

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 forego engaging with the state because of vulnerability to opportunistic demands by state agents. We study two randomized interventions in Kinshasa, DRC designed to empower citizens: one provided information about statutory payment obligations and another offered protection from abusive officials. Testing predictions from a model, we examine the effects of empowerment 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, but not information had clear extensive margin effects, increasing the share of citizens making formal payments and engaging with the state in the first place. Overall, we show how empowering citizens can help countries transition away from a low-tax, high-informality 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 in the absence of perfect state enforcement capacity (Brennan and Buchanan, 1978; Levi, 1989; Besley and Persson, 2009). For many low income countries, the challenges of revenue collection are compounded by the fact that citizens often prefer to remain hidden from the state altogether. Indeed, many citizens in developing countries live in informality, maintaining an “uncertain, undocumented, and irregular relationship to the state” (Gottlieb, 2024, p.272). In practice, citizens do not obtain 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.

In deciding whether to pay taxes or engage with the state more broadly—by registering property, acquiring licenses, or accessing public services—citizens weigh the benefits of state-provided goods against the costs of their payments and non-state outside options (Levi, 1989; Timmons, 2005; Prichard, 2015; Joshi, Prichard and Heady, 2014). Importantly, the costs to citizens of paying taxes or becoming legible to the state are often exacerbated by opportunistic or even predatory demands for informal payments by street-level state agents (Shleifer and Vishny, 1993; Banerjee, 1997).¹ Such demands by state agents are common in low income countries due to inadequate wages, weak performance incentives, and poor state capacity to control state agents (Khan, Khwaja and Olken, 2016; Berwick and Christia, 2018; Martin and Raffler, 2021). For citizens, the costs and uncertainties associated with informal payments can be prohibitive given that they interact with street-level state agents in the course of making a wide range of tax and fee payments (Weigel,

¹We define informal payments as any tax or fee payments to state agents that are not legally mandated payments or amounts. Informal payments can take many forms, including illicit bribe or rent payments, voluntary ‘pinch’ payments to obtain services, or even informal payments that have become normalized and may even be perceived as legitimate (Prud’Homme, 1992; van den Boogaard, Prichard and Jibao, 2021). In developing our theory, we primarily focus on illicit bribe or rent payments to state agents. However, given uncertainty around statutory payments and amounts, citizens may or may not be aware that these payments are in fact informal.

2020; Khan, Khwaja and Olken, 2016; Bertrand et al., 2007; Olken and Barron, 2009).² Consistent with this, there is substantial evidence that vulnerability can be a significant disincentive to small firms becoming registered and paying formal taxes (Joshi, Prichard and Heady, 2014).

This study examines whether it is possible to shift away from a low revenue, low engagement equilibrium by empowering citizens in their interactions with opportunistic street-level state agents. Such empowerment may allow citizens to resist predatory informal demands by state agents, enable collective bargaining and the construction of more robust ‘fiscal contracts’ and, ultimately, make it more feasible for citizens to engage with—and become visible to—the formal state. Yet, in practice, interventions that aim to empower citizens and strengthen their ability to navigate demands for illegal payments have received little attention to date. Recent studies on tax collection have primarily examined whether it is possible to increase revenue by strengthening collection and enforcement (Allingham and Sandmo, 1972; Khan, Khwaja and Olken, 2016; Weigel, 2020) or by improving tax morale or changing social norms in a bid to encourage more voluntary compliance (Luttmer and Singhal, 2014). Meanwhile, efforts to address informality have focused primarily on reducing the direct costs of formalization and compliance, but with limited success (De La O, 2022; Jaramillo Baanante, 2009; de Mel, McKenzie and Woodruff, 2012). For instance, results from a recent EGAP Metaketa find that reducing transaction costs increased formalization in only two—and increased revenue in only one—of six coordinated studies (De La O, 2022).

The lack of attention to strengthening the bargaining power of citizens is surprising given that empowered citizens are viewed as necessary to building a strong but constrained state (Acemoglu and Robinson, 2020). Indeed, strong fiscal contracts emerge from citizen-state bargaining over the terms of taxation, which implies that empowered citizens will be able to extract more concessions from, and impose more constraints on, the state (Levi, 1989). Yet, empowering citizens is not necessarily an unambiguous good for state revenue. A well-known competing view offered by Scott (2010) contends that empowerment could also enable citizens to avoid the state. This view is echoed in a large literature that attributes tax non-compliance to a weak state pitted against savvy citizens bent on evasion (Allingham and Sandmo, 1972). All in all, more evidence is needed to determine whether empowering citizens is a promising, if under-explored, pathway to escaping the

²Granted, collusive payments can also reduce citizens’ costs (albeit while depriving the state or revenue) when paid in lieu of formal taxes (Shleifer and Vishny, 1993). Our theory accounts for this by allowing for informal payments both instead of and on top of, formal payment amounts.

low-revenue equilibrium.

We address this by examining the effects of two empowerment interventions, designed in collaboration with a Congolese civil society organization and implemented in Kinshasa, the capital of the Democratic Republic of the Congo (DRC). The *information* intervention provided households and businesses with guidance on what they should be paying according to the law.³ The *protection* intervention connected households and businesses to an influential civil society organization capable of advocating on their behalves. This intervention is motivated by the idea that social networks and ties to powerful actors reduce vulnerability to exploitation by state agents.

To clarify the predicted effects of these two interventions, we first develop a formal model, informed by the results from a baseline survey conducted with 1067 households and business owners in Kinshasa. Analysis of the baseline data shows three main patterns. First, few citizens make any payments to the state, consistent with a high degree of informality. Second, for households and businesses that do make payments, formal and informal payments are positively correlated, suggesting that these 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 kinds of payments. The baseline survey also confirms that bargaining over specific payments is frequent and that households and businesses suffer from both information and power deficits in their negotiations. Overall, the baseline data strongly suggests a pattern in which citizens either make no payments, avoiding the state entirely, or engage with the state to obtain benefits but at the cost of making both legal and informal payments.

To capture these patterns, we model citizens as making two decisions: whether to engage the state in the first place and, conditional on engagement, 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).⁴ Citizens who insist on making a formal payment can still face demands for rents by state agents. We model better information and greater protection as reducing bribes and rents that state agents can charge.

The model shows how information and protection can have not only intensive but also extensive

³For ease of exposition, we refer to households and businesses jointly as ‘citizens’ throughout the paper.

⁴The model presented here is a revised version of the model presented in our original pre-analysis plan. See Appendix C.9.1 for a discussion of the differences. The pre-registration was updated with the revised model before we analyzed the extensive margin effects.

margin effects. On the intensive margin, empowerment reduces informal payment *amounts* for citizens who are already engaging with the state. Empowerment also produces changes on the extensive margin, making some citizens more willing to engage the state in the first place and others more willing to switch from negotiating bribes to demanding formal payments. Yet, the model also highlights a little-regarded effect of empowerment interventions: they can actually increase citizens' informal payments while still being welfare enhancing by increasing their access to benefits.

We test predictions on 271 households and businesses randomly sampled within neighborhoods in Kinshasa and randomly assigned to either a control group (that only participates in data collection training and data collection), an information treatment group, a protection treatment group, or a group that received both information and protection treatments. We collected post-treatment data on a weekly basis by having all households and businesses report, via a smartphone survey, on all payments made in the previous week. Post-treatment data was collected for up to 19 weeks. We analyze the effects of the information and protection treatments on all payments and on a subset of five categories of common household payments (education, life events, electricity, sanitation, and water) and three categories of business payments (electricity, sanitation, and licenses).⁵

Overall, 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 who started to pay taxes for the first time as well as make formal tax 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. All in all, these results support the conclusion that empowering citizens 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 empowerment interventions can increase citizens' ability to negotiate predatory state demands and, ultimately, formal tax payments. Recent studies of state-centered interventions have noted that while stronger tax collection can increase revenue, it also poses a risk of expanded demands for bribes (Khan, Khwaja and Olken,

⁵We identified these categories from the baseline data because there is variation in whether they are paid at all (implying scope for extensive margin effects) and because our descriptive data confirms that they are subject to information and power asymmetries to varying degrees such that they might respond differently to the interventions.

2016; Weigel, 2020). Khan, Khwaja and Olken (2016), for example, find that strengthening incentives for tax collectors lead to increased bribe payments for many households because performance pay increased the collectors' bargaining power. Our results suggest that empowering citizens could be an important counterpoint to more state-centered interventions.

One potential explanation for the lack of attention to interventions to increase citizen bargaining power is that empowerment is often viewed as endogenous to taxation—indeed, research on the ‘fiscal contract’ sees efforts by the state to collect taxes as a catalyst that mobilizes citizens to challenge predation and, ultimately, constrain the state (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, stronger fiscal contracts, and more voluntary taxpayer compliance (Prichard, 2015). Consistent with this, Martin et al. (2021) find that a ‘bottom-up’ intervention that improved communication, transparency, and actual public goods provision increased the tax compliance of market vendors in Malawi. Others have tested the effects of empowerment interventions in non-tax settings to some effect. For instance, Peisakhin and Pinto (2010) examine whether information and protection interventions increased the ability of New Delhi slum dwellers to obtain ration cards, finding support only for the former.⁶ Banerjee et al. (2018) similarly find that information empowered citizens to negotiate with local officials to receive their fair share of a targeted aid program. Our study reinforces the value of citizen-centered approaches as one of the first, to our knowledge, to empower citizens in their actual negotiations over payments to state agents and to demonstrate the effectiveness of protection in particular.

Finally, our most striking finding is that empowerment interventions can lead not only to more formal payments but also to more engagement with the state. The formalization literature has generally theorized that citizens decide whether to become legible to the state (e.g., to obtain official identity documents or formally register a business) by weighing the benefits of greater access to goods against the transaction costs of registration and future *formal* tax payments (De La O, 2022; Bowles, 2023; Lee and Zhang, 2016). Yet, these studies too overlook that legibility could also entail additional costs in the form of demands for informal payments. Failing to consider

⁶The protection intervention in Peisakhin and Pinto (2010) was similar in spirit to ours in that citizens presented a letter of support from a local civil society organization when applying for a ration card.

these costs could help to explain why efforts to reduce transaction costs or remove other barriers to formalization are insufficient to induce greater citizen engagement with the state. Indeed, [De La O \(2022\)](#) conjecture that efforts to reduce the transaction costs of formalization could be undermined by the fact that local state agents often benefit from informality ([Holland, 2016](#)). The theory and evidence presented here suggest that empowering citizens could reduce the opportunistic benefits that state agents obtain from operating in the gray zone, making citizens more willing to start paying taxes—and become legible to the state—in the first place.

2 Context and Motivation

Increasing formal revenue collection has long been a central challenge in the DRC. 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. Political instability and conflict since the mid-1990s exacerbated the situation. During the conflict and after, many state agents resorted to compensating themselves by collecting informal bribes instead of statutory payments or rents on top of statutory payments ([Weijs, Hilhorst and Ferf, 2012](#); [Titeca and Kimanuka, 2012](#); [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. In reality, 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 statutes ([Englebort and Kasongo, 2014](#); [De Herdt, Titeca and Wagemakers, 2010](#)). At present, both state agents' salaries and state-provided services are often funded through fees imposed on citizens. A portion of these fees are commonly retained by state officials, many of whom receive little or no official salary. Consequently, a concern of local state officials is identifying means to extract revenue to pay their own salaries ([Englebort and Kasongo, 2014](#)). Citizens also face high costs to accessing many basic public services owing to the absence of adequate 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

are required payments for anyone who wants to ensure that they are not excluded from accessing benefits.

