Supplementary online appendix

A   Detailed data descriptions

A.1   Traffic accidents data

We are interested in traffic accidents on the city level. However, data on accidents are reported at different geographic levels of aggregation in different countries. Austria, Germany, and Switzerland's accident data align with city boundaries (see details below). In Finland, Norway, and Sweden, the accident data is reported at the municipality level, which does not always align with city boundaries. Details on countries' traffic accident data and specific cases that were not straightforward are listed and discussed below.

   We limited our study to cities of at least 100,000 inhabitants. A municipality in Scandinavian countries may include multiple localities, fractions of localities, and very remote rural areas. To harmonize the unit of observation across countries, we only consider Scandinavian municipalities with a population over 100,000 that also include a locality of at least 100,000 inhabitants. We implement this restriction based on the latest public population statistics for the respective countries. Large urban areas (mainly Helsinki, Stockholm, and Oslo) span multiple periphery municipalities in addition to a core municipality. Because our study is focused on e-scooter service impacts in large cities and to avoid the double-counting of the same urban areas, we exclude "suburban" municipalities, which differ in terms of traffic movements and infrastructure. The following municipalities met the population-based inclusion criterion but fall into our definition of suburbs and thus were excluded from our analyses: Norway: Bærum (a suburb of Oslo). Sweden: Huddinge and Nacka (suburbs of Stockholm). Finland: Espoo and Vantaa (suburbs of Helsinki).

A.1.1   Austria

Aggregated data on road traffic accidents is published by Statistics Austria on a quarterly basis. Statistics Austria provided us with the number of monthly accidents involving personal injuries, segmented by "politische Bezirke" (political districts), which is equivalent to the respective (single) municipality for Austrian cities above 100,000 inhabitants. In 2020, a more detailed table section was published (Statistics Austria, 2021d). Our data is equivalently structured as Table 127 of their publication from 2020 (Statistics Austria, 2021d). According to the definition by Statistics Austria (Statistics Austria, 2021c), a road traffic accident involving personal injury occurs when one or more persons are injured or killed as a result of traffic on public roads, where at least one moving vehicle was involved. Austria collects accident data on moving vehicles (i.e., a means of transport for use on roads, including cars, bicycles, and e-scooters) and vehicle-like means of transport (roller-blades, and skateboards). By law, all participants or witnesses of a road-traffic accident must report injuries or deaths to the police, who issue an electronic traffic accident report received by Statistics Austria with a high degree of completeness. Serious injuries result "in an inability to work or health problems for more than 24 days". Injuries that do not meet this criterion are classified as slightly injured. More information on definitions, comments, methods, and data quality is available from Statistics Austria (Statistics Austria, 2021c).

A.1.2   Finland

We obtain data from Statistics Finland which publishes the official statistics on road traffic accidents involving personal injury by municipalities and month. Data is published monthly as preliminary data and annually as final data (Statistics Finland, 2022b). An accident involving personal injuries is defined as an event "that has taken place in an area intended for public transport or generally used for transport and in which at least one of the involved parties has been a

moving vehicle" resulting in death or personal injury "which require medical care or observation in hospital, treatment at home (sick leave) or surgical treatment, such as stitches." Moving vehicles include micromobility transport, like bicycles and e-scooters. Serious injuries require medical care and "are classified as serious in accordance with the AIS Abbreviated Injury Scale". The authors define slight injuries as the remainder of non-serious personal injury accidents. Data is reported by the police through the police information system to Statistics Finland, and data for each month are updated three months after its ending. Deaths are reported with nearly 100% accuracy, while accidents with injuries are around 30%. The worst coverage is for cyclists injured in single road user accidents. Detailed documentation of the data is available (Statistics Finland, 2022a).

Accidents are reported at the municipality level. To make the data comparable to other countries, we further make use of the definition of urban settlements provided by Statistics Finland (Statistics Finland, 2022c). We only consider municipalities with at least 100,000 residents that also include an urban settlement with more than 100,000 inhabitants. In general, an urban settlement may be split between several municipalities. In our paper, we focus on the respective municipalities that align best with e-scooter service areas. Accordingly, we exclude the municipalities of Espoo and Vantaa, which are part of the urban area of Helsinki. In total, there are six Finnish municipalities that we consider: Helsinki, Tampere, Turku, Oulu, Jyväskylä, and Lahti. The vast majority of the population in these municipalities lives in the largest urban settlement. Lahti has the lowest share with 87.4% and Helsinki has the highest share with 97.7% (Statistics Finland, 2020).

A.1.3 Germany

The road traffic accident statistics in Germany include all accidents resulting from vehicular traffic on public roads (and squares) recorded by the police in which at least one person was personally injured (Statistisches Bundesamt, 2017, 2022c). Serious injury require immediate hospitalization lasting at least 24 hours. Slight injuries are "any other person injured in a road crash". Police are required to compile a report for all accidents they become aware of, however, the law only requires the reporting of accidents with death or serious injuries (Statistisches Bundesamt, 2022b) (henceforth Fachserie 8). Accidents with only property damage or only involving pedestrians are not included. The classification of 'moving vehicles' includes micromobility modes such as bicycles, e-scooters, and skateboards. The Federal Statistical Office of Germany publishes a monthly list of accidents in 100 cities (Fachserie 8). That list is unchanged since 2008 and contains all cities above 100,000 inhabitants (except Gütersloh) and a non-random subset of smaller cities. There are cases where traffic accident reports were not provided to the statistical agency (by the police) in time, which means that these accidents are not included in the monthly accident statistics. These accidents are, however, included in the annually published Unfallatlas (Statistisches Bundesamt, 2022a), which covers most German states. For 2020, all of Germany is covered. For 2019, the state of Mecklenburg-Vorpommern is omitted. For 2018, several states are omitted. For our analyses, we rely on the data from Fachserie 8 for all cities where it is available. We replace these numbers with numbers from the Unfallatlas for 9 city-months, where—based on stark discrepancies in trend—we deem the original data unreliable (see table 4 for the details). Accident data for Gütersloh refers to (Statistisches Bundesamt, 2022a) and thus only covers the period from 2018 onwards. Unfortunately, we cannot rely only on the Unfallatlas, as within-city cross-temporal comparisons are central to our analysis and the Unfallatlas has no data for several German states in 2018.

The city of Herne was omitted in the June 2017 issue of Fachserie 8. The city of Göttingen was omitted from the December 2016 issue. Neither are available from the Unfallatlas. In order to be able to retain the observations from Herne and Göttingen in a balanced panel, we impute accident numbers for the missing month by taking the average of the previous and succeeding months.

Table 4: Observations for which the data from the *Unfallatlas* (Statistisches Bundesamt, 2022a) was chosen over the data from *Fachserie 8* (Statistisches Bundesamt, 2022b)

City	Year	Month	Accidents (Unfallatlas)	Accidents (Fachserie 8)
Halle/Saale	2017	6	93	22
Halle/Saale	2017	7	63	3
Rostock	2020	5	36	1
Rostock	2020	6	52	5
Rostock	2020	7	41	0
Rostock	2020	8	58	1
Rostock	2020	9	51	5
Rostock	2020	10	39	1
Wiesbaden	2019	7	84	14

A.1.4 Norway

Monthly accident data at the municipality level is available from Statistics Norway. The data is limited to accidents reported to the police that "involve at least one vehicle, and that have taken place on public or private roads, streets or places open to general traffic" (Statistics Norway, 2022). Moving vehicles include small electric motor vehicles, such as e-scooters, and non-motorized vehicles, such as bicycles. Only accidents that resulted in at least one slight injury are counted, where the minimum threshold, slight injury, is defined as "Minor fractures, scratches, etc. Hospitalization is not required." Police reports are electronically submitted to Statistics Norway. Preliminary accident figures are published monthly, however, numbers are not finalized until May of the following year.

Accidents are reported at the municipal level, which does not always correspond with city boundaries. Data for population of urban areas, available from Statistics Norway, was used for our exclusion criteria. An urban area is determined by the positioning of buildings and the number of inhabitants, and is independent of administrative boundaries (Statistics Norway, 2021b). In total, we consider four corresponding urban areas and municipalities with more than 100,000 inhabitants: Bergen, Oslo, Stavanger, and Trondheim. The vast majority of the population (at least 97%) in these municipalities also live in the corresponding urban area. The urban area of Fredrikstad/Sarpsborg has slightly above 100,000 inhabitants but is not included in our data as it spans two different municipalities with significantly less than 100,000 inhabitants each. Furthermore, Bærum, which has over 100,000 residents, is excluded as a suburb of Oslo.

A.1.5 Sweden

Road traffic accidents with personal injury are compiled by the Swedish Transport Agency, Transportstyrelsen, based on accidents reported by the police involving death or injury. The police are obliged to report all accidents with physical injuries, and accidents with injuries must be reported to the police by participants in a certain time-frame. Injury assessment is determined by the police, usually on-site. "An injured person who is not seriously injured is slightly injured". "A person who has suffered a fracture, crushing injury, laceration, serious cut injury, concussion or internal injury is considered seriously injured", or if they are expected to be hospitalized. Only accidents involving moving vehicles (such as a car, bicycle, or e-scooter) in a road traffic area are counted. Transportstyrelsen provided the accident data at the monthly municipality level for 2018-2021, but could not provide data for 2016 and 2017 due to staffing constraints.

We use population data on localities from Statistics Sweden (2021a) for our exclusion criteria.

