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THE REAL STATE: INSIDE THE CONGO'S TRAFFIC POLICE AGENCY

Raúl Sanchez de la Sierra
Kristof Titeca
Haoyang (Stan) Xie
Albert Malukisa Nkuku
Aimable Amani Lameke

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ABSTRACT

This paper provides insight into a corruption scheme in Kinshasa's traffic police agency. First, various data collection branches show that the agency's revenue is five times that from fines and is derived from a coalition of traffic police officials, their managers, and judicial police officers scheming to extort drivers. Second, the analysis of an experiment suggests that the scheme subverts service. Third, the scheme appears to be a rational response to the context but its logic is widespread. The findings suggest that coalitions of officials, while being socially costly, can yield large illicit revenue, challenging the notion of state weakness.

Raúl Sanchez de la Sierra
Harris School of Public Policy
University of Chicago
1307 E. 60th Street
Chicago, IL 60637
and NBER
raul@uchicago.edu

Kristof Titeca
University of Antwerp
Institute of Development Policy
Prinsstraat 13
2000 Antwerpen
Belgium
kristof.titeca@uantwerp.be

Haoyang (Stan) Xie
University of Chicago
Becker Friedman Institute
5757 S University Ave
Chicago, IL 60637
stanxie@uchicago.edu

Albert Malukisa Nkuku
Universite Catholique du Congo
Kinshasa
Democratic Republic of the Congo
albert.malukisa@ucc.ac.cd

Aimable Amani Lameke
Marakuja Kivu Research
Bukavu, South Kivu
Democratic Republic of the Congo
amanilamek@gmail.com

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A study documentation is available at <https://www.dropbox.com/s/vn26td27zyu2leg/Study-Documentation.pdf?dl=0>

1. Introduction

A large literature in economics has examined the causes and consequences of corruption. Much of that literature posits corruption as the problem of an individual state official with discretion to use office for personal gain. Their incentives can be misaligned with the functions of the state and thus be costly for society (Shleifer and Vishny, 1993, Bardhan, 1997, Olken and Pande, 2012).

However, state officials are part of state organizations, hence rarely act in isolation. Indeed, managers often know about corruption by their agents and even take part in it; sometimes, their involvement might enable schemes that would otherwise be infeasible by individual officers. When officials form coalitions with their own managers, corruption could become invisible to the naked eye, and both its revenue and social cost could be much larger. In a seminal study, Wade (1982) describes one of such schemes in irrigation canals in India and suggests that the fact that corruption is a scheme may further exacerbate its social cost. Yet, research about these schemes is based on that one remarkable but qualitative study. The reason for this gap is obvious:

Given the extreme sensitivities at stake . . . one cannot work towards an understanding of the phenomena discussed here by the familiar methods of the random sample, the formal interview, and the structured questionnaire. One has to use, rather, more informal, more anthropological means. (Wade, 1982), p. 291

Attempting to fill this gap, this paper reports on a study that builds upon trust relationships developed during three years of qualitative fieldwork inside one specific agency—the traffic police commissariat of Kinshasa, Democratic Republic of the Congo (DRC). Drawing on these relationships, we experimentally varied in 2015 the managers’ corruption schemes with their agents and designed multiple independent data-gathering activities. Complementing the information gathered through these activities with three additional field visits and nine years of qualitative interviews thereafter, we detail how the corruption scheme operates in the commissariat, *quantify* the magnitude and nature of its profits, and *estimate* its effect on public service.

Kinshasa’s traffic police commissariat was well-suited to answer those questions. Like many traffic police agencies worldwide, officially, the dual mandate of the agency was to manage traffic and to issue charges in order to deter drivers from violating the traffic code. Police commanders (henceforth, *managers*), each posted at one of Kinshasa’s 29 traffic police stations, deployed officers (henceforth, *agents*) in teams to the intersections under their jurisdiction. The agents’ mandate was to manage traffic and to escort drivers who violated the code to the stations,

alongside producing allegations, so that the station officers could charge the drivers. Unofficially, agents were regularly seen taking bribes from drivers in the street. In the backdrop of this, managers purportedly asked the agents to escort drivers daily to the station (Baaz and Olsson, 2011), where station officers purportedly took bribes from the drivers (station bribes) in exchange for not charging the driver for alleged infractions that were sometimes reportedly arbitrary (i.e., extortion). Relationships built over three years provided an opportunity to analyze this scheme.

The first key input into our analysis is a randomized encouragement, designed in 2015 during 30 days inside one of the two battalions of Kinshasa commissariat, including 7 of the battalion's 14 stations, as well as Kinshasa's central station, thus totalling 8 stations (out of 29 in the city), 8 station managers, 16 JPOs, 16 fine collection agents (henceforth, *FCAs*), and 15 teams of agents, consisting of 63 agents. We randomly assigned, independently for each team, 15 of the team's 30 days to an encouragement given to the corresponding team's manager for the team's quota not to surpass a cap that day (henceforth, *quota reduction encouragement*). Each cap was half of the team's average quota level. The managers were compensated for the loss due to the reduction and knew that some days and teams would sometimes receive a quota cap encouragement, but they did not know when nor that its effect in the street was measured. Accounting for festivities and attrition, the sample (henceforth, *experiment sample*) is an imbalanced panel of 337 team-days.

The second key input into our analysis is a dataset on the universe of transactions in the experiment sample and the preceding day (henceforth, *Day Zero*). To construct these data, we designed five *independent* branches after collaborating with police colonels, station managers, JPOs, the Fine Collection Agency (DGRAD), the Transportation Division, National Drivers' Association members, and civilians. Overall, 160 individuals took a direct part in data collection activities. These branches relied on direct observation, self-reported data, and administrative data. We ensured that none of the individuals whose publicly observable behavior was recorded by a third party knew that their behavior was being recorded, with meticulous tailoring to community norms and ethics (see [Ethics Appendix](#)). The data from this endeavor include records of (at least) 1,255 vehicles inside the stations from two independent sources and verified by administrative records, 13,793 unique police agent/driver interactions in the street, 6,399 records of 2,997 hourly traffic outcomes, and 337 team-day vehicle quotas constructed from seven sources.

Our first result is descriptive. Using the union of Day Zero and the experiment sample's team-days not assigned to encouragement (henceforth, *descriptive sample*), and verifying the validity of this sample for descriptive analysis, we document the following three facts about the scheme.

Fact 1. The quota scheme generated the battalion's *real*, if mostly unofficial, revenue. Overall, driven by the station bribes, the real revenue was 5.2 times larger than the revenue from fines.

Fact 2. At the stations, station officials routinely took station bribes from the escorted drivers, seemingly whether or not the driver had made an infraction—i.e., *extortion*. Station bribes were determined through an informal negotiation between the escorted driver and the station officials, in exchange for the station officials not to charge the driver of an infraction; alleged infractions were often (67% of times) not verifiable by a third party (henceforth, *unverifiable*). Drivers who were unable to call a high-ranked state official to advocate for them during this negotiation (commonly called a “*protector*”) paid 2.2 times more in station bribes than those who could.

Fact 3. Negotiation over station bribes was facilitated by the agents' apparent ability to arbitrarily escort drivers to the station using force and make discretionary allegations of infractions. Qualitative notes indicate that agents often brought “fantasy” allegations to the station. Quantitatively, in 82% of pull-overs, observers reported that no infraction was visible in the street. In those, disputes or escapes were much more likely than those in which they reported one.

Our second result is that the quota scheme undermines public service. We use the experiment to analyze the effect of the quota on both of its mandates. *In the first step, we analyze traffic outcomes.* We find that the quota accounts for a large share of accidents and traffic jams at the intersections. This effect is partly (but not entirely) channeled by agent absenteeism induced by the effort to escort drivers. *In the second step, we analyze deterrence.* We provide evidence suggesting that the quota waters down the benefits faced by the drivers of respecting the code. Guided by a simple multitasking framework with a manager having the power to issue wrongful charges, we examine two testable implications for whether the quota improves or undermines deterrence. First, if the power to issue wrongful charges on escorted drivers is large—i.e., *extortion*, a necessary but not sufficient condition for the quota to undermine deterrence—the quota should induce agents to escort drivers who, irrespective of whether they committed an infraction, can be predicted to yield a large station bribe. Consistent with *extortion*, we find that the quota only increases the escorting of drivers without a protector and of those whose predicted station bribe, based on their characteristics, is high—against those with a high predicted fine. Second, the quota undermines the incentives of drivers to comply with the code if, and only if, it produces more false than true allegations. We find that the quota leads to more escorts of drivers with unverifiable alleged infractions and pull-overs of drivers in which the driver is the least likely to have made a true infraction. Overall, this suggests that the quota weakens both traffic management *and* deterrence.

Our third result is that the quota scheme can be rationalized as an equilibrium corruption contract in this setting. First, bargaining between the agents and the drivers to avoid costly escorting often breaks down because some drivers are liquidity constrained, are often agents for another person, and because of mutual *spite* ensuing often tense pull-over interactions; this explains why the quota does not unravel in equilibrium. Second, the level of the quota does not affect fine revenue, suggesting it is about corruption. Third, reducing the quota reduces total corruption revenue, suggesting that it might be the first best for the manager-agent pair.

We then place these results in perspective. First, we show, based on our qualitative interviews that while a scheme has existed since the 1980's, its specific form as a vehicle quota developed when local stations were created in 2010, became entrenched after a crack-down on street-level bribes, and remained thereafter because managers found it more profitable than asking for a fee. Second, describing similar schemes across the world, we document that while the way in which a scheme is organized is context-specific, its economic logic appears to be quite general.

These findings make three contributions. First, they document the role of manager-agent schemes in corruption. Our study is not the first to measure corruption. Indeed, a rich literature in economics has developed ingenious ways to measure corruption's prevalence, causes, and consequences (Olken and Pande, 2012, Banerjee, Hanna, and Mullainathan, 2013).¹ Our study does not introduce the notion that managers may also be corruptible. Indeed, it is a known problem in a large literature on the economic theory of organizations (Tirole, 1986, Dixit, 2012). Our study is also not the first to *empirically* document manager participation in corruption: in a remarkable qualitative study, Wade (1982) documents a sophisticated system in which the engineers in a canal irrigation agency redistributed revenue to superior officers and politicians, showing that corruption is connected through the organization; Mutahi, Micheni, and Lake (2022) and Baaz and Olsson (2011) provide qualitative evidence of similar practices in the police in Kenya and in Kinshasa. By leveraging trust relations to design an experiment on a scheme itself, our study presents the first causal evidence of the effect of corruption schemes; by scaling up methods from qualitative research into multiple independent data collection systems, it offers the

¹Through these techniques, officials in Uganda were shown to price discriminate in the setting of bribes paid by firms (Svensson, 2003); state officials in India were shown to misallocate driving licenses (Bertrand, Djankov, Hanna, and Mullainathan, 2007); state officials along the roads in Indonesia were shown to create sophisticated pricing schemes for their bribes designed to extract the drivers' willingness to pay, leading to sub-optimal incentives (Olken and Barron, 2009); and coercive bribe-taking in South African ports was shown to costly trade diversion (Sequeira and Djankov, 2014). Other studies collected prices paid for services as a measure of corruption (Banerjee, Deaton, and Duflo, 2004), which show that bribes induce misallocation of health services. Another ingenious strategy has been cross-checking the reports of a particular outcome with an independently collected measure. Reinikka and Svensson (2005), Fisman and Wei (2004), Olken (2007) provide remarkable examples of the promise of this approach.

first dataset on *one* corruption scheme. Our findings' contribution is that manager schemes can add huge profits and *distort* corruption's social costs beyond corruption without such schemes.

Second, these findings document extortion by state officials. Our study is not the first to analyze extortion. There is a rich literature in economic theory on the optimal incentives for state officials in the presence of bribery and extortion (e.g., Polinsky and Shavell (2001), Khalil, Lawarrée, and Yun (2010), Angelucci and Russo (2016)). A consensus is that extortion is costlier than bribery because it disincentivizes citizen compliance. Our study is also not the first to document extortion: Olken and Barron (2009) document the military on Indonesian roads carrying guns and asking for a bribe, suggesting they have, and use, the power to extort drivers. Our study's contribution is to present evidence characteristic of extortion, based on the types of vehicles stopped and the veracity of allegations, as well as the role of the managers in extortion.

Third, a growing literature in economics has been concerned with state effectiveness (e.g., Weigel (2020), Bergeron, Tourek, and Weigel (2021)). Much of that literature has examined the engineering of the state's official organization to promote its official prerogatives, such as taxation. Our findings provide evidence of an agency's revenue that is five times its official revenue. Regardless of whether it is illegal, this constitutes real capacity to generate revenue. This discrepancy between the official and the *real* state functions resonates with a literature in anthropology, which argues that "*real governance*" is central to what states are (Migdal and Schlichte, 2005, Titeca and De Herdt, 2011, Lund, 2006, Olivier De Sardan and Herdt, 2015).

2. A Brief Description of Kinshasa's Commissariat of the DRC's Traffic Police Agency

Aiming to contain congestion and accidents, the traffic police agency's mandate is to manage traffic flows and to charge traffic code violators. Its goal is valuable: the DRC's road traffic death rate was among the world's ten highest in 2022 (World Health Organization, 2022). In 2015, the traffic police commissariat of Kinshasa, a city of 17.2 million, was divided into two battalions, each with 14 stations, alongside Kinshasa's central station. Each station was run by a commander (henceforth, the *manager*), assisted by 10 officers in the street (henceforth, *agents*) and two Judicial Police Officers in the station (henceforth, *JPO*), who worked for the Office of Public Prosecution.

Officially, the manager had the power to authorize his agents to work in teams at street intersections (henceforth, *intersections*). One of the agents, the team leader, monitored the team for the manager. The agents' official power mirrored the agency's two mandates. For traffic

management, they could order drivers where to go; refusing an order was a code violation (in French, “*refus d’obtempérer*”). For charging violations, they could escort violators to the station.² The station was much like a tribunal: the JPOs acted as a judge—i.e., they had the power and discretion to charge drivers; the driver as the accused; the agent as the witness/prosecutor—i.e., they had the power to make reports of alleged infractions. The FCAs would issue a fine collection note for charged drivers to pay at the bank; the receipt thereof would entitle the driver to retrieve the vehicle. The agents and station officers made 69 and 100 USD monthly, respectively.

Unofficially, the managers were said to request their agents to bring a previously agreed daily target of vehicles to their station (the vehicle *quota scheme*). At the station, where JPOs were often idle waiting for drivers, drivers purportedly paid *station bribes* of two types. First, they reportedly paid the JPOs a bribe to avoid being charged (henceforth, *charge negotiation bribe*). Since the JPOs threatened to charge the driver based on an alleged infraction produced by the agent, station bribes amounted to collusion between the JPOs and the drivers—bargaining to avoid a charge in the presence of evidence of infraction. However, the allegations made by the agents could reportedly be arbitrarily false; thus, under the cover of collusion between the driver and the JPO, station bribes could sometimes amount to the station officials colluding with the agents in order to extort drivers. Second, the drivers also had to pay an unofficial pound fee to recover their vehicle (“*frais de fourrière*” in French, henceforth, *car pound bribe*), which was often less significant than charge negotiation bribes. JPOs reportedly shared station bribes with the managers, who then purportedly paid higher-ups in the agency’s hierarchy. Upon meeting their quota, the agents focused on collecting informal toll fees (the *salute of the cola nut*, “*mbote ya likasu*,” in Lingala) and occasional voluntary contributions from drivers and managed traffic; rarely, they made unofficial payments to their managers (the “*retrocession*” in French). The other actor in the stations, the FCA, had little discretion. While fine payments at the bank were official from the perspective of the driver, it is possible that some may not have reached government coffers. However, prior to the study, we did not find anecdotes supporting that FCAs or bank agents took bribes.

This scheme relied on unofficial sources of power but also on power officially bestowed upon the officials. First, as a pillar of the vehicle quota, managers used their officially bestowed power over the agents’ jackets, which were required for the agents to work in the street, to threaten to suspend them or to deploy them to places where they would make little bribe income for themselves (so-called *sterile intersections*). Also, managers’ kickbacks to higher-ups purportedly

²Figure A1, Panels A-D, shows the hierarchy, the stations, a map of Kinshasa, and a roundabout, respectively.

cultivated an “*umbrella*” that, according to our interviews, allowed them to “*better exploit their agents*.” A manager without an *umbrella* trying to impose a quota on his agents could risk being accused by his agents to higher-ups and thus risked official and unofficial sanctions. Second, to obtain station bribes from the drivers, the JPOs used the official power bestowed upon them by the Office of Public Prosecution, giving them the discretion to charge drivers. Informal negotiation between the JPO and the driver, who often called someone (the “*protectors*”) to talk to the JPO on their behalf, was reported to be central in the determination of the station bribes; this purportedly could include intimidation. Third, the agents used the power of discretion officially bestowed upon them to pull-over and escort drivers to the station; they were sometimes reported to have used that to produce false allegations in service of the JPO’s negotiation with the driver at the station; in that case drivers called the interaction with the agents the *tracasserie* and perceived it as illegitimate (Baaz and Olsson, 2011). The agents used various tactics, in some cases psychological or physical intimidation, coercion, humor, or violence (e.g., Figure A1, Panel E). It was common to see agents pinch tires, enter vehicles, confiscate keys, licenses, or radios:

Officers brutalize us, tear out our license plates. Too many *tracasseries*. After having seen our documents, [they] invent imaginary infractions. (Radiookapi, 2021)

Attempts to escort a driver did not always result in an escort to the station: indeed, the driver could escape or, in rare cases, the agents agreed to let the driver go, foregoing being escorted in exchange for a bribe paid to the agent in the street, called the “*amende*” in French.

The officials above the managers considered the scheme to be a self-financing tool for the police and were qualitatively described as tacitly encouraging it, as it purportedly allowed them to pocket the funds that were originally justified to higher levels to cover the stations’ costs; furthermore, it purportedly generated kickbacks for them too.³ On the other hand, it was also perceived to undermine service. First, since agents purportedly could fabricate allegations, few drivers in Kinshasa met basic requirements, investing instead in creating connections with powerful officials.⁴ As a result, the scheme was perceived to reduce safety. Second, in a context with no traffic lights, *tracasseries* purportedly distracted the officers from managing traffic, leading to disorder. The rest of this paper analyzes this scheme and its effect on public service.

³The managers purportedly drew on station bribes revenue to cover the gas expenses (Nkuku, 2022).

⁴The following encounter, described by various observers, illustrates the importance of protection relationships: “The agent and a soldier who owned a motorbike quarreled over the motorbike. The soldier said, ‘tomorrow you’ll see what I do; I will come back!’ The soldier came today with a jeep, heavily armed. They arrested the agent. They threw him in the jeep in front of everyone! The soldiers left with the agent, and they are going to beat him up.”

3. Experiment Design: Encouragement for the Manager to Reduce the Quota

The experiment was designed in 2015 in one of the two battalions of Kinshasa's commissariat, covering 7 of its 14 stations as well as Kinshasa's central station. This thus covered the staff of 8 police stations. Such staff was composed of 8 managers, 16 JPOs, 16 FCAs, and 15 teams of agents permanently posted to 15 distinct intersections, totaling 63 agents (13 teams had 4 agents, one of 5, and one of 6). We now describe the experiment design.

A. *The Encouragement to Reduce a Team's Quota for a Day*

The experiment was a *clustered randomized encouragement design*, where an encouragement was independently given at the level of team-day clusters to temporarily reduce the quota for one day. Specifically, the encouragement at a given team-day involved a previously vetted member of the research team asking the team's respective manager, on a given team day, to request no more than a capped number of vehicles from that team that day to the station through the quota.

The design leveraged the following features of the context. First, while quotas varied across team-days, *each team's* modal quota was known to the participants (henceforth, *natural quota*). The cap was always half the team's natural quota. We obtained the natural quotas in 6 months of qualitative fieldwork prior to the study; the encouragement was worded as not requesting more than [X] vehicles that day from this team, where "X" had previously been calculated to be half the natural quota. Second, the managers communicated the quota to each team in a routine early morning phone call. Thus, the research team called the team's manager each day *prior to this call* and communicated which of his teams had been selected for the cap that day alongside the cap of each. We reminded managers, including those for whom no team had been selected that day, that teams not assigned to the encouragement could have any quota. Third, each team was permanently assigned an intersection; thus, we expected the encouragement to affect the outcomes of that intersection. Fourth, the quota was the main source of manager income, hence managers were compensated for the estimated loss in income caused by the caps.

Implementation. First, the managers were informed in private that, for approximately one month, for some days, the research team would ask them not to surpass a quota of some teams and that they would be compensated for the loss. The managers did not know how frequently each team would be selected nor what days were selected. Second, the authors only informed the research team of the daily cap assignments at the start of each day. Third, as it

was their main source of revenue, we compensated the managers for the loss in their revenue induced by our reduction. To calculate the compensation, we gathered the average manager bribe revenue per vehicle in preparatory fieldwork *before* informing managers about a reduction. Fourth, to avoid influencing operations, those who administered the compensation (henceforth, *encouragement team*) were from police networks, with access inside the stations authorized by the managers; they compensated the managers privately early in the morning. Fifth, the managers were informed that if they took the compensation but failed to reduce the quota, they would forego participation in the project. Finally, an auditor, flown from another province to prevent collusion with the encouragement team, verified daily whether the managers had received the assigned compensation. Sixth, while weekday, month, calendar day, assignment lag, pre-study station bribe income, and number of agents per team are balanced across assignment status in the experiment sample, there is a mild imbalance in the encouragement assignment across calendar days, which we account for in the analysis of the experiment (Table A8).

Ethics. Compensating the managers for their quotas' reductions can raise legitimate ethical concerns: this cash may feed into a corrupt system and hence contribute to it. Key features of the context shed light on this concern. First, the quota is a corruption scheme. Thus, the encouragement amounts to reducing corruption by compensating the officials benefiting from corruption for the loss of their main source of income it induces. As such, it extends existing anti-corruption interventions aiming to examine whether financial incentives can reduce corruption, targeting the managers instead and further strengthening the incentive by conditioning it on reducing corruption. Second, the compensated encouragement, obtained legally, substitutes the illicit manager income obtained through extortion of drivers for questionable infractions and based on mutual threats. It remains possible, however, that engaging with commanders on an existing quota level amounted to a recognition of the legitimacy of the system. However, from the perspective of the scheme's participants, the approval of foreign researchers or of students is not relevant to determining whether the scheme is legitimate. Since the 1980s, a huge number of actors have formed a part of, benefited from, and been aware of the scheme. Their participation and the reception of society at large, more than the view of foreign researchers with a low budget, determines whether they perceive it to be legitimate. Ascribing the power to legitimize the system to these researchers, who are not working with the government, would imply misrepresenting the system, its relation to the social context, and the researchers' relevance to these actors. The [Ethics Appendix](#) discusses the ethics of this study comprehensively.

B. From Random Assignment in the Randomization Sample to the Experiment Sample

The randomization sample is 600 team-days, spanned by 15 teams over 50 days. Independently for each of the 15 teams, we randomly assigned the encouragement to 25 out of the team’s 50 days in the randomization sample (the randomization clusters); thus, the probability of assignment was equal across teams. This ensured that 300 team-days were assigned to encouragement, and 300 to no encouragement. The randomization sample was chosen under uncertainty over the budget; thus, the stopping rule was determined by budgetary constraints. Thus, to further reduce the likelihood of an imbalance, randomization was stratified within Team $\times$ Day 1-25 vs. Team $\times$ Day 26-50 (the 30 randomization strata).

This stopping rule pinned the end of the experiment on day 31. The proper subset of the randomization sample in which we aimed to conduct the experiment (henceforth, *target experiment sample*) was thus constituted of 465 team-days, which preserved their random assignment: 241 were assigned to the encouragement, and 224 were not. Then, in the target experiment sample, closure due to festivities and the removal of two teams for precaution led to the loss of 90 and 38 team-days, respectively (44 and 17 of which were assigned to the encouragement, respectively), resulting in a sample of 337 team-days (the *experiment sample*).⁵ Figure 1 presents a Consort diagram (Schulz, Altman, and Moher, 2010) of the experiment design.⁶

4. Data Collection and Data Construction

In this section, we describe the sample, data collection, and data construction.

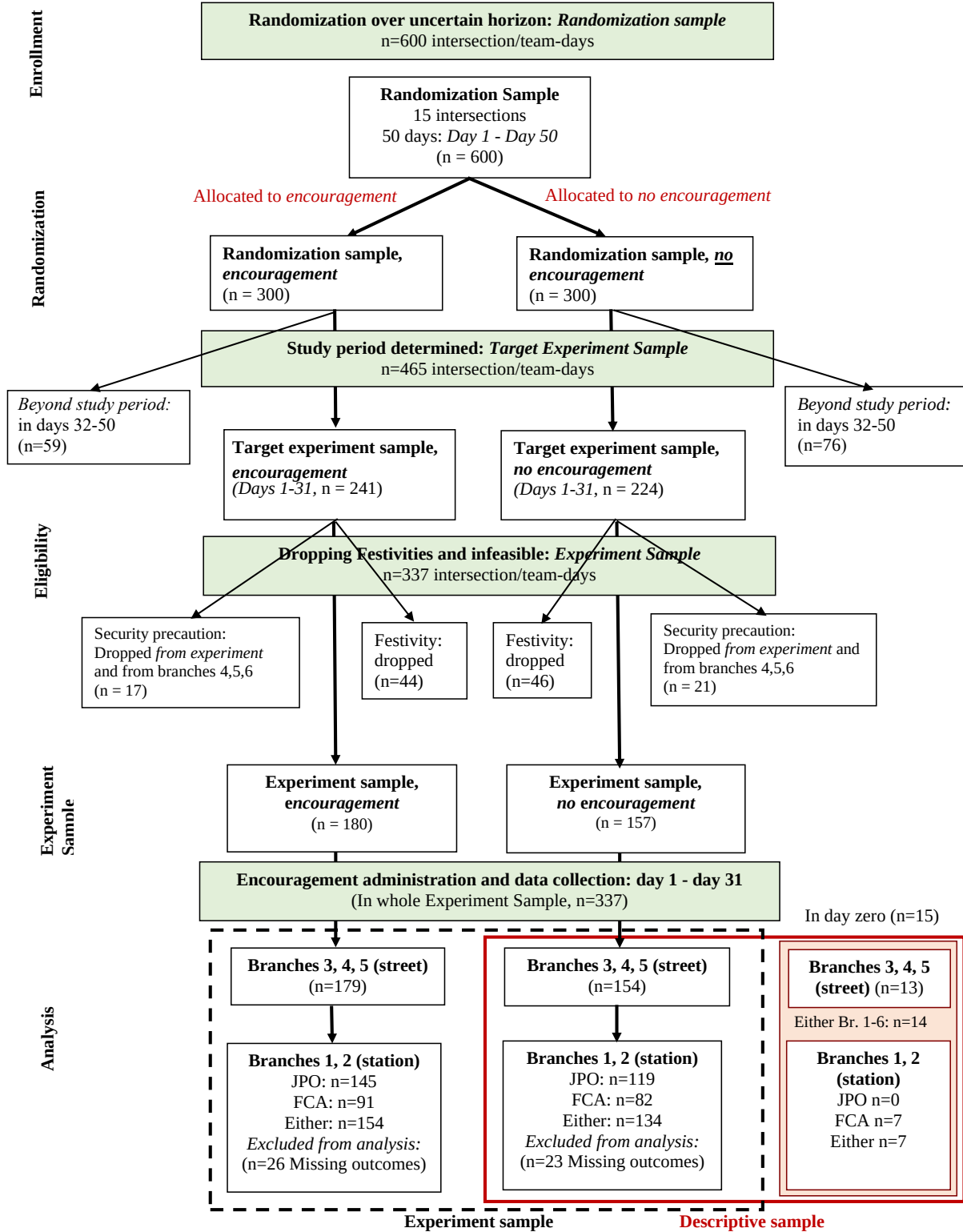
A. From the Experiment Sample to the Data Collection Sample

We designed five data collection branches inside the eight stations and 15 intersections (with 63 unique agents) of the target experiment sample to cover the 30 days of the *target experiment sample* and Day Zero. As described in Section 3.B, the *target experiment sample* was afflicted by festivities, attrition, and replacements; those affected the data collection activities in this sample,

⁵First, there were five Sundays and one festivity, leading to the loss of 6 days for each of the 15 non-dropped teams—i.e., 90 team-days. Second, in each of days 3, 5 and 6, we replaced one team, leading to no loss; the teams had been reshuffled; on day 8, we dropped two teams, leading to the loss of 38 (non-festivity) team-days; we had learned that the agents were tense and feared spying by the general (remnants of the “Kanyama psychosis” see Section 8.A).

⁶Figures A2, and A3 present the assignment in a representation of the experiment sample and in a map. Table A1 Panel A reports the number of teams in the experiment sample in each station-day, and the experiment sample size. A randomized trial reporting checklist is provided in [Study Documentation](#), Section 1.

Figure 1: Consort Diagram—Assignment to the Encouragement and Samples



Notes: This figure presents the Consort diagram (Schulz, Altman, and Moher, 2010) representing the assignment to the encouragement and the samples. We use Consort reporting guidelines through the paper (Schulz, Altman, and Moher, 2010). We report the checklist for randomized trial in [Study Documentation](#), Section 1.

too. Furthermore, on Day Zero, some branches could only cover 14 teams. Thus, the branches were implemented in the 337 team-days of the *experiment sample* and the 14 teams on Day Zero. This produced an imbalanced panel of $337+14=351$ team-days (henceforth, *data collection sample*), composed of eight stations and their eight managers, 16 JPOs, 16 FCAs, and 63 agents (and 12 replacements) in 15 intersections (and three replacements), shown in Table A1, Column 1.