To gain a better sense of the kinds of payments that citizens make, we conducted a baseline survey with 533 households and 534 businesses in Kinshasa across 18 payment categories for households and 22 payment categories for businesses.⁷ The survey measured self-reported payments and amounts, where formal (informal) payments were defined as legally (not legally) mandated. Our analysis of the baseline data reveals several patterns that inform our theoretical model below.⁸

First, the baseline survey confirmed that very few citizens in Kinshasa engage with the state when it comes to paying taxes and fees. The vast majority of Congolese households and businesses make no payments to state officials across all payment categories (see Appendix C.2). The median business only makes payments in one out of 22 tax categories: electricity. Even at the 90th percentile only four tax categories see any payments. The median household only makes payments in two out of 18 categories: education and water. At the 90th percentile, 11 out of 18 payment categories still see no payment.

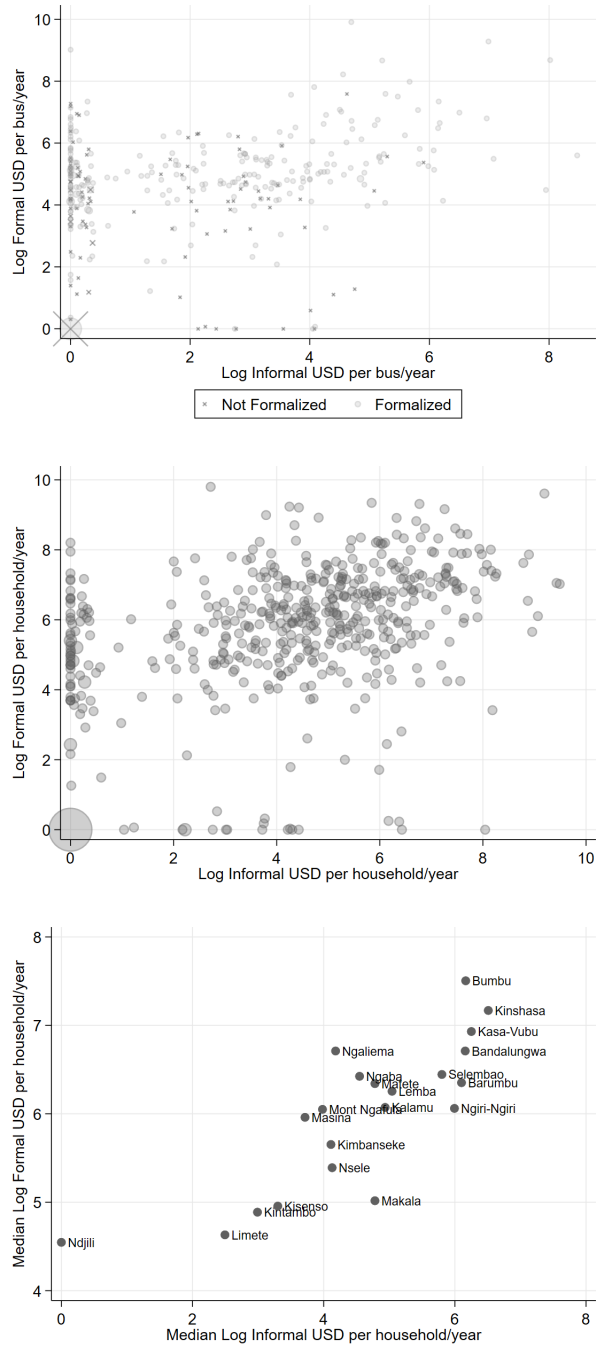
Second, citizens who do make payments tend to make formal *and* informal payments. This is important because previous studies suggest that formal and informal payments might be substitutes rather than complements in some settings (Shleifer and Vishny, 1993). Yet, as can be seen in Figure 1, there is 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 (see Appendix C.3).

Third, those households and businesses that do pay also tend to enjoy greater benefits, consistent with a story in which access to benefits entails higher formal and informal costs. The third panel of Figure 1 shows a strong relationship between median informal and formal payment by neighborhood for households. We also find that households in neighborhoods that pay more also enjoy better access to education and electricity (see Figure A2 in Appendix C.4). 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 A3 in Appendix C.4).

⁷Households 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. For more on the baseline survey, see Appendices B and C.1.

⁸In light of concerns about measurement or reporting bias with self-reported data, we also explore other approaches to coding formal and informal payments. The overall patterns in the data are not sensitive to coding approach.

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.

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 \$184.1 and \$52.3, respectively. 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. Evidence from the baseline survey shows that households and businesses report a high degree of variation in the percent of payments that were negotiable across fifteen different payment categories (see Appendix C.5). The majority of payments involve some negotiation: survey respondents indicate that 64% of their payments were a combination of formal and informal, while 14% are only informal. Survey respondents classified only 22% as purely formal and free from negotiation over the payment amount. The numbers of payments that are negotiated are also high in the absolute. For instance, as shown in Appendix Table A6, households report that 10 percent of education payments but 78 percent of their electricity payments are negotiable. Similarly, Appendix Table A7 shows that, for businesses, 39 percent of electricity payments, and 40 percent of licensing payments, are seen as negotiable.

Second, our baseline data (along with qualitative reports) reinforces the claim that citizens in Kinshasa face both information and power asymmetries in their bargaining 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.

Similarly power asymmetries arise because state agents use their superior status or connections to extract informal payments from citizens, for instance by demanding bribes in order to speed up service provision or prevent a service from being terminated (Sánchez de la Sierra et al., 2022). Table A9 in the Appendix shows that the median household knows no official in the tax agencies, local government, or security services related to nine different categories of payments; the median

business knows only one official.

This baseline data is consistent with extensive anecdotal evidence of bargaining under information and power asymmetries in the DRC. Our qualitative interviews revealed, for instance, that households and businesses that obtain state-provided electricity are frequently visited by state agents who demand payments, threatening to cut-off electricity access. Citizens often pay some negotiated amount to get the agent to go away or, if the household or business is well-connected, will 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 a variety of informal arrangements (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 on landlords (Sharma and Biswas, 2020).

While such interactions are widespread in contexts like the DRC, 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 what they should be paying but still lack the bargaining power to negotiate favorable arrangements. That 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 both information and 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 summarizes 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 finding above that payments are complementary instead of substitutes, which suggests that a key barrier to engagement is a citizen’s ability to negotiate informal payments. A citizen may be pessimistic about what they can gain from bargaining if they think they owe a lot

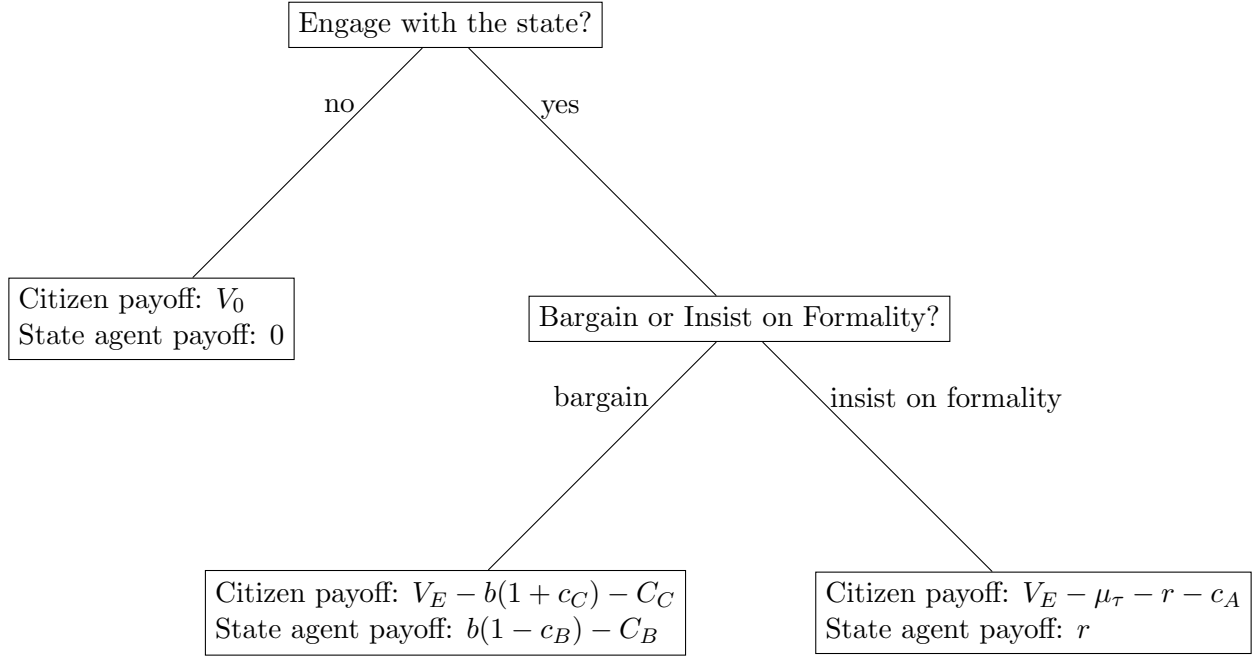
or if they feel vulnerable to state agents. We thus consider comparative statics on how improving knowledge about statutory payments (a goal of the information intervention) and lowering rents that government agents can extract (a goal of the protection intervention) affect how much citizens pay when bargaining collusively with state agents as well as citizens' willingness to become visible to those agents in the first place.

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. In Appendix Section A 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. If the citizen engages, they have a true payment liability τ^* , which the state agent knows but the citizen does not. Instead, the citizen has a prior belief about her payment liability, μ_τ . When engaging, the citizen can either (1) collude privately with the state agent over a bribe to be paid in lieu of the legal amount, or (2) insist on making a official payment, for instance by demanding a receipt or insisting on conducting the transaction at an official state office. If the citizen insists on an official payment, they pay the formal amount and an additional transaction cost. 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 (Shleifer and Vishny, 1993). Alternatively, the state agent and the citizen may prefer to collude in private. 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.

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 either the expected payment liability, rent payment, and cost of a formal transaction (when the bribe is too high), or the transfer and 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, τ^* , while citizens only have a guess, μ_τ . We construe additional information as intervening on μ_τ . 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.⁹

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 or official equilibrium and are not induced to switch by changes in μ_τ or r . The extensive margin predictions capture the effects on the share of citizens paying or the amounts paid when citizens are induced to switch equilibrium payment behavior by the parameter changes. We note that we can derive predictions for the effects of decreases in μ_τ and r on official payments τ , bribes b , and rents r , as summarized in Figure 3 and Table A10 in the Appendix. However, we state our hypotheses in terms of *total payments*. This is due to the empirical challenges of reliably distinguishing between formal and informal payment amounts in our self-reported data.

Our intensive margin predictions vary on whether citizens are in the collusive or official payment equilibrium and by the type of empowerment. In the collusive equilibrium, lowering the rent (r)

⁹In Appendix A we discuss why we believe protection operates on r and not a cost parameter.

reduces the payoff of 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 μ_τ will also reduce the bribe in equilibrium. When citizens are already making formal payments, lowering r directly reduces the citizen's payment. However, changing μ_τ will have 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, are intensive margin predictions are:

- **Intensive Margin Effect 1:** Lowering r (through protection) or lowering μ_τ (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 μ_τ (through information) should have no effect on the amount paid by these citizens.

