European Economic Review 160 (2023) 104593

Contents lists available at ScienceDirect

European Economic Review

journal homepage: www.elsevier.com/locate/eer

Shared e-scooter services and road safety: Evidence from six European countries☆

Cannon Cloud[a], Simon Heß[b], Johannes Kasinger[c,*]

[a] Goethe University Frankfurt, Theodor-W.-Adorno-Platz 3, 60323 Frankfurt am Main, Germany
[b] University of Vienna, Oskar-Morgenstern-Platz 1, 1090 Vienna, Austria
[c] Tilburg University and Leibniz Institute for Financial Research SAFE, Warandelaan 2, Koopmans Building, 5037 AB Tilburg, Netherlands

A R T I C L E I N F O

JEL classification:
R41
C23
O18

Keywords:
Urban mobility
Staggered rollout
Road infrastructure
Traffic accidents

A B S T R A C T

We estimate the causal effect of shared e-scooter services on traffic accidents by exploiting the variation in the availability of e-scooter services induced by the staggered rollout across 93 cities in six countries. Police-reported accidents involving personal injuries in the average month increased by around 8.2% after shared e-scooters were introduced. Effects are large during summer and insignificant during winter. Further heterogeneity analysis reveals the largest estimated effects for cities with limited cycling infrastructure, while no effects are detectable in cities with high bike-lane density. This difference suggests that public policy can play a crucial role in mitigating accidents related to e-scooters and, more generally, to changes in urban mobility.

1. Introduction

New technologies reshape urban life and mobility, challenging policymakers in metropolitan areas worldwide (Hall et al., 2018; Berger et al., 2018; Barreto et al., 2021). Shared e-scooter services are one example that emerged as a prominent mode of transportation. Globally, between 2018 and mid-2022, more than $5bn were invested in companies providing shared e-scooter services (Heineke et al., 2022). Despite their increased availability, the role of shared e-scooters in future urban mobility ecosystems remains a highly divisive discussion topic. Proponents argue that shared e-scooters can ease issues related to motorized traffic, such as air pollution, noise pollution, and congestion (Shaheen and Cohen, 2019; Gössling, 2020; Abduljabbar et al., 2021). Opponents raise concerns about sustainability, safety, and crowded sidewalks (Hollingsworth et al., 2019; James et al., 2019; Sanders et al., 2020).

One central point of contention is the social cost induced by shared e-scooter services through traffic accidents. In the EU alone, over 150,000 people are killed or seriously injured in traffic accidents every year. The OECD (2018) estimates that social costs of traffic accidents exceed 3% of the EU's GDP. Thus far, the public and academic discussion of the safety risks of e-scooters has mostly focused on the large relative change in injuries from e-scooter-related accidents over time (Choron and Sakran, 2019). However, this increase is inevitable because e-scooters were virtually non-existent before 2018. Additionally, this simple inter-temporal comparison is insufficient to inform policy because substitution between modes of traffic and other indirect effects are not accounted for by these raw numbers, as detailed below.

☆ Johannes Kasinger gratefully acknowledges financial support from the Leibniz Institute for Financial Research SAFE. Simon Heß thanks the Joachim Herz Foundation, Germany for financial support.

* Corresponding author.
E-mail addresses: cloud@econ.uni-frankfurt.de (C. Cloud), j.kasinger@tilburguniversity.edu (J. Kasinger).

https://doi.org/10.1016/j.euroecorev.2023.104593
Received 16 January 2023; Received in revised form 16 September 2023; Accepted 23 September 2023
Available online 27 September 2023

To address this scarcity of evidence, this article studies medium-run effects of the introduction of shared e-scooters on police-reported accidents involving personal injury in six European countries. We identify the effect on urban accidents using quasi-experimental variation in the availability of shared e-scooters across cities and time. In our analyses, treatment is defined as the city-specific launch date of the first shared e-scooter service. Once the technology and capital for dockless e-scooters became widely available and national regulation allowed their use, e-scooter firms rapidly expanded into large cities across our sample countries, Austria, Finland, Germany, Norway, Sweden, and Switzerland.

Our identification strategy assumes that city-specific treatment timing is exogenous to accidents conditional on city fixed effects and period fixed effects since the rollout of e-scooter services is mainly determined by national regulation, business capacity constraints, time-invariant city characteristics, and seasonal variation. In more technical terms, Ghanem et al. (2022) show that sufficient conditions for the parallel trends assumption underlying our identification strategy are that the treatment timing is independent of city-time-varying unobservables and that accident-affecting city-time-varying unobservables have a constant mean over time conditional on city-level time-invariant unobservables.

Under this assumption, we can estimate treatment effects in a difference-in-differences framework, using the monthly police-reported data on traffic accidents involving personal injuries in cities before and after the arrival of e-scooter providers. In our preferred specification, we use the estimator proposed in Borusyak et al. (2023), which also straightforwardly allows us to conduct heterogeneity tests for subgroups and to obtain estimates with and without using never-treated cities in our control group. All our estimates account for period- and city-specific fixed effects and heterogeneous treatment effects under staggered rollout.

Our data combine administrative traffic accident data from 2016 to mid-2021 at the city-month level with extensive data on the timing of the rollout of e-scooter services by over 30 providers in 93 major European cities. Our estimation sample consists of all cities with a population of at least 100,000 that are not suburbs of larger metropolitan areas, and where a shared e-scooter service was introduced or announced by the end of 2021. The six countries are selected based on publishing accident data in the required detail and launching e-scooter services early enough to observe medium-run effects. The launch dates of shared e-scooters are manually collected from public sources and contain data on all firms offering e-scooter services in the six countries (details in appendix A).

We find that accidents involving personal injury in cities with shared e-scooter services increased significantly by $8.2 \pm 2.9\%$ (mean estimate $\pm$ std. error) relative to a counterfactual, estimated from cities where e-scooters launched later. Treatment effect estimates are larger ($11.5 \pm 3.5\%$) for summer months when e-scooters are used and deployed more intensively, while estimates for winter months are statistically insignificant. Our results are qualitatively robust to different specifications, including (i) using alternative difference-in-differences estimators that allow for heterogeneous treatment effects, two-way fixed effects estimation, and Poisson regression, (ii) aggregating the data into an annual panel to use a simple 2-by-2 difference-in-difference estimator that sidesteps some potential problems of difference-in-difference estimation with many periods, (iii) addressing potential concerns related to endogenous treatment timing, (iv) restricting the sample to avoid confounding effects of COVID-19 countermeasures and seasonality, or (v) using never-treated cities as the control group.

To explore the underlying mechanisms, we study treatment effect heterogeneity between cities with different traffic and road characteristics. Following the literature on cycling safety—a closely related mode of transportation—we analyze accidents along the dimensions of separated micromobility-suitable infrastructure (henceforth bike lanes), registered cars per capita, and modal split. In cities with a low density of bike lanes or relatively many registered private cars per capita, the effects are substantial ($11.5 \pm 3.9\%$ and $10.1 \pm 4.1\%$ respectively). In contrast, we find no significant effect in cities with comparably extensive bicycle infrastructure and smaller effects in cities with a low number of cars per capita. Thus, our heterogeneity results point towards a central role of separated cycling infrastructure and public policy in mitigating accidents. This has important policy implications since many cities, regions, and countries have made political commitments to increasing their micro-mobility modal share (including the use of bicycles, e-scooters, and similar modes of transportation) in the face of climate change.[1] Our findings from the changes in modal shares induced by the staggered rollout of shared e-scooter services can inform how to safely increase micro-mobility in cities.

Our approach identifies the effect of e-scooters including direct effects (i.e., e-scooter accidents) and indirect effects. In our analyses, we consider all police-reported accidents involving personal injury, which naturally accounts for substitution between modes of transportation and other indirect effects. Alternatively, focusing exclusively on e-scooter accidents (which have no comprehensive database during our study period) would be ill-suited to inform policy because this approach cannot account for indirect effects. This is because if, e.g., e-scooters substitute for cars, then cities may see an increase in e-scooter accidents but a reduction in accidents involving cars. The resulting overall effect could be a reduction in accidents, despite an increase in e-scooter accidents. Therefore studying accidents involving personal injury, irrespective of the involved vehicles, is useful. The fact that we find large and significant overall treatment effects implies that the increases in accidents caused by e-scooter are not entirely offset by a reduction in other traffic accidents.

Another reason to focus on total accidents is that not all accidents caused by e-scooters must necessarily involve an injured e-scooter user. For example, the analysis of two Swedish datasets of injuries associated with e-scooters (Stigson et al., 2021) showed that 8% of such injuries were sustained by other road users. An additional 5% were pedestrians or cyclists injured by parked e-scooters. If shared e-scooters indirectly affect accidents among other modes of transportation, studying overall accidents can reveal those effects regardless of whether detailed classifications of all indirectly involved parties are available (which they are not for most countries).

[1] E.g., in the Pan-European Master Plan for Cycling, signed by 54 European nations, 16 nations have approved national cycling strategies stating an increased cycling modal share as explicit, measurable objectives (Küster et al., 2022).

The existing evidence on injuries caused by e-scooter accidents is of limited generalizability and not suited to inform public policy for two more reasons. First, previous studies are mostly descriptive and based on hospital data in single cities or countries. These analyses usually characterize accident risk factors and types of injuries (Badeau et al., 2019; Sikka et al., 2019; Trivedi et al., 2019b; Blomberg et al., 2019; Yang et al., 2020; Namiri et al., 2020; Stigson et al., 2021), but do not quantify the effects of e-scooter services on overall accidents resulting in injuries. Second, existing literature does not study the role of different city characteristics in mitigating accidents.

