The Social Fabric of Corruption: A Field Experiment in Congo's Public Transport

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Abstract. This article examines the role of social relationships in the transaction costs of corrupt exchange. Using a field experiment in Kinshasa, Democratic Republic of the Congo, we randomly assign public transport drivers to reroute their operations on certain days, targeting routes where they lack established relationships with line officers. We find that traffic officers interacting with drivers in rerouted rounds are significantly less likely to have pre-existing social relationships with them. These rerouted rounds also generate significantly lower revenue. Why? By analyzing two cross-randomizations, we find that this revenue loss stems from the absence of social relationships between drivers and officers. Where do these relationships originate? When rerouting is randomly induced to last several days rather than one, drivers make substantial initial investments, referred to as "buying the land," followed by increased profits and the formation of additional relationships over time—consistent with the creation of cooperative relationships through an initial bond. These findings highlight how social relationships lower transaction costs in corrupt arrangements and suggest that their structure is not exogenous but shaped by the very corruption profit opportunities they facilitate. **JEL Codes:** D23, D73, D74

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1. INTRODUCTION

Human beings are social animals. Our social environments have been shown to shape our choices, even those with long-lasting consequences (Bursztyn et al., 2019, 2020, Macchiavello and Morjaria, 2020). Despite these insights, the economic study of corruption——a significant failure of both market and state governance—has often treated it as if it occurs in a social vacuum. This oversight is particularly costly, as corruption is typically illegal and therefore, as documented by other social scientists, often informally regulated through social networks (Olivier de Sardan, 1999). A major challenge in studying the role of the social environment in corruption is that such transactions are typically illegal, and manipulating or varying the social environment can be difficult.

In this paper, to address this challenge, we leverage three years of qualitative research to gain access inside the Kinshasa driver association. We use this access to document the type of corrupt transactions between public transport drivers and police officials, to induce randomness in whether public transport drivers have social relationships with traffic police officers on the line on which they drive, and to induce randomness in the expected horizon over which there are profitable opportunities for corruption. This allows us to answer the following questions. What are the transaction costs of corrupt exchange? How do social ties between citizens and officials influence such cost? How do these ties form?

This setting is well-suited to examine the role of social relationships in corruption. First, public transport was privately provided through, homogeneous, old Mercedes minibuses, commonly called the *Spirit of Death* ("*Esprit de Mort*" in French), due to their poor safety (they were almost always in violation of the law). Second, drivers had large leeway to negotiate the ride price with the passengers, and could provide effort to attract new passengers, shouting the direction of the minibus and waiting at key intersections. Third, at each intersection, a team of police officers regularly sought shakedown or bribes. Officers had the power to stop drivers, which could lead to lengthy negotiations and large bribes. Thus, time negotiating with the passengers for the price or trying to attract passengers exposed the drivers to the risk of detection, potentially leading to revenue loss.

Fourth, against the backdrop of this risk, an unofficial toll, the *Kola Nut Greeting* ("*Mbote ya Likasu*" in Lingala) had developed. The toll consisted in 500 Congolese francs (53 cents in US dollars of 2015) once per day to each team of officers.

The first key input into our analysis is a dataset we gathered prior to the experiment applying a measurement method first introduced in Olken and Barron (2009). The dataset contains information on 15,426 occurrences of minibus driving through the city intersections, of which 2,423 involved interaction with police officers at the intersections, the bribes paid to the officers, and the time spent negotiating for a bribe; those are generated by 344 officials, 49 intersections, and ten minibuses. On the lines where drivers regularly drive and have ties with the police (henceforth, *the home lines*), negotiating a bribe can cause significant time waste—in some cases up to one hour—and can result in large payments—with up to 27 USD for a single bribe. However, of all interactions with the police, 84.1% are the *mbote ya likasu* toll fee, while only 2.7% are bribes that involve negotiation (henceforth, *negotiation bribes*); the remaining are amicable talks without payments. The toll fee (500 Congolese francs) involved on average less than 40 seconds of interaction with the officer versus 7 minutes for negotiation bribes.

The second key input into our analysis is a randomized experiment covering all ten minibus lines of the distribution network of East Kinshasa. Drawing on a research partnership with the national association of drivers (*Association des Chauffeurs du Congo*, henceforth, *ACCO*), we provided a randomized encouragement in some days and not others for ten drivers to drive on lines where they did not have relationships with the officers on the street (henceforth, *foreign lines*). Our experimental design allocates 270 driver-days over a period of 27 days as follows: for each driver, days were re-grouped into 13 *driving blocks* of one to three contiguous days, totaling 130 driving blocks in the study; within driver randomization strata, seven driving blocks were randomly assigned to driving in a foreign line and the remaining six to driving on their home lines. The design allows us to suggestively identify whether the effect of rerouting arises from the driver's expected interaction with the officers on the street from other features of driving in a new route. Borrowing from a practice commonly used by private drivers, high-ranking officers working with ACCO extended their protection to

drivers against extortion from officers in the street on randomly selected days. To separately identify not having a relationship vs. investing in a new one, we also cross-randomized whether the driver was rerouted for three consecutive days, or one. We measure driver revenue and bribe payments using again the measurement method introduced in Olken and Barron (2009).

The analysis of our randomized experiment yields three results. First, in rerouted driver-days, the fraction of interactions where the driver and the officer are socially connected decreases from 56 to 30%; the driving revenue net of operating costs (henceforth, *joint profit*) from 18.6 to 9.5 USD per day.

Second, the effect on joint profit is muted when the driver was assigned third-party protection. Thus, the first result is not simply moving drivers away from their preferred routes to ones that they are unfamiliar with: the effect on joint profit crucially hinges on expectations about interactions with the officers, independently of their knowledge of the route. A new route could hurt profits because (a) drivers lack social relationships with police or (b) other factors like knowledge of the route. Protection allows to isolate the components of this effect that arise from the nature of the (expectations of) interaction with the officers.

The interpretation from our analysis relies on the protection treatment only breaking down the benefits of social ties with the police. However, if protection has an independent effect on joint profit, the cross-randomization will also capture other confounds. That the mean of the dependent variable is comparable with and without protection in home lines provides suggestive evidence that protection might strictly be related undoing the benefits of social relationships.

Third, we find that short-term rerouting decreases joint profit by reducing the number of passengers and passenger prices, while it keeps bribes the same. The intuition, consistent with our qualitative interviews, is that when the driver does not have ties with officers on the line, calling and waiting for passengers to charge the bus and negotiating with them for a price, exposes the driver to detection and shakedown by the officers, discouraging the driver to take those actions; with ties, the driver can take such actions without such risk. In contrast, long-term rerouting decreases the joint profit by decreasing the trips per day, and increases bribe costs by 72%, a practice called "buying the land." Our qualitative interviews

suggest that drivers invest in new ties when the rerouting horizon is long, letting themselves be detained, engaging in long discussions and making large payments to the officers. Indeed, within three days, relationships, trips and profits catch up.

A potential alternative explanation for the daily increase in revenue in the course of the three-day foreign route assignments is that drivers may simply become more familiar with the route, regardless of any initial investment in relationships. This finding thus, is highly suggestive that forming new relationships pays off, though it is of course not conclusive—new relationships in this branch of the experiment are bundled with learning about other aspects of the road.

On average, creating new relationships reduces the joint profit by 9.7 USD from 22.2 USD and costs 5 USD in bribes for at least one day. This one-off 15 USD reduction in profit is small compared to the return of 30 USD each day. This raises the question of why drivers still have lines without relationships. Our qualitative interviews suggest that liquidity constraints and risk aversion are central. The 24 USD of daily joint profit is allocated into bribes, payments to the assistant who calls for passengers, rental fees for the owner, and the residual is a small profit for the driver. Also, buying the land is risky, where the vehicle could be taken to the station, yielding a very large loss. Without a buffer to overcome those, risk averse drivers with little cash prefer to avoid many new lines. Given these daily losses, even over a three-day period, the upfront investment can be a rational strategy to avoid the reduction in profit arising from actions to evade officers.

These findings complement the literature on corruption in economics. While the role of repeated interaction was proposed theoretically (Rose-Ackerman, 1999, Dixit, 2004) the literature has tended to analyze market forces taking the *governance* of corrupt exchange as given (Olken and Barron, 2009, Shleifer and Vishny, 1993, Bertrand et al., 2007, Banerjee et al., 2012). This study experimentally establishes that social relationships are central to the governance of corruption.

While a rich literature has examined home effects (social ties) of bureaucrats or auditors as an explanatory variable (Xu et al., 2021, Chu et al., 2021, Guan et al., 2016, Baber et al., 2012), the question addressed has typically been how home assignment affects bureaucrat performance or career outcomes. Instead, our study aims to isolate how a specific aspect of home assignment, *repeated inter-*

action, might help sustain corruption agreements that inadvertently are also more efficient in one dimension (the private profit they sustain). To establish this result, we collected fine-grained dis-aggregated transaction-level data and conducted an experiment at the level of day-citizen, a variation that is typically unavailable for bureaucrat rotation. While previous papers provide interpretations for their findings consistent with our results (e.g. Xu et al. (2021) explanation hinges on heterogeneous effects, finding that the effect of rotation on bureaucrat performance are driven by states with more corruption), we provide suggestive evidence for the effect of citizen-officer repeated interaction on *corruption governance*.

These findings also complement a body of research emphasizing that connections with officials can be profitable (Fisman, 2001, Fisman and Wang, 2015). A challenge to examine this question is that "political connections are not exogenously assigned, so [the] main results need be interpreted with some caution" (Fisman and Wang, 2015) and that connections are hard to measure. Our study documents the nature of these ties by measuring them directly with disaggregated transaction data and altering them through a randomized experiment. Furthermore, our question also differs: we analyze the role of these relationships in the broader context of a Cosean bargaining breakdown, leading to the destruction of a surplus that could have been allocated to both parties (and consumers).

Our analysis draws on a rapidly growing empirical literature on the role of relational contracts (Macchiavello and Morjaria, 2020) or social proximity (Fisman et al., 2017) in contractual performance and transactions' efficiency. Similarly, an extensive literature discusses how networks can help with many other types of exchanges (e.g., consumption spillovers, insurance), and even illegal ones (e.g., vote buying (Cruz, 2019)). Drawing on the insights of this literature, our study goes a step further: our findings contribute the notion that the social structure is *endogenous* to the benefits of relationships; the initial costly investment we document associated to the creation of new relationships resonates with results from economic theory showing that relationships form through an initial bond (Kranton, 1996). This finding about changing forms of interactions with state official is consistent with Coate and Morris (1995) where one party (in our case, the drivers) has incentives to transfer to others (in our case, the police officers).

2. BACKGROUND: THE PASSENGER TRANSPORTATION SECTOR IN KINSHASA

The public transportation system of Kinshasa in 2015 was privately operated by minibuses. The minibuses, most of which were Mercedes 207 and could theoretically transport 12 passengers, were commonly called the *Spirit of Death* ("*Esprit de Mort*" in French). The minibuses owed their ironic name to the dangers of travelling in them. At the time of this study, most minibuses were known to fail to comply with various technical or safety regulations (Titeca and Malukisa, 2019). The vehicles were typically bought in poor condition, were on average between 15 and 20 years old, and frequently loaded more than 20 passengers.[1] *Esprits de Mort* regularly had technical problems.

The driver was residual claimant and rented the minibus for a fee.[2] The main source of revenue to the driver was the number of passengers and the price they paid for the ride. *Esprit de Mort* lines of East Kinshasa are depicted in Figure 1. Minibuses followed pre-established lines, although the driver was free to choose which lines to drive on. In each trip, drivers passed through many intersections containing teams of police officers.

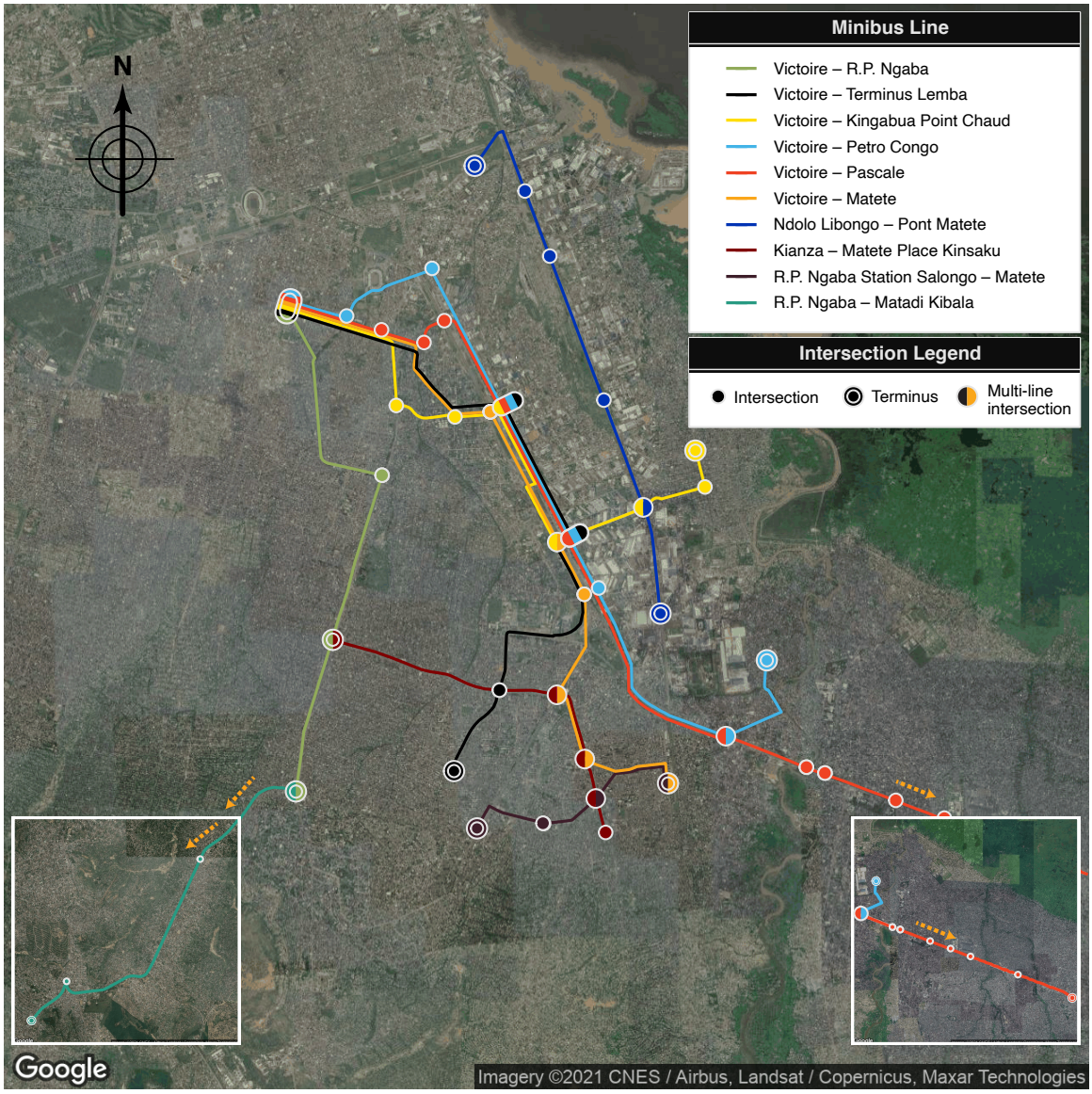
Revenue hinged on attracting passengers and negotiating with them for a price of the ride. First, to attract passengers, the driver usually relied on a conductor to shout the direction of the minibus through the window, and waited for passengers to respond to the call at buys intersections—or shouted the direction himself. Second, the price per passenger was determined as follows: there was a standard ride price—considered by most to be fair—but drivers had latitude to negotiate that rate. The driver would stop for discussions about those prices.

However, spending time negotiating for a price, or making oneself visible to the passengers through shouting, increased the revenue, but exposed the driver to significant risk. Indeed, various traffic police officers posted along the lines at strategic locations where there were many such passengers, such as busy street intersections. If one of these officers detected a minibus at an intersection, he could stop the driver, triggering a lengthy negotiation in which he could threaten

[1] They were among the oldest in sub-Saharan Africa (Tchanche, 2019). Figure A1 depicts one.

[2] Some arrangements also included ways of sharing risk between the driver and the owner.

FIGURE 1.—The Public Transportation Network in East Kinshasa: *Esprit de Mort* Lines



Notes: This figure shows the intersections and lines that *Esprit de Mort* minibuses drive through in East Kinshasa. Circles denote intersections, and lines denote bus lines. Circles with double border indicate termini (starting or end stations); circles with multiple colors indicate intersections crossed through by multiple lines. The two squares represent termini that are too far to fit on the current scale and are artificially closer. They are the termini of the lines the Rond Point Ngaba – Matadi Kibala (lower left quadrant), and the Victoire – Pascal line (lower right quadrant).

to seize the minibus to the police station, leading to time waste and high bribes. We were told of vehicles that were sold on the black market by the station staff.

To avoid seizure once a negotiation had started, the driver could pay a bribe to the officer (henceforth, *negotiation bribe*). These negotiations were lengthy and in the worst cases could be violent. In addition to the risk of having to pay a large bribe, the time negotiating with the officers was very costly in terms of lost revenue. Because of their inherently bad state, which made them in violation of various regulations, *Esprits de Mort* were particularly vulnerable to being detained for a negotiation. This implied that time spent at an intersection negotiating with the passengers or trying to attract passengers increased the risk of detection by officers, inducing a significant loss of revenue. Thus, driving and passing through intersections *quickly*, while losing potential passengers and forgoing high negotiated passenger prices, allowed them to reduce the risk of detainment.

Against the backdrop of these risks, minibus drivers and police officers had developed an unofficial toll fee called the *Kola Nut Greeting* ("*Mbote ya Likasu*" in Lingala). The system consisted of drivers paying only 500 Congolese francs (53 cents in US dollars of 2015) once per day to each team of officers at the intersection on the driving line. To take part in this system, drivers needed to be known to the officers on a line *and* to have social relationships with them. In some lines, the drivers had relationships, allowing them to drive and make profits (henceforth, *home lines*). In others, they did not (henceforth, *foreign lines*):

> The *Mbote ya Likasu* is money that the driver gives the officer as the officer's right. (Project notes of July 18th 2015)

Relationships were acquired through a process known as *buying the land* ("*acheter la terre*" in French). It consisted of taking part in lengthy discussions and ending with a large payment. Members of the implementation team told us:

> The way to familiarize oneself with the officer is to let oneself be arrested and endure long discussions, and through these discussions, we become friends! When we drive again, we are no longer new on this intersection. He "buys the land" like this . . . The driver therefore wanted to have good relations with this officer who helped him "buy the land" with the street-level officer: he gave him 5,000 Congolese francs. . . if *Esprits de Mort* get detained, they could be escorted very, very far! (Project meeting notes of September 4th 2015)

Empirically, thus, payments to buy the land were observationally equivalent from other negotiation bribes, differing only through the intention of the driver. These were further hard to distinguish because, in cases of involuntary large payments, a friendship/relationship is automatically made. We present more quotes from qualitative interviews in Section B1 in Online Appendix.

At all levels of the hierarchy, police officers offered their "protection," often for money. When detained by a traffic police officer, the driver can call his protector and communicate this protection in the negotiations with officers. Our research observed failure of attempts to detain and seize minibuses (or other vehicles) by street-level police officers, after an "intervention" by high-level state officials. Section B1 in Online Appendix present additional qualitative interview quotes.

Monthly fees for unconnected drivers to acquire high-ranking protection sold privately range from 100 to 500 USD. At the bottom of the hierarchy, street-level police officers also sold their protection, typically to wealthy drivers, by sitting in the car in the day, often for 30 USD a day. The power of these officers stems from their own relationships with their own "protectors" in the hierarchy. We now present stylized facts about corruption.

3. THE HOME LINES, IN PRE-EXPERIMENTAL DATA

We now present stylized facts about the system of corruption in the minibuses "natural" environment: the home lines.

