Crime, Punishment and Monitoring: Deterring Drunken Driving in India

Preliminary and incomplete

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The relationship between crime and police enforcement is both one of the most fundamental examples of behavior control through incentives as well as a pressing and relevant policy question. This paper reports the results of a new randomized trial of the effects of an anti-drunken driving program on road accidents and deaths in India—the first randomized evaluation of sobriety checkpoints in the literature and one of the only policing experiments in the developing world. The experiment varied both the frequency of police checkpoints (0-3 times per week) as well as whether those checkpoints were carried out always at the same location or randomly shifted across locations to maintain the element of surprise. Our results are consistent with a model in which potential drunken drivers choose to drink and drive based on the level of enforcement and potential travel costs to avoid the police, and hence are deterred further by more broad police enforcement. A simultaneous, cross-cutting experiment on the monitoring and incentivization of the police teams shows strong implementation benefits to employing dedicated, closely monitored teams to conduct sobriety checkpoints.

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Introduction

Since the pioneering works of Gary Becker (1968), economists have formulated theories on the responses of criminals to legal punishments, and hence the optimal design of these punishments. Empirical studies of the relationship between crime and enforcement have been carried out for at least as long, using a broad array of methodologies. This paper returns to and extends an early and sometimes controversial literature (Kelling, et al. 1974, Sherman and Berk 1984) that attempted to investigate the causal effects of enforcement through randomized variation in the intensity and strategy of policing. We study the effects of a randomized anti-drunken driving crackdown implemented in the Indian state of Rajasthan. We build upon these earlier studies in several dimensions: expanding the set of interventions carried out to learn about potential criminal behavior, greatly increasing the geographical scope of the project, and interpreting the results in the context of a simple model of potential drunken driver behavior.

In particular, our work investigates the shape of the relationship between the intensity of expected punishment and the response of drunken drivers. As the previous literature has highlighted, the shape of this relationship has strong implications for the optimal strategy for sobriety checks. To fix intuition, consider the case of two villages, each of whose inhabitants will engage in drunken driving as long as the probability of apprehension is less than 60%. If the local police can only enforce the law in one village at a time, spreading their resources across both villages will not be successful in deterring any villagers from drunken driving. An alternative strategy of a concentrated enforcement in one village, will, on the other hand, at least succeed in inducing one group to remain sober. This tradeoff, and more generally the tradeoff between concentration and dispersion of police forces, is one of the main theoretical and empirical focuses of this paper.

The context of this study, the Indian state of Rajasthan, also differentiates it from previous randomized evaluations of policing, and from most previous studies of crime in general. Studying drunken driving in a developing country setting has several advantages: First, our program was conducted in an environment in which there had previously been essentially no enforcement of drunken driving laws. We are thus able to estimate a very different range of behavior responses than most research in developing countries where the baseline level of perceived enforcement may be quite high. Second, low costs of surveying mean that unusually detailed data on vehicle types passing checkpoint and non-checkpoint sites as well as police behavior at checkpoint sites could be collected. Finally, due to the institutional structure of the Rajasthan police, we were able to conduct an additional, nested experiment on the effect of different incentives and monitoring on the effort exerted by the police themselves.

This second, human resource oriented, intervention was inspired by earlier research suggesting that Indian police staff require additional incentives even to carry out the duties ordered by their commanding officers in the police hierarchy (Banerjee, et al. 2012). We selected a group of staff who might be highly motivated by the prospect of a posting to a more desirable work location, perhaps the strongest incentive available in the Indian police system (Transparency International 2005). These personnel were informed that good performance on this assignment might result in higher probability of a transfer, and to make this incentive credible their performance was monitored by GPS systems attached to their vehicles.

The paper is divided into six sections. Section 1 provided some background on both previous anti-drunken driving research as well as the police procedure for drunken driving enforcement in India. Section 2 presents a simple model of the behavior of potential drunken drivers that will inform the empirical work later in the paper. Section 3 presents the details of the design and implementation of the experiment, and section 4 outlines the data sources used in the analysis. Finally, section 5 presents the results, and section 6 offers a brief conclusion.

1. Background

Anti-Drunken driving literature

Each year 1.2 million people die in traffic accidents worldwide, with as many as 50 million injured. A staggering 85% of these deaths happen in developing countries (Davis, et al. 2003). Moreover, death and accident rates are rapidly increasing in developing countries even though these rates decreasing in the developed world (Davis, et al. 2003, WHO 2004). By 2030, traffic accidents will be the third or fourth most important contributor to the global disease burden, and will account for 3.7 percent of deaths worldwide, twice the projected share for malaria (Habyarimana and Jack 2009).

Estimates of drunk-driving frequency vary widely across countries and across studies. The role of alcohol in road accidents is also difficult to measure, especially in developing countries where police often lack the manpower and technology to measure drivers' alcohol levels. The available evidence suggests, however, that alcohol does play a major role in traffic accidents. According to a review of studies conducted in low-income countries, alcohol is present in between 33% and 69% of fatally injured drivers, and between 8% and 29% of drivers who were involved in crashes but not fatally injured (WHO 2004).

Sobriety checkpoints have been evaluated by a number of studies in a wide variety of contexts, and the general consensus is that these checkpoints significantly reduce traffic accidents and fatalities. Several recent meta-analyses (Peek-Asa 1999, Erke, Goldenbeld and Vaa 2009, Elder, et al. 2002) suggest that sobriety checkpoint programs reduce accidents by about 17% to 20%, and traffic fatalities by roughly the same amount. These results are not entirely conclusive, however, since most of the existing literature has struggled with a variety of challenges and limitations. First, no previous research has been conducted in the framework of a randomized trial, leaving even the few studies that employ some sort of multivariate statistical analysis open to concerns of endogeneity based on the location and timing of the interventions. Second, the vast majority of research has been conducted in developing countries, and consists of increases in checkpoints over and above what is already a relatively high standard of enforcement. Thus little is known about the impact of carryout out sobriety checkpoints versus a counterfactual of essentially zero enforcement. Finally, existing studies provide relatively little information about which strategies are most effective, since they do not contrast different checkpoint (or non-checkpoint) approaches to prevention of intoxicated driving.

Drunken Driving Enforcement in India

In India, highway safety laws of all kinds are generally enforced by fixed sobriety checkpoints manned by personnel from the local police station. Barriers are arranged on the roadway so that passing vehicles are forced to slow down, and the officers on duty signal selected vehicles to pull over as they pass through the barriers. If the roadblock is intended to target drunken driving, police personnel then ask the driver a few questions on his or her identity, destination, etc., while observing the driver's demeanor and smelling his or her breath. If the police feel the driver may be drunk, then according to the official procedure they will order him or her to blow into the breathalyzer, following the results of which the driver is either charged or released. The printed results of a handheld breathalyzer are considered sufficient proof of drunkenness in court.

Once caught, drunken drivers' vehicles are confiscated by the police, and the driver must appear in court to pay a fine or potentially face jail time, although imprisonment is never observed in our data. The fine amount depends largely on the judgment of the local magistrate, with a maximum fine of Rs. 2000 (roughly $50) for the first offence[1]. The driver must then return to the police station to recover the vehicle from the police lot.

Even this official procedure leaves many factors undetermined and up to the discretion of the police manning the roadblock. The choice of how many, and which, vehicles to pull over for questioning and potential testing is the most important. Ideally the police should target vehicles with the highest probability of drunkenness, and in conversations the police often noted that if they saw a vehicle with a family, or driven by elderly people they assumed that the driver would be unlikely to be drunk and let it pass. Other considerations may also enter the decision: police may be less likely to stop vehicles with many passengers who would be difficult to deal with at an isolated police station if the vehicle were impounded. Similarly, the police might be hesitant to stop luxury vehicles whose owners might be well connected and cause problems if subjected to breath testing.

Unscrupulous police officers are faced with another decision: whether to follow the official ticketing procedure or instead to solicit (or accept) a bribe from the accused drunken driver. There are several stages in the encounter when this exchange could take place. Police may either demand (or take) a side payment prior to the breathalyzer test, if it is clear the driver is drunk, or only after the driver has proven his or her drunkenness via the official test. Corruption may also occur that the final stage when once-drunken drivers return to the police station to recover their impounded vehicles, when police may demand excess payments to release the vehicles.

To understand our results, it is important to note that roadblocks of this type occurred extremely rarely prior to the intervention and in the control police stations during the intervention. This rarity is primarily due to the fact that breathalyzers had not been widely distributed to police stations prior to the program, and without a breathalyzer the local police would have needed to take a suspect to the hospital for blood testing in order to be reasonably certain of securing a conviction under the

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Clause 185 of the 1988 Motor Vehicles Act stipulates:
"Whoever, while driving, or attempting to drive, a motor vehicle -

a) has, in his blood, alcohol exceeding 30 mg. per 100 ml. of blood detected in a test by a breath analyser, or
b) is under the influence of a drug to such an extent as to be incapable of exercising proper control over the vehicle

shall be punishable for the first offence with imprisonment for a term which may extend to six months, or with fine which may extend to two thousand rupees, or with both; and for a second or subsequent offence, if committed within three years of the commission of the previous similar offence, with imprisonment for a term which may extend to two years, or with fine which may extend to three thousand rupees, or with both."

30mg/100ml clause of the motor vehicles act. This procedure would have been very inconvenient for the police (who typically have a single vehicle per police station), and seems to have prevented enforcement at all except urban police stations in Jaipur, the state capital. While breathalyzers were distributed somewhat more widely prior to the program, control police stations hardly ever used them. In the 925 nights that surveyors visited control police stations, on only 7 (.76%) occasions did they witness the police carrying out a roadblock.

2. Theory

Theoretical work on crime and punishment has focused on two main questions: first, the optimal balance between the probability of punishment and the severity of punishment, and second, the shape of the relationship between the expected punishment for a crime and criminals' willingness to commit the crime. We abstract away from the first of these issues since our research design provides no exogenous variation in the fines levied for drunken driving, and instead focus on the second. Consistent with the role of the model on informing the empirical investigation, we do not solve for the optimal design of police programs in terms of the parameterization of the model, but instead focus on the broad implications of the model assumptions for different classes of enforcement strategies.