They define an urban area/locality as a concentration of buildings not separated by more than 200 meters and with at least 200 inhabitants (Statistics Sweden, 2022b). In total, there are eleven municipalities with at least 100,000 residents that have localities with more than 100,000 inhabitants. We exclude the municipalities of Huddinge and Nacka that contains part of the locality of Stockholm, which is already included through the homonymous Stockholm municipality where the majority of the locality reside. Traffic accident data for the remaining nine municipalities are included. Again, the vast majority of these municipalities' population reside in the largest locality with shares ranging from 70% for Linköping to 96% for Stockholm.

A.1.6 Switzerland

Road traffic accident data for Switzerland is published annually by the Bundesamt für Strassen (ASTRA) via the federal geoportal ("Geoportal des Bundes"). We use data from the traffic accident map, which shows all recorded accidents involving personal injury since 2011 geographically according to specific topics, including date and severity of injuries (Bundesamt für Strassen ASTRA, 2022). We use the municipality code, provided for each accident, to aggregate the number of accidents. The municipality code uniquely identifies a respective city, and the municipal boundaries correspond closely to city boundaries for cities with over 100,000 people.

According to the official definition, a road traffic accident is defined as an unforeseen event in a public traffic area that results in property damage and/or personal injury and involves at least one vehicle or vehicle-like device, such as a skateboard or scooter. Participants are required to call emergency services if there is an injury. A slightly injured person is defined as "anyone with a minor injury, such as superficial skin injury without significant blood loss or a slight restriction of movement", that can leave the crash site unaided. A serious injury is a National Advisory Committee for Aeronautics code 3 or higher, equating to at least an "impairment that prevents normal activities (e.g. unconsciousness and open bone fractures)". For better comparability with other countries, we only consider accidents involving personal injuries. Data is collected by the police in accordance with an accident recording protocol and stored and maintained centrally in the accident recording system of the Federal Roads Office (Bundesamt für Strassen ASTRA, 2018).

A.2 E-scooters service rollout dates

Figure 3 provides an overview of the recorded launch dates for different cities in our sample until December 2021 that also serve as a basis for figure 1, the map of launches, in the main text. Bars indicate the introduction of the first shared e-scooter service in the respective city, while black dots indicate the timing of additional scooter firms' rollouts. Darker shaded areas illustrate the first year after the rollout of the first shared e-scooter service. Numbers in the black dots show the total number of scooter firms that have launched in a city at each point in time. The vertical black line illustrates the end of our traffic accidents data observation period. The launch dates are hand-collected and verified from official press releases, the firms' social media channels, and websites of e-scooter providers, local newspapers, or municipalities. A file with launch dates for considered providers by city and corresponding sources can be found in the online appendix.

Tier and Voi provided us with launch dates for different cities directly. Cities, where the operator Voi stopped operations by 2022, were not included in their list. Furthermore, the dates in provided lists were referring to the latest relaunch dates for cities where services were paused for some time. We included the initial launch date for those cities based on our hand-collected data, after carefully double-checking our sources. The cities are: Linz (Austria), Ingolstadt (Germany), Erfurt (Germany), and Potsdam (Germany). In addition, there are some cases of cities for which the lists provided by Voi and Tier included earlier start dates. These dates seem to refer to the dates when Voi finalized the decision or initiated the launch—not when the first scooters were

Figure 3: Launch dates of first scooter service by city until 12/2021
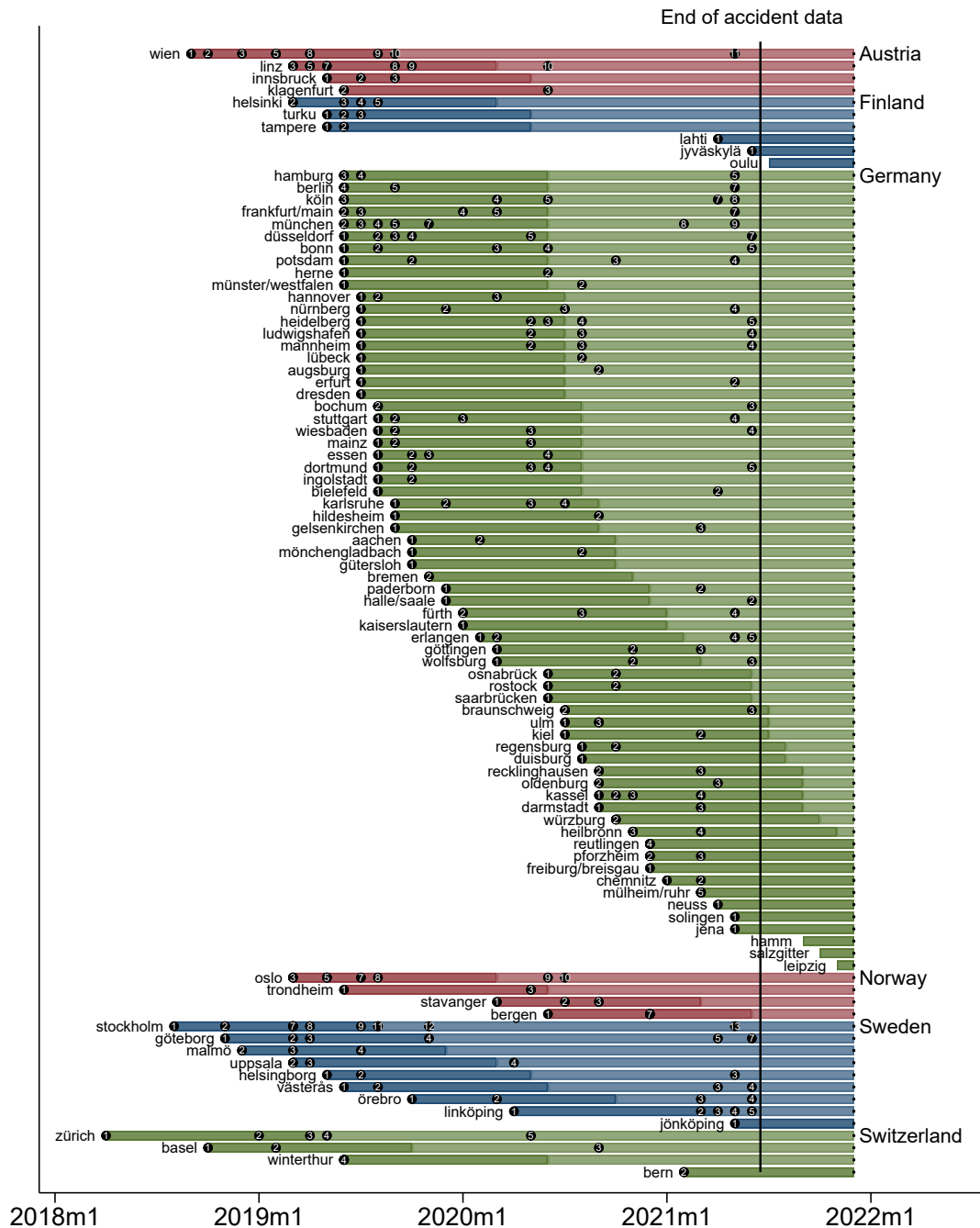
Notes: Bars indicate periods after the introduction of the first scooter service. Black dots indicate the timing of market entry of additional scooter firms until June 2021 (the end of the accident data time frame). Numbers in the black dots indicate the total number of scooter firms that have launched in a city at each point in time. Darker shaded areas indicate the first 12 months after the introduction of the first scooter service.

brought to a city. As we are interested in the timing of the actual rollouts, we carefully cross-checked and looked for additional sources if there were discrepancies. Launch dates may deviate from the provided lists if reliable public sources convincingly confirmed a different effective rollout of e-scooter services in a respective city (links to public source can be found in the data file, which is in the online appendix). Deviations from the lists provided by Voi and Tier do not affect our main results, as Tier and Voi were not the first providers in the relevant cities.

Rostock was late to adopt e-scooters. There are, however, some reports of Voi being active in Rostock already in 2019, but according to an event advertised on the provider's and municipality's social media channels, this was just a weekend trial (Voi Technology, 2019b). We thus use the effective rollout dates according to local newspapers and press releases by the providers (Bird and Moin were the first two providers, with Lime and Tier joining only after the end of our panel of traffic accident observations).

A.3 City-level variables data

A.3.1 Share of bike lanes

The ratio of separated bicycle infrastructure to road network length for cars is compiled with data from *OpenStreetMap* obtained in March 2022 (Gilardi and Lovelace, 2021). To determine road network length, distance is calculated from the geometries of *OpenStreetMap* objects where the `highway` key is tagged `residential, primary, secondary, tertiary` (including respective link categories) or `motorway, trunk` (link excluded). Similarly, separated bicycle infrastructure includes objects from the road network set where bicycles are separated or have priority, specifically objects with additional tags `cycleway=lane`, `cycleway=share_busway`, `bicycle=designated` and `cyclestreet`. In addition, all objects where `highway` is tagged `cycleway` or `livingstreet` are included. Objects with `highway` tagged `footway`, `path`, or `track` are included if bicycles are explicitly allowed with the additional tags `bicycle=designated` or `bicycle=yes`. We restrict and crop *opensreetmap.org* spatial objects (Pebesma, 2018) with high-resolution political boundaries based on the unit of analysis of the traffic accident data, i.e., cities for Germany, municipalities for Finland, Sweden, Switzerland, and Norway, and districts for Austria (Federal Agency for Cartography and Geodesy, 2022; Federal Office of Topography swisstopo, 2022; GEONORGE, 2022; National Land Survey of Finland, 2022; Statistics Austria, 2022; Statistics Sweden, 2022a).

