# Provisional COVID-19 infrastructure induces large, rapid increases in cycling

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**The bicycle is a low-cost means of transport linked to low risk of transmission of infectious disease. During the COVID-19 crisis, governments have therefore incentivized cycling by provisionally redistributing street space. We evaluate the impact of this new bicycle infrastructure on cycling traffic using a generalized difference in differences design. We scrape daily bicycle counts from 736 bicycle counters in 106 European cities. We combine these with data on announced and completed pop-up bike lane road work projects. Within 4 mo, an average of 11.5 km of provisional pop-up bike lanes have been built per city and the policy has increased cycling between 11 and 48% on average. We calculate that the new infrastructure will generate between $1 and $7 billion in health benefits per year if cycling habits are sticky.**

urban planning | active travel | generalized difference in differences

The COVID-19 crisis has led to important changes in transport behavior in 2020 (1). Early evidence points to shifts from public transport to car use as users have reacted to the pandemic (2). Governments have incentivized cycling as a low-cost, sustainable, equitable, and space-saving mode of transport that reduces the risk of COVID-19 transmission. A key measure has been the redistribution of street space in cities to create provisional bike infrastructure typically marked and protected by materials readily available from road construction companies. As of July 8, 2020, 2,000 km of these infrastructure changes had been announced in European cities (3).

Transport mode choices are influenced by a variety of behavioral effects that make people stick to their habits, such as status quo bias, default effects, and time-inconsistent preferences (4). This complicates the task of policymakers to encourage people to cycle, particularly in the short run. However, major disruptions to public transport, such as strikes, cause people to reconsider their habits (5) and the provision of dedicated infrastructure has been identified as an important means to increase cycling (6). Thus, the fast provision of new bike infrastructure during the COVID-19 pandemic is a suitable policy experiment to investigate the responsiveness of cycling under conducive conditions.

Here, we estimate the causal effect of the post-COVID-19 lockdown rollout of provisional ("pop-up") bike lanes in European cities. We compile new data on daily bike counts in 106 cities. We connect to the open data application programming interfaces (APIs) of these cities to download bike counts from a total of 736 counters. We combine these data with information on day-to-day kilometer changes in pop-up cycling infrastructure (Fig. 1).

The spatial placement of pop-up bike lanes has mainly been driven by the availability of street space that could be redistributed without restricting car traffic to one direction and the existence of "shovel-ready" construction plans. The exact timing of pop-up bike lane construction is driven by administrative idiosyncrasies and the availability and schedules of construction firms. Therefore, the timing of the pop-up bike lane rollout has been as good as random. This quasi-experimental setting allows us to address the important concerns that bike lanes could be built as a reaction to increased cycling traffic (reverse causality) or that both the implementation of bike lanes and bicycle counts could be driven by a third factor, such as local "green" preferences, that cannot be measured (omitted variable bias).

## Results

We use panel regressions to compare bike traffic in treated cities before and after they get treated with control cities. We find that pop-up bike lanes have led to substantial increases in cycling. This effect is robustly visible in comparisons over both a longer and a shorter time span. First, in Fig. 2 we show the effect comparing treated and control cities over several months before and after treatment. Second, in Fig. 3 we provide estimates from a range of more conservative specifications identifying the effect based on daily variation within a narrow time window in the same city.

The outcome in all our regressions is modeled as the natural logarithm of the cycling count. We use daily variation in this variable either at the counter or at the city level. Our coefficients can be interpreted as the average change in cycling caused by the pop-up bike lane program.

**Standard Difference in Differences.** Fig. 2 shows the dynamic treatment effect of the pop-up bike lane program. For the analysis shown here, we define March 2020 as the time of treatment and plot the estimated differences between treated and control cities over time. Since we expect cycling to increase in both treated and

### Significance

**Active travel makes people healthier and creates a wide range of additional social and environmental benefits. The provision of dedicated infrastructure is considered a crucial policy to increase cycling. However, evaluating the impact of this type of intervention is difficult because infrastructure changes are typically slow. The rollout of so-called pop-up bike lanes during the COVID-19 pandemic is a unique empirical context to estimate the pull effect of new cycling infrastructure. We show that the policy has worked. We find large increases in cycling. This result is robust for a variety of empirical counterfactuals. Further research is needed to investigate whether this change is persistent and whether similar results can be achieved in situations outside the context of a pandemic.**

