Seeing like a Citizen: Experimental Evidence on How Empowerment Affects Engagement with the State *

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Abstract

Building a strong and effective state requires revenue. Yet, in many low-income countries, citizens do not make formal payments to the state, or forego engaging with the state altogether, due to vulnerability to opportunistic demands by state agents. We study two randomized interventions in Kinshasa, DRC designed to empower citizens in their negotiations with opportunistic state agents: one provided *information* about statutory payment obligations, the other offered *protection* from abusive officials. We examine the effects not only on citizen payment amounts (intensive margin effects) but also on whether citizens start making formal payments, or any payments, to the state (extensive margin effects). We find that protection, and to a lesser extent information, had clear extensive margin effects, increasing the share of citizens making formal payments and engaging with the state. These findings show how empowering citizens can help countries transition away from a low revenue, low engagement equilibrium.

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1 Introduction

Raising revenue and building state capacity are central challenges facing governments in many low income countries. In prominent theories of the 'fiscal contract', the need for revenue induces states to invest not only in building monitoring and extractive capacities but also in providing services to elicit more voluntary taxpayer compliance (Brennan and Buchanan, 1978; Levi, 1989; Besley and Persson, 2009).

For governments in many low income countries, the challenges of revenue collection are compounded by the fact that citizens often prefer to avoid the state. Many citizens live in informality, maintaining an "uncertain, undocumented, and irregular relationship to the state" (Gottlieb, 2024). Citizens often forego identity documents (Bowles, 2024), opt for private over state-funded services (Auerbach et al., 2018; Bodea and LeBas, 2016), and fail to register businesses (Joshi, Prichard and Heady, 2014) to avoid the costs of greater exposure to the state. However, in doing so they also forego the potential benefits of engaging with the state, including better access to services, legal protections, and economic opportunities.

This study examines the possibility of shifting away from a low revenue, low engagement equilibrium by empowering citizens to reduce their costs of interacting with the state. We focus on empowering citizens in their day-to-day interactions with opportunistic street-level stage agents, whose demands for informal payments in numerous domains—from registering property to acquiring licenses to accessing public services—can dramatically increase the costs to citizens of engaging with the state (Shleifer and Vishny, 1993; Banerjee, 1997).¹ ¹We define informal payments as any tax or fee payments to state agents in addition to or in place of legally mandated payments or amounts. Informal payments can take the form of illicit bribe or rent payments, voluntary 'pinch' payments to obtain services, or even informal payments that have become normalized and are perceived as legitimate (Prud'Homme, 1992; van den Boogaard, Prichard and Jibao, 2021). Importantly, bribes paid *in lieu* of formal payments can reduce the costs to citizens of accessing services (Shleifer and Vishny, 1993); Such demands abound due to inadequate wages, weak performance incentives, and poor state capacity to control state agents (Khan, Khwaja and Olken, 2016; Berwick and Christia, 2018; Martin and Raffler, 2021). For citizens, the costs and uncertainties associated with informal payments can deter engagement with the state given the wide range of tax and fee payments that state agents collect (Weigel, 2020; Khan, Khwaja and Olken, 2016; Bertrand et al., 2007; Olken and Barron, 2009). Similarly, vulnerability is known to discourage small firms from registering and paying formal taxes (Joshi, Prichard and Heady, 2014; Gallien and Boogaard, 2023).

We study the effects of two randomized interventions designed to empower citizens in their interactions with opportunistic or predatory state agents. The interventions were developed in collaboration with a Congolese civil society organization and implemented in Kinshasa, the capital of the Democratic Republic of the Congo. The *information* intervention provided households and businesses with individualized guidance on statutory payment amounts for a wide range of tax and fee payments.² The *protection* intervention connected households and businesses to an influential civil society organization capable of advocating on their behalves.

Our expectations about the effects of these interventions are informed by a baseline survey conducted with 1,067 households and business owners in Kinshasa, which revealed three main patterns. First, citizens make few tax or fee payments to the state, consistent with a high degree of informality. Second, formal and informal payments are positively correlated for households and businesses that do make payments, suggesting that these payments are complements rather than substitutes. Third, most payments made by households and businesses are linked to accessing services, including payments related to education, electricity, water, sanitation, and accessing formal documents. Consequently, those who make

our model below examines how empowerment affects both these payments and informal payments made *on top* of formal payments.

²For ease of exposition, we refer to households and businesses jointly as 'citizens' throughout the paper.

more payments also tend to enjoy more benefits, indicating that service access comes with exposure to both formal and informal payments. The baseline survey also suggests that households and firms face potential information and power deficits vis-a-vis state agents in relation to these payments.

To capture these patterns we develop a formal model in which citizens make two decisions: whether to engage with the state, which we conceptualize as deciding to make an initial payment to the state to obtain some benefit; and, conditional on engaging, whether to make a collusive payment to the state agent or insist on making a formal, legal payment to the state (e.g., by demanding a receipt).³ Citizens who insist on making a formal payment can still face demands for rents by state agents. Incorporating both citizen decisions into the model is the main innovation in our approach, allowing us to examine how empowering citizens by providing better information or greater protection—which we model as reducing bribe and rent payments—affects not only payment amounts but also the decision to interact with the state in the first place. Specifically, the model shows how information and protection can reduce informal payment *amounts* for citizens who are already engaging with the state (an intensive margin effect). More notably, empowerment can make some citizens more willing to engage the state, or to switch from making only informal payments to making formal payments (an extensive margin effect). We also show that empowerment can have a counter-intuitive effect: by increasing engagement with the state it can actually increase informal payments while still being welfare enhancing for citizens by improving access to benefits.

We test predictions on 271 households and businesses from neighborhoods in Kinshasa ³The model presented here is a revised version of the one presented in our original preanalysis plan. See Appendix C.8 for a discussion of the differences. The pre-registration was updated with the revised model before we analyzed the extensive margin effects. Our anonymized pre-analysis plans can be found at https://osf.io/s3gy6/?view_only= 1fa9a27f07394045aac2236d1ecf692a. that were randomly assigned to either a control group that only participated in data collection or to treatment groups that received either information or protection or both. We collected post-treatment data on a weekly basis for up to 19 weeks by having households and businesses use a smartphone application to report all payments made in the previous week. We analyze the effects of the treatments on a wide range of fee and tax categories.

We find strong evidence that protection, and to a lesser extent information, produced positive extensive margin effects. The protection treatment increased the number of citizens making payments for the first time or making formal payments. Citizens in the protection treatment made almost five additional payments during the reporting period. These results were largely driven by households rather than businesses. Notably, we find even stronger extensive margin effects of the protection treatment in neighborhoods in which a follow-on advocacy campaign—in which our partner civil society organization delivered on its promise to advocate for citizens—was conducted. We also find suggestive evidence that the protection treatment (and again, to a lesser extent, information) reduced payment amounts on the intensive margin, consistent with the prediction that empowerment should reduce informal payments amounts. These results support the conclusion that empowering citizens, primarily by strengthening their ties to an influential civil society actor, can increase citizens' willingness to make formal payments and, for some, to engage with the state in the first place.

This paper makes several contributions. First, it shows that empowering citizens can help shift states towards a higher revenue, higher engagement equilibrium. In doing so, we speak to a long-standing debate over the advantages and disadvantages of empowered citizens in the realm of revenue collection. On one hand, an empowered citizenry has long been seen as central to the emergence of durable fiscal contracts (Levi, 1989; North and Weingast, 1989) and, ultimately, to strong but constrained states (Acemoglu and Robinson, 2020). Yet, a competing view contends that empowerment might enable citizens to better shield themselves from an extractive state (Scott, 2010). Similarly, research on tax non-compliance often adopts the view that citizens are already too capable and savvy vis-à-vis the state, allowing them to evade taxes in the face of weak enforcement capacity (Allingham and Sandmo, 1972). We lend support to the value of empowerment, showing that it is beneficial both to citizens and the state to strengthen citizens' ability to engage in individual, noncollective negotiations of predatory demands. While our interventions empower citizens at the micro-level, our findings on protection also speak to the importance of having strong civil society actors capable of acting on citizens' collective behalves at the elite level, as emphasized in the broader literatures on fiscal bargaining and state-building.

This paper further contributes to empirical research seeking effective interventions to increase revenue collection in low income countries. Important recent studies on taxation have focused primarily on increasing revenue through state-centered interventions (Khan, Khwaja and Olken, 2016; Weigel, 2020). Yet, these studies acknowledge that increased collection also poses the risk of greater exposure to the state and demands for bribes. Our results suggest that empowering citizens could be an important counterpoint to more statecentered interventions.

For their part, more citizen-centered interventions to increase tax revenue have tended to focus not on empowerment but on improving tax morale or changing social norms (Allingham and Sandmo, 1972; Khan, Khwaja and Olken, 2016; Weigel, 2020; Luttmer and Singhal, 2014). In one exception, Martin et al. (2021) find that a 'bottom-up' intervention conducted with market vendors in Malawi not only increased taxpayer compliance but also empowered vendors to better advocate for themselves. One possible explanation for the relative lack of attention to direct empowerment interventions is that empowerment is often viewed as endogenous to taxation—state efforts to collect taxes are traditionally the catalyst that mobilizes citizens (North and Weingast, 1989; Paler, 2013; Martin, 2023; Weigel, 2020). Yet, in reality citizens face substantial barriers to individual and collective action, highlighting the potential for empowerment interventions to facilitate more effective bargaining and stronger fiscal contracts (Prichard, 2015). Our study is one of the first, to our knowledge, to capture the potential of direct empowerment—which aims to strengthen citizens' concrete ability to navigate the challenges and risks of engagement with the state—to improve revenue mobilization.

Finally, our most striking finding is that empowerment interventions can lead not only to more formal payments but also to a greater willingness to interact with, and by extension become visible to, the state. This is a central concern of research on formalization and legibility (De La O, 2022; Bowles, 2024; Lee and Zhang, 2016), which has generally theorized that citizens decide whether to become legible to the state by weighing the benefits against the transaction costs of registration and future formal tax payments. Interventions to reduce informality have tended to focus on lowering the direct costs of registering, with limited success (De La O, 2022; Jaramillo Baanante, 2009; de Mel, McKenzie and Woodruff, 2012). Our study provides a potential explanation for the limited success of such interventions: they fail to consider, and address, the potential informal costs to citizens of becoming more visible to the state. We show that empowering citizens, and thus reducing opportunistic benefits to state agents, could make citizens more willing to start making payments to the state, with important implications for increasing citizen legibility and strengthening state-building processes.

2 Context and Motivation

Increasing formal revenue collection has long been a central challenge in the DRC, as in many low income countries in sub-Saharan Africa and beyond (Van Reybrouk, 2014; Van Damme, 2012). In ostensible recognition of the need to improve revenue collection and bring fiscal governance closer to the people, the central government in Kinshasa undertook sweeping fiscal decentralization reforms in 2008. The result has been an even greater proliferation of informality in revenue collection as local state actors use their political influence or exploit citizens' confusion about statutory payments (Englebert and Kasongo, 2014; De Herdt, Titeca and Wagemakers, 2010). Owing to inadequate central government funding, both state agents' salaries and state-provided services are often funded, at least in part, by informal payments by citizens (Englebert and Kasongo, 2014; Weijs, Hilhorst and Ferf, 2012; Titeca and Kimanuka, 2012). These payments can compound the cost of services, inducing many citizens to opt out of accessing benefits that are viewed as less essential; for those who want to access key services, such informal payments are often required.

To gain a better sense of the kinds of payments that citizens make, we conducted a baseline survey with 533 households and 534 businesses randomly sampled in Kinshasa.⁴ The survey measured self-reported payments and amounts across 18 categories for households and 22 categories for businesses, where formal (informal) payments were defined as legally (not legally) mandated (see Appendix A1 for payment category details). Our analysis of the baseline data reveals several patterns that inform our theoretical model.⁵

First, the baseline survey confirmed that citizens in Kinshasa rarely engage with the state to pay fees. Most households and businesses make no payments to state officials across most payment categories (see Table 1). For businesses, the median payment is non-zero in only one out of 22 tax categories: electricity. The 90th percentile value is non-zero in only four tax categories. Similarly, for households the median payment is non-zero in only three out of 18 categories—education, electricity, and water—with 90 percent of households making no payments in 11 categories.

Second, citizens who do make payments tend to make both formal *and* informal payments. ⁴We used a multi-stage cluster sampling strategy where street segments ("avenues") served as the primary sampling unit. The surveys were implemented from August-September 2015. ⁵Given uncertainty around statutory payments and amounts in the DRC, citizens may not be aware that their informal payments are informal. In light of this, and general concerns about measurement or reporting bias with self-reported data, we explore other approaches to coding formal and informal payments. The overall patterns in the data are not sensitive to coding approach.

		1	Household	s				Firms		
	Mean	SD	Median	p75	p90	Mean	SD	Median	p75	p90
Electricity	115.4	201.3	62.2	133.33	271.1	67.4	211.5	5.3	60.0	138.9
Property	17.4	144.4	0.0	0.0	6.7	5.7	128.5	0.0	0.0	0.0
Sanitation	71.9	514.1	0.0	3.0	66.7	19.4	76.1	0.0	0.0	51.6
Security/Judicial	0.0	0.8	0.0	0.0	0.0	3.6	49.9	0.0	0.0	0.0
Transport	16.2	120.4	0.0	0.0	0.0	22.4	273.0	0.0	0.0	0.0
Water	152.0	236.3	66.7	200.0	393.3	39.3	211.9	0.0	22.2	66.7
Animals	8.3	103.0	0.0	0.0	0.0					
Business	0.2	3.5	0.0	0.0	0.0					
Community	5.7	100.5	0.0	0.0	0.0					
Customary	0.1	1.0	0.0	0.0	0.0					
Documents	6.5	45.0	0.0	0.0	0.0					
Education	659.7	1,722.9	230.0	694.4	$1,\!676.7$					
Health	34.1	321.4	0.0	0.0	0.0					
Life events	92.9	455.0	0.0	22.2	200.0					
Public/legal	32.0	621.4	0.0	0.0	0.0					
Revenue	6.5	55.8	0.0	0.0	0.0					
Vehicles	10.3	107.6	0.0	0.0	0.0					
Communications						11.4	170.8	0.0	0.0	0.0
Contracts						0.0	0.3	0.0	0.0	0.0
Customs						6.0	91.7	0.0	0.0	0.0
Fuel						5.9	95.1	0.0	0.0	0.0
Insurance						0.0	0.9	0.0	0.0	0.0
Labour						8.8	68.4	0.0	0.0	0.0
Licensing						85.3	896.3	0.0	42.2	115.6
Maintenance						0.9	10.0	0.0	0.0	0.0
Marketing						3.9	58.5	0.0	0.0	0.0
Other Taxes						4.3	96.2	0.0	0.0	0.0
Packaging						4.7	80.7	0.0	0.0	0.0
Printing						0.3	5.6	0.0	0.0	0.0
Profit						2.6	20.5	0.0	0.0	0.0
Royalties						0.0	0.0	0.0	0.0	0.0
Sales Tax						31.7	443.9	0.0	0.0	0.0
Storage						1.3	23.8	0.0	0.0	0.0
All Categories	$1,\!424.8$	$2,\!439.1$	677.7	$1,\!554.2$	$3,\!507.2$	324.8	$1,\!422.9$	66.7	191.1	543.33
Observations	533					534				

Table 1: Yearly total payments per firm by category in USD

Survey respondents indicate that 64 percent of payments involved both formal and informal amounts while only 14 (22) percent involved exclusively informal (formal) payments (see Appendix B.1). While previous studies suggest that formal and informal payments might be substitutes (Shleifer and Vishny, 1993), Figure 1 shows few observations with positive informal payment and no formal payment amounts, which would be the expected pattern if informal payments substituted for formal ones. Rather, Figure 1 shows a positive correlation between formal and informal payments for both households and businesses. The bulk of these payments are user fees for services like water, electricity, sanitation.

Figure 1: Formal and informal payments for businesses and households



Notes: The top panel shows results for businesses where x's represent not formalized businesses and circles represent formalized businesses and bottom panel for households.

Third, most payments are motivated by service access, with those households and businesses that do pay also tending to enjoy greater benefits, consistent with a story in which accessing benefits entails paying formal and informal costs. Households list access to services as the main reason for making over 70% of payments, and households that pay more enjoy better access to services (see Appendix Table A1). Similarly, while the bulk of unregistered firms reside close to the origin point of Figure 1, formalized businesses have greater profits, revenues, numbers of employees, and electricity use (see Appendix Figure A3). Yet, this engagement with the state also comes at a cost. A multi-variate regression of formal and informal payments on an indicator for business registration suggests differences of \$184.10 and \$52.31, respectively (see Appendix Table A3). Overall these results suggest that while it might not be possible to escape the state entirely, households and businesses do remain hidden from state agents in many domains *if* they are also willing and able to forego access to state-provided goods and services.