In addition, our study contributes to the literature on the effects of urban mobility innovations on road safety and urban life (Greenwood et al., 2017; Hall et al., 2018; Berger et al., 2018; Barrios et al., 2023; Barreto et al., 2021). Prominent examples of such innovations are ride-hailing services such as Uber. Empirical evidence of their effects on road safety is mixed. Barreto et al. (2021) exploit a related empirical framework and find that Uber's introduction decreased traffic fatalities in Brazilian cities. In contrast, Barrios et al. (2023) find an increase in traffic accidents, while Brazil and Kirk (2016) find no significant effect of ride-hailing services in US cities. We present the first study that provides causal evidence of the effects of shared e-scooter services—another landscape-changing urban mobility innovation—on road safety. With our heterogeneity analyses, we further add to the literature on factors affecting and mitigating traffic accidents, such as traffic regulations (Peltzman, 1975; Abouk and Adams, 2013; Van Benthem, 2015; Bauernschuster and Rekers, 2022) or road characteristics (for an overview see Wang et al., 2013).

The rest of this article is structured as follows. Section 2 describes the monthly panel data of traffic accidents, city-level variables, and the rollout of e-scooter services. Section 3 describes the statistical model, and motivates our preferred estimation specification, alternative specifications, and inference. Section 4 reports the estimation results for the average effects and the heterogeneity analyses. Section 5 discusses robustness tests and Section 6 discusses implications and limitations and concludes.

2. Data

Our study focuses on cities with a population of at least 100,000 inhabitants across six countries: Austria, Finland, Germany, Norway, Sweden, and Switzerland. Our main sample comprises 93 cities, and the observation period spans from January 2016 to June 2021.[2] Our primary outcome variable of interest is accidents involving personal injury, measured at the city-month level using national police reports. The timing of treatment refers to the rollout date of shared e-scooter services in different cities, which we manually collected from various public sources such as local newspapers, e-scooter provider websites, and social media. Additionally, we gather city-level characteristics from Eurostat and *OpenStreetMap* to conduct heterogeneity analysis and robustness checks.

2.1. Traffic accidents

We use monthly administrative data on traffic accidents involving personal injury for January 2016 to June 2021. Data from Sweden is not publicly available for 2016 and 2017, so data for Swedish cities only starts in January 2018. All accident data are disaggregated at the city-month level and refer to official accident statistics as reported by the national police to the statistical offices of the respective countries. Austria, Germany, and Switzerland report traffic accidents by city; however, the Scandinavian countries report accidents by municipalities. For large cities, as in our sample, municipalities and cities are well-aligned: the population of the average Scandinavian sample city makes up 88% of the corresponding municipality's population (details in appendix A.1). For Stockholm, Oslo, and Helsinki, which span multiple municipalities, we use data from the homonymous municipality, covering most of the respective city's population, and we exclude the "suburban" municipalities, which differ in terms of traffic movements and infrastructure. The country-specific sections in appendix A.1 describe the data-collection procedure in greater detail.

The definition of a road traffic accident with personal injuries varies marginally between countries in our sample, primarily due to different thresholds for injuries and the inclusion of railed or off-road vehicles in the data. While our model's fixed effects absorb these time-invariant coding differences, three important shared minimum standards across all countries ensure the comparability of the data across countries and, thus, the interpretability of our results. First, accidents involving e-scooters are eligible to be reported as traffic accidents by the police to the national statistical agencies, as all countries in our sample define e-scooters in use as moving vehicles. Second, all countries at least record accidents on public roadways, including sidewalks, bike lanes, medians, and car travel lanes. Third, the sample countries have a reporting obligation for police and traffic accident participants for accidents with severe injuries or death, ensuring the vast majority of e-scooter accidents with serious injuries or deaths are reported.[3]

Accident numbers naturally vary substantially across cities and seasons. Table 1 reports summary statistics for average accidents by city. In the median sample city, the mean number of monthly accidents per 10,000 inhabitants is 2.8. The lowest number is reported for Tampere, Finland (0.4) and the highest for Klagenfurt, Austria (5.6). Seasonal variation is strong. The median city has a mean of 2.9 accidents per 10,000 inhabitants in non-winter months (March–October) and 2.4 accidents per 10,000 inhabitants during winter months (November–February).

[2] The panel horizon of 6/2021 addresses a trade-off: Shorter horizons reduce the available post-treatment periods to estimate effects. Longer horizons include periods for which few yet-to-be-treated cities remain to estimate the period fixed effects. Using 6/2021 ensures enough yet-to-be-treated cities remain (see appendix figure 3) without omitting the whole summer of 2021. Estimates are comparable when using different cut-offs, e.g., 12/2020 or 9/2021.

[3] One possible caveat regarding the measurements we use is that *single-user cycling accidents* are consistently under-reported, but under-reporting decreases in accident severity (Shinar et al., 2018) and that this may be similar for e-scooter accidents. We do not consider this a major concern for two reasons. First, our effect estimates are elasticities and would thus not be affected by under-reporting if under-reporting for e-scooter accidents is similar to under-reporting in the counterfactual transport modes (predominantly walking, public transit, other micromobility and taxi/ridesharing, see Wang et al., 2023) and, if anything, estimates would be downward biased if under-reporting is more pronounced for e-scooters than for the counterfactual modes. Second, since accidents with severe injuries are less under-reported, our estimates would still capture effects on the most severe accidents, as discussed in Section 6.

Table 1
City-level summary statistics.

	N	Median	Min	Max
population	93	206 537	100 030	3 669 491
month of first scooter introduction	93	Sep 2019	Apr 2018	Nov 2021
monthly accidents per 10k inhabitants	93	2.8	0.4	5.6
monthly accidents per 10k inhabitants (Nov–Feb)	93	2.4	0.3	4.1
monthly accidents per 10k inhabitants (Mar–Oct)	93	2.9	0.4	6.3
share of bike lanes in road network (%)	93	26.0	4.9	97.1
cycling modal split (%)	93	11.2	1.1	36.6
cars per 1k inhabitants	93	402.2	194.0	617.2

Notes: This table shows summary statistics for the 93 ever-treated cities in our main estimation sample.

2.2. E-scooters service rollout

The data on the launch dates of e-scooter services in different cities have been collected from official press releases, social media channels, and websites of e-scooter providers, local newspapers, or cities. We corroborate our data with dates provided directly by two large e-scooter firms. In total, we have data on rollouts for 38 different providers. We discuss the rollout data in detail in appendix A.2 and make the data available in the online appendix. Fig. 1 shows a map of the cities in our sample. The color of the circle indicates the relative launch date, while unfilled circles indicate never-treated cities. The earliest launch is April 2018 and the latest is March 2022. Never-treated cities are clustered in the Rhine-Ruhr metropolitan region. Most of these never-treated cities, such as Bottrop or Offenbach, are suburbs or satellite cities of a larger treated city. Appendix figure 3 shows that launches are evenly distributed across the observation period, with some excess mass in the summer of 2019 when Germany legalized the public use of e-scooters. As discussed in more detail later (see Section 5.3 and appendix B.2.1), e-scooter services seem to be launched earlier in larger cities and cities with a high share of bike lanes.

2.3. City-level variables for heterogeneity analyses

To investigate treatment effect heterogeneity along different city characteristics, we use three different variables which were previously linked to cyclist safety (Kraus and Koch, 2021): the share of separated bike lanes in the total road network for cars, registered cars per capita, and cycling modal share. The lengths of separated cycling infrastructure and total road network are collected from *OpenStreetMap*. Data on cars per capita and on cycling modal share by city are primarily obtained from Eurostat (2022), and supplemented with local administrative sources or academic papers. Details on the city-level data can be found in appendix A.3.

Table 1 reports summary statistics of our city-level variables. The median city's *share of bike lanes in road network*, calculated as the length of bike lanes as a percent of the road network for cars, is 26%, with a range from 5%, Saarbrücken in Germany, to 97%, Helsinki in Finland. The median *cycling modal split* is 11%. Likewise, the range is large, with some cities having as little as 1% of trips on bicycles while others have more than a third. *Cars per 1000 inhabitants* has a median of 402 and ranges from 194 to 617.

3. Empirical strategy

We are interested in the effects of the introduction of shared e-scooters on road traffic accidents. To identify the effect, we exploit the quasi-experimental variation induced by the staggered rollout of shared e-scooter services across cities and time. We define treatment as the city-specific first launch date of any shared e-scooter service.

Assuming that the city-specific treatment timing is exogenous to accidents conditional on city-level fixed effects and period fixed effects (for a detailed discussion of potential endogeneity concerns, see Section 5.3), we can estimate the causal effects in a difference-in-differences (DD) model that can be described by following the event-study (ES) equation:

$$\log \text{accidents}_{it} = \alpha_i + \beta_t + \sum_k \tau_k^{\text{ES}} d_{it}^k + \varepsilon_{it}, \tag{1}$$

where α_i is a city-level fixed effect, β_t is a month-level fixed effect, d_{it}^k is a city-month-level indicator for being k months away from the introduction of shared e-scooter services in a specific city and τ_k^{ES} is the treatment effect k months after introduction. The error term, ε_{it}, captures variation in the number of reported accidents unrelated to treatment that varies over time and space, e.g., measurement error due to reporting or coding of accidents. The corresponding regression equation for estimating the *average* treatment effect (τ^{ATE}) across all post-treatment months is given by:

$$\log \text{accidents}_{it} = \alpha_i + \beta_t + \tau^{\text{ATE}} d_{it} + \varepsilon_{it}, \tag{2}$$

where d_{it} is a city-month-level indicator for treatment, indicating if any e-scooter firm has launched in city i before month t. Therefore, τ^{ATE} is the treatment effect in the average post-treatment month.