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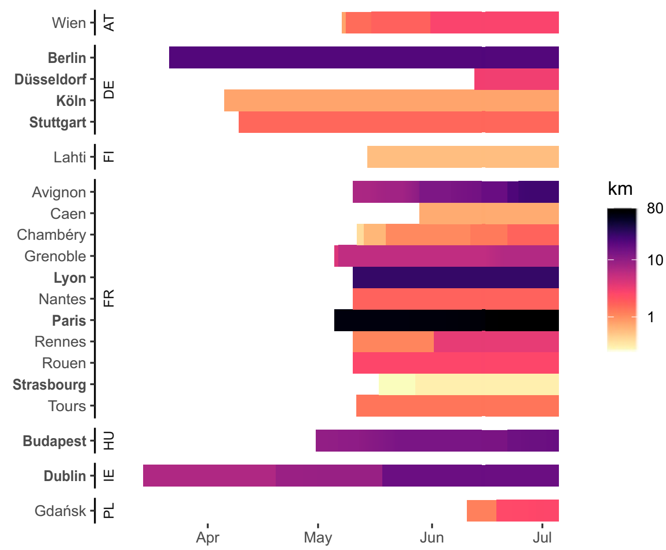


**Fig. 1.** Treated cities and their treatment intensities in terms of implemented kilometers of public bike lanes in service (cumulative) on a given day between March and July 2020. Cities used in the estimation sample for Fig. 3 are marked in boldface type. Control cities are plotted in *SI Appendix*, Fig. S2. London, Milan, Rome, and Lisbon are missing from the sample due to a lack of daily bicycle counter data. Data are from the European Cyclists' Federation (3).

control cities as a reaction to COVID-19, we take the difference between the cycling increase in treated and in control cities as our estimate of the average effect of the program. This difference in differences approach suggests an increase in cycling of 41.6% induced on average by the policy. A crucial assumption for this research design is that cycling would have evolved on a parallel trend in the treatment and control group in the absence of treatment. This is called the common trends assumption. Since we model the outcome as the natural logarithm of cycling counts, we make the assumption that cycling would have grown at the same rate in the treatment and in the control group.

Fig. 2 allows us to verify this assumption. The treatment effect becomes apparent after the treatment sets in. Before, treatment and control groups have been on the same trend. There is a slight, albeit statistically insignificant downward trend before treatment, hinting at the possibility of stronger mobility reductions due to COVID-19 in cities that have decided to build pop-up bike lanes. This could for instance be the case because local and national governments are more likely to take wide-ranging action if their country is hit by a more intense outbreak. It could also be due to governments acting upon stronger risk aversion of their local populations toward cycling in the context of emptier roads and increased speeding during the lockdown. We mitigate some of these potential selection into treatment effects by controlling for COVID-19–related dynamics with fixed effects at the country–day level. This removes the effect of daily national-level policy changes, such as lockdowns.

A remaining concern is that bike lanes could have been built as a reaction to locally increased cycling traffic (reverse causality) or that both the implementation of bike lanes and bicycle counts could be driven by an unaccounted third factor (omitted variable bias). We address these potential biases with regressions focusing on changes over a shorter time span as discussed in the next section.

**Generalized Difference in Differences.** In our second set of specifications (Fig. 3) we investigate more focused comparisons using both variation in the timing of treatment between cities and variation in the treatment dose, i.e., the number of kilometers of pop-up bike lane in service on a given day. With these specifications we robustify the more simple difference in differences design by using additional fixed effects and by including control variables for the weather, for changes in overall mobility and public transport, and for the number of active bike counters in a city. Crucially, we look at the effects of pop-up bike lanes in a shorter time span to investigate potential reverse causality between cycling and the implementation of pop-up bike lanes. Although pop-up bike lanes tend to be based on preexisting plans by city planners or civil society organizations and could therefore be implemented comparatively quickly, the erection of a bike lane needs at least a few days' notice and the exact timing of these road works depends on the availability and the schedule of construction firms. This has been confirmed in our conversations with local policymakers in Berlin and Paris. Our preferred specifications (Eq. **1**) are therefore based on comparisons of cycling counts on the days before and after a change in the treatment intensity (marked in blue). These comparisons are created by the

