

SUPPLEMENT TO “THE EFFICIENT DEPLOYMENT OF POLICE RESOURCES:
THEORY AND NEW EVIDENCE FROM A RANDOMIZED DRUNK DRIVING
CRACKDOWN IN INDIA”

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A1. FURTHER REDUCED-FORM ANALYSES: ROBUSTNESS AND ALTERNATIVE MECHANISMS

Pilot vs. main intervention

Our main dataset contains the pooled data from the 2010 pilot survey in 24 precincts, and the main 2011 intervention in 123 precincts. In Table A1 we estimate the specification from Table 4 separately in each intervention to verify that the results from both of these rounds are consistent with each other and with the pooled results. We find broad agreement between the two sets of estimates. In particular, for both interventions we find that the effect of rotating checkpoints on accidents and deaths in darkness is larger than that of fixed checkpoints, although this is only significant in the main intervention.

Intensity of intervention

Table A2 further decomposes the program effect by the intensity of checking. As the number of rotating checkpoints per week increases, the negative effect on nighttime accidents increases (column 3), further demonstrating the effectiveness of this intervention strategy. The effect of rotating monitoring on road deaths also increases from 1 to 2 checkpoints per week, though the coefficient on 3 checkpoints per week is (insignificantly) smaller than 2 per week. The coefficients values do not suggest increasing returns to intensity in this range of police enforcement although the standard errors are large at this level of treatment disaggregation. As predicted by

the model, increased intensity has no effect on night accidents or deaths in precincts with fixed checkpoints.

Between-Station Spillovers

While our focus has been on the within-station main effects and spillovers of the sobriety checkpoints, it is possible there may also have been cross-station spillovers. In principle, these might be positive or negative: drivers avoiding checkpoints near their own stations might move into different police jurisdictions, or citizens might hear that a crackdown has begun in a nearby town and infer that it has also begun in their own area. On the other hand, the isolated nature of many police stations in rural Rajasthan, the fact that they cover a very large catchment area, and the history of relatively independent enforcement would all serve to attenuate any spillovers.

We quantify the geographical extent of the spillovers in Table A5, which examines accidents and deaths, and Table A6 which examines the number of drunken drivers caught at the checkpoints. In short, neither table shows robust evidence of between station spillovers. Panel A of Table A5 displays the pooled effects of any intervention on accidents and deaths, analogous to Table 3, now including controls for the number of stations with any checkpoints in 10, 20, and 40 kilometer radii. None are significant, and size of the main effects are very similar to those in Table 3. Panel B disaggregates the main effects and spillovers by fixed and rotating police stations, analogous to Table 4. Of the 30 spillover effects estimated, only 1 is significant at the 5% level and it relates to the outcome of daytime accidents, suggesting it may be the result of chance.

Results from the checkpoint surveys in Table A6 are equally non-significant once we control for police station fixed effects. Columns 1 and 2, which do not include station effects (only district fixed effects), show that police stations with many treated close neighbors (stations in 10 km) have more drunken drivers caught, whereas stations with many treated areas far away (20 and 40 kms) have fewer. However, as in Blattman, Green, Ortega, and Tobón (2021), the distribution of treated neighbors is not random, so this likely reflects the fact that areas with dense police stations are inherently different (more densely populated, higher crime, etc.). Controlling for police station fixed effects in columns 3 and 4 removes this bias, and accordingly the coefficients on the spillover terms in columns 3 and 4 are smaller and not statistically significant. Similarly, column 5 shows that proximity to other treated police stations has no effect on the number of drunken drivers caught in the final check. Thus, we find very limited evidence that inter-station spillovers are important in the prosecution of drunken drivers, confirming our focus on the intra-station dynamics of criminal behavior.

Social Learning and Sober Exploration

Our primary model focuses on driver learning from personal experience encountering police checkpoints while engaged in risky drunken driving. However, it is possible that individuals might also learn about police activity while driving sober or through social learning from

friends who have encountered the police checkpoints. While these mechanisms are difficult to identify solely with data on aggregate accidents and arrests, we can gain some insights from the immediate response of road accidents to police checkpoints. Consider a model in which agents engage in drunken driving only occasionally, say with 10% probability, and thus only 10% of individuals personally witness a checkpoint. If drivers learn only through personal experience, the immediate impact of the checkpoint will be muted, since only 1% ($= .1 \times .1$) of the informed agents would have engaged in drunken driving the following day anyway. Any effects of the crackdown will appear gradually and smoothly. In contrast, if there is costless exploration or social learning, one might expect a large immediate effect of a checkpoint on drunken driving and hence on road accidents the following night, since a much larger fraction of the potential drunken driving population will be informed. Thus, the empirical drop in road accidents on the night immediately following a checkpoint may be informative about other mechanisms of individual learning.¹

