

Impacts of micromobility on car displacement with evidence from a natural experiment and geofencing policy

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Micromobility, such as electric scooters and electric bikes—an estimated US\$300 billion global market by 2030—will accelerate electrification efforts and fundamentally change urban mobility patterns. However, the impacts of micromobility adoption on traffic congestion and sustainability remain unclear. Here we leverage advances in mobile geofencing and high-resolution data to study the effects of a policy intervention, which unexpectedly banned the use of scooters during evening hours with remote shutdown, guaranteeing near perfect compliance. We test theories of habit discontinuity to provide statistical identification for whether micromobility users substitute scooters for cars. Evidence from a natural experiment in a major US city shows increases in travel time of 9–11% for daily commuting and 37% for large events. Given the growing popularity of restrictions on the use of micromobility devices globally, cities should expect to see trade-offs between micromobility restrictions designed to promote public safety and increased emissions associated with heightened congestion.

Shared micromobility, such as electric scooters and bikes (e-scooters and e-bikes), has rapidly flooded cities, offering cheap and convenient first/last-mile solutions for urban visitors in over 100 US metropolitan areas¹. Shared micromobility is a strategy for progress towards transport electrification and is projected to be a US\$300 billion market globally by 2030^{2,3}. When e-scooters and e-bikes displace internal combustion engine vehicles, life-cycle assessments indicate net reductions in associated emissions and environmental impacts⁴. E-scooters and e-bikes are thought to substitute active modes of transport (for example, distances 0–5 miles) that include both commuting and recreational use^{5,6}, but evidence that micromobility adoption can ease traffic congestion or provide sustainability benefits through substitution of travel modes has been controversial⁷. Many cities have banned micromobility devices citing personal safety or other concerns, while other cities have allowed its proliferation largely without changes in urban

infrastructure needed for widespread adoption. A fundamental challenge to learn whether micromobility is a complement or a substitute for vehicle choice is largely behavioural.

Causal evidence of the impacts of micromobility on urban sustainability outcomes has, to date, been relatively weak, relying on self-reported usage data from survey questionnaires, which is subject to hypothetical, hindsight or recency bias. Other evidence on travel mode choice has typically relied on simulations from smaller datasets, which present modelling challenges related to population sampling and endogeneity concerns. As a result, behavioural evidence on whether micromobility adoption displaces cars has generated contradictory claims. For example, self-reported data from scooter providers in French cities have produced claims that e-scooter adoption decreases 1.2 million car trips in Paris or about 4% of car trips in Lyon^{8,9}. By contrast, other studies from Atlanta, San Francisco and Chicago have

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generated cross-sectional survey evidence that e-scooters and shared micromobility riders do not always displace cars but often substitute for public transit, walking or other forms of micromobility that may not necessarily decrease emissions^{10,11}. Further, researchers have estimated that emissions may be higher when electric scooters replace other transportation modes besides personal vehicles (life-cycle emissions estimates of 202 g CO₂-equivalent (CO₂e) per passenger mile for scooters versus 414 g CO₂e per passenger mile for passenger vehicles)^{4,12}. Despite evidence from life-cycle assessments that shared micromobility adoption produces net decreases in carbon emissions, mixed behavioural evidence and a lack of reliable data has left the effects of micromobility on urban traffic congestion and emissions unclear.

Here we provide evidence that restrictions by cities on e-scooter use leads to unexpected trade-offs between measures designed to enhance public safety and increased traffic and tailpipe emissions. Results from our natural experiment in a major US city reveal congestion effects resulting in a 9–11% increase in urban travel time for recurring evening trips and a 37% increase for large sporting events following a micromobility ban during evening hours. We estimate a potential national value of lost time of up to US\$536 million, which captures the opportunity cost of lost time in traffic. We discuss behavioural insights for short-run emissions reductions through the substitution of e-scooters for cars.

High-resolution mobility data and the No Ride Zone

Recent advances in real-time data collection allow us to leverage highly granular digital data from mobile platforms to estimate effects about travel decisions^{13–17}. First, digital data provide users with instant information about travel options and costs using geolocation and Global Positioning System (GPS) tracking⁷. Second, digital platforms provide convenient mobile payment at the point of use, simplifying the process of deciding between multiple modes of travel. Third, data interoperability across multiple travel modes could allow for more effective management of transportation services across jurisdictions. However, regional data on micromobility use has been particularly hard for cities, policymakers and researchers to access. This is because micromobility data is proprietary and controlled by private entities with closed ecosystems and data restrictions at various levels of aggregation. Here we show that when real-time mobility data is more widely available, it is possible to evaluate transport policies with stronger causal inference as compared to studies using cross-sectional and costly government transportation surveys.