The shape of the relationship between crime and expected punishment has strong implications for policing tactics. If this relationship is concave (decreasing)--the marginal effect of enforcement is greater the higher the level of enforcement-- then a "crackdown" model in which police concentrate their enforcement in a limited geographical area and/or time period will generate a greater reduction in crime than the allocation of the same police forces at a lower level of intensity over a broader area or longer period. Conversely, a convex relationship with decreasing returns to policing would imply that a constant, less concentrated police presence is optimal for reducing drunken driving.

This tradeoff is at the heart of models by Lazear (2005) and Eeckhout, Persico and Todd (2010), with the latter suggesting that the crackdown model is indeed supported by the data from Belgian speeding enforcement. Both papers make the link between the shape of the enforcement response function and the underlying distribution of utility from violating the law. If the CDF of this distribution is convex over the range of expected punishements that would potentially be induced by the police, then a crackdown model is optimal. Conversely, if it is concave over this range then police should spread enforcement evenly. The resources available to the police and the exact shape of the distribution determine the extend and concentration of the crackdown.

The context of the Rajasthan anti-drunken driving campaign introduces another option for drunken drivers: avoid the roadblock by travelling on an alternate route. We model this by letting each police station area consist of a set of N^R roads, indexed by j: $\{r_j\}_{j=1}^{N^R}$. Roads' indexes correspond to their cost of travel, c_j, with the best road in the police station area, c_1 having the lowest cost of travel. In the context of Rajasthan police stations, we may think of this as a main highway, or a well-paved road going through a town center, with other roads of increasingly bad quality in the area. The police may choose to enforce an anti-drunken driving campaign on road j; if they do so the expected punishment to drunken drivers from travelling on that road is P_j, which embodies the combination of the probability of punishment multiplied by the expected severity of that punishment.

Potential drunken drivers, indexed by i, differ along two dimensions of heterogeneity, first in their utility from getting drunk, $V_i \sim [V^H, V^L]$, and second in their cost of travel, $d_i \sim [d^H, d^L]$. Let the joint distribution of these types be $f(V_i, d_i)$. Based on these characteristics they make the discrete choice to drink or not drink, expressed in the indicator variable $D_i \in \{0,1\}$. We assume all the parameters above are known to the driver, and that his utility of driving on the road takes the form

$$U(r_j, D_i; c_j, d_i, P_j, V_i) = -c_j d_i + D_i(V_i - P_j)$$

Drivers' idiosyncratic travel costs enter utility multiplicatively with the road quality in order to capture the intuition that travelers with high transport costs (for instance motorcycle drivers) will suffer more greatly from poor quality roads than those with lower idiosyncratic costs.

We consider two possible police strategies, chosen to reflect those actually implemented in the randomized evaluation: in a given police station, police may either announce that they are concentrating all their resource on the best road, r_1, with resulting expected punishment P^H or alternatively announce that they will carry out checks with equal probability on the best $n-1$ roads, which will then each provide expected punishment $P^L < P^H$, reflecting the fact that the police resources are spread more thinly.

Drivers face two choices: first, whether to drive drunk, and second, which road to take. If drivers choose not to drink the choice of road is obvious: they will take the best road r_1. For drunk drivers the decision reduces to the choice between r_1 or the least bad road on which the police are doing no checking, r_n, which in the case of the police concentrating their resources on r_1 will be r_2. We call those who choose to take r_1 “main road drunks”, and those who take r_n “avoider drunks”. Given the utilities above, the type space divides into three choices:

1. Stay sober: $P^H > V_i \cap d_i > \frac{V_i}{c_2 - c_1}$
2. Main road drunk: $V_i > P^H \cap d_i > \frac{P_H}{c_2 - c_1}$
3. Avoider drunk: $d_i < \frac{V_i}{c_2 - c_1} \cap d_i < \frac{P_H}{c_2 - c_1}$

These three cases are illustrated in Figure 1. Drivers with high utility from drinking but high transport costs remain on the main road, those with high desire to drink and low travel costs get drunk but avoid the main road, and those with either low desire to drink or high transport costs remain sober.

The red areas on Figure 1 demonstrate the alternative impact of the second police strategy of distributing forces more broadly. There are two effects: first, expected punishment from driving drunk on the main road goes down ($P^L < P^H$), thereby increasing the number of main road drunks. Second, the cost of avoiding the police increases ($c_n > c_2$), thereby decreasing the number of avoiders and increasing both the number of sober drivers and the number of main road drunks. The final effect on the number of sober drivers, however, is ambiguous. Depending on the increase in costs of driving relatively worse roads ($c_n\ vs.\ c_2$), the decrease in expected punishment ($P^L\ vs.\ P^H$), and the underlying distributions of V_i and d_i, the more dispersed police strategy may either increase or decrease total sobriety. On the other hand, the number of drunks on the main road unambiguously increases with a more dispersed police enforcement approach.

Second, consider a change in the amount of police enforcement under the two strategies. To first order, an infinitesimal increase in P_i increases the number of sober drivers by $\int_{P_j/(c_n - c_1)}^{d^H} f(P_j, d_i)\, \mathrm{d}d_i$, which is clearly increasing in c_n. Thus, all else held equal, a change in the expected punishment from drunken driving is most effective when drivers have already been forced onto poor quality roads by a more dispersed police enforcement strategy. However, the cost of increasing enforcement will no doubt be higher when spread across n locations.

The link between the model and the intervention design is illustrated in Figure 2. The three points on the lower line correspond to the surprise checkpoint strategies, and those on the upper line indicate the fixed checkpoint strategies, with strategies containing more frequent checkpoints higher and to the right along the line. As in Figure 1, the shaded area to the upper right of each point indicates the set of drivers who will remain drunk on the main road, the area below the point and the diagonal line indicates those who drink but avoid the roadblock, and the area to the left of the point contains types who remain sober. Because the vertical lines correspond to the probability of apprehension on the main road they are drawn more closely spaced for the surprise checkpoints where $P_1 = \{1/21, 2/21, 1/7\}$ (for frequencies 1,2,3) than for the fixed checkpoints with $P_1 = \{1/7, 2/7, 3/7\}$.

The shaded areas in Figure 2 do not correspond literally to the relative masses of the population, except in the case where $f(d_i, V_i)$ is bivariate uniform. Because of the large range of population responses that are possible given arbitrary distributions of drunken driving utility and travel cost, the model offers a limited number of unambiguous predictions:

1. For any value of checkpoint frequency, in stations employing the surprise strategy:
    a. The number of drunken drivers on the main road is higher
    b. The number of drunken drivers on alternate routes is lower
2. In particular, a switch from conducting fixed roadblocks once a week to surprise roadblocks three times a week will:
    a. Decrease overall drunkenness.
    b. Increase drunkenness on main roads and decrease it on alternate routes.
3. If the $f(d_i, V_i)$ distribution is continuous, then a marginal decrease in enforcement from the frequency 3-surprise intervention will decrease sobriety more than a marginal increase in enforcement from the frequency 1-fixed intervention.

3. Program Design

The anti-drunken driving program followed a straightforward randomized control trial (RCT) setup, consisting of three overlapping experiments, each varying a different aspect of how the campaign was implemented:

1. The frequency of the roadblocks. Roadblocks were carried out in the police station jurisdiction either 1, 2, or 3 nights per week.
2. The personnel carrying out the roadblock. Roadblocks were staffed by either,
    a. Police officers from the local police station (the status quo outside the intervention).
    b. A dedicated team selected from the police reserve force at the district level. These special teams were monitored by GPS devices installed in their vehicles.
3. The location of the roadblocks. Roadblocks occurred at either,

a. The same spot on every occasion, selected by the local chief of police as the location best suited to preventing accidents due to drunken driving.
b. One of a group of three locations, with each night's location chosen at random. The three spots were chosen by the chief of police as the best three suited to catching drunken drivers.

The program took place in two phases, an initial pilot, from September-early October 2010, and a larger rollout from September to the end of November 2011. The initial pilot covered 2 districts and 40 police stations, and the second covered 10 districts[2] and 183 police stations[3]. Treatment status was assigned randomly, stratified by district, whether a station was located on a national highway, and total accidents between 2008-2010. The assignment of police stations to treatment groups during both rounds of the intervention is reported in Table 1. In addition to the stations reported on this table, 16 and 60 were designated as control stations during each treatment round in which no additional sobriety testing occurred.

The 2010 and 2011 interventions were identical in implementation, with the exception that during the pilot all roadblocks occurred twice a week and the program lasted only one month. In the analysis and results stated below, we combine data from both intervention periods and control for any time trends using monthly fixed effects.

Roadblock locations

There is a common although certainly not universal hypothesis within the Rajasthan Police and the anti-drunken driving literature that surprise checkpoints are more effective than fixed checkpoints always held at the same location and time. To test this hypothesis, and the implicit assumptions of learning by potential and actual drunken drivers, we randomly assigned police stations to hold their checkpoints at either a single location, or a rotating set of three locations. In the single location group, the police station chief identified the best location in the station's jurisdiction for catching drunken drivers, and the checkpoints were carried out by either local staff or dedicated police lines teams at that location. Fixed

[2] The initial pilot covered Jaipur Rural district and Bhilwara. The project was extended to Alwar, Ajmer, Banswada, Bharatpur, Bikaner, Bundi, Jodhpur, Sikar, and Udaipur during the 2011 expansion.
[3] We drop one police station because it reported no accidents at all during the intervention period despite a substantial number of accidents in previous years. This station was assigned to treatment with fixed checkpoints done by a team from the police lines.

checkpoints were carried out, to the greatest degree possible, on the same day ever week, although scheduling difficulties occasionally made this impossible.

In contrast, surprise checkpoints rotated among the three best locations for catching drunken drivers, again as identified by the police station chief. Each police station's rotation was pre-determined in advance by the research team[4]. The differences among locations, in particular that the third best location usually has far fewer passing vehicles than the first, affect our analysis of these two program options. In regressions estimating the overall impact of the different strategies we do not control for checkpoint location fixed effects or characteristics, since these are themselves an outcome of the program choice. When analyzing learning by the public, however, we do include these controls to see how public behavior at a specific checkpoint responds to the enforcement strategy.

