

# On The Institutional Foundations of Corrupt Exchange: Social Relationships and Joint Profit in Kinshasa<sup>\*</sup>

Aimable Amani Lameke      Albert Malukisa  
Raúl Sánchez de la Sierra<sup>†</sup>      Vincent Tanutama      Kristof Titeca

13 August 2023

**ABSTRACT:** This article presents evidence from the Congo showing that private actors and state compliance officers strategically create mutual social ties between themselves to safeguard their shared profit from corruption's transaction costs. The study focuses on public transport, where a more efficient toll fee system has replaced costly bribe negotiations among mutually connected drivers and officers. A field experiment reroutes drivers in random days to lines where they do not have ties with the officers. Rerouting reduces the proportion of officers on the route who have ties with the driver. It also decreases revenue. The analysis of two cross-randomizations allows us to attribute this effect to the fact that having social ties with the officers reduces the risk of having costly police encounters—in part empowering drivers to bargain a higher ride price from passengers. When interaction with unfamiliar officers is expected to be recurrent, drivers invest in new relationships with the officers through a process commonly known as “buying the land,” and profit recovers as new ties form. The findings emphasize the severity of the fundamental problem of corrupt exchange, the role of social ties in mitigating it, and suggest that corruption profit opportunities shape the social structure. **Keywords:** Corruption, transaction costs, relational contracts. **JEL Codes:** D23, D73, D74

---

\*This study is the outcome of an eight-year collective endeavor drawing on an experiment and data collection conducted in August and September 2015. Ellie Chen, Anna Dagorret, Soeren Henn, David Ifkovitz, Akanksha Kala, Koko Kirusha, and Haoyang (Stan) Xie provided excellent research assistance. We thank the students of Economics, Politics, and African Societies at University of Chicago for comments. The [Ethics Appendix](#) provides an extensive discussion of the ethics of the study.

<sup>†</sup>Email: raul@uchicago.edu. Phone: +1 (917) 488-9151.

## 1. Introduction

Motivated by the notion that state failure impedes the functioning of markets, a huge literature in economics has been concerned with the economic causes and consequences of corruption by state officials (Shleifer and Vishny, 1993, Rose-Ackerman, 1999). Much of the literature has focused on the properties of corrupt exchange, taking as given that, as long as it is mutually beneficial, state officials and citizens are able to exchange the promise of a corrupt action and the promise of a bribe without frictions. This would be reasonable if individuals are generally able to agree on the terms of this exchange without costly negotiations and to credibly commit their respective promises. Stated differently, this presumes that there is no fundamental problem of exchange (Greif, 2000) that needs to be mitigated in the first place to enter in a mutually beneficial corrupt transaction.

However, corruption is typically illegal; thus, the way that society structures the corrupt exchange relationship could have important ramifications for the mitigation of this problem and for the spread of corruption. Complementing this omission, sociologists have suggested that corrupt exchange is “embedded” in social institutions such as social relationships (Polanyi, 1944, Granovetter, 1985). In simple words, this suggests that social relationships and relational contracts (Macchiavello and Morjaria, 2020) may be crucial to sustain corruption. A corollary of this possibility is that, if social relationships help increase the profits from corruption, opportunities for corruption could in turn lead individuals to invest in creating new relationships, shaping the structure of social ties. A challenge to study this question is that social relationships that sustain corrupt exchange are difficult to observe and form endogenously; thus, they have been hard to study and, to date, we have little evidence of how they operate in corruption.

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 is the cost of corrupt exchange? How do social ties between citizens and officials influence such cost? How do these ties form?

This setting was 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 significant revenue loss. Fourth, against the backdrop of this risk, an unofficial toll called the *Kola Nut Greeting* (“*Mbote ya Likasu*” in Lingala) had developed. To take part in it, drivers had to “buy the land” (“*acheter la terre*,” in French), through a large payment after a lengthy negotiation, marking the start of a relationship. 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, relying on relationships built over three years in the minibus sector. Our data collectors gained the trust of ten minibus drivers and travelled every day in the minibuses. This produced a unique dataset of bribes paid to the officers and the time spent negotiating for a bribe, containing 15,426 occurrences of driving through the city intersections in which police officers are posted, of which 2,423 involved interaction with the officers.

Using these data, we first document several facts of minibus bribe payments 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 the ten minibus lines of the distribution network of East Kinshasa. Drawing on relationships

we had built with ten minibus drivers spanning the ten lines and 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 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, totalling 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. To mitigate any financial harm to the drivers and satisfy their participation constraint, at the end of the study, drivers received previously negotiated compensation for participating in the experiment. This design allows us to make three contributions. First, we analyze the effect of foreign-line driving on the driving revenue, net of operating costs (henceforth, *joint profit*). Second, we separately identify whether this effect arises from the driver's expected interaction with the officers on the street. To do so, we borrow from a practice commonly used by private drivers, in which high-ranking officers intervene in negotiations with officers on the street, which were believed to ensure that the driver could be left alone. Assistance from ACCO allowed us to leverage connections with high-ranking officers and consequently to extend their protection to drivers on randomly selected days. Third, we decompose the effect of not having a relationship with the police into: a. not having a relationship, and not aiming to create one; b. not having a relationship, but investing in a new relationship. To do so, we experimentally varied the duration of the rerouted driving blocks. In some blocks, the driver was rerouted for 3 consecutive days; in others, for 1 day. None of the study participants was ever informed that they were part of an experiment.

The analysis of our randomized experiment yields three results. First, rerouting significantly decreases the prevalence of relationships from 56 to 30% and the joint profit from 18.6 to 10.0 USD per day. Second, the negative effect of rerouting on joint profit is muted when the driver was assigned third-party protection. This suggests that the reduction of the joint profit induced by rerouting is *explained by* the properties of the interaction the driver can expect to have with the police officers on the street, and not by confounders related to rerouting. Third, relationships with the officers

increase the bargaining power of the driver over the passengers, and drivers invest in new relationships when the horizon of rerouting is long. We reach this conclusion with the help of a simple two-period decision-theoretic model. In the model, a driver can choose to provide productive effort, which increases revenue per unit of time driving. However, effort also increases the risk of being detained by the police officer, causing time waste from negotiating for a bribe and eventually a bribe payment. Turning to the experiment, we find that: a. short-term rerouting decreases joint profit through lower passenger prices and keeps bribes the same; b. long-term rerouting decreases the joint profit by decreasing the trips per day, but increases bribe costs by 60%. Consistent with the model, this suggests that drivers in short-term rerouting are less patient when negotiating prices with passengers, reducing joint profit, and that in foreign lines, they spend resources to create new ties when the horizon is long. Indeed, within three days, relationships, the number of trips per day and profits catch up.

We combine these results to quantify the value of relationships for producer and consumer surplus. Short-term rerouting decreases joint profit from 24 to 9 USD when the driver was *not* assigned third-party protection. Note that this provides a *lower bound* for the value of these relationships for producers since, as we have shown, rerouting only decreases the frequency of interactions with a relationship from 56 to 30%, as there is some overlap between the lines. Analyzing the effect of unprotected short-term rerouting on the number of passengers driven per day, the duration of a trip, the price of a trip, as well as the operating costs, we further provide suggestive evidence that this increase in producer surplus comes from redistributing consumer surplus to the producers. Our interpretation, consistent with our qualitative interviews, is that driver-officer relationships allow the driver and the officers to collude against the passengers to extract consumer surplus through prices. The intuition is that, when the driver does not have a relationship, he negotiates prices in a rush and accepts lower prices—a common implication of patience in standard bargaining theory—reducing joint profits, but when he does, he can do so securely, recovering bargaining power vis-à-vis the passengers.

The value of these relationships for the joint profits prompts the question of whether creating a relationship is a profitable investment for the driver-officer pair. The dynamic analysis showed that, on average, creating new relationships reduces joint profits by 8

USD from 24 USD and costs 4 USD in bribes, for at least one day. This one-off 12 USD reduction in profit is small compared to the increase in 15 USD daily. Our qualitative interviews suggest two reasons why most drivers have a large number of unexplored lines: a. they are liquidity constrained, and typically start the day with close to zero savings; indeed, the 24 USD of daily joint profit are allocated into bribes, payments to the assistant who calls for passengers, rental fees for the owner, leaving a small profit for the driver; b. drivers are risk averse and, while the benefit of building new relationships may exceed the expected cost in most cases, the downside cost, such as if the vehicle is taken to the station where it is sometimes sold in the black market, can be very large.

Our experimental estimates take the prevailing equilibrium as given. Three theoretical questions are central for their external validity. First, it is possible that the cost of not having a relationship is jointly generated by the equilibrium strategy profile. This could be the (out of the equilibrium path) threat by the police that ensures the drivers do not forego paying the toll fee. 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 other type of agreement. They are the benchmark costs that the cooperative toll fee systems emerge to manage. 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 a prediction shared by a large class of models of repeated interaction in which there is uncertainty about new relationships forming. For example, Kranton (1996) shows that, in that case, the (constrained) optimal level of cooperation can be achieved by simple strategies involving a “bond” in the form of reduced utility at the beginning of the relationship—such as “buying the land.” Theoretically, many factors could 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. But 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, due to the expectations of receiving compensation at the end of the study. However, it is nonetheless a meaningful estimand: it indicates how fast relationships may form if drivers were financially unconstrained.

Our study presents evidence that the social structure shapes corruption and that the social structure responds to opportunities from corruption. This finding complements 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 take the governance of corrupt exchange as given (Olken and Barron, 2009, Shleifer and Vishny, 1993, Bertrand, Djankov, Hanna, and Mullainathan, 2007, Banerjee, Mullainathan, and Hanna, 2012). Our study is the first to empirically establish that social relationships are critical for mitigating the fundamental problem of corrupt exchange.

This finding also complements a body of research emphasizing that connections with state officials can be profitable (Fisman, 2001, Fisman and Wang, 2015). A challenge with the literature is that “political connections are not exogenously assigned,” and that connections are hard to measure, hence existing evidence relies on proxies for relationships and hinges on credible but not experimental identifying assumptions. Our study documents the nature of these ties by measuring them directly and quantifies their effect using a randomized experiment. Furthermore, if social ties are profitable, then profit opportunities should shape the structure of social ties. Drawing on the insights of an emerging empirical literature on relational contracts (Macchiavello and Morjaria, 2020) and randomizing the expected horizon of corruption opportunities, our study is the first to document how ties form in response to such opportunities.

Our results suggest that repeated interaction between citizens and state officials in the context of corruption, usually deemed to be undesirable because it promotes collusion, can also significantly reduce the cost of corrupt exchange—even as this comes in part at the expense of consumer surplus, and would be a second-best solution compared to a system in which drivers would pay a centralized fee to use the roads, and then optimize their routes. Furthermore, our result suggest that relationships could form surprisingly fast, and faster than is generally assumed by governments designing rotation systems for bureaucrats aiming to minimize opportunities for collusion (Greif, 2008).

## 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).<sup>1</sup> 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.<sup>2</sup> Low cost repairs predominated. Although many vehicles in Kinshasa had technical problems, this was especially true for *Esprits de Mort*.

### *Property rights structure and the generation of profit*

The driver was residual claimant and rented the minibus for a fee.<sup>3</sup> 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 preestablished 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 key 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 a lot of latitude to negotiate that rate, depending on their relative patience. The driver would stop for discussions about those prices.

### *The risk inherent to profit generation*

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

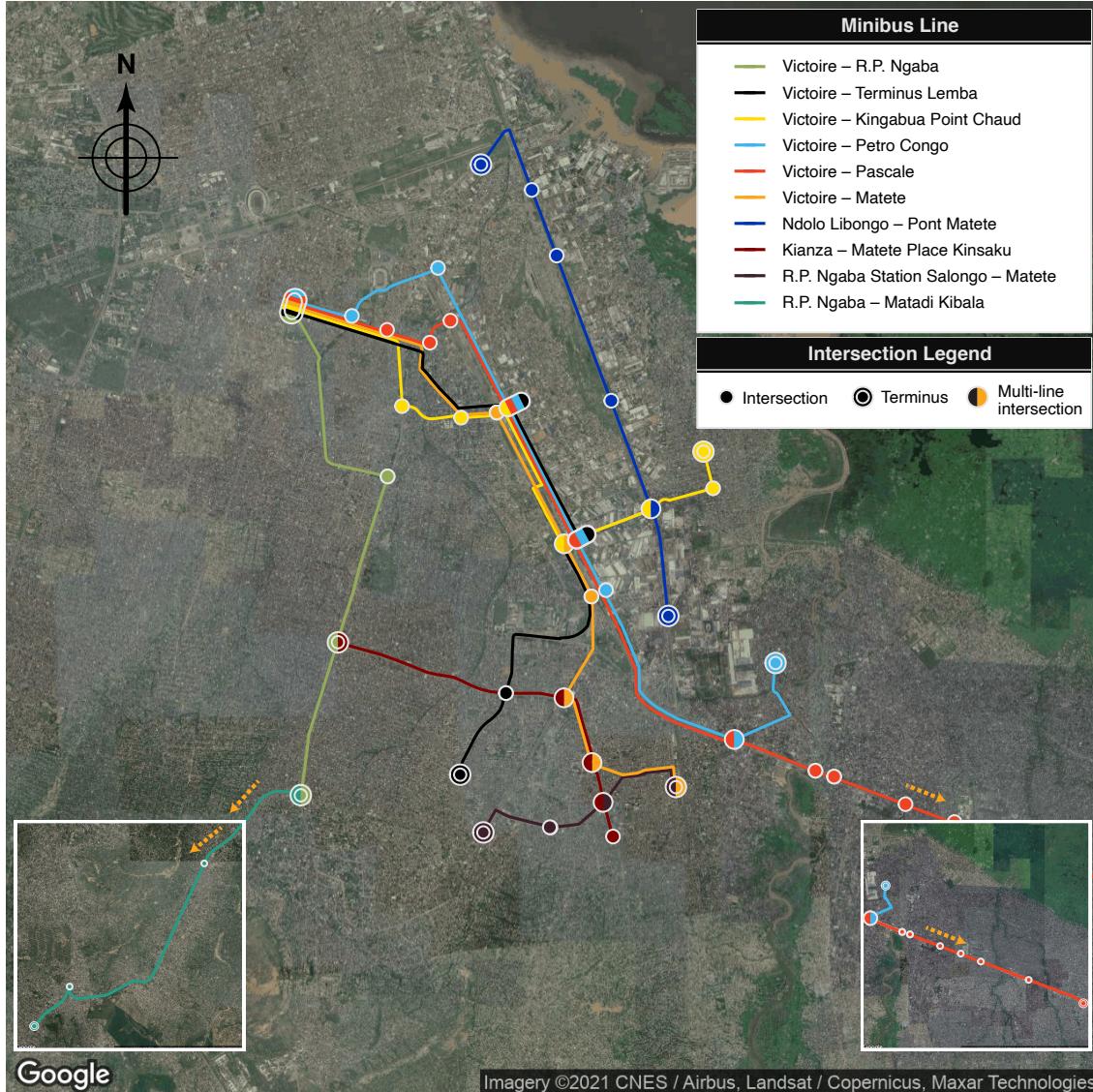
---

<sup>1</sup>Figure A1 depicts an *Esprit de Mort* minibus.

<sup>2</sup>This made them among the oldest in sub-Saharan Africa (Tchanche, 2019).

<sup>3</sup>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 universe of 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).

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 to seize the minibus to the police station, leading to time waste and high bribes. In some cases, it might even lead to the definitive loss of the vehicle: we were told of vehicles that had been sold on the black market by the police station staff upon failing to reach an agreement with the driver.

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.<sup>4</sup> 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 police officers stationed there, 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 avoid detainment with more ease.

#### *The social institution: relationships and the unofficial toll fee*

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.<sup>5</sup> 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*), in which case, they were vulnerable to detainment and lengthy negotiations:

The *Mbote ya Likasu* is money that the driver gives the officer as the officer’s right. (Project notes of July 18<sup>th</sup> 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. For example, members of the study’s implementation team told us:

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

---

<sup>4</sup>Figure A2, Panel A, illustrates a typical instance of a negotiation bribe.

<sup>5</sup>Figure A2, Panel B, illustrates the *Kola Nut Greeting* (“*Mbote ya Likasu*”) through a handshake.

arrested because he is known. Here I am new. It is easy for me to get arrested.

(Project meeting notes of July 10<sup>th</sup> 2015)<sup>6</sup>

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 4<sup>th</sup> 2015)

Empirically, thus, payments to buy the land were observationally equivalent from any other negotiation bribes: not only because the distinction depends on the intention of the driver, which is subject to interpretation, but also because even in cases of involuntary large payment, a friendship/relationship is automatically made.

*Another solution: seeking third party “protectors”*

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. The following description illustrates the value of third-party protectors in the negotiation with the officers:

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

---

<sup>6</sup>Project meeting notes are records that we kept on a daily basis recording everything that was said in the project meetings by the research project members, to coordinate and monitor implementation, as well as to gather information about the system we studied. Typically, there were two such meetings each day. Ten data collectors, three data collection and treatment assignment supervisors, a scientific data coordinator, an operations coordinator, and the PI's took place of that meeting. The field staff having close interaction with the drivers and observing their work each day, these meetings were an opportunity to absorb and immortalize rich qualitative data that the project was being exposed to while it was under way.

realized it was this one officer who knew the major who pushed his colleague to release the minibus. (Project implementation notes of September 4<sup>th</sup> 2015)

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. Corruption, Embedded in Social Relationships: The Home Lines

We now present stylized facts about the system of corruption in the minibuses “natural” environment: the home lines. The data were collected from ten minibuses between June 19<sup>th</sup> and July 20<sup>th</sup> 2015 in the ten lines of the East Kinshasa network.<sup>7</sup> The data contains information on 15,426 events in which a driver drives through one of the intersections of the network, which are observed from driving through 49 unique intersections.

Figure 2 presents the type of interactions between the drivers and the police officers.<sup>8</sup> The vast majority of interactions are events in which the driver pays a bribe without negotiation (84.1%), which are predominantly toll fee payments (83.0%). The remainder comprises amicable conversations with no payment (13.1%) and negotiations (2.7%).

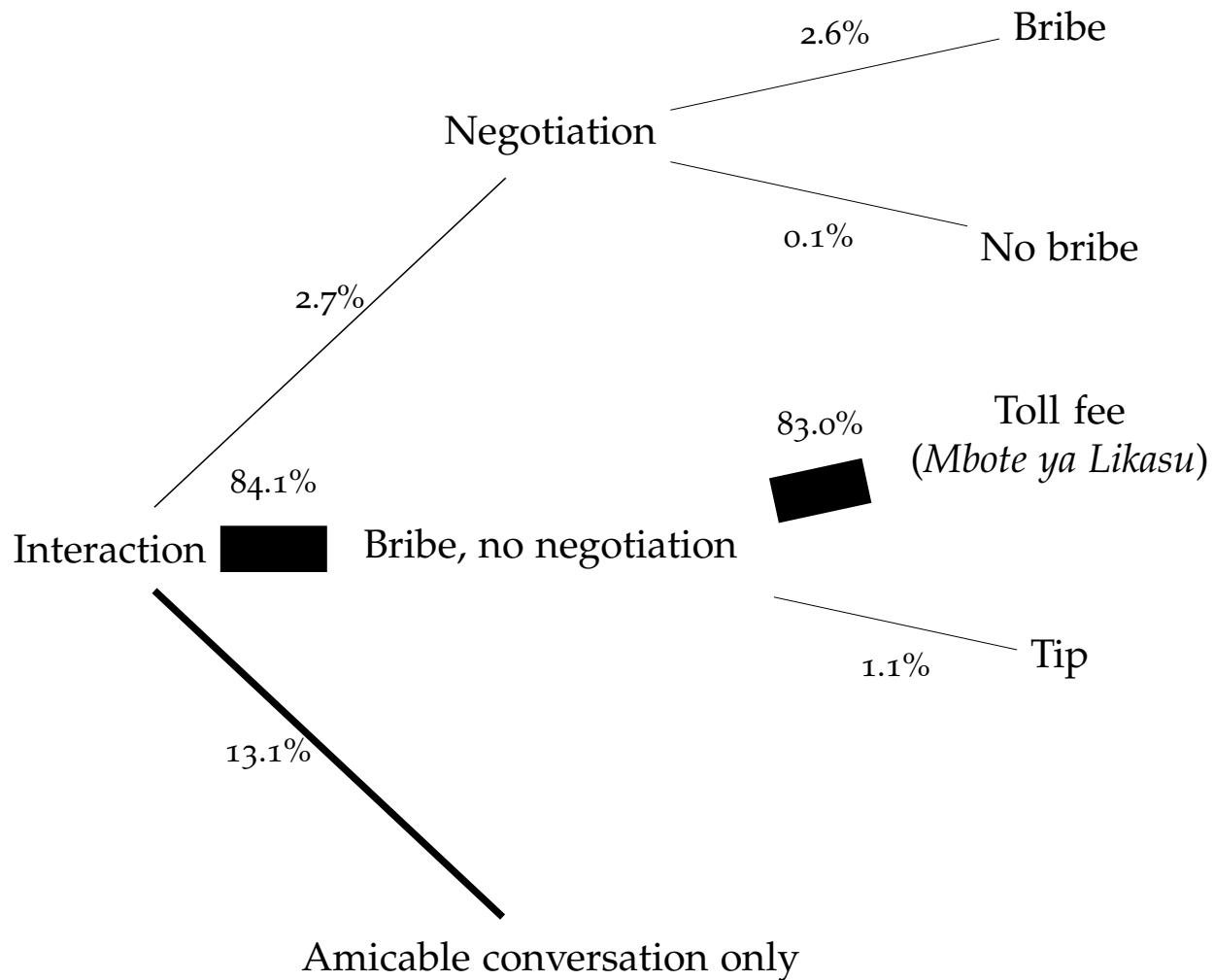
Table I, Panel (a), presents basic facts about the toll fee level vs. other types of bribes in these interactions. The toll fee is the smallest of all bribe payments, amounting to .53 USD on average. In contrast, the mean negotiation bribe payment is 9.79 USD. The toll fee is also extremely stable: while the toll fee standard deviation is .32, that of negotiation bribes is 7.05, suggesting enormous heterogeneity given that per capita income is around

---

<sup>7</sup>The collection of these data is the outcome of three years of qualitative work, six months of preparatory work for the quantitative data collection in which we field-tested various approaches to collect data on the minibuses. 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).