In this study, we provide credible evidence of the effects of mass e-scooter and e-bike use on traffic congestion. We use high-resolution data from Uber Movement to analyse a policy intervention in the city of Atlanta in which micromobility devices were banned during evening hours from 9:00 p.m.–4:00 a.m. with mobile geofencing and remote shutdown, resulting in near perfect compliance^{18,19}. During the hours of the ban, micromobility devices from all providers are automatically disabled from mobile apps to create a No Ride Zone. This natural experiment provides a plausible identification strategy to discover how travellers respond to policy changes when scooters are unavailable for last-mile transit. This is important because prior claims about the substitution of micromobility with other transit modes have suffered from empirical challenges related to the lack of granular travel data, unreliability of self-reported information or confounding factors that could limit causal interpretations.

To address these empirical challenges, we conduct three quasi-experiments to evaluate policy impacts on both recurring mobility (for example, evening commuting patterns) and event-based mobility (for example, travel for special events), as depicted in Fig. 1. In our recurring mobility experiments, we compare passenger vehicle travel time in both the city centre (Midtown Experiment, Fig. 1a) and around transit hubs (Metropolitan Atlanta Rapid Transit Authority (MARTA)

Experiment, Fig. 1b) against various counterfactuals. Similarly, in our event-based mobility experiment (Mercedes-Benz Experiment, Fig. 1c), we identify banning policy impacts on travel time on days coinciding with large stadium events. Atlanta is an important field site for analysis because it is one of the largest adopters of shared micromobility, with multiple competing providers already servicing over 4 million e-scooter and e-bike trips per year²⁰. Atlanta's investments in micromobility ridership are part of a larger trend by cities to redesign streets to accommodate micromobility and to promote clean transportation alternatives²¹.

Micromobility mode substitution

What do people do when scooters are not available? There is a rich behavioural literature on the conceptualization and importance of travel mode choice as a habit^{22–28}. Such theories of behaviour change indicate that when habits are disrupted, people reconsider their options in the context of their individual attitudes and values. We know from Verplanken (2008), who originally coined the habit discontinuity hypothesis (for example, habit discontinuity effect), that when consumers face disruptions or unexpected changes, habits associated with the context are (at least temporarily) broken, too, and thereby provide opportunities for behavioural change²⁴. Context change, induced by the micromobility ban, is conceptualized to activate important values that guide travel mode decisions. For example, it is well known that consumers who are more environmentally conscious often change their behaviour in response to interventions by using a personal car less frequently²⁴. More generally, there is a substantial emerging literature on policy efficacy and habitual travel mode choice in the broader context of climate change and sustainable behaviours^{25,29}. Under this habit discontinuity hypothesis, those who hold pro-environmental attitudes are more likely to resort to other pro-environmental transit choices after the ban is put in place^{24,25}. These behavioural insights motivate our hypothesis that if micromobility riders are more environmentally conscious, then we predict that they might not revert to personal vehicles or ridesharing following the ban but instead revert to other more sustainable modes (for example, biking, walking, rail transit or other micromobility). Limited cross-sectional survey evidence from cities points in this direction^{10,11}.

We test two opposing mechanisms. If individuals revert to personal vehicles or ridesharing in lieu of micromobility, then we expect to find that the banning policy should increase traffic for both daily commuting and special events. However, if individuals choose not to revert to personal vehicles or ridesharing and instead choose the more pro-environmental option such as public transit or walking, then we should expect to find no statistically significant effect on travel time.

The Uber Movement travel time dataset is among the largest and most granular transportation datasets, aggregated from over 10 billion individual trips¹⁸. In our analysis, we leverage 47,477 observations of travel time data aggregated daily from passenger vehicle trips taken in the greater Atlanta metropolitan statistical area for the 90 days surrounding the policy implementation. Our outcome of interest is the mean average travel time per mile during evening hours, including hours when the ban is active. Our research design allows us to uniquely isolate a particular mechanism of micromobility mode substitution from scooters to either private cars, taxis or ridesharing, all of which are captured in our outcome data and have important implications for marginal emissions reductions. However, in this study we do not quantify substitution between e-scooter use and travel modes expected to have less effect on marginal emissions reductions, such as walking, rail transit or other micromobility trips. Additional details about the quasi-experimental design and measurement are in Methods.