A historically common method for the empirical analysis of staggered roll-out DD is to estimate the above equations with ordinary least squares (OLS) in a multi-period DD framework accounting for time and unit fixed effects. This is commonly referred to as the

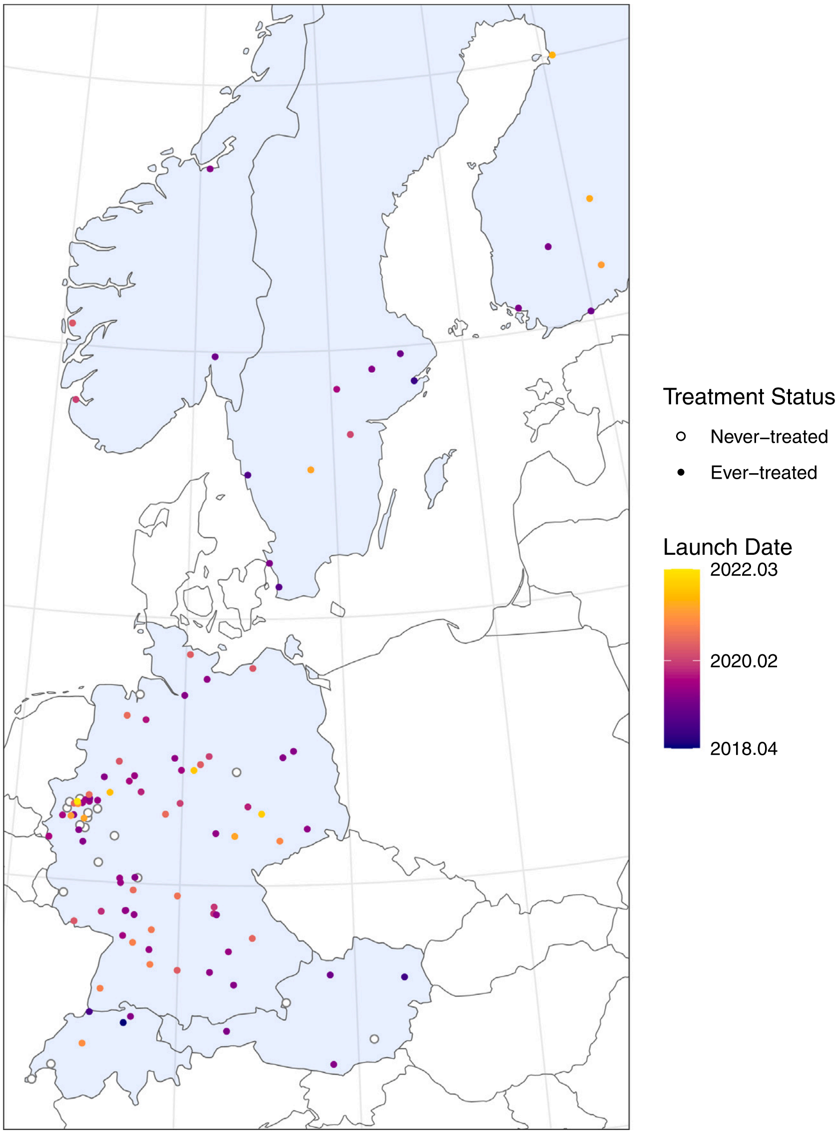

Fig. 1. Map of sample cities by treatment status and launch date.
Notes: The map shows sample city locations that are color-coded based on launch dates. Shaded blue countries are in the sample. Country outlines are provided by the public domain map dataset Natural Earth (Kelso and Patterson, 2010). (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

two-way fixed-effects (TWFE) estimator. However, recent work (Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021) has shown that this estimator yields an ill-defined estimand under staggered roll-out if treatment effects vary over time (e.g., increase with exposure duration) and when the estimation uses data spanning periods in which (i) earlier-treated units remain treated across periods while (ii) later-treated units start being treated. This problem arises because the standard TWFE estimator is based on comparing the average change in units that switch their status from untreated to treated in a given period to the average change in units with no change in treatment status. For multi-period staggered-rollout designs, the group with no change in treatment status consists of two subgroups: untreated units and units treated in earlier periods. Suppose the treatment effect on the earlier treated units changes over time. In that case, the earlier treated units do not provide a valid counterfactual because the evolution of their outcome combines the counterfactual change/trend and the changing treatment effect.

In our setting, these conditions that give rise to the bias of the TWFE estimator fully apply. Treatment timing varies considerably, and treatment effects are likely to vary over time for a number of reasons. For example, e-scooter fleet sizes grow over time and vary seasonally; urban traffic can adapt to the services, e-scooter technology improves, and adoption by commuters is likely gradual. It thus seems plausible that treatment effects increase over time. Estimation should, therefore, not be based on the assumption of constant treatment effects, and TWFE estimates are likely downward-biased.

De Chaisemartin and d'Haultfoeuille (2020) specifically show that the TWFE estimator does not identify the ATE of interest but a weighted average of city-month treatment effects, with possibly unevenly distributed weights that may even be negative for some observations. This is not the estimand of interest; therefore, alternative estimators have been proposed (e.g., Callaway and Sant'Anna, 2021; Borusyak et al., 2023). To further substantiate the claim that this point is of concern in our setting and that we need to use these alternative estimators, we study the weights on city-level treatment effects that are implied by computing the average treatment effect using TWFE. This provides a more formal test of whether TWFE estimates are biased. We compute these weights as suggested in De Chaisemartin and d'Haultfoeuille (2020). Based on this analysis, 12.5% of the 1704 individual city-level estimates have a negative weight in the TWFE estimate of the ATE. These negative weights only constitute the most extreme form of unequally distributed weights. We thus expect that, with heterogeneous treatment effects, these weights imply that TWFE estimates for the ATE and the pre-trends may be significantly biased.

For the above reasons, we focus our analyses on multiple sets of estimators that account for this issue. Our preferred specification uses monthly data and is based on an event-study estimator proposed by Borusyak et al. (2023). As robustness, we implement a simple 2-by-2 DD estimator based on annualized data using two groups of cities (early-treated cities and late-treated cities), comparing differences in accidents in 2018 to differences in accidents in 2020 (see details in Section 5.2). We discuss the differences between estimates with TWFE and alternative robust estimators and show robustness to using alternative estimators for monthly event-study designs based on Callaway and Sant'Anna (2021) and on Arkhangelsky et al. (2021). A detailed discussion of these alternative estimators can be found in Section 5.1.

All specifications account for city-level and period-level fixed effects to capture confounders that are stable across time (e.g., population, city-specific traffic characteristics, coding or reporting standards) or space (e.g., seasonality in traffic, technological development, climate change). Our results are also robust to a number of other additional robustness checks that we discuss in Section 5.

Main specification

As our preferred specification, we employ an event-study estimator using monthly panel data based on Borusyak et al. (2023). The estimation procedure can be described as follows. Generalizing the above specification, we assume outcomes in each city-period are described by

$$\log \text{accidents}_{it} = \alpha_i + \beta_t + \tau_{it} d_{it} + \varepsilon_{it}, \tag{3}$$

where τ_{it} is the city-month-specific treatment effect. The individual city-month-specific treatment effect cannot be identified (separately from the error ε_{it}). But we are interested in (conditional) expectations of these effects τ_{it}, e.g., the average treatment effect across all post-treatment months $\mathbb{E}[\tau_{it}] = \tau^{\text{ATE}}$ or the treatment effect in month k relative to a city's launch date, l_i, $\mathbb{E}[\tau_{it}|t = l_i + k] = \tau_k^{\text{ES}}$.

Following Borusyak et al. (2023), estimation then proceeds in three steps. First, only untreated observations are used to obtain estimates $\hat{\alpha}_i$ and $\hat{\beta}_t$. Second, these estimated city fixed effects and month fixed effects are used to estimate treatment effects for each treated city-month, $\widehat{\tau_{it}} = \log \text{accidents}_{it} - \hat{\alpha}_i - \hat{\beta}_t$. Third, these city-month estimates are averaged. This average is weighted to match the respective aggregate estimand of interest, e.g., the average effect for a specific month, k, after treatment (Fig. 2, column 1) applies a zero weight to all city-month estimands except those that refer to k months after treatment. Alternatively, the average effect across all post periods applies an equal weight to estimands from all treated periods (Fig. 2, column 3). The average effects by season (Fig. 2, columns 4–5) or effects on subgroups, as in our heterogeneity analysis (Table 2), are estimated analogously by applying non-zero weights only to the respective estimands.

Heterogeneity tests (Section 4.2) assume that accidents can be described by the model:

$$\log \text{accidents}_{it} = \alpha_i + \beta_t^{\text{h}} + \beta_t^{\text{h-l}} \mathbb{1}_{(x_i > \bar{x}_i)} + \tau_{it} d_{it} + \varepsilon_{it}, \tag{4}$$

where $\mathbb{1}_{(x > \bar{x}_i)}$ indicates if a city is above the country median of the specific heterogeneity variable, so $\beta_t^{\text{h-l}}$ allows for different month fixed effects depending on the heterogeneity variable. Estimation proceeds as before, estimating city fixed effects and both sets of month fixed effects on untreated observations only. Effect estimates are again obtained as a weighted average of month-city level estimates. Tests for equality between effects are obtained by using weights that sum up to 1 for cities above the median and to −1 for cities below the median of the variable of interest, thus estimating $\mathbb{E}[\tau_{it}|\text{above median}] - \mathbb{E}[\tau_{it}|\text{below median}]$. This estimate and the corresponding standard error can then be used to test the hypothesis that $\mathbb{E}[\tau_{it}|\text{above median}] = \mathbb{E}[\tau_{it}|\text{below median}]$. This approach is equivalent to estimating the effect according to Eq. (3) separately for two sub-samples, but provides a way to test for differences in the estimated effects and is also implemented in Borusyak (2023).

We estimate all models using the natural logarithm of the number of accidents as the dependent variable.[4] Effects sizes are likely not constant in absolute terms, but only relative to the baseline number of accidents, e.g., because the number of deployed e-scooters tends to be roughly proportional to city size. Also, the number of accidents exhibits seasonal swings with absolute magnitudes roughly proportional to levels. After applying the logarithm, the seasonal swings run parallel across cities, as visualized in appendix figure 11. Having parallel seasonality across cities is essential for the parallel trends assumption. Otherwise, there would be the risk

[4] As an alternative specification, we also estimate Poisson regression. Results are qualitatively robust. But Poisson regression does not allow for using the alternative estimators that account for staggered rollout, which is why we consider this (like the TWFE-regression) to be likely downward biased and only discuss these results in the appendix (appendix table 5).

that differential seasonal swings for large and small cities confound our treatment effect estimates. Using logarithmized dependent variables is unproblematic in our case as observations with zero accidents are rare. Only a single city has zero accidents for only a single month (Oulu, Finland, Feb 2019, two years before e-scooters were introduced). For the estimation, we impute one accident for this single observation, but our estimates are qualitatively robust to dropping this observation.