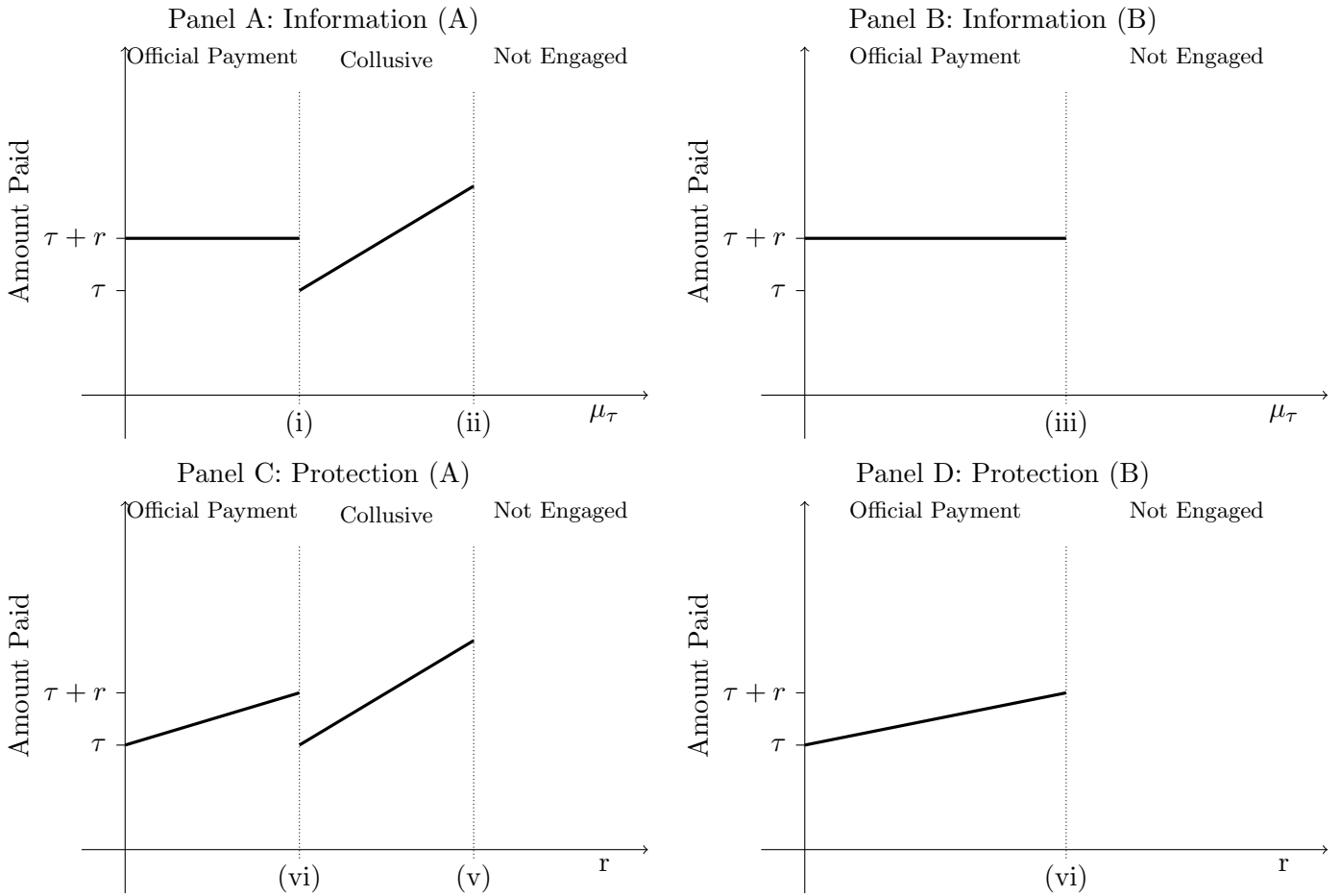
Our first extensive margin effect is for citizens who, prior to the intervention, are not engaging with the state. For some of these citizens, changes to r and μ_τ will induce engagement with the state and will decrease the costs of both bargaining and insisting on official payments. In a similar vein, for citizens who start in the collusion equilibrium, changes to r and μ_τ will induce some to switch to the official equilibrium.¹⁰ To summarize, our main extensive margin predictions are:

- **Extensive Margin Effect 1:** Lowering r (through protection) or lowering μ_τ (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 μ_τ (through information) will increase the share of citizens making official payments.

All in all, this model highlights the importance of testing different types of empowerment interventions when it is difficult to anticipate whether information or power asymmetries present the greatest obstacle to official payments. The model shows how additional information is unlikely

¹⁰We 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 being invisible to the state directly to the official payment equilibrium, as shown in Panel B and D of Figure 3.

Figure 3: Effects of intervening on r and μ_τ



Notes: These figures show the effect of changes to μ_τ (Panel A and B) and r (Panel C and D) on the amount of payments made. Panels A and C show the effects for the cases when bargaining is possible, this is, when there exists a range of r or μ_τ 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 A.7 in the appendix provides more detail on these two cases and characterizes the thresholds (i), (ii), (iii), (iv), (v), and (vi).

to impact citizens that are already making official payments. Moreover, information and power asymmetries can be independent of one another; for instance, it is possible for citizens to have perfect information on some payments, yet still face demands for rents by state agents due to a power asymmetry. This underscores the importance of investigating whether it is more effective to provide citizens with better information or strengthen their influence vis-à-vis state agents. We return in the conclusion to a discussion of scope conditions and of extensions to the theory developed here.

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.¹¹

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. 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.

Households and businesses assigned to the *protection* treatment also received weekly calls by an ODEP expert for a period of 18 weeks and were asked to report on their previous and upcoming payments. This treatment differed from the information treatment, however, in that citizens who reported suspicious payments were alerted to that fact and received an offer from ODEP to investigate the payment and state agent. ODEP also informed participants 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 officials) because ODEP regularly

¹¹ODEP employs several professionals with expertise in household and firm taxation. Our collaboration with ODEP was motivated in part by the fact that citizens are more trusting of civil society organizations than of government organizations (see Appendix C.8).

conducts such campaigns and is perceived as being expert on this subject.¹² 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. Citizens (and officials) viewed threats to bring in ODEP as credible because of the organization’s expertise and reputation.

4.2 Sampling and Randomization

We recruited households and businesses in Kinshasa 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 2.¹³ 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 one of the upcoming 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 provided participants with instructions on how to record tax and fee payments on a daily basis using a smartphone loaded with a custom application, which provides our main outcome data (discussed more below). All participants were asked to report data for up to 20 weeks.¹⁴ 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.) First, 48 avenues were assigned to treatment and 48 to control, blocking on commune. For avenues assigned to control, all households or businesses recruited from that avenue joined the control group. For avenues assigned to treatment, recruited households and businesses were further randomly assigned with equal probability to one of three treatment conditions (information, protection, or both).¹⁵ We took

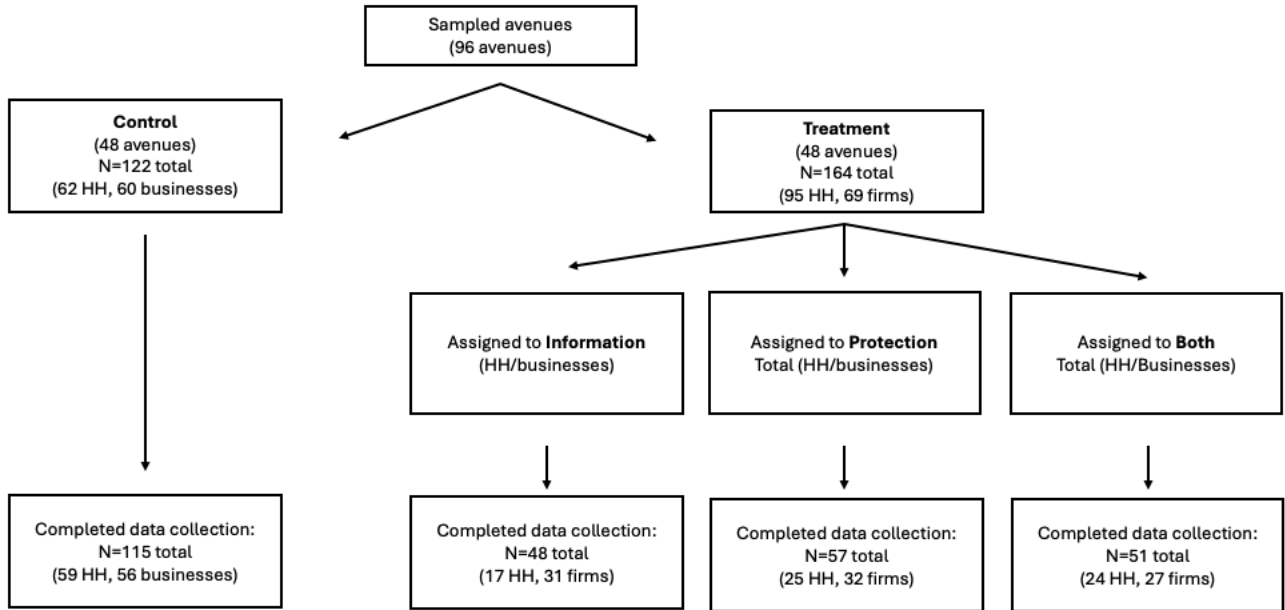
¹²ODEP also regularly holds a seat at parliament and in government meetings. The advocacy campaign was held as planned from November 19 to December 1, 16 weeks after the first training and 6 weeks after the last.

¹³Respondents 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.

¹⁴The exact length of reporting time varied for respondents depending on the time point at which they were recruited into the study and trained.

¹⁵Below we focus on estimating the main effects of the information and protection treatments since the model does

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.

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.¹⁶ 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 C).

not predict interaction, although we explore interaction effects in Table A18.

¹⁶We took measures to ensure that the smartphone data collection activities were separate from the tax consulting activities to minimize concerns about reporting bias.

4.3 Data and Measurement

To test our hypotheses, we need data on whether participants are making formal or informal tax payments and how much. We obtain this data from the weekly reporting of tax and fee payments via smartphone. A fundamental measurement challenge is that informal payments are often unknown and illicit payments are 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 often require relying on recall data, which can be biased. We aimed to overcome these problems by collecting data on payments directly from households and businesses. Additionally, we asked participants to report on informal payments broadly speaking to reduce the sensitivity of asking specifically about illicit payments. Overall, using the custom smartphone application, households (businesses) recorded daily payments in 18 (22) categories. Participants were trained on how to record these payments on a daily basis; all data was uploaded weekly. Overall, we have data on 4,706 payments.

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 are not looking at formal and informal *amounts* separately. While the model generates predictions on τ , bribes, and rents, and we did ask about formal and informal payments on the survey, it is difficult to distinguish among these reliably. The information treatment could lead respondents to classify a larger part of their payments as informal. Given that we are predicting the treatments will decrease the amount of informal taxes paid, this would have the effect of downward biasing our results. Second, the treatment could induce some respondents who had previously reported only paying formal taxes to now report paying some informal taxes. 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 [A20–A21](#)).

