When Curfews Backfire: Trip Inflexibility, Spillovers, and Road Safety

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March 27, 2026

Abstract

Curfews substantially raise the cost of being on the road at specific times of day. We study how this punitive timing policy affects road safety using a dusk-to-dawn curfew implemented in Kenya. Road traffic crashes fall during curfew hours, but these gains are fully offset by large increases in crashes immediately before the curfew. Crash risk conditional on exposure rises. Evidence from congestion and speed data indicates intertemporal compression of travel into the pre-curfew window and sharp increases in speed as the curfew boundary approaches. Comparative statics support this mechanism. The initial 19:00 curfew binds directly against inflexible evening work trips, generating congestion, speed increases, and crash spillovers before curfew. When the curfew is shifted to 21:00—after the evening commute—spillovers disappear, while reductions in curfew-hour crashes persist. A similar curfew in Paris, implemented in a context of widespread work-from-home that substantially reduced inflexible evening travel, shows no pre-curfew spillovers, consistent with the mechanism. We conclude that the road safety effects of curfews hinge on trip inflexibility and timing of travel demand. Curfews can backfire when they bind against peak-hour trips that cannot be foregone or rescheduled, a consideration that is especially relevant as curfews continue to be used globally as non-price mobility restrictions in contexts of social unrest, public order, and emergency response.

JEL Codes: R41, R42, I18
Keywords: Congestion Externalities, Curfews, Road Safety, Pandemic, Vehicle Crash, Behavioral Response, Temporal Displacement.

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‡The authors are grateful for the long-term collaboration with transport authorities in Kenya, including Kenya's National Police Service (NPS) and the Kenya Urban Roads Authority (KURA), which made this work possible. We thank the NPS, KURA and Flare for sharing data used in this paper. Kelvin Gakuo provided excellent data analysis and support, and Christine Okeyo provided excellent field support on the ground. We are grateful to Jishnu Das, Arlen Guarin, the seminar participants and discussants at the 2023 Meeting of the Urban Economics Association and anonymous reviewers for their useful comments and feedback. The research benefited from financial contributions from the United Kingdom Foreign, Commonwealth and Development Office (FCDO), the European Union (EU), the World Bank's Umbrella Facility for Impact Evaluation (i2i) and the World Bank's Knowledge for Change Program (KCP). The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the International Bank for Reconstruction and Development/World Bank and its affiliated organizations or those of the Executive Directors of the World Bank or the governments they represent. Computational reproducibility verified by DIME Analytics. The list of authors is in alphabetical order.

1 Introduction

Policies that change the relative cost of being on the road at different times are often used by governments to mitigate externalities arising from mobility—typically to reduce congestion or pollution, or to support public health objectives. Yet these policies themselves can generate externalities. One important but understudied margin is road traffic safety. Increasing the cost of being on the road has theoretically ambiguous implications for road safety. Fewer vehicles reduce crash risk because each additional driver imposes an externality on others (Edlin and Karaca-Mandic, 2006), but lower density may enable higher speeds, increasing the probability of a crash among those still driving (Ashenfelter and Greenstone, 2004). When restrictions are time-bound, they can also generate spillovers if drivers shift mobility to adjacent hours. The net effect on road safety therefore depends on behavioral responses of road users.

We study curfews—a widely used, non-price mobility restriction—as an extreme policy that sharply increases the marginal cost of being on the road at specific times. In 2020, COVID-era national dusk-to-dawn curfews in Kenya—and subsequent shifts in their start time—generated quasi-experimental variation in curfew timing within a lower-middle-income country (LMIC). This context is globally relevant: 92% of road traffic fatalities occur in LMICs despite accounting for 60% of the world's vehicles (WHO, 2023). Leveraging a multi-year investment that digitized police road crash records and integrated them with high-frequency mobility and speed data (sensor-generated traffic counts, Waze, Uber, Google Maps), we measure both crashes and crash risk conditional on exposure (crash probabilities). We exploit variation in curfew timing within Kenya and complement it with cross-context comparative statics from Paris.

Using difference-in-differences and triple-difference designs—benchmarked against pre-trends and sensitivity analysis following Rambachan and Roth (2023), we find that curfews substantially reduce crashes during curfew hours. However, these gains are offset by large spillovers: crashes increase sharply in the hours immediately preceding the curfew, fully canceling the reductions during curfew hours. Conditional on exposure, crash risk rises during the pre-curfew window. We show that these spillovers arise from intertemporal compression of travel into the pre-curfew window combined with sharp increases in speed as the curfew boundary approaches. These responses are consistent with high penalties for violation interacting with inflexible evening commute trips.

We further examine comparative statics that illuminate this mechanism. When Kenya shifts the curfew start from 19:00 to 21:00—after the evening commute—spillovers disappear while reductions in curfew-hour crashes persist. This contrast isolates the role of trip inflexibility: curfews backfire only when they bind against peak-hour trips that cannot be foregone or rescheduled. We complement the Kenyan evidence with evidence from Paris, where a similarly timed curfew was implemented in a setting with substantially more flexible evening travel, due to widespread work-from-home. In this context–where the conditions for the mechanism are substantially weaker and therefore predict little to no pre-curfew spillovers, we find that the curfew reduces crashes during curfew hours without generating pre-curfew spillovers.

Our contribution is threefold. First, we contribute to the literature on congestion pricing and road safety (Green et al., 2016; Tang and van Ommeren, 2022). By studying a non-price mobility restriction that forces temporal displacement, we extend the congestion externality literature beyond price-based instruments, while also providing insights for behavioral responses when the congestion price is binding and time flexibility and public transport options are limited. Existing literature has also largely focused on high-income settings. We provide evidence from an LMIC setting using directly measured exposure, speed, and crash outcomes—where baseline crash risk is extremely high and where congestion policies will increasingly be considered as mobility grows. Our findings also provide a new behavioral implication of the value of urgency documented by Bento et al. (2020). In their setting, a hard schedule constraint with a discrete penalty induces drivers to pay a toll to guarantee on-time arrival. We show that when the same constraint structure applies — a hard deadline with severe penalties — but a price instrument is unavailable, drivers instead accept higher crash risk as the implicit cost of meeting the constraint. This suggests that the urgency response is robust across instruments: when drivers cannot buy their way to on-time arrival, they speed instead.

Second, we contribute to the limited literature on the unintended consequences of curfews (Carr and Doleac, 2018). We show that curfews can worsen road safety when applied broadly to the population rather than only to inexperienced drivers (Williams and Preusser, 1997; Bolduc et al., 2013). By identifying intertemporal trip compression and increased speeds as the curfew start approaches as the key mechanism, we also provide insights for the design of safer curfew policies.

Third, our results expand the emerging evidence on the externalities of pandemic mobility restrictions (see Brodeur et al. (2021) for a review). The literature has largely focused on full lockdown measures, with the limited studies on road safety generally finding reductions in crashes (Brodeur et al., 2021; Lin et al., 2021). Unlike lockdowns, which reduce overall mobility, curfews impose sharp timing constraints that can compress travel into a narrower time window. While mobility restrictions were implemented during the pandemic in more than 90 countries (Brodeur et al., 2021), their road-safety effects remain largely underexplored. Our findings show that such policies can generate unintended safety risks when they bind against peak-hour travel.

2 Background and Conceptual Framing

Road traffic crashes (RTCs) are the leading cause of death for children and young adults age 5-29; between 20 million and 50 million people suffer non-fatal injuries each year (WHO, 2023), imposing large social costs through mortality, disability, and lost productivity.[1] Policies that alter road use, whether by design or as a side-effect, therefore have first-order consequences for public health in these settings. Curfews are one such policy. They sharply raise the marginal cost of being on the road at specific hours, but unlike congestion pricing or tolls, do so through non-price

[1]Following World Health Organization current practice, we use "road traffic crashes" rather than "accidents." The term "accident" implies inevitability, whereas most crashes are preventable through changes in behavior, design, and policy (WHO, 2023). Several papers we cite use the term "accident externality" (e.g., Edlin and Karaca-Mandic, 2006); when referring to this literature we retain the original terminology when necessary, but throughout our empirical work we refer to road traffic crashes.

enforcement. Curfews are used globally—in contexts of social unrest, public order, crime prevention, youth graduated licensing—and were also widely used during the COVID-19 pandemic. Despite their widespread use, curfews remain understudied as a mobility policy with potential externalities for road safety.

We study curfew policies implemented in Kenya in 2020. On March 27, 2020, Kenya implemented a dusk-to-dawn 19:00–05:00 curfew (OSAC, 2020), following school closures and work-from-home recommendations on March 15 and restaurant/bar operating restrictions on March 22 (Ministry of Health, 2020; Kagwe, 2020). On June 7, curfew start time was shifted to 21:00 (Kai, 2020). Penalties for violation included fines of up to KSh 10,000 (approximately US$100), imprisonment for up to three months, or both (Government of Kenya, 2022). For context, this fine is roughly one third of the median monthly wage for urban employed workers in Kenya.[2] The curfew was strictly enforced, with reports indicating that Kenyan Security forces detained those out after the curfew (Bearak and Ombuor, 2020). Exemptions existed for certain critical workers such as health professionals and utility workers (Government of Kenya, 2022). Kenya's baseline road safety risk is extremely high: the estimated fatality rate is 28.2 deaths per 100,000 population—substantially higher than the already highest regional average for Africa (19.4 per 100,000) (WHO, 2023). We focus on Nairobi, the capital of Kenya, and the county with the most fatal crashes (Republic of Kenya, 2024).