The log-linear specification has the benefit of providing estimates that can be approximately interpreted as semi-elasticities, i.e., the percentage increase in accidents due to the rollout of e-scooter services. Tables show transformed estimates expressing non-approximate semi-elasticities $100 \cdot (\exp(\hat{\tau}) - 1)\%$.

Standard errors allow for the clustering of the model error at the city level. Standard errors are computed from residuals based on Eq. (3) and the estimator explained above, following the procedure detailed in Borusyak et al. (2023, 4.3). This estimation of standard errors can be summarized as follows: To be able to obtain residuals we require a model that is parsimonious enough so that it can separate τ_{it} from ε_{it}. Hence we cannot (as in our main estimation) allow for arbitrary treatment effect heterogeneity. Instead, the model based on which the standard errors are computed assumes that treatment effects are constant within cohorts and use the quarter of scooter introduction to define such cohorts.[5] This model is then estimated, to obtain city-month-level residuals based on which standard errors are obtained. The assumption of constant treatment effects within cohorts bears the risk of being misspecified, even if Eq. (3) is not. However, it was shown that this potential misspecification yields *conservative* inference (Borusyak et al., 2023, 4.3). Throughout the paper, unless indicated differently, standard errors are computed through a leave-out procedure, that computes the cohort-level treatment effect to obtain the residual, under omission of the focal unit (details in Borusyak et al., 2023). For the heterogeneity analysis in Table 2, where the samples are split, defining cohorts based on quarters is not feasible, because several quarters contain too few cities. We thus rely on half-years to define cohorts. To have sufficiently many observations per cohort all cities that launched until June 2019 are considered one cohort and all that launched since December 2020 are considered as one.

4. Results

4.1. Main effects

From the left, Fig. 2 shows the average treatment effects by month since the city-specific e-scooter introduction using the estimator from Borusyak et al. (2023). We report the monthly estimates for the first 18 months after the introduction and pre-treatment estimates for the 12 months before (column 1). Column 2 shows an estimate for the average treatment effect for months more than 18 months after treatment (column 2). The right side of Fig. 2 reports three additional estimates in columns 3 through 5: the average treatment effect across all treated city-months, non-winter months (March–October), and winter months (November–February). Appendix table 10 reports the numerical values of the estimates shown in Fig. 2.

Post-introduction months consistently indicate increased accidents. Accidents involving personal injuries increased on average by $8.2 \pm 2.9\%$ (mean estimate ± std. error). The estimates can be interpreted as the average effects across all post-treatment periods.[6] To put the estimated effect into perspective: the median sample city in terms of accidents (Potsdam, Germany, population of 180,334) reports 54 accidents in the average pre-treatment month. An increase of 8.2% thus implies an additional 4.4 monthly accidents. Estimated pre-treatment coefficients are statistically insignificant, suggesting that there was no anticipation effect and no pre-existing differential trends. Additional balance tests and a discussion of (the lack of) pre-treatment differences are in Section 5 and appendix B.2.

Fig. 2, column 1, also shows that there is heterogeneity in the treatment effect over time. The fact that the effect of e-scooters on accidents temporarily drops after five months can be explained by a majority of launches occurring in spring and summer. Accordingly, for many cities, the fifth month coincides with the beginning of winter. Similarly, the drop around month 17 may be related to the second winter a year later. We expect the treatment effect to be concentrated in the non-winter months since the number of deployed e-scooters and e-scooters utilization drops considerably during winter, according to descriptive analyses and news reports (Mathew et al., 2019; O'Brien, 2021). In line with this conjecture, treatment effect estimates are more stable over time and larger if we exclude winter months from the month-level analysis. Columns 4 and 5 of Fig. 2 show the average treatment effect estimates for non-winter and winter months. Appendix figure 8 shows an event-study plot of non-winter months and columns 4 and 5 of appendix table 10 report the plotted numeric values. In contrast, the average treatment effect during winter months is statistically indistinguishable from 0.

The estimated treatment effects seem to show a slightly increasing trend. On the one hand, this trend could be explained by early-adopting cities having larger effects. Cities are mechanically excluded in estimating average period treatment effects for post-periods greater than the number of months they were exposed to treatment. Consequently, later post-period estimates are increasingly driven by earlier-adopting cities. Therefore, the apparent increase over time could be an effect of changing sample composition. On the other hand, it could be that effect sizes are increasing over time, e.g., because of adaptation or increasing numbers of scooters. While a conclusive statement about these two alternatives is not possible, appendix table 15 shows that effect estimates for the second twelve-month period tend to be larger than for the first twelve-month period, while first-year effect estimates for early-adopting cities are only marginally smaller than estimates for late-adopting cities. Taken together, these results may indicate early-adopters do not have larger treatment effects, and thus effect sizes are increasing in the medium term. But, this finding is only suggestive as there are few cities with multiple years of exposure.

[5] With two exceptions: Zurich, which is the first city that received e-scooters and the only city in Q2 of 2018, is added to the cohort of Q3 2018, and cities that launched scooters in Q1 2021 and Q2 2021 are considered as one cohort.

[6] For the average treated city, the data span around 18 post-treatment months. Since all city-months are weighted equally, early-launch cities (spanning up to a maximum of 38 treatment months) have a larger overall weight in the average estimate than late-launch cities.

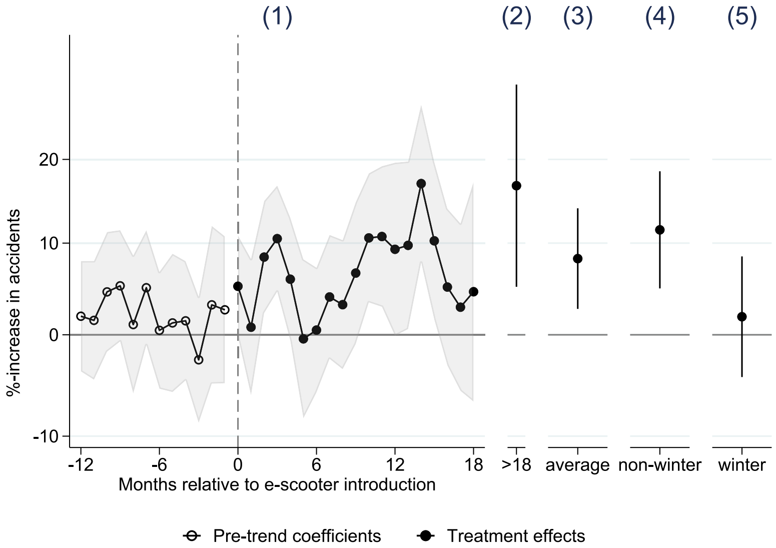

Fig. 2. Monthly treatment effects, pre-trends, and aggregate treatment effects.
Notes: The figure shows average treatment effects relative to treatment introduction. In the line plot, filled dots indicate treatment effect estimates for the first 18 months relative to the introduction of e-scooter services. Circles indicate estimates for pre-treatment months. Effects after month 18 are combined into a single coefficient because month-level estimates for long-term effects are estimated on small subsamples (few cities had e-scooters early enough for long-term effects to be observable). So, monthly estimates for later months cannot be estimated with comparable precision as for earlier months. In column 3, the average treatment effect estimate and corresponding 95%-confidence interval across all available post-treatment city-months is shown (see also appendix table 10, column 1). Columns 4 and 5 show separate estimates on accidents in winter months and non-winter months. Winter months are November–February, and non-winter the remainder. Shaded areas/bars indicate the 95%-confidence interval around the estimates, based on standard errors that allow for the clustering of the model error at the city level, computed via the leave-out procedure (see Section 3), using half-year (columns 1–2) or quarter (columns 3–5) of scooter launch as cohorts. All estimates account for city fixed effects and month fixed effects.

4.2. Mechanisms

To investigate mechanisms, we study treatment effect heterogeneity along different traffic and road characteristics of cities. Because e-scooters share several characteristics with bicycles, including applicable traffic rules and travel speed, city characteristics associated with cyclist safety are likely relevant to the effect of shared e-scooters on traffic accidents.[7] There is also a 'safety in numbers'-theory that could extend to e-scooter users, implying that e-scooters could be safer in cities with larger numbers of cyclists (Jacobsen, 2003). We thus use three heterogeneity variables related to cyclist safety, which were previously used in related work (Kraus and Koch, 2021): (i) the share of bike lanes in the road network, (ii) the number of registered cars per capita, and (iii) the cycling modal share. These three measures provide complementary evidence as they are collected from different sources and capture constraints faced and choices made by people when choosing their mode of transportation.

For each of the three dimensions, we classify cities into two groups depending on whether they fall above or below the country-specific median. We use country-specific median splits to address concerns about differential reporting standards (e.g., where cars are typically registered) and structural or cultural differences across countries (e.g., what types of cycling infrastructure are typically built).

Table 2 shows separate treatment effect estimates for the groups defined by each variable's country-specific median split. These estimates should not be interpreted as causal effects of the heterogeneity variables themselves, but rather as estimates of the group-specific treatment effect of introducing e-scooters with groups defined by the heterogeneity variables. While the effect of shared e-scooters on accidents in each subsample is identified under the same assumptions as our main effect, the differences in effects between subsamples are not necessarily caused by the heterogeneity variables. Other variables that are correlated with the heterogeneity variables, such as cities' road conditions, average vehicle speeds, speed limits, or income levels, could drive the observed heterogeneity in treatment effects.

Estimates in column 1 imply that e-scooters only had a small and statistically insignificant effect on total accidents in cities with an above-median density of bike lanes. In contrast, the implied effect for cities with a below-median density of bike lanes is large and highly significant, $11.5 \pm 3.9\%$. The difference between the coefficients is highly significant (p-value $= 0.018$). These results are in line with findings summarized in a literature survey on the impacts of infrastructure on bicycling injuries and crashes (Reynolds et al., 2009), suggesting that purpose-built bicycle-specific infrastructure can reduce traffic accidents.

[7] Regulations for e-scooters differ slightly across countries (e.g., Scandinavian countries legally treat e-scooters as bicycles, while Austria, Germany, and Switzerland consider e-scooters a separate vehicle category). However, in our sample countries the rules regulating cycle path use are the same for e-scooters as for bicycles (Forum of European Road Safety Research Institutes, 2020).

