

# 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 public goods 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, 2023](#)), 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 in order 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](#)).<sup>1</sup> Such demands abound due to inadequate wages, weak performance incentives, and

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<sup>1</sup>We define informal payments as any tax or fee payments to state agents that are on top of 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 even perceived as legitimate ([Prud’Homme, 1992](#); [van den Boogaard, Prichard and Jibao, 2021](#)). Importantly, bribes

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.<sup>2</sup> 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, few citizens make any 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, households and businesses that make payments also tend to enjoy more benefits, indicating that public goods access comes with exposure to both formal and informal payments. The baseline survey also confirms that households

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paid *in lieu* of formal payments can actually reduce the costs to citizens of accessing services (Shleifer and Vishny, 1993); our model below examines how empowerment affects both these payments and informal payments made *on top* of formal payments.

<sup>2</sup>For ease of exposition, we refer to households and businesses jointly as ‘citizens’ throughout the paper.

and businesses suffer from both information and power deficits in their frequent negotiations with state agents over 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).<sup>3</sup> 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). Even more notably, empowerment can make some citizens more willing to engage the state or to switch from making only informal payments to insisting on 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 and 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

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<sup>3</sup>The model presented here is a revised version of the model presented in our original pre-analysis 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](https://osf.io/s3gy6/?view_only=1fa9a27f07394045aac2236d1ecf692a).

post-treatment data on a weekly basis for up to 19 weeks by having all 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. These results were largely driven by households rather than businesses. 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 well-known competing view contends that empowerment might actually 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-a-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 even very individual, non-collective negotiations of predatory demands. While our interventions empower citizens at the micro-level, our findings on protection also speak to the importance of a having a strong civil society 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 state-centered 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. \(2021\)](#) investigate empowerment as an outcome, finding that a ‘bottom-up’ intervention conducted with market vendors in Malawi and focused on improving communication and service provision not only increased taxpayer compliance but empowered vendors to better advocate for themselves. Overall, 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, 2023; 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. For most of its post-independence history the DRC has suffered from fiscal mismanagement, corruption, and poor public service provision (Van Reybrouk, 2014). Under the regime of Mobutu Sese Seko, public services were systematically underfunded and many public servants stopped receiving adequate salaries, a situation exacerbated by political instability and conflict since the mid-1990s (Van Damme, 2012).

In ostensible recognition of the need to improve revenue collection and bring fiscal governance closer to the people, the central government 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). Both state agents' salaries and state-provided services are often funded through

informal payments imposed on citizens (Englebert and Kasongo, 2014; Weijs, Hilhorst and Ferf, 2012; Titeca and Kimanuka, 2012). Citizens also face high costs to accessing many basic public services owing to inadequate central government funding and the corresponding reliance on local service providers and informal user fees. While many citizens consequently opt out of accessing benefits that are viewed as less essential, these informal payments are often required payments for those wanting to access key services.

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.<sup>4</sup> The survey measured self-reported payments and amounts across 18 categories for households and 22 for businesses, where formal (informal) payments were defined as legally (not legally) mandated (see Appendix A1 for details). Our analysis of the baseline data reveals several patterns that inform our theoretical model below.<sup>5</sup>

First, the baseline survey confirmed that very few citizens in Kinshasa engage with the state to pay taxes and fees. The vast majority of Congolese households and businesses make no payments to state officials across all payment categories (see Appendix B.1). The median business only makes payments in one out of 22 tax categories: electricity. Even at the 90th income percentile only four tax categories see any payments. Similarly, the median household only makes payments in two out of 18 categories—education and water—with no payments in 11 categories even in the 90th income percentile.

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<sup>4</sup>Households and businesses were randomly sampled using a multi-stage cluster sampling strategy where street segments (“avenues”) served as the primary cluster unit. The surveys were implemented from August-September 2015.

<sup>5</sup>Given uncertainty around statutory payments and amounts in the DRC, citizens may or may not be aware that their informal payments are in fact informal. In light of this, and 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.

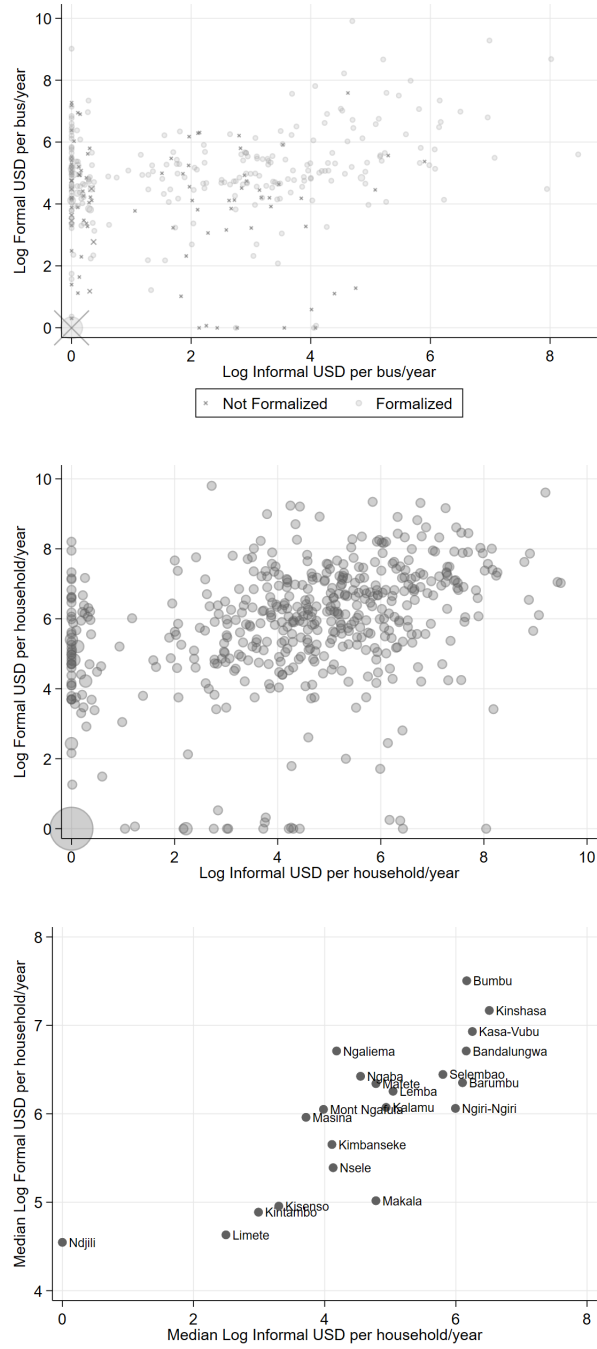


Second, citizens who do make payments tend to make formal *and* informal payments. Previous studies suggest that formal and informal payments might be substitutes rather than complements in some settings (Shleifer and Vishny, 1993). Yet, as can be seen in Figure 1, there is a positive correlation between formal and informal payments for both households and businesses. The bulk of these payments are attributable to usage fees for services like water, electricity, sanitation. Survey respondents indicate that 64 percent of their payments involved both formal and informal amounts; 14 percent involved informal payments only; and only 22 percent were entirely formal (see Appendix B.2).

Third, those households and businesses that do pay also tend to enjoy greater benefits, consistent with a story in which accessing benefits entails paying formal and informal costs. The third panel of Figure 1 shows a strong relationship between median informal and formal payment by neighborhood for households. Households in neighborhoods that pay more enjoy better access to education and electricity (see Figure A1 in Appendix B.3). 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 Figure A2 in Appendix B.3). 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 Table A5 in Appendix B.3). 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. For instance, households report that 10 percent of their education payments but 78 percent of their electricity payments are negotiable (see Table

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; middle panel for households; and bottom panel for household neighborhoods.

[A6](#) in Appendix [B.4](#)). For businesses, 39 percent of electricity payments and 40 percent of licensing payments were negotiable (see Table [A7](#) in Appendix [B.4](#)).

Second, our baseline data (along with qualitative reports) reinforces the claim that citizens in Kinshasa face both information and power asymmetries in their interactions with street-level state agents. Information asymmetries exist because state agents often have a better understanding of citizens' true payment liabilities than citizens themselves. While some statutory payment amounts are more transparent than others, many are obscured by their complexity or the fact that they rely on consumption readings by state officials (e.g., for electricity or water). As can be seen in Appendix Table [A8](#), large proportions of citizens state that they do not know their statutory payment obligations. For instance, respondents reported not knowing the statutory payment amounts for 32 percent of their education payments and 50 percent of their sanitation payments. Similarly, power asymmetries 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 from citizens ([Sánchez de la Sierra et al., 2022](#)). 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 [A9](#)).

This baseline data is echoed by extensive 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. Citizens often simply pay a negotiated amount. Others might 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 arrangements, such as using gas-fueled generators ([Banza et al., 2022](#)). 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 still lack the bargaining power to negotiate favorable arrangements. The fact that both information and power asymmetries are prevalent but vary across payment types *and* citizen types (citizens vary 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 striking findings above 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 pessimistic about what they can gain from bargaining if they think they owe a lot or if they 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 become visible to those agents in the first place. We believe the model is relevant to explaining a wide range of tax and fee payments, conditional on those payments being to some extent voluntary. We return in the conclusion to discussing

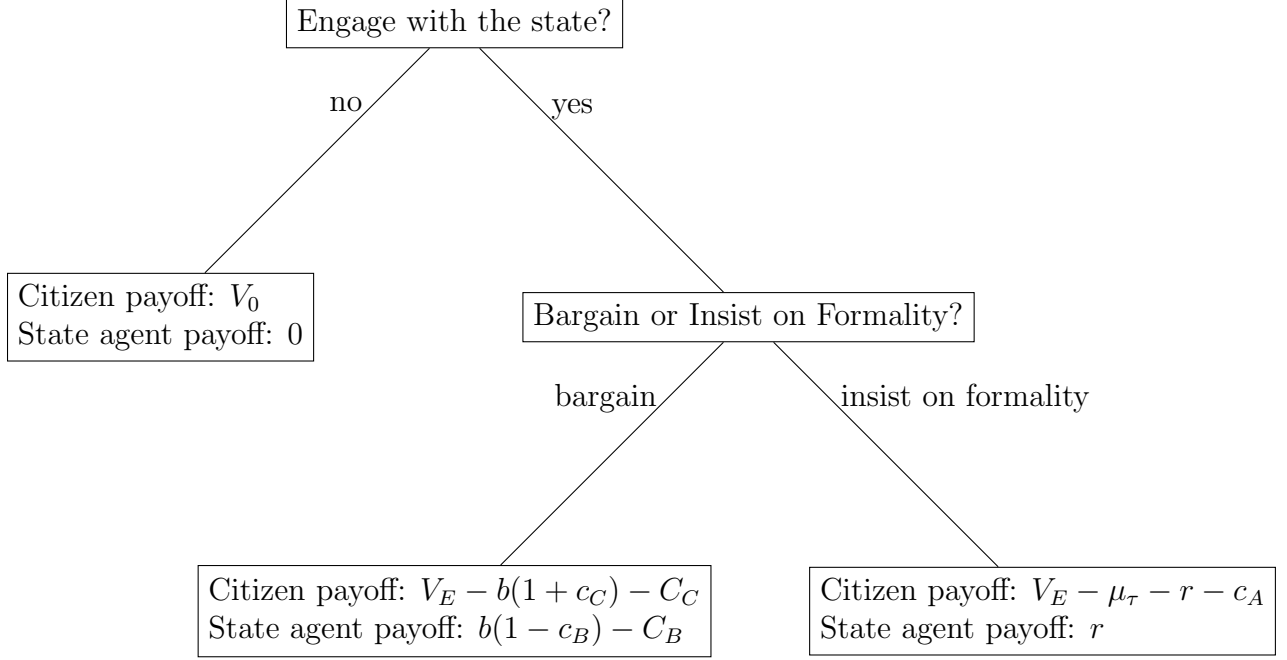
the model's scope conditions (along with possible extensions).