B. The Five Data Collection Branches

Branch 1: Data Reported by Judicial Police Officers inside the Eight Stations

Context: The JPO had firsthand knowledge of the intersection of origin, time of arrival, vehicle's make, brand, and function of the vehicle. The JPO made the initial request to the driver and had a private conversation with protectors, due to the call being sensitive, and then decided whether to charge the driver and whether to receive station bribes, often within the day. Thus, the JPO had firsthand knowledge of the alleged infraction, initial bribe request, protector, station bribes paid, and whether the JPO charged the driver.

Collection: In May 2015, we obtained the approval of the colonel in charge and from each station commander for the following data design. The JPOs of each of the eight stations recorded, on paper, for each escorted vehicle, the following information: intersection of origin and time of arrival, make and brand, vehicle's type, allegation (open-ended), initial bribe request, employer and rank of the driver's protector, station bribes, whether they were charged and the requested fine amount if charged. The JPOs also included each team's quota level daily. They received minimal compensation for their time. This produced details about 1,235 driver negotiations in the stations from the data collection sample, of which 95 were from unidentified intersections (the "*Unassigned intersections sample*"), respectively, likely from the *data collection sample* (Table A1, Panel B). Furthermore, in each station, the JPO data collection supervisors corrected the JPO reports using already existing bribe records.⁷ The JPO data collection supervisors also had informal discussions in the station daily and recorded quota levels independently (*Branch 1b*).

Branch 2: Data Independently Reported by the Fine Collection Agents inside the Eight Stations

Context: The FCAs sat in the same station as the JPOs, which was typically a single-office

⁷"The JPOs have a register in which they keep the cars for the FCA agents; but they also have a single paper on which they have written the true entries. This register is for the commander, to show what is sent to the FCA agent and what is for the police. We cannot copy this register, but we use it to verify the JPO data. Since about the 27th of June, we access it daily." Source: Evening data collection supervisors meeting, June 2015.

small container. FCAs were routinely aware of station bribes, which were discussed publicly in the office because those were not sensitive. However, without exerting additional effort, they did not easily know the brand and make of the car, the intersection and hour of origin, whether there was a protector and their identity, nor the allegation for drivers that were not charged.

Collection: The fine collection agency agreed on the following design in May 2015. The FCAs of each of the eight stations recorded the station bribes of each escorted driver based on hearing the conversation. They also reported their guesses about the intersection of origin, time of arrival, allegation, brand, vehicle type, daily quota level for each team and, for each charged driver, the fine paid. The FCA forms were blank to prevent Hawthorne effects. The FCAs received minimal compensation for their time. Reflecting their poor knowledge of the intersection of origin, Branch 2 produced details of 728 and 527 negotiations classified as from the data collection sample or from unassigned intersections, respectively (Table A1). This design ensured that Branch 1 and 2 were independent.⁸ In addition, the Branch 2 supervisors also had informal discussions in the station each day and recorded quota levels independently (*Branch 2b*).

Branch 3: Data Independently Reported by Street Observers in Each of the 15 Intersections

Agents were required to wear an orange jacket and a yellow helmet with large numeric identifiers; thus, they were easy to identify. The Division of Transports of Kinshasa agreed on the following design in May 2015. In each of the 15 intersections, two observers on opposite sides of each intersection recorded the events of agents interacting with the drivers in a smartphone app. Each observer recorded the universe of agent-driver interactions (events) of one randomly selected agent (henceforth, *target*), based on a roster of agents of each station obtained prior to the study. For each event, they recorded the bribe type and amount, whether it was a pull-over, an escort, the vehicle type and brand, and described the interaction in open-ended format. This produced details about 13,793 police-agent/driver interactions in the data collection sample (Table A1, Panel B), of which 1,841 were pull-overs. In addition, each round hour, observers entered whether, in the preceding hour, there was a traffic jam or an accident and, during one minute, estimated the number of passing vehicles and their average speed. This produces 6,399 measurements about 2,997 day/hour-intersection traffic outcomes.

⁸Based on a large amount of information obtained daily from the stations, including suspicions and conflicts, we were convinced that FCAs and JPOs were never aware of the existence of Branch 1 or Branch 2, respectively.

To ensure their safety and to minimize Hawthorne effects, observers were all employees of the Division of Transports; they worked alongside the beverage, phone credit, or currency exchange retailers that routinely operate at intersections; they gathered naturalistic observation of publicly observable behavior on which there could be no expectation of privacy—thus the agents did not need to be informed—and we randomly assigned them each day to different intersections.

Branch 4: Quota Level and Retrocession from Morning Informal Conversations with the Agents

After hours, the agents wrote a short diary about their day on a blank page and received appropriate compensation to complete it (see [Study Documentation](#), Section 2). Diaries were collected after hours by two local researchers connected to police networks who had no contact with nor knowledge of the rest of the study (henceforth, the “assistants”).

Branch 5: Quota Level Gathered from Evening Informal Conversations with the Agents

The *auditor* also informally and occasionally recorded the teams’ quotas once sufficient personal trust had been built. His role was introduced as an informal conversation verifying that the assistants had provided the diary compensation. The auditor being flown from another region, reduced the risk of collusion with the agents or the assistants.

Figure A4 and Table A2 offer a visual of these branches for a typical station and a summary of the data collected, respectively. [Ethics Appendix](#), Section 5, and [Study Documentation](#), Section 2, describe the ethics, information leakages, and management of data collection, respectively.

C. Data Construction

For station activity, by design, Branch 1 was designed to measure more outcomes, whereas Branch 2 observers had better incentives to report station revenues.⁹ Thus, we use Branch 1 in every analysis and complement it with Branch 2 when analyzing station revenues side by side. Since alleged infractions were made in the street, no station branch includes information on whether

⁹Table A3, Panel A, provides reassurance about this design-based assessment. It presents the result from regressing Branch 1 on Branch 2 (and vice-versa) station-day records of the number of escorts, fine and bribe revenue, and escorted vehicle characteristics in the data collection sample. First, the correlation between Branch 1 and 2 for the number of escorts and revenue (Columns 1-6) is high and significant. Thus, as these sources are independent, this provides confidence in each source’s quality to measure station revenues. Second, the mean number of escorts and mean bribe and fine revenue using the universe of vehicles reported are larger in Branch 2. Thus, consistent with Branch 2 having a weaker incentive to under-report, Branch 2 has better information about station bribes revenues. Third, Columns (7) - (16) support that only Branch 1 has usable information about protectors and allegations: there, the correlations are always nonexistent, and Branch 1 means of allegation or protector characteristics are much larger.

the alleged infraction is true or false; hence, we manually classify the open-ended descriptions of the alleged infractions in Branch 1 into whether it could be verifiable by a third party (e.g., not *owning* a driver's license) or not (henceforth, *unverifiable*). For *agent behavior*, we construct a team-day aggregate (Study Documentation, Section 3). Branch 3 recorded 13,793 events; of those, 1,841 were traffic pull-overs. Branch 3 reported escorts are highly correlated with station reports of escorts, but Branch 3 also includes pull-overs that did not result in escorts; thus, we use Branch 3 to analyze pull-overs.¹⁰ Branch 3 open-ended descriptions allow us to classify Branch 3 events into whether the agent used coercion or force, whether there was a dispute and, for traffic pull-overs, whether the driver clearly made a (visible) infraction. For *traffic outcomes*, we construct day/hour-intersection measures of traffic outcomes as the mean of each intersection's two observers' reports of each day/hour-intersection traffic conditions. Simple checks provide reassurance in the reliability of these data.¹¹ For *quota levels*, we use the mode of the team-day quota levels reported in Branches 1, 1b, 2, 2b, 4, 5, and of the outcome of daily end-of-day research staff meetings where quota levels are deliberated.

5. The Real Revenues of the Traffic Police Agency

Drawing on the data from these five branches, we now establish three facts about the scheme.

A. From the Data Collection Sample to the Descriptive Sample

For some analysis in this section, we create a random sample of team-days from the data collection sample not assigned to the encouragement (henceforth, *descriptive sample*). Specifically, it is the union of the 14 team-days in data collection sample's Day Zero and the 157 team-days from the experiment randomly sampled to *no* encouragement, totaling 171 team-days (Table A1, Panel A). It includes 535 and 373 vehicles at the stations from Branches 1 and 2, respectively, and of 6,629 driver-agent interactions in the street from Branch 3 (Table A1, Panel B).

¹⁰Table A3, Panel B, presents estimates of a regression Branches 1 and 2 on Branch 3 escorts (and vice-versa). First, mean escorts of an intersection-day were of similar magnitudes across all three branches; unsurprisingly, they were larger for Branch 3, since some of those were not to the station. Second, the correlation is strong and significant; given the independence of the sources, this provides reassurance that they are informative.

¹¹Figure A5, Panel A shows that, during the rush hour (after 3pm), Branch 3 source reports .3 hourly recorded traffic jams, while, outside the rush hour, it reports .1; thus, the measure of traffic jams is informative. As a placebo, Panel B shows that, in contrast, recorded accidents are not more prevalent during the rush hour. Figure A6 validates these data by comparing the consistency across different measures of traffic characteristics. In an intersection-hour block with a recorded traffic jam, twice as many vehicles are recorded passing through per minute, their guessed speed is half, and traffic is half as likely to be fluid.

B. The Three Descriptive Facts

Fact 1: The Real Revenue Generated Is Large, Mostly Unofficial, and Hinges on the Quota Scheme

Above and beyond the revenue it generated from fines paid at the bank, the agency generated significant revenue from bribes paid in the street to the agents and, more importantly, from station bribes. This revenue was orders of magnitude larger than its revenue from fines. Table 1, Panel A, presents the average monthly revenue generated by the staff associated with the average station by source of revenue, including the unassigned intersections sample. Each month, based on Branches 1 and 2, one station generated 1,816 and 1,891 USD of fine revenue paid at the bank, respectively. Yet, Branches 1 and 2 reports imply that bribe revenue (street and station bribes pooled) were 6,007 and 7,862 USD; these amounted to 77% and 81% of the sum of fine and (street and station) bribes, respectively (henceforth, *total revenue*). Total revenue was, therefore, between 4.2 and 5.2 times the official records from fines. Of the revenue from bribes, according to Branch 1, 58% is from station bribes; it is even 68% according to Branch 2. The remaining 42 and 32% according to Branches 1 and 2, respectively, are from bribes paid to agents in the street. The discrepancy in bribe revenue between Branches 1 and 2 arises from station bribes. As discussed in Section 4.C, Branch 2 reports of station *bribes* are less prone to under-reporting. We can thus conclude that at least 81% of revenue is from bribes; of that bribe revenue, 68% is from station bribes; overall, a station's total revenue is 5.2 times its fine revenue.

These station bribes, which were mostly charge negotiation bribes, were generated by the quota scheme. Panel B shows that, over one month, one police station requested 294 vehicles from its agents, who escorted 260 vehicles; of those escorted, 19 paid a fine, 115 paid a charge negotiation bribe, and 52 paid a car pound bribe, with means of 50, 14, and 8 USD, respectively.

In sum, *real* revenue, generated by an unofficial scheme enabled by the agency's official organization, was much larger than fine revenue. It is surprisingly large for a state with purportedly weak capacity. We now provide evidence of how exactly the *scheme* generated these bribes.

Fact 2: The Revenue Generated Through the Scheme Hinges, in Part, on Extortion in the Stations

Figure 2 Panel A shows that, of all 1,394 negotiations over infractions allegedly made by escorted drivers in Branch 1, 938 (67%) were allegations that third parties would be unable to verify (*unverifiable*). Those relied on the claims made by the agents and included allegations that the driver made a bad or illegal maneuver or that they refused to comply. Qualitative notes

Table 1: Fact 1—The Real Revenue Is Large, Mostly Unofficial, and Hinges on the Quota Scheme**Panel A: The Revenue (USD)**

Total Revenue [Ratio] (Source)	7,823.05 [4.3 × Fines] (Branch 1+3)	9,753.11 [5.2 × Fines] (Branch 2+3)
— Revenue from Fines [Percentage of Fine + Bribe Revenue] (Source)	1,816.37 [23%] (Branch 1)	1,891.48 [19%] (Branch 2)
— Revenue from Bribes [Percentage of Fine + Bribe Revenue] (Source)	6,006.68 [77%] (Branch 1+3)	7,861.63 [81%] (Branch 2+3)
– Of which, Station Bribes [Percentage of Bribe Revenue] (Source)	3,476.95 [58%] (Branch 1)	5,331.90 [68%] (Branch 2)
– Of which, Street Bribes [Percentage of Bribe Revenue] (Source)	2,529.73 [42%] (Branch 3)	2,529.73 [32%] (Branch 3)

Panel B: The Quota

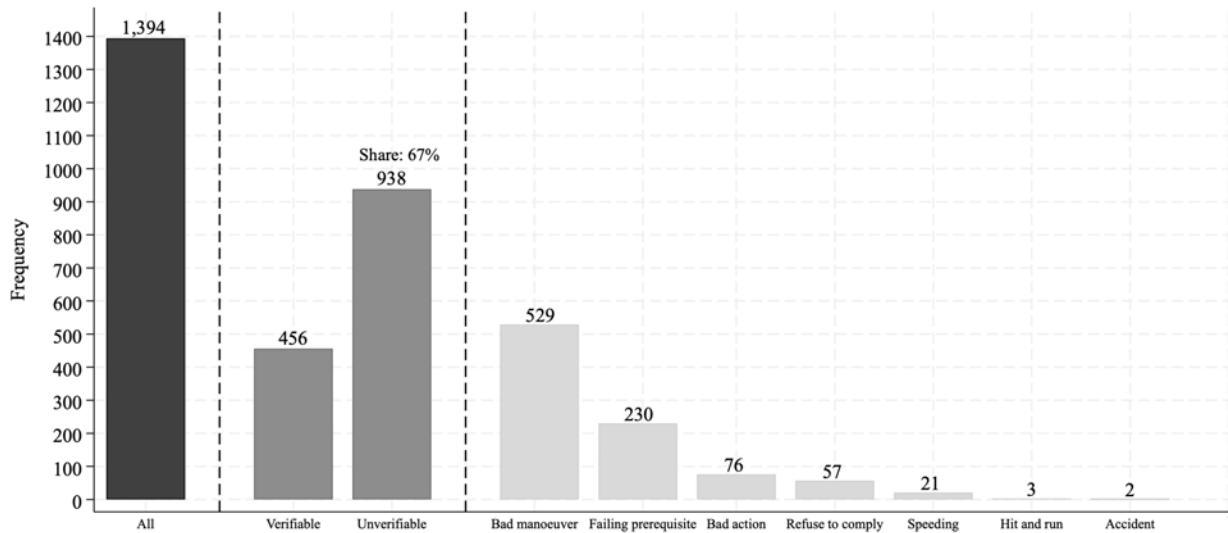
Number of drivers requested (Source)		294.23 (Quota sources)
Number of drivers escorted to the station (Source)	207.28 (Branch 1)	259.66 (Branch 2)
— Number of drivers who pay a fine [Mean fine] (Source)	25.13 [\$45] (Branch 1)	19.49 [\$50] (Branch 2)
— Number of drivers who pay a negotiation bribe [Mean negotiation bribe] (Source)	104.22 [\$12] (Branch 1)	115.06 [\$14] (Branch 2)
— Number of drivers who pay a pound bribe [Mean pound bribe] (Source)	26.56 [\$11] (Branch 1)	51.86 [\$8] (Branch 2)

Notes: Revenues and station transactions induced by all the staff of one police station (on average, out of eight in sample) over one month (on average). **Sample:** Descriptive sample + unassigned intersections sample.

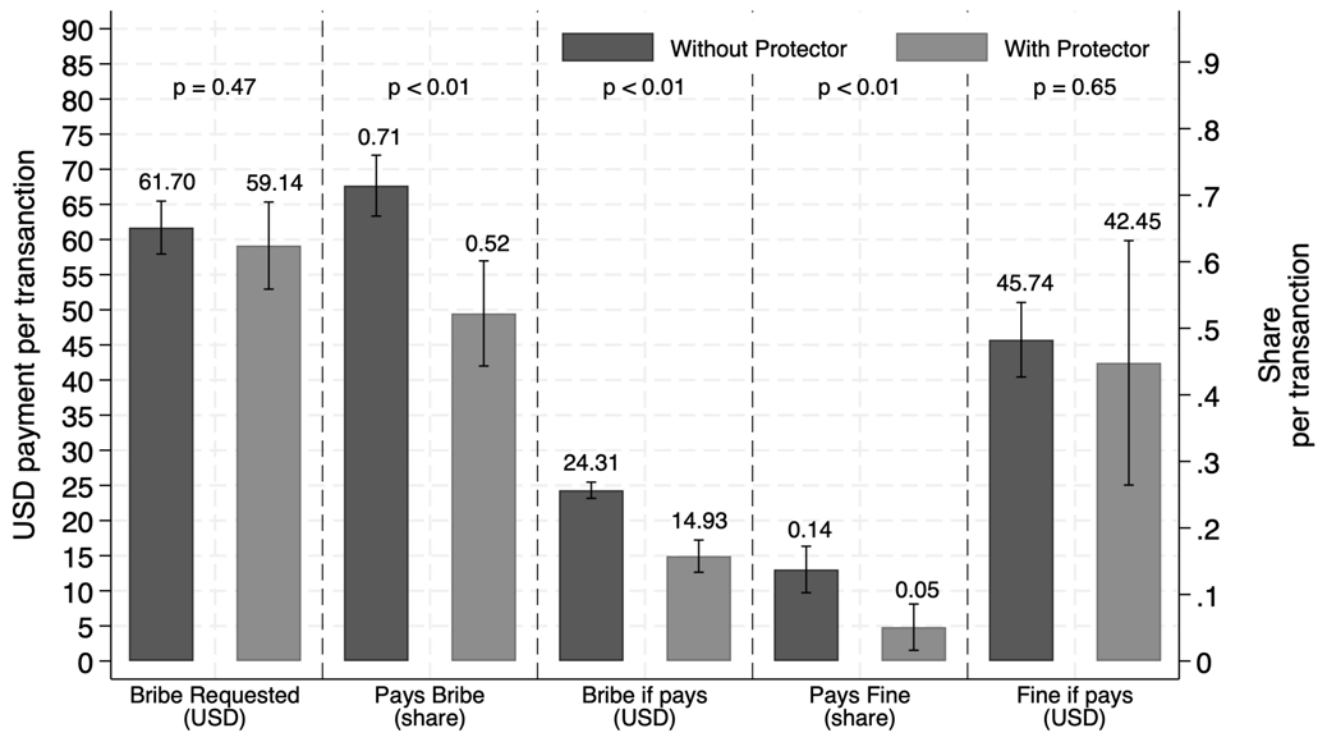
suggest that these allegations were sometimes false, which would mean that, colluding with the agents to accept or even encourage false allegations, station officials were able to extort drivers.

Figure 2: Fact 2—The Scheme’s Revenue Hinges, in Part, on *Extortion* in the Stations

Panel A. Officials Threatened to Charge for Allegations That Were for the Most Part Unverifiable



Panel B. Drivers Who Called a Powerful “Protector” Paid Less Station Bribes



Notes: Panel A presents the frequency of alleged infractions for each of the 1,394 reported infractions. Unverifiable allegations have been grouped into simple categories, which include bad or illegal stop, open door, not wearing a helmet, seat belt unfastened, refusing to comply, traffic obstructions, illegally boarded passengers, false maneuver and traffic lane violation. Panel B documents the mean outcomes in each step of the station bribes negotiation process between the JPOs and the drivers in the station, for drivers who did not call a protector vs. drivers who did. p denotes the p-value for a test of equality between means in dark vs. light bars. The brackets indicate 95% confidence intervals. Source in both panels: station events from Branch 1.

Furthermore, Figure A7, Panel A, shows that the average station bribe *paid* for an unverifiable allegation was mildly larger than for the rest; thus, in addition to potentially having the discretion to extort drivers, the station officials also had an *incentive* to do so.

What was the source of their power in making such threats? Once the agent escorted the driver to the station, the JPOs threatened the driver with a charge based on the allegation provided by the agent. Using all events recorded in Branch 1's data collection sample, Figure A7, Panel B shows that 32% of escorted drivers call a protector upon receiving the threat of a charge at the station and that protectors were high-ranked police and military officials. Individuals with these positions are known to routinely use the power of the state to exert blackmail (Titeca and Malukisa, 2018). Figure 2, Panel B, shows the steps in the station bribe negotiation. First, while the mean initial station bribe request was close to 60 USD regardless of whether the driver subsequently called a protector, only 52% of those who did call one paid a station bribe—against 71% for those who did not. Among those who paid one, those who called a protector paid 14.9 USD, but those who did not call one paid 24.3 USD. This implies that not having a protector they could call increased the expected station bribe from 7.8 USD to 17.2.¹² Drivers who did not call a protector were three times more likely to be charged. Overall, this indicates that the power of the protectors was to credibly impose a cost on the station officials if they charged the driver

In sum, this suggests that station bribes, the pillar of the quota scheme, could seemingly sometimes be *extortion* in the stations, hinging on the station officials' discretion whether to issue charges based on the agent's unverifiable reports. The station officials' ability to do so was thus in part dependent on the discretion of the agents to escort drivers even when they had not made an infraction, and to bring arbitrary allegations for the station officials to negotiate over.

Fact 3: Extortion at the Stations Hinges on the Agents' Power to Arbitrarily Escort Drivers from the Street and to Arbitrarily Allege Infractions

To examine the nature of allegations made by the agents, we analyze the process of escorting, which hinges on pulling over drivers in the street. Table 2, Panel A, shows that, of 1,841 events in which an agent pulls over a driver in the street in the data collection sample, Branch 3 observers saw proof that the driver committed an infraction in only 328 (18%)—e.g., “*The rider did not wear*

¹²Table A4 shows that having a protector is associated with a larger *reduction* from request to bribe paid.

Table 2: Fact 3—Extortion at the Stations Hinges on the Agents’ Power to Arbitrarily Escort Drivers from the Street and to Arbitrarily Allege Infractions

	Count	Percentage
Pull-overs	1,841	100%
Open-ended description missing	246	13%
Open-ended description non-missing	1,595	87%
A. Alleged infraction leading to pull-over		
<i>By Claim:</i>		
Clearly true	328	18%
Not clearly true (Dubious)	1,267	69%
<i>By Driver’s response:</i>		
Clearly disputed	533	29%
Not clearly disputed	1,062	58%
<i>By Outcome: (source: hard-coded outcome)</i>		
Driver escorted	1,285	70%
to station	417	23%
to unknown location	868	47%
Driver escaped	179	10%
Bribe (Amende) paid	377	20%
B. Use of force during pull-over		
Clearly coerced	372	20%
Not clearly coerced	1,223	67%

Notes: This table presents the mean characteristics of traffic pull-overs in the street by the agents. **Source:** Unless otherwise noted, the data in this table comes from manually coding after collection of the open-ended descriptions of each driver-agent interaction typed by Branch 3 observers; where noted, the source is Branch 3 hard-coded data. **Sample:** data collection sample. The data collection sample, described in Section 4A, is constituted of 351 team-days, constituted of 14 teams on Day Zero, and 337 team-days after Day Zero (the days of the data collection sample after Day Zero are the experiment sample). There are 13,793 agent-drivers interactions in the data collection sample, of which 1,841 are pull-overs.

*a helmet!” “Was unable to show any document,” “was parked in a pedestrian lane.”*¹³ The rest were based on no clearly apparent infraction or were simply dubious. Pull-overs were frequently seen as illegitimate by the drivers. Indeed, Panel B also shows that drivers escaped 10% (181) of the traffic pull-overs, and in 29% of them, they engaged in heated disputes with the agents. Yet, pull-over legitimacy was intimately connected to whether there was a true infraction. Indeed, Table A5, using the 1,841 pull-overs, shows that pull-overs with clearly true infractions induce more cooperation by the drivers (lower rates of escape, higher rates of a bargained bribe outcome,

and almost zero disputes), and induce significantly less agent brute force by the agents. Taken together, this suggests that the agents routinely pulled-over drivers based on claims that were unclear or illegitimate; consistent with existing qualitative literature (Baaz and Olsson, 2011), this also supports that they were poorly related to whether the driver actually violated the code.

What power did the agents use that enabled them to arbitrarily escort drivers? They often used physical force. Panel B shows that agents used force in at least 20% of pull-overs (Section C.A.). For example, agents routinely used force to seize the key, the battery, or the steering wheel, pinching the tires or leading to escape. The observers' open-ended descriptions of the escorts' attempts involved threats, brutality, disputes, injury, insults, fights, and bullying.¹⁴ Qualitative evidence suggests that agents routinely weaponized their official discretion to produce allegations in order to arbitrarily escort drivers shielded from repercussions, as well as to support the JPOs negotiation over station bribes in the station. For example, Branch 2 handwritten notes describe that agents escorted drivers under the pretense of "*fantasy infractions*:"

At the traffic police agency, there is what we call 'serfdom', which means that they work for their boss. This is why there are fantasy arrests to satisfy their commander vis-à-vis the quota that he requests. For example: defective brake light, yet when he [the driver] comes to the station, the driver declares, and the JPO verifies, and it is actually working (Qualitative notes from Branch 2 observer, July 2015)

This suggests that agents often escorted drivers regardless of whether the driver had complied with the law. And, when false claims might be implausible, they caused infractions themselves:

I really observe that the agents are not honest; they cause infractions. (Street observer end of day open-ended comments, July 2015).

In sum, this suggests that the agents regularly used force to pull-over drivers and escort them to the stations seemingly arbitrarily, and thus that escorting, the pillar of quota, could be in part under *extortion*. Taken together, these facts also imply that drivers who did not have a protector, so long as agents were able to detect that in the street, were a profitable target for pull-overs and escorting; thus, the quota could induce the agents to disproportionately attempt to escort drivers who do not have a protector, if the officers were sufficiently sophisticated and able to agree on it.

¹³In contrast, absence of evidence of an infraction does not imply evidence that no infraction was committed, as the driver may have done something wrong they did not see—such as whether the license, if privately seen by the agent, was expired. In some cases, they could said it was clearly arbitrary "*all his documents were in order*," but it is infrequent.

¹⁴Descriptions of pull-overs contain more anger, fear, and sadness than the rest of the interactions (Figure A8).

C. *Validity of the Averages from the Descriptive Sample*

The descriptive sample is a representative sample of the data collection sample team-days. However, if the experiment induced spillovers, it may not be representative of the potential outcomes in those team-days without encouragement. First, Figure A9, presents estimates of inter-temporal and intra-station spillovers for the variables used in Fact 1, 2, and 3, respectively. We find no evidence of spillovers within the team across days nor to other teams within the same station-day. Second, Figure A10 plots pre-study distributions of proxies for the quota and for the station bribe revenue against the estimates from the descriptive sample. The pre-study proxies were ballpark estimates of the means obtained prior to the study through various months of fieldwork with local informants, which we obtained to design the encouragement experiment and determine its compensation. The means of the descriptive sample and those of the pre-study proxies have comparable magnitudes and are positively associated across intersections. Third, in an exit survey after the completion of the study, we collected the agents' self-reported income: the mean, 14 USD, is almost identical to the estimates obtained in the descriptive sample (Table A6), 326 USD monthly, amounting to 12-14 USD daily. This analysis, therefore, provides reassurance that the street- and station-level averages based on the descriptive sample are valid estimates of the true means in the period of the data collection.

D. *Interpretation*

In sum, through the station bribes generated by the quota scheme, the agency had a *real*, if unofficial, capacity to raise revenue five times larger than that from fines. Our estimates imply that the mean ratio of the police officers' bribe income (net of the monetary payments they made to their respective superiors) over their total income was 83% and 86% for agents and station police staff, respectively; it was 5.8 and 7.3 times their official wage and 5 and 26 times the *actual* teacher income in DRC, respectively, which was 80 USD monthly (Brandt, 2013). This revenue was not generated by isolated state officials. It was shaped by the power of the state officials' managers had over their agents, shaping a coalition of officials with different powers vested in them by the organization of the state. It also hinged on the relative informal personal power of officials vs. citizens, which influenced extortion. Thus, our results suggest that this particular "weak state," in fact, has a quite strong capacity to generate illicit revenue from its citizens. In the following sections, we examine the effect of the scheme on public service.

6. The Effect of the Quota on Public Service: Analysis of the Experiment

In this section, we present an analysis of the effect of the quota on public service.