Table 2
Treatment effects of e-scooter introduction are heterogeneous.

	Share of bike lanes	Cars per capita	Cycling modal share
	(1)	(2)	(3)
%-increase in accidents for above-median cities	0.2	10.1***	9.6**
	(2.7)	(4.1)	(4.7)
%-increase in accidents for below-median cities	11.5***	4.7*	7.3***
	(3.9)	(2.6)	(2.6)
p-value H_0: coefficients identical	0.018	0.256	0.658
Cities	93	93	93
Observations	5880	5880	5880

Notes: * p<0.1, ** p<0.05, *** p<0.01. Table 2 shows estimated treatment effects from log-linear specifications (see Section 3). Standard errors in parentheses. In addition to city fixed effects and month fixed effects, regressions control for interaction fixed effects between month and the median-split indicator. Raw estimates are transformed to semi-elasticities: $100 \cdot (\exp(\hat{\tau}) - 1)\%$. The table illustrates the average effect estimates for different subsamples, defined by sample splits using the country-level medians of different city characteristics shown in the table header. Standard errors allow for clustering of the model error at the city level and are computed using the leave-out procedure recommended in Borusyak et al. (2023), defining cohorts as the half-year in which scooters were launched. Tests for coefficient differences are described in Section 3.

The results in column 2 indicate that cities with an above-median density of registered cars experienced a comparatively large treatment effect of $10.1 \pm 4.1\%$, while cities with a below-median density of registered cars experienced an estimated increase of $4.7 \pm 2.6\%$. When splitting the sample of cities by their cycling modal share, the two groups have numerically similar and statistically indistinguishable (p-value = 0.658) estimated effects. According to column 3, we do not find corroborating evidence for the 'safety in numbers' theory from the natural experiment of introducing shared e-scooters.

5. Alternative estimators, addressing endogeneity concerns, and robustness checks

This section discusses estimates obtained through alternative estimators and addresses potential endogeneity concerns in our setting. We present various robustness checks and placebo tests to corroborate the validity of our results further.

Sections 5.1 and 5.2 present and discuss estimation results of alternative estimators, such as two-way fixed effects (TWFE), the event-study estimator suggested by Callaway and Sant'Anna (2021), a synthetic difference-in-differences estimator (Arkhangelsky et al., 2021), and standard 2-by-2 difference-in-difference TWFE estimator applied to annualized data. In Section 5.3, we discuss potential factors influencing the timing of e-scooter rollouts, i.e., treatment timing, and address endogeneity concerns. Section 5.4 shows and discusses estimates obtained from a sample extending the control group to cities that never received shared e-scooter services.

5.1. Robustness to using alternative estimators in a monthly panel

5.1.1. Two-way fixed effects

There are several alternatives to estimate our treatment effects of interests in a monthly panel. Traditionally, staggered roll-out DD designs are estimated in a TWFE framework, which may lead to biased estimates as discussed above in Section 3. For reference, we discuss estimates based on TWFE in this section. Appendix table 5 shows an ATE estimate based on estimating Eq. (2) using OLS (i.e., a TWFE estimate). The estimated ATE, 4.7 ± 2.0, is statistically significant but should be interpreted with caution, given the biases discussed above.

One implication of the issue of TWFE estimator in staggered-rollout settings with negative weights, like ours is that "lengthening or shortening the panel can actually change the point estimate" (Cunningham, 2021, section 9.6.1). This corollary of the Goodman-Bacon (2021) decomposition for the TWFE estimator is an unfortunate property for an estimator, as it implies that seemingly irrelevant changes to the underlying data can have large repercussions on conclusions.

We observe the same in our data if we vary the start of the panel observation periods in ranges that could be expected to have no bearing on the results (more than two years before the first treatment starts).

Appendix figure 4 shows event-study TWFE-estimates. The TWFE estimates are similar to our main specification and like-wise statistically and economically significant if the observation period starts in April 2016, two years before the first launch (see figure 4a). However, starting the panel when our data availability begins in January 2016 (as in our main specification) changes the event-study estimates and estimated pretrends (see figure 4b). While the same temporal pattern as in our main specification in estimated treatment effects is observable, the point estimates of the treatment effects are smaller, and the pre-treatment estimates are larger in figure 4a.

Contrarily, when using an estimator that is robust to the issues that give rise to the bias of the TWFE estimator, like the Borusyak et al. (2023) estimator, this minor variation in the starting date of the panel (long before the actual treatment) does little to the point estimates of pre-trends or treatment effects. We take these differences in estimated effects between TWFE specifications as further support for basing our primary estimation specification on Borusyak et al. (2023).

Given the theoretical reasoning for why TWFE is ill-suited for our empirical analyses that we discussed in Section 3, the large share of city-level estimates that have negative weight in the average TWFE estimate (see Section 3), and the results discussed in this subsection, we consider the TWFE estimates not to be reliable.

Additionally, we provide results for a Poisson variant of the standard TWFE estimation in appendix table 5 to show that using a log transformation of the dependent variable, in contrast to modeling a Poisson distribution, does not bias our results. The estimated effects in this specification support the same conclusions but are smaller than in our main specification, as expected (see detailed discussion in appendix B.1.2.

5.1.2. *Callaway and Sant'Anna (2021) difference-in-differences estimator*

Callaway and Sant'Anna (2021) also propose an estimator that can account for the biases that arise with staggered rollout and dynamic treatment effects in a DD framework.

One principal difference between the estimators proposed by Callaway and Sant'Anna (2021) and Borusyak et al. (2023) is the reference periods which treatment effects are compared against.

Callaway and Sant'Anna's treatment estimands are interpreted relative to one period before treatment, while pre-trend coefficients are interpreted relative to one period before the period under consideration. In contrast to that, Borusyak et al. (2023) use the average of all pre-treatment periods as a reference period for treatment effect estimates. Their pre-trend coefficients reference the average of all periods preceding the window for which pre-treatment coefficients are estimated. Consequently, the Borusyak et al. (2023) estimator assumes parallel trends over a longer time horizon, but gains precision by comparing against the average of more reference periods. Roth et al. (2023) suggest preferring the estimator by Borusyak et al. (2023) when there is no concern of a violation of parallel trends over the earlier pre-treatment periods and when there is only modest serial correlation of error terms.

Appendix figure 5 shows the treatment effects by month using the Callaway and Sant'Anna (2021) estimator. The average treatment effect estimate based on this estimator is $5.6 \pm 2.9\%$ ($p = 0.048$). As expected, the confidence intervals are marginally larger. Given the noisiness of the pre-trend coefficients, we believe the Borusyak et al. (2023) estimator provides a more accurate estimate by averaging all pre-treatment periods to use as the reference period. Another reason to prefer the Borusyak et al. (2023) estimator is that the parallel trends over a longer time horizon is supported by the lack of a discernible diverging trend over the 36 pre-treatment periods (as illustrated in the pre-trend coefficients of Fig. 2 and appendix figure 9). Additionally, De Chaisemartin and d'Haultfoeuille (2022) note that similar results between the two estimators imply that the parallel trends over a longer time horizon is not violated. In our case, the results are similar.

Serial correlation of the error term, after conditioning on fixed effects, would be the second reason to favor the Callaway and Sant'Anna estimator. However, in our setting, serial correlation is less likely to be a concern, i.e., accidents in one month are unlikely to determine accidents in another, or time-varying city-specific accident determinants that may cause serial correlation, such as construction projects or safety campaigns, only influence a small number of overall accidents.

5.1.3. *Synthetic difference-in-differences*

As an additional alternative estimator, we estimate a synthetic DD model (Arkhangelsky et al., 2021). The previously discussed estimators weigh all controls included in the sample equally, irrespective of whether the control cities matched the pre-trend of treated cities. The synthetic DD method chooses from a pool of control observations and weights them with strictly positive weights based on goodness of fit to pre-treatment outcomes. In that way, the synthetic DD relaxes the parallel trends assumption because parallel trends need not hold for the average of the control cities in the sample but only for the weighted basket of controls that fit pre-treatment outcomes best. Control observations that exhibit very different accident patterns are weighted little or not at all. Details on the synthetic DD are in appendix B.1.5. Our main results are qualitatively robust. Moreover, the treatment effect estimate from the synthetic DD and the event study estimators discussed above are remarkably close when restricting the event study to the same sample period imposed by data requirements for the synthetic DD.[8] This suggests that the stricter parallel trends assumption of the event study estimators is not violated with our not-yet-treated sample.

5.2. *Robustness to using annualized data and without staggered rollout*

As additional corroboration, we use an annual DD framework comparing changes in accidents from 2018 to 2020 between cities that introduced scooters during 2019 and cities that introduced scooters in or after July 2020. The sample of cities used in these annual DD estimates is smaller than our main sample (because some cities were already treated before) but allows for simpler analyses. In this framework, there are only two periods and two groups of cities (those treated in 2019 and those treated later). This model is thus a standard 2-period DD model, which sidesteps the above-mentioned issues with heterogeneous treatment timing and time-varying treatment effects.

We use 2018, the last year before companies started introducing e-scooter services in most cities as the pre-treatment period and 2020 as the post-treatment period. Choosing a later post-treatment year would destroy the ability to maintain a comparably large

[8] The synthetic DD requires a fully balanced panel, which is why Sweden and one city from Germany are excluded from the analyses with the synthetic DD. To better compare our main specification to the synthetic DD we also exclude these observations from the comparison.

Table 3
Estimated treatment effects based on annualized data.