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. The benefit could be direct and immediate, as with an electricity payment, or it could be more distant, as with obtaining greater legal protections for property after paying property taxes. Following on the discussion in Section 2, engagement implies exposure to payment demands. If the citizen engages, they have a true payment liability  $\tau^*$ , which the state agent knows but the citizen does not. Instead, the citizen has a prior belief about her payment liability,  $\mu_\tau$ .

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 still 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 (Shleifer and Vishny, 1993). Alternatively, the state agent and citizen may prefer to collude in private. When transacting privately, the state agent and citizen have the potential to forego 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.

The citizen will bargain with the state agent if the expected bribe and cost of collusion are lower than the official payment, cost of insisting on 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).

Figure 2: Decision Tree



There are two ways in which we expect empowerment to work. First, officials know the true payment liability,  $\tau^*$ , while citizens only have a guess,  $\mu_\tau$ . We construe additional information as intervening on  $\mu_\tau$ . We view protection as acting on  $r$ , the rent that officials are 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.<sup>6</sup>

We derive intensive and extensive margin predictions for citizens. By intensive margin predictions we refer to the *amounts* paid by citizens who begin in either the collusion equilibrium (where they are paying only informal bribes) or the official equilibrium (where they are making some formal payment to the state) and are not induced to switch by changes in  $\mu_\tau$  or  $r$ . The extensive margin predictions capture the effects of empowerment when citizens are induced to switch equilibria by the parameter changes. We note that we can derive predictions for the effects of decreases in  $\mu_\tau$  and  $r$  on official payments  $\tau$ , bribes  $b$ , and rents  $r$ , as summarized in Figure 3 and Table A10 in the Appendix. However, we state

<sup>6</sup>In Appendix C we discuss why we believe protection operates on  $r$  and not a cost parameter.

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.

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. By a similar logic, reducing  $\mu_\tau$  also reduces the bribe in equilibrium. When citizens are already making formal payments, lowering  $r$  directly reduces the citizen's payment. However, changing  $\mu_\tau$  has no effect on the payment amount on the assumption that once a citizen makes an official payment they learn their true statutory payment obligation. Thus, the intensive margin predictions are:

- **Intensive Margin Effect 1:** Lowering  $r$  (through protection) or lowering  $\mu_\tau$  (through information) should decrease the total payment amount among those who start and stay in the collusion equilibrium (by decreasing bribe payments).
- **Intensive Margin Effect 2:** Lowering  $r$  (through protection) should decrease the payment for citizens who start and stay in the official payment equilibrium (by decreasing rent payments). Lowering  $\mu_\tau$  (through information) should have no effect on the amount paid by these citizens.

Our first *extensive* margin effect is for citizens who, prior to the intervention, have not yet engaged with the state over a particular payment. For some of these citizens, changes to  $r$  and  $\mu_\tau$  will induce engagement with the state by decreasing the costs of both bargaining and insisting on official payments. In a similar vein, for citizens who start in the collusion equilibrium, changes to  $r$  and  $\mu_\tau$  will induce some to switch to the official equilibrium.<sup>7</sup> To summarize, our main extensive margin predictions are:

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<sup>7</sup>We note that the model has explanatory power even in contexts where there is no scope for collusive bargaining; in such cases empowerment can push citizens from not engaging with

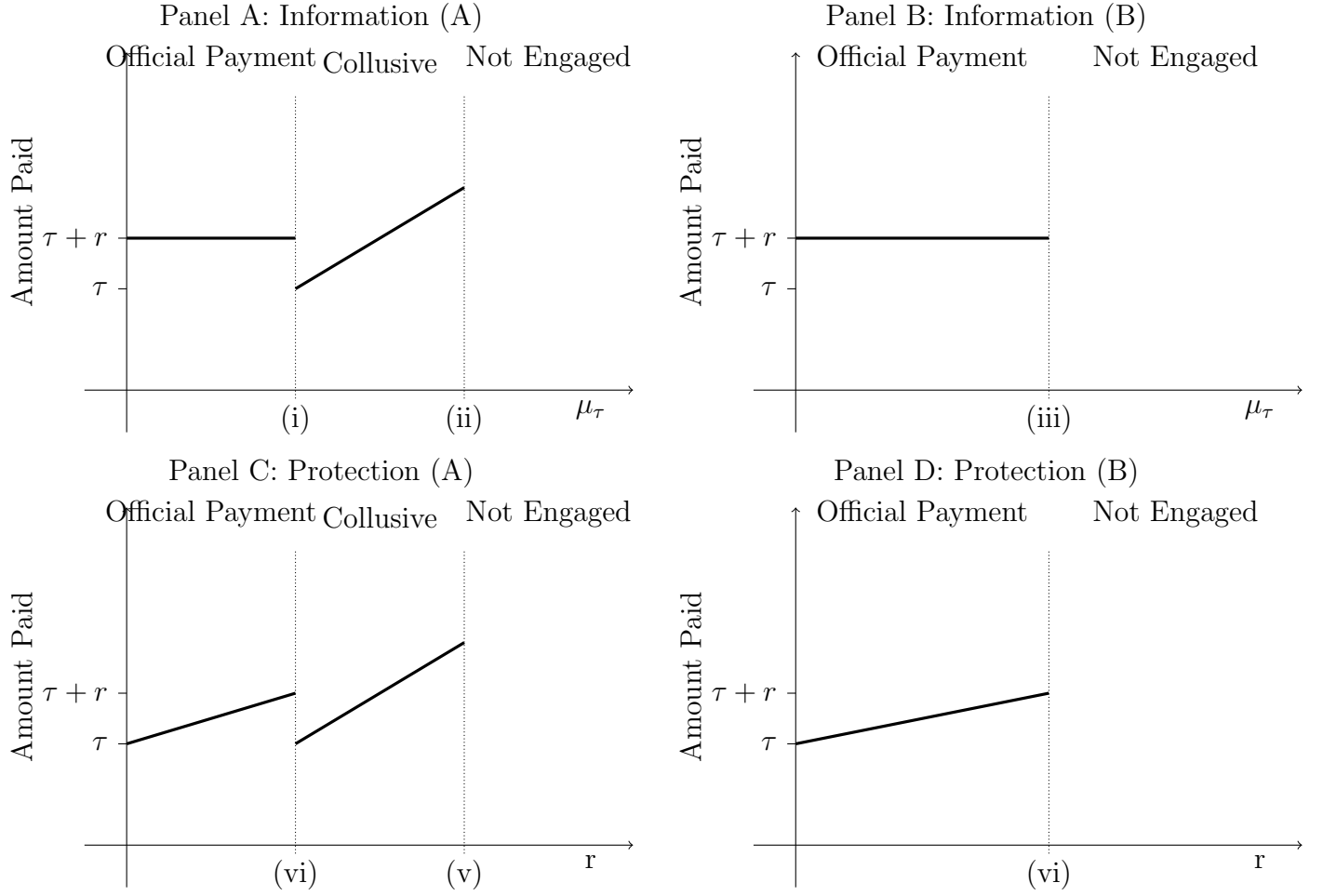
- **Extensive Margin Effect 1:** Lowering  $r$  (through protection) or lowering  $\mu_\tau$  (through information) will induce some citizens to engage with the state and thus increase the share of citizens making any payments.
- **Extensive Margin Effect 2:** Lowering  $r$  (through protection) or lowering  $\mu_\tau$  (through information) will increase the share of citizens making official payments.

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the state to making formal payments in the official payment equilibrium, as shown in Panel B and D of Figure 3.



Figure 3: Effects of intervening on  $r$  and  $\mu_\tau$



*Notes:* These figures show the effect of changes to  $\mu_\tau$  (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, this is, when there exists a range of  $r$  or  $\mu_\tau$  for which bargaining is preferred over official payments or not 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).

## 4 The Research Design

### 4.1 The 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, however, in that citizens were informed that any suspicious payments would be investigated by ODEP and that the identity of state agents implicated in predatory taxation would be publicized in an anti-corruption advocacy campaign. This was likely seen as a credible threat by citizens (and state agents) because ODEP regularly conducts such campaigns and is perceived as being expert on this

subject.<sup>8</sup> By 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.

## 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 533 households and 534 businesses that participated in the baseline survey described in Section 2.<sup>9</sup> Eligible respondents were asked if they would be willing to participate in an additional data collection activity, which would require attending a training and providing data on tax and fee 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 tax and fee 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.<sup>10</sup> Participants received phone credits to facilitate reporting and were allowed to keep the smartphones at the end of the study as additional incentive.

Random assignment to treatment was done in two steps (see Figure 4 and Appendix D.2.) First, 48 avenues were assigned to treatment and 48 to control, blocking on commune. For

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<sup>8</sup>ODEP also regularly holds a seat at parliament and in government meetings. The advocacy campaign was held as promised in late 2016, six weeks after the end of the intervention.

<sup>9</sup>Respondents 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.

<sup>10</sup>The exact length of reporting time varied for respondents depending on the time point at which they were recruited into the study and trained.

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).<sup>11</sup> We took this approach to randomization to minimize spillovers in control avenues; we were less concerned about spillovers in treatment avenues 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 tax consulting services (according to treatment assignment) and asked the participant if they would be interested in obtaining those services for free for 18 weeks (see Appendix D.1 for the recruitment script).<sup>12</sup> 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.3).