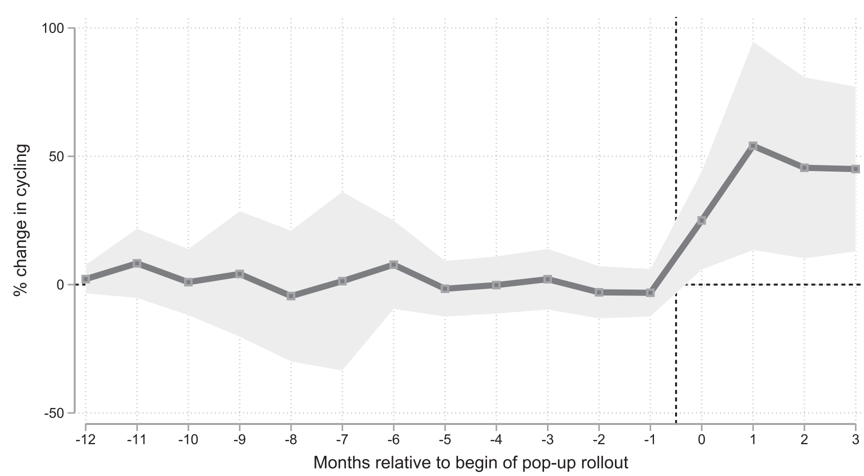


**Fig. 2.** Treatment effect (difference between treated and control cities) in months before and after the beginning of the pop-up bike lane policy. Observations are binned into months. Treatment for this plot is hard coded to March 2020 and the baseline category and the beginning of the sample are set to February 2019. Estimates are from Poisson regressions that include city and country–day fixed effects (*SI Appendix*, Eq. **S1**). The shaded area shows the 95% confidence interval. Data for the outcome variable are from the European Cyclists' Federation (3) and data for the treatment variable are from municipal bike counters (*Materials and Methods*).

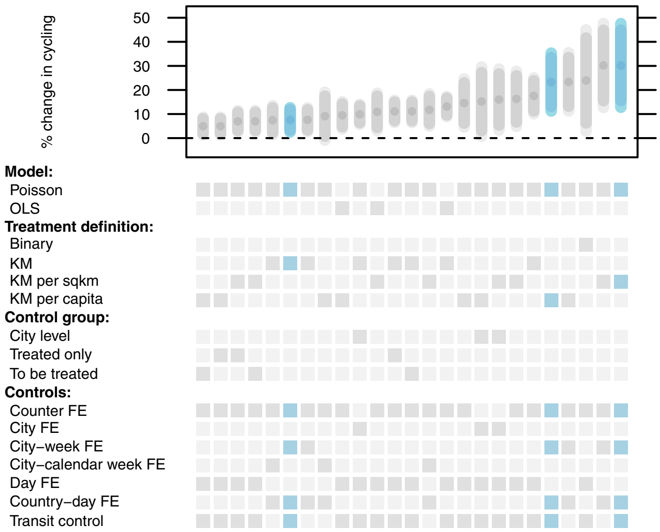


**Fig. 3.** Estimates of the average effect of pop-up bike lanes on cycling. Dose–response regressions (in kilometers, kilometers per capita, or kilometers per square kilometer in service on a given day) are multiplied by the average treatment dose. The 90 and 95% confidence intervals are shown in darker and lighter color. The unit of observation is the bike counter except for regressions at the city level. Preferred specifications are marked in blue (Eq. **1**) and are reported in more detail in *SI Appendix*, Table S5. Gray (and blue) indicators (*Bottom*) indicate the type of specification. Three estimates are from OLS specifications and therefore use the natural logarithm of the bicycle count as the outcome. All other specifications are Poisson regressions using the level of the count. Data for the outcome are from the European Cyclists' Federation (3) and data for the treatment are from municipal bike counters (*Materials and Methods*). All regressions include controls for the number of active counters in a city on a given day and for the weather (temperature, sunshine, wind, precipitation) (7). All regressions, except those that rely on observations before 2020, include a control for overall mobility (8). The transit control is from Apple routing requests (2020 only) (1). Code is from ref. 9.

inclusion of city–week fixed effect. This fixed effect ensures that our estimates are based on variation within the same city within the same week. If the exact (i.e., day-level) timing of the rollout of pop-up bike lanes has been as good as random, estimates from these specification are not driven by reverse causality.