5 Extensive Margin Results

5.1 Estimating Extensive Margin Effects

Our theoretical framework outlined in Section 3 predicts two extensive margin effects from either information or protection: inducing citizens to (1) start engaging with the state and thus start making formal and/or informal payments and (2) to stop colluding with state agents and rather insist on making official payments. We thus 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 does not only apply once for each respondent but 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 includes the mean-centered information treat-

ment variable and its interaction with the protection treatment as controls, and another regression analogously specified to estimate the effect of the Information treatment.¹⁷

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 extensive margin specification below), ϕ_c^a is a vector of commune fixed effects, and θ_j^a are payment category fixed effects. To account for time trends in payments we include a vector of reporting week fixed effects, η_t^a . Standard errors are clustered by avenue since the first-level treatment was assigned at by avenue, and we use weights to account for assignment probabilities. By randomization, β_1^a and β_2^a capture the extensive margin effects of the treatments. We use a multiple testing adjustment to account for the fact that we are working with two different operationalizations of formal payment.

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 the appendix 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 tax payments. Specifically, the protection treatment caused a 2.2 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 7%, and so the protection treatment effect represents a 31% 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 2.3 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, the

¹⁷Our hypotheses are with respect to the information and protection treatments, not to their interaction, which we would not have adequate statistical power to test in any case. Running the regressions separately allows us to use the features of the `estimatr` package for the mean-centered interaction model.

similar coefficients indicate that the protection treatment induced people to engage primarily in formal payments.¹⁸ The effect for “only formal” payments is somewhat smaller, but it indicates that the 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.

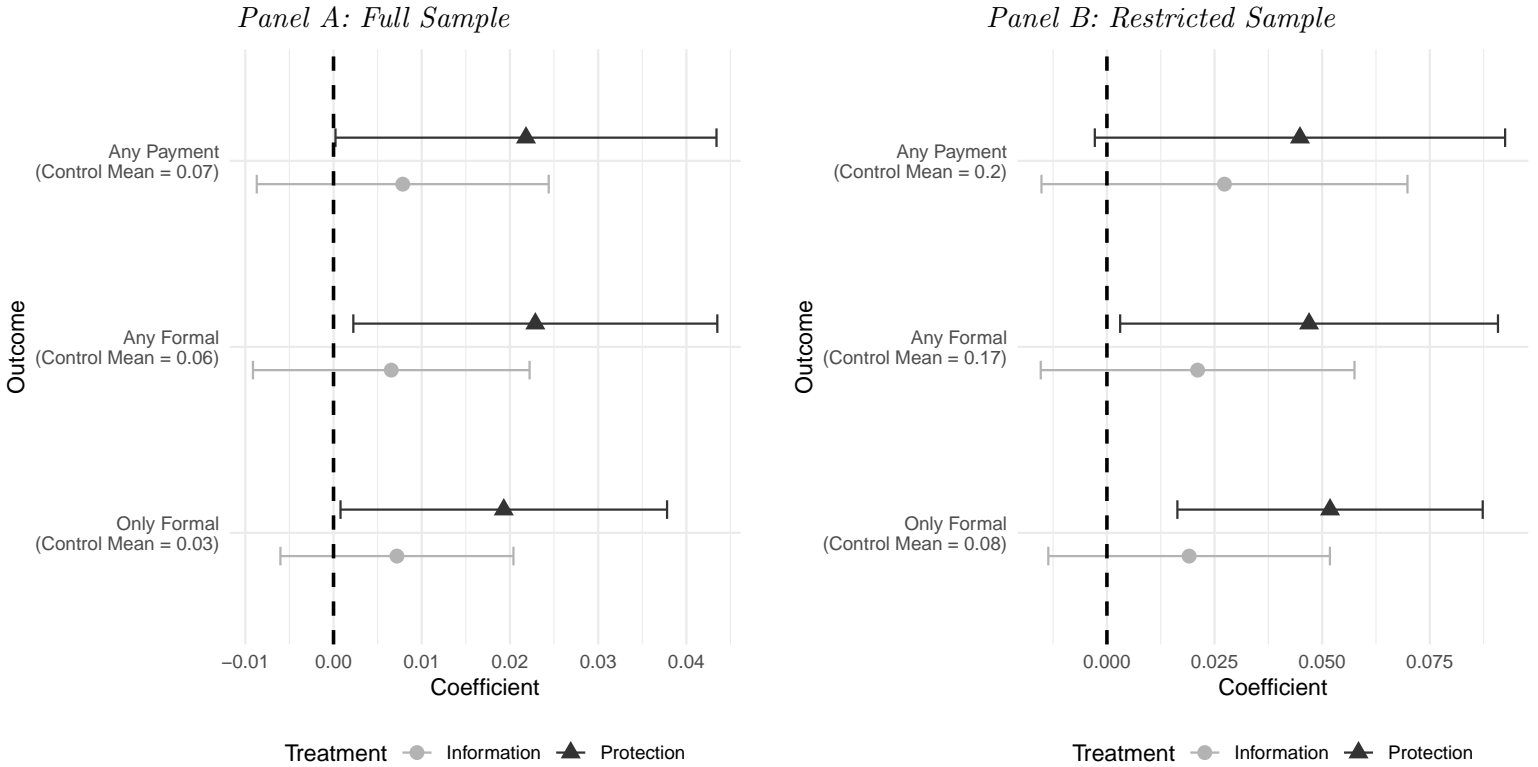
The results from Panel A are reinforced by the evidence presented in Panel B, which shows the same extensive margin analysis but on a subset of (pre-specified) payment categories that are high-volume and where we expect high levels of opportunistic bribes and rents (these include electricity, sanitation, and licenses for businesses, and education, health, life events, electricity, water, and sanitation for households). The protection treatment effects for the restricted sample are about 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 larger, but still not statistically significant.

Additional exploratory analysis sheds further light on the extensive margin results. Figure 6 presents the extensive margin effects separately for households (Panel A) and businesses (Panel B). While both households and businesses show an increase in payments from the protection treatments, the effects for households are 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).

We also examine the effects of the interventions across different payment categories. 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.

¹⁸In 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.

Figure 5: Extensive Margin Effects

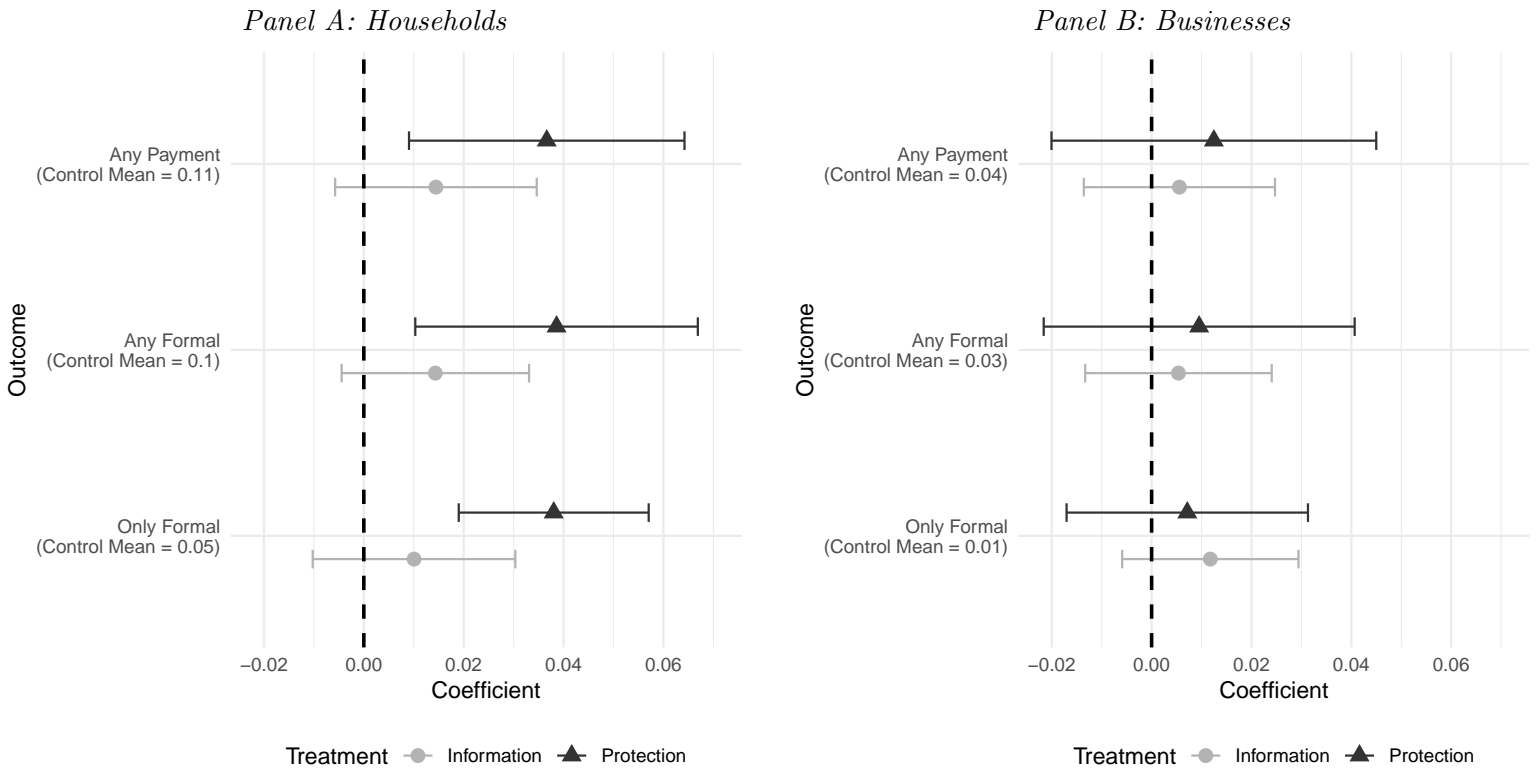


Notes: This figure shows the coefficients for the Protection and Information treatments on the extensive margins. Panel A shows all tax category while Panel B restricts the sample to a set of (pre-specified) payment categories that are most common and where we expect high levels of predation (these include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

The extensive margin effect estimates are robust to different covariate specifications (Tables A16 and A17). We also check the extensive margin effect estimates when including an indicator for whether *both* treatments were received (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.

