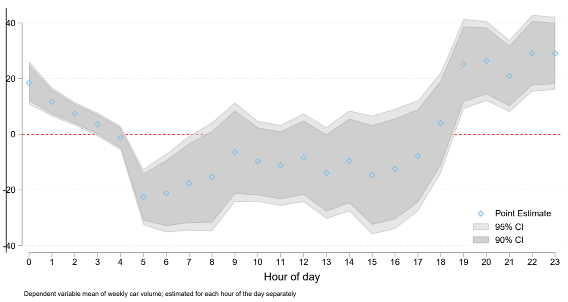
(a) Number of vehicles



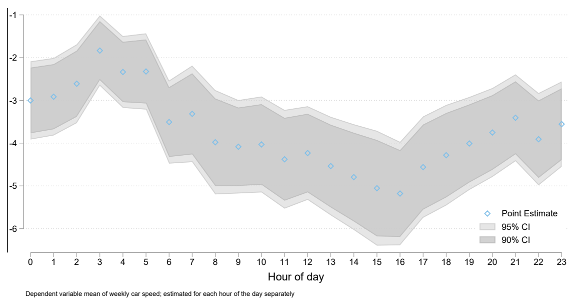
(b) Average speed

Figure A.2: Effects separated by hour of the day - logged outcomes

Note: The graphs show the results of separate two-way fixed effects estimations for each hour of the day. The outcome variable in Figure A.2a is the number of vehicles (in logs) while it is logged average vehicle speed in Figure A.2b. Blue dots represent the treatment effect of each estimation. Confidence intervals are depicted as area shades in light (95% CI) and dark gray (90% CI). The estimations include station fixed effects (FE), date FE, a construction dummy, a control for changes in speed regulations, and a station × post-lockdown interaction. The sample is restricted to streets with two or three lanes and all streets within a radius of 1km to a treated street are excluded from the estimations. Standard errors are clustered at 1km × 1km grid cell level spanning the city times a running week variable.