<sup>8</sup>Figure A3 shows that most of these interactions are with street-level traffic police.

**Figure 2:** 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<sup>th</sup> and July 20<sup>th</sup>, 2015, in a tree diagram. The data were collected from ten minibuses between June 19<sup>th</sup> and July 20<sup>th</sup> 2015 in the ten lines of the East Kinshasa network. The figure includes the percentage breakdown of 2,423 interactions that we observed between police officers and 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.

**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)
<i>Average per transaction made</i>	0.82	2.03	0.04	27.14	2,103
Toll fee ( <i>mbote ya likasu</i> )	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)
<i>Fraction of intersection crossings in which time interacting with police is strictly positive</i>	0.16	0.36	0	1	15,426
<i>Time wasted in interaction with police, per transaction conditional on interaction time strictly positive (duration of negotiation in minutes)</i>	0.83	2.13	0.1	55	2,103
Toll fee ( <i>mbote ya likasu</i> )	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)
<i>Number of trips per day</i>	5.92	2.56	1	15	261
<i>Number of times per day in which a payment is made at the average intersection (any payment)</i>	1.24	2.09	0	12	1,700
Toll fee ( <i>mbote ya likasu</i> )	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<sup>th</sup> and July 20<sup>th</sup> 2015, observed in the drivers' home lines. The data were collected from ten minibuses between June 19<sup>th</sup> and July 20<sup>th</sup> 2015 in the ten lines of the East Kinshasa network. 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 a driver drives through an intersection, from 49 unique intersections. Of these events, 2,103 resulted in the payment of a bribe.

1 USD per day.<sup>9</sup> 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 minutes.

In terms of prices and number of passengers, we did not collect such information in the pre-experimental data, which we used to develop the research questions. However, using the data that we collected in the experiment that we present in what follows, 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. In terms of passengers, 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 in that sample.<sup>10</sup>

## 4. The Experiment

In this section, we present our experimental design of the rerouting treatment, the assignment procedure, its implementation, and the measurement of minibus outcomes, as well as cross-randomization. We conducted the experiment *in August and September 2015*, after the original data collection. We randomly rerouted minibus-days within each of ten minibuses to drive in foreign lines in some days and not in others.

---

<sup>9</sup>Table A1, Panel (a), presents the per-trip equivalent. The large share of the toll fee in total bribe costs arises from the frequency with which it is paid.

<sup>10</sup>In the Appendix, we provide various extensions of this analysis that provide reassurance in the validity of these conclusions and in the external validity of the samples used in the paper. First, Figure A4 presents histograms of time waste by bribes, confirming that the toll fee payments are very stable compared to negotiation bribes. Second, Table A1, Panel (b), presents the per-trip equivalent. Due to the fact that negotiation bribes are infrequent in the home lines, they account for only 25% of time negotiating despite the fact that one negotiation bribe takes 11 times longer to negotiate than the toll fee. Third, Figure A5 presents the hourly time series of average bribe cost and time waste across all days in the pre-experimental data. The figure provides reassurance that these descriptive statistics of negotiation incidence, bribe level, and negotiation length are not explained by differential variation in the day. Fourth, Table A2 presents the same analysis as Table I using, instead, the data from the experiment, for the observations on home lines. It shows that the experimental sample is comparable to the pre-experiment benchmark, providing reassurance about the external validity of the experiment that follows.

## A. Rerouting to Foreign Lines

### Identifying foreign lines

Drivers had a set of lines they considered feasible, in the sense that they had enough relationships with police officers at the intersections (*home lines*). The rest of lines are henceforth labeled *foreign lines*. The rerouting treatment consisted of an encouragement for drivers to drive through lines considered “foreign lines” for a block of contiguous days. There are also 10 drivers each with one main home line, for 27 days. To determine which lines were home and foreign for each of the drivers, we organized meetings with the drivers. Those meetings were organized prior to revealing the details of the experiment to reduce concerns for misreporting. On average, drivers had 1.3 lines that they considered home, and 8.1 foreign lines.

### Randomization procedure

The randomization procedure selects driver-days to a given treatment assignment as follows. The sample for randomization consists of ten drivers spanning ten days, resulting in 270 minibus-days. The experiment time window was subdivided into a first wave of 23 days and a second immediately after, of four days.<sup>11</sup>

- **Step 1:** We randomly partitioned the 23 days of the first randomization wave 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:** the driving blocks were randomly assigned to home or foreign as follows. For each driver, three of the seven three-day driving blocks were randomly assigned to home line. The remaining four of the seven three-day driving blocks for each driver were assigned to rerouting toward a foreign line (the identity of what specific

---

<sup>11</sup>Due to initial uncertainty over the extent of funding, the experiment is divided into two periods, within which randomization occurs. The number of minibus used in the analysis reflects funding limitations. As described in this section, however, the experiment’s inference is based on 270 minibus-days treatment clusters and, overall, the experiment sample is constituted of 13,092 events of crossing an intersection produced by 1,935 trips (compared to 304 trips in Olken and Barron (2009)). The experiment was implemented prior to pre-analysis plans becoming a norm, in 2015. The original hypothesis was that rerouting destroyed the surplus due to bribe negotiations and the expectations of negotiations. Theoretically, the level of the bribes in home or foreign lines was not clear a priori given the lower rate of interactions in foreign lines and the possibility that the toll fee is priced to extract the value of the relationship.

foreign line is to be determined in the following step). All the days within each three-day block had the same home vs. rerouting assignment. Furthermore, for each driver, of the two one-day driving blocks, one was randomly selected to be a home line and the other to be a rerouting line.

- **Step 3:** We pinned down what specific line to drive, conditional on this assignment:
  - a. In driving blocks assigned to home, drivers were asked to drive on their main line—some minibuses had multiple home lines, in which case we excluded the remaining home lines from the pool of foreign lines from which to select foreign rerouting;
  - b. For each driver, in each driving block assigned to rerouting, we selected the unique foreign line to drive for all days of that driving block as follows. From the pool of lines that are considered foreign for that driver, we randomly selected one, *without replacement*. As a result of sampling *without replacement*, it follows that a foreign line could never be the selected line for a driving block in more than one driving block for a given driver.
- **Step 4:** Having implemented this randomization procedure to the first wave, we added four days to the study, the second randomization wave. Randomization for the four days of the second wave was implemented as follows: for each driver, the remaining four days were subdivided into four one-day driving blocks; then, for each driver, half (two) of these four days was assigned to home line driving (randomly selected) and the remaining half (two randomly selected days within driver) was assigned to rerouting. Among rerouting days, for each driver, foreign lines were randomly selected as in the first wave, among the pool of foreign lines that had not been previously selected, in any wave, for the corresponding driver.

Table A3 provides a *stylized* summary of this randomization procedure for a typical driver, *prior* to randomizing the sequence of driving-blocks, for presentation purposes. In reality, the sequences presented are randomized within each of the two waves, separately for each driver. The experiment of foreign line rerouting was thus of a stratified, clustered randomization design. We stratified the randomization by minibus driver, ensuring balance between foreign and home driving blocks. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving

block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata.

#### *Drivers' incentives to participate and to comply to the randomization assignment*

Compensation of drivers was tied to *participating* in the experiment, and was orthogonal to treatment status. This compensation was decided ex-ante per weeks-long conversations with the drivers. The amount of the compensation was calculated based on the expected losses from driving on foreign lines on some days and not on other days.<sup>12</sup> At the end of the study, drivers withdrew the remaining funds.

If a driver failed to comply with the randomization schedule, the research team would exclude the driver from the experiment. Given the value to the drivers of the funds kept by the experimenters for the end of the study, drivers had no incentive to violate the encouragement to reroute. Monitoring was tight to verify where each driver was driving in real time, using a supervisor. We detail data collection in Appendices A1-A3. We recorded no violation of the assignment thorough the experiment.

#### **B. Cross-Randomization: Third-Party Driver Protection**

To isolate the effect of foreign driving stemming only from the interaction between the driver and the police officers, we cross-randomized third-party protection.

We provided to each driver on randomly selected days a portfolio of actors representing various types of third-party protection. To extend third-party protection to the drivers, the Congolese members of our team identified the set of high-ranking police officers who regularly offered their protection, and a set of street-level police officers with whom we had established relationships in a previous study.<sup>13</sup> We also worked with

---

<sup>12</sup>The compensation was identical 500 USD for each driver and withheld until the experiment's completion. Drivers kept the right to request from the account sporadically if there were severe needs of cash. This happened few times—for example, after a few drivers were detained and escorted to the police station.

<sup>13</sup>Involvement of the non-Congolese members would have triggered expectations of high payment and extortion. Our design thus uses existing fees in an already existing market for protection.

ACCO to produce a sticker to be visibly fixed on the minibus' windshield that could easily be removed at the end of the day. Figure A6 presents one example.<sup>14</sup>

After having randomized the sequence of thirteen driving blocks within each driver, and having randomly assigned the home vs. foreign treatment across these blocks, assignment to each type of police third-party protection (high-ranking officers or street-level police officers) and to ACCO third-party protection was randomized as follows.

First, stratifying by the set of three-day blocks within each minibus, we randomly assigned third-party police protection in either one or two days in the block. Stratifying by the set of one-day driving blocks within each minibus, we randomly assigned the day to either receive the police third-party protection or not. This ensures random assignment and balance of blocks of third-party protection for all minibus driving blocks.

Second, among driver-days assigned to third-party protection, we randomly assigned to the type of police protection. In a first step, we randomly assigned either high-ranking or street-level police to those protected days. Street-level police third-party protection are the police officers who sat with the driver in the minibus (henceforth "*police escort*"). High-ranking police third-party protection are the colonels or majors who can intervene upon a call from the driver. In a second step, we randomly assigned either colonel or major to those high-ranking-protected days, within minibus driving blocks.

Third, we randomly allocated days of ACCO sticker protection stratifying by minibus-driving block-protection status as follows. 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. In contrast, the remaining day within the three-day driving block or the one-day driving block has a 0.43 chance of getting a sticker. Qualitative notes from the project indicate that drivers were eager to carry the sticker because of the protection

---

<sup>14</sup>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), intended to signal to the police officers on the street that the driver was affiliated to the ACCO.

it provided, and they asked to keep the sticker at the end of the study.<sup>15</sup>

To plan the third-party protection treatment schedule, we developed one calendar for drivers, one calendar for each of the protectors, and one calendar for each of the project implementers who accompanied the minibus drivers. Each of our project implementers also doubled as a data collector. They collected driver outcome data and were supervised by a project coordinator, and a data collection monitor. The calendar for drivers indicated, for each day, whether and from whom they had third-party protection. The calendar also indicated the phone number of high-ranking protector to be used in case of need. Drivers were trained that they would not be protected outside of their assignment. The calendar for the police protectors calendar indicated, for each day, which drivers to protect. The total number of driver-days in their calendar was equal to the value of the protection purchased by the researchers. Police third-party protectors were trained so that, if they received a call by a driver in our study, they would first inspect if they were supposed to protect the driver on that day. If the driver was protected that day, they implemented the “intervention” over the phone. Otherwise, they were trained to ignore it.

To ensure compliance with the treatments, each of driver’s project implementer had the following tasks. First, between 5 and 6 a.m., the implementers met with the police officers who worked as police escorts. Second, each implementer met each driver at their termini before the first trip. For drivers assigned to third-party protection by police escort, the implementer arrived with the police escort, and seated him before the passengers on the front seat, which is the usual seat for police escorts in this setting. Third, prior to the start of driving on each day, the project implementers verified the

---

<sup>15</sup>We also overlaid another type of protection that we did not previously pilot: protection by one of three captains. Protection by a captain was a priori the weakest, because: a. the captains had the lowest level on the hierarchy among the protectors not present in the minibus; b. unlike other third-party protectors, captain treatment was expected to only be effective in a small subset of intersections; c. we did not pilot the captain protection. As a result of these weak priors, assignment to captain protection is the only treatment that is overlaid after the number of units in treatment cells defined by the previous treatments was exhausted. To do so, every day of the study, we randomly drew half of the minibus drivers to receive protection by a captain. In addition to having weak priors about the meaningfulness of this layer of protection, during the implementation, we discovered that the effectiveness of captain protection 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. As a result from these priors and learning during implementation, in what follows, we exclude this third-party protection from the analysis. The results are *identical* if we include captain protection as a control.

calendar of assignments, provided reminders to the drivers, otherwise enforced the calendar. Each day drivers assigned to not carrying the sticker were monitored to remove it before the first round. Drivers were reminded of the protectors they had that day.

Table A4 presents the randomization balance of assignment to foreign treatment across days of week, and month. The characteristics summarized in Table A4 appear balanced.

### C. Cross-Randomization: Time Horizon of Rerouting

We designed the randomization into driving blocks to also generate experimental variation in the time-horizon of the rerouting. This variation allows us to examine whether longer-horizon of rerouting causes investment of a new relationship. As previously indicated, nine of each driver's thirteen driving blocks comprised three days; the remaining four driving blocks comprised one day. Since we randomize the sequence of driving blocks within the driver, the time at which he drives in a three-day vs. a one-day block is random. Randomizing foreign vs. home line for these driving blocks within each driver generates a random assignment to driving in a three-day or a one-day driving block. To ensure that foreign and home line driving were balanced for each of the three-day and one-day driving blocks, the randomization procedure for foreign vs. home line was also done blocking by three-day set of blocks, and by one-day set of blocks.<sup>16</sup> If three days were a sufficiently long horizon for drivers to deem creating a new relationship worthwhile, the effect of rerouting should be heterogeneous by whether their relationship time horizon is over 1 or 3 days.

Table A5 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 almost perfectly balanced. For this reason, the appropriate specification will include randomization blocks (minibus) fixed effects.

---

<sup>16</sup>Including this additional randomization block as fixed effects is inconsequential for the estimation.

## **D. Artificiality of the Research Design and Participants' Beliefs**

### *Design: To What Extent is it Artificial, and Where Does it Matter?*

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-part protector of various types. The nature of these arrangements is also sufficiently varied in this ecosystem that our contract did not generate feeling of it being artificial. The following quote, provided by the research manager in an exit interview after the study, articulates the extent to which the experiment design fit existing practice:

We have a friend Z., he bought a RAV4 vehicle, but he never even registered the license 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.

Second, it is also common for drivers to explore new routes. The research manager noted in the project's qualitative exit interview:

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.

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.

### *Beliefs Held by the Drivers*

First, 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. The following quote, from the research manager in an exit interview, describes this:

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.

Second, 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.

Third, 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.

Fourth, 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 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.<sup>17</sup> Drivers furthermore knew that no future opportunity was held conditional on how they interacted.

#### *Beliefs Held by the Third-Party Protectors*

Third-party protectors 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. The following quote, from the research manager in an exit interview, describes what was communicated:

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.

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 to intervene.

---

<sup>17</sup>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.

### *Beliefs Held by the Police Officers in the Street*

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 study). This made no difference between the home and foreign routes. The following quotes from the research manager describe how this was achieved. First, they did not know the driver was part of a study:

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 ...

Second, street officers did not find protection by an officer travelling inside the vehicle artificial, nor knew that was part of a study:

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.

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:

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.

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:

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.

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:

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.

In sum, we can quite confidently say that the officers in the street acted without perceiving anything out of the ordinary as a result of our study.

### *Learning by the Participants*

Drivers did not learn anything, they simply benefitted from the creation of lasting relationships that would follow them after the study. First, the creation of new relationships, buying the land, the value of protection by known police individuals, are all normal practice. Second, the rigid experimental protocol was rigorously adhered to, thus noncompliance could not induce correlation between driver blocks. Third, foreign lines were sampled without replacement, reducing correlation across driving blocks.

The strict adherence to protocol raises questions about interpretation and how drivers were recruited and incentivized to comply. We have already described the compensation package received by the drivers for accepting to participate. To monitor their compliance, an observer was always in the minibus, and the observers were themselves anonymously monitored by three rotating supervisors who were regularly acting as “mystery” passenger observing the data collector and the driver. The strict incentives to comply to the assignment were communicated to the drivers prior to the experiment and the drivers we recruited is a selected set of drivers who were willing to go through with this condition.

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 following quote exemplifies the scope for such learning:

There haven't been many troubles. It was only once on 7<sup>th</sup> 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)

In sum, meticulous attention to the system at the design stage ensured that participants took actions that were *not* out of equilibrium, that we simply incentivized variations of choices within normal practice in compliance with the community's norms. This provides reassurance that the behaviors do not reflect artificiality, Hawthorne effects, or hard to predict beliefs about situations that they never encounter.

## E. Ethics

The [Ethics Appendix](#) discusses the ethics of the study at length. In this section, 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 by the study, because it exposed them to the need to pay a bribe.

Three important features mitigate these valid concerns: a. 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); b. 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; c. Extending third-party protection did not promote or help conceal illegal behavior; instead, it reduced the extent of corruption taking place between drivers and officers (the alternative was to pay a bribe); d. The short-lived nature of our interventions limited opportunities for the participants to make investments that would have amounted to encouraging illegal behavior. If anything, the qualitative evidence we gathered in the exit interviews is rather consistent with the long-term effect of the income shock of our compensation package (which was always never fully used in its entirety during the project itself) might have in fact contributed to increasing compliance with the law:

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 study driver, August 2018)

## F. Sample Characteristics

The experiment sample 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 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.<sup>18</sup> 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.

## 5. Effect of Rerouting on Relationships and on Joint Profit

We now analyze how rerouting to foreign lines affects relationship and joint profit.

### A. Estimation

To identify the effect of driving on a foreign line, our analysis includes all observations in which the driver is on a home line and those in which he is on his first day of rerouting (the first day of a three-day block or the only day of a one-day block).

Let  $i$  index the intersection,  $r$  index the round (trip) number,  $d$  index the driver, and  $t$  index the day-of-study. Let  $\mathbb{1}_1(\text{Foreign}_{irdt}) \in \{0,1\}$  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. We estimate the following equation using ordinary least squares (OLS):

$$Y_{irdt} = \beta^F \mathbb{1}_1(\text{Foreign}_{irdt}) + \alpha_d + \epsilon_{irdt}, \quad (1)$$

where the dependent variable  $Y_{irdt}$  is an indicator taking value 1 if the driver has a social tie with the police agent with whom he interacts; in some parts of the analysis,

---

<sup>18</sup>Table A6 presents these basic characteristics. Figure A7 shows a schematic map with key statistics.

Equation 1 omits subscripts  $ir$  to indicate the daily joint profit.<sup>19</sup> Parameter  $\alpha_d$  denotes the foreign/home line treatment assignment randomization strata (i.e., the driver). 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 ten 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.<sup>20</sup> We thus include inverse propensity weights using these probabilities in weighted OLS (henceforth “*rerouting weights*”).<sup>21</sup> We henceforth refer to this as the baseline specification, and denote it the average treatment effect (henceforth, *ATE*), referring to the average effect of “driving on a foreign line, given the pool of foreign lines available to the drivers.”