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

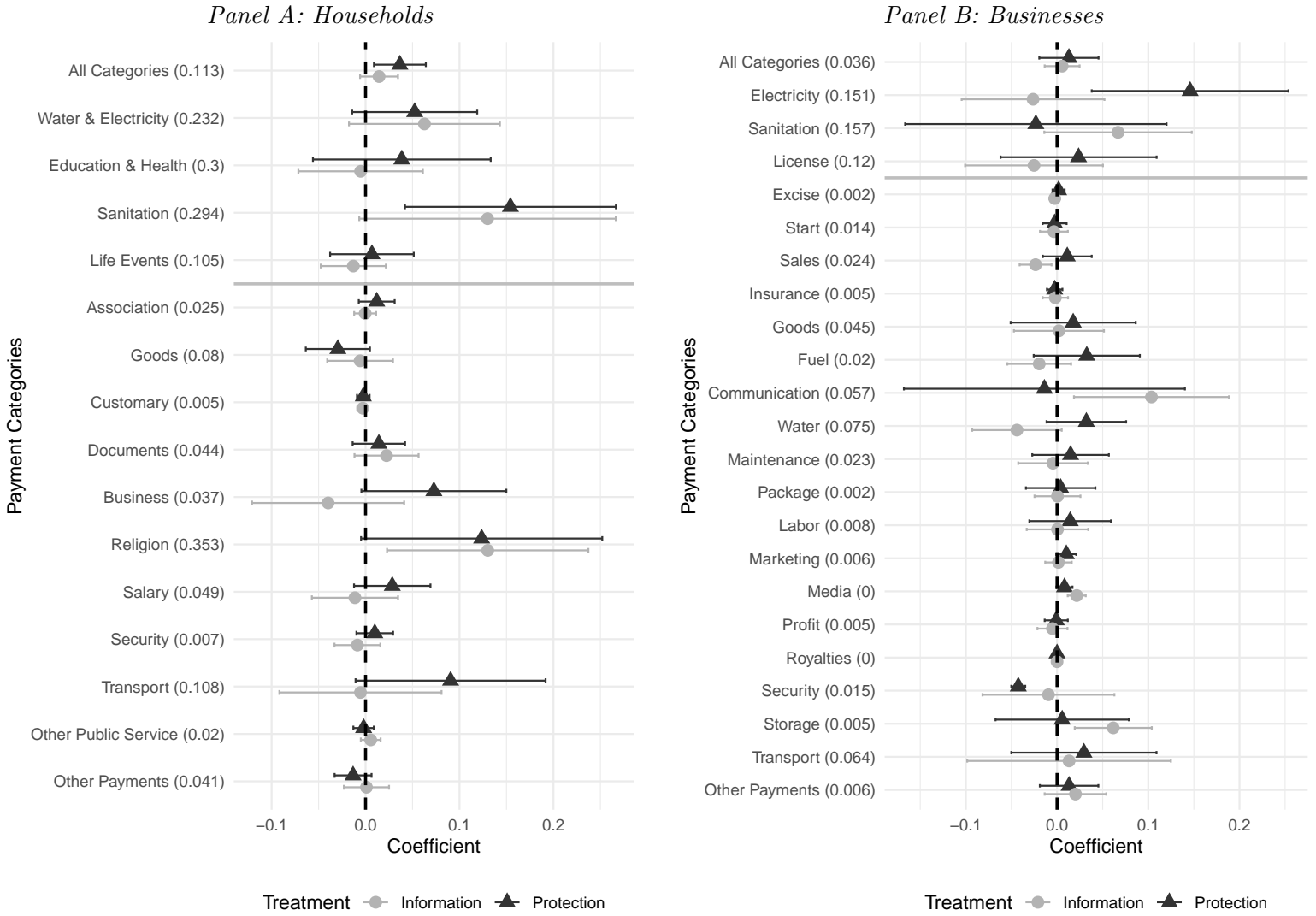
Figure 6: Extensive Margin Effects For Households and Businesses



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).

from 1 to 7 indicating no formal schooling through to post-university degree. Table A22 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.

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 horizontal line are those included in the restricted sample. The control mean for each category is in parentheses.

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 should decrease the payment citizens make when 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). This estimate is unbiased if there is no extensive margin effect. However, given that we have extensive margin effects, this estimate is biased 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 for the conditional on positives estimator. To construct these bounds on the effect for the always-payers, we use the extensive margin estimate to determine the share of units that were induced to pay. To estimate the upper bound on effect for always-payers, 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.¹⁹

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

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

¹⁹In line with our pre-analysis plan, in the appendix we also report conditional on *positive pre-treatment* outcomes estimates (subsetting to $Y_{i,0,j} > 0$ units, where $t = 0$ indexes pretreatment payments), which is unbiased if there are no trends under the control condition in whether people pay from the pre-treatment to post-treatment period. Given that we do observe strong trends, we construct Manski bounds on the conditional on positive pre-treatment outcomes estimator. As shown in the appendix, these bounds are much less informative than the trimming bounds on the conditional on positive post-treatment estimate.

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

6.2 ATE and Intensive Margin Results

Whether the interventions lead to a positive or negative average treatment effect on payment amounts depends on whether the extensive or intensive margin dominates. On the intensive margin, we hypothesize that the protection treatment would reduce payment amounts for all those already engaging with the state and that the information treatment would reduce payments for those who are in the collusive equilibrium, since citizens who are making official payments are assumed to already know their tax liabilities.

Figure 8 displays estimates of effects on payment amounts. At the top are the average treatment effect (ATE) estimates, which are precise zeroes for both information and protection. Given the positive extensive margin effects, the fact that we have net zero effects on average payment amounts suggests that average intensive margin effects are negative. The estimates presented below the ATE on Figure 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 42.62. We estimate that those paying non-zero amounts in the protection treatment are paying about half as much (USD -21.25 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 result in 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 -15.31 (not statistically significant) and the lower bound is USD -43.93 ($p < .01$), 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 -5.15 (not statistically significant) and the

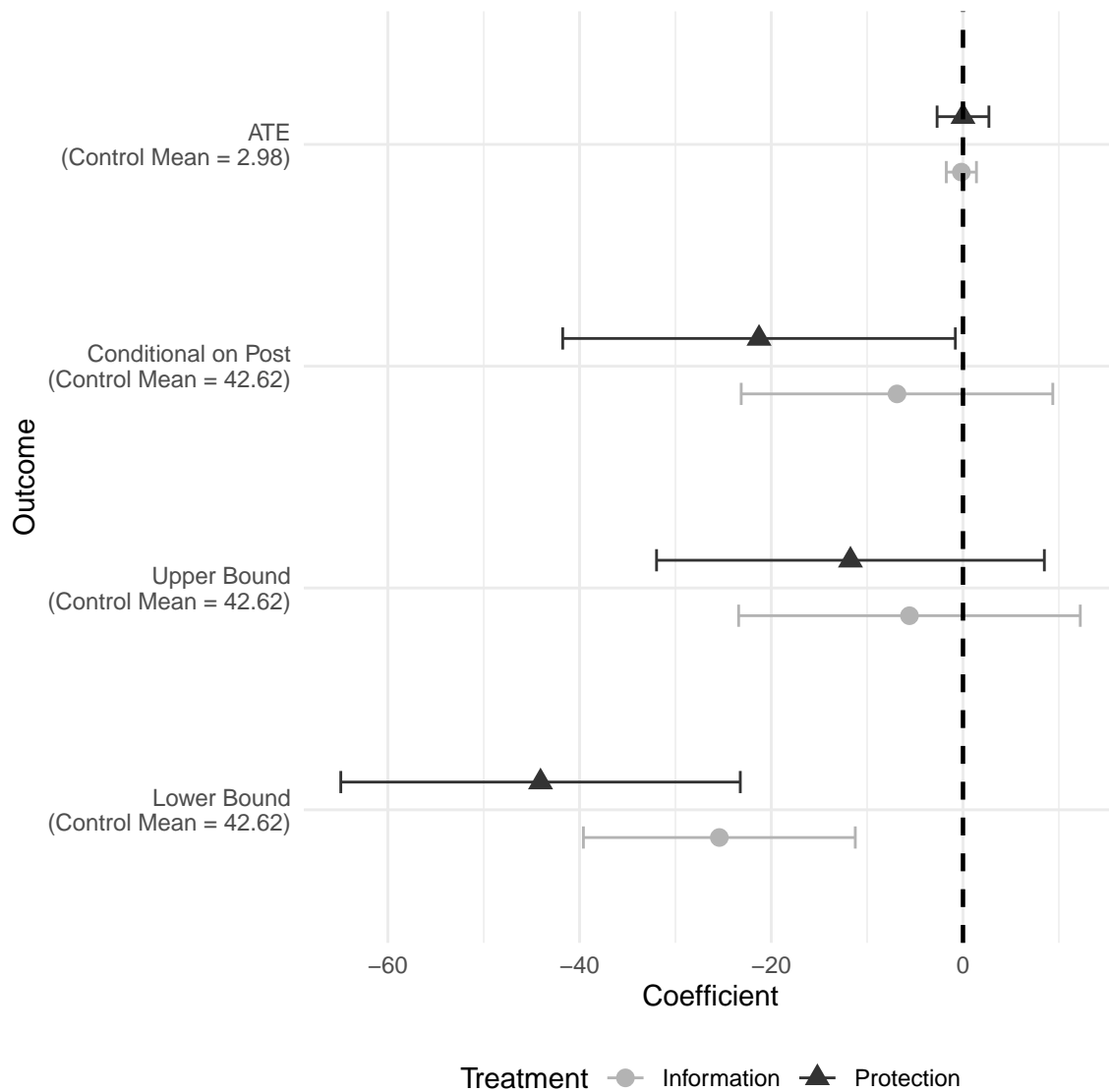
lower bound is USD -26.24 ($p < .01$).

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 dominate in water and electricity and household business activity, while for businesses, they dominate for license payments. The intensive margin effects dominate for households in goods-related fees and for insurance and security fees for businesses.

The Appendix displays additional results and robustness checks. Table A14 shows estimates for the higher-volume restricted sample. The estimated effects are qualitatively similar, but smaller in magnitude. This suggests that some, and possibly most, of the action is coming from relatively low-volume payment categories. Table A13 (5) shows the conditional on positive pre-treatment payments effects and Table A15 shows the associated Manski bounds. These are much less informative than the Lee bounds given that upper and lower Manski bounds are based on the extrema of the overall outcome distribution. Given the high skew in the distribution of payments, we estimate effects on outcome distributions winsorized at the 99th and 95th percentiles (Table A21). The estimated effects decline as we top-code the upper percentiles of the payment distribution. This suggests that the large average intensive margin effects are driven by lower payments in the top percentiles of the payment distribution, rather than a simple mean shift in the values of payments.

