On the mechanics of kleptocratic states: Administrators' power, protectors, and taxpayers' false confessions

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Preliminary study design — do not circulate

Abstract

Powerful state administrators can take advantage of their positions to extract resources, especially when political accountability is broken. We conjecture that administrators' power depends on their ability to inflict harm using the power of office, their ability to mobilize powerful networks, and on their privileged access to information. Measuring transfers to administrators is challenging, because they often involve secrecy, and surveys often draw on recall. To circumvent this challenge, we develop a smart phone application, and monitor 400 households of the Democratic Republic of the Congo to privately report every day the universe of payments made during 5 months. The DRC offers a well-suited environment, because administrators systematically use their power to extract payments from citizens at unusually high rates. We deploy three randomized interventions aimed to affect the balance of power between administrators and households. First, since administrators systematically take advantage of a tax code that is extremely confusing, we organize pro-bono weekly tax consulting to a group of households. Second, to affect the bargaining power that stems from unequal access to social networks, we extend a link from a reputed civil society organization to randomly selected citizens. The organization uses its political leverage to protect the selected citizens. Third, we organize a city-wide campaign to expose administrators known to have committed abuses in a random sample of neighborhoods. *This document proposes an exhaustive pre-analysis plan that uses a simulated assignment to treatment instead of the real treatment assignment.*

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1 Introduction

Current welfare states with a functioning rule of law that constrains the state and its administrators are an aberration in the historical process. Since their creation, states were mostly motivated to extract from the citizens they controlled; only more recently have rulers conceded power to large parts of the population (Acemoglu and Robinson, 2006; Acemoglu, Ticchi, and Vindigni, 2006; Bates, Greif, and Singh, 2002; Chrétien, 2000; Claessen and Skalnik, 1978; Greif, 2008; Sanchez de la Sierra, 2016; Scott, 1999; Tilly, 1990).

In most states in recorded history, as well as in undemocratic states today, predatory administrators (whether organized or in isolation) serve as a pervasive vehicle for economic redistribution by expropriating wealth from the weakest parts of society. The transfer of economic resources to administrators is often the outcome of a bargain conducted in the presence of both asymmetric information and an asymmetric endowment of power vis-a-vis citizens (Olken and Singhal, 2011; Khan, Khwaja, and Olken, 2016).[1]

In this paper, we deploy a field experiment in the Democratic Republic of the Congo (the DRC) that aims at empowering citizens vis-a-vis administrators, and develop an innovative fine-grained measurement of payments to administrators. First, given that citizens are frequently extorted to give "false confessions" of inexistent tax liabilities by tax collectors, we provide high frequency, customized tax consulting to citizens. Second, since the social networks of citizens can protect them from administrators' abuse, we extend new social networks links (protection) to citizens in order to empower citizens to bargain over their tax payments. To implement such interventions, we partnered with ODEP, a Congolese civil society organization with expertise on tax law, and with leverage and shaming potential with the government and the parliament, to develop the two treatments.

[1] While it is possible that informal taxation sometimes represents a mutually beneficial collusive bargain between citizens and tax collectors, the two are isomorphic as described below.

The context in the DRC is well suited to examining power relations between administrators and citizens. State institutions in the DRC are considered among the weakest and most predatory in the world today. The state collapsed under the rule of Mobutu See Seko, who established a highly kleptocratic system in which administrators could take advantage of the asymmetry of power to systematically extract payments from citizens. Today, dozens of state agencies systematically take advantage of an extremely confusing tax code; confusion that was recently worsened by decentralization, which multiplied the number of taxes and agencies authorized to collect taxes. Economic activity is systematically subject to numerous obstacles imposed by state officials who argue—often arbitrarily—that taxes must be collected. This discourages investment and is a driver of inequality.

The importance of bargaining over bribes between administrators and subjects suggests that increasing the power and outside option of citizens in states with abuse by administrators can improve the terms of bilateral bargaining in favor of the citizens, the weakest segments of society. This can be especially promising when collective bargaining, through institutions of democratic accountability, are largely unavailable and costly to develop. A challenge for research and policy, however, is how to empower citizens to bargain over their informal tax payments. A careful consideration of the structure of societies in predatory states provides a useful motivation. Administrators use their power to redistribute wealth in society, where social networks are a major source of power. It is thus natural to expect that interventions that redistribute social networks towards the weakest segments of society can empower the citizens against abuse by individual state officials. Similarly, to the extent that administrators often take advantage of the citizens' confusion about the tax code, and thus about the payments whose refusal can lead to harmful consequences, interventions that train households to navigate the tax system potentially empower citizens against administrators, and decrease the opportunities for coercive manipulations of their outside option.

We also develop an innovative measurement technology to assess the impact of these interventions. A fundamental challenge with measuring illicit payments is precisely that they are hidden, making it difficult to obtain such data from administrative sources. Some researchers (Sanchez de la Sierra, 2016; Jibao, Prichard, and van den Boogaard, 2016) have attempted to collect similar data using surveys, but such approaches often require relying on recall data, which can be problematic due to classical and particularly, non-classical measurement error. We address this challenge developing an innovation in measurement in this literature that enables us to collect fine-grained daily real-time payments for the following 5 months — prospective data collection. Specifically, we provided households and enterprises with smart phones, loaded with an application that we developed, that would enable them to record daily data on their tax payments, and upload this data on a weekly basis to a server. This approach allowed us to track payments made to state administrators on a daily basis for 310 households and enterprises for up to five months. Through the design of data collection, we ensured that the smart phone data collection activities were separate from the tax consulting activities and the network links we induced, to minimize concerns about reporting bias.

We also go a step further, and examine the impact of a a third experimental intervention on bilateral bargaining power between administrators and citizens: we implement an anti-corruption campaign aimed at making bribe taking riskier. While the tax consulting and protection interventions acted on the beliefs of households—beliefs about their bargaining power, and beliefs about the tax system—this intervention was designed to act on the payoffs of the tax officials. Since we promised households selected in the protection intervention that ODEP would launch a campaign against recorded abuses by tax officials, we worked with ODEP to organize such a city-wide campaign after three months of smart phone data collection. To be able to estimate the effects of this intervention, we randomly selected half of the neighborhoods of Kinshasa to receive the campaign while administrators in the remaining half were able to continue to operate with impunity. This allows us to estimate the impact of a city-wide anti-corruption campaign on the ability of administrators to extract payments households and small enterprises.

In what follows, we present our pre-analysis paper. We describe the study, and our theoretical framework, and present preliminary results using real variables. Instead of the real treatment, however, we simulate fake treatment assignment, which prevents us from basing inference on sampling error while allowing us to learn from our covariates to tailor measurement to the institutional framework. Section 2 presents the institutional framework, Section 3 presents the interventions, Section 4 presents the theoretical framework, Section 5 presents the empirical strategy, Section 6 presents the (simulated) results from the individual interventions, Section 7 presents the (simulated) results from the city-wide anti-corruption campaign, and Section 8 concludes.

2 Institutional framework

Responding to the challenges of conflict, state weakness and limited accountability, the government embarked on a formally ambitious program of decentralization beginning in 2008. Among other things, the decentralization reforms offered local authorities substantially expanded tax powers, and all provinces had established revenue authorities by 2009. Proponents of revenue decentralization, both in the DRC and elsewhere, offer a series of potential benefits. Decentralization may encourage expanded revenue collection and service delivery, thus spurring broader state building; bring government close to the population, thus more closely aligning tax and expenditure policies with popular preferences and characteristics; encourage popular engagement, greater access to knowledge about the tax code, and substantially greater scope for citizens to bargain with local governments. Expanded taxation is, in turn, critical to accounts of the potential governance benefits of decentralization. Expanded local government taxation helps local governments to become more autonomous, spur state building and promote accountability (Jibao and Prichard, 2015; Paler, 2013).

However, while the potential benefits of decentralization are well known, the decentralization of local revenue collection can have important adverse effects on households and businesses. These include the promulgation of complicated and poorly understood tax regimes, which are frequently regressive in their incidence on poor households (Olken and Singhal, 2011) — and often arbitrary owing to weak oversight; the duplication of taxes on the same tax base by multiple levels of government, leading to double (or triple) taxation; the proliferation of formal taxes, often referred to as "nuisance taxes", which raise little revenue but can be damaging to local businesses and open opportunities for corruption; the expansion of informal taxes by state and non-state agents, as the withdrawal of central agents, the complexity of new tax rules, and weak monitoring opens new space for abuse. For these reasons recent research on fiscal decentralization has stressed the importance of rationalizing the tax system across levels of government and empowering local civil society actors to monitor outcomes (Jibao and Prichard, 2015; Bird, 2011; Lough, Mallett, and Harvey, 2013).

The DRC is particularly well-suited to examine this environment. State weakness and extended legacies of conflict offer an enhanced risk of both uncoordinated tax activities and pervasive informality. Work by Englebert and Kasongo (2014) details these dynamics in the DRC, arguing that decentralization appear to have accelerated the proliferation of local government taxes, and enhanced the fiscal burden on households. Weijs, Hilhorst, and Ferf (2012) highlight evidence of pervasive informality by state actors, which they attribute to the legacy of the Mobutu era in which public servants, including the military and police, were encouraged to self-finance their salaries and operating costs through informal and predatory taxation. De Herdt and Wagemakers (2010) similarly demonstrate that the weak central state in the DRC enables local state actors to use their political influence or feigned ignorance to extract taxes with no legal foundations. In the conflict-affected regions of Eastern Congo, Van Damme (2012) shows that 'improving' the security situation involved the entry of a large number of state actors (including the military, the national police, the national intelligence services, and other government departments) where "the vast majority of state services collected illegal taxes, arbitrarily arrest or illegally detain people for money or demand large payments just to do their job". Meanwhile, even in more secure and urban areas, ODEP (2013) documents the wide range of market taxes confronted by small businesses in Kinshasa's central market. Research undertaken by Titeca and Kimanuka (2012) at Congolese border crossings reveals that informal taxes collected by customs agents are widespread and that traders often prefer to pay, cheaper, informal taxes than paying the formal tax. At the same time, traders argue that the unpredictability of informal taxation and the need to constantly renegotiate payments makes it hard to do business. The collection of taxes at border crossings has also exposed women to potentially higher levels of informal taxation both because they are more likely to be traders and more likely to be physically and sexually intimidated into making payments (World Bank 2011). Importantly, Titeca and Kimanuka (2012) find that military personnel — up to seven different units near one border crossing — were also extorting taxes in exchange for protection, along with unauthorized local authorities and non-contracted customs agents operating on behalf of various state agencies.

While these studies focus on informal taxation by state agents, it is equally important to note the potential for significant informal taxation by non-state actors. Recent research in Sierra Leone, for instance, has provided the most formal evidence of the widely cited suspicion that traditional authorities are often heavily involved in revenue collection in local government areas, straddling the line between the formal and informal (Jibao, Prichard, and van den Boogaard, 2016).[2]

[2]Research in other parts of Africa has similarly highlighted the role of community development or self-help organizations in mobilizing resources for local service provision (Olken and Singhal, 2011).