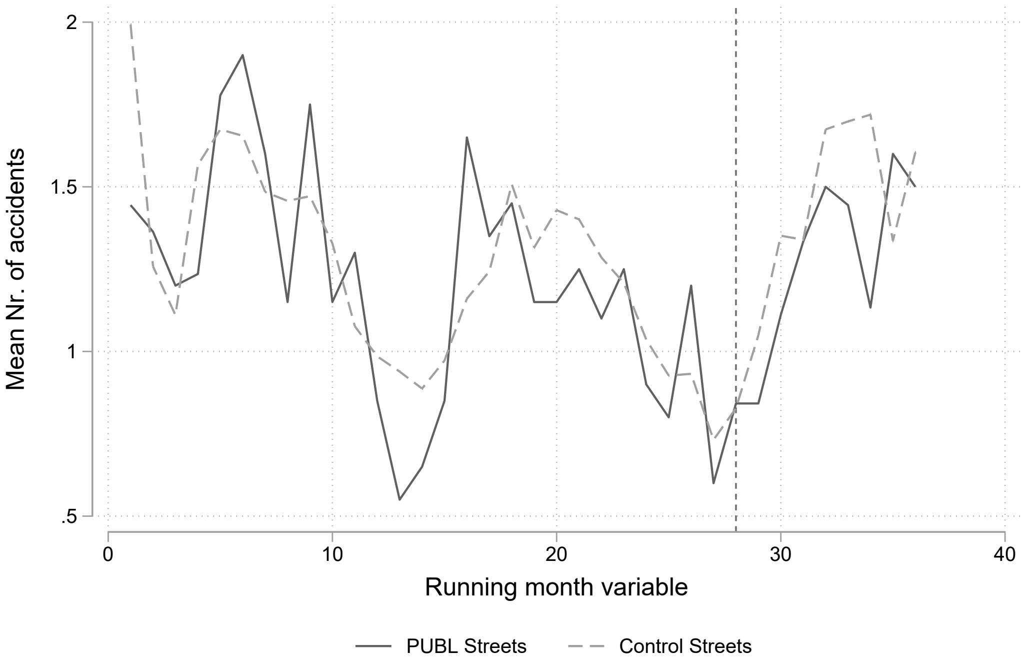
(a) Number of vehicles



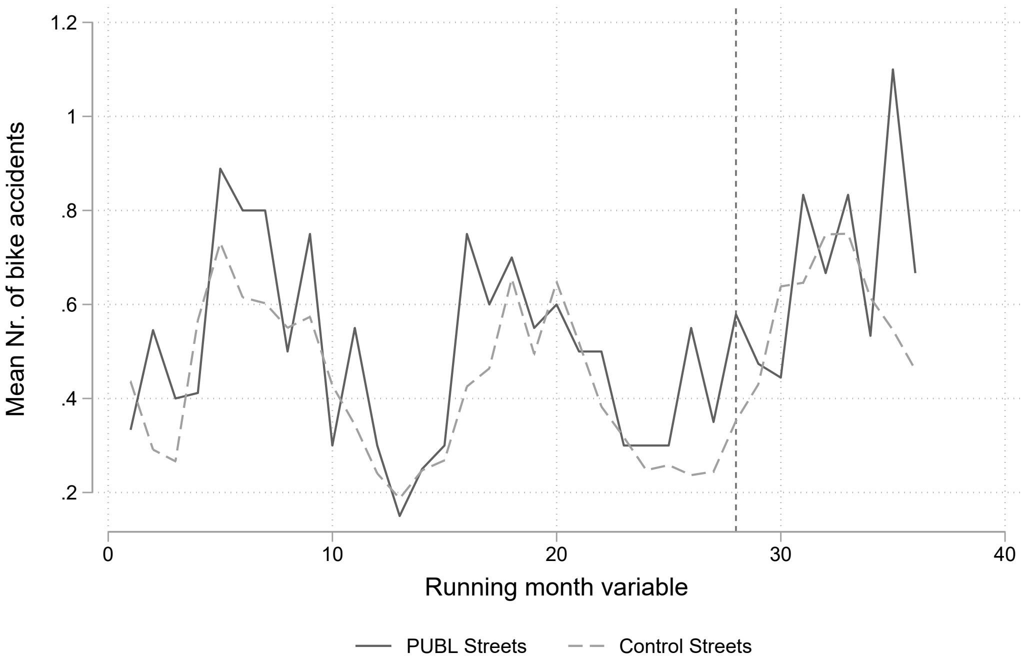
(b) Average speed

Figure A.3: Effects separated by hour of the day - absolute outcomes

Note: The graphs show the results of separate two-way fixed effects estimations for each hour of the day. The outcome variable in Figure A.3a is the number of absolute vehicles while it is average vehicle speed in Figure A.3b. Blue dots represent the treatment effect of each estimation. Confidence intervals are depicted as area shades in light (95% CI) and dark gray (90% CI). The estimations include station fixed effects (FE), date FE, a construction dummy, a control for changes in speed regulations, and a station × post-lockdown interaction. The sample is restricted to streets with two or three lanes and all streets within a radius of 1km to a treated street are excluded from the estimations. Standard errors are clustered at 1km × 1km grid cell level spanning the city times a running week variable.