The pre-experiment data in this section were collected between June 19_{th} and July 20_{th} 2015 in the ten lines Kinshasa (whereas the experiment we present in the subsequent sections begun one month after this). These data contains information on 15,426 events in which one of ten selected drivers through one of the 49 unique intersections of the network, where a 344 officers are posted.[3]

[3]To collect the data for this section, every day, each of ten data collectors was dispatched to one of ten minibuses under the supervision of a data collector supervisor. Each data collector gathered the details of every interaction with the police officers for every intersection crossing for their corresponding minibus. We programmed a survey on SurveyCTO and the surveyor carried a smartphone, prompting each intersection on the minibus line to answer the following questions: *1. "Did the driver pass through the intersection?" 2. "Details of the interaction with the police if any and bribe payments if any."* Time and place marker was also embedded. Thus, the measurement of enumerators was also not blind nor double-blind and follows the practice introduced in Olken and Barron (2009).

TABLE I: Characteristics of The Three Types of Bribes in Pre-Experimental Data

A. Bribe Costs Decomposition—Payments per Transaction (USD), by Type of Bribe

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Average per transaction made	0.82	2.03	0.04	27.14	2,103
Toll fee (*mbote ya likasu*)	0.53	0.32	0.04	5.43	2,012
Negotiation bribe (incl. police station)	9.79	7.05	2.17	27.14	64
Tip	1.19	0.62	0.43	2.71	27

B. Time Waste per Transaction by Type of Bribe

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Fraction of intersection crossings in which time interacting with police is strictly positive	0.16	0.36	0	1	15,426
Time wasted in interaction with police, per transaction conditional on interaction time strictly positive (duration of negotiation in minutes)	0.83	2.13	0.1	55	2,103
Toll fee (*mbote ya likasu*)	0.64	0.78	0.1	20	2,012
Negotiation bribe (incl. police station)	7.04	9.59	0.5	55	64
Tip	0.72	0.35	0.5	2	27

C. Frequency of Payment by Type of Bribe

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Number of trips per day	5.92	2.56	1	15	261
Number of times per day in which a payment is made at the average intersection (any payment)	1.24	2.09	0	12	1,700
Toll fee (*mbote ya likasu*)	1.18	2.07	0	12	1,700
Negotiation bribe (incl. police station)	0.04	0.19	0	2	1,700
Tip	0.02	0.13	0	2	1,700

Notes: This table presents the summary statistics on the three types of bribes in the pre-experimental data between June 19_{th} and July 20_{th} 2015, observed in the drivers' home lines. The pre-experiment data presented in this section were collected between June 19_{th} and July 20_{th} 2015 in the ten lines of the East Kinshasa network (whereas the experiment we present in the subsequent sections begun mone month after this). Panels A, B, and C present the bribe costs, the duration of the negotiation for each bribe, and the frequency of each bribe respectively. The statistics presented are computed from 15,426 events in which one of ten drivers drives through an intersection, from 49 unique intersections, in which 344 officers are known to be posted. Of these events, 2,423 are events of driver-office interaction generated by the ten drivers and 344 officers; of those, 2,103 resulted in the payment of a bribe.

The majority of interactions are bribes without negotiation (84.1%), predominantly toll fees (83.0%). The remainder are amicable conversations with no payment (13.1%) and negotiations (2.7%).[4] Table I, Panel (a), presents basic facts about the transactions. The toll fee is the smallest bribe payment, .53 USD on average. In contrast, the mean negotiation bribe payment is 9.79 USD. The toll fee is also extremely stable: while the toll fee standard deviation is .32, that of negotiation bribes is 7.05, suggesting heterogeneity given that per capita income is around 1 USD per day.[5] Panel (b) presents basic facts about the time wasted negotiating. Overall, the driver interacts with an agent only in 16% of intersection crossings. On average, each interaction with the police lasts 50 seconds. However, this average masks substantial heterogeneity by type of bribe: the toll fee interaction lasts only 38 seconds, and a tip 43 seconds. This contrasts the time spent negotiating for a negotiation bribe, which is, on average, 7 minutes and can take up to 55.

4. THE EXPERIMENT

In this section, we present our experiment design. We conducted the experiment *in August and September 2015*. Independently for each driver, we randomly assigned *minibus-days* for the driver to drive in foreign lines those days.

4.1. *Rerouting to Foreign Lines*

Each driver had a driver-specific set of lines they considered *home lines*. The rest of lines were considered, for each driver, their corresponding *foreign lines*.[6]

For each driver, the rerouting *treatment* consisted of an encouragement to drive through lines considered foreign during a block of contiguous days. The *control* consisted in letting the driver choose his own route in the set of his home lines.

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[4]Figure A2 shows interaction types. Most of those are with street traffic police agents (Figure A3).

[5]Table A1, Panel (a), presents the per-trip equivalent.

[6]To determine which lines were home and foreign for each of the drivers, we organized meetings with the drivers prior to the experiment. The information was elicited from drivers' own reports, which was straightforward given the saliency of the home vs. foreign line distinction. On average, drivers had 1.3 lines that they considered home, and 8.1 foreign lines.

Due to initial uncertainty over the extent of funding, the study was initially planned to last working 23 days, covering the period from August 10th 2015 to September 4th 2015 (henceforth, *first wave*). The randomization procedure in this first wave was applied for each separate driver as follows.

- **Step 1: Partitioning into blocks**. We partitioned the first 23 days into nine blocks of contiguous days (henceforth, "*driving blocks*"). In that window, each driver had *seven* three-day driving blocks and *two* one-day driving blocks.
- **Step 2: Assignment to home vs. foreign**. First, 3 three-day-long driving blocks were randomly assigned to *home line*. The remaining four were randomly assigned to rerouting in a *foreign line*. All the days within each three-day block had the same home vs. foreign assignment. Second, 1 one-day-long driving block was randomly assigned to home, and the other foreign.
- **Step 3: Selection of the home and foreign lines.** In blocks assigned to home, the driver was asked to drive on their main home line. In each block assigned to foreign, we *randomly* selected one unique foreign line for all days of the block from the pool of foreign lines for that driver *without replacement*.
- **Step 4: Block sequence randomization.** In blocks assigned to home, the driver was asked to drive on their main home line. In each block assigned to foreign, we *randomly* selected one unique foreign line for all days of the block from the pool of foreign lines for that driver *without replacement*.

Upon securing additional funding, a second wave of study was added, covering four additional days, September 7-10th 2015:

- **Step 5: Second randomization wave**. The remaining four days were partitioned into 4 one-day driving blocks; then, two of these were randomly assigned to home driving, and the was randomly assigned to foreign. Among days rerouted to foreign, foreign lines were randomly selected as in the first wave, among the pool of the driver's foreign lines that had not been previously selected, in any wave, for the driver.

Table II provides a *stylized* (non-randomized sequence, sorted for presentation purposes) summary of this randomization procedure for a typical driver.

This randomization procedure was independently applied to ten drivers each with one main home line, thus totaling for 23+4=27 days for ten drivers, 270

TABLE II: Stylized Representation of Randomization Clusters for one Driver

First Wave: 23-day period	Second Wave: 4-day period
(1) Home line: 3 days (1 protected, 2 not)	(1) Home line: 1 day (unprotected)
(2) Home line: 3 days (2 protected, 1 not)	(2) Home line: 1 day (protected)
(3) Home line: 3 day (2 protected, 1 not)	(3) Foreign line 1: 1 day (unprotected)
(4) Home line: 1 day (protected or not)	(4) Foreign line 2: 1 day (protected)
(5) Foreign line 1: 3 days (1 protected, 2 not)	
(6) Foreign line 2: 3 days (2 protected, 1 not)	
(7) Foreign line 3: 3 days (1 protected, 2 not)	
(8) Foreign line 4: 3 days (2 protected, 1 not)	
(9) Foreign line 5: 1 day (protected or not)	

Notes: This table presents the design of experimental driving blocks of home vs. foreign lines and of third-party protection assignment. Each of the 9 driving blocks in the first wave (Aug 10-Sep 4, excluding Sundays) was sampled without replacement to construct a 23-consecutive-day calendar of driving with assigned lines/protections. The driving blocks in the second wave (Sep 7-10) were also sampled without replacement up to 4 days had been sampled from the block. The second wave was a result of unanticipated budget availability at the end of the first wave.

driver-day observations. Since the foreign/home assignment clusters are randomly assigned across 13 driving blocks of contiguous days each for each driver, the clusters, in two waves, the experiment is a stratified clustered randomization of 130 clusters, stratified within 20 randomization strata (the driver-waves). The sample consist in 13,092 events of crossing an intersection produced by 1,935 trips (comparable to, for example, to 304 trips in Olken and Barron (2009)). The characteristics in Table A2 appear balanced across foreign vs. home.

Drivers were compensated for *participating* in the experiment, and compensation was orthogonal to treatment status. This compensation was decided ex-ante per weeks-long conversations with the drivers. The compensation, 500 USD, was calculated based on the expected losses from driving on foreign lines on some days and not on other days. Only after the end of the study, drivers withdrew the funds. The compensation was presented as the team's acknowledgment that the requirement of rerouting would otherwise have been financially costly on the drivers. If a driver failed to comply with the randomization schedule, they risked exclusion from the experiment. A supervisor verified implementation each day.

Data collection, following the same strategy as in the pre-experimental data, is detailed in Appendices A1-A3. We recorded no violation of the assignment.

This design would call for reasonable spillover concerns. First, given the horizon of the experiment, it is unlikely that officers meeting new minibuses will crowd out their cooperation with drivers with whom they had a relationship; vice versa, it is unlikely that drivers with new officer ties would change their behavior with the officers in their home lines within the span of the experiment. We note that it is theoretically possible that drivers who create ties in a more profitable line may have a shorter horizon of interaction in their initial home line; reassuringly, this would tend to reduce cooperation in their home line, thus tend to weaken the estimated value of relationships. Second, informational spillovers across drivers and officers are unlikely given that relationships are bilateral, and the presence of protection for a driver or whether the driver is home or foreign driving does not carry information that is germane to how to interact with other drivers. Finally, our interventions are unlikely to have any effect on the equilibrium, given the experiment drivers' number pales in comparison to the hundreds operating daily.

4.2. *Cross-Randomization: Third-Party Driver Protection*

Independently for each driver, we provided, on randomly selected days, third-party *protection* from the portfolio of existing third-party protection. Drawing on pre-existing relationships we had established in a prior study, we identified a set of high-ranking police officers and of street-level officers who regularly offered their protection.[7] We also worked with ACCO to produce a sticker for display on the minibus' windshield that could be removed at the end of the day. Figure A4 presents one example of the sticker.[8] Table II includes a stylized representation.

The randomization procedure to third-party driver protection was as follows. First, we assigned the *police* protection. For each driver: (a) among the *three-day*

[7]Involvement of the non-Congolese members would have triggered expectations of high payment.

[8]The third-party protection we provided included, only in the days in which it was provided: a. Police escort that would travel through the day next to the driver; b. Third-party protection by colonels and majors; c. A sticker on the windshield with the official logo of the association of drivers of the Congo (ACCO), signaling that the driver was affiliated to the ACCO.

driving blocks, we randomly assigned some three-day blocks to benefit from third-party *police* protection in two days of the block (high intensity) or instead one day of the block (low intensity). (b) among the *one-day driving blocks*, we randomly assigned some one-day blocks to receive the police third-party protection, and the rest to remain without third-party *police* protection. This procedure ensured strict balance of home/foreign assignment for third-party police protection. This procedure was independently and identically repeated for each driver.

Second, we determined what exact *source* of police protection was given each time a driver had been assigned for third-party police protection as follows. For each driver, among the days assigned to third-party police protection, we randomly assigned the source of police protection across such days in two steps. In a first step, we randomly assigned the police protection day to be sourced from either high-ranking (colonels or majors) or street-level police protection (the police officers who sat with the driver in the minibus, henceforth "*police escort*"). In a second step, we randomly selected some days assigned to high-ranking protection for it to be provided by a colonel, and the rest by a major. This procedure was independently and identically repeated for each driver.

Third, we determined the allocation of ACCO sticker protection as follows. Stratifying by minibus-driving-block-protection status, each of two days in the three-day driving blocks that had the same police protection status has a 0.5 probability of being assigned an ACCO sticker. The remaining day, as well as the one-day driving block, has a 0.43 chance of getting a sticker.[9]

[9]We also overlaid another type of protection: protection by one of three captains. This was a priori the weakest, thus assignment is after the number of units in treatment cells defined by the previous treatments was exhausted. Wery day of the study, we randomly drew half of the drivers to receive protection by a captain. Its effectiveness was limited due to the fact that: a. they often did not respond to phone calls; b. it was often time consuming to find them and drivers found it preferable to ignore this protection; c. they had indeed very limited jurisdictions over which they had power; d. they even requested additional payments from the driver per each intervention requested; d. the tracking of events shows that the intervention of a captain assigned by this treatment was only requested 5 times; in one of these cases, the captain never responded. In what follows, we exclude this third-party protection from the analysis. The results are identical if we include captain protection as a control.

4.3. *Cross-Randomization: Time Horizon of Rerouting*

As described in Section 4.1, for each driver, nine of thirteen driving blocks comprised three days and the remaining four comprised one day (step 1), the assignment to foreign vs. home was stratified by by whether the driving block was three vs. one day (step 2), and the sequencing of the blocks across the study was randomized (step 4). Thus, the sequencing of shorter vs. longer driving blocks was random and strictly balanced by block duration.[10] Thus, the time-horizon of driving was cross-randomized over the assignment to foreign vs. home. While three-day blocks are arguably not a very long horizon, the duration was determined in qualitative preparative interviews, which revealed that horizons as short as three days could be sufficient to alter the drivers' inter-temporal incentives.

Table A3 presents the randomization balance for all treatment assignments. By design, assignment to foreign line rerouting is balanced, while assignment to any type of third-party protection is balanced. For this reason, we include randomization strata (minibus-wave) fixed effects when analyzing rerouting. Figure 2 presents a CONSORT diagram of the resulting experimental design.

4.4. *Artificiality of the Research Design and Participants' Beliefs*

The research team spent many months of preparation tailoring the design to the practices that are common in this community. First, it is common to have a third-party protector of various types. The nature of these arrangements is also sufficiently varied that our contract did not generate feeling of it being artificial. Section B2 in Online Appendix provides qualitative interview quotes on this aspect. Second, it is also common for drivers to explore new routes. Thus, the situations created by the study fit naturally in an ecosystem of situations that emerge endogenously as various actors are trying to get by. Finally, one aspect of the study is obviously artificial by construction: the drivers had access to an envelope at the end of the study. We take into account this feature when interpreting the speed of relationship formation. Admittedly, researchers paying

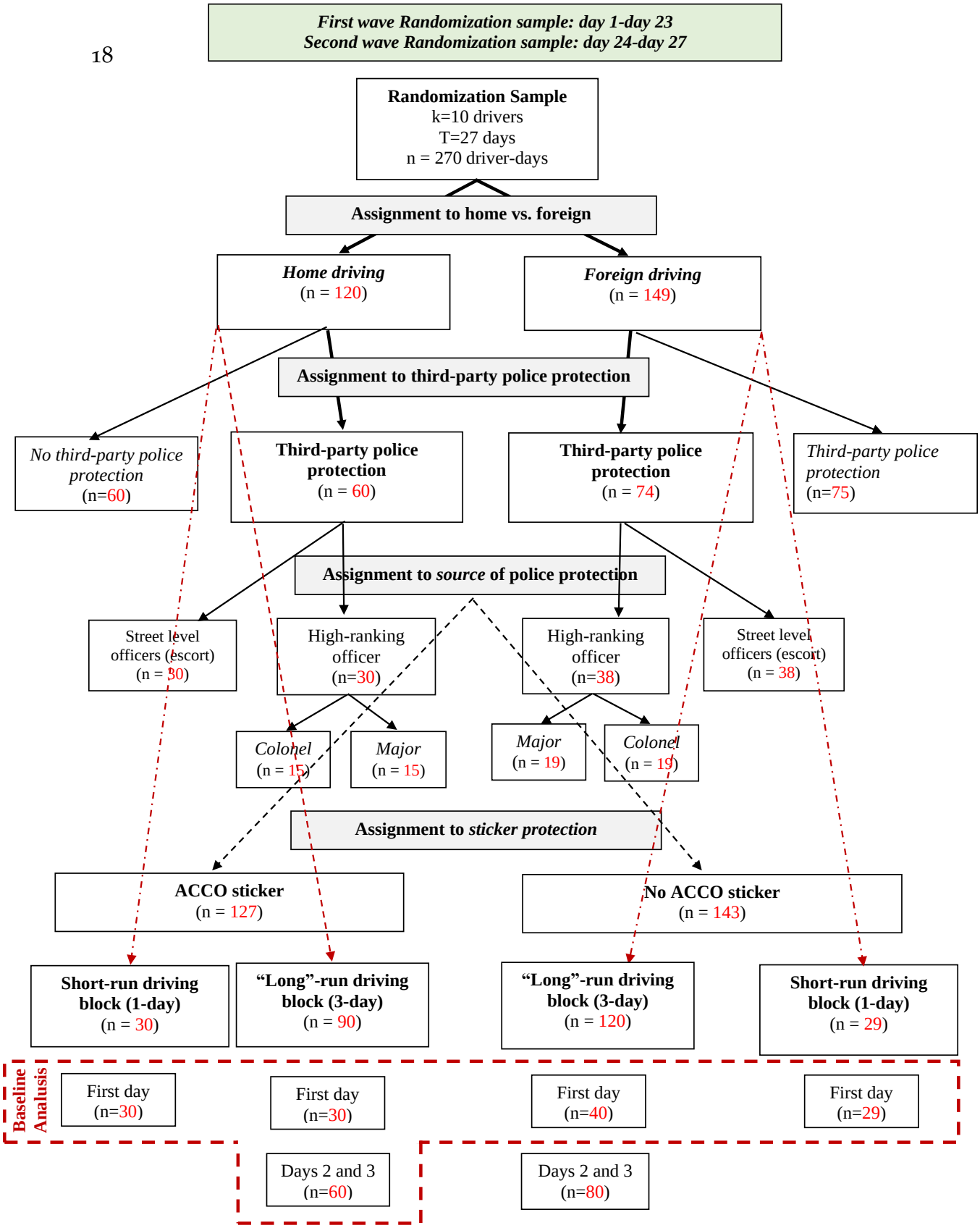
[10]Including this additional randomization strata as fixed effects is inconsequential for the estimation.

for protection could potentially achieve effects that could differ from protection that would have been obtained organically. Yet the effect of protection per se is not the question of interest: instead, we exploit that this type of protection breaks down the value of social ties with officers in the street, in order to isolate whether rerouting a driver influences joint profit because of the driver's expectations of the interaction with the officers, or instead other properties of the route.

Regarding the beliefs held by the drivers, drivers were made aware that they were part of a study but they *did not know the hypothesis being tested* nor that the interventions were part of an experiment. In training, drivers were informed that a Congo-based research team, led by a Congolese researcher from Kinshasa and by a Congolese research center from the east, sought funding to study minibus operations while also support the drivers' living conditions. Section B2 in Online Appendix provides a quote from an exit interview with the research manager illustrating this. Furthermore, for each driver, a calendar showed what unique line to drive each day and what type of recourse they had in case of problem. Thus, drivers knew *in advance* their driving and protection status. Our preparatory work suggest that this knowledge had no impact on their behavior vis-à-vis the police, such as how they relate with the police at the moment of interaction, other than influencing how they interact with the police. In addition, drivers were instructed to keep this information confidential, and we found no evidence that they shared this information with the officers on the street (we had observers sitting next to these interactions) nor officials about future assignments.