Estimated urban travel-time effects

We evaluate treatment effects in the urban centre for both recurring and event-based mobility. For recurring mobility in the Midtown

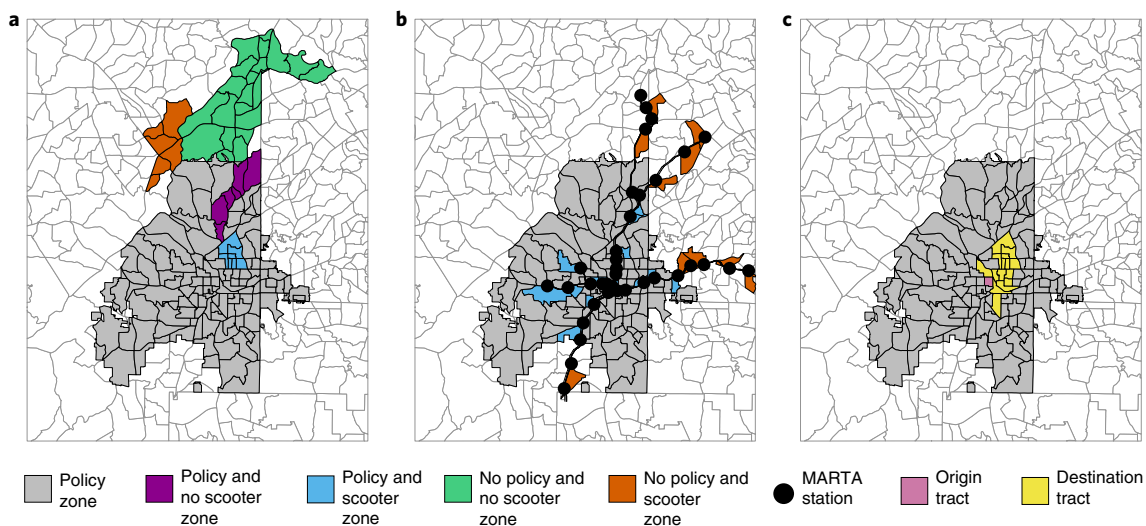


Fig. 1 | Quasi-experimental designs for recurring mobility and event-based mobility. Here we show the three quasi-experimental designs used to evaluate changes in travel time resulting from the evening micromobility ban in the city of Atlanta. Areas where the ban is enforced are shown in grey as the policy zone with various counterfactuals as the reference areas. **a, b**, The counterfactual analyses in the Midtown Experiment (**a**) and MARTA Experiment (**b**) measure the effects of the policy intervention on recurring mobility, such as daily commuting. **c**, The counterfactual analysis in the Mercedes-Benz Experiment measures the effects of the policy intervention on event-based mobility, such as sporting events. In **a**, the blue region represents the treatment area of interest in the city centre where scooters are available but are banned during evening hours. The colours purple,

orange and green are used to denote counterfactual regions with and without scooter availability, both inside and outside the policy zone. In **b**, we target analysis in blue regions near MARTA subway stations. These are then compared with counterfactual MARTA subway stations outside the policy zone, shown in orange. In **c**, we compare travel time before and after the policy from Mercedes-Benz Stadium, home to Major League Soccer matches and shown in pink, to nearby census tracts shown in yellow. The grey outlines represent US census tract boundaries. For all three quasi-experimental designs, we find statistically significant spillover effects of the policy on traffic congestion. More information about the statistical estimators and protocols used are in Methods.

Experiment, which measures travel-time impacts in the city centre, we find evidence of a congestion effect due to the banning policy of 0.241 (standard error 0.035) minutes per mile (Table 1). For an average commute in Fulton County, this translates to an estimated increase in evening commute times of 2.3 to 4.2 minutes per trip (between 373,000 and 679,000 additional hours for Atlanta commuters per year). For the typical commuter in Atlanta, this congestion effect due to the scooter ban translates to a 9.9% average increase in city travel time. Similarly, for the MARTA Experiment, which measures travel decisions around transportation hubs and with high levels of scooter use for last-mile transit, we find evidence of a congestion effect due to the policy ban of 0.255 (s.e. 0.051) minutes per mile. This translates to an estimated increase in evening commute times of 2.0 to 4.8 minutes per trip (between 327,000 and 784,000 additional hours for Atlanta commuters per year). For a typical commuter in Atlanta, this congestion effect due to the scooter ban translates to a 10.5% average increase in travel time. With these two different experimental designs, we find quantitatively similar congestion estimates for evening trips (for example, overlapping 95% confidence intervals). We infer that when scooters are not available, a statistically significant substitution between micromobility and personal vehicles occurs. For reference, based on the estimated US average commute time of 27.6 minutes in 2019³⁰, the results from our natural experiment imply a 17.4% increase in travel time nationally.