	2018 vs 2020 difference-in-differences	
	(1)	(2) Incl. never-treated cities
%-increase in accidents	9.2***	6.0**
	(2.8)	(2.6)
Mean pre-treatment accidents	1292.1	1154.0
Treated observations	48	48
Total observations	150	188
Cities	75	94

Notes: * p<0.1, ** p<0.05, *** p<0.01. The table shows estimated treatment effects from log-linear specifications (see Section 3). Raw estimates are transformed to semi-elasticities: $100 \cdot (\exp(\hat{\tau}) - 1)\%$. Standard errors are transformed correspondingly. Column 1 relies on yet-to-be-treated observations as controls. Column 2 additionally uses never-treated cities. Standard errors in parentheses are clustered at the city level. All estimates account for period fixed effects and city fixed effects.

set of control cities that are yet to be treated. Choosing an earlier post-treatment year would be disadvantageous because, in most sample cities, scooters were only introduced in 2019. The regression equation is

$$\log \text{accidents}_{it} = \alpha_i + \delta_t + \tau^{\text{ATE}}\text{evertreated}_i \times \text{post}_t + \varepsilon_{it},$$

where evertreated_i measures if city i launched shared e-scooters and post_t indicates observations from 2020. Parameters α_i and δ_t are city and year fixed effects.

Table 3 presents the estimates for the annual DD specification. Similar to the main specification, the control group in column 1 consists of all yet-to-be-treated cities that were not treated by July 2020. The estimated treatment effect of $9.2 \pm 2.8\%$ is qualitatively similar to the average event-study estimate, providing further support for our primary empirical findings.

Furthermore, the findings from heterogeneity analyses in the annual DD framework are similar to our main specification. The largest effects on accidents are in cities with a low share of bike lanes and a larger number of cars per capita. We find a similarly large but insignificant difference based on bike lanes. Additionally, the difference for cars per capita is significant in this specification (appendix table 16).

5.3. E-scooter rollout endogeneity concerns

The rollout of e-scooter services is not random. There are a number of factors that can predict market entry. These factors can be relatively time-invariant city characteristics, such as population and infrastructure. Our analyses reveal that e-scooter services are more likely to be rolled out early in cities with more inhabitants or a better cycling infrastructure (see appendix table 8). In addition, there are also time-varying factors that predict early launches, such as season and regulatory constraints. For instance, roughly half of the launch dates are either in June, July, or August (see appendix figure 3) when the service is attractive to customers. In addition, regulation poses a binding constraint to the timing of e-scooter launches in cities. For example, in Germany, the timing of launches for almost half of the cities coincided with federal regulation in June 2019 that initially allowed e-scooters on public roads (Gebhardt et al., 2021).

Time-invariant factors are accounted for through city-level fixed effects in our analyses. The month-fixed effects account for time-varying but global factors like seasonality. Time-varying predictors, like national regulation or proximity considerations for business expansion, are unlikely to be endogenous to city-level accident trends. In appendix B.2.1, we provide a more detailed discussion of these arguments and of the factors predicting e-scooter launches and argue that these factors are all either accounted for in our analyses through fixed effects or can be reasonably assumed to be unrelated to accident numbers.

Nonetheless, the remainder of this section includes robustness checks to alleviate related endogeneity concerns regarding our empirical approach and findings. Specifically, we discuss results that additionally account for flexible time trends, placebo tests, the sensitivity of our results to excluding the months with the most severe COVID-19 countermeasures, and an IV strategy.

5.3.1. Pre-trends, city-specific linear time trends, and controlling for potential confounders

A threat to the identification of causal effects would be if the city-level timings of launches were correlated with changing trends in accidents or the reporting and coding of accidents. Accordingly, a key identifying assumption of our empirical strategy is the parallel trend assumption, i.e., total traffic accidents in treatment and control cities would have followed a common trend in the absence of e-scooter services. While this assumption cannot be directly tested, it is more plausible if trends between treated and control cities are similar before the introduction of e-scooter-sharing services. The estimated pre-trend coefficients are insignificant and close to zero (see Fig. 2, column 1). Additionally, placebo tests (see Section 5.3.2) further substantiate the point that seasonality in accidents or differential trends are not driving our results.

Besides these insignificant pre-trends, we consider city-specific time trends to be unproblematic as potential unobserved drivers of accidents for two additional reasons. First, our main results are qualitatively robust to the inclusion of a city-specific linear time trend in our statistical model (see appendix B.2.3). Second, our results are robust if we interact time-invariant city characteristics with year dummies as additional controls (see appendix table 13).

5.3.2. Placebo tests on winter-time accidents and launch dates

We conduct two types of placebo tests to corroborate our results. First, we consider winter-time accidents as a sub-group of accidents that would likely show significant estimates if our estimates captured general differential trends in urban traffic policies or behaviors unrelated to scooters. E-scooters are a considerably less attractive transportation mode in cold weather, and companies tend to reduce the number of deployed scooters (Mathew et al., 2019; O'Brien, 2021). Therefore, shared e-scooters likely cause fewer accidents in winter. Consequently, the winter specification in Fig. 2 and in column 5 of appendix table 10 can be interpreted as a placebo test. While treatment effect estimates are large and significant in summer months, they are close to zero and insignificant in winter months. If our results were driven by differential trends in traffic risks (e.g., car-specific road infrastructure, general policy changes, or changes in the recording and reporting standards), the estimates would be less likely to exhibit this seasonality. In line with this analysis, appendix figure 8 shows that the apparent dips in treatment effects disappear if we restrict our event study analysis to non-winter months.

Second, we redo our estimation from Fig. 2, column 1 with placebo launch dates, shifted by 12 and 24 months to the past from the actual city-specific launch dates. If results were driven by non-parallel trends or seasonality not fully captured by the fixed effects, these placebo tests could also indicate significant estimates. These results are shown in appendix figure 9 and indicate no discernible patterns. We view this as reaffirming evidence that the parallel trend assumption is justified and that our empirical strategy sufficiently accounts for seasonality.

5.3.3. COVID-19 pandemic

To address possible concerns that the treatment effect estimates may be driven by developments related to the COVID-19 pandemic, column 6 of appendix table 10 shows the treatment effect across all months except the months with the most relevant COVID-19 countermeasures in the sample countries (March to May 2020 and November 2020 to May 2021). The average treatment effect estimate for the remaining months is $5.7 \pm 2.1\%$. While this estimate is smaller than our main estimate, it is very close to the estimate we obtain when ending the sample in 2020, $5.8 \pm 2.3\%$ (see column 4 appendix table 7). We thus believe that the relatively small difference between estimate excluding COVID-19 months and our main estimate can be explained by the fact that they both rather capture shorter-term effects, which seem to be smaller (see our discussion at the end of Section 4.1 and the results in appendix table 15). So, this suggests our results are not confounded by responses to COVID-19 that are correlated with treatment status.

5.3.4. Instrumental variable analyses

As additional support that endogenous timing of treatment is not driving our findings, we conduct instrumental variable analyses. For this, we use the interactions of four time-invariant city characteristics (population and the three variables used in the heterogeneity analyses above, see appendix table 8) and the number of e-scooter firms that were active in a given month in other cities of the same country as instruments. This analysis, which is carried out in a standard two-way fixed-effects framework—and is thus subject to the discussed caveats related to staggered rollout designs—is discussed and presented in appendix B.2. The instrumental variable analysis qualitatively supports our main conclusions.

5.4. Increasing the size of the control group by using never-treated cities

To address any concerns related to low numbers of control observations, we extend our analysis using never-treated cities. In the preceding analyses, the set of control observations consists of all pre-treatment months for cities where, as of 2021, e-scooters services were announced or introduced. We view these yet-to-be-treated cities as the most comparable counterfactual. Appendix figure 6 presents estimates using an extended set of control cities that includes never-treated cities where no e-scooter firm launched before 2022. Using this sample, the estimated treatment effect is an average increase in total accidents of $4.7 \pm 2.3\%$ (column 2 appendix table 10). We also estimate the annual DD including the never-treated cities in the sample (see column 2 of Table 3). Similarly to our preferred specification on the monthly panel data, this modification leads to a decrease in the estimated effect to $6.0 \pm 2.6\%$. In sum, our results are qualitatively robust but quantitatively smaller if we include never-treated cities in the control group.

Since the sample that includes the never-treated cities is larger, it may allow for a more precise estimation of the counterfactual. However, never-treated cities may be less comparable to (eventually) treated cities. Indeed, never-treated cities as of December 2021 are regionally clustered: 15 out of the 19 never-treated cities are German—9 of which are from one state (North Rhine-Westphalia). Thus, including these cities skews the counterfactual towards North Rhine-Westphalia. In addition, most of the never-treated cities are part of larger metropolitan areas, straddling the line between urban and suburban areas, which are less comparable to the other cities in the sample. We find evidence of statistically significant different pre-trends of never-treated cities (see appendix B.2.2), which suggests that the parallel trends assumption might be less valid for the never-treated control group. We thus consider the estimates in Fig. 2 more reliable.

An alternative way to address the low number of control cities towards the end of the panel is to rely on yet-to-be-treated cities and shorten the time frame of the analysis by ending earlier. Re-estimating the specification from column 1 on a sample ending in 2020 yields an estimate of $5.7 \pm 2.1\%$ (appendix table 7, column 4). Appendix figure 3 provides an overview of treatment dates, which shows the remaining yet-to-be-treated cities in each period. In summation, the still significant results from including never-treated cities as control observations or ending the sample earlier may reduce concerns that the main results are an apparition of the remaining small number of control observations.

6. Discussion

We provide evidence that the rollout of shared e-scooter services in large cities significantly increased traffic accidents involving personal injuries. In their 2018 road safety report, the OECD (2018) reports that the estimated socio-economic costs of road traffic accidents exceed €500bn for EU member states alone, which is equivalent to around 3% of the EU's GDP. In 2019, an average traffic accident involving personal injuries in Germany was associated with economic costs of around €61,000, including costs of €16,301 for damage to property and €44,778 for personal injuries (Bundesanstalt für Straßenwesen, 2021a,b).[9] Assuming these costs per accident apply to all six sample countries, the estimated effect in our main specification ($8.2 \pm 2.9\%$) would thus imply additional socio-economic costs of around €466,186 per month and €5.6 m per year for the average sample city with 93.2 monthly accidents before treatment (see appendix table 10).[10]

In addition, a recent study conducted at the University Hospital of Essen in Germany (Meyer et al., 2023) suggests that a large share of hospital-treated e-scooter injuries are not reported to the police. By focusing on police-reported accidents, our data is more likely to record accidents involving automobiles than accidents involving cyclists and includes relatively more accidents with severe injuries and higher costs (Langley et al., 2003). Findings from retrospective studies examining medical records further show that e-scooter-related accidents are often associated with serious injuries to the head and upper extremities with a substantial proportion of major trauma injuries (Trivedi et al., 2019a,b; Badeau et al., 2019; Moftakhar et al., 2021; Lavoie-Gagne et al., 2021). In sum, these findings imply that our estimated effects are associated with significant socioeconomic costs that have to be weighed against the potential benefits of adding shared e-scooter services to the urban transportation landscape.