We noted that project coordinators warned drivers that they would not be protected outside their randomly assigned protection days. This makes perfect sense within the context of the experimental design, but a natural concern is that these instructions might have primed the drivers on what they are supposed to do in those days. We ensured that this type of priming did not occur. In training, drivers were notified that they could do whatever they would normally do in those days, and that we just could not guarantee their protection in those days even if they tried. Finally, the presence of the data collector in the minibus was not double-blind. This follows the practice introduced in Olken and Barron (2009). Furthermore, in our context, all of the interactions we observed between the driver

FIGURE 2.—Consort Diagram—Assignments and Samples



Notes: This figure presents a CONSORT diagram of the experiment design. The area labeled "Baseline analysis" denotes the sample used for the first analysis, referred to as "baseline analysis," for the effect of foreign rerouting before driving in a foreign line begins to resemble home driving (thus excluding days 2 and 3 of foreign line long-horizon rerouting to separately identify the effect of foreign over home from the influence of new relationships having already formed in days 2 and 3.)

and the officers were considered normal practice. The presence of the data collector in the minibus thus played no role in the drivers' behavior, except for its role to enforce compliance of the driver with the experimental variations. Furthermore, in exit interviews, we found that the regular presence of the observer was positive in the sense that it created a relationship of trust whereby the driver did not feel the need to conceal interactions.[11] Drivers were furthermore told that no future opportunity was held conditional on how they interacted with the officers on the road, and that the researchers wanted to conduct further studies on this topic. Drivers understood that there was a study component (through the data collection in minibuses). Qualitative interviews after the study suggested that drivers interpreted the re-routing as a way to help improve drivers' welfare by helping them create new profitable ties, especially because of the compensation and support provided by the research team—which they later continued to use. Thus, while we can certainly assert that drivers did not know the specific hypotheses being tested, it appears at least likely that they did not modify their behavior in foreign vs. home routes in expectation of receiving different benefits upon their behavior.

Regarding the third-party protectors' beliefs, those include captains, colonels, majors, and street-level police agents who travelled in the minibuses. They were told that as part of their collaboration with the ACCO, they were asked to share some of their relationship capital with a subset of the drivers and only in a subset of days to be determined. Section B2 in Online Appendix provides a quote from an exit interview with the research manager describing what was communicated. Thus, protectors perceived this to be normal practice. As a result, it is unlikely that the fact that their protection was requested as part of the experiment influenced the way they interacted with officers in the street when they were called.

When officers encountered a driver with protection, they were never made aware that the driver only had protection because they were part of the experiment (and not, say, because they had strong political connections prior to the

[11] For example, in a study exit interview we conducted in August 2018, the driver was asked about the benefits of the project, to which he answered: "First, there are good contacts with you and your observer. We often call each other now." Source: Exit interview with study driver, August 2018.

study). This made no difference between the home and foreign routes. Section B2 provides quotes from qualitative interviews that underpin the following facts about the design. First, they did not know the driver was part of a study. Second, street officers did not find protection by an officer travelling inside the vehicle artificial, nor knew that was part of a study. Third, street officers did not find protection by majors, captains, etc. artificial, nor were they made aware by the driver nor the protectors that this was part of a study. Fourth, finding drivers in foreign routes was not artificial, nor were the officers made aware that drivers on foreign lines was part of a study. Finally, street officers were not made aware that the driver had access to an envelope (the compensation for participating) that was provided by a study. In sum, the officers in the street acted without perceiving anything out of the ordinary as a result of our study.

Regarding learning by the participants, the creation of new relationships, buying the land, the value of protection by known police individuals, are normal practice. Furthermore, the rigid experimental protocol was rigorously adhered to, thus noncompliance could not induce correlation between driver blocks. Finally, foreign lines were sampled without replacement, preventing learning from a line. Officers, too, had limited scope for learning. In general, a driver in a home line does not need protection. In a foreign line, officers might learn that a driver has protection and this might change their behavior if the driver is seen at a later day. However, this was impossible due to the fact that drivers either drove just one day in a foreign intersection for the entire duration of the study, or in a three-day block, in which case they tended to build ties from the first day. The quote in Section B2 in Online Appendix provides a vivid example of such learning.

The Ethics Appendix discusses the ethics of the study at length. Here we focus on the important issue of (directly or indirectly) paying bribes. First, the project did not pay bribes. Second, drivers regularly paid bribes, and those were considered legitimate. There is a large literature discussing the legality and morality of bribes in the DRC: in a context in which many civil servants do not receive a state salary, or a very low state salary, certain bribes are considered legitimate by many actors, including drivers, police officers, and the citizens themselves (De Herdt and Titeca, 2019). To the extent that drivers had access to a large envelope at the

end of the study to compensate them for participating, and that they received a daily compensation for the seat of the data collector, this implies that, indirectly, drivers might have used some of our compensation to pay these bribes. Potentially important, the existence of this compensation might have incentivized them to pay bribes where they would otherwise have chosen not to. For example, in reality a driver might not have chosen to drive in a particular foreign line when instructed, as it exposed them to the need to pay a bribe. Three important features might help assuage these real concerns. First, the project did not incentivize illegal behavior (whether the drivers complied to the law was determined prior to the study, and driving in a foreign line is as legal as driving in a home line). Second, the third-party enforcers were called leveraging pre-existing relationships with the ACCO, who had their own agreements with them, hence the project did not pay bribes to these individuals. Third, extending third-party protection did not promote or help conceal illegal behavior; instead, it reduced corruption between drivers and officers (the alternative was to pay a bribe). Fourth, The short-lived interventions limited opportunities for the participants to make investments that would have amounted to encouraging illegal behavior. We even detected positive law compliance effects: "The biggest impact is that I used to work without a driver's license, and I was often stopped as a result. However, thanks to the project, I was able to obtain one." (Interview with driver, August 2018)

4.5. *Characteristics of the Data*

The experiment dataset contains 13,092 events in which a driver crosses an intersection, obtained from 1,935 trips made by the ten drivers over 27 days. Of these 13,092 events, there are 1,682 payments from drivers to officers, 1,514 of which are toll fees, 62 are tips, 97 are negotiation bribes resolved in the street and 9 are negotiation bribes resolved as a of bribe in the police station. Overall, there are 38 events of escorts to the police station, of which 9 are those bribes and the remaining 29 ended without payment of any bribe. The number of unique officers interacting with the drivers is 344, of which 296 are street-level traffic police officers, 26 are ACCO agents, and 22 are other state officials. The drivers performed 8 trips on average per day, amounting to 4 round-trips, returning

to their starting termini. On average, a trip has 6.5 intersections and lasts 50.9 minutes.[12] Overall, there are 120 driver-days in which the driver drives in their corresponding home line and the remaining 149 are driver-days on foreign lines. The average price of a trip is 47 cents USD, in USD of 2015. This price is variable (standard deviation of 7 cents), and ranges from 22 cents to 53 cents. the average number of passengers in a trip is 18.02, and is also variable (standard deviation of 3.66 passengers), ranging from 0 to 32 passengers for a trip.[13]

5. EFFECT OF REROUTING ON SOCIAL TIES ON THE LINE AND ON JOINT PROFIT

We now analyze whether rerouting to foreign lines reduces the prevalence of driver-officer interactions in those days in which the driver and the officer have a tie, and whether it reduces the joint profit. To isolate the role of social ties in rerouting, we exclude in this analysis from the pool of rerouting the observations that constitute the second or the third day of the rerouting block. Indeed, in those observations, the driver can have already endogenously created new social ties with the officers on the rerouted line, confounding the effect relationships.

5.1. *Estimation*

Let i index the intersection, r index the round (trip) number, d index the driver, and t index the day-of-study. Let S_{irdt} be an indicator taking value 1 if the driver has a social tie with the police agent with whom he interacts and $\mathbb{1}_1(\text{Foreign}_{irdt}) \in \{0,1\}$ be an indicator variable taking value 1 if an observation indexed $irdt$ is assigned to the first day of a foreign line rerouting, and zero otherwise. Using the driver-day-trip-intersection dataset, we estimate the effect of rerouting on the fraction of officers on the driven line in which the driver has a social tie by estimating following equation using OLS:

$$S_{irdt} = \beta^F \mathbb{1}_1(\text{Foreign}_{dt}) + \alpha_{dt} + \epsilon_{irdt}, \tag{1}$$

[12]Table A4 presents these characteristics. Figure A5 shows a map of the experiment lines.

[13]Table A5 presents the same analysis as Table I using, instead, the data from the experiment, for the observations on home lines. It shows that the experiment dataset is comparable to the pre-experiment benchmark, providing reassurance about the external validity of the experiment.

where α_{dt} denotes the foreign/home line *randomization strata* (i.e., driver-wave) fixed effects. There are ten drivers and two waves, totalling 20 driver-wave randomization strata fixed effects. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization. There are 13 *driving blocks* for each of the drivers, totalling 130 driver *driving blocks*. We account for the probability, implied by our randomization procedure, that a given day, a given driver is assigned to home vs. foreign driving.[14] We thus include inverse propensity weights using these probabilities in weighted OLS (henceforth "*rerouting weights*").[15] We refer to this as the baseline specification, and refer to β^F in that specification as the average treatment effect (henceforth, *ATE*), the average effect of "driving on a foreign line, given the pool of foreign lines available."

Joint profit is measured at the level of each trip driven. Thus, to estimate the effect of rerouting on joint profit, we aggregate the data at the level of the driver-day-trip. The dependent variable is J_{rdt}, which is the joint profit in US dollars, computed as total revenue on the trip driven minus gas and repair costs; revenue on the trip driven is computed as the number of passengers on the trip times the typical price, as reported by the observer. We estimate:

$$J_{rdt} = \beta^F \mathbb{1}_1(\text{Foreign}_{dt}) + \alpha_{dt} + \epsilon_{rdt}. \tag{2}$$

In robustness analysis, we also estimate the following specifications. First, we exclude from the baseline specification the randomization weights and the randomization strata fixed effects; we refer to this as the sample average treatment effect raw ($SATE^{raw}$). Second, we exclude from the baseline specification the randomization weights, but include the randomization strata fixed effects; we refer to this as the sample average treatment effect (*SATE*). Third, to account for the fact

[14] To recover the probability of assignment to foreign or home lines, we conducted 10,000 simulations of the randomization procedure, and computed the probabilities of assignment to foreign and home lines as the fraction of said randomization simulations, which produced a random assignment to foreign vs. home lines. We do so accounting for the randomization of the driving blocks sequence.

[15] Since the driving blocks sequence was randomized prior to the assignment to foreign, and since the fraction of home vs. foreign days was also balanced, inclusion of these weights, while producing the correct estimator, has no significance on the estimated coefficients.

that the pool of lines available for foreign driving may be systematically different to the set of home lines, we add line fixed effects to the baseline specification; we refer to this as the average treatment effect controlling for line fixed effects (ATE^C). Fourth, to alternatively account for the possible imbalance in foreign and home lines, re-weighting observations by the inverse probability that a line appears as a foreign or home line for a given driver (henceforth, "*composition weights*"); we refer to this as the population average treatment effect (PATE).

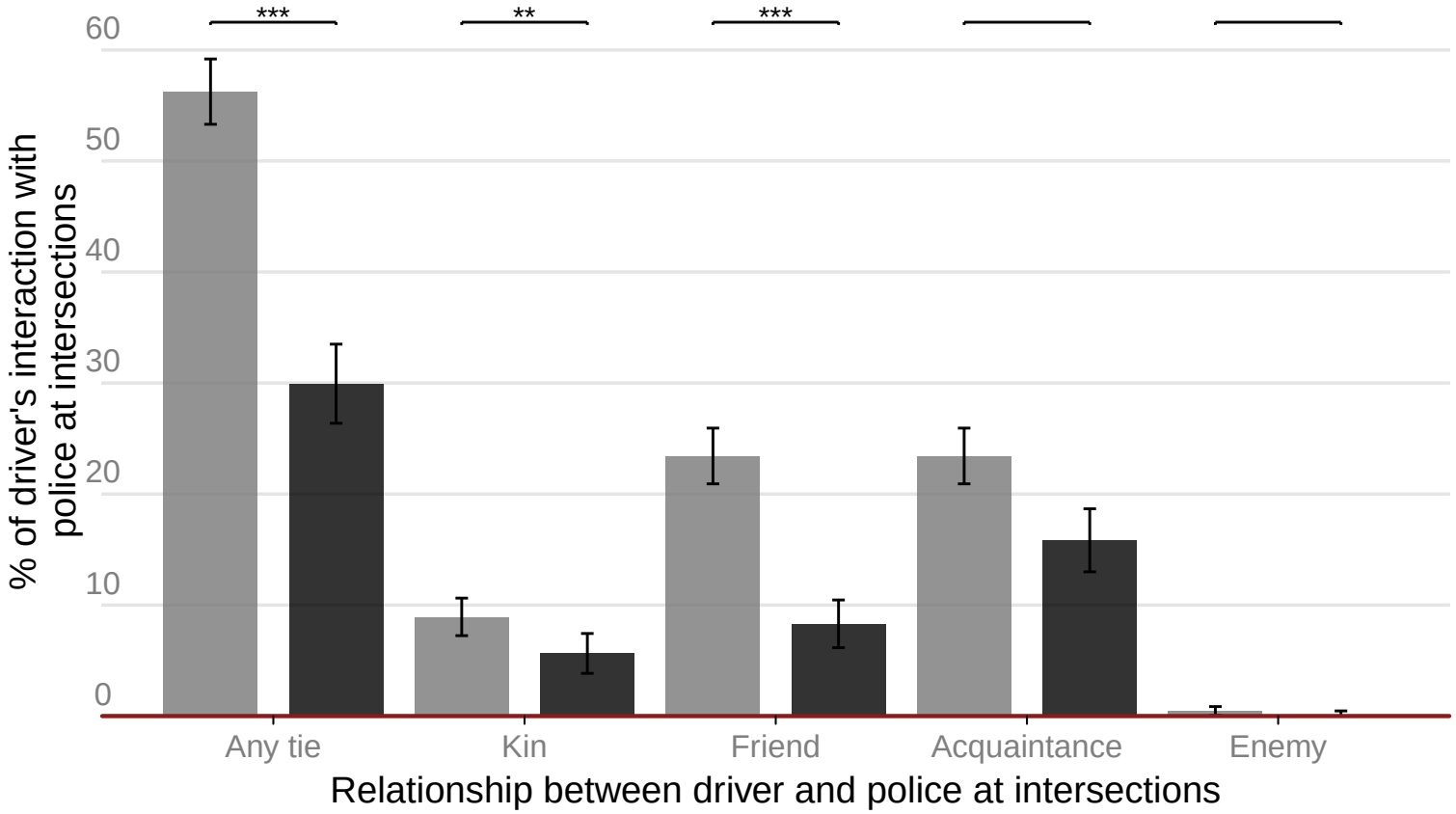
We first analyze the effect of rerouting on the fraction of interactions where the driver and officer have a social tie. Interaction with street-level traffic police officers make up the vast majority of interactions (881 out of 1,097).[16] The remainder are with the "police of the police" (78)—an agency purportedly in charge of auditing police behavior—with police officers at parking station (71), with the leader of a team of traffic police officers (15), with the biker police (21), and with non-police officers (31). Of these 881 interactions, 43.8% are without relationship, while in 23.4% the driver and the agent are acquaintances, in 23.4% they are friends, and in 8.9% they are kin. Figure 3 shows that while 56.2% of interactions with officers at the intersection are characterized as having a relationship on home lines, this fraction decreases to 29.9% on foreign lines.[17] The effect arises from a decrease in relationships described as friendships, for which the fraction decreases from 23.4 to 8.3%. Given that this large number of interactions is generated by a small number of drivers, it is important to note to interpret this figure that the unit of the observation is the tie. These ties are induced by 344 unique officers interacting with the drivers in the experiment's dataset.

We then analyze the effect of rerouting on the joint surplus. We construct the joint profit as the revenue from passenger payments, net of gas expenses and repairs. The joint profit is then allocated between the police officers through

[16]Source: Figure A6. The data are observations for which the driver is not assigned to any treatment hence represents the system in its baseline state of home lines. Of the 13,092 number of times a driver crosses through an intersection, 6,774 are in a home line. Of those, there are 1,097 occurrences in which the driver directly engages with a state official at the intersection.

[17]Figure A7 shows the result holds equivalently per intersection. Figure A8 compares the baseline coefficient estimate to alternative robustness specifications that account for this imbalance.

FIGURE 3.—Effect of Rerouting on Fraction of Interaction with Police Officers in Which Driver and Officers Have a Social Tie



Notes: This figure presents the prevalence of social relationships between drivers and police officers at the intersections, expressed as the percentage of drivers' 1,097 interactions on their usual "home line" and 638 interactions on the "foreign line" on their first day of rerouting. The line segments represent 95% confidence intervals around the reported means. The asterisks report the segments of the p-value of the test for whether the fraction in the home line is different than that on the foreign line, for each relationship category: *** indicates that the p-value is equal or lower to .01, ** indicates that it is larger than .01 but lower than .05, and * indicates that it is larger than .05 but lower than .1. This test is implemented through OLS, by estimating β^F of Equation 1, with standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster; since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. Kin includes both nuclear and extended family relationships. Acquaintance category is different from friend category (*ami* in French) in two ways. First, in some cases, the drivers' directly reported acquaintance (*connaissance* in French). Second, we analyzed the content of the conversation between the driver and the officer, and were able to deduce whether the drivers and the officers were acquainted.

TABLE III: Effect of Rerouting to Foreign Lines on the Joint Profit

	Joint Profit (per day)	Number of trips (per day)	Joint Profit (per trip)
	(1)	(2)	(3)
Foreign	-9.142^{**}	-0.967^{***}	-0.874^{*}
	(4.013)	(0.360)	(0.456)
Mean Dep. Var.	18.6	7.86	2.62
Observations	167	176	1,395
R^2	0.337	0.328	0.204
Rand. Strata FE	YES	YES	YES
Foreign IPW	YES	YES	YES
Composition IPW	YES	YES	YES

Notes: This table presents the OLS estimates of β^F from Equation 2. In Column (1), the dependent variable is the daily joint profit in USD. Columns (2) and (3) decompose the joint profit per day into the number of trips per day (Column 2) and the joint profit per trip in USD (Column 3). Joint profit per trip (Column 3) is revenue in USD minus operating cost per trip in USD, where revenue is derived from number of passengers per trip times the average price in USD each passenger pays per trip, and operating cost per trip is repair and gas cost per day divided by the number of trips per day. As the table notes indicate, all regressions include randomization strata fixed effects, and observations are weighted by inverse propensity scores to account for the randomized assignment procedure (*rerouting weights*) and to adjust for imbalance on foreign line composition (*composition weights*). Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. In subsequent tables, we no longer indicate inclusion of these fixed effects and weights in the table notes due to redundancy—it is the same specification, except in robustness where indicated. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the dataset of home line driving.

bribes, and the driver through profits. On average, on home lines, drivers make 3 USD of joint profit per trip.[18] This is generated from 8.79 USD in revenue from passengers, net of 5.79 USD operating costs per trip.[19] The 8.79 USD in revenue is produced by 18.36 passengers on average, who pay 48 cents of USD on average.[20] Table III presents Equation 2 estimates for daily joint profit (Column 1), decomposed in number of trips per day (Column 2) and joint profit per trip (Column 3). Rerouting decreases daily joint profit: in Column (1), the coefficient on rerouting is negative and statistically significant at conventional levels. Column

(2) shows that this effect comes, in part, through a reduction in the number of trips per day. Again, the coefficient is negative and statistically significant. This is consistent with the driver wasting more time negotiating with the police and providing less productive effort of the type that would allow him to do more trips. Column (3) shows that the joint profit per trip also decreases, consistent with the driver not making productive effort in anticipation of being more vulnerable to stops by police officers on foreign lines. Table A7 present alternative specifications to account for mis-specificaiton and outliers.

In sum, rerouting reduces the prevalence of relationships between the drivers and the officers with whom they interact and the joint profit.

6. SEPARATELY IDENTIFYING SOCIAL TIES: THE THIRD-PARTY PROTECTOR PLACEBO

In this section, we unbundle these effects by whether the driver benefits from third-party protection which, independently of whether protection mimicks relationships or whether it amounts to organically created protection, is a treatment that allows to shut down the channel of expectations of interaction with the police.

In Table IV, Panel A, we present the coefficient estimates of Equation 2, if the driver was randomly assigned to third-party police protection vs. not. Column (1) shows the effect of rerouting on the joint profit per day, for minibus-day observations in which the driver has not been assigned to third-party protection. In that case, the joint profit drops from 22.02 USD per day on home lines by 18.67 USD, and the drop is statistically significant. Column (2), in contrast, shows that rerouting decreases joint profit by -4.49 USD and this coefficient is insignificant. This suggests that rerouting reduces the joint profit through its effect on the driver's anticipation of the officers' behavior. Columns (3)-(6) replicate this analysis for the number of trips per day and the joint profit per trip. Rerouting reduces the number of trips per day, but only when the driver is unprotected. This is consistent with it not being optimal for drivers to create new relationships if they

[18]This description is based on Table A6.