A. Organizing Framework

The quota scheme can influence both traffic management and the ability of the agency to act as a deterrent. First, if it induces the agent to substitute their effort away from traffic management and into escorting, it could reduce the agent's effort to manage traffic (Prediction 1). Effort substitution is reasonable to expect because the agents allocate scarce time across tasks. Second, *in addition*, the effect of the quota on the agency's impact on the deterrence of code violations depends on whether the drivers escorted due to the quota have made true infractions. Indeed, if the manager has the power to *extort*—that is, to charge drivers even when the alleged infraction is false—then the quota could induce the agent to: a. target drivers who can be expected to yield a large bribe rather than those who committed an infraction (Prediction 2a); b. if, through this targeting, the quota induces the production of more false than true allegations, then the quota could reduce the wedge between the implied cost faced by drivers for complying with the code vs. not complying, thus undermining the deterrent effect of the agency (Prediction 2b).¹⁵

This simple framework hinges on four assumptions. First, we assumed that the agents could detect which drivers would generate a large station bribe. In reality, agents have some information about what *types* of drivers, based on their visible characteristics tend to be high-bribe at the station, such as low wealth. More importantly, after stopping a vehicle, they engage in a negotiation commonly called as *tracasserie*; agents routinely use this to extract a “signal” from the driver's bribe potential. For example: a. the agent can find out if the driver has a powerful protector; b. the agent also gets an informal sense of the driver's ability to wield cash. Thus, the assumption that the agents can condition who they target for escorting on the bribe yield is not unreasonable. Given that more than one stop is needed to identify a high-bribe driver, an additional testable implication from this analysis is that a higher quota should lead to more pull-overs, many of which may not result in escorting. Thus, when analyzing the effect of

¹⁵A similar argument is made in Olken and Barron (2009). Appendix A derives these predictions formally. In the canonical model of collusion in organizations (Tirole, 1986), a principal owns the output produced by a citizen and wants to incentivize the effort by the citizen (here, drivers' code compliance). A supervisor (here, the agency) may have *hard information* about whether the citizen provides effort but can conceal it from the principal in exchange for a bribe from the citizen (bribery); they may also have *soft information*, i.e., the power to *forge* evidence that the citizen has shirked—*extortion* (Polinsky and Shavell, 2001). While bribery may or may weaken incentives for effort, extortion can destroy them. The model takes this as given and focuses on the supervisory agency's manager and its agent.

the quota on traffic outcomes, we also examine whether the quota increases pull-overs. Third, because our experiment is not designed to affect the driver choices, we have omitted those. This raises the question of why drivers are unable to bargain with the agents for a bribe to avoid being escorted or why some drivers do not get protectors. In Sections 6.B and 6.C, we take as given that drivers *are* regularly escorted and use the experiment to examine both predictions. Then, in Section 7, we present additional evidence to explore why some drivers cannot avoid being escorted and how that may affect the interpretation of the main effect of the quota. Fourth, in reality, there are typically four agents per team. To prevent moral-hazard-in-teams, the managers always place one team leader (called the *Gradés d'Elite*, in French) who, in their words, acted as a “spy” for the manager, whose loyalty appeared to not be a concern; thus, with this organizational design, the cost of the unitary actor assumption for a team of agents is low for our purpose.

B. Effect of the Quota on the First Mandate: Traffic Management

We now use the experimentally induced team-day variation in the encouragement for managers to reduce a team quota to examine the effect of the quota on the agency’s service. The encouragement is bundled with a cash transfer to the manager. On the one hand, this ensures that the quota reduction does not induce an income effect for the manager and thus shuts down possible confounds arising from a behavioral response by the managers motivated by seeking substitute sources of income. On the other, this bundling could affect interpretation in various ways. First, the encouragement could have relaxed the managers’ time constraint. Were it to be true, this is unlikely to have had any influence, as it was not a binding constraint for the station operations: reports of the managers at the street were rare, and managers were not involved in the negotiations. Those were led by the JPOs, who spent little time with each driver and waited for vehicles most of the day. Second, the encouragement might have affected the behavior of the agents—for example, if they wanted a share of the manager’s compensation. This is implausible, as only the managers were informed about the experiment. To the best of our knowledge, no one else became aware. Third, given the fact that a bribe income substitute was given, the expectation that the experiment would end could have influenced the managers’ choice of quotas on days not assigned to the encouragement. For example, they could have raised the quotas in anticipation of harder effort in the future. Learning through the fieldwork, conversations with the managers after the study, and a comparison of study and pre-study estimates of the quota and informal income suggest this is unlikely (Figure A10 showed them to be similar).

Estimation

Let i and t index the team/intersection and t day, respectively. Informed by the design (teams are strata and team-days are clusters) and the randomization balance, we estimate the following equations in the experiment sample (henceforth, *baseline specification*) in two stages least squares:

$$y_{it} = \alpha + \beta_Q \hat{Q}_{it} + \eta_i + \eta_t + e_{it} \quad (1)$$

$$Q_{it} = a + \beta_T T_{it}^{quota} + u_{it} \quad (2)$$

where $T_{it}^{quota} \in \{0,1\}$ is an indicator taking value 1 if team-day it was assigned to the *encouragement*, $Q_{it} \in \mathbb{N}$ is the recorded level of the quota, and η_i and η_t are an intersection i (the strata) and a day t fixed effect. Of the 337 team-days of the experiment sample, records of the quota are nonmissing in 326 team-days. The dependent variable y_{it} is an intersection-day level aggregate of hourly traffic outcomes from Branch 3. We construct intersection-day-hour indicators taking value 1 if at least one of the observers posted to a given intersection reported there to be an event in the corresponding intersection-day-hour. For each traffic outcome (traffic jams and accidents):

- incidence* is the sum of these intersection-day-hour indicators in an *intersection-day*—this corresponds to the number of times the traffic outcome event occurred in intersection i day t ;
- occurrence* is an indicator for whether at least one of the intersection-day-hour indicators takes value 1 in the intersection-day—this indicates whether (at least) a traffic jam or an accident was reported at that intersection-day. We then consider robustness to data construction.¹⁶

Parameter β_Q measures the average effect of a one-driver increase in the quota on y_{it} for *compliers*. The assignment and the dependent variable are both at the level of the team-day, but we present standard errors as follows: not clustered, as implied by the assignment procedure; clustered by station-day; clustered by intersection, calculated using the score test for wild bootstrap inference for instrumental variable regressions (Kline and Santos, 2012).

Estimates

Table 3, Panel A presents the estimates of the first stage coefficients (Equation 1). The sample across columns changes according to the corresponding second-stage regression sample in Panel B. The coefficient on assignment to the encouragement is large and statistically significant at the 1% significance level; thus, the first stage is strong and the assignment to the encouragement is a valid instrument for the quota level (which, as has been described, is randomized).

¹⁶Table A7 suggests that attrition is not correlated with potential outcomes.

Panel B presents the two-stage least squares estimates of β_Q . In Columns (1) - (4) the dependent variables are the measures of traffic accidents' and traffic jams' incidence and occurrence. An increase in the quota for a team increases accidents and traffic jams at the corresponding intersections. These effects are statistically significant at conventional levels of significance.

Statistical significance. Clustering at the station-day level (175 clusters) or at the team level (18 clusters) does not affect the p-value. The significance of these estimates is not affected by inter-temporal or intra-station spillovers. Table 3, Section B presents the baseline estimates controlling for the lagged treatment indicator, T_{it-1}^{quota} , and for an indicator for whether another team under the same manager was treated, T_{-it}^{quota} . The coefficient and significance with such controls are unchanged. Table 3, Section C, presents the OLS intention to treat (ITT) estimates:

$$y_{it} = \alpha^{ITT} + \beta^{ITT} T_{it}^{quota} + \eta_i^{ITT} + \eta_t^{ITT} + e_{it}^{ITT} \quad (3)$$

The encouragement is associated with a statistically significant reduction in both accidents and traffic jams. Statistical significance is preserved when calculating the p-value of the two-sided test of whether β^{ITT} is different than zero using randomization inference (Duflo, Glennerster, and Kremer, 2007), clustering the standard errors at the station-day or team (wild bootstrap).

Robustness. First, the estimates are preserved when individual teams, days, or blocks of days are excluded one at a time (Figure A11). Second, the effect has the same sign, albeit statistically insignificant, uses Branch 2 handwritten descriptions of the allegation as accident records, n=14 (Table A9, Column (1)). Third, the effect is preserved using an alternative external source for traffic jams, based on observers traveling on public transport minibuses occasionally passing through the experiment sample's intersections (Column 2). Fourth, the quota also decreases the number of drivers who pass by the intersection when there is no traffic jam (Columns 3 and 4), consistent with more drivers being stopped. Fifth, the effect is preserved using the *maximum* and *minimum* number of observers in an intersection-day who report a traffic jam or traffic to be fluid, respectively (Columns 5-8). Sixth, a reduction at $t - 1$ does not affect the number of drivers passing through at t (Table A11) and the number of drivers passing per minute across all intersections does not exhibit a trend across the study period, oscillating around 20 (Figure A13), suggesting driver responses are unlikely to explain this effect. Finally, the effect on all other outcomes is unaffected controlling for traffic jams' incidence (Table A10).

Economic significance. The 2SLS coefficients for incidence imply that, for each 15.8/6.9 drivers requested, there is one more accident/traffic jam at the corresponding intersection, respectively,

Table 3: Effect of the Quota on the First Mandate: Traffic Outcomes

	Quota Level							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>A. First-stage estimates</i>								
Encouragement _{it}	-2.535*** (0.272)	-2.535*** (0.272)	-2.535*** (0.272)	-2.535*** (0.272)	-2.517*** (0.275)	-2.517*** (0.275)	-2.527*** (0.277)	-2.738*** (0.0866)
First-stage <i>F</i> -statistic	86.61	86.61	86.61	86.61	83.96	83.96	83.45	84.18
Dep. var. mean (no encouragement)	6.54	6.54	6.54	6.54	6.55	6.55	6.50	6.97
Observations	321	321	321	321	318	318	315	2735
	Accidents		Traffic jams		Pull-overs	Team size	Time away	Quota met
	Incidence	Occurrence	Incidence	Occurrence	#	[0,1]	hours	0/1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>B. Second-stage IV 2SLS estimates (LATE)</i>								
Quota level _{it}	0.063*** (0.022)	0.051*** (0.016)	0.145** (0.065)	0.034* (0.018)	1.087*** (0.371)	0.001 (0.008)	0.336** (0.154)	-0.063*** (0.011)
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.01	0.00	0.03	0.05	0.01	0.95	0.02	0.00
<i>p</i> -value, S.E. clustered at <i>team</i>	0.00	0.00	0.03	0.05	0.00	0.94	0.01	0.00
Dep. var. mean (encouragement)	0.09	0.08	1.38	0.46	8.90	0.86	2.79	0.32
Observations	321	321	321	321	318	318	315	2735
— <i>Controlling for temporal and organizational lags</i>								
Quota level _{it}	0.056*** (0.021)	0.045*** (0.015)	0.110* (0.060)	0.030* (0.018)	0.969*** (0.353)	0.002 (0.008)	0.328** (0.148)	-0.065*** (0.011)
Observations	308	308	308	308	305	305	302	2628
<i>C. Reduced form estimates (ITT)</i>								
Encouragement _{it}	-0.156*** (0.055)	-0.126*** (0.038)	-0.349** (0.168)	-0.084* (0.049)	-2.637*** (0.932)	-0.001 (0.022)	-1.015** (0.415)	0.175*** (0.036)
<i>p</i> -value, randomization inference	0.00	0.00	0.04	0.08	0.00	0.96	0.01	0.00
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.00	0.00	0.04	0.06	0.01	0.96	0.00	0.00
<i>p</i> -value, S.E. clustered at <i>team</i>	0.01	0.00	0.06	0.08	0.01	0.95	0.00	0.00
Dep. var. mean (no encouragement)	0.25	0.20	1.47	0.51	11.31	0.87	3.45	0.15
Observations	332	332	332	332	328	328	325	2781

Notes: Section A: Equation 1 estimates; First-stage *F*-statistic is Olea and Pflueger (2013)'s effective *F*-statistic of the first stage. Section B: Equation 1 estimates. *p* – *value* reports the *p*-value of the test of whether β_Q is different from zero. Standard errors clustered at the level of the team are estimated using wild bootstrap score (Kline and Santos, 2012); *p*-values using Wang and Zhang (2022) are qualitatively identical. *Controlling for spillovers:* estimates of Equation 1 also include, as controls: the lag of the treatment indicator, T_{it-1}^{quota} , and the neighbor-team treatment status, T_{-it}^{quota} (taking value 1 if at least one team was treated in the same station and day, and 0 otherwise). Section C: estimates of Equation 3, and applies randomization inference to compute the *p*-value following Dufo, Glennerster, and Kremer (2007) and wild bootstrap test when clustered at the team level. **Sample:** Experiment Sample. In all columns, the observations are at the team-day level (denoted *it*, in the text). The number of team clusters is 18; the number of station-day clusters is 175. **Source:** Branch 3. Incidence is the number of hour blocks in the day in which at least one observer reported a given event, occurrence is whether an event took place in the day. "Team size," aggregated per team per day, is constructed using the street observers' information of whether the randomly selected target agent is actually posted at the intersection that day, described in greater detail in Section [Study Documentation](#), Section 3. "Time away," aggregated per team per day, is the gap between the timestamp at which the observed agent is seen interacting with a driver for an escort to the station and the timestamp of the observed agent's next interaction at the intersection, aggregated at the team-day level. Quota met is at hourly level, thus standard errors in Column (8) are clustered at the team-day level.

and that shutting down the quota would reduce the daily incidence of accidents and of traffic jams in all study intersections from 3.75 to zero (100%) and from 22.05 to 13.35 (40%), respectively.¹⁷ Even if the intersections are a small subset of Kinshasa’s network, and thus the quota likely affects only a small set of the potential events in the network, this nonetheless suggests the quota has a large effect on traffic outcomes in central locations where most drivers transit.

Behavioral channels. Columns (5) - (7) show that the quota increased the number of pull-overs and absenteeism of assigned agents at the corresponding street intersections, but the number of assigned agents working (team size) remained unaffected. Taking as given that 87% of the 4.2 mean assigned agents per team are working on a 7-hour day (Panel C), these estimates imply that the quota would decrease time present in the street from 25.6 to 23.5 agent-hours, a 2.1 agent-hours, 8% decrease. If the effect on accidents is entirely channeled through absenteeism, this would mean that the decrease from .25 to 0 accidents per day per intersection (100% decrease) that would result from shutting down the quota would be caused by 2.1 additional agent-hour in the street (8% increase); a linear extrapolation would imply that one accident at the intersection per day is being avoided for (at most) each 8.4 additional agent-hours present (33% increase). This implied significance of agent presence is larger than existing quantitative estimates in the literature.¹⁸ While it is possible that the proportional effect of agents’ presence in this context is unusually high (which can be rationalized by the high baseline presence of agents, the fact the quota affects the intensive margin rather than the extensive margin, and the fact that there is a low baseline level of accidents), the existing quantitative estimates in other settings suggest it may be implausible that absenteeism alone can explain the whole effect of the quota on traffic outcomes. Branch 3 open-ended descriptions confirm that absenteeism can be, in part, a channel, but suggest other factors, such as the frequency with which agents are a hazard to traffic (excerpts

¹⁷These predictions are obtained as follows: $\hat{y}(Q = 0) = \bar{y}(\bar{Q}) - \hat{\beta}_Q \times \bar{Q}$, where $\bar{y}(\bar{Q})$ is the mean of y_{it} without quota reduction encouragement and $\bar{Q}$ is the mean quota without quota reduction encouragement. Since the encouragement is randomly assigned, the sample mean of y_{it} without quota reduction encouragement is an unbiased estimator of the mean of y_{it} without quota reduction encouragement. Then, we use the estimates for $\hat{\beta}_Q = .0629$ and $.144$ for accident and traffic jams incidence, respectively, the median quota level, 4, and the mean incidence of accidents and traffic jams without quota reduction encouragement, .25 and 1.47, respectively. Then, the predicted accident incidence at an intersection, were the quota to be zero, is lower than zero, 100% lower; the predicted traffic jam incidence is .89, 40% lower. We obtain the numbers at the level of the 15 study intersections by scaling up in a factor of 15. The quantification using the mean quota of 6.3 produces a reduction of 100% and 62%. This prediction is preserved if the estimating equation includes quadratic quota level terms instrumented by the level of encouragement.

¹⁸Same calculation as for traffic outcomes. The meta-analysis by Botteghi, Theofilatos, and Macaluso (2017) includes the following estimates: increasing mobile radar enforcement exposure by 100% results in a 21% reduction in injury accidents on rural roads Goldenbeld and Schagen (2005); increasing the average check length by 1 hour results in a 5.07% increase in monthly speed-related collisions (Li, El-Basyouny, Kim, and Gargoum, 2017); increasing police enforcement exposure by 1% results in a 0.248% reduction in crashes (Elvik, 2011); increasing conventional speed enforcement exposure by 1% results in an estimated 0.0711% reduction in crashes (Elvik, 2015)).

are in Sections C.B., C.C., and C.D.). Whichever are the agents' behavioral adjustments that channel the effect of the quota on traffic outcomes, Column (8) shows that increasing the quota implies that the quota is more often *unmet* across the hours of the day; this is consistent with the effect of the quota being channeled through changes in agent behavior resulting from spending more hours in the day working towards meeting the quota.

In sum, the quota worsens the agency's performance on its first mandate, traffic management. However, it does not follow that the quota harms public service provided. Its total effect on the value of service depends on its effect on deterring code violations, to which we now turn.

C. Effect of the Quota on the Second Mandate: Deterrence

The study was not designed to analyze driver compliance. Indeed, since the experiment induces idiosyncratic changes in the quota's (secretive) level in a context of frequent changes in agent behavior, it would be unlikely for drivers to respond through idiosyncratic changes in compliance.

Instead, we infer the implications for deterrence by analyzing the agency's behavior. Section 6.A provided two testable implications for whether the quota undermines deterrence about the characteristics of escorted vehicles. Namely, we showed that if the manager's power to issue false charges is large (high power to extort), and if the agents are able to select who they escort based on the drivers' expected station bribe, then a higher quota would disproportionately induce escorts of vehicles with a high expected station bribe—*Prediction 2a*; furthermore, if this targeting effect holds, then a large power to extort would translate into the quota leading to the production of more false allegations than true ones—*Prediction 2b*, corresponding to *weaker* deterrence.

Effect of the Quota on Types of Drivers Escorted (Prediction 2a)

We begin by empirically examining whether the quota disproportionately induces the escorting of drivers who could be expected to yield a high station bribe (Prediction 2a). First, we draw on the descriptive evidence presented in Section 5 to identify drivers who could be expected to yield a high station bribe. Section 5 showed that drivers without a protector yielded significantly larger station bribe revenue. How can agents target those drivers? As introduced in Section 6.A, the agents can easily learn in the street in the discussion ensuing a pull-over whether the driver has a powerful protector. Table 4, Panel A, Columns (1) - (2) present the estimates of Equation 1, with dependent variables the number of escorted drivers escorted by team i on day t that do not, and that do, call a protector, respectively. The estimates show that the quota increases the escorts

of drivers who do not call a protector; it does not increase escorts of drivers who call a protector. The coefficient of the former is 15 times larger than that of the latter.

Second, we draw on all observable characteristics of drivers escorted to the station to predict the station bribe potential of an escorted vehicle, independently of the actual station bribe. Columns (3), (4) and (5) and (6) use as dependent variables the number of escorted drivers whose *predicted station bribe* is above/below the median station bribe in the no encouragement sample, and those whose *predicted fine* is above/below the corresponding median. To determine the *predicted station bribe* and *predicted fine*, we regress observed station bribes on all observable characteristics in Branch 1 data and construct a *predicted station bribe* and a *predicted fine* for each driver using these estimates (results are the same using Lasso instead). Columns (3) and (4) show that the quota increases the escorting of drivers who, based on this prediction, can be predicted to yield a large station bribe; it does not increase escorts of drivers with low predicted station bribe. Columns (5) and (6) show that the quota induces escorts of drivers with low, rather than high, predicted *fine*; thus, the increase in high-bribe escorts is not an incidental outcome of the quota inducing targeting of high-fine drivers who may incidentally yield high station bribes.

In sum, the quota fuels *exclusively* escorting of drivers who can be predicted to yield a high station bribe but does not lead to escorting of drivers who should be expected to pay a fine. This is consistent with Prediction 2a, which is a testable implication of the quota causing extortion.

Effect of the Quota on the Type of Allegations (Prediction 2b)

We now turn to analyzing whether the quota scheme induces the agency to produce more false alleged infractions than it induces the production of true ones (Prediction 2b).

In the stations, the quota increased escorts with unverifiable allegations. Using Branch 1 reports, Column (7) shows that the quota increases the number of escorted drivers with unverifiable infractions, but it does not increase escorts of drivers with verifiable allegations. This result is robust to inter-temporal and intra-station spillovers and to alternative standard error calculations.¹⁹ In sum, this suggests that the quota disproportionately induces the escorting of

¹⁹We consider two sources of non-classical measurement error that may affect the validity of our interpretation. First, the quota could change the conditions in which Branch 1 observers (JPOs) report driver characteristics. Table A12 shows that the quota has no effect on JPO attendance, suggesting nonclassical measurement error may not drive this result. It is also unlikely: as described in Section 4B, the JPOs are idle at the station most of the day. Second, Table A13, Panel A, shows that these results are preserved by controlling for traffic jams' incidence, suggesting that, as before, endogenous visibility due to congestion does not explain these results.

Table 4: Effect of the Quota on the Second Mandate: Testable Implications for Deterrence**Panel A.** In the Stations (Predictions 2a, 2b)

	# Escorts by team-day that are... (source: Branch 1, JPOs)							
	Targeting of drivers who may yield a high bribe (Prediction 2a)						Allegations (Prediction 2b)	
	Unprotected (1)	Protected (2)	High-Bribe (3)	Low-bribe (4)	High-fine (5)	Low-fine (6)	Unverifiable (7)	Verifiable (8)
<i>A. Second-stage IV 2SLS estimates (LATE)</i>								
Quota level _{it}	0.191** (0.0771)	0.0127 (0.0541)	0.159** (0.0712)	0.0486 (0.0588)	0.0768 (0.0849)	0.131* (0.0677)	0.502*** (0.123)	0.169 (0.106)
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.01	0.79	0.02	0.42	0.31	0.04	0.00	0.09
<i>p</i> -value, S.E. clustered at <i>team</i>	0.02	0.79	0.02	0.38	0.30	0.04	0.00	0.08
Dep. var. mean (encouragement)	2.72	1.48	2.53	2.13	2.67	1.99	3.29	1.75
Observations	259	259	236	236	236	236	225	225
First-stage <i>F</i> -statistic	89.66	89.66	101.47	101.47	101.47	101.47	78.42	78.42
— Controlling for temporal and organizational lags								
Quota level _{it}	0.180*** (0.073)	0.023 (0.051)	0.175*** (0.069)	0.039 (0.057)	0.087 (0.081)	0.128** (0.066)	0.485*** (0.118)	0.153 (0.103)
Observations	248	248	228	228	228	228	219	219
<i>B. Reduced form estimates (ITT)</i>								
Encouragement _{it}	-0.526** (0.231)	0.009 (0.166)	-0.436* (0.239)	-0.132 (0.194)	-0.182 (0.260)	-0.385* (0.220)	-1.350*** (0.366)	-0.461 (0.318)
<i>p</i> -value, randomization inference	0.02	0.96	0.07	0.50	0.49	0.07	0.00	0.14
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.01	0.96	0.07	0.50	0.49	0.05	0.00	0.12
<i>p</i> -value, S.E. clustered at <i>team</i>	0.02	0.97	0.06	0.49	0.50	0.06	0.00	0.11
Dep. var. mean (no encouragement)	3.22	1.35	3.04	1.93	3.01	1.95	4.48	2.11
Observations	264	264	241	241	241	241	230	230

Notes: **Sample:** Experiment Sample. The experiment sample covers 337 team-days, which are also the units of assignment, 326 of which have nonmissing quota level. Of all 326 team-days in the experiment sample for which the quota level is nonmissing, 259 have nonmissing protector or type of vehicle data, 225 have nonmissing allegations data. In all columns, the observations are the number of vehicles seen escorted at the station from the intersection (*team*) *i* on day *t* with the characteristic listed in the respective table headers; except for Columns 7 and 8, which are the number of allegations in the day from the corresponding intersection. There are 17 team clusters and 159 police station-day clusters. **Source:** For the dependent variables, Branch 1 (JPOs); for the endogenous regressor (quota level), as described in Section C. **Variable Construction:** The dependent variables High-Bribe, Low-Bribe, High-Fine, and Low-Fine are constructed as follows. After computing the median bribe from team-days not assigned to the encouragement, we estimate, using OLS, the predicted station bribe of an escorted vehicle, using as regressors all the recorded characteristics of escorted drivers reported in Branch 1. Using the estimates from this model, we construct a “station bribe potential score” for each escorted driver, and compute, for each team-day, the number of drivers whose station bribe potential score is above the median of station bribes that we computed among escorts from team-days not assigned to the encouragement.

Panel B. In the Street (Prediction 2b)

	Infractions and Pull-Overs			Disapproval	Driver Cooperation			Coercion	All
	# Pull Overs, by Infraction			Any Event	Outcome of Traffic Pull Over			Any Event	Std.
	Any (1)	Not clear (2)	True (3)	Dispute (4)	Escape (5)	Escort (6)	Bribe (7)	Force (8)	Index (9)
<i>A. Second-stage IV 2SLS estimates (LATE)</i>									
Quota level _{it}	1.088*** (0.371)	0.864** (0.356)	0.211 (0.152)	0.399** (0.197)	0.0269** (0.0135)	0.0252* (0.0146)	-0.0522*** (0.0197)	0.282* (0.164)	0.997** (0.390)
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.01	0.03	0.17	0.03	0.05	0.08	0.01	0.07	0.03
<i>p</i> -value, S.E. clustered at <i>team</i>	0.00	0.02	0.17	0.03	0.00	0.02	0.00	0.06	0.02
Dep. var. mean (encouragement)	8.89	7.02	1.65	2.56	0.06	0.20	0.74	1.75	6.53
Observations	318	311	311	318	1701	1700	1701	318	273
First-stage <i>F</i> -statistic	83.96	77.72	77.72	83.96	26.80	27.09	26.80	83.96	67.71
— <i>Controlling for temporal and organizational lags</i>									
Quota level _{it}	0.972*** (0.353)	0.726** (0.338)	0.215 (0.143)	0.391** (0.192)	0.017 (0.011)	0.025* (0.014)	-0.042*** (0.017)	0.269* (0.161)	0.727** (0.351)
Observations	305	298	298	305	1617	1617	1617	305	260
<i>B. Reduced form estimates (ITT)</i>									
Encouragement _{it}	-2.640*** (0.932)	-2.070** (0.889)	-0.540 (0.394)	-0.963* (0.521)	-0.042* (0.024)	-0.048* (0.026)	0.090*** (0.031)	-0.675 (0.437)	-2.355** (1.037)
<i>p</i> -value, randomization inference	0.00	0.02	0.16	0.06	0.01	0.01	0.00	0.12	0.02
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.01	0.03	0.17	0.03	0.06	0.06	0.00	0.08	0.03
<i>p</i> -value, S.E. clustered at <i>team</i>	0.01	0.03	0.19	0.03	0.01	0.03	0.00	0.10	0.03
Dep. var. mean (no encouragement)	11.30	8.86	2.19	3.68	0.12	0.27	0.61	2.52	9.27
Observations	328	321	321	328	1756	1753	1756	328	281

Notes: **Sample:** Experiment Sample. **Source:** the source for the dependent variables in all columns is Branch 3 street observers; the sources for the endogenous regressor (quota level) are described in Section 4. In Columns (1) - (3) and (7) - (8), the dependent variables are constructed using the open-ended answers of all events in the experiment sample where open-ended descriptions were missing in this aggregation. Columns (4) - (6) restrict the analysis to the sample of traffic pull-overs. Column (9) is a standardized index of all proxies of arbitrary escorts; because they require common support, the sample is restricted to traffic pull-overs. In Columns (1) - (4) and (8) - (9), observations are aggregated at the team-day level; in Columns (5) - (7), observations are at the level of the agent-driver pull-over. **Variable Construction:** Among 1,841 traffic pull-overs, 1,595 had non-missing open-ended questions, and thus could be used to construct the dependent variables in Columns (2), (3), (4) and (8); among 1,595 non-missing open-ended pull-overs, 328 were reported by Branch 3 observers as being based on certainly true claims. Column (7) pools observed bribe payment (“*amende*” in French) with observed escort to an unknown location—which is often to find a discrete place to pay an “*amende*.” Column (9) is a standardized index of all proxies of arbitrary escorts. In Columns (1) - (3), (4), (8), the dependent variable is a count. In Columns (5) - (7), the dependent variable is an indicator taking value one if the pull-over ends in either of the following outcomes, respectively: escape, escort to the station, direct evidence of bribe, or escort to hidden location (where often a bribe is paid). There are 18 team clusters and 174 police station-day clusters.

drivers with allegations that are more likely to be false. It is a testable implication from the quota inducing extortion, but it also establishes that the quota reduces deterrence.

In the street, the quota increased pull-overs where the driver did not clearly make an infraction and that were perceived as illegitimate by the drivers. To establish this result, we proceed in two steps, analyzing Branch 3 reports of agent-driver interactions from the intersections. *First*, we ask whether the quota induces *more* attempts by the agents to escort drivers on the basis of arbitrary claims, instead of clearly true ones. Panel B, Columns (1) - (3) show that the quota increases traffic pull-overs, driven by traffic pull-overs based on claims that are not clearly true and hence potentially arbitrary. To examine whether the analysis of allegation's truth is meaningful, we undertook manual linking of the street events to the police station events using all of the available vehicle characteristics. Figure A12 shows that vehicles reported by Branch 3 observers as having made a true infraction and escorted to the stations are slightly more likely to have made a verifiable allegation, providing confidence in Branch 3's assessment. They are also just as likely to pay a bribe or a fine at the station. Overall, this suggests that the quota increases the agents' arbitrary attempts to escort rather than of drivers who clearly committed infractions.