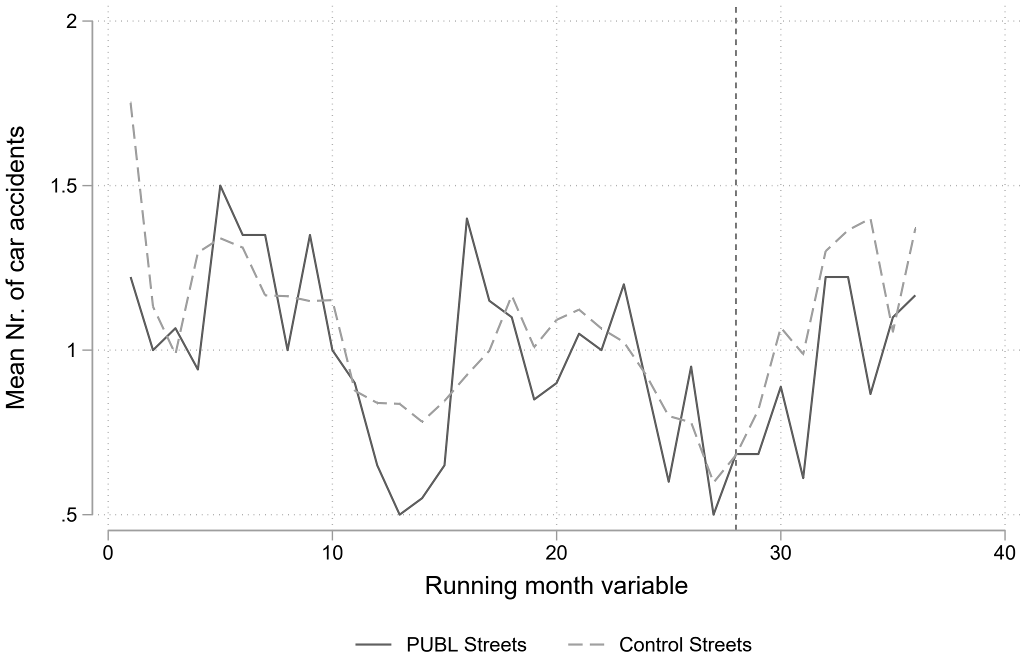


(a) Overall accidents



(b) Bike accidents

Figure A.4: Development of mean accidents by treatment status

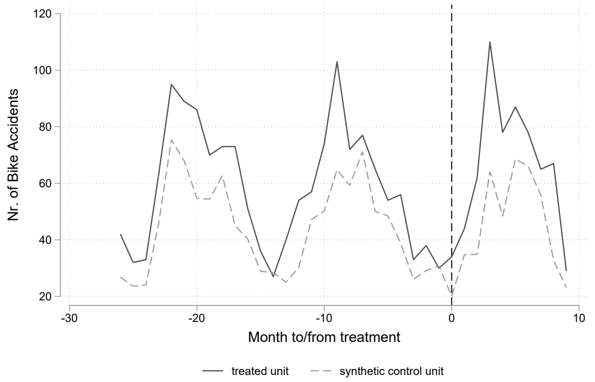


(c) Car accidents

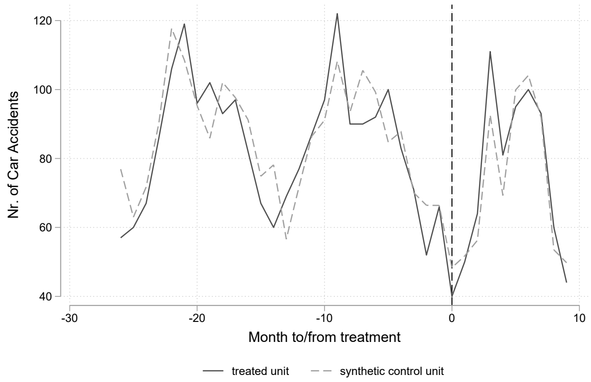
Figure A.4: Development of mean accidents by treatment status

Note: The graphs show the development of average street-level accidents separated by treatment status. They are presented by types of vehicles involved in the accidents, more precisely by overall accidents (A.4a), bike accidents (A.4b), and accidents with cars involved (A.4c). The vertical dashed line represents the timing of the installation of the first PUBL in the city.