In robustness analysis, we also estimate the following specifications. First, we exclude from the baseline specification the randomization weights and the randomization block 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 block fixed effects; we refer to this as the sample average treatment effect ( $SATE$ ). Third, to account for the fact 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)—where population refers to the population of lines. Fifth, when the dependent variable is continuous, we estimate the baseline specification using various ways to handle extreme values.

---

<sup>19</sup>In this case, Equation 1 is  $Y_{dt} = \beta^F \mathbb{1}_1(\text{Foreign}_{dt}) + \alpha_d + \epsilon_{dt}$ , where  $Y_{dt}$  is the daily joint profit, in USD, in  $\mathbb{R}^+$ . Recall that the joint profit is daily revenue minus operating costs, such as gas and repairs, whereas any bribes paid will be deducted from the joint profit so that joint profit net of bribes is profit.

<sup>20</sup>To recover the probability of assignment to foreign or home lines, we conducted 10,000 simulations of the above-described 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.

<sup>21</sup>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.

## B. Effect of Rerouting on the Fraction of Officers on the Line with a Social Tie

Interaction with street-level traffic police officers make up the vast majority of interactions (881 out of 1,097).<sup>22</sup> 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.<sup>23</sup> The effect arises from a decrease in relationships described as friendships, for which the fraction decreases from 23.4 to 8.3%.<sup>24</sup> 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 sample.

## C. Effect of Rerouting on Joint Profit

We refer to the joint profit as the revenue from passenger payments, net of gas expenses, and repairs (henceforth, *operating costs*). The joint profit is then allocated between the police officers through bribes, and the driver through profits. On average, on home lines, drivers make 3 USD of joint profit per trip.<sup>25</sup> This is generated from 8.79 USD in revenue from passengers, net of 5.79 USD operating costs per trip.<sup>26</sup> The 8.79 USD in revenue is

---

<sup>22</sup>Source: Figure A8. The subsample 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 in the sample, 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.

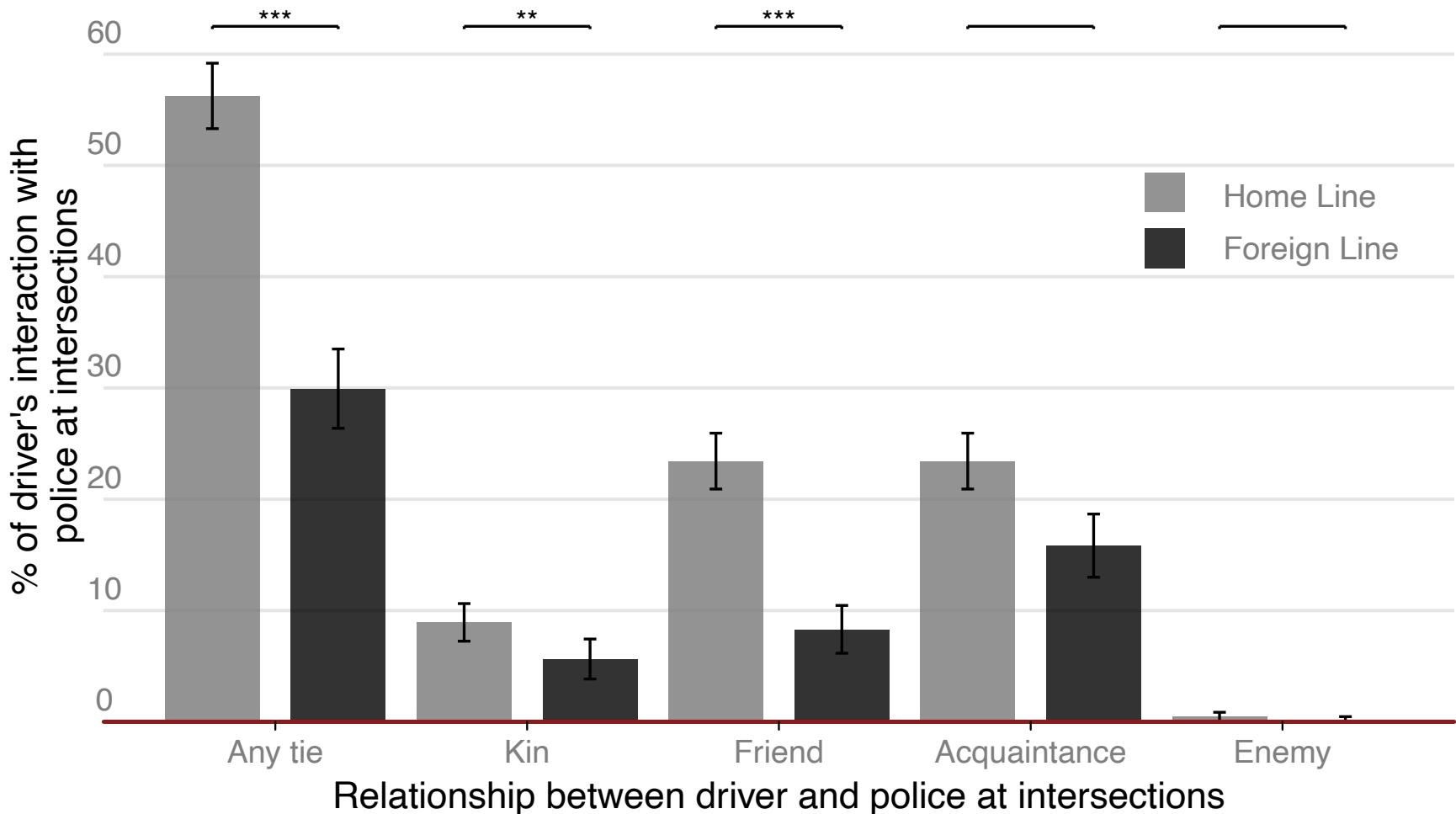
<sup>23</sup>Figure A9 shows the result holds equivalently per intersection. Figure A10 compares the baseline coefficient estimate to alternative robustness specifications that account for this imbalance. The results are preserved across specifications.

<sup>24</sup>The effects on relationship are largely underestimated if all “*mbote ya likasu*” payments were coded as having relationship with police officers of at least acquaintance if not friendship, as qualitative experiences dictate. However, we rely on enumerators’ raw coding of relationship to avoid data manipulation.

<sup>25</sup>This description is based on Table A8.

<sup>26</sup>Some costs, such as gas, are not incurred per trip. We impute the per trip average in this table.

**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 asterisks represent the statistical significance of the test for whether the fraction on the home line is statistically significantly different than that on the foreign line, for each relationship category. Statistical significance is obtained from estimating  $\beta^F$  of Equation 1. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata. Social relationships include those of kin, friend, acquaintance, and enemy. Kin includes both nuclear and extended family relationship. 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.

produced by 18.36 passengers on average, who pay 48 cents of USD on average.<sup>27</sup>

Table II presents Equation 1 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.<sup>28</sup>

In sum, this analysis confirms that rerouting is associated with a decrease in the prevalence of relationships between the drivers and the officers with whom they interact, as well as a destruction of the joint profit. This is consistent with drivers having created joint profit enhancing relationships on lines that become their home lines.

To isolate the mechanisms through which rerouting reduces the joint profit, in the analysis that follows, we follow two strategies. In Section 6, we decompose the main effect of rerouting by whether the driver is assigned to third-party protection, to purge the effects of rerouting arising through other channels than those related to the interaction between drivers and the officers. In Section 7, we separate the average effect of rerouting on joint into whether the driver has a long- vs. a short-time horizon. Leveraging the comparison of this effect across different time horizons, we also break-down the analysis into various outcomes. This allows us to examine whether rerouting affects the outcomes which, if arising from relationships, it should affect in short- vs. long-term rerouting.

## 6. Separately Identifying Social Ties: The Third-Party Protector Placebo

In this section, we test for the mediating role of relationships in explaining the effect of rerouting on the joint profit, by exploiting the randomly induced variation in the presence of third-party protection on the side of the driver. As in Section 5, we analyze

---

<sup>27</sup>A complementary graphical representation of the joint profit decomposition is in Figure A11.

<sup>28</sup>Table A9 present the estimates in alternative specifications to account for misspecificaiton and outliers.

**Table II:** Effect of Rerouting to Foreign Lines on the Joint Profit

	Joint Profit (per day) (1)	Number of trips (per day) (2)	Joint Profit (per trip) (3)
Foreign	-8.071* (4.297)	-0.982*** (0.363)	-0.855* (0.483)
Mean Dep. Var.	18.60	7.86	2.62
Observations	167	176	1,395
R <sup>2</sup>	0.324	0.318	0.194
Rand. Block FE	YES	YES	YES
Foreign IPW	YES	YES	YES
Composition IPW	YES	YES	YES

*Notes:* This table presents the OLS estimates of  $\beta^F$  from Equation 1. In Column (1), the dependent variable is the daily joint profit in USD·day<sup>-1</sup>. The daily joint profit is the sum of joint profit generated through each trip driven in the day. Hence, Columns (2) and (3) also decompose the joint profit per day as dependent variable into the number of trips per day (Column 2) and the average joint profit along each trip (Column 3). Joint profits per trip (Column 3) 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. All regressions include randomization block 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. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the sample of home line driving.

the effect of rerouting using the first day of a rerouting episode to prevent any newly created relationships from confounding the analysis.

As described in Section 4, the third-party protection treatment is a bundle of various forms of protection, so that the third-party protection is representative of the type of third-party protections used normally. In what follows, we are reporting the results using this bundle to classify observations, without distinguishing the type of protection, since the study is not powered to isolate the differential effect of any type of protection. However, we note that individual effects can be isolated and that the results are in part driven by third-party protection by an officer travelling in the bus, by the ACCO sticker, and by some high-level protection. In general, high-level protection was less meaningful, as they had to be called, they were not always responsive, and sometimes they had to drive to the intersection to talk in person, hence drivers used them less.

In Table III, we present the coefficient estimates of Equation 1 separately by whether

the driver was randomly assigned to third-party police protection on that day.<sup>29</sup> 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 23.78 USD, and the drop is statistically significant. Column (2), in contrast, shows that rerouting decreases joint profit by -3.29 and this coefficient is not statistically significant. This is consistent with the interpretation that rerouting reduces the joint profit through its effect on the interaction between the driver and the officers.

Columns (3)-(6) replicate this analysis for the number of trips per day and the joint profit per trip. Similarly, 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 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.

Another way to measure the value of protection to isolate the mechanism of rerouting could have been to measure the willingness to pay for such protection under different experimental circumstances. We can use the data from the qualitative interviews to paint a picture about this important parameter of the study:

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<sup>th</sup> 2015)

I still use the sticker until today because it always helps me. The police believe it's an ACCO vehicle. Once, I committed a parking violation, and the riders hesitated to approach me when they saw the sticker. (Exit interview with study driver, August 2018)

---

<sup>29</sup>Tables A10 and A11 show the same for whether the driver was assigned to wear the ACCO drivers' association sticker, or to police protection. The results are the same.

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

	Joint Profit (per day)		Number of trips (per day)		Joint Profit (per trip)	
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign	-23.78** (11.00)	-3.29 (4.95)	-1.66** (0.67)	-0.63 (0.44)	-2.30* (1.36)	-0.52 (0.50)
Mean Dep. Var.	22.02	22.24	8.21	8.27	2.95	3.01
Observations	47	120	50	126	388	1,007
R <sup>2</sup>	0.38	0.37	0.37	0.32	0.21	0.24
Protection	NO	YES	NO	YES	NO	YES
H <sub>0</sub> p-val.		0.079		0.142		0.217

*Notes:* This table presents the OLS estimates of  $\beta^F$  from Equation 1, decomposed by whether the driver-day was assigned to third-party protection. We present the estimates of  $\beta^F$  separately for the sample without third-party protection (odd columns) and with third-party protection (even columns). In Columns (1) and (2), the dependent variable is the daily joint profit in USD-day<sup>-1</sup>. The daily joint profit is the sum of joint profit generated through each trip driven in the day. Hence, Columns (3), (4) and (5), (6) also decompose the joint profit per day as dependent variable into the number of trips per day (Columns 3 and 4) and the average joint profit along each trip in (Columns 5 and 6). Joint profits 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. 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 block 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. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full sample of home line driving. Table notes denoted "H<sub>0</sub> p-val." present the p-value from the interaction term in the regression of outcome variables on foreign line and third party protection assignments. It is the p-value for the test of whether unprotected and protected rerouting have the same effect.

This section has shown that the effect of rerouting is *muted* if the driver has third-party protection. This provides conclusive evidence that rerouting, when not having third-party protection, identifies the effect of the relationships between drivers and officers on the efficiency of corruption. We analyze the mechanisms for this result in the next section, by exploiting the random assignment to different rerouting horizons.

## 7. Mechanisms: How Absence of Social Ties Destroys Joint Profit

To determine the channels by which relationships increase joint profit, in this section, we examine the randomized variation in rerouting horizon. Before analyzing its effects, we present a simple framework to guide which outcomes should be altered by this variation.

## A. A Simple Model of Relationship Horizon and Corruption

We compare the joint profit by a driver on a line where he has relationships with police officers, to that on a line where he does not, as a function of the time horizon.

There are two periods, denoted  $t = \{0,1\}$ . There is one driver. In each period, the driver can generate driving yield  $y(e) \in \mathbb{R}^+$ , an increasing function, by choosing to provide (costless) productive effort  $e \in \{0,1\}$ .<sup>30</sup> Let  $y(1) = y^1$  and  $y(0) = y^0$ . If the driver does not have protection, in any given period the driver realizes the driving yield with probability  $p(e)$ , where  $p(0) = p^0$ ,  $p(1) = p^1$ , and otherwise is detained and negotiates the whole period bribe  $B > 0$ , making no yield and paying  $B$ . If the driver has protection, the driving yield is realized with probability 1 in each period. In period 0, the driver chooses whether to purchase protection from an officer,  $\eta \in \{0,1\}$ . Protection costs  $F + f$  in period 0 and  $f$  in period 1 and is valid both periods, but also takes negotiation time to acquire. The opportunity cost of buying protection is  $(1 - k(\eta))y(e)$ , where  $k(\eta) \in [0,1]$  and  $k(0) = 1$ .<sup>31</sup>  $\delta < 1$  is the time preference parameter. In each period, the joint profit is  $S = k(\eta)p(e)y(e)$  and the driver's profit is  $\pi = S - (1 - \eta)(1 - p(e))B - \eta((1 - t)F + f)$ .

**Assumption A**  $y^1 > y^0$

**Assumption B**  $p^1 < p^0$

**Assumption C**  $\frac{y^1}{y^0} < \frac{p^0}{p^1}$

**Assumption D**  $y^1 - p^0 y^0 + (1 - p^0)B < F + f$

Assumptions A and B say that effort increases joint profit and the probability that the driver gets detained, respectively.<sup>32</sup> Assumption C says that effort increases joint profit less so than it increases the risk of being detained. Assumption D ensures it is not optimal to buy protection in one period. Proofs in Appendix, Section A5.

**Proposition 1** *If  $1 + \delta < \frac{F + y^1(1 - k)}{y^1 - p^0 y^0 - f + B(1 - p^0)}$  [short time horizon], in period 0, the driver does not buy protection nor provide effort; joint profit and profit are  $S = p^0 y^0$ ,  $\pi = p^0 y^0 - (1 - p^0)B(1 -$*

---

<sup>30</sup>Think of  $e$  as time negotiating with passengers to get larger price. We endogenize the yield under a bargaining context between the driver and the passenger in Appendix, Section A5.

<sup>31</sup>Parameter  $k$  captures the opportunity cost of time negotiating for the purchase of protection: both parties have private information so the purchase requires time. We assume protection is sold as a two-part tariff, consistent with the setting. We assume there is no commitment problem on the side of the officer.

<sup>32</sup>For example, effort or time spent negotiating exposes the driver to risk.

$p^0$ ). Otherwise [long time horizon]: in period 0, the driver buys protection (negotiates), provides effort and joint profit and profit are  $S = ky^1$ ,  $\pi = ky^1 - f$ ; in period 1, the driver has protection (does not negotiate) and provides effort, joint profit and profit are  $S = y^1$   $\pi = y^1 - f$ .

Proposition 1 says that, in period 0, the driver buys protection, and provides effort, if and only if the time horizon is large. This has the following implications. Consider a driver driving in a “home” line in our setting. By definition, the driver is in period 1 of a long horizon, hence has purchased protection and provides effort.

Rerouting to a foreign line has an unambiguous effect on joint profit. If rerouting is long horizon, the joint profit decreases from  $y^1$  to  $ky^1$ . If rerouting is short horizon, the joint profit decreases from  $y^1$  to  $p^0y^1$ . Hence, irrespective of the time horizon, rerouting decreases joint profit. The effect of rerouting on effort, detainments and time waste depends on the time horizon of the rerouting. There are two cases.

[*rerouting with Short Time Horizon*] Buying new protection is not profitable. Without protection, the driver does not provide effort to reduce the risk of being detained, which would reduce joint profit and lead to a bribe payment. This reduces the joint profit. The fraction of detainments increases from zero to  $1 - p^0$ . The effect on total bribe cost is ambiguous: if  $1 - p^0$  is sufficiently low, total bribe cost decreases, otherwise it increases.

[*rerouting with Long Time Horizon*] Buying new protection is profitable. With protection, the driver provides effort, hence probability of detention remains at zero. However, buying protection costs  $F + f$  in period 0 and  $f$  in period 1, and reduces the joint profit by  $(1 - k)y^1$  in period 0: time is wasted negotiating for protection. Empirically, we cannot distinguish  $B$  from  $F$ , but we can distinguish those from  $f$ .

## B. Effect of Rerouting on Joint Profit, by Time Horizon of rerouting

We just showed that, if relationships drive the effect of rerouting, short-term rerouting should decrease the joint profit per trip, while long-term rerouting should decrease the number of trips due to time waste. 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 IV presents the estimates from Equation 1 for time waste and number of trips (Panel A), and joint profit per trip decomposition (Panel B). The analysis in Panel A suggests that that rerouting to a foreign line *only* decreases the number of trips per day

(Columns 1 and 2) if the driver has a long-term driving horizon (from 8.3 by more than one), but it has no effect of rerouting is short-term. Rerouting also decreases the number of negotiations per trip, and the total time wasted, but again only in the long time-horizon (Columns 3-6). These results indicate that the driver takes time-costly actions on the first day of a long-term rerouting block in order to create new ties with and have protection from the police officers in the new route.<sup>33</sup> Columns (9) and (10) show that while rerouting in the short-term causes a significant decrease in bribe costs (arising from avoiding interaction to lower exposure to police officers, even when the model already predicts a low probability of detection). In contrast, when drivers have a sufficiently long horizon, bribe costs significantly increase, consistent with the drivers negotiating and making a payment to build a relationship. The table also shows that, for time waste, the difference between the short- and the long-term rerouting is also statistically significant in relation to the number of interactions without negotiation and bribe costs. In addition, while the difference between the two is not statistically significant for number of trips per day and number of negotiations, it remains clear that long-term rerouting has an economically significant effect on trips per day and on number of negotiations, while the short-term rerouting coefficient is neither economically nor statistically significant. Added to the fact that sample size is similar in short- and long-term rerouting, and coupled with the tendency for these effects to be statistically significantly different for both outcomes, we conjecture that weak statistical power might impede detecting that long-term and short-term rerouting is statistically significant.

Panel B shows that the joint profit per trip decreases both in long- and short-term rerouting. The effect is of similar magnitude, although it is only statistically significant for long-term rerouting (Columns 1 and 2). The channels of this increase, however, are telling of the role of relationships in the effect of rerouting. While long-term rerouting causes drivers to lose passengers, short-term rerouting does not. These results reveal passengers' unwillingness to partake in drivers' wasteful actions in building relationships

---

<sup>33</sup>The effect in short-term rerouting is driven by the time spent in amicable conversations and in negotiation bribes, for which the effect is large and significant, although it is also large among the toll fee payments (see Figure A12, Panel A). It is consistent with drivers strategically avoiding interaction with the police in order to avoid paying a bribe and wasting time due to the absence of relationships with the police. The effect in long-term rerouting is driven by an increase in time negotiating for negotiated bribes (see Figure A12, Panel B).