A.3.2 Registered cars per capita

The variable refers to the number of registered cars by city per 1,000 inhabitants in 2018. Data on all cities, with the exception of Austria and Gütersloh (Germany), is available from EUROSTAT. Details on the data can be found on the EUROSTAT website (Eurostat, 2022). For Gütersloh, we use administrative data from the federal state of North-Rhine Westphalia that reports the same measure in their 2018 mobility report (Ministerium für Verkehr des Landes Nordrhein-Westfalen, 2019, p. 68). Data on Austrian cities for 2018 is obtained from Statistic Austria (Statistics Austria, 2021a).

A.3.3 Cycling modal share

The cycling modal share is defined by the share of journeys to work by bicycle. Data is obtained from EUROSTAT where available. Most German and all Swiss cities are included in the EUROSTAT data set. We consider cycling modal shares for the year 2018. Details on the data can be found on the EUROSTAT website (Eurostat, 2022).

Data on Austrian cities are obtained from official municipality websites and refer to the latest traffic reports of the respective city. A list with the respective sources and year of data collection can be found in a spreadsheet online data appendix.

For Norwegian cities, we use the cycling modal shares from a study (Tennøy et al., 2022) examining "Urban structure and sustainable modes' competitiveness in small and medium-sized Norwegian cities". They provide modal shares for the largest Norwegian cities using data from the Norwegian National Travel Survey collected in 2013/14 and 2017/18. For Swedish cities, we use data from a study (Kenworthy and Svensson, 2022) reporting the "percentage of total daily trips by cycling" for ten Swedish cities. For Finland, the data are obtained from the regional publications of the Finnish National Travel Survey (FNTS). Detailed information on the FNTS is published by the Finnish Transport and Communications Agency (Finnish Transport and Communications Agency, 2020). Cycling modal share data for the city of Jyväskylä is obtained from the official region travel survey in 2019 (City of Jyväskylä, 2020) because detailed data for Jyväskylä was not published as part of the FNTS.

A.3.4 Income data by city

To get a proxy for the average income by city, we use data on the gross domestic product (GDP) at current market prices in 2018 converted to purchasing power standard per inhabitant.[1] The data is provided by Eurostat (2023) at the NUTS 3 region level (for more information on NUTS3-regions see: `http://ec.europa.eu/eurostat/web/nuts/overview`). Accordingly, the values for each city refer to the corresponding NUTS-3 region. There are two cases in our sample where two cities lie in the same NUTS-3 region (Zürich/Winterthur in CH040 and Malmö/Helsingborg in SE224).

A.3.5 Population data

For city population numbers, we use the most recently available public population data from each country's respective statistical agency as of 2021. Population data for Austrian political districts at the start of 2021 is provided by Statistics Austria (2021b). Finnish municipality population data for the end of 2021 is provided by Statistics Finland (2021). German city population data for the end of 2019 is provided by the Statistisches Bundesamt (2020). Norwegian municipality population data for 2020 is provided by Statistics Norway (2021a). Swedish municipality population data for 2020 is provided by Statistics Sweden (2021b). Lastly, Swiss district population data from 2021 is provided by the Swiss Federal Statistical Office (2021).

B   Addressing endogeneity concerns and robustness checks

B.1   Alternative estimators in a monthly panel

### B.1.1   Standard two-way fixed-effects

Our main specifications account for biases that may arise due to heterogeneous treatment timing and heterogeneous treatment effects that were studied extensively in the recent econometric literature on two-way fixed effects (TWFE) difference-in-differences (DD) estimators (Borusyak et al., 2023; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021).

For comparison, OLS estimates of standard TWFE DD regressions, which do not account for this heterogeneity, can be found in figure 4. If we start the observation period in January 2016 as in our main specification, the coefficients are smaller (figure 4b), with an estimated ATE of 4.7 ± 2.0 that is statistically significant (table 5). Marginally changing the length of the panel (starting two years before the first launch, i.e., April 2016) changes the point estimate substantially upward as

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[1] "The conversion to purchasing power standards (PPS) is based on national purchasing power parities (PPP) which are also regularly calculated and released by Eurostat. Regional PPP are not available. All regional accounts data published by Eurostat are based on PPP for the EU Member States" (`https://ec.europa.eu/eurostat/cache/metadata/en/reg_eco10_esms.htm`).

shown in figure 4a and discussed in section 5.1. Given these differences and the biases discussed above, the TWFE results should be interpreted cautiously.

Figure 4: Monthly treatment effects and pre-trends based on the TWFE estimator

(a) Panel starting 2 years before first launch (b) Panel starting 2.25 years before first launch
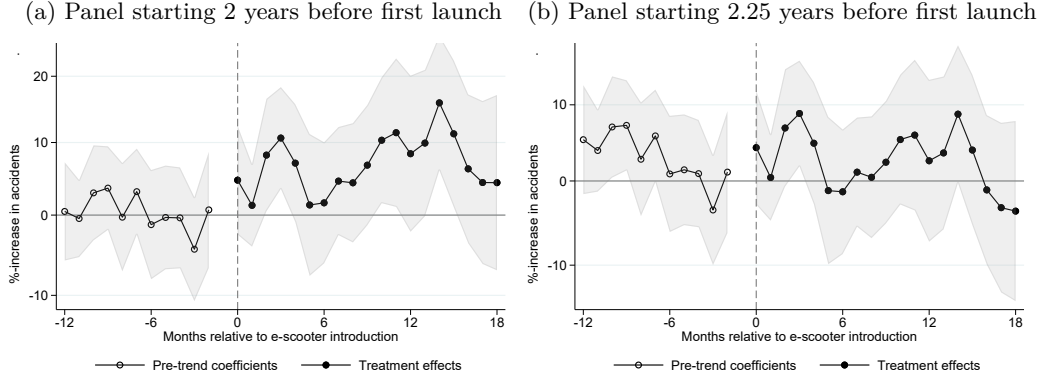
Notes: The figures show average treatment effects relative to treatment introduction. In the line plot, filled dots indicate treatment effect estimates for the first 18 months relative to the last month before the introduction of e-scooter services. Circles indicate estimates for pre-treatment months, also relative to the last month before the introduction of e-scooter services. 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. All estimates account for city fixed effects and month fixed effects

OLS estimates of the average treatment effect and a Poisson regression estimate of the same model (that we will discuss in appendix B.1.2) can be found in columns 3, 4, 7, and 8 of table 5.

Including more granular fixed effects (i.e., country-year) in a TWFE DD framework may partially reduce concerns about biases arising from heterogeneous treatment timing and effects (e.g., country-year fixed effects would avoid comparing later treated German cities against earlier treated Swedish or Austrian cities). Indeed, table 5 illustrates that including such fixed effects renders estimates from TWFE DD, based on the log-linearized model or a Poisson model, consistently more precisely estimated.

However, using country-year fixed effects is not a satisfactory remedy, as the described problem still exists for the implied comparisons within countries. More importantly, the inclusion of country-year fixed effects removes all variation stemming from countries and years where all cities are treated (i.e., Austria 2020–2021 and Norway in 2021). The estimand is thus only the average treatment effect in the remaining years and countries—especially if treatment effects grow dynamically, this restriction implies losing relevant years of possibly significant effects. The imputation estimator that we use addresses the issue of constructing a counterfactual that is not affected by dynamic treatment effects at its root by estimating the fixed effects from control observations only, which avoids any implicit comparisons of newly treated observations to earlier treated observations.

B.1.2 Poisson versus logarithmized dependent variables

We estimate the effect of introducing shared e-scooter services in our main model as a semi-elasticity by applying the natural logarithm to the dependent variable in a linear model that we estimate with OLS, which can be problematic when the dependent variable contains many zeros. In principle, semi-elasticities could also be estimated through a Poisson regression without transforming the dependent variable. We rely on the log transformation for two reasons. First, observations with zero accidents are extremely rare in our data (only a single month-city in our sample has recorded zero accidents, as pointed out in section 3). Second, to our knowledge, methods for Poisson

Table 5: Two-way fixed effects regressions, country-time fixed effect regressions, instrumental variable regressions, and Poisson regression.

	Event-study estimate (Borusyak et al. 2023)		Two-way fixed effects (TWFE)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Imputation	Imputation	OLS	OLS	IV	IV	Poisson (PPML)	Poisson (PPML)
Effect of introduction (%)	8.2***	4.6**	4.7**	4.6***	11.0	11.5**	2.7*	4.1***
	(2.9)	(2.4)	(2.0)	(1.7)	(7.0)	(5.4)	(1.6)	(1.3)
Country-year FEs		✓		✓		✓		✓
Kleibergen-Paap Wald rk F-stat					8.3	8.8		
Cities		93	93	93	93	93	93	93
Observations	5880	5784	5880	5880	5880	5880	5880	5880

Notes: * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses allow for clustering of the model error at the city level. Raw estimates are transformed to semi-elasticities: $100 \cdot (\exp(\hat{\tau}) - 1)\%$. Estimates for treatment effects are based on different specifications and estimators, as indicated in the table header. Even columns additionally account for year-country fixed effects. Column 1 repeats the ATE estimate from our main specification (figure 2, column 3) for convenience. The estimate in column 2 accounts for country-year fixed effects and excludes observations for Austria 2020-21 and Norway 2021 because, for those countries, there exist no "later-treated" cities to identify the country-year fixed effects from, as all cities that meet the sample criteria were treated. Columns 3–4 show TWFE-OLS estimates. Columns 5–6 show instrumental variable estimates as discussed in appendix B.2. The Kleibergen-Paap Wald F-statistics tests for the exclusion of the instruments in the first stage of the instrumental variable. Columns 7–8 show estimates from a TWFE Poisson regression.

regression that account for treatment effect heterogeneity in staggered rollout settings have not been developed.