Roadblock frequency and timing

The variation in roadblock frequency was designed to identify the shape of the relationship between police enforcement and criminal behavior. In particular, to determine whether this relationship is concave or convex requires at least three points of variation. Discussions with the police determined that it was not feasible to carry out more than 3 roadblocks per week per police station, thus giving us the final randomization categories of 1, 2, or 3 roadblocks per week (and, of course, 0 in the control group). These frequencies were at the police station level, not the road level; for example in a surprise group police station with a frequency of 2 checkpoints per week each of the three roads would have a roadblock twice every three weeks. Checkpoints were always held at 7:00pm-10:00pm in the evening.

Roadblock personnel

Previous work with the Indian government (Duflo, Banerjee and Glennerster 2010) and the Rajasthan Police(Banerjee, et al. 2012) suggests that the implementation of government initiatives often decreases dramatically in the medium term if the bureaucrats implementing the project are not sufficiently motivated. To gain further insight on the role of monitoring and motivation in project implementation, as well as to guard against a failure of the project due poor implementation, the anti-drunken driving campaign was implemented by two different sets of police staff, with different motivation, monitoring, and characteristics. We henceforth refer to these groups as "station teams" and "lines teams".

[4] The three potential checkpoint locations were chosen by the station chiefs prior to assignment of stations into treatment, control and enforcement strategy groups. They are thus available for all stations and not affected by the program assignment.

The station teams consisted of the staff of the police stations under whose jurisdiction the checkpoint sites fell—the status quo for special crackdowns of this type in the Rajasthan Police. For these personnel, manning the checkpoints was an additional responsibility on top of their existing duties. They were monitored by the existing police hierarchy, which consisted of a single supervisory officer assigned to a group of roughly 5 police stations, and a district level officer monitoring the implementation of the project in all police stations within the district. These district level officers were limited in their ability to verify that checkpoints were actually being carried out, since many of the police stations were at least an hour's drive from the district headquarters.

The second group of staff implementing the project were drawn from the Police Lines, a reserve force of police kept at the district headquarters who normally perform riot control and VIP security duties. The police lines are often considered a punishment posting in the Indian police system, since a position in the police lines effectively removes an officer from contact with the public. Because of the undesirable nature of the police lines, these staff were potentially a group for whom positive incentives, specifically a transfer out of the police lines, could be provided relatively easily. However, an objective measure of performance was required to make these incentives more credible.

We generated this measure by installing GPS tracking devices in the police vehicles allocated to the police lines staff to travel from their barracks at the district headquarters to the checkpoint locations. These devices provided a real-time display of the vehicle's location to the district-level supervisory officers via a simple internet interface, as well as stored the vehicles' travel routes for future examination. The online interface also recorded the usage of the system by the district supervisory officers, providing a proxy for the degree of monitoring by district. Police lines teams were told of the installation of the GPS devices, and were informed that good performance on this assignment might improve their chances for transfer out of the police lines.

The police lines teams differed from the station teams in their incentives, their monitoring, and perhaps also in their fundamental motivation and ability as police officers. Disentangling the various effects of these differences is a challenge given the limitations of the program design, but certain features of the setup do allow for some insight. First, the lines teams were monitored only on their attendance at the roadblocks, but not on their conduct once there. Thus any differences once at the roadblock, for instance in terms of vehicles stopped, cannot be directly attributable to the GPS devices. Second, conversations with senior police officials suggest that selection of police staff into the Police Lines is, if anything, negative. Police constables tend to be transferred to the police lines if they have shown themselves to be incompetent in a police station. Hence if we find that lines teams outperform station teams it may be despite, not because of, their inherent aptitude for policing.

4. Data

To evaluate the effects of the anti-drunken driving campaign we draw on a combination of administrative data on road accidents and deaths, court records, and breathalyzer memory, as well as data on vehicles passing and stopped at roadblocks collected by surveyors hired for this program.

Administrative data

This study's main results on accident and death rates are drawn from accident reports recorded by the police. For each accident on which data has been collected properly we know the police station, date and time of the incident, whether it was "serious" or not (the criterion used here is unclear), the number of individuals killed or injured, and the types of vehicles involved. We also have some additional information on the location, including whether it occurred on a highway. Unfortunately we do not know whether drunken driving contributed to the accident[5]. We collected daily accident data from August 1st, 2010 through December 31st, 2011, as well as monthly data for January and February 2012.

Summary statistics are presented in panel A of Table 2, with statistics presented for control stations and treatment stations prior to the intervention period. The data, displayed at the police station/month level, shows that police stations have roughly 3.7 accidents per month and 1.5 deaths. Of these, roughly 1/3rd occur at night. For lack of direct measure of accidents caused by drunkenness the number of night accidents and deaths may provide an outcome that varies more with the level of drunkenness than the overall total.

Court records provide a secondary source of administrative data on the project outcomes. For each police station, the court provided a list of all drunken driving cases heard, including the date of the offense, level of the fine, and breathalyzer reading.

Finally, the breathalyzers themselves provide a source of data on the actions of the police at the roadblocks. Each time that a breathalyzer is used, it records the date, time, and alcohol level measured. At the end of the project we copied the memory files off the breathalyzers and can use these to examine

[5] Police in India often arrive at the scene of an accident after at least one of the participants has fled, and thus data on causes of accidents, when it does exist, is often limited to broad categories such as "reckless driving".

police conduct. Unfortunately these records are partial, since many of the breathalyzer memories were wiped when their batteries ran out. Since few, if any, of the police personnel knew of the breathalyzer memory recording feature these erasures are likely to be random.

Survey data

We supplemented the police administrative data with additional data on the implementation of the checkpoints collected by surveyors sent to monitor a set of randomly selected checkpoint locations both on nights when the police were conducting anti-drunken driving checking and on nights when they were not, as well as at locations near the control police stations identified as the best roadblocks sites prior to those stations being assigned to the control group. Surveyors also collected a small amount of data from "alternate routes"—roads identified as routes that could be used by vehicles attempting to avoid police roadblocks.

After arriving at the designated stretch of road, the surveyor counted the number of passing vehicles, categorizing them by type into motorcycles, cars, luxury cars, trucks, autorickshaws, buses, and other. If the police were conducting a checkpoint, the surveyor also counted the number of vehicles stopped, the number that proved to be drunk, and the number of drivers that refused to stop when ordered to do so. Finally, the surveyor recorded the arrival and departure dates of the police from the checkpoint location.

In addition to their usual monitoring of checkpoint and non-checkpoint locations during the intervention, the surveyors also collected data from a special final round of roadblocks held in the week immediately after the main portion of the program had concluded. These checks were designed to evaluate the effect of the program on actual rates of drunken driving, and thus differed from the typical program police roadblocks in two respects: first, they were held once in all stations, regardless of earlier treatment or control assignment. Second, police were asked to set aside their normal practice of stopping only vehicles with a higher probability of containing drunken drivers and conduct checks either randomly or at a fixed interval of cars (e.g. one in ten get stopped). Surveyors were present for all of these final checks, where they recorded both the rate of drunken drivers as well as the extent to which the police carried out roadblocks randomly.

The summary statistics of the data collected by these monitors is displayed in panels B-D of Table 2. Panel B displays the number of vehicles passing by the check points on the average night, using surveyor counts from the locations identified by the control police stations as where they would have carried out the checkpoints. Fewer vehicles pass the second and third checkpoint locations than the first; this is particularly noticeable in the medians. Overall, police stopped 13.1% of passing vehicles, roughly 105 per checkpoint. The majority of these were motorcycles, of which 11.5% or 40 per checkpoint were stopped.

Panels D and E report the number of drunken drivers caught by the police on nights when normal checkpoints were carried out as well as on the final checking night when checks were conducted in a more systematic fashion. On average police caught 1.85 drunk drivers per roadblock, primarily motorcycles. This corresponds to a 2.23% overall drunkenness rate, and a rate of 3.36% for motorcyclists. Car drivers had substantially lower drunkenness rates, perhaps partly due to the fact that many cars in India are driven by professional chauffeurs whose employers would not tolerate drunkenness.

5. Results

We report program results in two formats: first as coefficients from OLS or fixed effect (FE) regressions of accidents and deaths on all program categories, and second as Poisson or fixed effect Poisson regressions of outcomes on specifications inspired by the model implications. Since the linear specifications test for differences in the conditional means of the outcomes over the different intervention categories, they provide a clear and transparent documentation of the effects of the program. The Poisson regressions, while less interpretable under misspecification, have the desirable property that their (transformed) coefficients are in semi-elasticity form, and thus express the percentage change in the outcome variables when the program is or is not present[6].

We begin with a simple summary of the effects of the program on accidents in Table 3. Columns 1-4 report the simple difference between mean accidents and deaths in the treatment and control stations during the 2011 intervention period. All coefficients are estimated with substantial noise, and we find no effect of the program. Columns 5-8 expand the analysis to include the full dataset and include police station and month fixed effects. Here the results are more indicative of an effect of the program: 7 out of 8 coefficients are negative, and there is a significant decrease in the number of accidents, deaths and

[6] Taking logs of dependent variables is not an option in this context due to the high number of zeros in the data. The Poisson specification may also be more appropriate for the count data that constitutes most outcomes of the program.

(at p<10%) night accidents in the 60 day period following the program. This pattern--that results are stronger in the period after the crackdown than during the crackdown--is apparent in many of the program outcomes, and suggests that the project only achieved its full effectiveness towards the end of the actual intervention period. The differences in point estimates between OLS and fixed effects results may be partially due to the slight imbalance in pre-program police station outcomes across treatment groups. Table A1 shows that, despite efforts at stratification, treatment stations have somewhat more total accidents and deaths than controls. Although these differences are not significant, they are of roughly the same magnitude as the program effects we ultimately find in the fixed effects specification, and thus may obfuscate the results in the simple differences.

Finally, columns 9-12 break down the program effects by the team composition and the intervention strategy. Here, results are mixed. Surprise checkpoints tend to perform better when measuring night outcomes, although these differences are hardly significant. Differences between lines and station teams are never significant, although point estimates tend to favor the police station staff. The lack of a significant difference between these two groups is puzzling given the huge differences in their implementation effectiveness documented below; resolving this mystery is a high priority for further analysis.