Kreuzberg-Friedrichshain

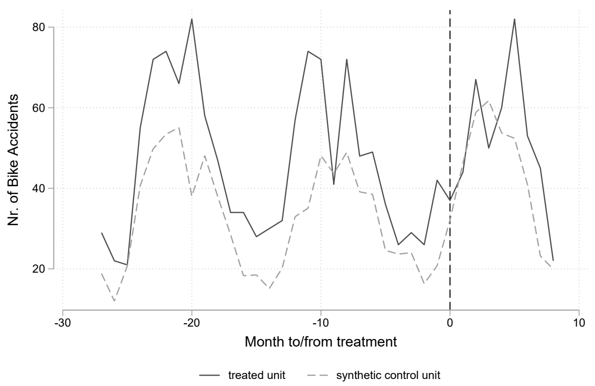


(a) Bike accidents

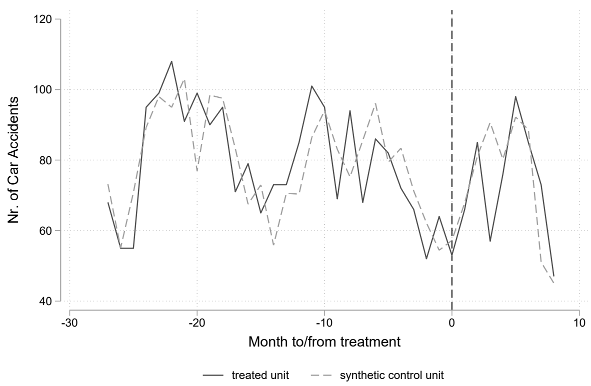


(b) Car accidents

Pankow

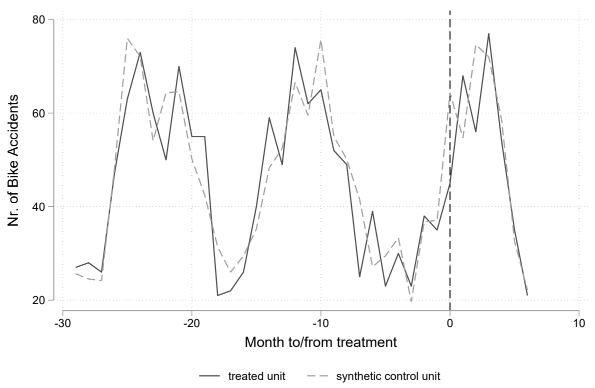


(c) Bike accidents

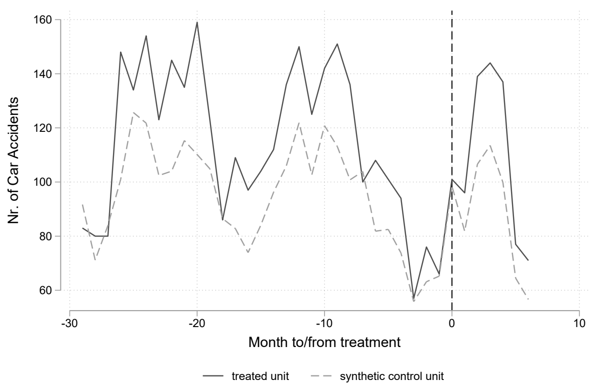


(d) Car accidents

Charlottenburg-Wilmersdorf



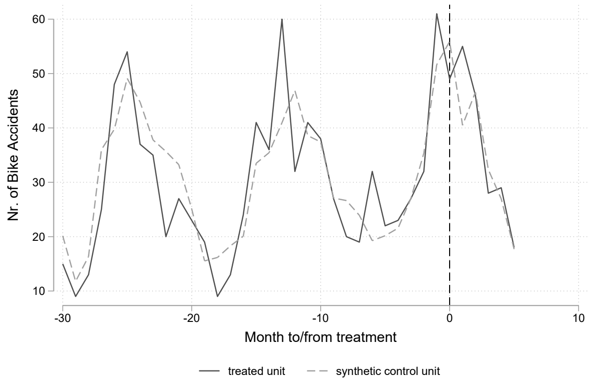
(e) Bike accidents



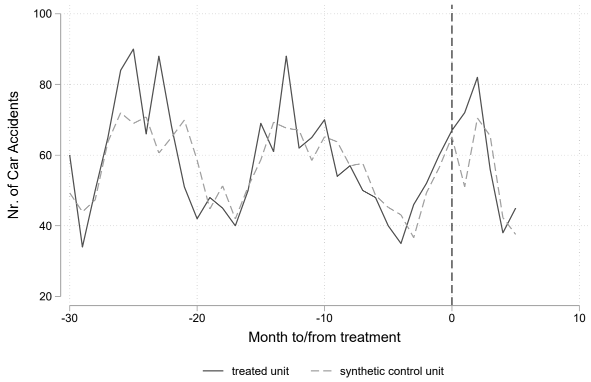
(f) Car accidents

Figure A.5: Effects on Bike and Car Accidents using synthetic control method

Treptow-Köpenick

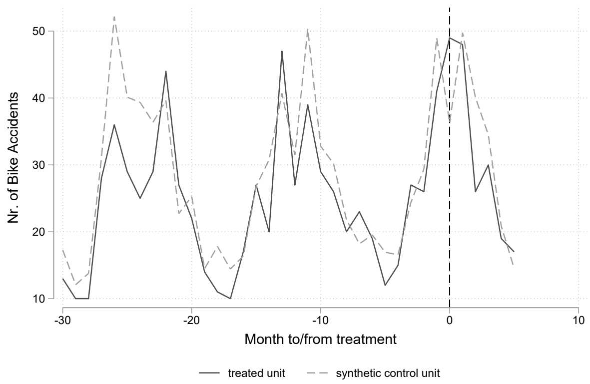


(g) Bike accidents

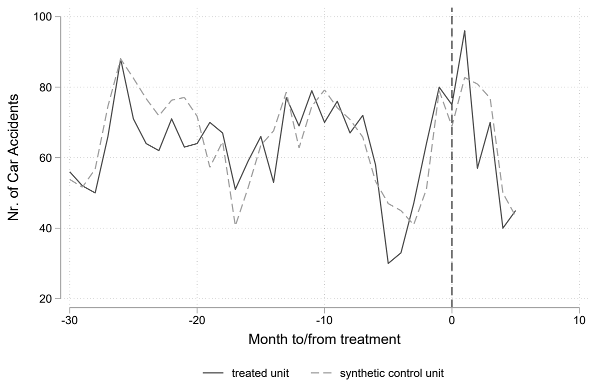


(h) Car accidents

Neukölln



(i) Bike accidents



(j) Car accidents

Figure A.5: Effects on Bike and Car Accidents using synthetic control method

Note: The graphs show the results of separate synthetic control group estimations following Abadie et al. (2010). Outcome variables are accidents with bicycles involved in the left panel and those with cars involved in the right panel (those are not mutually exclusive and may partly contain the same accidents). Treated units are the respective Berlin districts as mentioned in each sub-caption in which a PUBL was installed. The time of treatment is the month of the first placement of a PUBL within the respective district. The synthetic control unit consists of potentially all municipalities in Germany outside of Berlin that did not receive a PUBL. The matching between treatment and control units is based on the respective monthly outcome variable prior to treatment, traffic space, space used for settlements, population, election participation, the share of green party voters, the unemployment rate, and the absolute number of accidents from 2018 until 2020.

A.2 Appendix Tables

	Volume		Speed	
	(1)	(2)	(3)	(4)
Panel A: Outcome in absolute values				
1(PU lane)	-53.82***	0.550	-5.122***	-4.148***
	(5.354)	(7.547)	(0.311)	(0.538)
N	20196	20196	20194	20194
R^2	0.901	0.920	0.816	0.844
Stations	216	216	216	216
Interaction	No	Yes	No	Yes
Panel B: Log-transformed outcome				
1(PU lane)	-0.0292***	0.00788	-0.131***	-0.123***
	(0.00890)	(0.0135)	(0.00831)	(0.0139)
N	20194	20194	20194	20194
R^2	0.607	0.664	0.750	0.783
Stations	216	216	216	216
Interaction	No	Yes	No	Yes

Table A.1: TWFE: Effect on Volume and Speed in absolute and logged terms - Values aggregated to weekly levels