The formal model in the next section builds on two additional characteristics of the Kinshasa context, confirmed by both our baseline data and qualitative research. First, citizens and state agents frequently bargain over payments, with households and businesses reporting a high degree of variation in the percent of payments that were negotiable across fifteen different payment categories (see Appendix Table A1). For instance, households report that only 10 percent of their education payments and 21 percent of their water payments are negotiable, but 78 percent of electricity payments are negotiable. In contrast, for businesses, 39 percent of electricity payments are negotiable. Qualitative data collection summarized in Appendix Figure A1 helps to shed light on this variation in negotiability.

Second, our baseline data (along with qualitative reports) captures the extent to which citizens in Kinshasa face both power and information deficits in their interactions with streetlevel state agents. Power asymmetries vis-a-vis state agents arise from the fact that many households and businesses lack connections to officials or other influential actors who can intervene to prevent a state agent from extracting informal payments (Sánchez de la Sierra et al., 2024). Indeed, the median household knows no official in the tax agencies, local government, or security services who could intervene on their behalf; the median business knows only one official (see Appendix Table A4). For their part, information asymmetries can exist because state agents typically have a better understanding of citizens' true payment liabilities than citizens themselves. While some statutory payment amounts are more transparent, others may be obscured by their complexity or their reliance on consumption readings by state officials (e.g. for electricity or water). Appendix Table A1 shows the proportions of citizens who state that they do not know their statutory payment obligations. For instance, respondents reported not knowing the statutory payment amounts for 32 percent of education payments and 50 percent of sanitation payments.

This baseline data is consistent with anecdotal evidence of bargaining under information and power asymmetries in the DRC. Our interviews revealed, for instance, that households and businesses with state-provided electricity are frequently visited by state agents who demand payments, threatening to cut-off electricity access. While some citizens pay a negotiated amount, others who are well-connected call a friend or family member in a position of authority to intervene on their behalf. To avoid these encounters altogether many choose to forego state-provided electricity entirely, opting instead for informal or illegal arrangements (Banza et al., 2022, see also Appendix Figure A1). Households and businesses report similar interactions in numerous domains—from state agents calling on businesses to check for permits to tax collectors arriving at properties to elicit property tax payments from tenants that are also being collected from landlords—reinforcing that similar dynamics are repeated across a wide range of tax and fee payments.

That said, not all negotiations are equally subject to both information and power asymmetries. In some cases, citizens might lack both certainty over statutory payments *and* connections to influential allies. In other cases, citizens might have more certainty over statutory payments but lack the bargaining power to negotiate favorable arrangements. The fact that both information and power asymmetries are prevalent but variable across payment types and citizen types (citizens differ in their information and power endowments) makes it difficult to know a priori how to empower citizens most effectively. In the following section, we formalize how providing more information and greater protection could mitigate these asymmetries and empower citizens in their negotiations with state agents over a wide range of payment types.

3 Theoretical Framework

This section presents a theoretical framework that captures the costs and benefits of engaging with the state and the consequences of doing so for formal and informal payments. Our model builds on the findings that formal and informal payments are complements instead of substitutes and that citizens often bargain with state agents over informal payments. A citizen might be reluctant to bargain if they lack information on their true payment liability or feel vulnerable to state agents. We thus consider comparative statics on how improving knowledge about statutory payments (a goal of the information intervention) and lowering rents that government agents can extract (a goal of the protection intervention) affect how much citizens pay when bargaining collusively with state agents as well as citizens' willingness to engage state agents in the first place.

We believe the model explains dynamics around a wide range of payments to the state. While payments differ in important ways—for example, how closely they are linked to benefits and the extent to which they are vulnerable to information and power asymmetries—the baseline data reveals common patterns across a wide range of payments that this model seeks to explain. The model explains payments insofar as they are both *voluntary* (meaning citizens have some choice over whether to pay) and *linked to a benefit*, regardless of whether the benefit is immediate (as with receiving a service in exchange for a fee) or more distant (as with obtaining greater protection under the law due to paying property taxes).⁶ We return

 $^{^{6}}$ A payment would be *involuntary* if there were inelastic demand for a state-provided benefit

in the conclusion to discussing the model's scope conditions (along with possible extensions).

We briefly summarize the game here, as shown in the decision tree in Figure 2. An extended formal solution to the game can be found in Appendix Section C. The game begins when the citizen decides whether to engage with (make an initial payment to) the state in order to obtain a benefit.⁷ If the citizen engages, they have a true payment liability τ^* , which the state agent knows but the citizen does not. Instead, the citizen has a prior belief about her payment liability, μ_{τ} .

When engaging, the citizen can either (1) collude privately with the state agent over a bribe to be paid in lieu of the legal amount, or (2) insist on making an official, formal payment, for instance by demanding a receipt or insisting on conducting the transaction at an official state office. If the citizen insists on an official payment, they pay the formal amount and an additional transaction cost. Additionally, the citizen might also have to pay a rent r to the state agent, which captures that officials might use their power to extract illegal amounts on top of formal payments. Alternatively, if the state agent and citizen collude in private, they have the potential to avoid transaction costs from an official process and bargain over the surplus left by not making an official payment. We suppose that, when transacting privately, the citizen and state agent Nash bargain over the size of the bribe payment, b, from the citizen to the state agent.⁸

or if a state agent demanded a payment and there was no opportunity for non-compliance, which are both rare conditions. For instance, evidence from Kananga, DRC shows that attempts by state agents to collect property taxes often result in low compliance rates (Weigel, 2020).

- ⁷As detailed in Appendix Figure A1, in our empirical context citizens—not state agents commonly initiate engagement with the state. However, the logic of our model holds even if the state agent initiates the interaction as long as there is scope for negotiation or citizen non-compliance.
- ⁸In modeling r and b as distinct types of informal payments, we build on Shleifer and Vishny (1993), who distinguish between bribe payments with theft (equivalent to b in our model)



The citizen will bargain with the state agent if the expected bribe and cost of collusion (due to risks of being caught) are lower than the official payment, transaction cost of an official payment, and rent payment. The citizen will engage with the state if the relative benefits outweigh the expected payment liability, rent payment, and cost of a formal transaction (when the bribe is too high) or cost of collusion (when the bribe is low enough).⁹

There are two ways in which we expect empowerment to work. First, officials know the true payment liability, τ^* , while citizens only have a guess, μ_{τ} . We construe additional information as lowering μ_{τ} .¹⁰ We view protection as acting on r, the rent that officials are and without theft (equivalent to r). Both bribe payments and rents are also common in our empirical context (see Appendix Figure A1).

⁹We consider the benefits of engagement with the state (i.e., access to a state-provided good or service or to indirect benefits like greater protection under the law) versus obtaining the benefit from an alternative non-state option or foregoing the benefit altogether.

¹⁰See Appendix C for a discussion of why we interpret information as lowering μ_{τ} rather than reducing uncertainty around μ_{τ} . able to extract from citizens who make official payments, insofar as linking citizens to a civil society organization that will advocate for them should result in lower, or even zero, rent payments.¹¹

We derive intensive and extensive margin predictions for citizens. We note that we can derive predictions for the effects of decreases in μ_{τ} and r on official payments τ , bribes b, and rents r, as summarized in Appendix Figure 3 and Appendix Table A5. However, we state our hypotheses in terms of *total payments*. This is due to the empirical challenges of reliably distinguishing between formal and informal payment amounts in our self-reported data.

By intensive margin predictions we refer to the *amounts* paid by citizens who begin in either the collusion equilibrium or the official equilibrium and are not induced to switch by changes in μ_{τ} or r. Our intensive margin predictions vary depending on the type of empowerment and on whether citizens start in the collusion equilibrium or the formal payment equilibrium. In the collusion equilibrium, lowering the rent (r) reduces the payoff to the state agent when citizens insist on formality, which in turn makes state agents more willing to accept a lower bribe. Similarly, reducing μ_{τ} also reduces the bribe in equilibrium. When citizens are already making formal payments, lowering r directly reduces the citizen's total payment. However, changing μ_{τ} has no effect on the total payment amount on the assumption that once a citizen makes an official payment they learn their true statutory payment obligation.

The extensive margin predictions capture the effects of empowerment when citizens are induced to switch equilibria by the parameter changes. The model yields two extensive margin predictions: (1) changes to r and μ_{τ} will induce engagement with the state by decreasing the costs of bargaining, and (2) insisting on official payments will induce some to switch to the official payment equilibrium.¹²

¹¹Appendix C also explains why we believe protection operates on r and not a cost parameter. ¹²We note that the model has explanatory power even in contexts where there is no scope



Figure 3: Effects of intervening on r and μ_{τ}

Notes: These figures show the effect of changes to μ_{τ} (Panel A and B) and r (Panel C and D) on the amount of payments made. Panels A and C show the effects for the cases when bargaining is possible, meaning when there exists a range of r or μ_{τ} for which bargaining is preferred over official payments or not engaging with the state. Panels B and D show the effects when bargaining is not possible. Section C.7 in the appendix provides more detail on these two cases and characterizes the thresholds (i), (ii), (iii), (iv), (v), and (vi).

for collusive bargaining; in such cases empowerment can push citizens from not engaging with the state to making formal payments in the official payment equilibrium, as shown in Panels B and D of Figure 3.

4 Research Design

4.1 Treatments

We examine the effects of information and protection using a field experiment conducted in Kinshasa, DRC. The field experiment was developed and conducted in collaboration with the Congolese civil society organization *Observatoire de la Dépense Publique* (ODEP), which has a long history of working on citizen empowerment and tax advocacy in the DRC.

The *information* intervention sought to reduce information asymmetries by providing households and businesses with better information on legal tax and fee payments. Citizens in the information treatment group were called weekly by ODEP experts for a period of up to 19 weeks. In each call, an ODEP expert inquired into payments made in the previous week and anticipated payments for the coming week (see Appendix D.1 for details). The ODEP expert then provided information on the legal amounts for these different kinds of payments and gave advice on steps to take to navigate the process. While this intervention could in theory either increase or decrease what citizens believe they should be paying (depending on their priors), the context suggests that citizens were more likely overpaying and thus the treatment should empower households and businesses to reduce their expectation of formal payment amounts.

Households and businesses assigned to the *protection* treatment also received weekly calls by an ODEP expert and were asked to report on their previous and upcoming payments. This treatment differed from the information treatment in that citizens were informed that any suspicious payments would be investigated by ODEP and that the identity of state agents implicated in demanding informal payments would be publicized in a follow-on anti-corruption advocacy campaign (see Section 5.2.1). This was likely seen as a credible threat by citizens (and state agents) because ODEP regularly conducts such high-profile campaigns.¹³ By

¹³Because of its expertise and reputation, ODEP is recognized as having influence by officials

backing citizens in their interactions with street-level state agents, this treatment provided citizens with a connection to an influential actor and aimed to empower them to challenge demands for informal payments. Open-ended responses from a post-treatment survey reinforce the empowerment interpretation of both the protection and information treatments (see Appendix D.2).

4.2 Sampling and Randomization

We recruited households and businesses in Kinshasa into the experiment in two stages. Households and businesses eligible to participate in the experiment were identified from among the households and businesses that participated in the baseline survey described in Section 2.¹⁴ Eligible respondents were asked if they would be willing to participate in an additional data collection activity, which required attending training and providing data on payments for multiple weeks. Interested respondents were then invited to training sessions, which were held on a regular basis in the research team offices. Ultimately, 287 households and businesses participated in the training.

All 287 recruited households and businesses received the same training, which instructed participants on how to record payments on a daily basis using custom smartphone application developed by the research team. The data reported through this application is our main outcome data (discussed more below). Participants reported data for up to 19 weeks.¹⁵ Participants received phone credits to facilitate reporting and were allowed to keep the smartphones at the end of the study as additional incentive.

at multiple levels of the government administration. It also regularly holds a seat at parliament and in government meetings.

¹⁴Respondents were considered eligible if they were literate enough to read or write a letter in French and if the pre-set quota for the avenue had not yet been reached.

¹⁵The exact length of reporting time varied for respondents depending on the time point at which they were recruited into the study and trained.

Random assignment to treatment was done in two steps (see Figure 4 and Appendix D.3.) First, 48 avenues were assigned to treatment and 48 to control, blocking on commune. For avenues assigned to control, all households or businesses recruited from that avenue joined the control group. For avenues assigned to treatment, recruited households and businesses were further randomly assigned with equal probability to one of three treatment conditions (information, protection, or both).¹⁶ Clustering the controls by avenue minimizes spillovers from treated households; spillovers within treatment avenues is less of a concern because tax consulting was personalized to households and businesses.

Those assigned to control participated in the training and data reporting activities for the duration of the intervention but were not contacted by ODEP. Those assigned to one of the three treatment conditions were contacted by an ODEP expert a few days after the data reporting training. The ODEP expert explained their consulting services (according to treatment assignment) and asked if the participant would be interested in obtaining those services for free (see Appendix D.1 for the recruitment script).¹⁷ Ultimately, 271 of the originally assigned 287 households and businesses completed data collection. Our random assignment procedure obtained balance on pre-treatment covariates (see Appendix D.4). For further information on treatment compliance, see Appendix D.5.

4.3 Data and Measurement

To test our hypotheses, we need data on whether participants are making formal or informal payments and how much. A fundamental measurement challenge arises in that informal payments are often unknown, and illicit payments often hidden, making it difficult to obtain

¹⁶Below we focus on estimating the main effects of the information and protection treatments since the model does not predict interaction, although we explore interaction effects in Appendix Table A12.

¹⁷We took measures to ensure that the smartphone data collection activities were separate from the tax consulting activities to minimize concerns about reporting bias.

Figure 4: Randomization Design



Notes: Two-stage randomization design, where in the first stage avenues are randomly assigned to be either control or treated avenues, and then in the second stage, respondents from the treated avenues are randomly assigned to one of the three treatment conditions.

such data from administrative sources. Previous research (e.g., Jibao and Prichard, 2015) has attempted to collect similar data using surveys, but such approaches rely on recall data, which can be biased. To overcome these challenges, we collected data on daily payments directly from households and businesses using the customized smartphone reporting application described above. Overall, we obtained data on 4,706 payments across 18 categories for households and 22 for businesses. The categories and characteristics of associated payments are summarized in Appendix A1. For higher volume payments, payment frequency ranges from once to more than 10 times over the evaluation period (figure available upon request).

We use the smartphone data to create three main dependent variables. We note that while the model generates predictions on τ , bribes, and rents, and we did ask households and businesses to report formal and informal payments, it is difficult to distinguish among these reliably. For instance, it is possible that the information treatment—by providing better information on statutory payments—could induce some respondents to classify more of their payments (or a larger share of their payments) as informal, making it appear as if the treatment increased informal payments. Thus, our main dependent variables are total payment amount (for the intensive margin) and binary predictors of *Any Payment*, *Any Formal Payment*, and *Only Formal Payment* (for the extensive margin).

The first binary indicator, Any Payment, is 0 when the respondent reports no payment in a given category in a week and 1 if they report any payment no matter the amount or formal/informal classification. This variable helps us capture the first extensive margin effect as outlined in the next section. We use the second and third binary variables to estimate the second extensive margin effect. Any Formal Payment is 1 when the respondent reports any formal payments in a given category in a week (even if they also report an informal payment in that category) and 0 otherwise. Finally, Only Formal Payment, is 1 when the respondent reports only formal payments in a given category in a week and 0 otherwise. While outliers are a potential concern, all analysis below is robust to different approaches to dealing with outliers (see Appendix Tables A13–A14).

5 Extensive Margin Results

5.1 Estimating Extensive Margin Effects

Based on our theoretical framework in Section 3, we expect the information and protection treatments to increase the share of respondents who report any payments and increase the share of respondents who report only formal payments. We estimate these extensive margin effects using the following main specification:

$$1(Y_{i,t,j} > 0) = \beta_1^a \operatorname{Protection}_i + \beta_2^a \operatorname{Information}_i + \gamma^a X_i' + H_i^a + \phi_c^a + \eta_t^a + \theta_i^a + \varepsilon_{i,t,j}^a, \quad (1)$$

Where $1(Y_{i,t,j} > 0)$ indicates whether the household or business *i* paid *Y* at week *t* for category *j*. To capture the different types of extensive margin effects, we run the analysis with the *Y* outcome variable defined by *Any Payment*, *Any Formal Payment*, and *Only Formal Payment*. We index by *j* because payment in our theoretical framework can be considered separately for payment opportunities vis-à-vis different fee- or tax-collecting entities. For the covariate controls, X_i , we use the mean-centered interactions specification recommended by Lin (2013) to increase efficiency. This requires that we use one regression to estimate the protection treatment effect, using a specification that interacts the protection treatment with the mean-centered information treatment variable and covariates, and another analogous regression to estimate the effect of the information treatment.¹⁸

The controls include number of employees, revenue, book-keeping and network connections for businesses and household size, age and education of household head, wealth, and network connections for households. Since the treatment was assigned within recruitment week, commune, household/business, we use block cells defined by these dimensions: H_i^a indicates whether the respondent is a household or business (the *a* superscript is to distinguish from the intensive margin specification below), ϕ_c^a is a vector of commune fixed effects, and θ_j^a are payment category fixed effects. To account for time trends in payments we include a vector of reporting week fixed effects, η_t^a . Standard errors are clustered by avenue since the first-level treatment was assigned by avenue, and we use weights to account for assignment probabilities. The clustered standard errors account for arbitrary autocorrelation in outcomes (Arellano, 1987). By randomization, β_1^a and β_2^a capture the extensive margin effects of the treatments. We use a multiple testing adjustment to account for the fact that we are working with two different operationalizations of formal payment.