Table A7 presents regression specifications examining the immediate impact of checkpoints on night accidents and deaths in the following days (unlike our main results, these regressions are estimated using daily data). Columns 1 and 2 focus on the impact after exactly 1 night, with separate coefficients for the fixed and rotating stations. The results show no significant 1-night post checkpoint effect on accidents or deaths, for either intervention. The coefficient on main effect of the surprise treatment remains negative, statistically significant, and of a similar magnitude as in the main specification. However, this specification may be overly restrictive if there is social learning but the news of the intervention takes some time to disseminate. Therefore, in columns 3 and 4 we include 2- and 3-day lags of the checkpoint indicator. Once again, we find no statistically significant immediate effects of the checkpoints. Thus, while we cannot fully rule out forms of learning outside the model, we find no evidence of their implications in the data.

Implementation of the Crackdown Intervention

As we anticipated when designing the Police Lines intervention, implementation of the assigned checkpoints was far from perfect. Of the 1,565 checkpoints that the surveyors visited, only 980 (63%) were actually implemented by the police. This partial compliance would substantially modify the interpretation of our results if it were correlated with the treatment assignment in the intervention, for instance if fixed checkpoints were less likely to be carried out. In Table A8 we examine the impact of the treatments on police implementation of the intervention. Columns 1–3 focus on whether the checkpoint occurred (using OLS, probit, and focusing on the final checks only), while columns 4 and 5 examine the duration of the checkpoint and the number of vehicles stopped, respectively. The most striking finding is that the Police

¹In principle, the number of arrests on the following night would also be informative when police implemented another checkpoint the next night. However, since consecutive checkpoints may have affected arrest numbers through multiple channels (for example, police effort and morale) we focus on the accident data.

Lines teams substantially outperformed the station-based teams on all outcomes—for example, column 1 shows they were 44% more likely to carry out a checkpoint. We also find that the police perform worse in stations with 3 checkpoints assigned per week, perhaps due to fatigue or burn-out. This affects the cardinal interpretation of our reduced-form results: areas with 3 checkpoints per week did not have exactly 3 times greater intensity than those with 1 checkpoint per week. However, realized enforcement intensity is still increasing in assigned intensity. Reassuringly, we find no evidence that implementation is correlated with treatment assignment to fixed or rotating checkpoint locations. There is a fairly large (though non-significant) negative coefficient on the variable indicating that the checkpoint was held off the main road, which is likely due to the fact that some of the roads 2 and 3 had low volumes of traffic. In conclusion, the partial implementation of the intervention does not change the main qualitative conclusions of the reduced-form results. It does, however, require that we use instrumental variables in the structural estimation, an approach that we outline below.

Legal or Informal Sanctions for Drunken Drivers

After stopping a drunken driver, police may fail to follow the legal ticketing procedure, either because they choose to release the driver with a warning or because they prefer to take a bribe rather than register an official ticket. If this behavior were uniformly prevalent in the Rajasthan Police, it might weaken the strength of the intervention and reduce the effects. In fact, as our results show, drivers seemed very concerned about the consequences of being stopped while drunken driving, so punishments seem to have been quite strong whether legal or extra-judicial. A more serious issue is the potential correlation between the strength or legality of the punishment and the intervention. We examined this issue by collecting the court records on drunken driving cases legally prosecuted over the course of the intervention, since all drunken drivers were required to report to court to pay their fine. These documents contained the name of the police station issuing the ticket, so we were able to generate a measure of the number of legal tickets issued by each police station over the course of the intervention. Table A9 contains the regression of this measure on dummy variables for various intervention categories. In column 1 we see that regressing legal cases on an indicator variable for rotating checkpoint police stations yields a large, though non-significant, coefficient. This is not surprising since this intervention was relatively more effective. In columns 2 and 3 we control for the number of drunken drivers that surveyors observed being caught in the police station (a noisy measure of the total) and indicator variables for the other interventions. Introducing these controls substantially reduces the size and significance of the rotating checkpoints variable, as expected. Thus, while we cannot observe the exact share of offenders legally prosecuted, we find no evidence that the legal treatment of drunken drivers is correlated with the rotating or fixed-checkpoint intervention.

A1.1. *Example Sequences of Individual Driver Choices and Outcomes*

Drivers' learning behavior can generate complex patterns of travel and staying home. Even if all drivers have the same priors at the beginning of the crackdown (as we assume they do), by period t they will have a distribution of state variables $h(x_t, \epsilon_t; \Psi_0)$. To illustrate, Figure A1 shows the simulated histories of two drivers, the first in a fixed checkpoint station, and the second in a rotating checkpoint station. The background of the graphs denotes the road on which the driver is travelling: blue for road 1, red for 2, green for 3, and gray if the potential driver is staying home. The solid black line (and the left-hand axis) shows his belief regarding the probability that the checking is ongoing at the beginning of the period, π_t . The blue, red, and green lines show, respectively, λ_{1t} , λ_{2t} and λ_{3t} , and their scale is shown on the right-hand axis. The sequences of checkpoints and parameters used to simulate these histories are those estimated from the data.