### 4.3 Data and Measurement

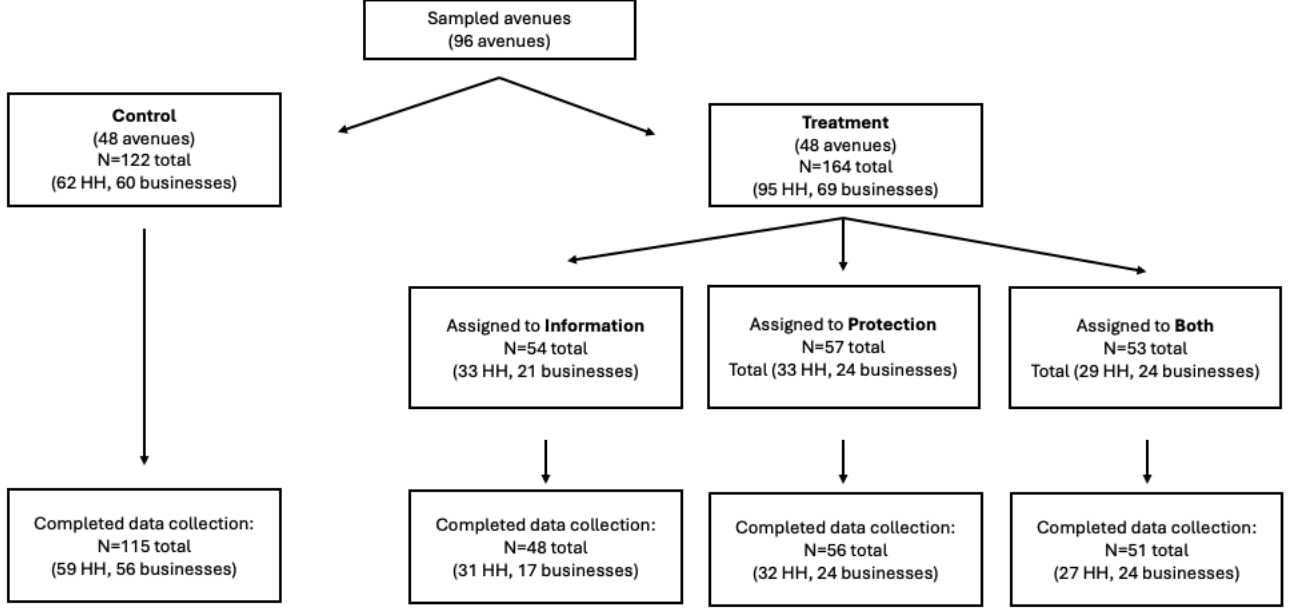
To test our hypotheses, we need data on whether participants are making formal or informal payments and how much. A fundamental measurement challenge arises in that informal payments are often unknown, and illicit payments often hidden, making it difficult to obtain such data from administrative sources.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

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<sup>11</sup>Below we focus on estimating the main effects of the information and s since the model does not predict interaction, although we explore interaction effects in Appendix Table A18.

<sup>12</sup>We 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.

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 associated payments are summarized in Appendix A1.

We use the smartphone data to create three main dependent variables. 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). We do not analyze formal and informal payment *amounts* separately. While the model generates predictions on  $\tau$ , 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. For this reason we focus on total payments and the three binary indicators.

The first binary indicator, *Any Payment*, is 0 when the respondent reports no payment in a given tax 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. To estimate the second extensive margin effect we conduct an exploratory analysis using two binary variables. First, *Any Formal Payment*, is 1 when the respondent reports any formal payments in a given tax category in a week and 0 otherwise. That is, if a respondent reports some formal payments *and* some informal payments in a tax category in a given week, then the variable *Any Formal Payment* is 1 for this week and category. Second, *Only Formal Payment*, is 1 when the respondent reports only formal payments in a given tax category in a week and 0 otherwise. That is, if a respondent reports some formal payments *and* some informal payments in a tax category in a given week, then the variable *Only Formal Payment* is 0 for this week and category. While outliers are a potential concern, all analysis below is robust to different approaches to dealing with outliers (see Appendix Tables [A22–A23](#)).

## 5 Extensive Margin Results

### 5.1 Estimating Extensive Margin Effects

Based on our theoretical framework outlined 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:

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

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-a-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 regression analogously specified to estimate the effect of the information treatment.<sup>13</sup>

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),  $\phi_c^a$  is a vector of commune fixed effects, and  $\theta_j^a$  are payment category fixed effects. To account for time trends in payments we include a vector of reporting week fixed effects,  $\eta_t^a$ . Standard errors are clustered by avenue since the first-level treatment was assigned at by avenue, and we use weights to account for assignment probabilities. By randomization,  $\beta_1^a$  and  $\beta_2^a$  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.

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<sup>13</sup>Our 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 2 percentage point increase in tax payment rate in a given tax category per week ( $p < .05$ ). In the control group, the weekly rate at which households and firms made payments was 6 percent, with the protection treatment effect representing a 1/3 increase. 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 2.1 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.2), the similar coefficients indicate that the protection treatment induced people to engage primarily in formal payments.<sup>14</sup> 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 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

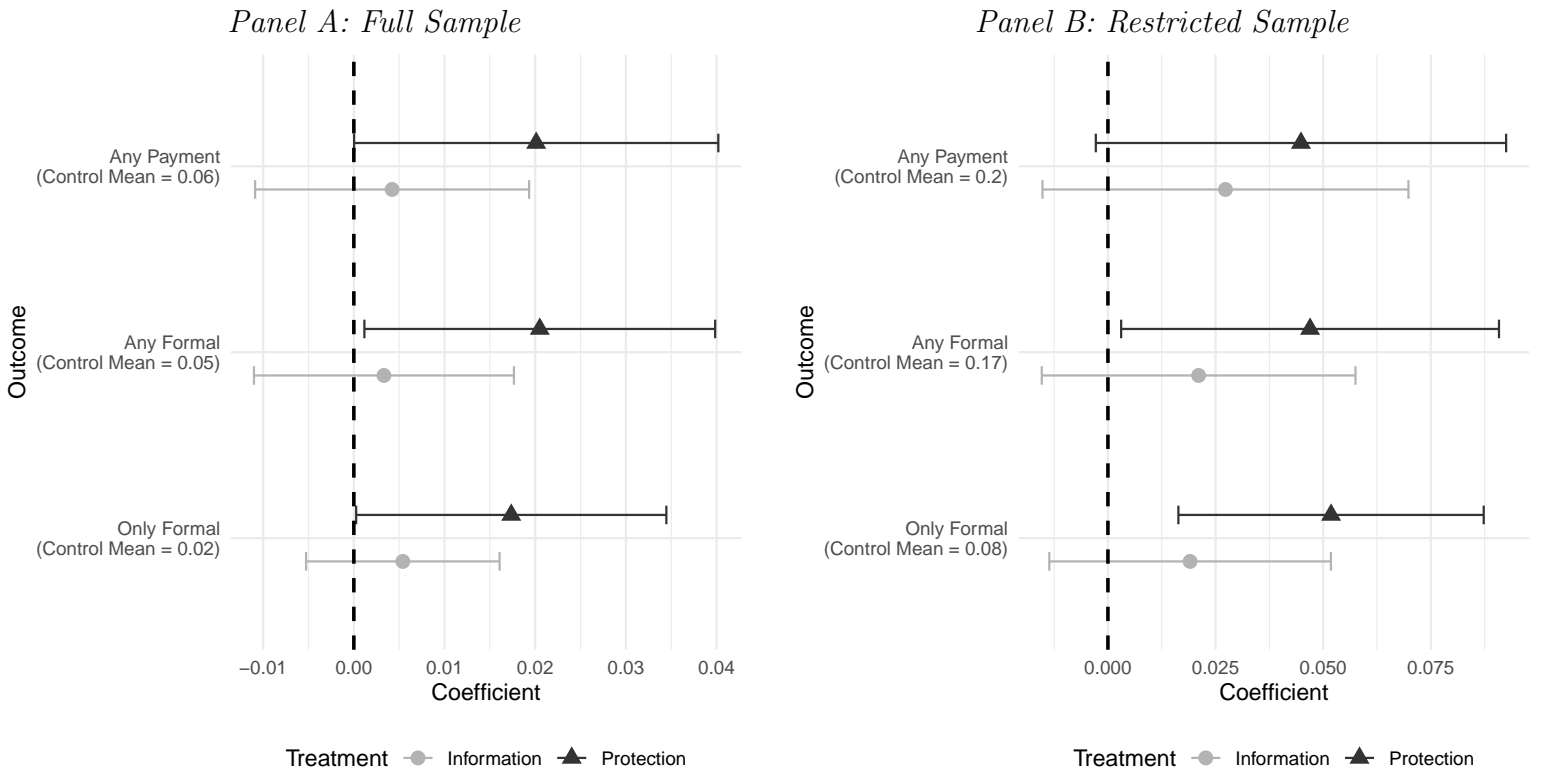
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<sup>14</sup>In theory it is possible that protection treatment induced citizens who previously did not engage with the state to start making collusive payments while an equal proportion of citizens previously making collusive payments were induced to make official payments. But the rarity of pure collusion in our data makes this scenario unlikely.



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, but still not statistically significant.