**Table IV:** Effect of Rerouting to Foreign Lines on Joint Profit and its Components, Decomposed by Time Horizon of rerouting

*A. Number of trips (and actions that affect it)*

	# Trips per day		# Negotiation		Time waste		# No negotiation		Bribe costs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Foreign	-0.271 (1.056)	-1.038*** (0.366)	0.001 (0.058)	0.075** (0.032)	-0.377 (0.244)	1.053* (0.537)	-0.034 (0.098)	0.176** (0.072)	-0.448** (0.223)	0.549** (0.217)
Mean Dep. Var.	8.3	8.3	0.1	0.1	1.2	1.2	0.8	0.8	0.9	0.9
Observations	124	167	1,018	1,334	1,018	1,334	1,018	1,334	1,018	1,334
R <sup>2</sup>	0.326	0.246	0.123	0.090	0.111	0.037	0.393	0.388	0.069	0.037
Time Horizon	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long
H <sub>0</sub> p-val.	0.571			0.232		0.011		0.055		0.001

*B. Joint profit per trip (and actions that affect it)*

	Joint Profit		# Passenger		Price		Operating cost		Trip duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Foreign	-1.052 (1.028)	-0.970* (0.520)	0.264 (0.260)	-0.722*** (0.252)	-0.062** (0.029)	-0.021* (0.012)	0.001 (1.079)	0.299 (0.509)	-0.315 (4.745)	1.705 (2.126)
Mean Dep. Var.	3.0	3.0	18.4	18.4	0.5	0.5	5.8	5.8	31.8	31.8
Observations	1,014	1,326	1,014	1,330	1,015	1,327	1,018	1,334	1,015	1,331
R <sup>2</sup>	0.210	0.168	0.806	0.735	0.796	0.408	0.142	0.094	0.268	0.146
Time Horizon	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long
H <sub>0</sub> p-val.	0.942			0.004		0.167		0.819		0.809

Notes: This table presents the OLS estimates of  $\beta^F$  from Equation 1, 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  $\beta^F$  separately for the sample in short-term rerouting (odd columns) and long-term rerouting (even columns). *Dependent variables:* In Panel A, the dependent variables are the number of trips per day (columns 1 and 2), in which case the sample is at the driver-day level. The dependent variables in the remaining columns, which include the sample 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), the number of events in which the driver makes a bribe payment without any negotiation, which are composed of the “*mbote ya likasu*” and tip (Columns 7 and 8), and the total bribe costs for the trip (Columns 9 and 10). In Panel B, the dependent variables are the total joint profit per trip, computed as the revenue from passenger tickets minus gas and repair costs (Columns 1 and 2). Gas and repair costs (operating costs) are observed at the daily level and imputed at the trip level. The dependent variables in the remaining columns, which also include the sample 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 (Columns 5 and 6), the the operating costs, 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 (Columns 9 and 10). All regressions include randomization block 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. Because the assignment to home vs. foreign was made at the level of the driver’s driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata. “Mean Dep. Var.” provides the mean of the dependent variable in the corresponding column computed in the full sample of home line driving. Table notes denoted “H<sub>0</sub> p-val.” present the p-value from the interaction term in the regression of outcome variables on foreign line and time horizon assignments. It is the p-value for the test of whether short- and long-term rerouting have the same effect.

with the police. Even more revealing of the passenger-driver interaction is the driver's bargaining power in price negotiations when he is in a rush. Columns (5) and (6) show that short-term rerouting causes a decrease in the price per passenger, while the effect in long-term rerouting is three times smaller and marginally significant.

Taken together, our results suggest that the absence of relationships with the police weaken driver's bargaining position with passengers when negotiating prices, decreasing prices and joint profit. On the first day of foreign line driving when the driver had a short-time horizon, the driver fails to take productive actions in order to avoid the increasing risk of being noticed and detained. On the first day of foreign line driving when the driver had a long-time horizon, the reduction of joint profit comes along with more time waste and negotiating with police, and a reduction in the number of trips per day, consistent with the driver investing in a relationship.

## 8. “Through these Discussions, We Become Friends!” Social and Economic Returns on Initial Investment in New Social Ties

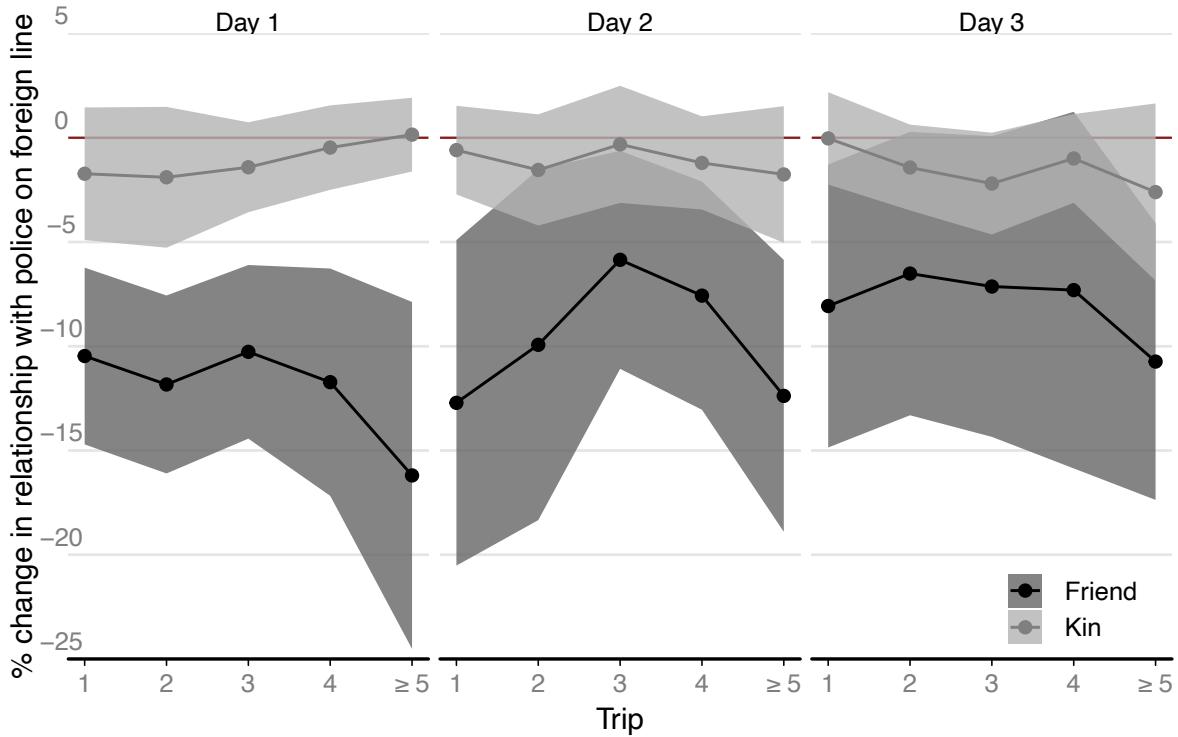
The previous section has demonstrably indicated the harmful effect of rerouting, which reduces the prevalence of relationships and joint profit. Moreover, in long-term rerouting, the destruction of joint profit came about through an increase in time waste. We conjectured that this time waste accrues from the driver spending time negotiating with the officers for payments that create new relationships. If this is true, then we should see new relationships forming on subsequent days.

In this section, we verify such an interpretation for the long-term rerouting joint profit destruction. To analyze the dynamics of the interaction with the police, we separately estimate the following equation for each day of the three-day rerouting blocks:

$$Y_{irdt} = \beta^F \mathbb{1}_1(\text{Foreign}_{irdt}) + \sum_{r=1}^5 (\nu_r * \beta^F \mathbb{1}_1(\text{Foreign}_{irdt})) + \alpha_d + \epsilon_{irdt}, \quad (2)$$

where the dependent variable  $Y_{irdt}$  is an indicator taking value 1 if the driver has a social relationship the police agent with whom he interacts, and the independent variable  $\mathbb{1}_1(\text{Foreign}_{irdt})$  is an indicator variable taking value 1 if observation indexed  $irdt$  is assigned to a Foreign rerouting, and zero otherwise.  $\alpha_d$  is the randomization block fixed

**Figure 4:** “Through these discussions, we become friends!” Social Return on Investment



*Notes:* This figure presents the coefficient sizes and their 95% confidence intervals of the estimates, for each round (trip)  $r$  within each of the three days of the driving blocks, of  $\beta^F + \nu_r + \nu_r \beta^F$  of Equation 2. 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 sample includes all observations on the home line. All regressions include randomization block 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. Because the assignment to home vs. foreign was made at the level of the driver’s driving block, each minibus driving block is a randomization cluster. Since there are, by construction 13, clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 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.

effects. In addition,  $\nu_r$  captures the trip fixed effects, so the sum with the interaction term  $\beta^F + \nu_r + \nu_r \beta^F$  captures the population average treatment effect on kinship/friendship from rerouting to a foreign line on trip  $r$  on each day of the three-day driving blocks.

Figure 4 presents the estimates for the effect on the indicator for whether the driver has a relationship with the officers. It presents the analysis per day. On day one, the driver and the officers are much less likely to be friends. On day 2, the effect is preserved, although the difference is smaller in the middle of the day. By day 3, the difference is smaller. There is no upward trend within the day, suggesting that the relationships are formed from one day to the next rather than within a day.

It is possible that the increase in friendship captures compositional changes in the police teams rather than a change in the nature of the relationship between them and the

driver. Therefore, as a placebo, we also included the existence of kinship as a dependent variable, because unlike friendship, kinship cannot be newly created. In contrast to friendship, we find no effect of rerouting on kinship.<sup>34</sup>

We have shown that the costly time waste negotiating bribes on the first day of a long-term rerouting is associated with the subsequent creation of new relationships. We then jointly analyze the evolution of the number of trips per day and the bribe negotiations per day, marks of the conjectured investment in relationships, to see if driver profits increase.

Figure 5 presents the estimates for the effect of long-horizon rerouting on the number of trips per day (Panel A), bribe negotiations per day (Panel B), and driver profits per day (Panel C). On day 1, the driver makes a significantly lower number of trips than on home lines. On day 2, however, the effect is smaller, and it is no longer statistically significant. By day 3, the numbers of trips on home and foreign lines are indistinguishable, and the coefficient is virtually zero. This is consistent with the cost of the investment in new relationships being incurred predominantly on the first and second days, enabling the protection required to make as many trips as on home by the third day.

In addition, we see a larger fraction of bribes negotiations on the first day. The difference is marginally significant. By day 3, the difference is negative, and not statistically significant. The number of negotiations converge to that on home lines.

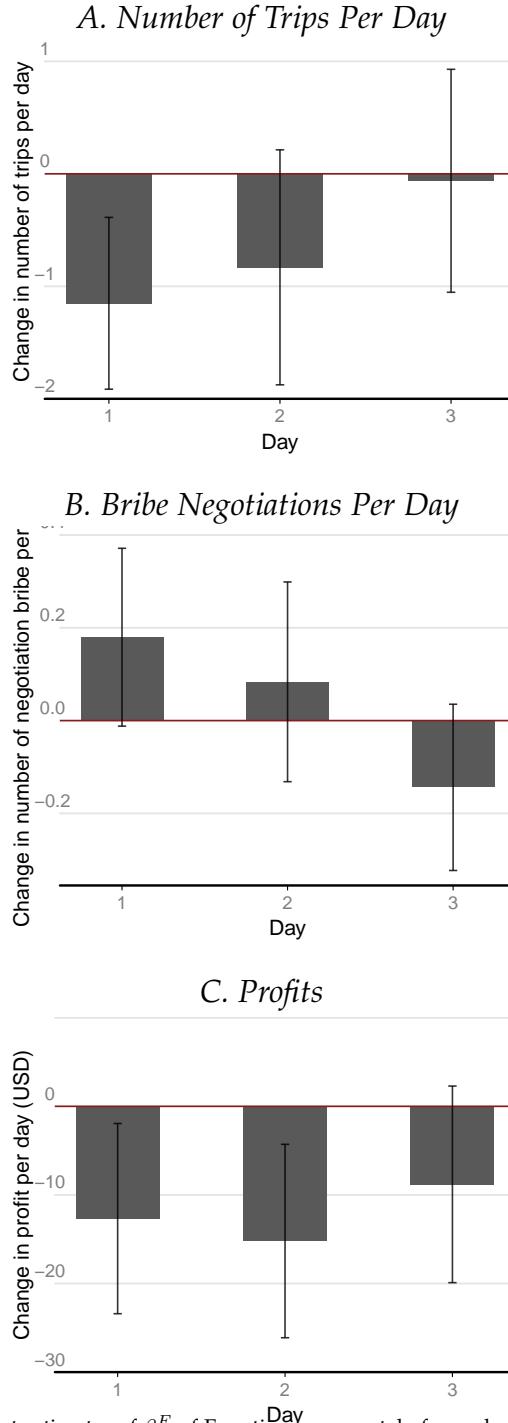
Finally, the profit of the drivers is significantly lower on day 1. It is also significantly lower on day 2. However, by day 3, the reduction in the profits compared to home lines has been halved, and the difference is not statistically significant. Again, profits converge to the home line levels by day three, consistent with the interpretation that drivers incur the main cost of relationship creation on the first two days.

One way to assess the formation of new relationships would have been to track the drivers for a longer duration after the study and measure whether they started driving in the new lines. Due to budget limitations, we did not follow drivers after the study was ended. However, we conducted three exit surveys with the research staff and with the drivers in the following years (in 2016 and 2017) as well as in 2023, in order to assess, if only informally, the long-term impact of rerouting and the endurance of relationships. The qualitative interviews confirm that drivers were actively building

---

<sup>34</sup>Figure A14 presents the analysis per day. The conclusion is identical.

**Figure 5:** Economic Return on Investment in new Social Ties



*Notes:* This figure presents the coefficient estimates of  $\beta^F$  of Equation 4, separately for each day of rerouting for long-term rerouting to foreign lines (rerouting to drive in 3 contiguous days on lines in which the driver does not have sufficient pre-existing relationships with the officers to drive through). The coefficients, represented as the length of the bars, thus are interpreted as how much larger is a given variable in long-term rerouting than on home lines for that specific day of long-term rerouting. The bars also include the corresponding 95% confidence intervals represented as brackets. In Panel A, the dependent variable is the number of trips per day. It is obtained in the sample of driver-days as the number of times a driver completes a return trip per day. In Panel B, the dependent variable is the total number of negotiation bribes (excludes the events in which the driver pays a bribe without any negotiation, which are *mbote ya likasu* and tips). In Panel C, the dependent variable is the profits from driving per day. Profits in USD-day<sup>-1</sup> are computed as the joint profit per day minus the bribe costs per day. Bribe costs per day is the sum of all the payments from a driver to the police across all intersections in all trips in a day. Joint profits per day is the sum of all the revenue from passenger tickets along a trip minus operating cost in a day. All regressions include randomization block 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. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata.

valuable relationships in new lines. First, as reported by project implementers:

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 4<sup>th</sup> 2015)

Second, the drivers, too, suggested these relationships were a lasting investment:

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].

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)

In summary, in this section, we have provided evidence that rerouting reduces joint profit in ways that are consistent with its explanation through the absence of relationships. We conjectured, and then validated, that these time and money investments are associated with the creation of new relationships, which allow the drivers to attain the same level of profits as that on home lines by the third day.

## 9. Implications for Consumer vs. Producer Surplus

In this section, we discuss where the value of these relationships for the joint profit comes about. Drivers and officers are the producers, and passengers are the consumers. In this context, passengers often do not have many outside options, because the time between minibuses of the same line is often long: thus drivers always exercise some monopolistic

power to extract passenger surplus through the price negotiation. While they do not all the power, variations in drivers' bargaining power thus could influence the prices. Driver-police relationships could increase producer surplus by shifting the supply curve downward (reducing the marginal cost), shifting the demand curve upwards (increasing the quality of a trip), or by influencing the allocation of consumer surplus between consumers and producers (if they increase the bargaining power of the drivers). The first two cases would imply that driver-officer relationships increase the total surplus, while the last case implies a relocation of consumer into producer surplus.

In Table V, we analyze the effect of short-term unprotected rerouting, which isolates the role of social relationships and allows us to exclude the interference created by the incentives to create new relationships (such as in long-run rerouting) and that from being protected. We analyze various outcomes to discuss how social ties increase producer surplus. In Panel A, we simply examine whether social ties increase producer surplus, aiming to quantify the role of these ties on producer surplus without interfering factors. In Panel B, we analyze various outcomes to test various hypotheses about its origin.

In Panel A, the outcome is joint profit, which as is clear by now, is the producer surplus. Unprotected short-term rerouting leads joint profit per day to fall from 24.22 USD to 9.5 USD (Column 1), a more than 60% reduction. Columns (3) and (5) show that this reduction arises from a reduction in the joint profit per trip. None of these effects are present when the driver is protected (Columns 2, 4, 6). Thus, this analysis confirms that rerouting decreases producer surplus by altering the profitability of each trip and through varying the social ties between the driver and the officers on the line rather than other idiosyncratic features of rerouting. Thus, in what follows, we focus on explaining the sources of this increase in producer surplus per trip driven.

In Panel B, we test various hypotheses for the effect of social ties on the producer surplus per trip. First, we examine supply shifters. In Column (1), we test whether social ties decrease the marginal cost of a trip. This might happen, if relationships with the officers on a line allow drivers to reduce the cost of gas or repairs. Yet, operating costs are unaffected by short-term unprotected rerouting. Column (2) finds the same when the driver is protected. Thus, driver-officer ties have no effect on the marginal cost of a trip.

Second, we examine demand shifters. In Column (3), we test whether social ties

**Table V:** Short-Term Unprotected Rerouting and Producer vs. Consumer Surplus

*A. Producer Surplus: the Joint Profit*

	Joint Profit (per day)		Number of trips (per day)		Joint Profit (per trip)	
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign	−14.970*	8.905	−0.254	−0.807	−2.377**	0.640
	(8.154)	(18.918)	(1.261)	(1.813)	(0.965)	(1.922)
Mean Dep. Var.	24.22	20.46	8.4	8.13	3.25	2.77
Observations	55	63	58	66	485	529
R <sup>2</sup>	0.561	0.405	0.373	0.451	0.402	0.240
Protection	NO	YES	NO	YES	NO	YES
H <sub>0</sub> p-val.		0.303		0.791		0.418

*B. Shifter of Producer Surplus: Marginal Cost, Demand, or Bargaining Power?*

	Operating Costs		# Passengers		Trip duration		Price	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Foreign	0.391	−0.021	0.058	0.570	−2.841	6.415	−0.105***	0.015
	(1.111)	(2.142)	(0.222)	(0.451)	(6.127)	(5.471)	(0.027)	(0.013)
Mean Dep. Var.	5.52	6.06	18.2	18.5	30.99	32.56	0.48	0.48
Observations	487	531	485	529	484	531	485	530
R <sup>2</sup>	0.402	0.134	0.824	0.794	0.229	0.330	0.849	0.891
Protection	NO	YES	NO	YES	NO	YES	NO	YES
H <sub>0</sub> p-val.		0.691		0.574		0.251		0

*Notes:* This table presents the OLS estimates of  $\beta^F$  from Equation 1, decomposed by whether the driver-day was assigned to short-term rerouting (driving blocks of only 1 day), and by whether the driver-day was assigned to benefit from 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), the joint profit per trip (columns 3 and 4), are computed as the revenue from passenger tickets minus gas and repair costs at the driver-trip level; in Panel B, the dependent variables in the remaining columns, all include the sample at the driver-trip level and are: the the operating costs, which are composed of the gas costs and repair costs observed at the driver-day level and imputed at the driver-trip level (Columns 1 and 2), the number of passengers that purchase a ticket for the ride (Columns 3 and 4), the duration of a trip (Columns 5 and 6), and the average price paid by passengers on a trip (Columns 7 and 8). All regressions include randomization block 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. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full sample of home line driving. Table notes denoted "H<sub>0</sub> p-val." present the p-value from the interaction term in the regression of outcome variables on foreign line and third party protection assignments. It is the p-value for the test of whether unprotected and protected rerouting have the same effect.

increase passenger demand. This might happen, for example, if passengers are *both* able to identify whether drivers have many social ties *and* to internalize it; in that case, demand for drivers with social ties might be larger than for other drivers. Yet, Column (3)

shows that the number of passengers is unaffected by short-term unprotected rerouting. Since rerouting leaves the number of passengers unaffected, it is unlikely that social ties increase demand by the passengers.