Second, we ask whether the quota induces traffic pull-overs perceived to be illegitimate by the drivers—and thus, which may be false or arbitrary. Recall that we saw in Table A5 that when the driver clearly made a true infraction, the driver was less likely to escape a pull-over attempt, to engage in a dispute, and the agent was less likely to use force. Column (4) shows that the quota increases the frequency of disputes among agent-driver interactions. Columns (5) - (7) examine the reallocation of the pull-over outcome, conditional on pull-over events: the driver is more likely to escape or to be escorted; in contrast, the only outcome in which driver-agent bargaining does not “break down,” *amende*, is less likely. This effect could be rationalized by the quota strengthening the agents' incentive to convert attempts into escorts. However, this simple demand-side explanation would tend to reduce escape—which is the opposite of what Column (5) showed, and thus inconsistent with this interpretation. Column (8) shows that the quota increases the use of force by the agents, although the effect is marginally significant. Supporting that breakdown of bargaining indicates lower cooperation, a Congolese analyst reported:

Alternatively, the driver has not committed any violation, and is upset due to the mistreatment of the officer. He then agrees to be taken to the office. . . Sometimes, the driver demands that he be given a payment slip to pay the fine at the bank and give nothing to the police. (Interview with Congolese analyst, 2023)

Taken together, these results underscore that the quota induces effort by the agents to pull-over drivers on the basis of arbitrary criteria for which it is implausible that true infractions were made—*extortion*. This is summarized in Column (9), in a standardized mean index.²⁰

In sum, guided by the framework in Section 6.A, this section provided evidence suggesting that the quota also undermines deterrence, the agency’s second mandate. Together with its effect on traffic outcomes, this suggests that, compared to corruption without the scheme, the scheme subverts public service, a social cost above and beyond that from agent corruption.

7. Why the Vehicle Quota? Drivers, Managers, and Agents’ Incentive

In this section, we examine whether the quota scheme can be rationalized as an equilibrium corruption contract in this setting. First, we examine why agents and escorted drivers could not bargain over a bribe to avoid escorting. As we have seen, this can happen, leading to the payment of an “*amende*” (bribe payment related to bargaining over not being escorted); yet, it fails for escorted drivers. Were bargaining to always succeed, the vehicle quota could not be rational. Our qualitative evidence suggests that the vehicle quota hinges on this bargaining often breaking down for three reasons. One reason is that the drivers have little cash on hand:

I’ll give an example...The driver...has only 30 dollars, but the police refuse this money. They demand 50 dollars, and the driver, in turn, makes it clear to them that he only has that amount. Dissatisfied, the police take him to the office where he is asked to pay 100 dollars to be released. (Interview with project manager, 2023)

Using data from Lameke, Malukisa, Sanchez de la Sierra, Tanutama, and Titeca (2024), even minibus drivers, who made cash through the day, had limited liquidity: accounting for their revenue from passenger rides and operational costs (gas and repairs), the typical minibus would have 14.5 USD at hand on average at midday and less in the hours before. Another reason is that, in the station, it was often the driver’s protector or the vehicle’s owner, not the driver, who would have to come in order to negotiate and pay on behalf of the driver:

The driver agrees to go to the office because he has the option to call an officer who will allow him to pay no fine, or to pay less. (Interview with Congolese analyst, 2023)

²⁰ All estimates in this panel are robust to spillovers and standard error calculation. Table A13, Panel B, shows that these results are preserved controlling for traffic jams’ incidence.

A third reason is that the driver or agent occasionally develops spite towards another:

At the time of the arrest, the driver angered the police with his behavior (insults, threats to call an officer, attempting to hit an officer, etc.).

These are transaction costs. Their regularity can explain why agents and drivers often fail to bargain a monetary payment, and thus, why the quota does not unravel into monetary kickbacks.

Second, we examine whether the quota is a corruption contract. An alternative interpretation of these results is that the quota may instead be a fiscal tool imposed by the hierarchy of the state on the stations in order to generate fines and that, incidentally, this only happens to create opportunities for station bribes to be collected. Table 5 presents the estimates of Equation 2 from the experiment sample. The dependent variable in Columns (1) and (2) are measures of fine revenue generated by the vehicles escorted by each team-day to their respective station. They show that the quota has no effect on fine revenue generated at the station from the escorts provided by the team. In contrast, Columns (3) and (4), which show the estimates for the dependent variable being the sum of station bribe revenues generated in the day by the vehicles escorted by the corresponding team, show that the quota significantly increases station bribes. Taken together, Columns (1) - (4) are consistent with the quota being a corruption contract rather than a fiscal instrument that creates opportunities for corruption at the stations.

Third, Column (5) presents the estimates when the dependent variable is, instead, the revenue by the team-day from all types of street bribes (toll fee, tips, and "*amende*"). The quota does not reduce the team's revenue from street bribes. This is not surprising. Indeed, it is consistent with the common strategy, which is also rational, of having an agent escort the driver to the police stations while keeping the other in the intersection collecting street bribes. Thus, the technology of street bribe collection, manifested in the weak relationship between the number of agents in the street and street bribes, is a pillar of the quota's optimality. Where this relationship may be steeper, a trade-off might render a vehicle quota less obvious. Overall, this suggests that the quota at its current level maximizes corruption joint profits compared to a lower quota.

In sum, the quota appears to be a rational response to the parameters of the setting, reflecting joint profit maximization from the perspective of the manager-agent pair and hinging on frequent transaction costs between the agents and the drivers, the power of station officials to collect station bribes, and on the technology of street bribe collection.

Table 5: The Quota as a Joint Corruption Profit Maximization Contract

	Fine Revenue (USD)		Bribe Revenue (USD)		
	Police Station		Police Station		Street
	Branch 1 (1)	Branch 2 (2)	Branch 1 (3)	Branch 2 (4)	Branch 3 (5)
<i>A. Second-stage IV 2SLS estimates (LATE)</i>					
Quota level _{it}	2.246 (1.488)	-0.317 (1.421)	3.386* (1.937)	3.259* (1.943)	0.401 (1.906)
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.14	0.84	0.10	0.08	0.83
<i>p</i> -value, S.E. clustered at <i>team</i>	0.18	0.84	0.10	0.09	0.82
Dep. var. mean (encouragement)	18.64	18.82	59.28	62.88	51.25
Observations	259	173	259	173	318
First-stage <i>F</i> -statistic	89.66	73.27	89.66	89.66	83.96
— Controlling for temporal and organizational lags					
Quota level _{it}	1.943 (1.425)	-0.612 (1.475)	3.437* (1.816)	3.329* (1.991)	0.673 (1.866)
Observations	248	172	248	172	305
<i>B. Reduced form estimates (ITT)</i>					
Encouragement _{it}	-6.013 (4.514)	1.032 (5.130)	-8.639 (5.852)	-10.606 (6.938)	-0.511 (5.022)
<i>p</i> -value, randomization inference	0.18	0.85	0.12	0.14	0.91
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.20	0.86	0.16	0.10	0.92
<i>p</i> -value, S.E. clustered at <i>team</i>	0.24	0.85	0.19	0.12	0.91
Dep. var. mean (no encouragement)	22.69	19.03	66.94	70.31	53.30
Observations	264	173	264	173	328

Notes: **Sample:** Experiment Sample. The experiment sample covers 337 team-day observations, which are also the units of assignment, 326 of which have nonmissing quota level. Of all 337 team-days in the experiment sample: a. 264 (Branch 1) and 173 (Branch 2) have nonmissing station revenue data, and 248 (Branch 1) and 173 (Branch 2) have both nonmissing quota level and escort revenue data; b. 328 have nonmissing street bribe revenue data, and 318 have both nonmissing quota level and street bribe revenue data. In all columns, the observations are at the team-day level (denoted *it*, in the text), and refer to the revenues from the vehicles seen escorted at the station from the intersection (hence, team) *i* on day *t* with the characteristic listed in the respective table headers. There are 17 team clusters and 157 station clusters. **Source:** Branches 1, 2, and 3, described in Section 4B. Branch 3 events are aggregated at the team-day level following the aggregation procedure detailed in [Study Documentation](#), Section 3; the sources for the endogenous regressor (quota level) are described in Section 4B.

8. Corruption Systems in Perspective

In this section, we discuss the specificity of our findings in a global and historical context.

A. *Corruption Systems within the Agency*

The interviews we conducted with officials reveal that Kinshasa's traffic police agency did not always have a vehicle quota. According to them, since 1975, their wages have become insufficient for survival. While bribe-taking purportedly followed the reduction in their wages, it only began to be organized as a scheme in the 1980's (Nkuku, 2022). Then, officers began making payments between 10 to 15 dollars daily to their commanders, who then made payments to higher levels of the hierarchy, purportedly under the oversight of a provincial commissioner; this practice came to be known as "*retrocession*."

After 2010, the demands of this practice on the officers increased. Commanders were asked to make weekly payments to a newly created structure, the "*Coordination*" in French (Titeca and Malukisa, 2014), and these payments amounted to a significant share of the bribes they were able to collect themselves directly. Members of the "*Coordination*" then purportedly made payments a Minister and a national commissioner. Some agents referred to that scheme as a "commercial enterprise."²¹ Agents considered asking money from drivers to be legitimate but described the payment requirements to their superiors as "servitude" or being a "blood donor" (Titeca and Malukisa, 2014). Sanctions for not paying also increased (see Section B.a).

Our interviews suggest that, up to 2014, the practice of paying the superiors changed into a quota of vehicles, reportedly in three steps. First, between 2010 and 2014, the number of stations increased from two to 29; thus, intersections got closer to any station. The commanders at the stations began requesting vehicles rather than money. According to one commander, this practice allowed to preserve a "facade of legality:" it followed the official procedure of bringing the vehicle to the station, which protected the agents from being arrested for taking bribes in the street. Second, in late 2014, as President Kabila had become unpopular in the capital, a new general of the Kinshasa police introduced measures to increase the President's popularity. Taking bribes in the street became severely penalized.²² The qualitative interviews we conducted during

²¹"Through the Coordination, the traffic police became a commercial enterprise; but only for [two commissioners]!" (interview with agent, 2013)

²²The general frequently posed undercover in the street, leading to arrests of various officers. The agents referred to the climate as the "*Kanyama psychosis*" and to the general as "*Esprit de Mort*" (spirit of death) due to his hard line.

this time suggest that bribes paid by drivers in the street decreased and that the vehicle quota scheme became entrenched. Third, taking street bribes eventually stopped being risky; even then, the vehicle quota scheme practice continued: commanders had reportedly learned through this experience that it was more profitable than relying on “*retrocession*.” One commander said:

Before, the agents could exaggerate in these negotiations and obtain a lot of money, of which they only gave a small amount to the commander. Now we, or our responsible agents, can directly negotiate with the drivers. It always is better to directly negotiate with the drivers. (Interview with commander, 2013).

Across the agency’s locations, the schemes varied. In Lumumbashi, the colonels obtained weekly payments from the agents in cash (so-called “reports”). In Goma, the scheme amounted to a vehicle quota scheme, but agents were allowed to substitute vehicles for cash at a given exchange rate (qualitative interviews in Goma and Lubumbahsi; see Section E.b).

In sum, the way in which bribe-taking schemes were organized in the traffic police agency varied over the years in Kinsahasa and across cities. Its specific organization appeared to reflect a combination of rationality, habits, and experimentation.

B. Corruption Systems in Global Context

This scheme could be understood as an exceptional characteristic of a weak state with little relevance to other settings. Indeed, the DRC has been described as a “paradigm of informalisation and criminalisation of the state” (Petit and Mutambwa, 2005), created over 30 years of Mobutu rule. Ex-President Mobutu’s famous quote “*Moto na moto abongisa*” or “Let each person sort things out at his own level” sent a strong signal to state officials generally—it was “ironically” reinterpreted in relation to bribe-taking and rule-breaking, as an invitation for “each and everyone to steal at his own level of responsibility” (Petit and Mutambwa, 2005: 482). As a result, state positions have been described as becoming “privatized” (Lemarchand, 1988): officials were encouraged to “steal cleverly” (Titeca and De Herdt, 2011). Although the public revenues eventually increased after Mobutu’s departure in 1997 due to international aid throughout the 2000s (Titeca and De Herdt, 2011), the practices created during Mobutu’s rule remained. Titeca and De Herdt (2011) document such systems in the education sector, Verweijen (2013) in the military, and Lake (2022) in other branches of the police (we provide some quotes in Section B.c).

However, similar schemes were documented across the world. Because of the illicit and organized nature of the corruption schemes involving managers and their agents, academic research on such schemes is rather limited. Among academic research, Wade (1982) concluded that organized corruption “may be a more fundamental influence on Indian administration and politics than has hitherto been realised,” for which Pant (1999) provided evidence in a different state. Weaver (2021), in a remarkable study, shows evidence of bribes for jobs in a public agency.

Notwithstanding the limited academic research on this topic, public legal investigations reveal that these schemes can be frequent and large. In middle and low-income countries, there is evidence of such schemes within public firms and in the education and health sectors. First, regarding state-owned firms, as recently as 2014, one of the largest corruption scandals in history was uncovered in Brazil. Operation Car Wash (Operação Lava Jato) revealed a money-laundering and bribery scheme involving Petrobras, Brazil’s state-controlled oil company. Reportedly, executives at Petrobras overpaid suppliers and persuaded them to channel back to the officials a share of the overpaying, who then shared some of it in kickbacks to politicians who gave them the position (Romero, 2017, News, 2017, Eco, 2019). Second, regarding the education sector, recently, evidence suggested corruption schemes in financing systems in the South Africa Department of Higher Education (The Africa Report, 2024): “We believe [the conspiracy] is carried out by sophisticated and protected syndicates . . . linked to the upper echelons of the Department of Higher Education.” Third, recent studies suggest this might be prevalent in the health sector. While these studies report nepotism and cronyism across the board, sometimes with smoking-gun evidence of rent-sharing agreements that incentivize lower-level corruption, the latter has been explicitly documented in some cases. For example, in Uganda, “promotion was dependent on continued demonstration of loyalty to the appointing patron by participating in other corrupt acts such as demanding informal payments (bribery) and embezzlement” (Kirya, 2020). In Greece, for example, evidence became abundant after the investigations that followed the outbreak of the financial crisis, pointing to grand corruption schemes. Schemes were reportedly found involving the operation of a major hospital, the compensations for diagnostic arthroscopies, the pharmaceutical policy and drug supplies, a health entity involved in clientelism and patronage, and large pharmaceutical companies. In general, it was reported that health suppliers paid kickbacks to influence the referral and prescription process of doctors, and the procurement process in health structures (Stamouli, Gasparinatou, and Kouroutzas, 2023).

In richer countries, there is also evidence of similar schemes. For example, concerning police

forces, the Knapp Commission report revealed in the 70s that “a large part of [New York] city’s police force has been guilty of some form of illegal activity,” where: “both high-ranking police officers and high-ranking members of the Lindsay administration had Information about the scope of this activity but did nothing about it,” suggesting that in order to incentivize looking the other way, rent-sharing agreements might have occurred (The New York Times, 1973). Transnational corruption schemes have also been documented in the health sector. Indeed, recently, an investigation reported evidence supporting a possible grand corruption scheme in global supply chains in “the largest ever project funded by the US Agency for International Development (USAID)” within its first two years of its operation (The Bureau of Investigative Journalism, 2023): “Inadequate financial monitoring . . . combined with potential collusion between Chemonics and Zenith staff, allowed the fraud to remain undetected.” The investigation “found that an unnamed senior project director allegedly implicated in the collusion, who signed off on all distribution invoices, was “living substantially beyond their Chemonics salary,” pointing towards a scheme.

Evidence from investigations thus paints a picture of manager-agent schemes inside state administrations as potentially relevant across the globe but also potentially sophisticated and large. Yet, the evidence from investigated cases likely represents only the “tip of the iceberg” of all existing corruption schemes: it is limited to those that are discovered and investigated.

Historical records provide evidence that the practice was also common throughout history across the globe. The public administrations in France, Spain, Italy, Netherlands, Germany, the Ottoman Empire, China, as well as the colonial administration of European Empires are replete with similar examples (Swart, 1949). The practices in that period (e.g., “*venalité des offices*” in France’s Ancien Régime) developed out of the practice of farming out offices, was widespread in the Middle Ages—with the idea that public office was a source of profit rather than a duty towards a king. In France, as early as the fourteenth century, notaries and tax collectors were allowed to transfer their offices for a payment (Dupont-Ferrier, 1930). In the next century, it was prevalent among sheriffs and courts of justice. After the end of the fifteenth century, it spread to provide cash for rulers in wars (Ardant, 1975).²³

In sum, Kinshasa’s traffic police scheme would appear to be a rather common consequence of the personal privileges provided by a state administration rather than an exception.

²³Francois I^{er} even created an agency in charge of all the sales of offices, named the *Bureau des Parties Casuelles* (Dupont-Ferrier, 1640), and the annual sum to be paid, called *la Paulette*, began to be centrally dictated.

9. Conclusion

The results of this study suggest that when institutions are underfunded and cannot pay the wage of their officials, state officials, and their managers may use the official powers bestowed upon them to collude against the public shaping corruption schemes that support real revenue-generating capacity and that may be even more distortionary than corruption without a scheme. This has some implications for policy. First, interventions aiming to curb corruption could focus on the levels of the hierarchy above the frontline civil servants and on the ways in which the official powers bestowed upon officials at different levels can create opportunities for collusion against the public. This could also have implications for policing more generally (e.g., Ba, Knox, Mummolo, and Rivera (2021), Rivera and Ba (2023)). Second, in the context of enforcement, technologies that reduce the officials' discretion to fabricate allegations could mitigate the effect of corruption arising specifically from extortion—dampening the incentives for citizens to comply with the law—even when corruption continues to exist; furthermore, the efficacy of such technology hinges on the specific organizational design of the state—e.g., who has the discretion to make allegations or to issue charges and what is the relationship between them.

Our study suggests that corruption may be the “tip of an iceberg” involving broader coalitions of state officials and suggests three possible avenues for research. First, the scheme is just one small cog in a broader system. Agents, drivers, and commanders regularly make large payments to higher-ups to seek their “protection.” In qualitative interviews, we found that station managers made monthly unofficial payments to individuals higher up in the hierarchy, amounting on average to 225.53 USD monthly. Understanding the players, strategies, games played, what they maximize, and how equilibria structure the behavior of its participants is a promising avenue of future research. Second, this context may be prevalent in many weak states, but, even as we have found evidence of their existence across settings, it is likely that they are either less prevalent or present in a different form in high-income countries and in low-income countries with stronger states. Given the challenges of uncovering such schemes, more research is needed to even determine how prevalent they are beyond the public scandals for which we have provided evidence. Third, we do not understand how such schemes are “embedded” in specific cultural ways of understanding relationships (e.g., the concept of “*Guanxi*” networks in China (Yin, 2017)). The power of managers may also matter in understanding corruption schemes beyond their material power: their role as leaders may also shape organizational cultures of corruption—

e.g., the “behavior of superiors” is often reported to be the most influential factor on unethical decisions (Baumhart, 1961, Brenner and Molander, 1977).

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For Online Publication—Online Appendix

Appendix A. Mathematical Appendix

A.. *The Model*

There is one period. There is an exogenous set of drivers. The drivers make no choices, and are characterized by two dimensions: whether they have committed a true infraction, $v_i \in \{0,1\}$, and whether they yield a bribe for the agency if they are charged with making an infraction, $b_i \in \{0,1\}$ (henceforth, *high-bribe drivers*). Let $\sigma_{bv} = E(b_i|v_i = 1)$, $\sigma_{vb} = E(v_i|b_i = 1)$, and $\sigma_{b\neg v} = E(b_i|v_i = 0)$ be the fraction of high-bribe drivers conditional on those who committed an infraction, the fraction of drivers who have committed an infraction conditional on being high-bribe, and the fraction of high-bribe drivers in the set of those who have made no infraction. We assume $\sigma_{bv} > 0$, $\sigma_{vb} > 0$, $\sigma_{b\neg v} > 0$.

There is a police agent. The agent chooses how much effort to provide in managing traffic, $e_T \in \mathbb{R}^+$. She exerts effort to detain drivers, $e \in \mathbb{R}^+$. Share s of that effort e is allocated only towards drivers with $b_i = 1$; all of the remaining $1 - s$ is allocated only towards drivers with $v_i = 1$. The agent’s payoff is $\beta^A e_T - c(e_T, e)$, where $c(e_T, e) = (e_T + e)^2$, and $\beta^A > 0$.

The agent has a manager. The manager can charge drivers who have committed an infraction, and has the *partial* power to charge drivers who did not commit one—i.e., to extort them. Specifically, the manager can charge share $\alpha \geq 0$ of drivers who have not committed an infraction. We assume that charging is costless, and hence the manager always charges when he can. The manager’s payoff is $ay(e)b + \beta^S e_T$, where $b > 0$ is the bribe paid by drivers who pay a bribe, $y(e)$ is the number of escorted drivers as a function of e , where $y'(e) > 0$ and $y''(e) < 0$, and $\beta^S > 0$ is an exogenous parameter. $a > 0$ is the fraction of escorted drivers who yield a bribe, thus $a = s(\sigma_{vb} + (1 - \sigma_{vb})\alpha) + (1 - s)\sigma_{bv}$.²⁴ In the first step of the period, the manager chooses

²⁴This fraction is determined as follows: σ_{vb} is the fraction of drivers who have committed an infraction, amongst high-bribe drivers. These yield a bribe. The remaining fraction $1 - \sigma_{vb}$ of drivers who yield a bribe when charged with an infraction did not commit a true infraction, but the manager charges share α of those regardless. Thus, in addition to share σ_{vb} , share $(1 - \sigma_{vb})\alpha$ of drivers who have committed an infraction also yield a bribe.

e and s anticipating that, in the second step of the period, the agent takes e and s as given and chooses e_T , which the manager cannot observe. The manager's problem is:

$$\begin{aligned} \max_{e,s} \quad & ay(e)b + \beta^S e_T \\ \text{s.t.} \quad & \max_{e_T} \beta^A e_T - c(e_T, e). \end{aligned}$$

The quota, e , can affect any of the agency's two mandates: traffic management and deterrence of illegal behavior. First, the effort to detain, e , induced by the manager's incentive, b , could reduce the agent's effort to manage traffic, e_T , if it induces effort substitution away from traffic management, e_T . Formally, the manager's opportunities to take bribes have the following effect on the agents' effort to manage traffic.

Proposition 1 $\frac{de_T^*}{de} < 0$, if, and only if $\frac{\partial^2 c(e_T, e)}{\partial e_T \partial e} > 0$.

The intuition is that the manager's opportunities for bribe-taking induce agent effort substitution away from traffic management if, and only if, escorting makes the marginal unit of effort of traffic management costlier to the agent. This assumption is reasonable in our context, where the agent has limited time and attention to allocate to multiple tasks.

Proposition 2 provides sufficient conditions under which an increase in e reduces the wedge between the expected cost of violating vs. not violating the code:

Proposition 2 *a. If the manager's power to extort is large relative to the distribution of drivers, specifically if the fraction high bribe drivers who pay a bribe either because they commit an infraction or because they are extorted, is larger than that of drivers who committed an infraction, who pay a bribe anyways—i.e., $\left(\alpha > \frac{\sigma_{bv} - \sigma_{vb}}{1 - \sigma_{vb}}\right)$, then $s = 1$; b. If $s = 1$, more detaining, e , reduces the wedge if, and only if $\alpha \sigma_{b-v} > \sigma_{bv}$.*

The intuition for Proposition 2 is that, by targeting high-bribe drivers rather than drivers who have committed an infraction, the effect of effort to detain on the wedge (and thus on deterrence) may vanish if false charges predominate.

B.. The Proofs

The unique solution for e is $e^* = h\left(\frac{\beta^S}{b(s(\sigma_{vb} + (1 - \sigma_{vb})\alpha) + (1 - s)(\sigma_{bv}))}\right)$, where $h(x)$ is a decreasing function of x . The manager's bribe-taking opportunity b increases the agent's effort to detain.

Proof of Proposition 1 We solve the problem by backward induction. The agent's reaction function is determined by the agent's first order condition with respect to e_T . It is straightforward

to see that:

$$e_T^* = \frac{\beta}{2} - e$$

The manager's problem becomes:

$$\max_{e,s} y(e)b(s(\sigma_{vb} + (1 - \sigma_{vb})\alpha) + (1 - s)(\sigma_{bv})) + \frac{\beta^S \beta^A}{2} - \beta^S e.$$

The manager's first order condition with respect to e is:

$$y'(e)b(s(\sigma_{vb} + (1 - \sigma_{vb})\alpha) + (1 - s)(\sigma_{bv})) - \beta^S = 0.$$

Hence,

$$e^* = h\left(\frac{\beta^S}{b(s(\sigma_{vb} + (1 - \sigma_{vb})\alpha) + (1 - s)(\sigma_{bv}))}\right).$$

Without specifying $c(e_T, e)$, from the agent's maximized level of e_T^* , we can get

$$\frac{de_T^*}{de} = -\frac{c_{Te}(e_T, e)}{c_{TT}(e_T, e)} < 0$$

since we assume $c_{Te}(e_T, e) > 0$ and $c_{TT}(e_T, e) > 0$. □

Proof of Proposition 2 First, we show that the manager will order the agents to allocate all escorting effort towards drivers with $b_i = 1$ if, and only if, the marginal benefit to the manager of an additional unit of s is positive. The manager's first order condition with respect to s is:

$$\frac{\partial u^S}{\partial s} > 0.$$

Which holds if, and only if:

$$\sigma_{vb} + (1 - \sigma_{vb})\alpha - \sigma_{bv} > 0$$

which is equivalent to

$$\alpha > \frac{\sigma_{bv} - \sigma_{vb}}{1 - \sigma_{vb}}.$$

Then, we show that a higher level of s dis-incentivizes drivers to comply with the regulation. To see this, consider the expected payment by a driver who has committed a true infraction, and compare that to that of a driver who did not commit a violation. There are two cases: either the agent is focusing fully on drivers who can be induced to pay a bribe ($s = 1$); or she is focusing on those who have committed an infraction ($s = 0$).

In the first case ($s = 1$), the probability that a randomly selected driver who has committed a true infraction is escorted is $\sigma_{bv}e$, as only fraction σ_{bv} of drivers committed an infraction and, of those, the agent only attempts to escort fraction e . The driver's expected cost for these drivers is thus $\sigma_{bv}e(b)$. Since they have committed a true infraction, all of those escorted will be induced to pay a bribe. In contrast, the expected cost to a driver who made no true infraction is $\sigma_{b-v}e(\alpha b)$. Here, only fraction α will pay a bribe since with no charge, they do not pay a bribe.

In reality, it is reasonable to assume that drivers who would yield a bribe if they have committed an infraction are less likely to find it optimal to commit an infraction, compared to those who would not yield a bribe (e.g., because they are protected). Hence, a more realistic assumption would be $P(b = 1|v = 1) < P(b = 1|v = 0)$ —i.e., $\sigma_{bv} < \sigma_{b-v}$ ²⁵. This would further amplify the conclusions in this section that the wedge in the expected cost is reduced by α , and implies that, even when α is less than one, the resulting wedge could be zero. Indeed, the wedge in this case is $\Delta_1 = eb(\sigma_{bv} - \alpha\sigma_{b-v})$. This means that an additional unit of effort escorting decreases the wedge, in that case, if and only if $\alpha > \frac{\sigma_{bv}}{\sigma_{b-v}}$.

In the second case ($s = 0$), the same reasoning shows that the difference in the expected costs when committing a true infraction vs. not committing an infraction is $\Delta_2 = e(C^E + b) > 0$. That is, when the agent is focusing on drivers who have committed an infraction, the correlation is larger than when the agent focuses on drivers who can be induced to pay a bribe.

When $\alpha > \frac{\sigma_{bv} - \sigma_{vb}}{1 - \sigma_{vb}}$, the manager will choose $s = 1$. In this case, specifically when $\alpha > \frac{\sigma_{bv}}{\sigma_{b-v}}$, there would be no incentives to comply for a driver hypothetically choosing whether to comply or not.²⁶ Manager corruption creates a social cost since $\frac{de_T^*}{de} < 0$ from Proposition 1, and that cost is not compensated by an increase in the incentives to comply.