The driver in Panel A encounters a checkpoint on road 1 on night 29 of the crackdown. His beliefs about λ_1 spike upwards since (as we discuss below) drivers have very diffuse priors about the initial probability of checking on road 1. His belief about the likelihood that the crackdown is ongoing (π_t) also shoots up, reaching about 80%. For several weeks afterwards the driver has a positive belief that a crackdown is ongoing and stays home given the high cost of being caught drunken driving. Finally, at night 66 the driver returns to drunken driving, starting on road 2 which yields the highest utility among alternative roads (where beliefs about intensity of checking remained unchanged). Later trips on road 1 in periods 79 and 86 cause reductions in λ_1 and eventually driver activity returns to normal.

Panel B depicts the history of a (particularly unlucky) driver in a police station with checking on all roads.² This driver initially encounters a checkpoint on road 3 at night 9. Again, π_t rises to about 80%, and since his priors on λ_3 are very diffuse, λ_3 spikes to 1 and the driver will never drive back on that road. The driver's awareness that the crackdown might be ongoing is sufficient to dissuade her from driving for about 30 nights before resuming back on roads 1 and 2. Unfortunately (for her) she encounters a checkpoint on road 1 at night 56, which increases her posterior on λ_3 to about 0.33 and sends π_t back to 0.8. This scares her away from drunken driving for the remainder of the intervention.

A2. MISSING CHECKPOINT IMPLEMENTATION DATA

Not all of the assigned checkpoints were actually conducted by the police and we only have data on the implementation status of 76% of the checkpoints (either through surveyor observation or GPS tracking of the police vehicle). Thus, in 24% of cases we do not know with certainty whether a checkpoint was actually implemented. We incorporate this fact into the simulations by allowing the fraction of agents who encounter an assigned checkpoint to be less than 1 whenever the actual implementation status is unknown, and specifically to depend

²This history was chosen for illustrative purposes and contains more interaction with the police than typical.

upon the treatment group of the police station. We first run a LASSO probit regression of an indicator that the police implement the checkpoint on the fully interacted set of all intervention categories (including the Police Line intervention, which affects implementation, as we saw in Table A8), plus variables indicating checkpoints that occurred in the early, middle, or late stage of the intervention, police station fixed effects, the location of the checkpoint, days since the previous checkpoint, and interactions.³ Using the variables selected by the LASSO algorithm, we predict each unobserved checkpoint's compliance probability and use this to determine how many of the simulated agents choosing to drive on that road would encounter the checkpoint on that night. For example, suppose 100 simulated agents in station i find it optimal to drive drunk on road 2 on night t , when there was a checkpoint assigned on road 2, and station i would be predicted to implement the checkpoint with 75% probability. Then $100 \times .75 = 75$ agents would experience the checkpoint, and hence update their beliefs negatively about checking on road 2, while the remaining 25 would see no police presence and update positively.

A3. COUNTERFACTUALS WITH INCOMPLETE IMPLEMENTATION

The enforcement strategies discussed in the main body of the paper do not take into account potential partial compliance with the crackdown protocol by the police. However, as we show in Table A8, the police are less likely to carry out a crackdown when the intensity of enforcement is high (particularly in the three-checkpoint-per week intervention). To incorporate this factor into the design of the counterfactual policy, we adjust the probability of a crackdown occurring to be $\Phi(.615 - .139(F^*))$ where F^* represents the assigned station-level frequency of checking and $\Phi(\cdot)$ is the Normal CDF.⁴ These strategies may be more reasonable than those that assume full compliance, particularly for policies that imply very high-intensity checking.

If we consider partial compliance (A13) the optimal strategy is basically unchanged for the first two scenarios, although somewhat less effective with a decrease in drunken driving of 60%. Again, incomplete police compliance has little effect on the overall strategy but decreases its effectiveness. Looking at row 3, accounting for partial compliance does make a difference in the strategy, extending the optimal duration to 37 days and shifting the location of 95% of checkpoints to road 1.

³This regression is analogous to column 1 of Table A8, with the inclusion of a variety of additional control variables as selected by the LASSO probit estimator.

⁴These parameters were estimated from the police implementation data in a probit regression analogous to column 2 of A8, except using a continuous measure of assigned frequency.