Note: The table presents the coefficients of the treatment effects of separate two-way fixed effects estimations with vehicle volume and vehicle speed as dependent variables and data aggregated to weekly levels. Panel A shows the coefficients of interest with outcomes in absolute terms. Panel B shows the same for logged outcome variables. Even columns include an interaction term between a unique measuring station identifier and a post-lockdown dummy variable while uneven columns do not. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, and an indicator for a change in speed limits. Stations within a one kilometer radius of a treated street are excluded and the sample is restricted to streets with two or three lanes. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1) Volume	(2) Speed
Station 1	-0.00500	-0.125***
	(-0.39)	(-8.93)
Station 2	-0.00510	-0.129***
	(-0.39)	(-8.69)
Station 3	-0.00874	-0.124***
	(-0.67)	(-8.15)
Station 4	-0.000635	-0.121***
	(-0.05)	(-8.10)
Station 5	-0.0183	-0.147***
	(-1.60)	(-10.52)
Station 6	-0.00569	-0.120***
	(-0.45)	(-8.23)
Station 7	-0.0114	-0.120***
	(-0.90)	(-8.02)
Station 8	-0.00390	-0.115***
	(-0.30)	(-7.45)
Station 9	-0.00203	-0.123***
	(-0.15)	(-7.86)
Station 10	-0.00577	-0.117***
	(-0.45)	(-7.86)
Station 11	0.00779	-0.133***
	(0.61)	(-8.72)
Station 12	-0.00140	-0.128***
	(-0.11)	(-9.05)
Station 13	-0.00574	-0.124***
	(-0.45)	(-8.34)
Station 14	-0.00648	-0.120***
	(-0.49)	(-7.85)
Station 15	-0.00590	-0.0863***
	(-0.45)	(-6.48)
Station 16	-0.00579	-0.123***
	(-0.46)	(-8.43)
Station 17	-0.00564	-0.122***
	(-0.45)	(-8.36)
Station 18	-0.00556	-0.123***
	(-0.44)	(-8.47)
Station 19	-0.00619	-0.123***
	(-0.49)	(-8.40)
Station 20	-0.00567	-0.119***
	(-0.45)	(-8.40)
Station 21	-0.00506	-0.118***
	(-0.40)	(-8.34)
Station 22	-0.00567	-0.123***
	(-0.45)	(-8.30)
Station 23	-0.00520	-0.123***
	(-0.41)	(-8.33)

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2: Results of Leave-one-out analyses by station

	All Stations				No outlier stations			
	Volume		Speed		Volume		Speed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln	abs	ln	abs	ln	abs	ln	abs
1(PU lane)	-0.00907	-7.060	-0.121***	-4.109***	-0.0117	-6.540	-0.0851***	-2.711***
	(0.0119)	(7.999)	(0.0145)	(0.540)	(0.0124)	(8.644)	(0.0134)	(0.463)
N	771418	772950	771404	771404	767695	769227	767681	767681
R^2	0.617	0.740	0.602	0.766	0.618	0.741	0.602	0.767
Stations treated	23	23	23	23	23	23	23	23
Stations Overall	215	215	215	215	214	214	214	214
Interaction	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.3: TWFE results at peak hours

Note: The table presents the coefficients of the treatment effects of separate two-way fixed effects estimations with vehicle volume and vehicle speed as dependent variables. Only peak traffic hours between 6 a.m. and 9 a.m. as well as between 4 p.m. and 7 p.m. are considered. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, an indicator for a change in speed limits, and an interaction term between a station identifier and a post-lockdown dummy. Columns 1-4 include all stations of the main sample, columns 5-8 exclude outlier stations as identified by leave-on-out analyses. Stations within a one kilometer radius of a treated street are excluded and the sample is restricted to streets with two or three lanes. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	Log outcomes		Abs. outcomes	
	(1)	(2)	(3)	(4)
	Volume	Speed	Volume	Speed
hour 0	0.170***	-0.0786***	18.50***	-3.001***
	(8.00)	(-6.93)	(4.71)	(-6.44)
hour 1	0.158***	-0.0765***	11.61***	-2.915***
	(7.11)	(-6.94)	(4.35)	(-6.30)
hour 2	0.172***	-0.0677***	7.347***	-2.610***
	(6.88)	(-5.96)	(3.45)	(-5.55)
hour 3	0.106***	-0.0495***	3.510	-1.835***
	(5.39)	(-4.86)	(1.62)	(-4.38)
hour 4	0.0707***	-0.0612***	-1.335	-2.336***
	(4.68)	(-5.98)	(-0.60)	(-5.45)
hour 5	0.00323	-0.0612***	-22.52***	-2.324***
	(0.30)	(-5.80)	(-4.38)	(-5.11)
hour 6	-0.00452	-0.0992***	-21.15**	-3.507***
	(-0.38)	(-7.86)	(-2.94)	(-7.07)
hour 7	0.00145	-0.0970***	-17.59*	-3.315***
	(0.10)	(-6.51)	(-2.03)	(-5.75)
hour 8	-0.00224	-0.119***	-15.32	-3.979***
	(-0.13)	(-6.95)	(-1.54)	(-6.39)
hour 9	-0.00199	-0.121***	-6.457	-4.084***
	(-0.13)	(-8.04)	(-0.71)	(-7.33)
hour 10	-0.00640	-0.121***	-9.718	-4.031***
	(-0.44)	(-7.69)	(-1.32)	(-7.04)
hour 11	-0.0103	-0.131***	-11.20	-4.378***
	(-0.67)	(-8.05)	(-1.51)	(-7.43)
hour 12	0.000502	-0.127***	-8.404	-4.232***
	(0.03)	(-8.31)	(-1.04)	(-7.58)
hour 13	0.00918	-0.139***	-13.91	-4.537***
	(0.32)	(-8.58)	(-1.65)	(-7.72)
hour 14	-0.0203	-0.145***	-9.566	-4.795***
	(-1.42)	(-8.08)	(-1.04)	(-7.60)
hour 15	-0.0201	-0.155***	-14.65	-5.054***
	(-1.14)	(-7.94)	(-1.35)	(-7.38)
hour 16	-0.0257	-0.158***	-12.44	-5.179***
	(-1.73)	(-8.88)	(-1.13)	(-8.39)
hour 17	-0.0278*	-0.137***	-7.773	-4.561***
	(-2.14)	(-7.97)	(-0.77)	(-7.54)
hour 18	-0.0151	-0.125***	4.003	-4.282***
	(-1.20)	(-7.32)	(0.43)	(-7.14)
hour 19	0.0150	-0.113***	25.17**	-4.008***
	(1.21)	(-7.82)	(3.05)	(-7.23)
hour 20	0.0415**	-0.104***	26.37***	-3.752***
	(3.15)	(-7.59)	(3.62)	(-7.07)
hour 21	0.0659***	-0.0921***	20.90**	-3.406***
	(3.36)	(-7.15)	(3.16)	(-6.57)
hour 22	0.111***	-0.107***	29.11***	-3.908***
	(4.89)	(-6.98)	(4.15)	(-7.09)
hour 23	0.127***	-0.0929***	29.04***	-3.555***
	(5.97)	(-7.45)	(4.35)	(-6.99)