To frame interpretation, we draw on simple canonical congestion externality literature linking traffic flow and road safety. Holding speed constant, more vehicles increase potential vehicle encounters and thus raise crash risk via the marginal externality of an additional driver (Edlin and Karaca-Mandic, 2006; Newbery, 1988). Holding density constant, higher speed increases both the probability and severity of road crashes (Ashenfelter and Greenstone, 2004; Bauernschuster and Rekers, 2022). Policies that sharply change the cost of being on the road therefore operate through both density and speed, and their net effect is theoretically ambiguous. A curfew is an extreme version of this: the marginal cost of being on the road jumps discretely at a known time threshold, generating both (i) a large collapse in density during curfew, and (ii) potentially large behavioral responses just before curfew. When costs rise discontinuously, drivers may respond at both the extensive margin (foregoing trips) and the intensive timing margin (shifting trips to lower-cost periods) (Kreindler, 2024). Among drivers whose evening trip timing is inflexible, this sharp discontinuity induces intertemporal compression of travel into the pre-curfew window, which can simultaneously increase density and generate urgency as the curfew start approaches. As shown by Bento et al. (2020), discrete penalties for being late create sharp increases in the marginal cost of delay, leading individuals to accelerate travel and adjust behavior as deadlines bind. Applied to curfews, this urgency channel implies that drivers facing a hard cutoff may respond by increasing speed immediately preceding the curfew. Thus, whether curfews improve or worsen road safety is ultimately an empirical question that depends jointly on speed and density responses both during curfew hours and just before curfew—especially when trip timing is inflexible.

[2]Median monthly wage earnings among urban employed workers in Kenya were approximately KSh 30,000 in 2019 (Kenya National Bureau of Statistics, Economic Survey 2020). Approximate exchange rate KSh 102–105 per US$1 during March–June 2020 based on Central Bank of Kenya historical monthly averages.

3 Data

We combine administrative crash records with multiple high-frequency mobility and traffic data sources to measure road safety outcomes, exposure, and driving behavior. An overview of data sources and variable construction is provided in Table A.1.

3.1 Road Traffic Crash Data

Our primary outcome data come from administrative records of road traffic crashes (RTCs) produced by the Kenya National Police Service (NPS). We worked with the NPS to digitize all paper-based Situation Reports from the 14 traffic police stations in Nairobi, creating a novel dataset with precise information on crash timing, location, and severity. These reports cover crashes involving injuries, fatalities, or substantial property damage. The final dataset includes 9,760 crashes recorded between January 1, 2014 and December 31, 2020. While minor crashes not reported to the police are not captured, these data represent the most comprehensive source of citywide information on serious RTCs in Nairobi.

As a robustness check, we cross-validate police data with independent emergency dispatch records from Flare, Kenya's largest ambulance coordination platform. Flare aggregates emergency calls from bystanders (50% of calls), police (25% of calls), and private fleets (25% of calls) and operates a centralized 24/7 dispatch system. Aggregated hourly crash counts from Flare closely track police-reported crashes during overlapping periods (2019–2020), supporting the reliability of the administrative data.

3.2 Mobility, Congestion, and Speed Data

To study mechanisms underlying changes in crash risk, we combine several complementary sources of high-frequency mobility data that capture congestion, exposure, and speed.

We measure congestion using traffic delay data from the Waze for Cities program (Waze, 2025). Waze provides geocoded information on traffic jams—including speed and delay—updated at two-minute intervals. We aggregate total traffic delay across all road segments and use this measure as a proxy for congestion citywide.

To proxy for vehicle exposure, we use traffic counts from road sensors installed at six major intersections in Nairobi by the Kenya Urban Roads Authority. While sensor coverage is limited in time and space, these locations span major commuting corridors. We combine observed counts with Waze congestion data to predict hourly vehicle volumes during periods without direct sensor coverage. Appendix B describes the prediction model and validation exercises; predicted and observed counts are highly correlated across all intersections. We summarize exposure using an index of vehicle counts normalized to pre-curfew hourly baseline values and averaged across intersections. This index captures relative changes in vehicles on the road over time rather than absolute traffic volumes.

To measure speed, we collect real-time travel times from Google Maps along thirteen major road segments that

span Nairobi's primary commuting corridors. Travel times are queried every 30 minutes and converted into average speeds by segment length. We validate these measures using Uber Movement data, which report independently aggregated speeds at the road-segment level through March 2020. Speed patterns from the two sources align closely during daytime and pre-curfew hours (Appendix Figure A.6). Because traffic volumes are extremely low during late-night curfew hours, speed estimates in those hours are noisier; accordingly, we focus our speed analysis on daytime and pre-curfew periods.

3.3 Time Windows and Measurement Considerations

All analyses distinguish three time windows: daytime (5:00–17:00), pre-curfew (17:00–20:00), and curfew (20:00–5:00) for the initial Nairobi curfew. Although the curfew formally begins at 19:00, we classify the 19:00–20:00 hour as pre-curfew to account for (i) delays between crash occurrence and police reporting around the curfew boundary, and (ii) behavioral continuity, as drivers on the road shortly after 19:00 are typically completing trips initiated before curfew enforcement. We construct analogous time windows for the other two curfews we study. For crashes and congestion we take the sum during the time window, while for speed and the index for vehicle exposure we use the average value.

Finally, while smartphone-based mobility data can raise concerns about selection (Milusheva et al., 2021), our congestion and speed measures are driven by aggregate traffic conditions rather than individual device ownership. Moreover, consistency across independent data sources (Waze, Google Maps, Uber, and traffic sensors) provides reassurance that observed patterns reflect underlying changes in driving conditions rather than measurement artifacts.

4 Empirical Framework

Our main empirical strategy is a modified double-difference specification modeled after Tarduno (2021). We compare crashes in the period after the first curfew was implemented (March 27-June 6) to the period before any COVID-19 policies were implemented (January 1-March 15). We examine the difference between these two periods in 2020 and 2019.[3] We use 2019 as the comparison year, as it is closest to our year of interest and therefore most comparable in terms of road infrastructure and policies. To check for pre-trends, we also examine the difference between these periods from 2014 to 2018 against 2019. Our main specification is:

$$C_{yt} = \alpha + \sum_{y \neq 2019} \beta_y \cdot \mathbf{I}\{\text{Year} = y\} \times \text{Post}_{yt} + \sum_{y \neq 2019} \gamma_y \cdot \mathbf{I}\{\text{Year} = y\} + \theta \text{Post}_{yt} + X'_{yt}\boldsymbol{\delta} + \lambda_{\text{dow}(t)} + \lambda_{\text{week}(t)} + \varepsilon_{yt} \quad (1)$$

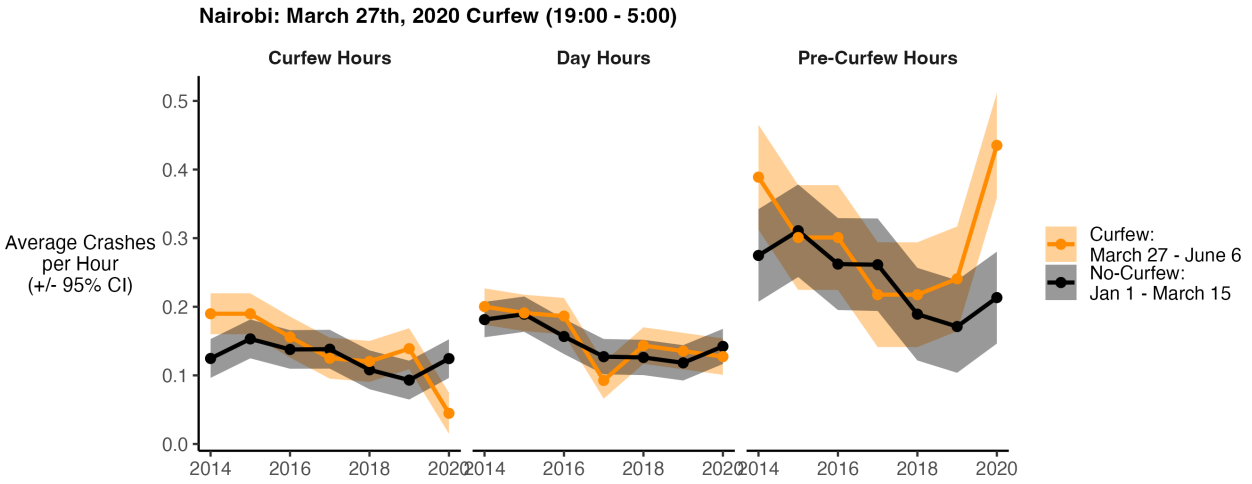
where C_{yt} is the outcome of interest on day t of year y. $\sum_{y \neq 2019} \mathbf{I}\{\text{Year} = y\}$ are indicator variables for each year from 2014 to 2020 excluding 2019 as the reference year. Post_{yt} is an indicator for all days from March 27 until June

[3] We do not include the days from March 16-March 26 in the analysis because some policies had been implemented to limit mobility but the curfew was not yet implemented.