One of the main theoretical questions motivating this study was the relationship between the frequency of enforcement and the decrease in road accidents and deaths. Table 4 reports results on this relationship based on checkpoint frequency, again using both a simple OLS and fixed effects approaches. Here too the OLS results are noisy and show no significant impacts of the crackdown. The fixed effect results, in columns 5-8, show some effects of the program particularly on deaths in the period after the program (although these are significant only at p<10%). Unfortunately, the coefficients are measured with too much noise to clearly establish the concavity or convexity of the relationship between crime and enforcement. The coefficients indicating checkpoints occurring 3 times per week are always more negative than those for 2 times per week, suggesting that the marginal return to additional roadblocks is positive, but is simply too noisily measured to warrant further inferences.

The police stations in the study were certainly not isolated from each other, and drivers may cross the jurisdictions of several police stations even on short trips. To accommodate the potential spillover effects that crackdowns in one police station might have on the accidents and death rates in neighboring stations, we measured the driving distance between all pairs of police stations in the same or adjacent districts and generated variables indicating the number of other police stations implementing the crackdowns within each 10 kilometer radius. Table 5 reports the results of including these additional controls in the fixed effects specification. We find substantial effects of treatment of nearby stations--an addition treated station at a distance of 20-30 kilometers decreases accidents and deaths by almost as much as the direct effect of conducting the checkpoints, and the results are more precisely estimated. While it may appear counterintuitive that spillovers should be stronger for stations 30 kilometers apart than 10 kilometers, this result can be attributed to the fact that police stations in Rajasthan are rarely less than 30 kilometers apart, and those that do have neighbors in the 10 kilometer range tend to be in urban or peri-urban areas (see distribution of distances in Figure 5). If roads are denser in these areas and travelers take shorter trips, one might expect the spillovers to be commensurately lower.

The checkpoints themselves may have had a direct effect on accidents and deaths on the day on which they were conducted. Drivers might slow down to pass checkpoints and thus avoid speeding accidents, or alternatively, the presence of additional police on the roads might allow them to gather additional accident reports from motorists who might not have gone to the police station to report the incident. We test for this possibility in Table 6 by including controls for whether a checkpoint was held on the day from which the accident data was collected or each of the preceding 3 days. We find no effects at all, suggesting that the effect of the project may be more through deterrence and learning rather than physically removing drunken drivers from the roads.

Dynamic program effects

The significant project effect in the post-intervention period suggests that much of effects on accidents and deaths may be driven due to gradual learning by drivers about levels of police enforcement. If this is the case we expect to see the effects of the project slowly becoming stronger as police teams carry out increasing numbers of checkpoints and then gradually decreasing once the checkpoints cease. We test this phenomena in a set of tables looking at the dynamic program effects on accidents, vehicles passing by the police roadblocks, and on the number of drunken drivers caught.

The first of these, Table 7, displays the effects on accidents and deaths, both overall and at night. Panel A presents a simple specification including a dependent variable for the total number of previous roadblocks carried out at the station. For a given station, this variable is equal to zero prior to the intervention, is increasing during the intervention period, and during the post-period becomes a constant equal to the total number of checkpoints held in that station. Other dependent variables include the number of weeks after the intervention, the weeks after the intervention interacted with the number of previous checkpoints, and police station and month fixed effects. By specifying the regression in this manner, we constrain the effect of the program to increase linearly in the number of checkpoints during the project duration, and then gradually change as the number of weeks after the intervention increases. In particular, the effect of the program is constrained to be continuous across the intervention and post-intervention periods. Unfortunately the results are not statistically significant, with the signs frequently going in unexpected directions. Increasing the number of checkpoints seems to (weakly) decrease accidents, as hoped, but these effects are amplified, not attenuated by the passage of time after the intervention. Even in columns 3 and 4 where the post-project time effect on accidents is positive, this is overwhelmed by the interaction of time with number of previous checkpoints (which are on the order of 20 for the post period).

Panel B of Table 7 estimates a more flexible specification, allowing the program effects to vary discontinuously at the end of the intervention period (as might be the case if the checkpoints have a direct effect as well as contribute to driver learning). As before, coefficients are estimated very imprecisely, making it difficult to draw any firm conclusions. There does appear to be a large level (significant in column 1) shift downwards in accidents during the post-period, not captured by the continuous specification in Panel A. Again, the sum of coefficients on the weeks post intervention and the interaction between weeks and number of previous checkpoints puzzlingly hints that the program effects amplified over time -- although the large standard errors make reliable inference difficult.

While the effects of the program on accidents are noisy, we have much better and more plentiful data on the more direct intervention effects on vehicles passing and drunken drivers. Effects on passing vehicles are presented in Table 8, at both the main check point site (columns 1-3) and at an alternate road identified by the surveyor as the route drivers would take to avoid the checkpoint (column 4). Drivers may have avoided police roadblock by either taking alternate routes and often, as we frequently observed, made u-turns once they had noticed the police checkpoints on the road ahead. A primary concern is that the police decision to carry out the checkpoint may bias OLS estimates of the effect of checkpoints on passing vehicles. For instance, police may only decide to carry out the roadblocks on nights when they know many vehicles will be passing. To account for this in Panel B we instrument variables containing an indicator for whether a roadblock occurred on a given night with whether a roadblock was scheduled for that night. Doing so tends to magnify the coefficients, while their signs generally remain the same.

Focusing on the IV specifications, the results suggest several forces operating simultaneously. Significantly fewer vehicles of all kinds pass by on nights when police are carrying out surprise checkpoints but there is no effect on the number vehicles passing at fixed checkpoints. Over time, as more checkpoints are carried out, the avoidance effect at surprise checkpoints disappears while the effect of fixed checkpoints remains negligible. Simultaneously, however, there appears to be a small but significant negative effect of carrying out checkpoints on passing vehicles on nights when there is no checkpoint. These results suggest that in general drivers learned *not* to avoid the checkpoints, at least on nights of surprise checkpoint. Perhaps many drivers initially feared that the checkpoints were multipurpose and that they would be fined for any violations, but gradually came to know that police were only targeting drunken drivers (recall that only roughly 2.3% of drivers are inebriated). The fact that this effect is primarily in the surprise stations may simply be due to the fact that this learning occurred so quickly in the fixed checkpoint areas that we cannot detect it. The avoidance effects reported in column 4 have signs consistent with drivers taking these routes to escape the roadblocks, but unfortunately the small sample size makes all avoidance estimates insignificant.

The final, and perhaps most direct dynamic effect of the anti-drunken driving intervention is on the number of drunken drivers caught. These results are presented in Table 9; in order to capture the effect of drivers learning rather than police becoming more skilled at carrying out checkpoints these regressions include controls for the type of police team, the number of vehicles stopped, and, in column 4, the number of roadblocks the team had carried out[7]. Because we only have data on drunken drivers from nights on which the police conducted roadblocks during the intervention period, we include district level fixed effects rather than station fixed effects in order to estimate the effects of fixed checkpoints and police lines teams from cross station (within district) variation.

The results show substantial learning effects on the number of drunken drivers caught--with each additional roadblock the number of drunken drivers apprehended falls by .051, or almost 3%. Interestingly, this effect is concentrated in the stations with fixed checkpoints (columns 3, 4), and is much smaller and insignificant in those with surprise checkpoints. This is consistent with a learning model in which the surprise checkpoints prevent drunken drivers from learning the extent or the locations of police roadblocks. Finally, the coefficient on the number of checkpoints performed by a

[7] Because police station teams worked only at one station, the team experience variable and number of checkpoints variable are the same for these teams. Thus the effect of team experience is identified only off the police lines teams who worked at police stations that conducted varying numbers of checkpoints per week.

given team (column 4) are small and insignificant, suggesting that police lines teams did not become more productive at catching drunken drivers as the intervention progressed.

Testing Model Predictions

In a second set of tables we estimate specifications inspired by the results of the simple model of driver behavior in section 2. In these tables we estimate coefficients by Poisson MLE, in order to capture the proportional effect on drunken driving prevalence implied by the model.

Table 10 tests the first of these predictions: that the number of drunken drivers at the lowest cost location r_1, proxied here by the first location chosen by the police to carry out roadblocks, is always lower under fixed than surprise checkpoints. The data supports this prediction; results in columns 1-4 show that the number of drunken drivers caught by the police at the first chosen location is decreasing (often significantly) in the frequency of fixed checkpoints but is not affected by the frequency of checkpoints in the surprise strategy stations. Consistent with the model predictions, these effects appear particularly strong for motorcycles, whose drivers may have the highest travel costs. Columns 5-8 allow the intercept of the relationship between the frequency of checkpoints and number of drunken drivers caught to differ between surprise and fixed checkpoints, a dimension of variation not predicted to matter if we take the model literally. Reassuring, these coefficients are always insignificant and are small for the case of drunken motorcyclists. Predictions on differences in drunks caught between strategies remain robust to the introduction of this additional control.

The second model prediction, that the number of drunken drivers on the alternate routes is always higher for the fixed checkpoint strategies, is more difficult to test because checkpoints were almost never conduction on alternate routes in the fixed checkpoint stations. The sole exception was during the final check period, when all checkpoints were conducted at the second location listed by the station police chief. Here, however, we can only compare the fixed checkpoints with surprise stations that were also conducting their final checks at the second checkpoint location, which was included in the program. Thus the model's prediction remains that we expect fewer drunken drivers in the surprise police stations, but the mechanisms behind this prediction are slightly different.

Table 11 reports the results of these regressions. The results are consistent with the model predictions in terms of the overall difference in levels between surprise and fixed checkpoints: for most levels of treatment frequency we find fewer drunken drivers on the secondary checkpoint locations when the checkpoints are done using the surprise, dispersed model. However, contrary to the model predictions we find that the number of drunken drivers at the second roadblock point is *decreasing* in the frequency of roadblocks at the fixed strategy stations. This is inconsistent with the model's intuition that increasing the probability of punishment on the main road induces more drunken drivers to take alternate routes. This puzzling result is robust to controlling for the type of team that conducted the roadblocks and is particularly strong for motorcycles, suggesting that the model is not capturing some fundamental aspect of drivers' perceptions of the chances of being caught or avoidance behavior.