Similarly, for event-based mobility, we analyse nearby travel times pre- and post-policy for days of major sporting events at Mercedes-Benz Stadium. The timing of the ban coincided with Major League Soccer season. Given the more concentrated travel patterns during sporting events, we could expect to find a larger congestion effect from the banning policy as compared with our recurring mobility estimates. Consistent with this, we find an increase in travel time of 0.886 (s.e. 0.169) minutes per mile during soccer game days. For example, for a suburban resident who lives an average of 13 miles away from the city,

the ban produces an increase in travel time of 11.9 minutes in returning home from the soccer game, a substantial 36.5% increase in travel time.

We note that the congestion effects that we measure extend beyond typical sources of congestion including: traffic-influencing events (that is, as traffic incidents, work zones and weather), traffic demand (that is, fluctuations in normal traffic) and physical highway features (that is, traffic control devices and physical bottlenecks)³¹. Although a 2- to 5-minute delay for evening commuting and a 12-minute delay for special events could appear to be a minor inconvenience, the cost of additional time in traffic quickly adds up when aggregated across large commuter populations. In the next section, we quantify the potential economic impacts of these delays in dollar terms and consider the persistence of this congestion.

To contextualize these impacts, we converted our mean congestion effects to US dollars by using the published Value of Time (VOT) multiplier of US\$26 h⁻¹ for the city of Atlanta³². This results in an estimated impact for recurring mobility of US\$3.5 million to US\$10.5 million per year (Methods provide additional calculation details). For reference, the city revenues in permitting and device fees totalled half a million US dollars across 10,500 dispatched devices (the city of Atlanta collected US\$455,600 in permitting fees as of April 2019)³³. Although these costs are primarily internalized by commuters, the unintended damages are equivalent to approximately eight years of the city's micromobility operating revenues. On a national basis, we estimate that such banning policies could potentially be worth up to US\$536 million in congestion-related costs (Methods).

Behavioural persistence

To understand how these effects might change over time, we estimated daily treatment effects for the Midtown Experiment beginning with the day after policy implementation. These dynamic effects indicate immediate behavioural modifications in travel mode choice following the ban. Figure 2 reveals that a peak congestion effect of up to 0.8 minutes

Table 1 | Estimated travel-time increases

	I	II	III	IV	V
	Mean congestion effect (min per mile)		[Lower 95% CI, upper 95% CI] (min per mile)	Percent increase in travel time [†]	[Travel-time increases (min)] [†]
	Difference-in-Differences/ Fixed Effects	Triple differences			
Recurring mobility					
Midtown Experiment [‡]	0.201*** (0.025)	0.241*** (0.035)	[0.171, 0.311]	9.94%	[2.29, 4.17]
MARTA Experiment	0.255*** (0.051)	-	[0.150, 0.359]	10.50%	[2.01, 4.82]
		-			
Event-based mobility					
Mercedes-Benz Experiment [§]	0.886*** (0.169)	-	[0.554, 1.218]	36.54%	[7.43, 16.32]

Significant at the level $P < *0.1$, $**0.05$, $***0.01$. Standard errors are clustered at the origin tract level. [†]For this calculation, we use the average commute distance of 13.4 miles for Fulton County published by the Atlanta Regional Commission⁴⁸. [‡]The upper and lower 95% confidence intervals (CI) in Column III and the range of travel-time increases in Column V are based on the triple-differences estimator. [§]This estimate is based on a fixed effects estimator.

per mile (about an 11-minute delay for the average driver) occurs within the first five days of the policy change. We provide detailed point estimates for the daily treatment effects for the Midtown and MARTA experiments in Supplementary Table 1 and Supplementary Table 2, respectively. The immediate congestion that we observe is the result of the inability by riders to anticipate the ban or plan effective travel alternatives that do not also increase traffic during the first few days. We note that micromobility mode substitution such as cars or rideshare has an additive treatment effect, whereas mode substitution such as walking or public transit has a subtractive or negligible treatment effect, which does influence measurement. We find that after about a week, users partially account for the policy change in their travel planning and habits. This behavioural response suggests that as riders pivoted from micromobility devices back to personal cars or ridesharing, the congestion effect following the ban stabilizes to a mean treatment effect of 0.25 minutes per mile after five weeks.