6. β_y captures the difference in the outcome for year y relative to the baseline year 2019 in the post-curfew period. X_{yt} is a vector of weather controls (precipitation, temperature, and wind speed). $\lambda_{\text{dow}(t)}$ and $\lambda_{\text{week}(t)}$ are day-of-week and week-of-year fixed effects, respectively. ε_{yt} is the error term. We estimate this equation for three time intervals focused on crashes during curfew hours (20:00-5:00) as well as during the pre-curfew hours (17:00-20:00) to test for spillovers, and daytime hours (5:00-17:00) as a comparison. The analysis is conducted at the daily level. Our main coefficient of interest is β_{2020}, which captures the impact of COVID-19 policies on our outcomes.

The main assumption required in order for us to interpret β_{2020} as a causal impact relies on parallel trends between the two time periods that we are calling treatment and control. We look at the raw data, comparing the treatment period when the curfew was implemented (March 27-June 6) to the control period when there were no COVID-19 policies in place (January 1 - March 15) across years from 2014 until 2020 (Figure 1). We analyze the three time intervals separately. Overall the two time periods move in parallel, with a steady decrease in crashes across the years from 2014 to 2019. We capture this explicitly in our specification by including the pre-trends, which are captured by β_{2014} - β_{2018}. We also apply the robust inference method of Rambachan and Roth (2023) which allows for bounded deviations from pre-treatment trends to conduct valid inference under plausible violations of the parallel-trends assumption. We test the degree to which the slope of the pre-trend could change to still maintain valid inference.

Figure 1: Trends in crashes



In the absence of the curfew, mobility and the number of cars on the road would still be lower due to the other mobility policies, including the closure of restaurants, bars and schools. In order to try to difference out the effect of lower mobility due to other policies, we estimate a triple-difference specification as in Equation 2, integrating all hour intervals into the same regression. We interact the indicators for the curfew year $\mathbf{I}\{2020 = y\}$ and curfew period $Post_{yt}$ with the curfew hour intervals c, where the two periods of interest are the curfew hours from 20:00-5:00 and the pre-curfew hours from 17:00-20:00, with the comparison being the daytime hours from 5:00-17:00. Therefore, $\delta_{2020,c=curfew}$ and $\delta_{2020,c=precurfew}$ are our coefficients of interest. As with the double-difference, we estimate the

equation using data from 2014 to 2020 and also estimate triple difference parameters for years 2014 through 2018 to help test for pre-trends and conduct sensitivity analysis following Rambachan and Roth (2023). The analysis is conducted at the daily level. The daytime interval may have been affected by the curfew as well if individuals shift trips they would have performed in the evening to the daytime, but this would bias us towards finding smaller spillover effects during the pre-curfew hours.

$$C_{ytc} = \alpha + \sum_{y \neq 2019} \beta_y \, \mathbf{I}\{\text{Year} = y\} + \theta \, \text{Post}_{yt} + \sum_{c \neq \text{daytime}} \phi_c \, \mathbf{I}\{\text{CurfewHours} = c\} + \sum_{y \neq 2019} \sum_{c \neq \text{daytime}} \delta_{ytc} \, \mathbf{I}\{\text{Year} = y\} \times \text{Post}_{yt} \times \mathbf{I}\{\text{CurfewHours} = c\} + X'_{ytc}\boldsymbol{\gamma} + \lambda_{\text{dow}(t)} + \lambda_{\text{week}(t)} + \varepsilon_{ytc} \quad (2)$$

We estimate two outcomes. The first is the number of crashes. The second outcome is the crash rate, which is a measure of the probability of being in a crash. Changes in the rate of crashes are an important indication of the additional externality of vehicle density and driver behaviors as an additional car on the road or more aggressive behavior could lead to an increase in the likelihood of a crash that goes beyond the proportional increase (Edlin and Karaca-Mandic, 2006; Green et al., 2016). We use the index for vehicles on the road during each time interval as a proxy for changes in total vehicles on the road in Nairobi. We divide crashes by the index to measure the proportional change in crashes. We use bootstrap standard errors given the estimated nature of the outcome variable.

5 Results

5.1 Crashes During Curfew Hours and Spillovers

We first examine the impact of the curfew on road traffic crashes during curfew hours, from 20:00 until 5:00. Crashes decrease sharply by 1.14 per day during curfew hours (Table 1, Column 1). This result suggests that the contraction in vehicle travel during curfew hours more than offsets any increase in risk per vehicle that may arise from higher speeds among vehicles remaining on the road.

A central question is whether the curfew generates spillovers to other hours of the day, particularly if trips that would typically occur during curfew hours are shifted earlier. To examine these effects, we focus on the three hours preceding the curfew, from 17:00 to 20:00. Crashes increase by 0.45 per day during this pre-curfew window (Table 1, Column 2).[4] This result indicates negative spillover effects of the curfew in the form of more crashes occurring right before the curfew takes effect. During daytime hours, crashes decline modestly by -0.38 per day, but the effect is not statistically significant (Table 1, Column 3).

[4]Excluding crashes occurring between 19:00 and 20:00 yields similar results, with a significant increase in crashes from 17:00 to 19:00; see subsection 5.4.

Table 1: Impacts of Mobility Reducing Policies on Daily Severe/Fatal Crashes in Nairobi, Kenya

	N Crashes				Crash Rate		
	Curfew Hours (1)	Pre-Curfew Hours (2)	Day Hours (3)	All Hours (4)	Curfew Hours (5)	Pre-Curfew Hours (6)	Day Hours (7)
2020 X Curfew Period	−1.139***	0.447**	−0.375		1.638**	0.805***	0.226
	(0.232)	(0.216)	(0.322)		(0.758)	(0.231)	(0.430)
2020 X Curfew Period X Curfew Hours				−0.746*			
				(0.395)			
2020 X Curfew Period X Pre-Curfew Hours				0.836**			
				(0.387)			
Num.Obs.	1017	1017	1017	3051	291	291	291
R2	0.167	0.086	0.102	0.180	0.167	0.236	0.169
R2 Adj.	0.128	0.044	0.059	0.160	0.060	0.141	0.063
Analysis Years	2014-2020	2014-2020	2014-2020	2014-2020	2019-2020	2019-2020	2019-2020
Dependent Variable 2019 Average	1.041	0.621	1.531	1.064	1.944	0.702	1.943

Notes. Robust standard errors (in parentheses) are reported for crash-count regressions; bootstrap standard errors (200 replications) are reported for crash-rate regressions. *** (**) (*) denote significance at the 1% (5%) (10%) level. All regressions include year, week, and day-of-week fixed effects and weather controls. "2020 × Curfew Period" indicates days between March 27 and June 6, 2020, relative to the same calendar period in 2019. Column (4) reports triple-difference estimates using daytime hours as the comparison group. Only 2020 coefficients are shown; estimates for 2014–2018 appear in Appendix Figure A.1. Crash data are from the National Police Service. Data cover 2014-2020 for 'N Crashes' and 2019-2020 for 'Crash Rate'.

Because these estimates reflect the combined effects of the curfew and other contemporaneous mobility restricting policies (e.g., school and bar closures), we next isolate the impact of the curfew by comparing changes during curfew and pre-curfew hours to changes during daytime hours (5:00-17:00), when the curfew does not bind. Using a triple-difference specification following equation 2 that compares crashes in the pre-curfew hours and during the curfew hours to crashes during the daytime—across pre- and post-curfew periods and relative to 2019—, we find that the decline in crashes during curfew hours remains negative and significant, but it is now smaller in magnitude at -0.75 per day (Table 1, Column 4). In contrast, the increase in crashes during pre-curfew hours becomes larger, at 0.84 per day. Thus, once common mobility shocks are netted out, spillovers during the pre-curfew hours fully offset the safety gains realized during curfew hours.

Identification relies on the assumption of parallel trends between the January 1–March 15 and March 27–June 6 periods. Visual inspection of the raw data (Figure 1) and event-study estimates for pre-2020 years (Appendix Figure A.1) provide support for this assumption. Recognizing the limitations of pre-trend testing (Roth, 2022; Rambachan and Roth, 2023), we conduct sensitivity analysis following Rambachan and Roth (2023). For the double-difference specification, the breakdown value is $M > 0.05$ for curfew-hour effects and $M = 0.03$ for pre-curfew effects. For the triple difference, the pre-curfew coefficient remains robust with a breakdown value of $M = 0.04$, implying that post-treatment deviations from linear pre-trends would need to exceed $\pm 55\%$ to overturn the spillover estimate (Appendix Figure A.2 and Table A.3).

Finally, to distinguish between changes in crashes driven purely by traffic volumes and behavioral responses reflected in crash risk conditional on exposure, we examine crash rates. Despite the decline in total crashes during curfew hours, the probability of a crash increases significantly (Table 1, Column 5). This indicates that, among the vehicles remaining on the road, the high cost of traveling during curfew hours is associated with riskier driving behavior-most plausibly higher speeds-which increases the likelihood of a crash. Crash rates also increase sharply during pre-curfew

hours (Table 1, Column 6), while remaining unchanged during daytime hours. These patterns indicate that spillovers do not operate through a mechanical increase in crashes due to an increase in vehicles but through either a behavioral response (Bento et al., 2020) or the crash externalities imposed by an additional vehicle on the road (Green et al., 2016; Edlin and Karaca-Mandic, 2006).