Figure 5: Extensive Margin Effects

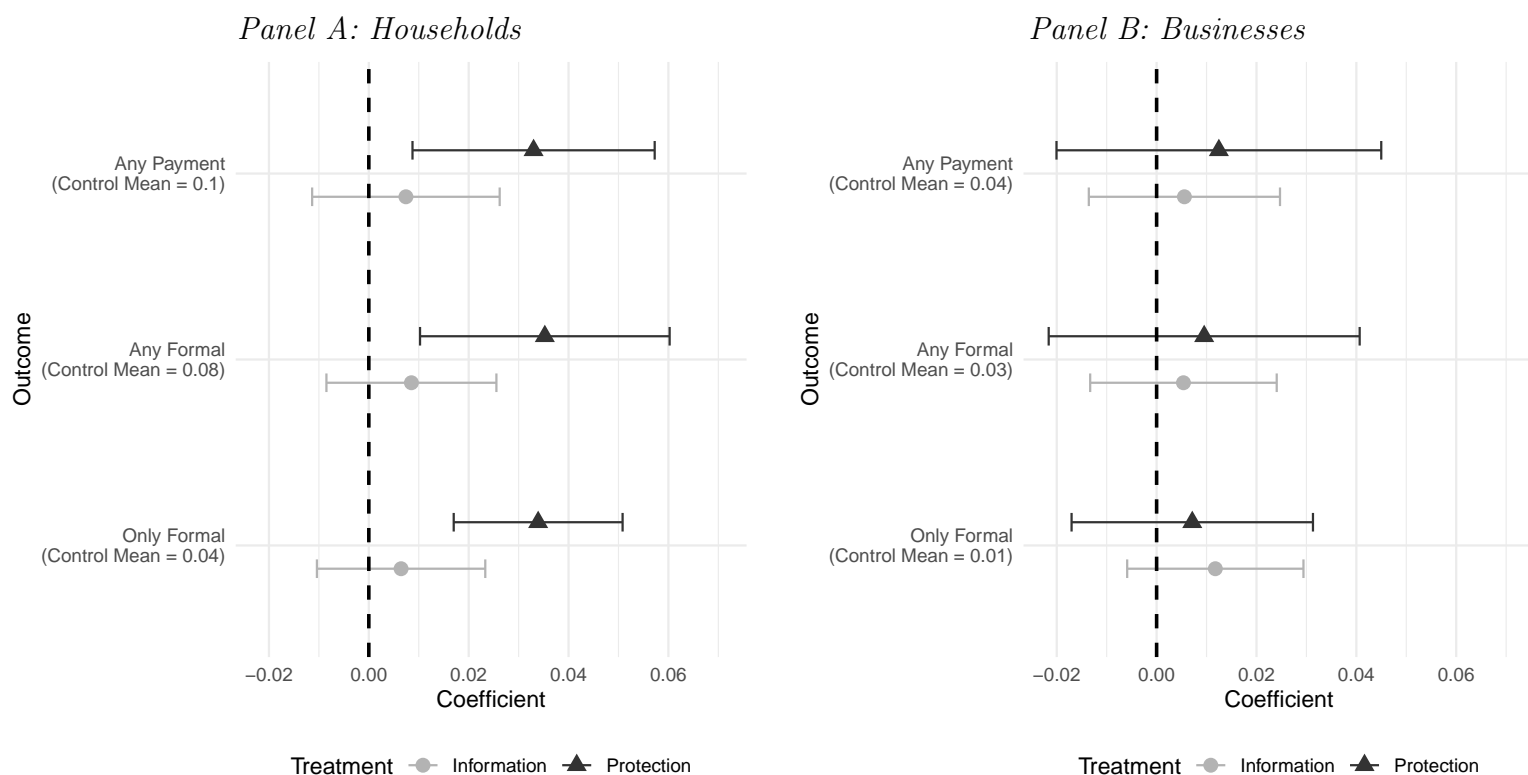


*Notes:* This figure shows the coefficients for the protection and information treatments on the extensive margins. Panel A shows all tax category while Panel B restricts the sample to a set of (pre-specified) payment categories that are most common and where we expect high levels of predation (these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

Additional exploratory analysis sheds further light on the extensive margin results. Fig-

ure 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 3–5 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).

Figure 6: Extensive Margin Effects For Households and Businesses

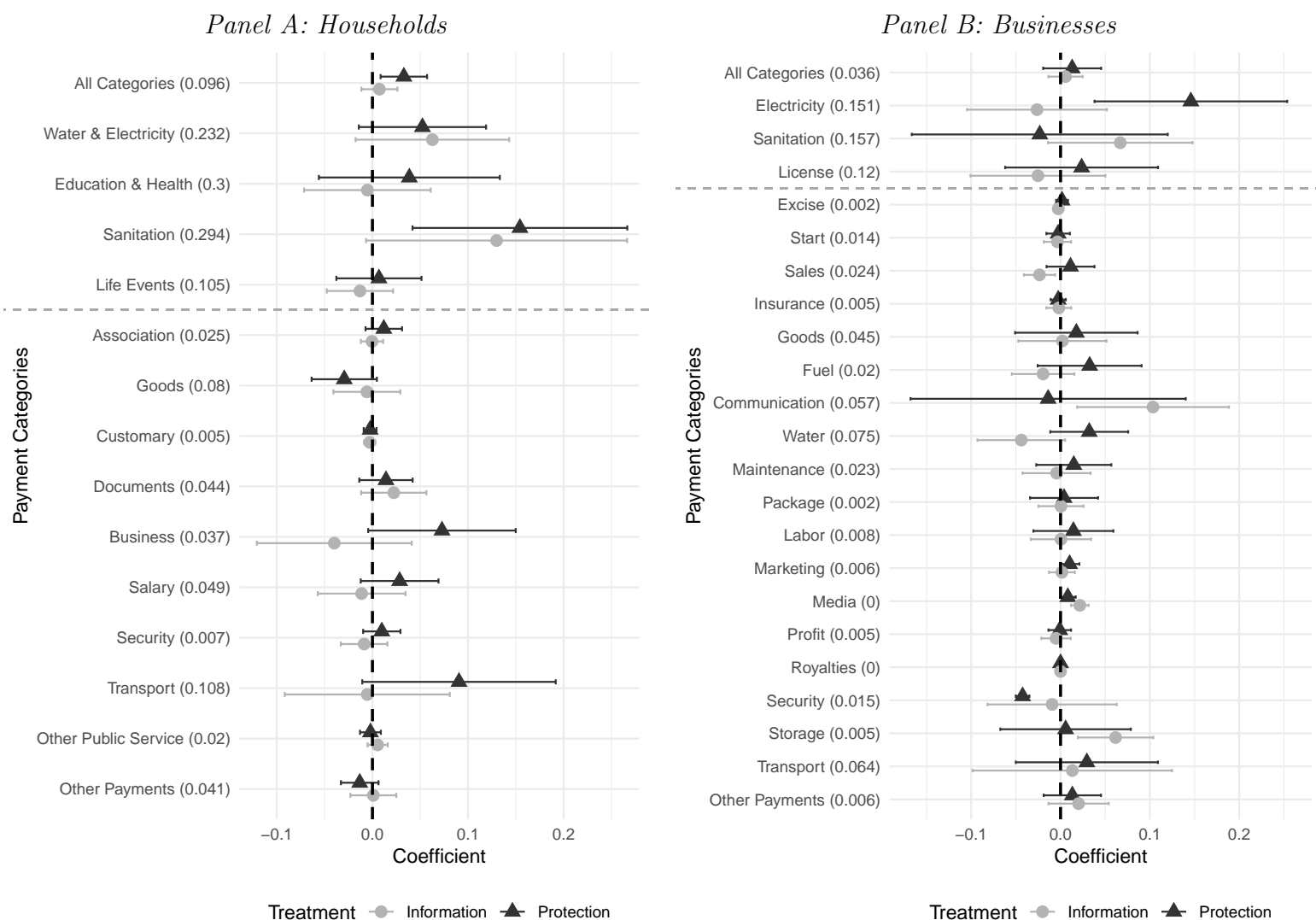


*Notes:* This figure shows the coefficients for the protection and information treatments on the extensive margin for all payment categories for households (Panel A) and businesses (Panel B).

We also examine the effects of the interventions across different payment categories (see Appendix A for descriptions). Figure 7 plots the coefficients for the extensive margin in 15 payment categories for households and 21 for businesses. For households we observe large effects of the protection treatment in payments for water and electricity, education and health, sanitation, business affairs, religious affairs, and transport. We see similar for information, except for education and health, business, and transport. For businesses we

also observe large effects in multiple categories for protection, including electricity, fuel, and water, and for information, in sanitation, communication, and storage. The analysis reveals that empowerment can induce both households and businesses to engage across a variety of service and payment categories.

Figure 7: Extensive Margin 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 businesses (Panel B). Categories above the grey dashed horizontal line are those included in the restricted sample. The control mean for each category is in parentheses.

The extensive margin effect estimates are robust to different covariate specifications (Ap-

pendix Tables [A16](#) and [A17](#)). We also check the extensive margin effect estimates when including an indicator for whether *both* treatments were received (Appendix Table [A18](#)), 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 Figure [A4](#) shows that the results are not driven by any one payment category.

As a result of the protection treatment abusive behavior by state agents was noted by ODEP. Halfway through the intervention period ODEP followed up on these and implemented an advocacy campaign. In randomly selected neighborhoods ODEP complained to high level officials about abusive behavior. In Table ?? we test the effect of this campaign. It shows that while the extensive margin effect is present across the whole period, it is amplified—to more than double the size—after the campaign in selected neighborhoods.

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. Appendix Table [A20](#) 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.

## 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 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). The difference in mean payment levels across treatment and control mixes the extensive margin effect (those going from zero payment to some positive payment) with the intensive margin effect (changes in payment levels among those who would always pay). 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). 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. 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.<sup>15</sup>

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

$$Y_{i,t,j} = \beta_1^b \text{Protection}_i + \beta_2^b \text{Information}_i + \gamma^b X'_i + H_i^b + \phi_c^b + \eta_t^b + \theta_j^b + \varepsilon_{i,t,j}^b, \quad (2)$$

Where  $Y_{i,t,j}$  is the amount of the relevant payment made during the post-treatment smart-phone 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 8 displays estimates of average effects on payment amounts. At the top are the

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<sup>15</sup>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 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).

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 8 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 USD 47.62. We estimate that those paying non-zero amounts in the protection treatment are paying about half as much (USD -22.45 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. The point estimates for the bounds are always negative. For the protection treatment, the upper bound is USD -14.49 (not statistically significant) and the lower bound is USD -50.27 ( $p < .01$ ), which implies implausibly that payment amounts are driven to zero, although the data cannot rule this out. For the information treatment, the upper bound is USD -6.18 (not statistically significant) and the lower bound is USD -27.39 ( $p < .01$ ).

Figure 9 shows how the ATE varies across tax categories. Given the results thus far, positive effects are likely due to extensive margin effects dominating and negative effects are likely due to intensive margin effects dominating. For households, extensive margin effects tend to dominate in water and electricity and household business activity, while for businesses, they dominate for license, labor, storage, and transport payments. The intensive margin effects tend to dominate for households in life events, goods-related fees and for sanitation, insurance, and security fees for businesses.