[19]Some costs, such as gas, are not incurred per trip. We impute the per trip average in this table.

[20]A complementary graphical representation of the joint profit decomposition is in Figure A9.

already have third-party protection for at least one day in the three, but it could also indicate that the investment in new relationships is less costly if the driver can leverage the third-party protection. Likewise, rerouting only decreases trips per day or joint profit per trip if the driver does not have third-party protection.

To identify whether the effect of rerouting is significantly smaller when the driver is protected, we estimate the following equation:

$$J_{rdt} = \beta^{FNP}\mathbb{1}_1(\text{Foreign}_{dt}) + \beta^{FP}\mathbb{1}_1(\text{Foreign}_{dt}) \times \mathbb{1}_1(\text{Protection}_{dt}) + \alpha_{dt} + \epsilon_{rdt}, \quad (3)$$

where $\mathbb{1}_1(\text{Protection}_{dt})$ is an indicator variable taking value 1 if the observation dt is assigned to third-party protection, and thus the coefficient β^{FNP} estimates the effect of rerouting when unprotected, and β^{FP} estimates how much larger than the latter is the effect of rerouting if the driver is protected. We report these estimates in Panel B.

Across columns, β^{FNP} is negative, large, and statistically significant, indicating that, when unprotected, rerouting significantly decreases the joint profit per day, arising both from a reduction in the number of trips and the joint profit per day; the coefficient β^{FP} is of similar magnitude and of the opposite sign, and is statistically significant in Column (1). This shows that the effect of rerouted is not only muted when the driver is protected, but it is also statistically distinguishable from the effect when the driver is unprotected.

Tables A9 and A10 replicate the above analysis, decomposing third-party protection into the two main types: whether the driver was assigned to wear the ACCO drivers' association sticker, and whether it was assigned to police protection. The results are the same for each type of protection, which provides reassurance in the main analysis. Supporting that protection shuts down threats from the police, our qualitative interviews paint a picture about drivers' willingness to pay for such protection :

> Protection by officer in the minibus remains the absolute protection. It puts everyone at ease. This protection allowed the driver to save the money they had for *mbote ya likasu*; drivers loved it. Of all protections, the most powerful was the officer on the minibus. No arrest, no harassment, no *mbote ya likasu*. The vehicle is respected, officers let pass as if it were an ambulance. (Project implementation meeting notes of September 4$^{\text{th}}$ 2015)

TABLE IV: Effect of Rerouting to Foreign Lines on Joint Profit, *by Third Party Protection*

A. Baseline Coefficient, With and Without Third-Party Protection

	Joint Profit (per day)		Number of trips (per day)		Joint Profit (per trip)	
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign	−18.67**	−4.49	−1.48**	−0.66	−1.66*	−0.56
	(7.14)	(4.61)	(0.61)	(0.48)	(0.87)	(0.49)
Mean Dep. Var.	22.02	22.24	8.21	8.27	2.95	3.01
Observations	47	120	50	126	388	1,007
R^2	0.65	0.40	0.55	0.31	0.47	0.24
Protection, Any	NO	YES	NO	YES	NO	YES

B. Baseline Coefficient, Decomposed by Third-Party Protection

	Joint Profit (per day)	Number of trips (per day)	Joint Profit (per trip)
	(1)	(2)	(3)
Foreign	−19.83**	−1.55**	−1.78*
	(8.02)	(0.60)	(1.01)
Foreign * Protection, Any	16.33*	0.91	1.41
	(9.31)	(0.70)	(1.21)
Mean Dep. Var.	22.24	8.27	3.01
Observations	167	176	1,395
R^2	0.34	0.27	0.21

Notes: Panel A presents the OLS estimates of β^F from Equation 2, decomposed by whether the driver-day was assigned to not having any third-party protection (odd columns) and to having third-party protection (even columns). In Columns (1) and (2), the dependent variable is the driver's joint profit per day in USD. Columns (3), (4) and (5), (6) decompose the joint profit per day into the number of trips per day (Columns 3 and 4) and the joint profit per trip in USD (Columns 5 and 6). Joint profit per trip (columns 5 and 6) is revenue minus operating cost per trip, where revenue is derived from number of passengers per trip times the average price each passenger pays per trip, and operating cost per trip is repair and gas cost per day divided by the number of trips per day. Panel B presents the OLS estimates of β^{FNP} and β^{FP} from Equation 3. In Column (1), the dependent variable is the daily joint profit in USD. Columns (2), (3) decompose the joint profit per day as dependent variables into the number of trips per day and the average joint profit in USD along each trip, respectively. Third-party protection is assigned in the form of police escort on the minibus, access to higher-level police, or Association of Drivers (ACCO) sticker on the minibus. All regressions include randomization strata fixed effects, and observations are weighted by inverse propensity scores to account for the randomized assignment procedure (*rerouting weights*) and to adjust for imbalance on foreign line composition (*composition weights*). Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full dataset of home line driving.

In sum, this section has presented evidence suggesting that the effect of rerouting is *muted* if the driver has third-party protection. This suggests that rerouting to a foreign line only captures the effect of rerouting on driving surplus through the properties of the (expected) relationship between the driver and the officers.

7. MECHANISMS: HOW ABSENCE OF SOCIAL TIES DESTROYS JOINT PROFIT

To determine the channels by which relationships increase joint profit, we now examine the randomized variation in rerouting horizon. Personal Webpage Appendix offers a simple motivating framework to guide the analysis of the effect of rerouting on the joint profit. We now examine how the short-term vs. long-term rerouting affects the joint profit per trip and the number of trips per day.

Table V presents the estimates from Equation 2 for joint profit per day (Panel A), as well as for number of trips and proxies of time waste (Panel B) and of joint profit per trip (Panel C). Panel A shows that rerouting reduces joint profit, although the effect is only significant in long-term rerouting, and the estimate is larger. This suggests rerouting leads drivers to take joint profit-reducing actions if they expect that the interaction will be repeated, as investment.

The analysis in Panel B suggests that rerouting to a foreign line decreases the number of trips per day (Columns 1 and 2) if the driver has a long-term driving horizon but does not when the driver has a short-term driving horizon. This is consistent with the driver in long-run rerouting taking actions that sacrifice the number of trips in order to invest in new relationships. While the estimates for number of negotiations per trip are of similar magnitude with short- and long-run rerouting, the estimate for time waste is three times larger in long-run rerouting (Columns 5 and 6). Consistent with the driver making more costly investments in new relationships (e.g., "buying the land"), Columns (7) and (8) show that, if and only if rerouting is long-run, the bribe costs significantly increase. The effect amounts to an 72% increase in bribe costs compared to home.

The analysis in Panel C shows that short- and long-run rerouting decrease the joint profit per trip (Columns 1 and 2), but through different channels (Columns 3-10). Columns (3), (4) show that rerouting decreases the number of passengers per trip in both, although the estimate is 40% larger in long-run rerouting; Columns

TABLE V: Effect of Rerouting on Components of Joint Profit, by Time Horizon

A. Main Effect: Daily Joint Profit (Number of Trips per day × Joint Profit per Trip

	Joint Profit (per day)	
	(1)	(2)
Foreign	−7.47	−9.69*
	(5.39)	(5.68)
Mean Dep. Var.	22.19	22.19
Observations	137	139
R^2	0.34	0.32
Time Horizon	Short	Long

Notes: This table presents the estimates from Equation 2 for joint profit per day in USD (Panel A), for number of trips and proxies of time waste (Panel B) and of joint profit per trip (Panel C), decomposed by whether the driver-day was assigned to the first day of long-term rerouting (driving blocks of 3 contiguous days) or short-term rerouting (driving blocks of only 1 day). We present the estimates of β^F separately for the dataset in short-term rerouting (odd columns) and long-term rerouting (even columns). *Dependent variables*: In Panel B, the dependent variables are the number of trips per day (columns 1 and 2), in which case the dataset is at the driver-day level. The dependent variables in the remaining columns, which are from the dataset at the driver-trip level, are as follows: the number of events in which the driver engages in a negotiation with a police officer (Columns 3 and 4), the duration of interaction between driver and police along a minibus trip in minutes (Columns 5 and 6), and the total bribe costs for the trip in USD (Columns 5 and 6). In Panel C, the dependent variables are all at the driver-trip level, and are the joint profit per trip in USD, computed as the revenue from passenger tickets minus gas and repair costs (Columns 1 and 2); gas and repair costs in USD (operating costs) are observed at the daily level and imputed at the trip level. The dependent variables in the remaining columns, which are also in dataset at the driver-trip level, are as follows: the number of passengers that purchase a ticket for a ride (Columns 3 and 4), the average price paid by passengers on a trip in USD (Columns 5 and 6), the operating costs in USD, which are composed of the gas costs and repair costs observed at the driver-day level and imputed at the driver-trip level (Columns 7 and 8), and the duration of a trip in minutes (Columns 9 and 10). All regressions include randomization strata fixed effects, and observations are weighted by rerouting and composition weights. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full dataset of home line driving.

B. Component 1: Number of trips (and actions that affect it)

	# Trip (per day)		# Negotiation (per trip)		Time waste (per trip)		Bribe costs (per trip)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Foreign	−0.40	−1.28**	0.07**	0.05	0.31	1.08	0.11	0.62*
	(0.55)	(0.51)	(0.04)	(0.05)	(0.29)	(0.88)	(0.19)	(0.34)
Mean Dep. Var.	8.25	8.25	0.11	0.11	1.16	1.16	0.86	0.86
Observations	143	148	1,165	1,187	1,165	1,187	1,165	1,187
R^2	0.31	0.29	0.12	0.10	0.10	0.04	0.07	0.04
Time Horizon	Short	Long	Short	Long	Short	Long	Short	Long

C. Component 2: Joint profit per trip (and actions that affect it)

	Joint Profit (per trip)		# Passenger (per trip)		Price (per trip)		Operating cost (per trip)		Duration (per trip)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Foreign	−1.10*	−0.70	−0.39*	−0.54**	−0.04**	−0.02	0.31	0.03	0.78	1.17
	(0.57)	(0.63)	(0.23)	(0.25)	(0.02)	(0.02)	(0.61)	(0.56)	(2.28)	(3.31)
Mean Dep. Var.	3	3	18.36	18.36	0.48	0.48	5.81	5.81	31.83	31.83
Observations	1,159	1,181	1,161	1,183	1,160	1,182	1,165	1,187	1,162	1,184
R^2	0.21	0.20	0.76	0.79	0.70	0.48	0.14	0.13	0.27	0.15
Time Horizon	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long

(5), (6) show that short-run rerouting significantly reduces the passenger price, while long-run rerouting does not, consistent with drivers in short-run rerouting negotiating with the passengers in a rush as they fear being detained by officers; Columns (7)-(10) show that short- and long-run rerouting both fail to significantly affect operating costs and trip duration.

Overall, this suggests social ties reduce the risk to the driver to be shaken down, enabling them to attract passengers and negotiate a higher price without incurring such risk; while creating new relationships comes at an additional reduction in the joint profit, from where a larger bribe cost is also netted. This is consistent with our qualitative evidence, presented in Online Appendix, Section B3.

8. "THROUGH THESE [ARRESTED] DISCUSSIONS, WE BECOME FRIENDS!"

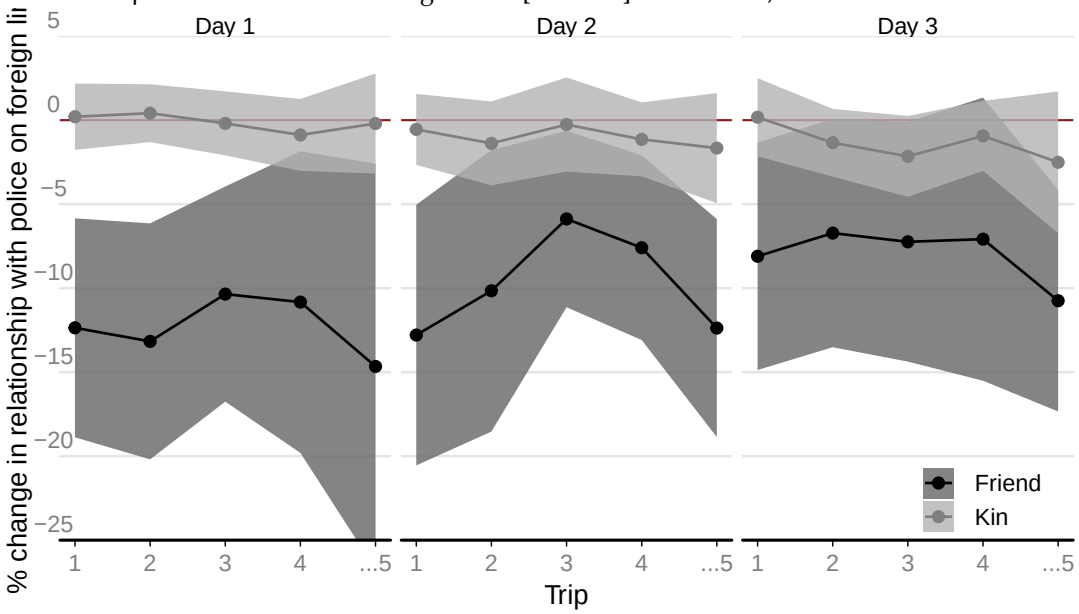
We examine whether rerouting leads to the creation of ties. We separately estimate the following equation for each day of the three-day rerouting blocks:

$$S_{irdt} = \beta^F \mathbb{1}_1(\text{Foreign}_{dt}) + \nu_r + \beta_r^F \mathbb{1}_1(\text{Foreign}_{irdt}) \times \mathbb{1}_r(\text{Round}_{irdt}) + \alpha_{dt} + \epsilon_{irdt}, \tag{4}$$

where the dependent variable S_{irdt} is an indicator taking value 1 if the driver has a social relationship the police agent with whom he interacts, $\mathbb{1}_1(\text{Foreign}_{irdt})$ and $\mathbb{1}_r(\text{Round}_{irdt})$ are indicator variables taking value 1 if observation indexed $irdt$ is assigned to a Foreign rerouting, or if the round rank ($\text{Round}_{irdt} \in \{0, N\}$) in the day is r, respectively. The parameter α_{dt} is the randomization strata fixed effect. In addition, ν_r captures the round number fixed effect. Thus $\beta^F + \beta_r^F$ captures the treatment effect of rerouting on the prevalence of kinship/friendship on trip r on each day of the three-day driving blocks.

In Figure 4, we present the estimates of $\beta^F + \beta_r^F$ from Equation 4, where the dependent variable is an indicator for whether the driver and the officer are connected. The coefficients in day 1 and 2 are negative and their confidence intervals do not overlap with zero. This means that, in days 1 and 2 of a foreign long-term rerouting driving block, the driver and the officers are much less likely to be friends in any round, compared to the rounds driven in an average day in home line. However, the coefficients get closer to zero by day 2, and, by day 3, they

FIGURE 4.—Social Return: "Through these [arrested] discussions, we become friends!"



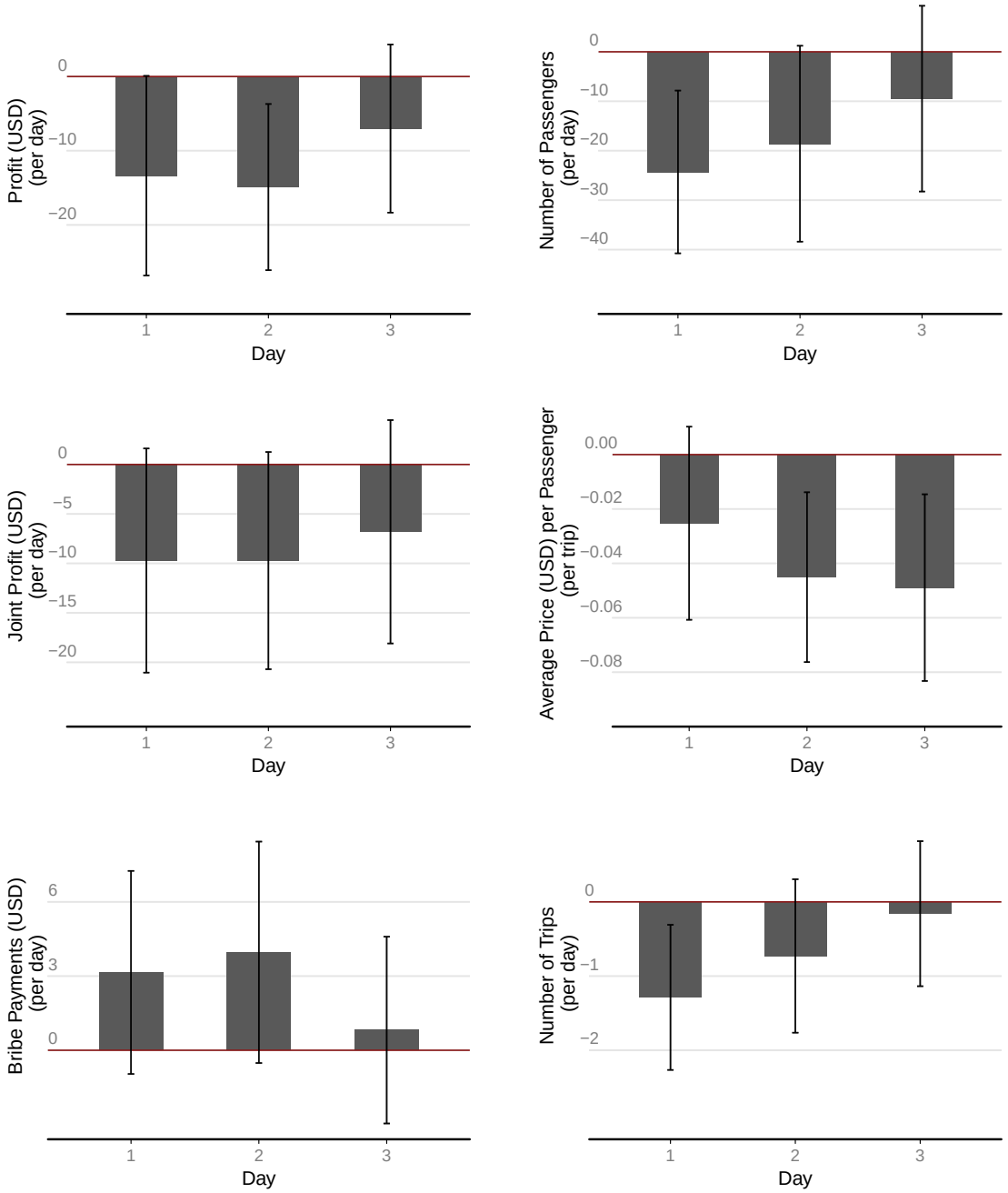
Notes: This figure presents the sizes and their 95% confidence intervals of the estimates of $\beta^F + \beta^F_r$, for each round (trip) r within each of the three days of the home and foreign long-run rerouting driving blocks of Equation 4. The coefficients and confidence intervals presented in light gray are the estimates when the dependent variable is an indicator for whether the driver and the officer are kin; those in dark gray are the estimates when the dependent variable is an indicator for whether the driver and the officer are friends. The figure presents estimates from three separate regressions, one for each day of the long-term rerouting, from left to right: day 1, day 2, and day 3. The control dataset includes all observations on the home line. All regressions include randomization strata fixed effects, and observations are weighted by rerouting and composition weights. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction 13, clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. Trip is defined as a return trip between two termini. The number of trips is winsorized at 5 per day. The number of observation above 5 is extremely small.

are already indistinguishable from zero in most rounds of the day. This suggests that the driver and the officers are becoming friends as day pass in a long-term rerouting, consistent with the interpretation that the driver makes a costly investment in the first day, which results in the creation of new relationships (he buys the land). In contrast to friendship, we find no effect of rerouting on kinship on any day; thus, the increase in the fraction of interactions on the foreign line where the driver and the officers are friends is not driven by spurious correlations.