□

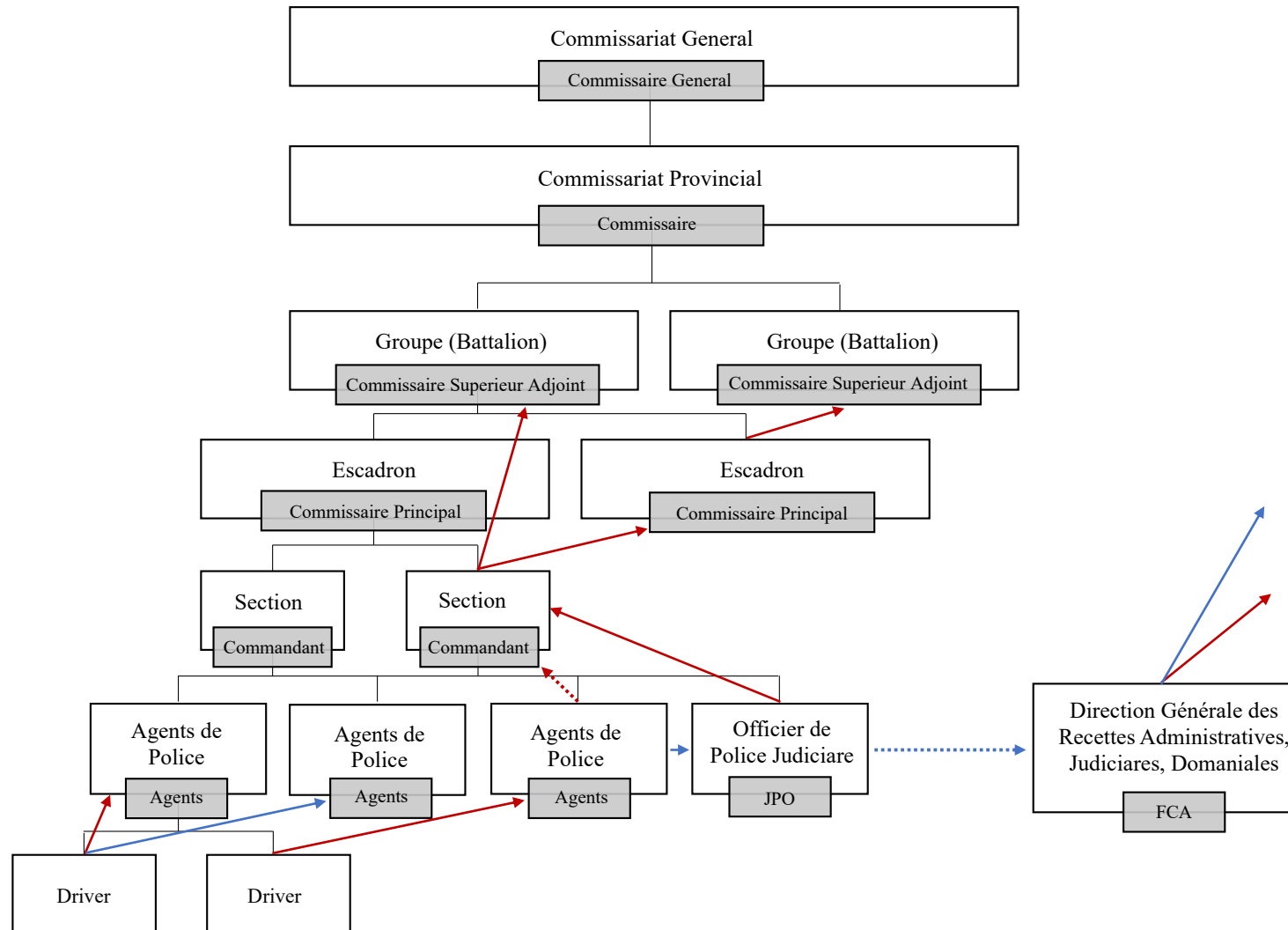
Appendix B. Appendix Figures and Tables

²⁵If $\sigma_{bv} = \sigma_{b-v}$, the wedge is $\Delta_1 = e\sigma_{bv}[(1 - \alpha)b]$. Clearly, the better the manager is at forging infractions, the lower the correlation between making an infraction and the expected cost. In the extreme, if the manager can arbitrarily make charges, the correlation is zero.

²⁶If $\sigma_{bv} = \sigma_{b-v}$, then $\Delta_1 = 0$, where there are still no incentives to comply for a hypothetical driver.

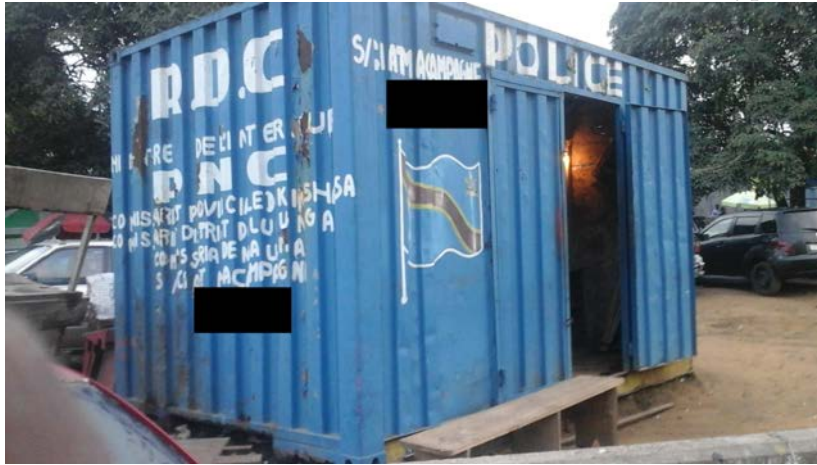
Figure A1: The Context

Panel A. Organizational Structure of the DRC's Traffic Police Agency



Notes: At the station, the managers were each assisted by two Judicial Police Officers (*JPO*), *Officiers de Police Judiciaire* in French. The Fine Collection Agency officials who were also located in the station (*FCAs*), *Direction Generale des Recettes Administratives et Domaniales* in French. Blue arrows indicate the official process. Red arrows indicate the unofficial process. Dashed arrows indicate that the transaction is infrequent. White-background boxes indicate the hierarchical division in the traffic police agency's hierarchy. Gray boxes indicate the title of the person leading the corresponding division described in the attached white-background box.

Panel B. Typical stations in Kinshasa



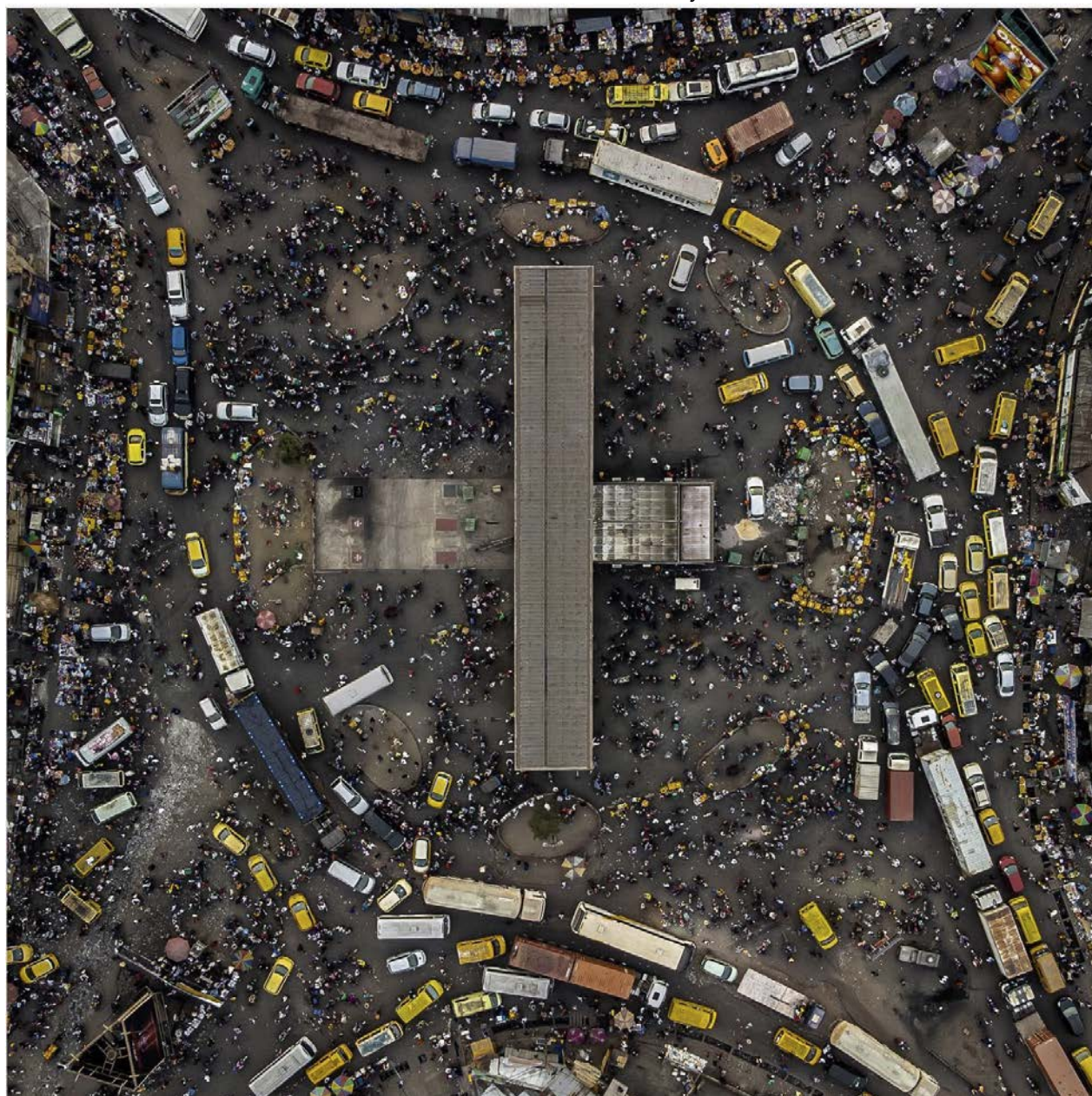
Notes: This figure shows four stations of the traffic police commissariat in the city of Kinshasa. These stations are similar to the eight stations included in this study. Source: research team.

Panel C. The City of Kinshasa



Notes: Map of the city of Kinshasa. The River Congo separates Brazzaville, the capital of the Republic of the Congo (North side) from Kinshasa, the capital of the Democratic Republic of the Congo (southern side). Source: Google maps.

Panel D. One Roundabout Controlled by the Battalion



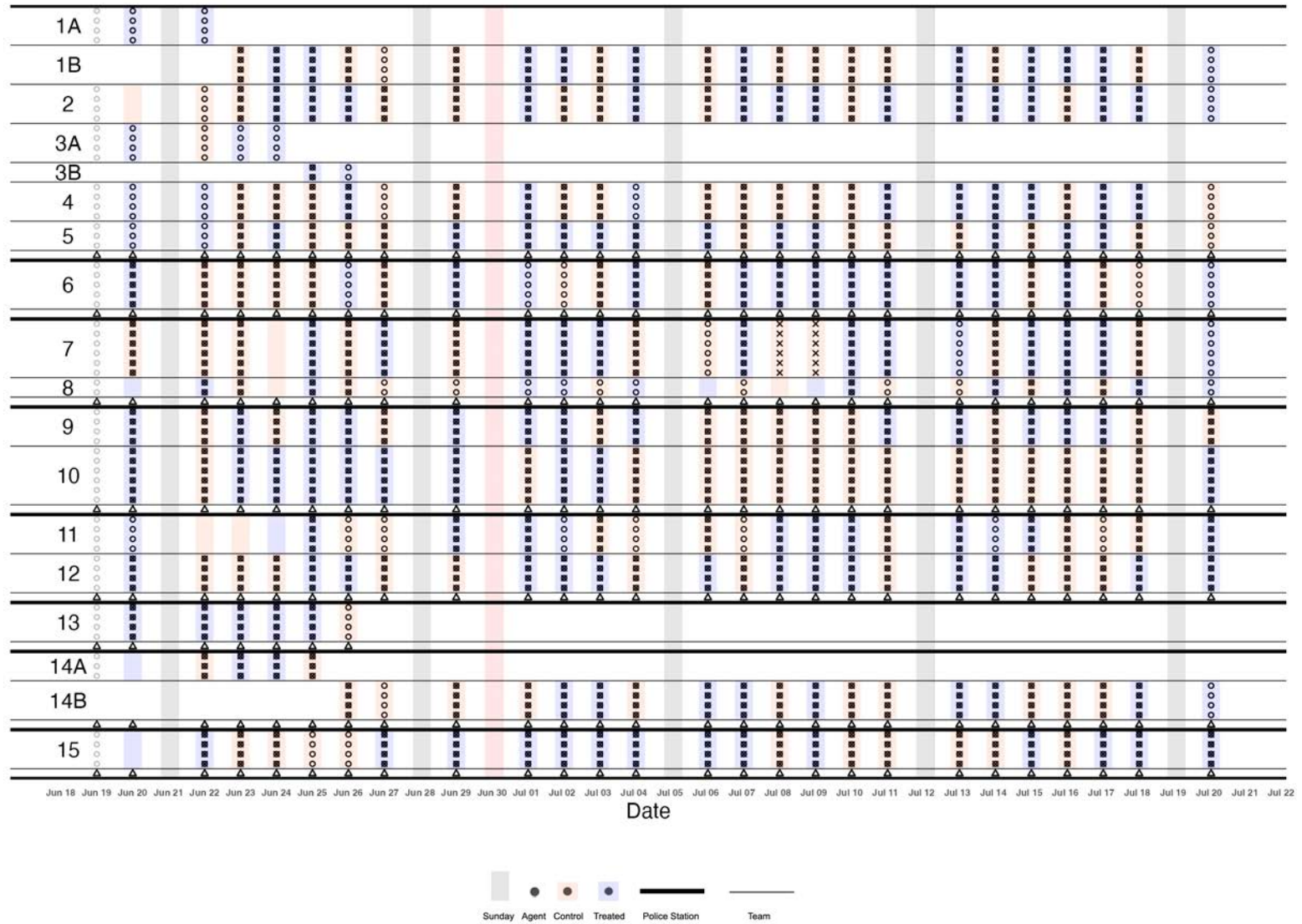
Notes: Image of one of Kinshasa's roundabouts, where traffic is controlled by agents of the traffic police battalion. The roundabout is home to four teams of agents, who are deployed to four different intersections/street-corners.
Source: Oia Photography.

Panel E. Traffic Pull-Overs



Notes: The process of pulling-over the driver and escorting them with the vehicle into the station was often long, involved negotiations, assessment of whether the driver had a powerful protector, as well as the use of force to take the vehicle away from the driver's possession. **Source for images on top:** [Amen Ministry TV](#), *Bitaka tracasserie ya ba roulage na kin ekomi koleka makasi*. **Source for image on bottom:** [Youtube Congo Avenir](#).

Figure A2: Data Collection Sample: Day Zero + Experiment Sample (With Assignment to the Encouragement)



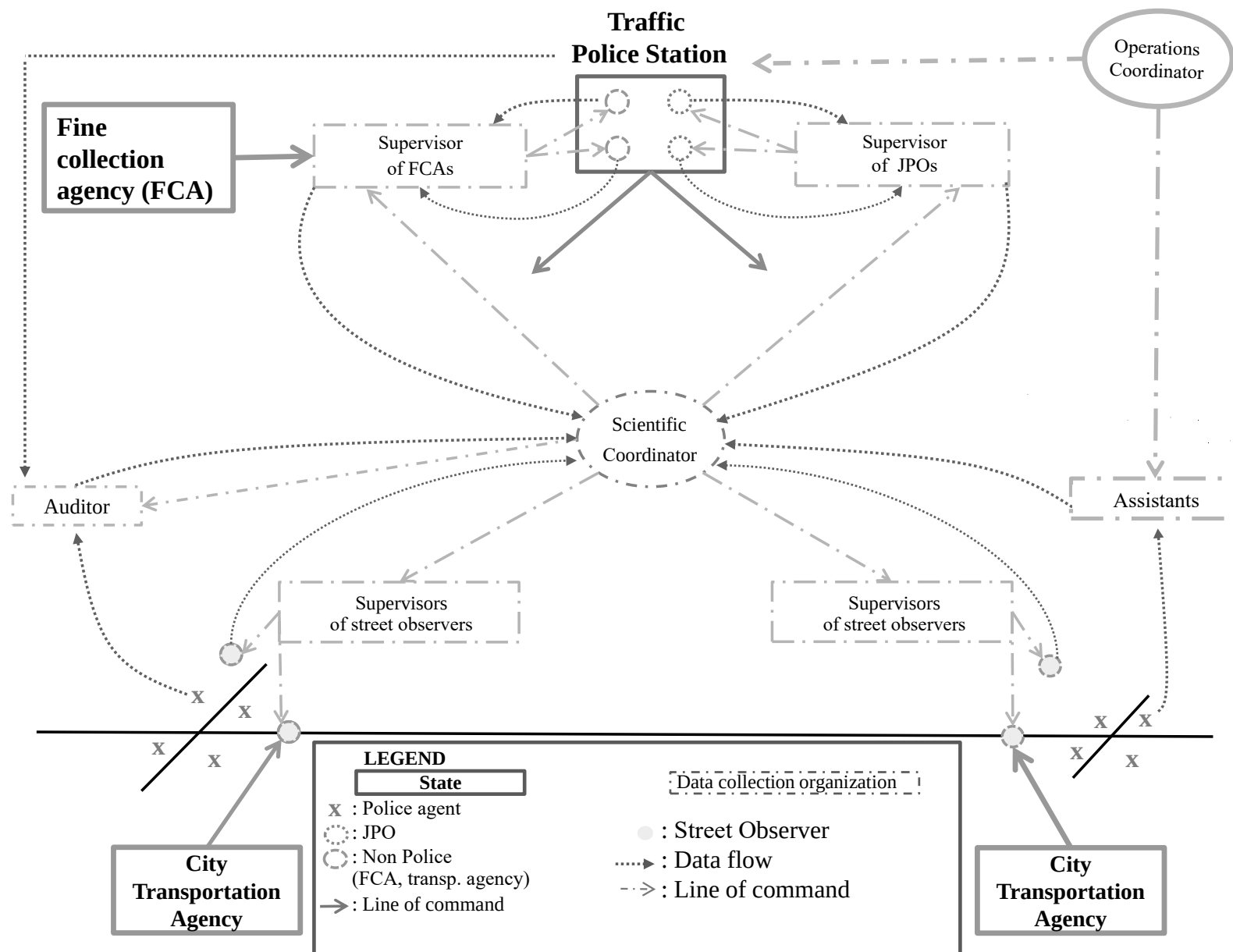
Notes: Team-days in the experiment sample assigned to the encouragement and those assigned to no encouragement form a partition of the experiment sample and are represented in blue and red, respectively. In addition to the data collection sample represented by the circles, triangles also present the *Unassigned intersections' sample*.

Figure A3: Assignment of the Quota Reduction Encouragement (Examples)



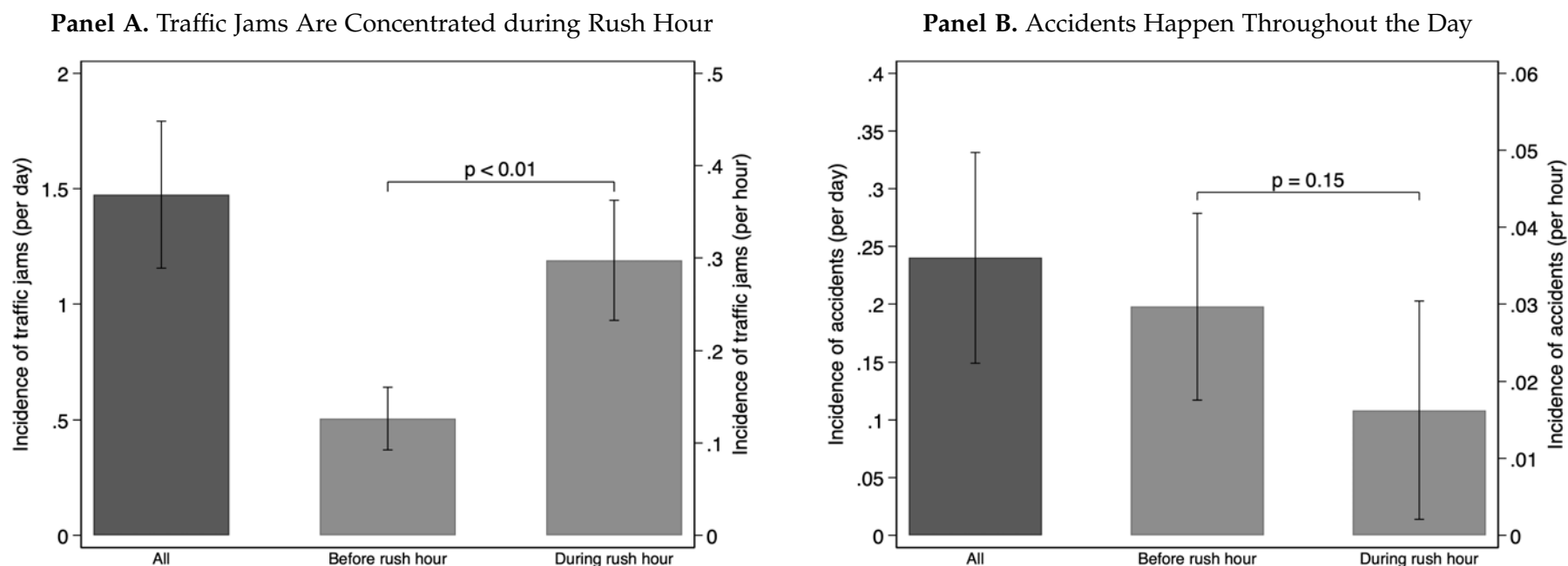
Notes: This figure shows the assignment of the quota reduction *encouragement* to the corresponding intersections in four study days as an example. The experimental sample covers 337 teams of agents/intersections-days, which are also the unit of assignment. **Source:** experiment's randomization calendar. **Sample:** Experiment Sample; the construction of the experiment sample is described in Section 3.B.

Figure A4: The Five Data Collection Branches



Notes: This figure presents the five data collection branches for one typical station of the eight in which the five branches took place. JPO stands for Judicial Police Officer (Officier de Police Judiciaire in French) and FCA stands for Fine Collection Agency (DGRAD, Division Generale des Recettes Administratives et Domaniales in French).

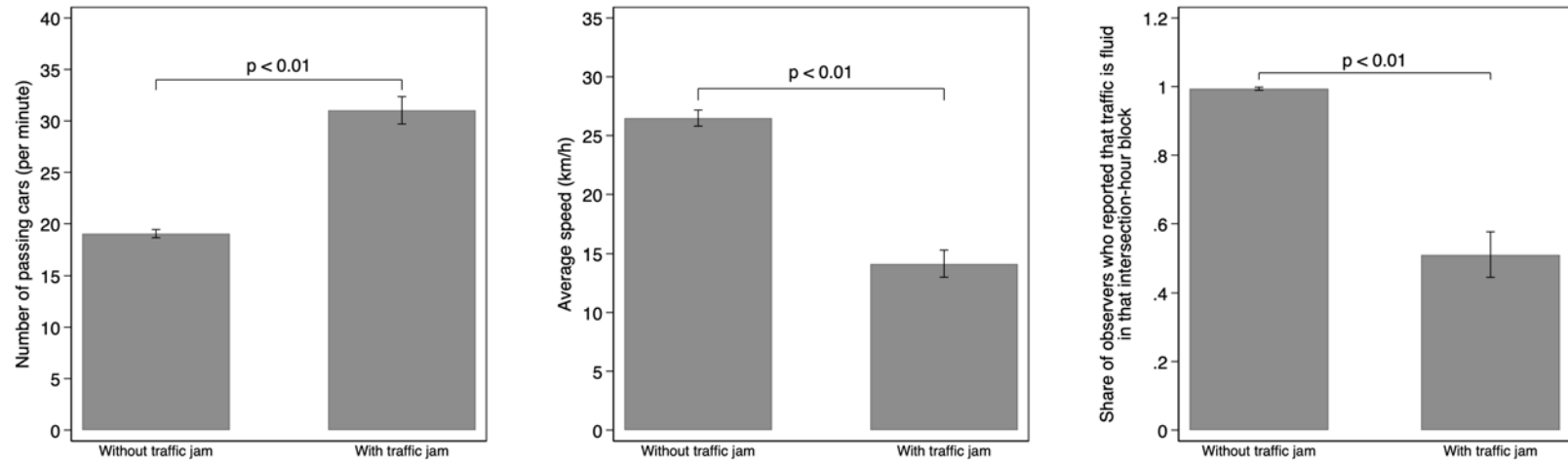
Figure A5: Accidents and Traffic Jams, Before and After the Rush Hour



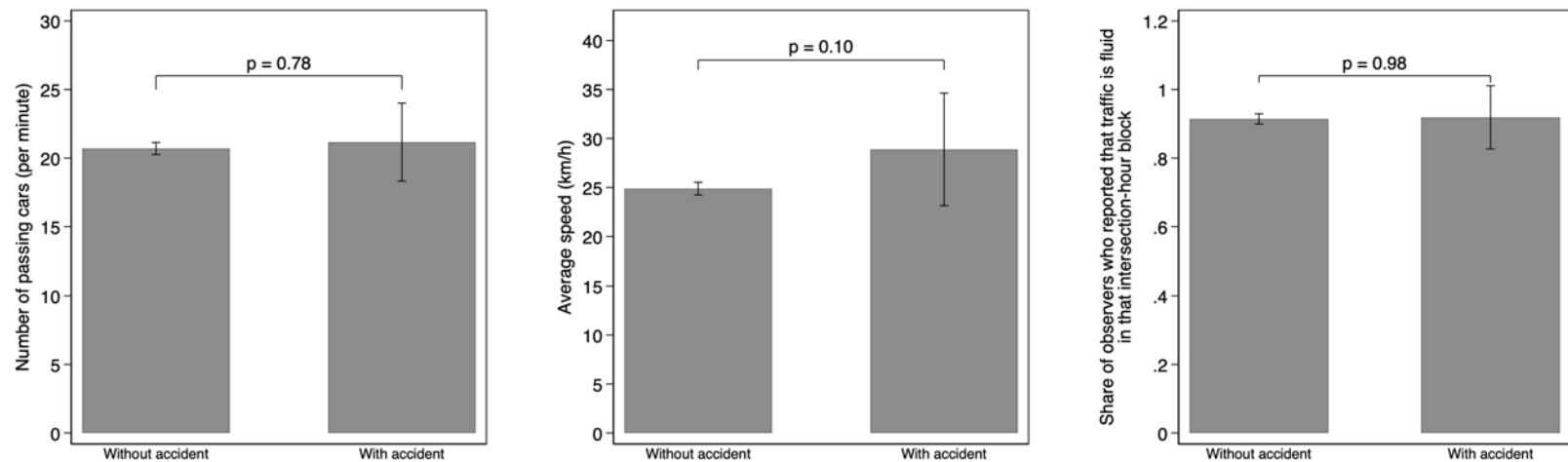
Notes: In Panel A, the first bar presents the mean incidence of traffic jams in the day, the second and third bars present the mean incidence of traffic jams *per hour*. The rush hour cutoff is defined as 3pm. The results are robust to different rush hour cutoffs (1pm, 2pm and 4pm). In Panel B, the first bar presents the mean incidence of accidents in the day, the second and third bar represent the mean incidence of accidents *per hour*, for the sample before the rush hour and during the rush hour, respectively. Brackets indicate the 95% confidence interval. p is the p -value obtained from a two-sample t -test of the equality of means. **Source:** Branch 3 (street observers), described in Section 4B. **Sample:** data collection sample. Of the 351 team-days of the data collection sample, street observers' data collection was active in 346 team-days; for those, there were 345 team-day with nonmissing traffic jam or accident information as observed by the street observers, thus the street observers source (Branch 3) traffic jam dataset is composed of 345 nonmissing observations (Table A1).