A4. APPENDIX TABLES

TABLE A1
PILOT VS. MAIN INTERVENTION RESULTS

	Daylight		Darkness	
	(1) Accidents	(2) Deaths	(3) Accidents	(4) Deaths
<i>Panel A: Pilot</i>				
Fixed checkpoints during & post intervention	-0.007 (0.017)	0.018 (0.013)	-0.005 (0.012)	-0.014 (0.012)
Rotating checkpoints during & post intervention	-0.008 (0.018)	0.00 (0.013)	-0.012 (0.010)	-0.030 (0.023)
Police Lines team during & post intervention	0.002 (0.020)	-0.005 (0.013)	-0.003 (0.012)	0.019 (0.017)
Mean of dep. variable	0.082	0.026	0.036	0.016
N	1123	1123	1123	1123
<i>Panel B: Main Intervention</i>				
Fixed checkpoints during & post intervention	0.00 (0.006)	-0.006 (0.004)	-0.002 (0.005)	0.00 (0.003)
Rotating checkpoints during & post intervention	0.011 (0.007)	0.00 (0.005)	-0.014 (0.005)	-0.004 (0.003)
Police Lines team during & post intervention	-0.005 (0.006)	0.001 (0.004)	0.005 (0.004)	0.00 (0.003)
Mean of dep. variable	0.086	0.031	0.032	0.017
N	3601	3601	3601	3601
Month fixed effects	Yes	Yes	Yes	Yes
Police Station FE	Yes	Yes	Yes	Yes

This table presents the impact of fixed and rotating checkpoint interventions on the number of daily road accidents and deaths per police station in the pilot intervention (Aug. 2010-Jan. 2011) and the main intervention (Feb. 2011-Oct. 2012). The during & post intervention variable is positive for the duration of the crackdown and 90 days afterwards. Accident/death counts have been re-normalized to the per-day level. Each observation corresponds to a police-station month, with months that span the beginning or end of the intervention divided into 2 observations using daily accident/death data and weighted accordingly. All data were taken from police administrative reports, as collected in both treatment and control police stations. Standard errors in parentheses are clustered at the police station level.

TABLE A2
FIXED VS. ROTATING, INTENSITY OF CHECKING

	Daylight		Darkness	
	(1) Accidents	(2) Deaths	(3) Accidents	(4) Deaths
Rotating checkpoints×1/week during & post intervention	0.0110 (0.0100)	-0.0007 (0.0083)	-0.0099 (0.0064)	0.0035 (0.0055)
Rotating checkpoints×2/week during & post intervention	0.0025 (0.0071)	-0.0035 (0.0044)	-0.0102 (0.0043)	-0.0102 (0.0043)
Rotating checkpoints×3/week during & post intervention	0.0050 (0.0069)	-0.0013 (0.0066)	-0.0152 (0.0048)	-0.0069 (0.0034)
Fixed checkpoints ×1/week during & post intervention	-0.0058 (0.0067)	-0.0036 (0.0057)	-0.0034 (0.0045)	-0.0032 (0.0031)
Fixed checkpoints×2/week during & post intervention	0.0010 (0.0056)	-0.0043 (0.0044)	-0.0022 (0.0057)	-0.0025 (0.0040)
Fixed checkpoints×3/week during & post intervention	-0.0001 (0.0081)	-0.0088 (0.0052)	-0.0031 (0.0049)	-0.0043 (0.0035)
Police Lines team during & post intervention	-0.0013 (0.0052)	0.0033 (0.0040)	0.0035 (0.0032)	0.0017 (0.0028)
Month FE	Yes	Yes	Yes	Yes
Police Station FE	Yes	Yes	Yes	Yes
Mean of dep. variable	0.085	0.029	0.033	0.016
N	4724	4724	4724	4724

This table presents the impact of fixed and rotating checkpoint interventions on the number of daily road accidents and deaths per police station, using monthly data from August 2010–October 2012. The during intervention variable is positive for the duration of the crackdown, and the post intervention variable is positive for 90 days afterwards. Accident/death counts have been re-normalized to the per-day level. Each observation corresponds to a police-station month, with months that span the beginning or end of the intervention divided into 2 observations using daily accident/death data and weighted accordingly. All data were taken from police administrative reports, as collected in both treatment and control police stations. Standard errors in parentheses are clustered at the police station level.