3 The interventions: motivation and design

To influence the bilateral balance of power between administrators and citizens, we develop a partnership with the leadership of ODEP (*Observatoire de la Depense Publique*), a major Congolese civil society organization widely respected for its effectiveness at combating abuses by tax officials. ODEP is a reputed organization that combats the leakage of tax revenues and corruption at the highest levels of the government. ODEP has leverage: they are recognized at multiple levels of the government administration and they hold a seat at the parliament and government meetings. Importantly, ODEP is an organization of tax experts. Figure 1 shows the distribution of trust to different state and civil-society organizations, as reported in a survey by the subjects that were part of this experiment. Clearly, civil-society organizations enjoy much more trust than state agencies, and this applies to general civil-society organizations as well as ODEP. The choice to partner with ODEP is also a policy relevant one. In a state like the DRC, which does not have a credible internal mechanism to ensure that corrupt tax officials are sanctioned, non-state, civil-society organizations are in most cases the only alternative to such otherwise fundamental guarantors of the state of law. We examine a first step to explore the potential of homegrown civil society organizations to expand their scope of action when the state has failed to do so.

3.1 The tax consulting intervention: information is power

When one talks to citizens across all communes of Kinshasa, it does not take long before one is struck by the complaints of citizens and businesses that tax officials, and administrators in general, abuse their power and take advantage of public ignorance about the tax code. Tax officials often come to collect the rental tax on tenants, despite the fact that it is supposed to be collected on landlords, with the explanation that the incidence of the tax is passed onto the tenant, so the tenant must pay, even if they also collect the same tax from the landlord. Examples of this phenomenon abound: small businesses are often told false liabilities on the basis of false size restrictions or turnover thresholds. If businesses fail to pay, not knowing if the rule is a false one, tax officials may be able to inflict harm on the businesses — more so than if the rule violation was false. Facing such uncertainty and risk, citizens and small businesses often pay without knowing if the payment is legal in order to be protected from potential sanctions. The tax code is extremely confusing, and popular narratives support the view that this confusion is intentionally created to increase the power of administrators to take advantage of uninformed households.[3] Whatever the source of the extreme ambiguity in the tax code, citizens claim that if they were better informed, they would be in a better position to negotiate, since tax officials would need to prove to their superiors and other actors that citizens are in violation, exposing the citizens to a non-trivial risk if they are caught collecting illegal taxes. Our survey data provides support for this information asymmetry. Figure 2 shows the distribution of self-reported knowledge of the tax code among the experimental subjects using the pre-treatment survey data. More than 70% of subjects report that they do not know much the relevant tax code, or not at all.

To manipulate the informational foundation of citizens' bargaining power, we thus designed an intervention that provides, on a weekly basis, customized tax consultancy on the tax code to a random sample of citizens and businesses. Instructed and funded by our research team, the tax law experts of ODEP provided personalized weekly phone tax consultancy on what taxes are legal as well as the legal rates (based on the taxes paid in the previous week or expected to be paid in the coming week). As part of the tax consulting package, the ODEP experts also provided advice on how to navigate the administration in the event that the citizen was discontent with specific payments or interactions, usually a complex mechanism as well. For the sample of citizens in this treatment group, there was no claim that ODEP would take any action on the citizens' behalves: it was clearly communicated

[3] Yet it is also partly the result of a complex decentralization process that assigns the right to tax to a multiplicity of agencies for political reasons.

that the involvement of ODEP in this group was strictly limited to providing tax consulting in the form of weekly personalized tax consultancy by phone for 5 months.

3.2 The protection intervention: the power of social networks

When one witnesses any episode of bargaining for taxes or fees between citizens and administrators in the Democratic Republic of the Congo, it is straightforward to observe the importance of the unobserved power of the social network. Since the social connections of an individual or business are unobserved unless that citizen is known to the tax collector, administrators often engage in a lengthy negotiation process mostly aimed at extracting a costly signal about the connections of the citizen.[4] The importance of social networks, and the profoundly unequal access to powerful networks underscores Olken and Singhal (2011)'s finding that informal taxation is regressive. The equilibrium payments ultimately depend on the allocation of bargaining power in society, mostly determined by the power of citizens' social networks—power that is unequally distributed. Households' connections have strong impacts on the payments that administrators are able to extract. Usually, connections with high ranks in the most powerful state agencies are the most protective connections, because of the harm the network can inflict on the official intending to tax. For instance, high ranked Army officers, police officers, agents of the intelligence services, or officials in Ministries, offer the most protective connections.[5] A major channel to empower citizens against administrators, thus, is to directly (re)distribute network links with powerful individuals or organizations, expanding access to

[4] A few anecdotes may provide useful motivating examples. In 2012 one of the authors of this paper was traveling through red zone, in the state controlled part, where Army battalions well known to be predatory and extremely violent raised barriers along the route in order to extort drivers. In each barrier, the author had to hide to avoid indicating that the car had international connections, which could raise expectations about a bribe, but could also signal that the car had powerful connections; thus to reduce the risk, the foreign author had to hide. However, in each roadblock, the driver barely stopped, showing a sticker facilitated by the drivers' uncle, which showed that the car had links to the intelligence services, one of the most powerful networks within the state administration. As a result, each time soldiers saw the sticker, they were intimidated: they realized the driver may have powerful connections (protectors), and extorting nuisance taxes could generate harmful retaliation.

[5] Sanchez de la Sierra and Titeca (2016a,b) demonstrate the existence, and estimate the value of protection markets within the police administration.

bargaining power to protect against expropriation.[6]

We randomly assigned protection by ODEP to a separate sample of households and businesses. In this group, ODEP implemented weekly calls to gather data from the household about the universe of interactions with tax officials and administrations for the week. ODEP did not provide any tax consulting content received by the tax consulting treatment group described above. After collecting the data by phone on interactions, ODEP then guaranteed that they would investigate and act on instances of abuses through campaigns aimed at sanctioning the responsible administrators. Thus, ODEP took on the role of a powerful connection, as well as an intermediary with powerful networks within the state administration who can inflict harm on tax officials who commit abuses. In addition to passing along abuses reported by phone, however, ODEP can also draw on its credibility to undermine the reputation of individual tax collectors and tax collection agencies, thus allowing it to exert influence on the behavior of the supervisors towards mis-behaving tax officials.

Equipped with the credible backing of ODEP's involvement, drawing on the baseline knowledge of their tax liabilities, the selected citizens can credibly threaten tax officials who commit abuses, thus effectively reducing the equilibrium payments to administrators that they are not supposed to make. Later, ODEP then followed up with an anti-corruption campaign targeting tax officials in selected neighborhood. We analyze the impact of the campaign in Section 7.

4 Theoretical framework and testable implications

To illustrate the relationships that arise between citizens and administrators, we develop a simple model. There are two players, the "official" and the "citizen." The citizen has a true tax liability

[6] For obvious ethical reasons, we did not consider creating links with powerful Army commanders, an example of useful patrons, but instead, with an internationally and nationally renowned organization that has leverage at the top level of the administration, ODEP. "Re"-distribution indicates that, potentially, there is a limited supply of resources, and thus network links that can be maintained, and thus extending links to the weakest citizens potentially redistributes them away from where they would otherwise have existed. This, however, is a general equilibrium effect that we do not study in this paper.

τ^*, which the official knows but the citizen does not. Instead, the citizen has a common knowledge distribution of prior beliefs about his tax liability, $\hat{\tau} \sim F(\mu_\tau, \sigma_\tau)$. Since the citizen does not know how much she owes the government, she has the option of verifying her true tax liability at a cost. The cost of verifying τ^* is c_V. The term c_V captures the difficulty of gaining access to the tax code and the difficult of understanding the tax code. If the citizen decides to verify, then she pays the formal tax, and her expected payoff is $-\mu_\tau - c_V$, and the official's expected payoff is $r\tau^*$. We allow the official to receive a fraction $r \in \{0, 1\}$ of the total tax paid, τ^*, since in practice, this "formal" payment is formal insofar as it follows the liabilities established by the law, but nothing prevents the tax official from keeping a share of it, or engaging in rent sharing with his supervisor. Unlike in Khan, Khwaja, and Olken (2016), r thus does not capture the official piece rate to the tax collector: it may reflect simply that he is able to keep a share of payments we observe as formal. The official and the citizen may prefer to avoid the costly verification — note that the expected payoff of verification decreases in c_V but also on μ_τ, hence if the tax collector could, he has an incentive to manipulate μ_τ and c_V. In a collusion equilibrium, the tax official and the citizen are able to forgo the socially costly verification process, and bargain over the surplus left by not verifying the tax liability. Note that a collusion equilibrium may be extortionate or coercive: the tax official may know that $\tau^* < \mu_\tau$ and fool the citizen to extract a larger surplus; similarly, even if not credible, he may try to convince the citizen that $\tau^* - \mu_\tau$ is large, if he is able to overcome the problem of cheap talk; alternatively, he may be able to increase c_V through his actions, thereby increasing the surplus he can extract. Extortion can coexist with collusion here, as long as the parameters that determine the outside option can be manipulated by the tax official, or that he can take advantage of the citizen's wrong priors.[7] In a collusion equilibrium, the two players Nash bargain over a transfer, b, from the citizen to the official.

[7] In an extension, we examine the possibility of tax officials to communicate with households. While a message about the true liability may be cheap talk if all tax officials are opportunistic, as long as there exists a proportion of honest officials, it is possible that opportunistic investors take advantage of this to increase μ_τ.

The parameter γ is the official's bargaining power and $1-\gamma$ is the citizen's. If the players decide to collude, however, they face a cost of collusion that captures the risks associated with illicit transfers. Let the official's and household's payoffs under collusion be $b(1-c_c^O)-C_c^O$ and $-b(1+c_c^H)-C_c^H$ respectively. The joint surplus from collusion is now $S=\mu_\tau+c_V-(C_c^O+r\tau^*+C_c^H)-b(c_C^O+c_C^H)$. Note that the surplus decreases in b because the level of bribes increases the cost of collusion.[8] The Nash bargaining solution implies:

$$b^*=\gamma\frac{\mu_\tau+c_V-C_c^H}{1+c_c^H}+(1-\gamma)\frac{C_c^O+r\tau^*}{1-c_c^O}$$

The dollar amount of bribes that are non-zero increases in the bargaining power of the tax official, the mean of the household's prior distribution about her tax liability, and the cost of verifying such liability, which the tax official can take advantage of. The observed bribe decreases in the household's marginal and fixed costs of paying the bribe, and increase in the tax official's fixed and marginal costs of bribery. We can rewrite the total surplus as

$$S=\frac{1+(1-\gamma)c_c^H-\gamma c_c^o}{1+c_c^H}\left(\mu_\tau+c_V\right)-\frac{1+(1-\gamma)c_c^H-\gamma c_c^o}{1-c_c^o}\left(r\tau^*+C_c^o\right)-C_c^H\left(1+\gamma\frac{c_c^O+c_c^H}{1+c_c^H}\right)$$

Table 1 presents the testable implications. Bargaining is more likely to occur the higher is μ_τ, c_V, and the fixed and marginal cost of colluding to the tax official, c_c^O and $C_c^O+r\tau^*$. It is also increasing in the marginal cost of colluding to the household only if $c_c^O>1$, because while c_c^H decreases the surplus, for a given bribe level, it also nonetheless decreases the level of the bribe paid, which increases the surplus. The household fixed cost of bribery naturally decreases the likelihood of bribes. Overall,