However, there are various ways in which the number of sales could be unaffected in short-term unprotected rerouting while it still being true that social ties might increase demand. One possibility is that, perhaps, social ties increase passenger demand, but that this effect is confounded by other aspects inherent to re-routing. For example, passengers might be willing to pay less for a driver without ties but, at the same time, they might expect that a rerouted driver has other valuable properties such as being more flexible, leading the number of sales to be similar between home and foreign line driving. In other terms, short-term rerouting is a bundle that includes other dimensions of rerouting. To test whether other aspects of rerouting, other than social ties, influence passenger demand, Column (4) examines the effect of short-term unprotected rerouting as a placebo. The effect is the same; thus, the absence of an 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. Another possibility is that, even if social ties do not increase sales, they might still increase passenger demand. This might happen if the driver is able to pass through the entirety of the increase in value of driving to the passengers, which would be the case if demand is very inelastic. Social ties between drivers and officers could be valuable to the passengers if they allow the trip to go smoother, without interruptions with the officers, hence trip duration is a good proxy for the passengers' valuation.<sup>35</sup> To test whether social ties increase the value of a trip to the passengers, Column (4) measures the effect of short-term rerouting on the major dimension that passengers value of a trip in this context: trip duration. We find that short-term unprotected rerouting has no effect on trip duration. This suggests that social ties between the driver and the officers do not allow the driver to make shorter trips.

---

<sup>35</sup>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, even if they were significantly less safe, were known to be faster.

Yet, since short-term rerouting is a bundle, a remaining possibility is that drivers might have some speed advantage from driving in a foreign line that is unrelated to whether they have social ties with the officers, and that this advantage coexists with a slowing down effect of not having social ties. Column (5) tests for this possibility and serves as a placebo: short-term protected rerouting has the same effect. Thus, it is safe to conclude that driver-officer social ties have no bearing on the duration of a trip, and hence, on the value of a trip to the passengers.

Third, we examine whether, instead of these two productive explanations, the increase in the producer surplus from driver-officer ties comes at the expense of the consumer surplus. To test for this possibility, Column (7) analyzes the effect of short-term unprotected rerouting on the average price paid by passengers. We find that the average passenger price falls from .85 to .74 cents, a 12.5% statistically significant reduction. This suggests that drivers who have no social ties are less able to extract the consumer surplus through negotiating over the price. One concern with this analysis is that this effect might reflect other features of rerouting than social ties with the police officers. For example, it could be that drivers who are on a foreign line are less able to identify the best consumers because they lack line-specific skills and information (those with a highest willingness to pay). If that were true, this effect should arise independently of whether the driver is protected, as this would not affect their line skills. To test for this possibility, Column (8) analyzes the effect of short-term protected rerouting. We find that there is no effect whatsoever on the price in that case, and the p-value of the difference in these two effects is 0. Thus, this suggests that drivers who have social ties with the officers on the line are better able to extract consumer surplus by negotiating over the price.

Two important theoretical considerations are in order to interpret these tests. One is that this pattern 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 (often it is a sticker, sometimes the availability of a phone call, and otherwise it is an agent that has no better knowledge of the road than the driver). Another consideration is that, taken together, constant sales and a decrease in the price would be consistent with an upward shift in passenger

demand in a perfectly competitive market. However, this is unlikely for three reasons. First, this requires that passengers are able to observe the fraction of intersections of the line on which a driver has social ties prior to choosing whether to enter the bus, but this is very unlikely, because passengers rarely know the drivers. Second, even if the passengers were able to recognize some drivers as a regular on the line and thus condition their demand on whether they recognize the driver, it is unlikely that passengers can price the third-party protection we provided—yet, we do find a starkly significant effect on the price by whether rerouted drivers have third-party protection. Indeed, recall that protection is a bundle of high ranking officers who are absent or an agent in the bus and of the ACCO sticker; only the ACCO sticker might be visibly interpretable to the passengers. Yet Tables A10 and A11 showed that the reduction in joint profit per trip is identical with ACCO sticker (visible) and an agent in the minibus (not really visible). This is inconsistent with the passengers being able to price in even the visible social ties or third-party protection differently. Third, passengers have very little agency in choosing what minibus they take. At the terminii, charging of minibuses is organized by ACCO agents, who are not state officials, following the order of minibus arrival. Along the line, the gap between minibuses can be large, thus passengers have very little choice than to take the first minibus that shows up. Furthermore, the minibuses have the same look, age, and make, contributing to the perception that all minibuses are the same.

One explanation for our results is that social ties between drivers and officers might allow the drivers to bargain a larger price from the passengers. Given the structure of bargaining, demand does not need to be responsive to small one-off deviations in the price because of monopolistic competition between the drivers. The intuition is also supported by our qualitative evidence: drivers who do not have social ties with the officers negotiate for the price in a rush, because they know the time without driving and negotiating exposes them to being detained by an officer on the line, which can lead to long negotiations during which they make no revenue, and could lead to the permanent loss of the vehicle. Consistent with standard bargaining theory, this leads to a reduction in their bargaining power over the passengers, resulting in a lower equilibrium price. This explanation has a lot of support from the qualitative interviews.

This explanation presupposes that the set of passengers negotiating with the minibus

is given. In reality, it is possible that the set of passengers is endogenous/self-selected, if the driver can choose which locations to select in order to stop and charge new passengers. It is possible that, without social ties with the officers, the drivers find it too risky to stop to charge passengers at the locations that concentrate the passengers with the highest willingness to pay, if those locations are also those with the highest risk of being seen and detained by an officer. For example, the terminii might be too risky for the drivers to charge passengers, because they entail a long wait, the minibus cannot escape, and police officers might sometimes also come. If passengers at the terminii have a higher willingness to pay, then our results could reflect selection of passengers. For example, this could happen if passengers with long rides might be more concentrated at the terminii and if terminii have police officers. We have no way to rule out this possibility based on the empirical evidence we have presented. However, we do not see a decrease in the visits to the terminii in our data, suggesting that this is not taking place. We also do not have qualitative evidence to support this selection mechanism. While our evidence against this selection mechanisms is weak at best, it remains an important avenue for future research with important implications for welfare.

In sum, this analysis suggests that relationships between drivers and police officers increase producer surplus at the expense of consumer surplus. This is consistent with the following interpretation for the transaction costs inherent in corruption: the opportunity cost of revenue lost in negotiations (and if the vehicle is permanently apprehended); to avoid incurring this cost, drivers negotiate with lower patience, decreasing their bargaining power over the social surplus from driving. This finding is important, as it implies that private actors and officers might create social relationships to jointly extract consumer surplus; these relationships would facilitate the spread of corruption by making corrupt exchange more effective, but they do not increase total economic surplus.

## 10. Conclusion

Leveraging relationships that we had built over three years of qualitative research inside the public transport sector in Kinshasa, DRC's capital, we analyze the role of relationships between private operators and state official enforcers on the governance of corrupt transactions. We empirically document the prevalence of a nonmarket social institution,

bilateral relationships, to govern the corrupt transaction and, using an experiment, we quantify its effect on the (shared) cost of corrupt exchange. We found that the transaction costs arising from negotiating corrupt transactions can be huge, and that, when drivers have the cash, relationships form relatively quickly to mitigate these costs.

Our findings document that, in the absence of social relationships between the driver and the police, corruption’s transaction costs arise from the fact that drivers, anticipating lengthy and costly negotiations with the police, refrain from taking productive actions so as to avoid being detected by the officers, hence they can attract less passengers and negotiate a smaller price. Our qualitative evidence suggested that these actions might have included spending enough time searching for and calling passengers or negotiating with passengers for the price of their ride. The absence of relationships thus reduces the joint profit significantly as a direct result of the fact that there are otherwise no institutions to govern the corrupt transaction between the officers and the drivers. With relationships, both are able to achieve a larger joint profit, and share the joint profit through transfers such as bribes. Interestingly, we find that the driver-officer relationship comes at the expense of the passengers and that the larger “production surplus” that we document comes, in part, from colluding to better extract passenger surplus through drivers’ bargaining power over passengers. The relationships are very much a second-best solution, in that a system whereby drivers pay a centralized licensing fee to use the roads, and then optimize their routes, would be more efficient if it could be implemented.

It is impossible to know how representative the particular scheme we observe is. To the extent that we were able to gain access into the system, and given how open its participants spoke about the arrangements, our setting is particularly well-suited for studying the role of governance of corrupt exchange, as it is unlikely that the need to conceal played a role in the shape of these arrangements. Obtaining parallel data for other organizations remains an important challenge. Furthermore, the costs we document emerge under quite general conditions, rather than being a peculiar artifact of our specific setting. We have discussed how, even if the costly negotiation is also what induces compliance among drivers to pay the toll fee once they are in that arrangement, it is also the benchmark default behavior for officers to generate income, and is therefore self-enforcing. We have also discussed how the seemingly peculiar “buying the land”

procedure to enter the cooperative relationship can be rationalized, and has been theoretically already, in settings of bilateral repeated interaction in which new relationships may form in the equilibrium path. It appears a quite general feature of problems with similar features as the ones we study.

Our study has omitted from the explanation two fundamental aspects of this context of relevance for economics and social science generally. First, the interactions between the various actors are governed by established norms of behaviour. Thus, the seemingly bilateral relationship that we describe draws on a collective equilibrium, and thus belief system, that obviously informs the selection of the types of relational contracts that emerge between two parties—mixing economic and sociologist terminology, a “portfolio” of culturally acceptable or expected actions available for bilateral relationships, endogenously determined in a multi-lateral equilibrium. Indeed, provided each actor sticks to the known rule of the game, little bilateral trust needs to be built for such contracts to be sustained. Our omission is exemplified in the simple model, where we impose by assumption that there is no commitment to be solved on the side of the officer, so that we focus on studying how drivers behave strategically in this environment. Yet, it is precisely how society has enabled the police officers to credibly commit to their side of the deal, through the emergence of norms and expected heuristics, which is inherent in the notion of “social embededness” (Polanyi, 2018) that we allude to at the opening of this paper, and which constitutes, in our view, a more interesting question for social science emerging from this setting. These bilateral interactions might produce different patterns of behavior depending on the social context on which they are “embedded.” Answering this question would require observing social equilibria in a longer time period, or ingenious experimental study designs within the equilibrium to isolate the role of these norms. 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: a meaning of social identity shared by the participants; separate spheres of economic exchange; the psychological significance of friendship and social relationships beyond their cynical

reduction to their material consequences. These remain promising areas of research in economics of corruption—and are inhererent in the notion of “social embeddedness.”

## References

- Banerjee, A., S. Mullainathan, and R. Hanna (2012). Corruption. In *The Handbook of Organizational Economics*, pp. 1109–1147. Princeton University Press.
- Bertrand, M., S. Djankov, R. Hanna, and S. Mullainathan (2007). Obtaining a driver’s license in india: an experimental approach to studying corruption. *The Quarterly Journal of Economics* 122(4), 1639–1676.
- De Herdt, T. and K. Titeca (2019). *Negotiating public services in the Congo: State, society and governance*. Bloomsbury Publishing.
- Dixit, A. (2004). *Lawlessness and Economics: Alternative Modes of Governance*. Princeton, New Jersey: Princeton University Press.
- Fisman, R. (2001). Estimating the value of political connections. *The American Economic Review* 91(4), 1095–1102.
- Fisman, R. and Y. Wang (2015, 05). The Mortality Cost of Political Connections. *The Review of Economic Studies* 82(4), 1346–1382.
- Granovetter, M. (1985). Economic action and social structure: The problem of embeddedness. *American Journal of Sociology* 91(3), 481–510.
- Greif, A. (2000). The fundamental problem of exchange: A research agenda in historical institutional analysis. *European Review of Economic History* 4(3), 251–284.
- Greif, A. (2008). Towards a political economy of implementation. In E. Helpman (Ed.), *Institutions and economic performance*, pp. 17–63. Harvard Univ. Press.
- Kranton, R. E. (1996). The formation of cooperative relationships. *Journal of Law, Economics, Organization* 12(1), 214–233.
- Macchiavello, R. and A. Morjaria (2020, 12). Competition and Relational Contracts in the Rwanda Coffee Chain\*. *The Quarterly Journal of Economics* 136(2), 1089–1143.
- Olken, B. A. and P. Barron (2009). The simple economics of extortion: Evidence from trucking in aceh. *Journal of Political Economy* 117(3), 417–452.
- Polanyi, K. (1944). *The Great Transformation: The Political and Economic Origins of Our Time*. Boston: Beacon Press.
- Polanyi, K. (2018). The economy as instituted process. In *The sociology of economic life*, pp. 3–21. Routledge.

- Rose-Ackerman, S. (1999). *Corruption and Government: Causes, Consequences, and Reform*. Cambridge: CUP.
- Shleifer, A. and R. W. Vishny (1993). Corruption. *Quarterly Journal of Economics* 108(3), 599–617.
- Tchanche, B. (2019). A view of road transport in africa. *African Journal of Environmental Science and Technology* 13, 296–302.
- Titeca, K. and A. Malukisa (2019). Real taxation practices among the traffic police. *unpublished paper*.

## **Appendix: Measurement and Experimental Procedure**

We now describe the data collection, outcomes, and basic characteristics of the sample.

### **A1. Data collection**

To collect our data, we hired one data collector for each minibus driver. Every day, data collectors were dispatched to the minibuses under the supervision of a data collector monitor. In each minibus, we purchased a ticket for the whole day close to the driver, which enabled the data collector to observe closely and listen in to the conversations of the driver with the police. A crucial aspect of this design is that the collectors had developed good relations with the minibus drivers, which ensured that the driver did not have incentives to act in a different way than usual in the presence of the observer.

We bought smartphones, installed SurveyCTO, and programmed, for each route, and for each trip direction, a survey that prompted the data collector to each intersection in the route. A minibus line is composed of a sequence of intersections.<sup>36</sup> The data collectors entered information for each expected intersection prompted by the app, and completed a full survey with information about the trip at the end of each round, in addition to the information collected per intersection.

In addition to the lump-sum compensation for the risk, drivers were compensated at a rate of 15 USD per day for assisting the data collectors who travelled with them in the minibus and for answering questions during their idle time at the termini.

### **A2. Outcome Data**

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

---

<sup>36</sup>The data collectors were the same individuals as the implementers ensuring compliance to the calendar. Overall, we hired one data collector for each minibus, four monitors of data collectors, one data coordinator whose role was to track the quality of the data in real time from a secure location in Kinshasa, and one operations' coordinator whose role was to ensure the safety of all participants.

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 to their operations 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.<sup>37</sup> Observers knew they were being monitored each day.<sup>38</sup>

### A3. 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.

---

<sup>37</sup>We recorded only one event in which the data collector was detained for interrogation related to their activity. In that instance, the data collector was drunk, and had caused trouble with secret service agents travelling in the minibus. He was released within minutes without payment of any bribe.

<sup>38</sup>In addition to this data, we tracked the events of seizing of a minibus as follows. Drivers were instructed to call the study data coordinator every time that a minibus was seized. That call was sometimes placed from the driver's own phone, sometimes from the data collector's phone. In that call, the data coordinator asked the following questions: month, day, name of driver, code of minibus, home line of the driver, route on which the seizing took place, exact hour of seizing, a series of questions verifying the treatment assignment, whether the seizing resulted in escorting to the police station, initial money requested by the police, whether a bribe was paid and how much, details on the interaction, duration of the whole seizing process. The data coordinator filled a tracking document in Excel with the answers to each of these questions for each seizing event. Overall, the tracking of seizing events recorded 38 seizing events in the study period. We link the two sources of data for this variable.

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.

### A. *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 10 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 B).

We sampled these blocks without replacement, totaling the number of experimental days. Table A3 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  $J = \{1, 2, \dots, 9\}$  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<sup>39</sup> (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.

### **B. 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 A3. 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.

### **C. 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 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 A3) 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.

---

<sup>39</sup>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.

## A4. Accounting for Line Imbalance in Foreign vs. Home Sets

In this section, we first demonstrate that in failing to account for the composition of foreign and home lines in the inverse propensity weights, a composition bias is created. We then show, for each route, the probability that the route is part of a home or a foreign treatment observation, as well as the conditional probability that an observation is foreign, conditional on being a given route.

Consider a minibus  $d$  on day  $t$  that was randomly assigned to reroute to a line  $L_{dt}(D_{dt} = 1) \in \{0,1\}$ , where importantly a foreign line  $L_{dt} = 1$  is a function of randomized rerouting  $D_{dt} = 1$  for minibus  $d$  on day  $t$ . Let  $Y_{dt}(D_{dt})$  denote the corresponding potential outcome under randomized assignment. Route randomization introduces an assignment bias because a route has differential probability of being foreign to different minibuses and thus minibus-days. In the bias derivation below, we omit subscripts  $dt$  and write  $L(1)$  as foreign line randomized rerouting assignment for brevity.

$$\hat{\beta}_{OLS} = [Y|L(1), D = 1] - [Y|L(0), D = 0] \quad (1)$$

$$= \frac{[Y|D = 1]}{\mathbb{P}[L(1)|D = 1]} - \frac{[Y|D = 0]}{\mathbb{P}[L(0)|D = 0]} \quad (2)$$

$$= \frac{[Y|D = 1]}{\mathbb{P}[D = 1|L(1)]} \frac{\mathbb{P}[D = 1]}{\mathbb{P}[L(1)]} - \frac{[Y|D = 0]}{\mathbb{P}[D = 0|L(0)]} \frac{\mathbb{P}[D = 0]}{\mathbb{P}[L(0)]} \quad (3)$$

$$= [Y(1)|D = 1] \frac{\mathbb{P}[D = 1]}{\mathbb{P}[L(1)]} - [Y(0)|D = 0] \frac{\mathbb{P}[D = 0]}{\mathbb{P}[L(0)]} \quad (4)$$

$$= [Y(1)|D = 1] \frac{\mathbb{P}[D = 1]}{\mathbb{P}[L(1)]} - [Y(0)|D = 0] \frac{\mathbb{P}[D = 0]}{\mathbb{P}[L(0)]} \quad (5)$$

$$- [Y(0)|D = 1] \underbrace{\frac{\mathbb{P}[D = 1]}{\mathbb{P}[L(1)]}}_{=0} + [Y(0)|D = 1] \frac{\mathbb{P}[D = 1]}{\mathbb{P}[L(1)]}$$

$$= \frac{1}{\mathbb{P}[L(1)|D = 1]} ([Y(1) - Y(0)|D = 1]) \quad (6)$$

$$- \left( \frac{1}{\mathbb{P}[L(0)|D = 0]} - \frac{1}{\mathbb{P}[L(1)|D = 1]} \right) [Y(0)|D = 0]$$

$$= \underbrace{\frac{1}{\mathbb{P}[L_{it}(1)|D_{it} = 1]}}_{\equiv w_{it}} \left( \underbrace{[Y_{it}(1) - Y_{it}(0)]}_{\equiv ATE} \right) \quad (7)$$

$$+ \underbrace{\left( \frac{1}{\mathbb{P}[L_{it}(1)|D_{it} = 1]} - \frac{1}{\mathbb{P}[L_{it}(0)|D_{it} = 0]} \right)}_{\equiv c} [Y_{it}(0)]$$

where the first line is what OLS estimator identifies; the second line uses the partition theorem; the third line uses Bayes' theorem; the fourth line recognizes that the probability of foreign/home assignment is unity under foreign/home lines due to randomized controlled experiment; the fifth line adds zero akin to selection bias decomposition of ATE; the sixth line factorizes common ATT term, changes the conditional on the average untreated outcome by independence between treatment and potential outcomes due to random assignment, and uses Bayes theorem  $\mathbb{P}[L(1)] = \mathbb{P}[L(1)|D = 1]\mathbb{P}[D = 1]$ <sup>40</sup> to cancel the probability of foreign/home. Final line uses random assignment to switch ATT into ATE. The resulting equation appears similarly to the selection bias of observational studies but weighting the ATE by the inverse probability of foreign route. Weighted least square solves the selection bias.

To obtain these probabilities, we relied on driver's reporting of their foreign lines before revealing the detail of the experiments. Table A7 decomposes the probability that a given line is assigned to foreign for each of the minibuses in the sample. Lines that are *home* for a given minibus can never appear as foreign for that minibus and hence has probability zero. Importantly, the pool of foreign lines varies by minibus and all lines are equally represented as home or foreign line.