Some may wonder why the effect of the ban initially tapers off before stabilizing to our final reported estimate. We acknowledge that it is not possible to fully characterize this phenomenon without more inductive or qualitative methods. However, in terms of possible mechanisms, we believe that after experimenting with other micromobility substitutes (for example, walking, rail, bus or other micromobility), riders gradually settle on their preferred alternative after two to three weeks of experimentation at which time the effect reappears and stabilizes using multiple methods and approaches. This behaviour is consistent with the habit discontinuity hypothesis that micromobility riders disrupt mobility patterns but do not necessarily revert to other sustainability-enhancing travel modes. We have some suggestive survey evidence for this mode settling. According to the Atlanta e-scooter survey, 42% of scooter users self-report that they would have made their trips by using a personal vehicle/rideshare had a scooter not been available¹¹. Although a full investigation of behavioural persistence beyond the 90-day period is out of scope in this study, we note that longer-term monitoring of the policy implementation becomes more difficult to justify as a source of exogenous variation. In future research, we suggest further study into scooter use volumes and mechanisms of mode substitution to better understand the relationship between short-run behavioural modification and long-run habit formation for micromobility use. Given that these types of policy interventions are becoming more prevalent, it will be critical for decisionmakers to weigh the relative priorities between public safety and traffic congestion, which is already estimated to cost up to US\$166 billion annually in the United States³⁴.

Critics of micromobility solutions point to the fact that scooters may not displace cars and hence do not achieve sustainability

co-benefits¹². Contrary to this view, we find that commuters revert to car-based travel (for example, personal vehicles, ride sharing or ride hailing) once micromobility devices are not available, resulting in statistically significant increases in travel time not intended by the original policy. These findings are consistent with other studies in Seattle and Beijing, for example, which suggest that micromobility rides can replace up to 18% of short car trips in congested corridors or mitigate traffic around subway stations by up to 4%, respectively^{35,36}. We find that the dominant behavioural response by riders is to substitute micromobility with cars. Although we do not observe micromobility trips directly, 52% of surveyed micromobility users in Atlanta reported that they used a scooter from least a few times per month to several times per week during the period of our study¹¹. Our results also indicate that micromobility users were largely not driven by environmental considerations in their travel mode choice following the safety regulation. This is important because as the micromobility user base is growing and consumer preferences are shifting towards longer e-scooter trip distances³, micromobility adoption presents increased opportunities to achieve emissions reductions from a broader set of consumers who are not necessarily environmentally conscious.

The results of this policy experiment affirm the importance of technology-based advances in mobile geofencing as a strategy to increase behavioural compliance. Observing near perfect behavioural compliance in response to environmental or safety regulations has been rare. These technology-based advances are helpful for policy analysis and impact evaluation but also raise challenges related to data access and governance. The availability of digital data streams can allow governments and policymakers to address gaps in service provision for urban mobility, but private platforms have little incentive to share proprietary data with decisionmakers. Several global organizations, such as the United Nations's Economic and Social Council and World Data Forum, have called for governance mechanisms and partnerships to support the implementation of disaggregated, high-quality open data for sustainable development³⁷. For example, bike-sharing platforms have similarly been shown to reduce car trips in the United States, Great Britain and Australia³⁸. Despite these national and international efforts, many practical challenges remain, and we suggest the following local and regional policies with respect to micromobility data infrastructure. On the basis of our discussion with city officials and data providers, disclosure policies should need to be developed so that city partners have a process for anonymizing and aggregating records that are granular enough for a wide range of analyses, while ensuring privacy protections for personal data from re-identification. For example, the Uber Movement makes data available at granular enough intervals to

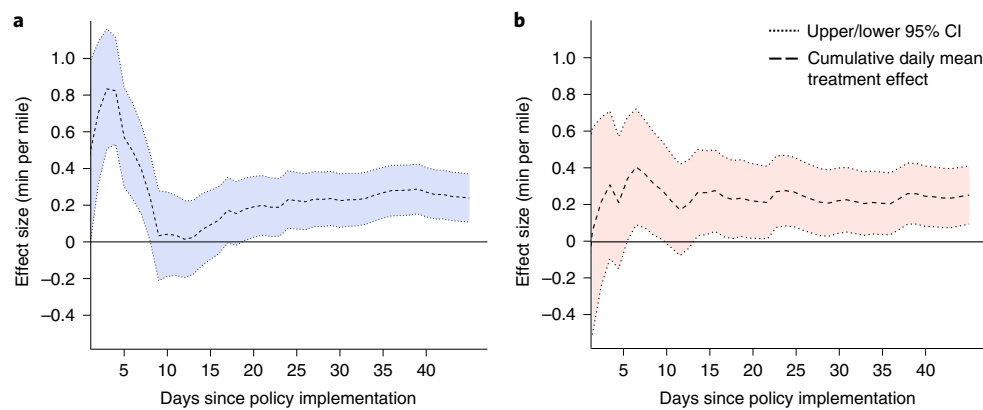


Fig. 2 | Dynamic treatment effects in the city centre. Dynamic treatment effects are estimated daily following the policy intervention. **a, b**, We report estimates from the Midtown Experiment (**a**) and the MARTA experiment (**b**), each beginning with the day after the policy implementation. We report an effect each day beginning the day after the policy implementation, 10 August 2019 through