[8] Note that in this case, the collusion payoffs are no longer the outside option payoff plus the bargaining weight times the joint surplus. To see this, let u_O be the payoff of the official and u_H the payoff of the household. Let $h(u_O)$ be defined as: $u_H=h(u_O)$. The Nash bargaining payoffs are given by: $-h'(u_O)=\frac{\gamma}{1-\gamma}\frac{u_H-d_H}{u_O-d_O}$, where d_i $i=O,H$ indicate respectively the no collusion outside options of the officer and household. Since the costs of collusion increase in the amount of the bribe, we have $h'(u_O)=-\frac{1+c_c^H}{1-c_c^O}$, thus, the NBS bribe is given by: $\frac{1+c_c^H}{1-c_c^O}=\frac{\gamma}{1-\gamma}\frac{\mu_\tau+c_V-C_c^H-b(1+c_c^H)}{b(1-c_c^O)-C_c^O}$ In simple problems of transferable utility, however, $h'(u_O)=-1$

μ_τ increases the level of bribes and the likelihood of bargaining (hence overestimation of the liability increases bribes and their occurrence), the cost of verification c_V increases the level of bribes and their occurrence, the marginal cost of the level of the bribe to the tax official increases the level of the bribe, but decreases the likelihood of bargaining because it decreases the surplus, and the marginal cost of the bribe to the household decreases the level of the observed bribes, and decreases the occurrence of bribery if the marginal cost of the tax official, c_c^O is small enough (smaller than 1). The fixed costs of colluding decrease the likelihood of bribery; however, while the household's private fixed cost of colluding decreases the average bribe, the fixed cost of colluding to the tax official increases the average bribe. Finally, note that the household's misinformation, $\mu_\tau - \tau^*$ increases the likelihood of bribes, whereas the true liability, τ^*, and the rate the inspector is able to keep, r, decrease the likelihood of bribes and increase the level of the bribes that do occur.[9]

Heterogeneous effects. Note that the main effect of changing the cost of verifying the true liability is $\frac{\partial b^*}{\partial c_V} = \frac{\gamma}{1+c_c^H}$. Two observations follow. First, since $\frac{\partial^2 b^*}{\partial c_V \partial \gamma} > 0$, interventions that decrease the cost of verification, have particularly large effects for households with weak bargaining power, the most marginalized. Second, since $\frac{\partial^2 b^*}{\partial c_V \partial c_c^H} = -\frac{\gamma}{(1+c_C^H)^2} < 0$, a reduction in the verification cost reduces the observed bribes, and less so if the cost curve of colluding for the household is steep. Thus, interventions that reduce the cost of verifying the tax liability have particularly strong effects on households for whom the cost of colluding does not increase very steeply in the amount of the bribe.

Treatment interactions. From the bribe and collusive surplus expressions it is straightforward to see that $\frac{\partial^2 b^*}{\partial c_V \partial c_c^O} = 0$, $\frac{\partial^2 b^*}{\partial c_V \partial C_c^O} = 0$. However, $\frac{\partial^2 S^*}{\partial c_V \partial c_c^O} < 0$, suggesting that while the tax consultancy

[9] To introduce coercion explicitly, consider that the tax official can manipulate the prior mean of the household at a cost. Concretely, consider that the cost of increasing μ_τ in one unit is $\Phi(m)$, where $\Phi(.)$ is an increasing and convex function and m stands for manipulation, whereby after manipulation, the posterior belief of the household is $\mu_\tau + m$. Rather than considering a communication game, simply allow the tax official to use his persuasiveness to intimidate the household about the true tax liability and influence his belief. Then, the tax official will maximize his payoff by choosing $\mu^* = \Phi'^{-1}(\gamma \frac{1-c_c^o}{1+c_c^H})$. Whenever $S = \frac{1+(1-\gamma)c_c^H - \gamma c_c^o}{1+c_c^H} \left(\Phi'^{-1}(\gamma \frac{1-c_c^o}{1+c_c^H}) + c_V \right) - \frac{1+(1-\gamma)c_c^H - \gamma c_c^o}{1-c_c^o} (r\tau^* + C_c^o) - C_c^H \left(1 + \gamma \frac{c_c^O + c_c^H}{1+c_c^H} \right) < 0$, then $m = 0$ and bargaining will not occur. However, m may not be zero when bargaining occurs, which depends on the shape of $\Phi(.)$, the cost of manipulating the household's beliefs. Similarly if we allow the tax official to manipulate the cost of verification.

treatment reduces the surplus available for bargaining, and thus reduces the occurrence of bribes, the tax consulting treatment is less effective if the protection treatment is also deployed.

5 Empirical strategy and analysis plan

This section presents the experiment design, the measurement strategy, and motivates with analysis plan with descriptive statistics.

5.1 Experiment design

We randomly sampled 576 households and 384 businesses on 96 avenues in Kinshasa to participate in household and business surveys.[10] From this pool, the research team recruited households and businesses to participate in an additional smart phone data collection activity.[11] A respondent was considered eligible for recruitment into the smart phone data collection activity if they were literate enough to read or write a letter in French and if the enumerator assessed them as having been willing to participate in the survey. If a respondent met these conditions and the target for the avenue had not yet been reached, the enumerator invited the respondent to take part in the smart phone data activity. Note that the targets for the avenues were per-determined and based on the first step of the random assignment, with a target of 200 households and 200 businesses. To ensure that the sub-sample of participants in the smart phone survey was random conditional on eligibility constraints, enumerators visited households on each avenue in a random order. Enumerators then invited households who agreed to participate in the smart phone data collection activity to attend training at the office of the research team in Kinshasa. A local research team then provided, at the trainings in the office, instructions on how to use the smart phones and on how to enter and upload their tax data

[10] Sampling was implemented in August and September 2015

[11] We further randomized how we framed the invitations to participate in the smart phone data collection activity for all those who were eligible. This is a separate experiment.

on a weekly basis for up to 20 weeks. The research team recruited households on a rolling basis as enumerators implemented the survey.[12] In return for their regular reporting, participants received a small compensation.[13] The training emphasized that the smart phone data collection activity was being undertaken by the same research team that had conducted the household and business surveys. A few days after the end of the smart phone training, individuals were contacted by an ODEP advisor to learn about the ODEP tax activities and to indicate their willingness to participate.[14]

The design followed the following protocols. First, ODEP activities are separate from the smart phone data collection activities to minimize the potential for reporting or social desirability bias. Second, participation in the smart phone reporting system was voluntary and unconditional. Third, the introduction script was generic and conveyed no mention to ODEP. A list of participants to the training activities was then passed to the research team, which implemented the randomization as described in the next section.

Two trained ODEP advisors (one specializing in household taxes and the other business taxes) implemented the treatments by calling participants on a weekly basis for five months. Each call followed a protocol with a highly structured format that followed the requirements of each treatment and minimized potential spillover in treatment content. Both treatments also emphasized that any data about payments provided by citizens would be kept strictly confidential so that any reports

[12] Approximately eight weeks of training were held.

[13] Participants were allowed to keep the smart phones at the conclusion of the study.

[14] The ODEP advisors used the following script: " I am a representative from ODEP, an emerging organization that works to improve the fiscal system in the DRC and to help households better confront the complex fiscal administration of the DRC, and the frequency of abuses by tax collectors. We are partly funded by DFID, the British development organization, and we sit at the table with the government in order to guarantee transparency of their decisions. We represent no political interest, except the interest of the people, and aim to improve the Congolese ability to operate in this predatory and confusing tax environment. You can contact us at x and our website is xxxx.xxx. We are in no way connected to the data collection training that you received or the data collection itself. We are contacting you because we have been informed you are concerned about your taxes, and we are going to make weekly calls to you in order to provide you with support on your taxes. We really hope that our support will help improve the fiscal problem in the DRC. Too many taxes are paid to private interests as a burden to households and we want to help you. Everyone would rather prefer that what you pay goes to public coffers so you can benefit from services the state owes you, isn't it the case?" The ODEP tax advisor then proceeded to obtain consent and record the contact information for those who were willing to participate in the ODEP consulting activities.

of abuses would not be linked back to them. In partnership with ODEP, we implemented the interventions described in Section 3, over-lapped in a 2x2 factorial design. The two treatments are summarized in Table A1 in the online appendix.

While the target sample of the experiment was 200 households and 200 businesses across the four experimental conditions, our final sample is 310 individuals, reporting daily data for up to 5 months. Taking into account the likelihood of potential spillovers if we were to assign individuals within avenues, we first randomly assigned avenues to treatment and control groups. In other words, of 96 avenues within Kinshasa, we assigned 48 to serve as a pure control and the other 48 to have ODEP activities. Within each of the pure control avenues, we set a target of one household and one business for the smart phone reporting (on two avenues we recruited an additional respondent), yielding a goal of 50 households and 50 businesses in the pure control.

The random assignment to specific ODEP treatments was done at the individual household or business level after obtaining consent. Taxpayers were randomly assigned to one of the three treatment groups (tax consulting, protection, and tax consulting + protection) blocking on strata formed by whether they were a household or business, commune, and framing experiment assignment. The target number of households and businesses to recruit into the smart phone data collection on ODEP treatment avenues was 200 households and 200 businesses across 48 avenues. Our final sample had 310. While we did not reach our recruitment goals, this does not create bias because randomization occurred within the recruited households and businesses, although it hurts our statistical power.[15]

[15] In actuality, due to challenges in the field, we recruited half of the target number of households and businesses. Note that this is not a compliance issue, rather an implementation failure that arises from management failures among the field teams.

5.2 Data collection strategy and measurement of outcomes

Our key outcome data comes from a smart phone application we developed for this project and distributed to households and businesses for daily entry, and weekly upload.[16] Participants in treatment and control groups reported weekly on what they had paid in formal and informal taxes, whether they had negotiated to lower their tax payments, whether that negotiation was successful, and their attitudes towards paying taxes. Since we made sure that the smart phone data collection activity and the ODEP tax intervention activities were independent of one another, we can be confident that any reporting bias is orthogonal to treatment assignment. We also draw on household and business surveys for key variables for checking balance, analysis of heterogeneous effects, and controls. To analyze payments, we use the estimated payments of informal and formal taxes. We allow for informal (and formal) payments to non-state actors. In addition, we also collected the following variables, which we will exploit in the analysis: Whether a negotiation occurred (HH Q9); starting amount, final amount, and difference (HH Q11-Q12); satisfaction with tax payment (HH Q15); and reasons for paying or not paying associated with bargaining (HH Q17-Q18). We next provide a rationale for the categorization of taxes by their degree of formality. Additionally, we use project implementation data that informs how the treatments were actually implemented.[17] In the remainder of the paper, we use the data collapsed at the week level for each respondent.

Figure A4 uses the survey data to validate the usefulness of the smart phone system. The smart phone system allows us to overcome under-reporting that may arise in retrospective surveys. As the figure shows, the average payments are higher in the smart phone system, likely because respondents

[16]Humphreys and Van der Windt (2014) use a similar strategy to collect village-level information about conflict events about 18 villages of Eastern Congo. The strategy in the current paper focuses on household-level payments to state officials, as opposed to publicly observable violent events, and we use a smart phone user friendly application. This allows us to decentralize the encoding of the information to the respondents thereby increasing the complexity of the information one can gather.