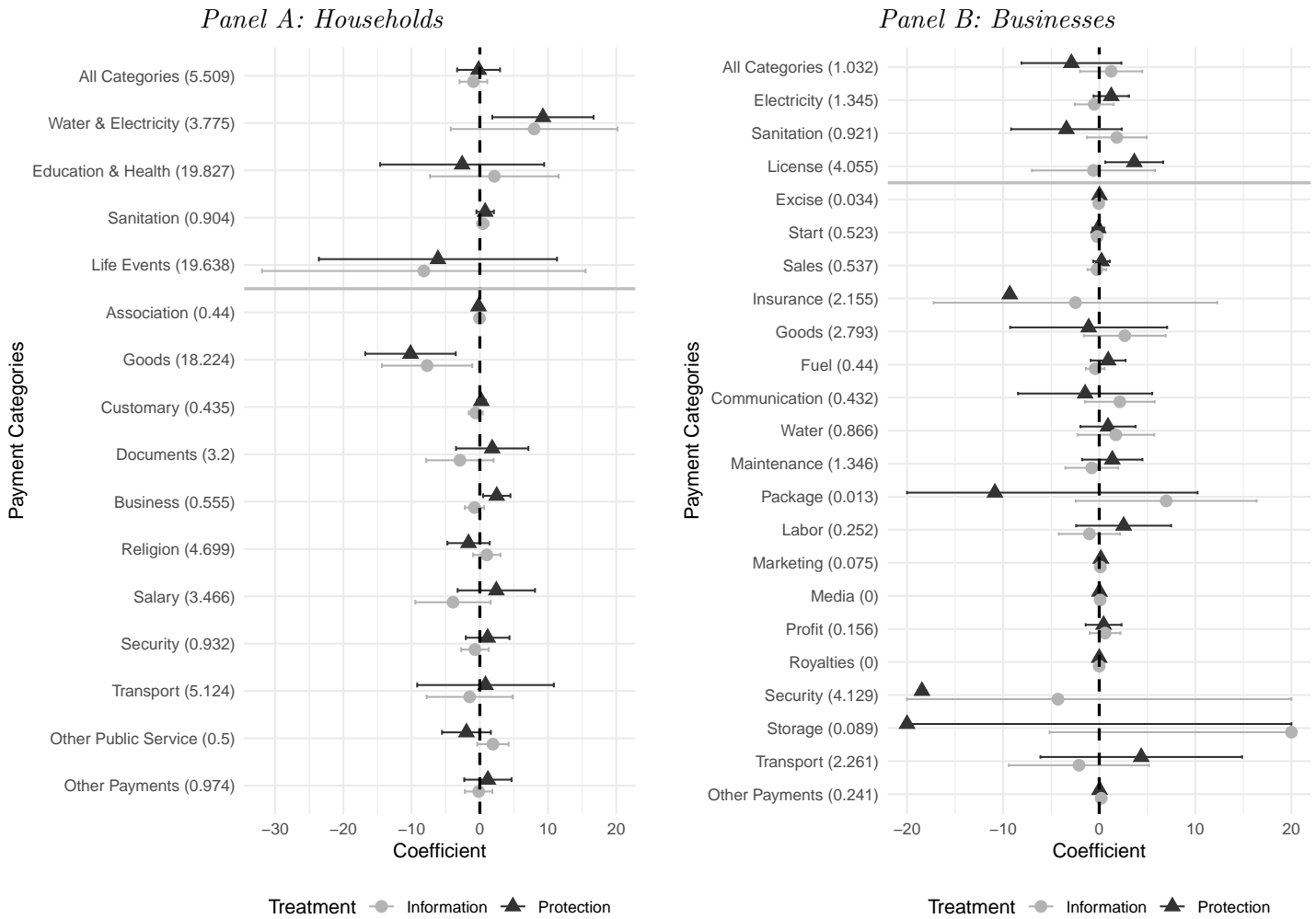
As discussed above, we estimate moderator effects for two measures of pre-treatment endowments to negotiation informal payments: a network z-score that measures connections to elites and education level. Recall that eligibility for our study required that the respondent be literate and able to operate a smartphone; as such, education levels in our sample are substantially higher than the overall population of Kinshasa. Table A22 shows a moderating effect of education with respect to the protection treatment on amounts paid. Higher levels of education push the conditional-on-positives effect toward zero and even toward becoming positive. Insofar as the intensive margin effect tracks with the conditional on positives effect, 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 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, particularly by offering them more protection from opportunistic state agents, has positive effects on the extensive margin—it increases the number of citizens paying formal taxes to the state and the number of citizens engaging with the state in the first place. It also has negative effects on the intensive margin, meaning that it reduces the amount that citizens are paying, which our theory indicates can be attributed to decreased bribe and rent payments.

These findings on the extensive margin effects of empowerment are not obvious and constitute the main contribution of this paper. As highlighted in the introduction, theory suggests contradictory effects of empowerment, by some accounts it could facilitate engagement and bargaining with the state (Levi, 1989) while, by other accounts, it could better enable citizens to evade the state (Scott, 2010). By modeling citizens as making a two-part decision over whether or not to engage with the state in the first place and, conditional on engagement, whether or not to insist on formal payments, we better capture the conditions under which empowerment will both change payments and induce citizens to start engaging with the state in the first place. Our evidence offers a micro-level perspective on the civil society side of 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 civil society.

Overall, we believe our theory and evidence helps 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. Citizens are only unable to avoid payment 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 also 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, which was more highly educated given our eligibility requirement for participating in the smartphone data reporting. But 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 very well be the case that information—or information combined with protection—would yield the greatest empowerment dividends for citizens, underscoring the need for more tests of citizen-centered empowerment interventions in different contexts.

Finally, we acknowledge that our theory and evidence reveal the short-run effects of citizen empowerment. However, they also provide insights into how information and protection might affect the welfare of citizens, the state, and street-level state agents in the longer run. 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 model yields the counter-intuitive insight that empowerment interventions, which are typically designed to reduce citizens' informal payments, might in fact, given our first extensive margin prediction, 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.

Citizen empowerment should also be welfare enhancing for the state insofar as it pushes more citizens towards engaging with the state in the first place and paying more formal taxes.²⁰ Yet, whether state coffers benefit from citizen empowerment could depend on factors such as the extent to which higher-level state agents collude with street-level state agents or whether 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 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. Our theory, however, abstracts away from strategic calculations by street-level state agents, and future work should do more to incorporate their responses to empowered citizens. Yet, our theory and evidence also suggest 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, and if the state were to use these funds for compensation, then citizen empowerment could also be welfare enhancing for street-level state agents in the longer run. This points to the importance of future research to incorporate the calculations of higher-level state authorities in their strategic responses to an empowered citizenry.

²⁰While 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.

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Appendix

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A 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.

A.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 households obtains greater property rights protections after paying property taxes or when a business obtains a greater ability to advertise and expand its customer base after it pays a fee to formally register.

If the citizen engages, they have a true payment liability τ^* , which the state agent knows but the citizen does not. Instead, the citizen has a prior belief about her payment liability, μ_τ . When engaging, the citizen can either (1) collude privately with the state agent over a bribe to be paid in lieu of the legal amount, or (2) insist on making an official 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 (Shleifer and Vishny, 1993).²¹ 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 .²²

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

A.2 Collusive bargaining

The joint surplus from collusion is $S = \mu_\tau + r + c_A - (C_B + C_C + r) - b(c_B + c_C)$. Note that the surplus decreases in b because the level of bribe increases the cost of collusion.²³ The Nash bargaining solution implies:

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

²¹The 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.

²²We 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 A.4 we will discuss what determines r .

²³Note 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 $h'(u_C) = -\frac{1+c_C}{1-c_C}$, 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, $h'(u_C) = -1$.

The dollar 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.

A.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.

A.4 Predictions for the effects of empowering citizens

There are two ways in which we expect empowerment to work. First, officials know the true payment liability, τ^* , while citizens only have a guess, μ_τ . We construe additional information as intervening on μ_τ . 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.

In what follows, 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 or official equilibrium and are not induced to switch by changes in μ_τ or r . The extensive margin predictions capture the effects on the share of citizens paying or the amounts paid when citizens are induced to switch equilibrium payment behavior by the parameter changes. We note that we can derive predictions for the effects of decreases in μ_τ and r on official payments τ , bribes b , and rents r , as summarized in Figure 3 and Table A10 in the Appendix. However, we state our hypotheses in terms of *total payments*. This is due to the empirical challenges of reliably distinguishing between formal and informal payment amounts in our self-reported data.

Intensive Margin Effects

We start by looking at the intensive margin effects on the amount paid by the citizens who start and stay in the collusion equilibrium. To assess the intensive margin effects on a bribe payment, consider the equilibrium bribe in equation 3. For citizens in the collusion equilibrium, lowering the rent (r) reduces the payoff of 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 μ_τ will also reduce the bribe in equilibrium. We note that we focus on predictions for lowering μ_τ because it is reasonable to assume that this is what happens in contexts like the DRC where citizens typically over-estimate their statutory tax obligations.²⁴

The predictions are different for citizens who start and stay in the ‘official payment’ equilibrium, meaning that they are already making formal payments plus paying any rents that are demanded of them. For citizens in the official equilibrium, lowering r directly reduces the citizen’s payment. However, changing μ_τ will have no effect on

²⁴The effects of increasing μ_τ are always the opposite, so increasing μ_τ would increase the bribe in equilibrium.

the payment amount on the assumption that once a citizen makes an official payment they learn their true statutory payment obligation.²⁵

This yields the following predictions:

- **Intensive Margin Effect 1:** Lowering r or lowering μ_τ 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 should decrease the payment for citizens who start and stay in the official payment equilibrium (by decreasing rent payments). Lowering μ_τ should have no effect on the amount paid by these citizens.

Extensive Margin Effects

Our first extensive margin effect is for citizens who, prior to the intervention, are not engaging with the state. For some of these citizens, changes to r and μ_τ will induce engagement with the state. Lower r or lower μ_τ decreases the costs of both bargaining and insisting on official payments. Thus, citizens who begin by not being legible could be induced either into the collusion equilibrium, in which case they will make higher payments in the form of bribes, or into the official equilibrium, in which case they will insist on making legal payments and also possibly face rent extraction from state agents.

For citizens who start in the collusion equilibrium, changes to r and μ_τ will affect whether they prefer to switch to the official equilibrium. Lowering r or lowering μ_τ will lower the costs of switching to the official equilibrium, making that outcome more likely. Citizens that switch from bargaining to official payments will pay less informally but will also start to make formal payments.²⁶ This would occur if the costs of collusion are sufficiently high. The effects of lower μ_τ and r on total payments is thus indeterminate, but we can draw inferences about what is driving the results depending on whether we observe higher or lower payments.

Thus, our main extensive margin predictions are:

- **Extensive Margin Effect 1:** Lowering r or lowering μ_τ 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 or lowering μ_τ increase the share of citizens making official payments.

We note that these predictions illustrate how the model can have explanatory power even in contexts where there is no scope for collusive bargaining; in such cases empowerment can push citizens from being invisible to the state directly to the official payment equilibrium, as shown in Panel B and D of Figure 3.

All in all, this model highlights the importance of testing different types of empowerment interventions when it is difficult to anticipate whether information or power asymmetries present the greatest obstacle to official payments. The model shows how additional information is unlikely to impact citizens that are already making official payments. Moreover, information and power asymmetries can be independent of one another; for instance, it is possible for citizens to have perfect information on some payments, yet still face demands for rents by state agents due to a power asymmetry. This underscores the importance of investigating whether it is more effective to provide citizens with better information or strengthen their influence vis-à-vis state agents. We return in the conclusion to a discussion of scope conditions and of extensions to the theory developed here.

A.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:

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

²⁵Even if this assumption did not hold and the citizen had some uncertainty about the portion of a payment that was formal or a rent, revealing this information would not change the total amount paid since r is fixed. If, however, r were to be the outcome of bargaining between the citizen and the state agent, knowledge about the tax liability could impact r .

²⁶One would only switch if the formal payment amount plus the rent is lower than the bribe, thus r should be lower than b assuming no exorbitant difference in the cost of making an official payment vs. collusion

Simplify:

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

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

Getting the other b isolated:

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

Divide by $(1 + c_C)$:

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

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

Getting all the b's to the left:

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

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

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

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

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

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

A.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:

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

Couldn't we just say?

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

Assuming that is right, now focusing on the rest:

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

A.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 μ_τ :

Bribe iff:

$$V^E - b(1 + c_C) - C_C > V^E - \mu_\tau - r - c_A$$

Plugging in b^* :

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

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

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

Engage iff:

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

Plugging in b^* :

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

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

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

Engage iff:

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

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

B Sampling and Randomization

B.1 Smartphone

A respondent was considered eligible for recruitment into the smartphone data collection activity if they were literate enough to read or write a letter in French and if the enumerator assessed them as having been willing to participate in the survey. If a respondent met these conditions and the target for the avenue had not yet been reached, the enumerator invited the respondent to take part in the smartphone data activity. Note that the targets for the avenues were per-determined and based on the first step of the random assignment, with a target of 200 households and 200 businesses. To ensure that the subsample of participants in the smartphone survey was random conditional on eligibility constraints, enumerators visited households on each avenue in a random order. Enumerators then invited households who agreed to participate in the smartphone data collection activity to attend training at the office of the research team in Kinshasa. A local research team then provided, at training workshops in the office, instructions on how to use the smartphones and on how to enter and upload their tax data on a weekly basis for up to 20 weeks. The research team recruited households over eight weeks on a rolling basis as enumerators implemented the survey. In return for their regular reporting, participants received a small compensation and were allowed to keep the smartphones at the conclusion of the study. The training emphasized that the smartphone data collection activity was being undertaken by the same research team that had conducted the household and business surveys.