The initial investment of on day 1 of long-term rerouting is followed by the formation of new social ties in the following days. If this investment has an economic benefit for the driver, we should also see that the profit of the driver improves in the days that follow, in the same foreign line. Figure 5 presents the

FIGURE 5.—Economic Return: Profits Recover, Driven by Bribes and Number of Trips

A. Daily Profit, Decomposed *B. Components of Joint Profit*



Notes: This figure presents the coefficient estimates of β^F of Equation 2, separately estimated in the following three datasets: the dataset of home line driving and of first day rerouting in a long-run (3-day) driving block (left bars); the dataset of home line driving and of second day rerouting in a long-run (3-day) driving block (middle bars); the dataset of home line driving and of third day rerouting in a long-run (3-day) driving block (right bars). The magnitude of the coefficients estimated in these three datasets is represented as the height of the bars, respectively from left to right. The bars also include the corresponding 95% confidence intervals represented as brackets. The interpretation of the bar's height as the coefficient magnitude therefore implies that the bar's height is the estimate of how much larger is the corresponding dependent variable in the corresponding day of long-term (3-day) rerouting, compared to the average day on a home line; if the confidence interval overlaps with zero, the difference is not significant at the 5 % level. In Panel A, we present this analysis for *profit per day* (joint profit per day, net of bribe costs) and its decomposition, that is, for the following three dependent variables: profit from driving per day (joint profit minus bribes); joint profit per day (revenues from passenger rides minus gas and repairs per day); bribe payments in USD per day. In Panel B, we present this analysis for the following three components of joint profit per day: number of trips; number of passengers per trip; average price per passenger for the average trip in USD. All regressions include randomization strata fixed effects, and observations are weighted by rerouting and composition weights. Trip is defined as a return trip between two termini. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata.

estimates for the effect of long-horizon rerouting, separately for each of the three days of rerouting, for the driver's profit and its components (Panel A), and for the components of the joint profit (Panel B).

Panel A shows that rerouting to foreign lines for three days significantly decreases driver profits in days 1 and 2, but profits have already recovered their average home line level by day 3. This effect comes from a reduction, and partial recovery of the joint profit (revenue minus gas and repairs), and by an initial increase in bribe payments, which then recover its home line level by day 3 as well. This suggests that the costs incurred in day 1, in terms of large bribe payments and actions by the driver that reduce the joint profit, translate not only into new social ties with the officers, also in higher profit.

The initial reduction and recovery of profit comes in part from the initial bribe costs, but also, in part, from the dip in the joint profit from driving. Panel B replicates the analysis separately for the various components of the joint profit. The first and last exhibit confirm that the initial dip in the joint profit arises from a reduction in the number of trips per day and as a result, the number of passengers per trip, and then those catch-up. This is consistent with our interpretation that drivers in the first day of long-run rerouted are willing to lose driving rounds above and beyond the losses incurred in short-term rerouting, while they negotiate with the officers to buy the land. It also underscores the importance of excluding days 2 and 3 from the main analysis of rerouting as, by day 2, the number of trips is already catching up just like the prevalence of social ties. The catch-up suggests that the initial bribe and time investments made by the driver, anticipating a long-run rerouting, allow the driver to recover the normal number of trips and of passengers once with social ties.

The second exhibit of Panel B allows to verify the interpretation of the results so far. It shows that the price charged to the passengers does not initially dip (as the driver might be interested in negotiating with the officers and thus unconcerned and potentially interested in being detected); after the first day, the price remains significantly lower than in the home line. This pattern is consistent with the finding reported in Figure 5 that friendships do not fully catch-up. It suggests that, after three days, the driver has created productive social ties with officers on the

line and is less involved in creating new ones, but new ties have not formed with many of the new officers, and hence that the driver is still somewhat vulnerable to shake down. This is also consistent with our analysis of short-term rerouting in the previous section, which showed that, when drivers have no incentive to create new ties in a foreign lines, they extract a lower ride price. Section B4 presents quotes from three exit surveys' interviews, suggesting that drivers built lasting relationships in new lines.

Having provided evidence that social ties increase the joint profit and that drivers and officers create them when they have a long horizon, we now turn to examining the economic significance of these relationships beyond joint profit.

9. IMPLICATIONS FOR CONSUMER VS. PRODUCER SURPLUS

In this section, we analyze whether these relationships benefit not just the joint profit, but also the economic surplus. In Panel A, Columns (1) and (2), the outcome is joint profit, the producer (driver) surplus. Unprotected short-term rerouting leads joint profit per day to fall from 22.02 USD to below zero (Column 1), a more than 100% reduction. Columns (3) and (5) show that this reduction arises both from a reduction in the number of trips per day, and in in the joint profit per trip. None of these effects are present when the driver is protected (Columns 2, 4, 6), hence this effect comes through varying the relationship between the driver and the officers on the line, rather than other features of rerouting. The decrease in the number of trips suggests that these relationships increase total surplus by increasing the quantity of trips. In what follows, we decompose the increase in producer surplus per trip driven to examine whether it arises at the expense of consumer (passenger) surplus per trip. In Panel B, Column (1), we test whether social ties decrease the marginal cost of a trip. This could arise, for example, if a driver spends more time without driving wasting fuel or taking physical risks when driving, as a result of re-optimizing pickup strategies when he does not have social ties with officers on the line. Short-term unprotected rerouting increases operating costs from 5.8 USD by 3.2 USD per trip. Column (2) finds no such effect of rerouting when the driver is protected. Thus, driver-officer social ties decrease operating costs per trip.

TABLE VI: Short-Term Unprotected Rerouting and Producer vs. Consumer Surplus

A. Producer Surplus and Its Components, With and Without Protection

	Joint Profit (per day)		Number of trips (per day)		Joint Profit (per trip)	
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign	−29.819***	−0.523	−1.506*	−0.210	−4.656***	−0.105
	(10.328)	(6.152)	(0.748)	(0.742)	(1.358)	(0.555)
Mean Dep. Var.	22.02	22.24	8.21	8.27	2.95	3.01
Observations	37	100	38	105	299	860
R^2	0.716	0.418	0.731	0.346	0.616	0.244
Protection, Any	NO	YES	NO	YES	NO	YES

B. Sources of Joint Profit Per Trip Change: Marginal Cost, Consumer Surplus, or Bargaining?

	Operating Costs		# Passengers		Trip duration		Price	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Foreign	3.242**	−0.209	−0.549**	−0.183	−3.249	2.856	−0.062***	−0.019
	(1.559)	(0.596)	(0.256)	(0.271)	(2.035)	(3.097)	(0.022)	(0.014)
Mean Dep. Var.	5.82	5.8	18.61	18.27	31.7	31.87	0.47	0.48
Observations	302	863	300	861	300	862	299	861
R^2	0.604	0.136	0.801	0.756	0.346	0.294	0.745	0.783
Protection, Any	NO	YES	NO	YES	NO	YES	NO	YES

Notes: This table presents the OLS estimates of β^F from Equation 2 in the dataset of driver-days assigned to either home driving or to short-term (foreign line) rerouting (driving blocks of only 1 day), decomposing this dataset into two separate regressions: the subset of driver-days assigned to not having third-party police protection (odd columns), and the dataset of driver-days assigned to having third-party police protection. *Dependent variables*: in Panel A, joint profit per day (columns 1 and 2), number of trips per day (Columns 3 and 4), and joint profit per trip (columns 5 and 6). Joint profit is the revenue from passenger tickets in USD minus operating costs in USD; operating costs are gas and repair costs. In Panel B, the dataset is at the driver-trip level, and the variables are: the operating costs in USD (Columns 1 and 2), the number of passengers that purchase a ticket for a ride (Columns 3 and 4), the duration of a trip in minutes (Columns 5 and 6), and the average price paid by passengers on a trip in USD (Columns 7 and 8). operating costs are gas and repair costs, and are observed at the driver-day level and imputed at the driver-trip level using the number of trips. All regressions include randomization strata fixed effects, and observations are weighted by rerouting and composition weights. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full dataset of home line driving.

Second, we examine consumer demand, examining quantity, quality, and prices. Column (3) shows that the number of passengers is significantly reduced by short-term unprotected rerouting. Thus, it is possible that social ties allow the driver to induce larger demand. This would be the case if they give the driver the safety to take actions that increase passenger demand, such as making himself more visible at key strategic intersections—which he would forego without such social ties. Column (4) examines the effect of short-term protected rerouting as a placebo. In that case, there is no significant reduction in the number of passengers; thus, the effect of short-term unprotected rerouting on the number of sales cannot be explained by other features of rerouting, other than the social ties between the driver and the officers on the line. To further examine whether properties of the trip cause a shift in passenger demand, Column (5) examines the effect of short-term rerouting on trip duration as a good proxy for trip quality.[21] We find that short-term rerouting has no statistically significant effect on trip duration, if rerouting is unprotected (Column 5) or protected (Column 6). That is, driver-officer social ties have no bearing on trip quality; thus, unprotected rerouting decreases sales for reasons unrelated to their effect on trip quality. Finally, Column (7) shows that the average passenger price falls from .47 to less than .41 cents, a 13% statistically significant reduction and Column (8) shows that short-term protected rerouting has no such effect. Overall, this suggests that drivers who have ties with the officers on the line are better able to induce demand.

Taken together, we have provided evidence that social ties with the officers on the line allow the driver to reduce the marginal cost of driving, to offer more trips, and to induce passenger demand, as seen in the combined effect on prices and sales. The evidence we have gathered from qualitative interviews also supports

[21]Our preparatory qualitative evidence suggests that trip duration is the dimension of a trip to which consumers are most sensitive. Another dimension might be safety. This might matter in our case if, for example, drivers with connections are perceived to drive more safely. It is unlikely that these connections have anything to do with safety, however, as our qualitative interviews suggest that these connections are all about the driver taking the time to negotiate and to call passengers at the terminii. Furthermore, safety does not appear a major concern on the side of drivers either. In the years before the study, the government introduced another type of buses, the *Esprits de Vie* (spirit of life, in French), which were safer. The trip on these buses was slightly costlier. The project was a failure because passengers still preferred the *Esprits de Mort* which were known to be faster.

these empirical facts and our interpretation (Section B5 presents relevant quotes). Two important considerations are in order to interpret these results. First, the decrease in demand might just capture that rerouting worsens other dimensions of a trip that passengers value, such as safety, and that that passengers internalize that. However, if this were true, rerouting should decrease the price whether or not the driver has protection, since protection is unrelated to driving safely. Second, the decrease in prices alone could also reflect that, in addition to reduced passenger demand, unprotected rerouting reduces the ability of the drivers to extract consumer surplus through harder bargaining conditions. This is consistent with the explanation we have provided about the effect of social ties: the driver negotiates in a hurry due to the risk of being shaken down. It is also supported by our qualitative evidence: as drivers negotiate for the price in a rush, this leads to a reduction in their bargaining power over the passengers, resulting in a lower equilibrium price, consistent with standard bargaining theory. While we have no empirical test to confirm that ties do decrease the drivers' bargaining power, their effect on passenger demand is seen by their effect on the number of passengers, price, and quality. Their effect on the drivers' bargaining power is consistent with our results and qualitative evidence.

These findings reveal the transaction costs in the corrupt transaction in our context: the opportunity cost of time spent in negotiations and the reduction of number of trips per day, through an increase in the marginal cost per trip and reduction of passenger demand as well as bargaining power over passengers. Thus, private actors and officers might create social ties to increase producer surplus while increasing total surplus too. These relationships would facilitate the spread of corruption. But they also increase total economic surplus, thus their effect on the spread of corruption might be even stronger.

10. CONCLUSION

Our findings use a field experiment in the minibus transportation sector of Kinshasa to establish that: a. social ties between citizens and enforcing officers enable corruption to have lower transaction costs; b. citizens create relationships with state officers through an initial costly bond when the horizon of repeated

corrupt interaction is long enough.

Our study has important internal validity limitations. While the protection treatments were designed to isolate the impact of relationships of the drivers with traffic police officers on the line, the presence of a police officer in the minibus, for instance, might influence outcomes through mechanisms other than replacing social connections, such as altering drivers' willingness to pay bribes, attracting different clientele, or affecting driving behavior. Consequently, attributing the observed loss of joint profit in foreign lines in the absence of protection solely to the absence of relationships is suggestive rather than conclusive.

Our study also has a major external validity limitation. It is important to acknowledge that while our estimates are well-powered to estimate the treatment effect of a rerouting streak for each driver, and then aggregating them over drivers (there are 130 experimental clusters and 270 units of observation for the main treatment), these estimates might not generalize if the underlying population of minibus drivers is very heterogeneous. Given that the minibuses in this context are extremely homegeneous (the famous Esprit de Mort operate in a very homogeneous manner), that we cover all the lines of the east Kinshasa network (a city of 11 Million), and all universe of traffic officers with whom minibus drivers interact (ties that are induced by 344 unique officers in our sample) interacting with the drivers in the experiment's datase), this provides confidence in the generalizability of the conclusions from these data for the population of driver-officer interactions in east Kinshasa.

Our study also deserves a reflection on the fact that our experimental estimates take the prevailing equilibrium as given. While this feature is shared in most applied economics studies, this issue is particularly important in settings that are less well-understood. This has three implications.

First, it is possible that the cost of not having a relationship is jointly generated by the equilibrium strategy profile. Indeed, there is evidence to suggest this. One of the drivers we interviewed said: "He who doesn't give will be shaken. If you do not give, you really don't want to return with your *mbote* [toll fee]," suggesting that curbing driver moral hazard could be a rationale for lengthy negotiations. However, even in this context, lengthy negotiations leading to a large bribe are

simply the only way for an officer to make money unless there is some agreement.

Second, one may worry that in certain settings where new relationships may form, less costly equilibria with no initial fixed costs might emerge. However, our finding about "buying the land" is the prediction of a "bond" at the beginning of a relationship, shared by a large class of models of repeated interaction in which there is uncertainty about new relationships forming (e.g. Kranton (1996). Thus, while many factors could theoretically explain the emergence and the size of such bond—including the driver and officers' discount factors, information asymmetry over different driver types, networks of relationships, and outside options that feed into the drivers' reservation value—the existence of this initial dis-utility we document is unlikely to be a peculiar feature of the context.

Third, the speed of relationship formation is influenced by the fact that drivers in the study are less financially constrained than in reality, introduced to allow for randomization. This is an important limitation of the measured speed of relationship formation. Our estimates thus measure how fast relationships may form if drivers were financially unconstrained, a meaningful quantity as well.

Future research could benefit from exploring additional issues beyond the scope of this paper, which this paper has omitted, of relevance for geeneral interest conomics and social science generally. First, the interactions between the various actors are governed by established *norms* of behaviour. Second, and relatedly, while our study has taken an economic approach to explain the emergence of such arrangements in equilibrium and their economic benefits and costs, future research would greatly benefit from branching outside of economics into other disciplines, such as anthropology, where the rationale for these arrangements is often described as motivated by much richer factors, such as the meaning of transactions and their value as signals to express and negotiate the nature of a relationship that, in addition to immediate economic instrumental value, also has intrinsic value or instrumental value derived from other less-immediately visible consequences (such as social status, or wealth-in-people).

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Online Appendix
A. Measurement and Experimental Procedure

A1. DATA COLLECTION

The data collector recorded detailed operation data of the minibus. At the end of each trip, once at the terminus, the data collector obtained the number of passengers who purchased a ticket in the trip, as well as the price for the ticket. This information was directly obtained from the receiver in the minibus, who is in charge of accounting the tickets. This is the total revenue for the trip (the lowest level at which we could obtain revenue data). In addition, each day, the data collector obtained detailed information on gas expenses and on repair costs, which constituted the day-level operating costs. The data collector also obtained the number of trips made per day. This allows computing the daily revenue in case the data collector was absent for some of those.

The data collector also gathered data about the interaction with the police for intersection crossing as well as on each terminus. At each intersection that belongs to the route, the data collector recorded: whether an interaction with the police took place; whether a payment was made, the type and amount of the payment; the time spent interacting with the police; the records of the conversation, which we then use to separate conversations that are simple greetings from negotiation attempts that resulted in no bribe payments; the time spent in traffic; details of the interaction at the police station in case the minibus is seized and escorted to the police station; as well as the type of relationship that the driver had with the police officer he interacts with in that moment.

Most of the data was obtained through direct observation and, in some cases, such as the relationship, the data collector would simply ask the driver. According to the drivers, this activity was not an encumbrance and they enjoyed building a relationship with someone who was interested in their activity. To reduce room for shirking, we also employed a supervisor, who spent the day as a ghost auditor, following the data collectors.

A2. PROBABILITIES OF TREATMENT ASSIGNMENT, FOR ALL TREATMENTS

Our experimental design comprised one treatment arm (rerouting to foreign line), and additional treatment arms of third-party protection to isolate the role of relationships. The experiment was implemented on 27 days, split into episodes of 23 and 4 days due to funding increase for the second period. Ten minibus drivers participated in the experiment. The following subsections outline each randomization strategy in detail, and compute the probability of treatment assignment for the purposes of OLS regression weights to recover the unbiased estimator for the effect of rerouting.

A2.1. *rerouting*

We randomly assigned all participating minibus drivers to drive on their usual lines ("home lines") and the lines they reported to not have relationships with police officers and had never driven before ("foreign lines"). In the first episode of 23 days, each minibus driver was assigned to ten days of his home line and 13 days of foreign lines; in the second episode of 4 days, each minibus driver was assigned to a maximum of 2 out of 4 days on his home line. For each minibus, we randomly allocated a sequence of driving blocks that was associated with home or foreign lines, as well as with the type of protection (Section A2.2).

We sampled these blocks without replacement, totaling the number of experimental days. Table II enumerates the line allocation. For example, if a minibus driver was randomly assigned the sequence of driving blocks {5, 6, 7, 8, 9, 1, 2, 3, 4} in the 23-day period, he would drive on his first foreign line on days 1-3, on a different second one on days 4-6, on a different third one on days 7-9, on a different fourth one on days 10-12, on another different fifth one on day 13, and finally on his home line on days 14-23. Bus drivers who had multiple home lines are assigned *only one, unique,* home line.

Such a randomization scheme implies that the probability of being assigned a foreign line varies by day. We compute the probability of foreign line treatment

for each minibus d and day t with the following formula:

$$\mathbb{P}_{dt}(\text{Foreign}=1)=\mathbb{P}_t(\text{Foreign}=1)=\sum_{j\in J}\mathbb{P}_t(\text{Block}=j)\times\mathbb{1}(\text{Foreign}=1|\text{Block}=j),$$

where $J=\{1,2,\ldots,9\}$ or $J=\{1,2,3,4\}$ depending on experimental period of 23 or 4 days. The second part of each sum is an indicator function dictated by experimental design. The first part of each sum is challenging to derive theoretically. We could and did resort to computational enumeration to calculate it, first listing all 362,880 possible combinations of block sequence given sampling without replacement in the 23-day episode[22] (or 14 in the 4-day episode), then calculating the probability that a given day takes on a driving block schedule. To minimize human error, we obtained the probability of treatment assignment through simulating the randomization procedure with 10,000 different seeds and took the average probability of treatment assignment.

A2.2. *Police protection*

We studied the role of police protection in mediating the role of relationships by experimentally varying protection at the minibus-line level according to the block sequence as outlined in Table II. Similarly to the computational procedure of the probability of rerouting, we obtained the probability of each type of police protection (colonel, major, police escort) by simulating 10,000 seeds of the randomization procedure and obtaining the average of treatment assignment.

A2.3. *ACCO sticker*

We studied the role of ACCO sticker as another type of third-party protection in mediating the role of relationships by experimentally varying ACCO sticker. The sticker associates minibus drivers with a powerful trade union of minibus drivers. The ACCO sticker treatment was randomized by day, at the minibus-line

[22]For example, the probability of driving on any of the 9 blocks in the *first* day is 1/9 because each block has an equal probability of being sampled. To derive the probability on the *second* day, note that once a three-day block is sampled, a driver continues on the assigned route for the next two days. This implies that the probabilities are 1/9 in the 3-day blocks, and 1/72 in each of the 1-day blocks.

reallocation block-protection level. We used the same simulation procedure to compute the probability of sticker assignment. The rule for sticker assignment within line reallocation block in the 23-day period (Table II) is as follows: (1) Each of the two days that have the same police protection status takes a 0.5 probability of being assigned an ACCO sticker. (2) The remaining day within the three-day line-reallocation block, or the day where a block only contains one day (line reallocation blocks (4) and (9)), has a 0.43 chance of getting a sticker.