¹⁸Our hypotheses are with respect to the information and protection treatments, not to their interaction. Running the regressions separately allows us to use the features of the estimatr package for the mean-centered interaction model.

5.2 Extensive Margin Results

We first evaluate the prediction that empowerment (either through information or protection) induces citizens to start engaging with the state. Figure 5 presents the results for the combined sample of households and businesses (see Appendix E for results in tabular form).

In Panel A, we present results for the protection and information treatments using all categories of payments. We find that, across all main outcome measures, the protection treatment caused a significant increase in payments. Specifically, the protection treatment caused a 1.9 percentage point increase in payment rate in a given category per week (p < .05). In the control group, the weekly rate at which households and firms made payments per category was six percent, with the protection treatment effect representing a one-third increase. Given that our data includes 20 categories and the average respondent reported for 12 weeks, this suggests that the protection treatment led to an average of five additional payments per respondent.

The protection treatment also increased rates of Any Formal payments and Only Formal payments (columns 2 and 3), consistent with the second extensive margin prediction. The protection treatment caused a 2 percentage point increase in citizens reporting that they are making any formal payments per week, almost identical to the estimate for any payment. Given that exclusively informal payments were relatively rare (see Appendix B.1), the similar coefficients indicate that the protection treatment induced citizens primarily to start making formal payments.¹⁹ The effect for Only Formal payments is only slightly smaller, meaning that most new payments involved only formal payments (as opposed to combinations of formal and informal payments). With respect to the information treatment, the estimated

¹⁹In theory it is possible that protection treatment induced citizens who previously did not engage with the state to start making collusive payments while an equal proportion of citizens previously making collusive payments were induced to make official payments. But the rarity of pure collusion in our data makes this scenario unlikely.

effects on all three outcomes are also positive, although they are more modest (generally less than one percentage point) and not statistically significant.

The results from Panel A are reinforced by the evidence presented in Panel B, which shows the same extensive margin analysis but on a subset of (pre-specified) payment categories that are high-volume and where we expect high levels of opportunistic bribes and rents (these include electricity, sanitation, and licenses for businesses, and education, health, life events, electricity, water, and sanitation for households). The protection treatment effects for the restricted sample are more than double the magnitude of the full sample estimates and, again, appear to be driven by inducing formal payments. Point estimates for the information treatment are also much larger for this subset of payments, and statistically significant at p < .1.

Additional exploratory analysis sheds further light on the extensive margin results. Figure 6 presents the extensive margin effects separately for households (Panel A) and businesses (Panel B). While both households and businesses show an increase in payments from the protection treatments, the effects for households are three to five times the size of the effect for businesses (although the estimated interaction effect is not statistically significant given the modest power to detect interaction effects). Appendix Table A18 shows that the effects are concentrated among respondents that did not make payments in the category at baseline.

To gain additional traction, we explore treatment effects for payments grouped by characteristics relevant to our theoretical framework. Specifically, our theoretical framework suggests that we should see bigger effects for payments characterized from the baseline data as being more negotiable; motivated by access to services; and subject to uncertainty about the official payment amount (see Appendix Table A). The results, presented in Table 2, are strikingly consistent with these expectations. We find the biggest effects of the protection and information treatments on payments reported as being more versus less negotiable (columns 1–2). Similarly, the protection treatment had the biggest effect on payments motivated by access to services versus not (columns 3–4). Finally, the protection treatment (and sugges-



Figure 5: Extensive Margin Effects

Notes: This figure shows the coefficients for the protection and information treatments on the extensive margins. Panel A shows all payment categories while Panel B restricts the sample to a set of (pre-specified) payment categories that are most common and where we expect high levels of predation (these include electricity, sanitation, and licenses for businesses and education and health, life events, electricity and water, and sanitation for households).

tively the information treatment) had a greater effect for payments characterized by a high versus low degree of uncertainty. (See Appendix Figure A5 for further detail on treatment effects by individual categories of payments).

To understand why the information treatment's effects were more modest, we can consider again the baseline data from households on knowledge about the statutory amounts owed for different types of payments in Table A1. As noted previously, while substantial percentages of households indicated lack of certainty about statutory amounts owed, we do not see that this holds for the vast majority of households. This uncertainty may be even less pronounced among households otherwise at the margin between making payments or not. If so, then the



Figure 6: Extensive Margin Effects For Households and Businesses

Notes: This figure shows the coefficients for the protection and information treatments on the extensive margin for all payment categories for households (Panel A) and businesses (Panel B).

scope for the information treatment to change payment behavior would have been limited.

The extensive margin effect estimates are robust to different covariate specifications (Appendix Tables A10 and A11). We also check the extensive margin effect estimates when including an indicator for whether *both* treatments were received (Appendix Table A12), even though the theoretical model does not generate a prediction for an interaction effect and treats the interventions as additive. The estimated interaction effect is positive but not statistically significant. Appendix Table A16 collapses the data for the whole reporting period (instead of weekly) and finds consistent though imprecisely estimated effects. Appendix Figure A6 shows that the results are not driven by any one payment category. Finally, Appendix Table A19 shows no substantial moderator effects for the extensive margin, although we do find indication of moderator effects for the amounts paid, which we discuss below.

		Dependent	t Variable	: Any Paymer	nt	
	Negotiable (1)	Non-Negotiable (2)	$\begin{array}{c} \text{Access} \\ (3) \end{array}$	Non-Access (4)	Certain (5)	Uncertain (6)
Protection	0.036^{**} (0.013)	$0.020 \\ (0.014)$	0.047^{**} (0.017)	$0.003 \\ (0.007)$	0.023 (0.015)	$\begin{array}{c} 0.049^{***} \\ (0.015) \end{array}$
Information	0.031^{**} (0.015)	-0.006 (0.010)	$\begin{array}{c} 0.013 \\ (0.013) \end{array}$	-0.006 (0.007)	-0.007 (0.011)	$0.023 \\ (0.014)$
R^2 (Protection) R^2 (Information) Observations	$\begin{array}{c} 0.223 \\ 0.222 \\ 11,854 \end{array}$	$0.081 \\ 0.079 \\ 21,679$	$\begin{array}{c} 0.124 \\ 0.122 \\ 17,749 \end{array}$	$0.060 \\ 0.064 \\ 9,857$	$0.091 \\ 0.087 \\ 13,803$	$0.180 \\ 0.180 \\ 11,838$

Table 2: Extensive Margin Effects of Protection and Tax Consulting When Subsetting PaymentTypes

***p < 0.01; **p < 0.05; *p < 0.1

Notes: This table shows the results of equation 1 while subsetting the analysis to different groups of household payment categories based on the categories characteristics in our baseline data summarized in Table A1: whether respondents in baseline report more than 50% of payments being negotiable (Column 1) and the rest (Column 2); whether the main reason for payment was "access to services" in at least 50% of payments (Column 3) and the rest (Column 4); whether the household reported knowing the payment amount in at least two thirds of payments (Column 5) and the others (Column 6). We include covariate controls via mean-centered interactions as described in Section 5.1. This requires that we use one regression to estimate the protection treatment effect, and another regression analogously specified to estimate the effect of the information treatment. All specifications include payment category and reporting week fixed effects. Standard errors, clustered at the avenue level, are in parentheses. ***p < 0.01; **p < 0.05; *p < 0.1

5.2.1 The Advocacy Campaign

The results above are further supported by evidence of greater extensive margin effects in neighborhoods in which ODEP conducted its follow-on anti-corruption campaign. The advocacy campaign was implemented by ODEP about three months after the start of the empowerment treatments as part of its promise to citizens in the protection treatment to raise awareness of abuses by state agents. To investigate the effects of the advocacy campaign on citizen payments, we worked with ODEP to randomly assign all 69 neighborhoods in Kinshasa to treatment and control with equal probability (blocking on commune). Since avenues are wholly contained within neighborhoods, this created a mix of protection, information, and control avenues within campaign treatment and control neighborhoods.²⁰

In advocacy treatment neighborhoods, ODEP organized a number of meetings with households and business associations, the heads of local services, and commune chief executives to inform them of abuses documented during the empowerment treatments.²¹ To reduce potential spillover, ODEP focused on publicizing reported abuses from treatment neighborhoods only. While some spillover of advocacy campaign effects from treatment to control neighborhoods is likely, we note that such spillover would make it harder for us to detect advocacy campaign effects.

Table 3 shows the extensive margin effects of the information and protection treatments ²⁰Specifically, the 35 campaign treatment neighborhoods cover 23 protection and/or information treatment avenues plus 17 control avenues; the 34 campaign control neighborhoods encompass 22 protection and/or information treatment avenues and 20 control avenues.

²¹It is worth noting that the individual abuses documented by ODEP and shared with local officials map closely onto the payment categories where we observe treatment effects. For instance, program implementation documentation (available upon request) lists individual instances of abuse involving electricity payments, which is one of the payment categories where we see the biggest effects of the protection treatment.

	Dependent Variable:			
	Any Payment	Any Formal	Only Formal	
	(1)	(2)	(3)	
Protection	0.020^{**} (0.009)	0.020^{**} (0.009)	0.018^{**} (0.007)	
Information	$0.004 \\ (0.007)$	$0.003 \\ (0.007)$	$0.005 \\ (0.005)$	
Post \times Advocacy (Protection)	-0.008 (0.008)	-0.003 (0.007)	-0.011^{*} (0.006)	
Post \times Advocacy (Information)	-0.001 (0.010)	$0.003 \\ (0.010)$	$0.001 \\ (0.008)$	
$\label{eq:Protection} {\rm Protection} \times {\rm Post} \times {\rm Advocacy}$	0.032^{**} (0.015)	$0.024 \\ (0.015)$	0.027^{**} (0.013)	
Information \times Post \times Advocacy	$0.007 \\ (0.014)$	$0.002 \\ (0.013)$	$0.007 \\ (0.010)$	
R ² (Protection) R ² (Information) Observations Control Mean	$\begin{array}{c} 0.109 \\ 0.109 \\ 63,747 \\ 0.06 \end{array}$	$0.097 \\ 0.096 \\ 63,747 \\ 0.05$	$0.082 \\ 0.083 \\ 63,747 \\ 0.02$	

Table 3: Extensive Margin Effects of Protection and Tax Consulting Including Post×Advocacy

Notes: This table shows the results of interacting the binary treatment indicators with indicators if the location was part of the advocacy sample and if the reporting week was after the start of the advocacy campaign. We include covariate controls via mean-centered interactions as described in Section 5.1. This requires that we use one regression to estimate the protection treatment effect, and another regression analogously specified to estimate the effect of the information treatment. All specifications include payment category and reporting week fixed effects. Standard errors, clustered at the avenue level, are in parentheses. ***p < 0.01; **p < 0.05; *p < 0.1

before and after the start of the campaign in advocacy treatment and control neighborhoods. Row one demonstrates that the protection treatment had a positive extensive margin effect even prior to the campaign. However, after the campaign these effects are significantly amplified—more than doubling in magnitude—for households and businesses who both received the protection treatment *and* resided in advocacy campaign neighborhoods.²² The differential effects of advocacy for those in the protection treatment are consistent with a story in which citizens in the protection treatment became aware of the campaign—either through the media or their weekly conversations with ODEP representatives—and felt empowered to demand formal payments with the knowledge that they had an influential civil society organization protecting their interests.²³

6 Average Payment and Intensive Margin Results

6.1 Estimating Intensive Margin Effects

Our theoretical framework implies two main intensive margin effects: either protection or information should decrease the amount paid by those in the collusion equilibrium. Additionally, protection (but not information) should decrease the payment amounts for citizens

- ²²These findings are unlikely to be driven by reporting bias. While the advocacy campaign could potentially influence the reporting of informal payments, our extensive margin outcomes are less susceptible to such biases. Furthermore, the strong pre-campaign effects observed for the protection treatment make it unlikely that the post-campaign results are artifacts of campaign-induced reporting changes.
- ²³If the campaign worked instead by making state agents less likely to demand informal payments (rather than by empowering citizens), we would expect to see a general effect of the advocacy campaign, which we do not (as indicated by rows three and four of Table 3). It is unlikely that state agents knew which households and businesses were in the protection treatment and reduced their demands on those citizens in particular.

in the official payment equilibrium.

Intensive margin effects are defined as effects for those who would be in a payment equilibrium in both treatment and control; as such, intensive margin effects are not point identified by randomization (Staub, 2014; Lee, 2009). For a given payment type and week, the difference in mean payment levels across treatment and control mixes the extensive margin effect (those going from zero payment to some positive payment for that category and week) with the intensive margin effect (changes in payment levels among those who would always pay that category in that week). Even if the extensive margin effect is weakly positive for all subjects ("monotonicity" per Lee, 2009), those who pay in the treatment group will consist of a mixture of "always-payers" and those induced to pay by the treatment, whereas the control group will consist only of "always-payers." Comparing amounts paid *among those who make a positive payment* is not an apples-to-apples comparison that isolates the intensive margin effect.

Given this complication, we report "conditional on positives" and "trimming bounds" estimates. The conditional on positives estimator subsets to $Y_{i,t,j} > 0$ units (units making positive payments post-intervention for a given payment type in a given week). Given that we have extensive margin effects, this estimate is biased for intensive margin effects insofar as "always-payers" have a different potential outcome distribution than those induced to pay. To address this possibility, we use Lee (2009) trimming bounds. To construct these bounds, we use the extensive margin estimate to determine the share of units that were induced to pay in a given category and week. To estimate the upper bound on the intensive margin effect, we trim the bottom of the outcome distribution for treated units by this share, and for the lower bound, we trim the top of the outcome distribution for the treated units. These bounds cover the true intensive margin effect if extensive margin effects are monotonic such that the treatments can only cause payment, and not cause non-payment.²⁴

 $^{^{24}}$ In line with our pre-analysis plan, Appendix E.2 discusses conditioning on positive *pre-treatment outcomes* to identify intensive margin effects. This approach is unreliable due to

Our conditional on positive estimates and trimming bounds use the following specification on the subsample of subjects making post-treatment payments:

$$Y_{i,t,j} = \beta_1^b \text{Protection}_i + \beta_2^b \text{Information}_i + \gamma^b X'_i + H^b_i + \phi^b_c + \eta^b_t + \theta^b_j + \varepsilon^b_{i,t,j},$$
(2)

Where $Y_{i,t,j}$ is the amount of the relevant payment made during the post-treatment smartphone reporting week t for individual i in category j. Other terms in the specification are the same as defined above, and again we fit the model using two separate regressions for the protection and information treatment effects, clustering standard errors by avenue and using weights to account for the assignment probabilities.

6.2 ATE and Intensive Margin Results

Whether the interventions lead to a positive or negative average treatment effect on payment amounts depends on whether the extensive or intensive margin dominates. While the extensive margin effects are positive, for the intensive margin, we hypothesize that the protection treatment would reduce payment amounts for those already engaging with the state and that the information treatment would reduce payments only for those in the collusive equilibrium.

Figure 7 displays estimates of average effects on payment amounts. At the top are the average treatment effect (ATE) estimates, which are precise zeroes for both information and protection. Given the positive extensive margin effects, the fact that we have net zero effects on average payment amounts suggests that average intensive margin effects are negative. The estimates presented below the ATE on Figure 7 show that this is the case. The second set of estimates from the top are the conditional-on-positive effect estimates. Among those paying non-zero amounts, the weekly amount being paid by those in the control group is

strong differences in payment rates between the period covered by the pre-treatment data (a full year) versus the post-treatment period (up to 19 weeks).



Figure 7: ATE and Intensive Margin Effect of Protection and Information

Notes: This figure shows the coefficients for the protection and information treatments on the ATE and intensive margin for all payment categories.

USD 47.62. We estimate that those paying non-zero amounts in the protection treatment are paying about half as much (USD -21.82 difference, p < .05). For the information treatment, the estimated reduction is more modest and not statistically significant.

As discussed above, these estimates do not isolate the intensive margin effects, because the extensive margin effects create a compositional change in the types of people who are paying in the control group versus the treatment groups. The bottom two sets of estimates show the Lee (2009) trimming bounds that account for this compositional change of payees for a given payment category and week. The point estimates for the bounds are always negative. For the protection treatment, the upper bound is USD -14.30 (not statistically significant) and the lower bound is USD -46.04 (p < .01), which implies implausibly that average payment amounts are driven to zero, although the data cannot rule this out. For the information treatment, the upper bound is USD -5.46 (not statistically significant) and the lower bound is USD -23.69 (p < .01). In sum, the evidence suggests that protection, but not information, reduced payment amounts.