5.2 Spillover Mechanisms: Congestion and Speed

The conceptual framing outlined earlier suggests two mechanisms through which curfews may generate spillovers into pre-curfew hours. First, trips that would otherwise occur during curfew hours (which are now high cost) may be displaced to earlier periods (which are lower cost), increasing vehicle density, and in turn crash incidence. Second, the discrete penalty associated with violating the curfew sharply increases the cost of being late, especially as the curfew start time approaches, which may induce urgency and higher driving speeds.

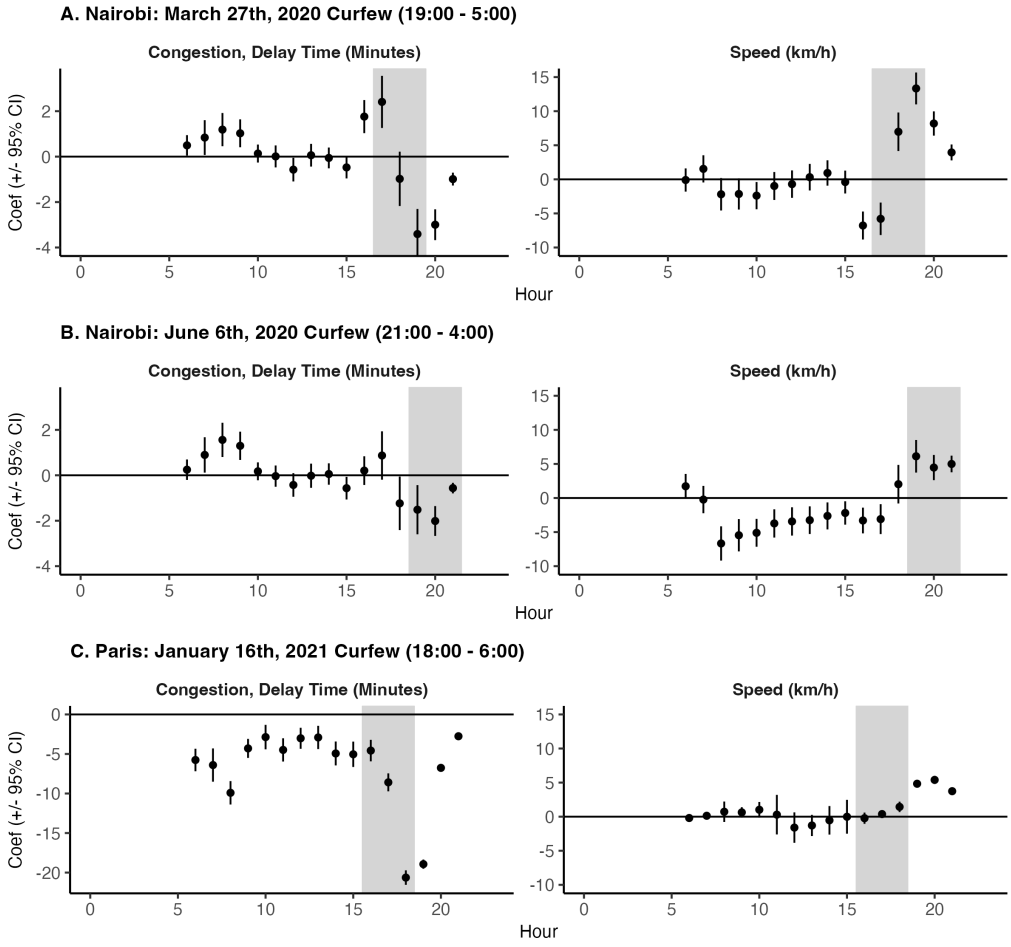
To examine these mechanisms, we use high-frequency data on congestion and travel time from Waze and Google Maps as proxies for traffic density and speed. Unlike road traffic crashes, which are rare events, these outcomes can be measured at high frequency, allowing us to isolate the incremental effect of the curfew itself by comparing the days immediately following curfew implementation to the days following earlier COVID-related policies but prior to the curfew. Figure 2 presents hourly difference-in-differences estimates. After the curfew is implemented, congestion shows little change from 6:00 to 15:00 compared to the period when other COVID-19 mobility policies were in place. Between 16:00 and 17:00, congestion increases significantly, followed by a decline at 18:00 and a much larger drop at 19:00 as drivers begin to clear the roads in anticipation of the curfew. In contrast, during the earlier COVID-policy period without a curfew, congestion changes are more diffuse across the day and do not display the pronounced concentration immediately before curfew onset observed once the curfew is implemented (Appendix Figure A.5). This pattern indicates that the curfew induces a reallocation of travel across hours rather than a uniform reduction in traffic. The congestion profile during pre-curfew hours is consistent with a combination of displacement and compliance: density rises earlier in the pre-curfew window as trips are shifted forward, then falls sharply as the curfew approaches. Given that the average one-way commute in Nairobi is close to one hour, many commuters must begin travel well before curfew to reach home on time, contributing to congestion earlier in the pre-curfew period.

Turning to speed, we find little change during most daytime hours. At 16:00 and 17:00, speed declines significantly when compared to the period after mobility policies were implemented but before the curfew, consistent with the increase in congestion. At 18:00 and 19:00, however, speed rises sharply. While part of this increase reflects reduced congestion, the magnitude of the speed response exceeds what would be predicted by congestion alone.[5] This asymmetry suggests an additional behavioral response: drivers increase speed as the curfew start time approaches, consistent with urgency induced by a discrete penalty for late arrival (Bento et al., 2020). Explicitly looking at driving

[5]Comparing speed to congestion, there is a particular deviation from the typical trend at 18:00, when speeds are much higher in relation to the congestion level during the curfew period (see Figure A.7).

over the speed limit of 50km/h, we see a significant 12% increase in the probability during the pre-curfew hours, and no increase during the daytime hours, despite congestion being lower on average during the daytime hours (Table A.2). There is a significant increase in the probability of driving over 40km/h both during the daytime hours and the pre-curfew hours, showing that during the daytime, people are increasing their speeds but it's only during the pre-curfew hours that they are speeding excessively.

Figure 2: Hourly Difference-in-Difference Impact Estimates on Congestion and Speed



Notes. Points show hourly difference-in-differences coefficients with 95% confidence intervals, estimated separately for each hour of the day. Coefficients measure changes during curfew periods relative to pre-curfew periods. Panel A compares March 27–June 5 (initial curfew, Nairobi) with March 20–27, using 2020 relative to 2019. Panel B compares June 6–July 31 (revised curfew, Nairobi) with March 20–27, using 2020 relative to 2019. Panel C compares January 16–March 31 (curfew, Paris) with June 6–December 11, using 2021 relative to 2023 for congestion and 2019 for speed (congestion data for Paris is only available starting in 2020). Congestion is measured as total traffic delay (minutes) from Waze at the hour–street level. For Nairobi, speed is the average travel speed across thirteen Google Maps routes; for Paris, speed is derived from vehicle count indicators from the Direction de la Voirie et des Déplacements (Service des Déplacements, Poste Central d'Exploitation Lutèce). Congestion regressions use hourly data from 1,011 streets in Nairobi and 3,413 streets in Paris; speed regressions use hourly aggregated data.

5.3 Trip Inflexibility and Evidence on the Mechanism

The joint increase in congestion followed by a sharp rise in speed before the start of curfew suggests that many displaced trips are inflexible and concentrated among evening commuters. We hypothesize that spillovers arise primarily when curfews bind against inflexible work-related trips.To assess this mechanism, we examine two cases in which the

prevalence of such trips is reduced.

We first analyze the revision of the Nairobi curfew on June 6, 2020, when the start time was shifted from 19:00 to 21:00.[6] When the curfew begins later, crashes still decline during curfew hours (-0.87 per day), but there is no longer an increase in crashes during the pre-curfew hours (Table 2, Panel A). Crash rates also show no significant change in either curfew or pre-curfew hours. These results indicate that when the curfew no longer coincides with peak commuting times, spillovers disappear.

Next we examine a different context: Paris, France, which implemented an 18:00 to 6:00 curfew starting on January 16, 2021. An important difference is that France imposed a mandatory work-from-home requirement, substantially reducing the share of inflexible evening work trips.[7] This reduction is evident in the Google Mobility data, which show a much larger and more persistent decline in workplace activity in Paris than in Nairobi (Appendix Figure A.3).

The Paris analysis is not intended as a causal cross-city comparison or as validation of effect magnitudes. Paris differs from Nairobi along many dimensions relevant for road safety, including baseline congestion, enforcement capacity, infrastructure, and concurrent pandemic policies. Instead, we use Paris as a context in which the conditions for the mechanism are substantially weaker–specifically, where inflexible evening travel is greatly reduced.

Using a parallel difference-in-differences (and triple difference) design and focusing on fatal and severe crashes for comparability, we find a large decline in crashes during curfew hours (-1.06 per day) but no increase in crashes in the pre-curfew hours (Table 2, Panel B). Consistent with these findings, congestion does not increase prior to the curfew, and speed increases are modest and confined to curfew hours (Figure 2, Panel C). Crash rates also do not change for any analysis periods (Table 2, Panel B, Columns 5-7).[8]

Looking at mechanisms in Paris, we find no evidence of pre-curfew trip compression. Panel C of Figure 2 shows no increase in congestion in the hours preceding the 18:00 curfew; instead, congestion declines sharply once the curfew begins. Speed responses are likewise muted before the curfew and increase only modestly during curfew hours—far smaller than the pre-curfew speed spikes observed in Nairobi.