Our unit of observation in most regressions is the cycling counter. This allows us to control for within-city differences despite doing a cross-city study. We do this by including a counter fixed effect that flexibly controls for any local confounders that are time invariant within the time frame of the variation used in the analysis. We thereby control for the density of public transport stops, population density, and topography, but also for additional, unobservable dimensions, such as social capital and local preferences for green lifestyles, at a high spatial resolution within the city. With the counter fixed effect we also rule out that our result is driven by new counters that get placed next to pop-up bike lanes. We assign treatment to each counter based on its city, since we measure daily changes in the pop-up bike lane network at that level. We investigate the effect of this source of measurement error by defining the treatment dose either as a binary variable or in terms of kilometers, kilometers per capita, and kilometers per square kilometer of city area. We find that measuring the dose–response in terms of kilometers attenuates the effect (7.6%). This indicates that the effect is not exclusively driven by the announcement effect of new infrastructure in a city, but by the de facto availability of new infrastructure in the neighborhood surrounding a counter, which is better approximated by a measure in per capita or per area terms (estimates of 23.3 and 30.2%, respectively). Remaining measurement error due to some counters being closer to or farther from new infrastructure than the rest of the sample is unlikely to be systematic conditional on fixed effects and control variables (detailed discussion of measurement error in *SI Appendix*). We also run specifications for which we take the mean of all counters in a city (marked as city level in Fig. 3) to show that the effect is not driven by our use of the counter as the unit of observation.

We use a variable capturing transit routing searches on Apple maps (1) to control for omitted variable bias that could be present if changes in public transport affect both pop-up bike lane construction and cycling. In our preferred specification this could still be the case, if daily changes in the provision or in the use of public transport in a city led to new pop-up infrastructure within the same week. Public officials may for instance have tried to schedule the erection of pop-up bike lanes for the same day as planned public transport disruptions. The transit control removes this potential remaining bias. Since the Apple data are available only for a subset of larger cities in our sample (marked in boldface type in Fig. 1), we run our main regressions (Fig. 3) on this smaller sample. *SI Appendix*, Table S4 shows robustness to lifting this sample restriction and to excluding Paris, which has had the strongest treatment, from the analysis.

We control for subnational changes in policies and behaviors related to COVID-19 with a variable that captures overall human mobility based on Facebook user movements. We control for the number of counters active in a city on a given day to account for unusual traffic situations, for instance when a counter gets shut down because of road works. We also include control variables for daily total precipitation and mean wind, temperature, and sunshine to address the concern that both the scheduling of pop-up bike lane construction work and daily variation in cycling could have been driven by weather conditions.

We check the sensitivity of our results to changing the time span of our identifying variation and to reshaping our treatment and control group definitions (additional specifications in Fig. 3). The effect is robust to including days from the same calendar week in previous years in these comparisons rather than days from 2020 only. We also provide estimates for the effects of the policy based on comparisons between 1) treated and untreated cities, 2) treated cities using only their variation in treatment timing, 3) cities that are already treated and those that have announced only pop-up bike lanes, and 4) treated cities only using their variation in treatment dose and treatment timing.

**Heterogeneity Analysis.** We investigate how the treatment effect of pop-up bike lanes varies depending on relevant features of the cities in our sample (Table 1). These heterogeneous effects should not be interpreted causally, since we cannot control for additional omitted variable bias or reverse causality created by the inclusion of these variables in our model. We find that the effect of pop-up bike lanes is stronger in cities with a higher population density [1] and a higher modal share of public transport in commutes [2], which are correlates of a built environment favoring active travel. The treatment effect is lower for cities with faster average speeds of car commutes [6] and for cities with more road death per capita [7]. It is also lower for cities with more cars per capita [5]. However, this estimate is imprecise. These heterogeneities confirm research that found that US cities with better safety, low car ownership, and more density have more cycling (10, 11).

Our analysis also shows that the baseline length of the bike lane network per capita [3] is correlated with a lower treatment effect. We interpret this as an indication that the pop-up bike lane effect is a phenomenon of catch-up growth in cities with a high cycling potential that was previously impeded by missing infrastructure. The effect of baseline cycling modal shares [4] is, however, statistically unclear.