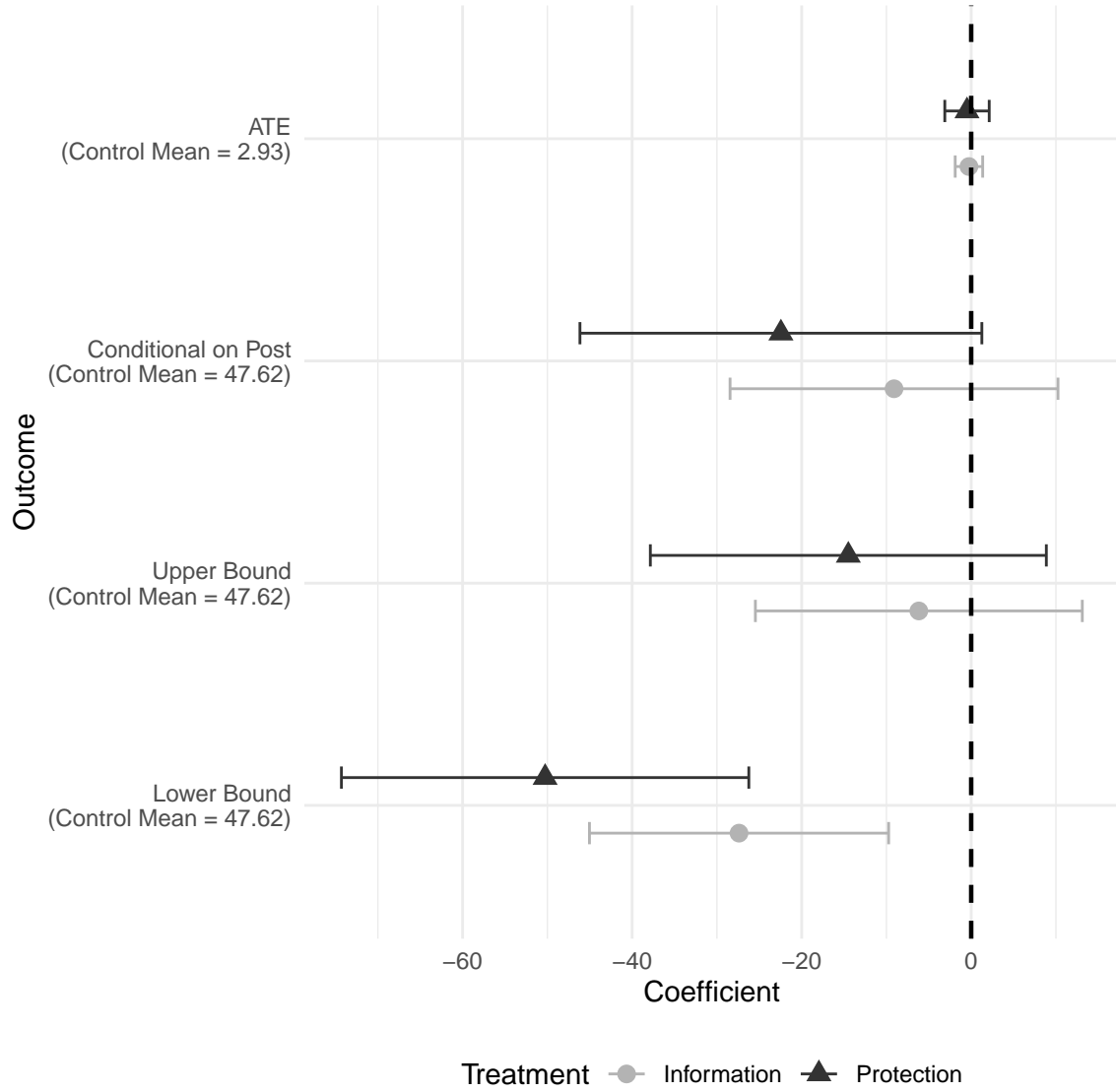
The appendix displays additional results and robustness checks. Appendix Table A15 shows estimates for the higher-volume restricted sample. The estimated effects are quali-

tatively 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 [A23](#)). The estimated effects decline as we top-code the upper percentiles of the payment distribution. This suggests that intensive margin effects are driven by lower 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.

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 [A20](#) 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.

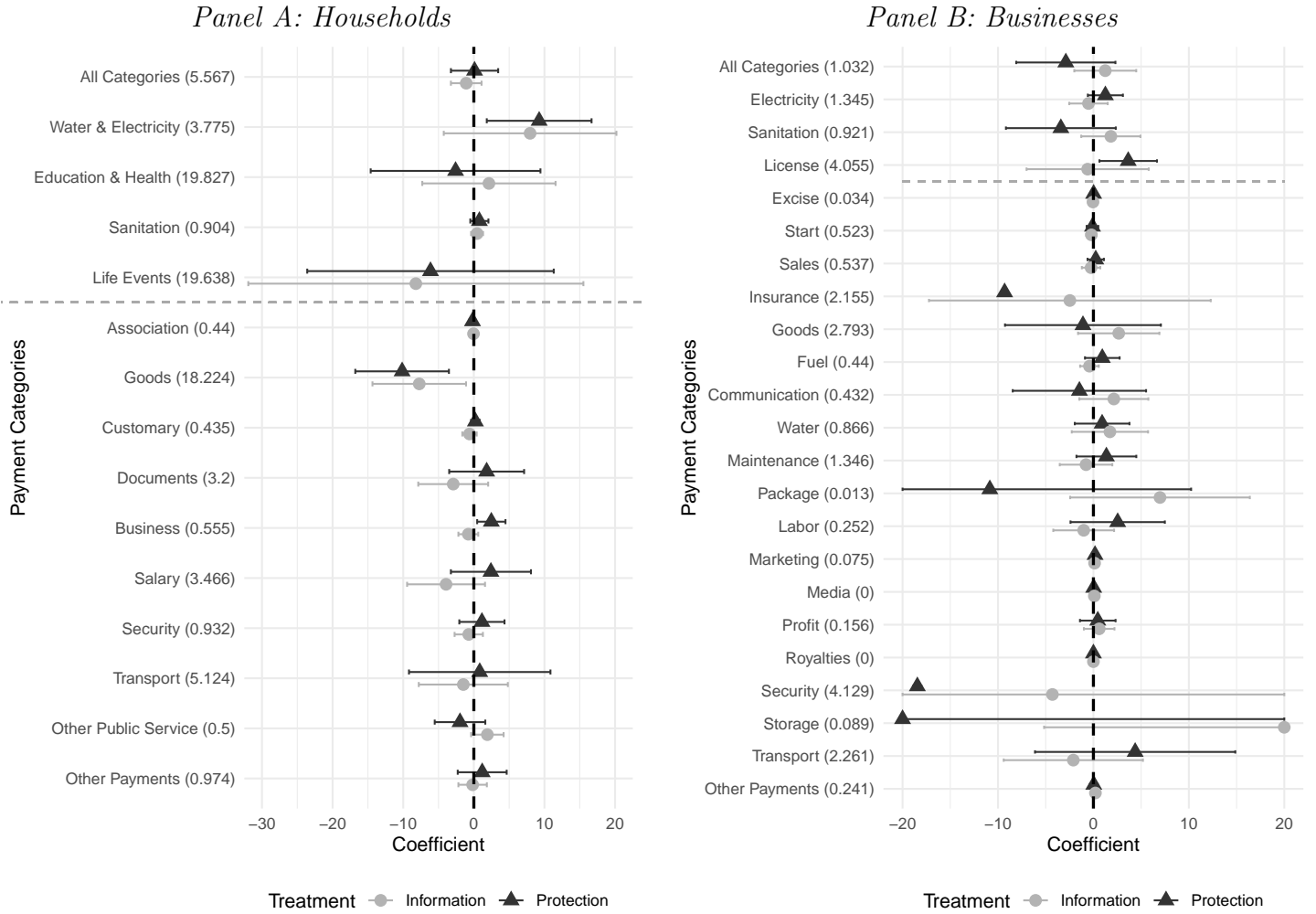


Figure 8: ATE and Intensive Margin Effect of Protection and Information



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

Figure 9: ATE by Payment Category



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

## 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 become legible to 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.<sup>16</sup> Yet, whether the state benefits from citizen empowerment could depend on factors such as whether higher-level state agents are colluding with street-level state agents in demanding informal payments from citizens or higher levels of government can be persuaded to exercise more control over lower-level agents, thereby reducing the scope for opportunistic demands and inducing more citizens to engage with the state and make formal payments.

For street-level state agents, the welfare effects of empowerment are less clear. In our

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<sup>16</sup>While it is still possible that some of that additional formal revenue gets lost to leakage, which we do not study in this framework, we think it is reasonable to assume that at least some of it makes its way to the state coffers.

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 provide more revenue to the state, then citizen empowerment could also be welfare enhancing for street-level state agents in the longer run *if* the state used those funds for better compensation for its agents. This underscores the importance of future research to incorporate the calculations of higher-level state authorities in their strategic responses to an empowered citizenry.

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# Appendix

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# A Payment Categories and Descriptions

Table A1: Payment Categories and Examples

Category	HH/Firms	Question Wording: “Did your household/business pay or was asked to pay a tax or informal payment [...]”	Examples Payments
<b>Electricity</b>	Firms & HH	[...] linked to electricity?	informal payments for repairing faults; informal payments to avoid cuts; tax on generators
<b>Goods</b>	Firms & HH	[...] linked to the rent on fixed assets and property owned?	tax on rental income, property tax, land registration
<b>Sanitation</b>	Firms & HH	[...] linked to hygiene, sanitation, and health regulations?	environmental and hygiene tax; pollution tax; health tax, TRA; Sanitation tax, removal of rubbish and household waste;
Security	Firms & HH	[...] linked to security or legal service?	protection taxes, conflict resolution, bribes to avoid imprisonment Hygiene and sanitation service/disinfestation/health control
Transport	Firms & HH	[...] linked to transport?	car/motorcycle registration fee; roadblock payment, port fee
<b>Water</b>	Firms & HH	[...] linked to water?	informal payments for repairing faults; informal payments to avoid cuts;
Association	HH	[...] linked to community or social development projects or to any types of associations/cooperatives/business groups?	labor contribution or cash payments related to activities community; 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
<b>Documents</b>	HH	[...] linked to obtaining or replacing an official government document?	passport, driving license; voter card, identity card; fees for attestation/certificate of loss of part; legalization signature or any other civil status act
Education & Health	HH	[...] linked to education or health services?	SERNE fees; registration fees; monitoring/motivation costs for teachers; patient records; informal fees for doctors/nurses
HH Business	HH	[...] linked to one or more small businesses or production units owned by the household?	
<b>Life Events</b>	HH	[...] linked to supporting events in social life?	funerals, weddings, births
Salary	HH	[...] linked to their income or generating activity?	income tax
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
Excise	Firms	[...] linked to the purchase of raw materials/inputs for the trade?	custom tax; sales taxes
Fuel	Firms	[...] linked to fuel/lubricant?	
Insurance	Firms	[...] linked to insurance/protection and fire protection?	Fees for protection and fire prevention
Labor	Firms	[...] linked to labor?	Tax on wages and labor; Professional tax on remuneration; Exceptional tax on remuneration of expatriate staff; Self-employment income tax
<b>License</b>	Firms	[...] linked to operating licenses?	registration license, operating permit, IPMEA, single establishment tax, etc.
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
Marketing	Firms	[...] linked to marketing/advertising?	display permission fee; taxes on the decoration of public buildings; tax on the production of advertising work
Media	Firms	[...] linked to press expenses?	newspaper, magazine, paper, printing expenses, stationery
Profit	Firms	[...] linked to business profits?	
Royalties	Firms	[...] linked to royalties?	
Package	Firms	[...] linked to stock of consumables/packaging materials?	
Sales	Firms	[...] linked to sales?	value-added/sales tax levy
Start	Firms	[...] linked to contracts?	
Storage	Firms	[...] linked to storage and refrigeration?	goods deposit tax
Other Bus Payments	Firms		

## B Baseline Survey

### B.1 Summary of payments by tax category

Looking at zero payments by category. This can be seen by looking at the median payments column in Tables A2 and A3.