## A5. Proofs

*Proof of Proposition 1* We proceed by backward induction. We omit the role of  $k$  for simplicity but it is straightforward to see that it can be added back to produce the result of Proposition 1. If the driver buys protection, his payoff is  $y(e) - F - f + \delta(y(e) - f)$ . In that case, by assumption A:  $e_1 = 1, e_2 = 1$ . Thus expected utility with protection is  $(1 + \delta)(y^1 - f) - F$ . If the driver does not buy protection, his payoff is  $p(e)y(e) - B(1 - p(e)) + \delta(p(e)y(e) - B(1 - p(e)))$ . By assumption C,  $e_1 = 0, e_2 = 0$ . Hence, the driver buys protection in period 1 iff.:

$$(1 + \delta)(y^1 - f) - F > (1 + \delta)(p^0y^0 - B(1 - p^0))$$

This is equivalent to:

$$(1 + \delta) > \frac{F}{y^1 - p^0y^0 - f + B(1 - p^0)}.$$

---

<sup>40</sup>Note that  $\mathbb{P}[D = 1|L(1)] = \mathbb{P}[D = 0|L(0)] = 1$

□

**Proposition 2** [One period] a. If  $y^1 - p^0 y^0 + (1 - p^0)B < F + f$  the driver does not buy protection and provides low effort, and gets utility  $p^0 y^0 - (1 - p^0)B$ ; b. If  $y^1 - p^0 y^0 + (1 - p^0)B > F + f$ , the driver buys protection and provides high effort, and gets utility  $y^1 - F - f$ . Willingness to pay for protection is  $y^1 - p^0 y^0 > 0$ .

*Proof of Proposition 2* We proceed by backward induction.

If the driver buys protection, his payoff is  $y(e) - F - f$ . In that case, chooses to provide productive effort iff.:  $y^1 > y^0$ . This holds true by Assumption A, hence if the driver buys protection, he provides productive effort. Thus, his utility with protection is  $y^1 - F - f$ .

If the driver does not buy protection, his payoff is  $p(e)y(e) - (1 - p(e))B$ . In that case, he provides effort iff.  $p^1 y^1 - B(1 - p^1) > p^0 y^0 - B(1 - p^0)$ , that is, iff:

$$B < \frac{p^1 y^1 - p^0 y^0}{p^0 - p^1}$$

This is always false by Assumption C. Hence, the driver does not provide effort if unprotected. Thus, utility if unprotected is  $p^0 y^0 - B(1 - p^0)$ . Hence, the driver buys protection in period 1 iff.:

$$y^1 - F - f > p^0 y^0 - B(1 - p^0) \Leftrightarrow y^1 - p^0 y^0 + B(1 - p^0) > F + f$$

□

We now analyze the Rubinstein alternating-offer bargaining model to demonstrate how driver's bargaining power decreases with time pressure when negotiating fees with the passenger. At time 0, driver makes an offer  $y$  to the passenger. If the passenger accepts the offer, then partition of  $c$  is achieved. Else passenger makes a counter-offer  $z$  to the driver at time  $\Omega > 0$ . If accepted, then partition of  $c$  is achieved, else driver again makes a counter-counter-offer at  $2\Omega$ , ad infinitum until an agreement is reached. Each player's payoff is  $U(x_i) = x_i \exp(-r_{it}\Omega)$  where  $i \in \{A, B\}$  indexes driver  $A$  or passenger  $B$ . We define discount factor  $\delta_i \equiv \exp(-r_i\Omega)$ . We assume that driver is less patient than the passenger due to risks of police harassment, so  $\delta_A < \delta_B$ —i.e.  $r_A > r_B$ . SPE will characterize the equilibrium partition. Furthermore, the SPE satisfies two conditions:

- **Condition 1** (No Delay) Whenever a player has to make an offer, his equilibrium offer is accepted by the other player.
- **Condition 2** (Stationarity) A player makes the same offer whenever he has to make one.

**Proposition 3** [Bargaining under time pressure] In the limit as  $\Omega \rightarrow 0$ , the share obtained by the driver is smaller than the passenger—i.e. driver accepts a lower price. Formally, the unique allocation SPE converge to  $y^* = \mu_{Ac}$  and  $z^* = \mu_{Bc}$ , where

$$\mu_A = \frac{r_B}{r_A + r_B} < \frac{r_A}{r_A + r_B} = \mu_B. \quad (8)$$

*Proof of Proposition 3* First, by condition 2, let  $y^*$  and  $z^*$  denote the equilibrium offer that player A (driver) and player B (passenger) make when making an offer. Consider an arbitrary point in time at which the driver has to make an offer to the passenger. It follows from conditions 1 and 2 that  $B$ 's equilibrium payoff from rejecting any offer is  $\delta_B z^*$ . Subgame perfection requires that player  $B$  reject any offer  $y$  such that  $c - y < \delta_B z^*$ . Because the driver's offer is accepted by the passenger,  $c - y^* \geq \delta_B z^*$ . In fact,  $c - y^* = \delta_B z^*$  because otherwise the driver could increase his payoff by offering  $y'$  such that  $c - y^* > c - y' > \delta_B z^*$ . Symmetrically,  $c - z^* = \delta_A y^*$ . The SPE that satisfy these two equations are unique where the equilibrium offers are

$$y^* = \eta_{Ac} \text{ and } z^* = \eta_{Bc}, \text{ where}$$

$$\eta_i \equiv \frac{1 - \delta_{-i}}{1 - \delta_A \delta_B} = \frac{1 - \exp(-r_{-i}\Omega)}{1 - \exp(-(r_A + r_B)\Omega)} \rightarrow \frac{1 - (1 - r_i\Omega)}{1 - (1 - (r_A + r_B)\Omega)} = \frac{r_{-i}}{r_A + r_B} \equiv \mu_i \text{ as } \Omega \rightarrow 0.$$

□

## Appendix 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 ( <i>mbote ya likasu</i> )	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 ( <i>mbote ya likasu</i> )	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 ( <i>mbote ya likasu</i> )	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<sup>th</sup> and July 20<sup>th</sup>, 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<sup>th</sup> and July 20<sup>th</sup> 2015 in the ten lines of the East Kinshasa network.

**Table A2:** 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 ( <i>mbote ya likasu</i> )	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 ( <i>mbote ya likasu</i> )	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 ( <i>mbote ya likasu</i> )	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<sup>th</sup> and July 20<sup>th</sup> 2015 in the ten lines of the East Kinshasa network.

**Table A3:** Driving Block for Random Reallocation of Lines

<b>First Wave: 23-day period</b>	<b>Second Wave: 4-day period</b>
(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, which allowed the experiment to continue for 4 days before the budget was exhausted.

**Table A4:** Randomization Balance of the Treatment of Rerouting to Foreign Lines

<i>Dependent Variable</i>	All	Home line	Foreign line
	(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
<i>N. Bus-Day Obs.</i>	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 A5:** 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 A6:** Sample Characteristics

	N
<i>A. Sample size</i>	
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
<i>B. Characteristics of events in which a driver crosses an intersection</i>	
Number of times a sample driver crosses an intersection, in which	
... a payment is made	1,682
... a toll fee ( <i>mbote ya likasu</i> ) 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
<i>C. Trip characteristics</i>	
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 sample 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 A7:** Empirical Probabilities of Appearing on Minibus Foreign Line Pool

Line $l$	Minibus $d$									
	(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  $\Psi_d[\text{Foreign} | \text{Line} = l]$ , 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 A8:** Joint Profit Decomposition

Variable	Statistic				
	Mean (1)	S.D. (2)	Min (3)	Max (4)	N (5)
Profit per day (USD·day <sup>-1</sup> )	15.18	30.90	-88.57	75.00	109
Profit per trip (USD·trip <sup>-1</sup> )	2.13	4.43	-29.85	11.10	945
Joint Profit per trip (USD·trip <sup>-1</sup> )	3.00	3.73	-20.46	12.18	945
Revenue (USD·trip <sup>-1</sup> )	8.79	1.80	0.43	15.20	945
Number of passengers	18.36	3.60	1	29	945
Price (USD ·person <sup>-1</sup> ·trip <sup>-1</sup> )	0.48	0.05	0.22	0.54	945
Operating costs (USD·trip <sup>-1</sup> )	5.79	3.53	0	31.75	945
Number of trips per day	8.32	2.21	4	13	109
Bribe costs (USD·day <sup>-1</sup> )	7.00	7.70	0	44.62	109
of which paid to <i>receveur</i> (USD·day <sup>-1</sup> )	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 A9: Effect of Rerouting to Foreign on Joint Profit—Robustness**  
**A. Joint Profits (per day)**

	SATE <sup>raw</sup> (1)	SATE (2)	ATE (3)	ATE <sup>c</sup> (4)	PATE (5)	Winsor 99 (6)	Winsor 95 (7)	Winsor 90 (8)
Foreign	-6.805 (4.884)	-7.844* (4.065)	-8.002** (4.046)	-8.053* (4.388)	-8.071* (4.297)	-8.011* (4.284)	-7.919** (3.919)	-7.567** (3.741)
Mean Dep. Var.	18.60	18.60	18.60	18.60	18.60	18.67	19.05	19.76
Observations	167	167	167	167	167	167	167	167
R <sup>2</sup>	0.012	0.284	0.291	0.327	0.324	0.323	0.388	0.401
Rand. Block 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	NO	NO	NO	NO
Composition IPW	NO	NO	NO	NO	YES	YES	YES	YES

**B. Number of Trips (per day)**

	SATE <sup>raw</sup> (1)	SATE (2)	ATE (3)	ATE <sup>c</sup> (4)	PATE (5)	Winsor 99 (6)	Winsor 95 (7)	Winsor 90 (8)
Foreign	-0.810** (0.384)	-0.869** (0.348)	-0.894** (0.347)	-0.911*** (0.321)	-0.982*** (0.363)	-0.959*** (0.359)	-0.956*** (0.358)	-0.956*** (0.358)
Mean Dep. Var.	7.86	7.86	7.86	7.86	7.86	7.86	7.86	7.86
Observations	176	176	176	176	176	176	176	176
R <sup>2</sup>	0.028	0.241	0.246	0.363	0.318	0.315	0.315	0.315
Rand. Block 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	NO	NO	NO	NO
Composition IPW	NO	NO	NO	NO	YES	YES	YES	YES

**C. Joint Profits (per trip)**

	SATE <sup>raw</sup> (1)	SATE (2)	ATE (3)	ATE <sup>c</sup> (4)	PATE (5)	Winsor 99 (6)	Winsor 95 (7)	Winsor 90 (8)
Foreign	-0.681 (0.561)	-0.867* (0.471)	-0.898* (0.466)	-0.804 (0.494)	-0.855* (0.483)	-0.846* (0.472)	-0.809* (0.418)	-0.759** (0.353)
Mean Dep. Var.	2.62	2.62	2.62	2.62	2.62	2.62	2.74	2.82
Observations	1,395	1,395	1,395	1,395	1,395	1,395	1,395	1,395
R <sup>2</sup>	0.007	0.167	0.175	0.201	0.194	0.197	0.262	0.344
Rand. Block 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	NO	NO	NO	NO
Composition IPW	NO	NO	NO	NO	YES	YES	YES	YES

Notes: This table presents the OLS estimates of  $\beta^F$  from Equation 1. In Panel A, the dependent variable is the daily joint profit in USD·day<sup>-1</sup>. The daily joint profit is the sum of joint profit generated through each trip driven in the day. Hence, Panels B and C also decompose the joint profit per day as dependent variables into the number of trips per day and the average joint profit along each trip. Joint profits 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 1 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  $\beta^F$  estimator in Equation 1 without randomization blocks fixed effects, identifying the raw sample average treatment effect of rerouting (SATE<sup>raw</sup>). Column (2) presents the coefficients from  $\beta^F$  estimator identifying the sample average treatment effect with randomization blocks (SATE). Column (3) presents the coefficients from  $\beta^F$  estimator which include randomization blocks 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  $\beta^F$  estimator, which additionally include line fixed effects (ATE<sup>c</sup>). Column (5) presents the coefficients from  $\beta^F$  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. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full sample 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 sample 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 sample with equal weight. Appendix A4 provides a comprehensive discussion of the bias that would arise failing to include this probability, and how weighted OLS allows to produce unbiased estimates. Table A7 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 A10:** Effect of Rerouting to Foreign Lines on Joint Profit,  
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	-15.05** (6.10)	-3.13 (6.04)	-1.34*** (0.47)	-0.71 (0.53)	-1.43* (0.78)	-0.61 (0.66)
Mean Dep. Var.	21.66	22.76	8.02	8.49	2.88	3.1
Observations	88	79	91	85	697	698
R <sup>2</sup>	0.34	0.41	0.30	0.38	0.20	0.24
ACCO Protection	NO	YES	NO	YES	NO	YES
H <sub>0</sub> p-val.		0.080		0.213		0.534

*Notes:* This table presents the OLS estimates of  $\beta^F$  from Equation 1, decomposed by whether the driver-day was assigned to third-party protection of type ACCO Driver's Association sticker. We present the estimates of  $\beta^F$  separately for the sample without third-party ACCO protection (odd columns) and with third-party ACCO protection (even columns). In Columns (1) and (2), the dependent variable is the daily joint profit in USD·day<sup>-1</sup>. The daily joint profit is the sum of joint profit generated through each trip driven in the day. Hence, Columns (3), (4) and (5), (6) also decompose the joint profit per day as dependent variable into the number of trips per day (Columns 3 and 4) and the average joint profit along each trip (Columns 5 and 6). Joint profits 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. All regressions include randomization block 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. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full sample of home line driving. Table notes denoted "H<sub>0</sub> p-val." present the p-value from the interaction term in the regression of outcome variables on foreign line and third party protection assignments.

**Table A11:** Effect of Rerouting on Joint Profit,  
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	-17.04** (7.62)	-2.52 (6.12)	-1.43*** (0.53)	-0.62 (0.48)	-1.68* (0.94)	-0.39 (0.61)
Mean Dep. Var.	24.22	20.46	8.4	8.13	3.25	2.77
Observations	79	88	85	91	675	720
R <sup>2</sup>	0.41	0.32	0.31	0.35	0.24	0.20
Police Protection	NO	YES	NO	YES	NO	YES
H <sub>0</sub> p-val.		0.365		0.293		0.717

*Notes:* This table presents the OLS estimates of  $\beta^F$  from Equation 1, decomposed by whether the driver-day was assigned to third-party police protection. We present the estimates of  $\beta^F$  separately for the sample without third-party police protection (odd columns) and with third-party police protection (even columns). In Columns (1) and (2), the dependent variable is the daily joint profit in USD·day<sup>-1</sup>. The daily joint profit is the sum of joint profit generated through each trip driven in the day. Hence, Columns (3), (4) and (5), (6) also decompose the joint profit per day as dependent variables into the number of trips per day (Columns 3 and 4) and the average joint profit along each trip (Columns 5 and 6). Joint profits 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. Police third-party protection combines police escort on the minibus and access to higher-level police. All regressions include randomization block 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. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata. "Mean Dep. Var." provides the mean of the dependent variable in the corresponding column computed in the full sample of home line driving. Table notes denoted "H<sub>0</sub> p-val." present the p-value from the interaction term in the regression of outcome variables on foreign line and third party protection assignments.

**Figure A1:** An *Esprit de Mort* Minibus



Notes: This image depicts an *Esprit de Mort* minibus in Kinshasa. Source: [Emilio Noorani](#)

**Figure A2:** Negotiation bribes vs. “Mbote ya Likasu”

*A. Negotiation bribes*

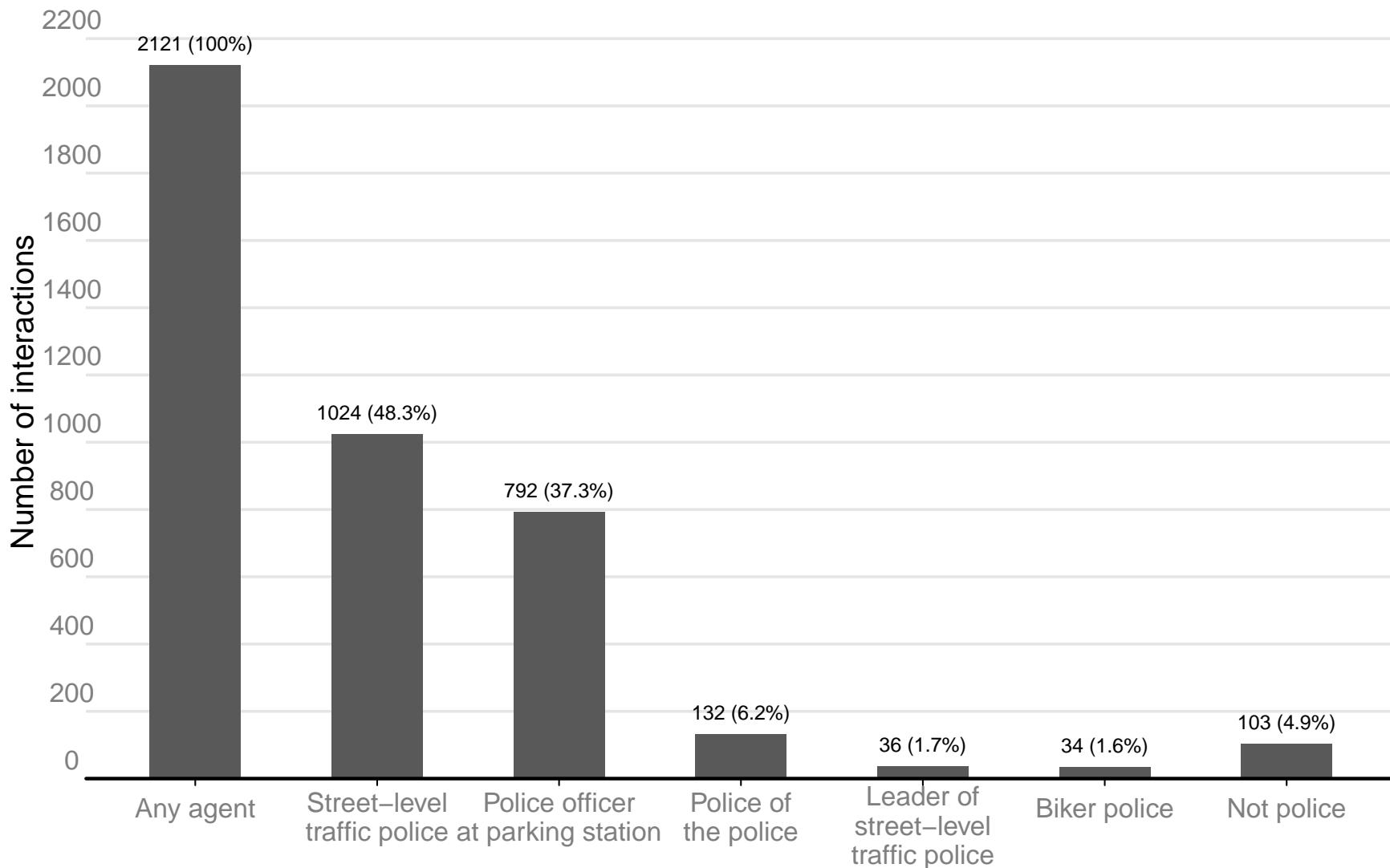


*B. Mbote ya Likasu (the toll fee)*



*Notes:* Panel A depicts a common instance of a negotiation bribe. Source: [Youtube Congo Avenir](#). Panel B depicts the “Mbote ya Likasu” handshake greeting between a driver and an intersection police officer, indicating a toll fee payment and signaling ongoing relationship. Source: [Congo Durable](#)

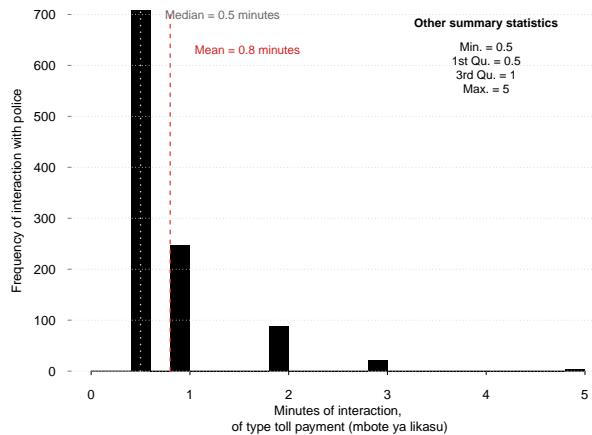
**Figure A3:** Interactions Between Drivers and State Officials in the Street, in Pre-Experimental Data



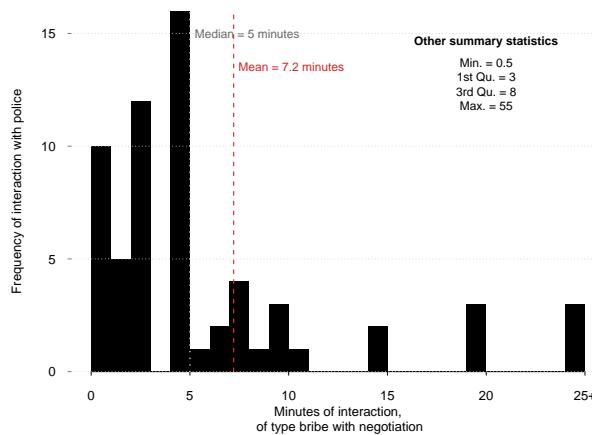
*Notes:* This bar chart displays the number and fraction of interactions between the driver and different types of agents when crossing an intersection on the home line. The figure is obtained from 15,426 crossings of an intersection from the pre-experimental data between June 19<sup>th</sup> and July 20<sup>th</sup> 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 unique identifiers such as badges and uniforms in the qualitative work in the years preceding the rerouting experiment, which is then codified into multiple choice survey questions on the SurveyCTO used by the data collector when reporting with whom the drivers interact.