To investigate whether using a log transformation of the dependent variable, as opposed to modeling a Poisson distribution, significantly changes conclusions in our setting, we estimate a Poisson variant of the two-way fixed effect regression

$$\mathbb{E}[\text{accidents}_{it}] = \exp(\alpha_i + \beta_t + \tau d_{it})$$

where d_{it} is an indicator for treatment in city i and month t. As can be seen in table 5, the estimates from the log-linear DD specifications (e.g., column 4) and the estimates obtained from the Poisson model (e.g., column. 8) support qualitatively comparable conclusions, especially when country-year fixed effects are included. This similarity reassures us that our main results are not a consequence of relying on a log-transformed dependent variable, as opposed to a Poisson model.

However, within the Poisson regression model, it is not possible to account for the treatment effect heterogeneity that can bias estimates in the staggered rollout settings. We, therefore, treat these results only as ancillary results and refrain from discussing the coefficient magnitude further.

B.1.3 Intensive margin

Our estimates up to this point have focused on the extensive margin (the effect of whether a city is treated at all). Using TWFE, we can additionally calculate the effect by intensity of treatment or dose, i.e., the number of e-scooter companies that have launched in a city. However, these estimates should be viewed skeptically, as estimating the average effect of a dosed treatment variable requires a much stricter identifying assumption. Namely, the parallel trends assumption needs to hold over not only untreated potential outcomes but also over all the levels of the treatment dose (Callaway et al., 2021). This would be violated if there was selection into different treatment intensities in a way correlated with potential outcomes, i.e., e-scooter companies launching in cities with fewer e-scooter accidents.

We estimate the intensive margin (table 6), the increase in the percentage of accidents for each additional launch, where we treat company launch as terminal to capture an intent-to-treat effect and because we do not have data on firm withdrawals from markets. Accidents involving personal injuries increased on average by $1.0 \pm 0.5\%$ for each additional company that launched in a city. Looking again at the median sample city in terms of accidents, Potsdam reports 54 accidents in the average pre-treatment month and has four e-scooter companies by May 2021. An increase of 4% thus implies an additional 2.2 monthly accidents. On the other hand, Stockholm (which had the most e-scooter company launches in our sample, 14) would have an estimated 14% increase in accidents. Relative to Stockholm's pre-treatment average number of accidents, 91, the estimated increase is 12.7 accidents per month.

Table 6: Specifications to study intensive margin effects.

	(1) OLS	(2) OLS
Effect of one additional company (%)	1.0**	1.0**
	(0.5)	(0.4)
Country-year FEs		✓
Cities	93	93
Observations	5880	5880

Notes: * p<0.1, ** p<0.05, *** p<0.01. Columns 1–2 follow the same order as columns 3–4 of table 5, but use the count of scooter firms, as opposed to the launch indicator for the first scooter firm, as the main independent variable. Robust standard errors in parentheses allow for clustering of the model error at the city level. Raw estimates are transformed to semi-elasticities: $100 \cdot (\exp(\hat{\tau}) - 1)\%$. Column 2 additionally accounts for year-country fixed effects.

B.1.4 Alternative estimators accounting for potential biases with staggered rollout

As an alternative, we use the estimator proposed by Callaway and Sant'Anna (2021), which is also intended to account for the biases that arise with staggered rollout and dynamic treatment effects in a DD framework. See section 5.1.2 for a comparison of the differences between the two estimators and their appropriateness for our setting. Figure 5 shows treatment effects with estimands to be interpreted relative to one period before treatment. Pre-trend coefficients are interpreted relative to one period before the period under consideration. See section 5.1.2 for a discussion of the estimates in the figure relative to our preferred specification.

Figure 5: Monthly treatment effects and pre-trends based on CS estimator
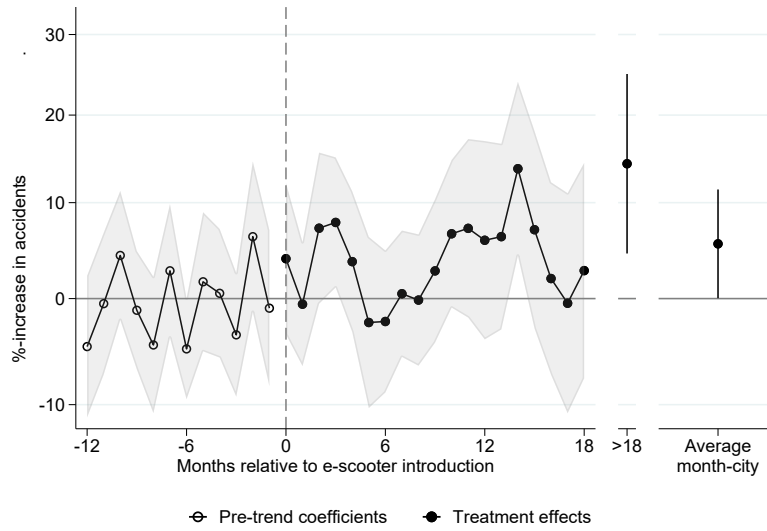
Notes: The figure shows average treatment effects relative to treatment introduction. In the line plot, filled dots indicate treatment effect estimates for the first 18 months relative to the month before the introduction of e-scooter services. Circles indicate estimates for pre-treatment months relative to the respective preceding month. 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 materialize). So, monthly estimates for later months cannot be estimated with comparable precision as for earlier months. 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. All estimates account for city fixed effects and month fixed effects

B.1.5 Synthetic difference-in-differences (Arkhangelsky et al., 2021)

Our main specification relies on an assumption of parallel trends for causal identification and we presented several tests as evidence for parallel trends. As an additional robustness check, we show in table 7 that results are similar when using the synthetic difference-in-differences (SDD) estimator for multiple treatment start periods as described in the appendix of Arkhangelsky et al. (2021). The SDD relaxes the parallel trends assumption, which states that in absence of the treatment the group of treated and the group of untreated observations would exhibit the same absolute change over time. The SDD instead makes a weaker assumption: that there exists a weighted average of control units that exhibits the same absolute change over time as the treated units would in absence of treatment. That weighted average is constructed such that the average outcome for the treated units is approximately parallel to the weighted average for control units before the treatment. In other words, the synthetic control fits the pre-treatment period outcomes of the treated observations as closely as possible while allowing for a constant difference. The assumption is that the synthetic control would continue to closely approximate the counterfactual trend of the treatment group. The coefficient estimates in table 7 are similar to our main results, though not as large, which can be explained by the fact that the SDD estimates have to rely on a slightly different sample, as discussed below. The event-study and SDD estimates are more similar when the former are restricted to the same time period, ending in December 2020 (see column 4 of table 7).

Aside from imposing a slightly less restrictive version of the parallel trends assumption, the SDD for staggered rollout designs, as discussed in the appendix of Arkhangelsky et al. (2021), has more demanding data requirements. It requires a balanced panel (which excludes Sweden for panels including 2016 and 2017) and that control units must be entirely untreated within the panel's time frame. Recall that in our main analysis, all yet-to-be-treated units are used to impute the month- and city-fixed-effects. To maintain a sufficiently large but comparable set of control cities in the SDD analysis, we restrict the analysis to end in December 2020. This implies that all cities that launched scooters in 2021 (see figure 3) can be used to construct the synthetic control. It also implies, however, that we have fewer cities and fewer periods left to compute the treatment effect. This required early cutoff is one reason we do not use the SDD as the main specification. By extending the time frame to include more post-treatment periods, cities that become treated fall out of the donor pool of control cities. Losing the yet-to-be-treated control cities reduces the goodness of fit of the synthetic control (measured in the pre-treatment average monthly mean squared prediction error, MSPE). Also, having a smaller donor pool jeopardizes inference by reducing the units that can be used as placebo-treated units for the placebo variance estimation. Similarly, extending the pre-treatment time period forces cities with missing data for earlier time periods to be excluded due to the balanced panel requirement.

Column 1 of table 7 shows the SDD estimate, approximately 4%, for the monthly average treatment effect of shared e-scooter services on accidents in treated cities until December 2020. This estimate loosely corresponds to column 1 of table 10, with the difference that the estimand excludes the treatment effects in 2021 and the treatment effect on Swedish cities. Synthetic controls are fitted on pre-treatment data from January 2016 to the date of introduction, which is a minimum of 27 months. Choosing December 2020 as the sample horizon allows for the estimand to include the effect from the summers of 2019 and 2020 when the majority of our study's cities introduced scooters. The donor pool of cities from which the synthetic control is constructed included 31 cities, including 13 yet-to-be-treated cities. The results do not substantively change when choosing an earlier cut-off (e.g., July 2020 has 45 potential controls but the MSPE does not improve and the estimate is similar). However, choosing a later cut-off quickly excludes the yet-to-be-treated control cities. The synthetic control's goodness of fit worsens (i.e. the MSPE doubles) and the treatment effect cannot be reliably estimated.

Column 1 excludes all Swedish cities and Gütersloh in Germany because of missing data.