Figure A6: Correlates of a Traffic Jam and of an Accident

Panel A: Traffic Jams Correlates



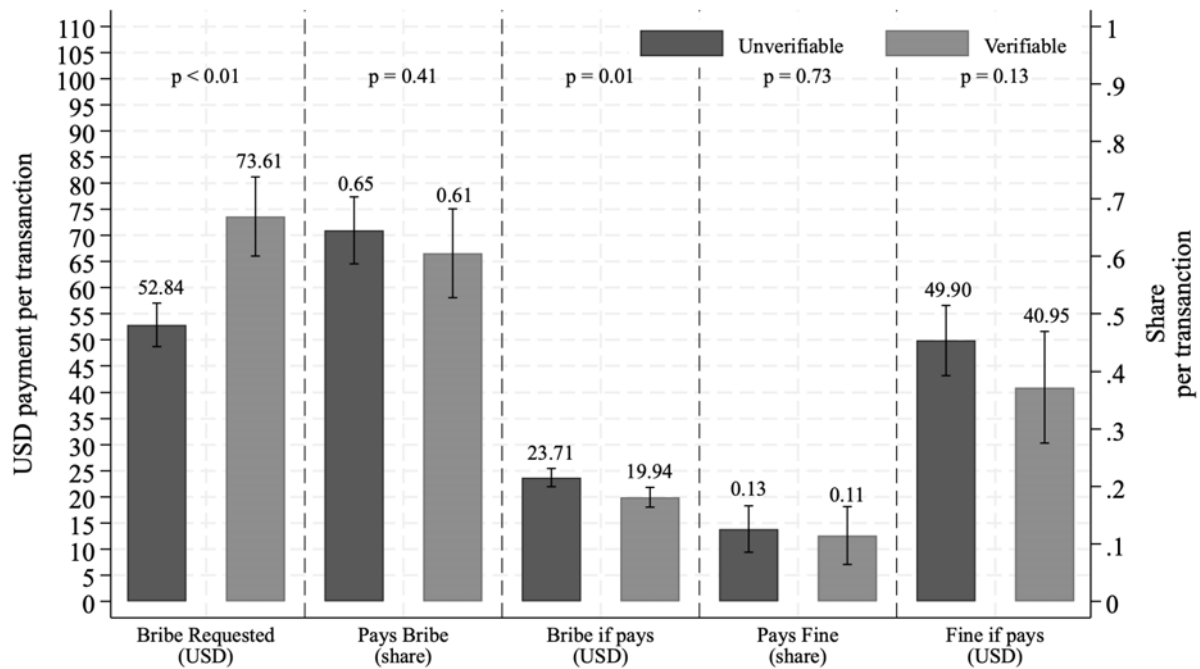
Panel B: Accidents Correlates



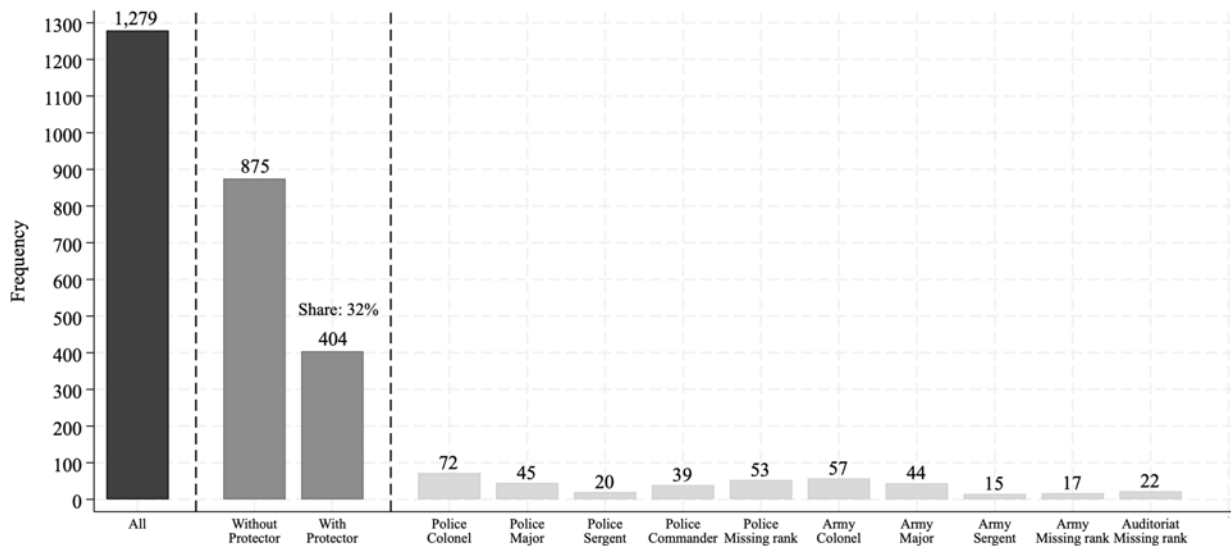
Notes: In Panel A, the mean of the corresponding variable is reported for intersection-hour blocks in which there was no traffic jam recorded (left bar) and hour blocks in which there was (right bar). Panel B does the same for intersection-hour blocks in which there was no accident recorded (left bar) and those in which there was one (right bar). In the leftmost quadrants, the variable analyzed is the number of vehicles that are present at the intersection in the last minute of each hour at each intersection. In the middle quadrants, the variable analyzed is the average speed of a vehicle in the last minute of each hour. In the rightmost quadrants, the variable analyzed is the share of observers who report, in each intersection-hour block, that traffic is fluid. Error bars indicate the 95% confidence interval. p is the p -value obtained from a two-sample t -test of equality of means. **Source:** Branch 3. **Sample:** data collection sample.

Figure A7: Fact 2—Breakdown of Protectors and of Allegations

Panel A. *In the Station, Unverifiable Allegations Yielded Equally Large Bribes as the Rest*



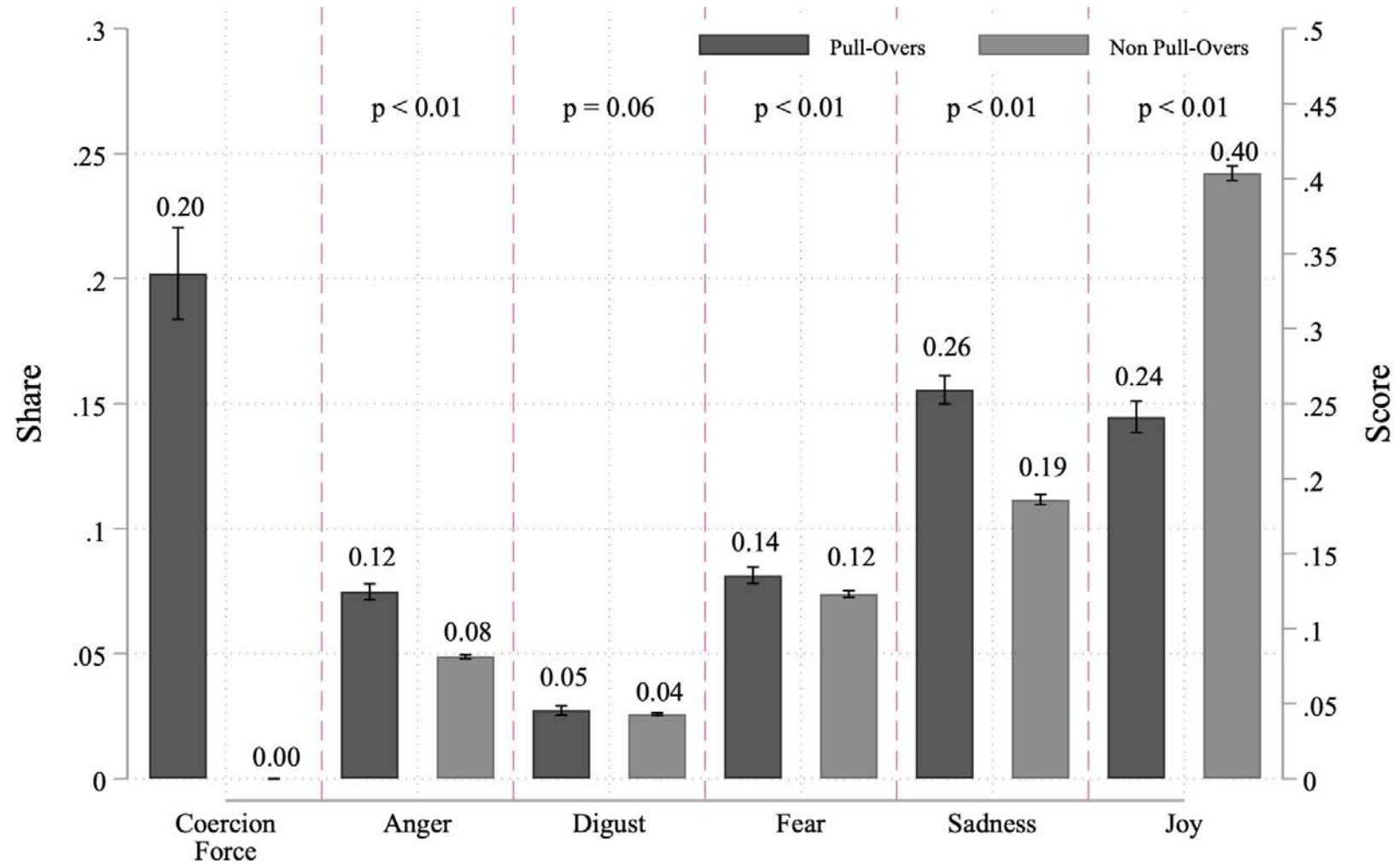
Panel B. *In the Stations, Drivers Had to Call Powerful “Protectors” to Intimidate Back the Officers*



Notes: **Panel A** documents the steps in the bribe negotiation process between the JPOs and the drivers in the station, for alleged infractions that are unverifiable vs. those that are verifiable. Allegations are coded at driver level: “Unverifiable” driver is a driver with all allegations that were classified as unverifiable; “Verifiable” driver is a driver that had some verifiable allegations (i.e., the driver with all verifiable allegations or the driver with some unverifiable allegations and some verifiable allegations.) “Unverifiable” and “Verifiable” are therefore a partition of the drivers. **Panel B** presents the frequency of drivers who have a protector from a given state institution (left quadrant) and the military rank of the protectors (right quadrant, ranked from top to bottom from higher to lower rank). The “Auditoriat [Militaire]” is the office of the prosecution for military affairs; S.A.D are State Administrators; ACCO – Association des Chauffeurs du Congo in French, is the national association of drivers. Six colonels of an unreported state agency were assigned to the police. **Source:** Branch 1 (JPOs), described in Section 4B. **Sample.** Data collection sample.

Figure A8: Natural Language Processing of Traffic Pull-Overs' Descriptions

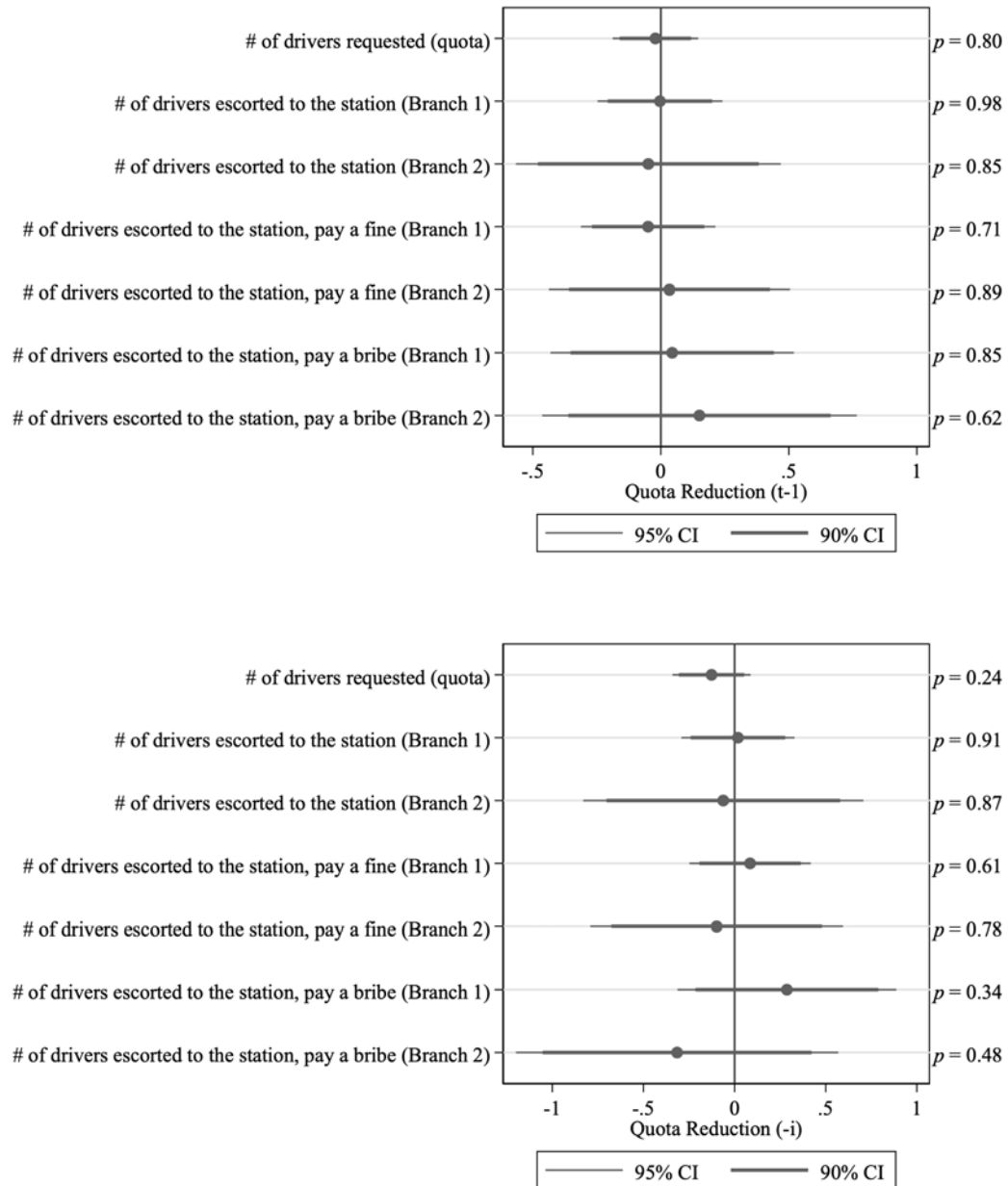
Panel A. Pull-Overs vs. All Other Events of Agent-Driver Interaction



Notes: Natural Language Processing was applied in the subset of agent-driver interactions that are pull-overs vs. the rest to produce a score for each emotion for each event. There are 13,793 agent-drivers interaction in the data collection sample, of which 1,841 are pull-overs. [Study Documentation](#), Section 6, details the construction of this score using NLP of the open-ended descriptions of each agent-driver event. **Source:** Branch 3 (street observers), described in Section 4B.

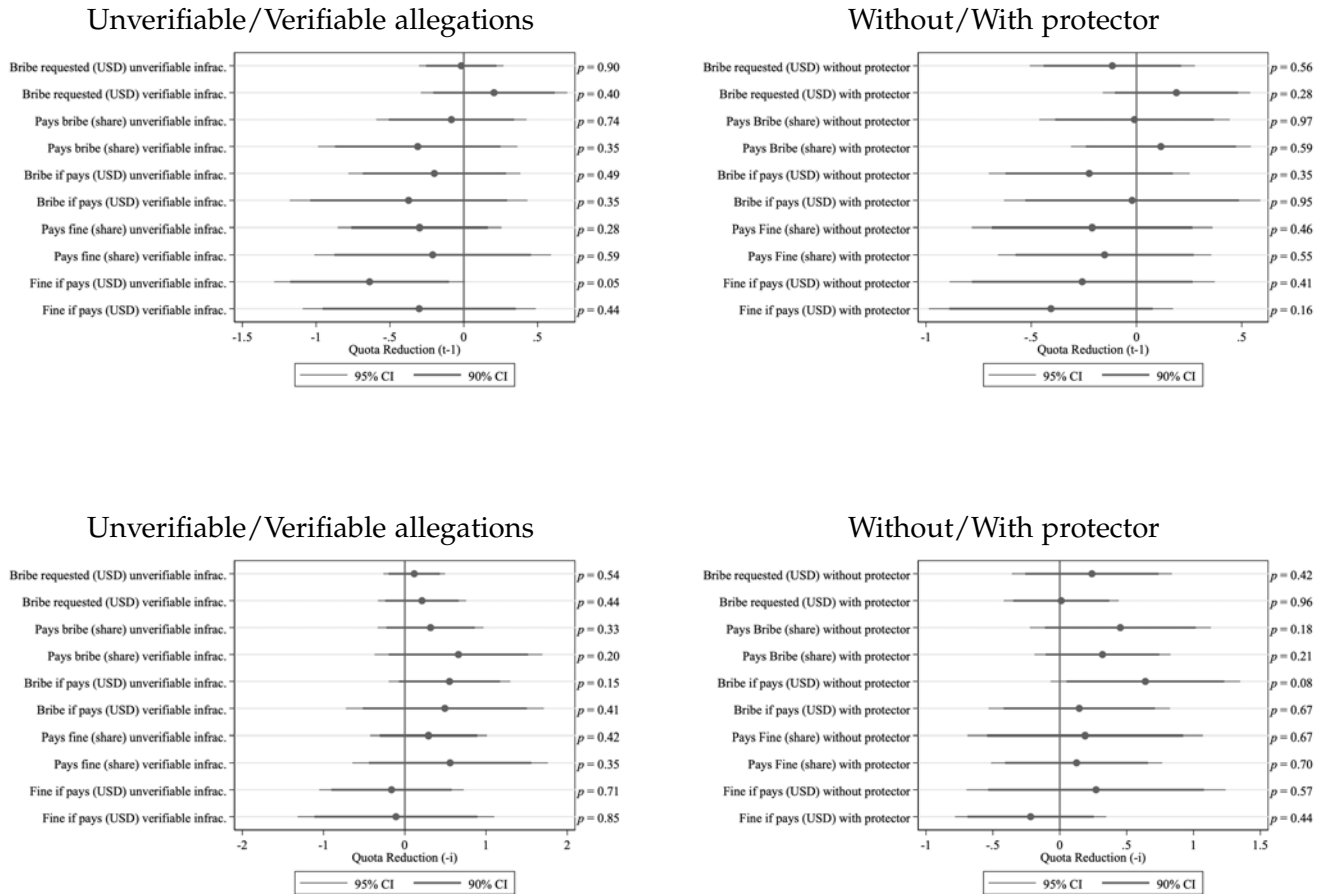
Figure A9: Interference on Descriptive Analysis Variables

Panel A. Fact 1 Variables



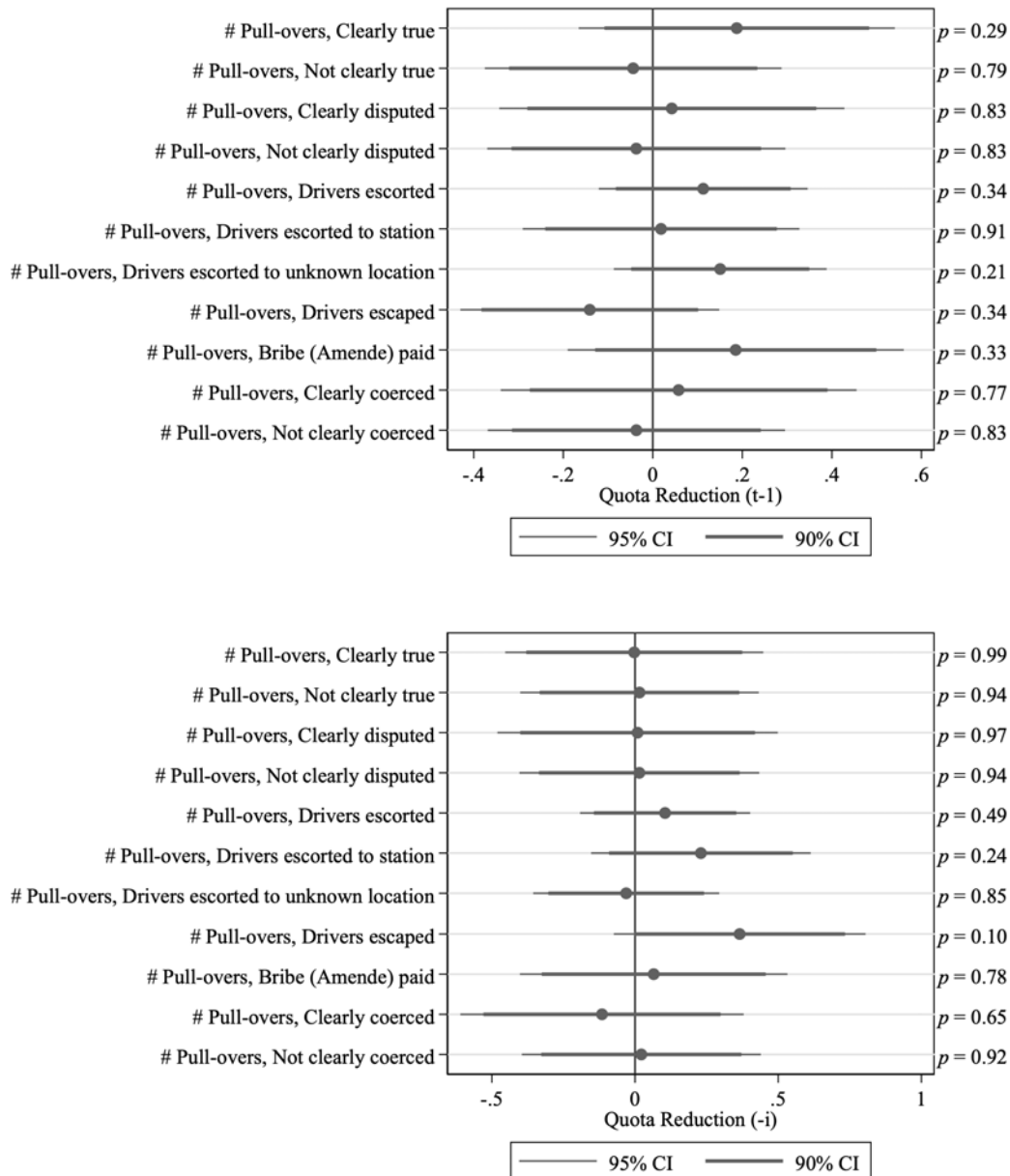
Notes: This figure presents the estimates of Equation 1 with Fact 1 variables as dependent variables on various indicators based on assignment. In Panel A, the independent variable is an indicator taking value 1 if the team was treated in the preceding period, and 0 otherwise; in Panel B, it is an indicator for whether at least one other team of the same station was treated in the same period. Across panels, the dependent variables in separate regressions are the variables used in the first part of the descriptive analysis, and regressions include team and day fixed effects. **Sample:** Descriptive sample.

Panel B. Fact 2 Variables



Notes: This figure presents the estimates of Equation 1 with Fact 2 variables as dependent variables on various indicators based on assignment. In Panel A, the independent variable is an indicator taking value 1 if the team was treated in the preceding period, and 0 otherwise; in Panel B, it is an indicator for whether at least one other team of the same station was treated in the same period. Across panels, the dependent variables in separate regressions are the variables used in the first part of the descriptive analysis, and regressions include team and day fixed effects. **Sample:** Descriptive sample.

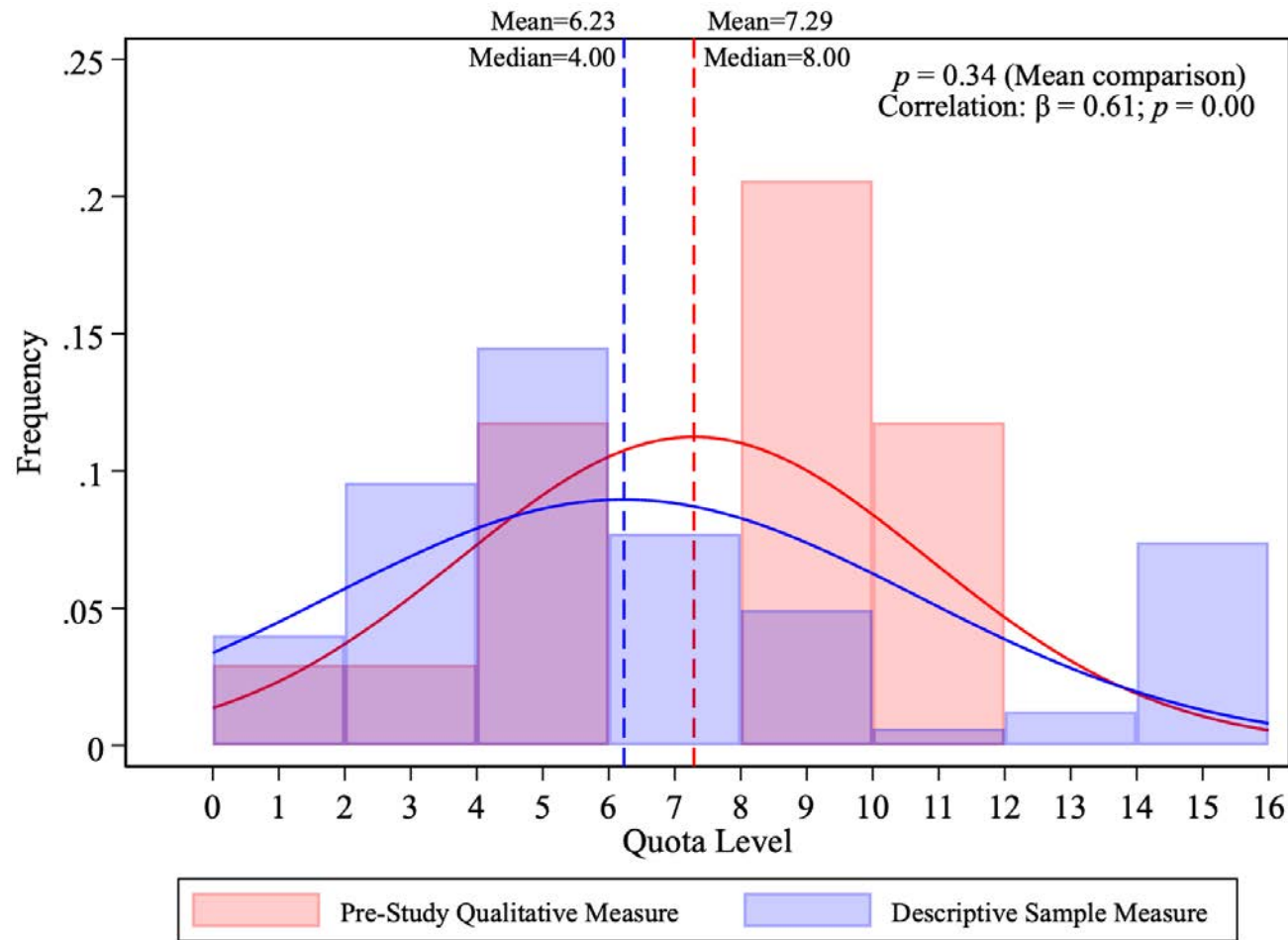
Panel C. Fact 3 variables



Notes: This figure presents the estimates of Equation 1 with Fact 3 variables as dependent variables on various indicators based on assignment. In Panel A, the independent variable is an indicator taking value 1 if the team was treated in the preceding period, and 0 otherwise; in Panel B, it is an indicator for whether at least one other team of the same station was treated in the same period. Across panels, the dependent variables in separate regressions are the variables used in the first part of the descriptive analysis, and regressions include team and day fixed effects. **Sample:** Descriptive sample.

Figure A10: Key Study Outcomes, Before and During the Study

Panel A. The Quota Level



This figure presents the distribution of the quota levels (Panel A) and station bribe income (Panel B) before vs. during the study. **Source and sample for data before the study:** The quota level and station bribe income before the study are estimates of each intersection's natural quota and of each station's total income from bribes, obtained through preparatory qualitative fieldwork and represents the reported averages between January and May 2015. **Source and sample for data during the study:** the quota level and station bribe income data are the intersection-day observations obtained from the *descriptive sample* during the study (hence excluding observations assigned to quota reduction). The quota levels during the study are the intersection-day level 327 observations of quota levels, obtained as described in Section C. The descriptive sample, described in Section 5A, is constituted of 171 team-days. Of those, the quota level is nonmissing for 160 days.