Our main results do not differentiate by severity of injuries. However, understanding the relative severity of injuries compared to pre-existing urban accidents is essential for comprehending the social costs imposed by shared e-scooter services. If the increase in accidents primarily involves cases with slight injuries, the overall social harm to society would be limited, given that accidents resulting in severe injuries or fatalities carry the most substantial social costs (Wijnen et al., 2017). We do not suspect that e-scooter-related accidents are considerably less severe than other urban accidents. First, police-reported accidents indirectly caused by e-scooters are likely similar to pre-existing urban accidents. Second, the clinical studies discussed above suggest that police-reported accidents directly involving e-scooters often involve severe injuries.

As an explorative analysis of the severity of e-scooter-related accidents, we investigate whether the average police-reported accident in a city after introducing e-scooters is significantly more or less severe. Appendix B.3.1 reports estimates of the treatment effects on the percentage of accidents involving only slight injury as an outcome variable.[11] We find that 85% of accidents involving personal injuries are classified as involving slight injuries and that there is no significant change after the introduction of e-scooters (95% confidence interval: -1.8 to $+0.8$%-points), see appendix table 14. While possibly lacking the power to detect very small changes in the composition of accidents, these estimates suggest that, while the total number of accidents changed with the introduction of e-scooters, the composition of the accidents in terms of severity did not change.

From our heterogeneity analysis, estimated effects are considerably larger in cities with poor separated cycling infrastructure and rely more on cars. This is important from an urban planning policy perspective, especially given that earlier studies suggest better bicycle infrastructure may be associated with more frequent and longer e-scooter trips (Caspi et al., 2020; Laa and Leth, 2020). Paired with our findings, this suggests that improving separated infrastructure may curb negative effects while simultaneously encouraging e-scooter usage. As highlighted in the results section, the effects of infrastructure are not causally identified. Investigating what additional measures those cities that have well-developed infrastructure are taking to prevent accidents is worth additional study. Identifying behavioral mechanisms that cause this increase in accidents goes beyond the scope of this paper. However, related medical studies suggest that driving under the influence of alcohol or drugs, reckless driving, and discarded e-scooters are important drivers (Blomberg et al., 2019; Sikka et al., 2019; Stigson et al., 2021; Lavoie-Gagne et al., 2021).

Our analyses focus on the extensive margin of treatment, i.e., whether or not shared e-scooters are available in a city. This is the most relevant dimension for two reasons. First, while there are examples of cities that restricted the maximum number of e-scooters (e.g., Bern, Switzerland; Innsbruck, Austria), the public debate usually revolves around the extensive margin, i.e., whether to ban shared e-scooters from a city's transportation mix or not (CNN, 2019). Second, the extensive margin analysis allows for an easy interpretation as a semi-elasticity and is less likely to be subject to measurement error and endogeneity issues. Even if exact data on the number of deployed e-scooters were available, e-scooter firms may continuously adapt to accidents and changing traffic conditions, raising concerns about reverse causality. Contrarily extensive margin estimates identify the net effect encompassing possible endogenous scaling decisions of suppliers or variation in service take-up by consumers. In that sense, our estimates can

[9] According to the German federal research institute, Bundesanstalt für Straßenwesen (2021b), 300,143 traffic accidents involving personal injuries were recorded in Germany in 2019. The total personal damage costs amounted to €13.44bn, which implies average personal damage costs of around €44,778 per accident involving personal injuries.

[10] The true costs may be higher, as Germany does not account for under-reporting in cost calculations (Wijnen et al., 2017), or lower as the cost estimates also include non-urban traffic accidents.

[11] To construct these percentages, we rely on the classifications by the local statistical agencies, as detailed in appendix A. Four countries directly report the number of accidents involving severe or slight personal injuries and death, and we estimate accident numbers for Austria and Norway based on the number of victims by injury severity.

be interpreted as intent-to-treat effects of offering e-scooter services. Studying how different variants (e.g., intensity, technology) of introducing e-scooters imply different effects is an interesting area for future study.[12]

Our findings should not be interpreted as long-run effects. Effects on traffic accidents may decrease in the future due to safer vehicles, greater experience of users of e-scooters, or changes in the behavior of other traffic participants. They may also increase. While our analyses account for substitution effects between modes of transportation, including walking (police are required to report pedestrian-vehicle accidents), we do not observe the total number of trips within a city. If the introduction of shared e-scooters considerably increases the mobility of citizens in treated cities, i.e., people travel more because e-scooters are available, the social costs, implied by documented effects, should be discounted by the higher number of trips in a cost–benefit analysis. A recent survey of e-scooter users in Paris, however, suggests that the vast majority did not increase their total mobility (Christoforou et al., 2021). Two recent literature reviews further indicate that e-scooter users predominantly substitute from public transport, walking, and cycling (Orozco-Fontalvo et al., 2022). However, the extent of substitution from other modes of transportation is contingent upon various factors such as the specific characteristics of the city and its regulatory framework (Wang et al., 2023). We see long-term effects and ways in which e-scooters may change traffic habits as an interesting area for future study.

Our analysis does not imply a comparative statement of the safety risks between different modes of transport and is agnostic to what kind of road user is directly responsible for the increased accidents. Consequently, our results cannot be interpreted as recommendations against the inclusion of shared e-scooters in urban transport landscapes, in particular, compared to automobiles that, due to their size and speed, likely pose the relatively largest urban safety risk. For instance, past studies find that cars and other large motorized vehicles contribute to other road users' deaths at rates 3–6 times higher than bicycles per mile driven (Aldred et al., 2021; Scholes et al., 2018).

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgments

We thank Kirill Borusyak, Fabrizio Colella, Markus Eyting, Nicolas Koch, Patrick Schmidt, and Matthias Schündeln for constructive comments and suggestions. We further thank the companies Tier and Voi for providing data on launch months per city. Map data copyrighted OpenStreetMap contributors and available from www.openstreetmap.org.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.euroecorev.2023.104593.

References

Abduljabbar, R.L., Liyanage, S., Dia, H., 2021. The role of micro-mobility in shaping sustainable cities: A systematic literature review. Transp. Res. D 92, 102734.
Abouk, R., Adams, S., 2013. Texting bans and fatal accidents on roadways: Do they work? Or do drivers just react to announcements of bans? Am. Econ. J. Appl. Econ. 5 (2), 179–199.
Aldred, R., Johnson, R., Jackson, C., Woodcock, J., 2021. How does mode of travel affect risks posed to other road users? An analysis of English road fatality data, incorporating gender and road type. Inj. Prev. 27 (1), 71–76.
Arkhangelsky, D., Athey, S., Hirshberg, D.A., Imbens, G.W., Wager, S., 2021. Synthetic difference-in-differences. Amer. Econ. Rev. 111 (12), 4088–4118.
Badeau, A., Carman, C., Newman, M., Steenblik, J., Carlson, M., Madsen, T., 2019. Emergency department visits for electric scooter-related injuries after introduction of an urban rental program. Am. J. Emerg. Med. 37 (8), 1531–1533.
Barreto, Y., Neto, R.d.M.S., Carazza, L., 2021. Uber and traffic safety: Evidence from Brazilian cities. J. Urban Econ. 123, 103347.
Barrios, J.M., Hochberg, Y.V., Yi, H., 2023. The cost of convenience: Ridehailing and traffic fatalities. J. Oper. Manage. 69 (5), 823–855.
Bauernschuster, S., Rekers, R., 2022. Speed limit enforcement and road safety. J. Public Econ. 210, 104663.
Berger, T., Chen, C., Frey, C.B., 2018. Drivers of disruption? Estimating the Uber effect. Eur. Econ. Rev. 110, 197–210.
Blomberg, S.N.F., Rosenkrantz, O.C.M., Lippert, F., Christensen, H.C., 2019. Injury from electric scooters in Copenhagen: A retrospective cohort study. BMJ Open 9 (12), e033988.
Borusyak, K., 2023. did_imputation: Stata Module to Perform Treatment Effect Estimation and Pre-Trend Testing in Event Studies. Statistical Software Components, Boston College Department of Economics.
Borusyak, K., Jaravel, X., Spiess, J., 2023. Revisiting event study designs: Robust and efficient estimation. arXiv preprint arXiv:2108.12419.
Brazil, N., Kirk, D.S., 2016. Uber and metropolitan traffic fatalities in the United States. Am. J. Epidemiol. 184 (3), 192–198.
Bundesanstalt für Straßenwesen, 2021a. Verkehrs- und Unfalldaten. https://www.bast.de/DE/Publikationen/Medien/VU-Daten/VU-Daten.pdf?_blob=publicationFile. (Accessed 8 April 2022).
Bundesanstalt für Straßenwesen, 2021b. Volkswirtschaftliche Kosten von Straßenverkehrsunfällen in Deutschland. https://www.bast.de/DE/Statistik/Unfaelle/volkswirtschaftliche_kosten.pdf?_blob=publicationFile. (Accessed 8 April 2022).

[12] To provide some suggestive evidence on the intensive margin effects, we estimate the effect of the launch of one additional e-scooter company in a city on traffic accidents (see more details in appendix B.1.3). Accidents increased on average by $1.0 \pm 0.5\%$ for each additional company that launched in a city (appendix table 6). While these treatment intensity estimates align with our main findings, the estimates should be cautiously interpreted because estimation is carried out in a TWFE framework, subject to the discussed biases, and causal identification requires even stricter identifying assumptions when treatment intensities are varying.