Table 7: Synthetic difference-in-differences estimate (Arkhangelsky et al., 2021)

Estimate:	synthetic difference-in-differences			event-study
	(1) 2016–2020	(2) 2018-2020 incl. Sweden	(3) 2016–2020 excl. winter	(4) 2016–2020, incl. Sweden & Gütersloh
Treatment effect	4.2** (1.8)	5.5*** (1.8)	4.7*** (1.6)	5.7*** (2.2)
Monthly MSPE	0.0168	0.0184	0.0114	
Treated cities	71	79	67	80
Total cities	102	111	102	93
Months in sample	60	36	40	60
Post-treatment observations	1022	1187	680	1202
Full year	Yes	Yes	No	Yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses. Columns 1-3 are estimated using the SDD method from Arkhangelsky et al. (2021). For the SDD columns, standard errors are computed using the placebo variance estimation method from Arkhangelsky et al. (2021) with 250 replications using Arkhangelsky (2022). Raw estimates are transformed to semi-elasticities: $100 \cdot (\exp(\hat{\tau}) - 1)\%$. Standard errors are transformed accordingly using the delta method. Monthly MSPE is the mean squared prediction error of the synthetic control for pre-treatment outcomes, normalized by the number of pre-treatment months. MSPE values are not transformed from the log-linear regression. MSPE is included here as a measure of fit of the synthetic control. Column 4 uses the event-study estimator described in Borusyak et al. (2023), and has standard errors that allow for clustering of the model error at the city level, computed using the recommended leave-out procedure where cohorts are defines as the quarter in which scooters were launched.

Column 2 shrinks the time frame of the panel to start in January 2018 in order to include the Swedish cities. This, unfortunately, reduces the pre-treatment observations for Zurich to 3 months, and Basel, Wien, Stockholm, Göteborg, Malmö, and Uppsala all have less than 12 pre-treatment periods.

The last reason we do not use the SDD as the main specification is its susceptibility to bias when there is greater seasonal movement in the outcome variable in treated cities than in any of the control cities (i.e., outside the convex hull of the controls). We found that the weighted average of the synthetic controls constructed for both columns 1 and 2 under-predicted accidents in the summer (April-September), and over-predicted in the winter (October-March) by the same magnitude, 3%. In other words, the average pre-treatment difference between the synthetic control and the treatment group is very close to zero, but the difference is consistently more in winter and less in summer. This volatility could bias the results if the number of summer and winter months with available data varies across cities in the sample. Balancing the seasons in our panel is the final reason we choose December 2020 as a cutoff. For column 1, summer months (April to September) compromise 50.1% of the pre-treatment months and 49.6% of the post-treatment months. Column 2 is 50.2% pre-treatment and 49.7% post-treatment summer months.

There is a risk of relying on an estimator with a synthetic control displaying seasonal bias. That is why our preferred specification for the SDD is column 3 of table 7, which removes over-compensating seasonality in the synthetic control by dropping the months of November to February from the analysis, where accidents numbers dip low. The goodness of fit increases by a third, and the synthetic control no longer has a detectable seasonal bias. The drawback is that the estimand is no longer an average of all post-treatment months, but just post-treatment months between March and October. This specification corresponds to the non-winter specification shown in column 4 of figure 2. The estimate is correspondingly larger, likely due to the fact the restricted set of better weather months in the estimand see more e-scooter trips and resulting accidents.

Column 4 of table 7 restricts our main specification (using the Borusyak et al. (2023) estimator) to also end in December 2020. In terms of sample composition, column 4 is closest to column 2, and their estimates are similar. The only two differences in the samples between columns 2 and 4 are that column 4 includes the city of Gütersloh and that it uses 2016–2017 to estimate the city-fixed effects. Both differences are owed to the fact that the SDD estimator requires a balanced panel. With the SDD estimator, we also replicated results for (i) winter months, (ii) excluding COVID, and (iii) heterogeneity by bike lanes and found they match our main analysis closely (results not shown). But even though synthetic controls have desirable properties, like a relaxed parallel trends assumption, we leave it as a robustness check due to the limitations regarding time coverage and the workarounds needed to overcome potential seasonality bias.

B.2 E-scooter rollout endogeneity concerns

B.2.1 Factors that predict treatment timing and why they are unlikely to confound our results

The rollout of e-scooter services is not random. There are a number of factors that are predictive of city-level market entry. These factors can be relatively time-invariant, such as population size or infrastructure, or they can be time-varying, such as season and regulatory constraints. In this section, we identify such factors and argue why they are either sufficiently accounted for in our analyses through the included fixed effects, or how they can be assumed to be unrelated to accident numbers.

Two factors that can be considered time-invariant over our period of study clearly predict the timing of rollouts. First, firms initially target larger cities. As shown in figure 3, each country's largest cities were among the first to be treated. Second, firms prioritize cities that are more bicycle-friendly, where e-scooters are arguably more likely to be successfully adopted. Table 8 regresses four time-invariant (relative to our short study period) city characteristics on the month of e-scooter introduction while controlling for country fixed effects, in order to see what city characteristics predict earlier launches. The results show that larger cities and cities with an extensive separated cycling road network received e-scooters significantly earlier. Cities with 100,000 more inhabitants received e-scooters on average 0.3-0.6 months earlier. Cities with a one standard deviation larger share of bike lanes received e-scooters on average 3-4 months earlier. Similarly, cities with a one standard deviation larger number of cars per capita received e-scooters 3-4 months later. In addition, e-scooter services tend to be launched earlier in richer cities. However, if we control for the other city characteristics the coefficient is statistically insignificant.

The coefficient of determination in the full specification is close to 39%, in spite of only accounting for five city characteristics and country fixed effects. We interpret this as supportive evidence that the timing of launches is, to a good extent, driven by time-invariant characteristics of cities. In our estimation of the treatment effect of e-scooters, all time-invariant characteristics of cities are controlled for through city-level fixed effects.

Three time-varying factors that predict rollout timing can also be identified. First, within a year the start of operations is often in summer when the service is attractive to customers. Roughly half of the launch dates are either in June, July, or August (see figure 3). Second, firms target cities close to recently added areas of operation, likely to exploit economies of scale. For instance, providers usually roll out their services country by country. When they decide to expand to a country, they usually quickly roll out their services in all key cities within the respective country (see for example the press statement by Voi on its expansion in Germany, Voi Technology, 2019a). Also within countries, this spatial correlation can be empirically observed in the data. Between January 2018 and June 2021, in the average sample city, the probability that a new firm launched in a given month was 6.6%. In the first two [one] months within a new firm launching in the nearest neighboring city (conditional on new launches anywhere else in the country) this probability is

Table 8: Predictors of early launches

	(1) Month of introduction	(2) Month of introduction	(3) Month of introduction	(4) Month of introduction	(5) Month of introduction
Population (in 100k)	-0.608*** (0.169)	-0.278 (0.192)	-0.637*** (0.231)	-0.574*** (0.180)	-0.301* (0.152)
Share of bike lanes	-3.925*** (0.979)				-3.379*** (1.136)
Cars per capita		3.545** (1.393)			2.712* (1.429)
Cycling modal share			-0.699 (0.831)		1.065 (0.875)
log(GDP per capita)				-2.213** (0.901)	-1.034 (0.945)
Cities	93	93	93	93	93
R^2	0.33	0.31	0.24	0.28	0.39

Notes: * p<0.1, ** p<0.05, *** p<0.01. The table shows the coefficients of an OLS regression, regressing the month of introduction on different city characteristics. All independent variables, except for population are normalized to have mean 0 and variance 1. Positive [negative] coefficients imply later [earlier] introductions. All regressions control for country fixed effects. Robust standard errors in parentheses.

higher by around 5.1 ± 1.5 [7.4 ± 2.4] percentage points than in other months (see table 9, columns 1 and 2). This pattern is likely driven by firm-level expansion waves, but it also translates into a spatial correlation of the overall e-scooter rollout (see table 9, columns 3 and 4). Third, e-scooter launches are subject to regulation, which poses a binding constraint to the timing and is unlikely to independently affect changes in accidents. For example, in Germany, the timing of launches for almost half of the cities coincided with federal regulation in June 2019 that initially allowed the use of e-scooters on public roads (Gebhardt et al., 2021). Scooter providers were ready to start operations right after the regulation allowed them to.

These three time-varying factors are unlikely to co-determine accidents (e.g., the number of e-scooter firms in a neighboring city can be assumed not to affect accident numbers, nor does national [de-]regulation of e-scooter usage), or are controlled for through the use of month fixed effects (e.g., summer months generally show higher accident numbers).

In sum, there are a number of factors determining e-scooter rollout. But the city-specific timing of scooter launches is unlikely to be endogenous to city-idiosyncratic trends in the number of accidents in our empirical model.

Table 9: Launches in neighboring cities predict launches of e-scooter firms.

Dependent variable:	=1 if new firm launches		=1 if first firm launches	
	(1)	(2)	(3)	(4)
Launches in neighbor city (2 months)	5.1***		1.5*	
	(1.5)		(0.8)	
Launches in neighbor city (same month)		7.4***		2.0
		(2.4)		(1.4)
Launches in country (2 months)	0.7***		0.4***	
	(0.1)		(0.1)	
Launches in country (same month)		1.4***		0.7***
		(0.3)		(0.1)
Mean dep. var.	6.6%	6.6%	2.3%	2.3%
Cities	93	93	93	93
Observations	3826	3839	3826	3839

Notes: * p<0.1, ** p<0.05, *** p<0.01. The table shows coefficients (scaled to be interpretable as percentage points) of a regression of indicators for firm launch between 2018 and June 2021 on firm-launch indicators for neighboring cities. All regressions control for city and month fixed effects. Clustered standard errors in parentheses allow for city-level clustering. 'Neighboring city' is the geographically closest sample city where e-scooters were ever launched.