B. Tables and Figures

TABLE A1: Characteristics of the Bribe Costs and Time Waste in Pre-Experimental Data—Per Trip Equivalent

A. Bribe Costs Decomposition—Payments per Trip (USD) by Type of Bribe

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Value of payments made (USD per trip)	1.12	2.70	0	30	1,544
Toll fee (*mbote ya likasu*)	0.69	0.83	0	7	1,544
Negotiation bribe (incl. police station)	0.41	2.43	0	27	1,544
Tips	0.02	0.18	0	3	1,544

B. Time Waste per Trip by Type of Bribe

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Time wasted interacting with police (minutes per trip)	1.13	2.86	0	55	1,544
Toll fee (*mbote ya likasu*)	0.83	1.36	0	22	1,544
Negotiation bribe (incl. police station)	0.29	2.39	0	55	1,544
Tip	0.01	0.11	0	2	1,544

C. Frequency of Payment by Type of Bribe

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Number of times payments made per trip	1.36	1.32	0	7	1,544
Toll fee (*mbote ya likasu*)	1.30	1.26	0	7	1,544
Negotiation bribe (incl. police station)	0.04	0.21	0	2	1,544
Tip	0.02	0.14	0	2	1,544

Notes: This table presents the summary statistics on the three types of bribes that are paid per trip in the pre-experimental data between June 19_{th} and July 20_{th}, 2015 observed in the drivers usual lines (their home lines). Panels A, B, and C present the bribe costs, the duration of the negotiation for each bribe, and the frequency of each bribe respectively. The statistics presented are computed from 1,544 trips which composed 15,426 events in which a driver drives through an intersection, from 49 unique intersections. The data were collected from ten minibuses between June 19_{th} and July 20_{th} 2015 in the ten lines of the East Kinshasa network.

TABLE A2: Randomization Balance of the Treatment of Rerouting to Foreign Lines

Dependent Variable	All	Home line	Foreign line
Proportion of driving on/in	(1)	(2)	(3)
Monday	0.186	0.175	0.195
Tuesday	0.186	0.192	0.181
Wednesday	0.186	0.208	0.168
Thursday	0.186	0.200	0.174
Friday	0.145	0.125	0.161
Saturday	0.112	0.100	0.121
August	0.706	0.692	0.718
September	0.294	0.308	0.282
N. Bus-Day Obs.	269	120	149

Notes: This table presents the randomization balance of the proportion of driving weekdays and months on the home line vs. foreign line. The statistics presented are computed from 269 minibus-days during the experimental period, 120 and 149 of which were assigned to home and foreign lines.

TABLE A3: Balance Table, by Minibus

A. Treatment Assignment

Minibus	Foreign	Third-Party Protection	Police Escort	High-level Police	ACCO Sticker
(1)	(2)	(3)	(4)	(5)	(6)
1	0.5385	0.7308	0.1538	0.3846	0.4615
2	0.5556	0.7037	0.2593	0.2593	0.5185
3	0.5556	0.7037	0.0741	0.4444	0.4074
4	0.5556	0.7407	0.1481	0.3704	0.5185
5	0.5556	0.7037	0.0370	0.4444	0.4815
6	0.5556	0.7037	0.1852	0.3333	0.4074
7	0.5556	0.7407	0.1852	0.2963	0.5185
8	0.5556	0.6667	0.1852	0.3704	0.3704
9	0.5556	0.7037	0.1481	0.3333	0.4444
10	0.5556	0.8148	0.2222	0.3333	0.6296

B. Treatment Administration

Minibus	Foreign	Third-Party Protection	Police Escort	High-level Police	ACCO Sticker
(1)	(2)	(3)	(4)	(5)	(6)
1	0.5385	0.6538	0.1154	0.3462	0.4615
2	0.5556	0.5556	0.1852	0.1852	0.5185
3	0.5556	0.5926	0.0370	0.3704	0.3704
4	0.5556	0.5926	0.0741	0.3333	0.4815
5	0.5556	0.7037	0.0370	0.4444	0.4815
6	0.5556	0.7037	0.1852	0.3333	0.4074
7	0.5556	0.6667	0.1481	0.2593	0.5185
8	0.5185	0.5556	0.1111	0.3333	0.3704
9	0.5185	0.5926	0.1111	0.2593	0.4074
10	0.5185	0.7037	0.1481	0.2963	0.6296

Notes: This table presents the randomization balance of the proportion of driving weekdays and months on the home line vs. foreign line. The statistics presented are computed from 269 minibus-days during the experimental period, 120 and 149 of which were assigned to home and foreign lines. Panel A shows the fraction of days each minibus is assigned to a given treatment. Panel B shows the fraction of days each minibus received the administration of a given treatment, hence accounts for noncompliance.

TABLE A4: Characteristics of the Data

	N
A. Sample size	
Number of times a sample driver crosses an intersection	13,092
Number of trips	1,935
Number of unique driver-officer matches (id-id)	344
B. Characteristics of events in which a driver crosses an intersection	
Number of times a sample driver crosses an intersection, in which	
... a payment is made	1,682
... a toll fee (*mbote ya likasu*) is paid	1,514
... a negotiation bribe is paid at intersection	97
... a negotiation bribe is paid at police station	9
... a tip is paid	62
... driver interacted with the police officer (including talk)	2,367
... money is demanded (including negotiation that resulted in no bribe)	1,839
C. Trip characteristics	
Number of trips per day	8
Number of intersections per trip	6.5
Duration of a trip in minutes	50.9

Notes: This table presents the data characteristics of the minibus operations in our experiment, using data from home and foreign lines. The number of unique driver-officer matches computes the unique intersection crossings in which driver interacted with officers at the intersections.

TABLE A5: Characteristics of the Three Types of Bribes in Experimental Data

A. Bribe Costs Decomposition—Payments per Transaction (USD) by Type of Bribe

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Average per transaction made	1.06	2.24	0.22	24.96	767
Toll fee (*mbote ya likasu*)	0.67	0.33	0.22	2.17	693
Negotiation bribe (incl. police station)	6.82	6.74	0.22	24.96	47
Tip	1.17	0.62	0.54	3.26	27

B. Time Waste per Transaction by Type of Bribe

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Fraction of intersection crossings in which time interacting with police is strictly positive	0.16	0.37	0	1	6,774
Time wasted in interaction with police, per transaction conditional on interaction time strictly positive (duration of negotiation in minutes)	0.95	1.65	0.10	40	767
Toll fee (*mbote ya likasu*)	0.84	0.63	0.10	5	693
Negotiation bribe (incl. police station)	2.50	6.03	0.10	40	47
Tip	0.87	0.61	0.10	3	27

C. Frequency of Payment by Type of Bribe

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Number of trips per day	8.25	2.21	4	13	115
Number of times per day in which a payment is made at the average intersection (any payment)	0.52	0.96	0	6	1,462
Toll fee (*mbote ya likasu*)	0.47	0.93	0	6	1,462
Negotiation bribe (incl. police station)	0.03	0.18	0	2	1,462
Tip	0.02	0.15	0	2	1,462

Notes: This table presents the summary statistics on the three types of bribes in the experimental data observed in the drivers usual lines (their home lines). Panels A, B, C present the bribe costs, the duration of the negotiation for each bribe, and the frequency of each. The statistics presented are computed from 6,774 events in which a driver drives through an intersection, from 45 unique intersections. The data were collected from ten minibuses between June 19_{th} and July 20_{th} 2015 in the ten lines of the East Kinshasa network.

TABLE A6: Joint Profit Decomposition

Variable	*Statistic*				
	Mean (1)	*S.D.* (2)	*Min* (3)	*Max* (4)	*N* (5)
Profit per day (USD·day^{-1})	15.18	30.90	−88.57	75.00	109
Profit per trip (USD·trip^{-1})	2.13	4.43	−29.85	11.10	945
Joint Profit per trip (USD·trip^{-1})	3.00	3.73	−20.46	12.18	945
Revenue (USD·trip^{-1})	8.79	1.80	0.43	15.20	945
Number of passengers	18.36	3.60	1	29	945
Price (USD ·person^{-1}·trip^{-1})	0.48	0.05	0.22	0.54	945
Operating costs (USD·trip^{-1})	5.79	3.53	0	31.75	945
Number of trips per day	8.32	2.21	4	13	109
Bribe costs (USD·day^{-1})	7.00	7.70	0	44.62	109
of which paid to *receveur* (USD·day^{-1})	6.62	4.67	0	19	109

Notes: This table presents the descriptive statistics of minibus profit and its operational decomposition. Indentation on variable indicates a decomposition of the parent variable. The statistics are computed from 109 days of home line observations in the experiment, encompassing 945 trips.

TABLE A7: Effect of Rerouting to Foreign on Joint Profit—Robustness

A. Joint Profit per Day

	$SATE_r$ (1)	SATE (2)	ATE (3)	ATEc (4)	PATE (5)	Winsor 99 (6)	Winsor 95 (7)	Winsor 90 (8)
Foreign	−6.805	−8.632**	−8.892**	−8.953**	−9.142**	−7.975*	−7.670*	−7.433*
	(4.884)	(3.811)	(3.786)	(3.887)	(4.013)	(4.045)	(4.028)	(3.987)
Mean Dep. Var.	18.6	18.6	18.6	18.6	18.6	19.81	19.61	19.36
Observations	167	167	167	167	167	167	167	167
R^2	0.012	0.318	0.322	0.370	0.337	0.290	0.284	0.277
Rand. Strata FE	NO	YES	YES	YES	YES	YES	YES	YES
Foreign IPW	NO	NO	YES	YES	YES	YES	YES	YES
Line FE	NO	NO	NO	YES	YES	NO	NO	NO
Composition IPW	NO	NO	NO	NO	YES	NO	NO	NO

B. Number of Trips per Day

	$SATE_r$ (1)	SATE (2)	ATE (3)	ATEc (4)	PATE (5)	Winsor 99 (6)	Winsor 95 (7)	Winsor 90 (8)
Foreign	−0.810**	−0.890**	−0.917**	−0.920***	−0.967***	−0.885**	−0.885**	−0.823**
	(0.384)	(0.357)	(0.356)	(0.324)	(0.360)	(0.347)	(0.347)	(0.326)
Mean Dep. Var.	7.86	7.86	7.86	7.86	7.86	7.97	7.97	7.88
Observations	176	176	176	176	176	176	176	176
R^2	0.028	0.258	0.262	0.375	0.328	0.244	0.244	0.236
Rand. Strata FE	NO	YES	YES	YES	YES	YES	YES	YES
Foreign IPW	NO	NO	YES	YES	YES	YES	YES	YES
Line FE	NO	NO	NO	YES	YES	NO	NO	NO
Composition IPW	NO	NO	NO	NO	YES	NO	NO	NO

C. Joint Profit per Trip

	$SATE_r$ (1)	SATE (2)	ATE (3)	ATEc (4)	PATE (5)	Winsor 99 (6)	Winsor 95 (7)	Winsor 90 (8)
Foreign	−0.681	−0.877**	−0.900**	−0.866*	−0.874*	−0.915**	−0.912**	−0.868**
	(0.561)	(0.443)	(0.439)	(0.461)	(0.456)	(0.463)	(0.443)	(0.433)
Mean Dep. Var.	2.62	2.62	2.62	2.62	2.62	2.77	2.69	2.63
Observations	1,395	1,395	1,395	1,395	1,395	1,395	1,395	1,395
R^2	0.007	0.187	0.192	0.226	0.204	0.176	0.184	0.180
Rand. Strata FE	NO	YES	YES	YES	YES	YES	YES	YES
Foreign IPW	NO	NO	YES	YES	YES	YES	YES	YES
Line FE	NO	NO	NO	YES	YES	NO	NO	NO
Composition IPW	NO	NO	NO	NO	YES	NO	NO	NO

Notes: This table presents the OLS estimates of β^F from Equation 2. In Panel A, the dependent variable is the joint profit per day in USD. Panels B and C decompose the joint profit per day as dependent variables into the number of trips per day and the joint profit per trip. Joint profit per trip (Panel C) is revenue minus operating cost per trip, where revenue is derived from number of passengers per trip times the average price each passenger pays per trip, and operating cost per trip is repair and gas cost per day divided by the number of trips per day. Each column modifies Equation 2 to account for biases in the estimation of average treatment effect of foreign line driving on the joint profit variables. Column (1) presents the coefficients from β^F estimator in Equation 2 without randomization stratas fixed effects, identifying the raw sample average treatment effect of rerouting ($SATE^{raw}$). Column (2) presents the coefficients from β^F estimator identifying the sample average treatment effect with randomization stratas (SATE). Column (3) presents the coefficients from β^F estimator which include randomization stratas fixed effects and weights for the inverse propensity of treatment assignment, identifying the average treatment effect of rerouting (ATE). Columns (4) and (5) account for potential line composition imbalances in the assignment of home vs. foreign treatments. Column (4) presents the coefficients from β^F estimator, which additionally include line fixed effects (ATE^C). Column (5) presents the coefficients from β^F estimator, which additionally include the weights for the inverse propensity that a given line is in the set of foreign or home lines (PATE). Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full dataset of home line driving. To re-weight the observations when estimating PATE, for each observation, we include the inverse of the probability that an observation is foreign or home, conditional on the specific line, and multiply it with the inverse of the probability that the randomly selected line appears as a home or a foreign line, given the dataset of drivers and of lines in the experiment. This obtains propensity weights for home vs. foreign that produce the average treatment effect of driving home vs. foreign that is representative of all lines in the dataset with equal weight. Table A8 catalogs these empirical probabilities of lines appearing on the foreign line pools. Lines that are *home* for a given minibus can never appear as foreign for that minibus hence has probability zero. Importantly, the pool of foreign lines varies by minibus and that all lines are equally represented as home or as foreign line.

TABLE A8: Empirical Probabilities of Appearing on Minibus Foreign Line Pool

	Minibus d									
Line l	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1	0	0	0.1112	0.1432	0.1111	0.1112	0.1250	0.1112	0.1117	0.1110
2	0	0.1252	0.1113	0.1427	0.1111	0.1110	0.1248	0.1113	0.1112	0
3	0.2499	0	0	0.1426	0.1110	0.1108	0.1255	0.1109	0.1108	0.1116
4	0	0.1250	0.1113	0	0.1109	0.1115	0.1252	0.1113	0.1106	0.1110
5	0.2495	0.1246	0.1108	0.1430	0	0.1107	0.1247	0.1108	0.1107	0.1108
6	0	0.1251	0.1113	0	0.1107	0.1110	0.1243	0.1113	0.1113	0.1111
7	0	0.1253	0.1110	0.1430	0.1116	0.1109	0	0.1110	0.1113	0.1112
8	0	0.1253	0.1110	0.1427	0.1114	0	0	0.1109	0.1114	0.1112
9	0.2499	0.1248	0.1113	0	0.1107	0.1114	0.1251	0.1112	0	0.1111
10	0.2507	0.1248	0.1108	0.1428	0.1114	0.1114	0.1252	0	0.1112	0.1110

Notes: This table presents the conditional probability $¶_d[\text{Foreign}|\text{Line}= l]$, calculated based on the empirical realities of each minibus d's set of available foreign lines. Probability of zero means driver is banned from a line due to risks of hefty harassment or safety.

TABLE A9: Effect of Rerouting on Joint Profit, With and Without Each Type of Protection

A. by ACCO Third-Party Protection

	Joint Profit (per day)		Number of trips (per day)		Joint Profit (per trip)	
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign	−13.95***	−3.26	−1.35***	−0.62	−1.23**	−0.65
	(4.81)	(5.91)	(0.47)	(0.60)	(0.61)	(0.63)
Mean Dep. Var.	21.66	22.76	8.02	8.49	2.88	3.1
Observations	88	79	91	85	697	698
R^2	0.53	0.47	0.40	0.41	0.40	0.27
Protection, ACCO Sticker	NO	YES	NO	YES	NO	YES

B. by Police Third-Party Protection

	Joint Profit (per day)		Number of trips (per day)		Joint Profit (per trip)	
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign	−12.82**	−5.29	−1.28**	−0.73	−1.22*	−0.65
	(5.89)	(5.48)	(0.55)	(0.51)	(0.71)	(0.54)
Mean Dep. Var.	24.22	20.46	8.4	8.13	3.25	2.77
Observations	79	88	85	91	675	720
R^2	0.58	0.39	0.38	0.39	0.40	0.24
Protection, Police	NO	YES	NO	YES	NO	YES

Notes: This table presents the OLS estimates of β^{FNP} and β^{FP} from Equation 3, decomposed by: in Panel A, whether the driver-day was not assigned to third-party protection of type ACCO Driver's Association sticker (odd columns), or instead assigned to third-party protection of type ACCO Driver's Association sticker (even columns); in Panel B, whether the driver-day was not assigned to third-party police protection (odd columns), or instead assigned to third-party protection by the police (even columns). In both panels, the dependent variables are the joint profit per day in USD in Columns (1) and (2), the number of trips per day in Columns (3) and (4) and the joint profit per trip in Columns (5) and (6), respectively. Joint profit per trip is revenue minus operating cost per trip, where revenue is derived from number of passengers per trip times the average price each passenger pays per trip, and operating cost per trip is repair and gas cost per day divided by the number of trips per day. Police third-party protection combines police escort on the minibus and access to higher-level police. All regressions include controls for each non listed type of third-party protection, randomization strata fixed effects, and observations are weighted by inverse propensity scores to account for the randomized assignment procedure and to adjust for imbalance on foreign line composition. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full dataset of home line driving.