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4: Results of hourly estimates

	Cars				Trucks			
	Volume		Speed		Volume		Speed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Outcome in absolute values								
1(PU lane)	-52.26***	-10.70	-4.715***	-4.290***	-1.818	3.497	0.337	0.262
	(5.131)	(7.457)	(0.313)	(0.545)	(1.785)	(2.723)	(0.233)	(0.350)
N	1546526	1546526	1543307	1543307	1546526	1546526	1536225	1536225
R^2	0.735	0.748	0.752	0.767	0.546	0.559	0.558	0.570
Stations	215	215	215	215	215	215	215	215
Interaction	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Log-transformed outcome								
1(PU lane)	-0.0219***	-0.0112	-0.116***	-0.124***	-0.0499**	0.0249	0.0154*	0.00170
	(0.00827)	(0.0144)	(0.00880)	(0.0144)	(0.0214)	(0.0320)	(0.00858)	(0.0119)
N	1543329	1543329	1543307	1543307	1536335	1536335	1532624	1532624
R^2	0.622	0.643	0.579	0.601	0.601	0.618	0.489	0.500
Stations	215	215	215	215	215	215	215	215
Interaction	No	Yes	No	Yes	No	Yes	No	Yes

Table A.5: TWFE effects for cars and trucks

Note: The table presents the coefficients of the treatment effects of two-way fixed effects estimations for cars and trucks separately. Panel A shows the coefficients of interest with outcomes in absolute terms. Panel B shows the same for logged outcome variables. Even columns include an interaction term between a unique measuring station identifier and a post-lockdown dummy variable while uneven columns do not. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, and an indicator for a change in speed limits. Stations within a one kilometer radius of a treated street are excluded and the sample is restricted to streets with two or three lanes. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	Only 2 lanes		Only 3 lanes	
	(1) Volume	(2) Speed	(3) Volume	(4) Speed
1(PU lane)	0.00195	-0.132***	-0.0253	-0.0951***
	(0.0156)	(0.0200)	(0.0176)	(0.0156)
N	1282105	1282065	261388	261388
R^2	0.623	0.614	0.699	0.537
Stations treated	16	16	7	7
Stations Overall	180	180	35	35
Interaction	Yes	Yes	Yes	Yes

Table A.6: TWFE results with different lane samples

Note: The table presents the coefficients of the treatment effects of two-way fixed effects estimations on vehicle volume and speed. Columns 1 and 2 only include two-lane streets in the sample. Columns 3 and 4 only include three-lane streets. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, an indicator for a change in speed limits, and an interaction term between a station identifier and a post-lockdown dummy. Stations within a one kilometer radius of a treated street are excluded. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	750m		1km		1.5km	
	(1)	(2)	(3)	(4)	(5)	(6)
	Volume	Speed	Volume	Speed	Volume	Speed
1(PU lane)	0.0809***	0.00526	0.0946***	-0.00740	0.0177	-0.00779
	(0.0249)	(0.0109)	(0.0202)	(0.00809)	(0.0161)	(0.00759)
N	2416577	2416351	2416577	2416351	2416577	2416351
R^2	0.798	0.621	0.798	0.621	0.798	0.621
Stations treated	54	54	84	84	129	129
Stations Overall	343	343	343	343	343	343
Interaction	Yes	Yes	Yes	Yes	Yes	Yes