B.2.2 Never-treated control cities

As an additional robustness check, we extend our analysis by including never-treated cities where e-scooter services were not launched before 2022. Figure 6 shows the corresponding monthly estimates. Including never-treated cities in our control group slightly decreases the magnitude of our estimates. The estimated average treatment effect is an increase in total accidents of $4.7\pm2.3\%$ (column 2 table 10), which is still statistically significant. While including never-treated cities provides a larger sample and potentially more precise estimates, the concerns about comparability and potential violation of the parallel trends assumption (see the paragraph "Additional pre-trend tests" below) make the specification without never-treated control cities our preferred specification.

Figure 6: Monthly treatment effects, pre-trends, and aggregate treatment effects, based on figure 2, but additionally using never-treated cities as control group.
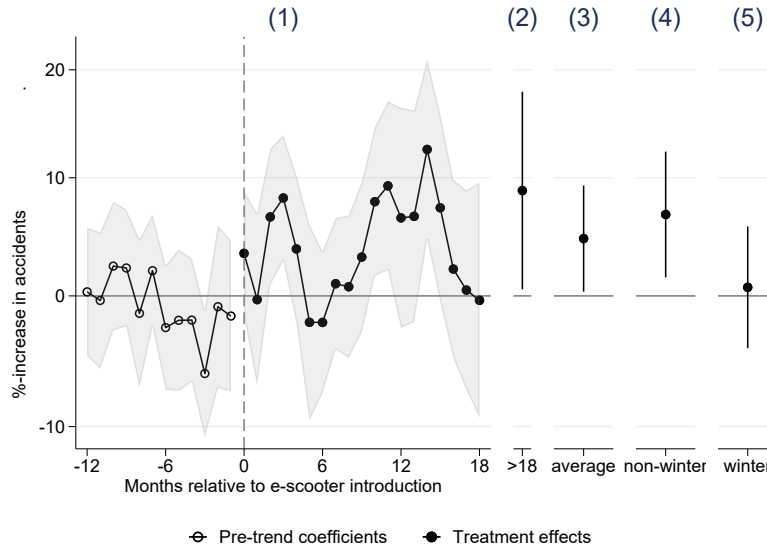
Notes: The figure shows average treatment effects relative to treatment introduction. Winter months are November–February, and non-winter the remainder. In the line plot, filled dots indicate treatment effect estimates for the first 18 months relative to the introduction of e-scooter services. Circles 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 materialize). 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 (table 10, column 2). 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.

Additional pre-trend tests   Table 11 shows results for regressions testing for differences in pre-trends, for different cohorts. To investigate pre-trends for different (potential) types of control groups, we restrict the sample to the years 2017-2018 and regress the natural logarithm of accidents on city and month fixed effects, while allowing for different time trends for different cohorts (never-treated, treated during 2020, and treated during 2021). The cohort of cities that become treated

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Table 10: Estimated treatment effects on police-reported accidents involving personal injury

|	Monthly event-study estimate					
	(1) Main sample	(2) Incl. never-treated cities	(3) First 12 months	(4) Non-winter	(5) Winter	(6) Excluding COVID
%-increase in accidents	8.2***	4.7**	5.3**	11.5***	1.9	5.7***
	(2.9)	(2.3)	(2.1)	(3.5)	(3.3)	(2.1)
Mean pre-treatment accidents	93.2	93.2	93.2	100.6	78.5	95.8
Treated observations	1704	1704	956	1140	564	948
Total observations	5880	7134	5880	5880	5880	5880
Cities	93	112	93	93	93	93

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. Columns 1 and 3–6 rely on yet-to-be-treated observations as controls. Column 2 additionally uses never-treated cities. Standard errors are in parentheses. 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 quarter in which scooters were launched.

in 2019 is the omitted category. Table 11 shows estimates based on the regression model

$$\log(\text{accidents}_{it}) = \alpha_i + \gamma_t + \beta t \text{cohort}_i + \varepsilon_{it},$$

where cohort_i indicates the cohorts. Each estimation sample is restricted to the sample of groups treated in 2019 and the respective group for which the model allows a differential trend. The results in Table 11 indicate that the two groups that get treated in 2020 and 2021 exhibit no significantly different trend in 2017-2018, compared to those treated in 2019. However, the never-treated group does have a relatively lower trend in these years ($p = 0.07$) that is marginally significant. This is suggestive evidence that never-treated cities may be experiencing a differential trend in accidents. We thus consider the specifications that do not rely on never-treated control cities preferable.

B.2.3 Controlling for city-specific time-trends and potential confounders

Table 12 shows variants of our main treatment effect estimates, controlling for city-specific linear time trends in the natural logarithm of accidents. One possible concern is that population (density) growth may affect accidents. If population growth was correlated with the rollout of e-scooters, this could potentially constitute a source for omitted variable bias in our estimation framework. To address this concern, we conducted analyses allowing for city-specific time trends in accident numbers. Population growth is likely relatively continuous within a time frame of four years, and thus, effects of population numbers on accident rates should be captured by linear city-specific time trends (note that city- and month-specific fixed effects are also still accounted for).

In addition, table 13 shows the average treatment effects controlling for year-specific relationships between accidents and time-fixed controls, including share of bike lanes, cars per capita, cycling modal share and the logarithm of city's population. Figure 7 shows the monthly treatment effects for the specification that controls for all four interaction terms. This specification corresponds to the estimation of the average treatment effects in column 5 of table 13. Including these additional control variables does not considerably change the size and significance of our estimated treatment effects.

B.2.4 Placebo tests on launch dates

Figure 8 shows the same analysis as figure 2 but restricts the estimand to summer-month treatment effects. As a result, the apparent dips in treatment effects around half a year after treatment

Table 11: Significantly different pre-trends in log(accidents) for never-treated cities.

	(1)	(2)	(3)	(4)
treated 2020×month	-0.14			-0.14
	(0.15)			(0.15)
treated 2021×month		-0.17		-0.17
		(0.24)		(0.24)
never-treated×month			-0.31*	-0.32*
			(0.17)	(0.17)
Mean dep. var.	4.2%	4.2%	4.2%	4.0%
Cities	73	61	66	104
Observations	1692	1404	1536	2424
Sample:				
—year	2017-2018	2017-2018	2017-2018	2017-2018
—city treated in ..	2019/2020	2019/2021	2019/never	2019-2021/never

Notes: * p<0.1, ** p<0.05, *** p<0.01. The table shows estimated treatment effects from log-linear TWFE specifications allowing for linear time trends. Raw estimates are transformed to semi-elasticities: $100 \cdot (\exp(\hat{\tau}) - 1)\%$. Standard errors are transformed correspondingly. Standard errors allow for clustering of the model error at the city level.

Table 12: Replication of main results, while allowing for city-specific time trends.

	(1) Main sample	(2) Incl. never-treated cities	(3) First 12 months
%-increase in accidents	9.3**	8.5**	4.7**
	(4.0)	(3.5)	(2.4)
Mean pre-treatment accidents	93.2	83.0	93.2
Treated observations	1704	1704	956
Total observations	5880	7134	5880
Cities	93	112	93

Notes: * p<0.1, ** p<0.05, *** p<0.01. This table shows estimated treatment effects from specifications similar to the log-linear specification described in materials and methods, section 3. The estimation sample specifications correspond to table 10, columns 1-3. In addition to city and year-fixed effects, these regressions allow for city-specific linear time trends.

Figure 7: Monthly treatment effects and pre-trends, allowing for year-specific relationships between time-fixed controls and accidents.
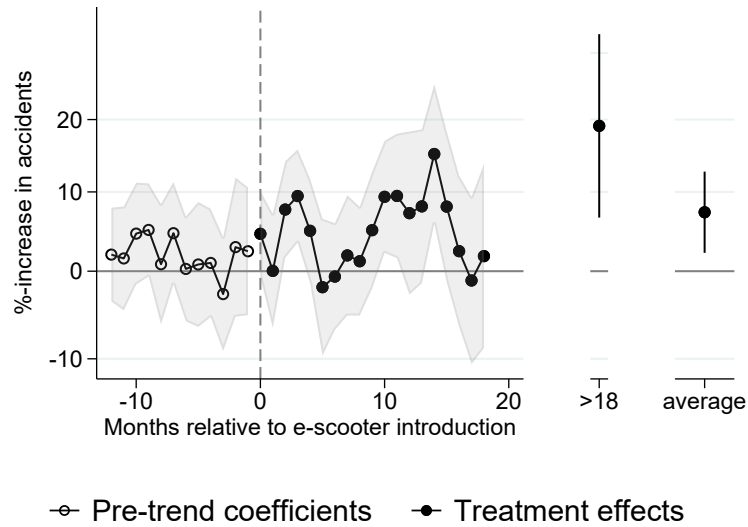
Notes: The figure shows average treatment effects relative to treatment introduction. In the line plot, filled dots indicate treatment effect estimates for the first 18 months relative to the introduction of e-scooter services. Circles indicate estimates for pre-treatment months. Effects after month 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 the right part, the average treatment effect estimate and corresponding 95%-confidence interval, across all available post-treatment city-months is shown. Shaded areas/bars indicate the 95%-confidence interval around the estimates, based on leave-out standard errors defining half-year (for monthly estimates) or quarter (for the average treatment effect) of scooter launch as cohorts, as described in materials and methods, subsection 3. All estimates account for city fixed effects and month fixed effects and additionally allow for year-specific relationships between time-fixed controls and accidents.