[17]This data includes information on how often participants were called by the ODEP advisors (client dataset), tracking sheets that provide detailed tracking data on the nature of each phone call (including what taxes were discussed, abuses reported, etc), and qualitative exit interviews conducted with recruited citizens at the end of the smart phone reporting period that checked on the quality of ODEP consulting.

do not need to recall their payments over long periods of time, and the proportions of formal and informal are similar.[18]

A key challenge is how to measure formal and informal payments. Definitions of what constitutes formal and informal taxation have been highly contested within existing research. Following recent work (Lough, Mallett, and Harvey, 2013, pg.3), we define taxation as "all payments—whether cash or in kind, including labor time—that are made as a result of the exercise of political power, social sanction or armed force." Within this definition, identifying and defining formal taxes is straightforward: Formal taxes refer to any compulsory tax or tax like payment stipulated in the statutory legal framework. At the local government level this includes levies formally referred to as "taxes", but includes licensing fees, rate and user fees for particular services. In practice, user fees are often particularly prominent as a means to finance services provision (Gibson, 1997). User fees are "imposed on specific persons, activities, or properties that receive a service or benefit" in return (Spitzer, 2012, pg.3). Common fees in developing countries like the DRC include those to access education and health services, obtain businesses licenses, or operate in markets (Weijs, Hilhorst, and Ferf, 2012) De Herdt and Poncelet (2011). Fees are often viewed as distinct from taxes because, unlike with taxation, there is a direct and immediate relationship between fee payments and the goods and services received in return. Yet, given the prevalence of user fees and the fact that they constitute compulsory payments in exchange for government provided goods and services, we also measure them.

Defining informal taxation is complex. This has given rise to contrasting definitions within existing research and policy discussion. In his classic work on informal taxation, Prud'Homme (1992) describes three types of informal taxes collected by state actors: 'pinch' informal taxes (the share of

[18] Furthermore, the survey data contains outliers that the smart phone data does not, suggesting potentially that recall induces distortions in the value of unusually large amounts. Note that because the baseline survey data covers the previous period, it is possible that the baseline survey has high seasonal payments that the smart phone period did not cover. The two measurements were not designed for comparison purposes, but instead the baseline survey role is to collect pre-treatment measurements.

formal taxes that are siphoned off by tax collectors and do not enter the formal budget); extortion (payments made to employees of semi-local governments in relation to authorization and rules); and requisitions (when government authorities ask enterprises and households to contribute to their activities). At the other end of spectrum, a recent study by Olken and Singhal (2011) defines informal taxes as a system of local public goods finance coordinated by public officials but enforced socially rather than through the formal legal system, thus focusing attention of informal taxation that gives rise to public services, and which may be collected by a combination of state and non-state actors, including traditional authorities. We define informal taxation as "all non-statutory payments—whether cash or in kind, including labor time—that are made as a result of the exercise of political power, social sanction or armed force (as opposed to market exchange)." This definition incorporates informal taxes by both state and non-state actors. Our research elsewhere indicates that informal taxation by non-state actors can be a critically important component of local tax collection. Furthermore, this definition is independent of how the funds are used. Whereas previous studies have, in some cases, focused attention only on informal taxes collected in exchange for public goods, or only on informal taxes embezzled by state officials, this definition avoids arbitrary distinctions. It can accommodate potential ambiguity about whether particular taxes are best understood as formal or informal.[19]

In this paper, we use multiple approaches to examine formal and informal taxation. We use three approaches to measure informal taxes (which we equally refer to as bribes here, and correspond to b in the theoretical framework).

We first obtain formality from the households and businesses self-reports if the payments they make are formal, state law backed payments, or instead informal payments to facilitate the process for instance. However, relying on households' self assessment of formality is problematic on multiple grounds. To begin with, a motivation of this paper is precisely that households do not know what

[19]See Prichard (2015).

their legal liabilities are, hence relying on self-reported formality may contain biases. Furthermore, the treatments themselves may induce households to relabel taxes between formal and informal in their reporting, without changing the payments. This can induce non-classical measurement error correlated with the treatment. Also, we know that a large fraction of payments made by household are "formal" in the sense that they are payments they should make according to the law, but are nonetheless bribes. There is a sense of formality in the social convention of paying the statutory taxes to tax officials, even if it is common knowledge that these will be used for private consumption of the official and his superior.[20]

Second, we use the pre-treatment survey data to construct scores of formality of each tax category. There is variation in the proportion in the survey of self-reported proportion of formal taxes in each category. To construct a measure of formality of payments where self-declared formality is not endogenous to the treatments, we use these scores in the main subsequent smart-phone analysis to estimate, probabilistically, the share of payments that are formal. This allows us to capture changes in payments that are immune to relabeling/non-classical measurement bias, since relabeling would only occur within categories.

Third, since self-reporting the formality of a payment, and its meaning, raises concerns of non-classical measurement error, we can focus on total payments, where predictions are immune to endogenous relabeling by households. Any payment to a tax official in the DRC has no guarantee to end up in the state coffers, hence one approach is to consider payments to tax officials who conduct visits to be bribes — formal taxes would instead be paid at the office.

[20] Figure A3 corroborates this interpretation: even though we know the level of illicit payments is huge, only 20% are self reposted as "informal" by households.

5.3 Descriptive statistics

Our sample of smart phone subjects totals 310 respondents, who submitted 3,894 smart phone application entries - weekly entries containing daily payments - about 13 submissions per respondent on average. There is a total of 6,419 tax payments and 1,067 tax payments being demanded but not paid. There were 1,204 surveys that were submitted without any taxes paid or demanded. Total taxes paid are $145,167.75, where formal taxes account for $100,453.90 and informal taxes for $21,970.56 (about 21 percent). A total of 4,529 payments included formal payment and 1,962 (or 40 percent) of payments were informal (in-part or completely). We next present the distribution of the main variables.

Table 2 shows the summary statistics. On average a respondent reported $18.7 of tax payments per week, $14.6 of which can be classified as formal and $2.4 as informal payments.[21] Respondents reported 1.3 tax payments per week and refuse 0.2 payments. They reported negotiating the size of the payment in 0.4 times per week. Turning to respondent level covariates, forty percent of subjects are female, the average education is post secondary school, the average household size is 6 individuals. To construct a measure of the value of the existing social networks of the respondent, we asked whether the respondent had any link with individuals inside the 8 most powerful state agencies/networks of power. These include the migration agency (DGI), the Kinshasa provincial tax revenue agency (DGRK), the commune, the neighborhood (Quartier), customs, the police, the army, and the secret service (ANR, refered to as Intel. in the tables). We then construct a standardized measure of the value of connections, which we label a zscore. The zscore uses the standardized values of each reported network links of the respondent with powerful state networks. Such networks include the intelligence services, the military, the police, and other agencies. For each agency, a score of zero

[21] The difference between total and the sum of formal and informal stems from payments where the respondents did not distinguish between formal and informal payments. In the analysis we will test whether the results hold for different ways of treating these observations

is assigned if there is no network link, and a fraction from zero to one is assigned, proportionally to the rank of the person the respondent knows within that agency. We then standardized the network variable for each agency, and standardized their mean. This provides the resulting zscore. Figure 3 presents the cumulative graph of payments over time. The submission rates, the number of payments, and the proportion of total taxes to formal and informal taxes remains stable from the start to the end of the experiment. Figures A3 and 4 pool the submissions and disaggregate the data by total, formal, and informal payments. Figure A3 shows the total taxes, formal taxes and informal taxes by type of respondent. While the sample is fairly balanced by type, households report almost triple the tax payments in all three categories compared to businesses. Both businesses and households report that approximately 80% of their payments are "formal" and 20% "informal". Figure 4 shows that approximately one third of the payments are made to non-state actors. Our survey allows us to break down tax payments by tax category. Tables 3 and 4 report the different categories of payments, for households, and for businesses. Respondents reported 31 coarse tax categories that vary starkly in terms of total amounts paid, formality, and frequency of payments. Educational taxes and taxes on life events such as weddings, births, and funerals signified the highest monthly average. Taxes on education are also among the most frequent together with religious taxes, and sanitation taxes which all are paid at least once a month. Figure A1 presents the average household monthly payments by household wealth. Figure A2 breaks down payments by categories, and shows that poor households have lower expenses of physical goods and transportation.[22]

5.4 Analysis Plan: empowering citizens

In what follows we present a pre-analysis plan. Given the exploratory nature of this project, while we will report the results on the pre-analysis plan as shown below, we will not constrain our analysis

[22]Because of the presence of extreme outliers, we run all regressions using the log of the dependent variables.

to this pre-analysis plan (Humphreys, Sanchez de la Sierra, and Van der Windt, 2013b). Precisely because of the opportunities for learning in such a complex environment, we expect to learn mostly from our exploratory results. The confidence intervals in such results must of course be interpreted with caution, but with sufficient robustness checks, we hope to find new results that reflect meaningful processes useful for learning.

We presented our main testable implications for the average treatment effect for each treatment in Table 1. As shown in Table 1, in addition to estimating average and conditional treatment effects, we are also interested in exploring how treatment effects vary for different subpopulations. For the heterogeneous effects, to measure the social connections of households, we use the knowledge of their networks to powerful actors as well as gender, ethnicity, level of income and wealth because they likely correlate to the households' value of social networks; to measure the distance of their priors from the statutory levels, we use the levels of education as well as self-reported measures of how well they know the tax code.[23]

Given random assignment, the main specification is a straightforward OLS equivalent to a comparison in means. However we consider the randomization blocks, treatment propensity, and the structure of correlations within the relevant units of randomization. Let Y_{it} indicate household (or business) i tax outcome (such as level of total tax payment) at week t; $P_i \in \{0; 1\}$ indicates whether household (or business) i was assigned to the *Power* treatment - power treatment indicates the creation of a network link with a powerful civil society organization; $K_i \in \{0; 1\}$ indicates whether household (or business) i was assigned to the *Knowledge* treatment - knowledge treatment indicates the weekly tracking and support to navigate the tax code and the existing complaint mechanisms; X' is a vector of time-varying (or constant) controls; ϵ_{it} is an additive error term. The fully saturated

[23] Additional heterogeneous effects will be estimated on prior history of tax bargaining (from the HH/business surveys), support for collusive taxation (from the HH/business surveys). We also use variables that are strong predictors of informal tax payments or select a priori, such as: Reporting duration (in case there was learning over time); Number of people doing smart phone reporting on each avenue (in case people helped each other).

regression is:

$$Y_{it} = \alpha + \beta_P P_i + \beta_K K_i + \beta_{PK} P_i K_i + X'\gamma + \epsilon_{it}$$

From this regression we can immediately recover the sample average treatment effects. The coefficient β_P is an estimate of the conditional effect of the power treatment (conditional on no knowledge treatment), and so respectively for the coefficient β_K; the average unconditional treatment effect of the power treatment is $\beta_P + \beta_{PK}$, and correspondingly for the knowledge treatment. Finally, the interaction term β_{PK} captures the marginal effect of the power treatment for the population where the knowledge treatment was implemented, compared to the population where it was not. We next discuss blocking and standard errors.