Taken together, these results are consistent with a common mechanism: curfews generate spillovers primarily when they bind against inflexible peak-hour trips and impose a sharp deadline that induces urgency. When commuting demand is either shifted outside the curfew window, as in the later Nairobi curfew, or substantially reduced through widespread work-from-home, as in Paris, curfews no longer generate pre-curfew congestion, speed responses, or crash spillovers. In this sense, the Paris evidence is consistent with the mechanism's prediction that when inflexible evening trips are largely absent, the spillover channel should be limited or absent.

[6] We focus on the first two months after the revision to match the duration of the initial curfew period. To account for delays in crash reporting, we treat 22:00 as the effective start time.

[7] Full-time remote work increased by more than 50% between early 2020 and early 2021(Cahuc, 2022). In contrast, in low- and lower-middle-income countries such as Kenya, the increase was minimal.

[8] For Paris, we compare the curfew period (January 16–March 31, 2021) to a period later in 2021 with no mobility restriction policies in place (June 20–December 11, 2021), and additionally to the same calendar periods in 2019, prior to the COVID-19 pandemic. Full lockdowns were in effect both before January 16, 2021 and after March 31, 2021; accordingly, we restrict attention to the interval in which the curfew was the primary mobility-restricting policy.

Table 2: Impact of Mobility Reducing Policies on Severe/Fatal Crashes in Contexts of More Flexible Trips

	N Crashes				Crash Rate		
	Curfew Hours (1)	Pre-Curfew Hours (2)	Day Hours (3)	All Hours (4)	Curfew Hours (5)	Pre-Curfew Hours (6)	Day Hours (7)
Panel A: Naribi, June 6th Curfew							
2020 X Curfew Period	−0.368**	−0.109	0.513		−0.166	0.022	1.529***
	(0.181)	(0.261)	(0.415)		(0.237)	(0.278)	(0.273)
2020 X Curfew Period X Curfew Hours				−0.866*			
				(0.446)			
2020 X Curfew Period X Pre-Curfew Hours				−0.589			
				(0.481)			
Num.Obs.	912	912	912	2736	261	261	261
R2	0.133	0.079	0.110	0.340	0.179	0.171	0.222
R2 Adj.	0.090	0.033	0.066	0.322	0.068	0.058	0.117
Analysis Years	2014-2020	2014-2020	2014-2020	2014-2020	2019-2020	2019-2020	2019-2020
Dependent Variable 2019 Average	0.638	0.569	2.054	1.087	0.819	0.609	2.38
Panel B: Paris, January 16th Curfew							
2021 X Curfew Period	−1.058***	0.165	0.015		0.001	0.002	0.000
	(0.254)	(0.207)	(0.317)		(0.003)	(0.005)	(0.002)
2021 X Curfew Period X Curfew Hours				−1.058**			
				(0.415)			
2021 X Curfew Period X Pre-Curfew Hours				0.160			
				(0.384)			
Num.Obs.	1751	1751	1751	5253	1738	1745	1746
R2	0.545	0.442	0.595	0.546	0.518	0.370	0.579
R2 Adj.	0.529	0.422	0.581	0.538	0.501	0.347	0.564
Analysis Years	2014-2019; 2021	2014-2019; 2021	2014-2019; 2021	2014-2019; 2021	2019 & 2021	2019 & 2021	2019 & 2021
Dependent Variable 2019 Average	2.28	1.348	2.748	2.125	0.022	0.026	0.018

Notes. Robust standard errors (in parentheses) are reported for crash-count regressions; bootstrap standard errors (200 replications) are reported for crash-rate regressions. *** (**) (*) denote significance at the 1% (5%) (10%) level. All regressions include year, week, and day-of-week fixed effects and weather controls. "2020 × Curfew Period" ("2021 × Curfew Period") indicates days when the curfew was in effect in Nairobi (Paris), relative to the same calendar period in 2019. Columns (4)–(7) report triple-difference estimates using daytime hours as the comparison group. Crashes include all officially recorded severe and fatal crashes. For Nairobi, data cover 2014-2020 for 'N Crashes' and 2019-2020 for 'Crash Rate'; for Paris, data cover 2014–2019 and 2021.

5.4 Robustness Checks

We conduct a series of robustness checks summarized in (Table 3).

First, the estimated effects could be biased if the curfew altered crash reporting—for example, through increased police presence or changes in the likelihood that crashes are reported. To assess this possibility we replicate the analysis using crash data from Flare, an independent source that does not rely on police reports. Panel A shows results that are qualitatively similar to those obtained using police data: crashes decline during curfew hours and increase sharply during pre-curfew hours. While the decline during curfew hours is smaller and no longer statistically significant in the triple-difference specification, the increase in pre-curfew crashes remains large and precisely estimated.

Second, to ensure that results are not driven by extreme outliers, we exclude the top 1% of daily time-interval observations with the highest number of crashes (five or more crashes within an interval). Panel B shows that the estimated effects are stable and remain statistically significant, indicating that the main findings are not driven by outliers.

Third, although the curfew formally begins at 19:00, our baseline definition includes the 19:00–20:00 interval as part of the pre-curfew period to account for reporting delays and because behavior early in that hour likely reflects pre-curfew urgency. Panel C shows that excluding this hour and restricting attention to 17:00–19:00 yields similar results, albeit with smaller magnitudes, consistent with the shorter time window. This confirms that the spillover effects are not driven by crashes occurring immediately after the curfew begins.

Finally, while the main analysis aggregates crashes at the daily level, we also estimate specifications at the hourly level and at the weekly level. Panels D and E show that the qualitative patterns remain unchanged across levels of aggregation, providing further support for the robustness of the results.

6 Conclusion

Curfews are widely used by governments to promote public safety, yet systematic evidence on their consequences is limited. By sharply increasing the cost of being on the road at specific times, curfews induce behavioral responses that may generate externalities beyond their intended objectives. Understanding these effects is particularly important given that curfews directly restrict mobility and are frequently deployed in urban settings.

This paper studies one dimension of public safety—road traffic crashes—in an urban LMIC context with exceptionally high baseline road mortality. Using high-frequency data from multiple sources, we find that while a dusk-to-dawn curfew substantially reduces crashes during curfew hours, these gains are offset by increases in crashes in the hours immediately preceding the curfew. Despite lower overall traffic volumes, crash risk conditional on being on the road rises, indicating that behavioral responses play a central role.

High-frequency congestion and speed data point to the mechanisms behind these spillovers. Trips that would have occurred during curfew hours are displaced into the pre-curfew window, increasing density earlier in the evening, while the discrete penalty imposed by the curfew generates urgency and sharp increases in speed as the curfew boundary approaches. Importantly, these spillovers are not inevitable. When the curfew is shifted to later hours—after most commuting demand has dissipated—or in a context where work-from-home substantially reduces inflexible evening trips, we find no evidence of pre-curfew increases in congestion, speed, or crashes, while reductions during curfew hours persist.

Taken together, our results show that curfews can backfire for road safety when they bind against inflexible peak-hour travel. The timing of curfews relative to the structure of urban travel demand is therefore critical. Aligning curfew start times with periods of lower and more flexible travel can preserve intended benefits while minimizing unintended harm to public safety.

Table 3: Impact of Mobility Reducing Policies on All Crashes in Nairobi, Kenya: Robustness Tests

	Curfew Hours	Pre-Curfew Hours	Day Hours	All Hours
Panel A: *Crashes Measured Using Data from Flare*				
2020 X Curfew Period	−0.721***	0.470**	−0.521*	
	(0.145)	(0.210)	(0.296)	
2020 X Curfew Period X Curfew Hours				−0.208
				(0.332)
2020 X Curfew Period X Pre-Curfew Hours				0.999***
				(0.367)
Num.Obs.	1017	1017	1017	3051
R2	0.327	0.316	0.381	0.352
R2 Adj.	0.295	0.284	0.352	0.336
Panel B: *Outlier Observations (5 or more crashes) Removed*				
2020 X Curfew Period	−1.139***	0.447**	−0.395	
	(0.211)	(0.216)	(0.299)	
2020 X Curfew Period X Curfew Hours				−0.725**
				(0.364)
2020 X Curfew Period X Pre-Curfew Hours				0.863**
				(0.368)
Num.Obs.	1010	1017	996	3023
R2	0.164	0.086	0.102	0.171
R2 Adj.	0.124	0.044	0.058	0.151
Panel C: *Crashes Occurring from 19:00 - 20:00 Removed*				
2020 X Curfew Period	−1.139***	0.283*	−0.375	
	(0.232)	(0.170)	(0.322)	
2020 X Curfew Period X Curfew Hours				−0.744*
				(0.394)
2020 X Curfew Period X Pre-Curfew Hours				0.690*
				(0.365)
Num.Obs.	1017	1017	1017	3051
R2	0.167	0.064	0.102	0.245
R2 Adj.	0.128	0.020	0.059	0.226
Panel D: *Analysis at Hourly Level*				
2020 X Curfew Period	−0.126***	0.149**	−0.033	
	(0.025)	(0.071)	(0.025)	
2020 X Curfew Period X Curfew Hours				−0.093***
				(0.036)
2020 X Curfew Period X Pre-Curfew Hours				0.179**
				(0.076)
Num.Obs.	9153	3051	12 204	24 408
R2	0.058	0.038	0.014	0.025
R2 Adj.	0.053	0.023	0.009	0.022
Panel E: *Analysis at Weekly Level*				
2020 X Curfew Period	−4.880***	4.566***	−1.479	
	(1.039)	(1.059)	(2.813)	
2020 X Curfew Period X Curfew Hours				−5.105
				(3.569)
2020 X Curfew Period X Pre-Curfew Hours				4.935
				(3.501)
Num.Obs.	157	157	157	471
R2	0.300	0.203	0.248	0.489
R2 Adj.	0.220	0.119	0.162	0.435

Notes. Robust standard errors are reported in parentheses. *** (**) (*) denotes significance at 1% (5%) (10%) level. Regressions include fixed effects for year, week, and day of the week, and controls for weather (temperature, precipitation and wind). Only regressions coefficients for 2020 are shown; coefficients for 2014-2018 are excluded. Crashes include all crashes recorded by official sources.