TABLE A3
CHECKPOINT SURVEYS DURING INTERVENTION (ROBUSTNESS)

	Drunk drivers and motorcyclists caught			
	(1)	(2)	(3)	(4)
Rotating checkpoint station	0.087 (0.134)	-0.834 (0.432)		
Frequency		-0.362 (0.135)		
Rotating checkpoint × frequency		0.463 (0.188)		
Weeks of checking			-0.049 (0.020)	
Rotating checkpoint × weeks of checking			0.053 (0.025)	
Number Previous checkpoints				-0.017 (0.008)
Rotating checkpoint × number previous checkpoints				0.015 (0.010)
Police Lines team	0.993 (0.160)	1.006 (0.154)		
Police Station FE	No	No	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Mean of dep. variable	0.794	0.794	0.794	0.794
Observations	1333	1333	1333	1333

This table reports the intensity and dynamic effects of the crackdown on the number of drunken drivers caught, with the number of drunken drivers caught set to 0 if the police did not implement the checkpoint. Results correspond with columns 1–4 of Table 5, except that in Table 5 checkpoints that were not implemented are dropped from the data.

All outcome variables are based on data collected by surveyors sent to monitor the checkpoints. The frequency of checking variable is the number of checkpoints per week: 1, 2, or 3. The weeks of checking variable is the number of weeks that have elapsed since the first checkpoint. The number of previous checkpoints is the number of checkpoints assigned prior to the given night, after the start of the intervention. All specifications include controls for whether the police station is located on a major highway, the pre-intervention accident rate, and assignment to the Police Lines intervention.

Standard errors in parentheses are clustered at the police station level.

TABLE A4
SUBSTITUTION PATTERNS ACROSS ROUTES

	Drunk drivers and motorcyclists caught		
	First 28 days of intervention	Full intervention without final check	All checkpoint data
	(1)	(2)	(3)
Location 1×# checkpoints at that location	-0.060 (0.047)	-0.043 (0.012)	-0.040 (0.012)
Location 2×# checkpoints at that location	0.131 (0.170)	-0.102 (0.064)	-0.010 (0.038)
Location 3×# checkpoints at that location	-0.306 (0.187)	-0.120 (0.060)	-0.146 (0.059)
Location 1×# checkpoints at other locations	0.103 (0.079)	0.017 (0.016)	0.002 (0.014)
Location 2×# checkpoints at other locations	0.068 (0.125)	0.055 (0.030)	-0.028 (0.014)
Location 3×# checkpoints at other locations	0.124 (0.060)	0.052 (0.032)	0.052 (0.033)
Police Station FE	Yes	Yes	Yes
Mean of dep. variable	1.355	1.243	1.242
Observations	346	852	960

This table reports the dynamic effects of the crackdown on the number of drunken drivers caught. All outcome variables are based on data collected by surveyors sent to monitor the checkpoints. Rows 1–3 report the effect of the number of prior checkpoints at a location on the number of drunken drivers arrested at that location. Rows 4–6 report the effect on the number of drunken drivers at a given location of the number of prior checkpoints held at other locations. Standard errors in parentheses are clustered at the police station level.

TABLE A5
ACCIDENTS AND DEATHS - SPILLOVERS

	Daylight		Darkness	
	(1) Accidents	(2) Deaths	(3) Accidents	(4) Deaths
Panel A. Spillovers main effects				
Treated station during & post intervention	0.0015 (0.0041)	-0.0022 (0.0029)	-0.0057 (0.0027)	-0.0039 (0.0021)
Treated stations in 10 km during & post intervention	0.0006 (0.0012)	0.0008 (0.0009)	0.0005 (0.0010)	0.0000 (0.0007)
Treated stations in 20 km during & post intervention	-0.0002 (0.0020)	-0.0004 (0.0015)	-0.0014 (0.0017)	-0.0011 (0.0010)
Treated stations in 40 km during & post intervention	-0.0008 (0.0019)	-0.0007 (0.0018)	0.0007 (0.0011)	-0.0007 (0.0010)
Panel B. Spillover results by intervention scope				
Fixed checkpoints during & post intervention	-0.003 (0.004)	-0.004 (0.003)	0.003 (0.003)	-0.001 (0.002)
Fixed checkpoints in 10 km during & post intervention	0.006 (0.003)	0.001 (0.003)	-0.001 (0.002)	0.001 (0.002)
Fixed checkpoints in 20 km during & post intervention	0.001 (0.004)	-0.001 (0.003)	-0.001 (0.003)	-0.002 (0.002)
Fixed checkpoints in 40 km during & post intervention	0.001 (0.003)	0.000 (0.003)	-0.001 (0.002)	-0.001 (0.001)
Rotating checkpoints in 10 km during & post intervention	-0.005 (0.003)	0.001 (0.003)	0.002 (0.002)	-0.000 (0.002)
Rotating checkpoints in 20 km during & post intervention	-0.002 (0.003)	0.001 (0.002)	-0.002 (0.002)	0.001 (0.002)
Rotating checkpoints in 40 km during & post intervention	-0.002 (0.003)	-0.002 (0.002)	0.002 (0.002)	-0.000 (0.001)
Month FE	Yes	Yes	Yes	Yes
Police Station FE	Yes	Yes	Yes	Yes
Mean of control	0.085	0.029	0.033	0.017
N	4724	4724	4724	4724

See note on the analogous regressions in Tables 3 and 4 for details on variables and specifications.