Across specifications, we also consider randomization blocks fixed effects. Since the treatment was assigned within recruitment week, commune, household/business, we use block cells defined by the interactions between all these dimensions. Since we have repeated observations of the same household, we must account for the correlations that would otherwise occur in Y_{it} within each avenue, and over time. For that matter, we present the results allowing the variance covariance matrix to account for (estimated) intra avenue correlation in ϵ_{it}. Furthermore, in the robustness, when we implement randomization inference, we replicate the assignment mechanism to account for any structure of correlation within avenues. In addition, we also use as robustness the cross-sectional specification, where we collapse the household data at the household level, a conservative way to account for autocorrelation over time (Bertrand, Duflo, and Mullainathan, 2004). Finally, we will present the results graphically to examine the divergence among the treatment groups over time.

As household level outcomes, we examine the effects on the total payments, total formal payments, and total informal payments, the frequency of visits by tax collectors for formal and informal taxes (broken down by type of tax). We focus our main controls on gender and education of the respondent because they allow to pool regressions of household and small businesses.[24] Furthermore, we conduct heterogeneity analysis on the following variables: household wealth, gender, ethnicity, household education households' prior social network (connections to different powerful actors, broken down by actor), frequency of visits prior to the intervention.[25]

6 Results (Pre-analysis)

This section presents the pre-analysis results.

6.1 The pre-analysis results approach

A fundamental problem of statistical inference is that since standard levels of statistical significance focus on the proportion of hypothetical samples in which the size of a given relationship would arise from pure sampling error, researchers interested in finding significant results can, after substantial search of variables, specifications, and samples, find significant results even if they reflect pure sampling error. Humphreys, Sanchez de la Sierra, and Van der Windt (2013b) demonstrate the pervasiveness of this (conscious or unconscious) "fishing" problem, using simulations on strategies to "torture the data" that result in "false confessions" by nature. Following Humphreys, Sanchez de la Sierra, and Van der Windt (2013a) and Humphreys, Sanchez de la Sierra, and Van der Windt (2013b), to prevent ourselves from conscious or unconscious biases that may arise in the process of analysis, and thus provide external confidence in the meaning of the confidence intervals we will report, we write this entire pre-analysis plan using fake treatment data. However, as discussed in Humphreys,

[24] We will further deploy regressions where the covariates are determined using LASSO. Since the intersection of controls between households and businesses is very small (gender, education, and frequency of tax visits per week of experiment), we will conduct LASSO separately for households and businesses as well. This allows us to include the following controls for both: tax burden (total, formal, informal, state non-state, by category), network connections; for household only: frequency of payments in the past year (total, formal, informal, by category), household characteristics (ethnicity, age, education and employment by member), migration background, perceptions; for business only: business characteristics (import export, registered, ownership structure, employees, inputs, sector).

[25] As well as heterogeneity by commune, since marginalization is spatially distributed.

Sanchez de la Sierra, and Van der Windt (2013b), important learning about the "measurement technology" occurs once the researcher has access to the data, which may allow the researcher to honestly improve the measures chosen, specification, and even the theory. To improve our ability to do so, we thus nonetheless use real data for the dependent variables and covariates, which allows us to examine where the relevant variation is without relying on ad-hoc ex-ante assumptions. Such assumptions would force us to jointly test a theory of human behavior given constraints, the type of the constraints themselves, and a theory about the appropriate measurement. We can circumvent such problems using real data for all variables except for the treatment, which we simulated for this draft. However, such plan will not bind us from learning further, and we will report separately learning that occurred between this pre-analysis plan, and the paper (whether by seeing the data, or for other reasons). Our model may be wrong and we may discover better theories to explain the empirical findings.

6.2 Main results

We first examine the first stage of the interventions, taking advantage of the exit survey we implemented on participants. At the end of the 5 months of smart phone data collection, we conducted phone interviews with respondents to measure the exposure with ODEP as well as the type of exposure. Table 5 presents the results of this validation exercise.

We then present the main results across dependent variables. Table 6 presents the results of the the main econometric specification. Column(1) presents the baseline specification without controls. Column(2) adds fixed effects (commune level, respondent type, framing treatment, and training week) and clustering at the respondent level. Column(3) adds controls taken from the baseline survey. Column(4) collapses the sample by individual (Bertrand, Duflo, and Mullainathan, 2004). Column(5) includes the interaction of the covariates with the treatment indicators (Lin, 2013). We report in the online appendix reports additional specifications to this result. Tables A2 to A3 present the results of regressing replacing total payments with formal and informal payments as well as the respective heterogeneity analysis for each variable. Table A4 presents the main result, disaggregating by payments to state and non-state actors. Finally, Table A5 presents the results, disaggregated by tax category.

We then turn to the change in the properties of negotiations with tax officials. The model predictions listed in Table 1 suggest that the protection intervention should reduce the occurrence of bribes, but that the average bribe level may increase, since tax officials for whom collusion is feasible require a higher payment for collusion not to break down. To distinguish these two potential effects, Tables A6 and 7 report the results regressing the number of payments made to tax officials on our interventions. Table A6 presents the baseline regressions. Table 7 presents the regressions disaggregated by type of payment — formal vs. informal. While columns (1) and (2) report the total, which potentially captures bribes of a varying degree of informality, columns (3) and (4) use instead the number of payments which smart phone holders report to be formal, and Columns (5) and (6) do the same for informal. Columns (7) to (12) replicate the main specification, but condition on a payment being made to separate average strictly positive payment from the non-payments. We then examine the effect of protection and information on the level of the average payment made by household. Table A7 presents the results. Columns (1)-(3) present respectively the effect on the average payment, the effect on the average formal payment, and on the average informal payment, and Columns (4)-(6) add the controls.

We also examine the effects on the following properties of the interaction with tax officials: frequency with which participants refused to pay a tax, frequency with which participants enter a negotiation process about their taxes, and frequency of attempted payments by tax officials. Table A10 presents these results respectively. Citizens which are more empowered should be more likely to refuse payments and negotiate for their taxes.

6.3 Heterogeneous effects

Table 8 presents the results of the main heterogeneous effects analysis. To capture the value of social networks, Table A8 replicates the main specification, using the properties of the respondent's network as interaction terms. Columns (1)-(10) report the regressions without treatment indicators. Finally, the remaining table breaks down the main result by the 9 broad categories of taxation to examine the distribution of the effect across different taxes.

7 Analysis of a targeted anti-corruption campaign

This section describes the impact of a city-wide, targeted campaign aimed at raising the cost faced by tax officials who commit abuses, in of randomly selected neighborhoods. This campaign should make it less likely that they engage in illicit tax extraction.[26]

7.1 Campaign experiment design

To make it costlier for tax officials to collect illicit payments from households, we focus mostly on extortionate bribes. Note that many illicit payments are actually formal, legal fees tied to a specific activity, that the collector nonetheless keeps for himself or his supervisors and exits the formal taxation channel as it flows upwards in the administration.

We take advantage of our existing partnership with the powerful civil society organization (ODEP), who, with the backing of international donors, has an effective track record at combating tax abuse by tax officials. Such organization has a strong reputation of being effective among households, as

[26]Theoretically, the effect will depend on the structure of the cost function. If tax officials perceive that they will be sued as a function of the amount collected, they may increase the number of occurrences in which they collect illicit amounts, and collect smaller amounts, thus spreading the collection across a larger number of households, and more difficult to detect. However, if tax collectors perceive that the effect will depend on the number of occurrences, they will have an incentive to concentrate tax extraction in fewer occurrences, but of a larger volume. While we have no prior over how a follow-up suing targeted, city-wide campaign translates into their beliefs about the cost function, we expect that it may discourage illicit tax extraction on both fronts. Overall, we expect the total amount extracted in targeted areas to be smaller.

our baseline data indicated, but it is also known that tax officials are aware of the consequences that may occur if such organization launches a campaign against them - shaming, reputation loss, job loss, or including prison. Again, this organization is policy relevant, especially in the context of a weak state like the Democratic Republic of the Congo, who does not have a state mechanism to ensure that corrupt tax officials are sanctioned. Drawing on such partnership, we organize with them a city-wide campaign in select neighborhoods, within each of all communes after the end of the third of fourth months of data collection, on December 1st 2015. The campaign was organized according to the following protocol. First, the organization leadership was given a set of randomly selected neighborhoods within each commune, where they were told to lead their campaign. Second, in each of the selected neighborhoods, which by the virtue of our prior randomization, there is comprehensive data on abuse, the organization had a list of abuses and tax collectors who had committed such abuses. Third, the organization organized meetings with the neighborhood leaders, and the respective community mayors, where they explained that they had the list of abuses in the selected neighborhood, and that they were going to launch a sanctioning campaign on the tax officials who were guilty of abuse. Fourth, importantly, the organization explained that they will intervene specifically in the selected neighborhoods because of lack of funds, and guaranteed that the neighborhoods not selected, they will not be able to intervene at all, and will never act upon the information they had collected, and importantly, the information they were collecting, about abuses. While we recorded no arrest or firing of tax officials during the month that this campaign lasted, it is nonetheless important to note that the campaign was in effect a credible threat, and taken seriously by the main actors involved. Figure 1 also corroborated that citizens trust the effectiveness of this organization more than any state and non-state organization.

The campaign started with meetings with community mayors and neighborhood leaders, where the organization explained they would start suing individual tax collectors suspected of abuse. They presented in each neighborhood a detailed list of abuses that they have recorded. In addition, they communicated with key relevant supervisors to communicate the abuses that took place in the selected neighborhoods. To the best of our knowledge, the campaign is limited to the implicit credible threat, where the organization credibly sends the message "we are watching you and we can take action again to publicize your otherwise unobserved behavior", and takes advantage of its reputation as an effective organization to combat tax evasion.[27]

7.2 Empirical strategy and campaign analysis plan

To estimate the impact of such campaign, we need to construct a counter factual group of areas that are protected from the campaign. Given that we had randomized the knowledge and the protection interventions, we thus begin by designing a randomization that is orthogonal to such interventions. Since randomization for such interventions was implemented within neighborhoods, and since the lowest administrative level at which action can be taken against tax officials is the commune, we were able to contact each of the 24 communes of Kinshasa, and within each, randomize the neighborhood where ODEP targeted the anti-corruption campaign. We drew lists of the universe of neighborhoods within each commune, and randomly selected half of the neighborhoods with equal probability. The selected neighborhoods are the target of the campaign: ODEP organized meetings where they announced the launch of the campaign, shared publicly the list of target neighborhoods, as well as shared the anonymized list of abuses that had been recorded in the first 3 months of data collection and ODEP communication with households and businesses part of the study. This provided a credible signal that ODEP had the capacity to watch, and that ODEP was ready to activate layers to sanction abusive tax officials in the selected neighborhoods. Yet, importantly, ODEP also guaranteed that they will never act on the information they have at hand in the control

[27]The campaign was also accompanied by radio components, public meetings, tracts, and public stickers. We report the minutes from the initial meetings and letters of invitation in the online appendix.

neighborhoods, and that tax officials could behave how they wish, because ODEP will not target corruption in such neighborhoods.