Police Team Performance

The project's second broad set of results pertains to the difference in implementation effectiveness between the police teams recruited from the district police lines and those from the local police stations. Here the results, presented in Table 12 are clear: the police lines teams perform better on all outcomes. The most important outcomes, whether the checkpoint actually occurred on a night when it was supposed to have been held, is shown in column 1 using data collected by the surveyors observations. Police lines teams were 28.4% more likely to show up for the roadblock at all. Columns 2 and 3 report whether to police began the roadblock by 7:00pm and continued until 10:00pm respectively. Again, police lines teams were 24.7% more likely to arrive on time and 16.8% more likely to continue the roadblock until 10pm. While these results clearly show that the dedicated teams carried out more checkpoints, it is difficult to disentangle the factors driving this difference since both the GPS monitoring and the inherent differences between the teams' characteristics and duties may have contributed.

The performance of the police personnel at the checkpoint site, conditional on a checkpoint occurring, is more informative on the factors driving the difference in performance between the two groups. Once the police teams had arrived at the roadblock site, there was no difference in the ability of senior officers to monitor their behavior; thus differences in performance here reflect differences in either characteristics of the individuals on the teams, or in their working conditions over the course of the rest of the day. These results, shown in columns 4 and 5, also show a large difference in favor of the police lines teams. Police lines teams stopped an average of 8.6% more vehicles per roadblock than staff from the local police stations, an increase of over 50%. Given that lines teams both stayed longer and stopped more vehicles, it is not surprising that they caught 1.8 more drunk drivers per roadblock than station staff, roughly double the amount.

While the superior performance of the lines teams in stopping vehicles at the roadblocks suggests that GPS monitoring is not the only factor in their better roadblock attendance record, it does not rule out that these GPS devices had some effect. Although the design of the experiment does not allow us to examine the GPS effect directly, we do have data on how frequently different districts actually signed in to the tracking website to observe the location of the police lines vehicles (although this data is only available starting in October, 1 month after the beginning of the project). While this usage may be affected by many unobserved factors, including some potentially correlated with lines team performance, the relationship between monitoring and performance may be somewhat instructive, and is presented in the scatterplot in Figure 4. We see very little relationship between the usage of GPS monitoring and the fraction of roadblocks carried out by police lines teams, or the fraction of the time they arrive on time for a roadblock (correlation coefficients are insignificant in both cases). While these relationships cannot be interpreted causally, they are inconsistent with the simplest stories in which districts that monitored their lines teams more induced better performance.

Data from the police teams' breathalyzer units provides another insight into their performance. All breathalyzer devices used during the project kept an internal record of the readings from each time they were used, and at the end of the project we recovered this data. This information has the advantage that it reflects the behavior of the police on all nights, including while they were not monitored by surveyors, although due to equipment failures much of the data is unusable. This missing data is unlikely to bias the results, since almost none off the police knew of the recording feature on the breathalyzers.

Table 13 presents the result on breathalyzer usage during the periods when the police were assigned to carry out roadblocks for the project. Column 1 contains a simple linear probability regression of whether there is any breathalyzer us at all during the checkpoint period, and the remaining columns 2-4 contain an analysis of the intensity of breathalyzer use conditional on any usage at all. Consistent with the results from the surveyors' reports, the breathalyzer data shows much better performance by the police lines teams on all outcomes. On the extensive margin, lines teams use breathalyzers 15% more than station teams, and, conditional on using the breathalyzer at all, they employ it more than 50% more frequently and generate more than 50% more readings indicating drunken drivers. Strikingly, the presence of a project surveyor at the checkpoint site has no effect on police lines performance at all, and only shows an effect on the extensive margin of police station team breathalyzer usage. This evidence, while not conclusive, is consistent with the hypothesis that the monitoring effect is not driving the large differences between police lines and station staff performance.

While it is tempting to conclude based on these results that the Indian police should adopt a policing strategy oriented around dedicated teams to enforce highway laws, an examination of the time trends in differential team performance suggests caution. Figure 3 presents kernel regressions of the probability of attending a scheduled roadblock and percentage of vehicles stopped over the duration of the 2011 intervention. While the lines teams perform better throughout the program, this difference begins to decrease after 1.5-2 months of the program, and by the end of the intervention there is no difference in performance on either outcome between the lines and stations teams. This result strongly resembles that found by Duflo, Banerjee and Glennerster (2010), who present a strikingly similar graph of the differential performance of incentivized government nurses over time. There are several possible explanations for the decline in lines teams results. Perhaps the lines teams' superior performance was due to the novelty of the task, and this gradually wore off over time. Alternatively, perhaps some of the lines teams gradually discovered that their district headquarters were not using the GPS devices regularly, and thus ceased to be motivated by the possibility of rewards for good behavior. Regardless, the substantial decline in the dedicated teams' performance over time remains a point of caution in the policy implications of this program, as well as a potential argument for adopting a crackdown model.

6. Conclusion

This paper remains in draft form, and hence any broad conclusions would be premature. The results show a moderate effect of the anti-drunken driving campaign on deaths and accidents, and changes in the incidence of drunken driving broadly consistent with a simple model of utility of potential drunken drivers. Some results—in particular the substantial spillovers from drunken driving enforcement both across and within police stations—are not well captured by the model and suggest that more complex dynamics are at work.

The results presented in this draft are the main, but not the only outcomes of the drunken driving study. In particular, changes in accidents and drunken driving incidence over time during the course of the program await further analysis. The fact that decreases in accidents and deaths are strongest after the program suggests that these dynamics, driven by delayed adaptations in public and/or police behavior may be of substantial importance. Police graft and corruption is another element of the project for which further analysis might be particularly rewarding. A careful examination of surveyor reports, breathalyzer memory records, and court registers may yield some insight into what fraction of drunken drivers actually pay the official penalty and how many make a side payment to the police.