**Figure A4:** Histogram of Time Waste per Bribe Interaction with Police,  
Decomposed by Type of Interactions

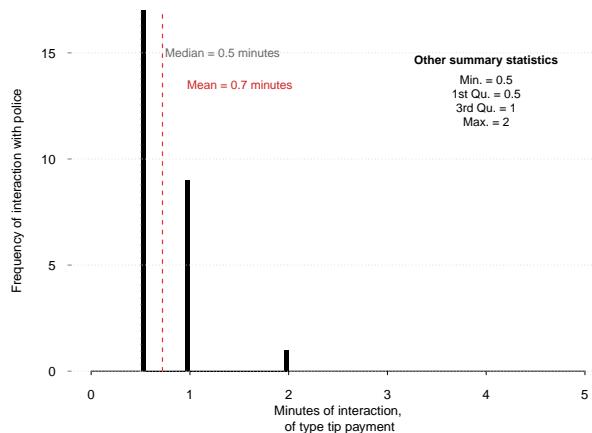
*A. Toll Fee (Mbote ya Likasu)*



*B. Negotiation Bribe (incl. Police Station)*

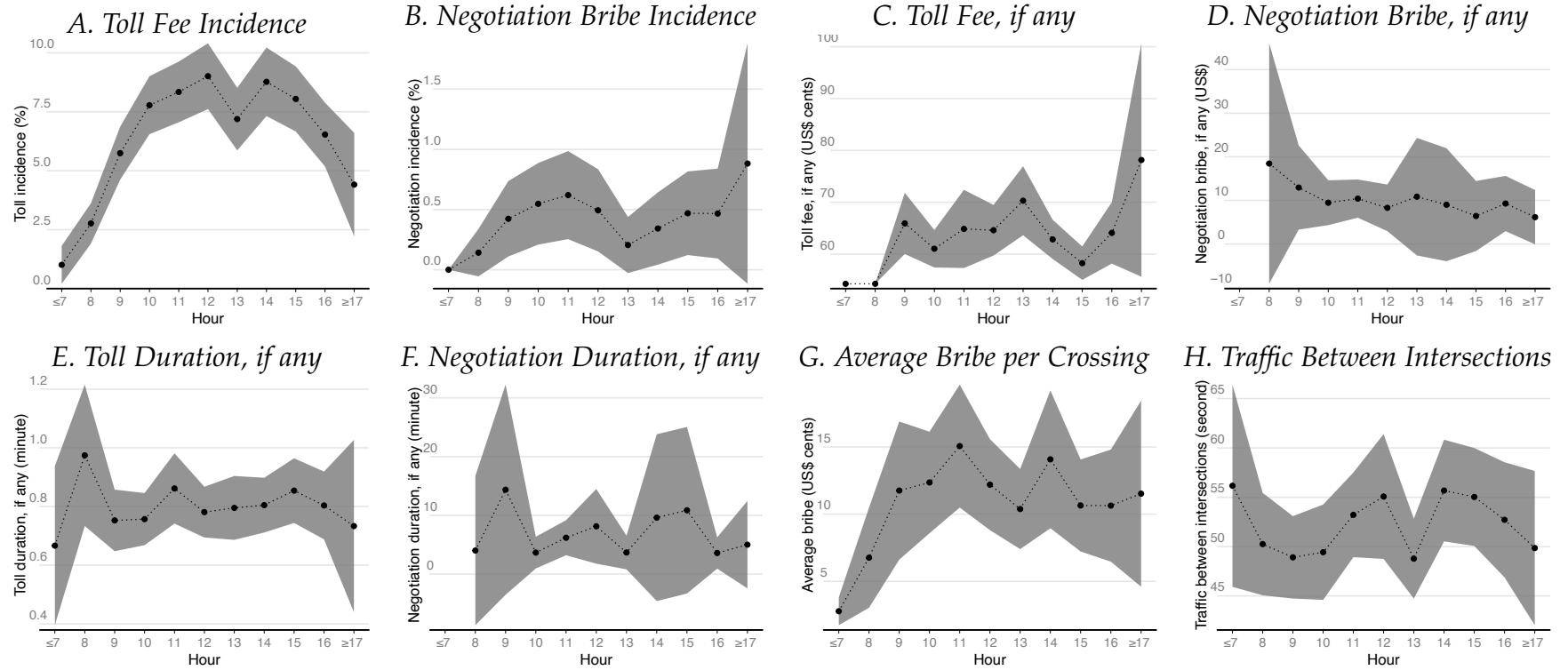


*C. Tip*



*Notes:* This histogram depicts the duration of time spent when drivers interact and pay bribes to the police officers on the home line from the pre-experimental data. These bribes are toll fee ("mbote ya likasu"), negotiation bribes (which include those paid at the intersection or at the police station), and tip.

**Figure A5:** Time Series of Bribe Costs and Time Waste per Crossing of an Intersection, by Hour



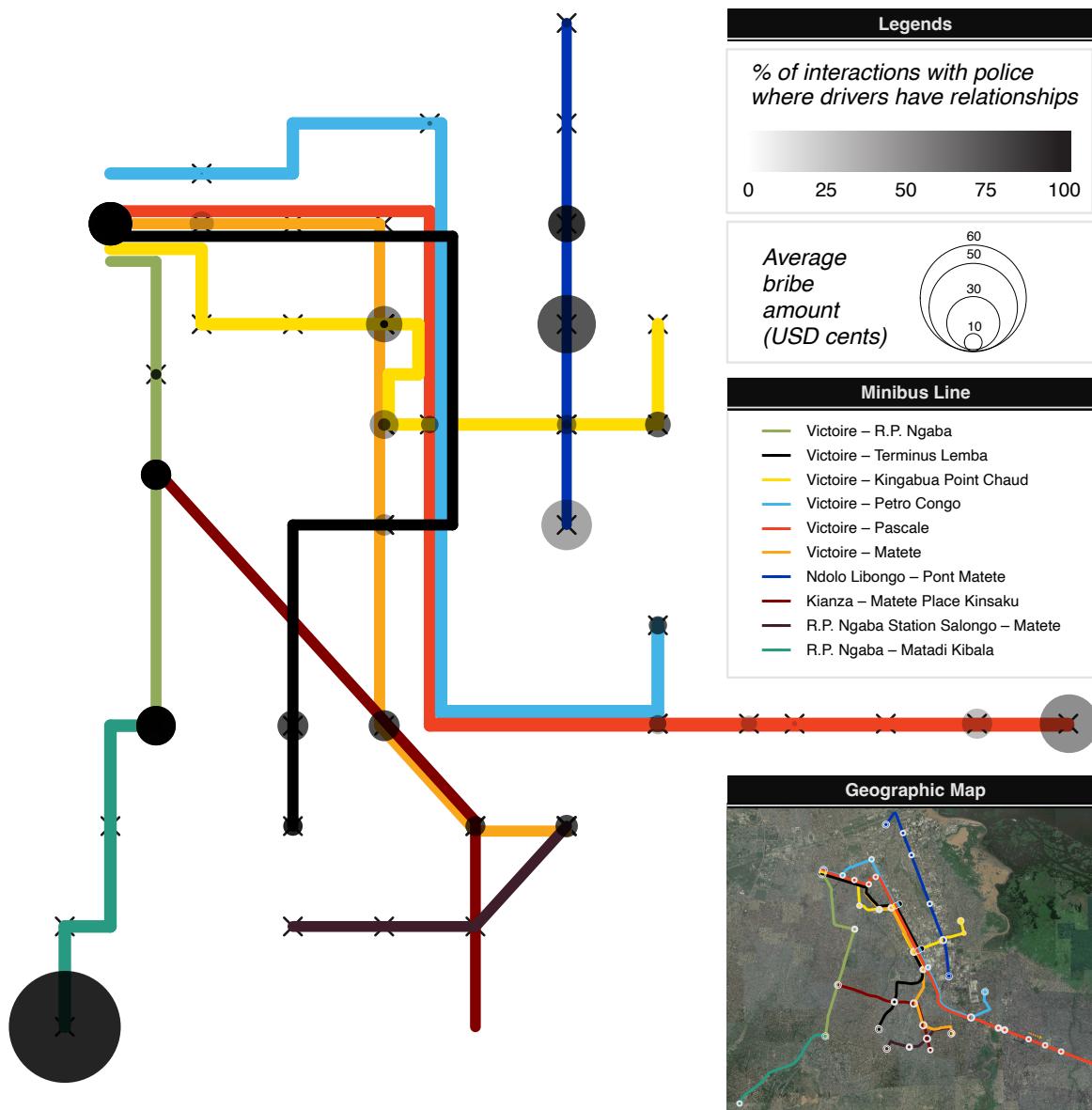
*Notes:* These time series plot the hourly average characteristics of the bribe costs of type toll fee (“*mbote ya likasu*”) and negotiation bribe, and their associated time waste, per crossing an intersection, as well as their 95% confidence intervals. Panels A and B show the average incidence, panels C and D show the amount in USD conditional on paying, panels E and F show the duration in minutes conditional on paying, panel G shows the hourly average bribe amount per crossing an intersection, and finally panel H shows the hourly average duration of traffic between intersections. Data are obtained from the pre-experimental observations on the home lines.

**Figure A6:** The sticker of the association of drivers of the Congo



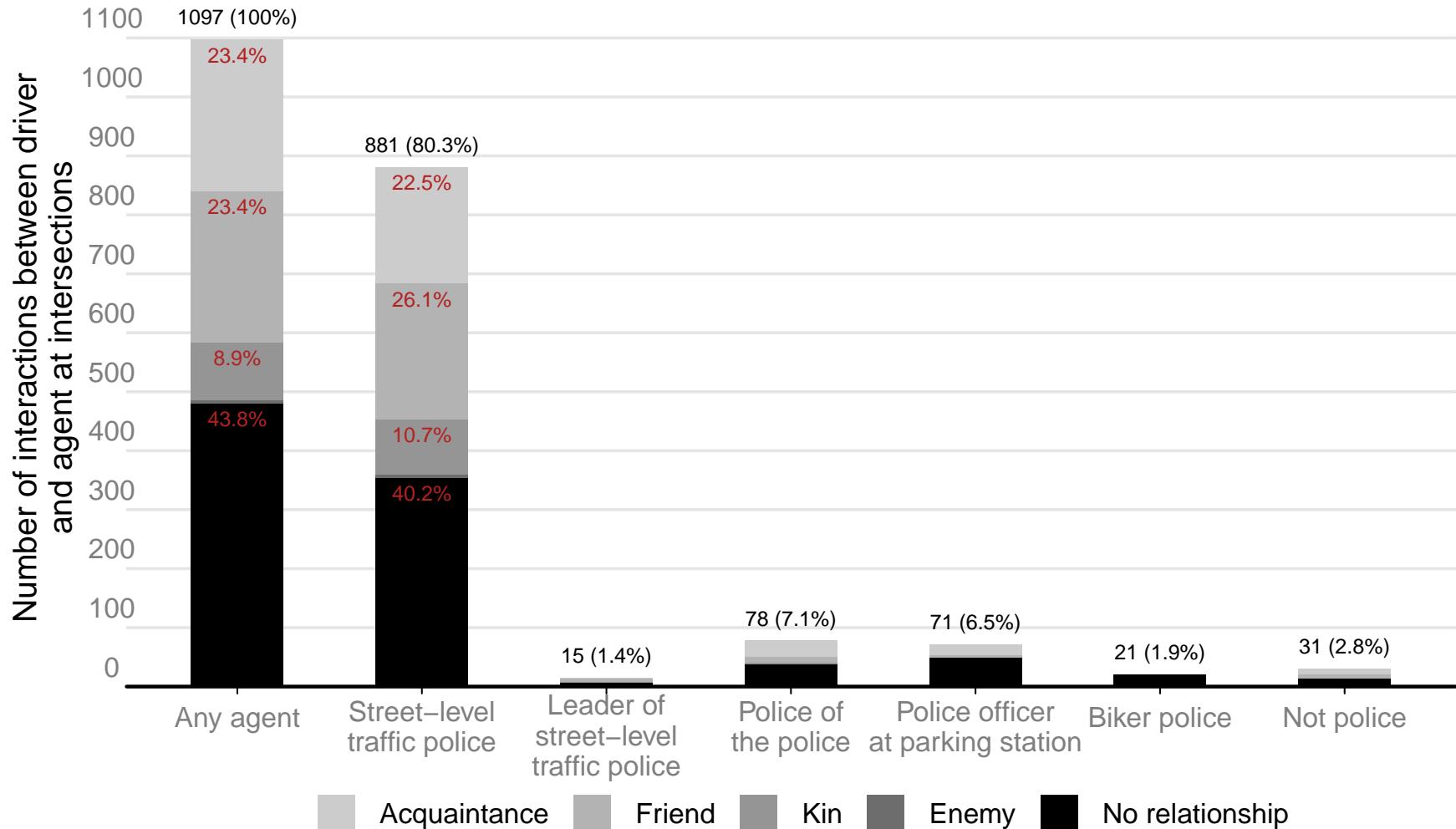
*Notes:* This figure presents the sticker of the association of drivers of the Congo, pasted on the body of the Mercedes 207 minibuses known as *Spirit of Death* ("Esprit de Mort"). **Source:** Research team.

**Figure A7:** Schematic Map of the Minibus Network of Lines in Kinshasa



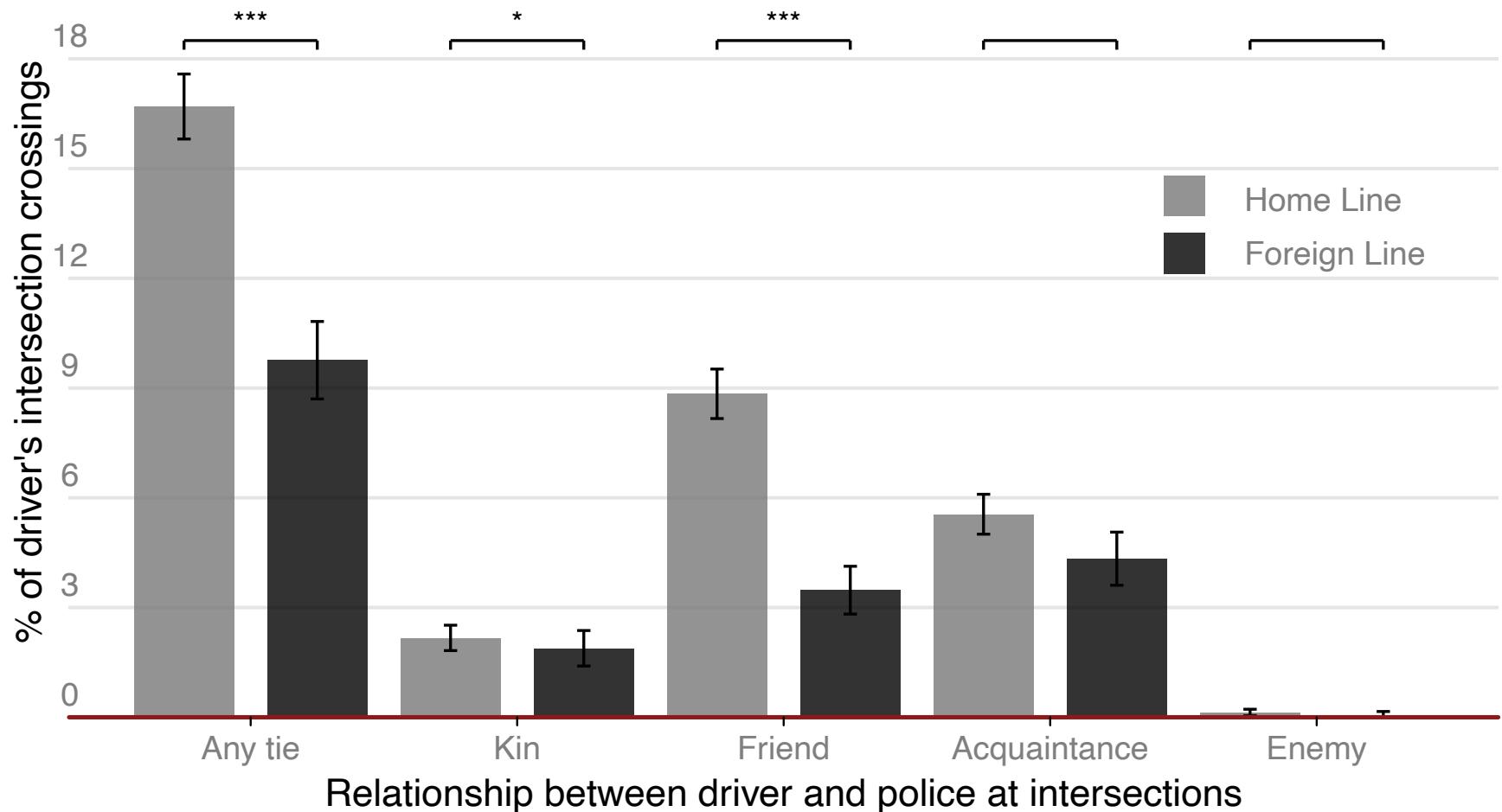
*Notes:* This figure schematically represents the lines through which “*Esprit de Mort*” minibuses pass and two key statistics for each intersection (x). The first statistic is the percentage of interactions at which home line drivers reported having relationships with police, depicted as grayscale in increasing blackness. The second statistic is the average amount of bribe that is paid by home line drivers in each intersection, depicted as circles with increasing diameter. Geographic proximity is preserved as much as possible in the schematic map.

**Figure A8: Relationships Between Drivers and State Officials in the Street**



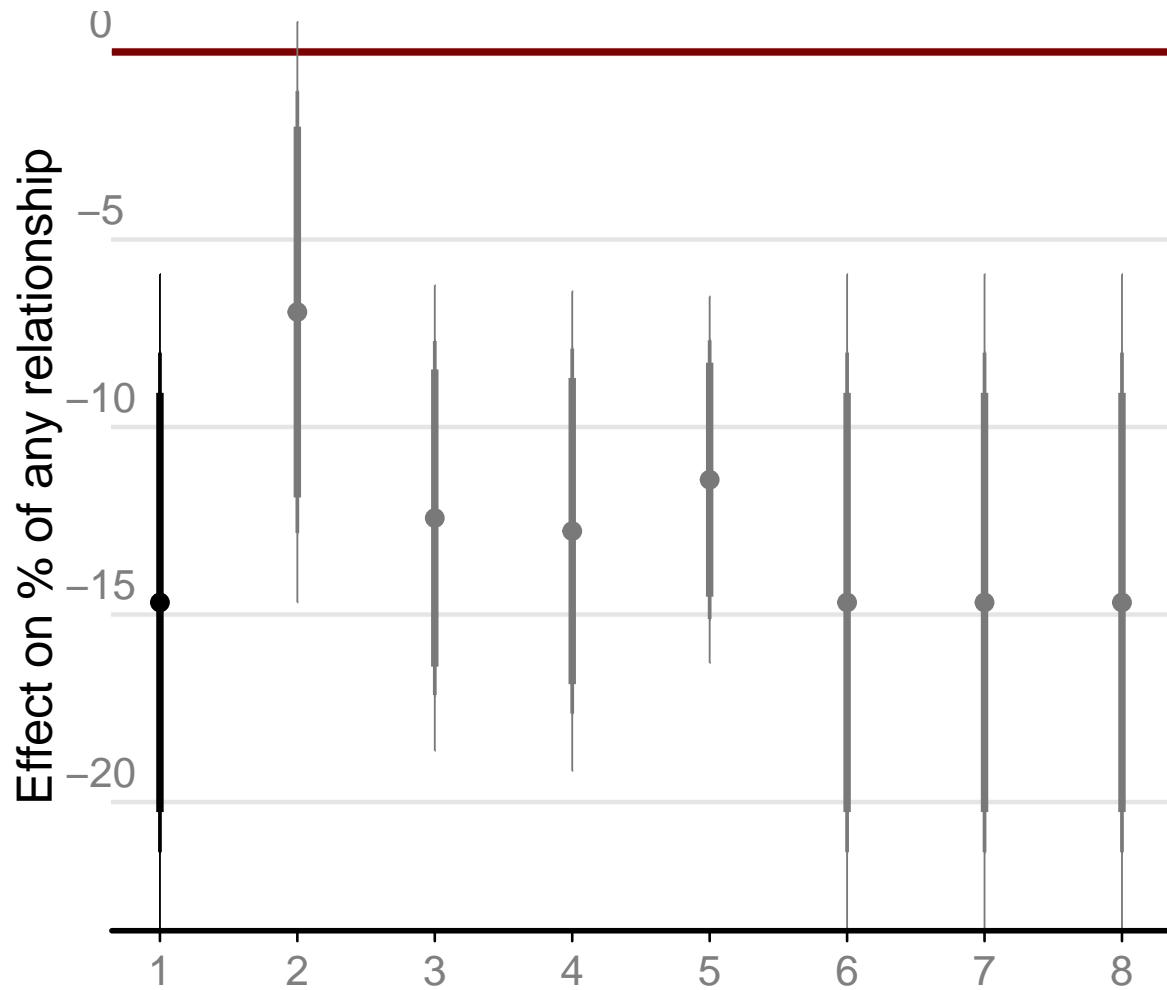
*Notes:* This bar chart displays the number and fraction of interactions in which drivers have relationships with police officers, obtained from interactions in driver's home lines in the experimental data. Each bar is further decomposed into the extent of relationship. The percentages reflect the proportion of interaction between driver and officers at the intersection for which the type of officers are described on the horizontal axis. The red labels 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 unique identifiers such as badges and uniforms in the qualitative work in the years preceding the rerouting experiment, which is then codified into multiple choice survey questions on the SurveyCTO used by the data collector when reporting with whom the drivers interact.