Table A.7: TWFE results for surrounding stations

Note: The table presents the coefficients of the treatment effects of two-way fixed effects estimations on vehicle volume and speed for streets surrounding the actually treated ones. Streets within a radius of 750m, 1km, and 1.5km respectively are now considered as treated units. Streets that actually received a PUBL are excluded from the estimations. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, an indicator for a change in speed limits, and an interaction term between a station identifier and a post-lockdown dummy. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	All Stations		No outlier stations	
	(1)	(2)	(3)	(4)
	Volume	Speed	Volume	Speed
1(PU lane)	-0.0268*	-0.130***	-0.0363**	-0.0932***
	(0.0140)	(0.0142)	(0.0158)	(0.0145)
N	1690820	1690780	1673979	1673939
R^2	0.647	0.578	0.647	0.577
Stations	215	215	213	213
Interaction	Yes	Yes	Yes	Yes

Table A.8: TWFE long-term results

Note: The table presents the coefficients of the treatment effects of separate two-way fixed effects estimations on vehicle volume and speed for a sample including the months from March until May of 2021. Columns 1 and 2 include all stations of the main sample, columns 3 and 4 exclude outlier stations as identified by leave-on-out analyses. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, an indicator for a change in speed limits, and an interaction term between a station identifier and a post-lockdown dummy. Stations within a one kilometer radius of a treated street are excluded and the sample is restricted to streets with two or three lanes. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	Station*week		Districts*week		Grids		Week	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Outcomes with all stations								
1(PU lane)	-0.00540	-0.122***	-0.00540	-0.122***	-0.00540	-0.122**	-0.00540	-0.122***
	(0.0115)	(0.0131)	(0.0138)	(0.0142)	(0.0301)	(0.0469)	(0.0103)	(0.00791)
N	1543493	1543453	1543493	1543453	1543493	1543453	1543493	1543453
R^2	0.650	0.607	0.650	0.607	0.650	0.607	0.650	0.607
Stations treated	23	23	23	23	23	23	23	23
Stations Overall	215	215	215	215	215	215	215	215
Interaction	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome	Volume	Speed	Volume	Speed	Volume	Speed	Volume	Speed
Panel B: Outcomes w/o outliers								
1(PU lane)	-0.00590	-0.0863***	-0.00590	-0.0863***	-0.00590	-0.0863**	-0.00590	-0.0863***
	(0.0122)	(0.0117)	(0.0141)	(0.0108)	(0.0314)	(0.0346)	(0.0107)	(0.00713)
N	1536042	1536002	1536042	1536002	1536042	1536002	1536042	1536002
R^2	0.650	0.607	0.650	0.607	0.650	0.607	0.650	0.607
Stations treated	23	23	23	23	23	23	23	23
Stations Overall	214	214	214	214	214	214	214	214
Interaction	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome	Volume	Speed	Volume	Speed	Volume	Speed	Volume	Speed

Table A.9: TWFE results with different standard error clusters

Note: The table presents the coefficients of the treatment effects of separate two-way fixed effects estimations with varying clusters of standard errors as described in the column headers. Panel A shows the coefficients of interest with all stations from the main sample. Panel B shows outcomes with outlier stations as identified by leave-on-out analyses being excluded from the sample. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, an indicator for a change in speed limits, and an interaction term between a station identifier and a post-lockdown dummy. Stations within a one kilometer radius of a treated street are excluded and the sample is restricted to streets with two or three lanes.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	Full sample & hours		Full sample		Cycling Infr.		Highways	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Volume	Speed	Volume	Speed	Volume	Speed	Volume	Speed
1(PU lane)	0.0206	-0.102***	-0.0303**	-0.116***	-0.0107	-0.145***	0.0251**	-0.0935***
	(0.0139)	(0.0126)	(0.0143)	(0.0144)	(0.0135)	(0.0151)	(0.0123)	(0.0141)
N	2980500	2980207	2002402	2002210	718615	718609	2962999	2962168
R^2	0.849	0.621	0.786	0.630	0.678	0.720	0.847	0.715
Stations treated	24	24	24	24	19	19	23	23
Stations Overall	283	283	283	283	102	102	450	450
Interaction	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.10: TWFE results with different samples

Note: The table presents the coefficients of the treatment effects of separate two-way fixed effects estimations with varying sample compositions. Columns 1 & 2 show results with all 24 hours of the day and the full sample including one and four-lane streets except for stations within a 1km radius of treated streets. Columns 3 & 4 make the same restrictions, but now only with times between 5 a.m. and 8 p.m. In columns 5 & 6 the sample is restricted to streets without bike lanes prior to treatment. Outcomes in columns 7 & 8 include observations from highways. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, an indicator for a change in speed limits, and an interaction term between a station identifier and a post-lockdown dummy. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	Jan2020		Aug2019		Apr2019	
	(1)	(2)	(3)	(4)	(5)	(6)
	Volume	Speed	Volume	Speed	Volume	Speed
1(PU lane)	-0.00917	-0.0106	-0.00963	-0.00731	-0.0180**	0.00867
	(0.00817)	(0.00654)	(0.00708)	(0.00525)	(0.00724)	(0.00640)
N	948552	948543	948552	948543	948552	948543
R^2	0.650	0.606	0.650	0.606	0.650	0.606
Stations treated	23	23	23	23	23	23
Stations Overall	215	215	215	215	215	215

Table A.11: TWFE Placebo tests (treatment starts in respective month)