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Appendix A Additional Figures and Tables

Figure A.1: Pre-Trend Coefficients Looking at the Impact of Mobility Reducing Policies on Crashes in Nairobi, Kenya (March 27 Curfew)

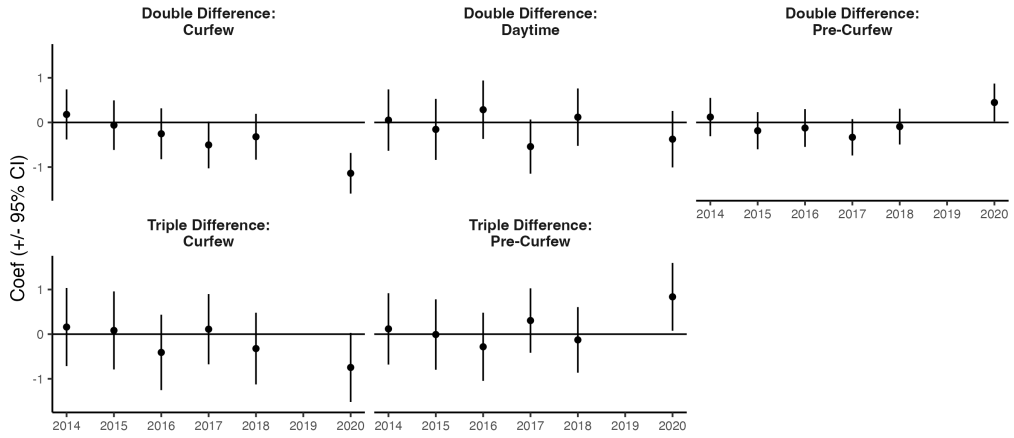


Figure A.2: Sensitivity analysis on $2020 \times CurfewPeriod$ coefficient for examining the impact of mobility reducing policies on all crashes in Nairobi, Kenya (March 27 curfew). Sensitivity analysis based on smoothness restriction of pre-trend deviations; M reflects allowable slope change between periods.

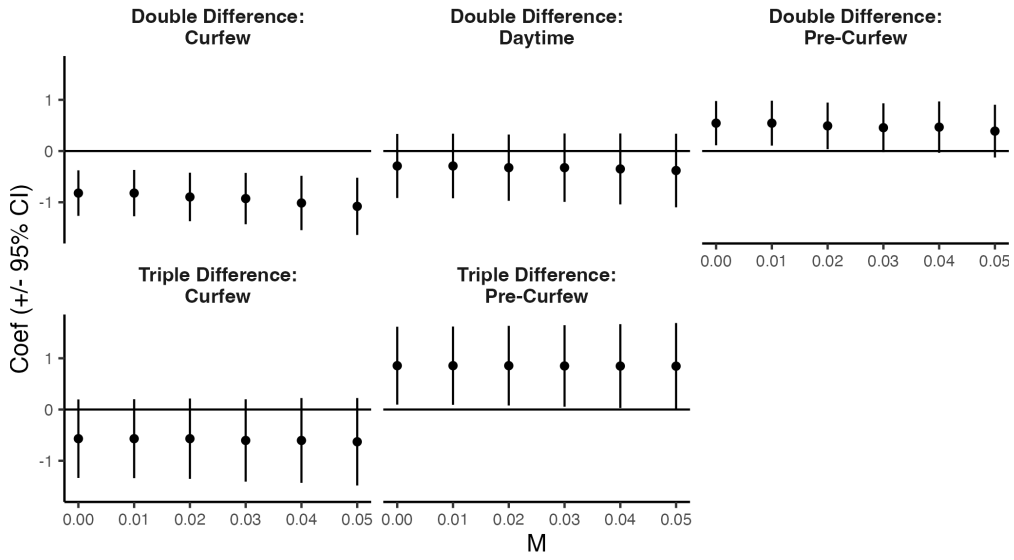


Figure A.3: Percent change in workplace activity from pre-COVID period. Data from Google COVID-19 Community Mobility Reports