Panel B. The Station Bribes Revenues—Sources: Branch 1 (Upper), Branch 2 (Lower)

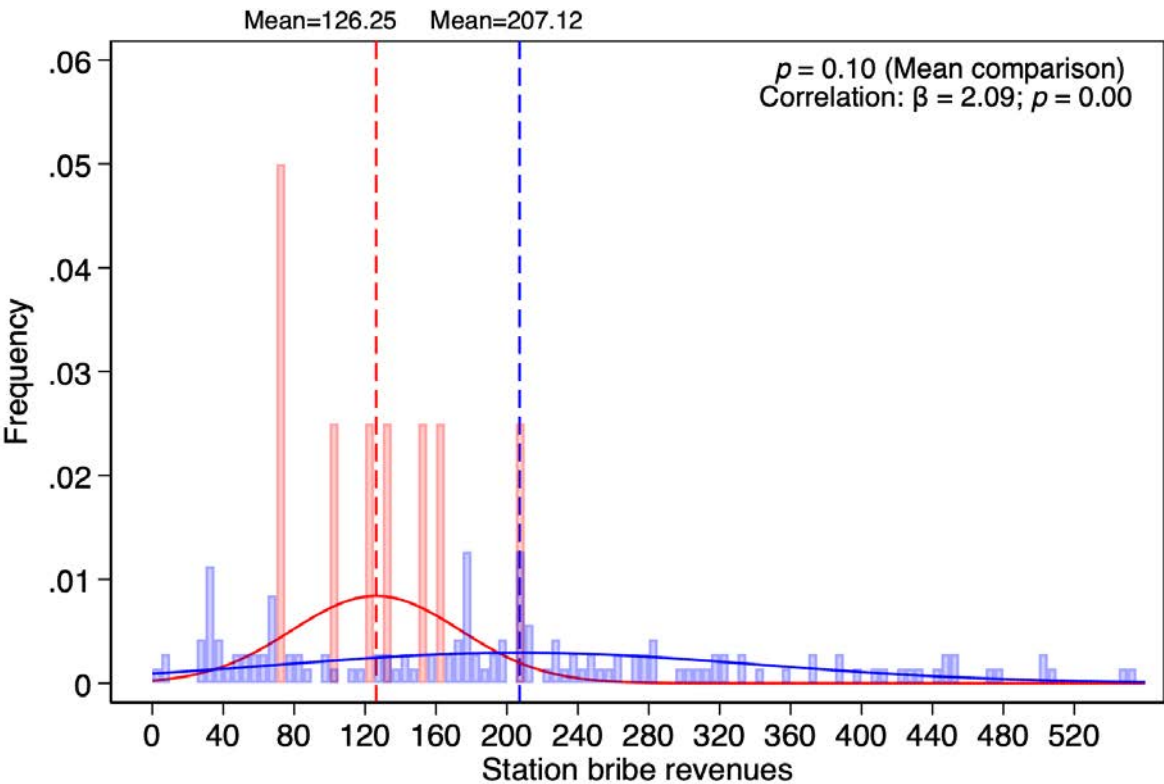
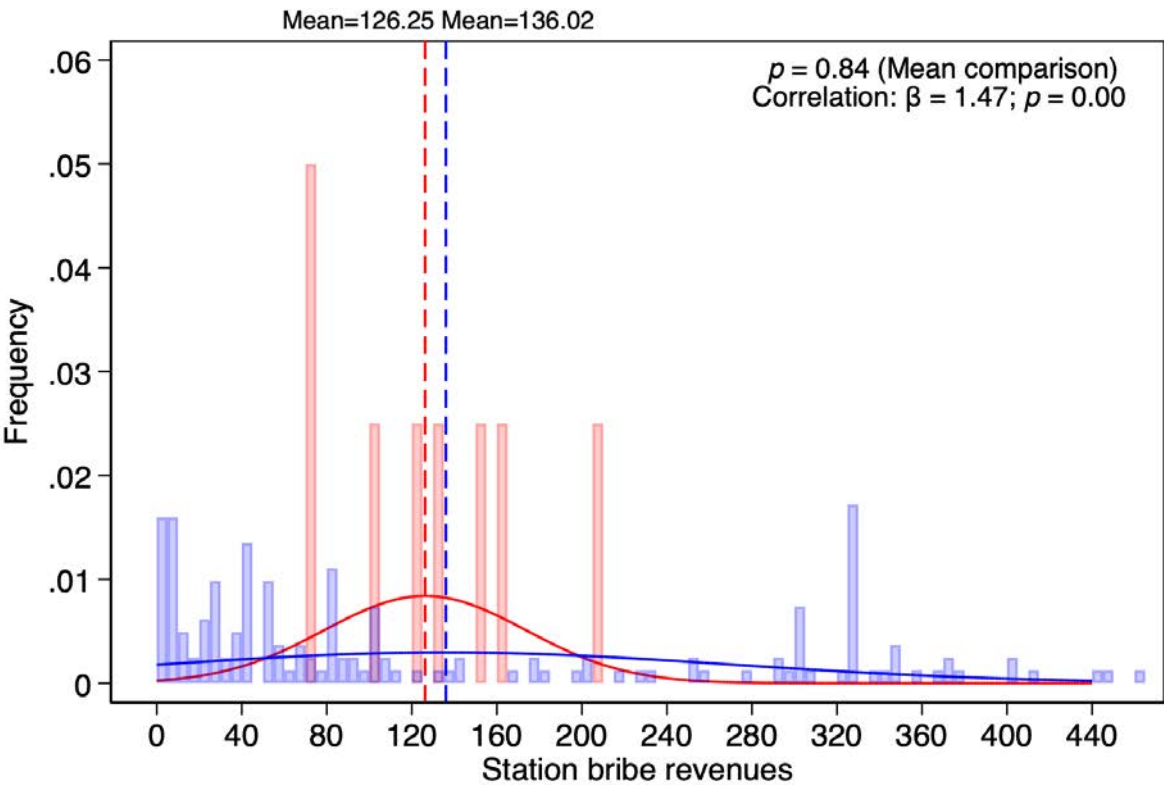
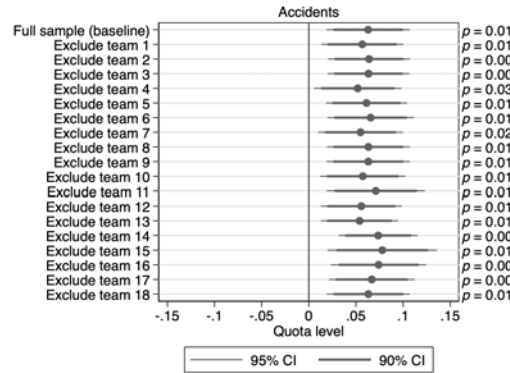
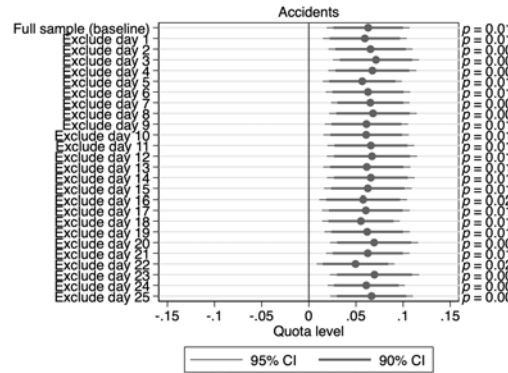


Figure A11: Effect of the Quota on the First Mandate: Traffic Outcomes—Sensitivity to Subsets of the Sample

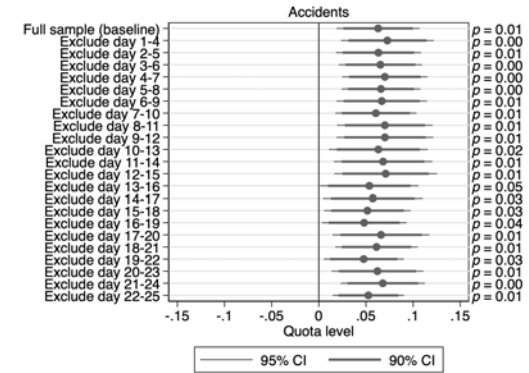
(a) Accidents Incidence: Removing team each by each



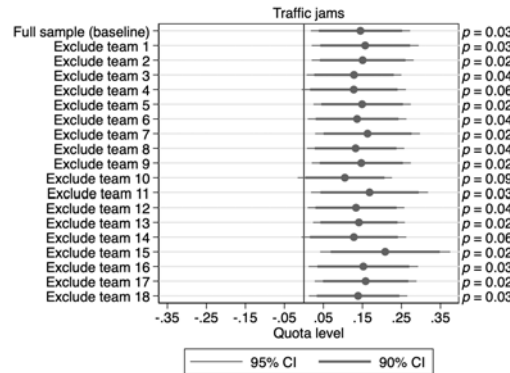
(b) Accidents Incidence: Removing day each by each



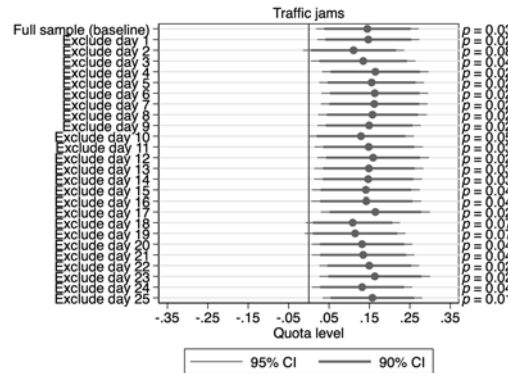
(c) Accidents Incidence: Removing 4 consecutive days each by each



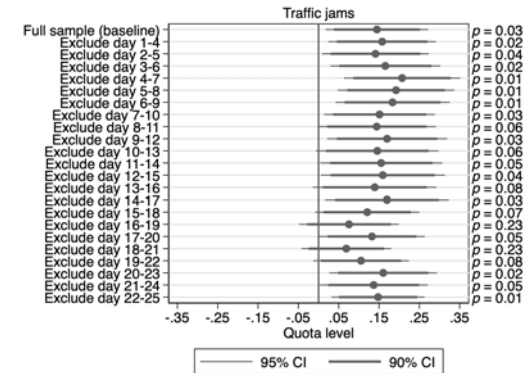
(d) Traffic Jams Incidence: Removing team each by each



(e) Traffic Jams Incidence: Removing day each by each



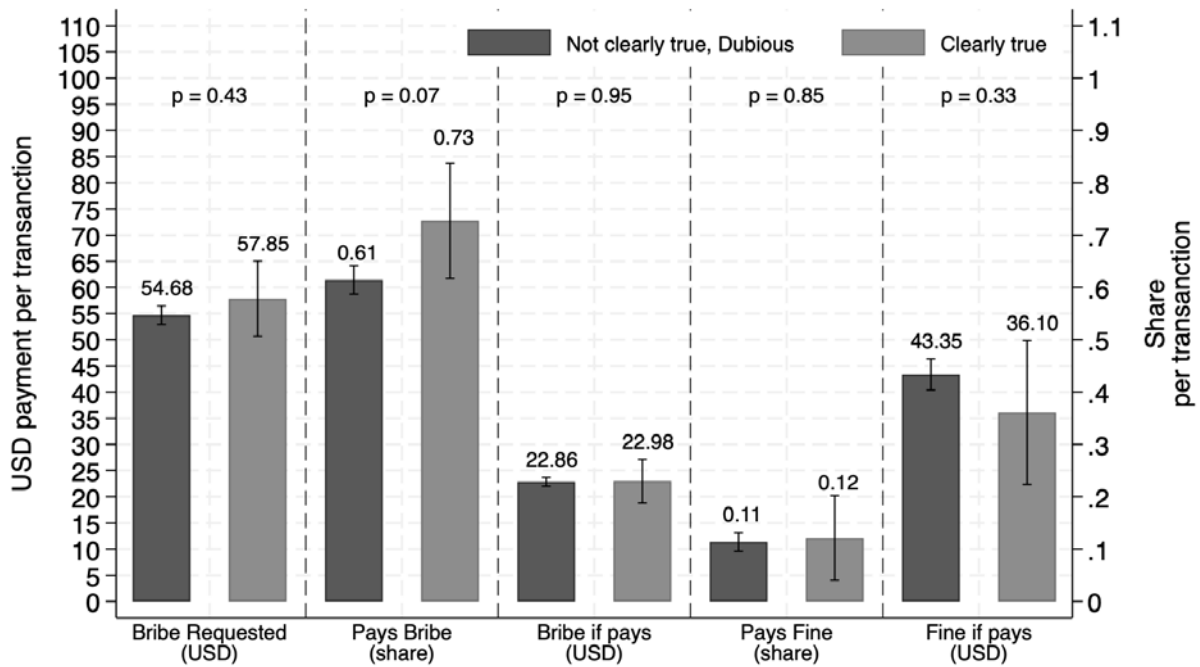
(f) Traffic Jams Incidence: Removing 4 consecutive days each by each



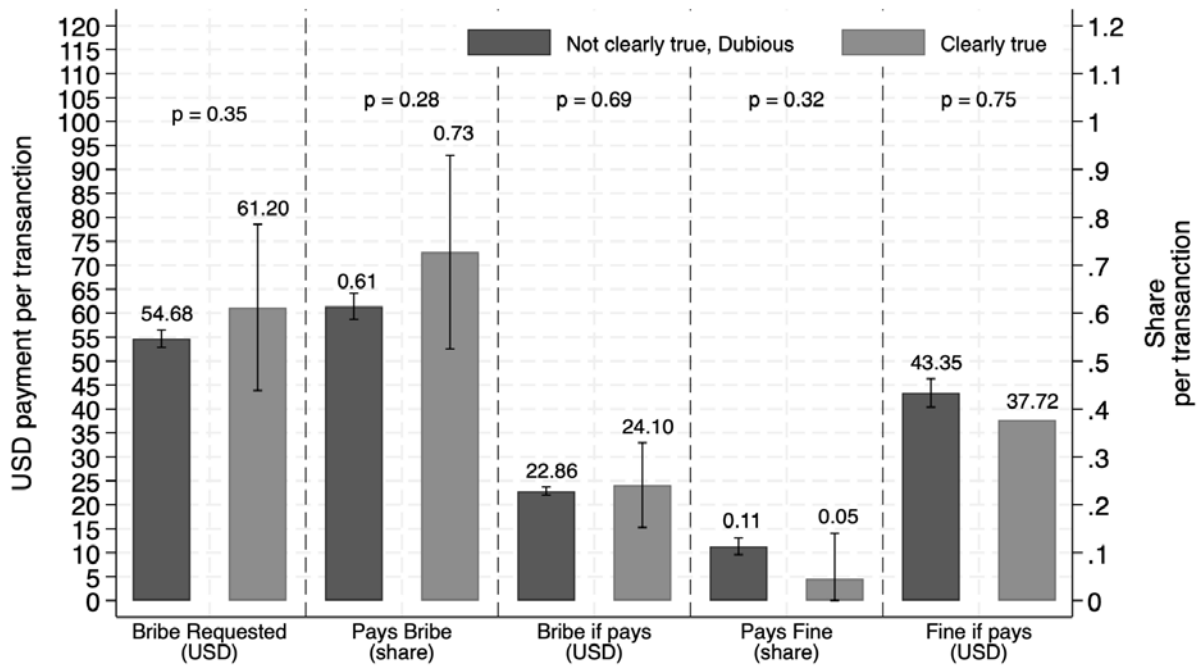
Notes: Panel A presents the coefficient estimates and confidence intervals of coefficient β from Equation 1 with dependent variable Incidence of accidents, as defined in Table 3, for various samples. In Panels (a) and (d), the sample excludes one team at a time; in Panels (b) and (e), the sample excludes one day at a time; in Panels (c) and (f), the sample excludes blocks of 4 days at a time. **Source:** The source for the reports of accidents and traffic jams used in this calculation is Branch 3 (street observers), described in Section 4B. Panel B presents the coefficient estimates and confidence intervals of coefficient β from Equation 1 with dependent variable Incidence of traffic jams, as defined in Table 3, for various samples. **Source:** The source for the reports of accidents and traffic jams used in this calculation is Branch 3 (street observers), described in Section 4B. **Sample:** Experiment Sample. The experiment sample covers 337 team-day observations, which are also the units of assignment. Of those, 5 observations have missing traffic data, resulting in a sample of 332 team-days.

Figure A12: Certainly True Allegations from the Street Linked to Outcomes in the Stations

Panel A. True Infractions and Station Outcomes—Escort to Station or to Unknown

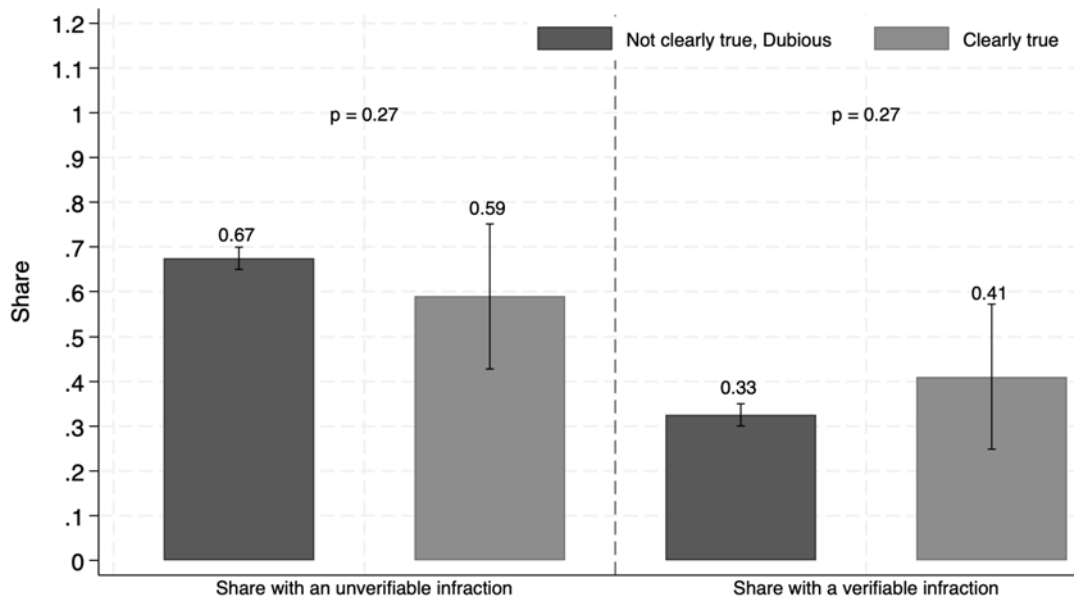


Panel B. True Infractions and Station Outcomes—Only Escort to Station

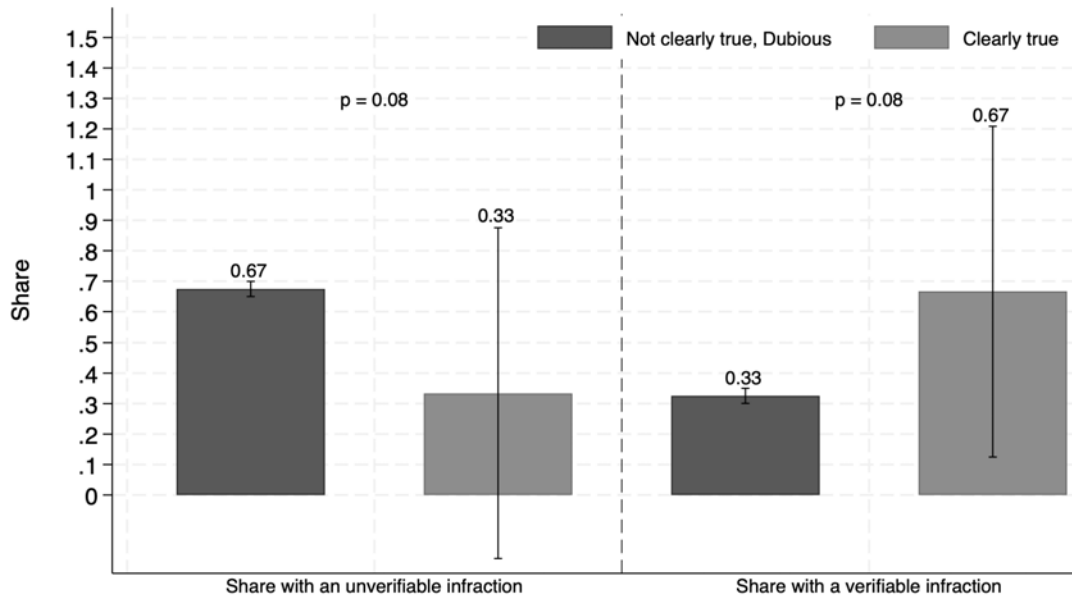


Notes: next page.

Panel C. True Infractions and Allegation Verifiability—Escort to Station or to Unknown



Panel D. True Infractions and Allegation Verifiability—Escort to Station Only



Notes: This figure presents the analysis from the linked dataset connecting Branches 1/2 vehicle events in the station to Branch 3 vehicle events in the street. The linking procedure and outcomes are described in [Study Documentation](#), Section 7. In Panels A and C, the sample includes the outcome of linking all 245 street events of pull-over with a true infraction that result in escorting to either the station or to an unknown location. In Panels B and D, the sample includes the outcome of linking all 72 street events of pull-over with a true infraction that result in escorting to the station only. Dark bars represent the means for unprotected drivers. Light bars represent the means for protected drivers. p denotes the p-value for a test of equality between means in dark vs. light bars. Brackets indicate 95% confidence intervals. **Sample:** Data collection and unassigned intersections' sample.

Figure A13: Drivers' Responses—Number of Drivers Who Pass by (per minute) Through the Experiment Sample's Intersections



Notes: This figure presents the mean number of drivers passing through an intersection per minute, by day. **Source:** Branch 3 (street observers), described in Section 4B. **Sample:** Experiment Sample.

Table A1: Samples

Panel A. Samples and Sources

I. Samples	(1)	(2)	(3)	(4)	(5)	(6)		
	Data collection sample (3) + (4) + (5)	Descriptive sample (3) + (4)	Day 0 sample	No Encouragement	Encouragement	Experiment sample (4) + (5)		
# Team-days	351	171	14	157	180	337		
# Team-days with observed quota	335	160	9	151	175	326		
# Team-days with Branch 3 collection	346	167	13	154	179	333		
# Team-days with Branch 1 collection	264	119	0	119	145	264		
# Team-days with Branch 2 collection	180	89	7	82	91	173		
II. Samples, by variable (incl. missingness)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
	Data collection sample (3) + (4) + (5)	Descriptive sample (3) + (4)	Day 0 sample	No Encouragement	Encouragement	Experiment sample (4) + (5)	Experiment sample with observed quota	Source
Section 6B: Street events (Branch 3, street observers)								
Traffic jams	345	167	13	154	178	332	322	Branch 3
Accidents	345	167	13	154	178	332	322	Branch 3
Traffic pull-overs	341	165	13	152	176	328	318	Branch 3
Time away due to escorts	338	165	13	151	174	325	315	Branch 3
Section 6C: Escorts								
Unprotected/protected (station, Branch 1)	264	119	0	119	145	264	259	Branch 1
Unverifiable/verifiable (station, Branch 1)	230	102	0	102	128	230	225	Branch 1
(Not) clearly true (street, Branch 3)	321	151	0	151	170	321	311	Branch 3
Section 7: Revenue								
From station (Branch 1)	264	119	0	119	145	264	259	Branch 1
From station (Branch 2)	180	89	7	82	91	173	173	Branch 2
From street (Branch 3)	339	163	11	152	176	328	318	Branch 3

Panel B. Number of Recorded Interactions Inside Stations and in the Street, and Agents

Sample	# escorts in station (Branch 1)	# escorts in station (Branch 2)	# interactions in street	# unique agents working in street
Data collection sample	1,140	728	13,793	75
Descriptive sample	535	373	6,629	73
Day 0 sample	0	29	539	62
No encouragement	535	344	6,090	67
Encouragement	605	355	7,164	75
Experiment sample	1,140	699	13,254	75
Unass. int. sample	95	527	—	—

Notes: Panel A presents the samples and data sources for the variables used in the experiment. Panel B presents the number of recorded interactions inside stations and in the street, and number of agents involved, in the different samples. The number of team-days included on day 0, 14, are the team-days for which we have either station (Branches 1 or 2) or street (Branch 3) data sources. The target number of teams for Day Zero was 15, but one could not be worked with yet.

Table A2: Summary of Data Collection Branches

	Data type	Who records	Where	Whose behavior	What is gathered and how
Branch 1	reported	JPOs	stations	JPOs	By team-day: quota. By vehicle: hour of arrival, vehicle details, alleged infraction, bribe paid by driver to JPO himself, whether driver was charged and fine imposed based on firsthand evidence; protector's rank and institution, based on the private conversation between JPO and protector.
Branch 2	<i>observed</i>	FCAs	stations	JPOs	By team-day: quota, based on station public conversations. By vehicle: station bribes paid by driver to JPO, alleged infraction. Unusable: protector information, from driver-JPO private conversation.
Branch 2	reported	FCAs	stations	FCAs	For each charged vehicle charged: fine given.
Branch 3	<i>observed</i>	Street Observers	intersections	Agents	By agent-driver event: bribe paid, escorting, open-ended description of event (including, unprompted, observer's assessment whether alleged infraction is true/false). By intersection-hour: traffic.
Branch 4	reported	Assistants	intersections	Manager	Quota, retrocession
Branch 5	reported	Auditor	intersections	Manager	Quota

Notes: This table describes details of data collection for each data collection branch.

Table A3: Relationship between Overlapping Reports across Sources

Panel A. Relationship between Branch 1 (JPO) and Branch 2 (FCA) Reports

	# Escorts		Bribe Rev (USD)		Fine Rev (USD)		# Protected		# Protected Police		# Protected Military		# Unverifiable		# Verifiable	
	Br.1 (1)	Br.2 (2)	Br.1 (3)	Br.2 (4)	Br.1 (5)	Br.2 (6)	Br.1 (7)	Br.2 (8)	Br.1 (9)	Br.2 (10)	Br.1 (11)	Br.2 (12)	Br.1 (13)	Br.2 (14)	Br.1 (15)	Br.2 (16)
Branch 2 (Br.2)	0.419*** (0.143)		0.375*** (0.123)		0.547*** (0.141)		-0.0490 (0.201)		-0.0690 (0.164)		0.0416 (0.405)		0.925*** (0.202)		0.179 (0.141)	
Branch 1 (Br.1)		0.245*** (0.0836)		0.292*** (0.0962)		0.307*** (0.0788)		-0.0161 (0.0662)		-0.0342 (0.0812)		0.00339 (0.0329)		0.236*** (0.0516)		0.117 (0.0925)
<i>p</i> -value	0.00	0.00	0.00	0.00	0.00	0.00	0.81	0.81	0.67	0.67	0.92	0.92	0.00	0.00	0.21	0.21
Dep. var. mean	7.72	5.97	112.58	98.24	38.63	29.39	2.52	0.81	1.33	0.63	0.92	0.07	6.24	2.75	3.02	2.07
Dep. var. mean (+ unassigned int.)	8.57	9.95	122.11	142.49	45.17	40.79	2.73	1.17	1.52	0.85	0.95	0.10	6.40	3.98	3.03	2.94
Observations	103	103	103	103	103	103	103	103	103	103	103	103	103	103	103	103

Panel B. Branch 3 Reports of Escorts to the Station

	# Escorts			
	Branch 1 (1)	Branch 3 (2)	Branch 2 (3)	Branch 3 (4)
Branch 3	0.126*** (0.0384)		0.0702* (0.0358)	
Branch 1		0.379*** (0.116)		
Branch 2				0.392* (0.200)
<i>p</i> -value	0.00	0.00	0.05	0.05
Team FE	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Dep. var. mean	4.41	2.44	4.01	3.11
Dep. var. s.d.	4.14	3.88	2.31	4.62
Observations	256	256	169	169

Notes: **Panel A.** This panel presents the estimates from a regression of the number of vehicles escorted to the station, the bribe revenue, and the fine revenue, and the characteristics of escorted vehicles as reported by the station observers Branch 1 against Branch 2 sources. All regressions include police station and day fixed effects. *Sample:* data collection sample and unassigned intersections' sample. The data collection sample, described in Section 4.A, is composed of 351 team-days, constituted of 14 teams on Day Zero, and 337 team-days after Day Zero (the days of the data collection sample after Day Zero are the experiment sample). Of the 351 team-days of the data collection sample, JPO data collection (Branch 1) was active in 264 team-days. FCA data collection (Branch 2) was active in 173 team-days. The sample is constructed from the aggregated team-days at the level of *station-days* (there are multiple teams per station). This results in a sample of 103 station-days in which there are *both* Branch 1 and 2 data. **Panel B.** This panel presents the estimates from a regression of the number of vehicles escorted to the station as reported by the station observers in Branches 1 and 2 and by the street observers in Branch 3. Escorts of Branches 1 and 2 are the number of vehicles reported by each source to have been escorted to the station in a given station-day. Escorts of Branch 3 is the number of events in the corresponding intersection. Branch 3 events include only those that are strictly confirmed to be escorted to the corresponding station. *Sample:* Data collection sample and, for Branches 1 and 2, unassigned intersections' sample. Of the 351 team-days of the data collection sample, 283 team-days have nonmissing escort information. The sample in the columns is constructed from the data from teams and on days in which the information of origin exists and belongs to the data collection sample's intersections, and: *both* Branch 1 (JPOs) and Branch 3 (Street observers) were active (Columns 1, 2), 256 team-days; *both* Branch 2 (FCAs) and Branch 3 (Street observers) were active (Columns 3, 4), 169 team-days.

Table A4: Fact 2—Having a Protector Increases the Gap between Bribe Requested by the JPO and Bribe Actually Paid by the Driver

Panel A. Decrease from Requested to Actually Paid, by Protector's Rank						Panel B. Differences-in-Differences, all Protectors			
Payment						Payment			
	(1)	(2)	(3)	(4)	(5)		(1)	(2)	(3)
Post	-48.58*** (2.722)	-49.71*** (3.576)	-48.79*** (4.126)	-67.67*** (16.63)	-28.77*** (5.159)	Protection	-9.164*** (2.467)		-2.512 (2.495)
Sample	All protectors with rank	Colonel	Major	Sergeant	Commander	Post		-44.15*** (1.649)	-40.39*** (2.000)
Classified rank		Yes	Yes	Yes	No	Protection × Post			-10.54*** (3.444)
Team FE	Yes	Yes	Yes	Yes	Yes				
Day FE	Yes	Yes	Yes	Yes	Yes				
Dep. var. mean	56.63	55.52	55.34	78.29	44.09	Dep. var. mean	42.69	60.79	61.75
Dep. var. s.d.	33.59	24.20	28.82	65.54	13.72	Dep. var. s.d.	34.10	36.80	36.10
Observations	236	91	75	28	36	Observations	969	969	969
R-squared	0.69	0.79	0.80	0.63	0.69	R-squared	0.14	0.50	0.52

Notes: This table analyzes the effect of having a protector on the initial bribe requested and the actual bribe paid. Panel A estimates a Difference-in-Difference model including *Protection*, *Post*, and their interaction as main regressors, as follows. The observations are each individual driver-JPO interaction at the station. In the negotiation process, there are two periods: in the first period, the JPO requests a bribe amount; in the second period, the actual bribe is paid, with the possibility that an amount different to the initial request is actually paid. *Post* is an indicator that takes value 1 if the observation is an actual bribe payment, and takes value 0 if the observation is only a bribe payment request. Since both are observed sequentially for each driver, the bribe payment is a “second period” in the negotiation. *Protection* is an indicator taking value 1 if the driver has a protector. Panels B and C focus on those drivers with protection and analyze the negotiation effect for different military ranks and affiliations of the protectors. We include randomization block fixed effects (Randomization block FE) and day fixed effects (Day FE) in all columns. **Source:** Branch 1. **Sample:** descriptive sample.