Table 13: Allowing for year-specific relationships between time-fixed controls and accidents.

	(1)	(2)	(3)	(4)	(5)
%-increase in accidents	8.5***	9.8***	9.4***	5.8**	7.3***
	(2.9)	(3.6)	(2.8)	(2.7)	(2.7)
%-increase in accidents during the first 12 months	4.9**	5.3**	6.0***	3.8*	4.1**
	(2.0)	(2.2)	(2.1)	(2.0)	(2.0)
%-increase in accidents during summer months	11.5***	13.0***	12.2***	9.0***	10.6***
	(3.2)	(4.5)	(3.4)	(3.2)	(3.0)
share of bike lanes × year FX	✓				✓
cars per capita × year FX		✓			✓
cycling modal share × year FX			✓		✓
log(population) × year FX				✓	✓
Observations	5880	5880	5880	5880	5880

Notes: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. This table shows estimated treatment effects from specifications similar to the log-linear specification described in materials and methods, section 3. The estimation sample specifications correspond to table 10, columns 1-3. In addition to city and year-fixed effects, these regressions allow for year-specific relationships between time-fixed controls and accidents.

observed in figure 2 are less pronounced. We take this as evidence for our conjecture that these dips are driven by winter months.

Figure 9 shows the same analysis as figure 2 but instead of using the actual month of e-scooter introduction, it uses placebo introduction dates that are shifted into the past by 12 or 24 months. Unlike in the real specification, we observe no significant treatment effects in this placebo specification.

Figure 8: Monthly treatment effects and pre-trends for non-winter months
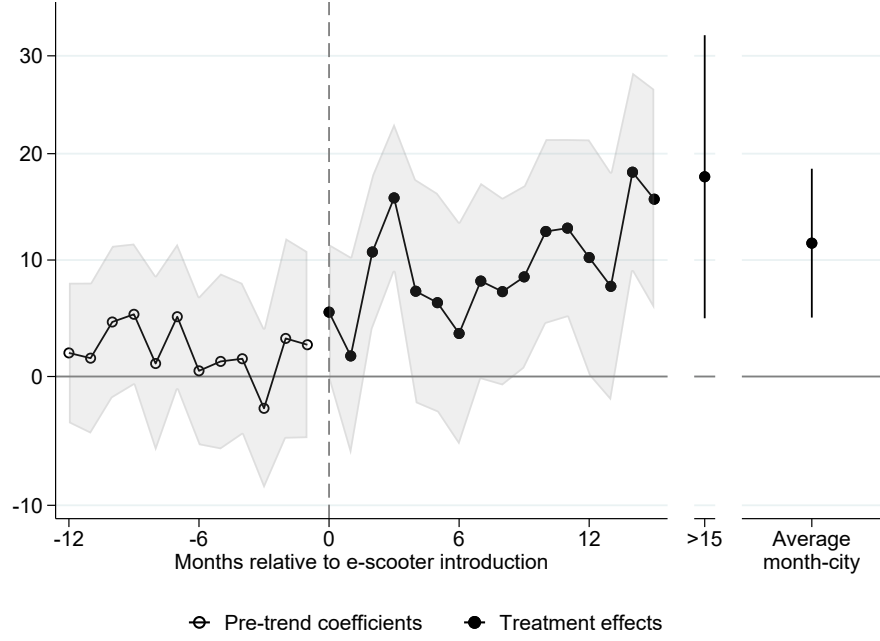
Notes: The figure shows average treatment effects relative to the treatment timing. City-months that fall into Nov.–Feb. receive a weight of zero. Raw estimates are transformed to semi-elasticities: $100 \cdot (\exp(\hat{\tau}) - 1)\%$. In the line plot, filled dots indicate treatment effect estimates for the first 18 months relative to the introduction of e-scooter services. Circles indicate estimates for pre-treatment months. Effects after month 15 are combined into a single coefficient because month-level estimates for long-term effects are estimated on small subsamples (only few cities had e-scooters early enough for long-term effects to materialize). Monthly estimates for later months could thus not be estimated with comparable precision as for earlier months. In the right part, the average treatment effect estimate and corresponding 95%-confidence interval, across all available post-treatment city-months is shown (table 10, column 3). Shaded areas/bars indicate the 95%-confidence interval around the estimates, based on leave-out standard errors defining half-year (for monthly estimates) or quarter (for the average treatment effect) of scooter launch as cohorts, as described in materials and methods, subsection 3. All estimates account for city and month fixed effects.

Figure 9: Placebo tests using treatment dates shifted by 12 or 24 months.
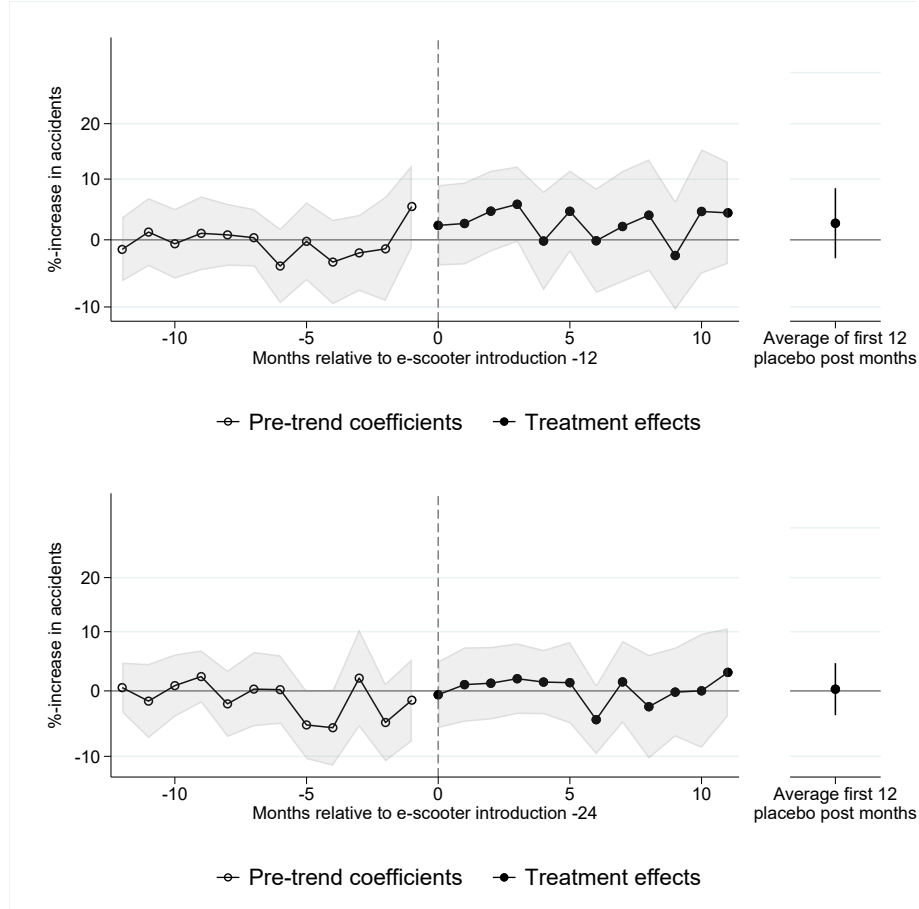
Notes: In each left panel, filled dots indicate treatment effect estimates by month since the placebo-introduction of e-scooter services. Circles indicate estimates for pre-placebo-introduction differences. Shaded areas indicate the 95%-confidence interval around the placebo-estimates, based on leave-out standard errors defining half-year of treatment launch as cohorts, as described in section 3. In the right panels, the average placebo-treatment effect estimates across all cities and the first 12 available post months are indicated (analogous to table 10, column 2).

B.2.5 An instrumental variable approach

As additional corroboration, we use an instrumental variable (IV) approach to address possible endogeneity concerns. Instruments need to vary over time and space. To construct instruments, we use cities' population and our heterogeneity variables, which vary cross-sectionally, and the number of active e-scooter firms in other cities of the same country, which varies over time. The interactions of the former with the latter are used to instrument for treatment in a two-way fixed effects model, accounting for time and unit fixed effects. In particular, for the cross-sectional variation, we use the three pre-treatment variables from the heterogeneity analysis in table 2 (demeaned at the country level). For the temporal variation, we instrument the binary treatment indicator by using a dummy variable for whether in other cities in the same country e-scooters were launched (table 5, columns 5 and 6).

For the interaction terms to be valid instruments, they need to fulfill two conditions. First, they need to predict e-scooter rollout, which they do (see appendix B.2.1). However, weak identification concerns may be valid and the results should be considered estimated with significant uncertainty. The first-stage F-statistics are reported in the table footer of table 5 in columns 5 and 6.

Second, the interaction terms between scooter firms and city-characteristics should affect accidents only through the supply of shared e-scooter services. Since the global development of the scooter market and city characteristics are already controlled for through the use of city and month fixed effects, this assumption may be justified.

The IV results, shown in table 5 columns 5 and 6, indicate large positive and statistically significant treatment effects that are, however, estimated with relatively large standard errors. While the TWFE-IV results reaffirm our main findings, we consider these results only of secondary importance because of possible weak instrument concerns and the inability of the TWFE-IV framework to allow for heterogeneous treatment effects. Most importantly, we do not consider endogeneity concerns a strong concern in our main specification.