Further research could also look at the effect of pop-up bike lanes in terms of improvements in bike lane network connectivity

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**Table 1. Heterogeneous treatment effects of the pop-up bike lane rollout**

| | × baseline (natural log) of | | | | | | |
|---|---|---|---|---|---|---|---|
| | [1] Population density | [2] PT modal share | [3] Bike lanes per capita | [4] Cycling modal share | [5] Cars per capita | [6] Car commute speeds | [7] Road deaths per capita |
| Pop-up treatment | 0.221* | 0.258*** | −0.194* | 0.093 | −0.592 | −0.509** | −0.351*** |
| | (0.121) | (0.100) | (0.115) | (0.082) | (0.485) | (0.233) | (0.058) |
| N | 59,521 | 27,486 | 24,611 | 27,486 | 34,408 | 26,886 | 34,922 |

Estimates are based on the interaction term of the treatment variable (in kilometers per city area) and the natural logarithm of the heterogeneity variables (column names). Coefficients are scaled to the average treatment dose in our sample. They can be interpreted as the unit change in cycling if a heterogeneity variable is one unit higher (when assuming treatment with an average pop-up bike lane program). All regressions include counter, city–week, and country–day fixed effects. They also include weather controls (7), a control for overall mobility (8), and a control for the number of counters active in a city on a given day. Data for the outcome variable are from the European Cyclists' Federation (3) and data for the treatment variable are from municipal bike counters (*Materials and Methods*). All heterogeneity variables except for bike lanes per capita (17) are from the European Urban Audit (18, 19). Standard errors clustered at the city level are reported in parentheses. Significance levels are $^*P < 0.1$, $^{**}P < 0.05$, $^{***}P < 0.01$.

and directness as proposed by ref. 12 and other more complex measures of a bike lane network, such as the level of protection of a bike lane and the treatment of intersections (13). In this context it is important to investigate how underserved communities can be provided with a pop-up bike lane network that is complete and inclusive and how additional political, cultural, and economic barriers to cycling for low-income and minority groups can be removed (14). Bike sharing can support changes in modal choice (15), but important barriers to adoption remain for underrepresented groups (16). We therefore think it would be valuable to investigate interactions between the pop-up bike lane policy and time series data on bike sharing policies including changes in pricing and the availability of bikes and stations.

## Discussion

We find robust evidence for substantial short-run increases in cycling in European cities due to new provisional cycling infrastructure. Independent of its potential impacts in reducing COVID-19 transmission, the net benefits of the intervention are likely to be large. The direct cost of cycling infrastructure is low. At the higher end, 1 km of bike lane in Sevilla has previously cost €250,000 (20). However, Berlin's approach of iterative planning with provisional infrastructure during the pandemic has for instance reduced costs to €9,500/km as of July 2020 (21). These costs are small compared to the substantial health benefits from the new infrastructure. Previous research has found that every kilometer of cycling generates health benefits of \$0.45 (22). As a complementary and more stylized analysis, we combine this estimate from the literature with our econometric estimates of policy-induced cycling increases to provide a projection of health benefits generated by pop-up bike lane programs. We calculate baseline values for total cycling in a city based on data on daily kilometers cycled in German cities in 2018 and extrapolate these numbers to the other European cities in our sample based on city-level data on transport and population (*Materials and Methods*). This extrapolation is approximate but sufficient to calculate a range of potential health benefits. Based on our regression-based estimate for the 95% confidence interval of the "treatment dose" in terms of kilometers per square kilometer, we project that the additional cycling induced by the pop-up bike lane treatment during its first months of operation has generated between \$0.5 and \$1.7 billion in health benefits. Thus, the new infrastructure may generate between \$2.2 and \$6.9 billion/y in health benefits if the new bike lanes become permanent and make cycling habits stick. We project this range to be between \$1.2 and \$3.5 in annual health benefits if we use our alternative estimate for the 95% confidence interval of the policy effect based on the "treatment dose" in terms of kilometers per capita.