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Figures

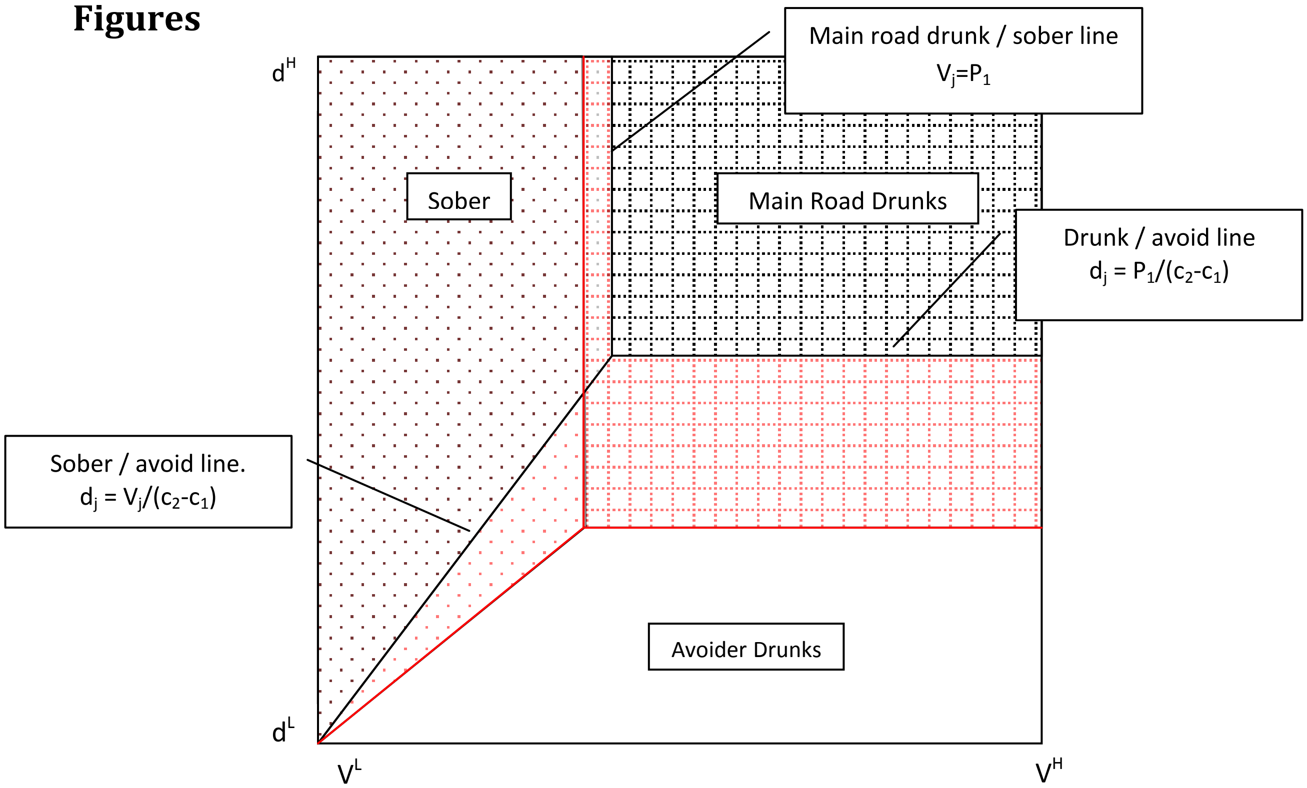

Figure 1: Model typespace and actions

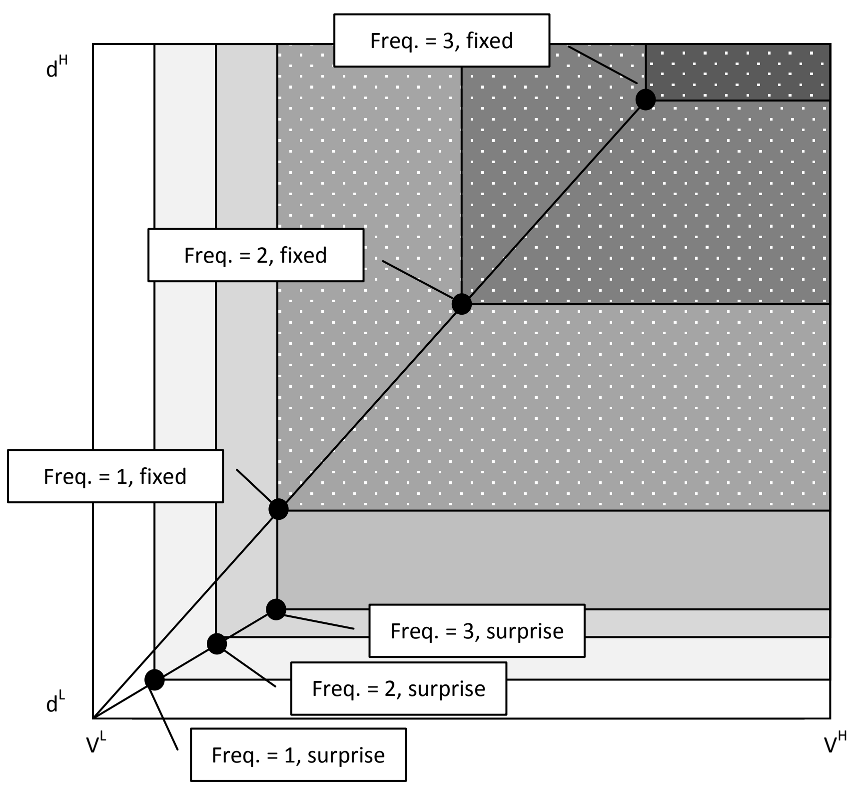

Figure 2: Program values and model typespace

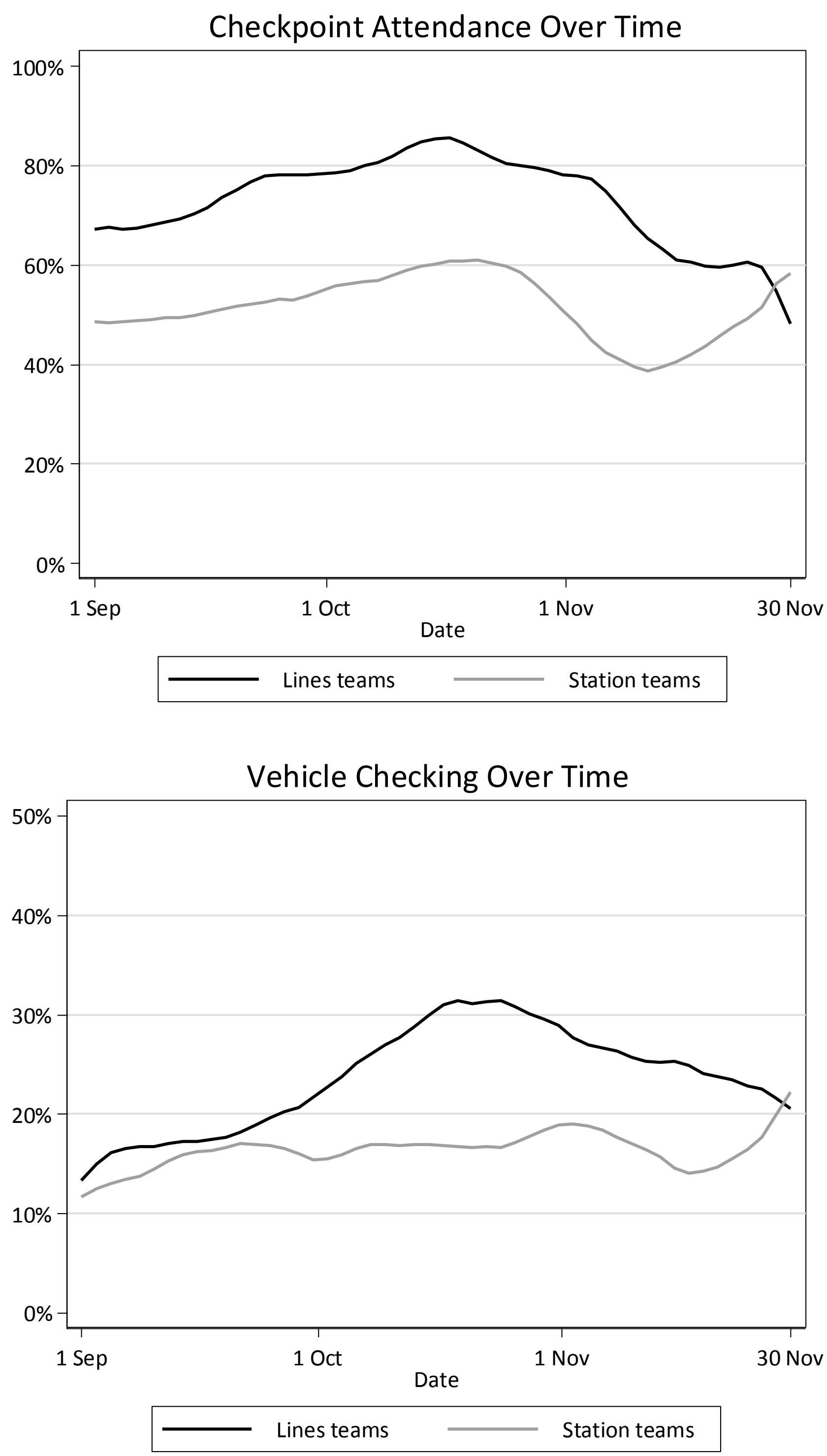

Figure 3: Police lines vs. Police station teams performance

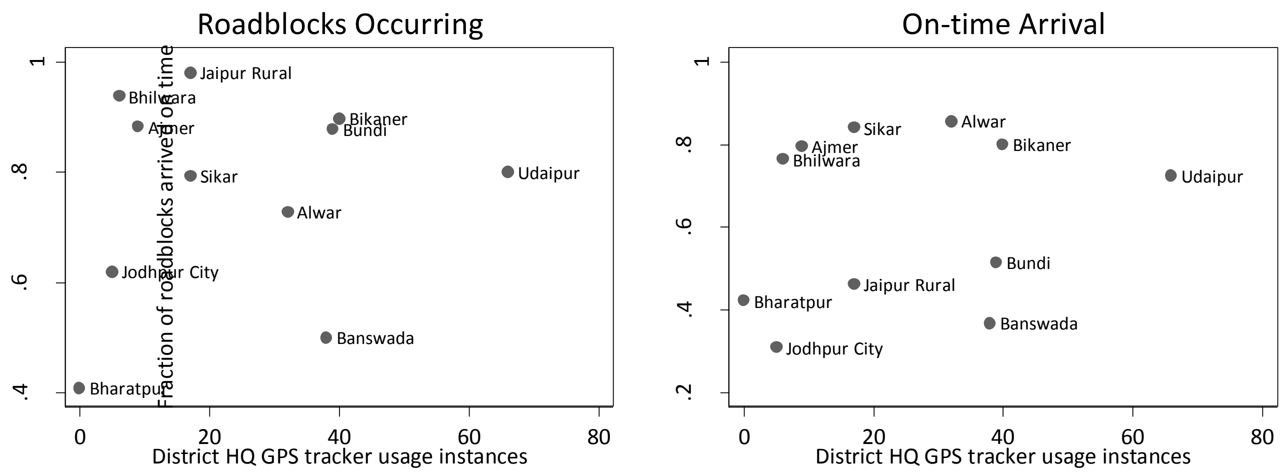

Figure 4: Police lines team performance and district HQ monitoring

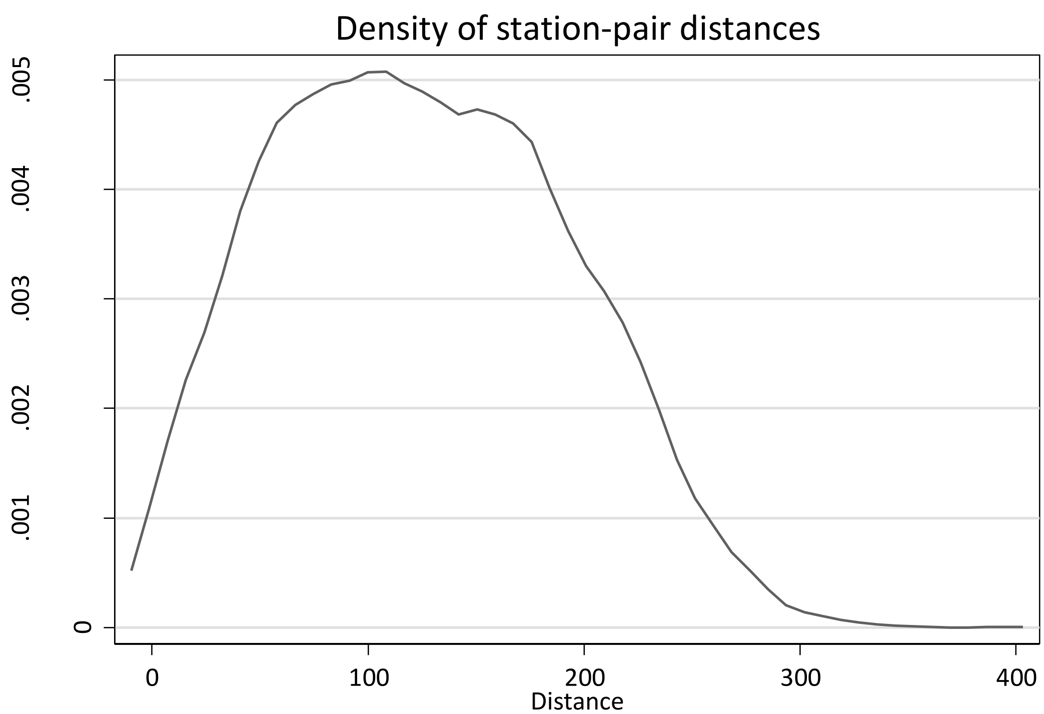

Figure 5: Kernel density of distances between police stations in same or adjacent districts

Tables

Table 1: Police station treatment assignment

Implementation Staff:

A. Sep. 2010 Round

		Police Lines Teams	Police Station Teams
Checkpoint Strategy	Surprise	5 stations @ 2/week	7 stations @ 2/week
	Fixed	6 stations @ 2/week	6 stations @ 2/week