We can thus use a difference in differences strategy to examine the impact of the anti-corruption campaign. Let $POST_t \in \{0;1\}$ indicate whether week t is after December 1st, $TARGET_n \in \{0;1\}$ indicate whether neighborhood n was targeted by the campaign, and $Y_{i(n)t}$ indicate household level outcomes, such as tax paid. We estimate the effect of the campaign using the following specification:

$$Y_{i(n)t} = \alpha + \beta_P POST_t + \beta_T TARGET_n + \beta_{PT} POST_t * TARGET_n + \epsilon_{i(n)t}$$

The coefficient β_{PT} estimates the effect of the campaign on household level outcomes. As household level outcomes, we examine: the total formal taxes paid, the total informal taxes paid, the frequency of visits by tax collectors for formal and informal taxes (broken down by type of tax). Furthermore, since weaker households are likely to benefit most from this protection that they previously did not have, we conduct heterogeneity analysis on the following variables: household wealth, households' prior social network (connections to different powerful actors, broken down by actor), frequency of visits prior to the intervention, as well as commune, since households' marginalization is spatially distributed.

7.3 Results of the campaign experiment with simulated treatment data

We first present the results graphically. Figure 5 shows the evolution of total payments by households in target neighborhoods and in control neighborhoods. Next, we will deploy the regression framework to estimate the effect of the campaign. Table 9 reports the results from the econometric specification.

8 Conclusion

This paper has shown that the bargaining framework between citizens and powerful administrators is a promising area of research and policy. Starting from the observation that administrators in states where the democratic accountability is broken are able to extract payments from citizens, and that such payments are the result of bargaining, we designed and implemented interventions aimed at increasing the bargaining power of citizens in this negotiation, especially the most marginalized and uninformed citizens. Our results suggest that a major source of inequality is the distribution of bargaining power in society, determined in part by access to complex information about the law and access to social networks.

Tables and figures

Table 1: Testable implications

Quantity	Parameter	Average bribe	Frequency of bribes
Bargaining power, official	γ	$b\uparrow$	more iff $\mu_\tau + c_V$ large
Prior mean	μ_T	$b\uparrow$	more[a]
Cost of verification	c_V	$b\uparrow$	more[b]
Fixed cost of collusion, HH	C_c^H	$b\downarrow$	less
Marginal cost of collusion, HH	c_c^H	$b\downarrow$	less
Fixed cost of collusion, official	C_c^O	$b\uparrow$	less
Marginal cost of collusion, official	c_c^O	$b\uparrow$	less
Main effects			
Tax consulting (if over informed)	$c_V\downarrow,\mu_\tau\downarrow$	$b\downarrow$	less
Protection	$c_c^O\uparrow$, $C_c^O\uparrow$	$b\uparrow$	less
Other possible main effects			
Tax consulting (if under informed)	$c_V\downarrow,\mu_\tau\downarrow$	$b\downarrow$	less
Protection, $\mu_\tau + c_V > c_C^H + \frac{1+c_c^H}{1-c_c^O}C_c^O$	$\gamma\downarrow$	$b\uparrow$	less iff $\mu_\tau + c_V$ large[c]
Protection, $\mu_\tau + c_V < c_C^H + \frac{1+c_c^H}{1-c_c^O}C_c^O$	$\gamma\downarrow$	$b\downarrow$	less iff $\mu_\tau + c_V$ large
Heterogeneous effects			
Tax consulting, by initial prior μ_τ	$\mu_\tau - \tau^*$	increases effect	increases effect
Tax consulting, by HH weakness	γ	increases effect	increases effect
Tax consulting, by HH marginalization	c_c^H	dampens effect	dampens effect
Protection, by initial prior μ_τ	$\mu_\tau - \tau^*$	no difference	no difference
Protection, by HH weakness	γ	dampens effect	dampens effect
Protection, by HH marginalization	c_c^H	no difference	widens effect
Interactions			
Protection, Tax consulting		zero interaction	negative interactions

[a] This is true whenever $\gamma c_c^O + (1-\gamma)c_cH < 1$. Since, by assumption, $c_c^O < 1$ and $c_c^H < 1$, this is always true.

[b] This is true whenever $\gamma c_c^O + (1-\gamma)c_cH < 1$. Since, by assumption, $c_c^O < 1$ and $c_c^H < 1$, this is always true.

[c] Note that$\mu_\tau + c_V > c_C^H + \frac{1+c_c^H}{1-c_c^O}C_c^O$ will always be true if $b > 0$ as long as $\gamma \leq 1/2$. However, since we assume $\gamma > \frac{1}{2}$, this inequality is not always true. This is not always increasing since, while the payoff of the tax official always under bribery increases in γ, the bribe comes with a marginal cost so it may not.

Table 2: Summary Statistics

	N	Mean	Std. Dev.
Total tax payments in USD	4,495	18.7	42.1
Total formal tax payments in USD	4,495	14.6	33.7
Total informal tax payments in USD	4,495	2.4	6.7
Number of tax payments made	4,454	1.3	1.8
Number of payments refused	4,495	0.2	0.8
Number of payments where price was negotiated	4,495	0.4	1.0
Observations	4495		
Gender	289	0.3	0.5
Education (ordinal)	258	5.8	1.3
Household Size	139	6.3	2.7
Yearly household income in USD	121	8,476.0	46,737.0
Household wealth in USD	140	456,355.9	5,114,337.5
Business Profit in USD	68	2,554.4	7,345.7
Total tax burden in USD	252	2,808.4	9,218.6
Formal tax burden in USD	252	1,090.1	2,494.7
Informal tax burden in USD	252	1,662.8	8,625.7
Informal tax burden in USD: State	252	440.2	1,666.2
Informal tax burden in USD: Non-State	252	1,222.6	8,314.4
Know someone in DGI	254	0.1	0.3
Rank in DGI	245	0.1	0.3
Know someone in DGRK	254	0.1	0.3
Rank in DGRK	248	0.0	0.2
Know someone in Commune	251	0.2	0.4
Rank in Commune	240	0.1	0.2
Know someone in Quartier	246	0.0	0.2
Rank in Quartier	245	0.0	0.2
Know someone in Customs	252	0.1	0.3
Rank in Customs	249	0.0	0.2
Know someone in Police	250	0.4	0.5
Rank in Police	244	0.2	0.3
Know someone in Army	252	0.2	0.4
Rank in Army	249	0.2	0.3
Know someone in Intel.	251	0.0	0.2
Rank in Intel	249	0.0	0.1
Z-score Network	216	0.0	4.6
Observations	289		

Notes: Gender 0 signifies female and 1 male. The variable education indicates the education level, from 1 to 8 grades. Network variables measure whether the respondents knows someone from that agency and what position that person holds To construct a measure of the value of the existing social networks of the respondent, we asked whether the respondent had any link with individuals inside the 8 most powerful state agencies/networks of power. These include the migration agency (DGI), the Kinshasa provincial tax revenue agency (DGRK), the commune, the neighborhood (Quartier), customs, the police, the army, and the secret service (ANR, refered to as Intel. in the tables). We then construct a standardized measure of the value of connections, which we label a zscore. The zscore uses the standardized values of each reported network links of the respondent with powerful state networks. Such networks include the intelligence services, the military, the police, and other agencies. For each agency, a score of zero is assigned if there is no network link, and a fraction from zero to one is assigned, proportionally to the rank of the person the respondent knows within that agency. We standardized the network variable for each agency, and their mean.

Table 3: Average individual monthly payments by category - businesses

Category	Avg per month per resp ($)	Formal (%)	Informal (%)	# Payments	Refused (%)	To State actor (%)	Mean per payment ($)
Licensing tax	10.5	76.5	15.9	0.5	25.7	60.2	20.2
Tax on physical goods	7.3	58.0	6.1	0.1	7.0	32.4	53.1
Transport tax	5.3	85.2	11.3	0.2	3.3	23.2	29.1
Security or judicial tax	4.1	37.0	63.4	0.0	5.7	72.5	110.3
Tax on electricity/power	3.7	59.8	30.2	0.6	10.6	70.5	6.5
Water tax	3.0	72.8	17.4	0.2	6.1	80.3	12.3
Fuel tax	2.6	73.8	5.6	0.1	5.3	35.5	35.6
Sanitation tax	2.4	71.8	24.0	0.4	12.8	65.3	5.9
Labor tax	2.2	75.1	15.7	0.1	13.6	26.0	38.2
Maintenance tax	2.2	92.6	8.0	0.1	12.7	70.2	36.8
Communication	2.0	95.3	4.2	0.1	5.0	16.5	14.0
Sale tax	1.0	60.7	22.4	0.1	13.4	47.8	17.1
Tax on packaging	0.9	97.8	0.4	0.0	8.0	16.5	20.5
Profit tax	0.9	84.0	15.4	0.0	23.5	69.3	48.7
Tax on purchases	0.6	71.6	12.5	0.0	16.0	58.5	15.1
Other taxes	0.4	59.5	27.9	0.1	35.9	42.2	5.8
Storage tax	0.4	46.8	0.9	0.0	32.6	11.5	17.2
Marketing tax	0.3	80.7	9.1	0.0	29.8	47.6	7.6
Insurance tax	0.1	38.3	53.1	0.0	39.3	35.4	4.3
Excise tax	0.1	87.8	12.9	0.0	37.5	40.0	7.7
Media tax	0.0	100.0	0.0	0.0	17.6	0.0	3.0
Royalties	0			0			0

Notes: This table shows the average monthly payments of a business respondent by category of tax. Formal and informal payments are calculated as a percentage of the total individual monthly average. We also report the average number of payments made in each category per respondent and the amount of payments refused. This allows us to calculate the average payment per category.

Table 4: Average individual monthly payments by category - households

Category	Avg per month per resp ($)	Formal (%)	Informal (%)	# Payments	Refused (%)	To State actor (%)	Mean per payment ($)
Education tax	52.1	72.9	12.7	1.2	9.9	62.1	41.8
Tax on life events	22.7	63.2	18.5	0.4	9.2	43.7	63.8
Tax on physical goods	15.4	74.6	12.9	0.2	17.5	43.4	71.4
Transport tax	15.3	74.6	17.3	0.7	5.3	45.5	21.0
Religious tax	14.0	56.2	33.7	1.2	10.4	1.5	11.4
Water tax	13.5	69.2	15.1	1.3	13.5	72.4	10.4
Document	8.8	78.8	10.2	0.2	21.5	65.2	41.3
Security or judicial tax	5.5	89.1	8.3	0.1	14.7	77.9	74.4
Sanitation tax	3.7	64.4	14.1	1.5	7.2	49.6	2.4
Tax on salary	3.5	39.1	13.2	0.2	7.0	79.6	15.7
Other tax on public services	3.4	61.3	9.8	0.1	14.5	63.3	25.0
Business tax	2.9	69.0	8.3	0.4	29.7	39.3	8.0
Other taxes	1.1	84.6	7.2	0.1	26.4	45.5	18.0
Tax to local leader	1.0	58.7	41.8	0.0	5.9	93.7	57.1
Community tax	0.6	42.7	2.6	0.1	17.1	27.4	7.7

Notes: This table shows the average monthly payments of a household respondent by category of tax. Formal and informal payments are calculated as a percentage of the total individual monthly average. We also report the average number of payments made in each category per respondent and the amount of payments refused. This allows us to calculate the average payment per category.