**Figure A9:** Effect of Foreign Line rerouting on Relationship with Agents at Intersections, by Intersection Crossing



*Notes:* This figure presents the 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 asterisks represent the statistical significance of the test for whether the fraction on the home line is statistically significantly different than that on the foreign line, for each relationship category. Statistical significance is obtained from estimating  $\beta^F$  of Equation 1. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata. Social relationships include those of kin, friend, acquaintance, and enemy. 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.

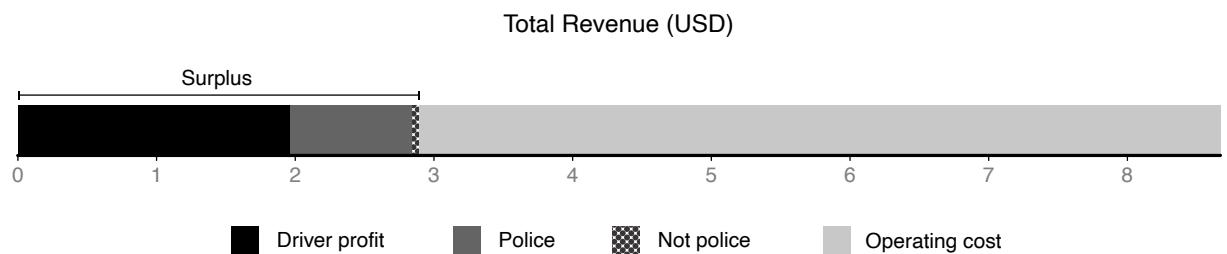
**Figure A10:** Effect of Rerouting to Foreign on Prevalence of Relationships—Robustness



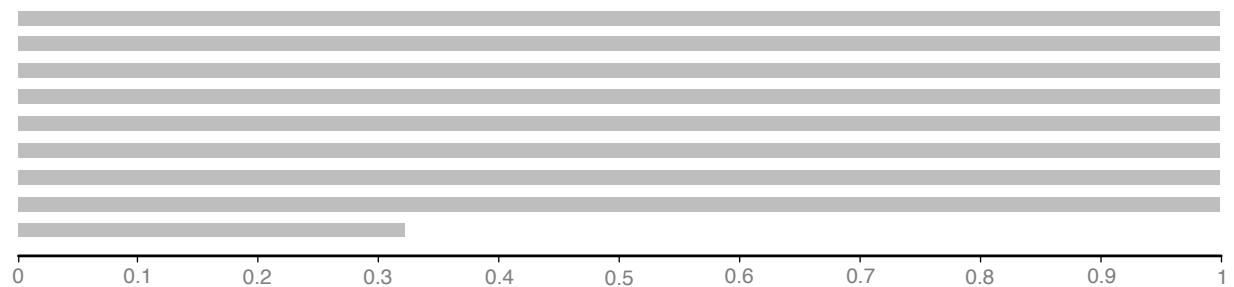
*Notes:* This figure plots the estimates of  $\beta^F$  coefficient from Equation 1 for relationship (any tie) under various specifications of regression weights and controls. The brackets depict the confidence intervals for the coefficient estimates at 1%, 5%, and 10% significance. Specification key on the x-axis: (1) PATE, our main specification, which is the population average treatment effect, (2) Sample average treatment effect without randomization block fixed effects, SATE<sup>raw</sup>, (3) Sample average treatment effect, SATE, (4) Assignment probability adjusted sample average treatment effect, ATE, (5) Assignment probability adjusted sample average treatment effect accounting for line composition imbalances, ATE<sup>C</sup>, (6) PATE for whose outliers in dependent variables are winsorized at 1%, (7) Specification 6, except at 5%, (8) Specification 6, 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. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 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 sample 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 sample with equal weight. Appendix A4 provides a comprehensive discussion of the bias that would arise failing to include this probability, and how weighted OLS allows to produce unbiased estimates. Table A7 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 A11: Trip Characteristics**

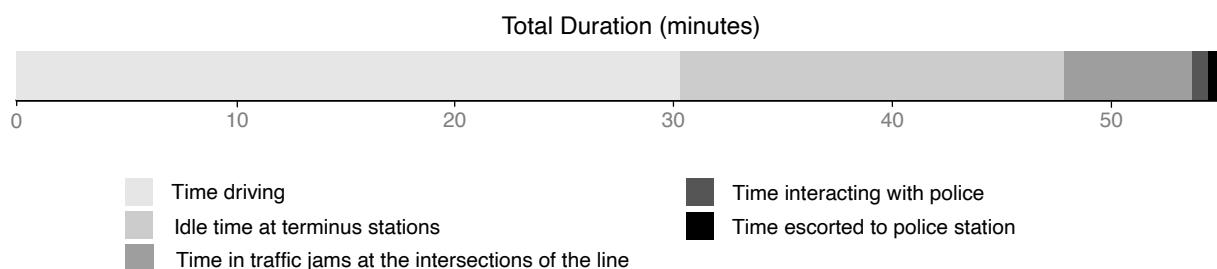
*A. Revenue of a Trip*



*B. Daily number of trips*

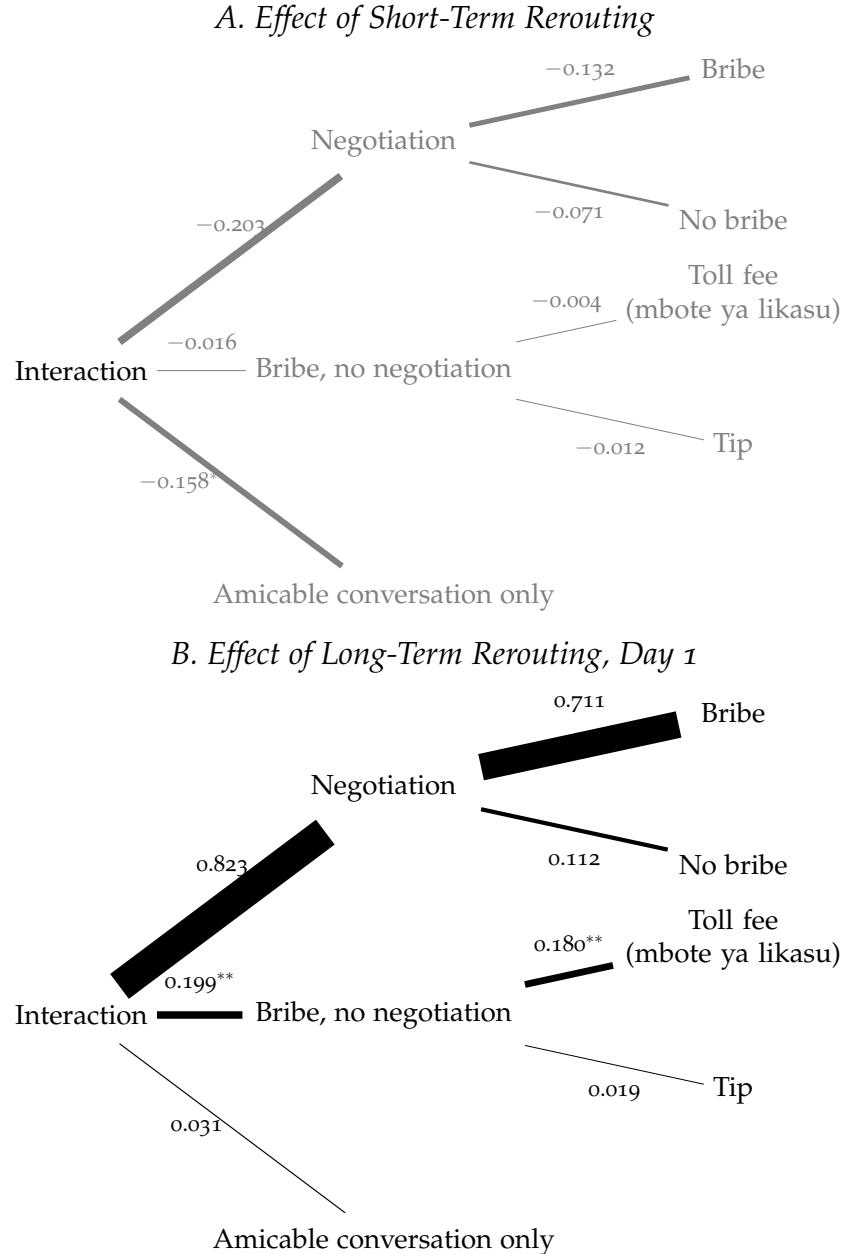


*C. Time Allocation in a Trip*



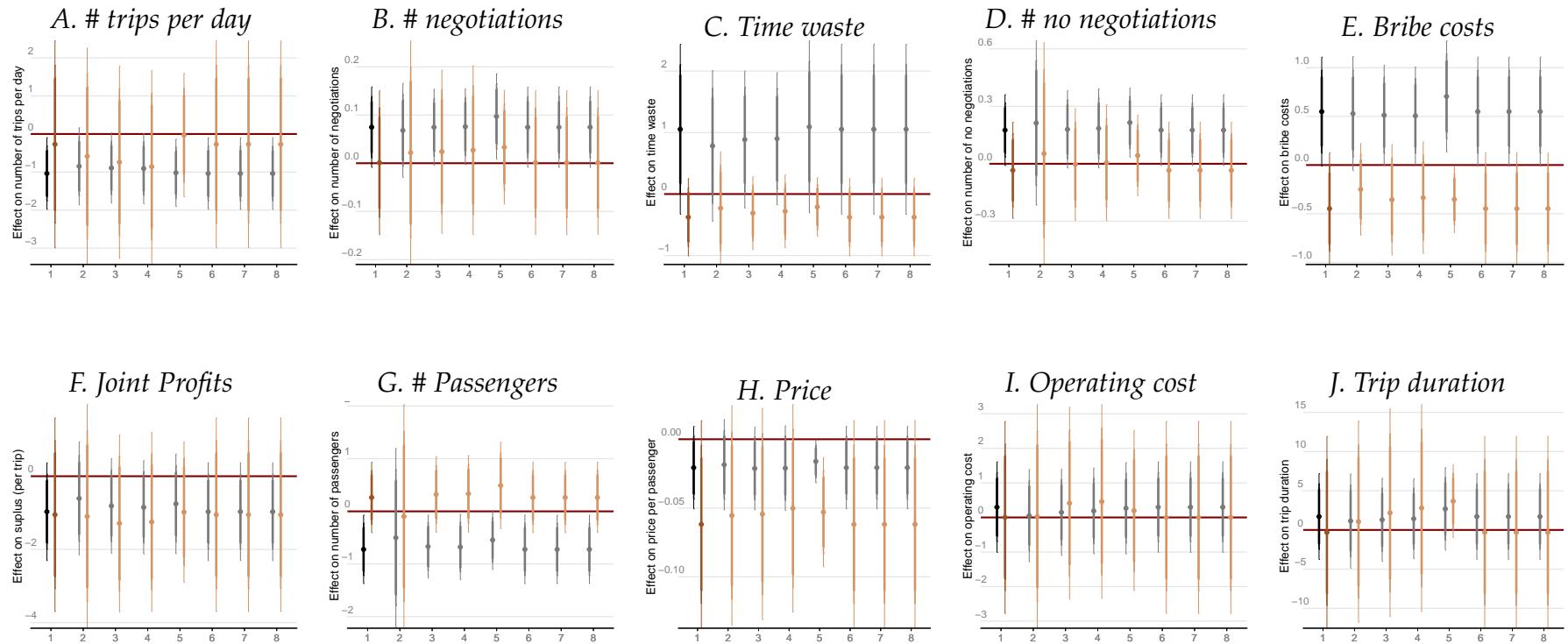
*Notes:* This figure presents the allocation of joint profit and time from an average trip from drivers on the home line of the experimental data. Panel A breaks down the driver's revenue from passenger's payment of minibus tickets into joint profit and operating cost. Panel B depicts a progress bar 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.

**Figure A12:** Effects of Rerouting on Duration of Interaction with the Police



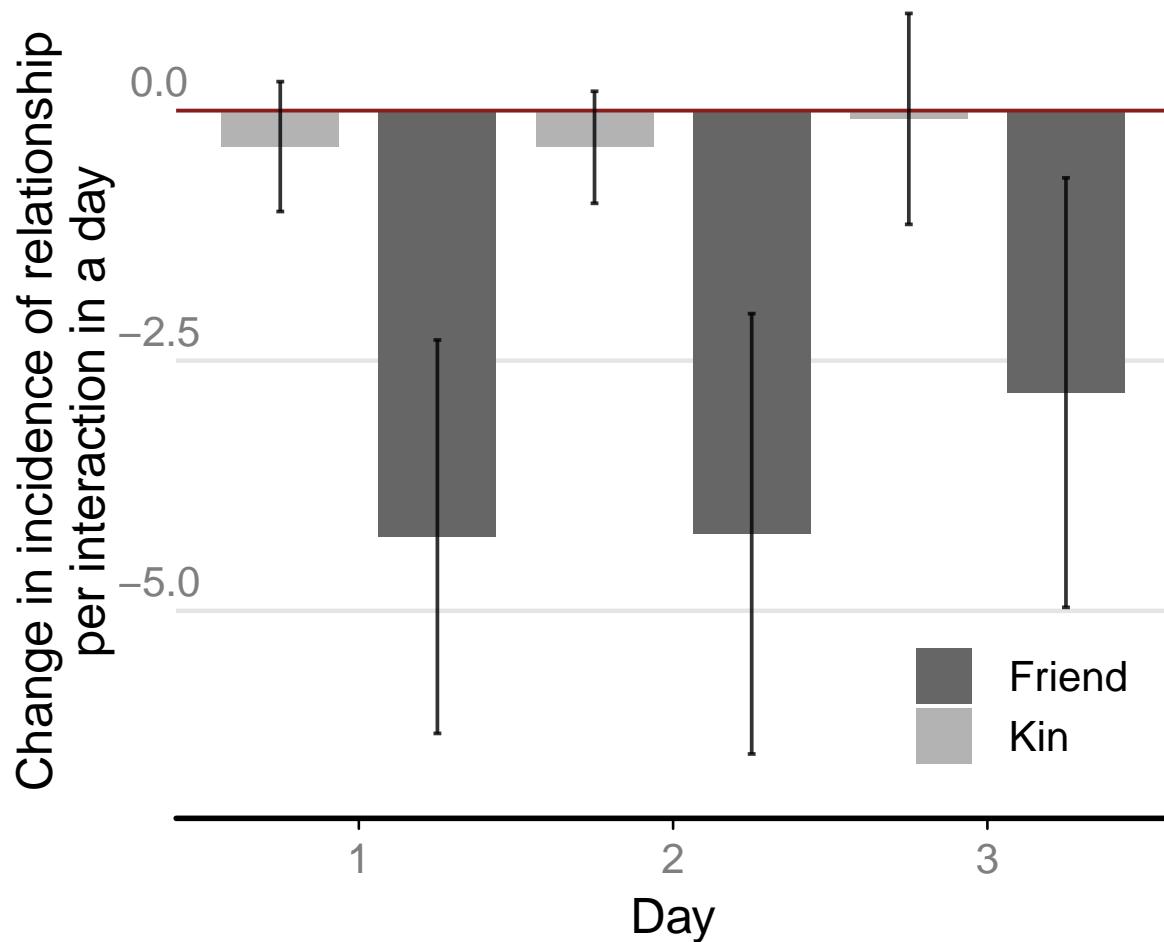
*Notes:* This tree diagram separates the effects of rerouting on the duration of interactions by the length of the time horizon of this rerouting, using the replica of Figure 2 as a baseline template for the type of interactions which occur when drivers interact with police officers on the driving lines. As the experimental design suggests, we partition the days of experiment and drivers into driving blocks of contiguous days (three days or one day, in order to allow the analysis of the impact of various time horizon.) Panel A presents the effect of short-term rerouting, that is rerouting which occurs on one-day driving block. Panel B presents the effect of long-term rerouting, that is rerouting which occurs on the three-day driving block. The effect of foreign rerouting is based on observations only on the first day of these driving blocks on the foreign line to remove the effects associated with having interacted with the police officers on the first day, as compared to observations which pool the driving blocks on the home line. The line thickness of each branch in the tree diagram is proportional to the coefficients from  $\beta^F$  population average treatment effect estimator in Equation 1, on the duration of each type of interaction. Each coefficient size of the estimate of this PATE estimator and the statistical significance of the coefficient are labeled on each branch. Negative coefficient identifying lower time waste is colored in grey, while positive coefficient identifying higher time waste is colored in black. Negotiation with bribe is coded by the enumerator as driver having negotiated payments with police and resulting with payments, whereas negotiation with no bribe is coded as having negotiated payments with police but ending up with zero payment (either due to protection, escape, promise of future payment, etc.)

**Figure A13:** Effect of Rerouting to Foreign Lines on Joint Profit and its Components—Robustness



Notes: These figures plot the estimates of coefficient  $\beta^F$  from Equation 1 under various specifications of regression weights and controls (Panels A-E), as well as those for which dependent variables are lead and lagged for 1, 2, and 3 days (Panels F-J), separately for subsamples of observations that were assigned to long and short time horizons. 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 block fixed effects, SATE<sup>raw</sup>, (3) Sample average treatment effect, SATE, (4) Assignment probability adjusted sample average treatment effect, ATE, (5) Assignment probability adjusted sample average treatment effect accounting for line composition imbalances, ATE<sup>C</sup>, (6) PATE for whose outliers in dependent variables are winsorized at 1%, (7) Specification 6, except at 5%, (8) Specification 6, 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. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 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 sample 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 sample with equal weight. Appendix A4 provides a comprehensive discussion of the bias that would arise failing to include this probability, and how weighted OLS allows to produce unbiased estimates. Table A7 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 A14:** Long-Term Horizon: Investment in New Relationships?



*Notes:* This figure presents the population average treatment effects of rerouting on the incidence of kinship and friendship, separated by observations on the first, second, and third day of the long time-horizon, i.e., experimental driving blocks that contain three contiguous days. The figure plots the coefficient size and the 95% confidence interval of the  $\beta_1$  estimate of the PATE estimator from Equation 2, which identifies the population average treatment effect of foreign line driving on the incidence of relationship within each trip and days of driving. Incidence of kinship is shown in lighter gray, while incidence of friendship is shown in darker gray. Trip is defined as a return trip between two termini. Trips are winsorized at 5 per day because of the low number of observation above it. Standard errors are clustered at the level of the *drivers' driving blocks*, which is the level of randomization into foreign vs. home driving. Because the assignment to home vs. foreign was made at the level of the driver's driving block, each minibus driving block is a randomization cluster. Since there are, by construction, 13 clusters for each minibus, the experiment randomizes 130 clusters, stratified by 10 randomization strata.