Table A2: Yearly total payments per business by tax category in USD

	Mean	Standard Deviation	Min	p10	p25	Median	p75	p90	Max
Communications	11.42	170.84	0.00	0.00	0.00	0.00	0.00	0.00	3,600.00
Contracts	0.01	0.34	0.00	0.00	0.00	0.00	0.00	0.00	7.78
Customs	5.95	91.69	0.00	0.00	0.00	0.00	0.00	0.00	2,010.00
Electricity	67.35	211.45	0.00	0.00	0.00	5.28	60.00	138.89	2,661.11
Fuel	5.88	95.11	0.00	0.00	0.00	0.00	0.00	0.00	2,160.00
Sanitation	19.36	76.06	0.00	0.00	0.00	0.00	0.00	51.56	1,160.00
Insurance	0.04	0.87	0.00	0.00	0.00	0.00	0.00	0.00	20.00
Labour	8.78	68.43	0.00	0.00	0.00	0.00	0.00	0.00	984.44
Licensing	85.34	896.32	0.00	0.00	0.00	0.00	42.22	115.56	20,550.00
Maintenance	0.93	10.01	0.00	0.00	0.00	0.00	0.00	0.00	150.00
Marketing	3.93	58.45	0.00	0.00	0.00	0.00	0.00	0.00	1,325.00
OtherTaxes	4.31	96.22	0.00	0.00	0.00	0.00	0.00	0.00	2,222.22
Packaging	4.67	80.67	0.00	0.00	0.00	0.00	0.00	0.00	1,777.78
Printing	0.28	5.64	0.00	0.00	0.00	0.00	0.00	0.00	130.00
Profit	2.56	20.53	0.00	0.00	0.00	0.00	0.00	0.00	400.00
Property	5.71	128.54	0.00	0.00	0.00	0.00	0.00	0.00	2,970.00
Royalties	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Sales Tax	31.65	443.92	0.00	0.00	0.00	0.00	0.00	0.00	8,000.00
Security/Judicial	3.57	49.86	0.00	0.00	0.00	0.00	0.00	0.00	1,126.67
Storage	1.33	23.81	0.00	0.00	0.00	0.00	0.00	0.00	533.33
Transport	22.43	273.03	0.00	0.00	0.00	0.00	0.00	0.00	5,333.33
Water	39.28	211.91	0.00	0.00	0.00	0.00	22.22	66.67	4,266.67
<b>All Categories</b>	324.78	1,422.94	0.00	0.00	0.00	66.67	191.11	543.33	25,446.11
Observations	534								

Table A3: Yearly total payments per household by tax category in USD

	Mean	Standard Deviation	Min	p10	p25	Median	p75	p90	Max
Animals	8.30	102.99	0.00	0.00	0.00	0.00	0.00	0.00	2,000.00
Business	0.23	3.48	0.00	0.00	0.00	0.00	0.00	0.00	70.44
Community	5.74	100.50	0.00	0.00	0.00	0.00	0.00	0.00	2,300.00
Customary	0.07	0.96	0.00	0.00	0.00	0.00	0.00	0.00	20.00
Documents	6.54	45.02	0.00	0.00	0.00	0.00	0.00	0.00	635.00
Education	659.73	1,722.91	0.00	0.00	0.00	230.00	694.44	1,676.67	29,500.00
Electricity	40.25	170.52	0.00	0.00	0.00	0.00	13.33	58.89	2,000.00
Health	34.08	321.41	0.00	0.00	0.00	0.00	0.00	0.00	6,720.00
Land/buildings	17.39	144.41	0.00	0.00	0.00	0.00	0.00	6.67	2,300.00
Life events	92.90	454.97	0.00	0.00	0.00	0.00	22.22	200.00	7,011.11
Public/legal	31.98	621.37	0.00	0.00	0.00	0.00	0.00	0.00	14,246.67
Revenue	6.45	55.80	0.00	0.00	0.00	0.00	0.00	0.00	801.11
Sanitation	71.91	514.10	0.00	0.00	0.00	0.00	3.00	66.67	9,574.44
Security	0.03	0.77	0.00	0.00	0.00	0.00	0.00	0.00	17.78
Transport	16.23	120.35	0.00	0.00	0.00	0.00	0.00	0.00	1,580.00
Vehicles	10.34	107.55	0.00	0.00	0.00	0.00	0.00	0.00	2,250.00
Water	151.99	236.32	0.00	0.00	31.11	66.67	200.00	393.33	2,420.00
<b>All Categories</b>	1,349.70	2,418.13	0.00	40.00	198.89	606.11	1,454.44	3,336.11	32,817.78
Observations	533								

## B.2 Formal vs informal payments

Table A4: Type of payments

Payment type	Share	Median	Mean
Formal only	22.29 %	\$ 33.30	\$ 122.46
Informal only	13.60 %	\$ 20.00	\$ 83.67
Formal and informal	64.11 %	\$ 50.00	\$ 133.72

### B.3 Payments and benefits

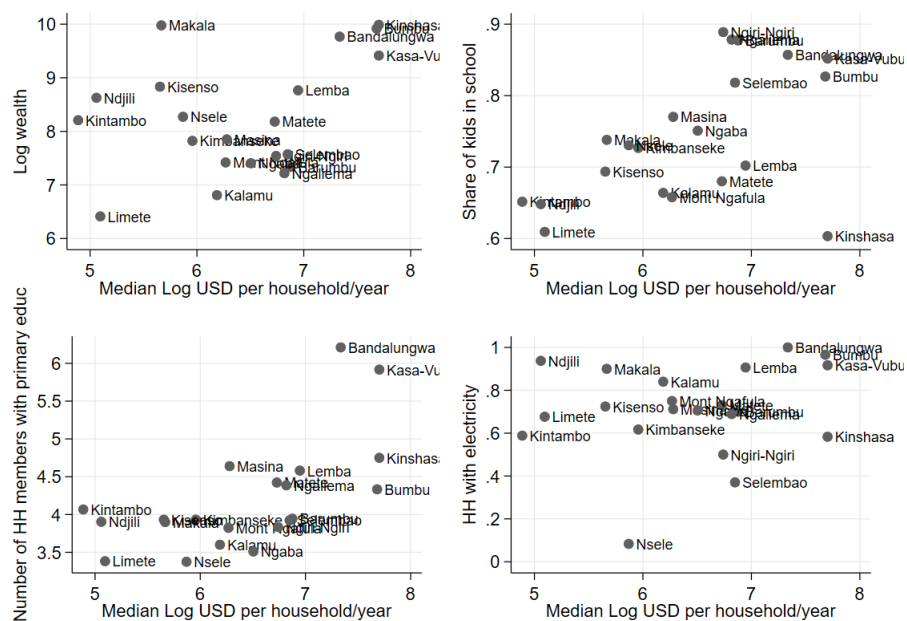
Figure A1: *Household benefits and payments*

Figure A2: *Firm benefits and payments*

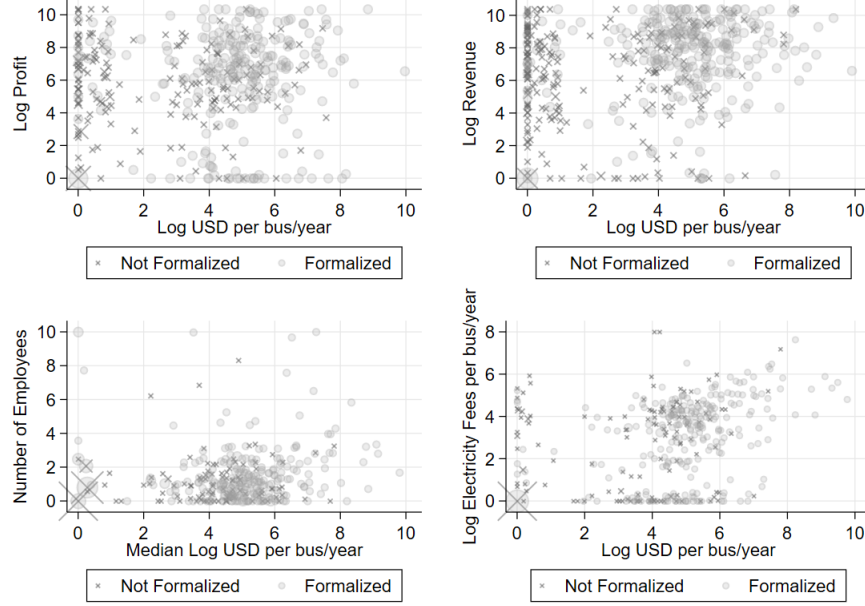


Table A5: Correlation between Registration and Tax Burden

	Formal USD per bus/year			Informal USD per bus/year		
	(1)	(2)	(3)	(4)	(5)	(6)
Registered	316.8*** (81.48)	327.3*** (75.88)	184.1*** (67.02)	68.17*** (20.14)	68.07*** (18.37)	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.4 Negotiability of tax payments by category

Table A6: Negotiability of household tax payments by tax category in USD

	Non Negotiable		Negotiable		Total
Animals	11	(58%)	8	(43%)	19
Business	3	(100%)	0	(0%)	3
Community	11	(69%)	5	(31%)	16
Customary	3	(60%)	2	(40%)	5
Documents	10	(40%)	15	(60%)	25
Education	936	(90%)	105	(10%)	1,041
Electricity	51	(22%)	184	(78%)	235
Health	35	(71%)	14	(29%)	49
Land/buildings	67	(70%)	29	(30%)	96
Life events	147	(46%)	170	(54%)	317
Public/legal	12	(52%)	11	(48%)	23
Revenue	12	(63%)	7	(37%)	19
Sanitation	52	(29%)	127	(71%)	179
Security	0	(0%)	1	(100%)	1
Transport	61	(66%)	32	(34%)	93
Vehicles	19	(49%)	20	(51%)	39
Water	420	(79%)	115	(21%)	535
<b>All Categories</b>	2,064	(69%)	934	(31%)	2,998