Table 5: Compliance

	(1) Contact with ODEP	(2) Questions asked	(3) Abuse identified	(4) Info. provided	(5) Info. useful	(6) Advoc. announced	(7) Advoc. useful
Protection	-0.177	-0.170	-0.139	-0.203^{+}	-0.234^{+}	-0.335*	-0.0968^{+}
	(0.122)	(0.105)	(0.101)	(0.110)	(0.124)	(0.143)	(0.0542)
Tax consulting	-0.200^{+}	-0.147	-0.185^{+}	-0.173	-0.388**	-0.277^{+}	-0.0662
	(0.119)	(0.113)	(0.106)	(0.114)	(0.128)	(0.144)	(0.0561)
Both	0.350^{+}	0.424**	0.371**	0.399*	0.511**	0.399^{+}	0.163*
	(0.182)	(0.143)	(0.126)	(0.164)	(0.172)	(0.202)	(0.0689)
Observations	185	184	184	185	162	122	183
R^2	0.231	0.341	0.387	0.351	0.371	0.411	0.355
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Resp.	Resp.	Resp.	Resp.	Resp.	Resp.	Resp.
Sample	Resp.	Resp.	Resp.	Resp.	Resp.	Resp.	Resp.

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table shows the results of OLS regressions with the dependent variables being responses to the exit survey. The current version of the table has treatments drawn randomly from a uniform distribution as independent variables. Fixed effects (commune level, respondent type, framing treatment, and training week) are included in all specification. Column(1) presents the results of regressing whether the respondent was contacted by ODEP on the three different treatment categories. Column(2) asks whether the ODEP expert asked questions about the respondent's tax payments. Column(3) asks whether the expert identified abuse. Column(4) asks whether ODEP offered any sort of protection or advocacy. Column(5) asks whether ODEP provided information about taxes. Column(6) asks whether the information was useful and Column(7) asks whether the protection provided by ODEP was useful.

Table 6: Effect of protection and tax consulting on total payments

	(1) Total taxes	(2) Total taxes	(3) Total taxes	(4) Total taxes	(5) Total taxes
Protection	-0.642	-0.612	-0.204	-0.506	2.367
	(0.716)	(0.617)	(0.340)	(0.655)	(1.864)
Tax consulting	-0.641	-0.180	-0.0496	-0.224	0.307
	(0.725)	(0.593)	(0.323)	(0.613)	(1.670)
Both	1.810^{+}	0.904	0.544	0.428	-2.609
	(0.995)	(0.851)	(0.452)	(0.912)	(2.646)
Gender			-0.421^{+}	-0.528	0.399
			(0.241)	(0.487)	(0.485)
Education			0.0134	-0.267^{*}	0.00990
			(0.0983)	(0.126)	(0.114)
Log tax burden			0.0864	0.231^{+}	0.166
			(0.0527)	(0.124)	(0.104)
Network Z-score			-0.301^{**}	-0.295	-0.0104
			(0.102)	(0.206)	(0.197)
Visits in week			1.777^{***}	1.288^{***}	1.798^{***}
			(0.0816)	(0.209)	(0.150)
Observations	4377	4081	3573	231	3573
R^2	0.010	0.151	0.608	0.467	0.617
FE	No	Yes	Yes	Yes	Yes
Cluster	Respondent	Respondent	Respondent	Respondent	Respondent
Sample	Weekly	Weekly	Weekly	Respondents	Weekly

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table shows the results of OLS regressions with the dependent variable being total weekly taxes by respondent. The current version of the table has treatments drawn randomly from a uniform distribution. Standard errors are clustered at the respondent level. Column(1) presents the results of solely regressing total weekly taxes on the three different treatment categories. Column(2) adds fixed effects (commune level, respondent type, framing treatment, and training week). Column(3) adds controls taken from the baseline survey. Column(4) collapses the sample by individual and the dependent variable becomes total payments during the survey period. Column(5) includes the interaction of the covariates with the treatment indicators (Lin, 2013).

Table 7: Effect of protection and tax consulting on number of payments made, and average value per payment

	(1) # Total	(2) # Total	(3) # Form.	(4) # Form.	(5) # Inf.	(6) # Inf.	(7) Total taxes	(8) Total taxes	(9) Form. taxes	(10) Form. taxes	(11) Inf. taxes	(12) Inf. taxes
Protection	-0.121	-0.0170	-0.241	-0.103	0.134	0.164	-0.373	-0.230	-0.461^{+}	-0.247	0.335	0.127
	(0.235)	(0.232)	(0.254)	(0.242)	(0.191)	(0.205)	(0.286)	(0.277)	(0.269)	(0.240)	(0.353)	(0.394)
Tax consulting	0.0176	0.209	-0.141	0.225	0.344^{+}	0.434*	0.369	0.655*	0.210	0.575*	0.737*	0.593
	(0.207)	(0.214)	(0.292)	(0.236)	(0.188)	(0.205)	(0.289)	(0.278)	(0.271)	(0.252)	(0.350)	(0.377)
Both	-0.0684	-0.236	0.0189	-0.321	-0.391	-0.459	-0.00343	-0.161	-0.0598	-0.286	-0.272	-0.206
	(0.340)	(0.350)	(0.420)	(0.377)	(0.290)	(0.318)	(0.375)	(0.354)	(0.388)	(0.340)	(0.461)	(0.450)
Gender		0.000280		0.00533		-0.222^{+}		0.370^{+}		0.483**		-0.340
		(0.158)		(0.157)		(0.120)		(0.198)		(0.164)		(0.255)
Education		0.0222		-0.0240		-0.00976		-0.168**		-0.121*		-0.0513
		(0.0573)		(0.0614)		(0.0539)		(0.0520)		(0.0554)		(0.0622)
Log tax burden		0.0307		0.0156		-0.00739		0.115**		0.132**		-0.00915
		(0.0379)		(0.0437)		(0.0302)		(0.0398)		(0.0414)		(0.0600)
Network Z-score		0.0264		-0.0216		-0.0506		0.105		0.0690		0.160^{+}
		(0.106)		(0.145)		(0.0681)		(0.0920)		(0.0868)		(0.0920)
Observations	4041	3570	4081	3590	4081	3590	2112	1889	1942	1737	1436	1281
R^2	0.136	0.160	0.156	0.144	0.068	0.082	0.090	0.121	0.087	0.124	0.080	0.101
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Respondent	Respondent	Respondent	Respondent	Respondent	Respondent	Respondent	Respondent	Respondent	Respondent	Respondent	Respondent
Sample	Weekly	Weekly	Weekly	Weekly	Weekly	Weekly	Weekly	Weekly	Weekly	Weekly	Weekly	Weekly

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table shows the results of OLS regressions with respect to the number of payments. The current version of the table has treatments drawn randomly from a uniform distribution. We use fixed effects (commune level, respondent type, framing treatment, and training week) and cluster at the respondent level in all columns. Column(1) presents the results of regressing the three different treatment categories on weekly number of tax payments. Column(2) adds controls. Column(3) regresses weekly number of formal payments on treatments. Column(4) adds controls. Column(5) regresses weekly number of informal payments on treatments. Column(6) adds controls. Column(7) regresses the total weekly tax by individual conditional on a payment being paid on treatments. Column(8) adds controls. Column(9) regresses the total weekly formal tax by individual conditional on a payment being paid on treatments. Column(10) adds controls. Column(11) regresses the total weekly informal tax by individual conditional on a payment being paid on treatments. Column(12) adds controls.

Table 8: Heterogeneous effects, total weekly taxes

	(1) Total taxes	(2) Total taxes	(3) Total taxes	(4) Total taxes	(5) Total taxes
Protection	-1.242	2.694	-0.658	0.00735	-0.452
	(0.775)	(1.939)	(0.592)	(1.395)	(0.493)
Tax consulting	0.442	-0.164	-0.140	-1.253	-0.383
	(0.828)	(2.588)	(0.590)	(1.453)	(0.462)
Both	1.021	-3.967	0.992	0.764	0.896
	(1.025)	(3.834)	(0.838)	(1.967)	(0.642)
Gender	-0.495				
	(0.929)				
Gender X Protection	2.146^{+}				
	(1.283)				
Gender X Consulting	-1.643				
	(1.219)				
Gender X Both	-0.949				
	(1.737)				
Education		0.137			
		(0.204)			
Educ X Protection		-0.538			
		(0.352)			
Educ X Consulting		0.0389			
		(0.445)			
Educ X Both		0.769			
		(0.661)			
Network Z-score			-0.443		
			(0.308)		
Network X Protection			-0.470		
			(0.724)		
Network X Consulting			0.539		
			(0.466)		
Network X Both			0.883		
			(0.931)		
Log tax burden				0.0882	
				(0.209)	
Tax burden X Protection				-0.0285	
				(0.234)	
Tax burden X Consulting				0.257	
				(0.255)	
Tax burden X Both				-0.109	
				(0.324)	
Visits in week					1.596***
					(0.193)
Visits X Protection					0.0597
					(0.263)
Visits X Consulting					0.253
					(0.241)
Visits X Both					0.0266
					(0.347)
Observations	4081	3748	4081	3625	4052
R^2	0.165	0.177	0.157	0.169	0.587
FE	Yes	Yes	Yes	Yes	Yes
Cluster	Respondent	Respondent	Respondent	Respondent	Respondent
Sample	Weekly	Weekly	Weekly	Weekly	Weekly

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table shows the heterogeneous effects. Column(1) presents the results of regressing the three different treatment categories, the controls, and the interaction of the treatments with the gender variable. Column(2) looks at the interaction with education instead. Column(3) looks at the interaction with the network z-score. Column(4) regresses on the tax burden in the baseline survey and its interaction terms. Column(5) regresses on the total amount of visits in the week in question.