The magnitude of our estimate is large compared to previous evaluations of new cycling infrastructure improvements that have found statistically unclear or modest effects, typically because of the limited scale of the interventions (23–25). Our estimate implies a higher responsiveness of cycling to new infrastructure than the associations found in cross-sections of US cities (10, 26). However, in cities in Europe (17) and the United Kingdom (27) additional infrastructure is associated with more cycling than in the United States. The case of Sevilla has shown that in a dense city with a high share of narrow, cycling-friendly roads the construction of bike lanes on major roads can create substantial cycling growth: 120 km of new bike lanes have led there to a fivefold increase of cycling between 2006 and 2011 (20). Similarly, pop-up bike lanes have often been placed on main roads. Thereby they have removed important bottlenecks for cyclists and generated important improvements for the overall cycling network. Many of the cities in our sample are fundamentally well suited for cycling. For instance, they are often dense and have a high share of side roads with slow car speeds. Therefore, they can be assumed to have a high potential for catch-up growth, which is one explanation for our larger effect estimate. In addition, the pandemic has led to a reshuffling of otherwise rather inelastic mobility choices and thus created the conditions for new infrastructure to induce shifts to active travel. However, this also means that our results cannot be directly generalized to other settings. Given this limitation in terms of external validity, we caution against an overinterpretation of our estimates as providing a benchmark value for increases in cycling that planners should expect from an additional kilometer of bike infrastructure. It remains to be evaluated whether the new bicycle use is sticky and how similar treatments influence behavior outside of the context of a pandemic.

Surveys indicate that separated, protected infrastructure is a key element to incentivize uptake of cycling (28–30). Cities have experimented with different measures to create new spaces for cycling, ranging from painted to provisionally protected bike lanes and from traffic calming with signs to built "modal filters" that prevent the passage of cars. Our data on pop-up infrastructure do not allow us to systematically distinguish between these types of interventions and the quality of their implementation.* Further research should investigate which types of infrastructure have more successfully increased cycling by previously underrepresented groups, such as women, older people, and children.

*In *SI Appendix*, Table S3 we show that the results are robust to specifying treatment in terms of 1) the total length of all types of infrastructure, 2) the total length of measures clearly marked as bike lanes in the data, 3) the number of measures, and 4) a binary indicator for treatment.

## Materials and Methods

**Bicycle Counter Data.** We connect to municipal Open Data Portals to obtain daily bicycle counts from bike counters in large- and medium-sized cities in 20 European countries. The raw data and code to download counter data are included in our code package (31). The outcome is modeled as the natural logarithm of cycling counts. This means that we investigate percentage changes rather than absolute increases in the number of cyclists. Our outcome varies daily at the counter level (summary statistics and cleaning procedures in *SI Appendix*).

**Bike Lane Data.** The data on planned and completed pop-up infrastructure projects have been collected and crowdsourced by the European Cyclists' Federation based on technical reports and media announcements. A visualization of the data can be accessed at https://ecf.com/dashboard. We merge these data with city polygons from the European Urban Audit 2020 (32) to generate a cumulative measure for the total number of pop-up bike lanes in service in a city on a given day (summary statistics and cleaning procedures in *SI Appendix*). We generate a range of treatment variables (binary, kilometers built, kilometers per capita, kilometers per square kilometer of city area) and assign this treatment to counters based on their city polygon.

**Control Variables.** Using fixed effects in our regressions, we remove and therefore control for time-invariant differences between cities and between the locations of the individual counters in our data. Therefore, any additional time-invariant control variables at the city and the counter level would be redundant in our analysis. We also use fixed effects interacting different spatial levels with time dimensions, thereby controlling for many time-varying observable and unobservable factors. We use additional controls to rule out any bias that may be introduced by time-varying factors below our fixed effect levels.

We control for daily changes in public transport supply and demand with the transit variable from the Apple COVID-19 Mobility Trends Reports (1). This variable captures daily variations in the number of requests for public transport directions on Apple Maps. We access these data using the covmobility package (33).

We capture average human mobility throughout the phase of the COVID-19 pandemic starting in March with a human mobility index based on Facebook data (8). The index is from a dataset called "movement range maps" that Facebook shares after aggregating individual user movements for humanitarian and research purposes with a reference to the principles outlined by epidemiologists and public health researchers (34). It measures the number of daily 600-m grid cells visited by Facebook users compared to a baseline in February. For most of our sample the index is aggregated to the state level, where we use the data. On average, in our sample period daily mobility has been below the February baseline.

We use weather data from the ERA5 climate model that generates hourly measures of surface temperature, ultraviolet (UV) radiation, precipitation, and wind at a $0.25^\circ \times 0.25^\circ$ resolution (7). We use the ecwmfr package (35) to aggregate this to the European Union Urban Audit city polygons (32) at the daily level.