The appendix displays additional results and robustness checks. Appendix Table A17 explores informal payments and amounts and suggests negative intensive margin effects of both treatments on the informal amount paid. Appendix Figure A5 shows how the ATE varies across payment categories, allowing us to see the categories in which extensive or intensive effects dominate. Columns 6–10 of Appendix Table A9 show estimates for the higher-volume restricted sample. The estimated effects are qualitatively similar, but smaller in magnitude. This suggests that some, and possibly most, of the action is coming from relatively low-volume payment categories. Given the high skew in the distribution of payments, we estimate effects on outcome distributions winsorized at the 99th and 95th percentiles (Appendix Table A14). The estimated effects decline as we top-code the upper percentiles of the payment distribution. This suggests that intensive margin effects are driven by lowering payment amounts in the top percentiles of the payment distribution, rather than a uniform shift in payment amounts. Appendix Figure 6 shows that the results are not driven by any one payment category. Appendix Table A15 shows that the treatments did not lead to more negotiations with state agents but did lead to a lower rate of refusal to pay.

As discussed above, we estimate moderator effects for two measures of pre-treatment endowments for negotiating informal payments: a network z-score that measures connections to elites and education level. Recall that eligibility for our study required that the respondent be literate and able to operate a smartphone; as such, education levels in our sample are substantially higher than the overall population of Kinshasa. Appendix Table A19 shows that higher levels of education push the conditional-on-positives effect toward zero and even toward becoming positive. This suggests that it is those with relatively lower levels of education that stand to gain the most in terms of reduced payment amounts.

7 Conclusion

This paper shows that empowering citizens—primarily by connecting them to an influential civil society actor that can protect them—not only reduces citizens' informal payments to opportunistic state agents but also increases formal payments to the state and induces citizens to expand engagement with the state.

These findings on the extensive margin effects of empowerment are not obvious and constitute the main contribution of this paper. By modeling citizens as having two decisions to make—whether or not to engage with the state and, conditional on engagement, whether or not to insist on formal payments—we better capture the conditions under which empowerment will change not only payment amounts but also the formality of payments and the decision to start making payments. Our evidence thus offers a micro-level perspective on what Acemoglu and Robinson (2020) refer to as the "red queen" effect, in which improvements in state revenue and capacity can follow from increasing the capacities of citizens and civil society more broadly.

Overall, we believe our theory and evidence help to explain citizen-state interactions over a wide range of payments that households and businesses make in weakly institutionalized contexts where states have imperfect control over street-level state agents and where both information and power asymmetries are prevalent. While our theory is most applicable to explaining payments that are in some part voluntary—meaning that citizens have scope to choose whether to evade or opt out—there is good reason to believe that this is true for almost all types of payments. Payments are only involuntary in contexts where payments
are perfectly enforced by the state or demand for a benefit is highly inelastic, which are both rare conditions.

It is worth noting that while we find that protection had a bigger effect than information in our context, this could reflect conditions in the DRC or the characteristics of our sample. Nevertheless, the theory points to both a lack of information and of influential connections as two distinct sources of citizen vulnerability. In other contexts it could be that information or information combined with protection—would yield the greatest empowerment dividends for citizens. It could also be that protection is *less* likely to be effective in contexts where civil society is weak and unable to advocate on behalf of citizens. This too highlights the ultimate importance not just of individually empowered citizens but also of an empowered civil society to building strong fiscal contracts and states.

Finally, while our theory and evidence reveal short-run effects, they also provide insights into how information and protection might affect the longer-run welfare of citizens, the state, and street-level state agents. Our approach suggests that empowerment will make citizens unequivocally better off in the longer run because empowerment will reduce bribe payments and incentivize more citizens to become visible to the state to obtain benefits. Interestingly, the extensive margin predictions from the model yield the counter-intuitive insight that empowerment interventions, which are typically designed to reduce citizens' informal payments, might result in some citizens making *more* informal payments if citizens are induced to engage the state. Nevertheless, this should be seen as a welfare-enhancing change. One potential caveat that merits future research, however, is whether increasing citizen engagement with the state in one domain (e.g., electricity) has the effect of making citizens more visible to the state—and consequently more exposed to demands for formal and informal payments—in other domains (e.g., water). Ultimately, the consequences for citizen welfare of increasing payments in one domain will depend on how the state invests in using the information from initial engagement to expand the breadth and depth of its knowledge of its citizenry.

Citizen empowerment should also be welfare enhancing for the state insofar as it pushes more citizens towards engaging with the state and paying more formal taxes.²⁵ Whether the state benefits from citizen empowerment could depend on other factors, especially the behavior of higher-level state agents. The state might realize few benefits if higher-level agents prefer to collude with street-level agents to extract informal payments from citizens; it could benefit more if higher-level state agents can be persuaded to exercise more control over lower-level agents, thereby reducing the scope for opportunistic demands.

For street-level state agents, the welfare effects of empowerment are less clear. In our approach, empowerment reduces bribes on the intensive margin but possibly increase rents on the extensive margin; thus the welfare of state agents depends on whether the extensive or intensive margin effect dominates. While our theory abstracts away from strategic calculations by street-level state agents, this paper suggests that the welfare of state agents could mainly depend on how higher-level officials respond to citizen empowerment. If empowered citizens produce more revenue for the state, then citizen empowerment could also be welfare enhancing for street-level state agents in the longer run if the state uses that revenue to improve their compensation. This underscores the importance of future research to incorporate how state agents at higher levels of government strategically respond to an empowered citizenry.

²⁵While it is still possible that some of that additional formal revenue gets lost to leakage, we think it is reasonable to assume that at least some of it makes its way to the state coffers.

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A Payment Categories and Descriptions

This appendix provides detailed information on negotiability and uncertainty related to specific payment categories. Table A1 documents sample payments associated with payment categories, along with the extent to which payments in each category were seen as negotiable, the extent to which there was certainty, and whether the main reason for paying was access to benefits. Appendix Figure A1 provides greater detail, based on qualitative data collection, on the nature of citizen-state interactions, informal payments, and negotiability, for key payment categories.

Category	$\mathrm{HH}/\mathrm{Firms}$	Question Wording: "Did your houshold/business pay or was asked to pay a tax or informal payment []"	Examples Payments	Negiotiable HH/Firms	Certainty	Reason Access
Electricity	Firms & HH	[] linked to electricity?	informal payments for repairing faults; informal payments to avoid cuts; tax on generators	78%/39%	29%	95%
Goods	Firms & HH	[] linked to the rent on fixed assets and property owned?	tax on rental income, property tax, land registration	32%/62%	69%	40 %
Sanitation	Firms & HH	[] linked to hygiene, sanitation, and health regulations?	environmental and hygiene tax; pollution tax; health tax, TRA;	71%/60%	50%	91 %
			Sanitation tax, removal of rubbish and household waste;			
			Hygiene and sanitation service/disinfestation/health control			
Security	Firms & HH	[] linked to security or legal service?	protection taxes, conflict resolution, bribes to avoid imprisonment	50%/53%	71%	0%
Transport	Firms & HH	[] linked to transport?	car/motorcycle registration fee; roadblock payment, port fee	39%/45%	71%	84%
Water	Firms & HH	[] linked to water?	informal payments for repairing faults; informal payments to avoid cuts;	21%/19%	62%	96%
Association	HH	[] linked to community or social development projects	labor contribution or cash payments related to activities community;	31%	100%	13%
		or to any types of associations/cooperatives/business groups?	payments to support maintenance/repair of community infrastructure			
Customary	HH	[] contributions by customary leaders or authorities?	contributions to the chef for specific events/festivals; payments to access land	40%	33%	20%
Documents	HH	[] linked to obtaining or replacing an official government document?	passport, driving license; voter card, identity card;	60%	67%	80%
			fees for attestation/certificate of loss of part;			
			legalization signature or any other civil status act			
Education	HH	[] linked to education?	SERNIE fees; registration fees; monitoring/motivation costs for teachers;	10%	68%	93%
Health	HH	[] linked to education or health services?	patient records; informal fees for doctors/nurses	29%	79%	98%
HH Business	HH	[] linked to one or more small businesses or production units owned by the household?		0%	50%	100%
Life Events	HH	[] linked to supporting events in social life?	funerals, weddings, births	54%	63%	68%
Salary	HH	[] linked to their income or generating activity?	income tax	37%	87%	37%
Other HH Payments	HH	[] linked to access to other public services?	use of public toilets; public library			
Communication	Firms	[] linked to the communication?	telephone, fax, post, mail, e-mail	0%		
Excise	Firms	[] linked to the purchase of raw materials/inputs for the trade?	custom tax; sales taxes	23%		
Fuel	Firms	[] linked to fuel/lubricant?		27%		
Insurance	Firms	[] linked to insurance/protection and fire protection?	Fees for protection and fire prevention	100%		
Labor	Firms	[] linked to labor?	Tax on wages and labor; Professional tax on remuneration;	42%		
			Exceptional tax on remuneration of expatriate staff;			
			Self-employment income tax			
License	Firms	[] linked to operating licenses?	registration license, operating permit, IPMEA, single establishment tax, etc.	40%		
Maintenance	Firms	[] linked to the repair and maintenance/service charge for work done by others?	repair and maintenance; service fees for work done by others	100%		
Marketing	Firms	[] linked to marketing/advertising?	display permission fee; taxes on the decoration of public buildings;	93%		
			tax on the production of advertising work			
Media	Firms	[] linked to press expenses?	newspaper, magazine, paper, printing expenses, stationery	60%		
Package	Firms	[] linked to stock of consumables/packaging materials?		60%		
Profit	Firms	[] linked to business profits?		71%		
Royalties	Firms	[] linked to royalties?				
Sales	Firms	[] linked to sales?	value-added/sales tax levy	40%		
Start	Firms	[] linked to contracts?		100%		
Storage	Firms	[] linked to storage and refrigeration?	goods deposit tax	60%		
Other Bus Payments	Firms			50%		

Table A1: Payment Categories and Examples

Payment type	State actor	Interaction (How	Alternatives (Private options,	When do citizens make informal	Negotiability – to what extent are
		initiated)	informal/illegal access)	payments to the state?	payments negotiable?
Electricity	SNEL	Citizens contact SNEL to establish a formal connection	Citizens may opt to access electricity without a formal connection by connecting via a neighbor's connection (most common), for an agreed fee, or by forgoing an electrical connection in favor of reliance on their own source of electricity (e.g. generator)	Those with formal SNEL connections commonly make two types of informal payments. First, they may pay bribes to reduce the amount that they are billed, though this is more difficult where functioning meters are installed. Second, state officials may extract additional rent payments to perform needed repairs, or by threatening to intentionally disrupt access to electricity.	High level of negotiation, primarily related to rent extraction by state officials. Because electrical connections require regular repairs and are easily disrupted state officials have substantial opportunities to seek rent payments, with the size of payments open to negotiation. Negotiation of amounts for bill payments sometimes also occurs where meters are not reliable. Level of negotiation and informal payments somewhat lower for businesses, as state officials are more cautious about threatening power cuts, or delayed repairs, to businesses owing to high costs of doing so.
Water	REGIDESO	Citizens contact REGIDESO to establish a formal connection	Citizens may opt to connect via a neighbor's connection (most common), for an agreed fee, or by forgoing a piped connection and relying in bore holes, community water pipes, neighbors or water trucks.	Those with formal REGIDESO connections may make two types of informal payments. First, they may pay bribes to reduce the amount that they are billed, though this is more difficult where functioning meters are installed. Second, state officials may extract additional rent payments to perform needed repairs, or by threatening to intentionally disrupt access to electricity.	Low level of negotiation. Opportunities for state officials to seek rent payments are relatively limited as water infrastructure requires repairs relatively rarely and is difficult for front line officials to disrupt, while disrupting access to water is very sensitive and thus generally avoided. Negotiation of amounts for bill payments sometimes occurs where meters are not reliable, and standard payments not agreed, but this is relatively rare.
Education	Schools	Citizens opt to send their children to school	Citizens may send their children to private schools, where they will similarly pay fees	Informal payments are a widely accepted and relatively standardized requirement to attend school.	Low level of negotiability as payments are generally jointly agreed between school and families and are collectively known.
Licensing	Ministry of Finance	Business may opt to visit government offices to formally register.	Remain unregistered, but with the risk of needing to make informal payments to front line bureaucrats.	Informal "acceleration" payments may be required to secure a license, though now less common owing to standardized and simplified processes.	High level of negotiability. Acceleration payments consistently negotiable based on urgency and negotiating power.
Sanitation	City Government	Citizens/businesse s opt to receive sanitation services in areas of the city where state provision is available	Citizens may rely on private garbage collection services, or may dispose of garbage themselves (sometimes illicitly)	Most commonly when they dispose of quantities or items that exceed formal allowances.	Where informal payments are <u>required</u> there is a high level of negotiation based on the particular service being delivered (e.g. quantity or type of waste being collected)

Figure A1: Additional detail on key payment categories

B Baseline Survey

B.1 Formal vs informal payments

	Table	A2:	Type	of	payments	Firms
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Payment type	Share	Median	Mean
Formal only	$\begin{array}{c} 22.29 \ \% \\ 13.60 \ \% \\ 64.11 \ \% \end{array}$	\$ 33.30	\$ 122.46
Informal only		\$ 20.00	\$ 83.67
Formal and informal		\$ 50.00	\$ 133.72

Notes: This table shows breakdown of payments made by firms in our baseline data on whether they were formal only, informal only or formal and informal.

B.2 Payments and benefits





Notes: This figure shows the relationship between service access and payments for households in our baseline data. Panel A shows the percent of respondents by the main reason they state for making a payment. Panel B shows a scatter plot of overall payment amount per household per year on the y-axis and an index of service access (comprised of electricity, water access, and percent of children in school) on the x-axis.



Figure A3: Firm benefits and payments

Table A3: Correlation between Registration and Tax Burden

	Formal	USD per l	ous/year	Informal USD per bus/yea			
	(1)	(2)	(3)	(4)	(5)	(6)	
Registered	316.8^{***} (81.48)	327.3^{***} (75.88)	$ \begin{array}{c} 184.1^{***} \\ (67.02) \end{array} $	$\begin{array}{c} 68.17^{***} \\ (20.14) \end{array}$	$\begin{array}{c} 68.07^{***} \\ (18.37) \end{array}$	$52.31^{***} (19.26)$	
Owner Secondary Education			68.92 (86.71)			22.33 (23.30)	
Years operation			$1.962 \\ (3.087)$			-0.332 (0.816)	
Number of employees			$110.3^{***} \\ (26.07)$			10.37^{**} (4.101)	
Bookkeeping?			111.5 (68.19)			6.366 (20.59)	
Observations	527	524	518	527	524	518	
R^2	0.030	0.057	0.084	0.022	0.038	0.043	
Commune FE	Yes	Yes	Yes	Yes	Yes	Yes	
Sector FE	No	Yes	Yes	No	Yes	Yes	
Cluster	Avenue	Avenue	Avenue	Avenue	Avenue	Avenue	

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

B.3 Network Connections

Panel A: Households									
	Min	P25	Median	P75	P90	Max	Mean	SD	Ν
Know Commune Chief	0	0	0	0	0	1	0.091	0.288	559
Know Neighborhood Chief	0	0	0	0	0	1	0.020	0.140	551
Know Avenue Chief	0	0	0	0	1	1	0.133	0.340	555
Know National Tax Official (DGI)	0	0	0	0	0	1	0.093	0.291	557
Know Provincial Official (DGRK)	0	0	0	0	0	1	0.048	0.214	560
Know Customs Official	0	0	0	0	0	1	0.074	0.261	557
Know Police Official	0	0	0	1	1	1	0.297	0.457	553
Know Army Official (FARDC)	0	0	0	0	1	1	0.180	0.385	550
Know Intelligence Official (ANR)	0	0	0	0	0	1	0.051	0.220	550
Percentage of Roles Known	0	0	0	0.22	0.33	1	0.110	0.153	562
Number of Connections	0	0	0	1	3	9	0.904	1.336	605
Panel B: Firms									
	Min	P25	Median	P75	P90	Max	Mean	SD	Ν
Know Commune Chief	0	0	0	0	1	1	0.129	0.335	528
Know Neighborhood Chief	0	0	0	0	0	1	0.036	0.187	523
Know Avenue Chief	0	0	0	0	.5	1	0.100	0.300	520
Know National Tax Official (DGI)	0	0	0	0	1	1	0.100	0.301	528
Know Provincial Official (DGRK)	0	0	0	0	0	1	0.053	0.224	529
Know Customs Official				-	-				
	0	0	0	0	0	1	0.029	0.167	526
Know Police Official	0 0	0 0	0 0	0 1	0 1	1 1	$0.029 \\ 0.330$	$0.167 \\ 0.470$	$526 \\ 528$
Know Police Official Know Army Official (FARDC)	0 0 0	0 0 0	0 0 0	0 1 0	0 1 1	1 1 1	$0.029 \\ 0.330 \\ 0.235$	$0.167 \\ 0.470 \\ 0.424$	$526 \\ 528 \\ 528$
Know Police Official Know Army Official (FARDC) Know Intelligence Official (ANR)	0 0 0 0	0 0 0 0	0 0 0 0	0 1 0 0	0 1 1 0	1 1 1 1	$\begin{array}{c} 0.029 \\ 0.330 \\ 0.235 \\ 0.030 \end{array}$	$\begin{array}{c} 0.167 \\ 0.470 \\ 0.424 \\ 0.172 \end{array}$	526 528 528 527
Know Police Official Know Army Official (FARDC) Know Intelligence Official (ANR) Percentage of Roles Known	0 0 0 0	0 0 0 0 0		0 1 0 0 0.22	0 1 1 0 0.33	$egin{array}{c} 1 \\ 1 \\ 1 \\ 1 \\ 0.67 \end{array}$	$\begin{array}{c} 0.029 \\ 0.330 \\ 0.235 \\ 0.030 \\ 0.116 \end{array}$	$\begin{array}{c} 0.167 \\ 0.470 \\ 0.424 \\ 0.172 \\ 0.146 \end{array}$	526 528 528 527 531

Table A4: Summary Statistics for Network Connections

C Formal Model

This section develops a theoretical framework that captures the costs and benefits of engaging with the state and the consequences of doing so for formal and informal payments.