Note: The table presents the coefficients of the treatment effects of separate two-way fixed effects estimations with treatment being simulated at different points in time. All estimations include station fixed effects (FE), date FE, hour FE, a dummy whether construction takes place, and an indicator for a change in speed limits. Stations within a one kilometer radius of a treated street are excluded and the sample is restricted to streets with two or three lanes. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	Volume		Speed	
	(1)	(2)	(3)	(4)
1(PU lane)	-0.00540	-0.00592	-0.122***	-0.122***
	(0.0126)	(0.0128)	(0.0144)	(0.0144)
Construction dummy	-0.0445***		-0.0487***	
	(0.00652)		(0.00480)	
Construction		-0.00722***		-0.00810***
		(0.00152)		(0.000942)
N	1543493	1543493	1543453	1543453
R^2	0.650	0.650	0.607	0.604
Stations	215	215	215	215
Interaction	Yes	Yes	Yes	Yes

Table A.12: TWFE results construction control

Note: The table compares the coefficients of the treatment effects of separate two-way fixed effects estimations with variations of the construction control. Uneven columns include a construction dummy independent from type of construction. Even columns show results with a construction variable, which explicitly controls for type of construction. All estimations include station fixed effects (FE), date FE, hour FE, an indicator for a change in speed limits, and an interaction term between a station identifier and a post-lockdown dummy. Stations within a one kilometer radius of a treated street are excluded and the sample is restricted to streets with two or three lanes. Standard errors are clustered at 1km $\times$ 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	Volume		Speed	
	(1)	(2)	(3)	(4)
1(PU lane)	-0.0378***	-0.00818	-0.127***	-0.117***
	(0.00808)	(0.0274)	(0.00991)	(0.0400)
N	1328753	1328753	1328739	1328739
R^2	0.626	0.652	0.588	0.613
Stations treated	23	23	23	23
Stations Overall	215	215	215	215
Interaction	No	Yes	No	Yes

Table A.13: TWFE results without heterogeneous timing

Note: The table presents the coefficients of the treatment effects of separate two-way fixed effects estimations with homogeneous treatment timing. Therefore, all observations between the first and the last installation date of a PUBL are deleted from the sample. Even columns include an interaction term between a unique measuring station identifier and a post-lockdown dummy variable while uneven columns do not. All estimations include station fixed effects (FE), date FE, hour FE, and an indicator for a change in speed limits. Stations within a one kilometer radius of a treated street are excluded and the sample is restricted to streets with two or three lanes. Standard errors are clustered at 1km × 1km grid cell level spanning the city times a running week variable.
t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	Treated	Synthetic
Traffic space	.2637255	.1923878
Space settlements	.6705883	.6443015
Population	290083	290159.5
Election participation	.773	.743195
Green Party Voters	21.28861	16.09941
Unemp. rate	9.7	9.403
Accidents 2018	1666	1693.033
Accidents 2019	1675	1709.326
Accidents 2020	1408	1405.261

Table A.14: Friedrichshain-Kreuzberg

Note: The table shows the predictor balance between treated unit and synthetic control group of the synthetic control group method with Friedrichshain-Kreuzberg as treated district.

	Treated	Synthetic
Traffic space	.1331137	.1409323
Space settlements	.5376865	.5178513
Population	409454	409117
Election participation	.793	.80261
Green Party Voters	14.6013	13.7961
Unemp. rate	9.7	8.8952
Accidents 2018	1578	1591.279
Accidents 2019	1552	1518.664
Accidents 2020	1350	1409.146

Table A.15: Pankow

Note: The table shows the predictor balance between treated unit and synthetic control group of the synthetic control group method with Pankow as treated district.

	Treated	Synthetic
Traffic space	.1941568	.1835291
Space settlements	.5084248	.5557473
Population	342950	342976.5
Election participation	.786	.750991
Green Party Voters	15.35358	14.92943
Unemp. rate	9.7	9.3712
Accidents 2018	2013	2018.721
Accidents 2019	2048	2027.964
Accidents 2020	1644	1660.739

Table A.16: Charlottenburg-Wilmersdorf

Note: The table shows the predictor balance between treated unit and synthetic control group of the synthetic control group method with Charlottenburg-Wilmersdorf as treated district.

	Treated	Synthetic
Traffic space	.0966434	.1098114
Space settlements	.3503845	.3659922
Population	273817	273322
Election participation	.766	.727015
Green Party Voters	7.744833	8.189585
Unemp. rate	9.7	9.2366
Accidents 2018	1115	1155.677
Accidents 2019	1119	1140.264
Accidents 2020	1128	1092.36

Table A.17: Treptow-Köpenick

Note: The table shows the predictor balance between treated unit and synthetic control group of the synthetic control group method with Treptow-Köpenick as treated district.

	Treated	Synthetic
Traffic space	.1684843	.1846853
Space settlements	.8032495	.7053794
Population	328666	327763.5
Election participation	.708	.757145
Green Party Voters	12.85852	11.76067
Unemp. rate	9.7	9.6853
Accidents 2018	1230	1266.639
Accidents 2019	1289	1287.227
Accidents 2020	1116	1121.362

Table A.18: Neukölln

Note: The table shows the predictor balance between treated unit and synthetic control group of the synthetic control group method with Neukölln as treated district.