TABLE A10: Effect of Rerouting on Joint Profit, Decomposed by Type of Protection

A. by ACCO Third-Party Protection

	Joint Profit (per day)	Number of trips (per day)	Joint Profit (per trip)
	(1)	(2)	(3)
Foreign	−15.35***	−1.46***	−1.41**
	(5.60)	(0.45)	(0.70)
Foreign * Protection, ACCO Sticker	14.87*	1.18*	1.25
	(7.72)	(0.65)	(1.01)
Mean Dep. Var.	22.76	8.49	3.1
Observations	167	176	1,395
R^2	0.34	0.28	0.21

B. by Police Third-Party Protection

	Joint Profit (per day)	Number of trips (per day)	Joint Profit (per trip)
	(1)	(2)	(3)
Foreign	−13.50**	−1.24**	−1.22
	(6.36)	(0.52)	(0.77)
Foreign * Protection, Police	9.49	0.64	0.80
	(8.18)	(0.64)	(1.04)
Mean Dep. Var.	20.46	8.13	2.77
Observations	167	176	1,395
R^2	0.33	0.26	0.21

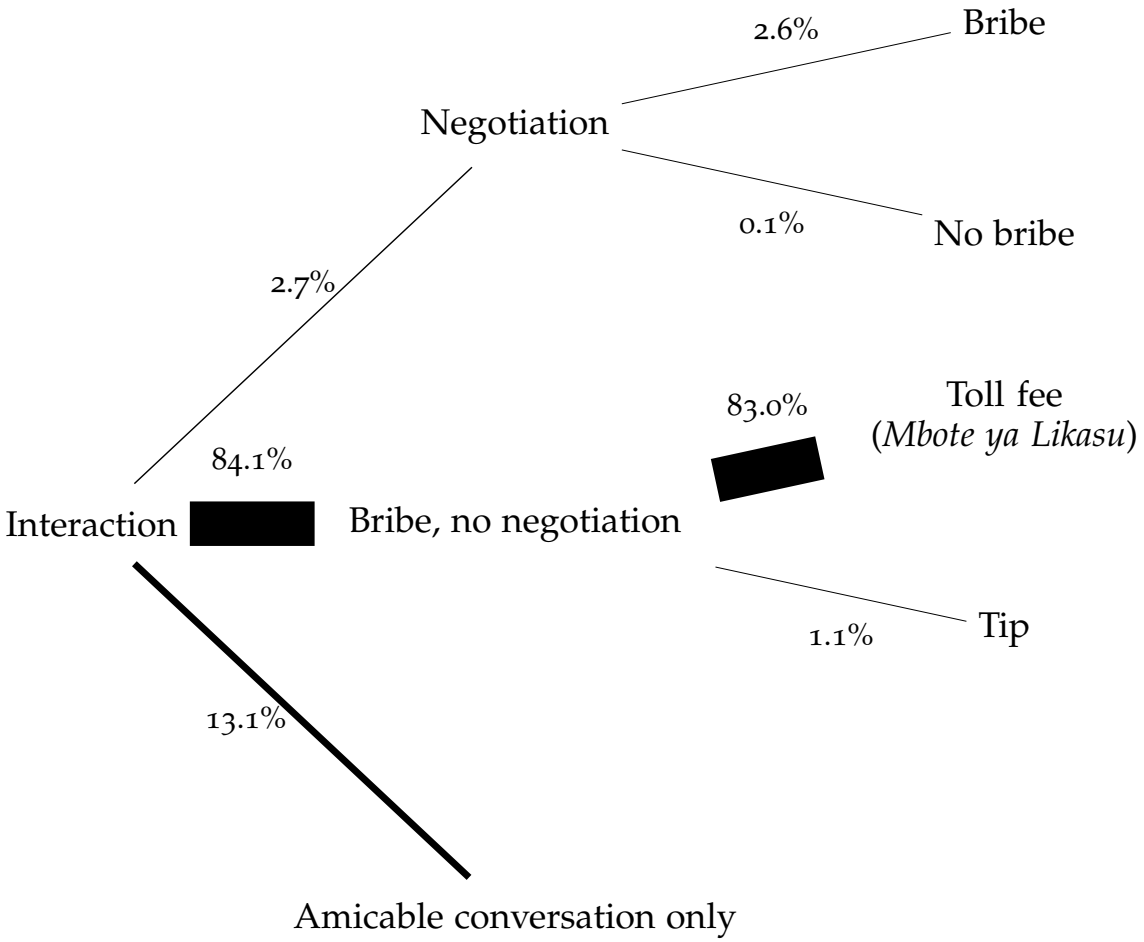
Notes: This table presents the OLS estimates of β^{FNP} and β^{FP} from Equation 3, separately for whether the driver-day was assigned to third-party protection of type ACCO Driver's Association sticker (Panel A), whether the driver-day was assigned to third-party police protection (Panel B). In Column (1), the dependent variable is the joint profit per day in USD. The dependent variables in Columns (2), (3) are the number of trips per day and the joint profit per trip, respectively. Joint profit per trip is revenue minus operating cost per trip, where revenue is derived from number of passengers per trip times the average price each passenger pays per trip, and operating cost per trip is repair and gas cost per day divided by the number of trips per day. Police third-party protection combines police escort on the minibus and access to higher-level police. All regressions include controls for each non listed type of third-party protection, randomization strata fixed effects, and observations are weighted by inverse propensity scores to account for the randomized assignment procedure and to adjust for imbalance on foreign line composition. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full dataset of home line driving.

FIGURE A1.—An *Esprit de Mort* Minibus



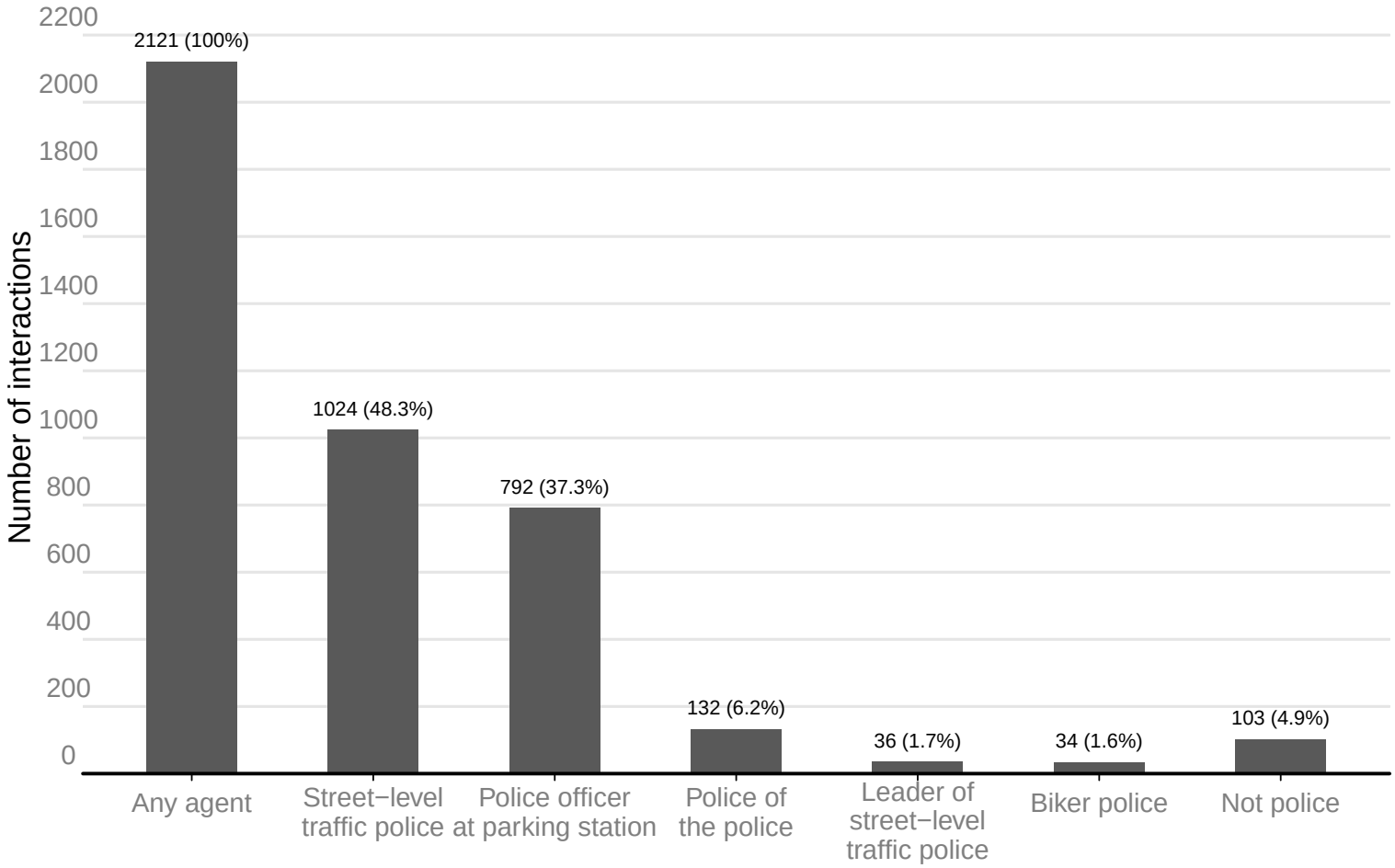
Notes: This image depicts an *Esprit de Mort* minibus in Kinshasa. **Source**: Emilio Noorani

FIGURE A2.—Classification of Driver-Police Officer Interactions in Pre-Experimental Data



Notes: This figure classifies the interactions observed between drivers and police officers at the intersections in the pre-experimental data between June 19_{th} and July 20_{th}, 2015, in a tree diagram. The data were collected between June 19_{th} and July 20_{th} 2015 in the ten lines of the East Kinshasa network. The data in this figure are conditionel on there being an interaction. The figure includes the percentage breakdown of 2,423 interactions that we observed between 344 unique officers and ten unique minibus drivers in their home lines. The branch thickness is drawn to be proportional to the *incidence* of each class of interactions. Enumerators coded "*negotiation*" for interactions between a driver and the police officers in which drivers negotiate for bribes. Negotiation events include negotiations that take place only on the street with the police officers (the majority) and negotiations that end in the driver being escorted to the police station for further negotiation with higher authorities (the minority). This class of interactions can only end in two types: a. payment ("bribe"); b. no payment ("no bribe"). No payment interactions are negotiations that result in the driver escaping, or the driving promising to pay in the future, or the driver and the officer agreeing that no payment is necessary because the driver announces to have a higher-level "protector" to avoid paying a bribe. Amicable conversation only is an interaction where drivers have conversation with police officers, either directly coded by enumerator or analyzed to be separated from negotiations, and results in no payment. Data collectors distinguish tip from "*mbote ya likasu*" by taking records of conversation and to further verify sometimes asking drivers.

FIGURE A3.—Interactions Between Drivers and State Officials in the Street, in Pre-Experimental Data



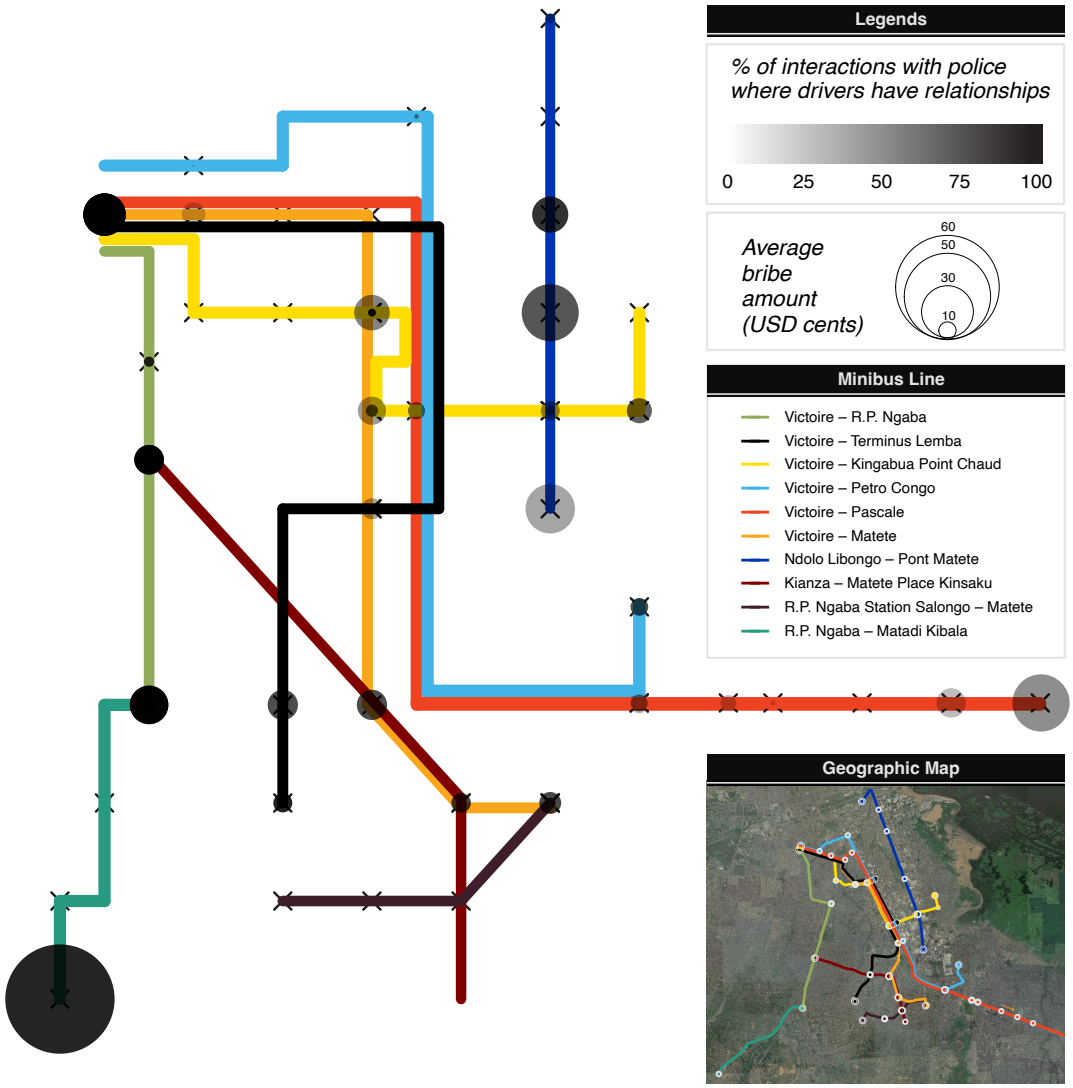
Notes: This figure presents the number (and corresponding fraction) of interactions between the driver and different types of agents when crossing an intersection on the home line. The figure is obtained from 15,426 crossings of an intersection from the pre-experimental data between June 19_{th} and July 20_{th} in the ten lines of the East Kinshasa network.. Not police includes City Transportation Agency, National Intelligence Agency, Congolese Army, and Congolese Army Elite Units. The different officers identified reflect the local knowledge of minibus drivers and data collectors based on the officers' badges and uniforms, which is entered as multiple choice survey questions on the SurveyCTO by the observer in the bus.

FIGURE A4.—The ACCO Sticker



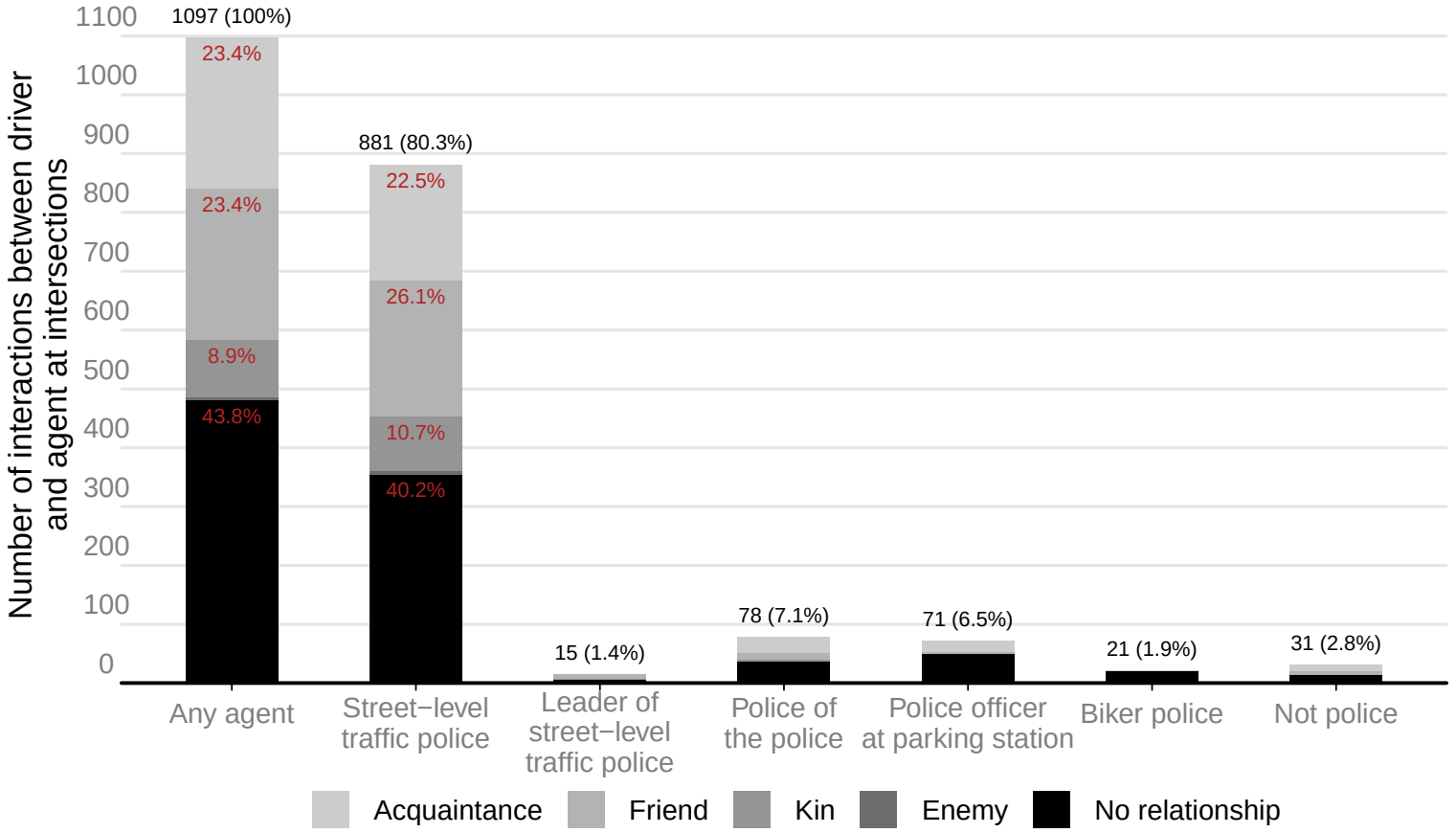
Notes: This figure presents an example of the sticker of the association of drivers of the Congo (ACCO). **Source**: Research team.

FIGURE A5.—Schematic Map of the Minibus Network of Lines in Kinshasa



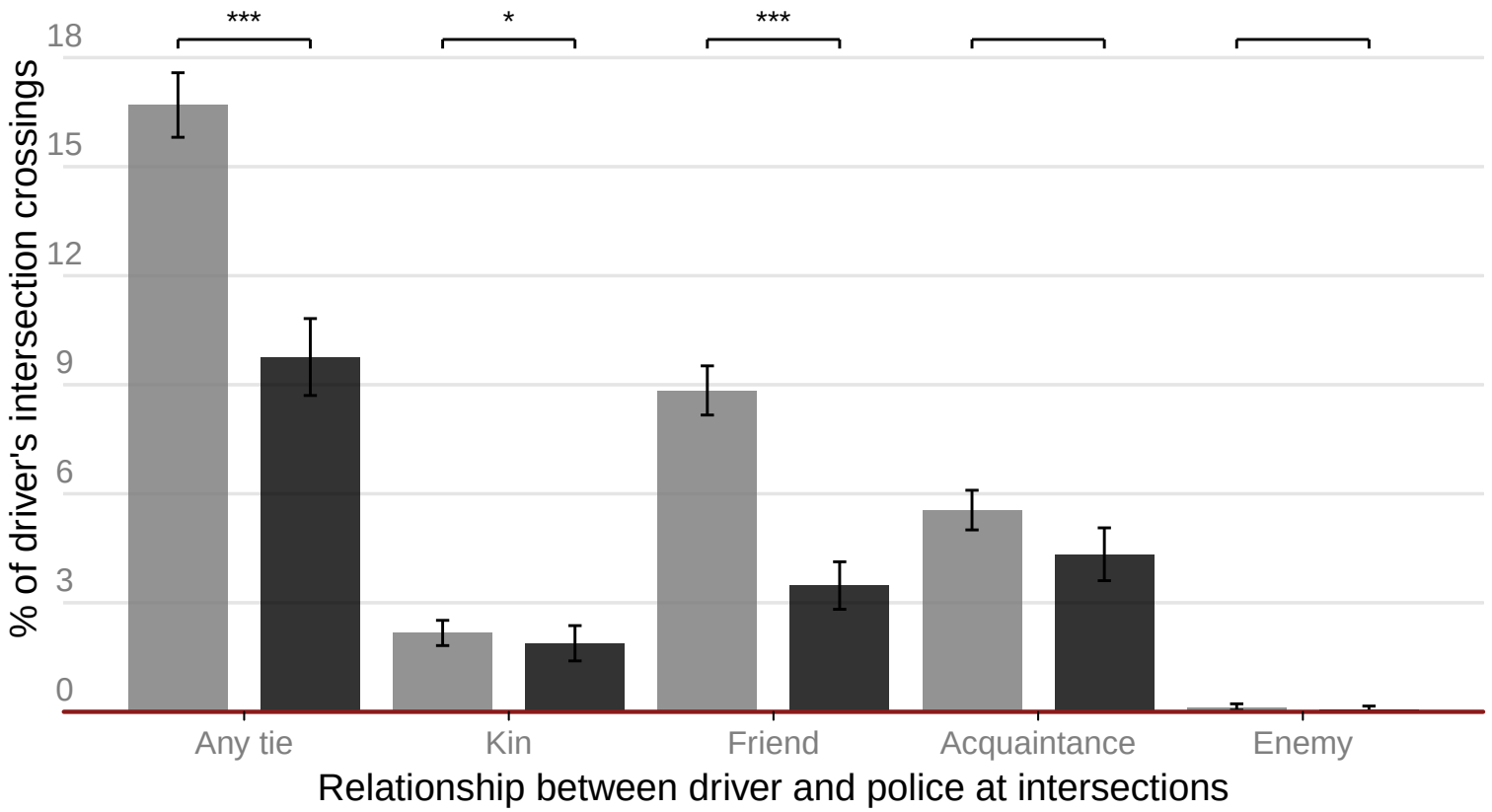
Notes: This figure represents, schematically, the lines through which "*Esprit de Mort*" minibuses pass, as well as: the percentage of interactions at which home line drivers reported having relationships with police, depicted as a gray scale with darkness proportional to the fraction of reported ties; the average amount of bribe that is paid by home line drivers in each intersection, depicted as circles with diameter proportional to the amount. Geographic proximity is preserved as much as possible in the schematic map.