B.3 Additional analyses

B.3.1 Accident severity

We investigate whether there is evidence that the average police-reported accident in a city after the introduction of e-scooters is significantly more or less severe. If this was the case, this would raise the question if the increase in accident numbers provides an indication for an increase harm. We find no evidence supporting a decrease in severity of reported accidents. Figure 10 and table 14 show estimates of the treatment effects on the percentage of accidents with personal injuries that involve only 'slight injury'. These estimates provide suggestive evidence 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.

The estimate in column 1 of table 14 has a corresponding 95% confidence interval from -1.8 to $+0.8$%-points), based on which we can reject even moderate decreases in severity, thus we see no grounds to assume that the increase in accidents involving e-scooters goes along with a decrease in accident severity. Note also that all our specifications include city-fixed effects that capture potential time-constant differences arising from estimating accident numbers from injury numbers.

To obtain these treatment effect estimates, a month-city level estimate of the percentage accidents involving slight injury versus severe injury or death is required. To construct estimates for this, we rely on the classifications by the local statistical agencies, as detailed in appendix A.1. While all countries report aggregated accident numbers, as used in the main analysis, countries differ in whether the numbers disaggregated by severity of injuries refer to the number of injured people or the number accidents. Austria, Germany, and Norway report the numbers of victims by severity of injury. Germany additionally reports the number of accidents, for the years cov-

ered in the Unfallatlas (Statistisches Bundesamt, 2022a) (see Appendix A). Finland, Sweden and Switzerland only report disaggregated numbers of accidents, i.e., for some countries the data allow us to compute the percentage of accidents with slight injury, while for others we can compute the percentage of victims with slight injury. For the 80 German cities we observe both. In these 80 cities, the two measures have similar means (85.3% of accidents involve severe injuries vs 87.1% of victims are severely injured) and a correlation coefficient of 90%. We combine the estimates across all six countries. We use linear regression, estimated on the sample of German observations where we observe the percentage of accidents as the percentage of victims with slight injury to project both into the same scale.

Figure 10: Monthly treatment effects and pre-trends on the percentage of police-reported accidents involving personal injury involving only 'slight injury'.
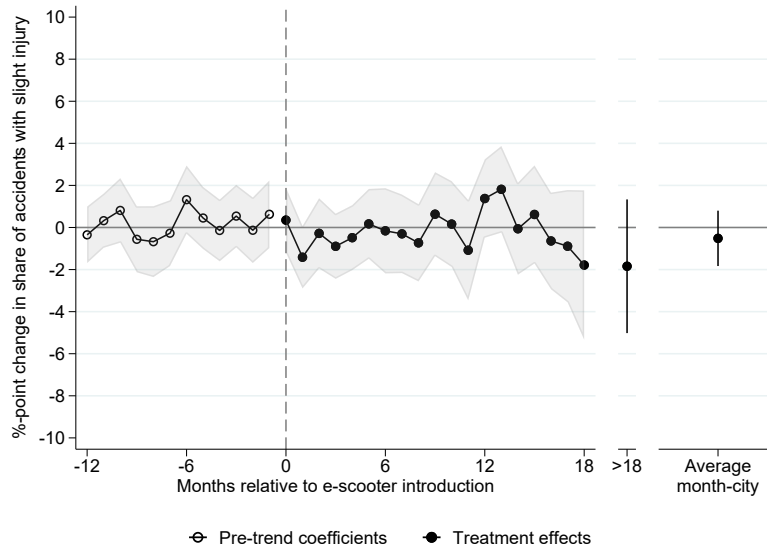
Notes: The figure shows average treatment effects relative to treatment introduction, on the percentage of accidents with only slight injury. Estimates have to be interpreted as percentage point changes. In the line plot, filled dots indicate treatment effect estimates for the first 18 months relative 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 materialize). So, monthly estimates for later months cannot be estimated with comparable precision as for earlier months. In the right part, the average treatment effect estimate and corresponding 95%-confidence interval, across all available post-treatment city-months is shown (table 14, col. 1). Shaded areas/bars indicate the 95%-confidence interval around the estimates, based on leave-out standard errors defining half-year of scooter launch as cohorts, as described in materials and methods, subsection 3. All estimates account for city fixed effects and month fixed effects.

Table 14: Estimated treatment effects on the percentage of police-reported accidents involving personal injury involving only 'slight injury'.

	(1) Main sample	(2) Incl. never-treated cities	(3) First 12 months
%-point change	-0.5	0.4	-0.3
	(0.7)	(0.5)	(0.5)
	[-1.8, 0.8]	[-0.5, 1.3]	[-1.2, 0.6]
Pre-treat. mean	85.4%	84.9%	85.4%
Treated observations	1704	1704	956
Total observations	5879	7133	5879
Cities	93	112	93

Notes: * p<0.1, ** p<0.05, *** p<0.01. This table shows estimated treatment effects from a specification similar to the one described in materials and methods, section 3, but using a percentage as the dependent variable. The estimation sample restrictions and weights correspond to table 10, columns 1-3. Standard errors are computed using the leave-out procedure recommended in Borusyak et al. (2023), defining cohorts based on the quarter in which scooters were launched. 95%-confidence interval in brackets.

B.3.2 Additional tables and figures

This section contains a number of tables and figures to support arguments in the main text.

Figure 11 plots city-level 3-months moving averages for the natural logarithm of accident numbers of all sample cities. The moving average is used to smooth out noise. The resulting figure illustrates that the logarithmized accident numbers follow a pattern, which can be well described through city- and month-level fixed effects.

Table 15 computes various aggregations of the monthly treatment effects to investigate differences between short-term and long-term effects and between effects on early adopters and on late adopters. This is suggestive evidence that effect sizes might be growing over time.

Table 15: Contrasting effect estimates for cities with early or late launches, and for the first or second year

	(1) Months 0–11	(2) Months 12–23	(3) Early adopters	(4) Late adopters
%-increase in accidents	5.3**	10.8***	4.6**	6.3**
	(2.1)	(4.1)	(2.3)	(3.2)
Cities	93	93	93	93
Total observations	5880	5880	5880	5880

Notes: * p<0.1, ** p<0.05, *** p<0.01. The table illustrates the average treatment effect estimates for the first 12 post-treatment months (col 1.), and for the second 12-month period (months 12-13) after the introduction of e-scooters (col. 2). Columns 3 and 4 show the average treatment effect estimates for "Early" and "Late adopters", considering the first 12 post-treatment months. "Early adopters" are cities in which e-scooters were launched on or before August 2018. "Late adopters" are cities that launched e-scooters after that. Standard errors in parentheses are computed using the leave-out procedure recommended in Borusyak et al. (2023) and defining cohorts based on the quarter in which scooters were launched. All estimations allow for month fixed effects and city fixed effects.

Table 16 repeats the heterogeneity analysis from table 2 in the annual DD framework. Quali-

Figure 11: After applying the natural logarithm, the seasonality in accident numbers runs parallel.
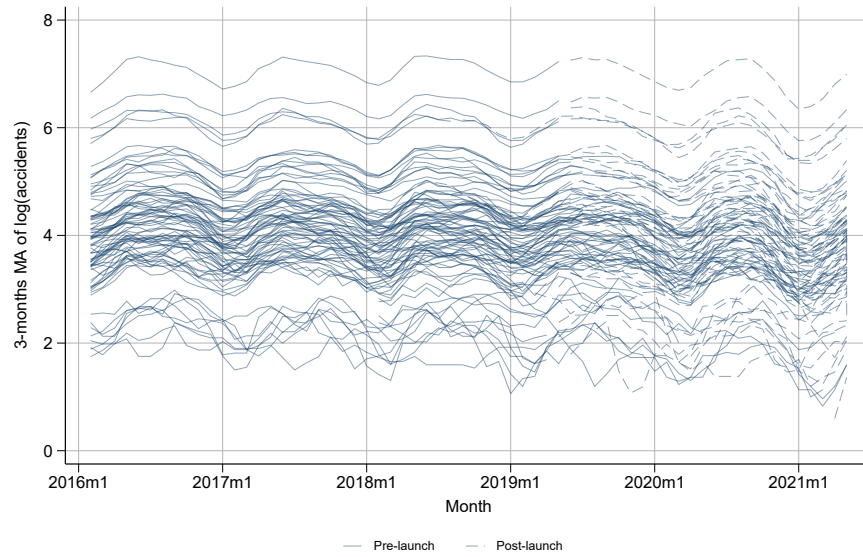
Notes: Each line represents the 3-month moving average of the natural logarithm of accident numbers for one city. Measures in the lower ranges are noisier due to the smaller absolute numbers. Dashed lines indicate accident numbers after the first launch of e-scooter services.

tatively the results are similar to the results from the monthly framework, estimating the largest effects for cities with a low share of bike lanes and for cities with the largest number of cars per capita.

Table 16: Heterogeneity of treatment effects, estimated in the annual difference-in-differences framework

	Share of bike lanes	Cars per capita	Cycling modal share
	(1)	(2)	(3)
Below-median:			
%-increase in accidents	11.7**	2.7	9.0***
	(4.8)	(4.0)	(2.9)
Above-median:			
%-increase in accidents	5.3	15.2***	9.7**
	(3.8)	(4.7)	(5.0)
p-value H_o: coefficients identical	0.29	0.05	0.91
Cities	75	75	75
Observations	150	150	150

Notes: * p<0.1, ** p<0.05, *** p<0.01. The table illustrates the average treatment effect estimates according to the annual DD framework for different subsamples that were split subject to the median of different city characteristics. Median splits are based on the country-level medians of each variable among sample cities. Standard errors in parentheses account for clustering at the city level.

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