B. Sep.-Nov. 2011 Round

		Police Lines Teams	Police Station Teams
Checkpoint Strategy	Surprise	8 stations @ 1/week 11 stations @ 2/week 10 stations @ 3/week	10 stations @ 1/week 9 stations @ 2/week 12 stations @ 3/week
	Fixed	8 stations @ 1/week 7 stations @ 2/week 9 stations @ 3/week	14 stations @ 1/week 13 stations @ 2/week 11 stations @ 3/week

Table 2: Summary Statistics

	Obs.	Mean	SD	Median	Min.	Max.
A. Police station monthly mean accidents and deaths (Control stations)						
Accidents	2821	3.69	2.93	3	0	19
Deaths	2821	1.51	1.79	1	0	14
Night Accidents	2821	1.10	1.37	1	0	9
Night Deaths	2545	0.54	0.99	0	0	13
B. Total vehicles passing police checkpoint locations in control stations						
Location 1	238	941.02	726.48	672.5	117	4862
Location 2	244	932.66	914.84	612	123	4998
Location 3	256	895.33	888.90	571	38	4743
C. Vehicles stopped by police at checkpoints						
Total	837	105.28	108.26	69	1	1180
Motorcycles	837	39.90	47.04	25	0	357
Cars	837	22.16	35.24	10	0	435
Trucks	837	19.52	35.25	9	0	580
D. Drunk drivers caught by police at checkpoints						
Total	837	1.85	2.36	1	0	21
Motorcycles	837	1.03	1.63	0	0	14
Cars	837	0.20	0.59	0	0	7
Trucks	837	0.23	0.61	0	0	5
E. Percentage found drunk in control police stations at final check						
Total	4988	2.23%	2.18%			
Motorcycles	2202	3.36%	3.25%			
Cars	1383	0.72%	0.72%			
Trucks	571	1.93%	1.89%			
F. Police roadblock attendance						
Roadblock occurred	1580	62.50%	23.45%			
Arrived on time	980	54.54%	24.79%			
Stayed until 10:00pm	980	72.23%	20.06%			

Omitted vehicle categories are vans, jeeps, buses, autorickshaws, and other (mostly tractors). The lower number of night deaths observations is due to the fact that this data is not available for January and February 2012. These months are omitted from the rest of the analysis.

Table 3: Accidents and deaths by strategy

z	Accidents	Deaths	Night accidents	Night deaths	Accidents	Deaths	Night accidents	Night deaths	Accidents	Deaths	Night accidents	Night deaths
	OLS	OLS	OLS	OLS	FE	FE	FE	FE	FE	FE	FE	FE
	1	2	3	4	5	6	7	8	9	10	11	12
Treatment * intervention period	0.006 (0.012)	0.004 (0.006)	-0.001 (0.005)	0.00 (0.003)	0.001 (0.007)	-0.004 (0.004)	-0.003 (0.004)	-0.003 (0.002)				
Treatment * post-intervention period	-0.008 (0.010)	0.002 (0.005)	-0.003 (0.005)	0.001 (0.003)	-0.012* (0.006)	-0.010** (0.005)	-0.006* (0.004)	-0.004 (0.003)				
Post-intervention period	-0.006 (0.007)	0.001 (0.005)	-0.001 (0.004)	-0.001 (0.002)								
Fixed checkpoints * intervention period									-0.002 (0.009)	-0.007 (0.006)	0.002 (0.005)	-0.002 (0.003)
Surprise checkpoints * intervention period									-0.003 (0.008)	-0.006 (0.007)	-0.011*** (0.004)	-0.005 (0.004)
Police lines teams * intervention period									0.009 (0.008)	0.005 (0.006)	0.004 (0.004)	0.001 (0.003)
Fixed checkpoints * post-intervention period									-0.009 (0.009)	-0.013** (0.006)	-0.004 (0.005)	-0.004 (0.003)
Surprise checkpoints * post-intervention period									-0.017 (0.011)	-0.011 (0.008)	-0.010* (0.006)	-0.004 (0.004)
Police lines teams * post-intervention period									0.003 (0.009)	0.006 (0.007)	0.002 (0.005)	-0.001 (0.004)
Constant	0.120*** (0.010)	0.046*** (0.005)	0.036*** (0.004)	0.017*** (0.002)	0.112*** (0.005)	0.046*** (0.004)	0.033*** (0.002)	0.017*** (0.002)	0.112*** (0.005)	0.046*** (0.004)	0.033*** (0.002)	0.017*** (0.002)
Month fixed effects	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	0.117	0.049	0.035	0.016	0.12	0.05	0.036	0.018	0.12	0.05	0.036	0.018
No. of observations	25428	25428	25428	25428	94276	94276	94276	94276	94276	94276	94276	94276

Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 4: Accidents and deaths by checkpoint frequency

	Accidents OLS 1	Deaths OLS 2	Night accidents OLS 3	Night deaths OLS 4	Accidents FE 5	Deaths FE 6	Night accidents FE 7	Night deaths FE 8
1/week *	0.021	0.01	0.003	0.003	0.001	0.001	-0.005	0.001
Intervention period	(0.019)	(0.011)	(0.008)	(0.006)	(0.009)	(0.007)	(0.005)	(0.004)
2/week *	0.004	0.003	0	-0.001	0.004	-0.005	0.001	-0.004
Intervention period	(0.014)	(0.007)	(0.006)	(0.003)	(0.009)	(0.006)	(0.005)	(0.003)
3/week *	-0.006	0	-0.006	-0.002	-0.002	-0.007	-0.006	-0.005*
Intervention period	(0.015)	(0.008)	(0.006)	(0.003)	(0.009)	(0.006)	(0.004)	(0.003)
1/week *	0.009	0.01	0.005	0.005	-0.011	-0.005	-0.004	0.001
post period	(0.015)	(0.008)	(0.008)	(0.005)	(0.012)	(0.008)	(0.005)	(0.004)
2/week *	-0.015	0	-0.006	0	-0.012	-0.011*	-0.005	-0.006
post period	(0.011)	(0.007)	(0.006)	(0.003)	(0.008)	(0.006)	(0.005)	(0.004)
3/week *	-0.011	0.001	-0.003	0	-0.012	-0.012*	-0.009	-0.007*
post period	(0.013)	(0.008)	(0.006)	(0.004)	(0.009)	(0.007)	(0.006)	(0.004)
Post period	-0.007	0.001	-0.002	-0.001				
	(0.007)	(0.005)	(0.003)	(0.002)				
Constant	0.120***	0.046***	0.036***	0.017***	0.112***	0.046***	0.033***	0.017***
	(0.010)	(0.005)	(0.004)	(0.002)	(0.005)	(0.004)	(0.002)	(0.002)
Month fixed effects	No	No	No	No	Yes	Yes	Yes	Yes
Mean of dep. variable	0.117	0.049	0.035	0.016	0.12	0.05	0.036	0.018
N	25428	25428	25428	25428	94276	94276	94276	94276

Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 5: Accidents and deaths with spillovers

	Accidents	Deaths	Night accidents	Night deaths
	FE	FE	FE	FE
	1	2	3	4
Treatment * intervention period	0.001	-0.004	-0.002	-0.003
	(0.007)	(0.004)	(0.004)	(0.002)
Treatment * post-intervention period	-0.011*	-0.009*	-0.005	-0.004
	(0.006)	(0.005)	(0.004)	(0.003)
# treated in 10 kms.* intervention period	0.002	0.001	-0.001	0
	(0.002)	(0.001)	(0.001)	(0.001)
# treated in 20 kms.* intervention period	0.004	-0.001	0.002	-0.001
	(0.003)	(0.002)	(0.002)	(0.001)
# treated in 30 kms.* intervention period	0.001	0.002	-0.004*	0.001
	(0.003)	(0.003)	(0.002)	(0.002)
# treated in 10 kms.* post period	-0.002	0.001	0	0.001
	(0.002)	(0.002)	(0.002)	(0.001)
# treated in 20 kms.* post period	-0.006*	-0.001	-0.003*	-0.001
	(0.004)	(0.003)	(0.002)	(0.001)
# treated in 30 kms.* post period	-0.007**	-0.004	-0.005**	-0.003**
	(0.003)	(0.003)	(0.002)	(0.001)
	0.112***	0.046***	0.033***	0.017***
_cons	(0.005)	(0.004)	(0.002)	(0.002)
Month fixed effects	Yes	Yes	Yes	Yes
Mean of dep. variable	0.12	0.05	0.036	0.018
N	94276	94276	94276	94276

Independent variables indicate the range of distances away from the station in which accidents occurred. E.g. the 20km variable counts all stations between 10-20 km away from the observation station. Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 6: Direct effect of checkpoints on contemporaneous accidents

	Accidents	Deaths	Night accidents	Night deaths
	FE	FE	FE	FE
	1	2	3	4
Treatment * intervention period	-0.003	-0.005	-0.001	-0.002
	(0.008)	(0.006)	(0.004)	(0.003)
Treatment * post-intervention period	-0.012*	-0.010**	-0.006	-0.004
	(0.006)	(0.005)	(0.004)	(0.003)
Checkpoint that day	0.011	0.002	0	-0.001
	(0.009)	(0.006)	(0.004)	(0.004)
Checkpoint 1 day before	0.004	-0.005	0	0
	(0.008)	(0.006)	(0.004)	(0.003)
Checkpoint 2 days before	-0.003	-0.002	-0.003	0
	(0.007)	(0.005)	(0.004)	(0.003)
Checkpoint 3 days before	0.001	0.01	-0.004	-0.003
	(0.007)	(0.006)	(0.004)	(0.003)
Constant	0.112***	0.046***	0.033***	0.017***
	(0.005)	(0.004)	(0.002)	(0.002)
Month fixed effects	Yes	Yes	Yes	Yes
N	94276	94276	94276	94276

Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 7: Dynamic effects on accidents