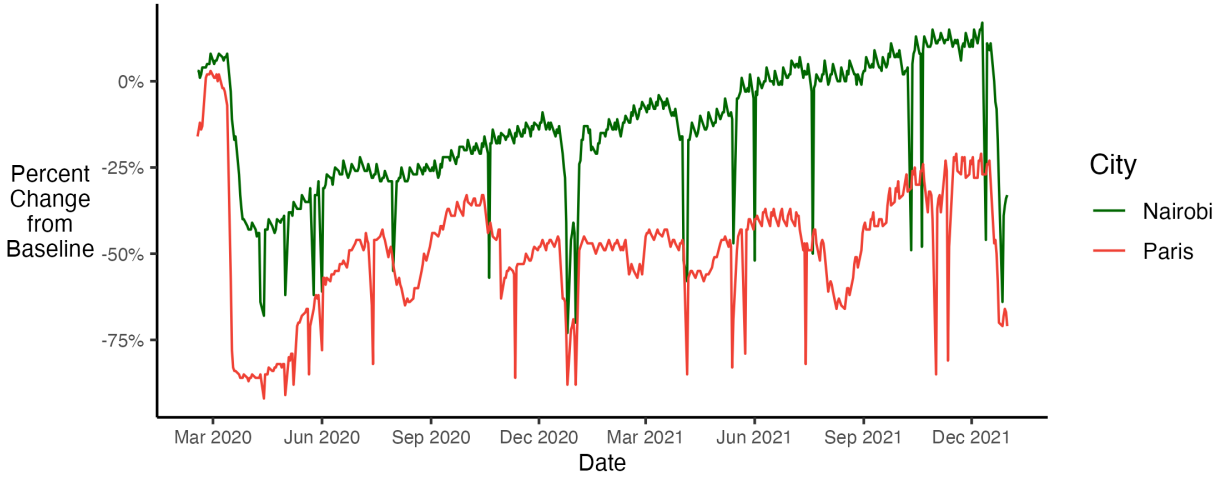


Figure A.4: Hourly speed across different time periods

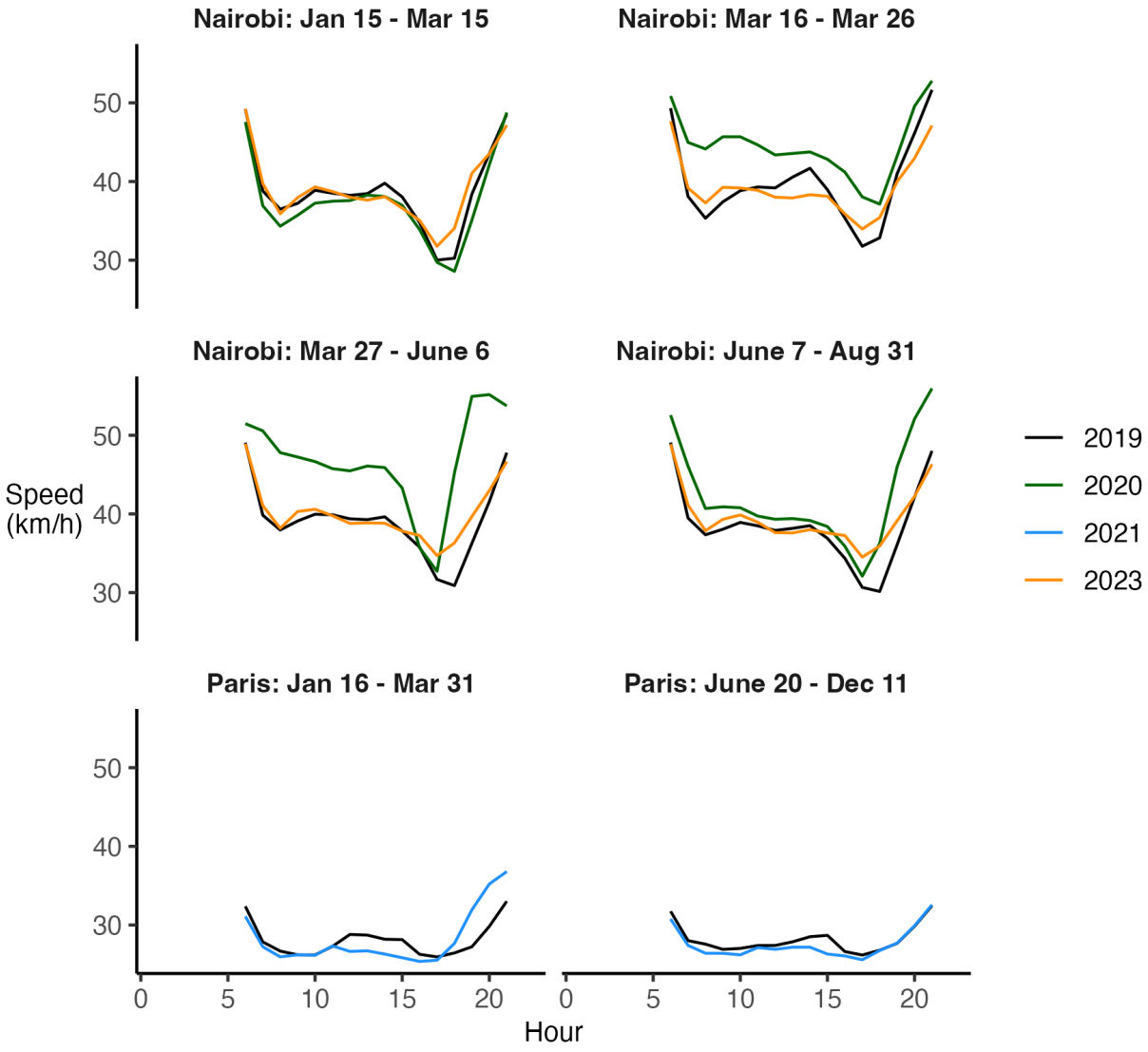


Figure A.5: Hourly congestion across different time periods. The total delay time is computed by taking the total delay time across all segments within an hour; the value report is the average value across hours in the time period

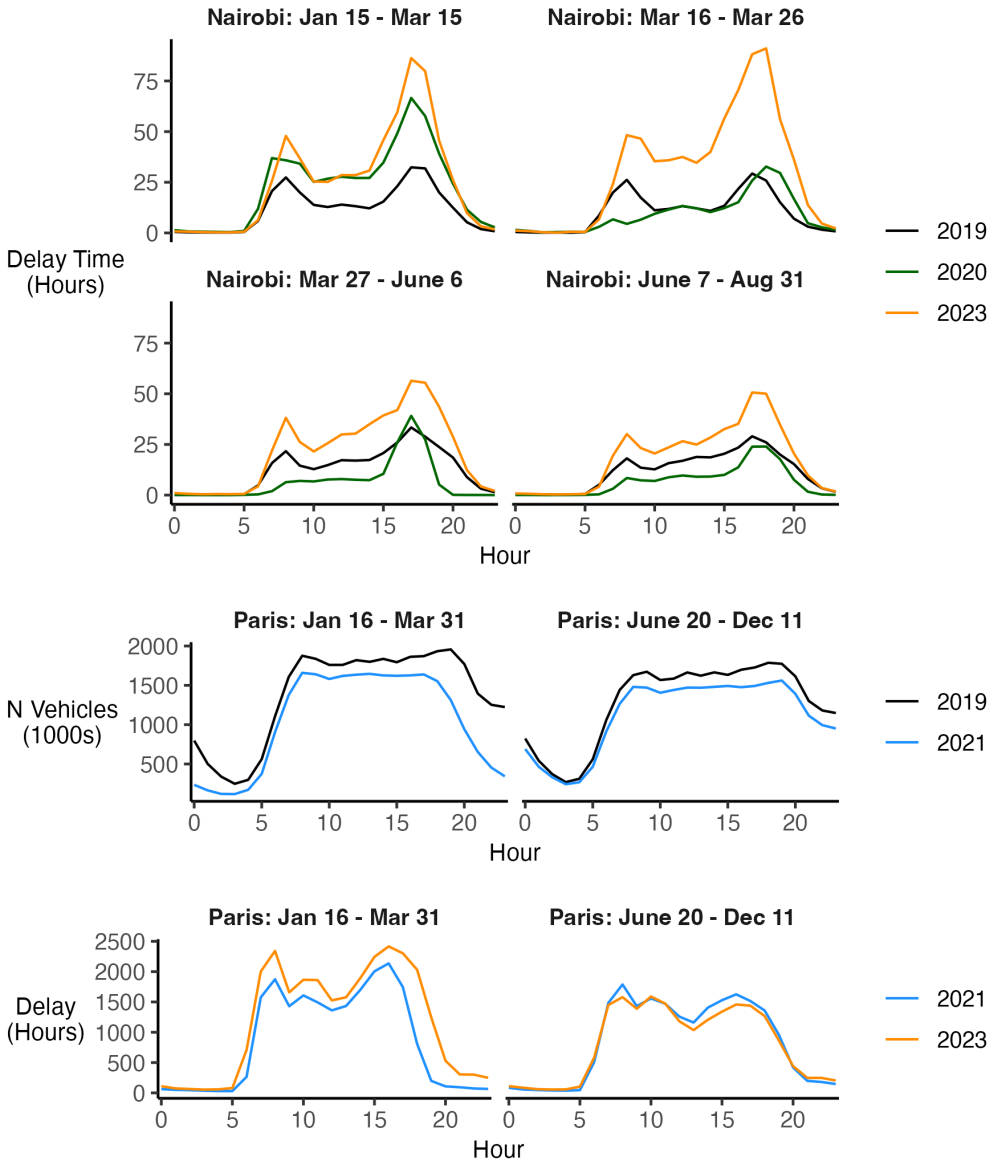


Figure A.6: Percent Change in Speed based on Google Data and Uber Data

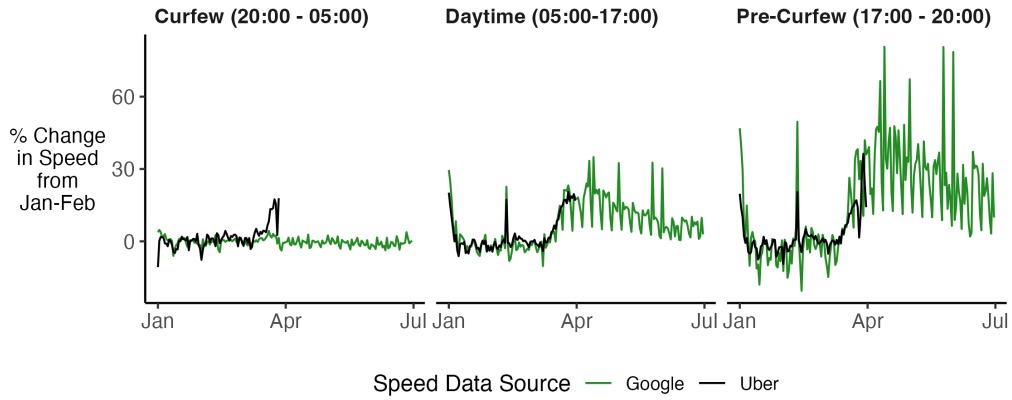


Figure A.7: Speed versus Congestion Hourly on Weekdays, After the Curfew Policy in 2020 Compared to the Same Time Period in 2019