TABLE A6
DRUNK DRIVERS CAUGHT - SPILLOVERS

	Main intervention				Final check	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment station					-1.021 (0.378)	-0.756 (0.443)
Rotating checkpoints		-0.327 (0.233)				-0.376 (0.328)
Weeks of checking	-0.041 (0.030)	-0.094 (0.042)	-0.033 (0.030)	-0.088 (0.043)		
Rotating checkpoints × weeks of checking		0.081 (0.036)		0.088 (0.037)		
Treated stations in 10 km	0.768 (0.255)	0.602 (0.518)			-0.196 (0.157)	-0.025 (0.443)
Treated stations in 20 km	-0.409 (0.264)	-0.291 (0.377)			-0.137 (0.332)	0.656 (0.799)
Treated stations in 40 km	-0.328 (0.185)	-0.681 (0.328)			-0.423 (0.284)	-0.057 (0.390)
Treated stations in 10 km × weeks of checking	-0.052 (0.018)	-0.013 (0.042)	-0.033 (0.023)	0.011 (0.054)		
Treated stations in 20 km × weeks of checking	0.041 (0.028)	0.036 (0.041)	0.019 (0.029)	-0.009 (0.049)		
Treated stations in 40 km × weeks of checking	0.019 (0.024)	0.046 (0.039)	0.000 (0.023)	-0.003 (0.039)		
Rotating checkpoints in 10 km		0.249 (0.955)				-0.329 (0.725)
Rotating checkpoints in 20 km		-0.249 (0.565)				-1.494 (1.102)
Rotating checkpoints in 40 km		0.688 (0.446)				-0.67 (0.627)
Rotating checkpoints in 10 km × weeks of checking		-0.051 (0.089)		-0.054 (0.096)		
Rotating checkpoints in 20 km × weeks of checking		0.006 (0.067)		0.053 (0.073)		
Rotating checkpoints in 40 km × weeks of checking		-0.029 (0.058)		0.024 (0.061)		
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Police Station FE	No	No	Yes	Yes	No	No
Mean of dep. variable	1.223	1.223	1.223	1.223	1.220	1.220
N	866	866	866	866	109	109

See note on the analogous regressions in Tables 5 and 6 for details on variables and specifications.

TABLE A7
SHORT-TERM EFFECT OF CHECKPOINTS ON ACCIDENTS

	Darkness			
	(1) Accidents	(2) Deaths	(3) Accidents	(4) Deaths
Fixed checkpoints during & post intervention	-0.00145 (0.00357)	-0.00285 (0.00235)	-0.00089 (0.00385)	-0.00284 (0.00257)
Rotating checkpoints during & post intervention	-0.00878 (0.00326)	-0.00393 (0.00323)	-0.00883 (0.00356)	-0.00352 (0.00359)
Fixed checkpoint 1 night before	0.00374 (0.00554)	0.00374 (0.00437)	0.00502 (0.00522)	0.00422 (0.00425)
Rotating checkpoint 1 night before	-0.00140 (0.00544)	-0.00342 (0.00403)	-0.00145 (0.00542)	-0.00347 (0.00402)
Fixed checkpoint 2 nights before			-0.00614 (0.00498)	-0.00293 (0.00383)
Rotating checkpoint before 2 nights			-0.00074 (0.00555)	0.00122 (0.00406)
Fixed checkpoint 3 nights before			0.00133 (0.00541)	0.00227 (0.00424)
Rotating checkpoint 3 nights before			0.00083 (0.00575)	-0.00366 (0.00396)
Police Lines team during & post intervention				
Month FE	Yes	Yes	Yes	Yes
Police Station FE	Yes	Yes	Yes	Yes
Mean of control	0.03558	0.01682	0.03558	0.01682
N	94,276	94,276	94,276	94,276

This table presents the impact of fixed and rotating checkpoint interventions on the number of daily road accidents and deaths from August 2010 through December 2011. The variables indicating a rotating/variable checkpoint 1, 2, or 3 nights before are station-level indicators equal to 1 if a checkpoint was carried out on any road in the past 1, 2, or 3 nights. The during & post intervention variable is positive for the duration of the crackdown and up to 90 days afterwards. All data were taken from police administrative reports, as collected in both treatment and control police stations. Standard errors in parentheses are clustered at the police-station level.