FIGURE A6.—Relationships Between Drivers and State Officials in the Street



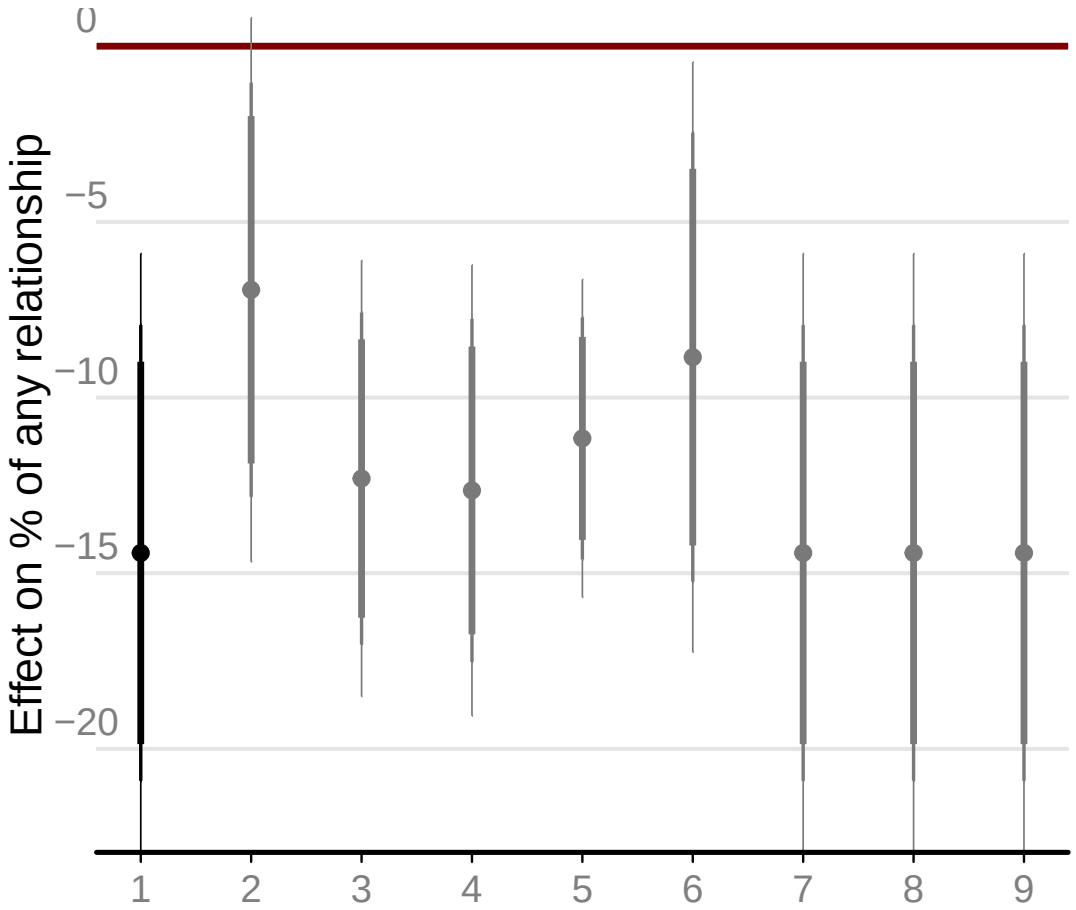
Notes: This figure presents the number (and corresponding fraction) of interactions between drivers and officers in which drivers have relationships with officers, obtained from interactions in driver's home lines in the experimental data. Each bar is decomposed into the type of relationship, stacked into the bar. The percentages reflect the proportion of interaction between driver and officers at the intersection for which the type of officers are described on the horizontal axis. The digits inside each bar segment describe the percentages of interactions with each agent for which drivers and officers have relationship described by the color in the legend. The figure is obtained from 1,097 interactions with officers in the experimental data. Not police includes City Transportation Agency, National Intelligence Agency, Congolese Army, and Congolese Army Elite Units. The different officers identified reflect the local knowledge of minibus drivers and data collectors based on badges and uniforms, which is then codified as multiple choice survey questions on the SurveyCTO.

FIGURE A7.—Effect of Foreign Line rerouting on Relationship with Agents at Intersections, by Intersection Crossing



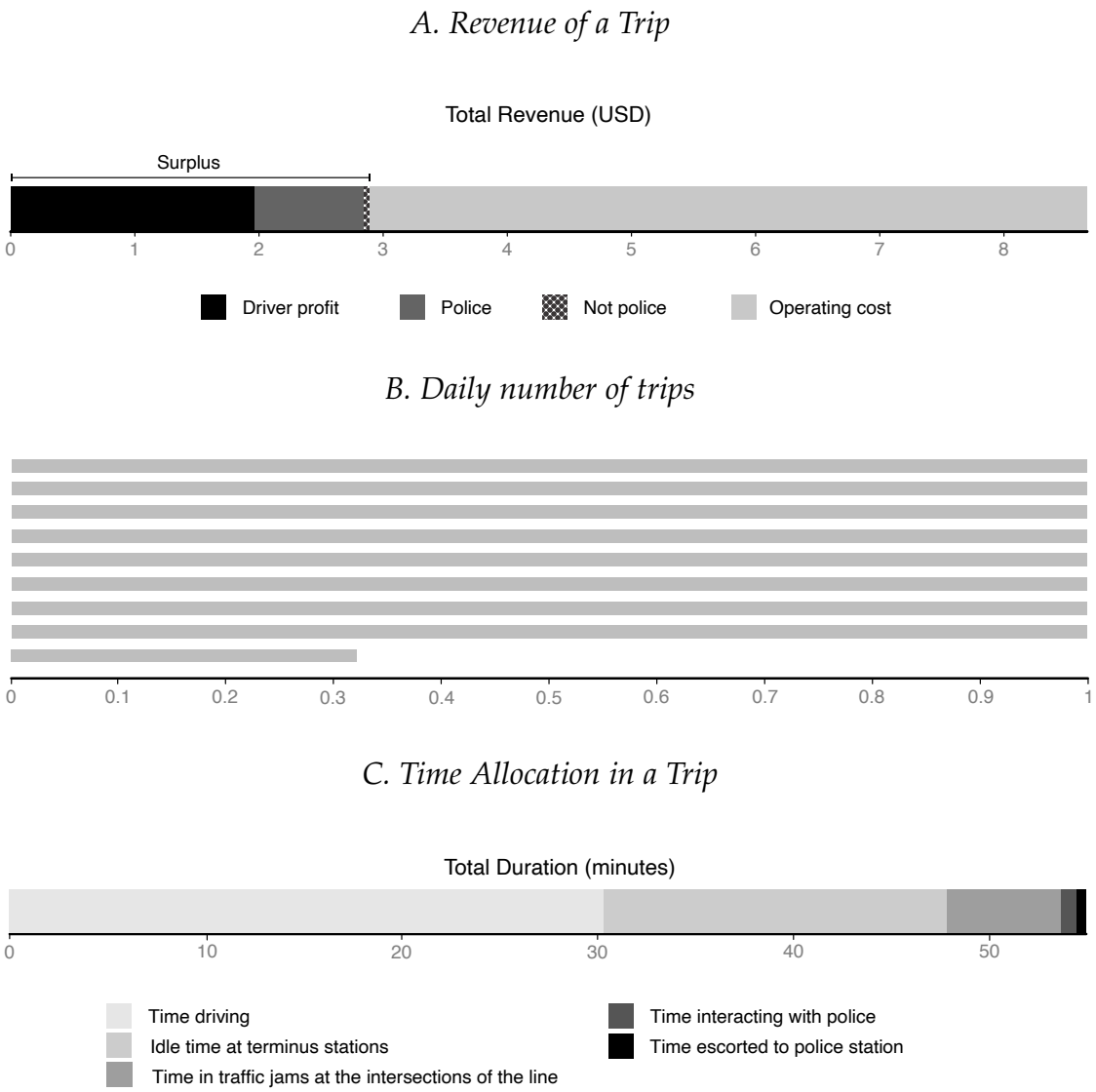
Notes: This figure presents the mean prevalence of social relationships between drivers and police officers at the intersections, expressed as the percentage of drivers' 6,774 crossings of an intersection on their usual "home line," and 3,022 intersection crossings on the "foreign line" on their first day of rerouting. The line segments represent 95% confidence intervals around the reported means. The asterisks report the segments of the p-value of the test for whether the fraction in the home line is different than that on the foreign line, for each relationship category: *** indicates that the p-value is equal or lower to .01, ** indicates that it is larger than .01 but lower than .05, and * indicates that it is larger than .05 but lower than .1. This test is implemented through OLS, by estimating β^F of Equation 1, with standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster; since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. Kin includes both nuclear and extended family relationships. Acquaintance category is different from friend category (*ami* in French) in two ways. First, in some cases, the drivers' directly reported acquaintance (*connaissance* in French). Second, we analyzed the content of the conversation between the driver and the officer, and were able to deduce whether the drivers and the officers were acquainted.

FIGURE A8.—Effect of Rerouting to Foreign on Prevalence of Relationships—Robustness



Notes: This figure presents the plotted estimates of the estimates of β^F from Equation 1. The dependent variable is relationship (any tie) under various specifications of regression weights and controls. The line segments depict the 95% and 90% confidence intervals for the coefficient estimates. Specification key on the x-axis: (1) PATE, our main specification, which is the population average treatment effect, (2) Sample average treatment effect without randomization strata fixed effects, SATE^{raw}, (3) Sample average treatment effect, SATE, (4) Assignment probability adjusted sample average treatment effect, ATE, (5) Assignment probability adjusted sample average treatment effect with line fixed effects, ATE^{C}, (6) Assignment probability adjusted sample average treatment effect, ATE, without randomization strata fixed effects, (7) PATE for whose outliers in dependent variables are winsorized at 1%, (8) Specification 7, except at 5%, (9) Specification 7, except at 10%. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving, the randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by ten randomization strata. To re-weight the observations when estimating PATE, for each observation, we include the inverse of the probability that an observation is foreign or home, conditional on the specific line, and multiply it with the inverse of the probability that the randomly selected line appears as a home or a foreign line, given the dataset of drivers and of lines in the experiment. This obtains propensity weights for home vs. foreign that produce the average treatment effect of driving home vs. foreign that is representative of all lines in the dataset with equal weight. Table A8 catalogs these empirical probabilities of lines appearing on the foreign line pools. Lines that are *home* for a given minibus can never appear as foreign for that minibus hence has probability zero. Importantly, the pool of foreign lines varies by minibus and that all lines are equally represented as home or as foreign line.

FIGURE A9.—Trip Characteristics



Notes: This figure presents the allocation of joint profit and time from an average trip from drivers on the home line of the experimental data. Panel A breaks down the driver's revenue from passenger's payment of minibus tickets into joint profit and operating cost. Panel B depicts a progress bar of the number of round trips conducted in a day. Panel C breaks down the duration of each trip into 30.8 minutes driving, 5.9 minutes waiting in traffic jams at intersections, and 17.8 minutes idle at parking terminus, 1.7 minutes of interaction with police, and 0.5 minutes on average of total driver-trips on the home line being escorted to police station.

C. Qualitative Interviews Appendix

B1. QUALITATIVE QUOTES ON BACKGROUND

Another will tell you that, on this line, he already "bought the land." This means that between him and the officers on the line, it is hard for him to get arrested because he is known. Here I am new. It is easy for me to get arrested. (Project meeting notes of July 10th 2015)

The officer on the street did not want to take the phone offered by the driver. Only when another officer who sat with him was there and the driver mentioned the name of the major several times did the arresting officer take the phone. After answering the phone, the minibus was released. The driver realized it was this one officer who knew the major who pushed his colleague to release the minibus. (Project implementation notes of September 4th 2015)

B2. QUALITATIVE QUOTES ON STUDY DESIGN

Design: to what extent is it artificial and where does that matter?

A friend Z., he bought a RAV4 vehicle, but he never even registered the plate. He often uses his car on Sundays, as the police are not very strict on Sundays. If he has a trip to make during the week, the Regiment Commander, his friend, provides him with one of his bodyguards. Throughout the day, Z. runs errands while the military officer sits in the front seat of the car. He is never stopped by the police. On days when he doesn't have any errands, the guard stays at home and never wonders why or how he wasn't called to accompany Z. Similarly, during our project in Kinshasa, when the police officer was not called, he was certain that we didn't need him that day. Remember that sometimes we would take a lieutenant, other times a captain, which allowed us to change the police officers. They were very sure that we only needed protection to avoid getting stopped by the police. (Source: exit interview with project manager, 2015)

But that doesn't mean he can't go on other routes, such as if his friend tells him that the route he is currently on is more profitable. (Source: exit interview with project manager, 2015)

Beliefs held by the drivers

For the drivers, they were aware that it was a project because we invited them to the office, especially during the second phase [the experiment], to inform and explain how the variations would be made (changing routes, etc.) . . . In short, they knew it was a project, and their motivation to participate was mainly for the money they earned . . . It was predictable that they would think about certain things, especially on days when the routes changed, but the truth is that sometimes routes are changed without any form of intervention. Therefore, they cannot claim it's to measure the impact of the protection when they know very well that they are not under our protection on that day. (Source: exit interview with project manager, 2015)

Beliefs Held by the Third-Party Protectors

With regards to the police officers, we conducted the project in a highly confidential manner. Each officer involved in the protection system was aware that it was their job with us but they were not aware that it was part of a study. This system already exists, wherein police or military personnel earn money through collaboration with vehicle owners. They intervene when you get stopped by traffic officers either through a phone call or by coming directly to the place of arrest. This system was not new; it already existed. (Source: exit interview with project manager, 2015)

Beliefs Held by the Police Officers in the Street

I can confirm that the police officers on the road never altered their behavior because they were not aware of the project; otherwise, we wouldn't have observed the interactions we did. [Given the connections of the study, if they knew] they would have tried to pretend they act as good individuals . . . (Source: exit interview with project manager, 2015)

In Kinshasa, there are many vehicles used for public transportation, and the military personnel often lack means of transport. This leads to the military requesting service, and when they do so, the driver providing the service to the military or police is automatically exempt from paying road harassment fees. This means that when the traffic police (PCR) see a vehicle with a police officer or a military personnel in it, out of solidarity with their colleagues, they let the vehicle pass without any hassle. As a result, the PCR is not aware that this protection comes from us; they only know that they let the vehicle pass out of solidarity with their colleague. (Source: exit interview with project manager, 2015)

Usually, when someone is already stopped, they try to use various influences to get released without having to pay a lot of money, often through phone calls made by the police or military personnel. This method is often successful because many military personnel have vehicles used for public transportation to earn money and support their households. Thus, within the scope of our study, when we receive protection from someone who will make a phone call, the reason they explain to the traffic police (PCR) is that the vehicle belongs to them. PCR officers are accustomed to these types of interventions, and sometimes they readily accept without raising doubts, especially if it is an authority they know well. However, there are times when the PCR officer initially doubts the explanation, and in such cases, they may not immediately accept the intervention. Sometimes, the person providing protection is obligated to go to the location to intervene. In short, this situation was a normal occurrence because the police officers were familiar with this practice. (Source: exit interview with project manager, 2015)

Even if the driver gets stopped on a route that is not his own, he cannot claim that he wasn't on his route because he is part of the study. During our meeting with the drivers, this was one of the elements on which we insisted, and our investigator was in the vehicle, so there was no way

for the driver to disregard this rule . . . However, there is police harassment; when the police see a vehicle that is not familiar with the route, they stop the vehicle and demand a given amount of payment from the driver. Sometimes, this leads to significant discussions, and eventually, the driver is released. As a result, the driver feels that they have already paid the "land" (Kosomba mabele), and the police are accustomed to these kinds of situations. They do not question why the driver went on a different route. Some drivers even change routes frequently, sometimes every week, and adapt to each situation. (Source: exit interview with project manager, 2015)

The police officer was not informed about the amount we paid to the driver; we did our best to maintain confidentiality at every stage of the project. (Source: exit interview with project manager, 2015)

Beliefs Held by the Police Officers in the Street

There haven't been many troubles. It was only once on 7th Street [after the study] where an officer stopped me and asked me if the officer that had been given to me as a protection in the vehicle previously could be reached out so that I could be released. (Interview with study driver, August 2018)

B3. QUALITATIVE QUOTES TO INTERPRET THE MECHANISMS

a. The risk of getting stopped is linked to the lack of connections, as the majority of buses providing public transportation in Kinshasa do not meet transportation requirements . . . These conditions make the drivers of these vehicles vulnerable (liable to be stopped frequently) . . . It's possible and normal, a driver who operates a route that isn't their own is initially obstructed by those in charge of the route and runs the risk of being stopped at the loading (boarding) point. As a result, they desperately want to leave the point as quickly as possible before getting caught. The only thing that can motivate customers to get into their car quickly is a price reduction. I believe this is why several drivers were reducing the price . . . If there are less passengers, it would be related to the fact that they don't have much time at the parking to charge their vehicle. (Source: exit interview with project manager, 2015)

b. Aware of their vulnerability, when these drivers start a new route where they will be working in the upcoming days, they provide extra money (more than the Mbote ya Likasu fee). This is to establish relationships with traffic police officers assigned to that route (this was referred to in Lingala as "Kosomba Mabelé," which can be translated literally to "buying the land" in French). Thus, once the relationship is established, the traffic police officer for that route, knowing that they have received money from the driver, will only ask for the Mbote ya Likasu from the driver in the future. If the driver changes routes and encounters traffic police officers with whom they haven't established such relationships, they are aware of their vulnerability and therefore reluctant to operate on that route. (Source: exit interview with project manager, 2015)

B4. QUALITATIVE QUOTES ON THE SOCIAL AND ECONOMIC RETURNS ON RELATIONSHIP INVESTMENT

In the beginning, the drivers discovered the new lines. They wanted to go back there after the research. It's really an advantage for them. That's why they considered that creating good relations with the police was the most important (Project meeting notes of September 4th 2015)

The project opened up horizons, I made friends among the officers.
Yes, I have definitively changed my line to a new one, but also with a different boss. Because this line is more profitable than the one I was in. My new boss asked me among the lines I know, which one was profitable, and I suggested one of your diversion lines. Currently, I am driving through VP. [study rerouting line]. (Source: interview with study drivers, 2015)
I use the rerouting lines, but not every day, only for a few hours when there is a lack of passengers on my regular route due to low demand.
Currently, I use two routes - my former line and one of the rerouting lines. In the mornings, I use the VM. route because there are customers during those hours. (Interviews with study drivers, August 2018)

B5. QUALITATIVE QUOTES ON IMPLICATIONS FOR CONSUMER SURPLUS

On a route where the fare is 500 Congolese Francs (FC), if a passenger offers the driver 400 FC, the driver on his designated route would likely refuse, as he knows he's not in a rush. However, the driver who is not on his designated route and needs to avoid getting caught will do everything possible to leave the intersection, so he is more likely to accept the lower fare.
In reality, there are passengers who come with an amount lower than the fare of the route. They start by negotiating, which makes it easier for a driver who is not on his designated route to accept such passengers, compared to a driver who is on his designated route and isn't concerned about getting stopped.
In this matter, in reality, when a minibus operates on a route that it doesn't normally work on, there's a risk of being stopped by various individuals . . . who are stationed at loading points, which are often intersections with police presence. This is why when a driver is not on his designated route, he is afraid to stop at any random stop to pick up passengers, out of fear of being stopped. If he does stop, it's only for a few minutes to monitor the police and ACCO agents. In doing so, he accepts passengers at whatever fare to avoid spending a lot of time discussing with passengers. (Source: exit interview with project manager, 2015)