	Accidents	Deaths	Night Accidents	Night Deaths
	FE-Poisson	FE-Poisson	FE-Poisson	FE-Poisson
	1	2	3	4
		Panel A		
Previous checkpoints	0	-0.005	-0.004	-0.008
	(0.003)	(0.005)	(0.005)	(0.007)
Weeks after intervention	-0.001	-0.014	0.022	0.012
	(0.025)	(0.044)	(0.039)	(0.076)
Wks post intervention * previous checkpoints	-0.002	-0.001	-0.003	-0.003
	(0.002)	(0.002)	(0.003)	(0.004)
		Panel B		
Treatment*intervention	0.048	-0.057	-0.122	-0.321
	(0.075)	(0.159)	(0.146)	(0.255)
Previous checkpoints* intervention period	-0.006	-0.006	-0.005	-0.01
	(0.005)	(0.012)	(0.008)	(0.013)
Week of intervention	0.007	0.008	0.017	0.042
	(0.011)	(0.024)	(0.019)	(0.041)
Treatment*post-intervention period	-0.388**	-0.38	-0.476	-0.682
	(0.163)	(0.335)	(0.343)	(0.601)
Previous checkpoints* post-intervention	0.018**	0.008	0.01	0.006
	(0.007)	(0.013)	(0.016)	(0.020)
Wks post intervention	0.073*	0.053	0.097	0.106
	(0.041)	(0.075)	(0.077)	(0.131)
Wks post intervention * previous checkpoints	-0.006***	-0.004	-0.006	-0.007
	(0.002)	(0.004)	(0.004)	(0.006)
Police station fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Mean of dep. variable	0.12	0.05	0.037	0.019
N	94276	94276	92204	88578

Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 8: Dynamic effects on number of vehicles passing

	All vehicles	Motorcycles & Cars	Trucks	All vehicles - avoidance site
	OLS-FE	OLS-FE	OLS-FE	OLS-FE
	1	2	3	4
Checkpoint occurred	-77.706*	-54.574**	-15.137	123.489
	(42.187)	(24.853)	(12.160)	(289.703)
Checkpoint occurred* fixed location	14.543	10.008	-2.679	41.963
	(54.286)	(29.800)	(25.236)	(415.424)
Number of previous checkpoints (assigned)	-5.191**	-2.318	-1.184	0.75
	(2.477)	(1.493)	(0.889)	(16.110)
Checkpoint occurred* # previous checkpoints	2.531	1.011	0.908	7.419
	(2.874)	(1.801)	(1.029)	(16.000)
Fixed location checkpoint* # previous checkpoints	-4.012	-0.75	-2.089	-13.969
	(3.785)	(2.449)	(1.996)	(20.715)
Constant	792.110***	334.108***	233.231***	1588.109***
	(90.529)	(57.826)	(35.122)	(523.379)
Location rank controls	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
R-squared	0.046	0.047	0.048	0.129
N	2790	2790	2790	386
	Panel B - IV: Checkpoint occurred instrumented with checkpoint assigned			
Checkpoint occurred	-161.498***	-81.620**	-40.062*	1168.693
	(58.346)	(35.336)	(22.844)	(1082.642)
Checkpoint occurred* fixed location	147.837*	57.836	56.696	-1129.874
	(82.511)	(49.218)	(38.436)	(1205.081)
Number of previous checkpoints (assigned)	-6.797**	-3.070*	-1.477	-2.081
	(2.682)	(1.596)	(1.043)	(19.321)
Checkpoint occurred* # previous checkpoints	11.259***	4.511*	3.498**	-73.211
	(4.346)	(2.694)	(1.726)	(78.565)
Fixed location checkpoint* # previous checkpoints	-11.585**	-2.75	-5.786**	77.77
	(5.708)	(3.514)	(2.611)	(85.126)
Location rank controls	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
R-squared	-0.027	-0.023	-0.026	-0.263
N	2790	2790	2790	373

All regressions include controls for whether surveyor counted vehicles passing in one direction or both.

Table 9: Dynamic effects on drunken drivers caught

	Drunks Caught	Drunks Caught	Drunks Caught	Drunks Caught
	OLS-FE	OLS-FE	OLS-FE	OLS-FE
	1	2	3	4
Number of previous checkpoints (assigned)	-0.051***	-0.051***	-0.028	-0.028
	(0.017)	(0.017)	(0.017)	(0.018)
Fixed checkpoint		0.049	0.524	0.524
		(0.237)	(0.344)	(0.345)
Fixed checkpoint* # previous checkpoints			-0.054**	-0.054**
			(0.025)	(0.026)
Checkpoints done by team				-0.001
				(0.009)
Police lines team	1.681***	1.684***	1.671***	1.686***
	(0.254)	(0.256)	(0.248)	(0.384)
Number of vehicles stopped by police	0.004***	0.004***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Location rank controls	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
R-squared	0.057	0.056	0.06	0.206
Mean of dep. variable	1.855	1.855	1.855	1.855
N	836	836	836	836

Table 10: Drunks caught at checkpoint #1

	Total drunks	Drunk motorcyclists	Total drunks	Drunk motorcyclists	Total drunks	Drunk motorcyclists	Total drunks	Drunk motorcyclists
	Poisson	Poisson	FE-Poisson	FE-Poisson	Poisson	Poisson	FE-Poisson	FE-Poisson
	1	2	3	4	5	6	7	8
Fixed checkpoint * frequency	-0.119	-0.250*	-0.166***	-0.275***	-0.16	-0.248	-0.198***	-0.279**
	(0.133)	(0.149)	(0.040)	(0.086)	(0.154)	(0.185)	(0.062)	(0.138)
Surprise checkpoint * frequency	0.078	0.037	0.053	0.02	0.203	0.031	0.153	0.033
	(0.131)	(0.163)	(0.077)	(0.061)	(0.146)	(0.188)	(0.190)	(0.258)
Surprise checkpoint					-0.409	0.019	-0.326	-0.044
					(0.452)	(0.566)	(0.495)	(0.762)
Constant	0.222	-0.468			0.381	-0.477		
	(0.713)	(0.803)			(0.744)	(0.851)		
District FE	No	No	Yes	Yes	No	No	Yes	Yes
N	537	537	537	537	537	537	537	537

Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 11: Drunken drivers caught at final check at checkpoint #2

	Total drunks	Drunk motorcyclists	Total drunks	Drunk motorcyclists	Total drunks	Drunk motorcyclists	Total drunks	Drunk motorcyclists
	Poisson	Poisson	FE-Poisson	FE-Poisson	Poisson	Poisson	FE-Poisson	FE-Poisson
	1	2	3	4	5	6	7	8
Fixed checkpoint * frequency	-0.305***	-0.448**	-0.297**	-0.445*	-0.26	-0.674**	-0.093	-0.578**
	(0.099)	(0.197)	(0.116)	(0.234)	(0.258)	(0.291)	(0.156)	(0.229)
Surprise checkpoint * frequency	0.19	0.064	0.349	0.176	0.156	0.031	0.260**	0.133
	(0.237)	(0.332)	(0.239)	(0.357)	(0.141)	(0.292)	(0.127)	(0.325)
Surprise checkpoint	-1.164***	-1.197*	-1.472***	-1.380*	-1.178**	-1.762	-0.942*	-1.66
	(0.378)	(0.698)	(0.415)	(0.744)	(0.572)	(1.152)	(0.545)	(1.132)
Station team					-1.264***	-0.484	-1.522***	-0.469
					(0.483)	(0.534)	(0.480)	(0.582)
Lines Team					0.394	0.961	-0.087	0.606
					(0.496)	(0.638)	(0.366)	(0.511)
Constant	0.899***	0.533**			0.925***	0.520**		
	(0.219)	(0.245)			(0.198)	(0.224)		
District FE	No	No	Yes	Yes	No	No	Yes	Yes
N	537	537	537	537	537	537	537	537

Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 12: Police lines vs. police station team implementation performance

	Whether roadblock occurred	Whether police arrive on time	Whether police leave on time	Percentage vehicles stopped	Number of drunks caught per roadblock	Total number of drunk drivers in court
	LP-FE	LP-FE	LP-FE	LP-FE	LP-FE	LP-FE
	1	2	3	4	5	6
Police lines Team	0.284***	0.247***	0.168***	8.654***	1.814***	20.696***
	(0.036)	(0.050)	(0.039)	(2.028)	(0.267)	(5.228)
Constant	0.503***	0.345***	0.628***	18.570***	0.950***	14.417***
	(0.039)	(0.056)	(0.046)	(2.436)	(0.235)	(2.207)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.098	0.064	0.038	0.05	0.153	0.242
Mean of dep. variable	0.64	0.551	0.725	19.477	1.853	24.089
N	1282	797	805	837	837	112

Includes controls for stratifying variables: whether station in on a national highway, and total 2008-2010 accidents. Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 13: Breathalyzer use - data from program checkpoint nights only

		Conditional on Breathalyzer used		
	Whether Breathalyzer used	# of Breathalyzer uses	# of positive readings	# of drunks
	1	2	3	4
Police lines team	0.149***	3.745***	1.725***	1.268***
	(0.023)	(0.639)	(0.299)	(0.277)
Fixed checkpoint location	0.006	0.547	0.253	0.077
	(0.023)	(0.674)	(0.317)	(0.285)
Police station team * surveyor present	0.063***	0.591	0.122	-0.095
	(0.022)	(0.438)	(0.246)	(0.218)
Police lines team * surveyor present	0.007	0.251	0.233	0.345
	(0.025)	(0.419)	(0.300)	(0.266)
District fixed effects	Yes	Yes	Yes	Yes
Location rank controls	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
R-squared	0.017	0.147	0.104	0.097
Mean of dep. variable	0.387	7.147	3.142	2.538
N	2858	1105	1105	1105

Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Appendix:

Table A1: Balance Check

	Mean of program group:					
	Control	Lines teams	Station teams	Fixed roadblocks	Surprise Roadblocks	Frequency
Outcome:	1	2	3	4	5	6
Accidents	0.12	0.129	0.128	0.127	0.13	0.053
	(0.008)	(0.010)	(0.010)	(0.010)	(0.010)	(0.003)
Deaths	0.046	0.052	0.057	0.054	0.056	0.023
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.002)
Night accidents	0.035	0.037	0.035	0.033	0.039	0.015
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.001)
Night accidents	0.017	0.019	0.017	0.015	0.021	0.008
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)

Standard errors in parentheses clustered at the police station level. *** significant at 1%, ** significant at 5%, * significant at 10%.