B.2 Recruitment

The ODEP advisors used the following script:

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

B.3 Randomization

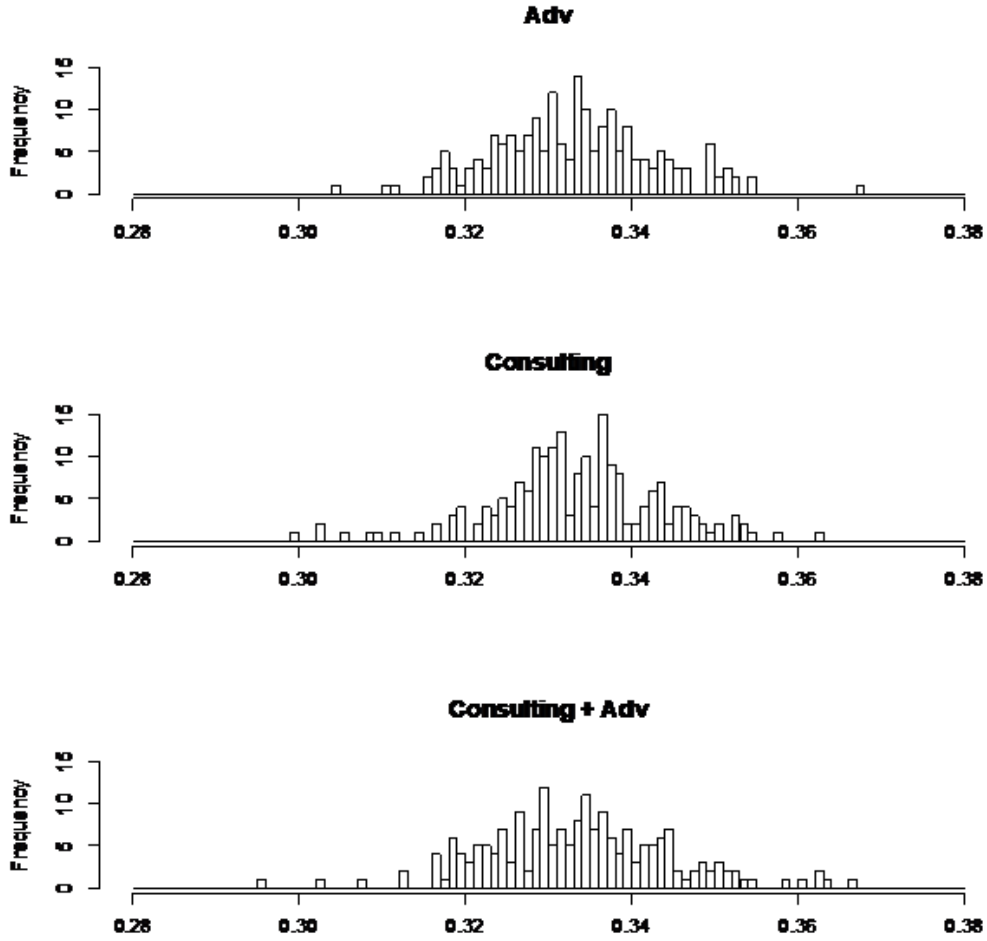
This second-stage random assignment was a form of “restricted random assignment” that required that the assignments be balanced within strata defined by commune and household versus business survey sample (Bruhn and McKenzie, 2009). The implementation was done by generating 15,000 treatment assignment 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. Figure A1 shows that the restricted randomization did not depart very much from a uniform assignment (which would yield second stage assignment probabilities of 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:

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

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

Figure A1: *Distribution of second-stage treatment assignment probabilities*



Notes: Distribution of second-stage treatment assignment probabilities for respondents from treated avenues. The histograms show that the treatment assignment probabilities did not depart from what would have obtained under uniform random assignment (that is, uniform assignment probabilities of $1/3$)

C Balance

C.1 Baseline Data Collection

Table A1: 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

Table A2: 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
FE Cluster	Commune Avenue	Commune& Sector Avenue	Commune& Sector Avenue	Commune Avenue	Commune & Sector Avenue	Commune& Sector Avenue

Standard errors in parentheses

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

C.2 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 [A3](#) and [A4](#).

Table A3: 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
All Categories	324.78	1,422.94	0.00	0.00	0.00	66.67	191.11	543.33	25,446.11
Observations	534								

Table A4: 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
Religion	195.55	918.70	0.00	0.00	0.00	0.00	41.11	265.56	14,400.00
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
All Categories	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								

C.3 Formal vs informal payments

Table A5: 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

C.4 Payments and benefits

Figure A2: *Household benefits and payments*

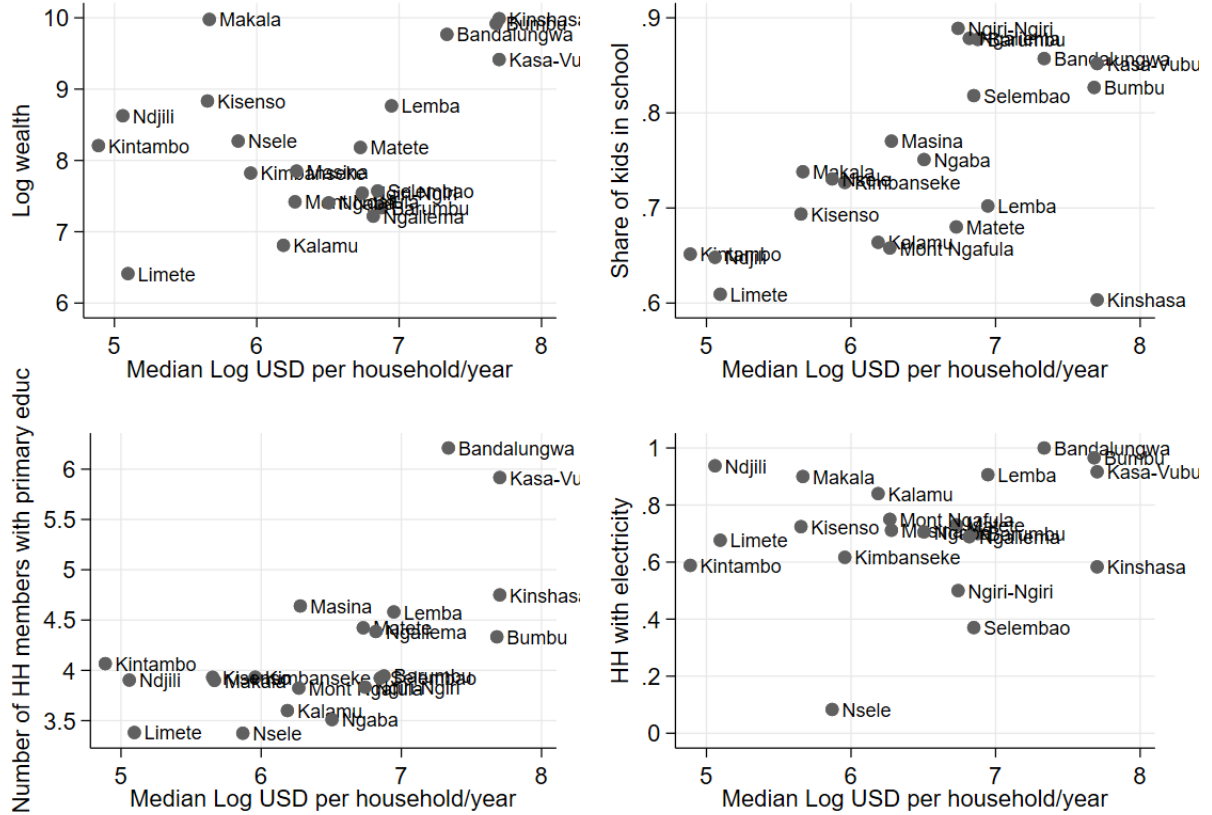
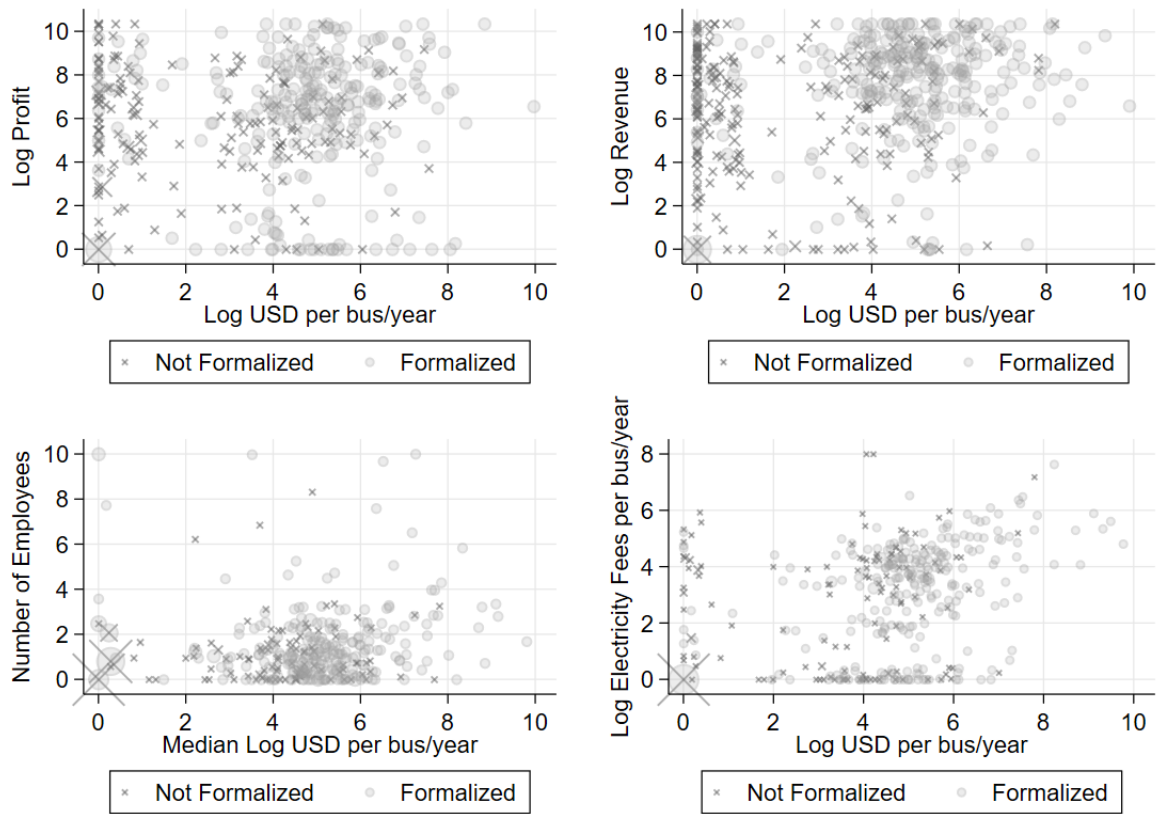


Figure A3: *Firm benefits and payments*



C.5 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
Religion	214	(71%)	89	(29%)	303
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
All Categories	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
All Categories	839	(58%)	610	(42%)	1,449

C.6 Perceived 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
Religion	37	(40%)	55	(60%)	92
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
All Categories	999	(64%)	566	(36%)	1,565