C.1 Set-Up

In the game, the citizen first chooses whether to engage with the state to obtain benefits. When citizens engage the state, they interact with a street-level state agent who collects the fee or tax.²⁶ When engaging, the citizen can either collude privately or make an official payment. Figure 2 shows the decision tree. Below, we use backward induction to solve for a subgame perfect Nash equilibrium.

The game begins when the citizen decides whether to engage. Following on the discussion in Section 2, engagement implies exposure to payment demands in exchange for access to a benefit. Thus, if they do not engage they get the benefit V_0 while engaging brings the benefit V_E . We can conceptualize V_E as a benefit obtained directly from paying, for instance when a citizen obtains electricity in return for paying a user fee. It could also refer to more indirect benefits that arise from being more visible, for instance when a households obtains greater property rights protections after paying property taxes or when a business obtains a greater ability to advertise and expand its customer base after it pays a fee to formally register.

²⁶Future research might consider how incorporating a broker as an intermediary between the citizen and the state agent affects predictions. Our qualitative evidence suggests that brokers are rarely used in the DRC and our model therefore abstracts away from this possibility.

If the citizen engages, they have a true payment liability τ^* , which the state agent knows but the citizen does not. Instead, the citizen has a prior belief about her payment liability, μ_{τ} . When engaging, the citizen can either (1) collude privately with the state agent over a bribe to be paid in lieu of the legal amount, or (2) insist on making a official payment, for instance by demanding a receipt or insisting on conducting the transaction at an official state office. If the citizen insists on an official payment, they pay the formal amount and an additional transaction cost, c_A (e.g., the cost of demanding a receipt or of traveling to a state office to pay the formal tax). Additionally, the citizen might still have to pay a rent r to the state agent, which captures the reality that officials often use their power to extract illegal amounts on top of formal payments.²⁷ In this case, the citizen's expected payoff is $V_E - \mu_{\tau} - r - c_A$, and the state agent's expected payoff is $r.^{28}$

Alternatively, the state agent and the citizen may prefer to collude in private. Note that the expected payoff of insisting on an official payment decreases in c_A but a collusive transaction has implications for μ_{τ} , since in this "collusive" setting payment levels are negotiated. If the state agent could, he has an incentive to manipulate μ_{τ} and c_A . When transacting privately, the state agent and citizen have the potential to forgo the socially costly official process, and bargain over the surplus left by not making an official payment. We suppose that, when transacting privately, the citizen and state agent Nash bargain over the size of the bribe payment, b, from the citizen to the state agent. Let the parameter γ denote the state agent's bargaining power and $1 - \gamma$ the citizen's bargaining power. We also suppose that there is a cost of collusion that captures the risks associated with illicit bribes. Thus, the state agent's and citizen's payoffs under collusion are $b(1 - c_B) - C_B$ and $V_E - b(1 + c_C) - C_C$ respectively.

C.2 Collusive bargaining

The joint surplus from collusion is $S = \mu_{\tau} + r + c_A - (C_B + C_C + r) - b(c_B + c_C)$. The surplus decreases in b because the level of bribe increases the cost of collusion.²⁹ The Nash bargaining solution implies:

$$b^{*} = \gamma \left[\frac{\mu_{\tau} + r + c_{A} - C_{C}}{1 + c_{C}} \right] + (1 - \gamma) \left[\frac{C_{B} + r}{1 - c_{B}} \right]$$
(3)

The amount of informal transfers that are non-zero increases in the bargaining power of the tax official, the mean of the citizen's prior distribution about her payment liability, and the cost of making an official payment, which the state agent can take advantage of. The observed bribe decreases in the citizen's marginal and fixed costs of paying the transfer, and increase in the state agent's fixed and marginal costs of collusion.

C.3 Citizen's decisions

The citizen will bargain if the expected utility of bargaining is larger than that of making an official payment. That is, they will bargain if the bribe and associated cost of collusion is lower than the expected payment

²⁷The cost of making an official payment, c_A , and the rent extracted by the state agent is likely to vary depending on whether the official payment is made on the street with the state agent or at a state office. Allowing c_A to go to 0 or only letting the state agent receive a portion of r does not change the results substantively.

²⁸We consider r to be an extractive informal payment whose amount is set by the state agent. We therefore do not allow for bargaining over r as we do over bribes b below. When analyzing the effects of the interventions in Section C.4 we will discuss what determines r.

²⁹Note 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_B be the payoff of the state agent and u_C the payoff of the citizen. Let $h(u_B)$ be defined as: $u_C = h(u_B)$. The Nash bargaining payoffs are given by: $-h'(u_B) = \frac{\gamma}{1-\gamma} \frac{u_C - d_C}{u_B - d_B}$, where $d_i \ i = B, C$ indicate respectively the no collusion outside options of the state agent and citizen. Since the costs of collusion increase in the amount of the transfer, we have $h'(u_O) = -\frac{1+c_C}{1-c_O}$, thus, the NBS bribe is given by: $\frac{1+c_C}{1-c_B} = \frac{\gamma}{1-\gamma} \frac{\mu_{\tau}+r+c_A-C_C-b(1+c_C)}{b(1-c_B)-C_B-r}$ In simple problems of transferable utility, however, $h'(u_O) = -1$.

rate, cost of insisting on an official payment, and rent payment.

Given this decision whether or not to bargain with the state agent, backing up in the game tree, the citizen decides whether to engage with the state in the first place. When the bargaining outcome would yield a bribe that is so high that the citizen would prefer to make a formal payment, then the citizen will engage with the state if the relative benefits are larger than the expected payment liability, the rent payment, and the cost of securing a formal transaction. If the negotiated bribe is low enough such that the citizen prefers bargaining, then the citizen will engage with the state if the relative benefits are higher than the transfer and the associated cost of collusion.

C.4 Predictions for the effects of empowering citizens

There are two ways in which we expect empowerment to work. First, officials know the true payment liability, τ^* , while citizens only have a guess, μ_{τ} . We construe additional information as intervening on μ_{τ} . There are several reasons why we interpret additional information as a reduction of μ_{τ} rather than reducing its variance. For one, the distribution of bias in beliefs about tax liabilities should be truncated among those not paying formally. If the bias is too high (i.e., they think the liability is much lower than it actually is), then they would be engaging. Those that already pay formally likely know the true liability. Further, insofar as state agents are able to manipulate beliefs of citizens, they would do so by increasing the μ_{τ} , thereby increasing the equilibrium bribe amount in a collusive deal. Also, if decision making under uncertainty is maxmin, then reducing variance would also imply reducing μ_{τ} . Finally, our qualitative evidence further confirms that citizens overestimate tax liabilities.

The second way in which we expect empowerment to work is by affecting the ability of officials to extract a rent, r, from citizens even when they make official payments. We view protection as acting on r insofar as linking citizens to a civil society organization that will advocate for them should result in lower, or even zero, rent payments.

Importantly, in expecting protection to operate on r, we start with the assumption that citizens will be unwilling to report collusive bribes b since this is an illegal agreement that benefits both the citizen and the state agent. We thus do not expect the protection to operate directly on the citizens' bargaining power in the collusion equilibrium. Rather, we allow that, by reducing the amount of rents the citizens have to pay when making an official payment, protection incentivizes official payment over a collusive agreement. Specifically, we assume that when setting r the state agent considers the vulnerability of the citizen to rent extraction, which protection reduces. As we show, however, reducing r has complex effects in that it can, under some conditions, reduce informal payments—by reducing the rent associated with official payments—but, under other conditions, increase informal payments—by inducing some citizens who previously have not engaged with the state to start engaging, which might be associated with paying bribes or rents.

C.5 Solving Bribe

Equilibrium condition per Muthoo and with linear cost:

$$\frac{1+c_C}{1-c_B} = \frac{\gamma}{1-\gamma} \frac{\mu_{\tau} + r + c_A - C_C - b(1+c_C)}{b(1-c_B) - C_B - r}$$

Get rid of b at the bottom of the fraction:

$$(b(1-c_B) - C_B - r) \left[\frac{1+c_C}{1-c_B} \right] = \left[\frac{\gamma}{1-\gamma} \right] (\mu_\tau + r + c_A - C_C - b(1+c_C))$$

Simplify:

$$b(1+c_{C}) = \left[\frac{\gamma}{1-\gamma}\right](\mu_{\tau} + r + c_{A} - C_{C}) - b(1+c_{C}))\left[\frac{\gamma}{1-\gamma}\right] - (-C_{B} - r)\left[\frac{1+c_{C}}{1-c_{B}}\right]$$

Divide by $(1 + c_C)$:

$$b = \left[\frac{\gamma}{1-\gamma}\right] \frac{\mu_{\tau} + r + c_A - C_C}{1+c_C} - b\left[\frac{\gamma}{1-\gamma}\right] + \left[\frac{C_B + r}{1-c_B}\right]$$

Getting all the b's to the left:

$$b\left[\frac{1}{1-\gamma}\right] = \left[\frac{\gamma}{1-\gamma}\right] \frac{\mu_{\tau} + r + c_A - C_C}{1+c_C} + \left[\frac{C_B + r}{1-c_B}\right]$$

Now we just need to multiply by $1 - \gamma$:

$$b^* = \gamma \left[\frac{\mu_{\tau} + r + c_A - C_C}{1 + c_C} \right] + (1 - \gamma) \left[\frac{C_B + r}{1 - c_B} \right]$$

C.6 Solving Surplus

plug in b^* into S:

$$S = \mu_{\tau} + r + c_A - (C_B + C_C + r) - \left[\gamma \frac{\mu_{\tau} + r + c_A - C_C}{1 + c_C} + (1 - \gamma) \frac{C_B + r}{1 - c_B}\right] (c_B + c_C)$$

Separating the last bracket:

$$S = \mu_{\tau} + r + c_A - (C_B + C_C + r) - \gamma(c_B + c_C) \left[\frac{\mu_{\tau} + r + c_A - C_C}{1 + c_C}\right] - (1 - \gamma)(c_B + c_C) \left[\frac{C_B + r}{1 - c_B}\right]$$

Simplify the right half of the bracket:

$$S = \mu_{\tau} + r + c_A - C_C - \gamma(c_B + c_C) \left[\frac{\mu_{\tau} + r + c_A - C_C}{1 + c_C} \right] - (C_B + r) \left[1 + \frac{(1 - \gamma)(c_B + c_C)}{1 - c_B} \right]$$

Now focusing on the rest:

$$S = (\mu_{\tau} + r + c_A - C_C) \left[1 - \frac{\gamma(c_B + c_C)}{1 + c_C} \right] - (C_B + r) \left[1 + \frac{(1 - \gamma)(c_B + c_C)}{1 - c_B} \right]$$

C.7 Effects of Interventions

Below we provide more explanation to understand the effect o the interventions on the amount of formal and informal payments as visualized in Figure 3.

First, we can rewrite the bargaining constraint with respect to μ_{τ} : Bribe iff:

$$V^{E} - b(1 + c_{C}) - C_{C} > V^{E} - \mu_{\tau} - r - c_{A}$$

Plugging in b^* :

$$V^{E} - \left[\gamma \frac{\mu_{\tau} + r + c_{A} - C_{C}}{1 + c_{C}} + (1 - \gamma) \frac{C_{B} + r}{1 - c_{B}}\right] (1 + c_{C}) - C_{C} > V^{E} - \mu_{\tau} - r - c_{A}$$

Threshold (i): $\mu_{\tau} > \frac{1+\gamma}{(1-\gamma)}C_C + \left[\frac{1+c_C}{1-c_B}\right](C_B+r) - r - c_A$

Similarly we can rewrite the engagement constraint if the citizen would bargain: Engage iff:

$$V^O < V^E - b(1 + c_c) - C_C$$

Plugging in b^* :

$$V^{O} < V^{E} - \gamma(\mu_{\tau} + r + c_{A} - C_{C}) - (1 - \gamma) \left[\frac{1 + c_{C}}{1 - c_{B}}\right] (C_{B} + r) - C_{C}$$

Threshold (ii): $\mu_{\tau} < C_C - r - c_A - \frac{V^O - V^E + C_C}{\gamma} - \frac{1 - \gamma}{\gamma} \left[\frac{1 + c_C}{1 - c_B} \right] (C_B + r\tau^*)$

The engagement constraint if the citizen would go to the authorities is more straight-forward: Engage iff:

$$V^O < V^E - \mu_\tau - c_A$$
$$\mu_\tau < V^E - V^O - c_A - r$$

To plot how μ_{τ} affects the amount of taxes and bribes paid, we need to distinguish between two cases, namely whether the bargaining constraint of (i) is feasible, that is, whether it is lower than the engagement constraint of (ii).

C.8 Differences from our Pre-Registered Framework

The theoretical framework outlined above differs from what was presented in our original pre-analysis plan in three ways. First, and most importantly, the new PAP extends the theoretical framework by including the citizen's decision of whether or not to engage with the state in the first place. By including this decision in the theoretical framework we allow for changes in r and μ_{τ} to also change the incentives for citizens to become legible and start to make more formal *and* informal payments.

Second, the original PAP specifies that in the official payment equilibrium, the state agent obtains a rent that is a percentage of the tax paid by the citizen, $\tau\tau$. Instead, we now model the payoff for the state agent to be a simple lump sum payment in addition to the formal payment. We believe that this change better reflects common extractive situations in the DRC. It also better maps onto our measurement strategy since the citizen on whose reports we rely cannot assess what percentage of the formal tax reaches the state coffers. This is not to say that the state agent might not also appropriate some of the formal payment, but this is less relevant to our intervention since the citizen cannot observe it. This change does not affect the comparative statistics in a meaningful way, other than including the extractive rent payment in the citizens decision whether or not to bargain.

Third, our interpretation of the information and protection treatments as shifting μ_{τ} and r, respectively, differs from our original PAP. Our original PAP argued that the protection and information treatments moved the cost of collusion, $c_B \& C_B$, and the cost of going to the authorities, c_A , respectively (we also considered if they moved the expected tax rate and the state agent's bargaining power). For the information treatment we now focus on μ_{τ} since our interpretation of c_A has changed slightly. Instead of the cost of verifying the real tax liability, it is the cost of making an official payment. The comparative statics are the same for both parameters. In terms of the protection treatment we now argue that it would not be rational for citizens to report about (and be protected from) collusive agreements made with state agents since they are to their advantage. Instead, citizens would report bribes made on top of formal payments and be protected from such abuses by the civil society organization. Interpreting the protection treatment as lowering r means that the intervention makes official payments relatively cheaper. Previously, by lowering the cost of collusion for the state agent, the protection treatment would have moved citizens to bargain more.

C.9 Comparative Statics

Table A5: Effects of lowering r and μ_{τ} by behavior without treatment

Quantity	Parameter	Intensive Margin	Extensive Margin
Better information (if under informed)	$\mu_{\tau}\downarrow$	no change	no effect
Lowering rent extraction	$r\downarrow$	$r\downarrow$	no effect
		•	•
Already Legible	e and Bargai	ning Collusively Wi	thout Treatment
Quantity	Parameter	Intensive Margin	Extensive Margin
Better information (if under informed)	$\mu_{\tau}\downarrow$	$b\downarrow$	Pushes towards Formal Payment:
			in that case: $\tau \uparrow, r \uparrow, b = 0$
Lowering rent extraction	$r\downarrow$	$b\downarrow$	Pushes towards <i>Formal Payment</i> :

Already Legible and Making Official Payments Without Treatment

Not Legible to the State Without Treatment

in that case: $\tau \uparrow, r \uparrow, b = 0$

Quantity	Parameter	Intensive Margin	Extensive Margin
Better information (if under informed)	$\mu_{ au}\downarrow$	no change	Pushes towards Engagement with State:
			in that case either <i>Bargaining</i> : $b \uparrow$
			or Formal Payment: $\tau \uparrow, r \uparrow$
Lowering rent extraction	$r\downarrow$	no change	Pushes towards Engagement with State:
			in that case either <i>Bargaining</i> : $b \uparrow$
			or Formal Payment: $\tau \uparrow, r \uparrow$

D Research Design

D.1 Treatment Details

ODEP consultants initially recruited participants using the following scripts. All recruitment calls began with:

Hello, my name is [NAME]. I am calling you 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.