TABLE A8
IMPLEMENTATION OF INTERVENTION

	(1) Checkpoint occurred - OLS	(2) Checkpoint occurred - Probit	(3) Checkpoint occurred - Final Check OLS	(4) Duration of checkpoint	(5) Number vehicles stopped
Fixed checkpoints			0.006 (0.106)		
Rotating checkpoints	-0.001 (0.046)	0.000 (0.153)	0.06 (0.067)	-1.856 (5.443)	-0.235 (7.893)
Intensity 2/week	0.021 (0.049)	0.047 (0.154)	-0.009 (0.065)	-4.827 (4.982)	-16.280 (9.455)
Intensity 3/week	-0.105 (0.053)	-0.328 (0.163)	-0.057 (0.080)	-11.701 (5.041)	-15.624 (9.786)
Locations 2 or 3	-0.024 (0.038)	-0.105 (0.132)		-0.326 (4.779)	-2.786 (6.090)
Police Lines team	0.284 (0.035)	0.921 (0.117)	0.337 (0.120)	23.653 (4.439)	25.390 (6.886)
District FE	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable	0.641	0.641	0.599	159.293	62.537
N	1353	1353	182	867	867

The outcome variable in columns 1–3 is an indicator equal to 1 if the assigned checkpoint was implemented by the police during the main intervention. The outcome variable in column 4 is the analogous indicator for the final checks. The outcome variable in column 5 is the duration of the checkpoint in minutes (from the time the police arrived to when they stopped checking), and the outcome in column 6 is the number of cars and motorcycles checked. Columns 5 and 6 use only data conditional on the checkpoint being conducted. All data on outcomes were collected by surveyors monitoring the police checkpoints. All specifications include controls for whether the police station is located on a major highway, the pre-intervention accident rate.

Standard errors in parentheses.

TABLE A9
LEGAL PROSECUTION OF DRUNKEN DRIVERS

	Cases Processed by Court		
	(1)	(2)	(3)
Rotating checkpoints	4.258 (4.338)	3.095 (3.997)	-0.520 (3.332)
Drunk drivers caught in station		5.572 (1.563)	2.488" (1.673)
2 checkpoints per week			10.756 (3.246)
3 checkpoints per week			17.656 (4.085)
Police Lines teams			-17.715 (4.893)
District FE	Yes	Yes	Yes
Mean of dep. variable	25.55	25.55	25.55
N	112	112	112

The outcome variable is the number of drunken driving tickets that were processed by the criminal court. Criminal cases in the court records were linked to the police station from which the ticket originated. Several local courts were not willing to share data. All specifications include controls for whether the police station is located on a major highway, the pre-intervention accident rate, and district fixed effects.

Standard errors in parentheses.

TABLE A10
SIMULATED FIXED VS. ROTATING POOLED RESULTS

	Darkness
	(1)
	Accidents
Fixed checkpoints during & post intervention	-0.00495 (0.00056)
Rotating checkpoints during & post intervention	-0.00684 (0.00072)
Month FE	Yes
Police Station FE	Yes
Mean of dep. variable	0.03549
N	55692

Results shown on nighttime accident data simulated for the Aug.–Jan. 2010 and Aug.–Jan. 2011 period using the baseline model and parameters as described in Section 6. Specifications are analogous to those estimated from real data in Table 4. Controls include: fixed or rotating treatment indicators, day of week, urban/rural, national highway, police-station fixed effects and the number of prior accidents in the police station. Standard errors in parentheses are clustered at the police-station level.

TABLE A11
SIMULATED CHECKPOINT SURVEYS DURING INTERVENTION

	Drunk drivers and motorcyclists caught			
	(1)	(2)	(3)	(4)
Rotating checkpoint station	0.158 (0.191)	-1.057 (0.513)		
Frequency		-0.539 (0.189)		
Rotating checkpoint × frequency		0.629 (0.246)		
Weeks of checking			-0.104 (0.023)	
Rotating checkpoint × weeks of checking			0.099 (0.027)	
Number previous checkpoints				-0.043 (0.010)
Rotating checkpoint × number previous checkpoints				0.037 (0.012)
District FE	Yes	Yes	Yes	Yes
Police Station FE	No	No	Yes	Yes
Mean of dep. variable	1.236	1.236	1.236	1.236
N	852	852	852	852

Results shown on number of simulated drunken drivers apprehended during the 2010 and 2011 interventions, using the baseline model and parameters as described in Section 6. The specifications are analogous to those in columns 1–4 of Table 5.

Standard errors in parentheses are clustered at the police-station level.