Table A5: Characteristics of True and False Alleged Infractions among Traffic Pull-Over in the Street

	Pull-Over Outcome					Pull-Over Process	
	Escaped	Escorted to Station	Escorted to Unknown	Visible Bribe	Bribe or Escort to Unknown	Disapproval Dispute	Coercion Force
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Clearly True Infraction	-0.0535*** (0.0188)	-0.0265 (0.0265)	-0.0155 (0.0250)	0.0939*** (0.0308)	0.0794*** (0.0294)	-0.344*** (0.0279)	-0.232*** (0.0256)
p-value	0.00	0.32	0.54	0.00	0.01	0.00	0.00
Dep. var. mean (Not clearly true)	0.11	0.25	0.21	0.43	0.64	0.40	0.28
Observations	1595	1592	1595	1592	1595	1595	1595

Notes: This table presents the estimates of a regression of the dependent variable listed on the column headers on an indicator taking value 1 if the traffic pull-over was reported to be certainly true by Branch 3 observers and zero otherwise. **Sample:** Data collection sample, raw data of 1,841 events of traffic pull-overs. The information of whether an infraction is true is only applicable to the 1,841 events of traffic pull-overs. Hence, this analysis can only be implemented in the sample of traffic pull-overs. **Variable construction:** The dependent variables in Columns (1) - (7) are all indicators taking value 1 if the event has the characteristic listed on the header. The source for Columns (1) - (5) are the hard-coded Branch 3 responses of whether the driver escaped, was escorted to the station for sure, was escorted to an unknown location (where they typically end up paying a bribe to the agents or is actually an unconfirmed escort to the station), or paid a visible “*amende*” bribe (associated to a bribe payment to resolve a traffic pull-over through a bribe). In Column (5), the dependent variable is an indicator taking value 1 if either an *amende* bribe was paid or the agent escorted the driver to an unknown location. The dependent variable takes value 1 if either of these events takes place. The source for Columns (6) - (7) are the open-ended Branch 3 descriptions of the driver-agent interaction.

Table A6: Fact 1—Real, not Official, Revenue-Generation Capacity Is Large, Hinges on the Quota

Variables	Mean				
	Descr. sample		Descr. sample + Unassigned int. sample (Br.2)		
	Br.1	Br.2	Raw	Mean	[Min, Max]
Implied Police Officers' Income (USD)					
Agents (per capita)					
Wages	69.00	69.00	69.00	—	—
Gross bribe income	326.14	326.14	326.14	—	—
Kickbacks (retrocession to manager)	14.36	14.36	14.36	—	—
Bribe income net of kickbacks	311.78	311.78	311.78	—	—
Total (official+net bribe) income	380.78	380.78	380.78	—	—
Share of agents' income that is unofficial	0.83	0.83	0.83	—	—
Police Station Staff (Manager + 2 JPO, per capita)					
Wages	100.00	100.00	100.00	—	—
Gross bribe income	1,135.91	1,235.78	1,795.05	1,826.21	[1,812.63, 1,841.56]
Kickbacks (retrocession to higher up)	41.14	42.90	42.90	—	—
Bribe income net of kickbacks	1,102.68	1,200.68	1,763.43	1,794.59	[1,781.01, 1,809.94]
Total (official+net bribe) income	1,202.68	1,300.68	1,863.43	1,894.59	[1,881.01, 1,909.94]
Share of staff income that is unofficial	0.75	0.87	0.90	0.90	[0.90, 0.90]

Notes: This table presents the means of the main outcomes, computed at the station level, and averaged over the eight stations of the data collection sample. While we observe the universe of unassigned intersections' escorts to the station (like for any escort, we see them in the station data), we do not observe the unassigned intersections' street bribe collection; thus, we present the estimates of the unassigned intersections' street bribe collection under various assumptions: *Raw* uses only the data as we observe them and thus includes the vehicles escorted by the unassigned intersections but sets the unassigned intersections' street bribe collection to zero; *Min* imputes the unassigned intersections' street bribe collection using the smallest station-level average of an agent bribe collection as estimated from the descriptive sample; *Max* imputes the corresponding station-level maximum of agent bribe as estimated from the descriptive sample; and *Mean* the mean. We impute the bribe and fine for driver files that are missing at the end of the day, following the description in [Study Documentation](#), Section 4.

Sample: Descriptive sample. **Source:** Branches 2 and 3.

Table A7: Attrition**Panel A.** Loss of Intersection/Teams and Their Replacement

Intersection	Start date	End date	Replacement
1A	Day 1	Day 3	Replaced by 1B
3A	Day 1	Day 5	Replaced by 3B, out sample after Day 7
14A	Day 1	Day 6	Replaced by 14B
13	Day 1	Day 7	Out sample after Day 7

Panel B. Relationship between Attrition and Assignment Status

	Attrition dummy 1			Attrition dummy 2		
	(1)	(2)	(3)	(4)	(5)	(6)
Assignment status (t)	-0.0487 (0.0460)	-0.0327 (0.0336)	-0.0398 (0.0420)	-0.00814 (0.0117)	-0.00496 (0.00798)	
Assignment status (t-1)				-0.000363 (0.0118)	0.00111 (0.00804)	
Mean assignment status until day t						0.127 (0.109)
Dep. var. mean	0.22	0.10	0.20	0.01	0.01	0.01
Observations	375	375	420	360	360	375

Panel C. Relationship between Data Missingness and Assignment Status

	# Team-days missingness		
	Branch 3 (1)	Branch 1 (2)	Branch 2 (3)
Assignment to Quota Reduction Encouragement	-0.00800 (0.0132)	-0.0476 (0.0450)	0.0167 (0.0547)
Dep. var. mean (no encouragement)	0.02	0.24	0.48
Observations	337	337	337

Notes: This table presents the analysis of attrition in the experiment. Panel A provides a description of the teams lost due to attrition. Panel B analyzes whether attrition is related to treatment status by regressing various dummies for whether the team-day is lost to attrition on treatment status, lagged treatment status, and the fraction of days treated for each team up to day t . Attrition dummy 1 takes value 1 if team/day has been dropped out; Attrition dummy 2 takes value 1 for intersection i on day t when it is lost. Columns (1), (4), and (6) treat a replacement as an attrition, Column (2) and (5) treat a replacement as a non-attrition, and Column (3) treats a replacement intersection as a different intersection. Panel C is like Panel B, but the dependent variable is whether the data is missing for that it , independently of whether the intersection has been lost. **Source:** for Panels A-C, the source is the project tracking documents and the randomization calendar. The criteria for inclusion in station analysis are provided in Section 4.C. **Sample:** next page.

Panel D. Relationship Between Attrition and Observable Characteristics

	Mean Difference		
	diff.	p-value	# Observations
Natural quota level	-2.33	0.20	16
Agents assigned to teams	-0.31	0.59	18
Section 6B: Street events (Branch 3)			
Traffic jams	1.50	0.13	18
Accidents	-0.04	0.57	18
Traffic pull-overs (street)	3.11	0.42	18
Time away due to escorts	1.24	0.37	18
Section 6C: Escorts			
Unprotected (station, Branch 1)	-1.61	0.30	17
Protected (station, Branch 1)	-0.35	0.55	17
Unverifiable (station, Branch 1)	-2.28	0.25	17
Verifiable (station, Branch 1)	-0.72	0.44	17
Not clearly true (street, Branch 3)	3.01	0.35	18
Clearly true (street, Branch 3)	0.40	0.62	18
Section 7: Revenue			
Fines (station, Branch 1)	-8.15	0.33	17
Bribes (station, Branch 1)	-44.80	0.33	17
Fines (station, Branch 2)	-8.45	0.54	13
Bribes (station, Branch 2)	-2.00	0.95	13
Bribes (street, Branch 3)	-17.75	0.14	18

Notes: Panel D presents the mean difference in the corresponding variable between the intersections that are in the attrition set and those that are not. The mean difference is obtained by regressing, at the intersection-level, the time-invariant characteristic on an indicator for whether it is lost at some point during the study. **Source:** Panel D also uses all variables used in Sections 6 and 7 as dependent variables; the source of the outcomes of Section 6B is Branch 3, for Section 6C is Branch 1 and for Section 7 is Branch 2. **Sample in all Panels:** Experiment Sample. The natural quota level variable is only missing for one of the replacement teams.

Table A8: Randomization Balance

Panel A. By Time-Varying and Time-Invariant Outcomes

	Mean outcomes			Difference
	Full sample	No Encouragement Assignment	Encouragement Assignment	(3) - (2)
	(1)	(2)	(3)	(4)
Observations	337	157	180	
Monday _{it}	0.20 (0.40)	0.20 (0.40)	0.20 (0.40)	0.00 (0.04)
Tuesday _{it}	0.12 (0.33)	0.14 (0.35)	0.11 (0.31)	-0.03 (0.04)
Wednesday _{it}	0.16 (0.37)	0.15 (0.35)	0.17 (0.38)	0.03 (0.04)
Thursday _{it}	0.16 (0.37)	0.13 (0.33)	0.19 (0.39)	0.06 (0.04)
Friday _{it}	0.16 (0.37)	0.18 (0.39)	0.14 (0.35)	-0.05 (0.04)
Saturday _{it}	0.20 (0.40)	0.20 (0.40)	0.19 (0.40)	-0.01 (0.04)
June _{it}	0.34 (0.48)	0.37 (0.48)	0.32 (0.47)	-0.05 (0.05)
July _{it}	0.65 (0.48)	0.63 (0.48)	0.68 (0.47)	0.05 (0.05)
Day of month _{it}	15.10 (8.33)	15.78 (8.24)	14.52 (8.39)	-1.26 (0.91)
Encouragement administered _{it}	0.51 (0.50)	0.00 (0.00)	0.95 (0.22)	0.95*** (0.02)
Encouragement administered _{it-1}	0.50 (0.50)	0.48 (0.50)	0.51 (0.50)	0.03 (0.06)
Natural quota level, before study _i	7.74 (3.61)	7.83 (3.67)	7.65 (3.55)	-0.17 (0.40)
Commander income, before study _i	155.85 (45.35)	157.08 (43.88)	155.12 (46.62)	-1.96 (5.15)
# Agents per team _i	4.12 (1.02)	4.17 (1.05)	4.08 (1.00)	-0.08 (0.11)

Notes: Panel A presents a balance test for the assignment to the quota reduction *encouragement*. Column (4) presents the coefficients of an OLS regression (and their standard errors in parentheses) using the corresponding variable as the dependent variable and an indicator quota reduction *encouragement*. Panel B presents a balance test of days-of-study for the assignment to the quota reduction *encouragement*. **Source:** randomization calendar. **Sample:** Experiment Sample. The experiment sample is constituted of 337 team-day observations, which are also the units of assignment to the encouragement. The natural quota level variable is only missing for one of the replacement teams.

Panel B. By Day of Study

	Mean outcomes			Difference
	Full sample	No Encouragement Assignment	Encouragement Assignment	(3) - (2)
	(1)	(2)	(3)	(4)
Observations	337	157	180	
Day 1	0.04 (0.21)	0.01 (0.11)	0.07 (0.26)	0.06*** (0.02)
Day 2	0.04 (0.21)	0.06 (0.23)	0.03 (0.18)	-0.02 (0.02)
Day 3	0.04 (0.21)	0.06 (0.24)	0.03 (0.16)	-0.04 (0.02)
Day 4	0.04 (0.21)	0.04 (0.21)	0.04 (0.21)	-0.00 (0.02)
Day 5	0.04 (0.21)	0.03 (0.18)	0.06 (0.23)	0.02 (0.02)
Day 6	0.04 (0.21)	0.05 (0.22)	0.04 (0.19)	-0.01 (0.02)
Day 7	0.04 (0.19)	0.06 (0.24)	0.02 (0.13)	-0.05** (0.02)
Day 8	0.04 (0.19)	0.04 (0.21)	0.03 (0.18)	-0.01 (0.02)
Day 9	0.04 (0.19)	0.01 (0.11)	0.06 (0.24)	0.05** (0.02)
Day 10	0.04 (0.19)	0.02 (0.14)	0.06 (0.23)	0.04* (0.02)
Day 11	0.04 (0.19)	0.04 (0.21)	0.03 (0.18)	-0.01 (0.02)
Day 12	0.04 (0.19)	0.03 (0.18)	0.04 (0.21)	0.01 (0.02)
Day 13	0.04 (0.19)	0.05 (0.22)	0.03 (0.16)	-0.02 (0.02)
Day 14	0.04 (0.19)	0.04 (0.21)	0.03 (0.18)	-0.01 (0.02)
Day 15	0.04 (0.19)	0.05 (0.22)	0.03 (0.16)	-0.02 (0.02)
Day 16	0.04 (0.19)	0.03 (0.18)	0.04 (0.21)	0.01 (0.02)
Day 17	0.04 (0.19)	0.05 (0.22)	0.03 (0.16)	-0.02 (0.02)
Day 18	0.04 (0.19)	0.05 (0.22)	0.03 (0.16)	-0.02 (0.02)
Day 19	0.04 (0.19)	0.03 (0.16)	0.05 (0.22)	0.02 (0.02)
Day 20	0.04 (0.19)	0.03 (0.18)	0.04 (0.21)	0.01 (0.02)
Day 21	0.04 (0.19)	0.04 (0.19)	0.04 (0.19)	0.00 (0.02)
Day 22	0.04 (0.19)	0.04 (0.21)	0.03 (0.18)	-0.01 (0.02)
Day 23	0.04 (0.19)	0.04 (0.19)	0.04 (0.19)	0.00 (0.02)
Day 24	0.04 (0.19)	0.04 (0.21)	0.03 (0.18)	-0.01 (0.02)
Day 25	0.04 (0.19)	0.02 (0.14)	0.06 (0.23)	0.04* (0.02)

Table A9: Robustness to Alternative Measures on Traffic Jam and Accident

	Accident (Infraction) (1)	Traffic jam (External data) (2)	# Passing cars w/o jams (3)	w. jams (4)	Raw traffic report (max) Traffic jam (5)	Traffic fluid (6)	Raw traffic report (min) Traffic jam (7)	Traffic fluid (8)
<i>A. Second-stage IV 2SLS estimates (LATE)</i>								
Quota level _{it}	0.00240 (0.00888)	0.0352*** (0.0136)	-0.699** (0.295)	-0.839 (1.523)	0.0354** (0.0171)	0.000630 (0.00188)	0.00101 (0.00111)	-0.0418** (0.0172)
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.77	0.00	0.02	0.57	0.03	0.75	0.39	0.01
<i>p</i> -value, S.E. clustered at <i>team</i>	0.79	0.01	0.00	0.36	0.03	0.76	0.61	0.01
Dep. var. mean (encouragement)	0.03	0.47	19.73	27.49	0.38	0.99	0.00	0.57
Observations	227	326	2400	485	321	321	321	321
First-stage <i>F</i> -statistic	124.59	85.13	99.85	6.90	86.61	86.61	86.61	86.61
— <i>Controlling for temporal and organizational lags</i>								
Quota level _{it}	0.001 (0.009)	0.034*** (0.013)	-0.676** (0.298)	-0.140 (1.019)	0.034** (0.016)	0.002 (0.001)	-0.001 (0.001)	-0.041*** (0.017)
Observations	224	313	2325	444	308	308	308	308
<i>B. Reduced form estimates (ITT)</i>								
Encouragement _{it}	-0.008 (0.032)	-0.080** (0.035)	1.769** (0.814)	0.596 (1.782)	-0.092** (0.042)	-0.002 (0.005)	-0.002 (0.003)	0.104** (0.045)
<i>p</i> -value, randomization inference	0.85	0.02	0.00	0.61	0.05	0.69	0.19	0.03
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.80	0.02	0.03	0.71	0.04	0.75	0.44	0.02
<i>p</i> -value, S.E. clustered at <i>team, wild bootstrap</i>	0.81	0.03	0.00	0.62	0.04	0.76	0.81	0.02
Dep. var. mean (no encouragement)	0.03	0.51	18.33	31.11	0.42	0.99	0.00	0.52
Observations	232	337	2480	504	332	332	332	332

Notes: This table presents the estimates from Equation 1. **Sample:** Experiment Sample. **Variable Construction:** In Column (1), the dependent variable is the daily incidence of accidents per intersection. There 14 accident records in FCA data. There are 7 accident records that are in the experiment sample where 3 teams with 1 accident record in the day, and 2 teams with 2 accident records in the day. In Column (2), the dependent variable is the daily incidence of traffic jams per intersection as measured by an external data source (minibus riders). The source for the reports of traffic jams used in this calculation is mobile observers traveling in minibuses. In preparation for Lameke, Malukisa, Sanchez de la Sierra, Tanutama, and Titeca (2024), we also hired observers in 10 minibuses during this study. Observers travelled through some of the study intersections 5-10 times per day on average. This source has limited quality because drivers circulate through only some intersections and during only some times during the day. The dependent variable in Columns (3) and (4) is the number of drivers passing at the intersection per minute, when there is no traffic jam (column 3) and when there is a traffic jam (Column 4). The dependent variable in Columns (5)/(6) and (7)/(8) is the across-hour mean number of observers for a given intersection-day who report a traffic jam/traffic to be fluid, and the cross-hour *minimum* number of observers for a given intersection-day who report a traffic jam/traffic to be fluid, respectively. The source for the reports of accidents and traffic jams used in this calculation is Branch 3. In Column (7), Section B, the treatment is collinear with the intersection fixed effects, as some intersections always have a minimum of zero traffic jams, hence the estimates in Column (7) are without intersection fixed effects.

Table A10: Effect of the Encouragement on Accidents—Controlling for Traffic Jams' Incidence

	Accidents				Pull-overs		Team size		Time away	
	Incidence		Occurrence		#	#	[0,1]	[0,1]	hours	hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Second-stage IV 2SLS estimates (LATE)</i>										
Quota level _{it}	0.025*** (0.009)	0.025*** (0.009)	0.007*** (0.003)	0.007*** (0.003)	0.117*** (0.044)	0.124** (0.048)	0.001 (0.008)	0.001 (0.008)	0.036** (0.015)	0.038** (0.016)
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.01	0.01	0.01	0.01	0.01	0.01	0.86	0.93	0.01	0.02
<i>p</i> -value, S.E. clustered at <i>team</i>	0.00	0.00	0.00	0.00	0.00	0.00	0.75	0.86	0.03	0.04
Dep. var. mean (encouragement)	0.03	0.03	0.01	0.01	0.96	1.05	0.87	0.89	0.30	0.33
Observations	2898	2863	2898	2863	2932	2653	2932	2653	2902	2626
First-stage <i>F</i> -statistic	81.77	85.94	81.77	82.31	85.90	85.90	85.90	85.94	86.52	86.39
Traffic control	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table replicates Table 3, while also including as a control, the incidence of traffic jams. All regressions include randomization block fixed effects and day fixed effects. The dependent variables and traffic jams' incidence are at intersection-day-hour level, considering traffic jams' incidence as control.

Table A11: Drivers' Responses—Lagged Driving Route Response

	Number of passing cars (per minute)		
	(1)	(2)	(3)
<i>Second-stage IV 2SLS estimates (LATE)</i>			
Quota level _{<i>it</i>-1}	-0.069 (0.340)	-0.153 (0.314)	-0.990 (1.050)
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.84	0.62	0.33
<i>p</i> -value, S.E. clustered at <i>team</i>	0.62	0.25	0.17
Dep. var. mean (encouragement)	20.76	19.54	29.32
Observations	2724	2290	406
First-stage <i>F</i> -statistic	84.36	93.46	15.32
Sample	All	No traffic jam	Traffic jam

Notes: This table presents the estimates from Equation 1, with dependent variables listed in the column headers and independent variable lagged. **Source:** Branch 3 and quota level. *p* – *value* reports the p-value of the test of whether β_Q is different from zero; *p* – *value*, S.E. clustered at ... report the p-value, with clustering of the standard errors at the station-day level, and at the team level. **Sample:** Experiment Sample. The experiment sample covers 337 team-day observations, which are also the units of assignment, 327 of which have nonmissing quota level, and 322 have both nonmissing quota level and traffic data (Table A1, Panel A). In all columns, the observations are at the team-day level (denoted *it*, in the text), and refer to the number of vehicles seen escorted at the station from the intersection (*team*) *i* on day *t* with the characteristic listed in the respective table headers. **Variable Construction:** The dependent variables are the number of drivers who pass by through the intersection per minute, in the subset of intersection-days in which there is a traffic jam, and for those in which there is not.

Table A12: Nonclassical Measurement Error by Branch 1 JPO Reporters

	# Branch 1 JPO Reporters
	(1)
<i>A. Second-stage IV 2SLS estimates (LATE)</i>	
Quota level _{it}	-0.016 (0.023)
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.51
<i>p</i> -value, S.E. clustered at <i>team</i>	0.53
Dep. var. mean (encouragement)	1.75
Observations	320
First-stage <i>F</i> -statistic	83.76
— <i>Controlling for temporal and organizational lags</i>	
Quota level _{it}	-0.021 (0.022)
Observations	307
<i>B. Reduced form estimates (ITT)</i>	
Encouragement _{it}	0.031 (0.060)
<i>p</i> -value, randomization inference	0.60
<i>p</i> -value, S.E. clustered at <i>police station-day</i>	0.62
<i>p</i> -value, S.E. clustered at <i>team</i>	0.67
Dep. var. mean (no encouragement)	1.75
Observations	330

Notes: This table presents the estimates from Equation 1, with dependent variable the number of JPOs present at the station that day. **Source:** Branch 1 and quota levels. **Sample:** Experiment Sample. **Variable Construction:** the observations are at the team-day level (denoted *it*, in the text), and refer to the number of JPOs present at the station that are relevant to intersection (hence, team) *i* on day *t*.

Table A13: Effect of the Quota on the Second Mandate: Testable Implications for Deterrence—Controlling for Traffic Jams

Panel A. In the Stations									
	# Escorts by team-day that are... (source: Branch 1, JPOs)								
	Targeting of drivers who may yield a high bribe (Prediction 2a)						Allegations (Prediction 2b)		
	Unprotected (1)	Protected (2)	High-Bribe (3)	Low-bribe (4)	High-fine (5)	Low-fine (6)	Unverifiable (7)	Verifiable (8)	
Second-stage IV 2SLS estimates (LATE)									
Quota level _{it}	0.0219*** (0.00835)	0.00207 (0.00573)	0.0318* (0.0166)	0.00923 (0.0145)	0.00916 (0.0176)	0.0319* (0.0185)	0.0608*** (0.0140)	0.0187* (0.0113)	
p-value, S.E. clustered at <i>police station-day</i>	0.04	0.64	0.06	0.51	0.61	0.08	0.00	0.29	
p-value, S.E. clustered at <i>team</i>	0.04	0.76	0.09	0.58	0.64	0.08	0.00	0.08	
Dep. var. mean (encouragement)	0.29	0.17	0.81	0.66	0.84	0.64	0.35	0.19	
Observations	2264	2264	712	712	712	712	1962	1962	
First-stage F-statistic	99.18	99.18	94.49	94.49	94.49	94.49	84.69	84.69	
Panel B. In the Street									
	Infractions and Pull-Over			Disapproval	Driver Cooperation			Coercion	All
	# Pull Overs, by Infraction			Any Event	Outcome of Traffic Pull Over			Any Event	Std.
	Any (1)	Not clear (2)	True (3)	Dispute (4)	Escape (5)	Escort (6)	Bribe (7)	Force (8)	Index (9)
Second-stage IV 2SLS estimates (LATE)									
Quota level _{it}	0.124*** (0.0481)	0.108** (0.0448)	0.0276 (0.0176)	0.0459** (0.0218)	0.0261** (0.0131)	0.0253* (0.0144)	-0.0515*** (0.0192)	0.0297* (0.0167)	1.079*** (0.359)
p-value, S.E. clustered at <i>police station-day</i>	0.01	0.02	0.12	0.03	0.05	0.08	0.01	0.07	0.00
p-value, S.E. clustered at <i>team</i>	0.00	0.00	0.05	0.00	0.00	0.03	0.00	0.02	0.00
Dep. var. mean (encouragement)	1.04	0.79	0.19	0.30	0.06	0.20	0.74	0.19	5.52
Observations	2653	2653	2653	2653	1687	1686	1687	2932	2932
First-stage F-statistic	85.94	85.94	85.94	85.94	27.31	27.55	27.55	85.90	85.90

Notes: This table replicates Table 4, while also including, as a control, the incidence of traffic jams. The dependent variables and traffic jams' incidence are at intersection-day-hour level. Variable construction and variable definition are described in Table 4.

Appendix C. Evidence from Open-Ended Descriptions Made by Observers

A.. Evidence of Brute Force in the Pull-Over Interactions

“The agents brutalized the driver.” Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

“Brutal entry of the agents into the vehicle and brawl!! The agents are fighting over the control of the wheel with the driver!” Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

“There was a violence exerted by the police of the police, they even pinched the tires.” Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

“A trailer blocked the way, so the agents took the opportunity to enter in the vehicle!!!” Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

“The agent tried to climb into the minibus through the front door but the receiver was agile, he closed the front and back door.” Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

“The agents are such thieves...” Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

“The agent confiscated the key of the driver, until the driver paid 3 USD!” Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

B.. Evidence of how the Quota Affects Accidents exactly

Agents' Absence Induced by the Quota Increases the Risk of Accidents

“The agents arrived late, but they were also making excessive absences, which led to the accident between a Mazda taxi and a Mercedes 207.” Source: Street observer end of day open-ended comments, July 2015.

“The day went well, although in the morning, we witnessed accidents at our intersection due to a lack of agents entering the intersection. The agents arrived late, and they moved around too much at the same time, leaving the intersection empty.” Source: Street observer end of day open-ended comments, July 2015.

“In this intersection, it is really necessary that the agents are always present to assist pedestrians in crossing safely, to prevent accidents, and to manage traffic in case of congestion.” Source: Street observer end of day open-ended comments, July 2015.

Agents' Attempts to Escort Increases the Risk of Accidents

"Also, to report that while targeting motorcycles, an agent caused a serious accident. He wanted to disconnect the motorcyclist's ignition, and the motorcyclist was carrying a mother with her child. The agent was holding onto the motorcycle's handlebars. In an attempt to avoid the pedestrian, the motorcyclist tried to forcefully pass by, swerving into the opposite lane where there was a trailer approaching. The motorcyclist lost control and nearly went under the trailer. Fortunately, luck was on their side, and as the police colonel passed by, the poor mother was able to explain the situation to him. The colonel, in turn, took the mother's belongings and placed them in the trunk of his Hyundai jeep." Source: Street observer end of day open-ended comments, July 2015.

"There was a strong presence of very active police of the police officers, who stopped vehicles and even punctured tires, causing panic and accidents." Source: Street observer end of day open-ended comments, July 2015.

C.. Evidence that the Effort to Escort Causes Traffic Jams

"There was a group of very aggressive agents and a lot of harassment for money outside the office, even causing traffic jams." Source: Street observer end of day open-ended comments, July 2015.

"They had a discussion between the driver and the agent, which caused a traffic jam." Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

"Blocking of traffic and entry into the car. The reason for the arrest: the driver didn't wear his seatbelt." Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

"It was really a catastrophe for them to stop this car; they even caused a traffic jam." Source: Street observers (branch 3) open-ended comments made on an agent-driver interaction, July 2015.

D.. Additional Evidence for how the Quota Causes Bad Traffic Outcomes

From Project manager interview, 2023:

"It's true that sometimes they come to regulate traffic, but often they are the ones causing jams to catch drivers... when a driver knows they have irregularities in their vehicle's documents, they won't yield to others; they try to force their way... if they reduce their speed, they risk being caught. Therefore, when there is a reduction in the quota... drivers... drive normally." ²⁷

²⁷In another interview: "At 10.30am, I saw with my own eyes a minibus crashing into an electricity pole as the driver and two agents fought over the steering wheel... Colleagues of these agents, explained to me that their aggressiveness was because the commander was waiting for vehicles at the office." (Source: Project manager 2 interview, 2023). This is in line with common accounts of struggles between drivers and agents causing accidents (for example, reports of two minibuses crashed into each other when a driver tried to flee the police (Digital Congo, 2013), and an accident struggling for the steering wheel (Afrique Redaction, 2011)).

Sometimes, road users resist when they are stopped by the police, who generally operate in teams. Officers sometimes forcefully enter a vehicle to immobilize the driver. The quota puts additional pressure on the police officer, especially when some drivers manage to escape during attempted arrests ... When a driver resists, they may block the passage of other vehicles, leading to traffic jams in such situations.

Similarly, when police officers forcefully enter a vehicle, they may struggle for control of the steering wheel with the driver. The driver may lose control of the vehicle, resulting in accidents under these conditions. Sometimes, it's by accelerating to escape the police that drivers collide with passengers or other vehicles.

E.. Qualitative Evidence for External Validity

a. Our Fieldwork in the City of Kinshasa before the study

"The Lt Colonel gave me a particular intersection, for which he asked me to pay his brother-in-law, who's a colonel, 500 USD per week, as this would guarantee me an easy life. The problem was that I was based on a sterile intersection: no cars were passing by! As a result of this I could no longer pay this amount, and I lost my position." (Interview commander, Kinshasa, 28 January 2015).

b. Our Fieldwork in the Cities of Lubumbashi and Goma

"All the officers you see at the head of PCR companies in Lubumbashi must pay our colonel 500,000 congolese francs (FC), the equivalent of USD 544 every Saturday." (Nkuku, 2022)

"The quota is merely a threshold, which can be in terms of vehicles, motorcycles, or money. The commander in Goma has a certain amount to deliver to the provincial police commissioner ... The specific amount at each intersection is known. Different vehicles have different values ... There is a price for a truck, a price when the vehicle has the required documents, and a price when the vehicle lacks proper documentation." (Anonymous officer, Goma, September 2023).

c. Qualitative Evidence from Other Studies

"[Fines] are shared at the level of the station. The commander simply distributes the money among his companions according to his discretion." (Lake (2022), p. 865).