22 September 2019. The upper and lower 95% confidence intervals are shown by the shaded regions above. We find that peak congestion effects occur within the first week following the policy implementation for both the Midtown and MARTA experiments, likely reflecting a temporary reversion to travel in personal vehicles as commuters adjust to the micromobility ban.

be applicable for time series analysis while also protecting the privacy of Uber users. Second, ensuring continuity and consistency in archival data access will be necessary, particularly when smaller data owners exit the market or services are otherwise interrupted. This could be considered through the issuance of licences to operate micromobility devices. Third, data standards are needed at a regional scale to enable interoperability at different levels of aggregation and time periods. The Uber Movement data releases provide a promising path forward.

Conclusions

Decisions that shape our cities can lead to unexpected effects. We have established that when scooters and e-bikes are banned, drivers experience statistically significant increases in traffic congestion as many riders revert back to passenger vehicles for last-mile transit. Due to the precise nature of the intervention, we observe effects that are greatest in the first few days after the micromobility ban but show durability for many subsequent weeks. The persistence of these effects may compound the economic costs of increased traffic congestion, which we estimate can be worth up to US\$536 million globally (Methods). It remains unclear whether greater public awareness of these unintended congestion effects could shift public pressure on micromobility bans. Metropolitan areas around the world such as Singapore, Montreal and West Hollywood have instituted bans and other restrictions on shared micromobility, which risks further economic costs of increased commuter travel times. To accelerate the adoption of micromobility and achieve its associated sustainability benefits, we argue that cities will need to make additional investments in both physical and digital infrastructure. For physical infrastructure, land use and space allocation will require longer-term planning such as converting lanes usually reserved for cars into bike lanes that can be used for micromobility. If further micromobility adoption happens at the expense of ‘polluting’ modes like private vehicles or other car-based travel, then these investments become even more critical for urban sustainability and will carry larger policy implications. We are already seeing evidence of this in large cities such as Milan, Brussels, Seattle and Montreal³ and mid-sized cities such as Raleigh, NC, Alexandria, VA, and Tucson, AZ¹⁰. With its potential to displace cars for personal travel and drive short-run emissions reductions, micromobility is poised to continue its strong growth as an urban mobility solution.

Methods

Geofencing policy

The micromobility ban was implemented in the city of Atlanta on 9 August 2019. We use high-resolution data from 25 June 2019 to 22

September 2019 from Uber Movement to measure changes in evening travel times between 7:00 p.m. and midnight, pre- and post-policy implementation. This allows for a window of analysis of 45 days pre- and post-policy implementation (Supplementary Fig. 1). We designed three quasi-experiments to evaluate both recurring mobility (for example, daily community patterns) and event-based mobility (for example, travel for special events). The policy zone covers a total land area of 136.8 square miles (354.3 square km) as shown in Fig. 1. Unlike other interventions such as fines or usage rules that might discourage but do not eliminate scooter riding, we are able to observe treatment effects with near perfect compliance. This is because the mobile apps digitally shut off access to all devices during non-operating hours automatically between 9:00 p.m. and 4:00 a.m. with mobile geofencing.

The travel time data, as provided by Uber Movement, are derived from anonymized and aggregated trip location data that are spatially resolved to the nearest census tract. We downloaded intra-day travel times at the highest resolution available that includes the start of the ban, which Uber defines as between 7 p.m. and midnight. Thus, we analysed evening peak hour congestion impacts before and after the policy, where there is a time overlapping of peak hours and policy implementation hours that could be leveraged for the analysis. Because the travel distance for every tract may differ, we normalized the travel time data by the distance between origin and destination tracts. This allows for direct comparisons between trips to different parts of the city. The dependent variable for analysis in the Midtown and MARTA experiments is therefore the daily evening travel time per mile (Supplementary Table 3 provides descriptive statistics). In the Mercedes-Benz Experiment, we normalize the travel time per mile by the number of attendees to each event during July and August. In this way, we mitigate the possibility that during post-ban dates there could be more people at the stadium than before.

The independent variables include location-based statistical controls such as census tract characteristics, proxy variables for number of transit alternatives and measures for common time trends that could impact travel times including daily precipitation and time dummies. The census tract characteristics are variables that impact traffic congestion in the area include the number of vehicles owned per tract, which measures residential density. Because the ban was implemented coincident with the academic school year, we include school enrolment per tract as a control for differential impacts on traffic due to school size. The transit alternative variables impact travel mode choices made by commuters and include the number of transit routes, Walk Score and number of bike-share hubs. We also considered other transit alternative

variables such as the Transit Score, but these could not be used in the analysis due to high correlation with other features. Because travel patterns may differ during rainy weather, we include a dummy variable for daily precipitation during the evening. To merge precipitation data with the tract-level observations, we found the nearest weather station to each tract using published data from the National Oceanic and Atmospheric Administration³⁹. It is possible that there could be different congestion effects on weekdays and weekends. Additionally, general traffic congestion could increase during the summer months such as mass gatherings during summer events. To capture this and other unobserved time-varying factors, we include monthly and day-of-the-week dummies. We include descriptive statistics by area in Supplementary Table 4 and provide additional descriptors for our variables in Supplementary Table 5.