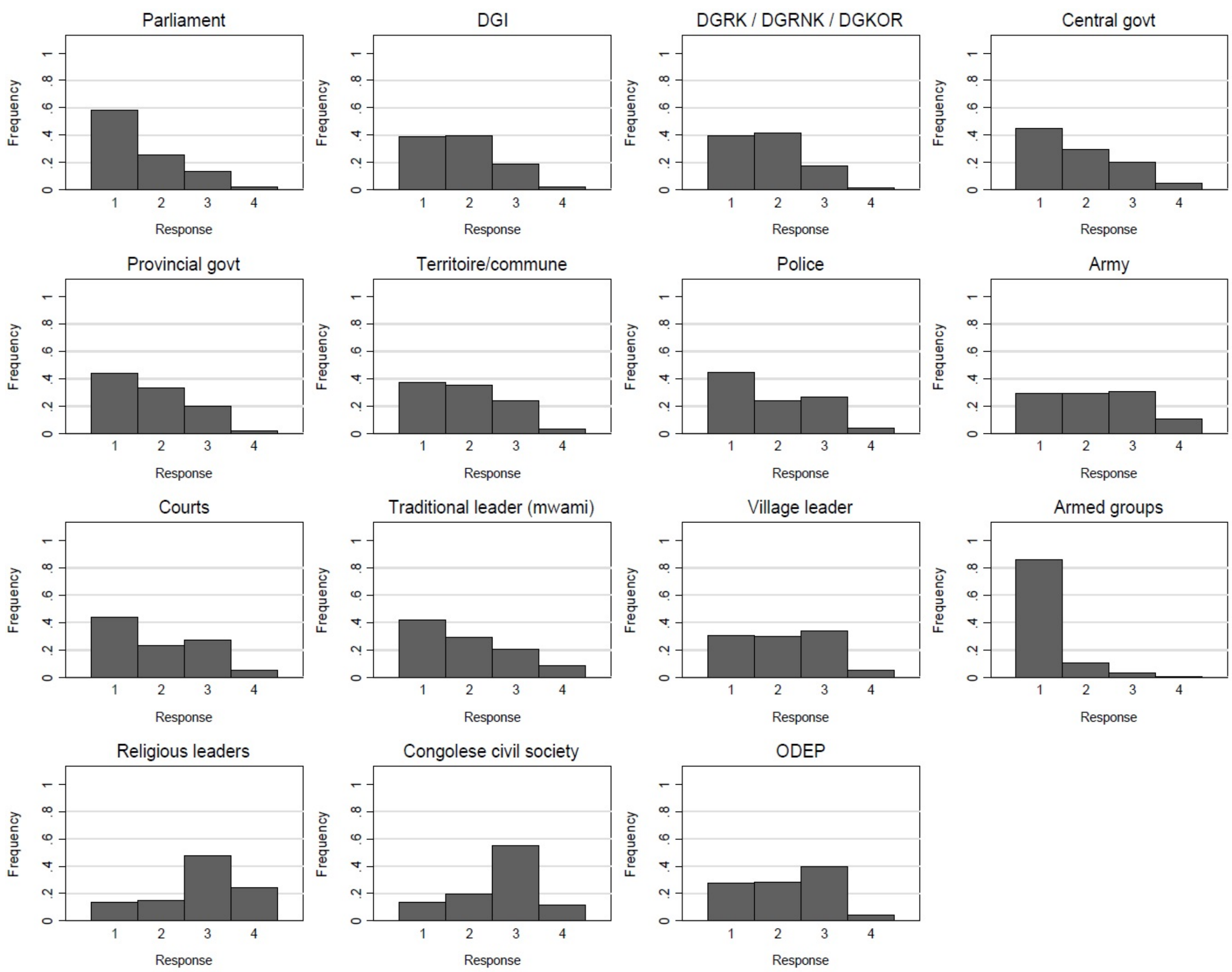
Table 9: Effect of anti-corruption campaign on value of payments made

	(1) Total taxes	(2) Total taxes	(3) Total taxes	(4) Total taxes	(5) Total taxes
Campaign	-0.322	-0.396			
	(0.439)	(0.247)			
Post	-0.930***	-0.0940	-0.692**	-4.680***	-0.922
	(0.207)	(0.162)	(0.220)	(1.071)	(1.329)
Campaign X Post	-0.376	-0.282	-0.351	-0.493	0.527
	(0.321)	(0.210)	(0.321)	(0.632)	(1.539)
Gender		0.284			
		(0.253)			
Education		0.129			
		(0.0957)			
Log tax burden		0.137**			
		(0.0426)			
Network Z-score		-0.360***			
		(0.0805)			
Visits in week		1.717***			
		(0.0812)			
Protection		0.0564			
		(0.205)			
Tax consulting		-0.0810			
		(0.209)			
Observations	3624	3395	3624	212	3395
R^2	0.014	0.572	0.476	0.060	0.612
FE	No	No	Respondent	No	Respondent
Cluster	Respondent	Respondent	Respondent	Respondent	Respondent
Sample	Weekly	Weekly	Weekly	Respondents	Weekly

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

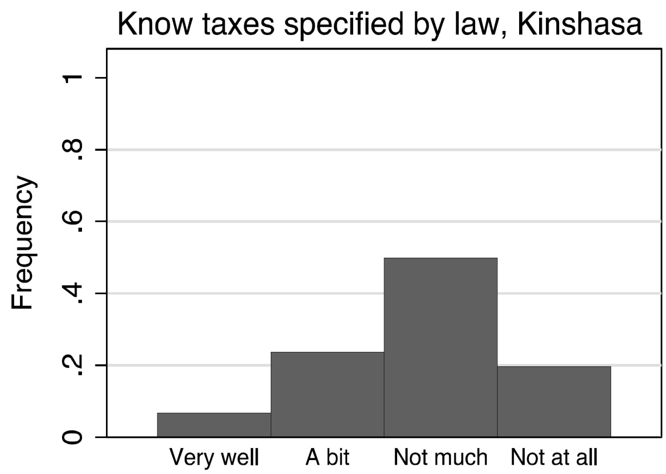
Notes: This table shows the results of OLS regressions that estimates the impact of the anti-corruption campaign, with the dependent variable of total weekly taxes by respondent. The current version uses a random draw for the treatment that differs from the actual treatment. Standard errors are clustered at the respondent level. Column(1) presents the results of solely regressing total taxes per week on the treatment. Column (2) includes control. Column(3) adds fixed effects (commune level, respondent type, framing treatment, and training week). Column(4) collapses the sample by individual and the dependent variable becomes total formal payments during the survey period. Column(5) replicates Column (3) but adds the interaction of the covariates with the treatment indicators (Lin, 2013).

Figure 1: *Trust in state agencies and in civil society organizations*



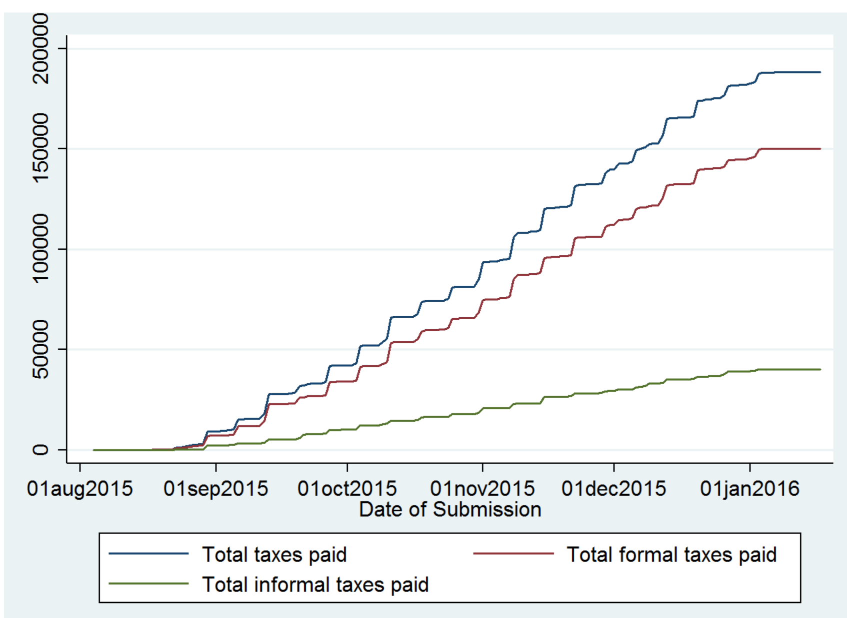
Notes: This figure shows the distribution of categorical answers to the question "How much do you trust the following organizations?". The possible answers were (1) very distrustful, (2) a little distrustful, (3) a little trusting, and (4) very trusting. The graphs display the share of respondents selecting each answer choice. Calculations incorporate survey weights. Source: total tax burden survey and authors' calculations.

Figure 2: *Knowledge about the relevant tax code*



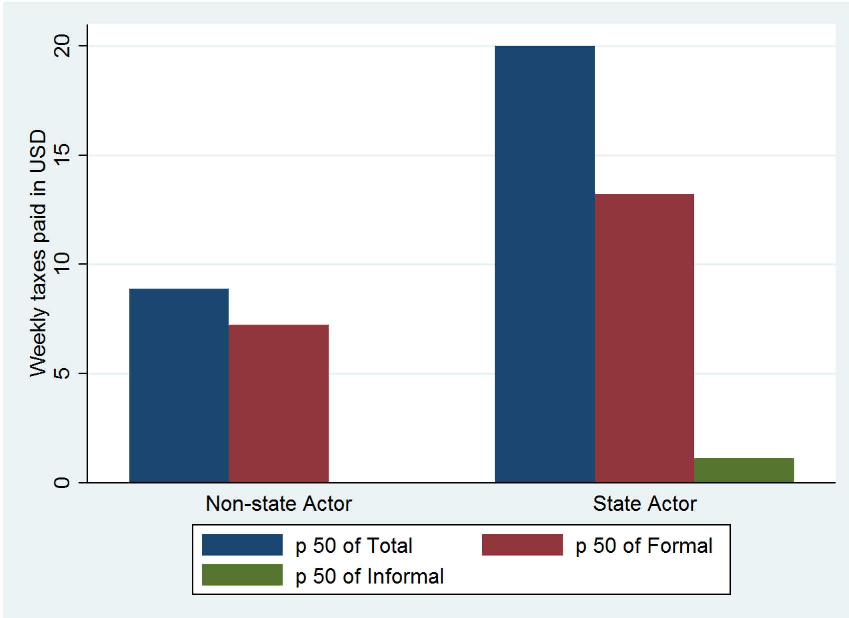
Notes: The graph shows opinions on how well respondents know the taxes that they have to pay according to the law. Results indicate shares of respondents indicating each of the choices. Respondents had to pick one of the following 4 choices: very well, a bit, not much, not at all. Shares add up to 1. Calculations incorporate survey weights. Source: total tax burden survey and authors' calculations.

Figure 3: *Formal and informal taxes over time*



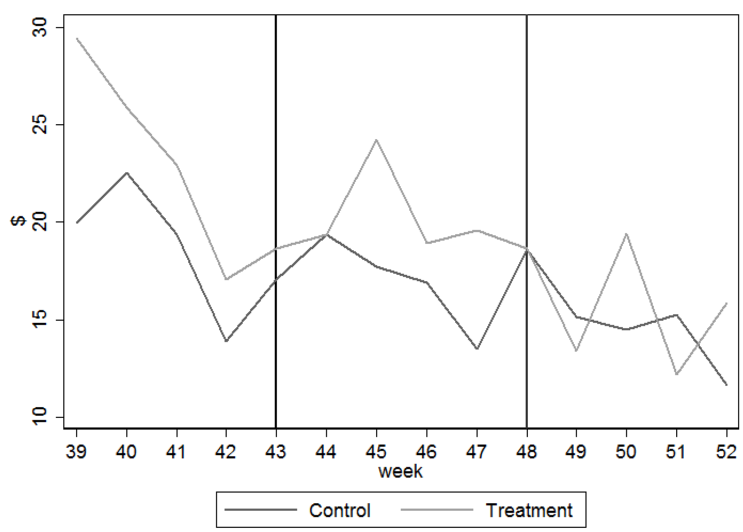
Notes: This figure shows the cumulative total taxes, informal taxes, and informal taxes over the time of the study starting in August 2015 and ending in January 2016

Figure 4: *State vs Non–state*



Notes: This figure shows median weekly total, formal, and informal tax payments by type of actor that collected the tax separated into state and non-state actors. Whether a payment is formal is reported by the respondent. In the analysis we deploy alternative measures of formality of payments. Note that many taxes paid to non-state actors are also in fact reported to be formal - these include education and church, for instance, considered to be formal becaue of the formality of the social norm.

Figure 5: *Average weekly payments, by targeting of the anti-corruption campaign*



Notes: This figure presents the evolution of average weekly payments by week for the 20 weeks of the study The vertical line indicates the timing of the neighborhood level city-wide anti-corruption campaign. Treatment and control neighborhoods were simulated with a fake assignment indicator.

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