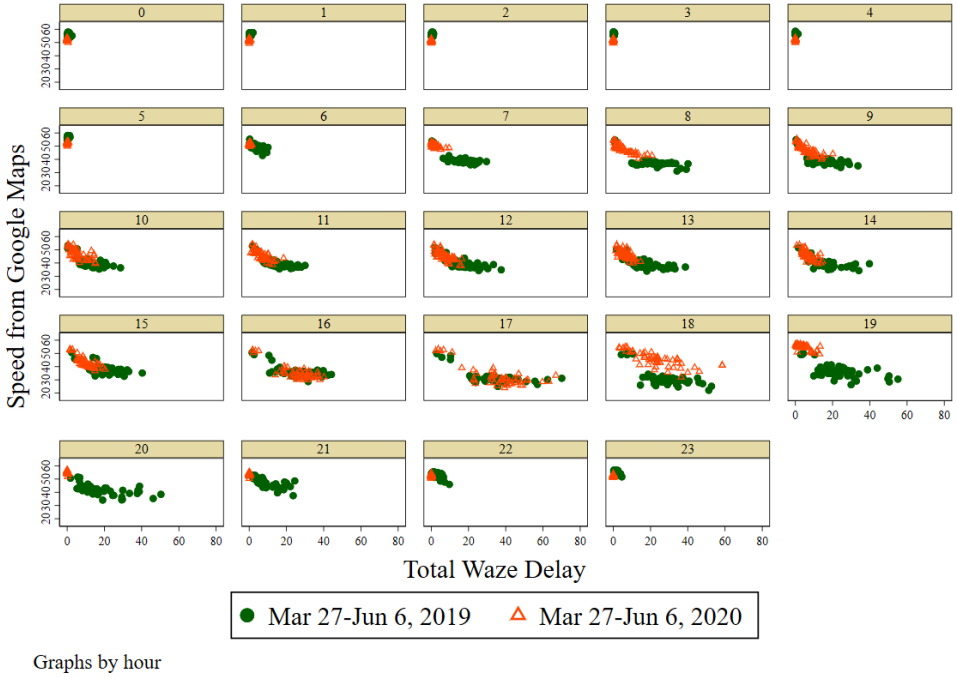


Table A.1: Data and Variable Construction

Indicator	Description	Source
Road traffic crashes (Nairobi)	Crashes with an injury/ fatality/ major property damage in police situation reports.	Kenya National Police Service (NPS)
	Crashes reported to the largest emergency response platform in Kenya (24/7) – 200 ambulances in Nairobi.	Flare
Road traffic crashes (Paris)	Crashes involving bodily injury recorded by law enforcement unit that responded to the crash	The French Road Safety Observatory (ONISR)
Vehicle speed	Average speed (km/hr) = distance road segment / estimated travel time from Google maps for select road segments.	Google maps
	Average speed (km/hr) aggregated from Uber trips at the road segment level across Nairobi per hour, available prior to March 2020.	Uber
Congestion proxy	Traffic delay (in hours) due to traffic jams in Nairobi for select roads.	Waze
Vehicle count (Nairobi)	Count of vehicles on the road from sensor strips installed in six major intersections in Nairobi available for Jan-Jul 2020.	Kenya Urban Roads Authority (KURA)
Vehicle count (Paris)	Count of vehicles on the road from sensor strips installed across Paris.	Ville de Paris
Crash rate proxy	Proxy of crash rate (crashes per car density) estimated as total road traffic crashes divided by an index of vehicle counts in 6 principal intersections in Nairobi.	Authors' estimates based on KURA, NPS and Google data
Predicted number of vehicles	Estimated from a regression of vehicle counts at 6 principal intersections on congestion (Waze), weather and time indicators.	Authors' estimates based on KURA, NPS and Google data
Index of number of vehicles	Index of number of vehicles based on data from 6 principal intersections. Number of vehicles/number of vehicles before curfew (Jan 1, 2020 – March 15, 2020) for each day of week and hour interval (daytime, pre-curfew hours, and curfew hours).	Authors' estimates based on KURA, NPS and Google data
Weather indicators	Temperature (measured in Celsius), Precipitation (mm), and Wind Speed (kph).	Visual Crossing Weather API

Table A.2: Impacts of Mobility Reducing Policies on Average Traffic Speeds Over 40 and 50 km/h Across Road Segments

	Speed Over 50 km/h			Speed Over 40 km/h		
	Day Hours (1)	Pre-Curfew Hours (2)	Day/Pre-Curfew Hours (3)	Day Hours (4)	Pre-Curfew Hours (5)	Day/Pre-Curfew Hours (6)
2020 X Curfew Period	0.011	0.120*		0.139*	0.251***	
	(0.042)	(0.067)		(0.075)	(0.079)	
2020 X Curfew Period X Pre-Curfew Hours			0.118*			0.239***
			(0.065)			(0.077)
Num.Obs.	34 801	8633	43 434	34 801	8633	43 434
R2	0.698	0.577	0.661	0.484	0.511	0.446
R2 Adj.	0.698	0.576	0.660	0.484	0.509	0.445

Notes. Robust standard errors (in parentheses) are reported for crash-count regressions; bootstrap standard errors (200 replications) are reported for crash-rate regressions. *** (**) (*) denote significance at the 1% (5%) (10%) level. Regressions estimated at the road segment level, using the 13 segments where Google Maps data was queried. All regressions include year, week, and day-of-week fixed effects and weather controls. "2020 $\times$ Curfew Period" indicates days between March 27 and June 6, 2020, relative to the same calendar period in 2019. Columns (3) and (6) report triple-difference estimates using daytime hours as the comparison group.

Table A.3: Pre-Trend Slopes

Time Period	Specification	Slope
Curfew	Double Difference	-0.1427
Pre-Curfew	Double Difference	-0.0546
Daytime	Double Difference	-0.0315
Curfew	Triple Difference	-0.0999
Pre-Curfew	Triple Difference	-0.0235

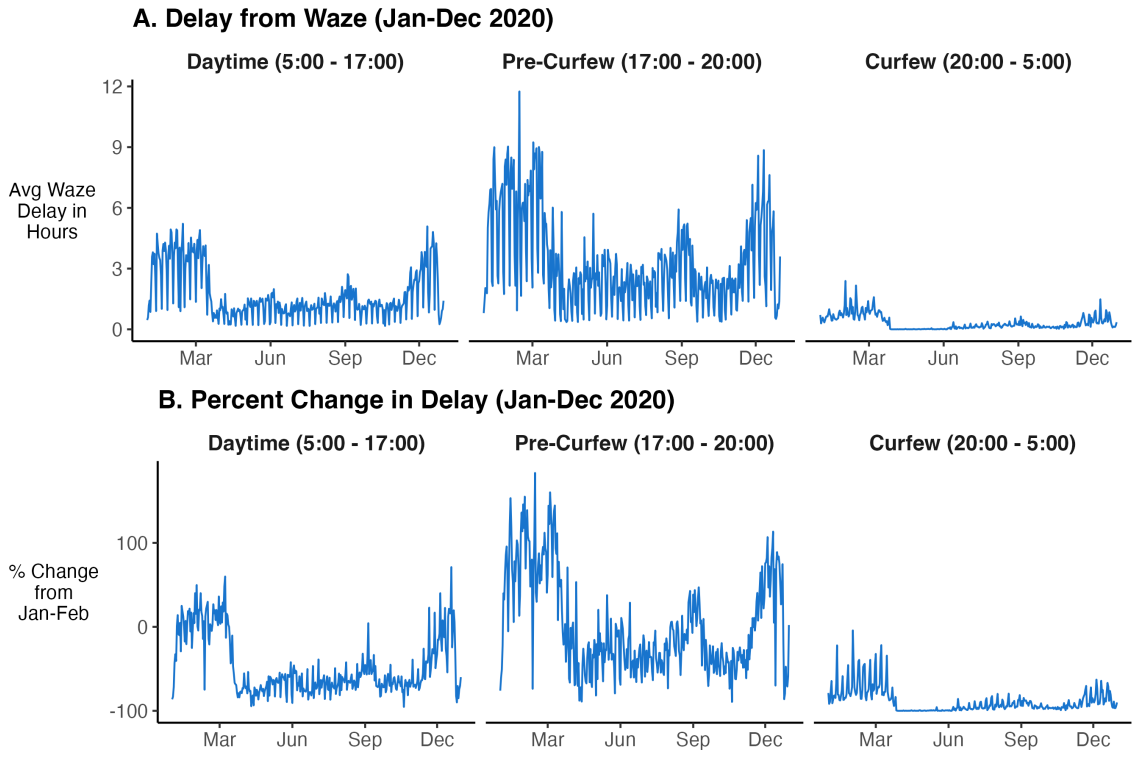
Appendix B Predicting Vehicles on the Road

We predict the vehicles on the road for six major intersections with available data.[9] The main predictor is congestion measured using Waze data (Figure B.1). This congestion is measured as the total amount of delay time due to traffic jams across the city. To capture heterogeneity in the relationship between delays measured by Waze and vehicle counts across time of day, we conduct this comparison within each time interval (daytime, pre-curfew, or curfew).[10]

$$y_t = \beta_0 + \beta_1 waze_t + \beta_2 waze^2{}_t + X_t'\boldsymbol{\delta} + \epsilon_t \tag{3}$$

Our outcome of interest, y_t, is number of vehicles crossing the intersection. We use a poisson regression given the count nature of this outcome variable. We include both linear and quadratic terms for the main predictor, congestion measured using Waze data (Figure B.1). We also control for indicators of weather (temperature, precipitation and wind), X_t'. We predict vehicle counts for each interval and intersection separately. Table Table B.1 shows regressions for a single intersection as an illustration and figure Figure B.2 presents actual and predicted values for the six intersections. The correlations between the actual vehicle counts and the predicted counts are .955, .932, .923, .919, .904, and .901, respectively, for each intersection.

Figure B.1: Trends in Waze Data



[9] Data for four of the intersections is available for January and April 2020, for one of the intersections data is available for February and April in 2019 and 2020, and for one intersection data is available for January, February, March and April 2020.

[10] Equivalently, we can interact each right-hand side term with the time interval dummy.

Table B.1: Regression Estimates of Vehicle Counts on Waze Congestion Data

	Day hours	Pre-curfew hours	Curfew hours
	(1)	(2)	(3)
Waze delay (hours)	0.005***	0.005***	0.035***
	(0.00002)	(0.0001)	(0.0001)
Waze delay (hours)^2	−0.00001***	−0.00002***	−0.0002***
	(0.00000)	(0.00000)	(0.00000)
Precipitation	0.010***		−0.004***
	(0.0003)		(0.0002)
Temperature	0.039***	0.034***	0.114***
	(0.001)	(0.001)	(0.001)
Wind speed	0.004***	0.003***	0.014***
	(0.0002)	(0.0002)	(0.0003)
Constant	8.537***	7.732***	5.539***
	(0.014)	(0.014)	(0.026)
Observations	112	110	112
Note:			*p<0.1; **p<0.05; ***p<0.01

According to the Visual Crossing Weather API, there was no recorded precipitation during the pre-curfew hours during the time period considered

Figure B.2: Total Vehicle Counts Compared to Vehicle Counts Predicted Based on Congestion Data from Waze