Experimental design

To analyse the effects of the policy intervention, we implemented various counterfactuals chosen carefully to mitigate the observable bias between treatment and control areas. For example, in the Midtown Experiment, Cumberland areas were chosen as counterfactual because of statistically similar observable characteristics including median age, median income, race distribution and education level. Other counterfactuals that we tested include Sandy Springs and Buckhead (Fig. 1a). Although these are similar in socio-economic characteristics, we did find statistically significant differences in vehicle ownership between counterfactual areas as measured in the American Community Survey provided by the US Census⁴⁰. For this reason, we included vehicle density per tract as described above. In the MARTA Experiment, subway stations outside the policy zone and within the same train system were chosen as a counterfactual because of similarities on transit services and amenities provided to commuters (Fig. 1b). For example, banks, pharmacies, hospitals and gyms are all typically within ten minutes or less walking distance from a station and a common set of intermodal transit alternatives. In the Mercedes-Benz Experiment, we study travel time per mile from the Mercedes-Benz Stadium to destination tracts in nearby areas permitted for scooter use (Fig. 1c).

For the econometric analyses in the Midtown experiment, we implement a difference-in-differences estimator that compares mean travel time per mile for the policy zone and counterfactual pre- and post-policy. To provide more robust quantitative estimates, we also implement a triple-differences (DDD) estimator with secondary counterfactuals, as DDD models can reduce bias relative to a difference-in-differences approach, especially in the presence of any omitted variables⁴¹. The unit of analysis is at the tract level. Each mean travel time per mile, Y , is calculated for a given time period and area of the city. Equation (1) describes the DDD estimator.

$$\begin{aligned} \text{DDD} = & ((Y_{P,S}^{\text{post}} - Y_{P,S}^{\text{pre}}) - (Y_{NP,S}^{\text{post}} - Y_{NP,S}^{\text{pre}})) \\ & - ((Y_{P,NS}^{\text{post}} - Y_{P,NS}^{\text{pre}}) - (Y_{NP,NS}^{\text{post}} - Y_{NP,NS}^{\text{pre}})) \end{aligned} \quad (1)$$

To designate the policy zone, P represents the areas affected by the policy ban and NP represents the area not affected by the ban. To designate scooter service areas, S represents areas where micromobility services are available and NS represents areas where micromobility services are not available. Given the unexpected nature of the policy ban and its timing, our identification strategy allows us to estimate treatment effects during evening hours. We are not able to estimate congestion effects during other hours of the day.

To validate the assumptions of our statistical estimators, we present parallel time trends pre-policy in Supplementary Fig. 1. We note that for the triple-differences design in the Midtown Experiment, the secondary counterfactuals in Sandy Springs and Buckhead tracts are generally parallel but do not strictly need to be to achieve statistical identification with triple differences^{41,42}. We also included several additional control variables that could also impact travel time per

mile. For example, we included dummies for the existence of large co-events (for example, State Farm Arena, Truist Park, Music Midtown, large concerts and so on) in our Midtown and MARTA experiments and included additional time dummies (such as weekly) as covariates in the regression models to mitigate other time variability.

Seasonal variation in travel-demand patterns

Prior studies have established that there could be seasonal variability in travel patterns, particularly during the summer months, that could affect the uncertainty in our impact estimates^{43,44}. It is well known that day-to-day travel behaviour can experience higher variability when using trip-based methods as compared to time budget methods⁴⁵. Specifically, Elango et al. (2014) find that households with children in Atlanta exhibit high-travel-demand variability during the summer. To address the role of high-travel-variability households, we performed a series of additional robustness checks for both our Midtown and MARTA experiments. To mitigate the effect of high-variability households, we tested an additional control variable in model specifications using school enrolment as a proxy for households with children. We found quantitatively negligible differences with either a difference-in-differences estimator or triple-differences (DDD) estimator (Supplementary Table 6). Our estimates are robust to one-way and two-way clustering⁴⁶ (Supplementary Table 7), inclusion of additional controls related to travel-demand variability including school enrolment and large event indicators. On the basis of this evidence, we reasonably conclude that high variability due to seasonality is not a major driver in the uncertainty of our estimates.