**Heterogeneity Variables.** We analyze heterogeneous treatment effects along seven city-level variables. Bike lanes per capita measures the length of the bike lane network in a city based on Open Street Map data (17, 36). Population density is from the European Urban Audit (19). Public transport (PT) modal share, cycling modal share, cars per capita, car commute speeds, and road deaths per capita are based on city transport statistics from Eurostat (18). We use the natural logarithm of these variables to obtain the unit change in cycling for a unit change in the respective heterogeneity variable.

**Empirical Strategy.** We estimate a panel regression model at the counter level with daily counts of cyclists as the outcome variable and the number of kilometers (kilometers, kilometers per capita, or kilometers per square kilometer of city area) of pop-up bike lanes in service in a city on a given day as the treatment. This regression analysis forms comparisons between treatment and control groups before and after treatment for each cohort of new bike lanes and for different treatment intensities (generalized difference in differences). This separates the effect of pop-up bike lanes from overall changes in cycling due to COVID-19. We use a set of indicator variables (fixed effects) that remove remaining variation from our estimation sample that would otherwise bias our estimates. Our study design thus allows for systematic differences in the level of bike traffic between treatment and control groups, but relies on a common trends assumption, that bike traffic in treated and control cities would have evolved on a parallel trend in the absence of treatment. We cannot observe treated units in their untreated state after treatment (potential outcome). However, we can investigate pretreatment trends between treated and control cities and check the sensitivity of our estimates to changes in the control group definition, i.e., in the way we construct the empirical counterfactual (Fig. 3).

In our preferred specification we model the relationship between cycling traffic and the pop-up bike lane treatment as

$$\ln \text{Count}_{id} = \beta \text{Bike Lanes}_{cd} + \mathbf{X}_{cd} + \lambda_i + \sigma_{cw} + \varphi_{nd} + \varepsilon_{id} \qquad [1]$$

where $i$ indexes a counter, $c$ a city, $n$ a country, $d$ a day, and $w$ a week.

$\lambda_i$ is a counter fixed effect that controls for time-invariant factors at a high spatial resolution. $\sigma_{cw}$ is a city–week fixed effect that controls for week-specific time-varying factors, thereby restricting identifying variation to days before and after treatment within the same week in the same city. $\varphi_{nd}$ is a country–day fixed effect that captures any daily changes common to all cities in a country.

We cannot include fixed effects for factors that vary at the city level over time, such as local mobility or weather, since this is the geographical level at which our treatment is measured. $\mathbf{X}_{cd}$ is a vector of control variables that account for these factors. It includes an index for public transport use from Apple (1), an index for overall mobility based on Facebook data (8), weather variables (temperature, UV radiation, wind, precipitation) (7), and the number of counters per city active on a given day.

The coefficient of interest is $\beta$. It captures the effect of the pop-up bike lane treatment on bicycle counts. Our treatment variable is defined either as a binary indicator for treatment or as the number of kilometers (kilometers, kilometers per capita, or kilometers per square kilometer of city area) of pop-up bike lanes in service on a given day.

Figs. 2 and 3 and Table 1 present the transformed estimate $100 \times (\exp \beta - 1)$.

Since our outcome is a count variable, we use Poisson pseudo–maximum-likelihood (PPML) regressions to estimate this model (37). As a robustness check we also use ordinary least squares (OLS) with the natural logarithm of the bicycle count as the outcome (Fig. 3). We cluster standard errors at the city level, where treatment is assigned (38).

**Calculating the Health Benefits.** We calculate the health benefits by combining our regression estimates of cycling increases for each kilometer of pop-up bike lane with an estimate of the average health benefits of a kilometer cycled ($0.45 converted from 0.62 Australian dollars), which is lower than typical values from the gray literature (22). Our dose–response regressions give us the percentage increase in cycling per kilometer of bike lane divided by the city size or city population. For each city in our sample we multiply this effect by the size of its pop-up bike lane program. We then convert this result into additional kilometers cycled in a city based on baseline values of kilometers cycled per person from a detailed transport behavior survey in 135 German cities (39). We impute values of kilometers cycled for other European cities based on ordinary least-squares regressions using information on baseline values of a city's modal split (trips) of commutes, its population density, the length of its initial bike lane network, the modal share of public transport, the number of cars per capita, the average speed of car commuting, and road deaths per capita (more detail in *Heterogeneity Variables*).

**Data and Code Availability.** Raw data and code have been deposited in Zenodo (DOI: 10.5281/zenodo.3973038) (31).

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