Participants then heard the following treatment specific language:

Information treatment: I am calling you to offer you advice pro-bono, and propose to renew this call every week, in order to discuss your taxes, their legality, and what you can do to avoid paying illegal taxes.

Protection treatment: I am calling you to offer advocacy on abuse by tax collectors only, and propose to renew this call every week, in order to hear about your experience and, keeping your confidentiality, following up for you doing advocacy in order to prevent such abuse from happening again.

The call then concluded with the following information and request for consent:

You can contact us at [NUMBER] and our website is [WEBSITE]. We are partly funded by DFID, the British Department for International Development, 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. Would you be willing to receive help from us?

On each weekly call, the ODEP consultants took the following steps, logging all information (see Figure A4 for a sample call log).

1. Asked about payments in the previous week. If there were payments, the ODEP expert assessed the legality of the payment and rate, with reference to official tax law as needed. The ODEP expert then determined whether any informal bribe or rent payment had likely been made.

2. Addressed potential abuse:

- Information treatment only: If the ODEP expert concluded an informal payment had been made, they informed the household or business but emphasized that no further advocacy or support could be provided to rectify the payment.
- *Protection treatment only*: If the ODEP expert concluded an informal payment had been made, they informed the household or business and announced that they would investigate the abuse.
- 3. Asked about upcoming payments. The ODEP expert then asked respondents if there were any anticipated tax or fee payments in the coming week.
- 4. Addressed potential future abuse:

0 0 1

~ .

- *Information treatment only*: The ODEP expert provided additional information on statutory payments and rates for anticipated payments, additional detail on how to navigate the payment process, and information on how the household or business could themselves report an abuse.
- *Protection treatment only*: The ODEP expert provided no further information on statutory payments, how to navigate the payment process, or self-report abuse. Instead they reminded the household or business that they could report future abuses to ODEP and ODEP would conduct follow-up.

Figure A4: Call log entry for a business. The figure shows the call log entry for one of the weeks (21-26 September) that ODEP engaged with one of the firms (id No. Kinshasa D.8.5) that was in the joint protection and information group. The log shows that the ODEP consultant discussed a payment that the business had to make with the national electricity company (Société Nationale d'Electricité, SNEL), that the payment involved an instance of "somewhat severe" abuse, and that the ODEP consultant documented the abuse and offered advice.

A	B	C	D	E	F	G	н	11	1	K	11	1.04
Taxe [UTILISEZ code]	Avez-vous discuté de la légalité de cette taxe avec le client? Non Dui	Pensez-vous qu'il y a eu de l'abus? [ANALYSE PRIVEE]	Avez-vous informe le client de l'abus? (Remarque seuls les clients dans les groupes I et III doit être informé)	S'fl y a eu abus, avez- vous annoncé qu'il n'y aurait pas de suivi de la part de l'ODEP?	S'il y a eu abus: Commune où il est survenu	Avenue où Il est survenu	Date à laquelle il est survenu	Organisme/agence de collecte de l'impôt	Collecteur de taxe (Note: Enregistrez cette information si elle est fournie, mais demandez cette information uniquement pour les groupes II et	Ampleur de l'abus 1 Très grave 2 Un peu sévère 3 Pas trop grave 4 Pas grave du tout	Avez-vous fourni de soutien de navigation dans le système fiscal au client?	Uniquemen groupes II and III Avez-vous fourni des information d'assistance sur les abus
204	σιί	σιί	ชาน	σщ	Lemba	Mban29 iemba	/	SNER	Agent	2	σιų	σω

D.2 Open-ended Responses

Qualitative accounts reinforce the empowerment interpretation. After the final week of smartphone data collection, we asked respondents who discussed payments with ODEP in the prior week about the nature of these discussions. Some respondents offered open-ended responses, and among those who did, we noted differences across treatment arms. In the information-only treatment, respondents regularly emphasized that the interactions with ODEP helped to avoid paying excessive taxes and how to "pay taxes properly," while in the protection-only treatment, respondents mentioned "claiming our rights" and "protecting ourselves from people who harass and tax us informally." In both treatment groups, respondents regularly indicated that they understood ODEP to be trying to reduce the extent of informal taxation and sometimes indicated that a consequence of the interactions with ODEP was to begin paying taxes at the agencies, town hall, or bank rather than to street-level collectors.

D.3 Randomization

Our two-stage random assignment used "restricted random assignment" that required that the assignments be balanced within strata defined by commune and household versus business survey sample.³⁰ The implementation was done by generating 15,000 treatment assignment permutations. Then, only those assignments that satisfied the balance constraints were retained and, from among the retained assignments, one was chosen. This procedure allows us to determine the probabilities of assignment to each of the treatment conditions by examining the permutations that were admissible under the balance constraints. The restricted randomization did not depart substantially from uniform assignment (with second stage assignment probabilities of being very close to 1/3 each).

To calculate the relevant propensity scores, we use the first stage assignment probability for control subjects and then the product of first and second stage assignment probabilities for treated subjects. The formulas are as follows:

Pr(Control) = (No. avenues in commune in control)/(No. avenues in commune) $Pr(\text{Information}) = [(\text{No. avenues in commune treated})/(\text{No. avenues in commune})] * p_{inf}$ $Pr(\text{Protection}) = [(\text{No. avenues in commune treated})/(\text{No. avenues in commune})] * p_{pro}$ $Pr(\text{Information} + \text{Protection}) = [(\text{No. avenues in commune treated})/(\text{No. avenues in commune})] * p_{infpro},$

where p_{inf} , p_{pro} , and p_{infpro} are the second stage assignment probabilities. Our analysis weights by the inverse of these propensity scores.

³⁰Bruhn, Miriam and David McKenzie. 2009. "In Pursuit of Balance: Randomization in Practice in Development Field Experiments." American Economic Review 1(4):200-232.

D.4 Balance

Panel A: Effects of Pro Outcome Variable	tection Treatment Coefficients	P-value
Gender	0.0065 (0.076)	0.932
Household Size	-0.024(0.25)	0.925
Education	-0.38(0.14)	0.0114
Age	-0.41(1.3)	0.748
Wealth (log)	-0.27(0.29)	0.372
Network Z-score	-0.036(0.13)	0.786
Number of Employees	-0.14(0.22)	0.54
Profit (log)	$0.21 \ (0.15)$	0.176
Panel B: Effects of Con	sulting Treatment	
Panel B: Effects of Con Outcome Variable	sulting Treatment Coefficients	P-value
Panel B: Effects of Con Outcome Variable Gender	Coefficients	P-value 0.936
Panel B: Effects of Con Outcome Variable Gender Household Size	sulting Treatment Coefficients -0.0055 (0.068) -0.038 (0.25)	P-value 0.936 0.878
Panel B: Effects of Con Outcome Variable Gender Household Size Education	sulting Treatment Coefficients -0.0055 (0.068) -0.038 (0.25) -0.016 (0.19)	P-value 0.936 0.878 0.933
Panel B: Effects of Con Outcome Variable Gender Household Size Education Age		P-value 0.936 0.878 0.933 0.646
Panel B: Effects of Com Outcome Variable Gender Household Size Education Age Wealth (log)	sulting Treatment Coefficients -0.0055 (0.068) -0.038 (0.25) -0.016 (0.19) 0.64 (1.4) -0.43 (0.27)	P-value 0.936 0.878 0.933 0.646 0.127
Panel B: Effects of Com Outcome Variable Gender Household Size Education Age Wealth (log) Network Z-score	sulting Treatment Coefficients -0.0055 (0.068) -0.038 (0.25) -0.016 (0.19) 0.64 (1.4) -0.43 (0.27) -0.11 (0.098)	P-value 0.936 0.878 0.933 0.646 0.127 0.257
Panel B: Effects of Com Outcome Variable Gender Household Size Education Age Wealth (log) Network Z-score Number of Employees	$\begin{array}{c} \text{sulting Treatment} \\ \hline \\ $	P-value 0.936 0.878 0.933 0.646 0.127 0.257 0.894

Table A6: Effects of Treatment Indicators on Coefficients

D.5 Compliance

To assess compliance, we had ODEP record what happened after each attempt to contact one of the respondents assigned to a treatment condition. The ODEP staff recorded the mode of contact and then whether the attempt to contact succeeded in allowing for a discussion with the intended respondent. We obtained records for 1,009 such contact attempts. The mode of contact was primarily via telephone—78% of attempts in the information-only group, 78% in the protection only group, and 75% for the information plus protection group, with the remainder being cases where contact information was in error or missing (between 9% to 14%) or attempts made in person (between 11% to 13%). Contact attempts were successful in 67% of attempts in the information only group, 63% in the protection only group, and 66% in the information plus treatment group. The similarity in these rates across treatment conditions mean that differences in compliance would not explain substantial differences in treatment effects across the arms.

E Additional Results and Robustness

E.1 Main Results Tables

		Full Sample		Restricted Sample				
	De	Dependent Variable:			Dependent Variable:			
	Any Payment (1)	Any Formal (2)	Only Formal (3)	Any Payment (4)	Any Formal (5)	Only Formal (6)		
Protection	0.019^{**} (0.009)	0.020^{**} (0.009)	0.017^{**} (0.007)	0.043^{**} (0.019)	0.043^{**} (0.018)	0.047^{***} (0.014)		
Information	$0.004 \\ (0.007)$	$0.003 \\ (0.006)$	$0.005 \\ (0.005)$	0.029^{*} (0.016)	0.025^{*} (0.014)	0.022^{*} (0.013)		
R^2 (Protection) R^2 (Information) Observations Control Mean	$\begin{array}{c} 0.109 \\ 0.109 \\ 63,747 \\ 0.06 \end{array}$	$\begin{array}{c} 0.096 \\ 0.096 \\ 63,747 \\ 0.05 \end{array}$	$0.082 \\ 0.083 \\ 63,747 \\ 0.02$	$\begin{array}{c} 0.095 \\ 0.094 \\ 13,994 \\ 0.16 \end{array}$	$\begin{array}{c} 0.089 \\ 0.092 \\ 13,994 \\ 0.14 \end{array}$	$\begin{array}{c} 0.104 \\ 0.105 \\ 13,994 \\ 0.06 \end{array}$		

Table A7: Extensive Margin Effects of Protection and Tax Consulting

***p < 0.01; **p < 0.05; *p < 0.1

Table A8: Extensive Margin Effects of Protection and Tax Consulting Separate for Households and Firms

	Households			Businesses				
	De	pendent Variab	le:	De	Dependent Variable:			
	Any Payment (1)	Any Formal (2)	Only Formal (3)	Any Payment (4)	Any Formal (5)	Only Formal (6)		
Protection	0.027^{**} (0.011)	0.029^{**} (0.011)	0.030^{***} (0.008)	0.013 (0.015)	$0.010 \\ (0.014)$	0.007 (0.011)		
Information	$0.008 \\ (0.008)$	$0.010 \\ (0.008)$	$0.008 \\ (0.007)$	$0.006 \\ (0.008)$	$0.005 \\ (0.008)$	$0.012 \\ (0.008)$		
R^2 (Protection) R^2 (Information) Observations Control Mean	$\begin{array}{c} 0.128 \\ 0.126 \\ 33,533 \\ 0.09 \end{array}$	$\begin{array}{c} 0.114 \\ 0.113 \\ 33,533 \\ 0.08 \end{array}$	$0.105 \\ 0.101 \\ 33,533 \\ 0.04$	$\begin{array}{c} 0.090 \\ 0.092 \\ 30,214 \\ 0.04 \end{array}$	$\begin{array}{c} 0.078 \\ 0.082 \\ 30,214 \\ 0.03 \end{array}$	$0.067 \\ 0.073 \\ 30,214 \\ 0.01$		

***p < 0.01; **p < 0.05; *p < 0.1

Table A9: Intensive Margin Effects and ATE of Protection and Tax Consulting

				Depe	ndent Variable:	Amount Pa	id (USD)			
			Full Sample				F	testricted Samp	le	
	ATE (1)	Conditional on post (2)	Lower bound (3)	Upper bound (4)	Conditional on pre (5)	ATE (6)	Conditional on post (7)	Lower bound (8)	Upper bound (9)	Conditional on pre (10)
Protection	-0.344 (1.099)	-21.815^{**} (10.194)	-46.039^{***} (10.270)	-14.303 (9.976)	0.673 (1.762)	$\begin{array}{c} 0.873 \\ (1.537) \end{array}$	-5.517 (6.272)	-25.946^{***} (5.325)	-1.487 (5.983)	$1.250 \\ (1.903)$
Information	-0.269 (0.686)	-7.527 (8.082)	-23.694^{***} (7.308)	-5.456 (8.290)	$2.105 \\ (1.349)$	-0.185 (1.252)	-4.377 (8.065)	-22.635^{***} (7.243)	-1.322 (6.918)	$1.609 \\ (1.317)$
R^2 (Protection) R^2 (Information) Observations Control Mean	$0.013 \\ 0.012 \\ 63,747 \\ 2.69$	$0.156 \\ 0.109 \\ 4,341 \\ 44.18$	$0.237 \\ 0.158 \\ 4,253 \\ 44.18$	$0.157 \\ 0.110 \\ 4,269 \\ 44.18$	$0.023 \\ 0.020 \\ 9,404 \\ 4.52$	0.018 0.016 13,994 5.53	$0.104 \\ 0.097 \\ 2,397 \\ 34.00$	$0.224 \\ 0.228 \\ 2,242 \\ 34.00$	$0.109 \\ 0.098 \\ 2,246 \\ 34.00$	$0.023 \\ 0.021 \\ 7,191 \\ 4.55$



Figure A5: Effects by Payment Category

Notes: This figure shows the coefficients for the protection and information treatments on the extensive margin by payment category for households (Panel A) and firms (Panel B) and the average treatment effect estimates on payments for the protection and information treatments by payment category for households (Panel C) and firms (Panel D). Categories above the grey dashed horizontal line are those included in the restricted sample. The control mean for each category is in parentheses.

E.2 Conditioning on positive baseline outcomes

In our pre-analysis plan, we proposed that we could combine the monotonicity assumption discussed in the main text with an additional assumption on baseline outcomes to point identify intensive margin effects, at least for a subset of our target population. Suppose that t = 0 indexes the pretreatment period, and t > 0 indexes periods after the intervention started. Under monotonicity, for t > 0, those with $Y_{i,t,j} > 0$ in control group are always responders, while in the treatment group those with $Y_{i,t,j} > 0$ are a mix of always responders and units that would have had $Y_{i,t,j} = 0$ had they not been treated. If it were the case that, any unit with $Y_{i,0,j} > 0$ would assuredly have $Y_{i,t,j} > 0$ for t > 0, then by conditioning on both $Y_{i,0,j} > 0$ and $Y_{i,t,j} > 0$ for t > 0, we could point identify an intensive margin effect that is local to always responders for whom $Y_{i,0,j} > 0$. Unfortunately, the necessary identifying assumption is badly violated in our sample. Indeed, we find that among units for whom we recorded a pre-treatment payment in category j, fully 84% recorded no payment during the post-treatment periods. This means that we cannot point identify the local intensive margin effect, nor could we construct informative bounds. The reason for this discrepancy, we believe, is that the baseline survey recorded payment activity over a period of a full year, whereas the post-treatment smartphone data collection only ran for up to 19 weeks.