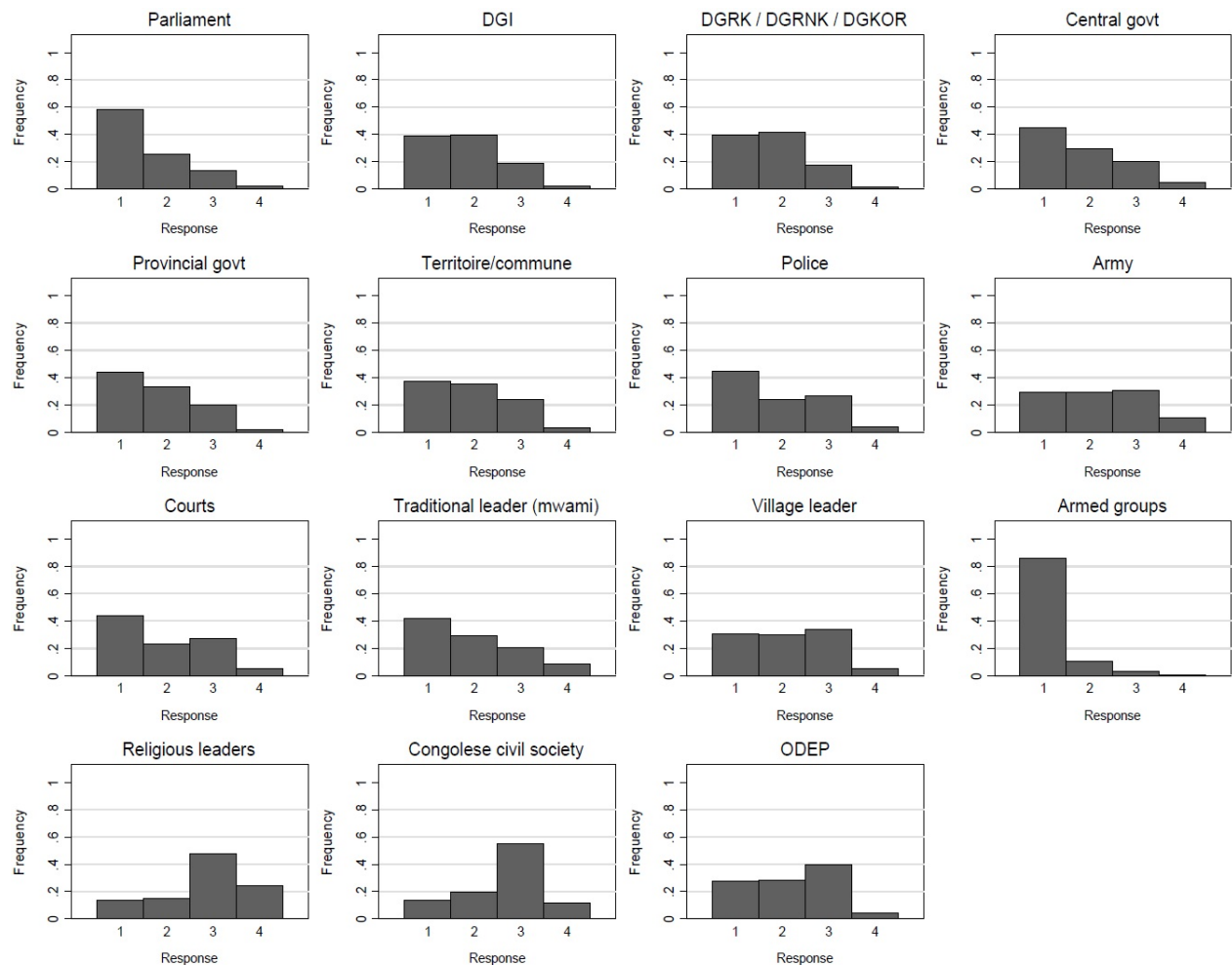
C.7 Network Connections

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.8 Trust in institutions

Figure A4: *Trust in state agencies and in civil society organizations*



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

C.9 Theoretical Framework

C.9.1 Differences from our Pre-Registered Framework

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

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

C.9.2 Comparative Statics

Table A10: Effects of lowering r and μ_τ 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> : $\tau \uparrow, r \uparrow$

C.10 Additional Results and Robustness

Table A11: 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	0.022** (0.010)	0.023** (0.010)	0.019** (0.009)	0.045* (0.023)	0.047** (0.021)	0.052*** (0.017)
Information	0.008 (0.008)	0.007 (0.007)	0.007 (0.006)	0.027 (0.020)	0.021 (0.017)	0.019 (0.016)
R ² (Protection)	0.166	0.152	0.131	0.121	0.112	0.125
R ² (Information)	0.166	0.151	0.132	0.117	0.114	0.123
Observations	59,785	59,785	59,785	12,013	12,013	12,013
Control Mean	0.07	0.06	0.03	0.2	0.17	0.08

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

Table A12: 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	0.037** (0.013)	0.039** (0.014)	0.038*** (0.009)	0.012 (0.015)	0.010 (0.014)	0.007 (0.011)
Information	0.014 (0.010)	0.014 (0.009)	0.010 (0.010)	0.006 (0.008)	0.005 (0.008)	0.012 (0.008)
R ² (Protection)	0.191	0.175	0.164	0.090	0.078	0.067
R ² (Information)	0.191	0.174	0.159	0.092	0.082	0.073
Observations	29,571	29,571	29,571	30,214	30,214	30,214
Control Mean	0.11	0.1	0.05	0.04	0.03	0.01

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

Table A13: Intensive Margin Effects and ATE of Protection and Tax Consulting

	<i>Dependent Variable: Amount Paid (USD)</i>				
	ATE (1)	Conditional on post (2)	Lower bound (3)	Upper bound (4)	Conditional on pre (5)
Protection	-0.444 (1.212)	-21.247** (9.745)	-43.926*** (9.670)	-15.305 (10.145)	0.058 (1.876)
Information	-0.207 (0.763)	-7.325 (7.891)	-26.240*** (6.936)	-5.149 (8.484)	3.454* (1.979)
R ² (Protection)	0.018	0.159	0.240	0.160	0.035
R ² (Information)	0.017	0.111	0.166	0.111	0.031
Observations	59,785	4,706	4,525	4,503	8,633
Control Mean	2.98	42.62	42.62	42.62	6.72

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

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

	<i>Dependent Variable: Amount Paid (USD)</i>				
	ATE (1)	Conditional on post (2)	Lower bound (3)	Upper bound (4)	Conditional on pre (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 ² (Protection)	0.030	0.124	0.249	0.126	0.036
R ² (Information)	0.025	0.108	0.221	0.110	0.031
Observations	12,013	2,566	2,427	2,417	6,122

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

Table A15: Conditional on Positives in Baseline with Horowitz-Manski Bounds

	<i>Dependent Variable: Amount Paid (USD)</i>		
	Conditional on pre (1)	Lower Bound (2)	Upper Bound (3)
Protection	0.058 (1.876)	-3881.050*** (93.519)	0.057 (1.876)
Information	3.454* (1.979)	-109.945 (82.662)	3.454* (1.979)
R ² (Protection)	0.035	0.707	0.035
R ² (Information)	0.031	0.685	0.031
Observations	8,633	8,633	8,633
Control Mean	6.72	6.72	4,239.36

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

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	0.023* (0.011)	0.023* (0.011)	0.019* (0.011)	0.045 (0.027)	0.048* (0.024)	0.052** (0.023)
Information	0.005 (0.008)	0.004 (0.008)	0.005 (0.007)	0.028 (0.023)	0.021 (0.020)	0.018 (0.017)
R ²	0.155	0.141	0.117	0.096	0.092	0.098
Observations	59,785	59,785	59,785	12,013	12,013	12,013
Control Mean	0.07	0.06	0.03	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	0.021* (0.011)	0.023** (0.011)	0.016 (0.010)
Information	0.006 (0.008)	0.004 (0.008)	0.006 (0.008)
R ²	0.151	0.136	0.107
Observations	59,785	59,785	59,785
Control Mean	0.07	0.06	0.03

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

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	0.022** (0.010)	0.023** (0.010)	0.019** (0.009)
Information	0.008 (0.008)	0.007 (0.007)	0.007 (0.006)
Both	0.012 (0.016)	0.012 (0.014)	0.002 (0.013)
R ² (Protection)	0.166	0.152	0.131
R ² (Information)	0.166	0.151	0.132
R ² (Both)	0.173	0.159	0.142
Observations	59,785	59,785	59,785
Control Mean	0.07	0.06	0.03

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$ 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	0.022** (0.010)	0.023** (0.010)	0.020** (0.009)
Information	0.008 (0.008)	0.006 (0.007)	0.007 (0.006)
R ² (Protection)	0.166	0.152	0.132
R ² (Information)	0.166	0.151	0.132
Observations	59,785	59,785	59,785
Control Mean	0.07	0.06	0.03

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

Table A20: 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	-0.444 (1.212)	0.181 (0.834)	0.155 (0.495)
Information	-0.207 (0.763)	-0.479 (0.558)	-0.268 (0.321)
R ² (Protection)	0.018	0.044	0.060
R ² (Information)	0.017	0.040	0.058
Observations	59,785	59,785	59,785
Control Mean	2.98	2.38	1.84

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

Table A21: 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	-21.247** (9.745)	-10.255* (5.557)	-6.122** (2.876)
Information	-7.325 (7.891)	-6.951 (5.625)	-4.409 (3.092)
R ² (Protection)	0.159	0.263	0.353
R ² (Information)	0.111	0.229	0.321
Observations	4,706	4,706	4,706
Control Mean	42.62	34.05	26.31

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

Table A22: Heterogeneous Treatment Effects

	Any Payment (1)	Any Formal (2)	Only Formal (3)	ATE (4)	Conditional on Post (5)
Protection	0.022** (0.010)	0.023** (0.010)	0.019** (0.009)	-0.444 (1.212)	-21.247** (9.745)
Information	0.008 (0.008)	0.007 (0.007)	0.007 (0.006)	-0.207 (0.763)	-7.325 (7.891)
Education (Protection)	0.000 (0.002)	-0.002 (0.003)	-0.001 (0.002)	-0.857 (0.566)	-4.509* (2.558)
Education (Information)	0.002 (0.004)	0.001 (0.004)	0.002 (0.003)	0.176 (0.405)	0.615 (3.042)
Network Z-Score (Protection)	0.004 (0.006)	0.006 (0.006)	0.007 (0.004)	1.073 (0.937)	2.241 (5.490)
Network Z-Score (Information)	0.004 (0.007)	0.003 (0.007)	0.005 (0.005)	-0.129 (0.444)	-2.219 (3.087)
Protection \times Education	0.003 (0.005)	0.005 (0.005)	0.006 (0.004)	1.527* (0.787)	10.251** (4.083)
Information \times Education	0.000 (0.004)	0.001 (0.004)	0.002 (0.004)	-0.685 (0.660)	-8.121 (6.075)
Protection \times Network Z-Score	-0.006 (0.009)	-0.009 (0.009)	-0.007 (0.009)	-1.682 (0.994)	-9.267 (6.369)
Information \times Network Z-Score	-0.001 (0.010)	0.001 (0.009)	0.002 (0.007)	2.037 (1.710)	16.814 (15.013)
R ² (Protection)	0.166	0.152	0.131	0.018	0.159
R ² (Information)	0.166	0.151	0.132	0.017	0.111
Observations	59,785	59,785	59,785	59,785	4,706
Control Mean	0.07	0.06	0.03	2.98	42.62

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 A23: 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 ² (Protection)	0.094	0.082	0.071	0.017	0.338
R ² (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

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

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