Placebo tests

Scholars have established that the standard deviation of a travel time per unit distance often has a linear relationship with its corresponding mean value⁴⁷. Given the higher average travel time in treatment tracts versus counterfactual tracts, it is possible that our effects could be influenced by this difference in variability. To ensure the robustness of our results to any differences in tract variability, we implemented placebo tests in two ways. First, we replicated our data collection process to gather out-of-sample travel time data for 12 months before our natural experiment, using a similar date range as used in our main analysis. This gave us a total of 20,189 travel-time observations across the same 40 census tracts used in the main analysis for placebo tests. As expected, we recovered treatment effects not statistically different from zero with the same treatment and counterfactual tracts. These results, shown in Supplementary Table 8, are also robust to various one-way and two-way clustering options. Thus, we conclude that differences in tract variability are unlikely to artificially drive our estimates.

Second, we also conducted placebo tests with all in-sample data before the ban by testing for treatment effects two weeks before the actual policy intervention in the MARTA and Midtown experiments. As expected, these placebo tests revealed treatment effects that were not statistically different from zero. These additional analyses are presented in Supplementary Table 9.

Estimated travel-time increase

To calculate the estimated increase in travel time for a typical commute in the city of Atlanta, we multiply the mean congestion effect from our experiments by the average distance of a typical commute in the city. The Atlanta Regional Commission estimates that, on average, a resident of Fulton County drives 13.4 miles to work each way⁴⁸.

Calculating economic damages from increased congestion

To calculate the economic damages from increased congestion, we used the published Value of Time (VOT) estimates for the city of Atlanta, which is US\$26 per hour spent in traffic in the evening³². This value allows us to generate more conservative estimates of economic damage than if we were to use the US\$36 VOT estimate for morning trips. To get the total

number of trips, we referenced the number of daily commutes in Fulton County and share of evening commutes (approximately 11%) to get a more precise estimate^{49,50}. For example, for the Midtown Experiment, the estimated congestion effect of 0.241 minutes per mile is multiplied by the average commute distance of 13.4 miles, which results in a value of 3.23 minutes per trip. To get the economic impact, we convert from minutes to hours and multiply this figure by the VOT of US\$26, which gives an impact of US\$1.40 per trip. The derived economic impact in this example is US\$4.9 million per year. The ranges that we report in the paper of US\$3.5 million to US\$10.5 million reflect the congestion effects from the upper confidence interval of the MARTA experiment and lower 95% confidence interval of the Midtown Experiment. These estimates reflect only the direct effects of the VOT and do not include other indirect effects.

National value of time lost

We estimated the potential value of lost time in traffic at the national level in two ways. In the first approach, we used our lower bound on the aggregate time lost by Atlanta drivers from the MARTA experiment of 327,000 hours and the VOT estimate of US\$26 from the city of Atlanta and then scaled to a per capita value of US\$17.41. We then multiplied this value by the US population to arrive at an aggregate loss value of US\$5.73 billion. To generate a conservative estimate, we assume that only 10% of the US population experiences the increase in traffic congestion due to a micromobility ban for a final value of US\$573 million. Additionally, we calculated an estimate using an approach that assumes the ban is experienced by all individuals living in an urban centre in the United States, or 71.2% of the population, to calculate an upper limit on the potential national value of lost time. Under this assumption, our estimate for national value of lost time rises to US\$4.08 billion.

In the second approach, we started with our estimate of potential economic loss in the city of Atlanta (US\$10.5 million) and generate a per capita value based on the population of Atlanta of US\$22. We then scale this to the US population living in urban centres, once again assuming that 10% of the population is impacted by the ban, to arrive at a national estimate of US\$536 million.

COVID-19 impact statement

Although there have been substantial impacts of COVID-19 on travel patterns, the results derived in this study are not affected by the pandemic response because the time period analysed in the study occurs at least six months before the restrictions implemented in the city.

Data availability

The datasets generated and/or analysed during the current study are available in the Zenodo repository, <https://doi.org/10.5281/zenodo.4924424>. Spatial and neighbourhood features are downloaded from AllTransit, Walk Score, the Census American Community Survey and the National Oceanic and Atmospheric Administration's National Center for Environmental Information. The raw travel time data for the city of Atlanta are publicly available from Uber Movement, 2022 Uber Technologies, Inc., at <http://movement.uber.com>. Source data are provided with this paper.

Code availability

To support scientific replication, all computer code used to generate the study's main findings are available in the Zenodo repository, <https://doi.org/10.5281/zenodo.4924424>.

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Competing interests

The authors declare no competing interests.

Additional information

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