E.3 Alternative Specifications and Subsamples

Table A10: Extensive Margin Effects of Protection and Tax Consulting Without Interacting Covariates and Treatment

	Full Sample Dependent Variable:			Restricted Sample Dependent Variable:			
	Any Payment (1)	Any Formal (2)	Only Formal (3)	Any Payment (4)	Any Formal (5)	Only Formal (6)	
Protection	0.019^{*} (0.010)	0.019^{*} (0.010)	0.016^{*} (0.009)	$0.038 \\ (0.024)$	0.040^{*} (0.021)	0.044^{**} (0.020)	
Information	$0.002 \\ (0.008)$	$0.001 \\ (0.007)$	0.003 (0.006)	$0.024 \\ (0.019)$	$0.019 \\ (0.017)$	$0.017 \\ (0.014)$	
R ² Observations Control Mean	$0.100 \\ 63,747 \\ 0.06$	$0.087 \\ 63,747 \\ 0.05$	$0.069 \\ 63,747 \\ 0.02$	$0.074 \\ 13,994 \\ 0.16$	$0.071 \\ 13,994 \\ 0.14$	$0.082 \\ 13,994 \\ 0.06$	

***p < 0.01; ** p < 0.05; * p < 0.1

Table A11: Extensive Margin Effects of Protection and Tax Consulting Without Covariates

	Dependent Variable:						
	Any Payment (1)	Any Formal (2)	Only Formal (3)				
Protection	0.018*	0.019**	0.014 (0.009)				
Information	(0.000) (0.003) (0.007)	(0.002) (0.007)	(0.000) (0.004) (0.006)				
R ² Observations Control Mean	$0.097 \\ 63,747 \\ 0.06$	$0.084 \\ 63,747 \\ 0.05$	$0.062 \\ 63,747 \\ 0.02$				

	De	pendent Variab	le:
	Any Payment	Any Formal	Only Formal
	(1)	(2)	(3)
Protection	0.019^{**}	0.020**	0.017^{**}
	(0.009)	(0.009)	(0.007)
Information	0.004	0.003	0.005
	(0.007)	(0.006)	(0.005)
Protection \times (demeaned) Information	0.013	0.007	-0.002
	(0.016)	(0.014)	(0.014)
Information \times (demeaned) Protection	-0.000	-0.005	-0.013
	(0.018)	(0.016)	(0.014)
R^2 (Protection)	0.109	0.096	0.082
R^2 (Information)	0.109	0.096	0.083
Observations	63,747	63,747	63,747
Control Mean	0.06	0.05	0.02

Table A12: Extensive Margin Effects of Protection and Tax Consulting and Both

***p < 0.01; **p < 0.05; *p < 0.1

	Dependent Variable: Amount Paid (USD)						
	Original Coding (1)	Winsorized 99 (2)	Winsorized 95 (3)				
Protection	-0.344 (1.099)	$0.236 \\ (0.753)$	$0.232 \\ (0.446)$				
Information	-0.269 (0.686)	-0.505 (0.488)	-0.290 (0.276)				
R^2 (Protection) R^2 (Information)	$0.013 \\ 0.012$	$0.031 \\ 0.028$	$0.043 \\ 0.041$				
Observations Control Mean	$63,747 \\ 2.69$	$63,747 \\ 2.14$	$63,747 \\ 1.64$				

Table A13: ATE of Protection and Tax Consulting with Different Coding

Table A14: Intensive Margin Effect (Conditional on Post) of Protection and Tax Consulting with Different Coding

	Dependent Variable: Amount Paid (USD)						
	Original Coding (1)	Winsorized 99 (2)	Winsorized 95 (3)				
Protection	-21.815^{**} (10.194)	-9.995^{*} (5.717)	-5.059^{*} (2.915)				
Information	-7.527 (8.082)	-6.839 (5.690)	-3.842 (2.892)				
R^2 (Protection)	0.156	0.254	0.346				
\mathbb{R}^2 (Information)	0.109	0.223	0.318				
Observations	4,341	4,341	4,341				
Control Mean	44.18	35.12	26.85				

	Uncondi	tional	Conditional		
			on Payment	on Interaction	
		Depen	dent Variable:		
	Negotiated (1)	Refused (2)	Negotiated (3)	Refused (4)	
Protection	$0.009 \\ (0.007)$	-0.001 (0.003)	-0.008 (0.044)	-0.056^{*} (0.029)	
Information	$0.009 \\ (0.006)$	-0.005 (0.004)	$\begin{array}{c} 0.111^{**} \\ (0.050) \end{array}$	-0.066^{*} (0.034)	
R^2 (Protection) R^2 (Information) Observations	$0.045 \\ 0.044 \\ 63,747$	$0.014 \\ 0.016 \\ 63,747$	$0.218 \\ 0.218 \\ 4,341$	$0.138 \\ 0.143 \\ 4,938$	

Table A15: Extensive Margin Effects of Protection and Tax Consulting on Refusal to Pay and Negotiation

- ***p < 0.01; **p < 0.05; *p < 0.1

Table A16: Effects of Protection and Tax Consulting When Collapsing to Whole Period

	Any Payment (1)	Any Formal (2)	Only Formal (3)	$\begin{array}{c} \text{ATE} \\ (4) \end{array}$	$\begin{array}{c} \text{COP} \\ (5) \end{array}$
Protection	$0.040 \\ (0.030)$	$0.028 \\ (0.029)$	$0.010 \\ (0.029)$	-4.932 (13.764)	-51.183 (34.287)
Information	$0.005 \\ (0.023)$	$0.006 \\ (0.024)$	$0.009 \\ (0.018)$	-4.540 (9.388)	-31.188 (30.489)
R^2 (Protection) R^2 (Information) Observations	$\begin{array}{c} 0.339 \\ 0.334 \\ 4,762 \end{array}$	$0.323 \\ 0.320 \\ 4,762$	$0.234 \\ 0.240 \\ 4,762$	$0.113 \\ 0.103 \\ 4,762$	$0.236 \\ 0.186 \\ 1,138$

***p < 0.01; **p < 0.05; *p < 0.1

	Dependent Variable						
	Any Informal	Informal Amount	Informal Amount				
			COP				
	(1)	(2)	(3)				
Protection	0.002	-0.308	-5.989				
	(0.007)	(0.300)	(3.653)				
Information	-0.001	-0.288	-8.142^{*}				
	(0.006)	(0.341)	(4.428)				
\mathbb{R}^2 (Protection)	0.057	0.009	0.162				
R^2 (Information)	0.056	0.008	0.145				
Observations	63,747	63,747	2,457				

Table A17: Effects of Protection and Tax Consulting on Informal Payments and Amounts



Figure A6: Effect of Protection and Information When Dropping Payment Categories

Notes: This figure shows the coefficients for the protection and information treatments when dropping each payment category individually. Panel A shows the extensive margin effect on any payment and Panel B shows the intensive margin effect on total amount paid conditional on positive post payments when dropping each payment category individually.

	Baseline Payers			Baseline Non-Payers		
	Any Payment (1)	Any Formal (2)	Only Formal (3)	Any Payment (4)	Any Formal (5)	Only Formal (6)
Protection	-0.005 (0.024)	0.010 (0.023)	$0.027 \\ (0.024)$	0.020^{**} (0.008)	0.018^{**} (0.008)	$0.012 \\ (0.008)$
Information	$0.028 \\ (0.019)$	$0.027 \\ (0.019)$	$0.018 \\ (0.018)$	-0.000 (0.007)	-0.002 (0.007)	$0.004 \\ (0.004)$
R^2 (Protection) R^2 (Information) Observations	$0.151 \\ 0.159 \\ 8,085$	$0.133 \\ 0.141 \\ 8,085$	$0.132 \\ 0.127 \\ 8,085$	$0.128 \\ 0.119 \\ 44,842$	$\begin{array}{c} 0.114 \\ 0.105 \\ 44,842 \end{array}$	$0.096 \\ 0.094 \\ 44,842$

Table A18: Extensive Margin Effects of Protection and Tax Consulting By Baseline Payment Status

	Any Payment (1)	Any Formal (2)	Only Formal (3)	ATE (4)	Conditional on Post (5)
Protection	0.019^{**} (0.009)	0.020^{**} (0.009)	0.017^{**} (0.007)	-0.344 (1.099)	-21.815^{**} (10.194)
Information	$0.004 \\ (0.007)$	0.003 (0.006)	$0.005 \\ (0.005)$	-0.269 (0.686)	-7.527 (8.082)
Education (Protection)	-0.001 (0.002)	-0.002 (0.002)	-0.001 (0.002)	-0.800 (0.534)	-4.651^{*} (2.621)
Education (Information)	$0.003 \\ (0.003)$	$0.002 \\ (0.003)$	$0.003 \\ (0.003)$	$\begin{array}{c} 0.172 \\ (0.372) \end{array}$	0.547 (2.922)
Network Z-Score (Protection)	$\begin{array}{c} 0.001 \\ (0.005) \end{array}$	$0.003 \\ (0.005)$	$0.004 \\ (0.003)$	$0.884 \\ (0.821)$	4.025 (6.692)
Network Z-Score (Information)	$0.002 \\ (0.006)$	$0.002 \\ (0.006)$	$0.003 \\ (0.004)$	-0.131 (0.403)	-2.670 (3.169)
Protection \times Education	$0.005 \\ (0.005)$	$0.006 \\ (0.005)$	0.007^{*} (0.004)	1.438^{*} (0.729)	$\begin{array}{c} 10.234^{**} \\ (4.173) \end{array}$
Information \times Education	-0.001 (0.004)	-0.001 (0.004)	$0.000 \\ (0.003)$	-0.660 (0.611)	-8.886 (6.612)
Protection \times Network Z-Score	-0.003 (0.008)	-0.007 (0.008)	-0.004 (0.007)	-1.391 (0.835)	-10.741 (6.922)
Information \times Network Z-Score	$0.000 \\ (0.008)$	0.001 (0.007)	$0.002 \\ (0.006)$	$1.865 \\ (1.615)$	$19.145 \\ (16.596)$
R ² (Protection) R ² (Information) Observations Control Mean	$\begin{array}{c} 0.109 \\ 0.109 \\ 63,747 \\ 0.06 \end{array}$	$\begin{array}{r} 0.096 \\ 0.096 \\ 63,747 \\ 0.05 \end{array}$	$\begin{array}{c} 0.082 \\ 0.083 \\ 63,747 \\ 0.02 \end{array}$	$\begin{array}{r} 0.013 \\ 0.012 \\ 63,747 \\ 2.69 \end{array}$	$\begin{array}{c} 0.156 \\ 0.109 \\ 4,341 \\ 44.18 \end{array}$

Table A19: Heterogeneous Treatment Effects

Notes: This table shows the coefficients for the Protection and Information treatments and their interactions with respondents education and network connections. Standard errors, clustered at the avenue level, are in parentheses. ***p < 0.01; **p < 0.05; *p < 0.1

E.4 Heterogeneous Effects

The logic of our theory suggests that empowerment would especially benefit those who previously lacked information or power endowments either to bargain effectively with street-level state agents or navigate demands for informal rents in combination with formal payments. Our pre-analysis plan proposed to test this using two measures of such endowments: (1) a "network z-score" that is a standardized count of the number of ties that the respondent has to elites at different levels and from different government agencies and (2) an educational attainment variable that varies from 1 to 7 indicating no formal schooling through to post-university degree. Table A19 shows no substantial moderator effects for the extensive margin, although we do find indication of moderator effects for the amounts paid.

Additional Online Appendix

"Seeing like a Citizen: Experimental Evidence on How Empowerment Affects Engagement with the State"

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A Advocacy Campaign

The two documents below provide important detail on the advocacy campaign. The document *Plan de Plaidoyer* outlines the plan for the advocacy campaign. The most relevant sections include Section 7 *Plan des actions precises secteur pour le plaidoyer* (Action Plan for Advocacy by Sector) and Section 8 *Cas d'abus* (Cases of abuse), which documents cases of abuses documented as part of the protection treatment.

The document "Report of the Meeting with Mayors" presents sample minutes from a meeting that ODEP organized with the chief executives of the communes to advocate for households and businesses to reduce informal and illegal fees.

PLAN DE PLAIDOYER

1. Objectif :

D'ici fin décembre 2015, réduire de 30% les paiements d'Impôts et Taxes informelles dans les 98 ménages et 68 Petites et moyennes entreprises de la ville de Kinshasa.

2. Résultats attendus :

30% des ménages et Petites et moyennes des avenues cibles ne payent plus d'Impôts et Taxes informels

3. Identification des services d'assiette ciblée

Les services d'assiette concernés par les actions de plaidoyer sont les suivants :

- 1. Service environnement/ Commune
- 2. Industries, Petites et Moyennes Entreprises « IPEMEA »
- 3. Direction Générale des Migrations « DGM »,
- 4. Agence Nationale des Renseignements « ANR »,
- 5. BRIGADE DE MŒURS
- 6. POLICE NATIONALE CONGOLAISE PNC
- 7. Société Nationale d'Electricité « SNEL »
- 8. Régies des voies Maritimes « RVM »,
- 9. Direction Générale des Recettes de Kinshasa « DGRK »
- 10. Fond de promotion culturelle
- 11. Service d'habitat/commune
- 12. Service d'Etat-civil
- 13. REGIDESO

4. Identification des corporations de ménages et entreprises

a. Pour les entreprises

- Confédération des Petites et Moyennes Entreprises du Congo « COPEMECO »
- Fédération des Organisations non gouvernementales laïques à vocation économique du Congo « FOLECO »
- Fédération Nationale des Artisans, Petites et Moyennes Entreprises Congolaises « FENAPEC »
- Syndicat national des Vendeurs du Congo

b. Pour les ménages

- Associations des Consommateurs
- Associations des Parents d'Elèves et des Etudiants du Congo
- Associations des Santés
- ONG

5. Règles à appliquer

Le plaidoyer prendra en comptes les cas des entreprises ou ménages ayant faits l'objet des prélèvements des taxes illégales constatés par les experts dans les communes concernés.

6. Acte générateurs à observer

a. Pour les Petites et Moyennes Entreprises

- Patente
- Taxe sur l'environnement et l'hygiène
- Pollution, Ets dangereux et insalubre,
- Culture et art,
- Sécurité de la police (DGM, ANR, BRIGADE DES MŒURS)
- Fourniture d'électricité
- Taxe au port domestique
- Taxe sur procès-verbal de vérification des documents commerciaux
- Taxe de promotion culturelle
- Taxe journalier/prélèvement Informelle

b. Pour les ménages

- Fournitures d'électricité
- Impôt foncier
- Taxe sur fiche parcellaire
- Conflit parcellaire
- Autorisation de bâtir
- Achat fiche de recensement
- Frais spécial de roulage
- Frais de suivi des côtes
- Frais de suivi des côtes
- Permis de conduire
- Tracasserie routière
- Obtention des documents à l'Etat civil
- Fournitures de l'eau par la REGIDESO
- Frais informels d'inhumation
- Taxe d'homologation de l'Hôtel
- Accès à l'aéroport de Ndjili pour voyager
- Plaque de numérotation parcellaire
- Vignette
- Vente des mitrailles
- Obtention des documents au service de l'IPMEA ET ECONOMIE

7. Plan des actions précises par secteur pour le plaidoyer

Etapes	Activités	Objectifs	Résultats attendus	Périodes	Responsables	Observations
1	Rencontres avec des responsables des services d'assiette concernés et des associations membres des entreprises et des ménages	Présenter les cas d'abus commis par les agents des différents services	les cas d'abus commis par les agents des différents services sont présentés	Du 17 au 19 Novembre	Charles ISULA, Adelard MPAKA ; Olivier LUTUMBA	distribution des invitations
2	Correspondances auprès des responsables hiérarchiques des services d'assiette	Informer les responsables hiérarchiques des cas d'abus commis par les agents des différents services	Les autorités hiérarchiques des différents services sont informées des cas d'abus commis	23 et 24 Novembre	Charles ISULA, Adelard MPAKA ; Olivier LUTUMBA	
3	Publication des cas d'abus constatés	Informer la population des cas d'abus commis par les agents des différents services	La population est informée sur les cas d'abus commis	25 Novembre	Rycky MAPAMA	
4	Rencontre avec les autorités politico- administratives	Influencer les autorités politico- administratives en vue de prendre des décisions sur les cas d'abus commis par les agents des différents services	les autorités politico- administratives sont influencées	Le 27 Novembre	L'équipe de l'ODEP	

8. Cas d'abus

a. Pour les Petites et Moyennes Entreprises

Acte générateur	Plainte enregistrée ou faits à observer	Services
-		Service environnement/
Taxe sur l'environnement	Paye 5.000 FC sans quittance	Commune
Patente	Paye 13.000/trimestre pour la patente soit 52.000FC l'an	IPEMEA
Taxe sur l'environnement et l'hygiène	Paye 8.000 FC sans preuve de payement	Service environnement
Pollution, Ets dangereux et insalubre,	4000fc respectivement aux services (DGM, ANR, BRIGADE DES MŒURS) et ne reçoit aucune preuve de payement,	Service environnement
culture et art,	15.000 FC en date du 04/10,	Culture et art de la commune
sécurité de la police	4.000 FC chaque semaine	POLICE, DGM, ANR, BRIGADE DE MŒURS (voir parquet de la république)
Fourniture d'électricité	Avoir payé 10 dollars pour rétablissement d'électricité sans suite	SNEL
Taxe au port domestique	2.900 FC de recouvrement/ sac de 100 kg	Recouvrement
	1.600 FC au poste de la police pour 100kg,	POLICE, RVM, DGRK
Taxe sur procès-verbal de vérification des documents commerciaux	avoir payé 15\$ au lieu de 5\$	IPEMEA
Taxe de promotion culturelle	Avoir payé 150\$ en deux tranches et pourtant la loi exige 50\$ pour une cordonnerie ordinaire,	Fond de promotion culturelle
Taxe journalier/prélèvement Informelle	500FC par table et 500 FC chaque samedi pour le Salongo	POLICE Communale