Table A7: Negotiability of business tax payments by tax category in USD

	Non Negotiable		Negotiable		Total
Communications	6	(100%)	0	(0%)	6
Contracts	0	(0%)	1	(100%)	1
Customs and Borders	10	(77%)	3	(23%)	13
Electricity	238	(61%)	151	(39%)	389
Fuel	11	(73%)	4	(27%)	15
Hygiene and Sanitation	95	(40%)	141	(60%)	236
Insurance	0	(0%)	1	(100%)	1
Labour	15	(58%)	11	(42%)	26
Licensing	256	(60%)	172	(40%)	428
Maintenance	0	(0%)	9	(100%)	9
Marketing	1	(7%)	13	(93%)	14
Other Taxes	1	(50%)	1	(50%)	2
Packaging	2	(40%)	3	(60%)	5
Printing	2	(40%)	3	(60%)	5
Profit	6	(29%)	15	(71%)	21
Property	5	(38%)	8	(62%)	13
Sales Tax	15	(60%)	10	(40%)	25
Security and Judicial	8	(47%)	9	(53%)	17
Storage	2	(40%)	3	(60%)	5
Transport and Vehicle	22	(55%)	18	(45%)	40
Water	144	(81%)	34	(19%)	178
<b>All Categories</b>	839	(58%)	610	(42%)	1,449



## B.5 Awareness of statutory tax payments

Table A8: Household reports knowing the official payment amount (by category in USD)

	Know		Don't Know		Total
Animals	7	(78%)	2	(22%)	9
Business	1	(50%)	1	(50%)	2
Community	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
Land/buildings	49	(68%)	23	(32%)	72
Life events	79	(63%)	46	(37%)	125
Public/legal	10	(71%)	4	(29%)	14
Revenue	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
<b>All Categories</b>	<b>999</b>	<b>(64%)</b>	<b>566</b>	<b>(36%)</b>	<b>1,565</b>

## B.6 Network Connections

Table A9: Summary Statistics for Network Connections

<i>Panel A: Households</i>									
	Min	P25	Median	P75	P90	Max	Mean	SD	N
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
<i>Panel B: Firms</i>									
	Min	P25	Median	P75	P90	Max	Mean	SD	N
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	1	1	1	0.330	0.470	528
Know Army Official (FARDC)	0	0	0	0	1	1	0.235	0.424	528
Know Intelligence Official (ANR)	0	0	0	0	0	1	0.030	0.172	527
Percentage of Roles Known	0	0	0.11	0.22	0.33	0.67	0.116	0.146	531
Number of Connections	0	0	1	2	3	6	1.020	1.294	538

## 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. 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 household 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.

If the citizen engages, they have a true payment liability  $\tau^*$ , which the state agent knows but the citizen does not. Instead, the citizen has a prior belief about her payment liability,  $\mu_\tau$ . 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 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.<sup>17</sup> In this case, the citizen's expected payoff is  $V_E - \mu_\tau - r - c_A$ , and the state agent's expected payoff is  $r$ .<sup>18</sup>

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  $\mu_\tau$ , since in this “collusive” setting payment levels are negotiated. If the state agent could, he has an incentive to manipulate  $\mu_\tau$  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  $\gamma$  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.

<sup>17</sup>The 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.

<sup>18</sup>We 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  $r$ .

## 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.<sup>19</sup> The Nash bargaining solution implies:

$$b^* = \gamma \left[ \frac{\mu_\tau + r + c_A - C_C}{1 + c_C} \right] + (1 - \gamma) \left[ \frac{C_B + r}{1 - c_B} \right] \quad (3)$$

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 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,  $\tau^*$ , while citizens only have a guess,  $\mu_\tau$ . We construe additional information as intervening on  $\mu_\tau$ . Second, officials are able 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.

<sup>19</sup>Note that in this case, the collusion payoffs are no longer the outside option payoff plus the bargaining weight times the joint surplus. To see this, let  $u_B$  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,  $h'(u_O) = -1$ .

## 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 of 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  $\mu_\tau$ :

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:

$$\begin{aligned} V^O &< V^E - \mu_\tau - c_A \\ \mu_\tau &< V^E - V^O - c_A - r \end{aligned}$$

To plot how  $\mu_\tau$  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  $\mu_\tau$  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,  $r\tau$ . 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  $\mu_\tau$  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  $\mu_\tau$  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 A10: Effects of lowering  $r$  and  $\mu_\tau$  by behavior without treatment

Already Legible and Making Official Payments 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 and Bargaining Collusively Without Treatment			
Quantity	Parameter	Intensive Margin	Extensive Margin
Better information (if under informed)	$\mu_\tau \downarrow$	$b \downarrow$	Pushes towards <i>Formal Payment</i> : in that case: $\tau \uparrow, r \uparrow, b = 0$
Lowering rent extraction	$r \downarrow$	$b \downarrow$	Pushes towards <i>Formal Payment</i> : in that case: $\tau \uparrow, r \uparrow, b = 0$
Not Legible to the State Without Treatment			
Quantity	Parameter	Intensive Margin	Extensive Margin
Better information (if under informed)	$\mu_\tau \downarrow$	no change	Pushes towards <i>Engagement with State</i> : in that case either <i>Bargaining</i> : $b \uparrow$ or <i>Formal Payment</i> : $\tau \uparrow, r \uparrow$
Lowering rent extraction	$r \downarrow$	no change	Pushes towards <i>Engagement with State</i> : in that case either <i>Bargaining</i> : $b \uparrow$ or <i>Formal Payment</i> : $\tau \uparrow, r \uparrow$

## 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 A3 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:**
  - *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.

## D.2 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.<sup>20</sup> 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:

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<sup>20</sup>Bruhn, Miriam and David McKenzie. 2009. “In Pursuit of Balance: Randomization in Practice in Development Field Experiments.” *American Economic Review* 1(4):200-232.

Figure A3: 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.

Semaine du 21 Septembre au 26 Sept 2015 Numéro identification du formulaire : 01 Kinshasa D.8.5

Impôts/taxes payés au cours de la semaine précédente												
A	B	C	D	E	F	G	H	I	J	K	L	M
Taxe [UTILISEZ code]	Avez-vous discuté de la légalité de cette taxe avec le client?	Pensez-vous qu'il y a eu de l'abus? [ANALYSE PRIVÉE]	Avez-vous informé le client de l'abus? (Remarque: seuls les clients dans les groupes I et III doivent être informés)	S'il 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 III)	Ampleur de l'abus (Note: 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?	Uniquement groupes II and III: Avez-vous fourni des informations d'assistance sur les abus?
204	oui	oui	oui	oui	Lemba	Mbanza Lemba	—	SNEL	Agents	2	oui	oui

$$Pr(\text{Control}) = (\text{No. avenues in commune in control}) / (\text{No. avenues in commune})$$

$$Pr(\text{Information}) = [(\text{No. avenues in commune treated}) / (\text{No. avenues in commune})] * p_{inf}$$

$$Pr(\text{Protection}) = [(\text{No. avenues in commune treated}) / (\text{No. avenues in commune})] * p_{pro}$$

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

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



## D.3 Balance

Table A11: Effects of Treatment Indicators on Coefficients

<i>Panel A: Effects of Protection Treatment</i>		
Outcome Variable	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
<i>Panel B: Effects of Consulting Treatment</i>		
Outcome Variable	Coefficients	P-value
Gender	-0.0055 (0.068)	0.936
Household Size	-0.038 (0.25)	0.878
Education	-0.016 (0.19)	0.933
Age	0.64 (1.4)	0.646
Wealth (log)	-0.43 (0.27)	0.127
Network Z-score	-0.11 (0.098)	0.257
Number of Employees	-0.028 (0.21)	0.894
Profit (log)	-0.0097 (0.13)	0.942

## E Additional Results and Robustness

### E.1 Main Results Tables

Table A12: Extensive Margin Effects of Protection and Tax Consulting

	Full Sample			Restricted Sample		
	<i>Dependent Variable:</i>			<i>Dependent Variable:</i>		
	Any Payment (1)	Any Formal (2)	Only Formal (3)	Any Payment (4)	Any Formal (5)	Only Formal (6)
Protection	0.020** (0.010)	0.021** (0.009)	0.017** (0.008)	0.045* (0.023)	0.047** (0.021)	0.052*** (0.017)
Information	0.004 (0.007)	0.003 (0.007)	0.005 (0.005)	0.027 (0.020)	0.021 (0.017)	0.019 (0.016)
R <sup>2</sup> (Protection)	0.146	0.128	0.098	0.121	0.112	0.125
R <sup>2</sup> (Information)	0.145	0.127	0.099	0.117	0.114	0.123
Observations	57,820	57,820	57,820	12,013	12,013	12,013
Control Mean	0.06	0.05	0.02	0.2	0.17	0.08

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$

Table A13: Extensive Margin Effects of Protection and Tax Consulting Separate for Households and Firms

	Households			Businesses		
	<i>Dependent Variable:</i>			<i>Dependent Variable:</i>		
	Any Payment (1)	Any Formal (2)	Only Formal (3)	Any Payment (4)	Any Formal (5)	Only Formal (6)
Protection	0.033** (0.012)	0.035*** (0.012)	0.034*** (0.008)	0.012 (0.015)	0.010 (0.014)	0.007 (0.011)
Information	0.007 (0.009)	0.009 (0.008)	0.006 (0.008)	0.006 (0.008)	0.005 (0.008)	0.012 (0.008)
R <sup>2</sup> (Protection)	0.177	0.156	0.131	0.090	0.078	0.067
R <sup>2</sup> (Information)	0.175	0.154	0.125	0.092	0.082	0.073
Observations	27,606	27,606	27,606	30,214	30,214	30,214
Control Mean	0.1	0.08	0.04	0.04	0.03	0.01

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ 

Table A14: Intensive Margin Effects and ATE of Protection and Tax Consulting

	<i>Dependent Variable: Amount Paid (USD)</i>				
	ATE	Conditional on post	Lower bound	Upper bound	Conditional on pre
	(1)	(2)	(3)	(4)	(5)
Protection	-0.495 (1.256)	-22.448* (11.373)	-50.265*** (11.460)	-14.494 (11.186)	0.162 (2.059)
Information	-0.266 (0.771)	-9.103 (9.229)	-27.388*** (8.406)	-6.176 (9.203)	3.354 (2.019)
R <sup>2</sup> (Protection)	0.018	0.163	0.247	0.164	0.036
R <sup>2</sup> (Information)	0.017	0.114	0.162	0.114	0.032
Observations	57,820	4,035	3,587	3,580	8,085
Control Mean	2.93	47.62	47.62	47.62	6.59

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ 

Table A15: Intensive Margin Effects and ATE of Protection and Tax Consulting for Restricted Sample