TABLE A12
SIMULATED DRUNK DRIVERS CAUGHT ON FINAL CHECK

	All Stations		Rotating checkpoints		Fixed Checkpoints	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-1.471 (0.422)	-0.955 (0.758)	-1.656 (0.512)	-1.719 (0.291)	-1.451 (0.564)	-0.452 (0.557)
Days since last checkpoint	0.018 (0.009)	0.009 (0.021)	0.019 (0.014)	0.053 (0.024)	0.022 (0.009)	-0.010 (0.030)
Frequency		-0.249 (0.270)		-0.005 (0.220)		-0.523 (0.381)
Days since last checkpoint×frequency		0.005 (0.009)		-0.015 (0.009)		0.017 (0.015)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	2.169	2.169	2.169	2.169	2.169	2.169
Mean treatment effect, @ freq = 2	-1.232	-1.218	-1.405	-1.431	-1.161	-1.186
P-value of mean treatment effect	0.00476	0.00580	0.00923	0.00700	0.00100	0.000657
N	108	108	77	77	74	74

This table reports the reduced-form correlations between the interventions and the number of simulated drunken car and motorcycle drivers caught at the final check conducted after the end of the intervention in all police stations, including control stations. The specifications are analogous to those estimated with real data in Table 6. Columns 1 and 2 compare pooled treatment police stations with control stations, columns 3 and 4 compare rotating checkpoint stations with controls, and columns 5 and 6 compare fixed checkpoint police stations with controls. Robust standard errors are in parentheses.

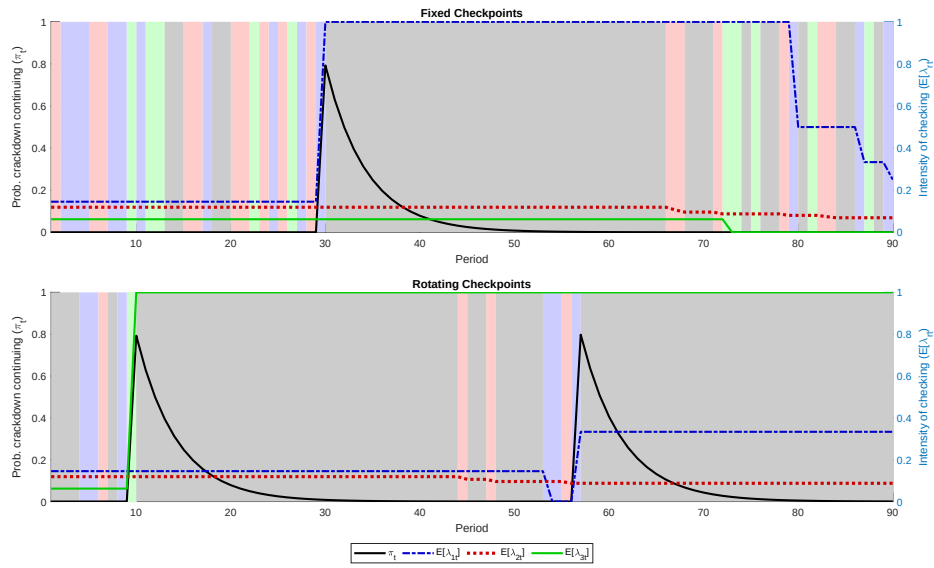
TABLE A13
OPTIMAL CRACKDOWN STRATEGY AND DURATION (PARTIAL COMPLIANCE)

	Prior Warning	Duration	Pct. on road 1	Decrease in drunken driving
		(1)	(2)	(3)
Estimated beliefs	No	98 days	40%	60%
	Yes	99 days	45%	66%
Equilibrium beliefs	No	37 days	95%	90%
	Yes	>22 days	5%-95%	100%

This table displays the parameters and effectiveness of an optimal anti–drunken driving campaign that allocates 20 checkpoints over up to 120 days and across 3 locations under partial police compliance. Effectiveness is measured by the undiscounted share of agents drinking and driving on any road over the 120 days after the first checkpoint. Parameters used in rows 1 and 2 (“Estimated beliefs”) are all as estimated in the baseline model. Parameters used in rows 3 and 4 are as estimated from the data for drivers’ utilities, but with beliefs constrained to equal police strategies in expectation; see details in Section 6.4. Counterfactual strategies estimated in rows 1 and 3 assume that drivers have no prior warning of the beginning of drunken driving enforcement. Rows 2 and 4 assume that all drivers are informed of the beginning of the campaign. Column 1 shows the duration of the optimal campaign, and column 2 shows the share of checks allocated to road 1. The remaining checks are equally divided between roads 2 and 3. Column 3 shows the decrease in drunken driving induced by the campaign relative to the share of drivers drinking and driving on the night just prior to the first checkpoint.

A5. APPENDIX FIGURES

FIGURE A1.—Example of Agents’ Simulated Actions and Beliefs



REFERENCES

Blattman, C., Green, D. P., Ortega, D., and Tobón, S. (2021). Place-based interventions at scale: The direct and spillover effects of policing and city services on crime. *Journal of the European Economic Association*, 19(4), 2022–2051. [2]