<u>b. Pour les ménages</u>

Acte générateur (taxes à observer)	Plaintes enregistrées ou faits à observer/ Services
Fournitures d'électricité	Cotisations pour éviter les coupures (Frais informels pour éviter les coupures d'électricité)
	Remplacement des câbles d'électricité endommagés
	Remplacement des câbles d'électricité endommagés
	Remplacement des câbles d'électricité endommagés
	Accélérer la réparation de pannes
	Implantation des poteaux de transport du courant électrique
	frais informels pour accélérer les réparations
	Achat délestage
	Participation à la réparation de départ de l'avenue de câbles de la SNEL
	Paiement de pourboire de 10 \$ pour retarder l'interruption de la fourniture de l'énergie électrique/ recouvrement forcé
	paiement pour éviter la coupure de la fourniture électrique 2000FC/porte
Impôt foncier	Les agents de la DGRK ont exigé à cet assujetti de payer 70 \$ pour payer au lieu de 6 000 FC comme l'exige les textes réglementaires en la matière, avoir payé les 6 000 en mains auprès des agents de la DGRK sans preuve de paiement
	Les agents de la DGRK ont exigé à cet assujetti de payer 500 \$ au lieu de 30 000 FC comme l'exige les textes réglementaires en la matière.
Taxe sur fiche parcellaire	Taxe de 6 800 CDF sur la fiche parcellaire
Conflit parcellaire	Vente d'une partie de sa parcelle à Kinkole C/ N'sele, paiement de 150 \$ pour régler le conflit et annulation de la nouvelle fiche parcellaire du 2ème acquéreur de la partie de sa parcelle au Quartier Kinkole

Autorisation de bâtir	Avoir été taxé 200 \$ en vue de l'obtention sous menace des agents/Autorisation de bâtir pour une maison d'habitation sans étage
Achat fiche de recensement	Recensement des habitants dans une parcelle 3000 FC/porte soient un total de 9 000 CDF pour toute la parcelle
	Recensement des habitants dans une parcelle 1 000 FC avec menaces en y imixant la Police Par l'Agent BUANGA de l'Etat civil
Frais spécial de roulage	Mbote ya likasu
	Mbote ya likasu (10 000 CDF pour une infraction imaginaire du clignotant existant
	Mbote ya likasu
Frais de suivi des côtes	Frais de branchement 50 \$
Frais de suivi des côtes	frais de branchement non spécifié
Permis de conduire	Avoir été taxé 210 \$ au lieu de 50 \$ selon les prescrits des textes
Tracasserie routière	Véhicule arrêté ayant tous les documents requis pour l'exploitation sur la route et avoir été sommé à payer 70 000 FC auprès des policiers circulant à bord des jeeps, immatriculées PNC
Obtention des documents à l'Etat civil	Attestation de perte de pièce au Commissariat de la Police 10 000 CDF au lieu de 5 000 CDF
Fournitures de l'eau par la REGIDESO	Attestation de bonne vie, conduite et mœurs, de naissance 14 000 CDF aux termes de l'Arrêté du Gouverneur de la ville de Kinshasa, n° SC/072/BGV/PSD/FINECO&IPMEA/2013 du 26 mars 2013 est fixé à 2 \$
	Motivation aux agents de la REGIDESO pour éviter l'interruption de la fourniture d'eau
	frais informels la REGIDESO remis aux agents de pour la fourniture d'eau 3500 FC
	frais informels pour la fourniture d'eau pour éviter la coupure
Frais informels d'inhumation	paiement pour passage du corps au cimetière de Mikonga
Taxe d'homologation de l'Hôtel	Paiement de 15 \$ comme pourboire aux agents communaux du Tourisme à part le montant de 200 \$ payés normalement à la DGRK, en plus accompagné par le service d'environnement pour la taxe rémunératoire annuel de 150 \$
Accès à l'aéroport de Ndjili pour voyager	Paiement de 5 000 CDF à titre informel pour se frayer le passage sur 5 barrières érigées, pour accompagner son fils en voyage pour Lubumbashi à part le Go-pass et la taxe provinciale de transport
Plaque de numérotation parcellaire	Avoir été taxé 5 000 CDF pour la plaque de numérotation parcellaire
Vignette	voudrait qu'il v ait vérification des documents de bord pour certitude du montant à paver)
Vente des mitrailles	Avoir payé 25 000 CDF en mains auprès des agents de la DGRK sans preuve de paiement
--	---
Obtention des documents au service de l'IPMEA ET ECONOMIE	Paiement de l'Autorisation d'ouverture et du permis d'exploitation de la pharmacie respectivement 50 \$ et 30 \$ sans note de perception (du cash entre les mains des agents de la commune)

Note: This is a translation from the French by Chat GPT. The original document is below

REPORT OF THE MEETING WITH MAYORS

AGENDA ITEM:

A single item was on the agenda: advocating for households and businesses to reduce informal and illegal fees and harassment by 30%.

Location: Municipal Hall of NGABA

Moderator: Olivier LUTUMBA

Start Time: 10:45 AM

Date: December 1, 2015

The Mayor of the Ngaba commune, as the Chair of the Kinshasa City Steering Group and host of the meeting, delivered a welcome address.

The meeting began with a brief introduction of the attendees, each stating their name and position (attendance list attached).

Following the introductions, the moderator, Mr. Olivier Lutumba, introduced Mr. Valery Madianga, in charge of communications for ODEP, who eloquently presented the organization's objectives. Subsequently, Mr. Jacques Katchelewa, ODEP's Program Manager, presented the results of a study, explaining the findings and methodology used in the 22 targeted communes of the Kinshasa city-province.

In his speech, the Program Manager expressed gratitude to the attending mayors and their deputies, as well as Bureau Chiefs, for the successful preliminary meetings held on November 19, 2015. These meetings paved the way for the current discussions with revenue-generating services in the three districts affected by complaints regarding informal and illegal charges, namely Tshangu (Ndjili, Kimbanseke, Masina communes), Funa (Kalamu, Ngiri-Ngiri, Bumbu, Bandal, and Selembao communes), and Mont Amba (Lemba, Ngaba, Matete communes).

ODEP's approach focuses on synergy with various services and taxpayers to mobilize revenues without harassment or barriers, aligning with the belief that excessive taxation ruins tax collection.

The Mayor of Masina noted that the issues were not new and that even the city's Mayor was aware of them. She shared an incident where she had reported an agent from the Kinshasa Revenue Directorate for similar actions; however, the agent was released from the Makala central prison three days later due to internal support.

The Mayor of Bandalungwa emphasized addressing the root causes of these issues, considering the country's socio-economic conditions.

Kimbanseke's Mayor suggested that perception issues did not solely rest on assigned agents. Instead, ODEP and collaborating civil society structures should educate the public about the complexities of tax procedures through theater and media in national languages.

The phenomenon of informal taxi operations in Kinshasa communes, which exacerbates illegal and informal charges, was raised as a concern. It was suggested that establishing an orientation service at entry points directing users to a single-window system—successfully implemented in Kimbanseke—could address this issue.

Agreements:

- Post legal and regulatory texts on portable noticeboards outside each municipal office.
- Educate the public on tax collection procedures through popular theater.
- Advocate with decision-makers to support communes in asserting their rights.
- Train state personnel involved in tax collection as part of a participatory budgeting framework.
- Support communes in developing participatory budgets to mobilize up to 80% of revenues.
- Raise awareness among tax agents through civic education to reduce misconduct and naivety among taxpayers.

All participants agreed that the discussions were productive and called for more such meetings in the future to find collaborative solutions to public interest issues between the public authorities and civil society.

The meeting was concluded with closing remarks by the host, the Mayor of Ngaba.

Start Time: 10:45 AM End Time: 12:05 PM Kinshasa, December 2, 2015 Reporter: Charles ISULA, ODEP Fiscal Consultant

COMPTE RENDU DE LA REUNION AVEC LES BOURGMESTRES

POINT INSCRIT A L'ORDRE DU JOUR :

Un seul point était inscrit à l'ordre du jour : plaidoyer en faveur des ménages et entreprises afin de réduire de 30 % les tracasseries et les prélèvements informels et illégaux

Lieu : Maison communale de NGABA

Modérateur : Olivier LUTUMBA

Heure de début : 10h45

Date : le 1^{er} décembre 2015

Mot de bienvenue du Bourgmestre de la commune de Ngaba en sa qualité du Président du groupe de pilotage de la ville de Kinshasa, hôte de la rencontre.

La réunion a commencé par une brève présentation des personnes présentes en déclinant chacun son nom et sa fonction dont liste de présence en annexe.

Après la présentation, le modérateur Monsieur Olivier Lutumba a introduit Monsieur Valery Madianga en charge de la communication de l'ODEP commis à la tâche à présenter avec brio, le bien-fondé de l'ODEP structure, puis vint le tour de Monsieur Jacques KATCHELEWA le chargé des programmes de l'ODEP commis à la charge de la présentation des résultats de l'étude, a expliqué à l'assistance et motivé, **les résultats des enquêtes et la démarche poursuivie dans les 22 communes concernées de la ville province de Kinshasa**.

Monsieur le chargé des programmes dans son allocution a commencé par remercier tous les bourgmestres présents et ceux représentés respectivement par leurs adjoints ainsi que les Chefs de Bureau, pour la réussite des différentes rencontres organisées le 19 novembre 2015 comme prélude à la rencontre du jour avec les différents services générateurs des recettes ou service d'assiettes dans les trois districts concernés par les plaintes notamment, les districts de la Tshangu (communes de Ndjili, Kimbanseke, Masina), Funa (Kalamu, Ngiri-Ngiri, Bumbu, Bandal et Selembao) et Mont Amba (Lemba, Ngaba, Matete) objet des tracasseries et des prélèvements informels.

L'approche de l'ODEP n'est pas celle des accusations mais de travailler en synergie avec les différents services et les contribuables afin d'arriver à mobiliser les recettes sans tracasseries ni entraves auprès des assujettis. Car trop d'impôt ruine l'impôt.

Madame la Bourgmestre de la commune de Masina a declaré qu'il n'y avait rien de neuf, même le Maire de la ville en connaissait quelque chose. Il y a quelques jours, elle avait remis à la justice pour les mêmes faits, un agent de la Direction Générale des Recettes de Kinshasa, trois jours plus tard, relaxé sur cotisations des siens de la prison centrale de Makala.

De son côté, son collègue de la commune de Bandalungwa a ajouté que leur souci était de s'attaquer à la cause non à la conséquence tenant compte de la situation socio-économique du pays.

Pour celui de Kimbanseke, la perception n'incombe pas aux agents commis aux services d'assiette, l'ODEP et les autres structures de la société civile avec lesquelles elles collaborent, dans le cadre de contrôle citoyen devra éduquer la masse à travers le théâtre, les saynètes en langues nationales à travers les médias, les méandres de la procédure fiscale.

Une préoccupation a été soulevée par l'assistance sur le phénomène taxi dans les communes de la ville province de Kinshasa, qui ruine et accentue les prélèvements illégaux et informels ?

La réponse à cette question a été pertinente il suffit de placer à l'entrée, un service d'orientation, vers le guichet unique, expérience réussie dans la commune de Kimbanseke.

Tous ont convenu de (d',du) :

- Vulgariser par affichage des textes légaux et réglementaires en vigueur à travers les valves portatifs devant chaque bureau communal,
- Eduquer la masse à travers les pièces de théâtre populaire la procédure de recouvrement d'impôt,
- Plaidoyer de l'ODEP auprès des décideurs pour aider les communes d'entrer dans leurs droits,
- Le recyclage du personnel de l'Etat commis à la phase administrative de perception de droits et taxes, dans le cadre du budget participatif,

- L'ODEP est d'avis d'appuyer les communes dans l'élaboration de budget participatif dans le but de mobiliser à hauteur de 80 % des recettes.
- Eveiller la conscience des agents percepteurs par l'éducation civique, réduirait à coup sûr, les velléités des agents et la naïveté et l'indolence des assujettis.

De l'avis de tous, ces échanges ont été fructueux et souhaitent que ce genre de rencontre puisse se multiplier dans l'avenir, afin de trouver des solutions concertées aux questions d'intérêt public, entre le pouvoir public et la société civile.

Le mot de clôture par le bourgmestre de Ngaba hôte de la rencontre.

Commencée à 10h45', la réunion a été levée à 12h 05'

Fait à Kinshasa, le 2 décembre 2015

Le rapporteur

Charles ISULA

Consultant Fiscal ODEP

B Additional Tables

	Households				Firms					
	Non N	egotiable	Neg	otiable	Total	Non 1	Negotiable	Neg	gotiable	Total
Electricity	51	(22%)	184	(78%)	235	238	(61%)	151	(39%)	389
Goods	78	(68%)	37	(32%)	115	5	(38%)	8	(62%)	13
Sanitation	52	(29%)	127	(71%)	179	95	(40%)	141	(60%)	236
Security	12	(50%)	12	(50%)	24	8	(47%)	9	(53%)	17
Transport	80	(61%)	52	(39%)	132	22	(55%)	18	(45%)	40
Water	420	(79%)	115	(21%)	535	144	(81%)	34	(19%)	178
Association	11	(69%)	5	(31%)	16					
Customary	3	(60%)	2	(40%)	5					
Documents	10	(40%)	15	(60%)	25					
Education	936	(90%)	105	(10%)	1,041					
Health	35	(71%)	14	(29%)	49					
HH Business	3	(100%)	0	(0%)	3					
Life events	147	(46%)	170	(54%)	317					
Salary	12	(63%)	7	(37%)	19					
Communications						6	(100%)	0	(0%)	6
Excise						10	(77%)	3	(23%)	13
Fuel						11	(73%)	4	(27%)	15
Insurance						0	(0%)	1	(100%)	1
Labour						15	(58%)	11	(42%)	26
License						256	(60%)	172	(40%)	428
Maintenance						0	(0%)	9	(100%)	9
Marketing						1	(7%)	13	(93%)	14
Media						2	(40%)	3	(60%)	5
Packaging						2	(40%)	3	(60%)	5
Profit						6	(29%)	15	(71%)	21
Sales						15	(60%)	10	(40%)	25
Start						0	(0%)	1	(100%)	1
Storage						2	(40%)	3	(60%)	5
Other Bus Payments						1	(50%)	1	(50%)	2
All Categories	2,064	(69%)	934	(31%)	2,998	839	(58%)	610	(42%)	1,449

Table AP1: Negotiability of payments by category in USD

Table AP2: Household rep	ports knowing the official	payment amount (by	category in USD)
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	ŀ	Know	Don'	Total	
Association	8	(100%)	0	(0%)	8
Customary	1	(33%)	2	(67%)	3
Documents	12	(67%)	6	(33%)	18
Education	462	(68%)	217	(32%)	679
Electricity	2	(29%)	5	(71%)	7
Health	15	(79%)	4	(21%)	19
HH Business	1	(50%)	1	(50%)	2
Goods	56	(69%)	25	(31%)	81
Life events	79	(63%)	46	(37%)	125
Security	10	(71%)	4	(29%)	14
Salary	13	(87%)	2	(13%)	15
Sanitation	58	(50%)	58	(50%)	116
Transport	39	(71%)	16	(29%)	55
Vehicles	16	(67%)	8	(33%)	24
Water	190	(62%)	117	(38%)	307
All Categories	999	(64%)	566	(36%)	1,565

Notes: This table shows breakdown of payments made by households in our baseline data on whether they report knowing the official payment amount or not. This question was only asked for formal payments and we exclude cases where the households report the official payment rate is zero.

	Any Payment (1)	Any Formal (2)	Only Formal (3)	ATE (4)	Conditional on Post (5)
Protection	$0.016 \\ (0.015)$	$0.012 \\ (0.014)$	$0.010 \\ (0.012)$	-3.278 (2.587)	-83.358^{***} (22.656)
Information	$0.008 \\ (0.011)$	$0.009 \\ (0.010)$	0.014 (0.009)	$1.394 \\ (1.531)$	16.738 (27.474)
Registered (Protection)	0.055^{**} (0.021)	0.054^{**} (0.021)	0.021 (0.013)	2.867 (2.527)	11.094 (15.687)
Registered (Information)	$0.008 \\ (0.020)$	$0.006 \\ (0.019)$	-0.001 (0.017)	-0.798 (1.316)	-7.107 (13.116)
Protection \times Registered	-0.027 (0.030)	-0.026 (0.030)	$\begin{array}{c} 0.012\\ (0.028) \end{array}$	-1.789 (2.693)	4.902 (19.634)
Information \times Registered	-0.073^{*} (0.032)	-0.048 (0.028)	-0.022 (0.031)	-6.681 (6.227)	70.391 (51.318)
R ² (Protection) R ² (Information) Observations Control Mean	$\begin{array}{c} 0.094 \\ 0.094 \\ 27,266 \\ 0.07 \end{array}$	$\begin{array}{c} 0.082 \\ 0.083 \\ 27,266 \\ 0.06 \end{array}$	$\begin{array}{c} 0.071 \\ 0.076 \\ 27,266 \\ 0.03 \end{array}$	$\begin{array}{c} 0.017 \\ 0.014 \\ 27,266 \\ 2.98 \end{array}$	$\begin{array}{c} 0.338 \\ 0.205 \\ 1,107 \\ 42.62 \end{array}$

Table AP3: Heterogeneous Treatment Effects by Registered

***p < 0.01; **p < 0.05; *p < 0.1

C Additional Figures

Figure AP1: Reasons for Payment in Smartphone Data

Panel A: Household



Notes: This figure plots the distribution of choices selected by households for the reasons why they made a payment. The data come from the smartphone data, our main outcome data.





20 30 40

Panel B: Firms



Notes: This figure plots the number of payments households and firms made given that they made at least one payment, per category.