	<i>Dependent Variable: Amount Paid (USD)</i>				
	ATE	Conditional on post	Lower bound	Upper bound	Conditional on pre
	(1)	(2)	(3)	(4)	(5)
Protection	0.326 (2.160)	-8.396 (8.173)	-31.403*** (7.530)	-3.444 (7.710)	0.957 (2.465)
Information	0.104 (1.603)	-2.246 (8.112)	-24.838*** (7.607)	1.444 (7.028)	3.585 (2.165)
R <sup>2</sup> (Protection)	0.030	0.124	0.249	0.126	0.036
R <sup>2</sup> (Information)	0.025	0.108	0.221	0.110	0.031
Observations	12,013	2,566	2,427	2,417	6,122

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

## 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 A16: Extensive Margin Effects of Protection and Tax Consulting Without Interacting Covariates and Treatment

	Full Sample			Restricted Sample		
	<i>Dependent Variable:</i>			<i>Dependent Variable:</i>		
	Any Payment (1)	Any Formal (2)	Only Formal (3)	Any Payment (4)	Any Formal (5)	Only Formal (6)
Protection	0.020* (0.011)	0.020* (0.010)	0.016 (0.010)	0.045 (0.027)	0.048* (0.024)	0.052** (0.023)
Information	0.002 (0.008)	0.001 (0.008)	0.003 (0.006)	0.028 (0.023)	0.021 (0.020)	0.018 (0.017)
R <sup>2</sup>	0.136	0.118	0.084	0.096	0.092	0.098
Observations	57,820	57,820	57,820	12,013	12,013	12,013
Control Mean	0.06	0.05	0.02	0.2	0.17	0.08

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

Table A17: Extensive Margin Effects of Protection and Tax Consulting Without Covariates

	<i>Dependent Variable:</i>		
	Any Payment (1)	Any Formal (2)	Only Formal (3)
Protection	0.019* (0.010)	0.020** (0.010)	0.015 (0.009)
Information	0.003 (0.007)	0.001 (0.007)	0.004 (0.007)
R <sup>2</sup>	0.133	0.115	0.077
Observations	57,820	57,820	57,820
Control Mean	0.06	0.05	0.02

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

Table A18: Extensive Margin Effects of Protection and Tax Consulting and Both

	<i>Dependent Variable:</i>		
	Any Payment (1)	Any Formal (2)	Only Formal (3)
Protection	0.020** (0.010)	0.021** (0.009)	0.017** (0.008)
Information	0.004 (0.007)	0.003 (0.007)	0.005 (0.005)
Protection $\times$ (demeaned) Information	0.014 (0.017)	0.008 (0.015)	-0.001 (0.015)
Information $\times$ (demeaned) Protection	-0.000 (0.019)	-0.005 (0.017)	-0.011 (0.015)
R <sup>2</sup> (Protection)	0.146	0.128	0.098
R <sup>2</sup> (Information)	0.145	0.127	0.099
Observations	57,820	57,820	57,820
Control Mean	0.06	0.05	0.02

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ Table A19: Extensive Margin Effects of Protection and Tax Consulting Including Post $\times$ Advocacy

	<i>Dependent Variable:</i>		
	Any Payment (1)	Any Formal (2)	Only Formal (3)
Protection	0.021** (0.009)	0.021** (0.009)	0.018** (0.008)
Information	0.004 (0.007)	0.003 (0.007)	0.005 (0.005)
Post $\times$ Advocacy (Protection)	-0.012 (0.008)	-0.007 (0.007)	-0.013* (0.006)
Post $\times$ Advocacy (Information)	-0.002 (0.011)	0.001 (0.011)	0.002 (0.009)
Protection $\times$ Post $\times$ Advocacy	0.037** (0.016)	0.028* (0.016)	0.031** (0.014)
Information $\times$ Post $\times$ Advocacy	0.005 (0.015)	0.001 (0.014)	0.004 (0.011)
R <sup>2</sup> (Protection)	0.146	0.129	0.099
R <sup>2</sup> (Information)	0.145	0.127	0.099
Observations	57,820	57,820	57,820
Control Mean	0.06	0.05	0.02

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

Table A20: Heterogeneous Treatment Effects

	Any Payment (1)	Any Formal (2)	Only Formal (3)	ATE (4)	Conditional on Post (5)
Protection	0.020** (0.010)	0.021** (0.009)	0.017** (0.008)	-0.495 (1.256)	-22.448* (11.373)
Information	0.004 (0.007)	0.003 (0.007)	0.005 (0.005)	-0.266 (0.771)	-9.103 (9.229)
Education (Protection)	-0.000 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.873 (0.576)	-5.582* (2.965)
Education (Information)	0.003 (0.003)	0.002 (0.004)	0.002 (0.003)	0.191 (0.419)	0.709 (3.530)
Network Z-Score (Protection)	0.001 (0.005)	0.003 (0.005)	0.005 (0.003)	1.020 (0.936)	4.864 (7.911)
Network Z-Score (Information)	0.001 (0.006)	0.001 (0.006)	0.003 (0.004)	-0.176 (0.457)	-3.228 (3.547)
Protection $\times$ Education	0.004 (0.005)	0.006 (0.005)	0.007* (0.004)	1.592* (0.815)	11.917** (4.912)
Information $\times$ Education	-0.001 (0.004)	-0.000 (0.004)	0.001 (0.003)	-0.709 (0.680)	-9.225 (6.827)
Protection $\times$ Network Z-Score	-0.006 (0.009)	-0.009 (0.008)	-0.007 (0.007)	-1.585 (0.960)	-11.944 (8.195)
Information $\times$ Network Z-Score	-0.001 (0.009)	-0.000 (0.008)	0.001 (0.006)	2.190 (1.851)	23.481 (20.154)
R <sup>2</sup> (Protection)	0.146	0.128	0.098	0.018	0.163
R <sup>2</sup> (Information)	0.145	0.127	0.099	0.017	0.114
Observations	57,820	57,820	57,820	57,820	4,035
Control Mean	0.06	0.05	0.02	2.93	47.62

*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. \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

Table A21: Heterogeneous Treatment Effects by Registered

	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)	0.012 (0.028)	-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 <sup>2</sup> (Protection)	0.094	0.082	0.071	0.017	0.338
R <sup>2</sup> (Information)	0.094	0.083	0.076	0.014	0.205
Observations	27,266	27,266	27,266	27,266	1,107
Control Mean	0.07	0.06	0.03	2.98	42.62

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ 

Table A22: ATE of Protection and Tax Consulting with Different Coding

	<i>Dependent Variable: Amount Paid (USD)</i>		
	Original Coding (1)	Winsorized 99 (2)	Winsorized 95 (3)
Protection	-0.495 (1.256)	0.173 (0.888)	0.186 (0.504)
Information	-0.266 (0.771)	-0.547 (0.575)	-0.312 (0.309)
R <sup>2</sup> (Protection)	0.018	0.042	0.060
R <sup>2</sup> (Information)	0.017	0.039	0.058
Observations	57,820	57,820	57,820
Control Mean	2.93	2.36	1.76

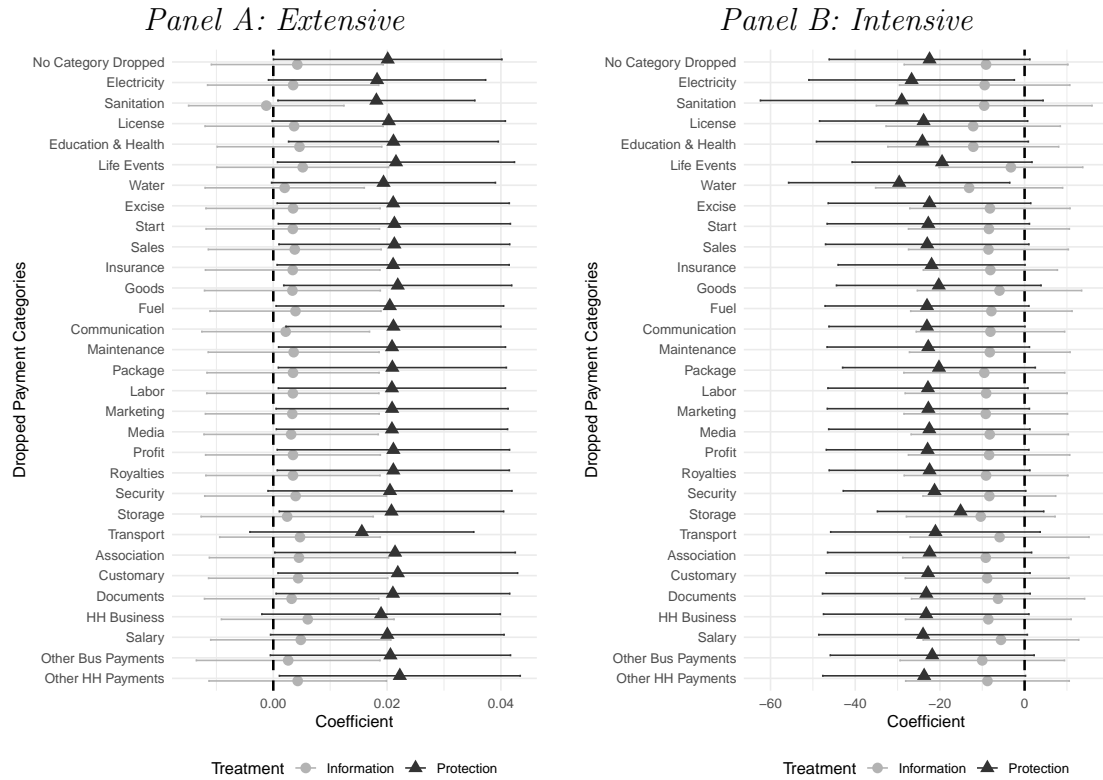
\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$ 

Table A23: Intensive Margin Effect (Conditional on Post) of Protection and Tax Consulting with Different Coding

	<i>Dependent Variable: Amount Paid (USD)</i>		
	Original Coding (1)	Winsorized 99 (2)	Winsorized 95 (3)
Protection	-22.448* (11.373)	-11.165 (6.689)	-5.616* (3.136)
Information	-9.103 (9.229)	-8.322 (6.512)	-4.597 (3.201)
R <sup>2</sup> (Protection)	0.163	0.261	0.352
R <sup>2</sup> (Information)	0.114	0.224	0.319
Observations	4,035	4,035	4,035
Control Mean	47.62	38.32	28.57

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

Figure A4: Effect of Protection and Information When Dropping Payment Categories



*Notes:* This figure shows the coefficients for the protection and information treatments when dropping each tax 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 tax category individually.