Callaway, B., Sant'Anna, P.H., 2021. Difference-in-differences with multiple time periods. J. Econometrics 225 (2), 200–230.
Caspi, O., Smart, M.J., Noland, R.B., 2020. Spatial associations of dockless shared e-scooter usage. Transp. Res. D 86, 102396.
Choron, R.L., Sakran, J.V., 2019. The integration of electric scooters: Useful technology or public health problem? Am J Public Health 109 (4), 555–556.
Christoforou, Z., de Bortoli, A., Gioldasis, C., Seidowsky, R., 2021. Who is using e-scooters and how? Evidence from Paris. Transp. Res. D 92, 102708.
CNN, 2019. E-scooters suddenly appeared everywhere, but now they're riding into serious trouble. (J. Buckley). https://edition.cnn.com/travel/article/electric-scooter-bans-world. (Accessed 20 April 2022).
Cunningham, S., 2021. Causal Inference: The Mixtape. Yale University Press.
De Chaisemartin, C., d'Haultfoeuille, X., 2020. Two-way fixed effects estimators with heterogeneous treatment effects. Amer. Econ. Rev. 110 (9), 2964–2996.
De Chaisemartin, C., d'Haultfoeuille, X., 2022. Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. Econom. J. utac017.
Eurostat, 2022. Transport - cities and greater cities (urb_ctran). https://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=urb_ctran. (Accessed 14 February 2022).
Forum of European Road Safety Research Institutes, 2020. E-scooters in Europe: Legal status, usage and safety - results of a survey in FERSI countries. https://fersi.org/wp-content/uploads/2020/09/FERSI-report-scooter-survey.pdf. (Accessed 18 July 2022).
Gebhardt, L., Wolf, C., Seiffert, R., 2021. 'I'll take the e-scooter instead of my car''–The potential of e-scooters as a substitute for car trips in Germany. Sustainability 13 (13), 7361.
Ghanem, D., Sant'Anna, P.H., Wüthrich, K., 2022. Selection and parallel trends. arXiv preprint arXiv:2203.09001.
Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. J. Econometrics 225 (2), 254–277.
Gössling, S., 2020. Integrating e-scooters in urban transportation: Problems, policies, and the prospect of system change. Transp. Res. D 79, 102230.
Greenwood, B.N., Wattal, S., et al., 2017. Show me the way to go home: An empirical investigation of ride-sharing and alcohol related motor vehicle fatalities. MIS Q. 41 (1), 163–187.
Hall, J.D., Palsson, C., Price, J., 2018. Is Uber a substitute or complement for public transit? J. Urban Econ. 108, 36–50.
Heineke, K., Kloss, B., Möller, T., Scurtu, D., 2022. How the pandemic has reshaped micromobility investments. McKinsey Center Future Mobil. https://www.mckinsey.com/features/mckinsey-center-for-future-mobility/mckinsey-on-urban-mobility/how-the-pandemic-has-reshaped-micromobility-investments. (Accessed 1 September 2022).
Hollingsworth, J., Copeland, B., Johnson, J.X., 2019. Are e-scooters polluters? The environmental impacts of shared dockless electric scooters. Environ. Res. Lett. 14 (8), 084031.
Jacobsen, P.L., 2003. Safety in numbers: more walkers and bicyclists, safer walking and bicycling. Inj. Prev. 9 (3), 205–209.
James, O., Swiderski, J., Hicks, J., Teoman, D., Buehler, R., 2019. Pedestrians and e-scooters: An initial look at e-scooter parking and perceptions by riders and non-riders. Sustainability 11 (20), 5591.
Kelso, N.V., Patterson, T., 2010. Introducing Natural Earth data-naturalearthdata. com. Geogr. Tech. Special Issue 2010, 82–89.
Kraus, S., Koch, N., 2021. Provisional COVID-19 infrastructure induces large, rapid increases in cycling. Proc. Natl. Acad. Sci. 118 (15), e2024399118.
Küster, F., Colli, E., Žganec, M., 2022. The State of National Cycling Strategies in Europe (2021). European Cyclists' Federation, https://ecf.com/files/reports/national-cycling-strategies-in-europe-2021. (Accessed 12 September 2022).
Laa, B., Leth, U., 2020. Survey of E-scooter users in Vienna: Who they are and how they ride. J. Transp. Geogr. 89, 102874.
Langley, J.D., Dow, N., Stephenson, S., Kypri, K., 2003. Missing cyclists. Inj. Prev. 9 (4), 376–379.
Lavoie-Gagne, O., Siow, M., Harkin, W., Flores, A.R., Girard, P.J., Schwartz, A.K., Kent, W.T., 2021. Characterization of electric scooter injuries over 27 months at an urban level 1 trauma center. Am. J. Emerg. Med. 45, 129–136.
Mathew, J.K., Liu, M., Bullock, D.M., 2019. Impact of weather on shared electric scooter utilization. In: 2019 IEEE Intelligent Transportation Systems Conference. pp. 4512–4516.
Meyer, H.-L., Kauther, M.D., Polan, C., Abel, B., Vogel, C., Mester, B., Burggraf, M., Dudda, M., 2023. E-Scooter-, E-Bike-und Fahrradverletzungen im gleichen Zeitraum–eine prospektive Vergleichsstudie eines Level-1-Traumazentrums. Die Unfallchirurgie 126, 208–217.
Moftakhar, T., Wanzel, M., Vojcsik, A., Kralinger, F., Mousavi, M., Hajdu, S., Aldrian, S., Starlinger, J., 2021. Incidence and severity of electric scooter related injuries after introduction of an urban rental programme in Vienna: a retrospective multicentre study. Arch. Orthop. Trauma Surg. 141 (7), 1207–1213.
Namiri, N.K., Lui, H., Tangney, T., Allen, I.E., Cohen, A.J., Breyer, B.N., 2020. Electric scooter injuries and hospital admissions in the United States, 2014–2018. JAMA Surg. 155 (4), 357–359.
O'Brien, O., 2021. Winter is coming: European shared e-scooter update. https://zagdaily.com/trends/winter-is-coming-european-shared-e-scooter-update/. (Accessed 20 April 2022).
OECD, 2018. Road Safety. Annual Report, International Transport Forum, Organisation for Economic Co-operation and Development.
Orozco-Fontalvo, M., Llerena, L., Cantillo, V., 2022. Dockless electric scooters: A review of a growing micromobility mode. Int. J. Sustain. Transp. 1–17.
Peltzman, S., 1975. The effects of automobile safety regulation. J. Polit. Econ. 83 (4), 677–725.
Reynolds, C.C., Harris, M.A., Teschke, K., Cripton, P.A., Winters, M., 2009. The impact of transportation infrastructure on bicycling injuries and crashes: A review of the literature. Environ. Health 8 (1), 1–19.
Roth, J., Sant'Anna, P.H., Bilinski, A., Poe, J., 2023. What's trending in difference-in-differences? A synthesis of the recent econometrics literature. J. Econometrics 235 (2), 2218–2244.
Sanders, R., Branion-Calles, M., Nelson, T., 2020. To scoot or not to scoot: Findings from a recent survey about the benefits and barriers of using E-scooters for riders and non-riders. Transp. Res. A 139, 217– 227.
Scholes, S., Wardlaw, M., Anciaes, P., Heydecker, B., Mindell, J.S., 2018. Fatality rates associated with driving and cycling for all road users in great britain 2005–2013. J. Transp. Health 8, 321–333.
Shaheen, S., Cohen, A., 2019. Shared Micromoblity Policy Toolkit: Docked and Dockless Bike and Scooter Sharing. Research Report, Institute of Transportation Studies, Berkeley.
Shinar, D., Valero-Mora, P., van Strijp-Houtenbos, M., Haworth, N., Schramm, A., De Bruyne, G., Cavallo, V., Chliaoutakis, J., Dias, J., Ferraro, O.E., et al., 2018. Under-reporting bicycle accidents to police in the COST TU1101 international survey: Cross-country comparisons and associated factors. Accid. Anal. Prev. 110, 177–186.
Sikka, N., Vila, C., Stratton, M., Ghassemi, M., Pourmand, A., 2019. Sharing the sidewalk: A case of e-scooter related pedestrian injury. Am. J. Emerg. Med. 37 (9), 1807.e5–1807.e7.
Stigson, H., Malakuti, I., Klingegård, M., 2021. Electric scooters accidents: Analyses of two Swedish accident data sets. Accid. Anal. Prev. 163, 106466.
Sun, L., Abraham, S., 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. J. Econometrics 225 (2), 175–199.
Trivedi, B., Kesterke, M.J., Bhattacharjee, R., Weber, W., Mynar, K., Reddy, L.V., 2019a. Craniofacial injuries seen with the introduction of bicycle-share electric scooters in an urban setting. J. Oral Maxillofac. Surg. 77 (11), 2292–2297.
Trivedi, T.K., Liu, C., Antonio, A.L.M., Wheaton, N., Kreger, V., Yap, A., Schriger, D., Elmore, J.G., 2019b. Injuries associated with standing electric scooter use. JAMA Netw. Open 2 (1), e187381.
Van Benthem, A., 2015. What is the optimal speed limit on freeways? J. Public Econ. 124, 44–62.
Wang, K., Qian, X., Fitch, D.T., Lee, Y., Malik, J., Circella, G., 2023. What travel modes do shared e-scooters displace? A review of recent research findings. Transp. Rev. 43 (1), 5–31.

Wang, C., Quddus, M.A., Ison, S.G., 2013. The effect of traffic and road characteristics on road safety: A review and future research direction. Saf. Sci. 57, 264–275.

Wijnen, W., Weijermars, W., Van den Berghe, W., Schoeters, A., Bauer, R., Carnis, L., Elvik, R., Theofilatos, A., Filtness, A., Reed, S., Perez, C., Martensen, H., 2017. Crash cost estimates for European countries. https://ec.europa.eu/research/participants/documents/downloadPublic?documentIds=080166e5b1e92ba3&appId=i. (Accessed 10 April 2022).

Yang, H., Ma, Q., Wang, Z., Cai, Q., Xie, K., Yang, D., 2020. Safety of micro-mobility: Analysis of e-scooter crashes by mining news reports. Accid. Anal. Prev. 143, 105608.